Not Even Wrong

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Introduction

March 17, 2004
Categories: Uncategorized

This is an experimental new weblog. Let's see if I can find anything of interest to put on here, and how long it takes for someone else to find out it exists.

Comments

1. **Peter**
   March 21, 2004

   Hi Pyracantha,
   I enjoyed seeing your weblog, the posting about "Science Porn" was pretty funny and very much on target. I hadn't seen 't Hooft's page on how to become a theoretical physicist before. Interesting, but you don't need to know absolutely ALL of that stuff!

2. **Pyracantha**
   March 19, 2004

   Dear Dr. Woit:

   At the risk of putting myself forward I write this comment. I found your blog through Sean Carroll's "Preposterous Universe" which I read daily. I am a beginning math/physics student – in my middle age. I have NO crackpot "theory of everything" to promote nor will I ever have one. I am an artist who was inspired to begin studying math/physics by a trip to Fermilab in 2000. I have my own blog, "Electron Blue," which is my journal about this process. It's available on the Website whose URL I have posted above. I will be reading your blog and understanding as much as I can. I hope I will understand more as time goes on.

   Yours, Pyracantha

3. **Peter**
   March 19, 2004

   Thanks! This encourages me to try and write something more here, also I hope to find time to finish writing up the last few representation theory notes and get to work writing up the notes for my QFT class.

4. **Santo D'Agostino**
   March 18, 2004

   Your course notes for Lie groups and representations are very good, and your criticisms of string theory are interesting.

   And yes, someone out here now knows that your blog exists.

   Best wishes,
   Santo

Talk by David Gross

March 19, 2004
Categories: Uncategorized

Last Friday I went to hear a talk by David Gross at the CUNY Graduate Center on "The Coming Revolutions in Fundamental Physics". This was more or less Gross's standard advertisement for string theory that he has been giving for nearly 20 years now. He explicitly started off with the claim:

"Fundamental Physics = String Theory"

His first PowerPoint slide was a quote from "Hannibal" by Thomas Harris, a sequel to the novel "Silence of the Lambs". Evidently late in this novel the main character is depicted manipulating the "symbols of string theory", with "equations that begin brilliantly and end in wishful thinking". It sounds like Harris really has got the right idea about string theory, but Gross said his talk was designed to argue that string theory was not wishful thinking.

He then went on to claim that in 3-4 years there will be a headline in the New York Times about the discovery of supersymmetry at the LHC. From what I can tell, the LHC should have first beams in 2007, but even if everything goes according to plan, it won't be until 2008 that the experiments there will have accumulated a non-trivial amount of data and analyzed it. Experience with colliders generally has been that getting them running at a useful luminosity can take quite awhile after they are first turned on. So 3 years from now is definitely out, 4 years is optimistic. This is now getting close enough that Gross and others seem intent on ignoring the failures of string theory, desperately hoping that superpartners will pop out of the LHC, thereby providing at least some vindication of the train of reasoning that lead to string theory. What will be interesting to see will be what Gross et. al. do when this doesn't happen. Will they drop string theory? Quite possibly the LHC will revolutionize physics by showing us what is really causing the spontaneous breaking of the electroweak gauge symmetry. If this happens, everyone will abandon string theory and start working on this, 1984-2008 then becoming a period in the history of physics that particle theorists try and not think about.

Gross's talk contained the usual tendentious pro-string theory points, here's a few of them with commentary:

1. " String theory is in a period like that of 1913-1925, it's like the Bohr model, we're waiting for the analog of Heisenberg's or Schrodinger's breakthroughs"

The problem with this is that the Bohr model was actually predictive, for instance it predicted a lot about atomic spectra that could be experimentally checked. There clearly was something right about the Bohr model, there is no good evidence there is something right about string theory.

2. "String theory is better than QFT, because QFT Feynman diagrams have these interaction vertices you can assign any interaction strength you want to"

This is not true of gauge theories, the different vertices are related by gauge symmetry. True you have to pick over-all gauge groups and representations, and the Higgs sector is problematic, but the claim that there is just one string theory is just wishful thinking.

3. "String theory is better than QFT because interactions are not at points, so short distance behavior is better"

Gross should be well aware that asymptotically free gauge theories are extremely well-behaved at short distances despite having point-like interactions, since he discovered this. It is also true that string theory perturbation theory is only known to be well-behaved up to two loops. My colleague Phong claims that higher loops remain very much not understood.

4. "String theory is a consistent, finite quantum theory of gravity"

Simply not true. Peturbative string theory is a divergent expansion, non-perturbative definitions don't work for four large flat dimensions, rest small.

5. "String theory inspired brane-world scenarios, although I don't really believe these"

Why would you think that an argument in a theory's favor was that it inspired some clearly wrong models that you don't believe and that don't predict anything?

While Gross mentioned the "discretium", he didn't really explain exactly how disastrous this is for string theory, since it makes it essentiallly vacuous. He made a big deal of string theory implying that our notions of space and time need to be changed, but made it clear that no one really has a viable idea about how to change them. He puts his hopes in the fact that we still don't understand what string theory is. This seems to me to be exactly the sort of wishful thinking that he claimed at the beginning was not what string theorists were doing.

His talk went on for more than an hour and a half. Several questions from the audience were taken, including one from Michio Kaku who claimed that dark energy was evidence of supersymmetry and asked about theories with two times. Gross didn't seem interested in saying much about such theories. I noticed that two string theory postdocs I know were in the audience. They've both told me that they think the subject is at a point of crisis and they are thinking of quitting. I don't think anything Gross said was likely to encourage them to continue.

Comments

1. **Arun**
   June 22, 2004

   Epicycles, aether, string theory?

2. **Peter**
   March 27, 2004

   I don't believe at all that it would be a "stunning triumph" to find a consistent version of string theory that is compatible with any QFT. Quite the opposite: it would be a stunning failure. It would mean that, as a theory of particle physics, string theory was completely and utterly vacuous. There's no reason other than wishful thinking to believe that such a theory would predict anything (if your theory fundamentally has nothing to say about matter, there's no reason to believe it's going to be able to precisely predict anything about any physical process).

   Gross doesn't believe this nonsense. He's known to start quoting Churchill, shouting "Never, never, never, never give up!" when people like Susskind (or Srednicki) start talking about how great it would be to have a theory that is compatible with anything. At his CUNY talk and elsewhere he emphasized that it is the role of particle theorists to find a theory that explains the things about particle physics that the standard model doesn't, not to congratulate themselves for having found a theory that is consistent with anything.

   Gross now stakes his hopes on the idea that the presently known version of string theory is inconsistent and that when a consistent theory is found it will have better uniqueness properties. I (and many others, including Susskind) think this is wishful thinking. I also think he should ditch his powerpoint slide that says that string theory is a finite and consistent theory of quantum gravity, when later in his talk he is pinning his hopes on its inconsistency.

3. **Matt**
March 27, 2004

I think Mark's point is this: it would be a "stunning triumph" to find a candidate for a UV completion of the Standard Model including gravity. Surely this would make new predictions. Granted, there is the issue that perturbative string theory is not provably finite. But, this does not mean the string theory would not let us calculate things we could not otherwise do.

Of course, this won't lead us immediately to the "theory of everything," whatever that might mean. But it could be an important step. We could try to test, for instance, whether the real world looks like some intersecting brane scenario.

4. **Peter**
March 26, 2004

We really do strongly disagree here.

First of all, even if you believe in finiteness of higher loops in string perturbation theory, perturbative string theory is not a finite theory. All evidence is that the expansion in the string coupling constant is a divergent series. What you've got is presumably an asymptotic expansion. This may give a useful approximation if the string coupling constant is very small, a useless one if it isn't. Saying that perturbative string theory is a "finite, consistent theory of quantum gravity" is simply not true. You can't sum the series to get something finite, and if you cut it off at a finite order your theory is inconsistent (not unitary).

So if you can get any QFT + gravity out of perturbative string theory + a choice of background, what have you actually got? You've got an exceedingly complex theory that predicts absolutely not a single thing about anything and is inconsistent to boot. Would this be a "stunning triumph"?

If you want to engage in extreme wishful thinking and believe that string theorists will come up with a finite, non-perturbative string theory that could have any QFT as its low energy limit, calling such a thing a "stunning triumph" would still be complete hype. It would predict nothing about anything. Theorists should save terms like "stunning triumph" for the day they actually manage to make a single solid prediction that in anyway goes beyond the standard model.

5. **Mark Srednicki**
March 26, 2004

"While Gross mentioned the "discretuum", he didn't really explain exactly how disastrous this is for string theory, since it makes it essentiallly vacuous."

I strongly disagree with this. The extreme case of the discretuum is that *every* QFT is realizable as the low-energy limit of string theory. This probably isn't right, but suppose it is. Then we could take *any* QFT (with some exceptions: no Landau poles below the Planck scale, for example), include gravity, and have a finite quantum theory (assuming string finiteness holds up, which it may not). This should be considered a stunning triumph!

Furthermore, probably not *every* QFT is realizable in this way. Some are, some aren't. Which class is the Standard Model in? It seems to me that this is a very important (though also very hard) question.

The great dream was that there would be only *one* QFT realizable as the low energy limit of string theory, and that it would turn out to be the Standard Model. It would've been great, but right now it doesn't seem likely. That certainly doesn't mean that string theory is vacuous.

6. **Peter**
March 24, 2004

Hi Fred!

The interesting thing about Gross's talk was that he was kind of going out on a limb on the issue of supersymmetry at the LHC. I'm more and more convinced that he and a lot of others are getting discouraged about string theory and no supersymmetry at the LHC will be the final straw.

The worst possible thing for particle theory would be if the standard model Higgs shows up, behaving like a standard elementary scalar field with a certain mass. Then we would still be in the situation of having no idea where the Higgs potential or couplings come from, and no prospects for doing experiments in our lifetime to find out.

I'm also hoping the LHC doesn't find a standard Higgs field, but evidence for some more interesting way of breaking electroweak gauge symmetry, one that we haven't thought of yet.

7. **Fred**
March 24, 2004

Hi Peter..

If they don't find any SUSY at LHC, I doubt it will be the death knell of String Theory. Alas, there is enough degrees of freedom there to just argue that it breaks at a higher scale, no big deal!

The nasty thing of course is that it just adds more fine tuning to minimal SUSY, and much of the original point would be lost.

I'm more interested though, in the (seemingly absurd) case that the LHC doesn't discover the Higgs! AFAICS, all reasonable models put it firmly in reach of the LHC.. If we don't find it, well, something drastic has got to give. I don't see many people talking about that, so entrenched is the SM gospel in our minds. What a strange higgs sector that would imply.

8. **Peter**
March 21, 2004

Hi Matt,

Have you decided where to go to grad school yet?

Today I've put up somewhat more positive posts, and in general hope to mix the positive and negative. Unfortunately even the positive posts will probably be so speculative as to be "Not Even Wrong". When I figure out a unification idea that really works I'll dump the blog and start instead enjoying the fruits of fame and fortune that accrue to super-star physicists.

Your idea of pursuing applications of string theory to better understand strongly interacting gauge theories is quite reasonable. I don't have any problem with that part of the string theory program. My problem very specifically is with the continual hyping of the failed idea of unifying the standard model and gravity in a 10/11d supersymmetric string/M-theory.

I'm not so convinced that new information about strongly interacting gauge theories coming from string theory duals will solve the problem of spontaneous gauge symmetry breaking. The technicolor idea is quite beautiful, but all implementations of it seem to involve adding a huge amount of new structure to one's theory, while not being able to actually calculate anything one wants to calculate. On the other hand, maybe one can use string duals to
find a way of calculating things, and if this is the way the world works, there should be real evidence for it a few years from now.

9. **Peter**
March 21, 2004

Hi Sean, thanks for the advice about graphics, I'll give that some thought. I'm still trying to see exactly how this thing works, so not sure how to comment on your comment in some way that news of this gets back to you. If you're lacking topics to write about in your own blog, I've put up an entry here today with some questions for cosmologists.

10. **Matt Reece**
March 19, 2004

Interesting to see that you have a blog. I've followed some of your s.p.r posts in the past with interest.

I'm an undergrad now, but I'll be going into particle theory as a grad student next year. I have some skepticism about the virtue of a lot of research in string theory, but I can't share your extreme skepticism.

I notice you divide your links into "non-string theory" and "string theory," and title your blog "Not Even Wrong." Does this indicate you intend to focus on anti-string theory polemics? Personally I would find some more positive posts interesting.

I was much more skeptical of string theory until the past year or so. Curiosity about various models of "large extra dimensions" led me to read papers of Hall and Nomura on orbifold GUTs, which then led to Nomura's papers on warped geometry and "Higgsless" models. These led me to all sorts of other phenomenological articles, and at some point it became clear that the AdS/CFT correspondence is quite important in the phenomenology of Randall-Sundrum models and their variations.

Although it's certainly not proven (but then, little in field theory is), I think the indications of dualities of field and string theories are too compelling to ignore. You bring up that asymptotically free gauge theories are well behaved. One interesting possible for electroweak symmetry breaking, as I'm sure you know, is the possibility that the Standard Model is embedded in a strongly interacting gauge theory at higher energies. The simplest technicolor scenarios aren't feasible, and harder ones are tough to construct. Thus the discovery that certain such models might be dual to stringy models in higher dimensions seems to me a compelling reason to learn about string theory. These dualities might provide calculational tools that are lacking in field theory. Effective field theory, lattice gauge theory, and string theory, I think, should all be viewed as powerful tools for elucidating the behavior of physical theories we otherwise can't understand.

I'm rather more skeptical of claims that string theory, on its own, will give us a theory of fundamental physics. Certainly we need further experimental input to progress much farther. The apparent existence of a "string theory landscape" that doesn't uniquely predict a vacuum is disappointing. But, as they say, let's not throw the baby out with the bathwater.

11. **Sean**
March 19, 2004

Peter, welcome to the blogosphere. I found your blog in the usual way — your link to mine. I suspect I will disagree with many things you say (I'm a fan of string theory, although not a string theorist myself), but that's all part of the fun.

By the way, if you want to get a lot of hits, I've found it helps to stick up a pretty picture from the Hubble Space Telescope. Posts with words in them don't seem to draw as well, for some reason.

The Holy Grail of Physics

March 21, 2004
Categories: Uncategorized

It's pretty common these days for people to refer to successfully quantizing general relativity as "the Holy Grail of Physics", but it seems to me that there is a different problem that better deserves this name:

"Why does the vacuum state break electroweak gauge symmetry?"

If we could answer this question, we'd probably understand where masses of particles come from, as well as just about all of the undetermined parameters of the standard model (except for a couple ratios of the strengths of the gauge interactions). The exciting thing about this problem is that we have good reason to expect experiments to give us some new clues about it in 2008 when data from the LHC begins to come in.

The standard unification paradigm these days explains this in terms of the potential for a Higgs field, with various grand and super-unification schemes allowing an appropriate Higgs field but somehow never being able to predict anything at all about it. Even worse, such schemes not only don't explain anything about this field, but also require the addition of extra Higgs fields beyond the single one required by the standard model.

An idea I've always found appealing is that this spontaneous gauge symmetry breaking is somehow related to the other mysterious aspect of electroweak gauge symmetry: its chiral nature. SU(2) gauge fields couple only to left-handed spinors, not right-handed ones. In the standard view of the symmetries of nature, this is very weird. The SU(2) gauge symmetry is supposed to be a purely internal symmetry, having nothing to do with space-time symmetries, but left and right-handed spinors are distinguished purely by their behavior under a space-time symmetry, Lorentz symmetry. So SU(2) gauge symmetry is not only spontaneously broken, but also somehow knows about the subtle spin geometry of space-time. Surely there's a connection here…

This idea has motivated various people, including Roman Jackiw, who has several papers about chiral gauge theories that are very much worth reading. The problem you quickly get into is that the gauge symmetry of chiral gauge theories is generally anomalous. People mostly believe that theories with an anomalous gauge symmetry make no sense, but it is perhaps more accurate to say that no one has yet found a unitary, Lorentz-invariant, renormalizable way of quantizing them. In the standard model, the contributions to the anomaly from different particles cancel, so you can at least make sense of the standard perturbation expansion. Outside of perturbation theory, chiral gauge theories remain quite mysterious, even when the overall anomaly cancels.

So, this is my candidate for the Holy Grail of Physics, together with a guess as to which direction to go looking for it. There is even a possible connection to the other Holy Grail, I'll probably get around to writing about that some other time.

Comments

1. **Jacques Distler**
   April 3, 2004

   > Jacques means it is good for string theory if there are few realistic KKLT sorts of vacua because the alternative is a huge problem. If, as some people believe, there are 10^500 or so relatively realistic looking vacua, string theory is utterly useless for ever predicting anything.

   You will kindly *refrain* from putting words in my mouth, especially when they are incorrect ones.

   If there were, indeed, a plenitude of "realistic looking vacua", then string theory would be reduced to the level of quantum field theory, where the particle content, gauge groups, and coupling constants are *input parameters*, not calculable from some underlying principle.

   This would not be a *disaster* for string theory, but it certainly would be a disappointment. I, for one, would hope that at least *some* of these seemingly arbitrary parameters (say, the electron-muon mass ratio, or the ratio of the QCD scale to the Planck Mass, or the values of the KM mixing angles) would be *calculable* in a fundamental theory.

   It could turn out that they are just as arbitrary in string theory as they are in field theory. Disappointing? You bet! A disaster? Not exactly.

   But, then, for a variety of technical reasons, I don't think that's the way things work in string theory.

2. **Peter**
   April 1, 2004

   Jacques means it is good for string theory if there are few realistic KKLT sorts of vacua because the alternative is a huge problem. If, as some people believe, there are 10^500 or so relatively realistic looking vacua, string theory is utterly useless for ever predicting anything. In this case, those who take string theory on faith will keep trumpeting how wonderful it is, while anyone who believes in the scientific method will stop paying attention to it.

   His preferred situation is that either

   1. Just a small number of the vacua now under study are realistic, and one of them is an approximation to the real world.

   2. Some new non-perturbative insight will explain why the vacua now under study are not true vacua, at the same time identifying the true vacua, one of which we live in.

   Needless to say, there is no evidence for either 1. or 2. other than wishful thinking.

3. **Fred**
   April 1, 2004

   " Vacua of this sort which look even vaguely like the real world: acceptably small rate of proton-decay, suppression of flavour-changing neutral currents, etc, etc, are very rare indeed — if they exist at all."

   What, why is this good for String theory? You better hope they exist, or else you have a close to falsified theory on your hands.

   I think I agree however, it would be nice if the vacua that describes are world would look like an isolated point. It makes it in principle, easier to single out sometime in the future if we ever get a realistic mechanism that picks out vacua.

4. **Jacques Distler**
   March 29, 2004

> I'd be genuinely interested to hear what you mean by this.

In short, I believe that de Sitter vacua with small cosmological constant exist, but are not as dirt-common as KKLT would have you believe. Vacua of this sort which look even *vaguely* like the real world: acceptably small rate of proton-decay, suppression of flavour-changing neutral currents, etc, etc, are *very* rare indeed — if they exist at all.

To say more would require equations and stuff. Perhaps I will post something on my own blog.

> Note that I never said that it would be a disaster if KKLT can be made to work. I don't agree with that claim. If deS can be extracted from string theory, even in a lame way, that would be major progress…

You didn't. Peter did. Maybe, in the meantime, the two of you could settle between yourselves whether KKLT being right/wrong would be a triumph/disaster for string theory.

5. **serenus zeitblom**
March 29, 2004

Jacques Distler wrote:
My personal opinion, contra KKLT, is that it is hard to find appropriate (meta)stable de Sitter vacua. And I view that difficulty as a good thing.

I'd be genuinely interested to hear what you mean by this. Note that I never said that it would be a disaster if KKLT can be made to work. I don't agree with that claim. If deS can be extracted from string theory, even in a lame way, that would be major progress, provided of course that everyone admits that it is indeed lame….. The *worst* thing that can happen is for everyone to run off and hope that the cosmological constant will somehow go away while they work on something else.

6. **Peter**
March 28, 2004

It certainly seems to me that if KKLT is right, it's a bad thing for string theory, if it's wrong it's a good thing but just in the sense that a bad thing didn't happen.

If I understood what a Rutgers post-doc was trying to explain to me on the shuttle bus there a few weeks ago, he and others had found that certain conditions needed to make KKLT work were not so easy to achieve, although they did have some solutions for these conditions. So, my impression was that they thought there were fewer viable solutions than the vast numbers that were initially expected (no idea whether "fewer" means 10 or 10^50). Caveat: no, I'm no expert on this and I'm just recounting what I understood of a conversation on a short bus ride. Maybe I've got this wrong somehow..

7. **Jacques Distler**
March 28, 2004

> But
> the persistent failure to get de Sitter out of
> string theory—that *is* a crisis.

I see. So, if KKLT are right, it's a disaster for string theory. If KKLT are wrong, it's also a disaster for string theory.

Poor bastards, they're doomed either way!

My personal opinion, contra KKLT, is that it is hard to find *appropriate* (meta)stable de Sitter vacua. And I view that difficulty as a *good* thing.

8. **serenus zeitblom**
March 28, 2004

"The rate of progress in any field is far from uniform. I'd be the first to agree that the rate of progress in string theory recently is slower than we have been accustomed to. But to call that a "crisis" is seriously overblown."

I agree that *that* is no sign of crisis. But
the persistent failure to get de Sitter out of
string theory—that *is* a crisis. I am reliably
informed that the Rutgers gang has been trying
hard to get the KKLT proposal — and it is no
more than that — actually to work. And they
are getting badly discouraged. But Lubos Motl
isn't, of course — he just claims, apparently
with a straight face after that nasty tic is
modded out, that it is "premature"
to worry about this! Well, the latest data
show that the Big Crunch is at least 40 gyr
away, so we still have time to get string
cosmology to work.

9. **Peter**
March 27, 2004

You really can't help yourself…

"Just because SU(3)xU(1) is nonchiral doesn't mean it is well-understood nonperturbatively."

Did I say anywhere that SU(3)xU(1) gauge theory with fermions is well-understood?

"If you turn off SU(3)xU(1), you obtain an SU(2) gauge theory that is *nonchiral*."

Did I say anything about turning off SU(3)xU(1)?

"Your repeated dismissal of the twistor stuff as insignificant"

Did I ever refer to Witten's work on this as "insignificant"?

I don't think so, since that's not what I've ever thought about it. I have repeatedly, here and elsewhere, said I don't really understand its significance, hoping to get explanations of its significance from others more knowledgeable than myself. The comment explaining to me its significance for calculating tree-level, multi-gluon amplitudes was the first response I've gotten to the question from someone knowledgable. The only other responses I'd gotten were incoherent rantings from Lubos Motl.

No, I'm not going to tell you who the postdocs and more senior theorists were that I was referring to, and they are unlikely to speak publicly. Besides the obvious reasons, they are unlikely to want to get publicly attacked as incompetent fools by various fanatics

10. **Jacques Distler**
March 27, 2004

> Glad to see that you're following my weblog so closely. If you keep doing this I'm sure you'll find many more opportunities…

I doubt it. Uninformed anti-string rants get real old, real fast. You'll have to come up with something more interesting if you want to hold my attention.

> To try and say things in a way that will be less easy for you to misconstrue, all I was saying was the following. If you turn off the weak interactions, you have a SU(3)xU(1) non-chiral gauge theory. Turn on the weak interactions and you have something much trickier and less-well understood non-perturbatively: a chiral gauge theory with spontaneously broken gauge theory.

Just because SU(3)xU(1) is nonchiral doesn't mean it is well-understood nonperturbatively. The U(1) does not exist nonperturbatively. You identified the SU(2) gauge theory as the part that is chiral (and hence hard to understand nonperturbatively). This is *incorrect*. If you turn off SU(3)xU(1), you obtain an SU(2) gauge theory that is *nonchiral*. It is very hard to come up with an interpretation of your post, and of your responses to Mark that does not involve your asserting the contrary. Take, for instance:

> An idea I've always found appealing is that this spontaneous gauge symmetry breaking is somehow related to the other mysterious aspect of electroweak gauge symmetry: its chiral nature. SU(2) gauge fields couple only to left-handed spinors, not right-handed ones…

> All I was saying was that if you think of SU(2) gauge theory as a purely internal symmetry, it is in some sense surprising that it is chiral…

> The chirality of the SU(2) couplings in this case may be a clue…

> The SU(2) gauge symmetry knows the difference between right and left-handed spinors…

There are other misapprehensions in your post. Most importantly, as Mark says, the generic quantum field theory is chiral, so "chirality" is hardly a puzzle to explain. But it's hard to discuss the big picture when you persist in misapprehensions about the basics.

> 1. Multiple string theory postdocs who have recently told me they find the field in crisis, very depressing, no new ideas, to the extent that they are thinking of quitting.

> 2. Multiple senior theorists, string theorists and others, who have told me that there are no interesting new ideas in the subject (with the possible exception of Witten's twistor stuff).

There are dozens of recent papers I find as interesting, or more interesting than the developments in twistor theory.

The rate of progress in any field is far from uniform. I'd be the first to agree that the rate of progress in string theory recently is slower than we have been accustomed to. But to call that a "crisis" is seriously overblown.

> 3. Attending talks about string theory, looking at recent talks on the web, scanning the new papers on the arxiv. No, I don't understand everything I read or hear this way, but do understand enough of it to note that the experts I talk to seem to be right.

Your repeated dismissal of the twistor stuff as insignificant (a dismissal you've made repeatedly, both in this venue and elsewhere) is, I think, indicative of the general fact that you haven't the *context* necessary to evaluate the significance of the talks you've heard and the eprints you've tried to read.

I might take seriously the opinions of the unnamed "senior theorists" and "string theory postdocs" if you'd care to tell us who they were, and exactly what they said.

Or, better yet, they can speak for themselves.

11. **Peter**
March 27, 2004

Hi Jacques,

Glad to see that you're following my weblog so closely. If you keep doing this I'm sure you'll find many more opportunities to take something imprecise that I write, make up an interpretation of it that is incorrect, then use this to demonstrate my ignorance of basic facts. In the future when you do this, if you're more careful, your correction might even be correct. You're quite right that "this is getting really embarassing", but I think we probably disagree for whom.

To try and say things in a way that will be less easy for you to misconstrue, all I was saying was the following. If you turn off the weak interactions, you have a SU(3)xU(1) non-chiral gauge theory. Turn on the weak interactions and you have something much trickier and less-well understood non-perturbatively: a chiral gauge theory with spontaneously broken gauge theory.

No, I wasn't the guy who "had no clue as to why anyone would want to calculate multigluon perturbative Yang-Mills amplitudes", that guy lives only in your over-worked imagination. I was the guy who knew very well that multigluon perturbative YM amplitudes were important things to calculate in collider physics, but would have guessed that the calculations were pretty straight-forward for tree amplitudes, only really difficult when you put in loops. I was very pleased to learn something new from the comment posted here.

As for whether there is any progress being made in the last few years in string theory, I base my judgment on the following evidence:

1. Multiple string theory postdocs who have recently told me they find the field in crisis, very depressing, no new ideas, to the extent that they are thinking of quitting.

2. Multiple senior theorists, string theorists and others, who have told me that there are no interesting new ideas in the subject (with the possible exception of Witten's twistor stuff).

3. Attending talks about string theory, looking at recent talks on the web, scanning the new papers on the arxiv. No, I don't understand everything I read or hear this way, but do understand enough of it to note that the experts I talk to seem to be right.

So, let's get this straight: the students I mentioned are "fools", I'm an embarassing, incompetent fool and the post-docs and senior faculty who think string theory is in crisis are also probably fools. The only people in the world who aren't fools are you and perhaps some of the few other string theorists out there who spend their time hyping a theory which has utterly failed to provide its promised new insights into unification of physics. Have I got this right?

12. **Field Theorist**
    March 26, 2004

    Make that "real," not "pseudoreal."

13. **Field Theorist**
    March 26, 2004

    > It's surprising if you think geometrically and take the standard view that internal and space-time symmetries have nothing to do with each other. The U(1) and SU(3) gauge symmetries are not chiral and are truly independent of the space-time geometry. The SU(2) gauge symmetry knows the difference between right and left-handed spinors so in a subtle way it knows something about space-time geometry. More precisely, it knows about the spinor geometry.

    This is getting really embarassing.

    For the last time. The left-handed fermions of the standard model form a *real* representation of SU(3)xU(1), and they form a *pseudoreal* representation of SU(2). It's only when you look at the *full* SU(3)xSU(2)xU(1), that you find that they form a complex (ie, chiral) representation of the gauge group.

    > Sure, I think the NSF and DOE should stop funding theorists who send in grant proposals that say "String theory has made huge progress in the last few years, we intend to keep doing the same thing". The fact of the matter is that string theory hasn't been making any progress and this needs to be honestly addressed.

    Weren't you the guy who, until yesterday, had no *clue* as to why anyone would want to calculate multigluon perturbative Yang-Mills amplitude, much less how the recent work on twistors represents a *revolution* in our ability to do so?

    What makes you *competent* to decide whether progress is being made?

    > I personally know quite a few really good young students who have left physics because they saw no way to make a career for themselves in the field other than by working on string theory, which they didn't believe in.

    Then they are fools. The job market, at the moment, for good young phenomenologist is considerably better than that for young string theorists. (Not that the latter is bad, just that good young phenomenologists are much sought-after nowadays.)

14. **Peter**
    March 26, 2004

    About chirality again:
    It's surprising if you think geometrically and take the standard view that internal and space-time symmetries have nothing to do with each other. The U(1) and SU(3) gauge symmetries are not chiral and are truly independent of the space-time geometry. The SU(2) gauge symmetry knows the difference between right and left-handed spinors so in a subtle way it knows something about space-time geometry. More precisely, it knows about the spinor geometry.

    Is it a coincidence that the piece of the standard model gauge symmetry that is chiral is also the piece that is spontaneously broken? Maybe, or maybe there's GUT lore about this I'm not thinking about. Is it a coincidence that, besides having its gauge symmetry spontaneously broken, the SU(2) chiral gauge theory is also really hard to understand non-perturbatively? Maybe.

    I've never, anywhere, denigrated string theorists as "dilettantes and poseurs". That's not what I think about them. Some of my best friends are string theorists, and most people doing string theory are very smart and harder-working than I am. On any metric for how smart and hard-working someone is, Ed Witten is off the scale.

    About funding:
    Sure, I think the NSF and DOE should stop funding theorists who send in grant proposals that say "String theory has made huge progress in the last few years, we intend to keep doing the same thing". The fact of the matter is that string theory hasn't been making any progress and this needs to be honestly addressed. The panels that allocate very scarce funding for theoretical physics are the place this is supposed to happen.

    The over-hyping of string theory is doing more damage to theoretical physics than any cut in government funding ever could. I personally know quite a few really good young students who have left physics because they saw no way to make a career for themselves in the field other than by working on string theory, which they didn't believe in.

15. **Mark Srednicki**
    March 25, 2004

    "As far as I can tell your point is essentially just the obvious one that since we know how to write down chirally asymmetric theories, a generic theory is going to be chiral."

    If this point is obvious, why did you say that it is "in some sense surprising" that the QFT that describes the real world is chiral?

    As for anomalous gauge theories, if you (or Roman Jackiw or anyone else) can construct them, great! I'd be very interested. But I think there are very good reasons why it can't be done, reasons that are as close as we ever get to a proof in QFT. I don't agree that it's "more accurate" to say that these theories just haven't been quantized yet.

    As for arrogance, when you denigrate a whole group of dedicated, hard working scientists as dilettantes and poseurs because you don't share their taste in interesting problems, it's hard to want to be polite. Change the name of this blog from "Not Even Wrong", a famous insult in physics, to something more neutral, and I'll be a lot more polite in the future.

    You say that you're "not in the business of telling people what they should work on and there is no danger they would pay much attention to me if I was". But there is. You are seeking publicity in the larger public arena, via your article in American Scientist and this blog. You denigrate a lot of hard honest work done by brilliant people. There is a small but significant chance that this denigration could ultimately have a detrimental impact on science funding.

    You think that it is a terrible crime that string theory has been overhyped. But if science was better funded, more young people would have more of a chance to make an impact, however they choose to try to do it, before irrevocable decisions have to be made about their futures.

    That's why I took the time and effort to respond in the first place.

16. **Peter**

March 25, 2004

We probably did overlap somewhere back in the 80's, I was a grad student at Princeton 1979-84. Unfortunately one aspect of middle age for me involves the inability to remember exactly people I was glancingly acquainted with back then.

Your latest post starts with the generic argument for string theory in terms of quantum gravity. Responding to that seriously would take a while and someday I'll get around to it on the weblog.

The other comments you make about the sociology of why people do string theory are interesting and worth responding to. Again, I'll try and do so at length at some point soon, but a couple quick comments: No, I don't think it's a conspiracy and I'm sure most of them do so because they think it is the best bet for making some kind of progress (this is less true of those poor bastards trying to get a permanent job). On the other hand I don't agree with you at all that the community is now open to new ideas.

I'm not in the business of telling people what they should work on and there is no danger they would pay much attention to me if I was. My interest here is in trying to do two things:

1. Explain exactly what the present state of string theory is, minus the hype. You and others are welcome to argue with me when I do this. It's a complicated subject and probably I'll get some things wrong.

2. Explain some highly speculative ideas of my own. These are also "Not Even Wrong" at this point and will be clearly labeled as such.

17. **Peter**
March 25, 2004

About chirality:
As far as I can tell your point is essentially just the obvious one that since we know how to write down chirally asymmetric theories, a generic theory is going to be chiral. If you are a believer in Susskind's landscape then maybe our world is governed by a generic theory and of course it is not surprising it is chiral. I may be wrong, but my point of view is to try and find something special about SU(3)xSU(2)xU(1) and the standard model representations that would explain where this symmetry comes from. The chirality of the SU(2) couplings in this case may be a clue. You may think this kind of speculation is pointless, a waste of time, has all been done before, whatever. But it's clearly labeled as speculation, it's just as "Not Even Wrong" as string theory. If you think my speculative ramblings are wrong-headed, don't read them.

Anomalous gauge theories:
You seem really exercised that someone would even implicitly question conventional wisdom. Clearly we're not going to get along here. At most points in the history of science spending one's time figuring out the implications of new ideas is a more fruitful thing to do than questioning conventional wisdom. I think this point is different: there are no new good ideas and we need to question how we got to this point. Certainly something goes wrong with our standard understanding of gauge theory when you try and quantize an anomalous theory. I happen to think it's worthwhile to think about this and try and better understand exactly what happens. One of my reasons is that in 2d there is some really beautiful and new mathematics that makes its appearance. Maybe this is irrelevant to 4d, maybe not.

My "tone about poor benighted theorists":
Well, for 20 years the smartest people in the business have been to a large extent off thinking about quantizing gravity instead of electroweak symmetry breaking. Right now when I talk to them they are bitching and moaning that in string theory there are no new ideas, everything that anyone has tried either has failed or can't be pushed further. Under the circumstances I do think it would be healthier if more of them would stop doing this and would start instead bitching and moaning about how hard it is to come up with a new idea about electroweak symmetry breaking.

About arrogance:
First of all your initial post was simply rude and insulting. Today also brought an even more juvenile string of insults from a Junior Fellow at Harvard, Lubos Motl. What is with you people? Grow Up! Do your parents know you behave like this? You don't know me and seem to think it's all right to start insulting someone you don't know on the basis of your interpretation of a couple vague sentences they write about complex issues in QFT. What about posting a comment saying you disagree with something I wrote, explaining why and asking me to justify what I wrote? Then when I can't justify it, you're welcome to start accusing me of ignorance.

While I was about to finish this, another post from you came through. I'll stop this one, continue with another after reading your latest.

18. **Mark Srednicki**
March 25, 2004

Looking at your credentials, I see that we must have overlapped at Princeton in the early '80s. I confess that I thought you were a mathematician who was dabbling in physics.

As for string theory being "not even wrong", the title of your blog, it is QFT that deserves this description.

Pick up a book. Hold it at arm's length. Let go. Did it fall? You just disproved the Standard Model, which does not incorporate gravity.

The only known way to deal with gravity in QFT is to put in an ad hoc cutoff at the Planck scale. Then you lose either Lorentz invariance or unitarity or both. We could perhaps live with violations of Lorentz symmetry, but before giving up on it, perhaps we should search for a more clever cutoff.

And that's exactly what string theory provides. It *may* be that it doesn't work in the end; that's still an open question. But *this* is why string theory is exciting: it's an extension of quantum field theory that (1) incorporates gravity in a natural way and (2) provides the *possibility* of a finite theory that does not require an ad hoc cutoff.

The tradeoff is that it's *much* harder to find specific quantum field theories as the low energy limit of string theory. There's been a huge amount of work on this, work that continues at a feverish pace. A QFT that looks like the Standard Model has not been found yet. Whether or not one exists is an open question.

Senior string theorists work on string theory because they think it's the best bet for a more fundamental theory than QFT with an ad hoc cutoff. That's all there is to it. No vast conspiracy. People with tenure who want to work on other things, do. People without tenure have to hedge their bets on what to work on. One can certainly criticize the fairness of the tenure system in general, but there is nothing unique or special about string theory in this regard.

Indeed, my experience is that the particle theory community is about as open to new ideas as a group of human beings can possibly be. Let's not forget that 20 years ago, the string community was pretty much down to Green, Schwarz, and Brink. It's grown enormously since then because people were genuinely interested and excited about it, not because they were forced to work on it.

I am *extremely* skeptical of *any* claim that scientists in general are working on the "wrong thing" and should be told to work on something else. I trust the community at large far more than the judgement of any one person.

19. **Mark Srednicki**
March 25, 2004

"The arrogance of people in the particle theory community never ceases to amaze me. Assuming that anyone who dares to criticize what is going on in the subject must be ignorant is all too common behavior."

I did not *assume* you were ignorant because you criticized what is going on (though your understanding of what is going on is mistaken, in my view). I *concluded* that you were ignorant because you made several statements that indicated misunderstandings of various issues in quantum field theory and particle physics.

20. **Mark Srednicki**
March 25, 2004

"All I was saying was that if you think of SU(2) gauge theory as a purely internal symmetry, it is in some sense surprising that it is chiral, since chirality is an aspect of behavior under Lorentz symmetry, which is a space-time symmetry."

This betrays a profound misunderstanding of the nature of chiral symmetry. One more time: if you start with three or more fermion fields, and one or more scalar fields, and include all couplings allowed by renormalizability, your theory will automatically be chiral, unless you choose special values for some of the couplings. Thus chirality in nature is no surprise; it's a generic prediction of quantum field theory.

"I was just making the uncontroversial comment that if you take a classical chiral gauge theory such that quantizing the fermions produces an anomaly in the gauge symmetry, no one knows how to quantize such a theory."

The conventional wisdom is that such theories DO NOT EXIST, not that we haven't figured out how to quantize them. Your original comment questioned this belief. No one can prove anything either way. But the arguments against the existence of these theories are about as strong as any arguments in QFT.

All bets are off in two dimensions, where lots of special things happen.

"Maybe an elementary scalar field is what is causing electroweak symmetry breaking, and one has to ultimately understand this field and its couplings as coming from fluxes in a Calabi-Yau or who knows what. But theorists should not be so arrogant."

Arrogance has nothing to do with it. Theorists have thoroughly explored all the alternatives they could think of (with some, like large extra dimensions, still under active investigation). Most died because they made predictions in immediate or eventual conflict with experiment. Theorists would be *thrilled* if something unexpected turned up at the LHC. The whole tone of your original comment was that poor benighted theorists are off thinking about quantizing gravity instead of electroweak symmetry breaking. This is wrong!

21. **Peter**
March 25, 2004

The arrogance of people in the particle theory community never ceases to amaze me. Assuming that anyone who dares to criticize what is going on in the subject must be ignorant is all too common behavior.

I won't recite my qualifications extensively here, other than to note that the places where I first learned QFT included Weinberg's course on the subject at Harvard that lead to part of the books Srednicki mentions. I also took Coleman's QFT course there, as well as Gross's course at Princeton where I got my Ph.D in particle theory in 1984 (Curt Callan was my advisor there).

It's hard to extract from the torrent of personal abuse what Srednicki's criticisms are. The weird thing is he doesn't deal with my views on string theory, which are controversial, but here gets very worked up about what are accurate and not at all controversial statements.

"Wrong, Wrong, Wrong" number one. Hard to tell what he is getting worked up about here. All I was saying was that if you think of SU(2) gauge theory as a purely internal symmetry, it is in some sense surprising that it is chiral, since chirality is an aspect of behavior under Lorentz symmetry, which is a space-time symmetry. Chiral gauge theory is tricky, as Srednicki should know well, since he started out his career, like I did, doing lattice gauge theory. If you try and put spinors on a lattice and couple them chirally to gauge fields, it's surprising how much trouble you run into.

"Wrong, wrong, wrong" number two. I can't see what he's upset about here either. I was just making the uncontroversial comment that if you take a classical chiral gauge theory such that quantizing the fermions produces an anomaly in the gauge symmetry, no one knows how to quantize such a theory of coupled gauge fields and spinors in such a way that you end up with a unitary, Lorentz-invariant, renormalizable QFT. I didn't say anything about the gauge invariance of such a QFT or claim that I have any idea how to do this.
Jackiw does claim to be able to do this (for the chiral Schwinger model) in 1+1d, maybe Srednicki should send him an e-mail about how ignorant about QFT he is.

"Sigh" Again, I can't figure out what he is getting so excited about, since what he quotes is completely uncontroversial. I'm well aware that there is by now a long history of unsuccessful attempts by theorists to find a good alternative to using an elementary scalar field to break electroweak symmetry. Maybe an elementary scalar field is what is causing electroweak symmetry breaking, and one has to ultimately understand this field and its couplings as coming from fluxes in a Calabi-Yau or who knows what. But theorists should not be so arrogant. There's a long history of experiments coming up with results that theorists had good reason to believe were not possible. In the late sixties theorists would have claimed that the strong interactions were such that scaling in deep inelastic scattering was impossible at high energies. And yet, that's what was found, leading Gross et. al. to the discovery of asymptotic freedom. Let's wait and see what the experimental results have to say about the dynamics of the Higgs sector before being so sure that all possibilities for what it can be are well-known.

22. **Mark Srednicki**
March 25, 2004

I came to your web site because I was told that you are a critic of string theory, and I wanted to see what you had to say about it. What I find is appalling ignorance. You really ought to spend some time learning some physics before you attack it. I recommend starting with Weinberg's three-volume text on quantum field theory.

I don't have the time or the energy to explain *all* the errors on this web site, or even all the ones in this single comment. But I'll tackle a few of them.

"The SU(2) gauge symmetry is supposed to be a purely internal symmetry, having nothing to do with space-time symmetries, but left and right-handed spinors are distinguished purely by their behavior under a space-time symmetry, Lorentz symmetry."

Wrong, wrong, wrong! RH spinors are hermitian conjugates of LH spinors; you can't have one without the other (in 3+1 dimensions; things are different in different numbers of dimensions). Parity swaps the LH and RH spinors (as does hermitian conjugation). The question is, in a particular theory, can we assign parity transformation rules to the scalar and vector fields (if any) so that parity is a symmetry of the action? If there are more than 3 LH spinors, and at least one scalar, then the answer is generically "no", even if there are no gauge fields at all. (By "generically", I mean you include all couplings allowed by gauge symmetries and renormalizability.) If there is some gauge symmetry, then the answer is also "no" if the LH spinors form a complex representation of the gauge group (with or without scalars). In no case is there any need for some mysterious intertwining of Lorentz and gauge symmetry.

"People mostly believe that theories with an anomalous gauge symmetry make no sense, but it perhaps more accurate to say that no one has yet found a unitrary, Lorentz-invariant, renormalizable way of quantizing them."

Wrong, wrong, wrong! In the case of anomalous violation of a global symmetry, we know from experiment that the symmetry does not hold: this is proven by the decay rate of the neutral pion (for electromagnetic anomalies) and by the nonexistence of a ninth light pseudoscalar (for chromodynamic anomalies). Coupling a vector field to a non-conserved current violates gauge invariance. Of course, since *no* quantum field theory (in more than two dimensions) has been proven to exist (and gee, isn't this one of the mighty criticisms you aim at string theory?), you are free to believe that something could be constructed that could be interpreted as an anomalous gauge theory. But it wouldn't actually have any gauge symmetry.

"Why does the vacuum state break electroweak gauge symmetry?
If we could answer this question, we'd probably understand where masses of particles come from, as well as just about all of the undetermined parameters of the standard model (except for a couple ratios of the strengths of the gauge interactions). The exciting thing about this problem is that we have good reason to expect experiments to give us some new clues about it in 2008 when data from the LHC begins to come in."

Sigh. Now we come to appalling ignorance of the history of ideas in particle physics. This sounds like it was written in 1979. A *huge* amount of effort went into exploring alternatives to the standard Higgs field in the 1980s. All these models died at the hands of the experimentalists, because they all kept predicting things that were not observed. We are left believing that the Higgs field is a fundamental scalar because there is no alternative that agrees with experiment, not because we are muddle-headed idiots who can't think of the obvious.

I'm horrified that the ideas of someone as ignorant as you can be so widely circulated without serious rebuttal. You haven't gotten serious rebuttal because the serious people have better things to do with their time. If you want to learn some physics first, fine. Unless and until you do, please stop trying to tell funding agencies (all staffed with people far more knowledgable than you are) how to best use their meager sums.

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Questions for Cosmologists

March 21, 2004
Categories: Uncategorized

I was pleased to get a comment from cosmologist Sean Carroll frighteningly soon after starting this thing up. Here's some questions about cosmology that have been on my mind recently, maybe he or someone else will be able to answer them:

Witten has argued to me that "results about CMB fluctuations which are suggestive of inflation at the GUT scale" provide evidence that "grand unification is on the right track". What exactly does the CMB data say about inflation? Can one extract the GUT scale from either the current CMB data or any conceivable better future CMB data?

More generally, while I've heard a lot about attempts to extract information about Planck-scale physics from CMB data, what about all the scales in between where accelerators stop (hundreds of Gev) and the Planck scale? Can one find out anything about electroweak symmetry breaking? Could this have anything to do with inflation?

There's another question that often bothers me: "How can you have a field called 'String Cosmology' when string theory isn't really a theory and can't be used to predict anything?", but I'll be good for now and leave that rant for another time.

Comments

1. **Gary**
   March 29, 2004

   Who can really know for a certainty where future CMB research will lead (if but to answer the aforementioned question regarding the extraction of the GUT scale from CMB data.) We will just have to wait and see. But I will tell you this much, that when we do hopefully we'll be smart enough to recognize a designer involved in the greater scheme of creation, and not the theories themselves.

2. **Alexander Crawford**
   March 25, 2004

   However interesting the statistical/mathematical models may or may not be, the actual difficulties aren't addressed very seriously in terms of experimental problems or circuit design methods (errrr… the standard deviation measure of step rise times from step response to impulse response doesn't suggest "extra-dimensionality" in a physical sense).

   It'd probably be very interesting for most theoretical physicists to look into the wire spring relay developed by Western Electric and current in the mid-1960's, especially for those interested in String Theory. It might be as interesting as the M-M interferometer design as that relates to other somewhat older cosmological models. The magnetic field model is available at MIT and a couple of other sites (view source option-read comments) for a curious take on m-m data.

   Regarding the questions specifics…

   Here's a link to the NIST page for phase noise and metrology:

   http://www.boulder.nist.gov/timefreq/phase/

   Here's the optical frequency measurement group links

   http://www.boulder.nist.gov/timefreq/ofm/index.html

   useful glossary:

   http://www.boulder.nist.gov/timefreq/general/glossary.htm

   (The USNO has a very comprehensive library of technical papers for the serious.)

3. **Sean**
   March 21, 2004

   These are very good questions, so much so that I started a multi-part answer over at my blog. Basically, the energy scale of inflation is roughly equal to the Planck scale times the square root of the perturbation amplitude; this works out to be the GUT scale. It's nicely consistent, but by no means direct evidence; Witten was right to say that it's "suggestive," but not more than that. If we were able to measure gravitational-wave modes, that would give a direct measurement of the energy scale.

   This is only in the simplest models, of course. If you get more baroque you can do whatever you like, including inflation at the electroweak scale. But you have to work at it. Likewise with getting information about Planck-scale effects; it's unlikely, but worth thinking about.

   We should talk about string cosmology another time. String theory is not a completely well understood theory, but it's not vacuous, either. But string cosmology is in its infancy, to say the least.

Various Stuff on the Web

March 23, 2004
Categories: Uncategorized

One can keep track of what is going on in theoretical physics now by taking a look at conference websites. Often after the conference they put up speaker's transparencies or even audio or video of the talk. Some very recent examples:

[Strings and Cosmology]

a conference last week at Texas A and M, and

[Spring School on Superstring Theory and Related Topics]

at the ICTP in Trieste. Among the Trieste lectures, Marcos Marino's notes give a nice discussion of some things you can do with topological strings. Brandenberger's notes on "Challenges in String Cosmology" include the peculiar statement that "String cosmology does not exist because non-perturbative string theory is not yet known". That was my impression too, but this doesn't really explain why he is lecturing on a subject that doesn't exist or why people devote many conferences to it.

Among the new papers appearing at the arXiv is Witten's latest:
[Parity Invariance for Strings in Twistor Space]

I'm still kind of not seeing why Witten and others are so interested in this. Using strings as a dual to QCD to understand its strong coupling behavior is obviously interesting, but why is reformulating something you understand well (perturbative Yang-Mills) in terms of strings in a super-version of twistor space so interesting? The interesting thing about twistors always seemed to me that they were naturally parity asymmetric, one chirality of spinors is tautologically defined. Witten's latest paper seems to just be showing how to get rid of this natural chiral asymmetry in this case.

Another new paper is:

[The Emergence of Anticommuting Coordinates and the Dirac-Ramond-Kostant operators]

by Lars Brink. The Kostant version of the Dirac operator is pretty amazing and too little known, both among mathematicians and physicists. I'm not so sure what Brink is trying to do with it leads anywhere, but there are other applications of it I'll try and write about some day.

Comments

1. **Matt**
   March 27, 2004

   I think the problem with computing gauge theory at weak coupling is that, even though one can easily write down Feynman diagrams, the number of diagrams grows more or less factorially with the order in perturbation theory. Computers can be fast but they can't handle calculations at high order when the number of graphs grows so quickly. Any technique that lets you get away with fewer diagrams will lead to better predictions.

   There may be issues aside from the computational one, but this strikes me as a major advantage.

2. **Peter**
   March 26, 2004

   We'll have to see how far this can be pushed. But the thing that has bothered me about it from the beginning is that, unless Witten has something up his sleeve he isn't telling us, the most optimistic thing he seems to be hoping for is to get a reformulation of perturbative YM in terms of a string theory on twistor space. The string theory on twistor space is a topological one that seems to have nothing to do with the string theory that is supposed to unify gravity and the standard model. So this doesn't help at all with the idea of quantizing gravity via the string. It would be kind of like AdS/CFT, where a string theory helps you understand a gauge theory. But even so, it is helping you understand not the mysterious aspect of gauge theory (strong coupling), but the weak coupling aspect, which I had thought was well understood. It was very interesting to see the comment that, in terms of computability, weak coupling gauge theory is not as well understood as I thought.

3. **serenus zeitblom**
   March 26, 2004

   It's pretty amazing that the twistor stuff is actually "useful". But the techniques of the CSW paper look "string-motivated" to me, not really stringy. If this "useful twistor" stuff pans out, to what extent can that be interpreted as a plus point for string theory per se?

4. **Peter**
   March 25, 2004

   Hi Mark,

   Thanks for writing this, that's very interesting to hear. I didn't know that tree-level perturbative YM amplitudes could be that hard to compute, so didn't realize that Witten's results would be so useful. The fact that abstract theory involving strings and twistors is leading to phenomenologically important calculational techniques is pretty amazing. Will be interesting to see what happens with loops, presumably there are a lot of people working on that now!

5. **Marcus Spradlin**
   March 25, 2004

   Hi,

   I am replying to Peter's comment that

"It wasn't clear whether the twistor ideas really help do any calculations these people [at the Tevatron and LHC] care about."

Here at the KITP we've had a workshop on collider physics for the past two months, and all of the experts in exactly this field have been at hand. It's hard for me to overstate the excitement, in particular of Zvi Bern (workshop organizer and undoubtedly one of the leading experts on perturbative QCD calculations). Calling the paper hep-th/0403047 a "home run", Zvi said that this is exactly the technology they've wanted for more than a decade. For more, see Zvi's colloquium at
http://online.itp.ucsb.edu/online/colloq/bern1/

To give an example, consider the 8-gluon amplitude with helicities ++++—-. As recently as March 2, 2004, one could have invested many, many months of effort into calculating this and written a lengthy paper on it (7-gluon amplitudes appear in a 40-page paper in 1990). After March 3, I (and independently David Kosower and I think Zvi as well), spent part of a lazy afternoon writing this amplitude down "just for fun". It's only 44 CSW diagrams, versus 34,300 Feynman diagrams.

People who write Monte Carlo programs for the Tevatron and LHC need to know these (and other) amplitudes in order to get a better handle on experimental backgrounds. Zvi said that these programs would all have to be rewritten now to use the fantastic new CSW technology. So, quoting from the mouth of an expert: Yes, people care!

Of course, so far Witten's new technology has been only applied at tree level. The REAL (practical) payoff from twistors will come from finding some way to apply this technology to loops, where the perturbative QCD calculations start getting VERY hard.

I inserted the word practical above to emphasize that there are other, more theoretical payoffs from Witten's work (such as some interesting new ideas on S-duality for topological strings, as well as other ideas surely to be discovered), but I specifically wanted to limit my reply to your question in the context of the usefulness of twistors for QCD calculations.

Mark

6. **Peter**
March 25, 2004

Oh no! Now I'm going to have to start reading Jacques Distler's postings about how to keep cranks from spamming the comment sections of one's weblog. Maybe Jacques can explain how to filter out just physicists from Harvard with Junior Fellowships.

As usual, Lubos is devoting his efforts to insulting others while avoiding the issue under discussion. He's still not answering the question I asked him when Witten's paper first came out: "What good is this new connection between perturbative YM and strings on super-twistor space?"

The claim that "twistors used to be nearly as popular as string theory is nowadays" is pretty weird. One could check this out with the SPIRES database, but I'd guess that in any of the last 20 years there have been 10-100 times more papers about string theory than the number written about twistor theory in the year of its peak popularity, whenever that was. Given Witten's paper and the complete lack of any other ideas for string theorists to work on, maybe 2004 will be the year of the peak number of twistor theory papers.

7. **Lubo? Motl**
March 25, 2004

Amazing, Peter! If I did not see this, I would not be able to believe that you are able to establish your own blog. I wonder whether you will erase this comment from your page, even though the people who don't know yet can learn that you are a bitter and intellectually limited person.

Recall that you were saying on sci.physics.research that this twistor stuff is beautiful, as soon as Witten started, and it's great that the people in the field would have to follow Witten. Your comments about physics are not not obsolete, silly, and annoying, but as we see now and again, they are also internally inconsistent.

Twistors used to be nearly as popular as string theory is nowadays, and Penrose himself believed that it was important to investigate this unusual description of the Minkowski spacetime because it might teach us something about quantum gravity.

His dreams have not been realized because their research was always very primitive. In the best case, it allowed them to calculate various things in a new way – for example the moduli space of instantons (ADHM) was first obtained using these methods. The twistors are now studied seriously, i.e. by string theorists, and all the potentially good and useful ideas and notions (such as D-brane instantons etc.) can be applied.

Twistors make the left-right parity symmetry very obscure, and therefore an explicit proof of this symmetry was (and is) very desirable. Witten's proof of this fact (and our earlier proof in Berkovits+Motl, as well as the proof in the paper by Volovich et al. a day later) is perhaps not a new revolution in physics, but it is certainly an interesting result – definitely more interesting than anything that you have done during the last 12 years, and therefore I would expect a little bit of respect from you, especially if you are – please accept my apologies for being straightforward – such a loser.

8. **Peter**
March 23, 2004

I saw that Witten gave a talk about this stuff to the workshop at the KITP where some people are doing perturbative QCD calculations necessary to understand backgrounds at the Tevatron and the LHC.

It wasn't clear whether the twistor ideas really help do any calculations these people care about, and they are the only ones I can think of who might be able to use better ways of doing perturbative YM computations. I'd guess Witten mainly hopes to get new insight into the relation of string theory and gauge theory out of all this, but I don't see anything that promising here yet.

9. **Matt**
March 23, 2004

Brief comment on the twistor stuff, but I don't know very much: I was visiting Harvard yesterday (as I might go there for grad school) and Nima Arkani-Hamed explained a bit about this to me. Apparently scattering amplitudes in Yang-Mills theory turn out to have much nicer forms than one sees in doing the perturbative calculation. My impression is that you might hope to calculate things more easily this way — you don't have to sum as many graphs — and also maybe certain analytic properties are easier to understand. But, I've only heard this briefly explained, so I'm not too sure of what the advantages are. People do seem convinced that this is useful.

My (Not So) Brilliant Career

March 26, 2004
Categories: Favorite Old Posts, Uncategorized

One of the weirder things that happened yesterday was that I noticed there was a long thread in a discussion group about my academic qualifications. You can find this at [Physics Forums]

My academic career has been a bit unusual, and the current position I have at Columbia is kind of confusing, so for those who want to carefully examine my qualifications before deciding whether to take anything I write seriously, here's a short outline:

1979: B.A. and M.A. in physics, Harvard University. As an undergraduate spent one summer working on a particle physics experiment at SLAC.

1984: Ph. D. in theoretical physics, Princeton University, advisor Curtis Callan, thesis title "Topological Charge in Lattice Gauge Theory".

In my thesis I developed a workable way of calculating the topological charge of lattice gauge fields and did Monte-Carlo calculations using it. This led to joint work with collaborators including N. Seiberg at the Institute in Princeton and about seven published papers on the subject in the mid to late-eighties.

1984-87 Postdoc at the Stony Brook ITP
Got interested in spinor geometry,TQFT and representation theory, started talking to a lot of the mathematicians at Stony Brook

In 1987 it became clear to me that someone who didn't believe in string theory but wanted to apply mathematics to QFT didn't have much of a future in physics depts in the US. I spent 1987-88 as an unpaid visitor at the Harvard physics dept., earning a living teaching calculus in the Tufts math department.

1988-89 Postdoctoral fellowship at MSRI in Berkeley. Published a couple papers on spinor geometry and the standard model, TQFT and representation theory.

1989-1993 Assistant professor, math department, Columbia.

This wasn't a tenure-track position, so at this point I needed to find a new one and my current job became available in the math department. It is an unusual, "off-ladder" untenured but permanent position with the title "Director of Instruction". Its responsibilities include administering the dept computer system, teaching a course each semester, and participating in research activities of the department. I've held this position for ten years.

It should be made perfectly clear that I'm not a regular, tenured professor at Columbia and have never claimed to be. On the other hand, I've spent a lot of time learning mathematics, often by teaching it. I've taught many of our undergraduate courses and some of our graduate courses, including Representation theory and QFT for mathematicians.

So that's my weird academic background and status. make of it what you will.

Personally I feel rather lucky at how this has turned out. It all started with having relatively well-off parents who could afford to send me to Harvard, I then enjoyed about the best education in particle theory possible, and now I have a permanent job surrounded by very talented people that I like, one that gives me a fair amount of time to think about what I choose. Anyone who thinks I'm an embittered soul doesn't know me very well. While I've seen a lot of talented people be badly treated by universities and by the atrociously bad job situation in many fields, I don't have anything to complain about.

One problem with this is I don't know what career advice to give young people interested in particle theory. They'd be fools to do what I did, but if they follow the standard path they'll probably get screwed. It seems to me that a very big question the particle theory community needs to be addressing is how to provide a career path for really smart students that gives them encouragement to strike out in new directions, with a viable chance at making a permanent career of it. Right now many of the young people in the field I talk to are very discouraged, feeling that their choice is to either try and make a name for themselves by working on a not very promising but trendy string theory topic, or to commit academic suicide by trying something different that probably won't work out. This situation is extremely unhealthy.

Comments

1. **Peter**
   March 28, 2004

   Hi Pyracantha,

   It's hard to talk about physics in general, different subfields have different things happening in them. As far as particle physics goes, progress has undeniably slowed in the past 25 years (people will disagree about how much…) for two overriding reasons:

   1. The standard model is just too good, making the field a victim of its own success. It was in place by the mid-seventies and all experiments agree with it (or to be precise, with a slight extension of it to include neutrino masses). It's vastly harder to come up with good new ideas about physics when you don't have experimental results that conflict with theory and point you in the right direction to improve the theory.

   2. Available accelerator technology is reaching its limits: there are diminishing returns and going to higher energies is more and more difficult and expensive. There will finally be a big new jump in energy at the LHC in a few years, but both political and financial problems have caused this jump to be a long time coming.

2. **Peter**
   March 28, 2004

   Hi Erin,

   I spend most of my time among mathematicians, so I'm very aware what they think of QFT and string theory. With few exceptions, they're pretty confused about the distinctions between QFT, string theory, supersymmetric QFT, etc. They generally see all of these as one uniform, mysterious set of methods that undeniably has lead to some fantastic mathematics. This isn't really surprising: these are complicated ideas, with complex relations between each of them and between them and more standard mathematics.

   So, they're often surprised to hear me criticizing string theory, saying "but it has lead to so much great math!". To a large extent this is right: string theory has lead to some great math and in many ways its influence on math has been very positive. Unfortunately many mathematicians believe that everything that has come out of physics came out of string theory, believing for instance that the work that Witten got the Fields medal for was string theory. It's hard to explain to them that many things like this came out of QFT, not string theory. It gets even more confusing since some of the most interesting math has come out of conformal field theory, which is both 2d QFT and a crucial part of perturbative string theory.

Personally I'm pretty convinced that, while good mathematics will continue to come out of string theory, QFT will ultimately have an even bigger impact on math. QFT involves mathematical structures that are infinite-dimensional examples of structures that are well-known to mathematicians in the finite dimensional case. Mathematicians often hadn't thought too hard about how to generalize to infinite dimensions, because typically there are many ways to do this and, until QFT came along, it wasn't clear which if any ways lead to something interesting. Even after getting guidance from QFT about which way to go, it is still very difficult to rigorously deal with these infinite dimensional things, so mathematical progress is frustratingly slow. If the study of QFT in physics was in a healthier state, there might be even more fruitful interchange between math and physics than there is at the moment.

3. **Pyracantha**
March 28, 2004

Is there a sense of "diminishing returns" in current physics research – more studies and more theorizing, but fewer results and less ability to verify by experiment?

Pyracantha

4. **erinj**
March 27, 2004

"Hype" indeed. String theory thrives upon hype and media attention in my opinion; for example, Greene's `The Theory of Everything' TV series recently shown here [known as `The Elegant Universe' in the U.S.]. Topics like the Standard Model just aren't the sort of thing people wish to popularize into TV series.

I recently read a mathematician refer to string theory as a particular quantum field theory (but perhaps he meant string field theory instead of string theory…?), and seeing any success – past, present or future – in string theory is difficult. Yes, I too hope things will change for the better. Surely the study of QFT is far from over, and is really only just beginning, for both physicists and mathematicians.

Incidentally, I thought Carlo Rovelli's `A dialog on quantum gravity' provided an excellent catalogue of many of the things string theory keeps on not doing and highlights many misconceptions surrounding the alleged successes of string theory. I especially liked the way Rovelli included a debunking of the claim that the Veneziano amplitude is `correct' (not something many string theory papers discuss these days, I suppose).

Erin

P.S. I would leave a valid e-mail address, but one never knows who's reading this (anyway, I'm not actively involved in academic research).

5. **Peter**
March 26, 2004

Hi Erin,

It seems to me that situation in the US is polarized in exactly the same way as in the UK. To an increasing extent, this seems to be the case around the world. Globalization also applies to particle theory, I guess.

The standard story you'll get from string theorists is that QFT is essentially a closed book, and that everything interesting about it is known. From their point of view, someone wanting to work on field theory is either too much of a wimp to do string theory, or just too ignorant to realize that everything of interest about field theory has already been done.

This point of view makes sense if you believe the hype that string theory is a successful extension of QFT that explains all the things QFT can't. When and if people realize that string theory actually doesn't live up to its hype, maybe things will change.

I think the situation now is very analogous to what happened in the late 60s, early 70s. Most of the leaders in particle theory had abandoned QFT and were doing S-matrix theory (and later, string theory). The few people like Veltman still doing QFT weren't taken very seriously, I'm sure many people explained to him why he was a fool to work on quantizing and renormalizing non-abelian gauge theory. This all changed with the advent of the standard model, one can hope that a similar shift will happen again in our lifetimes.

6. **erinj**
March 26, 2004

Hello Peter,

Ever since `String Theory: An Evaluation', I've made sure I read your articles and papers, and only a few days ago I discovered that you had begun a weblog: it's marvellous and fascinating, and I certainly agree with your views on string theory.

I myself am a British, disgruntled former particle theory postdoc, and I can relate to your comments on young people entering – or attempting to enter – the field of particle theory research. My PhD research was concerned with field theory, and I wished to continue with field theory research, but found that the opportunities for that in the U.K. were far more limited than I had expected. Increasingly left out in the cold, and struggling to do something which might earn me another postdoc in field theory (which no one seemed interested in), I was dropped and now consider myself a failure. I do wish I could have continued.

To me, there seems to be a stark choice those interested in particle theory research have to make: do phenomenology, or do string theory. This is the message I received during my time in research. Perhaps if I had chosen one of these, instead of being interested in field theory, I might still be a postdoc.

It would seem that if you choose to do research in field theory or mathematical physics – or indeed anything other than phenomenology or string theory – one has to be prepared to work towards either string theory or phenomenology, for doing anything else seems to offer no stable, long-term employment or any career development prospects in particle theory research. Why is it that present (U.K.) research in particle theory seems to be so polarized? Is it the same situation in the U.S., or even worldwide?

Incidentally, during my PhD, my field theorist supervisor was just beginning to turn into a string theorist, and now the transformation is complete. It was a rather discouraging thing to see. I do wonder if he felt pressure to migrate to string theory in order to remain in academia.

Erin J

Atiyah and Singer Share Abel Prize

March 26, 2004
Categories: Uncategorized

The Abel prize is a new yearly prize in mathematics, intended to function somewhat like a Nobel Prize for mathematics. The first one was awarded last year to Jean-Pierre Serre and this year's has gone to Sir Michael Atiyah and Isadore Singer, specifically for their development of the Atiyah-Singer index theorem.

Atiyah and Singer are great heroes of mine, especially Atiyah. They have both taken a great interest in the relation of mathematics and quantum field theory and are responsible for much of the fruitful exchange of ideas between the two subjects over the last 25 years. I consider much of my mathematical education to have come from a lot of time spent reading through Atiyah's five-volume collected works. His interests have ranged over a wide swath of modern mathematics and his writing is always a model of clarity. A good case could be made that Atiyah has been the most important figure in mathematics during the second half of the twentieth century.

The Atiyah-Singer index theorem is perhaps the single most important theorem of the last half century. It links together analysis, topology, geometry and representation theory in a fundamental and surprising way, one that is dear to any physicist since it involves the Dirac operator. Very roughly, what Atiyah and Singer discovered was that the dimension of the space of solutions of certain PDEs on compact manifolds was a topological invariant, one that they could explicitly compute in terms of more well-understood topological invariants: the cohomology classes of the manifold.

They did this by noticing that the general case could be reduced to the case of the Dirac operator, twisted by the various possible vector bundles on the manifold. They actually rediscovered the Dirac operator for themselves in the course of their research. The natural abstract framework for these new topological invariants is K-theory, where classes are represented by vector bundles, much as cohomology classes are represented by differential forms.

Things get even more interesting if there is a symmetry group acting on the whole set-up. Then the space of solutions carries not just an integer, the dimension, but is a representation of the group. You can actually use the Atiyah-Singer index theorem to classify and construct geometrically the representations of many classes of Lie groups, or going the other way, use representation theory to get more powerful topological invariants. The explicit cohomological formulas you get in these cases often have versions which localize to fixed points of the group action; so you can find your answer just by locating the fixed points and looking at what happens in a neighborhood about them.

I hope Atiyah and Singer enjoy their shared $875,000!

Comments

1. **Danny Ross Lunsford**
   March 31, 2004

   Hi Peter,

   Could you elaborate on Grothendieck's new ideas of "space"? Do you mean change of element (e.g. Pluecker) or something totally new?

   I can hardly believe I'd never heard of this person!

   -danny

2. **Danny Ross Lunsford**
   March 31, 2004

   Hi Erin,

   I can't make any definite statements about LQG because I don't know enough about it. My understanding is – it makes more physical sense than strings but there are fewer "results". Neither is finished in the sense of having a definite schemata and methods. IMO string theory is ab initio preposterous, while LQG is excessively formal. Having some detailed knowledge of the history of physics, progress just does not come that way.

   I don't want to spam Peter's blog with details, but if you are interested in what I did I'd be happy to explain it elsewhere. In contrast to LQG or strings, it's a complete scheme with sensible elements and very little arbitrariness, and does not purport to be quantum gravity up front (although I am quite confident that strict locality in this theory makes it a much better candidate for quantization than GR). It is a shame to have to work in isolation because I could really use input from field theorists with physical grounding at this point. This after all is why collaborative environments in universities were invented.

3. **Peter**
   March 30, 2004

   Grothendieck's work and life are truly amazing. Someone should write a biography and make a movie; it would make John Nash's story ("A Beautiful Mind") look pretty prosaic.

   I had looked at that web-site last year, but there is a lot more material there now. It has a pdf of "Recoltes et Semailles" the ravings of Grothendieck's later years, scanned by a student here at Columbia (Max Lipyansky) from a copy I had in my office. For physicists, I recommend reading the article by Pierre Cartier about Grothendieck's work, subtitled "The evolution of concepts of space and symmetry". Grothendieck's mathematical concerns were far from physics, but he developed fantastic new ideas about how to think about a "space". Since physicists are now so sure that the standard geometrical concepts of space and time need to be generalized, they might get some inspiration from Grothendieck's ideas.

4. **erinj**
   March 30, 2004

   Hi Peter,

   Yes, it would be very interesting if the Abel Prize were to be awarded to – or attempted to be awarded to – Grothendieck. I believe he also refused to travel to Moscow in 1966 to collect his Fields Medal. Have you ever seen the following website? I recommend it:

   http://www.grothendieck-circle.org

   Grothendieck is/was a fascinating mathematician! His work is astonishing, in my opinion. (I'm a bit of a fan of his, even if most of his major works remain untranslated into English.)

5. **Peter**
March 30, 2004

Hi Erin,

I think the whole idea of setting up the Abel prize was to have something more or less equivalent to a Nobel prize in mathematics. One reason for this certainly is to raise the public profile of mathematics. We'll see if it works.

The Fields medal until now has been the most well-known prize in mathematics (although there are some others, including the Wolf prize). It is somewhat different in that it is given only once every four years (to 2-4 people), is restricted to mathematicians under the age of forty, and carries little or no money with it. So there are relatively few Fields medalists compared to, say Physics Nobelists, and quite a few very good mathematicians don't have one since either they didn't do their best work or it wasn't recognized before they were 40. An example is Andrew Wiles, who was a bit over 40 by the time he found a proof of Fermat's last theorem.

It's kind of a strange situation, to start up a new prize like this. One has all the best living mathematicians to choose from; many of them have Fields medals, some don't. There was some speculation the Abel prize committee might decide to first award prizes to those who didn't get a Fields medal (e.g. Wiles), but that isn't what they are doing. I'd guess a lot of mathematicians would list as the top three living mathematicians of the last half century Serre, Atiyah and Grothendieck. It will be interesting to see what the Abel committee does about Grothendieck. He's still alive, but stopped doing mathematics and hid himself away in the mountains in France. The last time someone tried to give him a prize (the Crafoord Prize), he very publicly rejected it. I'd assume he'd do the same thing if the Abel committee tries to award him the prize.

6. **erinj**
March 30, 2004

Hi Peter,

It's wonderful to see great mathematicians of our era such as these recognized and honoured like this. (I realize I should have posted these comments earlier, but I'll do so anyway now, disconnected as they are from the preceding posts.)

I've often wondered if the existence of a Nobel Prize in Mathematics would have improved the global public perception of mathematics, and especially made mathematics be considered on a par with the other sciences for which Nobel Prizes exist (which surely it is, isn't it?). Perhaps more people would study mathematics if they knew there were Nobel Prizes awarded for it. I myself think Nobel did mathematics a bit of diservice by excluding it.

So if the Fields Medal is the `equivalent' of the Nobel Prize in mathematics (despite being awarded only every four years, restricted to those younger then 40 years of age and having a smaller prize fund), then where does that leave the Abel Prize? Doesn't the Abel Prize perhaps go some way to being the Nobel Prize in mathematics (awarded annually, Scandanavian, large prize fund, likely to be awarded to those over 40 years of age… perhaps always awarded to fewer than three people?)?

Another thing: both Serre and Atiyah already have Fields Medals, but Singer does not. How many other future Abel Prize winners will also be Fields Medallists or have worked with one?

Erin

P.S. Danny: isn't the theory of loop quantum gravity equivalent to background-independent – presumably synonymous with "background-free" – Yang-Mills gauge theory? If it is, then there are already at least some physicists working on this theory.

7. **Peter**
March 30, 2004

Hi Chris,

I've seen the academic system (at least its US version) at work from closeup in two different fields, particle physics and math, and the differences are interesting. There are problems with how it works in math that are somewhat the same as those in physics: it is tough for young people who want to try to do something really ambitious, and the safest bet for one's career is to work on a not too ambitious topic in a hot area. But things are very different in math for several reasons:

1. The ratio of good young people to jobs is nowhere near as bad as in particle theory. Very roughly I'd guess the situation in math is that there are perhaps twice as many people getting Ph.D.s in math as there are permanent teaching jobs that might allow you time to do research. In particle theory the situation is closer to ten times as many people as permanent jobs. The game of musical chairs for young people in particle theory is a lot more brutal than in math.

2. Particle theory has always been a much trendier subject than math, partly for the good reason that , historically, as unexpected new experimental results became available, the smart thing to do was generally to be thinking about what they meant. The lack of any unexpected experimental results for nearly 30 years has been hugely problematic for particle theory.

So, while we agree that the particle theory academic system is quite unhealthy these days, I don't think that moving the power base to the young would help much (besides being extremely unlikely to happen; the old do not give up power so easily!). If older people as well as younger ones were all in the same situation of way too few jobs, with the only way to survive to be working on the latest trend, things might not be any better.

My own general ideas about how to make particle theory a healthier business are something like the following:

1. First of all, the hype has to be brought under control. As long as ideas that don't work are being sold as huge successes, the field is going to be really unhealthy no matter how it is organized.

2. Bring the ratio of good young people to jobs under control, either by finding a way to fund more positions (this may happen to some extent naturally as the rate of retirements picks up) or by limiting the size and number of particle theory Ph.D. programs.

3. Find ways of encouraging people to work on less trendy things. This would require a real change in the whole sociology of how particle theory is organized. There are a lot of ways you could imagine encouraging such a change.

I must admit though, that prospects for any such changes look pretty dismal.

8. **Chris Oakley**
March 30, 2004

Hi Danny,

Nice to hear an assenting voice. Or any voice at all, for that matter. My beliefs on QFT [anti-renormalization] and academic research in general [anti-gerontocracy] have thus far provoked a deafening silence. Maybe there are too many vested interests. If Dirac had lived 50 years later, I might have survived better. I particularly liked his quote, when talking about QED: “Just because a theory agrees with experiment, it does not mean that it is correct.” Thank-you, Paul. As usual concise and to the point.

I have to say, though, that in many ways I believe that I am only stating the obvious. In the first case it is the obvious fact that you can get any answer you want by subtracting infinity from infinity and in the second case, I, like everyone else, was more creative and mentally agile at the age of 22 than I am now at 44. One should accept these things, and it is grossly unfair that someone my age now who has an academic post can put obstacles in the way of a young researcher with new ideas just because they are not *my* ideas.

I am not answering your question … I am *sort of* still working on QFT in my spare time, but on a kind of QFT text book (sans renormalization, obviously) rather than research. I will post it on my site in the not-too-distant future.

In the meantime, in particular as regards the komissar, do not forget the old adage:

Non Illegitemi Carborundum

9. **Danny Ross Lunsford**
   March 30, 2004

   You just have to stick to the ideas and hope the world gets better. At least you are still working.

   I remember your work, it was interesting. Are you still at it?

   If it’s any consolation, I actually managed to solve Einstein’s problem and get it published, only to be blacklisted from the archive by its komissar. No one in the entire world seems to be interested in background-free electrodynamics (and coming soon, background-free Yang-Mills!) This to me is the deepest mystery of all.

   Keep at it, all you can do is – all you can do.

   -danny

10. **Chris Oakley**
    March 30, 2004

    This is neither an ad for sexual-performance-enhancing chemicals or an assessment of whether or not Michael Atiyah deserved $437,500 for his index theorem, being rather some general observations about academic research which have already been made on my web site, but will be expanded on when I can be bothered. It follows on from Peter’s post “My (Not So) Brilliant Career”.

    As far as I can tell, the research situation now is much the same as it was when I was a graduate student/research fellow (1982-1987). The basic fact is that an institution (pure academic research) that has no clearly-defined objective will by definition lack a self-correction mechanism. This is not the case in financial software, which is what I work in now, as measures of success and failure are often brutally apparent, even to those who have never written a line of code in their lives.

    If the academic system worked as it should, every graduate student entering the field would be told something along the lines of the following: “You are where you are because have exceptional ability in mathematics and science. It may well be, although it does not necessarily follow, that you have valuable contributions to make to your subject. It does not necessarily follow because what you are now embarked upon is essentially different to what you have done up till now. Instead of jumping through hoops that others have set up, you will now be deciding the nature and shape of the hoops themselves. If the task of passing exams were to be likened to being an accountant, the task of doing research is like being an entrepreneur. Read and absorb everything you can get your hands on. Do not rely on others to tell you what is important as these assessments have to come from within yourself. If having done this, you decide that, in all honesty, all you can hope to contribute is a few dotted i’s and crossed t’s then get out and do something useful instead. You will almost certainly be better paid if you do.”

    No-one said this to me in 1981. What it was about was not doing what you thought was right, but getting noticed. You had to attach yourself to some research project, however little you believed in it, and then hope that the “big shots” in this field liked you enough to offer you employment. Fine if you believed in it, but if there was *nothing* that they were doing that you believed in, and you wanted to start your own line of enquiry, then there was nowhere to go except out. The herding tendency was embarrassing. “Safety in numbers” was and is the principle, and I am amazed at the extent to which researchers were and are prepared to sacrifice integrity, enjoyment and quality of life for the sake of the distant prospect of a poorly-paid tenured academic job. If the situation in particle physics was bad 20 years ago, then it is ten times worse today.

    The model of researchers as inquisitive children rather than flocks of sheep would work much better for me. How might one get there? The obvious step would be to move the power base to younger people, by (a) only granting research posts on a rolling 5-year basis, i.e. NO permanent posts and (b) giving equal votes in hiring, etc. to ALL involved in research, including graduate students. These steps would perhaps not solve *all* the problems, but they would at least make academic research more interesting.

Various Hype

March 29, 2004
Categories: This Week's Hype, Uncategorized

A recent new experimental result from Brookhaven has lead to some news stories like:

Theory of matter in for a “sensational” revision

which sounds pretty exciting. If you look into this more carefully, you’ll find that it’s based on a report from the E949 experiment at Brookhaven of the observation of a single event of a decay

K+ to pi+,nu, nubar

The preprint is at
hep-ex/0403036
From reading it, as far as I can tell the bottom line is that this is the third candidate event for such a decay ever observed, the two previous ones were from Brookhaven E747 in 1997 and 2002. The standard model prediction is for about 1.5 events for the combined data from both experiments, with expected background of about .5 events. So, all together, the expected number of events is two, they have seen three. Where does the “sensational” come from?

At Slashdot, there’s some more string-theory related hype:

Testing Relativity

about a proposed experiment for the International Space Station that would “test general relativity to a precision within the bounds of superstring (and other) theories to predict deviation.” This article links to a NASA webpage

Evicting Einstein

which goes on about string theory and extra dimensions in the usual way, then describes the proposed “Laser Astrometric Test of Relativity” (LATOR) experiment, which would precisely measure the effects of general relativity on solar system scales. If you read the NASA page closely you’ll see that its author was careful to just say

1. String theory, etc. predict deviations from GR

2. LATOR will test GR precisely

and not to mention that 2 has nothing to do with 1, since the deviations that LATOR could see aren’t the deviations you expect to see from string theory (although since string theory can be used to “predict” just about anything, perhaps you could claim that it “predicts” some unobserved nearly massless field whose effects LATOR would see).

No Comments

String Theorists Finally Admit Defeat

April 1, 2004
Categories: Uncategorized

The news that next week's "Science Times" will run an article by NYT reporter James Glanz in which several leading string theorists say that they are giving up on the idea is rapidly spreading throughout the particle theory community. Evidently Glanz recently went down to Princeton to interview Edward Witten, who took the opportunity to announce that he has changed his mind about whether string theory will ever be a "Theory of Everything". When Glanz contacted other string theorists and read to them what Witten had said, almost all of them told him that they too had been having their doubts about the theory.

Glanz quotes Witten as follows:

"One night a few weeks ago I was sitting at my kitchen table trying to make sense of Douglas's latest work on the KKLT proposal and all of a sudden it really hit me that this is a completely lost cause. If perturbative string theory has any relation to Planck scale physics, then KKLT or something like it should work and string theory is vacuous since it can never predict anything. If perturbative string theory isn't useful then we really don't have anything since we've never been able to come up with a non-perturbative version that makes sense. Twenty years of this is enough. It's time to give up."

When Glanz asked him what he intends to do now, Witten responded:

"I don't really know. There are still promising ideas about using string theory to solve QCD, and I could keep working on those. Maybe I should take up something completely different, like biology. I'm starting to worry that John Horgan was right about the 'End of Science'. Right now I just definitely need a long vacation."

When Glanz read Witten's statement over the phone to David Gross, Frederick W. Gluck Professor of Physics at UCSB and Director of the Fred Kavli Institute for Theoretical Physics, Gross thought for a moment and then told him "Yeah, despite my quote last year from Churchill, I've also been thinking of giving up. Not sure though how I'm going to break this to the two Freds."

The news of Glanz's article has had dramatic effects at many universities and research institutes. At MIT yesterday, Prof. Barton Zwiebach shocked students in his Physics 8.251 "String Theory for Undergraduates" class by announcing that he wasn't going to collect the homework due that day and was canceling his lectures for the rest of the semester. He also asked Cambridge University Press to halt publication of his new undergraduate textbook called "A First Course in String Theory", the release of which had been planned for next month.

Search committees at several institutions that hadn't finished their hiring yet this season held new meetings to decide how to react to the news. A prominent theorist at a UC campus told me in an e-mail that "our chair had the phone in his hand and had already dialed the number of a string theory graduate student from Princeton we were going to offer a post-doc to. I ran into his office as soon as I heard the news and stopped him just in time. Last week we were sure that string theorists were the smartest guys around and considered only them for jobs, but now there's no way we're going to hire any more, ever!".

At the Institute in Princeton this year's "Summer Program for Graduate Students in String Theory" scheduled for July has been canceled, with one of its organizers remarking "what graduate student would now be crazy enough to show up for a program like this?" Next week's conference on "The Status of M-theory" at the Michigan Center for Theoretical Physics has also been canceled on very short notice. The director there, Michael Duff, commented "We had to do this because the status of M-theory is all too clear. It's passed on! This theory is no more! It has ceased to be! It's expired and gone to meet its maker! … This is an ex-theory!"

Comments

1. **Peter**
   April 5, 2004

   Hi Urs,

   There are non-perturbative string theory calculations out there, but they have no known relation to the real world, so can't be used to predict anything.

   I'm not sure what Douglas and Vafa work you are referring to. One guess would be toy d=2 string models and topological strings. Neither of these has any understood connection to 4d physics. Similarly with matrix models. There simply is no such thing as a non-perturbative formulation of string theory that allows one to calculate anything in the situation one cares about of four large dimensions.

   Even worse, you not only don't have a theory you can calculate with, you don't even know the energy scale of the mythical theory you want to believe in. Is it the Planck scale, is it 1 Tev? It could be anything as long as it's not too small since then it would be ruled out by experiment.

   So, not only is there no prediction of an infinite tower of new states, there is no prediction of how many new states to expect from the theory, what their properties will be, or even at what energy scale they will occur.

2. **nnyhav**
   April 5, 2004

   Seems a shame the parrot sketch will only share space with the Holy Grail for a bit longer … but BBC4 chimed in on the latter …

3. **Urs Schreiber**
   April 4, 2004

   Hi –

   true, the full tower of massive states would only be observable for vanishing coupling. At finite coupling string self-interactions lead to a collapse of highly excited states as computed in papers concerned with string/black hole correspondence, e.g.

   Thibault Damour & Gabriele Veneziano, Self-gravitating fundamental strings and black-holes (1999)

   This effect leads to the creation of '*string balls*':

   Kingman Cheung, Black hole, string ball, and p-brane production at hadronic supercolliders (2002)

(I recall that we had a very similar discussion before.)

Yes, the above paper mentiones large extra dimensions such that the string scale would come into reach of next-generation colliders. If the string scale is further away, this means that these experiments will be possible only at higher energies. But it is possible in principle.

Regarding the general criticism that only perturbative effects are known: Recent results by Douglas on D-branes and by Vafa and others are all concerned with non-perturbative effects. But I don't understand these very technical paper well enough to say much more about them.

Regarding the fact that Gross and Witten say that a full non-perturbative formulation is still not known:

Sure. Robert Helling for instance (together with Nicolai and others) has shown that BFSS can only apply to a certain 'sector' of M-theory. But there it is a fully non-perturbative formulation. Helling tells me that the advent of the AdS/CFT 'bandwagon' has caused many people to reduce research in BFSS and concentrate on Maldacena's conjecture. Hence counting number of people and papers in these fields doesn't say much about the field itself.

4. **Peter**
April 2, 2004

Hi Urs,

A few comments:

About non-perturbative string theory: I did quickly look at the matrix model references you gave and saw no real evidence there of any hope of extracting realistic, 4d physics out of them. Maybe you or someone else can make progress on this, but it looks to me like a very unpromising idea to pursue. The statement that there is no known consistent non-perturbative formulation of string theory that gives realistic physics (only four large dimensions, recognizable particle spectrum) is not controversial. Gross and Witten for years have made clear in all of their general talks about string theory that they feel they don't have a good idea about what non-perturbative string theory is.

About Distler's comments: He says that string theory "promises you" universal behavior of quantum gravitational effects at high energies. The problem is that this is still a promise, not reality, and will continue to be only a promise until either someone shows that perturbative string theory is a consistent approximation to something or finds a true non-perturbative theory.

About the string theory "prediction" of an infinite tower of states:

This is a "prediction" of perturbative string theory. Why should one believe that perturbation theory gets the properties of the higher mass states right but not the vacuum state? Isn't this just because one can experimentally check whether a prediction of the vacuum state is right, but you can't check "predictions" of higher energy states so you are free to claim them? Non-perturbatively you are going to have all sorts of things happening with these higher energy states. Won't you start producing not just perturbative excitations of the string, but black holes, branes, and who knows what else? The high energy spectrum could be just about anything.

5. **Peter**
April 2, 2004

"Just wondering–and no disrespect meant–is this an April Fools Day joke?"

Ummmm, yes. Some people think it was rather too subtle, some not.

"I heard from Harvard yesterday that Lubos Motl has decided to take up professional wrestling now that his job there is being cancelled."

I had thought about including news about Lubos, but he is pretty hard to parody.

6. **Urs Schreiber**
April 2, 2004

Hi Thomas Larsson –

I think it is a reasonable standpoint to say, as you implicitly do in your last comment, that quantum gravity as such is a problematic enterprise because it is not clear yet if and how the theory can be experimentally tested *in practice*.

If anyone feels that this is reason enough for him or her not to be interested in quantum gravity then that's fine with me. Similar comments probably apply to research for instance in theoretical quantum computing as well as some aspects of (classical) cosmology.

7. **Thomas Larsson**
April 2, 2004

>
It's not very subtle. String theory predicts for instance a tower of infinitely many massive particles.

At the moment we cannot check this, just as we cannot check if the core of Pluto isn't made of green cheese.
>

Hence this prediction is not falsifyable within a reasonable amount of time. Or is there any experiment that can produce this tower of infinitely massive particles?

This is probably the most disturbing aspect of string theory. It may make predictions in principle, like LQG predicts quantization of area and volume, but no experiment will ever be able to disprove these predictions, so they are not falsifyable. As Dan Friedan points out, only large-distance physics is really science.

I find it very disturbing that most string theorists simply don't seem to understand that a theory is not testable unless there is some experiment that can test it, and falsify it, within a reasonable amount of time.

8. **raj**
April 2, 2004

Just wondering–and no disrespect meant–is this an April Fools Day joke?

9. **Urs Schreiber**
April 2, 2004

It's not very subtle. String theory predicts for instance a tower of infinitely many massive particles.

At the moment we cannot check this, just as we cannot check if the core of Pluto isn't made of green cheese.

10. **Thomas Larsson**
April 2, 2004

>
I can't help but feel that the 'doesn't predict anything' mantra is of the same quality as the 'will give us particle masses in two months' missaprehension was.
>

Please, Urs! Do you really claim that string theory makes hard predictions, that can be conclusively tested, and possibly falsified, within our lifetime or indeed within the present millenium? Would you then please tell us exactly which experimental result that would conclusively prove string theory wrong? No light Higgs at the Tevatron? No sparticles or extradimensions at LHC? No proton decay at SuperK? A positive cosmological constant?

Unless you can give us an example of a falsifyable string theory prediction that can be tested within a reasonable amount of time, I think you should stop insinuating that there is.

I agree with you that LQG cannot make such predictions neither, and that it has other problems as well, but at least Ashtekar and Rovelli don't go around and claim that LQG is more predictive than QFT.

11. **Urs Schreiber**
April 2, 2004

Peter insinuated that string theorist's have

> never been able to come up with a non-perturbative version that makes sense

Last time that I tried to discuss this with you I didn't hear back from you, unfortunately.

I believe this could be a rewarding discussion for both sides, when done with care and focusing on the technical aspects. Over at the Coffee Table there are Robert and Lubos who have both worked on Matrix Models and Arvind is an expert on AdS/CFT. So if there really is any desire to discuss the status of non-perturbative formulations it should be possible. I'd enjoy it.

12. **Danny Ross Lunsford**
April 2, 2004

Serenus – ROFL!

"Cage match – the ROCK vs. The Cancelled Czech!"

-danny

13. **Urs Schreiber**
April 2, 2004

I can't help but feel that the '*doesn't predict anything*' mantra is of the same quality as the '*will give us particle masses in two months*' missaprehension was.

I rather see more truth in this statement by Jacques Distler where it is pointed out that even with lots of vacuua string theory is more predictive than any other candidate theory we can currently think of.

14. **serenus zeitblom**
April 2, 2004

Yes, that's very exciting. I heard from Harvard yesterday that Lubos Motl has decided to take up professional wrestling now that his job there is being cancelled.

15. **Sean**
April 1, 2004

You sense of humor may be too subtle, Peter. Didn't I also hear that Bush "doesn't feel right" about the Florida election fiasco in 2000, and has decided not to run for re-election this year?

16. **Danny Ross Lunsford**
April 1, 2004

Hmm, so much for the promising idea that dark matter is really made of stray lint and dust bunnies.

Back to the loom for me, it is!

-drl

Young String Theorist Did OK On His Final

April 5, 2004
Categories: Uncategorized

A while back when looking for theoretical physics related weblogs I ran across

The Search For A Theory of Myself: The Struggle Against String Theory

which at first sounded right up my alley. It turns out to be the weblog of a grad student at UCSB who is taking a course in string theory there from Polchinski. He's working quite hard at doing well in the course, and seemed very worried about his final on March 15. After the final postings stopped, so I was kind of worried about what had happened to him. Yesterday he finally posted again. He got an 88 % and seems to be all right.

No Comments

KKLT Smackdown

April 7, 2004
Categories: Favorite Old Posts, Multiverse Mania

I was dubious of the value of a new “sci.physics.strings” newsgroup when it was first proposed, but now must admit it seems to have been a great idea It started up a week or two ago, and quickly someone asked the seemingly innocuous question of how many different possible vacuum states were expected in string theory. This is a hugely controversial issue among string theorists, largely because recent evidence is that the number is definitely astronomically large, and this makes it very unlikely that current ideas about string theory can ever be used to predict anything about the real world.

A lot of the discussion revolves around the “KKLT” proposal for constructing a large number of these vacuum states. The acronym is the initials of the authors, three of whom are at Stanford: Shamit Kachru, Renata Kallosh and Andrei Linde. Also at Stanford is Lenny Susskind, who has been spending the last year or so going around giving talks on the “Landscape of String Theory”. It’s hard to believe this, but Susskind’s claim is essentially that the lack of predictivity of string theory is a good thing, since it allows so many possibilities that anything can happen. One can then invoke the “Anthropic Principle” to explain why the world is the way it is. It seems that Susskind is even writing a book about this wonderful “discovery”.

Amazingly enough, the thread about this on sci.physics.strings, entitled “Conceptual question”, has brought a public attack on the “Stanford propaganda machine” by a well-known European string theorist (Wolfgang Lerche), a detailed defense of his ideas by one of the KKLT authors (Kachru), contributions from the inimitable Lubos Motl from Harvard, and, while I was writing this, a defense of the anthropic principle from Joe Polchinski just appeared, which attacks the “cult of monovacuism” embodied by David Gross and Ed Witten.

Thanks are due to the creators of this newsgroup. Pass the popcorn!

Comments

1. **Peter**
   April 13, 2004

   Hi String Theorist,

   The argument that maybe the things we don’t understand about the standard model are artifacts of history and not something a more fundamental theory can predict deserves a longer response than I have time for right now. It’s an important issue and I’ll try and write up some thoughts about it soon.

   You’re not answering the question I’ve been trying to get an answer to from string theorists: what exactly do you expect string theory to be able to ever predict?

2. **D R Lunsford**
   April 13, 2004

   Comments to comments by Anonymous Stringer:

   “Just because string theory is a “theory of everything” does not mean it can be used to calculate everything! Let me explain by analogy.”

   This is like absurdist fiction. No other comment required. One invokes the “prima facie preposterous idea” argument with success.

   “Do you think that Newton’s theory of gravity is vacuous because it cannot predict the number of planets in the solar system, or calculate their distances from the sun (0.39, 0.72, 1.00, 1.52, etc A.U.)?”

   This is an absurd analogy. What Newtonian gravity comprehensively succeeds at is explaining orbital motion and dynamics, and that is all that is required. The proper analogy would be a version of string theory that, given a proper limiting argument, showed that weak interactions did not conserve parity, explained the tiny mass of the neutrino, gave a theoretical justification for the phenomenology of the Higgs mechanism etc.

   “One of the hallmarks of scientific progress is that frequently, what we had thought were interesting scientific questions turn out to not be scientific questions at all.”

   This is more absurdist fiction. The same questions – what is motion, what is the structure of matter, etc. – are as self-evident and vital today as they were in the time of Democritus. There are no real questions of science that later turn out to be misguided – what DOES exist is a clear history of trying to explain away the uncomfortable facts, as in mechanical models of the ether.

   “Nowadays, of course, we understand that there is no reason to require physics to be able to calculate the number of planets, or their orbital periods or sizes. They are what they are.”

   This is sheer sophistry and an embarrasing confusion of phenomenology with theory.

   “This is why I asked you what you thought string theory should be able to calculate. And, as I expected, the things you mentioned are all things to be observed, not things to be calculated.”

   What can be calculated can only be determined after a theoretical model is in place. There is no real model in string theory other than the on-the-face-of-it preposterous idea of fibrills in an umpteen-dimensional wonderland without any bearing on history or reality.

   In fact the goal of any subsuming theory should be to explain the previous one by a limiting argument that not only shows why the last one failed, but how the new one points to new phenomena to be expected. All the previous successes in physics are like this.

   “It is no more “necessary” for the gauge group of the universe to be SU(3)xSU(2)xU(1) than it is “necessary” for there to be four gas giant planets in our solar system.”

   This is totally wrong-headed. Particle physics is phenomenology that will one day be explained theoretically when the excresences of this generation are forgotten. The gauge group of the Standard Model is simply the encoding of the phenomenology – this is not a criticism, rather praise for experimentalists and field theorists to get at least part of it unmistakably correct. There is more truth in one generator of this “arbitrary” group than in all the decades of windmill-tilting of stringers.

   “Similarly, when you say “Calculate for me the fermion mass matrix” it sounds the same to me as “Calculate the masses of the planets.”

   More absurdist fiction. A real encompassing theory, such as the one that explains the relation of electricity to magnetism, always includes dynamical explanations of previously phenomenological inputs. The masses of the Fermions are the LEAST things to expect from a good theory. And in fact, there already exists a good theory of

Fermion masses based on analysis of Clifford algebras – so clearly this is not going to be the hardest problem.

"There are still plenty of people clinging to the modern day equivalent of Bode's Law or Kepler's solids, believing that there might be a unique consistent quantum field theory which therefore is necessarily the one describing our universe. But I think this point of view will soon be widely seen as outdated."

Who are they? The only physicists who seem manifestly Dadaist to me, are you people. If you only knew how foolish are the things you claim, you would desist and try something else.

3. **String Theorist**
April 12, 2004

Hi,

I think it is very important to discuss the issue of what kinds of things we should and should not expect to be able to calculate within string theory. Just because string theory is a "theory of everything" does not mean it can be used to calculate everything! Let me explain by analogy.

You seem to think that string theory is vacuous because (among other things) it cannot predict the gauge group, or calculate the fermion mass matrix.

Do you think that Newton's theory of gravity is vacuous because it cannot predict the number of planets in the solar system, or calculate their distances from the sun (0.39, 0.72, 1.00, 1.52, etc A.U.)?

This is a fair analogy: Newton's gravity is a theory of everything relevant to the dynamics of non-relativistic gravitationally interacting bodies (such as planets), while string theory is a theory of everything relevant to the dynamics of elementary particles (such as gauge particles and fermions).

One of the hallmarks of scientific progress is that frequently, what we had thought were interesting scientific questions turn out to not be scientific questions at all.

For example, the ancients thought there were five planets, and spent a great deal of intellectual effort theorizing why there HAD to be precisely five planets. Once it was realized that the Earth also orbits the sun, Johannes Kepler tried to explain the existence of precisely six planets (and to explain the sizes of their orbits!) by inscribing the five Platonic perfect solids between the orbits of successive planets.

The discovery of Uranus in 1781 relegated the Platonic solid idea to the history books. Now the best theory was Bode's law, originally pointed out in 1766 by Johann Titius, which predicted the location of Uranus almost spot-on! Unfortunately, Neptune didn't fit into the picture very well.

Nowadays, of course, we understand that there is no reason to require physics to be able to calculate the number of planets, or their orbital periods or sizes. They are what they are.

This is why I asked you what you thought string theory should be able to calculate. And, as I expected, the things you mentioned are all things to be observed, not things to be calculated.

It is no more "necessary" for the gauge group of the universe to be SU(3)xSU(2)xU(1) than it is "necessary" for there to be four gas giant planets in our solar system.

Similarly, when you say "Calculate for me the fermion mass matrix" it sounds the same to me as "Calculate the masses of the planets."

There are still plenty of people clinging to the modern day equivalent of Bode's Law or Kepler's solids, believing that there might be a unique consistent quantum field theory which therefore is necessarily the one describing our universe. But I think this point of view will soon be widely seen as outdated.

String Theorist

4. **Peter**
April 12, 2004

The list of things an improvement on the standard model should be able to predict is well-known: choice of gauge groups and representations in the SM, gauge couplings, Higgs sector parameters (including fermion mass matrix), Newton's constant (for instance as ratio of Planck/GUT scale), cosmological constant. Presumably the standard model breaks down at some energy scale; one should be able to say what that is and predict what new phenomena occur above that scale. Especially impressive would be a prediction of one of the still unmeasured SM parameters (the Higgs or neutrino masses).

The closest thing I know of to a framework that can do any of this is the supersymmetric GUT framework, where one can get a prediction of one gauge coupling in terms of the two others that works at the 10% level. Unfortunately this comes with a lot of baggage, including another hundred or so undetermined parameters of the supersymmetric extension of the standard model. If one is pursuing this idea about unification, these are on the list to be predicted.

I'm curious how string theorists look at this. What are the prospects now for using string theory to make any predictions of the sort listed above?

5. **String Theorist**
April 11, 2004

Hi Peter,

OK, I'll bite. You wrote:

Last year as a joke I was telling people that the way things would end up is that string theorists would simply declare victory, announcing that string theory was the final TOE even though it was inherently incapable of calculating anything.

What exactly would you most like to see us (try to) calculate? Maybe you could give us a 10 most wanted list?

String Theorist

6. **Peter**
April 11, 2004

Hi Thomas,

I did notice that Lerche was on the "multivac" side of the discussion; that he was just annoyed that others were claiming credit for it.

On the sci.physics.strings newsgroup only Motl seems willing to defend the "univac" side (presumably Gross and Witten still haven't given up, but are unlikely to take to posting on a newsgroup).

Last year as a joke I was telling people that the way things would end up is that string theorists would simply declare victory, announcing that string theory was the final TOE even though it was inherently incapable of calculating anything. This year this doesn't seem to be a joke.

7. **Thomas Larsson**
   April 9, 2004

   It seems that Wolfgang Lerche does not object to the AP per se. Rather, he complains that people who proposed the AP back in 1986/87 (himself??) did not get credit for it.

8. **D R Lunsford**
   April 7, 2004

   Heh, made me look. Without this blogodatum I would have probably avoided it.

   -drl

A Hole in Texas

April 12, 2004
Categories: Book Reviews

A short book review.

This past weekend my scientific activities included reading Herman Wouk's new novel "A Hole in Texas". The plot revolves around the story of the cancellation of the SSC and a supposed discovery of the Higgs Boson by a group of Chinese physicists. Wouk clearly did a lot of careful research and/or had some very competent advice since the technical and historical parts of the story are reasonably accurate.

Wouk has the US Congress and media getting tremendously excited over the Chinese Higgs discovery, leading to massive new funding for high energy physics, a charming but unlikely idea. In general the book is somewhat of a romance/wish fulfillment novel for older particle physics experimentalists. The protagonist, an experimentalist formerly involved with the SSC project, gets huge media attention, a lot of money and the use of a private jet, an old romance revived, a new romance with a beautiful Congresswoman who loves to listen to him explain physics, and funding for his current project.

Comments

1. **Chris Oakley**
   April 13, 2004

   Peter & Danny –

   I am very keen on theories which have a unique vacuum defined simply as the zero-particle state. As yet, I have seen no reason why I should abandon this requirement. Whether there is some value in doing mathematical tricks with Lagrangians that lead one to temporarily abandon this notion, I am not sure, but certainly the Higgs mechanism as it stands does not pass the test since, apart from anything else, the relevant manipulations only make sense for classical fields. However, I cannot help noticing that neither of you believe any more than I do that there are spin zero particles with mass tantalising out of reach of current experiments, just waiting to be discovered by the next generation of colliders. If, on the other hand, the required particles *are* found, then my personal view (note – not shared by anyone in the field that I know of who is prepared to be counted, anyway) is that it will set the subject back in that it will give all that dirty mathematics that led us there a new lease of life.

   By way of comparison, consider the W/Z boson discoveries in the eighties – a wealth of indirect evidence led us there. Unlike the Higgs particles now, even if one did not fully buy into the Glashow-Weinberg-Salam model, it would have been very surprising if the particles had *not* been found.

2. **Peter**
   April 13, 2004

   Hi Pyracantha,

   Actually the congresswoman was not just "beautiful", but an ex-Hollywood starlet. Wouk spends a lot of time describing how attractive the female protagonists of the book are; this is after all the fantasy of a 90 year old guy who grew up in the 1930s.

   On the other hand, the leader of the Chinese team that supposedly discovers the Higgs is also a woman, depicted both as extremely competent and of course as "beautiful".

3. **D R Lunsford**
   April 13, 2004

   Chris –

   In addition to Peter's explanation the following analogy may be helpful. If you want to calculate the motion of a ball bearing on a surface, you can either do it in three dimensions with a complicated constraint that forces the ball bearing to remain on the surface, or you can introduce a "surface force field" via Lagrange multipliers – in the Lagrangian for the problem the constraint appears like a effective force of undetermined origin. If we didn't know about the surface, we might be impelled to introduce just such a force to phenomenologically explain the motion of the ball bearing. Then we might discover the surface and say "aha! That phenomenology term is just the constraint in action!"

   Afsar Abbas showed that the Higgs can have arbitrary isospin and hypercharge and the SM still holds unchanged of experimental content. This is exactly the same state as with the above analogy – the normal force on the ball bearing is exactly what it needs to be to maintain the constraint – so the Higgs represents a sort of vacuum in itself, the referent to which the other elements must resort to have a meaning.

   http://arxiv.org/abs/hep-ph/9912243

   Peter's explanation in terms of Cooper pairs is perfect and right on – in the London theory there was simply the issue of a suddenly massive photon – in the BCS theory we now have a theoretical explanation of the pairing process that shows up as the apparent gain of photon mass. If the "real" theory of particles retains the Higgs and basis in Yang-Mills ideas, then it is very likely that the gauge invariances will be exact and the Higgs mechanism will have an encompassing explanation as with Cooper pairs. It could be said that this IS the real goal (or should be), of research in field theory.

   -drl

4. **Pyracantha**
   April 12, 2004

   yeah, women in book reviews and blurbs are always "beautiful." In fact, in this kind of writing, "beautiful" is code language for "female." Just like in real life.

   Beautifully,

   Pyracantha

5. **Peter**
   April 12, 2004

   Hi Chris,

One way to convince yourself there might be something to the Higgs mechanism is to study a non-relativistic analog: the Meissner effect in a superconductor. You can think about superconductivity as something that occurs when electrons can lower their energy by pairing together into “Cooper pairs”. Then one says that electrons have “condensed” in the lowest energy state, and low-energy excitations about this state are given by these pairs. There’s an effective complex scalar field theory describing how these excitations behave. In this theory the complex scalar field has a non-zero expectation value in the vacuum and so the U(1) symmetry of phase transformations of this field is spontaneously broken. If you now couple this theory to EM, i.e make it a U(1) gauge theory, you find that the photon field acquires a mass term.

All of this can be made sense of in a nonrelativistic QFT setting where infinities are completely under control, and describes real-world aspects of superconductivity that you can check experimentally. The relativistic, non-abelian, version of this used in the standard model is much trickier and one can argue about whether it makes sense rigorously. It also is bad news in that it introduces most of the undetermined parameters into the standard model. There are two well-known philosophies about this:

1. The standard model Higgs field is just an effective field for some kind of more basic fermion fields that pair together something like Cooper pairs. “Technicolor” is one implementation of this idea. There are various problems with it.

2. The standard model Higgs field is a superpartner of some fermionic field in a supersymmetric QFT. Supersymmetry helps with some of the divergences of pure scalar field theory, but this idea also has its problems.

Quite possibly a new idea about this is needed, maybe the LHC will give us a clue. But the success of the standard model means that something is causing spontaneous symmetry breaking of the electroweak gauge symmetry, we just don’t know exactly what it is yet.

6. **Chris Oakley**
April 12, 2004

Excuse me: missing the words “universe” and “a”. See if you can guess where.

7. **Chris Oakley**
April 12, 2004

Although neither beautiful nor a congresswoman, I would not mind someone explaining the Higgs mechanism to me, especially if they feel confident of persuading me that it is not the one of most contrived arguments that anyone has ever taken seriously (other than, of course, the notion that the is 10 or 11 dimensional entity composed of tiny vibrating strings).

Are Fermion Masses Like Planetary Orbits?

April 14, 2004
Categories: Uncategorized

A more and more common argument one hears from string theorists these days (for one version see a recent anonymous comment posted here) goes more or less like this:

"A fundamental theory shouldn't be expected to predict things like fermion masses or the standard model gauge group anymore than it should be able to predict the physical properties of the planets. Anyone who expects this is making the same mistake as Kepler, who tried to relate Platonic solids to planet orbits."

The idea here is that many or even all of the things we don't understand about the standard model are not fundamental aspects of the theory we should expect to be able to predict. Perhaps they are determined by the details of the history of how we ended up in this particular time and place, just as the properties of the planets were determined by the detailed history of the formation of the solar system.

As far as we can tell, the properties of the standard model hold uniformly throughout the observable universe, so to adopt this point of view one needs to postulate the existence of an unobservable "multiverse" of which we see only one small part. The so-called "landscape" of an unimaginably large number of possible vacuum solutions for string theory provides one realization of such a multiverse.

What are the problems with this idea? First of all, it is not so easy to dismiss out of hand. One can certainly imagine the possibility of the existence of an M-theory (maybe now the "M" is for "Multiverse") with a local vacuum state that corresponds to our universe, and some dynamics that allows evolution from one universe to another. Perhaps tomorrow night a preprint will appear on arxiv.org containing a simple equation expressing a dynamics such that the possibility of a universe exactly like ours does arise as some part of a solution. Should we believe in such a new theory, whatever it is?

There seem to me to be two possible cases in which such a theory would be compelling. The first would be if the theory made experimentally testable predictions. Perhaps it would have only one solution that agreed completely with current experimental observations. Then the properties of this solution could be used to predict the results of experiments not yet done. If these predictions were accurate, the theory would have strong evidence in its favor.

Even if the theory had so many solutions that one couldn't readily use it to make predictions, one still might find it compelling due to its "beauty" or "elegance". If it were based on a very simple equation or idea, the fact that the relatively complex structure of the standard model could be made to fall out of a much simpler equation would again be strong evidence for such a theory. Just how compelling this would be would depend on how much simpler it was than the standard model. If the new equation was more or less as complicated as the equations which determine the standard model, it wouldn't be compelling at all.

The current state of affairs in particle theory is that many people believe that they are on the road to finding such a compelling theory, but all the evidence is that this is nothing but wishful thinking on their part. There is no viable proposal for an M-theory based on a simple set of equations with a solution corresponding to the real world. This simply does not exist. An easy way to embarass a string theorist who is going on about the beauty of the theory is to ask them to write down a simple set of equations that characterize this beautiful theory. They can't do it now and I don't see any reason to believe they ever will be able to in the future.

What string theorists have now is not a single, consistent theory, but a set of several inconsistent fragmentary theories that they hope can be turned into a consistent whole. This circle of ideas is significantly more complicated than the standard model that it is trying to explain.

Even this complex of ideas might be compelling if it could be used to explain one or more not yet understood aspects of the standard model, or if it made new experimental predictions that could be checked. All the evidence of recent years is that this is impossible. If the whole framework makes any sense at all, it appears to predict nothing and explain nothing about the standard model. Not a single one of the parameters of the standard model can be calculated, not a single experimental prediction, at any energy scale, can be made. It is becoming increasingly clear that the circle of ideas known as "M-theory" is completely vacuous.

Strong evidence that this is the case comes from the fact that string theorists have no idea what, if anything, M-theory is supposed to be able to predict. Polchinski and others feel they have demonstrated that M-theory can't predict the cosmological constant, but can't come up with anything else it can predict and, increasingly, seem happy to live with the idea of promoting a theory that can't predict anything. This wholesale abandonment of the scientific method upsets some physicists such as David Gross quite a bit, but more and more people seem to have no problem with this. Frankly I find this all bizarre, disturbing, and becoming ever more so all the time.

Comments

1.
   April 27, 2004

   Er… empirical Bode's law fits the planetery orbits, isn't it. And I have heard some good attacks to try to derive the orbits from chaotical dynamics, particularly at the moment of condensation of the planets.

2. **Peter**
   April 19, 2004

   I'm afraid I'm really the wrong person to ask the questions you're asking, since these aren't the kinds of alternative ideas I've ever tried to pursue. They are close enough to string/M-theory that you are more likely to get good answers from a string/M-theorist.

   I don't think any of these things have rigorous no-go theorems. The closest may be the higher-spin problems, where I think what happens is if you look at the structure of the poles of the propagator, you can see that there's a problem with finding a prescription for dealing with them in a way that maintains locality and causality. I've never looked at this carefully, but if this is the heart of the problem, there may be a rigorous argument possible.

   For dealing with the conformal factor, a lot of people have worked on this, often in the context of trying to get strings to work in 4d. You really need to find an expert on this, not me.

   Similarly with theories of quantized membranes. A lot of people have tried hard to do this and they are the ones who would know what the fundamental problems are.

3. **gut**
   April 19, 2004

   For the cases of massless spin > 2 particles in interacting quantum theories, is there a rigorous theorem that says these theories don't exist? Or is it mostly "circumstantial evidence" of repeated failures in attempts to construct these particular interacting theories? (ie. we don't know what to do, so we just ignore the problem?)

   Can the same be said about d > 11 brane theories with respect to (conformal) anomaly cancellations?

(ie. “rigorous theorem” vs. “we don’t know what to do, so we just ignore the problem”?)

Has anyone ever produced a quantized membrane or p-brane theory that at least has a perturbative Feynman diagram type of expansion (with or without renormalization)?

4. **Peter**
   April 19, 2004

   Superstring theory is in d=10 because that’s the dimension in which the conformal anomaly cancels. For higher d, you’ll need to figure out some way of dealing with the conformal degree of freedom. M-theory conjecturally handles this in 11d, I don’t think anyone knows what to do for d>11. The dimension of your branes is limited by the space-time dimension.

   Interacting QFTs with spin >2 have problems with unitarity and causality that no one knows how to resolve. This limits the number of supersymmetries you can have.

   Nothing sensible I can say about the other questions.

5. **gut**
   April 19, 2004

   I have a question.

   Why stop at 11 dimensional M-theory or 10 dimensional superstring theory?

   Why don’t we have interacting quantum theories with massless particles with spin 3, spin 4, spin 66, spin 500, spin 9678900, spin aleph_0, and higher?

   Why don’t we have quantum theories of 30-branes, 32-branes, or for that matter 100-branes, 210-branes, 10024-branes, and higher?

   Why don’t we have field theories with N=43, N=200, N=1000, N=12345 supersymmetries, and higher?

   Why don’t lie algebras with 3-grading, 5-grading, 1001-grading, 665543-grading, and higher show up as a more “general supersymmetry”?

   Why do we only count dimensions by positive integers? Why don’t we have a non-integer number “dimensions”, such as 0.4444 dimensions, log(500) dimensions, pi^300 dimensions, -34 dimensions, -log(35353522) dimensions, etc …?

   Why don’t we have aleph_1, aleph_4, aleph_700, aleph_34588345, etc … being used in the number of dimensions, number of supersymmetries, lie algebra grading, or aleph_34 branes, massless spin aleph_2 particles, etc …?

   Why would a “unified field theory” reject such objects mentioned above?

6. **Peter**
   April 19, 2004

   Note that I didn’t say this was what string theorists as a whole think, I said it was an argument one hears from string theorists more and more often. For other examples of this, search the last year or so of sci.physics.research to find Robert Helling arguing that expecting string theory to predict standard model parameters is like expecting a fundamental theory to predict the properties of screws. I also recall Urs Schreiber making the analogy with predicting his phone number. Here’s a quote from Lenny Susskind:

   “Physicists always wanted to believe that the answer was unique. Somehow there was something very special about the answer, but the myth of uniqueness is one that I think is a fool’s errand. That is, some believe that there is some very fundamental, powerful, simple theory which, when you understand it and solve its equations, will uniquely determine what the electron mass is, what the proton mass is, and what all the constants of nature are…. On the other hand you could have a theory which permitted many different environments, and a theory which permitted many different environments would be one in which you would expect that it would vary from place to place. What we’ve discovered in the last several years is that string theory has an incredible diversity?a tremendous number of solutions?and allows different kinds of environments. A lot of the practitioners of this kind of mathematical theory have been in a state of denial about it. They didn’t want to recognize it. They want to believe the universe is an elegant universe?and it’s not so elegant. It’s different over here. It’s that over here. It’s a Rube Goldberg machine over here. And this has created a sort of sense of denial about the facts about the theory. The theory is going to win, and physicists who are trying to deny what’s going on are going to lose.”

   As for consistency and beauty, all the things you mention either are completely inconsistent with reality (four large dimensions and the standard model)

   N=1 supergravity in 11 dimensions, known facts about matrix theory, N=2 and N=4 supersymmetric vacua

   or completely ill-defined

   M-theory, conjectural matrix theories with better properties

   N=1 supergravity in 11d may or may not be beautiful, but it doesn’t look at all like the real world.

   I never said that there aren’t people who think that “low-energy N=1 supersymmetry might be a prediction of string theory”. But even though many if not most string theorists wish this were a prediction of string theory, it isn’t. It’s a nonsensical abuse of language to say that something “might” be a prediction. To make a prediction is to claim that something “will” be true, not that it “might” be true.

   String theory has failed utterly as a unification idea. It has no consistent version that can give 4d and the standard model and it makes absolutely no predictions whatsoever. If most of the energy of the particle theory community hadn’t gone into string theory for the past 20 years, we might have some good alternatives now.

7. **Thomas Dent**
   April 19, 2004

   An anonymous comment is good evidence for what string theorists as a whole think? At least, try to find someone speaking ‘on the record’. Otherwise we will be at the level of letters that begin “As a lifelong Democrat…”.

   Please tell us how string theory (or theories) is or are “inconsistent” as you claim. On the contrary, it is believed that many perturbative N=4 and N=2 supersymmetric vacua are theoretically perfectly consistent. I don’t think you have any counterargument to this. Perhaps you mean that more recent developments are inconsistent. But in what way?

   Some people (in Santa Cruz, Michigan and other places) think that low-energy N=1 supersymmetry might be a prediction of string theory. You could at least acknowledge that such people exist.

Do you not think that N=1 supergravity in 11 dimensions is a beautiful theory? And this is what M-theory is in some limit. There are lots of other beautiful things about, for example matrix theory. Of course, they take a little time to explain.

What is the alternative to string theory? Because nothing else could possibly “predict” the Standard Model parameters either, in the strong sense that you want.

In QFT we have GUT’s and other symmetric constructions, that build on our prejudice for simplicity and unification, but strictly, field theory has no predictions, since there might be a field theory that is not unified but has values of the SM parameters very close to GUT values: there is a continuous infinity of equally consistent QFT’s in flat space.

But, of course, no field theory is completely consistent as a description of the world. If we are not to do string theory, what can we do to make QFT work with gravity? After all, people didn’t go into string theory for no reason at all.

8. **gut**
   April 18, 2004

   A few very telling lines on page 87 of the postscript version, or on the web version

   http://relativity.livingreviews.org/Articles/lrr-2004-2/articlesu15.html

   Another confession of Pauli?s went to Paul Ehrenfest:

   ?By the way, I now no longer believe in one syllable of teleparallelism; Einstein seems to have been abandoned by the dear Lord.? 195 (Pauli to Ehrenfest 29 September 1929; [250], p. 524)

   Pauli?s remark shows the importance of ideology in this field: As long as no empirical basis exists, beliefs, hopes, expectations, and rationally guided guesses abound.

9. **D R Lunsford**
   April 18, 2004

   gut (?) –

   Seems like a very fine review, and the list of references is comprehensive, but oddly misses the main point for modern work, namely:

   The unification of Kaluza is non-dynamical. This is most easily seen by dimensional analysis. Mathematically, the “leftovers” in bottom corner of the extended metric must be constrained in an artificial, purely formal way that forces the n5.. components in the metric to mimic the required behavior. The vacuum problem of string theory is the price paid. That this is the death blow to Kaluza’s ansatz was already known by Pauli ages ago. I can’t see how anything has changed since then. The “cylinder condition” is never really lifted, only explained as projective differential geometry in a particular representation. The lack of a cogent variational principle without essential arbitrariness is further evidence for the ad-hoc nature of the ideas.

   Given the leading role of string theory, I find it strange that so little is said about the basic nature of the Kaluza ansatz. It would be interesting to understand how Kaluza’s theory became popular again after having so completely failed as physics and for so simple a reason. I have no idea how this happened. One should emphasise the marked contrast to Weyl’s ansatz, which is dynamical from the outset, because of the fundamental role of Am in the geometry. The persistent hints that masses are associated with conformal scaling should inspire more interest in this idea, but in spite of all efforts this seems unlikely to happen.

   In the newsgroup the issue of a theory of trivectors (three-forms) came up. I’ve actually worked this out and, as an illustration of the non-essential nature of the Kaluza ansatz, can exhibit it in “unified” form with the 4-d metric. The potential is a bivector and one need only form symmetric objects from it – these are easy to find (think of the energy tensor for electrodynamics). There is a gauge invariance that gets mimicked under the cylinder condition in exactly the same way the gauge invariance of the vector potential does in the original. Is it a UFT? Of course not. The metric is what it is, and the gauge potentials are what they are. The “twain may meet”, but only in a more comprehensive geometry than Riemann’s. Attempting to make the gauge potentials part of the metric itself is physical folly.

   I was pleased to see the work of V.A. Fock acknowledged – he was the first person to take seriously the role of the Dirac gammas as geometrical objects (frame basis). I once owned a galley proof of the paper “L’Equation d’Onde de Dirac et la Geometrie de Riemann” autographed by Born, from his own personal effects (a friend at Maryland had found it on rummaging through some discards of the Physics dept.) It got lost in a move and could not be recovered 🙁 IMO this work is still interesting in the context of Weyl’s ideas because gamma_5 suddenly acquires an essentially dynamical role.

10. **gut**
    April 16, 2004

    Are you referring to this article:

    http://relativity.livingreviews.org/Articles/lrr-2004-2/index.html

    “On the History of Unified Field Theories”
    by Hubert F.M. Goenner

    ????

11. **D R Lunsford**
    April 16, 2004

    Peter –

    The situation is very reminiscent of the deluge of “unified field theories” from 1930-50 from Einstein, Schroedinger, and Eddington. Unlike Weyl’s theory which has a simple, cogent principle at base (“pure infinitesimal geometry”), none of these others – “fernparallismus”, asymmetric metric, asymmetric connection, “lambda” invariance – had anything other than a purely formal standing and they are all reducible (no unique vacuum). Because the times were ripe for experimentalists, these theories were rightly forgotten or held up as examples of what not to do.

    Included among these theoretical husks was of course the Kaluza theory, including Klein’s modifications – because like the others, it was based on a purely formal principle without any direct connection to physics.

    That an already rejected, dead theory would be taken so seriously is a sign of either theoretical desperation, or a semi-conscious effort to prop up a publication and funding bonanza.

12. **Peter**

April 15, 2004

I'm actually agnostic on the "univac" vs. "multivac" issue, since my best guess is that the whole set-up in which this discussion is taking place isn't such that one can get well-defined answers to questions like this.

What I was writing about was something different. I'd always assumed that all serious theorists would agree that if the "multivac" scenario is correct and there really are so many vacua that you can't use string/M theory to predict any of the parameters of the standard model, then you would have to abandon the theory as being useless and not explaining anything. David Gross clearly recognizes what a disaster for string theory the "multivac" scenario is, and I assumed his was the majority view these days. I seem to have annoyed Jacques Distler by thinking he was in Gross's camp. Now I have no idea whether he is "univac" or "multivac" and don't want to get in trouble by speculating about this.

It has amazed me recently to see how many string theorists are willing to argue the case that even if the theory can't explain anything about the standard model, this is not a disaster. They seem to intend to continue believing in string/M-theory unification, even when convinced that there is no way to use it to make predictions or explain any known aspects of the universe. As far as I can tell they are willing to abandon the scientific method in order to keep defending string theory.

Perhaps I'm misrepresenting people's views, but my evidence for this includes the following:

1. The anonymous string theorist here who argued that the standard model parameters and gauge groups are no more likely to be predictable from fundamental theory than planetary orbits. You can find the same argument made in many places these days by many people.

2. Srednicki's claim here that even if string theory could not explain anything about the standard model, if it were consistent it would be a "striking success".

3. Polchinski, Kachru and others uniformly refuse to answer a question I keep trying to put to them: in the "multivac" scenario, name something that you expect to be able to predict. The fact that they don't answer this question seems to me strong evidence that the answer is that they retain the wishful thinking that something, someday may be predictable, but have no idea what it may be.

13. **serenus zeitblom**
April 15, 2004

Peter, you should look at http://arxiv.org/abs/hep-th/0404075
It's clear that the claim that string
theory can produce "anything" just isn't
true. In particular, it is very far from
clear that it can produce a deSitter
background. The KKLT proposal essentially
suggested that you could get dS out of string
theory if you were willing to tolerate extremely
contrived models involving known gadgets such
as fluxes. Now it seems likely that even
this is not true. NOTE that I am not saying
that it can't be done — nobody knows this
yet. What we do know now is that getting dS
out of string theory is going to be very hard.
In view of this, it seems rather odd that people
are worrying about a vast landscape when, as
Vijay B reminds us, we do not have even *one* credible dS background for string theory! Perhaps *this*
is *what* Jacques *Distler* was *going* to
explain, no *doubt* with a *superfluity* of
italics, in his *blog*, which *unfortunately*
is *mainly* about *itself* these *days*……

Censorship

April 16, 2004
Categories: Uncategorized

It seems that the second most important web-site in the particle theory community (the first is obviously the arXiv) has been shut down by the University of Washington. The Theoretical Particle Physics Jobs Rumor Mill has for years been a comprehensive source of information about who's hot, who's not, the hiring plans of all the theoretical particle physics groups in the United States and Canada, and the career moves of more established theorists. I hope a new home for the site can be found at a less fearful institution. I'd consider putting it up here, but I've got enough particle theorists annoyed at me already…

Comments

1. **Peter**
   April 19, 2004

   At least in the US, academic freedom traditionally does mean that academic staff have the freedom to use the university resources normally made available to them to disseminate information they choose about their scholarly field.

   That said, I suppose one could argue that the sort of information being disseminated there was not the sort that traditionally is protected by guarantees of academic freedom. I have no idea who at UW was running this web-site or why the department there insisted that it be closed down.

2. **Thomas Dent**
   April 19, 2004

   Actually, the person responsible for the Rumor Mill page is perfectly free to pay for his own private website hosting and display the exact same information in his free time.

   I don't know exactly why U-W physics no longer wanted that webpage to be hosted on its server, but it might have something to do with a contract between the faculty member and the university concerning proper use of the physics department webspace.

   Freedom of speech doesn't mean freedom to put your speech on a website paid for by another person or institution.

HEPAP

April 22, 2004
Categories: Uncategorized

HEPAP is the Department of Energy's "High Energy Physics Advisory Panel", which holds meetings 3-4 times a year. At these meetings, people from the DOE and NSF report on the latest news about US government funding for particle physcs, and physicists from the universities and national labs report on how their experiments are going.

The latest HEPAP meeting was held this past weekend in Washington, and some of the presentations there have already been made available online. These include a detailed report on the progress of the LHC and the two experiments (CMS and Atlas) that will do physics there. The LHC construction is 90% complete, with magnets beginning to be installed in the tunnel. Things seem to be on track for turning on the machine in the spring of 2007. Optimistically, this would mean the first physics results should be available sometime in 2008.

Another presentation gave an overview of the DOE's support of university-based particle physics. This includes the largest source of support in the US for theoretical particle physics. In FY 2003 the DOE spent \$23.3 million supporting theory research at 68 universities, funding 215 faculty, 116 postdocs and 114 graduate students. The other main source of US funding for particle theory is the NSF, which spends about half as much as the DOE (\$12 million in FY 2003).

HEPAP also adopted a new report on the "Quantum Universe". This follows the trend of recent years of trying to justify particle physics research by emphasizing its relation to the very healthy and sexy field of cosmology. The acid test of this over the next few years will be to see if it helps with the difficult problem of getting funding for the Linear Collider.

No Comments

Rumors Available Again

April 23, 2004
Categories: Uncategorized

The Theoretical Particle Physics Jobs Rumor Mill has a new home. It's no longer at the University of Washington, now it's at the College of William and Mary Physics department.

Now that it's available again, the Rumor Mill has the striking news that Harvard has chosen for a faculty position one of its postdocs: Lubos Motl. Lubos is well-known as undoubtedly the most rabidly fanatic string theorist around, always willing to heap abuse and scorn on anyone who questions the idea that string theory is the language in which God wrote the world. Unlike many string theorists though, he actually knows what is going on in the field and is someone who can give you an accurate view of exactly what the state of the theory is (all you have to do is strip out his ravings about how string theory is unique and the source of all good ideas in physics and mathematics). He's also not foolish enough to swallow the "Anthropic" nonsense that is becoming ever more prevalent among string theorists, and it's a been a bit scary recently to see him acting as the voice of reason in the subject.

Comments

1. **Peter**
   April 28, 2004

   I think you may already be better informed about this than I am. I took a course from Georgi when he was still a junior faculty member (and sharing an office with Witten) and remember when he got tenure. Coleman had already been tenured for quite awhile when I got there and I didn't know he had started on the tenure-track. The others you mention got tenure after I shifted over into mathematics and wasn't paying much attention to exactly what was going on in the Harvard or Princeton physics departments.

2. **gut**
   April 28, 2004

   Besides Maldacena, were Vafa, Georgi, and Coleman the only particle theory folks (in recent times) who started off their careers as junior faculty at Harvard and eventually got tenure? Was Klebanov the only recent particle guy at Princeton who started off as a junior faculty at Princeton and eventually got tenure?

3. **Peter**
   April 28, 2004

   Since Harvard and Princeton generally feel that they can hire just about anyone they want directly at the senior level, they mostly don't tenure their junior people who are nominally on the tenure track. So, for those people the reason to take the job is not so much that you are likely to get tenure there, but you'll be in a very good place for quite a few years and in a good position to jump to a tenured job elsewhere. I remember that one of my fellow Princeton grad students, Jon Bagger, did this, going from a tenure-track position at Harvard to a tenured one at Johns Hopkins where he is now. It also is not unheard of for these places to end up promoting one of their junior people. But, while at many less august places the idea is that if tenure-track people do reasonably well, they can expect to get tenure, this isn't the case at Harvard or Princeton.

4. **gut**
   April 28, 2004

   Is there any truth to the folklore stories that assistant professor jobs at places like Harvard, Princeton, etc … aren't much more than the functional equivalent of an "extended postdoc" that lasts for 6 or 7 years? On the surface, Harvard doesn't seem to give tenure very often to folks who start off their careers there as an assistant professor.

5. **D R Lunsford**
   April 26, 2004

   Chris –

   The antropic principle has many forms. Here's one

   F = ma

   Why?

   Because I said so!

   Then there's the Dirac equation with "anthropic coupling" in psiberspace

   (ym dm + im ok you're ok) psi approx = 0

   Note that this eqn includes an explanation of "Me" generations.

   Of course there is a background-free version

   R_mu_nu_and_Tyler_2 = NULL;

   This one is easy to simulate on the computer.

   -drl

6. **Chris Oakley**
   April 26, 2004

   Am I to assume that the "Anthropic Principle" means that we postulate that "we are lucky winners of a Monte Carlo simulation in which every choice was tried and one survived?" [NB: borrowed words]. I would not mind clarification here.

7. **Thomas Larsson**
   April 25, 2004

   John Horgan wrote somewhere that Ed Witten was “the most naive ironic scientist I ever met”.
   He hadn’t met Lubos Motl.

   It is sad, really. Motl is undoubtedly very hard-working and technically skilled, perhaps even a genius. In another time, he could have used his brilliance to advance physics rather than to do the opposite.

8. **D R Lunsford**
   April 24, 2004

   Peter,

   You’re no fun 🙂

   I always imagined a place like Princeton would be just intellectual paradise. But, I’ve had friends who took PhDs at MIT, Princeton, etc. and they all seem glad to be away. I suppose in retrospect the situation at Ga. Tech in fact *was* a sort of paradise, with an unlimited copier budget and open stack library with complete series of all the major and minor journals back to inception – and a nice college bar/pizza joint about 2 blocks away and the beautiful city of Atlanta before it got too big.

9. **Peter**
   April 24, 2004

   I don’t have a lot of contact with the physics department here, although I know quite a few people over there and talk to them every so often. My knowledge of the short list for their latest job comes from the Rumor Mill. Their latest theory hire is not a string theorist.

   I first met Brian around the time he got his Ph.D. more than fifteen years ago, and interact with him regularly since he joined the department here nearly eight years ago. When he first heard about my anti-string activities a few years ago he offered me 10% of any bump he saw in sales of “The Elegant Universe”. I haven’t seen any cash.

   Math department faculty meetings are pretty boring, as well-run faculty meetings should be.

   Hmm, whoever you are, your IP number is very local. You should probably be stopping by Math 421 sometime to say hello so I can tell you the real dirt that could never be discussed in public.

10. **Tim**
    April 23, 2004

    Do you have any contact with the Columbia physics department? Regardless, has their hiring been exclusively string theorists?

    Also, what is your interaction with Brian Greene? Any interesting Math department faculty meetings?

The Good Old Days

April 26, 2004
Categories: Uncategorized

Alvaro de Rujula has posted on the arXiv under the title "Fifty years of Yang-Mills Theories: a phenomenological point of view" some of his recollections from the mid-seventies. These bring back my own memories of taking a course on particle theory from him at Harvard around 1977-78. One amusing aspect of the course was that when introducing a concept carrying someone's name, de Rujula would always say something like "this is the so-called Weinberg angle, which of course was discovered by Glashow". In one lecture he did something a bit different, saying something like "this is the Cabibbo angle, which, strangely enough, I think actually may have been discovered by Cabibbo". de Rujula's paper contains one of his famous drawings from the period and an amusing picture of Georgi and Glashow arguing. His asides are entertaining, but some so obscure I confess to not knowing exactly what he is referring to.

Today's arXiv postings also contain a review talk on the state of string theory. It discusses the "landscape" with the comment "However, with such large numbers of vacua involved, one must wonder whether the scheme is at all testable, even in principle." Normally string theory reviews start by describing the theory as the "only known" or "best candidate" or "most promising" approach to unification. This one replaces those phrases by "dominant framework", and one certainly can't argue with that.

Comments

1. **D R Lunsford**
   April 27, 2004

   Peter –

   Well the story is very interesting, and if we ever meet I'll relate it.

   IMO Zweig deserves a great deal of credit – for one thing he wasn't just thinking about symmetry, but suppressed reactions being explained by new conservation laws. "Aces" implies 4 things, so his idea foreshadowed charm before anyone had heard of strangeness. There is more to the story as told to me, which I would not feel comfortable relating here.

   There is the well-known story that Zweig didn't publish his ideas because that would have meant putting it in a CERN report instead of Physical Review. This recalls Stueckelberg's diagrams, which he also did not publish, based on the same idea as Feynman's (backward-in-time -E electron = positron).

   Of course there are many such "accidents" in the history of science.

   -drl

2. **Peter**
   April 27, 2004

   I've read various versions of the story of Zweig and his "aces", but don't know what story you are referring to.

   This is something though where I think Gell-Mann deserves the lion's credit. He (and independently Yuval Ne'eman) was the one to realize strongly interacting particles could be organized using SU(3) representations and got the prediction of the Omega-minus and its mass. I've heard that several people claim to have recognized that since you can construct all SU(3) reps out of tensor products of the fundamental 3-dim one, the existence of a fundamental triplet was a natural conjecture. The problem was that you quickly found the charges to be non-integral, disagreeing with experiment. So I think SU(3) was worth a Nobel prize, but after that postulating the existence of the triplet was not such a big deal.

   Gell-Mann was involved in another similar story, the V-A theory of weak interactions, where you'll find a whole bunch of people claiming credit for the idea around the same time.

3. **D R Lunsford**
   April 27, 2004

   In school one of my professors who was present at Caltech told me a certain story about Gell-Mann and Zweig and the issue of credit. Anyone heard it? I won't repeat it here.

   I am happy to see that these days, Zweig gets shared credit.

   -drl

4. **Peter**
   April 27, 2004

   Actually according to de Rujula, Gell-Mann claims to have discovered the Cabibbo angle, in an obscure footnote. Thus his reference to it as the "funny angle" instead of "Cabibbo angle". I also remember from long ago Howard Georgi being very amused by a Harvard secretary's typo in one of his papers. The Cabibbo "angle" had become the Cabibbo "angel".

   "The Second Creation" by Crease and Mann is by far the best book I know of about the standard model and its history, and I recommend it enthusiastically to anyone learning about particle physics.

5. **Thomas Larsson**
   April 27, 2004

   Re Cabibbo angle. I must confess that I bought Veltman's book mainly because of the last paragraph – as you can see at the Amazon.com review, he also uses the phrase "not even wrong" about string theory. However, the book contains a lot of amusing stuff. Apparently Gell-Mann insisted on calling it "that funny angle", so at one conference Cabibbo wrote "Funny Cabibbo" on his name-tag.

6. **Alejandro Rivero**
   April 27, 2004

Alvaro is a sort of “show bussiness” by himself. I remember him to actually explain the popular “turtles all way down” joke, in a speech to undergraduate/secondary school teachers, adscribing it to himself.

As for Cabibbo angle, a pesky Spanish nuclear physics teacher, recently deceased, is said to have tried to claim discovery. I do not know if Rujula was aware of it, but it could be.

7. **Chris Oakley**
   April 27, 2004

   The story of the J/Psi and charm is well told in “The Second Creation” by Robert Crease & Charles Mann. Their account is accessible to non-specialists and is as gripping as a good novel.

8. **Peter**
   April 27, 2004

   I’m sure just about all theorists are very disappointed that there hasn’t been much of an active interchange between theorists and experimentalists since the 70s. de Rujula himself has pretty much left particle physics and is doing astronomy.

   The underlying problems are that the standard model is just too good and progress on getting higher energy accelerators has been slow. If the SSC had been constructed, maybe we would already be well into a new period of exciting experimental results.

9. **D R Lunsford**
   April 26, 2004

   Peter –

   That’s a wonderful read. What exciting times! I was just learning calculus “ex curriculis” in those days – I remember some NOVA programs on the Standard Model, and the interviews with Feynman in particular. I can’t remember how I first learned about all this stuff in detail, but I know it was much later – I was really busy working through Sommerfeld’s lectures and deliberately avoided new results that I would not understand yet. In retrospect, the early to mid 80s was a great time in physics history to be in school – funny it didn’t seem that way at the time…

   The whole paper is about what got calculated and why. From this day’s perspective – how quaint 🙂

   Is there any disappointment that the theory is still in the post-phenomenology phase? Are people surprised that not a great deal more progress has been made since W/Z?

   -drl

The Fundamental Lemma

April 27, 2004
Categories: Uncategorized

For quite a few years now, when I ask my colleague Herve Jacquet about what is going on in his field, he tells me something like: "Maybe someone will soon be able to prove the Fundamental Lemma". This is a bit of a joke since the terminology "lemma" is supposed to refer to an easy to prove, simple technical result needed on the way to proving a real theorem. In this case the name "Fundamental Lemma" has ended up getting attached to a crucial conjecture that is part of the so-called "Langlands Program", and this conjecture has resisted all attempts to prove it for more than twenty years.

A few weeks ago Jacquet told me that he had heard that two French mathematicians, Gerard Laumon and Bao Chau Ngo, finally had a proof and today a manuscript has appeared on the arXiv. The techniques it uses are way beyond me (and even Jacquet claims he doesn't understand them), but are related to those used in the so-called "Geometric Langlands Program" that has interesting relations to conformal field theory.

I won't embarrass myself by trying to explain in any detail the little that I know about this kind of mathematics, but in extremely vague terms the Langlands Program relates representations of the Galois group (which tell one about the number of solutions of arithmetic problems) to representations of algebraic groups like the general linear group. One example of this kind of thing is the Taniyama-Shimura-Weil conjecture that was proved by Wiles and implies Fermat's Last Theorem. One way of approaching the Langlands program uses generalizations of the Selberg trace formula, and the lack of a proof of the fundamental lemma has evidently been the main obstruction to getting all that one would like out of the trace formula methods. Maybe someday I'll understand some of this enough to try and write something more, but that will probably take quite a while. In the meantime, one of the few expository papers I've found about the fundamental lemma is here.

Comments

1. **Peter**
   April 27, 2004

   Right now Fermat already is just a corollary of Taniyama-Shimura-Weil (as proved by Wiles-Taylor). It is quite possible that at some point Taniyama-Shimura-Weil will be a corollary of a very general theorem coming out of the Langlands program. However, this seems very far off at this point.

2. **Dick Thompson**
   April 27, 2004

   Would this mean the Fermat theorem would be reduced to a minor corollary? That's my uniform expectation about all these famous problems; they are just special cases in temporary lack of an enveloping general theory.

What Is Thought?

April 30, 2004
Categories: Book Reviews

Eric Baum was a fellow physics student both at Harvard and Princeton, completing his Ph.D. in the early 1980s on a topic in quantum gravity. During his years as a physics postdoc he came up with an argument for why the cosmological constant is so small that is sometimes referred to as the "Hawking-Baum" argument. He finally left physics, joining NEC Research in Princeton to work in cognitive science.

Eric has a new book out from MIT Press called "What Is Thought", and you can read a review by Witten on the Amazon web-site. There's also a web-site for the book.

His point of view on cognitive science is very much that of a physicist, emphasizing the way the brain encodes a very compact understanding of how the world works that has been made possible by the huge amount of computation and experiment that has taken place during the evolution of the human organism. One thing that most impressed me about the book is the underlying theme that he refers to as his version of Occam's razor and summarizes as follows:

"mind is a complex but still compact program that captures and exploits the underlying compact structure of the world."

To understand something about the world is to capture its features in a compact subroutine that allows one to effectively interact with it. This is clearly related to what theoretical physicists mean when they discuss the "beauty" or "elegance" of the fundamental equations and concepts that they are exploiting. So, if you have an interest in cognitive science, and enough interest in physics to be reading this weblog, I recommend heartily that you find yourself a copy of Eric's book.

Comments

1. **Peter**
   May 8, 2004

   My comment involved a different kind of "compactness", not the compactness of the supposed extra dimensions. A description of nature is a "compact" one if it can be expressed simply in terms of a small number of fundamental objects or ideas. A small number of simple equations, if you like. String theory is a very complex business and its proponents hope that it can be reduced to some simple principle or equation, but there is no evidence that this is the case.

2. **sol**
   May 8, 2004

   Peter,

   http://www.tech.port.ac.uk/staffweb/seahras/images/compact.jpg

   "The are several different ways in which one can "roll-up" a non compact manifold, like the one on the left, into a compact space, like the other surfaces"

   http://www.tech.port.ac.uk/staffweb/seahras/neat_physics/extra_dimensions/fifth.htm

   I looked at number 2 of your response here and what you thought of string theory as not having this compaction principle?

   What is your response to the above?

3. **Peter**
   May 5, 2004

   I guess I think of the mathematical formalisms of theoretical physics as more powerful extensions of the sort of understanding of the world built into our minds that Eric is writing about. As we try and improve them, the criteria should be:
   1. Do they agree with the real world, as our minds have managed to understand it?
   2. Are they compact, since to understand something new seems to mean to find a compact representation of it.?

   For the three theories you mention, there's still no compact form of them that agrees with the world. String theory is in the middle of desperately trying to get agreement with the world by abandoning the requirement of compactness, and adopting a huge, nearly infinitely complex theory.

4. **Sol**
   May 5, 2004

   "To understand something about the world is to capture its features in a compact subroutine that allows one to effectively interact with it. This is clearly related to what theoretical physicists mean when they discuss the "beauty" or "elegance" of the fundamental equations and concepts that they are exploiting. So, if you have an interest in cognitive science, and enough interest in physics to be reading this weblog, I recommend heartily that you find yourself a copy of Eric's book."

   As I mentioned on another site, I find the need here to understand how such mathematical discriptions are born in mind to demonstrate the natural expressions that emerge from describing that nature.

   This has been the quest as far as I understood it, from a student perspective. If one looks to Penrose or Smolin, there is always this need to explain the world from a basis of logical undertanding. So it first startsfrom a philospohcal discussion and then moves from this basis of logic, into the forms and requirments of the new math?

   Examples here would have been Twistor Theory, Loop Quantum Gravity, or String Theory.

   What are your thoughts here?

The Kostant Dirac Operator

May 8, 2004
Categories: Uncategorized

Quantum mechanics and representation theory are very closely linked subjects since the Hilbert space of a quantum system with symmetry group G carries a unitary representation of G. To the extent that one has a way of quantizing a classical Hamiltonian system with G-symmetry, one has a way of constructing representations of G out of symplectic manifolds with G-action. This "geometric quantization" approach to constructing representations has been a very fruitful one.

For the case of G compact, connected, with maximal torus T (the crucial example to keep in mind is G=SU(2), T=U(1)), the "flag manifold" G/T (the 2-sphere for G=SU(2)) is a symplectic manifold (actually Kahler) and can be thought of as a classical phase space with G-symmetry. Choosing a representation of T (a "weight") allows one to construct a line bundle over G/T, which turns out to be holomorphic. The Borel-Weil theorem says that irreducible G-representations are given by holomorphic sections of this line bundle, for "dominant" weights.

For weights that are not dominant, one gets not holomorphic sections, but elements in higher cohomology groups. These can be expressed either in terms of the sheaf cohomology of G/T with coefficients in the sheaf of holomorphic sections of the line bundle, or in terms of Lie algebra cohomology. This is known as the Borel-Weil-Bott theorem, which first appeared in:

Bott, R., Homogeneous Vector Bundles, Ann. of Math. 66 (1967) 203.

the Lie algebra version was further developed by Kostant in

Kostant, B., Lie Algebra Cohomology and the Generalized Borel-Weil Theorem, Ann. of Math. 74 (1961) 329.

Instead of using complex manifold methods and the Dolbeaut operator to construct cohomology classes, one can use spinors and the Dirac operator, with the representation appearing as the kernel of the Dirac operator (or, more accurately, its index). For this point of view, which fits in beautifully with equivariant K-theory and the index theorem, see:

Bott, R., The Index Theorem for Homogeneous Differential Operators, in: Differential and Combinatorial Topology: A Symposium in honor of Marston Morse, Princeton (1964) 71.

The Dirac operator approach to representation theory has been extended to some cases of G non-compact by various authors. In the last few years, Kostant has come up with a new version of the Dirac operator in this context which has quite interesting properties. He likes to work algebraically, so his Dirac operator on G is given as an element of U(Lie G)XCliff(Lie G), where U(g) is the universal enveloping algebra of the Lie algebra Lie G and Cliff(Lie G) is the Clifford algebra of Lie G. The Kostant Dirac operator is the standard one you would expect, with the addition of an extra cubic term. For the details of all this, see Kostant's paper:

Kostant, B. , A Cubic Dirac Operator and the Emergence of Euler Number Multiplets of Representations for Equal Rank Subgroups, Duke Math. J. 100 (1999) 447.

Things get interesting when you consider the case of H a subgroup of G of the same rank (one example is H=T, another important one is G=S0(2n+1), H=SO(2n), where G/H is an even-dimensional sphere). Taking the difference of Kostant Dirac operators for G and H gives something that corresponds to a Dirac operator on G/H, which acts on the product of a G rep with the spinors associated to Cliff (Lie G/Lie H). For H=T, one gets back the old Bott-Kostant construction of representations, but with the Lie algebra cohomology replace by the index of a Dirac operator.

Part of this story is that one finds that, starting with an irreducible G-representation, the kernel of Kostant's Dirac operator consists of a "multiplet" of H representations of size given by the Euler characteristic of G/H. The existence of these multipliets was first noticed by Ramond for the case H=SO(9), where SO(9) is the massless little group in 11 dimensions and the multiplets appear in the massless spectrum of N=1 11d-supergravity (the low energy limit of a conjectural M-theory). These SO(9) multiplets come about because SO(9) is an equal rank subgroup of the exceptional group F4, so for each irreducible F4 representation one gets a multiplet of SO(9) representations.

The first paper about this was by Gross, Kostant, Ramond and Sternberg, for more about this from a geometrical point of view, see a paper by Greg Landweber. For a discussion of the relation of this to supersymmetric models in physics, look up recent preprints by Pierre Ramond, one of which is by Brink and Ramond.

Greg Landweber has applied these ideas to loop groups, getting a beautiful interpretation in terms of loop group representation theory of certain N=2 superconformal models first studied by Kazama and Suzuki in 1989. This paper also contains a detailed exposition of the story both for finite dimensional groups and loop groups.

More recently, Freed, Hopkins and Teleman have used a modified version of the Kostant Dirac operator to give a proof of their theorem relating the Verlinde algebra and twisted K-theory. Their construction is quite beautiful and gives a new point of view on the whole story of the relation of geometric methods of quantization to K-theory and the index of Dirac operators. I'll try and write something about this at some later date.

Comments

1. **erinj**
   May 18, 2004

   JC,

   It's been a while since I looked at fundamental aspects of supersymmetry, and my own copies of the Coleman-Mandula and HLS papers are boxed away, but I could offer an answer to your question. (Though the last time I wrote about SUSY involving these papers was in my thesis, which I also boxed away, and I'm no longer an academic.)

   I believe – possibly incorrectly – that the answer is no, there have been no other ways to circumvent the Coleman-Mandula `No-Go Theorem'. I believe this is the case because:

   (a) I myself once pondered this question and either asked someone once or read up on it to provide an answer for myself, and

   (b) Doesn't the paper by HLS, or a follow-up paper by HLS, actually contain a proof that, on physical grounds (e.g. positive energy), there is no other way to circumvent the Coleman-Mandula theorem other than SUSY. That is, SUSY is the unique and only way to circumvent the constraints imposed on the S-Matrix by the Coleman-Mandula theorem?

   Please correct me if I am wrong, as I think my SUSY has become very rusty from lack of use.

   Erin

2. **JC**
May 14, 2004

Has anyone ever found any other loopholes around the Coleman-Mandula theorem, besides the Z_2 grading SUSY case covered by the Haag-Lopuszansky-Sohnius theorem?

Such as:

A*A*A ~= vector, where A is like a "one third vector"

or

B*B*B*B ~= vector, where B is like a "one quarter vector"

or in general,

Q^n ~= vector, where Q is like a fractional "one n'th vector"

3. **Peter**
May 13, 2004

I'd never heard of the "half vector" terminology, but it sounds plausible. People often say that spinors are "square roots" of vectors. More generally, given a vector space, the space of antisymmetric tensors can be identified with the spinor space times its dual.

I don't know of any way in which there would be more general fractional powers or fractions of vectors.

4. **JC**
May 13, 2004

Did Landau refer to a spinor as a "half vector"?

Is there any mathematical objects that could possibly be described as a "one third" or "one quarter" vector? Or for that matter any non-integer fractional vector?

5. **Peter**
May 12, 2004

I'm afraid the lack of much response to this post is due to it being rather obscure. I've been thinking about the Kostant Dirac operator, so wanted to write something about it, but to write something readable by a significant number of people would require giving a lot more detail. Maybe I'll try and do that at some later point.

The geometry of spinors is truly amazing, and all evidence is that it is more fundamental than the geometry of vectors and tensors. The Dirac operator is a fascinating thing, and Kostant's recent version of it is something that deserves to be better known. I hope at least some people will be inspired to look into some of this, I especially recommend the Landweber paper as being pretty readable.

6. **erinj**
May 12, 2004

Isn't it startling, Peter, how much attention a squabble over comments made in a string theory forum attracts, whereas your post on a fascinating and intriguing area of mathematics and mathematical physics seems to receive much less?

Admittedly your post is a rather technical one, but nonetheless it is illuminating. I myself find the applications and historical development of the Dirac operator continually amazing. Is there no end to its uses? No bound to its development?

Spinors and Dirac's operator seem to keep surfacing in surprising places, and always seem to simplify things when they do – as in the representation theory in your post, and in other examples such as Witten's proof (using spinors) of the positive mass theorem in General Relativity. I couldn't resist recalling Hermann Weyl's remark about spinors (and the orthogonal group) whilst reading your post:

"Only with spinors do we strike that level in the theory of representations on which Euclid himself, flourishing ruler and compass, so deftly moves in the realm of geometric figures."

It's wonderful, inspirational mathematics and mathematical physics! Now I certainly appreciate Konstant's work, which beforehand I was only very vaguely aware of, after reading your post.

The only downside, for me, upon reading your post, is not being able to access some of the papers you refer to in it, now that I have left academia :\

The Landscape at Davis

May 15, 2004
Categories: Uncategorized

I'm in Nothern California this week, and have been attending some of the talks at the conference at UC Davis celebrating Albert Schwarz's 70th birthday. The landscape at Davis is exceedingly flat, but this morning Lenny Susskind gave a remarkable talk with the title "Exploring the Landscape".

It was a pretty strange talk for a mathematical physics conference since it contained zero mathematics (and it's arguable whether there was any physics…). Susskind blamed Iz Singer for this, claiming that Singer told him he should talk about the landscape stuff since it was leading to a new mathematical field of "statistical topology". He began by holding up a copy of Steven Weinberg's "Dreams of a Final Theory" and reading a quote from it about the cosmological constant. He liked this so much he read the same quote a second time a little while later.

He then discussed some of the recent history of string theory, noting that for a long time string theorists were hoping for a mathematical silver bullet that would provide a more or less unique solution to the theory that looked like the real world. He announced that now the probability of this is less than 1 in 10^500.

Susskind then explained a bit about KKLT vacua, saying that his main reason for discussing them was to show how silly and inelegant they are. He compared them to a Rube Goldberg machine and called Shamit Kachru the "master Rube Goldberg architect".

The most dramatic part of Susskind's talk was something new: an attack on the idea of low-energy supersymmetry. He explained the standard fine-tuning argument for supersymmetry, but then indicated that he thought an anthropic argument made more sense. The reason the Higgs mass is so much smaller than the Planck mass is not supersymmetry, but instead because that small size is necessary for our existence. He said that the question of low-energy supersymmetry is something that Douglas's statistical analysis of vacua should address (Douglas will talk tomorrow), but his view is that low-energy supersymmetry will be very unlikely.

In the question session, John Schwarz challenged him about this, claiming that there were other reasons to believe in low-energy supersymmetry, including the unification of coupling constants and the idea that dark matter is the lowest mass superpartner. Susskind's response was that even though there were a couple reasons like those, there were many more that indicated problems with the idea of low-energy supersymmetry, including problems with too fast proton decay.

It was pretty amazing to see someone challenging the supersymmetry orthodoxy. On the other hand, the whole program Susskind and others are pursuing is completely loony. String theory predicts absolutely nothing, and instead of drawing the obvious conclusion that it is a useless idea, Susskind is trying to turn this failure into some perverse sort of virtue.

Update: In Michael Douglas's talk today he said that his calculations show no reason for a low-energy supersymmetry breaking scale to be especially likely. So he expects that supersymmetry will only be broken at a high energy. Maybe somebody should tell the people working on the LHC experiments that the whole supersymmetry thing is now off, they should find something else to look for.

Comments

1. **Jack Sarfatti**
   June 19, 2004

   The Landscape at Davis

   "I'm in Nothern California this week, and have been attending some of the talks at the conference at UC Davis celebrating Albert Schwarz's 70th birthday. The landscape at Davis is exceedingly flat, but this morning Lenny Susskind gave a remarkable talk with the title "Exploring the Landscape".

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   [Sarfatti Commentary: According to Lenny's world hologram idea, the effective Planck scale Lp* should be increasing as the universe expands with scale factor R(t) in units of length as

   Lp*(t) = Lp^2/3R(t)^1/3

   This means that time's arrow is tied to the expansion of the universe with the Hawking hologram entropy of the universe S(t) obeying

   S(t)/k ~ R(t)^2/Lp*^2 ~ R(t)^4/3

   On the other hand, the effective G* from

   Lp*^2 = hG*/c^3

   cannot operate on the large scale where we know G is Newton's.

   Note that Lp*(now) ~ 1 fermi

   This is very curious, making G* ~ 10^40G, which we can posit to act only on the scale of Lp*.

Note also that Lp** = Lp^2/3(c/Ho)^1/3 is also ~ 1 fermi

Einstein's cosmological constant is /\ ~ (Ho/c)^2

where

/\ = (Lp*)^-2[(Lp*^3|Vacuum Coherence|^2 – 1]

H = R(t)^-1dR(t)/dt

Lp* ~ R(t)^1/3

So we seem to get a differential equation in this model. However, Vacuum Coherence also has a dynamics. It has a covariant Landau-Ginzburg eq with a Mexican Hat Potential for spontaneous broken vacuum symmetry.

So the idea is still not coherent in my mind, but it seems to be pointing to something important.]

"In the question session, John Schwarz challenged him about this, claiming that there were other reasons to believe in low-energy supersymmetry, including the unification of coupling constants and the idea that dark matter is the lowest mass superpartner."

[Sarfatti Commentary: This is a wrong idea. Dark matter is exotic vacuum with positive zero point pressure. Dark matter detectors will never click in principle with the "Right Stuff" to explain Omega(DM) ~ 0.23. So far observation is on my side in this hard exact prediction. The Italian claims to contrary have proved wrong. So far, so good.]

"Susskind's response was that even though there were a couple reasons like those, there were many more that indicated problems with the idea of low-energy supersymmetry, including problems with too fast proton decay."

[Sarfatti Commentary: Susskind is on right track here.]

"It was pretty amazing to see someone challenging the supersymmetry orthodoxy. On the other hand, the whole program Susskind and others are pursuing is completely loony."

[Sarfatti Commentary: But is it loony enough to be true?]

"String theory predicts absolutely nothing, and instead of drawing the obvious conclusion that it is a useless idea, Susskind is trying to turn this failure into some perverse sort of virtue."

[Sarfatti Commentary: Indeed string theory has much less observational support than

1. Flying saucers

2. Parapsychology

3. Cold Fusion

It probably has as much observational support as does Ashtekar's non-perturbative background independent quantum gravity?

I do think however that all of the above have some interesting ideas and factual support that will survive – even sting theory, oh pardon me, I meant even string theory. ;-)]

http://www.math.columbia.edu/~woit/blog/archives/2004_05.html

On Jun 19, 2004, at 5:43 PM, Doc Savage wrote:

bcc

Isn't Brian Greene a member of Professor Woit's Department? You mean the landscape is not elegant? Kidding aside, I think Lenny's work is not as bad as it is made out to be below. "Sting Theory" may be a more appropriate name after all? 🙂

Lenny's holographic universe idea may turn out to be true. It is certainly very interesting.

The generalized uncertainty relation is also interesting

uncertainty in position ~ h/(uncertainty in momentum) + (quantum of area)(uncertainty in momentum)/h

is a good idea coming from black hole formation when too much energy is concentrated into too small a volume.

On Jun 19, 2004, at 9:58 AM, Doc Savage wrote:

More Landscape Stream of Consciousness

It looks like particle theory has now degenerated to the point where its leading figures can't think of anything better to do than to write rambling articles with virtually no equations that reach no real conclusions. Last week was Lenny Susskind, tonight there's a new article by Michael Douglas.

His conclusion, such as it is, goes like this:

"If I had to bet at the moment, I would still bet that string theory favors the low scale, for the reasons outlined above, but it is not at all obvious that this is what will come out in the end.… We should keep in mind that 'favoring' one type of vacuum or mechanism over another is not a strong result, if both types of vacuums exist…"

So, maybe string theory "favors" a low supersymmetry-breaking scale, maybe not. As usual, not only can't it predict anything, it can't even predict the scale at which it can't predict anything. I really cannot understand why anyone thinks this kind of thing is science."

http://www.math.columbia.edu/~woit/blog/archives/000031.html

The stupendous Landscape of sting theory vacua

"At an early stage in the Los Alamos preprint archive it was split up into hep-th (for more formal or speculative work not directly relevant to experiment) and hep-ph (for "phenomenological" papers directly related to experiment). Susskind has just come out with his latest and now seems to feel that his ideas about the "Landscape" are directly of interest to experimenters and so belong in hep-ph.

The preprint is riddled with typos, for instance the third paragraph starts like this:

"During the last couple of years an entirely new paradigm has emerged from the ashes of a more traditional view of string theory. The basis of the new paradigm is the stupendous Landscape of sting [sic] theory vacua — especially the non-supersymmetric vacua. These vacua appear to be so numerous that the word Discrtuum [sic] is used to describe the spectrum of possible values of the cosmological constant….."

You get the idea.

Some high points of the article:

1. "low energy supersymmetry – an ugly solution" to the naturalness problem. Now he tells us. From what I remember the "beauty of supersymmetry" has always been one argument made in its favor.

2. "the ashes of a more traditional view of string theory". It seems that the picture of the world according to string theory that has been heavily sold for the last twenty years has burned down to the ground.

3. The argument in his last paper, such as it was, was wrong. Now he's got a new one with a similar conclusion.

4. "… a prediction that supersymmetry will not be seen at the TEV scale seems warranted". OK, string theory is finally making a prediction.

5. "If it turns out that low energy supersymmetry is a feature of TEV physics, then we will have to conclude that other considerations outweigh the counting of vacua on the Landscape". So, even though string theory predicts no low energy supersymmetry, if it is found it doesn't mean string theory is wrong. Got it?"

http://www.math.columbia.edu/~woit/blog/archives/2004_06.html

2. **Peter**
May 19, 2004

Hi Thomas,
I saw that paper last night and it is pretty amazing. It attacks the central idea of the whole supersymmetry/string theory ideology. I'll write something about it in the weblog later today.

3. **Thomas Larsson**
May 19, 2004

The anthropic virus has evidently hit Harvard.

hep-th/0405159
Title: Supersymmetric Unification Without Low Energy Supersymmetry And Signatures for Fine-Tuning at the LHC
Authors: Nima Arkani-Hamed, Savas Dimopoulos

4. **Peter**
May 18, 2004

I've tried to get some action on the Kaku/Horgan bet from string theorists, with no success. They don't seem to be willing to put their money where their mouth is (actually they increasingly seem unwilling to put their mouth there either: Horgan had trouble finding someone to bet against him and most string theorists are now unwilling to make any guess as to when string theory will ever predict anything).

Rube Goldberg was a cartoonist who drew cartoons of absurd complicated devices that accomplished something very simple. He seems to have fallen out of favor, these things were well-known (at least in the US) during the 60s. For an example, see

http://www.rube-goldberg.com/html/pencil_sharpener.htm

No, Susskind didn't discover branes, that's generally attributed to Polchinski. Susskind was one of the co-discoverers of string theory: he and a couple others realized that the Veneziano amplitude could be derived from quantizing a string.

Susskind isn't necessarily giving up on supersymmetry, just the idea that it will appear at accessible energies. One argument for supersymmetry at LHC energies has always been that you could then explain why the electroweak breaking scale was so much lower than the Planck mass without "fine-tuning". Susskind's argument is that one shouldn't worry about fine-tuning, just figure out what parameter ranges are consistent with human existence, and if there is a uniform distribution of string vacua with those scales, then any one is as good as any other. If so, it is very unlikely that supersymmetry breaking will happen at LHC energies, more likely it will be at very high energies near the Planck or string scale.

5. **erinj**
May 18, 2004

Well, if Lenny's right about that probability – and I hope he is – then Michio Kaku's bet on LongBets.org is almost certainly scuppered! Loser!

I mention Kaku's bet as I first mistook your report of Susskind's remark about Kachru as being one about Kaku, who I then remembered has made a long bet involving string theory at LongBets.org… ever tempted to place a bet there, Peter? Increasingly, the reports in your April 1st post look like they may yet come true!

But back to your post: I once saw a talk by Susskind and I was amazed that so many field theorists came out of his talk seemingly convinced of the validity of string theory. Like the talk you saw, I found his equally repetitive and with minimal mathematical (and physical) content.

Wasn't Susskind the guy who introduced (or, less likely, `discovered') branes? If so, I think him guilty of prolonging the atrocious waste of effort and time that string theory has and had already inflicted upon theoretical high energy physics.

Just what is all this anthropic nonsense, too? String theory is correct because we are here, and because we have `discovered' string theory? Loony all right.

I agree that to see a proponent of superstring theory, and not just string theory (as don't string theorists dislike working on plain old string theory, given how many extra dimensions and other complications string theory without supersymmetry brings with it?), loosing faith in supersymmetry is suprising. Where would matter fields in string theory be without the simplifying axiom of supersymmetry? As far as I know, all those elaborate intersecting brane theories in which the Standard Model supposedly appears depend upon SUSY at higher energies neatly tying things together.

Also, shouldn't string theorist's listen to the experimentalists? (Not more, but in the first place, I mean 🙂

About two years ago I saw a talk by an experimentalist working on some DESY experiment involving a search for `low-energy SUSY', and he stated that their results permitted them to categorically rule out the lightest possible SUSY superpartner, the higgsino, at the present highest energies attainable (though perhaps a heavy Higgs or something might rescue this). So even back then low-energy SUSY looked unlikely, according to the people who actually go out there and look for these things. I certainly came out of that talk with much less belief in SUSY.

By the way, who or what is "Rube Goldberg"?

More From Davis

May 17, 2004
Categories: Uncategorized

The last two talks at the Davis conference were quite interesting. Alexandre Givental gave one entitled "Twisted Loop Groups and Gromov-Witten Theory", which went by way too fast. He has an interpretation of the generating functional for Gromov-Witten invariants that uses a loop group. Some infinite dimensional symplectic geometry involving this group gives a conjectural explanation of the properties of these invariants. The talk covered a lot of material so, while intriguing, was quite hard to follow.

Witten gave the last talk, and his philosophy was the opposite of Givental's. He covered only a little material, at a level that was easy to follow. The nominal topic of his talk was his recent work on the relation of strings in twistor space to gauge theory scattering amplitudes, but he didn't really get to this. He began by saying that there were at least a couple different possible talks he could give about the background to his recent work. One was the one he gave a couple weeks ago at a conference at NYU which covered gauge theory scattering amplitudes. Luckily for me since I had heard that one, he decided to give a different one at Davis, mostly covering some of the ideas about twistors used in his work. He discussed the twistor theory construction of an SU(2,2) representation using the massless single particle solutions of a fixed helicity. This is related to a non-compact version of the Borel-Weil-Bott theorem, constructing a representation on a higher cohomology space. Presumably one could also do this using the Kostant Dirac operator techniques I mentioned recently.

Comments

1. **Peter**
   May 21, 2004

   SU(n,n) means special unitary transformations that preserve a metric of signature (n,n) (i.e. 2n-dimensional complex space with metric with n + signs and n – signs). For supergroups people write them with a bar, like SU(n|n) for a group with n bosonic and n fermionic generators.

   Part of the twistor story is that SU(2,2) is the group of conformal transformations in 4d.

   There are a lot of different ways of thinking about twistors, but they are pretty natural objects if you are thinking about the geometry of spinors in 4d in a way that makes explicit the conformal symmetry.

   I'm also mystified about why Witten remains such a believer in string theory. I think one reason he keeps working on it is that there continue to be interesting relations between string theory and gauge theory, so even if it doesn't work as a unification idea, string theory can still lead to interesting new ways of thinking about gauge theories (e.g. AdS/CFT).

   His recent work has certainly lead to interesting results about perturbative gauge theory amplitudes, although the relation to string theory seems somewhat tenuous to me even though it is clearly a big part of his motivation.

2. **erinj**
   May 21, 2004

   I saw Witten's paper and thought that this was another attempt to concretely link string theory with gauge theory, or to indicate that gauge theory is contained within string theory in a convoluted manner and therefor subsumes it – thus demonstrating the correctness or superiority of string theory. This has been attempted before, hasn't it, with things like AdS/CFT, dualities, etc. Surely if this strategy was correct, they would have found much simpler and more obvious connections between string theory and gauge theory than all this stuff about bulks and boundaries, SUSY, AdS, SUGRA and twistors.

   It seems to me as if the string theorists are grasping at straws with this approach – that is, try and dig up gauge theory quantities out of string theory in peculiar situations. They just keep re-hashing the same old arguments and try and squeeze gauge theory out of strings all the time. I'm a fan of Witten's marvellous non-string theory work (e.g. SUSY QM & Morse theory, TQFT & knot theory, bosonization, Seiberg-Witten/Donaldson theory, QFT & elliptic genera, etc.), and I thought his Fields Medal was a crowning achievement, but why he keeps on spending so much cerebral energy on string theory mystifies me.

   Also, it would seem preferable, and surely much simpler, to build representations on higher cohomology spaces using the Kostant Dirac operator and spinors than with twistors, which appear to be far less intuitive objects and are harder to work with.

   Incidentally, Peter, I've always had some confusion over the notation SU(n,n) – isn't the (n,n) notation something to do with the signs of the metric components? And isn't SU(n,n) a `supergroup', meaning that it's Lie algebra is modified to include anti-commutators (as with the super-Poincare group)? I ask as I've never been able to find a proper definition for this notation or a reference which defines it clearly.

3. **Pyracantha**
   May 17, 2004

   I am looking forward with a kind of eschatological eagerness to the time when I will be able to recognize and understand what you are talking about in this entry, and what the speakers at that conference are talking about. However, by the time I get that far in my physics and math self-education project, all of that will probably be very obsolete.

   Pyracantha at "Electron Blue"

More Crackpots

May 19, 2004
Categories: Uncategorized

Slate this morning has an article by Jim Holt about an interview with Andrei Linde. In the interview, Linde speculates that universes like ours could be created in a lab, that maybe we live in such a universe, and that the creator of such a universe could communicate with his/her creations by tuning the parameters of its "Landscape".

Comments

1. **Peter**
   May 21, 2004

   I heard Linde talk about inflation and the landscape at Davis, and his talk was no more crackpot than others. Still I don't understand why he thinks it is a good idea to sit around promoting the idea that maybe we live in a universe created by an intelligent being who may try and communicate with us through the fundamental parameters of physics. That's definitely over the line into serious crackpot territory.

2. May 21, 2004

   As far as I know Linde's idea is not new. It is his version of inflationary scenario, called chaotic inflation. The only new component in the story is its connection to string landscape.

3. **erinj**
   May 19, 2004

   This idea has already appeared before in science fiction (Gregory Benford's `Cosm') and is not science. So I agree, Peter, I'm awaiting the arrival of the `SuperCosmos' bandwagon, hope to leap on it, publish trillions of papers on it (say, at one a month) and become an eminent physicist with permanent tenure and a big office.

   If an `eminent' physicist like Linde can borrow such ideas and promote them as science, what is the scientific world coming to?

   Perhaps the book store clerk who placed that copy of F. David Peat's `Superstrings' book I once found in the science fiction section was doing the right thing after all…

   (Incidentally, I've just seen a book titled `Superstrings and Other Things: A Guide to Physics', by Carlos Calle, and it's a British product… as if superstrings are established physics! The shame of it – using string theory's hype and media presence to sell a book about physics!)

   Given that the vague term "landscape" keeps cropping up in association with strings, perhaps somebody should write up a glossary of string theory buzz-words/phrases or `flavours of the month' for easy later reference, which would also help chart the mess the `theory' has gotten itself into.

4. **Simplex**
   May 19, 2004

   Peter, great blog!
   I have to admit Linde's interview (in which I detected humor and irony) could be gravely misconstrued and abused by a religious Told-you-so.

   But the journalist warned his readers about Linde's irony before getting into it:

   "Linde, it should be said, is famous for his mock-gloomy manner, and these words were laced with irony. But he insisted that this genesis-in-a-lab scenario was feasible, at least in principle…"

   and he included this rather irreverent quote from Linde:

   '"You might take this all as a joke," he said, "but perhaps it is not entirely absurd. It may be the explanation for why the world we live in is so weird. On the evidence, our universe was created not by a divine being, but by a physicist hacker."'

   I suspect there is nothing new here, just Linde's usual schtick. And it sounded as if Linde's imagined Hacker was communicating to us mainly, if at all, by chosing basic dimensionless constants like the ratio of proton to electron mass
   1836.152…
   if a nut wanted to decode those digits and discover the ten commandments then he could have already started working on the number pi.

   "But then Linde thought of another channel of communication between creator and creation?the only one possible, as far as he could tell. The creator, by manipulating the cosmic seed in the right way, has the power to ordain certain physical parameters of the universe he ushers into being. So says the theory. He can determine, for example, what the numerical ratio of the electron's mass to the proton's will be. Such ratios, called constants of nature, look like arbitrary numbers to us: There is no obvious reason they should take one value rather than another…But the creator, by fixing certain values for these dozens of constants, could write a subtle message into the very structure of the universe…"

   I guess I want to say I wasn't as shocked as you and Linde seems less goofy than Sarfatti to me—and more entertaining.

5. **Chris Oakley**
   May 19, 2004

   Personally, I subscribe to the views of the Jatravartid People of Viltvodle Six. They firmly believe that the entire universe was sneezed out of the nose of a being called The Great Green Arkleseizure. They live in perpetual fear of the time they call The Coming Of The Great White Handkerchief.

   But seriously … however wacky people may think Linde or other's ideas about Cosmology, it is hard to prove them wrong. Why? Because (in my humble opinion, obviously) Cosmology is not a science, and probably never will be. Maybe it would help if people concentrated on ideas that *can* be proved wrong.

Attack on the Main Argument for Supersymmetry

May 19, 2004
Categories: Uncategorized

The hundreds of expository articles about supersymmetry written over the last twenty years or more tend to begin by giving one of two arguments to motivate the idea of supersymmetry in particle physics. The first of these goes something like "supersymmetry unifies bosons and fermions, isn't that great?" This argument doesn't really make a whole lot of sense since none of the observed bosons or fermions can be related to each other by supersymmetry (basically because there are no observed boson-fermion pairs with the same internal quantum numbers). So supersymmetry relates observed bosons and fermions to unobserved, conjectural fermions and bosons for which there is no experimental evidence.

Smarter people avoid this first argument since it is clearly kind of silly, and use a second one: the "fine-tuning" argument first forcefully put forward by Witten in lectures about supersymmetry at Erice in 1981. This argument says that in a grand unified theory extension of the standard model, there is no symmetry that can explain why the Higgs mass (or electroweak symmetry breaking scale) is so much smaller than the grand unification scale. The fact that the ratio of these two scales is so small is "unnatural" in a technical sense, and its small size must be "fine-tuned" into the theory.

This argument has had a huge impact over the last twenty years or so. Most surveys of supersymmetry begin with it and it justifies the belief that supersymmetric particles with masses accessible at the LHC must exist. Much of the experimental program at the Tevatron and the LHC revolve around looking for such particles. If you believe the fine-tuning argument, the energy scale of supersymmetry breaking can't be too much larger than the electroweak symmetry breaking scale, i.e. it should be in the range of 100s of Gev- 1 Tev or so. Experiments at LEP and the Tevatron have ruled out much of the energy range in which one expects to see something and the fine-tuning argument is already at the point of starting to be in conflict with experiment, for more about this, see a recent posting by Jacques Distler.

Last week at Davis I was suprised to hear Lenny Susskind attacking the fine-tuning argument, claiming that the distribution of possible supersymmetry breaking scales in the landscape was probably pretty uniform, so there was no reason to expect it to be small. He believes that the anthropic explanation of the cosmological constant shows that the "naturalness" paradigm that particle theorists have been invoking is misguided, so there is no valid argument for the supersymmetry breaking scale to be low.

I had thought this point of view was just Susskind being provocative, but today a new preprint appeared by Nima Arkani-Hamed and Savas Dimopoulos entitled "Supersymmetric Unification Without Low Energy Supersymmetry and Signatures for Fine-Tuning at the LHC". In this article the authors go over all the problems with the standard picture of supersymmetry and describe the last twenty-five years or so of attempts to address them as "epicyclic model-building". They claim that all these problems can be solved by adopting the anthopic principle (which they rename the "structure" or "galactic" or "atomic" principle to try and throw off those who think the "anthropic" principle is not science) to explain the electroweak breaking scale, and assuming the supersymmetry breaking scale is very large.

It's not suprising you can solve all the well-known problems of supersymmetric extensions of the standard model by claiming that all effects of supersymmetry only occur at unobservably large energy scales, so all we ever will see is the non-supersymmetric standard model. By itself this idea is as silly as it sounds, but they do have one twist on it. They claim that even if the supersymmetry breaking scale is very high, one can find models where chiral symmetries keep the masses of the fermionic superpartners small, perhaps at observably low energies. They also claim that in this case the standard calculation of running coupling constants still more or less works.

The main experimental argument for supersymmetry has always been that the running of the three gauge coupling constants is such that they meet more or less at a point corresponding to a unification energy not too much below the Planck scale, in a way that works much better with than without supersymmetry. It turns out that this calculation works very well at one-loop, but is a lot less impressive when you go to two-loops. Read as a prediction of the strong coupling constant in terms of the two others, it comes out 10-15% different than the observed value.

I don't think the argument for the light fermionic superpartners is particularly compelling and the bottom line here is that two of the most prominent particle theorists around have abandoned the main argument for supersymmetry. Without the pillar of this argument, the case for supersymmetry is exceedingly weak and my guess is that the whole idea of the supersymmetric extension of the standard model is now on its way out.

One other thing of note: in the abstract the authors refer to "Weinberg's successful prediction of the cosmological constant". The standard definition of what a prediction of a physical theory is has now been redefined down to include "predictions" one makes by announcing that one has no idea what is causing the phenomenon under study.

Comments

1. **Aaron**
   May 23, 2004

   To various people, I'm sorry, but I really don't have to time to answer questions on QFT in a comments thread. You might want to consider posting to sci.physics.research. Anyways, various theories of SUSY breaking do have hidden sectors in which the breaking occurs and is then transmitted in some way to the visible sector. Finally, it's a theorem that particles statistics are governed by one dimensional representations of the fundamental group of the configuration space of n points in *space*. For d>2, that's the symmetric group and the only reps are the trivial and the antisymmetric one. In two dimensions, it's the braid group which has all sorts of fun one dimensional representations. This gives what are called anyons.

   To Peter, I continually wonder what physicists you hang out with. None of this stuff is secret. I was at a department wide talk which brought this stuff up in the past few months. Again, I'm rather curious what else you would have us tell the experimentalists to look for.

2. **Peter**
   May 23, 2004

   Hi Aaron,

   I wasn't under the impression that the degree of fine-tuning needed if the bound on the Higgs goes up by 5-10 Gev was such that it would cause people to give up on supersymmetry. If so, one might not even need to wait for the LHC if the luminosity at the Tevatron improves sufficiently.

   There are at least two reasons why a lot more honesty about the failings of low-energy supersymmetry would be a healthy thing (and I don't see a downside since I don't think there's any danger the experimentalists at the LHC won't look for superpartners).

   1. Experimentalists deserve an honest appraisal of how likely various possibilities are so they can make intelligent allocations of their resources.

   2. The dishonesy and over-hyping of supersymmetry is part of a larger problem in particle theory. The amount of this in superstring theory/M-theory is vastly greater, and no experiments are likely to save us from this there. Theorists should start behaving a lot more like scientists, with a lot less hype and a lot more healthy skepticism about ideas that don't work.

3. **Peter**
May 23, 2004

Hi Chris,

Sure, you can add other gauge groups, but if the particles we see have charges with respect to them, they'll experience a new force and will behave differently, so there are all sorts of bounds on such a thing.

If you assume that the particles we know about are all uncharged with respect to the new gauge group, then it is a "hidden sector", and its effects will be hard to observe (perhaps only through gravity).

4. **Peter**
May 23, 2004

Hi JC,

The "Planck Scale" is only the scale of quantum gravity if you assume that however you calculate the effective Newton's constant, you get dimensionless numbers of order unity and dimensionful ones coming from the scale. If the calculation of the effective Newton's constant contains an exponentially small dimensionless number, the scale could be quite different.

I don't see any reason to try and get the standard model forces as induced from some other theory, but people have speculated that quantum gravity is induced from the other gauge forces. One intriguing fact about 2d WZW models is that they can be formulated as theories with a gauge (or loop group) symmetry, but the Sugawara construction shows they automatically also are non-trivial reps of the diffeomorphism group, so in some sense one is getting for free a theory of gravity from a theory with just a gauge symmetry.

5. **JC**
May 23, 2004

On a different note, how do we know that the known elementary particles ONLY obey bose and fermi statistics? How do we know that they are NOT obeying more general parabose and parafermi statistics, or for that matter some other generalized permutation symmetry groups?

6. **Chris Oakley**
May 23, 2004

There is a point raised by JC that I have never heard discussed, but I feel ought to be taken seriously. If there was a "new" kind of force that conserved all the quantum numbers of (say) the Strong interaction, but was much weaker, how would we ever know about it? What is to stop me claiming that SU(3)xSU(3)xSU(2)xU(1) is the "real" theory of the world with my weak-strong interaction as the other SU(3)?

7. **Aaron**
May 22, 2004

That should be

G = <T>

the expectation value of the stress energy tensor.

8. **Aaron**
May 22, 2004

Just by dimensional analysis, the Planck scale is the scale where gravity becomes relevant. On the other hand, the Planck scale might not actually be what we think it is. Some theories with extra dimensions have the Planck scale and the weak scale coinciding.

The problem with keeping gravity classical is that you have to couple it to quantum stress energy. The obvious way to do that

G =

has problems. I'm open for suggestions on other ways to do it. Sakharov's induced gravity gets things off by orders of magnitude as I remember it, but I don't remember it very well.

Epicycles are completely irrelevant here. When you add epicycles, it means you're adding parameters to your theory to account for the fact that the theory doesn't fit the data. The standard model fits the data amazingly well.

There are theories where gravity unifies at the same point as the other forces. GUTs are going to have massive gauge bosons at the scale of the breaking. You can break large gauge groups in stages E8 -> E6 -> SO(10) …, for example. There's all sorts of stuff that can happen, really.

The almost convergences of the couplings was not imposed in any form. It follows directly from the formulae.

Finally, I doubt the electroweak forces and QED are "induced" in any way (I'm not sure what that would mean, really.) The theories that we have for them now work just fine.

9. **JC**
May 22, 2004

Aaron,

I never quite understood exactly why the scale for a quantum theory of gravity has to be taken at the Planck scale specifically. Are there any rigorous and/or mathematical arguments which shows that this always has to be the case? Why can't the quantum gravity scale be at something like several million or billion TeV's, or for that matter several billion or trillion times larger than the Planck scale itself? Is the Planck scale for quantum gravity just accepted on faith or by fiat decree?

Is there a possiblity that gravity is just strictly a classical force with no quantum counterpart? Are there any experiments that have confirmed any quantum gravity phenomena, such as the Unruh effect? (Would the Unruh effect even be a good candidate to search for in a tabletop experiment?) Is this belief in quantum gravity just an extrapolation of the three other forces being quantized? Could gravity just be nothing more than a "residual force" produced by higher order quantum effects, in the style of Sakhrov's induced gravity?

On the surface, adding in more particles, symmetries, and other "junk" into the standard model feels a lot like adding in more "epicycles" and free parameters into an already grotesque looking theory. (Well, maybe not completely "grotesque" looking.) Could the electroweak and qcd forces be an "induced" force of some sort, in the spirit of Sakharov?

With the idea of three coupling constants merging at one point at some high energy scale, why doesn't gravity also converge too to that exact same point as the other three coupling constants? Or for that matter if there's a 5th or 6th force that we aren't aware of yet, would their coupling constants also converge to this exact same point too? Is this convergence point just something that was noticed by running the coupling constants to higher energy scales, or was it imposed a priori by fiat decree?

10. **Aaron**
May 22, 2004

There are a few answers to your question about the standard model. The most obvious is that it doesn't incorporate gravity and we've got pretty good evidence that gravity exists, so any theory really ought to include it. One can ask at what energy scales does a quantum theory have to take gravity into account. The answer to that is approximately the Planck scale. That's why it's important.

The other answers are a bit more technical. One is that, as has been discussed a bit here, the standard model isn't natural. There are two types of naturalness, actually. What it really comes down to, however, is that the scale of the electroweak interaction is much, much smaller than the Planck scale. We don't know why this is true. This is dissatisfying.

There's also technical naturalness which says that the Higgs mass is naturally at the Planck scale because of loop effects. Supersymmetry stops this from happening. So, why it doesn't explain why the weak scale is so much smaller than the Planck scale, it does stabilize the Higgs mass.

A third reason to believe that there might be something beyond the standard model is that if you were to draw three random lines on a piece of paper, there's little chance they'll intersect at a simply point. Nonetheless, if you look at the runnings of the coupling in the standard model, you get three lines to come reasonably close to intersecting. This is a coincidence that is not explained by the standard model. It could just be random, of course, but it could also be a clue.

As for why finding just the Higgs would be depressing, where would we go, then? Building a larger accelerator is unlikely. It means that this dataless purgatory we currently inhabit is likely to last for the foreseeable future. That would suck.

11. **Aaron**
May 22, 2004

If you don't like fine tuning, then, as I understand it, there's just not much more room for the Higgs left. Another 5 or 10 GeV and you've got issues. Are you telling me that other considerations give you the same bounds?

"I just think that if one is honest one sees that the GUT and low-energy supersymmetry ideas are failures. If you don't acknowledge failure and keep promoting failed theories you're not doing science anymore."

Such pessimism. You can't wait five more years for them to turn on the damn machine before you make these sweeping pronouncements? Even if you want to argue that, in your sense of the word, SUSY makes no predictions, I assume you agree that there are plenty of things that would strongly support SUSY if discovered at the LHC? Are we now not only theorizing without the aid of experiment, but also pronouncing theories abject failures before even attempting to test them?

Let the experimenters do their work. If they just see a Higgs, we all go home. If they see SUSY, then, presumably you admit it wasn't a completely false direction to purse, and we start measuring the weak SUSY breaking parameters. If we see something else entirely unlike SUSY, we all go out and study that.

That's the joy of experiment.

12. **JC**
May 22, 2004

Are there any empirical reasons why the standard model is NOT the final answer to particle physics?
Or is it mainly ideological or philosophical reasons why people think the standard model is not the final answer?

Why would finding the Higgs particle be a depressing event? Could there be a 4th or 5th family of quarks/leptons heavier than the mass of the Higgs?

Why should we take the notion of a "Planck scale" energy seriously? Could it be possible that the "Planck energy" is just a product of several known physical constants which produces a "number" that has no physical relevance in the real world? Is there any empirical and/or fundamental reason why Newton's gravitational constant is important at higher energy scales?

13. **Aaron**
May 22, 2004

There are unitarity arguments that tell us something has to happen really soon. There are Higgsless models of EWSB. The ones I've heard about use boundary conditions in extra dimensions, but there could be others. I don't want to say too much because I really don't know much about them, but I'm pretty sure they predict something happening at LHC.

People might find this paper interesting as an outlook on what might come:

hep-ph/0312096

If you ask any particle physicist what lives in their nightmares about the next few years, the answer is this: that the LHC discovers a Higgs, and that's it. Everything is completely described by the standard model. If that happens, we might just all have to go home.

14. **Peter**
May 22, 2004

The proliferation of undetermined parameters in supersymmetric GUTs comes not just from the supersymmetry breaking, but also from the GUT symmetry breaking (via the Higgs mechanism or whatever). In principle one can hope that some compelling model for supersymmetry breaking exists that predicts the 100 or so parameters, but the fact of the matter is that all pheonomenologically viable methods of supersymmetry breaking are complicated, contrived, and don't do much in the way of fixing those parameters.

I really wish people would reserve the term "prediction" for its standard usage: one has a well-defined model in which something is calculable and you calculate it. In the standard supersymmetry picture, you don't know what causes supersymmetry breaking or even what its scale is, and the mass of the Higgs is constrained only by the very vague "naturalness" principle (just how much fine-tuning is too much?).

Essentially all low-energy supersymmetry and naturalness are telling you is that the Higgs can't be too heavy. But there are other reasons for that (Higgs sector becomes strongly coupled if the Higgs mass is too high), so it isn't really telling you anything new.

If I had a successful extension of the standard model I'd be preparing my speech for Stockholm. I just think that if one is honest one sees that the GUT and low-energy supersymmetry ideas are failures. If you don't acknowledge failure and keep promoting failed theories you're not doing science anymore.

15.
May 22, 2004

How would the picture of the standard model change if no Higgs particle is discovered for several hundred thousands of TeV above 1 TeV? What viable mechanisms could replace the Higgs in this case?

16. **Aaron**
May 22, 2004

SO(10) also nicely accommodates neutrino masses. The dark matter candidate is also more robust than you indicate, I think. If you just write down general Lagrangian, it's easy to find Flavor Changing Neutral Currents (FCNCs) from SUSY. One way to eliminate them is by imposing a symmetry called R-parity by fiat. This then necessitates a Lightest Supersymmetric Partner (LSP) that fits all the cosmological requirements for a WIMP.

The 100 or so parameters you refer to are in the weak SUSY breaking lagrangian. They really parametrize our ignorance. One expects that when the mechanism of SUSY breaking is understood (assuming low energy SUSY really exists), there will be tons of relations among the parameters of that lagrangian. It would probably work the other way around. We'd measure the parameters in the weak SUSY breaking lagrangian, discover relations and use that to better understand the mechanism of SUSY breaking. Regardless, the idea that there really are 100 (or whatever) free parameters isn't really true.

I already gave you a prediction of SUSY: the light Higgs. That it hasn't been seen is one reason, I think, that people are getting a bit more skeptical. Also, one would hope to see all sorts of fun stuff at the LHC.

No one's claiming SUSY is perfect. But, before you knock it too much, what other extensions of the standard model do you propose people study? Because, believe me, people are always interested in something new.

17. **Peter**
May 22, 2004

Hi Aaron,

At some point soon I'll try and write up a more detailed discussion of the pros and cons of supersymmetry. The issues have been around now for more than two decades and the only interesting change I've seen recently is that Witten is concerned enough about this to have given several talks in the past year or two discussing the problems with supersymmetry. Also,now Susskind, Arkami-Hamed and Dimopoulos seem willing to argue that the problems of low-energy supersymmetry are such that perhaps the idea should be abandoned.

The standard ideology is that the idea of low-energy supersymmetry combined with grand unification is, as you say, "very compelling". I've always disagreed with this and continue to do so strongly. I think the only statement that can be defended is that the idea has aspects that are "suggestive". The two best arguments for this ideology are:

1. The fact that the particles in one generation fit rather naturally into the SO(10) spinor representation.

2. The coupling constant unification, which is off by 10-15%, an amount one could hope to ascribe to threshold effects.

(A third standard argument, that in this framework there is a candidate particle that could be dark mattter, seems to me such a weak argument that I don't think it is worth taking very seriously)

I don't think either of these is "very compelling", partly because they come at a huge cost. A supersymmetric GUT is a vastly more complex theory than the standard model with at least 10 times more undetermined parameters. In return for this huge increase in the complexity of the theory, one gets the measly return of the two points mentioned above and a huge number of problems one has to find some way of arguing away (too much baryon number violation, FCNC, CP violation, etc.)

The huge number of undetermined parameters and lack of any idea of what the supersymmetry breaking scale is mean that it is extremely hard to get a real prediction out of this framework. The closest thing to one would be that of proton decay, but even here a prediction is possible only in a very tenuous sense. For a generic choice of the undetermined parameters one gets proton decay faster than experimentally observed. One can choose parameters that make the rate much smaller, although there is a point at which you run into trouble doing this. So supersymmetric GUTs are consistent with a huge range of possible proton decay rates, which is not exactly much in the way of a prediction.

18. **Aaron**
May 22, 2004

There's no trial going on. As I'm sure you know, the standard model works depressingly well. SUSY extends it in useful ways. In addition to what I've mentioned it also provides a useful candidate for a WIMP. The fact that it does this, betters coupling unification and sort-of-solves the naturalness problem seems like pretty good reasons to look at it.

With the Higgs not found yet, that's a strike against it, sure. But, why the opprobrium? It's a very nice idea that may or may not be correct.

As for doing QFT rigorously, none of the directions that I've seen out there interested me all that much. I'm glad people out there are working on it, and I'm glad they aren't me. In the meantime, QFT works pretty damn well as we use it, so I'm not too constrained.

19. **Chris Oakley**
May 22, 2004

Aaron –

One difference between a civil and a criminal case in English law is that the verdict is based on the "balance of probabilities" in a civil case, whereas "proof beyond reasonable doubt" is required in a criminal case. With SUSY in the dock, I would deliver the former verdict against it, but not the latter. To put it another way, I do not think that SUSY is leading anywhere good (and I am not sure that I ever did). Feel free to prove me wrong, though.

As for the Millenium Prize, the fact that it exists at all is proof enough of failure on all of our parts. We should not be relying on someone to provide a silver bullet to cure the elementary problems of QFT. More people should be working on solving these problems: not just a handful of German mathematicians. I did what I could – what did *you* do?

20. **Aaron**
May 21, 2004

There are plenty of QFTs without Lagrangian descriptions. Look up minimal models in CFT, for example.

Also, of course SUSY has guided experimental searches. You have to trigger on something, and SUSY has been the most viable extension of the standard model for quite a while. I don't think anyone would argue that it's not worthwhile to look for. Experimentalists trigger on all sorts of other things however.

SUSY favors a light Higgs, by the way. (The tree level prediction is around the Z-mass, I think, which is what's being referring to. Loop effects push it up.) In fact, the nondiscovery of the Higgs below 115 GeV or so has begun to push SUSY into the realm of fine tuning. Push it up too much more and the model loses a lot of its appeal.

The equation of string theory and SUSY is not a good idea. There are plenty of reasons to believe SUSY completely irrespective of strings. As you allude to, the two major ones are that it seems to protect the hierarchy of the weak scale versus the Planck scale (although I think this gets a bit iffy when you actually break the thing). The second is that, in the standard model, if you run the couplings up a ways, they almost unify, but not quite. If you add in SUSY, this agreement gets a lot better. If you believe that it's too much a coincidence for the coupling to almost unify, then the fact that SUSY gets them closer is a big deal. Unfortunately, apparently, the two loop calculations spoil this a bit. Nonetheless, it's still better than the standard model.

I'm rather surprised by the statement "The GUT and supersymmetry ideas have been intensively investigated, but don't lead to any convincing predictions or compelling picture." This is certainly false. The pictures are very compelling. The unification of the couplings points towards GUTs. The SO(10) GUT has a number of nice properties. GUTs also make predictions, the most important being proton decay. For this reason, the nonsupersymmetric SU(5) GUT is pretty much dead. IIRC, the supersymmetric SU(5) GUT isn't doing so well, either, though there may be more room for fudging on that.

SUSY also makes predictions like the light Higgs and presents a very compelling picture. If you think that the only reason people work on supersymmetry is because string theorists need it, trust me when I tell you that the idea is laughable.

The fact is that if you can come up with an interesting extension of the standard model, people would be very interested in. Precision electroweak measurements strongly constrain what you can do. For example, I always thought technicolor was a very pretty way to resolve the hierarchy problem and get rid of the Higgs kludge. Unfortunately, it has also been almost completely killed by experiment.

I also want to note that I don't think anyone has a problem with the infinities in individual diagrams in QFT. There are prescriptions which avoid this, but the regularization procedure both works and is understood. This is not to say that we understand QFT. I don't think we do. I'd love to replace it with something better defined. There's a million bucks waiting for you if you manage to do it, so good luck.

Lastly, I'm sort of astounded how you manage to draw such sweeping conclusions from such minor issues. You want to believe that the field is much more dogmatic than it really is. Nobody's abandoned much of anything, much less "the main argument for supersymmetry". In fact, what is being done is that people are exploring new directions. I would think that you would think that this is a good thing. For a long time, people thought that naturalness was a useful criterion for model building. If some version of the anthropic principle turns out to be correct, then we can abandon naturalness for certain (but *not all*) parameters. This opens up the possibility of new, possibly, interesting models to explore. That's what Nima and Savas are doing.

I like exploration. Don't you?

21. **Steve**
May 21, 2004

It looks increasingly likely that supersymmetry (and supergravity) has had its day and it is probably not a true symmetry of nature, even in broken form. Nature (or god if you prefer) is not obliged to follow the aesthetic ideals of mathematicians/physicists and might not even care about mathematical beauty/elegance as much as we do. If anything, this universe is looking more and more messy, inelegant and confusing! Personally though, I always found the idea of pairing off known bosons and fermions with superpartners like "squarks" artificial and I would bet money that none of
these "sparticles" are ever going to show up in any upcoming experiments. This is especially serious for string theory and "M-theory", where supersymmetry is required of course in order to wield any analytical control (and to get the dimensions down to a mere 10.) It was the Green-Schwarz superstring that started the whole string craze back in 1984 after all. Before that nobody simply cared. Even if superparticles did turn up in future experiments it does'nt prove string theory but does make it more likely; but without supersymmetry somehow existing in nature the work of the past 20 years all crumbles–back to the much maligned old bosonic/tachyonic string in 26 dimensions. In effect string theory sinks like a lead hippopotamus without supersymmetry. Of course string theorists will conveniently take refuge in arguments that supersymmetry is manifest only at immense energies
just like they are now taking refuge in cop-out anthropic arguments. Supersymmetry and the consequent superstrings were indeed powerful and promising mathematical ideas and were worth pursuing, but are wearing a bit thin now after 20 years.

22. **JC**
May 21, 2004

Is there a possibility that the only reason why quantum field theory and renormalization is "accepted" these days, is that nobody else has found a better theoretical framework which does NOT produce garbage calculations? It seems like any framework that increases the amount of calculational labor involved, just to get the same results as quantum field theory, largely gets ignored for the most part after awhile. Why bother with these "alternative" frameworks when they reproduce the same results (with many more times the labor involved) as ordinary quantum field theory?

If quantum theory is more "fundamental" than classical physics, why do we still use the framework of Lagrangian dynamics as a "stepping stone" to eventually do quantum field theory? Is this just another case of "nobody has found a better way to do things"? Are there any "intrinsic quantum" frameworks which do NOT involve the intermediate step of using Lagrangian dynamics with an "ad-hoc" quantization prescription?

Are there any better quantum entities to calculate, besides the S-matrix?

Should we even believe that quantum theory retains it's present form, when we go to higher and higher energy scales?

23. **Peter**
May 21, 2004

One thing I've wondered about is how heavily the search for supersymmetry is biasing the data analysis or even experimental design of detectors. In the worst case it might affect the design of the triggers. Presumably experimentalists have a healthy enough skepticism about theory that they are likely to not believe in the supersymmetry picture too strongly.

Still, maybe they should be made aware of exactly how tenuous the argument for supersymmetry really is.

24. **Alejandro Rivero**
May 21, 2004

A recent worry I have got is to wonder if SUSY was even biasing the results of experiments. Some years ago, a small signal was detected for a charged Higgs at L3 in CERN in a quark channel. Regretly it was 69 GeV thus too low for MSSM. Fast, it was averaged with measurements in *lepton* channels so the effect went down from 3.5 to less than 2 sigmas; and then the next edition of the pdg claimed a 95%confidence level above 75 GeV… I am thinking that all this "manoeuvre" was justified only if the faith in SUSY was (is) so strong as to be sure that H+ runs well above the Z0 mass.

25. **Chris Oakley**
May 21, 2004

Hello Matthew,

We are digressing from the main topic, so when I have had a chance to collect my thoughts, I will answer you by e-mail.

One last thought in regard to the main topic: if we are marking the demise of supersymmetry, then I personally do not mourn its passing as like Twistor theory or Strings it is an interesting mathematical idea that never had much of a connection with physics. Superfields & superspace were interesting to study, but at the end of the day one ends up doing much the same things as before, only with less of a prospect of explaining reality.

As for mass hierarchies, this never bothered me anyway. Worrying about this problem, if it is a problem, is premature.

26. **Matthew Nobes**
May 21, 2004

Hi Chris

Your points about QFT seem wrong to me… I'll grant that there is no mathmatically rigorous construction, but each of the points you make don't impact on that.

"1. It is only a theory of scattering. The
much-vaunted calculation of the anomalous
magnetic moment of leptons takes the Compton
scattering of a single photon as representing
the interaction of the lepton with an external
magnetic field without satisfactory
justification."

Well the first justification is that it agrees to many decimal places 🙂 But more to the point, you can consider the scattering off of a heavy Dirac lepton (say the muon) and take the limit as the mass of the heavy lepton goes to infinity. As far as I know, there's nothing wrong with that.

"2. No theory of bound states exists, so how can
one claim the prediction of the Lamb shift in a
single-electron atom as a success?"

This is simply untrue. One can extract energy levels, for example, using lattice field theory. There is an algorithmic way to extract bound state properties from the path integral, we just need big enough computers.

As for the Lamb shift, IIRC you start with Hydrogenic wave functions and consider perturbations to them, rather than the "textbook" plane wave states. Again, since the system is essentially non-relativistic this is justified.

"3. If, as seems to me to be likely, we need to
look beyond local field equations to cure the
infinities, then I would much rather not do it
with this framework. It is too messy,
complicated and self-contradictory."

It's unclear which infinities you're worried about. The diagrammatic infinities are cured by renormalization. Thanks to Wilson (and others) we actually understand this fairly well.

27. **Peter**
May 21, 2004

Hi Erin,

Unfortunately I think the present situation is that we don't have any good ideas about how to extend the symmetries of the standard model. The GUT and supersymmetry ideas have been intensively investigated, but don't lead to any convincing predictions or compelling picture. Some new ideas are desperately needed.

28. **Peter**
May 21, 2004

Hi JC,

My own best guess about what will ultimately happen with QFT is that it will turn out that a certain class of them, including the one describing the real world, can be interpreted as a formalism for dealing with representations of certain infinite dimensional symmetry groups, including the gauge and diffeomorphism groups. The idea is that there is some fundamental geometric structure, it has an infinite dimensional symmetry group with a distinguished representation, and this is the underlying mathematical structure that the standard model QFT is related to.

I posted a preprint about this a year or two ago, which hasn't attracted any interest, partly for the quite good reason that the ideas I discuss are too vague and ill-formed. I'm working on a much more specific version of all this that I think I more or less understand now in 1+1 d, hope to have something written about it this summer.

29. **erinj**
May 21, 2004

Hi Matthew,

Thanks for pointing out Bondi's name – to be honest, I wasn't sure if he was still alive or not (I always think of him being stuck in the era when Fred Hoyle was around), so I thought this was another Bondi, not the Bondi.

I'd forgotten about CMBR when I made my comment. Isn't this evidence all a bit vague still?

After I read up on MOND (Modified Newtonian Dynamics), I became much more skeptical about dark matter. MOND certainly seems a viable alternative, and seems to be at a nascent stage where an empirical rule makes predictions which appear to agree in most cases with observed galactic rotation curves, although the physical origin of this law is presently unknown (but hey, we've been here before, with things like Balmer's formula, etc., which do work and ultimately are consequences of a physically meaningful theory).

Aside from this, if supersymmetry is soon to be abandoned as you suggest, Peter, then what other ideas are there that might unify the coupling constants at high energy? I'm not aware of any, and without SUSY or other symmetry principles, how else does one extend the Standard Model?

30. **Chris Oakley**
May 21, 2004

Peter –

My own investigations do not contradict what you just said. But good old Feynman-Dyson perturbation theory is IMHO an obstacle to further progress. As well as the reasons given by me at (boring) length elsewhere, let me add –

1. It is only a theory of scattering. The much-vaunted calculation of the anomalous magnetic moment of leptons takes the Compton scattering of a single photon as representing the interaction of the lepton with an external magnetic field without satisfactory justification.

2. No theory of bound states exists, so how can one claim the prediction of the Lamb shift in a single-electron atom as a success?

3. If, as seems to me to be likely, we need to look beyond local field equations to cure the infinities, then I would much rather not do it with this framework. It is too messy, complicated and self-contradictory.

I do not think that quantum field theory is wrong *per se*, but higher standards of mathematical consistency need to be demanded.

31. **JC**
May 21, 2004

Are there any promising candidates at the present time that could possibly replace the quantum field theory framework?

It appears ideas like SUSY, string theory, supergravity, Schwinger's source theory, analytic S-Matrix theory, stochastic quantization, geometric quantization, Epstein-Glaser construction of the S-Matrix, Wightman's axiomatic quantum field theory, Haag's algebraic/local quantum field theory, loop quantum gravity, Isham's category theory approach to quantum theory, Penrose's twistor theory program, etc … all appear to be "stalled by potholes" of various sorts and are either going out of style and/or they only have a handful of supporters that nobody really pays attention to them outside of their circles.

It seems like anything that isn't constrained by experimental data, becomes governed by trendiness and ideology. Perhaps Freeman Dyson's quote is really telling:

"Physics is littered with the corpses of dead unified field theories."

32. **Peter**
May 20, 2004

I don't think we've reached a final understanding of what QFT exactly is, especially in 4d. But there is so much physics that the present QFT formalism correctly predicts and there is so much beautiful mathematics related to it, that I think there is something very right about the formalism. Whatever replaces it in the long term is likely to be so close to it that it will still be called QFT. So I think it is kind of a different situation than things like supersymmetry and string theory, where the whole concept will likely have to be thrown out.

33. **Chris Oakley**
May 20, 2004

I notice that QFT is on your list of "well-tested" theories.

What is well-tested is not quantum field theory. It is a calculational recipe centred around Feynman graphs, to which mathematically meaningless infinite subtractions are applied when pathological divergences show up.

"Quantum field theory" is too grandiose a name for this arbitrary and unattractive setup, and I when I hear mathematicians condoning it, I wonder whether we'll ever progress beyond it.

34. **Peter**
May 20, 2004

I'm no expert on cosmology, but from everything I've seen the evidence for the standard picture of cosmology is a lot stronger than the authors of this statement are willing to concede. It seemed to me that the WMAP data was in close agreement with what was expected, and there was no reason for this to be true if the standard picture is completely wrong.

One problem is that it is unclear what is meant by the "Big-Bang model". If you include all the conjectural stuff about inflation and dark matter, it probably is true that it hasn't been convincingly tested, but the idea of an expanding universe does seem to have passed quite a few tests.

From my experience with string/supersymmetry, I'm inclined to be sympathetic to claims that there are problems with certain orthodoxies in physics. But I've also seen that there are quite a few people who reject not just the more speculative parts of physics, but large parts of well-tested theories also (e.g. rejecting not just string theory, but the standard model, QFT, even quantum mechanics). This statement sounds to me suspiciously like something that may be along those lines.

35. **Matthew Nobes**
May 20, 2004

Well, I don't know about that cosmology statment. There are some names of respected people I recognize on there (Bondi, for example). There are also a few crackpots though (Arp, Marmet and Lerner) who have been throughly debunked in the past.

The statement seems rather strong as well. Certianly the Big Bang theory has had lots of quantitative sucesses. The microwave background was a predicition, as was the abundence of light elements. I don't think those have been seriously challenged (though I'm by no means up to date on the field).

As for dark matter, there are a few different observations which point to it. If I recall correctily they're even roughly consistent. That is, if you determine the dark matter density by looking at galactic rotations and by cosmological observations they come out roughly the same.

36. **erinj**
May 20, 2004

I realize that this isn't (perhaps) directly related to the topic under discussion here, but I've just come across this suprising open letter on big bang theory, at

http://www.cosmologystatement.org

and the first sentence of it made me think about string theory and supersymmetry, as theoretical proposals which may never yield observable or measurable predictions. I quote:

"The big bang today relies on a growing number of hypothetical entities, things that we have never observed– inflation, dark matter and dark energy are the most prominent examples,"

and

"What is more, the big bang theory can boast of no quantitative predictions that have subsequently been validated by observation."

Are the signatories of this letter doing the right thing? (I myself only recognized the names Woodward and Narlikar amongst them.) Might there be similar statements in the future for supersymmetry and string theory?

37. **Peter**
May 20, 2004

Perhaps one supposedly finite theory would be N=4 super Yang-Mills, where the beta-function is zero, so there is no coupling constant renormalization. The superconformal invariance of the theory may eliminate the usual sources of infinities. I believe there are also similar examples with N=2 supersymmetry.

The other "finiteness" has to do with finiteness of Feynman diagrams at 1, 2, etc. loops. For a while people believed that N=8 supergravity might have all terms in its Feynman diagram expansion finite, but now the belief seems to be that at high enough order one gets infinities.

38. **Chris Oakley**
May 20, 2004

Your post prompted me to dig out my Supersymmetry notes from about 1982. One of the promises made by Peter West at the time was that Supersymmetric theories were "finite". That would in itself be a reason for studying it, but the two pages I have on "Perturbation theory in Superspace" looks depressingly like the familar infinity-plagued Feynman-Dyson perturbation theory. I am wondering therefore whether this finiteness is mere wishful thinking. Do you or anyone else who might be reading this know more about this?

New TopCites

May 23, 2004
Categories: Uncategorized

For many years now the SPIRES database at SLAC has been used to produce a list of the most frequently cited papers during each year. Since 1997 Michael Peskin has been doing this, while at the same time writing up a description of what is in the 40 or so most popular papers, together with comments on what this data shows about trends in particle physics. The 2003 edition of Peskin's review has recently appeared.

Peskin notes that SPIRES has begun indexing more astrophysical papers during the last two years, and many particle physicists have turned their attention to cosmology. He has expanded the number of top papers he reviews from 40 to 50 to take into account the greater coverage of the database.

The most frequently cited article, this year and every year, is the Particle Data Group's "Review of Particle Physics" compilation of experimental particle physics data. It is conventional for experimental papers to often refer to this instead of to the original papers. This year the number two and three positions are held by papers from the WMAP experiment, with number four the original results on high redshift supernovae that indicated a non-zero cosmological constant.

The first particle theory paper is the Randall-Sundrum one at number five, and Maldacena's AdS/CFT paper is at number seven. For many years the top part of this list was heavily dominated by relatively new string theory papers, but the situation is now dramatically different. The highest-ranked post-1999 paper is one about PP-waves at number 18, the next is one at number 37 by Ashoke Sen about time-dependent backgrounds. The only other post-1999 paper in the top 50 is the Dijkgraaf-Vafa paper about supersymmetric gauge theories, which is at number 39.

This list provides pretty conclusive evidence that the field of particle theory more or less flat-lined about 5 years ago, with only a small number of minor blips of brain activity since then.

There's also a cumulative list of the most highly cited papers of all time. Here the dramatic movement one can watch is the speed with which Maldacena's paper accumulates citations. At the end of last year it was at number 6 on the list of all-time most frequently cited papers; it has now moved to number 5 and soon will overtake number 4. Within a couple of years it should be at number three, only outranked by the Review of Particle Properties and Weinberg's original paper on the Weinberg-Salam model.

Comments

1. **Peter**
   May 24, 2004

   The SPIRES numbers are probably quite accurate for the last ten years or so (and thus for the Maldacena paper), since just about all particle papers are at the arXiv. Their coverage is less good for older papers.

   In any case, people sooner or later stop referring to older papers as they become part of the common lore of the field. I'm suprised that so many people continue to refer to Weinberg's paper, since the standard model is now so well-entrenched.

2. **JC**
   May 23, 2004

   Is there a possible "undercounting" of older papers that have become "common knowledge", such as Weinberg's 1967 lepton paper? It seems like once something becomes common knowledge or "textbook material", people seem to cite it less and less. These days how many people still cite the old Schrodinger and Dirac papers from the 1920's on their respective equations?

   How many citations of these older papers would be missed in the spires database? It would be interesting to see how many citations show up for them in the citation indicies, coming from papers which do not show up on spires (ie. long forgotten papers and/or papers published in obscure journals that nobody cares about).

   Would it be a safe bet to say that the citations on the Maldacena AdS/CFT paper on spires, has had a reasonable and accurate tracking of it? It would seem that almost every paper that would have cited it, would have been submitted to the arxiv preprint server. Perhaps even crackpot papers (that slipped through the cracks onto arxiv) which cited it, would have also been tracked too.

Not Even Not Even Wrong

May 24, 2004
Categories: Multiverse Mania

I find it just completely unbelievable that anyone thinks [this kind of thing] is science.

Comments

1. **Alejandro Rivero**
   May 28, 2004

   My view is that the "anthropic principle" is a way to camouflage experimental input as if it were theoretical input. Surely most of the papers can be driven to a more standard format, separating really theoretical restrictions from just empirical restrictions. If they prefer this way, it is perhaps because the theoretical restrictions are not, say, very spectacular by themselves.

   I could write a paper claiming that atoms of, well, 82 protons, are necessary for human beings, and that the perturbative stability of this kind of atoms ask for a fine structure constant smaller than 1/82. But nobody should mistake such paper with a theoretical work.

2. **Thomas Larsson**
   May 26, 2004

   Maybe you can argue that human life requires that the aether wind be neglible, which would explain the Michelson-Morley experiment.

3. **JC**
   May 25, 2004

   Did anyone ever try to cook up an "anthropic" explanation for something like the Balmer spectra of hydrogen, before the Bohr model ever existed?

   It would be interesting to see what sort of phenomena had "anthropic" explanations more than century ago, which today have relatively well established theories and experiments.

   It seems like "anthropic" explanations are not much more convincing that saying something happens because of "God's will" or that "it's the work of Satan" type of stupidity.

4. **Peter**
   May 25, 2004

   Weinberg's argument is just that the cosmological constant can't be too large, or else galaxies won't form. So he is telling you that even if you have no idea what causes the cosmological constant, there is an allowed range it can be in. People are calling this a "successful prediction" because the observed value is (by some measure) in the middle part of that range. I don't think this deserves to be a called a scientific prediction because:

   1. It is not based on understanding anything about the phenomenon at issue, it is based on claiming utter ignorance about it.

   2. It can never be made precise, because you really are not actually understanding anything.

   The idea that one can make "predictions" by looking at all vacuum states and saying we should be in a generic one, could in principle be falsifiable, if you had a good understanding of what all the vacuum states are. The problem is that you don't really know what string theory is well enough for this to be a well-defined problem. Susskind can thus go and do silly things like claim that if you stack enough branes on the KKLT construction you can evade falsification.

   There's a pretty standard human behavior of "group-think", where each member of a group of people starts doing something stupid because others around them are doing it. This has a lot to do with the whole string theory phenomenon, now an even more bizarre version of it seems to be going on at Stanford. Susskind devotes a lot of his time to proselytizing for this nonsense, evidently he's even writing a popular book to promote these ideas more widely.

5. **erinj**
   May 25, 2004

   Isn't astronomy the only physical science where quantitative predictions within "a couple of orders of magnitude" are thought of as being "successful"? Are such predictions acceptable in cosmology?

   Surely the "anthropic principle" is a misnomer, as even if it is falsifiable, it hasn't been established as a scientific principle – so I certainly agree, Peter, that it isn't science. OK, it's nonsense. It reads even worse if you call it, as Dimopoulos does, "habitability criteria". Now that's just silly!

   Isn't there a pattern here as well? Susskind is at Stanford. So is Dimopoulos, Kachru and Kallosh. Andre Linde is there too. So we have KKLT, the `anthropic virus', Susskind's "landscape" and Linde's `universes in the lab' all emanating out of Stanford. Are all these things the result of infighting and petty competition between these Stanford guys? Are they trying to out-do each other? Something in whatever they drink there?

6. **D R Lunsford**
   May 25, 2004

   What is Weinberg's 1987 paper like?

7. **serenus zeitblom**
   May 25, 2004

   Well, the desperation revealed there at least shows that the anthropic principle *is* science. Clearly LS is worried that the AP can be falsified. The only catch is that it *has* been falsified by the longevity of the proton.
   Actually this longevity-of-the-proton argument has been around for a long time, and it is the reason I never took anthropism seriously. I thought that this was well-known, and I am very puzzled that it is being bandied about now as if it were some sensational new discovery….anyway the point is that anthropism is clearly wrong, and LS's paper has the virtue of advertizing this obvious fact!

Other People's Stuff

May 27, 2004
Categories: Uncategorized

It's always a little worrying when this happens, but sometimes I find myself very much agreeing with at least parts of what Lubos Motl has to say. For example see this recent posting to sci.physics.strings. In it Motl argues that

"I think it is premature to try to construct this major framework that would explain the character of vacuum selection and very early cosmology in string theory"

and

"So my belief is that we will have to understand the nonperturbative structure of the stringy arena using some new universal definition of string theory – a definition that is both non-perturbative (reaches the strongly coupled regions) as well as background-universal (reaches the geometries and non-geometries that are different from the starting one), and only afterwards, we will be able to start answering the stringy cosmological questions in a better context. Without this new tool, everything is just vague guesswork.

In my opinion, the research of string cosmology; stringy inflation; de Sitter space in string theory; scattering in backgrounds with non-standard causal diagrams; and all similar things that have essentially be started by the observation of accelerating Universe back in 1998 – has led to a very small number of intriguing results. There is almost nothing non-trivial and mathematically intriguing going on here; there is as much output as much input we insert. It remains a combination of phenomenology and speculations where conjectures can rarely be clearly ruled out."

I've never really understood why there are fields of "string phenomenology" and "string cosmology" when the theory is still in a state that it can't reliably calculate anything. While I think it is wishful thinking to believe that if you understood string theory better it would reproduce the real world, at least Motl's is a consistent scientific position.

Motl is also a fierce opponent of the "anthropic" arguments that have become popular among string theorists. For the latest example of anthropic argumentation, see this posting at Jacques Distler's weblog.

Comments

1. **Thomas Larsson**
   May 30, 2004

   How depressing!

   I found my info on the web a couple of months ago, but was evidently outdated.

2. **Peter**
   May 29, 2004

   Hi Thomas,

   I was going by a famous graph I've seen in many places, e.g. here, which I read as saying that discovery of a 115 Gev Higgs requires

   1. Almost 2 fb^-1 for 95% confidence level

   2. About 5 fb^-1 for 3 sigma

   3. Nearly 20 fb^-1 for 5 sigma

   The most optimistic Tevatron Run II projections now seem to be that 2 fb^-1 would be available by 2006, 5 fb^-1 by the time the LHC turns on (late 2007), and it's unlikely they'll ever get 20 fb^-1 since they're not likely to run the machine for too many years after the LHC is up and running.

3. **Thomas Larsson**
   May 29, 2004

   Peter:

   I think you are overly pessimistic. I have also seen that 2fb^-1 is required for a 5-sigma discovery of the Higgs (whether combined or for each individual experiment is unclear to me), but surely you can get hints with less than that. People have been excited about 2.5 sigma signals before. It also takes a lot less statistics to rule out a light Higgs; a 5-sigma discovery will take until 2009, but it will be possible to rule out a light Higgs at 95% CL already in 2006. Now that is only two years away, which suggests to me that if the LEP signal is real, some kind of bump should be visible at 115 GeV very soon, if not already.

4. **Simplex**
   May 28, 2004

   "So my belief is that we will have to understand the nonperturbative structure of the stringy arena using some new universal definition of string theory – a definition that is both non-perturbative (reaches the strongly coupled regions) as well as background-universal (reaches the geometries and non-geometries that are different from the starting one), and only afterwards,…"

   Background-universality reads for practical purposes like a euphemism for background independence! Holy Smolin, Lubos, could this be a nod in the Loop Gravity direction?

5. **Alejandro Rivero**
   May 28, 2004

   Thomas,

   That one got a lot less of press coverage. It was hinted, preliminary, in hep-ex/0105057 and then dismissed progressively via statistical methods. See also plots in hep-ex/9909044 and hep-ex/0009010. The statistic dismissal was based, it seems, on adding other dissintegration channels where signal was not seen. Pretty much the same history than 115 Gev.

Probably a motivation to forget about this event is that a low Higgs+ it is not compatible with the minimal supersymmetric standard model. I had also forgotten it, until recently a model forced my neurons to expel these papers from my deep repressed memories 🙂

6. **Peter**
May 28, 2004

Grants generally aren't given on a year-to-year basis, more typically people are part of a group that has a grant that may have to be renewed every five years. So, if you have a permanent job, you don't need to worry about this each and every year, but every few years you very much need to worry about whether you've been publishing enough articles.

Postdocs and junior faculty are under a lot more pressure. Their jobs may be as short as one-year appointments, and they definitely need to be producing as many publications as possible as quickly as possible if they want to continue to eat.

There certainly are a lot of very routine, resume-padding articles appearing, but this is not specific to string theory or high-energy theory. Pretty much every academic field, even very healthy ones, have this phenomenon. What's scary about particle theory in recent years is not how many routine papers there are, but how few there are with any sort of interesting new idea at all.

7. **Peter**
May 28, 2004

With only 200 pb^-1 in Run II, the Tevatron can't see much that they couldn't see in Run I, where they had around 100 pb^-1. To start pushing up the mass limit on the Higgs above that from LEP, they need 2000 pb^-1 or so. They hope to get that over the next few years before the LHC gets going.

8. **Thomas Larsson**
May 28, 2004

What is the 68 GeV particle? I evidently missed it.

Yesterday there was a status report from the Tevatron on the arxiv. CDF and D0 now have an integrated luminosity ~200$ pb**-1 (per experiment), but evidently they don't see any deviations from the standard model. Does anyone know if this is significant?

9. **Alejandro Rivero**
May 28, 2004

No, I am pretty sure that the inertia of stringthings is independent of funding gravy. It is just that when people is trained with a weak mechanism of choosing goals, the herd effects happen (stock market jargon; no pun intended).

Perhaps experimentalists are more sensible to funding phenomena. I am still astonished about the 115 and 68 GeV particles in LEP2; they were so in hurry to finish the experiment that they did not got time to take more measurements there.

10. **JC**
May 28, 2004

Perhaps from an ulterior motive, it's a way of folks attempting to justify the government grant funding gravy train to string theory or particle theory in general?

Regardless of whether string theory is a legitimate or illegitimate field of research, is there any truth to the folklore stories that if you don't publish anything during a year you'll end up losing most, if not all, of your grant money for the next year?

Many papers that show up every day on the arxiv preprint server look like they aren't much more than the equivalent of "resume padding" for a researcher's CV. Perhaps it's not so surprising if it's papers produced by some 2nd or 3rd rate researchers at some 3rd or 4th rate universities, hoping to get tenure. Or for that matter, postdocs hoping to get an assistant professor job.

Slides from Davis Conference

May 29, 2004
Categories: Uncategorized

Slides used by many of the lecturers at the recent Davis mathematical physics conference in honor of Albert Schwarz are now online.

Comments

1. **Peter**
   May 30, 2004

   I'm just getting this from Lawson-Michelson, Spin Geometry, Theorem 8.4, Chapter I, where they also get the cover correct.

2. **D R Lunsford**
   May 30, 2004

   Hmm I thought the double (actually 4-fold) cover for SO(p,q) was Spin(p,q)…how did you pull out SL(4,R)?

   The Clifford algebra Cl(3,3) does have a "Weyl"-like representation

   Bmu = [[0, gamma_mu],[gamma_mu, 0]]

   B5 = [[0, -gamma_5],[-gamma_5, 0]]

   B6 = [[0, i],[-i, 0]]

   and since the gammas have a purely imaginary (Majorana) representation (for spacetime = —+) I can see SL4R coming in…

3. **Peter**
   May 29, 2004

   I don't really know anything about SO(3,3). It's not the conformal symmetry group of Minkowski space. Its spin double cover is not SU(2,2), but SL(4,R). The tricks Witten is talking about that get representations of SU(2,2) by "quantizing" C^4 and using the action of SU(2,2) on C^4 won't work for SO(3,3).

4. **D R Lunsford**
   May 29, 2004

   How does the whole argument change if we start with SO(3,3)?

5. **Peter**
   May 29, 2004

   He never really got to talking strings. In the last part of his talk he was just explaining his formula for gauge theory scattering amplitudes as integrals over 2d-subspaces D (actually algebraic curves of genus zero) in CP^3. You can try and interpret these curves as world-sheets of strings, but he didn't get into that in this talk.

6. **D R Lunsford**
   May 29, 2004

   3/4s of Witten's talk was fun (I think it's just a rehash of Penrose) but he lost me when he started talking strings. What was his point?

Anti-Big Bang Open Letter

May 30, 2004
Categories: Uncategorized

Sean Carroll has some comments about the anti-big bang petition. He also points to Ned Wright's explanation of what is wrong with various proposed alternatives to the big bang scenario. In particular this explains in detail what the problems with Irving Segal's "Chronometric Cosmology" are, something I'd always wondered about.

Segal was a very good mathematician, who did serious work on quantum field theory in the 50s and 60s. He's the "Segal" in what is sometimes called the "Segal-Shale-Weil" representation. Segal is a counter-example to Carroll's observation that, for the most part, opponents of the big bang are not very smart. Unfortunately, it seems that quite smart and otherwise reasonable people can have unshakable faith in ideas that don't work. Segal's student John Baez wrote up some of his memories of his advisor and his cosmological research.

Comments

1. **Jack Sarfatti**
   July 4, 2004

   On Jul 3, 2004, at 8:33 PM, Paul Zielinski wrote:

   Everyone agrees that the laws of GR are formally covariant under
   general coordinate transformations — or under the group Diff(4)
   of point set diffeomorphisms on a 4-dim pseudo-Riemannian manifold
   (which is subtly different).

   But this is not enough to give us *actual physical relativity*
   with respect to accelerated motion.

   The reversible tetrad map LNIF(P) LIF(P) does that

   i.e.

   guv(LNIF at P) = Eu^a(P)(Minkowski)ab(LIF)Ev^b(P)

   After all, in classical mechanics Newton's second law F = ma also
   holds *formally* in non-inertial frames; but this cannot amount to
   true relativity with respect to such frames, since the observed
   forces are regarded as "fictitious" — i.e., only apparent, a
   kinematical artifact.

   Bad use of language since we feel and measure fictitious forces same as gravity!

   So whether we do or do not have physical relativity depends not only
   on the formal symmetries of the theory, but also on the *physical
   interpretation* of the covariant laws.

   Einstein's original approach was to interpret such "fictitious"
   forces as real, based on his concept of the unified gravito-inertial
   field, described by the transformable/deformable metric tensor g_uv.
   This was at the core of his original concept of general relativity, as
   I have previously argued.

   Still true today.

   Einstein himself explained this very clearly in his earlier papers
   and in several books on relativity. I have all the quotes.

   If Einstein's math is interpreted differently — as some have
   proposed — then we lose physical general relativity, even while the
   formal general covariant character of the laws is left undisturbed.

   Again I do not think these gyrations of the informal language that leave the formal equations alone and do not change any operational procedures and gedankenexperiments are important scientifically – at best a matter of psychology and cognitive style.

   So general covariance is not the same as Einsteinian "general relativity".

   Never said it was. It is necessary not sufficient. Puthoff seems to say in PV it is NOT necessary – an error.

   As I understand it, the current consensus in gravitational physics is
   indeed in favor of a NON-"general relativistic" interpretation of the
   formal theory.

   Show me with exact quotes.

   If I am confused on this, then so are most contemporary gravitational
   physicists. Even Wheeler writes that "'general relativity' is the name
   Einstein gave to his theory of gravitation". Weinberg and Feynman,
   to name just two others, thought that Einstein equivalence is a red
   herring — a mere heuristic tool that happened to lead to the current
   theory, but not now part of the correct interpretation of that theory.

I have not read Weinberg on this. I do not read Feynman that way. Cite specifics.

If there is no "general relativity", then there is in reality no physical relativity, except in some weak phenomenological sense (which latter, ironically, was Einstein's original 1905 view of SR as a: theory of principle").

The "beef" of GR is the TENSOR eq (under Diff(4) globally and O(3,1) locally)

Guv + /\zpfguv = – (8piG/c^4)Tuv("matter")

Therefore, I simply do not understand what you are saying.

Einstein pretty much implied all this himself: he viewed his proposed extended relativity principle as a natural development of his special relativity principle to encompass accelerated motion.

Right. That's the way I understand it.

If you only include observers in uniform relative motion and assume invariance of c under that limited group of transformations including global translations as well as all 6 space-time rotations you get 1905 SR. SR means do NOT locally gauge the Poincare group! It means do not locally gauge any space-time symmetry group under the constraint that the speed of light in vacuum is an absolute invariant. That c is an absolute invariant does NOT imply it is an upper speed limit to anything. To get that, one must make something like an Arrow of Time additional postulate that future causes of past effects is impossible. That is an entirely different story beyond relativity as simply a symmetry theory of space-time.

If you include COINCIDENT observers at SAME event P (i.e. in tiny ball centered at P) in arbitrary relative motion then you get GR(1916) relative to that choice of connection field that comes from ONLY locally gauging the 4-parameter translation sub-group T4 of the 10-parameter Poincare group.

Now, Einstein did NOT historically derive GR that way because the modern understanding of the organizing idea of "local gauge invariance" did not exist until after he died.

If further, you locally gauge the 6-parameter Lorentz group O(1,3) you get an additional torsion field piece to the connection field for parallel transport of tensors (and spinors using Penrose-Newman) along vector fields in the base space. You now have a larger local symmetry group than GR's 1916 Diff(4)xO(1,3). If you go even further and locally gauge the full 15-parameter conformal group, or even the 16-parameter GL(4,R) you will get even a bigger connection field with new physical consequences to be explored.

But it now looks like this aspect of his program was not successful.

I think it has been extraordinarily successful.

Of course, the term "general relativity" has in the meantime been quietly redefined to mean something quite different: the reciprocal influence of gravitating matter on the vacuum, and vice versa.

I thought Einstein always thought of it that way – I mean from at least ~ 1916 on? I am not up on the detailed history of how his thought evolved. What he may have said between 1905 – 1916 is not really relevant.

Even Einstein abandoned "Mach's principle" (really a hypothesis) by 1920, since it clearly entails instantaneous action at a distance in order to explain inertial phenomena — which of course leads us to a very different view of inertia as arising locally from a matter-vacuum interaction.

Agreed, Mach's principle is not a necessary part of GR even though it heuristically motivated Einstein. Also the recent discovery that 96% of the stuff of the universe is not "matter" as Mach and Einstein thought of it means that the whole idea of Mach's Principle rests on very shaky ground and must be re-evaluated in the light of new surprising observations.

The ghost of the departed Lorentzian ether. Quite a different kettle of fish.

Paul

"Ether" is back in, although not the old Galilean group version. "Ether" like "tensor", "spinor", "connection" are all relative terms defined relative to a choice of symmetry group G.

P.S. I have been revisiting SR, and I think I now may have a bulletproof version of the clock paradox that I'd like you and Hal to take a look at.

Would you be willing to do that?

Depends what you mean by "clock paradox" – time dilation is a proven fact in many experiments.

I know this must sound like a crank perpetual motion thesis — but I'm serious.

Jack Sarfatti wrote:

General coordinate transformations handle all – so I do not understand what you mean.
I think you are confused here.
True, given any symmetry group G you can make the laws covariant under G.
GR deals with a special choice G = Diff(4).
Diff(4) handles LNIF –> LNIF'

Also it includes EEP tetrads LNIF LIF

On Jul 3, 2004, at 1:28 AM, Paul Zielinski wrote:

I meant physical relativity of all motion, including accelerated motion.

The general principle of relativity was initially supposed by Einstein to be modeled on the special principle and was supposed to be an extension of it.

That's not the way it turned out — or at least that's what I understand to be the current view of the matter.

On Jul 3, 2004, at 9:07 PM, Paul Zielinski wrote:

Jack Sarfatti wrote:

The general principle includes the special principle.

Only if the special principle is re-interpreted *ad hoc* to bring it in line with the general principle as currently interpreted.

OK – what's wrong with that?

Jack, here you are disagreeing not just with me, but with most contemporary authors.

This is all explained in Ohanian & Ruffini, "Spacetime and Gravitation", which I believe you have.

What pages specifically?

Who now actually believes with Einstein 1907-1916 that physical gravitation is simply a form of variable frame acceleration controlled by the distribution of matter?

I do if you change "simply" to "essentially" and if you change "matter" to "matter and exotic vacua."

Do you?

Of course.

When curvature is zero everywhere when special relativity works globally, i.e. there exist global inertial frames GIF

OK.

When there is curvature the special principle works locally subject to the 2 restrictions I mentioned previously.

In general, only at a spacetime point.

More precisely in a neighborhood of space-time point P of scale L small compared to scale of local radii of curvature. Since the latter at surface of Earth is ~ 1AU that is not much of a restriction on L for terrestrial measurements.

Also L >> Lp* which is usually taken as 10^-33 cm though it may be larger.

What this means is that the predictions of SR in an LIF are *empirically compatible* with those of GR when the LIF is contracted to a point.

No to a "ball" and here at Earth it can be a pretty big ball. You need to put this "point" thing into proper perspective with numbers.

Where there is non-zero Riemann curvature (i.e. gravity) in any finite volume of spacetime, they are only approximately compatible.

As explained in detail in MTW for example. Obviously the 4th rank curvature tensor is NOT generally zero and that includes LIFs as well as LNIFs – but its practical effects on surface of Earth are very tiny and for a majority of practical purposes are ignorable.

Of course this does not mean that even in such a contracted LIF the predictions of SR *match* those of SR — they are only a subset. For example, SR makes no predictions regarding tidal forces, even if we extend SR to handle accelerated frames. Yet according to modern GR, tidal effects may be empirically detectable everywhere in an LIF.

What's your point? It is trivial that SR is a sub-theory of GR and that SR needs GR corrections if one does precise enough measurements. All covering theories transcend the theories they cover. All this confusion about "aether" for example is because people try to use SR outside of its proper domain of validity. Our universe has a Hubble flow in which absolute velocity and absolute global cosmic time are practically and usefully defined in terms of the cosmic black body radiation isotropy and temperature respectively. This is no different from fact that non-spherically symmetric ferromagnets exist even though their Hamiltonians are spherically symmetric. The particular solutions do not share the symmetries of the dynamics! Not all atomic electron states of hydrogen are S states! In particular, when the ground state of a complex system does not share all the symmetries of its dynamical action we say the symmetry is spontaneously broken. This same thing happens with the cosmology of our universe! This is common indeed ubiquitous! Read PW Anderson's "More is different" and other papers in his "A Career in Theoretical Physics" (World Scientific) – worth buying.

Note let a,b be LIF indices and u,v be LNIF indices both in small neighborhood of same local event P

Ruvwl(P) = Eu^a(P)Ev^b(P)Ew^c(P)El^d(P)Rabcd(P)

Curvature is a local field measurable in principle in the LIF.

OK.

E's are the tetrad components, i.e. local fields

In globally flat space-time the E's are Kronecker deltas &au^a, distinction between a's and u's disappears – degenerate limit e'u(P) = Eu^a(P)ea

[e0, e1,e2,e3] is basis for a LIF at P

[e'0,e'1,e'2,e'3] is basis for a "coincident" LNIF at P

OK.

INTRINSICALLY

e'u(P) = ea&u^a + Lp*^2(Macro-Quantum Coherent Vacuum Phase),u (NEW to my theory

,u is ordinary partial derivative

guv(P) = nuv(Minkowski) + (1/2)[e'u,v(P) + e'v,u(P)] = Eu^a(P)nab(Minkowski)Ev^b(P)

So far all this is for usual torsion-free connection.

So your "curvature field" is directly determined by the coordinate derivatives of the macroquantum
phase of your virtual BEC?

Yes, of course. I have been saying this over and over and it's in my two books from late 2002.
This is the elastic analog to Bohm's hydrodynamic constraint of IT by quBIT in

velocity of IT particle = (hbar/m)Gradient of phase of quBIT pilot waved)

I replace the quantum of circulation (AKA vorticity flux) hbar/m in 3D by the "Quantum of Area" in the 4D elastic world crystal lattice picture of Hagen Kleinert from Free University of Berlin.

The LIF ea with zero partial derivatives have dimensions of length in above formulae and guv is dimensionless.

If so, this looks like a flat-background quantum field model with a correspondence bridge to
"curved" Einstein g_uv.

Only FORMALLY not PHYSICALLY – important you make that distinction!

That is, there is no assumption of perturbation theory here.

In

guv(P) = nuv(Minkowski) + (1/2)[e'u,v(P) + e'v,u(P)]

In no sense do I assume

nuv(Minkowski) >> (1/2)[e'u,v(P) + e'v,u(P)]

The way Feynman does in his Lectures on Gravity using spin 2 quantum field on Poincare symmetry group background vacuum.

I am not doing that at all! Also these are SMOOTH MACRO-QUANTUM ODLRO functions. There will be a micro-quantum normal fluid spin 2 tensor quantum field on this smooth curved space-time background of course along with all the spin 1/2 and spin 1 quantum fields ALL together contribute to /\zpf!
But my theory is automatically BACKGROUND-INDEPENDENT and NON-PERTURBATIVE since guv(P) is a dynamical field from the beginning determined globally self-consistently in a self-organizing manner!

I'm curious: What in your model causes mechanical inertia? And what in your model explains, or
at least might explain, the exact proportionality of inertial and gravitational mass?

Exactly same as John Wheeler explains "Mass without mass" in his classic book "Geometrodynamics" with new feature Lp* ~ 10^20Lp on scale of 1 fermi to make the spatially-extended quasi Kerr-Newman micro-geons of the lepto-quarks out of exotic vacuum with "charges" as quantized vortex trapped flux fields not only U(1) but also SU(2) and SU(3).

The lepto-quark masses m ~ Vacuum Coherence consistent with Higgs mechanism – not Haisch's & Puthoff's random EM ZPF friction.

~ 1 Mev for first generation that dominates Omega(Matter) ~ 0.04 where total Omega = 1 the remaining 0.96 is ALL w = -1 ZPF exotic vacuum! Hence dark matter detectors silent in principle sans false positives.

Then use QCD Lite bag model (Frank Wilczek) to get hadronic masses M ~ 1Gev from lepto-quarks glued together again by /\zpf in a cascade process.

I have not proved consistency as yet. This is all heuristics based on my physical picture.

OK.

Z.

On Jul 3, 2004, at 1:28 AM, Paul Zielinski wrote:

I meant physical relativity of all motion, including accelerated motion.

The general principle of relativity was initially supposed by Einstein to
be modeled on the special principle and was supposed to be an extension
of it.

That's not the way it turned out — or at least that's what I understand
to be the current view of the matter.

Jack Sarfatti wrote:

I think the cutoff is much larger than Planck distance at scale 1 fermi in fact it is 1 fermi on scale of 1 fermi

i.e. Lp* = 10^20Lp on scale of 1 fermi to stabilize electron as an extended micro-geon.

On Jul 2, 2004, at 7:36 PM, Paul Zielinski wrote:

Jack Sarfatti wrote:

On Jul 2, 2004, at 12:19 AM, Paul Zielinski wrote:

I was under the impression that there is a class of v^3 ZPE
density distributions that are Lorentz invariant?

Yes, that's what I am alluding to below. However, that does not have a finite cut-off in it, which is the problem.

The nitpicking point I first raised here is really not relevant to the main issue,
which is what happens to LI when the emptically confirmed v^3 ZPE spectrum
(which I suppose is actually a v^2 density distribution) is truncated at the Planck
scale.

Everyone here (except perhaps TS) says that this would destroy exact LI.

Puthoff seems to be saying that such a cutoff would nevertheless not have currently
observable consequences, while Ibison is not so sure.

What do you think?

Z.

I say it will have observable consequences that should be looked for in angular correlations in Lambshift radiation for example.

2. **Joe Papp**
   June 30, 2004

   Guys:

   You can debate theory up your yin-yang.

   Check out reality at http://www.proton21.com.ua

   If anyone wants to whine about the lack of detail

   on making "coherent electron bunches", just try reading ALL of Adamenko's papers.

   It's not really that mysterious.

   Here's the point: If you can MODEL stellar processes with an actual EXPERIMENT…then you can begin to understand them.

   Forget all this "zero point energy" stuff, let's start with SHN's and see where we get from there.
   (We may get to ZPE, and to large amounts of antimatter…cheaply.)

   Dr. Papp

3. **Jack Sarfatti**
   June 22, 2004

   bcc

   Memorandum for the historical record on the emergence of metric engineering concepts.

   Dear Marc

   Mine is the only one of your proposals coming in which has even a ghost of a chance of working as practical metric engineering because:

   It only uses battle-tested mainstream physics, i.e. Einstein's GR, quantum theory, condensed matter physics of macro-quantum coherent order parameters described by P.W. Anderson as "More is different" that includes Andrei Sakharov's "metric elasticity" 1967 for emergent gravity as "generalized phase rigidity" of the local macro-quantum vacuum coherence order parameter. Also, unlike ALL of your proposals coming in, mine has a seamless connection with the major problems in physics today e.g. the dark energy/cosmological constant problem, the renormalization problem in quantum field theory (e.g. stability of electron under its self-charge), stability of galaxy, universal Regge slope of hadronic resonances, Ken Shoulders' "charge clusters." Dare I also mention the heretical 3 letter word "UFO"? No, I guess not. 🙂

   Hal Puthoff's competing program to mine, i.e. the "PV" dielectric analogy approach to GR is much further out of the mainstream and so far his published results disagree with actual empirical data. Hal tries to avoid standard tensor calculus with metrics, consequently he has yet to publish a model with a rotating source, and he has yet to show how his theory based on a special action principle can give a warp drive in the sense of Alcubierre and the recent paper by Visser et-al
   http://www.arxiv.org/PS_cache/gr-qc/pdf/0406/0406083.pdf
   Also Hal has yet to show how an applied electromagnetic Fuv field can change his vacuum K function by a large enough amount to make a practical difference for actual metric engineering. He cannot do it using orthodox QED, the effect is too small.

   Note that Visser and Lobo also make reference to dark energy in the context of a warp drive – probably first in a mainstream paper, but my two books from end of 2002 already have that idea clearly spelled out. Visser also bends over backwards to caution the reader not to get too enthusiastic about imminent technological applications since the "fact" of UFOs is denied otherwise he could not publish his paper – quite obviously.

   One important technical point on Visser's latest paper on warp drive

   He uses only

Guv = (8piG/c^4)Tuv

Not the equation I use which is

Guv + /\zpfguv = (8piG/c^4)Tuv

That I approximate as the pure exotic vacuum equation

Guv + /\zpfguv ~ 0

assuming G/c^4 ~ 10^-33 cm/10^19Gev

so that, with my subsidiary vacuum coherence equations

/\zpfguv >> (8piG/c^4)Tuv

Is the "metric engineering regime" of practical interest with

/\zpf controlled by an electromagnetic Au potential in a Bohm-Aharonov- Josephson gauge covariant "weak link" in the relative phase between a control macro-quantum coherent field and the vacuum coherence of virtual electron-positron pairs.

Ken Shoulders EVO data http://qedcorp.com/destiny/ExotcVacuumObjects.pdf seem to confirm this basic idea, as well as the absence of dark matter particles after 14 years of trying with only false alarms.

On Jun 22, 2004, at 11:42 AM, Marc G. Millis wrote:

PS The work is published in 3 books copyrighted and in the US Library of
Congress database.
The titles of these books are given in the xls, which I find extremely
difficult to understand BTW I mean what the categories even mean in some
cases. I am not used to dealing with USG bureaucratic jargon and dotting all
the i's etc. I ran into this same problem with Paul Murad at STAIF.

I appreciate the difficulty of converting one's work to fit the formats of others. Considering that I'm expecting dozens, perhaps hundreds of submissions, I need some uniform way to compare them. After I've had a chance to screen the submissions, I'll get back with you if yours needs adjustments to be considered for inclusion.

Marc

4. **Jack Sarfatti**
June 21, 2004

On Jun 21, 2004, at 9:34 AM, michael ibison a signer to the anti-Big Bang letter wrote:

"That I don't embrace BB is an accurate description. I do not hold a strong view on whether or not the currently accepted view is correct. That makes me a radical. Not because I say it is wrong, but because I am an agnostic.

Cheers,

Michael"

That's only because you have not kept up with advances in the field. I agree that prior to say end of 2002 the position held in that May 22, 2004 New Scientist Letter was slightly plausible and defensible as a long shot – but no longer. Without applying a double standard, the Baysean probability that the standard model with chaotic inflation, dark energy/matter is essentially correct is very close to 100% and getting closer all the time as new data comes in. What we have here is a debate on how to weigh the new data that you admit you are not very up on. I have been to 2 APS meetings on this topic in 2003 and listened closely to Mike Turner and Saul Perlmutter – the experimental work is beautiful – some of the best in the history of physics. This is a real turning point. It also explains the saucers, the emergence of Einstein's gravity from the cohering of random noise ZPF not from its friction, the stability of electrically charged elementary particles as spatially extended structures that shrink when hit hard, the stability of the galaxy, the universal Regge slope, Ken Shoulder's charge clusters, the Arrow of Time and even the emergence of consciousness in the many worlds. What more do you want? This is grand!

Strong prediction that can falsify my idea: Dark matter detectors cannot click, in principle, with the right stuff to explain Omega(DM) ~ 0.23 any more than the motion of the Earth through the Galilean ether can be detected with a Michelson-Morley interferometer in the domain of validity of special relativity where the scale of the relative phase measurements are small compared to the radius of curvature that the Sun makes at the position of the Earth in its orbit.

PS How do Marshall Trevor/Haisch, Puthoff, Rueda, Cole et-al explain observed sub-Poisson statistics of photon anti-bunching in laser light which requires negative probability if you use SED?

5. **Jack Sarfatti**
June 19, 2004

WHAT IS THE UNIVERSE MADE OF?
The emergence of gravity and dark energy/matter from the cohering of zero point energy.

JACK SARFATTI
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Abstract

Ordinary matter made from real on-mass-shell lepto-quark fermions and gauge force bosons only accounts for approximately 4% of all the large-scale stuff of our universe, which may be one of a infinity of parallel universes in hyperspace that we call ?Super Cosmos.? I propose that the remaining 96% of our universe consists of two forms of partially coherent exotic vacuum dominated by a condensate of bound virtual electron-positron pairs. Einstein?s gravity emerges from the variations in the macro-quantum coherent phase field of the condensate. This condensate is the inflation field in the large-scale cosmological limit. Both dark energy and dark matter are

simply residual total zero point energy densities that emerge from the vacuum condensate?s intensity variations. Approximately 73% is anti-gravitating zero point ?dark energy? density with equal and opposite negative pressure that is causing our universe to accelerate in its expansion rate. The remaining gravitating 23%, called ?dark matter,? is also zero point energy density with equal and opposite positive pressure found concentrated in large-scale structures like the galactic halo that prevents our solar system from escaping into inter-galactic space. Astrophysical scale geon structures of w = -1 dark matter simulate w = 0 CDM in terms of their gravity lensing. The electron, as a Bohm ?hidden variable? on the micro-scale for example, is a spatially extended structure whose self-electric charge repulsion, Casimir force and repulsive spin rotation are balanced by the strong short-range zero point energy induced gravity from its exotic vacuum core. The electron, and the quarks, shrink in size, up to a certain minimum, when hit with large momentum scattering transfers from strong space warping that makes their surface areas small compared to what they would be in flat space for a given radial distance. An experimental appendix by Ken Shoulders on “exotic vacuum objects” or “EVO” charged geons made from large numbers of electrons glued together by zero point energy is included. The zero point force holding as many as one hundred billion electrons together is not the QED Casimir force, which may even be repulsive, but is the entirely different strong short-range gravity force induced by the zero point energy by the entirely different process of Einstein’s general relativity omitted from the flat space-time QED calculations. These EVOs show anomalous motions and energies that seem to be examples of Alcubierre’s “warp drive” and “cold fusion” respectively.

A preliminary copy of the Ken Shoulders EVO experiments is at
http://qedcorp.com/destiny/ExoticVacuumObjects.pdf

Ken worked for years in USG Intelligence as a gadgeteer for James Bond types. He holds patents on tiny electronic devices and is a microwave expert. Some of his devices were crucial to the microchip revolution by Intel et-al.

6. **Jack Sarfatti**
June 16, 2004

Both Ibison and Soares are signers of the New Scientist letter. I am defending mainstream precision cosmology against this letter’s position. Herman Bondi from Cambridge, who also signed the letter, got very upset with me last night because of this.

I support intelligent debate on this topic of course. However, having attended two major APS meetings on the topic with Mike Turner, Saul Perlmutter talking at both, and I will also be at GR17 in Dublin, it is clear to me that you are fighting a hopeless rear guard action. In the course of doing so, you will be led to change your mind. Resistance is futile. I think Sean Carroll’s attitude is the objective optimum one – the best Bayesean estimate. 😉

Note best fit in latest Physics Today is a Big Rip Phantom Energy w ~ -1.31 (if I recall correctly) but it is ~ 1 standard deviation away from my hard prediction of w = -1 as an exact result. So we will see how new data affects this. Also recall my other hard prediction, no dark matter detectors will click with the right stuff that explains Omega(DM) ~ 0.23. That would be like Michelson-Morley showing the speed through aether – I mean once we remove curvature corrections from general relativity. There is some small discrepancy there as I recall, but it is my impression that it can be explained by GR?

On Jun 16, 2004, at 10:23 AM, michael ibison wrote:

Thank you for your note Domingos.

“I appreciate your comments and am interested in what you have to say.
Sean Carroll (according to Jack) cites the following alleged PREDICTIONS
made by BB:

‘How about acoustic peaks in the power spectrum of temperature fluctuations in the cosmic microwave background? And the polarization signal, and its spectrum? And the baryon density as deduced from light-element abundances agreeing with that deduced from the CMB? And baryon fluctuations in the power spectrum of large-scale structure? And the transition from acceleration to deceleration in the Hubble diagram of high-redshift supernovae? And the relativistic time delay in supernova light curves?’

I think I can contest the allegedly predictive role of BB for some of these, but would appreciate a view from someone more qualified. Would you be good enough to offer a short comment on each of them?

Best wishes,

Michael”

On Jun 16, 2004, at 12:35 PM, Jack Sarfatti wrote:

On Jun 16, 2004, at 5:23 AM, Domingos Savio de Lima Soares wrote:

16jun04

“Hello Jack,

Thanks for your message — enthusiastic message, I should say — defending the inflationary Big Bang model. I, and my students as well, have now material for much discussion. Thanks, indeed. Ned Wright’s web page is present in my own personal page for a long time now, I would like to mention. And I always recommend it for those interested
in cosmology. In any case, I added, after receiving your note on the “Errors” section, a special link for this particular section of Ned’s page. What you, Sean Carroll, Mike Turner and followers, need to explain is

1- Why the “dark cake”, Fig. 10 of Freedman and Turner (astro-ph/0308418), has only 0.5% that is actually observed? Remember, from the 4% of baryons,there are some 3.5% still dark, i.e., invisible, that is to say, we can’t see, measure, whatever. Do they exist or perhaps should we wait a bit more?”

I do not claim to be an expert in data analysis. That’s Turner’s et-al’s job. I seem to recall Turner saying that was mostly cold hydrogen gas? What is important however is the 96%!

“2- Take w=-1.”

That’s what my theory says it must be – both dark energy and dark matter. However globs of w = -1 dark matter will look like w = 0 CDM from far away in terms of gravity lensing. This is a crucial test of my theory BTW. No dark matter particles! We, I mean Ken Shoulders, seem to be seeing this phenomena on lab scale!
http://qedcorp.com/destiny/ExoticVacuumObjects.pdf

“What do you do with the so-called ‘cosmological constantproblem’?”

I have solved it! Vacuum coherence explains it. The dominant contribution to Vacuum Coherence is a condensate of virtual electron-positron pairs bound together by virtual photons all occupying the same center of mass bound pair state. The phase variations in this macro-quantum occupied single particle state give Einstein's metric guv field.

The incoherent estimate of Einstein's cosmological constant /\ is ~ 1/Lp^2

My theory says that the coherent value of the cosmological constant is

/\ = Lp^-2[Lp^3|Vacuum Coherence|^2 – 1]

The "equilibrium" is at /\ = 0 where the Vacuum Coherence is the Planck density.

Vacuum Coherence obeys a covariant Landau Ginzburg eq. that must be solved self-consistently with Einstein's eq

Guv + /\guv = (8piG/c^4)Tuv

"You know, the value implied by the inflationary BB model makes our universe 10 to the minus 10 second old and 3 cm large, which is definitely, and frankly, not observed."

Again this is no longer a problem – it is solved conceptually.

"3- Why not make science with a character, I mean, following strictly "scientific method" and listening to what Nature presents and we can see?"

This is polemics – not a valid objection. It is obvious to my mind, that the preponderance of evidence is on the side of mainstream precision cosmology with dark energy now that I have explained the cosmological constant paradox as a simple vacuum coherence effect analogous to the two-fluid model of superfluids. Einstein's gravity emerges as a simple ODLRO effect with general coordinate transformations as derivative from local phase transformations on the vacuum coherence field. Curvature and torsion are simply local compensating gauge fields from stringy "vortex" singular lines in the phase of the vacuum coherence where the intensity of the vacuum coherent scalar field(s) drop to zero. Space-time physics is local because of the locality of the ODLRO vacuum coherence.

"Of course, you may turn out to be right in the end, BUT — sorry fot the capitals — you are not with all that just now. So, it is perfectly normal that others have their chance also. That's the point.

Cheers, explain that,

Domingos"

==
Domingos S.L. Soares
Depto. de Fisica – ICEx
UFMG – C.P. 702
30161-970 – Belo Horizonte
==

Let's keep the censorship issue distinct from the physics content. I completely support your complaint of censorship and black listing of physicists. Censorship prevents the kind of debate we are having here. I personally agree with Sean Carroll that funding of the kind of alternative you profess is a bad bet. Up until ~ 2003 you may have had a valid case there, but no longer. The kind of cosmology you profess is dead in the water and will lose support rapidly with all the new data coming in. It's time to move on. The same is true for the two eccentric theories Hal Puthoff has been promoting in the media i.e. PV theory of gravity and random EM ZPF origin of inertia of lepto-quarks, which BTW cannot explain the rest mass of the neutrinos. Both gravity and inertia are like Siamese Twins. They BOTH come from vacuum coherence (e.g. Higgs mechanism). You cannot have real matter Omega ~ 0.04 without vacuum coherence. All you have then is pre-inflation false globally flat vacuum. Vacuum coherence is also the inflation scalar field(s).

7. **Jack Sarfatti**
June 16, 2004

The local pure gravity stress-energy density tensor is simply

tuv(Gravity) = (c^4/8piG)Guv

where Guv = Ruv – (1/2)Rguv

For ordinary vacuum

tuv(Gravity) = 0

But for exotic vacuum

tuv(Gravity) = (c^4/8piG)/\zpfguv

where /\zpf is a local scalar field

/\zpf = (Quantum of Area)^-1[(Quantum of Area)^3/2|Vacuum Coherence|^2 – 1]

8. **Jack Sarfatti**
June 16, 2004

Some wag quipped that mathematical physicists are mathematicians who cannot do first-class pure math and that theoretical physicists are physicists who cannot do experimental physics. John Baez is a mathematician pretending to be a theoretical physicist. Indeed, most of modern theoretical physics has been taken over by mathematicians in theoretical physicist's clothing. In this context, both string theory and loop quantum gravity are what Richard Feynman would call "Cargo Cult Pseudoscience" and what Wolfgang Pauli would call "not even wrong." Are string theory and spin foams et-al interesting as conceptual art? Most certainly, but they are not yet real physics and it is intellectually dishonest for Brian Greene to misrepresent them as important physics on NOVA.

9. **Jack Sarfatti**
June 16, 2004

Re: Physics Today conclusively laughable in light of May 22, 2004 New Scientist Letter

On Jun 15, 2004, at 10:32 AM, michael ibison wrote:

I haven't read the Freedman paper http://arxiv.org/abs/astro-ph/0308418 If Domingos has, perhaps he would be good enough to offer comments on it.

Jack Sarfatti wrote: The point here is that there is a preponderance of evidence from independent sources for the basic picture that essentially all the stuff of the universe is, at its core, exotic vacuum zero point energy density including Omega (atoms, radiation etc) ~ 0.04. Mundane explanations for anomalous dimming of Type 1a supernovae have been clearly eliminated observationally by the evidence. There will be more evidence. The ideas in the May 22, 2004 are not contenders to the objective mind without an ax to grind or a hobby horse to ride – assuming one can find such a mind? 😉

Not only that, but this large-scale picture solves unsolved basic problems of the particle physics,

I. Is the electron (lepto-quark) a point particle? NO.

Hence no more need for infinite renormalizations and no more need perhaps for using distributions and the need for regularization that plays such a big role in say Ashtekar's program of spin foams and weaves to make smooth space-time.

II. What stabilizes the spatially extended electron (perhaps with "tight atomic states" J.P. Vigier)? Zero point energy pressure. These are the Lorentz-Abraham-Becker stresses.

III. Why does the electron (and the quark) appear to shrink from 10^-11 cm at low energy imaging to ~ 10^-17 cm at high energy imaging? Strong space-warp from the exotic vacuum zero point energy core.

IV. Why Regge universal slope for hadronic resonances? Kerr-Newman "micro-geon" (e.g. A. Burinskii) solution in strong short-range gravity induced in the zero point exotic vacuum core.

Which interpretation of quantum theory is needed? Bohm's.

Does the quantum wave describe individual particles or only statistical ensembles? Individual particles.

Why does our solar system not escape into inter-galactic space? For the same reason that the electric charge on a single electron does not explode! i.e. the pressure from w = -1 zero point energy cores.

Note that the present "best fit" for dark energy is not w = -1, however, I say that more data will show w = -1. I also say that dark matter detectors will never "click" with The Right Stuff. These are two crucial tests that will falsify my theory.

Finally, look to the skies. Heads up. What do you see "Out There" Michael? How do they fly? Dark energy that's how.

Therefore, I have a clear story to tell that most people can understand compared to the alternatives. Also my story is clearly falsifiable in Karl Popper's sense.

Michael Ibison: Personally, I have no strong opinion on the matter of dark matter, but if you feel there is 'REAL SCIENTIFIC EVIDENCE', perhaps you could share what you think that is – i.e. what specific data do you find so persuasive?

Sarfatti replied to Michael who is one of the signers of the New Scientist letter:
http://arxiv.org/abs/astro-ph/0308418 Here is the evidence. It is more than satisfactory to my mind. Not only that, it explains what Hal Puthoff has been attempting to explain for at least 30 years.

BTW you need to distinguish "dark energy" from "dark matter" though I think they are essentially the same, i.e. net zero point energy density of negative and positive pressure respectively modulated by variable partial vacuum coherence, I am alone in that opinion. All of The Pundits think dark matter consists of real on-mass-shell particles whizzing around in space. I say that is a mistake. Dark matter, like dark energy, is a form of exotic quantum vacuum that is essentially 100% of all the stuff of the Universe. Even ordinary matter made from spatially-extended lepto-quarks (Bohm's hidden variables) is exotic vacuum quantum pressure gluing together shells of charge as I show in a recent paper with Ken Shoulders on "charge clusters" http://qedcorp.com/destiny/ExoticVacuumObjects.pdf still under construction. Note that Ken worked with Hal on this same problem for many years and has now obviously voted with his feet! The point here is that Hal has never written down the correct zero point energy/gravity relationship, which in the simplest Newtonian limit of Einstein's GR is the static Poisson equation, sans factors of pi

Laplacian of Gravity Potential Energy Per Unit Test Mass of Exotic Vacuum ~ c^2/\zpf

/\zpf = (Quantum of Area)^-1[(Quantum of Area)^3/2|Vacuum Coherence|^2 – 1]

Vacuum Coherence obeys a generally covariant Landau-Ginzburg equation with a Mexican Hat Potential.

10. **jason**
June 9, 2004

"Right, I get it. All the evidence counted in favour is "weak", and anything that is hard to explain is "heavy counter-evidence". It sure seems easy to be anti-big-bang."

WHAT EVIDENCE IN FAVOR?????? There is NO evidence that favors BBT. Only misinterpretations of data based on huge assumptions. I have only seen eveidence that says BBT must be wrong.

11. **Matthew**
June 1, 2004

"The CMBR is not predicted by BB, it is an assumed and tunable side-effect of nucleosynthesis and atom-building and subsequent thermalization of radiation, and also depends on a strict separation of EM and gravity. The WMAP data is open to any number of interpretations (I've seen some that go against BBw/I). Compared to this, the large scale "walling" and enormous filamentary structures actually *seen* are heavy counter-evidence."

Right, I get it. All the evidence counted in favour is "weak", and anything that is hard to explain is "heavy counter-evidence". It sure seems easy to be anti-big-bang.

12. **Chris Oakley**
June 1, 2004

Just found this

http://www.zarm.uni-bremen.de/2forschung/gravi/publications/papers/2003DittusLaemmerzahlSelig.pdf

.. so ignore my previous comment.

13. **Chris Oakley**
June 1, 2004

This comment is made in the context of knowledge that hardly extends beyond an introductory G.R. course 23 years ago, and should be treated accordingly, but following on from Danny's point about electromagnetism & gravity … is it possible to test the equivalence principle for charged particles? Obviously if it only applies when positive and negative charges are in exact balance then that would knock away one of the pillars supporting General Relativity.

14. **D R Lunsford**
May 31, 2004

Yes, I know the orthodoxy and cathechism, and how to genuflect 🙂 I prefer heresy because it is more interesting and contains a grain of hope.

15. **YHS taking a Break**
May 31, 2004

Quoth D R Lunsford :

The CMBR is not predicted by BB, it is an assumed and tunable side-effect of nucleosynthesis and atom-building and subsequent thermalization of radiation, and also depends on a strict separation of EM and gravity. The WMAP data is open to any number of interpretations (I've seen some that go against BBw/I). Compared to this, the large scale "walling" and enormous filamentary structures actually *seen* are heavy counter-evidence.

As a custom it must be by now, this website

http://background.uchicago.edu/~whu/physics/tour.html

reading must be required before one open the mouth to speaketh about the Big Bang and CMBR.

(Ok, that's an unsuccessful attempt at Old English Yodaspeaketh.)

16. **D R Lunsford**
May 31, 2004

"The Big Bang model is also well confirmed, the CMBR, for one. And the elemental abundances. Plus, the clear peak in the WMAP data is a generic prediciton of inflationary models."

The CMBR is not predicted by BB, it is an assumed and tunable side-effect of nucleosynthesis and atom-building and subsequent thermalization of radiation, and also depends on a strict separation of EM and gravity. The WMAP data is open to any number of interpretations (I've seen some that go against BBw/I). Compared to this, the large scale "walling" and enormous filamentary structures actually *seen* are heavy counter-evidence.

If somehow the long-range forces *are* related on some level then everything is in thrown into a cocked hat. Cosmology as it stands, even without the ad-hockery of inflation etc. is simply too much extrapolation from too little data, IMO. Mind you I am not a steady-stater – I'm just against religiosity in science.

17. **Matthew**
May 31, 2004

"GR, although well-tested in the (very) weak field case provided by the vacuum neighborhood of the Sun, is simply assumed to hold under all conditions, and is made the cornerstone for building the entire universe"

I'm not really sure that's true. There's at least one "strong field" prediction of GR that's been confirmed, the behaviour of the Husle-Taylor binary plusar.

There are other weak field predicitons that seem to hold as well. Lensing, for example. And, as I understand it, the dark matter density inferred from lensing (a GR effect) is consistent with that inferred from rotation curves (essentially a Newtonian effect).

The Big Bang model is also well confirmed, the CMBR, for one. And the elemental abundances. Plus, the clear peak in the WMAP data is a generic prediciton of inflationary models.

GR and the big bang model may have issues, and it's appropriate to point them out. But, to pretend that the observational support for them is limited to extrapolations of solar system tests is just wrong.

18. **D R Lunsford**
May 31, 2004

I can assure you Lerner understands GR. I think he believes that it is incomplete, and the energy problem is a symptom of that, and that it is not the only operative factor in the large scale structure of the universe.

I find it difficult to construct the right sentences to express the heterodox opinion. GR, although well-tested in the (very) weak field case provided by the vacuum neighborhood of the Sun, is simply assumed to hold under all conditions, and is made the cornerstone for building the entire universe. Something in the soul of the the skeptic just cannot accept this extreme extrapolation, the moreso since large-scale effects of electromagnetism seem to be deliberately ignored, in spite of the existence of enormous filamentary structures which are well-explained as plasma phenomena. The skeptics do not trust the objectivity of the orthodoxoi, and adduce Markarian 205 etc. and the ostracism of Halton Arp as examples. See for example

http://www.angelfire.com/id/jsredshift/review.htm

A scientific community that embraces string theory and anthropomorphism in particle physics, is fully capable of equal excrescence in other fields. So, it is not GR as such that is being questioned – it is the entire attitude of the scientific community that Lerner et. al are fighting against, IMO with great courage.

I would also point out that the energy problem is resolved in Weyl's approach, but it is nearly impossible to get anyone to pay attention to this remarkable fact.

19. **Chris W**
May 30, 2004

Here is another reminiscence about a talk given by Segal, written by a former research associate of his, Ray Streater.

20. **Simplex**
May 30, 2004

I dont quite understand your objection to Carroll's criticism of Lerner. It seems to me that Lerner raised the issue of energy non-conservation. So your remark–that it is meaningless to even bring energy up–actually applies more to Lerner than to Carroll. Here is a quote (in which Carroll quotes Lerner):

" Consider this quote by Eric Lerner, petition signatory and author of The Big Bang Never Happened:

[i]'No Conservation of Energy
The hypothetical dark energy field violates one of the best-tested laws of physics–the conservation of energy and matter, since the field produces energy at a titanic rate out of nothingness. To toss aside this basic conservation law in order to preserve the Big Bang theory is something that would never be acceptable in any other field of physics.'[/i]

Actually, there is a field of physics in which energy is not conserved: it's called general relativity. In an expanding universe, as we have known for many decades, the total energy is not conserved. Nothing fancy to do with dark energy — the same thing is true for ordinary radiation. Every photon loses energy by redshifting as the universe expands, while the total number of photons remains conserved, so the total energy decreases. An effect which has, of course, been observed."

Maybe you can straighten me out, but though I dont always like Carroll's style or tone of voice this seem legit on his part.

21. **D R Lunsford**
May 30, 2004

The argument against Lerner is specious – in fact the main problem with GR is that of defining energy – so when he says "energy is not conserved" it's wrong both technically and philosophically. Technically, energy is not even local, so of course it's not "conserved" as that term is usually understood – philosophically, insofar as GR is a theory of tensors, and there is no tensor for the gravitational energy, it's meaningless in a real sense to even bring it up. This kind of dismissive comment is what annoys many people who see Bangers as a priesthood with a recevied wisdom to fall on.

More Landscape Stream of Consciousness

May 31, 2004
Categories: Multiverse Mania

It looks like particle theory has now degenerated to the point where its leading figures can't think of anything better to do than to write rambling articles with virtually no equations that reach no real conclusions. Last week was Lenny Susskind, tonight there's a new article by Michael Douglas.

His conclusion, such as it is, goes like this:

"If I had to bet at the moment, I would still bet that string theory favors the low scale, for the reasons outlined above, but it is not at all obvious that this is what will come out in the end…. We should keep in mind that 'favoring' one type of vacuum or mechanism over another is not a strong result, if both types of vacuums exist…"

So, maybe string theory "favors" a low supersymmetry-breaking scale, maybe not. As usual, not only can't it predict anything, it can't even predict the scale at which it can't predict anything. I really cannot understand why anyone thinks this kind of thing is science.

Comments

1. **Alejandro Rivero**
   June 13, 2004

   An addendum about Nomura.

   I had forgotten it, but time ago I had quoted W.T. Shaw in the first page of my essay physics/0001033.

   "if we are getting discrete data from the market, how can we claim that the derivative is not zero? Should we say that our derivative is *almost non zero*? What control do we have over the inversion process?"

2. **Alejandro Rivero**
   June 5, 2004

   Hi Chris, :DDD

   I am still happily surprised that some minor, unpolished, works (the "Lecture on Divergences" between them) get random attention even if unpublished. In this case, I took care to blame Gabay, the librarian, by indirectly drawing my attention towards such pathologial matters. I know I am setting a bad example for future archeologists (I doubt it will be a younger generation of physicists), but at least now I understand why our elders were not afraid of divergences (btw, baez did some nice postings recently on borel transforms etc). As for the problem of renormalization itself, I am optimistic that it can be tamed, and it will show to be a problem in the concept of derivative. Long long time ago I heard a Field medallist to say that, of course, trivialy, blah blah, the derivative is just the simplest example of controlled substraction of infinities.

   Let me to do a bit of propaganda for Nomura: hi guys, write Oakley! Imagine, you will be able to pay a flat in Newton street (pretty smail address), to buy your ve/getables at Holborn market, and even to walk straigthly to the work, no need of tube, no need of car!

   Around the end of 1999 I visited some of these financial industries in London City -I can not say if Nomura itself, but Chris could look in the CV database- and I found them to be very nice places to work. There exist some meetings on econophysics, so you can still train your research sword if you want and meet interesting people around.

   On other hand, the financial research is interesting because it is a purely artificial system, so it helps you to understand what facts come from physics and which ones come from mathematics.

3. **JC**
   June 5, 2004

   Alejandro,

   After when I left academia, initially I thought of doing some independent research on my own. It turned out after a year or so I found myself pursuing numerous "dead ends" and drifting around aimlessly, bouncing around from idea to idea and making very little progress. Despite keeping in contact with old colleagues via email, I still found it was difficult to settle down and work on calculations. Whether it's my own lack of self discipline or laziness, I know now from experience that pursuing independent research outside of academia to be a long hard "road less travelled".

   Despite having long periods of time devoted to thinking and contemplating about math and physics, in between job searches and sleeping, I still found it relatively hard to bring many of my research ideas to fruition while working alone in a solitary manner. At times I wonder how folks like Julian Barbour, Grigori Perelman, or Andrew Wiles worked during their "self imposed exiles" away from mainstream research communities.

4. **Chris Oakley**
   June 5, 2004

   Hi JC,

   My detailed knowledge of string theory comes from Peter Woit – the most unbiased source – so I am unable to shed light on issues you raise. I would suggest an e-mail to William – he's busy, but I am sure that he would be happy to discuss these matters.

   I'm similarly useless on path integrals. I never liked them and they never liked me. I have always suspected sleight of hand here, but have never been able to prove it. A bit like having a dishonest cleaning lady. It just seemed to me that the good old operator-commutator methods were stronger tools, and that if something went wrong that it would be more difficult to weasel out. This way one gets a clearer picture of what is going on.

5. **JC**
   June 5, 2004

   Chris,

   I took a look at William Shaw's www page at Oxford and noticed he had a pdf file of his recent presentation in finding an anomaly free way of doing string theory in 4 dimensions via twistors.

Over the years I've always wondered if there's any compelling loopholes in "quantizing" string theory which bypasses the anomalies that locks the number of spacetime dimensions at 10 (superstring) or 26 (bosonic string). Some paper a few months ago by Thiemann hep-th/0401172 attempted to "quantize" the bosonic string by loop quantum gravity methods which appears on the surface to bypass the usual string anomaly. From what I could figure out of Thiemann's paper and in the various discussions about it on the string coffee table (at Distler's site) and physicsforums.com (in the string section), it appears on the surface to be a "quantization" which seems to lack a path integral formulation and looks almost "vacuous". So far I haven't seen anybody attempting to do a tree level amplitude type of calculation to see what the classical limit produces. (ie. Do we get back anything that looks like a Veneziano type of formula?)

It seems like the anomalies in the conventional quantizations (ie. canonical and path integral) of string theory are very well entrenched in standard treatments of string theory, with so much effort expended over the last 20 years on dealing with the "extra dimensions" compactified on Calabi-Yau manifolds. If there is indeed an "alternative quantization" method which bypasses the anomalies in string theory which lock the number of spacetime dimensions to 10 or 26, it may possibly put into question all that volume of work done on Calabi-Yau compactifications. From a self-serving perspective, I would probably find it difficult to give up on all that compactification stuff in string theory if I was still doing particle/string research today. Less "resume padding" type of papers to work on in string theory, if all that Calabi-Yau stuff turns out to be irrelevent in the end.

Sometimes I wonder if the path integral is really the most general way of thinking about quantization. At this point I would be somewhat reluctant to throw it away into history's trash bin, considering how effective it's been in understanding Yang-Mills theory and the Standard Model. Arguably many folks would be hard pressed to think of something else that could possibly replace the path integral.

6. **Chris Oakley**
   June 4, 2004

   Hi Alejandro,

   No hires have as yet resulted from that permanent job ad on my CV on my web site, partly because the CVs I have been sent as a result have never quite fitted the vacancies at the time. The most prominent physicist at Nomura – although he is really more a mathematician – was William Shaw, who now runs the Mathematical Finance course at Oxford. We still see him from time to time. He has written a book on solving mathematical finance problems with Mathematica, and has a book on Complex Analysis in preparation. He started out doing Twistors, and still works on this from time to time. He gave a talk recently to the String theorists at Cambridge, showing how one could do Strings in four dimensions without anomalies using Twistors. This talk, he says, "went down like a lead balloon". I guess that they don't want to be deprived of the exciting challenge of explaining what happened to the six they have spare in *their* formulation.

   By the way, as for your 2002 "Short Lecture on Divergences" – wash your mouth out! You should be apologising for pathological divergences, not justifying them. What kind of example is this setting to the younger generation?

7. **Alejandro Rivero**
   June 4, 2004

   As for research outside academia, I find that it is very hard to do calculations when one is alone. But forming a research team of "exiles" sharing a common seems not easy; I do not know any.

   One could mention solitary snippers, as for instance Barbour, but the research then is very exotic by itself (not to mention, ahem, mine).

8. **Alejandro Rivero**
   June 4, 2004

   Chris, how many years does a physicist hang in nomura? I am amazed you keep needing people all the time.

9. **Peter**
   June 4, 2004

   Hi JC,

   I don't know much about Steklov, but I think the IAS is pretty different. It has only a small number of permanent people, and those jobs are among the hardest to get in the world, so they are held by absolutely the most highly respected physicists around. It's true they have no formal duties, and some end up doing nothing, but in general the level of the people and their activity there is very high.

   The theory groups at the national labs might be a bit more similar, but again these have only small numbers of permanent people, and I assume there is some sort of evaluation process they are subject to. I don't think the US has ever had anything analogous, i.e. a large institute with a lot of people hired on the old communist system of "we don't really pay them, and they don't really work".

10. **Peter**
    June 4, 2004

    Hi Chris,

    Job advertisements for mathematicians and physicists are encouraged. A huge improvement over the penis enlargement ads that spam the comment section and have to be periodically deleted!

11. **Chris Oakley**
    June 4, 2004

    I'm embarrassed to put a job ad in a physics discussion forum, but we did briefly discuss jobs in Finance …

    It so happens that we are looking for a numerate C++ programmer with UNIX experience here at Nomura (in the City of London). The job is mostly programming, but expect to do maths as well. Salary ?50K + benefits. If interested, send me a CV (chris.oakley@uk.nomura.com)

12. **JC**
    June 4, 2004

    Peter,

    Do you think the IAS resembles the research institutes that were common in Soviet Russia, such as the various Steklov Institutes? Several old Russian friends and colleagues mentioned that in some of those research institutes, there was virtually no teaching duties nor was anyone required to publish anything. Almost everybody was paid something like $200 per month salary, regardless whether they did anything or just sat around doing nothing for the whole time. After the communist Soviet system fell apart, some of the better researchers as well as many younger folks left Russia and took up faculty or postdoc jobs at American universities.

Or would these Russian research institute jobs more closely resemble tenured jobs at places like Los Alamos, Argonne, Brookhaven, etc …?

13. **Peter**
June 3, 2004

Hi Erin,

While I think it's really hard to do great research outside of academia, you're right that at this point it's also very hard in some subjects like particle theory to do it inside academia also.

Two of the greatest achievements in math during the last decade both took about 7-8 years of sustained, very independent work. One was Wiles's proof of Fermat's Last Theorem, he did this from a permanent academic position. The other was Perelman's proof of the Poincare conjecture, which he seems to have done from outside academia. So both are possible, but both required somebody at the height of their powers who was able to devote most of their intellectual energy for 7-8 years to the project. This certainly is not the kind of thing a postdoc is in any position to do, since they need to get a new job every 2-3 years, and no one is going to give them one based on being in the middle of a difficult ambitious project that may not work out.

14. **Peter**
June 3, 2004

Hi JC,
There certainly are examples of people who never did very much after getting permanent positions at the institute. Hard to tell if it would have been different if they were somewhere else. I always found the atmosphere there a bit weird, the isolation may not be a good thing for many people.

It will be interesting to see what happens with Seiberg and Maldacena. I think Witten spends a lot of his time traveling and interacting with people who come to Princeton, so he hasn't become as isolated as some others there in the past. The one thing that he may be missing is the experience of regularly teaching graduate-level courses, something that forces one to regularly rethink one's understanding of the basics of one's subject.

15. **JC**
June 3, 2004

Chris,

Seems like some Nobel laureates went beyond the deep end and never climbed out of the abyss, such as Brian Josephson working on psychic/voodoo type of phenomenon, or Julian Schwinger working on cold fusion in the last years of his life. The only Fields medalist I can think of offhand who turned into an oddball, was Grothendieck disappearing in 1991 without a trace.

Anybody know of other oddball cases of Nobel laureates or Fields medalists?

16. **Chris Oakley**
June 3, 2004

Hello erinj,

You raise some interesting points.

The stories about Einstein and Newton do not surprise me. Anyone who has done real research (as opposed to just CV padding) will be well aware of just how chaotic and haphazard the process is. The organising of the chaos that the academic research establishment tries to do is somewhat reminiscent of Hollywood, where production companies try to turn the successful one-off, off-the-wall idea into a steady revenue stream, forgetting that the very reason why the idea succeeded was *because* it was different and crazy.

It seems to me that the amount of "craziness" (for want of a better word) that is tolerated depends on your status. For Nobel laureates and Fields medallists, any amount of craziness is tolerated – not only that, but many will follow one into the abyss. For tenured academics, ditto, but probably few will follow. For graduate students or post-docs, on the other hand, deviation from the party line is not tolerated at all. Given that in the last century almost every significant idea in theoretical physics was from someone in their 20's, it would seem to me that something is very wrong here.

17. **erinj**
June 3, 2004

I agree with your comments, Peter, on independent research, but surely there are examples of successful independent research which has taken place outside of academia, although some degree of intellectual stimulation may have been present? An obvious example would be Einstein, working as a patent clerk in Berne and having meetings with others as part of the `Olympia Academy'. Einstein must have had, or found, the time to dedicate to his three 1905 papers. Newton's `miracle year' (1665/6?) was one were he was, as far as I've read, isolated on his mother's farm, Cambridge having closed due to the plague. Also, people like Hawking and John Nash appear to have drifted through their PhDs, waiting until the last year or months before finally focussing on a problem (and then solving it). Hey, and didn't Louis de Broglie, wealthy aristocrat that he was, do physics research for fun?

Having been a postdoc, I often wonder whether the time and freedom (and possibly lack of pressure) necessary for `great', or major, works, is more likely to be found outside of academia, and perhaps the really big problems of the past, present and future do require a proportionately large amount of time and thought to solve. No publish or perish rule, no scrabbling or begging for funding – perhaps a menial job in the background to earn enough to live on whilst one concentrates on the `real' work. I admit that this reads as an exceptionally difficult route to acheivement, but perhaps in such conditions the `survival instinct' takes over and intellectual challenges become all-consuming instead of being part of the potentially droll `day job'.

This seems to be the approach that many – if not all – major, award-winning and famous novelists, dramatists and poets take, as they are even less likely to be paid to produce work in these areas than scientists. The sheer difficulty of their circumstances perhaps drives them to brilliance and creative heights, but I suppose this requires immense resilience and dedication, perhaps to the detriment of family, friends, health, etc. In fact, the parallel goes further, as I read that Einstein essentially slept for two weeks, exhausted and ill, after arriving at his final formulation of special relativity. Didn't Newton's hair gradually turn white after his `miracle year', as well? Maybe the cliche `no pain, no gain' really does hold.

I personally don't believe there is sufficient freedom for risk-taking and novel research in academia as funding bodies and research leaders are very cautious, which is probably to be expected (their jobs being on the line as well, if things go badly…). Then again, string theory research seems to have garnered a very large part of global particle theory research funding over the past ten years or more. I myself believe string theorists gain funding because of links to particle theory rather than any claims about string theory being the "best candidate" for a quantum theory of gravitation. Quantum gravity is a problem that I believe is not going to be readily solved inside academia, unless funding for loop QG and other `alternatives' to string theory QG receive more funding so that they can be explored more fully. The really big and difficult problems just don't receive the funding necessary to resolve them, inside academia: the risk is just too great, and in consequence I wouldn't be surprised that a lot of research becomes "resume padding". Surely we need more of Feynman's "leaps into the wild blue yonder" (I do hope that quotation is correct).

18. **JC**
June 3, 2004

Peter,

Do you think the IAS was a cause or symptom for folks like Einstein, Goedel, Oppenheimer, etc … in producing nothing of significance for the rest of their lives, and/or producing garbage research bordering on crackpottery?

I remember reading something in a short biography of Kurt Goedel, where his wife thought that the IAS wasn't much more than the equivalent of a "retirement home" for famous academics who are long past their prime.

19. **Peter**
June 3, 2004

It's pretty unusual for people to be able to do much research outside of the standard academic system these days. Perelman is an interesting special case, I actually don't know to what extent he really was cut off from the math community in Russia while he was doing his research, or whether he did have colleagues to talk to. One funny thing about him is that he's in line for a million dollars from the Clay foundation, but seems to have no interest in the money. Definitely an unusual character, very devoted to a pure approach to abstract research.

One problem with being outside academia is getting the necessary blocks of time to devote to thinking about research. It also is much harder to make progress on one's work if one doesn't have people to talk to about it, or continuing stimulation of new ideas to think about. I've always thought the Institute in Princeton wasn't such a great idea, because by isolating people from having to teach classes and deal with students, it can keep them from having to think about anything other than the one approach to their research they are pursuing. It can be quite helpful to be forced to think about different topics, and may give one ideas about new ways to think about one's research.

20. **JC**
June 3, 2004

Peter,

How common is it for math or physics folks to take up independent research outside of the formal academic system, such as Grigori Perelman working on a proof of the Thurston geometrization conjecture in relative isolation for many years in St. Petersburg?

Arguably there's no pressure to publish any "resume padding" type of papers, or for that matter publishing anything, when one isn't a part of the formal academia system.

At times I wonder how many folks would be working independently (ie. outside of the formal academic system) on things like SUSY and/or string theory, if there were no tenured faculty nor any untenured faculty or postdocs working on it. I always wondered if John Schwarz would have worked on string theory during 1970's and early 1980's if Murray Gell-Mann had never kept him around as a "permanent postdoc" when string was in hibernation, and possibly would have had to take a day job in the private sector outside of academia.

21. **Peter**
June 2, 2004

I mainly know what happens these days with grad-students and young faculty in math. Very few grad students leave without getting a degree. When they graduate, some don't even try and get an academic job, instead going directly into finance jobs, computer jobs, or maybe doing something like working for the NSA.

Of those who get a first academic job, many leave academia after a couple of years or so.

For both categories of people, the situation is similar. Getting a postdoc at a good place is hard, and getting a tenure-track job at a good institution that will support your research is harder. So people find that the jobs they can get are either in places they really don't want to live, or at primarily teaching institutions. Many people find they really don't enjoy not being paid very well to do a lot of very low-level teaching. A well-paid job in New York doing financial math looks a lot more attractive in many ways.

Once people get to the point of having a tenure-track job in math and do it long enough to know they want to stick with it, most end up getting tenure, either at the place they start or somewhere else.

The job situation is particle theory is much worse than in math, but I haven't been following the careers of more than a few young grad students and postdocs in it, so don't have much data about what is happening to them right now.

22. **JC**
June 2, 2004

Peter,

What reasons do math/physics people have for leaving grad school or academia these days?

The main reasons I've heard of over the years from old friends and colleagues (excluding political or gross misconduct reasons), were involuntary ones like

– a grad student couldn't get a postdoc
– a postdoc couldn't get another postdoc and/or couldn't get an assistant professor job
– an untenured faculty member was denied tenure or couldn't get a tenured faculty job
– an untenured faculty member lost most of their grant funding (especially in the case of an experimentalist)

while voluntary reasons were ones like

– a grad student didn't like graduate school and didn't feel like it was worth the effort to finish their PhD research work
– a grad student got bored or sick of their field and quit after getting their PhD
– an untenured person got sick and tired of their field, and had a hard time getting any funding when they tried changing into another field (in some cases not even getting any funding at all in their new field)
– a postdoc felt they had no chance in hell of getting a faculty job, and subsequently quit
– a postdoc got bored or sick and tired of their field, and subsequently quit

– a tenured faculty member got sick and tired of cranking out meaningless “resume padding” type of papers just to maintain their research grant money; ended up quitting to find more greener pastures outside of academia
– a postdoc got sick and tired of cranking out tons of meaningless “resume padding” type of papers, with no end in sight (ie. the “permanent postdoc” types who are on their 4th, 5th, or higher postdoc); ended up quitting out of discouragement

23. **Peter**
June 2, 2004

Since the bubble burst I don’t know anyone who has gone into the computer industry. All the people I can think of who have left academia recently have gone into finance. Part of this may just be because I’m in New York, where there are lots of finance jobs and few computer ones. Maybe the experience of people on the West Coast is different.

24. **JC**
June 2, 2004

Peter,

How common is it for math or physics PhDs to go into computers ever since the dotcom bubble bursted a few years ago?

Almost everybody I know who still has a paying job in computer/hi-tech and even the defense sectors are paranoid of being laid off, especially with many jobs being “outsourced” to places like India or China. This seems to be almost universal from the folks who do data entry all the way up to PhDs who do the cutting edge research & development work, who are not in upper management. On the surface it sounds “suicidal” for a company to be firing many of their research and development folks just to save a few bucks in costs, or “outsourcing” their research and development work to countries which don’t have much in terms of intellectual property laws and enforcement.

25. **Chris Oakley**
June 2, 2004

I’m keeping out of the Cosmology discussion (although, if pressed, am prepared to vociferously defend my beliefs about the Great Green Arkleseizure) …

My comment is on Peter’s remark below:

Unfortunately the thing that seems to me the most sensible idea in the present circumstance, that of going back and trying to really understand the things one doesn’t understand about gauge theories and the standard model, doesn’t seem to have any takers. People have convinced themselves that there’s nothing new to be found that way.

There are a handful of takers, but, being focused on irrelevancies, they never got anywhere.

26. **D R Lunsford**
June 1, 2004

One thing that is true of anti-bangers (and perhaps bangers) – one makes up his mind early, and tends to stick to that decision. e.g. I have always found the BB hateful, even before inflation which only made it much worse. There is too much “play” in GR for comfort, so the extrapolation to cosmic time scales is very suspect, the moreso since the entire idea of energy is on shaky ground – and so also time. Time is all balled up in GR no matter how you cut it. The following thought experiment illustrates – you can never “watch a movie” of the formation of a black hole – since you can’t see anything beyond the horizon, that stuff can never have been “out here”, from my perspective, and therefore a black hole cannot form in finite local time, and must have always existed, from my perspective. Now the problem here is not the horizon, but time itself – and it’s just the other side of the energy problem. I cannot think of any counter-argument that allows a Big Bang. Time is more than duration, it’s “persistence”. GR is only complete when the substance – persistence – comes automatically out of the basic idea of curvature. That is, by its nature GR is a vacuum theory, and in fact the only part of it that is well-tested is $R_{mn} = 0$.

27. **Peter**
June 1, 2004

I don’t have a lot of information about what is happening to current particle theory PhDs, but a large fraction of the ones from my era (early-mid 80s) ended up working in the computer or finance industries (about equal numbers in each).

Over the last ten-fifteen years I’ve seen a lot of young math grad students and faculty leave academia, mostly for finance. Ten years ago the academic job market was terrible and it wasn’t so hard to get a finance job. More recently the academic job market has improved, but finance jobs seem to take a lot more effort for people to get. One reason is that there are now a lot of financial math master’s degree programs (we have a very successful one here in our department). So there are a lot of students coming out with some training and good credentials in financial math. It is harder for straight math PhDs without any experience in finance to compete with them.

28. **Chris Oakley**
June 1, 2004

Hello erinj,

If there is less tolerance now for quants who don’t program in the City, then I accept part of the blame. I have always argued that they should. The problem is that if bringing the model into production means involving a team of programmers to turn the model into something that can work in a production environment then the process will take too long, and as I probably do not need to tell you, traders are not patient people. The system we have at Nomura just involves deriving a (C++/COM) class from one called “Trade” and overriding some virtual functions. That way our quants can have a new trade type in the system on the same day (there is a bit more to it than that, but that is the basic idea).

We are now getting way off topic. Drop me an e-mail with your CV to chris.oakley@uk.nomura.com and I will see if I can come up with some suggestions.

29. **erinj**
June 1, 2004

Hello Chris,

I made two serious attempts to gain employment in the City (of London, banking and finance sector), about two years apart, and came up against a brick wall on both occasions – once as a new theoretical physics PhD, and once as a recent high energy physics post-doc. Despite advice to the contrary, I found it impossible to gain work as a junior `quant’ (quantitative analyst).

I contacted many banks and finance firms, got advice from an economics university lecturer who was a former `quant’ team member, and read up on as much financial mathematics and economics as I could. I couldn't even secure an interview! My CV was listed on quite a few specialist finance employment agencies, and I could get nothing. I was, and still am, baffled by the lack of interest my skills, which I assumed were valued by the finance sector, brought me.

After asking for feedback about this, I was told by employment agencies – who relayed what quant team leaders told me about my applications – that the reasons I was never shortlisted were: lack of experience in the financial world, too many other PhDs applications, and that my programming skills were not as extensive as other applicants. Well, if they wanted computer programmers, why did they give the impression that they wanted theoretical physicists – people who can work with and solve mathematical and modelling problems?

I think that there are, and have been, too many applicants for finance sector jobs who don't have enough programming skills, like myself, and who have also been misled about how much computer programming is required in these jobs. The aforementioned former quant told me that it would be about 75% of the time – is this true in your experience, Chris?

My impression (and I've seen job ads asking for candidates in this direction) is that, in the post-9/11 era, combined with the economic down-turn, economics and financial mathematics PhDs are the candidates more likely to be successful, as well as graduates who have worked as part of summer programmes with banks (internships/work experience), and even computer science PhDs.

So now I'll never get to see the `front office' or the trader's floors, nor be paid the fabled big salaries and bonuses of the City.

30. **JC**
June 1, 2004

Chris,

Perhaps it was the folks I was speaking with over the years. These old colleagues in particular ended up quitting Wall St. after 6 or 7 years, after getting sick and tired of trading or writing computer code that priced derivatives securities. I never asked them specifically what they defined as "burnout" in their line of work. My trading friends were always complaining about their work related stress, such as their telephone ringing every 30 seconds and having to keep track of information simultaneously on 5 or 6 computer screens all day. The friends who did the computer derivatives calculations stuff had less complaints, other than many of them finding their work boring and/or not very interesting after awhile. In some ways I got the sense they felt their work started to become "routine" or a "chore" to them, and the initial "excitement" was gone for the most part.

Whether these folks chose the "wrong" profession to go into, I don't know for sure. Some thought the money would keep them happy, but in the longer term it didn't. Others I suspect saw the money as a "means to an end", where they were able to make a large enough fortune to "retire" and do whatever they wanted afterwards with the rest of their life.

Certainly none of them had any complaints about the money they were making. Every one of them were able to pay off all their student loan debts, as well as a large portion of their mortgages if they lived outside of Manhattan.

I have not kept contact with any of these old friends and colleagues over the last few years. So I don't know what they're thinking these days, or if they changed their minds about what they regard as "burnout". A few folks I'm aware of went back to writing particle and/or string papers while others literally "disappeared" without a trace.

31. **Chris Oakley**
June 1, 2004

Speaking as one who "sold out" and left particle physics to to work in Wall Street (the City of London, actually) – I feel it my duty to correct the bleak picture painted earlier of this career path. First off, if you choose to go into trading as did former string theorists Kelly Kirklin and Paul Miron then you can make *a lot* of money. I don't know the exact score with these two, but my guess is that they both have enough to retire. Kelly is now a post-doc at Cambridge. Whether he is paying for himself, or denying some much more deserving youngster by holding this post I do not know, but the point is that with plenty of money in the bank, he is now able to do what he wants, which is physics – a bit like the "gentleman scientists" of old, like Lord Rayleigh. Personally, I never went into trading, mainly because I find software far more interesting, but having said that, although I have not done nearly as well as these two, I nonetheless was able to pay off my mortgage about two years ago.

Some basic points that all physicists thinking of making this transition ought to be aware of:

= There is a lot of interesting mathematical work to be done in the Finance world.

= The route from science PhD to quantitative analyst in an investment bank is well-trodden & you will not have to explain or justify yourself. Mathematical skills *are* valued.

32. **D R Lunsford**
June 1, 2004

Weinberg in astrophysics – how nice.

I heard a talk a UMd on early string theory (the 26-dim weaving of it) by Gell-Mann. He opened his talk with a comment about the "half-asstrophysicists" in the audience. I wanted to punch him out. Instead I asked him an impromptu group theory question he could not answer extemporare, which was almost better.

33. **JC**
June 1, 2004

How popular is it for physics or math folks to go to Wall St. these days?

I knew many particle theory and some string folks that went the Wall St. route through the 1990's. Main drawback I've heard from old friends and colleagues is that there's a very high burnout rate on Wall St. with a half-life of around 6-7 years for many physics and math types.

Only other major "competitive" route I was aware of that many math and physics folks went to, was management consulting. Last I heard from friends and colleagues that went into consulting, many consulting firms (ie. Accenture, McKinsey, etc …) were laying off folks like crazy in periodic "purges" over the last few years.

34. **Peter**
June 1, 2004

A lot of people are already abandoning particle theory for astrophysics, Steven Weinberg would be one prominent example. Many string theorists have moved into cosmology, often hoping there is something to be done there with string theory, my colleague Brian Greene is an example.

I hear my advisor (Curt Callan at Princeton) is mostly doing biology these days. A few people have moved into mathematics.

Presumably John Schwarz would be one of the last to give up on string theory. Witten certainly seems to continue to believe in string theory, something which mystifies me. He hasn't gone in for the anthropic nonsense, or for "string cosmology" or "string phenomenology". I'd bet he's well aware that the present state of string theory is such that progress can't be made in those areas.

I suspect the SUSY people will hang in there until data from the LHC comes in. If something interesting that isn't SUSY turns up there, they'll all jump on it.

Unfortunately the thing that seems to me the most sensible idea in the present circumstance, that of going back and trying to really understand the things one doesn't understand about gauge theories and the standard model, doesn't seem to have any takers. People have convinced themselves that there's nothing new to be found that way.

35. **JC**
May 31, 2004

Any bets on what string theory folks will be working on, if string theory falls out of favor and into hibernation like it did in the late 70's and early 80's? Any bets that somebody like Edward Witten would abdicate from string theory, and/or John Schwarz would continue slogging on with string theory to the day he dies?

Any bets on what the SUSY crowd will be working on, if SUSY falls out of favor too?

Apart from my very limited knowledge of particle physics history, I don't even have any clue as to what I could be working on if I was still doing particle theory, outside of SUSY and/or string theory. If I was a young naive grad student today, I would be tempted to go into something like astroparticle theory or possibly do an MBA or law school instead.

Witten on Electroweak Symmetry Breaking

June 3, 2004
Categories: Uncategorized

Witten has contributed an essay to the latest issue of Nature about electroweak symmetry breaking. He describes the main conventional ideas about this, ending with the latest anthropic ones. Here are his comments about those:

"One approach is the anthropic principle, according to which the dark energy and the Higgs particle mass take different values in different parts of the Universe, and we inevitably live in a region in which they are small enough to make life possible. If so, many other properties of the Universe that we usually consider fundamental ? such as the mass and charge of the electron ? are probably also environmental accidents. Although I hope that this line of thought is not correct, it will inevitably become more popular if experiment shows that electroweak-symmetry breaking is governed by the textbook standard model with a Higgs particle and nothing else."

He ends with the eminently reasonable summary:

"As yet, none of these theoretical proposals about electroweak-symmetry breaking are entirely satisfying. Hopefully, by the end of this decade, experimental findings at the Tevatron and the LHC will set us on the right track. But the diversity and scope of ideas on electroweak-symmetry breaking suggests that the solution to this riddle will determine the future direction of particle physics."

Comments

1. **Peter**
   June 4, 2004

   Witten certainly didn't need me to point out to him the importance of this problem. But maybe the reason he just said that current proposals are not "entirely satisfying" but didn't offer any speculation about alternatives is that he didn't want Srednicki and Distler telling him he was an incompetent moron for thinking there might be alternatives.

2. **Simplex**
   June 4, 2004

   Too bad the Nature article is online to subscribers only. This
   2003 talk by Witten on a similar theme might be useful for comparison.
   http://conferences.fnal.gov/lp2003/program/papers/witten.pdf

   The title is "Supersymmetry and Other Scenarios". The talk
   was given at a conference at Fermilab and there was some discussion afterwards, which is included at the end.

3. **Thomas Larsson**
   June 4, 2004

   Unfortunetely the link is for Nature subscribers only and I don't have a subscription. However, I distinctively recall that somebody else recently emphasized that EW symmetry breaking may be the key problem. So it looks like Witten is following Woit's suggestion. One may wonder if the connection is causal or merely temporal. 🙂

Dartmouth Talk

June 4, 2004
Categories: Uncategorized

I was visiting the math department at Dartmouth the past couple days, and gave a colloquium talk there. It's now available online.

Comments

1. **Peter**
   June 9, 2004

   I'm using the term "weight" in the precise sense it is used in representation theory, the label for a representation of T. More generally it can label reps of some Abelian subgroup, e.g. multiplicative groups. If the group is the real numbers, the weights are given by arbitrary numbers, not necessarily integers like in the U(1) case.

2. **D R Lunsford**
   June 8, 2004

   Why specifically is the word "weight" used? Weyl was very precise with language so by weight he would have meant the exponent on a multiplicative factor, in whatever sense (in the talk, the context is projective representations of spin groups).

   In the sense of Weyl geometry, the weight of the Dirac spinor field is imaginary, ie. The weight of a Yang-Mills field should be something like iek Tk as a kind of dyadic.

3. **Peter**
   June 8, 2004

   Hi Danny,
   The "weights" I'm talking about are for compact Lie groups G and come about as follows:

   1. For G of rank r, this means there is a subgroup T of r copies of U(1). For any representation of G, you can restrict attention to T and think of it as a representation of T. If you start with an irreducible representation of G, it will break up as several irreducible representations of T (all one-dimensional since T is Abelian). These representations of T are the "weights" of the original G representation.

   2. More explicitly, an irreducible representation of U(1) is labeled by an integer n, and e^{i\theta} acts by multiplication by e^{in\theta}. An irreducible representation of T is labeled by r integers.

   Things are different and much more complicated for non-compact groups (like the conformal group), then you typically have copies of R (the real numbers) in the maximal Abelian subgroup. Representations of R are labeled not by integers but by real numbers.

4. **D R Lunsford**
   June 7, 2004

   RE Peter-Weyl and Borel-Weil theorems –

   One thing I have been thinking about is extending the idea of conformal weight in an attempt to understand the relation of the covariant derivative

   (d/dm + ie Tk Akm)

   in field theory, to

   (d/dm + N Am)

   in Weyl conformal geometry – where N is the Weyl conformal weight. Am I right in saying that the "weights" mentioned in your talk are some kind of generalized phase which will turn out to correspond in gauge theory to charges?

5. **Peter**
   June 5, 2004

   I used the latex macro package called "prosper". For now the latex source file is at
   http://www.math.columbia.edu/~woit/dartmouth.tex

6. **D R Lunsford**
   June 5, 2004

   I love the interview with Dirac – that's priceless!

   Once a friend drove Dirac from the airport to the hotel in New Orleans. It had recently rained a great deal and snakes had emerged from the swamp, which Dirac noticed with a short comment like "snakes are interesting". My friend was on the organizing committee for the conference, and part of his job was to deliver Dirac and find something entertaining for him to do in New Orleans. So, he picks the zoo and the herpatarium in particular and asks Dirac if he'd like to see it "since you like snakes". Dirac said "I did not say I liked snakes. I said they were interesting."

   I felt really good about both Dirac and Weyl after reading that story. On his part, Weyl was the first person to really understand the significance of the Dirac equation. Weyl was very generous with praise when it was deserved.

7. **D R Lunsford**
   June 5, 2004

   Aside, what tools did you use to create this?

Bogdanovs Redux

June 5, 2004
Categories: Uncategorized

A couple years ago two French brothers, Igor and Grichka Bogdanov, managed to get Ph.Ds in France and publish several nonsensical papers about quantum gravity in refereed physics journals, several of them rather well-known and prestigious ones. John Baez has a useful web-page about this story.

This whole thing seemed to me strong evidence of how in recent years there has been a collapse of any real intellectual standards in this part of theoretical physics, and I ended up being quoted about this in various places. The "Affaire Bogdanov" died down fairly quickly, and the scandal doesn't seem to have lead to much in the way of higher standards.

I recently heard from Fabien Besnard, who wrote to tell me that the Bogdanovs have a new book out, called "Avant le Big-bang" (Before the Big Bang), in which they quote me as endorsing their work. Besnard has a web-page (in French) on the latest developments in the L'affaire Bogdanoff.

The Bogdanovs wrote me last year, here's a copy of their e-mail. I made the mistake of thinking "maybe these guys aren't so bad, just overly-enthusiastic sorts who could use a little helpful advice", and wrote this back to them. In their book they use part of my e-mail, mis-translating:

"It's certainly possible that you have some new worthwhile results on quantum groups.." (I was being too polite here; while possible, it is unlikely)

as

"Il est tout a fait certain que vous avez obtenu des resultats nouveaux et utiles dans les groupe quantiques" (It is completely certain that you have obtained new worthwhile results on quantum groups).

One lesson from this is not to write back to crackpots. Another strange part of this story: late last year I received an e-mail purporting to be from a "Prof. L. Yang" at the "International Institute of Mathematical Physics" at Hong Kong University. It appeared to come from

th-phys.edu.hk

a domain name that is registered with the Hong Kong DNS, supposedly by the Hong Kong University of Science and Technology. I connected to the web-site at this address, which at the time contained an official-looking web-page for this Insitute. It now contains just a listing of directories, one of which is full of .pdf files of the papers of Arkadiusz Jadczyk.

This web-site is hosted by a US web-hosting company "Everyone's Internet, Inc." If you look carefully at the header for this e-mail you see that while it purports to be from

"liu-yang.imp@th-phys.edu.hk"

it really comes from

th-phys.edu.hk (ATuileries-117-1-27-138.w193-253.abo.wanadoo.fr)

which appears to be a machine connecting to the internet from Paris, set to claim to be "th-phys.edu.hk".

It's looking more and more like the original idea that the Bogdanovs were hoaxers, putting on the physics community, was closer to the truth than the idea that they are serious, just not very good, researchers.

Update: The comment section received a message from a supposed mathematician named "Roland Schwartz" defending the Bogdanov's work on quantum groups. The source of the comment was
IP number 217.128.255.129. The DNS shows

nslookup 217.128.255.129

Name: ATuileries-117-1-29-129.w217-128.abo.wanadoo.fr
Address: 217.128.255.129

Funny, this seems to be a very close neighbor in Paris of Prof. L. Yang…..

I also just noticed that Jacques Distler has posted an account of his experiences with "Prof. L. Yang" et. al.

Comments

1. **Jack Sarfatti**
   June 20, 2004

   What is a "crackpot."?

   John Baez has a definition, which, if used without a double standard, implies that his own thesis advisor Irving Segal at MIT was/is a crackpot. Baez's criteria are perhaps too strong. A distinction must be made between amateurs who clearly have no idea what they are talking about and those with serious credentials in the field they are writing about who are expressing deviant, unpopular, strange (because they are so far ahead of the pack perhaps like Feynman was with his diagrams, or Bohm was with his quantum potential) or perhaps they are wrong, and even "not even wrong" on some particular hobby horse. In any case "crackpot" is a "smear" word like Joe McCarthy's use of "Red" etc, which should be avoided. By all means correct technical errors but best to avoid charged personal terms. Is Lenny Susskind, for example, a "crackpot."? By Baez's criterion he would have to be. I don't think he is. I worked with Lenny at Cornell in 1963-64.

2. **the-real-yang!**
   June 13, 2004

   Schwarz-petitot-bogdanoff said :
   "A few days ago a debate of this kind was organized and it showed that Bogdanoffs were very convincing as far as their domain of research is concerned."

   Where and when ? I can't remember it!

3. **Bogdanoff**
June 12, 2004

Comments: Bogdanov Thesis Reports

Good morning,

We would like to react to Peter Woit comment on our thesis and reports.

Woit wrote : "I wouldn't be surprised if some reports were missing."

IGB : There are no reports missing. These are the "official reports" that everyone has the possibility to check with University of Bourgogne. This makes a total of 15 reports (which is quite unusual in itself since normally a thesis only requires 2 reports). This answers clearly the question : there is no "hidden report".

Woit wrote : "I've always had some sympathy for the people who ended up on the Bogdanov's thesis committees. It's a difficult position to be in when you have to decide what to do with students who seem to be enthusiastic and have worked hard, but are very weak and have completed not very good theses.

IGB : How do you know that we have completed not very thesis? We are mathematicians, you are physicist. In a sympathetic letter, you wrote us that you do not know quantum groups theory ; "A large part of your work has to do with quantum groups and I'm not an expert in this field."

Question : why don't you trust Majid when he says "Bogdanov's ideas about signature fluctuations are to my mind about the more original and interesting that I have come across?"

Why do you refuse to admit that we have build a bicrossproduct "of a type not seen before" (theorem 3.3.2) The basic theme is tomix algebraic structures associated to the Euclidean and the Lorentzian signatures intosingle algebraic constructions. Bogdanov identifies this as constructing certain cocycle Hopf algebras ot a type not seen before : " These cocycle bicrossproduct results, in section3.3, from a body of original work which could certainly be the basis of a published researchpaper. "

We have spent many, many years working with Majid. He knows our ideas from A to Z. Do you think that he would have allowed the work to contain "some mistakes" or nonsensical parts?

The answer is NO.

Woit wrote : " A not unreasonable thing to do under the circumstances is to do one's best to find something of value in their work, "

IGB Yes. That would be a good thing to do.

Woit wrote : But the Bogdanov theses, especially Igor's, were so full of egregious nonsense, in particular with respect to topological quantum field theory, that they should have been beyond the pale.

IGB : It is rather bizarre that you insist so much on the "nonsense" of our work. After all you wrote that you are not an expert in quantum groups. This theory is the mathematical basis of our signature fluctuations model. If you do not understand quantum groups, you do not understand our model. Nothing wrong about it. But then, stop saying "it's nonsense".

Woit wrote : While some of these reviewers were string theorists, others weren't, so the whole mess can't be blamed on string theory.

IGB : Do you seriously think that a scientist of the magnitude of Jackiw signed his report without filtering every idea, sentence, proposition, of the thesis? We worked extensively before he would agree with the quality of the work. Jackiw would never have approved "nonsensical" work just because he found us "enthousiastic" (by the way, we never met him before he wrote his report : all the work was based on reading the thesis and exchanging informations or arguments by mail).

We can accept that you disagree, as a physicist, with our signature fluctuations model. But again we are mathematicians. And we developped our work on mathematical basis. Therefore the only thing that matters is : is our major theorem (a cocycle bicrossproduct of new type) valid or not?

The answer given by experts is YES, without any doubt.

If you disagree with this yoy know will have to prove it.

Thank you for your attention,

Igor Boganoff Grichka Bogdanoff
Posted by Bogdanoff at June 12, 2004 09:42 AM

I have always felt that the American system, where you kick out weak students after two years if they don?t pass their qualifying exams, is better for everybody than the European system, where the only real checkpoint is the dissertation, and people can linger forever. It is not really fair to let people work for a decade and then deny them a Ph. D.

I am not sure how consistently the American system is implemented in the US, however. It is probably not so easy to keep people out if they don't require funding.

Posted by Thomas Larsson at June 12, 2004 02:14 AM

Post a comment

Name:

Email Address:

Remember personal info?
YesNo
Comments:

4. **Lehnardt**
June 11, 2004

I did not have any opinion untill now. But after my reading of all the reports, I must admit that I was very interested and also surprised. After all, it seems that B&B have really done something.

5. **JC**
June 11, 2004

Peter,

Speaking of the parallels between postmodernism and string theory becoming dominant, one can see a similar parallel in the "neoconservatives" becoming dominant in conservative thinking which first started in academia and eventually made it's way into official political policy in the Dubya administration.

Many of the ideas which eventually came to fruition in the Bush administration's policy in pre-emptive wars in invading and taking over Iraq, were ideas cooked up by folks like political philosopher Leo Strauss and eventually taken up by guys in Dubya's administration like Paul Wolfowitz, Richard Perle, etc … who started to spread their ideas of pre-emptive wars and an American empire all the way back to the days when they were in the Reagan administration. With the present disaster in Iraq with oil pipelines being blown up every few weeks, American soldiers being killed every other day by insurgents, etc … despite Saddam being caputured and out of the picture, the "neoconservatives" have been slowly falling out of favor and being blamed for everything that is going wrong in Iraq.

It seems like the easiest way "unpopular" and/or "unrealistic" ideas can become dominant is attributed to Nazi propaganda minister Joseph Goebbels, (paraphrased as) "If you tell a big lie and keep on repeating it often enough, people will eventually perceive it as the truth."

It would be interesting to see if the popularity of ideas such as postmodernism, string theory, neoconservativism, etc … came about by hype and propaganda being repeated over and over again for many years.

On the surface it appears things that are "propped up" mainly by propaganda and hype, seem to follow speculative "bubble" type of patterns in their rise and downfalls. During the upward rise of the bubble, many people are willing to "suspend disbelief" in the promises made. Eventually a "bubble" bursts when the hype can no longer fool anyone and "reality" starts to set in. After the point of the bubble bursting, it seems to be only the diehard fanatics who still believe in the hype and propaganda while everybody else has moved on.
In the end, making too many unrealistic promises and repeatedly proclaiming triumphalism in an idea, seems to doom it when the promises are unfulfilled and "reality" takes over.

6. **P.Rozinski**
June 11, 2004

I wish to add the following comment about these reports. My opinion is that most of them (probably because of the public personality of Bogdanoffs) go far beyond a simple formal or academic expertise : everyone can feel that people like Kounnas or Jackiw have done a real work on Bogdanoffs thesis. Moreover all the reports following the defense are (in essence) not academic and only aiming at a technical evaluation of the manuscrit. All this shed a new light on Bogdanoffs work which (contrarely to the image created by the rumor) suddenly appears as quite original and serious.

7. **P.Rozinski**
June 11, 2004

I have read all the reports about the two thesis of Bogdanoff brothers : I must admit that it is not only "supportive", it is also very impressive. Mainly because one can obviously see that experts like Majid did invest a lot of time and work on the content of the quantum groups part of Bogdanoff thesis. His 3 different reports are extremely precise and there is no doubt that his conclusions regarding the importance of Bogdanoff theorem are deeply based on his own expertise of the field and a serious knowledge of Bogdanoffs work. His report is a real example of what a good and sound report of expert should be.

8. **Alejandro Rivero**
June 11, 2004

I do not know why they have preferred to register them as CERN-OPEN instead of CERN-EXTernal. I supposse it is because of the involvement of some CERN researchers.

The report of Majid is certainly supportive. It is perhaps on his nature. It should be remarked we are speaking of a couple of PhD thesis, no less but no more. As Majid says, it is the *basis* for future work.

9. **J.Devers**
June 11, 2004

BOGDANOFF THESIS REPORTS

Note that for the first time since the beginning of this affair in 2002, all the 15 reports concerning the thesis of Bogdanoffs are available on the CERN document server :

http://cdsweb.cern.ch/search.py?recid=740747&ln=en

10. **petitot**
June 10, 2004

Dear Grantot,

1) Prof. Yang = Roland Schwartz = J.Petitot = the Bogdanovs.

True. It's a perfect equivalence.

The only way I see out of this mess would be a public grilling of the brothers (on TV, in front of the public, since they want the public to take part) by experts on quantum groups and topological field theory, who would ask them basic questions and then more involved specific stuff.

The bogdanoffs would be delighted to debate with any expert in the fields. A few days ago a debate of this kind was organized and it showed that Bogdanoffs were very convincing as far as their domain of research is concerned. So yes : let's organize this "grill session" as soon as possible.

11. **Grantot**
June 10, 2004

For those who lost the plot:

1) Prof. Yang = Roland Schwartz = J.Petitot = the Bogdanovs. [You can easily recognize them, at least until now, by the way they quote messages, and the way they are aware of absolutely everything about the brothers.]

2) crankbuster = Grantot = somebody who is fed up with this story, in particular to see that they distorted what Schreiber and Woit said and published it in a book now being read by thousands of french people… Under crankbuster I showed that Roland Schwartz talks nonsense. The brothers realized that and then appeared as J.Petitot. I posted again under crankbuster and then Grantot to reply to the messages by J.Petitot. [The name Grantot is a joke: since Petitot could be translated as Small-oh in english, I went for Grantot, which is Big-oh.]

3) Moyentot = somebody else, who has a better sense of humor than me ;-). I think I recognise him from fsm. [Moyentot, as you've guessed, means Medium-oh.]

4) If the brothers can come back to this affair anyway it is also, apart from the aformentioned new distortions, because some of the members of their PhD jurys spoke out in favour of them 3 years ago. That left the door open. We know that Robert Oeckl or Alain Connes said they work on quantum groups was weak, but Majid has been apparently a little more sympathetic. This is on what they are entirely relying, as can be seen when they write (under Petitot) "Regarding my other remarks (Oekl) you did not answer any of my observations : does that mean that you agree that Majid is a more reliable source regarding the quality of Bogdanoffs work than Oeckl?. As far as I am concerrned, I follow his conclusions: Bogdanoffs theorem in quantum groups is original and important."

The only way I see out of this mess would be a public grilling of the brothers (on TV, in front of the public, since they want the public to take part) by experts on quantum groups and topological field theory, who would ask them basic questions and then more involved specific stuff.

12. **petitot**
June 9, 2004

Dear polytot,

Here are some quick reactions to your last message :

You wrote : "since you have published precisely 4 papers and only 4 papers, indeed, a fixed number of 4 papers,

How do you know that I published 4 papers?

Therefore, dear Moyentot, may I kindly ask you to explain to me how it works to make bunches of people seriously discuss a paper that is based on the observation that when setting T=0 in the expression beta=1/T things look really weird?

There is nothing mysterious here : beta is only the inverse of temperature. Come on : you know quite well that T = 0 can be seen as a topological limit of any field theory. And in this case (on this limit) beta goes to infinite (as usual). There are tons of references on this particular point.

Ah, you mean it suffices to mention the term 'quantum groups' in the process, because it will intimidate everybody to such an extend that he forgets to laugh?

Be serious for a minute, please. Would be much more interesting four our readers.

13. **Peter**
June 9, 2004

Hi JC,

The Postmodernism article generator is great. Perhaps someone should write a string-theory version, surely this is possible.

The whole history of string theory and how it took over the major physics departments in the US is very similar to the experience many humanities departments had with Postmodern "theory". The parallels between these two stories are remarkable.

14. **Moyentot**
June 9, 2004

Dear Moyentot –

since you have published precisely 4 papers and only 4 papers, indeed, a fixed number of 4 papers, I am willing to trust your opinion precisely 4/3 the amount that I trust Besnard, who has published 3, no more no less, precisely 3, and 3 only.

(Since I cannot tell right from wrong myself and cannot explain to anyone the mysterious relation between Foucault pundulums and FRW cosmologies, I spend my time counting the number (and precisely the number, the fixed and invariant number) of publications of some people.)

Therefore, dear Moyentot, may I kindly ask you to explain to me how it works to make bunches of people seriously discuss a paper that is based on the observation that when setting T=0 in the expression beta=1/T things look really weird?

Ah, you mean it suffices to mention the term 'quantum groups' in the process, because it will intimidate everybody to such an extend that he forgets to laugh?

My, dear Moyentot, this was a very helpful answer indeed.

Sincerely,
Moyentot.

15. **grantot**
June 9, 2004

Dear Grantot,

you wrote : " The very fact that you ignore what this is (Mathscinet) says it all."

I ignore where you found that I do not know mathscinet. In fact, in my post dated June 8, I wrote : "I have read the article in reference and contrarely to what you say it is obvious that Oeckl is *forced* to recognize the value of Bogdanoff Theorem : he simply cannot deny their result."

A simple contextual reading shows that I wrote having read the paper in Mathscinet. I know this database for years (this is not exceptional, no?).

(By the way, the only J.Petitot in Mathscinet is an epistemologist from Ecole Polytechnique, who never published anything on quantum groups.)

So what?

You wrote : "Now I found this, also published in 2001:

F. Besnard: Number Operator Algebras, Mathematical Physics Electronic Journal Vol 7, paper 4, 59pp, 2001.

OK. I must admit that this electronic publication did not attract my attention beyond the arXiv version. Nevertheless the total number of publications is invariant : 3 and only 3. Even if the quality of a research is not based on the number of published papers, it is a "sign" : Besnard should not give any lessons to anyone, simply because he is not a reference. He failed to meet bogdanoffs proposal regarding a detailed scientific report on their quantum groups theorem. Instead he developped a "dark rumor" on his site and sent all sorts of emails to different people, hoping that he would create a new "affair" (see his site). Do you really think Besnard reacted honestly and objectively? I do not think so and I maintain that his attitude is definitly not scientific.

16. **JC**
June 9, 2004

This thread is reading more and more like an episode of Beavis and Butthead (B&B). Perhaps a better name for B&B (Bogdanov & Bogdanov) would be "Beavis & Bogdanov" and their never ending misadventures in physics hoaxes.

At times I wonder whether the previous posts by Grantot, Petitot, Roland Schwartz, crankbuster, etc … are really just B&B "talking" to themselves in an attempt to produce an "illusion" of dialog. Astroturfing or generating hype deliberately to get even more attention for themselves.

What would be amusing is if somebody rewrote the code to the Dada Postmodernism random essay generator at

http://www.elsewhere.org/cgi-bin/postmodern/

and

http://dev.null.org/dadaengine/

such that it randomly generated legitimate looking "theoretical physics" papers in the style of Beavis & Bogdanov (B&B).

17. **Grantot**
June 9, 2004

Dear Petitot,

you wrote: "the reference does not appear in any database, even not arXiv where the paper is "due to be published in lmp"

but you've just been told that they appear in Mathscinet! The very fact that you ignore what this is says it all. (By the way, the only J.Petitot in Mathscinet is an epistemologist from Ecole Polytechnique, who never published anything on quantum groups.)

You also wrote: "Nevertheless, all in all (and this time I am sure) Besnard had only 1 paper published in 2001."

Now I found this, also published in 2001:

F. Besnard: Number Operator Algebras, Mathematical Physics Electronic Journal Vol 7, paper 4, 59pp, 2001.

18. **Alejandro Rivero**
June 9, 2004

Noticing the relationship between this new surge and the B&B new book, Urs has suggested to go to the amazon.fr website and similar selling points and upload the criticisms there.

19. **petitot**
June 9, 2004

This is a reply to Crackbuster.

You are quite right regarding the publication in L.M.P. : I had not seen that Besnard had indeed one paper published in this journal (the reference does not appear in any database, even not arXiv where the paper is "due to be published in lmp). So yes, I admit I was wrong.

Nevertheless, all in all (and this time I am sure) Besnard had only 1 paper published in 2001. He never published anything since then. Nothing wrong with that. But he should not pretend (as he does on his site) that he is the mighty scientific Judge of Bogdanoffs case. They have published 6 papers, Besnard only one (and still, in LMP : do you know that this journal was created by M.Flato, the first director of thesis of…Bogdanoff? and do you also know that LMP is managed today by D. Sternheimer, the second director of thesis of Bogdanoff?)

Regarding my other remarks (Oekl) you did not answer any of my observations : does that mean that you agree that Majid is a more reliable source regarding the quality of Bogdanoffs work than Oeckl?). As far as I am concerrned, I follow his conclusions: Bogdanoffs theorem in quantum groups is original and important.

20. **crankbuster**
June 9, 2004

An now a reply to Petitot (and I'll stop there):

3. anybody can go to the website of Letters in Mathematical Physics and see that Besnard indeed published there the following:

F.Besnard, Number Operator Algebras and Deformations of ε-Poisson Algebras, Letters in Mathematical Physics, 55 (2): 113-125, February 2001.

So now who is lying ?

21. **Petitot**
June 8, 2004

This is a message to "crankbuster" in reply to certain elements of his post that appears not to be correct.

Contrarely to him, I am an expert but (not for the same reasons) things also look dodgy to me. Here are my answers :

1. Do you know that there is a review of one of the B&B papers by R.Oeckl (who did his PhD with Majid apparently) in Mathscinet, see the review MR1894907 (2003f:81231) at . In it Oeckl says:

"even if they had shown the existence of the bicrossproduct this is completely at odds with the sentence following the "proof": "Clearly, Proposition 4.1 proves the possible `unification' between the q-Lorentzian and the q-Euclidean Hopf algebras at the Planck scale."

This is not a valid example. I have read the article in reference and contrarely to what you say it is obvious that Oeckl is *forced* to recognize the value of Bogdanoff Theorem : he simply cannot deny their result. Majid himself recognized this original result in his report dated Jan.2000 : "The (Bogdanoff work) contains useful algebraic constructions of cocycle Hopf algebras of various kinds motivated from physics.The basic theme is to mix algebraic structures associated to the Euclidean and the Lorentzian signatures into single algebraic constructions. Bogdanov identifies this as constructing certain cocycle Hopf algebras ot a type not seen before. These cocycle bicrossproduct results, in section 3.3, from a body of original work which could certainly be the basis of a published research paper.

So you have a specialist in quantum groups (the thesis director of Oeckl himself) who writes that Bogdanoff results are original and could certainly be the basis of published research paper."

Question : Who shoul we beleive? you (and your vague quotation of Oeckl) or Majid himself?

3. This is wrong: Mathscinet says F.Besnard published 3 papers.

Sorry : this is a typical distortion of the truth. Besnard has never published any paper in a refereed journal : the 3 papers that appear in the database are only published in the archive (and not in a journal). Besnard has never published in a journal based on experts committee.

7. It is not the first part of the sentence which is biased but the second part. I agree with your traduction of the first part as: "As the physicist Peter Woit said to us with some despite, these new instruments (quantum groups) are not well known by physicists:".

But the traduction of the remaining french text is ": it is absolutely clear that you have obtained new and useful results about quantum groups, but to understand what you wrote and how it relates to what is already known requires an expertise that only a handful of people in the world possess." So I confirm what P.Woit wrote in the first message in this thread: they seriously distorted what he meant.

No. Because when you read the whole sentence in french it does not sound at all like a supportive text. You see, the important thing is the "global content" of a phrase, not a framgment of it. The first part of Woit's sentence (where Bogdanoff insists on Woit's dispite) relates to the rest of the phrase and, in a way, disqualifies any support. This was recognized by anyone around me.

In conclusion, here, the "twisters" are Besnard and Woit. Not the Bogdanoff.

22. **crankbuster**
June 8, 2004

This is a reply to the post by R.Schwartz. I'm not an expert but even on the surface things look dodgy to me. Numbers refer to his post:

1. Do you know that there is a review of one of the B&B papers by R.Oeckl (who did his PhD with Majid apparently) in Mathscinet, see the review MR1894907 (2003f:81231) at . In it Oeckl says:

"even if they had shown the existence of the bicrossproduct this is completely at odds with the sentence following the "proof": "Clearly, Proposition 4.1 proves the possible `unification' between the q-Lorentzian and the q-Euclidean Hopf algebras at the Planck scale." " So here you have a specialist in quantum groups who disagrees.

3. This is wrong: Mathscinet says F.Besnard published 3 papers.

7. It is not the first part of the sentence which is biased but the second part. I agree with your traduction of the first part as: "As the physicist Peter Woit said to us with some despite, these new instruments (quantum groups) are not well known by physicists:".

But the traduction of the remaining french text is ": it is absolutely clear that you have obtained new and useful results about quantum groups, but to understand what you wrote and how it relates to what is already known requires an expertise that only a handful of people in the world possess." So I confirm what P.Woit wrote in the first message in this thread: they seriously distorted what he meant.

23. **Thomas Larsson**
June 7, 2004

The book "An guide to quantum groups" by V Chari and A Pressley has a 60-page biography with references to just about anybody that has ever written anything remotely connected to quantum groups, including an obscure reference to myself. Oddly enough, this huge biography does not mention neither to R Schwartz nor to L Yang…

24. **Urs Schreiber**
June 7, 2004

It is not true that it is hard to understand what Bogdanov & Bogdanov are saying. It is easy.

To see if I got them right I once wrote a little summary of their ideas. They told me that this summary is 'very accurate' and that I am among the few who understands what they are aiming at. But what I had summarized of their ideas was a list of nonsense! (See here for more details)

Luckily, they even quote my summary in their new book. They removed the phrases that were obviously critical and made it sound like I am supporting them, but neverthelesse fortunately now everybody can read these couple of lines containing very simple reasoning, understandable to everyone who knows what a partition sum and what a topological field theory is. And what they write is obviously nonsense.

It is the familiar kind of nonsensical speculations that some students make when being confronted and while learning new theories and techniques. The only difference is that the usual student realizes that he was on the wrong track and moves on. B&B wrote a book about it. Apparently their only concern is being in the media as often as possible.

25. **Roland Schwartz**
June 6, 2004

Let me first introduce myself : I am a mathematician and I think I know quantum groups theory well enough to clearly make up my mind about Bogdanoff's work. Following an article that was written by a certain Fabien Besnard against the Bogdanioffs, I decided to post my answer on fr.sciences physics newsgroup. My remarks were as follows :
1. The quantum groups work made by Bogdanoffs is sound and original. They have constructed a bicrossproduct of a new type between lorentzian and euclidean forms.

This construction gave a sound theorem. The value of this theorem has been accepted as such by experts of quantum groups theory (Majid, Gurevich, Oeckl, etc. By the way, Oeckl is nos a supporter of Bogdanoffs but he had to admit their results – see SPR).
2. Fabien Besnard criticized the Bogdanoffs without having the expertise to do so. He does not know quantum groups and therefore is not even able to read Bogdanoff's work.
3. Fabien Besnard never published any paper. He can hardly be considered as an expert in any field.
4. In a private mail to Bogdanoffs (see ref on this page) Peter Woit recognized that he cannot understand any of their work in quantum groups.
5. Nevertheless, he claims that Bogdanoffs work is nonsensical.
6. My conclusion is that (based on his own words) the opinion of Peter Woit on the particular point regarding the value of Bogdanoffs work is therefore totally nonsensical.
7. I have read and compared the translation made by Bogdanoffs in their book regarding the comment of Peter Woit. Contrarely to what claims Fabien Besnard, this (very short) phrase reported by Bogdanoffs does not give at all the feeling that Woit is supporting them : "Ces nouveaux instruments (groupes quantiques) sont tr?s peu connus des physiciens comme nous l'a confi? avec une pointe d'amertume le physicien Peter Woit : Il est tout ? fait certain que vous avez obtenu des r?sultats nouveaux et utiles dans les groupes quantiques, mais comprendre rapidement la signification de ce que vous avez ?crit et comment cela se raccorde ? ce qui est d?j? connu requiert une expertise que seule une poign?e de gens poss?de dans le monde." This phrase translates as : "As the physicist Peter Woit said to us with some despite, these new instruments (quantum groups) are not well known by physicists..;" etc. In french language the work "amertume" has a negative connotation and it is quite clear that Woit does not appear in the book as a supporter of Bogdanoffs work. This prooves that Mr Besnard twists the reality and appears as a dishonest and unrelyable agent in this artificial polemique. Once again, there is no phrase, nowhere in the book, indicating that Woit is a supporter of Bogdanoffs work. From our own view, following a serious expertise of their construction in quantum groups, we maintain that it is sound, original and very interesting. If Fabien Besnaard or Peter Woit think the countrary, then they have to demonstrate it.

26. **D R Lunsford**
June 5, 2004

Well that's a good point. It's OK to be a crackpot if you know what you are talking about 🙂 It's not OK to be just wrong.

27. **Steve**
June 5, 2004

A quick web search shows that the "International Institute of Mathematical Sciences" does actually exist in Hong Kong, with a Prof Yau as director(same as in Calibi-Yau fame I think). However, nothing can be found on a "Prof. Liu Yang" except for the same pro-bogdanov email posted to an obscure web forum/discussion group. See http://www.matthewyglesias.com/archives/
002028.html
and scroll down.
This was not posted by "Prof. Yang" himself but by "one of his colleagues" lol. Says "something also about "we are mathematicians and not showmen like John Baez". It seems that this "Prof Yang" and "his colleagues" not only have the time, expertise and desire to fully study, comprehend and appreciate the depth, originality and potential of the Bogdanov papers in detail, but also have additional time to trawl the web and participate in any obscure forums where the Bogdanovs are criticised in any way, and thus come to their defence with "expert opinions". Sounds highly suspect and highly unlikely in the extreme. And like you mentioned, the internet connection seems to be coming out of France. Still you have to give these dudes an "E" for effort lol.

28. **Alejandro Rivero**
June 5, 2004

Well, I write back to crackpots sometimes, simply suggesting readings or points to work out; if/when then show they are not interested in physics but in "his theory", I hang the line.

On the other side, I have become sort of crackpot myself. The whole history is not to be told yet, but the result is that my last work seems very crackpotty if read from some standpoints. As a byproduct I have had the opportunity to see the rejection mechanism completely at work. I didn't know, for instance, that the barrier was not in the referee side but in the editors. A double edged sword, because when a referee receives a work, he could already to assume that the filter has been passed, thus no further checks against "bogdanoff maladie" are done.

de Branges and the Riemann Hypothesis

June 10, 2004
Categories: Uncategorized

Louis de Branges is a mathematician at Purdue who has had a long history of claiming proofs of the Riemann hypothesis. His latest claim has lead to a press release from Purdue. The press release points to what seems to be an older manuscript by de Branges outlining some of the history of the Riemann hypothesis and his work on it. This also includes some history of his ancestors, and de Branges has taken to calling himself “Louis de Branges de Bourcia”. He more or less promises that if he wins the Clay million dollar prize for solution of this problem he would use it to restore the ruined chateau de Bourcia for use as a mathematical research institute.

The actual purported proof is here. One mystifying thing about it is that in the abstract and introductory paragraph it repeatedly refers to relations to quantum mechanics, but there seems to be nothing about this in the body of the paper. Weyl’s book on quantum mechanics and group theory appears in the references, but nothing in the text seems to refer to this.

de Branges has a checkered history as a mathematician, with several of his claimed proofs of the Riemann hypothesis and other problems turning out to be incorrect. On the other hand, he did produce a correct proof of one well-known problem, the Bieberbach Conjecture. In that case his initial manuscript was pretty impenetrable, but after he explained his ideas to a group of Russian mathematicians, they gave a more understandable version of the proof and it became clear that de Branges really did have a proof. It looks like this one may also take some major effort to see what he really has.

For more about de Branges and the Riemann hypothesis, see the recent popular book “The Riemann Hypothesis: the Greatest Unsolved Problem of Mathematics” by Karl Sabbagh. A review of this book has some interesting comments about de Branges and his NSF funding.

A couple weeks ago a preprint appeared on the arXiv by R. A. Arenstorf, a mathematician at Vanderbilt University, claiming a proof of the twin prime conjecture. I asked one of my colleagues who is an expert on the subject about it and he said he didn’t believe it and would bet $100 it was wrong. Today I see that Arenstorf has withdrawn the preprint, saying that a serious error has been found.

Comments

1. **v.z.n**
   June 22, 2004

   cutting edge algorithmics & mathematics including
   the 7 claymath $1M problems. esp P vs NP

   http://groups.yahoo.com/group/theory-edge/

2. **Jack Sarfatti**
   June 20, 2004

   Peter has done the honorable thing and removed the objectional message making false allegations about me and my ideas.

   There is a book review in current Physics Today making reference to relation of this math to quantum theory of random Hermitian matrices describing energy eigenvalues of systems that in classical limit show chaotic behavior.

3. **Jack Sarfatti**
   June 16, 2004

   Is “Trackback” an alias for Peter Woit? If so, he should apologize for his slander of me based on entirely false information and gossip. That is shocking for a professor at an Ivy League University.

4. **Thomas Larsson**
   June 11, 2004

   Never mind. It has resurrected.

5. **Thomas Larsson**
   June 11, 2004

   The link http://www.math.purdue.edu/ftp_pub/branges/riemannzeta.pdf is dead.

6. **Alejandro Rivero**
   June 10, 2004

   “One mystifying thing about it is that in the abstract and introductory paragraph it repeatedly refers to relations to quantum mechanics, but there seems to be nothing about this in the body of the paper.”

   It was Connes who, some years ago, stressed the relationship between quantum mechanics and some methods to attack Riemann hypothesis. As Alain had part in the Clay formulation of the problems, I guess that the remark is just implicit hommage.

Bogdanov Thesis Reports

June 11, 2004
Categories: Uncategorized

From one of the comments here I see that the Bogdanovs have put the [reports]() on their theses on the CERN document server. One should perhaps take these with a grain of salt given their source. For instance, I wouldn't be surprised if some reports were missing.

I've always had some sympathy for the people who ended up on the Bogdanov's thesis committees. It's a difficult position to be in when you have to decide what to do with students who seem to be enthusiastic and have worked hard, but are very weak and have completed not very good theses. A not unreasonable thing to do under the circumstances is to do one's best to find something of value in their work, and leave the job of keeping nonsense out of the literature to journal referees.

But the Bogdanov theses, especially Igor's, were so full of egregious nonsense, in particular with respect to topological quantum field theory, that they should have been beyond the pale. While some of these reviewers were string theorists, others weren't, so the whole mess can't be blamed on string theory.

Comments

1. **Bogdanoffez**
   June 12, 2004

   Well, dear Peter, you see that one of the consequences of your constant position to deny any value to our work gives reactions like : "It would be interesting to see if University of Bourgogne starts to get fed up with all the bad press coverage about the Bogdanovs, that it starts an investigation of their own into their PhD theses work and eventually revokes their PhD degrees."

   It really goes beyond our mind to realize that it seems to be impossible de discuss what we did on "normal" basis. As it seems impossible to everyone to recognize, honestly, that we did serious work. What is going on in your mind? Do we criticize your work? do we say that you produced "nonsense" in all your articles? You perfectly know that we could say so very easily. So please respect our work and we will respect yours.

2. **Peter**
   June 12, 2004

   This is pretty different than an experimentalist faking data. If universities start investigating every theoretical physics thesis that doesn't make sense, this could become a very large project….

3. **JC**
   June 12, 2004

   Just noticed these articles today about Jan Hendrik Schoen (the guy who was discovered to have pulled off a huge data falsification job at Bell Labs in 2002), being revoked of his PhD by University of Constance

   http://www.lasvegassun.com/sunbin/stories/tech/2004/jun/11/061107856.html

   and

   http://science.slashdot.org/science/04/06/11/2142223.shtml?tid=134&tid=146&tid=99

   It would be interesting to see if University of Bourgogne starts to get fed up with all the bad press coverage about the Bogdanovs, that it starts an investigation of their own into their PhD theses work and eventually revokes their PhD degrees.

4. **Peter**
   June 12, 2004

   While I'm not an expert on quantum groups, I know a lot about topological quantum field theories, and I am not relying on anyone else's judgement but my own when I say that those parts of your theses involving TQFT are nonsense. Why this is true has been explained to you in several places by several people. The fact that you have managed to get some physicists to write reports saying something positive about what I know to be nonsense doesn't give me any confidence that the positive reports about the quantum groups stuff are any better founded.

5. **Bogdanoff**
   June 12, 2004

   Good morning,

   We would like to react to Peter Woit comment on our thesis and reports.

   Woit wrote : "I wouldn't be surprised if some reports were missing."

   IGB : There are no reports missing. These are the "official reports" that everyone has the possibility to check with University of Bourgogne. This makes a total of 15 reports (which is quite unusual in itself since normally a thesis only requires 2 reports). This answers clearly the question : there is no "hidden report".

   Woit wrote : "I've always had some sympathy for the people who ended up on the Bogdanov's thesis committees. It's a difficult position to be in when you have to decide what to do with students who seem to be enthusiastic and have worked hard, but are very weak and have completed not very good theses.

   IGB : How do you know that we have completed not very thesis? We are mathematicians, you are physicist. In a sympathetic letter, you wrote us that you do not know quantum groups theory ; "A large part of your work has to do with quantum groups and I'm not an expert in this field."

   Question : why don't you trust Majid when he says "Bogdanov's ideas about signature fluctuations are to my mind about the more original and interesting that I have come across?"

   Why do you refuse to admit that we have build a bicrossproduct "of a type not seen before" (theorem 3.3.2) The basic theme is tomix algebraic structures associated to the Euclidean and the Lorentzian signatures intosingle algebraic constructions. Bogdanov identifies this as constructing certain cocycle Hopf algebras ot a type not seen before : " These cocycle bicrossproduct results, in section3.3, from a body of original work which could certainly be the basis of a published researchpaper. "

We have spent many, many years working with Majid. He knows our ideas from A to Z. Do you think that he would have allowed the work to contain “some mistakes” or nonsensical parts?

The answer is NO.

Woit wrote : ” A not unreasonable thing to do under the circumstances is to do one’s best to find something of value in their work, ”

IGB Yes. That would be a good thing to do.

Woit wrote : But the Bogdanov theses, especially Igor’s, were so full of egregious nonsense, in particular with respect to topological quantum field theory, that they should have been beyond the pale.

IGB : It is rather bizarre that you insist so much on the “nonsense” of our work. After all you wrote that you are not an expert in quantum groups. This theory is the mathematical basis of our signature fluctuations model. If you do not understand quantum groups, you do not understand our model. Nothing wrong about it. But then, stop saying “it’s nonsense”.

Woit wrote : While some of these reviewers were string theorists, others weren’t, so the whole mess can’t be blamed on string theory.

IGB : Do you seriously think that a scientist of the magnitude of Jackiw signed his report without filtering every idea, sentence, proposition, of the thesis? We worked extensively before he would agree with the quality of the work. Jackiw would never have approved “nonsensical” work just because he found us “enthousiastic” (by the way, we never met him before he wrote his report : all the work was based on reading the thesis and exchanging informations or arguments by mail).

We can accept that you disagree, as a physicist, with our signature fluctuations model. But again we are mathematicians. And we developped our work on mathematical basis. Therefore the only thing that matters is : is our major theorem (a cocycle bicrossproduct of new type) valid or not?

The answer given by experts is YES, without any doubt.

If you disagree with this yoy know will have to prove it.

Thank you for your attention,

Igor Boganoff Grichka Bogdanoff

6. **Thomas Larsson**
   June 12, 2004

   I have always felt that the American system, where you kick out weak students after two years if they don?t pass their qualifying exams, is better for everybody than the European system, where the only real checkpoint is the dissertation, and people can linger forever. It is not really fair to let people work for a decade and then deny them a Ph. D.

   I am not sure how consistently the American system is implemented in the US, however. It is probably not so easy to keep people out if they don’t require funding.

Cosmic String Hype

June 12, 2004
Categories: Uncategorized

According to a press release from UCSB, three theoretical physicists have proposed “the most viable test to date for determining whether string theory is on the right track”. This is based on a paper about cosmic strings where the authors manage to cook up a highly unlikely scenario where large strings exist and produce gravitational radiation observable by LIGO in the next couple of years.

Normally in the English language, calling something a “test” of a scientific theory would indicate that if it doesn’t work the theory is wrong. When LIGO doesn’t see this effect in the next two years I kind of doubt that there will be wholesale abandonment of string theory.

Comments

1. **Peter**
   June 17, 2004

   Hi JC,

   That’s an interesting looking article, and the older article by Feynman they reference is probably worth reading. I don’t know much about this stuff, but have often seen how tricky gauge theories are exactly because there is no simple set of gauge-invariant variables to work with.

2. **JC**
   June 17, 2004

   Just noticed this paper this morning which reviews the variational principle in nonperturbative qcd:

   “Variational techniques in non-perturbative QCD”
   by Kovner and Milhano, hep-ph/0406165

   Progress appears to be relatively slow over the years in finding a reliable variational setup, when taking account of gauge invariance issues.

3. **Peter**
   June 17, 2004

   Hi Urs,

   That’s fine. If I put something on the weblog, it’s for public consumption and you quoted me accurately and fully. I just wanted to explain why I wasn’t even trying to answer your further question, and to reiterate that I’m no expert on this and definitely not speaking for d’Hoker or Phong.

4. **Urs**
   June 17, 2004

   Re: superstring finiteness

   Peter, I apologize if you don’t like to see your summary on sps. I thought that since you made it publically available here at your weblog you are content with its public distribution. I took care to fully indicate your qualifications that you just reported, possibly incompletely, the opinion of third person(s). I hope nobody is harmed by the fact that Phong and D’Hoker see a subtlety in Berkovits’ approach.

5. **Peter**
   June 17, 2004

   Since I’m not an expert, I really don’t want to get involved in a discussion of the details of this issue. If Berkovits, D’Hoker and Phong want to discuss this issue in a public forum that would be great, but I’ve already perhaps gone too far in reporting a perhaps garbled version of private conversations.

   As far as I can tell though, everyone involved agrees that there is no proof of higher-loop finiteness, with one difficulty being understanding what Berkovits calls the “unphysical divergences” in moduli space.

6. **Urs Schreiber**
   June 17, 2004

   Re: superstring finiteness

   Peter paraphrased Phong and D’Hoker as saying:

   > To make them [the picture changing operators] well-defined in a way that is gauge invariant requires understanding some global terms.

   Many thanks for this information about your discussion with Phong and D’Hoker. I have taken the liberty to quote your comment over at sci.physics.strings. Maybe we get the chance to see Nathan Berkovits’ opinion on this issue.

   For instance, is D’Hoker&Phong’s criticism dealt with by the remarks on p. 14 of Berkovits’ hep-th/0406055?

7. **JC**
   June 16, 2004

   Peter,

   Has anyone ever successfully applied the variational technique in examining “nonperturbative” or strong coupling phenomena in any quantum field theories? The variational technique at getting approximate solutions seems to be in almost every quantum mechanics textbook, while it seems to be a rarity in most quantum field theory books.

If I didn't know any better, the only "trial functional" that appears to be easily integrable is something that resembles a gaussian. I can't think of any other obvious "trial functional" that could be easily dealt with analytically in the variational setup.

8. **Peter**
June 16, 2004

You can't claim to have a proof based on an unproven assumption.

I don't want to speak for D'Hoker and Phong, but this is my probably somewhat garbled understanding from having discussed this with both of them:

Berkovits's claim of finiteness explicitly assumes "there are no unphysical divergences in the interior of moduli space". This assumption (that these divergences cancel) is exactly what is hard to prove in the two-loop case and no one knows how to do for higher loops. In conformal gauge Berkovits argues that "there are no obvious potential sources for these unphysical divergences in the interior of moduli space since the amplitudes are independent (up to surface terms) of the locations of picture-changing operators." D'Hoker and Phong have found that the correct definition of picture-changing operators is quite subtle here. These are operator products at a point and their definition is ambiguous. To make them well-defined in a way that is gauge invariant requires understanding some global terms. Unless you do this you don't have well-defined picture-changing operators and can get whatever answer you want.

Again, I'm obviously not an expert at this, but that is my understanding of what the experts told me. While I don't want to speak for them, from what they told me I am under the strong impression that these experts don't believe that Berkovits has a proof.

9. **Urs Schreiber**
June 16, 2004

Concerning the higher-loop finiteness of string theory:

A couple of days agao Nathan Berkovits claimed to have a proof for the finiteness of the superstring at every order.

See his message on s.p.s.: apparently one unproven assumption enters the proof, which is argued to be true in covariant contexts at least.

10. **JC**
June 16, 2004

Thomas,

I was thinking of something outside of the various lattice models, such as a Thirring or Sine-Gordon type of model beyond 2 dimensions.

Lately I've been reading Kleinert's book on path integrals, where he presents a path integral way of doing many of the traditional quantum mechanics problems like the hydrogen atom, infinite square well, etc …. and other interesting looking cases I've never seen before. I was thinking more along the lines of whether somebody has ever come up with an exact analytical expression for the path integral of an "interacting" quantum field theory in 3 or more dimensions, without resorting to any approximations.

On a slightly different track, awhile ago I was reading some book (don't recall the author's name offhand) about various "quasi-exact" solutions to the Schrodinger equation for various potentials with terms like x^4 and/or x^6 with particular coefficients that makes it easier to get an analytical solution of some sort. (On the surface, it appears to be a very sophisticated way of doing a WKB approximation). Though still an approximation, I wondered whether the same "quasi-exact" tricks can be applied to some interacting quantum field theories to get a non-trivial path integral, which doesn't use perturbation theory. The upshot of the "quasi-exact" method seems to be finding coefficients for the x^4, x^6, etc … terms such that the expression under the square root sign inside the WKB integral takes on a "nice" form. Usually this "nice" form looks like a factorization of the expression under the square root sign, for the less horrible cases.

Naively I tried to see whether it can be applied directly to something like phi^4 or QED, but so far I've been stumped. I went searching through the literature for path integral treatments of 'quasi-exact' type of problems in quantum mechanics, but so far they haven't led to much further insight. After awhile I just dropped the problem and moved on to something else.

If "quasi-exact" types of solutions can be found for something like phi^4 theory, it would be interesting to see what a semiclassical calculation around these "quasi-exact" solutions would produce.

11. **Thomas Larsson**
June 16, 2004

"Has anyone ever found any exact analytical solutions to any "interacting" quantum field theories in 3 or more dimensions, without using any approximations at all?"

JC,

Would exact solutions to integrable lattice models count? In 2D, a sufficient (and in practice necessary) condition is that one has a solution to the Yang-Baxter equation,

R_12 R_13 R_23 = R_23 R_13 R_12,

where R_12 acts one the tensor product of three spaces, trivially on the last. At criticality, such a lattice model is described by a conformal field theory.

There is an analogous condition in 3D, known as the tetrahedron equation,

R_123 R_145 R_246 R_356 = R_356 R_246 R_145 R_123,

which acts on the tensor product of six spaces. A solution to this equation can be translated into a solution of some 3D lattice model, which would be described by a field theory at criticality.

Unfortunately, I am not aware of any good solutions to the tetrahedron equation. Zamolodchikov found one solution in 1981 (and introduced the tetrahedron equation in the same paper), but his model lack unitarity; Baxter later translated his work into a lattice model and showed that some of the Boltzmann weights are negative. Nevertheless, the Zamolodchikov model is believed to be at a critical point and should therefore be a field theory.

12. **JC**
June 15, 2004

Has anyone ever found any exact analytical solutions to any "interacting" quantum field theories in 3 or more dimensions, without using any approximations at all?

13. **Peter**
June 15, 2004

In the context of string theory, where the perturbation expansion is the only thing that is well-defined, "non-perturbative" is to some degree a synonym for "things we don't understand but would like to exist"

In many quantum field theories, the theory is well-defined outside of a perturbation expansion and there can be lots of different ways of trying to do "non-perturbative calculations", e.g.

1. lattice Monte-Carlo

2. 1/N expansion

3. semi-classical methods: take a non-zero solution to the equations of motion and do your perturbation theory about that instead of about the zero field. This is where instantons, solitons, etc. come in.

14. **JC**
June 15, 2004

I get the sense folks like to throw around the word "nonperturbative" as if it was a "fudge factor" that solves all the "diseases" and problems in their theories.

I see that the popular choices of nonperturbative things that many folks like to invoke, seem to be objects like monopoles, instantons, or some other "soliton" type of object. Are there other "nonperturbative" effects and/or objects than these ones, which are not too horrible to deal with analytically and algebraically?

Over the years whenever I saw the word "nonperturbative" in many papers, I started to become very skeptical. Many seem to be mostly "hot air" and hype, than anything concrete. Though one case that looked impressive on the surface was the Seiberg-Witten stuff from a decade ago, in calculating "nonperturbative" instanton corrections to N=2 SUSY Yang-Mills. To a lesser extent in the early 1990's, the mirror symmetry stuff in doing instanton calculations in string theory also looked impressive on the surface at the time.

15. **Peter**
June 15, 2004

You can set up the perturbation series for any choice of Calabi-Yau, with any choice of moduli parameters fixing its size and shape. If the series is only asymptotic you can hope that unknown non-perturbative effects choose the Calabi-Yau and fix the moduli. If the series is finite, you have a consistent theory for each Calabi-Yau and each value of the moduli.

16. **JC**
June 15, 2004

Peter,

What's the argument behind point 3, where there would be an infinity of consistent string theories if the full perturbative series is finite?

17. **Peter**
June 15, 2004

I was thinking of writing a blog entry related to this sometime, but here are the facts:

1. One-loop and two-loops are finite, the latter is a recent result of Phong and D'Hoker.

2. Higher than two loops are conjectured to be finite, but this has not been shown.

3. The full series is conjectured to be asymptotic. No reason to believe it is finite (and if it were there would be an infinity of consistent string theories).

18. **JC**
June 15, 2004

Has anyone ever shown that the perturbative expansion of superstring theory is renormalizable or finite, to all orders in perturbation theory? Is it even a convergent series, or at best only a asymptotic series? Can this even be done at all in a rigorous manner without much "hand waving"?

19. **Peter**
June 15, 2004

No, I'm not happy. You still don't seem to understand the difference between having a well-defined theory and not having one. QCD has a precise and simple non-perturbative definition (lattice QCD) and well-understood calculational methods that give controlled approximations to the exact theory. For some physical variables (the low-lying spectrum) you can do reasonably accurate calculations with control of the errors and, within the errors, you get results that agree with experiment. For others (S-matrix elements), you have a well-defined theory, but no good calculational methods. If string theory is ever going to be useful it probably would be in attacking this problem, rather than as a TOE.

People can do calculations in 11d supergravity and, when asked about the divergences, can say "oh, I'm really doing M-theory, the magical, mystical ultraviolet completion of supergravity" if they want. But they should be a lot more honest about what is wishful thinking and what is real science. You're welcome to tell us that supergravity is just an "effective theory", but normally when people work with an effective theory it is one that can be compared to experiment. In QCD the sigma-model is an effective theory for pions; you can calculate things about pions and compare them to experimental results. The "effective theory" you are working with predicts absolutely nothing, it is a complicated setup for making excuses for not being able to predict anything, not real physics.

20. **Thomas Dent**
June 15, 2004

For QCD, read "QCD at strong coupling". Happy now? Then perhaps you would give the reference of the paper which predicts the proton-proton scattering cross-section at low energies from the QCD action. Maybe the lattice people will get round to it about the same time as they do matrix theory.

Supergravity is an *effective theory*. Just like the theory of pions at low energies. It works if corrections to it are sufficiently small, which is true in the small curvature and low energy regimes.

Or it can be thought of as like electromagnetism. You don't need to know the short-distance structure of an electron to use Maxwell's equations. However, the fact that Maxwell's equations admit a solution with a pointlike charged object is a strong indication that a more complete theory should include something that looks like such an object at long distances. In supergravity (mutatis mutandis) this is a p-brane.

Polchinski showed that the supergravity calculation of the force between two p-branes and the perturbative string theory calculation of the force between two D-branes exactly corresponded. This calculation, and the fact that the electric and magnetic charges behave correctly, is regarded as a "good enough" reason to admit branes as objects in string theory. They have not led to any fatal internal contradiction so far.

Indeed string theorists are very commonly doing calculations of a similar sort, finding correspondences between different theories that lead many people to believe that (unless there is some stupendous conspiracy of coincidences) the objects of apparently different string theories can be translated from one into another, and into the objects of 11d supergravity including solitonic (magnetic) and singular (electric) charged solutions.

Some people call this "M-theory": of course this is just a name. If you want to attack string theorists for saying the words "M-theory", you are free to do so, but it is not a strong scientific argument. Such an argument would begin to focus on specific claims about specific theories, e.g. whether 11d supergravity on R^10 X S^1 does really correspond to the IIA superstring.

21. **Peter**
June 14, 2004

Actually many of the "M-theory" things I see are just classical supergravity. There's often no quantum theory, much less higher loops. As far as I can tell, absolutely no one has the slightest clue about how to make progress on finding the new magical theory that will do what they want. The existence of this now ossified ideology of the magical eleven-dimensional theory is part of what is killing off the field. Everyone runs around repeating to each other the same misguided wishful thinking, and people end up spending their whole careers wrapped up in trying to make sense of an idea which just doesn't work.

22. **JC**
June 14, 2004

When folks are doing calculations of supergravity and/or Yang-Mills theory in 4 or more dimensions, what exactly are they trying to find if the theory is nonrenormalizable at higher loops? Are they searching for some "miracle" that will cancel out the nonrenormalizable stuff? I remember a number of older supergravity papers from the 1980's which attempted to find "miracles" in higher order loop calculations. In the end many papers had a conclusion of the sort "there are no miracles in field theory".

Every time I came across papers which attempted to deal with nonrenormalizable theories via "nonperturbative" means, I always got the impression many of these papers were mostly "hot air" and hype than anything concrete. Many of the "nonperturbative" calculations done weren't entirely convincing.

23. **Peter**
June 14, 2004

The only thing at all like even a conjectural definition of M-theory is certain versions of Matrix theory, which only work on special backgrounds (e.g. flat 11d). With seven compactified dimensions, there isn't even a conjecture of what might work, just wishful thinking that something will. Whatever M-theory is, it is supposed to reduce to 11d supergravity at low energies while somehow getting around the non-renormalizability problems at high energies. In practice when people say they are calculating something in "M-theory", they almost always mean they are doing an 11d supergravity calculation.

24. **JC**
June 14, 2004

Has anyone ever come up with a better "definition" of M-theory, besides just saying that it has a low energy limit of 11 dimensional supergravity and/or it reproduces the other string theories as some limit in 10 dimensions? Everybody I've asked this question to, almost always gave those two limits as a "definition". A few folks stated "M-theory" as being the limit of some matrix theory like BFSS a number of years ago, which doesn't seem to be as popular these days. I haven't heard of any better "definitions", which does NOT use the statement "in the limit of …*blah blah blah*".

At times I wonder whether this is just pure wishful thinking on the part of string folks, by being purposely vague about what M-theory is really all about.

25. **Peter**
June 14, 2004

So, if the conjecture that there is an underlying M-theory is just wrong, and no such theory exists, string theorists will never, ever give up?

Comparing a theory that doesn't exist (M-theory) to QCD is really just silly. There are two huge differences:

1. QCD is a beautiful, well-defined theory with no free parameters (1 if you count the theta angle), which makes an infinite number of detailed, specific predictions. The reason a mathematics foundation has a prize for its solution is that it's a well-defined problem to rigorously prove things about QCD. Many things can be reliably calculated to fairly high precision, using perturbation theory or lattice methods.

2. Not only does the theory make predictions, but they all work. To within the accuracy that one can calculate, you get complete agreement with experiment. These predictions cover a huge range of particle physics phenomena: e+ e- annihilation, deep inelastic scattering, phenomena at hadron colliders, properties of charm and bottom bound states, etc. The theory has been tested and tested and tested again and has passed every test. For an example, see the first figure in Wilczek's hep-ph/0212128.

Compare this to M-theory, where there is no theory at all, just a bunch of people's wishful thinking that a theory might exist with properties that they would like. This non-existent theory makes zero physical predictions, so it can never be tested, allowing some people to spend twenty years going on about how wonderful their "theory" is and now doing really silly things like claim that its status is similar to that of QCD.

26. **Thomas Dent**
June 14, 2004

Susskind's baby technicolor was very popular at one point, until it was realized that one can't really calculate much in it, due to strong coupling, and that so far as one could calculate the electroweak loop corrections probably went in the wrong way compared to data.

Nowadays strongly coupled gauge theory is understood slightly better – thanks mainly to SUSY and string theory! – but it's still difficult to get technicolor off the ground in the sense of agreeing with LEP precision data. It's a very elegant idea, more so even than SUSY, but somehow Nature doesn't appear to be sold a bunch on it.

As to what would cause string theory to be abandoned: if a real underlying M-theory were formulated and solved and none of the solutions were anything like the real world.

In other words, the theory isn't sufficiently well understood that it can be discarded.

As for renormalizable gauge theories providing precise testable predictions, this hasn't really happened with QCD. It's taken several decades of lattice computations to come up with a reasonable (to within a few percent) baryon and meson spectrum. Chiral fermions were only recently latticized. And this hypertrophy of computation, although impressive, is about as far from a simple and elegant explanation of observed facts as can be. The million dollar prize for explaining confinement in QCD goes unclaimed.

For decades, QCD has also satisfied the description "not sufficiently well understood that it can be discarded"…

27. **JC**
June 14, 2004

Peter,

The cases of physics folks being denied tenure I'm familiar with, usually fit into one of two categories:

1 – folks who changed from a trendy hot area of research to another field that wasn't as trendy or hot

2 – folks who change from one trendy hot area of research to another field that was also trendy and hot

The first category of folks were frequently denied tenure already at the department level. Their papers were not getting many citations, if any citations at all, besides citing their own papers. These particular cases seemed to be pretty clear cut as to why they were denied tenure.

The second category of folks weren't quite as clear cut. They were commonly cranking out average to below-average papers which resembled "resume padding" stuff in their new field, compared to their papers in their previous field which got more citations and were somewhat better than "average". Perhaps their tenure committees used the "decline" of the quality of their papers, as an excuse to deny them tenure? A few cases even used teaching evaluations as an excuse to deny tenure, just to get rid of a person who they didn't like.

The exceptional cases of folks changing fields who eventually got tenure, usually were producing better papers in their new field and were able to attract many more citations. To top it off, some were even invited to lecture at summer schools such as TASI or Les Houches. Being denied tenure would have been surprising for these particular exceptional cases, unless it had to do with nonresearch reasons like politics or misconduct.

28. **Peter**
June 14, 2004

It's pretty premature to speculate about what will happen if SUSY and string theory are no longer perceived as promising things to work on. But many people in this business are very used to shifting gears and if something else replaces SUSY/string theory they'll jump on that pretty quickly.

29. **JC**
June 14, 2004

Peter,

What do you think will happen to all those grad students, postdocs, and assistant professors up for tenure review, who invested many years on SUSY and/or string theory if both SUSY and string theory die and become "illegitimate" fields of physics research? (ie. "illegitimate" in the sense of folks still doing research on something like bootstrap analytic S-matrix theory after the wholesale abandonment of it, for example). Arguably for folks who already have tenure, their careers could die away like Geoff Chew's career (ie. a painful decline into irrelevance), if they don't change into another field

I suppose the grad students and early postdocs (ie. folks on their first postdoc) could always change fields more easily. I wonder if the later postdocs and assistant professors (without tenure or on tenure track) can easily change fields without much "career disruption". Without mentioning any names, I knew of a number of cases of assistant professors in physics who attempted to changed fields about a year or two after getting their faculty jobs. By the time they were up for tenure review, almost every single one of them were denied tenure regardless of how many citations their papers were getting, except in a few very exceptional cases where the person in question became a "superstar" overnight.

30. **Peter**
June 13, 2004

Yes, in the technicolor idea the Higgs would be something like a Cooper pair.

The vague idea about chiral gauge theories is more something like this: when there's an anomaly one way of thinking about what happens is that the gauge degrees of freedom acquire non-trivial dynamics. Then there are problems with non-renormalizability and/or unitarity, which is why people think the such theories are inconsistent. In the standard model, the quarks and leptons each separately have an anomaly, but they cancel against each other. Perhaps there's more to this story than one sees in perturbation theory, and it might have something to do with the Higgs.

31. **D R Lunsford**
June 13, 2004

That is very nice, I've been thinking a lot about directional time as well 🙂

You need a natural idea of orthogonality in time.

So the technicolor theory is basically Cooper pairs for the Higgs?

As for "non-perturbative quantization of chiral gauge theories", do you mean something like the Dirac monopole argument? It always seems that gamma_5 is the center of the mystery.

32. **Peter**
June 13, 2004

The standard speculation is some new strong dynamics for which the Higgs is a bound state (technicolor).

Two completely ill-defined speculations I've always found attractive, but have no idea how to turn into anything real are:

1. The non-perturbative quantization of chiral gauge theories is more subtle than we think and if we understood it better we'd find that the Higgs appeared naturally.

2. Despite appearances, we really live in Euclidean 4-space, with group of frame rotations Spin(4)=SU(2)xSU(2). One of the SU(2)s is spatial rotations, the other is the electroweak gauge group. A choice of time direction is what breaks the electroweak SU(2).

You really shouldn't take any of the above seriously unless I can someday figure out how to turn them into a well-defined proposal of a new theory.

33. **D R Lunsford**
June 13, 2004

Peter said

"something more interesting than a scalar Higgs"

Care to speculate? 🙂

34. **Peter**
June 13, 2004

I think it was Max Planck who commented that the way science progresses is by people dying, not by them changing their minds. There will be plenty of supersymmetry die-hards, but I get the impression that Witten and others are already starting to get used to the idea that the whole low-energy supersymmetry scenario might be wrong.

Optimistically, what will happen in 2008 is that the LHC will find evidence for what is really causing electroweak symmetry breaking and it will be something more interesting than an elementary scalar Higgs. If so, most people will drop low energy supersymmetry immediately, pretending the whole thing never happened.

35. **JC**
June 13, 2004

Any bets on whether string theory will die if a light Higgs is NOT found at LHC? If I didn't know any better, I would think many string folks will just keep on pushing up the SUSY breaking scale and attempt to justify SUSY only as a symmetry at higher and higher energies such as at the GUT or Planck scales. It seems like a neverending game of "suspending disbelief" where the masses of SUSY particles keep on getting heavier and heavier with time.

Perhaps it will take another generation or so for SUSY and/or string theory to completely disappear from the thinking of physicists, if LHC rules out SUSY at the low energy scales.

It would be interesting to look back into physics history more than a century ago, and examine how long it took to shake off the idea of an "ether" in many physicists' thinking. I wouldn't be surprised if many physicists of the generation directly preceding Einstein, were still "true believers" in an "ether" to the day they died (ie. Lorenz, etc …).

36. **Peter**
June 13, 2004

Hi JC,

Actually at this point the only thing I can think of that would cause a wholesale abandonment of string theory is Witten publicly giving up on the idea. And I don't think that's going to happen until 2008. He and others who are completely invested in the idea will hang on until results from the LHC come out, hoping that superpartners will appear, thus validating part of the whole scenario.

37. **Steve**
June 12, 2004

Typical string-theoretic hype aimed again at convincing the public–one thing the theory has been very highly successful at. Well at least they are trying to connect with experiment and real observations of the universe. However, I would agree that a theory is supposed to be discarded once its experimental predictions simply don't happen. That is the central crux of what science is about, but this won't ever happen in string theory, and that is my main problem with it and the people who do it (not so much the theory itself). There is more chance of a ufo landing in your back yard and Elvis getting out of it than there is of this cosmic string scenario and its predictions actually coming about. I would confidently bet money (a fair bit) that these signals will never appear. Such is the current state of theoretical physics–these guys are tied up in cosmic strings, anthropic Lenny S is lost and wandering in the "Landscape", Ed has exhumed twistors, and the Brothers Bogdanov are again obsessing over their fluctuating metric signature and quantum groups! No disrespect to any of these guys actually (and none intended) but my point is the field is getting crazier each year and people are really in danger of getting lost in/obsessed with their own mathematical fantasy scenarios. The line between crackpotdom and science is now very blurred indeed. Also, some seem so totally convinced they are right that they feel obliged to bring out popular books:the Bogdanovs have one out now and I hear LS is putting one out on the "Landscape". I don't know what the way forward is and maybe (hopefully) things will be much clearer near the end of the decade. But in the meantime, some of these guys could check themselves into the "Rest Home for Deranged Scientists" as was featured in the Thomas Dolby video of the 80s hit "She Blinded me With Science". Not that I can talk–this jacket is too tight, forcing me to type one letter at a time with a pencil in my mouth, and they won't give me a sharpener for my crayons…

38. **Chris Oakley**
June 12, 2004

JC,

There must be something in what you say, as Lubos Motl, the most in-your-face String theorist around, is also an Astrologer.

39. **JC**
June 12, 2004

Peter,

What exactly do you think will cause a wholesale abandonment of string theory?

It seems like having no viable physical predictions isn't going to stop people from being true believers in string theory. Historically, was the first demise of string theory and Geoff Chew's analytic S-matrix theory in the 1970's caused by renormalizable gauge theories producing precise physical predictions that were testable in accelerator experiments, while string theory hardly produced anything other than Regge trajectories at the time?

Offhand I can't think of any obvious compelling reasons that would cause a wholesale abandonment of string theory as if it was like a sinking ship. Even Geoff Chew was still cranking out papers on the bootstrap principle well into the 80's, despite everybody else moving on to gauge theories many years before.

Only naive reason I can think of for a wholesale abandonment of string theory, is if somebody ever found an easy way around the Sagnotti nonrenormalizable 2-loop divergence result in 4 dimensional pure quantum gravity and where additional divergences can be renormalized when fermions, gauge bosons, etc … are added in. This naive scenario would probably require a HUGE miracle to be pulled off, and I'm not particular optimistic about it happening.

At times I wonder if areas like string theory, loop quantum gravity, SUSY, twistor theory, etc … are resembling how economics research is done. Various schools of economics don't appear to be much more than "normative" prescriptions imposed by decree, especially when it agrees with a particular brand of political ideology. Some schools of economics seem to have diehard "true believers" regardless of any empirical data, such as the "supply siders" behind Reaganomics, the Keynesians which managed the economy in the 1970's to 20% inflation in America (or worse, such as 300% inflation in Israel in the early 1980's), the Monetarists which ran the monetary policy in America, Germany, England, etc … in the 70's and 80's by managing monetary aggregates, etc ….. There's some folks who don't even believe in "supply and demand".

It's amusing about a year ago or so, the economics Nobel laureate Milton Friedman finally admitted that his "monetarist" theories of managing monetary aggregates were a total failure in the end, despite being popular policies in government central banks over the last 30+ years. It took millions of lives and decades of economic mismanagement for economists to finally figure out that Marxist/communist, Nazi/fascist, and other totalitarian types of policies run in these sorts of regimes were total economic basketcases and failures in the end.

Perhaps there's a lot of truth in what economist John Kenneth Galbraith said:

"The only function of economic forecasting is to make astrology look respectable."

Can the same be said about the various schools of "quantum gravity" type of research?

The Top Quark Mass

June 13, 2004
Categories: Uncategorized

Recently I've been reading a new book, The Evidence for the Top Quark, by a philosopher of science named Kent Staley. It's a combination of a history of the CDF collaboration's work leading up to their claim to have discovered the top quark, together with an extensive discussion of issues in the philosophy of science raised by the different methods used to analyze the data. The book is very topical since last week the D0 collaboration published an article in Nature claiming a new, more accurate, mass for the top quark based upon a re-analysis of their data from Run I of the Tevatron, which lasted from around 1992-96. The old analysis of the D0 data gave a top mass of 172.0 +/- 7.1 Gev, the new analysis gives 179.0 +/- 5.1 Gev. Combining the D0 data with the CDF data, the old analysis gave 174.3 +/- 5.1 Gev, the new 178.0 +/- 4.3 Gev.

Measuring the top quark mass is quite tricky since there are not a lot of events to work with and one needs to precisely measure the energies of jets. If a linear collider ever gets built, it would allow much more precise measurements. Knowing the top quark mass accurately is very important for the following reasons:

1. In the standard model one can try and use precision measurements of the electroweak parameters to observe the effects of higher loops including the Higgs and get a prediction for the mass of the Higgs. This crucially involves the top quark mass, since that is the strength of the top-Higgs coupling and the top quark couples far more strongly to the Higgs than any of the other fermions. With the latest D0 value for the top quark mass, one now expects (95% confidence level) that the mass of the Higgs is less than 237 Gev. For more details see websites at CERN and Fermilab.

2. In the minimal supersymmetric extension of the standard model there is an upper bound on the mass of the lightest neutral Higgs, a bound that depends strongly on the top mass. There's an explanation of this on Jacques Distler's weblog. With the newest value for the top quark mass one expects that the Higgs mass should be below 140 Gev in the supersymmetric case.

There are a few funny things about this report from D0:

1. It was published in Nature rather than the more conventional Physical Review Letters. Nature is not where high energy experiments normally announce their results and this appears to be an attempt to get wider publicity than is usual for such a result. For some comments on this, see David Harris's weblog.

2. The new method of analysis is similar to one discussed extensively by Staley in his book: the "dynamical likelihood method" due to Kuni Kondo. Ten years ago the CDF collaboration was rather skeptical of this method and decided not to use it, basically seeing it as too complex to be reliable. Have they changed their minds? Will CDF re-analyze its Run I data using this technique too?

3. Much is made in the paper and the associated Fermilab press release that this result changes the "best estimate" of the Higgs mass from 96 Gev (which is excluded by LEP results) to 117 Gev, which isn't. While this sounds impressive, it would be a lot less so if you do what you are taught in high-school physics and quote error bars with your numbers. As mentioned above, while it is true that the new result is that 117 Gev is "most likely", it is also true that a very wide range of values is almost equally likely. A more sensible but much less impressive way of saying things would be to just say that at 95% confidence level the Higgs mass has to be between about 50 and about 250 Gev.

Update: The D0 Nature article is now at the arXiv.

Comments

1. **John Martin**
   July 1, 2004

   http://elasticity2.tripod.com/
   On above site I give formula for the field structures of particles and atoms. The particle formula is based on the mass of the electron as this is the most accurately known particle mass. This indicates that a mass of 174GeV is the correct figure for the top quark.

2. **D R Lunsford**
   June 20, 2004

   Peter, the reason your b-log is so well received is very simple – the topic is physics as it once was practiced, with excursions into math for variety. You seem to me to be close to unique in having a mathematician's sensibility and depth combined with a physicist's common sense. It really shows in all your writing.

3. **Peter**
   June 17, 2004

   Glad to hear you enjoy the weblog and thanks for the encouraging words. I've been surprised how much attention it has gotten.

   The string theory situation is a rather weird one and the public discussion of the theory has been almost all coming from its promoters. I hope I'm redressing that situation a bit, and that people will look at both sides of the story and make up their own minds.

4. **raj**
   June 16, 2004

   FWIW, I am not a physicist by profession (although I did undergrad and graduate work in physics many years ago, and continue to follow some of the literature), I wanted to let you know that I enjoy your blog very much. I am particularly interested in your criticisms of string theory. It has gotten such hype in the popular press, but I have yet to see anything that appears to make sense about it. I read Brian Greene's book and, although I found it interesting (particularly the first quarter, relating to quantum mechanics and general relativity), I hate to say it but it did seem more than a bit unpersuasive.

   Regardless, I do enjoy the blog, and I would encourage you to keep it up.

5. **Peter**
   June 14, 2004

   Hi Thomas,

   A good recent reference for all this is chapter 5 of some lectures by Sally Dawson, hep-ph/0303191.

   In the SM there is no tree-level restriction on the Higgs mass. However, increasing the Higgs mass requires increasing the quartic Higgs coupling, and perturbation theory breaks down if this gets too large. For perturbation theory to be valid the Higgs mass can't be more than a few hundred Gev.

In the MSSM you have to introduce a second Higgs field (otherwise the anomaly from the superpartners of the first one are uncanceled). You can show that, at tree level, the lightest neutral Higgs should have mass less than that of the Z. Taking into account one-loop effects the upper bound goes up. It depends on the extra parameters in the theory (e.g. tan \beta, the ratio of vacuum expectation values of the two Higgs), but overall it has to be less than 140 Gev.

6. **Thomas Larsson**
June 14, 2004

   Is a light Higgs compatible with the ordinary standard model?

   I have seen somewhere that the tree-level prediction from the SM for the Higgs mass is m_H = m_Z = 91 GeV; that's the same with or without SUSY. The big difference is that the uncertainty in the radiative corrections is much smaller with SUSY, so the upper bound gets much tighter. So a light Higgs wouldn't really be a proof for SUSY, but the absense would be a proof against it.

   OTOH, I have seen an argument by Frank Wilczek that a light Higgs would be problematic for the ordinary SM, but I don't remember the details.

   The discovery of sparticles at the LHC would be a proof SUSY though, and would be a pretty strong indication that the string line of though is on the right track. But that doesn't seem to be too likely, does it?

7. **D R Lunsford**
June 13, 2004

   Tony Smith's analysis: http://www.innerx.net/personal/tsmith/TQvacua.html

   He gets these numbers as ratios of volumes within Shilov boundaries (as I understand it).

The stupendous Landscape of sting theory vacua

June 17, 2004
Categories: Uncategorized

At an early stage in the Los Alamos preprint archive it was split up into hep-th (for more formal or speculative work not directly relevant to experiment) and hep-ph (for "phenomenological" papers directly related to experiment). Susskind has just come out with his latest and now seems to feel that his ideas about the "Landscape" are directly of interest to experimenters and so belong in hep-ph.

The preprint is riddled with typos, for instance the third paragraph starts like this:

"During the last couple of years an entirely new paradigm has emerged from the ashes of a more traditional view of string theory. The basis of the new paradigm is the stupendous Landscape of sting theory vacua — especially the non-supersymmetric vacua. These vacua appear to be so numerous that the word *Discrtuum* is used to describe the spectrum of possible values of the cosmological constant….."

You get the idea.

Some high points of the article:

1. "low energy supersymmetry – an ugly solution" to the naturalness problem. Now he tells us. From what I remember the "beauty of supersymmetry" has always been one argument made in its favor.

2. "the ashes of a more traditional view of string theory". It seems that the picture of the world according to string theory that has been heavily sold for the last twenty years has burned down to the ground.

3. The argument in his last paper, such as it was, was wrong. Now he's got a new one with a similar conclusion.

4. "… a prediction that supersymmetry will not be seen at the TEV scale seems warranted". OK, string theory is finally making a prediction.

5. "If it turns out that low energy supersymmetry is a feature of TEV physics, then we will have to conclude that other considerations outweigh the counting of vacua on the Landscape". So, even though string theory predicts no low energy supersymmetry, if it is found it doesn't mean string theory is wrong. Got it?

Comments

1. **serenus zeitblom**
   July 1, 2004

   "I wonder what made Susskind withdraw his paper today, that was the original subject of this thread?"

   Look carefully at the final part of

   http://arxiv.org/abs/hep-th/0405279

   [version 4]

2. **JC**
   June 30, 2004

   I wonder what made Susskind withdraw his paper today, that was the original subject of this thread?

   Any good guesses on this?

3. **JC**
   June 22, 2004

   Perhaps it's not so surprising in seeing some folks pursue weird or crackpot ideas, the closer they are to retirement and/or the less they have to answer to anybody for approval or money (either as a salary, a grant, or a pension)?

   If it's the case of a tenured professor towards the end of their career, some may very well fall into a "lame duck" phase of their careers and just do whatever they want, with very little to no pressure to publish anything or get research grant money. Some folks may view that they have nothing to lose in pursuing weird or crackpot ideas, while counting down to the day when they can start collecting their pensions and social security checks.

   In the case of Stephen Wolfram, it appears he's independently wealthy from his company selling Mathematica over the years. Considering Wolfram hasn't published extensively since he left academia, his case appears on the surface to be almost like a "vanity" (ie. self indulgent and/or self aggrandizement) type of research project which cumulated into his 2002 book "A New Kind of Science". Some stuff in his book looked interesting, though some parts seemed too outlandish to be readily believable. At times I wonder if his book would have even been published if he didn't publish it through his own publishing company. (In principle anybody can publish anything through their own publishing company or journal, if they have all the money in the world to squander on it and convince enough bookstores to sell it). Perhaps this project isn't much more than an ego trip for Wolfram?

   These days any "crackpot" can just set up their own web site and "publish" whatever they want.

   If string theory and/or loop quantum gravity ever fell out of favor to the point where even journals like Nucl. Phys. B or JHEP, started to refuse to publish any string and/or loop gravity papers, I wouldn't be surprised if the "fanatical" string and/or loop gravity folks will just "publish" their papers on their own web site or even start their own dedicated string and/or loop gravity journals, while giving the "middle finger" to the older journals which won't publish any of their papers.

4. **Alejandro Rivero**
   June 21, 2004

   Second try: 't Hooft, Gerard

5. **Alejandro Rivero**

June 21, 2004

‘t Hooft holds a webpage, so perhaps his powerpoint presentations are available. Usually funnier than the papers.

I would put appart Stephen Wolfram’s. He left physics very early to go to computer science. I was very surprised when his articles on one dimensional cellular automata were published in Physical Review. Now the book is a separate, personal quest. If to someone, he could be alike to Julien Barbour.

As for Julian Schwinger and cold fusion, google suggest a pair of links:

http://www.infinite-energy.com/iemagazine/issue1/colfusthe.html
http://arxiv.org/pdf/physics/0303078

From it I wonder if he overreacted mainly to counter the general tendence, specially phys rev letters as he mentions. The old fathers as Schwinger are surely more humble about the success of modern quantum theory, they can have more perspective.

6. **Thomas Larsson**
June 21, 2004

‘t Hooft has a bunch of papers in the arxiv on Planck scale determinism, e.g.

http://www.arxiv.org/abs/hep-th/0104219

(An Essay in honour of John S. Bell!). However, ‘t Hooft undoubtedly knows a lot more about Bell’s theorem and its possible loopholes than I do, so I am certainly not in the position to judge. Determinism gives me bad vibrations, though.

7. **Urs Schreiber**
June 21, 2004

Re: t’Hooft on hidden variables

I have heard about that, but no details. Does anyone know what precisely t’Hooft is thinking about in detail with respect to hidden variables?

8. **Thomas Larsson**
June 21, 2004

Coming back to the issue of famous people that have done strange things. Here are some more recent examples that made my eyebrows rise a bit: Julian Schwinger’s support for cold fusion, ‘t Hooft’s work on hidden variables (or something very similar, and Stephen Wolfram’s New Kind of Science. I wouldn’t go so far as calling any of this crankish, however, and I observe that cold fusion apparently is starting to get some funding again. The stuff that Brian Josephson does is really weird, though.

9. **Alejandro Rivero**
June 20, 2004

Steve, yep, I concede that it is too many pages for just an artistic parody; It seems you are right, he is serious about this.

On other hand, the high quality of the graphics and the insistent sexual metaphor, sometimes almost Dali-nian, seem to point to an artistically inclined mind. The insistence on using physics vocabulary reminders me of another famous internet crackpot, Archimedes Plutonium, but in this case the ego is not so inflated, more the contrary.

Have you seen the “about” page and the crico.com website? Some years ago, all Spain was invaded by these puzzles. Guess he can pay the $ of the homepage, if he has been able to collect the royalties.

10. **D R Lunsford**
June 20, 2004

I find it somewhat astonishing that Dyson feels free to make such statements about Dirac, given his own lack of any definitive creative act in physics. I don’t consider mathematical technique to be creativity in physics. If blame needs to be assigned (it doesn’t) for the lack of success in fundamental theories, then put it at the door of chez Bourbaki and the formalists.

11. **JC**
June 20, 2004

Thomas,

The Dyson review of Greene’s book is at at:

http://www.nybooks.com/articles/17094

It’s interesting how Dyson asserts that revolutionaries and conservatives in physics come and go in an almost cyclic sinusoidal manner (with a phase shift of pi between the revolutionary and conservative curves of “popularity”). I wonder if Dyson’s assertion is true going back further in time, to the time periods before Newton or Kepler or Copernicus.

In the Dyson article, it sounds like the pioneers of quantum mechanics like Heisenberg, Dirac, Schrodinger, etc … were attempting to create an “encore” performance after their first “standing ovation” performance of creating quantum mechanics. Possibly at that time period in the 1900’s to 1920’s, the deviation between experiment and classical theory became so great that many radical theories were proposed and tried out? It would be interesting to go back and find old journal papers of theories which were competitors to quantum mechanics in the 1920’s, but which are now long gone and forgotten.

Perhaps there is a time and place for radical new ideas and theories in physics when the deviations between experiment and theory become really big, and where the old theories don’t seem to work anymore regardless of how many “epicycles” are added into the old theories. These days there doesn’t seem to be enough empirical data yet to reveal a huge deviation between classical and quantum gravity phenomena. It would be interesting to see in the near future how much new astro data will come in, and whether there’s a really compelling case that reveals a significant deviation from classical general relativity.

In today’s cases of string theory, loop quantum gravity, etc … and other forms of “quantum gravity” research, it appears to fit very much into Dyson’s category of “revolutionaries” attempting to build “castles in the sky”. Perhaps it can be argued that today’s “quantum gravity” folks are really just “pseudo revolutionaries” masquerading in a “revolutionary’s drag”, in the sense of folks attempting to create a “revolution” when there’s very little to nothing there to be “revolutionary” about in the first place? An analogy would be the difference between the “real revolutionaries” like the Bolsheviks, the Irgun, the solidarity movement in Poland, etc … compared to the “pseudo revolutionaries” like the american hippies from the 1960’s, or the British punk rockers from the late 1970’s and early 1980’s. Certainly the pioneers of quantum theory like Dirac, Schrodinger, Heisenberg, etc … would fit into the “real revolutionaries” category when they were creating quantum mechanics in the 1920’s, but they gradually turned into “pseudo revolutionaries” when they got older and were advocating their radical “crackpot” ideas that had very little to no experimental support.

In many ways, “pseudo revolutionaries” are probably common throughout history and will continue to be that way well into the future. Perhaps the only big difference between the “pseudo revolutionaries” and the “real revolutionaries”, is that the “real revolutionaries” just happened to be in the right place at the right time by coincidence. In another time and place, these “real revolutionaries” would have been written off as “pseudo revolutionaries” or “crackpots”, and eventually would have faded away into obscurity and largely forgotten, like what happened eventually to the American hippies and British punk rockers. In another place and time, somebody like Adolf Hitler would have been an insignificant 4th rate artist, homeless street bum, or even a Paris or Vienna coffee house intellectual, instead of the destructive Fuehrer of Nazi Germany.

Perhaps the differentiating factor between “revolutionaries” (whether pseudo or real) and “conservatives”, has a lot more to do with a person’s personality? I wouldn’t be surprised if during “revolutionary” time periods, the “conservatives” would just be plodding along into obscurity with their old theories/paradigms and become largely forgotten by history in the end, while during “conservative” time periods, the “revolutionaries” would just be making fools of themselves and also become largely forgotten by history in the end too. If string theory dies and fades away into the dustbins of physics history, will physics history remember who Witten, John Schwarz, or Maldacena are in a hundred years? (I think Witten would be definitely remembered in the math history books, as a Fields medalist winner). For somebody like Geoff Chew, he seems to have already faded away into obscurity and into the dustbins of physics history, other than as a footnote reference to the analytic S-Matrix theory origins of string theory.

12. **Steve**
June 20, 2004

Actually, the guy who wrote this stuff, a Dirk Lauryssens seems deadly serious (or else pretends to be). Although it does look like a parody of string theory and the buzzwords/jargon used. He is either a genuine crackpot or just pretending to be one to wind people up (presumably physicists/real string theorists). Don’t understand why he is getting space on a Hollywood info website though–would’nt that cost him $?–but the net is full of strange stuff. I noticed this site a while back when someone pointed it out to me but it has grown quite a bit now since he seems to have gotton very inspired by Brian Greene’s PBS series on string theory. I also notice on his “conclusion” page he quotes an abstract of S. Majid to support his claims! Poor old Majid. First the Bogdanovs now this guy lol. Don’t what he has done to deserve it.

13. **D R Lunsford**
June 20, 2004

Recall that the list of crackpots at one time included Clifford, Kaluza, Bolyai, Grassmann, etc. etc. Pauli himself blasted Dirac’s idea of “holes” then later, perforce, retracted. Boltzmann committed suicide in despair, followed by Ehrenfest. All really new ideas generaate enmity it would seem.

Yes, some of the later work of people like Einstein and Dirac seems futile in retrospect – but even a hopeless idea like “lambda invariance” is more interesting than brainless triumphalism. And it’s entertaining! I had great *fun* reading Dirac’s papers on the superconducting vacuum. There’s more to life than eternal verities.

14. **Alejandro Rivero**
June 20, 2004

Steve, it is just elaborate irony with a lot of sexual clues. Note for instance as the “Mama brane” is drawn milking her particles.

15. **Steve**
June 20, 2004

Check out this bunk.
http://www.hollywood.org
and scroll down to “Breaking Science News.Cosmology”. This is not just an obscure crackpot page…this page must get major hits. String theory unfortunately seems to be giving credibility to this sort of thing and I worry if real rigorous science can ultimately survive. You could use the email link and complain but sadly the author(s) would probably relish the attention.

16. **Thomas Larsson**
June 20, 2004

“It would be interesting to go back and find some weird or crackpot unpublished papers that were written by folks like Dirac, Einstein, Fermi, Schwinger, Heisenberg, Eddington, etc …. ”

Dyson recently wrote a review of Brian Greene’s latest book (I lost the reference, but it appeared on this blog so maybe somebody else can fill it in). In particular, he remembered that when he was young in 1950, old heroes like Einstein, Heisenberg, Born, Dirac each had their own wacky idea, which they thought should to start another revolution like the quantum one. Since I am a big fan of PAM, it disturbed me that Dirac’s idea seemed the weirdest; it had something to do with probabilities ranging between +2 and -2.

Dyson remarks that seeing these old men making fools of themselves turned his generation, with Schwinger, Feynmann and others, into conservatives. I would not be surprised if today’s excesses by the aging string theorists will have a similar effect on the next generation.

However, Dyson is too conservative even for my taste, since he seems happy with gravity being described by a classical theory.

17.
June 19, 2004

Well, I have a recent paper that got into a real journal (Feb this year, IJTP) but is banned from the archives! I feel a perverse pride in this 🙂

18. **JC**
June 19, 2004

Tim,

So far none of Susskind’s anthropic preprints appear to have been published in any real journals which require peer review. Maybe at this point he doesn’t even care if they end up in a journal or not? His first anthropic paper (hep-th/0302219) seems to be getting many citations already.

It would be interesting to go back and find some weird or crackpot unpublished papers that were written by folks like Dirac, Einstein, Fermi, Schwinger, Heisenberg, Eddington, etc …. that would be good candidates for a “physics hall of shame”. Maybe old age and/or senility are the root causes of some folks starting to work on weird or crackpot research ideas? I can’t think of any good reason for younger folks wanting to work on weird or crackpot ideas, other than perhaps simple brainwashing or just plain intellectual laziness.

19. **Tim**
    June 19, 2004

    Do any of these ridiculous Susskind preprints ever get published in journals?

20. **JC**
    June 19, 2004

    The only stuff I’ve ever came across that’s even more mindless and incoherent than Hegel or Derrida, would be the lyrics of most pop music songs especially in its more extreme forms like heavy metal or punk rock. Whether intentional or not, most pop song lyrics make very little to no sense and frequently appears to be just pure nonsense attempting to say something profound. Perhaps it only makes “sense” to a person who is high on drugs or really drunk?

    What I found amusing over the years, is how many pop songs frequently have the same underlying structures in musical chord progressions and rhythms such as

    intro – verse – chrous – verse – chorus – bridge/solo – chorus

    with the lyrics fitting into the same sort of rhythms of 4 or 8 syllables per line. It seems to be almost like a formula regardless of who the artist is, the year it was written, the particular style of pop music, or even what language or country it was written in.

    What would be amusing is if anyone has ever used that Dada Engine at

    http://dev.null.org/dadaengine/

    to “randomly” generate pop song lyrics, where it’s difficult to figure out if it’s a computer or person writing the lyrics.

    An even more sophisticated thing would be to get that Dada Engine to actually generate musical rhythms and melodies that sound typical of pop music. It seems like many pop songs are written in the same 4/4 time signature, with chord progressions fitting into the same sequences of musical notes and scales, usually some minor pentatonic scale (that’s frequently the first scale almost every musician learns when they picked up their first guitar or sat behind their first keyboard/piano).

    It would be even more amusing if this sophisticated “Dada Pop Music” generator actually produced pop music songs that are indistinguishable from the human written pop songs on MTV or the radio! Then they can rename this “Dada Pop Music” generator as the “Milli Vanilli” machine that fools everyone!

21. **Steve**
    June 19, 2004

    Brilliant and beautiful series the “Ascent of Man”. I have it on video. Bronowski was a truly unique intellectual figure and polymath discussing science and human cultural evolution with a warm poetic eloquence. In that episode “Knowledge and Certainty” I remember he describes Hagels as a philosopher he “specifically detests” and really knocks him down to size. That episode also ends with the famous powerful and moving scene at the pond at Auschwitz, which is the back sludge of the ashes of millions of people, including some of his family. He says, “when people believe they have absolute knowledge with no test in reality, this is how they behave. This is what men do when they aspire to the knowledge of gods”. If you can get the series on video or dvd at your university or local library then check it out. Well worth the time.

22. **D R Lunsford**
    June 19, 2004

    Jacob Bronowski in his series “Ascent of Man” bashed Hegel mercilessly by pointing out that, directly after publication of his Pythagero-philosophical proof that only 8 planets could exist, Ceres was discovered 🙂

    http://www.hegel.net/nature/ceres.htm

23. **JC**
    June 19, 2004

    Thomas,

    Judging from the hep-th abstracts, I’m sure what passes as “deconstruction” in string theory is a lot less sinister than Derrida’s notion of “deconstruction”.

    If you ever come across anything that’s written in a very “revisionist” manner, most likely the person is engaging in Derrida’s version of “deconstruction”. Some folks will say outright that they’re invoking Derrida, while other folks may be doing “deconstruction” without even being conscious of it!

    In the end, Derrida’s “deconstruction” isn’t much more than a person trying to rationalize their own self-denial. In many ways it’s sort of like Nazi propaganda minister Joseph Goebbels’ notion of “keep on repeating a lie, and in the long term it eventually becomes ‘true’ “. (It appears Bush, Cheney, Rumsfeld, etc … have all taken a cue from Goebbels and are actively applying it to everything they’re saying on television, hoping to eventually fool enough voters through election day in November).

    Perhaps all these string folks who are “true believers” in the anthropic stuff, are really unconsciously engaging in “deconstruction” without even knowing it! They’re all secret worshippers of Derrida. 😉

24. **Thomas Larsson**
    June 19, 2004

    JC,

    A hep-th search for eprints with the word “deconstruction” in the title during the past year gave 9 hits. Derrida rules!

25. **Peter**
    June 18, 2004

    A lot of string theory research can be compared to Derrida, Hegel, postmodernism, etc., in that a complex and difficult formalism is used to obscure some rather ill-thought out ideas. But Susskind is something different. It’s not hard at all to understand exactly what he is saying. He doesn’t hide behind a complicated set-up in the slightest and his mathematics is mostly what one learns in junior high school.

The weird thing about this is that someone can be taken seriously when they so obviously are not really doing science.

26. **Thomas Larsson**
June 18, 2004

4. "… a prediction that supersymmetry will not be seen at the TEV scale seems warranted".

I wonder how this affects Lubos Motl's experimental-SUSY-by-2006 bet.

27. **JC**
June 17, 2004

Hmmm …. Reading again over my description of Derrida several times, I just realized my statements make absolutely no sense.

I suspect this is the sort of twisted thinking many folks fall into when they are in denial about their own ignorance, whether philosophers, physicists, or anybody else. It's very easy to fall into.

28. **JC**
June 17, 2004

Blah. Forgot to put my name to the previous post.

29.
June 17, 2004

I never quite understood the "beauty of SUSY" arguments that were propagandized over the years.

Only thing I found SUSY useful for was in checking my Feynman diagram calculations by taking a SUSY limit, where if I did my calculaton properly it should reproduce the known SUSY results in the various SUSY limits. Other than using SUSY as a way of checking my calculations, I never quite bought into the "beauty" argument completely.

I think the only time I ever bought into the "beauty of SUSY" propaganda, was when I was a naive clueless undergrad with "stars in my eyes". I first heard about SUSY from some of my professors who did particle phenomenology, and just took their word for it without questioning it much. For the most part, it was somewhat "over my head" at the time. When I finally got around to learning SUSY in grad school, that's when I started to have second thoughts about it.

When I was still doing string stuff, I treated SUSY as if it was a Planck scale symmetry more or less by fiat decree.

On a different note, I wonder what Susskind's future book on the "Landscape" will look like. If it resembles the sort of papers he's been cranking out over the last year or so, will it start to read more like a philosophy book than a book about physics? What will be scary is if it ends up reading like "Tractatus Logico-Philosophicus" by Ludwig Wittgenstein, or worse, anything by Georg Wilhelm Friedrich Hegel or Jacques Derrida. If I didn't know any better, Hegel's work reads very much like nonsense masquerading as "legitimate discourse" in a manner similar to postmodernism stuff. Wittgenstein's work attempts to assert that all philosophical problems which are not scientific, are really ambiguities in our language. Derrida's "deconstruction" work seems to be searching for hidden or underlying meanings in something by "reading in between the lines", that are not so obvious on the surface. If I didn't know any better, I always got the sense "deconstruction" wasn't much more than a thinly veiled attempt at a "rorschach test" of a person's underlying biases and prejudices, when reading and critiquing something. It would be a sad day when physics starts to read a lot like Derrida, Hegel, or postmodernism.

Mazur and Basic Notions

June 17, 2004
Categories: Uncategorized

There's a quite remarkable article by Barry Mazur in the latest issue of the Bulletin of the AMS. It brings together ideas about elliptic curves and deformations of Galois representations that were used by Wiles to prove Fermat's last theorem, mirror symmetry, quantization, non-commutative geometry and much more. I'm not convinced it all hangs together, but it's a wonderful piece of expository writing.

Mazur claims to be inspired by a very interesting seminar held every week in the Harvard math department called the *Basic Notions Seminar*, parts of which have recently been put online. This issue of the Bulletin is dedicated to the great French mathematician Rene Thom, who died nearly two years ago. The articles by Michael Atiyah and Dennis Sullivan about Thom's work in topology are well worth reading.

Comments

1. **Tom Slade**
   June 22, 2004

   I happened to see this article and the following comes to mind:

   a^p + b^p = c^p. If the p-norm, p of a metric space is not 2, then we have cyclotomic fields in a non-Hilbert space. Elliptic curves are just projections of SUSY spinors and are examples of cyclotomic fields. When p is 2, we can't have cyclotomic fields and can have only simple harmonic functions. The interesting thing is that a Riemann manifold requires just one spinor to fix it's position (like a rotating basketball), but when p>2, four spinors are needed and the space is quantized, by an integer ratio of a/c. Note that if 'a' is prime the field can't be cyclotomic.

   Wick ordering is a property of cyclotomic fields as well as NCG. My guess it that manifolds in non-Hilbert space are comprised of SUSY spinors and are exclusively solutions to non-linear PDEs. All non-Hilbert spaces are quantized — that's a shock and Banach space is not a linear or complete vector space — interesting.

2. **Alejandro Rivero**
   June 18, 2004

   About the introduction of unstability in classical mechanics (page 22/328) I have from time to time redirecting people to Newton's Principia, proposition 1, Book I, to read how Newton himself decides explicitly to carry any quantum of angular momentum to zero. A lot of people mistakes this theorem as a trivial one of angular momentum preservation, didn't noticing that the continuum character of classical mechanics is defined there.

   By the way, I remember to have read a preprint where someone uses the unstability concept not only to get "h" out of classical mechanics, but also to get "c" out of galilean group. It speaks of stable and unstable theories or so.

Tevatron Run II Luminosity Record

June 21, 2004
Categories: Uncategorized

After an expensive upgrade to increase its luminosity, the Tevatron at Fermilab was turned back on in March 2001 to start "Run II", during which it was hoped that the machine would run with a dramatically higher luminosity than during "Run I" which ended in 1995. During Run I, about 140 inverse picobarns of collisions were generated, at the rate of about 3 inverse picobarns/week by the end of the run. The plan for Run II was for the new machine to get to a luminosity of about 17 inverse picobarns/week soon after the recomissioning, and ultimately to reach about 100 inverse picobarns/week.

This plan turned out to be wildly overoptimistic, as it took nearly a year and a half to get the machine operating even at the luminosity achieved during Run I. During FY02 the initial hope was to accumulate 320 inverse picobarns, rising to 830 in FY03 and 1300 in FY04. Instead 80 were produced in FY02 and 330 in FY03. While in FY02 the machine performed much worse than planned at the beginning of the year, in FY03 it did slightly better than planned.

As this fiscal year (FY04) is coming towards its end, the machine is doing significantly better than planned at the beginning of the year. The last few weeks have seen about 13 inverse picobarns/week being produced. Another measure, the luminosity at the beginning of a "store", reached a record value on Monday of 8.28×10^31 cm^2/sec. This is about five times higher than achieved at the end of Run I. You can follow the progress (as well as the trials and tribulations) of the Fermilab accelerator physicists through on-line daily reports and continually updated luminosity charts. For a collection of documents showing the history of the Run II problems and the most recent estimates of what will be achieved, see the proceedings of the latest review of the Tevatron luminosity, which took place last February.

About 2000 inverse picobarns of collisions will probably be needed before the Tevatron experiments can push up the current experimental limit on the Higgs mass (114 Gev) that comes from experiments at LEP. There now seems to be a good chance the Tevatron will get to this point before the LHC starts operating at much higher energy in FY 2008. If the LHC achieves anything like its design luminosity, it will quickly make the Tevatron obsolete. Then again, having seen how hard it was to get the upgraded Tevatron running, the job of commissioning the LHC may not be so easy…

No Comments

String Theory by Press Release

June 22, 2004
Categories: Uncategorized

The KITP at Santa Barbara seems to be going all out to hype claims of observability by LIGO of effects of cosmic strings. The front page of the KITP website prominently features the story, and adds the personal phone numbers of the authors to encourage the press to contact them (something I've never seen theoretical physicists ever do before). Maybe these guys have already hired an agent.

Comments

1. **JC**
   June 25, 2004

   I always felt a lot of that stuff which attempted to connect QCD with a string theory, looked kind of "ad hoc" and never really appeared to be all that convincing. When Maldacena's AdS-CFT stuff first came about it looked like a more sophisticated version of that "ad hoc" QCD string stuff, which initially I thought looked promising. After many years of folks working out the consequences of the AdS-CFT duality, I'm less convinced today of it's promises. Attempting to do string loop calculations on the AdS side seems to be a difficult problem, which so far hasn't produced many convincing results.

   In the end, that Maldacena AdS-CFT stuff may very well be just another "ad hoc" result in a long line of other "ad hoc" QCD string models.

2. **Alejandro Rivero**
   June 25, 2004

   JC,

   Search for "QCD String" in both hep-th and hep-ph. It started to be propagandised with the new century. I saw this campaign, then, as a way to drive people back into physics. Sort of leaving a scape gate.

3. **JC**
   June 25, 2004

   Speaking of weird string papers, just noticed this paper

   "Hadron pair photoproduction within the Veneziano model"
   hep-ph/0406267

   In 2004, I never would have thought anybody was still doing the string theory or dual resonance model stuff with respect to hadronic physics. The last time anybody took this stuff seriously, was when "California Dreamin" was still a top 40 hit song?

   A time warp back in time, to a more innocent and naive time period in physics history.

4. **Peter**
   June 24, 2004

   Thanks, I'm glad to hear that that you find the material here interesting, and I've been pleasantly surprised by how many people take and interest in it and make comments. But I'd rather not get involved in adding anything more elaborate to the site, mainly because I don't want to put any more time into it than I already do.

   That said, if you or anyone else wants to set something up and manage it themselves, I'll certainly set up links from here.

5.
   June 24, 2004

   OT request – would it be possible to implement a discussion section? Or desirable from your point?

   Some of us have a homemade board that runs on software written by one of our gurus (based on Zope and MySQL) that would be easy to implement for someone who knows Linux.

   I find this place more interesting than sci.physics.*.

6. **raj**
   June 23, 2004

   Maybe string theorists are adopting the techniques of the "creation science" crowd. And just when they caught the attention of US "public television." Witness Nova's series in the last year or two about string theory.

7. **JC**
   June 22, 2004

   Thomas,

   Some folks are willing to make all kinds of weird long term bets. Since they are not betting large amounts of money, if any money at all for that matter, they're simply not "putting their own money where their mouth is".

   If their bets end up being correct, they'll claim victory. If their bets end up being incorrect, they'll just "whitewash" it away by saying something like "I was young and stupid, but now I'm wiser", etc … or some other amusing excuse. There's no huge monetary losses on their part, regardless of whether they are right or wrong in the end.

   If they're wrong in the end, the most that will happen to them perhaps would be the depressing realization that they wasted a large portion of their working lives on something that was garbage in the end. It would be like "waking up" from a bad dream of being a member of a weird religious cult.

8. **Thomas Larsson**

June 22, 2004

For the last 20 years, Ed Witten has repeatedly stated that string theory makes one postdiction, gravity, and one prediction, supersymmetry. Alas, the natural signatures of supersymmetry have *already* been ruled out by experiments – this is what people mean when they say that supersymmetry requires finetuning at the percent level, isn't it? Thus, unless one gives up naturality (which was the only experimental motivation for SUSY in the first place), Witten's one string theory prediction has *already* been proven wrong. Pretty much bottom line.

9. **Peter**
June 22, 2004

The Fermilab story is about results of a fixed target experiment called SELEX, see hep-ex/0406045.
They stopped taking data in 1997, but recently went back to reanalyze their old data to look for to see if they could find evidence for the new states Babar was claiming.

They seem to have pretty good evidence for a new meson with a strange and charm quark, and supposedly much narrower than theoretical predictions. I really don't know how reliable the theory of the spectrum for such states is, so don't know how surprising this is.

Perhaps this is the kind of thing that could be confirmed and studied in more detail at SLAC or Cornell. Maybe there is something unexpected lurking in the physics of these mesons.

10. **JC**
June 22, 2004

The string theory advocates and "fanatics" over the years have sounded very similar in "spirit" to those folks who were ranting and raving about the "New Economy" during the 1990's dotcom bubble. After the dotcom bubble bursted in 2000, there were still many folks ranting about the "New Economy" for several years after, thinking that the Nasdaq "correction" was just temporary and that "prosperity" was just "around the corner". After about a year or so, the more level headed folks "woke up" and became more skeptical and less willing to "suspend disbelief". With time many of the advocates finally came to accept the notion that the "new economy" was just overblown hype, while the "fanatics" to this very day are still steadfast in their beliefs and refuse to capitulate. With time even some of the "fanatics" will eventually come to their "senses". For many folks, it would be very difficult emotionally to invest one's self in movements or ideas which don't work in the long term, especially when there's a huge investment of time, money, and ego involved.

A very similar sort of pattern happened the years before and after the 1929 stock market crash too.

Frequently movements or ideas that are built up by hype and overblown promises, end up following speculative "bubble" type behavior with the bubble frequently "bursting" after awhile when the hype deviates too much from reality to be sustainable in convincing many people to "suspend disbelief".

11. **D R Lunsford**
June 22, 2004

There is some kind of excitement at Fermilab:

http://www.theregister.co.uk/2004/06/18/meson_weirdness/

Any ideas?

12. **Peter**
June 22, 2004

The problem isn't that string theorists need encouragement to try and come up with an experimental prediction that can be tested. Most of them desperately would love to be able to do this. But the theory is so ill-defined it inherently can't predict anything.

The claims for "tests" of string theory are never-ending. I'd guess there's on average a new one once a month or so. A lot of science journalists are fooled by these claims, see for instance last year's article about string theory in Scientific American by George Musser that tells us that "in the mid-1990s the theory started to click together conceptually. It made some testable, if qualified, predictions."

Pretty much all the string theory "tests" are of the sort: "string theory predicts that X could happen" where X is something potentially observable. Note carefully the "could", they never say X "will" happen. In all these "predictions" string theory is compatible with some huge range of possible phenomena which include X (although X is exceedingly unlikely and there is no good reason to expect it to happen). These aren't real scientific predictions since when X isn't observed, this doesn't imply string theory is wrong. But it's amazing how many people are taken in by this stuff.

13. **Tim**
June 22, 2004

To JC,

Maybe this is a good thing. While their "test" may be dubious, if they get enough attention (and it seems like they have) it will encourage others to attempt to develop (and publicize, of course) other experimental string searches. Grounding theory in experiment is a good thing. Of course, if the theory is as poorly developed and vague as advertised here no possible experiment would be conclusive (not even wrong).

14. **Peter**
June 22, 2004

Hi Thomas,

This particular hype is different than the landscape hype and not inconsistent with the monovac philosophy, so no reason for Gross to suppress it. The truly sad day will be when the KITP starts issuing press releases about the stupendous Landscape.

15. **JC**
June 22, 2004

Perhaps this is the beginning of a series of "last gasps" of air for string theory?

When theoretical physicists are using the same hype and propaganda tricks that are common in political or marketing campaigns, one wonders if there's something really rotten underneath the surface that they're attempting to "whitewash" over. (ie. The anthropic stuff may be similar to maggots or termites slowly eating away into the string theory edifice of "legitimacy".)

It’s sort of like what they call the “cockroach effect” on the financial markets. Every time there’s “hints” of anything going wrong inside a company, everybody starts selling the company’s stock in a knee-jerk vote of “no confidence”. A sign of “bad news” (whether explicit or implicit) may very well be just “one cockroach” popping up at the surface, while underneath the surface is a whole colony of cockroaches and numerous deeper problems about the company in question.

16. **Thomas Larsson**
June 22, 2004

A long time ago, when Walter Kohn was director and the ITP didn’t have Kavli’s initial attached to it, I spent a year in grad school at UC Santa Barbara (a totally awesome place, fer sure). It saddens me that a place which meant a lot for my scientific worldview now is the center of such excesses. And what does the prophet of the cult of monovacuism in the office below Polchinski’s say about this?

Is that “Fau” for “Faux”?

June 25, 2004
Categories: Uncategorized

Just got the following e-mail:

Date: Fri, 25 Jun 2004 19:40:58 +0200
From: fau.alex (fau.alex@wanadoo.fr)
To: woit@cpw.math.columbia.edu
Subject: Mr Woit

hi,
I am a french student and I will go in 1re(in France) the next year
and I want to speak with specialists because I want to work in science
when I will be adult … That’s why I write to you !
I want to know who are you ? What did you study, …
Thanks in advance for your answer
Best regards
Alexandre Faure

At first I thought “Oh yes, just another in the avalanche of fan-mail. Wonder if I should take the time to respond?” Then, looking more carefully at the HTML version of the e-mail, I found an embedded link at the bottom:

```
href="mailto:liu-yang.imp@th-phys.edu.hk"
```

That address looks awfully familiar.

Comments

1. **Phil**
   June 30, 2004

   It is already funny for a long time ago.
   How could you have met the prestigious professor Yang ? I have never met ghosts, maybe some ghost writers — those who write wrong mathematical sentences which are never re-read by the official writers of the book — but ghosts and imaginary creatures emerging from imaginary institute of “cosmomogy”, no, never. So Fabien you don’t have to answer to the common fallacies of these two people : almost nobody believe them. And by the way, thank you for your website.

2. **Fabien Besnard**
   June 30, 2004

   I can assure you I never met professor Yang ! (This is really getting funny !)

3. **Phil**
   June 29, 2004

   Dear Iogr and Grikcha Beudganov,

   (Learn how to write my nickname)

   I am sure that Mr “Yang”, from the famous Hong-Kong university devoted to your world-known works and thesis, would have been a perfect gentleman if I have had the honour to have met him. His very particular style showed it previously to me, but with most regrets I don’t even know his figure (with slavic features ?). No trace of your former posts about this so kind friend : could you remember me where they are, if you signed them ? By the way : I don’t know if you are only making juvenile provocation but I am not (and neither YBM is) Fabien Besnard : thanks to your delightful methods you have now several and distinct opponents.

   With most respect (ha ha ha),
   Phil Romnulphe

4. **Igor Grichka Bogdanoff**
   June 29, 2004

   Dear Phil Ronuphle,

   It is always a pleasure to read your kind messages. In fact (but there is nothing wrong with it) you did not read the posts where we spoke about the professor Yang. So please, go to our previous posts and read what we wrote about him. He is a charming and delighfull researcher, passionate with fundamental physics and complex maths problems. I am sure you would really like him (am I wrong? but I think that he already met Fabien Besnard some days ago).

   Kind regards

   IGB

5. **Phil**
   June 29, 2004

   So at last the Bogdanoffs do admit that they are in contact with the famous (and mysterious) Prof. Yang ? Could they talk more about him, for they forget to answer in the french newsgroups when he is on topic ? Is his famous Institute of “Cosmomogy” in Hong-Kong similar to the one in Riga ? As serious as it ? As we could say in French (forgive my “gallicisms”), ridicule un jour, ridicule toujours. Et j’ajoute : imposteurs d?masqu?s.

6. **Peter**
   June 28, 2004

The “Yang” address wasn’t in the header, it was embedded as a link in the HTML version of the message. Maybe young Faure is right and it appeared there because he was e-mailing “Yang”. It seems likely to me there’s more to this story than that, but I’m not wasting any time trying to figure it out.

7.
June 28, 2004

Dear Peter,

Our mutual friend “YBM” has alerted us about this “fau effect”, pretending that it was (another) Bogdanoffs pseudo. After checking we can assure you that Alexandre Fau has nothing to do with a) Yang, b) ourselves. You obviously got mistaken (somewhere) in the reading of the header of his email.
Nothing to really write home about.
Best regards

I/G

8. **Faur? Alexandre**
June 28, 2004

I just try to contact the Professor Yang ! That’s why this email is in this message …

9. **Peter**
June 28, 2004

I have no idea who you are, or what your relation to the Bogdanovs is, but I assure you that the e-mail address in question was embedded in your message to me.

10. **Faur? Alexandre**
June 28, 2004

Hi,

Contrary to that you said, or that you think, I am Alexandre Faure and I live in Toulouse.

First, I sent a message to you at :woit@math.columbia.edu

Secondly, read the real details of the message …

From: “fau.alex”
To: >
Subject: Mr Woit
Date: Fri, 25 Jun 2004 19:40:58 +0200
MIME-Version: 1.0
Content-Type: multipart/alternative;
boundary=”—-=_NextPart_000_0023_01C45AEC.56ABAE10″
X-Priority: 3
X-MSMail-Priority: Normal
X-Mailer: Microsoft Outlook Express 6.00.2800.1106
X-MimeOLE: Produced By Microsoft MimeOLE V6.00.2800.1106

I am sorry but the professor Yang’s adress can not be in this message …

Fird, if you don’t belive me, you can contact my mother Anne Faur? at the adress : anne.fau@wanadoo.fr

“Je reste ? votre disposition”

Alexandre Faur?

11. **ark**
June 27, 2004

Hi,
I received a similar email:

From: “fau.alex”
To:
Subject: Fw: Bonjour

> hi,
>
> I want to congratulate you fro the preface of the Bogdanov’s book !
>
> I want to know who are you ? What did you study, …
>
> I am a french student and I will go in 1?reS (in France) the newt year and I want to speak with specialists … That’s why I write to you !
>
> Thanks in advance for your answer
>
> Best regards
>
> @lexandre Faur?

There was no embedded email from .hk. I got two versions: one in French and one in English. The header reads:

Mon, 21 Jun 2004 15:12:52 -0500 (CDT)
Received: from mwinf0801.wanadoo.fr (smtp8.wanadoo.fr [193.252.22.23])

by mailscan.eahosting.com (Postfix) with ESMTP id 617ED2EFC1
for ; Mon, 21 Jun 2004 15:12:52 -0500 (CDT)
Received: from alexordi1 (AToulouse-152-1-39-155.w82-125.abo.wanadoo.fr [82.125.29.155])
by mwinf0801.wanadoo.fr (SMTP Server) with SMTP id 19D1B18003B2
for ; Mon, 21 Jun 2004 22:12:51 +0200 (CEST)
Message-ID:
From: "fau.alex"
To:
Subject: Bonjour
Date: Mon, 21 Jun 2004 22:08:30 +0200

………..

The sender seems to be in Toulouse? Few days later, after a short correspondence, I received this:

> Thanks Mr Jadczyk for these suggestions …
>
> I will finsih reading Bogdanov's book and I will read other scientific's books because I am very fond of sciences …
>
> Mr Schreiber suggest me Feynman's books … What do you think about this author and which books can I read (I will go to "1?reS" (scientific) and I want to know if theses books are good for me ….)
>
> Thanks in advance for your answer .
>
> Alexandre Faur?

ark

Summer Conferences

June 27, 2004
Categories: Uncategorized

Two of the year's largest particle theory conferences are taking place around now, with Lattice 2004 attracting 280 physicists to Batavia, Illinois this past week and Strings 2004 drawing almost 500 to Paris starting tomorrow. Normally I feel kind of sorry for string theorists since their field is in such bad shape, but this week I'm jealous since I would have loved to have an excuse to go to Paris this summer (I'm not jealous of the lattice gauge theorists who are getting to spend the week in Batavia).

Maybe I'm wrong about this, but the Paris string theory conference seems to me to be the largest gathering of particle theorists that I can remember ever having taken place. In recent years these things have been huge, with attendance around 400-450, but this one should be even larger. It is so over-subscribed that they haven't been taking on-line registrations for weeks.

Both conferences should have transparencies from the talks available soon on-line and the list of titles and speakers in Paris is now available. Some of the ones that look like they might be interesting are Robbert Dijkgraaf speaking about "Topological M-theory", Nikita Nekrasov on "Chasing M/F theory" and maybe Greg Moore, whose title is the mystifying "Anomalies, Gauss laws, and Page Charges in M-theory". One big theme of the conference looks like it will be N=4 super Yang-Mills theory. This is an interesting and well-defined quantum field theory, and one can study it whille claiming to be a string theorist because of the AdS/CFT conjecture and Witten's recent work reformulating it in terms of topological strings in twistor space.

Next Saturday in Paris there will be a whole day of talks devoted to the unrelenting hyping of string theory to the general public, something which is a standard feature of the "Strings XXXX" conferences, but not the "Lattice XXXX" ones. Somehow I suspect the speakers will neglect to emphasize the utter lack of any progress towards making any contact with reality during the past twenty years. In case the Paris conference is not enough, there are quite a few satellite conferences, including a pre-conference workshop at the IHES and post-conference workshops at CERN and Durham.

Update: It looks like Jacques Distler will be reporting direct from the conference.

Further Update: It seems that they have a WiFi connection at the Paris conference site. From my web server logs, it appears that one thing attendees at the conference are doing during the more boring talks is reading "Not Even Wrong" on the web. Hi Guys!

Comments

1. **JC**
   June 29, 2004

   Thomas,

   When I was a young naive undergraduate student, I took some smarter and older grad student's "advice" that the "best" place to learn field theory and string theory was Polyakov's "gauge fields and strings" book. At the time I didn't know any better, and was very much clueless about the field theory and string literature. I know now this smarter and older grad student was just joking around, and was trying to scare me away from theoretical physics in a "tongue and cheek" manner at the time. The last time I saw him, he joked about it and couldn't believe I took his "advice" as if it was a "gopel truth". Turns out that trying to learn introductory field and string theory from Polyakov's book, would be like trying to learn introductory quantum mechanics from Dirac's book. Both Polyakov and Dirac's books are really nice books to read once you know the subject well, but are difficult to learn from if you don't have any previous knowledge of the subjects.

   I ended up wasting a whole summer trying to figure out the calculations in Polyakov's book, where I spent most of my time in the library trying to figure out his papers that were referenced in his book. After that I still had a hard time figuring out Green, Schwarz, and Witten's books on superstring theory. Somehow I was able to reproduce most of the calculations in Polyakov and the Green, Schwarz, and Witten books, but I was still relatively clueless as to what the physical picture underneath all the mathematics was about. At the time I think took the Feynman idea of "shut up and calculate" mentality to the extreme. (I don't know if the quote was originally Feynman's, but over the years it seems to be attributed to him like an "urban legand".)

2. **Thomas Larsson**
   June 29, 2004

   Sasha Polyakov travels fast. Yesterday he was in Paris and gave a talk that Jacques Distler didn't like, and today he gave a talk at the 4ECM here in Stockholm. I haven't had any academic affiliation for a long time, but since P was one of the big heroes of my youth (for the BPZ work), I took the afternoon off and sneaked into the Aula Magna.

   The talk was intended for an audience of mathematicians and had the presumptious title "Physicist's view of mathematics". P started out with the thesis that math history has had three phases, characterized by the slogans

   "Things are numbers" (5th century BC)
   "Things are topological invariants" (20th century AD)
   "Things are words" (21st century AD)

   By topological invariants he means things which do not change at all under small deformations (I might have preferred the word "universal"), and gave three examples from physics:

   1. The quantum Hall effect, where conductivity is given by sigma_xy = n e^2/hbar, n integer.

   2. Instantons and their connection to the Atiyah-Singer index theorem and Donaldson's invariants.

   3. Critical exponents in the 2D Ising model and minimal models. With Schramm, Smirnov and Werner in the audience, I was somewhat surprised that he didn't make the very timely connection between CFT and SLE at this point.

   The "things are words" part turned out to be about his recent work on the connection between some 4D Yang-Mills theory in loop variables and some theory of random surfaces in 5D. This seems eminently plausible; if you take the intersection between a generic surface in 5D and a 4D hyperplane you'll end up with a loop in 4D, which may perhaps be a Wilson loop of some Y-M theory.

   Alas, the model stood by itself, without any reference to 10D or 26D or Calabi-Yau, even as a motivation. It seems like the smart string theorists are starting to pretend that those things never happened and go back to study 4D field theory again. It is difficult to be critical about using an extra dimension as a technical trick to study Y-M theory in 4D, although I got the impression that P's model isn't the physical Y-M theory of the standard model. Anyway, I missed the details because P didn't give any due to time limitations.

At this point one may wonder where the “things are words” slogan enters. The point, if there is any, is that correlation functions can be regarded as words, with elementary fields and their derivatives being letters. Natural enough, but surely people knew about OPEs before the 21st century…

In the question section it became clear that P didn’t think very highly of Wolfram’s New Kind of Science. But then again, who does?

3. **Steve**
June 28, 2004

Forget Lattice 2004 and Strings 2004–the best and most informative conference of 2004 has already taken place.

http://www.xs4all.nl/~rpronk/conference.htm

With the conference proceedings detailed here:

http://www.j-walk.com/other/conf/summary.htm

For those with a rightious sense of political correctness who don’t appreciate the humour of these links, that will quickly disappear if (like me) you have to delete 20-30 of these damn things virtually every day! At any rate, physicists could perhaps adopt the conference advice given on “uppercase letters” in order to make their preprints stand out in the hep archives:)

International Institute of Mathematical Physics in Riga

June 27, 2004
Categories: Favorite Old Posts, Uncategorized

The DNS has the following entry for phys-maths.edu.lv

domain: phys-maths.edu.lv
descr: International Institute of Mathematical Physics
admin-c: 23391-LUMII
tech-c: 23391-LUMII
nserver: DNS1.SUPREMESERVER11.COM
nserver: DNS2.SUPREMESERVER11.COM
changed: dns-reg@nic.lv 20031201
source: LUMII

person: BOGDANOFF IGOR
address: none
phone: 33608250825
e-mail: igor.bogdanov@wanadoo.fr
nic-hdl: 23391-LUMII
source: LUMII

That address now hosts a web-site for the Mathematical Center of Riemannian Cosmology, which mysteriously seems devoted to the work of the Bogdanovs. The newsgroup fr.sci.physique has all sorts of threads devoted to the Bogdanovs. In one posting by the brothers, where they give their e-mail address as "igor.bogdanoff@phys-maths.edu.lv", they helpfully explain that the University of Riga set up the site for them and that's why it is in the Lithuanian DNS.

One problem: Riga is in Latvia, not Lithuania.

I take this kind of personally because, besides being an American citizen, I also have a Latvian passport (the Latvian version of my name is "Voits"). My father was born in Riga, and he and his parents became exiles at the time of the Soviet occupation starting during World War II. I've visited Riga several times (including a visit to the university), first soon after independence on a trip with my father while he was still alive. Riga is a beautiful city, with the downtown not much changed since before the war. In recent years the old city and much of the downtown have been elegantly renovated, and Riga is now once again a large, vibrant city with great restaurants, hotels, shops, etc. And now it has an International Institute of Mathematical Physics.

Comments

1. **Bogdanoff**
   July 1, 2004

   Dear Fabien,

   Just a precision. You write that our quantum group paper is in our book. But if you compare the "annexes" of Avant le Big Bang with the paper itself, you will have to admit that only a short fragment of it was published in the book. In fact, we published this extract (our theorem 3.3.2) much more as an example of a scientific paper (which may be of interest for a large audience). The objective was not to be understood by specialists of quantum groups theory (many readers said that they liked it the same way they like some creation of modern art.). But at the end, the only thing that matters is the "impression" left by a book. In the case Avant le Big Bang laks of precision or contains some mistakes, we hope that grace to you and others, we will be able to correct it.

   I/G

2. **Fabien Besnard**
   July 1, 2004

   The intersection is not the empty set since your quantum group paper IS in your "large audience" book ! When I say you do not follow the scientific method I include a lack of intellectual rigor which is a minimum requirement even in a large audience book.

3. **Bogdanoff**
   June 30, 2004

   Dear Fabien Besnard,

   Thank you for your response which has the advantage to be honest.
   But now : how can you compare a quantum group paper with a large audience book? From our view, the intersection between these two categories is really an empty set. A scientific paper is written for experts; by definition, it requires a high level of technicity and very few readers are able to grasp the real meaning of it. At the opposite, a book is not written for experts, even not for specialists of scientists : it is meant to be read and understood by a large audience. Before the Big Bang was written as a large audience book. Not as a scientific paper. Indeed you are right when you say that it does not follow the scientific method. Of course not. But it still reflects a certain "state of the Universe" by an other path. It may contain some mistakes, erros or misquotings (Woit, Schreiber); but at the end "something" good may remain; So we propose that instead of burning the whole, you make the effort to look after this tiny "something" that may be hidden somewhere between the pages.

   Thank you for your attention

   I/G

4. **Fabien Besnard**
   June 30, 2004

   To IGB : it is not the place to discuss about that, but since I have been attacked at several places on this weblog, I will made one last comment : I began to read your article but then I read your book, and there I found many evidences that you do not follow the scientific method (to say the least). So it was pointless to go on reading about bi-crossproducts. Another important thing is that I realized that if ever I turned out saying that just one line of calculation was right in your paper, you would go everywhere misquoting me, perhaps making me one of your supporters in your next book !

5. **Bogdanoff**
June 30, 2004

Tell us, dear Fabien Besnard, how you can follow such a line of de-reasoning? You see, since you wrote your article, you got involved in a matter that is obviously beyond your field of expertise. You even wrote us (May 20,2004) : " I had a glance on your "bicrossproduct article". Since I do not know this matter, I have to refer to Majid and it may take some time." You never got back to us. Following your own statement (since you admit in you letter that you do not understand our work in quantum groups) on what basis do you claim that this work should be "empty"?

Obviously, your reaction is neither "objective" nor scientific.

Thank you for your attention,

IGB

6. **serenus zeitblom**
June 30, 2004

By the way, I wonder how the people in the universities in Hong Kong feel about an address like http://th-phys.edu.hk/? Has anyone asked them?

7. **Fabien Besnard**
June 30, 2004

Tell me, dear Ark, how someone who "thinks" can still use such a line of reasoning as "A is a crank and go unnoticed, B has been caught, so I will defend B" ? I never heard of the people you speak of (Tiller et al) so they don't bother me. But now each morning I hear an advertisement for Bogdanov's book on my favourite radio station. This is enough for me not to turn my attention away from B&B at the moment…

8. **ark**
June 29, 2004

Nobody knows for sure why the world seems to be 4-dimensional. Anthropic principle is one pseudo-explanation. That I have five fingers on my hand, and one is evidently different than the other is another "explanation", as good as any other one, including one with four different kind of numbers.
Some people think, some don't. Some use their cerebral cortex, some don't. Some have souls, some, evidently, don't. That's normal.

Now, to just shift your (YBM (pseudo of Besnard))attention to a slightly different subject let me quote from a book by three American "scientists". In fact I invite all of you to visit my web page

http:/quantumfuture.net/cass/bogdanov3.htm

There you will find:

> "Conscious Acts of Creation: The emergence of new physics by William A. Tiller", PhD, Walter E. Dibble, Jr., PhD, and Michael J. Kochane, Ph. D. The Preface to the book is signed by Rustum Roy, Penn State, Wayne B. Jonas, USUHS and …. E.C. George Sudarshan, University of Texas, Austin.
>
> Are we ready to be bitten by monopoles? Here is a quotation:
>
> "Through the agency of a nine-dimensional coupling substance in the vacuum, called deltrons, the faster than light magnetic monopoles functioning in reciprocal space can interact with electric monopole substance functioning in direct space of physical matter. The vacuum phase transition mentioned above involves an ordered phase formation of magnetic monopole substance phase of R-space. These magnetic monopoles of R-space travel so fast that they 'write the waves,' thought to be de Broglie pilot waves, controlling the movement of particles in D-space. A symmetry principle, called the mirror principle is thought to operate between D-space substances and R- space substances so that the monopole charge singularities in one space produce dipole images through the mirror into the other space. Likewise, the monopole mass singularities in one space produce images through this mirror into the other space. Thus the negative negative monopole mass singularities of R-space are thought to be the origin of the 'dark matter' we currently detect with our instruments in D-space.(…)"
>
> If you think that these concepts are explained somewhere in the book – you may be surprised to find out that after you are through with the book, you will not have an idea of what the "theoretical part" is about.
>
> Now, let me mention that I wrote papers on magnetic monopoles and on "reciprocity" (including conformal symmetric spaces). Therefore, I suggest, that after we are done with KMS, signature fluctuactions, and topological field theory, there is a new field to be exploited in the space of hoaxes: D-spaces and R-spaces and monopoles that write the waves, thought to be de Broglie pilot waves…. All supported by PhDs.
>
> ark
>
> Conscious Acts of Creation is being madly promoted by the "alternative crowd" as "proof" that all their "you create your own reality" ideas are true.
> Conscious Acts of Creation, tells us:
> This book marks a sharp dividing line between old ways of scientific thought and old experimental protocols, wherein human qualities of consciousness, intention, emotion, mind and spirit cannot significantly affect physical reality, and a new paradigm wherein they can robustly do so! ?utilizing a unique experimental protocla on both inanimate and animate systems, that the human quality of focused intention can be made to act as a true thermodynamic potential and strongly influence experimental measurements for a variety of specific target experiments.
>
> After almost 400 pages of speculation and descriptions of experiments and very little math, we are told:
>
> Under some conditions, it is indeed possible to attach an aspect of human consciousness, a specific intention, to a simple electrical device and have that device, when activated, robustly influence an experiment conducted in its vicinity in complete accord with the attached intention. Thus, if they do it right, humans can influence their environment via specific, sustained intentions. [?] Some new field appears to be involved in the information passage that occurs between conditioned locales that are widely separated from each other in physical space. Even with transmitters and receivers located inside electrically grounded Faraday cages, highly correlated patterns of information appeared in the remotely located locales. [?] Although we don't fully understand them, we now have some new tools with which to probe the deeper structures of the universe and a new adventure is underway for humanity.
>
> It is important to note that the "intenders" of the experiments were long-time practitioners of Siddha Yoga and could thus be considered metaphysically "in tune" to some considerable extent. The question is: What did they accomplish? Based on the descriptions, it sounds pretty earth-shaking, right?

> Well, as noted, after almost 400 pages we find that the most significant result seems to have been changing the pH of a small sample of water.
>
> Yup. That's it.
>
> But we notice that nobody says a word about this sort of thing. At this point in time, the physics community hasn't said a discouraging word about Tom Bearden and his follower, Richard Hoagland – nor the interesting fact that Bearden is a former associate of Ira Einhorn, Andrija Puharich, the gang at Esalen and Penn State who flirted with the edges of the Secret Government experiments in mind control and the promotion of LSD to the young people of America. Nobody has cried "Hoax!"

Happy reading!

ark

9. **YBM**
June 29, 2004

? YBM (pseudo of Besnard) ?

The last line of defense : pretend that everyone and his brother questioning their work is in fact one person. It is so desperatly ridiculous (anyone familiar with fr.sci.* would know) that I wonder if I should deny.

BTW, Mr Woit, did you know that in their book they pretend (sqrt(5)+1)/2 is transcendental ? And that the Universe has four dimensions because there are four kind of number (integers, rationnals, reals and complexes) ? Therefore, in their spirit, I aprove the following statement :

if the golden ratio is not albebraic, then I am Fabien Besnard (and the Pope as well)

10. **Igor Grichka Bogdanoff**
June 29, 2004

Dear Peter,

We do not understand how someone (like you) who is very familiar with internet technics cannot make any basic difference between a post coming from us and a message coming from someone else. Instead of checking the situation with us you immediatly write an article on it, prentending that you got a message from us hidden behind a "double pseudo". Once for good : Alexandre Fau is NOT a pseudo of Bogdanoff! We do not know this person, we never met him, but we sent him an email yesterday. He must have sent you another email and we hope that you have a clearer view of the situation by now. If we want to write you, we do not need to hide ourselves behind anyone. We do it directly, as we wrote you this morning. Our advise is that you should not listen too much people like Besnard or YBM (pseudo of Besnard). There are much more serious things to do and other important problems to solve in the Universe.

Best regards

I/G

11. **ark**
June 29, 2004

Hi Peter,

I agree that misquoting was bad and you did the right thing pointing it out. As for problems "at the top levels" – well …. It all started long ago with the "publish and perish" ideology and research grants and money tied to politics. I decided to try to create an "alternative research institution". Whether it will fly or fall – we will see. For a couple of years I was a v-ce director of the Institute of Theoretical Physics in Uni Wroclaw, Poland. So I was witnessing how the level of the research deteriorates with the institution of plans and grants. It deteriorated to the extent that I decided to leave organized science completely. Ambitious and/or original projects have little chance to get support. In the US we have, in addition, the military complex that intercepts what is in their field of interests, which additionally severely restricts the freedom of research.

These are just few thoughts that are on my mind right now. I wish you success in your mission!

ark

12. **Peter**
June 28, 2004

Hi Ark,

Actually I'm not spending my time criticizing the scientific activities of the Bogdanovs. They're not much of a problem for the scientific community. Much more serious is the situation at the top levels of the US (and other country's) scientific establishment in particle theory, and it is that problem that I have been trying to do something about, despite warnings from many that it is unwise of me to challenge powerful interests.

On the other hand, if they are going to do things like publish books misquoting me and send me e-mail purporting to be from someone other than who it is really from, I'm going to use this forum to tell about this activity, and, yes, ridicule it.

13. **ark**
June 28, 2004

If only to amuse you (as apparently some of you guys has nothing better to do than follow Igor and Grichka, why don't you choose Thomas Bearden or Myron Evans, with connections to US DOD and DOE agencies?):

It does not matter when the server is – if it is convenient to have a server in Thailand – it will be in Thailand. The cheapest servers are certainly in US, but due to Bush Reich lies, some organizations do not like to have servers in US. While some do.

Notice that International Institute of Mathematical Physics has updated its web page at

http://th-phys.edu.hk/

(I did it) though it is still in status nascendi, so it will have its reasonable web site in due time. For the time it is just a "project", even though officially registered, yet it will take about a year to give it a shape.

If you wish to have me help you to find american targets for your attacks (because France has been already used), I will certainly be willing to help you. If you are really so moved by the idea that physics publications should all make sense, and all mistakes should be ridiculed – just try to read carefully, checking every formula and every sentence, all papers in arxiv.org. 80% of these papers will be nosnsensical, and can be easily ridiculed – if one is determined to do so, and has nothing better to do.

ark

14. **Mcrc**
June 28, 2004

There is nothing "mysterious" with the existence of this website : it has been legally registered and it is ruled from Paris. So what? Where is the problem? Why bother any further?

15. **YBM**
June 28, 2004

"Mathematical Center of Riemannian Cosmomogy"

"Cosmomogy" is the name ! Ridiculous and nonexistant : the place where you consider the consequences on the Universe of the hypothesis that the golden number is transcendant ?

16. **igor**
June 28, 2004

YBM wrote in message news:…
> Tiens, puisque vous ?tes r?veill?s, Peter Woit vient de mettre
> en ligne un r?sum? de votre pitoyable tentative d'inventer encore
> un Institut International de Math?matiques tout entier d?volu
> ? votre gloire :
>
> http://www.math.columbia.edu/~woit/blog/archives/000045.html
>
> Comment pouvez-vous ?tre si maladroits ?

Re bonjour,

A la suite d'une conf?rence faite ? Riga (2001), les responsables de l'informatique de l'Universit? nous ont propos? de cr?er un site. Comme il fallait bien lui donner un nom, nous avons pens? ? Mathematical Center of Riemannian Cosmomogy, qui refl?te exactement notre domaine de recherche.
Il y a environ un an, nous avons cr?? les quelques pages correspondant ? nos orientations de travail, papiers, etc. et le site a ?t? cr?? ? l'automne 2003. Comme tous les autres sites, il est administr? par l'institution suivante :

University of Latvia
Institute of Mathematics and Computer Science
Department of Network Solutions (DNS)
Rainis Boulevard 29
Riga LV-1459

Voil?. Depuis 2001, nous ne sommes plus jamais retourn?s ? Riga et n'entretenons aucun lien particulier avec ce pays. Notre seul lien tient au nom de domaine qui nous a ?t? attribu?s et que nous op?rons depuis la France.

Merci de ton attention,

Amicalement,

I/G

17.
June 27, 2004

Oops that's Estonia. Guess I was thinking of Mikhail Tal? 🙂

18. **D R Lunsford**
June 27, 2004

Paul Keres!

19. **JC**
June 27, 2004

Also amusing is how some of the links on phys-maths.edu.lv to the Bogdanov's papers and *.gif picture files, all appear to be on an expired account at http://www.mydocsonline.com

These guys have been doing a piss poor job of even trying to perpetuate their own hoax. Probably even a high school computer whiz kid can do a better job!

20. **Peter**
June 27, 2004

Doing a "whois" search on the networksolutions.com website shows that the address is hosted by a web hosting company called "Ancient Media" at an IP address in South Carolina.

21. **JC**
June 27, 2004

It appears the machine hosting phys-maths.edu.lv is not even physically in Lithuania nor Latvia. A traceroute reading to phys-maths.edu.lv looks like it's hosted in north america. If you do a traceroute to machines located physically in Lithuania (.lt) or Latvia (.lv), you'll typically get longer delay times that are characteristic of transatlanic connection times to eastern europe or Russia. Even America -> England connections, shows the transatlanic delay.

The Tevatron as Earthquake Detector

June 29, 2004
Categories: Uncategorized

It seems that the Fermilab Tevatron is a quite sensitive detector for earthquakes. According to the Accelerator Update for yesterday, a small earthquake in Illinois caused the machine to lose its beam and a superconducting magnet to quench. A few hours later, a more significant earthquake in Alaska was quite visible to the Tevatron accelerator operators, and would have caused another quench, but they hadn't restored the beams yet.

Comments

1. **Thomas Larsson**
   July 1, 2004

   I think one of the CERN experiments once detected some very mysterious bidayly effect. It turned out to be correlated to the timetable of the Lyon-Geneve express.

2. **Dick Thompson**
   June 30, 2004

   Since you provided the link, I've been following the daily updates. What amazes me is that they ever get it to work at all, given the number of interacting super high-tech pieces being run near or at their design envelopes.

3. **Yu**
   June 29, 2004

   Maybe this can be used as a reason to ask for more funding!

4. **Alejandro Rivero**
   June 29, 2004

   CERN is sensitive to the level of water in the lake of Geneve.

Lattice 2004 Talks

June 30, 2004
Categories: Uncategorized

Some of the talks given at the Lattice 2004 conference are now available on-line. Particularly interesting is Kenneth Wilson's historical talk about his roles in the development of the renormalization group and of lattice gauge theory. He also refers to the website of the Dibner Institute.. One interesting thing there is a collection of materials relevant to the history of the renormalization group.

The Dibner Institute is at MIT, but during my high school days I recall visiting the library for the history of science that Bern Dibner had founded which was located close to where I lived, in Norwalk, Connecticut. The collection was moved up to Cambridge after Dibner's death in 1988.

No Comments

Revising the Landscape

July 1, 2004
Categories: Uncategorized

One contributor to the comments here (JC) has pointed out that Susskind has withdrawn from the arXiv his recent paper on the stupendous Landscape of sting theory. This is pretty unusual, but often when it happens the author puts in the withdrawal statement some indication of why the paper was withdrawn, something Susskind didn't do in this case. Another contributor to the comments (Serenus Zeitblom) points out that one should look at recent changes to Douglas's paper on the arXiv, which is now up to its fourth version.

One feature of the arXiv is that all posted versions of papers are available, so one can compare them and see what changes the author made. The history of Douglas's paper is quite something. The original version was posted on May 30. Susskind's now withdrawn paper was posted on June 17, and in it he claims that Douglas's paper showed that Susskind's argument in an earlier (May 21) paper (which exists in three versions) was wrong. The latest version (4) of Douglas's paper now says that earlier versions of the paper are wrong. So, one reason Susskind withdrew his paper is presumably that its claims that his earlier paper was wrong were now wrong because they were based on Douglas's wrong paper. Got that? This all seems to me to be a new and original version of the "Not Even Wrong" phenomenon.

Some other high points of the changes in the four versions of Douglas's paper:

1. Going from version 1 (May 30) to version 2 (June 2), he changes

"If I had to bet at the moment, I would still bet that string theory favors the low scale, for the reasons outlined above, but it is not at all obvious that this is what will come out in the end."

to

"At this point, it is not at all obvious whether high or low scales will be preferred in the end."

2. Going from version 2 (June 2) to version 3 (June 22), he adds a reference to Susskind's June 17 paper, some criticism of it, and the sentence

"The correct assumptions could be determined from string/M theory considerations with more work, and we are optimistic that this can be done in time to make convincing predictions before LHC turns on in 2007."

3. Going from version 3 (June 22) to version 4 (June 29), he removes the sentence above (I guess he became less optimistic last week) and announces that the argument in the previous versions was wrong.

Comments

1. **sol**
   July 3, 2004

   I wanted to add this as well.

   "*Quantum gravity is the field devoted to finding the microstructure of spacetime. Is space continuous? Does spacetime geometry make sense near the initial singularity? Deep inside a black hole? These are the sort of questions a theory of quantum gravity is expected to answer. The root of our search for the theory is a exploration of the quantum foundations of spacetime. At the very least, quantum gravity ought to describe physics on the smallest possible scales – expected to be 10-35 meters. (Easy to find with dimensional analysis: Build a quantity with the dimensions of length using the speed of light, Planck's constant, and Newton's constant.) Whether quantum gravity will yield a revolutionary shift in quantum theory, general relativity, or both remains to be seen."*

   http://academics.hamilton.edu/physics/smajor/quantgrav.html

2. **sol**
   July 3, 2004

   JC,

   The Elegant Universe, by Brian Greene, pg 231 and Pg 232

   "But now, almost a century after Einstein's tour-de-force, string theory gives us a quantum-mechanical discription of gravity that, by necessity, modifies general relativity when distances involved become as short as the Planck length. Since Reinmannian geometry is the mathetical core of general relativity, this means that it too must be modified in order to reflect faithfully the new short distance physics of string theory. Whereas general relativity asserts that the curved properties of the universe are described by Reinmannian geometry, string theory asserts this is true only if we examine the fabric of the universe on large enough scales. On scales as small as planck length a new kind of geometry must emerge, one that aligns with the new physics of string theory. This new geometry is called, quantum geometry."

   You were right to point this out and from a common perspective, as a starting point .

   But these discription were not limited to string theory alone as we have come to witness from the participants of LQG or dynamical traingulations of quantum gravity(quantum geometry).

   Again you were right to point out the many facets of the attempts here, including Penrose and his twistors.

   I am trying to find a common bound between them all, as we all are?:) Some of my perspectives are being revealled on physicsforum, and the imput back to me, is pointing towards the usage of Glast as a possible discriptor of the metric.

   All speculation of course.

3. **JC**
   July 3, 2004

   sol,

   What is your definition of "quantum geometry"? Over the years I've noticed the term "quantum geometry" being used and abused for all kinds of things, depending on who you're speaking to.

I don't really have a good precise definition for the term "quantum geometry", other than perhaps (in a vague sense) looking at what happens to the geometry of classical general relativity when quantum corrections are added in. Since not many of the quantum gravity paradigms are particularly that successful, I don't feel there's really any easy way to even quote a universal definition of "quantum geometry" that everybody can agree upon. At this point in time, string theory has the furthest progress in terms of looking at a "quantum geometry".

In a non gravity context, perhaps "quantum geometry" could be defined as examining the "geometry" underlying quantum Yang-Mills theory.

Anyone else have a better or more precise definition of "quantum geometry"?

4. **Thomas Larsson**
July 3, 2004

Alejandro,

All I meant was that values of atomic masses are to some extent due to historical accident. The allowed ranges are of course too small to really affect the viability of human life.

5. **sol**
July 2, 2004

I was always fascinated by the relationship to the isometrical relationship orbitals presented in terms of self similarity. To cosmological situations the representations eem awfully uncannny.

Arivero, and from that standpoint, could we have indeed found a escoteric(has it been removed?) way to speak on the nature of quantum geometry?

6. **Alejandro Rivero**
July 2, 2004

Last week I was informally presenting to my elders my work on nuclear masses, and someone mentioned Rydberg but I was unaware of all the history. Thanks very much for bringing it here. Although I can not see how the isotope theory is anthropic-environmental (?). Well, of course, a sort of anthropicity principle could be to request the existence of Dalton's atoms. Plausibly this requires the proton mass to be well smaller than the ones in the higgs/electroweak sector, but this is too modern for Rydberg 🙂

Aside, I believe to remember that the original isotope proposal was linked somehow to esoterism.

7. **Thomas Larsson**
July 2, 2004

There is one obvious example successful of anthropic, or at least environmental, reasoning that I am surprised not to have seen mentioned: the isotope theory. 120 years ago, Janne Rydberg was probably Sweden's most famous physicist. Nowadays he is remembered for the series and the constant, but his main interest was to derive some deep explanation for atomic masses. When it eventually turned out that atomic mass is due to a rather random mixture of isotopes, he became so depressed that he had to seek professional help.

There are of course many differences, e.g. that we can successfully explain the masses of individual isotopes, at least in principle. But I don't see a problem if some numbers must be explained by historical accident, as long as the main predictions of your theory (extra-dimensions, SUSY, massless scalars, proton decay, huge and negative CC, etc.) agree with experiments.

8. **Alejandro Rivero**
July 2, 2004

JC, indeed it is easier and a lot more honest simply to point out the empirical data your theory needs. Newton does no predict the mass of the Earth nor Moon, and still it is a pretty theory.

The point here is that the "monovacuum" and "anthropic" folks actually share a common goal of defining a single theory having a single solution. But the second group has evolved towards a tricky, not very honest, methodology. The original anthropic principle, as I remembered it, claimed that we can not determine theoretically some fundamental constants because they could have any random value in some range, and we just happen to be in one of the possible ranges. The now-selling A.P. claims that the value of the fundamental constants is determined theoretically just because we are here.

The "monovacuum" folks follow a different argument. They are on the [very extended, but rarely explicit] oppinion that there is only a consistent way to describe spatial change (ie movement) in a mathematically consistent manner. In fact it is surprising how few methods do we have to describe dynamics, so it could be. In this view Classical Mechanics is logically incomplete or inconsistent (zero limits, incompatibility with electromagnetism) and the same happens with Classical Field Theory (self-energy of electromagnetism) and Quantum Field Theory (Renormalisation), while the fundamental yet to be string theory should be completely sound. The argument is antropic in a deeper sense than the antropic principle: it assumes that Reality must be exactly described my using [matematical] Words.

Hmm I feel I am not answering your comment but simply expanding or re-stating the previous of mine. Still, I wanted to go over it because of your mention of "compactness". A side of the "compactness" of a theory is related not to its mathematical complexity, but to its degree of uniqueness.

I supposse that at the end one can reduct this uniqueness to some counting of the axioms and postulates needed in the theory (while complexity could be a count of lemmas and theorems), and to count the external input too.

9. **JC**
July 1, 2004

Alejandro,

Many years ago I use to think of physical theories as a "compact" way of representing information about a particular phenomenon, in a vague sense. If a theory is so complicated and cumbersome, that it takes up more computer "storage space" to represent the mathematical formulas than it takes to represent the original experimental data, it would be easier just to quote the experimental data instead of the cumbersome "theory". (This is sort of an abuse of "Occam's Razor" without being too precise). The obvious case is that of Newton's laws representing many different phenomena like Kepler's laws, pendulum motion, free fall motion, etc … With quantum mechanics, the n-body Coulomb problem for the Schrodinger equation can account for the spectral lines of atoms, etc … The theories of Newton and Schrodinger are a very "compact" mathematical representation, compared to quoting all the experimental data for all the possible phenomena they represent. (I think it was Gregory Chaitlin who advocted this point of view over the years).

For really highly nonlinear systems that show up in fields like economics, biology, politics, psychology, etc … a lot of them are so mathematically cumbersome to the point that they are hopelessly complicated, where the most "compact" way of representing the phenomena is just to quote the original empirical data. A typical example of this is the empirical weather data, compared to the complicated nonlinear fluid dynamics systems which attempt to approximate the weather (in a less than satisfactory manner for the most part).

I'm being somewhat vague on purpose, since I don't have a precise definition for "compact" representation of a physical phenomenon. Over the years I have changed my views on some of these ideas. I may very well end up dropping the entire framework in the future if I can't find a way to resolve the issues that I have been deliberately vague about.

With respect to string theory, the "monovacuum" folks want a single unique vacuum solution which in a vague sense gives a "compact" representation for a quantum theory of gravity. With a proliferation of a zillion Calabi-Yau manifolds and many other solutions in the String Landscape, then string theory becomes one of those nasty cumbersome theories which becomes hopelessly complicated to deal with. It may very well be easier just to state the empirical data from the particle physics data books and the Standard Model, along with whatever empirical data is coming from the astrophysics folks (ie. cosmological constant, etc …) as the most "compact" representation of the phenomena that string theory originally was "advertised" to represent.

10. **Alejandro Rivero**
July 1, 2004

I had always though of it as a cosmologist/philosopher gadget. The Weak Anthopic Principle and all that.

A decent motivation for it can come from
"Things are words" (20, 21st century AD), as Larsson quotes Polyakov. But forget the explanation from P. I understand that the "things are words" principle can be used to tell us that any theory of Reality must be done with words, and thus all the physics we do is limited to this. A physicist can say "words, and more concretely mathematical words", and then look for a proof of existence and uniqueness of mathematical theories fitting the known data. This was the spirit of GUT and it is the spirit of string field theoretists, to use the "things are words" principle to get a single unique theory.

My understanding is that string is failing to get uniqueness, and then they perturb their own philosophical principle in order to say "things are words coming from empirical input", or something so. But then it loses all its appeal.

11. **JC**
July 1, 2004

Alejandro,

Do you have any insight as to why exactly some physics people end up believing in anthropic arguments and ideas?

If I didn't know any better, I get the sense that a person using the anthropic principle is attempting to justify a particular idea/theory (whether popular or unpopular) that they are not willing to give up on easily. In the days when I was still a true believer in string theory, I would have found it very difficult to give up on it as a solution to quantizing gravity. I probably would have been making every possible excuse and argument to rationalize my own personal biases and prejudices in favor of string theory. If the anthropic landscape ideas were around when I was still doing string theory, I certainly would have tried to use them as my own personal justification to continue to believe in string theory.

For some folks, it may possibly be an "ego" thing where they don't want to admit that they spent many years of their life, time, and energy on something that may very well be completely wrong in the end.

12. **D R Lunsford**
July 1, 2004

JC-

One thing that comes immediately to mind is Poincare and his wrestling with the metaphysics of F=ma (see "Science and Hypothesis" and "Science and Method"). If he had just forged ahead without worrying too much about how people are related to physics, he might have hit on relativity before Einstein.

-drl

13. **Alejandro Rivero**
July 1, 2004

I keep telling, suppose that we ask for sentient persons able to measure an earthly acceleration of 9.8 meters/second^2. Is this an anthropic requeriment? To me, it seems just experimental input. As people become aware that antropic=empirical input, the club will dissolve. I hope.

14. **JC**
July 1, 2004

It will be interesting to see how many "monovacuum" folks will still be left actively working on string theory, if the anthropic folks end up having the most influence on the string research agenda.

It would be interesting to go back into physics history and see which paradigms and research fields had a "kiss of death" once the experts were starting to use the anthropic principle in a "serious" manner.

Hawking Buzz

July 3, 2004
Categories: Uncategorized

The buzz is that Hawking has a new idea about how to resolve the “Black Hole Information Paradox”, the well known incompatibility between standard ideas about black holes and the unitary time evolution of the wave function that is fundamental to quantum mechanics. Evidently Hawking has asked to give a talk about this at GR17, a big conference on general relativity that will be held July 18-23 in Dublin.

The abstract for his talk goes like this:

“The Euclidean path integral over all topologically trivial metrics can be done by time slicing and so is unitary when analytically continued to the Lorentzian. On the other hand, the path integral over all topologically non-trivial metrics is asymptotically independent of the initial state. Thus the total path integral is unitary and information is not lost in the formation and evaporation of black holes. The way the information gets out seems to be that a true event horizon never forms, just an apparent horizon.”

I can’t tell exactly what that means either, so I guess we’ll have to wait for the talk. My own prejudice about quantum field theory is that the relation between the Euclidean and Minkowski space formulations of quantum field theory is actually much more interesting and subtle than people think. It’s not just a technical trick. So I’ll be interested to see what Hawking has to say about this.

Something else at the conference that may be interesting will be Sir Roger Penrose’s public talk entitled “Fashion, Faith and Fantasy in Modern Physical Theories”. Guess what he is referring to by “Fashion”.

Comments

1. **Alejandro Rivero**
   July 8, 2004

   Another subtlety about Minkowskian signatures is the embedding theorem. Not usually mentioned, but worth to have it at the back of some neuron.

2. **Urs Schreiber**
   July 8, 2004

   Wick rotation in QFT on a fixed background is certainly deep. But for quantum gravity it is for the obvious reason very problematic, as has been discussed at great length on s.p.r. for instance.

   Hawking has worked a lot on Euclidean path integrals for cosmology in the past, and he even goes as far as introducing his ‘Universe in a nutshell’ book with saying that our universe is essentially a Euclidean 4-sphere in a sense. (I think he should have mentioned that this is more of a personal view, a hunch, which is not necessarily shared by the community or even demonstrated by results, I’d think.) Maybe it is just due to my ignorance of the literature, but I am having trouble coming up with past results or even hints that the Euclidean path integral for gravity is the thing to look at. Now, maybe Hawking’s new idea about black hole information is just that hint which I am looking for, I don’t know. But currently I am a little sceptical.

3. **Godfrey Miller**
   July 6, 2004

   I saw Sir Roger when he gave the same talk(s) at Princeton on Oct. 17, 20, and 22. On that occasion, Fashion, Faith, and Fantasy were each the subject of one night’s talk.

   I thoroughly enjoyed what he had to say, but Princeton is a hub for string theory, and I got the distinct feeling that my position was in the minority.

   You can watch them at the following address:
   http://www.princeton.edu/WebMedia/lectures/

4. **D R Lunsford**
   July 5, 2004

   Peter,

   Could you give some information about it?

   To me the signature seems physical, and I don’t see how it is possible to alter it.

   Complex structures that arise naturally out of geometry are another matter. (Think for example of the circular points at infinity in plane projective geometry.)

5. **Alejandro Rivero**
   July 4, 2004

   The french girls did a good work to introduce the subtleties of path measuring, at the end of their geometry manual.

6. **Bogdanoff**
   July 3, 2004

   Quote : “My own prejudice about quantum field theory is that the relation between the Euclidean and Minkowski space formulations of quantum field theory is actually much more interesting and subtle than people think.”

   Well, it is exactly what we think. And by the way, certain questions raised by S.H. are developed in our “Topological field Theory of the Initial Singulariry of Space time” paper (CQG) that was extensively discussed but not really understood as a new formulation regarding the transition between lorentzian metric (real time, still valid et Planck scale) to euclidean metric imaginary time, only valid at 0 scale). In our model, this transition becomes effective between Planck scale and 0 scale and is characterized by quantum fluctuations of lorentzian and euclidean metrics (superposition (KMS) state of metrics). In this model, the “apparent horizon” evoked by Hawking could be the result of an “euclidean evolution” of events in the black hole whose final singularity does not exist in real (lorentzian time) but in imaginary (euclidean) time.

There They Go Again…

July 6, 2004
Categories: Uncategorized

Throughout the mid-to-late 80s, the NSF and other organizations would periodically issue alarmist reports about an impending dangerous shortage of scientists and engineers in the U.S. These projections for shortages turned out to be utter nonsense, as anyone who looked at the situation honestly could have foreseen. By the early 90s, instead of a shortage, the bottom dropped out of the employment market for scientists. In the math department here I saw a large number of very good graduate students and post-docs leave the field because there were no jobs for them.

Particle theory is a field in which the job market has been varying degrees of awful since about 1970. One might argue that while this is tough on young particle theorists, it means that the few who get jobs will be truly outstanding and the subject will flourish. The problem with this argument is that particle theory has seen extremely little progress since the mid-70s, about the time that one would expect the effects of the tight job market to be seen. The main reason for this is that the standard model is just too good, but one could plausibly argue that the evidence is that a very tight job market is bad for the field. Good people don't go into it; those that do and survive do so by not working on anything too ambitious, because it could easily fail and they'd be out on the street.

Mathematics is a much more normal job market than particle theory, but there still have always been a lot more Ph.D.s than jobs where they can use their talents. The job market in math was terrible in the early-to-mid nineties, got better in the late-nineties and is not so bad now. Our students seem to be doing relatively well at getting jobs. Budgets are tight, especially at state universities, but a lot of people hired during the 60s are finally starting to retire.

The NSF is now at it again, with its National Science Board issuing a glossy report entitled [An Emerging and Critical Problem of the Science and Engineering Labor Force](). A good [article]() in the Chronicle of Higher Education reports on this, but also has a lot of information debunking the report.

Why does the NSF repeatedly engage in this kind of alarmism and dishonesty? If you take a look at the membership of the National Science Board, you'll find no young scientists, but a lot of university and NSF administrators, corporate executives, and senior professors. All of these people have a large vested interest in flooding the scientific labor pool in the U.S. so that it will provide a lot of cheap labor. The NSF gets much of its funding from Congress by emphasizing its role in training scientists, so of course it wants to claim that more of this needs to be done. Universities and corporations want lots of new Ph.D.s so they can get the best ones to work for them for peanuts. Universities want lots of grad students to provide cheap labor as TAs. Senior professors, at least in the experimental sciences, want lots of grad students to staff their labs cheaply. These people all seem to firmly believe that a system that produces huge numbers of underemployed and badly paid young scientists is the best thing for science and for the U.S. as a whole. In fact, it is just the best thing for their personal interests.

Comments

1. **DrP**
   July 16, 2004

   wrong wrong wrong

   briefly:

   Just because our society doesn't 'value' science doesn't mean our society, the job market, the corporations, whatever, is right.

   The question is how to make the valuation of science match it's value.

   There's tons of incomplete scientific projects. Protenomics, diseases we didn't know we had, brain/computer research, nanotech, whatever… lots of valuable potential for hundreds of years. The fact that these are not getting the attention that they deserve, should be regarded as a fault of the system, not an affirmation of the system and it's correct working due to infallible economic 'laws'.

   People would benefit. If society structures (ie corporations, governments) don't "benefit", so much the worse for all. For what is it all for?

   An investment in science is an investment. If the current system can't see beyond the shortsighted next accounting period, so much the worse in my regard for the system.

   Wealth is result of science. Lawyers, MBAs don't create wealth, they just administer the moving of the wealth from pile to pile. (Even though they may be 'knowledge workers'.)

   If our society pays more for other jobs, encouraging our best to enter other fields, it's just evidence of being a badly organized society. It is not living up to what it COULD be. If planet Earth had to "compete" on a comparison basis with a similar planet Earth2 that had a much stronger scientific culture, which would be getting ahead? So why not make this world be that other world?

   The question, (as has been said,) is how to change it. (And if you don't recognize what quote I'm making an allusion too, well, so much the worst for us all.)

   Further weblog blatherings on this topic are available at
   http://science.slashdot.org/article.pl?sid=04/07/09/1941201&mode=thread&tid=134&tid=146&tid=99

2. **erinj**
   July 13, 2004

   My two pence on this: I also agree with Peter, and also JC and Steven. I too was appalled at how post-doc research actually was in reality (in the UK in my case), and I worked similar hours to you Steven. Increasingly I see my post-doc year as simply an experiment: I wanted to find out what it was like, and try to achieve something lasting, but looking back I am beginning to regret it, as it seems to have done little for me in terms of career prospects, it's value when applying for other occupations and my health.

   I could immediately relate to your comment about having "too many" degrees, JC: since finishing my post-doc, it has taken me months to find office-based temporary work, and I believe I only got the job because I omitted my PhD from my resume! For every other job I applied for, in which I included my PhD in my resume, I was either rejected or never contacted about the job.

   I never realized possessing a PhD would make things harder for me (especially when it comes to employment), but it definitely seems to have done so. People with PhD's are just not meant to find work easily!

3. **Alejandro Rivero**

July 13, 2004

Wish it were completely invaluable. I mean, that we had so huge quantity of reasoning minds that their monetary value were zero.

Actually I wish the same about production of commodities, but that is politics 🙂

4. **Ralph Lee**
   July 11, 2004

   appreciated the perspective, thanks.

5. **bob**
   July 10, 2004

   I'm so glad the academia is feeling the pain physical labour workers have known for a long time. Your work once it becomes commoditized is no longer valuable because of the surplus amount of workers competing for jobs you can exploit them for all they are worth for the benefit of the owners of the school or business in question.

6. **bob**
   July 10, 2004

   Welcome to the wonderful world of capitalism. Now you know why people like Albert einstein endorsed some form of socialist policy and regulation of capitalism, he didn't want to eliminate it totally he wanted to eliminate the exploitation that capitalism brings and the negative capitalistic traits. Capitalism is not about being competitive it's about making it to the top and making sure you and your family stay that way. Once you get a significant amount of money you can keep making more and more because the amount gives you the power to become an economic imperialist where you just buy out your competitors or sit on your nest egg and sell things below cost until your competitors go out of business and you have the monopoly of ownership once more.

7. **Jon H**
   July 10, 2004

   " I could really become a fat rat and get an MBA. There is no shortage of MBA's that have an actual clue about the technology. But the real question is this. Do I want to surround myself with assholes all day, or be in the technical trenches with my network administration team, discussing routing issues and solving congestion problems? Do I always want to have to rely on a geek like me to stay afloat in front of the board because I am clueless? Do I want to be in charge of a team that has no respect for me, aside from work authority and my witty personality? I am continuing my education in CS. Why? Because Im not a fucking moron that will dumb myself down for a bigger pool or lexus."

   Getting an MBA doesn't necessarily mean you have to work as an MBA.

   There's probably a good case to be made that, after getting a degree in science, getting an MBA would be a good way of making contacts with business people – either your professors, or your classmates.

   Knowing such people would make it easier to start up a company if you have an idea for a technical service or product. You'd do the science, they'd do the business.

   Or, if they know you have an advanced degree in science, they might turn to you if they need such a person as a partner in a new venture they are considering.

   It seems to me that one place where you do often find people with graduate science degrees is among the founders of startups or small companies. Getting an MBA and knowing MBAs are things that would help *you* be the founding partner with the graduate science degree.

   Come to think of it, maybe college science departments should start throwing meet & greet parties or dinners with the business department.

8. **Dennis Kipman**
   July 10, 2004

   Seven years ago I graduated from high school and went into the workforce building PC's. From building PC's at 7.50 an hour, I learned that a good way to make more money would be to learn how to build a network. So I did, reading $24.99 books at Barnes and Noble and taking advantage of any work opportunities (employers that didnt want to pay $$ for a college educated employee). I currently make $85k as a network analyst in IT after jumping a few jobs to increase my salary as I learned more topics over the years. Since my most recent job pays for school + all books, I couldn't pass up going to school and getting educated. Having gone through a BS in CS now, and about to embark on more schooling, I made a choice. I could really become a fat rat and get an MBA. There is no shortage of MBA's that have an actual clue about the technology. But the real question is this. Do I want to surround myself with assholes all day, or be in the technical trenches with my network administration team, discussing routing issues and solving congestion problems? Do I always want to have to rely on a geek like me to stay afloat in front of the board because I am clueless? Do I want to be in charge of a team that has no respect for me, aside from work authority and my witty personality? I am continuing my education in CS. Why? Because Im not a fucking moron that will dumb myself down for a bigger pool or lexus.

9. **D R Lunsford**
   July 9, 2004

   Faraday a janitor? How do these legends get born? Is there an antilegend for every legend?

   Faraday was "Davy's greatest discovery". After Faraday presented him with a bound copy of his lectures on chemistry, Davy hired Faraday as a lab assistant.

   The info is only a google away.

   -drl

10. **Alejandro Rivero**
    July 8, 2004

    Michael Faraday was a janitor at the Royal Institution, wasn't it?

11. **JC**
    July 7, 2004

    Interesting www site about the "real deal" about science type jobs:

    http://scijobs.freeshell.org/

I don't know whether it's just "sour grapes" from the author, but a lot of the info at the site looks informative.

12. **Fabien Besnard**
July 7, 2004

Peter, I can't agree more with you !
In fact, what you have said fits really well with the situation here in France. What surprises me, is that I thought things were better in the States. We always consider the States to be a heaven for research, but it seems to have changed. However, the job market in France is certainly in a much poorer situation than in the USA, and this is the main reason for the huge protest of reaserchers that happened last year (it was the first time in the history of the country). We also have a report saying that there will be a shortage of scientists in the years to come, but this time it is true, because since about ten years, students leak away from science. There have been something around 30% loss of undergraduate students in ten years (the situation is especially critical in maths and physics).

13. **JC**
July 6, 2004

When I was in grad school, I was sort of a night owl type and was literally on a first name basis with the night shift janitors on my floor. Over the years I found out they were making around the same salary as many professors, and working a lot less hours.

One janitor I found out actually went to grad school for math in the 1970's but dropped out without finishing his thesis. He mentioned that it was difficult finding any science or engineering type jobs in the 70's, and he ended up doing various janitorial type jobs ever since. By the time more "jobs" opened up in the 80's, he was already a manager that dealt with overseeing janitorial duties for several large buildings on campus, and was being paid half decently for it. (He also had kids to support, which was a huge factor for him staying in the same job over the years).

A few other folks I knew over the years, who never pursued any post-secondary school education, ended up working at other menial or blue collar jobs, like garbage collection, post office, driving trucks, etc … Most of them are making more than many professors at the local universities nearby, and working a lot less hours.

At times I wonder what kids today are thinking, when word is already out that there's hardly any good paying jobs that require a PhD. In many workplaces, a degree isn't much more than an easy way to "weed out" job applicants. When HR has several hundred or thousands of resumes to sort through, it's easier on the first cut to just delete all the resumes which don't have any degrees or have "too many" degrees, so that they have a "managable" number of resumes to look at. Almost everybody I knew over the years who worked in HR at various corporations of all sizes, mentioned that most of their daily work is to "weed out" as many resumes as possible that are submitted over the internet or (less commonly) by postal mail.

I don't have a reference handy, but there's allegedly already huge declines in folks majoring in computer science and engineering over the last few years at places like MIT. Despite Bill Gates trying to advocate kids into majoring in fields like computer science, kids seem to be able to see through the farce and propaganda and aren't taking the bait so easily.

In the near future, will there be a day when going to college no longer makes any financial sense? At the present time the propaganda advocating college education seems to be things like low interest student loans, along with "dreams" of a better life and career after finishing college. In some cases it's family pressures. Many kids find out the hard way after graduation that their degrees are worth jack shit, and are hardly even worth the paper they're printed on.

At times I wonder if there will ever be another great "self-taught" mathematician and/or physicist that comes out of the blue, like a Ramanujan or Fermat. These days many of the "self-taught" physicists and/or mathematicians seem to be mostly crackpots, instead of serious scientists.

Historically before the 20th century, it seems to be an exception for mathematicians or physicists to be on faculty at a university. It appears that the expansion of university faculty jobs in America came after World War 2, with the GI Bill and Sputnik. With that huge surge in government money in the 1950's, it seems to have made many sciences a viable middle class career for a period of time afterwards.

14. **Steven**
July 6, 2004

Peter, you are spot on there. I got a phd in the 90s (from the UK, I am a Brit) and took up a postdoc in the US at a major university (an Ivy League one actually). This was in theoretical biophysics/biology although my background is in theoretical physics. I went with high ideals and enthusiasm but quickly became disillusioned and deflated to realise that the big postdoc pool there was badly paid and had virtually no status or respect. They were neither students or faculty and existed in a sort of limbo. I was under the illusion that the title "phd" at least carried some (a lot of) respect but in US universities even the secretaries consider themselves a cut above you. Most of them (postdocs) were from China and India, were paid very little, follow orders without question, and would work very long and hard for peanuts (including weekends/Saturdays and holidays). Any of them could easily be replaced. (I believe the Polish word is "Robotnic").

Basically they can have a pool of slave labour doing experiments and laboratory grunt work virtually 24/7. Very little academic freedom although being in theory was somewhat better. The campus (at postdoc level) felt like the intellectual equivalent of a slave plantation, far from the very niave ideal I once had of civilised minds working together to advance science and better humanity. Also NO American postdocs did I ever encounter and very few american graduate students. Most of them seem to have had wised up to the fact that science is no longer a viable middle-class career path.

The Darwinian misconception in all of the sciences at this level is that this system "weeds out" the weakest students/postdocs and that "the best" then survive. What actually happens is that it weeds out the people who don't want to destroy their lives working 60-70 hour weeks worrying about how ions affect protein folding dynamics and so on. It seems to get rid of the people who want balanced lives and other interests outside of science. Who want to go to art galleries, theatre/music concerts, restaurants, spend time with their families and have relationships etc–ie the sort of people who in the end actually make the best scientists!! The ones that make it to permanent positions often turn out to be (although not always) those that can withstand the most mindnumbing drudgery and who work the politics right, and who play safe, take no risks, and don't do anything that can actually advance science.

I published 4 papers in (good)journals but my supervisor did'nt consider it "useful or practical enough" and the funding eventually trickled away. Even if you do good work you can still be at the mercy of your superiors who might not like your style of doing things. Doing good work your superiors don't understand is also asking for trouble. One day I was in a diner, when it was not busy, listening to two waitresses at another table talking and filling in their tax stuff and I realised they had better money, hours and conditions than myself and any of my fellow postdocs! That was it for me really lol. I had had enough.

Later I tracked down some of the guys I graduated with (at degree and phd level) via internet and email to see how they were doing. One really brilliant guy I knew as an undergrad had done a years postdoc in particle theory and had also packed it in and left academia. So I am convinced academic science is losing a lot of good people (if not all the best people). I wish I could paint a brighter picture but I am sure very many other poeple have had virtually the same experience.

15. **JC**
July 6, 2004

Is it true that corporations really want PhDs in the first place? Over the years I got the impression the private sector aren't as interested in PhDs, compared to folks who only have a bachelor degrees. It seems like they will only hire PhDs when they really desperately "have to". In many ways, the overproduction of PhDs is the classic economic case of “supply WITHOUT demand”.

Getting the NSF to do the advocacy work for more PhDs with the government partially funding the programs which create more PhDs, in some ways is the ideal framework for many corporations. It’s an easy way of getting the taxpayers to pay most of the costs in educating more PhDs, while the private sector reaps all the rewards. With a huge flood of PhDs on the labor market, it acts like a damper on the salaries of white collar workers.

The only obvious cynical reason I can think of for corporations and universities advocating the production of more PhDs, is mainly to bring down the pay scales of white collar workers. If you have a huge flood of PhDs and other educated workers, the pay scales will be brought down eventually to the point where PhDs and other educated workers are a “dime a dozen”, or about the same salaries as blue collar workers.

Since slavery is illegal in most richer countries, the best they can do is to turn many skill sets and jobs into a “commodity”. It’s a lot easier to run a business when workers can be easily interchanged and/or replaced like a “commodity”, and where unions have very little to no power. All employers have to do these days is to threaten that they will move their operations overseas to places like India, China, etc … and it will stop most workers from asking too many hard or demanding questions.

Twisted K-theory

July 6, 2004
Categories: Uncategorized

Michael Atiyah and Graeme Segal have a new foundational paper out on twisted K-theory. It doesn't have too many examples or applications, but lays a rigorous foundation for a certain point of view on the subject. Section 5 is the one quantum field theorists should pay attention to, it explains the relation to the fermionic Fock space. For a more explicit construction relating QFT to twisted K-theory, besides the papers of Freed, Hopkins and Teleman, one can look at "Gerbes, (twisted) K-theory, and the supersymmetric WZW model" by Jouko Mickelsson.

Comments

1. **Urs Schreiber**
   July 15, 2004

   Hi again –

   many thanks for your reply!

   I should have made myself clearer. I think my question concerned a slightly different issue. I'd be grateful if you had a look at this refined question and drop me a note on what you think about it.

2. **Urs Schreiber**
   July 15, 2004

   Hi Peter –

   since you are an expert on loop space: Could you help me with the question that I ask here?

3. **Urs Schreiber**
   July 15, 2004

   > I was referring to my old problem of putting fermions on the lattice.There Kahler-Dirac fermions are basically Kogut-Susskind fermions, and disentangling a single spinor (an ideal in the Clifford algebra) is tricky.

   Ah, you are thinking of Dirac-Kähler on the lattice. This has problems even for a flat 'metric'. I have once thought about this a little together with Eric Forgy, when writing section 5.2 of our notes math-ph/0407005 on discrete differential geometry (where we used the loop space deformation techniques that describe superstring backgrounds and applied them to the point particle limit). Maybe Dirac-Hestenes spinors are the best choice for the lattice.

   What happens when lattice QFT is done with something that is not exactly a spinor in a single ideal? For small lattice spacing the undesireable mixing should disappear. Wouldn't that be sufficient? Or does a finite mixing survive the continuum limit?

   > Another way of getting spinors from forms is by picking a complex structure. Then spinors are (up to a twist by a square root of the top dim form) just the holomorphic exterior algebra, and this is preserved by the Levi-Civita connection when your metric is Kahler.

   Yes, when the metric is Kähler. Alternatively, if spinors are already modeled in left ideals on the exterior bundle, then a Kähler background allows one further split of degrees of freedom and admits to increase N=1 susy to N=2. When twisting one of these two susys one obtains the topological string and all kinds of major wonders begin to happen.

4. **Peter**
   July 14, 2004

   When I was quoting Witten I was referring to my old problem of putting fermions on the lattice.There Kahler-Dirac fermions are basically Kogut-Susskind fermions, and disentangling a single spinor (an ideal in the Clifford algebra) is tricky.

   In your case presumably what you say works. Another way of getting spinors from forms is by picking a complex structure. Then spinors are (up to a twist by a square root of the top dim form) just the holomorphic exterior algebra, and this is preserved by the Levi-Civita connection when your metric is Kahler.

5. **Urs Schreiber**
   July 14, 2004

   Hi Peter –

   you wrote:

   > As you might guess, I'm interested in this model as a 1+1 d QFT, not a string theory.

   That's fine with me. If one wants, for instance, to think of the WZW model as *not* describing a string in a group manifold, but as describing a group valued field on some low-dimensional spacetime, that doesn't change any of the results, of course.

   > I remember Witten pointing out to me "Yeah, but what you want are spinors, not the exterior algebra", which is the heart of the problem.

   Hm, now I am surprised. Of course it is true that we are interested in spinors and not in differential forms. But…

   The point is that the supercharges on the worldsheet simply *are* Dirac-Kähler operators. We are not choosing them to be such.

   So there is a puzzle, and it does have a resolution:

   The puzzle is: How can it be that the string has spinors in its spectrum, when the supercharge is really the Dirac-Kähler operator?

   As was noted already by Kähler himself, his equation models spinors on flat spacetime, but no longer on a curved spacetime. That's because the connection on the exterior bundle does not respect the spinor ideals, but mixes them. (I have once tried to give a hands-on physicist-like way to see how this works here.)

So how can it be that superstrings have spinors in their spectrum even on curved spacetime, if, as I claim, their supercharges are Dirac-Kähler operators?

Well, the reason is, for superstrings there are conditioons on the background fields. You cannot just arbitrarily curve the background. Something has to provide the energy-momentum density for this curvature and the background equations of motion have to be satisfied. These extra fields will enter into the Dirac-Kähler operator as additional connection-like terms, and these terms will *cancel* the problkematic mixing effect.

We know this has to be true in principle (it is essentially nothing but the chiral splitting on the worldsheet) but it is probably instructive to see how it works in specific examples. The best example I know is just the super WZW model. Here we have target space curvature (target space is a Lie group manifold with the standard Killing metric). But there is also a 2-form field on target space. This 2-form field induces a torsion term in the Dirac-Kähler operator. This is precisely the *parallelizing torsion* on the group manifold. But this means that it exactly *cancels* the Levi-Civita connection in the inavriant orthonormal basis. This way the mixing effect is removed and we have true spinors described by the WZW Dirac-Kähler operator! 🙂

I discuss this and all the details in section 2.1.4 of hep-th/0311064. (Unfortunately this paper is not very readably written, I have to admit.)

So the Dirac-Kähler operator is indeed at the heart of it, but there is no problem! 🙂

6. **Peter**
July 14, 2004

Yes, this is the worldsheet Hamiltonian. As you might guess, I'm interested in this model as a 1+1 d QFT, not a string theory.

The Dirac-Kahler stuff is very interesting, especially in its relation to TQFT, as in Witten's original Morse theory paper. I actually first got interested in this way back in grad school when I was thinking about fermions on lattices and Dirac-Kahler. I remember Witten pointing out to me "Yeah, but what you want are spinors, not the exterior algebra", which is the heart of the problem.

7. **Urs Schreiber**
July 14, 2004

Hi Peter –

I think that unless something weird happens looking at one chirality is fully sufficient, since both chirality sectors are decoupled and identical.

I would like to know what precisely you mean by the Hamiltonian picture. Probably and hopefully this refers to the worldsheet Hamiltonian, so that you are perhaps thinking of looking at the WZW QFT in the Schrödinger picture (operators do not depend on worldsheet time) instead of the usual Heisenberg picture of CFT (where they do)? That would be nice, because this is exactly the point of view which leads from superstrings to differential geometry on loop space.

Namely loop space is of course the configuration space of the string, which only makes an autonomous appearance in the Schrödinger picture. In other words, loop space is the *midi-superspace* (super in the sense of Wheeler, not in the sense of supersymmetry) of the worldsheet gravity theory.

My original motivation to look into strings at all was that I had found that all kinds of supersymmetric field theories, and in particular N=1, D=3+1 SUGRA, look like Dirac-Kähler systems (i.e. systems governed by (d + del)) when formulated in Schrödinger picture on their configuration space.

Since it is hard to make progress with this insight in 3+1 dimensional gravity, I applied it to 1+1 dimensional gravity and arrived at the string. Turns out that the Dirac-Kähler point of view here works wonders, in my opinion.

But it is also an unfamiliar point of view for physicists. It had been hinted at by Witten in his 'Morse theory' paper, but that's about it. I am hoping to convice some people that it is a fruitful point of view.

The comments you made suggest that this might in fact be easier in more mathematically inclined circles, where the notion of differential geometry on loop space is something that people can associate interesting results with.

So if you find the time to sketch some ideas by Mickelsson and others, you are guaranteed to have at least one interested reader.

8. **Peter**
July 14, 2004

I've always been somewhat confused by the relation between what Mickelsson does and supersymmetric WZW. For one thing, he's in a Hamiltonian picture, and only appears to be looking at one chirality.

At some point in the next few weeks I plan to get back to working on this and maybe the references you give will help me sort it out.

9. **Urs Schreiber**
July 14, 2004

So what is it Mickelsson is doing with the supersymmetric WZW model?

In the intro it says (and more I haven't read so far)

> We also discuss the construction of twisted K-theory classes by families of supercharges for the supersymmetric Wess-Zumino-Witten model.

The generic supercharges of the WZW model are just the left- and rightmoving worldsheet supercharges, which are (Kähler-)Dirac(-Ramond) operators on the loop group with respect to a connection which is Levi-Civita plus/minus the parallelizing torsion (as discussed for instance in hep-th/9310187 and hep-th/0311064). For certain groups there might be more supercharges (like when the group is complex Kähler – is that possible?).

So are these twisted K-theory classes constructed from Dirac operators, in general? Or how else are they related to supercharges?

Slate Article

July 7, 2004
Categories: Uncategorized

There a new article on Slate about string theory and my colleague Brian Greene. Also some commentary about it on David Appell's weblog Quark Soup.

Comments

1. **D R Lunsford**
   July 9, 2004

   Urs, your comment is that of the apologist.

   It's perfectly possible to give non-equation explanations that hang together of real physics. It can't be done for strings because there is nothing to really describe.

2. **Urs Schreiber**
   July 9, 2004

   Semiclassical elegance

   Popular descriptions of sciences which involve lots of math are always problematic. An infinity of analogies does not capture the content of an equation. It's indeed very much like talking about Beethoven's music without ever listening to it. You will never get the idea, no matter how many words you read about it.

   One thing is this common statement that 'all particles would be vibration modes of string'. While not fully wrong, this completely misses the point that the interesting thing about the superstring's spectrum is its *massless* part, where there is essentially no vibration at all!

   There is however sort of a 'supervibration' since the fermionic worldhseet modes are excited. The nature of the massless spectrum is in fact much better understood in terms of representation theory, very much as for point particles.

   I once tried to give a semi-popular description of how the superstring's spectrum really arises, trying to improve on the 'particles are violin string notes'-statement.

   If one is really interested in the pedagogy of string theory, i.e. in finding better ways to get this abstract formal entity into semi-laymen's brains, then I would rather recommend for instance the 'Geometric Algebra' approach, as a neat notational framework to fill all kinds of spinor constructions with life. For instance I claim that the world sheet spin fields that generate the Ramond-sector states of string, and hence the target space fermions, are nothing but slightly souped up 'rotors' that they are so very fond of in Hestenes' school of thinking about spin geometry.

   (As you all know and appreciate, every good idea is part of string theory – and rotors are a good idea. 😉

   By the way, in the slate article that Peter mentioned it says:

   > Greene plays fast and loose with terms like beauty and elegance, using them in a semiclassical, semiromantic sense

   Heh, I like that: 'Semiclassical elegance'! 🙂 If we could just fully quantize elegance Brian Greene might after all be able to prove that string theory is quantum-elegant. There might be quantum corrections to that, but I suspect most of the string's elegance is protected by supersymmetry or is dual to its romaticism.

Witten in Crete

July 8, 2004
Categories: Uncategorized

Witten is lecturing at a conference in Crete this week and some of his transparencies are already online. He is talking about perturbative gauge theory amplitudes and the idea of interpreting them in terms of strings in twistor space. He motivates this by noting that AdS/CFT is useful for understanding gauge theories at large g^2N, but at short distances asymptotic freedom implies g^2N is small and to understand gauge theory in terms of strings you need to do so for all g^2N. He warns "I can't promise that what I'll explain will turn out ot be useful in a string description of QCD, but at least I'll tell you interesting things about perturbative gauge theory!".

For something completely different, the latest on the Landscape is that, at least this week it predicts low energy supersymmetry, maybe.

Comments

1. **Urs Schreiber**
   July 14, 2004

   Hi Peter –

   yes, that's what I am talking about – in the abelian case. In the nonabelian case things are not as simple. There it does not make sense to simply intergrate over S^1, since that amounts to comparing elements in different fibers. So in the nonabelian case some parallel transport using a 1-form has to be there in order to relate the values of the 2-form at different points of the loop.

   Something along these lines was what Christiaan Hofman originally guessed, or proposed. I think the crucial point missing in Hofman's proposal is that there is parallel trasport back and forth, as in the equation at the very bottom of this MathML enabled entry. This also explains why you don't see the parallel transport in the abelian case.

2. **Peter**
   July 14, 2004

   Here's the way I know to construct a one-form on loop space from a 2-form on the space itself. Is your construction the same?

   There's a tautological gadget called the evaluation map

   ev: S^1 x Maps(S^1,M) \rightarrow M

   that just takes a point on the circle parametrizing the loop to the corresponding point on M given by the loop.

   Given an n-form on M, pull-back to S^1 x Maps(S^1,M) and integrate over S^1 to get an n-1 form on Maps(S^1,M)

3. **Urs Schreiber**
   July 14, 2004

   > what sort of geometry is a "connection 2-form" supposed to represent?

   Ah, I see. That's due to sloppy terminology on my part.

   The point is that a 2-form on target space lifts to a 1-form on loop space. What I derive in that paper is that a deformation of the worldsheet supercharges by something which should describe strings in nonabelian 2-form backgrounds indeed does produce a connection 1-form on loop space which is this 2-form lifted to loop space.

   This is – almost – what had been expected before, in hep-th/0207017 (see top of p. 7). But I derive a little correction to the formula given there and show that this correction is necessary for some crucial properties.

   An easy heuristic way to see why target-space 2-forms correspond to loop space 1-forms is the following physical picture:

   A point particle is charged under a 1-form and the 1-form is integrated over the worldline. A string is charged under a 2-form and the 2-form is integrated over the worldsheet. If you make an ADM split on the worldsheet, i.e. introduce a slicing of the 'tube' into 'circles', then you can see how 'each point' on these circles is similar to a point particle which is charged under the 1-form obtained by contracting B with the tangent to the circle.

   Now sum this up for all points in the 'circle' and you get the 1-form connection on loop space.

   At least this is the simple picture for the abelian case. In the nonabelian case you cannot just sum up the contributions from the different points, since you cannot compare elements of the bundle at different fibers. This is why Hofman expected the target space 1-form to play the role of parallel transporting the nonabelian 2-form values from each point of the string slice to some (arbitrary) origin on the circle. And indeed, this is what drops out from my deformation, which also demonstrates that there has to be parallel transport back and forth.

   In writing this, I am thinking of superstrings. Most of the work on higher gauge theory has been in broader contexts, mostly coming from a field theoretic or a purely mathematical point of view. But in any case a 2-form YM theory must somehow be about 'line particles', and then the above reasoning is relevant.

   Probably loop people like Baez, Girelli and Pfeiffer are thinking of parallel transport of spin network edges, instead. BTW, I think the text

   Girelli & Pfeiffer: Higher gauge theory – differential forms versus integral formulation (2004)

   is the most important one on 2-form gauge theory that I have seen. It clarifies an important open issue in John Baez' paper and very nicely elucidates the geometrical visualization. But it also seems to leave the authors puzzled: Namely they derive that the whole 2-form gauge theory businenss can only be consistent when the 2-form equals minus the field strength of the 1-form. This seems to drastically constrain the number of interesting higher gauge theory Lagrangians that one can write down.

   But from the string/loop space perpective this condition, as I show, is natural. It is equivalent to the 1-form connection on loop space to be flat, which again is the condition that closed strings don't couple to the nonabelian 2-form, and they should not, since they cannot carry Chan-Paton factors.

   I wasn't aware of Girreli & Pfeiffer until after I had derived this condition myself, but I think that my derivation still helps understanding 2-form gauge theory.

I'll look up the literature that you mentioned. Many thanks!

4. **Peter**
   July 13, 2004

   Hi Urs,

   I looked a little bit at your "higher gauge theory" stuff, but I'm afraid my initial reaction is that of a mathematician. What's the underlying geometrical gadget that this is supposed to be? A connection is an equivariant way of relating nearby fibers in a fiber bundle, and curvature is the holonomy about an infinitesimal loop in a bundle. I can make these ideas explicit using connection 1-forms and curvature 2-forms, but what sort of geometry is a "connection 2-form" supposed to represent?

   The kind of thing I'm doing with Dirac operators is essentially what is in a recent Freed-Hopkins-Teleman paper, the closest point of contact with string theory is probably some work of Greg Landweber's which is basically about N=2 superconformal coset models, see math.RT/0005057 on the arXiv.

5. **Urs Schreiber**
   July 13, 2004

   Peter wrote:

   > the Dirac operator on certain loop spaces is also a crucial part of what I've been thinking about

   Oh, interesting. I didn't know that you have been thinking along these lines. Maybe we can discuss some stuff.

   As you may have seen, I currently think that loop space differential geometry has something to say about what is called 'higher' gauge theory. The point is that many people have tried to more or less guess or use trial and error to find general properties of 2-form generalizations of Yang-Mills.

   In a kind of reversed fashion I tried to study 2-form Yang-Mills from the worldsheet point of view, seeing how the worldsheet theory determines the target space effective field theory.

   After some steps in the dark I think that I now understand what's going on. As soon as my draft passes my advisor's revision I'll put it on the arXiv. Since I have learned in the past that there is nothing like shameless self-advertisement, here is the link:

   Nonabelian 2-form connections from 2d BSCFT deformations

   I have had discussion about this with all kinds of people by now, but I am always happy to receive more comments.

   🙂

6. **Urs Schreiber**
   July 13, 2004

   I don't know much about Dixon's work, but he gave a talk in Paris about how to use N=4 SYM to compute QCD amplitudes and how that leads to connections with all kinds of computations done in AdS/CFT.

7. **Peter**
   July 13, 2004

   Hi JC,

   Weird but true: I just checked the SPIRES HEPNames database trying to remember who Dixon's advisor was (Dixon and I overlapped as grad students for several years at Princeton). The example SPIRES gives for a search of this database is

   find name dixon and field hep-ph and not undergrad mit

   which leads precisely to Dixon's entry.

   Back when Dixon got his Ph.D.(1986), there wasn't a whole lot of choice in the matter, you pretty much absolutely had to be working on string theory if you wanted to get a job. I'd also be curious to know why he stopped working on string theory and what he thinks of it these days.

8. **JC**
   July 13, 2004

   Peter,

   I remember in early 90's when string theory was in sort of a slump. From what I remember, all that work on conformal field theories and string field theory didn't seem to make much new progress, while the mirror symmetry stuff eventually worked its course and subsequently "flatlined" shortly thereafter. (It was years before Seiberg-Witten theory and D-branes revived string theory).

   It seems like even back then, many string folks were finding many rationalizations and excuses to justify working on string theory. I remember some folks were even looking at "string inspired" stuff like Lance Dixon's work in the early 90's that found easier ways to calculate Yang-Mills amplitudes, as an "excuse" to justify doing string theory. (It's interesting that Witten's present work on twistors is attempting to explain the simple looking formulas that showed up in the papers of Dixon, Kosower, et al. from that era). I always wondered what made Dixon defect from string theory back in those days, considering many of his big papers from the 80's were all on string theory.

   It seems like the sociology behind string theory is very much like being sold the "dream" of the "holy grail" of a consistent theory of quantum gravity,

   In the 1970's, the sociological circumstances in the mid 70's which led to the mass abdication of string theory seemed to be 't Hooft's renormalization results and the subsequent "rebirth" of field theory. In this case, what led to string theory's first demise is pretty much how traditional science should work, when a better theory makes predictions that are subsequently confirmed by experimental results. Perhaps why there hasn't been a mass abdication yet in today's string theory, maybe has to do with the fact that the "dream" of the "holy grail" in a consistent theory of quantum gravity, is still very much alive and well in the spirits and minds of many string theory folks.

   For many "mass movements" in history, whether benevolent or malevolent, there's almost always a "dream" and/or "utopia" of some sort that is used to sustain the movement which keeps the "true believers" in line, as well as a way to get new recruits to their cause. Even once the "mass movement" becomes the "status quo", the "dream" and/or "utopia" is still repeated over and over as propaganda. (This is what happened in many communist countries, like Soviet Russia and China, after awhile).

The systems created by the "mass movements" usually start to crumble and eventually self-destruct when the propaganda of the "dream" and/or "utopia" can no longer sustain the spirits and minds of the people.

The only obvious scenario I can think of that could greatly destroy the "dream" of the "holy grail" of a consistent quantum gravity theory in the form of string theory, would be if Witten publicly abdicates and drops string theory for good.

9. **Peter**
July 13, 2004

Hi Urs,

Sure, one could attack any kind of speculative work as "wishful thinking", but I don't think I'm doing that. In 1984 when people were hopeful that their new speculation was about to lead to some real predictions, that was one thing. Twenty years later, after the accumulation of a lot of evidence that this idea doesn't work, that's something else.

My comment about sociology was meant to refer more to the question of why the more senior leaders of this field haven't given up and moved on to something else. The question of what a young theorist should do and what the sociological pressures are for them is a bit different. String theory is by now such a huge subject that there are plenty of things for someone starting out to try and do something with, and they're not the ones who should be expected to take leadership and change the direction of the whole field. Whether LQG or string theory or whatever, it's very hard to start a career in this business, and no one does it because it is the easy thing to do.

The fact that string theory includes so many different approaches allows you to find one that is at least mathematically interesting and may lead somewhere. While I'm not doing string theory, the Dirac operator on certain loop spaces is also a crucial part of what I've been thinking about, so if string theory leads you there, that's great.

10. **Urs Schreiber**
July 13, 2004

Hi Sean Carroll –

since this blog does not support threaded comments my reply to Peter Woit's comment appeared as a reply to your comment.

Yes, I fully agree with what you say. 'Fortunate' and 'unfortunate' are inappropriate adjectives in this context, anyway, their only purpose being in casual conversation. There are facts and there is right and wrong, and we need to figure it out.

P.S. BTW, thanks for all the stuff that you make available online. I have learned GR from your notes (well, and some other reading, too, of course) and have tutored a GR class using them. Really nice. But I guess you have heard that before… 🙂

11. **Urs Schreiber**
July 13, 2004

Sure, it's wishful thinking. No matter which person starts to think about any physics beyond the standard model, he or she will tend to wish that there is a chance that this thinking actually leads in the correct direction. No matter which approach you favor over string theory, if its about beyond-the-standard-model then it is necessarily wishful thinking.

BTW, I am a good counterexample for the 'sociological reasons to go into string theory'. I found that it is pretty hard for someone like me to get 'into' the community. Experiences from talking to people indicated that it would have been much easier for me, as far as sociology is involved, to do some LQG, for instance.

Trust me or not, but I do find strings more interesting as such, and here is absolutely nobody in my vicinity who could have tried to talk me into believing that.

Maybe a curious side remark: One reason why I like strings (by no means the only or most important one, but still one reason) is due to the major role the Dirac operator plays in that theory.

But I didn't want to get into that kind of discussion again. Sorry. From now on only technical remarks/questions. Promised! 🙂

12. **Sean**
July 13, 2004

Urs, by "unfortunately for string theorists" (referring to the observed positive vacuum energy), I presume you mean "fortunately for string theorists." The AdS vacua we understand are all supersymmetric and not like the real world. It's hard to understand supersymmetry breaking, and it's hard to understand a positive vacuum energy, and it's hard to understand moduli stabilization. The hope has to be that these difficulties are somehow related to each other, which would help explain why easy-to-understand solutions to string theory don't look like the real world. I think that's a perfectly plausible scenario — no reason why a correct theory has to be easy for us mere mortals to immediately master, especially in the absence of detailed experimental input.

13. **Peter**
July 13, 2004

Hi Urs,

Thanks a lot for the detailed summary of the current point of view on the "landscape". I hope that helps Pyracantha, but if not here's a much over-simplified version:

String/M theory supposedly is a theory of strings and maybe other objects in an 11-dimensional space-time. We see 4 of the dimensions (3 space, 1 time), what about the other 7? The initial hope was that there would be a small number of possible consistent choices of these 7 dimensions, so there would be a small number of calculations you could do and see if one of them agreed with experiment.

Lately people have started to believe that there are an astronomically large number of possible consistent choices, and these are referred to as the "landscape". The reason for this terminology is that each such choice comes with an important number attached, the energy of the vacuum, and if one imagined mapping out all possible choices on a plane, one could imagine making a topographical map, with the energy the altitude. Zero energy choices would be at sea-level, and the whole thing would presumably have peaks and valleys, with our universe sitting at the bottom of some valley.

Anyway, that's very roughly the idea.

The standard ideology has always been that this large number of choices is just due to the fact that one only knows an approximation to the real string/M-theory, and that if one knew the real thing one would find that all or most of these choices were inconsistent. The other part of this ideology is that there is a unique real string/M-theory, for which all these choices are just possible lowest energy states. In this scenario, maybe they are only approximately at lowest energy and the true lowest energy state is something else, or maybe there really are an extremely large number of possible lowest energy states (this may include metastable states, not at lowest energy, but separated by an energy barrier from lower energy states).

My own point of view is that this standard ideology is just wishful thinking. My guess is that there isn't a simple unique 11-dimensional theory, but something rather complicated, involving a possibly infinite number of choices as to how to set it up. People are free to keep believing the standard ideology, since it's hard to prove a negative, to show that what they would like to exist doesn't.

The really strange thing that has happened in recent years is that a lot of string theorists, most prominently Susskind, have adopted the point of view that, whatever string theory is, it has an astronomically large "landscape" of equally good vacuum states, and thus equally good models of the universe. I would have thought that once someone had convinced themselves that, even if there was a unique real string theory, it would be consistent with an unimaginably large number of possible models of the universe and quite possibly be completely vacuous and unable to predict anything, they would give up on the whole idea. The idea Urs mentions, that maybe only a small number of these models is consistent with some simple facts one knows about the standard model, so you could use these to make predictions about other things, seems to me to be just more wishful thinking.

Given that you don't know the underlying theory, and what you do know leads to an essentially infinite number of possibilities, it's not clear that the kinds of arguments that Susskind et. al. are making are even science at all. Some of these papers are weird documents, with virtually no equations, just a lot of hand-waving arguments involving massive amounts of wishful thinking and no solid conclusions. My take on all this is that the time is long past at which a reasonable person should have given up on the whole idea and moved on to something more promising, but sociological reasons are keeping this from happening.

14. **Urs Schreiber**
July 13, 2004

Hi Pyracantha –

in so-called perturbative quantum theories one chooses a solutiuon of the classical equations of motion, the so-called 'background' and then studies quantum corrections to that background order by order.

For instance in ordinary quantum field theory the background might be flat Minkwoski spacetime and in that background we can imagine photons and electrons to propagate and interact in Feynman-diagram fashion. The 'vacuum' background together with all these particle whizzing around would then be a full perturbative state of the theory.

(One problem is that not all aspects of the full quantum theory are captured by such a perturbative procedure.)

Now, in string theory the idea is pretty much the same, only that here the particles are not pointlike but a have a small linear extension. This seemingly simple modification has drastic consequences. While in field theory there are many possible choices of fundamental particles, their interactions, and choices of background, the consistency of string interaction very much constrains all three of these. The big open question is: How much exactly?

When people talk about the 'string theory landscape' they are thinking of the abstract space in which each point is one consistent perturbative string theory background, i.e. one consistent choice of particle content, particle interaction and classical spacetime that they propagate in. In principle the number and position of points in this 'theory space' is determined by the background equations of motion of string theory (or equivalently, if you want to hear the technical terms, by the requirement that there is a supercfonformal field theory with central charge 15 on the worldsheet of the string).

There has been some recent progress in better understanding this space – but it is still immensely ill understood. Still, the progress that has been made has appeared significant enough to some people to base some more far reaching speculation on it. That's because a good understanding of which background solutions string theory admits is the key to be able to apply string theory to pheonomenological considerations. When a string theory background is found which is consistent with the observed particles of nature, then studying the stringy quantum corrections to it would allow to deduce what this background predicts as corrections to the currently known physics.

Peter Woit here has pointed out repeatedly that some of the speculations concerning the landscape that have been published are not at all based on results that have really been calculated.

On the other hand, the mere fact that a discussion of such a 'theory landscape' is possible (even though not easy) is important. It is not possible in field theory of point particles. There we also have some restrictions on the Lagrangians (i.e. the particle content and interaction) that we are allowed to consider as a consistent field theory, but they are far less severe than those found in string theory.

As has been pointed out very nicely by Jacques Distler in his weblog, the points in the landscape which are consistent with the experiments that we have made are probably very rare. In any case, none has been found so far. If there is none at all, then string theory is wrong as a theory of nature. If there is a single such point, then string theory, based on the currently known data, could make predictions about for instance new particles that could be found in future colliders. (These predictions could still be disporved by experiments, of course.). If however there are very many such points then predictions for new particles etc. would be very difficult. One might, in this case, still try to make some *statistical* predictions. Such statistics about properties of the 'landscape' are currently what some people are trying to do. But it seems fair to say that this is, while an intersting idea, quite premature.

Finally, there is the theoretical possibility that the world we live in cannot be understood as a small perturbation of some background. The success of perturbative field theory suggests otherwise, but nobody can know this for sure. So one possibility is that none of the points in the 'landscape' correspond to the world we live in, but some nonperturbative description of string theory is necessary to describe our world. Nonperturbative description of string theory tend to be described not by full classical backgrounds, but by *asymptotical* backgrounds, this means roughly that at spatial infinity the background is fixed, while 'in between' physics is described fully nonperturbatively. Nonperturbative discriptions of string theory are known for instance for universes which asymptotically have the geometry of what is called 'anti-deSitter Space'. This is, roughly, the shape of a universe with a negative cosmological constant.

Now, unfortunately for string theorists, recent very exciting measurements of various cosmological parameters have shown that instead we observe a cosmological constant which is *positive*. This means that the particular anti-deSitter non-perturbative deswcription of string theory appears not to be applicable to describe the universe that we live in.

This is probably the main reason for the current excitement about landscape discussions. Namely people are trying to find out if in the landscape admits universes which only temporarily have a positive cosmological constant, while *asymptotically* this constant goes negative. If this were the case then there would still be hope that the nonperturbative string theory description which involves asymptotically anti-deSitter space could be used to describe the world we observe.

So that's what all this landscape talk is about. Unfortunately, since there is so little known for sure about the 'landscape' (even though the landscape is a well defined mathematical object (space of all superconformal 2d theories with c=15) which can in principle be understood exactly), some of the discussion concerned with it recently has tended to be more philosophical than scientific.

15. **Pyracantha**
July 13, 2004

Pyracantha from "Electron Blue" here, the artist who is trying to learn math/physics in middle age. I read your site in the hope that someday I'll understand what you and your colleagues are talking about. But I have heard one phrase many times and it intrigues me. What is the "landscape?" Could you explain it in terms that a beginner like me could understand?

16. **Dick Thompson**
July 12, 2004

AR, Here are my notes on the article "Lovely as a Tree Amplitude" by Stephen K. Blau, in the Search and Discovery section of the July 2004 Physics Today. I urge you to read the original article whenever you can, it is well-written and has a lot more detail.

It reports on a three month long workshop on collider physics at the Kavli Institute in Santa Barbara. The workshop centered around the work of Witten and colleagues on a duality between string theory and perturbative QCD. Last December, Witten discovered this relationship between a certain string theory (B-theory) and the weak coupling domain of QCD [hep-th/0312171]. Witten, F. Cochazo, and P. Svrcek (CSW) used it to greatly simplify the expression for the leading perturbation terms in some QCD diagrams [hep-th0403047] .

In the covariant expression for an all-gluon Feynman diagram each gluon helicity is described by an overdetermined 4-vector. The extra symmetry doesn't affect the physics, but it makes the expression complicated. Following an alternative strategy of collecting particles by helicity, S. Park and T. Taylor in 1982 proposed a formula for maximal helicity violating (MHV) diagrams. The formula was proved later; it only handles massless particles. CSW show how to apply to off-shell MHV diagrams sewn together with propagators to to yield non-MHV amplitudes. The CSW construction has not been proved, but Zvi Bern at the workshop called it convincing. It satisfies certain powerful factorization properties.

Now, the twistors. A vector can be represented by two spinors. Witten considers tree-level amplitudes in spinor language, and asks how they behave under conformal transformations. To simplify expressions and symmetries, he fourier transforms into twistor space. Then when the momenta of nonvanishing MHV amplitudes are expressed in twistor space they lie in a straight line. Other helicity combinations lie on quadratics and cubics. A feature of the duality is the instanton string configurations reproduce QCD amplitudes and they wrap around structures in twistor space, giving a winding number.

R. Roiban, M. Spradin, and A. Volovich used the Witten duality conjecture to get 5-gluon amplitudes with two + helicities and three – helicities. [hep-th/0402016]

17. **Alejandro Rivero**
July 12, 2004

Lunsford, no, I was not thinking on Regge methods. I am not fluent enough on the trajectories, so I can no bet about how deep this methodology was. Was it related to the primitive string theory?

I mentioned *Runge* methods because Brouder pointed out that the systematics to classify them was the same than Connes-Kreimer for Feymann loops.

18. **Dick Thompson**
July 11, 2004

AR, about the article. I read it at the public library. I'll be going back there tomorrow (Monday July 12) and I'll take some notes.

19. **D R Lunsford**
July 9, 2004

AR, you mean Regge methods of course.

20. **Alejandro Rivero**
July 9, 2004

No surprise to know that there is hidden structure in Feynman diagrams. From Bogoliugov to Kreimer there has been deep insights on it (not to mention the strange connection with Runge Kutta methods, found by Brouder). But I am unsure about if the string framework is the most adequate to look into this.

A pity the Physics Today article is not open for general public. Can you tell more about?

21. **Dick Thompson**
July 8, 2004

There's a good article on this in the current Physics Today. Witten and coworkers can only do tree level, but others working in their wake have got plausible results for loops. The idea is to do better calculations on low energy (several TeV) soft scattering. When Witten plotted the maximum helicity violating states in twistor space, they fell on a straight line.

22. **Alejandro Rivero**
July 8, 2004

The icon in the upper left corner of the webpage is Phaistos disk, an undeciphered inscription that rivals string theory as a magnet for speculation.

Slides 1, 2, and 6 show that String theory is not more claiming to be a theory of elementary entities but a yet-weakly-foundamented area of mathematics. I could have told this to you already nine years ago. It is very much as the things topologists do to study spaces: To swept a manifold with a lower dimensional one, and to look for singularities in the evolution of the map.

This does not imply that they have lost hope on an elementary entities theory, as one can read between lines. But it is an aptitude change, and it goes back to take seriously Quantum Field Theory, no more a disposable effective theory.

23. **Thomas Larsson**
July 8, 2004

I am confused about the status of twistor-string theory. Berkovits and Witten seem to claim that the twistor string does not give you the right answer beyond tree level: "Those loop amplitudes will therefore not coincide with the loop amplitudes of pure super Yang-Mills theory. " Still, Witten keeps working on it. Does anyone understand what the goal is?

New Books

July 13, 2004
Categories: Uncategorized

Two new books from Cambridge that are now available:

A First Course in String Theory by Barton Zwiebach, based on a course on string theory for undergraduates taught at MIT. It's available for \$42 at Barnes and Noble, sales rank 565, for \$60 at Amazon, sales rank 13,559. The whole idea of trying to teach a very speculative theory that hasn't really worked and which is based on 2d quantum field theory to undergraduates seems to me to be utter lunacy. But maybe I'll even buy a copy.

Topology , Geometry and Quantum Field Theory, the proceedings of a symposium that I went to at Oxford in 2002 in honor of Graeme Segal's 60th birthday. This conference had some wonderful talks and I'm looking forward to reading many of the contributions. Supposedly it also contains Segal's manuscript "The Definition of Conformal Field Theory", which has been circulating in samizdat for years. My copy (which like many others contains the hand-written notation "Do Not Copy" on the front) is falling apart, yet another reason why I just ordered the book, even though it is \$90. The story I heard is that Segal didn't want his manuscript reproduced, but finally agreed on the condition that it not be re-typeset, but appear exactly as in the original, so that it would be clear that it was still something preliminary and tentative, with no corrections or improvements made since he wrote it.

Comments

1. **Peter**
   July 14, 2004

   Not anywhere I know of. I think what is on-line is what Segal had at the time (99), but don't know whether he has continued working on writing up these notes or how much progress he has made if so.

2. **D R Lunsford**
   July 14, 2004

   Thanks, I wonder where is lecture 4 (mainly CFT)?

3. **Peter**
   July 13, 2004

   Some of Segal's notes are on-line at

   http://www.cgtp.duke.edu/ITP99/segal

   His book "Loop Groups", is about the most amazing math book I know. Worth every penny. His conformal field theory notes are in some sense an extension of this book, although presumably he was never that happy with them, partly probably because soon afterwards Witten came out with his Chern-Simons stuff, which provides yet another perspective on CFT.

4. **D R Lunsford**
   July 13, 2004

   So where can we learn about Segal's real deal online?

Talks From Strings 2004

July 18, 2004
Categories: Strings 2XXX

Transparencies from the talks at Strings 2004 in Paris are starting to appear on-line. You can see a listing of what is available so far here. None of the ones I'd be most interested in seeing (Dijkgraaf, Nekrasov, Moore, Witten) have appeared yet.

On the hot topic of whether or not the landscape picture of string theory can predict whether supersymmetry will be seen at LHC energies, Douglas first gives an argument that there will be low-energy supersymmetry, then another that there won't. Recall that he kept adding and subtracting from his arXiv paper a sentence saying he thought there would be a solid argument by the time the LHC was operating in 2008. In Paris, he puts it this way:

"I *start to think* that *fairly* convincing predictions *could* come out of this approach in the next few years."

which contains enough qualifiers to cover any eventuality.

Of the talks for the public, Veneziano's was pretty much historical, with a couple comments about cosmology, Maldacena's was mostly about black holes, and John Schwarz's was remarkable mainly in that it completely ignored any developments of the last ten years. None of these three breathed a word about the landscape.

Comments

1. **serenus zeitblom**
   July 18, 2004

   I thought that Maldacena's public talk was excellent. He even drew the black hole singularity correctly, that is, as being spacelike, not a common thing in popularizations. Indeed, it is something beyond the comprehension of a lot of professional physicists, who *still* persist in drawing diagrams of black hole interiors which reveal the embarrassing fact that they think that the singularity is a point in space which has a timelike "worldline". With a heroic effort I resist the temptation to name names….the other nice thing about Maldacena's talk was that it was about something of genuine interest, which alas cannot be said about most of the talks in the conference proper. It is sad for example that there were almost no talks about cosmology, where all the action is nowadays….

Segal Conference Proceedings

July 18, 2004
Categories: Uncategorized

My copy of the proceedings of the conference in honor of Graeme Segal's 60th birthday finally arrived and I've been spending some enjoyable time reading parts of it. To me, the most interesting contributions were the ones by Ben-Zvi and Frenkel, Dijkgraaf, Moore, Stolz and Teichner, Teleman and Witten. Unfortunately, Dijkgraaf's beautiful paper about how matrix integrals give you Gromov-Witten invariants of Calabi-Yau manifolds doesn't seem to be available on-line. Neither is Witten's very interesting paper, which is about explaining the SL(2,Z) symmetry seen in N=4 SSYM in four dimensions in terms of the existence of a six-dimensional superconformal theory.

The Stolz and Teichner paper is quite interesting . They are pursuing the idea that conformal field theories provide geometrical representatives of elliptic cohomology classes. Segal and Mike Hopkins worked on this a bit in the late 80s, with no conclusive results. Recently Hopkins has reformulated the whole elliptic cohomology story in terms of a new cohomology theory he calls "topological modular forms". He gave a beautiful series of talks about this at the Segal Conference; this isn't written up in the proceedings, but was for the 2002 ICM. For a more expository version of the ideas of Stolz and Teichner, see Teichner's survey talk at a conference in Santa Barbara last summer.

Finally, the proceedings volume contains Segal's wonderful unfinished manuscript "The Definition of Conformal Field Theory", together with nine pages of very interesting comments about what he was trying to do then, what he would do differently now, and what had kept him from finishing the manuscript. The main problem seems to have been that he was unable by his methods to explicitly construct the "modular functor" that one should get out of WZW models, so for this reason the crucial chapter 11 on WZW models remains unwritten.

His comments begin with:

"The manuscript that follows was written fifteen years ago. On balance, though, conformal field theory has evolved less quickly than I expected, and to my mind the difficulties that kept me from finishing the paper are still not altogether elucidated."

Comments

1. **D R Lunsford**
July 18, 2004

   Re Witten's paper:

   Lie algebra of SO(3,3):

   Let indices go 1..4 (spacetime) with Pm=Jm5, Qm=Jm6, and M=J56.

   [Jmn, Jrs] = -i(gmr Jns – gms Jnr + gns Jmr – gnr Jms)

   [Jmn, Pr] = -i(gmr Pn – gnr Pm)

   [Jmn, Qr] = -i(gmr Qn – gnr Qm)

   [Pm, Pn] = [Qm, Qn] = -iJmn

   [Pm, Qn] = -[Qm, Pn] = -iM gmn

   [Pm, M] = iQm

   [Qm, M] = -iPm

   [Jmn, M] = 0

   Let Rm = Pm+iQm, Sm = Pm-iQm. Then Rm and Sm separately combined with Jmn, are Poincare algebras, implying finite translations (two are needed to get a real translation on spacetime). The appearence of algebraic translations is reminiscent of the appearence of translations as repeated supersymmetries.

   Is this related to what Witten is talking about?

   Note also that

   [Rm, M] = Rm

   [Sm, M] = -Sm

   [Rm, Sn] = -2(M gmn + iJmn)

   so the elementary translations are distinguished with respect to the ?mass? M=J56 and they don't commute. One can imagine this as a primitive distinction between matter and antimatter on the lowest level. (sorry in advance for sign errors).

Hawking in Dublin

July 21, 2004
Categories: Uncategorized

Hawking gave his widely anticpated talk in Dublin today and reports are on CNN and all sorts of other places in the media. Sean Carroll has managed to get ahold (via Dennis Overbye of the New York Times) of a transcript.

Here's the part where he summarizes his argument:

"I assume the evolution is given by a Euclidean path integral over metrics of all topologies. The integral over topologically trivial metrics, can be done by dividing the time interval into thin slices, and using a linear interpolation to the metric in each slice. The integral over each slice, will be unitary, and so the whole path integral will be unitary.

On the other hand, the path integral over topologically non trivial metrics, will lose information, and will be asymptotically independent of its initial conditions. Thus the total path integral will be unitary, and quantum mechanics is safe."

His argument is in Euclidean quantum gravity, which he describes as "the only sane way to do quantum gravity non-perturbatively", something which some might disagree with. What he seems to be arguing is that, while it is true you get information loss in the path integral over metrics on a fixed non-trivial black hole topology, you really need to sum over all topologies. When you do this you get unitary evolution from the trivial (no black hole) topology and the non-trivial topologies give contributions that are independent of the initial state and don't contribute to the initial-final state amplitude.

I guess what this means is that he is claiming that, sure, if you knew you really had a black hole, then there would be a problem with unitarity, but in quantum gravity you don't ever really know that you have a black hole, you also have to take into account the amplitude for not actually having one and when you properly do this the unitarity problem goes away.

He has some proposal for doing some kind of calculation that implements his proposal using the AdS/CFT correspondence.

Comments

1. **Peter**
   July 27, 2004

   I'm afraid I've never followed this whole black hole information paradox story very carefully. It always seemed to me that in the absence of a real theory of quantum gravity, you can't tell which if any of these proposals really makes sense. People seem to hope that by thinking about this they might make progress on understanding quantum gravity, but I haven't seen that happen. Spending some time thinking about the Hawking stuff has just reinforced this opinion. He has a vague plausible resolution of the paradox in Euclidean quantum gravity, so do other people in other frameworks. So it looks like the paradox is pretty much gone, which is bad since the hope of many was that solving it would help solve the quantum gravity problem.

   By the way, John Baez has a nice first-hand description of the scene in Dublin and a good explanation of Hawking's argument. See
   This week's finds in Mathematical Physics. Week 207

2. **Chris W.**
   July 26, 2004

   Peter,

   Have you discussed this issue with your Columbia colleague Maulik Parikh? He seems to have another take on the whole debate, the relative simplicity and clarity of which I find appealing. See:

   hep-th/0405160
   hep-th/0402166

   (..not that I know enough to render any kind of a judgment.)

3. **Stephen Lavelle**
   July 24, 2004

   I was there…he didn't really say that much about anything…i guess we just have to wait for the paper to come out (and even then…ah well, he's probably right)…Though they havn't finished up doing the general case (still not done for general geometries and topologies) yet according to his PhD student (i can't remember his name off hand…).

4. **Urs Schreiber**
   July 23, 2004

   It's hard to tell what really is Hawking's argument. One sees Maldacena here and a negative cosmological constant there – I don't know.

   Right at the beginning when he says

   > Since the conformal field theory is manifestly unitary, the argument is that supergravity must be information preserving. Any information that falls in a black hole in anti de Sitter space, must come out again. But it still wasn't clear, how information could get out of a black hole. It is this question, I will address.

   It sounds as if he is just going to clarify a detail of AdS/CFT.

   He does need a negative Lambda to make any sense of the Euclidean path integral, so that's where the need for AdS comes from. But if he really thinks about going to susystrings on AdS_5 times S5 I don't know. Before he could come to that he was busy talking about bets and encyclopedias.

   I don't know if one can do AdS/CFT on something else than AdS5 times S5. There are lots of CFT/gravity dualities mentioned in the literature, but I don't have a good overview. Since it's really sugra that is involved here, though, every lower dimensional thing must probably come from reducing the full 10d scenario.

   The funniest thing is that apparently Witten has done the calculations that are missing in Hawking's talk already six years ago, as recalled by Jacques Distler.

5. **Peter**
   July 23, 2004

   Well, at various points he is clearly arguing from the point of view of euclidean quantum gravity. I don't understand how he's bringing in AdS/CFT other than to show how his ideas are not in conflict with it, at least to the extent you interpret it as a relation between supergravity and a CFT.

   By the way, isn't AdS/CFT about 5d gravity? Can you really use AdS/CFT to study 4d gravity, i.e. is 4d quantum gravity dual to a 3d CFT, and if so which one?

6. **Urs Schreiber**
   July 23, 2004

   Well, Hawking seems to be relying on AdS/CFT. That tells you the evolution on the gravity side must be unitary. But it also tells you that you don't have just pure gravity – but susystrings and all that. This again means that it is most likely that the black holes described by AdS/CFT are not just the plain old black holes of Einstein-Hilbert, but have stringy degrees of freedom. It is not clear yet how these should look like. But Mathur has made some educated guesses.

   That's what I found most surprising about Hawking's talk: He just admitted that old AdS/CFT is the solution to it all and then tried to argue that his Euclidean semiclassical pet is not obviously in conflict with that.

   No wonder that this approach raises some eyebrows. Did you see what Susskind commented? 🙂

7. **Rahul Jain**
   July 22, 2004

   Decoherence would cause a non-unitary step in the evolution of the system, and a black hole will have to either be formed or not. This is irrespective of the fact that there may be an observer at infinity. The experiment with hot fullerene diffraction shows that a system can cause decoherence with itself if it emits a particle that contains enough information to betray a specific state of the source.

   However, the two particles that cause Hawking radiation may be entangled across the event horizon and the state of some particle within the event horizon may be teleported to the particle that escapes away from the horizon.

8. **Peter**
   July 22, 2004

   Somehow the idea seems to be that, asymptotically, the amplitudes for the black hole sector are just constants, don't depend on the initial state, so I guess they just change the normalization of the overall amplitude.

   Important disclaimer: I'm no expert on this stuff, and Hawking's transcript is so vague that you need a real expert to know what precise sense can be made of his statements. Hopefully he'll produce a real scientific paper with some details soon.

9. **Scott Aaronson**
   July 22, 2004

   I read the transcript, and I'm having a basic difficulty. It's *possible* that a black hole never formed, but the amplitude for that could be incredibly small, no? And yet that amplitude bears the whole burden of storing the information? So when the hole evaporates, you see nothing with probability 1-epsilon, and the information with probability epsilon? That isn't unitary. Maybe you can tell me if I'm missing something trivial.

Polyakov: String Theory is Crazy

July 26, 2004
Categories: Uncategorized

Alexander Polyakov is one of the most prominent figures in theoretical physics and one of the most well-known string theorists at Princeton. He has written a review of his career and of his efforts to understand the relation between gauge theory and string theory. His penultimate paragraph goes as follows:

"In my opinion, string theory in general may be too ambitious. We know too little about string dynamics to attack the fundamental questions of the 'right' vacua, hierarchies, to choose between anthropic and misanthropic principles, etc. The lack of control from the experiment makes going astray almost inevitable. I hope that gauge/string duality somewhat improves the situation. There we do have some control, both from experiment and from numerical simulations. Perhaps it will help to restore the mental health of string theory."

Seems to me he's saying that, while using string theory to understand gauge theory is sensible, those claiming that it provides a theory of everything have gone nuts. I wonder what his colleagues at Princeton think of this.

Comments

1. **Chris Oakley**
   July 31, 2004

   Excuse me for pointing out the obvious, but if Motl, Hawking, or any other "genius" out there can't back up speculations or half-baked science regarding black holes with a consistent quantum theory of gravity then I will continue to exercise my right to ignore them.

2. **serenus zeitblom**
   July 31, 2004

   I'm sad to have to report that my contribution to Lubos' education will not be shared with the readers of sci.physics.strings. Lubos replied that my contribution did not meet the high standards of his group [I wish you people wouldn't snicker like that–] and that anyway it should be obvious why nobody cares about the observations of black hole observers who are not infinitely far away from one. In the immortal words of Peter Woit: got that?
   Next question: whence this strange enthusiasm for Hawking's work on the part of LM?

3. **D R Lunsford**
   July 31, 2004

   Ah, the famous Schroedinger's soul problem!

4. **serenus zeitblom**
   July 30, 2004

   By the way, in case anyone doubts the literal truth of Peter's title for this section, you might like to have a look at Lubos Motl's latest writings on sci.physics.strings. His conclusion is that we shouldn't really worry about information loss since we are all gonna [sic] die anyway and *some* information will be lost in that way. Here is my response — it will be interesting to see how he behaves in his capacity as moderator:

   Lubos Motl wrote in
   >
   > Yes, no one has really resolved and defined the correct laws of physics as
   > seen by the infalling observers. Well, they're gonna die which
   > automatically means "some" loss of information from their point of view.
   [My reply]:
   Profound, truly profound. We're all gonna die and that will mean
   the loss of "some" information.

   Thank you. My doubts about unitarity in quantum gravity have
   all been answered.

   On second thoughts, what if we go to heaven? Or, in
   the case of some of us, to the Other Place? Will
   unitarity be preserved by supernatural tunneling?

5. **Urs Schreiber**
   July 28, 2004

   JC –

   I haven't looked at which calculations precisely Dixon does. But I am sure he said that he can relate QCD calculations of N=4 SYM and that hence any better understanding of the latter has an effect on the QCD calculations.

6. **JC**
   July 27, 2004

   Urs,

   Most of the stuff Dixon alludes to doesn't appear to use the AdS-CFT duality directly. The most that appears in his strings 2004 talk that alludes to AdS-CFT, doesn't look like it's much more than hand waving. I didn't see any explict AdS-CFT calculation results in his talk, nor in any of his previous papers. Where did you see Dixon's stuff on explict AdS-CFT type of calculations?

   In Dixon's earlier work on calculating QCD amplitudes, SUSY appears to be used as a calculational "tool" and not really as a fundamental symmetry. How he's able to this, I'm not entirely sure offhand. The only semi-plausible reason I can think of offhand is in the first quantization picture of gauge theories. There were a number of papers by Michael Schmidt (at Heidelberg, I think) from the 1990's which looked at QED in the first quantization picture. Turns out the Lagrangian for first quantized ordinary QED has an explicit “SUSY” symmetry, which doesn’t readily appear in the textbook version of 2nd quantizated QED. In some sense, the first quantized picture of ordinary QED gives you a “SUSY” symmetry literally for “free”. Where exactly this SUSY symmetry comes from in the first quantized ordinary QED, I don’t know offhand. There’s a section in Polyakov’s book which discusses the first quantization picture for ordinary QED.

It appears Schmidt, nor anybody else yet, has been able to generalize this result to the non-Abelian gauge group case. In principle, a first quantization picture of ordinary Yang-Mills theory coupled to fermions, should reproduce the results Dixon got in his earlier QCD calculation papers that used SUSY as a “tool”.

7. **Urs Schreiber**
July 27, 2004

Thomas –

there is string/gauge duality and it is best understood for cases which do not very much resemble the real world. N=4 SYM is best understood because of its high symmetry. That makes it easy. As Witten nicely explains in his intro to the Clay institute challenge on YM, the reasoning is as follows:

– we want to understand YM

– but YM is hard

– so as a first step move to a point in field theory space which is easier to handle

– this leads to the study of supersymmetric QFT and N=4 SYM in particular

– so let’s study this as a first approximation to what we are really interested in

– surprisingly it turns out that N=4 SYM is apparently equivalent to superstrings on AdS5 times S5 (this is a conjecture, true, which has been check to 2.5th order or something, pretty impressive already)

8. **Urs Schreiber**
July 27, 2004

JC –

at Strings04 in Paris Dixon gave a talk on how to compute QCD stuff by mapping it to N=4 SYM. For instance, if I recall correctly, he said that the tree level amplitudes are the same when you identify the SYM fermions appropriately, and that similarly higher loops can be mapped in a certain way to SYM.

But once you are doing anything with N=4 SYM you can equivalently compute on the dual string theory side. For instance the recent progress in computing anaomalous dimensions in N=4 SYM is all based on BMN, spin chains, semiclassical strings in AdS5 and so on.

9. **Thomas Larsson**
July 27, 2004

Urs,

I admit that I find the equivalence between N=4 SYM and gravity very confusing. However, this kind of correspondence only seems to work (if it does, Maldacena is still a conjecture, right?) for YM theories plagued by almost-falsified supersymmetry. I am positively sure that vanilla Yang-Mills, of the kind present in the standard model, does not contain gravity. The standard model is not a theory of gravity, is it?

I will come back to loop space gauge theories later.

10. **JC**
July 26, 2004

Urs,

Lance Dixon’s work doesn’t appear to be using the AdS-CFT duality stuff. Most of Dixon’s results look like they’re perturbative amplitude calculations in Yang-Mills theory, using string inspired methods such as in his lecture notes hep-ph/9601359

Looks like Dixon’s later work on calculating gravity amplitudes via Yang-Mills results, appears to be generalizing a result between open and closed strings in Kawai, Lewellen and Tye’s paper “a relation between tree amplitudes of closed and open strings” Nucl. Phys. B, 269 (1986), where Dixon et. al. takes the point particle limit. The net result he found appears to be writing perturbative gravity amplitudes as the “square” of some corresponding Yang-Mills amplitudes. On the surface it appears that it’s an easy way to get gravity amplitudes by just recycling old Yang-Mills amplitudes calculated previously. There’s a review paper by one of Dixon’s collaborators, Zvi Bern, that reviews all of these perturbative gravity calculations without having to calculate Feynman diagrams directly by brute force from first principles gr-qc/0206071

11. **Urs Schreiber**
July 26, 2004

Hi Thomas –

I should definitely learn more about gauge theory in loop space formulation and maybe you can teach me something. (BTW, did you see my reply to your SCT comment.)

Concerning YM in 4D and gravity I don’t understand what you are saying. The big exciting fact is that N=4 SYM is equivalent to a theory of gravity. People like Dixon are even using this to investigate QCD amplitudes by using recent results on strings in AdS. If you like this or not or if you think the dual gravity can have any relation to the gravity that we observe, it is still there.

12. **Thomas Larsson**
July 26, 2004

Polyakov is probably my greatest living hero (Einstein and Dirac are unfortunately dead), so it makes me happy that he is ready to acknowledge problems in the string theory, despite his important contributions to that subject.

I don’t think that anybody has seriously questioned that gauge theories can be formulated in terms of stringy variables. This idea may go back to Faraday and his flux tubes, and it was completely clear with the introduction of Wilson lines in 1974. It is less clear to me how useful such a formulation is; the Migdal-Makeenko loop equations are now 30 years old and still defies any hope of solution. Loop space is really messy because it is so big.

However, Yang-Mills theory cannot be equivalent to string theory even if formulated in stringy terms, since it does not contain gravity and is consistent in 4D. There are of course close analogies between Yang-Mills and gravity, exploited e.g. in LQG, which is a loop space formulation of gravity in Ashtekar variables. But if you find gravity in Yang-Mills you must have done something more than a reformulation.

13. **Urs Schreiber**
July 26, 2004

This always leaves me puzzled: I may acknowledge that strings know about gauge theory but I may not be excited about them also knowing about gravity? 😉

14. **Alejandro Rivero**
July 26, 2004

The QCD strings, old or new ones, are the most promising silver bridge for trapped physicists -specially for younger ones- to escape from string theory. It is not rare, from time to time, to heard invitations to use it. In this way it does not seem that you have been years in the blue; instead you can claim a consistent career path and keep going.

Smolin on the Anthropic Principle

July 26, 2004
Categories: Uncategorized

Lee Smolin has a new preprint discussing the “anthropic principle”. He argues that one standard form of the anthropic principle that has been invoked by proponents of the “Landscape” is not falsifiable and he gives an eloquent explanation of the importance of falsifiability for a shared scientific enterprise. He also discusses the “prediction” of the rough magnitude of the cosmological constant that supposedly uses the anthropic principle and is due to Weinberg. He points out that this argument really isn’t an anthropic one, since it is independent of the existence of intelligent life. It just relies on showing that there is a relation between the cosmological constant and the existence of gravitationally bound structures. Then, since we see galaxies, we know something about the cosmological constant.

One of Smolin’s concerns is to show that his theory of “cosmological natural selection” (discussed in his book “The Life of the Cosmos”), while being a theory of a “multiverse” just like the string theory Landscape, is different in that it is potentially falsifiable, unlike some recent anthropic arguments.

He states well the predicament that theoretical physics finds itself in, with the tactic that worked so well throughout the 20th century, that of searching for unification by exploiting symmetry, no longer having much success. While I agree with most of what he has to say in this preprint, I’m more optimistic than him that future progress through new ideas about unification and the exploitation of symmetry is still possible. My point of view is more that the reason the last twenty years have seen no progress of this kind is that virtually all the field’s effort has gone into pursuing one very speculative and not very promising idea about unification, ignoring other possible lines of research.

Comments

1. **Thomas Larsson**
   July 28, 2004

   Mathematically, exploitation of symmetry=representation theory. There are several techniques for constructing and studying representations that mathematicians have developed over the last 50 years, but physicists have never learned. In addition, for some of the groups that are experimentally known to be physically relevant (4d gauge and diffeomorphism groups), very little is known about their representations.

   Actually, the classical representation theory of these groups is well known – it is called *differential geometry*.

   This is pretty obvious once you think about it. Differential geometry is the theory of well-defined objects, which are precisely those that transform consistently under diffeomorphisms. Thus, tensor densities are (often irreducible) modules, the exterior derivative is an intertwining operator, a connection is a central extension of the module of (1,2) tensor fields, etc. Special geometries, like symplectic or contact geometry, can likewise be regarded as the representation theories of the appropriate subgroups of symplectomorphisms and contactomorphisms. To treat spinors, you need to consider groups of local frame rotations and vielbeine, etc.

   However, we know from conformal field theory that the Virasoro algebra has two qualitatively different kinds of reps: classical modules, which are called primary and secondary fields, and modules of lowest-weight type (Verma, Fock, vertex operator, coset, minimal models, etc.) which are relevant to quantum theory. Differential geometry only deals with the higher-dimensional analogues of the classical modules. This is easy to see, because all interesting (= non-trivial and unitary) quantum irreps always have a positive central charge.

   The Virasoro algebra is a central extension of the diffeomorphism algebra in 1D, so generalizing it to several dimensions would be a first step towards constructing quantum reps of the diffeomorphism group in several dimensions. At first sight this seems hopeless, since a no-go theorem tells us that the diff algebra has no central extension except in 1D. Nevertheless, a multi-dimensional Virasoro algebra does exist, although the extension is not central in higher dimensions. The first quantum reps were found in the seminal paper

   S.E. Rao and R.V. Moody,
   Vertex representations for N-toroidal Lie algebras and a generalization of the Virasoro algebra,
   Commun. Math. Phys. 159, 239-264 (1994)

   The underlying geometry was clarified in http://www.arxiv.org/abs/math-ph/9810003, which led to considerable generalization. We can now a construct quantum rep for each tensor density and each non-negative integer.

   Much remains to be done, of course. In particular, I don’t understand how to generalize the minimal models, which are needed to make physical predictions in CFT. One must expect that the full representation theory is much more complicated than for the Virasoro algebra. This is true already on the classical level, where the differential geometry of a circle is not very exciting. But the construction of a quantum analogue of differential geometry seems to me to be a quite interesting result in its own right. Anyway, I cannot imagine that I will ever discover anything as important again.

   So, it seems to me that the kind of new mathematics that is needed is clear and there are plenty of techniques to try and attack the problem with. But no physicists want to think about this, their reaction generally is “What does this have to do with M-theory?”

   Hear, hear! When I started to search for a multi-dimensional Virasoro algebra in 1987, one of my main motivations was that I wanted to apply it to quantum gravity. The fact that such an algebra exists and that it has good quantum representations (other anomalous algebras like Mickelsson-Faddeev apparently have not) is to me a strong indication that my physical motivation was not completely wrong.

   Whereupon I will disappear into a computer-free zone for a week.

2. **Peter**
   July 27, 2004

   Crackpots are much easier to deal with in mathematics since mathematicians insist that things be well defined and logically coherent before publishing them or taking them seriously. Coming up with math that hasn’t been done before that satisfies this criterion is hard, much too hard for the kind of crackpots that infest physics.

   Mathematically, exploitation of symmetry=representation theory. There are several techniques for constructing and studying representations that mathematicians have developed over the last 50 years, but physicists have never learned. In addition, for some of the groups that are experimentally known to be physically relevant (4d gauge and diffeomorphism groups), very little is known about their representations. So, it seems to me that the kind of new mathematics that is needed is clear and there are plenty of techniques to try and attack the problem with. But no physicists want to think about this, their reaction generally is “What does this have to do with M-theory?”

3. **JC**
   July 27, 2004

Peter,

If Smolin's scenario of symmetry principles becoming less and less useful in physics turns out to be true, what do you think physicists will do in place of exploiting symmetries?

Naively if I didn't know any better, I would be tempted to search through the math literature looking for mathematical structures that have not been used extensively in physics previously. Albeit, this would seem like a haphazard "brain dead" way of doing theoretical physics that could easily lead to numerous dead ends and wasted effort. This would definitely be an act of desperation, when everybody else has run out of brilliant ideas and are floundering around.

Only other thing I can think of offhand, would be to "invent" some new mathematics. Though inventing new mathematics out of "nothing" seems like it would be a lot harder than it looks. At times I wonder what the crackpot to non-crackpot ratio is these days in the area of "inventing" new mathematics.

Black Holes at the LHC

July 31, 2004
Categories: Uncategorized

Jacques Distler has something interesting about the prospects for [producing black holes at the LHC](). This has often been promoted as one of the most exciting possibilities for new physics from the LHC. Evidently it turns out that cosmic-ray experiments currently in progress also are sensitive to this kind of hypothetical black hole prediction. Depending on one's assumptions about the minimal mass of a black hole that can be cleanly distinguished, either all or almost all of the range of new fundamental gravity scales accessible to the LHC will have been covered by the cosmic-ray experiments. So, if there is a new unexpected gravity scale in the range of a few Tev, this is very likely to first be seen in cosmic-rays, not at the LHC.

These Tev-scale gravity models have gotten a lot of attention in recent years, much of it in the form of claims of string-theory "predictions" that could be tested at the LHC. A few days ago I was talking to one of my colleagues, who believed, based on hearing a talk by Nima Arkani-Hamed, that string theory made predictions about what would happen at the LHC. Of course this is nonsense. There are lots of models you can construct involving string theory and any gravity scale you want. Past experiments rule out the possibility of a gravity scale up to the range of 100's of Gev-1 Tev, but there is no reason other than wishful thinking and a desire to have something to say to people who point out that string theory makes no predictions, to believe that there is a gravity scale just a bit too high to have been seen at the Tevatron, but observable at the LHC. Such an assumption actually ruins what string theorists consider the major success of the whole string theory/supersymmetry picture, the fact that the 3 coupling constants in the standard model nearly come together at an energy somewhat below the Planck mass.

Personally I've always found the current "Large Extra Dimensions" Tev-scale gravity models to be just hideous and completely unmotivated. On the other hand, the one thing we know is that the electroweak-breaking scale is in this region, and since we don't yet know what is causing this breaking, it is not completely inconceivable that it has something to do with gravity. The argument that the scale of quantum gravity is the Planck scale is not water-tight. It is based on the assumption that whatever generates the Einstein-Hilbert action as the effective low-energy action for gravity produces it with a magnitude of order one. If instead it comes with an exponential factor, the underlying gravity scale could be quite different than the Planck scale.

Besides some enthusiasts whose talks give the impression that Tev-scale gravity is a prediction of string theory, most string theorists believe that this is something unlikely to occur (Distler is one of these). One of the weirder things I've encountered in arguing with prominent string theorists is that they like to say that the Tev-scale gravity models are one of the major achievements in string theory in recent years, while at the same time saying they don't believe in these models. What's up with claiming as a point in favor of your theory that it "predicts" something you don't believe?

Comments

1. **Alejandro Rivero**
   August 1, 2004

   For extra dimensions in general, I like the reinterpretation of Giorgi, Cohen and Arkani, going from 4D to higher dimensions by using a dinamical process and getting symmetry breaking from it.

   Also, to see mathematically the Higgs field as an extra dimension is already a well known theme.

Low Point in Hawking Coverage

July 31, 2004
Categories: Uncategorized

Gregg Easterbrook is a senior editor at the New Republic, and gives every indication of being a complete moron. His column about Hawking's recent talk on black hole information loss is a masterpiece of anti-intellectualism. He appears to believe that the problem with physics is that, in trying to understand the big bang, ideas have been invoked that "don't do especially well on the common-sense test".

Only someone with zero common-sense would think that common-sense is going to explain the state and dynamics of the universe billions of years ago when conditions were utterly unlike those in which humans evolved and continue to exist.

Comments

1. **Peter**
   August 1, 2004

   Hi Deane,

   Thanks for pointing out the earlier Easterbrook piece about string theory. I had seen it a while back but forgotten about it.

   Easterbrook seems interested not so much in criticizing string theory as in promoting religion. His older article is more of the same anti-intellectualism as in his Hawking piece, attacking physicists for trafficking in ideas that he doesn't understand. It shows clearly what I see as one of the main dangers of where string theory is leading physics. If a scientific field abandons the standard norms of what is science and what isn't, it will lose its credibility and people like Easterbrook will try and promote the idea that we can never understand much about the universe and try and replace science by theology. Wait till someone tells him about the recent "anthropic" nonsense….

2. **Chris W.**
   August 1, 2004

   I read the Beliefnet piece. Peter is right; he is a moron. He touts string theory's resemblance to theology, apparently in the hope of promoting the legitimacy of the latter (understood as a belief in an "unseen realm of the spirit").

3. **Deane**
   July 31, 2004

   But, Peter, he's on your side, when it comes to
   string theory! Check out

   http://www.beliefnet.com/story/96/story_9602_1.html

   And, if you think about it, his argument against
   string theory is remarkably similar to yours.

This is Powerful Stuff

August 1, 2004
Categories: Uncategorized

Now string theory is being used to peddle Verizon DSL service (courtesy of Lubos Motl). According to the writers of the advertising copy :

“String Theory: The so-called unified theory is gaining credibility among young scientists”

Right.

Comments

1. **JC**
   August 1, 2004

   What would be amusing is if it said

   “String Theory: The so-called anthropic unified theory is indoctrinating young scientists into making fools of themselves”

More Landscape Looniness

August 1, 2004
Categories: Uncategorized

Susskind has posted a new preprint entitled "Cosmic Natural Selection". It's just two pages of various attacks on Lee Smolin's theory of cosmological (not cosmic) natural selection. Smolin's theory is described in detail in his book "The Life of the Cosmos", published seven years ago. As far as I know the terminology of "Landscape" entered physics in this book, where Smolin adopts the term "fitness landscape" from evolutionary biology.

Why is Susskind all of a sudden taking an interest in Smolin's old theory? Well, last Tuesday a preprint by Smolin appeared entitled "Scientific Alternatives to the Anthropic Principle" in which he gives a detailed criticism of the anthropic principle as unscientific. The next day Susskind tried to post a response to Smolin, consisting of a 3 page paper, half of which was just a quote from a summary of his argument that Smolin had sent to him. The one and a half pages that Susskind himself wrote were pretty much incoherent, and showed no sign that he had bothered to actually read Smolin's article.

This was such a bizarre document that someone responsible for the arXiv actually refused to accept it. I've heard of several non-mainstream physicists who have had problems getting their articles accepted, but this is the first time I've heard of this happening to a well-known mainstream physicist. Whoever did this was doing Susskind a huge favor, but he then immediately forwarded a copy of the paper via e-mail to a long distribution list.

So that's why Susskind's latest begins "In an unpublished note I criticized Smolin's theory of cosmological selection…" Note that he is not addressing any of Smolin's criticism of his own work as unscientific, instead he is attacking Smolin's own speculative ideas. I've personally experienced this kind of thing from more than one string theory fanatic. They don't respond to criticism of what they are doing (because they don't have much of a response), and instead forcefully and incoherently attack one for any speculative comments one may have made.

This just gets weirder and weirder all the time….

Comments

1. **Peter**
   August 15, 2004

   In his initial article submitted to the arXiv he states, concerning Smolin's article, "I took a quick look at the paper…", which I take as an explicit acknowledgement that he didn't read it. What he wrote showed no sign he had read it. Personally, this initial article seemed to me a disgraceful travesty of a scientific paper, for reasons I stated earlier. I don't know who at the arXiv rejected Susskind's article, but evidently they agree with me.

   To date Susskind has not publicly answered the detailed criticisms of his use of the anthropic principle contained in Smolin's article. If he has finally read the article and has an answer to Smolin's criticisms, scientifically his first priority should be to deal with this issue, not to attack Smolin's more speculative ideas. Doing what he has done gives the strong impression that he has no answer to Smolin's criticisms, and instead of confessing to this, has decided to go on the attack. Maybe "thuggery" was too strong a term, but I don't believe this has much to do with trying to get at scientific truth.

2. **Gil Steinberg**
   August 15, 2004

   Susskind must have read Smolin's work at some point, because the preprint "Cosmic Natural Selection" presents significant scientific criticisms of Smolin's theory, as detailed in my previous post. It is certainly appropriate that such criticisms be made public — this is how science works. I see nothing in Susskind's preprint that justifies your use of the highly charged terms `thuggery', `disgraceful', `beat up Smolin,' and `attack on Smolin.' I don't know what else appeared in the unpublished note, but according to your post it was a `defense of his [Susskind's] own work,' which again hardly seems to justify your strong language.

   By the way, Susskind is famous for not pulling punches, and he does not distinguish `string theorists' from non-`string theorists' when he thinks that someone is wrong. Have a look at his hep-th/9405103, `Comment on a Proposal by Strominger,' and also his contribution hep-th/0204027 to the Hawkingfest. Susskind's style is not for everyone, but I believe that it is his way of trying to get at the scientific truth, and I am sure that Smolin will take it in this way.

3. **Peter**
   August 15, 2004

   Hi, whoever you are at UCSB,

   Let me summarize again the facts about what Susskind did in response to Smolin's 40 page or so paper containing a detailed and serious critique of the anthropic principle and Susskind's use of it. Without even bothering to read it, within a day of its appearance he wrote a completely incoherent one and a half page defense of the anthropic principle and attack on Smolin, then tried to post it on the arXiv. This was such a ridiculous document that he was told he couldn't do this, something which is completely unheard of. A couple days later he stripped out the nonsensical defense of his own work, added a more detailed attack on Smolin, and managed to get this posted.

   I think this kind of behavior is disgraceful, and it has nothing to do with doing serious science. Susskind clearly just wanted to beat up Smolin for pointing out that what Susskind is doing is not science. I've seen all too much of this kind of thuggery, directed at anyone who dares criticize what string theorists are doing.

4. **Gil Steinberg**
   August 15, 2004

   Smolin's paper was explicitly about a scientific (that is, falsifiable) _alternative_ to the anthropic principle. So why are you criticizing Susskind for trying to falsify Smolin's alternative theory? Susskind's paper, although short, raises several very valid scientific objections to Smolin's theory. In particular, Smolin argues that production of new universes at a black hole singularity is on a firmer theoretical footing than production of new universes via bubble nucleation in an inflating universe (as in the landscape picture). Susskind argues that the reverse is true: bubble nucleation is observed in a wide variety of physical systems, and Einstein's equations imply that vacuum energy makes the universe expand (aside from the observational evidence that our universe has gone through two periods of vacuum-energy-driven expansion); it is hardly speculative to combine these two effects. The black hole bounce is on its face a more speculative thing, and as Susskind points out there are several problems here that have not been addressed: the effect of the Planckian energy densities at the singularity, and the relation to holography and the information problem. Moreover, even if the black hole bounce occurs, Susskind points out that inflation plus bubble nucleation is a vastly more efficient means to produce new universes than the black hole mechanism, so Smolin's theory is falsified. You are really putting string theorists in a no-win situation: thay are criticized for ignoring alternate theories, and then they are criticized for not ignoring them but instead subjecting them to the same level of theoretical self-consistency that they impose on string theory.

5. **Thomas Larsson**
   August 9, 2004

The Ginsparg-Glashow article to which I referred earlier is evidently available on the arxiv. Not much has changed since 1985, except perhaps that we now know that supersymmetry is close to being ruled out by experiments – it requires fine-tuning at the percent level.

6. **Peter**
August 4, 2004

Hi JC,

See my recent comments on "Black Holes at the LHC". I agree these models are basically silly, and most string theorists I've talked to say they don't believe them. The motivations for this kind of work are not just to get grants, but something more complicated. For one thing there is a desperation among string theorists to do something that can somehow count as an experimentally testable prediction, so the fact that you can cook up models of this kind that would have observable effects at the LHC is a huge motivation, even though the models are highly implausible and cooked up. A second motivation is that this stuff is technically very easy. You don't have to know any algebraic geometry, conformal field theory, etc. Much of this work just comes down to setting up and solving simple differential equations. It's a good subject for undergraduate projects.

7. **JC**
August 4, 2004

Peter,

Either I'm seeing things or asleep, but I recall seeing a comment on this thread about your view on those large TeV scale extra dimensions models.

When those models first appeared, I thought they weren't much more than somebody working out the consequences of some Kaluza-Klein theory, which didn't seem much more than a hypothetical exercise. After awhile when more and more folks were cranking out papers on this topic, I got the sense the subject was trying to making predictions that looked too silly to be true or even plausible. (ie. It didn't even pass the initial "laugh" test.)

After a few years of these large TeV scale extra dimensions models proliferating, I got the impression they seemed fit into the category of "let's propose these silly models that we know are too outlandish but are nevertheless 'trendy', just to get NSF or DOE funding". On the surface it sure smelled like an excuse for a grant funding grab. At times I wonder what sort of weird things people will propose, just to get grant money from the government, even if they know deep down in their hearts that the work is bogus from the start.

8. **Steven**
August 3, 2004

I read Smolin's article a few days back and have just finished the Susskind response. I generally think Smolin is a sober and level-headed physicist who usually puts forth interesting ideas and engaging and viable arguments.(Even if you don't agree with everything he says). I think he puts down a good case here for why the anthropic principle is simply unscientific.

The Susskind article however, does read like a blog/forum rant and not a carefuly thought out scientific article. He even admits he does'nt understand Smolin when he says" the detailed astrophysics that goes into Smolin's estimate is extremely complicated–too complicated for me–but the basic assumptions that go into the theory can be evaluated in the light of what string theory has taught us about the Landscape and black holes". How can he evaluate or respond to the article when he admits he doesnt understand the underlying astrophysical arguments?

Also, string theory has not really given any insights into real black holes or cosmology and is not in a sufficiently developed state to do so (many string theorists would actually agree). The Strominger-Vafa paper and other (admittedly interesting)papers in the 90s dealt with extremal black holes in string theory–not real astrophysical black holes. Similarly, string theory simply can't deal with deSitter space and a very small positive cosmological constant/accelerating universe despite the artifical constructions of the Stanford group.

Basically he seems only interested in promoting his own string-based agenda, even though it can't actually connect to anything in the observed universe, and any argument that does not conform to his dogma/agenda must be wrong in his eyes. I do find the whole thing somewhat strange. The pro-anthropic camp will have to really do a lot better if they are to maintain credibility. I really wish Susskind would produce one detailed cleary thought-out journal quality article carefully and rigorously promoting his ideas at least. (Papers he wrote years ago I found both interesting and informative, I just don't get this recent trend of his).

I think the arXiv is a double edged sword:one can instantly access the latest ideas/research in physics from anywhere but at the same time it is filling up with resume padding, career/grant maintaining, ego messaging stuff too. I try and stick to stuff in the Arxiv that at least has a journal acceptance/submission also. Peer review seems to have broken down in hep and string theory especially and I think it is going to be a problem for the field in the long run. I think it is fine for the people like Witten or Weinberg to just post their stuff but I think most others need the conventional journal route. String theorists should actually be the strongest critics of each others work rather than be a mutual admiration society (which is the impression I often get, whether that's right or wrong). Every other branch of science, engineeering, medicine has to go through the very rigorous "old fashioned" peer-review process via paper journals with subsequent online access. Although it is not perfect I think all scientists (whether students or established professors) still need the discipline/feedback provided by that or else the archive will end up like a ranting open physics forum. If the archive is going to be more rigorous in their acceptance criteria now then that could be a good thing.

9. **Peter**
August 3, 2004

I wasn't aware that the arXiv did any screening other than by author's affiliation, and wonder how often they reject papers from people with sponsors or with academic affiliations. Danny's story is a bit scary, I would have thought that some reason would have to be given to reject a paper, especially if it is sponsored.

But I think the right thing was done in rejecting Susskind's article. The arXiv shouldn't become like sci.physics.research, where people post comments of all sorts. It should remain a place where people post scientific articles of the sort that will ultimately appear in a peer-reviewed journal. I can't believe any such journal will ever publish the kind of thing Susskind has been putting out recently, so arguably others of his articles should also have been rejected. In any case this isn't an example of censorship of non-mainstream ideas, since Susskind is certainly part of the mainstream. His paper was rejected not because of his ideas, but because of its embarassingly incoherent and unscientific nature.

10. **D R Lunsford**
August 3, 2004

NEWS FLASH – Direct CP violation in B0 -> K+ pi- decay. Big news in real physics.

The gravity experiment (does antimatter fall up in the gravitational field of matter?) NEEDS to be done!

11. **D R Lunsford**
August 3, 2004

All I know is, I got a sponsor, the paper was submitted and a number assigned, it appeared in the archive and was then immediately removed. Never got an explanation, nor did the sponsor.

12. **Chris Oakley**
August 3, 2004

I was not aware that ArXiv was subject to any filtering at all other than the requirement that the sender should be at, or be sponsored by someone at a higher education establishment. If it is then that is a pity as instead of being a representative snapshot of the state of the art, it becomes a party political tool for whoever runs it.

The advent of the internet has enabled one to see what a physics discussion that is a complete free-for-all looks like. I am talking of course about the sci.physics forum. I like this newsgroup because of its entertainment value, but the serially-spamming crackpots and the immune reaction that they engender (headed by "Uncle Al") become predictable after a while. Having said that, "Uncle Al" can be very funny.

I think that the lightly-moderated sci.physics.research is about the level that ArXiv ought to aim for. If people want to make fools of themselves in public, then they should be allowed to do so, provided that they stay on topic and keep the personal abuse to a minimum.

13. **D R Lunsford**
August 2, 2004

Peter-

I have no idea how arxiv works. What I know about it comes from a friend who was stupidly blacklisted – I mentioned him in the "ACKNOWLEDGEMENTS" section. It could be just that petty, or it could be that a reappraisal of Weyl's theory in a new context is too much for them to swallow. All I know is – it's not string theory, it's not a Kaluza approach, the idea is inherently interesting, the calculations are correct, and the interpretation is as conservative as possible. There is no actual reason to reject the paper, particularly since the older theory lives in it as a first approximation. On the other hand there is a lot in it that is very suggestive of possible ways to make progress in understanding gravity and field theory together, from a conformal standpoint.

To be sure I never pressed the issue because the whole episode seems sordid in some way.

The preprint was immediately accepted by CERN without any complaints. In any case I think CERN automatically serves preprints of the usual journals.

-danny

14. **Peter**
August 2, 2004

Hi Michael,

I won't divulge my confidential sources, but a copy of the e-mail Susskind sent out to a large distribution list made its way to my inbox. If you think the tone of Susskind's arxiv posting was unscholarly, you should see the one that was rejected.

One unexpected aspect of doing this weblog has been that tricky questions of journalistic ethics arise. Should I repeat things people tell me privately? Probably not without asking them. How much of something that is forwarded to me can I make public if the original sender didn't intend to put what he wrote on the internet. What if he originally tried to put it on the internet but was thwarted and instead sent it to a lot of people? Shouldn't I help him out? By making it public or by keeping it private? Complicated questions….

15. **JC**
August 2, 2004

It seems like for many folks, the more indefensible their position is on something that is controversial, the more they resort to personal ad hominem style of attacks when defending their position. I always found it amusing in working up folks of this sort into a huge ad hominem attack frenzy, to the point that they're about to punch me in the face over it. It may very well be the case that in our human brains, our emotions can greatly overpower our own "logic".

It seems kind of weird and amusing at the same time, in watching string theory and loop gravity folks throwing ad hominem personal attacks at one another over various "ideological" type of issues in approaches to doing quantum gravity research.

Will a huge ad hominem flamefest between string and loop folks, and/or between pro and anti anthropic folks, be the final "end battle" which destroys the legitimacy of string theory and other forms of quantum gravity research as we know it?

16. **Michael Williams**
August 2, 2004

Where did you hear that his first hasty reply had been rejected by the arXiv? The new paper certainly seems to be written in a rather unscholarly tone.

17. **Thomas Larsson**
August 2, 2004

Does anyone know where Ginsparg stands on string theory?

On the one hand, it has been claimed that the Arxiv may in the end turn out to be string theory's most important contribution to science. So in some sense Ginsparg is a string theorist, and the phrase "Ginsparg archipelago" resonates somewhere in the back of my head (these are probably some isolated c=1 CFTs, perhaps associated with the E's of some ADE classification). OTOH, I think that he coauthored the infamous "Desperately seeking superstrings" article in Physics today in 1986, after which Sheldon Glashow came out as a string critic.

18. **Thomas Larsson**
August 2, 2004

A year or two ago I commented on spr that Smolin's review of quantum gravity looked like a declaration of war on string theory. Smolin denied that, claiming that many of his close friends are string theorists. The exchange can probably be found somewhere on the web if you really want to read it.

Anyway, even if that was true, it is hard to avoid the impression that his last paper really *is* a declaration of war on string theory in general, and on the anthropic principle in particular. Apparently Susskind draws the same conclusion.

19. **Peter**

August 2, 2004

Hi JC,
I think it is very unlikely that the powers that be at the NSF and DOE will step in and start defunding this kind of research. They generally rely on a system of peer evaluation, which has allowed string theory to do well as string theorists generally evaluate other string theory research highly. So the real question is whether the united front of string theorists will crack further. How many string theorists are willing to give a low score to grant proposals to do “anthropic” research?

This is the real significance of Smolin’s recent paper. He is making a strong argument that a lot of people are no longer doing science, and this is a huge threat to them, in particular to their NSF or DOE funding. This is why you see Susskind so upset that he writes bizarre articles and launches attacks on Smolin.

20. **Peter**
August 2, 2004

Hi Danny,
No, you’re the only example I know of where someone has had a peer-reviewed article rejected by the arxiv. By the way, do you know who it is who is now making these decisions about what to accept and what to reject? Is it Paul Ginsparg, who set up and runs the thing, or someone else?

It seems to me that rejecting Susskind’s article opens up a whole can of worms for the arxiv, and I wonder what criteria they used to reject it. Up until now they could claim that they were just rejecting based on the lack of certain kinds of academic affiliation, but here they’ve rejected something presumably based on it not being scientific. But if they start really rejecting things from string theorists whether or not they are scientific, this is just the beginning.

21. **JC**
August 2, 2004

Peter,

It sure seems like many of these anthropic and anti-anthropic papers over the last year or so, read almost like a “Pot Vs. Kettle” competition, especially when it’s between different quantum gravity camps (ie. strings vs. loops).

What do you think the government grand funding agencies (ie. NSF or DOE) will do, once they catch on to all this anthropic silliness going on in string theory and other quantum gravity research areas?

22. **D R Lunsford**
August 2, 2004

Peter-

You say you’ve “heard of several non-mainstream physicists” etc. etc. – but do you know of any examples of papers that are already published in reputable peer-reviewed journals being rejected from the archive? I find myself in just this situation, and while I try to maintain a good humor about it and not take it too seriously, still I would not mind having the work properly dissected, particularly since there are mathematical aspects of it that need to be investigated, that are, for various reasons, not in my reach.

Given that a great deal of the archive in the last few years is devoted to string theory, the collapse of one can hardly avoid impacting the other.

23. **JC**
August 2, 2004

Years ago somebody showed for a joke, the correlation of crashes of the stock market and/or companies, with things like building really tall buildings and/or a company putting their name on a sports stadium. The classic case is that of Enron going bankrupt a few years after sponsoring the Enron field stadium in Houston, Texas. Another classic case is the stock market crashing in 1929, after the Empire State Building construction was proposed. In more recent times the Asian financial markets crashed in 1997 after the Petronas twin towers (in Kuala Lumpor) construction was proposed. They also showed for a joke, how the rises and falls of Donald Trump’s fortunes were highly correlated with the up and downs of the real estate markets in america.

The rationale that’s usually given to explain this correlation is that many grandiose ideas and stupid things are proposed and done, during times when the economy is booming.

Another joke correlation was how a company would decline and fall into problems, after when the company’s CEO and other upper management were on the front cover of magazines like Time, Newsweek, BusinessWeek, Fortune, Forbes, etc … During the 1990’s, Bill Gates picture showed up on the cover of Time Magazine and subsequently Microsoft nosedived from the anti-trust case against it. Something like in 1999, Jeff Bezos (CEO of Amazon.com) showed up as “man of the year” on the front cover of Time Magazine, and the dotcom bubble imploded shortly thereafter.

In a similar joke-like manner, there’s a spurious correlation between the idea of Zwiebach’s “string theory for undergrads” course & textbook, and the subsequent decline of string/M theory into the anthropic mess. Or earlier from the 80’s era, there’s a spurious correlation between the idea of Kaku’s “intro to superstrings” textbook or Luest & Theissen’s intro to string theory book, and the subsequent decline of string theory around 1990. From the 1970’s era, there doesn’t appear to be any “textbooks” on string theory. The closest I can think of offhand would be Paul Frampton’s book on “dual resonance models”, which looks like it was published just right around the time everybody was abandoning string theory in 1974.

It seems amusing that a highly speculative field subsequently comes crashing down and declining after a “textbook” on it is proposed and written.

Perhaps there’s some truth to the notion of

“history repeats itself, first as low comedy and then as mindless farce”

(attributed to Karl Marx)?

24. **JC**
August 2, 2004

This sure sounds a lot like a major decline of quantum gravity, falling into a downward spiral into the abyss. Something has really gone astray when the “experts” start to sound more and more like crackpots.

At times I’ve noticed in politics when somebody’s popularity is falling, many of their press releases become more and more difficult to differentiate from “satire” press releases (ie. Matt Drudge style), or political news stories on “serious” 24 hour news channels (ie. CNN, MSNBC, etc …) starts to resemble the stuff on late night television comedy shows (ie. John Stewart, Bill Mahr, Dennis Miller, etc …).

It would be interesting to see what will happen to quantum gravity research, if this stupidity persists for several years.

Beauty, Fashion and Emperors

August 4, 2004
Categories: Uncategorized

Roger Penrose has a new book out in England, called "The Road to Reality". It is 1000 pages long and is now ranked number 17 on Amazon's UK site. There's a review of the book here.

It sounds like the book contains all sorts of things, including some of the material about string theory that Penrose has presented in public talks various places about "Fashion, Faith and Fantasy in Modern Physical Theories". One version of these talks is available online from Princeton. By "Fashion" Penrose is referring to string theory, and he considers the question of why it is so fashionable. String theory has been heavily sold as "beautiful" and to some extent Penrose seems to go along with this, but his invoking of the term "fashion" indicates an awareness of how problematic notions of "beauty" can be. The latest fashionable clothes are heavily promoted for their beauty, although after a few years, when they become unfashionable, this beauty is no longer so obvious."Beauty" is very often a social construct, with many people willing to agree that something is "beautiful" if everyone around them is saying so. Recall the story of the emperor and his fashionable outfit.

I've never understood these claims that string theory is "beautiful", and was again struck by this when I got ahold of a copy of Barton Zwiebach's new book on string theory aimed at undergraduates called A First Course in String Theory. Most of the first 270 pages of the book are devoted to working out in detail the quantization of the bosonic string in light-cone gauge, and I find it hard to believe that anyone finds this a beautiful subject. It is mathematically rather complicated and not that interesting, and has no real connection to any observable physics.

Later on in the book Zwiebach does devote a fair amount of space to trying to connect string theory to the standard model, mainly using the construction of intersecting D6 branes. At the end of this section, he acknowledges that this construction is truly hideous and looks all too much like a Ptolemaic use of epicycles on epicycles to explain planetary motion, saying "the models seem contrived, at least in the sense that they are engineered to give the physics that we observe, rather than obtained naturally as the simplest solutions of string theory". He quotes Alfonso the Wise (1221-1284) as having said the following about Ptolemaic epicycles:

"Had I been present at the creation, I would have given some useful hints for the better ordering of the universe."

He tries to end on a more optimistic note, hoping that some deeper meaning of string theory will emerge with more work, quoting Maimonides as follows:

"In the realm of Nature, there is nothing purposeless, trivial or unnecessary"

which begs the question of whether string theory is part of the "realm of Nature".

Comments

1. **Mal**
   August 20, 2004

   There are several reviews in top UK newspapers of Penrose's "The Road to Reality", many are referenced from here (including one by John Gribbin):

   http://www.321books.co.uk/reviews/the-road-to-reality-by-roger-penrose.htm

2. **Chris Oakley**
   August 6, 2004

   I do not believe that Newton or Einstein, or most others who have made significant contributions to science have been significantly cleverer than their contemporaries. What they have tended to bring to the party was independence of mind. They come up with things that the others could have come up with but did not because they were not asking the right questions. This quality enables them to ignore what people around them are saying and just figure things out for themselves. Following this principle – the Sinatra doctrine – they end up being either much more right or much more wrong than average (Einstein managed both). Although I cannot claim to have all the answers in this regard, it seems to me that the research machine as presently structured, is not an efficient factory for research ideas. If the tenured professors, who make almost all the decisions about the direction of research, were infinitely wise there would not be a problem. The reality though is that their students may be able to do better, and if they can, and end up losing the support of their supervisors as a result then they have no choice other than to leave. This kind of hierarchical structure may work for the military, but it does not work in areas where creativity and lateral thinking are an important element.

3. **erinj**
   August 6, 2004

   But imitation is the sincerest form of flattery, and surely the fact that Newton's words live on after over 300 years is testimony to the impact his life and work made upon Western civilization and, more broadly, humanity. I don't believe that these comments are poking fun at Newton the person, but rather at his legacy, which I think is as vast as any one human being has ever left to us.

   As you rightly imply, Chris, none of us are likely to leave such an enormous and influential legacy behind, and I believe that anyone who has studied any physics very likely has the utmost respect for Newton. I know I do, and for me this made such remarks more interesting: in fact, I found your analogy involving Newton's famous quotation fascinating. I recall (perhaps incorrectly) that the `beach' quotation originally appeared in a letter Newton wrote to somebody. How likely, though, are we to quote passages from Newton's Optiks or Principia Mathematica today I wonder?

   As far as I know, Newton was not known for his humour during his lifetime, and even had a dour expression (I would think this fitting given how seriously and deeply he must have contemplated things). I recall reading that his theory of universal gravitation was subject to ridicule after it became famous, yet Newton was far more concerned with his ongoing arguments involving fellow natural philosophers such as Hooke and Leibniz.

   "Have legs, use legs."

   – Sir Isaac Newton

4. **Chris Oakley**
   August 6, 2004

   I just got a message from Isaac Newton through a medium:

   You guys are really beginning to piss me off. Yes, I wrote that thing about the pebbles and seashells. And the stuff about giants. But that doesn't mean you're entitled to *twist my words in ridiculous ways just to amuse yourselves. Have **you** got a unit of force named after you? Did **you** figure out the universal law of gravitation? Thought not. So maybe if **you** come up with some great new physics, preferably something where you get the number of dimensions right (three space, one time, in case you've forgotten) then you might, and I say **might** be in a position to fun of me.*

5. **Arun**
   August 6, 2004

   And for those stupified by string theory : If I haven't seen further than others, it is because I stood in the footprints of giants……

6.
   August 5, 2004

   "…wandering in the car show room and diverting myself and now and then finding a shinier car or a faster SUV than ordinary…"

   Argh! I get the "half-asstrophysicists" thing now. Excuse me for not being up to speed on US slang :s

7.
   August 5, 2004

   Yes, I too spent time watching The Flintstones as a youngster, but they were all repeats on British television, so by then they were probably `classics', considered worthy of repeating. I'd really like to see the towering giant become a loud and aggressive promoter of string theory as he gets older, shouting his defence of it in television interviews, though I doubt it would come to that.

   By the way DR, was "asstrophysicists" a typo?

8. **D R Lunsford**
   August 5, 2004

   Ok can't resist…

   Chris made me think of Newton in a modern context

   "To myself, I have seemed but an itinerant contractor, stubbing out a bummed cigarette on the threshold of the loading dock, while all before may lay mostly undiscovered the great parking lot of truth."

9. **D R Lunsford**
   August 5, 2004

   Start of a Gell-Mann lecture on string theory at Maryland – "I see we have a mixed audience of astronomers, physicists, and half-asstrophysicists…"

   -drl

10. **Peter**
    August 5, 2004

    I often tell people I learn a lot from these comments, never expected to be learning Flintstone trivia. What is with all you people, are you as old as I am so also spent your childhood watching Flintsone cartoons (or I guess maybe they are classics and go on and on)?

    I loved the Gell-Mann quote, which was new to me. Ah, for the glory days of particle physics, when the leaders of the field were obnoxious, competitive, entertaining megalomaniacs. Now, we just have one person towering over a bunch of dwarves, and he's a very nice, polite, soft-spoken guy.

11. **erinj**
    August 5, 2004

    Thanks, will do. If anything, I suppose the current state of this thread of comments seems to indicate that it's quite easy to go astray from `The Road to Reality' – in both senses 🙂 Reminds me of the office banter that used to go on whilst a research student, taking a break from the equations 🙂

12. **Chris Oakley**
    August 5, 2004

    No – "The Flintstones at Viva Rock Vegas" was a live action prequel to the 1994 film, but IMHO much better. It got panned by the critics, but they know nothing. It came out about 4 years ago. Get the video out and tell me what you think … although I suggest by e-mail rather than as a comment on a physics web log (however subversive it may be).

13. **erinj**
    August 5, 2004

    I thought `Pebbles' was Fred and Wilma's daughter… or was that `Bam Bam'? Maybe `Bam Bam' (the one who wielded a club) was Barney Rubbles' son? What was `Viva Rock Vegas', Chris? I don't remember that, but it's gotta better than the live action film (though I thought Halle Berry as `Sharon Stone' was good). Wasn't `Dino' the name of Fred and Wilma's pet purple dinosaur?

14. **Chris Oakley**
    August 5, 2004

    "Pebbles" was their boy, wasn't he? We're getting way off topic, but am I the only one who thinks that "Viva Rock Vegas" is the best Flintstones thing ever?

15. **erinj**
    August 5, 2004

    I dunno about you, but all these comments about pebbles made me think of the Flintstones.

16. **Chris Oakley**
    August 5, 2004

Danny and Steve –

Thank-you for sharing your (most profound) insights with me. I'm sure that Sir Isaac, tracking Peter's web log from his office in that great university in the sky, is regretting ever saying a damn thing about Oceans of Truth.

17. **Steve**
    August 5, 2004

    "If I have seen farther than others it is because I am surrounded by dwarves"
    Murrey Gellmann

18. **D R Lunsford**
    August 5, 2004

    Renorm: Pretending you can play with pebbles that are infinitely massive.

    Strings: Pretending that pebbles can fly.

    Branes: Pretending that flying pebbles can sing.

19. **Chris Oakley**
    August 5, 2004

    Without wishing to take the analogy too far –

    Renormalization: Pretending your rough pebbles are smooth.

    Superstrings: You never walked on the beach at all. You only dreamed it.

20. **D R Lunsford**
    August 5, 2004

    Right! – and I think it goes on, "If I have seen farther than others, it was only because I stood on the shoulders of giants."

21. **Chris Oakley**
    August 5, 2004

    You mean this one?

    "I do not know what I may appear to the world; but to myself I seem to have been only like a boy, playing on the seashore and diverting myself and now and then finding a smoother pebble or a prettier seashell than ordinary, while the great ocean of truth lay all undiscovered before me."

    Sir Isaac Newton

22. **D R Lunsford**
    August 5, 2004

    Nice quote – on the other hand we have Newton, who actually accomplished some physics – who said "Hypotheses non fingo" – a modest admission that he wasn't God or even privvy to His Thought, just a working stiff attempting to understand how the planets move the way they do. He also said in effect "I couldn't have done this without your help" to Galileo and Kepler. In these days when it's popular to bash Einstein and Dirac, that quote – the "great ocean of truth" one – seems much more powerful to me.

    I sometimes wonder if obession with the ineffable realm of abstraction is a sort of drug addiction that is clouding the collective judgment.

23. **Peter**
    August 4, 2004

    Hi Eric,

    Wonderful quote. Your book gives an excellent and precise explanation of the way in which good physics really is "beautiful" or "elegant", along the lines of Leibniz.

    For more about Eric's book, see something I wrote a few months ago here

24. **Eric Baum**
    August 4, 2004

    God has chosen the world that is the most perfect, that is to say,
    the one that is at the same time the simplest in hypotheses and the
    richest in phenomena.
    — Gottfried Von Liebniz

This Week's Online Conferences

August 6, 2004
Categories: Uncategorized

A couple conferences going on this week have already put some of the talks online.

SLAC has a summer school each year, aimed more at experimentalists than theorists. This year's topic is "Nature's Greatest Puzzles" and there are quite a few interesting talks already online there.

The Michigan Center for Theoretical Physics is hosting this year's String Phenomenology 2004 conference. The "Landscape" seems to be a big topic; two online talks are Michael Douglas's, which is more or less the same as his one at Strings 2004 a few weeks ago, and Michael Dine's. Dine seems optimistic that the Landscape will lead to predictions, saying

"If we adopt the anthropic viewpoint, we may be lead to predictions – perhaps the first predictive framework for string theory"

Before taking this too seriously, one should note that Dine has been giving review talks about "superstring phenomenology" and claiming that predictions are right around the corner since before most of our incoming students at Columbia were born (see his 1986 Erice lectures, no, they're not online, this was way before the arXiv).

And I'm headed off soon for a short vacation out of the range of the internet, back late next week.

Comments

1. **JC**
   August 15, 2004

   Just to be silly about the "137 loop" QED argument in my previous post, let's change one of the assumptions.

   In the case of an exploding number of Feynman diagrams at n-loop order, if we have instead have (n!)^2 diagrams at the nth order of perturbation theory (instead of n! diagrams in my last post), then the A_n's should be proportional to (n!)^2 after all the Feynman diagrams are summed up at n-th order.

   In this case the R_n ratios will become

   R_n ~= alpha * n^2
   (with all other assumptions the same).

   For divergence the R_n's are greater than 1.
   So we now have n^2 greater than 1/alpha, which now says the "QED series" will start to diverge after n = 11 loops.

2. **JC**
   August 15, 2004

   Peter,

   I thought about the "137 loop" assertion thing for QED. Here's sort of a naive argument that I can think of offhand.

   We start with a QED perturbative series

   "QED series" = sum_{n=0 to infinity} C_n

   where
   C_n = A_n * alpha^n,
   alpha = fine structure constant

   From a naive look at the explosion of the number of Feynman diagrams at each order of n-loops, there appears to be n! number of diagrams for the nth order in perturbation theory. Just summing up all the Feynman diagrams at n-loop order, the coefficient A_n should possibly be proportional to n!. So we factor out n! from A_n as

   A_n = B_n * n!

   So now C_n = B_n * n! * alpha^n

   Now we look at the ratio R_n between succesive orders in perturbation theory as

   R_n = C_n/C_{n-1} , where n = 1 to infinity
   R_n = [B_n/B_{n-1}] * [n!/(n-1)!] * alpha^{n-(n-1)}
   R_n = [B_n/B_{n-1}] * n * alpha

   For convergence, we want R_n to be always less than 1. If we naively say that all the B_n's (left over after factoring out the alpha^n and n!, from the C_n's) have roughly the same order of magnitude each, then the ratios [B_n/B_{n-1}] will be approximately an order of magnitude 1.

   (If the B_n's all have the same order of magnitude that is not of "order of magnitude" 1, then the overall "order of magnitude" can be factored out from the overall "QED series" expansion).

   With these naive assumptions, now the ratios R_n become

   R_n ~= n*alpha

   For divergence, R_n will be greater than 1.
   n will be greater than 1/alpha = 137.
   Hence above n=137 loops the "QED series" will start to diverge.

3. **Aaron**
   August 15, 2004

   The hierarchy problem is really just dimensional analysis. It doesn't depend on any details of a theory of quantum gravity. It's just that, in natural units, the natural scale of things is the Planck scale and one might like an explanation of why the ew scale is so much less than the natural scale.

   The technical naturalness problem also doesn't depend on the theory of qg. The divergence in the Higgs mass is there in the low energy theory.

   Lastly, some theories of SUSY breaking do have an intermediate scale.

4. **Peter**
   August 15, 2004

   Hi Aaron,

   "There may be no hierarchy with the GUT scale, but there certainly still is one with the Planck scale."

   Umm, maybe that's why my initial comment that you objected to saying that the hierarchy problem went away if you didn't believe in GUTs had a parenthetical caveat "ignoring gravity…"

   The hierarchy problem for GUTs is a well-defined one since a GUT is a well-defined theory that includes the standard model with electroweak symmetry breaking. To have a hierarchy problem involving the electroweak scale and the Planck scale, first you need a well-defined unified theory that includes quantum gravity and the standard model, including an explanation of the electroweak scale. Some of us don't believe this exists. All supposed candidates for such a theory have a far more serious problem, the vacuum energy problem. You have to first figure out why the absolute value of the effective potential is completely wrong before worrying about its second derivative.

   Then again, the latest trend seems to be to announce that the anthropic principle solves all such problems, so we can stop worrying about them.

5. **JC**
   August 15, 2004

   Aaron,

   Will the hierarchy problem with the Planck scale disappear if gravity is just strictly a classical force with no quantum counterpart?

   Are there any simple ways to "preserve" naturalness when SUSY is broken?

   I haven't checked the literature lately, but has anyone showed that supergravity can easily deal with the hierarchy/naturalness problems? Or is supergravity coupled to Yang-Mills theory just another disaster with the same hierarchy problems between the electroweak, GUT, and Planck scales?

   As a side note, are there any speculative particle models which have an "intermediate" scale that's higher than the electroweak scale but lower than the GUT scale? If there are any from the past, have they all vanished into the dustbins of physics history?

6. **Aaron**
   August 15, 2004

   There may be no hierarchy with the GUT scale, but there certainly still is one with the Planck scale.

   There are really two kinds of naturalness problems. One is to ask what stabilizes the Higgs mass against loop corrections which tend to drive it up to the Planck scale. The other is what explains the hierarchy between the weak scale and the GUT scale.

   SUSY has some nice features regarding the naturalness problems (and nice features not involving the naturalness problem) which I think I've discussed here before. As I think I've also said, some of these nice features have proved to be less nice than they first appeared. But such is life. When you break SUSY, for example, I think the naturalness problem can easily reappear, but I don't want to make any big pronouncements about SUSY breaking because I'm not really up on it.

7. **JC**
   August 14, 2004

   Peter,

   What's the exact argument for the QED perturbative series diverging beyond 137 loop order? I remember seeing this "137 loop" assertion over the years as if it was some folklore wisdom. I never figured out an easy semi-rigorous argument to justify it.

8. **Peter**
   August 14, 2004

   It almost certainly is just an asymptotic expansion. What determines the running of the coupling constants is the beta-function and presumably the perturbation expansion for this is just an asymptotic expansion. But one expects the situation to be like in QED, where the expansion is expected to be good up to an order that goes like the inverse of the coupling constant, i.e. 137 in QED. So, roughly in this case one expects the perturbation calculation to be good up to an order given by the inverse of the coupling constants in the range that you want to follow how they run. This should be less than 137, but still probably much larger than the order to which one can realistically do the calculation.

9. **JC**
   August 14, 2004

   Peter,

   If the 3-loop calculation (or for that matter higher loops) changes the 2-loop result significantly, will some folks try to argue that peturbative expansion is really just an "asymptotic series" that starts to break down early and diverge? Besides being rhetorical, are there any good estimates as to what loop order the perturbative SUSY calculations will start to break down due to asymptotic nature of the series expansion?

10. **Peter**
    August 14, 2004

    Hi Aaron,

I'm not sure what you mean by the "hierarchy problem". I was referring to the problem of keeping the electroweak and GUT scales separate, i.e., the electroweak scale being so small in GUT scale units is technically "unnatural" and requires fine tuning. By definition this problem disappears if there is no GUT scale. Supersymmetry solves it (kind of, there's something called the "mu-problem" of it reappearing) by pairing the electroweak Higgs with a fermion whose mass is small because of chiral symmetries. Technicolor solves it because running of the coupling constant makes the coupling strong at exponentially smaller scales than the unification scale (also this is why the small size of the strong scale in GUT units is not a problem). But if there is no GUT with symmetry breaking occuring at a high GUT scale, there is no problem.

The coupling constant unification you mention is far and away the best evidence for a GUT (specifically a supersymmetric GUT), but, using the two-loop results, it is far from convincing. I don't know if anyone has done 3-loops but one wouldn't expect 3-loops to change the 2-loop results much. If it did one would have to worry about whether the whole perturbative set-up was making any sense.

11. **JC**
August 14, 2004

Aaron,

Has anyone calculated the SUSY 3-loop result, or for that matter any other higher loop result? If so, does it improve or worsen the coupling constant convergence scenario?

12. **aaron**
August 14, 2004

If you ignore GUTs, the hierarchy problem does *not* disappear.

The reason a lot of people believe in GUTs is that the couplings of the standard model, when you run them to high energy, *almost* meet. Now, there's no particular reason for three lines to almost meet, and no one likes coincidences. Thus, it's not implausible to believe that the couplings really do unify, that the reason for this is a GUT, and that some higher energy stuff fixes the running of the couplings.

For a long time, supersymmetry seemed to be exactly that higher energy stuff. It causes the agreement of the couplings to improve significantly. The one loop calculation works almost perfectly. Unfortunately, as I understand it, the two loop calculation ruins the agreement which is one reason why people are somewhat less sanguine about SUSY these days.

13. **Peter**
August 13, 2004

Supergravity was certainly the most popular idea about unifying gravity and the standard model during the late seventies, early eighties, but it never dominated things the way string theory has. By 1984 it was kind of killed off by

1. Witten showing that you couldn't get the kind of chiral spectrum you needed if you compactified 7 of the 11 dimensions of 11d supergravity on a smooth manifold.

2. Calculations showed that potential counterterms appeared in higher orders, so presumably supergravity was not a renormalizable theory.

Actually, what people are almost always doing when they say they are working on "M-theory" is really just 11d supergravity. Now it's back with a vengeance, people just stopped worrying that the compactification manifold can't be smooth.

14. **JC**
August 13, 2004

Peter,

On a similar note, was there any silly hoopla surrounding supergravity in the mid-late 1970's and early 80's, before string theory got popular? Only thing I can recall offhand was reading some 1980 speech Stephen Hawking made which advocated supergravity as some sort of "grand unified theory", when he first got the Lucasian chair.

15. **Peter**
August 13, 2004

Oops, correction, SO(10) is rank 5.

16. **Peter**
August 13, 2004

The choice of those GUT groups predates their use in string theory. To fit SU(3)xSU(2)xU(1) into a simple group in a way where they commute with each other, you need a group of at least rank 4 (the sum of the ranks of those 3 groups). SU(5) is probably the simplest example of such a group. SO(10) is also of rank 4, and has the added advantage that you can fit all the particles of a single generation in one irreducible representation (the 16 dim spinor rep).

The simplest examples like SU(5) just with the Higgs sector needed to get the standard model can be ruled out because they predict something (proton decay) that doesn't happen. The problem with most possible GUTs is that they don't really predict anything. Depending on how you break the symmetry you can get pretty much anything. It's kind of bogus to claim that these are really "unified" models, because you have to assume some complicated Higgs mechanism to break the symmetry. You end up adding lots of new parameters to the standard model, not explaining any of the ones you've got.

Yes, if you don't believe in GUTs (count me in..), the hierarchy problem goes away (ignoring gravity…), and thus one of the main motivations for supersymmetry.

Pink Floyd and Led Zeppelin certainly bring back memories from high school ("Stairway to Heaven" was the theme song of my High School prom, shudder….). I think Kiss appealed mainly to those a bit younger than me, or with different interests. Never paid much attention to Blue Oyster Cult.

17. **JC**
August 13, 2004

Peter,

Is the only reason why anybody bothers paying attention to GUTs like SU(5), SO(10), E_6, etc … along with their SUSY versions, is mainly due to the gauge groups which show up in string theory like SO(32), E_8 x E_8, etc …? Have any of these GUTs beyond SU(5) been experimentally ruled out yet? Or are there so many free parameters in these GUTs that anybody can "fudge" their way through and get an answer that isn't ruled out by experiment? I always found adding in tons of free parameters to be somewhat on the distasteful side.

If GUTs were wrong from the very beginning with no basis in experiment or even reality, wouldn't this remove one of the major theoretical arguments in favor of low energy supersymmetry (ie. naturalness)?

On a different note, were rock bands like Pink Floyd, Led Zeppelin, Kiss, and Blue Oyster Cult very popular when you were in high school?

18. **Peter**
August 13, 2004

Actually SU(5) was even before my time, 1974, when I was still in high school. Throughout the late seventies there was a lot of work on SU(5) and other GUTs, but they were nowhere near as dominant as string theory is today. They were just one of several popular ideas to work on (instantons, supergravity, lattice gauge theory, etc.). Once the experimental results started coming in (around 1980?) showing that protons didn't decay at the rate SU(5) predicted, interest went down, although there continues to be lots of research in GUTs, partly since they are supposed to be the effective field theory for whatever string theory is supposed to explain the world.

19. **JC**
August 13, 2004

Peter,

When the SU(5) GUT was first around, did it have the same hoopla and propaganda surrounding it, like string theory in the 80's and 90's? Was it taken seriously at all at the time? Or did Sheldon Glashow have such a huge influence that people were willing to read and/or believe anything he said and wrote about?

20. **Peter**
August 13, 2004

The problem, as with Douglas, is how to weight the probability of all these vacuum states. In KKLT, the hope is that all continuous parameters are fixed, so your only choices are discrete, and you can assign every choice the same weight (although without a more fundamental theory that determines how the universe settles into a single vacuum, there's no real reason to believe this).

If you start with a GUT with a large gauge group and try and break its symmetry with a Higgs sector, problem is there are lots of parameters associated with the Higgs. How you choose these determines what the low energy physics looks like. How do you weight these possible choices of parameters? Best guess is they should all be around the one scale in the problem, the GUT symmetry breaking scale, but then you can't explain why the electroweak scale is so low. This is the problem supersymmetry is supposed to address, but that solution doesn't really work.

21. **Arun**
August 10, 2004

Re the Douglas talk : suppose we take a QFT with a very large gauge group, with many possible ways of symmetry-breaking. We then try to catalog vacuums by the same criteria that is suggested – a symmetry-breaking resulting in particular low energy parameters is more "natural" if many vacua produce these parameters. From a QFT perspective, will it be true then that we live in a unnatural vacuum?

22. **JC**
August 6, 2004

It sure sounds like Dine is taking the same approach that many economists take when doing economic forcasts. Many economists frequently just keep on making the exact same forcast year after year according to their own particular biases, hoping that it will become "true" one day. This type of mentality is just like an old broken clock that is always right two times every day.

Twentieth Anniversary of the First Superstring Revolution

August 12, 2004
Categories: Uncategorized

Today a symposium is being held in Aspen to celebrate the twentieth anniversary of the "First Superstring Revolution". The canonical story of this "Revolution" is that twenty years ago this month, on a dark and stormy night at a workshop in Aspen, Michael Green and John Schwarz completed a calculation showing that gauge anomalies canceled in a specific superstring theory, thus changing physics forever. The more complete story of what happened at that time goes more or less as follows:

By 1984 Witten had been taking some interest in superstring theory for a while, giving a talk on the subject in April 1983 at a conference on Grand Unified Theories, but not publishing anything about it himself. In 1983 at the Shelter Island conference he had shown that the popular unification idea of the time, using supergravity on higher-dimensional spaces and the Kaluza-Klein mechanism, could not give the kind of asymmetry between left and right handed particles that occurs in the standard model (this has had a revival in M-theory, which these days invokes singular compactification spaces to get around Witten's no-go theorem). The failure of supergravity ideas had gotten him interested in superstring theory, but he was concerned about the issue of gauge and gravitational anomalies in the theory, anomalies that he worried would render the theory inconsistent. In his 1983 paper about gravitational anomalies with Luis Alvarez-Gaume, they noted at the end that these anomalies canceled in the supergravity theory in ten dimensions that is the low energy limit of the type II superstring. This theory didn't allow for low-energy gauge theories so wasn't useful for unification. Witten suspected that the type I superstring (which could be used for unification) would have insurmountable problems with anomalies, but the Green-Schwarz calculation showed that these anomalies could be canceled for a specific choice of gauge group.

Evidently Green and Schwarz talked to others about their new result at the 1984 Aspen workshop, but I would suspect that no one was very impressed (if anyone who was there knows differerently, I'd be interested to hear about it), since superstring theory was generally considered pretty much a far-out, highly unlikely idea. Green and Schwarz were well aware that their only real hope for getting attention was to get Witten interested, so on September 10th they sent him a copy of their paper via Fed Ex (this was before e-mail) at the same time they sent it off to Physics Letters B. Witten immediately went to work full time on superstring theory, with his first paper on the subject arriving at Physics Letters B on September 28th. I think this is really the point at which one should date the First Superstring Revolution.

Witten was at the height of his influence, and the news that he was now working on superstring theory spread very quickly through the particle theory community. I had just finished my graduate work at Princeton and was starting a post-doc at the Institute for Theoretical Physics at Stony Brook when I heard the news. Over the next six months to a year I remember hearing from a couple colleagues who had gone down to Princeton to talk about their work with Witten, only to hear from him that, while what they were doing was all well and good, the future was in superstring theory, so they should drop what they were doing and start working on that.

My own attitude was that it didn't look like a very promising idea. It was a complicated theory and didn't really explain anything at all about the standard model. I figured that there would be a lot of smart people working on it for a while and within a year or two either they would get somewhere with the idea and it would be clear I had been wrong, or they wouldn't, and everyone would lose interest. Neither I nor anyone else could conceivably have guessed that 20 years later superstring theory would still not explain anything about the standard model, but would completely dominate particle theory.

One reason for this is the string theory hype machine continues in high gear. The University of Chicago has issued a press release telling us that "growing numbers of physicists see superstring theory as their best chance" to formulate a theory of everything. Jeff Harvey is quoted as saying that "It?s an intellectual enterprise that?s extremely exciting and vigorous and full of ideas", which is very different than what I hear string theorists telling me in private.

Comments

1. **JC**
   August 24, 2004

   Danny, Chris,

   The only TV shows and documentaries I remember seeing on PBS when I was a kid, were mostly space and/or NASA oriented ones. I guess I missed all the shows that were on particle physics, as well as many of the biology related ones.

   The only other science related documentary from that time I really remembered were ones about the Manhattan Project and the making of the nuclear bomb. At the time I didn't really know what the significance of the Manhattan Project was in the context of World War 2, but I had a "morbid" fascination of the pictures and footage of the mushroom cloud explosion and the subsequent shock waves at the Trinity test site. It was many years later when I started to understand what the destructive power and significance of the nuclear bombs were really all about.

2. **D R Lunsford**
   August 23, 2004

   JC – as a kid (teen) I well remember the NOVA episodes about the gauge revolution – interviews with Feynman, Glashow, Weinberg etc. etc. Compared to today's pabulum (did you see that abortion by Greene?) they were outstanding shows.

   I also remember "The Ascent of Man" and "America", miniseries on PBS. I think these shaped my whole intellectual outlook way before college. So there was good stuff on the TV back in the day.

   The awful "Cosmos" series was a watershed. Since then it's been mostly downhill for "hard" science.

   -drl

3. **Chris Oakley**
   August 22, 2004

   Maybe that's the advantage of having fewer channels in the U.K.: if the BBC decides that it wants to try to educate you then there are fewer alternatives to switch to. The most prominent science/medicine documentary series was/is a BBC one called "Horizon". This was much the best (the commercial channel ITV had a series called "World in Action" but the brow was significantly lower both in choice of topic & presentation). Horizon programs were and probably still are "must see" for A-level (16-18 yr old) science students. They were generally topical and informative, although were structured like investigative journalism and would not normally provide the level of technical detail that one would find in a "Scientific American" article.

4. **JC**
   August 22, 2004

   Chris,

How common were science type of shows on British television in the 1970's?

I guess where I grew up in north america, I hardly ever saw any science shows when I was kid, largely because my folks didn't have cable TV for a long time and when they finally did get cable TV, they were always watching crap TV and seemed to have very little to no interest in science type of TV shows. The only channel from that time which had science type of shows every once in a while, was mainly the publicly funded PBS affiliate. Even then most of the shows on PBS were crap for the most part too.

The only way I found out about science in those days was mainly from reading Scientific American, which my father had a subscription to and he left them laying around on the coffee table in the living room. Sometimes I would go to the local public library and borrow books about physics or science in general, where most of these books were largely descriptive with very little to no math.

5. **Chris Oakley**
   August 22, 2004

   What sparked your interest in particle physics originally?

   Simple really: the popular science programs on TV. It seemed like a happening area and I liked the idea of it being the most fundamental of sciences. There really was a lot of coverage in the media and the main players (Feynman, Gell Mann, etc.) came across as interesting and entertaining people.

6. **JC**
   August 21, 2004

   Chris,

   What sparked your interest in particle physics originally?

   If I didn't know any better, the 1970's looked like it was the heyday for experimental particle physics and the Standard Model. If I was around your age or Peter's age, my interest would have probably been sparked from seeing the successes of particle physics in those days.

   In my case I came across several Scientific American articles on particle physics when I was in high school. At the time I thought it was fascinating that most of matter and the forces of nature could be explained by a small number of fundamental particles. It sure looked a lot better and more "compact" than the periodic table, which I was learning at the time in my high school science and chemistry classes. Not knowing any better at the time, I thought Feynman diagrams looked really cool and was fascinated by how one could calculate a lot of physics by just writing down a bunch of squiggly looking diagrams. It sure seemed a lot more interesting than the science class experiments I was doing at the time, like masses hanging on springs, measuring pendulums, concave & convex mirrors and lenses, etc …

   In fact in high school, I actually hated math and science for the most part. When I was a freshman in college, I also hated physics! All those freshman and sophomore introductory physics courses seemed so boring and tedious at the time, that I thought about quitting and not returning to college after the term was over. I thought it seemed like a waste of money at the time, in paying tuition for a bunch of boring and tedious classes which I had very little to no interest in. It felt like I was a sucker and/or a glutton for punishment at the time.

   During that summer when I was thinking about not going back to university in the fall, I actually went to a nearby college library and tried to find some books about particle physics and how to do Feynman diagram calculations. (I was bored that summer and didn't have much else to do). Besides reading the first two or three chapters (usually some qualitative discussions of the elementary particles, interactions, and/or hadrons, with very little math) of several graduate level particle physics books, I got lost very quickly afterwards mainly from the mathematics level being way beyond my ability at the time. Though by skipping over most of the math I didn't really understand at the time, I was amazed at how the final answers for many Feynmann diagram calculations looked kind of simple and had close agreement with the quoted experimental data. Not knowing any better at the time, I thought the form of many of those final answers for cross sections and decay rates could be guessed by just naively looking at the dimensional units of the possible quantities involved, and getting the final expressions to have the same units as a cross section or decay rate. At the time I was wondering why it involved a lot of tedious nasty looking math just to eventually get a numerical coefficient, for the final expressions with forms which could otherwise be guessed from dimensional analysis and semi-intuitive arguments.

   With some advice from my parents and ignoring my misgivings and gut feelings at the time about the tedium and boredom of college, I decided to go back to college that fall, hoping that I would one day be able to understand how to do Feynman diagram calculations. It took another few years of undergraduate physics before I was able to understand how Feynmann diagram calculations were actually done.

   Years later I heard about string theory also from reading some Scientific American, and thought the idea looked neat at the time. I guess I fell for the propaganda and hoopla surrounding it at the time, to the point of literally being "converted" overnight on the spot and becoming a "true believer" in string theory. I was definitely quite naive and clueless at the time. I even bought a copy of the Green, Schwarz, and Witten (GSW) book on superstring theory, despite not knowing a thing about string theory. This may sound kind of silly today in hindsight, but at that time I literally thought that GSW's "superstring theory" book was like "the Holy Bible".

7. **Chris Oakley**
   August 21, 2004

   I think that this goes back to Peter's earlier point that in the 1970's elementary particle physics really looked like it was motoring, whereas now it looks like it is sputtering. Brian Greene's efforts to get the public excited about the subject are heroic and commendable but it really would make all the difference if the experimentalists were part of the journey. If I were 25 years younger I would probably be looking to get into molecular biology or computer science and not particle physics.

8. **JC**
   August 21, 2004

   Thomas, Chris,

   I wonder how much the ratio of "genius" to "non-genius" in the population has changed over the years, or whether it's a ratio that remains more or less constant with time. Except maybe in another time, place, and circumstances, the "genius" may not be recognized as easily and/or is squandered and wasted away.

   One ominous example in the 1900-1902 period would be the case of Heinrich Himmler who was born on Oct 7, 1900, and later became the head of the Nazi SS, Gestapo, and was the main architect behind the "final solution" holocaust. Whether this was a satanic-like "genius" for "evil" is debatable, but arguably this could be the classic textbook case of a "genius" that was squandered and wasted away.

   In more recent times, other cases of "genius" being squandered and wasted away would perhaps be the case of hardcore computer hackers who like breaking into computers and/or stealing data, such as Kevin Mitnick or Kevin Poulsen when they were younger.

   If there's less kids going into science and engineering these days, perhaps some of the future "geniuses" will end up in non-science areas like business, law, high finance, etc … instead of physics areas like quantum gravity and string theory.

9. **JC**
   August 19, 2004

   Chris,

   I remember going through several old books and papers on analytic S-Matrix theory from the 1960's, when I was bored one afternoon.

   In one approach, it turned out they took the approach of "extracting" some results from quantum field theory (QFT), and then throwing away the QFT afterwards. At the time it sure seemed like they were "cheating" when they were trying to do analytic S-Matrix theory which didn't believe in QFT, but they were using the very same QFT to "extract" results from!

   I spoke to several older particle theory professors who were around when analytic S-Matrix theory was at its peak heyday during the late 1950's and 60's. They all mentioned that Geoff Chew's "axioms" for the analytic S-Matrix were just "too general" to get any useful results from. Since nobody was really getting any useful results from just looking at Chew's axioms alone, many folks ended up going back to QFT in an attempt to "extract" some useful results, literally as an act of desperation towards the end in the late 1960's. That was probably the breaking point which marked the beginning of the end for analytic S-Matrix theory as a viable framework for particle physics.

   This sort of thing reminds me of the attitude many economists had towards math when economics was first starting to use "advanced math" extensively. At the time, economists had the mentality of using the math to "extract" useful results and then throwing away and "burning" the math afterwards.

   At times I wonder if string theory has reached its "breaking point" yet, and whether there is a single event when could be marked as the "beginning of the end" of string theory as a viable framework for a quantum theory of gravity. If I didn't know any better, the anthropic junk sure looks like it could be something which may mark the "breaking point". The point if and/or when Witten abdicates from string theory would perhaps be the "beginning of the end" for string theory. The earliest possible date I can envision would perhaps be shortly after the LHC is producing data, and there's no supersymmetric partner particles and/or there's no "light Higgs" found in the data.

10. **Chris Oakley**
    August 19, 2004

    I can well believe that Landau might have made that proposal. Of course the whole "S-matrix" phenomenon of the sixties was based on the notion that quantum field theory did not really exist.

    As for renormalization, my views are (hopefully, by now) well known although I might post more detailed stuff on my web site about what I think of "effective field theories with an unknown ultraviolet completion", and why. BTW this is a great phrase, but I think "arbitrary set of rules inspired by but not derived from quantum field theory" is more accurate.

11. **JC**
    August 19, 2004

    Chris,

    Didn't Landau make the proposal of burying quantum field theory forever?

    I think it was in some paper where Landau did some calculation which showed that if you tried to eliminate the scale dependence in the coupling constant of quantum electrodynamics after using the renormalization trick, the only way possible he found was to make the coupling constant zero?

12. **Chris Oakley**
    August 19, 2004

    …*infinity problems which held up progress in quantum field theory for almost 20 years, until Feynman and Schwinger came along with the renormalization "crutch"*.

    It is very likely that banging your head vigorously and repeatedly against a brick wall is likely to lead to brain damage. People were looking to create quantum field theory in the image of quantum mechanics, and it does not seem to have occurred to them that the reason that they were unable to do it was because it was *not possible*!

    Take this, for example:

    $H = H_0 + V$

    This is a tremendous equation for quantum mechanics, but a diabolical one for anything relativistic. If we were being relativistic we ought to write

    $P_\mu = P^0_\mu + V_\mu$

    … but the the idea of a "free" and "interaction" three-momentum seems to be singularly unhelpful, so we carry on with the original equation, using the interaction picture derived from it that is horribly unrelativistic and just block our ears when the mathematical mines start to go off.

    The lesson? Don't get too hung up on formalism. Symmetry and consistency are more important. The formalism can be adjusted to fit.

    IMHO, the same considerations apply to quantising gravity: it could just be that no-one has been able to do this simply because it cannot be done! Formal quantisation simply does not work. What would make much more sense would be to be a little more practical. Do we really need all that much anyway? After all, in the absence of any experimental knowledge about quantum gravity all we require is the classical limit, which need not even be GR – an SR gravity theory that reproduces the experiments would do just as well.

13. **JC**
    August 19, 2004

    Thomas, Chris,

    Perhaps some people's genius becomes forgotten by history largely because they were born in a different time and place, and/or it was squandered and wasted away?

    Maybe if folks like Witten, Polyakov, etc … were born in Germany or Austria around the same time as Pauli, Wigner, Heisenberg, etc … their genius could have been recognized if they worked on the foundations of quantum mechanics during the mid 1920's?

    On the other hand, if folks like Pauli, Heisenberg, Wigner, Dirac, etc … were born around the same time as Witten, Polyakov, Schwarz, Gross, etc … (ie. around the 1940's and 50's), perhaps they would have become just "another face" in the large crowd of particle and/or string theorists during the 1970's and 80's? Maybe the competition in the 70's and 80's was a lot more intense than in the 1920's?

If Hitler was assasinated before 1933 or shortly after he became chancellor of Germany, perhaps Germany would have remained as the “Weimar Republic” well into the 1940’s with no war, no Nuremberg race laws, no holocaust, etc ..? If this would have happened, perhaps there would have been no mass defection of scientists to America, while Germany would have possibly remained the world’s powerhouse in physics and math, as well as no nuclear or hydrogen bombs being built?

If there’s a wholesale abandonment of string theory from Witten abdicating from it, how many folks will still be actively working on it other than perhaps Schwarz, Green, and maybe Lubos Motl and a few other unnamed “string fanatics”?

On a silly note, if Lubos Motl was born in Austria (ie. Czechoslovakia was still a part of Austria in 1901) around the same time as Pauli, Dirac, Wigner, etc … perhaps he would have became a “quantum theory fanatic”, or even a “quantum field theory fanatic” despite all the infinity problems which held up progress in quantum field theory for almost 20 years, until Feynman and Schwinger came along with the renormalization “crutch”. In this case his “quantum fanaticism” would have largely been right for most of the rest of his life.

14. **Chris Oakley**
August 19, 2004

To be precise –

Pauli: born 24 April 1900
Fermi: born 29 September 1901
Heisenberg: born 5 December 1901
Dirac: born 8 August 1902
Wigner: born 17 November 1902

I saw Dirac in person once, in 1981, where he gave a talk at Edinburgh University while I was at a summer school there. He wrote down his equation, in components, using alphas and betas – no gammas, of course (you can’t teach an old dog new tricks) and talked about how he arrived at this equation through considerations of beauty and symmetry. Considering the bluntness of some of the things he is famous for saying, he struck me as a delightful man. Wigner I saw at a conference on Group Theory at ICTP in Trieste in 1983, but did not speak to him as I could not think of a particularly intelligent question to ask or intelligent comment to make.

Thus Quantum Mechanics, being developed between 1925 and 1928 by three of these men, plus the relative “old man” Schroedinger (he was in his late thirties) was mostly a children’s crusade. There is no denying their brilliance, but nonetheless this still does say to me that (i) they were lucky to be born at this time and (ii) it is time that our society acknowledged that young people are, or at least should be, the driving force behind frontier research.

15. **Thomas Larsson**
August 19, 2004

JC,

Re people who are born at the right time. Arguably almost half of the top ten theorists of the twentieth century (Dirac, Heisenberg, Pauli, Fermi, with runners-up like Wigner and von Neumann) were born in 1901 or 1902. Nobody doubts of course that they were among the smartest people of their generation, but anyway. What are the odds that such brilliance would be concentrated to 2% of the century?

16. **Chris Oakley**
August 18, 2004

On second thoughts the scepticism/credulity scale ought to be dimensionless (a bit like probability). We’ll call the quantity C (=Credulity).

C = 0 – You believe nothing
C = 1 – You believe everything

These are obviously the limits to the allowed values.

However, as a matter of convenience in representing extremes of scepticism and credulity without having to use very small numbers or numbers very close to one, one could define a transformation

C = atan(WM)/pi + 1/2

The quantity WM (the Woit-Motl index) then is unbounded. Negative values are known as “Woit” values and positive values are known as “Motl” values.

17. **D R Lunsford**
August 18, 2004

Leading to “Boil’s Law”

WM = const

so a nanoMotl is the same thing as a gigaWoit.

-drl

18. **JC**
August 18, 2004

The “Motl” becoming the unit for scientific fanaticism!

1000 Motls = 1 kiloMotl

😉

19. **Chris Oakley**
August 18, 2004

Yes – the “Woit” could be a unit of scientific scepticism

1,000 Woits = 1 KiloWoit

20. **D R Lunsford**
August 18, 2004

JC,

IOW the times make the men? This is the “misanthropic principle” 🙂

I don’t think we should project the failures of current efforts onto the past. In any case we don’t need even 1 more Pauli – at this point we need 1000 Woits.

21. **JC**
August 18, 2004

Chris,

Perhaps there’s a lot to be said that being in the right place at the right time, makes a huge difference. If somebody like Sir Issac Newton was born a hundred years earlier, perhaps today all the physics and math with Newton’s “name” would have somebody else’s name in place of Newton. I suspect the same thing if Einstein, Schroedinger , Heisenberg, Dirac, Pauli, etc … were born in a different time and place. If Einstein never wrote the letter to president Franklin D. Roosevelt and/or Hitler created a nuclear bomb before the Americans, the world of physics may very have become “Nazified” with the names Einstein, Pauli, etc … purged away from physics history in a Nazi revisionistic rewrite of physics history?

Just to be silly, if Lubos Motl was born 100 years earlier perhaps he would have been an Austrian “aether” fanatic?

22. **D R Lunsford**
August 18, 2004

Chris –

Much more important to the US gov’t in that time frame were radar, sonar, and aeronautical technology. The US still imagined it had the atomic genic bottled and there were no plans or funding as yet for the “super”. That needed a few more years to develop.

-drl

23. **Chris Oakley**
August 18, 2004

JC,

If you want my honest opinion, the thing that caused the increase in government funding after WW2 was the fact that people were impressed by the achievements of physicists during the war. My apologies to any Japanese people who might be reading this who have some connection to the horrendous and unforgivable events of August 1945, but however destructive this episode may have been, it clearly impressed everyone, turning the likes of Oppenheimer, Fermi and Feynman into minor celebrities. I seriously doubt that governments would have been prepared to commit billions of dollars to particle physics twenty years later had not these events taken place. I know that there have been a few practical things that have come out of the study, but it would be foolish to pretend that these would justify such enormous expenditure.

My earlier point, although somewhat flippantly made, was not about government funding *per se*, but about how the money is spent. It is a very simple truth that one is more mentally agile when one is younger and I maintain that this agility would generally be better deployed in doing research in the ways that one sees fit rather than in devising especially ingenious ways of grovelling to the establishment. Having a more rapid turnover of research staff, however it is achieved, would help a lot in preventing ossification. Power and influence needs to be shifted to younger people. I do not have all the answers about *how* this can be done, but it certainly needs to be done in some way.

24. **JC**
August 18, 2004

Chris,

The only other thing I can think of if there’s no tenure for professors, would perhaps be a lot more competition amongst folks fighting for various lecturer/researcher jobs. The immedidate consequence perhaps would be a lot more “resume padding” type of research papers being produced.

I think it would also be highly dependent on how the selection process for new faculty worked. If the selection committee consisted of people who were around for a long time (ie. after several consecutive re-appointments), then these “old timers” may have a conflict of interest where they would prefer to hire a crappy or crackpot researcher who wouldn’t be able to compete against them, and hence less of a threat to the old timers’ job re-appointment prospects. I can’t really see an easy way out of this conflict of interest, unless there’s some sort of “outside” independent selection committee making the selections, with no conflicts of interest.

It seems like when many folks get tenure, some slow down and don’t publish as many papers. In fact some folks seem like they fell off a cliff after getting tenure, and hardly published anything afterwards. Seems like many folks have less motivation afterwards and/or have different priorities, when they are not “under the gun” any longer after getting tenure. Some folks seem like they “burned out” by the time they got tenure, and hardly produced anything of significance afterwards.

Perhaps it’s not so surprising as to why there’s some tenured folks who end up as “deadwood”, who look like they had all the passion and soul beaten out of them. I knew many folks over the years who were computer whiz kids in the past (ie. programming stuff since they were 12 years old) who ended up going into the computer/hi-tech profession as adults. After a many years, a large number of them seemed to really dislike and started to really hate computers. It seems like for many “dream job” type of professions, a lot of folks become “deadwood” after many years where they have less interest and very little to no passion left for the job, while just “going through the motions” to get things done. (ie. “dream job” professions like music, sports, acting, writing, etc …). Maybe this is a sign of job “burnout” for many folks?

25. **JC**
August 18, 2004

Chris,

Do you think progress in particle physics and quantum field theory would have progressed as fast as it did over the last 50+ years (ie. after Sputnik), if there was no tenure and/or very little to no government funding for physics?

Looking at the pre world war 2 generation, there didn’t seem to much extensive government funding of physics at the time either in America or Europe. It seems like many universities were focused more around teaching than the heavy research focus after Sputnik.

When the center of extensive physics activies was still in Germany and central Europe before Hitler took power, the Privatdozent (ie. sort of like a postdoc with some assistant professor duties) were frequently unpaid academic jobs, where they usually had to find other sources of income like teaching courses or family support. It seems like the generation of quantum theory pioneers in the 1920's could come up with groundbreaking research work, with many folks being an unpaid or a lowly paid Privatdozent with very little to no direct government funding. At times I wonder if the German Privatdozen system was implemented in America along with no post-war boom in science funding after world war 2, would particle physics have progressed as fast as it did?

26. **Thomas Larsson**
August 18, 2004

I would say this though: I went to a talk by Chris Isham about 20 years ago where he wrote down the algebra of GL(4,R) and the transformation law of a vector in the fundamental representation. He then announced that he had just quantised gravity. I could not see it, but maybe there were fifty steps missing that were obvious only to him.

It is the full diffeomorphism group – general covariance – that is relevant to general relativity. GL(4,R) is a subgroup, whose algebra is generated by vector fields of the form x^i d/dx^j. Many properties can be deduced from this subgroup, but not all; e.g., GL(4,R) cannot distinguish a connection from a tensor field of type (1,2), symmetric in the lower indices.

I don't understand the import of the Virasaro algebras, or the Kac-Moody algebras. I don't think that I'm incapable of learning this, but I can't find any coherent presentation of why these things are physically important and interesting.

The physically most important application, in the sense that it makes predictions that are in agreement with observation, is in 2D statistical mechanics. Lowest-weight irreps of the Virasoro algebra with L_0 eigenvalue = h (i.e. L_0 |vac> = h |vac>) correspond to fields whose correlation functions decay as G(x,y) ~ |x-y|^-2h. There are standard ways to relate these h's to other critical exponents, which have been intensely investigated, both theoretically and experimentally. The discrete spectrum of critical exponents predicted by CFT has been perfectly confirmed. If you compare with exact solutions of integrable lattice models, there are *infinitely* many numbers which agree *exactly*.

A good online presentation was written by Ginsparg.

27. **Peter**
August 17, 2004

Thanks for the correction about the circumstances under which Greene and Schwarz sent Witten their paper. I was hoping if I had any of this wrong someone who knew more about what happened at Aspen would correct it, and said so in the posting. If I've gotten anything else wrong about this, I'd be interested to know it.

28. **Jeff Harvey**
August 17, 2004

The statement that Green and Schwarz knew they had to get Witten interested and so sent their paper to him is simply false. I was at Princeton when Witten heard about their work, through Larry Yaffe who was at the Aspen Center for Physics during the summer of '84. After hearing about anomaly cancellation, Witten asked G&S for a copy of their paper by FedEx. I also disagree strongly with the statement that people were not impressed by this work independently of Witten. Dislike string theory all you want, but please don't try to rewrite history.

29. **JC**
August 17, 2004

Chris,

At this point I'll remain anonymous. I can't have any of my colleagues (both present and former) knowing that I read and post to this blog.

30. **Chris Oakley**
August 17, 2004

JC,

How do we know that you are not an "insider" making maximum use of your anonymity here (Sheldon Glashow, maybe?)

Incidentally, I would dearly love it if low-ranking academic staff did not feel constrained by career considerations in their choice of research. My dream is a Utopia like the world of "Logan's Run" (great film, by the way, apart from that annoying silver robot) where everyone over the age of thirty is eliminated. People would not then have to worry about getting a tenured job because there *wouldn't be* any tenured jobs. They would be forced to leave and do something useful with their lives instead.

31. **JC**
August 17, 2004

Chris,

Perhaps folks who are still "insiders" in academic physics/math, are more likely to say the "party line"?

Repeating the same "status quo" dogma over and over again like a broken record, hoping one day it will eventually be perceived as "the truth". It seems like in any large cohesive group of people, conformity to a particular "ideology" is the biggest factor which keeps it together.

On the other hand, former "insiders" and/or "outsiders" are more likely to "speak their minds" with very little to no self-censorship?

You just have to read books written by former politicians after when they were ousted, in how they show their displeasure at how things were done and/or the gross incompetence of the folks running the government. Perfect example of this is former US treasury secretary Paul O'Neill's memoirs of his two years in the Bush administration, after he was fired at the end of 2002. His book was talking about all the stupidity and incompetence that went on behind closed doors with Bush and his cabinet. I would be almost willing to bet that one of the big reasons why US defence secretary Donald Rumsfeld hasn't been fired yet for the Iraqi postwar disasters, is largely because the Bush administration doesn't want Rumsfeld yet to write his memoirs and/or to go on television criticising Bush and his cabinet, in a style similar to the memoirs of former US defense secretary Robert McNamara (who oversaw the Bay of Pigs, and Vietnam War disasters), especially before the upcoming presidential elections this November.

I can imagine for “insider” folks who have very little to no political power in academia (ie. grad students, postdocs, faculty who don’t have tenure yet, etc …), they don’t want to say anything which may compromise their future prospects in academia. For people who are no longer “insiders” or who were never “insiders” at all, there probably won’t be any major consequences for yelling “the emperor has no clothes on” in public, or being branded as a heretic or traitor by their former colleagues.

Perhaps there’s a lot of truth to the Machiavellian notion of “keep your friends close, but keep your enemies even closer to you”. Former “insiders” and/or “outsiders” can become loose cannons.

32. **D R Lunsford**
August 17, 2004

I’m unemployed and have been for more than a year – all my posts are out-of-pocket 🙂

Yes, that was exactly what he was doing as far as I can tell. But physics seems to be post-modernized, where simply saying “there exists an N-bein formulation” is not good enough or important-sounding enough.

-drl

33. **Chris Oakley**
August 17, 2004

Danny,

I am sorry, but once again I have to plead ignorance on all counts. This sounds a lot like treating gravity as a gauge theory with a gauge group of GL(4,R) – similar to the vierbein formalism – but that is as far as I go.

By the by, how many of us commentators are actually professional (as opposed to wannabe) physicists/mathematicians?

Me: Programmer/quantitative analyst working for a Japanese bank in London.

You: Software developer working in the U.S.

JC: Obviously an old-timer like us. Background in particle physics but otherwise unidentified – something to do with the Second Coming, perhaps?

Thomas Larsson: Working for a Swedish company that makes motors, by the look of it, but obviously with a very strong mathematical/physics background.

And why do our employers allow us to spend a significant portion of the day doing stuff that is clearly not work related?

34. **D R Lunsford**
August 17, 2004

Chris, check out this post from SPR:

A thought just occured it me. Isn’t it possible to eliminate the metric field g in general relativity in a simple way? Just introduce a GL(4,R) connection with the tangent bundle TM acted upon by GL(4,R) and insist every point has at most an SO(3,1) holonomy. For generic connections, this would determine g up to a global rescaling factor. In fact, since we’re making restrictions on the holonomy, this formulation would suggest we work directly with Wilson loops and lines instead. Also, generic connections would give rise to torsion.

..stifles cough..

Perhaps you should have paid attention to Isham, you blighter!

(Serious note: Math is dangerous in the wrong hands. Don’t do this at home. Save it for the office.)

-drl

35. **Chris Oakley**
August 17, 2004

Thomas,

At the moment I can only manage silence. The scorn will have to wait until I have learned more about Virasoro algebras.

I would say this though: I went to a talk by Chris Isham about 20 years ago where he wrote down the algebra of GL(4,R) and the transformation law of a vector in the fundamental representation. He then announced that he had just quantised gravity. I could not see it, but maybe there were fifty steps missing that were obvious only to him.

36. **D R Lunsford**
August 17, 2004

I don’t understand the import of the Virasaro algebras, or the Kac-Moody algebras. I don’t think that I’m incapable of learning this, but I can’t find any coherent presentation of why these things are physically important and interesting.

I would not be too concerned over silence out of SPR – it’s mostly struggling students I think. I have always enjoyed your posts, when I could understand them.

-drl

37. **Thomas Larsson**
August 17, 2004

Tell us more.

OK, although I have become reluctant to do so. I have tried to make the point on spr for the past three years, and so far only met with silence and scorn.

We know that the groups of diffeomorphisms and gauge transformations play an important role in general relativity and the standard model, respectively. We also know that the representations that are relevant in quantum theory are projective and of lowest-energy type. So unification of the symmetry principles would amount to the construction of projective, lowest-energy representations of the diffeomorphism and gauge groups.

On the Lie algebra level, this means that one must generalize the Virasoro and affine Kac-Moody algebras to several dimensions, and construct interesting classes of representations. On the surface this looks impossible, in view of two no-go theorems:

1. The diffeomorphism algebra has no central extensions except in 1D.

2. In field theory, there are no diff anomalies in 4D.

These two theorems are correct, but the axioms are unnecessarily strong; the keywords are "central" and "in field theory".

The standard objection is that diffeomorphisms and gauge transformations are *gauge symmetries*, which are not genuine symmetries but rather redundancies of the description. Perhaps so, but I believe that in order to understand what is described, it helps to understand the description. Moreover, even if the total diff anomaly should cancel (which I don't believe), subsystems must have a non-zero anomaly. Ironically, the prototype model here is first-quantized string theory, which can be regarded as quantum gravity in 2D. The ghost sector has central charge c = -26, so the requirement that the total central charge vanishes leads to bosonic strings living in 26D. Hence the coordinate subsystem has a non-zero central charge c = 26.

Alas, unification of the symmetry principles is not the same thing as unification of the theories themselves, and I am stuck at this point. Nevertheless, I believe that the study of mathematical objects which naturally arise in experimentally proven physics has a physical value, even if no specific predictions have yet come out of it. At any rate, what can you demand from a theory which is 20,000 man-years younger than string theory?

Here are some references. The last one is an attempt (and only that) to apply the formalism to physics.

physics/9705040
math-ph/9810003
math-ph/0101007
math.QA/0101094
math-ph/0210023

38. **JC**
August 17, 2004

I always wondered when did this "mentality" of adding in zillions of new particles and forces into a theory, become an "acceptable" practice in particle theory research?

Back in the days of Pauli, he jokingly thought that creating a new particle (the neutrino) which could not be observed, was the ultimate "cardinal sin" in physics.

In other physics fields outside of particle theory, I get the sense that objects and/or entities which have very little to no experimental basis are generally NOT taken seriously at all. Folks in many of these other physics fields seem to be a lot more skeptical of speculative ideas and theories, compared to particle and gravity folks.

39. **Chris Oakley**
August 17, 2004

Having discovered the correct unification of the symmetry principles underlying GR and QM, I think that I have strong reason for this position.

Tell us more.

40. **Thomas Larsson**
August 17, 2004

JC,

Going back to the original topic of this thread, what are your thoughts on what would have happened if Witten had never advocated nor worked on string theory in the first place. (This is sort of a "what if" scenario in an alternate world).
…
One non-obvious scenario I can think of in this "alternate world", would be perhaps a faster decline of particle theory due to a lack of many interesting problems to work on.

Maybe people would be taking general relativity and the standard model more seriously. Not only phenomenologically, but as something which might be quite close to the final answer. After all, there is no clear experimental evidence that anything more is needed. For sure, there are a few question marks, like dark matter and dark energy, but no clearcut disagreement with experiments. Besides, the standard model have shown an extraordinary ability to mend its cracks before.

There are of course two strong theoretical reasons against this idea: GR and QM are mutually inconsistent, and the SM is somewhat ugly, with 25 free parameters (used to be 19) and a totally ad hoc gauge group. This indicates (to me) that we need a chunk of new mathematics, which would make the combination of GR and QM consistent and beautify the SM, rather that loads of new physics for which there is zero experimental support. Having discovered the correct unification of the symmetry principles underlying GR and QM, I think that I have strong reason for this position.

The standard objection is of course that people thought that the end of physics was at sight already a century ago. But the situation was really different then, with many unexplained phenomena already at low energies. The impressive experimental agreement of GR and SM is true magic and mystery.

– Perhaps nobody would have even heard the word "GUT" (grand unified theory) being used after the experimental proton decay result ruled out SU(5)?

Other GUTs, especially SO(10), were quite popular already around 1980.

41. **Chris Oakley**
August 17, 2004

What would have happened if Witten had never advocated nor worked on string theory in the first place.

I think that in this scenario the Berlin Wall would never have come down, and Lubos Motl would have been a Czech Freedom Fighter. He would have been captured by now and would be doing Supergravity calculations in his prison cell to pass the time, no doubt advocating, with his familiar unwavering certainty, that Supergravity is the language in which God wrote the world.

One non-obvious scenario I can think of in this "alternate world", would be perhaps a faster decline of particle theory due to a lack of many interesting problems to work on.

There is no shortage of interesting problems to work on, and I doubt that there will ever be. But if particle physicists are so content to break the basic rules of mathematics then *of course* they will soon end up with something that is not worth bothering with.

42. **D R Lunsford**
August 17, 2004

Peter – a friend told me that the really hard part of PhD Princeton was making and firing the minature brass cannon 🙂

BTW I asked my advisor in 1984 what he thought of string theory. He was in a position to know.

"It's horseshit."

"I agree."

That was the extent of our discussion of string theory 🙂

43. **JC**
August 16, 2004

Going back to the original topic of this thread, what are your thoughts on what would have happened if Witten had never advocated nor worked on string theory in the first place. (This is sort of a "what if" scenario in an alternate world).

The obvious answer would be only a small minority of folks around John Schwarz and Michael Green working on string theory. A silly case would be Lubos Motl never being a string fanatic, but perhaps being a fanatic in something else.

One non-obvious scenario I can think of in this "alternate world", would be perhaps a faster decline of particle theory due to a lack of many interesting problems to work on.

– SUSY possibly could have been a non-mainstream topic in particle theory, if string theory wasn't there to give support to its advocates?

– The non-renormalizable 2-loop pure quantum gravity result of Sagnotti in 1985, along with the non-renormalizable divergences in higher loop supergravity could have brought a lot of "quantum gravity" research to an eventual flatlining? Perhaps supergravity would have then faded away quickly into the dustbins of physics history shortly thereafter as another failed attempt at a "unified field theory"?

– Perhaps nobody would have even heard the word "GUT" (grand unified theory) being used after the experimental proton decay result ruled out SU(5)? There would be very little motivation to look at more complicated GUTs, if there were no string theory results of SO(32) or E_8 x E_8 gauge groups to motivate further inquiries into the GUTs which were the low energy effective theories from string theory?

Can anybody else think of anything that would have either faded away and/or would have never been that popular if string theory was never popular in the first place?

On the surface, it seemed like string theory opened up a huge "treasure trove" of new exciting problems to work on at the time, while stuff like GUTs, supergravity, etc … were falling out of favor.

44. **JC**
August 16, 2004

The book "Disciplined Minds" reads like a semi-serious study of "Dilbert-ism", with some reactionary "Marxist" sounding overtones.

45. **Thomas Paine**
August 16, 2004

Sorry, forgot to give the URL of the book mentioned in the previous post:

http://disciplined-minds.com/

46.
August 16, 2004

Since the discussion here steered towards the qualification exams in Physics, thought I would recommend the book "Disciplined Minds", by Jeff Schmidt, who got his Ph.D. in physics from UC Irvine. He devotes considerable number of pages to the physics quals in the states. Has some thoughts similar to ones discussed here.

47. **Thomas Larsson**
August 16, 2004

I can boast that I made an attempt to learn string theory before the revolution. In the summer of 1984, I spent some time in the library doing leasurly reading, trying to learn about things that seemed cool. Among other things, I spent some time with John Schwartz' 1982 Physics Report. Alas, this stuff was way above my head at the time. Besides, I was specializing in statistical physics, and particle physics and gravity were only side interests.

Instead I spent most of the summer reading Mandelbrot's Fractal geometry of nature, which was at a more appropriate level. What I didn't realize at the time was that many interesting fractal dimensions can be computed with string theory methods. The catch is that it only works for planar graphs, essentially because you can regard the plane as a string worldsheet.

48. **Chris Oakley**
August 15, 2004

Maybe Mrs. Thatcher got to hear of Salam's comment (as you can imagine, a complete anathema to a grocer's daughter) as 1984 was about the time that she drastically reorganised the way in which the UK contributes to CERN, treating it as a standard part of the science budget rather than a special case.

49. **Thomas Larsson**
August 15, 2004

I remember that Weinberg was in Stockholm in December 1984 (as a former Nobel laureate, he has a permanent invitation to the Nobel party, and he usually shows up when one of his old buddies gets the Prize, which makes him a rather frequent visitor; in 1984 it was Rubbia and van der Meer). Anyway, I remember him predicting that one of the young bright string stars would win the Prize in 1991. Well, 1991 came and went, and the leading string theorists are perhaps still bright but definitely no longer young, and their Nobel prizes seem more distant than ever.

Btw., Salam was also in the panel, and he said something which struck me as one of the most foolish things I ever heard. He said that society should be grateful to its big spenders, like experimental particle physics, because as society becomes richer it will run out of things to spend its wealth upon. I'm still waiting for a politician to complain about resources being too plentiful.

50. **Maynard Handley**
August 14, 2004

> Chris,
>
> Seems a lot of the sociology behind physics research is along the lines of "monkey see, monkey do" or a "pied piper" luring away kids. Arguably one can see that in many other activities in the world, as in the "copycat" syndrome when something very popular and/or profitable comes up.
>
> At times I wonder how much of the string and particle community is exactly that of "monkey see Witten's work, monkey tries to do Witten's work".

……………………………

Especially pertinent to this is this
http://www.johnkay.com/society/348
which reviews a new book by J K Galbraith on the nature of conventional wisdom in finance.
Physics likes to believe that its practitioners operate outside the rules of normal human psychology and sociology, something that I've not noticed as especially true.

51. **Chris Oakley**
August 14, 2004

JC,

I think that you've grasped my essential point. This is not to say that there should be *no* bookwork, it is just that the majority of the marks should be for the problem, which it should not be possible to answer unless one has actually *understood* the material. Call them silly if you like, but the examples you give are exactly the kind of thing I had in mind.

52. **JC**
August 14, 2004

Chris,

If you were the professor making up some of these particle theory or gravity exam papers for the tripos III, what sort of trick problems would you put on it that would require some ingenuity and/or lateral thinking (which are not of the "$500 parrot" or "regurgitation" variety)?

If I didn't know any better, the only "trick" questions I can think of offhand would be mostly hypothetical "what if" types of problems. Perhaps silly ones like:

– "Imagine if the gluon is not a gauge field in qcd, but is really a scalar field with an SU(2) symmetry in the scalar fields. (a) Write down a possible renormalizable "scalar qcd" Lagrangian and the Feynman rules from it. (b) Calculate various tree level quark-antiquark scattering amplitudes. etc … etc …"

– "Imagine if parity is really conserved in the weak interactions. (a) How would this change the V-A interaction picture? etc … etc …"

– "Imagine if in the Standard Model, the up, down, strange & charm quarks all had the same identical non-zero mass, while the top & bottom quarks have the same identical mass but heavier than the masses of all the other quarks. (a) Are there any new symmetries, which are not present in the regular Standard Model? (b) Write down the tree level electron + positron –> quark + anti-quark scattering amplitude. (Electron mass can be taken as "small" relative to quark masses). etc … etc …"

– "Imagine if QCD had a gauge group of SU(7) and 23 vertical particle families, instead of the normal QCD SU(3) gauge group and 3 generations of vertical particle familes in the Standard Model. (a) Write down the Feynman rules. (b) What is the new beta function, and how does it compare to the normal QCD Standard Model case? etc … etc …"

– "Imagine if the electroweak gauge group is really U(1)xSU(3) instead of U(1)xSU(2). (a) Is there a Higgs breaking pattern which produces a massless photon? (b) If there is a massless photon in part a, what are the other gauge bosons? If there is no massless photon, what are the gauge bosons anyways after Higgs breaking? etc … etc …"

I just made these up out of thin air. Not really all that brilliant or even that insightful, when all I did was just do things like change the group structure and/or properties of various particles.

Anybody else have a better criteria for particle physics and/or gravity problems which require some ingenuity and/or lateral thinking, besides just copying problems out of some old long forgotten research papers from many years ago?

53. **abc**
August 14, 2004

Recent princeton math quals are here: http://www.math.princeton.edu/graduate/generals/

A click on a name gives a summary of that session. Some are quite fun to read actually: "Can you give a heuristic proof of Fermat's Last Theorem? — this last one was asked by Aizenman, the physicist!! Wiles and Faltings seemed only slightly amused." 😉

Reading the begining of David Nadler's one, it looks like the level did go down a bit as compared to some decades ago…

Harvard is here: http://www.math.harvard.edu/graduate/quals/

I couldn't find the physics ones.

54. **Chris Oakley**
August 14, 2004

Hello JC,

If you do mathematics research (including mathematical branches of theoretical physics) at Cambridge then getting a "distinction" in their part 3 exam is more or less a requirement. In my year there were about 56 of us taking part 3 with "applied" options and about the same number taking the "pure" options. Of the "applied" group, about 13 got distinctions, although not all of them stayed to do PhDs in Cambridge. The more informal structure you discuss with assignments, etc. was not possible because of the competition involved. Having said that, a small part of the course was an "essay" (mine was on SU(5)), but most of the marks were based on 15 hours in the exam room. Outside of Cambridge, part 3 is largely ignored. The first year of a graduate course achieves the same purpose elsewhere and arguably more efficiently as without the need to photographically memorise lecture notes one can concentrate on understanding rather than rote learning. I remember being at interview at Sussex University and Tony Leggett telling me that Cambridge expected the rest of the nation to await the results of the part 3 exams before awarding research places, but their time scales were such that they were unable and unwilling to do this.

One advantage of a parrot test is that it is easier for the examiners. If you pose a question that can only be correctly answered if the student has understood the material, as opposed to having just memorised his/her lecture notes, then fifty students will give fifty widely different answers and a lot of time will be spent by the examiner chasing blind alleys in the responses, just to make sure that they *really were* blind alleys.

The other problem is that the more advanced the course, the smaller the gap between examiner and student, and therefore the smaller the right of the examiner to judge.

Cambridge is cynical about this: the message is just this – doing well in our memory test is a necessary hurdle and if you don't like it, then just go elsewhere.

BTW: I am not sure I agree with the point about there being a limited number of bite-size questions one can ask in a graduate exam. It may just be that more ingenuity/lateral thinking is required.

55. **JC**
August 13, 2004

Peter,

How recent and widespread has it been for math departments to move away from the comprehensive/preliminary exams tradition, to just students passing a certain number of courses? Are there any particular strong reasons for this change?

Over the years, I remember overhearing several older physics professors talking about how even the Princeton and Harvard comprehensive/preliminary exams for PhD students, have been getting "watered down" and "easier" or "less demanding" over the years. Has this sort of thing been happening too over the years in math PhD programs?

I remember several professors I knew who did their math PhDs at Harvard in the 60's and early 70's. They all mentioned that the math comprehensive/preliminary exams at the time were really demanding and "killer". (Whether they were exaggerating things deliberately, I don't know). They said that it was common for many students to quit grad school by the time they completely passed all of their exams, largely due to reasons like burnout and/or a subsequent loss of interest in math grad school. They thought that it was the math department's way of weeding out as many students as soon as possible, and that the folks who didn't flunk out or quit were the "cream of the crop".

56. **JC**
August 13, 2004

Chris,

Was there any purpose and/or advantage in doing the tripos III exams? Did the doctorate program at Cambridge or other British universities require it as a prerequisite? Was there a thesis and defense required for the tripos III?

On the surface, for stuff like quantum field theory, particle theory, general relativity, etc … it seems like it would be more practical if the course material was done as assignments and perhaps a final take-home exam and/or an oral exam. Except for the more "trivial" quantum field theory and particle theory problems, there doesn't seem to be many "insightful" problems that are doable in a timed 3 or 4 hour examination setting (ie. problems that are more insightful than the "regurgitation" or "$500 parrot" variety).

I remember in one graduate course I took which covered things like scattering theory, second quantization of the Maxwell field, Dirac equation, etc … (the sort of stuff covered in Sakurai's "advanced quantum mechanics" book), we were given a take-home exam which had one really long nasty calculation to do. It turned out it took most of us around 40-50 pages to do this calculation from start to finish, for which we were given a week to finish it. The professor didn't think a timed 3 or 4 hour exam format was practical for this particular course. All other courses I took afterwards like quantum field theory, particle theory, supersymmetry, Lie group theory, etc … were mostly just assignments and sometimes an oral exam or presentation. In fact many of the course assignments were just "optional" and the professor frequently didn't even care whether anybody handed them in or even did any of them. A few courses didn't even have any assignments or exams. At that level the professors just assumed the students had enough self-motivation to figure things out on their own, and didn't feel that it was worth their time to make up assignments for the students.

57. **Chris Oakley**
August 13, 2004

Part 3 was the closest to an "arts" exam in a science subject that I have ever experienced.

I.e. mostly short questions, which required long answers which were entirely bookwork.

E.g. Q. 2 of "Advanced Quantum Field Theory", 1981

"Explain how the Higgs mechanism works in the Salam-Weinberg model, including a brief discussion of the physical motivation for the structure of the symmetry in the model."

You can give a "perfect" answer to this just by memorising your lecture notes, and that is what I think they wanted you to do. A $500 parrot (see below) would be able to do this.

I chose to actually digest the information and produce an answer which may or may not have corresponded to my lecture notes. Maybe that is why I did not do especially well.

58. **JC**
August 13, 2004

I came across Cambridge's web site that has old copies of the mathematics tripos III exams.

http://www.maths.cam.ac.uk/ppa/PartIIIyy.html
http://www.maths.cam.ac.uk/ppa/

The quantum field theory and particle physics exam papers look like they're just generic problems almost straight out of field theory books like Peskin & Schroeder or Ryder, and particle physics books like Griffiths or Halzen & Martin. The general relativity and comology exams look like they're just generic problems out of various books like Weinberg, Wald, etc … and other GR and cosmology textbooks.

To top it off, there's even exam papers for supersymmetry and string theory! It looks like they aren't much more than the sort of problems and calculations straight out of the Polchinski and Wess & Bagger textbooks.

Though if I was writing any of these relativity and/or particle theory related tripos III exams today, I think it would be a matter of me being able to write out the solutions as fast as I can in 3 or 4 hours. Whether or not I (or anybody else) actually learned anything, is another question.

I've always noticed over the years that folks doing very well on physics and/or math exams, has very little correlation to their understading of the subject in general. I knew one guy that did very well in all his undergrad and masters level math and physics courses, but it turned out he was good at memorizing many solutions and guessing at answers, but not really understanding what he was doing. When he started doing research, he literally slammed into a wall and didn't know what to do, like a "deer in the headlights". He ended up dropping out of grad school without even starting on any research projects.

59. **Peter**
August 13, 2004

I don't know much about what is going on in physics departments these days, but the math department here has moved somewhat away from this kind of system towards one where the students just have a pass a certain number of grad courses.

When you look at exams of this kind, you realize that most physics problems are either basically trivial and can be solved in a couple lines, or quite difficult and would require several days of work to solve correctly. There's only a limited number of non-trivial problems that can be solved in a half-hour or so, these end up being used over and over again, often creatively dressed up to look different.

60. **JC**
August 13, 2004

Peter,

Sounds like the same sort of comprehensive exams I remember having to write in graduate school.

Years later I came across several books that were a compilation of problems from exams of this sort: one from was University of Chicago, another was from Princeton, and two additional books looked like they were from Stony Brook. There was a further series of books that looked like they were compiled in China, from many graduate level comprehensive/preliminary exams from many American physics PhD programs.

Over the years I saw many other physics comprehensive/preliminary exams of this sort from many other universities. It seems like many of these exams (even recent ones from Princeton), were recycling many of these problems every 15-20 years. Not too many really "original" looking problems that stood out over the years.

I always wondered what the origins of these sorts of exams are, and whether they are still just a "custom" that is done to this very day. Several physics PhD programs don't even have these exams anymore! Only other reason I can think of for them still being done today, is as an easy way of weeding out the weaker and/or lazy students.

I wonder if these sorts of exams are also commonly done in physics graduate programs overseas, say in Britain, Japan, and continental europe. One case I can think offhand would be the notorious series of Landau exams which Laudau used as a prerequisite for entry into his research group. Another set of exams that looked similar on the surface to the american preliminary/comprehensive exams, would perhaps be the "tripos" series of exams at Cambridge. Anybody know more about these Cambridge "tripos" exams, and how they compare to the comprehensive/preliminary exams done in American universities?

61. **Peter**
August 13, 2004

Actually a book has been published containing many of the problems from these exams, on Amazon you can even see the table of contents. It is called "Princeton Problems in Physics"

From what I remember, the topics covered on the prelims were mechanics, EM, quantum mechanics and stat. mech., at the level of a standard first year graduate course. The generals covered solid-state physics, special and general relativity, nuclear physics, particle physics and atomic physics. No QFT. These were exams everyone had to take, both experimentalists and theorists, so they weren't heavily theoretical.

62. **JC**
August 13, 2004

Peter,

What exactly was covered in those preliminary and general exams that physics grad student had to pass in their first and second years at Princeton? Would it be stuff at the level (or slightly higher) of Jackson (E&M), Goldstein (classical mechanics), Merzbacher and/or Sakurai (quantum mechanics), etc …? Was anything like quantum field theory covered (ie. at the level of Peskin & Schroeder, Ramond, and/or Weinberg's books)?

63. **Peter**
August 13, 2004

I don't know how much physics Witten took as an undergrad at Brandeis, but he majored in history and minored in linguistics. His father, Louis Witten is also a physicist, does general relativity, so he probably learned some physics early on. After Brandeis he seems to have spent a little time as a grad student in economics at Wisconsin, and worked on McGovern's 1972 presidential campaign. He enrolled at Princeton in the applied math program, which allows you to work in many different departments, then ended up shifting over to physics.

Princeton has a very serious set of preliminary and general exams that grad students have to pass in their first and second year. I certainly learned a lot of physics studying to pass those exams, and I'll bet Witten did too.

Up until the whole string theory thing, it's really clear why Witten was so respected. His papers are marvels of clarity compared to most people's, and many of them contain remarkable insights into the still mysterious problems of how to understand QFT non-perturbatively. There's no mystery at all about why many physicists and mathematicians think so highly of him, his intellectual accomplishments are just huge, even if they are not to everyone's taste. His impact on mathematics has also been huge, he richly deserves the Fields medal he got.

One sad thing about the dominance of string theory is that it has so overshadowed his best work. Within string theory he has done quite a lot, but you could argue that others have done more important work there than him (Green, Schwarz, Polchinski, Maldacena).

64. **Chris Oakley**
August 13, 2004

A typical reaction to my research work between 1984 and 1987 was the following: "You are wanting to abandon a theory that has been verified to eleven places of decimals. The burden of proof is therefore on you. You must at least deliver results to the same degree of precision before you can expect anyone to sponsor you."

Whether the anomalous magnetic moment, etc. can be calculated to the required accuracy in my approach is still an open question. No-one has been working on it. On the other hand the approach works in obtaining basic scattering amplitudes and is content to reside in 3+1 dimensions.

In regard to Superstrings, there is *certainly* no guarantee that the standard model or even, it seems, general relativity will be reproduced (despite the fact that producing a quantum theory of gravity was part of the original motivation), and yet it seems to get pretty much all the sponsorship.

I call this inconsistent, and I call it stupid.

I was a bit more pig-headed than average, but I wonder how many other young people (as I was then, anyway) have had good ideas quoshed because Ed Witten or whoever else who happened to decide the direction of research did not think it was cool.

65. **JC**
August 13, 2004

Chris,

Seems a lot of the sociology behind physics research is along the lines of "monkey see, monkey do" or a "pied piper" luring away kids. Arguably one can see that in many other activities in the world, as in the "copycat" syndrome when something very popular and/or profitable comes up.

At times I wonder how much of the string and particle community is exactly that of "monkey see Witten's work, monkey tries to do Witten's work".

66. **Chris Oakley**
August 13, 2004

The Ed Witten phenomenon reminds me of a joke:

A man goes into a pet shop to buy a parrot.
"How much is that one?" he asks.
"Five hundred dollars," says the sales assistant.
"*Five hundred dollars!?* for a *parrot*?"
"Ah yes, but she can touch type, and take dictation."
"And what about this one?"
"Seven hundred dollars."
"*Seven hundred?* That's ridiculous!"
"I know that sounds a lot, but this parrot is fluent in three languages and knows how use Word and Excel."
"And this one?"
"A thousand dollars."
"A *thousand*. What does he do, then? Write operas?"
"Actually we don't know what he does. But the other two call him 'Sir'."

When I was a graduate student (1980-1984) it was no exaggeration to say that Ed Witten was the single most influential mathematical physicist. Although I am not sufficiently *au fait* with his work to say whether this was justified, I would venture the observation that the emphasis was, and remains *mathematical*, i.e. pretty mathematical constructs appeared to excite him more than having the theories make contact with reality. It was the – in my view – excessively theoretical nature of his work that put me off joining the bandwagon.

67. **JC**
August 12, 2004

Peter,

I remember hearing stories about Witten majoring in something like history or political science when he was an undergrad. Did he take any physics courses at the time? If not, was he literally a "self taught" guy who taught himself everything he needed to know about undergraduate and graduate level physics, by cranking out zillions of problems and reading some papers on his own?

68. **Peter**
August 12, 2004

Witten was very clearly the leading figure in particle theory before string theory, during the period 1980-84. It's because of this that the string theory idea took off so fast. By 1984 everybody was looking very carefully at anything Witten was doing and often immediately going to work on it themselves. It's very hard to over-estimate his influence at that time.

He got his Ph.D. in 1976, and very much impressed the faculty when he was a student. I first met him
when he was a post-doc at Harvard in 1978, and was just starting to make his reputation. By 1980 he was already a young star and Princeton offered him a tenured professorship even though he was only a post-doc.

69. **JC**
August 12, 2004

Peter,

How popular was Witten in the particle theory community before string theory got popular? I knew someone who knew Witten personally in the 1970's before he was a superstar. In those days he thought Witten was definitely above average, but he didn't think was a superstar or Einstein-like genius at the time. If nothing else, he thought that Witten could have at least gotten an assistant professor job at a place like Princeton or Harvard in those days.

Correction

August 17, 2004
Categories: Uncategorized

Jeff Harvey sent in a comment correcting me on a point of history in my last posting. I'd read somewhere about Green and Schwarz fed-exing their paper to Witten, and had assumed this was their idea. Harvey, who was at Princeton at the time, recalls that it was Larry Yaffe who brought news of Green and Schwarz's result from the Aspen Workshop to Witten at Princeton, and Witten was the one who asked Green and Schwarz to fed-ex him the paper.

Harvey also strongly disagrees with the statement that people were not impressed by the work independently of Witten. He was at Princeton at the time, so knows far more about what attitudes there were. For those who weren't at Princeton or at Aspen, news of Green and Schwarz's paper and Witten's arrived more or less at the same time (they were published in the 13 and 20 December issues of Physics Letters B, the preprints were circulating in October). At that time any new paper by Witten was a major event, especially one in which he took up a new topic. I stand by my recollection that for people I was talking to at the time, the fact that Witten was working on the subject overshadowed the Green-Schwarz result itself.

Update:

This morning I tracked down my original source for what happened at Aspen. It is John Schwarz's article entitled "Superstrings – a Brief History", published in the proceedings of a conference on the history of particle physics held at Erice in 1994. The volume is entitled "History of Original Ideas and Basic Discoveries in Particle Physics" and contains many things very much worth reading. Schwarz describes in detail what happened at Aspen, ending with the following remarks:

"But still, given our previous experiences, neither of us had any idea of how sudden and enthusiastic the response of the physics world would be. In my opinion this was largely due to the influence of Edward Witten, who immediately grasped the implications of our result. Without that, string theory would probably have emerged much more gradually. As soon as our letter came off the printer we sent Witten a copy by Federal Express. (This was before the days of TeX and email!). I am told that the next day everyone in Princeton was studying it. Our letter on anomaly cancellation was submitted September 10, 1984. The deluge began 18 days later with Witten's letter suggesting ways to compactify SO(32) superstrings to get anomaly-free theories in four dimensions."

This is what led me to believe that it was Schwarz and Green's idea to Fed Ex Witten their paper. Presumably Harvey is right that he had asked them to do this. But that's about the only thing that I think I got wrong in the original posting.

Comments

1. **JC**
   August 21, 2004

   Anybody have any good guesses as to how physics could have turned out in an alternate world, if George McGovern had won the American presidency in 1972 against Nixon, and Witten became a presidential speech writer and/or got a low level post in the McGovern administration?

2. **Peter**
   August 17, 2004

   I don't think there ever has been anyone quite comparable to Witten. Einstein never had the same sort of influence, and he was always kind of working off by himself, whereas Witten has always been very much in the mainstream of what the majority of the field is doing.

3. **D R Lunsford**
   August 17, 2004

   All this hero worship is missing the point. String theory *as physics* was a ridiculous idea at face value. I don't know how I knew this, or how my advisor knew it, or why we were right – but my absolute initial reaction was one of total, utter disgust that such a thing would be taken seriously. Now, in the course of learning a lot of things that were complex, weird, or both, I *never* had this kind of visceral reaction to anything but string theory. The idea of setting the pointer of physics back to before Democritus was, in itself, ridiculous on the face of it, without further consideration. The world could *never possibly* have been arranged like that.

   My question is – why is this aspect of physics – that is, intuition – practically ignored? Physics is not mathematics, it's not even really *similar* to mathematics. Physics is about the actual world and its patterns. To get at these patterns, you need something other than the ability to crank out vast amounts of math, be it ever so interesting.

   rant

   Something pathological got into physics, and I would like to know when and where. Note that if you succeed in demolishing the entire string society, you'll still be stuck with dark matter, inflation, the first three minutes, Euclidean continuations, wave functions of the universe etc. etc. etc. String theory in itself is not the problem – it's a kind of uncritical global credulity that affects all intellectual endeavor.

   /rant

   -drl

4. **JC**
   August 17, 2004

   Peter,

   Can you think of anybody else in recent times, who had the sort of influence Witten had over the last 25+ years, in the sense of "monkey see Witten, monkey tries to do Witten's work"?

   I can't think of anyone offhand in physics history, other than maybe Einstein.

5. **Peter**
   August 17, 2004

   Without Witten, I think Green and Schwarz would have slowly gotten more attention for superstring theory, partly because no other ideas were really working. Their anomaly cancellation result would have helped get some attention, but on nothing like the scale that Witten's interest generated. It might still be percolating along as a subject with a relatively small group of people working on it.

It's very hard to guess what would have happened to particle theory without the dominance of string theory. Witten did amazing work relating QFT and mathematics during the late 80s and early 90s that was completely independent of string theory (including the work that got him the Fields medal) and presumably he would still have done this. My own prejudice is that if he hadn't put so much effort into string theory, and instead had put it into the new insights into QFT and math that he was getting, he and others following along with him would have achieved even more along these lines and the field would be in much better shape.

6. **JC**
August 17, 2004

Peter,

Do you think string theory would have faded away with a whimper or would have died a painful death, if Witten never became a huge advocate of string theory? It sure seemed like nobody really paid attention to Schwarz and Green for more than a decade, when they were working on string theory in obscurity during the 1970's and early 1980's.

7. **Peter**
August 17, 2004

Hi JC,

Particle theory in general has always been quite faddish, although often the fads have been driven by a new experimental result, or by the appearance of a good explanation of some previously unexplained experimental data. I don't know of any previous example in the history of physics of the whole field swinging so quickly behind a very speculative idea without experimental backing.

't Hooft's work certainly generated interest in the Weinberg-Salam model, which had been pretty much ignored until then. It was quickly followed by asymptotic freedom in 1973 and the "November revolution" in 1974 when the J/Psi was discovered. The standard model did quickly fall into place during 1971-74 and then dominate particle theory, but this was a series of related theoretical discoveries finding quick experimental support, a situation much more comparable to QM in 1925-26 to string theory in 1984.

8. **JC**
August 17, 2004

Peter,

Can you think of any other scenarios in the past which had tons of people jumping onto the same bandwagon after a particular paper was published, similar in spirit to how string theory became very popular after the Schwarz-Green result?

One that I can think offhand would be perhaps be the 't Hooft result which showed that gauge theories are renormalizable. Perhaps other cases would be the BCS superconductivity paper, or going as far back to the original Heisenberg & Schroedinger papers on quantum mechanics along with Wolfgang Pauli's review paper on the subject.

Smolin and Susskind at the Edge

August 19, 2004
Categories: Uncategorized

John Brockman at his "Edge" web-site has put up an exchange between Smolin and Susskind about the "multiverse" and the anthropic principle. This includes the page and a half paper by Susskind that was rejected by the arXiv. Susskind seems quite willing to give up the idea that a physical theory should be falsifiable, so his response to Smolin's argument that his use of the anthropic principle is not falsifiable is basically "Yeah, and so what?".

To Smolin's claim that non-falsifiable theories aren't really science, Susskind answers by listing several prominent physicists (Weinberg, Polchinski, Linde, Rees), their titles, affiliations and prizes they have won. He then announces that since these prominent people agree with him and think the anthropic principle is science, Smolin should just shut up. I don't know about other people, but one reason I went into physics was that it was supposed to be a subject where issues could be decided by rational argumentation, not appeals to authority. That doesn't seem to be the case anymore.

Comments

1. **serenus zeitblom**
   August 22, 2004

   "To the extent that this is supposed to be taken as "scientists have little to learn from philosophers, and philosophers have much to learn from scientists" it is simply false"

   In general, I agree. But there are bad philosophers just as there are bad physicists. On this *specific* issue, namely declarations that things we cannot see [yet] are "unscientific", too many physicists tend to quote bad philosophy. The example of quarks is canonical. I really liked Susskind's
   "Throughout my long experience as a scientist I have heard un-falsifiability hurled at so many important ideas that I am inclined to think that no idea can have great merit unless it has drawn this criticism". I think that hits the nail on the head.

2. **Chris W.**
   August 22, 2004

   Susskind said: "Let's not put the cart before the horse. Science is the horse that pulls the cart of philosophy."

   To the extent that this is supposed to be taken as "scientists have little to learn from philosophers, and philosophers have much to learn from scientists" it is simply false, in light of the entire history of physics. Of course it probably expresses the attitude of most of the last 2-3 generations of physicists, and I have seen a statement by Witten to the effect that he and most of his colleagues see little to interest them in recent work (of the last two centuries?) in the philosophy of science.

   My point is that no one seriously interested in a problem like quantum gravity can afford to adopt the narrow perspective of a specialist, or to be complacent about questions of epistemology. John Wheeler has cautioned against any physicist adopting this attitude, although given the vastness and technical complexity of the field, and the demands of modern research as a career, it is perhaps inevitable that such attitudes will be fairly widespread.

   The contrast in outlook between Smolin and Susskind on this issue is quite obvious.

3. **serenus zeitblom**
   August 20, 2004

   I must reluctantly confess that I found Susskind surprisingly reasonable. Surely he is right when he says "Good scientific methodology is not an abstract set of rules dictated by philosophers. It is conditioned by, and determined by, the science itself and the scientists who create the science." It is good to see somebody explicitly refuting people who drone on about how things beyond the horizon can never be seen, hence theoretical efforts to determine what is there are "not science". What crap! On the other hand, S does fall right off the cliff when he claims "the black hole controversy has largely been resolved." The only question here is whether Susskind really believes such blatant nonsense.
   The other strange thing was that Susskind was so polite. He only starts to sound like Lubos Motl right at the end. In fact, right at the end he sounds so much like LM ["Smolin's tendency to set himself up as an arbiter of good and bad science"] that it is now clear to which organ-grinder the monkey belongs.

4. **JC**
   August 20, 2004

   Thomas,

   What do you think of the CFT book by Ketov?

   Lately I've been slowly making my way through the book "conformal invariance and critical phenomena" by Henkel.

5. **Thomas Larsson**
   August 19, 2004

   Danny:

   Unfortunately I don't really have any good introductory references to CFT, since I picked up most of it from the original literature around 1986-1990. Conformal field theory by Di Francesco, Mathieu and Senechal is the canonical reference, but its intimidating thickness makes it more suited as a reference rather than as a tutorial.

   One probably also needs some general background on phase transitions, statistical lattice models, and renormalization in that context. The review series edited by Domb and Green, and later Domb and Lebovitz (or maybe Green and Lebovitz – anyway, one of the original editors died) is a classic, but maybe out of print. A more recent textbook is Scaling and renormalization in statistical physics by Cardy. It has the advantage that its size is manageble.

6. **D R Lunsford**
   August 19, 2004

   JC –

   Beavis and Butthead know their limitations. Uh huh huh, yeah. Shut up, Beavis!

(Seriously, there are lots of interesting *physics* questions that lofty fartknockers such as S&S might hold forth on at length.)

7. **JC**
   August 19, 2004

   D R Lundsford,

   How much is this different than two idiots like Beavis and Butthead arguing which rock music bands “Rule” (ie. Metallica, AC DC, etc …) and which rock bands “Suck” (ie. Billy Idol, U2, Boy George, etc …)?

   Or for that matter discussions between fans of various sports teams (ie. basketball, baseball, hockey, etc …) as to which teams “Rule” and which teams “Suck”?

   This seems to be very much a human thing that is independent of the field of interest or inquiry, whenever there’s disparities in opinions which are not backed (nor ruled out) by empirical data and/or the questions asked are too vague.

8. **D R Lunsford**
   August 19, 2004

   Thomas –

   To Glashow’s credit, he threw off his rope belt and robe and re-entered the mundane world:

   http://www.pbs.org/wgbh/nova/elegant/view-glashow.html

   And that 1988 paper on CFT by Ginsparg you linked is mighty interesting 🙂 Do you have a list of favorite references on this topic?

9. **Thomas Larsson**
   August 19, 2004

   “Is further experimental endeavor not only difficult and expensive but unnecessary and irrelevant? Contemplation of superstrings may evolve into an activity as remote from conventional particle physics as particle physics is from chemistry, to be conducted at schools of divinity by future equivalents of medieval theologians. For the first time since the Dark Ages, we can see how our noble search may end, with faith replacing science once again. Superstring sentiments eerily recall ‘arguments from design’ for the existence of a supreme being. Was it only in jest that a leading string theorist suggested the ‘superstring may prove as successful as God, Who has after all lasted for millenia and is still invoked in some quarters as a Theory of Nature’?”

   – Paul Ginsparg and Sheldon Glashow, Physics Today, May 1986.

   Some people are true visionaries!

10. **D R Lunsford**
    August 19, 2004

    1 word – scholasticism. They both sould like two Schoolmen going round and round about theophany or some such ridiculous arcanity.

    It would be very interesting to make a close comparison of scholasticism and this pseudoscientific form of debate. One thing’s certain – physics, at least as practiced by these two, is deader than Julius Caesar.

Comments From Larry Yaffe

August 19, 2004
Categories: Favorite Old Posts, Uncategorized

Jeff Harvey's comment that it was Larry Yaffe who brought news of the Green-Schwarz anomaly cancellation result to Witten gave me the idea of contacting Larry to get a first-hand recollection of what the reaction was at Aspen back in 1984. He was a junior faculty member at Princeton at the time and I knew him since I had been a grad student there and we both were working on lattice gauge theory. I've always respected his work and had noticed that he was someone who had never joined the string bandwagon, so I took the opportunity to ask him for his views on string theory. I think they're pretty reasonable and reflect the views of a lot of the sensible people in the particle theory community these days. He agreed to let me post them here:

"What Jeff Harvey related is correct: I was at Aspen when Green and Schwarz presented their anomaly cancellation result, and I told Ed and others about it a few days later when I got back to Princeton. (Of course, John and Michael may have sent Ed a copy of their paper completely independently. I don't know about that. But he hadn't seen it yet when I was asked "what's the news from Aspen?".)

As for whether it was Michael Green or John Schwarz who gave the seminar in Aspen, I think it was John — but I'm not 100% sure. (The different talks I've heard from John and Michael get mixed up in my memory.)

Concerning reaction to the Green-Schwarz result, my recollection is that there was relatively little immediate buzz about it at Aspen. John had a fairly diffident style of presentation, and I don't recall anyone jumping up and saying 'this will change the course of physics!'. As best as I can reconstruct my own reaction, it seemed like a technically slick calculation and a nice result but it wasn't, of course, addressing any of the conceptually hard questions about quantum gravity, and it seemed very far removed from the practical concerns of particle physics. But the reaction back in Princeton was different: Ed certainly saw the significance immediately and I think others did as well (certainly quicker than I did). I think the speed with which others in the particle theory community jumped into string theory had a lot to do with Ed's involvement and proselytizing, but I expect that even without his involvement, interest in string theory would have steadily grown, albeit slower.

Since you asked about my views on string theory, I'll try to give a summary. I think it is clear that:

String theory has been wildly over-hyped by some people. Even calling it a 'theory' is really a misnomer, given the lack of any adequate non-perturbative definition of string theory.

String theory has not yet made any convincing connection with the world we live in.

The predictive power (or the falsifiability) of string theory leaves much to be desired, especially in light of the emerging picture of the landscape of string theory vacua.

But at the same time:

The oft-repeated argument that string theory is the most promising framework we have for combining quantum mechanics and gravity remains true. Even though there is no real non-perturbative definition of string theory, I don't think one can dispute this assertion. (As an aside, so-called "loop quantum gravity" is an interesting one-parameter family of statistical mechanics models, but has not been shown to have anything to do with gravity. Does it have a large-volume limit? Does it have long distance dynamics described by some effective field theory plus classical GR? Who knows…)

The perturbative consistency of string theory, combined with all the consistency checks of the (largely unproven) web of duality relations, are compelling hints that there is something deep and meaningful to string theory, even though it remains poorly understood.

String theory has made remarkable contributions to mathematics, allowing previously unforeseen connections to be found between very different areas. This has shown up in new (provable!) results in enumerative geometry, Gromov-Witten invariants, mirror symmetry, etc.

String theory has given partial insight into a few conceptual questions involving quantum gravity, such as (the absence of) black hole information loss, via the connection between BPS states and extremal black holes.

Improved understanding of gauge theories, especially strongly interacting theories, is emerging from string theory via "gauge-string" (or AdS/CFT) duality. Understanding is, as usual, frustratingly incomplete, but I think the message that non-gravitational ordinary field theories, and higher dimensional theories containing gravity, can be different representations of the *same* physics is revolutionary, and hints at some synthesis we are far from understanding. I think this point is already somewhat lessening the split in the theory community between 'string theorists' and 'non-string theorists'.

Personally, I find this last point the most compelling reason to be interested in string theory, despite its lack of experimentally testable predictions. It is, of course, a matter of personal taste whether the 'pro' reasons to work on string theory outweigh the 'cons'. Some people are comfortable working on an intellectual enterprise whose connection with the real world may never emerge during their lifetime. Some people aren't — and that's fine."

Comments

1.
   August 20, 2004

   I'm just correcting your misapprehension about the mentioned work. I've done the pro- anti- string stuff way too often to have any desire to get into it now.

2. **Thomas Larsson**
   August 20, 2004

   Aaron,

   I didn't claim to have a very good understanding about algebraic geometry. This was how I understood one of Witten's original papers, and he didn't say which xxx-Witten invariant he described there.

   But my main point was really that no mathematical discovery, however cool, is by itself evidence that this math has anything to do with physics. This is true for various invariants and dualities in algebraic geometry, as well as the higher-dimensional generalizations of Virasoro and affine algebras and their global group generalizations.

   For me to accept something as physics, something more is needed: experimental support. It seems to me undeniable that the groups of diffeomorphisms and gauge transformations play a significant role in experimentally confirmed theories. I don't see such a role for various dualities, Gromov-Witten invariants or mirror symmetries. But maybe its just me being too skeptical about hidden worlds.

3. **Aaron**
August 20, 2004

You're understanding isn't correct. I think you might be getting at issues in topological field theory where the correlation functions are independent of the metric. Donaldson-Witten theory is a twisted N=2 SYM, for example.

Gromov-Witten theory, mirror symmetry and the like come from topolgical versions of string theory where the worldsheet CFT is twisted into a topological theory.

4. **Thomas Larsson**
August 20, 2004

String theory has made remarkable contributions to mathematics, allowing previously unforeseen connections to be found between very different areas. This has shown up in new (provable!) results in enumerative geometry, Gromov-Witten invariants, mirror symmetry, etc.

My impression is that this has very little to do with physics, and not really that much with string theory neither (unless you define string theory to be whatever Witten does). My very limited understanding of these matters is that some correlation functions in N=4 SYM turn out to be independent of separation. This has two consequences:

1. These correlators are smooth (Gromov-Witten?) invariants of the underlying four-manifold.

2. We may freely move the points to convenient positions, probably very close to each other, where the correlators can actually be calculated.

This is wonderful in enumerative geometry, because it gives us calculable smooth invariants which you can use to prove a lot of theorems. But exactly the same properties seem to be rather useless in physics, where we usually want correlation functions to decay with distance.

5. **Tony Smith**
August 20, 2004

D. R. Lunsford asked whether my string = worldline model does such things as
"eliminate time", etc.
It does not, in my opinion. To see how it works in sufficient detail to answer
such questions, please read the entire CERN CDS preprint EXT-2004-031 at
http://cdsweb.cern.ch/search.py?recid=730325&ln=en

The spirit of the model is similar to that proposed by Elitzur and Dolev
in their paper at http://xxx.lanl.gov/abs/quant-ph/0207029
where they say:
"… we propose quantum mechanical experiments that yield inconsistent histories,
suggesting that not only events but also entire histories might be governed
by a more fundamental dynamics. …".

The basic idea is that each "entire history" would be represented by a "string".
and that the fundamental interactions of quantum theory are
not between mere point particles
but
are between strings/entire histories.

Elitzur and Dolev, in that article, describe
"… quantum mechanical experiments yielding apparently inconsistent histories …[which]… would give rise to an account like
"first a retarded interaction brings about history t1x1, t2x2, …
and then
an advanced interaction transforms this history into t1x'1, t2x'2 …"
and they say
"… Perhaps … changes affect not only events but also entire histories.
… Such a model will be better capable of explaining quantum peculiarities
of the kind described above, as well as a few other surprising results
discovered lately by similar techniques …".

Please note that the Elitzur-Dolev paper also discusses some aspects of
Hawking's recently recanted position regarding information loss in black holes,
but that aspect of the paper seems to me to be substantially independent
of their proposal that it is
interaction between entire histories (world-lines/strings)
and not
event-interactions between mere point particles
that is fundamental in quantum physics.

Tony Smith

6. **D R Lunsford**
August 19, 2004

Tony –

Doesn't the usual argument against rigid bodies in relativity eliminate your interpretation of the world-line of a particle as a "string"? If not, then you are saying that a "stringicle" acts as a unit over its entire history – and that any creation-annihilation events in its history have to be included, extending the stringicle to another, to another etc. etc. So, in effect you're eliminating time, and returning to a Eucldidean-Pythagorean static world picture without dynamics. Odd how this matches the "back to Democritus" approach implicit in the "nothing but fibers and the void" approach.

7. **Tony Smith**
August 19, 2004

Larry Yaffe says "… String theory has not yet made any convincing connection with the world we live in. …".

While that may be true of conventional formulations of string theory,
what about formulations based on unconventional physical interpretations
of strings (such as strings = world lines of point particles) from
which string theory structures such as 26-dim spacetime, D8 branes,
and discretizing by orbifolding can be used to make models such as
that described in CERN CDS preprint EXT-2004-031 at
http://cdsweb.cern.ch/search.py?recid=730325&ln=en
?
There has been a small bit of discussion about that model on sci.physics.strings,
where the model itself is labelled "Speculation" and comments range
from
".. pure numerology … coincidence …"
to
"… a nice formulation of (bosonic) M-theory (as Susskind refers to the 27-dimensional theory), from where we work down dimensionally … to recover fermions …".
The above quotes are given without stating full detailed context in order
to illustrate the range of opinions that seem to exist about the model.
Anyone who is really interested in details should read that sps thread
and the paper itself, as well as related material cited therein.

Anyhow, the basic point of this comment is that even though Larry Yaffe
may be correct that current conventional formulations of string theory
have "… not yet made any convincing connection with the world we live in …",
his statement may not be correct with respect to some unconventional string-based models.

Myhrvold on the Anthropic Principle

August 19, 2004
Categories: Uncategorized

My old friend and Princeton roommate Nathan Myhrvold has written an excellent piece about the anthropic principle and the Smolin-Susskind debate that has just been posted on the Edge web-site. It seems to me to summarize the issues very clearly.

After getting his Ph.D. in quantum gravity at Princeton, Nathan went to work as a post-doc with Stephen Hawking, and one of the topics he worked on involved a possible mechanism for explaining the small size of the cosmological constant. Nathan left physics and joined with some of my other friends from grad school days to start a software company near Berkeley that they called “Dynamical Systems”. They soon sold the company and themselves to Microsoft, where Nathan ended up in the position of Chief Technical Officer. He periodicaly reminds me that if I had taken up one of his many offers to come work with them back in the mid-eighties, I could be obscenely wealthy too. At the time I remember it seemed clear that it was much smarter to stay as a postdoc at Stony Brook, being paid enough to live on and able to think about whatever I wanted, than to go to California to work twenty hour days writing operating system code and getting paid in worthless pieces of paper.

Oh, well.

Comments

1. **Aaron**
   August 25, 2004

   The formalism computes tree level amplitudes of gluon scattering in SYM very efficiently. No conformal invariance needed. These diagrams are the same as in the conformal theory.

   For JC, the formalism reproduces the tree level amplitudes which is substantive in and of itself. Unfortunately, according to a followup by Berkovitz and Witten, it appears that the theory with loops will most likely reproduce the diagrams of conformal supergravity rather than those of N=4 SYM.

   So, it seems likely at this point that the original proposal will not reproduce all of SYM, but, again, that doesn’t mean that there will be no future results on the subject. You can read Witten’s original big paper (the first half is all QFT — no string theory) for some intriguing facts about those loop amplitudes you referenced.

   But, none of this changes the fact that the referenced paper is all about the very physical idea of doing scattering computations in Yang-Mills theory.

2. **JC**
   August 25, 2004

   Aaron,

   Do you know if anybody has been able to get Witten’s twistor formalism to work for the QCD 1-loop amplitudes and beyond?

   If Witten’s twistor formalism has any substance in the end, it should be able to reproduce the 1-loop formulas found by Dixon, Kosower, & Bern during the 1990’s.

3. **Chris Oakley**
   August 25, 2004

   How pessimistic is it to think that something in its initial stages will never be extended to compute diagrams involving things other than gluons?

   I am not claiming to be qualified to judge the details of this paper, but I would remind you that sooner or later conformal invariance must be broken as the world is quite obviously not conformally invariant. Penrose’s group worked for decades on ways of putting in masses and did not come up with anything very convincing. Maybe Witten’s team can do better, and I would be delighted if they could, but as things stand, this is a huge, and possibly insurmountable barrier.

4. **Aaron**
   August 25, 2004

   “Thanks for that. Once again we see Witten et al, given a choice between physics and interesting math choosing the latter. ”

   Interesting math? That paper gives new ways of doing computations in QCD. I consider that pretty damn physical. Don’t you think that’s at least somewhat interesting?

   You can’t compute everything using this method, but I’m really astounded that you immediately dismiss it as ‘interesting math’. Did you read the paper?

   And how pessimistic is it to think that something in its initial stages will never be extended to compute diagrams involving things other than gluons?

   Your comment frankly couldn’t be more inapposite.

5. **JC**
   August 24, 2004

   Danny, Chris, Steve

   I remember years ago stories about some younger particle theory professors attempting to teach quantum field theory (QFT) courses in a “modern” context.

   There were guys who started off with the path integral on the first day, and didn’t really mention much of the old canonical quantization way of doing things. By the end of the 1st semester, they were already finished covering numerous tree level calculations in phi^4, QED, and even some electroweak and/or QCD Standard Model stuff. Some folks even got as far as covering the renormalization of phi^4 theory or even QED in the 1st semester!

   After about a month or so into the 1st semester, almost all of the experimentalists were scared off and subsequently stopped attending the lectures and/or dropped the course. By the time it was the 2nd semester of the course, the only folks left mostly consisted of theory students interested in particle physics and/or condensed matter. Mid way through the 2nd semester, many of the condensed matter folks disappeared too.

   It seems like teaching QFT starting off with the path integral is almost like the equivalent of “culture shock” for many students.

6. **Chris Oakley**
   August 24, 2004

   Danny,

   Thanks for that. Once again we see Witten et al, given a choice between physics and interesting math choosing the latter. Twistor space cannot deal with massive particles. This is unfortunate as not only is there clear experimental evidence for massive particles, but a lot of these masses have been measured to a high degree of precision. Maybe they can have massive particles as some kind of composite, but I am not aware of any developed theory that works on these lines. Maybe someone can prove me wrong – ?

7. **D R Lunsford**
   August 24, 2004

   Attn Chris – a paper you may find interesting:

   http://xxx.lanl.gov/abs/hep-th/0403047

8. **Chris Oakley**
   August 24, 2004

   I like Weinberg QFT Vol. 1 a lot. I think that this is better than any other text book I have looked at. The later volumes I have not studied in detail, but one gets the impression that he is really trying to make sense of the material, and not just reproducing the results of other's scientific papers. One also gets this sense from Bjorken and Drell – although these books can be annoying – but precious few others. Regarding Steve's point about people not wanting to question Feynman-Dyson perturbation theory, I have the following comment: they question practically everything else! It seems however that this one thing is taboo. It is like a tower of Hanoi after ten turns – people fear that the tiniest disturbance in the air will cause the whole thing to collapse and they will no longer be able to talk about the "most accurate" or "best" theory of all time. Personally, I am not bothered as I never thought the tower existed anyway, except in people's minds.

9. **D R Lunsford**
   August 24, 2004

   Weinberg is very odd as a writer. His book on gravity goes to great lengths to establish his heterodox opinion that "gravity is not geometry", then he develops gravity as geometry in the most impeccable way. There is some of that in his QFT books.

10. **Steve**
    August 23, 2004

    I quite liked a book called "Quantum Field Theory of Point Particles and Strings" by B. Hatfield (some of it anyway). The last couple of chapters introduced perturbative string theory in a way that was much clearer to me than GSW vol 1. Ryder was also quite a good book and readable.

    All QFT books are much of a muchness though and just present the usual sequence of stuff: free scaler, fermion, em fields, commutators, then Feynman-Dyson perturbation theory, S-matrix, phi^4 theory, QED, Feynman rules, vacuum polarisation etc. Then same results via path integrals, then renormalisation/regularisation. As a student, you never really get any clear or deep explanations in any text as to why you are even doing/learning this stuff in the first place.

    I have never really enjoyed perturbation (it can also cause blindness:)or the whole Feynman-Dyson formalism. Quite tedious to learn. Amazingly, it works out though and lots of things can be computed to high accuracy in the end so students/authors don't seem to question it or some of the underlying dodgy math. It is very entrenched in the theoretical physics culture so people like Chris who have chosen in the past to question aspects of it, won't go down well with the establishment or journal editors. There is probably room for another QFT book if someone can put a new modern twist or approach on the subject with some vitality. Weinberg's volumes were good but he is very much from the old school.

    A big problem now though for the S-matrix formalism, and esp. for string theory, is you can't actually define it on deSitter space, the space on which we appear to actually reside.

11. **JC**
    August 23, 2004

    Dan Lunsford,

    I skimmed the Jauch and Rohrlich book many years ago, but didn't read it closely enough. On the surface it looked like it was written in a style and spirit similar to Heitler's book.

    In the intermediate zone of "advanced quantum mechanics" before quantum field theory (QFT), I really liked Paul Strange's "Relativistic Quantum Mechanics: with Applications to Condensed Matter and Atomic Physics" book. It seems to cover many of the same sorts problems as undergraduate quantum mechanics, except done for the Klein-Gordan and Dirac equations. It attempts to explain intuitively what's going on in various relativistic systems, which seems to be glossed over in most other "advanced quantum mechanics" books. Main thing I liked about Strange's book was that it covered many relativistic quantum problems such as bound states for various potentials.

    Most other "advanced QM" books seem to only really cover the Klein-Gordan & Dirac equations for various scattering processes and sometimes the hydrogen atom, with a view towards quantum field theory. For a long time I always felt that the study of bound states in relativistic quantum mechanics and/or quantum field theory, was generally neglected in favor of scattering processes.

12. **D R Lunsford**
    August 23, 2004

    JC – I've tried to answer this post 3 times, and I just don't know. A lot of QFT is "overconceptualized" and one ends up with too many mantras. I'd frankly admit that it was a mess.

    Did you ever see the book by Jauch and Rohrlich? (Theory of Photons and Electrons)

    It's remarkable that none of us can think of a totally acceptable text. This contrasts very strongly with GR, where one has any number of good texts, from all points of view.

    BTW there is an intermediate zone, "Advanced Quantum Mechanics", that is relatively neglected. There's a bool by Schwabl I like. A good QFT book would I think build on that.

13. **JC**
August 23, 2004

Danny,

If you wrote your own textbook on quantum field theory (QFT), what would you do to make the presentation better than other books like Ryder, Weinberg, Heitler, Kaku, etc …?

Originally I found the Ryder book the easiest to read when I was first learning QFT. Over the years I haven't really found any QFT books that I really liked a lot. Though if I had to pick a book, it would be either Ramond's field theory book or Veltman's "Diagrammatica".

14. **Chris Oakley**
August 23, 2004

This whole notion of using power series expansions in the free field grew out of my complete failure to get to grips with Feynman-Dyson perturbation theory. The more I tried to understand it, the less clear I was. This meant either one of two things: either (i) I was stupid or (ii) there was something wrong with it. It was not just conceit that made me decide on the latter: one thing about good scientific theories is that they are robust. Renormalization theory is not robust. If you take the limits in integrals in weird ways then you get weird answers. To say "well don't take the limits in weird ways" is not acceptable. If it matters then the likelihood is that the limit does not exist and the whole thing is mathematically meaningless.

Anyway … the power series method is, under certain circumstances at least, consistent and comprehensible. Also I do not see why there should be a problem with the notion of an interacting field being a sum of tensor products of free fields – one can still see interactions taking place, and what is more, I do not see why the Wigner arguments about unitary irreducible irreps of the Poincare group should suddenly cease to be valid just because there are interactions. After all, all Wigner is doing is organising the vector space of a relativistic quantum mechanical system in a particular way and I see no reason why this should not be one that involves interactions. It is a bit like coupled oscillators in mechanics – just because one can pick out normal modes with their characteristic frequencies, etc. it does not mean that the individual oscillators that comprise the system have ceased to exist.

To answer the questions: (i) where it has gone – it is a structure that does not necessarily fall apart when one examines it carefully & is capable of generating scattering amplitudes that agree with tree Feynman graphs; (ii) where is is going – two-body bound states could be examined now; the anomalous magnetic moment of leptons could be calculated once it is clear how to represent the classical EM field … a number of things …

I suppose that if I had not been so easily discouraged I would have just hammered away at this regardless, but the fact was that I got very tired of being the only one saying these things and having programmed computers from the age of 11 it was easy to do this for a living and be thanked and paid properly for doing it. However this in no way means that I changed my mind about any of the physics issues. If anything the last 17 years of "fundamental" physics research has vindicated what I thought then. You cannot make a silk purse out of a sow's ear, as they say.

15. **D R Lunsford**
August 22, 2004

Chris –

The 1986 paper is extremely interesting. Could you summarize your attitude at this moment about where this has gone, and could go? What most needs to be done?

-drl

16. **JC**
August 22, 2004

Chris,

I wonder how much of the quantum field theory (QFT) framework as we know it today, is heavily biased from the Feynman perturbation theory picture. It seems like research into almost all other possible alternatives to QFT are largely ignored by the mainstream and/or are on the fringes of the physics community. (Only big exception would perhaps be string theory).

Do you know of any alternatives which does NOT use the "particle == field" correspondence, besides the string theory picture of particles corresponding to various string and/or brane excitations?

One of "promises" of string theory in the 1980's that I really bought into at the time, was the idea that string theory could eventually explain the results obtained by renormalization in QFT. That "promise" seems to have fallen by the wayside over the years, where today not many string folks seem to like to talk about it.

With various stopgap measure like renormalization and path integrals, at times I wonder if we're even looking at the correct degrees of freedom in QFT. Years ago I wondered why nobody has found an easy way to get quark bound states of mesons and baryons directly out of QCD, other than perhaps the lattice gauge theory folks running computer simulations and seeing the bound states. I felt that if there was a good direct QCD and/or electroweak derived model for hadrons, you should be able to easily see why the proton and neutron are relatively stable while the pi mesons, hadrons with strange quarks, etc … are all unstable, and where one could easily calculate their decay rates and bound state energies.

17. **Chris Oakley**
August 22, 2004

Danny,

No, you have not asked that before, so no apology required. My May 84 paper does not do much other than establish that you can expand an interacting quantum field in powers of the free field, and thereby calculate (interacting) matrix elements by inspection. One can do the same thing for any fields whose field equation has some characteristic coupling and which is such that the equations reduce to free field equations when this coupling goes to zero, so obviously it could be done for Yang-Mills as well. The nearest I have got to actually studying Yang-Mills in this framework was when I was looking at chiral fermions with a minimal coupling to a vector field; unfortunately I probably have not retained my notes (this was 1987) but I seem to remember finding that one had to have a vector self-coupling exactly on the lines of a Yang-Mills if one was to satisfy the requirement that fields (anti)commuted for spacelike intervals.

I am not working on it now, as (when I am not writing trading systems for banks) I am still trying to get basic stuff like the classical limit of a quantum field clear in my mind.

18. **D R Lunsford**
August 22, 2004

Chris –

Sorry if I asked this before, but can you do for any Yang-Mills field what you did for phi4 in the May '84 paper? Are you working on that?

-drl

19. **Chris Oakley**
August 22, 2004

I've noticed over the years that many particle physics books (and even many review papers) seem to gloss over most of the QFT background, and frequently just goes straight into stating the Feynman rules and calculating Feynman diagrams. It seemed like the Feynman rules and Feynman diagram calculation prescriptions literally popped up out of thin air from nowhere, with very little to no theoretical justification on the surface (ie. just "take our word" for it, type of arguments).

Ahem, excuse me, but they might as well do this. The Feynman rules and renormalization prescription are all that they have. A connection with QFT is only possible if one permits the mathematically unpermissible, namely infinite subtractions.

The case against renormalization is argued at boring length on my web site (and will be argued at even more boring length when I have time to write the next installment), but let me add this snippet: *there is no such thing as an infinite constant*. If one has a divergent loop diagram then it is not acceptable to treat it as an "infinite constant" (to be cancelled by a counterterm) plus a finite functional part. Infinity plus an arbitrary function of any variables in the universe can be called infinity with no less justification. The "renormalized" amplitude can therefore have a functional dependency on variables that never even entered the original equations!

20. **D R Lunsford**
August 22, 2004

JC –

I think it would be hard to improve on a combination of Ryder, Weinberg, and Heitler, but there really isn't an ideal single-volume treatment. I wish Ryder were fatter (more topics). I really love that book. As Chris suggests, Ryder places Wigner's work in a prominent position.

I like the Kaku book as well, but it's not a good textbook. I really think there is still room for a better textbook.

21. **JC**
August 22, 2004

Chris,

Years ago I wondered about what it would take to write a book on quantum field theory (QFT) for undergrads. I was thinking of something along the lines of a QFT book which would be relatively "painless" to read, like the undergraduate particle physics book by David Griffiths. Of all the books I came across over the years on particle physics, the Griffiths one was the easiest to read and was relatively "painless" in comparison to most other books on particle physics.

I've noticed over the years that many particle physics books (and even many review papers) seem to gloss over most of the QFT background, and frequently just goes straight into stating the Feynman rules and calculating Feynman diagrams. It seemed like the Feynman rules and Feynman diagram calculation prescriptions literally popped up out of thin air from nowhere, with very little to no theoretical justification on the surface (ie. just "take our word" for it, type of arguments).

In QFT books which cover some particle physics, a lot of the really dense "difficult to understand" stuff seems to be largely justifying the perturbation theory prescriptions for doing the actual Feynman diagram calculations. I don't know how it can be done offhand, but the challenge would perhaps be in coming up with a presentation which makes the dense "difficult to understand" machinery of QFT more transparent in an "easy to understand" language to a reader who only knows some undergraduate level quantum mechanics, electromagnetism, classical mechanics, and maybe statistical mechanics.

The simplest way I can think of offhand in presenting the "difficult to understand" machinery of QFT, would perhaps be looking at a single real scalar field theory with a phi^3 interaction as a simplified pedagogical example with the least amount of baggage to carry along.

I took a look at Heitler's "quantum theory of radiation" book recently again, and noticed a lot of the classic quantum electrodynamics (QED) results were obtained from a first quantization framework, without really using QFT. It seemed like a messier formalism to do QED calculations, though Heitler did attempt to explain things in a more intuitive semi-classical picture than I've seen in modern QFT and particle physics books.

22. **Chris Oakley**
August 20, 2004

Hi JC,

The book would be a formal text whose thread would run somewhat like this:

1. Wigner unitary irreps of Poincare group, up to and including norms of vectors in these. Vectors in irreps as classical particles/fields. NB: First draft here (excluding classical limit arguments):

http://www.cgoakley.demon.co.uk/qft/RQM.pdf

2. Fock space as sums of tensor products of these vectors; Creation & annihilation operators defined from Fock space as per nice argument given in Weinberg Vol. 1 pp. 173-174; Identical particles and exchange symmetry; Quantum fields as 4D Fourier transforms of annihilation/creation ops; Spin-statistics theorem.

3. "Toy" interacting models with interacting field as sum of products of free fields in various configurations; calculation of scattering amplitudes using correspondence with time-dependent perturbation theory in ordinary QM.

4. Quantum Electrodynamics.

At this point I get a bit vague … I need to understand better how to get classical electrodynamics from the quantum one.

Er … that's it so far. You understand that a lot of this is about providing a framework on which to build. This may well go in the direction of quasi- or non-local field equations to avoid infinities popping up. As you see, I have at no point formally quantized a classical field as I don't believe that one needs to although I will do it for interest at some point.

23. **JC**
August 20, 2004

Chris,

If you wrote a book about particle physics and/or physics in general, what would it be about?

24. **Peter**
August 20, 2004

The book you mention, “Nobel Dreams”, is by Gary Taubes (the mathematician Cliff Taubes’s brother). It was written during the mid-eighties and is quite entertaining. I read it a long time ago, and from what I remember it is very hard on Carlo Rubbia, to the extent of being pretty unfair. Rubbia is an overbearing guy, but he was the one who got the collider built at CERN that discovered the W and Z. Not at all clear that someone without his will-power could have done this.

I remember taking a class from Rubbia when I was an undergraduate. He was commuting back and forth to CERN and some days didn’t quite make it, but was quite a force of nature. I remember once when I was sitting in the second row I asked a question. He then pushed aside the seats in the front row to get to me, and gave me a vigorous answer right in my face. He really wanted to answer the question, but is was a bit of a scary experience.

25. **JC**
August 20, 2004

Wasn’t there a book titled something like “Nobel Dreams …”, which discussed the dark side and politics of experimental particle physics research?

26. **Peter**
August 20, 2004

Hi Chris,

I should point out that I don’t have a tenured position at Columbia, although it is essentially a permanent one, which is the most important thing.

There’s a long on-going story about my attempts to write a book about some of these issues. I’ll tell this sometime when the situation is clearer about what is going to happen with that project.

27. **Chris Oakley**
August 20, 2004

Peter,

If the press were to ask me what I thought about string theory I would probably answer something on the lines of the following:

“I haven’t studied it, but I have trod in it.”

The problem is that my views carry very little weight. I could not get research posts after 1986 and have been out of the subject for over 17 years. They would probably just decide that my views were simply sour grapes. You, on the other hand, are a tenured professor at a prestigious university and as such, people *do* listen to what you have to say.

As for finances, maybe there might be some mileage in a popular science book arguing against the superstring collective insanity – ?

Even if it does not make you rich, it would make you feel better.

28. **D R Lunsford**
August 19, 2004

FWIW, I think you came out on top, money or no, to have avoided being tainted by Microsoft. Some things are more important than money (GASP).

A generation of people who sought to make an honest living at IT have suffered for those riches they enjoy.

The Linear Collider Will Be Cold

August 20, 2004
Categories: Uncategorized

For many years now, the highest priority of experimental particle physicists for a next-generation accelerator project has been a new electron-positron linear accelerator. The last high energy electron-positron collider, LEP, reached a total energy of 209 Gev before being shutdown in 2000. To get to higher energies than LEP, a ring isn't viable because synchrotron radiation losses go as the fourth power of energy, so a ring with much higher energies than LEP would use an intolerable amount of electric power.

A linear collider, where you build two linear accelerators and collide their beams together, doesn't have the synchrotron radiation problems (although the electric power demands are still a problem since the beams you accelerate only can collide once, not many times like in a storage ring). There have been several competing designs for a linear collider, with one of the main difference in the designs being whether the RF accelerating structures are superconducting ("cold") or room temperature ("warm"). These designs all are for a machine that would start out with a total energy of 500 Gev and ultimately reach 1 Tev. A committee was formed called the "International Technology Recommendation Panel" (ITRP), and it has issued a press release announcing its decision today. The ITRP came down on the superconducting side; this is a design mainly developed at DESY in Hamburg as part of the TESLA project.

The German government has decided to use the TESLA technology to construct a free-electron X-ray laser at DESY called XFEL. They have done this in a way that would allow XFEL to ultimately be upgraded to a linear collider at DESY, but have put off any decision about whether to actually fund and build such a machine.

The ITRP decision will allow work on a final design for the linear collider to begin, but the trickiest questions still lie ahead. Where will the thing be built and who is going to pay for it? The order of magnitude of the cost is $5 billion and the general assumption is that this will be an international collaboration. Besides the possibility of siting it at DESY in Germany, sites that have been discussed in the US mainly are at Fermilab in Illinois (being pushed by Fermilab), or somewhere in California (being pushed by SLAC). Even the most optimistic time scales for designing, funding and building a linear collider don't have it running until late in the next decade. More realistic might be the mid 2020's. The question of where the machine is located is crucial to the long-term future of the SLAC and Fermilab laboratories. If it is at their site or nearby they have an assured future, if not their future becomes much more problematic. CERN has its own design for an even higher energy linear collider called "CLIC", but CERN's funds for the forseeable future are committed to constructing and funding the LHC, as well as possible future upgrades of that machine.

There's a bewildering array of web-sites with information about this, including the new International Linear Collider Communication and linearcollider.org ones, another one at SLAC, one at Fermilab (which seems kind of out of date..) and Michael Peskin's home page. This last one contains links to many talks by Peskin about the physics to be done by a linear collider as well as a web page of links to other information about the linear collider.

The ITRP decision was announced at one of the year's biggest high energy physics conferences, the ICHEP being held now in Beijing. The web site for that conference contains many talks giving the latest results from experimental groups around the world. Unfortunately, as far as I can tell there's nothing very earth-shattering being reported.

Comments

1. **RT**
   August 23, 2004

   All this talk about theory/ego shit, and not a single comment (informed or otherwise), thus far, about any, possible, experimental light to be cast upon our darkness

Smolin on Loop Quantum Gravity

August 22, 2004
Categories: Uncategorized

Larry Yaffe's comments about string theory reflect well mainstream opinion in the particle physics community. On matters of fact I think what he has to say is pretty accurate, but I disagree with some of his statements that reflect not facts but scientific judgements. Of his positive comments about string theory, the ones about its impact on mathematics and about AdS/CFT are right on target. For an interesting talk explaining the status of attempts to use AdS/CFT to say something about QCD, see Larry's colleague Matt Strassler's talk this month opening a workshop in Santa Barbara (don't miss the heated exchange at the end of the talk about whether or not this is all just supergravity).

Larry's comments about "compelling hints" that there is something "deep and meaningful" to string theory and that it has provided "partial insights" into conceptual problems in quantum gravity are hard to to argue with. But while these hints seem to point in the direction of the existence of an interesting 11 dimensional supersymmetric theory, they provide no evidence that it has anything to do with the standard model. Quite the opposite, the evidence of the "landscape" suggests that any attempts to relate such a theory to the real world produce a framework that is completely vacuous, and can never explain anything (or, equivalently, can explain absolutely anything you choose).

The one place where I think I really disagree with Larry is his claim that, indisputably, "string theory is the most promising framework we have for combining quantum mechanics and gravity". This "most promising framework" locution has been around now for nearly twenty years. It was justifiable when people were just starting to try and understand the implications of superstring theory, but the failure of twenty years of effort by thousands of very talented physicists has to be taken into account. The fact is that despite all this effort, string theorists still don't have a consistent theory of 4-dimensional quantum gravity and prospects are not promising that this situation is going to change anytime soon.

As part of this "most promising" comment, Larry has critical things to say about loop quantum gravity. I'm no expert on this myself, but, like many theorists, he seems to me to be holding string theory and loop quantum gravity to quite different standards. Lee Smolin recently wrote to me and Larry to respond to Larry's comments, he allowed me to reproduce his e-mail here:

"Dear Peter and Larry,

Thanks for the comments, most of which I agree with. But in case either of you are interested, Larry's comments about loop quantum gravity do not reflect the real results.

A side effect of the sociology of string theory seems to be that there is as much ignorance of the genuine results concerning loop quantum gravity and other approaches to quantum gravity as there is overhype in string theory. It is fascinating that, just there are results that are believed to be true in string theory, despite never having been shown, there are results that have been shown, in some cases rigorously, in lqg, about which many people seem not to have heard about, in spite of being published 5-10 years ago on the archive and in the standard journals.

To combat this I wrote a recent review hep-th/0408048 which I would gently suggest reading before making public pronouncements about the status of the field. There are also good reviews on the rigorous side by Ashtekar and Lewandowski and by Thiemann, as well as two textbooks in press from CUP, one from Rovelli and one more rigorous from Thiemann.

Larry says of LQG that it "has not been shown to have anything to do with gravity. Does it have a large-volume limit? Does it have long distance dynamics…"

Can I mention some of the results that show that lqg quite definitely is a quantum theory of gravity, with details and referenceds in the paper? Larry, if you think any of these results are wrong, please tell us on what step of what calculation or proof someone made an error. Otherwise, we invite you to study the results and the methods by which they were gotten. You might surprise yourself by coming to agree with us, after all this is just quantum gauge theory, but in a diffeomorphism invariant setting rather than on a background manifold.

A key result is the LOST uniqueness theorem which shows that for d >=2 the hilbert space LQG is based on is the UNIQUE quantization of a gauge field that carries a unitary rep of the diffeo group, in which both the wilson loop and non-abelian electric flux operator are well defined operators. (see the paper and references for the precise statement).

Given that GR and supergravity are well understood to have configuration spaces defined as configurations of gauge fields mod diffeos, to which the theorem applies, this implies that the hilbert space used is uniquely suited to the quantization of those theories.

It is further shown that the hamiltonian contraint of GR for d=3+1 is rigorously defined on the hilbert space of diffeo constraints, allowing exact solutions to all the quantum constraints to be constructed.

As far as the path integral is concerned, using the method of spin foam models, based on the observation that GR and supergravity in all dimensions are constrained topological field theory, leads to rigorously defined path integral measures corresponding to the quantization of these theories. There are in addition rigorous UV finiteness results. There are also results that establish correspondences with Regge calculus in various limits.

These results all are quite sufficient to establsih that these theories are precisely the quantization of GR or supergravity. Surely this has something to do with gravity.

Regarding the low energy limit, more explicitly, there are several classes of candidate ground states that have the property that 1) measurements of coarse grained geometrical operators agree with classical flat or deSitter spacetime, up to small fluctuations. 2) small excitations of the gravitational degrees of freedom, which satisfy the constraints to linear order in l_Planck/wavelength have two spin two massless degrees of freedom per momentum mode (i.e. the gravitons are recovered for wavelengths long in planck units, again showing this is a quantization of gravity.) 3) after coupling to any standard matter field, excitations of the ground state yield a cutoff version of the quantum matter field theory on the classical background, cutoff at the planck scale because of the finiteness of quantum geometry.

The classes of states these statements characterize are a) coherent states, b) eigenvalues of coarse grained 3-geometry (sometimes called weave states) and c) for non-zero cosmological constant, the Kodama state.

I would think that finding explicit states with these properties proves that at least linearized gravity and effective field theories correspodning to qft on background manifolds is recovered. Certainly this is again something to do with gravity.

In addition, we can mention 1) the black hole results, which give an exact description of the quantum geometry of an horizon, in agreement with all semiclassical results and 2) the loop quantum cosmology results, which again recover all results of semiclassical quantum cosmology and go beyond them in the context of a rigorously defined framework. Again, some things to do with gravity.

See my paper for complete references.

I'm sorry for the tone, but one loses patience after 15 years. We have always been careful to state results precisely with full qualifications and never to overclaim. By now there are sufficient results that I think the many good people who are working hard in this field deserve to have their results much better known.

As always, I and my (rapdily growing number of) colleagues are happy to talk to anyone and go anywhere to explain the results and the methods by which they were gotten.

Indeed, the number of invitations for talks at places that previously expressed no interest previously is growing. I'd certainly be glad to recommend good speakers who could educate your department about the state of art in quantum gravity.

Thanks,

Lee

Comments

1. **Charles Kilmer**
   September 6, 2004

   would the resolution of the Poincare Conjecture have any effect on string theory?

   Mathematical Mystery Believed to Have Been Solved
   The Scotsman ^ | Mon 6 Sep 2004 | John von Radowitz
   http://www.freerepublic.com/focus/f-news/1208744/posts
   http://news.scotsman.com/latest.cfm?id=3461424

   One of the seven great unsolved mysteries of mathematics may have been cracked by a reclusive Russian who is not remotely interested in the ?560,000 prize his solution could win him, it emerged today.

   The Poincare Conjecture involves the study of shapes, spaces and surfaces and makes predictions about the topology of multi-dimensional objects.

   Basically, it says that a three-dimensional sphere can be used in an analogous way to describe higher-dimensional objects that are impossible to visualise.

   Since Henri Poincare suggested the theorem in 1904, some of the greatest mathematicians of the 20th century have struggled to prove it either right or wrong.

   All have failed. But now the world of maths is buzzing with the news that an answer might at long last have been found.

   Dr Grigori Perelman, from the Steklove Institute of Mathematics at the Russian Academy of Sciences in St Petersburg, has published two papers offering a solution to a larger-scale problem called the Geometrization Conjecture.

   This is also concerned with geometry, and experts say that contained within it is proof that the Poincare Conjecture works.

   If Perelman can satisfy his peers that this is the case, he stands to win a one million dollar cash prize from the Clay Mathematics Institute in the United States.

   The Institute is offering million dollar prizes for solutions to each of the mathematical conundrums it calls the Seven Millennium Problems.

   But there is a more fundamental problem the general community of mathematicians needs to solve first. Perelman does not seem to be interested.

   Dr Keith Devlin, a leading mathematician from Stanford University in California, explained: ?He?s very reclusive, and won?t talk to anyone. He?s shown no indication of publishing this as a paper, and he?s shown no interest in the prize whatsoever.

   ?Has it been proved? We don?t know, but there?s good reason to think it has been. My guess is that in about 12 months people will start to say okay, this is right, but there?s not going to be a golden moment.?

   Dr Perelman published his two papers in November, 2002 and March last year.

   A third is yet to be published.

   By all accounts, Poincare will come out of the first two papers, said Dr Devlin.

   If the conjecture was proved it would have profound ramifications, he told the British Association Festival of Science at the University of Exeter.

   Scientists working on the frontiers of cosmology and physics frequently dealt with hyperdimensions. A solution to the Poincare Conjecture would greatly increase their understanding of the shape of the universe.

   Dr Devlin compared proving Poincare with setting off an avalanche. If you are on top of a mountain, and it is spring, and you jump up and down, a little bit of snow moves. But at the bottom a whole lot of snow comes down.

   ?It can?t fail to have enormous implications; it will just be huge.?

   He said solving mathematical problems such as the Poincare Conjecture was more like writing a story than doing a sum, which was why it took so long.

   ?It?s just so damn complicated, he said. It really can take two or three years to certify the thing.?

   Proving the Poincare Conjecture would be the first great mathematical breakthrough since Andrew Wiles solved Fermats Last Theorem in 1994.

   This year, Professor Louis de Branges de Bourcia, from Purdue University in the United States, claimed to have proven another of the Millennium Problems called the Riemann Hypothesis.

   The hypothesis is a 150-year-old theory about Prime Numbers ? numbers that divide only by one and themselves and are considered the atoms of arithmetic.

   De Branges claimed to have confirmed a conjecture made by the German mathematician Bernhard Riemann in 1859 about the way prime numbers were distributed.

   But, unlike in the case of Poincare Conjecture, the worlds mathematicians are becoming increasingly convinced that he has got it wrong.

   Marcus du Sautoy, Professor of Mathematics at Oxford University, said: ?The mathematical community is sceptical whether the methods of Louis de Branges are capable of proving the Riemann Hypothesis.?

   If de Branges turned out to be right, it would have a dramatic impact on both global business and national security.

Encrypted codes are based on the randomness of prime numbers. If a system could be found that made them predictable, no secret would be safe.

?What mathematics has been missing is a sort of maths prime spectrometer, like the machine chemists use to tell them what things are made of,? said Prof du Sautoy. ?If we had something like that it would bring the world of e-commerce to its knees overnight.?

2. **Alejandro Rivero**
   August 24, 2004

   Hmm Urs, 1+1 has always appeared to me as pathological. But yep, I can not remember any arguments about the gauge invariance of the area operator, so perhaps you are right about having a problem there. I believe that Rovelli did, in a different focus, a proposal about how general transformations should affect to the eigenvalues and to the mean expected values of an area operator, I can not tell if it applies here.

3. **Urs Schreiber**
   August 24, 2004

   For something different: I have listened to the talk by Strassler (on what AdS/CFT has to say about QCD) that Peter provided a link to. Very intersting. We had recently discussed that topic here already.

   It is indeed not true that only supergravity plays a role in these calculations. In particular anomalous scaling dimensions of operators on the SYM side is matched to at least first non-sugra order on the string side by a whole small industry. Search the arXive for papers by Tseytlin and by Zarembo (2004), for instance.

4. **Urs Schreiber**
   August 23, 2004

   Sorry, that link was supposed to be:
   [Phoenix Project/Master Constraint Programme]

5. **Urs Schreiber**
   August 23, 2004

   Is it clear that area is quantized in LQG?

   I know that the area operator constructed in LQG has discrete spectrum (BTW, which spectrum precisely? Last time I checked there were at least two different 'proposals' for this spectrum.)

   But the area operator is not gauge invariant, but has single spin networks as eigenstates. But any state that actually solves all of the constraints, including the Hamiltonian constraint (if it can ever be defined) must be a superposition of spin network states (first of all it must be a knot state (solving the spatial diffeo constraints), but surely even a superposition of knot states) and most probably (nobody knows!) a continuous superposition.

   This means that it is not clear that on physical states the area operator has any eigenstates at all.

   Often LQG properties are argued in terms of spin network states. But these are just a particular choice of basis – of the *kinematical* Hilbert space. Statement like 'the universe is a huge spin network' or things like that make an unjustified identification of a choice of basis with a physical observable.

   BTW, does anyone know about the status of the '[Phoenix Project/Master Constraint Programme]' to actually construct the Hamiltonian constraint? As long as this hasn't been done it seems pretty vain to discuss any properties of LQG.

   BTW, as I discuss [here] the 1+1D example once again helps to understand the situation: When you decide to restrict to the space of spatially rep invariant functions in 1+1 D gravity and then try to apply the Hamiltonian constraint as an operator on that, you know you cannot succeed. That's because this amounts to imposing the constraints

   L_n – \bar L_{-n} for all n

   together with

   L_0 + \bar L_0 .

   The first infinite set of constraints can be imposed all right, and even with the methods used in LQG, because these generators have no anomaly among themselves. But then standard results in 1+1d gravity tell you that the Hamiltonian constraint L_0 + \bar L_0 cannot annihilate any of the resulting states.

   I am speculating that this is in fact the reason why Thomas Thiemann in his LQG-string paper decided not to precisely follow the standard LQG procedure (which consists of applying the quantized Hamiltonian constraint as an operator on the space of spatially rep-invariant states) but decided to treat both L_n and \bar L_n by 'relaxed canonical quantization'.

6. **Alejandro Rivero**
   August 23, 2004

   Independently of LQG, the interesting issue of having a quantum of area is that is can be used to singularise 4D over any other number of dimensions (we want Newton constant to have units of area). If strings had some way to justify the right compactification, ie to justify 4D, I could understand the criticism of the foamy world. But as it stands, LQG holds a very interesting card in his hand.

7. **Urs Schreiber**
   August 23, 2004

   Thomas –

   let's try to avoid bickering and general accusations and just focus on the discussion of some technical facts and problems. I'd be very interested to see Lee Smolin reply here (maybe mediated by Peter Woit).

   You claimed that something is a problem for string theory and I asked you to please demonstrate it. Your reply

   > As all string theorists, you consistently require much higher standards from the competition than from yourself.

   leaves me in puzzlement. From past discussions I can guess that you are thinking that somehow results about anomaly cancellation for chiral fermions have to do with anomalies in the canonical quantization of gravity. I frankly admit that I don't see what you have in mind. But if you can make it precise you should probably do so and show it to somebody who knows more about it than I do.

It is precisely this problem with discussing full 1+3d nonperturbative quantum gravity that makes me rather wish to discuss a toy example like 1+1d gravity. All open questions of LQG are also non-trivial and unsolved in this context.

8. **Thomas Larsson**
August 23, 2004

Urs,

I do indeed agree with your claim that for an understanding of a non-perturbative quantization of gravity it should be very helpful to have an idea of the existing quantum reps of the symmetry generated by the classical constraints (though I would not call that alone a quantization of gravity, as you sometimes do).

I have never claimed (I hope) to have quantized gravity. I do claim, however, that I have succeeded in quantizing *the symmetry principle* underlying gravity, and I believe that this will prove to be an essential ingredient in quantum gravity itself.

But I don't claim that I know how to similarly 'fix' the LQG approach in higher dimensions. If you do and if you think that you can make a nonperturbative quantization of the action of closed string field theory and if you think you can deduce some kind of problem for string theory from that please do so and let us know about your result.

As all string theorists, you consistently require much higher standards from the competition than from yourself. I create (not alone, but anyway), the mathematics that makes it possible to discuss diffeomorphisms on the same footing as conformal transformation, and you say this may be interesting if I also succeed in using this to quantize gravity. The last guy who succeeded in creating both mathematical infrastructure and physics was called Newton, and I think that my results may be valuable even though I am a lesser soul than him, especially since everybody else has failed to make any progress towards on quantum gravity. Not even Einstein created his own math; Ricci and Levi-Civita created tensor calculus for him to use.

Meanwhile, you seem completely undisturbed by the fact that string theory makes no testable predictions whatsoever and that recent landscape ideas show that it never will, a situation which Witten describes as "[String theory] is also more predictive than conventional quantum field theory." I don't believe in LQG, but as long as Smolin does not claim that LQG is more predictive than field theory, he deserves good marks for honesty. Misrepresentation is Fraud's cousin.

BTW, did you see our comments on your 2-form gauge theory over here? (Possibly you were on vacation when these were posted.) Since you are only using a 2-form B you have to face the problem that the surface holonomies that you compute are not independent of the 'parameterization' of your surface, i.e. of the order in which you multiply the group elements on the plaquettes.

The amplitudes depend on the triangulation (or quadrangulation, rather); even the number of indices depends on that. This is why it's only the lattice model, were there is a canonical triangulation, that is well defined. However, there is no order dependence in the sense of non-associativity. Contraction of all index pairs associated with internal links is order independent; finite sums can be performed in any order.

9. **Urs Schreiber**
August 23, 2004

Thomas,

there is non-perturbative quantization of gravity, where we try to solve the canonical constraints exactly, and there is perturbative quantization, where we compute quantum corrections to classical solutions. The discussion here is about how to do the non-perturbative quantization.

I do indeed agree with your claim that for an understanding of a non-perturbative quantization of gravity it should be very helpful to have an idea of the existing quantum reps of the symmetry generated by the classical constraints (though I would not call that alone a quantization of gravity, as you sometimes do).

This is precisely what I claim is missing in the LQG approach, and in the special case of 1+1 dimensions one indeed finds that the idea you are proposing gives us the correct constraint algebra (namely the Virasoro algebra), while LQG misses it.

But I don't claim that I know how to similarly 'fix' the LQG approach in higher dimensions. If you do and if you think that you can make a nonperturbative quantization of the action of closed string field theory and if you think you can deduce some kind of problem for string theory from that please do so and let us know about your result.

BTW, did you see our comments on your 2-form gauge theory over here? (Possibly you were on vacation when these were posted.) Since you are only using a 2-form B you have to face the problem that the surface holonomies that you compute are not independent of the 'parameterization' of your surface, i.e. of the order in which you multiply the group elements on the plaquettes. To ensure well-defined surface holonomy you'd need to add a 1-form A and the condition B+F=0 and then you'd make contact with Girelli&Pfeiffer's work.

BTW, one maybe interesting piece of trivia in the triangle of topics constituted by LQG, strings and non-abelian 2-form field theories is the following:

If we restrict as in LQG to spatial rep-invariant states we obtain, in 1+1 dimensional gravity, the so-called *boundary states*. A particularly natural such state is the loop space function which assigns the holonomy of some connection A to a loop. Incidentally, this function is precisely the boundary state which describes a non-abelian 2-form B = -F_A .

See this entry and hep-th/0408161 for the details.

10. **Thomas Larsson**
August 23, 2004

Urs,

The following statements are almost identical:

1. LQG is only quantization in a weaker, nonstandard way. In particular, if the quantization of 3+1D gravity in any way resembles quantization of 1+1D gravity on a world sheet, then the diffeomorphism constraint should acquire an anomaly which cancels against ghosts.

2. All symmetries, including gauge symmetries like diffeomorphisms, need representations of lowest-energy type.

Thus, apparently to your great embarrassment, we are saying roughly the same thing (except that ghosts are problematic, because normal ordering doesn't just ruin nilpotency but makes the BRST operator ill defined). However, spacetime diffeomorphisms do not use lowest-energy representations in string theory neither, although it does treat worldsheet gravity in this correct way.

So exactly why do you expect LQG people to take your argument seriously, if you don't take the logical implications seriously yourself?

11. **Urs Schreiber**
August 22, 2004

When quoting results about uniqueness of the Hilbert space in LQG it must be emphasized that the quantization presciption used there is not what is usually called canonical quantization, and by this I mean differences over and above the ambiguities of canonical quantization itself. LQG uses ‘relaxed’ canonical quantization where not both of canonical coordinates and momenta are represented as operators on a Hilbert space.

To me this is the crucial but hardly ever emphasized assumption in LQG. Some people in LQG that I have talked to didn’t even realize that this is a controversial step. It seems that people expected that it is obviously fine to use this ‘relaxed quantization’. But it is known that when applied to systems whose standard quantization we do understand quite well, like the free particle and even 1+1 dimensional gravity coupled to scalar matter, the ‘relaxed canonical quantization’ gives grossly different results than usual quantum theory (e.g. that obtained from path integral quantization).

So no matter what one can prove within the LQG framework it is a fact, confirmed for instance by Thomas Thiemann and Josh Willis themselves, that LQG changes the usual rules of quantization.

I know that the idea is that in any theory with such a ‘relaxed’ canonical quantization a limit exists where the ordinary quantization is reobtained. But apparently the only paper investigating this idea is the ‘Shadow States’ paper by A. Ashtekar, Josh Willis and Fairhurst. But the construction done in that paper explicitly makes recourse to quantum effects of the standard quantum theory, so that this does not prove anything.

Of course it is true, as Thomas Thiemann has put it, that only experiment can show if standard quantum mechanics still holds at the Planck scale. But

1) if a propsal for quantum gravity does use non-standard quantum theory this should be made quite clear to everybody, so that people are aware of such a step

2) it would be nice if there were some kind of hints how the non-standard quantum theory used can flow to the standard theory in some limit. Otherwise even the theoretical motivation to believe in the viability of the non-standard approach is weak. To date no such evidence has been given, and indeed I consider the results given in the ‘Shadow states’ paper as a counterexample.

12. **Aaron**
August 22, 2004

Lee Smolin seems much more sanguine about the state of things than some of his colleagues. This is the first I’ve heard anyone claim that lqg has any semiclassical limit. Can one compute graviton scattering? I’ve also been under the impression that the Hamiltonian constraint remains (as of last May, at least) quite mysterious.

Finally, as for the black hole results, from the reading I’ve done, they’ve been deeply unimpressive, not the least of which because they don’t actually appear to be done in any theory I’d call lqg. Put another way, I don’t see how any black hole state was constructed in any theory. Instead what seemed to be done was to take a spacetime with a black hole, cut out the black hole, quantize what remains, *discard degrees of freedom associated with the bulk* and get a result proportional to the area of the horizon. Forgive me if I don’t find that result very impressive.

The Landscape in Scientific American

August 23, 2004
Categories: Uncategorized

The latest issue of Scientific American is devoted to articles about Einstein and his legacy. One article in the magazine doesn't really have much to do with Einstein and I believe would make him gag if he were still around. The article, entitled "The String Theory Landscape" is by Raphael Bousso and Joe Polchinski. In it they claim credit for the pseudo-scientific idea of "explaining" the value of the cosmological constant by the existence of the "landscape" and the anthropic principle. It's sad to see this nonsense being purveyed by the most respected and well-known popular science publication in the US.

For something more sensible about the anthropic principle, see a recent column from Nature.

Comments

1. **D R Lunsford**
   August 30, 2004

   Not at all. It's precisely the opposite IMO – they've been so poisoned by Bourbakian formalism that they've lost their esthetic judgment and are willing to "settle for less".

   My own "feeling" about this is – why should we expect our little efforts to somehow be vastly different from past efforts? That is, whatever works will almost certainly fit nicely into the existing pattern of development, going right back to Air Earth Fire and Water.

   To be more precise – a good theory needs a unique context. Every theory to date has provided its own unique context. This even goes for phenomenological theories that work (e.g. BCS – Cooper pairs – WGS – Higgs mechanism etc.)

   The thing that gives GR a unique context is background independence.

2. **Chris Oakley**
   August 30, 2004

   Well, OK, but then what is it that you are looking for? Interesting mathematics or physics? It seems that fundamental physics has reached its present impasse exactly because people have been – to use Newton's image – too busy picking up and examining pretty mathematical seashells rather than trying to build a fully consistent understanding of the physical world.

3. **D R Lunsford**
   August 30, 2004

   I would not consider it to be gravity, period. There is already a formulation like this for classical GR, which also is not gravity.

   Change of space element as in the Pluecker line geoemtry – say, building spacetime and matter from twistors anf getting some sort of quantification from that – might barely make it under the wire.

   -drl

4. **Chris Oakley**
   August 30, 2004

   If someone came up with a QG model where gravity was just another field on a Minkowski background, which was such that the classical limit gave one Newtonian gravity with light bending, etc. would people just dismiss it as too boring? I ask this question seriously. The main approaches to QG at the moment (Loops and Strings – NB: in alphabetical rather than slice-of-science-budget order) were inspired by, rather than derived from known physics, and in neither case has it been demonstrated that GR emerges as a classical limit. And with regard to Danny's requirement of background independence, given that SR is one of best verified theories yet, is this requirement not more to do with a feeling of mathematical "rightness" rather than any physics?

5. **D R Lunsford**
   August 29, 2004

   In short – anything that claims to extend or sharpen GR will certainly be background-free – that is the unique context that GR introduces. (Every real theory introduces a new physical context.)

   -drl

6. **JC**
   August 29, 2004

   Danny,

   What would be the dire consequences of a quantum gravity NOT being background free?

   I never quite understood the arguments in favor of background independence in quantum gravity. On the surface they so far seem like hand waving arguments, which extrapolate classical general relativity notions and impose them by "decree" in the quantum regime.

7. **D R Lunsford**
   August 29, 2004

   The essential thing about gravity, quantum or not, is background-freeness. Clearly you're never going anywhere without that as the central idea. That's what makes GR what it is. All this obessive "quantum gravity" kvetsching is just misguided. That will probably take care of itself in it the long run, and probably fix field theory as a side effect. The absolutely key thing is background-free field theory.

8. **Thomas Larsson**
   August 29, 2004

Of course, this is a danger for all research in quantum gravity. I find myself thinking that we need a radically simpler and deeper insight into the problem very soon, or we need to let the field lie fallow for a while. Perhaps a shock will come from outside that brings this about.

When all contrieved ideas have failed, maybe people will have to turn to the obvious ones. Like the quantum representation theory of the diffeomorphism group might be useful for understanding diff-invariant quantum theories.

There is of course no guarantee that this is the right thing to do. But it seems to me that good new ideas are in short supply indeed. When the old ideas have failed to describe the physics of our universe (and why else do string theory leaders focus on other universes?), what's the alternative?

9. **Chris W.**
August 28, 2004

Back to the Landscape — I just came across this remark supposedly made by Bill Clinton in another context:

"When ideologues find themselves in a hole, they dig harder."

The manifestation of this in string theory is that its adherents seem to be convincing themselves that they making headway with every interesting new *mathematical* result, and the mathematical territory they have opened up seems to offer an unlimited supply of them, enough of which offer hints of real physics to continue reinforcing the ideology.

Of course, this is a danger for all research in quantum gravity. I find myself thinking that we need a radically simpler and deeper insight into the problem *very soon*, or we need to let the field lie fallow for a while. Perhaps a shock will come from outside that brings this about.

10. **Chris Oakley**
August 28, 2004

Matt:

QED is obviously part of a bigger theory, and, who knows, maybe it is a limiting case of some as-yet-undiscovered Planck-scale dynamics. In the absence of any observational data about quantum gravity, the imagination can run free. AFAIC people can construct whatever theories they like about QG, provided that they have the right classical limit and do not contradict QM. My personal preference is toward simpler rather than more complicated ones, but that is just me.

One must, however, make a distinction between approximation and inconsistency. Non-relativistic mechanics works fine when motion is not relativistic. Classical electrodynamics works well when the number of photons is large. Na?ve QED, OTOH fails at *all* energy scales, and can only be rescued by mathematics that would get you an "F" if you were to do it in an exam.

Steve:

Would you agree that the natural ultraviolet regulator in QFT is provided by spacetime foam with a lower bound l_{b} acting as a natural cutoff ?

Under no circumstances! *A posteriori* tampering with a theory that has failed to give the correct, or even a finite answer can never be acceptable.

All the problems in field theory and qft (and yes string theory) no doubt arise from having a static deterministic classical background manifold which is an ok approximation at large scales/low energies. Thus the bag of mathematical tricks needed to make qft theory work.

You are asking me to abandon Minkowski space just because an approach to QFT that I would not have expected to work, does not work. I refuse to do it.

11. **Steve**
August 27, 2004

Chris et al.
Would you agree that the natural ultraviolet regulator in QFT is provided by spacetime foam with a lower bound l_{b} acting as a natural cutoff ?(Where this would be the Planck scale typically). This is not a new idea and was considered decades ago by Salam I believe. Not much subsequent work on the idea really (an old paper by Smolin, and some others, and a paper by L H Ford).

One might try and formulate QFT or classical field theory on a fluctuating/stochastic classical geometry rather than a smooth deterministic manifold. Hard vertices in Feynman diagrams should then get smeared out. Of course this was one of the motivations behind perturbative string theory but the idea can exist outside of string theory when one tries to accomodate the presumabley discrete or foamy micro-structure of spacetime. Delta function singularities associated with propagators in qft should be regulated in a natural way.

As for classical electrodynamics and the problem of point charges, the introduction of a lower bound l_{b} in spacetime or 3-geometry corrects it quite easily. Delta functions simply get smeared out into very narrow highly peaked Gaussians of width l_{b}. If G(x-y) is a simple Green's function for an electrostatic potential \phi(x)where

nabla^{2}G(x-y)=-delta^{3}(x-y)

then this simply becomes a nonlocal Laplace field equation, smearing out the delta fn. into a very narrow Gaussian of width l_{b}:

nabla^{2}G(x-y)=-Aexp(-(x-y)^{2}/l_{b}^{2})

A point charge then gets smeared out into a ring or a "string". The corresponding classical self energy is also finite and becomes infinite only as l_{b}–>0. There is an old paper by a guy called Namsrai that considers this and the subsequent theory of QED that follows (might be in the archive as a scanned pre-latex entry). Such nonlocal electrostatic equations are used for real in computational chemistry in order to accomodate the finite size of ions, large charged molecules or charged polymers in solutions, which can be modelled as charged strings.

All the problems in field theory and qft (and yes string theory) no doubt arise from having a static deterministic classical background manifold which is an ok approximation at large scales/low energies. Thus the bag of mathematical tricks needed to make qft theory work.

12. **Matt**
August 27, 2004

Chris —

If you'll accept that classical electrodynamics need only be a limiting case of QED (one might even say, suggestively, an effective theory), why do you think that QED needs to be “fixed” rather than only exist as a limiting case of some fundamental theory of quantum gravity?

Matt

13. **Chris Oakley**
August 27, 2004

…fixing QEM means first fixing CEM

What you are saying is pretty much the received wisdom on the matter, and I do not agree with it. Classical Electrodynamics only needs to exist as a limiting case of Quantum Electrodynamics. In any case, Classical Electrodynamics only breaks down in situations where QED would be expected to take over, i.e. point charges. Fix QED and you automatically fix CED.

14. **D R Lunsford**
August 27, 2004

Chris –

Dirac was none too happy with classical EM, which is also horribly broken. He tried on three different occasions spanning 50 years to fix it.

As Einstein said, “the electron is a stranger in electrodynamics”. IMO nothing short of the full background-free Monty will ever fix it, and that fixing QEM means first fixing CEM.

Still, the *forms* of EM persist – so the *structure* of the theory is (almost) right.

-drl

15. **Chris Oakley**
August 27, 2004

Thomas,

I don’t want to go on about this too much as we are getting off topic, but clearly Feynman, Schwinger and Tomonoga were not proud of the infinite subtractions they had to do and were looking for a future development to correct this. The future, or at least 57 years of it, has just said, “well it was good enough for Feynman, et al, so it’s good enough for us”. My point is just that, in fact, it was *not* good enough for them, and they *were* looking for something better.

16. **Thomas Larsson**
August 27, 2004

Chris,

There is certainly room for improvement of the standard model; e.g., I believe that understanding the group of gauge transformation may cast light on things like confinement and renormalization. However, an improvement would not make the standard model wrong, like special relativity didn’t render Newtonian mechanics wrong. Incomplete, yes, but wrong, no.

17. **Chris Oakley**
August 27, 2004

A website dedicated to slaying the standard model would rightly be laughed at.

Read the Nobel prize speeches of Feynman, Schwinger and Tomonoga and decide for yourself whether or not they saw room for improvement in QED.

18. **D R Lunsford**
August 27, 2004

Thomas, I heartily concur – in fact Peter has brought back my enthusiasm for discussing physics, starting with his courageous posts to SPR.

I have access to some excellent forum software that runs (flawlessly) on Linux. I’m sure if we put it up, and created some topics, the boards would be jammed with conversations.

If you want to see it in action go to z spot iwethey spot org.

-drl

19. **Thomas Larsson**
August 27, 2004

In general, the important question is how to deal with the fact that the academic power structure in particle theory is now completely dominated by an entrenched cadre of people devoted to a failed dogma. Trying to set up a new, alternative power structure looks near to impossible, but the current one is based on such a shaky foundation that its collapse sooner or later seems inevitable. The question is how to help that process along so it happens before we’re all dead and gone.

Peter, I think you underestimate your own power. You, and this forum, are undoubtedly starting to wield non-neglible power, for the simple reason that you are right and people know it. Of course, pointing out that the emperor is naked will only be taken seriously if he really lacks clothes. A website dedicated to slaying the standard model would rightly be laughed at.

20. **D R Lunsford**
August 27, 2004

ROFL!

21. **Chris Oakley**
August 27, 2004

I’m not convinced by Vafa’s argument. When Dorothy entered a parallel universe, the first thing she found was a wicked witch.

OTOH the aliens, good and bad alike, may have left our universe because they found it too boring ("What, the Earth-bars don't serve Ammonia? The Earthlings can't levitate? Nobody smokes Magnesium? … Let's get out of here!")

22. **D R Lunsford**
August 26, 2004

OMFG! The "Over the Rainbow" Hypothesis!

Das Regenbogens?berf?hrungshypothese!

23. **Chris W.**
August 26, 2004

From a footnote in physics/0308078:

"Cumrun Vafa thinks that the fact that we do not see aliens around could be the first proof of the existence of brane worlds: all advanced aliens would have emigrated to better parallel universes (our Universe has zero measure)."

Read it and weep…

24. **Peter**
August 25, 2004

Hi Chris,

A conference devoted to alternative to string theory would in principle be a good idea, but someone with funding and credibility has to be willing to organize it.

Conferences tend to fulfill two separate kinds of functions.

1. Allowing people working on the same topic to get together, talk and find out what each other are doing.

2. An important social function of determining power and status within the community. This is determined by who gets invited, who gets invited to speak, who hangs out with whom, etc.

One problem with an alternatives to string theory conference would be that mostly people not doing string theory are doing very different things, with very different philosophies, and it's unclear how much they really could communicate usefully with each other. The loop quantum gravity community is one of the few that has enough people to do this usefully, and they have their own conferences.

If a conference were organized by people or an institution with little or no status in the particle theory community, it couldn't fulfill the social function. It would be completely ignored by mainstream physicists, no one would care who was invited or who spoke. Networking at the conference wouldn't be very helpful as you would be meeting people who have as little power as oneself. Keeping the crackpots from dominating the thing would be no easy task either.

In general, the important question is how to deal with the fact that the academic power structure in particle theory is now completely dominated by an entrenched cadre of people devoted to a failed dogma. Trying to set up a new, alternative power structure looks near to impossible, but the current one is based on such a shaky foundation that its collapse sooner or later seems inevitable. The question is how to help that process along so it happens before we're all dead and gone.

25. **Chris Oakley**
August 25, 2004

Peter,

It seems to me that it might not be a bad idea to organise an "Alternatives to Superstrings" conference/workshop. What do you think?

26. **D R Lunsford**
August 24, 2004

On the "Science Channel" tonight (formerly "Discovery Science") Hawking is repeating the party line, CC, canceling to 120 places not enough, accelerating, string theory takes us close, nothing left to discover, etc. etc. etc.

Is it possible to just have a science show that DOESN'T have an obsession about "it all"?

Hawking, Galileo, and Einstein are described as "daring revolutionaries, scorned tradition, brilliant radicals" etc. etc. More BS. Einstein scorned tradition? Which one? The guy in Manhattan who owns the bagel shop?

27. **Peter**
August 23, 2004

I'm certainly no expert either, but my impression was that the answer to this question is that these solutions are not static, but metastable. Sooner or later they tunnel to zero energy.

28. **Steve**
August 23, 2004

In concede I am no expert in these matters and my knowledge is probably out of date by now, but how can one use "The String Theory Landscape" to justify a cosmos with a tiny positive lambda (ours)?

A clear-cut no-go theorem (e.g as discussed by Maldacena and others) states that you cannot compactify 10-dimensional superstring effective actions (or supergravity) on static internal compact geometries, down to 4 dimensions and then get an induced positive potential or CC in the dimensionally reduced effective string action (leading to accerating cosmological solutions or deSitter space). Even Witten says he doesn't know how to get deSitter space or a CC out of even classical string compactifications. ("Quantum Gravity In de Sitter Space" preprint.)

Also, a small cosmological constant or potential term in a dimensionally reduced effective string action will manifestly break S-duality (at least for toroidal compactifications). In some ways that might be good since very small numbers in physics (e.g. non-zero particle masses for example) seem to be associated with broken symmetries. One can compactify string actions on time-dependant internal hyperbolic spaces for example (relaxing the constraints of the no-go theorem) and try to get accelerating cosmological solutions that way, but I just don't get all this "Landscape" reasoning to "explain" a small CC.

I heard a bit about the Stanford stuff and flux compactications but it seems very artificial. A whole anthropic dogma seems to be emerging based on this "Landscape" though. Even with a vast landscape of solutions/vacuua for 4-dimensional string theory, based on known compactication technology, surely none of them should have a small CC? I admit I have lost the plot now with string cosmology, not having followed any literature for quite a bit, although I don't think the subject is developed enough to really deal with such cosmological issues with any real confidence anyway. Maybe Urs or someone can enlighten/update me here or point me to some useful preprints? (which I would welcome).

I still believe the CC (or dark energy if you prefer) is a hard number that has to be (somehow) rigorously calculated or explained and not "anthropicised" away.

29. **D R Lunsford**
August 23, 2004

The funny thing is – I wrote this paper which shows that in order for Riemannian geometry to be a proper limit of Weyl geometry, lambda=0 – and this was already known by Pauli in the 4d context long, long ago.

Never was so much effort spent by so many on something so zero. The CC is nothing but an artifact of an assumed global symmetry, and somehow it got promoted to God in the Machine.

A scientifically honest person must spend his days filled with Sartrian nausea at such a spectacle.

30. **Peter**
August 23, 2004

Maybe, but religious fanatics will always be with us and can generally be easily ignored. More serious is the abandonment by leading physicists of the scientific method for understanding the world in favor of a dogma which explains nothing and for which there is no evidence. And it's not even a very attractive or satisfying dogma.

31. **JC**
August 23, 2004

Any bets the anti-science religious fanatics will use articles like this to justify their religions and/or ideologies, or as a way to discount "science" as irrelevant?

New Word Enters English Language

August 25, 2004
Categories: Uncategorized

A new preprint by Tom Banks is out, about his idea of "Cosmological Supersymmetry Breaking". One notable aspect of the paper is a new terminology to describe Weinberg's "prediction" about the cosmological constant. Since the term "anthropic principle" normally applied to this has acquired a bad odor as it becomes clear it is not science, Banks decides to come up with a different name for the argument. He refers to it as the "galactothropic principle of Weinberg". Let's see if this catches on…

Comments

1. **D R Lunsford**
   August 29, 2004

   I hated Dr. Faustus. If you want to read a good book about a composer's life, try "Jean Christophe" by Romain Rolland. And it's French! Impress your Republican friends!

   (This public service message brought to you by Citizens Against Post-Romantic Fulmination.)

2. **Urs Schreiber**
   August 28, 2004

   > WHAT DOES LEVERKUHN DO FOR A LIVING

   Let me tell you that pretty much all I know abut this novel is its title:

   'Doktor Faustus. Das Leben des deutschen Tonsetzers Adrian Leverkühn, erzählt von einem Freunde'

   (where this friend is of course your namesake).

   'Tonsetzer' is an old-fashioned (and maybe never really popular) word for composer.

   Is anyone feeling this is getting off-topic?

   Ok, let's see how we get from literature back to physics: A couple of weeks ago I read Leon de Winter's 'God's Gym'. Among other things a tragic car accident gives rise to some general philosophy – and to a discussion of – string theory.

   Oops, again off-topic… 😉

3. **serenus zeitblom**
   August 27, 2004

   Ok, now you are free to correct the numerous shortcomings in my English texts… 🙂 (And in fact I'd appreciate it.)

   Sorry, English isn't my native language either!
   I have struggled to understand that wonderful book in both German and English and I am afraid that I have not succeeded in either case…..for example, Urs, WHAT DOES LEVERKUHN DO FOR A LIVING, that's what I would like to know!

4. **Steve**
   August 27, 2004

   This universe (or the region we occupy in the parameter space or "landscape")is not exactly conducive to intelligent life or even life itself at a simple level. Statistical mechanics can allow complex structures to briefly arise here and there before entropy washes them out, some of which (like us) can briefly be aware of their existance and the universe they find themselves in. But the frigid interstellar spaces are not exactly awash with radio signals from other intelligent civilisations, and all the other worlds we have discovered and explored in this solar system and elsewhere are just balls of molten/frozen rock or poison gas. It is a very very harsh and forboding place.

   There are so very many vagaries and frozen accidents that have led to our existance as an intelligent species. For example, some 6 million years ago severe drought turned the dense forests in Africa into savannah so the apes were forced to come down out of the rapidly vanishing trees.
   If we run the whole history of the earth again there is no gaurantee that any intelligent life (or even the simplest life)would arise on this planet at all, and it will be the same elsewhere. There would be many universes in the parameter space/landscape that still allow complex structure to form and some of this structure could end up "self-aware" but it does not at all follow that we would recognise it as life or it would recognise us. The Hoyl and Weinberg arguments therefore just state that bound structures like carbon nuclei, and stars and galaxies can form and the parameters we have allow layers of complex structure to briefly evolve.

   In a universe where electrons/fermions feel nuclear forces for example, there would just be nuclear physics and gravity and no complex structures like molecules, cystals, planets, life. I am not even quite sure what I am arguing about now:) However, I am not convinced such other universes or a "landscape" exists though and we need to explain why this one is the way it is with these parameters without anthropic arguments.

5. **Fabien Besnard**
   August 27, 2004

   I would like to comment about the scientific status of the anthropic argument. All the known constants of physics (including some integers like the number of dimensions, the signature of the Lorentz metric, etc…) live in a rather huge parameter space P. All I have seen so far are hand-waving arguments claiming to show that only a small neighbourhood of the known point in P we occupy gives rise to a universe with enough carbon production to host (intelligent) life. First I would like to know if someone claims to have investigated all of the parameter space, because as I remember, only a few constants are varied at the same time (sometimes only one at a time !). Also, I think this kind of argument overestimates our ability to draw complex conclusions from first principles. Would one be really able to calculate how a universe would be like with only the standard model, GR, (even less likely : string theory ?) and a point somewhere in the parameter space ? One thing that history should have taught us is that Nature has always more imagination than we do. I do not think we can seriously claim to know every possible way for a universe to host intelligent life. Is carbon really necessary ? It makes me think of these old sf movies where alien creatures are obviously humans in disguise : our imagination never goes very far from where we stand.

6. **ksh95**

August 26, 2004

"….I always wondered whether some of the old technical terms in math and/or physics, had a German origin. Terms like: ansatz, eigenvalue, eigenfunction, zitterbewegung, bremsstrahlung, etc

It seems like the usage of German words in math and/or physics, became less and less common after World War 2…."

Hmmm…Seeing how Germany was the science mecca post WWII with the US assuming that role in the post war era, I'd say that makes perfect sense.

7. **JC**
August 26, 2004

Urs,

I always wondered whether some of the old technical terms in math and/or physics, had a German origin. Terms like: ansatz, eigenvalue, eigenfunction, zitterbewegung, bremsstrahlung, etc …

It seems like the usage of German words in math and/or physics, became less and less common after World War 2.

8. **raj**
August 26, 2004

>Milky way translates to Michstrasse not Milchweg – the 'milky street'!

Milchstrasse, not Michstrasse. "Mich" is the accusative of "Ich" ("I"). "Milch" is "milk"

9. **Chris Oakley**
August 26, 2004

Re: Humor – actually the *Schlaf-mit-Ihren-Frauen-Prinzip* did make me chuckle … BTW Danny – why don't you have a web site? There are physics web sites out there that make mine look tame, but I still read them … you really ought give us a comprehensive "DRL's view of the Universe" rather than the snippets we get here.

10. **D R Lunsford**
August 26, 2004

Humor Urs. Humor.

11. **Urs Schreiber**
August 26, 2004

Milky way translates to *Michstrasse* not *Milchweg* – the 'milky street'!

And unless the milkman serves a homosexual household it will be the *Schlaf-mit-Seiner-Frau-Prinzip*, or, if he is good, the *Schlaf-mit-Ihren-Frauen-Prinzip*.

Finally we learn that 'Serenus Zeitblom' has read Thomas Mann but is not a native German speaker. 😉

Ok, now you are free to correct the numerous shortcomings in my English texts… 🙂 (And in fact I'd appreciate it.)

I don't know what a good German translation of 'galactothropic principle' would be.

First of all 'anthropic principle' is just *anthropisches Prinzip* in German.

Next 'galactothropic' seems to be ill-motivated all by itself, after all the '-thropic' comes from the second half of Greek 'anthros' or something like that, doesn't it? Better would be the 'galactic principle' or the 'galactocentric principle', I'd say.

But in my opinion it would be even better to realize that all these principles are really known as 'deduction from observation'. Something is observed (the existence of human beings, of life, of galaxies) and conclusions about future observations are made from that. There is nothing more common in science:

I observe that the photographic plate has been darkened and deduce that some radiation must have hit it. Should I call this the 'photographicplatethropic principle'? 😉

12. **serenus zeitblom**
August 25, 2004

Menschenmilchwegzentrumsprinzip!
That is not the correct translation of galacto-anthropic. It should be "Milchmannprinzip". Also known as the Schlafmitihrerfrauprinzip.

13. **JC**
August 25, 2004

Heh. Sounds like a semi-Orwellian attempt at whitewashing the anthropic principle.

What would be amusing is if there ends up being a "thought police" which attempts to stop and/or discourage all uses of the word "anthropic" in string theory, while the underlying research work is exactly that of the anthropic principle.

14. **D R Lunsford**
August 25, 2004

These things sound better in German!

Menschenmilchwegzentrumsprinzip! Jawohl, ich glaube streng daran!

DPF 2004

August 31, 2004
Categories: Uncategorized

The Division of Particles and Fields of the American Physical Society has been having its annual meeting at UC Riverside during th past few days, and some of the plenary talks have been put on-line.

Particularly interesting is the talk by Jamie Rosenzweig about Advanced Accelerators: Near and Far Future Options. It reviews ongoing development of existing technologies for use in the next (post-LHC) generation of accelerators, including the superconducting RF cavity technology recently chosen for use in a possible electron-positron linear collider. But it also covers some of the more exotic acceleration technologies that people are thinking about, including optical lasers and plasma wake-fields. Some of these technologies, if they could be made to work, hold the promise of creating much higher accelerating gradients and might allow the construction of much higher energy linear accelerators. The future of particle physics may end up depending on the success of these efforts.

The review of New Models of Electroweak Symmetry Breaking is interesting, although mainly in that it shows that the ideas going around about this aren't very compelling, and perhaps some dramatically new ones are needed. The reviews of heavy flavor and neutrino physics give a good idea of the current experimental situation. Still to be posted are talks by Clifford Johnson on "Current Trends in String Theory" and by Sean Carroll on cosmology. Carroll also has an interesting discussion of the current state of tests of general relativity on his weblog.

Comments

1. **Matthew Nobes**
   September 4, 2004

   Conceptually the charmonium states can be thought of like positronium (that's the reason for the name), but you cannot use a pure 1/r potential in a model. Model calculations typically use a 1/r + r or 1/r+log(r) potiential, where the latter term "mock's up" confinement. Even then you have to account for relativitic corrections, which can be quite large for charm. For the bottom system, the potiential model approach works better.

   This only works because the quarks are heavy, so essentially non-relativistic. You cannot use this approach for bound states with light quarks, or to compute decays. But, to the extent that a 1/r + r potential is "funky QED" it can be done for heavy quarkonium states.

2. **Steve Lidia**
   September 3, 2004

   If you're interested in advanced accelerators, you might want to consider coming to our 'Advanced Acceleration Concepts' Workshop. It's held in the US every other year – this year's was hosted by Brookhaven Nat'l Lab and held at SUNY Stony Brook. There's a good website if you want to see the range of topics and interests covered – http://www.bnl.gov/atf/AAC04.htm .

   Also – you might want to visit the physics dept. at Columbia. Tom Marshall has been a long-time participant and contributor to this field.

3. **Chris Oakley**
   September 3, 2004

   My point was just that the spectrum of c/c-bar resonances look a lot like the energy levels of positronium, so there must be something going on that is similar in terms of the binding force. Someone gave a talk about the similarities when I was at Oxford 20 years ago, but I can't remember all the details.

4. September 3, 2004

   Well on refreshing my memory there are other resonances for c-cbar states, see here:

   http://www.e835.to.infn.it/people/gollwitz/pdg_charmonium.html

5. September 3, 2004

   Chris,

   Charmonium is just another name for the J/psi meson (c-cbar). It's made in electron-positron scattering and decays into pions, so
   something like

   e+e- –> c-cbar = J –> pions

   -drl

6. **Matthew Nobes**
   September 3, 2004

   It's remarkable that any experimental information at all could be pulled from the theory, and to be honest I don't know exactly how this was done. Someone?

   By hook and crook. Before QCD there was the SU(3) models of Gell-Mann. And the whole current algebra programme. Both of these produced information that follows from symmetry considerations in QCD. Then there was Bjorken scaling, which pointed the way to the full theory of QCD.

   Nowadays, there are plenty of ways to extract information from QCD

   1) Perturbation theory for high energy processes

   2) Lattice calculations

   3) Effective field theory analysis (chiral perturbation theory, heavy quark effective theory, …)

7. **Chris Oakley**
September 3, 2004

You really can't even naively imagine QCD like "a strong funky QED"

Charmonium?

8.
September 3, 2004

Chris,

You really can't even naively imagine QCD like "a strong funky QED" because the basic interactions are completely different, i.e. in QCD one has gluon loops as well as quark-antiquark pairs in the vacuum.

Plus, the coupling is on the order of 1 rather than 1/100, so even the simplest calculations are intractable.

It's remarkable that any experimental information at all could be pulled from the theory, and to be honest I don't know exactly how this was done. Someone?

-drl

9. **Chris Oakley**
September 3, 2004

I am a bit rusty, so can someone remind me why one could not have quarks being extremely heavy with extremely high binding energy, making it always energetically preferable to produce composites in scattering events rather than lone quarks?

10. **Peter**
September 3, 2004

With strongly interacting relativistic particles like quarks or gluons, you really can't usefully think about the mass of a state as being a sum of masses of its composites and a potential energy contribution. In the limit of massless quarks the spectrum includes:

1. massless pions that are in some sense composites of two quarks.

2. massive baryon states that are in some sense composites of three quarks.

3. massive "glueball" states that in some sense have no quarks.

There's no way of understanding these things in terms of free particles bound by a potential. For one thing, as you note, you can't even define the mass of an unbound quark since such a state carries infinite energy.

11. **James Graber**
September 3, 2004

Although I've heard something like this before, and even read articles by Frank Wilczak on the same subject, I still find Matthew Nobes' comment hard to understand. A helium atom and a neutron weigh less than a deuterium atom and a tritium atom. The difference, the binding energy, is available to be released by a fusion interaction. Similarly, a neutron weighs more than a proton plus an electron plus a neutrino, and hence decays with a positive release of energy. The rule is mass of bound particle equals mass of constituents minus mass equivalent of binding energy. Therefore if two or three massless quarks are stably bound together with a positive binding energy, it would appear that the bound meson or nucleon should weigh less than zero. What gives?

I guess it somehow relates to "asymptotic freedom / infrared slavery" and hence the quarks are only massless when confined, but are actually infinitely massive when deconfined. Could one then say that most of the mass comes from the fact that the quarks are only imperfectly confined? At least this way the binding energy has the right sign. In this way of speaking, the "massless pion" is massless only because the two infinitely massive quarks are bound together by an equally infinite amount of binding energy.

Jim Graber

12. **Matthew Nobes**
September 2, 2004

Well, the standard model does not predict masses for the elemtary particles, that much is true. It is interesting to note though, that if you take QCD with two massless quark flavours, you still get a theory that roughly approximates the real world. That is, very light pions (in this case massless) and nucleons with approximately the right mass.

This is because most of the mass that we see around us is actulally the binding energy of the gluons in protons and neutrons. The actual masses of the up and down quarks contribute very little to this.

Now of course it's very hard to predict the proton mass using QCD, even with just two flavours of massless quarks. But it is possible in principle, and it's something that can be done in the framework of QCD alone.

13. **Peter**
September 2, 2004

In the standard model, the masses of elementary particles are the product of the electroweak vacuum symmetry breaking mass scale and the strength of the coupling of the particle to the field causing the symmetry breaking. No one knows why either of these two factors take the values they do.

14. **Tim M.**
September 2, 2004

I haven't studied particle physics (or any advanced physics) and have a simple question: do any theories predict masses of particles? From what I understand, this is cited as a failure of the Standard Model, so I am curious to know if other theories predict masses for any of the elementary particles.

15. **Peter**

September 1, 2004

Hi William,

I may take you up on that offer next time I'm in Berkeley. I've never understood why this sort of research doesn't get more attention given its potential importance.

16. **Simplex**
September 1, 2004

Sean Carroll's DPF talk on cosmology has now been posted.
(You say "still to be posted" so this is an update)

17. **william barletta**
September 1, 2004

It is great to see such a nice plug for advanced acclerators in your blog. These devices have still a long way to go to be useful for particle physics. However I expect that they will be the basis of a user facilty for other fields of physics in the next decade. If you visit LBNL, we will be happy to give you a tour of our laer wakefield laboratory.

Self-congratulatory Meta-post

September 1, 2004
Categories: Uncategorized

When I first started this weblog I thought very few people would be interested in reading it. I've been very pleasantly surprised both by the general high quality of the comments people contribute and by the ever increasing number of people reading "Not Even Wrong". For the last few months I've been running a program that gathers statistics from the web server logs. Here are the monthly numbers for accesses to the main weblog page:

May: 4532
June: 7194
July: 8697
August: 10427

These numbers don't include a lot of the traffic, which consists of people coming directly to one of the postings, via a Google search or a link from somewhere else.

Comments

1. **sol**
   September 7, 2004

   Arivero*Last year I proposed a parallel example: alchemical transmutation in grounds of the general theoretical principle of preservation of energy is viable (Mercury, Hg 201, transmuting to gold, Au 197, plus He and electron debris), but no mechanism is known to achieve a decent rate*.

   It is funny that you would speak on alchemical associations, and make it sound like cold fusion transformations? 🙂

   Blackholes reproduced for inspection?

   One has to wonder then, which method will help us find this free energy source? Maybe it calls for a philosophers stone(a new quantum theoretical position)?

   http://physicsweb.org/objects/news/6/2/3/020203.gif

2. **Alejandro Rivero**
   September 6, 2004

   It doesn't fit here. The CF dispute was about experimental procedures, not about theoretical or mathematical issues. Of course theories were proposed, but it was not the relevant point.

   The theory part of experimental procedures is touchy thing; you are expected to have a valid or credible mechanism and also to verify experimentally its collateral consequences.

   Last year I proposed a parallel example: alchemical transmutation in grounds of the general theoretical principle of preservation of energy is viable (Mercury, Hg 201, transmuting to gold, Au 197, plus He and electron debris), but no mechanism is known to achieve a decent rate.

3. **RT**
   September 5, 2004

   Re cold fusion

   Julian Schwinger invested a significant effort in an attempt to understand cold fusion. (This episode is described, with a palpable twinge of embarrassment, in Milton and Mehta's Climbing the Mountain)

   As Schwinger:

   1) remained true to quantum field theory, defying the dictates of fashion

   2) evinced considerable scepticism about theories that invoked things that could not be observed and/or vaulted over many orders of magnitude in energy or whatever without a thought

   perhaps cold fusion has found its spiritual home on this site. Let's see what happens.

4. **Chris W.**
   September 4, 2004

   More on this story from the Boston Globe, focusing on Peter Hagelstein of MIT:

   [Heating up a cold theory](#) (July 27)

5. **Arun**
   September 4, 2004

   The return of cold fusion:
   http://www.spectrum.ieee.org/WEBONLY/resource/sep04/0904nfus.html

   seems appropriate for Not Even Wrong.

6. **Rafael**
   September 4, 2004

   I found your site vis-?-vis a search on mathematics blogs. Well I like physics too so I started reading it. Still haven't found any math blogs unfortunately.

7. **sol**
September 3, 2004

Most would know me as a hobbist, and outside of academia. I like exploring the world you gentlemen venture into.

Like someone else said, keeping current with the information is nice.

This trend with the educators showing there ideas is a nice way to see what the thinking is from those different perspectives.

I can't speak with any authority, although the models of discovery on the issues of quantum gravity, and depending on which model you choose, I think the desire and interests are the same all around, to trying to find a way to describe the nature of the geometry at planck length.

Am I wrong about this?

Those statistics of site visits is a testament to the way the internet can work espcially if your posts are linked from other sites. This drives the hits, and the hits the information you are supplying.

Even knowing your bias Peter and some of the others that post here, no one can be faulted for looking for a method. Even Lubos. What personality issues might rub others, should not deter one from the value of information that is put across.

I like the blog set up as well, even though I have used other site characteristics for demonstrations to learn. Good on you, Sean and others.

8. **Alejandro Rivero**
September 2, 2004

Thomas, I was not using charge eigenvectors. For the fermions I was counting mass eigenvectors, or subspaces if you want to account for degeneracy due to charges (but note mass and charge does not commute). Well, that is 12 fermions. For the bosons I was counting the number of generators in U(1)xSU(2)xSU(3), that is 12 again. Sorry the different criteria, but in both cases I was avoiding the use of charge eigenstates.

Now, how should strings organise this experimental input? I would welcome something between F-theory and heterotic strings. I'd assume, from the evidence both in NCG and deconstruction, that Higgses are similar to spatial coordinates, each complex higgs doublet providing four of these. Then 4 space time plus two Higgs doublets make 12 "coordinates". Thus the question is how to arrange these 12 "coordinates", 12 fermions and 12 bosons. I'd put the SM bosons instead of the bosonised fermions of heterotic strings, and I'd join the 12 coordinates with the 12 SM fermions to make superfields.

This is, I am inclined to believe, from experimental and mathematical input, that the maths of string/M/F theory are not completely wrong, but that the physical interpretation is completely, completely different of the ones they are trying.

9. **Arun**
September 2, 2004

Seems obvious to me that that string theory should be explored, and who better to explore it than those who believe in it? A theory of everything with no experimental signature is discouraging to be sure. But are there any objective arguments that that some other set of problems will be more fruitful to attack?

But he who believes in a speculative theory should not disparage other people's exploration of other approaches, e.g., as in "if it is not string theory, it cannot be a description of gravity". Pointing out the shortcomings of other approaches, specifically, LQG, is, of course, a good thing to do. But then, one should do it for the theory one believes in as well. Sound scientific skepticism is a virtue.

-Arun

10. **Thomas Larsson**
September 2, 2004

Alejandro,

Sorry, but I don't understand what you are saying. Should the coordinates in the superspace of the heterotic string be identified with physical particles? Surely at least four bosonic coordinates should be identified with the spacetime that we know, love and inhibit.

Besides, how do you count? Each quark has 3 color * 2 spin degrees of freedom, and the electron has two spins. So with that counting, you have 15 fermionic dofs *per generation*, not counting anti-particles? Moreover, you probably should count the graviton and Higgs particle as well. They have of course not been seen experimentally, but something like that must surely be there.

11. **Alejandro Rivero**
September 2, 2004

Well, as I have just stated, the experimental input is that there are 24 different kinds of particles, and the bosonic string has 24 transversal directions.

Say this, I agree that string theory researchers have a very strong tendency to neglect experimental input. They just got fortunate that the theory itself have some appreciation for it.

12. **Thomas Larsson**
September 2, 2004

I suspect that this blog has a large audience of believers in string theory [like me]

Serenius, while I don't doubt your sincerity, I seriously don't understand how you (and others) can believe in string theory.

We all know that string theory makes no hard predictions (hence the name of this forum), but it makes quite a few soft predictions, e.g. supersymmetry, extra-dimensions, new gauge bosons, a negative cosmological constant, new long-range forces (massless scalar particles associated with moduli), etc. The most striking thing with these soft-predictions is that *none* of them has been confirmed by observation, and some (the negative cosmological constant and perhaps also supersymmetry) may even be in direct conflict with experiments.

Does this not count? Maybe I'm no longer a physicist, but whatever physicist is still left in me reacts strongly when people simply ignore experimental information. There is no doubt that string theory is in apparent disagreement with experiment, and by far the simplest explanation for apparent disagreement is that it is due to real disagreement.

String/M theory may be the language in which God wrote the word, but experiment is the language with which He communicates with mortals. And He seems to say very clearly that general relativity and the standard model are very special.

13. **Alejandro Rivero**
September 2, 2004

On the positive side, LM insistence has carried me, and I suppose more people, to read the GSW (volume 1, only…), a task I had been escaping from for years.

BTW, Comparing the heterotic string with the real experimental input of the standard model (12 fermions and 12 bosons) one suspects that the M stands for "Missing". They must be looking for a missed theory with 12 fermionic degrees instead 10. Or they should.

14. **Chris Oakley**
September 2, 2004

"LQG is not string theory, and therefore it can't describe gravity"—you know who

Motl is sort of like the bouncer to the superstring night club – guaranteed to be loyal, provided you keep paying him, and always ready to keep out or evict those who refuse to abide by the rules of "the management".

Once again, though, I pose the question, what has this got to do with science?

15. **Arun**
September 1, 2004

Hehe, I suggested "used car salesman" not as a path to career success, but as a good fit with a known set of obnoxious characters.

16. **JC**
September 1, 2004

Main thing I like about this blog is how some folks are willing to "call a spade a spade" (even when it's "unpopular" to do so in some circles), and pointing out when the "emperor has no clothes on". Sort of like a physics version of Bill Mahr's "politically incorrect" show.

Speaking of Lubos Motl, if I never heard of the guy originally, I would have thought he was a "cartoon" character of some sort. Sort of like a cross between Wile E. Coyote and Charles Manson, except fanatical about string physics instead of violence.

17. **serenus zeitblom**
September 1, 2004

"If he ever considers a career outside of physics, I suggest used car salesman"
Sorry, you are wrong. No offence to Peter, but I think that the popularity of this blog owes much to the sheer *incompetence* of string theorists as salesmen. As someone said to me, "I don't think that string theory is wrong. But when I read sci.physics.strings, I wish it were!" [That was in the days when postings at sps averaged more than one or two a day, as at present.]Another good reason to read this blog, even if [as is my case] one does not agree with much of what is said here: where else can you get the latest news? How else would we learn about the adventures of El Susskind? If LM had any sense, *he* would be broadcasting all this stuff over at sps instead of [ab]using it as a place to humiliate people whose papers offend him. I suspect that this blog has a large audience of believers in string theory [like me] who use it as a news service, because no believer is commenting on the latest developments!

"LQG is not string theory, and therefore it can't describe gravity"—you know who

18. **Arun**
September 1, 2004

I too give up on Lubos Motl.

If he ever considers a career outside of physics, I suggest used car salesman.

19. **D R Lunsford**
September 1, 2004

Chris –

Even in a pefect world this site would be fun. It's like having a beer with physics buddys. If only Peter would install a pool table!

-drl

20. **Chris Oakley**
September 1, 2004

Peter,

I too have nothing but praise for this weblog, but it does beg the question – Why should it be necessary? Why should a large number of otherwise extremely smart people be so soft-headed that they are unable to tell the difference between science and mathematical speculation, fact and fiction? If the research establishment is sending a large number of its best minds off on a wild goose chase then is it these young people's fault for not rebelling, or is it the academic system's fault for not teaching or encouraging criticism of itself?

Could it be that the academic system considers that there are things that are more important than advancing physics?

21. **BB**
September 1, 2004

Hi Peter,

I must congratulate you on the quality of the website. I am an ex-particle physicist and find myself in agreement in much of what you say, most of the time. My philosophy wrt string theory is the same as with other fields—I just enjoy learning about new non-trivial ideas, be it condensed matter, or string theory or algebraic geometry (some day I hope to understand it!). But the zealotry is unbearable.

I am no longer in academia, but am very keenly interested in what real progress is being made—the hype from the real thing. Of course, I look at arxiv, but my time is limited, sadly. So I really appreciate the varied variety of posts (as opposed to a String Theoy blog)—the recent DPF post is an excellent example.

I happened to find your website by chance via Google (perhaps a Motl related search? :)). You have no idea how much I appreciate reading your posts and the follow-up comments; you often make my day!

Please keep up the great work!

BB

22. **D R Lunsford**
September 1, 2004

This is my favorite physics site, because it’s sincere and specific. Well done sir!

Perelman and the Poincare Conjecture

September 8, 2004
Categories: Uncategorized

One of the great stories of mathematics in recent years has been the proof of the Poincare conjecture by Grisha Perelman. This has been one of the most famous open problems in mathematics and has been around for about one hundred years. In technical terms the conjecture is that if a space is homotopically equivalent to a three-dimensional sphere it is homeomorphic to the three-sphere. In less technical terms it says that if you have a bounded three-dimensional space in which all loops can be shrunk down to points, it has to be the three-sphere. In dimensions other than three the analog conjecture has been proved, but the case of three dimensions has resisted all attempts to solve it.

Perelman spent time as a visiting mathematician at Stony Brook, Berkeley and NYU, then went back to St. Petersburg where for eight years he seemed to disappear from mathematics research. In November 2002 he posted a preprint on the arXiv, which quickly drew a lot of attention. He seemed to be claiming to have a proof of an even more general conjecture than Poincare, known as the Thurston Geometrization conjecture, but the way his preprint was written, it wasn't clear whether he was claiming to really have a proof. The method he was using was one pioneered by my Columbia colleague Richard Hamilton, called the "Ricci flow method". This involves something like a renormalization group flow to a fixed point (for more about this, see the talks by Ioannis Bakas at a recent conference in Crete). If you start with an arbitrary metric on a space you think might be a three-sphere, the hope was that Hamilton's Ricci flow would take you to the standard metric for the three-sphere. Hamilton had made a lot of progress using his techniques, but as far as pushing them through to give a proof of Poincare, he was stuck.

In the spring of 2003, Perelman traveled to the US and gave talks at several places, including a long series at Stony Brook. By then he was explicitly claiming to have a proof, but few of the details were written down, although he did post two more preprints to the arXiv. His talks were major events in the math community, and at them he was able to answer anyone who asked for details on specific points of his argument. He gave a somewhat informal talk at Columbia one Saturday, a talk that I attended sitting next to Hamilton, who was hearing Perelman speak for the first time. Hamilton was clearly very impressed, and soon thereafter he and most other experts began to become convinced that Perelman really did have a way of proving the conjecture.

By now the situation seems to be that the experts are pretty convinced of the details of Perelman's proof for the Poincare conjecture. The full Geometrization conjecture requires some more argument and I gather that Perelman is supposed to at some point produce another preprint with more about this. A workshop was held a couple weeks ago about Perelman's work at Princeton and several people have been carefully working through the details needed to be completely sure the proof works. For this material, see a web-site maintained at Michigan by Bruce Kleiner and John Lott.

One interesting part of this story is that the Poincare conjecture is one that the Clay Mathematics Foundation has put a one-million dollar price tag on. There's an elaborate set of rules that Perelman should follow to collect his million dollars. This is supposed to begin with the submission of a detailed proof to a well-known refereed journal, something Perelman hasn't done and shows no signs of doing. As far as anyone can tell, his attitude is that he's not interested in the million dollars. If you look closely at the rules, it doesn't necessarily have to be Perelman who writes up the proof. Someone else may do it, with Perelman still getting the money. Ultimately the question of the million dollars is to be decided by the Scientific Advisory Board of the Clay Mathematics Institute, and one question they will have to face is whether to split the award between Perelman and Hamilton.

Another interesting question concerns the Fields medal, the most prestigious award in mathematics. These are awarded every four years at the International Congress of Mathematicians, the next one of which will take place in Madrid in the summer of 2006. One stipulation for the award of the Fields medal is that a recipient must be under the age of 40. Seeing Perelman speak, I had assumed he was already at least forty, but this is not so clear. No one seems to be sure exactly what his age is and whether he will be under 40 in 2006. Some news reports from spring 2003 referred to him as being in his late 30s or even 40, some recent ones claim that he is now 37. His first scientific paper was published in 1985, so he would have had to have been 19 or younger at the time to be under 40 in 2006. If Perelman really is under 38 now, he's a sure thing for a 2006 Fields medal.

For a really dumb news article about this, go here (no, proving the Riemann hypothesis won't bring down the internet, and Perelman's Poincare proof won't explain the nature of the universe).

Comments

1. September 15, 2004

   Here is the link to the scores for that 1982 Olympiad

   http://www.srcf.ucam.org/~jsm28/imo-scores/1982/scores-order.html

2. September 14, 2004

   As a non-mathematician, I'd sure like to see this thread grow before it gets replaced by something else. These blogs (the good ones) are like gardens where the new blossoms crowd out the old ones, so you end up with only the topmost flowers. What a waste.

   This place needs a forum.

3. **Peter**
   September 14, 2004

   Ooops.

   I just accidentally deleted two interesting comments. They pointed out that Perelman was the winner of the Math Olympiad competition in 1982, that if he was under 17 then he would be under 41 in 2006, and that Noam Elkies came in 4th behind Perelman that year.

4. **dolt**
   September 10, 2004

   There is a nice, short article in Notices by John Milnor on the Poincare conjecture and Hamilton's work and a few words on Perelman's results, which I found helpful.

   http://www.ams.org/notices/200310/fea-milnor.pdf

   The nice property of Ricci flow equation is that is it like a heat equation (clear in the weak-field approximation, see Bakas). Just as heat flows from hot to cold so object gets uniform in temperature, the Ricci flow behaves similarly so "curvature tries to become more unoform", though there are several complications. My understanding is that Perelman showed how to take care of all that.

The article also seems to suggest that the choice of the Ricci flow equation chosen by Hamilton was analogous to Einstein's derivation of his field equation: essentially R_{ij} is the unique 2-index
tensor arising naturally from the first and second order derivatives of the metric. Persumably all other terms that can be written (terms of higher order in the Hilbert action in an effective field theory approach) will contain no additional geometric information.

I am merely scratching the surface, I am sure, but very interesting stuff…

5. **dolt**
September 10, 2004

Peter,

Many thanks for your patient explanations; you are an invaluable resource! I will try to get a hold of the original references and try to understand it better. At least, now I have a pretty good chance of understanding it!

6. **Peter**
September 9, 2004

Just talked to Richard about this since he was upstairs at tea. He started work on this in the late seventies and didn't hear about the connection to the renormalization group and non-linear sigma models until many years later. He thinks the first time these equations occurred in physics were in Dan Friedan's thesis (1980), which he only heard about years later.

7. **Peter**
September 9, 2004

As far as I know, the way Hamilton and Perelman are using these equations has nothing to do with the fact that they are approximate renormalization group equations for a non-linear sigma model. (If I see Hamilton maybe I'll ask him if he knew about renormalization group eqs. when he started studying this equation). For them, the important point is just that the topology of the manifold doesn't change as you evolve the metric according to the Ricci flow (at least until you hit a singularity in the solution of the PDE). The idea of the proof of geometrization (drastically over-simplified…) is to show that, no matter what metric you start with, you end up at one of the finite number of possibilities on the list that Thurston conjectured were all the possible topologies of 3d manifolds. Solutions to the Ricci flow eq. certainly do develop singularities, which is one of the things that makes this very hard.

8. **dolt**
September 9, 2004

I read with great interest the talk of Bakas and tried to understand Perlman's papers.

I have a basic question that perhaps somebody can answer.

I understand that the Ricci flow equation is nothing but a 1-loop RG equation of the 2-dim (world-sheet) sigma-models with target space metric G_{\mu\nu}(X), i.e., \beta(G_{\mu\nu})= -(Ricci Tensor)_{\mu\nu}. The higher order terms are nonzero, but may be neglected for weak curvature. This Ricci flow equation is the basis for Perelman (and Hamilton's) analysis.

How does this 1-loop approximate result become the foundation of studying geometry, especially global toopolgical questions like the Poincare conjecture.

Issues like manifold surgery, Thurston geometrization conjecture are definitely way over my head.

Thanks

9. **Thomas Larsson**
September 9, 2004

Re age limits:
I think Nobel's will stipulates that the Prize should go to the person who, within the appropriate field, has made the most important contribution to the benefit of mankind *during the past year*. One may argue whether academic research really benefits mankind the most – the Swedish inventors organization challenge that, arguing that society benefit more from inventions (like those of AN himself) than from basic research. Be that as it may, it is still hard to argue that your average Nobel laureate did his best work during the last year.

10. **Peter**
September 9, 2004

Thanks for pointing this out, the link to the conference with the Bakas talk should now work correctly.

11. **dolt**
September 9, 2004

The link to Bakas' talk is broken.

I thank you for your field-theoretic explanation of the general idea of the method of Perelman's proof (and the work of Richard Hamilton).

dolt

12. **Peter**
September 8, 2004

It looks like most of the recent media articles were generated by a program at the British Association Science Festival in Exeter a couple days ago called "Million Dollar Maths" where Perelman's proof was discussed by Keith Devlin.

I've been hearing more about Perelman recently from my colleague John Morgan, who was involved in the recent workshop in Princeton. He was the one who told me that Perelman might be young enough for the Fields medal, something I hadn't realized.

It has suprised many people during the last few months to see that maybe Perelman won't bother to write a real paper about this and really doesn't seem to care about the money.

I don’t know if there will ever be any sort of definite point at which people working on Perelman’s papers announce that, yes, it is a proof. As far as I can tell, everyone now believes Perelman’s Poincare result (if not the full Geometrization), so how the story of the million dollars plays out will be interesting to watch.

13. **Suresh**
September 8, 2004

btw why is there a sudden burst of interest in the poincare conjecture again ? has there been a recent verification of the proof ? I have been reading about Perelman’s proofs for at least a year now.

14. **Peter**
September 8, 2004

It is kind of dumb, and most recently this was made clear when Wiles proved Fermat’s last theorem, but was too old for a Fields. The positive argument for this age cut-off is that all too often what happens with these prizes is that a bunch of old guys sit around and decide to give prizes to each other. The age cut-off is a way of ensuring that recognition goes to people who have recently done something exciting and are not already well-established.

15. **D R Lunsford**
September 8, 2004

Age discrimination is a terrible problem in IT work. I can’t believe the Fields Medal is tied to an arbitrary number! What’s the point of that?

Grothendieck Biographical Article

September 14, 2004
Categories: Uncategorized

The latest issue of the Notices of the AMS contains the first part of a long biographical article about Grothendieck written by Allyn Jackson. Evidently Winfried Scharlau is writing a biography of Grothendieck, and Jackson's article is partially based on materials he has gathered. Much of this material is brought together at a website maintained by the "Grothendieck Circle".

This issue of the Notices also contains a short expository piece on one of the most abstract ideas due to Grothendieck, that of a "topos". Illusie was a student of Grothendieck's, and Jackson's article has some of his reminiscences about what that experience was like. Illusie's piece is not very accessible; a better place to try and get some feeling for these ideas is Pierre Cartier's Bulletin article.

Comments

1. **bhargav**
   October 1, 2004

   Regarding books on algebraic geometry, I'd like to point out two sources not mentioned above.
   a) EGA – This is Grothendieck's monumental work and probably not what most people are looking for since it is extremely detailed, formal and dry. But if you prefer abstract nonsense, who knows? This might just be your thing since it is pretty easy to read.
   b) Algebraic Geometry and Arithmetic Curves – This relatively new Oxford University Press title does a fine job of explaining schemes in the first 7 chapters before delving into details about arithmetic curves. Complexity wise, this comes somwhere between Hartshorne and EGA (EGA being way too detailed, Hartshorne being way too dense). This will also be more useful if you're a number theorist since Liu, unlike Hartshorne, does not shy away from char. p and, in fact, devotes many sections/examples to it.

2. **Alejandro Rivero**
   September 18, 2004

   BTW, I asked about G. last time I was in Paris and I was told he left France. The last published notice was about him being hiddeng at some place in the Pyrinees. Any gossip about?

3. **JC**
   September 16, 2004

   Levi, Dan

   Years ago I went through the 2nd volume of Shaferevich's "basic algebraic geometry" books, which covered algebraic geometry from the complex manifold perspective like Griffiths & Harris' book. At the time I ended up glossing over the chapters on schemes, largely out of laziness.

   For the entire time I was in college (both undergrad and grad school), I never really appreciated the notion of abstract math which didn't involve doing heavy computations. When I took math courses like abstract algebra and real analysis, I was always thinking to myself "what's the point of this junk?!?!?!" Even doing particle and/or string theory research, most of it was largely doing computations and not really much hardcore abstract math.

   It was after grad school when I started to look at abstract math again like algebraic geometry, commutative algebra, number theory, category theory, etc … and started to appreciate it a lot more. I guess it came with some mathematical maturity and a change in perspective, that I started to appreciate abstract math more. It took me awhile to become adjusted from the heavy computation mentality to a mindset of proving general theorems and seeing the "beauty" in it. I even went back to some old books on general topology and functional analysis and started to see them in a new light, in comparison to when I previously thought it was all a "waste of time" in the past.

   I guess the only way I can describe "beauty" in abstract math, would be similar how an artist or musician sees "beauty" in a particlar piece of art or music. To a person who has no interest in math, art, or music, they would not see much "beauty" in it and wonder "what's the point" of it all.

4. **D R Lunsford**
   September 16, 2004

   I find the biggest barrier to *any* math learning in a modern context is that *all* of it seems dry and lifeless as presented – of course it is anything but, only the formalist methods of presentation just make it seem so IMO. I wish there were a modern Klein or Weyl running around. This seems to be a niche that someone should fill.

   I was told by a former Princeton math PhD that oral tradition is now essential in math, and that learning from textbooks without some personal mentor is very hard. In Klein's day, his "Vorlesungen" were taken directly from his personal style, so the presentation and the results were mixed in a way that made for exciting reading. We need something like that again IMO. Shlomo Sternberg comes close to filling the bill. Harris tries, but assumes too much of readers with my level of patience and skill.

5. **JC**
   September 16, 2004

   I always found the biggest barrier to understanding scheme theory and/or the more abstract formalism of algebraic geometry, was a solid understanding of commutative algebra. Years ago I worked through Atiyah's book on commutative algebra, which I thought was kind of dry and boring at the time.

   Awhile ago I went through Diers' book "categories of commutative algebras" which seems to make the subject more interesting and less dry, though I don't know how much more useful it is for understanding algebraic geometry.

6. **D R Lunsford**
   September 16, 2004

   There is a smaller book called "Algebraic Geometry – A First Course" by Harris alone that is good, and is accessible to physics types (i.e. me). At one time I had the preposterous idea of mastering scheme theory.

   -drl

7. **Levi**

September 16, 2004

JC–Some friends and I are currently reading “Basic Algebraic Geometry” by Shaferevich. It may not be what you are looking for, though, since it makes a concerted effort to be no more abstract than is absolutely necessary.

8. **sol**
   September 15, 2004

   First off, I appreciate the information in your article on Grothendieck. I am deeply interested in how different minds came to discover the foundational values of mathematics, albeit from the sidelines, of your perspectives and collegues, Peter.

   As a general reader of Smolin’s, Three Roads to Quantum Gravity I saw this same road taken, and assumed the essence of his distilliation, was topos theory.

   http://math.ucr.edu/home/baez/topos.html

   For more information I added this to my main page because of this set relationship I found being demonstrated in the developement of LQG perspectives in terms of quantum gravity.

   At a fundamental level of this association was quantified in terms of quantum grvaity, then I saw computerization deeply valued in relation to discriptors of that early uiverse.

   Like gamma ray detection, we saw the deeper fundamental realites emerge on the Windows of the universe. We might have to wait, to go deeper then this?

   I found it difficult to find some comparison on how such a view could have been exemplified, so by studing the Glast perspective I found a general relevance, that would have supported LQG.

   But did this go far enough? I see the limitations now, in relation to early universe perspective?

   The universe had to be smooth at its most earliest time?

9. **JC**
   September 15, 2004

   The “easiest” books on algebraic geometry I’ve come across over the years would be “ideals, varieties, and algorithms” by Cox, Little, & O’Shea, and perhaps the two undergradate books by Reid on algebraic geometry and commutative algebra. None of them seem to really go into the more abstract formalism of schemes, advocated by Grothendieck.

   I’ll take a look at Eisenbud & Harris’ book on the geometry of schemes.

10. September 15, 2004

    JC: Eisenbud, Harris: “The Geometry of Schemes” seems to be a more accessible introduction to the language, with examples and motivation behind the ideas. Relatively cheap, too…

11. **JC**
    September 14, 2004

    Peter,

    How common is it for physics folks to change into math? What’s the most popular area of math that physics folks like to change into?

    In recent years, the cases of physics folks changing into math that I’ve heard of were frequently some string theory folks who changed into some algebraic geometry areas related to Calabi-Yau manifolds (ie. the sort of stuff in the language of Griffiths and Harris’s “principles of algebraic geometry” book). I haven’t personally come across many physics folks who changed into the more abstract algebraic geometry stuff (ie. the more abstract Grothendieck style, such as in Hartshorne’s book). Though on the surface, I can understand why string folks would prefer the “Griffiths & Harris” language of algebraic geometry. Last time I tried to tackle Hartshorne’s book, I still found it a difficult read.

    Anybody know of an easier book than Hartshorne, which covers the more abstract Grothendieck style of algebraic geometry?

12. **Peter**
    September 14, 2004

    I just fixed that link, should work now.

13. **Cosma**
    September 14, 2004

    The link to the expository piece on topoi is empty.

CERN at 50

September 16, 2004
Categories: Uncategorized

This month is the 50th anniversary of the formal founding of the CERN laboratory near Geneva. There's a very interesting article in Physics World about CERN and its future plans. LHC construction seems to be proceeding more or less on schedule, although there has been a delay in beginning to install the magnets in the tunnel due to problems with the distribution line that will provide liquid helium to the magnets.

Jos Engelen, the chief scientific officer of the lab, is quoted as wanting to see any decision about building a linear collider wait until 2010 or so. The scientific reason for this is that it may take that long to for the LHC to produce results, and the sort of linear collider one wants to build may depend upon these, e.g. on the mass of the Higgs. CERN has its own linear collider technology called "CLIC" which it is working on. CLIC is quite different than the TESLA superconducting cavity technology developed at DESY and recently endorsed by the ITRP committee charged with evaluating which technology to go ahead with. CLIC uses a second electron beam to accelerate the main beam and in principle is capable of higher accelerating gradients than TESLA. Whereas a machine using TESLA technology would probably have an energy of 500 Gev, upgradeable to 1 Tev, CLIC might be able to reach 3-5 Tev. CERN is now increasing the resources devoted to the CLIC project, and clearly hopes that a delay in the decision about whether to build the linear collider would give them time to develop and prove the viability of CLIC.

Comments

1. **Chris W.**
   September 23, 2004

   The subject of this post is experimental particle physics, so I'll perversely take the opportunity to draw attention to two theoretical papers that I find very intriguing. I hope nobody is too annoyed; according to SPIRES-HEP neither paper has been cited by anyone but the author, who does not appear to have any academic affiliation:

   > Spin foams, causal links and geometry-induced interactions (hep-th/0403137)
   > ABSTRACT: Current theories of particle physics, including the standard model, are dominated by the paradigm that nature is basically translation invariant. Deviations from translation invariance are described by the action of forces. General relativity is based on a different paradigm: There is no translation invariance in general. Interaction is a consequence of the geometry of spacetime, formed by the presence of matter, rather than of forces. ….
   >
   > Relativity in binary systems as root of quantum mechanics and space-time (hep-th/0408116)
   > ABSTRACT: Inspired by Bohr's dictum that "physical phenomena are observed relative to different experimental setups", this article investigates the notion of relativity in Bohr's sense, starting from a set of binary elements. ….

Motl on String Field Theory

September 17, 2004
Categories: Uncategorized

Lubos Motl has an interesting post on sci.physics.stringsthat gives a detailed explanation of the current state of string field theory.

One way of motivating quantum field theory is to start with a "first-quantized" quantum theory of particles (perhaps defined by integrating over paths), then "second-quantize" by considering a quantum theory of fields, where the fields are defined on the space the points in the path move in. The natural generalization to string theory would be to start with the "first-quantized" theory of strings given by doing path integrals over the possible worldsheets traced out by the moving strings (these are conformal field theories), then "second quantize" by quantizing fields defined on the infinite dimensional space of loops. It has always been a hope of string theorists that this would somehow give a true non-perturbative definition of string theory.

Lubos explains what some of the problems with this idea are. For one thing it is in conflict with the M-theory philosophy that a non-perturbative theory should involve on the same footing not just strings, but also higher dimensional "branes". He goes on to speculate about what can be done about this problem, saying that perhaps one shouldn't be trying to find a fundamental set of degrees of freedom and an action functional of them. Instead maybe one just needs to find a set of self-consistent rules, which will be obeyed by all sorts of different degrees of freedom. As he notes at the end, this is similar to the old "Bootstrap Philosophy" of Chew and others that dominated thinking about the strong interactions during the 1960's. It didn't work then, and I'll bet it won't work now.

Comments

1. **Thomas Larsson**
   September 19, 2004

   June 14, of course. The spelling "Deseret" is due to APS News, though.

2. **Thomas Larsson**
   September 19, 2004

   Peter, here is a second-hand quote from the Aug/Sept issue of APS News:

   "I hope they're wrong, but I can't prove it. And I bet my life work on their being wrong."
   -Andrew Strominger, Harvard University, on skeptics who say that there's nothing to string theory, Deseret Morning News (Salt Lake City), June 41, 2004.

   It seems to me that your voice is being heard. But AS is perhaps referring to Glashow and Veltman.

3. **D R Lunsford**
   September 18, 2004

   Yawn.

4. **Ted Erler**
   September 17, 2004

   Most of what Lubos says I agree with.

   However, I think that the string community has been a little over critical of string field theory. Many theorists would even like to forget its existence, frequently saying that string theory has "no nonperturbative definition."

   As far as we know, Witten's cubic open bosonic string field theory gives a full nonperturbative definition of open+closed bosonic string theory. This is not to say that every concievable nonperturbative result in string theory has been reproduced in string field theory. It just means that we have no definitive argument that such nonperturbative information is not there, in principle.

   String field theory is a very complicated formalism and is far from unique. There are an infinite number of string field theories, each corresponding to a choice of conformal background and a particular decomposition of the moduli space of Riemann surfaces. There are open string field theories, closed string field theories, open+closed string field theories, superstring field theories… Presumably, each of these could provide a nonperturbative definition of string theory, but certain features which might be obvious in one string field theory (such as the perturbative spectrum around its conformal background) may be quite nontrivial in another (one would first have to construct a classical solution describing the background, and then study fluctuations about this solution). All of these formulations are presumably related by a complicated field redefinition, but our current understanding is primative.

   Faced with this situation, most string theorists hope that a more elegant, background independent formulation of string theory will at some point present itself. Personally, I have been inclined to take string field theory seriously, hoping that graudually a deeper understanding of its complicated but presumably profound struncture will emmerge.

5. **Tim M.**
   September 17, 2004

   Brian Greene is doing next week's physics colloquia on this topic. I'm curious to go, but not curious enough to miss a class that takes attendance 😉

6. **JC**
   September 17, 2004

   How many places has Chew's "bootstrap philosophy" worked to even a small degree? The only cases I can think of offhand would be some semi-contrived two dimensional models which appear to be exactly solvable. (ie. quantum inverse scattering sort of stuff). Other than that, I would be hard pressed to think of anything else which was not a failure.

String Geometry at Snowbird

September 19, 2004
Categories: Uncategorized

Thomas Larsson wrote in a comment mentioning a news story that appeared early this past summer in the Deseret Morning News (yes, that's Deseret, not Desert; this is a name Mormons use to refer to Utah). The news story is about a conference on "String Geometry" held at Snowbird, Utah in June. Evidently at Andy Strominger's talk at this conference someone actually mentioned that there were people who were skeptical about string theory and asked him to comment. His response was that "I hope they're wrong, but I can't prove it, and I bet my life work on their being wrong" , which I guess characterizes the attitude of many string theorists these days ("things don't look good, but I've got too much invested in this to give up, so I'll keep on engaging in wishful thinking even though I no longer have much of an argument for why I'm doing this").

Many of the talks at the conference are online. These include a couple of interesting talks by Gukov and Spradlin about recent work on twistor theory and perturbative Yang-Mills amplitudes, as well as the usual Michael Douglas talk with its wishful thinking that analyzing the astronomically large "landscape" will somehow lead to some sort of prediction of something. There's also a talk by Radu Tatar about non-Kahler superstring theory backgrounds. I've always wondered about this since I hear from an algebraic geometer colleague that although no one knows whether there are an infinite number of Calabi-Yaus in the Kahler case, if you relax the Kahler condition there definitely are an infinite number of them. If these non-Kahler backgrounds make sense, you can stop worrying about whether the landscape contains 10^100 or 10^500 possibilities.

Tomorrow here at Columbia my colleague Brian Greene is giving a colloquium on "The State of String Theory". His abstract says he'll "assess both its current shortcomings and major achievements".

Comments

1. **D R Lunsford**
   September 23, 2004

   Offend? I must up my flippancy daemon priority.

   But seriously, I like to know who's saying what.

2.
   September 23, 2004

   My apologies on not giving a name. My intent was not to offend. If it makes you feel better, I am not a regular poster being coy, but merely a casual reader who was posting a flippant comment. If I happen to post something of substance I'll be sure to give a name.

   But, since you asked, I'll just say that Georgi isn't a loon at all. Actually I loved the class I took with Georgi, and him as a person as well.

3. **D R Lunsford**
   September 21, 2004

   That is true, although I do have a nice letter from Georgi from my teenage days. He isn't (wasn't?) a loon. In fact I can't remember ever meeting a math/physics loon before the advent of this Internet thing.

   (Simple request – can we please use names here? There's nothing to be afraid of. I'd like to think/pretend this isn't the run-of-the-mill free- republic-type anony-blog.)

4.
   September 21, 2004

   "He's a loon with a professorship at Harvard.

   How'd that happen?"

   You've clearly not spent any time as a physics student at Harvard…

5. **Thomas Larsson**
   September 21, 2004

   Serenius, I have no doubt that most string theorists and LQG'ists alike are nice people. Motl may be an exception, but Gell-Mann does not give a particularly nice impression neither. This is not important. Besides, I think that Motl has a point about LQG, more politely expressed by Hellig-Policastro.

   Nobody doubts that most string theorists are nice and bright, only that they are right. To call Strominer a loon is certainly not fair.

6. **D R Lunsford**
   September 20, 2004

   He's a loon with a professorship at Harvard.

   How'd that happen?

7. **serenus zeitblom**
   September 20, 2004

   I don't think that Strominger's attitude is wrong. He has a hunch that string theory is right and he is following that up. He has infinitely more reason to be triumphal about [eg] black hole entropy than Lubos Motl or others of that ilk, yet I have never heard that he has tried to exaggerate the real but modest achievements of string theory in that direction. He's a good guy and a sensible one.

   Speaking of LM, this is from his proposed entry on loop gravity in wikipedia [the online encyclopaedia, http://en.wikipedia.org/wiki/Main_Page%5D:

   most loop quantum gravity advocates are not good physicists, and they try to avoid learning anything from particle physics and other fields even though it is clearly necessary for a proper understanding of many questions in quantum gravity. They believe that a very narrow-minded understanding of reality that they propose – and that has not made any real progress for decades – is everything we need. They are making incorrect mental links between different concepts and they are unable to learn better.

This was spiked by the wikipedia people on the grounds that it was “over the top”. LM has reached the stage where even non-physicists can tell that he’s a loon.

8. **Urs Schreiber**
September 20, 2004

It is due to phenomenology that N=1 susy to many people seems/seemed to be a very attractive property of an effective field theory of the fundamental forces.

The potential to shed light on the hierarchy problem as well as apparently better gauge unification properties are what made many people consider supersymmetric extensions of the standard model. And models like that drop out of string theory if six of ten dimensions are compactified on a CY manifold.

However, there is no known dynamical reason within string theory that six dimensions should spontaneously compactify as a CY space. This does not necessarily mean that such a dynamical mechanism does not exist, but these questions concerning the dynamical choice of string ‘vacuum’ are not at all well understood. For these reasons people just choose a CY compactification for phenomenological reasons and proceed from there.

This choice is compoletely analogous to (but somewhat more complex than) choosing a value for the parameter k in the Friedman-Robertson-Walker cosmological solutions of plain GR. For each value of k one obtains a valid solution, but one does not have (yet) a dynamical explanation why one value of k is observed in nature while the others are not. So one chooses the parameter that is preferred phenomenologically and then proceeds from there.

The math of supersymmetry as an abstract concept is extremely nice and supersymmetry concepts play an important role in many branches of physics and mathematics that are not directly related to high energy phenomenology. However, the supersymmetric extensions of the standard model and the scattering computations done in them are far from being very elegant. They have a plethora of free parameters and many new particles and interactions. Even though supersymmetry still ensures certain cancelations and makes some computations quicker, in general supersymmetry phenomenology is a *mess*.

9. **Dick Thompson**
September 19, 2004

Urs (or anybody), why is N=1 SUSY so compelling? Suely it can’t be because of phenolenology. Is it just that the math is graceful?

10. **JC**
September 19, 2004

Are there any backgrounds which break ALL the supersymmetry, without introducing any additional “diseases” to string theory?

11. **Urs Schreiber**
September 19, 2004

The condition that after the compactification of 1oD string theory down to four dimensions the remaining effective field theory has precisely N=1 supersymmetry is that the compactified 6 dimensions form a Calabi-Yau space, which is in particular Kähler. Relaxing that means not having N=1 susy in four dimensions (at high energy before 4d susy breaking).

In principle there is no compelling reason that the ten susystring dimensions must be compactified such that the resulting 4d theory has N=1 susy. This is just the scenario that people find/found most interesting. (Because susy in 4d is/was considered attractive and N=1 (as opposed to higher N) is the only choice not in contradiction with observation from the outset).

12. **JC**
September 19, 2004

Peter,

If you relax the Kahler condition, do you get an aleph_0 infinity of backgrounds or an aleph_1 infinity or beyond?

Physics wise what would be the dire consequences of relaxing the Kahler condition, besides definitely having an infinite number of backgrounds?

This Week's Predictions

September 20, 2004
Categories: Uncategorized

A new preprint by Michael Douglas indicates that, at least this week, the latest "predictions" from string theory are for:

1. No large extra dimensions.

2. No low scale supersymmetry.

So it looks like the "prediction" of the string theory "Landscape" will be that no physics related to string theory beyond that of the standard model will ever be observable. Thus the only "prediction" of string theory will be that you can never see any physics related to it. This kind of "prediction" is great since it proves string theory must be true. Either you don't ever see any effects of string theory in which case you have confirmed its predictions so it must be true, or you do see effects of string theory, in which case string theory is even more true.

Nothing really new at Brian's physics colloquium today. About 300 people showed up, which I think is probably a record for a physics colloquium. Brian doesn't like Susskind's "anthropic" arguments, which shows good sense. He still hopes that some new form of non-perturbative string theory will explain the standard model by picking out the right Calabi-Yau, but admits there's no known reason for this to happen.

Comments

1. **Lubo? Motl**
   September 29, 2004

   Good to hear that Brian remains a believer in the task to find the *right* string-theoretical description for the world around us.

2. **ksh95**
   September 27, 2004

   On September 25, 2004 03:56 PM Simplex said:
   …But if you have some articles by LQG people criticising string (besides Rovelli's Dialog) please let us know!…

   I don't know of any articles. I don't even read the archives regularly. I do however know several of the LQG principals and I've heard my share of "string theory is a horribly idea" tirades.

   Ironically, one of the most persecuted LQGists, Lee Smolin, is some one I've heard make comments similar to, "we have to take string theory seriously…".

3. **Thomas Larsson**
   September 27, 2004

   I am still not following your conclusions. For 2-d gravity string theory has the correct canonical quantization. That's what we talked about so far.

   I agree.

   When canonically quantizing gravity in 3+1 dimensions one finds that the commutator of the generators has divergences and hence cannot be well defined. LQG circumvents that by using a singular GNS representation where the generators themselves are not represented at all. By comparison with the 2d case one see that this step is not backed by ordinray QM. So the conclusion is that the LQG way to make canonical quantization of 3+1 D gravity work (work for the spatial diffeos that is -even with that trick the Hamiltonian constraint remains a problem) is using some very unusual notion of 'quantum'.

   One comment: the full Dirac algebra of ADM constraints is physically equivalent to the 4-diffeomorphism algebra. As emphasized by Rovelli, phase space is a covariant concept – the space of solutions to the classical field equations. We may label a solution by its positions and momenta at t = 0, but this is just one way to coordinatize phase space. However, some 4-diffeos do not preserve the standard coordinatization, so if we insist on keeping it, we must add compensating transformations, and we wind up with the Dirac algebra. But conceptually we are still dealing with 4-diffeomorphisms, and if we canonically quantize in the covariant phase space instead, that is our constaint algebra.

   So in conclusion we find that no way at all is known to apply canonical Dirac-like/inspired quantization to 3+1d gravity.

   Because there are additional anomalies which the standard formalism cannot handle.

   Although I don't know much about Liouville field theory, it seems to be some sort of archetype of an anomalously broken gauge symmetry. There is an extra mode, the Liouville mode, which becomes physical when D != 26. Analogously, we may expect that there are extra modes, which are passive in the classical theory, become physical when the diffeomorphism constraints are quantized.

   In fact, it is easy to describe these modes. To build representations of the diffeo algebra, we first of all need to expand all fields in a Taylor series around "the observer's trajectory" q(t), viz.

   f(x) = \sum_m f_m(t) (x-q(t))^m

   (We need some conditions on the Taylor functions f_m(t) to ensure that f(x) is independent of t). Classically, we can reformulate the field equations for f(x) as a hierarchy of equations for the Taylor coefficients – awkward but in principle straightforward. Upon quantization, the passive modes q(t) become physical – they have canonical momenta and are represented on the Hilbert space – in pretty much the same way as the Liouville mode.

   This is necessary in order to quantize a general-covariant theory in a way that represents the diffeomorphism constraints unitarily, because this is the way to build anomalous (and thus potentially unitary) representations of the diffeomorphism algebra. The need for such an expansion is clear since the anomaly is a functional of q(t). In string or field theory, where one never introduces this reference curve, the anomaly is invisible.

   In string theory the answer to this problem seems to be that the action for 3+1 d gravity is really embedded into a 'UV completion' whose path integral (namely the SFT path integral) can perturbatively be computed or which, for certain asymptotic backgrounds, can be computed non-perturbatively.

   If this means if and/or that the canonical Dirac constraints of gravity ever appear here is something that I don't see. You seem to think so but you haven't provided *evidence for it.*

One thing that can nicely be seen in the toy example of 1+1d gravity is that the Dirac prescription for quantizing constraints is just a first guess. The next best guess is Gupta-Bleuler quantization, which tells you that indeed not all states need to be annihilate by the constraints.

These may be regarded as different Virasoro representations. So from my point of view, the right way to quantize is to build unitary Virasoro representations.

But even that is based on ad-hoc assumptions. In the end it is the path integral that counts.

This is a bold statement – let's just ditch canonical quantization. It is nice that you make this assumption of string theory explicit.

Do you really believe that string field theory or M field theory will not have any canonical formulation?

I believe that before one can draw any conclusions from the results on diffeo algebras that you have in mind, one would have to understand if and in which way they are actually connected to a process called 'quantization'. Currently it seems that there is no such thing as Dirac constraints for gravity in more than 2-d.

I have previously claimed to have quantized general covariance, and associated quantum reps with any general-covariant theory. However, after having looked into the covariant phase space, I'm leaning towards the opinion that the construction in math-ph/0210023 is really an honest quantization of gravity, although the nomenclature in the paper is not quite right. Here what I do:

1. Start with some general-covariant theory, containing gravity and other fields.
2. Regularize the theory by expanding all fields in a Taylor series and truncate at some finite order p. This is the unique regularization compatible with diffeomorphisms.
3. Construct the space of functions over the covariant phase space, as cohomology spaces. The diffeo algebra acts on these functions spaces by Poisson brackets.
4. Replace Poisson brackets by commutators and represent the Heisenberg algebra on a Fock space. The diffeo algebra acquires an anomaly, so this must be quantization.
5. To remove the regulator, the anomalies must not diverge in the p -> infinity limit. Check and solve the conditions for this.

The cool thing is that these finiteness condition naturally require that spacetime has four dimensions.

There is a catch, though. I need twice as many variables as one would expect, so I really construct the ring of differential operators over phase space in point 3. But this is really the only way that my prescription differs from ordinary QM. In particular, since the regularized theories live in 1D, they don't need any renormalization beyond normal ordering.

4. **Urs Schreiber**
September 27, 2004

Hi Thomas –

I am still not following your conclusions. For 2-d gravity string theory has the correct canonical quantization. That's what we talked about so far.

When canonically quantizing gravity in 3+1 dimensions one finds that the commutator of the generators has divergences and hence cannot be well defined. LQG circumvents that by using a singular GNS representation where the generators themselves are not represented at all. By comparison with the 2d case one see that this step is not backed by ordinray QM. So the conclusion is that the LQG way to make canonical quantization of 3+1 D gravity work (work for the spatial diffeos that is -even with that trick the Hamiltonian constraint remains a problem) is using some very unusual notion of 'quantum'.

So in conclusion we find that no way at all is known to apply canonical Dirac-like/inspired quantization to 3+1d gravity.

In string theory the answer to this problem seems to be that the action for 3+1 d gravity is really embedded into a 'UV completion' whose path integral (namely the SFT path integral) can perturbatively be computed or which, for certain asymptotic backgrounds, can be computed non-perturbatively.

If this means if and/or that the canonical Dirac constraints of gravity ever appear here is something that I don't see. You seem to think so but you haven't provided evidence for it.

One thing that can nicely be seen in the toy example of 1+1d gravity is that the Dirac prescription for quantizing constraints is just a first guess. The next best guess is Gupta-Bleuler quantization, which tells you that indeed not *all* states need to be annihilate by the constraints. But even that is based on ad-hoc assumptions. In the end it is the path integral that counts. Evaluating the path-integral of 1+1d gravity correctly yields the BRST quantization, and this finally shows when and in which sense Dirac and Gupta-Bleuler apply. Since already in 2d the BRST quantization highlights a couple of subtleties that are missed with Dirac/Gupta-Bleuler, this teaches us to be careful with applying these methods to higher dimensions.

I believe that before one can draw any conclusions from the results on diffeo algebras that you have in mind, one would have to understand if and in which way they are actually connected to a process called 'quantization'. Currently it seems that there is no such thing as Dirac constraints for gravity in more than 2-d.

5. **Thomas Larsson**
September 27, 2004

It seems to me that Urs Schreiber, Helling, and Policastro have shown that LQG seems to have made a wrong turn, in making an unphysical construction. Can LQG be rescued?

I think not. However, the substance of my critique of string theory (apart from the overselling and lack of experimental support, which everybody sees) is essentially the Helling-Policastro argument transcribed to 4D:

H-P: Any unitary representation of the conformal algebra in 2D on a conventional Hilbert space is necessarily anomalous. LQG does not admit such anomalies. Hence LQG is wrong.

Me: Any unitary representation of the diffeomorphism algebra in 4D on a conventional Hilbert space is necessarily anomalous. String theory does not admit such anomalies. Hence string theory is wrong.

Urs Schreiber claims that this argument does not apply to string theory, because only the perturbative definition is understood. This only reflects our limited understanding of a complicated theory and is no problem. However, if string theory does not admit a formulation in a conventional Hilbert space with a unitary action of the diffeomorphism group even in principle, then it breaks with conventional quantum theory far more than LQG. And if it does admit such a hypothetical formulation, then the 4D Helling-Policastro argument applies to it.

6. **Arun**
September 25, 2004

It seems to me that Urs Schreiber, Helling, and Policastro have shown that LQG seems to have made a wrong turn, in making an unphysical construction. Can LQG be rescued?

7. **sol**
September 25, 2004

I added this because it was important to dig deep for the positions Smolin has assume and for the general public this might not be apparent, so I of course went looking.

Sol2

A case in point, is the understanding of Smolin's position.

I have followed his thinking as a example of the rigourous, and summations, although at a much generalized level. I do not think I should have been faulted on this as a crank(an overall generalization) of those who have not developed fully from the roads GR has taught us. Including those who wish to try and make a discription of the gravity waves fit some gravitonic expression?

Smolin tells us that General Relativity is not about adding to those structures or even about substituting those structures for possible new structures. Of course, we must understand what he is refering to here .

The basis of Smolins position rejects the idea, that space and time are fixed. He believes it evolves dynamically. He then subcribes, in my way of thinking, to the value of using this structure(it is contradictory to me as I stated up in previous post about photon intersections[Glast limitations] and the intersection of gravtonic considerations.

In the methods explained in terms of Glast's experimental standing we see where this can be taken further? Smolin calls it a set of relationships between events that take place.

To place such attempts at rediscribing the nature of the spacetime fabric disturbs him?

http://www.physicsforums.com/showpost.php?s=5c7da106e4775f720f0aefeedf9be54e&p=308810&postcount=8

Are we to deny some method to geometrical expression classically defined and not consider this at the quantum levels?

What is Quantum geometry if we cannot linearly describe the action that is taking place at the most suttles levels of existance?

I would appreciate any corrections in my thinking.

8. **Simplex**
September 25, 2004

ksh95 at September 25, 2004 01:02 PM says:
"In my opinion, both camps could benefit by worrying more about getting their own houses in order and paying less attentions to these petty squables."

Please point me to some criticisms of String by Loop Gravitists.
I see plenty of criticism of String but almost none from LQG researchers.
there was Carlo Rovelli's entertaining Dialog (between a junior researcher and a senior) but that was some months back and comparatively light reading.

Lee Smolin has joined in the widespread criticism of the Anthropic Principle but that is not String per se. Thomas Thiemann tried to bridge the gap inspired by a senior string theorist (Hermann Nicolai). I did not think his attempted bridge was at all hostile, though it did not get a very favorable response from string theorists.

What I hear in the way of "squabbling" is very one-sided. String folk like Lubos Motl giving many reasons why LQG cannot possibly be right.

I see little evidence to suggest that Loop people are wasting any effort on squabbling or on criticizing String. Their field is undergoing rapid change and growth, the production of papers has shot up in the past couple of years (although still very small by comparison with more established lines of research) and I do not imagine they have much extra time to devote to controversy.

But if you have some articles by LQG people criticising string (besides Rovelli's Dialog) please let us know!

9. **sol**
September 25, 2004

In all fairness, looking at how the history developed helps to shape the perspective in regards to the history of strings as it unfolds.

http://xxx.lanl.gov/PS_cache/hep-th/pdf/9909/9909016.pdf

For those inclined to developement a resource for continued reference to hold the higher road to edcuational value, I would draw your attention to the following post.

http://superstringtheory.org:8080/forum/edonline/discussion.jsp?thread=16

Regardless of the demeanor each discussion forum holds, this is an attempt to get minds to further embrace a place that would develope the conceptions beyond personal perspectives.

I defintiely need this spirit of cooperation to draw from.

Regards

10. **ksh95**
September 25, 2004

As a condensed matter theorist I have no horse in this race so I feel I can offer an non-expert but unbiased opinion. Frankly, it seems like this whole quantum gravty enterprise could be headed south. String Theory's short comings are will documented. This website being an excellent example. Loop Quantum Gravity is facing some serious criticisms. Examples can be found in Urs's post below or in http://www.arxiv.org/abs/hep-th/0409182.

In my opinion, both camps could benefit by worrying more about getting their own houses in order and paying less attentions to these petty squables. Funding sources have neither infinite resources or patience. I hear there have been some great advances in AIDS research these days. Similarly, nano sized complex molecules seem poised to usher in the next era of ultra small electronic devices. If you people aren't careful you'll find the technologically relevent areas of physics moving into the engineering building, while all the high end theorists adjust to their new offices next to the philosophers.

11. **Urs Schreiber**
September 23, 2004

Chris W. wrote:

> The string theorists who disagree should roll up their sleeves, grit their teeth, and demonstrate its insolubility. This would be an extremely interesting result. Unfortunately they apparently think it's so obviously true as to be not worth the effort

This is not quite true. There has been some input to LQG from string theory recently.

A while ago Edward Witten himself demonstrated that the Kodama state which Lee Smolin is so fond of is not normalizable and hence not really a physical state after all.

Then Hod and Dreyer and some other people became quite excited about a numerical coincidence between quasinormal mode spectra of black holes and BH entropy calculations in LQG. Motl and Neitzke published a paper (which is by now TopCited 50) showing that it is indeed just a numerical coincidence. Shortly afterwards it turned out that the entire entropy calculation in LQG was based on a wrong assumption.

H. Nicolai emphasized that LQG people should try to apply their methods to 1+1 dimensional gravity and see if any of the well known results could be reproduced. Shortly afterwards Thomas Thimemann did exactly that and found that the LQG method misses all the standard results. He used the GNS construction as well as Pohlmeyer invariants to do so.

I showed that the Pohlmeyer invariants that he used and could not quite quantize are quantizable in the standard non-LQG-like context. A while later it turned out that this result was already found in the 80s by Isaev, but apparently forgotten. A few days ago Helling and Policastro demonstrated how the GNS construction that Thiemann used leads to the ordinary non-LQG-like quantization when one uses a continuous GNS state instead of the highly singular one used by Thiemann, which is also used in full LQG.

There is a very simple argument that the standard LQG prescription to first solve the spatial diffeos and then impose the Hamiltonian constraint cannot work in principle in 1+1 dimensions. Thiemann circumvented this by including the Hamiltonian constraint in the GNS construction. It was clear that his singular construction has nothing to do with known physics, and indeed he admitted that it is motivated only by the speculation that such 'unusual quantum mechanics' might turn out to be correct at the Planck scale.

12. **Chris W.**
September 22, 2004

After enduring LM's sermon I perused some other recent sci.physics.strings threads and found one of the most lucid and informative discussions (by Urs Schreiber) of "background independence" in string theory that I've come across. Its relevance to the topic of this post is conveyed in the following remarks:

> String theory is nicely "background independent" in the first sense, even more so than ordinary GR, for instance. Not only is the metric a dynamical quantity, but even the number of (macroscopic) dimensions, the coupling constant, and to a large extend the entire field (particle) content of the theory is not a fixed ingredient of the Lagrangian but is dynamical.
>
> That's the very reason why one can even consider dynamics in the string theory "landscape". String theory is so immensely "background independent" in the first sense of the word that it is at present very hard to say anything about which values all these dynamical quantities it contains actually will obtain after some evolution. The landscape discussion is one of dealing with a theory which is highly background free, so that you first have to solve equations of motion to even be able to say something about the matter content of the theory. This is quite in contrast to other approaches to quantum gravity, which are often considered to be truly "background independent", where all the matter content, the coupling constants, the number of dimensions is fixed by hand.

I wonder if anyone in the string theory community would bother to articulate this if they weren't being pressed by the LQG'ers. By the way, I doubt that the present inability to identify a sensible classical limit in LQG will ultimately prove to be an insoluble problem; see hep-th/0404156. The string theorists who disagree should roll up their sleeves, grit their teeth, and demonstrate its insolubility. This would be an extremely interesting result. Unfortunately they apparently think it's so obviously true as to be not worth the effort.

13. **Arun**
September 22, 2004

This one by LM is another in the crusade to rid the world of LQG.

14. **raj**
September 22, 2004

Um, so, if there is no observable, how can the theory be–um–verified?

It strikes me that they are getting more and more into pseudo-physics

15. **peng**
September 21, 2004

blablabla
really good comments on problems that everyone sees.
however, not really constructive in any way.

16. **Peter**
September 21, 2004

Well, the "non-perturbative string theory will make everything work" line has been used for nearly twenty years, and has been a huge success for string theorists. I don't think they'll drop it anytime soon.

Witten's most important contributions to mathematics just use QFT, not string theory (Chern-Simons-Witten invariants of 3-manifolds, Seiberg-Witten invariants of 4-manifolds). Much of what people think of as string theory contributions to math are really 2d CFT, not string theory. Seems to me that if you're just doing CFT, you're doing 2d QFT, not string theory. You're only really doing string theory if you try and sum up contributions from different genus calculations. Some of the recent work in topological string theory does give things like Gromov-Witten invariants for all genera simultaneously, so seems to really be string theory.

17. **dolt**
September 21, 2004

I think the string theorists are entering very dangerous territory by the constant over-selling of what string theory can or cannot do. The hype over the supposed `Theory of Everything' is going to backfire and be detrimental to the lay public's perception of what physicists do.

I contrast the claims of string theorists with QED (anomalous moment of electron, etc), the 'standard model of strong and electro-weak interactions' (rho parameter test, etc). Pretty much everone thought (and knew) they were effective theories—hence the modest term 'model'. Considering what constitutes a model, much more is expected of a 'theory', let alone a 'theory of everything'.

Of course, the problem is the lack of dearth of unexplainable experimental data. So just say that strings/susy etc are one of the possible alternatives—nothing more.

Otherwise, string theory will be viewed as mathematical theology.

The only interesting results in string theory are those that are of interest to mathematicians. BTW, could anyone tell us how many of the new results of interests to mathematicians are of string theory in particular, not QFT? My understanding is that most (all?) of work of Witten of profound mathemtical importance are really based on applications of QFT. So Witten's work is of lasting importance in terms of his role in progress of mathematical physics, even if string theory does not work out; others, such as those based on 'string phenomenology' and the like, I am not so sure.

18. **JC**
September 21, 2004

Seems like "non-perturbative" is everybody's favorite buzzword and faint hope whenever they have run up against a brick wall and/or ran out of good ideas to work on in particle and/or string theory research. I wonder what they will think up of next, when the "non-perturbative" buzzword starts to wear thin and becomes too much like an excuse of "crying wolf". (That is without entering into the anthropic stupidity).

What the Bleep Do We Know?

September 27, 2004
Categories: Favorite Old Posts, Film Reviews

Last night I went to see a movie which was advertised as being about quantum physics, called "What the Bleep Do We Know?". I was expecting something pretty dumb, but am always interested to see what people think about quantum mechanics. The film surpassed all expectations; it was certainly the stupidest thing I can remember seeing in a movie theater, and that's saying quite a lot (I see a lot of movies…).

There was some sort of plot involving a woman photographer (played by Marlee Matlin), who wanders around and has anxiety attacks. Interspersed with the plot were interviews with various supposed scientists with something to say about quantum physics, consciousness, God, etc. On the whole they were a bunch of complete flakes, although one of them (David Albert) is a philosopher of science here at Columbia. Evidently Albert claims he was taken advantage of, that his interview was heavily edited to misrepresent his views.

The general idea was that since quantum mechanics supposedly says that there isn't one reality, but an infinite number of possibilities, one just has to be enlightened to an awareness of this, and then you can make whatever you want happen. Somehow the main character of the movie was learning these amazing facts about quantum physics, and this then helps her deal with her anxiety attacks, bad body image and sex addiction (the film really goes off the rails in a bizarre scene where she is the photographer at a wedding party that turns into a grotesque kind of orgy).

The film has a web-site, and there is a long article in Salon explaining that the whole thing is really the production of a cult based in the Pacific Northwest that believes that a woman named JZ Knight is able to channel a 35,000 year old mystic named Ramtha. She does play a large role in the movie and you can read all about her nonsense here.

The whole thing is really moronic beyond belief. One of the scientists interviewed is John Hagelin who, besides being part of the TM cult surrounding Maharishi Mahesh Yogi, presidential candidate of the Natural Law Party, and "Minister of Science and Technology of the Global Country of World Peace" is a rather prominent particle theorist. Prominent if you go by citations that is. His 73 papers are mostly about supersymmetric GUTs and considered quite respectable, with a total of over 5000 citations, including 641 citations for one of them alone.

Hagelin was a grad student at Harvard when I was an undergrad and I met him when we were in the same quantum field theory class. A roommate of mine was interested in TM and I think it was he who introduced us. I remember Hagelin wanting to discuss how quantum field theory could explain how TM'ers were able to levitate, something about how they did this by changing the position of the pole in the propagator. The fact that someone who spouts such utter nonsense can get a Ph.D. from Harvard and be one of the most widely cited authors on supersymmetric models is pretty remarkable.

Comments

1. **Fabio**
   October 6, 2004

   Of Hagelin's thousands of citations, how many are from the Ellis/Nanopoulous et al paper mill?

2. **diggingdeeper**
   October 4, 2004

   Hope you don't mind me butting in, just thought you might like to see these facts which are so far undisputed about the film.

   1. In addition to the films three directors, there were actors and others involved in the production who are long time "students" of Ramthas' School of enlightenment.

   2. A disproportionate amount of time was given in voice and film to Ramtha, Dr. Joe dispenza, and Miceal Ledwith.

   3. Dr Joe Dispenza and Miceal Ledwith are both long time students and "appointed teachers at Ramthas' school of enlightenment (RSE)

   4. Dr Joe Dispenza (the one who creates his day) has gone to court and testified that his teacher (ramtha) has told him that terrible times are coming and that he needs to protect his family. He also invested over $10,000.00 in an infamous scam that infected RSE and was touted by Ramtha as a vehicle to gain fabulous wealth and many of the schools membership lost substantial sums of money. Some lost their entire life savings.
   This is the person who teaches the brain science in RSE.

   5.Miceal Ledwith a clergyman with a rather dubious past (see http://unison.ie/irish_independent/stories.php3?ca=36&si=770458&issue_id=7565) is the one chosen by the film makers to be the theological spokesman. He is also the theologian in residence of RSE.
   He also has been marketing several products within the school and its followers. Guess that could not have been done to easily in the Catholic church.

   6. The following persons in the film have all spoken at RSE and sold books there.

   Fred Allen Wolf
   Dr Candice Pert
   Amit Gotswami
   John Haglin
   Joe Dispenza
   Miceal Ledwith
   and of course the big guy himself, Ramtha

   7. One of the scientists who was in the film and had never appeared at the school is Dr David Albert Professor and Director of Philosophical Physics at Columbia university.
   He has stated in several venues that his views were totally misrepresented in the film. He claims that in over 5 hours of interviews he explained to the film makers why their concept of how Quantum Physics works has virtually no support in the scientific community.
   He even called in to a radio program the director was on to discuss this and was cut off. The host of the show said this was done because it was "negative"
   so much for no good or bad, that is unless it is convienent.

   8. To date, there has been no response as to where the information which lead to the story about the indians not being able to see the ships of Columbus originated from. There appears to be no evidence to support this claim. In addition, the film mentioned "clipper ships" which were not even in existence at that time. Perhaps that is why they couldn't see them.

There were many more, but I will leave them for others. If anyone has any information to refute any of the facts laid out here, I will be more then willing to retract them.

They are relevant because of the deliberateness on the part of the film makers to keep certain facts unknown (ironically, it is I making the unknown know) and misrepresent others.

3.
September 30, 2004

I am glad that someone else thought that that movie was a bunch of garbage. I had it recommended to me by no less than three people. Everyone said that "you have to see this movie!" So I did. It was really hard to sit through. It was the biggest bunch of garbage that I had ever seen. Blech.

I guess the difference between me and those other people is that I actually have some grasp on the concepts that the movie was trying to talk about. Never mind that the movie didn't even have a plot.

4. **Peter**
September 29, 2004

Hi Lubos,

For many years in the early-mid-eighties, the Maharishi was pushing N=8 supergravity as the unified field theory, I remember a colorful poster explaining how it agreed exactly with his philosophy that many people posted on their walls. At some point I guess he updated it to string field theory and the version you provided a link for.

It's true that Hagelin stopped doing physics in the mid-nineties to concentrate on his other nonsense, but I can vouch for the fact that as early as 1978-9 he was heavily involved in TM and thought it had a lot to do with QFT. By 1984 he had moved to Maharishi University and started building up the physics department there. In the late eighties I remember seeing Maharishi University preprints, perhaps about flipped SU(5). The main weird thing about them was they were printed on pink paper instead of white.

5. **Lubo? Motl**
September 29, 2004

John Hagelin was obviously a good phenomenologists. Unfortunately he's written no papers after 1995, see

http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=find+a+hagelin&FORMAT=WWW&SEQUENCE=

Well, people are able to undergo various transformations.

I want to mention another point – Maharishi Mahesh Yogi. Right after the Velvet Revolution in 1989 (the collapse of the Czechoslovak communism), many new spiritual and other directions were trying to find their ways to the new free countries.

A group of 3 Indian people claiming to be direct disciples of Maharishi Mahesh-Yogi visited our high school. They were spreading their methods of meditation – but that was not the main thing that impressed me. They were showing us the pictures of the waves converging to a point – a meditation trick – and this picture had a caption explaining that "the unified field theory has already been found". I was really impressed – and although it sounded totally crazy, the "Einsteinian language" that their brochures were using had nearly convinced me that they really know something about fundamental physics. 😉

It evaporated in a couple of weeks…

The web makes all such things available today, so I can give you a Google link to a page about Maharishis' unified field theory,

http://www.worldpeaceendowment.org/invincibility/invincibility6.html

6. **Alejandro Rivero**
September 29, 2004

Sol. you refer to the offspring of hep-ph/9803315. It was an interesting idea aiming to solve the hierachy problem, as announced.

On the other hand, one finds 3+1, or more concretely inverse square law, to be mathematically peculiar when it refers to gravity, ie when mass is the source of the force. Check my single page unpublishable http://dftuz.unizar.es/~rivero/research/simple.pdf

7. **JC**
September 29, 2004

Peter,

It was amusing when John Hagelin tried to run for US president representing the "Natural Law" party during the 1990's. A lot of the propaganda from them was hilarious, especially whenever candidates attempted to hold public speeches and performing demonstrations of "yogic flying".

If I didn't know any better, I would have thought it was something straight out of a "Cheech and Chong" movie.

8. **sol**
September 28, 2004

Opinions on:

Short Range Tests of Newton's Inverse-Square Law

Gia Dvali

Nima Arkani-Hamed

Sava Dimopoulos

9. **Peter**
September 28, 2004

Hi Matt,

No, that wasn't intended specifically as an attack on supersymmetric models; the fact that Hagelin worked on them isn't an argument for or against them. And while I don't think supersymmetric GUTs are anywhere near as promising as many people seem to think, they are a much saner idea than many that dominate research these days (take the Landscape, please…).

But whenever one is dealing with highly speculative ideas that have no connection with experiment, there's a danger of becoming delusional and thinking that you're doing real science when you're not. People tend to believe that the fact that a certain kind of research is pursued by sizable numbers of people with very good credentials is enough to mean it must be good research. I think it's a good idea for people to consider the example of Hagelin: he's completely delusional and has zero common sense, but able to function at a high level in the particle theory community. One should take seriously the danger that he's not the only one deluding himself.

10. **sd**
September 28, 2004

Matt, what is so nutty about Serge Lang's ideas about HIV and AIDS? He points out that Gallo et al anounced that AIDS is caused by HIV at a press conference, without there being a single paper published in a scientific journal substantiating this. Isn't Lang's viewpoint in line with the sort of criticism that string theory receives at this web site?

11. **Matt**
September 28, 2004

Is that last line — "The fact that someone who spouts such utter nonsense can get a Ph.D. from Harvard and be one of the most widely cited authors on supersymmetric models is pretty remarkable" — some sort of strange attack on SUSY models? Isn't that a bit like attacking algebra because of Serge Lang's nutty ideas about HIV and AIDS?

Hagelin might be crazy, but some of those papers are co-written by John Ellis and other respectable people. I'm not personally familiar with any of Hagelin's work but I'm sure there's some good physics in there.

12. **sol**
September 28, 2004

It took a while, but the comparison finally came through on the association of strings, as a quantum mechanical perspective, and the relationship to that movie.

From my perspective, once you had identtified Smolin's position(I gave this in previous post[**Posted by sol at September 25, 2004 04:18 PM**]), then you would know he holds Einsteins, in relation to the Solvay meetings, and strings have modified what Bohr and Schrodinger were doing in developing QM.

Yet, the battle still ranges, and we now know where we can class the distinctions of LQG and String theorists?

13. **Alejandro Rivero**
September 28, 2004

"Extreme quantum mechanics" could be the name of the speciality of SF writer (and physicist) Greg Egan. I suggest to read him if you want to know how QM is perceived in the SF community.

14. **sol**
September 28, 2004

Peter,

It took a while, but the comparison finally came through on the association of strings, as a quantum mechanical perspective, and the relationship to that movie.

It's okay to be cynical:)

I mean listen, you have very reputable individuals who believe in validation, as experimental proof. Who believe, in all kinds of things(God maybe?). That would be, very hard to quantify:)

Yet, they just do.

15. **Cosma**
September 27, 2004

I've not seen the movie, and don't intend to, but David Albert's book on *Quantum Mechanics and Experience* is one of the best things I've read on the interpretation of QM. If he says he's being misrepresented, I quite believe him.

2004 Nobel Prizes in Physics

September 29, 2004
Categories: Favorite Old Posts, Uncategorized

No, they haven't announced the Nobel prizes yet this year. The announcement of the physics prize is scheduled for mid-day (Stockholm time) next Tuesday. I have zero inside information about who is likely to get the prize this year, but in particle theory there is one obvious choice: Gross, Wilczek and Politzer for asymptotic freedom.

The discovery of the asymptotic freedom of Yang-Mills theory led very quickly to the realization that QCD was the right theory of the strong interactions, and this was what really completed the Standard Model. It is one of the most important discoveries of 20th century science. The calculation of the Yang-Mills beta function was completed about the same time by David Politzer (a student of Sidney Coleman's at Harvard) and David Gross working with his student Frank Wilczek at Princeton. Gross was actually trying to complete a proof that all QFTs had bad ultraviolet behavior; he still was suffering from the pre-QCD prejudice that the strong interactions could never be understood via QFT, that one needed instead to do S-matrix theory or string theory or something other than QFT.

I've always been surprised that a Nobel hasn't yet been awarded for this discovery. The only reasons I can think of are political ones:

1. Evidently 't Hooft had done the beta function calculation earlier, but hadn't realized how significant it was or written it up. He certainly didn't work out the experimental implications for deep inelastic scattering, which was what Gross, Politzer and Wilczek did. Unlike 't Hooft, they immediately realized the significance of the result. So the Nobel committee might have felt it that it would be unfair not to make an award to 't Hooft. But 't Hooft did receive the prize a few years back for his work on renormalization of Yang-Mills theory, so this reason should no longer hold.

2. David Politzer was made a tenured professor at Caltech at a very early point in his career, but hasn't done much since then. Some people might not be so happy about awarding him the prize.

3. There certainly are some people in the particle physics community who weren't personally fans of David Gross. I remember many years ago a lunch with one European physicist who claimed to be involved in the Nobel decision process, at which he vividly claimed that "David Gross will get a Nobel prize over my dead body!". He's dead now, so at least he's no longer an obstruction.

Anyway, Gross-Politzer-Wilczek is my bet for next Tuesday.

Comments

1. **praveen kumar jha**
   October 13, 2004

   nobel

2. **Sanjay**
   October 6, 2004

   And you're bang on (the winners were announced yesterday). The reasons you cite – especially 1, and probably 2, are the right ones…

3. **D R Lunsford**
   October 5, 2004

   It's a lot more fun when school is over 🙂

4. **geoff**
   October 5, 2004

   Physics is fun!

5. **D R Lunsford**
   October 5, 2004

   Kinda like when Paul Newman got the Oscar (finally) for "The Color of Money". Well done Peter 🙂

6. **Thomas Larsson**
   October 5, 2004

   Congratulations, Peter, you were right!

   Press Release: The 2004 Nobel Prize in Physics
   5 October 2004

   The Royal Swedish Academy of Sciences has decided to award the Nobel Prize in Physics for 2004 "for the discovery of asymptotic freedom in the theory of the strong interaction" jointly to

   David J. Gross
   Kavli Institute for Theoretical Physics, University of California, Santa Barbara, USA,

   H. David Politzer
   California Institute of Technology (Caltech), Pasadena, USA, and

   Frank Wilczek
   Massachusetts Institute of Technology (MIT), Cambridge, USA.

   A 'colourful' discovery in the world of quarks
   What are the smallest building blocks in Nature? How do these particles build up everything we see around us? What forces act in Nature and how do they actually function?

This year's Nobel Prize in Physics deals with these fundamental questions, problems that occupied physicists throughout the 20th century and still challenge both theoreticians and experimentalists working at the major particle accelerators.

David Gross, David Politzer and Frank Wilczek have made an important theoretical discovery concerning the strong force, or the 'colour force' as it is also called. The strong force is the one that is dominant in the atomic nucleus, acting between the quarks inside the proton and the neutron. What this year's Laureates discovered was something that, at first sight, seemed completely contradictory. The interpretation of their mathematical result was that the closer the quarks are to each other, the weaker is the 'colour charge'. When the quarks are really close to each other, the force is so weak that they behave almost as free particles. This phenomenon is called ?asymptotic freedom?. The converse is true when the quarks move apart: the force becomes stronger when the distance increases. This property may be compared to a rubber band. The more the band is stretched, the stronger the force.

This discovery was expressed in 1973 in an elegant mathematical framework that led to a completely new theory, Quantum ChromoDynamics, QCD. This theory was an important contribution to the Standard Model, the theory that describes all physics connected with the electromagnetic force (which acts between charged particles), the weak force (which is important for the sun's energy production) and the strong force (which acts between quarks). With the aid of QCD physicists can at last explain why quarks only behave as free particles at extremely high energies. In the proton and the neutron they always occur in triplets.

Thanks to their discovery, David Gross, David Politzer and Frank Wilczek have brought physics one step closer to fulfilling a grand dream, to formulate a unified theory comprising gravity as well ? a theory for everything.

Read more about this year's prize
Information for the Public
Advanced Information (pdf)
Links and Further Reading

David J. Gross, born 1941 (aged 63) in Washington DC, USA (American citizen). Doctor's degree in physics in 1966 at the University of California, Berkeley. Professor at the Kavli Institute for Theoretical Physics at the University of California, Santa Barbara, USA.

H. David Politzer, (American citizen). Doctor's degree in physics in 1974 at Harvard University. Professor at the Department of Physics, California Institute of Technology (Caltech), Pasadena CA, USA.

Frank Wilczek, born 1951 (aged 53) in Queens, New York, USA (American citizen). Doctor's degree in physics in 1974 at Princeton University. Professor at the Department of Physics, Massachusetts Institute of Technology (MIT), Cambridge MA, USA.

Prize amount: SEK 10 million, will be shared equally among the Laureates.

7. **Dirac Beckenboll**
   October 4, 2004

   In my personal view,the Nobel Physics Prize for this year should be given to the scientists who work in the field of String Theory and the Yang Mill's Theory due to the significent effect by them to the theoritical Physics.But however ,I would also like to say that nowadays there are really no quite exiting news in those fields,then why don't we change our previous thought of the candidate and focus on some old famous scientists?
   I mean probably we may award ChenNing Yang and Mills for their proficient work in the Gauge Field Theory.Why not?

8. **atrel**
   October 2, 2004

   Lubos Motl said:

   "I just don't understand how can you give a Nobel prize for testing an accepted theory a little bit further. I think that Nobel prizes can only be given for new discoveries, and this example is precisely a textbook example of having no new discoveries."

   Understand or not…

   Quote from

   http://nobelprize.org/physics/laureates/1993/press.html

   "Here a new, revolutionary "space laboratory" has been obtained for testing Einstein's general theory of relativity and alternative theories of gravity. So far, Einstein's theory has passed the tests with flying colours. Of particular interest has been the possibility of verifying with great precision the theory's prediction that the system should lose energy by emitting gravitational waves in about the same way that a system of moving electrical charges emits electromagnetic waves."

   I think prizes *should* be given to people who find cool ways to test theories beyond the regimes that were reachable before, whether the theories pass the test or not. This is a way to encourage experimentalists to think hard about new ways to push the limits, whether some theorists think it is worth it or not (didn't Pauli think measuring g was useless?)

9. **sol**
   October 2, 2004

   Some are behind the times here and in this article 2002, what is realized in 2004? The Perimeter institue and Smolin are well on top of the issue from a Glast perspective?

   Test of the Quantenteleportation
   over long distances in the duct system of Vienna

   Working group
   Quantity of experiment and the Foundations OF Physics
   Professor Anton Zeilinger

   Quantum physics questions the classical physical conception of the world and also the everyday life understanding, which is based on our experiences, in principle. In addition, the experimental results lead to new future technologies, which a revolutionizing of communication and computer technologies, how we know them, promise.

   In order to exhaust this technical innovation potential, the project "Quantenteleportation was brought over long distances" in a co-operation between WKA and the working group by Professor Anton Zeilinger into being. In this experiment photons in the duct system "are teleportiert" of Vienna, i.e. transferred, the characteristics of a photon to another, removed far. First results are to be expected in the late summer 2002.

   http://www.quantum.at/Kanal/

10. **Lubos Motl**
    October 1, 2004

    Dear Michael,

    yes, I was referring to rather specific experiments of Zeilinger et al. – some of them studied entanglement at distances of order kilometers.

    Well, you may say, together with some other physicists, that quantum mechanics could fail at distances of order kilometers. We think that it cannot. Well, physics has the virtue of having experiments. The experiments have been done, and you have been proved wrong. 😉

    I hope that this unsuccessful prediction of yours should at least reduce your self-confidence in claiming that amazing violations of quantum mechanics could be observed at distances of order 100 kilometers!

    But more generally, I just don't understand how can you give a Nobel prize for testing an accepted theory a little bit further. I think that Nobel prizes can only be given for new discoveries, and this example is precisely a textbook example of having no new discoveries.

    I also absolutely disagree with your statement that there are principles (you even say "a lot of principles") of quantum mechanics that have not been tested. What do you exactly mean? Quantum mechanics was *created* through experiments. When quantum mechanics was developed, even the most obvious conceptual steps in the theory were only done once the experiments showed that it was necessary. The statements called "principles of quantum mechanics" have been experimentally tested before the end of the 1920s. I just don't know what exactly you want to test today.

    There is no known theory that could explain the observed experiments in a consistent framework but that would violate a principle of quantum mechanics – and we can more or less show that such a theory can't exist.

    Best
    Lubos

11. **Michael Nielsen**
    October 1, 2004

    "Come on, Michael, there was just no way how quantum mechanics could conceivably break down at distances of order one mile."

    I'm not sure what you're referring to, here. Why did you pick "one mile"? Are you referring to a specific experiment? Or is this a general statement, that quantum mechanics can't possibly be wrong on everyday scales?

    Certainly, I think Zeilinger's done a lot of great work checking quantum mechanics in various regimes where it has never been tested before. Macroscopic superpositions of semiclassical states and controlled entanglement have only very recently been observed, and Zeilinger's been at the forefront of that effort.

    If you're saying that there's no way quantum mechanics could fail at distances on typical macroscopic scales, well, I disagree, and so do many other physicists. There are, quite simply, a lot of basic principles in quantum mechanics that have never been checked, or have been inadequately checked.

    "The value of Zeilinger's experiments is purely pedagogical, everyone around agrees with that, "

    I'm not sure who "everyone around" is, but I suspect only a tiny fraction of physicists would agree. Certainly, people I know within AMO physics – one of the largest subcommunities of physics – generally seem to feel that Zeilinger's work is important.

12. **Lubos Motl**
    October 1, 2004

    Come on, Michael, there was just no way how quantum mechanics could conceivably break down at distances of order one mile.

    Your approach "Zeilinger did not prove that QM works the way it does, it is just consistent" explicitly shows that you would be willing to spend new money to make similar experiments where the separation is 10 miles, 100 miles, and so forth.

    This would be a complete waste of the money, and it is not really true that these experiments would be probing a new regime. QED certainly works at distances of order miles.

    The value of Zeilinger's experiments is purely pedagogical, everyone around agrees with that, and the Nobel prize committees is not a bunch of lame journalists that could be fooled and confuse a funny experiment with a new discovery.

13. **Fabien Besnard**
    October 1, 2004

    Dolt : Alain Connes has a web site where you can find his latest papers : http://www.alainconnes.org/
    John Baez speaks of some recent math papers from time to time on his web page.

    On the whole I think Peter is right about the 'math style' but this is rapidly changing, I think mainly because of the increasing pressure to publish impressive number of papers.

14. **Michael Nielsen**
    September 30, 2004

    Lubos Motl: "Well, the website also suggests that Anton Zeilinger could be a conceivable candidate. His experiments are cute even though their value is purely pedagogical – they proved that quantum mechanics works the way that everyone well-informed has already understood in the late 1920s."

    No, those experiments proved that the real world works in a way consistent with a theory that's been known since the 1920s. This is a huge difference, especially since many of those experiments checked that theory in a regime that's never before been probed experibmentally.

15. **dolt**
    September 30, 2004

    Peter,

    Thank you very much for explaining the difference in the way mathematics and physics is done. I had not appreciated the significant difference in style.

Well, I will have to rely on the various mathematical awards to get a sense of what the community thinks the important papers have been
in mathematics.

MathScinet, as you said, needs institutional subscription. Too bad the mathematicians do not maintain their own webpages and post their papers! Some do (perhaps the younger ones), but most don't.

16. **Lubos Motl**
September 30, 2004

Hey Sol,

could I ask you to try to express at least one your ideas in a different way? I did not understand the meaning of a single sentence in your text. Who are those who "look at quantum mechanics"? Which viewpoint is outdated? Which Feynman's toy model are you talking about? What is the difference between your "new era" and the "old era"? Why is it related to all the previous questions?

Which "refinements of refined GLAST perceptions" are you talking about? Why do you think that this offers an insight to GR if it would contradict GR? Why do you think that these questions are related to string theory, and what do you mean by the "right" string theory?

Thanks,
Lubos

17. **sol**
September 30, 2004

If you noticed, Lubos constanty refers to the outdated viewsto those who look at quantum mechanical discriptions, even though there is a nice trail has been set up in Feynmen's toy model.

I think he is nicely trying to nudge people out of the "ancient views," to a more modern acceptance of quantum mechanical posturing, so that maybe the "new era," could have accepted other possibilties.

Maybe refinements in Glast gamma ray perceptions to have refined it to a much more deeper connection with GR?

I'm only speculating of course as to the "right" string theory:)

Maybe a NObel prize in this?

18. **Peter**
September 30, 2004

The main reason there haven't been many Nobel prizes in relativity is that there aren't a lot of unexpected experimental results. Pretty much all the experimental results agree with the GR prediction. About the only real non-GR prediction about gravity is Hawking radiation, but Hawking is unlikely to get a Nobel for this unless a radiating black hole is discovered.

The 1993 Nobel to Hulse and Taylor for measuring the effects of gravitational radiation on binary pulsars is one example of a relativity Nobel. Another example would be the 1978 Nobel for observing the CMB given to Penzias and Wilson.

Actually, another good bet for the Nobel prize this year would probably be for a prize for the WMAP experiment.

19. **Peter**
September 30, 2004

The culture in mathematics about preprints and papers is somewhat different than in physics. You can think of physics as being more "journalistic", with the emphasis being on getting something out there about the latest, hottest topic, in a form that as many people as possible can read, without overly worrying about whether it is completely correct.

Mathematicians are much more obsessed with writing something that is precisely correct; they often feel that they are writing for the ages, not just for the current fashion. The numbers of people working on any given topic is quite different in math and particle theory. Most mathematicians are working on problems that only a handful of other people around the world can understand the significance of, whereas for the latest fashionable theoretical physics topic there are hundreds if not thousands of researchers interested in following what is being done.

So mathematicians tend to be less interested in getting their work out quickly and having it widely distributed on the arxiv. This is changing as more and more of them see the advantages of this, but there still is a sizable part of the literature that doesn't show up on the arxiv (unlike the situation in particle theory). There isn't another large parallel preprint system, although the "MathSciNet" on-line version of Mathematical Reviews run by the AMS has very complete information about what papers are out there. But often you do need access to a library, or an affiliation with an insitution that is paying for electronic access rights to get to see papers.

Trying to put together a list of the most important papers in a wide variety of fields would be valuable, but it's very hard. To do this well you really would need a very wide range of expertise and spend some serious time looking into different fields and talking to the experts. Someone should do this, but I don't think I'm up for it. And of course the people whose papers you put on the list would be mildly pleased, but think it their due, while those you didn't put on the list would often become lifetime enemies.

20. **dolt**
September 30, 2004

Peter,

Here is a suggestion:

How about posting a list of important papers (in your opinion, or the experts) in mathematics and physics (as many subfields!) every year, say in December? Since you are in the math department, you have a rare opportunity of
sharing the progress in mathematics with us physicists. Of course, your post on Perelman is an excellent example.

BTW, I seem to find that mathematicians have not embraced the arxiv the way particle physicists have. Is that true? To someone outside academia, it is hard to get access to mathematical journals.
Or is there an alternate site where the papers are posted?

dolt

21. **D R Lunsford**
September 30, 2004

If not what Peter said, then I would think someone in materials research, magnetic or solid state. The development of disk technology has changed the world. (Last year was superconductors.)

22. **Thomas Larsson**
September 30, 2004

I'd vote for Belavin-Polyakov-Zamolodchikov, for the application of conformal field theory to 2D critical phenomena. 2D statphys is admittedly not the most important part of physics; e.g., it is of less physical importance than 3D statphys. Nevertheless, this is the only major theoretical discovery after 1980 which is also real, experimentally confirmed physics.

I will not make any bets, though. I have expected that BPZ would win the Prize for more than ten years, but it never happens.

23. **serenus zeitblom**
September 30, 2004

Politzer "hasn't done much"?! Well, I think all of us here would be delighted to have done so little!
By the way, re his famous work, what is impressive is not just that he has a paper that has been cited over 2000 times — what is even more impressive is that he *wrote it by himself*. Nowadays, when hardly anyone seems to be able to think for himself, that is really impressive.

24. **Matt**
September 29, 2004

other candidates for a QCD-like Nobel prize are usually out of the game ('t Hooft and Gell-Mann have already received one) – perhaps except for Bjorken.

How about Nambu? (I would love to see Bjorken get the prize, though.)

25. **D R Lunsford**
September 29, 2004

Good point _______.

I'd like to call you something other than _______, but you are too paranoid to be a person in this place of persons.

(WHY are people so afraid to use their names?)

26. **Lubos Motl**
September 29, 2004

David Gross was only added to the main "stock market" on a German Nobel prize toy website 2 days ago, but he immediately sold $50,000 of his stocks and is normally traded right now.

http://www.nobelpreisboerse.de/stocks.aspx?stc=1

You can buy the stocks of Gross and Wilczek! I think that they are much cheaper than the real value. 😉 Gross, Wilczek, and probably Politzer is both my wish as well as my bet for Tuesday – but the probability that a particular bet works out is always smaller than 50%, I would say.

But it has already been 5 years from 't Hooft and Veltman; other candidates for a QCD-like Nobel prize are usually out of the game ('t Hooft and Gell-Mann have already received one) – perhaps except for Bjorken.

Well, the website also suggests that Anton Zeilinger could be a conceivable candidate. His experiments are cute even though their value is purely pedagogical – they proved that quantum mechanics works the way that everyone well-informed has already understood in the late 1920s.

If a member of the Nobel prize committee reads this posting, they should know that the question whether David Politzer is included or not is probably secondary, and should not change the decision that Gross and Wilczek should be awarded this time.

27. **Anon**
September 29, 2004

Any thoughts on why the entire field of Relativity
has been short shrifted by the Nobel committees –
from forever?

Is it true that Chandrasekhar received the only
Nobel for (an application of) Relativity?

Some History

September 30, 2004
Categories: Uncategorized

There's an interesting new preprint by the historian of mathematics Erhard Scholz about the early history of the use of representation theory in quantum mechanics. Immediately after the beginnings of quantum mechanics in 1925, several people started to realize that the representation theory of the symmetric and rotation groups was a very powerful tool for getting at some of the implications of quantum mechanics for atomic spectra. One of the main figures in this was Eugene Wigner, who was trained as a chemical engineer, but worked on this topic with his fellow Hungarian, the well-known mathematician von Neumann.

Equally important was the role of the mathematician Hermann Weyl, who in 1925 had just completed his main work on the representation theory of compact groups, perhaps the most important mathematical work in a very illustrious career. Weyl was in close communication both with the group at Gottingen (Heisenberg, Born, Jordan) who were developing matrix mechanics, as well as Schrodinger who was working on wave mechanics. Weyl and Schrodinger both were professors in Zurich and knew each other well (Schrodinger's first paper on quantum mechanics thanks Weyl for explaining to him some of the general properties of equations such as the Schrodinger equation). In 1927/8 Weyl gave a course on quantum mechanics and representation theory, which became the basis of his extremely influential book "The Theory of Groups and Quantum Mechanics", first published in 1928.

Scholz has also posted another preprint about Weyl's work, one that focuses on how his conception of the relation between matter and geometry evolved from 1915 to 1930. Weyl worked on general relativity and wrote an influential book about it (Space-Time-Matter, 1918). At that time he, Einstein and others believed that matter could somehow be described by a unified theory expressed in terms of some generalization of Riemannian geometry. Perhaps particles were some specific singularities or special solutions to the non-linear equations for the metric. The advent of quantum mechanics convinced Weyl (unlike Einstein), that this was a misguided notion, that matter should be described by a complex wave function. The right mathematics was not the geometry of a metric, but (in modern language) the geometry of gauge fields and of sections of a vector bundle with connection. The close connection between the basic ideas of representation theory and of quantum mechanics was quite clear to him, so, unlike Einstein, he enthusiastically adopted the new point of view of quantum physics.

One part of the close connection between Weyl and the history of quantum mechanics isn't mentioned by Scholz. Weyl was not only a close friend of Schrodinger's, he was Schrodinger's wife's lover. Schrodinger didn't believe much in monogamy; it's a well-known story that he discovered the Schrodinger equation while on holiday in the mountains with a girlfriend.

Comments

1. **Lubos Motl**
   October 3, 2004

   The page

   http://www.physics.umd.edu/robot/wigner.html

   contains a lot of other interesting stuff – history, pictures etc. – about Wigner as collected by Y.S. Kim, the famous guy with the robot who announces the conferences. 😉

2. **Chris Oakley**
   October 1, 2004

   would you care to share a recommendation?

   I would have to check. I certainly do not remember any 20 years ago, although Schiff chapter 7 goes some distance along this path.

3.
   October 1, 2004

   1. The theory of continuous groups and their unitary representations ought to be taught in undergraduate physics courses. It is not that hard and plays an absolutely crucial role in quantum mechanics. For example, the connection between angular momentum in quantum mechanics and the rotation group is more important than the connection with classical angular momentum and yet, in the course I was on, at least, the former was hardly mentioned.

   Chris,

   Are there any undergraduate level texts available? If so, would you care to share a recommendation?

4. **Lubos Motl**
   October 1, 2004

   Dear Thomas,

   I mostly agree with the statement about the evidence supporting QM and special relativity – although if I were describing these questions, I would probably separate these two things. (Also, I would use a less mathematical language.)

   We have a lot of evidence in favor of special relativity, including classical physics (which has no unitary representations), and a lot of evidence for quantum mechanics (in the nonrelativistic regime). Well, if we unify these two structures, we must introduce these unitary representations of the Poincare group, and this is what quantum field theory dictates. Well, we have a lot of evidence for quantum field theory, that goes beyond special relativity and quantum mechanics separately, too.

   I am not sure what (nontrivial) is meant by the unitary representations of the diffeomoprhism group in the second part of the text. If we require the representations to be unitary, it's because we want to get positive squared norms (positive – and conserved – probabilities) – which is a condition for *physical* states. However, physical states also need to be gauge invariant, and therefore they always form many copies of the trivial, singlet representation of the gauge group – at least of the part of the gauge group that is close to the identity (the part that is connected with the identity, and described by a normalizable wave).

   On the other hand, if you consider unphysical states in a formalism "before" you impose the physical constraints, there is no reason for the representations to be unitary. Indeed, the Hilbert spaces including the unphysical states are typically non-unitary representations of various algebras. For example, consider QED with all the time-like and longitudinal polarizations of a photon.

   All the best

Lubos

5. **Thomas Larsson**
October 1, 2004

2. The fact that "fundamental" particles/fields look a lot like the vectors of unitary irreducible representations of the Poincare group is an excellent endorsement for our theories of special relativity and quantum mechanics. Indeed, one seems to require little more than this representation theory to build up the whole of QFT.

This is indeed my understanding as well. This is my point in a post which I just sent to sps – let's see if LM accepts it. Anyway, I'm rather proud of the punchline:

At the most basic level, a quantum theory is defined by a Hilbert space and a unitary time evolution. If the theory has some symmetries, they must be realized as unitary operators acting on this Hilbert space as well. If time translation is included among the symmetries, which is the case for the Poincare algebra (and more subtly for diffeomorphisms), requiring a unitary representation of the symmetry algebra seems to be enough for consistency.

From this viewpoint, there is a 1-1 correspondence between general-covariant quantum theories (GCQT) and unitary representations of the diffeomorphism group on a conventional Hilbert space. Namely, if we have a GCQT, its Hilbert space carries a unitary rep of the diffeomorphism group. And if we have a unitary rep of the diffeomorphism group, the Hilbert space on which it acts can be interpreted as the Hilbert space of some GCQT. Since all unitary quantum irreps of the diffeomorphism group are anomalous, apart from the trivial one, all interesting GCQTs carry anomalous reps of the diffeomorphism group. So rather than being inconsistent, the second type of gauge anomaly is in fact a necessary condition for non-trivial consistency.

6. **Chris Oakley**
October 1, 2004

Two comments:

1. The theory of continuous groups and their unitary representations ought to be taught in undergraduate physics courses. It is not that hard and plays an absolutely crucial role in quantum mechanics. For example, the connection between angular momentum in quantum mechanics and the rotation group is more important than the connection with classical angular momentum and yet, in the course I was on, at least, the former was hardly mentioned.

2. The fact that "fundamental" particles/fields look a lot like the vectors of unitary irreducible representations of the Poincare group is an excellent endorsement for our theories of special relativity and quantum mechanics. Indeed, one seems to require little more than this representation theory to build up the whole of QFT.

7. **sol**
October 1, 2004

Thanks Peter,

This is the kind of history that is important for my understanding.

Unfortunately, speaking to Lubos from another article does not make it possible to speak to him about "ancient views" and new perspectives?

So history here, as you have shown and leading to today perspectives on gamma ray detection?

Photons and Smolins position, versus, strings. Photon and Graviton intersection? If spacetime is vibrating…….then…….

Just a thought.

Some of us are not so **lucky** to have the proper insights at the Time of our questions?:)

http://www-conf.slac.stanford.edu/ssi/2004/lec_notes/Quigg/default.htm

8. **D R Lunsford**
September 30, 2004

Weyl and Klein are my absolute heroes among the mathematicians.

I don't think it is quite right to say Weyl regarded "pure infinitesimal geometry" as "misguided". He was, I think astonished that it didn't work out as he originally thought – and it must have been on his mind all the time, because when the time came for the idea to reemerge in a new context (gauge theory), he was immediately on it. The idea behind PIG is after all incredibly simple – completely localizing the metric. How can it be that the simpler and more natural idea fails? I wish he were alive to see how his idea succeeds in the most remarkable way possible, by bringing matter into full equality with space and time, just as he wished.

When I was a student, my advisor said "Read Weyl. He writes for smart people." It was the best advice I ever got. Not only are the results intrinsically interesting, one has the feeling on reading Weyl that a great adventure is underway – that the author is not out to pound his vision into your consciousness, but to illuminate the world and its beauties. If only this spirit had become the consensus, instead of the self-indulgent formalism of Bourbaki.

Whatever

October 1, 2004
Categories: Uncategorized

Over at sci.physics.strings there's the scary sight of Lubos Motl agreeing with me in a posting about "Stringy Naturalness". Well, maybe he isn't directly saying he agrees with me, but "It would be too difficult for me to pretend that I disagree with these Woit's remarks" is pretty close. Lubos is criticizing the new sort of "naturalness" critierion advocated by Miichael Douglas in a preprint reviewing his recent work on the "Landscape". By this criterion a low energy effective QFT is more "natural" when there are more supposed string theory vacua that have this low energy limit. As Lubos points out, the danger with this criterion is that it tends to lead you to the conclusion that the most "natural" effective field theory is the one that is least likely to be able to predict anything new.

The posting immediately before Lubos's is from Michael Douglas himself, responding to an earlier thread. In it he explains the goal of his work as follows. He wants to estimate N_SM, the number of vacua consistent with the observed known Standard Model behavior, then

"Based on this information, we can decide whether we should continue the search for the right vacuum directly (appropriate if N_SM <= a few), look for additional principles to cut down the number (if N_SM is large), or give up and start making anthropic arguments or whatever (if N_SM is ridiculously large)." The posting immediately before Douglas's asks for "what would cause string theory to become nonviable and abandoned", but hasn't gotten any responses. An obvious response would be that if it becomes clear that string theory has so many consistent vacua that it can't ever predict anything, the theory would have to be abandoned. Neither Douglas nor others working on the Landscape seem willing to mention this possibility in public, the closest he gets is the line about having to "give up and start making anthropic arguments or whatever".

Comments

1. **Fabien Besnard**
   October 7, 2004

   >This behavior of a perturbation series isn't mysterious

   Well, at least it is to me. But I admit I haven't been studying this problem in details. I guess this has something to do with what Connes and Kreimer are doing.

2. **Peter**
   October 7, 2004

   Presumably the perturbation series for QED is divergent but asymptotic. This means that, while you can't get an exact result at finite coupling by summing the whole series, if you take the first few terms of the series you get something which approximates the exact result more and more accurately as you take the coupling to zero. This behavior of a perturbation series isn't mysterious. The exact result is not analytic at the point you are trying to expand about (zero), so the power series expansion doesn't converge. You see the same thing in simple QM examples with the quartic potential.

   In QED, for small coupling, the perturbation series gives quite accurate results for most everything you want to calculate. People doing string theory like to believe that perturbative string theory is a good asymptotic expansion for some quantitities (e.g. giving a graviton and reproducing GR at low energies), but not others (e.g. they would like to believe that it fails to describe the vacuum state). As far as I have ever been able to tell, this is purely a matter of wishful thinking and string theorists are promoting perturbative string theory when it gives them what they want, ignoring it when it gives them something they don't want.

3. **Fabien Besnard**
   October 7, 2004

   >More accurately, one can show that the terms in >the perturbation series for QED are finite and >well-defined at all loops

   But the series does not converge, does it ?
   It is still a wonder to me how the first few terms of a divergent series can give the right experimental results to 16 decimal places or something like that. Could it be that the perturbation series is not really self-consistent and would be made so only by including quantum gravity effects ?

4. **Peter**
   October 6, 2004

   More accurately, one can show that the terms in the perturbation series for QED are finite and well-defined at all loops, for superstring theory this has only been shown up to two loops, although people hope it is true for all loops.

   Non perturbatively, you can rigorously define QED with an ultraviolet cutoff (e.g. the lattice), but the evidence is that when you take the continuum limit, the renormalized charge goes to zero, and you end up with a non-interacting theory. This doesn't happen for QCD (related to why Gross-Politzer-Wilczek got a Nobel prize this week). QCD can be perfectly rigorously defined with a cutoff, and all evidence is that you get exactly the physics you expect when you remove the cutoff (although there is no rigorous proof of this).

   Non-perturbatively, no one has even a good conjecture about what the definition of non-perturbative superstring theory or M-theory is. It's a very different situation than QFT, not only can't you prove anything, you don't even know what it is you would like to prove.

5. **mitch p.**
   October 6, 2004

   It's my impression that perturbative QED and perturbative string theory are about equally well-formed mathematically – they both exist as formal series but there's no proof of convergence. But I'm not sure how the dualities, branes, "11-dimensional limit", etc., rate.

6. **Peter**
   October 6, 2004

   The definition of M-theory is not "a little vague", it is non-existent. "Something which is 11d supergravity in some limit" is not a definition, it is an expression of a hope that there is a definition. QFTs can be mathematically rigorously defined, although in interesting cases it is difficult to rigorously prove that they have all the properties one would hope for.

7. **mitch p.**
   October 6, 2004

What I think would be helpful is to have a taxonomy of field theories, broad enough to include M theory. Then we could see its place in relationship to the other possibilities. It appears to be a deformation or extension of 11-dimensional supergravity. Can we say what *sort* of deformation or extension? And do other theories of the same sort exist, but derived from simpler field theories? Do they feature objects considered to be uniquely M-theoretic, like D-branes or T-dualities? Can we say anything a-priori about the possibility of such objects appearing outside of the M-theory framework? Etc.

I take your point about M theory's definition (founded on the union of the moduli spaces of the string theories) still being a little vague. But it's not as if the existence of most ordinary QFTs can be demonstrated rigorously either.

8. **Peter**
October 6, 2004

This would be a lot more plausible if anyone actually knew what M-theory was. So far the existence of a theory anything like what you are describing is just wishful thinking.

9. **mitch p.**
October 6, 2004

I think of M theory as the "Monster Group" of field theories. Finite simple groups are famously classifiable into a number of infinite families, with 26 'sporadic' groups left over, of which the Monster is by far the biggest. The Monster has a little bit of everything in its makeup, so if you are trying to identify an unknown simple group and know just a few generating relations, there would be a reasonable chance that it looked like some part of the Monster. In the same way, M theory is a sort of field theory (under a generous definition that includes topological field theories, noncommutative field theories, etc.), and it features so many sorts of excitations that perhaps 'most' field theories resemble some M-theoretic vacuum.

So I'm sympathetic to the critical tone of this blog. I don't think we should *presume* that M theory is the final theory, when its bare empirical adequacy has not yet been demonstrated. However, if it does turn out that the landscape's bounty is so great as to render the theory almost unfalsifiable, I think it will still warrant much more respect than it gets here. It will depend on how many *other* (non-stringy) field theories are *also* capable of reproducing the Standard Model. If there are lots, then yes, M theory will not be so special. The capacity to include gravity might be decisive here (there's obviously an infinity of non-stringy field theories containing just the gauge forces). If LQG, the Visser/Sakharov 'emergent gravity' program, or some other approach to quantum gravity pans out, that will be a blow to string theory's proclaimed uniqueness. If the other approaches falter, that will strengthen the position of M theory. Even if there are a million distinct ways in which M theory might give us the world we see, it would then still be the favorite, no matter how unhappy we might be with that epistemic situation.

But this is all cart-before-the-horse. First let's do the job of seeing whether M theory can describe reality at all; if it can, let's find out all the ways that it can; and once we've done that, then let's have this discussion again.

10. **Thomas Larsson**
October 6, 2004

**Any* framework capable of reproducing the Standard Model (i.e. capable of predicting "anything we can observe"!), even one made of gremlins, warrants interest, simply because it might be the truth.*

Mitch, is string theory really capable of reproducing all aspects of the standard model? No unbroken low-energy supersymmetry, no new long-range forces (massless scalar particles associated with moduli), no new gauge bosons, an extremely long-lived proton? And a small and positive cosmological constant.

11. **sol**
October 5, 2004

Would you rather a soccer ball for describing the universe?

http://www.hep.upenn.edu/~max/wmap3.html

12. **D R Lunsford**
October 5, 2004

Chris W – wonderful post.

There is another argument (third?) against string theory that doesn't get any attention. I call it the "prima facie preposterous" argument. String theory is not only wrong, it's *wrong-headed*. The fact that it can't predict anything, that it can't reproduce the simplest physico-mathematical structures, that its most ardent practitioners are willing to resort to anti-scientific arguments and polemics, and that a sterile hermaphrodite is held up as a "theory of everything that will enable us to see God, and give him football betting tips", are all symptoms of having taken seriously a ridiculous idea. I can't think of ANY example in physics history in which a ridiculous idea was seriously considered. The orbs of Kepler actually led him to right answers, and the epicycles of Ptolemy represented a sane theory in its day, even an esthetic one.

13. **mitch p.**
October 5, 2004

Any framework capable of reproducing the Standard Model (i.e. capable of predicting "anything we can observe"!), even one made of gremlins, warrants interest, simply because it might be the truth.

The most recent overview of string phenomenology that I read (from late last year), says (page 7) that no-one has yet exhibited a string model manifestly capable of giving the right values for the fermion masses and CKM matrix elements. Researchers are apparently busier exploring the many paths to *qualitatively* reproducing the Standard Model (e.g. the gauge group). These debates about the phenomenological fecundity of the string landscape, and whether it invalidates string theory as science, may be a little premature.

14. **Chris W.**
October 5, 2004

To Mitch P: Peter is making a methodological and epistemological point, not an ontological one. He is *not* saying that "Since string theory can generate millions of models that resemble our observations, the world can't have a string-theoretic foundation".

He is saying that the theory in its current form appears to be effectively immunized against refutation, other than by observations that would also refute its predecessors (the Standard Model and general relativity). If someone makes new observations that are inconsistent with one string-theoretic model, then theoreticians can trot out another model accounting for the observations in question, which they might well claim to be equally consistent with the basic principles of the theory. Those principles are thereby insulated against any possibility of being contradicted by observations. The models take the fall instead, and no one worries much about it because there are so many of them to choose from.

That is, no one worries if they think the theoretician's objective is to be always ready with another model (derived from his favored *mathematical* framework) whenever previous models have trouble accounting for observations. This makes sheer mathematical richness a virtue, and indeed the mathematical richness of string theory is often proclaimed as an important source of its fascination.

However, physics and science generally are not simply exercises in applied mathematics, ie, in mathematical model building. In his early years Einstein was quite suspicious of mathematical formalism, and even in later years lamented having to respond to the ideas of people who in his words "can calculate but can't think". He was trying to get at fundamental principles, and it was vital to him that those principles be lucidly expressed and subject to empirical refutation as directly as possible. Ensuring that this is the case depends in part on a methodological commitment to avoid immunizing stratagems. This is tricky — one mustn't give up on a theoretical idea too easily — but it is arguably the most essential aspect of doing science.

To many people string theory continues to "smell" right. The question has become, "exactly how does it smell right; what are its central and distinguishing *physical* principles, and what are their unambiguously testable consequences?" In the 1980s it was often said by Witten and others that, unlike general relativity, string theory's central principles remained obscure, and a major goal of research in the field was to elucidate them. This goal seems to have dissipated, or to have degenerated into much aimless wandering in a vast mathematical wilderness.

Peter: You said, "The standard model QFT can predict anything that we can observe, using only of order 20 parameters." In the context of this discussion I feel compelled to amend this to the following: "The standard model QFT can account for anything that we *have* observed, using only of order 20 parameters." Presumably (one hopes!) there are observations we will make someday that the Standard Model cannot account for, over and above the values of the 20 parameters themselves, whose inexplicability motivates so much recent effort to go beyond the standard model. However, I think we can be rightly suspicious of the notion that there are a vast profusion of such observations yet to be made which require a far richer theoretical framework to understand. String theory appears to be just such a framework. I suspect that much of it will ultimately prove to be irrelevant to fundamental physics. I hope that in the midst of it (and in loop quantum gravity) are the outlines of a few key ideas that will prove essential and lasting.

15. **sol**
October 5, 2004

Gremlins

If we did not consider the alternate view of what the spacetime fabric could be, how would we ever consider step off points for consideration?

In quantum mechanics, the vacuum of space is not a vacuum; rather, it is field with virtual particles, such as the graviton. Light passing through this field of virtual particles is refracted, just as it is when passing through water or any medium.

The graviton, being the essence of gravitational force, would interact with (or slow down) those particles with greater gravitational potential. With mass directly proportional to energy, as expressed in e=mc2, photons of higher energy have greater gravitational potential than lower-energy photons — as if they "weigh" more.

The highest-energy photons would therefore travel through space more slowly than lower-energy photons. (This does not violate the constancy of the speed of light, for light travels at the same speed only in an absolute vacuum.) To detect the very slight difference in photon speed, one needs an extremely distant source emitting extremely high-energy photons: that is, the gamma ray burst.

http://www.astronomytoday.com/cosmology/quantumgrav.html

16. **Peter**
October 4, 2004

A "theory" that doesn't predict anything isn't a scientific theory, and if you spend your time studying a theoretical framework that is inherently incapable of predicting anything you're not doing science.

The standard model QFT can predict anything that we can observe, using only of order 20 parameters. Present day string theory predicts absolutely nothing at all, using a much more complicated theoretical framework than that of Standard model QFT. It's looking increasingly likely that this is because the string theory framework is inherently vacuous: to get the standard model out of it as a limit you have to assume the existence of such a wide range of vacuum states that you can't predict anything at all beyond the standard model (which is put in by assumption). All you have is a complicated way of parametrizing an infinite variety of possible extensions of the standard model. Unless you can figure out some way to use this to predict something, whatever you are doing, it isn't science.

What if I believe in gremlins and said that my theoretical framework is that everything is determined by the gremlins and that's all we can ever know. When you complained that my gremlin theory can never predict anything and never be checked, I could answer you:

"This doesn't make sense. There are a million ways that a gremin-theoretic world might look like the one we inhabit – therefore we don't live in a gremlin-theoretic world?"

Just describing the world in terms of fanciful abstract entities is not doing science. Finding a simple set of abstract entities that can predict new things we didn't already know about the world is.

17. **mitch p.**
October 4, 2004

"if it becomes clear that string theory has so many consistent vacua that it can't ever predict anything, the theory would have to be abandoned"

This doesn't make sense. There are a million ways that a string-theoretic world might look like the one we inhabit – therefore we don't live in a string-theoretic world? therefore we don't think about whether we live in a string-theoretic world?

A similar argument could have been used to rule out "quantum field theory" as illegitimate. "You can make a QFT do just about anything – what sort of scientific theory is that?"

18. **Fred**
October 3, 2004

Well, I don't think its entirely fair to criticize String theory quite yet. As with all new work in progress theories, there is still room for deeper principles that may eliminate all sorts of degenerate vacua. Brian Greene gave a lecture here a few weeks ago where he *optimistically* thought there was still room for different formalisms that could solve things in a more elegant fashion.

Michael is entirely in his right to publish papers exploring statistical values on the landscape. And how much fine tuning/philosophizing ultimately such a distribution would require. If you want to follow the math, then follow it wherever it may lead. It may end up actually falsifying what you set out to do.

More interesting to me, is that I have yet to see a convincing phenomenological model that actually reproduces a stable Standard model with even one single handpicked choice of vacua. That is, without the appearance of to many hard to believe artifacts. Say one that adequately satisfies bounds on proton decay lifetimes and with the desired suppression of flavor changing neutral currents. Jacques Distler had a blog about that some time ago.

I think the idea is if you can get close enough to SDM physics, than you can argue that there probably exists some similar, but different enough choice that does give the proper suppression. Then again, Distler said that even that was probably quite difficult (something to do with R-parity appearing only in very isolated parts of the moduli space)

19. **JC**
October 2, 2004

Imagine if string theory dies a painful death and gets "defunded" shortly after the LHC experiment goes online and rules out low energy supersymmetry. It would be hilarious if 70 years passes away, some future particle/gravity theorist stumbles across string theory by "chance" and resurrects it again as a viable quantum theory of gravity with no "living memory" of it's previous incarnations. (70 years into the future, all of the proponents of today's string theory would be either dead or retired with very little to no influence in physics research).

String theory then becomes like the Energizer Bunny which keeps on going and going and going … and refuses to completely die!

20. **Chris W.**
October 2, 2004

Or, to put it more starkly (and a bit sarcastically), string theory will become nonviable and deserving of abandonment when it becomes clear that its practitioners are unable to specify any other conceivable circumstances (logical or empirical) under which it would become nonviable. At that point it would be abandoned because, as a physical theory, it has become a bore, ie, it is no longer interesting as a basis for an empirical research program, and most young physicists are looking at alternatives (and are being encouraged to do so).

That situation has not arrived, but it has become worth worrying about for anyone who sees lasting value in the ideas and accomplishments of the field.

21. **dolt**
October 1, 2004

Actually, I am not that surprised. As you yourself pointed out some time ago, ignoring his rhetoric like "superstring theory is the language God wrote the world" and all that, he is actually very sensible.

Although I personally don't think string theory is ever going to explain particle physics, I do enjoy, and often learn from, Motl's observations, who I believe is a reincarnation of Pauli. Maybe just as Pauli was proven wrong on the utility of Yang-Mills theories (massless gauge bosons; confinement and SSB unknown to him), he may be wrong on LQG, despite some pretty good observations on the field.:)

My Life as a Quant

October 4, 2004
Categories: Uncategorized

Last week I was in a bookstore and ran across a new book by Emanuel Derman called My Life as a Quant: Reflections on Physics and Finance. Derman got a particle theory Ph. D. here at Columbia in 1973 when he was one of Norman Christ's first students. He then went on to post-docs at Penn, Oxford and Rockefeller and a tenure track job at Boulder. By 1980 he had decided he didn't want to stay in Boulder, partly because his wife couldn't get a job there, so he left academia for a job at Bell Labs.

In 1985 he went to work in the financial industry at Goldman Sachs, staying there until 2002, interrupted by a one-year stint at Salomon. He's now back at Columbia, teaching in the Financial Engineering program run by the IEOR (Industrial Engineering and Operations Research) department of the Engineering school. This kind of master's program is extremely popular; besides IEOR, the math and stat departments collaborate on a separate MA program in the Mathematics of Finance which has been wildly successful. Each year we get more and better applicants, and they seem to do very well on the job market when they get out.

The first half of Derman's book gives a good view of what it was like to be a theorist of the phenomenological variety during the seventies and early eighties. The second half has a nice description of the mathematical problems involved in pricing options and mortgage-backed securities, as well as many comments on what it is like to work in the financial industry. He was one of the earliest particle theorists to do this, but many others have followed him there in recent years, some of whom are regular commenters here.

Comments

1. **Chris Oakley**
   October 8, 2004

   Getting off topic, but wouldn't it be nice if in the presidential debates the candidates bragged about their ingenuity in avoiding draft to Vietnam rather than the heroic and selfless ways in which they served their country – ?

   "Every American can be proud of the way that, by getting a physics post-doctoral position at a young age I managed to avoid getting involved in that stupid civil war in Indo-China. It was not easy. It required resourcefulness and careful planning. My fellow Americans, by this I have demonstrated that I can look after my own interests effectively – have no doubts, therefore that you can trust me to look after the interests of the whole nation." [Applause]

2. **Peter**
   October 8, 2004

   Not very usual for someone to get a Ph.D. in one year. I think Christ was a student of T.D. Lee's and was also a Columbia undergrad. He may have just done much of the standard graduate program while an undergrad, then started grad school at a point when he was already doing research.

   Since this was the mid-sixties, there also may be a story about the draft there. My colleague John Morgan also got a very quick Ph.D. around that time, and I remember him telling me some story about this that involved avoiding being sent to Vietnam. Perhaps it was that, at least during certain years, grad students could be drafted, but if you were a postdoc, there was some way to get out of it.

3. **Tim M.**
   October 8, 2004

   Regarding Norman Christ: According to http://www.college.columbia.edu/bulletin/faculty/faculty.php he earned his doctorate one year after his bachelor's degree. Does anyone know about his early career? For example, how is this possible? Is this a phenomenal feat or commonplace? Maybe I should have posted this in the callow youth discussion.

4. **Thomas Larsson**
   October 5, 2004

   Gross, Politzer, Wilczek got the prize, we (which includes Peter Woit, me, and others) were guessing correctly this time. 😉 *More precisely, the committee chose correctly.*

   Old news, Lubos! I posted the press release after the appropriate blog entry an hour ago 🙂

5. **Lubos Motl**
   October 5, 2004

   Gross, Politzer, Wilczek got the prize, we (which includes Peter Woit, me, and others) were guessing correctly this time. 😉 More precisely, the committee chose correctly.

   Congratulations!

6. **sol**
   October 4, 2004

   I mean if you really think about it, you have to orientate this view around the very small, and one tends to think, "*is there some possible organization going on here*?

   Okay, maybe some of you don't, but I think quickly here of the math mind, and the ideas of negotiated processes, and again as quick, John Nash comes to mind.

   So I think this crossover is more then just being feed up with doing the search for the unifying principal in particle physics, and a transferance to real time functions, in bank processes?

   Imagine looking for this principle with Robert Laughlin.

   http://large.stanford.edu/rbl/lectures/index.htm

   So the one who controls the dollar, controls the reality? I think shifting sands/dollars in population, could have easily destroyed institutions, had minds set themself towards shifting those same dollars?

7. **Chris Oakley**
   October 4, 2004

   Oxford particle physicists I know who have ended up in finance:

   Guy Coughlan – at JP Morgan.
   Kelly Kirklin – formerly AIG, now back in physics
   Paul Miron – National Westminster (UK Bank)/UBS/Chase Manhattan. No idea what he's doing now.
   Me – Barclays/Citibank/UBS/HSBC/Nomura. Doing mainly financial software.

   Guy is a "risk metrics" guru at JPM. This involves, inter alia, treating the financial portfolio as a mathematical optimisation problem. A mixture of maths, programming and giving presentations to customers.

   Kelly and Paul were successful interest-rate swap traders. Mostly this was about trying to buy low and sell high and is more gamesmanship than mathematics, although they needed mathematics in the early days to set the trading book up (nowadays this is not a requirement as systems are better).

   I, on the other hand have never traded. This is not because I abhor the idea of becoming rich, it is just that since the age of 11 software has been one of my main interests in life. I have still not managed to build the perfect trading system, but at Nomura, at least, I gave it a serious try.

Gloating

October 5, 2004
Categories: Uncategorized

Well, my prediction of who would win this year's Nobel prize in physics turned out to be correct. No, I didn't have any inside information about this at all. I suppose I should mention that many of my friends will not be so impressed by this feat since they have heard me make this same prediction incorrectly every one of the last 15 years or so. Sooner or later I had to be right, and it was dumb luck that it happened the first year I had a public forum in which to make this prediction.

Congratulations to Gross, Politzer and Wilczek!

Comments

1. **Lubos Motl**
October 9, 2004

I made the same prediction, and it seems more correct to say that we *had* some inside information about it, to some extent – certainly much more information than the average people on the internet who were also trying to predict.

The physics Nobel committee is not that different statistically from the community of physicists like us and they certainly had similar discussions – comparing QCD with some lasers or quantum entanglement experiments.

The confirmation of a prediction after 15 years is a good example that string theory can also be proved correct 15 years from now. A physicist must be often patient! 😉

Congrats to Gross, Politzer, and Wilczek.

2. **Peter**
October 8, 2004

Hi Chris,

Actually I do have a link to the Friedan paper, it's on

http://www.math.columbia.edu/~woit/reason.html

I probably should update that page, any other suggestions for what should go on it are welcome. Maybe the "Landscape" deserves a page of its own.

3. **Chris W.**
October 8, 2004

Sorry; I neglected to identify myself in the previous post.

4.
October 8, 2004

As Thomas suggests, subsection 1.6 of Friedan's paper discusses the failures of string theory at considerable length. His assessment is devastating. Consider this:

> In particular, there is no justification for the claim that string theory explains or predicts gravity. String theory gives perturbative scattering amplitudes of gravitons. Gravitons have never been observed. Gravity in the real world is accurately described by general relativity, which is a classical field theory. There is no derivation of general relativity from string theory. General relativity can be regarded as the large distance classical limit of quantum general relativity, if an ultraviolet cutoff is imposed to make sense of quantum general relativity. A cutoff quantum general relativity would give the same formal perturbative low energy scattering amplitudes for massless gravitons as does string theory. But it is illogical to claim, from this formal coincidence between two technical methods of calculating unobserved graviton scattering amplitudes, that string theory explains classical general relativity, or that string theory explains gravity, or that string theory is a quantum theory of gravity. String theory does not produce any mechanical theory of gravity, much less a quantum mechanical theory.
>
> In any case, a quantum theory of gravity is unnecessary. No physical effects of quantum gravity have been observed, and there is no credible possibility of observing any. What is needed is a theory which produces general relativity as an effective classical field theory at large distance. It might produce classical general relativity by producing at large distance an effective quantized general relativity that is deep in its classical regime. But what is essential to produce is the classical, mechanical spacetime field theory of gravity. String theory is only a perturbative theory. The widespread practice is to assume that there exists a nonperturbative formulation of string theory, and that this hypothetical nonperturbative formulation would be a quantum mechanical theory, microscopic in spacetime, invariant under some exact, fundamental spacetime supersymmetries. If such a nonperturbative formulation of string theory did exist, then it might well follow that the large distance physics in that hypothetical theory would be governed by supersymmetric spacetime quantum field theory, and that the fate of the degeneracy of the manifold of background spacetimes would be determined by nonperturbative field theoretic effects at large distance in spacetime in that supersymmetric quantum field theory. But it is only an assumption that there exists such a nonperturbative, microscopic, quantum mechanical formulation of string theory. Any reasoning about a hypothetical nonperturbative version of string theory is unreliable if it rests on the assumption of spacetime quantum field theory at large distance, without any way to derive spacetime quantum field theory from string theory.
>
> The assumption of fundamental, exact, quantum mechanical spacetime supersymmetry is a very strong extrapolation from perturbative string theory, where spacetime supersymmetry is only a perturbative symmetry of the scattering amplitudes in individual background spacetimes. Adopting this assumption requires accepting as inevitable the continuous degeneracy of the manifold of background spacetimes. An assumption as strong as fundamental spacetime supersymmetry loses credibility as a guide in searching for a theory of physics if it cannot lead to definite explanations of existing knowledge and definite predictions. There is certainly no physical evidence to support the assumption that spacetime supersymmetry is a fundamental property of nature. At most, it is possible that indications of approximate spacetime supersymmetry might be found experimentally in the not so distant future. Contrast the radical assumptions of the old quantum theory, which obtained credibility by giving definite explanations of the black body spectrum, the photoelectric effect, the Balmer series, the Rydberg constant, and much more of atomic physics, before eventually leading to quantum mechanics.

(**Peter**: Is there a specific reason why you don't have a link to this preprint [April 2002] on your home page? It would good to have it there with whatever editorial comments you feel like including.)

5. **Thomas Larsson**

October 8, 2004

Peter will enjoy the part where he says that every new revolution in string theory taught us a lot about string theory and was followed first by great excitement, and then disappointment as it was realized that nothing new had been learned about the real world.

String theory has introduced much interesting mathematics into physics. That this has not led to any new physical knowledge must be a disappointment to many. I suppose that different people handle this in different ways: Susskind and Douglas invent anthropic selection ideas, Motl makes increasingly ridiculous claims about how string theory is the maximally predictive theory, Friedan (who founded the string theory group at Rutgers and thus in a sense is Motl's scientific grandfather) is silent for 10 years and then proclaims that string theory is "a complete scientific failure", see hep-th/0204131, subsection 1.6. Different personalities, different reactions.

6. **String Theorist**
October 8, 2004

David Gross is having a huge celebration at the KITP in honor of his Prize (well it is ostensibly in honor of the opening of the new wing of the KITP, though the fortuitous timing is quite coincidental, if you are the type who believes in coincidences…) .

Steven Weinberg gave a beautiful talk on the
current state of particle theory,
http://online.itp.ucsb.edu/online/kitp25/
and used an analogy that I posted long ago on this forum: that we may be no more succesful in "calculating the electron mass" from some fundamental theory than Kepler was in "calculating the radius of the Earth's orbit" from some fundamental theory.

Peter will enjoy the part where he says that every new revolution in string theory taught us a lot about string theory and was followed first by great excitement, and then disappointment as it was realized that nothing new had been learned about the real world.

7. **Arun**
October 8, 2004

What are the features of an "interesting" problem (as when one says – most of the interesting problems are in molecular biology?)

I think some of the features are that the problem is not easy, but is within the realm of feasibility of solution, is within the reach of conceivable experiments, and that solving the problem leads to other interesting problems.

8. **D R Lunsford**
October 7, 2004

No one claimed that biology is not interesting, but when I was 5 the physical world was more interesting, and now that I'm 45 it's still just as interesting. Why quit because the problems are hard and the people are harder?

9. **Chris Oakley**
October 7, 2004

It could be just that Molecular Biology is where the interesting problems are these days.

10. **D R Lunsford**
October 6, 2004

Hi Thomas,

One of my professors used to say "When Mozart was my age he was dead for 4 years!"

To me, the most amazing feat of callow youth is the "Enzyklopaedie der Mathematischen Wissenschaften" article written by Pauli on general and special relativity when he was 19 and a student of Sommerfeld. This book instantly became the chief reference on the literature of relativity. It's still a valuable book. This was 1920 – there couldn't have been 100 people in the world who understood it.

Speaking personally, all the young genii I knew in school have burned out or turned to biology or the like. Harsh field this.

11. **Fabio**
October 6, 2004

Another alternative is they could start awarding the prize posthumously. That way they could work their way back all the way through the 20th century.

12. **Peter**
October 6, 2004

Hi Thomas,

The student you're thinking of is John Moody. He was in the same entering class as me at Princeton and we spent a lot of time together there. I haven't seen him in quite a few years, but after Princeton, then Santa Barbara, he ended up going into CS (neural nets) and financial engineering. He now seems to be at Berkeley and has a web-page

http://www.icsi.berkeley.edu/~moody/

13. **Thomas Larsson**
October 6, 2004

It is striking how young these people were in 1973 – Wilczek 21, Politzer 23, and Gross 31. How many of you people did Nobel work at the age of 21?

I must have met Wilczek during the academic year 1982/83, which I spent as a graduate student at UC Santa Barbara. Wilczek had just been appointed professor there at the time. He must have been around 30 but appeared older, at least to a freshman like myself (he didn't have a whole lot of hair even then). Anyway, in the beginning of the winter quarter a grad student of Wilczek's by the name of Moody (his first name has completely slipped my mind) materialized. His job was to convince experimentalists to look for axions, which was Wilczek's pet idea at the time. The idea was, as this Moody fellow explicitly expressed it, that "they would find axions so that Frank could win a Nobel prize". Oh well, it wasn't necessary in the end, was it.

Axions are not the only hypothetical particles that Wilczek has cooked up. IMO the best so far is the vaderon, the carrier of the dark force. Named after Darth Vader.

14. **Peter**
October 5, 2004

Hi Danny,

Witten is responsible for new understanding of lots of different kinds of QFTs, problem is, not precisely the one QFT of the standard model. Off the top of my head, here are some examples:

Current algebra: the phenomenological model of pions. Witten solved the "eta-prime" problem, showing how the ninth Goldstone boson gets a sizable mass in this model. He also showed how the proton is a soliton in this model (the Skyrmion). This is very physical stuff, but not a fundamental model.

Seiberg-Witten solution of N=2 super Yang-Mills. Problem is that the Standard Model doesn't have N=2 supersymmetry. But this did revolutionize the field of 4-manifold topology in mathematics.

Cohomological topological field theories: Witten wrote down and solved a large class of QFTs whose observables are mathematically extremely interesting, although the models don't describe known physical particles. These include the 4d TQFT that gives Donaldson-theory, and the 2d QFT that gives Gromov-Witten invariants. The latter, which people now like to call "topological string theory" has lead to a lot of beautiful answers to enumerative problems in algebraic geometry.

Chern-Simons theory: This lead to great new math involving new knot invariants and connections to all sorts of other math. Physically he related a 3d QFT to conformal field theory (WZW models), giving new insights into both sides of the relation.

Other 2d QFTs: Many different examples, including pure Yang-Mills and the 2d version of Donaldson theory, which he used to understand the cohomology of certain moduli spaces. There is a huge amount of beautiful math here, and completely new ways of thinking about 2d QFTs, solving them exactly. Other examples include WZW models, the supersymmetric models leading to elliptic cohomology.

Personally I find it implausible that all these new ideas and ways of thinking about QFTs, some of which are quite close to the standard model, are not going to sooner or later give us new insight into the standard model itself. One problem is that almost all the community's effort has gone into trying to use these new ideas to do string theory, and that seems to be a dead end. This is speculative stuff and you're welcome to be skeptical until it proves its worth as physics, but if you believe as I do that great math and great physics are very deeply related, there is good reason to expect great physics to come out of this some day.

15. **D R Lunsford**
October 5, 2004

Hi Peter, I was watching the debate so let me ask you a debate question…

How can you "greatly deepen the understanding" of QFT without adding any significant knowledge about its actors, the fundamental particles? This is like saying "Yamaha greatly increased our understanding of internal combustion engines, without actually contributing any new performance enhancements".

Seriously, what exactly did Witten do that made QFT in 2004 better than QFT in 1984? Are we really better off than we were 20 years ago?

16. **Peter**
October 5, 2004

The fundamental problem is that there have been no real advances in particle theory since the Standard Model came together in 1973 (largely because of the discovery of Gross-Politzer-Wilczek). So, of necessity the Nobel committee has to go farther and farther back in time. In my original post I speculated about some reasons why this one took so long. It could just as well have been awarded 25 years ago in the late seventies.

Because of this, particle theory Nobel laureates are definitely getting older. You could also calculate the time interval between discovery and award of prize, and that has grown considerably over the years.

In another 25 years or so just about all of those involved in putting together the standard model will be gone (the grad students 't Hooft, Politzer, Wilczek are the youngest). If there is still no progress in particle theory, either there will be no more Nobels in the field, or the Nobel committee will have to change its standards and start awarding prizes for work that doesn't have experimentally testable consequences. If they have to do that, a good place to start would be with Witten, who has greatly deepened our understanding of QFT, although unfortunately mostly in ways that don't explain anything new about elementary particles.

17. **Fabio Lanzoni**
October 5, 2004

Why this year rather than any of the last, say 30? I can remember people speculating about Gross, Wilczek and Politzer winning the prize at least 15 years ago. It seems every year that goes by without a Nobel-worthy discovery in physics they have to reach further and further back in time. Has anyone accumulated statistics on the average age of a physics Nobel recipient vs. time?

18. **sol**
October 5, 2004

Must be Psychic?:)

19. **Dick Thompson**
October 5, 2004

Couldn't have been more deserved! Props to them!

KITP Conference on “The Future of Physics”

October 8, 2004
Categories: Uncategorized

The KITP in Santa Barbara is having a conference in honor of its 25th anniversary on the topic of “The Future of Physics”. Some of yesterday’s talks are already online. I’ve been watching Weinberg’s talk on “Where do we Stand?” this morning (a commenter also wrote in a little while ago while I was watching to recommend it). Weinberg gives a good summary of the present state of conventional wisdom about particle theory. He goes over the standard arguments that the standard model should be thought of as an effective low energy theory, and that doing so explains many of its features, with the two big exceptions of the scale of the vacuum energy and the electroweak symmetry breaking scale.

He promotes his “prediction” of the cosmological constant, and recalls that supersymmetry is the standard way of dealing with the low electroweak scale or hierarchy problem. But he then explains the problems with all known ways of breaking supersymmety, concluding that “no satisfactory theory of supersymmetry exists, where supersymmetry breaking is accounted for in the framework of particle physics.”

As for the “Landscape”, he notes that Gross hates it, says that “I don’t love it”, that it’s a disappointment, but one that we may have to get over. He makes some extensive comments about string theory, saying that it has had a history of advances leading to momentary optimism, but ultimately disappointment, with the bottom line that after 20 years we understand string theory much better but are no closer to contact with physics. He ends his comments about string theory with a rather weird remark that maybe it is wrong to look for a “guiding principle” behind string theory, that all there is to it is that it is the only way of extending the standard model to include gravity in 4d. I guess he is implying that string theory is not a fundamental beautiful theory, but, like S-matrix theory just a general framework imposed by consistency.

In general, Weinberg sounded to me old, tired and discouraged. Like just about all the leaders in the field, he refuses to publicly acknowledge the obvious possibility that the explanation for why string theory doesn’t predict anything or have any known fundamental principles is that it is just a wrong idea. He’s so discouraged about string theory that he has stopped working on it himself for the last fifteen years, but doesn’t have the energy or optimism to envisage any alternatives. He ends his talk with some real downers, one of which he calls the “LHC nightmare”, that the LHC will just see a single new scalar particle and nothing else. The second nightmare is that observations of the CMB will never see anything that tells us more about the early universe, just a 1/f spectrum, no evidence of the effects of gravitational waves.

All in all Weinberg ended up not giving a very optimistic view of the “Future of Physics”, but something closer to John Horgan’s argument about the “End of Physics”.

Tomorrow there will be a panel on “Field Theory and Mathematics” which should be interesting. Also, Witten will be talking on the “Future of String Theory”. It will be interesting to see if he is any more optimistic than Weinberg, and more specifically if he’ll come down on the Gross (“I hate it”) or Weinberg (“I don’t love it, but maybe it’s right”) side of the Landscape issue.

Comments

1. **JC**
   October 15, 2004

   In Mlodinow’s book “Feynman’s Rainbow”, in chapter 13 he described an encounter he had with Feynman when he tried to bring up the topic of string theory. It was kind of funny, with Feynman saying things like:

   “…. This whole discussion is pointless! It’s getting on my nerves! I told you – I don’t want to talk about string theory!”

   “My take? My take is that you hit a dry spell, and now you’re scrambling, trying to find something to work on!”

   “What’s wrong is coming to me to talk about string theory!”

   This encounter happened in late 1981, when hardly anybody really cared about string theory other than guys like John Schwarz and Michael Green.

2. **Chris Oakley**
   October 15, 2004

   Yes … sad, but true, even this desperate antidote of physical models that needed less than ten dimensions could not save the great man.

3. **Arun**
   October 15, 2004

   JC,

   OK, I do not know what Feynman did after he stopped coming to office; but his final office blackboard had stuff about the Baxter model and other 2-D exactly solvable models.

4. **JC**
   October 13, 2004

   Arun,

   There was a book “Feynman’s Rainbow” by Leonard Mlodinow which mentioned the same folklore in the last chapter on p. 166 (in the hardcover version)

   ” … He was now weak, in pain, and often depressed. But physics still brought him vigor. He continued to teach a course on quantum chromodynamics. And, in his last months of life, he finally decided to learn string theory. Murray taught him, in a private ‘seminar’ they held each week.”

   I don’t personally know Mlodinow nor do I know any of his “sources” for this particular piece of folklore about Feynman finally giving in and learning string theory. By the time I heard about this particular piece of folklore, it may very well have been a 2nd or 3rd hand account that took on a “life of it’s own”. Without mentioning any particular names, my sources were former colleagues who were in the particle theory group at Caltech around the time of Feynman’s death. At the time, I just took their word for it.

5. **Arun**
   October 13, 2004

"From some folklore stories I vaguely recall, allegedly Richard Feynman finally gave in and decided to learn string theory in the last few months of his life in 1987-1988."

As far as I know, that is not true. You might ask Sandeep Trivedi about it.

6. **D R Lunsford**
   October 12, 2004

   Lubos intoned:

   I apologize, but your possibilities 1,2 are just plain stupidities especially if you think that one of them is likely. Both of them are silly. What you're saying that contradicts the results of 20,000 papers or so. We know that a theory that unifies these things exists, and it is most likely unique because its consequences for any question that we have been able to answer were unique.

   Obviously, arguing by paper volume, "reductio ad cellulosum", is the stupid thing. There were no doubt 20k papers by the LMs of Lorentz' day stating why the deformable electron "was posited by God to make the ether perfect". And it's not at all obvious that GR and QM are "unified" because their individual structures are utterly different (at the moment). You must find an encompassing structure to make such a statement, and neither you nor any other stringers, have.

   *There was never any non-dynamical freedom in string/M-theory found ever. If something can be adjusted about your theory, you can always view it as a choice of the environment, and in principle, you can create another environment *physically* within the Universe with the first environment.*

   What?? Was there a point in this?

   The "mystery" of M-theory in 11 dimensions

   Yada yada yada,
   We all know this mantra.

   So we distinguish the "big M-theory" that contains all insights that string theorists ever studied, from "narrow M-theory" which is the UV completion of 11-dimensional supergravity. The latter is more or less understood and defined. The former is well-defined by hundreds of "patches" which beautifully fit together – the "transition functions" are the dualities – but we would prefer to describe M-theory "without patches", as a single whole – to reveal the rules that illuminate immediately why all these patches belong to the structure. This is what the question "What is string theory?" means.

   So! You should call it "U-theory" for "unworldliness":

   U = (F – ma) + (Rmn + 8piTmn) + (2+2-4) + … = 0

   There it lies.

   I think that you are completely wrong if you say that you are "not spending of huge amount of your time" with string theory. You ARE spending a huge amount of time, even more than me, by these philosophical worthless speculations and attacks. You are just not spending this time efficiently. If you spent the same amount of time with learning string theory, you would have known it better than me.

   And thank God for it.

7. **Chris Oakley**
   October 12, 2004

   No it wasn't … String theory is what killed him.

8. **D R Lunsford**
   October 11, 2004

   And I thought it was cancer. Live and learn!

9. **JC**
   October 11, 2004

   From some folklore stories I vaguely recall, allegedly Richard Feynman finally gave in and decided to learn string theory in the last few months of his life in 1987-1988.

10. **JC**
    October 11, 2004

    For people who don't believe in string theory, they would see it as hype and propaganda. For somebody who does believe in string theory, they would not see it as hype and propaganda. It's a matter of a difference of opinion for different people.

    As long as there isn't a death penalty for holding a particular opinion on some topic of interest, there's always going to be a spectrum of different opinions on a particular topic.

    There's some folks today who don't even believe in renormalization. Even more extreme are some folks who today still don't believe in quantum mechanics, for whatever strange reasons they have.

    Though I do agree with Lubos and others that a dearth of impressive new results (to spark a new string revolution) and the anthropic stuff in today's string theory, has curtailed the optimism of some people in the field in recent years. The anthropic stuff has certainly curtailed my optimism about string theory these days. I certainly would want to see some impressive new results to be optimistic about string theory again, like how optimistic I felt about the field during the mid-late 1980's and mid-late 1990's. There has to be a better way of doing things without invoking the anthropic principle.

    I'm sure John Schwarz must have had a lot of faith and optimism about string theory during the latter 1970's and early 1980's when only a handful of people were working actively on string theory, as well as dealing with Feynman's sarcasm and jokes directed towards him. I don't know if I could have maintained an optimistic spirit about string theory if I was in Schwarz's shoes during those days.

11. **Lubo? Motl**
    October 11, 2004

There is no hype and propaganda about string theory. People just try to explain why it is a fascinating and unique theory. In the middle 1980s or middle 1990s, people were more excited and the progress was faster, and therefore the comments about string theory were more optimistic. Today it's closer to the opposite. As Witten pointed out on the KITP conference, even string theory *enthusiasts* underestimate how rich and powerful string theory is – so speaking about hype and propaganda is not a reasonable description of reality.

12. **JC**
October 10, 2004

I get the sense most of the criticisms of string theory on this weblog is largely about the hype and propaganda surrounding string theory over the last 20 years. The discussions about specific technical details about string theory and field theory seem to be more neutral and less controversial.

It seems like advocacy of any particular strong viewpoint is always going to be a hotly debated, regardless of what the subject is. One just has to see the sort of heated discussions on the *.advocacy newsgroups in the comp.* hierarchy, or the sort of stuff that goes on in debating clubs and societies on any university campus.

13. **Lubo? Motl**
October 10, 2004

"The real LHC nightmare will be if it sees charged particles at 70 GeV and a scalar at 115 GeV."

Well, as far as this sentence goes, it could be a confirmation of the LEP's 115 GeV Higgs, plus – for example – selectrons at 70 GeV that were missed by LEP II, because of some reasons mysterious to me. That would be a cool victory rather than a nightmare. 😉

14. **Alejandro Rivero**
October 10, 2004

The real LHC nightmare will be if it sees charged particles at 70 GeV and a scalar at 115 GeV. THis is two doublets higgs but not SUSY, and besides it should raise doubts about the statistical methodololgy of LEP2

15. **Lubos Motl**
October 10, 2004

Hey Peter!

Not just Gross. I am also saying that the question "What is string theory?" is the most important one.

But you severely misunderstand this question. This question is not meant to say that "string theory probably does not exist" or "the theory is probably ambiguous" or even "string theory is probably just a dream and hope".

On the contrary. We already know a lot of very well-defined things about "something" and all these things fit together beautifully and exhibit a lot of quantitative agreements, miracles, uniqueness. But we still do not really know "why" all these things work so well, but we know pretty certainly that an answer exists – a scheme that organizes all the insights about string/M-theory that we have already gathered in a very coherent way.

I apologize, but your possibilities 1,2 are just plain stupidities especially if you think that one of them is likely. Both of them are silly. What you're saying that contradicts the results of 20,000 papers or so. We know that a theory that unifies these things exists, and it is most likely unique because its consequences for any question that we have been able to answer were unique. And it is a theory whose qualitative features reproduce everything we know in the real world.

There was never any non-dynamical freedom in string/M-theory found ever. If something can be adjusted about your theory, you can always view it as a choice of the environment, and in principle, you can create another environment *physically* within the Universe with the first environment.

There is some confusion about the meaning of the phrase "M-theory". Before M-theory in 11D was understood and defined (e.g. by the matrix model), people were dreaming about it. It was the only vacuum with more than 10 dimensions and it was reasonable to think that this is the real "mother" of all theories, and understanding physics in 11 dimensions exactly is enough to understand any other background in string/M-theory.

This is not how we view it today. The "mystery" of M-theory in 11 dimensions diminished significantly, and we view this 11-dimensional vacuum on equal footing with other, stringy vacua – e.g. heterotic strings in 10 dimensions. It's just *another* limit of the big "theory of everything", and it differs from most others because it has no strings and corresponding stringy perturbative expansion.

So we distinguish the "big M-theory" that contains all insights that string theorists ever studied, from "narrow M-theory" which is the UV completion of 11-dimensional supergravity. The latter is more or less understood and defined. The former is well-defined by hundreds of "patches" which beautifully fit together – the "transition functions" are the dualities – but we would prefer to describe M-theory "without patches", as a single whole – to reveal the rules that illuminate immediately why all these patches belong to the structure. This is what the question "What is string theory?" means.

I think that you are completely wrong if you say that you are "not spending of huge amount of your time" with string theory. You ARE spending a huge amount of time, even more than me, by these philosophical worthless speculations and attacks. You are just not spending this time efficiently. If you spent the same amount of time with learning string theory, you would have known it better than me.

You are free to only learn string/M-theory once someone computes the exact Universe around us by string theory, but be sure that it will be already too late for others (and you) to become significant contributors to the subject. Sure, if you're only want to be an (average) teacher, it is enough for you to only learn the things that have already been proved and awarded by the Nobel prize – and you can even end up with classical mechanics. But it's not enough to become a physicist that contributed to the human knowledge. The previous sentence applies not only to string theory, but to everything. It just happens that string/M-theory is the most (and perhaps, the only) reasonable path to progress in theoretical physics.

So I understand that your statement "string/M-theory is not well-defined" was just meant as an unphysical empty emotional rhetorical statement that should not be controversial because it was meant to express no idea whatsoever. In that case, I find this statement totally OK, but I am not interested in it. Any interpretation meant to make your statement meaningful leads to the conclusion that it is bullshit, but of course if you don't want me to study your statement seriously, I can just view it as a set of words that respect the English grammar, and everything's fine.

Best
Lubos

16. **Peter**
October 10, 2004

Hi Lubos,

The point I was making shouldn't be a controversial one. Listen to Gross's talk at the KITP conference. He lists one of the big unknown questions to be answered in the future as "What is String Theory?" and describes the current situation as something like "we have discovered a mathematical structure that has a life of its own, but we don't know what it is".

You know very well that when people say "M-theory" you have to figure out from context exactly what they are referring to. Often they are just doing 11-d supergravity, sometimes they mean a specific proposal for what M-theory is in a specific background, like the one you mention. The problem with the version of M-theory you refer to is that it doesn't work in a physically realistic background with 4 large and 7 small dimensions. Maybe there is such a version of M-theory, but for now its existence is just a hypothesis. I have no idea whether it exists, but from everything I've seen the two most likely possibilities are:

1. No such theory exists.

2. Such a theory does exist, but there are an infinite number of them, one for an infinite number of possible backgrounds, and such theories may be able to parametrize an infinite number of possible extensions of the standard model, but will have no predictive value.

Maybe I'm wrong, but it's perfectly rational for me not to spend a huge amount of my time becoming expert on the details of such theories until you or someone else comes up with a version of M-theory that can reproduce the real world and has some predictive value.

17. **Lubos Motl**
October 10, 2004

Peter, what you say – that there is no theory – is just a piece of bullshit, and the fact that you like to fool yourself with this bullshit only proves your laziness. That's it. There is nothing deep about it.

Take the BFSS Matrix model – I am just trying to calculate the exact black hole entropy including 1/4 out of it, for neutral black holes, and solve the model using a matrix counterpart of the state-operator correspondence. It is a super (and supersymmetric) well-defined QM model, whose basic variables are nine hermitean matrices X^i, their canonical momenta Pi^i with the obvious commutators, and 16 hermitean fermionic matrices theta^alpha transforming as the spinor, and the Hamiltonian is

H = P^- = Tr (Pi^2 – [X_i,X_j]^2 – theta.gamma^i.[X_i,theta])

Is that ill-defined for you? Physics in decompactified 11D M-theory is the large N limit. You can prove that this reproduces 11D supergravity at low energies – gravitons, multiparticle states, their correct statistics, gravitational scattering amplitudes. You can prove that this theory contains M2-branes with the correct dynamics, and you can prove, after deforming it to the pp-wave, that it also contains M5-branes with the right excitations.

I am just trying to solve the SU(N) Matrix model in a more or less full, exact form.

You can prove that this quantum mechanical model contains states with the same scaling laws, up to numerical constants, as the black holes in 11 dimensions. You can show that compactification of this M-theory on circle leads to another matrix model, which is the 1+1 super Yang-Mills on a cylinder, and this matrix string theory exactly agrees with light cone gauge perturbative calculations if you take the perturbative limit.

It's just bullshit and a lie to say that M-theory is not at all well-defined – or that it is just a collection of dreams. M-theory in 11D is perfectly well-defined, much like string theory is perfectly well-defined in all of its perturbative regimes, and in all these cases, we are already able to actually calculate a huge percentage of the questions. The only missing piece of the definition is a definition of nonperturbative string/M-theory on a *completely general* background – something that may be needed to understand the vacuum selection questions reliably.

Moreover, the very explicit Matrix model above can be shown to be related by dualities to other descriptions of string/M-theory, and you can go to AdS/CFT and others. Be sure that Weinberg knows all these basics of string theory, and the only reason why *you* want to disagree is that so far, you are lazy and you prefer to pray that string theory will die, instead of trying to reduce your 15-year-long delay in particle physics and learning string theory. It is much easier to pray that string theory will die instead of learning something.

Instead of expecting a smart person like Weinberg to distribute wrong, stupid, narrow-minded lies about "string theory being a wrong idea and a collection of dreams and nothing more", you should try to learn this amazing set of constrained, rigid, and interconnected ideas called string theory, and the matrix model could be a good starting point for you to learn it. Matrix theory was the framework that brought me to active work a couple of years ago, and it is exactly because it is so well-defined. I apologize, but only an idiot could say about this particular model that it is not well-defined.

Try to prove to yourself, to me, as well as others that you are not such a dumbass as D.R. Lunsford and many others. I think that you are much more intelligent, and be sure that there is nothing wrong if you are able to learn something new and change wrong opinions from the past, as opposed to insisting on your wrong position that string theory is a fad.

All the best
Lubos

18. **Lubos Motl**
October 10, 2004

Hi JC,

as Weinberg emphasized – in his talk that I liked – it is guaranteed that a relativistic quantum theory at low energies will always look as a quantum field theory. Using the tools of QFT, we can restrict some properties of higher spin fields.

Higher spin fields could be excited strings; extra components of gravity from deconstruction; many other choices. We would have to see exactly how they're produced, how they decay, and what are their quantum numbers. In general, higher-spin fields would look very stringy, and usual quantum field theorists – particle phenomenologists – rarely work with theories with higher spin fields. However, the details may be incompatible with any stringy models. I just can't give you a universal answer.

Massless higher spin particles should not exist because they require gauge invariance to decouple the negative-norm time-like modes, but such gauge invariance is incompatible with any interactions. Moreover, I don't know how would the LHC help you to produce new *massless* particles. The LHC is useful to produce new massive particles with the mass above hundreds of GeV.

All the best
Lubos

19. **Lubos Motl**
October 10, 2004

Dear Matt, your comments about the Higgs sectory in conformal technicolor are interesting. Won't you post them on sci.physics.strings? This is the kind of discussion that I would love to see there. See

http://schwinger.harvard.edu/~sps

to see different ways how to get to the newsgroup.

20. **Peter**
October 10, 2004

The problem with string theory is not that there's no guiding principles, but that there's no theory. M-theory is not a theory, but a hope that a theory exists. No one knows what the fundamental variables are or what equations relate them. By any definition of a scientific theory, it's not one. String theorists would like to believe that not only does the theory really exist, but it's beautiful and based on some simple principles. At the moment, this is just wishful thinking.

21. **serenus zeitblom**
October 10, 2004

I'm sure that Weinberg is right that there is no "guiding principle" behind string theory. But that's no big deal — none of our best theories has such a principle. Think of General Relativity, which just says that spacetime is curved and that this curvature is the phenomenon called gravity. Where is the guiding principle? It's just a clever and beautiful idea that happens to be right. All those "principles" of relativity, equivalence, Mach etc are all just historical junk of no physical importance. So if string theory has no awesome but ultimately trivial "principle" behind it, then it is in good company.

22. **JC**
October 9, 2004

Lubos,

How would string theory deal with a hypothetical LHC scenario where a set of "new" massive "gauge particles" are found, which have a spin of:

a – spin-2

b – spin-3, spin-4, or higher

Also how would string theory change if a massless spin-3 or spin-4 gauge particle is discovered, which is found to be significantly weaker than any of the other four known forces?

23. **Matt Reece**
October 9, 2004

I suppose it's true that one generally does have a new scalar field in these models, but these have a very different origin from the SM Higgs. In Randall-Sundrum type Higgsless models one has the radion to worry about, but it might be stabilized in some way such that the LHC does not see it. (Of course there are also RS models with a Higgs.) In the "conformal technicolor" approach, they predict a scalar resonance below the TeV scale, which would look approximately like a heavy Higgs with couplings that aren't quite the Standard Model values.

My inclination at this point is to say that it's an open question whether one generically expects a Higgs-like scalar to show up at the LHC even in "technicolor-like" approaches. The phenomenological difficulties in building a model with no such scalar are large, but it's not entirely clear yet that this is impossible.

As for motivation, I think these models are well-motivated in the sense that they're the closest we can come to a solution to the hierarchy problem that does not involve supersymmetry. (Well, that is not entirely true; there are also Little Higgs models, which lately it appears can be made consistent with experimental results so far.) Should the LHC see signs of a Higgs-like scalar but no SUSY, one must either admit to fine-tuning, or look for other new physics that explains the hierarchy. In this case it would be good to have examples like "conformal technicolor." But there is always the chance the LHC will see no Higgs-like scalar at all, in which case we would like to have at least some idea of what *any* model with unitary WW scattering but no Higgs-like scalar looks like. We're getting there, although as you say the examples are convoluted. One would ideally like to have such a model solve the hierarchy problem with no fine-tuning, but it's not clear we can do this yet.

Then, one could always take the position that everyone should just wait for the experimental results before going too far on some of these lines, as one experimentalist expressed to me, but I think we stand to learn interesting lessons in the meantime from trying to understand the space of possibilities.

24. **Lubos Motl**
October 9, 2004

OK, Matt (Reece), let me soften the statement a bit. The discovery of strongly interacting physics behind electroweak symmetry breaking would mean the expansion of some (as of today) very new models with approximate conformal symmetries in some energy regime, their AdS duals, and consistent incorporations of technicolor. I've heard a couple of talks about it. My feeling is that these models always require you to have some scalar field at the beginning anyway, and the physics of this scalar field just becomes complicated because of the strong interactions. My opinion is that these models are not terribly well motivated and unnecessarily convoluted – especially because the Standard Model with the fundamental Higgs works fine. But of course, I am slightly open-minded about that.

25. **Lubos Motl**
October 9, 2004

New generations, preons, new gauge groups.

A – If new generation(s) are found, it's not a big deal. The neutrino from this new generation can't really be light because the number of neutrinos lighter than the Z bosons is measured from the decay rate of Z, and the experimental result is 3+-0.01 or so. One can imagine a very heavy new generation with a heavy neutrino, but it just sounds weird – especially because the fermions, if they get mass through the Higgs, have Yukawa couplings to the Higgs proportional to their mass, and a fermion much heavier than the electroweak scale seems to imply a very large Yukawa coupling (greater than one), which seems to lead to Landau poles (divergence of the coupling at some slightly higher energy scale) and inconsistencies in the field theoretical description.

An explicitly seen new generation that is really similar to the known 3 generations would be weird, and it would probably be interpreted as an evidence for many Higgs doublets – and the new heavy generation would mostly couple to this much heavier Higgs doublet.

I don't know – it's a weird phenomenological gedanken experiment that would have no impact whatsoever on 99.9% of the string theoretical research.

B – On the other hand, preons would mean a significant shock for string theorists because – as of today – we have not seen anything like that in the stringy models. Especially the idea of a composite gauge bosons – made of spin 1/2 subparticles – seem very weird from the string theoretical point of view. If the experiments supported the thesis of some people that only spin 1/2 particles can be fundamental, string theory would be in trouble. String theory as we know it today pretty clearly implies that all light particles with spins 0, 1/2, 1, 3/2, 2 can be equally elementary.

Preons would be very hard to reconcile with string theory, I think – even though, obviously, a few people would try. This holds in all cases.

C – on the other hand, new gauge symmetries, new heavy Z' bosons, and so forth, would be compatible with strings, and they could tell us a lot about the origin of the gauge groups (geometry of the branes etc.). String theory can generate the Standard Model and bigger groups, but it can also produce some pretty different gauge groups, and in some 4D realistic SM-like models, the amount of new U(1) (broken) symmetries can be rather large. C would certainly be an interesting arena for string theorists and many people would become string builders, having a new material to explain.

26. **Lubos Motl**
October 9, 2004

If SUSY is found but Higgs is not, I don't have any immediate solution, but no doubt, all people in particle physics and string theory would try to explain why the thing that seems like the electroweak symmetry is broken. I think that string theorists and particle physicists would be comparably surprised why one of the more solid and older predictions is not confirmed while the newer one (SUSY) is. I think that the normal particle physicists would be surprised more than string theorists because they view SUSY pragmatically as a natural mechanism to protect the mass of Higgs, and if there is no explicit Higgs, their main reason for SUSY disappears. For string theorists, SUSY is first of all a natural and nice friend of string theory that makes it work better, and Higgses are independent.

At any rate, if SUSY is found, it will be a boost for the type of thinking led by string theorists because it will show that we are on the right track. The electroweak symmetry would have to be broken otherwise, or it will have to be shown as an approximate phenomenon.

27. **Matt Reece**
October 9, 2004

Lubos wrote:

If (2) is realized and the Higgs is replaced by some more complicated physics breaking the electroweak symmetry, phenomenology will have very new, moderately interesting topics to study, and they will have very little to do with the teachings of string theory. String theory will be, in this case, disappearing from experiment-oriented physics departments, moving again to math departments – maybe even more rapidly than in the case (1).

I'm not at all convinced of this. If electroweak symmetry is broken in some way corresponding to new strongly interacting physics (a lot of the models being considered here involve new conformal physics, as suggested by Vafa, Strassler, and others), I think we might have to turn to stringy dualities to better understand these strong interactions. This is certainly true at large N (in the Randall-Sundrum models of this type that we have now, we find flavor-related difficulties in the third generation, just as in traditional technicolor, but it's not yet clear that these difficulties are insurmountable). Markus Luty and Takemichi Okui recently argued for the small N conformal case in a paper called "Conformal Technicolor," and there we have even fewer tools for understanding possible CFTs at small N. In general I think we have a poor understanding of the possible behavior of new gauge theories, and supersymmetric dualities have shed the most light on these issues so far. So, if confronted by data indicating new strong interactions, I think we'll have a difficult time figuring out what strongly interacting theory is right, and I think SUSY and string theory might give us the tools that allow us to do this. I also think such a scenario would be more than "moderately interesting," and I would greatly prefer it to clear evidence of SUSY (it would leave us with more fun things to explore).

28. **sol**
October 9, 2004

oops sorry…..that was suppose to be **back ground dependancy**

29. **JC**
October 9, 2004

Lubos,

On a slightly different twist, how would the string theory picture change in these hypothetical LHC scenarios (or a future particle accelerator):

A – a 4th vertical family of quarks/leptons is found

B – quarks and/or leptons are found to have an additional substructure, made out of "preons"

C – a "new" and different set of gauge bosons are found, which don't fit into any of the known theories like the standard model, grand unified theories, nor any of their supersymmetric extensions

30. **sol**
October 9, 2004

Lubos,

Linear extrapolation can only be done on back dependancy?

sol

31. **JC**
October 9, 2004

Lubos,

Just to be complete, how about an LHC scenario where no standard model Higgs is found, but SUSY particles are found?

32. **Lubos Motl**
October 9, 2004

Hi JC,

if the standard Higgs is found at the LHC, but no SUSY (and probably no other stringy things), then the activity, funding, and the number of new students going to string theory will decrease visibly.

People will have to live with the fact that the world is fine-tuned a bit, even as far as the mass of the Higgs goes, and they will create new models where various things are "unnaturally" fine-tuned. The influence of the anthropic principle will increase.

Those who survive in string theory will forget about phenomenology, and they will incline to mathematical aspects of string theory and the application of string theory to more formal questions related to mathematics and nice, but not realistic quantum field theories. Those that will survive in string theory, trying to match the experiments, will appreciate those who predicted that string theory naturally predicts SUSY breaking at very high energies (like Mike Douglas), but many people will be discouraged by this game.

Well, honestly, the activity in particle phenomenology, including all models with extra dimensions (which, to some extent, work *for* the LHC), will decrease even more in the case (1) – only the fathers of the Standard Model who have already made it will have a new confirmation that their theory is really far-reaching, and Higgs, Goldstone and some others will become hot candidates for a Nobel prize (after the Higgs experimental Nobel prize). The progress in particle physics will slow down even more than today, and the ideas about the new expensive colliders will probably be killed.

Unfortunately this is a very possible scenario although I've made a $1,000 bet against it (for SUSY) haha. Well, so if (1) occurs, I will also lose 1,000 dollars with Michal Fabinger but there will be more serious things haha.

Concerning your second choice. If no Higgs is found, there must be *something else* that corrects the unitarity in the WW goes to WW scattering. The pure interactions of the gauge theory, from the vertices that we know, just do not give you a consistent result. There simply must be some deviations from the no-Higgs standard model – simply because the no-Higgs standard model is not consistent; it is not unitary which means that it does not preserve unitarity (the total sum of probabilities of alternatives is not 100 percent).

If no SUSY, no extra dimensions or strings of course, but also no Higgs is found, the electroweak symmetry must be broken by other means. Let me declare technicolor to be the primary example – the Higgs may be composite and complicated enough so that it is not seen explicitly at the LHC. What happens with string theory if such scenarios get an experimental support? I think that it will suffer as much as in the case (1) because the composite Higgs models and stuff like that do *not* naturally follow from string theory (even though some people will try to make stringy models of technicolor); on the other hand, string theory has no problems with fundamental scalar particles and fields.

If (2) is realized and the Higgs is replaced by some more complicated physics breaking the electroweak symmetry, phenomenology will have very new, moderately interesting topics to study, and they will have very little to do with the teachings of string theory. String theory will be, in this case, disappearing from experiment-oriented physics departments, moving again to math departments – maybe even more rapidly than in the case (1).

As you can see, there is no permanent safety, and if you believe that string theory is fundamentally on the wrong track, you should also think that there are only 3-4 years left before string theory will be punished for that. Of course, such a punishment is never definitive – it is a crazy idea to think that a failure on one collider will remove *all* proponents of a model. One can always move the arguments to higher energies and make them less convincing, but still plausible. Maybe the next collider after the LHC could prove string theory anyway – it is a game of probabilities and arguments, and they are rarely 100% clear until the experiments are done.

Lubos,

33. **Peter**
October 9, 2004

Lubos's "less than" problem has been fixed.

34. **JC**
October 9, 2004

Lubos,

How would the string perspective change in the following hypothetical LHC scenarios:

1 – the standard model Higgs is found, but no SUSY particles are found

2 – no Higgs is found, and no SUSY particles are found

35. **Lubos Motl**
October 9, 2004

It ate my paragraph because after "d" I used the sign "is smaller", which is not compatible with HTML too well. Could not you, Peter, fix it, so that the sign "smaller than" would be translated to the appropriate HTML sentence, so that it's not interpreted as a HTML command?

I said that the models in d less than four dimensions are disconnected with reality, and their only virtue is that they are solvable. But Nature does not care whether we can solve something exactly. The qualitative conclusions from 2D gravity cannot be reliably extrapolated to 4D and other realistic spacetimes.

36. **Lubos Motl**
October 9, 2004

The anthropic principle cannot fuel a revolution or an explosion; the anthropic principle is, on the contrary and because of its very nature, a symptom of a *missing* revolution. If there will be many more years without really new and sharp results in theoretical particle physics, the anthropic principle will grow stronger, together with philosophical, non-quantitative hand-waving, and with satisfaction with not-too-impressive matches.

Well, to be honest, the revolution is not too likely to start with some lower-dimensional models in d less than 4 either because they are disconnected from the real world by their very nature, and the qualitative conclusions of these vacua can never be reliably extrapolated to real physics. Their only virtue is that many things are calculable exactly, but Nature does not care whether we can calculate something exactly.

There are potential sources of breakthoughs – discrete-like structures at the Planck scale (see Vafa et al. papers). Even if these things are relevant, they will be very different in details from all discrete approaches to QG that have been tried so far. An example – the "quantum of entropy" is often naively said to be log(2) or log(3) because the system is thought of as a system of discrete blocks with 2 or 3 states. I believe that it will be shown that the entropy of some special objects, such as extremal rotating black holes, is always a multiple of 2.pi (or at least a rational multiple of pi).

OK, let me say something about what I really believe. Below, there are two different scenarios, theory-dominated, and experiment-led. Let's start with the theoretical one.

I think that the next revolution – and not sure whether next year or in 20 years – will start with revealing some underlying "string theory" behind the theories which we do not consider to be stringy today – namely the CFT on the worldsheet. Well, N=2 strings whose target space behaves as a worldsheet of strings is the pre-example. This will allow us to interpret all effects on the worldsheet – handles, boundaries, crosscaps – as results of underlying "2D quantum gravity" that will have a more complicated nonlocal extension.

A new non-geometrical generalization of the principles of CFT will be found, and it will allow to extend the success of S-matrices etc. to the non-perturbative realm. A geometry-like original of dualities – such as E_k in supergravity – will be clarified. Non-perturbative physics on general backgrounds will become calculable, and supersymmetry breaking will be shown to be very different in details than previously anticipated. Realistic N=1 4D vacua with SUSY breaking will be connected and the potential will pick up a rather small number of priviliged points – close to the "heterotic strings on Calabi-Yau three-folds" and/or "M-theory on G2 manifolds" and/or "intersecting brane models with some warping". Two years after the beginning of the revolution, the people will calculate the masses of the heaviest quarks, the (small) QCD theta-angle, and other things, and they will predict the first new physics beyond the SM, which will be only confirmed several years later experimentally.

Meanwhile, the structure of M-theory in 11D will be solved exactly, and the position of poles of the scattering amplitudes in 11D will be known more or less exactly.

Alternatively, theorists won't be that fast, and the revolution will start experimentally in 2007, most likely with the LHC. A rather simple pattern of masses of superpartners will be found, together with supersymmetry, and it will match one of the popular SUSY scenarios within string theory. Alternatively, small black holes or excited strings are gonna be seen, and their precise patterns will be used to reversely engineer the shape of branes and hidden dimensions.

Most string theorists will jump on stringy phenomenology, and they will refine the models. It will be soon realized that these models have a very natural explanation why they're special from the string theory viewpoint, and the structure of the "landscape" will be viewed very differently than today. It will have a very hierarchic, organized structure, as opposed to the stupid chaotic democratic structure with 10^300 equal members, and the people will start to realize how the early cosmology "creates" the right compactification/branes naturally.

37. **JC**
October 9, 2004

Lubos,

What do you think will be the main ideas which will propel the "third" superstring revolution starting next year? Hopefully it will be something which does not involve the anthropic principle.

38. **Lubos Motl**
October 9, 2004

Hi JC,
that's a very nice gedanken experiment.

First of all, if they abandoned string theory, there would probably be some immediate reason for their decision, and be sure that I would study this reason in detail because this would become one of the most important questions. If the reason were right (or at least very convincing), I would join them.

Second, if I found this reason to be unconvincing or plain wrong, I would just declare that these guys got mad. I would probably also write a couple of humiliating articles about them (so be careful, Gentlemen!). 🙂 In some sense, it would be even more encouraging to do research, and I am sure that some young people would be attracted to the field *because* of the feeling of being highly original and dissenting, but it is likely that we would have no funding of it without these guys, at least for some time. 😉 Who knows.

This is not a truly realistic scenario. The reality is that the people will probably stay in the field, but they are reducing their optimism and excitement about more specific aspects of string theory and its subfields. Although all of them still certainly view string theory as a unified structure, a single theory, the opinion which directions of research and questions are "big" but also "doable" start to differ. It is completely obvious that the more thrilling period in the scientific field we live in, the more focused the research becomes. If less impressive results are created, the interests become fragmented, and it is the case of today.

The third superstring revolution should start roughly next year – by linear extrapolations of the 1st and 2nd revolutions, and also because 2005 is the World Year of Physics. 😉 So stay tuned.

All the best
Lubos

39. **JC**
October 9, 2004

Lubos,

Imagine a hypothetical scenario where guys like Gross, Witten, Schwarz, Greene, Polchinski, Susskind, etc … all abdicated from string theory and stopped believing in it. Would you personally continue doing string theory research if this particular scenario happened?

40. **Lubos Motl**
October 9, 2004

BTW, Peter, Weinberg, although he loves to attack nonsense – like philosophers 🙂 – is not fighting against string theory, and the reason is simply that string theory is not quite nonsense ;-), and he knows about it.

Weinberg wrote the foreward to the book "String theory" by Joe Polchinski:

"From the beginning it was clear that, despite its successes, the Standard Model of elementary particles would have to be embedded in a broader theory that would incorporate gravitation as well as the strong and electroweak interactions. There is at present only one plausible candidate for such a theory: it is the theory of strings, which started in the 1960s as a not-very-successful model of hadrons, and only later emerged as a possible theory of all forces. … There is no one better equipped to introduce… than … Polchinski … D-branes … Polchinski has a rare talent for seeing what is of physical significance in a complicated mathematical formalism, and explaining it to others. In looking at his book, I was reminded … Texas… where I had benefits from his patient, clear explanation of points that puzzled me in string theory. I recommend this book to any physicist who wants to master this exciting subject."

By the way, each string theorist is also using the name of 't Hooft roughly 3 times a day in average. We don't view him as a string theorist, but he would certainly joined the top ten if he described himself as a string theorist. 😉 't Hooft is teaching a course on string theory and has extensive lecture notes, see

http://www.phys.uu.nl/~thooft/lectures/string.html

The Dutch are very good in our field, I would say, and of course support of their leaders is helpful.

Sorry, Peter, but these people probably know more about string theory than you do. It is great to criticize nonsense, but it is also good to learn in advance whether something is nonsense or not.

41. **Lubos Motl**
October 9, 2004

Well, Edward Witten is (still, I hope) on our anti-anthropic side, although he is not as radical as Gross. 😉

Today, Weinberg is not a string theorist, and therefore he can afford a completely neutral – and to some extent detached – point of view. He knows that the progress in particle physics is significantly slowed down because of missing new experiments, and he also knows that string theory is the only really big idea/framework around, but it is not understood well enough to beat the absence of the new experiments.

Peter, I am pretty sure that if Weinberg thought that the reason why string theory is not connected to new experiments is different – namely that it is not the right idea – he would definitely say it. Instead, he says that he does not know, but he guesses that the prediction that the current string theory research will be viewed (in 100 years) as a heroic path towards the theory of everything is more likely (PBS, The Elegant Universe).

We would be happier if Weinberg remained in the business of quantum field theory because even though it is not the most intriguing era right now, it still looks much more interesting to many of us than the calculations of acoustic waves from early cosmology… 😉

SUSY breaking is a big issue. My feeling is that many people sort of think that they already understand at least some mechanisms of it quantitatively, but it's not quite my feeling, and I can still imagine that the correct final understanding of SUSY breaking will imply a small cosmological constant, for example.

42. **sol**
October 8, 2004

Peter

Weinberg is certainly one of the smartest people in the business, and he in general is a very clear and elegant expositor and thinker. Normally he's very skeptical of nonsense (see his public comments on religion), so I've always been surprised that he is not more critical of string theory. However, if anyone is a leading member of the particle theory establishment it is him, and I think he's not willing to countenance the idea that he and many of his colleagues have gone off on a 20 year wild goose chase.

In this same vein, I wonder why you have been so critical of string theory, and have remained silent on the issue of gravitational waves?

The relationship is obvious in terms of what is needed for proof, yet we have modelled a deeper reality in our methodology for proof?

I ask this in a nice way, to point out the discrepancy I see as a outsider, looking in.

43. **Peter**
October 8, 2004

Weinberg is certainly one of the smartest people in the business, and he in general is a very clear and elegant expositor and thinker. Normally he's very skeptical of nonsense (see his public comments on religion), so I've always been surprised that he is not more critical of string theory. However, if anyone is a leading member of the particle theory establishment it is him, and I think he's not willing to countenance the idea that he and many of his colleagues have gone off on a 20 year wild goose chase.

In my experience, good mathematicians and physicists often keep doing very original and impressive work into their sixties, but almost all slow down a lot when they hit 70 or so. Weinberg is 71.

While I find a lot of Weinberg's accomplishments impressive, I do find that I have a very different set of prejudices about particle theory. I came into the field just after the advent of the standard model, when people were just realizing how beautiful and geometric the model was. So my prejudice is kind of like Dirac and Einstein's: one should be looking for new beautiful, simple and powerful geometric principles as the way to make progress. My prejudice is also that QFT is not just an effective theory, but is the most fundamental theory we know of. The example of asymptotically free theories shows that QFTs can make perfect sense to arbitrarily short distances.

Weinberg began his career in the era of S-matrix theory, when the prejudice was that QFT could not be a fundamental theory, that it was just an effective theory at low energies. He never swallowed the full S-matrix nonsense, but has always thought of QFTs and geometry as being just low energy approximations to something unknown. His prejudice is that QFT is not fundamental, but a structure one is lead to by imposing consistency of locality, special relativity and quantum theory. This prejudice shows up in his comments against geometry in his GR book and in the way he does gauge theory in his QFT books. I think it is still showing up in his talk yesterday when he hypothesizes that string theory may also be some non-fundamental framework not based on a simple geometric principle, but whose necessity is imposed by consistency.

44. **Chris Oakley**
October 8, 2004

"Nonsense" does not quite get there.

"We think that SU(3)xSU(2)xU(1) is pretty good although there are a number of unresolved problems; we invented something called 'Supersymmetry' which is an interesting but thus far inapplicable idea; we can't seem to quantize gravity, what shall we do? I know, let's start again. Let's suppose that instead of point particles we have one-dimensional strings. Unfortunately it does not work in less than 26 dimensions. Is the theory wrong? No, the world must be 26-dimensional. Hey, if it's supersymmetric it could be only 10-dimensional! Great! Only six unobserved dimensions to explain away (plus the fact that the world is not supersymmetric, but never mind that). Oh dear, there seem to be five ways of doing this, what shall we do? How about adding another (unobserved) dimension? Great! Now what? Um …"

45. **Fabio**
October 8, 2004

I remember seeing a Feynman quote apropos string theory. He said something like: " when I was a young man, I heard all these old physicists saying all this newfangled stuff is nonsense. Now I'm an old physicist, and I must say I think all this newfangled stuff [string theory] is nonsense."

46. **JC**
October 8, 2004

It sure sounds like a lot of the older particle and/or string guys have fallen into the "old fogey" pattern of cynicism, after many years of hype which didn't pan out and the eventual letdowns. I wonder if this is an "age" thing in general, independent of the topic of discussion?

47. **Chris Oakley**

October 8, 2004

Anyone can give “state of the union” type speeches about their subject in general terms; the real test is when they talk technical. The vast majority of technical seminars I went to when I was in theoretical physics were incomprehensible to anyone who had not worked in the specific area related to the seminar. Not so Weinberg: he spoke with clarity and precision, and I was able to follow everything despite not being a specialist in all the topics that he covered. Being able to communicate effectively seems to me to be a trait of a good scientist. I seriously doubt that people can really have understood something if they are unable to communicate it in a comprehensible way.

48. **D R Lunsford**
October 8, 2004

He’s a curious fellow. In his gravity book, he goes to great lengths to explain how the geometric approach is wrong-headed, that Lorentz invariance and the Principle of Equivlance force gravity to be a spin-2 gauge field, then he gives the most perfectly geometric exposition possible. One can never really tell what he “believes” rather than “knows”.

49. **Rob**
October 8, 2004

For the record, I talked to Weinberg about a few things last weekend when he was here at the Perimeter Institute and he didn’t seem old, tired or discouraged. Indeed, I remember thinking that he had remarkable energy for someone who has been in the game so long.

Witten on "The Future of String Theory"

October 9, 2004
Categories: Uncategorized

Witten's talk this morning at the KITP on "The Future of String Theory" is now available. He only talked for about fifteen minutes and then took some questions. I thought it was a rather weird performance. Not only did Witten not really have anything to say about the future of string theory, he didn't even discuss the present state of the theory. The most recent thing about string theory he mentioned is the now seven year-old AdS/CFT correspondence. He drew the standard picture of Feynman diagram vs. string world-sheet, claiming it indicates that space-time is an "emergent phenomenon". He even noted that he has been drawing the same picture for nearly twenty years now and he still doesn't know in what sense this "emergent space-time" idea is true, although AdS/CFT is the closest thing to the kind of thing he is looking for. Now not only space-time is supposed to be "emergent", but so is the string itself, although he admits he doesn't know what this means.

Instead of looking optimistically to the future, Witten's talk was extremely defensive. He started off trying to defend why string theorists work on string theory (basically because it is a non-trivial extension of QFT, contains gravity and has lead to important mathematical results). Much of his very short talk was taken up by mentioning criticisms of string theory and giving unconvincing responses to them. He didn't say anything in the bulk of his talk about the Landscape or twistor string theory, or anything else going on these days in the field.

There were several questions from the audience. Someone asked him if he would still believe as strongly in string theory if the LHC didn't find supersymmetry. He somewhat evaded the question, saying he would be less optimistic about how well we can ever understand the world, but implying that he wouldn't consider this as evidence against string theory itself, repeating the same defense of why he did string theory that his talk started with.

The last question was about the anthropic principle and the Landscape. He began his answer with something like "Well…..(nervous laughter)… uh….." then finally said more or less "I'd be happy if it is not right, but there are serious arguments for it, and I don't have any serious arguments against it." So I guess he comes down on the Weinberg side ("I don't like it, but maybe we have to accept that our fundamental theory can't explain any of the things it is supposed to") vs. the Gross side ("people who think this way have given up doing physics").

Comments

1. **D R Lunsford**
   October 13, 2004

   Everyone knows that gauge invariance requires experimental support to be massless.

2. **Chris W.**
   October 13, 2004

   ..and, in light of the string theory landscape it is unclear what "massive experimental support" could ever mean. The landscape's partisans seem to be thrilled that it might contain a hill or valley that resembles whatever observations one might anticipate making.

   Even discounting the landscape, the profligate mathematical richness of string theory is a liability (and a distraction), not an asset. The theory doesn't appear to make any *new* experimental meaningful statements of the form "X can never happen", "X will never be observed", or "X must happen in situation Y". Cocky and dangerous predictions like that are what make a theory truly testable. To make them, string theory will have to become a very different sort of theory than the one we now have. That won't happen unless its practitioners make a deep commitment to making it happen. I don't sense such a commitment or the kind of active and imaginative critical effort it produces.

3. **Thomas Larsson**
   October 12, 2004

   If you were Witten, what would you do to "fix up" string theory as it's known today (besides fixing up diffeomorphism anomalies)?
   What would convince you to change your mind and be in support of string theory?

   In the unlikely event that string theory acquired massive experimental support, I guess that I would have to believe in it. But the present situation is rather the opposite.

   The construction of a quantum theory with some prescribed symmetries is, from my perspective, the same thing a constructing the representation theory of the group of symmetries. There is really a 1-1 correspondence:
   1. Given a quantum theory, its symmetry group acts by a unitary representation on the Hilbert space.
   2. Given a unitary representation of some group, the Hilbert space on which it acts is the Hilbert space of some quantum theory.

   In particular, the Hilbert spaces of the fully interacting gauge-invariant or diff-invariant theories carry unitary representation of the groups of gauge transformations and diffeomorphisms. Perhaps one should factor out gauge symmetries, although I don't see why – it is definitely not necessary for consistency (unitarity). But this is really irrelevant for the argument. The anomalies must be there at least before factoring them out, so if you cannot write down the anomalies in the first place, you lose.

   I am pretty sure that there is no way to fix string theory. The representations look the way they do, and their Hilbert spaces look rather like fixed versions of field theory. I don't see any way to "fix" SU(2) to allow for unitary spin-1/4 representations either.

   I don't have a clue what I would do if I were Witten, and I don't really care. It's not my problem.

4. **Peter**
   October 12, 2004

   Some comments about Arun's questions #2 and #3.

   There are two main ways people have tried to relate string theory (whatever it is…) to real-world physics. The first, and in some sense the oldest, is the idea that it provides a dual formulation of gauge theories like QCD. The latest version of this is the AdS/CFT correspondence, which some string theorists go so far as to take as an answer to the "What is string theory" question, defining string theory in terms of a QFT. I don't have any problem with this. It is a promising way to learn more about gauge theories like QCD, worth investigating and may lead both to better understanding of how to calculate things in QCD, as well as to a better answer to the question of what string theory really is and what it is good for.

   The second way people try to relate string theory and the real world is the one that has got the most attention over the past 20 years, and it's the one that I think is completely misguided. This is the idea that one can use anomaly cancellation conditions to pick out one or more supersymmetric theories in 10 or 11 dimensions with specified gauge groups, and that this will give a theory of everything. There are two separate kinds of arguments against this, besides the operational one of 20 years of failure to get anywhere.

1. Nothing comes out right: We live in 4 dimensions, gauge group SU(3)xSU(2)XU(1), non-supersymmetric particle spectrum. String theory predicts none of these things. If anything, anomaly cancellation predicts 10 dimensions, E8xE8 or SO(32), supersymmetric spectrum. To get things to look like physics you need to engage in complicated, ugly, ad hoc constructions (latest example is KKLT) that completely ruin any predictive power of the theory. This is extremely strong and convincing evidence that the initial idea is simply wrong. I don't understand why people refuse to admit this.

2. There is no good non-perturbative formulation that does what you want. The great dream of the subject is that there is some beautiful 11d theory based on some elegant principle that, once it is found, will automagically solve the problems in 1. I think there is zero evidence for this despite 20 years of effort, and at this point the people who go on about this are simply engaging in wishful thinking. Witten, Gross and others at some point need to either put up or shut up about this. If for twenty years now they haven't been able to find this wondrous new principle, they should admit it probably doesn't exist and do something else with their lives. The fact of the matter is that, mathematically, the standard model involves far deeper and more elegant mathematics than that used in string theory.

5. **JC**
October 12, 2004

Thomas,

If you were Witten, what would you do to "fix up" string theory as it's known today (besides fixing up diffeomorphism anomalies)?

What would convince you to change your mind and be in support of string theory?

6. **Thomas Larsson**
October 12, 2004

#2. he expands later to "What is the core idea analogous to the principle of equivalence in the case of general relativity?"
#3. he expands later to "Even if it occurs {i.e., we understand what string theory is}, and if string theory is on the right track, will we be able to learn how to use it to understand nature?"
…
But it seems that Peter thinks that #2 is also dubious? I had thought that while the final answers for #2 are not available yet, there is little doubt that there is something there; in fact the seeming wealth in an answer to #2 is why physicists study string theory, and why we have strong opinions that #3 will be answered in the affirmative.

Given that Witten and hundreds more of the world's brightest people have spent 20 years on #2 and still haven't figured it out, why do you believe that there is something there? And even if there is something, who will figure it out, when Witten obviously wasn't smart enough?

Witten may well be the smartest person since Aristotle, but that doesn't mean that Wittensian gravity is more successful than Aristotelian gravity. Besides, Witten at 55 is probably less smart than Witten at 35.

It is almost a theorem that string theory will fail. It doesn't account for the diffeomorphism anomalies in 4D which must be there in canonical quantization by analogy with the conformal anomaly on the worldsheet. Anomalies are non-negotiable things, and if your theory cannot describe all necessary anomalies, it is in deep trouble.

7. **Arun**
October 12, 2004

Don't let physicists, too, fall into the slick slide trap!

8. **Lubo? Motl**
October 12, 2004

I agree – industry seems to be a decade ahead of the physicists in presentations. The mathematicians, on the other hand, have not invented the overhead projector yet, and therefore they always prefer the blackboard.

Well, some of us are trying ;-), see

http://schwinger.harvard.edu/~motl/twistors.ppt
http://schwinger.harvard.edu/~motl/rutgers-talk.ppt

9. **Fabio**
October 11, 2004

Wow. Looking over some of the slides from the talks, the thing that impresses me most is how primitive they look (with the exception of t'Hooft's). Having been in industry for a decade or so I was sort of surprised to see that people would still scribble things on transparencies with markers for a conference talk. It's kind of quaint.

10. **Arun**
October 10, 2004

Just watched Gross's talk, very worth watching.

Witten had three questions at the start of his talk:

1. Why do physicists work on string theory?
2. What is string theory?
3. Can we connect string theory to particle physics?

#2. he expands later to "What is the core idea analogous to the principle of equivalence in the case of general relativity?"

#3. he expands later to "Even if it occurs {i.e., we understand what string theory is}, and if string theory is on the right track, will we be able to learn how to use it to understand nature?"

I had understood Peter's criticism of string theory to be basically centered on #3, i.e., his stand is that the connection of string theory to nature has been tenuous all through its history, and the situation shows no signs of improving. And if #3. has no good answer, then #1. needs an answer – why should physicists work on it?

But it seems that Peter thinks that #2 is also dubious? I had thought that while the final answers for #2 are not available yet, there is little doubt that there is something there; in fact the seeming wealth in an answer to #2 is why physicists study string theory, and why we have strong opinions that #3 will be answered in the affirmative.

I think asking the question #3 in the form of "what experimental results or observations in the next 10 years will help us decide whether string theory is relevant?" can be a useful scientific exercise; while attacking #2. is a waste of time.

-Arun

11. **Lubos Motl**
October 10, 2004

Dear Arun,

the emergent time is a great question – David Gross was exactly mentioning this puzzle in his talk at KITP.

Special relativity guraantees that if space is emergent, time must be emergent as well.

String theory in various formulations is Lorentz-invariant, and therefore it should agree with this principle.

However the specific formulations we have are able to show that *space* is emergent, but time is never emergent in these pictures. Well, if you have operators or wavefunctions or whatever, and even if you want to predict the future from the past, you need a concept of time.

It's not a contradiction. The manipulations that we are able to make with the space cannot be easily done with time – time is different in details, at the end, for example it can have an arrow (time-like intervals have a universal arrow, past vs. future, while spacelike intervals don't).

If one says that time is emergent, the idea of predicting the future from the past must be approximate and emergent as well. Well, it's not shocking if we study the S-matrix: it is the set of amplitudes between the infinite past and infinite future, and with infinite separation, time becomes sharp and well-defined much like space.

If we look at the gauge-fixed descriptions, such as the light cone gauge ones (Matrix theory, for example), the gauge-fixing always guarantees that there is a well-defined notion of time, and the other operators are simply functions of it.

There have been speculations, e.g. by Aharony and Banks, see

http://arxiv.org/abs/hep-th/9812237

that M-theory – and little string theory in particular – has some inherent non-locality in time. But these conclusions have not been universally accepted yet, I would say.

A few more comments: if you adopt the formalism of the S-matrix, the questions go away – the only invariant object is the amplitude at infinite separations both in space and time, and these emergent notions are "emerged fully" anyway.

However, the S-matrix is not enough to study cosmology. If we want to understand the early cosmology, it seems sort of necessary (or useful) to understand in what sense the time emerges after the Big Bang. It probably does not make sense to ask what was "before the Big Bang" or "before the Universe was Planckian in size" – because before this moment, the concept of time (and the word "before") had not emerged yet. Nevertheless there is a clear feeling that something is missing, and we should be able to say something about "which universes can emerge" from the Big-Bang and which cannot. And the answer about this Planckian super-early cosmology seems to require us to learn HOW time can be emerging and what is it emerging from.

Best
Lubos

12. **Arun**
October 10, 2004

Lubos,

It helps a bit. Let me ask: is the notion of time – smoothly flowing, continuous – essential to any formulation of string theory, or will it emerge out of string theory? Does the notion of time exist in quantum foam/quantum geometry?

-Arun

13. **Lubos Motl**
October 10, 2004

Gross's talk is very cool! Highly recommended.

14. **Lubos Motl**
October 10, 2004

Hi Chris,

thanks for your comment. Was this blog meant to debunk the subject? 🙂 Well, I think that this purpose belongs to the past. I view it as a place for philosophizing about some general questions of string theory and theoretical physics and its future, and especially about the importance of various ideas.

But if one looks at this blog, it is rather obvious that string theory *is* the most important set of ideas we have. Look at the percentage of the postings dedicated to the subject. 😉 Well, there are some essays by Peter that are just anti-stringy and that are "not even wrong", but this is just a natural component of this blog. This blog is not a refereed journal. Nevertheless, I think that there are also some entries in this blog that do have a meaningful content.

All the best
Lubos

15. **Lubos Motl**
October 10, 2004

We have an overwhelming evidence that specetime is an emergent phenomenon. It is not just some Witten's progressive idea, it is an idea that even Brian Greene always says for the popular audience.

The statement means that we should not think about the objects and events to take place on a well-defined background geometry. Quantum mechanics guarantees that the concept of completely smooth geometry is incompatible with quantum mechanics that make things fluctuate.

But string theory goes much further. Geometric descriptions, such as general relativity, are only approximations valid at very long distances. At very short distances, comparable to the “length of the string” (string scale) or “the smallest meaningful black hole” (the Planck scale), physics does not admit a simple description in terms of usual geometry. Geometry is generalized to something much bigger, and the difference between geometry and matter disappears – this is the content of unification of gravity with other forces and matter.

String theory implies a lot of dualities. For example, T-duality shows that the Universe with a circular dimension of radius R is physically indistinguishable from the universe with a circular dimension L^2/R, where L is the constant length associated with the strings (string scale). A radius smaller than L has identical physics as the inverse radius which is greater than L. If one compares these two equivalent universes, one must create a dictionary: for example, objects with momentum N/R in the “small” Universe map to string that are wound N times around the circle of the large Universe, and vice versa.

The momentum is the generator of translations, and you can see above that it behaves physically in the same way as the winding number (how many times a string is wrapped). It is just a matter of convenience whether we call something “momentum” or a “winding number” in these Universes with circular dimensions. Also, mirror symmetry analogously relates two very different 6-dimensional shapes (mirror duals) which nevertheless lead to identical physics if you put string theory on it.

The momenta etc. can have the interpretation of winding numbers, electric charges, and so on, in various equivalent descriptions of the reality. Different equivalent descriptions of reality do not agree what the spacetime geometry is. One of them can become much more reasonable than others, but it is only in the case in which the radii and size of this geometry are much greater than the fundamental scale (for example the string scale). In this case, one geometry is much more realistic and convenient description than others. But because I need the size of the geometry to be large, geometry is just an emergent phenomenon.

At very short distances comparable to the fundamental scale, geometry is replaced by a generalized, quantum, stringy geometry that contains much more stuff that we don’t usually consider to be “geometry”. Geometry becomes unseparable from other physical concepts, objects and phenomena.

The notion of topology of the space(time) manifold also makes sense as the approximation in the limit where we study the long-distance behavior only. At very short distances, quantum mechanics guarantees that even the topology is fluctuating (quantum foam) – one can imagine that the geometry at very short distances is non-commutative, although one must be ready that the word “non-commutative” in the most general situation must be extended and generalized.

Non-commutative geometry is something that allows one to replace functions on a manifold by discrete matrices – the smooth, commutative geometry appears for very large matrices.

Much like in the naive discrete approaches to quantum gravity (such as “loop quantum gravity”), the character of the spacetime is very different if we probe it with a very good resolution. However, the effects in string theory are not just that “space is made of atoms of space”. Instead, there are many new objects, fields, concepts appearing in this regime and all of them are “fuzzy” and mixed up in some way.

Does it illuminate the question?

16. **Chris Oakley**
October 10, 2004

If there is a source of disappointment, it’s the fact that the hundreds of smart people – old and renowned ones as well as the young ones – have not been able to make greater progress in the recent years.

Lubo?, I am glad that I am able to agree with you on something for once. What is the world coming to? The chief bodyguard of the superstring orthodoxy posts comments on a web log devoted to debunking the subject – ? Maybe that is a reflection of the state of affairs in fundamental physics – that we have to huddle together to protect ourselves from the greater threat of all becoming irrelevant.

17. **Arun**
October 10, 2004

“Space-time is an emergent phenomenon” – while even Witten doesn’t know what it actually means, what could it conceivably mean?

18. **Lubo? Motl**
October 10, 2004

Wilczek’s talk is very funny – he says that he will not allow his first Nobel prize to… Well, you should see it.

19. **Lubo? Motl**
October 10, 2004

I think that Witten’s talk is good and pretty optimistic! What are you talking about, Peter? Yes, it is a rather standard talk about uniqueness in quantum field theory and string theory, and about richness of string theory (underestimated even by the enthusiasts, as Witten points out), and about the stringy counterparts of the principles of GR, and what is string theory – and it does not cover the newest developments, but it is announced in the very first sentence, and this orientation of the talk is probably determined by the audience. I would almost certainly give him an “A” for this talk. 😉

I also think that his answers were very honest. If they ask him how (non)-SUSY on the LHC will affect him, he is realistic and talks in terms of “level of belief” and probabilities. Whoever expects some radical statements that one experiment will change totally everything about his belief system, does not really appreciate the huge number of very different rational reasons and insights behind Witten’s opinions.

Dear DRL, I have no idea what you find astonishing and infuriating. Maybe you misunderstood something? We just said that Witten is not pessimistic about string theory itself – that has displayed no real problems with its framework – but about the limited human abilities to study it, which have slowed down the progress in recent years, and it is not completely clear (and it is a matter of hopes) whether we will be able to do better in the future.

At any rate, it is an objective fact that the progress in string theory today is slower than in the middle of 1980s or middle of 1990s. One more thing about the intelligence of string theory vs. the intelligence of us: string theory is much smarter than all of us – me, you, but also Andy Strominger, Cumrun Vafa, or Edward Witten. I am sure that all of them – maybe except for you, DRL – would fully agree with this statement of mine, and some of them have said this statement themselves. You’re probably the only one who thinks that he is smarter than string theory – but this is, I apologize for being honest, a symptom of limited intelligence, not high intelligence. And if you ask why I feel the moral right to conjecture such things about you because of this single topic, it’s because I have known you for quite a while, so this judgement is not just because of this specific question!

If there is a source of disappointment, it’s the fact that the hundreds of smart people – old and renowned ones as well as the young ones – have not been able to make greater progress in the recent years.

20. **D R Lunsford**
October 10, 2004

Lubos, I find a statement like that both astonishing and infuriating. Who the hell are you to make statements about any single PERSON's intelligence, not to say the whole shebang? Not only do you claim to read God's mind, you know what he has in store for poor suffering Man.

I would not only be embarrassed to be credited with such a statement, I would be ashamed.

-drl

21. **Lubos Motl**
October 10, 2004

I agree that Witten's limited optimism stems from the limited human intelligence, not from defects in string theory. In some sense, there is a disappointment that all of us have not been able to go much further.

Brian Greene's TV example with the dog learning GR is very cute, but I still hope that despite the finite brains of all of us, we have some abilities to crack anything that makes sense in the future.

22. **Lubos Motl**
October 10, 2004

I don't believe that reasonable professional physicists believe that the principle of equivalence is unimportant as a foundation of GR. (Did the comment mean Hawking?)

Weinberg has been criticized for not sharing the "religious geometric" viewpoint about general relativity. But Weinberg's point is certainly not that the equivalence principle – or diffeomorphism invariance – is not important.

Weinberg's point – and I am sure that this is shared by an overwhelming majority of particle physicists and string theorists – is that general covariance is different in details, but otherwise it can (and it should) be treated with the same tools as other gauge invariances such as Yang-Mills invariance. Well, string theory unifies these two naturally in many "senses". 😉 Already the Kaluza-Klein theory unifies them, and string theory goes much further.

The special feature of gravity is only in its low energy manifestations – spin 2 fields and particles; coupling to stress energy tensor; geometrical interpretation of the gauge invariance. But these are just visual differences. In the fundamental way, gravity is not that different, which is why it can be unified.

23.
October 10, 2004

To be fair, Witten was talking to an audience who hadn't even heard of Ads/CFT, so you could not expect much. Two things stood out from the talk though: first, people like Witten *still* believe that the Principle of Equivalence is somehow the foundation of GR. People in that field stopped believing that decades ago [hint: search for the POE in Hawking and Ellis' book —from 1973!]. Second, Witten points out that the *real* reason Ads/CFT is interesting is because it might give us a dual to the actual spacetime in which we live. It's great that he says this. It would be even greater if people took his words to heart and actually tried to apply Ads/CFT in that way. I don't think that anyone really believes that *our* spacetime fails to be globally hyperbolic on cosmological scales, do they?

24. **Chris Oakley**
October 9, 2004

Following on from Danny's comments – though less eloquently – I knew that I had no future in the subject just from the kind of casual discussions I was having with my peers 19 years ago. I was a harsh critic, and remain a harsh critic, of things I perceived as inconsistencies in QFT. I worked on the principle that one should wipe one's bottom before thinking about powdering one's nose, which automatically ruled out spending time on grandoise super-unified schemes before basic mathematical problems were solved. None of my colleagues agreed with me on this, and when I would dismiss their string theoretical efforts as unmotivated and overly speculative they would take on the appearance and vocabulary of religious fanatics prepared to sacrifice all for some inexplicable but strongly-held set of beliefs. They are still doing it.

Fine … but don't pretend it's science.

25. **D R Lunsford**
October 9, 2004

Perhaps it is the recognition of hubris in himself and others. After all, what string theory proposed was 1) a revision of Democritus, thus a supposed "new beginning" for science 2) an abandonment of the essential idea of indentical particles and their statistics 3) a deliberate willful disregard of the scientific method in favor of a mediaeval-scholastic imposition of a false order – a return to "reading God's mind" 4) A PR campaign on the level of the Neocons to convince everyone that strings beget gravity, when this was never in fact true.

Wouldn't you be depressed to look in the mirror and know you took part in such a shell game – such a carnival ruse?

In a larger sense, the intellectual climate in the West seems utterly poisoned by fashion and faddery. Facts and imagination are so pre-post-modern.

26. **String Theorist**
October 9, 2004

Witten's disillusionment stems not from any defect in string theory itself, but from the limitation of human intelligence.

String theory exists, regardless of one's prejudices, and the question of whether the landscape exists is a scientific question, regardless of one's prejudices. (It is the question of what to DO about the landscape, if indeed it exists, that some people like to answer non-scientifically.)

We can't expect a dog to understand quantum mechanics, and it may be that we are reaching the limit of what humans can understand about string theory. Maybe there are advanced civilizations out there to whom we appear as smart as dogs do to us, and maybe they have figured out string theory well enough to have moved on to a better theory…

27. **Peter**
October 9, 2004

I know that Witten has been somewhat discouraged about string theory for a while, but still doesn't want to give up on it. Under the circumstances I would have been a lot happier to hear him say something like "things are discouraging for the following reasons, but I still continue to have hope for these other reasons. To continue to be viable, string theory needs to make progress on the following problems, and here are some ideas about them. What Susskind is suggesting about the Landscape is not science and I'd abandon the idea of string theory before going down that path".

In other words, I wish he had come down on Gross's side of the Landscape argument.

28. **Lubos Motl**
October 9, 2004

I am surprised that Peter Woit is surprised to see that Edward Witten and perhaps some others feel slightly disillusioned today. I thought that you, Peter, were following what's going on in psychology of our field, and you were even comparing the citations of the most cited articles in 2003 and 1996, for example. 😉 Yes, it's much less today, and our optimism is correlated with that, of course. It does not apply to some people ;-).

Witten would certainly like some experimental confirmation, too. Also, there is a lot of sense in which he deserves at least one Nobel prize, but clearly, without a breakthrough that will allow experimental verifications of predictions, it's hard to imagine.

Your description does not suggest that Witten's talk is something I can't miss, but because I know in which way you twist, I will try to watch it anyway. 😉

BTW Bush may have seemed uncertain, defensive, and feeling bad about the reports from Iraq in the first debate, but I think that he was slightly better than Kerry in the second debate.

29. **Peter**
October 9, 2004

I think you're very right that that's part of the psychology going on here.

And I should make clear that I don't think Witten is a cynic; I think he believes in what he says (although I wouldn't say the same about our current administration and what they say about Iraq).

30. **JC**
October 9, 2004

Perhaps being in "denial" is a very ingrained human trait, especially when one has invested a lot of time, effort, and pride into a particular endeavor. A very common manifestation of this is in the "sunk cost fallacy" where a person figuratively or literally "throws good money after bad money". On the stock market, it would be where somebody stayed with a crappy investment hoping to one day make back all the money they "lost" on it. Emotionally it's very hard for them to "sell" the crappy investment before reaching the break-even point, since selling and taking a capital loss would tell them that their personal judgement was "wrong" about it.

31. **Peter**
October 9, 2004

Witten's defensive performance reminds me of George Bush being confronted with the fact that the war in Iraq was based on a mistake and the whole thing seems to be going very badly and hasn't worked out at all as planned. Just like Bush, Witten appears to be unwilling to face up to what a disaster the current situation is, or admit that the whole thing may have been a mistake. Unfortunately, Witten's sticking to his guns and seeming highly unlikely to ever give up or acknowledge defeat is good for string theory, but bad for physics. If he ever gives up, string theory is doomed, but as he and others refuse to acknowledge what a disaster it is, the field of theoretical physics suffers immensely.

The way things are going, the endpoint is going to be that theoretical physics will be dominated by the endless study of a horrifically complex and ugly framework that can never explain anything, together with an associated "Landscape" ideology that justifies never having to relate the theory to the real world.

32. **JC**
October 9, 2004

Is this an ominous sign of an eventual day of reckoning for string theory? Or is Witten becoming very cynical about the whole enterprise altogether?

Wilczek Weblogs

October 10, 2004
Categories: Uncategorized

This weblog thing is getting out of control. It seems that Frank Wilczek's wife has one, as well as his daughter. What about you, Frank?

Comments

1. **Lubos Motl**
   October 13, 2004

   Hi Sol, nope ;-). If you follow sci.physics.research, you of course know who is it.

2. **sol**
   October 13, 2004

   Lubos

   But what would you think if you met someone else, someone nice, someone who is around, namely someone who can be described as a leader of quantum computation, who argues that there is really no difference between renormalizable and non-renormalizable theories as far as predictivity goes (he immediately and explicitly gives you the Standard Model and quantized General Relativity with all counterterms up to five loops as examples) – and he even states that drawing a graph of a function (which is a part of the input of a theory) is giving you a more predictive theory than if you know the function analytically, as long as the analytical function looks too complicated to you?

   Who were you talking about here? Smolin and the Perimeter Institute?

3. **Lubos Motl**
   October 11, 2004

   Haha, sol, that's entertaining. The article explaining the problems of loop quantum gravity – the article that you presented to those guys on the other forum – was, of course, written by me as well. 😉

   A newer version of this article is today at

   http://en.wikipedia.org/wiki/Objections_to_the_theory_of_loop_quantum_gravity

   I am not surprised that those guys who want to promote loop quantum gravity dislike Wikipedia for having such articles. But fortunately it is impossible for them to hide knowledge from the internet users.

   All the best
   Lubos

4. **sol**
   October 11, 2004

   lubos

   Also, I link Wikipedia because Wikipedia is free and relatively good. Most Wikipedia articles about string theory (and a nonzero part about adjacent fields of physics) were more or less written by me ;-), and your comments about "defaming them" sound very puzzling. Who was defaming Wikipedia?

   http://www.physicsforums.com/archive/t-42996_Strings_or_LQG_and_why???.html

   I hope that is the right thread(its been archived), but I went on to defend, encouraging others of expertise to add their's, for a truly wonderful reference point for consideration.

   http://superstringtheory.org:8080/forum/edonline/discussion.jsp?thread=16

   I hope you don't mind I added this post to the links in defence?:)

5. **Lubo? Motl**
   October 11, 2004

   Hype and propaganda? Come on.

   There is no hype and propaganda about string theory. People just try to explain why it is a fascinating and unique theory. In the mid 1980s or mid 1990s, people were more excited and the progress was faster, and therefore the comments about string theory were more optimistic. Today it's closer to the opposite. As Witten pointed out on the KITP conference, even string theory *enthusiasts* underestimate how rich and powerful string theory is – so speaking about hype and propaganda is not a reasonable or fair description of reality.

6. **Lubo? Motl**
   October 11, 2004

   Dear sol,

   it is not clear whether I understand you. But my blog, http://motls.blogspot.com/ , definitely allows replies of anyone, including anonymous users.

   Also, I link Wikipedia because Wikipedia is free and relatively good. Most Wikipedia articles about string theory (and a nonzero part about adjacent fields of physics) were more or less written by me ;-), and your comments about "defaming them" sound very puzzling. Who was defaming Wikipedia?

Best
Lubos

7. **Thomas Larsson**
October 11, 2004

This weblog thing is getting out of control.

This seems literally true for the program powering this weblog. It seems like Movable Type 2.661 cannot handle the number of replies to the entry "KITP Conference on "The Future of Physics"". Thus let me respond to JC here instead.

I get the sense most of the criticisms of string theory on this weblog is largely about the hype and propaganda surrounding string theory over the last 20 years. The discussions about specific technical details about string theory and field theory seem to be more neutral and less controversial.

My main criticism is quite technical. String theory failed as a theory of quantum gravity because particle physicists didn't understand that diffeomorphism anomalies exist in 4D – you need to go slightly beyond field theory to see them.

If you miss the relevant anomalies, you will fail.

8. **sol**
October 11, 2004

Well this is fine Lubos, but you have no place in which to respond?

Plus, I had been criticized greatly for using wikipedia in the **physics forum** for reference, so the authors of the sited revisons on those selective topics you have linked, would they have been inspected by yourself, so that this maybe referenced, quieting those whose comments defame the wikipedia references?

I assume because you had linked them it would be yes. Just reaffirming would help greatly.

Then we will have to see if Peter logs your site, on his.

Lubos

Emergent space and emergent time

In string theory, we now have overwhelming evidence that space is an emergent phenomenon. It is not just one of Witten's progressive ideas. Instead, it is an idea that even Brian Greene often explains to his popular audience. The statement means that we should not think about the objects and events to take place on a well-defined background geometry; we should not think about space and time as basic assumptions whose existence is guaranteed before we consider anything else.

9. **Matthew Nobes**
October 10, 2004

http://www.livejournal.com/~manobes

While we're advertising. Nothing exciting though, unless you like perturbation theory, or lattice QCD.

10. **Lubos Motl**
October 10, 2004

OK, you convinced me, my new English blog is at

http://motls.blogspot.com/

Best wishes,
Lubos

11. **Lubos Motl**
October 10, 2004

Hi!
Last year, I experimentally started a blog

http://lumo.blogspot.com/

in Czech, but I was disappointed by the small number of visits of that blog, so I have not contributed anything for a year or so…

But maybe, I will revive that 🙂 and add English stuff.

All the best
Lubos

12. **sol**
October 10, 2004

To tell you the truth I like this bloggery format, as to why Frank hasn't, who knows?

Maybe Lubos needs one.?:)So we can hurl mud and stuff at him figurtively speaking.:)

I like getting the up to date stuff here with Peter's. Plus, we get the negative side psychologiclaly speaking, to balance ourselves if we assume to far ahead.

13. **Lubos Motl**
October 10, 2004

The wife seems as a left-wing activist while the daughter is a kewl writer about nature. 😉

14. **Alejandro Rivero**
October 10, 2004

The most delicate weblog thing, as fas as I now, is the string coffee and it is not working after all. Why should Wilczek risk his weblife?

Witten replaces WMAP

October 11, 2004
Categories: Uncategorized

Witten is giving a colloquium talk next week at Princeton on the topic of "Supersymmetry Pro or Con". His talk is a last-minute replacement for one about "Recent Results from WMAP" by Lyman Page. WMAP was supposed to report the results from the analysis of the second year's worth of satellite data early this year, but this has been delayed quite a bit already, and evidently is being delayed even more. Does anyone know why?

Comments

1. **D R Lunsford**
   October 14, 2004

   No – Pauli is right and Kaku – or whoever authored this quote (and I apologize in advance if Kaku was misquoted or not quoted) – is wrong.

2. **sol**
   October 14, 2004

   DRL,

   I was looking for specific quotes by Kaku, but immediately did not find it. I will supply when I do.

   However, since our universe obviously has 4 dimensions (3 spatial and one temporal), people were troubled by this extra 5th-dimension. Oskar Klein, a German mathematician, offered an explanation; the 5th dimension could not be seen because it was much too small. It had curled up, or compactified. For example, picture a garden hose. From far away, it appears to be a line (one-dimensional), but upon closer examination, one finds that it is a tube (a 2-D surface curled up).

   http://www.ecf.toronto.edu/~quanv/String/string4.html

   Is this a historical difference of opinion?

3. **sol**
   October 14, 2004

   I must apologize D.R. Lunsford for mispelling your name in post previous and ask you to click on Lubos's name on information you quote, that you took the statement from.

   The html has to be placed in front of each paragraph to show italiced statement. This did not come about and looks like my writing, when its not. I should have previewed, and then posted.

4. **D R Lunsford**
   October 14, 2004

   Sol,

   It is absolutely false in every detail.

   -drl

5. **sol**
   October 14, 2004

   D.R Lundsford,

   You must always look to how these statements are produced. As a laymen I am at the mercy of minds better educated, so I would look at your response to further my position of understanding.

   What do you think of this statement?

   By adding a 5th-dimension, Kaluza unified in one stroke Einstein's theory of gravity with Maxwell's theory of electromagnetism.

   regards

6. **D R Lunsford**
   October 14, 2004

   Sol,

   I take strict exception to some of your statements below , viz:

   Theodor Kaluza made a shocking suggestion back in 1919 when he proposed that the electromagnetic waves are actually ripples on the shape of spacetime in the 5th dimension. This "Kaluza-Klein theory" was the first serious proposal to unify gravity and electromagnetism.

   The first (and IMO only) "serious" attempt to unify EM and gravity, in the sense of an irreducible, background free theory where Am and gmn have equal roles, is Weyl's theory of 1918. KK theory is spurious unification, as can be seen by the simplest possible argument (given by Pauli) – *any* theory at all that can be made generally covariant, can be cast in Kaluza's form, where the extra fields appear as part of the metric. So for example I can immediately write down a spurious "unification" of gravity and the Dirac theory, where the extra components will look like

   (psibar ym psi) (psibar yn psi)

   KK theory is *not* unification in the same sense as, say, Maxwell's melding of electricity and magnetism.

In all cases, electromagnetism and gravity are unified into something like "generalized geometry".

Not so. It's Riemannian geometry. Weyl's theory, on the other hand, *is* a more general geometry.

I don't know how these false mantras become commonplace "knowledge", but it is not good for science. Trust but verify.

7. **sol**
   October 14, 2004

   Is this the same CM?

   Remember we are talking about the Loop Quantum gravity article.

   Charles Matthews
   *Guys, **I'm completely dumb in the subject** but I feel the article is simply too big. May be we should move most of it to something like Loop quantum gravity versus string theory? And let's decide upon quantum gravity and loop quantum gravity articles — they should be something like merged with this article. Having three articles with much similar content is really strange. I believe with can do it in a bloodless way.*

   *Disagree, having looked around. Loop quantum gravity at 42K is already longer than WP likes. It has various articles hanging off it, and I've done the work required to make this another of those. **Further, quantum gravity is not something that should be merged into an article on a particular theory**: it should be a top-level article setting the general scene. So in a sense I'm coming closer to what User:Lumidek suggested, just because this seems to be generating a great deal of interesting writing. Still needs work, of course. **Charles Matthews** 11:26, 10 Oct 2004 (UTC)*

   http://en.wikipedia.org/wiki/Talk:Objections_to_the_theory_of_loop_quantum_gravity

   I am amazed at your credentials and I do appreciate the work that your doing. Even you do not understand the wave you are caught in:)

   Resources for accurate information does require that we support free institutions that would cater to those less informed, and requesting, more information to draw from.

   If, the Third Superstring Revolution goes ahead as it seems it is unfolding, how would "Quantum gravity" serve as a heading and these two aspects of strings and LQG under it?

   The third String Revolution would continue to undermine current positions. Can we tolerate such speculation?:)

   I recognized Lubos work early in Wiki when none were the wiser. I can show this. I found this a admirable quality to do work, where none had not considered, where laymen like myself would draw from.

   By now, you should understand how you got where you are, by your commmenting.

   To Wiki and Beyond:)

8. **sol**
   October 13, 2004

   One last link.

   http://wc0.worldcrossing.com/WebX?14@33.1mcNcPDzSZV.0@.1de13e11

9. **sol**
   October 13, 2004

   sorry paragraph didn't highlight of Lubos, so here's link anyway.

   http://groups.google.com/groups?hl=en&lr=&ie=UTF-8&frame=right&th=5829f107ca301d1f&seekm=Pine.LNX.4.31.0410102104410.7758-100000%40feynman.harvard.edu#link2

10. **sol**
    October 13, 2004

    Well CW, it would be hard to classify your statement, since it didn't compute:) Not surprise eh?:)

    Lubos

    I was wondering if the string theory says that photons are actualy
    > waves in the space-time fabric.

    Photons have been quanta of electromagnetic waves since they were first proposed in the early 20th century. Theodor Kaluza made a shocking suggestion back in 1919 when he proposed that the electromagnetic waves are actually ripples on the shape of spacetime in the 5th dimension. This "Kaluza-Klein theory" was the first serious proposal to unify gravity and electromagnetism.

    This KK theory has become a part of string theory. In some cases (in some spacetimes that solve the constraints of string theory), the electromagnetism can be interpreted as curvature of spacetime (including an extra, hidden, small dimension). In all cases, electromagnetism and gravity are unified into something like "generalized geometry".

    Moreover, a photon, much like a graviton, can be interpreted as a string moving in space and vibrating in a certain way. Gravitons are always closed strings (circle-shaped loops) while photons can be both closed strings as well as open strings (in the braneworlds).

    > Also I would like to know if their is a theory that says, instead of
    > having many tiny strings, space-time itself would be The String and
    > what appeared to be tiny strings would be ripples (interference?)in
    > the space-time fabric.

    Actually, there is one proposal that sort of matches your – otherwise a bit confusing – remark. Some authors have proposed that the whole Universe started, a very short time after the big bang, as a single string. No four-dimensional spacetime really existed at that moment; the Universe could have been interpreted using two-dimensional theory describing two dimensions of stringy worldsheets (one spatial dimension, one time).

Moreover, in some sense, the spacetime is always made of strings – it is a condensate of strings organized in a very specific fashion.

See CW, if we didn't consider the spacetime fabric and its constituents, we wouldn't understand where the Perimenter institute leaves off(background independance), and strings begin.

Quantum entanglement(?), between gravitons and photons, and you can be sure my tones will be heard in some way, or seen in some kind of gravitational spectrum? Non?

I am still learning the language, but once I decipher it to the simple responses given by lubos, you can be sure CW I'll give you a key.:)

I take the hint all around.:) I'll be watching you CW for your contributions.

11. **CW**
October 13, 2004

Serenus,

I think you can take that as a 'yes'.

(This simulator is better suited to writing some form of postmodern literary criticism.)

12. **sol**
October 13, 2004

Serenus Zeitblom
No offense, sol, but you wouldn't be a computer program designed to simulate a physicist, by any chance?

You know I can read too, don't you:)Even if Lubos is critical of others, there are issues developing around the Perimeter institute in regards to quantum entanglement?

Maybe using glast will settle at least some of the issues of this computerization as shown in gamma ray detection.

Yes, my quantum entanglement is not complete, but when it is, there will be no arguing as I will have mapped out the reality for you. So all you have to do is log on, and you are in the world **you fellows** created. 🙂

I just have to find the right geometry(emergent reality)) that describes quantum gravity.

Although this has been pointed out, in the Third Superstring Revolution(TSR) as possibly negated?

Peter's Woits Blog should recieve credit for TSR initated by one of its members,in asking a question of Lubos.

Lubos has revealled this unconscious desire of reform, so you Serenus Zeitblom are caught in a "**new wave**", and don't even know it.:)

LOL:)

13. **serenus zeitblom**
October 13, 2004

"That they might have extended the vision of what the cosmo is capable of, when they regarded strings as a quantum mechanical discritpion of that same universe?"

No offense, sol, but you wouldn't be a computer program designed to simulate a physicist, by any chance?

14. **Matti Pitkanen**
October 13, 2004

Dear All,

If I would be allowed to dream freely I would guess that Witten with his full authoricity would summarize briefly the same that I try to summarize below.

During last 25 years I have developed a unification that I call Topological Geometrodynamics based on the assumption
that space-times are representable as 4-surfaces in H= M^4xCP_2 (Cartesian product of Minkowski space and CP_2).

TGD can be regarded either as a generalization of string model by replacing string world sheet with space-time 4-surface or as a fusion of special and general relativities to obtain a Poincare invariant theory of gravitation.

In contrast to Kaluza-Klein theories, classical gravitation and gauge fields are unified in terms of induction of M^4xCP_2 metric and spinor structure. Standard model quantum numbers are understood in terms of isometry and holonomy groups apart from family replication. Color is not spinlike quantum number at fundamental level but analogous to rotational degrees of freedom of
rigid body. Baryon and lepton numbers correspond to different chiralities for H-spinors induced to space-time surface and quark and lepton numbers are separately conserved.

The basic (not the only one) conformal invariance is naturally associated with metrically 2-dimensional light like causal determinants (call them X^3_l) which by the general coordinate invariance can be selected as representatives of 3-spaces. This conformal invariance implies effective 2-dimensionality: the physics is coded by certain 2-dimensional sections X^2 of X^3_l so that a formalism reduces to a form very reminiscent of conformal field theories. Family replication corresponds to the different genera (sphere, torus, etc.) for X^2 and there is an argument explaining why only the 3 lowest genera are realized.

Super-conformal symmetry is a pure gauge symmetry and does not have imbedding space counterpart: no sparticles. There is Higgs which gives a dominant contribution to intermediate gauge boson masses but only a small contribution
to fermion masses: the rate for producing Higgs is about one percent from that predicted by standard model since Higgs-fermion couplings giving the dominant modes are weak.

The construction of S-matrix leads to a generalization of the notion of Feynman diagram. By the above mentioned conformal invariance all Feynman diagrams represented as light like 3-surfaces connecting initial and final states

are equivalent and fermions are “on mass shell” but not with respect to four momentum which does not appear at space-time level at all. Any generalized Feynman diagram is equivalent to a tree diagram. The notions of functional
integral and virtual particle become obsolete and S-matrix can be seen as a generalization of S-matrix associated with braids with braiding induced by hydrodynamical flow defined
by energy momentum tensor at the light like causal determinants.

Particle massivation can be understood as a
loss of correlations due to the ergodicity of this flow and p-adic thermodynamics constructed for a decade ago expresses this quantitatively and reduces the understanding of the ratios of elementary particle mass scales to Planck
mass scale to number theory.

There are four books about TGD and its applications at http://www.physics.helsinki.fi/~matpitka/ . The chapter “Overview about the Evolution of Quantum TGD” explains the evolution of the ideas can be found at http://www.physics.helsinki.fi/~matpitka/tgd.htlm#tgdevo .

With Best Regards,
Matti Pitkanen

15. **sol**
October 12, 2004

QM is not only acceptable from a pragmatic point of view for describing:

#4 thermo-dybnamical properties of material and light

That they might have extended the vision of what the cosmo is capable of, when they regarded strings as a quantum mechanical discritpion of that same universe?

Sort of brings the clumping issue in line, or Andrey Kravtsov computer models.

They had to see past Smolin(Glast indications), in order to bring something new to the table:)

16. **Alejandro Rivero**
October 12, 2004

Something about fractality?

17. **Sean**
October 12, 2004

They’ve been repeatedly delaying for quite a while now, and have been extremely good at not leaking any info. The simple explanation would just be that they have a lot of work to do and want to get it right; more exciting to imagine that they’ve discovered something unexpected, but we’ll just have to wait until they tell us.

Hidenaga Yamagishi

October 13, 2004
Categories: Obituaries

A special issue of Physics Reports has appeared entitled “Hidenaga Yamagishi’s World”. Unfortunately it’s only available online if you are paying Elsevier, so I won’t post a link (it’s volume 398, issue 4-6). This issue is a memorial to the Japanese particle theorist Hidenaga Yamagishi, who died tragically a few years ago.

Hide was in my entering class at Princeton and we spent a lot of time discussing physics together during our graduate student years and later. He was Witten’s first student, and Witten contributes a touching piece about Hide to the memorial issue, including the comment about his maturity “I suspect that to other students he must sometimes have seemed more like a professor than a fellow student”. I can vouch for the accuracy of that and recall that Hide was probably the one of my fellow theory students that I learned the most from.

Hide came to Princeton from the University of Tokyo, already with a strong background in quantum field theory and particle physics. He got his Ph.D. quite a bit faster than me, and left for a post-doc at MIT. Towards the end of my time as a post-doc at Stony Brook, he arrived there to take a tenure-track job in the nuclear theory group of Gerry Brown.

After I left Stony Brook and moved into the mathematics community, I didn’t hear much about what Hide was doing, until some point in the early-mid 90s when I heard from a mutual friend that he had gone back to Japan, perhaps had been ill, and didn’t really seem to be his old self. Around this time for a few years I got Christmas cards from him and he sent me a couple letters. The last one was in early 1998 and included a manuscript of recent ideas about the topological susceptibility in QCD, a topic we both had worked on and often discussed.

Hide’s thesis was about the effects of a magnetic monopole background on the quantum field theory of electrons. Witten discusses this a bit, but there is a much more extensive discussion in the introduction of the article by Goldhaber, Rebhan, van Nieuwenhuizen and Wimmer. To see some of what he was thinking about near the end of his life, see his article with Ismail Zahed entitled “Is Quantization of QCD Unique at the Non-Perturbative Level?”. They ask the interesting question of how well-defined the whole notion of the theta-vacuum is, given that BRST quantization only fixes invariance under infinitesimal gauge transformations, not addressing what happens with so-called “large” gauge transformations. The manuscript Hide sent me in 1998 was more along these lines.

Comments

1. **Chris W.**
   October 16, 2004

   For what it’s worth, here is the TOC for this issue.

2. **Peter**
   October 14, 2004

   Thanks for the correction!

3. **Amitabha**
   October 14, 2004

   That should be Ismail Zahed. I actually had a look at some of the papers of Dr. Yamagishi … which I probably wouldn’t have if it weren’t for your blog. Thanks for keeping an informative blog.

Sean Carroll on the Landscape

October 15, 2004
Categories: Uncategorized

Over at Preposterous Universe Sean Carroll has some comments on the anthropic principle and the landscape.

He describes one extreme of the spectrum of opinion about this as people who think the whole thing is completely non-scientific, giving what he sees as being the two kinds of objections such people make, neither of which he thinks make sense. Since I'm one of these extremists, I think I should try and explain why and exactly what the nature of my objections are, since they're not exactly the ones Sean mentions.

The first objection Sean attributes to extremists like myself is that of accusing users of the anthropic principle of "giving up" by assigning the parameters of the standard model to a selection effect instead of calculating them. This is very much David Gross's objection, and while I would agree with it as a socio/psychological characterization of the behavior of Susskind et. al., my own version of this objection is a bit differerent. For any given supposed fundamental theory, some observables will be calculable from first principles, and others will be aspects of the particular state we are in, dependent on the history of how we got here. Given a particular observable, in some fundamental theories it may be calculable, in others environmental. But the theory is supposed to tell us which it is going to be. The standard model tells us that the earth-sun distance is environmental, and that the magnetic moment of the electron is calculable. It is silent about the origin of its 20 or so parameters, and whether they are environmental or calculable. It is one of the first jobs of any theory that purports to go beyond the standard model to give some sort of explanation of where these parameters come from, which of them are in principle calculable and which aren't.

The problem with the whole Landscape idea is that it is so ill-defined that it can't even tell you what things are calculable and what things are environmental. You don't know what the fundamental M-theory is that is supposed to be producing the Landscape and governing the dynamics of how the universe evolves in it. String theorists would probably claim that while they don't know exactly what the fundamental theory is, they may know enough about it to make conjectures about what the Landscape should look like, at least in certain limiting cases. The problem is that their conjectures not only don't allow them to calculate anything, they don't even allow them to determine what is going to be calculable. The problem with string theory is not that it can't calculate the vacuum energy, it is that it can't calculate anything. Some string theorists are now using the Landscape picture purely as an excuse to get them out of this embarassing situation. "Not our fault we can't calculate anything beyond the Standard Model, because maybe nothing beyond the Standard Model is calculable". If they had a well-defined fundamental theory which exhibited this behavior, one might take them seriously, but until they do, the whole picture is nothing more than an elaborate excuse for failure. A question that should be asked of anyone promoting this stuff: show us using string theory which of the Standard Model parameters are calculable and which are environmental. If they can't do this they shouldn't be taken seriously.

The second objection Sean attributes to the likes of me is that we object to the explanatory use of entities that are unobservable in principle, like multiple universes. This isn't really my objection to the Landscape. If a compelling fundamental theory existed that made lots of correct testable predictions, and such a theory predicted lots of unobservable universes, I'd happily believe in their existence. But, absent such a compelling theory, people who go on about unobservable multiple universes are not behaving very differently from those theologians who supposedly took an interest in angels and pins. Science is about coming up with explanations for the way the world works, explanations that can in principle be tested by making more observations of the world. If you've been working on a theory for twenty years and it has totally failed to make any testable predictions, you should admit failure and move on, not engage in elaborate apologetics for why your theory can't predict anything.

Comments

1. **D R Lunsford**
   October 20, 2004

   Thomas,

   Can your formalism be applied in a Weyl conformal space? The geometry is rather more natural than Riemann space because "metricity" is strictly local. The reducibility of the metric (9 direction cosines + volume element) is removed. I would expect the unit pseudoscalar to emerge prominently from the Clifford algebra of constraints in the correspondence you talk about. In the case that interests me (signature —+++), this looks like

   ```
   | iy5   0  |
   |  0  -iy5 |
   ```

   -drl

2. **D R Lunsford**
   October 20, 2004

   TL – rofl. That was classic.

3. **Thomas Larsson**
   October 20, 2004

   Urs,

   The Fock representations are constructed by first expressing the diffeomorphism generators in terms of canonically conjugate variables q and p, and then representing the q's and p's on a Fock space.

   In principle you can quantize in other ways, e.g. by looking at submodules of Verma modules or vertex operator algebras or Sugawara/coset constructions or whatever. For the Virasoro algebra this is equivalent, due to a quantum equivalence theorem which I think is due to David Olive.

   Look at the papers, for God's sake! 100 man-years of work by professional mathematicians is not necessarily invalided if a physics grad student doesn't immediately understand how to do things.

   I have posted a description of how to quantize in the covariant phase space on spr.

4. **Urs Schreiber**
   October 19, 2004

   Thomas –

   there is a misunderstanding here: The problem is not to find a representation of the constraint algebra alone. The problem is to find a representation of the embedding algebra of canonical data, i.e. the p and q. That of course is what gives rise to the divergences when the constraints are expressed in terms of these ‘p’ and ‘q’.

And that’s what makes the difference between quantizing a system on the one hand side or talking about abstract algebras on the other side.

5. **Thomas Larsson**
   October 19, 2004

   Urs,

   As far as I can see it is not known if there is any meaningful way in which the constraint algebra of pure GR in d=4 or higher could receive quantum corrections, since it seems to be impossible to find a weakly continuous representation of these constraints in terms of operators on some Hilbert space. No matter how the operator ordering is dealt with the commutators of the would-be constraint operators contain diverging quantities and are hence ill defined.

   The problem you point at here is of course the reason why people failed to generalize the Virasoro algebra to higher dimensions for 25 years. I found one of the extensions rather rapidly, but I was unable to find any interesting representations for seven years (1988-1995), precisely because I had to struggle with the problem you mention. In the end I didn’t solve it myself, but read about the solution in the Rao-Moody paper, and at that time I had already run out of funding. An irony is that Rao sent me a preprint of their paper in 1992, but I didn’t realize that they had solved the problem I faced.

6. **Thomas Larsson**
   October 19, 2004

   Ted,

   I think you would agree that the fact that a classical constraint algebra recieves quantum corrections does not mean that there is an anomaly in gauge symmetry.

   That is what I mean. But I could say that quantum gravity must have an “anomalous, global diffeomorphism symmetry” if that makes you happier. People say that the 2D Ising model at criticality has an “anomalous, global conformal symmetry”, but this is really only a game with words.

   In general, a true gauge anomaly is a bad thing, since unphysical (usually negative energy) modes do not decouple from the dynamics.

   The bad thing with chiral-fermion type gauge anomalies is lack of unitarity, which mathematically means a lack of unitary, lowest-energy representations. Virasoro type algebras have at least representations of lowest-energy type.

   Physically, what happens is this. The observer’s trajectory decouples classically; it will probably be a timelike geodesic but Einstein’s equations couldn’t care less. The quantum system cannot be observed without perturbing it, so these degrees of freedom no longer decouple. A classically unphysical mode has become physical upon quantization. Mathematically, this is crucial because the anomaly is a functional of the observer’s trajectory; first described in math-ph/9810003.

   I think the clearest way to understand whether gauge symmetry is anomalous is whether a quantum theory admits a nilpotent BRST charge. As you know, the worldsheet theory of a string does admit such a charge in the critical dimension, and in this sense gauge symmetry is not anomalous. Still, as you noted, the classical Virasoro algebra in the matter sector recieves a quantum correction, aka the central charge.
   As you mention, the constraint algebra of GR very probably recieves quantum corrections. However, is it known whether the theory admits a nilpotent BRST charge? If it does not, the theory is not gauge invariant at the quantum level and is probably not physically meaningful.

   There seems to be no BRST charge at all, nilpotent or not. Normal ordering gives rise to infinities. I disagree about physical meaningfulness, because I believe in locality. If general covariance remains a gauge symmetry after quantization, then there is no Hamiltonian, no energy, no time evolution, and no locality. This seems utterly unphysical to me.

   I don’t know of a calculation in string theory which clearly addresses the question you are asking, i.e. how the classical constraint algebra of GR is corrected quantum mechanically.

   I was not asking a question. Large classes of projective, lowest-energy representations of the diffemorphism algebra are known, cf. the references in my reply to Urs. They all satify the algebras described in physics/9705040 and math-ph/9810003. Besides, all possible extensions of the diffeomorphism algebra (by modules of tensor fields) were classified in

   A. Dzhumadildaev,
   Virasoro type Lie algebras and deformations,
   Z. Phys. C 72 (1996) 509–517.

   cf math-ph/0002016. The N-dimensional Virasoro extension are not quite tensor fields, but rather closed (N-1)-forms, but they are closely related to exact (N-1)-forms, and thus to (N-2)-forms. When N=1, a closed 0-form is a constant function is a central extension, but otherwise the extension is not central.

   My guess is you would have to consider this question in the framework of closed string field theory. However, there the gauge symmetry of 4-diffs is embedded in a complicated way in the huge gauge symmetry of closed strings, which includes not only gravitons but an infinite number of massive fields. Still—virtually by construction—closed string field theory is gauge invariant at both the classical and quantum levels. It is for the purpose of gauge invariance that the BV formalism is so useful for constructing the action of closed string fields.

   Why should string field theory be relevant for anything?

7. **Thomas Larsson**
   October 19, 2004

   Urs,

   As far as I can see it is not known if there is any meaningful way in which the constraint algebra of pure GR in d=4 or higher could receive quantum corrections, since it seems to be impossible to find a weakly continuous representation of these constraints in terms of operators on some Hilbert space. No matter how the operator ordering is dealt with the commutators of the would-be constraint operators contain diverging quantities and are hence ill defined.

   Sigh. The quantum representation theory the diffeomorphism algebra is equivalent to finding continuous representations of these constraints in terms of operators on some Hilbert space. The anomaly by itself is nothing, because it could easily lack representations; the Mickelson-Faddeev algebra, describing chiral-fermion anomalies, apparently does so. The important thing with the Virasoro algebra, in any dimension, is that it has a natural representation theory. Here are a couple of references:

   S. Berman and Y. Billig,
   Irreducible representations for toroidal Lie algebras,
   J. Algebra 221 (1999) 188–231.

S. Berman, Y. Billig and J. Szmigielski;
Vertex operator algebras and the representation theory of toroidal algebras,
math.QA/0101094 (2001)

Y. Billig,
Principal vertex operator representations for toroidal Lie algebras,
J. Math. Phys. 7 (1998) 3844–3864.

Y. Billig,
Energy-momentum tensor for the toroidal Lie algebras,
math.RT/0201313 (2002)

Y. Billig,
Weight modules over exp-polynomial Lie algebras,
math.RT/0305293 (2003)

T.A. Larsson,
Lowest-energy representations of non-centrally extended diffeomorphism algebras,
Comm. Math. Phys. 201 (1999) 461–470.
physics/9705040

T.A. Larsson,
Extended diffeomorphism algebras and trajectories in jet space.
Comm. Math. Phys. 214 (2000) 469–491.
math-ph/9810003

T.A. Larsson,
Extensions of diffeomorphism and current algebras,
math-ph/0002016 (2000)

T.A. Larsson,
Multi-dimensional diffeomorphism and current algebras from Virasoro and Kac-Moody Currents,
math-ph/0101007 (2001)

T.A. Larsson,
Koszul-Tate cohomology as lowest-energy modules of non-centrally extended diffeomorphism algebras,
math-ph/0210023 (2002)

R.V. Moody, S.E. Rao, and T. Yokonoma,
Toroidal Lie algebras and vertex representations,
Geom. Ded. 35 (1990) 283–307.

S.E. Rao, R.V. Moody, and T. Yokonuma,
Lie algebras and Weyl groups arising from vertex operator representations,
Nova J. of Algebra and Geometry 1 (1992) 15–57.

S.E. Rao and R.V. Moody,
Vertex representations for \$N\$-toroidal Lie algebras and a generalization of the Virasoro algebra,
Comm. Math. Phys. 159 (1994) 239–264.

OK, a caveat. The reps act on linear spaces rather than Hilbert spaces because of problems to define an inner product. The essential difficulty is finiteness, not unitarity, though. Finiteness is obtained by first expanding all fields in a Taylor series around the observer's trajectory (have I said that before?). This gives us a classical non-linear realization on **finitely many functions of a single variable**, which is precisely when normal ordering works. This is the crucial step which evades any no-go theorem that you can come up with.

LQG circumvents this on the technical level by dropping the assumption that quantization should use weakly continuous representations. For such reps no quantum correction is found. But dropping weak continuity means leaving the realm of well-established physics and in particular any relation to the path integral.

The lack of anomalies is a clear symptom of this.

I think with respect to Thomas Larsson's speculation that the existence of higher-dimensional Virasoro algebras with certain extensions implies anything about anomalies in gravitational theories it is important to note that the constraint algebra of any GR-like theory is not in general isomorphic (as a whole) to any diffeomorphism algebra, due to the special nature of the Hamiltonian constraint.

I have explained this several times. The Dirac algebra has the form it has because of the way we put coordinates on phase space. Some diffeos move us out of a fixed timeslice, and we must therefore add compensators to get back. 4-diffeos + compensators generate the Dirac algebra. But the physics cannot depend on our way to put coordinates on phase space. There is a covariant definition of phase space, as the space of histories. In this formulation, as in the Lagrangian formulation, the symmetries of GR generate precisely the 4-diff algebra.

But it does not really matter. Essentially I'm only saying this:
1. GR is a constrained Hamiltonian system with an infinite-dimensional constraint algebra.
2. Any reasonable quantum theory of gravity must have a classical limit with this property.
3. Infinite-dimensional constraint algebras generically acquire quantum corrections upon quantization. Conformal symmetry in string theory is only the simplest, and best known, example, but things do not miraculously getter simpler in higher dimensions.
Thus it must be possible to express anomalies in any reasonable quantum gravity theory, even if you think that they must be cancelled in the end. String theory does not satisfy this criterion.

The fact that in 2-dimensions (e.g. on the string's worldsheet) the canonical constraint algebra is isomorphic to two copies of lightlike 1-dimensional reparameterizations is due to the magic of conformal invariance in two dimensions and, as far as I am aware, does not have analogs in higher dimensions.

Of course not, and I never said that. What I do say is that because of this isomorphism, we can reinterpret CFT as diff-invariant QFT in 1D. Then generalize this to 4D.

Aware of that, Thomas Larsson speculated that his algebra might hence apply to the diffeo symmetry acting on the space of (classical) solutions to GR. I don't see what *this should mean in detail.*

math-ph/0210023

I find it interesting that and how higher-dimensional analogs of the Virasoro algebra exist, but it is far from clear that and how these play a role for quantum gravity. Cautious statements seem to be more in order that bold speculations.

Tell that to the guy at Princeton who talks about Theories of Everything and chants Magic and Mystery.

It is no speculation that the multi-dimensional Virasoro algebra is the *only* way to combine quantum theory, general covariance, and locality. People seem happy to state that there are no local observables in quantum gravity. Well, I'm not, and 't Hooft is even willing to introduce hidden variables to get locality. The lesson from CFT, regarded as a spacetime symmetry in 2D, is that locality is compatible with infinite-dimensional symmetries, but only in the presence of an anomaly. This is no speculation, but a well-known fact since 20 years, and it has been amply verified that nature behaves in this way in 2D condensed-matter systems.

8. **Urs Schreiber**
October 19, 2004

Ted Erler wrote (to Thomas Larsson):

> As you mention, the constraint algebra of GR very probably recieves quantum corrections. However, is it known whether the theory admits a nilpotent BRST charge? If it does not, the
> theory is not gauge invariant at the quantum level and is probably not physically meaningful.

As far as I can see it is not known if there is any meaningful way in which the constraint algebra of pure GR in d=4 or higher could receive quantum corrections, since it seems to be impossible to find a weakly continuous representation of these constraints in terms of operators on some Hilbert space. No matter how the operator ordering is dealt with the commutators of the would-be constraint operators contain diverging quantities and are hence ill defined.

LQG circumvents this on the technical level by dropping the assumption that quantization should use weakly continuous representations. For such reps no quantum correction is found. But dropping weak continuity means leaving the realm of well-established physics and in particular any relation to the path integral.

I think with respect to Thomas Larsson's speculation that the existence of higher-dimensional Virasoro algebras with certain extensions implies anything about anomalies in gravitational theories it is important to note that the constraint algebra of any GR-like theory is not in general isomorphic (as a whole) to any diffeomorphism algebra, due to the special nature of the Hamiltonian constraint. The fact that in 2-dimensions (e.g. on the string's worldsheet) the canonical constraint algebra is isomorphic to two copies of lightlike 1-dimensional reparameterizations is due to the magic of conformal invariance in two dimensions and, as far as I am aware, does not have analogs in higher dimensions.

Aware of that, Thomas Larsson speculated that his algebra might hence apply to the diffeo symmetry acting on the space of (classical) solutions to GR. I don't see what this should mean in detail.

I find it interesting that and how higher-dimensional analogs of the Virasoro algebra exist, but it is far from clear that and how these play a role for quantum gravity. Cautious statements seem to be more in order that bold speculations.

9. **Ted Erler**
October 18, 2004

Dear Thomas,
Let me see if I can clarify for myself what you're saying.

I think you would agree that the fact that a classical constraint
algebra recieves quantum corrections does not mean that there is an anomaly in gauge symmetry. In general, a true
gauge anomaly is a bad thing, since unphysical (usually negative
energy) modes do not decouple from the dynamics.

I think the clearest way to understand whether gauge symmetry
is anomalous is whether a quantum theory admits a nilpotent
BRST charge. As you know, the worldsheet theory of a string
does admit such a charge in the critical dimension, and in this
sense gauge symmetry is not anomalous. Still, as you noted,
the classical Virasoro algebra in the matter sector recieves a quantum correction, aka the central charge.

As you mention, the constraint algebra of GR very probably
recieves quantum corrections. However, is it known whether
the theory admits a nilpotent BRST charge? If it does not, the
theory is not gauge invariant at the quantum level and is
probably not physically meaningful.

I don't know of a calculation in string theory which clearly
addresses the question you are asking, i.e. how the classical
constraint algebra of GR is corrected quantum mechanically.
My guess is you would have to consider this question in the
framework of closed string field theory. However, there the
gauge symmetry of 4-diffs is embedded in a complicated way
in the huge gauge symmetry of closed strings, which includes
not only gravitons but an infinite number of massive fields.
Still—virtually by construction—closed string field theory is
gauge invariant at both the classical and quantum levels. It is for the purpose of gauge invariance that the BV formalism is so
useful for constructing the action of closed string fields.

–Ted

10. **D R Lunsford**
October 18, 2004

TL, thanks for an excellent answer. Yes, you did mention Ginsparg, thanks again.

-drl

11. **Thomas Larsson**
    October 18, 2004

    DRL,

    Do you have a good reference for anomalology? How is the "Dirac algebra of ADM (Arnowit-Deser-Misner?) constraints" equivalent to 4D diff? Is this a fancy way of saying there is an equivalent tetrad formalism and that the constraints become orthogonality and normalization conditions?

    In the Lagrangian formulation, GR is invariant under all 4-diffs. In the Hamiltonian formalism, you break this symmetry by specifying a timeslice where the canonical variables live. So the physical symmetries are 4-diffs, but some of them move you out of the chosen timeslice. Therefore you must add compensating transformations to get back to it, and the combination of 4-diffs and compensating transformations generate the Dirac algebra. But the compensating transformations don't really have anything to do with physics, only with our way of parametrizing the phase space. There is a covariant way to define phase space, namely as the space of solutions to the equations of motions, i.e. the histories. The constraint algebra in this covariant phase space are the 4-diffs, because here we don't need any compensation anymore.

    I find all this talk about constraints interesting but puzzling. I don't really understand this way of thinking. As far as I can tell, there really isn't a good Hamiltonian formulation of ordinary GR, that is, one with an indentifiable, unambiguous energy. When you say H, do you mean this in the more general sense of simply having a variational principle?

    The Hamiltonian constraint is probably the thing that I found most puzzling about GR. H = 0 seems to imply that there is no energy and no time. I now believe that this naive interpretation is indeed correct. The only quantum theories where a Hamiltonian constraint seems to be successfully implemented are various types of topological field theories a la Witten. Since the correlation functions in such theories are smooth invariants, they are indeed timeless.

    However, in the presence of an anomaly, the Hamiltonian ceases to be a constraint. One can no longer demand that diffeomorphisms annihilate the physical states, because that would mean that the anomaly, in the simplest case the unit operator, annihilates them. So further states become physical. In particular, one can now define the energy E of an eigenstate |psi> of the Hamiltonian by H|psi> = E|psi>.

    People might say that a diff anomaly means that things depend on the choice of coordinates. This is not true any more than for the Poincare algebra. There are three cases:
    1. No anomaly. The theory is then "coordinate free", in the sense that it can probably be formulated without coordinates altogether.
    2. An anomalous diff symmetry: The theory is then "coordinate independent". We must introduce some reference coordinate system, but our choice does not matter because everything is guaranteed to transform covariantly.
    3. No diff symmetry altogether. The theory can then only be formulated in a preferred coordinate system. This is not good.

    This line of thinking is very similar to how people think about conformal symmetry in statphys, I think. At least that is what shaped my worldview. In statphys, conformal symmetry is a spacetime (or spatial) symmetry, anomalies are always present, and the dilatation operator plays the role of the Hamiltonian. I think that I recommended Gispargs old reference before. Read that, imagine that he really talks about diffeomorphisms in 1D rather than conformal transformations in 2D, and generalize to 4D.

12. **D R Lunsford**
    October 18, 2004

    Thomas,

    Do you have a good reference for anomalology? How is the "Dirac algebra of ADM (Arnowit-Deser-Misner?) constraints" equivalent to 4D diff? Is this a fancy way of saying there is an equivalent tetrad formalism and that the constraints become orthogonality and normalization conditions?

    I find all this talk about constraints interesting but puzzling. I don't really understand this way of thinking. As far as I can tell, there really isn't a good Hamiltonian formulation of ordinary GR, that is, one with an indentifiable, unambiguous energy. When you say H, do you mean this in the more general sense of simply having a variational principle?

    -drl

13. **Thomas Larsson**
    October 17, 2004

    Good morning, Lubos.

    Let me come back to the part that you said that you don't understand, namely quantization of constrained Hamiltonian systems with an infinite-dimensional constraint algebra. The string worldsheet with its Weyl symmetry is an important toy model, because it is the simplest example of such a system.

    We quantize first and impose the constraints afterwards. However, if the constraint algebra is infinite-dimensional, quantization of the fields alone (before ghosts) will yield an anomaly because of normal-ordering effects. This is a quite generic feature of any infinite-dimensional constraint algebra. On the string worldsheet, the coordinate fields have an anomaly c = 26. The LQG string is not string theory and conformal transformations may not play a role there. However, there is still an infinite-dimensional constraint algebra, and that is what matters. The absense of an anomaly is a very clear symptom that we are dealing with quantization in some diluted sense of the word, as Urs Schreiber noted.

    General relativity can also be cast in the form of a constrained Hamiltonian system with an infinite-dimensional constraint algebra, namely 4D diffeomorphisms or the physically equivalent Dirac algebra of ADM constraints. Therefore, canonical quantization and normal ordering of the fields alone will inevitably give rise to a diff anomaly. This general feature does not depend on the details of the Einstein action; the only thing that matters is that the ADM constraints generate an infinite-dimensional algebra.

    Any theory which purports to be a quantum theory of gravity must be possible to cast in this form, string theory, LQG, or whatever. The only assumptions are a Hamiltonian formulation, general covariance, and that we do real quantization rather than some diluted version thereof. In this situation we do get a diff anomaly, at least intermediately. Perturbative string theory might not see this anomaly, since it is only a first-quantized theory; first-quantized particle theory does not see any anomalies at all. But if string field theory does not generate a diff anomaly when only the fields are taken into consideration, there is no way that it can be considered as a quantum theory of gravity, because it violates the most basic properties that such a theory must have.

    I don't need to know the details about string theory to say that. It is simply a fact that quantization of Hamiltonian system with an infinite-dimensional constraint algebra must introduce anomalies in the constraint algebra.

14. **Thomas Larsson**
    October 17, 2004

This is something that you have been unable to understand at least for 5 years, as far as I remember: the local symmetries of a theory are a redundancy in the description, and they depend on the specific description – and the local symmetries are usually necessary to make a description with too many new variables consistent. But the gauge symmetry is not "measurable" in any way. The measurable things are only those objects in the coset/quotient "description divided by the local symmetries and redundancies".

This is true classically, because classically there are no anomalies, so you can always write down a nilpotent BRST operator and pass to the reduced phase space. After quantization, you can still do that if there is no anomaly. With an anomaly, you can not, because some (or all) gauge degrees of freedom become physical. Instead, your classical gauge symmetry becomes an anomalous global quantum symmetry. Neither possibility is inconsistent. It is important to realize that the classical limit of an anomalous global conformal symmetry is a conformal gauge symmetry, because all anomalies vanish in the classical limit.

But only if quantum gravity has a global anomalous diffeomorphism symmetry rather than a gauge diff symmetry can we resolve the conceptual difficulties: we get a real Hamiltonian rather than a Hamiltonian constraint, real time evolution rather than a gauge transformation, a local definition of mass, etc.

Concerning the Ising model – the "local symmetry" is not the usual local symmetry that we talked about before (one that must keep the states invariant).

That *you* talked about.

Of course, if you use the Ising model as a model of classical statistical mechanics, the rules are very different – but jumping from string theory to classical mechanics is not a controllable approach to a discussion.

The rules of classical statistical mechanics are very different from QFT? That's bad news for the people doing lattice gauge theory, isn't it?

The generalization of the Virasoro algebra may exist in higher dimensions, but it does not have the properties to lead to a generalization of string theory.

I'm not interested in generalizing a complete scientific failure. The physically successful theories are general relativity, the standard model, and conformal field theory applied to 2D statphys. I'm applying insights from the third theory to the two first.

15. **Peter**
October 17, 2004

One of the main contributions to science in recent years from string theorists has been assigning new meanings for the term "prediction". When Lubos and other string enthusiasts say that "string theory makes predictions", in old-style lingo this means roughly "if everything we would like to be true about string theory really is, then in principle one can use it to make predictions".

The Landscape crowd has yet another new-fangled notion of prediction. If there's an experimental upper bound on an observable, you say that the Landscape "predicts" that the observable will be some number less than the upper bound. See for example the many references to Weinberg's successful "prediction" of the cosmological constant.

16. **Matthew**
October 17, 2004

On the contrary, string theory predicts everything – its character certainly makes it the most predictive theory one can imagine.

Hi Lubos,

Pray tell, in light of your above comment, what is the "string theory" prediction for the SUSY breaking scale? Say accurate to 1%? Please make that a first principles prediction, starting with one of the five superstring theories, or better with "M-Theory", not some "string inspired" model.

Until you can actually *predict* parameters like that (or retrodict the parameters of the standard model), don't you think it's a tad bit early to be calling string theory "the most predictive theory one can imagine"?

17. **Lubos Motl**
October 17, 2004

That's right, Thomas. I don't understand the difference between being unable to write the anomaly in the first place, and having a theory in which the anomaly is guaranteed to cancel.

Conformal symmetry is a local symmetry obtained by switching from the Nambu-Goto action to the Polyakov action; the latter has new (auxilliary) degrees of freedom (the worldsheet metric), and they must be unphysical, which is guaranteed by the diff and Weyl symmetry which must therefore be unbroken. The residual symmetry from diff x Weyl is the conformal symmetry, and if it is broken by the quantum effects, it proves that diff x Weyl was broken, too.

But there is nothing wrong with theories that don't allow you to write a conformal anomaly in the first place. There are many descriptions of physics of string theory that do not require any 2D conformal symmetries – such as those from AdS/CFT – and they are equally consistent.

This is something that you have been unable to understand at least for 5 years, as far as I remember: the local symmetries of a theory are a redundancy in the description, and they depend on the specific description – and the local symmetries are usually necessary to make a description with too many new variables consistent. But the gauge symmetry is not "measurable" in any way. The measurable things are only those objects in the coset/quotient "description divided by the local symmetries and redundancies". All the physical states must be invariant under local symmetries, for example – they transform as the singlets. This makes the representation theory of local symmetries physically irrelevant – all these things are just auxilliary concepts that we encounter in the middle of the calculation, but that disappear in the final results.

If you criticize LQG for having no conformal symmetry, it is not a fair criticism. It is not only a criticism that "it is not string theory" – it would be a wrong criticism even in string theory because there are many (nonperturbative) descriptions of the same physics that do not rely on conformal symmetry.

Concerning the Ising model – the "local symmetry" is not the usual local symmetry that we talked about before (one that must keep the states invariant). If it were – e.g. if you use the Ising model as a part of string theory worldsheet action – then the total theory MUST cancel all anomalies in the conformal symmetry. Of course, if you use the Ising model as a model of classical statistical mechanics, the rules are very different – but jumping from string theory to classical mechanics is not a controllable approach to a discussion.

The generalization of the Virasoro algebra may exist in higher dimensions, but it does not have the properties to lead to a generalization of string theory. The direct generalization of string theory to higher-dimensional fundamental objects simply does not work because of hundreds of reasons, and if you still think that it is a straightforward thing to make a theory based on higher-dimensional replacements for strings, it proves that your knowledge these matters is highly superficial.

18. **Thomas Larsson**

October 17, 2004

String theory backgrounds do not have any anomalies in local symmetries. They don't have a conformal anomaly on the worldsheet. String theory is only called string theory if the perturbative portion of it is anomaly-free conformal field theory. Bosonic string theory in flat spacetime whose dimension differs from 26 is not a string theory.

You don't seem to understand the difference between writing down an anomaly and then cancelling it out, and being unable to write down the anomaly in the first place. LQG cannot write down the conformal anomaly in the first place. But neither LQG nor string theory can write down the diff anomalies in 4D, which exist mathematically (multi-dimensional Virasoro algebra), and must be present physically to ensure locality.

It's not clear to me why you want to extrapolate 2D conformal anomaly to higher (four) dimensions. It seems to me that you are confusing the worldsheet and the spacetime.

2D conformal transformations are isomorphic to (twice) 1D diffeomorphims. A general-covariant theory transforms covariantly under diffeomorphisms. I generalize this property from 1D to 4D.

The rest of your text is comparably weird. You seem to be looking for a theory that has an anomaly. Why do you want it? A theory with an anomaly in local symmetries is not really a theory, it is a garbage.

A local symmetry can very well have an anomaly, provided that you don't try to impose a physical state condition. It is then no longer a gauge symmetry on the quantum level. The prime example is the application of CFT to 2D phase transitions. E.g., the 2D Ising model has local conformal symmetry with an anomaly (c = 1/2), but it is perfectly consistent: it is unitary, and *realized in nature*. Ultimately, that's what physics is about. It is sad that you don't understand that.

But it is not a FLAW, as you seem to be saying, it is one of the critical virtues of string theory. Sorry, there may be someone else who will see something coherent in your text, but it does not make any sense to me.

Some people seem to think that locality is worth many sacrifices – 't Hooft even seems to be willing to give up quantum mechanics to get it. However, hidden variables are not necessary. You can combine quantum theory, general covariance, and locality, but only with a diff anomaly.

At any rate, it is easy to verify that a generalization of the Virasoro algebra exists in any dimension. It is thus a diff anomaly, something that Weinberg claims does not exist in 4D. Missing an anomaly is a gross oversight. Your reaction is in fact very LQGish: "I don't like this anomaly, hence it does not exist". It is good that you reveal your LQG mentality.

19. **Lubo? Motl**
October 17, 2004

Sorry, "Peter" should be "Thomas" in the previous post.

20. **Lubos Motl**
October 17, 2004

I was trying hard, but it's still not comprehensible to me what is the "missing anomaly syndrome".

Let me try to explain you a couple of elementary points, Peter.

String theory backgrounds do not have any anomalies in local symmetries. They don't have a conformal anomaly on the worldsheet. String theory is only called string theory if the perturbative portion of it is anomaly-free conformal field theory. Bosonic string theory in flat spacetime whose dimension differs from 26 is not a string theory.

It's not clear to me why you want to extrapolate 2D conformal anomaly to higher (four) dimensions. It seems to me that you are confusing the worldsheet and the spacetime. It's the whole point of perturbative string theory that the objects must be one-dimensional (therefore strings), and therefore the worldvolume is 2-dimensional (worldsheet).

Virtually nothing in perturbative string theory can be generalized to higher dimensions of the object. String theory has higher-dimensional objects, but they are never as fundamental as the string. String is the only object that can generate a consistent spacetime theory, and all the magic of 2D conformal symmetry and complex calculus is essential.

The rest of your text is comparably weird. You seem to be looking for a theory that has an anomaly. Why do you want it? A theory with an anomaly in local symmetries is not really a theory, it is a garbage. It is the reason why many things that we can write down are nonsensical at quantum level. Do you really want string theory to reproduce this garbage? The purpose of string theory is certainly NOT to reproduce any idiotic inconsistent pseudo-theory that someone invents. Indeed, many problems and inconsistencies are solved AUTOMATICALLY by string theory's basic rules. The framework just does not allow us to write down backgrounds that would have some kinds of problems that can appear in other theories.

But it is not a FLAW, as you seem to be saying, it is one of the critical virtues of string theory. Sorry, there may be someone else who will see something coherent in your text, but it does not make any sense to me.

21. **Thomas Larsson**
October 17, 2004

Lubos,
String theory suffers from a missing anomaly syndrome, very similar to Loop quantum gravity. It follows from the following observations:
1. In quantization of the string, a conformal anomaly generically arises. In the special case of c = 26, it can be cancelled against ghosts, but generically it is there, also when c = 26 before cancellation. You know this.
2. Things don't get simpler in higher dimensions. Canonical quantization gave rise to an anomaly already in 1+1D, so canonical quantization of a diff-invariant theory in 3+1D will also give rise to an anomaly. You realize this, because you are not foolish enough to believe that the anomaly will miraculously disappear in higher dimensions.
3. There are no diff anomalies in field or string theory in 4D. You know this (or if you don't know it, check out Weinberg's second book, chapter 22.

Taken together, this implies that canonical quantization in 4D must give rise to a diff anomaly which isn't present in string theory. This can mean one of two things
1. No second-quantized Hamiltonian formulation of string theory exists.
2. A second-quantized Hamiltonian formulation of string theory exists and is anomalous, which is inconsistent with the path-integral formalism.

Either way, this must mean that string theory is a very pathological theory. A missing anomaly is a very serious sickness in a theory. It was fatal for LQG, and it is presumably fatal for string theory as well.

22. **Fran?ois Belfort**
October 17, 2004

I read a book once where it was said that also pigs can say that the universe is made for them – thus one should call it the “porcine principle”.

An anthropic or a porcine principle is not a theory – it just says that the prediction must come out as observed. It is a test of a theory, or a test for consistency among observations.

Also talking about the existence of things that cannot be observed is impossible; “existence” is *defined* as what can be observed.

FB

23. **Lubos Motl**
October 16, 2004

Sean, I would probably disagree with your interpretation of Peter’s statement.

You say that he says that string theory is not a solid framework to include the anthropic reasoning. Even though I think that string theorists should avoid anthropic reasoning, it’s just not true that string theory does not allow you to put anthropic reasoning on firm ground.

On the contrary, it is the only known theory that offers a scientific encapsulation for all these ideas. In order to treat anthropic principle scientifically, you need a large number of Universes – solutions – and the existence of dynamical mechanisms to get from one to another, and so forth. This is possible in string theory.

You also need to assign some measures – probabilities of different Universes. This is of course the whole problem of the anthropic reasoning – what is a “generic” Universe? But if one accepts the highly controversial assumptions that each stringy vacuum that admits basic life-like things has the same weight, then it’s just true that string theory – with its discrete ensemble of vacua – gives you a more or less rigorous realization of this anthropic counting.

Once again, this counting seems unscientific to me by its very basic properties, because the “less predictive” vacua are favored – but nevertheless it seems clear to me that one can play this game.

24. **Lubos Motl**
October 16, 2004

Just like I agree with Peter’s algorithm to decide whether a scientific theory is nontrivially predictive and interesting, it seems to me that he does not quite understand some subtle issues about string theory.

First of all, it is not true that you can’t calculate vacuum energy in string theory.

In the Standard Model, which is a non-gravitational theory, the sum of the vacuum diagrams does not really matter because the vacuum energy has no effects in a non-gravitational theory.

However, if you couple such a theory to gravity, the vacuum energy does matter, because it curves the space. However, in quantum field theory, you can always add a counter-term and adjust your vacuum energy to whatever you want. You need fine-tuning, but you can always adjust such “constants” in quantum field theory.

That’s not the case of string theory. Here, you can really calculate vacuum energy, and in the simplest models, you obtain a far too huge value of the cosmological constant – assuming that some serious bug about our understanding of SUSY breaking don’t invalidate the whole conclusion.

In this sense, the CC problem in string theory is or was more serious because string theory is a very rigid theory that does not allow you to mess with the parameters.

Well, the anthropic industry in string theory is more or less meant to put the CC constant in string theory to a comparable level to the CC in field theory. The freedom to continuously fine-tune the vacuum energy in field theory is replaced by the large number of vacua in string theory – and you can really see that some of them are more or less guaranteed to predict a qualitatively correct value of the C.C.

You know, I am among those who believe that we don’t quite understand this counting, especially after SUSY breaking, properly, but the anthropic people will disagree. Shamit Kachru et al. will tell you how to calculate all possible contributions to the potential, and he will argue that nothing is neglected and all approximations they make are justified.

They will tell you that they have a full control over the class of the KKLT vacua (that was elaborated by Mike Douglas and his collaborators and others), and this full control allows them to state quite certainly that string theory *does* predict a large number of vacua – even those controllable ones form large classes.

Once again, Peter, you are absolutely wrong if you think that string theory’s nature is its inability to predict. On the contrary, string theory predicts everything – its character certainly makes it the most predictive theory one can imagine. In fact, the appearance of the landscape in string theory is a consequence of its strong predictive power – because some people just became convinced that a choice of one of a few “simple” and “natural” enough vacua is more or less guaranteed to predict an *incorrect* cosmological constant. This is why people start to propose the convoluted vacua that can give you, more or less by chance, a realistic C.C., too.

It’s wrong to think that string theory is less predictive than field theory. Even if you imagined that string theory could give you virtually any field theory at low energies, it is still *more* predictive because it is a UV complete theory containing quantum gravity – something impossible in quantum field theory. Its low energy physics may have many types, and in this sense the “Landscape” of stringy vacua is analogous to the “Landscape” of quantum field theories – with the difference that the landscape of QFTs has continuous parameters, while string theory only has discrete ones.

Best wishes
Lubos

25. **Sean**
October 16, 2004

Peter, your objections don’t seem to be to the anthropic principle, but to the claim that the string theory landscape provides a sensible framework in which the anthropic principle can be implemented. And I would agree, at least in terms of the current state of the art. Our disagreement is that I am quite optimistic about the prospects for string theory in the future, so I think it’s well worth pursuing. But right now we don’t understand nearly enough to go around using the landscape idea to make predictions.

Interview with Atiyah and Singer

October 17, 2004
Categories: Uncategorized

There's a fascinating interview with Atiyah and Singer now on-line. It was conducted in May at the time they were awarded the Abel prize. The interview and Atiyah and Singer's acceptance speeches are also available in video form.

The whole interview is very much worth reading and both Atiyah and Singer make extensive comments about the relation of mathematics and physics. Atiyah makes the provocative prediction that ideas from quantum theory will ultimately have a revolutionary effect on number theory, helping to understand why the Riemann hypothesis or Langlands conjectures are true. He notes that Wiles says this is nonsense. He also predicts that new progress in theoretical physics will come from a better understanding of classical four-dimensional geometry. By this I think he has in mind something like twistor methods. Singer's comments about string theory are probably typical of the attitude of many mathematicians. He says that, because of the Landscape "you cannot expect to make predictions from string theory. Its inital promise has not been fulfilled", but he still is an "enthusiastic supporter of superstring theory", largely because of the interesting mathematics it leads to.

Singer also makes the following sociological comment about mathematics, but I think what he has to say is also very true in physics:

"I observe a trend towards early specialization driven by economic considerations. You must show early promise to get good letters of recommendations to get good first jobs. You can't afford to branch out until you have established yourself and have a secure position. The realities of life force a narrowness in perspective that is not inherent to mathematics. We can counter too much specialization with new resources that would give young people more freedom than they presently have, freedom to explore mathematics more broadly, or to explore connections with other subjects, like biology these day where there is lots to be discovered.

When I was young the job market was good. It was important to be at a major university but you could still prosper at a smaller one. I am distressed by the coercive effect of today's job market. Young mathematicians should have the freedom of choice we had when we were young."

Comments

1. **sol**
   October 22, 2004

   Chris W.,

   I appreciate the link below you gave On the numbers.

   You know what is interesting to me, is how any number system could have began?

   If probabilistics determinations rules our lives, then what said that the Pinball drop could may have defined how life could have manifested, in the number sequence of this flower, and the number of it's petals?:)

   Pascal's Triangle

   If Ramanujan modulars functions can well serve to explain the string's world sheet, then how much more abstract are we going to get, if we wanted to apply some other kind of math to this function. Etc. Etc. Etc:)

   Quite early in my playing around with numbers, I was quite surprise to see how the Ancients used these numbers, as told by Manjul Bhargava. I seem to have a certainty affinity to rythmns, as well as the sequences describe by Manjul, may also be found in Pascal's triangle.

   The movie PI has some weird ideas here, but may not be so weird when considered in context of what rythmns are found, as patterns in life?

   To be caution for sure the slight's given to the Indian influence that Lubos warn's, John Baez, also gives us this in the link following the pinball at is source.

2. **Chris W.**
   October 20, 2004

   Speaking of Andrew Wiles (mentioned briefly in this post), a couple of days ago NPR did a profile of one of his recent advisees, now a full professor at Princeton at age 28.

3. **sol**
   October 18, 2004

   Sorry, this is the link I meant you to have as well

4. **sol**
   October 18, 2004

   Then I am sure you would like to see the issues on
   cosmic clumping and what is being done here in the latest research with Max Tegmark.

   The pics are direct links.

   The ultimate geometry would have been Martin Rees snake biting it's tail:), in the unification of the small with the very large, that we have psychologically induced reform. The big gumball(that's what my wife calls it) that you find in the links by Tegmark, as very revealling.

   I joined Andrey Kravtsov's models, to Tegmarks.

5. **D R Lunsford**
   October 18, 2004

   This is so much fun I thought I'd mention it here:

   http://www.hep.upenn.edu/~max/toe.pdf

You might call this “hospitality theory” since its basic rules are 1) we need self-aware systems, so be kind to them 2) all consistent systems that are compatible with SASes are physically real!

I just love Fig. 7, although I disagree with his green zone injunction 🙂

-drl

6. **sol**
October 18, 2004

Klein`s Ordering of the Geometries

“A theorem which is valid for a geometry in this sequence is automatically valid for the ones that follow. The theorems of projective geometry are automatically valid theorems of Euclidean
geometry. We say that topological geometry is more abstract than projective geometry which is turn is more abstract than Euclidean geometry.”

http://www.ensc.sfu.ca/people/grad/brassard/personal/THESIS/node21.html

7. **D R Lunsford**
October 18, 2004

Peter,

Of course some of Klein’s work (theory of the top, automorphic functions, icoashedron) relates number theory (implicitly) to spinors – and this was ages ago. See for example

http://store.yahoo.net/doverpublications/0486495280.html

-drl

8. **sol**
October 18, 2004

“Mathematics is always a continuum, linked to its history, the past – nothing comes out of zero” **Atiyah**

I found this very statement revealing.

One could not of denied any mathematical interpretation that would have arisen in theory, that could have postulated some emergent property out of string theory? Is this statement valid?

I thought I would challenged any mathematician then to discount the validation of string theory, if it did not emerge from some mathematical interpretaion, how it could not have been considered?

9. **Thomas Larsson**
October 17, 2004

Between 1870 and 1970, the number of freshly minted physics PhDs in the US rose from 1 to 10,000 anually. Today we have a steady-state situation, where each advisor can expect only one of her students to become an advisor, on the average. This is nothing to compain about. Exponential growth cannot be sustained forever, and the number of positions in academia has probably saturated.

10. **Peter**
October 17, 2004

Hi Lubos,

One sociological fact that Singer was referring to was that in the late fifties and early sixties, due to the huge expansion in the size of American universities, there were lots of jobs to go around. Since the competition for jobs was much less stiff, people could get away with being less focused on getting results quickly, so could take the time to learn about different fields and not stay so specialized.

I’ll go out on a limb and make a more specific conjecture along the lines of Atiyah’s comments about number theory. Maybe this is the kind of thing Atiyah had in mind, maybe not. From one point of view, the central object in number theory is the absolute Galois group of the field of rational numbers, and the study of its representations. Langlands theory relates these to other sorts of representations (“automorphic representations”). One point of view on particle theory is that central objects are the gauge and diffeomorphism groups and their representations. There are tantalizing analogies between these groups and the ones that appear in number theory. Perhaps new ideas about these representations coming out of a QFT framework may give new ideas about how to study the representations that occur in number theory.

11. **Peter**
October 17, 2004

I’m guessing Brian was referring to the “large extra dimensions” scenarios where there are effects that in principle could show up at the LHC when it starts collecting data in 2007-8. Here the idea is that some of the extra dimensions besides the four we know about are not all curled up and unobservably small, but instead we live on a 4d subspace of some higher dimensional space. One might see effects of this as energy disappearing from our 4d world as it moves into the other dimensions.

Most string theorists don’t actually seem to think that these scenarios are at all likely, but they do pull them out when they want to hold out hope that there will some day be experimental results relevant to string theory.

12. **Matti Pitkanen**
October 17, 2004

I find resonance with Atyiah’s vision about connections between number theory and physics.

Topological Geometrodynamics allows a formulation as what might be called a generalized number theory. p-Adic number fields and the requirement that real theory allows algebraic continuation to various p-adic number fields is an extremely powerful constraint.
The notion of infinite primes is second powerful notion and very physical: their construction is structurally equivalent to a repeated second quantization of a super-symmetric arithemetic quantum field theory with states labelled by primes.

TGD has inspired also a proposal for a proof of Riemann hypothesis based on a very simple conformally invariant dynamical system having zeros of Zeta as conformal weights. Rieman Zeta defines overlaps for general coherent states labelled by conformal weights z which are zeros z=1/2+iy of Riemann Zeta. Rieman hypothesis follows from the absence of state with negative norm (which corresponds to z=0).

M. Pitkanen (2002), A Strategy for Proving Riemann Hypothesis, math\@arXiv.org/0111262.

M. Pitkanen (2003), A Strategy for Proving Riemann Hypothesis, Acta Math. Univ. Comeniae, vol. 72.

The zeros of Zeta and their certain combinations appear also as conformal weights of super-canonical algebra playing together with Super Kac-Moody algebra a key role in TGD. Hermiticity requirement implies what I call conformal confinement: net conformal weights are real for physical states. For instance, quarks and gluons could have complex conformal weights and color confinement could reduce to conformal confinement.

For details see for instance

http://wwww.physics.helsinki.fi/~matpitka/tgd.html#mless .

http://wwww.physics.helsinki.fi/~matpitka/tgd.html#number .

http://wwww.physics.helsinki.fi/~matpitka/padtgd.html#mass1 .

Matti Pitkanen

13. **Rafael**
October 17, 2004

Your colleague Brian Greene came to Portland to talk about his new book. It was an interesting talk which he finished with a couple of statements. One of them was that there was going to be an experiment in the near future that could substantiate string theory because it predicts that under this experiment “energy would disappear.” No one asked him if String Theory was even still valid.

Do you know any more details about this experiment?

Thanks!

14. **Lubos Motl**
October 17, 2004

That’s very interesting.

Well, I don’t quite see the particular connections between various fields of mathematics and physics that Atiyah envisions, but he certainly has some reasons to talk about them. 😉 I might propose similar, but different connections, and no one else would understand me. 🙂

I would expect Atiyah to have more visions about higher-dimensional (e.g. seven-dimensional) geometry etc.

Concerning overspecialization, it’s of course a wrong tendency, but I don’t think that the change is due to a different social environment. People are just deciding differently today than they were deciding when Singer was getting started.

In my opinion it has two main (and related) reasons:

* there is a general (and probably true) feeling that the obvious “big questions” have been solved, and therefore people must attack smaller, more specialized questions

* people in average are less ambitious today than they were a few decades ago, and a bigger fraction of the talented youth with big goals is eaten by the commercial sector and similar enterprises

It’s not quite clear to me whether it is better if the good people try to concentrate at the top universities, or they spread all over the world (or at least over the country). Concentration may help communication, but of course the “second class” universities suffer.

Grothendieck and Motives

October 19, 2004
Categories: Uncategorized

The latest issue of the Notices of the AMS contains several things very much worth reading. There's the second part of a wonderful biographical article about Grothendieck written by Allyn Jackson (for some comments about the first part, see an earlier posting).

There's also an excellent short expository piece by Barry Mazur that explains a bit about one of Grothendieck's influential and still only partially understood ideas, that of a "motive". In algebraic geometry the standard ways of defining topological invariants of topological spaces are of limited use, and one wants a much more algebraic notion of such an invariant. This is what a motive is supposed to somehow provide, but to even show that such conjectural motives have the properties one would like requires solving perhaps the biggest open problem in algebraic geometry, the Hodge Conjecture.

Finally there's a thought-provoking piece called The Elephant in the Internet by Daniel Biss about the effect of the internet on the mathematics literature. It contains some comments about the difference between standards in physics and mathematics, including an analogy of mathematics as classical and physics as popular music. His conclusion that "our current relationship to the Internet has the undeniable effect of degrading the sacrosanct status of the mathematical text" seems to me excessive and it's a shame that he feels "hesitant to post my papers online; it always feels a little like leaving my infant in a dumpster." I have some sympathy for his worry that preprint archives and contact with the more journalistic physics literature may make the mathematics literature much less authoritative than it used to be (this was also the concern of a similar article by Jaffe and Quinn published in the AMS Bulletin in 1993). But the lost golden age that Biss yearns for was not so golden. Much of the math literature was written to very high standards of rigor, but often in ways that made such uncompromising demands on the reader that virtually no one who was not already an expert could hope to understand what was being said. The fact that the internet has provided venues for much sloppier, unpolished, but more expository articles also has its very positive aspects.

Comments

1. **sol**
   October 21, 2004

   Just trying to keep pace Thomas Larsson:)

   *As physicists know, "accidents: do not appear without a reason. When performing a long and difficult calculation, and then suddenly having thousands of unwanted terms miraculously add up to zero, physicists know that this does not happen without a deeper, underlying reason. Today, physicists know that these "accidents" are an indication that a symmetry is at work. For strings, the symmetry is called **conformal symmetry**, the symmetry of stretching and deforming the string's world sheet*

   Hyperspace, by Michio Kaku, Page 173

2. **Chris W.**
   October 21, 2004

   John Moffat posted a preprint in August written along somewhat similar lines, although the tone of his introductory discussion was unmistakably negative — along the lines of "I'm not sure it's worth it, but let's take this idea [the multiverse] seriously and see what we can do with it." It's worth reading as a review.

3. **Thomas Larsson**
   October 21, 2004

   Here we go again. This time it's Dine.

4. **sol**
   October 20, 2004

   Since demystification is the desire, then what do we realize when math generates these principles that espouse new theories?

   Why 10 Dimensions?

   When strings vibrate in space-time, they are described by a mathematical function called the Ramanujan modular function.26 This term appears in the equation:27

   [1-(D – 2)/24]
   where D is the dimensionality of the space in which the strings vibrate. In order to obey special relativity 9and manifest co-variance), this term must equal 0, which forces D to be 26. This is the origin of the 26 dimensions in the original string theory.

   In the more general Ramanujan modular function, which is used in current superstring theories, the twenty-four is replaced by the number eight, making D equal to 10.28

   *In other words, the mathematics require space-time to have 10 dimensions in order for the string theory to be self-consistent, **but physicists** still don?t know why these particular numbers have been selected.*

   http://www.ecf.utoronto.ca/~quanv/String/string9.html

5. **sol**
   October 20, 2004

   In La Clef des Songes he (alexander-grothendieck)explains how the reality of dreams convinced him of God's existence.

   Do we see some pattern here when we consider Ramanujan's Modular Functions and the source from which these numbers were pulled? As if from some fifth dimensional realization(abstract realizations)?

   A recent author has suggested that math ability derives from the brain abilities used in social understanding. Think of living in a tribe or small town where "everybody knows everybody". By growing up in such an environment you know not only everyone else's name, but their preferences and personal characteristics. You are freely able to think what so-and-so and such-and-such would talk about if they had a conversation. And it is proposed that mathematicians have this same ability, only with the abstract things they think about and discuss, rather than people.

   http://superstringtheory.com/forum/metaboard/messages18/187.html

Maybe Peter, I could then have convoluted the math realm with further ideas here as expressed in the Elephant link.

Sometimes it appears as mysterious? But really, when you look at how a "Cab number" could have been calculated, one wonder 's what nonsense mathematicians could raise, without recognizing the consistancy of this math that comes into existance? 🙂

Witten Colloquium

October 21, 2004
Categories: Uncategorized

I was down in Princeton today and went to hear Witten's physics department colloquium on the topic of "Supersymmetry: Pro or Con". He spent most of the hour going over the 25 year-old hierarchy argument for supersymmetry (that supersymmetry provides a reason for the Higgs to be much lighter than the Planck scale, since it is paired with a fermion whose mass can be protected by an approximate chiral symmetry).

He gave the following arguments for believing in GUTs:

1. Can naturally get small neutrino masses via the see-saw mechanism.
2. Coupling constant unification to 1%
3. Tentative evidence from CMB that fluctuations come from GUT scale.

Actually none of these seem to me very convincing (and to claim 1% coupling constant unification I think he has to use 1-loop results, at 2-loops it is more like 5-10% off, but this may depend on exactly what you are comparing to what).

His points in favor of supersymmetry were:

1. Solves hierarchy problem.
2. Coupling constant unification again.
3. Prediction of top mass from supersymmetric SO(10) GUT.
4. Supersymmetry is consistent with all accelerator data.
5. Lowest mass superpartner a good candidate for dark matter.
6. Part of string theory.

Again none of these are really convincing. If you don't believe in GUTs, the GUT scale is irrelevant, and since we don't understand quantum gravity, the significance of the Planck scale is also unclear. I'm no expert on supersymmetric GUT "predictions", but they seem to depend on lots of choices for the details of the GUT, how its symmetry breaks, and how fermions get masses from the symmetry breaking. Saying that supersymmetry is consistent with all accelerator data is kind of strange since the standard model without supersymmetry is consistent with all accelerator data and there is no evidence for supersymmetry. You can guess what I think of his last argument.

His points against supersymmetry were:

1. The Higgs mass bound is already embarassingly high, need some fine-tuning to get a Higgs that massive in a supersymmetric theory.
2. Supersymmetry spoils many of the experimental successes of the standard model since it generically has experimentally disallowed amounts of violation of CP, baryon and lepton number conservation, flavor-changing neutral currents.
3. No good picture of how to break supersymmetry.

Well, for me the con has it over the pro, but Witten still seems to hold out hope that supersymmetry will be found at the LHC. At the end of his talk, he discussed what he called the "worst case scenario"; that LHC sees a Higgs particle, but nothing else: no supersymmetry, no technicolor, no Little Higgs, no extra dimensions. He said that if this happens people will look for anthropic explanations of the hierarchy problem, whereas if the LHC found something that explained the hierarchy problem, they might be encouraged to look again for non-anthropic answers to the cosmological constant problem (which he claimed was analogous to the hierarchy problem). He did say "I hope it is wrong" about the anthropic explanation of the cosmological constant.

On the anthropic front, Michael Dine is claiming that maybe the statistical analysis of the landscape will "predict" that supersymmetry breaking is at a low energy scale. The arguments he gives sound to me like a complete joke, and from what I remember Michael Douglas was recently claiming that the same kind of analysis indicated that supersymmetry was broken at a high energy scale. One other funny thing about Dine: he doesn't say that the landscape makes predictions, but that it is "the first predictive framework we have encountered". This is a guy who for nearly twenty years has been giving talks on "superstring phenomenology" and claiming that any day now string theory would make predictions. I wonder why in all of those previous talks he neglected to mention that not only were there no predictions from string theory, there wasn't even a "predictive framework".

Comments

1. **Chris W.**
   October 28, 2004

   A new paper by Sean Carroll and Jennifer Chen deals with some of the same issues, and cites both DKS and Albrecht and Sorbo:

   Spontaneous Inflation and the Origin of the Arrow of Time (hep-th/0410270)

2. **Chris W.**
   October 25, 2004

   The authors of hep-th/0410213 acknowledge helpful input from Andreas Albrecht, who posted the following interesting preprint in May:

   Can the universe afford inflation?
   hep-th/0405270

   > Cosmic inflation is envisioned as the "most likely" start for the observed universe. To give substance to this claim, a framework is needed in which inflation can compete with other scenarios and the relative likelihood of all scenarios can be quantified. The most concrete scheme to date for performing such a comparison [due to Dyson, Kleban, and Susskind (DKS)] shows inflation to be strongly disfavored. We analyze the source of this failure for inflation and present an alternative calculation, based on more traditional semiclassical methods, that results in inflation being exponentially favored. We argue that reconciling the two contrasting approaches presents interesting fundamental challenges, and is likely to have a major impact on ideas about the early universe.

3. **Arun**
   October 25, 2004

   http://arxiv.org/PS_cache/hep-ph/pdf/0307/0307049.pdf

It has been shown that a SM electroweak fit including the anomalous magnetic moment of the muon a? and the branching ratio Br(b ! Xs
) yields a probability of about 5%. The total □2 is improved in the MSSM,
mainly because of a?, but the probability does increase only marginally due to the larger number of free parameters in the MSSM. However, in both cases the 3 □ discrepancy in sin2 □W from Ab
FB and ALR is
contributing to the low probability. Since at present no arguments to doubt any of the measurements can be found, we tested the Particle Data Group?s procedure to rescale the errors of these two measurements by the corresponding pulls. This yields considerably improved □2 values, both in the SM and MSSM, without significantly changing the fitted parameters.

4. **Chris W.**
October 25, 2004

Serenus: I should have remembered your comment and checked the URLs in it before I posted. You beat me to the punch on hep-th/0410213.

5. **Thomas Larsson**
October 25, 2004

It is strange that nobody seems to know about the 2-loop calculation. Maybe it is just a rumor. Maybe Witten didn't mention it because it isn't true, or because it is true but he doesn't know about it.

The relative error depends on how you parametrize things, but the comparison between the 1- and 2-loop results does not. If x = 1 + e and y = x^7, then y = 1 + 7e, so the error in y is 7 times bigger than the error in x. However, that the 2-loop error is ten times as big as the 1-loop error (if that is true) does not depend on parametrization, since (7e')/(7e) = e'/e.

6. **Thomas Larsson**
October 25, 2004

Lubos,

That the two-loop calculation was off by 10% is something that Aaron Bergman claimed in this forum – at least I, and presumably everybody else, interpreted him in that way. Maybe you should check with him.

7. **Chris W.**
October 24, 2004

See this:
Birth of the Universe from the Landscape of String Theory
(4 pages, posted 10/20/2004):

> "We show that a unique, most probable and stable solution for the wavefunction of the universe, with a very small cosmological constant …, can be predicted from the supersymmetric minisuperspace with N vacua, of the landscape of string theory without referring to the anthropic principle. …."

..from the abstract, with correction of typos. (The authors are at UNC-Chapel Hill; Laura Mersini-Houghton is an assistant professor there.)

String theory as such has little to do with their argument, other than providing a motivation for consideration of the landscape. The authors consider a SUSY minisuperspace, which leads to a lattice-like solution, and motivates them to employ a condensed matter analogy. It's only 4 pages long, and well worth reading.

Another recent analysis based on a condensed matter analogy has received some attention:

Cosmological constant and vacuum energy
(G.E. Volovik) gr-qc/0405012

8. **Fred**
October 24, 2004

I'd point out that neutrino masses are a complete mystery if you don't invoke a GUT theory.

Thats the one area the standard model notably fails experimental bounds, and why people should be interested in additions.

As far as SUSY goes, the coupling constant unification is a decent argument, but probably not the most convincing to me, particularly b/c its still a perturbative mess. However in GUT phenomenology there are various naturalness arguments in the Higgs sector, that favor (in simplicity) some sort of minimal SUSY model. Again the nastiness comes in the representation you choose, and I think its still pretty much an open question in physics phenomenology.

9. **Lubo? Motl**
October 23, 2004

That's very funny. Good joke. As far as the available data go, the previous posting could have been written by me, just to make fun out of myself – because everyone who is serious and observant enough knows that the arguments are usually on my side. 😉

10.
October 23, 2004

"I think you're right. Among thousands of exchanges I've had in my life, all of them had some facts and rational arguments."

Well, that's a good start! Now, the next thing for you to do is to have some of those facts and rational arguments on *your* side.

11. **Lubo? Motl**
October 23, 2004

Dear anonymous,

I think you're right. Among thousands of exchanges I've had in my life, all of them had some facts and rational arguments.

Once again: some more quantitative data on SUSY and gauge coupling unification are on motl.blogspot.com

All the best
Lubos

12.
October 23, 2004

In yet another piece of inadvertent humor from Lubos M, we have him writing about global warming on his blog:
"The actual reply from SHS has surprised me, shocked me, terrified me. It was a rather long e-mail, but it contained nothing else than personal insults against the four "sceptics" listed above"
Yes, it's a terrible thing when serious scientists are reduced to personal insults. Lucky thing that LM would never stoop so low, isn't it?

13. **Matti Pitkanen**
October 22, 2004

I am wondering why so few consider the possibility that the dead alley in the particle theory might derive from wrong assumptions, which were made already three decades ago and became soon sacred by the illusory belief on the linear progress of big science.

What created strong visceral reactions in me at that time was that quarks and leptons of single generation were forced into single multiplet although masses are widely different. The same was done even for different fermion generations having completely different mass scales. This
is certainly the least imaginative approach to unification that one can personally imagine. Therefore I believed that the observation that proton did not decay had been taken as an important message from the elegance loving Nature.

No one however cared although the message is easy to decipher: if quark and lepton numbers are separately conserved, they could do it as chiralities of 4+N dimensional spinors. Color would not be spin like quantum number but more like a partial wave in compactified dimensions. Completely different strategy to unification
would open up.

Anyone interested on details of this option can do this at
http://www.physics.helsinki.fi/~matpitka/ . See also
http://www.physics.helsinki.fi/~matpitka/newtgd.html for the newest developments in the understanding of particle spectrum and mass calculations.

Matti Pitkanen

14. **sol**
October 22, 2004

Drl,

Only I can be considered in that vain:).

Truth is, I didn't know that Lubos was to refer to Nima. I have been following this line of thinking for a bit now(google sci pysics strings early days:). Witten's reference, keeps me looking along these lines and the latest work.

Short Range Test's of Inverse Square Law

If your going to rebuttal, please do so on the reasons why not, so I can stay informed, and so, that others can too.

The poetic inflections, that such comments leave from personalities(Lubos or Peter's) are in no way my repsonsibility, or, are they your's.:)

The Standard Model and Beyond:)

15. **D R Lunsford**
October 22, 2004

It's the sol and Lubos core reference dump!

16. **sol**
October 22, 2004

Little attention has been given here Peter. As a method to testability, it had to be considered. They approached this properly and of course await for confirmation. It was not considered devoid, as a philosphical approach?

When symmetry breaks down, by Edward Witten

*Other ideas about electroweak-symmetry breaking go even further afield. **One line of thought links this problem to extra dimensions of space-time, subnuclear in size, but observable at accelerators**. This approach is probably a long shot, but the pay-off would be huge ? discovering extra dimensions could give us the chance for direct experimental tests of the quantum nature of gravity and black holes*

http://www.sns.ias.edu/~witten/papers/Symmetry.pdf

17. **Lubos Motl**
October 22, 2004

Some more quantitative comments about the gauge coupling unification

http://motls.blogspot.com/2004/10/gauge-coupling-unification.html

18. **sol**
October 22, 2004

Sometimes a nice [map](#) helps out for reference.

If all these events were taking place, lets say from a [true vacuum perspective](#), would this imply that the false vacuum existed in the first place?

19. **Lubos Motl**
October 22, 2004

I recommend the paper

http://arxiv.org/abs/hep-ph/9602206

and its references [1-4]. It makes it pretty clear the the unification works great if you compare the assumed values at the GUT scale, and the known value at the Z scale.

On the other hand, the alpha3 coupling at low energies is 12-20 percent different, which shows some neglected running at *low* energies, below the Z scale, and it is usually assigned to a light gluino.

It's sort of embarassing if it's true and Gordon Kane lives with his serious misconception. 😉

20. **Lubos Motl**
October 22, 2004

I think that I found the/a paper that contains a similar unusual statement.

It is the last (or 2nd last) paper of John Hagelin before he joined Maharishi Mahesh-Yogi. 😉

http://arxiv.org/abs/hep-ph/9406366

I leave it to the reader to decide whether this 10-page paper with 4 citations is trustworthy. I am not sure about the answer yet. 😉

21. **Lubos Motl**
October 22, 2004

So far, no one knows anything about it. I've asked a couple of phenomenologists.

All of them say that they did the 1loop calculation themselves, and they believe that the full 2loop result is even better – and the nice intersecting graphs we usually draw are the full 2-loop results.

At any rate, you are on the same boat with Gordon Kane about this ;-). On the other hand, it may be helpful if you found the scientific references containing this actual calculation – because once again, what two of you say sounds bizarre.

22. **Lubos Motl**
October 22, 2004

Thanks! I did not know that – and of course, I will recheck elsewhere.

We should be careful about two-loop results. Several years ago, Brookhaven claimed some deviation for the muon anomalous magnetic moment, or something like that, and it was used as evidence for new physics etc.

Finally it turned out to be a sign error in the theoretical 2-loop calculations.

I would still prefer to trust the 1-loop results more than the 2-loop results because they can be quite easily verified.

I agree that a 10% error in such a calculation is pretty large. Nevertheless, the running is better than in the pure SM, so in this case, it is still experimentally suggested that GUT prefers to get SUSY as its ally.

23. **Peter**
October 22, 2004

I could find a better reference, but here's a quote from Gordon Kane (hep-ph/0202185) (I seem to have been too kind to quote 5-10%)

"The supersymmetric gauge coupling unification misses by about 10%. More precisely, the experimental value of the strong coupling \alpha_3 is about 10-15% lower than the value computed by running down theoretically from the the point where the SU(2) and U(1) couplings meet. The details are interesting here — the one-loop result is somewhat small because of a cancellation, and the two-loop contribution therefore not negligible. If one only took into account the one-loop effect the theoretical value would be close to the experimental one, but the two-loop effect increases the separation."

I'll let my comments about the arguments for supersymmetry stand. I think all the arguments are very weak, the strongest is coupling constant unification, and even there, being off by 10-15% is not impressive.

If Witten really is quoting one-loops and ignoring two-loops, he wouldn't be the first one, but I don't think that's an honest thing to do.

24. **Lubo? Motl**
October 22, 2004

It looks like a good talk by Witten.

I agree with Witten's summary of the arguments for SUSY – and it even looks like the phenomenologists would agree, too.

Peter, could you give us some references about the 5-10%-off two-loop results on unification? I've never heard something like that, and it seems to be your only argument against SUSY (and GUT), except for totally unjustified nervous and hostile comments.

I understand your point of view "if I don't believe anything, I don't need to believe anything else". I just hope that you don't expect me to think that this set of arguments of yours goes too much beyond the thinking of the average monkeys. 😉

It's much easier to agree that the anthropic predictions of the SUSY breaking scale are vague hand-waving. But once again, being negative about everything does not solve anything, and you seem to be negative about everything.

25. **Peter**
October 22, 2004

I don't think there's been anything new about neutrino masses in quite a while. You can easily extend the standard model to incorporate neutrino masses in at least two ways:

1. Just add right-handed neutrinos with no charge under the standard model gauge group and then, just like the other leptons, Yukawa Higgs couplings give neutrinos mass. These couplings have to be much smaller than the couplings to the other leptons, but then again, no one knows why such couplings have such a huge range of values already in the standard model.

2. Add a right-handed neutrino with a lepton-number violating Majorana mass term. If you make this large enough you can generate small masses for the observed neutrinos through the"seesaw" mechanism.

SO(10) GUTs naturally contain a right-handed neutrino state with no SU(3)xSU(2)xU(1) charge.

26. **dolt**
October 22, 2004

Peter thank you for your summary of the colloquim and for stating the obvious!

But one definite pro for SUSY and string theory ("unique/parameter free"), which is left unstated is that they are great fields for publishing lots of papers! A definite pro when it comes to careers in academia these days and explains the herd mentality.

BTW, what is the latest on incorporating neutrino masses?

DMS

27. **serenus zeitblom**
October 22, 2004

Very often a resort to anthropy is just a sign of lack of imagination. Even the landscape may not need it:
http://arxiv.org/abs/hep-th/0410213
and similarly the famed "cosmic coincidences" don't require it either:
http://arxiv.org/abs/astro-ph/0410508

On Beauty and String Theory

October 25, 2004
Categories: Uncategorized

One of the arguments often given for string theory is that it is somehow exceptionally "beautiful". This has always mystified me, since that's certainly not the way I would describe it. Over the years I've paid close attention whenever I see someone trying to explain exactly what it is about string theory that is so beautiful. Lubos Motl has just posted his own detailed answer to this question, something I read with interest.

As usual, Lubos is not exactly concise, so I won't quote him extensively, but let me try and summarize his arguments for calling string theory beautiful, together with some of my own comments.

1. Symmetries are beautiful and just about every symmetry you can imagine gets used somewhere, somehow in string theory.

Even Lubos is not so sure of this argument, since he says " I don't really thing that we view symmetries as the most important reason why string theory is beautiful". What is beautiful about symmetries is the way they constrain things. If your theory is based upon a simple symmetry principle (take for example gauge theory and the gauge symmetry principle), a huge amount of structure follows from a single, simple principle. String theory is not based on a simple symmetry principle, rather it is a complicated framework, into which you can fit all sorts of different symmetry principles. But because they are not fundamental, these symmetries don't constrain the theory much if at all. This is very different than the standard model, where at a fundamental level the theory is built around a single symmetry principle, one that governs a large part of the structure of the theory and its physical predictions.

2. The way in which "miraculous" cancellations occur in string theory, constraining the theory by only allowing it to make sense for certain specific choices.

The most well known example of this is the way in which anomaly cancellation picks out 10 dimensions and SO(32) or E_8 times E_8 for the superstring. This was the main reason people got so excited back in 1984, when they thought that the anomaly cancellation principle would give them a nearly unique theory that could be used to make predictions. If the anomaly cancellation principle had picked out four dimensions and SU(3)xSU(2)xU(1), that certainly would have been a beautiful explanation of why the standard model is the way it is. In the standard model itself, anomaly cancellation for the chiral gauge symmetry does work in an impressive way. If you take just the leptons or just the quarks, you have an anomalous theory, but the anomalies of the one cancel those of the other.

In string theory, all anomaly cancellation does is pick out a much too large dimension of space-time and a much too large gauge group. You can certainly embed the standard model in this structure, but you could also embed just about anything you want in it because there is so much room. In the end you are stuck with some version of the "Landscape", essentially an infinite number of different possibilities with no way to choose amongst them. The anomaly cancellation ends up providing very little constraint on what the structure of low energy physics looks like.

3. String theory is a unique theory that can predict everything about the physical world.

Lubos likes to go on about how unique and predictive string theory is. While I understand this is the dream of every string theorist, the reality of what they actually have is a long ways from what they hope is true. The vision of what they would like to be true may be beautiful, but the reality is something else. The reality is that there is no "unique" string theory that can reproduce the real world, just a dream that such a theory exists. And as for predictions of string theory, there are none. When Lubos says that "string theory predicts" things, what he really means is that if every thing he would like to be true actually were, then in principle you could predict things from string theory.

4. String theory manages to extend quantum field theory in a consistent way, something which is very non-trivial and the way this happens can be described as beautiful.

This seems to be Witten's main argument these days for promoting the continued study of string theory and I have a certain amount of sympathy for it. There certainly is something of interest going on behind the complicated framework that people are studying under the name "string theory" and maybe it will someday lead to insight into something about physics, most likely the strong coupling behavior of gauge theories. But the fact that there is interesting structure you don't understand doesn't mean that this structure has anything to do with a fundamental unification principle for physics.

5. There are beautiful connections to new pure mathematical structures.

The relation of string theory to mathematics is a huge topic, and I'll comment on it at length at some other time. In brief though, while I think string theory has been an utter disaster for theoretical physics during the past 20 years, it has lead to many interesting things in mathematics. However, most of these interesting things really come from 2d conformal QFT, and I would argue that it is QFT which is having a huge impact on mathematics, much more so than string theory. Witten's Fields medal was for his work on the relation of QFT to math, not for anything he has done using string theory.

Comments

1. **D R Lunsford**
   October 31, 2004

   Chris W – re those Smilga papers:

   Alright, I read these papers. I was disappointed.

   1) His main idea seems to be – 3d ang. momentum to SU(2) spin nets is the same as spacetime angular *and* linear momentum to SO(3,2) spin nets. This isn't true generally, so he spends most of his effort patching up the problems by contracting SO(3,2) to Poincare. This is a mistake – the idea itself is good, but the realization is not (see below).

   2) His transition from 2-spinors to 4-spinors is hand-waving nonsense. As we know, this amounts to going from the restricted Lorentz group to the full one including spacetime parity. The physical net gain is antimatter, which he never mentions.

   3) His identification of s_m4 with y_m is itself not correct, so he gets the Clifford algebra wrong. For the purpose of his application this is harmless, but for the purpose of the correct approach it's fatal.

   What's the correct approach? You need to do it for SO(3,3) – then one has real translations without the need for approximation, like this…

   The Lie algebra has terms that look like (indices go 1-4)

   [Jm5, Jn6] = i gmn J56 = i gmn M (mass!)

[Jmn, Jp5] = -i (gmp Jn5 – gnp Jm5)

[Jmn, Jp6] = -i (gmp Jn6 – gnp Jm6)

etc.

Calling Pm = Jm5 + iJm6 and Qm = Jm5 – iJm6 we see that both Pm and Qm represent translations abstractly, but they go off shell into complex spacetime, so one needs *two* of them in succession to get a *real* displacement. Since they are translations, they still commute. Thus one can build up finite translations in spacetime *without contraction*. This should allow one to build a 4-d checkerboard model on the lines of Gersh's and Feynman's 2-d model. One should also be able to construct a spin network model of the 6d spacetime. I've been meaning to do this but occupied with other things. Note that the Weyl theory on SO(3,3) – SO(3,3) neutrinos – leads to the Dirac theory on spacetime, with the two neutrino helicities correpsonding to positive and negative mass terms. A paper on this subject should appear soon.

-drl

2. **sol**
   October 29, 2004

   Thanks DRL for responding.

   I have to walk very slowly in these circumstances, being the layman, I am just learning to walk on these topics.

   Speculation?

   1. Background Independence And The Holomorphic Anomaly

   Finding the right framework for an intrinsic, background independent formulation of string theory is one of the main problems in the subject, and so far has remained out of reach. Moreover, some highly simplified special cases or analogs of the problem, which look like they might be studied for practice, have also resisted understanding.

   I assume a high energy consideration from the early universe.

   G -> H -> … -> SU(3) x SU(2) x U(1) -> SU(3) x U(1)

   Here, each arrow represents a symmetry breaking phase transition where matter changes form and the groups – G, H, SU(3), etc. – represent the different types of matter, specifically the symmetries that the matter exhibits and they are associated with the different fundamental forces of nature

   Some might have assumed a false vacuum as existing instead of "nothing", and from it, a true vacuum to emerge. This would encapsulate, and like bubble eversions, explain what would have been held to the brane, and what is allowed to roam in the bulk? I know this might seemed far fetched as I am trying comprehend the nature of of the abstract world given to defining feymans integral paths as loops, defined in that abstract geoemtrical thinking. This because of it's background dependancy, had to be modifed in order to to speak from the compactified dimensions, and find themselves revealled in spacetime. You see?

   I apologize to those of you who may find this easy.

3. **D R Lunsford**
   October 29, 2004

   Sol,

   You don't have to indulge in any speculation. If a theory has a Lagrangian and variational principle where everything in sight is varied, leading to equations involving all the elements, then that theory is background independent practically by definition. So for example the Einstein-Hilbert action is

   S = integral R sqrt(det(g)) dx..dt

   R is a function of g_mn and its first and second derivatives, and this appears again in the volume element sqrt(det(g)) – so the equations that come out of delta S = 0 are background independent. The Maxwell action

   S = integral Fmn Fab g_ma g_nb sqrt(det(g)) dx..dt

   leads to Maxwell's equations in vacuo and these are not, because the g_mn are not varied. You might ask what happens if I vary the g_mn in the latter as well as the A_n (F = curl A) – answer: because no derivatives of g_mn appear in the action the Euler equations are simply

   F_am F_mb + 1/4 F_mn F_mn g_ab = T_ab = 0

   F_mn,n = 0

   that is, the energy tensor for the EM field also vanishes, and so there is in fact no field.

   The best you can do with both, in the existing scheme, is to paste together an ad-hoc Lagrangian

   L = R + k W

   where k is an additive (dimensional) constant – one then can get

   R_ab – 1/2 R_mn R_mn g_ab = -k T_ab

   that is, the Einstein-Maxwell equations (Maxwell in curved space with the field energy as a source of curvature). So, Einstein-Maxwell is background free, but *not* unified, because the Lagrangian is not irreducible. To sum up, background-freeness is not a mystical property, it's simply the statement that all the essential elements can change from place to place.

   -drl

4. **sol**
   October 29, 2004

   Well let's see here. Neocons(playing with words very creative:)

The Problem of Dynamics in Quantum Gravity

The problem of dynamics in quantum gravity is still a big challenge. We don't know how to make spacetime a truly dynamical entity with local degrees of freedom while taking quantum theory into account. Neither string theory, nor loop quantum gravity, nor the spin foam and causal dynamical triangulation approaches have yet found a background-free quantum theory with local degrees of freedom propagating causally. We sketch some avenues for making progress in this direction.

As I was trying to comprehend how gravity was to be inclusive in string theory, it soon became apparent that it was background dependant. Truly as John Baez implies this is not desired, by others as well.

But if we assume background dependancy, then from what I understood, it became the background and the quantum mechanical discription of the spacetime fabric. Please anyone correct this perception if it is wrong.

Thus from this perspective, a emergent geometry would have been allowed to surface, where all other geometrical approaches, could not have been allowed?

Again this is not what is desired of string theory and the background independance is most preferred. I have many links on quantum grvaity that would innuadate your selection DRL.

I would rather a concensus on whether any geoemtry shall emerge(what shall emerge in the Third Superstring Revolution) and how it shall do that. If we do not consider this context, then we are left to consider, the value of glast determinations and the link Peter offerred.

Inside Gamma Sphere:

The device's 110 gamma-ray detectors point to the center of the spherical array, where a beam of nuclei from a particle accelerator smashes into a thin target. The collisions create unstable nuclei that decay by emitting gamma rays, an extremely high-energy form of light. Gammasphere catches and measures as many of the gamma rays as possible, so that scientists can study what happens to nuclei under extreme physical conditions.

http://www.symmetrymag.org/cms/?pid=1000017

Is this a bad thing? No it reaffirms the direction of glast on the cosmological scale and secures QF visionist on the roads to percieving how quantum grvaity makes sense leaving GR alone(Smolin's position ?).

But as I said, without th econsistancy of a geoemtry to emerge, there is no hope for a perspective to form around quantum geometry as a discritor of quantum gravity?

5. **D R Lunsford**
October 28, 2004

Chris O,

It's damned unbelieveable, what?

Strings depend essentially on KK theory, a failed attempt to extend Riemannian geometry to encompass the A field as part of an ersatz cylindrical metric in 5-d. The simplest contradictory logical loopbacks seem to be beyond the Greenes of the world. They remind me of our neocons.

Now there really *is* an effort to make quantum geometry, see for example

http://www.physics.gatech.edu/people/faculty/dfinkelstein.html

You won't find any hippocoprolitic stringism there.

-drl

6. **Doug**
October 28, 2004

drl wrote:

Furthermore, what sort of disingenuousness is it to say "field theory is ugly", when string theory would have to reproduce that same ugliness to be taken seriously? (It won't, but this is rhetoric.) Whatever next step turns out to be the right one, it will have things that look like gauge fields, energy tensors, etc. etc. and people will say "Ah! Now we know why field theory was that way – how beautiful!"

Great observation! As Hestenes, in his *New Foundations for Classical Mechanics*, observes: *"[Newton] deserves the title of "founder" (of classical mechanics) because he integrated the insights of his predecessors into a comprehensive theory."*

However, while your statement hits the nail right on the head, it also assumes that a successor will be successful, as was Newton, in moving modern physics forward "by integrating the insights of his predecessors into a comprehensive theory." This may not happen. As Hestenes points out, Newton did more than integrate these insights into a more encompassing theory, he launched "a well-defined program of research into the structure of the physical world."

The two great revolutions in physics of the 20th century, QM and GR, are the great insights to be integrated, but it may take a theory that does more than integrate them to move us forward, it may require a new "program of research into the physical world." Of course, that's a tall order, but it's the "new physics" everyone talks about these days, but in the true sense of the word; that is, in the sense of a new program of research, rather than new phenomenology.

That such a program would be characterized by the simplicity of geometry and the beauty of fundamental,powerful ideas, unencumbered by the excesses we see today, is certain in my mind. The mathematical indulgences and wild speculations so popular in normal science today remind me of the days when doctors proudly wore blood smeared smocks as testimony of their professional virtuosity. The irony is palpable.

According to Hesetenes, the central hypothesis of Newton's program "is that variations in the motion of a particle are completely determined by its interaction with other particles," and that [Newton's program of research] has been interpreted as a dictum: **to focus on forces.** He says, "The aim is to classify the kinds of forces and so develop a classification of particles according to the kinds of interactions in which they participate."

Perhaps, however, such a focus cannot deliver the goods. I think that Newton understood that force was a property of motion, and that motion was the proper study of physics, not forces, and that a new program of research, focusing on motion, that is, the reciprocal relation of space and time, may be more fruitful now than to continue the present course, as successful as it has been. We have explained the diverse properties of objects in our experience in terms of a few kinds of interactions among a few kinds of particles, but it appears that we have reached the limit of our methodology. Thus the challenge has reverted to the epistemological side of the reciprocal relation between methodology and science as described by Einstein. The old man, it turns out, was right on once again.

7. **Chris Oakley**
October 28, 2004

Cough *Splutter* Just a minute … until they've proved that they can get "classical" G.R. as a limiting case, String theorists have no right to claim this as part of their theory.

8. **sol**
October 28, 2004

Interesting link CW. It made me think, that the efforts that Einstein went through to quantize grvaity, had the opposite effect as well that they(Dirac, Bohr, Schrodinger) would work to understand GR as well

?The gravitational treatment of point particles thus brings in one further difficulty, in addition to the usual ones in the quantum theory.? This is rather curious coda since the above problems are really as relevant classically, and of course they are very different from the perturbative nonrenormalizability issues that have dominated all subsequent studies. After this pioneering foray, Dirac?s original publications in the field waned, apart from one later paper [17] on conformally invariant extensions of GR.

Now for me when a picture enters my mind, it is unassociative for a bit, but neuronically connected, and relevant. So to Mona Lisa's smile and the trampoline.

DRL,

Figure 10.1 *When standing on the Mona Lisa trampoline, the image becomes most distorted under your weight*

The Heart of Reimannian Geometry

This example cuts to the heart of Reimann's mathematical framework for describing warped shapes. Reimann, building on earlier insights of mathematicians Carl Friedrich Gauss, Nikolai Loachevsky, Janos Bolyai, and others showed that a careful analysis of the distances between all locationson or in an object provides a means of qauntifying the extent of its curvature…..

The Elegant Universe, by Brian Greene, page 232 & 233

One has to still endure the geometry that is progressive, and has developed along side of the physics. One does not disavow what the world of gauss(the roads leading too hyperdimensional space) is doing in light of Maxwell's gains. That any toy model is found correlated in relation to QFT, was a monumental effort by Kaku, in his loop calculations?

You see. So between Smolin who adopts the GR attitude of leave Gr as it is, a Einsteinain way, and strings to adopt, the quantum mechanical discription of the background, neither can reject the ideals of the quantization of gravity, and a geoemtrical perspective.

What geometry shall emerge?

"*But now, almost a century after Einstein's tour-de-force, string theory gives us a quantum-mechanical discription of gravity that, by necessity, modifies general relativity when distances involved become as short as the Planck length.* ***Since Reinmannian geometry is the mathetical core of general relativity, this means that it too must be modified in order to reflect faithfully the new short distance physics of string theory****. Whereas general relativity asserts that the curved properties of the universe are described by Reinmannian geometry, string theory asserts this is true only if we examine the fabric of the universe on large enough scales. On scales as small as planck length a new kind of geometry must emerge, one that aligns with the new physics of string theory.* ***This new geometry is called, quantum geometry***."

The Elegant Universe, by Brian Greene, pg 231 and Pg 232

Enjoy:)

9. **Chris W.**
October 28, 2004

Dirac's 1935 (!) paper is discussed in this short review by Stanley Deser.

10. **D R Lunsford**
October 27, 2004

Chris W – the work by Smilga seems to pick up on an idea of (who else?) Dirac, "The Electron Wave Equation in deSitter Space", and "On a Remarkable Representation of the 3+2 deSitter Group" (I think those are more or less the correct titles). The former appeared, I think, in either the Royal Society Proceedings or some Russian journal – the latter appeared in JMathPhys sometime in the early 60s I think. Dirac clearly was thinking about (anti)deSitter space in some detail, over a number of years.

I read these papers years ago in school, and filed them in the "very interesting" drawer.

Also interesting here is the dropping of the name Wyler and his work on the fine structure constant.

Again, nice papers, very nice.

-drl

11. **D R Lunsford**
October 27, 2004

Chris W – those are intriguing, related to what I'm working on now! Thanks for that.

12. **Chris W.**
October 27, 2004

The quote attributed below to Einstein ("any intelligent fool ..") actually seems to be due to E. F. Schumacher.

13.
October 27, 2004

DRL,

You can't do "new physics at the Planck scale", or anywhere, until there is new physics. The stringers have one thing right – this needs a new idea about matter. I have a *model that should be explored, and probably will be eventually. Like the things that preceeded it, it is evolutionary, not revolutionary.*

Doesn't all this radical talk ever get on your nerves? What I find most strange about the stringers is how they can be so satisfied with nothing but a lot of radical talk, triumphal hand-waving, and weird imagery.

Would I be wrong to say that quantum mechanics started off theoretically, and that in having developed, brought the science along?

For me, I am quite young in terms of my academic career but older in age, that if I did not find some consistancy in the way the geometry evolved, then yes, these abstract spaces that topological genus figures would never make sense. But, this is one end of the geometry that is revealled in the hyperdimensional realities, and might have found it's correlates, in the cosmos?

Klein`s Ordering of the Geometries

"*A theorem which is valid for a geometry in this sequence is automatically valid for the ones that follow. The theorems of projective geometry are automatically valid theorems of Euclidean*
geometry. We say that topological geometry is more abstract than projective geometry which is turn is more abstract than Euclidean geometry."

http://www.ensc.sfu.ca/people/grad/brassard/personal/THESIS/node21.html

So to me this geometrical math developing has to be based on the predessors somehow, and along side, the physics? Would you not consider Reinmann in the value of GR? His Teacher, Gauss?

Brane World models only carry this unique topology solution a bit further returning in part, though often modified in present format, to Klien's solution to the unification of gravity to electromagnetism through an additional dimensional set. ***The beauty of this path as we should rightly call it is that it is a natural progression of Einstein's dream of a pure geometric explanation for everything we see around us in nature."***

What do you think of this statement above?

I also draw your attention to Lubos's post and note the comments on Robert Lauglin.

What is the nature of the math that should emerge, if not by string theory?

Mathematics is not the rigid and rigidity-producing schema that the layman thinks it is; rather, in it we find ourselves at that meeting point of constraint and freedom that is the very essence of human nature. – **Hermann Weyl**

14. **Chris W.**
October 27, 2004

Speaking of economy of ideas, I urge readers of this weblog to take a thoughtful look at **these papers**, so far uncited except by the author himself.

In my humble (and relatively unschooled) opinion they are extraordinarily well conceived and superbly written. The author's professional career apparently began in the 1960s (in Germany).

15. **Frank**
October 27, 2004

Hmmmm…. Appealing to beauty is not neccesarily out of touch with reality. But it may be that for the time being our concepts and modells of beauty are inadequate, thus we have to resolve to patchworking.

Some more quotes:

"I think that there is a moral to this story, namely that it is more important to have beauty in one's equations that to have them fit experiments. If Schroedinger had been more confident of his work, he could have published it some months earlier, and he could have published a more accurate equation. It seems that if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress. If there is not complete agreement between the results of one's work and experiment, one should not allow oneself to be too discouraged, because the discrepancy may well be due to minor features that are not properly taken into account and that will get cleared up with further development of the theory."
– P.A.M Dirac

What i think is most critically missing in physics today is search for simplicity. Many people are interessted in making bigger badder more complicated theories, a few are interessted in making existing ones more rigorous, but who is actually trying to simplify them?
Perhaps we are just not ripe yet to move in that direction.

"Any intelligent fool can make things bigger and more complex… It takes a touch of genius – and a lot of courage to move in the opposite direction."
– Albert Einstein

That to me rings to the core issue with Stringtheory.

16. **Chris Oakley**
October 27, 2004

…the expediency of renormalization seems to be a wart on the face of it

I'm glad I'm not the only one saying this. Having said that, in the (few) post-doc job interviews I had in 1984/5 I am sure I detected a groan when I said that my ambition was to "crack renormalization".

It is unfortunate that something that claims to be as brilliant and beautiful as string theory hopes at best just to reproduce all this garbage.

17. **D R Lunsford**
October 27, 2004

Doug,

Beauty in some sense = economy of ideas. Field theory is very economical in ideas. That it is phenomenology, is no fault of the thing in itself. And, in the context from which it emerged – utter pessimism about HEP – it was certainly beautiful in the sense that it gave reasonable answers.

Furthermore, what sort of disingenuousness is it to say “field theory is ugly”, when string theory would have to reproduce that same ugliness to be taken seriously? (It won’t, but this is rhetoric.) Whatever next step turns out to be the right one, it will have things that look like gauge fields, energy tensors, etc. etc. and people will say “Ah! Now we know why field theory was that way – how beautiful!”

I’ve never been the least bit impressed by Hawking, other than at his phenomenal virtuosity.

-drl

18. **Doug**
October 27, 2004

drl,
Personally, I think Hawking has a fairly solid point of view on the matters of modern physics. At least, I wouldn’t criticise his characterization of philosophical motivations for the pursuit of a unified theory seeing that he is sitting in Newton’s chair. After all, the man isn’t exactly an incompetent crank. However, that’s beside the point. In the matter of judging truth and beauty, clearly the standard model suffers from the fact that it is “phenomenology backed by simple, general ideas.” A theory able to calculate the masses of the particles from first principles would certainly be superior in this regard, and one would certainly like to see how those masses emerge in the generations they do. Also, the expediency of renormalization seems to be a wart on the face of it to my mind. However, some believe that “beauty is in the eye of the beholder,” but if this is true, I don’t believe truth could be beauty or beauty truth.

Regards,

Doug

19. **D R Lunsford**
October 27, 2004

Sol,

I hold quite a definite view of things, for which I can fortunately only blame myself. My view involves, not breaking, but encompassing Lorentz symmetry, which I see as an artifact of the separation of spacetime and matter.

You can’t do “new physics at the Planck scale”, or anywhere, until there is new physics. The stringers have one thing right – this needs a new idea about matter. I have a model that should be explored, and probably will be eventually. Like the things that preceeded it, it is evolutionary, not revolutionary.

Doesn’t all this radical talk ever get on your nerves? What I find most strange about the stringers is how they can be so satisfied with nothing but a lot of radical talk, triumphal hand-waving, and weird imagery. It’s like living on LSD and Hershey bars washed down with absinthe. I’m still grapping with the Mona Lisa trampoline, a decidedly disturbing hallucination for some reason.

Back to Lorentz invariance – you will *never* detect Lorentz symmetry breaking with *any* model of matter that posits a “form and substance” ontology, that is, matter of *whatever* shape cruising through spacetime of *whatever* structure, because Lorentz invariance arises *before* any specific form for matter is assumed. The Einstein-Weyl idea of matter and spacetime having a joint origin thus has to be right.

As far as S and S goes, I could not care less – all these folks are obviously on the completely wrong track and they bore me to tears. Our own Thomas Larsson is far more interesting.

-drl

20. **sol**
October 27, 2004

DRL,

I hate to rain on your parade.:)

*If Lorentz symmetry is broken by some mechanism originating at the Planck scale, is there any hope of detecting such an effect? Surprisingly, the answer is yes. Over the past decade **Kostelecky and co-workers have been exploring how a violation of Lorentz symmetry might provide evidence for new physics arising at the Planck scale**. However, rather than smash particles together at high energies to explore this, researchers are turning to ultrahigh-precision experiments at low energies to search for signs that Lorentz symmetry has been broken. The idea is that such low-energy effects are caused by corrections involving inverse powers of the Planck scale.*

Possible violations of Lorentz invariance are an ideal signal of new physics because nothing in the Standard Model of particle physics permits the violation of special relativity. Therefore, no conventional process could ever mimic or cover up a genuine signal of Lorentz violation.

Since a viable theory of physics at the Planck scale remains elusive, it is difficult to make precise predictions for the small corrections that could occur due to Lorentz violation. However, we can obtain a rough estimate. The rest-mass energy of the proton, for example, is about 1 GeV, and the ratio of this energy to the Planck scale is about 1 part in 1019. If an experiment with protons is sensitive to effects at or below this level, then it is effectively probing the Planck scale.

http://physicsweb.org/articles/world/17/3/7/1#pwfea1_03-04

One tends to accept that being in the unique position of holding no substantial view in regards to the evolution of thinking here, that you tend to look for the essence of things that might of been driving research and mathematics?

Lucky guesses?

Recognizing Smolin’s positon, and then Susskind’s would it hurt to include both in the analysis, and find yourself a onely sol, who wanders searching for how these abstract spaces answers the question of whether they are back ground dependant or not?

21. **D R Lunsford**
October 27, 2004

Hi Doug,

Let’s deconstruct this statement:

“The real reason we are seeking a complete theory, is that we want to understand the universe, and feel we are not just the victims of dark and mysterious forces.”

This sounds suspiciously like an apology for witch-burning. I never recall Dirac or Einstein stating the “real reason” they were so occupied. The “real reason” for their efforts was never in doubt.

“If we understand the universe, then we control it, in a sense.”

That’s very strange – I never knew physics was about control. I always thought it was about curiosity.

“The particles are grouped in an apparently arbitrary way..”

That is an interesting statement. I did not know until this very moment that the relevant gauge groups were arbitrary! That is fascinating!

“..and the standard model depends on 24 numbers, whose values can not be deduced from first principles, but which have to be chosen to fit the observations. What understanding is there in that?”

That’s the nature (ha ha) of a phenomenological theory. One gets the feeling that Hawking was just tossing word salad to come up with a justification for his Faustian ambition.

Now, for comparison we must cite the supposedly non-arbitrary “shuttlecock” model of Hawking, wherein the actual hyperbolic structure of the world is, “without any arbitrariness whatsoever”, converted to an elliptic one “just for the sheer rollicking hell of it”. Right.

-drl

22. **Doug**
October 27, 2004

drl,

The exact Hawking quote:
“The real reason we are seeking a complete theory, is that we want to understand the universe, and feel we are not just the victims of dark and mysterious forces. If we understand the universe, then we control it, in a sense. The standard model is clearly unsatisfactory in this respect. First of all, it is ugly and ad hoc. The particles are grouped in an apparently arbitrary way, and the standard model depends on 24 numbers, whose values can not be deduced from first principles, but which have to be chosen to fit the observations. What understanding is there in that?”

The link to the Hawking talk is here:

http://www.damtp.cam.ac.uk/strtst/dirac/hawking/

23. **D R Lunsford**
October 26, 2004

Arun,

I would not say that beauty in *anything* is ineffable – precisely the opposite, beauty is compelling and, usually, obvious. This does require some sort of aesthetic judgment, and the latter may be missing.

In the case of Kepler, I think you also have it backwards – he had a specific problem to work on, not an axe to grind. His polyhedric orbs were no different in principle than the Lorentzian deformable electron, and no longer lived. It was *because* of his orbs that he eventually made progress, not *in spite* of them.

24. **DMS**
October 26, 2004

“I wrote another article on motls.blogspot.com that clarifies some ridiculous statements made by Peter Woit about Witten’s reasoning being “unrelated” to string theory.”

I am sorry but I remain unpersuaded. By this logic, theta functions is really a part of string theory because theta functions arise in string theory! And so is algebraic geometry and so on.

People knew about a lot of stuff before string theory came along, and quite likely most of the neat mathematical stuff would have been discovered (like results that have made Witten justly famous) whether or not string theory was studied.

25. **D R Lunsford**
October 26, 2004

I have to admit the torrent of verbiage that issues from Bon-Motl is a source of continual wonderment and awe. So you see, string theory is not without interest.

-drl

26. **Lubos Motl**
October 26, 2004

I wrote another article on motls.blogspot.com that clarifies some ridiculous statements made by Peter Woit about Witten’s reasoning being “unrelated” to string theory.

27. **Arun**
October 26, 2004

Beauty in physical theory is an ineffable thing.

Its been a long time since I read about Kepler, but I think among things that he tried, and was excited about, was the possibility of relating planetary orbits in some way to the regular polyhedra, one inscribed into the next. Thus, the ratios of planetary distances from the Sun would be explained. But the culmination of his research was the laws governing planetary orbits in general, and not why the particular planetary orbits of the solar system had particular radii.

We do not think science has been held up or Kepler’s laws are ugly because we can’t explain the ratio of the distances from the Sun of Earth and Jupiter. We also see that Kepler did try to imagine a solution, a solution that we would find to be beautiful, if it was true!

String theory may appear to be beautiful too, once we find out what is proper for it to explain.

28. **Peter**
October 26, 2004

Hi DMS,

The result about infinitely many smooth structures on R^4 is just one aspect of Donaldson's results, and it's a hard one to visualize or say much about. You can think of R^4 as just a topological manifold, but you need to add some extra structure then to be able to differentiate functions (to even say which functions are differentiable and which aren't). You can think of this extra structure as a choice of coordinates, defined by choosing local coordinate patches. If you choose one global coordinate patch using the standard coordinates on R^4, that gives you one smooth structure, but it turns out there are other ways of putting coordinate patches on R^4 that give an inequivalent notion of which functions are differentiable. I don't know that anyone has explicitly constructed such things, Donaldson just proves that they exist.

More relevant are Donaldson's results about compact 4-manifolds. Here he produces a set of polynomials that are invariants of the smooth structure on the 4-manifold, and these can be used to solve problems in 4-d topology. The correlators in the N=2 twisted SUSY model are exactly the Donaldson polynomials. In general these can be hard to compute and prove things about. The big breakthrough of Seiberg-Witten was to relate the non-abelian N=2 twisted SUSY model to another dual one, where the gauge symmetry is Abelian, but there are monopoles. In this new "Seiberg-Witten" model, computations and proofs are easy.

29. **DMS**
October 26, 2004

Peter,

I very much enjoy your posts, especially this one and the previous one. I saw a lot of 'hype' when in grad school over string theory and SUSY and that proof was just 'around the corner', if not already there; I am still waiting. Nothing wrong in trying to see if a certain idea works, but please be honest about it. The gauge coupling unification bit is an example — choose a 'nice' parameter set to make your result really good, and omit mentioning that it does not look as good with a different set.

Like you I am not bitter. I am doing fun things, gainfully employed outside academia and getting the satisfaction of doing work that is actually tested. I try to follow recent developments (your blog helps!) and I see that I am not missing much.

Regarding Donaldson invariants, I realize that he proved that there are infinitely many manifolds diffeomorphic to R^4. (I guess the other famous result was Milnor's proof that there are 28 differentiable structures on S^7.) Could you explain what that means, maybe with a simple example? Somewhere I read, it means there are 'infinite number of ways to differentiate'—what does that mean?

Also, regarding Witten's method of getting them (and more invariants?) using his work on Seiberg-Witten theory, what are the topological correlators in the N=2 twisted SUSY that he is calculating?

Many thanks in advance!

DMS

30. **D R Lunsford**
October 26, 2004

Peter said:
Sure there's a personal reason behind my criticisms (and no, it's not that I'm resentful failure. Academia has ended up treating me far better than I ever expected. Lots of people go through life resentful that they've never had their talents properly recognized. I'm not one of them.) The reason is simple: I think what has been going on in string theory recently is an intellectual and moral disgrace, and it defiles something extremely important to me, the whole idea of trying to better understand how the universe works in an honest way.

This is a wonderful summation of how many of us *feel*. This is not just a matter of thinking – the intellectual fraud that is string theory stabs at the heart of science, in the sense that those who do science need at some point the judgment that derives from being *emotionally involved* with finding things out, and not just occupying an academic position.

-drl

31. **D R Lunsford**
October 26, 2004

The SM is not "ugly" and it's certainly not "ad hoc" – if Hawking said this it confirms my opinion that, as a physicist, he's a superior mathematician. The SM is phenomenology backed by simple, general ideas.

In contrast, string theory could not *possibly* be more postmodernly ad-hoc, and sets a new bar of loathsome ugliness, both in itself and in the culture it engendered. It is a ridiculous, poisonous, viral fiction.

-drl

32. **Peter**
October 26, 2004

Hi Francois,

I was referring to Witten's TQFT for Donaldson theory, which is a twisted version of N=2 supersymmetric Yang-Mills. This is a 4d Yang-Mills theory with fermions, the fermion charges are just somewhat different than in the standard model. Mathematically this theory has a beautiful interpretation as computing the euler class of a certain bundle.

33. **raj**
October 26, 2004

>I think the state of modern physics, now in the throes of two, incompatible, 20th century revolutions, is looking more and more pathetic from an outsider's point of view.

I tend to agree, to some extent. Physicists who rave over a theory merely because it is "beautiful" (a/k/a, full of jargon) instead of having something to do with reality (a/k/a, that it has some experimental justification) strike me as being, well, divorced from reality. One of the reasons that I found physics so interesting, when I was studying it in University in the late 1960s and early 1970s, was that it appeared to be *grounded* in reality. It is somewhat sad to see the discipline devolve as it apparently has.

34. **Thomas Larsson**
October 26, 2004

The circle is undoubtedly the most beautiful geometrical shape. Hence we conclude that planets must move in circles, or in circles around circles.

It is interesting that people like Motl, while promoting symmetry principles as an argument in favor of string theory, are vehemently opposed to the investigation of symmetries which are not restricted to one complex dimension. Perhaps this limitation to one-dimensional symmetries is a sign of a one-dimensional mind.

35. **Francois**
October 26, 2004

Hallo Peter, you just wrote

"The QFT whose observables are the fundamental 4-manifold topological invariants is mathematically very close to the QFT of the standard model. I find it hard to believe this is a coincidence. There's very deep mathematics, somehow related to supersymmetry, going on here, mathematics whose proper understanding might lead to new ways of thinking about the standard model."

Could you explain what you mean by the "closeness" in more detail? Apart from being enlightening in its on right, the answer would clarify the discussion on the beauty/ugliness of string theory.

Francois

36. **Peter**
October 26, 2004

Hi Z,

An interesting response, here's what I think about some of the issues you raise (although I really can't do justice to the whole issue of string theory and math, it's a long and complicated story).

I don't think 11d supergravity has had any appreciable impact on mathematics yet, or that it ever will (other than in perhaps indirect ways. People have and will continue to get involved in really interesting math sometimes because of questions that came up when they were looking at 11d supergravity).

About D-branes and CFT: depends what aspects of D-branes you mean. In some sense D-branes are just boundary conditions on CFTs on worldsheets with boundary. A CFT on a worldsheet with a boundary is still just a QFT of a certain kind. The only things that seem to me to definitely be non-QFT results are those involving summing over all genus worldsheets.

I think CFTs are of great importance not because they can be used to construct string theories, but because our understanding of QFT in general is very crude. By studying CFTs we can get a lot of insight into QFT in general, and hope to apply this to 4d QFT.

I don't think I dismiss all of string theory at every possible opportunity. My view of work that has come out of string theory ranges from very positive (CFT, mirror symmetry) to neutral (adS/CFT may solve QCD, may not, seems worth trying, but may not work) to very negative (the Landscape).

Sure there's a personal reason behind my criticisms (and no, it's not that I'm resentful failure. Academia has ended up treating me far better than I ever expected. Lots of people go through life resentful that they've never had their talents properly recognized. I'm not one of them.) The reason is simple: I think what has been going on in string theory recently is an intellectual and moral disgrace, and it defiles something extremely important to me, the whole idea of trying to better understand how the universe works in an honest way.

Of course I'm trying to "destroy the hype", and make no bones about it. I call things scientifically as I see them, but my point of view is a very specific one, that is not one you are used to hearing. With the most promising aspects of string theory, things like AdS/CFT, there are hordes of people willing to look at them from a very optimistic point of view, and emphasize the possibilities that they might lead to. I don't think it's my job to be as optimistic as possible about these things, there's plenty of that already.

Actually I've often written in a skeptical way about SUSY QFTs, especially about whether the MSSM or SUSY GUTs have anything to do with reality. On the other hand, at least in the form of twisted supersymmetry, there is amazing mathematics going on in certain SUSY QFTs, even though I don't think anyone knows how to properly relate the mathematics of these theories to physical reality. The QFT whose observables are the fundamental 4-manifold topological invariants is mathematically very close to the QFT of the standard model. I find it hard to believe this is a coincidence. There's very deep mathematics, somehow related to supersymmetry, going on here, mathematics whose proper understanding might lead to new ways of thinking about the standard model.

37.
October 26, 2004

I am definitely *far* from being an expert about the impact of string theory to mathematics, but I couldn't resist this. It would be great if some real experts gave definite examples, but here goes:

"However, most of these interesting things really come from 2d conformal QFT, and I would argue that it is QFT which is having a huge impact on mathematics, much more so than string theory. Witten's Fields medal was for his work on the relation of QFT to math, not for anything he has done using string theory."

I am not sure if this is just a statement about what has happened up to now, or meant to be a prediction about the future impact of string theory to mathematics. If it is the latter, it would be clearer if you made it more explicit. Do you, for example, predict that the 11-dimensinal aspects of string theory will not stimulate mathematics in any appreciable way?

More to the point: Well, the perturbative expansion of string theory happens to be given by 2D CFT. To say that these results really come from 2D CFT, not string theory, seems to me like saying these results actually come from calculus, not QFT. Do you think that the things that came "really from 2d conformal QFT", like mirror symmetry, would have come our way if we weren't looking at the 2D CFT from the point of view of strings propagating in background spaces?

Furthermore, I am not sure *what* you would consider something as coming from string theory, "not just 2D CFT". Would you, for example, consider mathematical developements related to D-branes to be coming from 2D CFT, not string theory, because D-branes were discovered by a CFT calculation?

On another note: Even though I would not consider myself an advocate of string theory, and am definitely against the fanatic approach some string theorists seem to be taking, it seems to me that many of the strong criticisms you make of the subject have some personal reasons in the background, which hinder your ability to be objective. It is obvious that string theory hasn't been able to live up to its premise yet, but stating this as an objective fact and debating about the it (and speculating on the future) is one thing, dismissing the field at every possible opportunity is another thing.

I can understand the arrogance of some string theorists pissing one off, but string theory is not bullshit, even if it turns out that it doesn't describe the universe at the most fundamental level.

You don't seem to attack the zillions of people writing papers about various SUSY field theories and exploring their properties, even though it is obvious that many of those theories do not contain the standard model. These people are trying to understand the dynamics of SUSY QFTs with the hope that this will eventually help us understand something about the universe. Well, helping understand dynamics of SUSY field theories is one of areas where string theory has achieved a lot. If you think this is rubbish, tell the same to SUSY field theorists!

Once again, even if string theory turns out not to describe the universe at the most fundamental level, I think even just AdS/CFT proves that string theory is a valuable subject that gives us fascinating insights about the structure of the theories we have been using as building blocks.

I don't think there is anything wrong about debating on the eventual success of string theory as a fundamental theory. However, it seems clear to me that what you are trying to do is not just that, but it is to "destroy the hype".

I don't have problems with the latter aim, either, per se, but I don't think the methods you use for that purpose are scientifically honest.

z

38. **Doug**
October 25, 2004

While I find your crusade against the indulgences of string theorists enlightening, I'm also reminded of Stephen Hawking's observation concerning the standard model: "It's ugly and ad hoc," he said. So the case you make may be a case of the kettle calling the pot black. I think the state of modern physics, now in the throes of two, incompatible, 20th century revolutions, is looking more and more pathetic from an outsider's point of view.

Einstein said that "the reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is — insofar as it is thinkable at all — primitive and muddled." Actually, I think we are seeing the wisdom of this observation more and more clearly today. To my mind, Keat's ode is still the most compelling, and relevant, dictum of all. Not just because it rings so true, but also because it is so concise.

39. **sol**
October 25, 2004

As I read Lubos's comments about the beauty, of simplicity, something came to mind of what was linked by Chris W. in regards to the number theory, and a related article about Mona Lisa and Math. It triggered the relation, to the following

On page 65 of Hyperspace by Michio Kaku, *he writes, "Picasso's paintings are a splendid example, showing a clear rejection of the perspective, with woman's faces viewed from several angles. Instead of a single point of view, Picasso's paintings show multiple perspectives, as though they were painted by someone from the fourth dimension, able to see all perspectives simultaneous*

Now I know there are a lot of good intellectual minds here. We cannot ignore I feel what could have existed in the realization of the compactified dimensions(how did you get there ?) as we view the four spacetime.

As you look through the historical, recognize the Euclide's fifth postulate, and recognize a geometry that was being lead through, or as Linda Henderson writes,"*the fourth dimension and non euclidean geometry emerge as among the most important thgemes unifying much of modern art and Theory."*

This might seem insignificant to many of you, but without it there is no possbility of entertaining the subjects of Cubist art perspectives and Monte Carlo effects of quantum gravity, as a perception that hopes to explain the geometry of a world that we are having diifficulty explaining.

I hope this idea of perception and the ideas of this simplicity is more or less asking that we look around the picture of Picasso, and see that the views of Mona Lisa smile, ask that we not look at her mouth directly.:)

Is this where the landscape issue originated?:)

Chern-Simons-Witten, Some History

October 26, 2004
Categories: Favorite Old Posts, Uncategorized

Lubos Motl is promoting a [revisionist history] of topological quantum field theory according to which it was all inspired by string theory. Unlike him, I was working on the subject at the time it was developed, and remember the history quite clearly. I've recently checked my memories against the literature, learning some more details of what happened back then. Here's an outline of the history of TQFT (or at least of one small part of it, the part leading to Witten's Chern-Simons theory):

1982: Witten comes up with a beautiful reinterpretation of Morse theory in terms of supersymmetric quantum mechanics, writing an extremely influential paper on "Supersymmetry and Morse Theory", which is published in a math journal, the Journal of Differential Geometry.

Spring 1987: Atiyah conjectures that Andreas Floer's new homology groups (inspired by Witten's supersymmetry and Morse theory paper) are the Hilbert space of a QFT. There are two cases where Floer theory works: 1+1 dimensions where the observables of the QFT would count curves (later to be known as Gromov-Witten invariants), and 3+1 dimensions where the observables count instantons (Donaldson invariants). Atiyah conjectures the existence of two corresponding QFTs, and also notes that the new knot polynomials of Vaughan Jones might correspond to a QFT in 2+1 d. He talks to Witten about this and gives an amazing lecture at a conference at Duke explaining these ideas. Witten tries to find a supersymmetric QFT that will do what Atiyah wants, but initially doesn't succeed.

Late 1987: Atiyah visits Witten again at the IAS and keeps after him about the TQFT idea. Witten finally realizes that things work if he uses a "twisted" version of N=2 supersymmetry.

February 1988: Two papers by Witten appear, one "Topological Quantum Field Theory" about the 3+1 d case, one "Topological Sigma Models" about the 1+1 d case. The second paper contains some vague speculation at the beginning about the relation of these "topological strings" to physical string theory, perhaps in some kind of "unbroken phase". At the end it also contains a sketch of an attempt to get Jones polynomials by using a 3+1d TQFT that would couple together his 3+1 topological gauge theory with a topological sigma model on the worldsheet swept out by a knot in 3 dimensions moving through time. This doesn't actually work.

Summer 1988: At a conference in Swansea, talking to Atiyah and Segal about Segal's ideas about conformal field theory and "modular functors", Witten realizes that the right theory to get Jones polynomials is a 3d QFT whose Hilbert space is the finite dimensional space of conformal blocks of a 2d WZW theory. He also realizes that one can think of the Lagrangian of this theory as being the Chern-Simons functional. His paper "Quantum Field Theory and the Jones Polynomial" appears in September. There's not a word about string theory anywhere in it and he has completely abandoned the idea of relating Jones polynomials to topological sigma models.

I was in Berkeley at MSRI for the academic year 1988-89. In January there was a workshop there involving Atiyah, Bott, Witten, and many other mathematicians and physicists. Initially many of the mathematicians were a bit skeptical, but by the end Witten had convinced the skeptics that what he had made complete sense, and they were very impressed. In the summer of 1990 he was awarded the Fields Medal for this work.

New ideas about relations between branes, topological strings, and Chern-Simons appeared about ten years later, and that's an ongoing story, one which Lubos conflates with what was going on in 1988-9 that got Witten the Fields medal. These are two completely different stories.

Comments

1. October 31, 2004

   DRL,

   I assume you understand the context in which the instrument is being offerred for introspection?:)

   [It is important that, while the argument presented in this paper can only be conclusive in a full treatment including gravity, it also indicates where one should look for a quantum mechanical theory that encompasses gravity. It makes clear that any approach to quantum gravity should treat matter and spacetime as two manifestations of the same thing. An approach that puts matter on a spacetime will encounter the cosmological constant problem.]

   So here we might find [Robert Hellings] currents comments on his blog hepful?

   He offers this link for [consideration] and for me Witten summarizes for us as well, in regards to Glast determnations.

   I would like to understand Robert's position very well.

   Leading to the undertsanding of kinetic potential values would say to me that the evolution of this instrument, is a continued effort of understanding Mercuries orbits? IN the quantum perceptin reductionistic physics asks us to consider something else, so while the quantum ranger suggests one position, we have to ask about the other?:)

2. **D R Lunsford**
   October 31, 2004

   Oh priceless! I love the harpsichord reference!! Well done! So string theory fails the Bach test..

   "Which came first, the string or the plectrum?"

   -drl

3. **Quantum_Ranger**
   October 31, 2004

   Lubos – "regarding the predictivity of string theory: be sure that string theory predicts a precise value for the mass of the electron"

   The electron rest mass,symbolized me,as measured when its speed is zero relative to an observer?

   What came first, the string or the Plectrum?

   The precise value is based on the two paramiters of OBSERVATION and ELECTRON, one see's and one cannot be seen!

   Defining the accuracy is like stating that string theory, predicts and proofs the unpredictable, using observable quantities?, but only if one subscribes to the notion that this occurs as a process of 'Seeing' without looking!

Its ok if you dont look, but just incase anyone happens to be 'thinking' of looking, string theorists like to hedge their bets on every conceivable notion of WE TOLD YOU SO!

As if.

4. **DMS**
   October 27, 2004

   "I don't have any morons comparable to DMS and similar stuff.

   There is absolutely no way how could I ever lose this argument, and I am sure that you know it very well – unlike DMS, who probably really believes that your position is defendable. Of course, if someone has excrements instead of brain inside his skull, like DMS, he will never understand that maths and physics did not end in 1989."

   Seems like I have touched a nerve 🙂

   But if one can dish it, one should be be prepared to take it.

   Well, I really don't care what Prof. Motl thinks of me. I actually learn from his posts about interesting work going on string theory and I (seriously) thank him for it; it is a valuable public service.

   But I know enough particle theory (and some mathematics) to know where factual statements end and heavily biased opinions ("String theory is the mother of everything") begin. I simply read the arguments and drew my own conclusions.

   DMS

5. **sol**
   October 27, 2004

   DRL,

   Oops, I posted last my last comment in the wrong spot:)

6. **D R Lunsford**
   October 27, 2004

   Sol,

   In the same post you imagine the Mona Lisa on a trampoline, then let it drop in a sort of passive-aggressive way that Einstein was senile. The juxtaposition of these fantasies struck me as ironic.

   Forgive me for believing that Einstein will yet have his day. I do NOT forgive you for joining the ranks of Einstein and Dirac bashers.

   -drl

7. **Thomas Larsson**
   October 27, 2004

   I can think of no better way in which to steer promissing physics students AWAY from string faerie fantasies than to have them read the blog and usenet rantings of Lubos M-
   I often wonder if he is some kind of LQG activist using obvious reverse psychology on his readers to repulse them from strings- becasue that is what happens most of the time!

   /:set\AI:

   This idea has also struck my mind. However, in my experience things are usually precisely what they seem to be. In fact, this is one thing that makes me think that string theory is wrong. String theory seems to disagree with experiments, and thus I believe that it actually does disagree with experiments. LM seems to be a rabid string zealot, and thus I believe that he actually is a rabid string zealot. But I guess that only shows how naive I am.

8. **Aaron**
   October 27, 2004

   The citations for Witten's Fields Medal are available online here for the curious.

9.
   October 26, 2004

   DRL,

   Pardon me Sol, but it's hardly a discourse when factual evidence from a direct participant is attributed to cerebral intra-aural doo-doo. LM doesn't do discourse, he barks like a carvinal shill, and he's usually wrong to boot. You can't have a discourse with a living scowl.

   This is a sideline issue about Witten, but there are more important things that are mathematically being describe, and they seem to have a place in the hyperdimensional reality.

   A picture crossed my mind today with the Mona lisa on the trampoline. I do not think it so absurd that the definition of Ramanujan's modular functions point on the stringy world sheet less relevant then other topological considerations in that abtract space(I'm still learning).

   It had to be consistant with what we know, and accepting Einstein's position in regards to the beauty of GR(smolin's positon?), and attempts to explain these ideas on a quantum mechanical scale have been a most troubling issue to contend with.

   Einstein did it for the last thirty years of his life, and we would have those from the quantum mechanical perspective who would of thought Einstein Senile? Yet his concept was complete, yet he decides to contend with the issue of a quantum gravity perspective, for the rest of his life?

Why I needed to understand whether Peter supports Smolin.

10. **D R Lunsford**
October 26, 2004

Pardon me Sol, but it's hardly a discourse when factual evidence from a direct participant is attributed to cerebral intra-aural doo-doo. LM doesn't do discourse, he barks like a carvinal shill, and he's usually wrong to boot. You can't have a discourse with a living scowl.

11. **sol**
October 26, 2004

Unfortunately I have to add my comment here not to slight any individual, but to encourage two indiviudals to continue the discourse they are having, about views of Witten, regardless of whether the fields medal was awarded for this or that.

Those who intrude with comments, did not pay attention to the focus, on the landscape, instead focused, on the Mona Lisa's mouth:)

Maybe you like Salvador Dali's's version better?:)

Previous, a discussion took place here in Peter's Blog on Susskind and Smolin. I would like to know if Peter supports Smolin's position?

NATHAN MYHRVOLD

I found the email debate between Smolin and Susskind to be quite interesting. Unfortunately, it mixes several issues. The Anthropic Principle (AP) gets mixed up with their other agendas. Smolin advocates his CNS, and less explicitly loop quantum gravity. Susskind is an advocate of eternal inflation and string theory. These biases are completely natural, but in the process the purported question of the value of the AP gets somewhat lost in the shuffle. I would have liked more discussion of the AP directly

http://www.edge.org/discourse/anthropic.html#myhrvold

The thing I like about the oppositon of minds who embrace the Solvay attitude, is that it forces another to bring forward a history that few of us would have seen. So outside of the comments of opposing views what kind of harmony could have been produced?

So in following up, I thought it would have been a good opportunity to bring this debate forward again, in context, of opposing views?:) Why do you think Susskind wrong or Smolin wrong? Is there is a certain accpetance here, of what one chooses, that another view is recogized? A certain symmetry?:)Etc.

SMOLIN VS. SUSSKIND: THE ANTHROPIC PRINCIPLE

Leonnard Susskind and Lee Smolin

While this is a conversation written by physicists for physicists, it should nonetheless be of interest for Edge readers as it's in the context of previous Edge features with the authors, it's instructive as to how science is done, and it's a debate that clarifies, not detracts.

http://www.edge.org/documents/archive/edge145.html

Sorry for intruding, and all due respect to you both, Lubos and Peter.

12. **D R Lunsford**
October 26, 2004

Toutes-vous avez des excrements entre des oreilles! Vive les cordes! Vive les fonctionnes thetaines! Vive les blogs de savants-des-cordes! These things sound better in French! Voulez-vous des pommes frites avec cela?

M. de Bois-Motile, Compte de Bourbaki

13. **Mubos Lotl**
October 26, 2004

Let me begin by reminding you all that you are all fools, morons, idiots, professors at MIT, monkeys, and have excrement between your ears. Right, with that out of the way, let me assure you that I am right about everything I have ever said, so resistance is futile, is not it? Finally, about all those taunts
regarding the predictivity of string theory: be sure that string theory predicts a precise value for the mass of the electron, modulo a certain p-adic cohomology class which is of no interest because Andy Strominger already mentioned it in 1992 and so a computation of it would be entirely superfluous.
Yours in eternal friendship,
Mubos.

14. **Fabio**
October 26, 2004

"Of course, if someone has excrements instead of brain inside his skull, like DMS…"

Years of debating usenet cranks have left poor Lubos with a wit as sharp as a plastic knife.

15. **/:set\AI**
October 26, 2004

oops- the last comment was by me- name got dropped from the post for some reason

16.
October 26, 2004

I can think of no better way in which to steer promising physics students AWAY from string faerie fantasies than to have them read the blog and usenet rantings of Lubos M-

I often wonder if he is some kind of LQG activist using obvious reverse psychology on his readers to repulse them from strings- becasue that is what happens most of the time!

17. **Peter**

October 26, 2004

Hi Lubos,

No, the Fields medal is not given for a single paper. Atiyah's address to the 1990 ICM about the award to Witten explains what work of Witten's mathematicians considered important at that time. String theory isn't mentioned. I can assure you from my many conversations with mathematicians about this around that time that the Chern-Simons theory stuff was the deciding factor in awarding him the medal.

Witten's work on the the positive mass conjecture and Morse theory, both using supersymmetry, were also cited by Atiyah. But this work was done around 1980, long before Witten got interested in string theory. It really has nothing to do with string theory, unless you adopt the attitude that string theory encompasses everything.

18. **Lubos Motl**
October 26, 2004

I found a web page that explains, in detail, that he received the medal also for his proof of the positive mass theorem, using supersymmetry – a clearly superstring-inspired proof – and the relations between supersymmetry and Morse theory. In that work he used a supersymmetric quantum mechanical model – which is a typical stringy approach because all other (nonstringy) people who are interested in SUSY are focusing on d=4.

19. **D R Lunsford**
October 26, 2004

Bon-Motl has once again invoked "argumentum ad cellulosum". My understanding is, if this happens 3 times in the same month, Einstein's ghost will tear the universe a new nether region and we'll all be sucked through it into the missing 11 dimensions, in a process called "Calabi YEOW". I mention in passing that Halloween is near, and toying with the unseen world at this dicey time is just plain uncivilized.

So, for God's sake, let's act before it's too late. Millions of lives are at stake.

20. **Lubos Motl**
October 26, 2004

One more comment, Peter:

This "Fields medal only" is a completely ridiculous game. By the way, can you show that the medal was given him for a specific paper? No one around is aware of it. I personally don't think that it is important for anything, but it seems that even these statements about the nature of Fields medal – is something that you invented.

21. **Lubos Motl**
October 26, 2004

Dear Peter,

I must admit that even though some of the contributors to my blog (comments) may be disappoitining, I don't have any morons comparable to DMS and similar stuff.

There is absolutely no way how could I ever lose this argument, and I am sure that you know it very well – unlike DMS, who probably really believes that your position is defendable. Of course, if someone has excrements instead of brain inside his skull, like DMS, he will never understand that maths and physics did not end in 1989.

I don't feel that it is quite appropriate for us to waste so much time with these idiots, but I must state it clearly, in a language that attracts enough attention, that this whole Peter Woit's way of describing history of maths and physics is simply a result of the fact that Peter Woit, as a talented mathematician and physicist, died back in 1989, and unless he will be able to wake up, he will never be able to say anything reasonable about physics and maths after 1989.

And be sure that physics of 1990s has brought many key new discoveries, much like physics of the 1980s.

http://www-spires.slac.stanford.edu/spires/find/hep/www?rawcmd=FIND+A+WOIT%2CP+and+date+before+2000

Best
Lubos

22. **Peter**
October 26, 2004

Hi Lubos,

As I said in my initial posting, the whole topic of the relation of string theory to mathematics is a huge and complicated one and I don't have the time or energy to do it justice now. However, as I also said in my initial posting, the work that mathematicians in 1990 thought was worth a Fields medal was not work that came out of string theory. You claim that I am wrong about this, I've presented a factual history to back up what I said. If you have any facts to demonstrate that the work for which Witten was given the Fields medal came out of string theory, let's see those facts. Show me a pre-1990 paper explaining Chern-Simons theory in terms of topological strings.

Peter

23. **DMS**
October 26, 2004

When losing an argument,

get personal

"1989: Peter Woit loses any contact with theoretical physics for at least 15 years. This prevents him from seeing all new important connections between these subjects that were found between 1989 and 2004."

or move the goalposts,

"Moreover, it's just incredibly silly if someone judges the importance of ideas according to some fictitious links between a specific paper and some medal."

or, better still, change the subject

"Moreover, this CS story is just one of tens of important examples how string theory – not just "field theory" – influences math. Another class of these influences is associated with mirror symmetry."

but never accept defeat.

24. **Lubos Motl**
October 26, 2004

I apologize that I must write it so explicitly, but it is sort of necessary.

Your history is missing one important entry.

1989: Peter Woit loses any contact with theoretical physics for at least 15 years. This prevents him from seeing all new important connections between these subjects that were found between 1989 and 2004. Some of these connections are described in my "revisionist" history. This lost contact with reality in science also implies that even in 2004, Peter Woit writes entries on his blog that are very far from being up-to-date. These entries may have been fair in 1989, but they don't express the correct relation between the ideas as understood in 2004.

Moreover, it's just incredibly silly if someone judges the importance of ideas according to some fictitious links between a specific paper and some medal. If you care about these irrelevant medals so much, there are also different stories.

In the spring 2004 or so, for example, Witten received the national science medal from the US president, and be sure that it was not mainly because of the only papers you want to see. No doubt, he is credited for many other discoveries that you don't want to see, and most of us think that these other discoveries are more important.

Today, it is also very important that the 3D CS-theory is the worldvolume theory living on the D-branes of the A-model, and it allowed completely new methods to calculate all these invariants – correlators of the Wilson lines etc.

Moreover, this CS story is just one of tens of important examples how string theory – not just "field theory" – influences math. Another class of these influences is associated with mirror symmetry. I hope that you would not argue that even *mirror symmetry*, which has been converted to many rigorous statements in mathematics, has nothing to do with string theory.

Symmetry Magazine

October 27, 2004
Categories: Uncategorized

SLAC and Fermilab have joined forces and replaced their "FermiNews" and "Beam Line" publications with a new one called "Symmetry". I like the title; it's nice to see that the major US particle physics labs are supporting a publication about group representation theory.

Comments

1. **sol**
   October 28, 2004

   A layman pondering apple pie

   The Nature of Quantum Mechanics by Gread't Hooft

   But below we have some guiding principals to live by?

   Question:**In conventional QM and QFT unitarity is the consequence of the time translation symmetry of the dynamics. In GR there isn't, in general, an analogous notion of time translation symmetry**

   A lot of people (in fact, most people) argue that this is not a problem. They are wrong. It is very closely related to locality. How can you have locality in a theory where there is no a priori notion of distance? This is really a serious difficulty, and it had made some deep thinkers (read: 't Hooft) so desperate that they are even willing to abandon quantum mechanics. In the beginning of the talk at(above links in my postings) , 't Hooft explains that he simply believes in locality, and that this is why he explores these weird ideas about hidden variables. It is interesting that the inventor of holography has such a strong belief in locality.

   It all sounds spooky to me. How do you identify the smell of apple pie? It tastes like…..?:) From one island to the next????

2. **sol**
   October 28, 2004

   Mathematics is always a continuum, linked to its history, the past – nothing comes out of zero *Atiyah*

   How soon I forget sometimes:)

   I have a story about two monkey's on a island that I thought funny, but hey, we can work with "shit for brains", like mom's apple pie wafting through the neighborhood.:)

3. **RT**
   October 28, 2004

   It's the trivial things in life that make the difference. The re-sizable/scrolling window is indeed a boon. Its former immutable form was almost as distressing as the recent (let's not mince words) 'shit for brains' interlude. Thanks

4. **D R Lunsford**
   October 28, 2004

   Thanks! That worked beautifully.

   -drl

5. **Peter**
   October 28, 2004

   Hi Danny,

   I made the change suggested on the link you posted, resizable comment windows seem to work now.

   About Atiyah's work:

   He's actually one of the clearest writers of any mathematician, so one piece of advice is to just get ahold of the five volume set of his collected works, and try reading, starting with the expository pieces he has written, of which there are quite a few. Unfortunately I don't know of many more readable expositions of Atiyah's work than his own. For certain specific things that have made their way into physics (such as the basic index theorem for the Dirac operator), there is some physics literature.

6. **D R Lunsford**
   October 28, 2004

   Peter,

   What would a set of books for "Atiyah 101" look like? I want to understand this business with the index theorem, at least as far as is necessary for physics.

   -drl

7. **D R Lunsford**
   October 27, 2004

   OT – Peter – try this:

   http://www.ericsiegmund.com/fireant/archivesmt/000342.html

8.

October 27, 2004

This nothing point(?) sure comes up a lot.:)

If you asssumed a false vacuum, and that a true vacuum could emerge, there really wasn't **nothing** at all?

In fact, our universe could once have been so symmetrical that it amounted to nothing at all. "Nothing" is as perfect a symmetry as you can imagine, since there's nothing you can do to it that makes a difference. This nothing would have been unstable, however?like a pencil balanced perfectly (which is to say, symmetrically) on its tip. And that means?as Frank Wilczek has put it?the answer to the question "Why is there something rather than nothing?" would simply be that "nothing" is unstable.

9. **Peter**
October 27, 2004

Hi Danny,
If someone can point me to a source of information that explains how to do this, I'll look into. In general I don't want to invest much time in messing with Movable Type. As far as I can tell the thing to do is to migrate this at some point to Word Press, a project that will have to wait for some mythical point at which I have some free time.

10. **D R Lunsford**
October 27, 2004

OT – Peter, is there anything you can do to MT so that IE popup windows are not fixed in size and menuless? It works fine on Linux but not on Windows (go figure).

(In case I was not clear, the posting window cannot be maximized or resized at all, in fact.)

-drl

11. **D R Lunsford**
October 27, 2004

Well I for one endorse the defense of Bumo-er-Lubos, and hope that he will not in future refer to excrements in any place or context.

But we should stick to physics, yes.

-drl

12. **CW**
October 27, 2004

..and he has been known to express his interest in loop quantum gravity by suggesting that its practitioners be deprived of research funding. (See some of postings on sci.physics.research.)

13. **Fabio**
October 27, 2004

You've got it backwards. It was Lubos who introduced "excrement for brains" into the discourse here. He's shown himself to be quite thin skinned and quick to go personal when someone starts to get the better of him.

14.
October 27, 2004

C'mon. Whoever is writing these mock Lubos Motl entries needs to stop. This blog is about physics, not mockery and insults.

Some of you might not realize that the reason why Lubos spends any time on this website is that he is, at least in part, sympathetic to some of its views. Quite reasonably, therefore, he is compelled to defend string theory and his research. His point of view is defendable.

It would be a shame if Lubos and other string theorists stopped using this website on account of unprofessional attacks. Let's keep our eye on what the real issues are and debate them.

15. **ltom sobul**
October 27, 2004

This is obviously an implicit endorsement of string theory by the two top US accelerator labs, since string theory has more symmetries than any other theory.

Georgi on “Women and the Future of Physics”

October 28, 2004
Categories: Uncategorized

Howard Georgi gave a colloquium at Fermilab last week, and the slides and video from his talk are now online. He has gathered quite a lot of interesting data about women in the various sciences at the undergraduate and graduate level, and he discusses his experiences at Harvard over the years as he became more aware of the problems experienced by women studying physics. As chair of the department and in other capacities, he has tried to understand why there are so few women studying physics, significantly fewer than in the other sciences, concluding that “Many of our women physics concentrators were trapped in an emotionally abusive relationship with the Harvard Physics Department!!!”. He also concluded that it was “past time to outgrow the hypermacho lone-ranger approach to physics”, and that this would make the field more fun for everyone.

The whole issue of why so few women study physics (and math) seems to me a complicated one since it is mostly about the very complex and tricky ways in which people deal with how others expect them to fit into certain behavior and roles appropriate to their gender. I don’t think the “emotionally abusive relationship” that Georgi describes the Harvard department as having with its students is limited to the female ones. While I can say that in many ways I very much enjoyed my time as an undergraduate there, the great majority of the faculty were less than friendly to the students (with Georgi a prominent exception), and the general level of social skills of both the faculty and many of one’s fellow students left a lot to be desired. According to Georgi, changes have been made to the culture of the place and it is much more encouraging of its students. This is part of a general trend at many US institutions, partly because of increased sensitivity to gender issues, partly just because the students are paying a lot more to be there than they used to, and their increased dollars get them increased attention and respect.

Then again, they now have Lubos Motl, so the Harvard department’s traditions of hyper-aggressive behavior have not totally been lost.

Comments

1. November 3, 2004

   The election is not that interesting. Get to it guys. As Kurt Kobain had it: here we are now, entertain us

2. **Matti Pitkanen**
   November 2, 2004

   drl: Matti – that is – well, not even wrong.

   I have no personal opinion about this. I just wanted to bring in a new point of view.
   I am not very convinced concerning the purely statistical criterion used. Believe or not, it was comparison of ratio for the lengths of index finger and ring finger: on the average 1 for math oriented and .98 for non-math oriented persons!

   Matti

3. **D R Lunsford**
   November 2, 2004

   Matti – that is – well, not even wrong.

   It is undeniably true that men are better at making cognitive maps. This is almost certainly related to skill at math, which is necessary for skill at physics theory. “Grothendieck believes…” SO? “Motl believes…” SO? I don’t give a fig what anyone believes.

   In any case, none of this means a damn thing. The phenomena are all there, not the least bit interested in the sex of the observer. The pebbly shore of the great ocean of truth is open to all waders.

   -drl

4. **Matti Pitkanen**
   November 1, 2004

   In the last New Scientist (20 October 2004) there is an interesting article “Career choice begins in womb” according to which a good mathematician or physicist might have a more feminine brain. There is evidence that men in these disciplines are being exposed to a female pattern of aestrogen and testosterone in the womb. The absence of women from mathematics and physics would therefore look even more a cultural phenomenon (around 1984 immediately after super string revolution I participated a conference in which there were around 100 men and not a single woman: quite an experience!).

   Grothendienck has mentioned that he believed of having more feminine than male brain. In some New Scientist for some weeks ago (unfortunately cannot give the issue number) there was a little article mentioning that mathematician’s brain hemispheres are in more intense communication making possible rapid interaction between holistic and analytic modes of thinking. Higher connectedness indeed characterizes feminine brain.

   Matti Pitkanen

5. **Plato**
   November 1, 2004

   Maybe the female and male tendencies use the neurons, to manifest ideas? That would mean gender, has nothing to do with what the brain manifests, just that it does?

6. **Frank**
   November 1, 2004

   An old problem…. but consider this angel: Why do we try to get more women into physics but, for example, vets or literature don’t try to get more men?
   In Germany there are more women then men studying to begin with. If you balance the sciences you’ll end up with vastly more women studying. This alone indicates that an isolated women oriented physics program must fail. You could also ask why so many men study physics, and try to curtail the male population in the degree. Doesn’t sound entirely logical does it?

   Any reasonable approach to the issue would by neccesity need to be interdisciplinary and look at both genders.

7. **sol**
October 31, 2004

Should read with link:

One hopes that finding psychological processes embedded in thinking might have found some benefit in seeing GHZ entanglement as a process that brings forth mulptiple firing of neuron connectors(maybe like a supersymmetrical reality without the brain burning up?).

8. **sol**
October 31, 2004

One hopes that finding psychological processes embedded in thinking might have found some benefit in seeing GHZ entanglement as a process that brings forth mulptiple firing of neuron connectors(maybe like a supersymmetrical reality without the brain burning up?).

I had this under the heading of Venn Logic and Transactional analysis

Human Experience Masks Inherent Patterns

Parent EGO

As we grow up we take in ideas, beliefs, feelings and behaviours from our parents and caretakers. If we live in an extended family then there are more people to learn and take in from. When we do this, it is called introjecting and it is just as if we take in the whole of the care giver. For example, we may notice that we are saying things just as our father, mother, grandmother may have done, even though, consciously, we don't want to. We do this as we have lived with this person so long that we automatically reproduce certain things that were said to us, or treat others as we might have been treated.

http://www.businessballs.com/transactionalanalysis.htm

In this the EGO states would be extremely important, and if such thinking is harmonically driven, then what would one hope to accomplish in the adult ego?

Sly words of Osama Bin Ladin, about kings, and states, in light of countries and their leaders? His distain for life, and we discover another Wotan as hitler disguised?

In cosmological proportions, does it matter if it is male or female? I don't think so, as the emergence of neurons are projected by the minds thinking?

So maybe in Sean's Preposterous universe, that CW just linked again, one might have to see the context of Andrey Kravstov's early universe as , the neuron connectors of the cosmos, as strings that eventually clump, as neuronic discriptors? Look at my comment there under Arrow of time.

Entropic consideration in belief structures, and ideas, theories, when at a early time, it was all one?

9. **Jesse**
October 31, 2004

Lubos wrote:
Observationally, the number of neurons is correlated with the probability that a person ends in physics.

What's your source for this claim? Do you believe there is less of a correlation between neuron number and the probability a person ends up in math? If not, why are there more women in math than in physics?

10. **sol**
October 30, 2004

I just wanted to share a interesting perspective.

DocN,

Jean Shinoda Bolen, M.D. Ring of Power was interesting.

Strange that we could have seen A Jungian Understanding of the Wagners Ring cycle, portrayed in todays world and how could have this been accomplished. But by re-introducing a fictional story and embueing it with the archetypal structures of what Jean Shinida Bolen called, "The Abandon Child, The Authoritarian Father, and the Disempowered Feminine."

I was very intrigued by the way in which this story was used and applied. A Alice in Wonderland Fairy tale and what could we have learnt? Is it unrealistic to give such fairy tales the substance of importance, not to have recognized the structures underneath, and to have recognized the way in which things could have been transmitted? Have mathematicians been guilty of same?

Another mask I suppose, but it gave the flare of understanding that we might of not grasped otherwise and how deep a impression(emotional imprinting) could have been dealt with such attempts?

I think we have learnt to conceal our histories in our view points. Learnt to build structures. Have we missed the emotive consequences of such attachments, of that same history?

Imagine, Mother branes and baby universes and the intellectuals have never really left home:)Abstract spaces, like dreams?

Mothers and Fathers are in all of us? As a harmonic principle, selective actions above intellectual considerations that are sound judgements(identified) particle considerations?:)

It's all creative math and Physics?????? about the nature's reality, recogizing these principles in our natural world.

Regards

11. **Lubos Motl**
October 30, 2004

http://motls.blogspot.com/

I've expanded my article, added links to many documents about the difference between male and female brains, and analyzed "aggressiveness, competitiveness, and arrogance".

12. **Arun**
October 30, 2004

Three proposed explanations for why the proportion in women in physics is small, that have been proposed are:

1. Nature (i.e., different biology of men vs. women)
2. Nurture (i.e., differences in how society as a whole brings up boys and girls)
and
3. The culture of physics itself (not the content of the science, but the way physics is taught, faculty are appointed, how groups of physicists interact among themselves, etc.)

Now, while the representation of women in the higher levels of mathematics seems to be as dismal as that in physics, at least as of 1997 in the US (see the graph http://www.physicstoday.org/pt/vol-53/iss-7/p52b.html)
women were taking about 45% of the bachelor's degrees in math as compared to less than 20% in physics.

It would be interesting to see arguments that these figures do not support the explanation #3; or that these figures do not rule out #1. and #2.

-Arun

13. **Lubos Motl**
October 30, 2004

Hi Arun! I've already answered this question many times.

Observationally, the number of neurons is correlated with the probability that a person ends in physics. There are good reasons to think that this correlation has a biological explanation, but the system under consideration is just too complex to make any definitive statements about it, and moreover it is not my field and you can ask specialists who know much more about the interplay between brain's anatomy and physiology.

On the other hand, there is no doubt that one of the most important evolutionary steps that we had to make since the era we were monkeys was a growth of the cortex – the advanced part of brain. Because of this difference, chimps and gorillas only have 7-9 billion neurons, and be sure that this is one of the numbers that makes a difference. (Rats have 65 million only.)

14.
October 30, 2004

"In contrast, 47 percent of physics faculty members in Hungary […] are women"

Although i don't know the 1994 data, but as a hungarian student, i can assure all of you that today, this is very far from reality. I checked the homepages of the three most prominent hungarian universities, and my guesses are about 5%, 5% and 10%, respectively (and these are upper bounds). Math depts are not better, either.

15. **Arun**
October 30, 2004

Sorry! Here is the
http://www.physicstoday.org/pt/vol-55/iss-5/p24.html

16.
October 30, 2004

That was supposed to be a hyperlink, right? The href attribute was omitted. -C

17. **Arun**
October 29, 2004

Physics Today, on the first International Conference on Women in Physics

Quote: Communication among physicists, particularly in the US, was described as "combat physics": At talks and in individual conversations, explains Kim Budil of Lawrence Livermore National Laboratory, "you'd like people to engage in scientific discourse, but it often goes beyon that to become a fight for ego, to decide who is the smartest person in the room."

Lots of other interesting stuff there.

-Arun

18. **Arun**
October 29, 2004

Lubos,

I'll ask again – how is the ratio of neurons between men and women's brains relevant to the observed gender ratio in physics?

-Arun

19. **Lubo? Motl**
October 29, 2004

Those things are certainly relevant if we want to study the physiology of brains, and compare different types of brains.

You must say "relevant for what" if you ask if something is relevant. You know that if it were totally irrelevant for anything, I would not have written it.

You have probably misunderstood why I said that comment about the countries. I said it because the American science – which happens to be the scientific community with the large portion of women, as you say – is one of the successful ones, and it is conceivable that the competitive environment that does *not* try to respect any quotas is one of the reasons why it's successful.

In other words, other societies can artificially attract proportional shares of different groups of the population, but such an engineering is counterproductive for science as such.

20. **Arun**
October 29, 2004

So, Lubos, you agree that the following is mostly irrelevant?

"The male and female brain work differently in details. The average male brane has 20 percent more neurons than the average female brain. The latter has more connections between the neurons than the male brain. When thinking about language, one can show that both female hemispheres, but only one male hemisphere, is active, and so forth."

Or do American brains have more neurons than brains of other folks? Does immigration to the US suddenly increase the number of neurons in the brain?

-Arun

21. **Lubos Motl**
October 29, 2004

Well, the percentages of women in individual professions are different in various countries, and the variations are clearly a result of a different culture and different policies.

But you may also want to notice that the results of scientific research in different countries are also different, and it is the same United States in which the results seem to be better.

If you want to do some social engineering, and you have the politicial power to do it – there is nothing easier than to attract more people from some category to some profession. You just define some new advantages, and so forth. I am just not sure whether you really want to do such a thing.

And don't forget that quotas are unconstitutional in the USA.

22. **Arun**
October 29, 2004

Some other opinions:

Women in Science blog

Stubborn Equation Keeps Women on the Minus Side

Is the physics classroom any place for girls?

23.
October 29, 2004

Very interesting links, Arun. I found the following quite perceptive:

"One Latina woman told Ong that in her all-female study group, women feel comfortable voicing their uncertainties and hunches with language like "I'm not really sure about this, what do you think?" However, they have found that those same phrases, in a mixed-gender group, more likely would be coded as "I don't know what I'm talking about, I'm really dumb," and so women who persisted in physics have learned to speak with more assertion when working with male peers."

However, she is wrong below 😉

"The woman told Ong she found an irony in this approach: Good scientists never claim to know something absolutely, but instead deal in degrees of uncertainty. "

24. **Arun**
October 29, 2004

And afterwards, Lubos, if he is so kind, can go over to MIT and quiz this physicist who wrote

Recruiting Women (to physics) Takes More Effort

Incidentally, these 1994 stats are interesting:

Of 20 countries surveyed for another paper cited by the authors, the United States was tied with Korea for the lowest percentage of women in physics faculties, at three percent, they noted. In contrast, 47 percent of physics faculty members in Hungary and 30 percent in the former USSR are women. In addition, only nine percent of US physics doctorates went to women during the period studied, though that figure was 21 percent for France, 31 percent for Brazil and 60 percent for the Philippines. Within America, some universities have made more progress than others in increasing the numbers of women in physics; "the variations from one school to another are enormous," Professor Dresselhaus observed.

Perhaps Lubos can explain these variations in terms of the different numbers of neurons in the brains of women from different countries. Based on what he wrote earlier, the average female/male ratio of number of neurons in Filippinos should be much higher than that for Americans.

In general, I am appalled that some people have such strong opinions without any regard for any data. On the other hand, this place is primarily for discussing string theory, so can anything better be expected?

25. **Arun**
October 29, 2004

Here is some actual research, and perhaps Lubos can interview the researcher for us, because she is in Harvard.

[Researcher Mia Ong: Physics ‘glass ceiling’ intact]

26. **Fabio**
October 29, 2004

I always had the impression that the Russian physics culture was more aggressive and macho than the American one, at least in the good old days of the cold war. How did women fare in that system? The only example that comes to mind is Kallosh.

27. **Lubos Motl**
October 29, 2004

I apologize, but as an alien I feel the moral right to say that the American self-confidence is one of the features – certainly not the only feature though – that makes America special in the modern world.

It’s perfectly fine if the students are quiet at all seminars and all classes. There’s nothing wrong with it, but there’s also nothing cool about it.

Vocal students that go over the edge may be counterproductive for the teacher and other students, but on the other hand, a certain degree of activity is nearly always helpful.

Most of European science is un-emotional, boring, lacking self-confidence and big ambitions. That’s just wrong, and God bless American science for its different atmosphere.

28. **DMS**
October 29, 2004

Interesting subject. I knew a couple of physics majors from Harvard and they echo what Peter said regarding faculty’s unfriendly attitude towards students. They did not feel they got what they paid for. Well, they are taxpayers (and some are even science advisors to senators and congressmen) and I am sure they are not as sympathetic to funding for theoretical physics, especially particle physics. It is best not to bite the hand that feeds. 🙂

Regarding rewarding of hyper-aggressive behaviour, that is partly an American phenomenon; I did not notice that too much from people of (most 🙂)other countries, but I may be wrong.

I also notice a lot more Asian women who take up hard sciences (electrical engineering, CS etc) than N American. One factor seems to me are the “women’s magazines” , which are anything but feminist. It is paradoxical since women’s rights are more advanced in US than Asia. But many women in US are in fields like medicine and law; is the culture less aggressive in such fields? From my experience, particle theorists are the most aggressive bunch 😉

Another factor is probably the teaching style, where often the teachers pay attention to the more “vocal/aggressive” students in the class, discouraging the quiter ones, be it male or female.

Actually, Georgi (with several female Ph.Ds who are faculty members)and S.T. Yau (Karen Uhlenbeck mentions the important role he played in her career when she was being dismissed by other male colleagues) are notable exceptions.

DMS

29. **Lubos Motl**
October 29, 2004

Many points are waiting here to be answered.

Concerning the discrimination in USA vs. Central and Eastern Europe: Arun, I said it because I believe that the higher employment and salaries of women that we “achieved” in socialism – with women who don’t have too much time for their children – is not such a great thing after all. I have no idea whether the American or European reality – concerning the respect to women – is “better”.

The ratio is female politicians is small almost everywhere, for example.

I agree with you that the level of women’s salary is the reason, and the perception of various acts being viewed as jokes is an effect, but unlike you, I am not so sure whether it implies that someone is more right. Don’t you think that you should ask: Aren’t those critics in Europe right? Is not our American feminist system ridiculous?

For the other participant: Well, if someone has other options, she (or he) can choose other options, does not she? It’s not clear to me what it means to “create an environment”. If it is a general feeling that an average woman is happier if she has a family etc., instead of physics career, then it is a general feeling, and her decision will be affected by her plans, her opinions, as well as general feelings of the people around. I don’t understand how someone wants to separate our culture from these decision.

I may also expect the physicists to be slightly antisocial, arrogant etc. – but the reality is very different. The physicists are usually very nice people. In many contexts, I view it as a symptom of a problem, but that’s a different issue.

Here we discuss the gender. I certainly don’t think that women have to be nicer than men, and if YOU believe the stereotype, then it is YOU who discriminates women.

Sorry, but the answer of the women who did not to too well at the exams is most likely rubbish. One can always invent a cheap justification of this quality to explain why she or he failed at the exams. My interpretation is that the women over there just had a smaller score than the men. That’s the experimental reality, and it is just pseudoscience and biased ideology if someone tries to invent a virtual reality behind it.

There exist exams in which women have a better score in average than the men, but if the previous group is more frequent, one should not immediately invent hypotheses that it is because of some “unfairness”.

I don’t believe that women and men in science are talking two different languages. In 2000, I remember a polemic I had with a crazy postmodernist on a party in San Francisco. He was wearing an obnoxious T-shirt with a coffee cup, if I remember well, and the T-shirt was expressing his opinion that science is arrogant (I forgot the details), and something like that. He claimed that he would always be able to figure out whether a physics paper was written by a female or male authors.

I am absolutely sure that I could give him a sample that he would guess much less than 50% correctly ;-), but unfortunately we lost e-mail contacts of each other.

These people are just incredibly stupid and obnoxious. They believe that physics is a matter of cultural creation, something invented artificially, something into which the scientists reflect their gender, race, and whatever else – and our physics, as we know it, is a male white physics that would differ a lot from a female black physics, for example. Of course, they believe so because they don’t believe in any objective reality or science. Don’t you agree that these people are just pompous fools and morons? I get very upset already when I realize that many of these people are able to acquire academic positions. 😉

30. **Chris W.**
October 28, 2004

The issue (Lubos!) is *not* one of providing special support for women so they can succeed in a demanding and competitive environment, and to prevent them from becoming discouraged and dropping out. The issue is creating an environment that talented, competent, and mature people can respect, ie, that they feel is worthy of their abilities and strengths, *knowing that they have other options*.

What Georgi is pointing out are fundamentally dysfunctional attitudes and behaviors, which are tolerated because, well, we expect physicists to be somewhat arrogant, antisocial, and clueless about the wider world (and sometimes just plain weird). More and more, that kind of excuse won't fly, if only because most employers can't afford it.

I recall reading a report on this topic in *Science* a number of years ago. It discussed a particularly demanding exam that students pursuing a physics Ph.D at Harvard were expected to take in the course of their careers there. It noted that female students tended not to do as well on the exam. The author of the piece interviewed some women in the physics department at the time and asked them about this. They said, yes, that was true, and it was because this particular academic gauntlet "was just too nerdy for an intelligent woman to take seriously."

Any professional subculture can become insular and obtuse. (Take stock trading and investment banking, for example.) That is really what is at issue here, not simply accommodating excluded groups. Of course admitting that one is a complacent member of a spoiled, insular, and obtuse subculture, and then doing something about it, is not easy.

—
It should also be noted that the place of fundamental research in our society is not so secure that its practitioners can afford to ignore issues like this. The demise of the SSC is an object lesson; there could be several more like it in our future, given the state of the federal budget.

31. **Arun**
October 28, 2004

Lubos:

You wrote : "….some quantitative measures show that women may be more "equal" in the Czech Republic – for example, their employment rate is higher, and the ratio of female over male salaries is higher than in the USA. No one considers these things to be overly sensitive issues in the Czech Republic, and everyone is making jokes about America where the feminists can sue you if you look at her inappropriately…".

You've put your finger on something important but IMO, you've got cause and effect reversed. Where women's employment rate is higher and salaries are more equal, discrimination seems like a distant joke, and no one is sensitive. Where discrimination is a reality, it hurts.

-Arun

32. **Dick Thompson**
October 28, 2004

Well it looks like the commenters have achieved their dream. A nice little flame war. I am waiting for Godwin's law to come into play

33. **Lubos Motl**
October 28, 2004

Dear DRL, if I imagine that you are an "intellectual", it's really not that hard to become a hyper-aggressive anti-intelectual. 😉

34. **D R Lunsford**
October 28, 2004

Mary,

Nerds of all sexes and colors are hated most everywhere. No monopoly for woman nerds. The US is fundamentally anti-intellectual. I've learned to cover up at work and not make waves.

PS I virtually offer to carry your books 🙂 Hang in there.

-drl

35. **Lubos Motl**
October 28, 2004

That's great that Mary wrote her statement – because it will force many of us to think about reality, and perhaps avoid a certain type of "help" that really does not help.

I totally agree that the girls don't need too much special treatment because of their hypothetical "weakness".

For example Melissa Franklin, my very interesting colleague, 😉 is a truly strong personality. She is one of the first female professors from some category – and under some circumstances, Peter Woit is – as a critic of string theory – a shy piece of soap compared to Melissa Franklin.

Melissa has no problem to sit with 11 string theorists to a table at the Faculty Club, and explain them that she thinks that they are symbols of degeneration of the physics community, and it's no fun to talk to them. In order to prove her point in detail, she picks a string theory postdoc and starts to ask him, at the end of the lunch, how is it possible that he did not say anything during the lunch! 🙂

Well, really, many women don't need any special legal support to get their point through.

What I find important is that the very feeling that the females are being positively discriminated must be pretty devastating for those ambitious girls who simply want to be equally good or better than their male (and other female) colleagues. By the positive discimination, they're being told: "If you do something fine or get a job or an award, it's just because you have this artificial support for your not having a penis." 😉

That's just an unpleasant state of affairs, especially because I know many girls in physics who simply do not need such an artificial support. Well, such affirmative action may help some other women to get the same jobs or awards as the talented ones, but I think it is just unfair, especially towards the talented females.

There should be no discrimination, neither negative nor "positive". We've lived in a regime based on the idea that the working class is being exploited by the "capitalists" and something radical must be done about it – and let me conclude that the idea that the females are being exploited by the men and radical actions must be taken – is a very similar idea that can lead to very similar results.

36. **Lubos Motl**
October 28, 2004

Yes, Arun, you are right that I don't know too much about the situation in American families, and how much the parents support the daughters to study physics, and so on.

But in the socialist bloc, we have just lived without this tension. Even though Americans often like to criticize some Czech traditions – e.g. when boys beat the girls during the Easter holidays 🙂 – some quantitative measures show that women may be more "equal" in the Czech Republic – for example, their employment rate is higher, and the ratio of female over male salaries is higher than in the USA.

No one considers these things to be overly sensitive issues in the Czech Republic, and everyone is making jokes about America where the feminists can sue you if you look at her inappropriately. 😉 The same situation like in Czechia is in Taiwan, for example, I was told by Melissa Liu, a female mathematician who has worked with Shing Tung Yau. Nevertheless everyone knows that the "natural" outcome will be that most girls will NOT choose specialized physics and engineering careers.

On the contrary, they will choose teaching (at high schools), and this may be a stereotype, but it may also be correlated with maternal instincts.

Also, if some parents just believe that a job is wrong for their son or daughter, they have the right to promote their opinion within the mantinels given by the law – they're the parents! And the children must often become rebels. At the end, I am pretty sure that every parent is happy if her or his daughter becomes a skillful engineer or physicist.

To summarize: I don't think that there is much difference between the characteristics of men and women in different Western countries – which again counts the Czech Republic, too. And we – from the Central Europe – probably have a more balanced point of view.

37. **Mary Messall**
October 28, 2004

As a female graduate student, I resent the crap I hear about women just thinking differently from men. I also resent the idea that women are just so much weaker and more sensetive that they can't take the competitive environment, and any suggestion that women should face, or do face, less strict standards of admission or success. And finally, I resent the notion that women are just so unable to stand up to men that they have let themselves be oppressed for centuries, and that the ghost of this oppression keeps them out of physics even now.

So what's my theory? Simple. It costs women more to be perceived as nerds than men, and physics is the nerdiest science. Popularity and attractiveness are status, for a woman. You want the rewards of status — a mate, the respect of your peers (especially other women, mothers and aunts and grandmothers and girlfriends), stimulating friends and politeness from strangers, self-esteem and etc — you'd better not be a bespectacled obsessed loner. Guys can get away with it, because status for guys comes from public success, money and power. Those things are available in academia.

The solution? I figure if we could just get girls to read more science fiction, the field would seem more glamorous. That's how I got sucked in, after all.

38. **D R Lunsford**
October 28, 2004

Yes, Peter I *insist* that these abuses against Lubos cease at once, at once I say, at once! It's plainly discrimination against expatriate short-fused addled tongue-tied hyperaggressive anintuitive Harvard physics professors. Shame on you.

39. **Arun**
October 28, 2004

Lubos,

You are a scientist. Why then is it an automatic assumption that the social situation in the US, the high-school teachers' attitudes to girls good in math and physics, parental attitudes, and so on are the same in the US as in the Czech Republic? I think the natural assumption has to be that it is different until proven similar.

(IMO, we lack a science of cultural differences because it is so difficult not to make these automatic assumptions.)

-Arun

40. **sol**
October 28, 2004

As most of you know I like to admonish multi dimensional perspectives :), so of course Mona Lisa's smile, as a movie should have been considered in context, as well:)

Traditions are keenly recognized over the last century, and I'd like to think, that mothering propective students, would have been more then saying okay, because of the rote system by which you eager students have memorized, I am going to break up this line of thinking, by presenting a whole new perspective(remembering the movie now:)?

The method of teaching changed ole tradition, and incorporated new facets of thinking. Gained a appreciation over artistic views, and mothers?

So we should find independance and brilliance based on the content and material of dissertation, rather then, what neurons fire more then likely(move your arm enough and you create the pathways), to what should fire outside of rote(creative potentials).

Of course we do not forget the foundation. Thanks Peter:)

Oh, I am male.

41.
October 28, 2004

Please Peter, stop the cruel digs at Lubos, lest you drive away this delicate flower for good.

42. **Lubos Motl**
October 28, 2004

It's great that I can confirm Peter's last sentence 😉 by the following comment:

The influence of feminism (and related ideas about the males exploiting the females, and similar rubbish) at the U.S. academic institutions is highly annoying and discouraging for an freedom-loving person. Of course, it is partly a matter of culture and traditions.

Let me tell you that my diploma thesis advisor in Prague – whom I consider my friend, and we wrote a textbook together – married my classmate. She simply fell in love with him during the first lecture, and finally it worked out.

I would personally find it highly disturbing if someone had the courage to publicly question their relationship just because it was a teacher and his student. Such a questioning simply violates what I consider to be a respect to basic human freedoms, and a respect to important relationships between the people – it's a disrespect to love.

Let me also say that today it is nonsense that the girls are discriminated virtually anywhere in physics. Average girls simply do not like physics as much as boys do, even if they are supported. This is an observable fact, regardless of its explanation. Genders have played slightly different roles in the society for centuries and millenia – but even if they did not, there are just so many biological differences that a different "typical" focus of the two genders just could not be surprising.

The male and female brain work differently in details. The average male brane has 20 percent more neurons than the average female brain. The latter has more connections between the neurons than the male brain. When thinking about language, one can show that both female hemispheres, but only one male hemisphere, is active, and so forth.

It's also very wrong and silly to create a false stereotype in which the teachers are trying to pick their students, and not the other way around. I know many more examples of the second category.

By the way, the physicists should be much more aggressive than they are today. There are some shining exception of physicists who know what they defend – and some of them are women. 😉

Physics Today on Ethics

November 4, 2004
Categories: Uncategorized

A correspondent points out that this month's Physics Today has a couple articles about ethical issues involved in how physics research is conducted in the U.S.

Most of this doesn't really apply to the kind of research I know best, theoretical research in physics and research in math. One main issue considered is the trustworthiness of experimental data, and as far as I can tell, in elementary particle physics the data is quite trustworthy. Since the collaborations that produce these results are so huge, many people are involved in going over any published result of any interest, so even if someone were tempted to fake or manipulate data, it would be hard to get away with.

Another issue of concern is the treatment of young experimentalists, who are often overworked and under-recognized. But the situation of theorists is generally different. In most cases the problem for them is thesis advisors who ignore them, not ones who pay close attention to what they are doing and make them work too hard. There is a fundamental ethical problem in the treatment of young theorists by the physics community, that of producing far more particle physics Ph.D.s than there are jobs for. This creates a brutal situation for young people, while it is to some degree in the interests of those who are established in permanent positions to let this go on.

The main ethical problem in particle theory research these days, a fundamental lack of honesty in how the results of this research are evaluated, doesn't seem to be addressed at all in the Physics Today articles.

Comments

1. **D R Lunsford**
   November 6, 2004

   Matthew,

   It's actually an exciting time to be a theorist, because everyone in academe is so out to lunch, there's plenty of open space for real work.

2. **Chris Oakley**
   November 6, 2004

   Matthew:

   Agreed – my statement should have been qualified accordingly (a lot of groups here in the UK work on lattice gauge theory). As you will have gathered, I was never tempted, but I do not deny that it connects directly with the experimentalists.

3. **Matthew**
   November 6, 2004

   Hi Chris,

   What is called "particle physics theory" now really belongs in the mathematics (or arguably even the theology) department, with "particle physics theory" as I understood it 25 years ago, which took a keen interest in experiments, virtually ceasing to exist. The result of this is that the experimentalists now do their own modelling, expecting little or no help from the theorists.

   Just to be somewhat more optimistic, there is a active phenomenolgy community in particle theory which still cares about experiments. Many of the people here at Cornell attempt to understand what will be seen in the next round of experiments at the LHC. Also, there is a large group of theorists (myself inculded) who interface with the ongoing b and c quark physics programs.

   Now, you don't see that in the popular press 🙂 which is mostly string theory focused. Of course, some of the models propose (large extra dimensions, for one) may be wacky, but they are testable at the LHC, and the people who propose them do make detailed (or as detailed as they can) predictions about how to do these tests.

   From my perspective, it's an exciting time to be a phenomenolegist.

4. **Fabien Besnard**
   November 6, 2004

   Hello everyone,
   I agree with Peter on the No of Ph.d/academic jobs issue. The problem is not that, in principle, there should be a perfect adequation betweens these two numbers. The actual problem is with the ratio. I don't know the figures for usa but in France, for mathematics, the ration is about 20/1 and this is ridiculously high. It's even worse in theoretical physics. For each academic position in any french university there is commonly over 150 candidates, many of them very good. It means 90% of the CV sent won't be considered as they should be : they are sent directly to the trash can if at first glance they boast fewer (maybe uninteresting and repetitive) published papers than the others. This is huge encouragement to uniformity of curiculum.

5. **Michael Williams**
   November 5, 2004

   Having damned the NYRB article with faint praise in my previous comment, I would like to add that it is very good on the universal scientific problem of priority and credit. Definitely worth a read!

6. **Michael Williams**
   November 5, 2004

   Followers of this discussion may find this New York Review of Books article interesting. It reviews a book and the Union of Concerned Scientists' paper on the Bush administration. As is often the case, the review (and possibly the book) assumes science is the same as medical trials, but it's interesting nevertheless.

7. **Fabio Lanzoni**
   November 5, 2004

I probly woulda still gotten my PhD had I known in advance I would not stay in academia; as Emanuel Derman says in his book, it's not a bad way to waste away your 20's. So I don't think its necessarily unethical to train more students than there are faculty openings, as long as you are honest with the students about their prospects.

8. **Chris Oakley**
November 5, 2004

Peter,

One would of course get the same problem if everyone with a classics or history degree wanted to be an academic, but most realise that this is not possible, or they want to do something different anyway. A PhD in mathematics or a "hard" science is however an asset in most cases if one chooses to go into the so-called "real" world, and the economic equation may work out to one's advantage even taking account of poor remuneration when one is a student.

I am not sure that universities can do anything much differently … ultimately one's product is only worth what people are prepared to pay for it and this applies as much to academic research and teaching as to cars or cosmetics. It seems inevitable that with limited money and a lot of people wanting to be academics, salaries will be driven down and people will be exploited. My only gripe is that the limited resources could be used better … i.e. more emphasis on new ideas and less on vested interests.

9. **Peter**
November 5, 2004

There isn't a direct economic reason for a particular faculty member doing theory to want to produce more Ph.D. students. But there are lots of indirect ones. For instance, if you want to be able to teach advanced graduate courses in your specialty, you need a large enough audience that will take the course. As far as the university as an institution is concerned, it has many direct economic incentives to produce as many Ph.D.s as possible:

1. Sometimes you can get either the student or someone else to pay large amounts of tuition dollars (e.g. the student may be on some sort of fellowship paid for by a grant or some other sort of outside money).

2. If they don't have outside money, you appoint students as teaching fellows, thereby getting employees who will work quite cheaply. Paying graduate students is not cheap, but it's a lot cheaper than hiring full-time regular faculty.

3. Once they get their Ph.D.s, students will join a large underemployed labor pool, and everytime you have a faculty job you want to fill, hundreds of them will apply for it, many of whom will be willing to take the job no matter how badly it pays. By handing out a lot of Ph.D.s you create a large pool of cheap labor.

University administrations are the ones that set the number of how many Ph.D. slots they have, departments basically always take as many as they can get.

10. **Chris Oakley**
November 5, 2004

I do not think that the "training more people than there are jobs for them" argument holds water. If it did, then we might as well give up altogether on the study of Classics, Philosophy and many other subjects where almost all the relevant jobs are just teaching the subject to others. Apart from subjects like Medicine, only a minority of students that will be expecting their degree course to be a specific vocational training. For example, the Aeronautical Engineering PhD's we hired at Nomura to do financial modelling were perfectly happy to leave air flow calculations behind them when they joined the company.

The problem with particle physics theory as I see it is that the nature of the beast has changed. What is called "particle physics theory" now really belongs in the mathematics (or arguably even the theology) department, with "particle physics theory" as I understood it 25 years ago, which took a keen interest in experiments, virtually ceasing to exist. The result of this is that the experimentalists now do their own modelling, expecting little or no help from the theorists. The problem is that, unaided, the experimentalists are less successful in getting the public's interest, so given the high costs involved, this can only lead in the long term to decline.

11. **Alejandro Rivero**
November 5, 2004

Well, about the data, I am a bit worried on the use of the concept of "confidence level", and the current methods seem to be a mix of bayesianism and standard frequentist approaches, plus some random 🙂 montecarlo comparisions. Not unethical, but enough to let people to choose the approach they feel better.

12. **JC**
November 5, 2004

Peter,

Are there "economic" reasons for established folks to produce many more PhD students than the number of available jobs? (ie. Do folks get more government grant money for producing more papers, theses, etc …?)

The Triumphs and Travail of the Theory of More Than Everything

November 4, 2004
Categories: Uncategorized

Shamit Kachru gave the physics colloquium at Rutgers yesterday. His title was “The Theory of More Than Everything” and I heard from people who attended that he was promoting research into the “Landscape” as a new model of how to do theoretical physics, especially cosmology.

I was down there today and heard two talks in the mathematical physics seminar, by Abhay Ashtekar and Tom Banks. Ashtekar’s talk was a standard exposition of a few of the basic ideas of loop quantum gravity, also reviewing an attempt to apply these ideas to cosmology. The talk by Banks was titled “Triumphs and Travails of String Theory”. The first hour was about the triumphs, giving a pretty standard survey of the supposed accomplishments of string theory. Banks emphasized the importance of supersymmetry, and described string theory as not quite background independent, but depending only on a choice of “asymptotic background”. He dealt with matrix models, holography, BPS states, dualities, getting gauge bosons out of string theory, and AdS/CFT.

His talk contained quite a lot of content, unlike some promotional talks of this kind, but it did come off a bit like an hour-long infomercial (“And, there’s even more! It slices, it dices, it ….”). The last five minutes were devoted to the travails of string theory, of which, according to Banks, there is really only one (although he did mention that the lack of observed supersymmetry is also a problem). The one travail is the fact that the cosmological constant seems to be positive, so the universe is de Sitter, not anti de Sitter or flat. This creates well known problems with defining an S-matrix. He went on to explain the “Landscape” idea with its de Sitter states that are only metastable, saying this “leads to a new philosophy of doing physics that many are exploring”. He didn’t seem interested in directly criticizing this new philosophy, but did end by promoting his own, different, ideas about how to deal with the cosmological constant problem.

During the question session afterward, someone asked if there was any overlap between loop quantum gravity and string theory. After some hemming and hawing by the speakers, Michael Douglas spoke from the audience, saying that since string theory was really 20 or so different kinds of approaches to quantum gravity, it was quite plausible that LQG was another related one.

Comments

1. **Plato**
   November 7, 2004

   I thought Lubos and Peter might like a picture of the Landscape of the Week link:)

2. **Plato**
   November 7, 2004

   Dear Chris,

   OK – I’ll stop doing the book, provided that you explain what a “dimensional advocate” is.

   If we were to suspect that all matter distinctions existed in some other form then the solid things we see, we understand I think that before spit can bcome from saliva, one had to had the emotion first?:)

   Now if DRL was having a emotive experience, in his psychological state, the manifestation of the liquid(saliva) would have taken a condensation of thinking, to arive at such a conclusion.:)

   If such solid things are to be taken for granted, we then know that if DRL was a **dimensional advocate**, he would have known that the spit arises from thinking emotively:)

   LOL

3. **Chris Oakley**
   November 7, 2004

   My Dear Plato,

   OK – I’ll stop doing the book, provided that you explain what a “dimensional advocate” is.

   Do they for example charge less or more than ordinary lawyers?

4. **Plato**
   November 7, 2004

   I’m writing a book about Dionysus, loosely based on Jim Morrison (or should it be the other way round?).

   Chris please don’t.:)It takes all of ones faculties to enter into such allegorical states that you need some anchor, not drugs:)

   You see both Lubos and Peter are under the arch of science, yet they both move from different perspectives? Although they agree on (?)that science should validate?

   John Baez speaks on the abstractness of **quantum gravity** and we experience abstract values, in brane scenarios.

   So whose right? Whose on firm ground?

5. **Chris Oakley**
   November 7, 2004

   ‘…addicted to imaginary power and fractally vibrating nonsense, incapable of judgment, phase-locked into an eternal inward self-indulgent “journey of discovery”.’

   ‘… dimensional advocate?’

   ‘If you classify the 60’s and 80’s you have spoken to many who have travelled paths before you.’

What are you guys *on*, man, and more importantly, where can I get it?

I'm writing a book about Dionysus, loosely based on Jim Morrison (or should it be the other way round?). I won't need to take drugs to write the more high-faluting bits as I can just borrow phrases from the comments section of this blog.

6. **Plato**
   November 7, 2004

   DRL,

   The radicals of the 60s and the 80s have combined to breed a race of intellectual midgets, addicted to imaginary power and fractally vibrating nonsense, incapable of judgment, phase-locked into an eternal inward self-indulgent "journey of discovery".

   You failed to identify the relation I drew between Peter and LUBos. I asked if you applied the question which one would follow which process of thought. Lubos would point to heaven, while Peter would be showing his hands where?

   Plato knew of pythagorean studies.:

   Anyway you tried to compact a much information in a **short space of time(dimensional advocate?:)** as you could by using words that have deep association to your "**feelings about the views of strings**"? That it was hard to remove the emotive impact of your bias from the saliva you spit. I am sorry:)

   But this is a view, that **I have shown** and the struggle to this day:) Peter looks hard around, and some of the religious orientated although firmly attached to ground by Peter's Dialogue, recogize the need for defining this higher attribute?

   If you classify the 60's and 80's you have spoken to many who have travelled paths before you.

   Would you quickly denounce their contributions as we would in moving forward the thought experiments of Solvay?

   Although we would harbour differing points of view we do not denounce each other. We use each other to fuel what is truth? Right?

7. **D R Lunsford**
   November 7, 2004

   Oops – in my rant, I mistook "3 Minutes" with "Brief History of Time" – sorry. "3 Minutes" is a nice book on some level. I suppose all trimuphalism seems to get to me.

   I hate being mad. Science is supposed to be fun.

8. **D R Lunsford**
   November 7, 2004

   Pythagoras was a stringer! Oh how rich.

   This website illustrates all that is wrong with physics. A crew analogous to the Neocons has taken over, who must fill every moment with world-shaking radicalism and triumphalism, couched in the most airy-feathery faux-wonderment about things that are not real. "Light shining from the fifth dimension" – what sort of nonsense is that?

   This all seems to have started with the Dr. Seuss Lectures on Physics, otherwise known as "The Dancing Wu-Li Masters", and the Disney Guide to Cosmology, "The First Three Minutes" (a time formerly reserved for running trailers of coming attractions). The success of these meant it had became possible to fill the exquisite yuppie-radical bombasto-conservative mind with all manner of self-propagating nonsense. It is, after all, more fun for most to indulge in reflexive hallucinations rather than make the effort to get to facts – it is easy to bash Pauli and Dirac and Einstein and ignore what they say, because they are "establishment" figures ripe for a knockover, pitiful old-worlders from a nighttime of the soul. The febrile postmodern narcissist must always ponder the universe as revealed in his own reflection, distorted by passive consumption of endless surreal images that compete with his wan self-awareness in his infinite reflecting pool. It is not enough to let one's mind reach only to Vega – the narcissist-addict's grasp must extend to the far reaches of what passes for imagination.

   The radicals of the 60s and the 80s have combined to breed a race of intellectual midgets, addicted to imaginary power and fractally vibrating nonsense, incapable of judgment, phase-locked into an eternal inward self-indulgent "journey of discovery".

   It makes me ill. That's what I think.

9. **Plato**
   November 6, 2004

   Which is the String Theorist and which is the LQGist

   It seems the struggle is deeply embedded in consciousness? That we have brought forward age ole struggles, to do them all over again?

   What do you think?

10. **Mubos Lotl**
    November 5, 2004

    the parents of "Mubos Lotl" whom I assume is either Lunsford himself or someone very similar – should spank their son thoroughly

    Why, Lubos, do you deny writing the words I cited from your own blog? Perhaps you would like to explain their real meaning if I have misinterpreted them? That is, perhaps you can explain how being rude and obnoxious will help string theory to get moving again. AdS, here we come!

11. November 5, 2004

    It's true, Tom Banks is much older than he was 15 years ago.

12. **Plato**
    November 5, 2004

    Lubos
    Your questions may be fine, but they don't go as far as string theory, and I don't have time to explain vacuum bubbles in QFT now.

I was thinking of supersymmetrical reality.

Symmetry breaking, and I looked at the current universe, and it structures. For me, this evolution lead from Planck epoch through to cosmic clumping(cosmic string)from it's origination. Andrey Kravtsov images are really helpful along with Max Tegmarks research.

But yes QFT, in comsological proportions, had to be consistent with the dynamics of the universe?

I understand your time, and appreciate post.

Peter can clear up the identity of **name arranger** in a minute, using blogspot. Best not to speculate. Standards here of human beings, should be impeccable, dealing with such fine subjects.

13. **D R Lunsford**
November 5, 2004

Lubos, I have better things to do on my limited worldline than to mock you. When I want to point out how utterly clueless, arrogant, and annoying you are, I need not retreat behind a mocking alias.

14. **Lubo? Motl**
November 5, 2004

I don't know if physics should be like wrestling, but what I know is that D.R.Lunsford's parents – and the parents of "Mubos Lotl" whom I assume is either Lunsford himself or someone very similar – should spank their son thoroughly. They should try to avoid the evolution of their son to a moral and intellectual cripple. But it may already be too late.

15. **Lubo? Motl**
November 5, 2004

Hi Plato!
I was talking about papers of Tom (about background independence) such as

http://arxiv.org/abs/hep-th/0011255

See also his list of papers

http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=FIND+A+BANKS%2CT+

Your questions may be fine, but they don't go as far as string theory, and I don't have time to explain vacuum bubbles in QFT now.

16. **D R Lunsford**
November 5, 2004

This issue of NEW is beyond surreal. Bon-Motl wants to make physics into wrestling.

I walk away, muttering, drooling, and weeping.

17. **Mubos Lotl**
November 5, 2004

String theory is in trouble at the moment, but recently an explanation for this has been given. According to Lubos Motl, "Current string theorists and physicists in general are just too "nice", and this atmosphere is correlated with the reduced amount of progress that we're doing (whichever is the cause vs. the effect). I know that Nima Arkani-Hamed agrees with me, for example, and the people who say the opposite statement seem to be disconnected from reality." According to my model, "niceness" is a scalar field generated by good-natured, polite string theorists, which has the effect of "uplifting" the anti-de Sitter background to an apparent de Sitter state. The solution is for string theorists like LM and Arkani-Hamed to go against their fundamental natures and adopt the manner of unusually obnoxious
10-year-olds. The result is that the vacuum will subside back into its natural AdS state. True, this will ultimately destroy the cosmos, but at least it
will reveal that string theory is correct.

18. **Plato**
November 4, 2004

Lubos*Tom has also investigated the question under which conditions you can create a bubble of a different vacuum within another vacuum/Universe*

During a first-order phase transition, the matter fields get trapped in a `false vacuum' state from which they can only escape by nucleating bubbles of the new phase, that is, the `true vacuum' state.

http://www.damtp.cam.ac.uk/user/gr/public/cs_phase.html

Do you have any other information resources that would help clarify this in terms experimental considerations like this membrane in a vacuum? (make sure you let it load)

Would it be wrong to think of (mem)branes in this context?

A few Saturdays ago, he had his heart set on bubbles. "We have a copy of ***C. V. Boys' book Soap Bubbles*** *here on the ISS. It was published in 1911 and it's still a wonderful treatise on thin films. Every space station should have a copy," he laughs. "I wanted to see what thin films and bubbles might do in zero-g and felt it was a topic ripe for discovery."*

http://science.nasa.gov/headlines/y2003/25feb_nosoap.htm

19. **Lubos Motl**
November 4, 2004

Tom Banks is a very nice person. I am happy that he keeps on emphasizing some ideas that are more important for him than many others.

Tom believes that supersymmetry may play a more fundamental role in the scheme of things than just a particular symmetry of physics. And he's studied the question of background dependence in depth. His remark that difference asymptotic boundary conditions define different superselection sectors that can be treated as separate theories is a very good remark.

Tom has also investigated the question under which conditions you can create a bubble of a different vacuum within another vacuum/Universe.

I am sure that Tom has said many things that most others don't emphasize enough.

The measure of importance that Mike Douglas assigned to LQG seems to be the maximum one can imagine to be realized – it would be just the 21st equivalent/related approach to the same theory of quantum gravity (called "String/M-theory"), if it's correct. Of course, I don't think it is, but the idea that LQG should be visualized as a structure comparable to the *whole* string theory is absolutely ridiculous. It's like comparing Castro's Cuba to the USA. In the best case, you may try to compare Cuba to Florida, or loop quantum gravity to D-branes on orbifolds (by Douglas and Moore), for example. Of course that the brane on orbifolds will still be a more important insight about quantum gravity than loop quantum gravity, and Florida will defeat the current Cuba. But at least, these are somewhat more reasonable comparisons.

20. **Peter**
November 4, 2004

I think the last time I heard him speak was nearly fifteen years ago, so my main impression was "Gee, the guy looks a lot older". To be fair, the extent to which he was trying to sell string theory was no greater than most other talks of this kind. He did try and pack more content in than many such talks, which is what gave the impression of relentless adding on of more wondrous features.

21. **JC**
November 4, 2004

Does Banks normally sound like a "used car salesman" type in his talks?

Khovanov Homology

November 8, 2004
Categories: Uncategorized

At his talk last year at the conference in honor of Gelfand's 90th birthday, Atiyah posed the question of whether there is a quantum field theoretic explanation of why the coefficients of the Jones polynomial are integers. Witten's Chern-Simons-Witten theory is a 3d QFT that computes the Jones polynomial (a topological invariant of knots or links inside a 3d manifold), but gives no obvious reason the coefficients should be integral.

One thing about the Chern-Simons-Witten story that has always bothered me is that, unlike his other TQFTs, this one is not of a homological nature. In the other TQFTs, the Hilbert space is finite dimensional because there are fermionic variables which cause cancellations such that only the homology of some complex contributes to the observables. To make any real sense of the idea of a path integral whose Lagrangian is the Chern-Simons functional, one has to do something like add a Yang-Mills term, then take a limit. By doing this one can move all but a finite part of the usual gauge theory Hilbert space off to infinite energy. It would be very interesting if there were a version of the theory which instead worked homologically like other TQFTs.

A hot topic in low dimensional topology recently has been the notion of "Khovanov homology", which associates to a knot a complex whose homology is the Jones polynomial. For an introduction to Khovanov homology, see papers by Dror Bar-Natan (a mathematician who was a student of Witten's) or Jacob Rasmussen. Bar-Natan has a lot of other material about Khovanov homology on his web-site.

One way of answering Atiyah's question would be to find a 4d TQFT whose Hilbert space is the Khovanov homology of the boundary. Maybe there is some sort of gauge-theory based QFT which generalizes the Chern-Simons-Witten theory and computes Khovanov homology. But after consulting the local expert on these things (Peter Ozsvath), it seems that no one knows whether it is even possible to reformulate Khovanov homology in any sort of gauge-theoretical terms. The only known definitions of it are kind of like the pre-Witten skein relation definitions of Jones polynomials. They are based on working with a projection of the knot onto two-dimensions.

A couple weeks ago Sergei Gukov gave a talk in the math department at UCSD with the title "Topological Invariants and Khovanov Homology", and perhaps his work has some relation to the above speculations.

Gukov is also the co-author of a paper that just appeared on the arXiv entitled "Topological M-theory as Unification of Form Theories of Gravity". Like M-theory itself, it appears that no one knows what "topological M-theory" is, but it is supposed to be some sort of seven-dimensional theory that is related to topological strings on 6d Calabi-Yaus in much the same way M-theory is a conjectural 11d theory related to 10d superstrings. Lubos Motl has even more questions about this than I do.

No Comments

Proof of the Jacobian Conjecture

November 10, 2004
Categories: Uncategorized

The Jacobian Conjecture is one of the most well-known open problems in algebraic geometry. It now seems that a proof has been found by Carolyn Dean of the University of Michigan, for the case of polynomials in two complex variables (for more variables, many people believe it is not even true). For more information about this, see Graham Leuschke's weblog.

Dean hasn't published any papers in almost 15 years and is nominally a lecturer in mathematics education at Michigan. There have been many false proofs of this conjecture over the years, and if this one holds up it will be quite a story. The paper doesn't seem to be publicly available yet, but Dean will be lecturing on the proof at Michigan next month. One of the experts in the field, Mel Hochster, has gone over it carefully and is convinced it is correct. The rumor I hear is that it has been submitted for publication to the Journal of the American Mathematical Society.

Update: There's an announcement of Dean's talks posted on sci.math.research.

Update: Someone wrote in with a comment to another post pointing out that Dean has found a hole in her proof. For some more information about this, go here.

Comments

1. **Chris W.**
   November 15, 2004

   The contrasting examples of string theory and the Jacobian Conjecture (and the other notable recent achievements mentioned below) illustrate why physics is different, and arguably harder, than mathematics. Once a mathematical proof has been carefully reviewed and published it stands forever. One may argue about its significance, but one doesn't have to worry that it will be contradicted by existing or future observations.

   The other crucial point is that, as Shiing-shen Chern observed in an essay published during the Einstein Centennial year, mathematicians' problems, while often very difficult to solve, come to them more or less clearly formulated. In the natural sciences one must sometimes struggle for years to properly *formulate* the problem that needs to be solved.

   I would suggest that the epistemological pathologies of string theory stem from an inclination to evade those painful realities, knowing the enormous and often lonely dedication that went into the theory's early development. Also, one should consider the example of Einstein's lonely pursuit of a unified field theory in the latter part of his career, and the disdain with which he was treated by many younger researchers engrossed in the development of quantum mechanics and quantum field theory, with the support of an ample supply of experimental input. No one relishes being in Einstein's position for very long.

2. **Chris Oakley**
   November 15, 2004

   Did not Superstring theory come from a similar multi-year, fashion-resisting, career-endangering focus?

   It did, which only goes to show that when rebels become the establishment, they are even more reactionary than those they supplanted.

3. **Arun**
   November 14, 2004

   Did not Superstring theory come from a similar multi-year, fashion-resisting, career-endangering focus?

4. **mm**
   November 13, 2004

   One correction to the original post. There is no department of Math Education at Michigan. There is Math in LSA and there is the School of Education. Carolyn is a lecturer in the Math Department. Her research interests are listed as being in math education. Bass and Ball both have joint appointments in Math and Education. After she was denied tenure, she founded and initially was the director for MMSS, a summer program for high school students. I believe she has recently taught, among other things, honors calculus and math for future (elementary?) teachers.

   Carolyn had a postdoc at University of Chicago. She probably went to Michigan in 1989. She was definitely there in 1992.

   I agree with Graham's comments about tenure. It is perhaps bittersweet for Carolyn that she was aided in solving the Jacobian conjecture by being a lecturer at Michigan given that I am pretty sure that she was working on it while she was working towards tenure. Of course, it could just have been the passage of time. In any event, she could have chosen a research agenda with a shorter time horizon. Instead, she gambled and lost (the tenure game, at least).

5. **Graham**
   November 13, 2004

   As far as I know, Bass hasn't done any research mathematics in several years — he's been thinking about mathematics education, much of it with Deborah Ball. I would seriously doubt that he even knows Dean was working on JC.

   About Dean: I'm told that she has been at at Michigan since the mid-nineties, when she had a tenure-track position and was denied tenure. Her husband is also on faculty there (Stafford), which I imagine is part of the reason she's still there. I would be surprised if Michigan now hired her with tenure; it would be tantamount to admitting that they'd made a mistake. However, she is clearly a "serious mathematician", as one of your other commenters put it. The fact that she's not tenured does not detract in the slightest from her talent or skill or intelligence, only from her paycheck and job security.

   It is interesting that recent quantum leaps in mathematics have been due to people who showed exceptional ability to focus on one problem for years at a time, but it's certainly not surprising. In fact, this is in part what the tenure system is designed to encourage — having nothing to lose. If there were no such thing as tenure, Wiles would never have been able to devote years to working on the solution to an incredibly difficult problem — he would have had to show some progress on something in order to keep his job every year. Dean is in the same position for a different reason; she has nothing to lose because she's not regular faculty. Perelman, too — his position in St. Petersburg wasn't going to be taken away from him. Doing away with tenure isn't going to bring about this kind of freedom for everyone — for most of us, it would mean we'd have to work on simplistic problems that we knew we could publish on before the year's end.

6. **yuhan**
   November 13, 2004

   Peter,
   good to hear from you too.

   Actually I know little about Dean beyond some googling. And yes, I was wondering about the "tenure question" too which I'm sure must be a delicate one. But I didn't know that she had been thinking about the jacobian conjecture for several years. Tenacity!

   If I'm not mistaken, in some parts of europe there's more of math culture of keeping postdocs on practically indefinitely (at just slightly above a grad students level) and giving them a huge block of time to create a significant work. Is this correct?

   I must correct myself in my last post: Hyman is *both* at math and education at Michigan.

7. **Chris Oakley**
   November 13, 2004

   if you want grants, etc. you should be working on much shorter term projects. It's also remarkable that two out of three of these people didn't have a regular tenured position.

   Of the two functions of a University, namely teaching and research, the former IMHO tends to be done a lot better than the latter, possibly because it is easier to apply correction mechanisms when things start to go wrong. Research, OTOH, tends to be islands of excellence amid a sea of mediocrity, and the mediocre seems to be highly resistant to any attempt to reform it. I think that something needs to be done, but I am not quite sure what. My proposal was to abolish tenured positions. Then at least people who are useless will only allowed to be useless for a limited time. One of the most frustrating things possible for a young researcher with a new idea is people of this kind being in a position to block their every move. And these are tenured staff who more often than not would not be shortlisted if they were forced to reapply for their jobs. I do not believe that my suggestion would ever become a reality, but am interested to know whether those reading this blog (who I suspect may well be more critical of "the establishment" than average) either (a) think that there is a problem with academic research and (b) have any ideas as to what to do about it.

8. **Peter**
   November 13, 2004

   It is remarkable that the last decade has seen great progress in math (Wiles proving Fermat's Last Theorem, Perelman proving the Poincare Conjecture, now Dean the Jacobian Conjecture), all achieved by people willing to spend 7 years or more focusing on a single problem. That's not the way academic research is generally structured, if you want grants, etc. you should be working on much shorter term projects. It's also remarkable that two out of three of these people didn't have a regular tenured position.

   I think particle theory should learn from this. If some of the smarter people in the field would actually spend 7 years concentrating on one problem, the field might actually go somewhere instead of being dead in the water

9. **The Great Gazoo**
   November 13, 2004

   Does it seem strange that several conjectures have fallen in recent years due to mathematicians that have spent nearly a decade solely focusing on a single problem? This is an oddity isn't it? I mean don't mathematicians generally not pour all of their energy into one thing that may or may not pan out?

10. **Peter**
    November 12, 2004

    Hi Yuhan,
    Good to hear from you, and I'm glad you've been enjoying the weblog.

    I really don't know anything about Carolyn Dean, it sounds like you know more than I do. So I don't know whether Hyman was in any way involved in her work on the Jacobian Conjecture. It is an amazing story and a very impressive achievement. It will be interesting to see whether academia rewards her the way it should. Will Michigan give her a tenured position?

11. **yuhan**
    November 12, 2004

    Dear Peter,

    I've been a lurker, enjoying reading your blog, for some time now. But I am blown away by the news that Carolyn Dean proved the jacobian conjecture (not that I know her personally). I am wondering how she will be regarded — after all conventional wisdom has it that lecturers can't do research and here she has clearly outdone "serious mathematicians". She sure has a good brain and a good heart too considering her passion in running math summer camps for high school kids.

    Interesting thing is that our ex-columbian Hyman Bass, a key contributor to the jacobian conjecture, is also at Michigan but in the education dept. An internet search shows that Dean and Bass had contact in education forums. Any connection?

12. **Peter**
    November 10, 2004

    I don't know anything about Carolyn Dean personally, just that one place on the Michigan web-site refers to her as a "lecturer", another as a "visiting lecturer". As I'm quite well aware from personal experience, these kinds of titles can refer to all sorts of different kinds of actual positions. So the title doesn't tell you much, which is what I was awkwardly expressing.

13. **mm**
    November 10, 2004

    Just curious. What exactly did you mean by "nominally a lecturer"?

Provocative Comments From Veltman

November 10, 2004
Categories: Uncategorized

Martin Veltman gave a colloquium talk at Fermilab two weeks ago and, as usual, had some very provocative comments to make. At the end of his talk he made the claim that the only thing astrophysics has contributed to particle physics is information about the number of neutrinos (from Helium abundance observations). He claims "Apart from this, Astrophysics is so far useless to us."

He then gave some purported data about how particle physicists really felt about the impact of astrophysics and cosmology on their field. His slides say:

"Question put to many particle physicists: Do you feel that astrophysics and particle physics are joined at the hip?

Response:

Refusing to respond on the grounds that it is an obscene proposition (99.9%)
Do not know what you are talking about (9.671%)
Undecided (rest)

Questions put to particle experimenters:

Your experiment is justified by claiming that it will tell us about the first seconds of the big bang. Do you agree?

Response:

No (98.312%)
Do not know what you are talking about (1.671%)
Undecided (rest)

Do you feel that we need a new machine (linear collider) because it can be used to discover dark matter (dark energy)?

Response:

No (98.312%)
Do not know what you are talking about (1.671%)
Is this related to the death star of Darth Vader? (3%)
Undecided (rest)"

I think Veltman has a very good point. The particle physics community seems to have decided to try and sell the public on supporting particle physics, specifically a new linear collider, by claiming that such a machine will "solve the mystery of dark energy", find "extra dimensions of space", and tell us "how the universe came to be" (see for instance the HEPAP Quantum Universe report). This all sounds very sexy, but there's no good reason to believe that a linear collider will do any of this. Maybe this is the right way to sell the linear collider, but personally I'm rather uncomfortable with this level of hype and wouldn't want to be the one testifying under oath before Congress about this.

Veltman also comments that "It appears to me that the only viable solution is that this machine will be located in the US", but given the massive deficit the Bush administration has created and current political realities, I find it hard to believe we'll see the kind of budget increases for particle physics that would be required to make this happen anytime soon.

Comments

1. **Thomas Larsson**
   November 18, 2004

   Since in 1 dimensions everything is pretty simple these susy field theories in 1+0 dimensions are not awfully exciting and you can get away with ignoring the susy that appears here.
   Or you may be inspired by it.

   The question is for how many years, decades and centuries inspiration is enough. Signatures for supersymmetry have had ample opportunity to show up in quite a few different types of experiments, in the 1/3 century that has passed since SUSY was proposed. No such signatures have been found. A common way of summarizing the experimental situation is to say that SUSY requires fine-tuning on the percent level. This seems like a serious critique of an idea which was mainly meant to cure fine-tuning.

   Anyway, we evidently have different opinions about how much inspiration should be constrained by experiments. I would like to know one things, though. The discovery of sparticles at the LHC would obviously be a great triumph for SUSY, and it would prove me dead wrong in many ways. But do you think that the non-discovery of sparticles at the LHC would in any way lessen the promise and importance of SUSY?

2. **D R Lunsford**
   November 18, 2004

   ,

   Thanks, that was very interesting, and you have an excellent way of explaining things (I imagine because you actually understand it).

   -drl

3. November 18, 2004

   Hi DRL –

   Ok, well, I am not going to try to further explain why {D,D} = 2Box is the algebra of worldline susy. Whoever does not believe it should check the standard literature.

Concerning your question:

There is nothing artificial (‘clever’) going on, I think. The point is that the standard Polyakov-like action of the single free relativistic particle is, when regarded as a theory defined on the worldline, a 1+0 dimensional field theory of a couple of bosonic fields (the coordinate fields) coupled to a lagrange multiplier in such a way that the whole thing can be regarded as 1+0 dimensional gravity coupled to massless scalars plus a ‘cosmological’ constant – on the worldline.

This is just a fact. This action is there and it describes known physics (for instance you can compute QFT loop amplitudes *in target space* from it) and it has this interpretation as a 1+0 dimensional field theory.

When this 1+0 dimensional field theory is susy-ed, which means that in addition to free bosons we allow free massless fermions propagating on the worldline, it gives us the action for single fermions in target space.

Up to this point one is firmly in the realm of known physics. All that has happened is that one has realized that single particle trajectories have field theoritic descriptions on the worldline – and supersymmetric field theories in particular. (Check Siegel’s book for instance for the superfield formulation of them.)

Since in 1 dimensions everything is pretty simple these susy field theories in 1+0 dimensions are not awfully exciting and you can get away with ignoring the susy that appears here.

Or you may be inspired by it. Turns out that in 1+1 dimensions susy field theories are still pretty simple, but taken as theories on the parameter space of single objects again (as we did before for the worldline), they are not just a curiosity but give rise in target space to perturbatively quantized superGR+YM.

This automatism that you cannot have a worldsheet which describes target space fermions without having it supersymmetric and hence have the target space theory supersymmetric is the reason why people say that string theory ‘predicts’ supersymmetry.

But one has to interpret this carefully. Just having the target space theory being supersymmetric (locally) does not mean that its physically relevant solutions exhibit this symmetry, as you know. Hence while the assumption of strings sort of ‘predicts’ that the world is locally supersymmetric, it does not (necessarily) say anything about the observability of globally supersymmetric solutions (which would give rise to superpartners observable in accelerators).

So, to make it very clear once again: Nothing of what I have said here is meant as a proof that target space supersymmetry exists. But since worldline supersymmetry does exist (since it is so trivial) the concept of supersymmetry seems very natural from some point of view. Even more since in 1+1 dimensions worldsheet susy does imply target space susy (while in 1+0 dimensions it only implies target space fermions.)

Make of that what you want.

4. **D R Lunsford**
November 18, 2004

“Bow out?” Why? this is very interesting and at least for me informative.

RE p. 134 – OK fine, now I see what you were getting at – but isn’t this just a clever jamming together of actions, one of which alone would lead to the Dirac (neutrino) equation and the other to a free classical particle?

-drl

5.
November 18, 2004

As I said, when you go from the vector fields to their Noether charges of the sigma model you get the quadratic terms (like (partial phi)(partial phi) for the Polyakov action).

The Dirac operator as well as its square that we are talking about all along are Noether charges of a sigma model action that come from vector fields of the sort that you are talking about.

As I said, you should consider the simple case of nonrelativistic supersymmetric quantum mechanics to clarify the picture.

There is one of your vector fields, namely d_t on the worldline. Its charge is the Hamiltonian H. You can write down the square root of d_t as usual, to get the ‘covariant’ superderivative D that satisfies {D,D} = i d_t . The charge associated with D is the supercharge Q. This has Poisson bracket {Q,Q} = 2H, giving the same algebra as your vector fields.

To summarize:

There are symmetry generators which are vector fields on super-parameter space and satisfy

{D,D} = i d_t ,

where the bracket is the super-Lie bracket of super-vector fields

These have associated conserved charges which have the Poisson bracket

{Q,Q} = H ,

which is the same algebra as that of the symmetries they come from (which is no surprise).

For the relativistic particle it is precisly analogous, only that here t becomes an unphysical parameter tau and H becomes a constraint (the KG operator).

All this is elementary and standard. I’ll bow out at this point.

6. **Thomas Larsson**
November 18, 2004

The {D,D} = 2Box algebra is just a truncation of the super-Virasoro algebra and there, too, as you know, L + \bar L is quadratic in derivatives.

There is some confusion here. The even generators of the super-Virasoro algebra are the vector fields
L_m = z^(m+1) d/dz + f_m z^m \theta d/d\theta,
which are first order (I don’t want to work out the constants f_m). L_0 is certainly not second order, not even if you add \bar L_m.

In fact, the superconformal algebra is the algebra of vector fields in superspace which preserve the contact one-form
\alpha = dz + \theta d\theta
up to a function. Vector fields are always first-order differential operators; this is the definition of a vector field.

We can also realize super-Virasoro as an infinite sum of bilinears, like
L_m = sum_n :a_n a_(m-n): + (n + km) :b_n c_(m-n):
i.e. as a subalgebra of gl(infinity). These may be viewed as vector fields acting on an infinite-dimensional vector space whose coordinates are modes with positive energy (m>0), and the negative-energy modes are derivatives. With that interpretation, some of the L_m's contain terms that are zeroth or second order, but L_0 only contains first-order terms.

7.
November 18, 2004

Danny –

Read on to p. 134. See exercise IIIB1.2 .

8. **D R Lunsford**
November 17, 2004

,

RE p. 132 of Siegel – all I see is typical particle actions on a trajectory. What does this have to do with KG and "adding Fermions"?

-drl

9. **Plato**
November 17, 2004

Thomas,

Regarding the third string revolution: a minimal requirement for a something to qualify as a revolution is that outsiders notice it. Most physicists of all kinds noticed that many particle theorists started to talk about strings in 1984, about branes in 1994, and about antropic selection in 2003. Much fewer outsiders, if any, has noticed the emergent spacetime.

—

Lubos,
What do you think will be the main ideas which will propel the "third" superstring revolution starting next year? Hopefully it will be something which does not involve the anthropic principle.
Posted by JC at October 9, 2004 10:24 AM

I have to give credit where credit is duein reference to Third Superstring Revolution

Identifyng this translation is necessary?

10.
November 17, 2004

Thomas –

don't mistake the symmetry generators in the Lagrangian, like partial_mu, with their corresponding Noether charges which generate them by means of Poisson brackets. Box is the (quantized) Noether charge of the partial_tau translation symmetry on the worldline and has no reason to be linear in derivatives.

So sure {D,D} = 2Box is a susy. Box is the wordline ADM Hamiltonian. And you really new this: The {D,D} = 2Box algebra is just a truncation of the super-Virasoro algebra and there, too, as you know, L + \bar L is quadratic in derivatives.

I bet once you think about this in terms of the nonrelativistic particle this becomes clearer: The Hamiltonian is quadratic in the momenta and still it generate time evolution.

And if you like, you can surely do some field theory using just worldlines, just like you compute string amplitudes just using worldsheets. This is known as the 'worldline technique' for field theory. See for instance vanHolten's hep-th/9408027 and references given there.

DRL –

N is a Lagrange parameter, the lapse function on the worldline, i.e. the single component of the worldline metric. It's presence makes this action equivalent to the (maybe more familiar?) NG-like action of the free particle.

All this is explained in introductory textbooks. For instance look at pp. 132 of Warren Siegel's book "Fields", which you can download for free at

http://www.imsc.ernet.in/physweb/Fields.pdf

11. **Thomas Larsson**
November 17, 2004

Particles on a world-line is first quantization – that formalism cannot account for creation and annihilation of particles. People invented something called quantum field theory to cure that defect in the late 1920s.

To consider the appearence of superalgebras as a hint of SUSY seems to me like a very long shot. {D,D}= Box and Box commutes with everything is a superalgebra, which is relevant for fermions and spinors whose physical relevance nobody has denied. This is not a supersymmetry because the d'Alembertian is a second-order differential operator, whereas in {Q,Q} = P_mu the RHS is a translation, i.e. first order.

However, I appreciate that people try to apply beautiful mathematical structures to physics, although most of the beauty of SUSY is inherited from its superalgebra structure. In fact, I discovered the higher-dimensional generalization of the Virasoro algebra because I wanted to apply it to quantum gravity. We now know that such an algebra exists and that it has a wonderfully beautiful representation theory which seems to fit the needs of quantum gravity exactly. This indicates to me that my original physical motivation was correct. So mathematical beauty is certainly an important search criterion, but it can never be a success criterion by itself. Even if EW says that it is magic and mystery.

It would of course be nothing wrong with SUSY, had it been seen in experiment. However, there are just too many experiments that should have seen signatures of SUSY but did not. The absense of sparticles is the most obvious one, but SUSY also suggests a light Higgs, proton decay, permanent electric dipole moments, WIMPs, a deviation in muon g-2, etc. None of these things have been seen, at least not conclusively, which to me is a clear indication that SUSY just isn't there.

I guess that my position is similar to Veltman's, expressed e.g. at the end of his book:
"The fact is that this book is about physics, and this implies that the theoretical ideas discussed must be supported by experimental facts. Neither supersymmetry nor string theory satisfy this criterion. They are figments of the theoretical mind. To quote Pauli: They are not even wrong. They have no place here."
I didn't make up this quote; it can be found in one of the reviews at the amazon.com link. Do you really think that you or your physics professor are competent enough to question the opinion of a Nobel laureate in theoretical particle physics?

Regarding the third string revolution: a minimal requirement for a something to qualify as a revolution is that outsiders notice it. Most physicists of all kinds noticed that many particle theorists started to talk about strings in 1984, about branes in 1994, and about antropic selection in 2003. Much fewer outsiders, if any, has noticed the emergent spacetime.

12. **D R Lunsford**
November 16, 2004

..so this L is 1/2N (ds^2 – m^2 N^2) ?

What happened to (grad N)^2 (Klein-Gordon)?

-drl

13.
November 16, 2004

Do you understand how the Klein-Gordon particle is described by the action

S = (1/2)\int dtau (N^-1 \dot X^m \dot X_m – Nm^2)

and how this action can be regarded as 1+0 dimensional 'gravity' coupled to scalar 'matter' (just formally)?

14. **D R Lunsford**
November 16, 2004

No I don't – "what" are you "adding", and to "what"? Explain it to me, without repeating the last two posts. Just jot down a few comments and I'll interpolate.

From my perspective, a Dirac particle is a solution to the Dirac equation in the usual Fock space treatment. It is a primitve object. Explain to me how it is not a primitive object. I don't know what you mean by "on the world-line" and so on.

-drl

15.
November 16, 2004

Do you know how the relativistic bosonic particle is described by a field theory of massless bosonic fields on the worldline (a 1-dimensional sigma-model)?

If you add massless fermions to such a field theory on the worldline it becomes supersymmetric on the worldline (a 1-D susy sigma model) and now describes fermions in target space instead of (or in addition to, depending on some details) bosonic particles.

16. **D R Lunsford**
November 16, 2004

a) if you regard the massless Klein-Gordon particle as a 1+0 dimensional bosonic field theory on the worldline and add fermionic fields on the worldline to it in order to get target space fermions (the Dirac particle) the worldline theory automatically becomes supersymmetric..

Umm.. what exactly does this mean?

-drl

17.
November 16, 2004

Thomas –

recall the previous discussion. Steve M said about
superalgebras appearing in nuclear physics:

> This doesn't 'prove' supersymmetry of course but makes its realisation in nature in particle theory, seem a whole lot more likely.

It was then said that if you are willing to take the mere appearance of superalgebras as a hint that supersymmetry will play a more important role (which you, Thomas, are not, but others might be) that then it would be 'more' interesting to consider worldline susy which indeed exists.

As you indicate by your reaction, and as was not disputed by anyone, this worldline susy is rather trivial and hardly a hint for anything.

But some people may find it amusing to note that

a) if you regard the massless Klein-Gordon particle as a 1+0 dimensional bosonic field theory on the worldline and add fermionic fields on the worldline to it in order to get target space fermions (the Dirac particle) the worldline theory automatically becomes supersymmetric

b) if you regard the bosonic string as a 1+1 dimensional bosonic field theory and add worldsheet fermions to it in order to get target space fermions the worldsheet theoy automatically becomes supersymmetric (this is how susy was found in the western hemisphere) and is quantumly consistent only if the target space solves susy GR+YM

c) if you regard the bosonic membrane as a 1+2 dimensional bosonic field theory and add worldsheet fermions to it in order to get target space fermions the worldvolume theory becomes supersymmetric and is classically consistent only if the target space solves susy GR+YM .

This is not a proof for anything and if you don't like it you are welcome to ignore it.

Others may find it noteworthy that while spacetime susy is a delicate issue, woldvolume (-line, -sheet) susy is inevitable, since in low dimensions adding massless fermions to massless bosons is (almost) bound to give susy, as is exemplified by the boring old Dirac particle. And here a small step in parameter space (going from 1+0 to 1+1) is a large step in target space (going from nothing at all to susy-GR+YM).

18. **Plato**
November 16, 2004

Thomas,

Plato, the Third Superstring Revolution, also known as the Anthropic Revolution, happened in 2003. The interpretation is that experimental agreement is now irrelevant. String theory is correct even though it disagrees with observation, because God created the Universe to be compatible with human life.

One thing I can tell you, though, is that most string theorist's suspect that spacetime is a emergent Phenomena in the language of condensed matter physics.**Witten**

I developed a post today on my blog, in response to your statement. Sometimes, the very language one uses, orders the sequences of thought in another way? I present this to you in this light, and hope you will read post when it materializes later.:) I am learning.

The substance of the official **Third Superstring Revolution** was posted on this Blog and officially(I recogize your statement) raises the line I have used of Witten.

Would you agree to entertain the **Third Superstring Revolution** in this way?

I might ask you then, that if this language had been changed, then what would the professor crossing the room mean? One would have look to what is materializing from condensed matter physics, by what is being presented, by Robert Lauglin. You see?

19. **Thomas Larsson**
November 16, 2004

Anonymous, the electron can to a good approximation be considered a free Dirac particle. The electron was discovered in 1895 and supersymmetry around 1970. Maybe this is an indication that having a Dirac particle is not sufficient for supersymmetry. However, I find it quite likely that electrons will be found at the LHC. If you wish, you may hail that as proof for supersymmetry.

Plato, the Third Superstring Revolution, also known as the Anthropic Revolution, happened in 2003. The interpretation is that experimental agreement is now irrelevant. String theory is correct even though it disagrees with observation, because God created the Universe to be compatible with human life.

20.
November 15, 2004

Hi DRL, {D,D} = 2 Box ! 🙂

21. **Plato**
November 15, 2004

Is it nice that Veltman recieved award with Gerard t Hooft in 1999? Is this correct?

IN response to Peter's post I posted about five threads in relation to what Peter and others might reconsider, as to the question of how we might look at quantum computerization(non)?

With the Third Superstring Revolution, what shall emerge from a change in Fundamental interpretation of Quantum Mechanics?

22. **D R Lunsford**
November 15, 2004

I'm sorry, but there is no Bosonic context for a Dirac particle. There is the old "neutrino theory of light" but those are not Fermions because of an implicit correlation that ruins the statistics.

Perhaps you could scribble here a few lines about the claim *infra*.

23.
November 14, 2004

If one really wanted to see 'formal' evidence for spacetime supersymmetry as in

> 'Superalgebras play a role here and there in physics, hence they are likely to also play a role in HEP.'

I would suggest to take the Dirac particle. It has worldline 1+0 dimensional N=1 supersymmetry in the 'right' sense. There is a Hamiltonian constraint which generates time evolution (the Klein-Gordon operator) and the Dirac operator is the corresponding supercharge.

This might be a coincidence of low dimensions. But one might take it as a 'hint' for spacetime susy that by just going from this worldline susy to an analogous worldsheet susy yields spacetime susy GR+YM .

24. **Thomas Larsson**
November 13, 2004

By definition, a Lie superalgebra is like a Lie algebra, except that some brackets are fermionic, so

[even, even] = even

[even, odd] = odd
{odd, odd} = even

A supersymmetry is a particular kind of superalgebra where some of the even generators can be identified with spacetime translations. Whereas superalgebras are a very beautiful and far-reaching generalization of Lie algebras, the extra translation condition that makes it into a supersymmetry does not seem particularly interesting.

But the two concepts are often blurred, as in the case of gold nuclei. One reason seems to be that supersymmetry has been more heavily sold.

25. **D R Lunsford**
November 12, 2004

I dont' see how you can have half a ball of wax. Please explain? How is it a superalgebra if

{Q, Qdot} not = some kind of translation

Perhaps I missed the definition of superalgebra.

-drl

26.
November 12, 2004

Yes, this 'supersymmetry' operation between nuclei has nothing to do with whether the (effective) field theory that describes the particles and interaction in our world enjoys symmetry under a graded extension of the Lorentz group (locally).

Graded algebras and Grassmann parameters are just concepts useful enough that they show up all over the place, not necessarily in the context of supersymmetry in high energy physics.

For instance in statistical physics many people will tell you that they are using 'supersymmetry methods' to evaluate their free energy or something. What they really do is conveniently express determinants as Grassmann integrals. So this method should rather be called a 'BRST-like method'. It has nothing to do with spacetime supersymmetry, either.

Of course some people feel inclined to argue that any piece of sufficiently nice mathematics should play a role in a TOE…

27. **Thomas Larsson**
November 12, 2004

To answer drl's question regarding what physical evidence points to supersymmetry, "nuclear supersymmetry" has been observed and verified experimentally. At Yale in 1990 it was suggested that supersymmetry effects might be seen in nuclear physics. A super-algebra was proposed that allows a nucleus with even nos of protons and neutrons(even-even), 2 nuclei with even nos protons/odd nos. neutrons (even-odd)and odd nos protons/even nos neutrons (odd-even), and an (odd-odd) to transform into each other via this supersymmetry algebra. Platinum-194(even-even), gold-195(odd-even) platinum-195 (even-odd) and gold 196(odd-odd)form such a supersymmetric quartet.

From what I've heard, this is not a supersymmetry, but rather an internal superalgebra symmetry. No bosonic subalgebra can be identified with spacetime transformations, which is the definition of a supersymmetry. An internal superalgebra symmetry does not necessarily imply superpartners of equal mass.

A genuine supersymmetry seems to exist in the tricritical Ising model in 2D, however; this is the CFT with c = 7/10. It is unclear to me what this supersymmetry corresponds to in experimental realizations such as a monolayer of Argon atoms on an inert graphite substrate.

Finally, I doubt that Lubos Motl will stay in his restroom for the rest of his life when no sparticles are found at the LHC. On the contrary, he will aggressively explain how absense of sparticles and extra-dimensions confirms that string theory is correct.

28. **D R Lunsford**
November 12, 2004

Steven M,

That's interesting, it means that SuSy works as a dynamical symmetry (like say isospin for a simple nuclear model). Of course it doesn't imply that the actual Bosons and Fermions of the world are related this way.

In the experimental situation described, how does the anticommutator

{Q_r, Qbar_sdot} = 2 sigma^a_r_sdot P_a (P_a = translation)

show up physically?

29. **Anonymous**
November 11, 2004

I don't want to comment on how much particle physics
has contributed towards astrophysics. One thing however is certainly true. in the last decade many experimental high energy physicists have switched over to astrophysics/gravitation experiments such as LIGO, GLAST, supernova csomology, CMB
experiments, TeV gamma ray astronomy experiments,
uhe cosmic rays, etc.
If no evidence for physics beyond the standard model is found by the time LHC starts I
expect more high energy physicists to switch over
to astrophysics. Also both Fermilab and SLAC
have set up astrophysics groups.
OTOH I don't know of a single example of an astronomer switching over to experimental high energy physics or
national astronomy institutes such as NRAO setting up a high energy physics group.

30. **Mubos Lotl**
November 11, 2004

Lubos Motl said: “I don’t care too much about dark matter – and I would agree with Veltman that the influence of astrophysics on theoretical high-energy physics has been small”.
Just to clarify: we string theorists don’t really care about dark matter and ESPECIALLY — Sean — we don’t care about so-called dark energy. String theory clearly favors a negative cosmological constant, so all current observations suggesting a positive one are clearly wild monkey speculations that have no connection with reality. String theory likewise says nothing at all about dark matter, so obviously it doesn’t exist.

31. **Steve M**
November 11, 2004

To answer drl’s question regarding what physical evidence points to supersymmetry, “nuclear supersymmetry” has been observed and verified experimentally. At Yale in 1990 it was suggested that supersymmetry effects might be seen in nuclear physics. A super-algebra was proposed that allows a nucleus with even nos of protons and neutrons(even-even), 2 nuclei with even nos protons/odd nos. neutrons (even-odd)and odd nos protons/even nos neutrons (odd-even), and an (odd-odd) to transform into each other via this supersymmetry algebra. Platinum-194(even-even), gold-195(odd-even) platinum-195 (even-odd) and gold 196(odd-odd)form such a supersymmetric quartet. Given the energy spectra of gold-196 the supersymmetry algebra gives the spectra for the other 3 nuclei in the quartet. Verified in the lab now by several groups. Other supersymmetric nuclear quartets have been found. This doesnt “prove” supersymmetry of course but makes its realisation in nature in particle theory, seem a whole lot more likely.
See
http://physicsweb.org/articles/world/12/10/3

32. **Hacik**
November 11, 2004

I believe expectations for the top quark mass were always “just around the corner”, creeping upwards year after year, until it was finally found. So there’s still hope.

33. **Matthew**
November 11, 2004

Just to rain on Lubos’s parade a little bit, most of the phenomenology talks here over the past couple of months have started out with why the current bounds on the Higgs mass strongly disfavour the MSSM. They don’t rule it out, but it’s strongly disfavoured.

My understand of the history is that SUSY has been “around the corner, next accelerator” since it was first dreamed up.

34. **Plato**
November 11, 2004

I thought I would add this in the hopes of “expanding” current limitations in matter considerations.

http://eskesthai.blogspot.com/2004/11/sound-waves-in-cmb.html

35. **D R Lunsford**
November 11, 2004

So Motl,

What physical evidence points to SuSy? Name one thing. One.

-drl

36. **Lubos Motl**
November 11, 2004

Of course that it will be shocking when SUSY is found. It will be so shocking that the people who have fought against SUSY will have to hide in their restrooms for the rest of their lives, Thomas. 🙂

At least I can’t really imagine how could you be able to appear publicly on the internet after your teachings will be devastated beyond reasonable doubts, and after everyone will see that you were trying to return the humakind to the Middle Ages. 😉

Moreover, it’s pretty likely that it will happen! SUSY is gonna be found, it will be dramatic, and string theorists are ready to take credit for it. 😉

37. **Thomas Larsson**
November 11, 2004

It’s easy to be provocative, as we know. If Veltman doesn’t think that astrophysics has contributed to particle physics, he must not care about the discovery of the muon, evidence for non-baryonic dark matter, dark energy, neutrino masses and mixings, constraints on everything from time-dependence of alpha to the mass of the photon, and just about all of general relativity. In the last thirty years, astrophysics has contributed enormously more to particle physics than ground-based experiments have.

In the last 30 years? Since November 1974? Even if you discount the J/psi, do you really think that beauty, top, W, Z, and the tau neutrino are nil? It may be easy to be provocative. Perhaps it’s also easy to win a Nobel, like Veltman.

38. **Sean**
November 11, 2004

It’s easy to be provocative, as we know. If Veltman doesn’t think that astrophysics has contributed to particle physics, he must not care about the discovery of the muon, evidence for non-baryonic dark matter, dark energy, neutrino masses and mixings, constraints on everything from time-dependence of alpha to the mass of the photon, and just about all of general relativity. In the last thirty years, astrophysics has contributed enormously more to particle physics than ground-based experiments have.

Of course you still need colliders at the energy frontier, and it’s misleading (although not completely dishonest) to sell them through the connection to cosmology. Denying that there is a connection is just dishonest.

39. **Thomas Larsson**
November 11, 2004

Lubos,

but of course I do care about the superpartners, if they exist, and/or the extra dimensions which would be even more shocking if they could be seen.

So it would be shocking if superpartners were seen? OK, I might agree on that, but I'm surprised that you think that it would be shocking if you won your experimental-susy-at-the-LHC bet.

Of course a linear collider must be built. I don't really care where, but it would be a disaster if this project were lost like the SSC, or apparently unable to produce any relevant information like Tevatron II. It is most likely the last big collider to be built in my lifetime.

40. **Plato**
November 11, 2004

Well I am on the outside looking in.:)

What value would Iscap be to the population of scientists then, and for that matter, subjects that would treat Bekenstein Bound, in relation to spin networks. You would have to throw John Baez in the bunch?

Would mathematicains then be left in abstract spaces not theoretically developing theIr math along side of the physics?

41. **Lubos Motl**
November 11, 2004

Of course that the linear collider would be built in order to learn the mind of God in the most scientific way possible. Do you think that these billions of dollars would be spent to learn the mind of Peter Woit? No! No one smaller than God is relevant in this game. 😉

Moreover, the linear collider has many advantages over the synchrotrons like the LHC. The latter is a hadronic machine, and the collisions are pretty dirty and most of the events are just pure QCD. Even if SUSY is seen, it may be pretty difficult to measure various parameters on the LHC.

On the other hand, the lepton machines are much better to measure the energy of all the particles in the game, and the date are sharper. It is hard to imagine that a useful new lepton machine could be built as LEP, and the natural step after the LHC is therefore a linear collider.

The linear collider would provide us with completementary information compared to the hadronic machines. I don't care too much about dark matter – and I would agree with Veltman that the influence of astrophysics on theoretical high-energy physics has been small – but of course I do care about the superpartners, if they exist, and/or the extra dimensions which would be even more shocking if they could be seen.

42. **Thomas Larsson**
November 11, 2004

I'm not so sure. The SSC was supposed to uncover the God particle, but it was cancelled anyway.

43. **Hackticus**
November 10, 2004

Physicist should just start sayin that their accelerators will "probe the mind of God" or somthin, they'll get all the money they want from this gumint. I'm sure Lubos would be happy to testify before Congress.

QCD and String Theory at the KITP

November 18, 2004
Categories: Uncategorized

The KITP at Santa Barbara is holding a conference on [QCD and String Theory](#) this week, and the talks have started to appear online.

Of the ones I've taken a quick look at so far, there doesn't seem to be any obvious recent progress on the 30-year old main question that everyone would like the answer to: can one find a reliable analytical technique for dealing with QCD in the infrared regiion where the effective coupling is strong? The best hope for this in recent years has been the AdS/CFT correspondence, but after seven years the state of the art there still seems to be a long ways from solving the problem one wants to solve (although it does give solutions to other problems). I'm looking forward to seeing what some of the later talks will have to say, including Larry Yaffe's one tomorrow on "Large N gauge theories: old and new".

Comments

1. **BEB**
   November 19, 2004

   Actually, contrary to what Strominger and Motl may think, some physicists (even string theorists!) do consider it to be among the "top ten "millenium problems" posed:

   "Can we quantitatively understand quark and gluon confinement in Quantum Chromodynamics and the existence of a mass gap? "

   http://feynman.physics.lsa.umich.edu/strings2000/millennium.html

   Most of the other problems seem to be string theory inspired, and some (like 1 and 7) frankly not answerable, IMHO. They do not have the flavor of Hilbert's problems of really encompassing the breadth of the subject. I would have expected a better list.

   What are some of the more important subjects to pursue? String "phenomenology"? Glad I am not the one being brainwashed and be led astray.

2. **Peter**
   November 18, 2004

   The problem with QCD isn't that there's no rigorous proof of confinement, it's that there's no controlled analytical approximation that captures what is going on in the theory at long distances. Lacking this, there's a real sense in which we don't understand quantum gauge theories. For QCD itself, maybe we don't care that much since we have lattice calculations, and the evidence for the theory is so overwhelming we don't really need to extract more predictions from it to compare with experiment.

   But it's an important part of physics and we really should be able to understand what is going on there. The best bet for this kind of understanding has always been to find a consistent 1/N expansion of the theory, probably using string theory methods. AdS/CFT is tantalizingly close to getting somewhere with this, it's surprising to me there hasn't been more progress recently. I don't think Strominger is helping the situation if he is dismissing the importance of this problem and encouraging students instead to concentrate on the doomed project of unifying the Standard Model and gravity via some sort of 11d supersymmetric M-theory.

3. **Lubos Motl**
   November 18, 2004

   One thing that Peter is not able to appreciate is that the character of the *important* questions is changing with time, as our understanding becomes deeper.

   In fact, this question of confinement was *exactly* the example used by Andy Strominger last week when he was explaining the situation of string theory to the new grad students.

   He said that when he was a student, the most important question in theoretical physics was to prove that QCD confines at low energies. "It's still an interesting question and you may get 1 million USD from the Clay Institute if you solve it," Strominger said, but he explained that we simply have overwhelming evidence that confinement is true and QCD works, and we must simply move on.

   I totally agree with Strominger on this point. There are many other important questions whose answers we really do not know, and answering them would be much more important than to find a rigorous proof of some answers that we're anyway pretty sure that are correct.

4. November 18, 2004

   That pretty much summarizes all progress in string theory since the 1980's: never solving the problems you thought you wanted to solve, but finding solutions to problems it never occured to you to ask.

String Theory and Philosophy

November 18, 2004
Categories: Uncategorized

From Sean Carroll's weblog I see that he's in Austin now for a session at a meeting of the Philosophy of Science Association. Philosophers of science seem to actually write up their talks in advance, and many of the talks for this meeting are already available online. Poking around on their website with papers from earlier conferences, I ran into one on Scientific Realism and String Theory by Richard Dawid.

Dawid appears to have swallowed the hype about string theory hook, line and sinker. He believes that string theory exhibits a new paradigm of how to do physics, one where the idea of being able to calculate anything about the real world and compare it to observations is passe. All that matters now is "theoretical uniqueness", that one's theory is the only possible one. He doesn't seem to notice that there's something kind of funny about people claiming that they have a wondrous unique theory, but don't know quite what it is and can't calculate anything about the real world with it. The S-matrix theorists of the 60s also promoted the idea that they had a wondrous unique theory, but didn't know quite what it was. Probably one can dig up philosophy of science articles from that period about how a whole new paradigm of how to do science was required.

Dawid also seems to believe that the dualities of M-theory imply the "dissolution of ontology", that "the ontological object has simply vanished". In reality, what has vanished is not the ontological object, but the theory.

Over at Robert Helling's web-site you can read an example of the latest philosophical excuses about why string theory now can't predict anything, together with implausible wishful thinking about how this might change since "It's just at this stage we are not yet powerful enough to make these kinds of predictions".

For the life of me I can't figure out why smart physicists and philosophers can't see the obvious fact that is staring them in the face. You don't need a new paradigm of how to do science, the old one works just fine. If you have a conjectural theoretical scientific idea, there are two ways in which it can turn out to be wrong. Either it predicts something that disagrees with experiment, or it is so vacuous that it predicts nothing. The evidence is now overwhelming that, if string theory is consistent at all, it is wrong for the second reason.

Update: Dawid actually has a whole fancy web-site about Realism and String Theory. He also has a newer paper on Undetermination and Theory Succession from a String Theoretical Perspective.

The PhilSci archive does have another paper about string theory, one by Reiner Hedrich entitled Superstring Theory and Empirical Testability. Hedrich is much less credulous than Dawid, noting about superstring theory "above all, it has fundamental problems with empirical testability – problems that make questionalbe its status as a physical theory at all."

Comments

1. **plato**
   November 21, 2004

   Arun,

   As noted earlier in Peter's blog, there are recent mathematical advances which required concentrated work for several years of silence for the authors.

   There is no doubt in my mind the contemplative thinking asks that we recognize the essence of any equation, and a lot of times. The mind clicks a notch, when sitting by a stream(ask witten), and the mind had disassociated itself, from such efforts.

   There is a always a incubative time for consideration, as you quote saids. I thought, all mathematicians had good focus?

2. **Arun**
   November 21, 2004

   Are there reasons other than the overselling of string theory that the situation hold that Peter describes?

   For instance, perhaps string theory presents research problems that are more likely to reach fruition in one post-doc term than other areas?

   As noted earlier in Peter's blog, there are recent mathematical advances which required concentrated work for several years of silence for the authors. Are there sociological factors operating in physics that preclude the kind of work that might be needed for such breakthroughs?

3. **Thomas Larsson**
   November 21, 2004

   Steve M.,

   Peter is of course completely capable of answering for himself, and has already done so. My perspective is somewhat different, in that I have a well-developed suggestion for the missing mathematical structure.

   Let's also assume absence of any new experimental guidance, then what alternative mathematical structures or approaches in our opinion (ie non string theory) would have enabled a viable leap (or even step) beyond the SM or GR? I assume you would approve of deeper investigation into the mathematical underpinnings of the SM like gauge fields/YM and Dirac operators, but people have looked (are looking) at this anyway surely?

   I have looked one particular mathematical underpinning of the SM and GR since 1987, but despite rather remarkable progress the physics community has been completely uninterested. From this I have concluded that people who already know the correct Theory of Everything have simply no interest in learning about new mathematics, not in the 80s and not now. Mathematicians have in my experience always been much more open-minded, perhaps because everything that I have done has also been worked upon by some mathematicians because of its intrinsic mathematical interest.

   More precisely, I'm talking about the quantum representation theory of the correct constraint algebras of GR and the SM. The constraint algebra (in covariant formulations) of GR is the group of 4-diffeomorphisms, and the constraint algebra of the SM is the group of gauge transformations based on SU(3)xSU(2)xU(1). Upon quantization these algebras acquire quantum correction, making them into higher-dimensional analogues of the Virasoro and affine Kac-Moody algebras. That such algebras exist, and that they have an interesting representation theory is by itself some kind of mathematical success.

   This goes beyond field theory, because a 4D Virasoro algebra is a diffeomorphism anomaly in 4D, something which can not exist within field theory. Nevertheless, there are no assumptions about new physics here. 4-diffeomorphisms and SU(3)xSU(2)xU(1) gauge transformations are simply the correct constraints of GR+SM. So one reason why people have failed to go beyond GR+SM might be that there isn't anything there; at least there are no conclusive experimental hints. There are some well-known mathematical problems, e.g. that GR and QM are inconsistent and that the SM is somewhat ugly (although by no means as ugly as string theory), but this may well be explained by the fact that people have missed the existence of the relevant anomalies.

In the absence of experimental guidance in 25 years now surely all possible alternatives are just wishful thinking too and can't make any predictions either? Everything else that has ever tried to go beyond the SM and GR has run into their own technical brick walls too so you can't really just single out string theory esp when it was the most promising.

I don't think that loop quantum gravity is particularly promising neither. However, when evaluating the promise of an approach it is not fair only to judge the physical achievements (string theory: 0, LQG: 0, me: 0), without taking into account the amount of time and funding invested in it. Public spenders must expect public scrutiny, and big public spenders must expect more public scrutiny than small ones. If the funding to LQG were raised to string theory levels, e.g. by redistributing funding from string theory, and LQG didn't make any spectacular progress after a decade neither, then it would be fair to criticize it too.

Another intrinsic aspect of string theory is the consistent overselling for the past 20 years. I always felt dubious about a theory without experimental support, but what convinced me that string theory is not only wrong but detrimental to science was a Physics Today article by Gordon Kane in 1997, entitled "String theory is not only testable, but super-testable". The title vividly illustrates the complete discrepancy between the hype and the actual achievements. In this respect, string theory is totally unique in serious science, and can only be compared to pseudo-science.

4. **Peter**
November 20, 2004

Hi Steve,

A couple comments. You're right that my own point of view is that, given the lack of experimental guidance, the most promising thing for theorists to do is to try and learn more about the mathematical structures used in the standard model and see if new and better ways of understanding them can be found. There seem to me to be quite a few things of this kind to investigate, but I can tell you from personal experience that if you try and talk to most theorists about these issues, they're really not interested. The attitude has become very widespread that the only possible mathematically sophisticated approach to particle theory is string theory, and that anyone who thinks otherwise is just too stupid to be a string theorist. Throughout the late eighties and into the early nineties, besides doing string theory, Witten came up with a lot of new ideas about gauge theories, but for the last ten years or so he seems to have decided to devote his full energies to string theory.

I don't think there's only one thing people should be working on. In the absence of any particularly promising idea, which is where we are now, they should be trying out a wide range of different things. But there needs to be a much more honest process of recognizing when ideas don't work and moving on to try something else. What's completely dysfunctional about the field is the way it continues to be dominated by the idea of unifying the SM and gravity in an 11d M-theory of some kind, long after it should be clear this can't work. It would have been unreasonable to demand predictions from string theory during the first few years that people were seriously looking into it, but twenty years later, the fact that it can't produce a single prediction of any kind should count very heavily. String theorists should be honest about how bad the situation is, and stop heavily hyping and promoting the theory unless they can make some real progress with it.

The main question facing the field seems to me to be that of how to encourage more people to try different things, and not just stick to one failed idea because it is what every one else is doing.

5. **Steve M**
November 19, 2004

Peter, I am curious about something. Let's suppose we could turn the clock back 20 years to 1984 and let's imagine that Green and Schwarz never did their famous Colorado calculation. String theory remains a backwater of physics in the 80s pursued by only a few diehards. So the question is: how would you have liked physics to have evolved–if you had had your way–to the present day, and would the situation really be any better than it is now? Or perhaps even worse? Let's also assume absence of any new experimental guidance, then what alternative mathematical structures or approaches in our opinion (ie non string theory) would have enabled a viable leap (or even step) beyond the SM or GR? I assume you would approve of deeper investigation into the mathematical underpinnings of the SM like gauge fields/YM and Dirac operators, but people have looked (are looking) at this anyway surely? In the absence of experimental guidance in 25 years now surely all possible alternatives are just wishful thinking too and can't make any predictions either? Everything else that has ever tried to go beyond the SM and GR has run into their own technical brick walls too so you can't really just single out string theory esp when it was the most promising.

6. **plato**
November 19, 2004

DRL,

I do not want to defer on recognizing your enthusiam either:)

The increasing complexity, of what one might reveal of the cosmo, with a microcosmic view, had me consider how either the string theorist or the LQGist might interpret, that same cosmos. Gamma detection is one view right.:)

There are obvious attempts to interpret quantum gravity. I was looking for how they might view the picture supplied in my previous post.

If it is becoming increasing complex , the basis of their **determnations** would have to have a foundation to begin with, right?

Mathematically, how could two views of picture supplied in previous post, be so different, and are they?

As I stand, under the arch of reason, I am asking about **that one thing**. City Slickers, was very interesting from my historical perspective:)

7.
November 19, 2004

Lubos,

String theory, once it's understood completely, cannot really be an approximation of anything deeper – it follows from its basic mathematical features.

Can you explain this in more detail for a layman?
(layman = math grad student, in this case)

8. **Lubo? Motl**
November 19, 2004

Hi,

I just want to say that I've seen your replies, and I find it inappropriate to answer this kind of feedback.

All the best
Lubo?

9. **Chris Oakley**
November 19, 2004

String theory, once it's understood completely, cannot really be an approximation of anything deeper – it follows from its basic mathematical features. There were many complicated historical coincidences that have led up to this point – namely string theory – but once we know that it's a fact, there is some sense in which it could have been derived without explicit data from experiments, by "pure thought" much like many ancient philosophers were dreaming.

I am going to go into suspended animation for a few decades. Could someone please revive me when Harvard physics professors start doing science again?

10. **Thomas Larsson**
November 19, 2004

But this feels worse, as it is based on what is being peddled by very smart and established people. Someone once told me that in the 80s David Gross predicted that string theorists would be able to predict the quark mass matrix within a decade. It is safe to say that that day will never come.

Stephen Weinberg was here in Stockholm in December 1984, attending the Rubbia-van Meer Nobel party. At a colloqium he predicted that some smart young string theorist would win the Nobel Prize in 1991. Well, that prediction was at least falsifyable.

11. **Chris W.**
November 18, 2004

This old [post]() by Michael Nielsen has some relevance to this discussion. (Don't ignore the comments.)

So does this well-known [commentary]() by Albert Einstein.

12.
November 18, 2004

"What sort of scenario do you have in mind? How could string theory be proved irrelevant?"

Bon Motl believes string theory follows rigorously from the existence of God. So, if you can just disprove the existence of God, then even he will admit string theory is irrelevent.

13. **Chris W.**
November 18, 2004

Of course that all these conclusions would break down in the (unlikely) scenario that string theory will eventually be proved irrelevant.

What sort of scenario do you have in mind? How could string theory be proved irrelevant? I can't imagine what a "proof" of irrelevance is supposed to mean here. On the other hand, I can imagine how the majority of theoretical physicists might lose interest in string theory's continued development, even in the absence of such a "proof".

And, while I'm at it, how would we know that string theory is a "fact"? We don't know that any scientific theory is a fact. Theories may successfully *account* for facts, but they are never known to be facts themselves. That they have achieved some empirical success can certainly be a fact; indeed, these successes are among the more important facts that we hope to explain with subsequent theories.

Of course, if a theory has never achieved any empirical success, or at least no *unique* empirical success, it is more likely* to be regarded as irrelevant, simply because it offers nothing that calls for a explanation.

(* Physical theories often retain some relevance as sources of ideas for building new theories, but in this respect they share the stage with many ideas from mathematics, philosophy, and even engineering.)

14. **D R Lunsford**
November 18, 2004

Plato,

While I admire your enthusiasm, you have absolutely no idea about what is going on in physics. You should keep that in mind before posting.

-drl

15. **D R Lunsford**
November 18, 2004

My feelings about philosophers are usually negative, but this guy was able to capture some important philosophical differences between string theory and previous pictures how to do science that should undoubtedly affect the way how philosophers think about science and its developments.

And this is what I specifically detest about you folks – as if you, who have predicted nothing, and accomplished nothing, and DO nothing other than produce hot stupid uninteresting AIR, have the right to stand in judgment on the methods of Newton, Einstein, Maxwell, Faraday, Hertz, Kelvin…

You in particular, Motl, have never written a scientific word worth reading, notwithstanding your "lofty" position. Your bombast and bluster are nothing but a cover for your empty sophist's self-indulgent house of cards. You will never accomplish anything in science – your very presence is an affront to reason.

You toss around the word "old fashioned" like a radical's gas cocktail thrown through the window, a crazy Bolshevik on a mission from your "God". Save me from the "good" people.

16. **plato**
November 18, 2004

Maybe people just do not like dealing with the **complexity** of the issues?

We all like solid things, we just don't know how they got that way from a earlier time?

As a outsider, it is not to hard to maintain a view of the arguements that are presented when theoretical minds engage themselves.

Maybe it as easy as saying that the following picture, can be taken one of two ways? Are The extremities of these theoretical positions, presenting themselves this way?

I thought of putting the old woman versus beautiful woman picture, that changes when perspective is changed. But the picture choosen, should do.

17. **Lubos Motl**
November 18, 2004

My feelings about philosophers are usually negative, but this guy was able to capture some important philosophical differences between string theory and previous pictures how to do science that should undoubtedly affect the way how philosophers think about science and its developments.

Of course that all these conclusions would break down in the (unlikely) scenario that string theory will eventually be proved irrelevant. However, if it's relevant, the various standard old-fashioned ideas must be revisited. Every previous theory was an approximation of a better theory. String theory, once it's understood completely, cannot really be an approximation of anything deeper – it follows from its basic mathematical features. There were many complicated historical coincidences that have led up to this point – namely string theory – but once we know that it's a fact, there is some sense in which it could have been derived without explicit data from experiments, by "pure thought" much like many ancient philosophers were dreaming.

Of course, these statements are just "in principle" because – first of all – history was very different and observations were totally essentially for us to move in essentially the right direction and – second of all – we still don't know exactly how to complete string theory in such a way that the true quantities in the Universe can be calculated with the desirable precision.

I am not gonna comment on Peter's conjectures about what's the reason why string theory "is wrong" because I think that these comments of Peter are stupidities that simply don't deserve my time right now.

18. **BEB**
November 18, 2004

Well said, Peter.

I think I am seeing another example of the over-selling that will ultimately backfire. The previous one being some of the things said in support for the SSC (that it could lead to cure for cancer!). I too wanted the SSC, but I wanted us physicists to be honest about it.

But this feels worse, as it is based on what is being peddled by very smart and established people. Someone once told me that in the 80s David Gross predicted that string theorists would be able to predict the quark mass matrix within a decade. It is safe to say that that day will never come.

Or maybe the problem is me; I do not believe strongly enough in the "rapture" from "faith-based" physics;)

BEB

19. **D R Lunsford**
November 18, 2004

The opening paragraph of that Dawid opus made me laugh out loud. What a pile of garbage. I can't believe people get paid to make up this tripe.

More Hype On Its Way

November 21, 2004
Categories: Uncategorized

The latest trend among prominent theorists seems to be the writing of popular books hyping the unsuccessful speculative ideas they have been working on. Two new examples of this have been pointed out by Lubos Motl over at sci.physics.strings.

Both of these books are due to appear at the beginning of next May. One, by Leonard Susskind of Stanford, is entitled An Introduction To Black Holes, Information And The String Theory Revolution: The Holographic Universe. The second, by Lisa Randall of Harvard is called Warped Passages : Unraveling the Mysteries of the Universe's Hidden Dimensions.

Randall's book presumably is not so much about string theory as about the idea that we live on a brane inside a higher dimensional space. As far as I can tell, there's even less evidence for this idea than there is for string theory itself. I don't know exactly what her attitude about string theory is, but at a public debate at the Museum of Natural History here in New York a few years ago, I remember that she scornfully dismissed the argument that string theory predicts gravity, saying something like "Yeah, it predicts ten-dimensional gravity."

Comments

1. **Chris Oakley**
November 24, 2004

   Steven Hawking has already achieved notoriety as a gangsta rapper. As for Lubos's band, if he changes the name of his newsgroup to alt.religion.strings instead of sci.physics.strings then I don't mind playing the Sax for him (I'm a bit rusty, but I could brush up).

2.
November 23, 2004

   The physics community has spent the past half century or so basically living off the glory of having created the atomic bomb. A few farsighted physicists have recognized that this will not last forever and that physicist will have to find a new way to market themselves to the public: as entertainers! Steven Hawking's greatest contribution to physics is in pioneering this transition. Susskind and Randall are the latest to hop on the bandwagon and show great promise for creating appealing public personaes. Lubos is well positioned to follow in their footsteps provided he develop some distinctive peronality "hook" that will resonate with the public. Perhaps he could take up dueling or wear an eye patch, or start a rock band with some Harvard postdocs.

3. **wino**
November 23, 2004

   "Sorry that post came out pompous sounding. I just mean that folks don't need to be "protected" from popularizations of speculative physics."

   It is not a question of "protection" of the lay public from "speculative physics"; it is a matter of clearly distinguishing between speculative physics and physics. There are lots of beautiful ideas, which turn out not to have anything to do with reality; they are not physics.

   When one throws about about grandiose terms like the "Theory of Everything", the impression is that it fully describes and explains the world in detail; no hint of speculation here. This is very different from say, electrodynamics, or the standard model, which are in some sense correct in a very detailed sense (precision measurements), (though that does not necessarily mean they explain all of condensed matter physics, biology or chemistry in detail, as practitioners in those fields will argue).

   Of course, I must reserve judgement on the books until I have read them. But the track record in recent times of what appears in popular science magazines (like Scientific American) about such subjects does not inspire much confidence.

4. **D R Lunsford**
November 23, 2004

   Steve – once we had George Gamow. Now, we don't.

   -drl

5. **Steve Esser**
November 23, 2004

   Sorry that post came out pompous sounding. I just mean that folks don't need to be "protected" from popularizations of speculative physics.

6. **D R Lunsford**
November 23, 2004

   C'mon Lubos. surely you know some good old time Bohemian insults! I feel like I just got flamed by Eddie Haskell.

   -drl

7. **Steve Esser**
November 23, 2004

   Gentlemen: let me defend the educated layperson's ability to read popular accounts of developments in physics and draw reasonable conclusions about what is going on in the field. Greene's books, for example, were clearly and I think honestly written — such that despite his admirable enthusiasm one could come the conclusion that the last 10 years of string/M-theory has been somewhat of a disappointment relative to previous high expectations.
   Here's hoping talented physicists of all stripes endeavor to write good books for the lay audience.
   Regards, – Steve

8. **Lubos Motl**
November 23, 2004

Dear DRL, I am not surprised that your co-workers think that you don't enjoy mental health. In fact, I agree with them. I hope that they will call a doctor to save you. 😉

9. **D R Lunsford**
November 23, 2004

Peter –

Thanks a lot, I have coffee dribbling from my nose…

"String theory is mathematical science fiction" – my coworkers are all wondering what I'm laughing about…the best comment of the decade to date…

-drl

10. **Thomas Larsson**
November 23, 2004

Lubos, a couple of days ago you frankly asserted that SUSY *will* be seen, and that I would have to stay in the restroom for the rest of my life. Evidently you get cold feet as soon as somebody calls your bluff. Oh well, its your credibility…

11. **Lubos Motl**
November 23, 2004

Thomas, certainly not.

If we see supersymmetry, it will be a good circumstantial evidence that we are on the right track. It won't really prove string theory – one can really think about SUSY independently of string theory; it will just increase the value of string theory's stocks.

If the LHC does not see supersymmetry, it will be a discouraging news, but it certainly won't prove that string theory is wrong. First of all, SUSY can be broken at slightly (or much) higher energies than the LHC scale. Second of all, it is an open question whether there exists inherently non-supersymmetric backgrounds in string theory. The only fact is that SUSY is important for us to have a mathematical control over what happens in string theory.

Moreover, it is still possible that the LHC will not see SUSY, but it will nevertheless PROVE string theory – by seeing the excited strings, and/or various detailed phenomena related to extra dimensions. Of course, an excited string mode is a more typically stringy phenomenon than SUSY itself.

12. **Thomas Larsson**
November 23, 2004

Lubos,

Would you state for the record that the discovery of superpartners, or not, is a lithmus test for string theory? If sparticles are discovered string theory is probably on the right track, but if no sparticle is found within five years after the LHC is commissioned, then string theory has been disproven?

For the past 20 years, Witten has repeatedly stated that string theory makes one postdiction, gravity, and one prediction, supersymmetry. Are you willing to stand up and proclaim that if Witten's single prediction fails at the LHC, then string theory has failed?

13. **wino**
November 23, 2004

I think the books that are being talked about here are not science fiction, they belong to the category of "scientific fiction" 🙂

What the string theorists (posting here, and in general) do not understand/realize is that they are ultimately going to cause great damage to the public perception of what physicists/scientists do (non-string theorists, in particular) and what physics/science is all about. By lowering the bar on what constitutes "physics" to such an extent that it is not terribly important to do things testable in the foreseeable future, is to do great disservice to the testable theoretical work (verified and falsified) done in so many sub-fields. Forget Peter Woit, a layman (taxpayer) will ultimately wonder if there is any difference between string theory and theology. I know it is an unfair comparison, but it will be an increasingly harder case to make as the great claims being routinely made cannot be proven. By peddling "sexy" stuff, of course book sales will be good, but this will be at a cost in the future: damage to credibility of science. Then, we physicists will be under siege, not unlike the biologists these days.

I have not seen the books, but I suspect they will
convey the impression that these are verified and tested ideas and gloss over (or even mention) the obvious problems, such as the wide variety of outcomes from "unique string theory". Even if they sell it as a really good piece of mathematical physics, it will be alright. But no, I suspect, it will be the pretentious and condescending "Theory of Everything", the "final word" with "a few details" left to be worked out. This is in sharp contrast with popular books on QED or GSW model, or even pure mathematics (Wiles) which are much more intellectually honest.

14. **Lubo? Motl**
November 23, 2004

Thomas Larsson, I have not read Lenny's book, so how can I criticize it? This would probably be your approach to criticize books that you have not read. Anything that is connected – at least by some vague link (like the name of the author) – with string theory immediately becomes a target of your criticism, even though you have no idea what it actually is.

This is the stupid, blinded, fundamentalist way to judge the world around – it is your way, Thomas and Peter. Sorry, but we're at a slightly different level. I apologize, but this way of approaching reality is one of the main reasons why the two of you are viewed as morons and your opinions do not have much value.

I suppose that Lenny will also write about the landscape, but my assumption is still that it will be mainly a book about holography and string theory and such, and because I know Lenny, I expect this book to be a good book. But I just can't tell you before I see it, do you understand this point???

15. **Lubo? Motl**
November 23, 2004

Chris Oakley, you misunderstood the identity of different people on this board. Peter Woit has been criticized by non-string-theoretical particle physicists, such as Matt, as well as by string theorists.

It's just the stupid belief of these hard core Woit believers that Peter Woit knows what he's talking about. He has no idea where string theory is and what it is, but he also has no idea about current particle physics and its real tasks for the future.

Peter Woit does not really understand the hierarchy problem, which is pretty bad for a person who tries to write something about particle physics.

16. **Lubo? Motl**
November 23, 2004

I have not answered these things – right now I'm on a modem. 😉

Sorry, glueless, but your basic assumptions and understanding of the meaning of popular books is absolutely wrong.

Popular books must ALWAYS be focused on the current cutting edge research and the future, otherwise they can't really be popular and virtually no one will read them.

You are also absolutely confused if you call the material of Brian Greene's book "fringe science". Fringe science means something absolutely different. Brian Greene describes the *mainstream* science, and it is just your personal problem if you have not been able to understand that string theory is mainstream.

Other people raised a lot of totally ridiculous points that people are only writing popular books when their best years are over. Of course that some people can only afford to write a book that will be popular *after* they become famous, but it by no means implies that they cannot make other important discoveries in the future.

I find your criticisms to be a complete misunderstanding of the role of science, its popularization, the interests of the readers, and the books themselves – and it seems as a waste of time (and cents for modem) 😉 to reply. It's pretty clear that an average person with some common sense understands what drives the books and what's important much better than you do – so try to ask average people on the street if you want to have at least a glimpse of an idea.

Best
Lubos

17. **JC**
November 23, 2004

My impression is that this place seems to resemble a "mob of kangaroos", with very little order and a very fleeting loyalty of any sort. It resembles less like a cult with a "high priest" as its leader.

If this was a political party of some sort, it would confirm the idea that "loyalty" cannot be "bought" but can only be "rented out" temporarily. (In a metaphorical sense, that is).

18. **Chris Oakley**
November 23, 2004

There are a number of posts earlier by String theorists attacking Peter's real or perceived views on what needs to be done next in particle physics phenomenology. I feel that these miss the point. Any one of you could give your views on any subject at all, and anyone else could pick holes in it. This is part of the whole process. The thing that Peter is objecting to, and the reason for the title of the blog, supported by his so-called "loyal following" (which I am afraid, apart from agreement on the one issue, is in practise not likely to be much more loyal than a herd of cats) is the way in which the devotees of one highly speculative approach seem to have hi-jacked the subject, thereby, given the limited resources available overall, all but silencing new proposals that do not fall within the String theory umbrella. *These critcisms are long overdue*.

19. **Peter**
November 22, 2004

John Horgan has a nice description of string theory and the like as "science fiction in mathematical form". Personally I have nothing against smoking dope, watching Star Trek and thinking about how cool all the multiple universes and extra dimensions are. But the reason I went into physics was that it was science, not science fiction.

If people want to write books about things they think are cool, but not likely to be right, they should acknowledge that they are writing science fiction, not science. Right now, I think more and more people can't tell the difference.

20. **Aaron**
November 22, 2004

Well, I disagree about the desert of books between Feynman and the present. There are plenty of books devoted to general relativity, cosmology and the standard model.

You'd like books on TQFT, condensed matter and CFT, I see. Good luck selling those books. Hell, if anyone could write a popular book about TQFT, I'd be impressed. It's the sexy stuff that sells.

And really, I don't understand this word 'hype'. People aren't writing these books to get more funding or anything like that. Many scientists really do like to share what they're working on with other people. Hell, when I had a blog once upon a time, I wrote a long post on faster than light travel in physics. Has that been 'vetted'? Is it likely to be right? Of course not. But, it's cool stuff, and I thought it would be fun to share it.

So, are braneworlds likely to be right? No. But are they cool? Yes. Could they make a good book? Definitely.

Lighten up. Physics is supposed to be fun. People being excited about their work is a *good* thing.

21. **Bob McNees**
November 22, 2004

The notion that "Writing popular books is something physicists usually do when their most productive years are behind them" is absurd. There are a lot of very active scientists who, like Aaron points out, are excited about sharing the ideas they are working on. For the most part our work is supported by government agencies like the NSF or DOE. So the public thinks that fundamental science is a worthwhile endeavor and they're willing to fund it. Some of us are grateful for that, and we'd like to explain what it is we've been doing.

Also, I'd like to see you tell Lenny that his good years are behind him.

22. **glueless**
November 22, 2004

Lubos and Aaron,

You haven't answered Peter's seemingly
obvious point, about the disproportionate
number of books devoted to new-age physics
written by current leading practitioners.
To some extent this is a trend in all of
academia (professors retaining literary
agents hoping to cash in a la Schama or
Norman Diamond), and the accelerated
publication and funding cycles leading
to excess hype.

Here is an experiment you could try;
certainly in the Boston area
Lubos can check that the following is true:

Go to a non-technical bookstore with a popular physics section. You will find that there are
several books on material that is speculative,
fringe, or simply unproven — written by current working theorists. e.g., Smolin on loop gravity,
Greene on strings, Maguiejo on variable
speed of light, and now the Susskind and Randall
books. By contrast, there is LITTLE OR
NOTHING available, let alone written by leaders
in the field, about established but no less
exciting subjects like the standard model, conformal field theory, quantum topology a la Atiyah and Witten, condensed matter theory,
etc. The one such popularization I can remember is Lederman's reminiscences published a few years ago. The math section had some books on knot
theory that, in passing, mention TQFT, but that's
about it.

There is a "big desert" of (non)publications
between Feynman's QED popularization, and
the new-age books of Greene et al.

It's hardly a matter of self-appointed guardianship of the public or scientific morals.
There is a general tendency to hype results
(or theories without "results" in the
ordinary sense of the word) before they
are vetted in the traditional ways.

23. **Peter**
November 22, 2004

Hi Aaron,

I think this is kind of repeating myself, but, no, I don't think particle theorists should all be thinking about mathematical foundations and not trying to extend the standard model in some phenomenolgically useful way. The fact of the matter is that there isn't much in the way of promising new ideas around, so people should do a wide range of different things, trying to come up with something new. But whatever they do, they should be honest with themselves and with others about the prospects for the ideas they are working on. Despite many years of work on large extra dimension scenarios, as far as I can tell there's nothing even close to a compelling model of this kind and virtually no one I know of would be willing to bet any significant amount of money on the LHC turning up evidence for these scenarios.

24.
November 22, 2004

Writing popular books is something physicists usually do when their most productive years are behind them. So I can understand Susskind, but Randall should still have a few good years left in her.

25. **Aaron**
November 22, 2004

Some people have proposed giving up on SUSY to solve the hierarchy problem and just using it to achieve unification of the couplings and maybe a dark matter candidates.

Now, the motivation for this work was anthropic, but it's important to say that it's perfectly legitimate to consider it as a fine tuned model with definite predictions for what will happen at the LHC. I find the anthropic verbiage that accompanies a lot of this sort of work distasteful, but I don't see why we shouldn't look at fine tuned models that have experimental consequences.

I share Matt's astonishment that Peter would rather have the high energy physics community mucking around with the mathematical foundations of the standard model rather than trying to extend it in ways that have definite, observable consequences within the decade.

As for writing of the books, if Peter wants to be the self-appointed guardian of the public interest, so be it, but might I suggest that maybe the reason people want to write books is that they are excited about their ideas and want to share them with the public?

Well, that and money.

26. **D R Lunsford**
November 22, 2004

I have the perfect title:

"Malice in Wonderland"

"Being the story of a six-foot clueless scowl"

27. **JC**

November 22, 2004

Lubos,

Since you seem to like to write a lot, have you ever thought about writing a popular press book on string theory in the form of a personal autobiography?

28. **Thomas Larsson**
November 22, 2004

Matt,

The experimental situation is often summarized as "Supersymmetry requires fine-tuning at the one percent level". Has this any relevance to SUSY as a solution to the hierarchy problem?

Lubos,

Earlier you have been quite critical about the Landscape and the Anthropic Principle. It seems like Susskind's book is mainly about such ideas. Is there any reason why you promote it now?

Peter,

Another interesting energy scale is the cosmological constant (or CC^(1/4)) at about 10^(-3) eV, close to the estimated neutrino masses.

29. **Plato**
November 22, 2004

I wonder how they ever arrived at here? Maybe this is better?

30. **Peter**
November 21, 2004

Hi Lubos,

The Planck mass is not necessarily the fundamental mass scale governing quantum gravitational effects. This is only true if you assume that whatever physics is producing the coefficient in front of the low energy effective Einstein Hilbert action is some combination of one particular mass scale and some dimensionless numbers of order one. The large extra dimensions scenarios evade this, and you can imagine otherways of evading it. For instance the dimensionless numbers that enter into the calculation of the effective low energy theory may be exponentially small. I wouldn't be entirely surprised if the fundamental scale at which quantum gravity became relevant was a TeV, but I don't find the large extra dimension scenarios very convincing (they're kind of ugly and they don't predict anything).

Believe it or not, I had heard somewhere that fermion masses came from Yukawa couplings to the Higgs…. My point was that we know that these dimensionless numbers (ignoring issues about neutrino masses) range in values over many orders of magnitude: the ratio of the electron mass to the top quark mass is about 2 x 10^-6. Given the experimental fact that such small dimensionless numbers occur in the theory, I'm not sure why one should take seriously issues like the "little hierarchy" problem of the Higgs VEV being 40 times smaller than one might expect.

31. **Lubos Motl**
November 21, 2004

Your point 2 is even more ridiculous, Peter. Writing cheap articles "string theory is bad" for some of your idiotic readers is a profession that you mastered well, but particle physics is something very different, it seems.

The little hierarchy problem is a small artifact of the hierarchy problem that survives even if we solve the usual hierarchy problem in one of the standard ways.

But why do I say that your comment 2 is ridiculous? Because you're treating different masses as "mass scales". They may be mass scales, but according to the Standard Model or derived theories, they're not "fundamental mass scales" in any way. The electron mass is small because the Yukawa coupling of the electron with Higgs is a small *dimensionless* number.

You've probably never heard of it, but in the standard model the masses are not fundamental parameters – they arise as the product of the Higgs vev and the Yukawa couplings.

It's just a dimensionless parameter that is small – one that can be perhaps extracted from string theory in the future (the intersecting brane models have the most natural explanation of the exponential smallness of the Yukawa couplings). But the only real *dimensionful* parameter in these masses is the electroweak scale, i.e. the Higgs vev.

The neutrino masses are a bit more subtle, and the see-saw mechanism relating their size to the GUT scale (where the electroweak scale is in the middle) is the most reasonable description of their smallness.

3. I am amazed how you're already scared – we would say in the Czech Republic that you've been shitting to your pants 😉 – by the two new popular books. You have not even seen them yet, but you already know that they're overhyped bad books. I think that all these morons who take your opinions about physics seriously must be incredibly stupid indeed because you are telling them quite explicitly that your opinions have nothing to do with reality – you judge books that you have not seen at all (not even the table of contents), and you are not even trying to hide it.

The people are promoting extra dimensions, supersymmetry, and holography because they are the dominant ideas in the current research and the people interested in fundamental physics should know about it. If there were other major important ideas for particle physics beyond the Standard Model, ones unrelated to string theory, people would write about them in their books, too.

32. **Matt Reece**
November 21, 2004

By the way, I also will try to address your comments more later, but don't have time at the moment. Just thought that a clear statement of the little hierarchy would help clarify that discussion.

33. **Lubos Motl**
November 21, 2004

Peter, note that almost every time the discussion becomes a little bit technical and concrete – and not just your permanent complaints "string theory is so evil, evil, evil" – your comments start to be a bit ridiculous.

What does it mean "I don't believe the hierarchy problem should be treated seriously?" You don't need to have grand unification to realize that there's something like the hierarchy problem. You can also work with the Planck scale.

It's the very logic of effective quantum field theory that it's valid up to some energy scale, but not higher. If we want to say that a field theory is valid even at higher energies, we must explain how its parameters are protected.

In the conventional quantum field theory approaches, the electroweak scale simply can't be the ultimate fundamental scale of Nature because this would mean that physics of QFT breaks down here and we have nothing to replace it with.

Even in the most low-energy gravity scenarios of string theory, we want to describe the accelerator physics with an effective field theory.

The fundamental scale where notions of geometry break down is the Planck scale. You can just calculate where the quantum corrections to gravity become of order 1 – and you will get the Planck scale, which is about 10^19 GeV. You don't need to assume anything special about supersymmetry or unification – the question "where gravity gets strongly coupled" is a totally meaningful basic question, and it has the usual answer "the Planck scale".

The extra large dimensions scenarios are those in which gravity gets strongly coupled earlier or much earlier – i.e. at lower energy scales. But at any rate, there is a huge numerical ratio between the Planck scale and the electroweak scale, and this ratio must agree with whatever scientific theory you use, and it is a nontrivial constraint especially for natural theories.

A solution of the hierarchy problem is the same thing as understanding some details behind the electroweak symmetry breaking – you don't seem to appreciate that it is the same thing. Right, in the Standard Model we don't understand at least something about the electroweak breaking, e.g. why the Higgs (if it's elementary) remains light, and this is why we study the hierarchy problem.

It is impossible for a physicist to present no solution of the hierarchy problem, and simultaneously say that it is not a problem. It's only possible for an anti-physics terrorist, not for a scientist. 😉

34. **Matt Reece**
November 21, 2004

You say:

"I'm not so sure either that there's a "little hierachy" problem either. We've got mass scales of all sorts going from neutrino masses to a wide range of lepton and quark masses, and also the strong interaction scale and the electroweak symmetry breaking scale. It's not clear to me that the way to think about any of this is that there is one underlying scale and then try to understand why some things occur at much lower mass."

I don't see how you address the little hierarchy here. Simply stated, these are known facts:

– the Higgs vev is 246 GeV, setting the electroweak symmetry breaking scale.
– precision measurements of electroweak observables or four-fermion couplings generally put the scale of new physics at > 5 – 15 TeV.

Taking an effective field theory viewpoint, one must write down all operators allowed by symmetries, and the coefficients of these determine the energy scales at which new physics must enter. *Even restricting to operators consistent with SM symmetries*, experiments suggest they are suppressed by scales of 5 – 10 TeV.

I don't see how one can avoid the conclusion that either there is finely tuned physics associated with EWSB, or there is some mechanism that stabilizes the Higgs mass enough to solve this "little hierarchy." (Granted, the fine tuning implied here is *much* less than that implied by assuming a GUT, but it's there, and the experimental evidence here is much stronger.)

35. **Peter**
November 21, 2004

Hi Matt,

Unfortunately I only have a few moments to respond to what you wrote right now, sorry, it deserves more. A few quick comments:

1. You're right, I don't believe the "hierarchy" problem should be taken as seriously as it is, not until we have better evidence for the existence of a GUT scale. The significance of the radiative instability of the Higgs mass is highly unclear, it is just one of many indications that we don't really understand electroweak symmetry breaking.

2. I'm not so sure either that there's a "little hierachy" problem either. We've got mass scales of all sorts going from neutrino masses to a wide range of lepton and quark masses, and also the strong interaction scale and the electroweak symmetry breaking scale. It's not clear to me that the way to think about any of this is that there is one underlying scale and then try to understand why some things occur at much lower mass.

3. My comment about extra dimensions was in the context of the news that Lisa Randall has written a popular book on the subject. I agree that some of the things you mention may be worth investigation by someone, but that doesn't mean anybody should be writing a book for a popular audience about how great these ideas are. The popular literature on string theory is virtually uniformly full of highly overhyped and misleadingly optimistic portrayals of string theory. I think these do a huge disservice to real progress in physics, and the Susskind and Randall books sound to me like they will be more of the same. Physicists have got to get a lot more honest with the public and with themselves about what the prospects are for most of the ideas they are working on. People who want to should go ahead and study complicated, ugly models involving supersymmetry, extra dimensions or whatever. They may very well end up learning something important that will give insight into the electroweak symmetry breaking problem. But they shouldn't promote this work to the public until they have something a lot more convincing and solid than what they've got now.

36. **Matt Reece**
November 21, 2004

I should clarify that I don't mean to say that the original Randall-Sundrum model is very much like technicolor, but some RS-type models are. But the original RS model still does involve coupling the SM to some CFT. The 4D way of thinking about generating these exponential hierarchies is through some large anomalous dimensions. Some of Matt Strassler's papers along these lines are interesting. I really think that Randall-Sundrum models and AdS/CFT are very powerful tools for studying field theory.

(The canonical reference for anyone who wants to start to read about some of this is probably the paper "Holography and Phenomenology" by Nima Arkani-Hamed, Massimo Porrati, and Lisa Randall, hep-th/0012148.)

37. **Matt Reece**
November 21, 2004

[continued]

Now, if you ask most of the people who work on extra dimensions in particle physics whether they believe there's good evidence for whatever model they're working on, they will say no, of course not. We don't believe that we can construct the *right* theory of physics up to 10 TeV or so at the moment, but when the LHC turns on we'll start getting all sorts of evidence pointing toward that theory. Either we can do nothing in the meantime, or we can try to start understanding the space of possible answers. And extra-dimensional theories are powerful tools for building new *effective* field theories valid up to several TeV. It's up to you whether you take the five-dimensional character seriously, but it gives us an alternative weakly coupled description where we can calculate things that are not calculable in the four-dimensional description. Extra dimensional thinking has also led to the idea of "deconstruction," which is another useful tool for studying four-dimensional field theories.

While I would expect some of your blog's readers to object to the idea of building effective field theories valid up to some energy scale, since many of them seem not to believe in renormalization, I would think you understand field theory well enough to know that this is a sensible approach. Instead you seem to want to critique both the top-down approach of string theory *and* the bottom-up approach of particle theory. That doesn't leave much.

I really think you misunderstand the attitude of many of us who study various sorts of theories. Even if our universe is not supersymmetric, or does not have large extra dimensions, that doesn't make studying these models worthless. In the past five years alone I think our appreciation of possible physics we could see at the LHC has grown vastly. People will be a lot more cautious about calling interesting events SUSY now, I think, because we realize that there are other realistic models, like little Higgs models or extra-dimensional models, that can give you the same sorts of results.

One can make this sort of argument not just for extra-dimensional theories but for SUSY as well. Even if there is no SUSY at low energies, you can't dismiss work like Seiberg's discovery of dualities between different SUSY QCD theories. This is important stuff about the qualitative behavior of field theories, which we don't understand well at all. The non-SUSY case is just really hard, though, while SUSY gives powerful constraints that make things easier to study.

I think at this point many of us working in particle theory take a pragmatic view. There are many possibilities for what can happen beyond the TeV scale, and sitting around thinking hard about what we already know just isn't enough to tell us what they are. Only experiment will settle that. But we want to get some idea of the possibilities, and this is what we're doing, with any tool we can find. Some of the models are ugly, some are implausible, but from an *effective field theory* point of view what we're really doing is looking at the different sorts of completions of the Standard Model up to the scale of several TeV that can alleviate the problems in the SM (like the hierarchy problem).

I have some sympathy for your viewpoint that the mathematical structure of the SM is interesting. I like geometry. If I weren't doing physics, I would probably be doing geometry or topology of some sort. But I don't think that thinking about these issues is going to be especially helpful for understanding particle physics at the TeV scale. (How do you see the hierarchy problem geometrically? It's inherently quantum, not classical. If you have a nice geometric version of it, great, I'd love to hear it, but I'm doubtful.) I think most particle theorists are taking a very pragmatic approach that will hopefully give us all the insights we need to start interpreting the LHC data when it turns on.

In short, I don't think your attacks on the state of particle theory are justified, and I wish you would at least get a better sense of what's going on and what the attitudes of particle theorists are before you start caricaturing us as being convinced that extra dimensions will be discovered at the LHC, or some such nonsense.

Matt

38. **Matt Reece**
November 21, 2004

Peter,

I find this post a little ridiculous. In the past you've said (something like) that you think that electroweak symmetry breaking is the central problem in particle physics today. Well, many of us who are working in particle theory agree with you. And I expect Lisa Randall is one of them.

You say about "the idea that we live on a brane inside a higher dimensional space" that "there's even less evidence for this idea than there is for string theory itself." I think this indicates that you're missing the point, and further that you're rather out of touch with the particle physics community.

Do you agree that there is a hierarchy problem? OK, so you're skeptical of GUTs. Fair enough. But there is certainly at least a little hierarchy problem. There's good *experimental evidence* for this. The Higgs mass is radiatively unstable, so either you must accept the distasteful idea that it is finely-tuned (some phenomenologists have been thinking along those lines) or you must start looking for explanations of the little hierarchy. One of the simplest is supersymmetry, but you don't like this idea. OK, again, fair enough, it involves predicting a plethora of new phenomena we haven't seen yet.

So, what are other ways of solving the hierarchy problem? One of the oldest ones (going back to Susskind and Weinberg in 1979, although some people had thought along these lines before) is that it is dynamically broken through technicolor. Unfortunately it's very hard to build a model of technicolor without lots of difficulty satisfying electroweak precision measurements, or flavor problems.

Lisa Randall and Raman Sundrum gave us a very nice alternative solution to the hierarchy problem, namely that it comes from warped space. But, because of Maldacena's AdS/CFT duality (which again, you've admitted is one of the most compelling reasons to study string theory), this is really just another type of technicolor! Again there are difficulties with electroweak precision measurements and flavor, but they take on a different character in the 5d picture, and some of us are thinking about both how to solve them and how they relate to older technicolor ideas.

[let me break this off here and continue in another]

39. **Peter**
November 21, 2004

Hi Lubos,

I think the Billy Cottrell story deserves more prominent display, so I'll post something in a minute about it. Thanks for pointing it out!

40. **Lubos Motl**
November 21, 2004

You know, Lisa's book has a lot about string theory, too – especially D-branes and their history, relations to p-branes, and so forth.

Sure, I will ask her what she exactly thinks about string theory's prediction of gravity.

Incidentally, Peter, string theory was proved to be physics in the courthouse – a trial about SUVs ;-), read http://www.pasadenastarnews.com/Stories/0,1413,206~22097~2541722,00.html

I think that it's OK to promote idas, even those not well-established ones, as long as you honestly say that they're not proved, and as long as you describe honestly how much your viewpoint is shared by other scientists.

Such books are written in the people interested in physics – which does not just mean the boring proved physics from the textbooks, but also the current cutting-edge and future physics!

41. **Peter**
November 21, 2004

Hi Lubos,

Thanks for the more detailed description of Randall's book. But I still think there is something wrong with writing books for a popular audience promoting speculative ideas for which there is no experimental evidence.

I don't think I misinterpreted what she had to say about the "string theory predicts gravity" argument, but she's your colleague, so perhaps you could ask her yourself and report back.

42. **plato**
November 21, 2004

Peter,

To remain dimensionally connected, it should be **Line Hook and Sinker**? Hopefully my blog will update by the time one looks, as a title should appear when being looked at, as such.

Sorry Lubos for following your posts, it just seems that when I am ready I find you had already done so.

The Comments about Lisa Randall seemed to say the same thing to me.

What exactly is the hierarchy problem? was a legitmate question?

43. **Lubos Motl**
November 21, 2004

Lisa Randall is a phenomenologist, the most cited high energy physicist in the last 5 years, and a string-theory-friendly phenomenologist.

I don't think that the sentence "Yes, string theory predicts ten-dimensional gravity" was her dismissal of string theory's predictions of the low-energy spectrum. You might have misunderstood her answer.

Her book is very advanced and it covers all the things that the laymen interested in particle physics should know – the hierarchy problem and proposals to solve it, with the emphasis on supersymmetry and extra dimensions in particular.

Of course, the book will be too difficult for your fans who always say "Yes Yes Mr. Woit" on your pages – like the moron signed "JC" before me. Don't you think that something is wrong with the things you write, Peter, if you attract this type of people?

I don't know if the book will be an easy reading for yourself, Peter, but I am sure that her book is a must read for all non-professionals genuinely interested in particle physics – not just string theory – and in fact, it's useful for professionals, too. It's been useful for me.

It's a book that goes beyond all popular books on particle physics published so far – and it has many witty metaphors in it, too.

I have not read Lenny's book yet.

44. **JC**
November 21, 2004

Is this "advocacy" (via popular press books) a sign of desperation amongst gravity/string theorists? Or is there something deeper going on here?

Autistic String Theorist Accused of Ecoterrorism and Being a Police Informant

November 21, 2004
Categories: Uncategorized

In the comment section of the last post, Lubos Motl points to the story of Billy Cottrell, a young string theorist at Caltech accused of being involved in the vandalism of SUVs. Evidently he has now testified against others at his trial, so the "Free Billy Support Network" (which was asking people to send string theory papers to him in prison) has been disbanded and he is being referred to as a "police informant".

Despite being a string theorist, Cottrell seems to not be the brightest bulb around, having supposedly used a Caltech computer he was logged into to send an anonymous e-mail to the media claiming responsibility for the SUV vandalism.

The local Pasadena newspaper's report on his testimony at his trial says that he corrected Judge Gary Klausner "when the judge asked if string theory, Cottrell's focus at Caltech, is "an area of physics.'

"It's the area of physics,' Cottrell said."

His lawyers "attributed his odd behavior in testifying to Asperger's syndrome", a mild form of autism.

Funny, Cottrell isn't the only one who goes on like this about string theory. Maybe there's a lot of autism going around.

Comments

1. **feh**
   November 24, 2004

   I hope he languishes in jail for the rest of his life as an example to the other anarchists who want to destroy this country. If they were serious about helping working people, they wouldn't put construction workers at medium-sized businesses out of work by bankrupting businesses with their nihilistic vandalism.

   Smash the ELF.

2. **Lubos Motl**
   November 23, 2004

   Asperger's syndrome was really a not-quite-working attempt of Billy's attorneys.

   I don't know whom you think you've insulted. Autism is an unusual condition, but the people with autism have done a great deal of work for the humankind.

   It's not quite clear whether they have done more for the humankind than the string theorists combined – but it is pretty likely.

   In this sense, it would be hard to be insulted by this comparison. On the other hand, I think that the people who have autism will not be insulted by a comparison with string theorists either.

   So if I summarize – you did not do anything wrong even though you wanted to. 😉

3. **plato**
   November 23, 2004

   Hate, when that happens:)

   http://eskesthai.blogspot.com/2004/11/mathematicians-and-physicists-do-is.html

4. **plato**
   November 23, 2004

   I think I have a better defintion of a rebel here

   I decided to change the subject from a**u**tistic to a**r**tistic. Is that okay?

   In Canada, BC challenged the right of government to help fund autistic rehabilitation for those who need this help, and the Provincial Government won.

   This sets precedence in Canada, although new challenges will be forth coming.

   We have a different view on heathcare then you Americans. It was established by a very good citizen, like Tommy Douglas.

   Today it is being eroded by the very powerful elite, with plans to move towards American systems. Ole Tommy would not of be very happy and it is the little people who will again gather to make sure all are equal.:)

   The Republic, is very interesting written work by Plato.

5. **Peter**
   November 22, 2004

   I don't think I was either joking about autism or making fun of those who suffer from it, and I apologize to anyone who feels that I was.

   On the other hand, I definitely was making fun of string theorists. Sorry you were offended.

6. **Bob McNees**
   November 22, 2004

   "Funny, Cottrell isn't the only one who goes on like this about string theory. Maybe there's a lot of autism going around."

That is funny, Peter. Especially the part where you joke about autism. Family and friends of autistic people everywhere salute your discriminating use of completely appropriate comparisons.

7. **Fabio**
   November 22, 2004

   Sorry Matt, my bad.

   Still, shouldn't you be studying for quals or something?

8. **Fabio**
   November 22, 2004

   Looking at Mr. Cottrell's extracurricular activities, as well as Matt's and Lubos's frequent long-winded postings on this forum, one can't help but conclude that string theorists have alot of free time on their hands, and that there are better ways of spending it than working on "the language in which god wrote the universe".

9. November 22, 2004

   Well, in general ethics and moral seems to be quite independent of talent.

10. **Thomas Larsson**
    November 22, 2004

    but Billy instead wanted us to go back to two dimensions and focus on Teichmueller theory.

    Teichmueller wasn't God's best child neither, sending his Jewish colleagues at Goettingen to Auschwitz, before he disappeared on the eastern front.

11. **Tim M.**
    November 21, 2004

    Indeed there is much autism: the second Google hit when searching "not even wrong" is a link to a book on Amazon about autism called Not Even Wrong.

12. **Mubos Lotl**
    November 21, 2004

    ">"He has a hostility toward anyone with different views,' Riordan said.
    >"It's a dripping hostility, it's powerful sarcasm, it's a passionate
    >self-belief.'"

    Obviously not a physicist.

13. **Lubos Motl**
    November 21, 2004

    Right, the opposite holds for me, too! 😉 I still find it more likely that I would share the opinions about string theory, maths, and physics with Billy rather than the opinions how to deal with the SUVs.

    Peter Woit is the ideal mixture because he has terrorist opinions both about SUVs as well as about string theory. 😉

14. **Lubos Motl**
    November 21, 2004

    Fascinating, Matt! BA about p-adic numbers in string theory is really cool. I hope that he will have access to the arxiv in the prison. 😉

15. **Peter**
    November 21, 2004

    Hi Matt,

    Thanks for the interesting story about Billy Cottrell. I still think he and I may share opinions about SUVs more than we would share an opinion about math and physics.

16. **Chris W.**
    November 21, 2004

    In a free market, SUV owners will decide for themselves what is easy to pay for. Ask the airlines (and their laid-off former employees) how easy it is to pay for fuel with crude at $45-60 a barrel.

    For now, consumption is declining seasonally, and oil prices are slacking off somewhat. The bottom line remains that increasing production and low prices cannot be taken for granted anymore, even if one assumes that the Mideast remains (relatively) stable. That assessment comes from the oil industry itself, which has been bailing out of some recent allegedly major oil finds that haven't panned out:

    > "One simple fact has never changed. Before oil can be produced it must first be found. Global oil discoveries peaked in 1964 and have been declining for 40 years. M. King Hubbert predicted the US oil production peak to occur 40 years after US oil discoveries peaked around 1930. He was right. Last year not a single field of 500 million barrels was discovered (for the first time since the 1920s) anywhere on the planet. The world uses a billion barrels of oil every eleven and one half days. We are now roughly 40 years after the peak of global discovery. This simple arithmetic has never changed. The outcome hasn't changed either."

17. **Lubos Motl**
    November 21, 2004

    It's easy for Billy to find time for such things, and it's also easy for the SUV owners to pay for the gasoline even if the price doubles. Don't be ridiculous, Chris.

18. **Chris W.**
November 21, 2004

How would a grad student in physics at Cal Tech find the time to run around the Los Angeles area spray painting and/or torching SUVs?

Anyway, such symbolic acts are becoming irrelevant. Given the way fuel prices are rising, SUV owners are going to start torching their own vehicles and buying gas-electric hydrids with the insurance payout (if they don't get caught).

19. **Lubos Motl**
November 21, 2004

Poor guy who had no other choice. He faced up to 40 years in prison, for the damage of 2.3 million dollars on the SUV.

If I were left-wing, I would have definitely tried to help him out of his troubles!

Could he get amnesty? He may deserve one for knowing that string theory is not just "an area of physics" but also "the area of physics"! 🙂

http://www.aiada.org/article.asp?id=4998&cat=Industry

http://portland.indymedia.org/en/2004/11/303898.shtml

Now he does not need it because he has become a police informant, and hopefully he will help to catch all these other ecoterrorist assholes. 😉

20. **Matt**
November 21, 2004

Billy and I were undergrads at Chicago at overlapping times. He wrote his BA thesis on "tachyon condensation in p-adic string theory," working with Freund, IIRC, so he was already quite into string theory at the time. (Apparently the mathematics actually simplifies in the p-adic case. I assume this was some calculation in string field theory, but I don't really know.)

I recall him as being highly talented in mathematics. The one class I took with him was a small (about six students, I think) proseminar on low-dimensional geometry and topology, taught by Benson Farb, focusing on Thurston's book. At one point there was a vote on what topic we should focus on for the last few weeks. Most of us wanted to study the eight Thurston three-dimensional geometries, consistent with the original plan for the course, but Billy instead wanted us to go back to two dimensions and focus on Teichmueller theory. At the time I knew little about string theory and this was somewhat mysterious to me, but in retrospect I know why he was so insistent. Still, he was outvoted.

Anyway, there isn't too much point to this anecdote, but Peter, you and Billy might have more in common with regard to thinking about the relationships between mathematics and physics than you would expect.

21. **Matt**
November 22, 2004

I'm a bit confused by that last comment — I'm not a string theorist (I'm a particle theorist, and just in my first year of grad school), and I've rarely posted long comments here.

Time Magazine Article

November 23, 2004
Categories: Uncategorized

For a depressing look at where theoretical physics is headed, see this new article from Time magazine. I agree with the analysis of it posted here.

Comments

1. **plato**
   November 29, 2004

   raj,

   Maybe from the outside perspective us commoners wonder whether the strategies to defining the physics has fallen short of the continued attempt to define it's geometry?

   Having attained certain perspectives, leading developement to the abstractness of the math used, has some way divorced itself, from what Peter demands, and yet all engage, in this strange language?

   Sometimes the fictional stories are better suited to the constrants applied these mathematicians, that everyone wants to know, what the heck they are seeing?

   Alice(**M**alice to some) presents all kinds of possibilties, and in the heart of Glast valuations, a good understanding of our universe?

   But truly, it goes much deeper then the Glast perspective? They have to agree that they all have a piece of that elephant and that spectrum continues to be much greater involved??

2. **raj**
   November 29, 2004

   >Physics is hard. Do it or shut up.

   Um, no. Or, well, maybe. Why should physics be hard? At base, physics relates to the interaction among things. Why should that be hard?

   The problem is that professional physicists apparently want to make physics *appear* to be hard. Hard to do. Hard to understand. So, when they publish an interesting result, they start in a way that basically causes most people to tune out.

   Professional physicists should seriously consider trying to figure out how to explain their discoveries to the "common"–or, let's say, educated–person.

3. **plato**
   November 25, 2004

   So which end of the elephant are working from DRL?:)

4. **D R Lunsford**
   November 24, 2004

   Well I can't get all depressed about the state of physics. This attitude is so self-indulgent – it's as stale as the denial-laden homilies of the stringer crowd.

   Physics is hard. Do it or shut up. This kind of whining is embarrassing.

   -drl

5. **plato**
   November 24, 2004

   http://www.simulation-argument.com/

   There are fundamental problems with this idea of computer simulation and Gerard t' Hooft expounds on this?

   Although the tendencies are well used in cosmological considerations. Example here are Andrey Kravtsov, and Max Tegmark's work. New post I constructed, but takes time to materialize, will come out later today.

   Fo one moment I speculate that qubit reformation would have to undergo non discrete photosynthesis examples, in information transfer? Glast indications, although sound from this perspective views, are troubling to me, if considered in computerization methodology.

   Quantum Entanglement?

   The basics of two-party entanglement

   http://xxx.arxiv.org/abs/quant-ph/9511030
   http://xxx.arxiv.org/abs/quant-ph/9511027
   http://xxx.arxiv.org/abs/quant-ph/9604024
   http://xxx.arxiv.org/abs/quant-ph/9707035
   http://xxx.arxiv.org/abs/quant-ph/9709029
   http://xxx.arxiv.org/abs/quant-ph/9801069
   http://xxx.arxiv.org/abs/quant-ph/9811053
   http://xxx.arxiv.org/abs/quant-ph/9905071

   Basics of multiparty entanglement
   —-
   http://xxx.arxiv.org/abs/quant-ph/9907047

http://xxx.arxiv.org/abs/quant-ph/9908073
http://xxx.arxiv.org/abs/quant-ph/9912039
http://xxx.arxiv.org/abs/quant-ph/0005115

Basics of secret sharing
—
http://xxx.arxiv.org/abs/quant-ph/9806063

I entertain other options and ways of seeing early cosmological events. Trying to think outside the box:)

6. **Fabien Besnard**
November 24, 2004

Thank you Urs. I tried it but I should have made a mistake somewhere.

So the URL are :

simulation argument

and

my answer

7. **Urs**
November 24, 2004

Fabien Besnard wrote:

> sorry but I don't know how to make the link appear as they should.

Use html tags as usual, i.e. write
[Text goes here]

Similarly, to make quoted text appear as quoted text enclose it in <blockquote> and </blockquote>.

For more details see the How-To page of the String Coffee Table.

8. **Fabien Besnard**
November 24, 2004

Distinguished philosophers think we are maybe in the Matrix :

http://www.simulation-argument.com/

Distinguished astrophysicists don't think so, but their refutation is even more funny. (See Brandon Carter's paper on this same page)

Others think the argument is self-contradictory :

http://perso.wanadoo.fr/fabien.besnard/refutation.html

PS : sorry but I don't know how to make the link appear as they should.

9. **Chris Oakley**
November 23, 2004

Anonymous –

That's the funniest comment I've seen yet on this blog. Or maybe it's not so funny. Either way, you should identify yourself, even with a nom de plume.

10.
November 23, 2004

Physicists need some sort of validation. Since it no longer comes from experimentation, seeing their names in the paper alongside prominent cranks will have to do.

New U.S. Science Budget

November 23, 2004
Categories: Uncategorized

The U.S. Congress has finally gotten around to producing a budget for fiscal year 2005. Some information about the budget numbers for scientific research is available here and here.

The NSF budget for research and related activities is being cut by .7% from its FY 2004 level, the first such cut in many years. The other main part of the NSF budget, that devoted to education, is being cut even more. A few years ago Congress passed a bill that was supposed to double the NSF budget over several years, but that bill is now very much no longer operative. It's not clear yet how physics and math specifically fare under this new budget, presumably we'll find out in the next few days.

The bulk of particle physics funding comes from the DOE Office of Science, and there the budget situation is brighter, with an increase of 2.8% for FY 2005. Again, the details of exactly what is being funded and what isn't should soon be available.

Update: More about the new NSF and DOE budgets can be found here and here.

Comments

1. **Chris W.**
   December 4, 2004

   [..from Sean Carroll. The following are excerpts from articles on the NSF budget, with *emphasis* added.]

   June 5, 2002 — By an overwhelming vote of 397 to 25, the U.S. House of Representatives passed a bill that authorizes a $2.5 billion increase in the National Science Foundation (NSF) budget over the next three years. Under the legislation, the NSF budget would increase by 15 percent per year, from $4.8 billion in FY 2002 to *$7.3 billion in FY 2005*. The bill would put NSF on track to double its budget in five years. The NSF authorization bill, which is entitled the ?Investing in America?s Future Act of 2002? (H.R. 4664), must still be considered by the Senate.
   .. . .

   December 3, 2004 — **The Cost of Congressional Caprice** (The New York Times)
   .. . .
   If only the bill were a mere laugh riot. But the truth is that the measure, cobbled together from 13 bills that Congress failed to weigh separately and thoughtfully, legislates the costs of next year's government by blindfold and bludgeon.

   Nowhere is this more graphic than in the shocking cut that Congress levied on the National Science Foundation, the research dynamo that does so much to feed the nation's economic growth through breakthrough advances in science and technology. Its budget will be $105 million less than last year's, even as lawmakers spared an estimated $15.8 billion for a record 11,772 pet projects. This binge of bipartisan pandering to voters includes such national priorities as renovating the Hot Springs bathhouses in Arkansas and bolstering the Paper Industry International Hall of Fame in Wisconsin.

   The science cut may seem minor in the context of the foundation's total, more than *$5.4 billion*. But it signals harsher times to come. For the past two years, a profligate Republican Congress has allowed the deficit to balloon by papering over such factors as the open-ended cost of the Iraq war and the revenues lost because of the Bush tax cuts. Now leaner, meaner government has become the rhetorical rage, with basic institutions like the Environmental Protection Agency and the affordable-housing program joining the science foundation in taking hits.
   .. . .

   — — — — — — — — — —
   The FY 2005 budget was already going to fall well short of the $7.3 billion that was authorized by the House (but evidently not passed) in 2002, without the cut relative to FY 2004. What's up? From an interview one year ago:

   > **[Terrence] McNally**: What I haven't heard quite yet is the point which you make very strongly in the book, that the purpose behind the tax cuts is to bankrupt the government, to undermine social programs, so that no one who comes into office after them will have an easy time restoring them.
   >
   > **[Paul] Krugman**: I'm not making that up. That's exactly what the lobbyists and the others behind these people say. The program that the Administration is following looks as if it was designed to implement their ideas. I think it is.

2. **Lubo? Motl**
   November 24, 2004

   Dear Hacik,

   as you can see by looking at the words more carefully, "conservativism" is derived from the word "conservation".

   So energy conservation is a conservative value, much like PCs. 😉

   Thanks for your understanding.
   Lubos

3. **Hacik**
   November 23, 2004

   Energy conservation is so PC and left wing Lubos, I'm surprised to hear you buying into it.

4. **Lubos Motl**
   November 23, 2004

   You cannot create energy because of a principle that is called the energy conservation law. 😉 You can only convert energy from a less useful form to a more useful form – and the energy that our experimental friends will use to initiate collisions that will lead to production of Kaluza-Klein modes – with energy escaping to extra dimensions – is certainly more useful energy than any energy that you've used in your life. 😉

If this scenario is correct, a couple of megajoules will be enough to create a Nobel prize. 😉

5. **Hacik**
   November 23, 2004

   The DOE should get out of the particle physics business altogether. The purpose of the DOE is to create energy, not make it disappear into hidden dimensions.

6. **Lubos Motl**
   November 23, 2004

   It sounds kind of logical that the DOE funding is more stable in this situation because this department has a more "strategic" character. I would probably agree that the big budget deficit is not an ideal moment to increase funding of pure abstract science too much, and it would be nice if the deficits were reduced once again.

   America spends too much – and the trade gap is very big, too. You, Americans, should reduce your eating of everything else from the world. 😉

Review Article About the Poincare Conjecture

November 30, 2004
Categories: Uncategorized

An excellent review article about the state of the proof of the Poincare conjecture by my colleague John Morgan has recently appeared. For more background on this, see an earlier posting. Morgan is a topologist, and his article contains an excellent survey of what this all has to say about the topology of three-manifolds. This past semester he has been teaching a course in which he has gone through Perelman's proof very carefully. So far it all holds together.

Comments

1. **Deane**
   December 1, 2004

   Peter,

   I sure wish I had the time to drop by and ask Hamilton and Morgan more questions about this.

   You indicate that the crucial gaps are analytic.
   This surprises me. It's not the geometric or
   topological arguments that are unclear?

   Any chance you could be more specific about what
   kinds of techniques in analysis that Perelman is using and that people are uncomfortable with?

2. **Peter**
   December 1, 2004

   After further consultation with experts I think it would be fair to say that the situation is something like this:

   Parts of Perelman's outline have been filled in and people agree on them. But the status of other parts is still unclear and no one is about to claim yet that a complete proof exists. One problem is that Perelman is using some difficult techniques, for which it is not clear that there are adequate references in the literature. So filling in the details of his proof is a very non-trivial business. To start with you have to become an expert in the techniques he is using, and then you may need to do some hard work checking that these techniques really do work exactly the way they are supposed to.

   So the problem is that the work that needs to be done is quite difficult and requires some real expertise in some little-known techniques in analysis. And some of the experts may not be too motivated to do this, since if they invest the time needed there is a serious danger that either Perelman will scoop them by producing his own full proof (no one knows if he plans to do this and he is not saying), or their work will not be very valued by anyone, since people will think that all they did was work out details already known to Perelman.

3. **Peter**
   December 1, 2004

   Problem is that at this point if X writes up a detailed proof no one is likely to refer to it as the "Perelman-X" proof, especially since so many people have now been going through the process of working out the details for themselves.

   If X were to do this, I think it would be viewed more as a piece of exposition, rather than original research, and the reward structure of the field is such that expository work is not very highly valued.

4.
   December 1, 2004

   Peter wrote:
   ….I don't think the math community will be completely satisfied that there's a proof until someone (or a group) finally writes out a real, detailed proof, and I don't know if anyone intends to do this anytime soon….

   Hmmm, that seems funny to me. Since Perelman only produced an outline, I would think some one would want to do the legwork and share the glory. I would have guessed mathematicians would be interested in filling the "whoever" slot in the Perelman-whoever proof of the Poincare conjecture.

   After all, that's a hell of a paper for a struggling post doc looking for a job.

   ksh95

5. **Peter**
   November 30, 2004

   Hi Deane,

   I don't know of anyone now willing to stand up and make that claim. The problem is that Perelman has really only produced an outline of a proof, so a lot of work is necessary to fill in the details. Some people like Morgan are working on it and they haven't found any problems. I haven't talked to Hamilton about this, but I'd suspect he's more interested in trying to do something new than in filling in the details of Perelman's outline.

   I don't think the math community will be completely satisfied that there's a proof until someone (or a group) finally writes out a real, detailed proof, and I don't know if anyone intends to do this anytime soon.

   Peter

6. **plato**
   November 30, 2004

   I do not mean to interrupt thought processes here in regardng Poincare conjecture, but a little history was importnat for the commoners(me) to be exposed to a way of thinking that few can concieve of. I assumed all mathematicians and physics people are blessed with such insight?

> *The discovery of non-Euclidean geometry shocked them into understanding their error in expecting to determine the "perfect state" by reasoning alone. On the other hand, non-Euclidean geometry showed us that the human mind can defy intuition, common sense and experience in order to experience what worlds reasoning could create. The historian of mathematics, Morris Kline, summed up the meaning of these new geometries. The importance of non-Euclidean geometry in the general history of thought cannot be exaggerated. Like Copernicus' heliocentric theory, Newton's law of gravitation, and Darwin's theory of evolution, non-Euclidean geometry has radically affected science, philosophy, and religion.* ***It is fair to say that no more cataclysmic event has ever taken place in the history of all thought.***

But that's not the point I wanted to raise. It had to do what might be concieved of this wonderful world abstractually creating a road to GR which continues to expand. I know I am probably wearing peoples patience thin who are well educated and very articulate in the mathematical realms, and realms of physics, but has no one consider the context of the way in whch we can now see, in relation too, Einstein's realization of that extra dimension? Could have led to a more comprehensive view of where Einstein left off?

So one saids, "extra dimensions," one would have to engage the new world of non-euclidean with some good understanding to meet these new mathematics I am seeing developed here. By others along side of that physics.

I hope this post is acceptable here, as a form of a question, about why Peter you will not accept those extra dimensions?

7. **Deane**
November 30, 2004

Peter,

This article gives the status of Perelman's
proof as of last August. Is anyone
(Hamilton, in particular) ready yet to proclaim
Perelman's proof complete and correct?

Paul Baum on Baum-Connes

December 1, 2004
Categories: Uncategorized

I was down in Princeton at the Institute yesterday and heard two interesting talks. The first was the beginning of a series of four lectures by Paul Baum about the Baum-Connes conjecture, the second was by Chris Woodward about "equivariant localization". I'll write a bit about Baum-Connes here, perhaps something about the topic of Woodward's talk in a second posting.

The Baum-Connes conjecture was first formulated in 1982 by Paul Baum and Alain Connes in an unpublished paper.Roughly it says that the K-theory of the reduced C* algebra of a group G is identical with the equivariant K-homology of a certain sort of classifying space for the group. Equivariant K-homology classes can be represented by certain generalizations of the Dirac operator, and the map to the K-theory of the C* algebra is given by taking the index of the operator.

There's a huge literature about this by now and a few years ago Nigel Higson put together a detailed bibliography. Some recent expository articles about the conjecture include an ICM talk by Higson, a survey talk by Wolfgang Lueck, and a book by Alain Valette.

The conjecture remains unproved for discrete groups in general, and Baum said that he suspects it is not true in full generality, invoking what he called "Gromov's principle". According to Baum, this principle states that "No statement about all finitely presented groups is both non-trivial and true." While the conjecture has been proved for some classes of discrete groups, there are many for which it is expected to be true but remains unproved (e.g. SL(3,Z)). For a while it was thought that the conjecture applied also to groupoids, but counterexamples for groupoids have been found.

I've always been fascinated by part of the philosophy behind the Baum-Connes conjecture, which is to use equivariant K-homology, classifying spaces and Dirac operators to get information about representation theory of groups in cases where little is known about this representation theory. This is in some very vague sense related to what seems to me to be going on in QFT, where Dirac operators and path integrals over the classifying space of the gauge group (the space of connections) are somehow related to the representation theory of the gauge group. The Baum-Connes conjecture itself just involves locally compact groups and thus doesn't say anything about gauge groups, so it is not directly relevant to the case that may be of interest in QFT.

Comments

1. **Paul Baum**
   December 8, 2004

   Gromov's principle can be used as a jumping off point for some very interesting discussions. Let G be a discrete group. BG is defined as any CW-complex which is connected, and has G for its fundamental group, and has all higher homotopy groups zero. We can then take the homology or cohomology H*(BG). On the other hand in the realm of pure algebra we can write down a complex whose homology is H*(BG). It is a theorem that these two are the same — and this is valid for any discrete group. Of course many mathematicians would declare this to be trivial. But it took the best mathematicians of the nineteen-thirties (e.g. Heinz Hopf) much thought and much work to figure this out. So a lot of the content of Gromov's principle hinges on exactly what we mean by "non-trivial". Another issue with GP is what "about" means. In the last of the four lectures at IAS I'll give two statements which are relevant to Baum=Connes for
   discrete groups and which are true for all discrete groups. In the sense that Gromov had in mind these are undoubtedly trivial statements.But it took my co-workers and me quite a few years to figure this out and to come up with the proofs. So in the colloquial sense that mathematicians use the term "non-trivial" these statements are (it seems to me) non-trivial. So exactly what does "non-trivial" mean in Gromov's principle. Perhaps it means a statement which tells us something really deep and really meaningful about all discrete groups. Gromov is probably right about such statements.

   Peter Woit's comments about QFT suggest that someday we might have something like Baum-Connes for gauge groups. I hope this happens. It might be really fascinating.

Witten Localization

December 1, 2004
Categories: Uncategorized

The second talk I heard yesterday at the Institute was by Chris Woodward from Rutgers. What he was talking about was a conjectural formula whose origins go back to a truly amazing paper by Witten from 1992 entitled Two Dimensional Gauge Theories Revisited.

There are quite a few very interesting things about this paper, but one of its ideas has become influential in mathematics under the name “Witten localization”. This involves a new principle for calculating integrals of equivariant cohomology classes. Before Witten’s work, it was well-known among mathematicians that such calculations could in many cases be reduced to a “localized” calculation about the fixed point set of the group action. This is related to the Atiyah-Bott version of the Lefschetz fixed point theorem they discovered in the mid-sixties, to general arguments about equivariant K-theory and fixed points due to Atiyah and Segal, as well as to the Duistermaat-Heckman theorem and generalizations due to Berline and Vergne. For some expositions of this material, see the paper by Atiyah and Bott published in Topology in 1984, and the book Heat Kernels and Dirac Operators by Nicole Berline, Ezra Getzler and Michele Vergne.

Witten’s idea involved a new localization principle, where integrals of equivariant cohomology classes can be localized about zeroes of the moment map rather than fixed points of the group action. This is sometimes referred to as “non-abelian localization” since it applies directly to non-abelian group actions, whereas the earlier fixed point formulas typically looked at the fixed points of actions by abelian groups.

One of the main applications of Witten localization by mathematicians has been to use it to prove in various contexts that “quantization commutes with reduction”. For physicists this is the idea that, given a classical mechanical system with a gauge symmetry, one hopes to get the same result either by first imposing constraints and then quantizing, or by quantizing and then imposing constraints. Even in the context of finite-dimensional classical mechanical systems, that this should be true is a very non-trivial mathematical statement. For a survey of some of this, see an article in the Bulletin of the AMS by Reyer Sjamaar.

Witten’s original paper applied his ideas to the calculation of the Yang-Mills partition function in two dimensions. This uses the fact that the space of connections for a non-abelian gauge theory in two dimensions is an infinite dimensional symplectic manifold, with moment map the curvature of the connection, something first observed by Atiyah and Bott in the late seventies.

Woodward’s talk involved an integration formula similar to Witten’s original one, for details about it see his recent paper. This kind of formula was also studied by Paul-Emile Paradan, see this paper and a recent detailed summary (in French) by Paradan of his work.

Woodward’s work is also motivated by trying to understand the 2-d Yang-Mills partition function as an integral in equivariant cohomology. He and Constantin Teleman have done work on a K-theoretic version of this, see their joint paper as well as Teleman’s contribution to the proceedings of the conference in honor of Graeme Segal’s 60th birthday, and his talk at the KITP in Santa Barbara last year.

Comments

1. December 7, 2004

   Peter, thanks for this informative post.

   z

2. **D R Lunsford**
   December 3, 2004

   Math is hard..
   ..and then 2 pi.

3. **Barbie**
   December 2, 2004

   Math is hard.

String Theory Gets Real – Not

December 3, 2004
Categories: Uncategorized

A recent issue of Science magazine has an article about the "Strings and the Real World" workshop at Aspen this past summer, entitled String Theory Gets Real — Sort Of. A more accurate title for the article might be "String Theory Would Like to Get Real — But Can't Because it Doesn't Work".

The article claims that up until recently string theorists were not even trying to connect string theory with experiment, but "Now a small but growing number of them are trying to forge connections between string theory and detailed data". This is really nonsense. There have always been plenty of people doing "string phenomenology", but it has always been a doomed subject, for reasons I've gone on about at length here and elsewhere. The article does mention the problem of the Landscape with the increasingly standard loony comment that "physicists may have to rethink what it means for a theory to explain experimental data". This is absurd. There's no question about what it means for a theory to explain experimental data and the simple fact of the matter is that this theory can't do it.

There's also a claim that "the cosmological constant now appears to be real, and string theorists hope to calculate its value". This misunderstands the whole Landscape argument, which tries to justify why no one can ever hope to calculate this value.

The article also includes a sidebar which tries to explain why young people go into string theory. It quotes a Penn postdoc, Brent Nelson, as saying that he read about string theory as a teenager and couldn't believe so many people accepted something so outlandish. But he went into string theory anyway, and now says "I haven't learned enough… I still don't know why I should believe". Sorry Brent, but no matter how long and hard you stare at this particular emperor trying to appreciate the beauty of his clothing, he's still going to be naked as a jaybird.

Finally, when asked how many revolutions will be needed to make string theory work, John Schwarz says "I don't know, but I think we'll need many more". At about a decade per revolution, it looks like Schwarz now doesn't expect to live to see this happen. Neither do I.

Comments

1. **D R Lunsford**
   December 7, 2004

   NYTimes article

   http://www.nytimes.com/2004/12/07/science/07stri.html

   Iterates the usual fabrication that ST contains GR. 5 pages of MOTS.

   -drl

2. **Lubos Motl**
   December 6, 2004

   "God that failed" – like the communist ideology – is a pretty good formulation that keeps the reader interested. It's obvious that a good journalist would always ask this question in this way.

   However, if you open The Elegant Universe by Brian Greene, for example, you will see a lot of comparably elegant possible criticisms against string theory (Chapter 9). 😉 String theory is undermining science like medieval theology; string theorists should not be allowed to convert sensitive students into perverse deviants etc. 🙂

   Of course that these questions and strong words are said exactly because the answer that they suggest is probably not true, which is the truly exciting part of the story.

3. **Chris Oakley**
   December 6, 2004

   Is string theory the physics equivalent of The God That Failed, as some people used to say about communist ideology?

   It is interesting that Scientific American should ask a loaded question like this. Maybe the tide is finally turning, and not in favour of those who think we need to change the dictionary definition of science.

4. **plato**
   December 4, 2004

   A perfect opportunity Peter, to tell them what you think?

   It is truly an exciting time in cosmology where experiments have produced and are continuing to yield an abundance of precision data. Similarly, collider experiments at Fermilab, SLAC and CERN, as well as precision low?energy experiments, have the potential to make remarkable new discoveries in particle physics in the next five years. Hence **string phenomenology** is both a timely and exciting topic of a workshop in 2005.

5. **Lubos Motl**
   December 4, 2004

   Concerning the interview with L. Krauss.

   I think that it is still an interview with a string theory layman. It's great to say that string theory has not revealed the nature of dark matter, but on the other hand it is not the most straightforward question that string theorists should ask.

   Eventually, unless the MOND-like theories will turn out to be right, the dark matter will just be *something*, a new particle (like bino, a superpartner), and there won't be too much to learn about other physics from this single piece of information.

   Krauss describes LQG and string theory almost on equal footing, which is of course ridiculous, but at least, he mentions the obvious fact that string theory has led to interesting mathematics, unlike loop quantum gravity.

Is there someone here who believes that LQG has led to interesting mathematics?

6. **plato**
   December 4, 2004

   Maybe they didn't understand the essence of the blackholes and the mathematical framework that was to go along side of the physics?:)

7. **Chris W.**
   December 3, 2004

   You know, Peter, I sense an increasing ennui spilling out into the popular science press, and a tinge of cynicism in the comments of journalists in spite of themselves. Another example occurs in this interview with Lawrence Krauss:

   > **SA**: Is string theory the physics equivalent of The God That Failed, as some people used to say about communist ideology?
   >
   > **LK**: Not exactly. But I do think its time may be past. String theory and the other modish physical theory, loop quantum gravity, both stem from one basic idea: that there's a mathematical problem with general relativity.
   > The idea is that when you try to examine physical phenomena on ever smaller scales, gravity acts worse and worse. Eventually, you get infinities. And almost all research to find a quantum theory of gravity is trying to understand these infinities. What string theory and what loop quantum gravity do is go around this by not going smaller than a certain distance scale, because if you do, things will behave differently. Both these theories are based on the idea that you can't go down to zero in a point particle, and that's one way to get rid of mathematical infinities. The main difference, I think, between the two theories is that string is intellectually and mathematically far richer.
   > String theory hasn't accomplished a lot in terms of solving physical problems, but it's produced a lot of interesting mathematical discoveries. That's why it fascinates. Loop quantum gravity hasn't even done that, at least in my mind.

   With respect to his comments on LQG, I'll have to bite my tongue for the moment.

8. **Fabio Lanzoni**
   December 3, 2004

   Give the poor science writer a break- he was probably on deadline.

Shiing-Shen Chern 1911-2004

December 5, 2004
Categories: Obituaries

Shiing-Shen Chern, one of the great geometers of the twentieth century, died last Friday at Nankai University. He was 93 years old. An article about his life is posted on the website of MSRI, the mathematics institute in Berkeley of which he was the founding director.

A lot of what I know about geometry was learned from his beautiful short book entitled "Complex Manifolds Without Potential Theory", published by Springer in 1979. Some of his most important work concerned the topology and geometry of fiber bundles, and its significance can be seen in the number of crucial ideas of this field that carry his name, for instance: Chern classes, the Chern character, Chern-Weil theory, the Chern-Simons secondary characteristic class.

Update: The New York Times has an obituary.

Comments

1. **D R Lunsford**
   December 15, 2004

   OK, much better,

   Chern classes (and characters) are a way of systematically classifying the possible types of field configurations: trivial (0), monopole (1), etc..

   *In other words, the monopole is in the *pure gauge theory without matter* (like instantons in Y-M).*

   Thanks, much clearer.

   -drl

2.
   December 15, 2004

   Maybe the confusion here is over the term "monopole". For lay physicists, it implies some sort of source.

   However, in this case, it is the *gauge field* A_{\mu}(x) that can have "monopole-like" properties, i.e., the field strength corresponding to the gauge configuration falls off like a monopole; it is as if there was a monopole at the origin causing this field.

   Chern classes (and characters) are a way of systematically classifying the possible types of field configurations: trivial (0), monopole (1), etc…

   An example is from the Fubini-Study metric (U(1) over S^2):
   F=(2i rdr d\theta)/(1+r^2)^2. Integral of (iF/2\pi) is -1; there is no "charge"!

   In other words, the monopole is in the *pure gauge theory without matter* (like instantons in Y-M).

3. **D R Lunsford**
   December 14, 2004

   Peter,

   Below, you used the example of the Dirac monopole to illustrate a Chern class. What I was saying is – apparently you can use this classification to distinguish between the end of a long string of end-to-end magnetic dipoles, and the (on the face of it) equivalent distribution of acutal poles. This isn't obvious and depends on the duality invariance of the energy tensor.

   What you just described was simply Gauss' theorem, other than the integer part.

   (I just used Maxwell as an example because it has a potential theory.)

   -drl

4. **Peter**
   December 14, 2004

   Chern classes are purely topological. All they do is look at the field strength on a sphere, and use it to tell you the number of monopoles inside the sphere (and that this is an integer).

   So this has nothing to do with the Maxwell equations, Chern's formalism doesn't care at all whether the field strength satisfies any equations at all. Neither does it have anything to do with the sources that occur in the Maxwell equations.

5. **D R Lunsford**
   December 14, 2004

   Peter,

   I don't think I was clear. Let's just say we replaced every pole by a charge and so, in the magnetodymanic world we'd have

   E = -D x A
   B = -Da – dt A

   and of course Maxwell is

   div E = 0

div B = m

curl B – dt E = 0
-curl E – dt B = M

with D^2 A = M etc.

In this world electric charges are associated with a singular magnetic vector potential. Because the energy tensor is duality invariant, nothing is any different as long as you stick to countably many discrete charges.

The point is, Maxwell-Lorentz really needs a density as a source, not a countable collection of poles each carrying a singular potential.

So the Chern must say something about the *sources*, in terms of how they can be locally "smeared out".

-drl

6. **sd**
December 14, 2004

Chern classes and Chern-Simons theory are discussed in Baez and Muniain, Gauge Fields, Knots and Gravity.

7. **DMS**
December 14, 2004

Peter,

Many thanks for the pointers on Index Theorem; Freed's notes look very nice!

DRL-

Here is a rough way to understand the Chern class.

Consider D=det(I+(i/2pi)F), where F is the curvature 2-form transforming as UFU^{-1} under gauge transformation U. Note that D is invariant under gauge transformations.

But D can also be expanded as a polynomial in F, in terms of homogeneous polynomial, P_j(F), of elements of F of order j. The j-th Chern class is the cohomology class determined by P_j(F), which is clearly independent of gauge (connection) choice .

8. **Peter**
December 14, 2004

Actually the syllabus that's up for that course is for the entire year. In the first semester we just did general manifold and bundle theory, will start next semester with Riemannian geometry, hope to get to the index theorem at the end.

There are quite a few good books about the index theorem from various perspectives. For the point of view I'll be taking in my course (if I get to this…), a good reference is Dan Freed's notes that are on the web, see his webpage for a whole course he taught on the index theorem:

http://www.ma.utexas.edu/users/dafr/Index/index.html

For the more abstract K-theory point of view, it's hard to beat some of the expository things Atiyah wrote about this. There are several such articles in Volume 3 of his collected works, one (Classical groups and classical differential operators on manifolds) in Volume 4 (this last one is highly recommended)

9. **DMS**
December 14, 2004

I just looked at the table of contents and excerpts of Morita's book on amazon and it looks like it is clearly written and even I should be able to grasp its contents!

But, unfortunately, no discussion of the Atiyah-Singer Index theorem, which you covered in the first part of your course.Do you know a good reference for that (that does more than just quote the theorem "analytical index=topological index")?

Perhaps there is a book that gives a glimpse of whatever has been happening since then in modern geometry (heard Gromov has done a fair bit, of which I know little).

10. **Peter**
December 14, 2004

Since my last class for the semester was yesterday, right now I'm just enjoying not having to think about geometry for a while, and being able to get to work on other projects. I don't intend to write up notes for the geometry course this year (it will continue next semester) for several reasons:

1. I'm way too busy.

2. This is the first time I've taught this, and whenever you teach something for the first time, you only realize after you've started explaining some topics that there is a better way of approaching things.

3. While there's no book out there I'm completely happy with, there are quite a few good ones of various kinds, and pretty much everything I've been talking about is covered reasonably in one or another. One book I've grown to like as I have looked at it over the semester is "Geometry of Differential Forms" by Shigeumi Morita. In the case of the representation theory course, the standard books didn't cover much of what I wanted to explain (especially connections to physics).

If I teach the course again (probably not for a few years), then maybe I will try and write up some formal notes for it.

Danny,

For better or worse, mathematicians have a whole apparatus for dealing with vector potentials and field strengths that avoids having to introduce singular vector potentials when the field strength is not singular. From this point of view Dirac singularities come about because you are trying to write down a trivialization of a topologically non-trivial bundle. When you do have a topologically non-trivial bundle, like in the case of a monopole, basically you can't choose the same gauge globally, unless you are willing to introduce singularities. By thinking of a choice of gauge as something you can only do locally, you avoid the whole problem of these singularities.

Peter

11. **DMS**
December 14, 2004

Actually, it would not be a bad idea if Peter (who may have a lot of time on his hands :)) could write up some notes on the Modern Geometry course he is teaching for the current (and next?) semester . Just like he did for the Lie group and representation course.

12. **D R Lunsford**
December 14, 2004

If I understand your previous reply, Chern classes are a formalization of what Dirac did when he concocted his "singular potential" by imagining the A field associated with an infinitely thin, infinitely long solenoid stretching off to infinity. Now why couldn't one just write

E = -curl V
B = -grad v – dV/dt

and pretend charges are "Dirac electropoles", thus replacing the nice potential A with a singular one V? Is it because you could not smoothly pack these electropoles into a volume and come up with an average density (you'd have "singular potential hair" – the electric solenoid strings) uniformly stretching out to infinity)? So is Chern theory must basically be a theory about the nature of sources…

-drl

13. **Peter**
December 13, 2004

I've put off writing about the geometric Langlands conjecture for a little while, partly because I wanted to take some time to see if I could understand it a bit better. But also I couldn't help myself from writing about the latest on the Landscape.

Chern's title is a somewhat strange one. I think he was trying to indicate that he concentrates not on the theory of holomorphic functions, but on the complex geometry in the sense that a complex manifold is one where you can consistently locally choose complex coordinates. Of course once you do this, you can't really avoid thinking about holomorphic functions. But a lot of what Chern studies involves complex vector bundles over complex manifolds, but not necessarily imposing the condition that these are holomorphic vector bundles (i.e. the transition functions you use to glue together the bundles may not be holomorphic).

14. **D R Lunsford**
December 13, 2004

BTW I meant to ask the meaning of the title "Complex analysis without potential theory". Doesn't analyticity imply potential theory?

-drl

15. **Peter**
December 13, 2004

Actually I was just about to write something about the Langlands program and physics. Either later today or tomorrow.

I hadn't heard about the hole in Dean's proof, will add an update to that weblog entry

16. **DMS**
December 13, 2004

Hi Peter,

I second Levi and drl; I enjoy your mathematical posts, especially when I can understand a significant portion 🙂 I think it is especially useful for physicists who would otherwise think that there is not much in mathematics outside current fashions in string theory-related mathematics (typically those results derivale using analytic methods).

Someday, maybe take up what was it about Grothendieck's work (schemes, Grothendieck group, etc.) that makes it so powerful even now (like Voevodsky's Field medal work). Or Langland's conjectures and their impact on mathematics, so we physicists can appreciate more the remarkable work being done in other areas of mathematics. In such instances, your dual background in math and physics is very helpful.

PS: Sorry to hear about a loophole in Carolyn's proof on the Jacobian conjecture; hope she fixes it.

17. **D R Lunsford**
December 12, 2004

I love the mathematical asides – otherwise I'd probably never know what was happening out there.

-drl

18. **Peter**
December 12, 2004

Thanks for the encouraging comments about the more mathematical postings. I certainly intend to continue to write more things of that kind.

19. **RT**
December 12, 2004

I wholeheartedly endorse Levi's comment; the postings that generate the least polemic and diatribe are almost invariably the most interesting

20. **Levi**
December 12, 2004

Peter,

In the comments to the post “String Theory at 20 Explains It All-Not” you note that there aren’t many comments when you post on the subject of mathematics and the mathematics of quantum field theory.

I hope you realize that a lack of comments doesn’t necessarily indicate a lack of interest. It often just indicates a lack of controversy. I wanted to mention this since your (too infrequent) posts on mathematics are my favorite posts on this blog.

21. **Peter**
December 8, 2004

Hi Danny,

Too bad you’re not near here, I just spent the last week or two lecturing about Chern classes in my graduate geometry class.

A good reference for physicists is

Eguchi, Gilkey and Hanson Physics Reports Vol. 66, number 6, 1980

There are also various books about geometry and topology for physicists that should discuss this.

To understand what is going on, first think about U(1) gauge theory. The field strength is a closed 2-form, so represents a cohomology class of the manifold. Physically, if you consider a sphere S^2 in R^3, integrating the field strength 2-form over the sphere counts the number of monopoles inside the sphere. So this cohomology class (the first Chern class) detects the non-trivial topology of a U(1) bundle on a sphere surrounding a monopole.

For U(1) you just get the first Chern class, for U(n) you get n different Chern classes, basically by taking powers of the field strength (=curvature) and then taking a trace. The algebra gets a little intricate, but it’s basically the algebra of symmetric polynomials.

Peter

22. **D R Lunsford**
December 8, 2004

Does anyone have a good reference for the appearence of Chern classes in physics? I’m extremely foggy on this stuff (mainly because I can’t understand modern math langauge and have no access to the oral tradition required to translate it).

-drl

23. **Deane**
December 6, 2004

This is easily proved:
http://www.genealogy.ams.org/html/id.phtml?id=6424
Note how many mathematical grandchildren he has!

24.
December 6, 2004

Sad news indeed. He seems to have been generous and an excellent mentor as well. Among his many outstanding students was the Fields Medallist Shing-Tung Yau of Calabi-Yau fame.

String Theory, at 20 Explains It All – Not

December 7, 2004
Categories: Uncategorized

This morning's New York Times has a long and prominently placed article about the 20th anniversary of the "First Superstring Revolution". The Times has a long history of producing overhyped uncritical articles about string theory, for a classic example, see "Physicists Finally Find a Way to Test Superstring Theory". This one does allow some critical voices to be heard, including Lawrence Krauss, who is quoted as describing string theory as a "colossal failure" (which is different than a miserable failure)

Krauss is also quoted as saying "We bemoan the fact that Einstein spent the last 30 years of his life on a fruitless quest, but we think it's fine if a thousand theorists spend 30 years of their prime on the same quest."

Witten is quoted extensively, but he doesn't sound very optimistic these days, saying "It's plausible that we will someday understand string theory", and making the rather weird statement that string theory is "so vast, so rich you could say almost anything about it" (for instance that it is a colossal failure?). He also seems to have given up on the idea that there is some fundamental new symmetry underlying string theory, instead putting his hopes on the existence of some new principle for constructing space and time.

The article also says that few theorists will give up on string theory when supersymmetry is not found at the LHC, with Witten interpreting this not as evidence that string theory is wrong, just that unfortunately it will be harder to get experimental evidence for it than he had hoped. String theorists in general seem to have trouble getting their minds around the idea that it is even possible the theory is wrong. Jeff Harvey does admit that sometimes he wakes up thinking "What am I doing spending my whole career on something that can't be tested experimentally?", but the question of "What am I doing spending my whole career on a colossal failure?" doesn't seem to keep him awake nights.

The article ends by quoting an exchange between Steve Shenker and my colleague Brian Greene. Shenker quotes Churchill, describing the state of research into string theory as "perhaps it is the end of the beginning". Brian seems to be one of the few string theorists around willing to actually consider the idea that the theory might be wrong, arguing that if string theory is wrong, it would be good to know this soon so physics can move on.

Comments

1. **Quantoken**
   December 15, 2004

   I am surry in my previous post I missed the PI:

   "G=1/2N
   N=exp(2/3*\alpha)"

   It should be
   N = PI * exp(2/3*\alpha)

   G is a dimentionful value and N is a dimentionless value. Does it really bothr you that in
   G = 1/(2N)
   I simply omitted the units that should follow 1/(2N). Is that a problem, 99.999% of physicists do not include notations of kg, meter, second in the equations they derived. I never see that as a problem.

   There is a reason for that specific form of exponent exp(2/(3*alpha)). These are not arbitrary choosen numbers, they are not any arbitrary fractional number. I can tell you it's related to the 3-D space and 4-D spacetime. But it is simply not possible to discuss all details of a great theory in short messages on a BLOG web page. Youc an discuss the basic ideas and give some typical calculations, but you simply can't go into too details.

   I have shown how I obtained the precise value of proton mass. I would have also shown how I derived the EXACT mass of the neutron, based on known half life of nutron being 1013 seconds. It has 9 places of effective digits and is well within the experimental error of the best known experimental value.

   I did not use any fractional number or expansion series. The calculation is extremely simple, and has physics reasons behind it. It is definitely and absolutely no coincident when you get a number accurate to the 9th digit, and matches NIST experimental values exactly.

   I respect Peter's request. I will refrain from posting more about my theory today. I will discuss how my neutron mass is calculated and give the detailed description, at a later time, if there is any curiousity.

2. **cosmologist**
   December 15, 2004

   Someone wrote:
   "Now in the natural unit let Newtons constant G be

   G=1/2N
   N=exp(2/3*\alpha)

   …………
   G is dimensionful and N is dimensionless–you can't equate dimensionful and dimensionless quantities! All you get for G is a dimensionless number thats wrong. In natural units (hbar=c=1)
   G still has units of m^{2} or cm^{2}so you still cant equate it to N or alpha in any way unless you have an extra factor that makes it dimensionful. Even then what you get is a total rubbish and meaninglessly contrived "formula" for G that could have been contrived in countlessly many ways from alpha.

   There is no mathematical basis, proof, derivation or justification or reason for presenting your N other than it will give the numbers you want by working backwards. Why not N=pi exp(4/5alpha) or
   N=pi exp(1/10alpha)? Since there is no rigorous derivation or justification then these are just as good. But those simply dont give the answers you want or the "mysterious coincidences" do they?

   Contrived numerical "coincidences" involving the fine structure constant are very easy to make up. For example, I can quickly make up my very own "magic N" right now and get a "theory". Lets see…if
   N=exp(9593/(10000 * alpha)) then calculate
   M=m_{p}* N were m_{p} is the proton mass I get

M=(1.67^{-24}g) exp(131.4241)
=1.99 x 10^{33} grams, which is the exact mass of the sun, assuming I have'nt made any mistakes. So what?! Totally meaningless since I just made N up to get the right answer I wanted, via 30 secs on a calculator. With a little more and time you can contrive really good ones and its made easier by the fact you are calculating very big or very small numbers. The trick is to take as many decimal points in the accepted values and work backwards. The fine structure constant is actually interesting but this is all nonsense.

3. **plato**
December 14, 2004

We need a way to measure NCG in the microstates.

If gravity is strong, there would be indication that such a resonance curve would have revealled itself? Spectrally, your Guitar would make wonderful music?:)

An important feature of atomic spectra that arises from relativity is fine structure – so called because what at first appears to be a single line in a spectrum is actually seen to consist of two or more closely spaced lines when analysed with high precision. Fine structure arises because the electron has an intrinsic angular momentum or spin that interacts with the magnetic field that is produced, for example, as the electron orbits the nucleus at relativistic speeds. The strength of this interaction is characterized by the fine-structure constant, a, given by e2/2e0hc, where e is the charge of the electron, e0 is the permittivity of free space, h is Planck's constant and c is the speed of light. This dimensionless constant has a value of around 1/137. The equations for fine structure contain a factor of a2, so the spacing between the lines is suppressed by over a factor of 10 000.

4. **Quantoken**
December 14, 2004

Plato:

I was aware of all the noises about changing alpha or other fundamental physics constants.

It's all nonsense. There hasn't been any solid experimental data showing alpha is ever changing. Any miniscue "change" observed are just experimental errors. Actually the very notion that alpha could be changing would be strange for any one to have, except for that the SuperString camp probably need to have that changing alpha, just as they desperately need to find a decaying proton.

Should alpha be different billions of years ago, then shouldn't it also be different today but at a location billons of light years away? The special relativity tells us time and space are all relevant. Two different time can well be transformed to two different location in another reference frame. So if alpha is different in different part of the universe, then there could be no physics because you no longer have a set of physics laws that are consistent within the whole universe.

Alpha could never be change and should have the same value here or there, now or billions of years ago. I have demonstrated that alpha is directly related to the size of the universe, because the big N can be calculated easily from alpha. And the radius of the universe equals to a trivial factor times the big N. If you measure the size of the universe from different location, you would get the same number, and hence the same alpha. For the same reason alpha should be the same at different times.

Some in the establishment crackpot science even suggested C be changing over time. That's absord!
How do you know one meter billions of years ago equals to one meter today, and one second time billions ago is the same as one second today? You need that scale consistency to make a meaningful comparison between a speed value billions of years ago and a speed value today. Otherwise you can't even do a comparision and find out any change. And what could provide that ruler that is consistent? Any ruler made of materials could change, even the space itself could be expanding or shrinking, according to theoretists, so light speed in vacuum is the ONLY ruole you can rely on for such a comparison.

So by that notion, the vacuum light speed has been a DEFINED VALUE and it could never possibly change. For the same reason, the time scale, which rely on atomic clocks, would not change either. So the physics that atomic clock rely on, including the alpha, can not change. So these physics constant can not change because they are the very things that define the scale of length, time, and mass.

Alpha does not change, hbar does not change, C does not change, G does not change, the Universe radius does not change. They are all connected and they have been in this universe even before there was space and time. and they can not change.
Without them being constant by definition, we don't even have rulers and clocks for any physics measurements.

Quantoken

5.
December 14, 2004

Not in natural units and not in QED where the fine structure constant is the coupling constant. I also meant dimensionless physical quantities Y are the dimensional multiples
Y=X* habr^{a}*c^{B}G^{c} for some suitable exponents a,b,c.

6. **plato**
December 14, 2004

Changes in the Fine Structure constant?

7. **Quantoken**
December 14, 2004

Matt also said:
"Natural or Planck units ONLY work when you have combinations of hbar,c and G that are dimensionless multiples as discussed."

I say: go figure and try to come up with a combination of hbar, C and G that is DIMENTIONLESS. Because if you do find such a combination, except for the trivial one hbar^0 * C^0 * G^0, then they will have to return all Nobel physics prizes ever awarded.

You can't find such a dimentionless combination out of hbar, C and G and nothing else. I thought your physics teacher should have taught you that in high school physics.

Quantoken

8. **Quantoken**
December 14, 2004

Matt:

I am curious where did you go to school and who taught you physics and how you get your degree, if you have one. They need to recall you for a failed physics education.

Alpha is the fine structure constant and it is a dimentionless physics constant. As such, the value of alpha never changes, regardless of the unit system you adapt, and regardless of any physics theory you develope. On another planet if there's intelligent life developing a different set of science, they will find alpha to be the same value.

Physicists like to write alpha as alpha = e^2/(hbar*C), for the convenience of calculation, Because normally to get quantities like interaction energy, the change e most times occur in the form of e^2. So any time you see a e^2 you do the calculation using the help of alpha. But you never actually plug in the e value in columb to calculate.

To do the calculation strictly in MKS unit sets, there are additional factors to be inserted in the formula, it is not just e^2/(hbar*C). It is actually e^2/(4*PI*Epsilon0*hbar*C). When you counted every thing in, alpha is still a dimentionless number, and remains unchanged under any unit system.

Quantoken

9. **Matt**
December 13, 2004

You defined a quantity in your natural units as

Mo=m_{e}/alpha=1.248 x 10^28kg = me *137

where m_{e} is the electron mass in kg and you used alpha=1/137. I just checked that on my calculator.

However, in natural units with hbar=c=1 (and leaving G fixed and NOT equal to one as you prefer) then
alpha=e^{2}, where e is the charge of the electron. Since alpha=e^{2}/(hbar* c) = 1/137 it is NOT dimensionless in this unit system when hbar=c=1.

You can't have a computation where hbar=c=1 AND alpha=1/137 like you have done, even if G is not set to one…even if you forget G exists! When hbar=c=1 then the quantity you computed is actually Mo=m_{e}/e^{2}, which of course is not in kg. Nothing you say therefore makes any sense at all especially the statement "Assuming the electron mass is equal to the fine structuree constant in natural units" (your words).
It is electron charge that is related to alpha in any unit system and not electron mass.

However, you also take your Mo and compute
an Eo=Mo*c^{2}then define a ro=hbar*c/Eo but you then used the FULL values of habar and c again when you already said these quantities where defined in "natural units" of for hbar=c=1! But it is all academic anyway since you can't define Mo in the first place using your own natural units!

Also your N =exp(2/(3alpha)) can't be defined in your natural units of hbar=c=1 since the exponent now has units of e^{-2} where e is the electron charge. Again, thats with hbar=c=1 and G left alone and NOT equal to one! Since you can't define N when hbar=c=1 then you cant define your G as G=(1/2N) either. When hbar=c=1 then N must have units of m^{-2}or cm^{-2}if G is a gravitational constant.

Natural or Planck units ONLY work when you have combinations of hbar,c and G that are dimensionless multiples as discussed. Even if your choice of natural units is only to set hbar=c=1 as you say and leave G the same you are still wrong since again alpha is still then not equal to 1/137 and your N is simply not defined since you can only take the exp of a dimensionless quantity.

I will quit here for good I think since I really don't want to clog up Peter's blog with all this pointless discussion or encourage any more. The readers here can easily decide for themselves about what is wrong or not even wrong.

10. **Quantoken**
December 13, 2004

Matt:

Before you further distort my words. Let me make one thing clear. My natural unit set is NOT the same as the claimed natural unit set on the textbook, which was obtained by setting hbar=C=G=1, i.e., the Planck Scale.

My natural unit set do NOT set G=1, that's a big difference. Just ask theorists why their calculation of vacuum energy or the cosmological constant is always off by a huge margin of 120 orders of magnitude? Why they can never get the number right? Because they used G as if it is a microscopic constant, stupid!

G is not microscopic constant! They can replace G and use the height of Mount Everest, or the mass of the moon instead in their calculation of vacuum energy, and it would not be better or worse than using G in their calculation. All of those are macroscopic numbers and they shouldn't use them to compute physics at microscopic scale.

Do you realize physicists are in a dead end for this 120 orders of magnitude problem? Or are you going to deny existence of this problem altogether?

Quantoken

11. **Quantoken**
December 13, 2004

Matt:
Why do you have to distort what I said in order to argue with me? My original wording is:

"But first let me adopt a set of natural unit, the unit is derived by setting HBAR and C to be exactly one, and assuming the electron mass is alpha, the fine structure constant, when measured in the natural unit:

m0 = Me/alpha = 1.2483×10^-28 kgs
E0 = m0*C^2 = 1.121928×10^-11 Joules
r0 = (HBAR*C)/E0 = 2.81794×10^-15 meters
t0 = r0/C = 9.39964×10^-24 seconds

r0 happen to be the classical electron radius, but don't read too much into it. There is nothing classical.

Now, in the natural unit set, let the Newton gravitational constant G be defined as:

G = 1/(2*N)"

Clearly, I fixed my natural unit set by setting the numerical value of hbar and c to be one, and fixing the numerical value of electron mass to be equal to alpha. Once you fix the three, you get a natural unit set because there are three measurements to be fixed, the length scale, the time scale and the energy/mass scale.

In the later formula I said in the context of natural unit set:
G = 1/2N

Clearly in no way did I imply G is dimention-less. G has an implied unit which I stated to be that constructed from the natural unit. So my formula tells the numerical value of G, in natural unit set.

The important thing is this is not just an arbitrary unit set. The time and length scale is the correct scale at which time and space lose continuity.

It remains that my theory arrives at the exactly correct CMB temperature, which no ther theory can. And as a bunus I also gave the correct solar constant in the same set of calculation.

Quantoken

12. **Matt**
December 13, 2004

Total nonsense Quantoken. For one thing, in the forum web pages you quoted, farther down in the thread in your first post, you begin by totally abusing the natural unit system. You defined c=hbar=1 then formed a “dimensionless quantity”
G=1/(2N) where N is a dimensionless number, which you give as
exp(2/3 * \alpha) where alpha is 1/137 and G is Newtons constant. However, when hbar=c=1 then G is about G=2.5 x 10^{-70}m^{2}. Note the units now. G is still dimensionful! N would have to have units of m^{-2}or cm^{-2} in this system and is therefore not dimensionaless. If you set hbar=c=G=1 then your number N is just N=1 and won’t give you the answers you want and then contradicts the value of N given by your exp formula. The rest of your derivation then falls on its face. All you did here was essentially take the accepted values of the constants or what you are trying to calculate, altered them at so many decimal places then worked backwards to contrive a “formula” that calculates them “with great accuracy”. There is no theoretical basis or underpinning to any of it.

All physicists know that it is virtually impossible in this natural system of units to use dimensional analysis or to check results for consistency even if you intend to put back c and h and G. The system only works in the first place because for all suitable exponents (A,B,C) and (A’,B’,C’) the interesting physical quantities are dimensionaless multiples of
M_{P}^{A}*L_{p}^{B}*T_{p}^{C}
= hbar^{A’}* c^{B’}* G^{C’}
where M_{P} is the planck mass, L_{P} is the planck length and T_{p} is the Planck temperature.
It is useful and elegant in quantum gravity and cosmology if done right. However, I see from previous posts that explaining anything concrete to you is like trying to nail a rice pudding to the wall.

13. **Quantoken**
December 13, 2004

Now I am going to give a detailed rebuttal to Mr. M-?’s comment on CMB. First quoting his words:

“Yes I CAN honestly say the CMB is NOT star radiation since the CMB is homogeneous and isotropic to an astonishingly high degree. That is, it is the same everywhere in the universe and in every direction. This has been measured with great precision. It is in thermal equilibrium, is blackbody and is absolutly uniform! It is also part of the horizon problem in cosmology, which inflationary models seek to address. The CMB background also has Gaussian fluctuations, which have been measured.

The energy density of starlight falls off as the inverse square so that far from a star, at a distace R, the luminosity is L/4 \pi R^{2}. At sufficiently large distances (and the interstellar spaces are pretty large!)then L–>0. Total radiation from all stars in the universe or a galaxy cannot reach thermal equilibrium and is NOT homogeneous and isotropic. If you have a spherical cluster of millions of stars then the radiation density or luminosity at R (say with respect to the centre of the cluster) is different from the radiation density/luminosoty at some R’. Far from the cluster the radiation density falls to near zero. Hence you need a big telescope to see it!

An infinite, eternal, static and Euclidean universe full of eternal stars can reach thermal equilibrium but then every line of sight would then terminiate at a star’s surface and the night sky would be blazing white and not dark. This is the Olbers paradox and is in direct contradiction obviously to what is observed. The resolution is that redshift effectively cuts off the infinite energy density of starlight.

Also The “M” in CMB stands for “microwave” and stars emit mostly in the visible and IR in case you hadnt noticed. The reason the CMB is “microwave ” is because it has been redshifted. It is a remnant of the time radiation and matter decoupled as the universe expanded. That is why is it so astonishingly homogeneous and isotropic. There is no other possible expanation for it and is the strongest piece of evidence for the Big Bang. Along with predicting redshifts of galaxies and the correct ratios of hydrogen to helium.”

Here are my comments:

I accept every observational facts you cited. But I reject every interpretations of them cited from standard textbooks. I agree that any sounding theory must be self-consistent, and be able to account observational facts without contradicting OTHER known facts. The textbook explainations do not meet this later criteria.

The textbook has NOT been able to explain away where does all the regular star radiations go? Unless you have a giant blackhole to suck up all star radiations, they should still be there and should be observed, in the right amount of energy density per unit volume. The CMB happen to provide the correct amount of energy accounted for the star radiations. we know the CMB is virtually all the radiation energy there is filling the cosmos space. All other frequency spectrums only acocunt for a very small proportion of all the radiation in the universe.

My theory, on the other side, do account for the homogeneous and isotropic and blackbody radiation spectrum properties of the observed CMB, and do not need to answer the puzzle where does all the star radiation go. Because star radiations ARE the source of CMB.

In my theory, star radiation do shift to the lower frequency end as they travel through the space for billions of years. This redshift, having nothing to do with Dopler Effect or gravity red shift, is made necessary by a fundamental requirement of my theory, that the quantum information in the universe is a conserved amount. If star lights do not red-shift, we would be able to obtain information from any arbitrarily far away distance, and hence the amount of information would not be a fixed amount, but rather an infinite amount.

Quantum information conservation not only leads to the Hubble Redshift we observe. It also leads to the conclusion that the observable universe is neither expanding nor shrinking. Hence the radius of the universe will not change. Therefore the relationship between G and the universe radius that was discovered by Paul Dirac is NOT just a coincidence that we happen to live at a time when the universe happen to expand to the correct radius for that relationship to be true. Instead it is not coincidence at all and the relationship reflects a fundamental physics principle that has always been true, regardless of what time we live in.

Remember the Mach principle which says the whole mass of the universe caused gravity and inertia?

Once again, my theory accounts for all observation facts and makes precise predictions, without any self-consistency problem. I just have not got a chance to reveal the full details of my theory, yet. Given time, I will do it. If you are serious in pursuing scientific truth, please reserve your judgement until you learn the full details of my theory.

Now, can any one do me a favor, and show a calculation of star radiation energy. And see what's the energy density is when averaged over the volume of the universe? Such calculation should be easy, given the known radius of the universe, the critical density according to Einstein Equation, and the baryon density being about 5.4%, and we know the radiation of a typical star like our Sun, which at 2×10^30kg mass, shines 1360 watts per square meter, at a distance 1.496×10^11 meter away from its center.

Can any one do such a calculation and compare the result with CMB energy density?

Quantoken

14. **Quantoken**
December 13, 2004

Mr. M-? said:

"So if I am "clueless" and a "moron", as you put it then I am in good company.(Here is another one for you–the average stellar mass is (hc/G)^{3/2}]/m_{p}^{2}=3.77 x 10^33 grams)"

I say: Sigh! Why do you have to invite insults to youself, by volunteering yet some more solid evidences that you are indeed incapable of doing the kindergarten arithmetics, even with a scientific calculator right at your hand.

Your calculation is simply not right. Let me teach you a bit arithmetics for free, even though Einstein would have done that for some coockies from a little girl.

(hc/G)^{3/2}]/m_{p}^{2}
= (2*PI)^(3/2) * (hbar*C/G)^(3/2) / (m_proton)^2
= 15.7496 * m_planck^3 /m_proton^2

Go find the Planck Mass and Proton Mass at this site:
http://www.physics.nist.gov/cgi-bin/cuu/Value?eqplkm|search_for=Planck+Mass
http://www.physics.nist.gov/cgi-bin/cuu/Value?mp|search_for=proton+mass

The result should be:
(hc/G)^{3/2}]/m_{p}^{2}
= 15.7496 * (2.17645×10^-8 kg)^3 / (1.67262171×10^-27 kg)^2
= 5.8039×10^31 kg
= 5.8039×10^34 gram

Your number 3.77×10^33 gram, is simply not right.
Even give you the benefit of doubt that you confused h and hbar, which is differ by 2*PI, the result would have been 3.685×10^33 gram, not 3.77×10^33 gram. I do not know how you could get the decimal numbers wrong by that much, with a computer sitting right in front of you.

There is a subtle difference between order of magnitude coincidence and exactly match to the precision of several decimal places. The later could not be by chance.

I never said I will not comment on the CMB thing, but I said I will save it until we have clarify the issue whether you are capable of doing simple math calculations. Looks like you don't.

So I will get back to discuss the CMB in just a little bit. Meanwhile why would I want to juin the establishment science camp and wasting my lifetime, without finding any real scientific truth, when I already have a decent job and marvelous income, doing the kind of things I like and at the same time I can do my own scientific explorations and find the real truths?

Quantoken

15.
December 13, 2004

Dear Quanto
It is clear any dialogue with you is pointless.
First of all the relation between proton mass and Hubble constant was discussed by Weinberg, Hermann Bondi (Cosmology, 2nd edition Cambridge University press, chapter III, 1960)and by Dirac.
So if I am "clueless" and a "moron", as you put it then I am in good company.(Here is another one for you–the average stellar mass is (hc/G)^{3/2}]/m_{p}^{2}=3.77 x 10^33 grams)

My comments on the CMB are matters of undeniable fact you can find in any basic textbook. Your not responding to it proves you don't actually understand the nature of what the CMB is.

I gaurantee that you won't ever find anyone anywhere with any real physics/math education who will ever take you seriously. Perhaps if you could tell us your degrees, institutions attended, papers published in peer-reviewed journals, or articles in the Arxiv (even if only one or two) then your opinion might carry some weight. BUT this is how science and mathematics is done and why it is so remarkably successful as a dynamic mode of human thought. The system is not perfect but it is the best there is and in the end it always works. Calculations, even simple arithmetic ones, can only presented within a rigorous, coherent and logically self-consistent structure or framework with reference to previous work. You have not done that.

If you think I was "rude" then submit your "piece of gold" arithmetical calculation to Physical Review or Astrophysical Journal then you might get some really rude replies. Of course, predictably, you will view all this as the "physics establishment" out to get you or censor you. You could never accept that you are just spouting ignorant self-deluded nonsense or are just plain wrong, but would only accept that you are being rejected or persecuted on "political reasons".

If you want to do science you have to go through this long hard route just like to be a heart surgeon takes 10 years of hard work and dedication at medical school! Self-taught surgeons are avoided. This is how science works and the only way it can work…something you don't seem to understand. Frankly I am getting really tired of crackpots always pushing "their theories" and I have already given you more time than anyone else would or will.

16. **plato**
December 13, 2004

Quanto,

You are running up against a well established trend of developing insight that has fallen short by some of our most profound mathematical minds here:)

Even I, as a commoner like to embrace all of them(theoreticists and mathematicians a like) and try and keep them in line, with a encapsulated view of reality, but first we have to contain the legitimacy of what is being relayed.

It has to all make sense?:)

17. **Quantoken**
December 13, 2004

Further commenting on M-"". He said:

"Also The "M" in CMB stands for "microwave" and stars emit mostly in the visible and IR in case you hadnt noticed. The reason the CMB is "microwave " is because it has been redshifted. It is a remnant of the time radiation and matter decoupled as the universe expanded. That is why is it so astonishingly homogeneous and isotropic. There is no other possible expanation for it and is the strongest piece of evidence for the Big Bang. Along with predicting redshifts of galaxies and the correct ratios of hydrogen to helium.

The mass of the proton can easily be evaluated from Mp=Ho^{2}/GN, where N=baryon number density, Ho is the Hubble constant and G is Newton's constant. The mass of a typical elementary particle is (h^{2}H_{o}/Gc, where h is Planck's constant. So what?? These are well known. And you can replace Planck's constant h with e^{2}/c=137h, the fine structure constant. These numerical coincidences have been known for a very long time."

Looks like that's further evidence M-"" does not know how to do simple arithmetic calculations. If that is not the case, please plug in some real numbers and show us exactly how you calculated the Mp from hubble constant and baryon number density, and what is the value of Mp you obtained that way. Also you seem to have no clue that astronomers first tried to obtain baryon mass density based on observation data, then try to use that, plus known Mp, to calculate baryon NUMBER density, instead of the other way around to obtain Mp.

I obtained a precise value of Mp based on first principle, to an accuracy of many decimal places.
I would be happy if you can show me that your calculation is only off by one or two orders of magnitude.

I would have answered to your comment on CMB and star radiations. But since you are so clueless, I will save that till you can show me that you are capable of doing the kind of elementary school arithmetics, that Einstein taught an eight year old kid in exchange for her sweet cookies, in his late years.

18. **Quantoken**
December 13, 2004

Mr ""(noname) said:
"Your 'derivation' of the proton mass is of course total bunk"

I am sorry. But tat's very rude and uneducated behavior on your part. Your comments only shows how ignorant you are. You have no idea what I have been taking about. You are totally clueless and you still feel you can make judgement of my theory. I am sorry you are such a M-?!.

An educated scholar would never be shy to admit things he doesn't understand, and would not comment on something until he learns at least some basic facts of the thing he is going to comment on.

I put a piece of gold in front of you but you could not recognize it, because you are blind. I have shown you how I obtained the precise mass of the proton, detailing every single step, though the computation is so simple there is really not that many steps to shown.

Wouldn't you at least politely acknowledge the fact that there was no math error in my calculation? Or you are unable to verify and make a judgement on my math?

Let me give you a hint. The spin state of proton is an external state, so it has nothing to do with proton mass. The "states" I was talking about are the internal, intrinsic states, which certainly can not be observed directly, but has to be infered from other facts. It's just like Quarks, they can never be observed as stand alone particles but their existence can only be infered from other experimental evidences.

Most physicists agree protons, as well as some other fundamental particles that can change. The fact that they changes implies that they have some sort of internal structure. Seeing those particles as point particles and hence there are no structure to be found of those particles in our 4-D spacetime, some of the physicists turn towards some presumably exist additional dimentions to look for that intrisic internal structure. That's the whole idea where Super String theory came from.

That's where the science made a wrong turn. Why do one need additional dimentions to provide background for some sort of intrinsic structure? You don't need to. You can have structures without a spacetime background, and certainly without a 11 dimentional super space.

It's rather illogical to me that physicists can accept the idea that below a certain small scale, the space and time loses continuity, and hence no longer exist, however they still can not accept the idea that you can have physics structures without spacetime background at all.

Also amazing to me that they believe the Planck Scale is the cut off, hence below Planck Scale there is no longer a continuous space and time dimentions, and hence these 4 dimentions no longer exist. However they believe in the other 6 dimentions, each though not longer than the Planck Scale, could still be continuous and meaningful, even if they are smaller than the Planck Scale!!! I do not know how their logic works.

At scales smaller than the discontinuity cutoff scale, both space dimention and time dimention are no longer meaningful physically, and hence physicists must throw away any equation that involves space and time, and use a brand new concept to construct some new physics.

That's the whole idea of my GUITAR theory, Generalized Universal Information Theory And Relativity. My theory has resolved all the puzzles the extablishment theories could not resolve, and has succesfully been verifies by many precise calculations.

Unfortunately it does not seem that there are any one here who can understand it. I meantioned the Bekenstein Bound, and it does not pull a ring for any one. Hasn't anybody heard about what Bekenstein Bound is? Also hasn't any one heard that Einstein said "God does not toss a dice". Hasn't any one heard about Paul Dirac's Large Number Postulation?

I do not just had a dream one night and came up with my new theory. Everything in my theory is solidly based on previous researches by well known scientists.

Quantoken

19.

December 12, 2004

Your ‘derivation’ of the proton mass is of course total bunk. The nuclear dipole moment of a proton in a magnetic field forms a 2-dimensional Hilbert space of states +> and ->, which can be identified with qubits 1> and -> as the basis for a quantum computer for example. You can calculate Shannon or Von Neumann entropies for qubits. Your entropy formula is wrong (the Von Neumann one is the correct one in this context of quantum information using the density matrix) and even if you can compute the quantum entropy associated with the states of a proton that has absolutely nothing to do with its rest mass!

20.
December 12, 2004

Yes I totally agree. I did say “eternal stars”. But as a resolution I was thinking more along the lines of the redshift argument in Weinberg’s Gravitation and Cosmology, pages 611 and 612. Olbers paradoxes are only relevant for steady state universes that have existed for an infinitely long time.

21. **cragwolf**
December 12, 2004

Someone wrote:

“An infinite, eternal, static and Euclidean universe full of eternal stars can reach thermal equilibrium but then every line of sight would then terminiate at a star’s surface and the night sky would be blazing white and not dark. This is the Olbers paradox and is in direct contradiction obviously to what is observed. The resolution is that redshift effectively cuts off the infinite energy density of starlight.”

Actually, the resolution of Olber’s paradox lies in the finite lifetime of stars. Expansion contributes no more than a factor of a few. See papers by Wesson and Harrison on the subject. Or just read the relevent sections out of Harrison’s textbook, “Cosmology: The Science of the Universe”.

22.
December 12, 2004

“Can you honestly say that CMB is NOT star radiation…where does all the star radiation go?”
………………
Yes I CAN honestly say the CMB is NOT star radiation since the CMB is homogeneous and isotropic to an astonishingly high degree. That is, it is the same everywhere in the universe and in every direction. This has been measured with great precision. It is in thermal equilibrium, is blackbody and is absolutly uniform! It is also part of the horizon problem in cosmology, which inflationary models seek to address. The CMB background also has Gaussian fluctuations, which have been measured.

The energy density of starlight falls off as the inverse square so that far from a star, at a distace R, the luminosity is L/4 \pi R^{2}. At sufficiently large distances (and the interstellar spaces are pretty large!)then L–>0. Total radiation from all stars in the universe or a galaxy cannot reach thermal equilibrium and is NOT homogeneous and isotropic. If you have a spherical cluster of millions of stars then the radiation density or luminosity at R (say with respect to the centre of the cluster) is different from the radiation density/luminosoty at some R’. Far from the cluster the radiation density falls to near zero. Hence you need a big telescope to see it!

An infinite, eternal, static and Euclidean universe full of eternal stars can reach thermal equilibrium but then every line of sight would then terminiate at a star’s surface and the night sky would be blazing white and not dark. This is the Olbers paradox and is in direct contradiction obviously to what is observed. The resolution is that redshift effectively cuts off the infinite energy density of starlight.

Also The “M” in CMB stands for “microwave” and stars emit mostly in the visible and IR in case you hadnt noticed. The reason the CMB is “microwave ” is because it has been redshifted. It is a remnant of the time radiation and matter decoupled as the universe expanded. That is why is it so astonishingly homogeneous and isotropic. There is no other possible expanation for it and is the strongest piece of evidence for the Big Bang. Along with predicting redshifts of galaxies and the correct ratios of hydrogen to helium.

The mass of the proton can easily be evaluated from Mp=Ho^{2}/GN, where N=baryon number density, Ho is the Hubble constant and G is Newton’s constant. The mass of a typical elementary particle is (h^{2}H_{o}/Gc, where h is Planck’s constant. So what?? These are well known. And you can replace Planck’s constant h with e^{2}/c=137h, the fine structure constant. These numerical coincidences have been known for a very long time.

Basically, everything you have said is not even “not even wrong”

23. **A.Nonymous**
December 12, 2004

Quantoken: your numerical result looks ok, but while you’re not telling us how you compute your number of possible states of a protons you’ll understand that we can only reserve our judgments. Have you written down the *details* somewhere ? (If not, the group sci.physics is unmoderated.)

24. **Quantoken**
December 12, 2004

Before I start to calculate the proton mass, a quest that no establishment theories ever dreamed of accomplishing, but so trivial in my theory, let me explain the main concept of my theory.

1.The universe is composed of nothing but quantum information, or quantum entropy. The entropy is measured in qubits, if the system takes one of two possible states, it’s one qubit. i.e., one qubit is ln2 entropy when using the traditional entropy definition of ln Omega.

2.The amount of quantum information is exactly integers of qubits. That’s the source of quantization of all physical observatives.

3.”God does not toss a dice”. The total amount of quantum information is a CONSERVED quantity, it never increase or decrease. In fact it could not possibly increase or decrease because quantum information do not exist in a space time background, hence there is not even a concept of increase or decrease.

4.Spacetime, and other observable properties of the physics world are just statistical manifestations of quantum information. Theories like the Bekenstein Bound, provides hint how to relate quantum information and use them to construct space time. Specifically, it can be described by a model where all quantum information distribute on the surface of a 4-D spacetime volume.

For point particles, it’s measured mass is associated and proportional to its intrinsic internal quantum information.

Now consider protons. Starting with an eight fold structure. I fugured out the number of possible states a proton can sit in to be 1, the base state, plus 3 kinds of excited state, each contain 5! possibilities, and one another kind which has 7! possibilities. Plus the charge can be in one of two different states. So the total number of states are:
S = 2 * (1 + 3*5! + 1*7!)
That’s how you calculate the proton mass. You can find the answer using a calcul

S = 10802

Measured in qubits, that's
S = ln 10802 / ln 2 = 13.399011

That would be equal to the proton mass. i.e., the
proton mass is 13.399011.

Remember I use the Natural Units, whose unit of mass equals to the electron mass divided by fine structure constant.

If you want SI unit,
Mp = 13.399011 * m0 = 13.399011 * 1.24831335×10^-28 kg
Mp = 1.67262×10^-27 kg

The published proton mass is at:
http://physics.nist.gov/cgi-bin/cuu/Value?mp|search_for=proton+mass

(1.67262171 x10^-27 kg)

You make the judgement.

Quantoken

25. **Quantoken**
December 12, 2004

What do you mean "back of envelope" calculations? Do you remember Occram's Razer? All things considered, the simplest theory that gives the correct answer is the correct theory.

OK, so my calculations takes the back of an envelope, so what they leads to the correct answer. OK, so Super String calculations are complicated. To calculate amoung some 10^123 different super string "landscapes" you probably needs to construct a quantum computer using the whole resource of a galaxy, and you still won't be able to come up with an answer. Not a correct answer, not even an incorrect answer. Nothing. Not even wrong.

That's what I call crackpot science when you can't even come out with a reality-relevant answer, be it right or wrong, given 20 years of development and after consuming thousands of the best intelligent minds on this planet. As I asked, how many multiples of the already spent in-vain efforts does it take the scientific community to waste, before you will finally give up?

You know, the special and general relativity was not developed by one of those thousands of people who had worked in the field for decades. They were developed by a little third class patent clerk whose name was never known. Who failed the colleage entrace exams, who could not even find a job teaching in high school, and who would probably never been able to publish anything in today's environment despite of much wider access to information. Next year is the centenual anniversary of his great discoveries.

My theory is not a back-of-envelope theory. It takes more papers to write down. But the main idea and some of the typical calculations are indeed simple enough to be written on a few pages.

Next, I will show you one more thing that's trivial, but has puzzled the physicists for so longer they no longer seen to be bothered any more.

I am going to show you exactly how much the mass of a proton should be, and why, and it's ratio to electron mass. Starting with a slightly different "8 fold method". And yes the main calculation can be written on the back of an envelope. And there will be no adjustable parameter.

26. **Chris Oakley**
December 12, 2004

Quantoken –

Saying that the big bang theory is wrong does not make you a crackpot, but claiming that you have an "ultimate correct answer" when all you have is a few plausibility arguments and back-of-envelope calculations does. Take your business elsewhere.

27. **Quantoken**
December 12, 2004

Plato said:
"You must have a "compass" and have a way of knowing your coordinates from your passenger's seat?:) Maybe "making coffee" and taking on stewardess functions as well?:)"

Are you sure you are not losing a bit of intelligence by knowing too much contemporary junk science? Like, here is a trivial question, do you know whether there exists a way how to get from point A to point B, without the aid of any type of manual or machine powered transportation carrier?

A sane person with the slightest intelligence would tell you directions without help of a compass, and without a need of coordinates. Actually the concept of directions like North, South, East, West were formed way back before compasses were invented and the concept of coordinate system was formed. Actually compasses do not tell you the directions, it merely tells you which way the north geo-magnetic pole is. On modern airplanes they no longer use a compass for navigation helps.

My theory is still in development, I do not expect it to explain every thing yet. But at least it explains some of the important things very well, that other theories fail to explain. There is not another theory which can calculate the correct CMB temperature. There is not another theory which gives the correct cosmological constant but mine can. There is not another theory explaining exactly what is the origin of gravity, but mine can.

The CMB temperature is certainly not the most important thing. As I said, the important thing is what we observe. We know stars radiate energy. No theorist can deny that. We can calculate how much energy stars radiate, and again there can be no dispute regarding the star radiation energies. These energy do not disappear. And the star radiation energy come up to be exactly the right amount of energy in the CMB background.

So can you honestly say CMB is NOT star radiation? Can you still continue to insist that CMB is the remains of BigBang? If you attribute CMB to BigBang, you've got to tell me where does all the star radiation go? Where are they now? All swallowed up by a giant black hole?

And I haven't even started to talk about my theory yet. I just gave a few calculations that any one with a calculator can verify and check against the most accurate experimental results.

Unfortunately the PhysicsForums web are so paranoid that once I uttered a word suggesting the BigBang was wrong. They immediately banned me before I could utter the first word explaining exactly what my theory is based on.

I have not started talking about my theory yet. But I can tell you it is based on what Einstein says “God does not toss a dice.”, it is also based on the Bekenstein bound and what is meant by “God does not toss a dice.”

Both the Super String Theory camp and Loop Quantum Gravity camp are wasting their time without the possibility of achieving any meaningful physics results, because they are working on the wrong scale, the Planck Scale. They keep wondering why their calculation of vacuum energy is off by at least 120 orders of magnitude, without realizing they used the wrong constant in their calculation.

The space and time loses continuity and hence is no longer meaningful, at a scale far higher than the Planck Scale.

Quantoken

28. **plato**
December 11, 2004

More Comedy Then:)

You must have a “compass” and have a way of knowing your coordinates from your passenger’s seat?:) Maybe “making coffee” and taking on stewardess functions as well?:)

Quanto as much and pleasing that these equations might be consistent with one more “prospective view,” without gravity in the understanding, how would you explain resonance curve?

*Since Reinmannian geometry is the mathematical core of general relativity, this means that it too must be modified in order to reflect faithfully the **new short distance physics** of string theory.*Brian Greene

Without the “potential” of the supersymmetical reality, you would have no consistent way to view the cosmo, yet, you recognize the temperature values as important?

Just wondering?

29. **Chris Oakley**
December 11, 2004

Hi Quantoken –

In regard your “ultimate correct answer”, thanks for the welcome comic relief. The discussion was getting too intense.

30. **Quantoken**
December 10, 2004

Let me comment on the dispute between Peter and Lubos. Lubos seems to suggest that for any one to have the qualification to even criticise Super String Theory one has got to be an expert in the field first, or else he/she should shut up.

Nothing is more absord than that logically. Surely ANY ONE can criticise SST, even one who know little physics. The important thing is SST has NOT delivered anything that it promised, and it has not proven itself to be a relevant physics theory of this physics world. It may be a good mathematical theory but so far there is no evidence it has anything to do with physics at all. That’s an accepted and undisputed fact.

Let me use an analog: As a passenger I boarded an airplane, and found it’s heading to the wrong direction. Shall I have the right to question and challenge the captain, or should I shut up because I absolutely no nothing about how to fly an airplane, and have not read the first few pages of the pilot manual? Surely I do, because it does not take an export to figure out that the pilot is not delivering what he is expected to deliver.

Let’s face it: SuperStringTheory is most probably wrong and is not going any where. Now let me ask this question: Suppose it is wrong. What does it take for the scientific community to recognize it as a colossal failure?

Certainly, several thousands of scholars of some of the best intelligences on this planet wasting the prime of their lifetimes without achiving anything physically significant, is still not enough to convince them they are on the wrong track. How about 3 generations of best scientists wasting their lifetime in an invain search for an ultimate theory in super string for over a hundred year, just to find out they are wrong?

Or how about 10 generations and a few hundred years? How much is too much? How long is too long? At what end of a fruitless pursuit, will superstring theoriests finally ackowledge that they have been all wrong and they have wasted their intelligence their whole lifes?

Serious I want to know the answer. How many more years should we continue to try, before finally giving up? Maybe you will give up whe the tax payer money runs dry?

Both SST and LQG are wrong all the time. I have the ultimate correct answer. The evidence being I have been able to calculate a number of important parameters of the universe to an amazing precision, including figuring out the exactly CMB temperature and solar radiation constant, with very simple calculation. See:

http://physicsforums.com/showthread.php?t=49938
http://physicsforums.com/showthread.php?t=49935
http://physicsforums.com/showthread.php?t=49934

Quantoken

31. **D R Lunsford**
December 10, 2004

This is what I mean, Lubos. Obviously, to anyone with a functioning mind, Larsson not only knows what HE’S talking about, he knows more about what YOU’RE talking about than YOU do. So to flame him like this makes you look ridiculous, like one of those snapping little toy dogs giving a big dog the business, while the big dog just yawns and thinks – “mmm snack”.

What I want to know is – how long have you been bamboozling your colleages, fellow students, parents, neighbors etc. etc.? You make a good Czech hockey player – you know, irritating and fast…

-drl

32. **Lubos Motl**
December 10, 2004

I'm somewhat uncertain whether it is reasonable to reply to pitiful creatures such as the Gentleman signed as "Thomas Larsson" below.

Nevertheless, even most of children interested in physics who are above 10 years old with IQ above 90 know that string theory predicts consistent dynamics of quantum gravity as well as all required physics of the Standard Model in the appropriate regime.

Some of the solutions of string theory are *pure* Standard Model at low energies, see e.g. the recent intersecting braneworld paper by Christos Kokorelis.

http://arxiv.org/abs/hep-th/0412035

But we don't know whether we should expect a pure standard model – SUSY and GUT at higher energies is more attractive because of many reasons.

Aaron Bergman has written a lot of relevant technical comments, and I encourage all participants with IQ above 70 to try to read and understand Aaron's comments as opposed to the idiotic rants signed by "Thomas Larsson".

33. **Thomas Larsson**
December 10, 2004

DMS,

Your wait for "ST-implies-SM-gauge-group" will be a long one. Because honestly speaking, ST can be used to derive any possible gauge group, not just the standard model—this is "prediction" for string theorists.
…
Clearly, there is nothing "unique" about this contrived construction. But throw in enough buzzwords (like branes, orientifolds), and you will silence the doubters by hiding the arbitrariness and keep harping about the "uniqueness of string theory". This is dishonesty, plain and simple

That Motl decides to lie about string theory predicting the SM is somewhat surprising. After all, it is not a sofisticated lie, it is just a plain **stupid** lie, which any non-specialist can see through. This is really stupid, because it leaves the impression that lying is all that string theory is about. What is even more surprising is that the rest of the string community seems perfectly happy to let this barking Harvard puppy define the public image of a string theorist. (It seems like he has been forced to retract his "interesting" opinions about women, though.)

(although Prof. Motl does surprise on occassion, as on the anthropic stuff),

I think we should be grateful to Susskind and Douglas to make string theory's inability to explain any experimental data manifest and quantitive. Today it takes days or maybe even months to work out the properties of a compactification (with the wrong sign of the CC). Even if you could automate this process to only take a fraction of a second, covering a Landscape of 10^1500 vacua will still take significant time; remember that our universe is only 10^80 seconds old. I think that the prospects of not making any progress in the next 10^1500 seconds might deter future students. Or does anyone believe that string theory will pick up steam once Witten retires?

34. **Aaron**
December 10, 2004

Just to get this out of the way, I'm not Lubos, and I'm not speaking for him.

To respond, here are a few examples. The relationship between Chern-Simons theory and the open topological string was derived by Witten from applying his open string field theory of the full string theory to the topologically twisted model.

Later Gopakumar and Vafa took Maldacena's ideas from AdS/CFT and applied them to large N transitions in topological strings. This led to the conjecture that the Chern-Simons theory on S^3 is equivalent to closed topological strings on the resolved conifold, (The transition here is the 'conifold transition' from T*S^3 to O(-1)+O(-1) -> P^1.) This has consequences for Gromov-Witten invariants.

If I remember, there are other conjectures that come about from embedding the topological string in the full string theory and using the various known facts (especially dualities) about the full string theory. I think the existence of Gopakumar-Vafa invariants follows from such embedding.

Marcos Mariño covers a lot of this in hep-th/0406005. Is that what you wanted?

35. **plato**
December 10, 2004

JC,

Arguably there's a fine line between a genius and a crackpot. The genius turns out to be correct in the long term, while the crackpot remains a "madman" forever.

So was Einstein a Genius, or a madman? If his last thirty years were unproductive, maybe a madman?:)

Einstein's search for a unified theory is often remembered as a failure. In fact, it was premature: physicists first had to understand the nuclear forces and the crucial role of quantum field theory in describing physics–an understanding that was only achieved in the 1970s.

So you see, you might of thought of him as a madman all the while, you just didn't understand him:)

36. **mathematician's query.**
December 10, 2004

Aaron and Lubos,

yes, as a mathematician I am well aware
that Gromov-Witten invariants, etc originated
(or were re-born) in physical models, notably topological theories. Many mathematicians
are also keenly interested (and participating in)

the prospected construction of "topological M-theory"
a la Okounkov, Nekrasov, Vafa, and others.

My question, however: what, SPECIFICALLY,
does Lubos have in mind when he claims that
such results are simply "truncations" of work
done in string theory. There is a huge and
obvious difference between
A. "truncating" the whole theory to form a
toy model, but using techniques specific
to the toy model to get results (e.g. as a test
for the larger theory), versus
B. taking a "truncation" of results in the big
theory to get results about the toy models.

My impression (from talks by Vafa, for example) is
that it's mostly A that's been done. Lubos
appears to be claiming that B happens, so that successes in Gromov-Witten theory, etc are also successes of string theory. I'm asking what is the situation from the point of view of someone who works
on string theory.

So far the only answer from Lubos was to call
me an idiot. Which only tanks his credibility
further.

37. **D R Lunsford**
December 9, 2004

Yes JC, but someone like LM is just wrong, period. He doesn't have a secret plan to save physics from the North Viet Cong and the insurgents. What he claims is just wrong. Peter doesn't go around claiming things that are flat wrong.

The funny thing is, I think LM would be better off if he'd get behind something that isn't so easily dismissed as nonsense. I'd rather believe in something completely implausible rather than in something that is demonstrably wrong and even anti-scientific.

-drl

38. **JC**
December 9, 2004

Plato, et. al.

Some folks like to bury their heads in the sand, for whatever reasons. Whether they are being in "denial" or are in a genuine state of "ignorance is bliss", is debatable.

Whether they are right or wrong in the long term, they are entitled to believe whatever they want. In the end, who really cares what anybody else thinks?

Arguably there's a fine line between a genius and a crackpot. The genius turns out to be correct in the long term, while the crackpot remains a "madman" forever.

39. **plato**
December 9, 2004

JC

Nobody is holding a gun to your head and forcing you to listen to Peter and/or Lubos in the first place.

Why would anyone ignore the basis of what is happening between these two gentlemen/unless you choose **not** to bury your head in quantum geometry?:)

Einstein refused to accept the quantum mechanical positions, but we have seen how things have progressed?:)

40. **JC**
December 9, 2004

As far as I'm concerned, people are going to say whatever they want. If you don't like what Peter and/or Lubos is saying or not saying, you're entitled to say whatever you want. Or for that matter, one is also entitled to completely ignore Peter and/or Lubos, or anybody else for that matter.

Nobody is holding a gun to your head and forcing you to listen to Peter and/or Lubos in the first place.

41. **Lubos Motl**
December 9, 2004

the Higgs mass.

the lightest sparticle mass.

OK, now I've defused all criticism once and for all. I never thought it would be so simple! 😉

42. **Chirally Challenged**
December 9, 2004

Lubos can defuse all criticisms once and for all by telling us the Higgs mass.

And for encore, the lightest sparticle mass.

43. **Aaron**
December 9, 2004

Responding to:

> Lubos, are you saying that there is
> specifically stringy reasoning involved
> in things like TQFT, Donaldson/Seiberg/Witten
> theory, Gromov-Witten invariants and other
> much-admired offshoots of string theory?
> i.e. where you think in terms of the full
> string theory, get some idea/result and
> degenerate back to the purely topological or conformal theory?
> We can all admire Vafa et al, but it's not
> clear to me how much (if at all) the spectacular
> successes in mathematical toy models have to
> do with strings per se.

As regards to anything related to the topological string (Homological mirror symmetry, Gromov-Witten, Gopakumar-Vafa, Donaldson-Thomas, yadda yadda yadda) many (if not most) of those spectacular successes were originally derived using intuition from string theory and were later (sometimes) proven to be correct by mathematicians. I think if you ask any mathematician who works on those areas, they will tell you that physical reasoning has been extraordinarily fecund.

44. **plato**
December 9, 2004

As a commoner(a child of the universe and a impressionble one) it has become important for me to distinquish where the **pulse** is in these exchanges:)

If both areas deal in mathematical structures then it would be essential that a common bond be linked through the mathematics? If agreement is struck in theoretical advances, then supportive features of the physics would be important?

Where Brain Greene had made such a statement about moving on if such a concept as strings/Mtheory was resolved, Lubos also made same. Being critical of what one has gained in information about subject would seem important to me, to make a careful assessssment?

But I am more intrigued about the advances of quantum gravity and the perspectives forming around this. Why are not, all models attack with such vigor? I have many links you can choose from:)

Do you not all have the same departure point?

I think I will post Smolin and Susskind exchange again, to hone up on what's going on.

It's good to see that both have agreed on some basis to move from and that Peter is asking questions. What other theory asks the question about the unity and arises from the planck epoch in a systematic way?

45. **Lubos Motl**
December 9, 2004

Haha, Peter, you're funny. You probably think that it is completely OK to misunderstand this statement, but you're wrong. Misunderstanding this statement means that you had to misunderstand the whole picture.

You know, one can follow some formulae on 6 pages, but if one does not understand why these formulae are written down, then it's completely useless – it's like the basic school kids who memorize something that they don't understand.

OK, you should not be intimidated by the first sentence you don't understand. You should try at least 150 pages of Zwiebach. Do it for me! 😉 And don't cheat – you know very well that the first 6 pages of the book are NOT string theory yet.

46. **Peter**
December 9, 2004

OK Lubos, I checked a few more pages in Zwiebach, and you're right. When I get to page 6 he writes "String theory is an excellent candidate for a unified theory of all forces in nature". Yup, there's a statement I don't understand.

47. **Lubos Motl**
December 9, 2004

Sorry, Peter, but I said – and I still say – that you don't understand the first *pages*, not just one page. Be sure that one page is not enough.

Indeed, you have written a lot of rubbish about string theory. And it's also true that the people may decide for themselves which writing is more scientifically reliable.

No doubt, there exist intellectual dwarves who will say "Peter Woit is the more trustworthy party about string theory". You know very well that I don't care a single bit about what complete idiots are thinking.

What I would find more disturbing would be if an *intelligent* person happened to attend your blog and if she were manipulated into thinking that your rants are relevant for science. That would be sad.

This is why I find it very important to emphasize that you, Peter Woit, have no idea what string theory is.

48. **DMS**
December 9, 2004

DRL—

Your wait for "ST-implies-SM-gauge-group" will be a long one. Because honestly speaking, ST can be used to derive any possible gauge group, not just the standard model—this is "prediction" for string theorists.

The rough explanation is that you get a U(N) Yang-Mills fields on the world-volume of N coincident D-branes. By a suitable kind of D-brane configurations, the standard model gauge group can be easily arrived at. A nice discussion can be seen in Chapter 15 of Zweibach's book, though I am sure there are summer school articles in arxiv that show the same. Furthermore, on intersecting D6-branes wrapped on a 6-torus (with orientifolds , to be precise) the standard model with fermion content can be "derived". But " little details" like symmetry breaking etc. remain to be worked out.

Clearly, there is nothing "unique" about this contrived construction. But throw in enough buzzwords (like branes, orientifolds), and you will silence the doubters by hiding the arbitrariness and keep harping about the "uniqueness of string theory". This is dishonesty, plain and simple (although Prof. Motl does surprise on occassion, as on the anthropic stuff), so it is not surprising to see personal attacks on Woit.

DMS

49. **Peter**
December 9, 2004

I just checked the first page of my copy of Zwiebach's "A First Course in String Theory", and I'm pretty sure I understand it.

As for the rest of Lubos's claims, by now there's a huge amount of material about string theory written by me and by him on our weblogs and various other places on the internet. People can judge for themselves which of these two bodies of writing is scientifically more reliable.

50. **Lubos Motl**
December 9, 2004

I did not make any attacks. I am just carefully explaining to everyone on this forum that Peter Woit has no idea about string theory.

In my opinion, it is extremely important to say so because many people who attend this blog – obviously and provably – don't realize this fact.

Peter Woit never studied string theory, he never understood even the first pages of the most elementary textbooks for the undergrads. Obviously this implies that he could have never written a paper about it and his opinions about string theory are scientifically irrelevant.

It's nothing personal. I insist that if Peter were honest, he would emphasize his ignorance about the field at the beginning of every single article that he writes.

51. **D R Lunsford**
December 9, 2004

Lubos,

I'm sick of your attacks on Woit. You know, some of the gibing here and there, between us in particular, is harmless. But make no mistake, you don't match up to Peter when it comes to having clues. Stop embarrassing yourself. Compared to Peter, you're a nuisance ankle-biter.

And I'm waiting for my ST-implies-SM-gauge-group paragraph.

-drl

52. **Lubos Motl**
December 9, 2004

Dear Michael Nielsen,

I think that you are confusing science and politics. Pro/con discussions don't necessarily advance science in any way. Sorry, but I think that it is just a wrong approach from you if you rely on the pro/con discussions.

The approach of a scientist is to find the relevant facts and data, and make his own conclusions using the brain. It's certainly impossible to do so if you rely on low-brow discussions between the people like Peter Woit who like to talk about string theory, but who have almost no idea what it is.

All the best
Lubos

53. **Lubos Motl**
December 9, 2004

Dear "",

you're choosing just the kindergarden mathematical toy model. String theory has much more impressive results than your brain is able to comprehend, and therefore it's completely meaningless to discuss with you about these larger achievements.

And yes, Cumrun Vafa's discoveries are certainly and always based on string theory.

Best
Lubos

54. **Thomas Larsson**
December 9, 2004

Peter's comment reminds me of that scene in Monty Python's "Life of Brian" where they ask
"What did the Romans ever do for us?"
Yeah, what? Apart from…

String theory undeniably brought a lot of popularity to the anthropic principle. This achievement makes at least some people happy. Though the same people are probably not overly fond of Lawrence Krauss, who has dared to criticize creationism despite being a layperson on Christian science.

55. **Peter**
December 9, 2004

Hi Michael,

Thanks for your support!

Peter

56. **Peter**
December 9, 2004

Hi Richard,

Actually I've posted several things of the sort you suggest recently. Besides the comments on Khovanov homology (by the way I met Khovanov for the first time this week, he was here at Columbia giving a series of talks), I also wrote about the Baum-Connes conjecture and Witten localization. I certainly intend to do more of this and to when possible make more explicit the links I see between these kinds of mathematics and basic questions about quantum field theory.

But you'll also notice that virtually no one comments on those posts, and part of the reason for this I understand quite well, and it has to do with why I think it's worth my time to complain about what string theory is doing to physics. It takes a non-trivial amount of time and effort to absorb new mathematical ideas and by so dominating the mathematical end of particle theory for twenty years, string theory has monopolized the time of the mathematically sophisticated members of the community. It has also quite literally driven out of the field a lot of people who were interested in other sorts of ideas about how to apply mathematics to questions in particle theory.

The mathematically interesting aspects of string theory form an incredibly difficult and complex subject. Mastering this area at all is a full-time job and if you do that you're not likely to have the time to learn other things (be they Khovanov homology, Baum-Connes, Witten localization or whatever). After many years I've become convinced that as long as string theory so thoroughly dominates the field, it will remain very difficult to get anyone to do the hard work and take the time necessary to learn other more promising subjects.

So, I've decided to spend my time not only pursuing the ideas about mathematics and its relation to quantum field theory that seem promising to me, but also pointing out the "colossal failure" of the string theory project. If particle theorists acknowledge what has happened to their field, they may finally be able to move on to something more promising. More concretely, as long as smart young theorists who want to work on the interface of math and physics find that they can get a job if they work on something related to string theory, and can't if they work on something not related to string theory, the field is going to continue to stagnate.

Peter

57. **Michael Nielsen**
December 9, 2004

In reply to Richard:

As a physicist who works in a field completely unrelated to string theory, I'm very interested to hear discussions (both pro and con) of the worth of doing string theory.

In my opionion, people like Peter Woit, Urs Schreiber and Co are doing a useful service to the physics community by carrying on this sort of public debate in a spirited but relatively civil manner.

58. **Richard**
December 8, 2004

If you think string theory is crap – don't tell us about it. Explain what you think is important in your area. What recent progress has there been in non-perturbative QCD? (quite a bit in lattice QCD, but I don't think that's what you reccomend everyone spend their time on) I found your article on Khovanov homology to be really interesting. Why don't you focus your writing on similar uplifting topics?

Just a thought
– Richard

59.
December 8, 2004

Lubos, are you saying that there is
specifically stringy reasoning involved
in things like TQFT, Donaldson/Seiberg/Witten
theory, Gromov-Witten invariants and other
much-admired offshoots of string theory?
i.e. where you think in terms of the full
string theory, get some idea/result and
degenerate back to the purely topological or conformal theory?
We can all admire Vafa et al, but it's not
clear to me how much (if at all) the spectacular
successes in mathematical toy models have to
do with strings per se.

To put it another way, suppose topological
M-theory is built tomorrow as a grand unified
theory of the "interesting to mathematicians
aspects of string theory". Would string theory
have anything further to say to mathematics
or would we (mathematicians) only have to
know about the topM-theory?

All the best,

60. **D R Lunsford**
December 8, 2004

Dear Bon-Bon,

Where exactly is this prediction? Just copy down the paragraph in which is appears.

-drl

61. **Lubos Motl**

December 8, 2004

Dear “”,

string theory has nothing to say about physics for those who don’t want to listen or those who can’t listen.

Incidentally, the prediction of realistic string models for the low energy gauge group is SU(3) x SU(2) x U(1).

As explained on my blog, the comparison of the rivalry between USA:Cuba could be more realistic than Microsoft:Apple.

All the best
Lubos

62. **Steve Esser**
December 8, 2004

I was entertained by the article’s bit on comparing string/LQG to Microsoft/Apple.

63.
December 8, 2004

Dear Prof. Bully from Harvard,

Come up with a definitive,*unique* prediction of the standard model gauge group and fermion content or parameters (promised by one of the string leaders in the mid-80s), then I will believe your sermons on string theory. Till then, string theory has absolutely nothing to say about physics. Mathematical beauty (some of it debatable) is not reality; otherwise the world would be superconformal.

The so-called superstring phenomenology, such as the brane models, is a joke that may fool a few people all the time. Most have a brain, you know, and are not sheep uncritically following whatever a shill may say on the subject.

The fact is that string theory ideas are being featured alongside religion in popular media as an explanation of the universe. Enough said.

64. **D R Lunsford**
December 8, 2004

Why our own pResident has given us the perfect description of ST:

“Catastrophic success”.

Logorrheically,

-drl

65. **Lubos Motl**
December 7, 2004

Dear “”,

thank you, even though there is only “” to thank for. The only thing I can say about your “” is “”. Zero. Ero. Ro. O. 😉

Lubos

66.
December 7, 2004

Lubos, your argument is deeply uninteresting and essentially superficial.

Or is it deeply essential and superficially uninteresting?

67. **Lubos Motl**
December 7, 2004

On my blog (Unity of strings) it’s argued that Peter’s separation of string theory to “success” and “colossal failure” is inconsistent.

68. **Lubos Motl**
December 7, 2004

Let me be more specific why all of these effects of string theory on strongly coupled gauge theories and on mathematics ARE derived from string theory as a theory of quantum gravity including gauge theories with fermions etc. – such as the Standard Model:

1. The strongly coupled limit of the gauge theory, according to the AdS/CFT correspondence, IS a theory of quantum gravity. For example, the N=4 d=4 super Yang-Mills is equivalent to type IIB on AdS5 x S5. In early 1998, you could have said that it is just type IIB supergravity, except that today we know a plenty of ways how you can see that the gauge theory is dual to the whole of string theory, including the excited strings. The strings are dual to operators like Tr(ABCDNBBBBCSB) where the letters are fields in the adjoint representation.

The same statement holds for the strongly coupled dual of any other theories that are studied, and if you ask sufficiently deep questions, you’re guaranteed to need the whole string theory. The quantum gravitational phenomena that you can see from the conformal field theory include topology change inside asymptotic AdS spaces, see Maldacena et al. recent paper, as well as black hole thermodynamics and other things typical for “quantum gravity”.

2. Mirror symmetry is a relation between two Calabi-Yaus that look geometrically different, but give you the same physics if you compactify string theory on them. By “physics” I mean that they will predict identical results for scattering of gravitons and Standard-Model-like quanta if you use the Calabi-Yaus as compactified manifolds. The Calabi-Yau compactifications are exactly the same compactifications that were used for 10+ years as the unique way to derive the real Universe – the Standard Model (or SUSY GUT) plus quantum gravity – from string theory.

If you want to study some mathematical questions about the geometry of Calabi-Yaus only, you truncate the full string theory onto topological string theory. It still has a lot of stuff, but it's clearly just a truncation, and all string theorists that work on topological string theory realize that there is a lot of stuff in string theory outside topological strings.

All these dual pictures to tasks in mathematics and gauge theories are very specific configurations in the full string/M-theory, and it is absolutely impossible to understand their physics and mathematics correctly without knowing that there is a consistent theory of quantum gravity that admits 10- or 11-dimensional vacua but that also predicts gauge-theory-like matter in its various compactifications.

69. **Peter**
December 7, 2004

Hi Lubos,

I don't think I've made an error in logic. Thinking about 10 or 11 d strings/M-theory may lead one to interesting things (in math or gauge theory) even though your original motivation turns out to be a wrong idea.

Actually, I think string theory has been such a success as math precisely because it has failed so badly in its original motivation. If in late 84-early 85 people had found some Calabi-Yau and some version of string theory on it that allowed the calculation of the parameters of the standard model, mathematically the whole field of string theory might have lead to a lot of information about one Calabi-Yau, and not much else.

Because string theory doesn't work as intended, string theorists have spent 20 years thinking about a wide array of mathematically very complex and rich structures. Coming at these structures from a very different perspective than the traditional mathematical one has lead to a lot of interesting new mathematics. The colossal failure of the string theory unification project as physics has ended up benefiting mathematics quite a bit.

70. **Urs**
December 7, 2004

Peter's comment reminds me of that scene in Monty Python's "Life of Brian" where they ask

"What did the Romans ever do for us?"

Yeah, what? Apart from…

71. **Lubos Motl**
December 7, 2004

Dear Peter,

if you say that 1,2 are OK and 3 is wrong, then you must be doing an extremely trivial error with your brain, and I hope that you will be able to fix it. All the successes in 1,2 do follow from specific refinements (or truncations) of the vacua in 3 that you deny.

Best
Lubos

72. **Peter**
December 7, 2004

I suppose I should write a macro so that whenever I write anything about the disastrous effect of string theory on particle physics it includes the disclaimer:

1. No, I'm not talking about the effect of string theory on mathematics, which, on the whole has been very positive.

2. No, I'm not talking about the idea of using string theory to get information about strongly coupled gauge theories, which has had some real successes.

3. Yes, I am talking about the idea that there is some fundamental 10 or 11 dimensional supersymmetric theory of extended objects which explains both quantum gravity and the standard model.

It's clearly point 3 that Krauss was referring to as a "colossal failure" and anyone who has read more than a few postings on this weblog would be well aware of points 1,2,and 3. The NYT article was not about whether string theory was successful as mathematics or whether it was promising as a way to solve QCD. It was very explicitly about the status of string theory as a unified theory and that is the issue to which my posting was addressed.

The view, prevalent among more than a few mathematicians, that since important new ideas about algebraic geometry and other parts of mathematics have come from work motivated by string theory, there must be something to the idea of string theory based unification of gravity and particle physics, seems to me to be deeply superficial and essentially uninteresting.

73. **anon_mathematician**
December 7, 2004

As a working mathematician, I want to reiterate a point that has probably been made here many times—-string theory is a fantastically vibrant and deep mathematical enteprise, radically changing and increasing our understanding of basic mathematical objects (notably curves and higher dimensional algebraic geometry, but also impacting on representation theory, the social acceptibility of homotopy theory, and the relationship of hard analysis to understanding new algebraic structure—analysis is less useful these days, though that will eventually change).

These insights come from a completely different intellectual tradition (often denigrated as "physical intuition").

It may be that string theory has no relation to the physical world; it clearly isnt testable yet. But mocking the whole enteprise is deeply superficial, and essentially uninteresting.

And a field which has so greatly changed the way we understand fundamental mathematical objects (some studied for centuries) probably has something to say about the observable world too…

74. **Aaron**
December 7, 2004

It's referring to a recent paper that claims that BF theory shows up in a conjectured thing called "topological M-theory". BF theory shows up in relation to LQG, but i think it’s more than a bit of a stretch to say that LQG (which is a much wider construct than just BF theory) is part of string theory.

The BF part of LQG isn’t the controversial part. It’s the quantization procedure that a lot of people have trouble with.

75. December 7, 2004

Interesting to read that now string theory considers
LQG also to be a part of string theory. From the screed of in Wikipedia and elsewhere, it seemed that string theorits thought that LQG was complete garbage. What is going on??

76. December 7, 2004

Peter,

Don’t you realize that whatever meaningful you and other naysayers will write, or will think about can all be explained by a speck in the vast rich holographic, hetorotic, D-brane landscape of conformally invariant matrix theoretic, dual of the compactified intersection of the F and M theories, upto a trivial combined S-U-T-duality that is unimportant for galactotrophic effects?

77. **Lubos Motl**
December 7, 2004

Hi Peter,

i don’t think that the comment about “colossal failure” by L. Krauss – who has otherwise nothing to say – is a punch line of the article that one can use to create a meaningful review of the article.

You’re confused if you’re combining the statement about the “colossal failure” with thinking of the string theorists. Of course that this is not how a physicist who continues to work in the field may be thinking. We’re just trying the best ideas we have, and this is why we still work on string theory.

I agree with Brian that it would be great to know that string theory is a wrong theory of Nature if it were so. However I don’t think that this would mean that everyone would “move on”. String theory would still remain an important mathematical structure that would be investigated.

I wrote an entry on my blog about this article, too.

Best
Lubos

Nobel Lectures

December 10, 2004
Categories: Uncategorized

The winners of this year's Nobel Prize in Physics gave their Nobel lectures in Stockholm on Wednesday. The lectures of David Gross and Frank Wilczek are available on-line, for some reason that of David Politzer isn't, at least not yet.

Over at Sean Carroll's Preposterous Universe there's a first-hand report about the lectures from Thomas Larsson (who often comments here). It's in the comment section of this post. One interesting detail from Politzer's talk was that Coleman had originally assigned the beta-function calculation to Erick Weinberg (who is now my colleague at Columbia over in the physics department), but Erick already had enough material for his thesis and wanted to move on.

Update: Politzer's lecture is now available at his web-site.

Comments

1. **plato**
December 14, 2004

   Just thinking out loud.

   This is a nice picture taken from ISCAP

   Try refreshing the ISCAP page about five different times and you find some interesting pictures to look at, as well.

   Something triggered, in what DRL last posted(10:03).

   Here is a quote from Brian Greene in response to that last post.

   How can a speck of a universe be physically identical to the great expanse we view in the heavens above? **Brian Greene**

   Maybe a topological change?:)

   I wonder sometimes, about "events" even within the context of the expansive universe.

2. **D R Lunsford**
December 13, 2004

   NGC 7603 is a strange object:

   http://perso.wanadoo.fr/lempel/red_shift_NGC_7603_uk.htm

   It consists of an interacting Seyfert galaxy and three odd objects of widely discordant redshifts. Some of these peculiar galaxies are as weird and mysterious as UFO photos. (Spend an hour or two with the "Atlas of Peculiar Galaxies" for a trippy experience.)

   A recent paper is a real blockbuster:

   http://arxiv.org/abs/astro-ph/0203466

   -drl

3. **D R Lunsford**
December 13, 2004

   (nor do I in any way deny his eightfold achievement)

   -drl

4. **D R Lunsford**
December 13, 2004

   Yes Chris, I was having a bilious moment. Sorry.

   Still, I never forgot the way he insulted his hosts.

   -drl

5. **Chris Oakley**
December 13, 2004

   Danny –

   I cannot possibly agree with your comments about Gell-Mann. In my opinion, the 1969 Nobel prize is the only particle physics Nobel prize since the war that has been deserved. Gell-Mann has this remarkable ability to find simplicity amid the confusion and complexity of experimental data. At a time when the bulk of theoretical activity is devoted to doing the exact opposite, his contribution ought to be appreciated that much more.

6. **ksh95**
December 13, 2004

   Why is NGC 7603 the most interesting object in the sky?

Which seems more likely to you?

1.)That one little speck in the sky is an optical illusion, and NGC 7603 is actually composed of several distinct objects, at different distances, along the same line of sight.

-or-

2.)That one little speck in the sky is exactly what it looks like. NGC 7603 is actually one object, and volumes of data supporting the big bang are are wrong.

7. **plato**
December 13, 2004

DRL,

Of course Gell-Mann, the very prototype of the quick-witted, stone-hearted, insightless boor, referred to "half-asstrophyisicists" – and now the army of mediocrities streaming into the academic world from PhD programs have usurped even those jobs.

It's a sick world.

Why don't you tell Brain Greene that you think this holds no value?:)

8. **D R Lunsford**
December 13, 2004

DMS,

The Motloids of this planet also deliberately inhibit science by denying telescope time to "controversial" researchers, and even by blindly refusing to look at things like NGC 7603 (IMO the most interesting object in the sky). Of course Gell-Mann, the very prototype of the quick-witted, stone-hearted, insightless boor, referred to "half-asstrophyisicists" – and now the army of mediocrities streaming into the academic world from PhD programs have usurped even those jobs.

It's a sick world.

-drl

9. **DMS**
December 13, 2004

Actually, Einstein's example is rather interesting.

Sir Edmund Whittaker in his detailed survey, A History of the Theories of Aether and Electricity, Volume II, included a chapter entitled "The Relativity Theory of Poincare and Lorentz", a clear attempt to deny any credit to Einstein. When Einstein was asked about this, he said something to the effect that he did not really care about the "priority" or "recognition", and that he knew what he had done and was pleased about it. It also seems to be the case that the Michelson-Morley experiment played no role in Einstein's developing SR.

He was perhaps an exception; most scientists today would jealously fight for priority over any little thing they did (boost up the citation numbers). So not only was Einstein a great physicist, he was a remarkably wise and modest human being as well (ok, he was not a perfect husband, and there are the anti-Einstein "writers", debunked by Einstein scholars http://physicsweb.org/articles/review/16/4/2) , a rarity at any time and place.

But, of course, nowadays, we have string theorists who are to be compared in genius directly to Newton.

10. **Lubos Motl**
December 13, 2004

I can give you Einstein's quotes that are saying the same thing as me. But I am afraid that it is useless to make any discussions with you, DRL.

11. **plato**
December 13, 2004

DRL,

The comments Lubos is making about the quest to understand NATURE are valid. I understand, at the "heart of the matters," there are **these qualities** students have about their professors?:)

Maybe we could call this bloggery, a psychological drama, where Mutt and Jeff are just the "innate," one sided feature of a coin on Monday, and another, on Tuesday?

Is it not known, all do not fair thee so well in times?:)

12. **D R Lunsford**
December 12, 2004

Rick,

Motl is that type of scientist that Einstein specifically despised – the quick mind without insight, blind and hungry for power and fame. He is NOT representative of physicists – he's representative of Bon-Motl.

Of course, our society turns out these blustering faux-macho clueless dorks by the tens of thousands.

-drl

13. **Lubos Motl**
December 12, 2004

Rick, it's not just personal glory, money, and fame – it's the understanding of Nature itself. Many physicists are also doing physics as their job – it's a form of career, and be sure that it is better for them to have interesting result.

Whatever the motivation is, it is absolutely clear that a physicist *wants* to find interesting results, and if (s)he does not want, (s)he should not be a physicist!

Of course that most of us are happy if someone else finds something interesting, but we would be even happier if we found it ourselves. I don't believe that either of your friends is happier if someone else finds something out – if they are saying it, then – I apologize – your friends are liars.

14. **Rick**
December 12, 2004

Prof. Motl, you have such an average sense of values. Your comment is depicting physicists as if all of them are chasing after personal glory. Maybe you're right, in the average. There are people who just enjoys doing physics, and happy to know even if it's their friends who discovered something. Your comment is such a disgrace to them. Maybe I am not practical when concerned with monew issues. But I don't want to sell my sould to fame or money.

15. **Lubos Motl**
December 12, 2004

Come on, Robert, it's ridiculous. Every theoretical physicist would love to be the discoverer of the asymptotic freedom or any other important insight, for that matter.

It is not just about the 400,000 dollars that Erick Weinberg rejected. It is about the fun of learning important secrets of Ms. Nature as the first one. Learning Nature is the real fun. It's a very special feeling, and no doubt, the physicists are doing physics because they want to find something interesting.

If a physicist pretended that he or she did not want to be the father or mother of QCD, I would not believe it anyway. It would be such an incredible hypocricy!

16. **Robert**
December 12, 2004

Prior to this, I have never felt that I could, in any way, be professionally competent to comment on the internecine abuse dispensed between the pro and anti string camps manifest on this blog. Professor Motl's most recent remark, about Politzer, Weinberg, the beta function , and who got to check it out, highlights an atrophy of the spirit that shows why theoretical physics is chasing itself up its own fundament.

17. **Lubos Motl**
December 10, 2004

Haha, the decision of Erick Weinberg was not one of the smarter ones. 😉

Landskepticism

December 13, 2004
Categories: Uncategorized

Tom Banks has a new preprint out, entitled Landskepticism: or Why Effective Potentials Don't Count String Models. In it he argues against the idea that one can use effective potentials to study the supposed "Landscape" of different vacuum states of superstring theory. His preprint, like most of the literature in the field, is kind of a bizarre document which doesn't even look like a conventional theoretical physics paper. In the course of twenty pages he only really manages to write down one equation (and it's just the Schrodinger equation).

One of his claims is that it doesn't make any sense to think of what is going on as one string theory Hamiltonian with a huge number of possible vacuum states. Instead one has to think of a huge number of possible string theory Hamiltonians, one for each asymptotic background. So I guess that's it for the "uniqueness of string theory".

He gets kind of vehement: "the concept of an effective potential on moduli space as a tool for finding string models of gravity, is a snare and a delusion, fostered by wishful thinking, and without regard to the actual evidence in front of us." Sounds kind of like things I say… He footnotes this "Perhaps some over the top rhetoric is in order".

On a different topic, he claims that the Weinberg "prediction" of the cosmological constant doesn't hold water, since if you allow both the cosmological constant and other parameters to vary, then typical values of the cosmological constant allowing galaxy formation will be orders of magnitude larger than the observed value.

I shouldn't give the impression that Banks is opposed to string theory. Like everyone else, he doesn't even mention the possibility that it might be wrong. He has his own ideas about holography and cosmological breaking of supersymmetry, which he alludes to at the end.

His paper is based on a talk he gave at a String Vacuum Workshop in Munich three weeks ago. Kind of scary to see how many theorists are now working on this nonsense. At first I was worried to see my old friend and fellow Princeton student Costas Bachas's name on the list of participants. Costas always seemed to me one of the more sensible theorists around, even if he did work a lot on string theory. Then I noticed that he wasn't giving a talk, just leading a discussion on the topic "Does the 'String Vacuum Project' make sense?" Wonder what their conclusion was.

Update: For Lubos Motl's take on this paper (Banks was his advisor), and the news that Nima Arkani-Hamed has gone over to the dark side, go here.

Comments

1. **D R Lunsford**
   December 15, 2004

   It should be sternly pointed out that astrophysicists are even worse – right this minute the HST should be collecting spectra from objects like NGC 7603, but you can't even get a post in edgewise on sci.astro.research about it. Like Pope Paul V and his hatstand minions, they refuse to look through the telescope at something that will require some new thought. This is directly connected, at least culturally, to the ST crackup IMO.

   -drl

2. **Chris Oakley**
   December 15, 2004

   Peter –

   Right on.

   (I was going to make the same comment, but you said it so much better).

3. **Peter**
   December 15, 2004

   There's nothing wrong with some people working on some aspects of string theory that are interesting. The fundamental problem is the overhyped, aggressive and not very honest way string theory research has been pursued and promoted, driving out other ideas.

   On the other hand, there really is something very wrong with the whole Landscape business and I don't think any legitimate scientist should be working in this area. Writing rambling papers about multiple universes with no solid basis and no plausible hope of ever predicting anything puts one deep into crackpot territory. It's a complete disgrace that leading figures in the field of particle theory are doing this. Whatever you want to call what they are doing, it is not any sort of legitimate scientific activity. If you really believe that the idea of string theory leads one inexorably to the Landscape picture, as a scientist one has to admit that this means that string theory really has failed. People are so desperate to avoid facing up to this failure that they are willing to throw out all the standard norms of what it means to do science. This is a complete disgrace.

4. **Thomas Larsson**
   December 15, 2004

   Arun, we know that 20,000 man-years of string theory work by some of the smartest people on this planet only resulted in anthropic nonsense (and some cool but physically irrelevant math). Unless you are a good deal smarter than Witten, why should you succeed where he failed?

5. **Arun**
   December 15, 2004

   Steve M writes that something good "has much more chance of happening if people dont all go down the landscape route, and start writing waffling philosophical and "statistical" papers all the time with one or two equations, and accepting that nothing can ever be calculated."

   I think Peter W. is saying something similar – that something good has much more chance of happening if people dont all go down the string theory route….

   Sometimes I get the impression he wants to shut down the whole string enterprise, but that I would not agree with. But there is some happy medium between being a crackpot and bowing to every current fashion.

6. **Steve M**

December 15, 2004

At least Banks believes the whole concept of the Landscape is not well established. Until there is a much firmer and deeper nonperturbative understanding and grasp of string theory one cannot really talk seriously about "string cosmology". Some very powerful underlying selection mechanism may yet emerge in string theory that yields a monovacuum theory. Perhaps this is (very) wishful thinking but has much more chance of happening if people dont all go down the landscape route, and start writing waffling philosophical and "statistical" papers all the time with one or two equations, and accepting that nothing can ever be calculated. I think string theory (and string theorists) can still do better than that. Also I dont think that sort of stuff will attract to the field or inspire the talented young people going into graduate school either.

It is frustrating for everyone in physics and cosmology I think, not just string theorists, since these are very deep and very hard problems conceptually and technically. I also think too much importance and significance has been attached to the whole recent KKLT construction though. This is admittedly a very clever albeit Rube Goldbergdish piece of mathematical technology after all. So much very fine, artificial and careful tuning involving flux compactifications, turning on fluxes, anti D3 branes to break susy and lift ads to metastable ds and all that. (phew!:) There could still be some general underlying flaw in it that could kill off the landscape idea and that should be intensely considered and studied. There could even yet be other ways of evading the desitter no-go compactification theorems. If landscape doctrine takes over then the field will probably end up as little more than philosophical and metaphysical debate. Essential names like Witten, Gross, Maldacena etc will probably drift off into other things mathematically and you wont see them at "String Vacuum Workshops"…the sort of guys the field still really needs probably more than ever.

7. **D**
December 14, 2004

Schmeltzer's crank page was hilariously applied to Bank's paper ipse, to wit,

No equations
No apparatus
Old theory (Democritus) wrong for wrong reasons
Captial letters (oh wait, that was a slideshow)

8. **Peter**
December 14, 2004

I've corresponded with 't Hooft a bit about string theory and he seemed rather skeptical about many of the claims of string theorists, but interested in learning more about the theory. Here's an analogy about string theory he gives in his popular book about the standard model:

"Imagine that I give you a chair, while explaining that the legs are still missing, and the the seat, back and armrest will perhaps be delivered soon: whatever I did give you, can I still call it a chair?"

He refers to string theory as not really a "model" or a "theory", but a "hunch".

9. **Peter**
December 14, 2004

Dear Quantoken,

Please stop repeatedly posting about your theory on my weblog. That's seven postings today alone, none specifically about the Banks paper that is at issue.

Peter

10. **Quantoken**
December 14, 2004

What is gravity?

Both Super String Theory and the Loop Quantum Gravity claimed they have derived gravity in their theory.

Gravity is reflected in the constant big G.
The G can not be zero, for then there is no gravity. The G has to be its current know value to provide the currently known gravity.

"Having derived gravity" means in the final equations they derive, must contain a G. How did they get an equation containing G? If you start with a calcuolation process and secretly inject the G into your equation at some point, then certain you end up with a set of equations that contain G. That's really not a derivation of gravity, because the G you end up with is the same G you initially inject arbitrarily. If your injected G happen to zero, you end up with nothing.

I can not accept the claim that Super String Theory or Loop Quantum Gravity derived the gravity, because they secretly injected the G in the first place, by using the Planck Scale, which already contains a G.

My GUITAR theory is the only known theory that truely derived gravity. Because using first principle, I obtained the numerical value of G, from alpha. I did NOT secretly inject G like the establishment camp do.

Shame on the establishment camp that they first secretly injected G and then claim they have derived gravity in their theory.

The correct theory is, in natural unit set:

G = 1/(2N),
N = PI*exp(2/(3*alpha))
alpha = fine structure constant = 1/137.03599911

Quantoken

11. **R. Giese**
December 14, 2004

Was Gerard 't Hooft ever a critic of string theory? On his website it indicates that he gives lectures on the topic.

12. **Quantoken**
December 14, 2004

To move physics forward one has to ask some of the most profound questions in physics. One has got to question those things that too many people take for as granted, without a second thought. It's just the same as Newton asking "why Apples fall to the ground." Or Einstein asking "What exactly does it mean the two clocks have the same time." Laymen took these things for granted and never give them a second thought. But if you do you find there is physics there.

One question I have been asking for a long time is, WHAT IS MASS EXACTLY?

We know there's inertia mass and there is gravity mass. The general relativity's equivalence principle tells us the two masses are equivalent. They are equal at all times. But what I found is even that is not right. There are never two different masses. There is neither inertia mass, nor gravity mass. There is only a geometric mass, which is defined by spacetime curvature. Geometric mass is the only mass that actually exists.

How come? First let's look at what defines inertia mass. What it says is if something has mass it has inertia. It keeps moving with the same speed until it is pushed by a force. When it is pushed by a force, depending on how much its mass is, it has an acceleration inverse proportional to its mass. That's Newton's second law. Inertia mass is determined by force and acceleration.

In that sense, Newton's Second Law is actually not a physics law, it is just a logic definition defining what is inertia mass, using the concept of force and acceleration. M = F/a, by definition.

So then we have to ask what is FORCE any way. Force is the kind of effect that makes an object accelerate. If we see something accelerating, we say there is a force acting on it, if the object is not accelerating, then we say there is no force.

So force is defined by inertia mass and acceleration. We are back to square one. mass is define by what is force. And force is defined by mass. So exactly what are they.

Certainly you can say force is gradiant of potential energy. But that does not resolve the problem. I will ask you what is energy then. Energy is just mass according to Einstein's E=MC^2. So again you are defining force using mass. We are still back to square one in trying to come up with a definition of fundamentally what is mass.

The conconsion is inertia mass really do not exist, unless other physics properties associated with mas exists.

The situation is only slightly better when we look at gravitational mass. The gravitational mass is the property that causes things around it to accelerate gravitationally. So this is probably the only way out finding an exact definition what exactly is mass.

According to GR, the gravitational field is equivalent to an acceleration field. So gravity is really geometry effect of spacetime curvature.

Therefore, I think the mass is really geometric mass. Mass does not cause spacetime curvature. The mass IS the spacetime curvature itself. The mass is a geometric property of the spacetime.

This is why the Bekenstein Bound is important. In Bekenstein Bound, it provides a relationship between entropy, the mass/energy, and a geometric length parameter. If I can attribute mass to geometric mass, and make it purely a spacetime property, then, the Bekenstein Bound tells us the relationship between spacetime and the entropy.

The Bekenstein Bound provides us a hint how spacetime can be constructed purely from quantum information, which is neat.

That's one of the fundational principle of my GUITAR theory. Quantum information is the only real physical existence. Spacetime is constructed from quantum information, and mass/energy is merely a geometric property of spacetime.

And everything can be boiled down to just a few dimentionless fundamental constants, especially important is the fine structure constant alpha.

I have shown that using just alpha, and nothing else, I can calculate the radius and mass of the universe, the CMB temperature, and I can even calculate the exact mass of protons, among other things.

Quantoken

13.
December 14, 2004

"Equations of string theory"

Write down the equations of string theory.

I sumbit that a real set of equations is always needed to describe what is really happening. If you can't describe what is happening, you don't really have any equations.

Here, just for contrast, are some equations:

Rmn = (2R/w) (FmaFan + 1/4 FabFab gmn) – (1/2W) {Dm,Dn} W

1/g d/dxm g R F nm = 5/4 Dn W

OK there are some equations. Show me the string theory equations.

-drl

-drl

14. **Quantoken**
December 14, 2004

I totally agree with Aaron. Physics should ONLY study the things in this universe that we can observe. A criteria separating real science and crack science is that real science makes predictions that can be verified by observation data to be either right or wrong. If you can not make predictions or your prediction can not possibly be checked in anyway, then it's crack science.

All those speculations about multiple universes and possible universes with alternative physics laws, are crackpot science. They say all kinds of different things about how the other universes could be like so they do make a bunch of "predictions". However, all their predictions are totally irrelevant, because the "other universes" does not belong to this world. So we have no direct or indirect way of observing any of them, and exam any of the predictions. Anything that can not possibly be checked or be otherwise falsifiable, are crackpot science.

Therefore all the talks of the Super String Landscapes, are crackpot science. You can talk all your talk and say all you want to say, but at the end of day, really none of the landscapes exists because none of them is verifiable, in this world. They are neither true nor false. They can't not checked for truthfulness and they are not science.

Any talk of an alternative universe is nonsense and none-science. We should talk only about this universe. That's all we ever know and to us that's all the existences. Nothing else exists outside this universe.

Quantoken

15. **Aaron**
December 14, 2004

The problem is worse than that. It simply doesn't make any sense to talk about the probability of us being in some universe or another. We are in a particular universe, so we have a probability of one of being in that universe. Counting all the vacua you want won't change that fact. If someone out there is sitting in a 'rare' universe, and they try to make 'predictions' based on vacuum counting, they'll be wrong.

And how do we know we're not that person?

16. **one mathematician's opinion**
December 14, 2004

re: Aaron's comment on "predictions"
from statistics of vacua (being worse
than landscape)…

Virtually any specific mathematical
object that one can write down,
such as a solution of equations
of string theory, will tend to be
either "very special" or
"very generic". Things in
between are rare. The trouble
with using statistics of the generic
case for prediction is that it's also
rare to know which case one is in.

17. **D R Lunsford**
December 14, 2004

I'm waiting for the 12-string SUX theory (supersymmetric X).

-neil jung

18. **Quantoken**
December 14, 2004

OK, I checked against all your links. Out of three URLs you provided, you mis-spelled two of them and only one worked straight. So you have a pretty good crackpot score already, Mr. M-?

Baez's index system is reasonable in most part, although I do not agree with them all. It's probably discriminative to none-English speakers when he said mis-spelling the name of Einstein etc was worth 5 points each.

I think I am scoring pretty poorly on the Baez index. The BigBang would score much higher. With tens of thousands of people working on BigBang, it failed to have made any verifiable predictions so far and each time some new observation data comes up that goes against the BigBang, they need to patch it up with something else.

Has BigBang been able to calculate a correct CMB temperature, for example? No. But I arrived at the exact correct CMB temperature. The remaining discrepancy between my calculation result and the observed data is well within the experimental error of the observational data. So here is one prediction I make here:

As experimental techniques improve and the experimental error is further reduced, the more accurate CMB temperature measurement in the future will approach my calculation result!

Here is another prediction:
The CMB temperature will remain unchanged thousands or billions of years from today. According to BigBang, CMB will be cooled down over time. According to my theory, that will never happen.

Here is one more prediction:
Physicists eventually will find BigBang as well as SuperStringTheory all wrong, eventually.

Can you say I am not making predictions?

The only thing I am scoring badly is the publishing factor. I certainly have not published anything on any journal. Not even ARXIV. They now use an endorse system and unless you are already in the establishment camp and agree with everything in the main stream, you can't get an endorsement and can't publish anything on ARXIV. Also they require you to use LATEX format and you have to write up exactly in the style they require. I do not have the time to learn LATEX and formatting my document the way they want. Remember, I do NOT make a living out of this. This is just my spare time entertainment.

My GUITAR theory does not compradict quantum mechanics or gravity laws in any way or form. Those are well established theory with solid experimental data backing. On the other hand, BigBang and SuperStringTheory are NOT well established theory. SuperString has no experiment backing at all. BigBang needs to have so many adjustable parameter to fit itself against experimental data.

My GUITAR theory has only one adjustable parameter, the fine structure constant, which is really not adjustable at all since it has been fixed precisely by experimental measurements. So, I have absolutely NO adjustable parameter but yet I came up with so many correct calculation results that match experimental data exactly.

Quantoken

19. **Quantoken**
December 14, 2004

You said:
"Classic stuff indeed:) Blog readers with nothing better to do can go here and compute the Baez Index of this theory."

Nonsense. My GUITAR theory hasn't even been known by many people yet. My 3 posts at PhysicsForums.com were the first attempt in trying to discolse my theory and before I had a chance to utter the first word describing it they banned me and said nothing. It's less than 2 months since then and I am definitely sure Baez haven't heard about my stuff and I have no connection with him. How could he have a page regarding my theory?

But I will check out your links anyway.

Quantoken

20.
December 14, 2004

Link should be
http://www.ilja-schmelzer.de/ether/crank.html

21.
December 14, 2004

Classic stuff indeed:) Blog readers with nothing better to do can go here and compute the Baez Index of this theory.

http://math.ucr.edu/home/crackpot.html

And check out a general overview of the theory here

http://www.ilja-schelzer.de/ether/crank.html

22. **Quantoken**
December 14, 2004

Thanks, that was a good joke. Indeed you can build a GUITAR when you go beyond just the strings. The establishment camp finds there are more than 10^122 different strings but they still haven't figure out how to built a guitar with that many strings 🙂 What is the use of strings if you can't built something useful out of them?

I do not just one day had a good dream and come up with this GUITAR (Generalized Universal Information Theory And Relativity) theory. The fact is, every little pieces of this GUITAR are based solidly on previous research by well respected physicists. All I did is connecting the dots and put the puzzle together into one piece.

The fundations of GUITAR theory ranges from Eddington's postulation that star radiations heat the space to a uniform temperature of 3K, to the Mach's principle that consider the gravity to be the result of collective interactions of all masses in the whole universe, to Einstein's famous notion that "God does not toss a dice.". To Paul Dirac's Large Number postulation. To the Hawking Blackhole Entropy Bound, to Bekenstein Bound. To the holographic principle.

None of those are invented by me. But I am the only one who successfully linked all dots together, arrive at a self-consistent picture which agrees with all observation facts, and directly leads to precise and unambiguous calculation results that have been accurately verified by experimental datas.

I obtained the exact observational radius and observational age of the universe. The precise baryon density value of the universe. The exact CMB temperature which agrees with observational data exactly, and the exact solar radiation constant of 1360 W/M^2. I derived the precise mass of protons based on first principle. I linked the value of G directly with the size of the universe, and everything is linked to the fine structure constant alpha = 1/137.03599911. It's a tremendous amount of success and its amazing one simple theory can come up with such a rich collection of results in so many different places!

Quantoken

23. **Joker**
December 14, 2004

Quantoken–> after the strings indeed comes the guitar; seems you're on the right track 😉

24. **Quantoken**
December 14, 2004

JPC:

You are absolutely right in saying "One good place to start is to insist that new theoretical approac hes be finite, background independent, and inherently quantum from the start, as it appears nature is."

The Generalized Universal Information Theory And Relativity (GUITAR) is such a theory. It starts with the basic assumption that everything in the universe are just quantum information. The total quantum information in the universe is a finite and conserved integer quantity. Spacetime background comes as statistical manifestations of the quantum information, as are all measurement quantities.

More over, I have correctly calculated many important parameters of the universe, using very simple assumptions and methods, and they all match actual measurements precisely, including the radius of the universe, the gravity constant, the CMB temperature, the solar constant, and the exact mass of proton.

The fact that total quantum information is an integer directly leads to the quantization of the physics world. The finite-ness of quantum information leads to Heisenberg Uncertainty Principle. The conservation of quantum information leads to a limited and closed spacetime by curvature, and it naturally leads to gravity. The same conservation also leads to energy conservation and other conservation laws in physics. The very notion that spacetime background is a statistical manifestation of quantum information leads to various entropy bounds like the Bekenstein Bound, the Hawking Blackhole Entropy. Although they all need to be modified to be in the 4-D spacetime, instead of merely in the 3-D space. In the case of Bekenstein Bound, the correct form is an equality, not an upper bound.

I do not understand why physicists are so stubburn when they keep hitting the same wall when they keep getting that 120 orders of magnitude discrepancy. Do they have the ability to infer from a wrong calculation result that they must be wrong on where they start, if the mathematics of their calculation could not be wrong?

Have any one noticed many many coincidences related to alpha? For example the CMB temperature equals to the boiling temperature of water times alpha. The natural abundance of element U235 equals to the U238 abundance times alpha, etc.

Quantoken

25. **D R Lunsford**
December 14, 2004

JPC,

I was clear to real physicists that ST was sheer bunkum already in 1984 – my advisor (a world-class theorist) called it flat-out "horseshit" and he wasn't saying it jokingly (that is, he was actually offended by it). See comments by Glashow, which are similar to the host's.

Why anyone took it seriously is beyond me. Must be a bad side-effect of neoconartistry.

-drl

26. **DMS**
December 14, 2004

Which Weinberg paper is being talked about here? Does the paper "ANTHROPIC BOUND ON THE COSMOLOGICAL CONSTANT" (sadly the paper is pre-arxiv and not scanned) place bounds on the fine structure constant as well? or is that a different paper?

27. **JPC**
December 14, 2004

It's quite interesting to read all of the dramatic to and fro on this and other blogs about the state of theoretical physics these days. I was a graduate student in the Harvard physics department c. 1984 when it was quite clear that the phenomenologists and standard model guys had reached the end of their run, no new experimental data was forthcoming, and graduate students despaired of anything to do.

This was just about the time string theorists started to arrive on the scene to a hostile reception from the old guard. The motivation for studying strings back then was it looked like a way forward – although forward to what nobody could be sure about. It seems silly to retroactively assault the motives of physicists at that time who began to study the subject. People like Coleman were unsure of the basic program but thought it would be a useful exercise to investigate it.

However, at this point in time, it's clear that things have gone awry, and that course corrections need to be made in the allocation of resources to other potential ways forward. Non-tenured academia has always been a fear-based culture, susceptible to bullying, group-think and political correctness. True leaders in physics need to de-couple from the sunk cost of string theory, mathematical arcana, and prematurely self-congratulatory conferences and books and re-attach theoretical physics to the natural world, especially as new experimental data becomes available over the next decade.

One good place to start is to insist that new theoretical approac hes be finite, background independent, and inherently quantum from the start, as it appears nature is.

28. **Aaron**
December 14, 2004

Having talked to someone who has worked through Weinberg's paper, my impression is that it isn't a prediction of anything. Rather, it's a clever experimental bound on the fine structure constant. So, I agree with Banks on that subject.

Banks is far from alone with his distrust of landscaping, although most probably don't agree with the details of his particular views. Most people would rather just go on with their own work rather than argue it out on the arXiv or in the newspapers, however.

What's much worse than postulating the existence of the landscape, in my opinion, is the attempt to make 'predictions' by counting vacua. As you might guess, not everyone agrees with my opinion on this subject either.

29. **plato**
December 14, 2004

Tom Banks must have the evolution of the Curvature Parameters, I am sure:)

Now, how would you take all this, to such small distances and come out with some intact view of the geometry taking place?

You needed a microscopic view of the cosmo, which seems to be lacking for some?:)

Bubbles just encapsulate it all. Which mathematics would you need to geometrodynamicallydescribe this movement?

Help:)

30. **plato**
December 14, 2004

Peter your real instigator aren't you?

Testing the principles of the Higg's field, psychological drama manifestation, of a professor crossing the room?

If Lubos is a associative professor, then, who is the professor?:)

The effect?

The interaction of educated people, are truly fascinating to me. Climatic stories, seem awfully familiar, when taken in context of the bell curve.

Statistically, you would have a hard time of ignoring the total view.:)A Steering Boid? hahaha

31. **D R Lunsford**
December 14, 2004

Peter,

Look at this – http://online.itp.ucsb.edu/online/colloq/banks1/pdf/Banks2.pdf

It’s more stream-of-consciousness random emission of buzzwords – my adviser used to call this kind of thing “word salad”.

I looked at numbers of his papers on arxiv. They all seem to have been made by cutting and pasting standard formulas from Messiah, Dirac, and other elementary textbooks after changing the symbols around. In other words, it’s all bullshit.

-drl

-drl

Langlands Program and Physics

December 14, 2004
Categories: Langlands

One of my minor hobbies over the years has been trying to understand something about the Langlands conjectures in number theory, partly because some of the mathematics that shows up there looks like it might be somehow related to quantum field theory. A few days ago I was excited to run across a web-page for a workshop held in Princeton earlier this year on the topic of the Langlands Program and Physics. Notes from some of the lectures there are on-line.

Unfortunately, after reading through the notes, I'm afraid there's relatively little there about the potential intersection of the ideas of the Langlands Program with Physics. From the physics end of things there are some pretty illegible notes of a lecture by Witten about the Langlands Dual Group in Physics. Part of this story involves the Montonen-Olive duality of N=4 supersymmetric Yang-Mills. This duality interchanges the coupling constant with its inverse, whiile taking the gauge group G to the Langlands dual group (group with dual weight lattice). The symmetry that inverts the coupling constant is actually part of a larger SL(2, Z) symmetry.

One possible explanation for this SL(2,Z) symmetry is the conjectured existence of a six-dimensional superconformal QFT with certain properties. Witten explains more about this in his lectures at Graeme Segal's 60th birthday conference in 2002. His article from the proceedings volume, entitled "Conformal Field Theory in Four and Six Dimensions" doesn't seem to be available online, but his slides are, and they cover much the same material. There has been a seminar going on at Berkeley this past semester in which Ori Ganor has been giving talks on this topic.

While the occurence of the Langlands dual group and SL(2,Z) symmetry are suggestive, the relation of this to the full Langlands program seems to be a bit tenuous. There is however a much closer relation between 2d conformal field theory and the Langlands program, a relation which is part of the story of what is now known as "Geometric Langlands".

Some of the other lectures at the Princeton workshop give a good explanation of the standard Langlands duality conjectures, although I'm not convinced that many physicists will find them easy going. These conjectures posit a duality between two very different kinds of group representations associated to a one-dimensional field (a number field or function field of a curve over a finite field). On the one side one has an analytic object, an "automorphic representation" on a space of functions on a group G(A), where G is a group over A, the adeles of the field. On the other side one has an arithmetic object, representations of the absolute Galois group of the field in the Langlands dual group to G. Typically this duality is used to get information about arithmetic objects using the more tractable analytic objects. The most famous example of this is the Taniyama-Shimura-Weil conjecture relating the arithmetic of elliptic curves to modular forms, which Wiles (with Taylor) was able to prove enough of to use it to prove Fermat's last theorem.

In general the Langland conjectures for the case of number fields remain an open problem, but for the case of function fields of a curve, they have been proven for G=GL(n) by Drinfeld for n=2 and Lafforgue for general n (which got both of them Fields medals). The geometric Langlands program involves reformulating the function field case in such a way that it still makes sense when you replace the curve over a finite field by a curve over the field of complex numbers. This idea goes back to Drinfeld and Laumon in the 1980s, and has evolved into a specific conjecture which was recently proved by Frenkel, Gaitsgory and Vilonen.

I confess to still being pretty mystified by this subject. The analog of the arithmetic side is clear enough, it's a homomorphism of the fundamental group of the curve into the Langlands dual group, or equivalently a vector bundle with holomorphic flat connection. But I still don't understand the analog of the analytic side, which is some sort of D-module over the moduli space of bundles over the curve, broken up into "Hecke eigensheaves". My colleague Michael Thaddeus explained to me today over lunch what a "Hecke eigensheaf" is supposed to be, but there's a whole web of relations of this to representations of affine Lie algebras, CFT and vertex operator algebras that neither of us understands very well.

While I don't understand this material, I do hope to find time in the future to try and figure some of it out. Various sources that seem to explain this are the following:

Edward Frenkel's web-site at Berkeley contains a lot of interesting material. Many of his papers are on this topic, especially relevant is his Bourbaki seminar report on Vertex Algebras and Algebraic Curves.

Another relevant web-site is that of David Ben-Zvi at Texas. Look at his very informal surveys of Langlands theory written in 1995 before he gets too embarassed by the mistakes in them and takes them down. He is joint author with Frenkel of a book Vertex Algebras and Algebraic Curves.

There's an on-going seminar on geometric Langlands at the University of Chicago which has a web-page.

Kari Vilonen has a web-site devoted to geometric Langlands and its relation to physics.

MSRI ran a workshop on Geometric Aspects of the Langlands Program in 2002 and the talks are on-line.

As usual, Witten has a hand in all of this, see his remarkable paper "Quantum field theory, Grassmanians and algebraic curves", Communications in Mathematical Physics, 113 (1988) 529-600, and his contribution to the 1987 conference "The Mathematical Heritage of Hermann Weyl" entitled "Free fermions on an algebraic curve".

For a different conjectural relation between Langlands and QFT, see:
Mikhail Kapranov, Analogies between the Langlands correspondence and topological quantum field theory, in Functional Analysis on the Eve of the 21st Century, Vol. 1, Birkhauser, Boston, pp. 119-151.

Comments

1. **plato**
   December 19, 2004

   If this were a moderated usenet forum you'd be told that your posts violate netiquette

   As to reading my stuff:)No!No! think of content first, then who wrote it.:)

   One of the things I had found was to respect people thoughts and if such a link of paragraph was to be read of theirs, then it must lead back to site where it was taken.

   I wanted to respect the source of information and places, so this compromise was thought of. If you do not like the "source," then I can not be faulted on its content. Only, that I linked it?

   So your rules of netiquette do not apply, hence the moderated usenet forum rules do not apply also. Your software should not support this if this is to be taken as netiquette issue. This is easily solved.

   In all cases, I link to the sources as best as possible with secondary links sometimes to source as well.

Interesting site of yours, with some debate about it's authenticity and law? Copyleft idealism?:)

2. December 19, 2004

   'Plato', since you haven't noticed yet: Posting hyperlinks that stretch over several lines is not a good thing to do if you want anyone to read your stuff and to take you seriously. If this were a moderated usenet forum you'd be told that your posts violate netiquette. Here this does not happen as others are violating basic principles of netiquette (and of civilized behaviour) even more.

3. **Urs**
   December 19, 2004

   > A.J. Tolland just explained us this stuff – he was excited that Peter Woit writes about his math research.

   What is this referring to? A talk at Harvard by Tolland? Or anything online?

4. **plato**
   December 18, 2004

   Hmmmm……interesting.

   To construct a consistent theory of all kinds of matter and all interactions, including gravity, is generally regarded as the ultimate goal of theoretical high-energy physics and is likely to remain so for a long time. But the development of theoretical physics is notoriously difficult to predict, and it is not easy to formulate a research plan that can be followed for several years, comprising both a main goal of great scientific value and concrete partial goals that can be achieved in a more limited time. Still, it seems essential to have at least a rough plan for the future, although one must be prepared to be flexible about it when this is needed. In the following, I will therefore try to give such a plan

5. **Lubos Motl**
   December 17, 2004

   A.J. Tolland just explained us this stuff – he was excited that Peter Woit writes about his math research.

   Peter asks too many questions, many of them have a very well-known answer.

   The six-dimensional theories that explain the SL(2,Z) symmetry of N=4 Yang-Mills in d=4 are a completely inevitable part of string/M-theory, and there is extensive literature about them. See the (2,0) theories.

   Obviously, Tolland et al. have had many more things to say.

6. **D R Lunsford**
   December 16, 2004

   Plato-

   You ask where to begin? Where most of the rest of us began – with friction problems, inclined planes, and our own common sense and respect for our scientific forebears. Throw in honesty and hard work and you may get somewhere.

   -drl

7. **D R Lunsford**
   December 16, 2004

   Plato,

   He's not "just" looking – he's NOT LOOKING AT ALL. No one in the usual gang of suspects is looking at anything except their own twisted hallucinatory reflections. These people are actively trying to destroy Western science, a collective creation of millenia that is disgraced by their presence.

   -drl

8. **plato**
   December 16, 2004

   DRL,

   Greene is justing pointing, and reveals a deeper concept. Oracle or not, the question is on how we percieve the implication AGN Jets in a consistant mathematical framework?

   If we deal with microstates, can we ever associate such math structures to explain this consistancy on a macroscale?

   I am in a space(between metric points) where NCG is operating and do not know how to express myself:)The point is where to begin? Yet the dynamical relations of the torus and spin, direct the jet?

   So if we accept euclidean postulates to the fifth, GR to gravity, Reimann sphere, why not here in this diagram(?) U(1) and say we see it in the jet?

9. **D R Lunsford**
   December 16, 2004

   Dear Plato,

   Greene is a carnival-barking buffoon – you might want to consider that before setting him up as your oracle.

   Yours,

   Ari

10. **plato**
December 16, 2004

Poster below:)

Without repeating ole cliche's and sounding like some repetitive impressionist, might we be masking the intent of what one might be saying?:)

I know it is very difficult to find the center, and topologically, impress the idea of "AGN jet" as a continued geometrical expression as well?

As part of, some continued function of the mathematical expressions that you fellows seemed engaged in?

I think Greene was specific on this point?

In fact, in the reciprocal language, these tiny circles are getting ever smaller as time goes by, since as R grows, 1/R shrinks. Now we seem to have really gone off the deep end. How can this possibly be true? How can a six-foot tall human being 'fit' inside such an unbelievably microscopic universe? How can a speck of a universe be physically identical to the great expanse we view in the heavens above? (Greene, The Elegant Universe, pages 248-249)

Updating on Blogs are slow so this link should encapsulate more of the idea that I am trying to express.

I mentioned Sklar before, in a topological expressed post, along side of the Kein bottle. Part and parcel of this continued evolution geometrically?

11.
December 16, 2004

Favorrite Cliches:

"..asymptotic background.."
""…robust,,,xyzxxyyx"
"huge number of possible string theory Hamiltonians…"
" my colleague at Columbia …"
"vertex operator algebras that neither of us understands very well.."
.

.

12.
December 16, 2004

Sometimes the poets have it right:

Things fall apart; the centre cannot hold;
Mere anarchy is loosed upon the world,
The blood-dimmed tide is loosed, and everywhere
The ceremony of innocence is drowned;
The best lack all conviction, while the worst
Are full of passionate intensity.

13. **plato**
December 15, 2004

Being a non mathematician I am interested in how these visulaizations are generated, so of course I wonder sometimes at statements Peter is making about CFT. This statement below is taken from thread of that discussion. You have to understand the expectancy has a predictive quality about CFT's future?:)

New non-geometrical generalization of the principles of CFT will be found, and it will allow to extend the success of S-matrices etc. to the non-perturbative realm. A geometry-like original of dualities – such as E_k in supergravity – will be clarified. Non-perturbative physics on general backgrounds will become calculable, and supersymmetry breaking will be shown to be very different in details than previously anticipated. Realistic N=1 4D vacua with SUSY breaking will be connected and the potential will pick up a rather small number of priviliged points – close to the "heterotic strings on Calabi-Yau three-folds" and/or "M-theory on G2 manifolds" and/or "intersecting brane models with some warping". Two years after the beginning of the revolution, the people will calculate the masses of the heaviest quarks, the (small) QCD theta-angle, and other things, and they will predict the first new physics beyond the SM, which will be only confirmed several years later experimentally.

I was looking for "some consistent method" that would tie together the vast framework of mathematicals in relation to our understood physics approach?

Is that unrealistic?:)

When looking a Klein ordering of Geometries, I had not see any other method that would point to this consistancy, so I am lost when it comes to all the facets of the maths that could be generated.

Maybe lacking a physics approach, mathematics has become limited(like string theory?:) and is no more then a abstract realm that people like to venture, as would artists who have found relevance to signatory styles of expression? There pictures are very unique sometimes like the move to non-eucidean views of Gauss who kept us in suspence?

You would have to forgive my ignorance here, and I fall back on DRL's encouragement for attempting to comprehend?:)

The gravitational collapse.

How would you define it, if you had a consistent geometrical method from it's previous developement??:)

If you look to the cosmo, where else will you find the counterparts of this expression of that same mathematics? I believe some have under estimated the value the comsological palette has in which to test the mathematics it is using.

That is my guess, as suspected, I too have no qualifications to speak on this, although I have a keen eye for artistic styles of expression(Penrose and his tillings? Arthur Miller, Gabriele Veneziano-The Myth of Beginning of Time. Scientific America, The Time before Time, May 2004)

If you think any of these gentlemen deficient in the use of artistic expression even cubist art in relation to the monte carlo effect, maybe the total value of the new math [that must emerge will have to wait for another Reinmann?:)]()

14. **Peter**
December 15, 2004

I don't see how moonshine could be related to the Riemann hypothesis, but who knows. Since it is a story about CFT and an interesting group, it may well somehow be related to the Langlands story.

In any case I think the motivation for studying moonshine is that it is (merely!) interesting mathematics. I wasn't aware of any evidence that the CFT involved could be used to explain anything about the real world.

15.
December 15, 2004

"In that interview, Atiyah made more extensive comments than Singer predicting that new ideas in number theory would come from physics. He explicitly mentioned the Langlands program, so was probably thinking about the same things I've written about here. He also mentions the Riemann hypothesis, perhaps thinking of Alain Connes's work on this subject, which has some physics motivation.

But it sounds like he tried this idea out on Andrew Wiles, who was very skeptical about the whole idea that number theoriists would learn very much from physics."

Yes, sorry, I meant Atiyah.

Do you have any idea if these things (Langlands, Riemann) with regards to their possible connections to physics have anything to do with the moonshine connections to string theory? Which further begs the question if string theory is wrong then what does that say for moonshine – merely interesting mathematics?

16. **Urs**
December 15, 2004

Sorry, forgot to insert my name. That last comment was of course from me.

17.
December 15, 2004

Actually, B takes values in Lie(H) and A in Lie(G), but otherwise, yes.

Note that for the application to Montonen-Olive duality -> 6D SCFTs -> M2s on M5s the kernel of t is nontrivial (in general) as descibed in hep-th/0409200. (We already noted [recently]() that semisimplicity does not seem to allow the observed n-cube scaling for these systems.)

In fact, the kernel of t in these situations is nothing but the abelian group in which the abelian 2-gerbe associated with the bulk of the membrane is associated. This makes it plausible that the abelian 2-gerbe holonomy over the bulk times the nonabelian 1-gerbe holonomy over the boundary can be given a well-defined meaning.

But I agree without you having to convince me: That constraint is unexpected. If you can find a nonabelian gerbe without that constraint but with self-dual 3-form field strength, please drop me a note! 🙂

18. **Thomas Larsson**
December 15, 2004

Let me see if I get this straight. B takes values in H, A in G, and the kernel of the homomorphisms t : H -> G is an abelian normal subgroup of H. So if H is semisimple then the closed surface holonomy is indeed zero?

19. **Thomas Larsson**
December 15, 2004

Oops, sorry. I evidently didn't read you whole post.

20. **Thomas Larsson**
December 15, 2004

But can you have non-zero surface holonomy if the surface has no boundary?

21. **Urs**
December 15, 2004

Thomas –

B is not auxiliary. The constraint says that its image under the Lie algebra homomorphism dt : \h -> \g (which I chose as dt = ad in my previous comment) can be expressed as the curvature of a \g-valued 1-form. The part of B in the kernel of dt is not restricted. By comparison with abelian gerbes which have B_i but not A_i (not to be confused with a_ij) it follows that if anything deserves to be called auxiliary then it is A_i.

The constraint implies that surface holonomy over closed surfaces takes values in the kernel of the homomorphisms t : H -> G, which is an abelian normal subgroup of H. Nothing implies that this holonomy has to vanish.

But I'd be glad to know if this constraint can be relaxed. However, it follows independently from results on standard path space connections, from categorification of ordinary gauge theory and is also the only known solution to the self-duality constraint for nonabelian gerbes. (The use of weak structure 2-groups instead of strict ones in 2-bundles, or equivalently of 'dynamical' group products in nonabelian gerbes relaxes it a little, though.)

22. **Thomas Larsson**
December 15, 2004

I do have a nonabelian surface holonomy for nonabelian 2-bundles and nonabelian gerbes for the case ${ad}(B_i) + F_{A_i} = 0$,

Urs, I really don't understand the logic behind this. Doesn't B = F(A) mean that B is an auxiliary field, completely determined by A? So you basically have a gauge theory with a 1-form connection. For such a theory, are not the only gauge-invariant quantities line holonomies?

It seems to me that your surface holonomy should equal the line holonomy of its boundary. In particular, can you associate a non-zero surface holonomy to a closed surface?

23. **Peter**
December 15, 2004

In that interview, Atiyah made more extensive comments than Singer predicting that new ideas in number theory would come from physics. He explicitly mentioned the Langlands program, so was probably thinking about the same things I've written about here. He also mentions the Riemann hypothesis, perhaps thinking of Alain Connes's work on this subject, which has some physics motivation.

But it sounds like he tried this idea out on Andrew Wiles, who was very skeptical about the whole idea that number theoriists would learn very much from physics.

24.
December 15, 2004

Is this the same connection between number theory and physics that Singer talks about in his Abel prize interview?

25. **Urs**
December 15, 2004

> Part of this story involves the Montonen-Olive duality of N=4 supersymmetric Yang-Mills. This duality interchanges the coupling constant with its inverse, whiile taking the gauge group G to the Langlands dual group (group with dual weight lattice). The symmetry that inverts the coupling constant is actually part of a larger SL(2, Z) symmetry.
>
> One possible explanation for this ${SL}(2,\mathbb{Z})$ symmetry is the conjectured existence of a six-dimensional superconformal QFT with certain properties. Witten explains more about this in his lectures at Graeme Segal's 60th birthday conference in 2002. His article from the proceedings volume, entitled 'Conformal Field Theory in Four and Six Dimensions' doesn't seem to be available online, but his slides are, and they cover much the same material.

The abelian case is well understood. The ${SL}(2,Z)$ symmetry of abelian YM follows (at least classically obviously) from realizing it as a toroidal compactification of the theory of an abelian 2-form with self-dual field strength in six dimensions, where the ${SL}(2,Z)$ is just the modular group of the internal torus.

It is *believed* that something analogous holds true for *non*abelian (super)Yang-Mills (for any A-D-E gauge group), i.e. that its Montone-Olive symmetry comes from a toroidal compactification of some 6-dimensional theory involving a non-abelian 2-form.

In this set of slides, Witten calls this nonabelian 6D theory a *nonabelian gerbe theory*. But certainly that is just a name, to be filled with content, right?

The most glaring problem with making this concrete seems to be this:

What precisely is the duality condition in the nonabelian case and under which conditions can it be imposed?

When I talked to nonabelian gerbe people about this, one thing they said is that it is not clear that in the nonabelian case the self-duality should still be ordinary Hodge self-duality, but that it might involve in addition to the Hodge star an operation on the Lie algebra factor. But I am not quite sure what that should be.

In lack of a better idea, let me assume in the following that we want ordinary Hodge duality. Now, one sufficient condition fulfilled by an ordinary bundle to admit a self-dual field strength is that the field strength transforms covariantly.

So if $U = \lbrace U_i\rbrace_{i\in I}$ is a good covering of the base space with open sets and F_{A_i} is the field strength on U_i, then on double overlaps

\[
F_i = g_ij F_j g_ij^{-1}\,,
\]

obviously.

Since the covariant transformation respects Hodge self-duality, it is consistent to impose Hodge self-duality in overlapping patches U_i.

It is not clear at all that this remains true in general for nonabelian gerbes!

For nonabelian gerbes the general transition law for the nonabelian 3-form field strenth H_i has a covariant part

\[
H_i = g_ij(H_j) + …
\]

plus a mess of noncovariant terms

\[
\cdots + \mathbf{d} d_{ij} + [a_{ij},d_{ij}] – A_i(d_{ij}) + \cdots
}]

and in particular involving this term

\[
\cdots + (F_{A_i} + {ad}(B_i))(a_{ij}) \,.
}]

(The notation here is taken from equation (55) in hep-th/0409200.)

Suppose we want H to be Hodge self-dual and hence H_i to be Hodge-self-dual on each U_i. This implies that on every double overlap all these additional terms in the above transition law have to be self-dual by themselves!

So self-duality on H implies further self-duality conditions on the fields A_i, B_i, a_{ij}, d_{ij} (which are the connection 1-form, it's 2-form cousin and two 'transition forms' that measure the failure of A_i and B_i to transform as usual.)

But these fields don’t transform covariantly themselves. So the self-duality condition on them involves still more conditions, now on triple overlaps. And so on. It is a huge mess of ever more complicated conditions that arise this way. (Unless there is some simplifying principle hidden in them, which I currently cannot see.)

It will be hard to find solutions to these conditions. One solution, though, is easy to see. Obviously, for H to be self-dual it is sufficient that

$$
d_{ij} = 0
$$

(actually this seems to be easy to weaken somewhat)

and

$$
{ad}(B_i) + F_{A_i} = 0 \,.
$$

The big question is: *Are there any further restrictions on the cocycle data of a nonabelian gerbe that would allow Hodge-self-dual H?* In particular, are there any with ${ad}(B_i) + F_{A_i} \neq 0$?

The above choice is curious, since it implies that, while A_i and B_i are nonabelian, H_i takes value in an abelian subalgebra of the full nonabelian Lie algebra.

It is also the only case so far in which we know (so far) how to associate a nonabelian 2-holonomy with the nonabelian gerbe. (A paper on that is due out by end of the year. Really, I should not be blogging but be working on that…)

The existence of that nonabelian 2-holonomy seems to be, apart from the self-duality of H, a further important condition on whatever Witten may mean by nonabelian gerbe field theory:

We known that when lifted to M-theory these nonabelian 6-D theories come from stacks of coinciding M5s with M2s ending in them. The action of these M2s should involve the abelian *volume holonomy* of an abelian 2-gerbe characterized by the 4-form dC_3, where C_3 is the supergravity 3-form potential, over the world-volume of the membrane, call that suggestively but by abuse of the integral notation $\exp(i \int_V C_3)$, times a *non*abelian surface holonomy of the nonabelian 2-form living on the M5s over the worldsheet of the boundary of the M2, call that ${Tr}{hol}_{\partial V}(B)$.

Due to global issues (completely analogous to how the coupling of the string to an abelian 2-form involves abelian gerbe holonomy) the product

$$
\exp(i \int_V (C_r)) {Tr}{hol}_{\partial V}(B)
$$

has a couple of subtleties. (For the case of 1-dimensional lower these, and their solution, are nicely discussed in the above mentioned paper by Aschieri& Jurčo).

Therefore, in order to understand nonabelian theories in 6D (and, incidentally, the general configuration of the fundamental objects of M-theory) it would be very helpful to have a notion of nonabelian surface holonomy ${hol}_{\partial V}(B)$ that makes the above expression well-defined.

I do have a nonabelian surface holonomy for nonabelian 2-bundles and nonabelian gerbes for the case ${ad}(B_i) + F_{A_i} = 0$, i.e. for the only known case in which the existence of a self-dual 3-form field strength is known. But I have not yet checked if it makes the above action for the M2 brane globally well defined.

(also posted to the String Coffee Table)

26. **Aaron**
    December 14, 2004

    For the physics side of geometric langlands (although they might not have exactly known it at the time), you can see

    hep-th/9501022
    hep-th/9501096

The First Evidence For String Theory?

December 16, 2004
Categories: Uncategorized

I was wondering why there were lots and lots of hits on this weblog today coming from Google searches for "first evidence for string theory". It looks like the answer is this lead article from the latest New Scientist magazine. I don't have access right now to the full article, but it's clearly based on the usual cosmic string hype. After all, according to the author, string theory "is our best hope of understanding how the universe works", so anytime astronomers see something unusual, what else could it be but a string?

Update: I finally got ahold of a copy of the full article. It is based on two separate anomalies seen by astronomers. The first is called "CSL-1", which was first reported nearly two years ago. It appears to be two nearly identical galaxies right next to each other, but the authors of a paper about it would like to believe there is some inter-galactic cosmic string producing two images of a single galaxy via gravitational lensing. Even if you believe this, there's no evidence this is a fundamental superstring, even Joe Polchinski doesn't think so (see Lubos Motl's excited posting about "astronomers prove string theory").

The second observation actually has nothing to do with the first (despite what the opening sentences of the story suggest). It's of a quasar called Q0957+561A,B that really is a gravitationally lensed object. One thing I don't understand is that in the case of CSL-1, the fact that there are only two images is taken as evidence that a string is doing the lensing (and claims are made that lensing by point like objects only produces odd numbers of images), whereas for Q0957+561A,B there are only two images, but an intervening galaxy, not a string, is what is doing the lensing. For the quasar pair, some changes in brightness by about 4% have been observed, so it has been suggested this is due to a nearby cosmic string (inside our galaxy, within 10,000 light years) which is moving around in our line of sight with the quasar pair.

I'd be curious to hear what professional astronomers think of this. To me it looks like just more string theory hype, and I now suspect that for the indefinite future, whenever an astronomer somewhere, somehow sees something anomalous, we're going to be subjected to claims that "strings have been observed!!".

Comments

1. **D R Lunsford**
   December 22, 2004

   http://groups-beta.google.com/group/sci.physics.research

   All you need is a browser.

   Anyone can write a paper and submit it for review. arxiv is more snobby, so ignore it.

   I'll be happy to read your paper. Of course I'm not very optimistic, since dicking around with the constants of nature is not very interesting. But I guarantee you a fair hearing.

   -drl

2. **Quantoken**
   December 22, 2004

   Dr. Lunfords said:
   "TAKE IT TO SPR AND GET FLAMED THERE. WRITE A PAPER AND SOLVE THE WORLD."

   I do not have access to SCI.PHYSICS.RESEARCH. For some reason I can no longer post on http://www.physicsforums.com. Repeated email inquiries asking why never gets any answer. If I am banned at least they need to let me know why.

   I do not have priviledge to submit to ARXIV either. They now require that you have got to have an endorser first to submit anything at all.

   I would love to "Get flamed". But that never happened. No one listens. I have a great theory that makes correct predictions and results in GR on one end of limit and QM on another, and resolves many of the puzzles that the establishment scientists are puzzling about. But one one seems to even want to listen. Too bad that some of the most intelligent people would have to waste their lifetimes at the end in vain.

   Quantoken.

3. **Alejandro Rivero**
   December 21, 2004

   "People tell me not to superpose any theoretical prejudice on their claim that their zodiacal sign is important to their destiny"

   Existence of a destiny is already a theoretical prejudice.

4. **Arun**
   December 20, 2004

   DRL,

   People tell me not to superpose any theoretical prejudice on their claim that their zodiacal sign is important to their destiny.

   This web-page from Jodrell Bank Observatory
   http://www.jb.man.ac.uk/booklet/GravitationalLenses.html
   says:

   "…it appears that approximatly one distant radio source in 500 is split into multiple images due to lensing by a foreground galaxy."

   Therefore a quasar and a foreground galaxy lining up seems to be relatively common, and there will be even more cases where one is in the periphery of the other.

   -Arun

5. **D R Lunsford**

December 20, 2004

The authors are observational astronomers – their job is to stoke the theoretical engines, not design them.

See the later papers, mentioned below.

My own opinion is that we are seeing the effect of joint gravitational/electromagnetic physics. However, the most important point is to take the observations at face value, and not to superimpose a theoretical prejudice.

-drl

6. **Arun**
December 20, 2004

http://arxiv.org/PS_cache/astro-ph/pdf/0203/0203466.pdf
does not dare conclude that the objects in NGC7603 are physically connected.

7. **Arun**
December 20, 2004

What are plausible mechanisms for intrinsic redshifts that would apply purely in quasar neighborhoods, and not hit anything else in astrophysics?

8. **D R Lunsford**
December 20, 2004

Arun said

I disagree that I've complicated things. In my opinion, you're putting the cart before the horse. How do we first show that the quasar and galaxy are physically associated..

At some point it becomes a matter of good judgment. When you see exact "chance" lineups again and again, the chin stroking begins…In the case of some of these objects, there are *clearly* whiffordills of matter streaming from one to the other. In the case of NGC 7603, this matter bridge *itself* contains two more interesting objects. Sometimes a cigar is just a cigar..

The other way to proceed is to say – here is a model of how quasars and galaxies are physically associated, e.g. quasars are ejecta from galactic cores. One immediage prediction is then that there should be star-like objects that are associated with a galaxy like the anomalous redshift quasars are; but are blueshifted relative the galaxy, because there is no reason that ejecta should always be directed away from us or traverse to our line of sight.

Agreed – and it is entirely possible for quasars to have a high intrinsic redshift, because of unknown physics, that swamps any blueshift Doppler-originated blueshift. Indeed the strange appearence of peculiar galaxies leaves one with the powerful impression that something very different is taking place from what we see in our placid corner. Again – all that needs to be shown is physical association of discordant Z objects. The origin of Z is then up for grabs. (Note you can still have the Hubble and Doppler Zs – just that it's not the whole story.)

If you or Arp or whomever finds such stellar objects, I bet the whole community will pay attention. If Arp has not ever even attempted such a survey, then I must count him among the cranks.

I'm sure Arp would love to do just that. Anyone who could produce "Atlas of Peculiar Galaxies" is permanently out of the crank ranks.

-drl

9. **Arun**
December 20, 2004

DRL,
I disagree that I've complicated things. In my opinion, you're putting the cart before the horse. How do we first show that the quasar and galaxy are physically associated?

Merely observing two objects along the same line of sight cannot tell us that they are physically associated. Physical interactions between the galaxy, gaseous filaments, and quasars are not observed/observable.

The standard model in fact says that since the redshifts of the quasar and associated galaxy are so different, it is merely happenstance that they appear together in the sky, and they are not physically associated. One way of overturning this is to show that the statistics of such anomalous redshift objects is observed to be different from what one would expect. Absent that, the standard model accomodates these "anomalous" objects quite well. The ratio of such objects to the number of gravitational lensing candidates should be computable.

The other way to proceed is to say – here is a model of how quasars and galaxies are physically associated, e.g. quasars are ejecta from galactic cores. One immediage prediction is then that there should be star-like objects that are associated with a galaxy like the anomalous redshift quasars are; but are blueshifted relative the galaxy, because there is no reason that ejecta should always be directed away from us or traverse to our line of sight. If you or Arp or whomever finds such stellar objects, I bet the whole community will pay attention.

If Arp has not ever even attempted such a survey, then I must count him among the cranks.

—
-Arun

10. **D R Lunsford**
December 20, 2004

BECAUSE YOU ARE BEING RUDE TO THE HOST AND HIS GUESTS. TAKE IT TO SPR AND GET FLAMED THERE. WRITE A PAPER AND SOLVE THE WORLD. BUT LEAVE IT OUT OF HERE.

-drl

11. **Quantoken**
December 20, 2004

Dr. Lunsford:

I do respect Dr. Halton Arp without necessarily agreeing with him totally.

But what right do you have to ask the establishment to respect his right to develope an alternative theory, when you do not respect mine in the first place.

We can agree to disagree and continue to talk about science. But you've got to put your vulgarity aside and pay me some respect.

Quantoken

12. **D R Lunsford**
December 20, 2004

Q, where did you learn manners? NO ONE HERE IS INTERESTED. TAKE IT TO SPR AND GET FLAMED THERE.

-drl

13. **Quantoken**
December 20, 2004

I might also add that the location relativity, which is a natural derivation from the fundamental principle of my GUITAR theory, also predicts the Pioneer Spaceship Abnormal Acceleration, and it gave the correct quantitative prediction as observed.

The predicted "acceleration", based on my calculation, is:

(4/PI)*C^2/(Radius of Universe)

This acceleration quantity does not depend on the spaceship's location. It always point to where the observe is located.

Please note the radius of universe is calculated from alpha, in my natural unit system:
Ru = PI * N
N = PI * exp(2/(3*alpha))
The length unit is equal to classical electron radius. That doesn't mean it's classical, but it's value happen to be calculated the same way as classical electron radius is defined.

It is not purely a numerology coincidence that I got the exactly correct neutron mass, a 9 digits accuracy, among other amazingly correct predictions. It is a real science that describes the nature correctly.

Quantoken

14. **D R Lunsford**
December 20, 2004

Yes, Arp is flat wrong with some of his ideas – but he's also an absolutely first-rate astronomer with a great track record, who has been "flat ostracized" by the clergy for speaking blasphemously.

BTW Q, SHUT THE F*CK UP ABOUT YOUR "THEORY" – TAKE IT TO SPR OR THE LIKE.

-drl

15. **Quantoken**
December 20, 2004

I have read about Halton Arp's point of view and things like "tired light". I do not agree totally with him. But it is definitely wrong that a well established astronomer be deprived his observation time and his right to look at the sky and study his theory further.

According to the Copernicusian, we do not live in a special time or special place in the universe. It's absolutely true we do not live in a special generation of human history.

Of course our generations are more advanced than the middle age. But of course future generations thousands of years later would be more advanced than us, too. Frankly, future generations will look at the way how true science is suppressed in the 20-21st century, the same way we look at how the establishment suppressed the opposite ideas in the Middle Age, if not worse.

We truely do not live in a special era.

Now on the Hubble Redshift. It is an observational truth that there do exist such a red-shift that is approximately proportional to distance. That should not be questioned. What needs to be questioned is how to interpret that distance correlated redshift.

I do not think the standard interpretation of Doppler Shift is right. Now do I think the tired light model is right.

We assume that the frequence of light from remote location would not otherwise change, unless there's either Doppler Effect, Gravity Redshift, or other physical reasons to cause it to change.

But I have the opposite idea. I think the light itself doe NOT change! The frequency does NOT change. What changes is the ruler that we use to measure that frequency!!!

What ruler do we use to measure frequence? We measure frequence by counting number of waveforms per second. So we need to have a clock to measure time to measure frequence!!! I think the very ruler that we use to measure time, the clock, has changed from the remote location to our location.

Yes, I am talking about relativity. Not Einstein's Relativity, but a more fundamental relativity. Time is different not only when measured on different reference frames. Time is also different when comparing two clocks billions of light years apart.

And just like the case of special relativity. This difference is relative: We see a remote clock billions of years away runs slower than our clock. But the other civilization billions of years away also see our clock as running slower than theirs.

This location related special relativity, relative to Einstein's inertia reference frame related special relativity, was neccesated, and predicted by the fundamental principle of my GUITAR theory, which believs that the total quantum information of the universe HAS to be conserved, which directly leads to a closed spacetime of the universe.

Certainly, in a closed spacetime, you can not have time propressing at the same pace at all locations! What I mean is you can not have a universal time and at the same time have a close 3-D space. It is that simple. Spacetime is one piece and you really can not separate the two and treat space and time differently. That's what Einstein told us.

My theory of relativity therefore also answers the observed super nova "time dilation". Of course time dilates, because clocks at remote location runs slower than ours!!!

GUITAR is a self consistent theory and agrees with all observed facts, and have made many amazingly precise predictions, including precisely calculated the CMB temperature, solar constant, proton and neutron mass, within the error bars of observational data.

Quantoken

16. **D R Lunsford**
December 20, 2004

Arun,

You complicate the issue needlessly. The main point is that quasars are assumed to have high cosmological redshifts, that is, they are far away. The only *first* thing that needs to be shown is that quasars are physically associated with galaxies having a widely discordant redshift (and that peculiar galaxies of discordant redshift are physically associated) – once this is accepted the search for causes can begin. Personally, I *do* think it is new physics – namely, these are situations in which the linking of gravity and light would be expected to show up. Perhaps it can be explained as standard plasma phenomena on a large scale, perhaps not. The main point is that something other than cosmical distance with Hubble's law is contributing to the redshift. Certainly, nothing will happen as long as pressure from the BB clergy prevents a campaign of observation of these objects. Ask Arp where he'd like to begin, and turn him loose.

-drl

17. **Arun**
December 20, 2004

A quick take on anomalous redshift objects:

The anomalous redshift examples consist of quasars with a high redshift, seemingly physically associated with a galaxy of low redshift.

a. Chance juxtaposition – the association is apparent, not real. If things line up often enough for us to have gravitational lensing candidates, then surely things line up often enough to have these anomalous examples; presumably we can work out the statistics.

b. Suppose the quasars are associated with galaxy. Then their high redshifts are either due to their velocities or are intrinsic to them.

b1. E.g., suppose quasars are ejecta from galactic cores. Then we should find also stellar objects associated with galaxies that are blueshifted relative to the galaxies.

b2. Suppose the redshifts are intrinsic – then a blue shift survey would find nothing.

b2a. It could mean Hydrogen is emitting its Balmer spectral lines inside a deep gravitational well; so quasars are super massive objects associated with a galaxy. Perhaps just as exotic as quasars in the standard model, where they are superbright and very far away. Now the quasars are associated with the galaxy by filaments of gas, and we should observe acceleration of the gas into such supermassive objects.

b2b. New physics – this is dubious – which of the very well understood laws regarding electromagnetic radiation would you want to abrogate?

The very first question I'd ask to someone who is pointing to anomalous redshift examples is – has the corresponding blueshift survey been done?

18. **JC**
December 20, 2004

It seems like the word "terrorist" is slowly losing it's original meaning, in the same way the meaning of words like "Nazi", "fascist", "commie" and "communist" changed over the 20th century. People are using "terrorist" to label their opponents in the same way folks used "Nazi" and/or "communist" to slur their opponents during most of the 20th century, where their opponents are anything but fascist or Marxist in political ideology.

19. **Matti Pitkanen**
December 20, 2004

It seems that this is the correct moment to inject really provocative experimental findings. I cannot resist the temptation to also say few words about TGD based model for findings. The model provides support for the role of magnetic flux tube structures (having cosmic strings as a limiting case) as carriers of dark matter in quantum states of astrophysical size. The ratio v_0^2= G/R^2, where R is CP_2 length, essentially the ratio of the cosmic string tension to string string tension, appears also as a basic parameter of the model, so that I can argue that the injection is loosely related to
the topic of the discussion.

1. Are planetary orbits Bohr orbits?

The basic finding is that there is evidence that planetary orbits correspond to Bohr orbits in a gravitational potential when Planck constant is replaced with a gigantic "gravitational Planck constant. For various kinds of experimental evidence (our planetary system, radii of exo-planets, matter in galactic halo, morphology of large scale astrophysical objects) see the article

D. Da Rocha and L. Nottale (2003), Gravitational
Structure Formation in Scale Relativity, astro-ph/0310036,

D. Da Rocha and Laurent Nottale have proposed that Schroedinger equation with Planck constant hbar replaced with what might be called gravitational Planck constant hbar_{gr}= GmM/v_0 (hbar=c=1). v_0 is a velocity parameter having the value v_0=about 145 km/s. This is rather near to the peak orbital velocity of stars in galactic halos. Also sub-harmonics and harmonics of v_0 seem to appear. The support for the hypothesis coming from empirical data is impressive. Nottale and Da Rocha believe that their Schroedinger equation results from a fractal hydrodynamics.

2. Is dark matter in quantum states of astro-physical size?

Many-sheeted space-time however suggests that astrophysical systems are not only quantum systems at larger space-time sheets but correspond to a gigantic value of gravitational Planck constant. This would imply astrophysical quantum coherence lengths and times. The gravitational (ordinary) Schroedinger equation or Bohr rules would provide a solution of the black hole collapse (IR catastrophe in case of hydrogen atom) problem encountered at the classical level.

The basic objection is that astrophysical systems are extremely classical. Many-sheeted space-time predicts however macro-temporal quantum coherence in the scale of life time of gravitational bound states. The resolution of the problem inspired by TGD inspired theory of living matter is that it is the dark matter at larger space-time sheets which is quantum coherent in the required time scale.

3. Beraha numbers and quantization of Planck constant

I have proposed

[see the chapter "Intentionality, Cognition, and Physics as Number theory or Space-Time Point as Platonia" at http://www.physics.helsinki.fi/~matpitka/tgd.html#intcognc%5D

the possibility that Planck constant is quantized and the spectrum is given in terms of logarithms of Beraha numbers

B_n= 4cos^2(pi/n), n>=3.

associated with both the so called type II_1 factors of von Neumann algebras, braid group representations, and quantum groups. The lowest Beraha number B_3 is completely exceptional in that it predicts infinite value of Planck constant.

The inverse of the gravitational Planck constant could correspond a gravitational perturbation of this as 1/hbar_{gr}= v_0/GMm. The general philosophy would be that when the quantum system would become non-perturbative, a phase transition increasing the value of hbar occurs to preserve the perturbative character and at the transition n=4–>3 only the small perturbative correction to 1/hbar (3)=0 remains. This would apply to QCD and to atoms with Z>137 as well.

4. The parameter v_0 in terms of the ratio G/R^2

TGD predicts correctly the value of the parameter v_0 assuming that cosmic strings and their decay remnants are responsible for the dark matter. v_0 is essentially the ratio sqrt(G)/R, where R is CP_2 size: v_0^2 gives also the reduction of cosmic string tension from its stringy value 1/G consistent with the velocity of orbiting stars in the galactic halo.

The harmonics of v_0 can be understood as corresponding to perturbations replacing cosmic strings with their n-branched coverings so that tension becomes n^2-fold: much like the replacement of a closed orbit with an orbit closing only aftern turns. 1/n-sub-harmonic would result when a magnetic flux tube split into n disjoint magnetic flux tubes.

5. Quantum model for evolution of planetary system

The study of inclinations (tilt angles with respect to the Earth's orbital plane) leads to a concrete model for the evolution of the planetary system as being induced by the quantum evolution of the dark matter. Only a stepwise breaking of the rotational symmetry and angular momentum Bohr rules plus Newton's equation (or geodesic equation) are needed, and gravitational Schroedinger equation holds true only inside flux quanta for the dark matter.

a) During pre-planetary period dark matter formed a quantum coherent state on the (Z^0) magnetic flux quanta (spherical shells or flux tubes). This made the flux quantum effectively a single rigid body with rotational degrees of freedom corresponding to a sphere or circle (full SO(3) or SO(2) symmetry).

b) In the case of spherical shells associated with inner planets the SO(3)–> SO(2) symmetry breaking led to the generation of a flux tube with the inclination determined by m and j and a further symmetry breaking, kind of an astral traffic jam inside the flux tube, generated a planet moving inside flux tube. The semiclassical interpretation of the angular momentum algebra predicts the inclinations of the inner planets. The predicted (real) inclinations are 6 (7) resp. 2.6 (3.4) degrees for Mercury resp. Venus). The predicted (real)inclination of the Earth's spin axis is 24 (23.5) degrees.

c) The v_0–> v_0/5 transition necessary to understand the radii of the outer planets can be understood as resulting from the splitting of magnetic flux tube to five flux tubes representing Earth and outer planets. The flux tube has a shape of a disk with a hole glued to the Earth's spherical flux shell.

d) A remnant of the dark matter is still in a macroscopic quantum state at the flux quanta. It couples to photons as a quantum coherent state but the coupling is extremely small due to the gigantic value of hbar_gr scaling alpha by hbar/hbar_gr: hence the darkness.

6. Connection between dark matter and living matter?

What is amazing that the period T =hbar_gr/E associated with n=1 orbit in the case of Sun is 24 hours within experimental accuracy for v_0. This and other rather amazing coincidences between basic bio-rhythms and the periods associated with the states of orbits in solar system suggest that the frequencies defined by the energy levels of the gravitational Schroedinger equation might entrain with various biological frequencies such as the cyclotron frequencies associated with the magnetic flux tubes.

These findings encourage to take with some seriousness the TGD based quantum model of living matter involving macroscopic quantum coherence in even astrophysical length scales (at space-time sheets representing topological field quanta of various fields with frequencies in ELF and ULF range) and flow of matter between different space-time sheets as basic mechanism of quantum control and metabolism. It would be quantal dark matter at topological field quanta which makes visible matter living.

For more details see either the article "Gravitational Schrödinger equation as a quantum model for the formation of astrophysical structures and dark matter?" at http://www.physics.helsinki.fi/~matpitka/articles/nottale.pdf or the chapter "TGD and Astrophysics" at http://www.physics.helsinki.fi/~matpitka/padtgd.html#astro".

Matti Pitkanen

20. **Arun**
December 19, 2004

http://www.metaresearch.org/publications/books/SeeingRed-Arp.asp

Quote:

Here are some brief quotes outlining what Arp has learned from these exchanges.

"When presented with two possibilities, scientists tend choose the wrong one." "

The stronger the evidence, the more attitudes harden."

“The game here is to lump all the previous observations into one ‘hypothesis’ and then claim there is no second, confirming observation.”

“No matter how many times something has been observed, it cannot be believed until it has been observed again.”

“If you take a highly intelligent person and give them the best possible, elite education, then you will most likely wind up with an academic who is completely impervious to reality.

“When looking at this picture no amount of advanced academic education can substitute for good judgment; in fact it would undoubtedly be an impediment.”

“Local organizing committees give in to imperialistic pressures to keep rival research off programs”

“It is the primary responsibility of a scientist to face, and resolve, discrepant observations.”

“Science is failing to self-correct. We must understand why in order to fix it.”

21. **Arun**
December 19, 2004

A Arp-Bahcall debate on anomalous redshifts occurred in 1972. What has changed since then?

22. **D R Lunsford**
December 19, 2004

OK Arun, but this is rather more important:

http://arxiv.org/abs/astro-ph/0401596

Another one, this time, with Zs from .12 to .22.

Ok, clear now everyone? The above is science, with spectrometers and eyeballs and brains.

-drl

23. **Arun**
December 19, 2004

Congrats. to Iyam for injecting some humor into this vitriolic thread.

There are a lot of hurdles to be cleared before we know whether we are seeing gravitational lensing by a dense quasi-one dimensional object (dunno why people object to the word ‘string’ 🙂). Let there be independent observations of this object; let the Hubble or Keck get some observation time. Remember that it is the astronomers, not the string theorists, who get the final say as to whether something is there. Meanwhile, the theoretical community, well symbolized by the barracuda, will come out with a burst of speculative papers.

This is the way it has always worked, string theory hasn’t changed anything.

24. **Iyam deRanjeed**
December 19, 2004

Dear Quantoken,
A colleague has pointed out to me your entries on this blog and I have to tell you that scientific plegiarism is a very serious issue! Your derivation of the CMB temperature and baryon masses look suspiciously like my cosmological work on Scalar-Hadron Interactions and Thermal Excitons (S-H.I.T.E), where I derive these numbers. I have 2 papers that I published on this while a visiting scholar at the Yonkers Institute of Technology in New York.

I derived an explicit differential equation for a time-dependant dimensionless quantity N(t)(although in my paper it is called B(t)) and get a solution in the infinite time equilibrium limit
such that N(t)–>N=4 pi exp(a/3alpha) where constant “a” is fixed by initial conditions (the Planck temperature). As the universe cools off and expands and energies fall below 100Gev the running coupling constant alpha falls from 1/128 to its present constant value of 1/137. The effective gravitational constant is then essentially “condensed” out from thermal quantum vacuum fluctuations, much like Sakharov said it should, and thats why it can be related to alpha via N. Because the solution is essentially a thermal equilibrium limit I can compute the CMB temperature and get an accurate answer within current experimental bounds.

The vacuum fluctuations also create regions of trapped quantum information called “excitons” which we experience as protons, neutrons and other baryons and hadrons, and the amount of information determines their masses since I can relate entropy S to E and get the masses from m=sqrt(E/C^{2}). When alpha is 1/128 the fluctuations are much greater at the beginning of the universe and the regions of trapped information are therefore heavier and within their own horizons, so they are actually microscopic quantum black holes with a Bekenstein entropy (equal to their von neumann entropy). But these can decay via Hawking radiation and contribute to the CMB. Only as
N–>N=4pi exp(a/3alpha)=const=1/137, as the universe cools, do the trapped regions become much lighter and stable so that they appear as massive particles like protons.

Although there are differences in my model the similarities are too much of a coincidence and you will not get credit for computing the CMB temperature, since I clearly did it first. I would expect that you would at least quote my work rather than shamefully present it as your own. Anyone visiting your blog will be made aware of this. The advantage here is that I am in print for all to see.
Yours
Iyam deRanjeed
India

25. **plato**
December 19, 2004

Lubos

For not lack of trying to understand, I appreciate the work you are doing to bring comprehension to this subject. I am always revising, to understand the theoretical positions people adopt.

According to T-duality, universes with small scale factors are equivalent to ones with large scale factors. No such symmetry is present in Einstein’s equations; ***it emerges from the unification that string theory embodies, with the dilaton playing a central role***by

Gabriele Veneziano

regards,

26. **Lubos Motl**
December 19, 2004

Those who are more interested in physics than the “Gentlemen”‘s inability to learn even the very basics of this subject (and their political statements) – I am not sure whether anyone like that attends this blog – should read my article about it

http://motls.blogspot.com/2004/12/astronomers-prove-string-theory.html

and the articles linked in it.

27. **Lubos Motl**
December 19, 2004

An idiot posted a nonsense above my message, so it may be safer to post my message once again:

It’s a waste of time. I will just keep on returning to these dangerous places to emphasize that the owner of this blog and most of the contributors on this blog are scientifically illiterate morons. They have no idea what they are talking about, and they have no respect to the basic rules of scientific integrity.

28. **Quantoken**
December 19, 2004

Lubos said:

“It’s a waste of time. I will just keep on returning to these dangerous places to emphasize that the owner of this blog and most of the contributors on this blog are scientifically illiterate morons. They have no idea what they are talking about, and they have no respect to the basic rules of scientific integrity.”

In that case I do will delete them so my readers do not waste time. But I will keep an archive of all your shits and explain why I have to delete them.

But for now, don’t start to “waste” your time there yet. I have not put anything there yet. Posting a ton of your rants there without me starting to say a word yet only make you look silly.

Quantoken

29. **Lubos Motl**
December 19, 2004

It’s a waste of time. I will just keep on returning to these dangerous places to emphasize that the owner of this blog and most of the contributors on this blog are scientifically illiterate morons. They have no idea what they are talking about, and they have no respect to the basic rules of scientific integrity.

30. **Quantoken**
December 19, 2004

Damn, another typo. I guess I really don’t type well on a keyboard when do it in a hurry:

t0 = time for light to go across the classical electron radius
t0 = C/(classical electron radius)
t0 = 2.817940325×10^-24 seconds

The middle line was wrong although the numerical value is correct. Waht I meant to say is:
t0 = (classical electron radius)/C

Quantoken

31. **Quantoken**
December 19, 2004

Chris:

Thanks.

And, peter, thanks for providing a place where I can discuss my thoughts, even just a small glimpse of my theory.

Indeed I have been a bit too lazy and havn’t even got a web site set up to discuss my theory fully. I will do it as soon as I find some time dealing with the chore of getting the typesetting right.

In the future, here is my BLOG:

http://quantoken.blogspot.com

There is nothing there yet at this moment. When time comes I will disclose the full details of how I achieve those amazing results of computating the CMB temperature, solar constant, proton and neutron mass, etc, all from one consistent theory.

I am not going to deal with Lubos any more. I thought he was a smart guy having got that ln3 result. But since he is not willing to admit mistakes like another guy who got ln3 wrong did, he is just too stubburned to be persuaded. I do hope he has a calculator and punch in numbers to try to verify my calculation, trying to pick my any possible arithmatic mistakes. Here is the source of oficial physical constants:

http://physics.nist.gov/cuu/Constants/index.html

I will try to post less on Peper’s blog if I can control myself a bit. He is a nice and tolerate guy. No surprise that he is a mathematician!

Quantoken

32. **Chris Oakley**
December 19, 2004

Peter –

Please don't – I'm enjoying it.

Quantoken –

Repeating your theories in the comments section of physics blogs will get you nowhere. Collect your thoughts on a web site & if people see fit, they will visit it.

Lubos –

The image of knowing an aeroplane is going in the wrong direction without necessarily being able to operate the controls is very apt. I have talked to laymen who have read "The Elegant Universe" & come away with the impression that the whole thing is going nowhere without any prompting by me. It's not good enough to be able to sell your ideas to fellow believers. Ultimately you have to sell your ideas to the rest of the world as well & behaving like a spoiled brat is not going to help your case.

33. **Quantoken**
December 19, 2004

Regarding the CMB discovery, that's the whole point I want to make. They discovered CMB and explain it as the remains of Big Bang and hence a supportting evidence for Big Bang, and they obtained the Nobel. If they discovered the CMB and explain it as star radiations, then that trivializes their discovery and there is no way they can get a Nobel out of it, even if it is the same discovery.

It's the whole secret how you cook your data. As I have shown, without dispute, that CMB is indeed just star radiations: I obtained the absolutely correct CMB temperature that matches oberved value exactly. No one else have obtained such a precise calculation of CMB. I got it out of the simple assumption of star radiation.

Actually Sir Eddington did similar calculation and obtained 3K, a very precise number, far before CMB discovery. BigBangers can't even hope to get 3K correct today. I got all digits correct!

BTW, Lubos, are you making physical threats to me? Science can not develop if physical threats like this exists. Scientists must be able to express any idea free of fear of threats, AND free of fear of FUNDING. Unfortunately the later part is lacking.

I am putting up a BLOG of my own and I guarantee no one will be censored there. Not even you, Lubos.

Quantoken

34. **Peter**
December 19, 2004

I enjoy the spectacle of vigorous scientific debate as much as the next person, but calling for physical violence (by the US military or anyone else) against one's intellectual opponents is beyond the pale. I'll be deleting any future comments by anyone that do this.

35. **Quantoken**
December 19, 2004

I missed something in the middle due to typo. It should be

sqrt(beta) = ((2/3)*(1/alpha) + (3/2)*ln(PI))/ln (sqrt(2/PI)*(Tau/(PI*t0)))

Not
sqrt(beta) = ln (Tu/sqrt(PI))/ln((Tau/(PI*t0))*sqrt(2/PI))

The starting formula and the numerical results are all correct:

sqrt(beta) = ln (Tu/sqrt(PI))/ln((Tau/(PI*t0))*sqrt(2/PI))

36. **Quantoken**
December 19, 2004

Lubos:

Go away. Your "explaination" of what I wrote is simply erase my words on your blog. "Explanation" by censorship is not an explanation and I am not interested in dialoging with a censorship dictator.

I am not commenting on you more and you should not comment on me more either. Is that fair enough?

Now my derivation of the exact neutron mass. So that you don't bullshit more on me. I am not going to give any reasoning. Just the formula. Any one can calculate and get the same result:

Theoretical Proton mass:
Mp = Me*(1/alpha)*(ln(2*(1+3*5!+7!))/ln2)
Mp = Me*137.03599911*(ln(10802)/ln(2))
Mp = 9.1093825×10^-31 kg * 1836.146836
Mp = 1.672626404×10^-27 kg

Theoretical Neutron Mass:
Mn = Mp + Me*beta
sqrt(beta) = ln (Tu/sqrt(PI))/ln((Tau/(PI*t0))*sqrt(2/PI))
Tu is age of the universe expressed in My Natural Unit Set, where the unit of time
t0 = time for light to go across the classical electron radius
t0 = C/(classical electron radius)
t0 = 2.817940325×10^-24 seconds

Age of universe
Tu = PI * N
with
N = PI * exp (2/(3*alpha))
So:

sqrt(beta) = ((2/3)*(1/alpha))/ln (sqrt(2/PI)*(Tau/(PI*t0)))

Tau is the free Neutron mean lifetime, 885.7 seconds.
You get:
sqrt(beta) = 93.07442757(31)/58.4372(9)
beta = 2.53677(9)
The brackets above means the possible discrepancy of the last few digits.

So:
Mn = Mp + beta*Me
Mn = 1.672626404×10^-27 + 2.31084(7)x10^-30
Mn = 1.67492724(7)x10^-27 kg

The above is my calculation result. The official value is:
Mn = 1.67492728(29)x10^-27 kg
(see http://physics.nist.gov/cgi-bin/cuu/Value?mn|search_for=neutron+mass)

My result matches up to the ninth digit and is completely within the experimental margin of error.

My computation is elegant all the way and there is absolutely no adjustable parameter, no fractional number, no expansion series. Only one additon occured to calculate neutron mass from proton mass. I obtained both the correct proton mass and the correct neutron mass. Can you say a theory that achieves 9 digits accuracy this way, is crackpot?

I can not disclose my reasoning behind because the kind of Lubos will probably erase my messages and then write up a string paper to publish and claim to have calculated neutron mass.

Quantoken

37. **Lubos Motl**
December 19, 2004

It's just totally incredible what all of you imagine science is.

Concerning "bird shits on the telescope" – there were some shits indeed, but even without the shits the signal survived. This is why Kapitsa, Penzias, and Wilson made the biggest discovery in cosmology in the last 50 years, and were awarded by the 1978 Nobel prize for their discovery of the microwave background.

You will probably never understand what science is. Science requires absolute integrity. Science cannot be done under threats. I insist that if a complete moron with 0 knowledge about physics tries to rape physics and force the scientists to publish whatever he wants, then this moron should be eliminated by the government's bodies if the government has any control over the security situation in the country.

It is also a very personal responsibility of every individual scientists to resist all pressures, threats, and blackmailings that are intended to twist his or her honest scientific investigation.

Every scientist with healthy enough muscles should give a proper thrashing to terrorists like Chris Oakley who are openly trying to destroy the very basic principles of unbiased scientific research.

38. **Lubos Motl**
December 19, 2004

Dear Quantoken,

I've already explained the reality about all the stupidities that you wrote, and your latest rant does not contain anything new that has not been explained yet.

Of course that I think that Oakley's comments about funding of these amazing projects are horrible. It's an approach of a totally uniformed terrorist. People like Oakley should be dealt with by the US soldiers with the gun – and I am sort of ashamed to waste my time with such immoral idiots.

Sincerely Yours
Lubos

39. **Quantoken**
December 19, 2004

Chris said: "I would love to be the person who cuts the grants of scientists who resort to bullshit explanations whenever they see anything slightly out of the ordinary."

Chris, unfortunately you can not do it even if you have that power. That's the most people there are in the fields. The culture is cultivated by "publish or perish." You would want to provide bullshit explanations than trivial, common sense explanations or nothing at all, if you want something published. Claiming "My telescope does not work perfectly because a bird shit on it" does not get your paper published. Claiming "this peck of dust is not peck of dust but something explainable by superstring" will guarantee publication. You can't cut them all.

One big exception I must make is for mathematicians. They do not have the burden of observation and proving their result by experiments. All they ever do is the most strict mathematical reasoning. So there is absolutely no room to be bullshiting in the field of mathematics.

So mathematicians are the most honest people on the earth. That's why we do not see a single politician came from a mathematics background.

Quantoken

40. **Quantoken**
December 19, 2004

Lubos:

Looks like Chris's comment of "cut funds" really hurt your feeling more than the language of "crackpot", right? Talking is cheap, but funding is not. "Publish or perish".

Regardly your ln3 result. Are you sure you are the only surviver? You are not, you are just still breathing. The other guy has been proven to be wrong, and you got the same answer as the wrong guy got. If you have the SAME wrong answer, how come he were wrong and you were correct. It's just no body has bothered to check your mistaken yet. That's worse than being found to have made a mistake: You know your answer is wrong but you do not know how you made that mistake.

Regarding your comment of seeing the picture of the paper to believe these are two identical images, not one. My comment is smart people do NOT always believe what they see. They have brains, they subject everything they see, hear, or touch to logic reasoning, filtering all the information, before deciding which part is to be believed, and which part is not. If your brain does not have that filtering or processing power and simply take everything published as granted, then it serves no better purpose than a paper recycling bin.

You say they hide the coordinate of that particular object so no one else can see it before they collect more data. But then they claim the phenomena is so common place that within 16 square arcseconds, a visual area just a size of one galaxy at 10 billion light years awy, they could find 11 pairs. Then any one can look at any part of the sky and find plenty of this sort of things.

The center of such "two" images being so close, even closer than the radius of a typical galaxy, the object itself. Even if you think you see two separate but identical images, it could be easily explain away by a few hundred different artifacts I can think of.

The simplest and easiest explaination is they are indeed just one slightly distorted image, wrongly interpretted as two images.

For example, do you have an idea how weak the light would be if it comes from a galaxy 10 billion light years away? Radius of the universe is only 14 billion light years. Do you know how many photons they would be able to collect, during a course of several hours observation?

You probably don't know the stuff, being working in such a narrow field. Astronomical observation is not taking point and shot photos. To get anything at all, you have to aim your telescope exactly towards the same object for several hours to collect a few photons to form an image. On the earth, that means you have to rotate your telescope all the time in a very precise way to keep it targeted.

If some how it is not targetting very precisely, during the whole course of several hours, like it is off just a little bit, 2 arcseconds, then the photos would not focus on exactly the same spot, but will form a slightly distorted image.

2 arcseconds is not much. All it take, is probably a fly by bird shitting on the telescope and the tiny extra weight bends the telescope so slightly. Or a truck drive by to dent the ground surface ever so slightly.

Even without bird shits or drive by trucks, an earth surface based telescope would have to deal with the fact that star lights were bend by the atmosphere. During different part of the day the injection angle is different and the bending will be different. Different weather condition or atmosphere composition also may cause the bending to be slightly different and it is hard to model it and compensate it, on top of compensating for the rotation of the earth.

On top of all these, you have the "atmospheric seeing". The lights are distorted due to random thermo movements of the molecules in the atmosphere. This seeing makes it hard to obtain anything better than 1 arcsecond resolution even under even most optimal observational condition. So 2 arcseconds is reall not that much!!!

I have done scientific experiments so sensitive that I absolutely have to conduct it under the ground, after 2:00am, and I must not make the slighted muscle movement even to type on a computer keyboard. It's damn too easy to introduce artifacts in observational data nowadays. Being a theoretician you probably know nothing more than your blackboard.

Quantoken

41. **Lubo? Motl**
December 19, 2004

Fortunately, the people in the agencies are not idiots like you, Chris. On the contrary, some of them know much about physics. Chris, I know that you would like to be powerful, much like Peter (well, and many others for that matter, too). But one can't get too powerful if it is too obvious that he or she is a complete idiot.

If the images are confirmed identical and identially strong and undistorted, a huge cosmic string's gravitational lensing is the only acceptable explanation. I am sure that the most important people in the agencies can understand why it is so.

42. **Chris Oakley**
December 19, 2004

I would love to be the person who cuts the grants of scientists who resort to bullshit explanations whenever they see anything slightly out of the ordinary.

43. **Lubos Motl**
December 18, 2004

Peter Woit obviously has not understood the basic points about gravitational lensing yet.

For generic, point-like sources of gravitational lensing, one obtains several images that are highly distorted and that have very different intensities.

Who wants to get a feeling why it's so can play with the following simulation applet:

http://www.iam.ubc.ca/%7Enewbury/lenses/lensdemo/demo.html

It's of course the case of the other lensed object, too – the two images are NOT identical. Moreover, it is known which object is causing the lensing.

CSL-1 is special among all lensings in having two IDENTICAL images – the spectra are equal with 99.999 percent confidence level or so. This occurs for lensing by cosmic strings, but not by pointlike objects.

44. **Lubos Motl**
December 18, 2004

Concerning the last paragraph.

Peter obviously cannot or does not want to understand that in the case that the images of CSL-1 turn out to be identical indeed (and not just two similar "twin" independent galaxies), then it is just guaranteed that it is either a big coincidence (poinlike particle lensing fine-tuned to give unusually similarly strong images), or a cosmic string.

If the other possibilities are excluded, then OF COURSE that conclusion will be “a string has been observed” because only the cosmic strings naturally produce identical images via lensing. The open question will then be “what kind of string have we seen”. But yes, if Peter is afraid that these strings will either be a support for string theory (fundamental strings themselves) or Grand Unification (that Peter Woit also hates) or a similar theory at the same scale, then he is very correct to be afraid, because the scale simply works this way.

If the deficit angle is between 10^-7 and 10^-6, then the tension of the string is at the GUT scale (which is near the string scale in the more-or-less conventional models), and it is naturally a GUT object, or a string theory object.

It’s certainly not guaranteed that these “anomalies” will survive and be interpreted in this exciting way, but if it happens to be so, then only a complete idiot will be able to oppose the statement “a string has been observed”. Already today, Peter Woit is obviously getting ready for playing this role. 😉

45. **Lubos Motl**
December 18, 2004

Peter, could not you reduce your insults, at least against other people than me?

What do you mean by “What professional astronomers think of this”? Is not Sazhin a professional astronomer? The professional astronomers, such as Mikhail Sazhin,

http://xray.sai.msu.ru/~sazhin/

think exactly the same thing as everyone else who understands what’s going on. It’s a potentially very important observation (let me talk about CSL-1 now), and it is either a weird coincidence of lensing by a pointlike object that happens to create two identical images – which is very unusual – or it is a pair of 2 different galaxies that look almost identical, or it is evidence for a cosmic string seen in the telescopes.

If you ask a professional astronomer which kind of cosmic string it is, he will tell you his theories but he will also add that he does not understand the stuff enough, and you should ask professional particle physicists or string theorists.

46. **Lubos Motl**
December 18, 2004

Sorry, but I will continue to be excited about exciting things, and angry about outrageous things, because this is one of the motors that drive science and not only science. If you deliberately want to be a bitter cynic, it’s YOUR problem.

47. **Chris Oakley**
December 18, 2004

Lubos –

I do not see how anyone can conduct science in the kind of emotionally-charged atmosphere that you seem to want to carry around with you.

It is not just about being clever. I’m sure that you are cleverer than I am and – who knows – you may be cleverer than Peter. Science is not just about that, it is about exercising judgement. If in the cold light of day, after you have thought about something a lot it appears to be especially good or especially bad, then you are duty bound to follow where that leads. And in my case, the direction is not towards yet more levels of abstraction. I seems likely, therefore, that I will continue to be the the bitter, cynical old sod that you describe.

48. **Lubos Motl**
December 18, 2004

Chris Oakley, you may be looking for unique exact predictions or whatever, but what you’re looking for is absolutely irrelevant for the question how Nature works.

If there happens to be a cosmic superstring – macroscopic fundamental string, for example – 10,000 light years from the Sun, then it will become a fact of Nature and we will have to live with it – and scientists will have to give a proper explanation. If this turns out to be the case, it will be absolutely obvious that no one could have predicted this string in advance.

We have not mastered the most subtle details of string theory and therefore we can’t still calculate every detail of reality accurately and unambiguously, nevertheless string theory is making so many bold and relatively specific qualitative predictions that most of you, the bitter critics, are – politely speaking – shitting into your pants in awe (so much that you are forced to move some of this stuff on the internet).

In the real history of physics, there have never been so many truly new predictions done in advance, before they’re actually measured, as in the case of string theory. Most of previous major breakthroughs in physics started by observations that were later interpreted in a new theory. Theories of relativity are, in some sense, exceptions, and string theorists are following this example.

We would be much happier if the gap between the theorists – which are much ahead today – and the experimenters and their experimentalists shrink. It’s YOU who does not want it, and therefore you’re scared by every possible new experiment that could give us some hints about the truth.

Let me emphasize once again that cynicism of sourballs like you, Chris Oakley, has no consequences for physics whatsoever. You’re just annoying and obnoxious, but your contributions to science are exactly zero. If you think that it is easy to make reliable and unique new predictions of phenomena beyond the Standard Model, try to compete with us.

The problem is that you could not make even 0.0001% of what the average particle physicist or string theorist does. The only thing you are able to do is to spread bitter remarks and stupidities.

49. **Chris Oakley**
December 18, 2004

“Most people would think that someone who runs around saying they have a wondrous TOE that predicts amazing new things, but they’re not sure whether the amazing new things happen at the Planck scale or the scale of a galaxy, would have to be almost by definition a crackpot”

I think, Lubos, that you’re missing the point here, perhaps deliberately.

Peter and a large number of others, including myself, are looking for a specific predictions which can be tested with specific experiments. If you cannot advance any, then what you do does not deserve the label “physics”.

50. **Robert**
December 18, 2004

Calm down, calm down. In the UK this soothing turn of phrase will be forever associated (thanks to the comic Harry Enfield)with feuding Liverpudlians (natives of Liverpool, for non-UK readers). Somehow, it seems only too appropriate that it should be deployed in this context. Get a grip guys.

51. **Lubos Motl**
December 18, 2004

Peter. Sure that string theory does predict strings. It’s not my problem with your language: it’s apparently your problem with the basics of the field of science called physics. You seem to be ignorant about several absolutely basic features of physics and cosmology.

Could not you do something for me – e.g. try to learn at least the basics from Tom Kibble?

http://arxiv.org/abs/astro-ph/0410073

He’s not a string theorist, so you should be capable to read his article. Of course, reading an article like that requires that you won’t stop once the first word “string” appears in the article. If you always stop reading an article once a word “string” appears in a review or another article, you will remain ignorant about physics until your death, do you know?

Virtually every model coming from string theory predicts that long-lived macroscopic strings may exist in the Universe – usually many different types of a string. The real question is whether such massive objects have actually been created in the history of the Universe and whether they can be seen.

Some models of inflation and cosmology based on string theory say “yes”, some models say “no”. Some models that say “yes” have been ruled out, some models that say “yes” are still alive.

It’s not just string theory. A theory with a spontaneously broken U(1) always predicts cosmic strings – the monodromy around the cosmic string corresponds to the Higgs field rotating around the circle of minima. Cosmology and causality guarantees that if such a spontaneously broken U(1) exists, then there should be strings as big as the horizon radius – not just the size of the galaxy. Of course, observations can then be used to make various bounds on the existence of such objects.

Once again. I find it amazing how little you know about very basic questions such as the conditions sufficient for the existence of cosmic strings.

You even wrote: “Most people would think that someone who runs around saying they have a wondrous TOE that predicts amazing new things, but they’re not sure whether the amazing new things happen at the Planck scale or the scale of a galaxy, would have to be almost by definition a crackpot.”

Are you joking, or are you also this incredibly under-educated in physics? Strings in string theory simply ARE, and they can have any size. Small strings are light, big strings are heavy. It is a question of dynamics and initial conditions whether some particular strings are large or small. The distance of a planet from the Sun can also be large or small. The distance is a degree of freedom, much like the position of a point on a string (and therefore its size). Is this basic concept of physics REALLY so difficult for you?

Obviously, my trivial explanation of the strings’ size that was initially addressed to Quantarzan or what’s the name of the moron can also be directed to you because you seem to be equally ignorant about this absolutely basic question as the crackpot.

OK, let me start again. Have you heard of a harmonic oscillator? It is a system with a position (x) and a momentum (p). You can visualize momentum to be the velocity of a ball attached to a spring, multiplied by its mass.

According to classical physics, the energy of this harmonic oscillator is any positive number. According to quantum mechanics, it is (N+1/2)hf, where h is Planck’s constant and “f” is a typical frequency of the oscillator, and N is a non-negative integer. For small N, quantum mechanics is very important and you get quantized energy. For large N, this variable is effectively continuous and one can think about the oscillator in classical terms.

A string is a collection of many harmonic oscillators (plus the center-of-mass degrees of freedom, which are analogous to pointlike particles) – a string is an infinite-dimensional harmonic oscillator. The low-lying states of the string are the elementary particles, but there are also high-energy, long strings that can be approximated by classical physics. They’re the same strings. Strings are real, not just some fictitious objects, and of course they can become large if one creates them (having enough energy).

I wish you better luck, Peter, with your learning of physics 101 issues because your ignorance is truly breath-taking.

52. **Peter**
December 18, 2004

Hi Lubos,

You still seem to have a language problem with the English verb “to predict”. So string theory doesn’t just “PREDICT” Planck scale strings, it also “PREDICTS” strings with scales the size of a galaxy?

Most people would think that someone who runs around saying they have a wondrous TOE that predicts amazing new things, but they’re not sure whether the amazing new things happen at the Planck scale or the scale of a galaxy, would have to be almost by definition a crackpot.

53. **Lubos Motl**
December 18, 2004

Dear Quantoken,

the quantum of area announced in the ridiculous article by Baez was not 4.sqrt(3) but 4.ln(3) times the Planck area. The calculation of the asymptotic quasinormal modes in my paper – and another paper of Andy Neitzke and myself – is the only thing that survived.

The asymptotic quasinormal modes of black holes have nothing to do with area quantization. Moreover, if one computes other black holes different from the Schwarzschild, one obtains a wrong result, different from 4.log(3).

Moreover, the heuristic “calculation” of the area quantum in loop quantum gravity has been showed incorrect – even if one would believe that there is something correct about loop quantum gravity itself (the a priori probability of this is about 0.0000001%). See e.g.

http://arxiv.org/abs/gr-qc/0407051
http://arxiv.org/abs/gr-qc/0407052
http://arxiv.org/abs/gr-qc/0411035

There are no logarithms of integers conjectured for area quantization anymore.

If you have "calculated" the mass of the proton and you don't use QCD or string theory, then you may be absolutely sure that you are a complete crackpot.

Best
Lubos

54. **Lubos Motl**
December 18, 2004

Hi Peter,

I think that it is fair that this crackpot Quantoken has similar ideas about physics as you, does not he? 😉 Just teasing you.

Let me now pretend that I think that it makes sense to discuss physics with people like "Quantoken" – to prove how extremely polite I am. 😉

Dear Quantoken,

you totally misunderstood the whole issue. Sazhin et al. are not string theorists. They are astronomers, and they think that they have observed something amazing (I repeat: amazing, not "desperate"), namely a huge cosmic string hundreds astronomical units in size or bigger.

Regardless of the type of that string, one would need an explanation if the observation is confirmed. String theory obviously offers several explanations – from the ordinary ones (cosmic strings from spontaneously broken gauge group) to the super-exciting ones, namely the fundamental superstrings grown to a macroscopic size.

To some extent, various particular stringy models PREDICT(ed) the existence of cosmic strings created after inflation, and this could very well be an experimental confirmation of these predictions. Polchinski estimate the a priori probability that such cosmic superstrings may exist to be 10 percent.

But the theoretical explanation is a different level of the story than just the possibly exciting observation.

Yes, if you open the papers by Sazhin et al., you will see the specific pictures of the pair of galaxies – they are like two circles in the digit "8" and almost touch each other. You can still distinguish that these are 2 objects. The theory that there is one image only – without lensing – is safely ruled out.

If you looked at the papers with the explicit pictures, you could have avoided writing complete stupidities on my blog, your blog, and Peter Woit's blog. You know, I am putting the links to the papers at arxiv.org because I expect the average reader to open these papers. By this sentence I want to say that you are much much worse than what I expect to be the average reader.

I think that even the average people who happen to visit your blog or Peter's blog are much more skillful than you, and they will be able to open the Postscript or PDF files with the papers by Sazhin et al.:

http://arxiv.org/abs/astro-ph/0302547
http://arxiv.org/abs/astro-ph/0406516

Consequently, they will be able to see the pictures with the pair of images, and see how stupid you must be if you were not able to open the arXiv web page and rule out your silly theory yourself.

Best
Lubos

55. **Quantoken**
December 18, 2004

Regarding the "cosmic string" Lubos meantioned. It merely shows how desperate super string theoriticians are in trying to find some observational evidence, any thing, that may remotely justify their stuff. When you have that kind of desperation, you tend to OVER-INTERPRET your data.

Lubos said: "The team has observed a pair of galaxies 10 billion light years away and gravitational lensing is supposed to be the origin. The angular separation of the pair is roughly 2 arc-seconds."

Note the two key numbers, 10 billion (10^10) light years distance and 2 arcseconds (9.7×10^-6 radian angle) angular separation. At that distance and that angular separation, if these are two galaxies their center barely separated by 9.7×10^4 light years.

A typical galaxy like our galaxy, has a diameter of 2×10^5 light years. If what they observed are two images of a typical size galaxy, they barely separate half of their diameters.

i.e, what they observe is instead one concrete image from the two half of the same galaxy, but be mis-interpretted as two images. It's that simple.

The distance from the center of one half of the galaxy to the other half happen to be about 1×10^5 light years, which is the angular "separation" they reported.

How desperate they have become? They reported that with an area of the sky merely 16 square arc seconds (4 arcseconds x 4 arcseconds) they found 11 pairs of such identical galaxy images. They must have counted each individual photons received as individual images 🙂

Quantoken

56. **plato**
December 18, 2004

Sometimes the synthesis of ideas need to be brought together, to help people see how the issue is further developed and spoken too, by Lubos and others.

I hope the essence of the following statement highlighted is understood. I don't have any theories, but what I am piecing together from information, that has been out there for sometime. I think in this context as more of a service to those less developed in these views.

Warped Space Creates Gravitational Lensing

57. **Quantoken**
December 18, 2004

To Matti and Lubos:
Matti:
Your theory looks interesting. But have you made any prediction or calculation which matches any observation data. Any theory that does not make a prediction that is verifiable by observational data, is not science.

Lubos:
I know you are one of the guys who used different approaches to arrive at a quantum of area which is 4*PI*SQRT(3), or whatever times sqyare root 3. That was at one time very exciting because it seem quite unusual that two completely different approaches reached the same result.

However, as meantioned on Baez's web site, some one found one of your guys' calculation to be wrong and missing something, after correcting that mistake, the answer came up to be something else. If one of your were wrong, both of you have got to be wrong, because you both got the same wrong answer!!! Any comment on that one?

I guess a lot of people don't realize that when dealing with the microscopic world where continuous space and time loses meaning, so does the very notion of space and time themselves. Therefore the concept of field or topology loses meaning because you no longer have a spacetime background. A new physics must be established on brand new concepts that do not rely on the notion of space and time.

That's where I have a success theory. I have calculated the correct radius, age, mass/energy and Hawking entropy of the universe, I derived the correct baryon density and the exact CMB temperature which matches observational data well within observational margin of error. I obtained the correct solar radiation constant based on the same calculation, again it matches observational data to within observational margin of error.

I derived the precise mass of protons, which I shown on this blog, and which is accurate for the first 7 digits, the discrepancy against experimental value, although more than the experimental margin of error, can be explained away if the proton has an extremely long delay half life (many orders of magnitude more than 10^33 years).

Further, I derived the precise neutron mass based on my calculation of none-decaying proton mass, and the known free neutron decay mean lifetime. The result matches the standard experimental value exactly to the first 9 digits, to well within the experimental error!

You can't say my theory is wrong if I can derive a result accurate to 9 or 10 digits and matches the most precise experimental value exactly.

Since Peter is not interested, I am going to Lubos's web site, and show exactly how I obtained the precise neutron mass, from my theoretical frame work. So every one can verify it.

I do not have a web site detailing everything yet. I hope to do so pretty soon.

Quantoken

58. **Matti Pitkanen**
December 18, 2004

It is amusing to see how the ideology "every good idea becomes part of string theory sooner or later", and if possible, without any reference to the person who discovered the idea originally. In my case this creative record keeping is easy since after the second super string revolution it became impossible to get anything related to TGD to ArXiv.org and the attempts to publish anything non-stringy in so called respected journals is waste of time. Before continuing, I want to make clear that I am not a bitter crackpot: see the link from the Mathematics Subject Classification Table of American Mathematical Society to TGD. The ethos of mathematicians seems to be different than the ethos of theoretical physicists.

Concerning cosmic strings, I developed for more than a decade ago a model of galaxy formation based on TGD counterparts of cosmic strings [2]. These cosmic strings are 4-dimensional surfaces X^2xY^2 in M^4xCP_2, X^2 a minimal surfac (string orbit) in M^4 and a Y^2 geodesic sphere in CP_2. For all practical purposes they look like strings. These objects carry magnetic monopole flux (the homologically non-trivial 2-sphere of CP_2 carries one unit of topological magnetic charge). String tension is about 10^(-7)/G and thus much lower than the string tension of super strings (I just wonder how M theorists manage to get the string tension correctly). The order of magnitude is determined by CP_2 radius, whose value is fixed by p-adic mass calculations from electron mass.

The value of the predicted string tension is consistent with the constant velocity spectrum of stars in galactic halo [2]. The most elegant explanation for the velocity spectrum is in
terms of gravitational field created by a very long cosmic string going through the galactic nucleus and containing a sequence of smaller galactic cosmic strings around it: somewhat like pearls in a necklace. The force created by the long string behaves as 1/rho and thus yields constant rotational velocity spectrum and free motion parallel to string. Galactic visible matter could result as decay products of galactic cosmic strings. Also the yet undecayed portions of cosmic strings in the galactic plane could contribute to dark matter and even yield the mass distribution M(R) propto R predicting constant velocity spectrum as I proposed in the original version of the model. The jets orthogonal to galactic plane usually assigned to a galactic black hole would move along long cosmic strings. Also the jets associated with super-novae are assigned with magnetic flux tube structures in the TGD based model for gamma ray bursts [4].

Cosmic strings are actually a limiting case of magnetic flux tube like structures at the limit when M^4 projection of flux tube becomes 1-dimensional. The fractal hierarchy of cosmic strings and magnetic flux tube structures forms the basic of TGD based cosmology and explains the formation of structures in all length scales including even formation of stars and planets. Fractal hierarchy of cosmologies inside cosmologies is predicted [3]. The very early cosmology is cosmic string dominated and singularity free in the sense that the density of gravitational mass goes to zero like 1/a^2 rather than 1/a^4 as in radiation dominated early cosmologies. The TGD counterpart of the inflationary cosmology is a period during which 3-space is flat: the only free parameter of Robertson-Walker metric corresponding to the critical cosmology is its duration so that the theory is extremely predictive (basically by the imbeddability to M^4xCP_2 requirement)[3].

Dark matter can be identified as ordinary matter residing at the space-time time sheets corresponding to magnetic flux tubes. It can leak to "our space-time sheet" and become visible in some circumstances: the formation of solar corona is excellent candidate for this phenomenon in astrophysical length scale [4]. The flow of matter between different space-time sheets plays a key role in TGD inspired quantum model of biology. Dark energy corresponds to the magnetic energy of the string like structures [3]. p-Adic length scale hypothesis resolves the cosmological constant problem: cosmological constant is inversely proportional to the square of the p-adic length scale L(k) of the space-time sheet and for space-time sheets with size scale determined by the age of the Universe it has the required order of magnitude [3]. Cosmological constant characterizes the density of gravitational mass of string like objects and magnetic flux tubes at given space-time sheets. The negative pressure can be understood in terms of 1-dimensionality of these objects.

TGD explains also the observation of objects with different red shifts along the same line of sight mentioned by D. R. Lunsford(http://arxiv.org/abs/astro-ph/0203466). The explanation is in terms of light captured inside a cylindrical space-time sheet having outer boundary through which light does not escape. The light from a distant object can rotate N times around before it is detected and a sequence of red shifted snapshots about the evolution of the object results. Some day this phenomenon will perhaps provide a powerful diagnostic tool allowing to get information about evolution of astrophysical objects.

I am eagerly waiting the moment when string theorists discover TGD inspired cosmology and represent it as a prediction of M-theory. Quantum gravitational holography might be seen as the first example of this creative record keeping. The geometry of infinite-dimensional configuration space of 3-surfaces, which I formulated fifteen years ago, relies on the assumption that space-time surfaces are absolute minima of so called Kaehler action. This means that the knowledge of 3-surface X^3 dictates corresponding space-time surface as a kind of generalized Bohr orbit with Bohr rules fixing the initial velocities when positions are given (there are delicacies involved due to the failure of the strict determinism which are of fundamental importance).

The coding of 4-D classical physics by 3-D surfaces is nothing but an abstract formulation of quantum gravitational holography, and follows from the requirement of general coordinate invariance alone: Diff^4 symmetry can be realized only if there is a unique X^4(X^3) associated with given X^3 and Diff^4 acts on it. Recently I have developed the concept to a much detailed form in which data at 2-dimensional sub-manifolds of 3-surface code all that is relevant for the configuration space geometry and quantum state construction.

I encourage M-theorists to visit my home page: it is a treasure trove of ideas and detailed models and the policy of arXiv.org
guarantees that everything can be taken freely! For instance, p-adic mass calculations might be very interesting stuff [5]. p-Adic mass calculations predict besides elementary particle
masses also CKM matrix and hadron masses from very general number theoretical considerations using minor empirical input. The essentially new element is the possibility of quarks to appear as fractally scaled up variants with mass scale coming as powers of sqrt(2): this occurs for quarks even inside light hadrons and explains the large mass differences of light hadrons. There is empirical evidence that also neutrinos can appear as different scaled up variants.

The chapters of Topological Geometrodynamics related to the cosmology and astrophysics are

[1] "TGD and GRT"
http://www.physics.helsinki.fi/~matpitka/tgd.html#tgdgrt

[2] "Cosmic Strings"
http://www.physics.helsinki.fi/~matpitka/tgd.html#cstrings

[3] "TGD and Cosmology"
http://www.physics.helsinki.fi/~matpitka/tgd.html#cosmo

[4] "TGD and Astrophysics"
http://www.physics.helsinki.fi/~matpitka/tgd.html#astro

The model for particle massivation based on p-adic thermodynamics is described in

[5} the second part of "TGD and p-Adic Numbers" at http://www.physics.helsinki.fi/~matpitka/padtgd.html#mless .

Matti Pitkanen

59. **Lubos Motl**
December 17, 2004

See my blog, motls.blogspot.com, my article is heavily updated, and it contains fixed links to various relevant articles – such as Kibble's recent review that explains everything.

It seems as a potentially very exciting stuff.

60. **Quantoken**
December 17, 2004

Lubos said:

"Chris W., it's not just you, there are many other people who don't know how to derive THINGS from string theory."

That statement is not just absolutely correct, its inverse form is also absolutely correct too, if THINGS means observables in the physical world. No one has derived anything from string theory that has anything to do with the real physical world, YET.

you also said: "It is certainly a well-established insight that the same fundamental strings, those that normally shrink to a supertiny size, also exist in the macroscopic form."

Could you clarify a bit what do you mean "supertiny size"? I think you mean the Planck Scale size. As the 4 normal dimentions we know all lose continuity and hence no longer exist as continuous dimention under Planck Scale, how come the other 6 unknown dimentions continue to enjoy continuity under that small scale, and hence not be treated as just a zero point?

Further "The tiny strings that constitute the particles are *really* just small versions of strings that can also be large if you have enough mass/energy."

Smaller size scales are associated with bigger energy, and bigger size scales are associated with smaller energy. That we know. We were told that due to the present of how huge energy, the other 6 dimentions all curved up into small strings. So I would expect a huge energy to be released, instead of being required, to spring the small strings into larger sizes.

Whatever you want to say about the 120 orders of magnitude discrepancy problem? As the universe presumably continue to expand, shall this disprepancy continue to increase in the future?

Quantoken

61. **Lubos Motl**
December 17, 2004

I've read the full article in New Scientist and it is much more serious and interesting than I thought before. See my blog

http://motls.blogspot.com/

for a report.

62. **plato**

December 17, 2004

I think gravitational lensing, can help set up, different conceptual frameworks.

63. **ksh95**
December 17, 2004

DRL writes

…http://arxiv.org/abs/astro-ph/0401147

The probability of a chance line-up of these discordant Z objects is – drum roll – .000000003.

Which, if you believe in probabilities of this type, means objects like this should exist.

If you're like me, you believe that probabilities in this context are meaningless. These particular objects are either lined up, or they aren't.

64. **D R Lunsford**
December 17, 2004

http://www.geomancy.org/labyrinths/chartres/images/firmament.jpg

65. **D R Lunsford**
December 17, 2004

Here is the woodcut of LM searching for cosmic strings:

66. **D R Lunsford**
December 17, 2004

Lubos,

Derive the Coulomb potential for an isolated charge from string theory. You have 1 hour. You may use 1 8 1/2 x 11 sheet of helpful formulae, and a non-ASCII calculator.

Go.

-drl

67. **D R Lunsford**
December 17, 2004

Chris W,

Why would a sane person believe such whoppers? It's a return to that guy poking his head through the Starrey Realme, therewith to Inspecte the Wheels whiche maketh the Dome of Heavene turn…

Sometimes I think, I've seen the worst of it. Fatte chance.

-drl

68. **Chris W.**
December 16, 2004

For the convenience of other readers of this post, the Polchinski article mentioned by Lubos is Cosmic Superstrings Revisited (hep-th/0410082).

69. **plato**
December 16, 2004

In physics, one way of seeing spontaneous symmetry breaking is through the use of Lagrangians. Lagrangians, which essentially dictate how a system will behave, can be split up into kinetic and potential terms

If you see the the way these points are distributed, what made you think, that such strings couldn't exist?:)

70. **Lubos Motl**
December 16, 2004

Chris W., it's not just you, there are many other people who don't know how to derive things from string theory.

It is certainly a well-established insight that the same fundamental strings, those that normally shrink to a supertiny size, also exist in the macroscopic form.

The tiny strings that constitute the particles are *really* just small versions of strings that can also be large if you have enough mass/energy.

Read the Polchinski article, mentioned on my blog, for more details.

71. **D R Lunsford**
December 16, 2004

Incoherence? There are no string theory equations, there is no apparatus, there is nothing but fetid steam.

-drl

72. **Chris W.**
December 16, 2004

I think this article is mainly an example of the careless journalism at *New Scientist*. Not long ago this observation would only have been discussed in the context of cosmic strings, understood as topological defects in spacetime resulting from gauge field phase transitions. This should still be the primary candidate for an explanation of such a phenomenon, I would think.

[Is it just me, or does the fact that string theory accommodates models that suggest that fundamental strings could be stretched to astrophysical scales indicate a pathological malleability, or outright incoherence, in string theory's interpretive framework?]

73. **D R Lunsford**
December 16, 2004

And we offer, in contrast, some real science:

http://arxiv.org/abs/astro-ph/0401147

The probability of a chance line-up of these discordant Z objects is – drum roll – .000000003.

An earlier paper:

http://arxiv.org/abs/astro-ph/0203466

This man and his coworkers are scientific heroes.

-drl

Book Review: The Quantum Quark

December 20, 2004
Categories: Book Reviews

Over the last couple weeks I've been reading several popular or semi-popular books about particle physics. I thought I'd make a few comments about them here.

The first one is called The Quantum Quark by Andrew Watson. It covers the Standard Model and its history, concentrating on quantum chromodynamics, the theory of the strong interaction. By limiting itself in this way, it is able to go into a much deeper, more detailed study of the theory than would otherwise be possible in a popular book. While avoiding the use of equations and trying to stick to as accessible a level as possible, the author manages to discuss a wide range of aspects of QCD not treated in any other book of this kind. These topics include a detailed description of jet phenomena in perturbative QCD, the behavior of quark structure functions (including their still mysterious spin dependence), the delta I=1/2 rule for non-leptonic weak decays, and many others.

The book contains several amusing stories I hadn't heard before, including the origin of the term "penguin diagram" to refer to a certain class of Feynman diagrams. Supposedly John Ellis and Melissa Franklin were playing darts one evening at CERN in 1977, and a bet was made that would require Ellis to somehow insert the word "penguin" in his next research paper if he lost. He did lose, but was having a lot of trouble figuring out how he would do this. Finally, "the answer came to him when one evening, leaving CERN, he dropped by to visit some friends where he smoked an illegal substance" (the only time he ever did that, I'm sure..). While working on his paper later that night "in a moment of revelation he saw that the diagrams looked like penguins". I'd always wondered why these diagrams had been given that name, they never looked very much like penguins to me. But then again I never tried looking at them under the same conditions as Ellis.

Witten makes an unusual appearance here, as Watson discusses Witten's Ph.D. thesis, the topic of which was the use of asymptotic freedom to study the photon structure function using deep-inelastic photon-photon scattering.

Comments

1. **JC**
   December 23, 2004

   DMS et. al.,

   I agree that large portions of Zwiebach's string theory book is at a level similar to David Griffiths' "Introduction to Elementary Particles" book. My main complaint about Zwiebach's book is that it mainly uses light cone quantization methods, and there is hardly any emphasis on other more elegant ways like conformal field theory and/or BRST. Also not much is mentioned of the superstring and supersymmetry. It seems like most of the book is largely looking at the bosonic string as a pedagogical exercise.

   On the surface, it would be a bit hard to justify subjects like supersymmetry and conformal field theory to a person who knows nothing about things such as the Klein-Gordon and Dirac equations. Also things like the path integral would seem like a mysterious object which came in "out of the blue", to somebody who has only seen basic Lagrangian-Hamiltonian dynamics and the Schrodinger equation previously in undergraduate courses on classical and quantum mechanics respectively.

   I suppose if a student has seen the raising and lower operator formalism in the harmonic oscillator and angular momentum problems from a first course in quantum mechanics, supersymmetry could possibly be presented first in the form of SUSY quantum mechanics. On the first day of class, the path integral could in principle be presented in the context of the quantum harmonic oscillator problem.

   Griffiths' particle physics book covers the basic ideas of the Klein-Gordon and Dirac equations. Arguably if students have already seen the plane wave and harmonic oscillator solutions to the Schrodinger equation along with the "ad hoc" spinor description of electrons in most undergrad quantum mechanics textbooks, the Klein-Gordon and Dirac equations shouldn't be that much more of a stretch.

   Offhand I have not been able to think of an easy non-obscure way to present conformal field theory, to undergrads who have only seen basic quantum mechanics. Presenting other methods like BRST to undergrads, would be even harder to justify offhand.

   It seems like the idea of teaching string theory to undergrads has the same pedagogical hurdles as teaching quantum field theory to undergrads. I would guess that a year-long or two semester course sequence would be required to include the superstring at the undergraduate level. Offhand I don't see it being feasible for a one semester undergraduate course covering the superstring, without doing the course at breakneck speed.

   It would be interesting to see what Lubos would do, if he ever decides to teach an undergrad course on string theory at Harvard.

2. **DMS**
   December 23, 2004

   JC

   Actually, Barton's Zweibach's book on string theory (aimed at undergrads) String Theory: A Fiest Course (Cambridge Univ Press 2004) is quite a nice book.

   Even if you think string theory is not what it is cracked up to be (as I do), you might still get a pretty good flavor of the field, including string theory on particle physics.

   I would say it is very clearly written, and should be understandable by good undergraduates since he goes through a lot of the background material quite nicely. It is at "Griffiths' level", only better IMHO.

   The other thing, that Zweibach mentions in the Preface, is that it enables physics undergrads to get a more mature understanding of the role of mathematics and symmetries in physics, and the physics covered in undergard curriculum. I agree.

3. **JC**
   December 22, 2004

   On a slightly different note, what would really impress me would be a book on quantum field theory that is as easy and painless to read as David Griffiths' "Introduction to Elementary Particles".

   Or for that matter, a string theory book that is as easy and painless to read as Griffiths' particle physics book would also be quite impressive. So far I have not come across any string theory books which are easy and painless to read. If Lubos Motl or anybody else wants a challenge, it would be to write a string theory book which is as easy and painless to read like Griffiths' particle physics book, for which even a good freshman undergrad physics student would be able to read easily.

Towards the end of my time in high school and during the summer before my freshman year of college (on the days when I didn't go out partying), I slowly slogged my way through Feynman's "Theory of Fundamental Processes" and "Quantum Electrodynamics" books, along with other books like Close's "An Introduction to Quarks and Partons", and Commin's "Weak Interactions", not knowing any better. (These books just happened to be at a local university library and I came across them by chance at the time). Besides reading the descriptive parts, I attempted to work out the calculations myself in a somewhat mindless blind manner following the "Feynman rules" as if they were a "recipe". (One can imagine how "blind" and haphazard it would have been for somebody who only knew high school level physics and calculus). Nevertheless I somehow was able to reproduce most of the correct answers in the books, but was still baffled about why these "Feynman rules" were correct in the first place. I found out later the "rules" were from quantum field theory, and I attempted to read several field theory books at the library like Schweber's "Introduction to Relativistic Quantum Field Theory" and Bjorken & Drell's two books "Relativistic Quantum Mechanics" and "Relativistic Quantum Fields". I pretty much got lost very quickly when I attempted to read through Bjorken & Drell's "Relativistic Quantum Fields" and the later chapters of Schweber's book which attempted to explain how the Feynman rules came from the quantum field theory formalism. (Bjorken & Drell's "Relativistic Quantum Mechanics" was a bit easier to digest after mindless working out similar sorts of calculations previously).

At the time I wished books like Griffiths' "Introduction to Elementary Particles" existed.

4. **Steve M**
December 22, 2004

One book I really liked was
"The Key to the Universe" by Nigel Calder. Written in 1978 there was also a bbc series accompanying it. I really liked the way this book was written and presented. It had lots of feynman-like diagrams, and plenty of coloured diagrams when the chapters on the strong forces and quarks came along. It focuses mostly on the standard model and the things that happened up to 1978.
I remember reading it in high school. I got a copy recently from a second hand book store. It is a bit dated now since a lot has happened since 1978 but still well worth a read. I still think one of the best layperson books on particle physics written.

5. **JC**
December 22, 2004

Many years ago I really liked the book "Introduction to Elementary Particles" by David Griffiths, for its easy readability. It was a lot easier to read than any other particle physics or quantum field theory books I've come across over the years. I don't know if it would be a good introduction to particle physics for a non-physicist.

If I had to choose a book for non-physicists, I really liked "The God Particle" by Leon Lederman. I always thought Lederman was a humorous speaker whenever he did a colloquium.

6.
December 22, 2004

There is a book similar in flavor by 't Hooft. The one noticeable thing about 'tHooft's book is the constant mentioning the contributions of some Dutch scientists.

't Hooft, of course, has the reputation of being (perhaps) the guy who did the most during his graduate studies (YM renormalizability, Y-M beta-function) than any other physicist in modern times. Seems like there was some tension between him and his advisor (Veltman) for some time; hopefully the Nobel Prize has dissipated that.

7. **Peter**
December 22, 2004

The Veltman book is also a good introduction to the standard model. It has a lot of interesting short profiles of various physicists, often with Veltman's typically provocative comments.

8. **RT**
December 22, 2004

How does Veltman's 'Facts and mysteries in elementary particle physics' stand compared with these others? If nothing else its penultimate sentence celebrates the title of this blog; no prizes for guessing the topic under discussion.

9. **Peter**
December 21, 2004

I saw the Shumm book in the store and only looked at it for a few minutes. Conceptually I very much agree with its point of view, which emphasizes the beauty of the structure of symmetries that determines the standard model. But I still like the Crease and Mann book better, by emphasizing the history and personalities involved it can be read with enjoyment by people with all sorts of backgrounds.

10. **xiggie**
December 21, 2004

Have you any comment on Deep Down Things by Bruce Shumm?

11. **Peter**
December 21, 2004

The Crease and Mann book is by far the best popular book I know of about particle physics. Can't recommend it too highly.

12. **DMS**
December 21, 2004

Interesting reading; I should take a look. I also liked a much older book on particle physics for the lay person: The Second Creation, by Crease and Mann. That has interesting stories, philosophical discussion by particle physicists (Georgi, Weinberg, etc), and crude idea about renormalization, and the remarkable triumph of Yang-Mills theories. I think it conveyed quite nicely what Coleman would say about the triumphs of QFT— ? made the spectator gasp with awe and laugh with joy.?

13. **Chris Oakley**
December 21, 2004

I *had* heard the "penguin diagram" story. When I was a graduate student in Oxford (1981-1984) John Ellis would occasionally appear. I cannot rule out the possibility of substance abuse, as he looked (& probably still looks) like an ageing hippy. There was a bit of role reversal going on here: unless they *had* to dress smartly, the faculty would mostly be in jeans and sweatshirts & there would be more jackets being worn by students than faculty (I for one never liked wearing jeans, although I would draw the line at the unnecessary wearing of ties).

I think that John Ellis and some of the faculty: in particular my supervisor Graham Ross & Chris Llewellyn Smith always saw themselves as the rebels, however much they pulled the strings (no pun intended).

14. **Amitabha**
December 21, 2004

The story is given with some sources and a picture on p.12 of

http://www.ph.ed.ac.uk/particle/Exp/muheim/prerow/prerow-lecture-1.pdf

Book Review: Out Of This World

December 20, 2004
Categories: Book Reviews

The second popular physics book I've read recently is infinitely sillier than Watson's book on QCD. It's called Out Of This World by Stephen Webb. Its subtitle "Colliding Universes, Branes, Strings, and Other Wild Ideas of Modern Physics" gives some idea of the author's viewpoint, and indicates this is something to buy if you just can't wait for this spring's forthcoming books by Lisa Randall and Lenny Susskind.

The book is about what you would expect, promoting the glories of extra dimensions, branes, M-theory, etc. I only noticed one part of one paragraph where the author mentioned that there was no experimental evidence for any of this. On the other hand, there are dozens of poorly reproduced pictures of string theorists in their offices, which should make their parents proud. The author devotes only one page to loop quantum gravity, with the excuse that he doesn't want to say much about it because it is just a theory of quantum gravity, not a TOE. This doesn't really explain why he then goes on to devote chapters to other string, brane, extra dimension, etc. ideas that aren't really TOE's either.

The whole thing is written in a breathless "Gee, isn't this just so kewl!" style. It's the kind of thing John Horgan refers to as "science fiction in mathematical form", except it's lousy science fiction and lousy mathematics.

There's another very similar new book out, entitled The Great Beyond by Paul Halpern. Here the subtitle is "Higher Dimensions, Parallel Universes and the Extraordinary Search for a Theory of Everything", from which you can guess what will be in it. The author was a grad student at Stony Brook during the 80s, so knows many of the people who worked on supergravity during that period. I didn't have the heart to spend more time with the book than a few minutes flipping through it in the bookstore.

Comments

1. **D R Lunsford**
   December 22, 2004

   TL – those are hilarious 🙂

   -drl

2. **plato**
   December 22, 2004

   Links to url's

   One of the ways in which I look at links supplied without having to use search engines, was to look at how a web page is set up.

   right click mouse, and view source.

   In Thomas's case below this link should show as:

   http://insti.physics.sunysb.edu/~siegel/parodies/

   I have seen enough to know some are more advance in using this medium. If Peter used more pictures for instance in the mathematics he espouses he would be limited becuase of the width of his comment area.

   For him, I calculated this width to be about 360. If he stays to this width, he will not have a problem, although this must be specified beside his image url"" width=360 height=?. This depends on the percentage of the "original picture" to coordinate this width and height. A simple calculation on proportional size based on the picture seen would help.

   Hope this helps others as well. With advancements of html in these blogs one will have to learn new methods for sure, but as long as the comments area is specified, then this will control your viewing capabilties.

3. **Thomas Larsson**
   December 22, 2004

   Speaking of Stony Brook, one may want to look at the html and pdf files at

   insti.physics.sunysb.edu/~siegel/parodies/

   However, Halpern?s book is not there.

4. **plato**
   December 22, 2004

   I hate to interject myself in this new found peace:)

   I sort of had a question about math minds and physics people. Can they ever get it together?

   Maybe there's hope?:)

5. **Lubos Motl**
   December 22, 2004

   I've seen it, too. It's not so bad – the photographic quality of black and white pictures simply always looks like that if you use this technology of printing. Actually a book with a hundred of figures is pretty attractive.

   CERN comes close – who's CERN? Is it those guys who are building the accelerator 10 times smaller than mine? 😉

6. **Peter**

December 21, 2004

I checked the photo credits, 17 are credited to you (more than any other source, only CERN comes close).

7. **Lubos Motl**
December 21, 2004

I can't argue now because I have simply not seen the book yet – and it is not even guaranteed that those pictures of mine were used. At any rate, it does not seem too important.

8. **Peter**
December 21, 2004

Hi Lubos,

Sorry I didn't check the photo credits, I should have known you were somehow involved in this.

The pictures are mostly much too dark, so hard to see well. In any case, while we may argue about the beauty of string theory, in the case of most string theorists, I don't think there's much to argue about…

9. **Lubos Motl**
December 21, 2004

Too bad to hear that the pictures are poorly reproduced – many of them are my pictures.

10. **D R Lunsford**
December 21, 2004

AR – more like a theory of anything (TOA), because I can write down a KK theory for anything coming from a Lagrangian (hence having a canonical energy tensor) that can be made generally covariant – and that's everything of interest.

This simple fact, already known to Pauli ages ago, dooms the entire stringer approach from the start. KK theory is sham dynamics.

-drl

11. **Alejandro Rivero**
December 21, 2004

Well, any Kaluza-Klein theory aspires to be a TOE, doesn't it?

12.
December 20, 2004

why do you torture yourself with these sort of books?

Strominger Interview

December 22, 2004
Categories: Uncategorized

There's an interview with Andy Strominger in the Calcutta newspaper The Telegraph. Strominger was presumably in India for the string theory conference there this past week.

The thing I found interesting about the interview was how skeptical the interviewer was, repeatedly asking about whether string theory might not be wrong. Perhaps at least some members of the media are starting to get a clue.

Comments

1. **Quantoken**
   January 1, 2005

   Mr. "" said:
   "Discreteness of space and time happens at much larger scale that corresponds to scale of elementary particles" Then it would be accessible experimentally and would have shown up already because we can probe these scales."

   I am discussing mistake in Lubos paper in respect to the currently established science. NOT in respect to my theory. So I really do not deviate from that topic and discuss my theory further. But since you meantioned it, you forced me to explain it one more time.

   The answer is, Mr. "", don't you see logical inconsistency in your sentence: Basically you are saying: If there is a NATURAL limitation of the smallest possible size approximately equal to electron radius, then we already have the technology to break that natural limitation, and probe some thing much smaller already!!?!

   If indeed that is the natural limitation of the smallest possible size, then, certainly, no human technology could even possibly break that limitation.

   You thought you have break that limitation and probing some thing smaller, only because you have accelerated a particle to extremely high momentum. And calculation of de Broglie's wavelength gives you a size smaller than that limitation already. But the matter is even the de Broglie's wavelength formula breaks down near that limitation. That limit is in-accessible as much as light speed is un-breakable.

   Mr. "" also said:
   "And for the very last time–there are NO derivatives against position and time at all at the Planck scale in the Motl paper! It is a perturbative analysis of a classical black hole, the motivation being that the log Immirzi parameter in question can be derived from the other end…via a quasinormal mode analysis in classical general relativity. If you don't agree with this paper then submit a comment or rebuttal to the journal it was published in. Otherwise your opinions dont mean a damn thing to anyone anywhere."

   Have you read the actual Motl paper? The very first equation, whatever you call it, takes derivatives against x (the first term) and time (the third term):
   http://www.arxiv.org/abs/gr-qc/0212096
   And frequency omega corresponds to a mass/energy of a black hole. We know a black hole has a mass at least one Planck Mass or more. So he was talking about derivatives below Planck Scale limit.

   Quantoken

2. **JC**
   December 31, 2004

   I agree with Chris Oakley on this one. Lubos I can handle without any problems. Quantoken is becoming too much to handle.

3. **Randy**
   December 31, 2004

   Anderson defends western science and then it would
   seem is also fond of the Santa Fe Institute. That seems strange as I do not understand why the umbrella term 'Complexity' programme at Santa Fe is credible and ST is not. A generalisation of these new age programmes (ST, Complexity, experimental mathematics) has the common property of elevating the digital computer's role in modern science.

   http://www.physics.hku.hk/~tboyce/ss/topics/anderson.html

4.
   December 31, 2004

   "Discreteness of space and time happens at much larger scale that corresponds to scale of elementary particles" Then it would be accessible experimentally and would have shown up already because we can probe these scales.

   And for the very last time–there are NO derivatives against position and time at all at the Planck scale in the Motl paper! It is a perturbative analysis of a classical black hole, the motivation being that the log Immirzi parameter in question can be derived from the other end…via a quasinormal mode analysis in classical general relativity. If you don't agree with this paper then submit a comment or rebuttal to the journal it was published in. Otherwise your opinions dont mean a damn thing to anyone anywhere.

   I agree with Chris. Let him discuss his ideas on his own blog. Trouble is that no-one, not even other crackpots, will waste any time reading it. Thats why he is here.

5. **plato**
   December 31, 2004

   Today I am writing a article for those less inclined to understand the overall perspective that can be formed into a intelligent framework for consideration.

   The post(**The Sphere is Not So Round**) should materialize later, if people want to have a look at it. I also wrote it in response to living review that was offered.

   The link paragraph below hopefully helps to orientate one first, to better conmprehend my article today, and answers, the living review article.

At the energy scales characteristic of the universe's earliest moments, one can no longer approximate matter and energy using an ideal gas formulation; instead, one must use quantum field theory, and at the highest of energies, one must invoke a theory of quantum gravity, such as string theory. Cosmology is thus the pre-eminent arena in which our theories of the ultra-small will flex their muscles as we trace their role in the evolution of the universe.

At such small scales, how would mini-blackholes ever make sense?:)If gravity can work on such large scales, then how would would you interpret gravity at such small scales? One would need to know, what would drive these, at both the cosmological and quantum scales?

Is there any similarities here?

6. **Chris Oakley**
   December 31, 2004

   Peter,

   The half-wit Quantoken is spoiling my enjoyment of your otherwise excellent web log. How about a new year's resolution to ban him?

7. **Quantoken**
   December 31, 2004

   RT:
   The argument of ITO is irrelevant because we are discussing whether Lubos's paper was wrong or not. Lubos did not use ITO in his paper.

   He was fundamentally wrong when he wrote equations involving derivatives against position and time at Planck Scale, where every one can agree continuous space time no longer exist.

   Quantoken

8. **Quantoken**
   December 31, 2004

   Mr. "" said:

   "Q wrote" most people agree that at the Planck scale continious space and time don't exist"
   This deduction/conclusion COMES PURELY FROM quantum gravity, string theory and noncommutative geometry, all of which you have repeatedly stated you don't believe in and which "secretly inject G" (your words). Different approaches to QG all seem to converge on this conclusion that the Planck length is the lower bound or the cutoff and that space and time becomes "foamlike". But you can't condemn these theories as being wrong, as you have done, and then use their main conclusion (or one of them) to support another argument.

   Again your logic seem to work the reverse way. The notion of Plank Scale did not came from and was not derived from Quantum Gravity, String Theory or Nonecommutative Geometry. The concept of Planck Scale was first proposed far earlier than any of these three. You simply stealed that existing notion and then claimed it to be yours and you derived them from your theory.

   It's not even a correct statement that you say I do not believe in those three. There is nothing to even believe in or not believe in! "Quantum Gravity" is a theory that people have been looking for and have not found yet. "String Theory" is a broad research direction on which people hope to finally propose a physics theory, again it's not found yet. They say there are 10^123 kinds of different String theories but they have so far not found a single one, yet.

   Until you can present me string theory in one self-consistent integral piece, instead of just a few corners and parts, don't even ask the question whether I believe it or not.

   Now, I do disagree with the notion of Planck Scale. I believe the discreteness of space and time happen at a much larger scale, one that corresponds to the scale of elementary particles.

   Logically that does NOT prevent me from agreeing with the notion that at Planck Scale time and space are discrete. Because if time and space has become discrete at large scale, then they certainly are even more discrete at the smaller Planck Scale. So there is no inconsistency in my logic.

   No one sees to disagree that at Planck Scale we do not have continuous space and time. So if any one discusses differential equations where derivatives against position or time are involved, then he/she is fundamentally wrong.

   Some food for thought: Assuming there are two spacetime-coordinates very close to each other. They are of the closest distance possible yet at different spacetime location. Now, how does the two observers at the two locations conclude they are at slightly different location, instead of exactly identical location? If you can understand this question, you will begin to understand QUITAR, and know why Planck Scale is wrong.

   Quantoken

9. **RT**
   December 31, 2004

   A stochastic Ito approach to (quantum) gravity is discussed by Hu and Vedagner

   http://www.livingreviews.org/lrr-2004-3

10.
    December 30, 2004

    Q wrote" most people agree that at the Planck scale continious space and time don't exist"
    This deduction/conclusion comes purely from quantum gravity, string theory and noncommutative geometry, all of which you have repeatedly stated you don't believe in and which "secretly inject G" (your words). Different approaches to QG all seem to converge on this conclusion that the Planck length is the lower bound or the cutoff and that space and time becomes "foamlike". But you can't condemn these theories as being wrong, as you have done, and then use their main conclusion (or one of them) to support another argument. For a noncontinious/random process, like fluctuations in spactime you could use Ito calculas definitions of the derivative like you do for Brownian motion and stochastic analysis. (I dont know if anyone has applied this though in quantum gravity/geometry).

11. **Quantoken**
    December 30, 2004

    Dr. Lunsford said:

"Whatever Witten wants to think of himself, or what anyone thinks of him, is irrelevant. To be in his position of authority, and to allow and encourage the endless drain of talent and useless sqaundering of scarce resources represented by string theory, is behavior that amounts to scientific crime. His name doesn't belong in the same paragraph as Einstein's."

"crime" is too heavy a word to use, Mr. Lunsford. Witten is not in control. He is a victim of the system himself. Really whoever want to spend his/her own lifetime on whatever subject is really one's own choosing and no one else is responsible for it.

LC said: "If the string folks turn out to be wrong in the end, they'll eventually disappear except for a few fanatics. As far as I'm concerned, people are going to do and say whatever they want irrespective of how legitimate or silly it is."

Wrong LC. I do not see how you are going to prove string theory wrong. It's neither right nor wrong. It's just useless. It's a mathematician's toy that some people just love to play with. Toys are not as useful as tools but they are neither right nor wrong. The only way string folks will disappear would be when they are bored of their toys.

Right or wrong does not make certain things go away. An example is palm reading or astrology. As long as there is business, they will continue to exist for a long time. Once there is no longer any business, they would disappear over night.

That's the ultimate universal truth Darwin discovered. Survival of the fittest. In the science community, fitting is measured by the ability of getting continued funding grant. Under the system, the ones who survive are the ones who can get funding, they may or may not be on the right track of scientific discovery, but as long as there is funding, they survive.

It is much easier to get funding for super string research. For one reason this is a majority group. For another, it does not take much to feed a string theoretician, all they need is a black board and a computer. And even a computer may be overkill, many of them probably mainly just use the computer for type-writting. If I am the one who grant fundings I would figure, what the heck, it's just a very small slice of the pork barrel. So even if they are wrong, not much money could be wasted.

12. **quantoken**
December 30, 2004

Thanks Peter for providing that link. I knew that living review site but have not got a chance to read everything there yet. Will comment after I study that material. Mean while, it is still wrong to push known QM to Planck Scale or below.

Most people agree that once you reach Planck Scale, familiar concepts like continuous spacetime etc. no longer exist. So how could you still have a legitimate differential equation which differentiate against position and time, and which hence depend on the assumption of continuous space and time for its legitimacy in physics. You can't!

I do not know how your logic works, talking about derivative like dF/dx is simply wrong at Planck Scale where there is no continuous x. Any one care to dispute that?

Quantoken

13. **plato**
December 29, 2004

Peter said:*The best historical analogy is to his predecessor at the IAS, Einstein, who initially had good reasons to hope for a unified theory using extensions of Riemannian geometry. Einstein also made the mistake of pursuing an idea long after he should have realized it didn't work.*

How else, would you show gravitational collapse(turn things inside/out)?

Cosmologically, you say this area, is much like strings? You don't like the math structures in string and M theory? You don't like blackholes and mini blackholes?:)

We know GR works well in cosmological areas, so the leading geometrical principals are consistent and lead to where?:) So is there such a thing as gravity?:)Further, geometrical and topological considerations?

Is there no proof for gravity(?), so like strings, this should be avoided?

P.S. Statistically, *I think,* sound of music beats out, clock ticking and Money of Pink?:)

14. **Steve M**
December 29, 2004

Rubbish JC! Geoffrey Chew is the man! And Saturday Night Fever still ranks as one of the best selling albums of all time…but when the Floyd come back with "Dark Side of the Moon II" and then III" they will blow "Fever" out of the water!
🙂

15. **JC**
December 29, 2004

DR Lunsford,

If the string folks turn out to be wrong in the end, they'll eventually disappear except for a few fanatics. As far as I'm concerned, people are going to do and say whatever they want irrespective of how legitimate or silly it is.

A silly example of this would be the folks who today still believe in the analytic S-matrix bootstrap stuff. Other less illuminating examples would be the people who still think late 1970's "disco" music still rules the world, and/or the aging hippies who still think "acid rock" will once again rule the world.

16.
December 29, 2004

Actually Q, the first equation Lubos writes in his paper is the differential equation for the radial perturbations of a Schwarzchild black hole with a Regge-Wheeler potential. It is a Schrodinger-like equation! Even if he calls it a "schrodinger equation" anyone with a basic physics education knows what he means. The phi(r) in his first equation describes the radial perturbations! He then analyses quasinormal modes ANALOGOUS to quasistationary states in QM whose frequency is permitted to be complex. He is studying the asymptotic real parts of the quasi-normal frequencies. Quasinormal modes are PHYSICALLY well defined as well as mathematially well defined. The BH wants to settle down to a static spherical configuration just like a bell wants to stop ringing. This black hole perturbation equation has also been known for along time: J A Wheeler and T Regge, Phys. Rev. 108, 1063 (157). Schrodinger-like equations appear in many problems with spherical symmetry. The classical heat and diffusion equations are "Schrodinger-like" equations. The Schrodinger equation is just a heat equation in imaginary time
Thats why "heat kernels" are discussed in QM and QFT contexts.

17. **D R Lunsford**
December 29, 2004

JC –

In other epochs there were fads, but these fads were not followed at the expense of other ideas, did not corrupt the very idea of science, and were not bolstered by a credulous public, an ignorant military-industrial government, and a power-hungry core group of theorists. It was not fashionable to bash Einstein and Dirac, and now even Feynman, for not being faddists, or for following their own paths.

I've been reading Feynman's lectures on gravitation. I can't tell you how wonderful it is to see Feynman, who was not regarded as a relativist in particular, show his total mastery of the subject and to discuss it with a tangible physicality – one can almost feel the tension in spacetime represented by curvature, when Feynman describes it. And yet it is OK for the stringers to "excuse" the poor blighter for not "getting it", to cast off his comments as the signs of approaching senility. Likewise Dirac – his little book on GR tells the story without BS, without hubris, and without ridiculous, boxed up, 2-tracked, egg-crated hyperbole prefaced by smart-assed, inappropriate quotes and goofy cartoons – but that sad man was, were are to believe, "misguided" because he thought of QFT as a horribly broken disaster that needed urgent repair. And Einstein – the man who invented 20th century physics – this pitiful lost soul spent his last years "pursuing a chimera", we are to believe.

How can anyone who cares a fig about science read these things and not blow his top?

As far as I'm concerned, physics, if not the entire Western tradition in science, has lost its soul. The torch is being passed to the East. The kings are dead – long live the mandarins.

-drl

18. **D R Lunsford**
December 29, 2004

Peter –

Whatever Witten wants to think of himself, or what anyone thinks of him, is irrelevant. To be in his position of authority, and to allow and encourage the endless drain of talent and useless sqaundering of scarce resources represented by string theory, is behavior that amounts to scientific crime. His name doesn't belong in the same paragraph as Einstein's.

-drl

19. **JC**
December 29, 2004

If for no other reason, folks mind as well work out the consequences of string theory just to see what it produces. As long as there's ways of getting funding for string theory research, that is going to happen anyways. It may be a different story if there's little to no funding sources for string theory research.

Since string theory doesn't have many other major competitors with a large number of researchers, many of the best graduate students will be attracted to it. (In the late 1970's the same sort of grad student "profile" seemed to have been attracted to supergravity). Whether it's animal behavior in general, herd-like behavior seems to be a dominant human trait.

20.
December 29, 2004

For Q: a tract on the physics of quasi-normal modes:
http://relativity.livingreviews.org/Articles/lrr-1999-2/

21. **Quantoken**
December 29, 2004

I completely agree with Peter's 3 points observation of Witten. He is an absolute genius and his intelligence really deserves some respect.

But the more so, the more it makes him a pity. Look at the picture that a genius that do not come often on this planet walking and getting lost on a wrong road that is decorated with all beautiful flowers, but lead to no where, and at the end he would have wasted his intelligent lifetime, with little achieved and soo to be forgotten once the correct theory has been recognized. Don't you want to say pity?

All other criticisms against Superstring Theory can at most be described as "skeptical". Because no one know for absolutely sure whether it is right or wrong. They can only make a probability judgement. For me, there is no ambiguity, I know for a fact it is wrong, because I know for a fact what is the correct road.

Quantoken

22. **Quantoken**
December 29, 2004

Mr. Dr. Lunsford said:

"On the other hand you have the real crackpots like this Q, who think that physics amounts to dicking around with physical constants. No wonder the Bon-Motls of the world are so entrenched."

I am sorry. I did not "dick around with physical constants". I showed you the relationship between alpha and G. This relationship does not come from numerology coincidences.

It's true there are bunches of crackpots playing numerology. But the relationship I pointed out are DERIVED from the fundamental principle of quantum information conservation. I have not shown any one exactly how they are derived, for obvious reasons I do not want any one to take credit for it. My caculation of universe "age", CMB temperature, baryon density, Pioneer anormality acceleration, etc. are all derived from the same principle. They are not numerology conincidences.

You can call Paul Dirac a crackpot when he played numerology with G, without explaining why the relationship holds. But I have found out the "why" and have derived it rigorously from the quantum information conservation principle. So it's not numerology, but real science.

"quantum information conservation" is one of the fundamental principles of my QUITAR theory. It has solid observational support.

I would not want to discuss QUITAR any more on Peter's blog. But I have to do it this time since you attacked me first. Again I am not going to elaborate or go to details.

Quantoken

23. **Peter**
December 29, 2004

A few comments about Witten, since I think my point of view about him is slightly different than most people's.

1. Yes, he's a genius. It's simply an awe-inspiring and humbling experience to read many of his QFT papers. Some of his string theory papers are also extremely impressive, even if one believes their motivation to be misguided.

2. He's a physicist, not a mathematician. What he really cares most deeply about is making progress on understanding fundamental physics. His accomplishments towards this have been modest, probably not enough to get him a Nobel prize. But you have to keep in mind that, since 1973, no one else has made any really significant progress in this area. The standard model is just too good, and no one has yet had a successful idea about how to go beyond it.

3. His problem isn't too little physical intuition. He decided to pursue aggressively the quite physical idea of string theory unification, based on a plausible hope in 1984 that this might work out. The reasons for believing in this were physical, not mathematical. His great failure has been an inability to recognize that, while initially plausible, this idea really doesn't work. The best historical analogy is to his predecessor at the IAS, Einstein, who initially had good reasons to hope for a unified theory using extensions of Riemannian geometry. Einstein also made the mistake of pursuing an idea long after he should have realized it didn't work.

24. **Quantoken**
December 29, 2004

Mr "" said: "The "establishment camp"? That must be the thousands of …thats why technology works…"

You are playing word games with me again. You know full well who I refer to using the "establishment camp" when I say "for 20 years". They have nothing to do with today's technology that we use daily, that are developed by scientists and engineers working on real problems relevant to this world. Your camp doesn't.

You whole post did not contain a single clause disputing the physics issues I argued. The only thing you meantioned relevant to Lubos's paper is that you say Lubos wasn't using QM Schrodinger Equation.

http://www.arxiv.org/abs/gr-qc/0212096

You are wrong. The very first equation (1) Lubos wrote in his paper is Schrodinger Equation. And in the very first sentence right after that equation, Lubos called it the "Schrodinger Equation". You must have came from another planet where Schrodinger Equation means something differently. Or your brain must be re-wired in such a weird way that you are no longer speaking English in its regular meaning.

Physics is a science about our observations of this universe, not about mathematics we think about in our minds. That's a physics 101.

Quantoken

25. **D R Lunsford**
December 29, 2004

God, on the one hand we have to deal with the Witten cult, and on the other, the Qs.

Is there some way we can annihilate all the crackpots against all the string cultists? This would leave a world delightfully free of posers.

-drl

26. **Quantoken**
December 29, 2004

Mr. "" said: "quasi normal modes were invented by relativists back in the 60s or 70s, they have a precise MATHEMATICAL definition, if you want to say something meaningful, you don't go around guessing what the term may mean, you make the effort and understand that precise mathematical definition. For that you may need to calm down first…"

Exactly, you can define your so called quasi normal state MATHEMATICALLY any way you want, as long as your math is self-consistent. But it's not physics. In math, any thing self-consistent is correct and OK. But physics is more than just self-consistent. You can derive a set of self-consistent "physics" laws in a presumed 1 dimentional universe mathematically but it's not physics.

Quantoken

27. **D R Lunsford**
December 29, 2004

Yes? And with what results? Do we know any more about the electron that post-Dirac? Neutrino post-WSG? No. Whatever Witten is doing – and frankly I don't really care – it's not physics. It may be math of crystalline perfection, but we have the real world to deal with.

-drl

28.
December 29, 2004

> I see almost no evidence of physical intuitive genius in Witten.

We know. But that's not Witten's fault. He is the one who has most deeply thought on quantum field theory.

29. **D R Lunsford**
December 29, 2004

To {anonymous lurker} –

When in school I spent countless hours poring over the journal stacks, going back into the 1800s. In this way I got a complete overview of the actual evolution of physics as it appeared in the journals. In this time I came across a large number of physicists, both famous and obscure. One of the obscure ones I'll mention off hand was a fellow named Jan Weysenhoff, a young Pole who died fighting the Nazis in the Warsaw uprising. Any single ONE of his physics papers is infinitely more interesting that ALL of Witten's physics output based on strings. There simply IS no physics content in string theory – it's a math game that does not in any way correspond to what is real. Therefore, in my mind, as a physicist, Witten is a complete bust – I find his work no more or less interesting than that of Hardy. If I had infinite time I might attempt to appreciate him from the mathematician's standpoint, as Peter does. I'm assured by Peter that Witten is a great mathematician – and I have no reason to doubt it. But, again, Weyl was a greater one, and what HE wrote regarding physics was interesting AS PHYSICS ITSELF, because Weyl not only had a Jupiter-sized brain, he had a powerful and nearly faultless intuition. I see almost no evidence of physical intuitive genius in Witten.

30. **DMS**
December 29, 2004

Well, Witten is a very good mathematical physicist. His papers are very clear and fun to read. But, in terms of impact (no I don't mean citations) he is by *NO MEANS* an Einstein (actually, John Schwarz likes to compare him to Newton, not mere Einstein) when it comes to physics, who actually did things which shaped the course of fundamental physics. And he is by no means the greatest mathematician, by *ANY* strech to imagination. There are plenty of other 20th century mathematicians who are also outstanding, and they would all be easily humbled by someone like Gauss who tackled several mathematical disciplines with ease (In contrast, all of Witten's beautiful work follows from QFT).

Witten, THE pied piper of the ST camp, has been making grandiose claims about string theory for two decades now. What has string theory to show for in terms of physics??

As Anderson correctly notes "The position is called "naive reductionism" and is typified by Witten's curious remark that "every exciting discovery in physics follows from string theory".
String theorists like to boast a lot."

Oh, but it is Witten saying it, so it must be true.

31.
December 29, 2004

Now, here is somebody calling Witten pitiful.

Reminds me of the well known phenomenon that beginners tend to disproportionally overestimate their total understanding of physics. As proven by others on this blog, too.

Learning nowadays is a humbling experience.

Like that guy who recently asked: 'How can ST have a classical limit when it makes no predictions?'

Answer: Sit down and try to understand what you are talking about and – in particular – what you are criticizing.

32. **D R Lunsford**
December 29, 2004

JC –

I don't know what else to call it, and in fact it has in common with literary criticism that what you say is less important than to whom you say it, and with how much bravado and flair. See l'affaire Bogdanoff for an example, complete with breathless groupies.

On the other hand you have the real crackpots like this Q, who think that physics amounts to dicking around with physical constants. No wonder the Bon-Motls of the world are so entrenched.

I watched three interviews last night in turn – Seiberg, Witten, and (what relief!) Dyson. Witten in particular is a total mystery to me. Here is a person with a staggering intellect, completely incapable of doing real physics. Compared to Dyson he seemed rather shallow and pitiful, despite his Jupiter-sized brain. He was more than willing for the interviewer (Ira Flato of NPR) to call him the "next Einstein". What a laugh.

-drl

33.
December 29, 2004

"The establishment camp have been playing word games in a confusing way. For 20 years you have found nothing physically significant but invent a bunch of terminologies that no-one knows what you are talking about. You have forgotton the basics and need to take physics 101"

The "establishment camp"? That must be the thousands of dedicated and hard-working physicists, engineers and scientists who have slogged their way through very difficult and demanding degree courses, sitting hundreds of exams, sitting in 4-5 hours of lectures a day and doing thousands of tutorial problems. Then went through the gruelling process of going through graduate school, learning to do research and then defending a phd thesis against very intense scrutiny. Then had to get their papers peer-reviewed and published. That is…those that actually did the work and all the hard study! Not all of them end up doing theoretical stuff you can't prove like string theory or cosmology–many, the majority, use their knowledge in applied physics and engineering to create and extend all the technology we have today. It is not perfect by any means but is the best way there is and it gets there in the end.

Thats why science works; thats why technology works. If the universe could be understood using only "physics 101" and if all the physics-based technology that exists could be created using only "physics 101" then you simply would'nt need to be educated beyond high-school level physics and maths. The "establishment camp" also deduced and "established" all the physics basics you are so very fond of so why should you believe them at one level but not at the other? Also, you can't condemn the "establishment camp" on the one hand and them on the other expect them to accept "your theory" and the arrogant way you present it.

The terminologies and technical jargon are usually straightforward and obvious to anyone who has done the work or who has at least done a good degree course. Terms like "quasinormal modes" for example ARE the basics! They have a precise mathematical definition. You don't just guess what the term might mean! Also, you don't need a black hole either to study them–just hit a bell with a hammer! This sort of math will turn up in engineering too so that suspension bridges and skyscrapers don't collapse and so on. It is not just some made-up abstract concept.

Even if the very theoretical-type physics is in a bit of a purgatory there have been many other advances these past 20 years in applied areas like condensed matter theory, superconductors, laser physics, quantum electronics and quantum optics, computing and a whole host of other areas. Physics as done by the "establishment camp" actually works! But they are still using the same quantum theory and physics concepts that underlines the more abstract and arcane areas like string theory that can't yet be proven.

You totally and completely misunderstood what the Motl paper was all about and the more you go on about it the more you show your ignorance.
He was'nt using a "wavefunction" or the QM Schrodinger equation. He was doing a classical analysis of black hole perturbations. Perturbation analysis of black holes and stars is a well-established area of astrophysics especially in establishing the stability of stars. I am sure there are people around this blog who would like to get one up on Lubos if he had made a mistake but I don't see anyone rushing to support your rebuttal because even vehemenently anti-string people can see you are talking BS. Besides, this thread is about the Strominger interview in India–why are you hijacking it with all this stuff? Put it on your own blog.

34.
December 29, 2004

quasi normal modes were invented by relativists back in the 60s or 70s, they have a precise mathematical definition, if you want to say something meaningful, you don't go around guessing what the term may mean, you make the effort and understand that precise mathematical definition. For that you may need to calm down first…

35. **JC**
December 29, 2004

"Postmodern" physics sounds like a cross between "new age" silliness with Beavis and Butthead. In many ways, "postmodern physics" sounds remarkably similar to economic forecasting.

"The only function of economic forecasting is to make astrology look respectable."
… (quote by economist John K. Galbraith).

36. **Quantoken**
December 29, 2004

Mr. "":

I know damped oscillation in English terms and in physics terms. But I do not know whatever a twisted alternative interpretation of terminology you may have in your camp.

Keep in mind wavefunctions in QM are different from classical definition of waves. It's a "probability wave". The phases in wavefunctions have NO physical significance. You can offset the Omega by a positive or negative number and it makes no difference. i.e., where is the zero point of energy is un-defined and can be set arbitrarily. That's a fundamental reason why QM is incompatible with GR, because GR requires an absolute reference point for zero energy.

Those are basic textbook stuff so dare you not question it. Once again, recite it exactly after me:

The phase in QM wavefunctions has no physical meaning!!!!!!

So, dare you NOT call the Omega in wavefunction using the term "oscillation"!!! An oscillation would have a physically significant phase and a physically significant frequency. But the wavefunction in QM has neither. Depending on how you arbitrarily define where the zero energy point is, the "frequence" could be offset by any arbitrary positive or negative number.

How could you call something that does not have a meaningful frequency "oscillation"? Calling such thing "oscillation" is really abusing the English language and twist the meaning of the terminology beyond recognition, and brings nothing good but logical chaos.

The wavefunction in QM is interpretted as a "probability wave". As such, the only thing that has physical meaning is the module of the wave function, i.e., the wave function multiplied by its complex conjugate. Integration of this module over all space must give a total probability of exactly 100%. That's the normalization condition of a valid QM wavefunction.

It is allowed that it transitions from one normal state to another. Such transition could show up as an imaginary term in Omega. Maybe that's what you call quasi-normal state. But such terminology is confusing, too. Such state transition itself is not a state, but merely a mix of two states.

In any case, the English prefix quasi- would mean something that's close, or something that transforms gradually and slowly. "Quasi-normal state" would have meant a state that is close to a normal state but undergoes a slow transition to another state.

In Lubos's paper, his imaginary part of Omega would be correspond to the order of Planck Mass scale. So the transition, or "damp", would happen in a time even shorter than the Planck Time. Calling such a quick transition "Quasi" whatever, is really abusing the English language.

Furthermore, any process that happens in a time shorter than Planck Time is simply none-physical. Because continuous time ceases to exist below Planck Time scale.

I do realize that probably the whole establishment camp is indeed playing the word games in such a confusing way. That only shows how crazy things have become. For 20 years you have found nothing physically significant but just invented a bunch of terminologies that no one knows what you are talking about. You have forgotten the basics and need to take physics 101 again.

Quantoken

37. **D R Lunsford**
December 28, 2004

He may be annoying and rude, but at least he's trying to think about things (without being willing to do any real work, it's true). I'm watching a TV show with Witten at this moment, who is reciting the string litany like a naive schoolboy mouthing his catechism. There is plenty of foolishness going around these postmodern days, among the great and the grotesque alike.

-drl

38.
December 28, 2004

No he has'nt heard of damped oscillations…and it is very clear from the stupidities he wrote that he does not understand black hole perturbation theory at all; nor does he understand what a quasi-normal mode actually is, and why by it's very nature it deals with complex frequencies; nor does he understand that schrodinger-type equations can arise in other areas of physics.

39.
December 28, 2004

Partial differential equations of the Schroedinger type are very common in classical physics, e.g the heat equation. They don't have to do anything with quantum mechanics, for example one can study the classical dynamics of a perturbed black hole. The black hole is stable- any perturbation thereof will lead to damped oscillations, which lead to the notion of quasi-normal modes. Those are complex by their very definition.

40.
December 28, 2004

Quantoken – ever heard of damped oscillations?

41. **quantoken**
December 28, 2004

Correction. I think I remembered the formula wrong. The units do not work out. The correct formula is:

V = sqrt(gravity*height)

I did say that "Further, if he even take a hot tub bath and swimmed in a swimming pool, he should have know that the wave propagate SLOWER in SHALLOW waters."

That's exactly correct that at deeper water the wave propagates faster, and when it reaches the coast it slows down. That's why the later and faster waves would push earlier but slower waves, and they pile up to create high tides that is unseen in the deep oceans.

Quantoken

42. **Quantoken**
December 28, 2004

Mr. "" said:

"You don't assume G either-G is directly related to the inverse string tension alpha'.
No-one "secretly injected G". You can get the Einstein equations out without assuming anything about G or even the Planck scale."

That's the whole point. That's exactly where you secretly injects the assumption of gravity. The "string tension alpha" has got to be inverse proportion to the size of the string. The smaller the string is, the higher the tension will be. You arbitrarily set the string size to be about Planck Length, that's where you introduced the G into the string tension, because Planck Length depends on G!!!

Tell me why strings HAVE to be of the size of Planck Length, not more and not less. Please give me a reason which does not contain the assumption of gravity. You can't.

The situation of Einstein is different. He never claimed to have "derived gravity". All he did is "describe" the gravity (in a way better than Newton). Since he simply answered the question of "how gravity works", but not the question "why there should be gravity", he is not responsible to explain where he got his G.

For string theoreticians, since you claim you have derived gravity, you have to show how you derived gravity, without prior knowing the existence of gravity. By secretly injecting G using the Planck Length, you are really not deriving gravity but simply play magic and "create" things that was never created on the stage.

Now, about Lubos's mistake in his famous paper deriving the 4*ln(3) "quantum of area". I now know he simply does not have the minimum level of basic physics training that one would expect for an assistant professor of any school, least to say Havard University.

This is reflected on his recent comments on his Blog, regarding Tsunami waves and large asteroid hitting earth. His physics instinctions do not seem to give him the ability to make judgements when an "Astronomy educator" gave an answer which is generally correct for very small asteroids but wrong for big ones. (the astronomy educator said stony ones would melt but metallic ones would not.)

Nor did he seem to instinctly realize that the propagation speed of ocean waves does not depend on frequence, wavelength or amplitude, but instead depends merely on the depth of water. The formula is roughly V = sqrt(density*gravity/height). It's actually a high school physics problem that any one can do a rough calculation.

Even if he does not know the exact formula, he should have the instinction from common sense accumulated from life experience. If you throw a big stone or a small stone into a small pond, the wave propagate at the same speed. So it's independent on amplitude, contrary to Lubos's claim. If you tap the water slowly or in rapid pace, it makes no different in the propagation either, so it is also independent on wavelength or frequence. One with basic physics training knows a phase speed dependence on frequency means dispersion, and we clearly do not see dispersion in water waves. Further, if he even take a hot tub bath and swimmed in a swimming pool, he should have know that the wave propagate slower in shallow waters. It's amazing that a physics Ph.D. would lack such basic physics instinctions, after receiving the basic physics training.

This lack of physics instinction is also reflected in his 4*ln(3) paper, which leads to a fatal mistake, which made his paper complete nonsense. Any one who studied the basic quantum mechanics and knows Schrodinger Equation should know, without having to look into textbook, that in the wavefunctions, the X (position) exponent coefficient can be an imaginary number, meaning it is a none confined and propagating wave. Or it can be a negative real coefficient, meaning the possibility of finding the particle at remote distance decreases exponentially, and hence it is a confined state.

Now the T (time) exponent coefficient, in exp(i*omega*T), The frequencey omega can be either positive or negative, meaning positive or negative energy, since energy = hbar*omega. But omega could never be an imaginary number or contain an imaginary part. The only exception is when you have slow transition between two states, then you may have an omega containing an imaginary part. Or, you can have an imaginary part but let that part approaches zero, just for the convenience of math calculation.

The reasons you can not have a frequence that contain an imaginary part, are two. One: The energy can be either positive or negative, but never imaginary. Two: more important, the wave function is a probability wave. At any given time T, you integrate the module of the wavefunction over the whole space, and it should give you exactly 100% probability of finding the particle. Once you introduce an imaginary part into omega, then the probability integration becomes time dependent and is not always 100%, which is not allowed.

That's where Lubos made his big mistake. He introduced an imaginary part into the frequence Omega, thought it is OK mathematically. But that is simply not allowed in physics.

Further, how silly he is, he called that Omega with an imaginary part some sort of "damped oscillation", "quickly brings the black hole into a SPHERICAL SHAPE". I can't help laughing when reading that. That omega in Schrodinger Equation has nothing to do with oscillation, and it certainly does not "damp". "damped oscillation" would mean the oscillation would eventually come to a complete stop. What does that mean in quantum mechanics? It's total nonsense.

Lubos does not have the "order of magnitude" instinction either. He should have realized that when talking about the black hole, the smallest mass M involved would be at least Planck Mass, in most cases it will be several times or many times more. Correspondingly the omega correspond to a time scale much shorter than the Planck Time. If it is a sort of "damped oscillation", obviously it will damp out to complete stop in a time much shorter than the Planck Time. We know time is really meaningless when the "time interval" is smaller than Planck Scale. What hell of a relaxation process was Lubos taking about?

Actually one really should not use classical quantum mechanics methods like Schrodinger Equation to study singular blackholes the size of a few Planck Mass. The mass is too high and the corresponding frequency corresponds to a time scale even shorter than Planck Time. We know at Planck Scale both GR and QM would cease to be workable because space and time is no longer continuous. One really should not push what we know at a larger scale to the limit of Planck Scale.

I am sorry to say, I expected better from an assistant professor at Havard University. Those people are supported by tax payers like me and the public do deserve to see what their tax dollars are worth.

Lubos's paper is at:

http://www.arxiv.org/abs/gr-qc/0212096

Quantoken

43.
December 27, 2004

The motion of the simplest bosonic string is goverened by the generalised nonlinear sigma model action. The requirement that the theory be free of Weyl anomalies (to lowest order) leads to the renormalisation group beta functions governing the physics on the world sheet. The resulting field equations are exactly those that can be derived from an Einstein-Hilbert action (coupled to a scalar, the dilaton, and an antisymmetric tensor field, the axion). GR emerges without really looking for it. You can't avoid it.

You don't assume G either-G is directly related to the inverse string tension alpha'.
No-one "secretly injected G". You can get the Einstein equations out without assuming anything about G or even the Planck scale. Even if you don't believe in string theory as a TOE it is the only theory that makes any kind of sense as a potential quantum theory of gravity. No a-priori assumptions at all are made about what the low-energy effective theory should be like, yet you recover it; the Hilbert space appears to contain black-hole like objects, what T Banks calls "asymptotic darkness",and you get some sort of sensible result for graviton-graviton scattering. Even when you take a basic relativistic string and quantise it the spectrum automatically contains a massless spin-2 mode.
When he realised this Witten called this "the biggest intellectual thrill of his life".
These facts put it head and shoulders above all other candidate quantum theories of gravity. It does'nt neccesarily mean string theory is correct but makes it look a whole lot righter and natural than any alternative theory of quantum gravity that you can come up with. LQG can't reproduce Einstein gravity in the large scale limit even though it begins with GR.

I eagerly await the comic relief "explanation" of the "mistake" of the Motl paper even though the reviewer is clearly way out of his technical depth.

44. **Quantoken**
December 27, 2004

Chris said:

"Repeatedly posting a crank theory after being repeatedly asked to desist. I think I now understand why Quantoken has been banned from other physics discussion groups."

Why I was "banned" on http://www.physicsforums.com had absolutely nothing to do with repeatition. I posted there a total of exactly 3 messages, which were actually three parts of the same article since I have to partition it to limit the size of each message. There was no repeatition.

I was not even sure whether I was banned or there was a technical problem. Repeated email inquiries asking what happened to my account and why never gets any response or explanation. So I have to assume I was banned and it must be for political reasons inconvenient for the message board host to explain.

Further, I am not "repeatedly posting my theory." Upon Peter's request, I have stopped talking about the physics details of my theory and what it is based on and what its predictions are.

Currently the only time I ever "talk" about my theory, is simply a reference to its name and its existence. I never elaborate. Are you saying I shall refrain from even meantioning the name GUITAR, the same strict way ancient Egyptian King condemned that the name "Moses" shall not be uttered from any month of any person, or the punishment shall be death? That would be worse a cersorship than the middle age, at which time they at least allowed Galileo to publish his book.

If any PhysicsForums host is reading this message, I demand an explanation to what happened to my account. If you indeed banned me, it's OK. But at least an explanation needs to be given to me why and what happened, and what it takes to remove the ban. Ny repeated inquiries falled into deaf ears.

Quantoken

P.S. I am not forgetting about pointing out Lubos's fatal errors in his well known 4*ln(3) paper yet. I just have not got the time to write it yet. Once I get a little bit time I will turn around and post it here. Mean while I still want to give Lubos a little bit time to find his own mistake, so he wouldn't lose too much face when it's eventually disclosed.

45. **Chris Oakley**
December 27, 2004

"… My theory, the GUITAR theory, is the only theory capable of deriving gravity in this fashion. I derived the G from first principle using nothing but just known parameters of one electron, including the fine structure constant alpha and the electron mass …"

Repeatedly posting a crank theory after being repeatedly asked to desist. I think I now understand why Quantoken has been banned from other physics discussion groups.

Returning to the main topic, "the only realistic possible unified theory of all the fundamental forces" is an oft-repeated selling point for superstring theory. However, if I understand Arun's comments correctly then although the spin 2 particle is incorporated, this does not necessarily mean that we reproduce gravity in the required way. It is a bit like saying of a lump of quartz, "There's no reason why this should not be the most advanced microprocessor yet manufactured – it's just that we haven't quite done it yet."

46. **Quantoken**
December 27, 2004

Sorry Arun, I did not intentionally quote your words out of context. I saw it that way in Chris's original message and did not realize it was only portion of your original sentence.

Regarding background dependence/independence. It is true that super string theory violates the GR's background independence principle. But that can not be interpret as evidence that they did not secretly inject gravity in the first place to derive the result of "predicted the existence of gravity." They can pick and choose just part of GR as their input.

The fact remains that they assumed the existance of gravity in the first place (by merely using Planck Length) in their theory. So how could they later claim they derived gravity?

They surely have not derived the GR, at least the GR we known from Einstein. How could a theory start with background dependence, and end up with a background independent result?

Neither the super string camp nor the LQG camp is very honest in that both secrectly injected gravity by using Planck Length in the first place, and both later claim they derived gravity.

You can bet your money that when you see a magician "created" (or "derived", for that matter) something on the stage, it must be something previously brought to the stage before the performance, even though you may not know the detail how the magic works.

Quantoken

47. **Arun**
December 27, 2004

Since what I wrote was:

"I guess the SuperStringers' attitude would be – we know that General Relativity is correct, because it emerges out of string theory"

I don't know what all the arguments are about. The string theorist would almost certainly say that General Relativity was not an input into string theory; that in fact, starting the study of string propagation on a fixed background space-time goes against the spirit of General Relativity, and is one argument that LQGers use against string theory.

48. **Quantoken**
December 27, 2004

Chris asked:
"we know that General Relativity is correct, because it emerges out of string theory"

Is this true? How can a theory that makes no predictions have a classical limit?

I say: the statement in quotations is incorrect because it states an incorrect causal relationship. It's the other way around. GR emerged PRIOR to string theory. The correctness of GR has nothing to do with string theory. GR is what it is, regardless whether string theory is right or not.

On the opposite, the correctness of string theory has to depend on GR. That is to say, since GR is so far believed to be correct, the string theory MUST derive GR as a necessary but not sufficient condition to be a correct theory. That is to say, if string theory can not lead to GR, it is then definitely wrong. But if string theory does lead to GR, it still may not be sufficient to say it is correct.

Now, to explain what it means string theory predicted GR in layman's term. It is indeed true that you will get GR out of string theory. It is also true LQG also leads to GR. Actually it is true that ANY theory that works on discrete space-time at Planck Scale, would necessarily leads to GR.

Because GR was a secret input parameter to those theories in the first place. Certainly you will get GR at the printout end if you have entered GR on the input end in the first place.

To put it this way, string theory suggests there exists such little tiny stringy ringy thingym in addition to the regular 3 space dimention and 1 time dimention. That little ringy is only one Planck length in size. Thouse little rings would necessarily have some effect of curving the regular 4-D spacetime in large scales. And that is what we call gravity.

But because the rings are small, the gravity is also weak. The smaller the rings are, the weaker the gravity will be. Needless to say, if the rings are infinitely small, then gravity will be zero. Let's say the gravity effect strenth will be roughly proportional to the area of those rings. That happen to be about one Planck Area, which is roughly the square of Planck Length, i.e., G*hbar/C^3. So gravity is exactly proportional to G.

Surprise! Surprise?!

You see where GR is secretly punched into the keyboard in the first place to get the GR output? To obtain the correct gravity, string theoreticians have to make their stringy ringy's not too small and not too big. And the size just have to be about one Planck Length. And the definition of Planck Length happen to contain a G. And the G, of course, came from GR in the first place.

I really do not think that can be called "predicted gravity" if you secrectly entered G in the first place.

To really predict GR, you have to start with absolutely no knowledge of G and at the end you have to be able to derive G out of your theory. It should NOT be allowed that you secretly enter a known G into your formula some where in the middle.

My theory, the GUITAR theory, is the only theory capable of deriving gravity in this fashion. I derived the G from first principle using nothing but just known parameters of one electron, including the fine structure constant alpha and the electron mass.

Next time you see a theory working on Planck Scale to start with, and claim to have derived GR at the end. You know they are cheating since G was enter secretly using Planck Scale.

If any stringy theoretician has a different opinion, then I want to ask, Do you have a legitimate reason you have to make your stringy thingy no bigger and no smaller than one Planck Length, other than just to cheat the computation and get the correct GR output?

Quantoken

49. **plato**
December 26, 2004

we know that General Relativity is correct, because it emerges out of string theory"

I was taken back by this as well.:)

I see Arun is answering, but the conceptual framework just does not seem to be clicking for some:) I have been incubating this for a couple of days and will be posting tomorrow, respective of the positions, Peter and Lubos have exemplified about the knowledge base that allows further developement by competent indivuduals in their respective areas. I'll be more specific then.

Just let me say that the quantum harmonic oscillator, if thought of as, in referrence to the holographical point on the brane, the relevance of Gr would have found value to me in the quoted statement above. I'll be more specific to this as well tomorrow.

Some might pre-empt me here if they wanted too, and expand on that point:)If both Peter and Lubos have a basis from which to speak, then what would happen if such manifestation of this thinking expanded into our world?:) How would we see the new concepts show theselves in the physics as some fractorial representation(Ramanujan) that would suit the common people?

I 'll answer this as well:)

50. **Arun**
December 26, 2004

Chris O.,

If one studies the propagation of strings in a background space-time metric then one finds a consistency requirement that the metric must follow the Einstein equations in vacuum (R_{mu,nu} = 0) at the one-string-loop level. There are stringy corrections at the 2-loop level, which are not relevant in ordinary conditions.

String theory is capable of subsuming our current theories, and also describing the limits of applicability of current theories. The problem is that where its predictions are firm (10 dimensions or stringy corrections to GR) they are inaccessible experimentally; and where its predictions are (potentially) accessible (e.g., low energy Super Symmetry) they are not necessary; i.e., if low energy SUSY is not found, then that does not invalidate string theory, because almost certainly string theory can accomodate not having low energy SUSY.

The to-us-experimentally-accessible-universe is not uniquely determined by string theory; and there perhaps is no experimental finding in our current capabilities that can rule out string theory.

51. **Chris Oakley**
December 26, 2004

"we know that General Relativity is correct, because it emerges out of string theory"

Is this true? How can a theory that makes no predictions have a classical limit?

52. **Arun**
December 26, 2004

Chris W.:

The point is well taken about how exactly theories are subsumed into other theories – how the wider theory explains the successes of the lesser theory and specifies under what conditions the lesser theory fails.

But if you ask your friendly neighborhood string theorist, and it seems inconceivable to them that string theory could be extendible in a similar way. I've already quoted to that effect. Perhaps it has to do with the converse of something else you wrote: "Any such theory that has empirical content also has the potential of being contradicted by observations."

To take up your example of the anomalous acceleration detected by Pioneer, I guess the SuperStringers' attitude would be – we know that General Relativity is correct, because it emerges out of string theory, so these experimentalists who propose spending hundreds of millions of dollars on a spacecraft experiment are stupid hacks who should not be paid to be scientists.

Maybe I'm being unjust.

53. **Quantoken**
December 25, 2004

Lubos erased my post on his blog which declared a fatal fraud in a paper he published, claiming that I do not understand mathematics enough to be able to find his mistakes and that I am not going to say what it is.

The fact is he could not find his own mistake even after I gave him a hint. Of course I am going to point out exactly what his mistake is, I said I will disclose it after Christmas. And of course Lubos is the one who does not know math. Do you have any idea what is elliptic curve, Montgomery multiplication, CRT, etc in mathematics? Don't know, do you?

Doesn't matter. Mathematics matters only where it matters to the actual physics. Your mistake in the paper, Lubos, is NOT trivial math or computation error. Your mistake is you used the wrong math in the wrong physics.

Would you tell me what is the value of Hawking Entropy or the Schiwaltzchild radius of one single hydrogen atom? Or would you tell me what is the temperature of the electron that circles the proton in a hydrogen atom?

Being an idiot like Lubos is, he will go right away and copy the formulas from textbook and plug in numbers to calculate. I am sure he will double check his math to make sure he did the calculation right. But still he will give you the wrong answer!!!

The point is the physics is no longer right when you push the physics/math formulas beyong where they should be used. You really can't calculate the Hawking Entropy of a hydrogen atom using that a quarter Planck Area formula. Because the hydrogen atom is too light to form even the smallest possible blackhole. And in a blackhole of any size, there could not possibly be any atom. You can't use another formula to calculate Hawking Entropy of a hydrogen atom either. The concept of Hawking Entropy simply do not apply to hydrogen atoms.

Now, I have given you enough hint why you made a fatal mistake in your original paper. Go figure. I will tell you after Christmas, if you can't figure out by then.

Quantoken

54. **Quantoken**
December 25, 2004

Ah ha, Wow!

Lubos was totally wrong in his ARXIV paper deriving the quantum of area 4*ln(3). See his paper:

http://www.arxiv.org/abs/gr-qc/0212096

The whole thing was wrong for a very simple reason. That's a mistake that he really should not have made if he has the least of physics instincts. At least I never expected a guy who was a Harvard assistant professor make such a mistake. The paper is 25 pages long but it took me no more than 5 minutes to find where he made a fatal mistake.

Now, Lubos, go find a spider hole first. Because once I tell you the mistake you made, you would be so ashamed of yourself, that you want to dig yourself a hole to hide in.

Had you shown a little bit respect to me before. I wouldn't have choosen to disclose your mistake publicly, and would instead email you in private. Or at least post it in your Blog only.

Oh well. Too late, you are going to be really embarrased this time. To give you a chance to save face. I will give you a hint first, see if you can find your own mistake before I tell you.
The hint is: what is the numerical value like of frequence Omega that you are talking about in your paper?

I will tell you where you made a fatal mistake in your paper, after Christmas.

Mean while, every one, Merry Christmas!

Quantoken

55. **Chris W.**
December 24, 2004

Arun,

I'm sorry for not responding more promptly to your comment. First of all, let me quote your main points:

> *The idea is, I suppose, that we can only show that a physical theory corresponds to reality to a certain precision within a certain domain. We can never rule out the possibility of a theory with wider validity, just as Newtonian gravity was replaced by and subsumed within General Relativity. So proof of a theory is not possible.*
>
> *But the Theory of Everything (or at least Superstring Theory) is different. It either applies to the real world entirely and wholly, or not at all. And here is what a professional physicist, Lubos Motl, wrote on his blog : "I am always a bit puzzled by Andy's [Strominger] statements that string theory is "just another step" – what sort of other step that goes "beyond" string theory but does not invalidate it is Andy thinking about?"*

I would amend your first remark to the following: "We can never be sure that all testable consequences of a theory will be confirmed by observation; proof of a theory is not possible. Our past experience gives us reason to *hope*, however, that a theory that conflicts with observation will eventually be subsumed within a theory of wider validity *that explains these failures, and specifies the conditions under which the failures will occur*."

More precisely, both the successes and failures of a theory are observations—empirical facts about the results of tests of the theory. Such facts call for explanations. We don't want to dismiss the success of classical celestial mechanics (for example) as meaningless or inexplicable. Indeed, part of the problem that Einstein set himself was understanding such successes, once he realized that he must reconsider the intertwined foundations of Newton's mechanics and Galileo's relativity principle in order to reconcile them with Maxwell's electrodynamics.

Many physicists and philosophers of the 19th century were ready to believe that Newton's mechanics and theory of gravitation constituted a "theory of everything", with electrodynamics relegated to the status of a continuum mechanics of the mechanical ether. The necessity of subsuming 19th century physics into a deeper and broader framework only became clear in retrospect, as did the crucial roles of the speed of light and the quantum of action, and the irrelevance of the classical idea of an ether.

So in response to your second point, nothing is fundamentally changed by considering a theory as a candidate for a "theory of everything", ie, a theory that is *presumed* to constitute a shared basis and explanation for everything we have observed about the universe, including the successes of any previous theories. Any such theory that has empirical content also has the potential of being contradicted by observations. Of course, one can try to interpret the theory so as to evade the contradictions, but one always does so at risk of destroying its empirical (as opposed to formal or mathematical) content. The most profound discrepancies are those that leave one unable to offer any scientifically defensible explanation within the framework of the theory. Either we accept them and admit that the theory fails to be both all-encompassing and scientific, or we set about undermining the theory's scientific status in order to preserve its metaphysical authority. (This is not to say that metaphysics can or should be avoided under all circumstances, which is itself a pernicious attitude.)

—-
It is worth reviewing the efforts of John D. Anderson, Slava Turyshev, and their collaborators to characterize the Pioneer spacecraft trajectory anomalies as a reminder of what a fundamental discrepancy with theory might look like, and how much effort is involved in eliminating auxiliary hypotheses that might explain it. (Anderson, Turyshev, and Michael Nieto are now advocating a new mission to the outer solar system designed specifically to investigate this problem. The Pioneer spacecraft have ceased communicating; Pioneer 10's last contact was in January 2003.)

56. **Matti Pitkanen**
December 24, 2004

I am afraid that string theory involves so many
wrong assumptions that there is no hope of making
progress by deforming algebraically a little bit. It would be better to take a more philosophical attitude, and look carefully for the strengths and weaknesses of string theory and how to possibly cure them. Here are some opinions about what "going beyond string theory" might mean.

1. Wrong realization of super-conformal invariance

Appropriately generalized super-conformal invariance is very probably something shared by any future theory. Personally I see the realization of conformal invariance at two-dimensional string world sheets un-necessarily restrictive and as the fatal flaw of the string theory.

The way out of dimension 2 is based on simple observation: light-like 3-surfaces of Minkowski space (or any 4-D space with Minkowskian metric signature) allow generalized conformal invariance by their metric 2-dimensionality and the dimension 4 for space-time becomes unique.

Also generalized symplectic structure is possible for light-like 3-surfaces, and the group of isometries for light-like surface consists of combinations of conformal transformations and compensating local scalings in light-like direction and is thus infinite-dimensional. Thus light-like 3-surfaces are really incredibly beautiful structures and I find difficult to understand why string theorists refuse to take them seriously. Also a new view about super-conformal symmetry emerges and no space-time super-symmetry (sparticles) is predicted.

2. Spontaneous compactification and Kaluza-Klein
philosophy as the second fatal mistake

Personally I experience spontaneous compactification and Kaluza-Klein approach as something absolutely ugly. To me it is a purely ad hoc attempt to extend the explanatory power of
string model beyond its natural limits. The accompanying ad hoc assumption is that of space-time super-symmetry.

Instead of trying to make 4-dimensional classical world to 11- or 2-dimensional, I would prefer to start from a real problem of general relativity. In general relativity the basic conservation laws due to the isometries of Minkowski space are lost, and one can ask how to combine the good aspects of special and general relativities. This is achieved by identifying space-time as 4-D surface of some higher-D space of form H=M^4xS, M^4 Minkowski space. Poincare invariance lifted to H. S can be fixed to S=CP_2 by requiring standard model symmetries.

In this framework the higher-D space cannot be nor need to be dynamical since the dynamics is at the level of space-time surface, and the horrors of landscape are avoided.

3. Infinite-dimensional existence is unique

M-theory has led to inflation of all kinds of deformations and variants of finite-dimensional geometry. There is however a different direction that could be followed in the attempts to generalize the notion of geometry.

Taking the uniqueness of physical existence as a guideline, one ends up with the notion of infinite-dimensional geometry. Already loop space Kaehler geometry is unique as Dan Freed showed in his thesis. The requirement that Riemann connection does not lead outside the tangent space forces infinite-dimensional isometry group and makes Kaehler metric of loop space unique. There is still a problem: although Ricci tensor is finite, curvature scalar is infinite, a clear symptom that basic objects cannot be 1-dimensional.

The space of 3-surfaces in M^4xCP_2, which possesses a maximal group of isometries, is the TGD inspired guess for the world of classical worlds as the unique arena of quantum dynamics. This space would be a union of infinite dimensional symmetric spaces with vanishing Einstein tensor labelled by zero modes having interpretation as classical, non-quantum fluctuating degrees of freedom characterizing the space and size of 3-surface. This formal analog for landscape is in key role in TGD based quantum measurement theory.

M^4xCP_2 is a very promising candidate for imbedding space. The canonical transformations of VxCP_2, V light cone boundary, act as isometries of this space and super-conformal invariance in generalized sense appears naturally in the construction of the configuration space geometry.

Configuration space gamma matrices in fact have identification as fermionic generators of super conformal algebra so that both fermionic statistics and super-symmetry have purely geometric interpretation. An important deviation from string models is that super-generators do not correspond to Majorana spinors (this leads to dimension D=10 or 11 in string models) but carry quark and lepton numbers.

4. Tangent spaces of space-time and imbedding space as number field like structures

H=M^4xCP_2 is the only option allowed by Particle Data Table and the construction of configuration space geometry favors strongly this choice. One might however argue that the preferred role of M^4xCP_2 should have some deeper, perhaps number theoretic, explanation.

The dimensions 4 and 8 for space-time surface and imbedding space of course bring in mind quaternions and octonions, and CP_2 labels the quaternionic sub-algebras of octonions just like CP_1 labels the complex sub-algebras of quaternions. The first guess is that space-time could perhaps be thought of as being surface for which tangent space corresponds to quaternionic, or more generally associative, sub-space of octonions at every point so that classical dynamics would reduce to number theory. The problems relate to the Euclidian signature of the metric. Also the non-commutativity and non associativity are questionable features.

Perhaps a better guess is that the tangent space of space-time corresponds to H_4, the algebra and “almost number field” of 4-D hyper-complex numbers. The tangent space of imbedding space could in turn be regarded as the space H_8 of 8-dimensional hyper-complex numbers.

Quite generally, hyper-complex numbers have dimension 2^n which is the same as the dimension of Clifford algebras spanned by gamma matrices, and the natural requirement is that units allow a representation in the Clifford algebra of the space in question.

a) For H_2 the commuting units are 1, e1 with e1^2=1.

b) The commuting units of H_4 are 1, e1, e2 and the product e1e2, all of them having squares equal to 1.

c) For H_8 the units are 1,e1,e2,e3 and their products. One can decompose H_8 number to two parts h= h_1+e_3h_2, where h_1 are H_4 coordinates, the “real” and “imaginary” parts of h.

The number-theoretic norm of hyper-complex number h in H_m, m=2^n, is expressible as the product of all m conjugates of h and is the m:th power of Minkowski length squared of m momentum. Lorentz group leaves the norm invariant. The failure of number field property manifests itself as the existence light-like hyper-complex numbers having no inverse and forming an ideal of H_m. Remarkably, the spectrum of momentum squared is integer valued in the restriction to hyper-complex integers: nothing but the stringy mass squared spectrum.

From the p-adic viewpoint the hyper-complex primes are of obvious interest, and the construction of infinite primes generalizing the notion of finite hyper-complex prime is especially interesting since the correspondence of infinite primes with the states of super symmetric arithmetic quantum field theory becomes very concrete.

5. M4xCP_2 as a unique 8-D hyper-complex manifold which is a homogenous space?

Hyper-Kaehler manifolds generalize the notion of complex manifold to the quaternionic manifold. This means that tangent space allows the representation of quaternionic units 1, I_1,I_2,I_3 as metric and antisymmetric covariantly constant tensors defining symplectic and Kahler forms.

In hyper-complex case similar idea works. The basic idea is that the generating units e_i are representable as contractions of covariantly constant antisymmetric tensors with sigma matrices, which commute and have squares equal to unit matrix. The 2-forms associated with generating units define what can be regarded as a generalized symplectic structure.

M^2 is the simplest hyper complex manifold and e_1 corresponds in light-like coordinates u=t+z, v=t-z to antisymmetric tensor with a non vanishing component E^1_uv=1. The square of the contraction with Sigma_uv gives unit matrix in Clifford algebra. E_uv defines a Minkowskian analog of symplectic structure since its square is metric (rather than -1 times metric).

In case of M^4 the coordinates u=t+z,v=t-z,x,y corresponding to the decomposition M^4=M^2xE_2 to longitudinal and transverse degrees of freedom are preferred. The appropriate generalization of these coordinates to what I call Hamilton-Jacobi coordinates play a key role in the construction of solutions of field equations in TGD framework.

The units e_1, and e_2 correspond to antisymmetric tensors E^1 and e^2 with non vanishing components E^1_uv=1, E^2_xy=i. iE^2_xy defines symplectic and Kaehler structure in E^2. The contractions of E^i with sigma matrices obviously commute. Note that the flatness of M^4 is absolutely essential prerequisite for having two covariantly constant 2-forms. e_1e_2 is represented as a production of contractions of E_1 and E_2 with sigma matrices.

In the case of M^4xCP_2 one additional covariantly constant 2-form E_3 is needed and CP_2 Kaehler form defines it. M^4 coordinates correspond to the real part of H_8 coordinate and CP_2 coordinates to its imaginary part. Of course, more general choices of coordinates obtained by applying generalized canonical transformations leaving the forms E_i invariant.

The conjecture is that symmetric space property plus the existence of hyper-complex structure select M^4xCP_2 as a unique candidate for H. Obviously, the existence of symplectic structure in the generalized sense is essential for the existence of hyper-complex structure. For instance, the replacement of CP_2 with symmetric space S^2xS^2 would bring in *two* additional symplectic forms whereas S^4 does not possess any Kahler form. Does the hierarchy of hyper complex manifolds contain other manifolds than M^2, M^4, M^4xCP_2, M^4xCP_2xCP_4, M^4xCP_2xCP_4xCP_8,… is an interesting question.

6. Number theoretical classical dynamics

Despite the fact that hyper-complex numbers are not a field, the notion of analyticity generalizes and leads to the idea that the classical dynamics of TOE could reduce to 4-D hyper-complex analyticity condition. Indeed, in string model the solutions of 2-D d'Alembert equation can be expressed as hyper-complex analytic functions of a hyper-complex variable assigned to the string world sheet with Minkowskian signature of metric.

A very attractive idea is that space-time surfaces correspond to surfaces of H=M^4xCP_2 for which the "imaginary" part of hyper-complex valued analytic function (possibly satisfying some additional constraints such as being polynomial) vanishes:

Im[f(h_1+e3h_2)]=0.

By implicit function theorem this gives 4-D hyper-complex number h_2 as an analytic function of h_1:

h_2= f(h_1).

This is an attractive candidate for a purely number theoretical realization variational principle, which should in TGD framework has absolute minimization of Kahler action as counterpart. The challenge is to prove this.

Matti Pitkanen

57. **Quantoken**
December 24, 2004

Lubos said:

"When Einstein started to propose his EPR paradoxes, he was sure that quantum mechanics had to be giving wrong predictions for these experiments. It's not just a matter of philosophy. He believed that the quantum entanglement was impossible, and using the current terminology, he would definitely agree that Bell's inequalities must be satisfied in reality."

Einstein was wrong in his judgements about quantum mechanics. But he was right on the profound philosophical believe that the physical world is fundamentally deterministic. Quantum mechanics is certainly a very successful theory in making predictions. But few people believe it is a final theory due to the difficulty of reconciling the difference between philosophy and the quantum mechanical picture. That's why, even in 2004, people are still debating about EPR paradox and struggle to try to understand what experiments like Alan Aspect's really tell us about reality.

It's far from being settled even today. Nobody can boast to understand EPR completely. If you think you know, it only shows you have learned the formulations of physics but really don't understand it.

Now about the famous Bohr – Einstein debate about that gedanken light box experiment. I do not why no body has pointed it out so far. The fact is both of these two great men had made serious mistakes in their great debate.

Bohr had made a big mistake in a fraud in his arguments, which used Einstein's General Relativity to prove that the uncertainty principle
still holds. And Einstein, astonished that his very GR theory was used against him, did not realize that Bohr made a fraudulent usage of his theory in his arguments.

Actually if Bohr had been right in his reasoning, he would have had discovered a way directly linking GR with the QM uncertainty principle. We wouldn't be still struggling so hard trying to associate GR with QM today.

Bohr's mistake being he confused the two delta T's. One being the deterministic and predictable time dilation due to GR effect. The other delta T being the actual uncertainty of time, which is random and un-predictable. The two delta T are totally different and unrelated. But Bohr tried successfully in confusing Einstein by using the same terminology to describe these two different delta T.

At the end of day, the uncertainty principle still holds, and Bohr could have defeated Einstein using another simplier and correct argument, by considering that photons have wavelengths related to their energy. So it is impossible to release a photon of certain energy, if the shuttle is opened for too short a time.

Quantoken

58. **Quantoken**
December 24, 2004

Dr. Lunsford said: "My God, Lubos, gigawatt source of utter BS, has the gall to set himself up as a critic of Feynman."

I am sorry, Dr. Lunsford, but Lubos is not Your God 🙂 The remaining part of your statement is correct.

Quantoken

59. **D R Lunsford**
December 23, 2004

My God, Lubos, gigawatt source of utter BS, has the gall to set himself up as a critic of Feynman.

Hows does a person like LM exist? How is it that he is able to get a job ANYWHERE, not to mention one of our best universities? He does nothing other than run his mouth – has no ideas of his own – reveals his shallowness and arrogance at every stage – is perhaps the most annoying person I've ever encountered online. How did this zero get a job at Harvard?

-drl

60. **Lubos Motl**
December 23, 2004

Future limitations of string theory.

Arun, we exchanged a few sentences with Andy S. about it today – well, there were some more material topics, too. When you ask Andy what kind of the new steps "beyond" string theory one can imagine, he says "I don't know".

It's of course partly a matter of terminology – what you consider string theory. Is M-theory still string theory or not? I think that if you accept the terminology that string theory contains things as different as M-theory, then there are only two possibilities. String theory will be proved completely wrong, irrelevant for the real world, or it will be correct and the progress will only be within string theory itself.

There is a difference from relativity and quantum mechanics. You know, the Newtonian mechanics contained three independent units, let's call them meter, kilogram, second. All other quantities have units that are products of powers of these basic units.

For example velocity is in meters per second. Special relativity showed that length and time are really related, and Newton's physics only holds for velocities much smaller than the speed of light. You set c=1 in relativity.

Similarly, quantum mechanics uses hbar, and things only look classical if the typical quantities of the same dimension as hbar (like action, angular momentum) are much greater than one. Naturally in quantum mechanics you set hbar=1, and you end up with one independent unit.

Now you're in hbar=c=1 units of quantum field theory, and everything is in powers of a GeV, so to say. If you now include quantum gravity, it also sets G=1, and says that the previous physics only worked at distances much greater than L_{planck} etc. But in these quantum gravity units, all observables are dimensionless, and there is really no other inequality that you can violate to go beyond the reach of validity of the previous theory.

String theory itself tells us exactly what sort of deformations you can do with its backgrounds and what sort of deformations you can't. There just does not seem to be any way to deform "away" from string theory. It must be exactly what it is.

61. **Lubos Motl**
December 23, 2004

Nope, quantoken, you were probably skipping your classes of history of physics.

When I write that Einstein did not believe quantum mechanics, it means that Einstein did not believe quantum mechanics. Your attempts to look for errors in my sentences are counterproductive. You should better look at the serious errors with YOUR thinking.

When Einstein started to propose his EPR paradoxes, he was sure that quantum mechanics had to be giving wrong predictions for these experiments. It's not just a matter of philosophy. He believed that the quantum entanglement was impossible, and using the current terminology, he would definitely agree that Bell's inequalities must be satisfied in reality.

He tried to invent dozens of possible contradictions in quantum mechanics, and all of these attempts of Einstein were wrong. (Well, one of them was also helpful in the understanding of QM, but not in the way Einstein anticipated.) Every time an experiment showed that Einstein was wrong, he invented another possible problem.

Moreover, in the last 30 years of his life, he really ignored the new discoveries in physics – like in nuclear physics. When he was constructing his unified field theory (until his death in 1955), it was always just a unified field theory of electromagnetism and gravity because he never considered any of the details of the emerging quantum field theories seriously.

Your comment that you don't believe in any limitation of determinism even in 2004 does not surprise me a single bit. I've already learned the level of your knowledge and understanding of physics in more detail than what is necessary.

62. **plato**
December 23, 2004

Arun,

> *It either applies to the real world entirely and wholly, or not at all. And here is what a professional physicist, Lubos Motl, wrote on his blog : "I am always a bit puzzled by Andy's [Strominger] statements that string theory is "just another step" – what sort of other step that goes "beyond" string theory but does not invalidate it is Andy thinking about?"*

I can't speak for Lubos, and as a commoner, the thinking of the journalist would fall comparatively to my domain?:) So if the confusion rests, it would rest in the lack of interpretation that is derived from the roads leading too? We acknowledge the planck Epoch? The timing in Steven Weinberg's model of the First three minutes?

The conceptual difference as we now look at this model I wonder indeed how such thinking has been altered.We had to assume that this universe is cyclical in nature? So where do strings fit in? Gabriele Veneziano made this point clear.:)

> *It might be that the laws change absolutely with time; that gravity for instance varies with time and that this inverse square law has a strength which depends on how long it is since the beginning of time. In other words, it's possible that in the future we'll have more understanding of everything and physics may be completed by some kind of statement of how things started which are external to the laws of physics.*

Richard Feynman

If such a inquistive naure is not acknowledged then how would we move beyond the boundaries which we like to hold to in the physics of approach. You needed to have these frameworks which would motivate exploration of concepts and then bring back for us, new roads for consideration. Would one quickly dispell Smolins Three road attempt at comprehension?

Nima and others dimensionally have challenged these current views:)

63. **Arun**
December 23, 2004

Chris W. asks "Why in 2004 are we talking about proving physical theories?" in his criticism of Pathik Guha's newspaper report.

The idea is, I suppose, that we can only show that a physical theory corresponds to reality to a certain precision within a certain domain. We can never rule out the possibility of a theory with wider validity, just as Newtonian gravity was replaced by and subsumed within General Relativity. So proof of a theory is not possible.

But the Theory of Everything (or at least Superstring Theory) is different. It either applies to the real world entirely and wholly, or not at all. And here is what a professional physicist, Lubos Motl, wrote on his blog : "I am always a bit puzzled by Andy's [Strominger] statements that string theory is "just another step" – what sort of other step that goes "beyond" string theory but does not invalidate it is Andy thinking about?"

If you disagree, then certainly a newspaper reporter has more excuse to be confused than a physicist. 🙂

64. **Quantoken**
December 23, 2004

Mr. M-* said:

"Einstein did not believe quantum mechanics, even though he was a genius. There were fortunately many others who did. Feynman did not believe string theory, but there were others who did. You don't need to go far – Gell-Mann was the guy who created the strong Caltech's string theory group."

Why said Einstein did not believe in quantum mechanics. He certainly believed in the formulations of quantum mechanics and their ability to predict experimental results correctly. He just did not agree with the way how quantum mechanics was interpreted, philosophically. He believed in a deterministic picture of the world. Such belief is reflected by a phrase he said often: "God does not need to toss a dice." He thought that quantum mechanics is incomplete and must be deriveable from a better theory that is deterministic.

I believe in the same. Actually I already see what that deterministic better theory is. The physical world can be completely deterministic, but yet still none-predictable, because we observers are part of the universe and we can not obtain the whole information of the whole universe to be able to predict things completely deterministically. The job of acquiring and processing that amount of information would require one whole universe to do.

There are two kinds of believes. The kind of "seeing is believing". Which is scientific beliefs. And the kind of "accepting is believing", like believe in God or Budha. Talking about string theoreticians believing in string theory, without seeing any experimental support. That is "believing with no seeing", which I think can be classified into the religious beliefs.

Such a belief look even more like religious belief. Just like you never know whether there is soul and whether there is a God, until the very moment you die, and by then it is too late for you to change your religious belief. Many of the die hard string theoreticians also probably never will know for sure whether they were right or wrong, until they have spent their whole lifes, and probably until they go to heaven and meet Einstein there, and got the answer from him. But by then it's too later to change their beliefs.

65. **plato**
December 23, 2004

Santos,

Even Richard Feynman was not sure. When I complete posting on, The Gravity for Instance Varies with Time, you will understand what I am saying in regards to dimension and what even Richard Feynman did not realize.

Einstein limitations, were his views of what did not exist yet, and his refusal to accept quantum mechanics. We know different now, as Lubos mentions, as you now know, in context of what you quote from that Canto book.

You have to bring yourself up to speed on the subject?:)

66. **Chris W.**
December 22, 2004

Pathik Guha's piece struck me as a fairly sophomoric effort (and poorly edited to boot). Why in 2004 are we still talking about "proving" physical theories? Robert Geroch, in his beautiful little book **General Relativity from A to B** (1981), remarks early on that he can't imagine what would constitute a proof of a physical theory. Guha's careless formulation does nothing to clarify the issues now at stake in fundamental physics.

One of those issues (as Feynman said) is testability and avoidance of "immunizing strategems" when confronting the results or even the possibility of empirical tests. More specifically, in quantum gravity a central problem is how to meaningfully and consistently define observables, when spacetime structure is not only dynamical but quite possibly only an approximation to some deeper structure. From Raphael Bousso (hep-th/0412197, Introduction):

> One problem with quantum gravity is that we don?t know what the theory should compute. In particle physics, the most precise observable is the S-matrix. But this quantity seems ill-suited to cosmology, where the observer is not outside the system, initial states cannot be set up, and experiments cannot be arbitrarily repeated to gain statistically significant results.
>
> This ignorance is not especially unusual or embarrassing. It is rarely clear at the outset what a theory should compute. For example, the insight that gravity is a theory of a symmetric, diffeomorphism-invariant tensor field in itself already constituted a significant part of the development of general relativity. But once a theory is in its final form, the observables should be apparent.
>
> If string theory is the correct quantum theory of gravity, then whatever it computes presumably are the observables. But string theory—perhaps because it is *not* in its final form—has so far sidestepped the problem of cosmological observables. It defines quantum gravity for certain classes of geometries characterized by asymptotic conditions, such as asymptotically flat or Anti-de Sitter spacetimes. In these geometries an S-matrix happens to make sense, and string theory computes its matrix elements. (In the case of AdS, it computes boundary correlators, which are a close analogue of the S-matrix.)
>
> However, we have yet to learn how to apply string theory to cosmology or to an observer inside a black hole, with the same level of rigor as in Anti-de Sitter space. Hence, it would be premature to conclude that the S-matrix will remain the only well-defined object. It is too early to know what, if anything, string

> theory has to say about cosmological observables.
>
> Fortunately, classical and quantum properties of cosmological solutions impose significant constraints on possible observables, and may even hint at some of the principles on which a theory computing them must be based. De Sitter space is a case in point. Semi-classical analysis has provided overwhelming evidence that *no* exact observables exist in eternal de Sitter space—at least, none that correspond to experiments that can be performed by an observer inside the universe. This is related to the presence of a cosmological event horizon in de Sitter space, which limits the accessible information and emits pernicious thermal radiation.

Of course, as Daniel Friedan has discussed in detail, string theory has difficulties with large distance physics in general, not just cosmology.

67. **Mubos Lotl**
December 22, 2004

“To move forward, Strominger argues, the string theorists should work on the problems that they are able to make a progress on. For example, he says, they should concentrate on explaining why the dark energy that is enhancing the cosmic expansion rate is so small. ?We aren?t producing ideas on that,? he observes”

Excellent! So who at Harvard is following this good advice?

68. **Dave Bacon**
December 22, 2004

Lubos: “(violated the copyrights)”

Come on Lubos, you really believe quoting a small passage of a book with a reference is a violation of copyright law?

I use all these petty little inccorect statements by string theorists such as yourself as yet more proof that string theory is wrong (that’s a joke, in case you’re flaming powers are firing up.)

69. **Arun**
December 22, 2004

Another Pathik Guha article:
http://www.telegraphindia.com/1041018/asp/knowhow/story_3871254.asp

While it is mostly about Trieste ICTP, here is a relevant excerpt:

The audience bursts into laughter as Prof. Alvarez-Gaume, while discussing the string theory (experts? concept of a single idea to explain all physical phenomena), flashes a surrealist painting on the screen. It shows a castle on top of a huge piece of rock hanging in thin air! ?I want to convey the idea that the string theory has a rock-solid base,? he comments, highlighting critics? arguments that the theory is merely a mathematical artifact and has not been corroborated by experiments yet.

70. **Doug**
December 22, 2004

Sorry,
I forgot to include my name in the previous post. My name is Doug, and I contribute here from time to time.

71.
December 22, 2004

Has the principle of impotence, used so widely and successfully by the proponents of quantum mechanics, come back to haunt them?

“There are three major problems,” explains Strominger. “One is the problem of finding a theory that in principle can unify all the forces and particles in Nature. The second one is consistently putting together quantum mechanics and Einstein?s General Relativity. The third problem is the mathematical explanation of the black hole attribute that we computed. String theory has mathematically solved all the three problems. We don?t know if Nature avails herself of the solutions provided by string theory. But, on principle, string theory is capable of resolving all three of the riddles. Loop quantum gravity hasn?t solved any one of the three problems.”

If string theory can solve these problems “in principle,” is that any less compelling than quantum mechanics’ ability to solve chemical problems “in principle?” Larson wrote, years ago, that adherents to quantum mechanics were employing the same “principle of impotence” in regards to chemical problems:

“The physicists who are attempting to apply the latest quantum concepts to the problem have been singularly unsuccessful, if we appraise their results by any realistic standards. Some very broad claims on their behalf are often made by overenthusiastic supporters, the following from G. G. Hall being a typical example: ‘Quantum mechanics… gives the solution, in principle, to almost every chemical problem,’ but the true significance of such statements becomes apparent when Hall goes on to say, ‘Very unfortunately, however, there is an enormous gap between this solution in principle and the practical calculation of the properties of any specific molecule.'”

What amazes me is that so few recognize that, mathematical complexity not withstanding, solutions need to be able to deliver the goods or they are NOT solutions. But even worse, now we are talking about “in principle” solutions to problems that are in themselves in principle “solutions.” Quantum mechanics is a “solution” to the Bohr model of the nuclear atom, that has become the “solution” to the behavior of high-energy debris in accelerator collisions. Of course, such “solutions” have to be given 19 or 20 parameters from observation first, and some “reasonable” limit chosen to cast it in, but given these and a few, more subtle, “adjustments,” it can deliver the goods all right, just check out the standard model.

Well, at least it can if you ignore gravity, that is. We need a “solution” to our quantum mechanics “solution” that is a “solution” to our gravitational “solution,” as well. How inconvenient that the fabric of our “space-time solution” won’t accommodate the vacuum fluctuations of our “virtual sea solution.” If it did, perhaps we wouldn’t need a “solution” to our two, most spectacular, “solutions.”

It should occur to us that perhaps we have more fundamental problems than unifying the forces of the nuclear atom, or reconciling GR and QM, or explaining hypothetical black hole attributes. Perhaps the problem is that we have overlooked the fundamental definition of force as a PROPERTY of motion. Perhaps, the reason we can’t get there from here is because of our focus on forces and the assumption that forces can exist autonomously, without underlying motions!

Maybe we ought to go back to the beginning and ask ourselves, “What is motion?” If we did this, we would find ourselves confronted with another basic question, “What are space and time?” Now, Gross says that they are emergent, and Witten says that they are doomed, but what do we care what string theorists say anyway? Let them go back to the comfort and joy of basking in the beauty of mathematics.

We all know the answer to this basic question and have known since childhood: space and time are the reciprocal aspects of motion, nothing else. Common sense tells us that combining space and time in a continuum may be something that we can think about, and it may even be fun to see what happens since now we know that motion affects time as well as space (now that's a shocker!), but, come on, let's face it, Nature is not likely to be out making up such an ungainly expedient.

We know that radiation and energy are discrete. We know that motion affects time as well as space. These things we know, so it's not much of a jump to suppose that motion, that is, the only known relationship of space and time, is discrete as well. If motion is discrete, then it follows that its two, reciprocal, aspects are discrete too, and there you have it – we are off and running with a whole new concept that offers us an incredible new approach.

For instance, since we've learned that the speed of propagation of radiation relative to matter is constant, we can take that as a pretty good indication that the reason is that it's the unit speed of motion: one unit of discrete space per unit of discrete time = a velocity of c. Wow, check out the symmetry in that baby, would ya? Wonder what happens when there are more units of space than time, or more units of time than space in that otherwise perfectly symmetrical equation of motion? What else might be lurking in there? Matter must be related to this basic motion, huh? Maybe magnetic and electric properties of matter are too, ya think?

Well, to tell you the truth, there is incredible promise in this approach, including, but not limited to, particle masses, charges, and physical constants of all kinds, but we now return you to the continuing saga of legacy physics: the motion of strings in a quantum world where the curves of space don't bother us, and its fluctuations happily evolve in time.

72.
December 22, 2004

Hey Santo,

I don't quite agree. Feynman kind of realized that he may have been doing a silly mistake. This is what the second part of his answer is all about. I don't quite understand how "more" can you be aware of your doing a mistake. If he knew exactly and clearly that his viewpoint were mistaken, then he would have changed his viewpoint, would not he? 😉

I am happy that you (violated the copyrights) and cited Feynman accurately because his authentic quote is much better than my variation of it. But the content is the same.

If he realized his mistake just one piece more than what he displayed, he would have understood why string theory is the right approach.

Happy holidays,
lubos

73. **Santo D'Agostino**
December 22, 2004

Lubos,

I posted my comment because I did not wish readers to get the mistaken impression that Feynman thought himself making a silly mistake in criticizing string theory. In fact the full interview shows that he emphatically believed that string theory is crazy.

Of course, I agree that one man's opinion proves nothing, and no one is suggesting that one should treat the opinions of anyone, even a great man, as God-like. However, I do strongly believe that when one is quoting someone briefly, one must do one's best to be accurate. But perhaps I should not hold people so strongly to task in an informal setting such as this, as I would in a more formal setting such as in a published article or book.

Wishing you all the best,
Santo

74.
December 22, 2004

Yes but Lubos did you even address Feynman's point about not calculating anything?

75. **Lubos Motl**
December 22, 2004

Santo Agostino, you're missing the point.

Of course that the silly people often do not think about themselves that they are silly. But it's irrelevant what they think about themselves.

Feynman's genius is that he realized very well that even the other great physicists, such as Einstein, say a lot of things when they're old – things that look stupid to others.

He knew well that the old men are too conservative and unable to follow the new stuff too carefully. Knowing this fact about biology is helpful, but it's not enough to follow the new developments in physics – and Feynman is an example.

You know, I am one of the biggest fans of Feynman in the world, as my choice of the e-mail address shows much like other things. But looking to the old Feynman as some sort of God whose opinion about very complicated things matters even if there is no actual science behind it – that would be silly.

Einstein did not believe quantum mechanics, even though he was a genius. There were fortunately many others who did. Feynman did not believe string theory, but there were others who did. You don't need to go far – Gell-Mann was the guy who created the strong Caltech's string theory group.

Best
Lubos

76. **Santo D'Agostino**
December 22, 2004

L. Motl's "quote" of Feynman makes it sound as if Feynman thought himself silly, but that's not the sense I get from reading the exact quote (from Superstrings: A Theory of Everything?, edited by Davies and Brown, page 193; I presume this is the source that L. Motl is "quoting"):

"I have noticed when I was younger, that lots of old men in the field couldn't understand new ideas very well, and resisted them with one method or another, and that they were very foolish in saying these ideas were wrong — such as Einstein not being able to take quantum mechanics. I'm an old man now, and these are new ideas, and they look crazy to me, and they look like they're on the wrong track. Now I know that other old men have been very foolish in saying things like this, and, therefore, I would be very foolish to say this is nonsense. I am going to be very foolish, because I do feel strongly that this is nonsense! I can't help it, even though I know the danger in such a point of view. So perhaps I could entertain future historians by saying I think all this superstring stuff is crazy and is in the wrong direction."

When asked what it is that he didn't like about string theory, he replied:

"I don't like that they're not calculating anything. I don't like that they don't check their ideas. I don't like that for anything that disagrees with an experiment, they cook up an explanation — a fix-up to say 'Well, it still might be true.' …"

He is very emphatic throughout the entire interview, and his full comments are worth reading.

77. **DMS**
December 22, 2004

Actually, I am a bit surprised, as in India journalists tend to be very deferential to physicists/mathematicians (they are VERY RUDE to politicians, unlike US media, I might add!). Atiyah remarked: "I have never received the kind of audience that I have in India. I feel like a pop star."

http://cities.expressindia.com/fullstory.php?newsid=67555

Perhaps the journalist has a good physics background.

78. **Fabio Lanzoni**
December 22, 2004

"Though the experts, not particularly fond of hypes, don?t like the name that much.."

Since when are string theory experts not particularly fond of hype? They seem to spend a good deal of their time cultivating it.

It's no accident this interview came from an Indian paper: this kind of skeptical journalism is not in fashion in the US. It can lead to people asking the wrong questions and upsetting advertisers.

79. **Lubos Motl**
December 22, 2004

This Pathik Guha (journalist) is a pretty self-confident guy. 😉 Andy Strominger explains him that string theory has no competitors. Of course that Andy Strominger knows what he's saying, and his sentence also implies "the enterprise called loop quantum gravity is not a competitor of string theory". Pathik Guha nevertheless says "Strominger may be not quite right". 😉

We like the comment of Feynman very much. "I know that it's silly, that all good physicists who become old suddenly start to say all these ridiculous things about science. We know that Einstein did the same thing with quantum mechanics. I know that I am going to repeat the same silly mistake (with string theory), but I just can't help myself." 🙂

80. **plato**
December 22, 2004

A question

What's better—

This-

*Although there?s no direct evidence that string theory is correct, Strominger points out, "**we?ve a number of signposts that it?s on the right track.**" What are they? String theory, Strominger explains, has shown beautiful connections with various branches of mathematics.*

http://www.telegraphindia.com/1041220/asp/knowhow/story_4152318.asp

—or this?

*Although there?s no direct evidence that string theory is correct, Strominger points out, "**we?ve a number of signposts that it?s on the right track.**" What are they? String theory, Strominger explains, has shown beautiful connections with various branches of mathematics.*

In reference to content. So math doesn't matter?:)This would put mathematics in a really awkward position?

81. **Lubos Motl**
December 22, 2004

Andy just returned from India – he also attended Shiraz Minwalla's wedding.

Mathematical Humor

December 22, 2004
Categories: Uncategorized

Now for some comic relief:

A new issue of the Notices of the AMS is out. It contains an entertaining article entitled Foolproof: A Sampling of Mathematical Folk Humor with many examples of mathematical humor. Physicists also put in an appearance.

Comments

1. **plato**
   December 29, 2004

   Peter,

   I understand you like films?

   The Sound of Music?🙂

   Enjoy

2. **plato**
   December 27, 2004

   Humor was once defined, in a early historical way. That now, humour might be envisioned as some aether requirement preceding mathematical recognition?:) Is it, just a good gauss(I mean guess)?

3. **Peter**
   December 24, 2004

   When I first set this up, comments were oldest first, but someone complained that they should be newest first, so I changed it. I think I'll leave it the way it is for now. At some point I'll move the weblog to newer software, then I'll see about maybe changing the ordering.

4. **dave**
   December 24, 2004

   Peter,

   You should make the comments such that when someone reads them, the first comment is at the top, and the last at the bottom.

5.
   December 23, 2004

   There are three types of mathematicians: those who know how to count and those who don't.

6. **Fabien Besnard**
   December 23, 2004

   The Wiener anecdote reminds me of another one about physicists this time, namely Pierre and Marie Curie. The Curie were one day eating their lunch, while discussing very intensely about some physics problem. Then, their very devoted maid, who had cooked the meal, asked them if the steack was good. "So it is a steack we had ? That may be." said Pierre. Apparently Marie was not more aware of what she ate.
   This one is likely to be true, since it is their daughter Eve who wrote about this anecdote.
   A lot less funny, the legendary absent-mindedness of Pierre seems to have costed him his life when he crossed the street without watching.

   PS : the anecdote is probably more funny from a french point of view, since it is very unlikely that a french pays no attention to what he is eating !

7.
   December 22, 2004

   Why did the flight to Warsaw crash?

   Because all the Poles were in the left half-plane.

   (Irrelevant context omitted)

   -drl

8. **Lubos Motl**
   December 22, 2004

   Haha, very good jokes.

9. **plato**
   December 22, 2004

   What Is Real?

   The abundance of joking questions involving plays on mathematical terminology, as well as the jokes involving the stereotype of the mathematician,suggest that mathematicians like to play. The delight in playfulness would seem to run counter to the stereotype of the humorless pedant who is concerned only with precision, but there is no necessary contradiction. A predilection for fantasy…..

whoa now…are you saying mathematician's actually have humour?:)Imagine Lewis Carroll, and the stories he could tell. I think this man would rate higher in the folklore of the mathematicians?:)

Maybe because mathematcians are getting older, they like to pass off physics, with mathematical myths?:)

On Crackpotism and Other Things

December 31, 2004
Categories: Uncategorized

I haven't posted anything new here in a while, with the holidays and trying to get over a bad cold keeping me otherwise occupied. Partly because of this the comments section has been to some degree taken over by people who want to discuss things I have no interest in. I'll try and put up something new soon (comments on Penrose's new book), but I did want to make some remarks about the problem of crackpotism in theoretical physics, something which is especially a problem for open forums on the internet like the comment section here.

When I first started studying particle physics during the 1970s, it was pretty clear to me how to tell the difference between serious people and crackpots. The Standard Model had just recently been formulated and it had started to accumulate an impressive amount of experimental evidence in its favor. So, at least in particle theory, serious people were doing one of a small number of things. The more phenomenologically inclined were analyzing the new experimental results to see if they further validated the Standard Model, or suggesting new experiments that would test different parts of the model. More mathematically inclined sorts were trying to understand the rich structure of the model, trying to get a better grasp of its aspects that were still poorly understood. People inclined to speculation were working on ambitious extensions of the model, hoping to find something compelling that would both explain some of the model's parameters and make new, testable predictions.

So, to my mind, crackpots were those claiming to have new ideas about particle physics, but refusing to really engage in some way with the Standard Model quantum field theory. There were plenty of them around, including S-matrix die-hards like Fritjof Capra, those who wanted to go on about what happened before the big bang and how that explained all properties of particles, and a wide variety of people with their own private TOE that completely ignored the Standard Model. All you had to do was learn to ignore such people.

During the last 20 years, distinguishing crackpots has become a lot tougher, and it has gotten much more difficult recently. Famous professors from the best research institutions in the world go on about the properties of the universe being determined by colliding branes, or by an anthropically determined point in a multiverse, or any number of similar ideas. The dominant idea in the whole field makes nothing like what would normally be considered a testable scientific prediction, and those pursuing it don't seem too bothered by the increasing evidence that this situation will never change. Personally I haven't much changed my criterion for crackpotism in particle physics: if someone is not engaging in a deep way with the Standard Model and/or the kind of mathematical structures it involves, they're probably a crackpot.

When I first wrote a critical article about string theory and made it public about four years ago, I got quite a lot of reaction. Almost all of it was gratifyingly positive, but I ended up hearing from quite a few people who were convinced that since I didn't like string theory, surely I would like their alternative. These alternatives spanned a wide range, from very serious work to complete crackpotism, including all shades of in-between. The one thing that caused me to worry that there might be something wrong with my criticisms of string theory was the nature of a small number of my supporters. Some of these people still write to me regularly, and my e-mail is full of crazier things than what appears in the comments on the weblog. It's embarassing to get cc'd on an e-mail to a long list of very prominent physicists by someone who is quoting my criticisms of string theory to back up their own even sillier ideas.

I've gotten very good at hitting the delete key or, in extreme cases, using procmail to automatically filter this stuff out of my inbox. I suggest similar tactics in reading the comment section here. The first line of defense against people who you think are not making any sense is just to ignore them. Do not give in to the temptation to point out to them that they are not making sense, because all this will accomplish is to clutter things up as they respond to your response to them.

I'm not about to start just deleting comments that I think are of a crackpot nature, partly because it is now hard to set up a clear criterion for what is crackpotism (should I delete Lenny Susskind's comments if he decides to write in some day?). But to the extent that the volume of off-topic comments starts to overwhelm those that are interesting and related to the postings, I will have to take some sort of action. If you are posting large numbers of comments, mostly far off the topic at hand, please stop doing it now. If you are responding to such off-topic comments, please stop doing that too, don't encourage them!

Comments

1. **D R Lunsford**
   January 7, 2005

   Do non-crackpots invite you to give seminars and generally let you present your case, or do they tend to suppress your views?

   Oh heavens no, in both cases. I've talked about it in not too much detail in the usual public places, sent it to a few people who I thought might be interested, but the proper way to do it is to put it on arxiv. I don't know why they blacklisted it – all I know is, it got sponsored, got put up, and then vanished – never got any explanation. I would like for the thing to be on arxiv just on general principles, but now that it's actually been peer-reviewed and published, it's not a big issue to me any more. (In any case the publisher now owns the copyright.)

   I do enjoy talking physics though. That's what you really miss being an outsider.

   -drl

2. **Doug**
   January 7, 2005

   drl wrote:

   *Whether or not I'm a crackpot (I could not care less), the idea was really Riemann's, Clifford's, Mach's, Einstein's and Weyl's. The odd thing was, Weyl came so close to getting it right, then, being a mathematician, insisted that his theory explain why spacetime is 4D, which was *not* part of the original program. Of course if you want to derive matter from the manifold, it can't be 4D. This is so simple that it's easy to overlook.*

   Well, if you are a "crackpot" then, you are in some pretty good company. Do non-crackpots invite you to give seminars and generally let you present your case, or do they tend to suppress your views?

   Weyl wrote:

   The question of the ultimate foundations and the ultimate meaning of mathematics remains open; we do not know in what direction it will find its final solution or even whether a final objective answer can be expected at all. "Mathematizing" may well be a creative activity of man, like language or music, of primary originality, whose historical decisions defy complete objective rationalization.

   So, again, it's a question of the meaning of mathematics. I believe that the non-crackpots should consider their own, canonical, forms in this light, and admit that they have no special claim to the one and only “correct,” creative, mathematized solution. The problems they face may eventually yield to the considerations of higher and higher abstractions, but most wise men seem to sense that they will not.

I believe that the importance of maintaining a connection with correct physical concepts is key in all this “creative activity.” For instance, your observation that “if you want to derive matter from the manifold, it can’t be 4D,” might make perfect sense in the context of creative mathematics, but we should always ask ourselves, whether in regard to your recourse to six dimensions, or to Witten’s recourse to ten dimensions: “can this make sense in a physical way?”

Clearly, no one has ever detected a unit of space, in any form, whether 3D, 4D, 6D or 10D. These dimensions of space are mathematical creations and nothing else, as far as the non-crackpots have been able to determine. Then, is it such a “crackpot” idea to take this physical observation at face value, and admit that there is no such thing as space, and stop continuing to give something that doesn’t exist properties that our mathematics need, such as “viscosity” (earlier generations), “warpage” (today), or “smooth tension” (tomorrow). We have filled it with fluctuating fields, made it a virtual sea of particles, and now want to roll it into tiny pieces and even say it’s a maze of glued together tetrahedrons!

Isn’t it clear that the true “crackpot” here, is the one who thinks that we can just freely invent dimensions of something that clearly doesn’t exist? Isn’t it the one who insists on giving space properties so that he/she can continue “mathematizing,” and indulging in the “creative activity of man,” as Weyl puts it? Present company excluded of course.

I realize that this “crackpot” point of view is like a voice crying in the wilderness, not likely to be heard today, but I want it on record that, like Diogenes, someone needs to hold the lamp in our faces: Honestly, where’s the space guys? Minkowski was right, when he predicted the ultimate union of space and time, but we have only unified space and time in the most unusual and unnatural way. Today, as the complexity of our perplexity grows, Witten predicts that space and time, as independent entities, are doomed, and Gross says that they are “emergent.” Well, I hope so, since they never existed as such anyway, except in our “mathematizing” activities.

The only way that space and time can be unified that makes any physical sense at all is as the reciprocal aspects of motion, as we clearly see in the simple equation of motion, v = ds/dt.

Think about it, it’s not hard to see. Can we measure either space or time without the other? No, we cannot. I defy anybody to do it. To measure space, we need time, and to measure time, we need space. Why is this so? It is so, because 3D geometry is a result of motion’s three degrees of freedom: ds/dt can take three, and only three, mutually orthogonal directions physically, and I defy the whole world to prove otherwise.

We may be able to easily format space in an endless number of ways to form manifolds, strings, or branes within our mathematical creations to our heart’s content, but we can’t move an object, or draw a figure, in more than three, mutually orthogonal, directions, no matter how hard we try. This physical fact ought to temper our “creative activity” of “mathematizing” in regards to investigating nature, but it won’t, and why is that? Because we are non-crackpots by social definition and we get paid so much for playing the role. But if we will only look in the mirror, we will have to conclude: “we have met the “crackpot,” and he is us!”

3. **D R Lunsford**
   January 7, 2005

   Whether or not I’m a crackpot (I could not care less), the idea was really Riemann’s, Clifford’s, Mach’s, Einstein’s and Weyl’s. The odd thing was, Weyl came so close to getting it right, then, being a mathematician, insisted that his theory explain why spacetime is 4D, which was *not* part of the original program. Of course if you want to derive matter from the manifold, it can’t be 4D. This is so simple that it’s easy to overlook.

   I always found the interest in KK theory curiously misplaced, sicne that theory actually succeeds in its original form, but the success is hollow because the unification is non-dynamical.

   -drl

4. **Doug**
   January 7, 2005

   drl,

   You lost me on that one, it’s too abstract for me. It’s beside the point too, because I’m not defending Hestenes’ treatment of spacetime, but only attempting to show how a mathematical model can work under certain circumstances even when the physical intrepretation is wrong or even non-sensical.

   This intrigues me though:

   The GR program was to derive matter and spacetime on a common ground. This never really happened because Tmn was posited on the right in analogy with (backgroundful) electrodynamics. This leads to the energy and scaling problem of GR which leads to its confounding the quantizers. Solve the matter problem and the rest should take care of itself.

   I am not skillful enough to understand your paper, but I have never heard of this idea before. Are you considered a “crackpot” for this approach? Why did they blacklist your paper?

5. **Momentus**
   January 6, 2005

   “If it disagrees with experiment, it is wrong.”
   I was googling on this phrase,because I have a simple experiment, which I cannot get to behave ‘right’. I also have not found any one who can tell me what the expected experimental result should be.
   The experiment is to hang a spinning wheel(Gyro) from a long cord.It does not Rotate (precess)about the c of g.
   My study of this observed fact has revealed a new force. Now that is crackpot, by definition.

   So most learned and erudite scholars, please what should the suspended mass do? Where have I gone astray?

6. **D R Lunsford**
   January 5, 2005

   Doug –

   The standard answer is that a change of basis casts the Dirac eqn in the form

   (y_mu d_mu – m y_5) psi = 0

   You could now start from this eqn and get the “real” one in the same way. So matter-antimatter is encoded in m – under spacetime parity m -> -m and matter antimatter.

This isn't mystical – you could have started with -m and have -m -> +m. You have to treat y_5 correctly to have this symmetry, and it seems to me Hestenes doesn't do that. That's why he gets a "thorn" in his eqn, an explicit appearence of y_2.

-drl

7. **Doug**
January 5, 2005

Oops,

Was in a hurry. Meant to say:

I wouldn't argue the idea that the role of antimatter is NOT crucial. Also, spacetime parity may not be as important as space/time parity in another context, but I'd have to explain that.

8. **Doug**
January 5, 2005

drl wrote:

His [Hestenes'] reformulation does violence to spacetime parity, so I don't have much faith in it.

But there is the issue of "hidden" changes of basis. I'm "looking through a glass darkly" here and quickly getting in over my head, but I wouldn't agrue that the idea that the role of antimatter is crucial, only that the mathematical abstractions seem to take on a life of their own.

Here is a paper that rings true for me, although I don't understand it all:

Parra Serra's paper

And here is a quote that rings true to me as well:

I sometimes wonder if obession with the ineffable realm of abstraction is a sort of drug addiction that is clouding the collective judgment. drl

And finally, this one:

God has chosen the world that is the most perfect, that is to say,
the one that is at the same time the simplest in hypotheses and the
richest in phenomena.
— Gottfried Von Liebniz

9. **D R Lunsford**
January 5, 2005

Doug wrote

Nevertheless, Hestenes writes that, by reformulating the mathematics of the Dirac equation, he can show that "the zbw is a ubiquitous phenomena with manifestations in every application of quantum mechanics, even in the nonrelativistic domain."

His reformulation does violence to spacetime parity, so I don't have much faith in it.

"Similarly with GR – the problematic aspects are artifacts of approximation." – I don't see how this follows. The incompatibility of a background free theory like GR with a background dependent theory like QFT cannot be attributed to "artifacts of approximation," can it?

Sure it can – the GR program was to derive matter and spacetime on a common ground. This never really happened because Tmn was posited on the right in analogy with (backgroundful) electrodynamics. This leads to the energy and scaling problem of GR which leads to its confounding the quantizers. Solve the matter problem and the rest should take care of itself. I have a proposal if you're interested.

10. **Doug**
January 5, 2005

drl wrote

Well, this is also true for example of the Pauli spin theory. Only the Dirac theory showed how right Pauli really was. Pauli's was actually the harder problem, and within the Dirac theory, he was totally justified, as an approximation.

But what this really indicates is that the mathematical interpretation must be related to the physical concept employed. If it were not so, we would stop with the mathematical formulation. We don't, because we want to understand a physical model, not just a mathematical one. Hence, there is still a lot of dissatisfaction in the non-crackpot community because of the metaphorical interpretation of spin: we want to know what it really means.

This has led to many attempts to show that it really is generated by some kind of rotation. Hestenes regards Schroedinger's zitterbewegung (zbw) model for such motion especially noteworthy because "it is grounded in an analysis of solutions to the Dirac equation." However, zbw doesn't result in anything real. Nevertheless, Hestenes writes that, by reformulating the mathematics of the Dirac equation, he can show that "the zbw is a ubiquitous phenomena with manifestations in every application of quantum mechanics, even in the nonrelativistic domain."

So, here we see, once again, that the mathematics can work, even when there is an incorrect interpretation of the meaning of the mathematics, or even when there is a non-sensical and metaphorical interpretation, but when a correct interpretation of the mathematics accompanies a solution, great advances result.
(see http://modelingnts.la.asu.edu/pdf/ZBW_I_QM.pdf)

BTW, part of Pauli's "harder problem," was never really solved by Dirac's formulation: quantum mechanics still cannot account for the structure of the periodic table accurately.

Similarly with GR – the problematic aspects are artifacts of approximation.

I don't see how this follows. The incompatibility of a background free theory like GR with a background dependent theory like QFT cannot be attributed to "artifacts of approximation," can it?

11. **D R Lunsford**
January 4, 2005

Doug wrote:

However, there's a possibility that, for some reason, the level on which "GR is undeniably correct," is not a physical level, but a mathematical level. In other words, it could be that it is an interpretation of physical concepts that works mathematically, but is physically wrong.

Well, this is also true for example of the Pauli spin theory. Only the Dirac theory showed how right Pauli really was. Pauli's was actually the harder problem, and within the Dirac theory, he was totally justified, as an approximation.

Similarly with GR – the problematic aspects are artifacts of approximation.

For instance, Einstein's theories are based on Maxwell's equations, which show that the law of conservation leads, not only to symmetry, but to electromagnetic waves that travel at the same speed in all directions, at the speed of light. There does not appear to be an error in these equations, yet the physical concept at the time included the idea of an all pervasive aether. Contrary to popular belief, Einstein didn't eliminate the aether concept, he just modified and renamed it.

It turns out that SR is *not* based on Maxwell's theory, but that the latter exhibits relativistic behavior in an essential way, because light goes at the fundamental speed C. In fact SR doesn't need any specific field theory at all to hold it up – it's just group theory and projective geometry (Klein's program, light cone = ideal domain). And so Einstein *really did* eliminate the aether (for a while, in my reducible, lambda days, I believed as you do 🙂 He eliminated it by refining the idea of space and time in such a way that propagation becomes a primitive fact, like ponderability, that doesn't require factitious causes.

Here's an idea, let's go down to the basement and rummage through the dusty trunks of history. Who's afraid of the dark? Us "crackpots" aren't, because we have nothing to lose.

Indeed! But watch for spiders.

12. **Doug**
January 4, 2005

drl,

I whole heartedly agree with this:

Nevertheless, my own opinion is that some things in science just can't be ignored or you aren't doing science, which is not a series of wacky revolutions.

Yet, many times that is exactly what is countenanced in non-crackpot science, and in a glaring fashion to boot. One example is Ivor Catt's anomaly. You would think that science has progressed way beyond elementary concepts such as those Oliver Heaviside wrestled with in stringing the Atlantic with telegraph cables. However, like the old farmer said, "It's not what I didn't know that done me in, it's what I knowed that weren't so!"

This is not just true in the field of physics; it's also true in the field of mathematics, where David Hestenes discovered that "some things…[that] just can't be ignored" are being ignored nevertheless. In fact, when it turns out that things aren't adding up, like the enigma faced today in the form of quantum field theory's incompatibility with GR, then it's a pretty good indication that something that "just can't be ignored" probably is being ignored.

However, it's usually the investigators labeled "crackpots" who are motivated, for some reason or another, to go back to the basics to find what it is that has been ignored. Usually, this is so because only "crackpots" can afford to challenge long held beliefs. Non-crackpots, even tenured ones, must protect their careers, pensions and reputations and, thus, are not likely to go down into the basement and rummage through the old, dusty trunks of history, searching for clues as to what went wrong.

Instead, they keep on trying to build on the existing foundations, because they trust and believe that what they *know* isn't going to "do them in," contrary to the folk wisdom of the old farmer. If we are so sure that

GR is undeniably correct on some level – not only does it make accurate predictions, it is also very tight math.

then we are going to seek to incorporate it in our efforts to understand Nature. However, there's a possibility that, for some reason, the level on which "GR is undeniably correct," is not a physical level, but a mathematical level. In other words, it could be that it is an interpretation of physical concepts that works mathematically, but is physically wrong. We see this all the time in other cases, and we even acknowlege it in the gravitational area where, in the low limit, we interpret the physical behavior of mass in terms of a physical force formulated by Newton. When we need the accuracy of GR, however, Newton's physical interpretation of force between masses changes to Einstein's interpretation of geometry that results from the interaction between mass and spacetime. Then, when we get to the quantum level, neither of these physical concepts serves us, so we again employ mathematics to rescue us and come up with the Higgs field, or something else – actually, the race is on to see who can come with something first that can be verified.

What is actually happening, though, as us "crackpots" can easily see from the outside looking in, is that what is being verified is the mathematics, not the physical concepts. Physically, what we have verified is that light and energy are quantized, that the speed of radiation is constant relative to matter, that gravity is equivalent to acceleration, that distant galaxies are receding from our location in all directions, and that this universal expansion is not slowing down for sure, and may even be speeding up.

We can predict the behavior of subatomic particles with mathematical precision, if we measure certain quantities first, and if we limit the range of our calculations in just a certain way. However, we haven't verified the physical concepts involved in the nuclear model of the atom, we have only found a mathematical solution that enables us to interpret the observed physical behavior.

All this means that there is always the possibility that another mathematical approach to interpreting the same results would, if successful, lead to a different physical concept.

But, as you say:

There are certain steps in the evolution of science that are not optional – you can't make a gravity theory that doesn't in some sense incorporate GR at this point, any more than you can make one that ignores Newton on that level.

This is only true if you are building on all the existing foundations. If an error is discovered in the foundations, then some dismantling is inevitable. For instance, Einstein's theories are based on Maxwell's equations, which show that the law of conservation leads, not only to symmetry, but to electromagnetic waves that travel at the same speed in all directions, at the speed of light. There does not appear to be an error in these equations, yet the physical concept at the time included the idea of an all pervasive aether. Contrary to popular belief, Einstein didn't eliminate the aether concept, he just modified and renamed it. Today, the concept of a physical field is as indispensible to modern concepts of physics as the concept of the aether was in Maxwell's day. However, now we know that, although Maxwell's equations were correct, the physical interpretation of the meaning of the math was not. So, it's just as likely that the physical concept of the field is as incorrect as the physical concept of the aether was, but the mathematics works in either case. Therefore, it's true that

Anyone who claims that Einstein's analysis is all wrong is probably really a crackpot.

because it's obviously not wrong. That is, it's not mathematically wrong, but the interpretation of the mathematics may incorporate incorrect physical concepts that will not work except in special cases, such as when we see GR's spacetime concept unable to work at quantum scales, and QFT's field concepts unable to work at large scales. While the maths in both cases work fine within the prescribed limits, the two physical interpretations of these mathematical concepts are incompatible; one is background dependent, and the other is background independent: obviously there is something wrong!

Here's an idea, let's go down to the basement and rummage through the dusty trunks of history. Who's afraid of the dark? Us "crackpots" aren't, because we have nothing to lose.

(I can't end this without saying how pleasantly suprised I was to learn that you have considered three dimensions of time in your own work. I very much appreciate the invitation to discuss these things with you via email, too. I'll probably take you up on it.)

13. **D R Lunsford**
January 4, 2005

Doug –

Perhaps I misunderstood you. I see you accept the 4Dness of SR at least.

I certainly know from experience that your point about the behavior of the gatekeepers is true – I worked out and published an idea that reproduces GR as low-order limit, but, since it is crazy enough to regard the long range forces as somehow deriving from the same source, it was blacklisted from arxiv (CERN however put it up right away without complaint).

Nevertheless, my own opinion is that some things in science just can't be ignored or you aren't doing science, which is *not* a series of wacky revolutions. GR is undeniably correct on some level – not only does it make accurate predictions, it is also very tight math. There are certain steps in the evolution of science that are not optional – you can't make a gravity theory that doesn't in some sense incorporate GR at this point, any more than you can make one that ignores Newton on that level. Anyone who claims that Einstein's analysis is all wrong is probably really a crackpot.

(BTW my work has three time dimensions, and just as you say, mixes up matter and space and motion. This is not incompatible with GR, and in fact seems to give it an even firmer basis. On the level of GR, matter and physical space are decoupled the way source and radiation are in elementary EM. Feel free to send email if you want to discuss it or your own ideas.)

-drl

14. **plato**
January 4, 2005

Arun said: *So, string theory embraces both General Relativity and not-General Relativity!!!! In other words, string theory says nothing definite.*

Plato said:*And about Arun's comment about GR. Phase transitions would be reduced holographcally from higher dimensions(the standard model would have been decribed from earlier states), would finally show up there?:)*

If one did not recognize earlier states of existence and just accepted the cosmological playground sight seen, it always existed in this form then:) That is, if we take the standard set by observation:)

I for one thought, topological considerations would have been formulated from earlier cosmic designs, but aparently this might have been subject to scrutiny, and thought out. Rejection of the soccer ball design as well?:)

I'll have to readjust accordingly. I'll be posting the reasons why on my own blog soon.

15. **Doug**
January 4, 2005

drl,

I know it's hard to believe that anything else could work, but the truth is there is another alternative. The reason GR works is because the speed of light is constant relative to matter and because gravity is an acceleration, not equivalent to an acceleration, mind you, but is a time rate change of motion. The question to be answered, then, is actually, "what is the nature of the gravitational motion?" not "what is the nature of the gravitational force?"

GR doesn't answer either question really, but instead eliminates the need for a force through an adjustment of the reference system mediated by covariance. Nevertheless, I'm afraid the discussion of GR is way off topic, which is "crackpotism." Hence, my point is not that GR or a 4D union of space and time is wrong, but that another point of view, especially one that not only works, but addresses the very foundations of our physics, cannot exist EXCEPT in the minds of "crackpots" by fiat.

Suppose that I claimed to have discovered that there exists, after all, an absolute frame of reference of motion, but I'm not a reputable physicist, just an honest investigator. What do you suppose the reaction of the non-crackpots would be? Would those who referee journals allow my findings to be published? Would those who teach at Universities take the time to listen to my arguments? Would those who advise philanthropists and public agencies brook my ideas?

The prevailing sense of the scientific enterprise is that at the core of its dynamics lies a merit-based ethics, but this is just not so. Discovering this is more traumatic than uncovering the deceit inherent in the alleged fact that the NY Times' real credo is not "All the news that's fit to print," but actually "Print all the news that fits," because scientific truth is so sancrosanct.

Nevertheless, it's a fact: regardless of merit, any concept far enough outside the accepted line of thinking will be labeled a "crackpot" idea by the guardians of the orthodox doctrine, and, of course, many people don't read beyond the label. Ironically, though, the really "crackpot" idea of string theory and its poly-dimensional approach and parallel universes of colliding branes, originating on the "inside" so-to-speak, has now emerged and overtaken orthodoxy, making the situation even more ludicrous.

Now, the completely ad hoc idea that space has 10 dimensions, and that seven of them are "compactified," and that the "strings" of gravity escape into a parallel universe, is embraced by Universities and published in thousands of journal articles and funded by public and private entities to the tune of untold millions of dollars.

However, if a voice of reason is raised, that points out that we live in a three-dimensional universe, where there are only three degrees of freedom observed, and that space has no properties of its own that can be observed, in spite of every effort to do so, except for the properties it has as an aspect of motion, and that it is more reasonable to assume, therefore, that time has three dimensions, and, with three dimensions of space, forms three dimensions of motion, that voice truly might as well be crying in the wilderness, for no one will hear it. If they do hear it, though, you can be sure that they will most likely just moan, and grunt, and mumble the word "crackpot."

16. **D R Lunsford**
January 3, 2005

Doug –

It's totally wrong-headed to think of the transition to 4D as a "correction". Rather, 3+1 is an approximation to bulletproof reality. I can well understand having a distaste for relativity, since it is usually very badly taught and written about (e.g. all the idiotic problems with pole vaulters in barns and the like). You'll have to get over any resistance to the idea before you can make real progress in any direction.

-drl

17. **Doug**
January 3, 2005

drl,

I assume that parts of your post are missing, but I think I get the gist of your point. I do not say that at high speeds the covariant technique does not produce the correct results. I only insist that the success of the procedure doesn't mean that the world is necessarily four dimensional.

For instance, I can get the same correction in a 3D world that includes *time* motion (the reciprocal of motion in space), as demonstrated in the correction accounting for the excess advance of the perihelion in the orbit of the planet Mercury. The effect of time motion is small at everyday speeds, but at high speeds, it accounts for the same descrepancy as your 4D approach.

The wonderful thing about it too is that it is the natural result of homogeniety and reciprocity!

Regards,

Doug

18. **D R Lunsford**
January 3, 2005

Looks like this wonderful software mangled my post.

19. **D R Lunsford**
January 3, 2005

Doug –

In the everyday world where speeds are

20. **Doug**
January 3, 2005

Hi drl,

You wrote:

The world is really, really 4D on some level. It can't go back to being 3D – this is quite impossible in the light of experience. It can't be anyD unless (any-4) has a damn good explanation.

Of course, you realize that you just violated Peter's admonition not to respond to "crackpots" don't you?! LOL. Don't worry though, I'm not going to harrangue you with a myopic and stubborn insistance that living in ignorance in the age of enlightenment is better than embracing the "reality" of 4D consequences. If I were to do that, Peter might put me in his filter.

However, to be honest, your comments do cause me to wonder at the conviction that the world "can't go back to being 3D." Actually, the "world" has never stopped being 3D in the minds of some of us "crackpots." While the theories you non-crackpots have embraced have given you a way to understand the apparent "force" of gravity in terms of non-3D, both the crackpots and the non-crackpots still move in three dimensions, right?

You see, if you combine one dimension of space with one dimension of time, as one dimension of motion (ds/dt), something magical happens: you can move one-dimensionally in a three-dimensional world. And guess what happens if you combine two dimensions of space with two dimensions of time (ds/dt)^2? or even more innovatively, dare to combine three dimensions of space with three dimensions of time (ds/dt)^3?

Well, enough of that from this crackpot, I'm probably overstepping my bounds in a non-crackpot 4D world where such nonsense is unwelcome. Nevertheless, I want to go on record as testifying that assuming that four dimensions of spacetime is real, is not necessary to understand the 3D world of motion. The only thing that is necessary is that you understand motion: the only KNOWN relationship of space and time.

Regards,

Doug

21. **Peter**
January 3, 2005

Hello anonymous,

I had no intention of promulgating a general theory of crackpotism, my comments were purely restricted to particle theory. Crackpotism in cosmology is a whole other subject, one I have no intention of entering into.

Peter

22. **Quantoken**
January 3, 2005

Lubos said:

"But there are loads of backgrounds where string theory gives absolutely unique, quantitative, sharp, and theoretically crosschecked predictions for a given Universe, and these contexts of course always agree with GR at low energies.

If someone believes that string theory is mathematically vague, then he completely misunderstood everything about it. The "fictitious" new Universes predicted by string theory, especially those with a lot of supersymmetry, are completely rigid and sharply defined."

Lubos: have you nothing to say since I pointed out the fundamental error you made in your 4*ln(3) paper?! See the comment section of the Strominger Interview. Basically I point out that writting down ANY differential equation that takes derivatives against space and time, and deal with quantities about or below the Planck Scale, is fundamentally wrong, because at Planck Scale there is no longer continuous space and time.

No one was able to argue against the notion that derivatives against Planck Scale space and time is wrong. They could only defend for you by claiming that the math symbols in your equation (1) does not mean derivaties (which is laughable), or that you are not dealing with Planck Scale (how could it be a black hole if the mass scale involved is less than one Planck mass). Both are invain arguments.

Now, about your "predictions" about a "given universe" or any "virtual new universes". Let me assure you that it takes me 20 minutes instead of 20 years to come up with a self consistent 13 dimentional self consistent theory about an alternative "given universe", outside this one we known, and I will make predictions. And certainly no one can prove me wrong because no one can observe the other universe in any way direct or indirect.

Physics deal with obsersations we can make in this universe, which is the only one known, as far as physics is concerned. Any study or even meantioning of alternative universes, which we can not provide observation evidence for, are NOT physics, but religion.

The whole landscape and alternative universe nonsense in ST seem to be pushing itself out of physics and towards the religion end, since to believe in something no evidence can be provided, faith in the deep of your soul may be the only way. Don't you see that in deed deep in your heart, you have that kind of faith towards ST? You want to curse every one who is infidel, right?

Quantoken

23. **Anonymous**
January 3, 2005

Peter ,
would you consider Narlikar , Hoyle, Arp,
Burbidge and others who still think Steady state theory of cosmology, is correct as cracpots?

24. **plato**
January 3, 2005

> *Of the infinite forms we must select the most beautiful, if we are to proceed in due order….* **Plato, in the Timaeus**

DRL said:*abandoning their little shadow-cave would force them to come to terms with their lack of insight and to stop hiding behind Platonic ideals.*

Ho, Ho, Ha, Ha:)DRL, I know you are smarter then this. They understood that there is a natural progression to the topological considerations valid in dimensions.

John Baez said:*The story goes on… but in higher dimensions one usually uses the term `regular polytopes' instead of `Platonic solids'. All the faces of a regular polytope must be lower-dimensional regular polytopes of the same size and shape, and all the vertices, edges, etc. have to look identical. Maximal symmetry, that's the name of the game! (Also, I'll only be talking about convex polytopes.)*

25. **D R Lunsford**
January 2, 2005

Peter –

Speaking from experience – taking the SM seriously does not get you off the crackpot roster 🙂 But, nice try!

(What is *really* hard is to take both the SM and GR seriously. Hard but not impossible…)

-drl

26. **D R Lunsford**
January 2, 2005

Doug –

The world is really, *really* 4D on some level. It can't go back to being 3D – this is quite impossible in the light of experience. It can't be *any*D unless (any-4) has a damn good explanation.

It's worth repeating – the 4D of relativity is *not* just a bookkeeping convenience, like say, virtual photons. It's *real* and has real consequences.

-drl

27. **Doug**
January 2, 2005

The interpretation of Newton's program of research, as a dictum to focus on forces, has led to the goal of describing nature in terms of a few interactions among a few particles. The standard model attempts to do that, but it leaves out a huge piece of the picture. It also must start with a large number of empirical observations, and, of course, it is completely at odds with recent cosmological observations.

The development of string theory started as an attempt to address the SM's many problems, and in fact received its biggest impetus when it seemed to offer a solution to the missing piece – gravity. If Supersymmetry and string theory can never succeed, what will happen to the SM? After all, it is an attempt to extend the SM, so what should young particle physicists replace string theory with? Should they abandon the SM altogether, concluding that "you just can't get there from here?" Or, alternatively, should they abandon the current interpretation of Newton's program of research as a dictum to focus on forces, and seek a new, innovative, intrepretation that might yield better results?

Would it be a crackpot idea to suggest that a alternative interpretation of Newton's program might be that we should focus on *motion* as prior to force, and thus examine the nature of space and time as two, reciprocal, aspects of motion? Might that simple idea offer some real prospects for describing nature in terms of a few types of motions, combinations of motions and relations between motions (forces)?

It's obvious that we live in a 3D universe, and we even see two types of 3D motion, the outward motion of the receding galaxies and the inward motion of gravity. The force aspects of these motions are interesting, but why not investigate these motions themselves? What are their characteristics? How do they originate? Can we simulate them someway without a 4D spacetime, but with just 3D motion?

These are some simple suggestions of a possible new line of inquiry that doesn't ingnore the successes of the SM, Peter, but does recognize that its limitations, as a means for describing nature in a satisfactory manner, call for some fresh ideas. Does that have to be a crackpot thought by definition?

28. **D R Lunsford**
January 2, 2005

One more comment to "X" and his claim that ST is a natural progression (?) from the idea of point sources – of course this is completely false: a natural progression would have been, say, an infinitely thin vibrating shell, retaining spherical symmetry. Of course such an idea is also laugable in the light of relativity – that it was taken seriously by the likes of Poincare and Lorentz just shows what a rare commodity is sound physical insight. I doubt that the latter can be taught, which is why the stringers defend their dysfunctional world so vehemently – abandoning their little shadow-cave would force them to come to terms with their lack of insight and to stop hiding behind Platonic ideals.

-drl

29. **D R Lunsford**
January 2, 2005

"X" wrote

Frankly, I don't understand why ST was not a natural progression (eg. Lunsfords belief that it was obviously 'horseshit' from the outset) from the standard model..

My *advisor* thought that ST was horseshit from the beginning. Given his pedigree (Weisskopf) and his achievement up to that time (it speaks for itself), I think I was well justified in agreeing with him for my *own* reasons, which were actually different than his but just as deeply felt.

ST is horseshit because it is postmodernly anhistorical and revisionist. Nothing in physics is ever revolutionary. Everything proceeds from past experience. Winding the clock back to before Democritus was a laughable idea that became pernicious. I thought it would die of its own accord. I'm totally amazed that anyone with half a brain takes it seriously for *any* reason (even if they are purely mathematical ones).

-drl

30.
January 2, 2005

"X" wrote

*I don't suppose D.R. Lunsford will ever be banned despite his enormous arrogance in calling a *mathematical* physicist such as Witten a failure while paradoxically demonstrating his admiration for other predominantly *mathematical* physicists such as Dirac and Einstein."*

Well this is rather self-evident, and you are mistaken. Dirac and Einstein were **actually** physicists. Even their side work is crammed with interesting physics (e.g. Dirac's superconducting vacuum and large numbers hypothesis; Einstein's Brownian motion). Both Dirac and Einstein used math as the *means to the end* – physics – and did not set it up as the *end in itself*.

Of course you must have actually sought out and read the side work, to know this. Since there is no time for detours in the usual sausage grinder curriculum, the chances are slim that the typical student will ever know anything about this side work. Nevertheless it is great physics and was to me very suggestive. I don't think I would have been able to solve my particular problem in the unexpected way that actually came about, if not for Dirac's side work. So, I have a very personal attitude about it.

(Ban me? I had no idea I was so formidable.)

-drl

31. **Lubo? Motl**
January 2, 2005

No, I don't see any difference. The LQG team also claims that they have a theory of a fundamental interaction at the Planckian energies, while they ignore the Standard Model.

You can't really go beyond the electroweak scale without properly explaining the electroweak and lower scales, i.e. the Standard Model.

Other crackpots say, for example, that all matter is made of letters X and T. They show how proton is constructed – and their goal is not to describe the Standard Model either. Their goal is to construct a theory of particles.

32. **Peter**
January 2, 2005

Hi Lubos,

No, I certainly don't think LQG is crackpotism. The LQG people are not claiming to say anything about particle physics, their claims are restricted to quantum gravity. And they are doing this using a mathematical set-up (connections and loop holonomy variables) which is close to the mathematical set-up of gauge theory. They're definitely not doing what crackpot particle theorists do, claiming to have a theory of particle interactions that ignores the standard model and its mathematical structure.

33. **plato**
January 2, 2005

Don't be hard on your self Lubos. When one plays with certain models, you can get burned?:)

The basis of the argument could be an + or – thing, and things will oscillate all day long, and no ones happy with the shape of things.:)

34. **Lubo? Motl**
January 2, 2005

The criterion about taking the actual structure and the required math of the Standard Model seriously is of course very important.

Incidentally, Peter, do you agree that not just Wolfram, but also loop quantum gravity and many other directions squarely fall to your definition of crackpotism?

35. **Lubo? Motl**
January 2, 2005

Maybe I should not have written the post about MOND at all if it is misinterpreted.

It's a complete speculation of mine that in cosmological setups, new phenomena may arise.

But there are loads of backgrounds where string theory gives absolutely unique, quantitative, sharp, and theoretically crosschecked predictions for a given Universe, and these contexts of course always agree with GR at low energies.

If someone believes that string theory is mathematically vague, then he completely misunderstood everything about it. The "fictitious" new Universes predicted by string theory, especially those with a lot of supersymmetry, are completely rigid and sharply defined.

36. **Lubo? Motl**
January 2, 2005

I kind of agree with what Peter wrote, and it is comforting to see that he at least looks at the problems in this way. Sorry, I have not read any comments yet, it's just too much of it.

The definition of the past crackpots 30 years ago is much less of a controversy than the definition of crackpots today. I believe that in 2030 they will have a better and more unifiable idea who were the crackpots back in 2004.

The S-matrix, bootstrap approaches etc. were not quite scientifically sound, and it's a matter of historical coincidences that a related line of research has led to something useful and deep – string theory in this case.

But don't forget: the history is written by the winners. The bootstrap people were simply losers, at least in the 1970s, and it makes them much more vulnerable to an attack. A reasonable person will resist the temptation to attack them, especially because some of their ideas were vindicated by recent research in string theory.

I am happy that Peter realizes that most of his supporters are much clearer crackpots than the worst idealized string theorist he can imagine, even though he can't state this fact as clearly as I can (because his blog is partly driven by them). 😉 Crackpots can be annoying, but someone similar to crackpot can, in very special cases, propose a great idea. People have different approaches, and I am sure that Peter understands the huge difference between Lenny Susskind and the unsophisticated crackpots.

Lenny is a genius, in some sense, and this can't be obscured by a couple of talks that many of us find too vague.

37. **Quantoken**
January 2, 2005

Arun said:

"String theorists are not crackpots. The problem with string theory is exemplified by the exchange on sci.physics.strings, where it is said that one can IMAGINE that string theory and holography have their ways to induce corrections needed to explain the Pioneer anomaly and others, and a MOND-like alternative to dark matter. So, string theory embraces both General Relativity and not-General Relativity!!!! In other words, string theory says nothing definite."

String theory and holography could only "imagine" (i.e., wishful thinking) to have a way of explaining Pioneer anomaly. But I see no solid and unambiguous solutions of such published. On another hand, Generalized Universal Information Theory And Relativity naturally leads to prediction of abnormal "acceleration" like the Pioneer, and it gives the correct quantitative calculation.

I used quotation around "acceleration" because it is actually not an acceleration, but a relativity effect proposed by my theory. The two are equivalent just like gravity field and acceleration field are equivalent.

In a nutshell, the principle that the total quantum information in the universe being a finite and conserved quantity, necessarily leads to the conclusion that our spacetime do not extend to infinity. Our spacetime is curved to be enclosed. That's because flat and infinite spacetime would require infinite amount of quantum information to describe and decipher each individual spacetime coordinate point.

Keep in mind when spacetime is curved and enclosed, that does not just mean space. Time is curved, too!

Now, when Einstein developed his special relativity, he made one assumption which is correct in a flat and infinite spacetime, but which is wrong in our universe. Einstein recognize that in different inertial frames, clocks may tick in different speed, and spontaneous events at different locations in one reference frame may not look spontaneous in another reference frame.

However, Einstein did presume that in the same inertial frame, as long as there is no acceleration or gravity to cause the clocks to change, all clocks at all different locations would all tick at exactly the same pace when observed by the same observer. Such an assumption is only true when the spacetime is infinite.

In a curved spacetime, you can NOT have all clocks ticking in exact synch. Having such means the time is still flat and extends infinitely, while as the space is curved and finited. That's impossible. The time axis would necessarily be curved and enclosed too.

So that's the conconsion of QUITAR, there is a universal relativity that any observer will see a clock at a remote location will tick slightly slower than his own. The further away the other clock is placed, the more it is slowed. That slowing is in addition to any special relativity or general relativity time dilation effect. At the edge of the universe, it will be slowed to a complete halt.

The time dilation described by universal relativity is relative: A sees B's clock slower than A's own. But B also sees A's clock slower than B's own.

How do we know two clocks at different locations are ticking at the same pace or not? We may bring the two together to verify they tick at the same pace. But that doesn't tell us because it's position dependent, once you move the other clock away, it slows down again. You can bring them together once in a while to see if the slowness has accumulated, but in the process of bringing them together you have accelerated them and that destroys comparability of the two clock, because acceleration causes time dilation.

In similar reason like Einstein did, the only feasible way to verify whether two clocks at remote distance are ticking in sync or not, is to use light signal as beacon and see if the beacon arrives at the correct interval or not.

And that's what we actually do!!! We observed that the light beacon from remote stars indeed are arriving ever slower, depends on the distance. That, we call Hubble Redshift, has been observed.

You can explain the slowdown of light beacons as evidence that the remote light source is moving away, or you can explain it as a universal relativity effect that remote clocks simply tick slower. The two explanations are equivalent, just as gravity and acceleration are equivalent.

However, the Doppler explanation would have to presume the remote clock ticks at the same pace as ours. We do not have evidence for that. On another hand, remote clock slowing has been mandated by quantum information conservation, which is very reasonable and logical. So, indeed, Hubble Redshift is indeed the universal relativity caused time dilation effect.

The same can be used to explain the Pioneer Anormalcy. Since we presumed the clock on Pioneer would tick in sync., we instead explain the observed discrepancy as an anormality acceleration towards us, the observer. The amount of such "acceleration", based on my derivation, is

(4/PI)*C^2/(Radius of Universe)

Quantoken

38. **plato**
January 2, 2005

Might this satisfy why astronomical valuations in Peter's eyes are not worth doing because of the magnitude of the orders of dollars associated?

Delivered by David Politzer on December 8, 2004 in Stockholm

There is a very active field of theoretical research which seeks to go beyond the Standard Model. Success in these endeavors would mean explaining the apparently arbitrary aspects of the Standard Model; success would mean bringing an account of gravity into the picture; and success would mean illuminating the previously mentioned issues in astrophysics. **However, we now face a very serious problem in advancing the experimental frontier, a problem which few people like to discuss**. It seems to me that ever since Leeuwenhoek, advances in the resolving power of our "microscopes" have come with similar investments of capital and manpower. I.e., an increase by an order of magnitude in the one required an increase by roughly an order of magnitude of the other — at least once we average over fits and starts and brilliant insights. The last big machine planned and canceled in the U.S. was to cost about $10 billion. (That's .) That would have allowed us to reach distances small enough to study the interactions of weak bosons directly. The realm of the conjectured "unification" of the forces of the Standard model, the realm of their possible unification with gravity, and the basic physics of String Theory, the most widely pursued approach to a physics more fundamental than the Standard Model, are all more than a dozen orders of magnitude further away. However, is simply not available for this line of research (or anything else for that matter).

And about Arun's comment about GR. Phase transitions would be reduced holographcally from higher dimensions(the standard model would have been decribed from earlier states), would finally show up there?:)

39. **Peter**
January 2, 2005

Hi Arun,

Actually there was nothing in this posting about string theory. In general string theorists are trying to reproduce the Standard Model, or even use string theory to solve QCD, so they're not crackpots by the criterion I was talking about.

The one part of the recent string theory enterprise that has definitely veered towards crackpot territory is the whole "Landscape" business.

40. **Peter**
January 2, 2005

Thanks for the link to the Politzer Nobel lecture. I'll update that posting to include it.

By the way, I've been closing the comment sections for posts that are more than a couple weeks old. Unfortunately this is necessary to keep the problem of "comment spam" under control.

41.
January 2, 2005

Mathematica makes errors on simple problems? I use Mathematica at times, and if that is really true…

Can you give some examples I can check for myself?

42. **Arun**
January 2, 2005

Using Peter's definition of crackpot, it is only because of crackpots that European sailors broke the confines of the Western Atlantic and Mediterranean.

It is crackpotism if Columbus continues to insist that he has found a passage to India.

String theorists are not crackpots. The problem with string theory is exemplified by the exchange on sci.physics.strings, where it is said that one can imagine that string theory and holography have their ways to induce corrections needed to explain the Pioneer anomaly and others, and a MOND-like alternative to dark matter. So, string theory embraces both General Relativity and not-General Relativity!!!! In other words, string theory says nothing definite.

43. **as**

January 1, 2005

This should have been a comment on the post “Nobel Lecture”, but it looks like it’s not possible to comment on that post.

David Politzer has put his Nobel lecture on his website at
http://www.theory.caltech.edu/people/politzer/

44. **plato**
January 1, 2005

I thought the least I can do is help to lead you in the direction of Penrose and the picture I gave you.

Remember Smolin was quite clear here about the three roads to quantum gravity, that I wonder how this road would seem more comfortable to you then the other two?:)I will wait and see what materializes I guess.

45. **plato**
January 1, 2005

Peter said,*“As long as you stick to this particular variety of speculation, you’ll have no trouble getting all sorts of silliness published. I don’t think this is progress”*

….was in response to Serenus and Brane theory models.

I would think that the brane models would set up new mathematical structures for consideration? If such cyclical natures was to assume an idea about the existance of this universe, then fundamental positions had to be assumed as well? In regards, to building the mathe structures that you want to deal with?

You then assume, that if it has always existed that the ability to formulate such structures would be relevant to the mathe you want to use?

You would have to forgive my ignorance on such technical matters, that I wonder if I should be warying or not, by speculatives opinions coming forth from all areas?:)

Just pretend I am the public, and remember what you are saying.

Already one clarification on the subject of this post, and a reference now to quantum gravity. I think as time goes on, the distilliation of what you want to say, should be mathematically voiced!

This would stand in support of what all of you demand. A position from which you speak in regards to the math you use and speak about the abstract world you deal in.

I would like to see how this world is represented by and what you people speak of, can be married to the real world we live in:)I characterize my self as plato and am well aware of my illusions which I can create and models I can play with.:)

I took a fundamental position of the post presented on crackpotism, and ciphered the demands placed, and was going to deal with the issues of the standard model and how these materialize, but as one realizes, many here who want to hold to a set way of speaking are all over the map.:)

Unbeknownst to you Peter you selected one of three roads to quantum gravity by Roger Penrose?:)

So indeed it can be quite fun to live by the rules that people would like to institute, but do not live by, and maybe then characterize it, as a freeflowing quorum of ideas?:) Non! Qui!:)

46. **Doug**
January 1, 2005

I’ve mentioned before that Hawking characterizes the standard model as “ugly and ad hoc,” and if it were not for the fact that he sits in Newton’s chair, and enjoys enormous prestige in the world of theoretical physics, he would certainly be labeled as a “crackpot.” Peter’s use of the standard model as the criteria for filtering out the serious investigator from the crackpot in the particle physics field is the natural reaction of those whose career and skills are centered on it. The derisive nature of the term is a measure of disdain for distractions, especially annoying, repetitious, and incoherent ones.

However, it’s all too easy to yield to the temptation to use the label as a defense against any dissent, regardless of the merits of the case of the dissenter, which then tends to convert one’s position to dogma, which, ironically, is a characteristic of “crackpotism.” However, once the inevitable flood of anomalies begins to mount against existing theory, no one engaged in “normal” science, can realistically evaluate all the inventive theories that pop up in response. So, the division into camps of innovative “liberals” vs. dogmatic “conservatives” is inevitable, and the use of the excusionary term “crackpot” is just the “defender of the faith” using the natural advantage of his position on the high ground.

Obviously, then, this constant struggle, especially in these days of electronically enhanced communications, has nothing to do with science. If those in either camp have something useful in the way of new insight or problem-solving approaches, they should take their ideas to those who are anxious to entertain them: students and experimenters. The students are anxious because the defenders of multiple points of view helps them to learn, and the experimenters are anxious because they have problems to solve.

The established community of theorists, on the other hand, are the last whom the innovators ought to seek to convince because they have no reason to be receptive to innovation that threatens their domains, and clearly every reason not to be. So, if you have a theory that suggests an experiment that Adam Reiss can reasonably use to test the nature of dark energy, by all means write to him. Indeed, he has publically invited all that might have an idea for an experiment. But don’t send your idea to Sean Carroll because he is not going to be receptive, even though he too publically acknowledged that “we need all the help we can get,” (see the Science Friday archives).

“All the help we can get,” in addressing the problem of dark energy, is to be understood in terms of the standard model of particle physics and the consensus model of cosmology. To suggest to Sean that the 4D “fabric” of space-time is not real, or to Peter that the quantum field fluctuations don’t exist, that they are, in reality, merely ad-hoc expedients employed to solve the problems of earlier generations, is going to inevitably get you labeled a crackpot. No matter how convinced you are of the merits of your innovation, and no matter how strongly they insist that they would entertain logically derived and “observationally supported” arguments, if only they existed, the truth is that they will not! (see Ivor Catt’s experience for a good example in a less complex field.)

It’s not a question of bad guys vs good guys, or smart guys vs dumb guys, or the foolish vs the wise, as much as the adherents to the different camps would like to believe that is in order to boost their egos as innovators or as defenders of the truth. Nope, it’s just a matter of social economics, the only science that might benefit from the study of the phenomenon.

47. **Daniel Doro Ferrante**
January 1, 2005

Wow! Amazing how can such simple a post generate so much noisy! (Sorry Peter, i don?t mean to diminish you, your post, your thoughts, etc… it?s just that i don?t remember seeing this kind of "heat" around here for quite some time now… >8-)

Also, there?s so much stuff here to talk about that i could probably go on and on for quite some time… but, if you guys excuse me, it?s still New Year?s day (here where i am) and i don?t want to miss my family?s cookout! >;-)

To begin with, let me say something which is a lot more pragmatic but, to my understanding, seem to reasonate with the "overall feeling" buried in some of the comments here: "This beautiful idea of freedom and democracy of ideas and thoughts only works on very restricted geographical regions of this planet of ours!" And, if any of you need an example of this, get a plane to anywhere below the equator; i?m sure it?ll be an enlightening experience. (An old joke comes to mind: "Democracy is when i say and you do; Dictatorship is when you say and i do." >;-)

As for Penrose?s comments on the precision of GR, there?s a very nice paper on the arXivs, which also happens to be on The Living Reviews of Relativity, that should help to clarify this issue: .

The new term added to my lexico: "psychoceramics"! Outstanding! >:-)

As for the more serious matter, one thing that is key to all of this meta-discussion but was introduced late into it, is FASHION. Or, as Peter put it, "orthodoxy". I agree wholeheartedly with the idea behind this comment by Peter: "Fashion cannot, and shoud not, lead anything, let alone science!" Unfortunately, because we all happen to be human beings, there?s always a "personal" touch on everything we do. From Newton vs Leibnitz to Clifford vs Boltzmann, science (as any other human endeavor) has been filled with "ego fights". As is usual with most animals, this can easily be translated into "mine is bigger than yours". Even a man like Newton could profit from some (not to say ?lots?) ideas from other folks, like Leibnitz for instance. Ditto for Boltzmann.

But, as we move higher in the "food chain of thought/academia", it becomes increasingly more difficult for us to learn from other people, to take responsibilitie for our mistakes… it becomes harder for us to keep our cool, to be humble.

[Un]Fortunately, or not, (for me, personally) i have participated and been involved in several discussions of the sthereotype "Strings vs Rest of the World". I have heard quite a few people that i respect say quite a lot of things that are just [mathematically/physically] wrong: I know it, they know it. However, no one does anything to change the pattern… (I?m sure that both Freud and Yung would have a LOT to say about patterns and their repetitions. >;-)

There?s a LOT of stuff to be done in physics, in math, in chemistry, in biology, in computer science, etc, etc, etc… in science in general! Using fashion to limit the highway of ideas is simply unfair.

Nowadays, in our posy-industrial society, called "Informatin Society" (not the band: ! >;-), our media/press plays a very crucial role. So, when one of us, physicists and/or mathematician, goes "out there" and writes a book selling an idea… Do you see my point, or should i spell it out?!

Good ideas do not need advertising…

As for Mathematica, the software, PLEASE: "Do NOT feel overwhelmed by it; it is quite crappy!" Do you want to talk about symbolic math…?! Try going back a bit in time and searching for "algebar" (or one of its GPLed versions). Maybe even "derive"! Those were very good indeed; they cared about the algorithms behind them. Mathematica chokes (or gives you the WRONG answer) even on quite simple problems… and this happens basically because they "cranking software" was really crappy! The one thing that it can really do well is plotting and PS graphs! Did you know, or did you ever wondered, why was Mathematica completely rewritten in this past couple of versions?!!!

This is just the begining, the tip of the iceberg… On top of this, the argument that someone smart enough to "create" Mathematica (BTW, Wolfram did not do that alone; there was a huge crew behind him) can be smart enough in some other area; this is called "argument of authority" and, as the name suggests, has very little reasoning behind it. If you really want to argue pro/con Wolfram, i suggest you ready some of his work, maybe from the 80s.

As for me, i do think that he is a quite smart person, i also think that his cellular automata are good for quite a number of things. What i don?t like is the fact that, after you read his bible-book, you have a sense that everything in the Universe has been understood BUT you have no evidence for it. 😉

48. **Quantoken**
January 1, 2005

Steve Wolfram's ideas are interesting but I do not know enough of it to make judgements. But a guy who created Mathematica shurely could not possibly be a complete idiot and there may be some thing in his "new science".

What do you think of Seth Lloyd's "computational universe", Peter:
http://www.edge.org/3rd_culture/lloyd2/lloyd2_index.html

49. **Peter**
January 1, 2005

Hi Robert,

Glad to hear that was a joke, I was worried about you for a minute there.

Actually Wolfram is a perfect example of my particle theory crackpotism criterion. From what I remember his chapter on particle theory pretty much completely ignores the standard model, while claiming that properties of particles can be understood using his ideas about cellular automata (and providing no evidence of this). This is crackpotism in a pretty unadulterated form.

50. **Quantoken**
January 1, 2005

Peter said:

"I should clarify something. My remarks about identifying crackpotism were specifically restricted to the subject of particle physics, where a huge amount of experimental data and a very successful theory exist. Other subjects, e.g. quantum gravity, are a whole different story, and I didn't intend to say anything about the much more difficult question of how to identify crackpotism in that area."

I do not think you can clearly separate particle physics from other areas of fundamental physics theory research. They are interweaved so tightly nowadays, especially in the view that a theory that unified GR and QM will necessarily deal with both the microscopic particle world and the macroscopic cosmological world.

Roger Penrose has his own share of crackpotism. One of his claim that impressed me very much is that he claimed often that GR has been verified to be correct up to 12 decimal places. One would thought that means to find any discrepancy between the GR and the nature one has to look beyond the 13th decimal place after the decimal point. What he actually meant is in the binary pulsar observation data by Taylor, who got a Nobel Prize for it, has to be accurate up to 12 decimal places to show any GR effect at all. If the accuracy of the observational data is less than 12 decimal places, then the GR effect portion would be too weak to be seen and be buried in statistical noise.

So the 12 decimal places claim actually put the Taylor data into credibility question. The raw data of individual pulsar pulse period is in no way accurate at all: they vary by as much as a few percentage. Only though statistical average of bizillions of data can you achieve better accuracy. The question to ask is did Taylor's statistical model really provide that 12 digits accuracy through statistical average?

Back in the 70's there wasn't even a 32 bits computer CPUs yet, so the computers can't even represent a number up to 12 decimal places. I checked the Fortran computer code of the original software Taylor used, there wasn't even any data types of double precision floating points defined in Fortran language at that time. Even on a 32 bits PC today, double precision floats, which takes 64 bits, gives you just 19 decimal places accuracy.

That puts Penrose in the rank of Sr. Eddington, who claimed his observational data from 1919 solar eclipse, obtained using a portable 4 inch telescope he brought to South America, confirmed the GR prediction of 0.75″ star light bending near the Sun's surface. In reality, neither the optical resolution of his 4 inch telescopy, or the exposure film, or the disturbance of atmosphere "seeing", would have allowed any credible measurement of that tiny bending.

When looking at experimental data, you have got to take a grain of salt. There are too much of an urge to bend the data to fit what one believes in, for various none-scientific reasons. And too little effort spent on verifying the credibility of experimental data. The "12 decimal places" of the Penrose kind is a good example.

Quantoken

51. **RT**
January 1, 2005

"Wolfram a great man comparable to Newton? I know that's what he thinks and tells everyone, but really…."

I was joking, honest (cf 'Mathematical Humour'). An attempt to introduce a note of levity into a debate that seems only too ready to lurch into the realms of acrimony. As an engineer/applied physicist, I found Penrose's book to be enlightening, entertaining and educational in equal measures. For what it's worth, many of my professional colleagues regard all theoretical physicists as crackpots, and my interest in their world as a signature of senescence.

52. **Peter**
January 1, 2005

Wolfram a great man comparable to Newton? I know that's what he thinks and tells everyone, but really….

I'm still not quite up to writing about Penrose, but, no, he's not a crackpot. He takes the standard model quite seriously, has an interesting discussion of it, and is properly skeptical about it just where it deserves skepticism (the Higgs mechanism). His speculative ideas about twistors have already had interesting implications for the standard model (in finding solutions to the Yang-Mills equations, recent "twistor-string" formulation of perturbative N=4 super Yang-Mills) and it is quite possible that more will be found.

I should clarify something. My remarks about identifying crackpotism were specifically restricted to the subject of particle physics, where a huge amount of experimental data and a very successful theory exist. Other subjects, e.g. quantum gravity, are a whole different story, and I didn't intend to say anything about the much more difficult question of how to identify crackpotism in that area.

53. **RT**
January 1, 2005

I look forward to finding out whether Sir Roger qualifies as a crackpot or not; the criteria Peter lays out seem a little stringent. And, almost without exception, great men, from Newton the alchemist to Wolfram the WANKOS, exhibited a touch of the balmpot at some point.

54. **Peter**
January 1, 2005

Hi Serenus,

I don't think that what has happened is that speculation is now encouraged, whereas it wasn't earlier. If you try and get something published that goes against orthodoxy (as an example, let's say you have an idea about how to make sense of theories with anomalous gauge symmetries), you'll still have a lot of trouble with referees. All that is new is that certain outlandish speculative ideas (e.g. brane-worlds) are now part of the orthodoxy, heavily funded by the NSF and promoted by prominent theorists. As long as you stick to this particular variety of speculation, you'll have no trouble getting all sorts of silliness published. I don't think this is progress.

What's wrong with brane-world scenarios is that they are ugly and don't actually predict or explain anything. The fact that you can work on them and have a successful career, while if you work on something less orthodox you'll have a lot of trouble, is at the root of a lot of the problems of the field.

Peter

55. **Quantoken**
January 1, 2005

Peter:

Don't you realize that crackpotism is a relativistic term? You call the string camp crackpotism, and they call you crackpotism. The string camp call the LQG cracks and so does LQG call the string camp. So who deserves to be the ultimate judge on this matter? Everybody? Nobody?

It's a relativistic term. Please do not abuse its usage. I would not use that term to criticize string theory, LQG or any theory in exploration. Certainly there do exist true crackpotisms, like perpetual motions kind of thing. Those stuff challenges well established theories and experimental evidences and are crackpots.

On the other hand, in the field of searching for a theory that unified both GR and QM, there has not been a single winner yet. There simply hasn't been any established theory in this area. So people from different camps really can not call each other crackpots.

Before a clear winner is well established and well confirmed by experiments, every potential theory could potentially be right and equally potentially be wrong. The only rule is the theory must be able to arrive at known limits and agree with known and established theories, GR and QM.

GUITAR does give the GR and QM limit and does not violate any of the established theories. It makes clear predictions that can be verified or ruled out by future experiments, predictions that is completely reachable in technology in near future. It explains a lot of things including CMB and Hubble Red Shift, and it even explains the origin of the uncertainty principle, all in a very natural way. My theory could still be wrong at the end of day but it is definitely not crackpot.

Quantoken

56. **Serenus**
January 1, 2005

"Famous professors from the best research institutions in the world go on about the properties of the universe being determined by colliding branes, or by an anthropically determined point in a multiverse, or any number of similar ideas."

There was a time, not so long ago, when the top journals routinely rejected papers for being "speculative". In effect, dull technical papers were preferred over anything that might look imaginative and interesting. Do you really want to return to the bad old days, Peter? I thought you only objected to the *overselling* of new ideas, but it seems that I may have been mistaken. What exactly is so awful about brane-world scenarios, as long as nobody pretends that there is observational evidence favoring them?

57. **JC**
January 1, 2005

I always thought that looking at crackpot "freakshows" was similar to activities like watching a car wreck and/or high speed police chases live on television. It's a bit like the equivalent of intellectual "junk food" for the brain, and a cheap way of indulging in a schadenfreund guilty pleasure in seeing the silly depths some folks are willing to lower themselves to.

58. **Peter**
December 31, 2004

Marko, thanks for the new vocabulary word!

About "Out of This World": if I thought that my review of it was likely to cause its author to immediately write another similar one, I certainly would have thought twice before doing it.

59. **Marko**
December 31, 2004

I believe the term used nowadays is "psychoceramics".

60.
December 31, 2004

Peter, look in the mirror. You are the one posting entries on books reviews for infinitely silly books such as "Out of this World". Why don't you take your own advice and just ignore it?

61. **Peter**
December 31, 2004

I wish people would start taking my advice, which I'll repeat here:

"The first line of defense against people who you think are not making any sense is to just ignore them. Do not give in to the temptation to point out to them that they are not making sense, because all this will accomplish is to clutter things up as they respond to your response to them."

62.
December 31, 2004

I don't suppose D.R. Lunsford will ever be banned despite his enormous arrogance in calling a *mathematical* physicist such as Witten a failure while paradoxically demonstrating his admiration for other predominantly *mathematical* physicists such as Dirac and Einstein. Those who have developed ingenious physical apparatus for experiments are not on his list, and then he has the gall to predict the decline of "western science" (whatever that is) because of its preoccupation with mathematical toys? Frankly, I don't understand why ST was not a natural progression (eg. Lunsfords belief that it was obviously 'horseshit' from the outset) from the standard model (see below)

Quantum Field Theory in a Nutshell
by A. Zee

"It struck me as limiting that even after 75 years, the whole subject of quantum field theory rooted in this harmonic paradigm, to use a dreadfully pretentious word. We have not been able to get away from the basic notions of oscillations and wave packets. Indeed, string theory, the heir to quantum field theory, is still firmly founded on this harmonic paradigm. Surely, a brilliant young physicist, perhaps a reader of this book, will take us beyond"

63.
December 31, 2004

"Do not give in to the temptation to point out to them they are not making sense…all this will accomplish is to clutter things up as they respond to your response to them"

I totally agree Peter. They crave attention, good or bad. I am afraid I am guilty of having responded a couple of times this week, for which I apologise. As much as I like vigorous scientific debate, it is clear that you simply can't have any with a crackpot at all–they are only interested in "their theory" and condemn just about everything else. I would say the difference between a real scientist/physicist and a crackpot is that the real scientist does not really believe their own ideas but treats them with a kind of playful irrelevance, and are the first to try and find fault with their own ideas. If it is right or is promising, great, if not them move onto something else. I think it is ok to work on strange stuff if you keep this sober attitude. You are trying things out a lot of the time and seeing where they might lead…if anywhere. I would hope that most string theorists have this attitude, and I think most of them do (the best ones anyway).

Much of theoretical physics and mathematical physics is like an advanced form of play for the human mind I think, but if you get completely obsessed with an idea and start ignoring all feedback and interaction the potential for self delusion is really vast.

The crackpot is charactersied by being totally obsessed with his ideas or one idea no matter how absurd, and absolutely nothing can convince them otherwise. They simply don't understand how science works. Rather than work on some small problem their theories are always "revolutionary" and everyone else is wrong or has missed the point. It must be some sort of psychological condition characterised by delusions of grandeur (I have the ultimate final theory) plus paranoia (the physics establishment wants to censor me and my ideas). Theoretical physics seems to attract them.

64. **JC**
December 31, 2004

Peter,

After the 1960's, how common were the hardcore analytic S-Matrix bootstrap guys like Fritjof Capra, who did not really pay attention to the Veneziano string stuff?

When I was bored one afternoon, I decided to go through several old journals searching for analytic S-Matrix bootstrap papers during the 1970's and 80's. The one thing I noticed was that there were less and less equations in those bootstrap papers as time went on. Many of Geoff Chew's later papers seem to be devoid of equations, especially in his "topological" bootstrap stuff from the late 1970's and early 1980's. At that point Chew could possibly be certified as a genuine "crackpot".

More Science Fiction

January 2, 2005
Categories: Book Reviews

It seems that every week there's a new book out about branes, M-theory, the multiverse, etc. by someone who doesn't really seem to understand the difference between science and science fiction. This week's example is Michio Kaku's Parallel Worlds: A journey through creation, higher dimensions and the future of the cosmos.

My impressions of the book come from a few minutes spent flipping through it in the bookstore, so maybe I missed something. The only reference I saw to the lack of any experimental evidence for anything he is writing about was where he noted that we'll need to travel faster than the speed of light to get to these parallel universes. So, we just have to wait for the development of warp drives. While references to experimental evidence were lacking, there were plenty of references to various science fiction novels.

I recently ran across a review of one of Kaku's very similar other books, called "Hyperspace". My favorite line in the review was near the end:

"Hopefully some time-traveler will go back and prevent this book from ever being published!"

Comments

1. **plato**
   January 4, 2005

   If one had to sum up Gr, how would you do it?

   Never mind about the lesser degrees of intelligences and references to angels and all kinds of things. You follow the history, it leads you where?:)

   As a laymen, I would have had to think about this very hard and assume it was about gravity, but of course once you meet this idea, all of a sudden you wonder what is being emitted? Taylor and Hulse have graduated the ideas for us in those elliptical paths that Mercury orbited to the discoveries of …..?:)

   So you move from this point to learn, what the historical forbears have revealled in their journies and come to meet Wheeler and Kip Thorne. Webber, and his alumininum bars and you understand that detection systems, are now being developed and have been employed.

   If you do not keep abreast of this continuing developement then of course one would become dismayed, about the new direction GR has gone?:)Numerical Relativity?

   So in weak field manifestations, you had to know that early cosmolgical consideration would have already included the standard model, and look to incorporate gravity into a whole view of the cosmo from planck epoch to now.

   Yes, the early uinverse and the symmetries involved of course in question:)

   Rest assured that we are talking about the cosmological discernation of the geometry thats leads us to consider, other things(topological considerations), and how quickly this is dimissed?

   I have heard Peter speak about the ideas of Reimann and the fruitless direction this has gone and am equally dismayed at how this could not have been estimated from a geometical standpoint not to have questioned the quantum mechanical discription of the small world[quantum geometry](it's incapatibilty GR) with the quantum world becoming very large?

   So again, it is a easy enough assumption to realize that the model in question changes what we view of the spacetime fabric, and becomes it? It then asks about light and if this is included, how would you ever get to what Smolin would have wanted in the Glast determinations as the final deal?

   This is what I have surmised, and I could be wrong:)

   This is a easy enough assumption about energy sources(gamma ray bursts) and the information that is release? Some would be very happy with the ideas of the compton scattering(glast determinations) that has gone on, to help us determine this information, as well as what will be revealled to LIGO and the SETI computers users, being utilized?

   This goes to Gerard Hooft's question of how much information could ever be assembled at such levels, that strings would have implied, that he would have been quite happy I am sure, with the way in which the SETI user screens are being utilized.

   I hope my asseessment has been correct and am open to any corrections.

2. **serenus zeitblom**
   January 4, 2005

   Were you referring to the s.p.r thread on whether Special Relativity is diffeomorphism invariant?

   Indeed I was. The thread title was "symmetries of GR" or something like that. One of the many things I found amusing at that time was LM's insistence that Carlip and I "wanted" general covariance to become meaningless. I guess we two are just a pair of terrorists who should be dealt with by the US army.

   I think Bertrand Russell's ABC of Relativity is a prime example of a bad popular book.

   Well, it's a very old book that probably reflects the majority view of the subject at that time. Nowadays we think about relativity very differently, but most popularizers, together with those who depend on them for an education, are stuck in 1920. In many cases this is just laziness — people writing books about string theory aren't really interested in telling you about the foundations of GR, so they just copy all the old junk about angels pulling up elevators etc, so that they can quickly get on to what really interests them.

3. **plato**
   January 4, 2005

   After consideration, I think I am in need of a exorcism?:)

   New post should turn up later, in real time:)

Cynicism can run from higher dimensional existance, to manifest as real emotive qualities called, human action, or words?

So if I attack cynicism from the right angle, I should be able to convince?:)Good thing, we have standards from which to proceed.

4. **D R Lunsford**
January 4, 2005

All the text below was Anderson, not I, notwithstanding software post mangling.

-drl

5. **D R Lunsford**
January 4, 2005

PW Anderson writes this morning in the Times, in response to the question "What do you believe but cannot prove?" –

Is string theory a futile exercise as physics, as I believe it to be? It is an interesting mathematical specialty and has produced and will produce mathematics useful in other contexts, but it seems no more vital as mathematics than other areas of very abstract or specialized math, and doesn't on that basis justify the incredible amount of effort expended on it.

My belief is based on the fact that string theory is the first science in hundreds of years to be pursued in pre-Baconian fashion, without any adequate experimental guidance. It proposes that Nature is the way we would like it to be rather than the way we see it to be; and it is improbable that Nature thinks the same way we do.

The sad thing is that, as several young would-be theorists have explained to me, it is so highly developed that it is a full-time job just to keep up with it. That means that other avenues are not being explored by the bright, imaginative young people, and that alternative career paths are blocked.

6. **Arun**
January 4, 2005

S.Z.,

Were you referring to the s.p.r thread on whether Special Relativity is diffeomorphism invariant?

—-

I think Bertrand Russell's ABC of Relativity is a prime example of a bad popular book.

-Arun

7. **Dolomite**
January 4, 2005

I've never seen any of his popular books, but I once made the mistake of buying one of Kaku's graduate level textbooks on String theory. The table of contents looked quite impressive, but the body of the book turned out to be a smorgasborg of equations from the various original papers, but with the symbols changed around in a failed attempt at making the different chapters look consistent, in the process removing much of the logical connective tissue and adding in typos. All in all negative value added. I've regarded him as a charlatan ever since.

8. **serenus zeitblom**
January 3, 2005

The usual response to criticisms of popular books is: what harm do they do? The answer was provided to me back in October 2003 when Lubos Motl very unwisely got into an argument with — of all people — Steve Carlip about the foundations of general relativity. It soon became painfully clear that LM had never taken a formal course in GR and had, in fact, derived all he knew about GR from reading popular books. The whole fiasco ended with Steve Carlip having to remind LM that it is actually quite Ok to use polar coordinates in special relativity! The problem of course is that popularizers tend to hand down basic misunderstandings from one generation to the next, so the account of GR you get in most of these books corresponds to Einstein's understanding circa 1912 when he was professor in Prague….

9. **Dave Bacon**
January 3, 2005

I saw Michio Kaku on Tech-TV (before it merged with G4) a few years ago. He talked about quantum computers. He said that the beauty of quantum computers was that they could efficiently multiply numbers. I almost went and burned his field theory book on the spot.

10. **D R Lunsford**
January 3, 2005

I like Kaku's field theory book, and he seems like a normal, straigtforward person in interviews (that is, not another carnival barker). Maybe he writes these books just to make money. Maybe his grad students write them for him for a cut. Certainly his "real" book is not full of hyperbole and sensationalism.

-drl

11. **quantoken**
January 3, 2005

Peter said:
"It seems that every week there's a new book out about branes, M-theory, the multiverse, etc. by someone who doesn't really seem to understand the difference between science and science fiction. This week's example is Michio Kaku's Parallel Worlds: A journey through creation, higher dimensions and the future of the cosmos."

Peter, looks like you are the one who does not understand the difference between science and science fiction. When you see a book in a book store, you should be able to clearly make a determination if this is a science book or a science fiction book, based on its content, not what the author claimed to be.

If I am a book author, I surely want to write a science fiction book and some how make it look scientific. Science fiction books sell better, especially those disguist as science books.

Any talk about things not happening in THIS universe, are pure fictions or religions, not science. Because science deals exclusively with this universe only, which is the only thing accessible by observation. So next time some one meantioning multiverse or alternative universe, you should automatically know it is not about science but fiction.

Now Lubos, are you really going to accept my criticism against the fatal error in your paper, and not even try to say a word to defend yourself? Admit defeat! You take derivatives against continuous space and time at Planck Scale where continuous space time simply do not exist. So the rest of your paper looked nice mathematically but are simply rubbish.

Quantoken

12. **Peter**
January 3, 2005

Sure, the book does go on about CMB, COBE, WMAP. But as you know well, the experimental CMB results don't provide any evidence for strings, branes, M-theory, the multiverse etc.

I didn't look closely enough to see whether Kaku explicitly acknowledges this. If he doesn't, his inclusion of reference to these experiments is just intentionally misleading.

13. **Lubos Motl**
January 3, 2005

I thought that this book spent a lot of time with experimental cosmology, such as WMAP, COBE, and others. Was I really wrong?

14. **Peter**
January 3, 2005

Hi Eleggua,

My comments about "Parallel Worlds" not explaining anything were directed at the idea of different universes being part of the same "multiverse", not at the many-worlds interpretation of QM.

I don't have anything much to say about interpretational issues in QM. These are something I've never found relevant to questions that do interest me. I am a big believer in Occam's razor, and it is not an argument against quantum fields. QFT is by far the most conceptually simple and compact theory that covers all the different things we know about how elementary particles behave.

15. **plato**
January 3, 2005

Correction: *Denmark*, should read **Vienna** in previous post.

16. **plato**
January 3, 2005

I really do not know what Peter means when he only has a slight look at what is being exposed to him from others opinions?:)I am to assume he read them?

No less then what I can give him and then to say, that if the brain as a muscle was not exercised, one would not have understood the comments made immediately by Michio Kaku about his artistic expression of the vision from the bridge and speaking about the gold fish:)

Imagine the surface of water and looking at it from two different perspectives:)

Not that I am a defender of the faith, but for sake of the abstract developement of the brain to move in the higher dimensions, he would have missed the obvious.

If the brain was further developed, one could also have distilled the understanding that part of crackpotism was not to have encourage discussion or respond, so I develope knowing full well, that resignation is always much easier, then being truely honest about how one could have become so cynical, no less then the psychological developement, of others.

Hmm…where to now?:)

To poster below me.

What was developed in Denmark(quantum entanglement) was a materialization of the points Penrose highlighted as a question.

THis is part of Smolins developmental attitude, as well as strings graviton graviton intersectional ideas that have yet to be developed.

Lubos might see the significance of the work he spoke on earlier in regards to gravitational lensing and clumping in the early universe:)

17. **Eleggua**
January 3, 2005

Hi Peter,

Now that I have had a look inside the book at my local bookstore, I see what you mean.

However, I am interested in your statement:

'The problem with "Parallel Worlds" is not that they aren't observable, but that they don't actually explain anything.'

Now, I am interested in what you think about the field of quantum computers.. In particular the statement by David Deutsch in Fabric of Reality that how do you explain the working of a quantum computer without parallel universes?
Of course, you can say that it is just Quantum parallelism, but I have never seen a DETAILED explaination of what Quantum parallelism would be for Quantum computers outside the Many-World scenario! (I would be very interested in references for such a thing!)

Anyway if you believe in the Many World scenario, surely it EXPLAINS away all of the problems in QM regarding measurement in a natural way?

I understand that Occam's razor could be used against it, but I believe Occam's razor can be used against Quantum Fluctuations in QFT as well!

Thanks in advance for any reply

18. **Peter**
January 2, 2005

Hi Jesse,
I wasn't trying to promote anything about that review other than the snide line about time travel. I couldn't care less about whether Kaku is a "materialist", dialectical or otherwise.

Hi Eleggua,

The book definitely contains large sections of material about branes, strings, M-theory, etc. The part about many-worlds seemed to me to be a small fraction of the material I saw there.

No, I'm not really a positivist. If a simple theory that successfully explains a lot of observed phenomena is based on some unobservable objects, I'm happy to believe in them. The problem with "Parallel Worlds" is not that they aren't observable, but that they don't actually explain anything.

Peter

19. **Jesse M.**
January 2, 2005

Some of the statements in that guy's review sound pretty crockpot-ish themselves. For example:

[However, it is all too easy to cook up theories that produce "predictions" that you already know the answers to, and then pretend the theory is thus a great success! Cosmologists are always doing this (e.g., with the Big Bang theory); indeed it seems to be their basic modus operandi.]

Or how about this:

And as presented by Kaku, string theory is essentially idealist besides. Pythagoras and Plato would have loved it; it is one of those theories that claims that matter and energy are composed entirely of "geometry". Although Kaku does not mention it in this book, these "strings of vibrating hyperspace" are assumed in the theory to be strictly one-dimensional, that is to say, geometric objects, not physical objects. He does constantly say in the book that the whole goal is to reduce physics to a geometrical theory. No real materialist can possibly acquiesce in the idea that the world is truly composed of geometric, rather than physical objects. All geometric concepts, such as points, lines and circles, are abstracted from physical objects in the real world, such as tiny things like dust motes, rows of things like pencil lines on paper (which are really composed of numerous graphite particles), etc. But points, lines, and other geometric "objects" do not literally "exist" in the world, and the world is certainly not "made" out of them!

The idea that the basic elements of reality should conform to preconceived ideas about what "physical objects" are supposed to look like is an idea Feynman disparaged in chapter 2 of *The Character of Physical Law* ('The Relation of Mathematics to Physics'), and it sounds pretty close to #17 on John Baez's crackpot index:

10 points for arguing that while a current well-established theory predicts phenomena correctly, it doesn't explain "why" they occur, or fails to provide a "mechanism".

20. **Eleggua**
January 2, 2005

As far as I can tell, Michio Kaku's book is NOT about String Theory, or Branes, instead it is about the many worlds hypothesis and theories about cosmology similar to Max Tegmark – see http://www.hep.upenn.edu/~max/.

Now, I understand that the Many World hypothesis is not strictly testable, but are your views about science really as positivistic as you are making out in this post? I mean Science has many concepts which are not observable like information/energy/action but have proven useful in the past!!!

Thanks for your reply in advance Eleggua.

21. **Peter**
January 2, 2005

You can't really blame the publishers on this one. These books are written either by people with serious academic credentials, or by science writers quoting such people. The problem is not that publishers are passing off science fiction as science, but that scientists are doing it.

22. **JC**
January 2, 2005

For many book publishers, isn't revenue maximization the main goal of their enterprise? Whether something is correct in factual and/or scientific terms, seems to be a secondary concern for many publishers. This is possibly one of the reason why there zillions of books on subjects like UFOs, astrology, ESP, political ideologies, etc …

The only time they seem to be concerned about getting things factually correct, is when there's the potential of a libel or slander lawsuit.

Anderson and Others at the Edge

January 4, 2005
Categories: Uncategorized

Several people wrote in this morning to tell me about Phil Anderson's comments about string theory that appeared in the New York Times today. These originally come from John Brockman's "Edge" web-site where he has gathered responses from more than a hundred scientists and others to the question "What do you believe is true even though you cannot prove it?".

Beside's Anderson's answer, also interesting is Paul Steinhardt's. Steinhardt refers to the currently fashionable use of the anthropic principle as "an act of desperation" and "millennial madness", notes that the Weinberg anthropic "prediction" of the cosmological constant gives the wrong value, and even acknowledges that string theory may just be wrong. For sheer weirdness, as usual these days, Lenny Susskind is hard to beat. Brockman doesn't seem to have located any string theorists who believe string theory but can't prove it. Since it can't be proved, I guess even they don't believe it anymore.

Phil Anderson has always been somewhat of an intellectual hero for me. He's really the person who discovered the Higgs mechanism, among many other things. Despite a reputation for being a curmudgeon, at one point he was quite kind to me. At some sort of social event at Princeton to mark students passing their generals, he came up to me and told me that he had graded my solid state physics exam. He complimented me on one problem in particular, one I had got wrong. I had realized something was wrong with my solution of that problem, noting on my exam that the result I was getting couldn't be right and explaining why. He told me that this had impressed him, that one should always know what the result of a calculation should look like before attempting it.

Comments

1. **Chris Oakley**
   January 6, 2005

   Ah yes, I remember this: T & V rishons, or whatever they're called. The sub-structure of quarks and leptons. Maybe when someone constructs a satisfactory relativistic theory of bound states we'll be able to see whether we can test Harari's ideas beyond the mere counting of quantum numbers.

2. **Chris W.**
   January 6, 2005

   See Haim Harari's response.

3. **plato**
   January 6, 2005

   I guess if we are to take you Peter at your word, about where we should spend our money, then such avenues of exploration are then unsuitable?

   A lot of people are picking up on Steinhardt, but lets say we discover something in regards to Planck, how would this effect areas of quantum geometry?

4. **Peter**
   January 5, 2005

   I don't think mathematical physicists doing string theory have gotten any funding that would otherwise go to molecular biologists.

   There's also no evidence that the overhyping of string theory, M-theory, branes, etc. in recent years has lead to any funding for new accelerators. The only thing that has gotten extra funding because of this are the grants going directly to those theorists doing the overhyped work.

5. **Anonymous**
   January 5, 2005

   Freeman Dyson's answer was the coolest.

6. **James**
   January 5, 2005

   Curious layman has two questions

   1. How can you attract large scale funding from either public or private sources without all the nonsensical hype(eg. wormholes, etc). In otherwords, you came clean and stated the impact on everyday life would likely be much smaller than even GR plus you have to pick up a huge bill from the cost of a collider. How do you sell that?

   2. RE: needless complexity of the mathematics. I read where the mathematician Rota stated that developments in knot theory and braids would probably be necessary to solve a lot of problems in *both* physics and biology (eg. protein folding). It has always seemed to me that mathematicians have played a minor role in molecular biology. Could it be that the umbrella of 'String Theory' is a clever backdoor for mathematical physicists to secure significant funds in an attempt to elbow into the turf of molecular biologists?

7. **plato**
   January 5, 2005

   Reading the developement of someone else's thesis is interesting approach to encourage further thought developement?

   That we know full well model comprehension can stimulate other areas, and is always worth looking at for execising the brain's further possibilties:)

   It is vital that such developement understand the evolution of geometrical design(roads to to higher dimensions) to include topological considerations, as part of the developing framework?

   The one common thread relating the twentieth century higher-dimensional models of the previous section is Klein's compactification paradigm. Standard Kaluza-Klein, supergravity, and superstring theories all explain our seemingly (3+1)-dimensional world via compact topologies for extra dimensions. However, one the sidelines there have been concurrent models that do not make use of Klein's idea; models that either make no assumptions about the size of extra dimensions or hypothesize that they have macroscopic extent. Up until very recently, such models were outside of the mainstream because of a fairly strong prejudice in favor of compactification. **But there**

has been a notable change of heart in the community at large, and there is currently a huge amount of interest in models that involve large extra dimensions, which we will now proceed to describe

I know what people think in general about these proposals, but I highlight the question here* above.

8. **Chris Oakley**
January 5, 2005

"In the case of special relativity, the reason why causality implies vanishing commutators at spacelike separation can be found in any textbook"

Not really. The best you can find is the notion that currents comprised of bilinear products of fields commuting for spacelike intervals implies commutation or anticommutation of the fields. It is just *assumed* that commuting currents mean causal separation.

The problem, AFAIC, is *what* currents? Physical states are of positive energy & so the same thing should apply to non-local convolutions of the currents formed by filtering out the positive energy parts. However these do *not* in general commute for spacelike intervals, even when the correct spin/statistics connection obtains.

Causality is in any case about the flow of information. What exactly one means by "information" is hard to define at the quantum level and what one sees in the text books – when they bother to address the matter at all – is (to my mind) far too glib.

9.
January 5, 2005

There are some interesting things here, but I've always thought Edge was one of the (untentionally) funniest sites on the Internet. Something about giving people enough rope to make fools of themselves.

10. **D R Lunsford**
January 5, 2005

Thomas,

C is independent of causality, micro or macro! That is, homogeneity and isotropy of space and time produce a light cone structure – whether or not the world is causal (how it *uses* that structure) is not determined at that level.

-drl

11. **ksh95**
January 5, 2005

Aperion wrote:

…and then, in the real future perhaps, trumpet the accuracy of the few that turned out to be correct. Is this not really pretty silly?

Relax, not everything in life needs to fit into some perfectly rigid framework. Sometimes, it's ok for things to be just fun or entertaining. It's not like the edge is PRL.

BTW

I can't stand all this talk about Occam's razor??? Nature feels no obligation to follow our wishy-washy, sorta-kinda, maybe-sometimes statements, and neither do I.

I propose a new scientific guiding principal. We'll call it Ksh95's Razor.

He who is guided by imprecise, unrigorous, folksy wisdom is an idiot.

12. **Thomas Larsson**
January 5, 2005

In the case of special relativity, the reason why causality implies vanishing commutators at spacelike separation can be found in any textbook, which can explain it much better than me. In general-covariant theories, 3+1 decomposition and the very notion of spacelikeness is problematic, as has recently been emphasized by Savvidou. Her (and Isham's) History Projection Operator formalism is philosophically close to what I suggest. However, she does not explicitly introduce an observer and can hence not formulate observer-dependent anomalies.

However, I tend to understand locality more intuitively. The problem with fundamental, non-local objects is that we cannot transmit information faster than the speed of light (or so I believe). So one endpoint of the string does not know what the other is doing, but only what is was doing a string-length/c ago. This is OK for real-world strings made up of atoms, but it just seems to be wrong for fundamental strings. The same argument applies to non-local observers. One part of a macroscopic, real-world detector does not know what the other is doing in this instant, but that is OK because it is made out of point-like quarks, gluons and electrons, which should be regarded as the irreducible observers.

It might be possible to transmit information faster than light over Planck-scale distances, so microscopic strings might (just might) be consistent with macrocausality. However, the by far simplest way to prevent superluminal signals over macroscopic distances is to forbid superluminal signals over microscopic distances.

13. **Chris Oakley**
January 5, 2005

Thomas,

I am thinking about this problem too. One could argue that the observed spin-statistics connection is excellent evidence for microcausality but personally I am still a little puzzled as to why (anti-)commuting operators for spacelike intervals should necessarily be the expression of the latter.

14. **Thomas Larsson**
January 5, 2005

Chris W ,

It might also help to ask what could physically motivate the transition (at the classical level) from point particles to 2-dimensional objects tracing out worldsheets in *spacetime?*

There are strong physical motivations for **no** transition from point particles to 1D strings (or higher-dimensional branes): locality and causality. These properties, which are fundamental consequences of special relativity, are automatic in point particle QFT by construction. You don't need to be Einstein to realize that there might be problems with locality in a theory of non-local objects – evidently it's enough if you are Gross (though apparently it took him 20 years to take this problem seriously).

One should distinguish between microcausality and macrocausality, where the border between micro and macro is perhaps at 1 fm. Macrocausality is firmly established and will not yield, but microcausality can of course never be tested – you can only push the demarcation line. However, just because microcausality cannot be experimentally tested does not mean that it is violated. QFT is microcausal, and that suffices for macrocausality. From this perspective, the idea of cosmic strings is really outlandish – how can you apply the cluster decomposition principle to a fundamental cosmic string?

I am convinced that the answer lies elsewhere. In QM, observation is a complicated, non-local process, and as such it is in conflict with the spirit of locality and (special) relativity. To make observation into a local process, one needs to assign it to a definite event in spacetime, and to treat the observer's trajectory as a material object to be quantized together with the fields. This is clearly a very small fix to the logical structure of QFT, and it does not really involve any new physics, but it has a profound consequence: new types of gauge and diff anomalies arise, which cannot be seen in conventional QFT because the cocycles are functionals of the observer's trajectory.

This does not rule out any new physics, of course, although observations do not seem to indicate anything beyond the SM and GR. But no matter what happens in future experiments, the insight that we need to localize the process of observation and that this leads to new anomalies can not be undone.

15. **Chris Oakley**
January 5, 2005

    Excuse me for pointing out the obvious, but if one applied the Principle of Parsimony, aka Occam's Razor, consistently, one would never have arrived at String theory in the first place.

16. **Apeiron**
January 5, 2005

    btw – bad spelling intentional

17. **Apeiron**
January 5, 2005

    I'm leaving this post – as admittedly a low investment observer.
    Observations:

    -Typical Question –

    Peter:- String theroy is so terrible – Lubos Motl is an idiot!

    Lubos:- Peter is not even qualified to understand why my superior knowledge is vodka-stle-eastern-european-vodka correct.

18. **Chris W.**
January 4, 2005

    Chris O,

    Given my characterization of string/M-theory as a theory generator, Occam's razor is more properly applied to the theories (models*) it generates. The question then becomes, what physical principles can we invoke to select among the generated theories?

    Consider general relativity. Its formulation in terms of Riemannian geometry inspired the consideration of numerous alternatives using more or less the same mathematical framework. The latter could be looked upon as a meta-theory from which one could draw many candidate physical theories for describing gravitation, some claiming to incorporate other physical fields in some kind of unification. However, the physical ideas underlying GR result in a remarkably restrictive sieve for choosing among alternatives; indeed, in the end it is hard to come up with viable alternatives.

    At least, this was the case before quantum gravity came on the scene, with GR regarded as a low energy effective theory. The question now becomes (in the QG context), what physical principles can help us select among the mess of alternatives offered by the string/M-theory meta-theoretical framework? After two decades of investigation we still don't know. It might help to at least state the problem in this way.

    It might also help to ask what could *physically* motivate the transition (at the classical level) from point particles to 2-dimensional objects tracing out worldsheets in spacetime? (Taking "physics as geometry" to the next level doesn't cut it, in my opinion; more on that later.) The original motivation in hadron physics seems completely irrelevant now. What should (or could) replace it? The mathematical discoveries that enthralled Witten and others in the early 80s strike me as things calling for an explanation—for deep physical insight—not just a mathematical treasure hunt.** Of course this is especially difficult when the original physical motivation is so obscure, and the mathematical territory is so vast and intriguing in its own right.

    —
    * BTW, I've always had some discomfort with the term *model*, regarding it as something of a weasel word.

    (** Peter's recollection of his encounter with Philip Anderson is relevant here.)

19. **Peter**
January 4, 2005

    Sorry Danny, but I'm a big believer in minimizing the amount of trouble software brings into your life by sticking to the defaults when they are something you can live with, and this default is something I can live with.

20. **D R Lunsford**
January 4, 2005

    Oak –

    How right you are! In fact KK theory is the ultimate theory generator. If you can make bowling balls generally covariant, you can strike up a theory of unified bowling ball gravity in your spare time. Then you can refer your theory to a moving frame. Of course, in the end you'll still never make a 7-10 split.

XXXXXXXXX/

21. **D R Lunsford**
January 4, 2005

Peter – aside – could you please change the font color to "black"? For some reason the MT people think charcoal grey is a nice default color for text.

Here's how:

http://www.elise.com/mt/archives/000677colors_background_banner_headline_and_font.php

-drl

22. **Apeiron**
January 4, 2005

I read the entire Edge comment group. Nearly all the postings involve saying something like:

"I personally believe that X is true, and I think that in the future, X will be universally believed to be true by people like me."

This is really quite a meaningless statement, and in fact may involve a serious misunderstanding of the nature of truth or potentially the conception of future and past.

I suspect the point of the exercise is to make unsubstantiated claims and then, in the real future perhaps, trumpet the accuracy of the few that turned out to be correct. Is this not really pretty silly?

23. **Chris Oakley**
January 4, 2005

Chris W,

The paradigm shift therefore is away from Occam's Razor towards something much more bizarre. I would never go there, as I view the activity as opening a Pandora's box. How many other "theory generators" besides M-theory could one dream up, I wonder? In any case, what satisfaction is there to be derived from explaining simple things in a complex way?

24. **Levi**
January 4, 2005

The response from Steve Giddings is also interesting. He thinks he knows what went wrong with Hawking's original argument about information loss in black holes. An excerpt:

"We base our argument on a principle we call the locality bound. This is a criterion for when physical degrees of freedom can be independent (in technical language, described by vanishing of commutators of corresponding operators). Roughly, a degree of freedom corresponding to a particle at position x with momentum p and another at y with momentum q will be independent only if the separation x-y is large enough that they are outside of a black hole that would form from their mutual energy. I believe this is the beginning of a general criterion (which will ultimately (be) more precisely formulated) for when locality breaks down in physics."

I don't know what to make of this idea.

p.s.: Sorry this comment is a bit off-topic.

25. **Chris W.**
January 4, 2005

At least one respondent, Keith Devlin, started by examining the problematic notion of *proof* itself. In practice, proof means (for many people) something like social or cultural acceptance—an idea is proven when it becomes widely discussed and applied, or commonly used as a jumping-off point for further investigations. Here "proven" is a shorthand for "proven to be of value"—interesting and useful at least, and at most, integral to how one understands the world.

Personally, I think that in the empirical sciences we should limit our use of the term to the sense in which mathematicians and logicians use it—ie, as showing that a conclusion follows logically from given premises. As Devlin discussed, this notion of proof is problematic enough by itself. I see no need to muddy the waters by talking about "proving" Newtonian mechanics or any other empirical generalization. It is enough to know that a theory is a conjecture that has been rigorously and broadly tested, and has stood up well to all (or most) of the tests. That fact alone makes the theory interesting, and in most cases useful as well.

So, the attention should be focused on tests—how can we know when a theory is wrong? Many people mistrust string theory because it apparently offers so many ways to avoid confronting this question. However, I get the sense that for many string theorists the question is irrelevant. They regard string theory (and M-theory) as a fabulously rich meta-framework from which one can derive a huge variety of interesting models, and modeling is what physics is really about. At least some of the models should be testable, but there is no reason why the meta-framework should be; its purpose is to spawn models.* All one can ask is that some of the models show some promise of producing successful physical predictions. In short, string/M-theory is not a physical theory in the conventional sense; it's a meta-theory and theory generator. Is a theory generator what we really need?

—-
* Of course, the meta-framework is such that the models are linked by a web of deep mathematical ideas and relations, which makes them interesting even to people who only care about solving mathematical problems.

26. **Apeiron**
January 4, 2005

If Susskind really doesn't understand why probability theory works [given certain very basic assumptions about the underlying processes involved] then it's not surprising he thinks that his impressionistic landscape stories and graphics are a real achievement.

27. **Peter**
January 4, 2005

Anderson has always had some degree of resentment about the way particle physics often views itself as the most important subfield of physics. He was far from the only one who felt that the cost of the SSC was crowding out funding for other subjects. I remember arguing at the time with various such physicists and mathematicians that if the SSC were canceled they certainly weren't going to see any of the money, which turned out to be right.

28. **DMS**
January 4, 2005

I have mixed emotions when it comes to Phil Anderson.

I agree that his contributions have been notable (actually, Schwinger also had a bit role when it comes to Higgs mechanism).

As an ex-particle theorist, I find it hard to accept that he advised the Congressmen to cancel the SSC and said that “particle physics is no more fundamental than any other branch of physics, \emph{ in any way whasoever}”(I don’t think his testimony was pivotal: budget considerations and lack of political support in the new administration killed it). My opinion is similar to Weinberg as he stated in “Dreams of Final Theory” on the subject: it is about the convergence of arrows of explanation.

But he is correct that reductionism is not everything. I think he may agree that “many-body physics”, be it relativistic as in QFT or NR as in condensed matter physics, is.

29. **plato**
January 4, 2005

Just so people understand this was a selective process (the many names that are presented) by which one will choose what to accept into their world or not?

I think Nicholas Humphrey deserves a lot of credit for thinking of the question, **“WHAT DO YOU BELIEVE IS TRUE EVEN THOUGH YOU CANNOT PROVE IT?”**🙂

If the rational, is to accept matter distinctions as the most relevant, then such thought manifestations have no other basis then the brain:)

Rest easy then:)

What is a Brane?

January 6, 2005
Categories: Uncategorized

Greg Moore has written an article for the latest Notices of the AMS entitled “WHAT IS… a Brane?”. He begins by noting that “The term ‘brane’ has come to mean many things to many people” and one of the difficulties of the subject is that one has to figure out from context what sort of brane someone is talking about.

For the case of branes of dimension greater than one, in general no one knows how to consistently quantize such objects. See Warren Siegel’s research summary for comments about this. He notes that now that M-theory shows that non-perturbatively strings are membranes in 11d, one doesn’t have a finite quantum theory of gravity, since membranes are infinite even in perturbation theory. All one has is an effective low-energy supergravity theory, which is what one had before one got involved with string theory. While at Siegel’s web-site, check out his latest parody paper called “The Everything of Theory”, which includes the following lines:

“The real problem with string theory is that there is no alternative. However, the reason there is no alternative is that no one ever bothers to look for one; in fact, there is a strong resistance to even considering looking for one. Consequently, practically all theoretical high energy physics (and even most of phenomenology) is now string theory. Thus, string theory is not so much the Theory of Everything (since it explains nothing), but rather the “Everything of Theory”, since it now encompasses all of theory. This era in string research is strongly reminiscent of the Dutch tulip trade just before the Tulip Crash of 1637. ”

I, for one, am missing the joke here…

For some new hyping of a different kind of brane, see the latest Nature, which has a piece on Nima Arkani-Hamed. Equal time is given to LQG, with a similar piece on Martin Bojowald.

Update: Lubos Motl has a long posting about branes and M-theory, which explains many things. As usual though, he insists that the full dynamics of the branes in M-theory is completely determined and unique even though he doesn’t know what it is in any phenomenolgically realistic background. To get a finite quantum theory of gravity that has anything to do with the real world out of M-theory, you need to show that you can get well-defined, finite results for the dynamics of these branes in the case of four large dimensions, the rest small. As far as I can see any claim to have this now is purely wishful thinking.

Comments

1. **Peter**
   January 11, 2005

   I think you’ll find that if you talk privately to many string theorists, they are very unhappy with the present state of the field. A common opinion would be that the standard picture of a 10/11 d string/M-theory TOE has been incredibly overhyped and now looks unlikely. But they’re not sure what else to work on. One of the few intellectually alive sub-fields of particle theory is that of trying to really understand QCD via its supposed string dual. A lot of people are working on that, keeping quiet about their doubts about the TOE. Siegel is one of the few not keeping so quiet (he has tenure, for one thing…).

2. January 11, 2005

   I read much of Siegel’s webpages and I’m surprised he seems to be able to hold contradictory opinions in the same webpage! In his “serious” pages, he is a proponent of string theory and he describes his work on it but in his parodies, he becomes very critical of string theory.

3. **Peter**
   January 7, 2005

   Well if some of the features of the physics look like they come from a vibrating string, it’s perfectly reasonable to try to model strong interactions with a quantum string. It just doesn’t work, or at least not in anything like the simplest such way of “quantizing a string”.

   I’d heard that lots of people had suggested a fourth quark with the right quantum numbers around 1964, but never that Zweig was one of them. The idea only became really compelling in 1970 with the GIM mechanism.

4. **D R Lunsford**
   January 7, 2005

   Peter – you mention Glashow and his 4th quark. Remember that George Zweig had his model of “aces”, which were really just 4 quarks at the outset.

   -drl

5. **D R Lunsford**
   January 7, 2005

   Peter –

   It looks like Siegel is carrying on the parton and Veneziano dual-resonance ideas in his own way, with modern tools. So he’s sort of going back to before gauge theory and s.s.b.!

   One thing I never understood – it seems that string theory got started because the dynamics of these models looked like that of a vibrating string. Why did they make the leap of faith to say that it really *is* a string??

   -drl

6. **Peter**
   January 7, 2005

   DMS: Sure, if when particles collide at the LHC, energy starts disappearing into extra dimensions, that would be a big deal. It also would be a big deal if these collisions caused the archangel Michael to appear at the collision point. These both seem about equally likely to me.

   Glashow and others had good arguments for a fourth quark. This was a simple extension of the three quark model that made the whole thing simpler and more elegant.

The extra dimension stuff is nothing at all like that.

ksh95: Yes p-branes for p>1 are described by a non-renormalizable sigma-model. Look at your favorite way of quantizing the string, increase the dimension of the world-sheet above 2 and see what happens.

Danny: I don't really understand exactly what Siegel has in mind, but he is clearly more interested in finding a 4d string that would help one understand QCD, rather than a 10d string that would explain gravity.

7. **DMS**
January 7, 2005

Getting to the article, I think I would agree with Nima that *IF* brane concept turns out to be correct, it will indeed be the biggest thing in science in 300 years. Don't you think?

Of course, I don't think it will be found, but who knows…

Maybe, it will be like the charm episode in particle physics, when Glashow made a bet that charm would be found with an estimate of the mass, and he won the bet(If my memory serves me right, it was based on the GIM mechanism based one-loop calculation of the K0-K0bar mixing.). But my understanding is that no one believed his prediction (unlike the case now where anyone who "matters" believes in it).

8. **ksh95**
January 7, 2005

Whoa, whoa, whoa,

You're telling me that branes give infinite results, even in perturbation theory?

What's this about? Are they doing S-matrix calculations or something else?

I'll ask around, but in the meantime if some one could kindly explain the present state of affairs I'd be much ablidged.

9. **Thomas Larsson**
January 7, 2005

A Brane New World always reminds me of a Swedish dotcom company, by Huxley's original name A Brand New World. It defaulted a couple of years ago, after burning more venture capital than Boo.com.

As they say on the stock market: never try to catch a falling knife. No matter how much the string stock falls, there is always 100% left to the bottom.

10. **plato**
January 6, 2005

sorry Peter it won't happen again

11. **D R Lunsford**
January 6, 2005

Peter –

I read thru all of Siegel's comments and it seems that he regards the useful part of ST as a phenomenological expression of some kind of theory in which X can be regarded as a bound state of Y andZ, Y and Z can be regarded as bound states of X and W, W can be regarded.. etc. etc. IIRC, ST had its origin in a naive nuclear model in which nucleons were literally banded together, one wrote a Lagrangian for the bands with their tension etc. etc. Is this a correct summary?

-drl

12. **Peter**
January 6, 2005

This has really gotten out of hand. I'm quite annoyed that people are ignoring my request to not post multiple off-topic comments and not to respond to those who do. This is damaging something that belongs to me and I'm about to start deleting all such comments. Please do not continually submit multiple comments attempting to turn this into a forum for discussion of your private interests. This is extremely rude and I'm not going to put up with it any more.

13. **plato**
January 6, 2005

Okay don't let it happen again.:)

Alan Sokal saids:A lot of the blame for this state of affairs rests, I think, with the scientists. The teaching of mathematics and science is often authoritarian

Now who are you going to believe and quote, when you've cried wolf?

The more one adds, the crazier it gets:)

14. **plato's assitant**
January 6, 2005

I'm sorry Plato.

As you guys guessed, Plato's assitant doesn't exist.

15. **plato**
January 6, 2005

Plato's Assistant,

It is unfortunate that you have refused to keep yourself confined to the club, (url listed on my name), that we will have to suspend you until further notice.:)

Brane and Brain, are not the same thing.:)

As to understanding the gist of my resources, you might find such accumulation of the data very interesting, and why, any attempts to devalue, is only a rejection of the reality that has been moved into this century:)

Cater to Peter's request, if you dare 🙂

16. **D R Lunsford**
January 6, 2005

Apparently, there really was a Tulip Crash:

http://slate.msn.com/id/2103985/

Most amusing is the noted battle of "Wittstock":

http://www.fact-index.com/b/ba/battle_of_wittstock.html

Obviously this was the first real-world test of flower power. (King Jiminy Hendrix of the Duchy of Fender prevailed over Count Rey Josef und die Fische. After the victory, a great celebration of music, a free "concerto grosso" was held at Maximilan Yasgar's fuedal estate. This was the first appearence of the band then known as Jefferson Bargepole with Grace Slick, a stimulating teenager from San Francisco di Zitterbewegung.)

17. **Chris W.**
January 6, 2005

This must be the Bogdanoffs' secret. Thousands of students could save themselves the expense and risk of buying term papers and phony theses with only a minimal decline in the quality of the work they submit.

18. **D R Lunsford**
January 6, 2005

Peter –

A few simple mods of the Postmodernism Generator should do fine! Just throw in a few classic formulas pulled from Dirac or Messiah, and make the obligatory reference to the CMB, and a postscripted estimate of the cosmological constant, references to Witten, etc. Making up references would be fun!

10. Stringfellow, Ariel. "Step-Ladder Potentials for Propagating Slinkies" *Physics Yarns* p. 278-314

-drl

19. **Peter**
January 6, 2005

It always seemed to that it couldn't be that hard to modify that guy's software to generate scientific papers about branes. Come on, doesn't anyone want to try this?

20. **Plato's Assistant**
January 6, 2005

Monsieur Plato is on holiday. These comments are computer-generated. Disparaging remarks concerning their coherence (or rather, the lack thereof) will, as usual, be ignored.

21. **plato**
January 6, 2005

DRL,

Mushrooms won't help you in your calculations, and push you any further.:)

Even if it's a primordial kind of thing that you think helps you, you must recognize other information is present from way back when? Please do not poison yourself with the analogies of higher dimensions that you attribute.

How did you get as far as you did?:)

22. **D R Lunsford**
January 6, 2005

Plato,

Where can *I* get some of these mushrooms?

Thanks in advance!

-drl

23. **Plato**
January 6, 2005

It's Brane New World?

Branes reside in the hidden dimensions, known as "the bulk." While matter and light stick to the branes, gravity traverses both branes and bulk. The hidden dimensions cannot be seen because only gravity can go there.

Maybe the tulip trade is reminscent and inferred by Veltman and Hooft, as a realization of a cosmological principle that can be mapped holographically, even if Hooft is overwhelmed?

Finally, we describe the embedding of branes in the 5d bulk using the phase space geometric methods developed here. In this language the boundary conditions at the branes can be described as a 1d curve in the phase space. We discuss the naturalness of tuning the brane potential to stabilize the brane world system.

You just have to adjust your views to the bulk is all?:)

Complex Structures on the Six-sphere

January 9, 2005
Categories: Uncategorized

A preprint by Andrei Marshakov and Antti Niemi appeared on hep-th this evening making a remarkable claim. According to this preprint, a few weeks before passing away recently at the age of 93, Shiing-Shen Chern completed a preprint entitled "On the Non-existence of a Complex Structure on the Six Sphere".

Whether or not a given manifold defined using real coordinates can be given the structure of a complex manifold is often a difficult problem. For the case of a d-dimensional sphere, clearly you can't do this in odd dimensions, but for even dimensions, you certainly can for the case d=2. For the cases d=4 and d=8 or more, there is a topological obstruction to even finding an "almost complex structure". In other words, you can't find a continous choice for each point on the sphere of what it means to multiply elements of the tangent space by the square root of minus one. The case d=6 is special: you can use the octonions to construct an almost complex structure, but this complex structure is not "integrable", it doesn't come from any local choice of complex coordinates. One of the most famous open problems in geometry has long been the following: is there another almost complex structure on the six-sphere that is actually integrable?

It has long been conjectured that there is no such integrable almost complex structure, but no one has ever been able to prove this. Chern's preprint contains a purported proof, but Marshakov and Niemi devote only a paragraph to the non-trivial part of his argument. From their preprint you can't tell whether Chern has a valid argument.

I've heard via e-mail from a knowledgeable authority on the subject who points out that there are serious flaws in the manuscript that was privately circulated. His opinion is that Chern's argument actually does prove something interesting, but not the full result Chern claims, so the conjecture about the non-existence of a complex structure on the six-sphere remains open.

Comments

1. **Peter**
   January 11, 2005

   The section of the paper on "Chern's Last Theorem" is almost entirely devoted to explaining well-known facts about G_2/SU(3)=S^6, including the well-known fact that the almost complex structure you get from the octonions is not integrable. I'm pretty sure all of this material goes back to Elie Cartan in the early part of the last century. The new argument due to Chern is only dealt with in the last paragraph of the section (I said it was only one paragraph, not one sentence. String theorists have a weird inability to quote me correctly…). It's the argument in this last paragraph that is new, but also has a hole in it.

2. **DMS**
   January 11, 2005

   Peter,

   You have captured what I tried to say. And in the process you have given me a glimpse of the complexities and subtleties of (dis-)proving statements in mathematics. Thanks!

3. **Lubos Motl**
   January 11, 2005

   It is not true that there is just one sentence dedicated to Chern's proof in Marshakov et al. Look at their conclusions – and you will see that they say that "we have explained his proof in detail". I also think that there's a lot of it over there.

4. **Peter**
   January 11, 2005

   I think what you mean by "physicist's proof" is something along the lines of what a mathematician would call an outline of a proof, with some steps in the outline possibly justified by appeal to a physical argument that such a step has to somehow work out correctly.

   There certainly are some reasonably good examples of this kind of thing. One would be Witten's work on supersymmetry and Morse theory. There he outlined an argument relating Morse theory, Hodge theory and index theory, using at crucial points expectations of what should happen based on semi-classical approximation techniques in quantum mechanics.

   But the Ricci-flow case is very different. Based on personal experience, I can tell you that claims by some physicists that the RG provides a "physicist's proof" of geometrization have convinced some mathematicians that said physicists are arrogant idiots. In this subject you can't assume that metrics are well-behaved, since they aren't. Singularities develop when you follow the Ricci flow, and understanding what happens then is what the whole subject is about. There is no "physical" argument for ignoring this phenemenon. If there were it would imply an infinite number of untrue mathematical theorems.

5. **DMS**
   January 11, 2005

   Someone wrote:
   "If it's wrong for a mathematician, it must also be wrong for the physicist."

   Of course that is obvious: don't need a Harvard education to see that.

   The question was: is there a strong physical reason for one to believe a particular statement to be true. Of course, such physical reasons may turn out to be wrong. Alternatively, the statement may be correct, but may require newer mathematics to demonstrate that rigorously (like Dirac delta functions —> distribution theory).

   "A difference between a physicist's proof and a mathematician's proof can only exist in physics where the physicists know what they mean and mathematicians are slower."

   Well, the recent proof of Thurston geometrization conjecture relied on Hamilton's Ricci flows, reminiscent of RG.
   http://www.math.columbia.edu/~woit/blog/archives/000077.html
   A physicist would be happy if he could show it for "well-behaved metrics", not a ("slow") mathematician.

6. **plato**

January 11, 2005

It's important that we can see where you are both focused instead of having the ramble of content that deters from appropriate discussion.

So we now have you both fixed in the appropriate direction, so that you can philosphically:) discuss the idea being expounded at the forefront of our knowledge base.

Is one, going to reject the mathematics of another? On what grounds? Physics?:)

7. **Lubo? Motl**
January 11, 2005

Impressive guy (Chern), even it it's not right.

Someone asked whether the proof is valid as a "physicist's proof". That's a ludicrous question. A difference between a physicist's proof and a mathematician's proof can only exist in physics where the physicists know what they mean and mathematicians are slower.

In mathematics, for example in discussions about the existence of a complex structure, the physicist's proof and a mathematician's proof is the same thing. If it's wrong for a mathematician, it must also be wrong for the physicist.

8. **plato**
January 10, 2005

At planck scale with topological considerations, no determination can ever be foolproof, as it is conjectured math already based, unless, you willingly admit and move into abstract spaces, detached from reality:)

But its still fun to conjecture?

9. **Peter**
January 10, 2005

The Marshakov-Niemi paper just gives an outline of Chern's argument, it doesn't in any sense provide a "physicist's proof".

10. **DMS**
January 10, 2005

What about Marshakov-Niemmi paper itself? Is it valid as a "physicist's proof"?

That is, even if Chern was wrong, it may be that someone is going to prove it rigorously, so it is basically correct (like a lot of QFT proofs).

11. **Matti Pitkanen**
January 10, 2005

The claim that S^6 does not possess complex structure is very interesting from TGD point of view. Since I cannot expect that anyone sees the trouble of getting bored of what follows I demonstrate my deep ignorance by representing my question immediately:

Does the possible existence of complex structure in S^6 imply the existence of Kaehler structure?

In the following I try to the physical meaning of Chern's last theorem emerges in TGD Universe.

1. Number theoretical dynamics for space-time surfaces

The motivation for my question comes from what I believe but cannot yet quite prove(;-). I have been working hardly to concretize my belief that the classical dynamics of space-times identified as 4-dimensional surfaces in 8-D imbedding space M^4xCP_2 can be expressed purely number theoretically. I have gone through several variants of the hypothesis and thought to describe one option since it seems most promising at this moment and also relates very closely to the Chern's last theorem.

The first guess was that tangent spaces of H and M^4 could be given octonionic resp. quaternionic structures. The Minkowskian signature however forces to modify the approach by replacing these structures by their hyper counter parts obtained by multiplying imaginary units by commuting sqrt(-1).

[This structure is not identical with the commutative algebra structure that I called hyper-complex structure and talked about in an earlier email containing several mistakes.]

Clearly a number theoretic analog for the transition from Riemannian to pseudo-Riemannian geometry is in question. M^4 resp. M^8 could be seen as a sub-space of complexified quaternions resp. octonions. This space does not form field nor algebra but this can be tolerated: for instance, the overall important notion of hyper-prime makes sense. For instance, in the quaternionic case the interpretation would be as four-momenta satisfying automatically the stringy mass formula M^4=p. Light-like hyper numbers having no inverse correspond to massless 4- or 8-momenta and hyper-units to Lorentz boosts in analogy with pslash/m in the case of Dirac equation. The notion of pole of analytic function is replaced with lightlike 3-surface, etc…

2. Number theoretical variant of spontaneous compactification as M^8M^4xCP_2 duality?

Since HO/HQ power series with real coefficients give end result in HO/HQ, the notion of HO/HQ manifold with hyper transition functions between coordinate charts makes sense. HO analyticity is not plagued by the complications due to non-commutativity and non-associativity. The reason is that this notion results also if product is Abelianized by assuming that different HO imaginary units multiply to zero.

The problem is that M^4xCP_2 very probably does not allow HO structure.

Here comes in rescue the old idea is that four-surfaces in M^8 define four-surfaces in M^4xCP_2 in a natural manner and vice versa assuming that field equations are satisfied. This duality would be a number-theoretical counterpart of spontaneous compactificatition. It would have nothing to do with dynamics but would be one item in the list of dualities relevant to TGD.

3. Justification for the number theoretical spontaneous compactification

The justification for the hypothesis comes from following observation. The space of quaternionic sub-spaces of octonions with a priori fixed complex structure (containing a fixed octonionic imaginary unit) is CP_2. Same applies in the hyper case.

This means that if one has a four-surface X^4 in M^8==HO with a hyper-quaternionic tangent space and a fixed complex structure, it defines a surface in M^4xCP_2. The

M^4 coordinates of a given point are obtained by a canonical projection from M^8 to M^4 and CP_2 coordinates as parameters characterizing the HQ tangent space at given point.

One can assume that the local complex structure depends on space-time point and thus characterized by a map

f: X^4–>S^6.

It is here where S^6 pops up in TGD framework.

4. Foliations of HO=M^8 and M^4xCP_2 by space time surfaces maps g: OH–>SU(3) satisfying integrability conditions.

For a given map f: HO –> S^6 defining a local preferred imaginary unit M^8-M^4xCP_2 duality allows to construct a foliation of HO/M^4xCP_2 by HQ space-time surfaces in terms of maps

g: HO–> SU(3)

satisfying certain integrability conditions guaranteing that the distribution of hyper-quaternionic planes integrates to a foliation by 4-surfaces.

The reason is that the bundle projection SU(3)–>CP_2 defines the local tangent plane at each point of SU(3). The foliation defines a four-parameter family of 4-surfaces in M^4xCP_2. The dual of this foliation defines a 4-parameter family HQ space-time surfaces.

5. Hyper-octonion analytic functions OH–>OH as a solution to the integrability conditions?

HO analytic functions HO–> HO with real Taylor coefficients provide a physically motivated ansatz, which might satisfy the integrability conditions.

a) The basic observation is that the complexified octonions decompose as 1+1+3+3bar under SU(3) automorphisms leaving a preferred imaginary unit fixed (easy to remember if you have heard about leptons and quarks!).

[Notice that SU(3) has interpretation as color group and isometry group of CP_2 whereas U(2) would act as vector fields in the tangent space of X^4 as well as the holonomy group of CP_2 identifiable as electro-weak gauge group. Standard model gauge structure would have purely number theoretical origin.]

b) If you have a map HO–>HO you can form tensor product 3x3bar of the states of 3 and 3bar states defined by the map and identify it as a Lie algebra element of SU(3) and exponentiate it to an element g of HO-local SU(3). The resulting map g defines the foliation of M^4xCP_2 by hyper-quaternionic space-time surfaces as desired.

6. How Chern's last theorem could be relevant for TGD?

If the conjecture holds it would mean that the foliations giving families of solutions of field equations are characterized by two functions.

a) The function f: HO–>S^6 characterizing the selection of preferred imaginary unit in OH. Physically the function would characterize the choice of the ground state. This function has also an interpretation as SO(7) local group element with SO(6) gauge invariance. The interpretation would be in terms of zero modes.

It is here, where Chern's last theorem becomes relevant. The interpretation as zero modes would conform with the claim that S^6 does not allow complex structure. Indeed, Kahler structure is absolutely essential for the identification of these degrees of freedom as quantum fluctuating degrees of freedom in infinite-dimensional context. As a believer on TGD based world view I would dare to believe also the non-existence conjecture.

b) The hyper-octonion analytic function g: HO–>SU(3) or effectively g: HO–>CP_2 guaranteing the integrability conditions would be second function involved with the general solution ansatz. Here complex structure is present and quantum fluctuating degrees of freedom are in question.

7. Is number theoretical dynamics equivalent with absolute minimization of Kaehler action?

The basic conjecture is that the absolute minima of Kaehler action correspond to the hyper-quaternionic surfaces in the proposed sense. The enormous vacuum degeneracy of Kaehler action would relate to the local selection of octonionic imaginary unit characterized by the map f: HO–>S^6. The known facts about the solution spectrum of Kahler action conform with the proposed general picture.

This conjecture has several variants. It could be that only the asymptotic behavior of absolute minima corresponds to a hyper-octonion analytic function. It could also be that maxima of Kaehler
function K of configuration space of 3-surfaces of H, with K being determined by absolute minimum of Kahler action, correspond to this kind of 4-surfaces. Etc…

With Best Regards,

Matti Pitkanen

The Problem of Predictivity

January 11, 2005
Categories: Uncategorized

In recent years, as it has become clear that string theory can never be used to predict anything about the real world, string theorists have reacted to this state of affairs in various often bizarre ways. Tonight there's a new review article by Steve Giddings about string theory which doesn't even pretend that the theory will ever make a real prediction about anything. Giddings seems to think that the particle phenomenology archive hep-ph is the place to post this kind of thing, not the hep-th archive devoted to less experimentally based work. This is pretty funny, but the really hilarious thing is the way Giddings motivates string theory. In a section entitled "The problem of predictivity" he argues that our inability to make quantum gravity predictions at high energy is a problem of supreme importance, then goes on to use this to motivate the introduction of string theory, which in the end gives a theoretical framework unable to predict anything about anything at any energy.

The review does actually claim at various points that string theory "predicts" gauge theory, fermions, supersymmetry, Dp-branes, and the cosmological constant. It just neglects to mention that it doesn't predict any characteristics of any of these things (value of the cosmological constant, any observable characteristic of a Dp-brane, how supersymmetry is broken, what kind of fermions, what gauge groups). String theory actually has nothing at all to say about even the things Giddings claims it "predicts".

Giddings seems to be a hard core anthropist, he ends with the exciting recent insight from string theory that:

"It may in fact be that anthropic considerations fix the small relative size of the Higgs mass as compared to the Planck mass. If so this ultimately answers the question we started with, 'why is gravity so weak?' This is clearly a very interesting line of research, and debate continues on these and other important points."

Actually this is only the next to last paragraph. He finally ends with the news that exp{10^120} years from now our region of the Universe will spontaneously decompactify, which he thinks is pretty kewl.

With the current anthropic nonsense exemplified by this review article, string theory has finally reached rock-bottom. It has given up any claims to being a legitimate science and has taken on the characteristics of a cult. It is long past time for those leaders of the field with any remaining scientific integrity to take a public stand that what is going on is not all right.

Perhaps this is too much ranting. My excuse would be that I'm not in the best of moods because I've spent my entire break between semesters being sick (don't worry, I'm getting better). I just can't believe the way essentially the entire particle theory establishment, including many people I have the highest respect for, continue to allow this situation to go on without public comment.

Update: Lubos Motl has news of a new, more elaborate set of anthropic nonsense coming soon from Savas Dimopoulos, Shamit Kachru, and his senior colleage Nima Arkani-Hamed (their innovation is to divide the landscape up into "countries". I kid you not). Lubos evidently has seen this paper early, the rest of us will have to wait until tomorrow night. Even though he pretty clearly sees how unscientific this is, he has to try to find something nice to say about it since his career depends on these people. Sad to watch, actually. Postdocs and untenured people can't take on the fight against this garbage unless they want to commit career suicide. It's up to the tenured people. Where are they?

Further update: It seems the "countries" terminology is due to Lubos, the authors refer instead to breaking the landscape up into "friendly neighborhoods". Which sounds even sillier than "countries".

Yet further update: The Arkani-Hamed, Dimopoulos, Kachru paper is now available. It consists of about fifty pages of few equations and highly convoluted anthropic sorts of arguments, not about any particular theory but somehow about whole classes of theories. Kind of a meta-argument. They don't seem to get anything at all like an actual prediction of anything out of this, the closest they get is in their conclusion about what to expect at LHC energies:

"Instead of finding a large spectrum of new particles and interactions typically needed for naturalness, we predict sparse models with few new particles and couplings, with dimensionful parameters finely tuned but close to dangerous environmental edges."

Pretty poetic, but I think the experimentalists working on the LHC detectors are going to have trouble using that as guidance as to what to be looking for.

Comments

1. **CapitalistImperialistPig**
   January 19, 2005

   Anthropicy is clearly a pretty desperate strategy, but what's a girl to do? QGr has been stubbornly resisting all sorts of attacks for 60+ years. What I object to is the string theory thought police, who seem to regard their theory as sort of a religion, and all other approaches as heresy. It's just possible that the QGr problem won't be solved without some new fundamental data.

2. **Quantoken**
   January 13, 2005

   Peter said:
   "The problem with the quantized superstring is not that it can't describe interesting physical degrees of freedom, but that it can describe almost anything. It's too ill-defined for anyone to be able to show it is inconsistent, but it is increasingly clear that it is VACUOUS ."

   I find it very amusing in the last word. Thanks for the humor, but string theoreticians are over-qualified languists in inventing new words:-)

   ST doesn't just describe "almost" any thing. It describes much much more than everything in the world adds up. That's exactly the problem. When the complexity of a theory allows it to describe more than what this universe can hold, that's no longer a physics theory since physics strictly limit itself to the observables within this universe only.

   I keep seeing STers sing the praise how ST is rich of mathematics structures and how much beauty you can find within the theory, and they can't believe that a mathematical frame work so rich in content could have nothing to do with the real world at the end.

   What they don't realize is it is exact the problem when you have a theory too rich and provides too much structure than what is needed to encompass the whole observable universe!!! It makes the whole thing "vacuous" when you realize just what an insignificantly small portion of that theory actually describes the universe 🙂

   One could propose a theory living in 137 dimentions, it surely will provide even richer structures than the current 11-D string theory. It would also be more "vacuous".

   I think even a flat 4-D theory is alittle too bigger than what the universe can hold, as we already see. A theory of flat, infinitely extending 4 dimentional spacetime of precise coordinates provides more structures and information than what the actual universe holds. The result is we see a universe that is limited in size and has fuzzy coordinates. The richness of the universe is bounded by boundary, so it is described by the conditions of the 3-D boundary of the 4-D spacetime. That's the holographic view.

Quantoken

3. **D R Lunsford**
January 13, 2005

Peter – OK fair enough, but if you can't make a non-Abelian gauge theory from line sources I just can't see how YM is going to work…perhaps I'll post something to SPR about it.

-drl

4. **Peter**
January 13, 2005

Hi Danny,

Please don't repeat the argument. I took a look again at your comment I deleted, and it still seems to me that it is about a calculation that interests you but doesn't have anything to do with the topic of this posting (predictivity and anthropic reasoning in string theory).

I don't think you can convincingly argue that the current superstring theory framework can't support YM fields. The problem with the quantized superstring is not that it can't describe interesting physical degrees of freedom, but that it can describe almost anything. It's too ill-defined for anyone to be able to show it is inconsistent, but it is increasingly clear that it is vacuous.

5. **D R Lunsford**
January 13, 2005

Peter – the point of the comment you deleted was – I don't even see how string theory can support Yang-Mills, much less be a TOE. I thought this was on-topic.

I will repeat the argument if you wish.

-drl

(Sorry if I resort to bad humor sometimes. Nip nip nip.)

6. **superpoincare**
January 13, 2005

This landspace stuff is seriously leading nowhere. Weinberg's original paper was "OM" and had an interesting result.

I think this "landscape" business is just playing with words. Finally string theory has a lot of ground states and one doesnt know which one to pick. The best thing is that _none_ of these vacua come close to describing the real world.

7. **Quantoken**
January 13, 2005

Peter said:
"The only other senior person besides Gross I can think of who has spoken up about anthropism is Steinhardt. It really is remarkable to me that they haven't gotten others to back them up publicly."

That's not surprising. No one could say anthropism is wrong, since it is derived from pure logic. How could we possible living on a planet hostile to life, and the planet sits in a universe impossible to create stars and planets? So it is without question that anthropic principles are CORRECT.

The only thing that can be questioned against anthropic principles, is not whether it is correct or not, but whether it is science or not.

There are things that are absolutely correct, but is not science. Science does not necessarily have to be correct, since a theory we think is correct now may be replaced by a more accurate better theory in the future. Science have to be able to make predictions of the nature to be useful, science have to provide a mean for someone to potentially disproof itself through experiments or observations. Only when a theory makes prediction that can be verified or contradicted, and when it came out such predictions are verified instead of contradicted, will that theory be considered science.

What's being disputed, is whether anthropic principles, although correct, does it have that predictive power at all? Some think it has, some do not. I personally think it does have predictive power, but very limited and definitely does not explain things unrelated to the condition of our existence. For example it does not explain why there's homosexuality, which is strange from the biological point of view. I would say anthropic principle is probably some sort of semi-science.

Even the widely accepted standard of what is considered science may be subject to question itself, due to the Godel theorem. For example mathmatics, 1+2=3, that's absolutely true, it does not even provide a potential opportunity for any one to possibly contradict itself, using experiment or whatever, so, then, is it science or not if it can never be dis-proven?

Quantoken

8. **Aaron**
January 12, 2005

"They think that if they speak out, it will be the end of their careers in physics."

Hardly. I just don't think that they care. As I said, plenty of people are perfectly content to continue working on whatever they're working on and let the anthropic people do what they want.

9. **Peter**
January 12, 2005

The only other senior person besides Gross I can think of who has spoken up about anthropism is Steinhardt. It really is remarkable to me that they haven't gotten others to back them up publicly.

10. **JC**
January 12, 2005

I can't think of many anti-anthropic folks who have spoken loudly against the anthropic deterioration on the subject, other than perhaps David Gross. Other former colleagues who are "quietly" anti-anthropic, generally won't speak out loudly about it because they either don't have tenure and/or they're a postdoc with no "political" power. They think that if they speak out, it will be the end of their careers in physics.

11. **the guts of a blackhole**
January 12, 2005

String theory lacks its underlying geometric entanglement that would widen and flush its application through what Einstein terms as our "persistent reality." The problem is without this extensive symmetry the theory gets stuck in a theoretical cul-de-sac; what I'm suggesting would suggest, too, that string theory be more accuratly catagorized as _Spring Theory_.

12. **Peter**
January 12, 2005

Hi Quantoken,

This web-site is not supported by public funds. Columbia University is a private, non-profit university. The computer equipment involved and the salary of the person who supports it (me) are paid for by this private institution.

This private, non-profit institution supports educational and research activities. As part of this they pay my salary to maintain the computer system, teach classes assigned to me and engage in other research and educational activities of my choice. What I produce in the course of such research and educational activities outside the classroom belongs to me. This is the standard way universities treat this sort of intellectual property. One of my colleagues has gotten extremely wealthy through this arrangement. I haven't.

If I thought there was any doubt about the above I would move this web-site to a commercial service. This could easily be done, but I feel it naturally belongs on the university-supported web-site since it is part of the educational and research component of my job. I also believe that the university's standard protections of the intellectual products of their academic staff ensure that there is no question about my control and ownership of this material.

The comment section is intended to allow others to participate in the educational and research activity of which the web-site is part. To the extent that people further the educational and research goals of this site, I will do everything I can to encourage them. To the extent that they damage these goals I have to do what I can to stop them. Again, this belongs to me, and I'm solely responsible for it. It does not belong to you in any way, shape or form.

For the answer to the question you ask about Feynman's argument that a massless spin-two particle necessarily implies general relativity, see his book "Feynman Lectures on Gravitation"

13. **Quantoken**
January 12, 2005

Peter:

Hope you feel better now. I am sorry to see that you had to start deleting comments. But I respect that, plus it's not my comments that you deleted.

Not wanting to be picking, but your statement is only 25% correct: "It is something that I created, that belongs to me." It does not belong to you. The Columbia web site, plus all your research activities, are supported by public fund. The things you create while being supported by public fund does not belong to your private property.

I am in a good mode today since I just got a few patents filed today. I am the sole creator of the patents, but it does not belong to me, since I was paid salary to create the stuff. The same principle applies to you, Peter.

Also it is not completely true you created this place. People come and discuss various things in your comment section. Without that your blog would be dead water that no one wants to look at.

That said, it is still best that people would focus a little bit more on the topics. It's hard though because in any heated debate it is commonplace that you started in one thing, and it could leads to something else totally unexpected. I think it is perfectly healthy that different opinions get exchanged this way.

Now back to the Giddings paper. One thing I have been long puzzling is string theoreticians claimed they "predicted" gravity, and I never know exactly where that came from. Giddings's paper seem to provide some hint. Based on what I read, the reasoning goes like this:
a.String theory leads to a spin 2 mass less particle.
b.Feynman said long ago "ANY theory that describes a spin 2 massless particle must describe gravity".
c.So super string theory predicts gravity.

Now, does any one know any thing where b. came from? Why Feynman said any spin 2 mass less particle must be associated with gravity?

Quantoken

14. **Peter**
January 12, 2005

I am extremely pissed off right now. I just deleted the last two comments, which had nothing to do with the topic of this posting, and whose authors seem to feel that this is a good place for them to go on about whatever they feel like. It isn't. It is something that I created, that belongs to me, and that you are damaging. Don't do this again.

15. **Peter**
January 12, 2005

Giddings's claim (and that of the 10000 other similar string theory propaganda pieces) is not that it contains gravity and fermions. The claim is that whereas the standard perturbative quantization of the gravitational field is non-renormalizable, quantized superstring theory contains the gravitational degrees of freedom but is term by term finite in perturbation theory.

There are several problems with this:

1. It actually is not known to be true for more than two loops.

2. Even if it is, finiteness of the terms in the perturbation series isn't enough for a finite theory. The series diverges, and conjectural non-perturbative definitions of the theory have their own problems with infinities.

Introducing fermions into general relativity is not the problem. The problem is quantizing the gravitational field. Unless you have a good argument that the work of Chen addresses this problem, please do not discuss it further here.

16. **Dick Thompson**
January 12, 2005

Since Giddings' clain for strings boils down to; it contains both gravity and fermions, what if we create another theory that does this? Which brings me to a modest proposal.

X. Chen has just posted a double Kalusza-Klein theory on GR which supports spinors (but does not, I think, generate them) and which accounts for quantum phenomena by zitterbewegung caused by the compacted two extra time dimesions. All we need is a way to get dirac spinors defined K-K style in an extension of this theory. Then with both Em and Dirac in a unified space time we can let them interact and tune the theory to produce QED (maybe).

Now in 1933 Infeld and van der Waerden produced a theory of spinors as a sort of bundle over GR spacetime (Sitzb. Pruss. Akad. 1933, pp 308-401.) Could modern techniques unify their spinor manifold with GR via a K-K mechanism? Is it worth looking into?

17. **Tony Smith**
January 12, 2005

Aaron, in reply to Peter's post statement about String Theory
"… Postdocs and untenured people can't take on the fight against this garbage unless they want to commit career suicide. It's up to the tenured people. Where are they? …",
you say
"… If, all of a sudden, this starts to affect hiring patterns, then maybe that's a big deal, but I haven't seen anything like that. …".

Since the current String Theory movement started out about 20 years ago, it has grown so that for years the hiring/funding for theoretical particle physics has been about 90% String Theory and 10% Loop Quantum Gravity with other approaches being miniscule.

Since String Theory already has 90% of the pie, of course you "… haven't seen anything like … all of a sudden …[effects on]… hiring patterns …" during the recent past. You would have to look at the early part of the last 20 years to see when "anything like that" took place.

As of now, a point related to Peter's post is that String Theory may be choking out alternative approaches and
that such elimination of alternatives may be tolerable if the dominant approach (String Theory) actually does produce realistic models, but
if the dominant approach (String Theory) is sterile, then the field (theoretical particle physics) stagnates.

Perhaps String Theorist should admit that they have had their chance.
Two decades of the smartest people on earth working on an approach with no realistic results indicates to me that the approach is unsound, otherwise all those smart people would have already announced realistic results.
It is time to let 1000 flowers bloom and try lots of alternative approaches with equal hiring/funding and see which approach works best.

You also say
"… You want to work on that sort of stuff, you go work on it. If you don't, you don't. No need for bickering on the arXiv. …".

For some of us with unconventional viewpoints (even string theory related unconventional viewpoints such as CERN CDS EXT-2004-031), it is impossible for us to engage in "… bickering on the arXiv …"
because we are blacklisted therefrom (an example being that, since I am personally blacklisted, all my recent work including the paper I just cited is barred from the arXiv).

18. **Peter**
January 12, 2005

I'm in a bad mood and I've finally had it. Any more comments at all like the last two will be immediately deleted. Please take off-topic nonsense like this elsewhere. I'm not going to put up with it here anymore.

19. **plato**
January 12, 2005

Okay from the physics then.:)

At very high energies, the density of the gluons in this wall is seen to increase greatly. **Unlike the quark-gluon plasma produced in the collision of such walls, the color glass condensate describes the walls themselves, and is an intrinsic property of the particles that can only be observed under high-energy conditions** such as those at RHIC.

I was also given the pdf file for consideration and wanted to know if you fellows thought any revision to KK theory would have allowed a expanded view of this area I am pointing out?

Summing over all topologies?

I'll have to add pdf in later

20. **Doug**
January 12, 2005

drl writes:
The aether investigation was honest, and physically motivated. Propagation in itself required a medium before it was known that space and time were part of one geometry, and that propagation was a primitive fact that needed no more explanation – so it's not fair to compare the idiocies of string theory with the honest but ultimately wrong-headed search for the aether.

Wasn't the original motivation for string theory (even if indirectly), the problem of infinities, of f = qq'/d^2 when the distance gets so small, in other words, the inherent problem of point particles in a background of space and time?

If we could rewind the clock and go back before Schwartz and Green, and before Nambu, Nielsen and Susskind, and, knowing what's coming, prevent it, where would particle physics be now? Of course, I realize that no one can really say, but it points out something important: given that string theory is such a calamity, what's the more reasonable alternative?

How do we know that the "honest but ultimately wrong-headed search for the aether," a search for something that didn't exist, isn't being repeated again with an "honest but ultimately wrong-headed search" for the elementary particle? Remember the importance of epistemology: it's not what we don't know, but what we "know" that's not so, that can really hurt us.

The apparent "idiocy" of "stringpots" should serve to warn us of this, and instead of continuously ranting about it, maybe we ought to look back to what started it all and ask ourselves why we don't know what an electrical charge is much less a quark? If the standard model is so successful in some ways, but disconcertingly deficient in others, why is this? Could there be something that we think we "know," an erroneous assumption that we have accepted as a fact that is doing us in?

21. **Aaron**
January 12, 2005

I was referring to anthropic-types, not string theory in general.

22. **D R Lunsford**
January 12, 2005

Peter –

I suppose we can look forward to some coset argument that groups the countries into competing blocs and pacts.

"Witten, tear down this membrane!"

Trace, but verify.

-drl

23. **Thomas Larsson**
January 12, 2005

Lubos, I think you should admit one thing, at least to yourself. People like myself, Woit, Lunsford, Baez, Schroer or Rovelli have never written a positive word about anthropism. The only person on your hate-list that has been close to anthropism is Smolin with his fitness landscape (which I find disturbing), but he is also the only tenured person to publicly criticize Susskind.

We all know who the landscapers are: stringpots like Susskind, Douglas, Polchinski, Dimopoulos, Kachru, and Arkami-Hamed. And Witten and Weinberg are the ones that cannot find good arguments against the AP. Draw whatever conclusion you wish from this.

24. **Lubos Motl**
January 12, 2005

Peter, as you know, I don't plan any career and it has no implications for my statements.

"Country" is my terminology – they only use "friendly neighborhood". 😉

I don't think that the claims in their paper are really justified, on the other hand it is a nicely written paper with a lot of interesting technology.

25. **D R Lunsford**
January 12, 2005

"" _

The aether investigation was honest, and physically motivated. Propagation in itself required a medium before it was known that space and time were part of one geometry, and that propagation was a primitive fact that needed no more explanation – so it's not fair to compare the idiocies of string theory with the honest but ultimately wrong-headed search for the aether.

Nor is there any resemblance of the current phase to spectroscopy before quantum theory.

The string cult is like nothing since the pre-Baconian schoolmen.

-drl

26.
January 12, 2005

One does not have to say nice things about garbage from people on whom one's career rests; one can simply keep silent.

This branch of physics is perhaps going through its "models of the luminiferous aether" phase. Perhaps as a proportion of published papers in the 19th century there are as many on the aether as there are now on strings.

The change is going to come from some young researcher who has found a cocoon of safety in some small obscure university somewhere, who has a good idea; and who does not hesitate to comment on the nakedness of the emperor.

27. **D R Lunsford**
January 12, 2005

Oak – HA!

Actually, the notion of countries made me think of a marathon game of "RISK!". Watch it, Bon-Motl is attacking Kamchatka from Alaska!

-drl

28. **Chris Oakley**
January 12, 2005

    This whole phenomenon makes me think of the way that stars swell up to red giants before they use up their fuel. A branch of "physics" is dying spectacularly. Eventually the hundreds of string theorists will become just a handful and they will mostly be found in mathematics departments.

29. **D R Lunsford**
January 12, 2005

    Aaron –

    Explain why [this site] has won awards. I get excited questions from friends and acquaintences about string theory all the time, and have to explain over and over again that it is total horseshit and it practitioners charlatans.

    Just because you are up to the neck in sand from the other direction doesn't mean things aren't really bad. I honestly think the Western world is in deep shit.

    -drl

30. **Aaron**
January 12, 2005

    Fight against them? Who needs to fight against them? You want to work on that sort of stuff, you go work on it. If you don't, you don't. No need for bickering on the arXiv.

    If, all of a sudden, this starts to affect hiring patterns, then maybe that's a big deal, but I haven't seen anything like that.

31. **Tony Smith**
January 12, 2005

    Peter, about hep-ph/0501080 by Steven Giddings, you say:
    "… string theory … has given up any claims to being a
    legitimate science and has taken on the characteristics of a cult.
    … I just can't believe the way
    essentially the entire particle theory establishment,
    including many people I have the highest respect for,
    continue to allow this situation to go on without public comment. …".

    There have been notable physicists such as

    Richard Feynman,
    who said in Davies and Brown, Superstrings, Cambridge 1988, pp. 194-195):
    "… I do feel strongly that this is nonsense! …
    I think all this superstring stuff is crazy and is in the wrong direction.
    … I don't like it that they're not calculating anything. …
    why are the masses of the various particles such as quarks what they are?
    All these numbers … have no explanations in these string theories
    – absolutely none! … "

    and Sheldon Glashow,
    who said at http://www.pbs.org/wgbh/nova/elegant/view-glashow.html
    "… superstring theory … is, so far as I can see,
    totally divorced from experiment or observation.
    … string theorists … will say,
    "We predicted the existence of gravity."
    Well, I knew a lot about gravity before there were any string theorists,
    so I don't take that as a prediction. …
    … there ain't no experiment that could be done
    nor is there any observation that could be made
    that would say, "You guys are wrong."
    The theory is safe, permanently safe.
    I ask you, is that a theory of physics or a philosophy? …".

    They have declared that the String Emperor has No Clothes,
    but
    they seem to be dismissed by the current physics establishment
    as being dead or senile reactionaries, to be pitied for their
    inability to perceive the beauty of String Theory.

    I am reminded of Kipling's Gods of the Copybook Headings
    which says in part
    "… we worshiped the Gods of the Market Who promised …
    that Wishes were Horses …[and]… that a Pig had Wings. …
    But,
    though we had plenty of money,
    there was nothing our money could buy …
    … Then the Gods of the Market tumbled,
    and their smooth-tounged wizards withdrew,
    And the Gods of the Copybook Headings
    said: 'If you don't work you die.'
    … And the hearts of the meanest were humbled
    and began to belive it was true

That All is not Gold that Glitters, and Two and Two make Four …”.

So it seems to me that the smooth-tongued String Theory Wizards
have historical precedent and will eventually withdraw,
but I might not live to see that day.

Lest anyone think that I am opposed to any form of string theory,
I will refer to my paper at CERN CDS EXT-2004-031
which is also available from my web pages at
http://www.valdostamuseum.org/hamsmith/E6StringBraneStdModelAR.pdf
It is an outline of a construction based on the E6 Lie algebra
and an interpretation of strings as world-lines (as opposed to
little stringy subparticles), which construction gives
concrete structures consistent with the Standard Model.
Maybe my construction is valid and realistic,
and maybe it has faults that may or may not be remediable,
but
it is interesting to me that the String Theory establishment
seems to have very little interest in serious exploration of
such a model that, although somewhat complicated,
actually might be a way to connect String Theory with
the Standard Model.

32. **Matti Pitkanen**
January 11, 2005

The situation in theoretical physics is indeed sad to watch. String theorists have become a kind of religious sect refusing to receive any communications from the world outside.

When string models came around 1984, two years after my PhD and the realization that PhD would not help to get support for my work, one thing looked obvious to me. 2-D space-time would soon become a convenient toy model being replaced with a realistic theory based on 4-D surfaces representing both particles and space-time in which they appear as topological in-homogenities. If a time traveller from future would have tried to convince me that after two decades people would be filling hep-th with “predictions” of a non-renormalizable theory, I would have laughed him back to the future.

In these discussions people avoid saying aloud what the real problem is: average theoretical physicist (the finger points also to me!) is full of vulnerable ego and so concentrated in optimizing the career of this vulnerable ego that he will never take a risk of wasting time by listening a person without name and position. My consolation is that I still has some years to develop TGD while looking how colleagues are getting sick of string theory. I try my best to avoid feeling some malicious pleasure and thinking that colleagues are now getting just what they deserve. Fortunately, when farce continues forever it ceases to be fun for both actors and audience and comes to an end.

Matti Pitkanen

33. **D R Lunsford**
January 11, 2005

As usual, there are no equations, no derivations, no apparatus, lots of buzzwords, etc. IOW this paper is the gradient of a crackpotential.

Moreover this person writes on a 4th-grade level. Don’t future bullshitters understand the importance of rhetoric? Where are the great teachers of our day?

-drl

New Policy

January 13, 2005
Categories: Uncategorized

As of yesterday, I've started deleting comments from this weblog if they seem to me to be completely off-topic or make no sense. By the end of last year, the comment section here had begun to turn into something I couldn't stand to read and didn't want to be associated with. This was not a tolerable situation.

Please only post comments if you have something interesting to say related to the topic of the posting. If you want to share your thoughts with the world about the biological basis of homosexuality, quantum computing, etc., etc., please do it somewhere else than in the comment section of a posting about the issue of the predictivity of current formulations of string theory.

This has already wasted far more of my time than I would like. I don't want to moderate or in any way be involved in a public discussion of this, so the comment section for this particular posting is closed. If you absolutely feel you must discuss this issue with me, send me e-mail, although unless there is a very good reason, I'm unlikely to take the time to write back in response. People with on-topic comments are encouraged to continue to contribute them to earlier postings and to any future ones.

No Comments

Quantum Diaries

January 13, 2005
Categories: Uncategorized

The world of particle physics web logging expanded by about an order of magnitude today, as a new web-site called Quantum Diaries came on-line. The idea seems to be to celebrate the 100th anniversary of Einstein's remarkable 1905 papers by getting 25 physicists from around the world to set up web logs so people can follow what they do during 2005.

Most of the participants are experimentalists, with just three theorists as far as I can tell. The theorists are John Ellis of CERN (see here for a story about him), Stephon Alexander of SLAC, and Jochen Weller of Fermilab.

Interestingly, all three of the theorists are spending at least part of their time working on cosmological or astrophysical topics, which gives you some idea of where the field is headed. Also, none of them are working on string theory at the moment, which also gives you some idea of where the field is headed.

As a completely unrelated aside, today I came upon the web-site of Brian Powell, a graduate student of Will Kinney's at Buffalo studying cosmology. He's more pro than anti string theory, but irreverently funny. His comment that "many people criticize string theory because it's sort of becoming fashionable to do so" warmed my heart. On the other hand, the fact that he links to something I wrote about Witten's talk at Santa Barbara with the terms "Witten gets socked in the groin" kind of upsets me.

Comments

1. **Peter**
   January 14, 2005

   Hi David,

   Good luck with your studies. Can't help with your prelims, but can help on one of the prevalent themes you mention for your blog.

2. **David Guarrera**
   January 14, 2005

   Apprapos, I'd like to take this opportunity to plug my own blog at http://web.mit.edu/guarrera/www/blog.htm . It's the musings of a theoretical physics first year grad student (namely, me)…

3. **Fabio**
   January 14, 2005

   Ellis is one of the world's most published string theorists, and also one of the world's most cited (if you include self-citations).

4. **Lubo? Motl**
   January 14, 2005

   Dear Peter,

   it's very sad if I am taking your naive, innocent, childish ideals 🙂 from you, but most of the people are working in string theory in one way or another.

   I am not sure whether you will believe me that determining the ten-dimensional string coupling by John Ellis et al. in December (20th) 2004 is a recent work on string theory

   http://arxiv.org/abs/hep-th/0412240

   Also, Stephon Alexander has the biggest hobby right now – he was explaining it at Harvard – about determining the string scale in the heterotic models

   http://arxiv.org/abs/hep-th/0409014

   Jochen Weller is a hard astroparticle guy, sure.

   Best
   Lubos

5. **JC**
   January 13, 2005

   I always found "first person" accounts and/or diaries quite interesting in seeing what people were thinking at a particular point in time, without the benefit of hindsight. The real world of physics is a lot more messy and uncertain as one is doing research, unlike how it is presented in future textbooks and/or monographs. Everything in hindsight looks "obvious" and/or "simple" for many things.

6.
   January 13, 2005

   Thanks for the informative post. Notice that these sort of posts, which do not merely concentrate on (possibly justified) opposition to something, and are emotionally neutral, do not attract the flood of crackpots. If you really don't want them, you know what to do.

7. **Fabio**
   January 13, 2005

   Wow, this Quantum Diaries thing is great. I can't think of any better way to celebrate Einstein's legacy than to read a bunch of random physicists detail who they had dinner with the other night or the latest movie they saw.

8. **Sean**

January 13, 2005

Please don't stoop to smileys; I'll just try to fine-tune my tongue-in-cheek sensitivity.

Sure, a lot of particle physicists are increasingly working on astrophysics. Too many, actually, especially on the experimental side. Ten or fifteen years ago that would have been a great career move; but right now particle experiment is just beginning to get interesting again.

9. **Peter**
January 13, 2005

Hi Sean,

The part about string theory was meant to be kind of tongue in cheek (I really got to figure out how to put in those smileys the way Lubos does…). But surely you agree with the first part (about more and more particle physicists working on cosmological and astrophysical topics), that was meant seriously.

10. **Sean**
January 13, 2005

Oh come on, Peter. Do you really think that the choice of blogging physicists tells you something about where the field is headed, rather than the predispositions of the certain individuals that run the site? Shall we conclude that condensed matter physics is basically dead, since it doesn't appear here? All we can conclude is that the site was set up by HEP experimentalists.

Model Building

January 14, 2005
Categories: Uncategorized

For some interesting comments by Nima Arkani-Hamed about his model-building activities, followed by some of my own, take a look here.

Update: Jacques Distler has some comments on the Arkani-Hamed et. al. paper.

Comments

1. **Peter**
   January 18, 2005

   Thanks for spelling correction, just fixed it.

2. January 18, 2005

   BTW, a spelling correction: Nima **Arkani**-Hamed

3. **Quantoken**
   January 15, 2005

   Superstar said:
   "For those of us outside the network, who nonetheless are the ultimate source of it's funding, what value does it provide? It's looking more an more like it's ultimate value is as entertainment. Or perhaps also as a way to impress the neighbors. Kind of like one great big living coffee table book for the nation."

   Entertainment is good. It is a much bigger sector of human expendition than what we collectively spend on the whole front of science research. Making one typical movie costs more than sending one PathFinder to Mars. My own daytime work relates to the routine creation of real multiverses in virtual spaces of many dimentions, for the joyful entertainment of the playful population.

   Expenditure on Superstring research is nothing compare with the rest of pork barrel. If it expands the imagination of a few SciFi novel writers or allows making of a few SciFi movies, that's more than the worth of the money already. The sad thing is so many of the smartest people jump into this seemingly hopeless quest, knowing full well they probably won't have better luck in the next 25 years than the previous one, and that it may ultimately be proven to a wrong road which leads to no where. If that's entertainment, isn't it a bit too much cruelty?

   I agree with the notion that "let 1000 flowers bloom". There should be an affirmative action in science research that when the main stream seem to be un-productive, alternative approaches and different tries should be encouraged, instead of being suppressed and censored.

   Remember, some of the greatest discoveries in science were made by a none-main-stream nobody, mere 26 years old, who could not even find a job teaching in a high school, and needed help to finally land a job as a third class clerk working in a small patent office. And whose ideas still look so wackered even today, that calling him "crackpot" would be a compliment to him.

   And this year is to be honored in his name.

   I still don't know how that guy managed to get his stuff published at the time. Wasn't there supposed to be a peer review process to maintain sanity in science publications:-)

   Quantoken

4. **Superstar**
   January 15, 2005

   The sociological network effect is certainly a big factor in the current dominance of string theory.

   For those of us outside the network, who nonetheless are the ultimate source of it's funding, what value does it provide? It's looking more an more like it's ultimate value is as entertainment. Or perhaps also as a way to impress the neighbors. Kind of like one great big living coffee table book for the nation.

   Is it any wonder then that so many physicists are working on their own coffee table books these days, and the documentaries to go along with them?

5. January 15, 2005

   "So just as network benefits accrue to users of inferior software [windows], inferior videotape systems [vhs], community benefits accrue to the dominant, if unsuccessful, scientific fad [conference invitations, x-archive postings, fellowships, book contracts, interviews, pbs specials]."

   This seems a bit cynical. The analogy between windows and vhs users with string theorists is improper. The software and videotape users (together with sufficient numbers of similarly acting people) provide the funding that in turn enables the community benefits. Clearly this is not the case with string theory research or other research that is funded publicly or by others who are not members of the string community.

   If community benefits are the primary coin of the realm, then a better analogy would be a community of parasites [the opportunists in the string community] feeding off the hosts [the funding agencies]. Not all parasites end up killing the host; let's hope this one doesn't [e.g., drying up of physics research funding due to inadequate benefits returned to the rest of the world].

6. January 15, 2005

   Comment on Peter's comment on Nima's comment:

   Think of the string theory community as a social group with a "network effect" function of utility to it's members. See for example:

http://en.wikipedia.org/wiki/Network_effect

In other words, regardless of utility to outsiders [rest of world] the community itself generates it's own utility as a power or exponential of it's own size.

So just as network benefits accrue to users of inferior software [windows], inferior videotape systems [vhs], community benefits accrue to the dominant, if unsuccessful, scientific fad [conference invitations, x-archive postings, fellowships, book contracts, interviews, pbs specials].

This is to be expected. The good news is that because of the power law of network utility a competing technology or theory with clear benefits can grow in size rapidly and overtake the inferior competitor. However the benefits have to be substantial to the defecting members, and outweigh the lost utility from detaching from the previous network.

In the case of the competitors to string theory it seems that currently any apparent benefits do not offer enough utility for large numbers to defect.

7.
January 15, 2005

Or

Quantum Field Theory:Standard Model & related
::
String Theory:specific predictive models built from a string vacuum.

8. **Tony Smith**
January 14, 2005

Commenting on the historical remarks of Nima Arkani-Hamed
"… a big realization leading to the SM was that gauge theories should be important to the description of nature. … But it did not uniquely predict 3-2-1 gauge theory! … during a period in 77 or so when the experimental situation got very confusing. Critics of gauge theory model-building … said "Its unconstrained! You can build so many models!". … When the dust settled, we had 3-2-1. …"
Peter Woit said:
"… in 1977 … SU(3)xSU(2)xU(1) with the SM particle content was … pretty well set in stone …".

Here is some chronology supporting Peter Woit's comment:

the electroweak force SU(2)xU(1) part of the SM was pretty much "set in stone"
in 1971 when 't Hooft proved its renormalizability;

the 1973 paper of Kobayashi and Maskawa, confirmed experimentally by the 1974 discovery of the charm quark and the 1976 discovery of the beauty quark, pretty much "set in stone" the color SU(3) 3-generation part of the SM; and

in 1974 the 3-2-1 SM was so well accepted that Glashow and Georgi constructed SU(5) GUT based on the fact that SU(5) contains SU(3)xSU(2)xU(1) as a subgroup.

Although some people in 1977 may have disagreed with the the 3-2-1 3-generation SM, their relevance to physics has been no more significant that the relevance of those who disagree with Einstein's relativity.

As to the situation today, it seems to me that string theory today has a financial / sociological dominance that is at least as strong as that of the 3-2-1 SM in 1977, but that string theory today has nowhere near the basis of detailed calculations and experimental results that the 3-2-1 SM had in 1977.

As Peter Woit said:
"… The real problem with particle theory these days is that the top-down approach has been nearly completely destroyed by the concentration on one idea of how to do this (string theory) and the refusal to acknowledge the colossal failure this has led to. …".

Another aspect of the problem is raised by Nima Arkani-Hamed's statement "… there are specific hardware triggers at the LHC that are about to be finalized … we're on touch with experimentalists and they will be modified …".
As Peter Woit said "… It's certainly a good thing that model-builders are pursuing as wide a range models as possible in preparation for the LHC, especially if they're talking to the people who are working on the LHC triggers. …".
However,
if the only model-builders who have influence with LHC trigger designers are conventional string theorists, then the LHC may fail to observe events that could be very useful in evaluating models not based on conventional string theory.

I repeat what I said in a comment to an earlier post:
It is time to let 1000 flowers bloom and try lots of alternative approaches with equal hiring/funding and see which approach works best.

For my part, since I have no influence on LHC trigger design, about all I can do is to be happy to defend my model in comparison with conventional string theory in any reasonable forum (for instance, I have submitted contributed talks about my model to the APS Tampa meeting 16-19 April 2005).

9. **Superstar**
January 14, 2005

In other words, Chris, you're saying string theory is meta-physics.

10. **Chris W.**
January 14, 2005

Arkani-Hamed's characterization of the work he and his colleagues are doing supports a picture* of string theory and landscape ideas as meta-theoretical frameworks — as theory generators. Given the constraints on the models he describes, this may well prove fruitful. However, any successes achieved by this path will be dubious empirical confirmations of string theory, since failures of these models couldn't be taken as empirical refutations of string theory. That is why I'm inclined to regard the specific models as the real physical theories, and string theory itself as a web of related ideas (largely formal schemes) from which testable physical theories might be derived.

Again, drawing an analogy with the case of general relativity, GR is the physical theory, whereas Riemannian geometry is only a meta-theoretical framework, notwithstanding the general notion that the structure of physical space (and later, spacetime) might be Riemannian and dynamic — an idea which was anticipated by Riemann and Clifford.

(* previously discussed in the comments on this post)

Not on the ArXiv

January 19, 2005
Categories: Uncategorized

Most new preprints in mathematics and physics these days are posted on the arXiv, but every so often I run into interesting new things worth reading that haven't appeared there for one reason or another. Here are some recent examples:

Some lecture notes on Lie algebras by Shlomo Sternberg. Lots of topics covered I haven't seen anywhere else, especially the material on the relation to Clifford algebras and the Kostant version of the Dirac operator.

Lecture notes by Constantin Teleman about his recent work on topological field theories and the Gromov-Witten theory of BG, the classifying space of a group. These are notes from talks given at Gregynog, Goettingen, and Miami. I confess that, like a lot of Teleman's work, I have trouble figuring out exactly what he is up to, but it looks quite interesting. I wish he and Dan Freed and Mike Hopkins would get around to finishing their paper on "K-theory, Loop Groups, and Dirac Families" that Teleman has been advertising as "coming soon" for quite a while…

David Vogan has an interesting draft of a review of A. A. Kirillov's book on the orbit method in representation theory. This is the most fully developed version of what is sometimes known as "geometric quantization". Vogan also has some notes from his lectures this past year on "Unitary representations and complex analysis" which include material on the Borel-Weil theorem and its generalizations.

Nikita Nekrasov has some Lectures on Nonperturbative Aspects of Supersymmetric Gauge Theories and a written version of his 2004 Hermann Weyl Prize lecture.

Eckhard Meinrenken has a a nice expository article on the de Rham model for equivariant cohomology.

Comments

1. **Lubo? Motl**
   January 20, 2005

   If someone learns the theta functions, their expansions, the forces between D8-branes etc., then he or she is a person familiar with string theory, and if he or she writes a paper about it, then he or she is working on string theory.

   He or she may have a different approach to all these issues than others and may be more or less successful, but it is still string theory.

   I am not like the kind of people who would like to eliminate (and often they DO eliminate) every piece of data that is inconvenient to them. And moreover I think that John Ellis is an interesting person with inspiring ideas, and I have absolutely no reason to try to verbally eliminate him from some group.

2. **D R Lunsford**
   January 20, 2005

   Wow, those Sternberg notes are excellent, thanks for that. Sternberg reads like a novel!

   -drl

3.
   January 20, 2005

   Here is what Peter was referring to Lubos about the Penquins

   I thinks John's post today directs attention to a certain amount of responsibilty?

4. **Fabio**
   January 20, 2005

   For some good fun look up Ellis and Nanopoulos's work on noncritical louiville string theory applied to the physics of the brain (the kind in your head).

   Of course, I'd be surprised if they haven't already found a way to connect Brane physics to brain physics also, the potential for cute irreverent paper titles being too much to resist.

   Why Lubos wants to brag about this guy being a string theorist escapes me.

5. **Chris W.**
   January 20, 2005

   Yes indeed, they came up* with quite a Rube Goldberg contraption just to produce some effects attributable to spacetime foam. To say that they produced a *model* of spacetime foam is a stretch, to say the least. John Wheeler would be appalled.

   In my opinion this kind of thing shows string/D-brane theorizing in the worst possible light.

   (* Footnote 1 of this paper [gr-qc/0501060] cites the paper in which the model was initially presented.)

6. **Peter**
   January 19, 2005

   Well, the comment that Alexander and Ellis were not working on string theory was a bit of a joke, I wasn't under the impression that they weren't going to work on string theory any more.

   I took a look at the Ellis paper, it is pretty amazing. The guy is clearly smoking a lot more powerful weed than back when he was seeing penguins in his Feynman diagrams.

7. **Lubo? Motl**

January 19, 2005

John Ellis, whom you say is not currently working on string theory, has a new paper tonight about D8-branes and D0-branes:

http://www.arxiv.org/abs/gr-qc/0501060

8. **Levi**
January 19, 2005

Nice links. There are still quite a few mathematicians who don't routinely post their work on the ArXiv.

String Fellows

January 20, 2005
Categories: Uncategorized

Today's Guardian has an article by a writer who recently visited the Institute in Princeton to talk to Witten and others there about string theory. The author of the piece makes the obvious analogy between Witten and Einstein, and asks the string theorists about Einstein's 20-year misguided and failed attempt to unify gravity and electromagnetism during his years at the IAS. String theory and Einstein's failed program get further identified by the author's claim that if Einstein were alive today he would be working on string theory, and by a quote from Seiberg that "Being in the place where Einstein was is clearly an inspiring idea."

Seiberg also has something very true to say:

"Most string theorists are very arrogant," says Seiberg with a smile. "If there is something , we will call it string theory."

Witten's attitude towards string theory seems to remain unchanged, he's quoted as saying:

"Critics of string theory say that it might be too big a step. Most physicists in other fields are simply agnostic and properly so," says Witten. "It isn't an established theory. My personal opinion is that there are circumstantial reasons to suspect that it's on the right track. "

His recent work on twistor string theory is mentioned, including the fact that there was a workshop at Oxford last week on the subject. About this, the writer reports

"Witten is not convinced yet. 'I think twistor string theory is something that only partly works,' he says."

I wonder exactly what he meant by that. What's the part of twistor string theory that he thinks doesn't work?

Comments

1. **Matti Pitkanen**
   January 22, 2005

   Instead of twistors, I believe in hyper-octonionic spinors! "Hyper" means multiplying imaginary units with commuting sqrt(-1) so that sub-space of complexified octonions with Minkowski signature of number theoretic norm results.

   The generalization of the notion of calibration

   [see the homepage of Jose Figueroa-O'Farrill at
   http://www.maths.ed.ac.uk/~jmf/Research/susy_7.html%5D

   leads to the notion of Kaehler calibration. When combined with the number theoretic spontaneous compactification of hyper-quaternionic M^8 to M^4xCP_2, this leads to the conjecture that the basic theorems about connection between calibrations, minimal surfaces, and spinors generalize.

   Absolute minima of Kaehler action would correspond to the solutions of massless equation for octonionic 2-spinor satisfying Weyl condition representables as hyper-octonion real analytic maps and satisfying d'Alember equation as a consequence of generalized Cauchy-Riemann conditions.

   Incredible it sounds but classical TGD would reduce to free Dirac equation for hyper- octonionic spinors equivalent with hyper octonions!

   For an enthusiastic blurb see

   http://www.physics.helsinki.fi/~matpitka/newtgd.html#calib.

   A detailed representation can be found at

   http://www.physics.helsinki.fi/~matpitka/tgd.html#visionb

   With Best Regards,

   Matti Pitkanen

2. **Jian**
   January 21, 2005

   "…Most physicists in other fields are simply agnostic and properly so,"

   "An agnostic thinks it impossible to know the truth in matters such as God and the future life with which Christianity and other religions are concerned. Or, if not impossible, at least impossible at the present time."

   Bertrand Russell, "What is an Agnostic?"

3. **D R Lunsford**
   January 21, 2005

   If Einstein were alive today, he'd probably be a patent clerk.

   -drl

4. **R**
   January 21, 2005

   "If Einstein were alive today, he would probably be a string theorist…"

   This made me laugh for a while….

good job 🙂

5. **Thomas Larsson**
January 21, 2005

as for the fate of N=1 SUSY in the LHC we just have to wait and see, don't we?

I have no doubt that people will look for SUSY at the LHC, and I am awaiting the results as eagerly as anybody. If it is found, then I will have learnt something. No big deal.

However, experimental particle physics did not start with the LHC. My post referred to several large and expensive experiments, and I think that it is wrong to simply ignore them. I have recently, and more than once, seen the situation summarized in the phrase "SUSY requires fine-tuning at the percent level". This is of course not conclusive, but it gives a hint what we may expect at the LHC.

Besides, LHC will not affect the outcome of a 2-loop calculation of the running coupling constants. If it has been done, I would like to see a reference. If not, it is about time that somebody does it.

Interest in SYM is not mainly due to hopes that it is a realistic theory, but that it is a more tractable version of SYM than those without SUSY.

Fine. But if some conclusion relies critically on SUSY (AdS/CFT perhaps), and SUSY is not part of nature, we haven't really learnt anything about physics, have we?

6. **raj**
January 21, 2005

"Most string theorists are very arrogant," says Seiberg with a smile. "If there is something [beyond string theory], we will call it string theory."

One wonders. Does arrogance among string "theorists" substitute for evidence among scientists? Quite frankly, I'd prefer evidence.

7. **Urs**
January 21, 2005

Interest in SYM is not mainly due to hopes that it is a realistic theory, but that it is a more tractable version of SYM than those without SUSY.

The goal is to understand field theory as such. YM is hard. So let's move in field theory space to a more symmetric version and try to understand that first. That's still hard enough. When done, we can try to move back to the asymmetrical point.

SYM in four dimensions is about the most symmetrical gauge theory that you can get. Therefore its importance. Therefore the interest in it.

See the introduction to the Clay millenium mass gap problem for a detailed discussion why field theorists want to understand SYM.

8.
January 21, 2005

The idea is to calculate QCD amplitudes by expressing them as combination of SUSY amplitudes (which can be now simplified using twistor methods), and one non-SUSY amplitude, that one can choose to be as simple as possible. In fact, one does not use SUSY directly in the calculations, just what is called "cut-constructibility" which works for some non-SUSY calculations as well. As nobody really knows the full ramifications of the idea before exploring them (and it does look good right now), it is not useful to make apriori judgements.

The twistor string itself was proposed only for N=4 anyhow; as for the fate of N=1 SUSY in the LHC we just have to wait and see, don't we?

M.

9. **Thomas Larsson**
January 21, 2005

I forgot: no deviation in muon g-2.

10. **Thomas Larsson**
January 21, 2005

Whether SYM amplitudes can be calculated with twistor methods may or may not be true. The big question is why physicists should care about SYM amplitudes in the first place, given the mounting evidence against supersymmetry (no sparticles, no proton decay, no light Higgs, no WIMPs, no permanent electric dipole moment, and perhaps the 2-loop result that coupling constants don't meet exactly at one point).

11. **serenus zeitblom**
January 20, 2005

"It sparked a whole slew of papers from his fellow theorists and interest is still growing."

103 cites after more than a year? By EW's standards, that is far from a "slew". Nor do I see any evidence that interest is growing in this rather dull, technical subspace of the string world.

12. **Lubos Motl**
January 20, 2005

Hi Moshe, I know that you can derive it (the connected expression) from the B-model, but you can also derive it from e.g. Berkovits' models. It's just too simple so that the details of the string theory are not too important. How's life? LM

13.
January 20, 2005

Lubos,

1. The "connected instanton" formula of Roiban et. al. is a new expression for helicity amplitudes which was not available before, and is "derived" (not very rigorously)

from the toplogical B-model. That expression is correct, though not as efficient as others.

2. The versions of topological strings currently on the market do not work on the loop level, but maybe there is some hope still, “whatever you do” covers a lot of ground…

Moshe

14. **Lubos Motl**
January 20, 2005

The picture of string theory – namely topological string theory – has not been too useful in any of the recent developments that shed light on the new formulae to calculate the Yang-Mills scattering amplitudes.

The picture of the topological B-model on CP3|4 is problematic also because it contains the extra conformal supergravity states, and it’s expected that whatever you do, you will get a disagreement at the loop level.

15.
January 20, 2005

Representation of SYM amplitudes in twistor space makes some of their simplicity manifest, and is very useful as a calculational tool. The idea of cooking up a topological string theory to reproduce these amplitudes works only at tree level, for loop level there are unwanted contributions (from conformal gravity) that one needs to decouple somehow.

16. **D R Lunsford**
January 20, 2005

What part of TST doens’t work?

Probably the usual issues of conformal ideas when sources are present (of whatever kind).

-drl

Dijkgraaf Coxeter Lectures

January 21, 2005
Categories: Uncategorized

Robbert Dijkgraaf is about the most lucid expositor around on the topic of what now goes under the name "topological strings". This week he's been giving the Coxeter Lectures at the Fields Institute in Toronto, and the slides and audio of his introductory talk are now available on-line. I hope there will be similar materials for his other, more detailed talks.

Last week the Fields Institute hosted a workshop on topological strings and the talks are on-line, although in many cases just the audio of the talk is available, which is pretty hard to follow.

No Comments

NUMB3RS

January 23, 2005
Categories: Uncategorized

There's a new TV show called "NUMB3RS" starting tonight, whose main character is a mathematican named "Charlie", who solves crimes using mathematics. His motto is "Everything is Numbers".

A secondary character is "Larry", a Caltech physicist working on 11d supergravity. In one scene he shows up trying to get mathematical help from Charlie, whose graduate student sneers at him "Why don't you do your own mathematics, like Ed Witten or Feynman?".

Comments

1. **D R Lunsford**
   January 27, 2005

   Selrach –

   Well I think the point I'm trying to make is – very little needed to be deduced at that point! Klein had his program, his problem, and his methods, and all that was lacking was *physical insight* into the meaning of the non-Euclidean geometry (which turned out to be Minkowski space) that *he himself*, with his particular sort of intuitionist brilliance, had created, and had used in this problem.

   It's *physical insight* more than anything else that's needed to make progress in physics. Almost anyone can learn the math – even learn it "in the bones" so to speak – but physical insight is the rarest of all gifts it seems.

   -drl

2. **Selrach**
   January 26, 2005

   Hi Lunsford,

   I am not sure the Classical Top is a good example of the difference between a physicist and a mathematician . My own opinion is that out of all physicists only Galileo , Newton, Maxwell ,Einstein and Feymann could have made the deductions that you alluded to in 1896!

   I have been looking at the history of great discoveries in Physics, and it seems that the Great Man Theory does not stand up as well as it might. Essentially, many people have different models which turns out to be essentially the same, after mathematicial inconsistences are straighten out! (String Theory and Loop Gravity anyone?)

   For example, the reason why we remember Maxwell rather than Riemann as the discover of Electrodynamicism, is that Riemann mixed in Gravity! i.e Riemann realised that if there was a link between Electric and Magnetic phenomena, this would lead to symmetry constraints on the physics. Physicists need an Einstein to explain this in terms that they could understand PHYSICALLY.

   Selrach

3. **D R Lunsford**
   January 26, 2005

   Selrach,

   What does being classical have to do with it? It's about representations of the rotation group via the complex plane (stereographic projection). Klein noticed that his problem – to describe the motion of the top subject to external forces – was much simplified if he introduced a non-Euclidean geometry with the fundamental quadric (in the sense of projective/metric geometry)

   x^2 + y^2 + z^2 – t^2 = 0

   Again, Klein goes to some lengths to disabuse his readers of any "metaphysical implication" in introducing a non-Euclidean geometry. Now, this was in 1896! Klein has basically all the mathematical elements for relativity not only in hand, but at work!

   The irony of this is priceless – Klein had clarified the relation of affine, pseudoaffine, hyperbolic, and elliptic non-Euclidean geometry to each other – he'd written a classic book on the subject – he was past master of function theory and operations in the complex plane – yet he missed relativity – not only missed it, but turned it away when it knocked on his door – from the inside!!

   The point is – being a good physicist is a completely different metaphysico-realistic setup than is being a good mathematician.

   -drl

4. **Matti Pitkanen**
   January 26, 2005

   Poincare was great mathematician. Einstein was a great physicists. Someone said that Poincare was too much of a mathematician to discover special relativity which was in the air and although he discovered Poincare group. If a kind of cognitive complementarity is involved, the replacement Poincare-Einstein–>Witten-Witten would not make sense unless Witten enjoys a positive variant of multiple personality disorder;-).

   Matti Pitkanen

5. **Selrach**
   January 26, 2005

   Hi Lunsford,

   I do no understand what SR has to do with the mathematical theory of the Top! This theory is ENTIRELY Classical!

I agree that Einstein was a good mathematician. However he was not a great one, unlike Ed Witten.

6. **D R Lunsford**
   January 25, 2005

   How do these rumors get started?

   Einstein did NOT "often have a mathematician to help him with the hard stuff". In fact the great mathematicians of the day, including Klein and Poincare, fell all over themselves getting things wrong. See for example Klein's "Mathematical Theory of the Top". The locus for this work is the complex plane under SU(2) – really SL(2,C), because the quadratic form arises:

   x2 + y2 + z2 – t2 = 0

   Klein EXPLICITLY mentions that the non-Euclidean geometry he is using should not be taken literally. In fact, it should be taken VERY literally because it's Minkowski space. In other words, the greatest mathematician of the day was flat wrong about his OWN WORK!

   GEEZ, save me from Einstein bashing!!

   -drl

7. **Rafael**
   January 24, 2005

   I actually liked the show. Sure the mathematics is over the top but hey it's fiction! It seems as if they're trying to make it educational and I only say this because they have the main characters narrate their thought process like a PBS special.

   "Why don't you do your own mathematics, like Ed Witten or Feynman?"

   Perhaps this physicist guy is supposed to be more like Einstein since Einstein often had a mathematican who would help him with the hard stuff (like Riemannian geometry).

8. **plato**
   January 24, 2005

   Would it have been more credible to the mathematician that number theory be based on some principle as posted here?

   It would make sense to me, why string theory might be rejected, and a new method considered as a basis?

9. **Walt Pohl**
   January 24, 2005

   It's not quite as bad as the ads made look. It's still painful for math-savvy people to watch.

10. **R**
    January 24, 2005

    Not even close to PI, an excellent movie.
    I bet it doesnt go longer that 3.1415… episodes 🙂

11. **A.J.**
    January 24, 2005

    OK,

    Step up, folks, and place your bets! How long until the show gets cancelled?

12.
    January 24, 2005

    >But on the positive side, maybe one benefit is that >it will portray math as being 'cool' 🙂

    Perhaps…but I'm not convinced…

13. **Steven S**
    January 24, 2005

    Funny. Though the one advertisement I saw for the show made it look positively awful.

14. **Plato**
    January 24, 2005

    Made me think of the show called PI

15.
    January 24, 2005

    Hey! I know! Let's make a math show and call it numb2.71828rs? No, well, then how about numb2.718rs? NO, GEEZ! WELL IS NUMB3RS 0K WITH Y0U?!?

16. **DMS**
    January 24, 2005

    Amusing 🙂 But on the positive side, maybe one benefit is that it will portray math as being 'cool' 🙂

The Thin Line of Theory

January 26, 2005
Categories: Uncategorized

There's a quite interesting article on the controversy over string theory that appeared yesterday in the Boston University student newspaper. It gives some insight into the political battle now going on in many physics departments.

The Boston University physics department has always been in the shadow of its more prominent neighbors just across the river in Cambridge. A few years ago they attracted Glashow away from Harvard, and I've been told a big selling point for him was that he would no longer have to be part of a department dominated by string theorists. He's one of very few particle theorists who has consistently and publicly complained about what is going on in string theory. In the article, he forcefully makes the analogy between Einstein's failed unification efforts and string theory:

"It is tragic," Glashow said, "but now, we have the string theorists, thousands of them, that also dream of explaining all the features of nature. They just celebrated the 20th anniversary of superstring theory. So when one person spends 30 years, it's a waste, but when thousands waste 20 years in modern day, they celebrate with champagne. I find that curious."

Ken Lane, one of Glashow's colleagues at BU, says that "String theory is not physics" and that he doesn't know of any BU faculty who think that string theory belongs in the physics department. He does seem to think that it belongs in a math department, something I have some problems with. While certain parts of string theory are mathematically interesting and do belong in math departments, most of what string theorists do is not mathematics. For instance, the many string theorists making anthropic arguments about the "Landscape" are not doing mathematics and it's pretty insulting to mathematicians to say that they belong in math departments.

Lane believes string theory is on its way out, and that the LHC will finish it off:

"I think I can safely predict that string theory is going to wither and die when exciting results start coming out of the LHC."

Cumrun Vafa of Harvard seems to be spitting mad at the idea that BU won't hire string theorists, referring to them as "foolish" and "childish", which is not normally language academics use when talking to the press about their Nobel-prize winning colleagues at neighboring institutions. Vafa was a student of Witten's a year or two behind me at Princeton, but I haven't talked to him since my postdoc days. He's definitely a smart guy, but also definitely a fanatic.

Vafa graduated from Princeton in 1985, just as the string theory fad hit. He went to Harvard as a postdoc, where most of the senior people were pretty skeptical about string theory, although willing to hire smart young postdocs doing it. I heard he was very upset in 1986 when Glashow published his article with Ginsparg in Physics Today attacking string theory, and even threatened to leave. But over the next decade or so he managed to marginalize Glashow, get more string theorists hired, and consolidate power around them. Finally Glashow left, and by now the string theorists heavily dominate the theory group. Of the active theory faculty, Vafa, Strominger, Minwalla and Motl are full-time string theorists and Randall and Arkani-Hamed do more phenomenological work, work whose justification is often given in terms of string theory. This just leaves Georgi remaining, and at the moment he has his hands full dealing with the fact that the president of Harvard is a sexist buffoon.

For Vafa to accomplish this undoubtedly took some single-minded dedication to furthering the interests of string theory and thwarting its opponents, but now that string theory so overwhelmingly dominates the field, it's pretty disturbing to see him continuing to behave like a complete fanatic. I've been told that after Brian Greene's Nova TV show about string theory came out, Vafa was heard to say that he didn't care if it was any good; as far as he was concerned anything that promoted string theory was great. He's quoted in the article as saying

"Theoretical developments have indicated string theory is a very important part of physics," Vafa said. "It has already proven foolish. It's past the point."

I'm guessing there's a typo here, one assumes he doesn't mean that string theory is foolish, but that opposition to it is. He completely ignores the argument that string theory has not predicted anything and thus is not science, calling people who make this argument "childish". His arrogant attitude towards those who don't believe what he does is pretty breath-taking, matched only by that of his younger colleague Lubos. He finally dismisses the whole BU physics department with the logically incoherent:

"I think they are doing a disfavor to BU. I don't want to pass judgment, but not having a string theory group puts out of first rate in my opinion."

I think he does want to pass judgement and already has. If you're a theorist who might someday have to deal with him as someone evaluating your grant proposal, deciding whether to hire your student, etc., do you think you might think twice before making a "childish" or "foolish" public comment about what is going on in string theory these days?

Comments

1. January 28, 2005

   Levi,

   Did you see that the paper was dedicated to *Giordano Bruno*? And that he paraphrased a Pete Seeger/Peter Paul & Mary song?? And that there is not ONE equation in the entire paper?

   The amazing thing to me – this guy was invited to MIT and CERN for 3 years each. Who makes these decisions? I wonder if these people know how to actually do any calculations?

   -drl

2. January 28, 2005

   We must keep this from Ed, he'll be devastated. Poor fellow, he seemed so excited…

3. **Thomas Larsson**
   January 28, 2005

   Peter,

   I realize that it makes sense to spend for Witten to spend a year or two on the twistor string if he finds it promising. However, from what he writes (and that is my only source, I don't know anything about the subject first-hand) I most definitely get the impression that he doubts the project's viability. Now, I prefer to work on presumably correct projects rather than presumably incorrect ones, and I suspect that Witten thinks likewise. So that he works on the twistor string but doesn't believe in it seems to indicate that he doesn't have any attractive alternative.

4. **Levi**
   January 27, 2005

   Chris W.,

   Your link in the thread you reference is to the most bizarre paper I've seen yet on the ArXiv. It's hard to imagine that Cumrun Vafa endorses the views in the paper, but who knows? Besides the passage you cite, the author of the paper also thanks Vafa in a footnote on page 3. In fact the paper is worth reading for the footnotes alone. My personal favorite is footnote 4 on page 4, which correctly conveys the flavor of the whole paper:

   although they could send unwanted anti-prisoners, their arrival being known as gamma-ray bursts

5. **Peter**
   January 27, 2005

   I'm deleting the last couple posts claiming the Pound-Rebka experiment was a fraud because they are off-topic nonsense. If anyone wants to discuss this with quantoken, do it at
   http://quantoken.blogspot.com
   not here.

   By the way, R. V. Pound taught my advanced laboratory physics course, and was an extremely impressive scientist.

6. **Chris W.**
   January 27, 2005

   View this old post on this weblog, and use your browser to search for "Cumrun Vafa":

   > "Cumrun Vafa thinks that the fact that we do not see aliens around could be the first proof of the existence of brane worlds: all advanced aliens would have emigrated to better parallel universes (our Universe has zero measure)." [– Beatriz Gato-Rivera]

   If he seriously expressed this opinion it really says something about his judgment.

7. **DMS**
   January 27, 2005

   "Also, so much of the (suddenly overhyped) particle "phenomenology" these days, a subject which cannot really be done without actual experiments, is simply string theory done badly."

   This is a joke, right? In case you are serious, LHC will falsify most (perhaps all) these models. That is a good thing. If you want to do something unfalsifiable, there are other subjects like theology.

   The string theorists also show such appalling ignorance about particle physics: like Lubos' statement (in his blog entry) that phenomenology deals with issues like "adding new digits to the 3 x 3 neutrino mass matrix". Have they heard about oblique precision electroweak observables S,T,U (hint: nothing to do with dualities)? Do they even care? Details, details, to be worked out later; everything is massless relative to Planck scale… There is a certain comfort in doing such work.

8. January 27, 2005

   Lubos' willfull misreading of Peter's funding comment is a pretty common "technique", if you want to call it that, from usenet type flame wars. It baits your opponent to go on the defensive, "no, what I really meant was…". Not the level of discourse one would normally associate with Harvard faculty (even untenured ones).

9. **Peter**
   January 27, 2005

   Lubos,

   My comment about funding was just pointing out what more than one person has told me. They don't dare criticize string theory in public because when powerful figures like Cumrun make it clear that they think anyone who criticizes string theory is a "fool" or a "child", there's the implicit threat that they will oppose funding grant proposals by fools and children.

   The story about Cumrun's reaction to the Nova program was told to me by a string theorist who heard him say this.

   Thomas:
   I don't think it is irrational for Witten or others to keep working on twistor strings for a while even though they have run into problems. They do get very useful tree-level results, and it's reasonable to see what else they can get out of this idea. If they keep doing it for twenty years though…

   The real problem is that they don't have any other new ideas about string theory to work on.

   Quantoken:

   Please stop trying to turn the discussion here to your pet ideas about GR. I'll delete any further comments by you or anyone else about this.

10. **Thomas Larsson**
    January 27, 2005

    Ever thought about the concept of "work in progress"?

    Work in progress on a non-unitary theory like conformal supergravity? Not really. Sounds kind of LQGish.

11. January 27, 2005

Re: Fabio's comment

Just like the audience at Bush's inauguration who booed Kerry:

Cumrun Vafa = sore winner

12. **Quantoken**
January 27, 2005

It's indeed interesting Witten suddenly became interested in the Twistor Theory, though no one knows why. String theoretists are a group of sheep blindly following the leading shepherd. Predictably, many people will immediately follow Witten's footstep into this Twistor thing, though many of them never figured out why. Just look at how many references to Witten's paper have already been made!

I am not going to comment on Lubos's long article, except for that he seems amazingly CHILDISH to even hate to meantion the name Boston University. He must be feeling pretty good about himself being a Harvard assistant professor, not realizing that there is a tradition of manipulating science at Harvard. For example like this INFAMOUS "Harvard Tower Experiment" where they dodged the data to fit what they wanted:

http://hyperphysics.phy-astr.gsu.edu/hbase/relativ/gratim.html#c2

Any one with a little bit common sense in physics can see why the data can not be true and can only be dodged, although I do not doubt the theory of Einstein's GR itself. I might explain if some one wants to hear.

Quantoken

13.
January 27, 2005

Ever thought about the concept of "work in progress"?

About the actual topic of the post, many smaller dpt. decide not to pursue some areas so that they have better representation in others. Celebrating your (perhaps inevitable) deficiencies is an odd attitude to life.

Also, so much of the (suddenly overhyped) particle "phenomenology" these days, a subject which cannot really be done without actual experiments, is simply string theory done badly. Might as well have the real thing.

14. **Thomas Larsson**
January 27, 2005

I didn't see any explanation why Witten is pursuing the twistor string although he evidently thinks that it is wrong.

15. **Lubo? Motl**
January 27, 2005

I've posted an article about it on my blog.

16. **Thomas Larsson**
January 27, 2005

It's pretty remarkable that Witten is doing cutting-edge research on the twistor string, which he himself apparently thinks is wrong. Aren't there any right ideas to investigate at the cutting edge?

17. **Quantoken**
January 27, 2005

Lubos:

What "cutting edge" research you are talking about? The only edge superstring research is cutting is dominating the whole theoretical physics field and marginalizing alternative approaches. It surely doe not cut any edge in the physical world of reality.

I don't know why you would interpret Peter's original words as "threaten Cumrun of denying his proposals". Is that a deliberate distortion or your incompetence in reading logically?

Peter said:
"If you're a theorist who might someday have to deal with him as
someone evaluating your grant proposal, deciding whether to hire
your student, etc., do you think you might think twice before making
a "childish" or "foolish" public comment about what is going on in
string theory these days?"

The question is not whether super string research should be supported. I think in the spirit of scientific openmindedness, all approaches need to be allowed even some may seem reasonable or unreasonable based on personal opinion. I have no problem continue funding super string theory research, as long as there are people willing to waste their lifetime on that pursuit.

But I see a problem when one mainstream camp becomes so dorminate and marginalize people of different opinions, to the point that people like Glashaw had to find a refuge away from Harvard. It's harmful to science.

Quantoken

18. **Lubo? Motl**
January 27, 2005

Some of the things you write, Peter, are not right.

Cumrun had only judged the program from PBS after he saw it, and he was excited by it – simply because it was a great program. He described it as "something in between physics and video games". His sons apparently liked it, too.

Cumrun is also right, of course, that the question for today, 2005, is not "Whether we should study string theory at all?". This is a question from the Middle Ages that only the people who have lost contact with the current thinking – or those who never had this contact – ask. Theoretical high-energy physics in 2005 without string theory is like quantum mechanics without Hilbert spaces.

The question is, as often in the past, "What is the right next important step that will be done in string theory?"

Your proposals to threaten Cumrun with denying his proposals etc. are pretty childish – using Cumrun's diplomatic speech. I would probably use stronger words. Science is organized differently than you think and the people with enough influence are usually admired physicists.

Most of us like Shelly Glashow for his personality and we admire him for what he has done several decades ago. But that's a slightly different question than cutting-edge research.

19. **Thomas Larsson**
January 27, 2005

We may not know much about whatever theory will ultimately unify QM and GR, but we do know what it will be called: string theory! Nati Seiberg has admitted as much.

Unless, of course, somebody with the right idea has the foresight to protect his intellectual property 🙂

20. **Thomas Larsson**
January 27, 2005

The Glashow-Ginsparg Physics Today article can be found here.

21.
January 26, 2005

We may not know much about whatever theory will ultimately unify QM and GR, but we do know what it will be called: string theory! Nati Seiberg has admitted as much.

22. **D R Lunsford**
January 26, 2005

Fabio – this is an excellent point. I just finished a book, "102 Minutes", which is a narrative of the demise of the World Trade Center pieced together from survivor stories and cell phone calls of people who would end up as victims. Immediately after the tragedy, Giuliani, then mayor of NY, attempted to spin it into a tale of selfless heroism. In fact it appears that massive incompetence from one end of government to the other, on all levels, lead to the insane scenario of 200 or so firemen standing around in a nearly empty Tower 1, 20 minutes after the collapse of Tower 2, unable to communicate with people a few hundred feet away. The book is a tacit indictment of public policy and the culture of incompetence that has taken over so many aspects of American life.

I see the string theory debacle as a direct result of the involvement of incompetent government bureaucracies in science, who will throw money at anything that might one day produce a super-bomb or Buck Rogers death ray.

The more the incompetence of these bureaucracies is demonstrated by history and repeated failure, the more intransigent they become.

-drl

23. **Jan Eidissen**
January 26, 2005

"…any more than it makes sense to count the Harvard string theorists as part of the math department"

I believe string theory (or LQG) may turn out to be nothing more and nothing less than a highly sophisticated human invention enabling many theoretical physicists to keep busy while another physicist comes out with a sound theory of quantum gravity in which they can work. Meanwhile, labeling is pending.

"If, on the other hand, there is a rich phenomenology at LHC, string theory enetrprise will be in serious trouble"

String theory is a well-devised survival tool (for it's non-falsifiable), so it shall end up being a marginal subject at math departments only when a simpler theory is able to unify GR and QFT or a more plausible alternative comes to scene. I don't expect the LHC to make any big difference, even after conceding that only SM particle debris has been found within its full energy range.

24. **Quantoken**
January 26, 2005

Peter said:
"Ken Lane, one of Glashow's colleagues at BU, says that "String theory is not physics" and that he doesn't know of any BU faculty who think that string theory belongs in the physics department. He does seem to think that it belongs in a math department, something I have some problems with. While certain parts of string theory are mathematically interesting and do belong in math departments, most of what string theorists do is not mathematics."

Wow that really amazed me! Peter, according to you, super string theory are neither physics nor mathematics. Then into which category shall we classify super string theory? Maybe religion? Religious cult do seem to be a fitting category for super stringers 🙂

Too bad politics played such a big role in the science community. Nothing surprising to me, though. According to Darwin's theory, which is the ultimate truth in the universe, "survival of the fittest", the ones surviving in the science community must be the ones most capable of getting fundings. They may not be the ones doing real science, but probably the ones who know to play politics in their hands.

Ever since science has matured into a MASSIVE industry in where a great portion of the human population jump in to earn a living of bread and butter, politics has become an ever more important factor in the science community. Sad but true. "Science" is as dirty as politics nowadays.

Quantoken

25. **Peter**
January 26, 2005

I was talking to someone last night who told me that Coleman (who is 68) has not been well and has been on medical leave for the last couple years.

The kind of cosmology/astrophysics done by the people at the CFA you mention may have some connections to particle physics, but I don't think it makes sense to count these people as particle theorists (any more than it makes sense to count the Harvard string theorists as part of the math department). There are good reasons that they work at the CFA, not at Jefferson.

26. **Anonymous**
January 26, 2005

Among the phenomologists at Harvard you missed
Sidney Coleman , plus a few others at CFA who do
work on interface of particle physics/Cosmology
such as Avi Loeb, George Field, Matias Zaldarriaga
and maybe a few others. So I don;t think Harvard
is all string theory.

27. **DMS**
January 26, 2005

I too was taken aback by Vafa's arrogant tone. This is what happens if one is a true believer, I guess.

I noticed the amusing sentence: " But Harvard's Vafa said that string theorists share excitement about the LHC, and that experimental results it yields will not prove or disprove string theory."

There you have it: straight from the horse's mouth. So much for predictivity and uniqueness 🙂

Anyone know the ratio of string theorists being offered post-docs and faculty positions to non-string theorists (among high energy theorists) recently?

I think, if LHC shows SM Higgs+nothing else (or nothing), string theory will complete the dominance in high energy theory.

If, on the other hand, there is a rich phenomenology at LHC, string theory enetrprise will be in serious trouble. That should worry Vafa and explain his behaviour.

28. **Fabio**
January 26, 2005

You make an interesting observation about how Vafa's fanatasism has not been softened by near total domination of the field, and it begs an anology with the political right in the US, who seem to get more fanatical and vitriolic in their rhetoric the more power they accumulate. There must be something about having near absolute power but knowing that it's built upon such flimsy foundations which causes one to obsessively squash dissent.

29. **flack**
January 26, 2005

I wasn't aware Glashow had left Harvard.

I heard Shamit Kaku say a few years ago that Harvard basically capitulated to stringification because they realized that, whether it was good physics or not, that was the only way to remain "hot" as a theory department.

30.
January 26, 2005

Lane said. "One of the reasons Sheldon Glashow, a Nobel laureate, left Harvard was because they had become way too stringy. We sort of led the way. We did not emphasize string theory, and we attracted Shelly"

What an achievement that is! Ha! "We sort of led the way". Yeah, right…..

31. **D R Lunsford**
January 26, 2005

"So when one person spends 30 years, it's a waste, but when thousands waste 20 years in modern day, they celebrate with champagne. I find that curious." – S Glashow

Now THAT is quotable! 🙂 Nice article.

-drl

32. **Dick Thompson**
January 26, 2005

I suspect the influence, if not the hidden hand, of Sheldon Glashow in BU's decision to deemphasize string theory.

Oxford Twistor String Conference

January 27, 2005
Categories: Uncategorized

The transparencies from the conference on twistor string theory held two weeks ago at Oxford are now available on-line.

Quite a few of the talks deal with the technical details of computing amplitudes. For the motivation from phenomenological particle theory, see the talk by Zvi Bern. As for the motivation and present state of the whole idea of relating QCD to a string theory in twistor space, the only person who really seems to have much to say about this is Witten himself. His transparencies are in three parts: part 1a and part 1b from his first talk and then a second talk in which he explains what the problems with the whole idea are and some ideas he's been thinking about using to try and get around them.

Comments

1. **D R Lunsford**
   January 27, 2005

   Related to Atiyah's notes is the following very interesting paper:

   http://arxiv.org/PS_cache/hep-th/pdf/0105/0105179.pdf

   -drl

2. **D R Lunsford**
   January 27, 2005

   I've been meaning to look into this question…

   Since one apparently invokes twistors to get some knowledge of spacetime (M), is there a technical reason one must build on SO(4,2) vs. SO(3,3)? All the ideas of incidence will hold, with suitable replacement of real configurations by imaginary ones. Of course one will lose the real scalings in M but we don't experience these in any case.

   -drl

Branches of the Landscape

January 27, 2005
Categories: Uncategorized

If you've been following the story of the "Landscape" over the past year or so you'd remember that its proponents felt that if it could predict anything it should be able to predict whether or not there will be supersymmetry at low energies. They had great hopes for making this prediction before 2008 when the LHC presumably will tell us whether there is supersymmetry at LHC energies.

Well, tonight one of the biggest proponents of this point of view, Michael Dine, has a new paper out with two co-authors, entitled Branches of the Landscape. In it they conclude:

"From all this, it appears that it is difficult, in principle, to decide whether or not the landscape predicts supersymmetry."

So, many string theorists now seem to believe that:

1. String theory predicts a landscape of possible vacua.

2. Given the existence of such a landscape, one can't predict whether or not there will be low-energy supersymmetry (or anything else either).

One wonders it these string theorists ever studied elementary logic and can draw the obvious conclusion from 1. and 2.

Comments

1. **Peter**
   January 31, 2005

   Thanks for the information about Lane's letter to the editor!

2. **DMS**
   January 31, 2005

   Thought you might be interested in an update to
   "The Thin Line of Theory".

   http://www.dailyfreepress.com/news/2005/01/27/Opinion/String.Theory.Article.Slanted-843320.shtml

3. **JC**
   January 29, 2005

   These landscape papers seem to look more and more silly as time goes on. Has this anthropic trend affected the number of graduate students and/or postdocs entering into string theory over the last two years or so? If I was still a grad student or postdoc, I surely would be very skeptical of any science which attempts to use the anthropic principle.

4. January 28, 2005

   I wonder if it's totally out of the question to think of string theory as a branch of mathematics that is very relevant to physics, but is not, in and by itself, physics.

   An example that comes to mind is complex analysis: It is very relevant to physics, and one can give all theorems in the subject a physical interpretation: simple poles are electric charges, branch points are magnetic monopoles, etc. But it is a very simple, two dimensional physics, and although it coincides with certain simplified physical phenomena, it is far from being a complete, and closed description of physical reality.

   The theory of complex function will always be a fundamental and correct ingredient of any mathematical description of the physical world, but it is not the whole story, and one cannot use it to say anything about, let's say the big bang or the mass of the electron. For that, one needs many more ingredients, and input from experiments.

   My idea is that string theory is a much larger example of the above. It is wonderful, beautiful, very rich, and very coherent, but it is far from being the full story. My point of view is that this is the reason why it cannot make predictions about the physical world, such as the choice of vacuum, for example.

5. **Thomas Larsson**
   January 28, 2005

   String theorists get together and sign a pact to commit suicide if supersymmetry is not found at the LHC.

   Why wait? Isn't the anthropic principle intellectual harakiri?

   It is of course true that we live in a universe which is not too hostile to human life. If anything, this must be evidence for Intelligent Design.

6. January 28, 2005

   Peter:

   The only viable explanation of why university physics departments do string theory is the anthropic principle. We live in that branch of the landscape where string theory is correct and the anthropic principle is valid.

   That string theorists exist at universities is a well-confirmed 6-sigma fact, and string theory predicts string theorists must exist.

   Therefore string theory must be true.

   Elementary logic!

7. January 27, 2005

   I’ve got an idea for a new version of the anthropic principle:

   String theorists get together and sign a pact to commit suicide if supersymmetry is not found at the LHC.

   Then, in the event it is found, they can claim to have anthropically explained it (if the universe were not supersymmetric, they would all be dead afterall!) and award each other prizes.

8. **D R Lunsford**
   January 27, 2005

   This is obviously string complementarity.

   -drl

Two-Loop Superstring Amplitudes

January 29, 2005
Categories: Uncategorized

Eric D'Hoker and D.H. Phong this past week finally posted two crucial papers with results from their work on two-loop superstring amplitudes. The first one shows gauge slice independence of the two-loop N-point function, the second shows that, for N less than 3 and for low-order terms at N less than 4, there are no two-loop corrections to the low energy effective action.

D'Hoker and Phong have been studying superstring amplitudes for nearly twenty years, and are justly proud of their recent results, which are a tour de force of careful calculation. Over the years there have been many claims made about two-loop amplitudes, but until their work, no one had managed to really sort out the gauge dependence issues and write down gauge-independent amplitudes. For some comments about some of the issues involved at genus 2 and higher, see postings by Jacques Distler here, here, and here.

I don't think D'Hoker and Phong will be coming out with complete results for genus 3 anytime soon, so the state of the art is that there is now a finite and well-defined version of the two-loop superstring amplitudes, with the problem of higher loops still open. While claims abound about the finiteness of higher-loop amplitudes, before believing them one should first take a look at the tricky problems that D'Hoker and Phong had to overcome to get well-defined two-loop amplitudes.

Update: Jacques Distler has a new posting about multi-loop amplitudes and potential problems with the Berkovits version of the superstring (he explains in more detail the possible problems with the BRST and picture-changing operators I mentioned). For some mysterious reason Jacques neglects to refer to my posting or comments about this. I encourage those commenters who seemed convinced I didn't know what I was talking about to now take up their arguments with him.

Comments

1. **Peter**
   February 3, 2005

   What would convince me of finiteness of these perturbative amplitudes would be hearing from the experts in these calculations that they understand theme well-enough to have a solid argument for finiteness. That's not what those I've talked to are telling me.

   I've given some detailed technical arguments about this and you're not addressing them or showing any signs of having looked into the details of what is going on in these two-loop calculations enough to even understand what I am talking about.

   If you talk to people who have done these recent two-loop calculations (d'Hoker-Phong, another group is Zheng-Wu-Zhu) or read their papers they'll explain to you the subtleties that happen at two loops and that they don't know what happens at three loops. As for covariant approaches like that of Berkovits, it is simply undeniable that he has yet to calculate a 2-loop amplitude.

   If you don't believe me, contact any of these experts, or sit down and read some of their papers. Until you've done that, you have no business going around making claims that finiteness of multiloop amplitudes is well-established fact. It isn't and such claims are extremely unfair to people who are working hard to actually understand what is going on, rather than making unsupported claims about the issue.

2. February 3, 2005

   Ok, you win, it is hard to imagine what will covince you of finiteness of perturbative string theory, you seem to be attached to the idea that it is insufficiently established. On the other hand you are quite convincing to your readers (there is a selection effect in place, of course).

3. **Steve**
   February 1, 2005

   D'Hoker and Phong are doing superstring theory the way it needs to be done: facing up squarely and honestly to specific and very hard technical problems, sticking with them over time, and getting rigorous results. I think this is the only way one can approach superstring theory because it remains a very wild beast to tame. This is in contrast to the waffling and speculative Landscape and string cosmology papers of recent times. Years ago I came across a review article in Physics Reports I think, written by D'Hoker and Phong on Differential Geometry and it was very readable and illuminating. These guys do know their stuff.

4. **DMS**
   February 1, 2005

   Peter,

   Many, many thanks for your explanations and for pointing out a nice (very readable!) paper!

   The D=11 supergravity starts having divergences at 2-loops.

   The conclusion seems quite strong (for perturbative supergravity, at least):
   "…there is no local, unitary (ghost/tachyon-free) quantum field
   theory whose action reduces to QGR or classical GR that is also free of infinities; the latter are
   almost certainly there at every order, requiring an infinite number of input parameters to define
   these theories. The conclusion includes all possible SUGRA models, i.e., from D=4 through D=11, as well. Although the presence of new counterterms at all loop orders (or at an infinite set of them) cannot reasonably be rigorously demonstrable, the fact that the ones we did see appeared at lowest permitted order (so that no ?hidden? invariances prevented them), is quite convincing evidence."

5. **Peter**
   February 1, 2005

   Yes, the general assumption is that string perturbation theory gives an asymptotic series, and so people like to claim that the situation is no worse than in QFT, where the same thing happens. But, at least for non-abelian gauge theories, in that case there is a simple non-perturbative definition of the theory which appears to be well-defined and give finite answers.

   Again, there's no evidence of a problem that will occur at higher loops in superstring multi-loop amplitudes, there just isn't a solid argument showing that no problems can occur. I'd have to look up the precise facts about supergravity, I believe they depend on N, the number of supersymmetries. But it has been shown that the known symmetries of the theory are such that at high enough loops you get terms that have no reason to be zero and are presumably divergent. It is true that these problems are at high enough loops that no one has been able to actually compute the diagrams to be sure the infinities are really there.

A quick search turned up this reference
http://arxiv.org/abs/gr-qc/9911073

6. **DMS**
   February 1, 2005

   Peter,

   It is pretty sad to see (some) string theorists being unreliable even about technical aspects of the subject. Prior to this, I thought string theory had already been "proven finite" to all orders: thanks for pointing out D'Hoker-Phong's remarakable paper.

   A couple of questions:
   "…even if the multiloop amplitudes are finite, their sum isn't"

   Does that mean string perturbation theory series is an asymptotic series?

   "Maybe someone will find that, just like in supergravity, if you go to high enough order, you run into trouble. "

   Surprising result to me. Is it easy to understand why this happens? A reference perhaps?

7. **Peter**
   February 1, 2005

   I think there are still potential problems with singularities in superstring amplitudes at the boundaries in moduli space, not just the interior. But sure, the reason you give was a good reason for Schwarz et. al to look into superstring theory as a theory of quantum gravity 30 years ago.

   But for the last 20 years this idea has been beaten to death by thousands of theorists writing tens of thousands of papers on the subject. The bottom line of all this work is that, even if the multiloop amplitudes are finite, their sum isn't, so this idea doesn't give a well-defined, finite theory of quantum gravity. There's a lot of wishful thinking about hypothetical M-theory and non-perturbative versions of string theory, but wishful thinking is all it is right now. The main achievement of this whole program, the supposed calculation of the entropy of certain special black-hole like configurations using branes, is pretty underwhelming.

   You can't just keep repeating the initial motivation for a speculative idea, long after a huge amount of effort has gone into working on it, with extremely disappointing results. At some point you have to admit failure and move on, something that most string theorists seem incapable of.

8.
   February 1, 2005

   OK, just to summarize the situation in my mind: there is a powerful physical mechanism that cancels the divergences that cause real problems for any other attempts to quantize gravity. This is the old insight that caused many, who were not born string theorists, to be attracted to string theory as a quantum theory of gravity (if not as a theory of particle physics), and what is referred to by them as finiteness. The potential problems with the interior of moduli space have to be sorted through, but are as far as we can tell they are just a technicality. Regardless of the hype, the semantics, etc., if you were interested in quantum gravity, is it not a good enough reason to work on the field?

9.
   January 30, 2005

   OK, I think we do agree on the facts, though I seem to give much more weight to that UV finiteness and unitarity, again since it is not acheivable in any other context. As for your points:

   1. The particle like limit of the string corresponds to various boundaries of moduli space. I was under the impression that these limits are simple and one is able to make general statements about them, though I am far from being an expert. The tricky parts have to do with the interior of moduli space. If there were some problems there they would not be called UV or IR diveregences, they would be inherently stringy. No reason to suspect they are there, but in any event I am more impressed with cancelling the divergences we already knew about, not with the elimination of all logical possibilities for problems.

   2. Divergences are wonderful since they give rise to proper understanding of field theories via the renormalization group. This very understanding tells you GR cannot be treated as QFT, as it is not renormalizable. This is a concrete and straightforward problem, not an interpretational issue or an aesthetic displeasure (as in "background independence"), one simply cannot calculate finite and unambigous results.

10. **Peter**
    January 30, 2005

    Lubos,

    What's his argument that divergences in the interior of moduli space can't occur in his formalism? Do you mean the footnote where he says that in conformal gauge, there are no obvious potential sources of divergences since the amplitudes are independent (up to surface terms) of the locations of picture-changing operators?

    This isn't a very solid argument. How do I know there isn't a "non-obvious" potential source of divergences? And how do I know that the picture-changing operators are really well-defined and do exactly what they are supposed to? If Berkovits could explicitly work out what happens at two loops so one could be sure that at least in that case he really had well-defined amplitudes and there were no "non-obvious" problems, that would be a lot more convincing.

    That's about all I have to say about this, if you really are so interested in this topic, you should read D'Hoker and Phong's papers carefully. I think if you do this you'll see that this is an exceedingly tricky business and that it is far from obvious that Berkovits has a solid argument.

11. **Peter**
    January 30, 2005

    To whoever my non-abusive commenter is:

    I think we're agreed about the bosonic string and the fact that at one loop it doesn't have the same problems that come about at one loop in a field theory due to integration over arbitrarily high momentum going around the loop. Of course it has other problems.

But two points about this:

1. The superstring is much trickier. If you look at D'Hoker and Phong's two-loop calculation it involves very subtle cancelations of singularities in order to get a finite result. It's not a matter of combining together well-behaved bosonic amplitudes with signs to get something well-behaved. There are singularities that have to cancel precisely. They are able to show that this happens at two-loops, but aren't able to do this for more than two loops. Whether these singularities have an interpretation as "IR" or "UV" singularities I don't know. I was under the impression that you couldn't cleanly separate which was which.

2. In non-abelian gauge theories, the fact that loop integrations diverge as you take the momentum cutoff to infinity is not a bad thing, but a good thing. For one thing, it tells you about asymptotic freedom. For another, via dimensional transmutation, in pure gauge theory you end up with a theory with zero free parameters. If all your Feynman diagrams were finite and no renormalization was required you would have a theory with a free parameter (and in the pure gauge theory case would be conformally invariant). So the fact that your diagrams require renormalization leads to a proper understanding of the theory's short distance behavior, and as a final result a very non-trivial theory with a complicated spectrum and zero free parameters. What's bad about that?
OK, this doesn't work for the Einstein action, but it indicates the problem is not necessarily inherent in the divergent integrations in Feynman diagrams.

12. **Lubos Motl**
January 30, 2005

Just to explain more clearly why I wrote that Peter was ridiculous.

Peter Woit wrote:

Berkovits himself says his argument only gives finiteness if you assume "there are no unphysical divergences in the interior of moduli space". But this absence of "unphysical divergences" is a crucial part of what one needs to prove.

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This is very funny because the reason why Nathan wrote this is *exactly* because this problem (unphysical divergences inside the moduli space) can *not* arise in his formalism.

13.
January 30, 2005

I hope nothing I writen here qualifies as abusive, though I have to admit I find the sociological points less than fascinating.

I am not saying anything that you don't know. Take the bosonic string in flat non-compact space and calculate (for example) one loop scattering, you get results that are UV finite. I am explicitely talking about the bosonic string because:

1. It has severe IR problems, which is not what we are discussing.

2. It has no technical problems due to fermions.

3. One cannot claim miracles due to SUSY, which should be irrelevant for UV phenomena anyhow.

What I mean by UV finite is very well defined: you look at the contribution to the amplitudes from the limit where the string is particle-like, and that particle moves at very high momentum. This is the region which leads to all the celebrated UV problems of gravity. The contribtions to the string amplitudes from this corner are finite, and one can look for specific mechanical reasons for this. These are covered in all standard textbooks, and have to do with duality , or more physically the exponential softness of tree level string amplitudes, which suppresses the UV region when performing loop amplitudes.

These properties cannot hold in any local QFT with finitely many degrees of freedom (consequently they do not look very natural in string field theory, and are lost in any truncation thereof). Any other attempt to make these amplitudes finite, a subject with a very long history, involves some ad-hoc cutoff and the expected problems with unitarity. I am not aware of any other physical mechanism to cut-off the problematic region of integration without loss of unitarity.

This is maybe less than a complete finiteness, and I am not claiming that's the most impressive statement one can make, but this is already plenty impressive, at least for me.

14. **Peter**
January 30, 2005

I agree with your statement that it is reasonable to let the multi-loop problem go and think about other things. But when string theorists do this, they should stop using multi-loop finiteness as their big selling point for the theory (or thuggishly abusing anyone who mentions the problem to them)

This is not an argument about semantics, but about a very specific factual question. Are there well-defined, finite, multi-loop amplitudes for the superstring? Right now the answer is yes up to two loops. For three loops and higher the answer to this is yes or no, but we don't know which yet. Maybe Berkovits will develop his formalism to the point where he really can explicitly construct such amplitudes. Maybe someone will find that, just like in supergravity, if you go to high enough order, you run into trouble. Nobody has a specific proposal for what this trouble would be, so it is reasonable to guess that it doesn't occur, but who knows?

I'm not at all sure what you mean by "UV finite" or that there is any sensible meaning to such a phrase. What fact is true for what specific reasons special to the extended nature of the string? Remember, in the case of strings you can't disentangle UV and IR phenomena (think about T duality and the implications of modular invariance). Multi-loop superstring amplitudes are certainly different kinds of objects (for one thing, they're a couple orders of magnitude more complicated…)than Feynman diagrams. They don't have the same problems with integrating momenta running around loops that Feynman diagrams have. But that doesn't mean they don't have potential problems with singularities, some of which one could even interpret as coming from short distances.

15.
January 30, 2005

Ok, another way of saying things is that the difference in opinion is about semantics, not about the facts. Maybe a better formulation is to state that string theory is UV finite, a statement which refers to the type of infinities one finds in QFT, but does not cover absolutely everyhting one may think of. This fact is true for very specific and well-understood reasons to do with the extended nature of the string. This is a precise statement which is correct, I believe.

It is not proven that there are no other mysterious divergences that will be specific for strings and will somehow conspire to appear at 3-loops (usually potential problems appear as soon as you include quantum corrections), but there is no reason to suspect it. Given that, and that nobody has specific reasons to be interested in the result of the multiloop amplitudes, it is a reasonable attitude to just let it go and think about other things.

16. **Peter**

January 30, 2005

I understand well the difference between math and physics cultures about proof, since my Ph.D was in a physics department and I work in a math department. That's not what's at issue here.

The situation is not like in QCD, where there's lots of evidence from computer calculations for confinement. There's also the fact that whenever you can check QCD against experiment it works precisely, and you never observe free quarks.

For superstring amplitudes there's nothing like this. There's no experimental evidence concerning them and the only existing actual calculation of a well-defined multi-loop amplitude is the calculation at two-loops. No one has any kind of approximate or computer calculation of an amplitude beyond two loops. There's not much known about these things beyond formal expressions, expressions you still need to do a lot of work with to make sense of. If Berkovits did this in his formalism for two loops and things worked out as he expects, it would provide some evidence that things would work at higher loops, but he hasn't done this yet.

The only "physical" argument for finiteness of multiloop superstring amplitudes is the bogus one I've seen a couple hundred times now at the beginning of every general string theory talk. This is the one where you draw a world sheet and a Feynman graph and point out that while the interaction takes place at a point on the graph, there is no well-defined interaction point on the worldsheet. Then the speaker claims this shows string theory doesn't have singularity problems. This ignores all kinds of problems, many of them associated with the fact that there is a moduli space of metrics on the worldsheet. As you go to the boundaries of this moduli space there is all sorts of singular behavior to worry about. In addition there are lots of other possible sources of trouble. Berkovits uses BRST and picture-changing operator techniques involving composite operators. When you multiply operators together in QFT, there are plenty of new singularity problems to worry about. Whether all of these problems can be handled beyond two loops (or even at two-loops in the Berkovits formalism) is still an open problem.

I don't have any problem with string theorists hoping for the best, assuming that superstring multi-loop amplitudes are finite, and then going on to see if they can get a sensible TOE out of the theory. But what has happened is that this has failed, superstring theory has not lead to a sensible TOE. Instead of abandoning the theory, they keep promoting how wonderful it is because of its finite multi-loop amplitudes. It's only fair to point out to them that this is an assumption, not a result. Just because you would like something to be true, doesn't mean it is.

17.
January 30, 2005

A bit of a clash of cultures here, physicists usually do not care if something is proven beyond mathematical doubt, anyone you will ask these days will believe in confinement of quarks for example.
To convince a physicist something is false, after enough evidence is presented in favor of it, a counter-argument is needed. The mathematician's line "you did not prove it" cannot be convincing- almost nothing is proven rigorously in any non-trivial dynamical system (which is reason to suspect that if you are after rigorous proofs you will end up studying trivial dynamical systems such as TFT).

18. **Peter**
January 30, 2005

Hi Lubos,

It's kind of pathetic the way you just start spewing insults when you don't have an argument.

What is in the D'Hoker-Phong papers and in the Berkovits papers, together with what is not in the Berkovits papers (a two-loop calculation) speaks for itself.

If I had to bet, I'd bet that at three loops superstring amplitudes are finite. But I don't know for a fact that this is true, and neither does anyone else at this point.

19. **Lubo? Motl**
January 30, 2005

You're ridiculous, Peter. You have obviously searched the paper for something that could be argued to be a loophole, so that you can make your simple readers happier. Well, it's your decision that you don't care if the physicists think that you are about as stupid as your readers.

It has never happened that string theory would suddenly display some inconsistency, and your belief that something like that could suddenly occur at 3 loops or anywhere else is silliness.

20. **Peter**
January 30, 2005

Berkovits himself says his argument only gives finiteness if you assume "there are no unphysical divergences in the interior of moduli space". But this absence of "unphysical divergences" is a crucial part of what one needs to prove.

If you look at the history of the two-loop calculation before D'Hoker/Phong, there were many people claiming to have written these amplitudes down and to have proofs of finiteness. But their amplitudes were gauge dependent, and once D'Hoker/Phong finally sorted things out, it turns out the correct amplitudes have a different structure than what earlier people had assumed in their "proofs". Berkovits has yet to write down explicitly a two-loop amplitude in his formalism and see if it really has the properties he would like.

21. **Lubo? Motl**
January 29, 2005

D'Hoker and Phong are doing very difficult stuff. On the other hand, the finiteness is easier to prove in formalisms with manifest spacetime supersymmetry where the vanishing of the surface terms is easier to prove.

The only known working covariant formalism of this type is Berkovits' pure spinor formalism. Nathan's calculations are of course extremely impressive, but they were doable and finite for him, and he finished the proof of the finiteness of stringy perturbative expansion as far as we're able to say (although I don't follow all the details):

http://motls.blogspot.com/2005/01/pure-spinor-formalism.html

This kind of humiliates the rest of us, especially the skeptics who have been saying for years that it is impossible to prove the finiteness at all orders, or who – the more weird ones among them – were even suggesting that it does not have to be true.

Loop Quantum Gravity Debate

January 29, 2005
Categories: Uncategorized

A couple weeks ago, three string theorists, (Nicolai, Peeters and Zamaklar) posted on the arXiv a critical assessment of loop quantum gravity. Today I received from Lee Smolin something he wrote responding to them, and I'm posting it here with his permission. Lubos Motl also has put up Smolin's text on his weblog this morning, but I thought it would be a good idea to provide a version that doesn't include Lubos's interspersed rantings. Smolin has some very interesting things to say, and his comments are well-worth reading by anyone who wants to understand what is going on in this field.

Somewhat off-topic, I'd also like to mention a paper by Freidel and Starodubtsev from earlier this week called Quantum gravity in terms of topological observables. The idea of trying to use topological quantum field theory to understand quantum gravity is one that I've always found appealing, and this paper is an interesting attempt to make this idea work. I don't think I find it completely convincing, for one thing they seem to be breaking the topological invariance by hand. For another, TQFTs are very subtle QFTs, and the kind that might be relevant to gravity is still very far from well-understood.

Dear Friends,

Thanks very much for all the time and work you put into your review. While I disagree with a number of your assertions, both in point of detail and of attitude, what is certainly very much appreciated is your evident willingness to "get your hands dirty," learn the technicalities and attack key problems. It is very good that you do this, as indeed too few of us loop people have taken the time to try to learn the details and attack problems in string theory.

Some points you raise have been underappreciated. The issue of what happens to the chiral anomaly, and whether there is fermion doubling in LQG is one I have suggested to many graduate students and postdocs over the years, but so far no one takes it up. It would be good to know if LQG forces us to believe in a vector model of weak interactions.

At the same time, the major difficulties you raise were underestood to be there more than ten years ago. This is especially true with respect to issues concerning the hamiltonian constraint such as the algebra and ultralocality.

What is missing from your "review" is an appreciation of how the work done over the last ten years addresses these difficulties.
Indeed the fact that much work in the field has been on spin foam models is exactly because the problems you worry about do not arise in spin foam models. I will explain this below. Other work, such as Thiemann's master constraint approach, also is motivated by a possible resolution of these problems.

As you will appreciate, like any active community of 100+ people there is a range of opinions about the key unsolved problems. I have the sense that you are aware of only one out of several influencial points of view.

The view your concerns reflect is what one might call the "orthodox hamiltonian" point of view towards LQG. According to this, the aim of work in lqg is not so much to find the quantum theory of gravity as to work through the excercise of quantizing a particular classical theory, which is Einstein's. From this point of view, the program would fail if it turned out that there was not a consistent canonical quantization of the Einstein's equations.

While I will refer to my own views so as not to implicate anyone else, you should beware that this is not necessarily the dominant view in the field. It is a respectable view, and I have the greatest respect for my friends who hold it. But, were it to fail, many of us would still believe that loop quantum gravity is the most promising approach to quantum gravity.

This is not avoidence of hard problems, there are good physical reasons for this assertion, which I'd like to explain.

What I and others have taken as most important about Ashtekar's great advance is the discovery that GR can be writen as a diffeomorphism invariant gauge theory, where the configuration space is that of a connection on a manifold Sigma, mod gauge transformations and Diff(Sigma). This turns out to be true not only of Einstein's theory in 4d but of all the classical gravity theory we know, in all dimentions, including supergravity, up to d=11, and coupled to a variety of matter fields.

This is a kinematical observation and it leads to a hypothesis at the kinematical level, which is that the quantum theory of gravity, whatever it is, is to be written in terms of states which come from the quantization of this configuration space. This as you know, leads directly to the diffeo classes of spin net states. Furthermore, given the recent uniqueness theorems, that hilbert space is unique for spacetime dimension 3 or greater. Thus, o long as the object is to construct a theory based on diffeomorphism invariant states, it cannot be avoided.

The main physical hypothesis of LQG is not that the quantum Einstein equations describe nature. It is that the hilbert space of diffeo classes of spin nets, extended as needed for matter, p-form fields, supersymmetry etc, is the correct arena for quantum gravitational physics. Given that the theorems show that this hilbert space exists rigorously, this is a well defined hypothesis about physics. It may hold whether or not the Einstein equations quantized give the correct dynamics.

A lot already follows from this hypothesis. It gives us states, discreteness of some geometric diffeo invariant observers, a physical interpretation in terms of discrete quantum geometry etc.

But there is also a lot of freedom. We are free to pick the dimension, topology, and algebra whose reps and intertwiners label the spinnetworks. This then gives us a large class of diffeo invariant quantum gauge theories, of which the choices that come from GR in d=4 are only one example. These are possible kinematics for consistent background independent quantum field theories.

Now let us come to dynamics. I believe the most important observation for an understanding of quantum dyannics in this class of theories is that all gravitational theories we know, in all dimensions, super or not, are constrained topological field theories. (See my latest review, hep-th/0408048, for details and references for all assertions here.) This means they are related to BF theories by non-derivative constraints, quadratic in the B fields.

A lot follows from this very general observation. It allows a direct construction of spin foam models, by imposing the quadratic constraints in the measure of the path integral for BF theory. This was the path pioneered by Barrett and Crane. The construction of the Barrett Crane and other spin foam models does not depend on the existence of a well defined hamiltonian constraint. The properties that have been proven for it, such as certain convergence results, also do not depend on any dynamical results from the hamiltonian theory.

The relation to topological field theory is also sufficient to determine the basic form of fields and states on boundaries. In 4d these give the role of Chern-Simons theory in horizon and other boundary states. Thus, it gives the basic quantum geometry of horizons.

Once we have the basic form of spin foam models, which follow from the general relation to BF theories, we can consider the problem of dyanmics in the following light. Given the choices made above, the spin foam amplitudes are chosen from the invariants of the algebra which labels the spin networks. There is then a large class of theories, differing by the choice of the spin foam amplitudes. Each is a well defined spin foam model, which gives amplitudes to propgate the spin network states based on the chosen dimension and algebra.

The lack of uniqueness is unaviodable, because there is a general class of theories, just like there is a general class of lattice gauge theories. These theories exist, and the general program of LQG as some of us understand it, is to study them.

>From a modern, renormalization group point of view, the first phsyical question to be answered is which of these theories lead to evolution that is sensible, i.e. which spin foam ampltidues are convergent in some approrpiate sense. The second physical question is to classify the universality classes of the spin foam models and, having done this, learn which classes of theories have a good low energy behaivor that reproduces classical GR and QFT.

It is of course of interest to ask whether some of these theories follow from quantizing classical theories like GR and supergravity, by various methods. But no one should mind if the most successful spin foam model, in terms of both matheamtical elegance and physical results, was not the quantization of a classical theory, but only reproduced the classical theory in the low energy limit. How could one object from a physics point of view, were this true?

This is the point of view from which many of us view the problems with the hamiltonian constraint you describe.
The next thing to be emphasized is that there is no evidence that a successful spin foam model must have a corresponding quantum hamiltonain constraint. There are even arguments that it should not. These have not pursuaded everyone in the community, and this is proper, for the healthiest situation is to have differing views about open problems. But it has persuaded many of us, which is why many people in the field turned to the study of spin foam models after the difficulties you describe were understood, more than ten years ago.

For example, Fotini Markopoulou argued that, as the generators of infinitesimal spatial diffeos do not exist in the kinematnical hilbert space, while generators of finte spatial diffeos do exist, the same should be true for time evolution. This implies that there should only be amplitudes for finite evolutions, from which she proposed one could construct causal spin foam models.

This was partly motivated by the issue ultralocality. (Btw, you dont emphasize the paper that first raised this worry, which was my gr-qc/9609034). The worry arises because moves such as 2 to 2 moves necessary for propagation do not occur in the forms of the hamiltonian constraint constructed by Thiemann, Rovelli and myself, or Borissov. This is because they involve two nodes connected by a finite edge.

However, the missing moves are there in spin foam models. This concretely confirms Fotini's argument. In fact, as Reisenberger and Rovelli argued, invariance under boosts generated by spacetime diffeo requires that they be there. For one can turn a 1-3 move into a 2->2 (0r 1->4 into 2-> 3) move by slicing the spin foam differently into a sequance of spinnetworks evolving in time.

So we have two arguments that suggest 1) that the problem of ultralocaity comes from requiring infinitesimal timelike diffeos to exist in a theory where infinitesimal spacelike diffeos do not exist and 2) the problem is not present in a path integral approach where there are only amplitudes for finite timelike diffeos.

One can further argue that if there were a regularization of the hamiltonian constraint that produced the amplitudes necesary for propagation and agreed with the spin foam ampltidues, it would have to be derived from a point splitting in time as well as space. This suggests that there is a physical inadquancy of defining dynamics through the hamiltonian constraint, in a formalism where one can regulate only in space and not in time.

Let me also add that there is good reason to think that the other issues such as the algebra of constraints arise because of the issue of ultralocality. Thiemann's constraints have the right algebra for an ultralocal theory.

It was for these and other reasons that some of us decided ten years ago to put the problems of the hamiltonian constraint to one side and concentrate on spin foam models. That is, we take the canonical methods as having been good enough to give us a kinematical frameowrk for a large class of diffeo invariant gauge theories, but unnecessary and perhaps insufficient for studying dynamics.

At the very least, making a point splitting regularization in both space and time seems a much more difficult problem and hence is less attractive than spin foam methods where one can much more easily get to the physics. Given that the relation to BF theory gives us an independent way to define the dynamics, and path integral methods are more directly connected to many physical questions we want to investigate, there seemed no reason to hold back progress on the chance that the problems of the hamiltonian constraint can be cleanly resolved.

Nothing I've said here means that I am not highly supportive of Thomas's and others efforts to resolve the problems of the hamiltonian dynamics-I am. But it must be said that a "review" of LQG that focues on this issue misses the significance of much of the work done the last ten years.

Let me make an analogy. No one has proved perturbative finiteness of superstring theory past genus two. I could, and have even been tempted to, write a review of the problem, highlighting the heroic work of a few people like d'Hoker and Phong to resolve it. I think it would be useful if someone did that, as their work is underappreciated. But it would be very unfair of me to call this a review of, or introduction to, the state of string theory. Were I to do so, I would rightly be criticized as focusing on a very hard problem that most people in the field have for many years felt was not crucial for the development of the theory. This is not a perfect analogy to what you have done in your "review", but it is pretty close.

There are other mis-statments in your review. For example, there are certainly results at the semiclassical level. Otherwise there could not be a lively literature and debate about predictions stemming from LQG for real experiments. See my recent hep-th/0501091 for an introduction and references. Of course semiclassical states do not necessarily fit into a rigorous framework-after all, WKB states are typically not normalizable. But I would suggest that it may be too much to require that results in QFT that make experimental predictions be first discovered through rigorous methods. At the standards of particle physics levels of rigor, there are

semiclassical results, and these do lead to nontrivial predictions for near term experiments. It is possible that a more rigorouos treatment will in time lead to a rigorous understanding of how classical dynamics emerges-and that is a very important problem. But given that AUGER and GLAST may report within two years, may I suggest that it is reasonable to do what we can do now to draw predictions from the theory.
In closing let me emphasize again that your efforts are very well appreciated. I hope this is the beginning of a dialogue, and that you will be interested to explore other aspects of LQG not covered by or addressed in your review.

Sincerely yours,

Lee Smolin

Comments

1. **Ludwig**
   February 4, 2005

   Urs, have a good time in California!
   Maybe we can resume this discussion next week when you get back.

2. **Urs**
   February 3, 2005

   Hi Ludwig,

   I will be flying to California this weekend visiting the deep M-theory thinker John Baez :-), right after end of semester. Last preparations will probably keep me from chatting on the web too much today and tomorrow. But when I find the time I will get back to you, maybe next week.

   One quick remark, though:

   Nice that we managed to agree on what is going on. In summary it seems to me that the following has happened:

   Somebody working on LQG/spin foams (namely Smolin and collaborators) has come up with some action S whose space of classical solutions (=extrema) contains (when suitably identified) those of 4-dimensional GR as well as those of some topological theory.

   This is a curious formal observation, though I am not yet sure if it is really deep or useful. But let's not argue about that. If something useful can be derived from this action (as suggested, but not yet demonstrated, by Freidel and Starodubtsev), fine.

   However, it should be made rather clear that this new action S is just some formal construct of which nobody has any reason to expect that it directly describes our world. Rather, it plays a role of a calculational trick so far. And this trick also so far has been demonstrated to work only classically. I think the authors of the papers that I have seen would agree with this statement.

   Even if any of these authors nourished hopes that this curious action S is actually phenomenologically important (in its full form, not just in its reduction to GR), one should make very clear that just writing down this action is not the same as doing LQG.

   But it now seems to me that, since Smolin is a prominent representative of LQG, and since he wrote about this action S, Vafa got the impression that this action is part of the program called LQG. Since this action has a 'sector' of solutions which are that of BF theory, it seems that Vafa concluded one can make the statement that 'the topological sector of LQG is BF theory'.

   Then, when he found BF theory in topological M-theory he commented that hence 'the topological sector of LQG' has appeared in topological M-theory.

   (If that is not what happened I'd be grateful for corrections. It sure *seems* that this is what is going on.)

   But really it is BF theory that has appeared in topological M-theory. BF-theory is not the 'topological sector of LQG', really, but merely the topological sector of the above discussed curious action – which was more or less *designed* to have BF theory solutions among its solutions.

   The statement that 'BF theory is the topological sector of that special action S, while GR is another 'sector" is true by construction of that strange action, really.

   The motivation for the construction of S is that it might help (which has not been shown yet, though) to approach GR with tools of topological field theory. But that should not be confused with the stament that 'LQG has a topological sector which is BF theory'.

   Unless, of course, one would go ahead and redefine the term 'LQG'. Maybe if something is investigated by Smolin people will tend to call it 'LQG'. In that case however it should be made very clear in every discussion what precisely is to be meant by the term 'LQG'.

   So in conclusion I think what happens is that

   Vafa found in topological M-theory a topological theory which, by construction is, classically, a certain sector of some action principle investigated by Smolin.

   If this is true it is nothing to be axcited about at all, I think.

3. **Ludwig**
   February 2, 2005

   Hi Urs,
   Hearty thanks for your reply!
   I am pleased that you approve my brief listing of some papers.
   For now I can only respond briefly to your question and comments.

   Urs said:

“Do you agree with this summary?
Again some comments:

1) What Vafa finds in topological M theory is BF theory, not any of its extensions that Smolin, Freidel and Starodubtsev discuss, right?

2) Having some action that involves BF theory is not yet the same as ‘doing LQG’, right?

3) I have more comments, but I gotta run. More later, if you like.”
Posted by: Urs at February 2, 2005 10:59 AM

To the extent that I can rely on my own inexpert judgment I agree fully with your comments:
1) absolutely! I do not hear Vafa connect top.M-theory to LQG (more like he creates a context or perspective in which both may be mentioned)
2) right! it is not the same as doing LQG
3) I am very glad you have more comments and hope you will share them with us.

One reservation is that I only heard the Vafa audio and did not see what he wrote at the board. I can only guess about some of what he said.

I also have several times read through your summary of the Smolin/Starodubtsev (2003) paper and find that I agree completely with your summary.

4. **Urs**
February 2, 2005

Hi Ludwig,

thanks for your efforts in replying and listing literature. It is appreciated.

I am short of time today so let me reply to your latest message first and postpone the previous discussion until later.

You wrote:

> After listening to Vafa’s whole talk, I would say that what he said, including towards the end where he again discussed LQG, has significant overlap with
> Smolin, Starodubtsev(2003)
> General relativity with a topological phase: an action principle

I have just had a look at this paper. Therein the following is discussed:

BF theory is a well known topological field theory. By adding a certain term to it that breaks both some gauge symmetry as well as the topological property one obtains an action that can be shown to be equivalent to the Einstein-Hilbert action.

Put the other way round: The Einstein-Hilbert action can be massaged into a form which is a topological term plus something else.

That ‘something else’ involves a fixed background structure (that gamma5 vector) which is needed to reduces the 5-d theory to 4-d.

In their paper Smolin and Starodubtsev proceed by adding yet another term to the action which makes this background structure dynamical. By the above discussion the result is a modification of the Einstein Hilbert action.

Because that first extra term is now dynamical, there are solutions where it reproduces the first step and hence the EH action, but there are also solutions where it takes other values and yields topological theories.

Hence, in conclusion, the authors find that there is an extension of the EH action which has some solutions that reproduce those of the EH action and some that don’t.

Do you agree with this summary?

Again some comments:

1) What Vafa finds in topological M theory is BF theory, not any of its extensions that Smolin, Freidel and Starodubtsev discuss, right?

2) Having some action that involves BF theory is not yet the same as ‘doing LQG’, right?

3) I have more comments, but I gotta run. More later, if you like.

5. **Ludwig**
February 1, 2005

The connection of TQFT (BF theory especially) with Loop-and-related Quantum Gravity seems to go back to 1995, see for instance the paper linked below
Smolin (1995)
Linking topological quantum field theory and nonperturbative quantum gravity

The first paper by Smolin dealing with QG in relation to BF theory explicitly, at least that I could find, is Smolin (1998), see link.

The history of the LQG/Spin Foam/BF connection has become interesting (for instance because of Cumrun Vafa’s discussion this month in Toronto, which was compensated by other remarks we saw tending to dismiss or downplay the relationship.
So I thought I would provide this sketchy bibliography to give some background perspective.

After listening to Vafa’s whole talk, I would say that what he said, including towards the end where he again discussed LQG, has significant overlap with
Smolin, Starodubtsev(2003)
General relativity with a topological phase: an action principle

It would be great to have a text copy of Vafa’s talk with some footnotes, because in the audio I couldnt catch what his sources were or if it was just general Vafa-knowledge

John Baez (1995)
4-Dimensional BF Theory as a Topological Quantum Field Theory
15 pages
http://arxiv.org/q-alg/9507006

"Starting from a Lie group G whose Lie algebra is equipped with an invariant nondegenerate symmetric bilinear form, we show that 4-dimensional BF theory with cosmological term gives rise to a TQFT satisfying a generalization of Atiyah's axioms to manifolds equipped with principal G-bundle. The case G = GL(4,R) is especially interesting because every 4-manifold is then naturally equipped with a principal G-bundle, namely its frame bundle. In this case, the partition function of a compact oriented 4-manifold is the exponential of its signature, and the resulting TQFT is isomorphic to that constructed by Crane and Yetter using a state sum model, or by Broda using a surgery presentation of 4-manifolds."

Smolin (1995)
Linking topological quantum field theory and nonperturbative quantum gravity
http://arxiv.org/gr-qc/9505028 (TQFT + QG)

Smolin (1998)
A holographic formulation of quantum general relativity
http://arxiv.org/hep-th/9808191 (explicitly BF + QG)

"…Thus, this approach is similar to that of MacDowell-Mansouri, in which general relativity is found as a consequence of breaking the SO(3, 2) symmetry of a topological quantum field theory down to SO(3, 1)[29]. However it differs from that approach in that the beginning point is a BF theory… "

John Baez (1999)
An Introduction to Spin Foam Models of Quantum Gravity and BF Theory
55 pages, 31 figures
http://arxiv.org/gr-qc/9905087

"In loop quantum gravity we now have a clear picture of the quantum geometry of space, thanks in part to the theory of spin networks. The concept of 'spin foam' is intended to serve as a similar picture for the quantum geometry of spacetime. In general, a spin network is a graph with edges labelled by representations and vertices labelled by intertwining operators. Similarly, a spin foam is a 2-dimensional complex with faces labelled by representations and edges labelled by intertwining operators. In a 'spin foam model' we describe states as linear combinations of spin networks and compute transition amplitudes as sums over spin foams. This paper aims to provide a self-contained introduction to spin foam models of quantum gravity and a simpler field theory called BF theory."

Smolin (2000)
Holographic Formulation of Quantum Supergravity
http://arxiv.org/hep-th/0009018

Smolin, Starodubtsev(2003)
General relativity with a topological phase: an action principle
http://arxiv.org/hep-th/0311163

"An action principle is described which unifies general relativity and topological field theory. An additional degree of freedom is introduced and depending on the value it takes the theory has solutions that reduce it to 1) general relativity in Palatini form, 2) general relativity in the Ashtekar form, 3) F wedge F theory for SO(5) and 4) BF theory for SO(5). This theory then makes it possible to describe explicitly the dynamics of phase transition between a topological phase and a gravitational phase where the theory has local degrees of freedom. We also find that a boundary between adymnamical and topological phase resembles an horizon."

Freidel, Starodubtsev (2005)
Quantum gravity in terms of topological observables
http://arxiv.org/abs/hep-th/0501191

6. **Ludwig**
February 1, 2005

Urs said:
4) What if spin foams could reproduce the kinematics of the canonical LQG approach. Would that imply that spin foams have no dynamics, either?
Posted by: Urs at January 31, 2005 01:11 PM

About Urs question #4, the answer is no, it would not imply that spinfoams have no dynamics. Indeed it has not been established that canonical LQG must have no dynamics. Nor has Smolin claimed this is necessarily the case. If anyone is interested in recent progress in LQG dynamics some relevant papers are
these five by Thomas Thiemann and Bianca Dittrich

http://arxiv.org/abs/gr-qc/0411138
Testing the Master Constraint Programme for Loop Quantum Gravity I. General Framework
42 pages

"Recently the Master Constraint Programme for Loop Quantum Gravity (LQG) was proposed as a classically equivalent way to impose the infinite number of Wheeler-DeWitt constraint equations …. The models themselves will be studied in the remaining four papers. As a side result we develop the Direct Integral Decomposition (DID) for solving quantum constraints as an alternative to Refined Algebraic Quantization (RAQ)."

http://arxiv.org/abs/gr-qc/0411139
Testing the Master Constraint Programme for Loop Quantum Gravity II. Finite Dimensional Systems
23 pages

"This is the second paper in our series of five in which we test the Master Constraint Programme for solving the Hamiltonian constraint in Loop Quantum Gravity…"

http://arxiv.org/abs/gr-qc/0411140
Testing the Master Constraint Programme for Loop Quantum Gravity III. SL(2,R) Models
33 pages

"This is the third paper in our series of five…"

http://arxiv.org/abs/gr-qc/0411141
Testing the Master Constraint Programme for Loop Quantum Gravity IV. Free Field Theories
23 pages

"… We now move on to free field theories with constraints, namely Maxwell theory and linearized gravity…"

http://arxiv.org/abs/gr-qc/0411142

Testing the Master Constraint Programme for Loop Quantum Gravity V. Interacting Field Theories
20 pages

"… Here we consider interacting quantum field theories, specifically we consider the non-Abelian Gauss constraints of Einstein-Yang-Mills theory and 2+1 gravity. Interestingly, while Yang-Mills theory in 4D is not yet rigorously defined as an ordinary (Wightman) quantum field theory on Minkowski space, in background independent quantum field theories such as Loop Quantum Gravity (LQG) this might become possible by working in a new, background independent representation."

Because of Urs' ungrounded assumption in his point #4, I must re-emphasize that not only has it not been established that a proper LQG dynamics is unattainable, but also Smolin does not say this. He says that at a certain point in history (in the 1990s) some researchers, including himself, redirected effort because they saw serious problems with constructing the Hamiltonian constraint in canonical LQG. So they began exploring other paths, like spin foams with their connection to BF theory and related areas, which have turned out to be interesting and are still being pursued.

Urs said:
5) If they can not reproduce the canonical LQG kinematics, what does this imply for the experimental predictions that it seems Lee Smolin wants to derive from these kinematics?
Posted by: Urs at January 31, 2005 01:11 PM

I believe the answer to Urs' question #5 is that it would not imply anything about Smolin's predictions. The latest word on what experimental predictions Lee Smolin has gone on record with is in this recent paper:
http://arxiv.org/hep-th/0501091
Falsifiable predictions from semiclassical quantum gravity
Lee Smolin
9 pages

"Predictions are derived for the upcoming AUGER and GLAST experiments from a semiclassical approximation to quantum gravity. It is argued that to first order in the Planck length the effect of quantum gravity is to make the low energy effective spacetime metric energy dependent. The diffeomorphism invariance of the semiclassical theory forbids the appearance of a preferred frame of reference, consequently the local symmetry of this energy-dependent effective metric is a non-linear realization of the Lorentz transformations, which renders the Planck energy observer independent. This gives a form of deformed or doubly special relativity (DSR), previously explored with Magueijo, called the rainbow metric. The argument is general, and applies in all dimensions with and without supersymmetry, and is, at least to leading order, universal for all matter couplings. The argument is, **illustrated in detail in a specific example in loop quantum gravity.**
A consequence of DSR realized with an energy dependent effective metric is a helicity independent energy dependence in the speed of light to first order in the Planck length. However, thresholds for Tev photons and GZK protons are unchanged from special relativistic predictions. These predictions of quantum gravity are falsifiable by the upcoming AUGER and GLAST experiments."

Urs gives the impression that he thinks Smolin's predictions concerning the up-coming experiments are derived from details of LQG kinematics. But this is not the case, although they can be *illustrated* as Smolin says by considering the example of LQG.

LQG is falsifiable, Smolin argues, and risks being refuted by GLAST. And the prediction is not based on particular LQG detail but is in a sense "generic". It applies to any of a broad class of approaches to Quantum Gravity which share the feature that they require an observer-independent energy scale but at the same time do not allow a preferred frame (they deform but do not break Lorentz invariance).

It will be interesting to see if GLAST falsifies LQG. This appears to me to be a valid prediction from the theory and a potentially solid experimental result. This has nothing to do with whether or not Spin Foams can or can not reproduce details of LQG kinematics (which was Urs question).

7. **Quantoken**
February 1, 2005

I think my message below is not off topic. But to not waste too much bandwidth on Peter's blog, please see the complete message at:
http://quantoken.blogspot.com/2005/02/blackhole-entropy-lqg-and-super-string.html

Both the super string camp and the LQG camp claimed their derivations of the Bekenstein-Hawking black hole entropy as their biggest success of their theories. In my judgement, claiming the derivation of Bekenstein Hawking entropy, such a trivial feat, as their biggest success, is completely "childish" and only shows the lack of "innate" ability on the part of each camp to comprehend what is the REAL physics behind the blackhole entropy!

I am going to show one very trivial derivation of the black hole entropy and how it is proportional to the event horizen surface area divided by Planck area. One that is different from Hawking's but much simpler.

But first, one has to realize two things:
1.Hawking entropy is not an empirical experimental evidence, but merely the result of a gedanken "experiment", e.g., mind exercise.
2. The entropy is a DIMENTIONLESS physical quantity.

See the complete message at
http://quantoken.blogspot.com/2005/02/blackhole-entropy-lqg-and-super-string.html

8. **Urs**
February 1, 2005

> it seems to me that it will not take long before LQG is "unified" into the stringy framework.

I don't think that the appearance of BF theory in 'topological M-theory' supports such an expectation.

Everybody interested in this question should pick up the recent paper

L. Freidel & A. Starodubtsev: Quantum gravity in terms of topological observables (2005) .

The crucial idea is expressed by formula (30).

It goes as follows:

Suppose we want to quantize some theory whose action can be written as a topological term plus a non-topological term.

Write down the generating functional for the action consisting of the topological terms alone.
Next consider the exponential of the non-topological part of the action as an observable. The expectation value of that observable can be computed by taking (infinitely many) functional derivates of the generating functional of the topological theory.

Since the topological theory is likely to be solvable exactly, this reduces the task of quantizing the full theory to that of computing that (highly nontrivial) expectation value of an exactly solvable theory.

Freidel and Starodubtsev demonstrate how this rewriting can be carried out for Einstein gravity in four dimensions with BF theory as the topological part.

The paper disucsses various path integral computations. It does not use any LQG techniques, though. At the end it says:

> This suggests that the techniques of loop quantum gravity and spin foam model are adapted to describe our perturbative expansion and lead to a finite result.

The authors want to study that in a followup:

> Our next paper is devoted to study in more details the perturbation theory in the context of spin foam.

So the relation of all this to LQG and spin foams is hypothetical at this point. Even if it can be made I don't see how the appearance of BF theory in topological strings has any bearing on it.

After all, at least in this paper, BF theory serves the purpose of a calculational trick in a way. The message is that some very compliocated expectation values in some theories are equal to partition functions of other theories. The hope is that computing some expectation value in some auxiliary topological theory reproduces the partition function of some other theory T. Right now I cannot see how the appearance of the auxiliary field theory in any context allows to make a connection to that theory T.

If the connection top-M-theory -> BF-theory -> LQG were meaningful, it would imply that topological M-theory is about gravity. But it is instead ordinary M-theory, which is.

But as I have said above, apart from all these considerations there is as yet no demonstration that spin foams and/or LQG are helpful in performing the calculation described by Freidel and Starodubtsev. To me, their discussion rather suggests that instead ordinary perturbative path integral quantization of gravity might maybe make sense if we were able to find a suitable reformulation of the EH Lagrangian.

9. **O**
February 1, 2005

From what I've learnt so far (from Vafa's talk, and comments by various people), and given past experience, it seems to me that it will not take long before LQG is "unified" into the stringy framework.

Whether it will be part of string theory proper, or only a "variation" on a stringy theme, is beside the point. I just don't think that the two theories will remain "two different approaches", the way that they are now, for too long.

Incidentally, it was Lee Smolin who predicted that this will be the case, many years ago, in an article in New Scientist.

10.
February 1, 2005

Interestingly enough Lee Smolin is giving a colloquim at
Lubos Motls 's university but in a different department:-)
later this semester.
http://cfa-www.harvard.edu/colloquia/latest.html

11. **O**
January 31, 2005

Vafa: "…The dimension 4 makes full contact with another approach to try to quantize gravity, in the context of Loop Quantum Gravity, which has also the same flavor of replacing a metric degree of freedom with, in that case, not just a form but a gauge field…"

Lubos, Any comments on Vafa's statement?

PS Ludwig, Thank you very much for that. The audio is quite unclear on my machine.

12. **Ludwig**
January 31, 2005

O said:
I listened to Vafa's talk at the topo strings meeting in Toronoto, a few weeks ago. The audio is not very clear, but my recollection is that he mentions LQG as an example of "a form theory of gravity" in 3+1 dimension, and therefore related to topo string theory…

O piqued my curioslity so I listened to the audio just now. Vafa discusses LQG briefly around minute 6 and again in more detail at minutes 16-18 into the talk. On my speakers the audio was quite clear. I transcribed what he said starting around minute 5:35

Vafa: "…The dimension 4 makes full contact with another approach to try to quantize gravity, in the context of Loop Quantum Gravity, which has also the same flavor of replacing a metric degree of freedom
with, in that case, not just a form but a gauge field…"

It was at this point you can hear Vafa interrupted by someone asking if he had really said LQG and he confirms yes he said LQG.

At minute 15:57, or about 16, he returns to a discussion of LQG but does not mention that he is talking about LQG until minute 18, where he says "…this is one of the starting points of Loop Quantum Gravity…"
What he is doing in minutes 16-18 is outlining the Ashtekar "new variables" formulation of General Relativity. His discussion thereafter is enlightening and makes me wish the slides were also available as well as the audio.

13.
January 31, 2005

higgs boson said:

"At least Brian Greene seems open to the idea that string theory could be wrong and if it does end up being wrong then perhaps he would be able to steer the other string theorists to better theories? If he has no sway with the physics community at large then you'll just have to wait until they all die off from old age."

In other words particle physics has become like a communist state: we must wait for the leaders to die out before we can hope for progressive change! Physics will have to spend 50 or so years behind an iron curtain, with L. Motl manning the machine gun nest shooting at anyone to tries to escape.

14. **Ludwig**
January 31, 2005

Here is Vafa's talk at this month's Topological String workshop in Toronto
http://www.fields.utoronto.ca/audio/04-05/topstrings/vafa/

Here is a recent LQG/BF paper
"Quantum gravity in terms of topological observables"
by Freidel and Starodubtsev
http://arxiv.org/hep-th/0501191

O said:
I listened to Vafa's talk at the topo strings meeting in Toronoto, a few weeks ago. The audio is not very clear, but my recollection is that he mentions LQG as an example of "a form theory of gravity" in 3+1 dimension, and therefore related to topo string theory.

Urs said:
I think Vafa is talking about BF theory. This is related to LQG, but it is not the same as LQG. I am not sure if it is helpful to call this the 'topological sector of LQG' as Vafa does…
Posted by: Urs at January 31, 2005 12:40 PM

There is also another LQG/BF paper by Smolin and Starodubtsev that may be of interest:
"General Relativity with a Topological Phase: An Action Principle"
http://arxiv.org/hep-th/0311163

15. **Urs**
January 31, 2005

So from reading Smolin's reply I get the impression that he is saying that

– there is little hope for the canonical quantization of the Einstein-Hilbert action

– one should instead study spin foams and see if one can come up with any amplitudes for these

– if so, one should check if the resulting spin foam theory has a limit in which it reproduces any known theory .

– The canonical approach is only good for studying kinematics, not dynamics.

– In conclusion, as Smolin writes:

> The main physical hypothesis of LQG is not that the quantum Einstein equations describe nature. It is that the hilbert space of diffeo classes of spin nets, […] is the correct arena for quantum gravitational physics.

So LQG = 'use diffeo classes of spin networks somehow' ??

Some comments:

1) The ordinary states of 2-dimensional conformal gravity coupled to matter are not diffeomorphism invariant and hence can hardly be expressed by diffeo classes of spin networks.

2) It is not known if spin foams reproduce any known theory and in particular not if any of them reproduces the kinematical results of the canonical LQG approach. So from the spin foam point of view what reason is there to keep the kinematical results of the canonical LQG approach?

3) What is a physical theory which has a kinematics but not a dynamics? Isn't kinematics pretty much just my choice of symbols that I am going to use for defining dynamics?

4) What if spin foams *could* reproduce the kinematics of the canonical LQG approach. Would that imply that spin foams have no dynamics, either?

5) If they can not reproduce the canonical LQG kinematics, what does this imply for the experimental predictions that it seems Lee Smolin wants to derive from these kinematics.

6) If one says that 'spin networks should play a role' shouldn't one go all the way and say that 'generalized Wilson lines should play a role'. (Which in particular generalizes the allowed groups from rotation groups to arbitrary Lie groups and their representations.)

7) If yes, then there is IKKT theory. 😉

16. **higgs boson**
January 31, 2005

"I think the reason Lubos (and other string theorists) is rabid on the topic of LQG is that it threatens the only remaining argument for string theory."

Perhaps they are afraid of what will happen if string theory is not valid. Think about it. They will have to drop string theory and spend a lot of time and energy to get up to speed on loop quantum gravity or they would have to think about creating some other theory. I would imagine that it would not be a pleasant prospect for your colleagues.

At least Brian Greene seems open to the idea that string theory could be wrong and if it does end up being wrong then perhaps he would be able to steer the other string theorists to better theories? If he has no sway with the physics community at large then you'll just have to wait until they all die off from old age.

17. **Urs**
January 31, 2005

I think Vafa is talking about BF theory. This is related to LQG, but it is not the same as LQG. I am not sure if it is helpful to call this the 'topological sector of LQG' as Vafa does.

See also the comment on that point made by Nicolai et al. in their recent paper.

18. **O**
January 31, 2005

I listened to Vafa's talk at the topo strings meeting in Toronoto, a few weeks ago. The audio is not very clear, but my recollection is that he mentions LQG as an example of "a form theory of gravity" in 3+1 dimension, and therefore related to topo string theory.

I think that someone in the audience was surprised by Vafa's statement and asked him to repeat it, and Vafa did, saying somthing along the lines of "in that case, there is not just a form, but the form is a gauge field".

Would anyone care to verify that my understanding is correct?

19. **Ludwig**
January 30, 2005

Blank said:Can we ignore the foam at the mouth and the shrill rantings and merely evaluate the merits and demerits of the arguments, please?

I agree it would be interesting to look at what Nicolai et al said in their article and how Smolin replied. I thought the tone was basically friendly and cool-headed and the points were substantive.

Nicolai et al pointed to difficulties with LQG dynamics, when attempted along lines of a canonical quantization of GR, and particularly with Thiemann's hamiltonian constraint (1997?).

Smolin replied that these difficulties were seen in the mid-1990s and that Nicolai and the others seem to have overlooked most of what has been happening in the past ten years in LQG.

Smolin could have cited many papers from the mid-to-late 90s backing up what he said: Loop people finding difficulties with the hamiltonian approach (which Nicolai was stressing) and beginning alternative lines of development. In fact Smolin only cited one paper, he 1996 gr-qc/9609034. I dont know if that was the most representative or the most obviously germane–he might have mentioned papers of about that time or a couple of years later by Lewandowski, Marolf, Gambini and others. I dont remember offhand but could get links if you wish them.

I thought Nicolai et al critique of Loop thoughtful and constructive but merely too narrow. It would be much appreciated, I think, if we could hear similar comment on what Loop people have been working on (for, I would say conservatively, the past 5 years, not as Smolin does, 10). These are approaches like Thiemann's master constraint programme, Gambini-Pullin discrete quantum gravity (which has an evolution operator, instead of a contraint), spin foams (with their connection to BF theory). To substantiate what I'm talking about I will try to fetch some links when I have time later.

The main thing that strikes me in the Nicolai-Smolin exchange is the absence of heat. Both Nicolai and Smolin work at institutes where research is balanced (both string and loop research is done at AEI-Potsdam and at Perimeter-Waterloo) and neither of them come across as threatened by the other's discipline. I was impressed by Nicolai's lively appreciation of the LQG research that he was familiar with and the overall friendly tone. Would like to see more like that and less of Lubos stink-bombs.

20. **Chris Oakley**
January 30, 2005

Only if you identify yourself … sorry to point out the obvious, but there is as yet no experimental evidence to suggest that we even *need* a quantum theory of gravity.

21.
January 30, 2005

Can we ignore the foam at the mouth and the shrill rantings and merely evaluate the merits and demerits of the arguments, please?

22. **Ludwig**
January 30, 2005

here's the Weinberg talk
http://online.itp.ucsb.edu/online/kitp25/weinberg/
and also Peter's October 8 blog commented on the talk and its
"only hope of extending SM to include gravity" or words to that effect utterance.
http://www.math.columbia.edu/~woit/blog/archives/000089.html
so the Only Hope notion goes beyond the rabidity and schadenfreude of a few and is at the foundation of string apologetics

23. **Ludwig**
January 30, 2005

Peter said:… string theorists) is rabid on the topic of LQG is that it threatens the only remaining argument for string theory. With the "Landscape", all hope is gone for ever saying anything at all about particle physics, so all that is left is to keep repeating loudly "only string theory can quantize gravity"…

I am hearing this as a kind of party line: string is "our one best hope", or sometimes "our only hope".

It has to be repeated a lot and defended zealously because appearances are rather to the contrary, I'd say, and it is the main premise justifying continued focus of research effort on string.

Steven Weinberg delivered the "one best hope" line in his KITP 25th anniversary talk last year. I'll get a link if anyone wants.
Still it's a thought that MTW fell on Lubos head when he was a baby.

24. **Peter**
January 30, 2005

I think the reason Lubos (and other string theorists) is rabid on the topic of LQG is that it threatens the only remaining argument for string theory. With the "Landscape", all hope is gone for ever saying anything at all about particle physics, so all that is left is to keep repeating loudly "only string theory can quantize gravity". If the idea gets around that LQG is a more promising idea about quantum gravity than string theory, there won't be any argument at all left for research on string theory as a TOE.

25. **serenus Z**
January 30, 2005

I must confess that I am totally fascinated by Lubos M's hysterical attitude to LQG. It obviously goes way beyond anything involving physics. Most people I know don't have much time for LQG, but I don't know anyone who gets even remotely as rabid as LM. In fact, it's pretty clear that LM has an intense fear of anything connected with general relativity; hence the various bizarre misunderstandings of the subject he has revealed over the years. What gives? Did a copy of Misner Thorne and Wheeler fall on his head when he was a baby? Or what?

26. **Wolfgang**
January 29, 2005

Caracciolo and Pelissetto investigated SO(5) on a 4D
lattice as a quantum gravity model back in the 80s.
The model goes back to a proposal by L. Smolin. I
wonder if the two lines of research are (or can be)
related ? It would be cute to dig out the old
results or redo the old simulations …

There was a great comment on Lubos' weblog:
Q: Why is the debate (between LQG and strings) so heated ?
A: The stakes are so low !

27. **Ludwig**
January 29, 2005

Peter, thanks for replying. I understand your point better now. I'm struggling to get a preliminary notion of BF theory and how they apply it. there seems to be a 4-manifold M and a principal SO(5) bundle P. I could be way wrong about this but I imagine it as a copy of SO(5) at every point of M, but each group is only a "torsor" or has lost track of its identity, so it is just something at each point of M that the real McCoy SO(5) can act on in a nice way. Sorry, have to go to lunch, back later.

28. **Peter**
January 29, 2005

Maybe I shouldn't have called it "breaking topological invariance", it was the breaking of SO(5) symmetry by hand that I was wondering about, which is more of a gauge symmetry (although since it is not an internal symmetry, but a symmetry involving local translations, I'm not sure whether "gauge symmetry" is the right way to refer to it either).

I haven't read this paper carefully yet, but often the subtlety with TQFTs formulated this way is that you have to somehow break topological symmetry to make sense of them, but then everything depends on how you do this. It seems to me that the whole game here is to start with a topologically invariant QFT, and somehow end up with a valid approximation to it that looks like perturbation theory about flat spacetime with the scalar curvature as low energy effective action. For such an argument to be convincing, I'd have to be sure that the final result one wants hasn't somehow been subtly smuggled into the derivation.

29. **Ludwig**
January 29, 2005

I agree with Wolfgang where he says
"I find it very interesting what Freidel and Starodubtsev have to say about perturbation theory."

It seems they have found a way to do background independent perturbative analysis, and they suggest that the non-renormalizablity and failure of perturbation in the past was due to going about it wrong: with a fixed background.

It does seem to be a very interesting paper and I hope we hear more comment.

I did not yet find any place where they broke topological invariance, either by hand or any other way.

It looks to me that they broke gauge symmetry by hand, from SO(5) down to SO(4)

but as to topological invariance per se they say in the abstract "We show that the partition function of quantum General Relativity can be expressed as an expectation value of a certain topologically invariant observable."

What am I missing?

30. **Wolfgang**
January 29, 2005

I find it very interesting what Freidel and Starodubtsev have to say about perturbation theory.

String Theory Article Slanted

January 31, 2005
Categories: Uncategorized

Ken Lane has written a letter to the editor of the Boston University student newspaper to complain about its article about string theory and the BU physics department discussed in a previous posting. Lane is annoyed about not having been given a chance to respond to Vafa's ad hominen attacks characterizing him and the BU physics department as "foolish" and "childish". He also complains that the author didn't seek other opinions about Vafa's claim that string theory is what the "youngest, most brilliant physicists" are all doing.

I don't remember whether they had shop classes at Harvard, but if they do now, maybe they should be talking to Vafa. According to the blurb for a recent talk by Jim Gates at Brookhaven, string/M-theory is "a 21st century lathe ? a machine capable of remarkable precision and versatility, but requiring a skilled and experienced operator for its success." Funny, back in the last millennium I remember when string theorists were claiming that string theory was a 21st century "supercomputer" or "spaceship" that had fallen into the 20th century.

Comments

1. **sobul ltom**
   February 3, 2005

   It's amazing the sort of nonsense that Lubos Motl is able to transmute into a "story". Ed Witten posted a paper on the internet. Wow. That's impressive, especially if tens of billions of things are posted on the internet every day.

2. **Lubos Motl**
   February 3, 2005

   It's amazing what sort of nonsense Peter Woit is able to transmute into a "story". Kenneth Lane wrote a mail to a Boston University student newspapers. Wow. That's impressive, especially if tens of billion e-mails are sent every day.

   The student newspaper probably decided that Cumrun Vafa's opinion is more important than Kenneth Lane's opinion – and Vafa is the person who should have the final word, at least in some questions. I am not surprised. If Kenneth Lane – with all of my respect to his countless contributions to particle physics – finds it insulting, it's his personal problem.

   Best
   Lubos

3. **Matti Pitkanen**
   February 1, 2005

   "Boring" is my spontaneous reaction while reading the formulations of string models containing the bare essentials. Not a single basic principle is identified, just action and functional integral measure are identified, and Feynman rules derived. Principles are replaced with poorly defined ad hoc notions like spontaneous compactification and branes fabulated in hope of bringing in the real physics.

   Real progress starts from a real problem. State function preparation and state function reduction are very poorly defined concepts. Even better, there is also a paradox. The non-determinism of quantum jump is not consistent with the determinism of Schroedinger equation if the standard identification of geometric time with experienced time is accepted. It is hard to imagine a better gift for an ambitious theoretician. To solve these problems physicist must become part of physics, ceasing to be an outsider inducing state function reductions. One must not only define physically the notion of self, but also consider seriously the physical correlates of cognition, intentionality, sensory qualia, emotions. String theorists have not noticed that everything includes also consciousness.

   Matti Pitkanen

4. **D R Lunsford**
   February 1, 2005

   What is the REAL question in quantum gravity?

   "What is the meaning of 'wave function of a cosmology'?"

   To have a wave function you need an experimental setup. To have an experimental setup you must prepare a system for observation. How is one going to prepare the entire Universe for observation? How is one going to prepare the Solar System for observation? How is one going to prepare a child's top for observation? How is one going to prepare a buckyball for observation? It's a ludicrous idea.

   I don't care how grandiose, or subtle, or both, the math issues are in LQG vs. ST. There is no physics question that needs to be answered, or will be answered, by either program. That is why they are both mired in minutiae and why the world has split into ridiculous "camps".

   -drl

5. **O**
   February 1, 2005

   I don't think "mathematical physics", and all of mathematics that's inspired by physics, particularly strings, is boring at all at this moment.

   Of course, if you insist on restricting your attention to one and only one subject, then, yes, things can be quite slow.

   In this sense, phenomenological high energy physics has been very boring for decades.

6. **D R Lunsford**
   February 1, 2005

   This strings vs. loops debate is monumentally boring.

Amazing. Indy's gone, Daytona's gone, movies suck, no one can write worth a damn, music is nothing but electronic log-beating, and physics is *boring*. What happened? Did we pass through the tail of Comet Ennui?

-drl

7. **Not a native speaker**
February 1, 2005

Why does Jim G say "lathe"? Why not just "tool"?

8.
February 1, 2005

Let us note that Lane's letter shows that the state of journalism appears to be as bad as the state of High Energy Physics.

9. **Levi**
February 1, 2005

Typical LM comment line:

anonymous said…

This post has been removed by a blog administrator.

Lumo said…

(insert insult here)

anonymous said…

This post has been removed by a blog administrator.

Etcetera. It's very edifying.

10. **Not a Nobel Laureate**
February 1, 2005

After reading Motl's blog and making a few comments there . . . and being mistaken for a Nobel Laureate for my meager efforts . . . I've concluded that Motl is to String Theory as a Political Commissar was to Marxist-Leninist Thought in Czechoslovakia after the Prague Spring.

The sociology of HEP theory these days is far more interesting and entertaining than any current metaphysics being promoted by the waring sects of Stringys and Loopys.

11.
January 31, 2005

God Save Peter Voit

12.
January 31, 2005

If LM is the "youngest, most brilliant physicist", we are in very bad shape….. God save Nature! 🙂

13.
January 31, 2005

Since everyone knows that Lubos Motl is the "youngest, most brilliant physicist", and he is working on string theory, I don't see how Lane can argue with Vafa's statement.

Manin Article

February 2, 2005
Categories: Uncategorized

An expository article by the the algebraic geometer Yuri Manin always has something interesting in it, and his latest, entitled [The notion of dimension in geometry and algebra] is no exception.

In this article Manin discusses various ideas related to the notion of dimension, ranging over fractal geometry, non-commutative geometry and theoretical physics. He begins with a quote from Glenn Gould, which is quite amusing, but of obscure relation to the notion of dimension. Then he goes on to some history, from Euclid to Leibniz, finally veering off into a fascinating discussion of the relation of algebra and geometry, and ending with the sociological comment that visual mass media is leading to a dominance of right-brain mental faculties, and thus "projects us directly into dangerously archaic states of collective consciousness."

The body of the article includes comments on Hausdorff dimension, dimensional regularization of path integrals, the theory of operator algebras, non-commutative geometry, a weird digression on databases, and supergeometry. He also discusses "Spec Z" (the "space" naturally associated to Z, the ring of integers) making various comments about it and giving arguments for its dimension being 1, 3 and infinity. Next there are some comments on modular forms, and finally a section on fractional dimensions in homological algebra.

Its not clear how seriously one should take all of this, but Manin's article is definitely thought-provoking.

Comments

1. **Yuri**
   February 3, 2005

   Mirror Symmetry of Yuri Manin is reminiscent of Occam's razor principle."Entities should not be multiplied unnecessarily."

2. **fooltomery**
   February 3, 2005

   Peter, don't be so literal!

   You write that Manin "begins with a quote from Glenn Gould, which is quite amusing, but of obscure relation to the notion of dimension."

   Your own quick summary of Manin's choice of topics shows that there's a whole lot of dimension-discussing (and other-stuff-discussing) going on in the article.

   Manin decided to begin his wandering journey by making a little self-deprecating joke.

3. **Thomas Larsson**
   February 3, 2005

   I once observed that many interesting fractals living in 2D have Haussdorf dimension of the form D = (100 – n^2)/48, for n integer between 2 and 10. Apart from obvious D = 2 and D = 0, this series includes the percolation cluster (D = 91/48), the percolation hull (D = 7/4), linear polymers (D = 4/3), and red links, which cut the percolation cluster in two (D = 3/4). The formula comes from conformal field theory; D = 2 – 2h, where h is in the discrete series in the formal c -> 0 limit.

   This was very much in the air in 1986, and I later learned that Hubert Saleur made the same observation a couple of months before me, but at least my letter was submitted when his appeared in print. And this observation did earn me a four-year postdoc, so it was important to me.

4. **D R Lunsford**
   February 2, 2005

   Glenn Gould was notoriously hard on himself. Beethoven would slam the piano lid down when someone expected him to play. The idea of "disgust" is built into some of Beethoven's late work – he said about his early work "why does it make such a bad impression on me? From this day forward I mean to strike out in a new direction." Gould may have been similar. Actually, he must surely have understood that his fans didn't care how he played.

   His first and last recordings were of the Goldberg variations. He was a hypochondriac who died of a stroke.

Future and Present Particle Accelerators

February 2, 2005
Categories: Uncategorized

John Ellis's weblog has a new entry on Future Particle Accelerators which discusses prospects for a linear collider. The plan for an "International Linear Collider", or ILC is now in its design phase, with work proceeding on a detailed design for a .5-1 Tev collider. No one has yet figured out where this would be sited or how it would be financed. The agencies responsible for this funding seem to have agreed to put off a decision about going ahead with the project until 2010.

By that time there should be a couple years of data available from the LHC, and if the Higgs particle or superpartners are found, it would be clear whether the ILC design would have enough energy to study them usefully. Also around that time is should be clear whether CERN's more ambitious design for a linear collider, called "CLIC" and perhaps capable of reaching 3-4 Tev, is really a feasible one. If the decision is made to build the ILC design, the hope would be to have construction finished in 2015 (although this sounds overly optimistic to me), allowing several years of joint running of the LHC and ILC. If no Higgs or superpartners are found, or their mass is too high, the decision would be made to concentrate on CLIC, with construction done at the earliest in 2021.

Back in the present, the Tevatron at Fermilab is now seriously back in business after a long shut-down, recently reaching record values of luminosity. To follow what is going on there, you can keep up with the weblogs of Tommaso Dorigo and Sandra Leone of the CDF collaboration, as well as Gordon Watts and Ursula Bassler of D0.

Comments

1. **Not a Nobel Laureate**
   February 7, 2005

   "Lubos said: "I just got a mail from Bill Gates. He WILL pay the Gates Supercollider (up to 20 billion USD), assuming that it will be a proton-antiproton machine and assuming that the director will be Melissa Franklin…."

   Are you saying it seriously or are you telling a joke? If you are not joking, then it really amazed me how could a young and brightest Harvard assistant professor be SO intelligently challenged, that you would actually believe it was an email from the real Bill Gates?"

   Well, Lubos did deduce, by the superior application of pure reason, that my post was from first Sheldon Glashow, then later revised to Paul Ginsarg (I don't even know who he is) leading to the predictable result – he's not even wrong.

   I'm reminded of Bronowski's story of how Hegel had shown, using pure reason, that there can logically exist only 7 planets. Shortly afterwards the minor planet Ceres was discovered followed the 8th and 9th planets.

   String theory and loop QG strike me as similar exercises – attempts to deduce how the universe work solely by the application of "pure" reason.

   Expect similar results.

2. February 3, 2005

   Bill is a great man. Windows stinks. Why is saying the second a cheapshot against Bill?

3. February 3, 2005

   Despite my cheap joke and my love-hate relationship with Windows over the years, and not wanting to get off topic, I do actually have the greatest respect for B Gates, what he has achieved, and how he uses his wealth.

4. **Dick Thompson**
   February 3, 2005

   Re Gates: See the latest issue of the Economist about the Gates foundation's donations to poor countries' health – vaccinations, malaria eradication and much more. See also Bill's techno attitudes toward how to go about that, which are treated very respectfully by the mag. Maybe time to stop with the cheap shots at Bill?

5. February 3, 2005

   "Gates would insists that all accelerator computers use Microsoft operating systems…"
   I can just visualise that now.
   "Yes…I think we really can analyse this scattering data for evidence of supersymmetric particle production…but first of all we need to download another patch…"

6. February 3, 2005

   Gates would insist all the accelerator computers use Microsoft operating systems, so I'd scratch that idea off the list if you actually want to get useful data from the machine. On the other hand, if you goal is just to skim off a little of that $20B, then go for it.

7. **Lubos Motl**
   February 3, 2005

   So far I am joking, of course, but we've considered various things along these lines.

8. **Quantoken**
   February 3, 2005

   Lubos said: "I just got a mail from Bill Gates. He WILL pay the Gates Supercollider (up to 20 billion USD), assuming that it will be a proton-antiproton machine and assuming that the director will be Melissa Franklin…."

   Are you saying it seriously or are you telling a joke? If you are not joking, then it really amazed me how could a young and brightest Harvard assistant professor be SO intelligently challenged, that you would actually believe it was an email from the real Bill Gates?

Could it not be that some one, like Quantoken, forged that email? Why do you have to post it here and entertain every one :-)?

Quantoken

9. **Lubo? Motl**
   February 2, 2005

   I just got a mail from Bill Gates. He WILL pay the Gates Supercollider (up to 20 billion USD), assuming that it will be a proton-antiproton machine and assuming that the director will be Melissa Franklin. Many people try to convince him to pay a Gates Linear Collider (GLC), but he rejected to pay for something that does not orbit around.

   Thanks, Bill!

10. February 2, 2005

    If letters are conserved I get
    MESON + SLIME BOLL + LOUT

11. **D R Lunsford**
    February 2, 2005

    Weyl was sure that his theory predicted 4D, because the Maxwell tensor squared was gauge invariant, in the original sense. In fact this actually doomed his theory from the outset. So Weyl was sort of doing “mathematical metaphysics”, which is fine.

12. **Fabio**
    February 2, 2005

    Personally, I’d like to collide Lubos Motl and Lee Smolin together at high velocity and see what comes out.

David Weise

February 4, 2005
Categories: Uncategorized

Slashdot today has something pointing to Larry Osterman's weblog where he tells the story of my ex-roommate David Weise's career, much of which has been spent at Microsoft. David is generally credited with almost single-handedly making Windows a viable product, when in 1988 he figured out how to get Windows to run on the 286 processor in "protected mode", something people thought couldn't be done. At the time Microsoft was planning on abandoning Windows and moving to IBM's OS/2, but David's work changed everything.

Osterman gets some things wrong. David, Chuck Whitmer and Nathan Myhrvold were fellow physics graduate students and my roommates at Princeton, not at MIT (for more about Nathan, see an earlier posting). David was in biophysics, Chuck was a student of Steve Adler's doing lattice gauge theory, and Nathan worked with Malcolm Perry on quantum gravity. It is true that David and Chuck were associated with the MIT blackjack team (this was the early eighties, just after casinos opened in Atlantic City, not the 70s as Osterman has it). There was a lot of practicing of card counting techniques and computer simulation of non-randomness in shuffles going on in our apartment during those days, although I never got really involved in it myself.

After getting their Ph.Ds, Nathan, Chuck and David (together with Nathan's brother Cameron) founded a software company called Dynamical Systems Research in Oakland, which they ended up selling (along with themselves) to Microsoft. They all ended up getting obscenely rich, with Chuck retiring quite a while ago, Nathan leaving more recently, and finally David is now leaving to work in molecular biology.

Comments

1. February 4, 2005

   The real issue is the "mode switch", which was nearly impossible on the 286. IBM had no 386 to shoot at when the idea for OS/2 was hatched, but they had ample time to change direction later, as "Windows 386" did. Once IBM got their plans right, they produced OS/2 2.0 and shortly after OS/2 2.11, which were fully preemptive OSes with a nearly perfect "DOS box", that is, a way of executing DOS programs in "virtual 386" mode. Windows NT was a solution to the very same problem, and a much inferior one until just recently. Windows 95 in contrast, and 98, 98SE, and ME, were still hybrids – cooperative tasking without full preemption. So Microsoft screwed the same pooch three times, and still won the game.

   The "DOS box" on early versions of Windows NT was, in contrast to that of OS/2, a total disaster. However, it didn't bite many people because by then, Microsoft had also captured the market for applications.

   You can still see the DOS box in action – fire up an old DOS program or 16bit Windows program under Windows XP, and look in the task list – you'll see "ntvdm", "NT Virtual DOS machine".

   -drl

2. **Larry Osterman**
   February 4, 2005

   Thanks D.R., I'll make the corrections… For some reason I'd assumed their PhDs came from MIT, don't ask why.

3. **D R Lunsford**
   February 4, 2005

   I thought the point was, that OS/2 ran on the 286 in protected mode, but the problem was getting into real mode again. Thus, OS/2 had a devil of a time running DOS programs. The 386 had a "virtual DOS mode" so it could go in and out of real mode at will. Windows was targeted at the 386 and the rest is history.

   -drl

Distler on Multi-loop Amplitudes

February 4, 2005
Categories: Uncategorized

Jacques Distler has a new posting about multi-loop string amplitudes. It's mainly devoted to the Berkovits superstring formalism, and explains in some detail the possible problems with this formalism that one might worry about. I'd alluded to some of these in the comment section of my posting about this last week, responding to commenters claiming that Berkovits had a proof of finiteness of multi-loop amplitudes. At the time, all I got in response was abuse about how ignorant I was. Presumably the same people will be either showering Jacques with abuse, or apologizing to me. Funny, for some reason Distler doesn't mention what I'd written about this. He also seems to have somehow neglected to put "Not Even Wrong" in his list of links to physics weblogs.

Comments

1. **Lubos Motl**
   February 4, 2005

   Dear Peter,

   I think that your comments are relatively far from those of Jacques, although – I hope that Jacques won't be terribly offended – they're not infinitely far. 😉

   Best
   Lubos

Douglas at City College

February 4, 2005
Categories: Uncategorized

Michael Douglas gave a colloquium at City College this afternoon, with the title “Are there testable predictions of string theory?” I went up there to the talk, figuring that I knew more or less what he would say, but he really surprised me. Douglas has given many talks over the last year or so about his program for trying to get predictions out of string theory by doing statistical analyses of string vacuum states. He has concentrated on what looks like the most promising case, trying to see whether vacua with low-energy supersymmetry breaking are favored over ones where supersymmetry is broken at much higher scales (e.g. the GUT or Planck scales). If he could make a prediction that the LHC will see supersymmetry, that would count as the first real prediction of string theory, and in 2008 or so we would see if it was right. I was expecting Douglas today to explain this whole program, report on what he had achieved so far, and offer hope that he and his collaborators would have a yes or no answer about supersymmetry sometime soon.

Instead he very much downplayed hopes for this kind of prediction, answering a question about it by Nair at the end of the talk by explaining some of the difficulties. Presumably he now agrees with recent claims by Dine that it is too difficult, even in principle, to decide whether or not the landscape predicts supersymmetry. Given this, in the conclusion of his talk, I was expecting him to answer the “Are there testable predictions” question in the negative. Instead, he did something very strange. He announced that string theory does make predictions, lots of them, adopting the Lubos Motl definition of a “prediction” of string theory as being anything consistent with string theory. Examples he gave included Polchinski’s cosmic string networks, where one could tell from the behavior of the network whether the strings were fundamental or not, and short distance modifications to GR. Of course these are not in any sense real predictions; all sorts of different modifications of GR at short distances are compatible with string theory, as are either no visible fundamental cosmic strings, or visible ones with a huge variety of possible different properties.

The weirdest part of his talk was when he explained what he considered the best prediction of string theory. This involved the negative prediction that the fine structure constant can’t have varied with time in the early universe, since effective field theory arguments would imply a corresponding variation in the vacuum energy, something inconsistent with observation. So his best prediction from string theory isn’t really a prediction of string theory at all, but actually a prediction of effective field theory. Furthermore this “prediction” is the purely negative one that something that hardly anyone expects to be true actually isn’t true.

In the question section, some obnoxious guy who has a weblog asked him whether it was really true that the best prediction string theory could come up with was the no variation of the fine structure constant one that was really an effective field theory prediction, and didn’t that mean there was no hope of string theory ever really predicting anything. For some reason this made him rather defensive, and he began by saying it depended on the meaning of the word “prediction”. After having it explained to him what most physicists consider a prediction to be, he launched into a sequence of analogies designed to explain why you can’t get real predictions out of string theory. They all were of the same genre: imagine some situation where you can only observe phenomena that are related in a very complicated and hard to calculate way to the underlying fundamental theory, and somebody tells you what the fundamental theory is. Shouldn’t you work on it and believe in it?

This argument makes it clear where the whole subject is going to end up. The standard scientific method of deciding whether a theory is true or not by figuring out its implications and comparing them to observations is no longer operative. In the case of string theory there’s a new method. You just believe because authorities tell you to, and from now on the activity of professional theorists will consist solely in the construction of elaborate scenarios designed to explain why you can’t ever predict anything. Feynman’s line that: “string theorists don’t make predictions, they make excuses” has been changed from a criticism into a new motto about how to do science.

Comments

1. **Amsterdammer**
   February 10, 2005

   Concerning the Feynman quote, accidentally I stumbled upon the following (from http://infoproc.blogspot.com/2005/01/string-theory-quotes.html)

   “…I do feel strongly that this is nonsense! …I think all this superstring stuff is crazy and is in the wrong direction. … I don’t like it that they’re not calculating anything. …why are the masses of the various particles such as quarks what they are? All these numbers … have no explanations in these string theories – absolutely none! … ”
   –Richard Feynman: in Davies and Brown, Superstrings, Cambridge 1988, pp. 194-195.

2. **D R Lunsford**
   February 7, 2005

   Chris –

   I was trying to point out that Minkowski space also has features that are not realized in naive experience, but we seem very happy with it and find it “useful”.

   -drl

3. **Chris Oakley**
   February 7, 2005

   Danny – I’m not following you. Mass is not dilatation invariant & therefore a universe with massive particles is not dilatation invariant & therefore not conformally invariant. Why is it more complicated than that?

4. **Chris Oakley**
   February 7, 2005

   Following on from Peter’s point, the twistor theorists I knew at Oxford: Lane Hughston, Paul Tod and William Shaw (a graduate student at the time) were always completely honest about what the framework could and could not do (“Don’t ask about massive particles, don’t ask about interacting particles” is what I seem to remember William saying to me once). Although they were called the “Mathematical Physics” group they were well aware of the fact that what they were doing was much more mathematics than physics. One thing, though where physicists could learn from them is the elegance with which they deal with the SL(2,C) covering group of the Lorentz group (which of course is one of the steps on the road to Twistors).

5. **Matti Pitkanen**
   February 7, 2005

   I think that the key question is “At what level the conformal invariance is realized?”.

   Assume the representatibility of physically realizable space-times as 4-surfaces of space M^4xS, S some compact space. Assume that 3-D lightlike boundaries of spacetime surface act as “causal determinants”. Causal determinants could correspond also to light-like surfaces representing “shock waves”.

From these assumptions you end up with the realization that these light-like 3-surfaces allow generalized conformal invariance by their metric 2-dimensionality. Hence conformal invariance and 4-dimensionality of space-time are very tightly related. This conformal invariance has nothing to do with the rather trivial conformal invariance (as compared to 2-D conformal invariance) of M^4.

Best,
Matti Pitkanen

6. **D R Lunsford**
February 7, 2005

Chris,

It’s somewhat more subtle than that – as an affine space Minkowski space allows dilations, and we should experience these along with boosts and rotations. Since we don’t, we “can’t” be in Minkowski space. So the argument goes both ways.

I think conformalism will eventually play a crucial role, but the existing approaches based on SO(4,2) will not.

-drl

7. **Peter**
February 7, 2005

In a word, no.

The idea of using twistors to do fundamental physics has mainly been pursued by people working with Penrose’s group at Oxford. It never became very popular outside of people associated with this group. The new work on twistor string theory is a big change, causing a lot more people to learn about twistors.

There’s been a steady interest in twistor techniques among mathematicians for quite a while. Some of this goes back to the late seventies, when Atiyah and others got interested in the fact that you could use holomorphic techniques on twistor space to get solutions to the YM self-duality equations. There’s been a lot of work by Ward and others on using twistors to solve various kinds of equations.

Claude leBrun has gotten a lot out of twistor techniques in his work on 4-manifolds, and there are lots of other examples of the idea being useful in mathematics.

8. **JC**
February 7, 2005

Peter, Chris

Were twistors ever hyped up in the past in a similar manner to string theory, except maybe at a smaller scale?

I vaguely remember a popular press book by Peat which discussed string theory and twistors in the 1980’s. Other than that, I don’t recall twistors being really excessively hyped up.

9. **Peter**
February 7, 2005

Hi Thomas,
There weren’t that many string theorists in the audience. Of those who were there, some of them have told me in the past they’re not at all happy with the present state of string theory. I had to leave fairly quickly after the talk, didn’t get a chance to talk to too many people afterwards. I can say there definitely were some people in the audience sympathetic to my complaints.

By the way, Nair isn’t really a string theorist, although he has done some work on string theory. He has worked on a lot of different things over the years. This includes YM amplitudes in twistor space, where 15 years ago, he was one of the first (if not the first, I don’t really know the history) to calculate them this way. Before the colloquium we talked about what is going on in twistor string theory, which he’s working on (and about “Not Even Wrong”, of which he is a reader).

10. **Chris Oakley**
February 7, 2005

I can very easily think of so many very serious topics in science that are not getting far as fast as they should mainly because not enough people are working on them. One example that comes to mind is twistors.

No way. Twistors are dead in the water. They rely on conformal invariance. Particles have mass & therefore the world is not conformally invariant. Twistors are therefore a blind alley.

Feynman evidently used similar reasoning to rule out superstrings. Superstrings require ten dimensions. The world is not ten-dimensional. Ergo, superstrings are wrong.

11. **Thomas Larsson**
February 7, 2005

Peter,
I’m curious about how the string theorists in the audience (like Nair) reacted to Douglas’ talk and your question. Were they upset about your irreverence? Do they think that the Landscape makes predictions? Do they care at all whether string theory disagrees with observation?

12.
February 6, 2005

While quote about pathological science rings true to my ears regarding string theory. There was a time when it appeared perilously close to being revolutionarily right. It is still in fashion in high circles and so will correspondingly incur opprobrium when it does fall.

Of the six Langmuir attributes of pathological science, 5. is the most recognizable re: string theory, namely,

5. Criticisms are met by ad hoc excuses.

The other points would need rewording for string theory, e.g.,

3. It makes claims of great accuracy &
4. It puts forth fantastic theories contrary to experience

13.
February 6, 2005

Regarding Feynman's remark, I would like to make a remark on the sociology of science: The amount of effort that's invested over so many years, by so many remarkable people, to keep string theory afloat is incredible.

I can very easily think of so many very serious topics in science that are not getting far as fast as they should mainly because not enough people are working on them. One example that comes to mind is twistors.

If it were not for Witten's involvement, strings would probably have remained in 10D with E8xE8 to this day. (Happily, Witten has revived interest in twistors recently.)

14. **Peter**
February 6, 2005

Hi Dave,
I answered that it one of the previous comments. It comes from Lawrence Krauss.

If you look at what Feynman did say about string theory in print, one thing he complains about is that the most straight-forward prediction was for d=10 and SO(32) or E8xE8 symmetry groups. He refers to the activity of string theorists invoking Calabi-Yau compactifications, etc. to get around these naive predictions as making excuses for a theory that gives the wrong answer.

And to the previous commenter with the quote on "pathological science". Do you want to defend string theory as "pathological science?" Or do you have some other off-topic pathological science you want to promote, in which case I should delete the comment.

15. **Dave BAcon**
February 6, 2005

Peter, where did you get that Feynman quote? Or is it a paraphrase? I've seen quite a few things by Feynman about string theory (and who knows, his personal "war" with Gell-Mann may as much to do with his opinion as anything else) but I'd never seen it worded quite this harshly.

16.
February 6, 2005

"Nothing is to be gained by castigating those who followed false paths in good faith and with the honest determination to add to human knowledge. ?Pathological science? is an epithet applied to *potentially revolutionary* discoveries that did not pan out. The passionate disdain implied by the phrase is not justified by the actions of those who have been so criticized. Rather, it may be an instance of *odium scholasticum*: the criticism is so furious not because the thing is so far removed from the acceptable, but because it comes so infuriatingly close to being remarkably right. "

{ From the conclusion of
http://www.hyle.org/journal/issues/8-1/bauer.htm }

17. **Peter**
February 6, 2005

About the Feynman quote: kind of funny, given the other discussion here, but the source is Lawrence Krauss. Two or three years ago the Museum of Natural History here in New York organized a public debate about string theory. On the pro side were Brian Greene and Jim Gates, on the negative side Krauss and Glashow, with Lisa Randall also on the panel. It was during this debate that Krauss quoted Feynman. You'd have to ask Krauss for his source, possibly he heard Feynman say this.

18. **O**
February 6, 2005

Peter,

"String theorists do not make predictions. They make excuses".

Do you recall where you read that?

19.
February 6, 2005

Hey,

This string 'theory' thing is turning pretty much as the 'intelligent-design' (ID) type of argument. I hope these guys adding ID to its curriculum didnt hear about what physics is ending up being (nor about Lubos please! 🙂)
It will be very difficult to argue with them if, as probably lumo would love to do, people go around saying strings should be taught in highschools as the theory of everything….."The last one turn off the light please" 🙂

Dont get me wrong, I find both fascinating human construction (and a beatiful one in the case of strings indeed), nothing to do with nature so far though :). We should be careful where do we put each of them, it might sound silly but don't forget who is supporting science and what's the main purpose of it.

By the way, Michael Dine is a great guy, I asked him recently about the landscape, SUSY and string theory, I asked "let's assume we dont find SUSY at the LHC, does that mean string 'theory' would be possibly wrong?' He answered that so far the evidence is not conclusive and he wouldnt be able to conclude that either case. That kind of honesty I find remarkable. I asked Ed Witten some time ago how it was possible string theory, originally developed in a fixed background, could describe fluctuations of the geometry. His answer was: "I'm afraid we still dont fully understand that", as one would expect of a real genius. By the way, did anybody see the word 'landscape' written in his papers? 🙂

I love math as much as l love nature, and I agree there is a deep and meaningful conection between the two we still 'dont fully understand', perhaps, as I have been recently discussing with some friends of mine, Math is not invented but discovered, and there is always a 'deep reason' for any one of its many branches.

We ended up asking the following questions: Could one predict the existence of mathematics? Is logic an emergent phenomena?
Do have these questions any meaning, or even answer? Are they even wrong? 🙂

Oh well, I never said I was doing science :p

Some of us are necessary I admit (I hope 🙂) but taking control of the community hasnt been, and it isnt, healthy in particular for young people like us. I still believe philosophy is what motivate us in the first place, the 'big questions', we shouldnt forget though to look for a REAL answer, or at least something we can confront with Nature somehow. It is a tough call in any case, specially 'standing up and taking a look around'……

best regards.

20.
February 6, 2005

OK, sorry to get sidetracked by referring to Krauss and the "fate question" (only as an example, should have left it nameless). I did not realize he is a hot item here, not everyone pays such close attention to who said what to whom.
Needless to say he is a good physicist and a great populizer etc etc.

The point I was making (playing a bit of the devil advocate) is that the main paradigm of particle physics research- finding a simple model and comparing the properties of its ground state to reality is a theoretical prejudice. In the context of cosmology the question of the true minimum is unphysical, and simplicity is there only to help with the analysis. Maybe the true model of the world is complicated, and it's properties that are relevant to reality are not that of the ground state. Think about protein folding- if you were presented with the relevant potential (especially if you were told it came from string theory) you would give up immediately trying to find any structure. Maybe quantifying the kind of structure which exists there and using it is doable and interesting, even if it is motivated by string theory. It is a matter of judgement if you think it is plausible, but it is certainly not outrageous to give it a try.

Sociologically, what I find funny is that a large majority of string theorists would agree with you completely on landscape issues. You will have to take issue mainly with some cosmologists and particle phenomenologists (no names
this time).

21. **Peter**
February 5, 2005

If you really didn't know that Krauss recently was quoted in the New York Times describing string theory as a "colossal failure", and is one of a very small number of people in the field who has been willing to publicly criticize string theory in recent years, then apologies for my tone.

But frankly I find this hard to believe. If you want to attack someone for being obssesed with something whose physical effects can't be measured, it's kind of strange you chose the one string theory skeptic around instead of the huge number of string theorists one could make the same criticism of. Maybe you know Krauss personally so know about his obsessions. But looking at his papers from the last few years, including the one you mention, I see no evidence of this obsession.

22.
February 5, 2005

For the first paragraph, agreed, so far it looks like there is no structure, though the methods for determining such structure are underdeveloped (and possibly interesting), and it is hard to find structure without looking for one. The set of questions to do with such structures is independent of string theory, and in fact finds much more resistance among string theorists than among say cosmologists.

As for the second paragraph, look for example at astro-ph/9902189. Krauss and Turner actually proved there is no finite set of measurments that will determine the "fate question", which would make that question uninteresting, I think you would agree. I was not aware that Krauss challenges string theorists, but I'm sure you know more about that than myself.

The tone has turned unpleasant, some would say "abusive", so I guess I am no longer invited. Have fun talking to your regulars.

23. **Peter**
February 5, 2005

The problem with assuming physics is determined by a metastable state of some complicated potential function is that the idea is completely vacuous unless you have some idea what this potential function is and its metastable states have some non-random properties. Current ideas about string theory produce such a wide variety of extremely complicated possible potential functions that there is zero hope of ever getting any predictions out of the idea (something that should have been obvious to Douglas, Dine and others from the beginning). In the case of the vacuum energy, the complete randomness of this property of the potential function is actually considered a virtue. You may think this idea is an "intellectual challenge", but it's one that has nothing to do with doing physics. If it weren't for the iron grip of the string ideology, no one serious would be paying much attention to this idea that obviously goes nowhere.

And if it weren't for the nasty way string ideologues deal with anyone who challenges them, you wouldn't be making stupid attacks here on Lawrence Krauss.

24.
February 5, 2005

The problem is not specific to string theory- suppose you have a theory complicated enough to have many meta-stable states. For example a few scalar fields with scalar potential function that has a few valleys and peaks. In a cosmological context the classic problem of finding the true minimum of that function becomes irrelevant- no doubt you will find yourself there eventually, but we are not interested in that- we may well be in process of getting there now, stuck in a false vacuum for a while. What happens eventually (at asymptotically large times) is an unmeasurable and uninteresting question (though some cosmologists, such as Lawrence Krauss, are obsessed with it).

Thinking how to do physics in this context is a new intellectual challenge, there is no reason really a theory should be "simple" in a superficial sense (just a few degrees of freedom) and it is not clear there is nothing at all that can be said in more "complicated" theories. I think Douglas' work is a good starting point, though not very satisfying as of now.

25.
February 5, 2005

On Aaron's comment, I must say that I have never understood how "more probable" amounts to "a prediction". Even if Douglas' program would show that low energy susy is more probable, and that doesn't turn out to be true, that wouldn't rule out string theory.

I think the only (kind) thing to say about string theory right now is that it has had it's many ups and downs in the past, and that right now it's in one of its down phases.

26.
February 5, 2005

What about the running of alpha with energy though? I don’t want to get off topic and will be very brief. Quantoken, at around 100Gev, alpha becomes alpha ~ 1/128 and not 1/137. This has been measured. Please respond or comment at your own blog though and not here.

27. **Quantoken**
February 5, 2005

The talk of the so called “running” physical constants, like an alpha that varies against time, is NONSENSE.

Tell me what “TIME” you are talking about when your alpha is “time”-dependent, since Einstein’s special relativity tells us time is all different from one reference frame to the next one, and from one location to the next. You would have to set up a Newtonian absolute spacetime, against the relativity principle, to be able to talk about “time-dependency” of alpha.

The fine structure constant alpha, being a dimentionless parameter, is a TRUELY Lorentzian invariant quantity. You could measure spectrum lines of star lights in a fast moving spaceship. Although each spectrum line is red-shifted or blue-shifted, when you calculate alpha the end result is the same as measured on earth.

A “running” alpha would also be against the strong equivalence principle, which says that physics laws do not depend on position, time, or reference frame. If alpha and other physics constants are “running”, we could well measure them and use it as a basis to establish a Newtonian absolute spacetime reference frame, against the insight Einstein’s relativity, and hence turn clock back and retro-progress the science back a couple hundred years!

Why would people even start to talk about “running” costants?

Quantoken

28. **Aaron**
February 4, 2005

As everyone I know is sick of hearing, I don’t think Douglas’s program has much hope of being predictive. Counting of vacua has no experimental consequences — as long a vacuum is consistent with current experimental results, it matters little how generic it is amongst other consistent vacua. We still could be in it.

So, while it is an interesting question to understand what vacua are out there (if only because exclusion is predictive), it’s still taxonomy and not prediction.

29. **Peter**
February 4, 2005

Douglas explicitly said that his argument was an effective field theory argument, not really a string theory argument. Sure you can write down effective field theories where changing alpha doesn’t change the vacuum energy, but if this is an effective field theory for some unknown short distance dynamics that determines alpha and the vacuum energy, you would generically expect changing alpha to change the vacuum energy. This doesn’t have anything to do with string theory specifically.

30. **Lubos Motl**
February 4, 2005

Dear Peter,

you’re wrong about all the points that are not completely fuzzy.

Concerning varying alpha: you can find hundreds of people who argue that it is perfectly OK to have a time-dependent alpha that changes independently of the cosmological constant and other parameters. In effective field theory, there is nothing wrong if you make a parameter time-dependent. In string theory, it’s not possible to vary these parameters separately because string theory does not allow any non-dynamical parameters. These things are powerful and nontrivial predictions.

Best
Lubos

31. **Peter**
February 4, 2005

Actually, I don’t know who posted it, except that their IP is in Russia.

But maybe that could be a motto string theorists could put over their office doors.

32. **Selrach**
February 4, 2005

Wow, I think such a comment as below
deserves to be booed!

Worse, we all know who posted it!

33.
February 4, 2005

If you want a prediction, go see an astrologer or palm reader.

FY 2006 Budget Request

February 7, 2005
Categories: Uncategorized

The FY 2006 budget requests to Congress are out today. In the parts relevant to funding for mathematics and physics, the information about the NSF request is here, and information about the DOE request is here.

One should really be a lot more expert on the details of government science funding than I am to be sure what these numbers mean, but here's my interpretation:

NSF: Funding request for mathematics is precisely flat at about $200 million. The only real change from last year is that $3 million is being moved from "Enhancing the Mathematical Sciences Workforce" (which funded things like the VIGRE grant our department used to have) to fund things like summer schools, workshops, conferences, etc. One strange thing is that mathematics is listed as one of four NSF priority areas, but still gets a cut in real dollars. Some of the other "priority areas" have very large cuts. I guess this means the NSF is changing its priorities.

The NSF physics request is up 2.3% to about $230 million. $13.5 million of this is for operations of the LHC detectors (CMS and ATLAS), $14.7 million for CESR and $32 million for LIGO. The part of the budget that includes research grants in high energy physics increased by $6.4 million to $152.4 million and theoretical physics is listed as a priority. Maybe string theorists will get more money. "POU", or Physics of the Universe, is listed as the highest priority, with emphasis on the question of "What about that dark matter and dark energy?".

DOE: The high energy physics budget request contains a large cut, going from $735.4 million in FY 2005 to $713.9 million in FY 2006. Highest priorities are listed as the Tevatron and NuMI at Fermilab and the B-factory as SLAC, but overall experimental HEP funding is down. Theoretical physics funding gets a small increase, from $49.0 to $49.1 million, so at least the string theorists will be all right, even if the experiments aren't.

More details on all of this should be available at the HEPAP meeting next week.

Of course this is just the request to Congress. Something very different may emerge later this year from the Congressional committees.

Update: A document with just the HEP part of the DOE budget is here.

Comments

1. **D R Lunsford**
   February 9, 2005

   This is o.t. but must be referenced:

   Feynman videos, the "QED" lectures!

   http://www.vega.org.uk/series/lectures/feynman/

   Real physics.

   -drl

Weil's Letter From Prison

February 10, 2005
Categories: Uncategorized

The great French mathematician André Weil spent the months of February-May 1940 in a prison in Rouen, as a result of what he referred to as "a disagreement with the French authorities on the subject of my military obligations". Others might have called this "draft evasion", and the story has something to do with why one of the most famous French mathematicians spent his post-war career not in France, but in Chicago and Princeton.

Weil's sister was Simone Weil, who could variously be described as a moral, political and religious philosopher, an activist and mystic. She died in 1943 in England, from some combination of tuberculosis and starving herself out of sympathy with her compatriots in occupied France. During her brother's prison stay they exchanged letters which were later published. One of these letters is a remarkable mathematical document that André Weil wrote to his sister, although she would have had little chance of understanding what he was talking about. It is reproduced in his collected works, and an English translation has just appeared in the latest Notices of the AMS.

The focus of Weil's letter is the analogy between number fields and the field of algebraic functions of a complex variable. He describes his ideas about studying this analogy using a third, intermediate subject, that of function fields over a finite field, which he thinks of as a "bridge" or "Rosetta stone". For function fields over a finite field, the analogies with number fields are quite close and many facts one knows about one subject can be used to make conjectures about what is true for the other. Some examples include the Riemann-Roch theorem and the Riemann hypothesis. After getting out of prison and leaving for the U.S., in 1941 Weil was able to prove the Riemann hypothesis for the function field case; of course for the number field case it remains an open problem.

For much more detail about this analogy, there's an interesting textbook by Dino Lorenzini called An Invitation to Arithmetic Geometry.

Comments

1. **Eleggua**
   February 19, 2005

   I think that the criticism of Bourbaki is over the top. If you are interested in philosophy, in particular of the analytical type, then actually Bourbaki group helped developed some very interesting mathematics which lead to these developments:

   Cleared up the notion of 'proof' for a mathematician (i.e there are an 'infinity' of levels at which mathematics can be done : – Category theory is just one)

   Created the machinery that allowed 2nd order logic to be formalised. This lead to P Cohen proof of the undecidablity of continuum hypothesis.

   Also to the proof that Mathematics is sound, of course you have to belive in ordinals greater than the continuum.

   Finally and not least, A A Markov 1958 proof of the impossiblity of solving the homeomorphy problem for manifolds, dimension 4 and above. A result I believe to be the equivalent of the impossiblity of solving 5 deg poly by radicals. This result in my opinion is the REAL reason why everyone is doing category theory. I must point out that category theory has many enemies in mathematics, who feel that it is only useful for expositionary work, not actual creative stuff (except for Grothendieck)

   Essential the Bourbaki spirit now lives in combinatorial group theory and model theory. At the moment these subjects are at the fringe, but I believe that out of these 2 subject will come the mathematics for the 21 century like topology was for the 20th!

   an amateur Mathematican

2. **Peter**
   February 14, 2005

   Hi Danny,

   Bourbaki's heyday was the fifties, part of the same over-emphasis on abstraction that lead to the "new math" disaster. By the 70s, Bourbaki's influence had started to wane. One reason was that mathematicians had begun to lose interest in overly formalist approaches, another was that Grothendieck showed that very different foundations were needed (i.e. category theory vs. set theory). The influence of physics on mathematics also had an effect.

   These days, Bourbaki has little influence in math. Some of their books are actually pretty good, but mostly they are used as technical references, nobody tries to learn anything from them. For a long time now the group has stopped writing more.

   Mathematicians still generally do a terrible job of writing readable expository material, but mostly they are no longer doing overly formalistic things. But what they do is inherently different than what physicists do, largely because there is a strong culture of not allowing people to be imprecise, and insisting on absolute clarity of in arguments. Physicists would do well to learn something from this.

3. **D R Lunsford**
   February 14, 2005

   Peter,

   One of my best friends was educated in the heyday of "new math", at Columbia and MIT. In high school of course he was at the top of his class, and in a class by himself technically. He related the following story. He learned calculus from Lang, was doing functional analysis as a high school student etc. etc. Then, he had to teach a course at MIT to engineers. He suddenly realized he couldn't do a simple surface integral da capo. He stated to me, that he began to consider the entire axiomatic program embodied in Bourbaki to be a total sham, and sought to restructure his knowledge on intuitionist lines (Brouwer, Weyl). Needless to say this was a complete success and now this peson is a world-authority on the math and modeling of turbulence. This was in spite of, not because of, the French program.

   I think it can safely be argued that Bourbaki and its intellectual worldview of lofty abstractions and airy-fairy axiomatics has been a disaster for science, has attempted to rip math and physics apart from each other at the sternum, and has created the mental climate in which a ruse like string theory can develop in the first place.

   -drl

4. **Peter**
   February 13, 2005

Hi Rafael,

It's not so much that prominent French Mathematicians were killed in WWI, but that many of the best math students were killed. Two-thirds of the students at the Ecole Normale Superieure died, this is the place that produced most mathematics teachers and researchers.

For more about this and about Bourbaki, see

http://planetmath.org/encyclopedia/NicolasBourbaki.html

5. **Rafael**
February 13, 2005

Hi Peter,

Which French mathematicians were killed in WW I? Also was Bourbaki created to "collect" all known French mathematics?

Thanks,

6. **D R Lunsford**
February 11, 2005

The parallel story of A. Raabe is worth mentioning. He was arrested in Krakow and died at Auschwitz before he could band together with other physicists for the purpose of making political statements or abstraction committees.

He did very interesting work on relativistic rotators as a kid.

http://arxiv.org/PS_cache/hep-th/pdf/0303/0303099.pdf

7. **Peter**
February 11, 2005

No, he would have been too young (born in 1906). Weil started his career at a time when much of the generation just older than him had been wiped out by WWI. This had a lot to do with why he and others of his generation ended up banding together to form Bourbaki. Also probably had a lot to do with why he was evading the draft. He had seen one generation of French mathematicians killed in WWI and didn't want this to happen again, especially not to himself.

8. **Alejandro Rivero**
February 11, 2005

Now I think about, was Weil also in prison during WWI or was he too young?

9. **Peter**
February 10, 2005

Hi Daniel,

Oops, typo in html. Fixed now. Thanks for pointing this out!

Peter

10. **Daniel Doro Ferrante**
February 10, 2005

Hi Peter,

I think that your link to the "English translation" is missing, at least as of now (10FEb05 @ 14:33:00h) it seems void. (Not that i can't find the link otherwise… but, just a heads up. 😉

Cheers,

The Road to Reality

February 13, 2005
Categories: Book Reviews

Roger Penrose's new book The Road to Reality is being released in the U.S. in a week or so. I'd been intending to write something about the book ever since I got a copy of the British edition a couple months ago, but this is quite a daunting task. The book is nearly 1100 pages long and actually comes close to living up to its subtitle: "A Complete Guide to the Laws of the Universe". It certainly is the most wide-ranging book on theoretical physics that I can think of, offering not just a summary of a lot of material, but an in-depth treatment of many of the more sophisticated ideas of the subject.

Penrose's point of view is that of a relativist, so his treatment of geometry, general relativity and classical field equations is the deepest and most detailed part of the book. But he also discusses quantum theory extensively as well as the various attempts to quantize gravity. Compared to the general relativity parts, his treatment of particle physics and quantum field theory is rather sketchy, but quite original.

One of the unique aspects of the book is its extensive use of drawings to illustrate mathematical, geometrical and physical concepts. In this respect it is unparalleled by any other mathematically sophisticated text I've ever seen. One of Penrose's main fascinations is the crucial role that complex numbers play, both in quantization and in the geometry of spinors. He has always been motivated by the idea that complex structures provide an important link between these two subjects, one that is still poorly understood. I very much agree with him about this. Related to this issue, some of the topics covered in the book that aren't in any non-technical reference that I know of are his discussions of hyperfunctions and the Fourier transform, the geometry of spinors and twistors, and the use of complex structures in quantization and quantum field theory.

Penrose also carefully lays out areas in which his point of view differs from the general consensus of most theoretical physicists. An example is his emphasis on the importance for cosmology of understanding why the universe had such low entropy at the Big Bang. For more about this, see a posting by Sean Carroll.

A second area where Penrose is less than orthodox is his belief that quantum gravity somehow modifies quantum theory and resolves its measurement paradoxes. He explains an experimental set-up that could in principle test whether gravity plays a role in quantum state reduction, but he doesn't have a concrete proposal for how standard quantum mechanics is to be modified.

Finally, there's a remarkable chapter on supersymmetry, extra dimensions, and string theory. Penrose is very skeptical of the whole idea of introducing more that 4 space-time dimensions. One reason is that the beautiful spinor and twistor geometry that fascinates him is special to 4 dimensions. Another reason he gives is the classical instability of higher-dimensional space-times. Under a small perturbation, such space-times should collapse and form singularities. The difficulties in stabilizing extra dimensions are at the heart of the problems of string theory, with the only known way of doing it leading to the "Landscape" picture and ruining any ability to get predictions out of the theory.

Penrose is critical of the supposed calculation of black hole entropy from string theory, noting: " As appears to be usual with such string-theoretic proclamations, this conclusion is very considerably overblown." He has quite a few other very critical comments about string theory and the way in which research in the field has been pursued. As you might guess, I'm very much in agreement with his point of view and glad to see it in print. I'd be very curious to know whether recent ideas about strings in twistor space and Yang-Mills theory have changed his views much on the whole topic of string theory.

Update: A commenter pointed out that Science magazine has a review of Penrose's book by Frank Wilczek (subscription required). Wilczek is right that there isn't very much about particle physics in the book and Penrose gets something wrong about neutral K-meson mixing. Wilczek also says Penrose makes incorrect statements about electroweak symmetry breaking, but in a quick look at the book I couldn't find what he was objecting to. He seems to object strongly to the speculative later parts of the book, but I don't quite understand why. Penrose is up-front about what is speculation (e.g. relations between twistor theory and QM) and what is solid science, and Wilczek's comment that "at present twistor ideas appear more as the desire for a physical theory than the embodiment of one", could equally be applied to string theory, leaving one wondering why he doesn't write strongly critical reviews of books on that subject.

If you want to read Lubos Motl's comments on a book he hasn't read, they're here.

Comments

1. **Peter**
   March 7, 2005

   Hi Matti,

   I've been deleting comments that seem to me off topic, repetitive, and purely designed to promote the interests of the writer. This has nothing to do with censorship of unpopular scientific ideas, I'd do the same if someone tries to promote their mainstream work on string theory or anything else here this way.

   If I allow you to continually post long comments promoting TGD, I also end up with long, multiple comments from Quantoken promoting GUITAR, and others promoting their favorite ideas, together with many hostile comments from other people who are annoyed that the comment section is being taken over by this kind of thing.

   If you want to write in detail about TGD, please do it on your own weblog, not on mine. If something I've posted seems to you really relevant to TGD, it would be best if you write a short comment here with a link to a longer discussion on your own weblog. I won't delete such short comments and links, as long as there is not an excessive number of them, and they do have some kind of relation to the topics I'm posting about.

2. **Matti Pitkanen**
   March 6, 2005

   I am one of the quite of many physicists who have been labelled crackpots after the establishment of M theory hegemony. In particular, it has not been possible to post anything to Archive-Org.

   Therefore the The Road to Reality was of special significance to me since it gives a clear signal for Paul Ginsparg and those responsible for this scandalous black-listing and also contains a reference to p-adic TGD. It is difficult to imagine that physicists like Roger Penrose would refer to the work of a crackpot.

   Roger Penrose's book The Road to Reality comes in two editions:

   UK edition (ISBN: 0224044478, Publisher: Jonathan Cape, July 29, 2004)

   and

   USA edition (ISBN: 0679454438, Publisher: Knopf, February 22, 2005).

The two editions are NOT identical.

For example:

The UK edition on page 1050 says in part:
"… Bibliography There is one major breakthrough in 20th century physics that I have yet to touch upon, but which is nevertheless among the most important of all! This is the introduction of arXiv.org, an online repository where physicists … can publish preprints (or 'e-prints') of their work before (or even instead of!) submitting it to journals. …as a consequence the pace of research activity has accelerated to unheard of heights. … … In fact, Paul Ginsparg, who developed arXiv.org, recently won a MacArthur 'genius' fellowship for his innovation. …"

but

The USA edition on its corresponding page (also page 1050) says in part: "… Bibliography … modern technology and innovation have vastly improved the capabilities for disseminating and retrieving information on a global scale. Specifically, there is the introduction of arXiv.org, an online repository where physicists … can publish preprints (or 'e-prints') of their work before (or even instead of!) submitting it to journals. …as a consequence the pace of research activity has accelerated to an unprecedented (or, as some might consider, an alarming) degree. …".

However, the USA edition omits the laudatory reference to Paul Ginsparg that is found in the UK edition.

For another example:
The USA edition adds some additional references, including (at page 1077): "… Pitkanen, M. (1994). p-Adic description of Higgs mechanism I: p-Adic square root and p-adic light cone. [hep-th/9410058] …".

I hope that this would serve as some kind of a signal also to Peter Woit, who has been continually censoring out my messages. This just to help the raise the level of discussion from what it is now.

Matti Pitkanen

http://www.physics.helsinki.fi/~matpitka/

http://matpitka.blogspot.com/

3. **Tony Smith**
March 6, 2005

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"… Pitkanen, M. (1994). p-Adic description of Higgs mechanism I: p-Adic square root and p-adic light cone. [hep-th/9410058] …".
Note that Matti Pitkanen was in 1994 allowed to post papers on the e-print archives now known as arXiv(obviously including the paper referenced immediately above), but that since that time Matti Pitkanen has been blacklisted by arXiv and is now barred from posting his work there. His web page account of being blacklisted is at http://www.physics.helsinki.fi/~matpitka/blacklist.html

It seems to me that it is likely that the omission of praise of arXiv's Paul Ginsparg and the inclusion of a reference to the work of now-blacklisted physicist Matti Pitkanen are deliberate editorial decisions.
Also,
since the same phrase "… physicists … can publish preprints (or 'e-prints') of their work before (or even instead of!) submitting it to journals. …" appears in both editions, it seems to me that Roger Penrose favors the option of posting on arXiv without the delay (and sometimes page-charge expense) of journal publication with its refereeing system.
Therefore,
a question presented by these facts seems to me to be:

What events between UK publication on July 29, 2004 and USA publication on February 22, 2005 might have influenced Roger Penrose to make the above-described changes in the USA edition ?

There are two possibly relevant events in that time frame of which I am aware:
1 – The appearance around November 2004 of the ArchiveFreedom web site at http://www.physics.helsinki.fi/~matpitka/blacklist.html which web site documents some cases of arXiv blacklisting etc;
2 – According to a CERN web page at http://documents.cern.ch/EDS/current/access/action.php?doctypes=NCP "… CERN's Scientific Information Policy Board decided, at its meeting on the 8th October 2004, to close the EXT-series. …". Note that the CERN EXT-series had been used as a public repository for their work by some people (including me) who had been blacklisted by arXiv .

Maybe either or both of those two events influenced Roger Penrose in making the above-described changes in the USA edition.
If anyone has any other ideas as to why those changes were made, I would welcome being informed about them.

Tony Smith http://valdostamuseum.org/hamsmith/

4. **D R Lunsford**
February 20, 2005

I got the book on Saturday, it should be in every physicist's library. It reminds me of Klein's various "Vorlesungen" – lectures, literally "readings". One thing Peter may not have mentioned is the clever prologue, which I assume takes place in Atlantis 🙂 This book appeals on many levels, including the very "tominess" of it!

-drl

5. **Paul Valletta**
February 17, 2005

Anyone who has purchased the Penrose book, will no doubt be daunted at 'not only' its size (the road to reality is a big one!), but must be prepared to journey across domains that alter the readers perspective about certain physical laws, I quote form the book:The spacetime singularities laying at cores of blackholes are among the known(or presumed)objects in the universe about which the most profound mysteries remain–and which our present-day theories are powerless to describe. As we have seen&&34.5,7,8,particularly, there are other deeply mysterious issues about which we have very little comprehension. It is quite likely that the 21st century will reveal even more wonderful insights than those that we have been blessed with in the 20th. But for this to happen,we shall need powerful new ideas,which will take us in directions significantly different from those currently being pursued. Perhaps what we mainly need is some subtle change in perspective-something that we have all missed…

Now correct me if I am wrong, or if I am not even right?..but Penrose clearly leaves the doors and windows open for 'a breath of fresh air', a humble way to entice the reader, whatever her/his previous thoughts were, you cannot help but wonder and reason?

I think it is a shame that Wilzeck seems to be riding the "BIG-NOBEL-WAVE", as his current joint undertaking in gravitational anomilies clearly shows?..would he have stepped into this arena previous to his 'nobel-prize'?..I think not, quantum-waves have more energy, and can travel further 😉

Lubos would never read such a book, but if the Epilogue is anything to go by, then he would definatly not understand the experience of…

6.
February 17, 2005

Thomas , see for example the panel debate on extra
dimensions at the Kavli conference on
http://www.phys.cwru.edu/events/cerca_video_archive.php
See the session on gravity and in particular the
panel debate on extra demisions (in that panel
wilczek argues against extra dimensions.)
See also http://mitworld.mit.edu/video/204/
and in particular his answer to scott hughes question about extra demisions.
However wilczek does believe in supersymmetry.
see also page 5 of astro-ph/0401347

7. **Ludwig**
February 17, 2005

Thomas asked:
"Where did Wilczek say that he doesn't believe in extra-dimensions? …"
Posted by: Thomas Larsson at February 17, 2005 03:47 AM

I dont know what Wilczek thinks or what he has said about extra dimensions. But here is something that could help round out the picture. Wilczek is evidently interested in quantum gravity and has just posted this paper with Sean Robinson
http://arxiv.org/abs/gr-qc/0502074
A Relationship Between Hawking Radiation and Gravitational Anomalies

—exerpt from Wilczek/Robinson introduction—
Hawking radiation from black holes is one of the most striking effects that is known, or at least widely agreed, to arise from the combination of quantum mechanics and general relativity…
…The literature contains several derivations of Hawking radiation, each with strengths and weaknesses. …

…Derivations based on string theory have a logically consistent foundation, but they only apply to special solutions in unrealistic world models, and they do not explain the simplicity and generality of the results inferred from the other methods[4, 5]…
—endquote—

to draw the obvious conclusion, Wilczek seems willing to entertain reservations about current attempts to join quantum mechanics and general relativity and to go out on his own looking for new ones, as in the case of this paper.
Hope this helps, even though not directly responding to the question.

8. **Thomas Larsson**
February 17, 2005

Where did Wilczek say that he doesn't believe in extra-dimensions? I know that he has worked a lot on supersymmetry and axions, which seems about as speculative as twistors, but maybe less so than strings.

9.
February 17, 2005

Incidently Wilczek and Penrose are in the same boat
as regards their opinions on extra-dimensions
(in other words both belive that they cannot be present.)

10. **D R Lunsford**
February 16, 2005

Peter,

Yes, that is the whole point, and is addressed by Weyl's ansatz. Hence, to make progress, one needs a way to introduce the gauge group in the context of conformal weight, which is just what I am trying to do.

-drl

11. **Chris W.**
February 16, 2005

Peter,
Sorry for that; perhaps the time of day (1:17 AM) had something to do with the manic state I was in when I posted. The articles in question aren't as far off-topic as one might think, but I won't push my luck by attempting to explain this assertion now. Thanks for indulging me and leaving the comment in place, if only as an example of misbehavior.

12. **Peter**
February 16, 2005

Hi Stephen,

I'll try and think if I can sensibly say more about this, but of course the underlying problem is that I don't have a good idea about how to control the dynamics of the metric degrees of freedom. In the Standard model the Yang-Mills + Dirac actions beautifully determine the dynmamics of the connections and spinor fields (leaving only scalar fields problematic). The Einstein-Hilbert action doesn't do the same for the metric degrees of freedom so one needs a new idea. LQG? TQFT? R^2 actions, twistors???

13. **stephen**
February 16, 2005

You should be trying to figure out how to use symmetries to gain control of the space-time degrees of freedom*, not throwing out the gauge symmetry, creating a higher dimensional mess whose dynamics you don't understand, then hoping to recover gauge symmetry as an effective low energy phenomenon.*

Posted by Peter at February 15, 2005 01:52 PM

Maybe at a later time you will speak to this in more detail? This clarifies to me the essence of your resistance to other theoretical approaches and helps to point towards more information to be look at. This is good.

Thank you

14. **Peter**
February 16, 2005

Chris and others,

Please do resist the temptation to post here about unrelated topics. Once one of you starts this, others join in and this starts to become like sci.physics again. I've deleted some comments and will delete any more that aren't about Penrose's book.

15. **Chris W.**
February 16, 2005

Sorry for the shift of topic, but I couldn't resist mentioning some interesting new and recent papers:

Supersymmetry and the Lorentz Fine Tuning Problem

This work was in response to the following, posted last March and last updated on 10/30/04:

Lorentz invariance and quantum gravity: an additional fine-tuning problem?

In January, none other than Seth Lloyd weighed in with an initial proposal (submitted to *Science*) for an approach to quantum gravity, starting with general notions of quantum computation theory, and with strong correspondences to causal set theory (Sorkin, Dowker, and others):

The Computational Universe: Quantum gravity from quantum computation

Finally, I'll draw these threads together more tightly by citing the following paper by Dowker, Henson, and Sorkin:

Quantum Gravity Phenomenology, Lorentz Invariance and Discreteness

I'll suppress my enthusiasm (and some thoughts begging to be expressed) and stop here.

16. **Peter**
February 15, 2005

Hi Lubos,

Thanks for plugging my "educational low-dimensional blog", I like that. From the picture I see I was completely wrong about Lane's lack of enthusiasm for string theory.

Actually I'll be up at Harvard next month for the conference there. If you want I could pose in front of the NYT article too, right next to the part where Lawrence Krauss makes his "colossal failure" comment…

17. **Lubos Motl**
February 15, 2005

Hi Peter!

I hope that your next article will also be celebrating 20 years of strings! See my blog – Kenneth Lane has already celebrated.

It's a matter of days before Shelly Glashow and others join! 🙂

Happy birthday 🙂
Lubos

18. **Peter**
February 15, 2005

Hi Z,

Anyone who wants to can look at what Penrose has to say about this stability issue, and then debate whether it makes sense or not. I'm just not interested enough in the question to spend time on this.

The problem with not fixing the moduli is that then your theory has massless scalar fields that couple to matter, producing long-range forces. We have very strong experimental bounds that these things don't exist. So such a theory is simply wrong.

I don't quite understand your last comment. If it's about the general landscape philosophy, which I see very good reasons to believe is inherently incapable of predicting anything, all someone has to do to prove me wrong is come up with a prediction. That hasn't happened yet.

19.
February 15, 2005

Peter Woit said:
"
Penrose's comments about higher dimensional theories were made in the context of a criticism of string theory, so I don't think it is unreasonable for me to discuss them in that context. If you have another context in which you want to discuss these issues, you'll have to make it explicit.
"

I think it was fairly obvious that the anonymous poster was talking about Penrose's claims on the classical stability of KK spacetimes encountered in the string theory literature. I think it is legitimite to dwell on what the precise objection here is. I don't think it is acceptable to show a tendency to sweep the issue under the rug if it turns out that this particular objection of Penrose's turns out not to be so well-founded, but put flashing banners if there is the slightest possibility that it might be a valid objection. This is not how scientists should work, though unfortunately similar tendencies prevail in both the string theory camp and the anti-string theory camp. (Penrose's objection *might* be well founded, I still don't understand what the precise objection here is.)

Peter also said:
"
If you don't like my commenting on string theory, just ignore it, or go somewhere else. I'm not going to stop. The situation with string theory is not, as you say, "almost the opposite of what you describe", it is precisely as I described it. The fact of the matter is that at the linearized level, you have a flat potential for moduli to deal with, and this leads to the disaster of predicting unobserved massless particles. Until recently, most proposals for getting a non-zero potential for the moduli lead to the disaster of moduli running off to infinity. Lately, the flux vacua proposals fix the moduli, but lead to the landscape disaster. Saying that "there is no known theoretical reason to fix the moduli" is absurd.
"

and concluded

"
The theoretical reason is that the theory is supposed to be a theory of the real world.
"

This sonuds to me like a strong claim about what a candidate "fundamental theory" (if such a thing exists) *must* be able to explain. (I am not saying that you believe in the existence of such a theory, but rather pointing out what you seem to be demanding from a candidate.)

Here is a fundamental objection of mine (which I think is a fairly obvious one) to the main theme in many of your posts againsts string theory (I am not a string theorist, by the way):

Little scientist bugs that live on a magnet might come up with a candidate microscopic theory of ferromagnetism. It might turn out that their theory cannot explain the mean magnetization that they so clearly observe. They might run around trying to invent schemes that would stabilize the "magnetisation modulus" thinking it is a fundamental thing that must be predicted by the "correct" microscopic theory. They might fail in doing so. Their friends might criticise them for working on a theory that has not shown the slightest possibility of coming up with the observed magnetisation. Despite all this, the bugs' theory might be correct.

Why not entertain a similar possibility for the issues about the "observed" gauge groups, etc.? Do you really think such a point of view would render a theory based on the latter totally unpredictive?
I don't think going after the quark masses etc. is the only way string theory can be tested.

z

20. **Peter**
February 15, 2005

To whoever you are who keeps posting hostile comments. First of all, unless you have a really good reason, I think you should put your name to your comments. At least pick a pseudonym, so I know when I'm having a back and forth exchange with the same person. At this point, the only way I can identify that the comments belong to you is that they generally contain some sort of criticism of me for publicly complaining about what is going on in string theory. When I want to be sure it is you I can go into the Movable Type software and check that the comment came from an IP in Ontario, but that is pretty tedious.

Penrose's comments about higher dimensional theories were made in the context of a criticism of string theory, so I don't think it is unreasonable for me to discuss them in that context. If you have another context in which you want to discuss these issues, you'll have to make it explicit.

If you don't like my commenting on string theory, just ignore it, or go somewhere else. I'm not going to stop. The situation with string theory is not, as you say, "almost the opposite of what you describe", it is precisely as I described it. The fact of the matter is that at the linearized level, you have a flat potential for moduli to deal with, and this leads to the disaster of predicting unobserved massless particles. Until recently, most proposals for getting a non-zero potential for the moduli lead to the disaster of moduli running off to infinity. Lately, the flux vacua proposals fix the moduli, but lead to the landscape disaster. Saying that "there is no known theoretical reason to fix the moduli" is absurd. The theoretical reason is that the theory is supposed to be a theory of the real world.

By the way, I notice you didn't take your argument that only an obtuse string theory hater would not believe that finiteness of superstring amplitudes has been demonstrated over to Jacques Distler's weblog, where he has written about this extensively. You really should; I think you and he would get along very well.

21. **Selrach**
February 15, 2005

Hi!

I have been following this discussion with interest.

I am shocked at what I have heared from the post after Peter's. Linearised stability theory was trashed in mathematical circles years ago as TOTALLY misleading. There have been Field Medals in mathematic given just for small advances in higher dimension stablity theory!

This is my interpretation of Penrose statement. Essentially, these states have large numbers of symmetries aka Solitons. The normally postion of the mathematical community regarding highly symmetric spaces is that they are essential CHAOTIC! This is because of problems of embedding conformal manifolds into real spaces.

Now, (my addition I apologise) recent research has confirmed this in admitted a low dimension setting. 3 dim constrained water waves have been show to have UNIVERSAL chaotic motions.

http://www-staff.lboro.ac.uk/~mamdg
/MarkGroves/Resources/spatialdynamics.pdf

Now, I should not have to tell you that if you had applied LINEAR analysis to this problem, you would have had never of found these CHAOTIC solutions!!!

Unfortunately, this analysis is effectly at the forfront of modern analysis and to write the sort of papers that is being asked is beyound anyone on the planet at this moment in time.

Is it really true that the Physic community believes in the stability of symmetric spaces due to Linear analysis?

I believe that it is time for a Hard Nosed mathematician to have a look at this problem and provide some help to the Physic community!

An amateur mathematician.

22.
February 15, 2005

General points of philosophy and arguments of authority are just a matter of taste. The facts are that the question of linearized stability of KK spacetime, to the extent that it is a mathematically precise question, was settled long ago by precise calcualtions. I suggest that if Penrose had something concrete to say about it, he would publish a paper on the subject, which would then be subjected to the usual scrutiny. In the absence of that there is really nothing to agree or disagree with. Just relying on his authority is unfair to many talented and devoted people who actually worked on the subject. Similar words can be said about Hawking and the fiasco of the information paradox resolution.

Now, the situation about string theory is almost the opposite of what you describe. There is no known theoretical reasons to fix the moduli, nothing is inconsistent or unstable in theories that have those moduli, not at the linearized level or any other level. This is a huge problem for string theory because we live in a world that does not have them. If Pensrose had some concrete way to kill these highly symmetric KK spacetimes, nobody will be happier than string theorists.

I really think you could understand everything I say if you actually read complete sentences instead of just look for points for
or against string theory.

23. **D R Lunsford**
February 15, 2005

Peter, that was extremely well said!

24. **Peter**
February 15, 2005

Penrose essentially claims that his and Hawking's singularity theorems also apply in this higher dimensional case. If you want the details, you have to take a look at the book, although Tony Smith just posted a relevant abstract.

I was just reporting what Penrose says, and I'm not interested enough in this issue to spend my time on the details of this. In any case I don't think Penrose has an air-tight argument against extra dimensions, because you can always claim that quantization solves the problem.

I'm certainly fond of true statements, In this case we have one of the world's leading experts on singularity theorems in classical GR making a claim about them in print. I strongly suspect that he knows what he is talking about here and is making true statements, that's why I reported on them. As for mathematically precise statements, they have their place, but in many contexts they're either not possible, not appropriate or not worth the investment of time and energy needed to get them.

In the case of string theory, there are much less subtle instability problems with extra dimensions than the ones you need Penrose's singularity theorems to see. You already have a huge problem at the linearized level. I'm referring to the well-known problems fixing the moduli parameters that describe the size and shape of the extra dimensions. Unless you first solve that problem, worrying about more subtle problems seems to me a waste of time. The only "solution" to this problem I know of leads to the "Landscape" and a completely useless theory.

As a general matter of philosophy though, I very much agree with Penrose's point of view about Kaluza-Klein. You've got enough trouble dealing with the metric degrees of freedom of space-time. You're just making things worse when you add in a dynamical metric for the fibers of your principal bundle or for some internal space.

Another way of saying it is that in the standard model you have an SU(3)xSU(2)xU(1) principal bundle, and the geometry of the fibers is tightly constrained by the gauge symmetry, which is why the theory works so beautifully. You should be trying to figure out how to use symmetries to gain control of the space-time degrees of freedom, not throwing out the gauge symmetry, creating a higher dimensional mess whose dynamics you don't understand, then hoping to recover gauge symmetry as an effective low energy phenomenon.

25.
February 15, 2005

Is there any scientific paper, peer-reviewed and published, which supports the claim that (some) spacetimes of the KK form are classically unstable to small perturbations?

This stability is a well-formulated question that can be answered precisely, there is no place to hide. Of course, it was answered precisely already for many such spacetimes, but some people don't like the answers. However, I thought the owner of this blog was fond of true and mathematically precise statements.

26. **Tony Smith**
February 15, 2005

Roger Penrose, in the UK edition of The Road to Reality, says at pages 905-907: "… 31.12 Classical instability of extra dimensions … a classical M x Y universe – subject to Ricci flatness – is highly unstable against small perturbations. If Y is compact of of a Planck size, then spacetime singularities … are to be expected to result within a tiny fraction of a second! … Let us first consider perturbations of M x Y that disturb only the Y geometry … That is to say, we examine a 'generic' ricci-flat (1 + [dimension(Y)]) spacetime Z ([Z is] the perturbed evolution of Y) … [and] E1 x Y …[is]… the (unchanging) 'time-evolution' … of Y … a singularity theorem … shows that we must expect Z to be singular. … As one of this theorem's consequences, any Ricci-flat spacetime that (like E1 x Y or Z) contains a compact spacelike hypersurface, and that is 'generic' in a certain specific sense … (and free of closed timelike curves …), must indeed be singular! The original E1 x Y escapes from being singular because the generic condition fails in this case. But the generically perturbed Z has to be singular. … If the perturbation away from Y is the same general scale as Y itself (i.e. Planck scale), then we must expect the singularities in Z to occur in a comparable timescale (about 10^(-43) s), but this timescale cold become somewhat longer if the perturbations are of a proportionally smaller size than Y itself. … the large Planck-sclae curvatures … that are likely to be present in Y will spill over into ordinary space, in gross conflict with observation, and will result in spacetime singularities in very short order. …".

Richard Feynman, in his book QED The Strange Theory of Light and Matter (corrected 7th printing, Princeton 1988), says in a footnote at page 129: "… perhaps the idea that two points can be infinitely close together is wrong – the assumption that we can use geometry down to the last notch is false. If we make the minimum possible distance between two points as small as 10^(-100) centimeters (the smallest distance involved in any experiment today is around 10^(-16) centimeters), the infinities disappear, all right – but other inconsistencies arise, such as the total probablility of an event adds up to slightly more or less than 100%, or we get negative energies in infinitesimal amounts. …".

Taken together, the Penrose and Feynman quotes seem to me to indicate that at the Planck scale spacetime (of any dimension) is probably discrete. It seems to me that a discrete spacetime is substantially consistent with a spacetime foam / LQG approach, and that it is consistent with physics models in which the M of M x Y is discrete (such as a Feynman Checkerboard) and the Y of M x Y is a compact manifold (such as my Clifford algebra model and perhaps Matti Pitkanen's p-adic model), but I am not sure that a discrete spacetime is consistent with conventional string theory (perhaps Lubos could comment on that).

Tony Smith – new web site URL at http://www.tony5m17h.net/

27.
February 15, 2005

The following might have been missed by the readers due to it having been posted a couple of days ago, but here it is (to humble ST people)

"…I do feel strongly that this is nonsense! …I think all this superstring stuff is crazy and is in the wrong direction. … I don't like it that they're not calculating anything. …why are the masses of the various particles such as quarks what they are? All these numbers … have no explanations in these string theories – absolutely none! … "

–Richard Feynman: in Davies and Brown, Superstrings, Cambridge 1988, pp. 194-195.

28.
February 14, 2005

The proceedings are published in a book format, knock yourself out, I only found words there.

BTW, nobody stands to profit more from some mysterious inconsistency of most (but not all) compactifications than string theorists, that used to be their holy grail.

29. **Steve M**
February 14, 2005

I am not convinced either of what Penrose is saying but what I briefly read about it (a few remarks in a preprint intro) sounded interesting and it stuck in my mind. Since I was'nt actually at the conference and since there seems to be no paper from him, one cannot really comment on it or really know what he has in mind. I agree that a solid argument from someone like Penrose for classical instability of higher-dimensional spacetime would be of considerable interest. If he was really serious though and had thought it
through then one might have expected it to appear by now in J.Class Quant Grav say.

30.
February 14, 2005

Steve M,

Even allowing for mights and mays, especially from such an accomplished scientist, it is hard to find an argument there. The usual singularity theorems, valid in 4dim asysmptotically flat space, are usually not taken to mean instability of flat space, or exclude it's existence. Even if there is some hypothetical singularity thm. in higher dimesnion, why would it imply the non-existence of higher dimensional gravitational theory?

A solid argument for a classical instability of higher dimensional space, especially coming from an authority like Penrose, would become immediately an extremely hot topic for research by at least 2 scientific communities, probably more. Alas, in this case it is hard to find some flesh behind the words. Maybe someone else here had better success.

Of course, Penrose himself is a hero for myself and many of my generation, but this is no reason to treat his assertions differently. I

31.
February 14, 2005

Speaking of book reviews, try http://schwinger.harvard.edu/~motl/rovelli.html

32. **Arun**
February 14, 2005

Something on Penrose and string theory here:

http://www.digitalmediatree.com/sallymckay/pageback/30638/

Refers to Penrose lectures at:
http://www.princeton.edu/WebMedia/lectures/

33. **D R Lunsford**
February 14, 2005

Steve M,

For sheer fun, find and read Penrose's early paper on the appearence of a moving relativistic object.

I ordered the book as well.

Though it's not generally mentioned AFAIK, twistors go all the way back to Pluecker's "change of space element", i.e. homogeneous line geometry. I would be interested to know if he mentions that in this new book.

-drl

34. **Steve M**
February 14, 2005

I know Penrose has suggested (Talk given at Cambridge conference in honour of Steven Hawkings 60th birthday) that a variation of the singularity theorems might rule out the existence of compact extra dimensions ala KK. The idea is that wrapping of light rays around compact dimensions would create an effect analogous to trapped surfaces in gravitational collapse. I can't find a published article though giving the details unless the proceedings have been published. Penrose is always interesting and original though and his new book certainly seems to be worth getting.

35. **D R Lunsford**
February 14, 2005

I too would like to read an elaboration of this instability issue. Does he mean N space + 1 time or N space + M time, or both? Does he mean the same thing that originally did in Einstein's cosmology (thought of as say deSitter space)?

-drl

36. **icecube**
February 14, 2005

I got the book last july, it's *very* impressive from a mathematical point of view (I can't comment on the physics).

I find that the book explains the geometric concepts of fibre bundles and spinors perfectly well (certainly better than I would have thought they could be explained).

I'm not convinced that his exposition of fourier analysis would be easily graspible for the beginner, but I sure as hell enjoyed it!

One weird thing is that he *completely* skips over basic calculus – I guess that's ok though – it leaves more time for fun stuff.

Still though, it's remarkable, and it will, if it's commercially successful bring up a whole generation of young intellectuals with a grasp of some concepts (mainly geometrical) that were previously viewed as very advanced…because, let's face it, not many of the useful concepts of modern mainstream mathematics have been brought down to the popular level ("topology is rubber-sheet geometry" isn't a useful concept, it's an unmotivated generalization (well…locale theory is a pointless generalization…)).

Anyway, it'll be a good thing if it's successful

(mathematically anyway)

37.
February 14, 2005

Under a small perturbation, such space-times should collapse and form singularities.

care to elaborate? for the usual compactification on CY, or torus, etc., this statement is just false with the usual interpretation of all the words appearing there. All such solutions are stable (tachyon free) classical solutions of supergravity.

Not that this is necessarily what Pensrose means when he talks about collapse, though it is hard to tell precisely what he does mean.

In my mind Penrose has more problems with higher dimensions than string theory specifically, but again he is perfectly capable in providing a tight mathematical argument, if he had one.

38. **D R Lunsford**
February 14, 2005

Article in the Times – a philosophical inquiry into bullshit!

http://nytimes.com/2005/02/14/books/14bull.html

Thesis: Bullshit is worse than outright lies, because lying presumes a truth, while bullshitting is an end in itself that disposes of truth altogether.

-drl

39. **Lubos Motl**
February 14, 2005

My text about the book is at

http://motls.blogspot.com/2005/02/frank-wilczek-about-penroses-new-book.html

40. **Daniel Doro Ferrante**
February 14, 2005

To complain about the 'objectivity' is just below the belt… and, in fact, it's a 'meta statement' in itself. I explain: Once "anonymous" is such an objective person, would you please do yourself (first and foremost) and ourselves the favor of checking out the reviews that the Lubos have on Amazon? Reviews of books like "PCT, Spin, Statistics and all that", "Local Quantum Physics", etc.

To me, at least, it sounds very awkward when a theoretical (hep-th or math-ph) physicist dismisses math as much STheorists do nowadays… and, before this last comment starts a flame war, let me just say that i only read about Donaldson Polynomials, Knot theory, gerbes and so forth on books either by the AMS (on QFT!) or by Kauffman or Baez. Personally, i never saw a single STheorist (mainly the 'pop' ones) talking about those topics; in fact, in more than one occasion i have been condemned for 'breaking up a discussion' with such 'mathematics' topics.

That's what you get for objectivity.

Besides, please, have and show (!) some respect for Penrose: He did more in a lifetime than most of us combined and/or put together will ever do! Or are you telling me that if Newton were alive you'd walk all over his ass because he 'was wrong'?!!! (Sorry, Peter, for the language; it's just too soon in the morning to read gigantic loads of crap… add that to a bit of Napolitan blood and you have a recipe for a (flame-)war! >;-)

Note that i'm not – nor do i intend to – defending Penrose or anyone else for that matter. I have the same opinion of folks like Witten (and others whose names decided to runaway just now, while i looked at the door). It's just hard to find, nowadays, people who are honest about what they know and about what they do not know. Then again, the market is kindda tight so, i guess this makes it all very understandable, does it not?! Afterall, we're all fighting to survive… and, as a good reductionist would deduct, in the end we are just trying to pass our genes forward… 🙁

41. **Thomas Larsson**
February 14, 2005

Meaning Penrose is untrustworthy. But surely Emperor's New Mind has already proven that. Interested in why the Lunsfords, Voits haven't lumped him in with the celebrity stringies they so detest. So much for objectivity I guess.

If you cared to look, e.g. in this thread, you would find that I have been quite critical of LQG in the past, for pretty much the same reason that I critized string theory: in order to quantize a constrained Hamiltonian system, like general relativity or the bosonic string, you must first understand its constraint algebra. As Urs Schreiber noticed in the link above, an infinite-dimensional constraint algebra is generically anomalous. In particular, GR has many constraint subalgebras isomorphic to infinite conformal symmetry in 2D, and most of these subalgebras should thus have conformal anomalies.

As for objectivity, I notice that people like Schreiber, Helling, Motl and Distler criticize LQGists essentially for missing conformal anomalies (this is a clear symptom if not the cause of the problem), but they have no interest in trying to understand why 4D diff anomalies do not arise in string theory. Note here that anomalies are physical effects seen in any reasonable quantization scheme – path integral quantization of the Polyakov action also singles out 26D, i.e. the conformal anomaly does not only arise in canonical quantizion.

Moreover, the string hype is without comparison, whereas LQG somehow reminds me of the description of earth in the Hitchhiker's Guide: "Mostly harmless".

42. **Robert**
February 14, 2005

" very interesting and challenging for beginners"

Perhaps the commonest response I've heard to this book, from working scientists (not neccessarily theoreticians) in the UK, is that of wishing it had been available to the reader when he/she was a beginning undergraduate. The exercises, friendly logos etc, sketches (by the author, not an illustrator) establish an intimate relationship with the reader not sought in the the Emperor's New Mind etc. It's hard not to see the whole thing as a textbook for the little Penrose – whose arrival is celebrated in rather extreme terms in the introduction – for use in later life.

43.
February 14, 2005

RE: but flawed at the highest level

Meaning Penrose is untrustworthy. But surely Emperor's New Mind has already proven that. Interested in why the Lunsfords, Voits haven't lumped him in with the celebrity stringies they so detest. So much for objectivity I guess.

44.
February 13, 2005

Real good review of Penrose's book by Frank Wilczek in current (Feb 11) Science. Not Free, unfortunately. Says book is very interesting and challenging for beginners, but flawed at the highest level.
Best,
Jim Graber

45. **D R Lunsford**
February 13, 2005

Lubos,

I have a gedanken experiment for you, which anyone with any familiarity with Penrose's work or indeed any real understanding of spinors will be able to immediately answer.

Just before a total eclipse of the Sun, the Moon is given a large velocity tangential to its orbit at mid-eclipse. Do the effects of relativity prevent the eclipse? Explain.

-drl

46.
February 13, 2005

Thank you Lubos for sharing your vacuous thoughts on a book you have not seen.

47. **Lubo? Motl**
February 13, 2005

Geez.

Penrose is a great and highly original guy, because of his contributions to GR, twistors, his triangle, his tiling, and so forth, but this kind of prayer is really bizarre. I have not seen the book.

Many of us have been fascinated with the complex numbers. But is this really a state-of-the-art fascination? I don't think so. The complex numbers are very important in advanced contexts – such as SUSY. Penrose's ideas about the relations between the interpretations of QM, quantum gravity, and collapses inside the brain would … well, let me not say anything because whatever I would say would be viewed as impolite.

Twistors are fun as a method to find solutions of various systems in 4D Minkowski spacetime, but the speculations that they could be related to quantum gravity or theory of everything just don't seem terribly promising right now (and they have not seemed promising for 30 years). Moreover, many recent gauge-theory calculations have been reduced back to the language of spinors, so that the idea of twistors plays less role than initially.

My guess is that the criticism of black hole string theory calculations is nothing more than misunderstanding. We've been told about his strange lecture in which he argued that string theory had some "problem" because of some singularities – he probably meant the singularities in the moduli space of Calabi-Yaus, or something like that. This is *exactly* the physical question that has been understood in detail by the string theorists. It seems that no one has explained him this stuff, which is sad.

But still, I admire him, don't get me wrong.

48. **Peter**
February 13, 2005

You're right that a lot of the book is quite technical, but it evidently did sell quite well in England. But even if most people could understand only half of it, they're getting 500 or more pages of interesting material to chew on, maybe they consider that a bargain at the price.

49. **Dave Bacon**
February 13, 2005

I also purchase a UK version of the book a few months back. It's definitely an interesting read (making up, perhaps, for "Shadows of the Mind" which I thought wasn't so great.) But the question I have is who exactly is going to read this book? I mean, for physicists who want a little easy and enlightening reading it's really a nice book, but I can't imagine the lay reader understanding more than half the book.

The Next Few Years in Particle Physics

February 17, 2005
Categories: Uncategorized

By far the most important event for particle physics during the next few years will be the beginning of operation of the LHC, now planned for 2007. Besides that, here are various sources of information about what else will be going on, especially in the U.S.:

A National Research Council committee called EPP 2010: Elementary Particle Physics in the 21st Century was formed last year, charged to:

"Identify, articulate, and prioritize the scientific questions and opportunities that define elementary-particle physics."

and

"Recommend a 15-year implementation plan with realistic, ordered priorities to realize these opportunities."

It has already had a couple meetings, and presentations to these meetings are available here. They plan to have more public meetings this year and produce a report by the end of the year.

If you want to follow the details of current and future funding for particle physics in the U.S., there's a lot of information in the presentations to this week's HEPAP meeting. The overall picture is for particle physics funding to decrease over the next few years, under the pressure of the huge U.S. budget deficits. Beyond a proposed 3.1% cut for particle physics next year, the DOE is planning for another 3.7% cut in its overall science budget over the following five years. In this environment it is very difficult to find funding for new projects. One proposed new one, called BTeV, which was to study B-physics at the Tevatron, is slated for cancellation. Another, RSVP, a search for rare decays at Brookhaven, is being reevaluated.

The DOE budget document points out that the future of Fermilab is a problematic issue. Tevatron operations are slated to wind down in FY 2009, when the LHC should start producing data. The new NuMI/MINOS neutrino beam and detectors will still be running then, but it is not clear for how long. Unless a major new machine (such as the ILC linear collider) is sited at Fermilab, it's not clear what the laboratory will be doing after 2010. Such a major new machine would be expensive, so it's not something that could be financed out of a DOE HEP budget that continues to decline. There's a comment about this in Jochen Weller's weblog.

There's a conference this week in Aspen on The Highest Energy Physics and some of the talks are already on-line.

Finally, Serkan Cabi at MIT has put together a nice collection of links to videos of physics seminars, he also has a weblog.

Comments

1. **Chris Oakley**
   February 18, 2005

   We don't need ten billion dollar experiments to tell us string theory is true!

   This requires further explanation. Are you saying

   (i) You can prove String theory for less than $10bn? or

   (ii) String theory is true regardless of what any experiment says?

   If (i), please tell us more. Apparatus, procedures, etc. If (ii), then I thought as much – String theory is a religion and not a science.

2. **Ludwig**
   February 17, 2005

   Anonymous poster Mr. Blank says (in jest? it is hard to tell)

   "We don't need ten billion dollar experiments to tell us string theory is true!"
   Posted by: at February 17, 2005 07:31 PM

   Astonishing. Is this a parody of what string-true-believers are supposed to be saying? Even as a joke it is in pretty poor taste.

3. February 17, 2005

   We don't need ten billion dollar experiments to tell us string theory is true!

4. February 17, 2005

   Don't be too worried about the futere of Fermilab. Steve Holmes (a higher-up there) gave a talk yesterday where he lays out some of the near future for the lab. The bottom line: BTeV is gone, but its kind of like getting that monkey off your back – so now they will be able to invest in Linear Collider and Proton Driver research. Research that had been held hostage to support the BTeV research program when LHCb was already well underway.

   http://tdserver1.fnal.gov/8gevlinacpapers/meeting_minutes/WeeklyMeeting/05_02_16_FY06_Implications_for_PD_Holmes.ppt

5. **Serkan Cabi**
   February 17, 2005

   Thank you very much for your kind advertisement Peter. I hope it reaches everybody who needs it. I know many places where listening a quality colloquium is a big event. We are very lucky here in US in this respect. But I am not sure how long it will continue. I should say that many interntional students here at MIT has severe concerns about staying US after their studies. I don't understand what Bush administration has in mind. I tend to think that they are just xenophobics and want us to leave the country.

Depression and Desperation

February 18, 2005
Categories: Uncategorized

In a Stanford University press release today, Susskind promotes the "Landscape", calling each different vacuum state a "pocket universe". Referring to people like David Gross who oppose the idea, Susskind says: "More and more as time goes on, the opponents of the idea admit that they are simply in a state of depression and desperation".

I'm wondering exactly which string theorists have admitted to him their depression and desperation.

It seems that Susskind's new book coming out in a couple months isn't about the Landscape, but rather black holes and holography. He's writing another one now, to be called "The Cosmic Landscape".

In other news, Witten will be giving a Distinguished Lecture Series in April at the Fields Institute in Toronto as part of their year-long program on the geometry of string theory. Witten seems to have decided that there's not much to say about string theory these days, since the topics of his talks are listed as "Relativistic Scattering Theory", "Gauge Symmetry Breaking", and "The Quantum Hall Effect".

Comments

1. **Juan R.**
   February 28, 2005

   Robert; Chris W,

   I agree that the funding of scientific-engineering activities is very limited to a rapid obtaining of tangible products.

   However, mi point is that all money devoted during 30 years to the string theory program has been, in general, a waste of money. 30 years x 1000 researches = a lot of money useful in other fields. My point is also that string theory was overly emphasized in mass media. I.e. all people will know the failure of the program and, yes I suspect that will be utilized by ?New Age anti-scientists?, ?political opportunists?, and TVC for the attack of our endeavour.

   The ?Depression and Desperation? that some of my colleagues have on theoretical chemistry is not used for attacking science, because the problem is known only by specialists on the field. Due to the success of string theory on popular media, I suspect that the ?Depression and Desperation? of string theorists will be exported to other fields of physics and science.

   Lunsford,

   With ?heroes? I mean that they have been advertising the heroic idea of that they are doing the most fundamental research, the most difficult stuff, they are even doing the mathematics for it, (so difficult that nobody more can understand it), they are solving all difficult questions on math and physics than mathematicians and physicists cannot, etc.
   Moreover, they have been sending the idea of that others physicists are very bad people and attacking them without compassion. Then people looked like authentic heroes.

   Of course, it is not true and people are completely misunderstood. Most part of string theorist are very, very arrogant people with a superficial knowledge of things. Moreover, they always were the first on heavy attack to every man that thinks or says the contrary.

   I think that all current non-string scientists (including ?renegades?) that heavily critiqued string program would joint now and write a letter explaining that the failure of string theory would be taken as a vital lesson on future funding of science, but also explaining that the rest of scientific communities would be ?favoured? in a future. I think that if a non-string theorist devoted many of its time to demystify string theory, now cannot be culpable (on funding, lost of public credit, etc aspects) of the failure of it.

   For example, I have read that perhaps Witten will research on a different field, perhaps biology. My question is, if Witten solicits funding from an agency for researching on Y biological point and Z respected biophysicist also solicits, will Witten get more ?credit? and thus money, even if most of their last decades research was nonsense? I think that Witten would be just a step below in the ?ranking? for obtaining funding when compared with Z author.

   That letter would be distributed between funding agencies, mass media, etc. I wait for interesting Woit questions on this topic.

2. **Chris W.**
   February 24, 2005

   Following up on Robert's post, the NIH and HHS are coming under significant pressure from the religious right, most notably the Traditional Values Coalition (TVC), to justify its granting decisions in most research related to HIV, AIDs, and sexuality and sexual behavior. The same critics sometimes pay special attention to research rationales that incorporate evolutionary arguments, including those that involve cross-species studies of behavior and physiology.

   For example, see the Washington Post (Oct 2003) and The Scientist (Nov 2003).

3. **D R Lunsford**
   February 24, 2005

   Penrose clearly states when he's speculating, and yes, "..Mind" is a turd of a book. "Road.." is not.

   -drl

4. **robert**
   February 24, 2005

   Funding in the areas ("engineering and "applied" research (medicine, electronic, etc.)"), assumed to benefit from cuts to that for "pure" research, is itself under very real pressure. Anything other than the next market driven quick-fix finds support very difficult to sustain. These less arcane fields of endeavour are also subject to attacks from New Age anti scientists, political opportunists etc; it's not just the hep-th guys who are feeling the pinch.

5. **Joe K**
   February 23, 2005

And Penrose (who you deeply admire) isn't a "carnival barking sensationalist"?

What is "Emperor's New Mind" if not ridiculous speculation?

6. **Matthew**
   February 23, 2005

   Laugh all you will, but it's a little known fact that Spears is an expert in semiconductor physics, as you can learn here. Likewise, Stephen Hawking is a bad ass gangsta rapper.

7. **D R Lunsford**
   February 23, 2005

   Juan R said:

   String theorists are "heroes" for people. I believe that is the direct result of string community propaganda.

   They mostly seem like assholes to me, or fawning Wittenite hero **worshippers**. Both attitudes are destructive of creativity. Perhaps you meant "heroes" with encheeked tongue.

   I predict a new era of further restrictions on funding of basci science on favor of enginnering and "applied" research (medicine, electronic, etc.). I also believe that people will look science carefully saying "perhaps this new book about the theory X is a fiasco as string theory was".

   I agree, and the fault lies with money-grubbing carnival-barking sensationalists like Greene, Davies, Zee, Kaku, etc. etc. etc. who sacrificed science on the altar of their own vaulting ambitions.

   Science was better when it didn't come in a Fisher-Price box.

   -drl

8. **Juan R.**
   February 23, 2005

   D R Lunsford said,

   "Good question, what will come next? One thing that won't happen – those who attacked string theory from the outset, and who have been proven abundantly correct, will get no credit."

   Sincerely, I hope that you are "wrong" in this aspect (I suspect that your comment was ironic here).

   String theorists are "heroes" for people. I believe that is the direct result of string community propaganda.

   I predict a new era of further restrictions on funding of basci science on favor of enginnering and "applied" research (medicine, electronic, etc.). I also believe that people will look science carefully saying "perhaps this new book about the theory X is a fiasco as string theory was".

   Will be the era for autolitarism, fanatic religion, and anti-thecnologists?

   How can we recover the image of science, so bothered by the string propaganda?

9. **Chris Oakley**
   February 23, 2005

   /:set\AI,

   I absolutely agree. Five years ago non-commuting catwalks would have been unthinkable. Now they are commonplace. Who do we thank for that? Britney Spears, of course!

10. **/:set\AI**
    February 22, 2005

    I think it is important to point out- that with all the Britney bashing many have forgotton that her work in blue shoes has led to important understanding concerning the uses of octonions in fashion

11. **Thomas Larsson**
    February 22, 2005

    Matthew,

    I didn't realize that SYM facilitates physical YM calculations. One is never too old to learn, and now the interest in super-twistors makes a lot more sense.

    However, when I said that the twistor amplitudes are "wrong at the loop level", I didn't meant that they disagree with YM, but that they disagree with SYM, due to various anomalies and connection to nonunitary conformal supergravity. At least, this is the impression I get from Witten and Berkovits, and especially from Motl. If Motl says that something is a problem in string theory, one can be sure that this problem is HUGE.

12. **Anonymous**
    February 21, 2005

    Note the "experimenter's wishlist" has at most 5 jets in the final state. Accompanied by W's and Z's. How are twistors going to help with this?

    In general, interesting physics channels have 2 or 3 hadronic jets in the final state, not counting b's — those would give you two or three more, but they aren't gluons.

    No one has ever shown me any convincing argument that N-gluon scattering amplitudes are at all relevant for LHC physics when N is not small.

13. **Matthew**

February 21, 2005

Well, my data point on the usefulness of the multi-leg YM amplitudes was the same talk that “anonymous” pointed to. I’m certainly willing to be corrected.

stop wasting your time and move to non-perturbative QCD

As a lattice person I’m forced to agree 🙂

The expressions may be concrete, but they are probably physically irrelevant (i.e. SUSic), and probably wrong on the loop level, no?

The tree level amplitudes are the same. So if you compute them in SYM or YM it makes no difference. At loop level, they’re “wrong”, but that’s misleading. Say you have an amplitude with a gauge boson loop in it. You can replace that with a combination of an N=4 SYM amplitude, an N=1 SYM amplitude and a regular YM amplitude, but with a scalar running in the loop rather than a gauge boson.

Now you can just write down the answer for N=4 SYM, it’s known. And it’s “easy” to compute the N=1 SYM amplitude (and the hope is that they’ll all be known soon), so all you’re left with is the scalar. And as we all know, computing loop diagrams with scalars in them is a lot easier than computing those with gauge bosons. You can pull the same game with fermion loops as well.

So the fact that you can write down all the N=4 SYM amplitudes really does make one loop regular YM calculations a lot easier. You can see the replacement rules in Lance Dixon’s lecture notes (hep-ph/9601359, equation 89).

14. **D R Lunsford**
February 21, 2005

Good question, what *will* come next? One thing that won’t happen – those who attacked string theory from the outset, and who have been proven abundantly correct, will get no credit.

This has really been an interesting episode in the history of science. We who were young in the 80s were in some sense not allowed a “championship season”, or even a career, but we got to see a timeless result proven with great force and clarity – you can be absolutely brilliant and have no Earthly f*cking idea what is really going on 🙂 Whatever physics is, mental horsepower is only one part of it. That in itself made it worth the ride. Good judgment still counts.

-drl

15. **Jean-Paul**
February 21, 2005

I don’t know the person who gave a talk at KITP listing what experimenters want to know about QCD etc. If you need a solid assertion, you should ask people like Kunszt, Keith Ellis, Mangano, Al Muller… who have no stakes in twistors. We won’t miss Higgs because of a miscalculated QCD background! More precision in hard scattering is completely washed out by our ignorance in soft fragmentation. To the anonymous QCD practitioner — stop wasting your time and move to non-perturbative QCD… unless you need a faculty position this Fall.

Jean-Paul

16. **Juan R.**
February 21, 2005

Hi,

I am completely sure that current string-M theory is wrong. My doubt is when this will be admited by own string-M theorists.

What will hapen then? A revolution on public perception of science? A new regime of ultraconservative funding of new ideas?

17. **anonymous**
February 21, 2005

Dear Jean-Paul,

I have to strongly disagree with your assertion that all relevant QCD amplitudes were known in the early 1990s.

There is a nice talk online which compares the (poor) theoretical status

http://online.kitp.ucsb.edu/online/collider_c04/campbell/oh/13.html

to “An Experimenter’s Wishlist”

http://online.kitp.ucsb.edu/online/collider_c04/campbell/oh/05.html

18. **Jean Paul**
February 21, 2005

I forgot the reference:
http://www.ims.uni-stuttgart.de/~jonas/piedpiper.html

19. **Jean-Paul**
February 21, 2005

SSC was the last challenge for perturbative QCD — by early 1990’s all QCD amplituted needed for the next 100 years of collider physics were known. The twistor business may be interesting in itself, but if you ask a honest QCD professional, he/she would tell you that it is completely useless for LHC jet simulations etc. The real challenge is to understand the fragmentation processes, i.e. soft physics related to quark confinement.

Now coming back to the interesting difference between blue shoes and twistors. By working on twistors, or in general, on the “hottest” (but in long-term completely irrelevant) topics a whole generation of high energy physicists were able to secure jobs at top ivy league institutions.
To conclude — you don’t get rich or famous by wearing Britney’s shoes but you do if you follow Ed the Piper.

Jean-Paul

20. **ksh95**
February 21, 2005

Matthew said

"…The difference is that Witten's work on twistors has lead to concrete expressions for Yang-Mills amplitudes, many of which could not be computed before…"

I'm not talking about YM amplitudes or the scientific usefulness of twistors. I'm talking about a herd mentality.

You say:

Everyone follows Witten because his utilization of twistors has guided us towards deeper understanding in the very important field of physics.

My wife says:

Everyone follows Brittney because her utilization of blue shoes has guided us towards deeper understanding in the very important field of accesorizing.

…I'm still not sure I see a difference in mentality.

21. **Thomas Larsson**
February 21, 2005

The expressions may be concrete, but they are probably physically irrelevant (i.e. SUSic), and probably wrong on the loop level, no?

22. **Matthew**
February 21, 2005

What's the difference between Britney Spears wears blue shoes so we all wear blue shoes, and
Ed Witten works on twistors so we all work on twistors.

The difference is that Witten's work on twistors has lead to concrete expressions for Yang-Mills amplitudes, many of which could not be computed before. There is a reasonable hope that this line of attack could lead to *closed*form* expressions for all one loop Yang-Mills amplitudes. That would be very useful, particularly in the LHC era, where huge QCD backgrounds will be the norm.

23. **D R Lunsford**
February 21, 2005

Umm, ST is based on one such dead end, KK theory. It was "dead before arrival".

-drl

24. **John Rennie**
February 21, 2005

http://relativity.livingreviews.org/Articles/lrr-2004-2/ gives a fascinating account of a previous dead end in field theory. Mind you he's writing about events 80 years ago so it's fairly easy to get a sense of perspective. Who knows how long it will take before a similar account can be written of the current era?

25. **Not a Nobel Laureate**
February 21, 2005

The last group that I can recall doing this type of "physics by press release" was Pons and Fleischmann announcing their discovery of "cold fusion".

But I'm being rather unfair to Pons and Fleischmann grouping them with Susskind.

Unlike Susskind's meta-physics, their result was testable.

Nor was the purpose of their press release to engage in emotional diatribes or ad hominem attacks.
It was to announce what they, mistakenly, believed to be a new physical phenomena.

As a physicist, I may not be driven to depression an despair by Susskind's sillines, but I certainly am embarrassed.

26. **Chris W.**
February 20, 2005

From Sean Carroll's summary of the AAAS meeting (just posted):

> [Lenny Susskind](http://www.itp.stanford.edu/~susskind/) went next, saying how happy he was to be at an AAAS meeting giving a talk on biology. That's because he went on to compare the number of possible vacua of string theory (the "landscape") to the number of possible biological organisms you could get by arranging base pairs in a DNA molecule — the former is perhaps 10500, while the latter is maybe 1025000000000. So biology wins, but the lesson we are supposed to learn is that a large variety of possibilities is what enables the development of intelligent life; in the context of string theory, it is the large number of stable vacua that makes it possible to find one with a sufficiently small vacuum energy so that life can evolve.

That line of argument strikes me as mostly fallacious, but maybe that's just because I'm depressed and desperate.

27. **ksh95**
February 20, 2005

Jean-Paul Said

"…The rise and fall of superstring theory is a fascinating social phenomenon that's certainly worth a book or two…"

Definitely fascinating, but in my opinion, not that surprising. An aptitude for complex mathematics is not an exemption from the hard wired instincts governing the rest of society.

What’s the difference between Britney Spears wears blue shoes so we all wear blue shoes, and
Ed Witten works on twistors so we all work on twistors.

This is absolutely no differnent that pet rocks, or the communist scare, or the Micheal Jackson moonwalk.

More on topic,

It’s also not surprising that Susskind would rather tear down the few hundred year old edifice of predictable testable science, than admit to himself that his lifes work and deepest beliefs are misguided.

We should just hope Susskind doesn’t succeed. History is rife with examples of highly intelligent people who, based soley on ego, pissed away a lot more.

28. **Jean-Paul**
February 19, 2005

ksh95 — you are making a good point. But one of the main reasons why too many young people went into
this type of pure research was hype and “false advertising” made under the presussure of your “public opinion”. The rise and fall of superstring theory is a fascinating social phenomenon that’s certainly worth a book or two — I assure that they would sell much better than “The Cosmic Landscape”.

Jean-Paul

29. **ksh95**
February 19, 2005

Jean-Paul Said

Even if it takes more than 20 years to figure out what (if any) is the physical content of superstring theory, serious scholars will keep working… while clowns will keep entertaining the public.

Hmmm, that’s easy to say for a physics purist who lives stricly in the physics world. It’s much harder to say for those of us living in the real world, where congressional budgets, public opinion, and academic politics reign supreme.

Personally, I haven’t a dog in the quantum gravity race, but I understand the value inherent in wowing the publics imagination, and framing research in terms of societal benefits.

It could be reasonable to question a clown’s contribution to pure research, but to question their global value…???

30. **Jean-Paul**
February 18, 2005

It is really puzzling how any serious physicist can listen to this nonsense. I think that the field was unintentionally brought down by “outsiders” who started proposing “n’importe-quoi”, as we say, ideas based on “everything goes”: large extra dimensions,
brane-worlds, which have nothing to do with strings but for general public appear far more comprehensible and sexy than superstring dualities etc. Split supersymmetry is another example of pure nonsense. Even if it takes more than 20 years to figure out what (if any) is the physical content of superstring theory, serious scholars will keep working… while clowns will keep entertaining the public.

Jean-Paul

31.
February 18, 2005

Perhaps in time physicists will look back and remember “the Landscape” as being but a pathological feature of string theory that eventually got sorted out, just like the infinities of quantum field theory were. But Susskind does remark:”Ed Witten worked very hard to show that there was only one or a small number of legitimate solutions to the theory and he failed–failed totally”. However, that does raise the obvious question: what chance does anyone else have of success then? It is noticable too that terms like “string”, “brane”, “M-theory” are not turning up in Witten’s preprints as of late.

32.
February 18, 2005

Well if I were still a string theorist, Susskind’s antics would certainly have driven me to depression and despair by now.

Atiyah’s Collected Works

February 25, 2005
Categories: Uncategorized

I recently acquired a copy of the new volume 6 of Atiyah’s collected works, which contains things he wrote from the late eighties until very recently (the latest article is his joint paper with Graeme Segal on twisted K-theory). Unfortunately the price of this book is very high (about $200). I’ve bought cars for less than what I paid for the book.

Even more expensive is the full six-volume set, which Oxford intends to sell for $1000. Luckily I bought the previous 5 volumes quite a few years ago at a somewhat more modest price. Atiyah is one of my great heroes among mathematicians. He’s up there among the top very few in any reasonable list of the greatest mathematicians of the second half of the twentieth century, and the extent of his influence in bringing together mathematics and physics is hard to overestimate. Witten’s great work on topological quantum field theory was done very much because of impetus from Atiyah. One of the articles in the new volume is the write-up of Atiyah’s amazing talk at the Weyl Symposium in 1987, where he first suggested that there should be a four-dimensional QFT whose observables were Donaldson invariants and whose Hilbert space was Floer homology.

Atiyah is also known as Sir Michael. Before I heard about this I had always thought that the British system of honorary knighthoods was pretty silly, but the fact that they chose him gave me some respect for the whole system.

It’s a shame the books are so expensive, since they are wonderful documents that deserve wide distribution. Atiyah has not only discovered wonderful new mathematics, but he writes about it in an elegant, inspiring and lucid way. The books contain many expository pieces he has written over the course of his career, and these are pretty much all well worth reading. I regard a large part of my mathematical education as having come from spending a lot of time with these volumes over the years.

Comments

1. **Peter**
   February 28, 2005

   Thanks for pointing out the problem with the link, should be fixed now.

2. **Anon**
   February 28, 2005

   I don’t know if it’s just me, but the link on twisted K theory doesn’t work.

   As for problem solvers? Um, Erdos is the only one that comes to mind for me at least.

3. **Juan R.**
   February 28, 2005

   Yes, they are expensive book,

   but that are $200 when compared with the $1200 of

   the Handbook of Molecular Physics and Quantum Chemistry by Whiley?

4. **DMS**
   February 28, 2005

   Noam Elkies (Harvard math) classified mathematicians in broadly two categories: “theory builders” and “problem solvers”. All three in the list are theory builders. Anyone know of major figures in the problem solver category (like Erdos)?

5. **Anon**
   February 27, 2005

   “About Beilinson and Drinfeld. They could learn a lot about exposition from Atiyah, Serre or even Grothendieck. Their writings make SGA look like a marvel of simple, lucid expository prose.”

   Hahaha. Did you poke around their recent text? (Chiral Algebras, I believe)

   But anyways, I agree; it isn’t pretty at all!

   🙂

6. **Peter**
   February 27, 2005

   I didn’t include Weil in my list of greatest mathematicians of the second half of the twentieth century since much of his most important work was done in the 30s and 40s.

   As for ranking Atiyah, Grothendieck and Serre; I guess I just don’t think their achievements are commensurable. All three have done amazing things, but of quite different kinds.

   About Beilinson and Drinfeld. They could learn a lot about exposition from Atiyah, Serre or even Grothendieck. Their writings make SGA look like a marvel of simple, lucid expository prose.

7. **Anon**
   February 26, 2005

   Interesting thoughts. I guess it’s hard (or perhaps a bad question to pose in the first place) about who is the “better” mathematician. Hm. Personally I’d lean towards Weil, Grothendieck or Serre.
   …

BUT since I'm at U of C, I have an obligatory vote for Beilinson and Drinfeld.

8. **JC**
February 25, 2005

Didn't Andrew Wiles (of the Fermat's last theorem fame) also get a British knighthood a few years ago?

9. **Peter**
February 25, 2005

If I had to list the greatest mathematicians of the second half of the twentieth century, I'd choose Atiyah, Grothendieck and Serre. Grothendieck and Serre are much more algebraists than Atiyah, who is much more a geometer than an algebraist. The parts of mathematics that have interacted strongly with physics in recent years have been much more geometrical than algebraic. So of these three, because my main interests lie at the intersection of math and physics, Atiyah is the one whose work I have found most directly relevant.

Grothendieck is an amazing figure, and he had a revolutionary impact on mathematics during the fifties and sixties. Unfortunately for mathematics, he stopped doing research during the seventies. If he had remained active, it would have been very interesting to see his reaction to the new ideas coming into mathematics from physics during the late seventies and eighties.

10. **anon**
February 25, 2005

And what are your thoughts on Grothendieck? Just curious. I've read of some of his work (Riemann Roch, etc), but I was curious of what you thought about it.

Best,
Anon

11. **Quantoken**
February 25, 2005

Peter said: "It's a shame the books are so expensive, since they are wonderful documents that deserve wide distribution. Atiyah has not only discovered wonderful new mathematics, but he writes about it in an elegant, inspiring and lucid way. The books contain many expository pieces he has written over the course of his career, and these are pretty much all well worth reading. I regard a large part of my mathematical education as having come from spending a lot of time with these volumes over the years."

Peter, books will get even more expensive in the next hundred years, since they are becoming antique and outdated. Try to buy a mechanical typewriter today and it will probably cost you more than an old car, if you could ever find one.

We are now in an age where information is more readily distributed and accessed through electronic means, it makes less and less sense to rely on dead trees that can decay and rot, as means of dissipating information. Both for the sake of environment protection, and for the sake of not letting book shelfs clog up the limited living space of your home, it is better to avoid buying books at all if you can.

I have accumulated books that fill cabinets lining up a whole 20 feet of wall at my home. Most of them contain outdated and useless information and I do not know what to do with them since I rarely have time to read any of them. I have stopped buying books where similar information is already accesible on the web.

As for the expensiveness of books. Part of it goes to the cost of killing trees and make paper and printing on them. A little part goes to the loyalty of the authors. And the biggest chunk goes to the greedy commercial publishers, especially publishers of scientific materials.

I think if it were not for publishers, today's scientific information would have been much more readily available on electronic medias and for very low cost or even free. wouldn't most paper authors agree that they would be happier if more people can access their work more easily? So publishers are really an obstacle to science developments.

Quantoken

12. **Steve M**
February 25, 2005

I am glad Atiyah has chosen Edinburgh University, my old undergraduate stomping ground. (Higgs is there too). I don't know where publishers get their prices for these kinds of books. There are many such books out there I would like but simply can't afford. If they would put out good paperback versions at reasonable prices I am sure they would actually make more money and shift more copies. Students (and even some professors) don't have that kind of money to spare. Incidently, Sir Roger Penrose has a knighthood too.

UFOs

February 25, 2005
Categories: Uncategorized

Last night ABC News ran a two-hour primetime special on The UFO Phenomenon — Seeing is Believing. As part of this special program, they interviewed "one of the world's leading theoretical physicists", who, according to Bob Park, "looked a lot like Michio Kaku." This physicist told ABC that UFOs should be taken seriously since "You simply cannot dismiss the possibility that some of these UFO sightings are actually sightings from some object created by ? a civilization perhaps millions of years ahead of us in technology." He also explained how aliens could get here using wormholes.

Kaku appeared yesterday on the radio show "Coast to Coast" to discuss UFOs and the ABC special. He appeared on the same show (in different hours) as Al Bielek, who evidently had a job in California, but regularly traveled by secret underground subway to Montauk, Long Island to work on the "Montauk Project". During the 1980s he traveled to Mars on several occasions, as well as to "a research station in 100,000 BC, other planets to get canisters filled with Light and Dark Energy, and to the year 6037."

Comments

1. **Quantoken**
   February 28, 2005

   Wake up Aaron, you don't even know what you are talking about. Because you are speaking from the opening of a wormhole: -)

2. **Aaron**
   February 28, 2005

   Quantoken —

   There are many problems with creating and utilizing wormholes. Unfortunately, all the ones you've put forth are wrong.

3. **J.F. Moore**
   February 27, 2005

   Kaku has been on coast-to-coast many times. He also has his own weekly hour-long syndicated radio program on Pacifica. Coast-to-coast is clearly and almost exclusively a bunch of fringe crap, not just a "popular forum", and they eschew the skeptical angle. It's below the critical thinking level where Larry King resides, if that is even possible. For a scientist to go on the show repeatedly and throw red meat to that select audience is pretty sad, but at least he makes his position regarding how he approaches science clear. What's truly unfortunate is that he somehow now occupies the public space that Feynman and Sagan once did.

   As for the ABC show, well, personally I've become numb to how uncritical and pandering the mass media have become. I guess they expect that very few people would tune in to hear Bob Park say that an instrumented balloon crashed at Roswell, nothing more; so they don't run it with that. At some point such choices become predestined.

4. **Quantoken**
   February 27, 2005

   Peter said:

   "In this case, the whole wormhole business is pure speculation. No one has any idea how to, even in principle, construct a wormhole that will connect to some other point on the universe. If you want to speculate that such a thing is possible, fine, but that's not really science right now, it's science fiction."

   One has got to understand science fiction is a much bigger enterprise than science itself. It's an industry worth several hundred billion dollars per year. For humans, the need of entertainment far exceeds the need of curiousity.

   The idea of wormholes is popular because it's entertaining, not because there is any scientific base in it. There is absolutely none.

   Theoretically spacetime wormhole can exist. But the only thing that can bend spacetime is the GR effect of mass/energy. To form any wormhole at all would take at least the amount of mass enough to form blackholes, or the equivalent amount of energy. Doesn't matter whether it is energy or mass, as far as GR is concerned.

   To form a wormhole of just a thousand light years long, like say, would take the amount of mass enough to form a blackhole a thousand light years in radius. Just being able to utilize all the energy of a star would be far from enough. You collect all mass of a giant galaxy and it is still not enough to form such a wormhole.

   You would need to collect the mass of a few million galaxies just to construct one wormhole allowing you to reach just a few thousand light years away. But to be able to collect masses of neighboring galaxies you need to be able to travel to those neighboring galaxies first, and that requires you need to have wormholes connection to those galaxies first. It's a chicken first or egg first dilemma.

   OK, now assume the alien civilization had already invented time travel machine. They could go to futuristic world to borrow a wormhole first. They would then be able to empty out a million galaxies nearby and construct a wormhole practical for inter-galactic traveling.

   What then? There would be a pretty big void in the space near our galaxy, suppose such a civilization is our next door neighbor and their wormhole is close enough to access the earth. They had used up all masses of a few million galaxies to construct their wormhole. We would then be able to see such a huge void if we aim our telescope into the sky. But no such huge void has been found?

   Or has such void been found? I heard rumors that astronomers do find big voids hundreds of millions of light years across with no galaxies. Maybe that's the evidence that aliens have used up all those galaxies to construct wormholes 🙂

   But after all, even if the alien civilization know something we don't know, and has the technology to carry out inter-galaxy travels. We can imagine it still costs an astronomical amount of resources to carry out, even if it does not take wiping out whole galaxies.

   They must also have some sort of political system, some sort of congressional budget committee, special interest groups and all that. Although their scientists would want very much make contact with the pity little creatures on earth called human, the prohibitive cost of resources would surely kill any such proposal just like how SSC was killed 🙂

So no, I do not think wormholes exist and I do not think any UFO is associated with alien visitors. But who knows, maybe the quantum teleportation is more practical than wormholes?

Quantoken

5. **Stephen**
February 27, 2005

Hi Wolfgang,

the Josephson paper claims that:
"Our mathematical skills are assumed to derive
from a special ?mental vacuum state? [..]"

I think what is being established here is how math models might have issued from the deeper recesses of math minds, to explain things.

The impact of visualization is relevant to how math minds work?

Peter Woit:*Grothendieck is an amazing figure, and he had a revolutionary impact on mathematics during the fifties and sixties. Unfortunately for mathematics, he stopped doing research during the seventies. If he had remained active, it would have been very interesting to see his reaction to the new ideas coming into mathematics from physics during the late seventies and eighties.*

George Lakoff :*Our answer is that the ordinary embodied mind, with its image schemas, conceptual metaphors, and mental spaces, has the capacity to create the most sophisticated of mathematics via using everyday conceptual mechanisms.*

http://www.edge.org/3rd_culture/lakoff/lakoff_p4.html

This process is very interesting to me. A keen eye, to a world that few of us could imagine?

regards,

6. **Peter**
February 26, 2005

Hi Tony,

I also haven't seen the TV show or heard the radio show, so I can't speak to the details of what Kaku did or didn't say in these programs.

Sure, I think it's not a bad thing, but a good thing, for physicists to appear in a popular forum. But when they do, they should be acting responsibly, and explaining to people real science. Both here and in his recent books, Kaku seems to me to be completely blurring the distinction between science and science fiction. In his recent book, he doesn't bother to clearly explain what is real science that we have good evidence for, and what is pure speculation.

In this case, the whole wormhole business is pure speculation. No one has any idea how to, even in principle, construct a wormhole that will connect to some other point on the universe. If you want to speculate that such a thing is possible, fine, but that's not really science right now, it's science fiction.

Shows like Coast to Coast are heavily caught up in the "paranormal" and the belief that there is all sorts of stuff going on that is completely incompatible with our understanding of science. People find this very appealing for deep psychological reasons. For a serious scientist to go on such a program and encourage this seems to me extremely irresponsible.

7. **Tony Smith**
February 26, 2005

Peter, thanks very much for clarifying your post about Kaku. As you say, "… "Coast to Coast" is a show often devoted to UFOs and similar topics. It is not unusual for them to be running interviews with the likes of Bielek. Kaku … decided to appear on Coast to Coast …".

Do you think that it is a bad thing for a physicist to appear on a popular forum ?

If it is a bad thing, then how will the general population who listens to such shows learn anything sensible about real science ?

If it is a good thing, is it fair to criticize a physicist for appearing on such a show ?

Please note that these questions are more general than the question of the validity of Kaku's statements, which I have not heard because I did not hear either the ABC show or the Coast to Coast show.

However, http://abcnews.go.com/Technology/Primetime/story?id=528724&page=1 and the page 2 that follows it contain the following material quoting Kaku on the ABC UFO Show:

"… Feb. 24, 2005 ??There have been countless accounts of alien visitations around the world, but one of the things that prompts skepticism is how they would get here in the first place. If aliens are from another world, they must have some extraordinary means of travel ? nothing like what is available anywhere on Earth. It is hard to underestimate the difficulty of going from star to star. …
However, Michio Kaku, one of the leading theoretical physicists in the world, says many scientists are too quick to dismiss the idea of other civilizations visiting Earth. Einstein may have said nothing can go faster than the speed of light, but he also left a loophole, said Kaku, a professor at the City University of New York. In Einstein's theory, space and time is a fabric. Kaku explained: "In school we learned that a straight line is the shortest distance between two points. But actually that's not true. You see, if you fold the sheet of paper and punch a hole through it, you begin to realize that a wormhole is the shortest distance between two points."
A civilization that could harness the power of stars might be able to use that shortcut through space and time, and perhaps bridge the vast distances of space to reach Earth, he said.
"The fundamental mistake people make when thinking about extraterrestrial intelligence is to assume that they're just like us except a few hundred years more advanced. I say open your mind, open your consciousness to the possibility that they are a million years ahead," he said. Kaku believes that only this type of civilization ? millions of years more advanced that us and capable of using wormholes as shortcuts ? could reach Earth and might be one explanation for UFOs.
"When you look at this handful of [UFO] cases that cannot be easily dismissed, this is worthy of scientific investigation," he said. "Maybe there's nothing there. However, on that off chance that there is something there, that could literally change the course of human history. So I say let this investigation begin." …".

Personally, I don't see any quoted Kaku statement that is inconsistent with generally accepted physics (bearing in mind that a lot of physicists such as Kip Thorne have written a lot about the physics of wormhole construction, etc).

It also seems to me that Kaku's statement "… "Maybe there's nothing there. However, on that off chance that there is something there, … let this investigation begin." …" is consistent with the scientific spirit of observation, experimentation, and inquiry, and is NOT a gullible Oh-Wow-look-at-the-aliens type of statement.

Again, my primary question is: If physicists are to be pilloried for merely appearing on a popular forum about subjects that interest the public, how is the public to become better educated about physics ?

The primary question having been put, I want to make clear that I have some substantial disagreements with Kaku about physics, such as his statement on his web page at http://www.mkaku.org/articles/proposal_uft.shtml where he says "… The fundamental problem facing physicists is that General Relativity and the quantum theory, when combined into a single theory, is not ?renormalizable, ? i.e. the theory blows up and becomes meaningless. … So far, only superstring theory can … give us a finite theory which combines these two formalisms. …".

My disagreement is with Kaku's statement that only superstring theory can so combine GR and the SM.

I also note that he is careful to NOT say that superstring theory HAS done such a thing, only that maybe it can, sort of like maybe the advanced civilizations can build such wormholes.

It would have been nice if the ABC UFO show had also had somebody like Thorne to point out how very difficult such a wormhole construction project might be, but it seems to me that ABC is being as one-sided in its view of UFOs as some conventional superstringers are in their views of approaches to theoretical physics.

Tony Smith http://www.valdostamuseum.org/hamsmith/

8. **Peter**
February 26, 2005

Hi Tony,
The schedule of the show that I saw didn't make clear they appeared in different hours, so it was incorrect to say that they appeared "together", and I'll change that.

But I don't think my comments were fundamentally unfair. Kaku was the one who decided to appear on ABC and lend his reputation to the pro-UFO side of their show. "Coast to Coast" is a show often devoted to UFOs and similar topics. It is not unusual for them to be running interviews with the likes of Bielek. Kaku was the one who decided to appear on Coast to Coast to discuss UFOs in a positive light, and I think it is likely that he would have known what the nature of that day's show was, and who else was appearing with him on the same day.

9. **Tony Smith**
February 26, 2005

Peter, you said:
"… Kaku appeared yesterday on the radio show "Coast to Coast" to discuss UFOs and the ABC special. He appeared together with Al Bielek, who evidently had a job in California, but regularly traveled by secret underground subway to Montauk, Long Island to work on the "Montauk Project". … he [Bielek] traveled to Mars on several occasions …".

I think that is a dishonest criticism-by-association of Kaku.

The Cosat-to-Coast website at http://www.coasttocoastam.com/shows/2005/02/24.html says:
"… Thursday's first hour guest, theoretical physicist Michio Kaku commented on the ABC special Peter Jennings Reporting: UFOs which had aired earlier in the evening.
…
…
In the middle two hours of the show Al Bielek revisited his claim that he participated in the Philadelphia Experiment aboard the USS Eldridge in 1943 as a man named Edward Cameron. … Bielek stated that while at Montauk he traveled via a wormhole to Mars …".

It appears clear to me that Kaku appeared in the first hour to discuss the ABC UFO special, while Bielek appeared on the middle two hours to discuss the Philadelphia Experiment and Montauk/Mars stuff. Obviously (to me), the first hour and the middle two hours were quite independent radio shows.

I think that it is highly intellectually dishonest to smear Kaku with guilt-by-association-with-Bielek, and I am disappointed in your failure to maintain the otherwise very high level of integrity that I have seen in your blog.

Just for the record, my own personal opinion of Kaku is somewhat mixed.

On the one hand, I agree with drl's comment to an earlier post (More Science Fiction, January 02, 2005) in your blog where drl said "… I like Kaku's field theory book …",
and
I also agree with Dolomite's comment to the same earlier post where Dolomite said "…The table of contents …[of]… one of Kaku's graduate level textbooks on String theory … looked quite impressive, but the body of the book turned out to be a smorgasborg of equations … with the symbols changed around in a failed attempt at making the different chapters look consistent, in the process removing much of the logical connective tissue and adding in typos. …".

Tony Smith http://www.valdostamuseum.org/hamsmith/

10. **Wolfgang**
February 26, 2005

Stephen,

the Josephson paper claims that:
"Our mathematical skills are assumed to derive
from a special ?mental vacuum state? [..]"

While this 'mental vacuum' state seems to be
important for superstring theory, I would
assume it also describes some of the contributers
to this discussion here pretty well …

11. **Quantoken**
February 26, 2005

People take UFO seriously not necessarily because there is good evidence UFOs are associated with aliens, but because such ideas are entertaining. People tend to believe things that are amusing and entertaining, and tend to reject things that's boring even though they are true. That's psychology. The same thing could explain why string theoretists so believe in their theory religiously, even though there hasn't bee any evidence their theory has anything to do with nature.

Just for entertaining, but also seriously, for the UFOs that do sudden motion changes. It does not necessarily mean their inertial masses have been reduced to virtually none-exist. More likely they acquire momentum change without excerting opposite momentum change to the immediate vicinity. That's why there is no shock waves.

One possible scenary is they are capable of ejecting strong directional streams of gravitons. The GR predicted existence of gravitons. If these particles exist then theoretically they can be controled using certain technology of another civilization, and be emitted towards certain direction in a controled way.

But then gravitons can be used to break the equivalence principle. How come? A particle invented by the GR theory comes back and destroy the very principle that GR is based on? I do not know how to reconcile that inconsistency? If UFOs exist they do provide evidence of existence of graviton and gravitational wave, right?

Do gravitons themselves gravitate and observe the equivalence principle? i.e., do they have equal inertia mass and gravitational mass? Or do they have inertia mass only?

Quantoken

12. **Matti Pitkanen**
February 26, 2005

Leaving aside ontological considerations and what to think about people taking UFOs seriously, one could take UFOs as a source of thought experiment.

Suppose for a moment that UFOs represent a real technology. According to the reports, UFOs seem to have a very small inertial mass (butterfly like motions involving sudden accelerations and changes of direction of motion without producing any shock waves). A technology able to reduce dramatically inertial mass of a material object would thus exist. What could this tell about fundamental physics?

A possible answer would be a modification of Equivalence Principle. Gravitational mass would be absolute value of inertial mass, which can have both signs.

One of the most obvious implications is an explanation for why gravitational energy is not conserved in cosmological scales whereas there is no evidence for the non-conservation of inertial energy. The simplest cosmology would be that creatable from inertial vacuum by energetic vacuum polarizations creating regions of positive and negative density of inertial mass. The 4-D universe could replace itself by a new one quantum jump by quantum jump and the difficult philosophical problems formulated as questions like "What was the initial state of the Universe and what were the initial values/densities of conserved quantities at the moment of big bang?" would disappear. The observations motivating the anthropic principle would find a natural explanation: the universe has gradually quantum engineered itself so that the values of these constants are what they are.

Technological implications would be also interesting. Forming an tightly bound state of systems with positive and negative inertial mass a large feather light system could be created. Could UFOs be real and utilize this kind of technology?

Accepting negative energies, one cannot avoid the questions whether negative energy signals propagate backwards in (geometric) time and whether phase conjugate light discovered at seventies could be identified as signals of this kind. Positive answer would have quite interesting technological implications. Negative energy signals time reflected as positive energy signals from time mirrors (lasers with population reversal for instance) would allow communications with geometric past. Our memory might be based on this mechanism: to recall memories would be to scan the brain of geometric past by using reflected in time direction (rather than in spatial direction as in seeing in the ordinary sense). Communications with the civilizations of the geometric future and past might become possible by a similar mechanism.

Matti Pitkanen

13. **stephen**
February 26, 2005

Peter Woit,

I do not understand this presentation from what I have read of your work here?

Is there a clear distinction from your position of what you would like to exemplfy of intelligent people, like Michio Kaku and others to that of string theory?

Allen Hynek was a disbeliever, and headed project bluebook, but eventually changed his tune. H eis shunned, by those of his peers. Is this what happens when those of science step outside of the box?

Brian D. Josephson
Department of Physics, University of Cambridge

Ed Witten, TV writer

February 26, 2005
Categories: Uncategorized

There's a story in this Sunday's New York Times television section describing how Ed Witten pitched a story idea to the people who make the new TV show Numb3rs. According to one of the show's executive producers, Cheryl Heuton, "Ed sent our script back along with an episode idea, which we used, telling us we should do something about a rogue mathematician who tried to crack Internet security by solving the Riemann hypothesis." Witten had received the Numb3rs script to look at from his brother, the writer Matt Witten.

For more about the Caltech mathematicians who are the main consultants for the TV show, see this USA Today article.

Comments

1. March 1, 2005

   I guess Ed should keep his day job.

2. **Alejandro Rivero**
   February 28, 2005

   "a rogue mathematician who tried to crack Internet security by solving the Riemann hypothesis".

   Plot already used, in the movie "Sneakers". The mathematician gets killed fast along the plot. According some web pages, the mathematical advisor contacted was Len Adleman, "the A in RSA".

3. **Chris Oakley**
   February 28, 2005

   I thought that Matt Damon was an actor. Or is this what "method acting" is about – totally getting into the part. So if you play Hitler you have to be a crazy megalomaniac who wants to annex the Sudetenland (not likely to be good for the Czech tourist industry) & if you play Lord Byron you have to have a "fling" (well, possibly) with your half-sister – ?

4. February 28, 2005

   Solving an undergrad level matrix problem might not seem like much to you, but this is Matt Damon we're talking about. A math coach can only do so much.

5. **Quantum_Ranger**
   February 27, 2005

   The programme that I recall Ed Witten walking through the countryside, was Stephen Hawkings "Universe"?..it also shows Smolin walking across stones of a stream/river?

   I met Ed Witten when he was here in Swansea University(the re-located stringtheory summer school for postgrads 95-96?)..where I first seen the words Brane and T-Duality. The conference was very intense, most of the Post-Grads were 'rebelling' against the new generalized theory of 'M-THEORY' and such.

   Ed Witten had a lot of problems with the post-grad students, and a number of them left the lectures totally bemused. One outcome was the emergence of VSL, which was put forward by a number of 'persons', in the snooker room, over the snooker table.

   I played my part in asking certain questions,(I was Porter at the Uni!), and some of the postgrads were really interested in some of what I asked regarding of why the speed of light had to be different if one rewinds Einsteins Field Equations, one response I recall:The speed of light and its constancy, is one of the most sacred postulates of science, you dont mess with any constants, if you want to get anywhere in the future,(context was wrt Academia).

   Rogue mathematician I am not:http://groups.msn.com/RelativityandtheMind/shoebox.msnw?Page=1

   😉

6. February 27, 2005

   The program with Witten walking through the wilderness and walking on a dry lake bed was probably a NOVA show on PBS about string theory.

7. **Redouan**
   February 27, 2005

   Steve,

   >Speaking of Witten and tv does anyone know what program/series >featured Ed Witten wandering through the countryside…

   Well, it's not exactly that program, but there was another one broadcasted on Dutch television showing some parts with Witten in Amsterdam during the 1997 summer String Theory seminar. For the most part, however, it's Michio Kaku in Manhattan doing the talking. [the program is 24 min. long.]
   You can watch it on
   http://www.vpro.nl/wetenschap/index.shtml?3626936+2848322+3855404+5409638

   Funny part: Einstein arriving in 1930 in NY surrounded by a horde of reporters.

   Reporter: What do you think of Prohibition professor?
   Einstein: Ich drinke nicht, also… ist mir dann ganz gleich.

8. **Chris Oakley**
February 27, 2005

Re: *Good Will Hunting*, it looks like someone at Harvard has posted the blackboard problem. I don't think that he is saying that one would need to be a genius to solve it, though.

9. **Alejandro Rivero**
February 27, 2005

The initial version of Bretch' Galileo was discussed by the author with the physicist of Copenhagen.

10. **Santo D'Agostino**
February 27, 2005

Chris (and anonymous),

Some of Good Will Hunting was shot in Toronto. University of Toronto high-energy physicist Pat O'Donnell was having lunch one day at a Chinese restaurant in Toronto's Chinatown. Pat's got a full white beard and a thick accent. The casting person from the movie was having lunch at a nearby table and offered him a small role in the movie. When the movie people found out his profession, they also made him a consultant, and Pat was responsible for putting the mathematics on the blackboards.

I seem to recall Pat saying he consulted with number theorists, but in any case one shouldn't rush to blame him if the math didn't come out realistic without finding out more about the situation. Hollywood has its own agenda, and being accurate is not necessarily part of that agenda in most cases. They may have given Pat certain instructions about what the math should be like, but I don't have any information about that.

Santo D'Agostino

11. **Stephen**
February 27, 2005

Steve

Speaking of Witten and tv does anyone know what program/series featured Ed Witten wandering through the countryside and sitting by rivers scribbling on a notepad, and walking across the boulders on some dried-up lake bed?

This is firmly emblazoned within my mind as well. Can't recall the name of this program either though.

Because of what it signified, and what I concluded, is that intense mental dealings with math could stifle the mind from finding anything further, but it was from these silent walks and divergences from the hectic, that allowed the breakthroughs to manifest.

I think this was the point that Witten was showing then. It also open up the idea that complex mathematics might from perspective, as if in some meditative stance, takes us closer to finding the language most relevent in innovation, when such math structures are contemplated.

Invention is no less in the same sequence of events as math might be in creativeness, that if you leave it, it will come later.

Looking for these areas of further intelligence developement in mathematics, raise the issue of cognitive functions spoken in previous post.

Why metaphorically, bubbly structures rising to the surface, from unconsious processes, might be more revealing then first anticipated?

For example it was Ramanujan's enviromental god processes that spoke to him about the math , but of his mind was dealing with complex variables in a way that metaphorically was the sign post of probabilities?

Yet out of the hectic, a system emerged. You just had to be aware of what the mind was capable of producing and how it would do that.

Ramanujan was no different then you and I and the envirnoment that we deal, with could manifest creative possibilties, as math would in comparison. Why Susskind's light switch turned on, with those loops called strings?

Perspective views of other systems could awaken more comlex structures, as seems to be the way of math structures continung to build on itself , when these minds become involved and contemplate.

regards

12.
February 27, 2005

I noticed that too Chris. You would at least think they could get a consultant mathematician and get it looking right. I guess they figure it is good enough to fool the layperson majority in the audience. At least this Numbers series has consulants, which I think is important. You see this all the time though in old sci fi films. In one old film I saw once there is a "scientist" talking away and on a black board behind him is chalked F=Gm1m2/r^{2} and nothing else! Why?! I guess it was meant to show that he was a "scientist". I did see the movie "Supernova" on dvd recently. Not a great sci-fi movie but not bad. In once scene the ship's computer does a stunning mathematical analysis of an alien weapon, that was dug up on an ice moon in orbit around a giant start about to explode. The weapon contains "9-dimensional matter"–I guess it must be superstring. Freezing the scene on the dvd remote the equations look pretty good. At least they put some thought into it. I also need to get a life:). Incidently the guy exposed to the superstring weapon becomes a deranged superhuman psychotic who proceeds to murder all the crew. So string theorists might enjoy the movie:)

13. **Chris Oakley**
February 27, 2005

Re: the *USA Today* article, Ramakrishnan is right on target about *Good Will Hunting*, the tale of a janitor at an Ivy League university who writes solutions to cutting edge mathematical research problems on white boards in between mopping floors. IIRC (I only saw the film once, on a noisy aeroplane) the "solutions", insofar as they were comprehensible at all, were just first-year-undergrad matrix calculations which, scarily enough, had *actual numbers* in them – something a true mathematician would shun at all cost.

14. **Steve**
February 27, 2005

I guess they are a very talented family. How many Witten brothers are there anyway? There is a Monty Python sketch–"the Golden Age of Ballooning" where they discuss "Barry Zeppelin" the "least talented of the Zeppelin brothers" so I am wondering if there is a "Barry Witten" somewhere:)who has been completely overshadowed by his brothers. It does happen. Speaking of Witten and tv does anyone know what program/series featured Ed Witten wandering through the countryside and sitting by rivers scribbling on a notepad, and walking across the boulders on some dried-up lake bed? I remember seeing this in the 90s on some science series…I really liked that scene though. It was the most memorable. In fact the only part I actually remember.

15. **Lubo? Motl**
February 26, 2005

    Maybe Juan has proved the Riemann hypothesis, and Ed has shown how to use it to compromise the internet security…

Wick Rotation

February 28, 2005
Categories: Uncategorized

There's a quite interesting discussion going on about Wick rotation over at Lubos Motl's weblog.

In flat space-time, the situation is well-understood: if your Hamiltonian has good positivity properties you can analytically continue to imaginary values of time, and when you do this you end up with "Euclidean" path integrals, which actually make sense, unlike QFT path integrals expressed on Minkowski space, which don't. You can see the problem even in free field theory: the propagator is given by an integral that goes through two poles, so is ill-defined. The correct way to define it to get causal propagation for a theory with positive energies is to go above one pole, below the other, which is equivalent to "Wick rotating" the integration contour 90 degrees to lie on the imaginary time axis.

In a curved space time, things are much trickier. And in a path integral approach to quantum gravity it is very tricky. Do you integrate over all metrics with Lorentz signature (ignoring the fact that the path integral doesn't really make sense for a single one), or do you integrate over Euclidean signature metrics (Euclidean Quantum Gravity)? There are arguments against either choice, not to mention the non-renormalizability problems that both may have. For some of the arguments, see the debate in Lubos's comment section, which gives some idea of how confused the state of this question is. Another good reference is the article by Gary Gibbons in the Hawking 60th birthday celebration volume. It doesn't seem to be on-line, but his talk at the workshop is.

I've always thought this whole confusion is an important clue that there is something about the relation of QFT and geometry that we don't understand. Things are even more confusing than just worrying about Minkowski vs. Euclidean metrics. To define spinors, we need not just a metric, but a spin connection. In Minkowski space this is a connection on a Spin(3,1)=SL(2,C) bundle, in Euclidean space on a Spin(4)=SU(2)xSU(2) bundle, and these are quite different things, with associated spinor fields with quite different properties. So the whole "Wick Rotation" question is very confusing even in flat space-time when one is dealing with spinors.

Over the years I've tried to sell the outrageous idea that one should define QFT in Euclidean space time, with one of the two SU(2)s in Spin(4) being Spin(3), the spatial rotations, the other being the SU(2) of the electroweak gauge group. I've never been able to get anyone to take this seriously, partly because I've never come up with a well-defined way of writing down path integrals which implement this idea.

Comments

1. **Quantum_Ranger**
   March 3, 2005

   Is there any chance of 'Peter', actually giving a glimpse of :Nucl Phys. B paper?..a direct link would be useful.

2. March 3, 2005

   Fixing the causal structure of spacetime will not work. A simple thought experiment will show this. A universe with no black holes initially which forms a black hole as a clump of matter collapses has a very different causal structure from a universe where no black hole ever forms. But this is the crucial point; whether or not some matter collapses to form a black hole depends upon the dynamics of matter and gravity. Clearly, this means making the causal structure independent of the dynamics will lead to a contradiction.

3. **Anonymous**
   March 3, 2005

   Dear Lubos,

   I must say that I find your posts, always, very entertaining. But, please, remember that you are no longer in your home satellite. In the US, the terms of discourse are somewhat different. Your commissar-like behaviour of trying to shout down critics with insults and denunciations will not work. You merely appear ridiculous. You may keep your stalinist committments (if you must), but you need to change your methods (hint: learn from Distler). I think there is a book called "Getting power in the US while keeping your stalinist committments intact"; will let you know the Amazon link.

   Peter: I don't think Distler is "better" because of english. He just understands the subtle power-politics here much better than Lubos, who is still trying out his stalinist vilifications techniques.

4. March 3, 2005

   Incidentally, \int dx e^{ikx}=2 \pi \delta(k).

   Therefore, we have a clear example of a case where Wick rotation doesn't hold. 🙂

5. March 3, 2005

   formally, we can still perform calculations with unbounded path integrals. Let me give you an example of what I mean.

   \int_{-\infty}^{\infty} dx e^{kx} looks divergent and not well-defined, but let's pretend its value is f(k), which is of course infinite. Then, f'(k)=\int dx x e^{kx}. Ignore for a moment the fact that this integral diverges badly and pretend that integration by parts works. Then, f'(k)=-\int dx 1/k e^{kx}=-f(k)/k. Solving this differential equation, we obtain f(k)=f(1)/k. But recall f(k) is infinite. So, we normalize everything by dividing infinity by infinity to get a finite value.

   \int dx e^{kx}/\int dx e^{x}=1/k

   I know this is vulgar speech which wouldn't be appropriate in the polite company of mathematicians, but we're all physicists, right? You won't get offended. Peter may be a "mathematician", but when it comes to physics, he has the mindset of a physicist.

6. March 2, 2005

   Is it possible to add higher order terms to the Einstein-Hilbert action so that the Euclidean action is bounded from below?

7. **D R Lunsford**
March 2, 2005

Lubos,

The thing that makes invariant integrals is sqrt(det(g)), not -det g whatever that is. So the issue is, do I take sqrt(det(-g)) as do the relativists, or, as demanded by tensor analysis, sqrt(det(g)) ipse, which, if g has 1, 3, 5.. – signs, imaginary (and branched)?

-drl

8.
March 2, 2005

Lubos, how can we have both signature changing metrics AND a global gauge fixing where -|g| = 1 everywhere?

9.
March 2, 2005

Rest assured, Lubos will never have to distort his text to look less smart.

10. **Frank**
March 2, 2005

On the Topic of Lorentzian vs Euclidean, I'm reminded on stuff by Renate Loll in 2 Dimensions a while back:
a couple of slides: http://cgpg.gravity.psu.edu/online/Html/Seminars/Spring1999/Loll/Slides/s01.html

Insisting on causal paths in the path integral the theory can be defined in the continuum limit and differs from what you get in Euclidean theory.
Something analogue to the Wick rotation is still going on in that an imaginary cosmological constant is required to ensure the existence of the continuum limit.

More recently numerical investigations suggest that a smooth 4D (Hausdorff Dimension) macroscopic geometry emerges from these causal path integrals.

http://arxiv.org/abs/hep-th/0404156

11. **Lubos Motl**
March 2, 2005

Dear "", diffeomorphisms don't change the value of the action which is exactly the reason why the modes induced by diffeomorphisms are pure gauge. Locally, one can always choose a gauge where (-det g)=+1.

Sometimes I wonder, "", that "" is really Jacques Distler who just distorts his text a bit to look less smart, by a few orders of magnitude, so that there is an "independent" debater who supports his viewpoints. 😉

12. **Peter**
March 2, 2005

I think it would be best if people discuss Lubos's mistakes on his blog, mine on this one….

13.
March 2, 2005

Dear Lubos, what you say sounds as an error that would prevent a first-year grad student of general relativity from passing her exam.

Of course that your confusion about diffeomorphisms and the conformal factor shows a complete lack of understanding of general relativity.

That's not so hard to see. Diffeomorphisms would not change the value of the action.

One can't be confused about elementary general relativity theory if she wants to seriously work in quantum gravity. 😉

By your own admission, Distler's objections are perfectly valid. So now, you're completely unable to describe what the mistake in Distler's reasoning was supposed to be, are you?

14. **D R Lunsford**
March 2, 2005

Well that joke went over like a plaster of paris bagel…

-drl

15. **Lubos Motl**
March 2, 2005

Peter, you can always learn new things here. For example, Dr. Lunsford now teaches you that the determinant of the metric tensor (i.e. the product of -1 times 1 times 1 times 1) equals "i". Enjoy your learning! 🙂

16. **D R Lunsford**
March 2, 2005

Um, -det g = -i 🙂

Funny, this is also the y5 issue.

-drl

17.
March 2, 2005

In response to Lubos:

The gravitational action is arbitrarily large if the conformal factor varies fast enough.
You thought this behavior can be "gauged away" because one can "always set (-det g) = 1.

Is this explanation good enough for you?
If not I suggest you read the available literature.

18. **Lubos Motl**
March 2, 2005

Dear "", it was not subtle, except that you're completely unable to describe what the mistake was supposed to be, are you? 😉

19.
March 2, 2005

In response to Lubos Motl:

I did not write (-det g) = 1 and your mistake was not subtle at all.

20. **D R Lunsford**
March 2, 2005

Peter,

Yes, something "funny" is going on and it amounts to using the wrong "i" in the context of spinors, which need spacetime algebraic invariants. In a representation of the Dirac algebra in which y5 is diagonal (Weyl rep) the spacetime "I" (-iy5) reduces (almost) to the usual "i". This issue comes up again and again and leads me to believe the "via regia" to geometry in the context of field theory is y5.

Here are references to the papers I mentioned before:

D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, Foundations of quaternion quantum mechanics, Journal of Mathematical Physics 3, 207 (1962)

D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, Some physical consequences of general Q-covariance, Helvetica Physica Acta 35, 328-329 (1962)

D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, Principle of general Q-covariance, Journal of Mathematical Physics 4, 788-796 (1963)

D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, Quaternionic representations of compact groups, Journal of Mathematical Physics 4, 136-140 (1963)

You'll find, essentially, WSG electroweak theory, long before gauge theory was cool.

-drl

21. **Lubos Motl**
March 2, 2005

Who are the others you learn from, Peter?

The anonymous guy below who believes that one can't set (-det g)=1 by gauge transformations (diffeomorphisms)?

Of course that you can always find a simple coordinate redefinition such that (-det g) is whatever function or constant you want.

I retracted a statement that all problems related to (-det g) are pure gauge problems (it's a very subtle question), and an anonymous reader immediately celebrates that I retracted something, even though she or he misunderstands what exactly was retracted.

22. **Peter**
March 2, 2005

Actually one ot the best aspects for me of this weblog is that I often learn new things from people who write in with comments. Sometimes even from Lubos….

23.
March 2, 2005

It is not such a big deal as long as you can admit them quickly WITHOUT insulting everybody.

24.
March 2, 2005

If Lubos celebrates the fact that Peter stumbled a bit in this case, then I would like to point out that he made a much worse blunder 2 days ago on his
Wick rotation thread.
He was of the opinion that the conformal factor in higher-dimensional gravity can be set to 1 by gauge transformation.
People make mistakes. It is not such a big deal as long as you can admit them quickly with insulting everybody.

25. **Peter**
March 2, 2005

Unlike some people I'm not claiming to have a wonderful theory here. All I'm pointing out is that there seems to me to be something funny going on when you try and think about Wick-rotating spinors. The standard point of view is that this is a purely technical problem, but I suspect it might be a clue to something much more interesting and mentioned some reasons for thinking this. There are others in my 1988 paper. But this is a vague idea about where to look for a new theory, not a new theory, and I haven't claimed otherwise.

26. March 2, 2005

    Theatre critics should never act. It's an important rule. Probably best to just keep knocking everyone else's work Peter. Much easier.

27. March 2, 2005

    The kinematics doesn't seem to work. It may work for fermions; the left handed ones are also the ones charged under electroweak SU(2), while the right handed ones are uncharged. The photon, however, transforms under both SU(2)s of the Lorentz group, while it is uncharged under the electroweak symmetry. This might already invalidate your proposed identification.

28. **Peter**
    March 1, 2005

    Hmm, it's too late at night for me to straighten this out but I think you're right I was saying something incorrect. But all I was trying to say was SO(4)/SO(3)=S^3, which as a manifold can be identified with SU(2), although you're right it doesn't get its group law from the SO(4) one.

    I got into this mixup by trying to oversimplify things by just counting degrees of freedom. A more accurate way of saying what is in my 1988 paper is that I look at the twistor space of orthogonal complex structures over a 4d Riemannian manifold, and try and identify the electroweak U(2) as the subgroup of SO(4) at each point in twistor space that commutes with the complex structure.

    I haven't thought about this much in a while and don't have time to try and write out more details, especially not here where it is hard to write math. If people want more details now, they should take a look at the paper.

29. March 1, 2005

    But the set of elements of the form (q_1,q_1^{-1}) doesn't form a group! SU(2) isn't Abelian. And we know the electroweak SU(2) forms a gauge group. And if you wish to identify the electroweak SU(2) with torsion, since you're not identifying it with gravitation, that can't work. The electroweak coupling has the form \bar{\psi} A_\mu \gamma^\mu \psi, not \bar{\psi} \Gamma_{\mu\nu} \sigma^{\mu\nu} \psi. It's a vector (well, V-A) coupling, not a tensor coupling.

30. **Peter**
    March 1, 2005

    Here's one way of thinking about what I meant:

    Pick an identification of R^4=H (H is the quaternions). Then elements of SO(4) are given by pairs (q_1,q_2) of unit quaternions, and they act on an element q of H=R^4 by

    q goes to q_1qq_2^{-1}

    Note that SU(2)=group of unit quaternions, SO(4)=SU(2)xSU(2) modulo a Z_2, since (q_1,q_2) and (-q_1,-q_2) give the same action. Spin(4)=SU(2)xSU(2), but if you only act on vectors you just see SO(4).

    With the standard choice of identification q=x_0+x_1i+x_2j+x_3k, the diagonal subgroup of elements of the form (q_1,q_1) leaves x_0 invariant, acts as SO(3) on the other 3 variables. By "anti-diagonal" SU(2) I was thinking of elements of the form (q_1,q_1^{-1}).

31. March 1, 2005

    My apologies, that's only true for Spin(n) for even n. "Inversions" act trivially upon Spin(n).

32. March 1, 2005

    Sorry, I accidentally hit the Post button.

    SU(2) × SU(2) has infinitely many diagonal subgroups, one for each automorphism of SU(2). Basically, if f is an automorphism of SU(2), the subgroup of elements (g,f(g)) forms a diagonal subgroup. The outer automorphism group of SU(2) is the two element group Z2, i.e. correspoding to a "reflection". This, I'm pretty sure is what Peter meant by the anti-diagonal subgroup.

33. March 1, 2005

    SU(2) × SU(2) has infinitely many diagonal subgroups, one for each automorphism of SU(2). Basically, if f is an automorphism of SU(2), the subgroup of elements (g,f(g)) forms a diagonal subgroup. The outer automorphism group of SU(2) is the two element group Z2

34. **Lubos Motl**
    March 1, 2005

    Dear Peter,

    your comment about the trademark Lubos Motl (R) is very illuminating and entertaining. Nevertheless you may want to focus on the essence which is the SU(2) x SU(2) group in this particular case. Once you see that there are only two truly inequivalent ways how to embed SU(2) into SO(4) and only one of them has another SU(2) left, you may decide whether you want to follow my advise or the advise of the other straw man.

    Good luck
    Lubos

35. **Alejandro Rivero**
    March 1, 2005

    A glimpse of the role of the electroweak group:

a_\mu=
{m_\mu \over m_Z}+ {m_e \over m_W}+
\frac12 {m_\mu^2-m_e m_\tau \over m_W^2}=.001165825

The conventional perturbative calculation (without hadronic corrections) of muonic (g-2)/2 gives .0011658487, about a 0.003% respect to the value given by the above empirical formula.

36. **Peter**
March 1, 2005

Hi Lubos,

I can't imagine why someone here was referring to the "trademark Lubos Motl mixture of straw man arguments, willful misreading, and insults"….

37. **Lubos Motl**
March 1, 2005

Dear Peter, what you say sounds as an error that would prevent a first-year undergrad student of linear algebra from passing her exam.

Of course that your things would violate the Coleman-Mandula theorem. But more easily, there is nothing such as "anti-diagonal" SU(2). You can divide the SU(2) x SU(2) generators to the diagonal ones, and the rest. The diagonal ones form a closed algebra, but the anti-diagonal don't.

That's not so hard to see. These are generated by J14, J24, J34, and the commutators of those are again in the diagonal algebra generated by J23, J31, J12.

One can't be confused about elementary SU(2) group theory if she wants to seriously unify interactions. 😉

38. **Peter**
March 1, 2005

Coleman-Mandula says you can't find a larger symmetry group that includes both Poincare and internal symmetries, mixing them in a non-trivial way. I'm not trying to do that.

39. **Thomas Larsson**
March 1, 2005

Peter,

If you try to unify spatial rotations with electroweak SU(2) without SUSY, don't you run into trouble with the Coleman-Mandula theorem?

Nobody seems to have mentioned AJL's simulations, which distinguish between Lorentzian and Euclidean spacetimes in a different way. Thay insist on strict causality everywhere, which means that topology change is ruled out. They work with Wick-rotated Lorentzian spacetime, which is thus different from Euclidean spacetime with topology change allowed.

I like their results for several reasons. The obvious one is that they get a smooth 4D manifold rather than a crumpled mess, in agreement with observation. Another feature is that if you couple the Ising model to Lorentzian gravity, as in hep-th/9904012, the critical exponents remain at their Onsager values, unlike Ising coupled to Euclidean gravity. This is good, because the one thing we do know that quantum gravity is that it is unimportant compared to electromagnetism; the Ising model is a condensed-matter model, and as such it is a rough model of electromagnetism. Moreover, claims that AJL violate unitarity are, AFAIU, false.

40. **Peter**
March 1, 2005

When you decompose Spin(4) as SU(2)xSU(2), you're making a choice of how to do it. Standard thing is to identify R^4 with 2×2 complex matrices, identifying the time-direction as the unit matrix. Then the diagonal action leaves this invariant, but rotates the space directions. From this point of view I'm hoping to identify the anti-diagonal SU(2) with the weak SU(2). The details of this are in my old Nucl Phys. B paper.

41. **Lubos Motl**
March 1, 2005

Dear Fyodor,

your criticism is of course completely valid. On the other hand, mixing up one of the SU(2)s with the diagonal SU(2) looks like an innocent idea compared to Peter's main proposal that the other part of the Euclideanized Lorentz symmetry is actually the SU(2) electroweak symmetry. 😉

With such Woitian proposals, anything goes.

Incidentally, in loop quantum gravity there is also a confusion about the self-dual vs. spatial SU(2) within SO(4). Originally the SU(2) gauge group of loop quantum gravity was derived as the self-dual SU(2), i.e. one of the factors, but eventually it's only kinematics that works, and therefore you're equally justified to say that the SU(2) used in the spin network constructions is actually just the spatial rotational group.

All the best
Lubos

42. **Fyodor Uckoff**
March 1, 2005

The subgroup of Spin(4) corresponding to rotations is the *diagonal* SU(2), is it not? So I'm not real sure what you mean by

one of the two SU(2)s in Spin(4) being Spin(3), the spatial rotations

43. **Peter**
March 1, 2005

For a biography of Gian-Carlo Wick:

http://books.nap.edu/books/0309066441/html/333.html

Sorry Lubos, it just can't be helped that you're more entertaining than Wick Rotation.

44. **Lubos Motl**
March 1, 2005

That's pretty interesting. Peter's article would suggest that the people would discuss the Wick rotation, but on this blog it's not such an interesting topic. So the participants discuss the first interesting topic related to the Wick rotation that happens to be Lubos Motl. 😉

Does someone know who Wick was or is?

45. **Peter**
March 1, 2005

Hi Robert,

The way in which Witten's N=2 twisted supersymmetry trick works to turn a QFT that is relatively close to the Standard Model into a TQFT, by mixing the internal and space-time SU(2) has always fascinated me. I suspect it's related to the idea I mentioned, but don't understand how. My general feeling about supersymmetry is that we don't understand its geometrical significance very well. If we did, we'd see that it was some "twisted" version of it, like Witten's TQFT, that was what is really interesting. Unfortunately I don't see how to use this specific TQFT to get what I'm looking for, maybe someone else will. Or maybe it requires some slightly different, but still unknown, construction.

46. **Arun**
March 1, 2005

If Lubos is the topic, then, in my opinion, he is best when he expounds on physics that he understands well; he is lucid and is a pleasure to read.

-Arun

47. **Matti Pitkanen**
March 1, 2005

A comment about possible connection of electroweak gauge group with Minkowski space that Peter is pondering. I hide the real background and just represent some observations, which I find intriguing.

a) If you construct stringy vertex operators in Minkowski space by the standard construction you have 2 transversal polarization degrees of freedom having interpretation in terms of a Kac-Moody algebra associated with some 2-D Cartan algebra. It extends by the standard vertex operator construction to SU(3) Kac Moody algebra. As if color group were somehow inherent to 4-D Minkowski space.

In fact, G_2 having same Cartan algebra is the maximal extension, but I have not managed to find whether the construction extends to the generators of G_2 representing its short roots (Olive et al argues that this is the case but do not give the construction in their 1984 article).

b) On the other hand, when you divide SU(3) by U(2) you get CP(2) having U(2) as holonomy group and couplings are just those of electroweak gauge group (once you couple spinors to Kahler potential to get a proper spinor structure, this was done already by Hawking and Pope). Isometry group is of course SU(3) and CP_2 spinor connection codes for standard model symmetries.

These observations lead you to ask what do you get if you construct SU(3)/U(2) coset theory. U(2) Kac-Moody would act precisely like electroweak gauge group in this theory. Is this theory analog of an electroweak gauge theory with symmetry broken down to U(2) and constructed using solely strings in M^4? Or could one somehow generalize the theory to get also color multiplets? In any case, spin (transverse polarizations) and "color", "ew" qnumbers, and spin are very intimately related in this picture.

Matti Pitkanen

48. **DMS**
March 1, 2005

Peter,
I am sure I am completely wrong here, but is the idea (of the two SU(2)s in Spin(4)) proposed similar to "twisting" used by Witten to derive the new invariants for 4-manifolds based on his Seiberg-Witten on N=2 SUSY?

PS1: Yes, the contribution to your earliest posts by two senior physicists are amusing reading 🙂

PS2: Although a recipient of a few LM insults myself, I actually feel he is being misunderstood and needs help. But in the hyper-aggressive/macho string community, such talk is likely verboten. I do hope some senior guys (whose opinion he respects) have the decency and humanity to help him out of whatever is bothering him (the example of the mathematicians' help to Nash comes to mind).

Often, when LM talks about QFT or strings (and not busy insulting people) he can be quite lucid (if too verbose, at times). And he does take time to answer some basic questions on physics from people, something you don't see from similarly capable physicists (ok string theorists :)).

49. **Robert**
March 1, 2005

This interpretation of the the left handed part of the euclidean Lorentz group sounds a bit like a topological twist: There (in the D=4 version) you start with a N=2 theory that has a SU(2) R-symmetry and swap the roles of SU(2)_left with SU(2)_R. By this trick, one of the supercharges becomes a scalar and can be used as a BRST operator. The upshot being that the BPS-states become elements of the BRST-cohomology and thus physical states. Is there a relation (given your background in TFT)?

50. **Matti Pitkanen**
February 28, 2005

Lubos Motle gave a nice summary about Wick rotation. An approach inspired by the conviction that the difficulties of path integral approach reflect deeper problems of principle is discussed in the mini article which I titled "How to put end to the suffering caused by path integrals?" at http://matpitka.blogspot.com/ .

51. **Peter**
February 28, 2005

I saw that too, and suppose he is tired of Lubos's rants and the nonsense they generate. Funny that he should have more trouble with Lubos than me. I guess it's because I don't engage in political commentary here, so Lubos and I have only one topic to disagree about.

52.
February 28, 2005

just read on preposterous universe that sean banned lubos from his weblog. is he serious?

53. **Peter**
February 28, 2005

The main thing that is weird about this idea is that you don't have the full Lorentz symmetry. You have in some sense picked a time direction, which determines an SU(2)=Spin(3) subgroup of Spin(4), which will be the spatial rotations, which are not spontaneously broken. The weak SU(2) acts non-trivially on this choice of time direction, which behaves somewhat like a Higgs field, perhaps spontaneously breaking the weak SU(2). (But still, I haven't written down dynamics that does this).

54.
February 28, 2005

One obvious problem would seem to be that the electroweak gauge symmetry is spontaneously broken, while Lorentz symmetry is not. How would you get around this?

55. **Peter**
February 28, 2005

Gravitons aren't quanta of the spin connection. With the standard Lagrangian, the field equation for the spin connection determines it in terms of the vierbeins. It's the vierbein fields, or equivalently the metric, whose quanta are the gravitons.

I wrote a paper about this "Euclideanized boosts=weak SU(2)" idea many years ago

Nucl. Phys. B303, pg. 329, 1988

but I certainly know a lot more now than I did then, and should write an updated version someday.
For one thing that paper wasn't even written in the context of QFT, just of a single-particle model.

56.
February 28, 2005

If one of the Wick rotated SU(2)'s happens to be the electroweak SU(2), this would explain why only left handed fermions interact with the weak interaction, but but wouldn't it also mean the W and Z bosons are gravitons?

57. **D R Lunsford**
February 28, 2005

I've never seen any evidence that LM has a clue about anything at all.

-drl

58. **D R Lunsford**
February 28, 2005

Peter – well said again, I agree in detail that the real problem in physics is the relation of QFT to actual geometry.

Have a look Finkelstein/Jauch, "Quaternion Quantum Mechnics", which may be related to what you are "selling".

-drl

59. **Peter**
February 28, 2005

Distler is no slouch himself when it comes to "straw man arguments, willful misreading, and insults", although he is capable of more subtle insults than Lubos since English is his native language. Check out his contributions to some of the early postings of this weblog.

Pretty funny to see him and Lubos in action. Do you think it's statistically significant that the two most prominent string theorists with weblogs are both incredibly arrogant and incapable of admitting that anyone who disagrees with them might have a point?

60. **Fabio**
February 28, 2005

That LM discussion is pretty interesting, especially the part where Jacques Distler tries to make a point, runs up against the trademark Lubos Motl mixture of straw man arguments, willful misreading, and insults, then ultimately decides it's not worth the bother.

61. **Alejandro Rivero**
February 28, 2005

Wow! Welcome to marginality, Peter!

2004 TopCites

March 3, 2005
Categories: Uncategorized

The SPIRES database is used each year to produce a list of the most frequently cited papers in particle physics. This year's list has appeared, although the usual annual discussion of the list from Michael Peskin still hasn't yet. The trends I commented on last year in the 2003 list are even more pronounced this year.

The top ten most highly-cited papers in particle physics are now dominated by experimental results in astrophysics and cosmology with five papers in this category. Particle theory is represented by three large extra dimension papers from 1998 and 1999, and a single string theory paper, Maldacena's 1997 article on AdS/CFT. The Maldacena paper is now the fourth most highly cited particle physics paper of all time, surpassed only by citations of the Review of Particle Properties, Weinberg's 1967 paper, and the 1973 Kobayashi-Maskawa paper.

Even more so than last year, this data shows that particle theory and string theory flat-lined around 1999, with a historically unprecedented lack of much in the way of new ideas ever since. Among the top 50 papers, the only particle theory ones written since 1999 are a paper about pentaquarks by Jaffe and Wilczek from 2003 at number 20, the KKLT flux vacua paper at number 29 and a 2002 paper on pp waves at number 32.

How many more years of this will it take before leaders of the particle theory community are willing to publicly admit that there's a problem and start a diiscussion about what can be done about it?

For some other interesting statistical data gathered from this database, check out the SPIRES playground.

One relatively recent idea that probably hasn't fully shown up yet in the yearly citation counts is Witten's late 2003 idea about relating gauge theory and the topological string in twistor space. While the idea of working in twistor space has lead to a lot new results about gauge theory amplitudes, Witten's original hope of relating gauge theory and string theory seems to be in trouble.

Comments

1. **Thomas Larsson**
   March 8, 2005

   Everyone would like to know what can be done about it. Of course, the right thing what to do about it is to discover new revolutionary ideas.
   Do you know how to do it?

   What about diff anomalies in 4D? At it should be new, since Weinberg (chapter 22) claims (correctly, within his axioms) that no such things exist.

   But diffeomorphisms generate a gauge symmetry, and you have repeatedly stated that the right approach to gauge anomalies is ignorance. Just tell me one thing, though. How can you know that there is something special about the bosonic string in 26D if you don't know about conformal (gauge) anomalies? Or, why is it ok to be ignorant about the quantum reps of the constraint algebra of GR, but not about the reps of the Virasoro algebra? After all, both GR and the bosonic string are constrained Hamiltonian systens.

2. **Lubos Motl**
   March 7, 2005

   I agree with Peter's statistics.

   As far as I know, everyone knows – and admits – that the high-energy theoretical physics is in a quiet period.

   Everyone would like to know what can be done about it. Of course, the right thing what to do about it is to discover new revolutionary ideas.

   Do you know how to do it? Let me just warn you in advance that the Dirac operator is not a revolutionary idea anymore. 😉

3. **Geoffrey Haselhurst**
   March 7, 2005

   Hello,
   I saw reference to a webpage of mine on Cosmology.
   I think in your discussion of a career in Physics and the censorship inherent in the system, that the Internet is now changing all the rules for the evolution of cultural knowledge.
   Plus there is a very simple sensible language for describing reality. I can't imagine this being ignored for that long. So I think physics will blossom in next 20 years, along with philosophy.
   Thoughts?
   Geoff Haselhurst
   http://www.spaceandmotion.com/contact-email.htm

4. **Peter**
   March 4, 2005

   I'm deleting the last few comments responding to Pedro about grad school and where particle physics is going since this discussion is getting both off-topic and there are some people who just can't stop themselves from being hostile and obnoxious, others who find it hard to ignore them.

   His question is a difficult and legitimate one. If people have some useful, non-hostile, advice about what the current situation means for people thinking about grad school in theoretical physics, go ahead and comment. But stop it with the obnoxious off-topic abuse.

5. **quantoken**
   March 4, 2005

   Petro:

   You are going to have a miserable life with little achieved, if you want to get into the field of theoretical physics research. As the state it is it is unlikely going any where until something is changed. You can not afford to waste your life on a pursuit that every indications show it's not going any where within your limited lifespan.

Please read this:

http://infoproc.blogspot.com/2004/12/dont-become-scientist.html

And read this:

http://wuphys.wustl.edu/~katz/scientist.html

And this, too, to open your mind a bit:

http://www.suppressedscience.net

And this, to see what exile scientists are like:

http://www.spaceandmotion.com/Cosmology-Big-Bang-Theory.htm

It is completely possible to make a **much easier and more comfortable living** on something more connected to reality, while at the same time pursuit some interests in fundamental physics research, with total freedom of mind, like I do. Einstein was a very young (26) amateur "crackpotist" who barely graduated from college, and would be unable to publish anything on today's ARXIV, when he made some of the greatest discovery in human history, within a period of just one year. Relatively, his professional career in his second half of lifetime achieved virtually nothing. See my blog:
http://quantoken.blogspot.com

Quantoken

6. **Alan**
March 4, 2005

I know that Peter W. is a stickler for keeping these discussion threads to the topic at hand (in this case, citation counts and whether they're a valid measure of the intellectual vitality of ideas), although in this case he appears to have let the thread veer off in some other directions.

For Pedro, who inquired about directions to take in graduate training, I would highly recommend the "Academic and Career Guidance" board at http://physicsforums.com . A direct link to the academic board is:

http://physicsforums.com/forumdisplay.php?f=139

In particular, there was an interesting recent thread comparing different graduate schools:

http://physicsforums.com/showthread.php?t=65634

7. **Wolfgang**
March 4, 2005

Pedro,

as Peter has pointed out on his blog several times before,
there are many interesting questions open in physics apart from quantum gravity or string theory.
If I were young, I would focus on those areas.

8. **pedro**
March 4, 2005

That is quite true – and by no means am I looking for a conciliation between competing approaches just for the sake of it. Rather, given the absence of significant empirical evidence towards either string theory or its 'main' competing theories and the fact these theories are, at the moment, incredibly open-ended; while at the same time, given the attractive 'transempirical' results/content of such theories, what are presently, if any, mathematically consistent and 'physically sound' alternatives to them (which could, perhaps, incorporate appealing elements of both fronts)? Or better, assuming there *is* a problem with current particle theory, *what* would be its tentative solutions? (either way, I grant you these are probably incredibly naive questions)

9. **Wolfgang**
March 4, 2005

Obviously I meant "view points" not "few points"

10. **Wolfgang**
March 4, 2005

Pedro,

I am afraid physics is not so much about "conciliate" different few points and rather about
finding and weeding out wrong assumptions and hypothesis.
The main tool to eliminate wrong ideas (experimental evidence) unfortuantely does not work so well due to the high energy scale at which quantum gravity becomes important. But this could change overnight if somebody discovers evidence in favor of one idea or the other.
I am afraid that there are no "safe bets" one way or another and I am afraid there is no good advice for you in this situation other than to work on ideas which may also apply to areas where empirical evidence is available …

11. **Pedro**
March 4, 2005

Hi,
I guess this is going to bring the tone of the current discussion down a bit; but since the ongoing argument seems to touch on something that has been bothering me for a while, thought of posting my concerns here.
I'm a physics undergraduate student, hopefully soon to embark on graduate studies, and, given the apparent polarisation of the 'unified theory' theoretical physics research into string theory and quantum gravity, I have to say I'm quite torn between which direction to follow.

Now, from my current quite limited perspective, it seems that both 'areas' are generally problematic: while a lot of the quantum gravity approaches (I'm thinking, slightly more specifically, of the 'causal' approaches) seem to provide a nice 'intuitive' picture/potential unification of QG and GR, say, they are still quite far (but I could be immensely wrong here!) from properly tackling the behaviour of particles and their interactions (I have the impression a lot of research is done on the structure of 'empty' space-time only); string theory, on the other hand, while growing out of theoretical particle physics research and despite all its mathematical 'triumphs', not only paints a very odd picture of the universe, but is also a long way from connecting to current 'experimental' particle physics – is that the case? If so, is there any current research projects attempting to perhaps conciliate this polarisation?
Lastly, assuming your conclusion from the 2004 TopCites is correct, and there really is a big problem with current particle theory/string theory, what are the viable alternatives, or one candidate for an alternative be? (and this is a genuine question – I don't mean to sound agressive).

12. **Peter**
March 4, 2005

Hi Wolfgang,

I agree with you that work on topological strings is the most interesting thing going on these days in string theory. Problem is, this is a case where people who say that string theory is "math, not physics" are right. There's a lot of interesting mathematical work going on in this area, and if SPIRES also included pure math papers, maybe Vafa's papers would get bumped up into the top 50.

I do disagree with you about 1990 though. At that time Witten's Chern-Simons theory and TQFT stuff had just come out, things which I think are far more interesting both for math and for physics than the topological string ideas of recent years. Of course topological strings have their origin in precisely this work of Witten's.

13.
March 4, 2005

Regarding citations of the Weinberg's '67 paper, physicists did not know then if spontaneously broken YM theories were consistent or renormalizable; it was a 'conjecture/belief'. Soon after 't Hooft (and Veltman)'s brilliant work on renormalizability of YM theories was understood (circa '71/'72), the citations on the Weinberg paper naturally picked up.

14.
March 4, 2005

"The NSF and DOE should stop giving grants to people who intend to pursue a dead idea that doesn't work."

Funding system works by innovating over ideas we known. The risk about calling for a "stop" is that there is no guarantee of getting funds for new ideas; just an increase of funds for the other old competing ideas.

An interesting system could be that each university had the right to claim a percentage tax on each group "justified research" funds in order to fund "unjustified research" of every tenured professors, without restriction. That should be a return to academic freedom and perhaps a way to get new ideas into the game.

15. **wl**
March 4, 2005

>
If either you or Matthew have a better objective measure for the number of new ideas coming out of particle theory that many people think are interesting enough to work on during a given period, let's hear it. I think the one I have given is pretty meaningful, and agrees with the perceptions of most working theorists.
>

Well I do not agree. For example, the things that happen in topological strings right now are in my opinion totally fascinating and meaningful. And more so than eg 5 years ago – there has been lots of substantial progress in this field, in contrast to what you seem to convey.

That these papers do not collect as many citations as others, like those on flux compactifications, is quite irrelevant. For example, it reflects how many people are working in the field, how many are able to follow the subject and make contributions, how difficult it is to write papers in this given field.

These are all sociological factors and there are others, eg it is also matter of taste what people find interesting/promising/rewarding. So one cannot draw general conclusions such as yours, namely that string theory would be declining, from such crude data. On the contrary, I find it an especially interesting time, definitely much more interesting than eg around 1990. I remember having heard similar statements at the time, and indeed physics was pretty boring then in comparison to today.

16. **DMS**
March 4, 2005

"Well, Michael, …"

Oops, that was Mathew, not Michael. Sorry.

Well, I do not have a problem with phenomenological attempts, like the Little Higgs models, that can be tested/falsified. It is amazing how many models can be discarded from precision measurements, which is a good thing.

This is very different from "superstring phenomenology", the TOE approach. Supposedly, a "unique" string theory leads to so many possibilities that it is not at all predictive. In what way is it more predictive than an arbitrary QFT, when it comes to being testable experimentally? The "bottom-up" approach is more meaningful when it comes to phenomenology, IMO.

But I think there is no compelling reason to believe any of these models are correct (unlike say GIM which predicted existence of charm and even a rough estimate of its mass based on Gaillard-Lee (?)'s famous calculation).

17. **Peter**
March 4, 2005

Hi Wolfgang and Matthew,

I often say things on this weblog that I know relatively few people in the field agree with me about, but I'm finding it strange that you object to this posting, because I don't think the point I am making is controversial at all among working particle theorists. Without exception, everyone in the field I've discussed this with privately in recent years, string theorist or non-string theorist, feels that the last few years has been a period of unusually few interesting new ideas. It's true that I mostly talk to more mathematically-minded theorists, perhaps more phenomenologically-minded ones don't so much feel this way.

For Matthew:

I guess you've only been working in this area for a few years, so I don't think you have first-hand experience about what the particle theory research environment was like 10,15,20,25 years ago and how different things were then. I encourage you to ask your more senior colleagues how they see the activity of the last five years as compared with earlier periods in their careers, especially consulting any string theorists you may know.

I also looked at the 94-96 topcite data, found 3 theory papers in the top 10, (4 in the top 11), and 12 in the top 50 (I think you missed Hull and Townsend, one Isgur-Wise, and a parton model paper). I think that it is undeniable that

1. 0 is less than 3 (number of papers in top 10)

2. 3 is less than 12 (number of papers in top 50)

and that these differences are statistically significant.

I'm not going to waste my time anymore today trying to get you to distinguish the difference between the way you're using the word "prediction" and the way most scientists in the world understand it.

Wolfgang,

The list I linked to orders papers by how often they were cited during 2004. For a paper to be in the top 50, it would have to have been cited by 155 or more papers written during 2004. None of Vafa's papers satisfy that criterion.

If either you or Matthew have a better objective measure for the number of new ideas coming out of particle theory that many people think are interesting enough to work on during a given period, let's hear it. I think the one I have given is pretty meaningful, and agrees with the perceptions of most working theorists.

18. **DMS**
March 4, 2005

Well, Michael, Peter has answered the "predictive" aspect of the brane models better than I could have. I suppose it depends what the meaning of the word "prediction" is; I am old-fashioned in that regard.

Aaron, Thanks for your comments and pointing out the Strassler-Klebanov paper.

On the Little Higgs models, (according to hep-ph/0502066), the initial hope that such models (at the least the popular ones) have less fine-tuning than the MSSM (already needing fine-tuning at 2% level) from precision electroweak constraints, may not be valid.

19. **Matthew**
March 4, 2005

Hi Peter,

I don't doubt you can find papers about brane models containing the word "prediction", but I do doubt you can find an actual prediction in any of them.

The paper I cited has acutal predictions in it.

None of these models is capable of making a real prediction that could falsify the brane world idea. For one thing, the energy scale of these things is completely undetermined (other than that it better be big enough so that you wouldn't already have seen something).

Well, I think technically this is true. But from a model building perspective, it's not. You construct a model to solve certain problems. If the energy scale is too low, then it's already rule out. If it's too high, then you no longer solve the problem you set out to solve.

What a legitimate "superstring phenomenology" should do is tell us what the size and properties of the brane will be.

Well, that would be nice, but since the string theorists haven't delivered said predictions yet us "mere mortals" build models.

The papers you quote are more along the same lines. They say something like: if we choose a Little Higgs Model with properties A, B, C, it has a particle X whose mass we know nothing about other than that it better not be too large (and we don't really know exactly what too large means).

Not quite. They say if I build a model with these parameter/this structure then it predicts X. Now of course you can change the parameters, but that's true of *any* model. Some models allow more flexibility on this point than others.

As you note, it is very hard to figure out how to distinguish such a model from anything else.

So we should not build models at all? Look, part of the problem of distinguishing models is because the LHC is a messy hadron collider. Since we can't change that, we make do as best we can.

Does any Little Higgs model actually predict the mass of anything?

Sure, pick the parameters, and the model is predictive. Not unlike the standard model. The second little higgs paper I linked has a plot of the new spectrum, for a certian choice.

I wasn't saying that there are fewer particle theory papers these days than ten years ago, I was saying there are many fewer that people think are worth citing. This indicates there are virtually no new (good) ideas coming out of the field.

And this is where I disagree. You're linking citations with "new ideas" is wrong. Nobody cited Weinberg's 67 paper for several (5?) years. Yet it was a good idea.

Look at the SPIRES topcites list of ten years ago, and you'll see lots of recent theoretical papers,

Okay, I did. The 94-96 top 50 list has 9 theory papers published from 1990 on. Two (Amaldi, Langacker) have do do with LEP, two (Isgur, Georgi) with heavy quark effective theory and one (Lepage and Mackenzie) with lattice field theory. Of the other four, two are Seiberg and Witten, one is Witten by himself, and one is Polchinski. Safe to say, this is not a hive of new ideas of what's just beyond the standard model.

20. **wl**
March 4, 2005

What's the logic of that spires list ? I know many famous papers
which have hundreds of citations, in particular have more citations than certain
papers which do appear in the list and which even are more recent. Eg do in Spires
http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=find+a+vafa&FORMAT=wwwcitesummary&SEQUENCE=
however not a single paper of Cumrun's appears in the list you cite.

I don't see how any conclusion, esp on an alledged "flatness" of how string theory evolves, can be drawn from this.

21. **Quantoken**
March 4, 2005

Peter,

Thanks for providing the URL to the paper you wrote several years ago. It is because of honest people like you, rare nowadays but still exist in the science research community, who are brave enough to speak out the truth, that the public can still maintain SOME confidence in science.

No need to pay attention to Michael, who as DRL put it best, is just "pissing in the wind". Some of his comments are worse than vulgarity. Shame for him that his "It just doesn't suit you well" comment kind of tried to intimidate you to shut up and mind your own business and stop criticize string theory, or else. It's not the kind of language an educated person will utter.

You comment of cutting funding is right on the spot! One has got to understand scientific researches are funded by tax moneys from the general public, and therefore it should serve the interests of the wellbeing of the public only. How much longer should the public continue to allow their tax money to be wasted on such totally fruitless pursuit?

Cut the damn funding now! String theoretists certainly are totally free to continue their fruitless pursuits, but on their own money, not the public's money. Or, like Lee Smolin did, go talk to some rich guy and convince him that your theory makes sense to him, so he can fund you.

Frankly I think the LHC project now is just a waste of money. Since no one is willing to make any definite prediction beforehand. And you are all waiting for the machine to start running, and then you can outfit your theory with whatever comes out of it. So at the end of day your theory will still be un-falsifiable but still useless and unable to make predictions. What is the use of experiments if it can not help falsify some of the theories and confirm some others?

Quantoken

22. **Aaron**
March 3, 2005

To Peter — If they don't find SUSY at the LHC, a lot of people will give up on weak scale SUSY definitely. Not because of fine tuning, but because if it's not there to stabilize the weak scale or to unify the couplings or provide a dark matter candidate, there's not much reason for it to be down there at all. High scale SUSY is what it is, but it's not particularly of experimental import.

To JC — The MSSM is actually a very pretty exercise. The reason people got excited about it wasn't because it was some slavish following of stringy fashion. It stabilized the weak scale, it provided a natural mechanism for electroweak susy breaking and it seemed to unify the couplings. There were good reasons to call it the best candidate for an extension to the standard model (other than neutrino masses). The problems really come when you break the supersymmetry. It's the effective lagrangian for the broken susy that has the 109 or whatever parameters that people always refer to when denigrating susy. But those aren't free parameters; they just depend on an undetermined model for breaking the supersymmetry. That's where things start to get ugly.

Split supersymmetry is pretty straightforward, at the cost of some major fine tuning (which Nima and co would like to attribute to some anthropic hand waviness). You just break susy at some high scale. Thus, most of the SUSY partners are very massive, resolving all the difficulties with FCNCs, proton decay and the like. Then, you have the gluinos be at the weak scale. Since they're chiral, this is technically natural (but not plain old natural). They give you some nice dark matter candidates and unify the couplings. Assuming I got all the details right, at least.

To Tom — you can have nonconformal stuff in AdS/CFT (oxymoronic, I know.) The most famous example of this is the Klebanov-Strassler solution, hep-th/0007191.

23. **TripleIntegral**
March 3, 2005

In re: the post's comments on leadership from the top particle theorist community, it seems to me that in his public statments Witten has at least recently been fairly honest about hopes for string [whatever] theory and its accomplishments and prospects. Others less so. For example, Susskind seems to be his own type of legacy-enhancing pathology. The small fry need someone's skirts to hide behind and crumbs to lick off the floor. Thus has it always been.

24. **JC**
March 3, 2005

Split SUSY and some of the SUSY models beyond MSSM, sure seem like the equivalent of "Rube Goldberg" machines in the particle phenomenology world.

25. **Tom Larsson**
March 3, 2005

AdS/CFT is interesting because it holds out some hope of producing strng theory duals to 4d QFTs you care about, like QCD.

My point was that dS/non-C QFT would be a lot more interesting duality, since it might have something to do with the real world. AFAIK, QCD is not conformal, although classical chromodynamics is.

PS. When I said that Nair was a string theorist, I might have confused him with Narain.

PPS. This is my first night with broadband. My oldest daugther has just reached the Internet chat age, and the telephone bills started to become scary.

26. **Peter**
March 3, 2005

What's the gluino mass bound? Is it within the reach of the LHC?

If the LHC really could rule out split supersymmetry, as well as regular supersymmetry since the fine-tuning required is implausible, will people really give up on supersymmetry for good?

27. **Aaron**
March 3, 2005

Split supersymmetry was born out of anthropic nonsense, but by itself, it's just a fine-tuned model that has some straightforward signatures. Split supersymmetry has most of the fermionic partners to avoid issues with FCNCs, proton decay and the like. It has the gluinos (IIRC) light, however, so as to give coupling constant unification. You either see that or you don't. If you don't, split supersymmetry's wrong. Simple as that.

28. **Peter**
March 3, 2005

Isn't split supersymmetry even less predictive than standard ideas about supersymmetry? Supersymmetry was supposed to eliminate the need to fine-tune the Higgs mass, which meant that superpartners couldn't be too massive.

In split supersymmetry, you decide to throw out the main motivation for supersymmetry and trust in God or the anthropic principle to explain why the Higgs is so light. The only thing you're left with is the (not quite accurate…) coupling constant unification. Perhaps if you want to keep this you get some constraint on how heavy your fermionic superpartners can be, but this is a long way from a definite prediction of anything.

29. **Aaron**
March 3, 2005

Split supersymmetry, at least, is a predictive fine-tuned model. If they turn on LHC and don't see what it predicts, it's dead. Coupling unification is a assumption of the model, and thus you need some lightish fermions to achieve it.

30. **Peter**
March 3, 2005

Hi Matthew,

First of all, just saw your response to DMS. I don't doubt you can find papers about brane models containing the word "prediction", but I do doubt you can find an actual prediction in any of them. None of these models is capable of making a real prediction that could falsify the brane world idea. For one thing, the energy scale of these things is completely undetermined (other than that it better be big enough so that you wouldn't already have seen something). Saying that "if there's a brane with this size and these properties I predict the LHC will see X" completely begs the question. What a legitimate "superstring phenomenology" should do is tell us what the size and properties of the brane will be.

And by the way, I (and I suspect many other people here) do read hep-ph.

The papers you quote are more along the same lines. They say something like: if we choose a Little Higgs Model with properties A, B, C, it has a particle X whose mass we know nothing about other than that it better not be too large (and we don't really know exactly what too large means). The problem with this is that you can get an extremely wide range of different things by changing A,B,C and choosing all the undetermined parameters and masses. As you note, it is very hard to figure out how to distinguish such a model from anything else. I said none of these classes of models give a "definite picture" of what will happen at the LHC and I stick by that. By a definite picture, I mean something like actually predicting the mass of a particle. Does any Little Higgs model actually predict the mass of anything?

I wasn't saying that there are fewer particle theory papers these days than ten years ago, I was saying there are many fewer that people think are worth citing. This indicates there are virtually no new (good) ideas coming out of the field. I don't think it is at all helpful to try and deny this in face of strong evidence. Look at the SPIRES topcites list of ten years ago, and you'll see lots of recent theoretical papers, few of which have anything to do with LEP data. The difference then was that people were still regularly coming up with new ideas about string theory that other people thought were promising enough to work on and to cite. This has stopped happening.

31. **rob**
March 3, 2005

There is one factor that has been overlooked. Spires has recently added the astro-ph to it's collection of papers. It is not surprising that all those astronomy papers cause cosmology papers to have a higher number of citations than they had in the past.

32. **Matthew**
March 3, 2005

DMS

Do you (or anybody else here) actually read hep-ph?

I know enough about the superstring "phenomenology" (like the wonderful brane "models") to know that they are not useful for particle pheonomenology and definitely not predictive.

The very first paper returned on a hep-ph abstract search for "collider brane" is hep-ph/0502031,

"In the context of an universal extra-dimensional scenario, we consider production of the first Kaluza-Klein electron positron pair in an e^+e^- collider as a case-study for the future International Linear Collider. The Kaluza-Klein electron decays into a nearly degenerate Kaluza-Klein photon and a standard electron, the former carrying away missing energy. The Kaluza-Klein electron and photon states are heavy with their masses around the inverse radius of compactification, and their splitting is controlled by radiative corrections originating from bulk and brane-localised interactions. We look for the signal event $e^+e^- +$ large missing energy for $\sqrt s = 1$ TeV and observe that with a few hundred fb^{-1} luminosity the signal will be hard to miss since standard model backgrounds remain well within control. We also comment on how this signal can be distinguished from similar events from supersymmetry."

My simple search pulled down 78 hits, with some more work I'd bet you can find over hundred papers on the predictions of various brane models.

33. **Matthew**
March 3, 2005

Hi Peter,

If you look at the SPIRES topcites data from ten years ago, you can see that things were very different than now

Yes, things were very different, LEP was new and taking data. There was experimental data to work with. I'm not arguing that the number of theory papers isn't down, just that this represents a problem. I'm much more inclined to think that this will be par for the course in the era where there is one experiment at the cutting edge at any one time.

It's not a problem though, when the LHC turns on, balence will be restored 🙂

Also, one has to couple in the massive amount of cosmology growth in the past ten years. That's a whole new set of papers that impact HEP citiations in a new way.

The new ideas you mention, like split supersymmetry, aren't getting a lot of citations because they aren't very promising.

There no more or less promising than any other new idea. That's the point.

They don't really explain any known experimental result

Sure they do. Most of them explain why the Higgs mass is not at the planck scale. Others attempt to explain the ferimon mass hierarchy. Now you might not think that the explanations given are good or sensible, but that's a different arguement.

or give definite predictions about what future accelerators will see

Sure they do. Some of the people here are engaged in making these predictions. Indeed, it's an active field of research trying to figure out what one should look for in order to distinguish different models, many predict very similar signals at the LHC.

If one of these ideas actually gave a definite picture of what will happen at the LHC, it would attract a huge amount of attention and number of citations.

Again, this is simply untrue. Any model builder worth his or her salt will give a picture of what will happen at the LHC (the level of detail depends on the model builder). For example hep-ph/0402037,

"We discuss possible searches for the new particles predicted by Little Higgs Models at the LHC. By using a simulation of the ATLAS detector, we demonstrate how the predicted quark, gauge bosons and additional Higgs bosons can be found and estimate the mass range over which their properties can be constrained."

or, to highlight some colleagues, hep-ph/0411264,

"We begin the study of the LHC phenomenology of the littlest Higgs model with T-parity. We find that the model offers an interesting collider signature that has a generic missing energy signal which could "fake" SUSY at the LHC."

Work like this appears for all sorts of models, the collider phenomenology of the MSSM, for example, is worked out in massive amounts of detail.

I think phenomenologists are in a very tough spot and will be for at least the next few years, through no fault of their own. I don't think there is much that can be done about this until there's new experimental data.

My own observations are different. Phenomenology has been getting more active over the last few years. The difference between when I started as a grad student, and now, is clear. There are more new ideas, and more people exploring them.

34. **Peter**
March 3, 2005

Hi Thomas,

AdS/CFT is interesting because it holds out some hope of producing strng theory duals to 4d QFTs you care about, like QCD. So, if you're a bit optimistic, it may help you solve QCD. But it has really nothing at all of any use to say about the idea of an 10/11 d string/M theory TOE.

35. **Thomas Larsson**
March 3, 2005

Anyway, your case it not convincing. String theory has evolved in phases, often referred to as "string revolutions". I'm not sure if Maldacena's ADS/CFT qualifies as a revolution, but I perceived it as one.

I find the enormous interest in AdS/CFT somewhat curious, since I thought that both AdS (negative cosmological constant) and CFT (absense of massive particles) were ruled out experimentally. And in order to establish the connection between these experimentally falsified ideas, you probably need SUSY, which itself has *a lot* of problems with observation (or lack thereof). But perhaps I'm just old-fashioned to believe that physics has anything to do with nature…

36. **DMS**
March 3, 2005

Michael,

Although I do not understand D'Hoker-Phong's brilliant achievements, I know enough about the superstring "phenomenology" (like the wonderful brane "models") to know that they are not useful for particle pheonomenology and definitely not predictive.

It may be sufficient for you what the "experts" say/think, it is not sufficient for me.

37. **Peter**
March 3, 2005

Hi Michael,

I've exchanged e-mail with Witten about this, so know exactly what his circumstantial evidence is, and think it is extremely weak. I have an incredible amount of respect for Witten, both for his talents and for his accomplishments. By far the best argument for string theory is that he believes it, but I happen to think he's wrong about this. What's superstitious is to just accept what someone says without evaluating the evidence for yourself.

What you quote Maldacena as saying was actually said by Seiberg. I don't think joking about how arrogant you are is really "self-criticism". Far too many string theorists are incredibly arrogant, and while some of them would admit this I don't notice any of them thinking there is anything wrong with it.

38. **Peter**
March 3, 2005

Hi Matthew,

First of all, I think there's a straightforward objective issue here: when was the last year in which, if you put together a list of the 50 most heavily cited particle theory papers, only a couple would date from the last five years? I'm pretty sure you would have to go back to around 1945, when WWII interrupted research for about 5 years. I don't know much about what things were like in the earlier part of the century, but I suspect you might have to go back to the 19th century to find another year in which this happened. If you look at the SPIRES topcites data from ten years ago, you can see that things were very different than now, and if anyone knows of a source of similar data from earlier periods I'd be interested to hear about it. Particle theory is in a new situation, quite unlike any other one of the modern period, and people should be thinking about what this means.

The new ideas you mention, like split supersymmetry, aren't getting a lot of citations because they aren't very promising. They don't really explain any known experimental result, or give definite predictions about what future accelerators will see. If one of these ideas actually gave a definite picture of what will happen at the LHC, it would attract a huge amount of attention and number of citations.

As I mentioned in a previous comment, I think phenomenologists are in a very tough spot and will be for at least the next few years, through no fault of their own. I don't think there is much that can be done about this until there's new experimental data. Whatever overhyping of ideas that is going on amongst phenomenologists is relatively harmless.

String theory is a very different story….

39. **Michael**
March 3, 2005

Hi Peter,

what you say sounds reasonable under the assumption that string theory is not worth pursuing. This assumption is what's in question, and I submit to you that it may not be for you to decide. The big figures in string theory have a lot better grip on this than you do. If you listen to them, they are very reasonable and smart people who do not rush to any conclusion — like you do.

Witten for example states that it's his personal opinion that there is circumstatial evidence that string theory is on the right track. How could you possibly argue with this without being superstitious? Let me tell you: you can't.

Maldacena says that string theorists are arrogant enough that whatever comes up in their research, they will call it string theory.

So you can see that these people show a measured amount of self-critcism. At the same time they are the ones who do prove that string theory *is* worth pursuing, even if we can't be sure at this point what it's relation to nature is.

You have absolutely nothing to add to this. Can you be honest enough to admit it?

Your claims of intimidation and such are unsubstantiated and wrong. You have no evidence and you just make up such things because you know that in the absence of such claims your authority in this matter drops to it's true value, namely zero.

Do you find it attractive to sit in one boat with crackpots like quantoken (see quantoken.blogspot.com, if you have the stomach for it)? You have learned a number of serious things in math and you can make your little contribution to this field. Why waste it and bark with the crackpots instead?

Best, Michael

PS: DMS, you know enough string theory to arrive at a conclusion that is completely inconsistent with that of the experts in the field? Who are you kidding?

40. **D R Lunsford**
March 3, 2005

Michael – I've read your betters, and what you say is just a lot of pissing in the wind. I'm sure it suits the postmodern intellectual Narcissus in you, but you aren't fooling anyone.

-drl

41. **DMS**
March 3, 2005

Michael,

I know enough physics to know string theory is highly over-hyped. All papers on "string phenomenology" are total, pure, unadulterated garbage. The people practising it know little about precision electroweak physics. It has made *zero* predictions: no, consistency with anything is not a wonderful thing.

Of course, some of the mathematical work is very valuable, but that is a very small portion.

It also does not help when people practising string theory start bullying and throwing insults at people who point out the obvious.

So far, the paying public has a pretty high regard and respect for physics and physicists. But it will not take much (a couple of "landscape" books?) before they catch on to the gimmicks, and unfairly lump theoretical physics with some other fields, like climate science.

42. **D R Lunsford**
March 3, 2005

It was clear since “An Evaluation” that what you are doing is rather heroic, and it is just great to have someone with the balls to tell the truth on the planet.

-drl

43. **Peter**
March 3, 2005

Hi Michael,

I’d be much happier not to be spending my time doing this, but the problem is that no one else is. A lot of people are out there hyping the glories of string theory, but virtually no one who understands the theory has been willing to publicly criticize what is going on. Several people who agree with me have told me they won’t do this because they fear retribution: string theorists are on the panels deciding whether they get a grant and whether their students get jobs. There’s a really ugly atmosphere of intimidation going on here, including nasty personal attacks from people like you, Motl, Distler, and others on anyone who dares to criticize the string theory hype.

The people who should be doing something about what is going on, tenured people at major research institutions, for one reason or another aren’t doing it. When they start, I’ll shut up.

44. **Peter**
March 3, 2005

Hi Kyle,

I’ve addressed this in various places, including the first public thing I wrote on the subject four years ago:

http://www.arxiv.org/abs/physics/0102051

Here’s a summary of what I think needs to be done:

1. Publicly admit the problem. As long as leaders of the field go around giving talks and writing books about how well things are going, no one is going to be willing to change anything.

2. Publicly admit that the idea of string theory as a TOE is a failure and needs to be buried. More phenomenologically motivated approaches to particle theory are in trouble because of the lack of new data, and unless the Tevatron or someone else comes up with something unexpected, this situation will last until 2008, when maybe the LHC will change things. But more mathematically driven “top-down” research still has a chance of getting somewhere. Some radical new ideas are needed, but as long as it is extremely difficult to get a job doing mathematically oriented stuff unless you are doing string theory, hardly anyone is going to be working on this.

3. The NSF and DOE should stop giving grants to people who intend to pursue a dead idea that doesn’t work. Once the community admits the problem, the NSF and DOE could play a role in promoting funding of proposals to try something new and different, discouraging funding of proposals to do the same old thing. As long as it is very hard to get a grant or a job working on mathematically sophisticated approaches to particle theory unless you are doing string theory, nothing is going to change.

What has been going on for far too long is that bright young people who come into the field end up spending years of their lives mastering the intricacies of string theory. They end up not knowing enough to do anything ambitious that isn’t string theory, and the reward system is such that even if they wanted to try they would soon be without a job. People have to think about how to change this reward structure. It’s not going to change until the underlying problem is acknowledged.

45. **Matthew**
March 3, 2005

My comment got a little long, so I posted a short response on my blog. In short, Peter, you’re wrong 🙂 Number of citations does not correlate with number of new ideas. Particle theory is acutally pretty active these days.

46. **Michael**
March 3, 2005

Hi Peter,

Sorry if I offended you too much. I didn’t mean to be what you called me.

Anyway, your case it not convincing. String theory has evolved in phases, often referred to as “string revolutions”. I’m not sure if Maldacena’s ADS/CFT qualifies as a revolution, but I perceived it as one. Understanding gauge theory using gravity and vice versa is extremely exciting if you keep in mind that gauge theories are proven to describe nature and gravity is what we need to understand better.

Are you worried about no more such major steps forward in the last 6 years? I doubt it. For one, your critisism has been around for several years, so you didn’t wait until nothing terrific had happened in 6 years. Also the time period between the string revolutions in the past was typically longer than that. The next may be just around the corner, and what are you going to say when it comes?

Peter, you are more outspoken about string theory than people like Ed Witten and Juan Maldacena. It just doesn’t suit you well. It is not humble and it is not honest on your part. You could earn a lot more respect by doing a good job at Columbia in the position you hold, talking about the things you do understand. Do you crave attention and publicity so much, even if it’s negative one?

Best,
Michael

47. **Peter**
March 3, 2005

Hi Michael,

I just presented some definitive evidence that string theory is intellectually dead. Instead of addressing this evidence all you can do is attack me personally. This is just pathetic.

Guess what? I’m not going to “knock it off”, no matter how many assholes like you I have to deal with.

48. **Alejandro Rivero**
March 3, 2005

I like to see Randall-Sundrum work (and Nima’s, and Antoniadis 1990 &c.) there in the top. It was clearly a provocative idea already back five years ago, and thinking pentadimensional is a funny thing, even if it is to go back to our beloved 3+1 dimensions Or 3+1+0 for a Connesian as I am.

It is funny that while the WMAP (what about the old presunt fractal behaviours, now?) replaces neutrinos in the top ten, the more quiet oscillation research has brough back a couple works of the past, Cabibbo 1963 and Maki, Nakagawa, Sakata 1962.

49. **Michael**
March 3, 2005

I know why string theory is in trouble! It’s because string theorists haven’t paid enough attention to the Woitian off-diagonal SU(2) embedding. Ignoring this crucial concept dooms any project in theoretical physics.

Knock it off, Peter, will you?! Your criticism is lame and repetitive.

I understand you must have dreamt of becoming a brilliant theorist until you found out that you are not capable of that. I know it hurts, but bitching about other people’s work for the rest of your life won’t help it.

Best wishes,
Michael

50. **Kyle**
March 3, 2005

(sorry, I was refering to the scentence that ends “admit that there’s a problem and start a diiscussion about what can be done about it?” in my comment below)

51. **Kyle**
March 3, 2005

If you’ve posted your opinion on what the solution is before Peter, could you link it? If you haven’t, or your opinion has changed, any chance you could lay out a reasonably concrete plan concerning it?

Thanks for your time,

Kyle

RSVP

March 4, 2005
Categories: Uncategorized

I recently mentioned that funding for the RSVP experiment is being reevaluated. More details about this are available in a recent issue of Science magazine.

The Rare Symmetry Violating Processes (RSVP) project is a proposed experiment at Brookhaven that would have two components. One, "MECO" would search for neutrinoless conversion of a muon to an electron, observation of which would indicate new physics beyond the standard model. The other component, "KOPIO", would try and measure the decay rate for neutral kaons to a pion, neutrino and anti-neutrino, a CP violating decay whose rate is predicted by the standard model.

Last fall the NSF had allocated money to start building the experiment, which was projected to cost $158 million. The idea was to use the AGS accelerator at Brookhaven, which in recent years has mainly been used as an injector for the heavy-ion collider RHIC. It seems though that revamping the AGS for use by RSVP may cost a lot more than people had originally thought, pushing the cost of RSVP up to as much as $300 million. The potential cost of RSVP is being reviewed, and HEPAP has been asked to evaluate the results that RSVP may be able to achieve at different levels of funding. According to Michael Turner, head of mathematical and physical sciences at the NSF, "We will reevaluate scientific value, its cost, and then make a decision."

Comments

1. **Arun**
   March 6, 2005

   Chris,
   Finding something precise experimentally beyond the Standard Model will wonderfully focus theorists' minds, IMO, and will liberate some of them from the thralls of string theory. We would have a precise term in the effective Lagrangian that has to be accounted for by theory.
   -Arun

2. **Chris Oakley**
   March 5, 2005

   Sorry to comment "on topic", but it is a general principle that if you want a precise answer you have to ask a precise question.

   Precise question, example 1: is there such a thing as luminiferous ether?

   Precise question, example 2: is there a narrow resonance in e+e- scattering at 3.1 GeV?

   And so on. Vague searching around for unexpected things does not fit this particular bill and is probably a reason to be pessimistic about the RSVP project. After all, the universe of possible unexpected things to look for is huge and it does seem slightly pointless to pick on two particular things forbidden by the standard model when no-one actually has a theory that could be brought into play if SM-violating events *are* found.

   What is more, with theory in its current state, I see no prospect of theorists being able to deliver any predictive, realistic alternatives to the SM.

3. **Peter**
   March 5, 2005

   Hi Lubos,

   Just wondering, are all the connections to my weblog from strings.*.*.edu and famoustheorist.physics.harvard.edu coming from stupid readers or very stupid readers?

   I also recommend that people in search of high-level intellectual discussion about Feynman check out Lubos's website. Just please keep that discussion over there amongst the much smarter readership.

4. **Lubo? Motl**
   March 5, 2005

   Feynman did not like string theory, so I suppose that both types of readers of this anti-string-theoretical blog – the stupid readers as well as the very stupid readers – will enjoy an article about Feynman here.

5. **Lubos Motl**
   March 4, 2005

   I personally don't know what we would do if we knew that mu can decay to e+gamma with some rate etc. We would know there is physics beyond the SM (which we know anyway), but what exactly it is would remain highly ambiguous.

   KOPIO would probably just confirm the Standard Model.

   But there are other people who may find this project very important, so let's not overestimate the importance of particular personal comments.

Higgs Search at the Tevatron

March 5, 2005
Categories: Uncategorized

Tommaso Dorigo of the CDF collaboration at the Tevatron has just posted (with commentary), the slides for his talk at Moriond later this month about the status of the search for the Higgs at the Tevatron. The bottom line is that with the data they have already analyzed they are still quite a ways from being able to see the Higgs, but, if its mass is just above the lower limit set by LEP2, they should be able to see it by two years from now. With quite optimistic assumptions about the performance of the Tevatron, by the end of 2009 they should be able to see the Higgs if its mass is less than 180 Gev. He ends by saying that at "95% confidence level" he thinks the Tevatron will be able to end up seeing a Higgs up to 135 Gev mass, and if its mass is just above the LEP2 limit at 115 Gev, they should have 3 sigma evidence for its existence.

By 2009, the LHC should be producing data and putting the Tevatron out of the Higgs discovery business. For a bewilderingly complicated schedule of the LHC construction and installation, go here. From what I can tell, they are still on track for first colliding beams in spring of 2007.

Update: See Tommaso's comment to this posting for a clarification. By "seeing the Higgs" I didn't mean to imply that they would be able to prove the Higgs was there, just that they would be starting to see some evidence of its existence.

Comments

1. **D R Lunsford**
   March 7, 2005

   "".

   Yes, that's the paper and the analysis is IMO very solid. One need not take the latter papers as seriously, however it should be mentioned that Penrose also points out how highly implausible it is that electrowesk symmetry breakdown should be the same everywhere in the Universe, as current comso. models assume.

   Note that his analysis is not at all controversial – he's simply pointing out the consequence of having the Higgs enter into the Lagrangian in just the way needed to supply masses to the gauge bosons, much as in the problem of a ball rolling on a surface as solved with Lagrange multipliers, where the multiplier term represents the "just right" normal force on the ball needed to keep it on the surface. There is also an obvious analogy in the BCS theory of superconductivity.

   -drl

2. **Alejandro Rivero**
   March 7, 2005

   Hello again… more magic from physicsforums, this morning. Just do this

   mu/mz+ mu^2/(114.5)^2.

   where mu= 0.105658369
   mz= 91.1876 +- 0.0021

   try some values around mz and compare with
   .001159652187

3. **Peter**
   March 7, 2005

   Hi Tommaso,

   Thanks for the clarification. My use of the term "seeing the Higgs" was intentionally ambiguous, meaning something like "seeing some evidence of the Higgs", not meaning "proving" the Higgs was there.

   Am I confused or is the situation the following: in 2007, if there is a 115 Gev Higgs, you hope to have a signal 2 sigma different than the null result, no? Do you then write a a paper with the title "Possible evidence for a 115 Gev Higgs" or are you more disciplined than that?

   I very much enjoy your weblog!

4. **Tommaso Dorigo**
   March 7, 2005

   Thanks for citing my blog here. However, we claim that in two years the Tevatron can probably _update_ the LEP2 _limits_, which is different from seeing it if it is there!
   It is typically easier, in fact, to exclude at 95% CL something, than to prove it at 5 sigma (95% is like two standard deviations).
   Same goes for the 180 GeV reach by 2009: we will probably be able to exclude it at 95% CL if we do not see any evidence, but finding it is a totally different matter…

5. **Alejandro Rivero**
   March 7, 2005

   I am not surprised, Lubos. Physics in the internet was mostly dead except by four or five pages, including Woit and your's.

   If you want to analyse web relationships, TouchGraph provides a visualizer for the proximity engine of google at http://www.touchgraph.com/TGGoogleBrowser.html

   You can click in any webpage to get more proxies to it, or right-click to go to the page.

   (Needs java. There is also a GoogleScholar version, you could had noticed it in physcomments at the lower left side).

6. **Lubo? Motl**
   March 7, 2005

   Wow, Not Even Wrong is the largest website that refers to The Reference Frame! See the counter on my blog.

7. March 6, 2005

   After the top discovery at 1/sqrt(2) of the Higgs vacuum, a lot of people will surely enjoy to have the W particle at 1/sqrt(2) of the Higgs.

8. March 6, 2005

   http://arxiv.org/pdf/physics/0006049
   http://arxiv.org/pdf/astro-ph/0003065

   Electric charge loses meaning above the SM symmetry breaking scale, there is no photon, no speed of light, no time, and hence no gravity. "It is hypothesized here that the Universe came into existence when the electro-weak symmetry was broken spontaneously".

   I think this qualifies for Not Even Wrong.

9. March 6, 2005

   drl, i suppose you're referring to
   http://arxiv.org/pdf/hep-ph/9912243 ?

   "…the full structure of the SM stands intact without constraining the quantum numbers isospin and/or hypercharge of the Higgs to any specific value."

   "The hypercharge of all the other particles are specified as being proportional to the Higgs hypercharge which itself remains unconstrained".

   "Higgs is a manifestation of the vacuum structure of the SM. Higgs shall never get pinned down as an isolated physical particle, but makes its presence felt through charge quantization and giving the SM its complete structure and consistency. **Hence it is predicted that Higgs shall not be discovered as a particle.**

   "No basic principle demands that the mass of the matter particle be given by Yukawa interaction, but since as we have no idea of where these masses come from, one just demands that they arise from such a coupling. If this be so then the Higgs isospin is necessarily T= 1/2. This just tells you that the 'vacuum' has this particular structure. But as Y_phi {hypercharge} is not constrained in any way, the Higgs cannot be a particle but just 'vacuum' which behaves in this fundamental and basic manner.

   "In summary, we have shown that the basic and fundamental structure of the standard model stands intact without specifying and constraining the quantum numbers of the Higgs. As such Higgs is very different from any known physical particle. Hence Higgs cannot be a 'particle' but represents the omnipresent vacuum with provides the 'root' to support the Standard Model'

   –
   I think when people talk of finding the Higgs, they mean the Yukawa-coupled Higgs, i.e., a definite isospin. Certainly all the theoretical constraints on Higgs mass come from such models. Since the hypercharge of all the particles are proportional to the Higgs hypercharge, one could equally well set the hypercharge of one of the particles as basic – i.e., if indefinite hypercharge makes the Higgs not a particle, then the same holds for all other particles.

10. **D R Lunsford**
    March 6, 2005

    The Higgs will never be seen, see the work of Afsar Abbas.

    -drl

11. **Quantoken**
    March 6, 2005

    The problem is obviously the standard model fails to predict and tell us how a Higgs boson should look like or how much its mass is, or we would not be guessing here. If the standard model is unable to confine the value of Higgs mass, then that means its mass really doesn't affect anything in the model. Then what will happen if you exptrapolate that mass to realy big or real small scale?

    There are certainly two possibilities that Higgs may or may not exist. Within the case it does exist, we can also list a number of possibilities.

    One, its energy is totally within reacheable range of today's running accelerators, but the cross-section is just too small to be detectable.

    Two, it's energy is many order's higher than reacheable level, and even approach Planck mass scale. Then it's beyond the technology in the near future to detect it.

    In the case Higgs does not exist, there are also several possibilities:

    One, physicists will just continue to search at higher and higher energy for Higgs particle, until they reach the Planck mass, at which point a wormhole occurs and physicists enter into another universe through the wormhole and continue searching for Higgs, because they simply can not believe that the standard model is wrong.

    Two, at certain energy level they detected something. Because of their eagerness to find Higgs, they call it Higgs boson. But it's actually not the Higgs boson they look for.

    Three, the third possibility does not exist, go back to possibility one above and search at higher energy.

    I think the odd that LHC exactly provides the right scale of energy, not too high and not too low, for detecting Higgs, and that the cross-section is big enough for detection. Is going to be very low.

    I do not know why some one worries about LHC creating blackholes? You need at least one Planck mass to create the smallest possible blackhole. LHC is many orders below that.

    Quantoken

12. March 6, 2005

    Ah, we already know that the Standard Model is correct, so discovery of the Higgs is "trivial". Seeing our first fundamental scalar is trivial. Why even waste money on this?

The corollary is that the most interesting result will be if the Higgs fails to show up at all.

13. March 5, 2005

    Isn't there a non-zero probability that the creation of a Higgs boson will create a black hole that will swallow up the Earth?

    If so, then the discovery of the Higgs truly will be only a few-day celebration as Lubos desires.

14. March 5, 2005

    not really. There are Higgsless models around

15. **Lubo? Motl**
    March 5, 2005

    My personal preferred guess (30%) is that the Higgs is at the 115 GeV level, and it's one of the light Higgs scenarios that are natural in SUSY.

    But the discovery of Higgs will be a few-day celebration only. It's a trivial thing. We know that something like the Higgs must be there to make the WW WW scattering unitary, and the fundamental scalar is simply preferred by precision measurements.

    The real question to answer is SUSY below TeV, and other potential new physics.

Hans Bethe 1906-2005

March 7, 2005
Categories: Obituaries

[Hans Bethe] died at home in Ithaca, New York on Sunday. There's an extensive [obituary] in the New York Times.

I believe Bethe was the last remaining figure still alive from the generation of physicists who came of age with the new quantum mechanics during the mid-to-late 1920s. Some popular lectures on the topic of "Quantum Physics Made Relatively Simple" that he gave for his neighbors in 1999 are available [on-line].

Update: There's more about Bethe and Cornell at Matthew Nobes's [weblog].

Comments

1. **Alejandro Rivero**
   March 9, 2005

   Hans A. Bethe, matured in 1928 in Zurich,
   was a disciple of Arnold Somerfeld
   who was a disciple of Von Linderman
   who was a disciple of C. Felix Klein
   who was a disciple of Julius Pl?cker
   who was a disciple of Christian Gerling
   who was a disciple of Carl Gau?
   who was a disciple of Johann Pfaff
   who was a disciple of Abraham Kaestner
   who was a disciple of Christian Hausen
   who was a disciple of Johann Wichmannshausen
   who was a disciple of Otto Mencke

   From Renardy: " Otto Mencke, founded the first academic journal in Germany, titled Acta Eruditorum, jointly with Leibniz (the journal existed 1682-1782). Otto Mencke should not be confused with his grandson Friedrich Otto Mencke."

   Bethe is survived by, among others, Roman Jackiw and John Irwin. If you know of more disciples, please feel free to add them to my wiki page.

2. **D R Lunsford**
   March 8, 2005

   RE his work:

   A nucleosynthesis "Dreimaennerarbeit" – "Alpher, Bethe, and Gamow" 🙂

   http://www.npr.org/templates/story/story.php?storyId=4505414

3. **Eleggua**
   March 8, 2005

   Hi Lunsford,

   Well he was recognised!

   Unlike Feymann he won the Nobel by HIMSELF!

4. **D R Lunsford**
   March 8, 2005

   That reminds me of a comment by the chessmaster Ludwig Mieses – he was being feted on his 80th birthday and said "Life expectancy is around 75 years. Now that I'm out of danger, I may as well go on living forever!"

   Bethe was 98 or 99! That's a good life. I think he should get more credit for his role in the development of QED than he seems to, in the various tomen. His calculation of the Lamb shift was really the turning point in the development of field theory.

   -drl

5. **raj**
   March 8, 2005

   A sad passing, but a reminder that people don't last forever.

   I wonder. Was the title "Quantum Physics Made *Relatively* Simple" meant to be something of a pun on the tension between quantum mechanics and relativity?

6. **Steve**
   March 7, 2005

   A sad loss but he lived to a very good age indeed.
   I read that when they were to detonate the first H bomb some people were worried it would set of a chain rection in the atmosphere and blow up the world. Bethe did the calculation that proved it would'nt. Such were his abilities that they trusted him with the fate of the world!

7. **Matthew**
   March 7, 2005

I believe Bethe was the last remaining figure still alive from the generation of physicists who came of age with the new quantum mechanics during the mid-to-late 1920s.

John Wheeler is still alive, though he was slightly behind Bethe's generation, I'd still put him in that class.

Brane Damage at Fermilab

March 8, 2005
Categories: Uncategorized

Last week Shamit Kachru gave a colloquium at Fermilab with the title String Theory and Cosmology. The scariest part was the beginning when he noted that what he would be talking about was work due to 500-1000 theorists and he put up a couple slides listing many of them.

He spent the first part of his talk laying out the "Landscape" story, somehow neglecting to mention that it was ugly, completely unpredictive, and told us nothing at all about the properties of the world today. He then moved on to discuss branes and cosmology, not making clear that branes explain absolutely nothing about the early universe or cosmology, although they do give you a new slogan he has come up with:

"Big bang as brane damage"

There were a couple questions at the end, with no one standing up and asking if this was a bad joke or something. I'm curious if anyone from Fermilab can explain to me what a typical experimentalist's reaction is to this kind of talk:

1. Are they impressed by this stuff and don't realize they've been fed a load of pointless nonsense for an hour?

2. Are they smart enough to realize they've just sat through an hour of pointless nonsense, but are too polite to say anything about this at the end of the talk?

3. Are they so smart they know in advance this will be an hour of pointless nonsense, so don't even attend, and are off somewhere else getting real work done?

Comments

1. **Juan R.**
March 14, 2005

Ok there is a problem with "less than sign".

I rewrite as Y > X.

2. **Juan R.**
March 14, 2005

Chris,

I forgoot the name and the second postulate that read X

3. **steve**
March 11, 2005

Edward Witten, Five-branes and M-Theory on an orbifold, preprint
available as hep-th/9512219. (1996)

the M stands for "magic", "mystery",or "membrane", according to taste.From a mathematical viewpoint a better term might be "murky", since apparently everything known about M-theory is indirect and
circumstantial, except for the classical limit, in which it seems to act as a theory of 2-branes and 5-branes, where an "n-brane" is an
n-dimensional analog of a membrane or surface.

4. **Chris Oakley**
March 11, 2005

Well, anonymous humorist, unfortunately it looks as though Weisskopf (Kon. Dan. Vid. Sel., Mat.-fys. Medd. XIV #6 (1936)) has beaten you to it. Inventing a new form of mathematics that allows physicists to "calculate" without having to worry about the consistency and logicality demanded by spoilsport mathematicians.

5.
March 11, 2005

According to the usual bizarre model of research on string theory, I propose the following theory of everything without the difficulties of compactification and saving both string theory and the scientific method.
The I-theory is very sImple, since is based in four postulates; moreover, it is ?background Independent? and explains the mysterious value of the cosmologIcal constant without the appeal to anthropIc principles!
Postulates of I-theory, being X and Y physical observables:
1) X > Y
2) X

6. **Kyle**
March 11, 2005

Those poor "working class" steelworkers. I mean, metallurgical engineers only make 60-80k a year. I bet most theoretical physicists would spit on such a dismal, blue collar salary.

More importantly, being involved with massive factories that use machines to produce steel makes people stupid and policitally uninvolved. That's why the government is frightened of string physicists and keeps them under tabs. University academians are far more dangerous than workplace engineers who have a couple of years less schooling.

Right?

7. **J.F. Moore**
March 11, 2005

Sorry, that was me below. Public machine, didn't change name.

8. **loser**
   March 11, 2005

   quantoken: well, my point was that hamburger makers _are_ pretty much replaceable. But in general I agree with your comments. Not too many people in society were crying when SSC was cancelled.

   I think it is important to be a little careful in arming the enemies of science funding with caustic and public criticism of sub-fields. Scientists generally lose when they compete for funding at the policy level with other scientists. Keeping the arguments "within the family" is wise. This blog is a good example of doing that.

9. **Arun**
   March 11, 2005

   I am sure we can show high correlation between economic growth of a country and the presence/absence of string theorists. This, in a Larry Summers way, proves the value of string theorists.

10. **Alejandro**
    March 11, 2005

    Perhaps it is shown in the Spanish Civil War and in the previous experience with the Escuela Moderna (anarchistic, at Barcelona) and the Instituto Libre de Ense?anaza (sort of socialistic, at Madrid), that the real danger is when both steel workers and science works start to develop links.

    Os to put it in a riddle, it is perhaps no coincidental the source from where [the money of] the Nobel prize come.

11. **quantoken**
    March 10, 2005

    J.F.Moore: Yes I appologize for my comments on steel workers. I did not mean to put down steel workers, McDonald workers, or any people of work classes. They are an inseparable part of the whole society and contribute values that's irreplaceable. On the other hand, I agree with you that I do not see any value (other than public entertainments) that string theoretists are contributing to the society, until the day they can some what make some association between their theories and the reality world, it's not even clear whether such a day will ever come.

    Quantoken

12. **J.F. Moore**
    March 10, 2005

    Quantoken, your glib comment regarding steelworkers isn't really warranted. They make much more than someone working at McDonalds, and rightly so, since they produce something of substantially more value, with more risk, and more skill involved. It is not beneficial to society or that person to have them underemployed. On the other hand, it's not really clear what society has lost if it is no longer supporting a string theorist.

13. **quantoken**
    March 10, 2005

    Surely jobless academics are much more dangerous than jobless steel workers. A steel worker who loses his job can just turn around and find a job at a street corner McDonald's before he was able to go home and report the unfortunate news to his wife.

    But what if a string theorist loses his job? A string theorist who have to go through decades of training, maybe some brain washing as well, before he could grasp all the math tools and be able to talk fluently in the vacuous stringy language. I do not know what these people can do if one day it is announced that string theory is a dead end and no more "research" in string theory is going to be founded by public money. Not only is it a total disater to these people personally, but it is also a disaster to the public's confidence of the general science research community.

    I do not think any one wants that to happen. A more likely scenary is the old people will continue to be supported until they die out and fade away. Meanwhile young generations should be discouraged from going into the field, with full and honest disclosures that this field has been fruitless in the past decades and could remain so for the foreseeable future.

    Currently CERN feeds half of all the world's scientists conducting research in particle physics and fundamental physics theory. Everybody is talking about the prospective of what to expect once the LHC starts to operate in 2007.

    But no body is talking about the prospect that the whole **LHC could be killed or left bleeding to death**, before it even begin to operate. Or that upon completion, this thing is unable to operate in the way it is designed. That's completely plausible. So why no one talks about THAT discouraging possibility?

    http://www.theglobeandmail.com/servlet/story/RTGAM.20050307.wphysics0307/BNStory/specialScienceandHealth/

    To me, the listed budget figure of merely **$1.8 billion US dollars** for the whole LHC budget, sounds **very suspiciously LOW** to me, for an undertake of this scale and this technological challenge. Digging a hole 27 km long underground would have costed far more than that. I know they already have the old tunnel to use, but constructing a new vacuum chamber 27 km long would have cost more than $1.8 billion already. Compare that with space shuttle launch. That's merely a feat of accelating a 90 ton sealed pressurize chamber containing some computers and electronics to 7.9km/second, which is certainly much easier than maintaining a high vacuum in a 27 km chamber and accelerate protons to 14 trillion eV.

    So I do not know where that **$1.8 billion** figure came from, or has scientists been totally honest about how much it could actually cost. If it ends up costing $18 billion, or even $180 billion, instead of $1.8 billion. It could well be killed by the budget committee half way through construction. Remember the SSC?

14. **Wolfgang**
    March 10, 2005

    The "patrons" might be smarter than you think.
    I assume that one reason to lock academics in their ivory towers is to ensure that they do not cause trouble in the real world.
    In the case of Russian physicists after 1989 this was explicitly stated as one reason.
    And jobless academics are much more dangerous to politicians than jobless stellworkers etc.

    Just my 2c

15. **Alejandro, again**
March 10, 2005

"by patrons who have no idea what the issues are"

I'd not be so sure of this neither. Back to the Aristotelian nonsense, a lot of people was being paid by patrons who know very exactly that the issue was "not having issue". To keep status quo, a class of educated priests, a determinate shape of society, etc… Small barons paying other researchers for practical reasons (ballistic and fortress theory, in the case of Galileo) caused havoc in the status quo. Which, on the other hand, was the real issue of these barons too.

16. **Alejandro Rivero**
March 10, 2005

Doug, I would not say it is unprecedented, I am sure we can find someone. For sure, all the Aristotelian blah blah before Galileo, Pascal and Kepler. A bit later, Cartesiasism, ie, the vortical theory of the universe, could qualify for a predecessor of strings, in the "not even wrong" sense.It could be interesting also to check what theories were being "investigated" in Cambridge when it happened that Cavendish left his appointment as teacher to keep researching privately (he come back to Cambridge later). Also, and relating to Bethe thread, I wonder where had particle physics gone in the mid XXth century were not by the Lamb shift anomaly and its almost instantaneous calculation by Hans et al.

17. **Doug**
March 10, 2005

Lubos: There is a point that is never raised in all this discussion: that the current crises in theoretical physics is not only ludicrous, but unprecedented in all the history of science, and that such erudite discussion of esoteric knowlege is being paid for, to the tune of billions, by patrons who have no idea what the issues are, let alone why they arouse such passionate discourse amongst the learned benefactors of their largess. It's a good thing too, because if they did, they might rise up and cut off all these high-priests who cloak themselves in the robes of the false priesthood we call academia.

18. **Alejandro**
March 10, 2005

The funny thing, Chris, is when the folder "reasons why the universe must be 3+1" relabels to "reasons thy the universe must compactify to 3+1". With a postscript added by hand, "or to stay in a 4-brane".

19. **Chris Oakley**
March 10, 2005

Here is an interesting thing for historians of science to contemplate. What is the value of the variable *date* below? I am guessing, about 1984.

"We've got this great theory, but it only works in *N* dimensions, where *N* is significantly greater than four."

Answer (pre-*date*):

Very interesting. File it in the circular file and try again.

Answer (post-*date*);

The universe must therefore be *N*-dimensional. Cool! Let us dedicate the rest of our lives to examining the consequences.

20. **Fyodor Uckoff**
March 10, 2005

Chris W said: What you're suggesting is what quite a few string theorists have been trying to do for years.

They have been *talking* like this for years, but surprisingly few of them have actually done much concrete work along these lines. For example Adams et al have shown that very strange things can happen to AdS orbifolds if you break enough supersymmetry. I guess that large chunks of the landscape can be shown to be internally inconsistent in such ways. As Lubos said, we don't really understand supersymmetry breaking very well; I would not be surprised to find that when we do, a lot of candidates for cosmological models will be ruled out and the landscape will collapse. If you can point us to some [failed?] efforts along these lines it would be useful.

21. **Steve**
March 9, 2005

Will some very smart person please write a killer paper that finally buries the landscape dogma, thus saving both string theory and the scientific method. To date, string theory has been formulated mostly perturbatively in classical backgrounds–the strings can be quantised but the background remains rigidly classical. Until string theory is (somehow) formulated in a way that goes beyond this it can't connect to the real universe nor can it say anything really concrete and certain about cosmology. The CC mystery is kind of like the mystery that surrounded superconductivity:when it was discovered around 1911 or 1912, superconductivity was a total mystery and all perturbative attempts to explain it failed. It was a quite a long while later that the correct (nonperturbative) explanation was finally found. I would say the situation with the CC today is something similar.

It seems that the only known way to stabilise the moduli leads to this landscape scenario. So there are three choices:
(a) You find an effective way to kill the landscape of string theory and reinterpret string theory (perhaps even starting the 3rd string revolution)and the theory can evolve again. From a string cosmology perspective it is also more natural to think of 3 dimensions decompactifying and 6 remaining compact (ala Brandenberger-Vafa) than compactifying down 6 large dimensions on CY space.
(b)You finally accept that higher-dimensional spacetimes, ala KK, simply can't actually work in physics, are not stable to perturbations and so you are forced to accept the universe is actually 4-dimensional after all.
(c)You simply refuse to give on up the idea of extra dimensions no matter what, fix the moduli or do whatever it takes to hold onto the idea (no matter how contrived) and accept that it leads to a landscape of quadzillions upon quadzillions of superfluous and redundant vacuua. Although it predicts nothing, this is then more acceptable and believable to you than a single 4-dimensional universe (for which there is some evidence!)

As much as I would be disappointed if there are no extra dimensions (it is a profound and beautiful idea) that might just be the way the universe really is. The KK idea has been around a long time now and has not really worked out. Nevertheless, from both a mathematical and physical perspective, there is something very special about 4-manifolds. Maybe you find 4-manifolds boring, but Kepler had to give up his cosmic system based on the Pythagorean solids and accept that planets actually just move on boring ellipses. Giving up your most deeply cherished beliefs and visions for the hard truth, no matter what it may turn out to be, is the heart of science after all.

22. **Quantoken**
March 9, 2005

Sorry Chris W. I did not credit you for the quote. If you had put an extra line breaker after the colon, it would have made it easiler to recognize it as what you say to Fyodor, not what Fyodor said. The style of Peter's blog is just different from some other blogs I visit.

Quantoken

23. **Chris W.**
March 9, 2005

That quote was in my *reply* to Fyodor, not in his own comment.

24. **quantoken**
March 9, 2005

Fyodor said: The oft-repeated refrain that **"the theory is smarter than we are"** is being invoked in this context.

I read the above sentence in the logically equivalent form that string theoretists admit "we are more stupid than the theory we invent, (which is a useless theory)" 🙂 Which is the unfortunate fact.

Peter said: "The **scariest part** was the beginning when he noted that what he would be talking about was work due to 500-1000 theorists and he put up a couple slides listing many of them."

What is scary is NOT that his talk is backed up by very solid scientific works of 500-1000 people. But rather the fact that such a **vacuous** thing is **all that they can show us**, after decades of work by 500-1000 of the most sophisticated theoretists. It's 10^120 more vacuous than the cosmological constant itself:-).

Quantoken

25. **Chris W.**
March 9, 2005

Fyodor: What you're suggesting is what quite a few string theorists have been trying to do for years. So far they haven't had much success. Susskind (at al) recommends that they stop worrying and learn to love the Landscape, while brushing aside concerns about testability as manifestations of an obsolete viewpoint on the philosophy of physics (and science) that has been transcended by the theory. The oft-repeated refrain that "the theory is smarter than we are" is being invoked in this context.

26. **Fyodor**
March 9, 2005

Well, with reservations I agree with both Peter and Lubos. I thought that Peter was just complaining about the evident bizarrerie of what Shamit K was talking about. For Lubos: it seems to me that what you should be doing is to look at cosmological models coming out of or inspired by string theory, and try to show that nearly all of them are somehow unstable or internally inconsistent. There are lots of examples of backgrounds in string theory that look ok until you consider some non-perturbative effect, and then they die or [AdamsPolchinskiSilverstein] get turned into something completely different. Maybe one can cut that 10^500 down to 10^1? Or 10^0 ?

27. **Lubo? Motl**
March 9, 2005

Shamit is a nice and extremely smart and technically powerful guy – which of course makes it slightly more difficult for me to say that I essentially agree with Peter.

The level of anthropicity of this thinking has been increasing in the past few years. A couple of years ago, Shamit would tell me things like "you don't need to believe the anthropic principle; this is a question we should understsand anyway". This kind of disclaimer has been disappearing.

It seems that now it is expected that one believes the anthropic thinking as the ultimate answer we can have about nature. It is the motivation for this kind of research as well as the broad framework in which the research is done. I just can't imagine how could I ever be convinced that a theory of this kind is a correct one because it lacks quantitative predictability.

Some people really seem to be excited by the very fact that they can embed a relatively convincing framework into string theory whose conclusion is that we can't predict anything – or perhaps, we can even choose which things can't be predicted and which things can be predicted, even though we can't actually make these predictions.

This opinion is contrary to everything I believe about constructing theories and determining their value. String theory is valuable only because it can naturally predict the right spectrum of particles and interactions (including gravity), at least qualitatively, from a starting point that has many less assumptions. The anthropic framework may be describe by the opposite words.

It has always been trivial to construct a theory that can't predict. The Bible, via the power of God and His Son, has also an "explanation" for everything – God wants it this way. The landscape of all possible field theories is another example.

The main difference between the anthropic explanation and the God explanation is that the Christians are often right-wing and prefer the church and family, while the anthropic people are mostly left-wing and better in calculating quantum field theory. But as far as the explanation goes, it seems on equal footing.

A theory can only become a convincing scientific theory if it explains more data than what is inserted, and this is a dogma for me, if you wish. Otherwise it's just a story, fairy-tale, mnemotechnical bookkeeping device at best. In effective QFT, we must insert a rather small value of the C.C. But if we create whole untestable God stories with 500 fields and complicated potentials etc. just in order to get one number, I just can't imagine that this approach can ever be promoted to science.

Equivalently, the probability that (assuming that there is a complicated mechanism that only generates the C.C.) we would guess the right mechanism behind the C.C. – just by looking at the single number and the plethora of tools that string theory gives us – is something like 10^{-200} and I see no point in trying to find the "right" mechanism.

It seems more appropriate to say that we don't understand one number, we won't know the right explanation of the C.C. (we only have real problems with the C.C. after SUSY breaking, and therefore the ignorance is because we don't understand SUSY breaking) until the complete theory of everything – which includes string theory in the cosmological context – is understood – and we should focus on places where we have a lot of data to unify into one theoretical description. (Well, we don't have too much data right now even beyond the C.C. but that's a different topic.)

Also, in some sense, the landscape business is "politically correct". For example, treating all conceivable vacua as equal is the ultimate example of egalitarianism and political correctness (and I would also add stupidity).

28. **Peter**
March 9, 2005

I don't object to Kachru's work because it's hard to believe or weird, I object because it's not science. It not only doesn't predict anything about anything, it inherently is a framework that can never predict anything about anything. To see leading figures in particle theory giving pretentious talks like this that are not science in any sense of the term is outrageous, disgraceful, and someone should call them on it.

No, Kachru doesn't "predict" the CC, he sets up a framework in which you can't predict the CC, and thinks that is a great achievement. I have no idea whether the CC will be computable in an ultimate theory. Right now we don't have a convincing unified quantum theory of particle physics and gravity. When we do, maybe the CC will be computable within it. But the problems with string theory go way beyond the CC. If Kachru's framework allowed one to calculate something, but not the CC, one could take it seriously. But it doesn't allow you to calculate anything. Not one single thing, nada, zip.

29. **Fyodor Uckoff**
March 9, 2005

Basically what you are saying, Peter, is that you find the work of Kachru et al hard to believe. And I agree — I think we should try our best to explain things within one Universe. And I'm pretty sure that Shamit would agree that such efforts should [also] be pursued. But what I find puzzling is this: you find this stuff hard to believe. But the observed fact that the cosmological constant is non-zero and small is utterly, completely mind-boggling from *any* conventional point of view. So when somebody comes along with a correct theory of all this stuff, it is *guaranteed* that it is going to be something extremely weird. I'm sure you don't expect someone to do a conventional quantum field theory calculation, however clever, and have the value of the CC fall out at the end. So the mere fact that Kachru's colliding branes in higher dimensions *looks* utterly incredible means nothing. The *correct* theory will probably look weirder still, no? So what's your problem with weird theories?

30. **Peter**
March 9, 2005

I didn't attend the Kachru talk in person, just looked at his slides online and watched some of it on the online video. When I have personally attended talks like this I have generally at the end stood up and asked a question along the lines of "From what you have said, isn't it true that this is a theory that can never predict anything" (most notably when Susskind gave a colloquium here, and when Douglas gave one at City College a while back). No I don't enjoy publicly confronting people like this, but I think it's outrageous that they give this kind of talk, then no one says anything.

I did attend Smolin's talk, don't remember why I didn't write anything about it. He basically went over his recent paper in which he argues that any quantum gravity theory based on connection variables implies "doubly special relativity" effects, effects that he was claiming have implications for the AUGER and GLAST experiments.

31. **Thomas Larsson**
March 9, 2005

In a way, all this reminds me of the contrast between Lorentz's and Einstein's explanations of the null result of the Michelson-Morley experiment.

We live in a region of the Landscape where the aether wind is very small, because a large aether wind is not compatible with human life.

32. **Chris W.**
March 9, 2005

In a way, all this reminds me of the contrast between Lorentz's and Einstein's explanations of the null result of the Michelson-Morley experiment. The fact to be explained had a stark simplicity about it, whose full significance Einstein grasped and Lorentz couldn't quite perceive, in spite of his gifts as a theoretician and mastery of the subject.

Of course, it has become clear that for Einstein the Michelson-Morley result was expected and almost trivial; he was led to the underlying principles by thinking about Maxwell's electrodynamics and certain observations which by themselves had not heretofore appeared to be problematic.

In contrast, string theory's birth was oddly accidental. The hope has been maintained for 25 years that its underlying physical principles would eventually become clear. Instead, it has begun to seem like a mockery of the very idea of physical explanation — a massive virtuoso exercise in mathematical modeling, supported by the "accursed fertility" of differential geometry (to use Kant's phrase*). As Kachru says without apparent irony:

> "In studying any of these issues in depth, [the] most striking feature is the *diverse array* of possibilities the theory encompasses."

Again, why is this diversity supposed to be a virtue, when there is so little empirical basis for believing that most of these possibilities are actually realized, and so little real insight offered to account for their absence?

—
* from the following remark, as quoted by Karl Popper:

> "Concerning metaphysics in general, and the views I have expressed on their value, I admit that my formulations may here or there have been insufficiently conditional and cautious. Yet I do not wish to hide the fact
> that I can only look with repugnance and even with something like hate upon the puffed-up pretentiousness of all these volumes filled with wisdom, such as are fashionable nowadays. For I am fully satisfied that the wrong way has been chosen; that the accepted methods must endlessly increase these follies and blunders; and that even the complete annihilation of all these
> fanciful achievements could not possibly be as harmful as this fictitious science with its accursed fertility."

33.
March 9, 2005

I heard Lee Smolin, the LQG guy, spoke at the string group at Columbia recently. Did you attend? If so why no comment?

34. **Jean-Paul**
March 9, 2005

Well, Shamit is an exceptional case, in a way similar to Douglas. Unlike most landcape loonies, they are respected string theorists so they deserve some attention.
Now they want to establish themselves as "phenomenologists", talk to experimentalists, give public lectures etc. Unfortunately, this leads to ridicule. The line "big bang as brane damage" joins "solution of unification by nullification",
"out of this world solution of the hierarchy problem", "little Higgs" and other absurd one-liners that dominate the infamous "physics beyond the standard model".
I remember one Witten's colloquium on unification: he started from experimental data and after 10 minutes he was deep into linear bundles. Fortunately, he didn't try jokes, so he avoided ridicule — it was just another weird talk…
Jean-Paul

35. **anonymous**

March 9, 2005

Peter, I take it from “and told us nothing at all about the properties of the world today” that you were there for the talk. If so, then why didn’t you make this comment? Is it easier to be brave online?

36. **Peter**
March 8, 2005

The same thought had occurred to me….

37.
March 8, 2005

I’m guessing that if one compared “stephen”‘s IP to “plato”‘s one might notice a pattern.

38. **stephen**
March 8, 2005

Not sure why you’re quoting this press release (which doesn’t have anything to do with Kachru’s talk).

I was aware of the cyclical universe idea and brane collisions, and watched the cosmic string develope from supersymmetrical valuations.

Please be patient.

One would have to know how to get there and “if,” from early comsological idea of a early universe, and we assume it is cyclical, something had to exist before the strings?

I used the whole example of Dvali’s analogy as a comparison to all the dimensions (yes I am listenng to you), in context of the fermions on the brane(what is held to it?) the water(represents the dimensions), and the idea of bosonic production off the brane, concealed as gravitational wave production.

If the torus existed from the genus figure collapse, how would rejuvenation take place? Anti-gravity production indicated by the jets? Swiss cheese universe?

Do you not feel such geometries/topologies can be comparative to cosmological associations Peter?

39.
March 8, 2005

“Big bang as brane damage”??
I think I will go off and read some of Bethe’s old papers instead…

40. **An experimentalist (Monad)**
March 8, 2005

Years ago, I went to one general ST talk by Brian Green. My (very) crude grasp of it was that even when they get it right and the masses of all the quarks and leptons roll out of an ab initio calculation, they will have exchanged the 20 or so free parameters of the SM for the free parameters of the topology of thier multi-dimensional space.

When (if) they get that far, I’ll take another look. But for now, yes, I stay in my lab.

41. **Peter**
March 8, 2005

Not sure why you’re quoting this press release (which doesn’t have anything to do with Kachru’s talk).

Dvali’s extra dimension nonsense doesn’t predict anything about anything (he has no idea how many extra dimensions there are, their sizes or properties). This isn’t science in any reasonable sense of the term.

42. **stephen**
March 8, 2005

I am sorry, I just realize the mistake I might of made.

I compared the metal plate Dvali hits, to a brane?

43. **stephen**
March 8, 2005

If gravity is modified at large distances, it’s modified everywhere. That would make it possible to verify modified gravity by measuring the orbit of the moon to within one millimeter

http://www.eurekalert.org/pub_releases/2005-02/uoc-sma021405.php

So would Arkani-Hamed, Sava Dimopoulos, and Gia Dvali doing extra dimensions… and the attempts to explain these dimensions, be a fruitless and not worth finding experimental opportunities?

44.
March 8, 2005

They probably showed up to hear professor Kaku talk about alien visitations and were sorely disappointed.

Clifford Modules

March 11, 2005
Categories: Uncategorized

John Baez had a weblog long before the term was even invented, and for many years now has been consistently putting out interesting current material about math and physics under the title This Week's Finds in Mathematical Physics. The latest edition has a beautiful explanation of the structure of modules of the Clifford algebra.

Traditionally one thinks about geometry in n-dimensions in terms of n-dimensional vectors and tensors built by taking tensor products of vectors. These are all representations of the general linear group GL(n), or if one has a metric, the othogonal group SO(n) of transformations that preserve the metric. However, it turns out that there are representations more fundamental than vectors, the spinor representations. These require a metric for their definition, and are projective representations of SO(n), or true representations of the double-cover Spin(n). When one tries to construct spinors, one quickly runs into a fundamental algebraic structure associated with a real n-dimensional vector space: the Clifford algebra C(n). Spinors occur as "modules" of the Clifford algebra, i.e. vector spaces that the Clifford algebra acts on. The structure of these possible Clifford modules is rather intricate, with a certain eight-fold periodicity. Baez gives a beautiful explanation of part of this story.

Physicists generally complexify everything in sight (i.e. assume all numbers are complex), which makes things much simpler. Then the story is periodic with period 2 instead of 8, and Clifford algebras are just one or two copies of a complex matrix algebra of k by k matrices, where k is some power of 2. Clifford modules (including the spinors) in this case are just complex vector spaces of dimension k, and tensors built out of these. One good place to read about all this, together with its relation to the index theorem, is in the book "Spin Geometry" by Lawson and Michelson, but there are by now lots of others.

If one believes in a deep relation between physics and geometry, these Clifford modules should somehow come into play in the structure of the most fundamental physical theories. To some extent this is already in evidence in the way spinors and the Dirac operator occur in the standard model. There are also tantalizing relations between the idea of supersymmetry and the Clifford algebra story. Many, many people have been motivated by this kind of idea over the years to try and use Clifford algebras to come up with a fundamental particle theory, one that would explain the structure of the standard model. While some of these attempts have very interesting features, none of them yet seems to me to have gotten to the heart of the matter and used this kind of geometry to give a really convincing explanation of how it is related to the standard model. Some crucial idea still seems to be missing.

Comments

1. **Chris W.**
   March 20, 2005

   New preprint on an application of supersymmetry in a condensed matter model:

   Supersymmetric Model of Spin-1/2 Fermions on a Chain

   Introduction (excerpt):

   > For many condensed matter systems, the key to understanding the physical properties lies in the analysis of a quantum many body problem with strong correlations. For the analysis of such systems, approaches that go beyond the standard perturbative techniques are always needed. It has recently been proposed [1, 2] that, for a special class of lattice models for correlated fermions, supersymmetry can provide a tool for non-perturbative analysis. In these models, questions about the existence and degeneracies of strongly correlated ground states at zero energy are easily answered with the help of supersymmetry and elementary combinatorics. Explicit properties of these same ground states are being studied with techniques that are, in various ways, associated with supersymmetry [1, 3, 4].

   Conclusion (excerpt):

   > We have introduced a model of interacting spin-1/2 fermions on a chain with a manifest SU(2) extended N = 4 supersymmetry. Our representation of N = 4 supersymmetry is highly non-linear, as it is entirely built from degrees of freedom that are fermionic. We have looked for a supersymmetric model where SU(2) spin symmetry is faithfully represented, and this has led us to a somewhat unusual restricted Hilbert space, with anti-ferromagnetic correlations built in from the start. The algebraic structure we have uncovered is very rich, but we are lacking a systematic mathematical framework. Such a framework will be most valuable, as it will allow us to further work out our present model and to decide on possibilities for alternative realizations of N = 4 supersymmetry.

   [For some background, see (eg) hep-th/0210161.]

2. **D R Lunsford**
   March 17, 2005

   CW, see also the "Space Time Code" series from the 60s and 70s, which I seem to recall he thought of as a relative failure, but good practice. Your exposition reminded me strongly of that.

   -drl

3. **D R Lunsford**
   March 17, 2005

   Yes of course, I thought that was well-known. Finkelstein's primary object is the chronon thought of as a type of simplex, and the edges are conceived as making up a Clifford algebra.

   http://www.physics.gatech.edu/people/faculty/dfinkelstein.html#research

   http://arxiv.org/abs/hep-th/0005039
   http://arxiv.org/abs/hep-th/0106273

   -drl

4. **Chris W.**
   March 16, 2005

   DRL,

Yes, but not for a long time. I do recall that his work attracted some significant attention as far back as the 1960s. I should revisit it; I remember enough about it to see what you're getting at.

Come to think of it, I did visit his home page a few months ago. BTW, I just went to the Quantum Relativity Group's page at Georgia Tech, and found this:

Clifford algebra as quantum language
(hep-th/0009086)

5. **D R Lunsford**
March 16, 2005

Chris W – have you looked at Finkelstein's work? (Starting probably with "Quantum Relativity".)

-drl

6. **Chris W.**
March 16, 2005

Linking Geometry and Dynamics

Introduction

In a comment on his weblog posting "Clifford Modules" (3/11/2005), Peter Woit said the following (in a reply to Tony Smith):

> …… But I still think there is a crucial idea missing here and wonder if you really strongly disagree with this. To be a little more specific, what seems to me to be missing is some deeper link between the geometry and the dynamics. In the path integral formalism, why are we integrating over a space of connections the exponential of the norm squared of the curvature of the connection? Why the determinant of the Dirac operator? I tend to think we need some insight into these mysteries in addition to more kinematical ideas about Clifford algebras.

In a brief email correspondence I had with Ray Streater 9 months ago he made the following remark in reference to Irving Segal:

> I notice that Baez was a student of Irving Segal, for whom I was research assistant in 1965, and from whom I learnt a lot of things. But my sympathies are with Chandrasekhar whose face fell when Segal introduced space-time as a poset: we want physics.

From Streater's brief memoir of the encounter (linked above):

> Atiyah then invited Chandrasekhar, as the century's most eminent astrophysicist, to open the questions. Chandra complained that gravity was nowhere mentioned, and that there were *no dynamical laws in the theory*. [emphasis added]

In the following I will introduce a simple (and fairly familiar) notion of dynamics that can be connected immediately with geometry in a way that may be initially surprising, but is quite natural. Furthermore, this notion will prove to be unexpectedly fruitful, in a way that is also initially surprising, but is again natural, indeed, almost obvious after some contemplation. More specifically, I will be drawing tight linkages between elementary geometrical (and topological) notions, a primitive notion of local gauge freedom which bears a provocative resemblance to supersymmetry, and causal sets, which are, of course, posets (partially ordered sets) as mentioned by Streater.

A Boolean Network as an Abstract Simplicial Complex

Consider a simple collection of binary elements or Boolean variables. Initially we assume no structure on this set other than the assumption that each of its members possess a binary state, which may change. Of course the collection (or ensemble) has a collective state, which may also change.

Assume that the changes of (and within) the collection can be recorded. We can accumulate a history:

S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S
S _ S S S _ S _ S S S _ _ _ S _ S _ S _ S _ _
S _ S _ S _ S S S _ S _ S _ S _ _ _ S _ S _ S
_ _ S _ S _ S _ S _ S _ S _ S _ S S S _ S _ _
S _ S _ S S S _ S _ S S S _ S _ S S S _ S _ S
S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S

Bear in mind that in representing the set this way we have assigned an implicit labeling of its members, which may be considered an integer {1, …, N} assigned from left to right. The changes in state are tracked with respect to this labeling, and we are forced—for the moment—to assume that the labeling can be meaningfully carried forward in "time", as the state of the collection changes.

We may now ask ourselves, what are the dynamics? We can record a history, but can we predict successive states? Can we describe stable correlations among members of the collection?

We must guess at the dynamics, and then test our guesses. The simplest thing we can do that seems likely to yield non-trivial results is to associate transition functions of two variables (two input states) with each element in our collection. These functions must of course be Boolean, and the number of possibilities are very limited. In fact, there are 16 *functionally complete* options from which to choose, all interrelated by a familiar web of dualities:

NAND(x, y) = ~NOR(~x, ~y)
NAND(~x, y) = ~NOR(x, ~y)
NAND(x, ~y) = ~NOR(~x, y)
NAND(~x, ~y) = ~NOR(x, y)

NOR(x, y) = ~NAND(~x, ~y)
NOR(~x, y) = ~NAND(x, ~y)
NOR(x, ~y) = ~NAND(~x, y)
NOR(~x, ~y) = ~NAND(x, y)

Here "~x" or NOT(x) can of course be taken as a shorthand for NAND(x, x) = NOR(x,x).

We specify a complete transition on our set of N binary variables by selecting functions from this set of 16, assigning them to ordered triples ({c; a, b}) taken from the set of N variables; one variable receives the "output"—the "next" state—and the other two provide the inputs. The elements of the triple are not necessarily distinct.

Let us focus attention on the inputs of the transition functions. In the “typical” case they associate each member of our set of Boolean variables with another member. This makes clear that in this context the variables may be thought of as vertices, and the transition functions as edges or links. Collectively they define a simplicial complex, which is carried forward in “time” as the set of variables evolves according to the transition rules.

We have arrived at the roots—at least in part—of geometry.

A Boolean Network as a Gauge Field

There is another way to view the foregoing. Let us reconsider the brief history of our small sample collection of variables:

```
S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S
S _ S S S _ S _ S S S _ _ _ S _ S _ S _ S _ _
S _ S _ S _ S S S _ S _ S _ S _ _ _ S _ S _ S
_ _ S _ S _ S _ S _ S _ S _ S _ S S S _ S _ _
S _ S _ S S S _ S _ S S S _ S _ S S S _ S _ S
S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S
```

It was mentioned before that the representation of the collection as a one-dimensional array implied nothing more that a simple labeling by a set of integers. It may now be noted that the representation employs a labeling of the states of the variables which is equally arbitrary. We can globally invert the assignment of our state labels (S, _) without making any essential change to the information content of our history:

```
_ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _
_ S _ _ _ S _ S _ _ _ S S S _ S _ S _ S _ S S
_ S _ S _ S _ _ _ S _ S _ S _ S S S _ S _ S _
S S _ S _ S _ S _ S _ S _ S _ S _ _ _ S _ S S
_ S _ S _ _ _ S _ S _ _ _ S _ S _ _ _ S _ S _
_ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _ S _
```

It is crucial that the state relabeling be *global*. We have no sensible way to propagate a selective relabeling of some variables throughout the history without corrupting our knowledge of the correlated changes in the collection, because we have no notion of dynamics, other than the raw fact of change reflected in our data.

Everything changes when we introduce dynamics, that is, our set of transition functions. An arbitrary local state relabeling can now be absorbed into the transition rules using the dualities that interrelate them. In this austere context we have hit upon two central ideas of gauge symmetry. (1) The variables’ states are like phases; we can detect changes and variations across the collection, but we can’t assign an invariant meaning to the states themselves. This dictates a global symmetry. (2) *The introduction of dynamics allows us to turn this into a local symmetry.*

A Boolean Network as a…Supersymmetric Gauge Field?

As we think about this more deeply we start to see some subtleties. There is another relabeling freedom in the collection of variables—the labeling of the variables themselves. We can permute the labeling, and as long as we do this consistently throughout our recorded history—prior to any assumption of dynamics—we change nothing essential in the record. On the other hand, if we have segments of a history that precede and follow relabelings of variables we must remember the permutations that were performed in order to compare the segments.

Now, let us consider again what happens when we perform a local relabeling of states and absorb the relabeling into the set of transition functions. The functions fall into at most 16 distinct classes or types; of course we may not have used all the types in equal proportions. The absorption of the state relabeling will transform the type of the affected functions, and the distribution of transition functions assigned to the variables among the possible classes will change, in general. Of course, in a very large Boolean network of this type with suitable statistical properties, the distribution may not change much at all. Indeed, we may merely witness what appears to be merely a shuffling—a permutation—of the *positions* of the affected instances of the 16 classes with respect to the labeling of the variables. It is quite conceivable that this shuffling could be *undone* by following the state relabeling with a relabeling of the variables, which implies a relabeling of the transition functions assigned to them.

It is clear that understanding the structure of these transformations is becoming rather delicate, and the structure—the geometry—of the simplicial complex is deeply involved. However, we can at least offer the observation that the internal gauge transformation—the state relabeling—appears to be intertwined with the “external” or positional transformation, ie, the selective relabeling of the Boolean variables. Indeed, they seem to be in some sense inseparable. This strikes me as deeply reminiscent of supersymmetry.

A Boolean Network as a Generator of a Causal Set

What precisely is being preserved in the gauge transformations we have been discussing? I hope it quite clear by now that the relabeling transformations preserve the identity of *events* or state transitions in our collection, and the causal connections among those events. In other words, the collection’s history has an invariant temporal “skeleton” which fits the description of a causal set. For those who are unfamiliar with causal set theory the significance of this can be better appreciated after reading a review such as this September 2003 article by Rafael Sorkin:

Causal Sets: Discrete Gravity (Notes for the Valdivia Summer School)

Also, in January, a highly speculative but extremely interesting and detailed preprint by Seth Lloyd appeared that sets forth an attempt to ground general relativity, and by implication, quantum gravity, in quantum computation. The idea, roughly speaking, is to generate the causal and metric structure of spacetime out of the dynamics of an ensemble of qubits. Lloyd acknowledges that his approach, as an attack on the problem of quantum gravity, is closest in spirit to causal set theory among the various competing alternatives.

What of Spinors and Clifford Algebras?

I will cut this short and simply assert that the relevance and role of spinors is near at hand. The reasoning behind this is still sketchy (like most of the above) but is deeply inspired by the following two articles, to be read in the given order:

Relativity in binary systems as root of quantum mechanics and space-time
(hep-th/0408116)

Spin foams, causal links and geometry-induced interactions
(hep-th/0403137)

Pregeometry and Entropy

In closing, I should acknowledge that the potential role of Jaynes’ Maximum Entropy principle has played a central role in my thinking about these questions for over 15 years. I recently attempted to sum this up in an email to Lee Smolin (8/5/2004) which can be taken as something of a manifesto:

Dear Dr. Smolin,

See http://www.arxiv.org/abs/nlin.AO/0408007. I thought this paper would be of particular interest to you, in light of your recent preprint (hep-th/0407213) and your past collaboration with Stuart Kauffman and long-standing interest in the structures of discrete networks and their relevance to spin networks and quantum gravity.

This new paper is to my mind relevant to the effort to put putative explanations of "anthropic" features of the universe on a sound scientific basis. The general point of view I have in mind is this:

One would like to in some sense show that:

- the fundamental theme of entropy maximization under constraints underlies the laws of physics and the structure of spacetime, and ultimately, cosmological structure and evolution,
- at the same time underlies the evolution of biological systems and, ultimately, sentient life,
- and *binds the two realms together* in a way that genuinely explains the familiar observations that have previously motivated the more problematic formulations of the anthropic principle.

The key element is this: Rather than suppose that there is a kind of super-universal selection of law-like alternatives (including values of fundamental constants), imagine that a sort of optimization through maximally unbiased selection constitutes, in essence, the foundation of physical law. Of course one might reasonably ask if this is so different than more conventional formulations. That is, fundamental symmetries provide the constraints, and some sort of selection sorts out the alternatives allowed by these constraints. The crucial question becomes, does this selection happen in time, along the lines suggested by biological evolution, or does it effectively happen outside of time, via a kind of timeless optimization principle? The latter is suggested by the intimate relationship of variational action principles and symmetries of an action functional. Can such extremization under variation be understood or recast as a entropy maximization under constraints?

Given the profound connection of MaxEnt principles to statistical mechanics and thermodynamics, and the equally profound "thermodynamic" character of general relativity, as witnessed for example in Ted Jacobson's gr-qc/9504004, one wonders if this might not be the preferred avenue to explore.

However, in his article Jacobson expresses a traditional point of view. If a putatively fundamental physical law has the character of a thermodynamic equation of state, then it can't be that fundamental. There must be a complicated microstructure (and micro-dynamics) underlying it whose governing law is obscured by statistical regularities. I am suggesting, in essence, that the statistical regularities are most of the story. The underlying microstructure is primordial, not derived (ie, not a "solution"), and possesses some primitive combinatorial symmetries and nothing more. Everything else comes out of a kind of statistical optimization (entropy maximization?) of the microstructure subject to its symmetries.

I would suggest furthermore that the symmetries follow largely from background independence of the microstructure. Because its possible configurations cannot be distinguished by differences in their "pinning" to a background, they must fall into equivalence classes — probably very large equivalence classes. This is fertile ground on which to build a potent kind of statistical mechanics.

What remains, then, is to say something more definite about the nature of the microstructure.

7. **D R Lunsford**
March 13, 2005

Everything points to y5 as the missing link.

-drl

8.
March 13, 2005

I don't get it. Why is a theory bad if it does not explain the gauge coupling strengths, the Yukawa couplings and the exact matter content? In principle, the standard model is enough to determine the unitary evolution. Why must we explain the parameters of the standard model? Why can't they just be what they are?

9. **Alejandro**
March 13, 2005

A serious puzzle when trying to relate Clifford algebras to the real world is the existence of three generations of particles, all having equal charges. I mean, you can suspect that the fact of having fermions around could have a geometrical motivation, and you can even be happy about having four fermions which you could perhaps use to build a well oriented slice of space time or whatever geometrical construct. But 12 formions? What in the hell does Nature expects we should do with them?

10. **Doug**
March 13, 2005

Peter wrote:

If one believes in a deep relation between physics and geometry, these Clifford modules should somehow come into play in the structure of the most fundamental physical theories. To some extent this is already in evidence in the way spinors and the Dirac operator occur in the standard model. There are also tantalizing relations between the idea of supersymmetry and the Clifford algebra story. Many, many people have been motivated by this kind of idea over the years to try and use Clifford algebras to come up with a fundamental particle theory, one that would explain the structure of the standard model. While some of these attempts have very interesting features, none of them yet seems to me to have gotten to the heart of the matter and used this kind of geometry to give a really convincing explanation of how it is related to the standard model. Some crucial idea still seems to be missing.

Peter, everyone: With all due respect, trying to use the Clifford algebras to "come up with a fundamental particle theory" that "would explain the structure of the standard model," actually could be the reason that "some crucial idea still seems to be missing." However, please understand: in making this statement it's not my intention to attack the greatest intellectual achievement of the 20th Century by suggesting this.

Nevertheless, I believe that it's important to recognize that string theory exists as a direct result of trying to avoid the difficulties of a "fundamental particle theory," and maybe we need to consider that the need to resort to the concept of fields was motivated in the same way: the idea that the existence of a fundamental particle, or set of fundamental particles, can explain nature, seems to be misguided.

However, replacing the point particle with the concept of a field, and the idea of force with the concept of interaction, does not change the basic assumption underlying a theory of fundamental particles, anymore than replacing it with the concept of a vibrating string does, even though we can point to the "spectacular" success of QED/QCD, and string theorists can point to the "astounding" beauty of their M theory. As we all know, the most important aspects of any theory for investigators is its failures, not its successes.

The most obvious failure of the standard model is its lack of an explanation of mass and the associated interaction (force) of gravity, yet we steadfastly fail to see this as a failure of a "fundamental particle theory." Frankly, though, what this seems to be shouting at us is that we really need to look for an alternative to a fundamental particle theory. If there is indeed "a deep relation between physics and geometry," and "these Clifford modules [actually do] come into play in the structure of the most fundamental physical theories," shouldn't we stop trying to force that structure into a particle theory and look for an alternative?

11. **Doug**
March 12, 2005

Quantoken wrote:

"What is time" is a question so profound that it is beyond mathematics to try to find an answer. The concept of time and time arrow inheritantly associates with the concept of causality. If there is no causal relationship between things, then time does not exist. But is the causal relationship just our perceptions of the world, or is it part of the reality?

It doesn't have to be that hard, though. Time has the same effect on motion that space does, and space, while it's also a problematic concept in some ways, it's not as mysterious. However, it's the concept of space and time in the definition of motion that is important.

Clifford algebra recognizes this in its noncommutativity: a^b = -b^a, a directed area. Clockwise rotation is different than counter-clockwise rotation. It doesn't mean that time can run backwards, but it does mean that there is an inverse, a mirror image to all directions, and if it exists in all directions then it follows that it exists in the relationship of space and time, or motion, as well. In fact, we can see it: A decrease in time has the same effect on motion as an increase in space. They are reciprocally related. That's the important thing to understand.

This reciprocity in turn implies a deeper symmetry: whereas time continuously progresses, so should space; and whereas space has three dimensions, so should time. Now that we can actually observe the progression of space in the receding galaxies, we are even more justified in assuming three dimensions of time to complete the symmetry, which, of course, means three independent dimensions of motion should exist as well.

In Cl3, we see a similar symmetry: 1 scalar, 3 vectors | 3 bivectors, 1 pseudoscalar. What does this mean? I'll offer one, simple, but profound idea: The scalar magnitude of motion can easily progress continuously outward from the origin of vectors (outward translational paths diverge, think of expanding volume), but, on the other hand, it cannot continuously progress inward translationally in the same manner lest, passing zero, it again becomes outward motion (inward translational motion converges, think of shrinking volume). Therefore, for continuous inward scalar motion to exist, it must exist as a rotation, not a translation.

To find that these two fundamental modes of scalar motion are represented perfectly in Clifford algebra, is sobering.

12. **Tony Smith**
March 12, 2005

Peter, I am somewhat concerned that this message may be drifting off-topic with respect to this thread subject of Clifford algebras, so it is OK with me if you delete it, but I am initially posting it mostly to respond to a couple of comments by others.

Quantoken, you say, about my approach: "… That is not how a theory gets accepted as science. … acceptance of a science is really acceptance by the general public …".
My objective is NOT for my model to be "… accepted … by the general public …". It is for my model to be available in the archived records of physics so that anyone who is interested in it can study it, criticize it, and perhaps improve it or make use of it or some part of it.
You also say: "… if at the end of day, only ten people out of the whole population of this planet manage to figure out what your theory is all about, and agree with you. So you have acceptance of 10 people. Does that make your theory an accepted scientific theory? No. …".
Actually, I would be happy if 10 people figured it out and accepted it. As I said above, my objective is NOT to make my model THE accepted theory.
In fact, I think that the current abysmal situation with superstring theory shows that it is BAD for ANY single model (mine included) to become so "accepted" that alternatives cannot be made available in the archived records of physics for evaluation by anyone with interest. The "archive" part is important, because sometimes it is decades before some useful stuff is appreciated as being useful.

Steve M, you say that you "… think your work is more suited to something like Journal of Mathematical Physics, Communications in Mathematical Physics or Advances in Theo. Math Phys. J. Math. Phys. … Phys. Rev. D is rigid and pretty conservative …".
My comments here may have been misleading to you in that I have emphasized mathematical structures. However,
I consider the phenomenological part of my model (not mentioned by me hereinbefore because of less connection with the topic of Clifford algebras) to be equally important. An example is the relevance of my model to the interpretation of Fermilab's T-quark event data, and the idea (based on a paper by Froggatt at http://xxx.lanl.gov/abs/hep-ph/0307138) that the data show not one single peak for the T-quark at around 170 GeV, but also two other peaks (one higher, one lower) that can be reasonably interpreted by seeing the Fermilab T-quark data as showing three peaks coming from a T-quark – Higgs – Vacuum system. Such material seems to me to be off-topic for the mostly purely mathematical journals that you recommend. It is my opinion that the most appropriate journal (in my home country of the USA) that covers both the math/theory and phenomenology aspects of my model is Phys. Rev.D, which is why I chose it as the journal for my prize offer.

Tony Smith http://www.valdostamuseum.org/hamsmith/

13.
March 12, 2005

"In the path integral formalism, why are we integrating over a space of connections the exponential of the norm squared of the curvature of the connection? Why the determinant of the Dirac operator?"

Renormalizability and universality classes?

14. **Quantoken**
March 12, 2005

Tony Smith said:

"Unless someone is already familiar with ALL of those things, they CANNOT understand my model without spending a lot of time and effort on learning background material that may not be of interest to them for any other reason."

Tony, I am troubled by that and your $100,000 reward. That is not how a theory gets accepted as science. Put it this way, if at the end of day, only ten people out of the whole population of this planet manage to figure out what your theory is all about, and agree with you. So you have acceptance of 10 people. Does that make your theory an accepted scientific theory? No. You could argue that your theory is correct, it is only because 99.9999% people are unwilling to spend the time to try to understand your theory. But that is useless.

Let's look at another example. Super string theory. Seemingly this is an **"accepted"** theory at current time, despite of the fact that it's unable to make any predictions. Super string theory is taught on almost every colleague campuses and many students accept it because their teachers seem to accept it. As for the general public, they are unable to understand the math involved, but they too have learned a few basic ideas like elementary particles are made of little ringy strings, etc. And they have watched scifi movies about wormholes and cosmic strings and such, and think those stuff are neat and cool. So they too, accept super string theory as a legitimate scientific theory.

So, at currently time, super string theory is a theory **accepted** by the general public and the general scientific community, and opponents are only minority, due to the propagation. That's a sad fact.

But it could change, eventually people will say: This thing can't make any predictions and seem to be useless, why should we accept it and continue to support it? By the time, there may still be a couple thousands "experts" in this field that continue to believe in the stuff, including all of the well known names like Witten, Lubos, Vafa etc. But they will be a minority groups and marginize and be reject as a colt group, once the GENERAL PUBLIC, I repeat, the GENERAL PUBLIC, not the elite peoples in ivory towers, become uninterested in the ideas. You can argue: "But I am the expert in the field" but it really doesn't help you if your whole field is rejected by the general "none-experts".

So acceptance of a science is really acceptance by the **general public**, not just acceptance by a elite group. Another example is General Relativity. There is a saying that initially there were only 12 people in the world who can understand GR. If it stay that way, GR will never become an accepted scientific theory. The truth is even most people are unable to understand the tensor mathematics. They are perfectly capable of underdstanding, and accepting the equivalence principle, so they have no problem accepting the GR as a correct theory derived from equivalence principle.

So my point is, whatever theory you have, to have any hope of being accepted. You must be able to describe the **basic idea or basic principle** in very simple language, to the general public. So even the general public can accept the basic principles of your theory. Then those interested can further study the detailed mathematics.

Science is not an enterprise in the ivory tower. Science is an enterprise in which not only researchers exchange ideas among themselves. But they also need to actively promote their ideas to the widest audiences in the general public.

That's why it's so important for dissident scientists like Peter et all, to turn towards the general public audience, and explain to them why there is a problem and why string theory does not work. The big problem is different opinions are **SUPPRESSED** in the public medias, so the public never realize that the main stream scientific theories have problems in them!

Quantoken

15.
March 12, 2005

It seems to me that the idea that beautiful mathematics will lead to new physical principles is as likely to succeed as string theory. I think it is the other way round, mostly, physical insight bends beautiful mathematics into its service. However, there seems to be no other way to proceed, so I can only hope that someone is successful.

16. **Steve M**
March 12, 2005

Hi Tony,
$100,000 eh?:). I am a (theoretical) physicist and have also worked as a freelance editor/copyeditor in the past for some pretty technical stuff in physics and biology. If I could make a few points. First I think your work is more suited to something like Journal of Mathematical Physics, Communications in Mathematical Physics or Advances in Theo. Math Phys. J. Math. Phys. is particularly open to new mathematical approaches and interpretations to physical problems and for development of mathematical ideas relevant to the formulation of physical theories. They publish some really mathematically dense and technical stuff. Second, I think you should be flexible about the title. As a journal Phys. Rev. D is rigid and pretty conservative. They reject a lot of technically sound papers (probably the most interesting ones).

Generally, it is very difficult to get a "new paradigm" published and accepted by the community (dreadful word I know:) especially as a single author, and much easier to publish something interesting or minor within an ongoing and accepted research direction. At any rate, a totally professional Latex presentation is always essential nowadays. Two or three readable shorter papers is also better than a huge submission, which is offputting to reviewers. A thorough review of the existing literature relevant to the problems you are attempting to solve is always essential, explaining what you think are the shortcomings of the standard approaches and why your approach might make progress where others have failed. It is also better to be have a modest but interesting presentation style/title rather than making huge claims. Making massive claims in titles and abstracts and having a pompous and self-important style of presentation (I am not saying this applies to you)usually results in a crackpot label and instant rejection, even if the paper has a lot of substance and very good ideas. Anyway, these are just some thoughts based on my experiences both editing papers and publishing my own.
regards

17. **Tony Smith**
March 12, 2005

Hi Garrett. You say that I should use LaTex in a self-contained introductory paper. Actually, back in the days before I was blacklisted, I did put up such a paper on what is now known as arXiv. It is at http://xxx.lanl.gov/abs/hep-ph/9501252 It does not incorporate some changes and corrections that I have made in the last 10 years, but the basic ideas are similar. It is over 100 pages long, and the part about the Mayer-Trautman-Kobayashi-Nomizu material does not appear until about page 60 or so.
Even though it was extensive and in LaTex, nobody paid any (contructive) attention to it, so I really don't think that any lack of the LaTex look is why people don't understand it. I think that the real difficulty is that you cannot formulate the model without using such things as:
1 – Clifford algebras;
2 – Quaternions and Octonions;
3 – the Mayer-Trautman-Kobayashi-Nomizu material;
4 – theory of bounded complex domains and their Shilov boundaries, as described by L. K. Hua in his book Harmonic Analysis of Functions of Several Complex Variables in the Classical Domains by Hua (Am. Math. Soc., 1979;
5 – generalization of MacDowell-Mansouri mechanism for gravity;
6 – I. E. Segal's conformal gravity material; and
7 – how to generalize the Hyperfinite II1 von Neumann factor to the case of real Clifford algebras from the case of complex Clifford algebras on which the usual Hyperfinite II1 von Neumann factor is based.
8 – For the lattice version of my model, familiarity with generalization of the Feynman Checkerboard is needed (see my paper at CERN-CDS-EXT-2004-030).?

Unless someone is already familiar with ALL of those things, they CANNOT understand my model without spending a lot of time and effort on learning background material that may not be of interest to them for any other reason.

Since I am a lone individual with no institution that could provide grants or jobs related to studying my work, it is understandable that nobody would spend such time and effort.

It is with that in mind that I have put up a $100,000 prize for the first person to meet its conditions, which include writing up my model, getting it posted on archives in

arXiv, and getting it published in Phys. Rev. D. A statement of the prize details is at http://www.valdostamuseum.org/hamsmith/VoDouPhysicsPrizeV.html
Maybe nobody will try to do the work for the prize, but on the other hand maybe somebody will, and at least the first person who succeeds will get some money for the time and effort expended.

Tony Smith http://www.valdostamuseum.org/hamsmith/

18. **Quantoken**
March 12, 2005

Doug:

"What is time" is a question so profound that it is beyond mathematics to try to find an answer. The concept of time and time arrow inheritantly associates with the concept of causality. If there is no causal relationship between things, then time does not exist. But is the causal relationship just our perceptions of the world, or is it part of the reality?

Mathematics, on another hand, is totally incapable to understand what is causality. Because causality relationships do NOT exist in any branch of mathematics. In math you can say 2+3=5, or you can say 5-3=2. But among 2,3,5, who is the cause, and who is the result caused by the cause? The answer is none, "2+3=5" is NOT the reason why "5-3=2", nor is it a result of "5-3=2". They are equivalent to each other but does not depend on each other as cause and result, so there is no causal relationship.

All of our difficulties in understand spacetime is because math does NOT have a causality relationship, but causal relationship must be introduced to understand time properly.

Quantoken

19. **Garrett Lisi**
March 12, 2005

Hi Peter,
First off, thanks for keeping such a cool journal — and for being a voice of reason speaking out on the current physics emperor's state of undress. Your posts and the poignant reactions to them have been very amusing to read. You've also now hit upon a subject close to my heart — it always bothered me that spinor fields seemed to be "cooked up" rather than really derived from geometry, so I got drawn into looking around for a better way. I've also been inspired by the attempts to derive the structure of the standard model using Clifford algebra — so far my favorite has been by Greg Trayling:

http://arxiv.org/abs/hep-th/9912231

The only cool thing I've found myself is a way of deriving spinors as BRST ghosts associated with the Clifford adjoint invariance of a frame in GR:

http://arxiv.org/abs/gr-qc/0212041

That's the closest I've been able to get to a true geometric derivation of what spinor fields are. Using that and Kaluza-Klein to connect with Trayling's work has been giving me fits because it's so close but not quite there. The universe doesn't just laugh at string theorists.

I see that Tony Smith is hanging around your journal as well (Hi Tony). I have looked around at his stuff, and found it to be very interesting and potentially good but I only wish he would spend the painful extra time necessary on exposition for people without his same eclectic background. If he put together an introductory paper to his stuff, maybe even using LaTeX… he'd probably get a much better reception, and I for one would have a better chance at really getting some of what he's saying. He especially needs to keep in mind people are lazy and need straightforward presentations of background material all in one place, rather than just references.

Anyway, good to finally come out from lurking and post something here.

Best,
Garrett

20. **Doug**
March 11, 2005

If fundamental physics deals with the matters of space and time, and fundamental mathematics deals with the matters of geometry, then the connection between the two should not be surprising, because ultimately geometry is the study of space. When advances in mathematics enabled men to add time concepts to these geometrical aspects, mathematics became indispensable to the investigators of physics.

However, it's the fundamentals of space and time that are the core of physics, not the mathematical expressions of space-like and time-like constructs, or various unions of these. The fact that spinors are a mathematical construct hearkens back to others, more primitive, but similar, such as Pythagoras' theorem. Clearly, Pythagoras' theorem tells us something about space and spinors tell us something about time, but whatever it is can be misleading if we begin to think of these properties as real. Space can have no meaning without time because you can't draw a line without the time to do it. Time can have no meaning without space, because, no matter how much time you have, you can't rotate it.

All that this suggests is that "the deep relationship between Clifford algebras and spinors, geometry, index theory and physics," exists because of the deep relationship between space and time. That relationship is reciprocal and symmetrical in the equation of motion, just like the 8 hour "Clifford clock," is reciprocal and symmetrical. What Lubos apparently doesn't understand is that the difference between the elementary and sophisticated is not profundity, but elaboration. The profound secrets we seek to uncover are hidden in the simple, but correct relationship of space and time. Sophisticated elaboration of an incorrect understanding of this elemental relationship only serves to further obscure the error.

21. **Tony Smith**
March 11, 2005

Peter, you say that you "… wonder if you [Tony] really strongly disagree [with the thought that] … there is a crucial idea missing here … what seems to me [Peter] to be missing is some deeper link between the geometry and the dynamics …".

I can see how what I think is an intuitively natural link between the geometry and the dynamics might appear to others as an ad hoc construction.

Roughly (again ignoring a lot of technicalities) the way that I link geometry and dynamics is:

1 – describe dynamics by a Lagrangian;

2 – define the Lagrangian in terms of Clifford algebra structure:

a – the 8-dim spacetime over which integration takes place is represented by the 8-dim vectors

b – the gauge term in the Lagrangian is based on the 28-dim bivectors

c – the Dirac term is based on the 8-dim fermions and antifermions.

3 – See what happens to the Lagrangian when the freezing/choice of a quaternionic subspace of the initial 8-dim spacetime changes it to a Lagrangian over a 4-dim spacetime. This is highly nontrivial but it does take you from a very simple-looking Lagrangian with 8-dim spacetime to a much more complicated one with 4-dim spacetime, but it turns out that the complicated 4-dim Lagrangian is actually quite realistic.
This is done by using some geometric techniques done about 20 years ago by Meinhard Mayer http://www.ps.uci.edu/physics/mayer.html who worked with A. Trautman. They used some key ideas from Kobayashi and Nomizu's book Foundations of Differential Geometry, vol. 1 (John Wiley 1963), particularly Proposition 11.4 of chapter II, and their work in some detail can be found in Hadronic Journal 4 (1981) 108-152, and also articles in New Developments in Mathematical Physics, 20th Universitatswochen fur Kernphysik in Schladming in February 1981 (ed. by Mitter and Pittner), Springer-Verlag 1981, which articles are:
A Brief Introduction to the Geometry of Gauge Fields (written with Trautman);
The Geometry of Symmetry Breaking in Gauge Theories; and
Geometric Aspects of Quantized Gauge Theories.

It may be from some points of view regrettable that my link between geometric structures and Lagrangian dynamics is heavily dependent on really understanding that material, but that is the way it is.
If the Mayer-Troutman-Kobayashi-Nomizu material seems so natural to you that it is (to use a phrase that is sort of a joke among mathematicians) intuitively obvious, then my link might seem natural.
Otherwise, my link would probably be thought of as a missing link.

The details are in my papers and web site, but I am not the world's best expositor (I wish I has 1/10 the talent of John Baez in that regard).

Anyhow, long story short, if anyone does not clearly understand the Mayer-Troutman-Kobayashi-Nomizu material then they will definitely not see my model as a natural construction.

On the other hand, when (years ago) I began to work through the 8-to-4 dimensional reduction part of my model using the Mayer-Troutman-Kobayashi-Nomizu material and to see how after grinding out the results that they actually look realistic, it made me think that there must be something fundamentally right/useful about the work.

I could go into more material related to dynamics of Lagrangians and path integral quantization, but this comment is already long and I thought that the most basic point was how the parts of the Clifford algebra give a realistic 4-dim Lagrangian, which is fundamentally an application of the Mayer-Troutman-Kobayashi-Nomizu material.

Tony Smith http://www.valdostamuseum.org/hamsmith/

22. **quantoken**
March 11, 2005

Peter said:

"But it definitely is one of my deepest beliefs that the most fundamental structures in mathematics are intimately connected with the most fundamental structures in physics. There seems to me to be a lot of evidence for this."

I can almost totally agree with you, except for one small reservation (which is actually not small). That is, what is considered **"most fundamental structures in mathematics"** is really "in the eyes of the beholder". i.e., it's totally opinious unless that judgement (what is more fundamental) is guided by empirical physics experimental evidences.

Mathematics is much bigger than physics. There are infinitely many possible self-consistent theories in mathematics. It's hard to make a judgement what is fundamental and what is not. And not all mathematics can have something to do with physics. Number theories, or the study of prime numbers or such, seems totally unrelated to physics. At least all I know, any prime numbers bigger than trivial ones like 2,3,5, never occurs in any physics formulas. The string theory, for it's "11" dimentions, may break this rule. But then maybe not if ultimately it's shown that string theory has nothing to do with physics:-)

Tom Smith, what you claim seems to be interesting if it is true:

"In short, you get all the particles of the Standard Model and none of the unobserved wino, gluino, squark, zino, etc particles of naive 1-1 supersymmetry, and you get reasonable values for all the otherwise ad hoc parameters of the Standard Model. "

But I glanzed at your web in the past and could not associate its content, which is hard to read and understand, with your claim. Maybe I will try to spend a little bit time figuring out exactly what your ideas are.

Quantoken

23. **Peter**
March 11, 2005

Hi Tony,
Sure, you're one of the people I had in mind when I wrote the posting. I've always been intrigued by the kind of thing you're doing (and think it's absurd that the arxiv happily accepts absurd string theory related papers but not yours). But I still think there is a crucial idea missing here and wonder if you really strongly disagree with this. To be a little more specific, what seems to me to be missing is some deeper link between the geometry and the dynamics. In the path integral formalism, why are we integrating over a space of connections the exponential of the norm squared of the curvature of the connection? Why the determinant of the Dirac operator? I tend to think we need some insight into these mysteries in addition to more kinematical ideas about Clifford algebras.

24. **Tony Smith**
March 11, 2005

Peter, you say:
"… none of … [the] attempts … to try and use Clifford algebras to come up with a fundamental particle theory … yet seems to me [Peter] … to give a really convincing explanation of how it is related to the standard model. …".

I cannot disagree with that statement because "seems to me [Peter]" is a statement of subjective opinion and you [Peter] are certainly entitled to your opinion.

Also, Lubos asked:
"… Which "ideas" exactly do you think would "explain the structure of the Standard Model"? …".

About the only strictly relevant thing that I can say is that I am one of the "… Many, many people [who] have been motivated by this kind of idea …", and that my efforts use structures related to Cl(8) which, as John Baez points out in his week 211 (among other writings) is, due to real 8-periodicity, a natural building block of any arbitrarily large real Clifford algebra. Roughly (ignoring many technical details) my physical interpretation is:

The 8-dim vector part of Cl(8) represents an 8-dim spacetime;

The 28-dim bivector part of Cl(8) represents 28 gauge bosons;

The 8-dim +half-spinor part of Cl(8) represents 8 first-generation fermion particles (e-; r,g,b up quarks; r,g,b down quarks; nu_e);

The 8-dim -half-spinor part of Cl(8) represents 8 first-generation fermion anti-particles (e+; r,g,b up anti-quarks; r,g,b down anti-quarks; anti-nu_e).

If you break the 8-dim spacetime into a 4-dim physical spacetime plus a 4-dim internal symmetry space,
by freezing out at low (current experimental energy levels) a particular quaternionic subspace, then:

The 28 gauge bosons split into 16 for U(2,2) which contains the conformal group SU(2,2) = Spin(2,4) which along the lines of work of I. E. Segal and MacDowell and Mansouri give you an Einstein-Hilbert lagrangian etc plus 12 for the SU(3)xSU(2)xU(1) Standard Model; and the fermion particles and antiparticles get a 3-generation structure.

If you look at the geometric structures in a way motivated by (but not identical to) the work of Armand Wyler, then you can unambigously calculate particle mass and force strength ratios, getting results that are at tree level quite realistic if you consider the quark masses to be constituent quark masses.

In short, you get all the particles of the Standard Model and none of the unobserved wino, gluino, squark, zino, etc particles of naive 1-1 supersymmetry, and you get reasonable values for all the otherwise ad hoc parameters of the Standard Model.

It is OK with me if you [Peter], Lubos, and anyone else find my structure unconvincing, because that is just an expression of your personal opinions and tastes, but it would be interesting to imagine what the PR blitz would be if a prominent string theorist were to come up with similar results.

Details of my work exist, but they are too long for this message. However, they can be found on my web site and on a few papers such as CERN EXT-2003-087 which I was able to post before CERN terminated the EXT series (in October 2004, possibly at the behest of arXiv, which has blacklisted some people including me and may have been unhappy that CERN EXT provided a way for some of us blacklisted people to post our work where it might be preserved for posterity (note that web sites, ISPs, etc come and go, and CERN's web site is likely to last much longer than any web site I might have)).

Tony Smith http://www.valdostamuseum.org/hamsmith/

25. **Peter**
March 11, 2005

Hi z,

I'm a bit leery of the term "beauty" in this context, what with "all in the eye of the beholder" and everything. Somewhere on the internet there's an argument between me and Lubos about the beauty of string theory.

But it definitely is one of my deepest beliefs that the most fundamental structures in mathematics are intimately connected with the most fundamental structures in physics. There seems to me to be a lot of evidence for this. In my posting I was explaining a bit about how fundamental Clifford algebras and spinors are in geometry (and I didn't even get into their importance in K-theory). One of the other most fundamental ideas in modern geometry is that of a connection and its curvature. The fact that all these structures show up in the standard model seems to me not a coincidence.

But neither mathematics nor physics is a finished subject. As another commenter mentioned, it's still a matter of debate among mathematicians whether we really understand the right way to think about the geometry of spin. We know spinors are fundamental because they are the representations we can build all others out of. But there are several different ways of constructing spinors, some that don't use Clifford algebras at all (see e.g. Graeme Segal's Borel-Weil sort of construction as a space of holomorphic sections). Clifford algebras are part of the story, but maybe not the most fundamental one. I don't think we know yet. Similarly, in physics we know that spinor fields, the Dirac equation, connections and the Yang-Mills functional are fundamental parts of the story, because we have strong experimental evidence for this. But there quite possibly is some more fundamental way of looking at these things, one which would give us the right idea about how to get beyond the standard model. I'm guessing that such a new idea exists, but is missing now, and that whatever it is, it will be related to some new perspective on the geometry of spin.

There's more detailed speculation of this kind in my paper of "Quantum Field Theory and Representation Theory". Maybe I'm wrong, and in any case, it's certainly historically been true that theorists have needed help from experiments to figure out what the right fundamental mathematical structures are. But in these times of no help from experiment, I don't think we have much choice but to try and see if mathematical insights can help us see which way to go with the physical theory.

26. **quantoken**
March 11, 2005

Z:

It's a profound philosphical question that has been debated for thousands of years. That is whether fundamental physics structures, like the standard model, can be derived from fundamental mathematics structures, like say if there can be a mathematics based theory naturally leads to the particular value of all 17 free parameters of the standard model, or you can not?

Every one can have an opinion on that. But I think it's far from being settled. So it is totally legitimate for Peter to ask that profound question as being insightful, and it is childish for Lubos to question Peter's intelligence in raising the legitimate question.

Personally, I do believe that the standard model as we know it, must be reducible in some way. Not all 17 free parameters can be foundamental. There must be a connection between them and there must be a mathematical model describing that connection, and therefore it would allow us to reduce the standard model into something simpler with less number of free parameters.

But I do not believe physics can be reduced to pure mathematics. When the ultimate theory of everything is discovered, you would have reduced all physics parameters to just a few, or maybe just one, the famous alpha. But you can not reduce it further to zero free parameter. The very last un-explained physics constant, be it alpha, would have absolute no mathematical explanation whatsoever why it is the value it is. It is just happenedness, just the way it is in this universe, with no more question allowed to be asked why or how.

Quantoken

27.

March 11, 2005

Peter said:

“Many, many people have been motivated by this kind of idea over the years to try and use Clifford algebras to come up with a fundamental particle theory, one that would explain the structure of the standard model.”

I can sympatize with these attempts–wouldn’t it be neat if some simple/nice algebraic structure were behind our “fundamental” theories? However, this kind of an approach tends to remind me that of trying to use platonic solids in planetary astoronomy. That, too, would have been neat if it worked.

I find Peter’s last sentence

“Some crucial idea still seems to be missing.”

rather strange. There seems to be a presumption here, that this idea of explaining the structure of the standard model by using Clifford algebras in some cool way is, in fact, correct.

There is no strong evidence for this. Beauty of a mathematical structure does not guarantee its relation to a field of physics of one’s choice.

I have to say I am rather amused by seeing this kind of a statement about such an issue, made by Peter.

z

28. **quantoken**
March 11, 2005

E:

Indeed this is an open question far from being settled. And peter is rightful in pointing that Lubos had said nothing intelligent but just personal attacks. But forgive him for feeling depressed a bit that people are unable to post comments on his weblog this past few days.

And why do you think Lubos is a “prominent” figure, just because he made himself famous by being active on the internet? He is just a junior assistant professor. In my judgements, Lubos is a smart guy and he may have learned a bit of math tools useful for string theory research but he is absolutely lacking in basic physics instinctions that I think any one with some good basic training in general physics should have.

One good example is he could not think of a possible method of measuring solar radiation intensity above the atmosphere, without a tool or mean to actually go above the atmosphere to measure it.

Quantoken

29. **E**
March 11, 2005

Dear Lubos,

You may be misunderstanding something… or perhaps you are simply part of a muscular new wave within the academy.

Peter is simply aligning himself on one side of a decades-old debate between some of the most distinguished living mathematicians. On one side are mathematicians, like Singer, who believe that after the rediscovery of the Dirac operator, spinors have been nearly perfectly understood. On the other side are mathematicians, like Sullivan, who have amassed evidence that we are merely nibbling around the edges of a giant poorly understood structure.

I suppose what suprises me is that someone as prominent as yourself could be simply unaware of the debate. Perhaps physicists have recently resolved this issues amongst themselves. Mathematicians are probably nowhere close to this point. Look at how long it took both groups to replace self-dual equations with SW. Think of how mysterious the spinor condition was in the role of the rigidity of the signature operator on loop space.

I myself am not sure which group is correct, but the idea of personally deriding people like Lawson, Sullivan, Woit, Bourguignon, Taubes, Hitchin, who have spoken publically about this belief in the incompleteness of spinor theory is quite bold (given the above).

Of course, I have heard Is Singer explain his convictions on this point which I suppose vaguely echo your beliefs. But even given his role in the history of the Dirac operator, he was always humbly aware that he might be wrong. Perhaps your impatience indicates that you will soon delight us with profound contributions in this area that will settle this debate conclusively. Should that happen, we will all be glad for the light shed on this fascinating topic.

I will read your blog with interest and wish you every success.

30. **Peter**
March 11, 2005

If anyone has anything intelligent to say about the topic of my posting, please do so, but I’ve already had to delete comments from people who think an intelligent response to anything I write is to personally attack me. If string theorists feel my comments about string theory are attacks on them and feel the need to personally attack me when I write about string theory, that’s fine. But I’m not going to put up with juvenile attacks based on my explanations of what a Clifford module is.

31. **Lubos Motl**
March 11, 2005

I just can’t resist. Your last paragraph, Peter, is such an incredible stupidity that I must emphasize it a bit more clearly.

Spinors and Clifford algebras are of course closely related and this relation is well understood. By “understood”, I don’t mean that everyone understands everything about it. I mean that mathematicians and physicists have understood it for decades (especially the physical consequences of it), in most aspects long time before the Standard Model was constructed. Spinors and their properties are just an elementary mathematical piece of a Standard Model, one of many technicalities. Spinors in 3 or 3+1 dimensions were understood a few months after quantum mechanics was first proposed.

Which “ideas” exactly do you think would “explain the structure of the Standard Model”? Spinor is just a spinor. It’s an almost complete triviality that the physicists today must understand as undergrads. Your “ideals” about physics are equivalent to those of a high school student who learns about spinors earlier than when she’s ready to comprehend them fully.

Of course that there are “tantalizing relations between supersymmetry and Clifford algebra” – it’s because the generators of supersymmetry transform as spinors. It’s not really tantalizing; it’s elementary.

You’re making an invisible mysterious elephant out of an ordinary gray rat.

32. **Lubos Motl**
March 11, 2005

Hi Peter,

is this article directed to the laymen, or the physicists? In the latter case, I suppose that it is a part of your plan to convince all physicists to study the Dirac operator. 😉

The things you write is elementary math from the early 20th century.

Best
Lubos, 2005

33. **Jody**
March 11, 2005

Hi, Peter,

There is a very nice discussion by Atiyah of the deep relationship between Clifford algebras and spinors, geometry, index theory and physics in the short article he wrote “The Dirac equation and geometry” for the book:

Paul Dirac: The Man and His Work
Edited by Peter Goddard
Cambridge 1998

BTW: I really enjoy reading your blog! Keep up the rant.

Cheers,
Jody

Skeptical SF Chronicle Article

March 14, 2005
Categories: Uncategorized

Today's San Francisco Chronicle contains an article about string theory entitled "Theory of Everything" Tying Researchers Up In Knots. It's by science writer Keay Davidson, and is about the most skeptical article on string theory I've seen in the mainstream press. The lead sentence is:

"The most celebrated theory in modern physics faces increasing attacks from skeptics who fear it has lured a generation of researchers down an intellectual dead end."

Davidson contrasts Michio Kaku's very pro-string theory point of view in his new book Parallel Worlds, with the much more skeptical views of Lawrence Krauss, who evidently has a book entitled "Hiding in the Mirror: The Mysterious Allure of Extra Dimensions" coming out in September. He also got comments about the current state of string theory from quite a few different people, including yours truly. The article contains a link to this weblog.

Some of the string theory critics quoted are just inherently opposed to any new mathematical approach to fundamental physics, something I have no sympathy with. One of these is Stanford's Robert Laughlin, who makes the point that string theorists are trying to camouflage the theory's increasingly obvious flaws by comparing the theory to "a 50-year-old woman wearing way too much lipstick." Because of Laughlin's extreme anti-mathematical theory views on the one side and those of his colleagues like Lenny Susskind on the other, "The physics department at Stanford effectively fissioned over this issue" says Laughlin. He goes on to say "I think string theory is textbook 'post-modernism' (and) fueled by irresponsible expenditures of money." For the record, I'm no more of a fan of Laughlin's views about particle theory than I am of Susskind's.

Some of the quotes from defenders of string theory are a bit strange, with none of them addressing the fundamental problem the theory is facing these days as it becomes obvious that it can't predict anything. John Schwarz is quoted as saying "string theory is the only approach that has the potential for explaining dark energy" which is kind of peculiar since it is well-known that superstring theory naturally leads one to expect a value for this energy density that is off by 120 orders of magnitude. The only way around this seems to be the "landscape" argument, in which you essentially give up any hope of ever predicting anything. The other defenders of string theory quoted in the article mainly try and claim that twenty years of work on the theory is still nowhere near enough, that it is way too early to be able to evaluate it yet. They don't give any indication of how much longer we should wait for such an evaluation, but if twenty years isn't long enough, it sounds like they hope this won't occur while they're still alive.

Update: For a very different take on this, see Lubos Motl's posting.

Comments

1. **Aaron**
   March 20, 2005

   To ksh95: You don't actually seem to have read anything I've said beyond the first sentence.

   Try, for example "On the other hand, I firmly believe that we have to practice science as if the anthropic principle were false, but that's another story."

2. **D R Lunsford**
   March 19, 2005

   ksh95, very funny, but..

   The real problem is, I don't think it's possible to argue with these folks. They just don't care. The watchword is, "Be the Wibble."

   -drl

3. **ksh95**
   March 19, 2005

   I forgot to mention, as presently formulated the anthropic principal isn't even a useful calculation tool. If, for instance, an anthropic principal based string theory could reduce the 20some parameters of the standard model to 5 parameters, then I would be at least willing to acknowledge the anthropic principal as a valuable calculation tool.

4. **ksh95**
   March 19, 2005

   Aaron said
   "…And, I agree with you that the anthropic principle is nonpredictive. My only point is that it really could be the right answer…"

   Your point of view really makes no sense to me.

   The anthropic principle can never be the right answer any more than it can be the wrong answer.

   As far as I'm concerned the anthropic is equilivant to:
   The reason Peter really started this blog, and the reason I replied to your post is because there is an acausal point in spacetime that effects all other points. That point is inside
   Wibble the three-toed chicken's head at time infinity + 7. Wibble determines the past by thinking in the future….Actually, this argument is slightly better than the anthropic principal since it may be possible to rule out
   acausal influences on present events.

   If, as you say, the anthropic principal is really the true state of the universe, then the answer produced by science **must** be that the true nature of the universe in unknowable. At this point physics reaches its end. Some go on to believe in the anthropic principal, others believe in Creation, while I believe in Wibble the three-toed chicken.

   Susskind may cloud his ideas in prose about a cloud covered earth and such things, but what he really advocates is reformulating science such that it looks something like "…If we assume A (which can never be proven or disproven) then the rest of the universe follows…"

   I say why make it hard on ourselves by letting A=multiverse. If we would have let A=Wibble the three-toed chicken we could all have avoided a lot of years of schooling.

5. **Juan R.**

March 19, 2005

Since Peter deleted my previous post, I reproduce it again but extended and without the **supposed** ?*speculations on own work*?.

During some time I wrote several comments regarding the impressive failure of stringy research. It is unnecessary a costly experiment for showing this.

The basic idea of stringy research is as follows: to take the hypothesis *A* and make one article, next neglect the hypothesis and make another article contradicting the first one. Even with this infinite malleability, string theory has shown to be a failure. Perhaps failure is a harsh strong word for sensible people, but I would remark that physics is just one of hard sciences, and ?hard? signifies hard.

Dear ??, do you know that some people is working in more than 11D whereas others are claiming for a 4D string approach?

The history of dimensionality (I do not include Kaluza-Klein) is
4D -> 26D -> 10D -> 11D -> 12D? 13D? 4D?…

History shows us that if you write two columns and at one (left) write the past claims of stringys and the other (right) our current knowledge, you find a surprising feature.

the entire left column was completely wrong!

Of course, string theorists will say you that the topic is open. *This is not an excuse!* Decades ago, stringys claimed in public that the theory was the Last Formulation. What arrogance! Some of use already knew that the elementary stringy approach could not be applied to simple piece of hot water. Now, they are using the TFD approach for a generalization of the ?old? brane theory based in the outdated Hilbert-Fock space mathematics and the standard vacuum. However, even using TFD, brane theory is still not sophisticated enough.

Therefore is not necessary experiment for understanding that string theorists are in the wrong way. I am not saying that are clever guys or no, just saying that are in the wrong way as Eisntein when ignored the rest of science and focuses on its own (wrong) idea about the world. Therefore, the recent words of Susskind are not surprising for some of us that studied string theory and pointed to its obvious flaws.
?*Many physicists hoped that string theory would be the mathematical ?silver bullet? that would uniquely explain our world. But the more we learn about cosmology and the more we learn about string theory, the less likely this seems*.?

Lisa Randall (from Harvard) says:

?*Originally, string thoeirsts hoped string theory would dictate these parameters. But this is looking increasingly unlikely*.?

On a this year comment, Witten states:

?*One of the few things we do know is that, with string theory, theoretical physicists have stumbled upon a theory that looks like it might be the unified field theory*.?

Humm… He now abandons his earlier grandiloquent evaluation of the theory and carefully uses the combination **look like it might**. Interesting!

6. **Thomas Larsson**
   March 17, 2005

   More comments on this article can be found here.

7. **Catt acolyte**
   March 17, 2005

   Witten is denounced by his mentor Penrose, 'Road to Reality' (UK ed., 2004). On page 896, Penrose analyses those who use string ?theory? as an obfuscation of gravity?s cause:

   ?In the words of Edward Witten [E. Witten, ?Reflections on the Fate of Spacetime?, Phys. Today, April 1996]:

   ? "String theory has the remarkable property of predicting gravity,"

   ?and Witten has further commented:

   ? "the fact that gravity is a consequence of string theory is one of the greatest theoretical insights ever."

   ?It should be emphasised, however, that in addition to the dimensionality issue, the string theory approach is (so far, in almost all respects) restricted to being merely a perturbation theory ??

   More at http://members.lycos.co.uk/nigelbryancook/Penrose.htm

8.
   March 16, 2005

   Lubos,

   Thank you for explaining your position. You are right: the article was weak and should have been stronger.

   I think you are however, exactly missing the point from my perspective. I hope you find this post informative; no one wants you to become yet another string theorist who never matured into a particle theorist.

   Richard Feynman (a non-expert in string theory who might have been interviewed if he were alive) left us the following admonition:

   "Details that could throw doubt on your interpretation must be given, if you know them. You must do the best you can–if you know anything at all wrong, or possibly wrong–to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it. There is also a more subtle problem. When you have put a lot of ideas together to make an elaborate theory, you want to make sure, when explaining what it fits, that those things it fits are not just the things that gave you the idea for the theory; but that the finished theory makes something else come out right, in addition."

   Can you hear the music, or is your mind already thinking about retorts?

   Those of us non-string theorists have been waiting for the informed self-critique of string theory to come FROM the string community. Some of us have been waiting for 20 years.

Because String theory is nearly opaque to an outsider, we are actually dependent on string theorists to be their own worst critics.

By all means Lubos, save us from the Woits, the Andersons, the Friedans, the Feynmans, the Glashows. Now is your moment. Tell us EVERYTHING you know to be seriously wrong with the theory, its practitioners and its history. When theories, mature, they become a bit more humble. Unfortunately, it appears from your posts that you do not yet enjoy that luxury.

Of course, maybe you just don't know the history.

Do you know that people who used to work on 11D supergravity before strings were derided by early string theorists who thought it irrelevant? That for years 10 and 26 were proclaimed as the ONLY relevant dimensions? Are you aware that there were once a provably small finite number of string theories and that this fact was used relentlessly to sell string theories to a sceptical and largely disbelieving world? Are you aware that for years there were emphatic dismissive answers to the question of 'why are strings the only extended objects with branes irrelevant'?

One could go on I suppose. Let me simply observe however that as string theory improves, it has been proving itself to be wrong in its earlier zeal and dogmatism for years. It just never stops to face its former incarnations.

Lubos, you are young and will be forgiven because of what I assume is your brilliance and contributions. But, like others before you, you will also run out of time if you don?t make contact with the world beyond string theory. We have been cheering for the String community for what is beginning to feel like an eternity. We hope like the Red Sox you will eventually get it together and win one for all of us.

Oh and one last thing. The theory community as a whole has built up a lot of good will over the years by achieving a great deal scientifically while teaching its non-experts critics rather than deriding them. Just make sure you put back scientifically far more than you withdraw.

That is quickly becoming a tall order.

9. **Aaron**
March 16, 2005

"My objection to Aaron is that he seems willing to take seriously theoretical ideas that don't make any predictions."

These days, I'm mostly interested in the mathematics part of strings. And, I agree with you that the anthropic principle is nonpredictive. My only point is that it really could be the right answer.

Depressing, I know, but not impossible.

10. **Peter**
March 16, 2005

I saw the Kolb et. al paper last night, was thinking of writing something about it here, but have been too busy. Also, I'm not enough of a cosmologist to know how to evaluate what they are doing. I hope we'll hear from Sean Carroll about this. (Sean, you reading this?).

If they're right, and the CC really is zero, that would be quite something. It would certainly put a big dent in the "Landscape" nonsense.

11. **Anonymous**
March 16, 2005

Peter, check out http://www.arxiv.org/abs/hep-th/0503117 : Rocky Kolb and some others obviate the need for dark energy by explaining how standard inflationary cosmology gives you accelerated expansion. If there's no cosmological constant, will string theorists give up on any of their nonsense?

12. **Lubos Motl**
March 16, 2005

Thanks, Peter, I've added a link, too. Today I have also huge traffic – because of the no-confidence vote – so it's about a fair deal… 😉

13. **Quantoken**
March 16, 2005

Peter said:

"There's nothing inherently wrong with the idea of fundamental constants varying with time and place."

I disagree. There is something inheritantly wrong by assuming fundamental constants may vary. It's inheritantly inconsistent with the principles of relativity, which says there is no special reference frame and no special place and time. All physics laws should look the same regardless where you make your observation and from what reference frame you make your observation.

Should findamental constants, like alpha, varies. Then we have to assume that all atoms and elementary particles around us would have to have the magic power of knowing where in space and time they sit, and collectively but independently decide on a specific alpha value, which is appropriate for their position and time, on which alpha value they thus exhibit their behaviors for us to see in the lab. Where would they acquire that **"magic power"** of knowing what is the "right" constant value? And how they would acquire that information? It's simply incomprehensible that is the case.

Certainly it is possible that experiments could indeed reveal a changing alpha, then we would have to re-think our reasoning and exam where it got wrong. But so far any experiment attempting to reveal a changing alpha has been very sketching, very doubtful, none-repeatable, and unconfirmed.

Remember the big noise of "changing light speed" by the Weber group 5 years ago? I was skeptical to start with. Where is that noise today? Why there is no followups or independent verifications? None!

In today's fiercely competitive academy environment there is full of dishonesty every where and one has got to be very skeptical in carefully evaluating any experimental results.

The so called CSL-1 cosmological string thing, is going to be the same Weber story 5 years from now. By this time there should be plenty of telescopes around the world take photo shots on this object. There is no independent confirmation so far! Isn't it laughable that string theorists jumped up to their joy at the first moment seeing this insignificant peck of dust in the image as "amazing confirmation (of their theory)" which is actually just a straw.

Quantoken

14. **Peter**
March 16, 2005

Hi Lubos,

Sure, I'll put a link to your posting. I had thought you would be too busy with the Summers affair to write about this….

15. **Lubo? Motl**
March 16, 2005

Great that I convinced you, Peter. For the sake of hearing both sides, I propose that we exchange links to our reviews of the article.

All the best
Lubos

16. **Peter**
March 16, 2005

Hi Lubos and anonymous,

No, I'm not anonymous. I also think Lubos's comment is perfectly relevant. I encourage anybody who is interested in the whole question of what is going on in string theory these days to read both what I write and what Lubos writes, then make up their mind themselves who has the better argument.

17. **Lubo? Motl**
March 16, 2005

Dear "anonymous",

it is not hard to determine that the author is not actually "anonymous" but it is Peter Woit.

All the best
Lubos

18. **Anonymous**
March 16, 2005

Dear Peter,

Could you please delete the last post — its completely irrelevant to the topic.

19. **Lubo? Motl**
March 16, 2005

For all readers who come from San Francisco Chronicle, a trivial comment: Peter Woit is ignorant not only about string theory, but also about modern theoretical physics and particle physics in general.

If you prefer a treatment of the string theory and theoretical physics topics that is more sensible by a few orders of magnitude, read

Lubos Motl's reference frane

L.M., Harvard

20. **Peter**
March 16, 2005

There's nothing inherently wrong with the idea of fundamental constants varying with time and place. But if you want to turn this idea into physics, you have to come up with a specific theory that implements this, one that makes some sort of predictions you can then go out and check. My objection to Aaron is that he seems willing to take seriously theoretical ideas that don't make any predictions. I'm not, especially when the whole motivation for doing this is to prop up a failed research program.

21. **Tlogmer**
March 16, 2005

Peter: Correct me if I'm wrong — I'm not a physicist — but the idea of multiple universes, while counterintuitive, seems grounded in reality, not merely "concievable". Relativity divides any unimaginably large, temporally bounded area into smaller universes (not with objectively defined borders, of course, but you get the idea); all you need in an unbelievably large area with fundamental constants shifting slowly over distance and voila, a huge number of slightly different universes.

(I'm not sure the terminology in that paragraph was right; hopefully the point came through.)

22. **Thomas Larsson**
March 16, 2005

Judging from a post in the beginning of this thread, Prof. Nauenberg, who was on the faculty (perhaps visiting) when I was a graduate student, has come out as a string (or at least Landscape) skeptic.

23. **Chris W.**
March 16, 2005

I posted a long comment on Peter's previous post ("Clifford Modules") whose final section elaborates on the ideas sketched in my comment below (somewhat off-topic) on science and the Anthropic Principle. The preceding sections are more directly concerned with the topic of that post (but perhaps not enough).

24. **D R Lunsford**
March 16, 2005

JC – I should also mention that the prime mover behind the Weyl theory, strict locality (what Weyl called "pure infinitesimal geometry"), is the very same prime mover behind the various conservation laws that appear in particle physics, and lives on nearly identical mathematical territory.

-drl

25. **D R Lunsford**
March 16, 2005

JC – Weyl's theory was almost exactly carried over into particle physics, only the gauge parameter was not the spacetime scale as such, rather, the phase of the wave function. I'd say this qualifies as "continuing essential relevance since 1918". In contrast, KK theory was already an historical footnote by 1935, and should have remained so in light of Pauli's analysis of its structure. It was clear to many of us that the string program was utterly doomed from the beginning, precisely because it relied essentially on retrieving a total failure from the dustbin of history. This has proven to be exactly right.

-drl

-drl

26. **JC**
March 15, 2005

When was the last time there was an ambitious theory or experiment which tied up 20+ years of many physicists' working lives, but still ended up in failure?

The only cases I can think of offhand were all those guys which worked on various unified field theories in the 1920's and for some decades afterwards, which largely produced nothing. Other than perhaps Kaluza-Klein theory, one hardly hears about any of their old theories today.

27. **Peter**
March 15, 2005

Quantoken + Tony,

Please discuss this somewhere else. I'll delete any further comments about this that are submitted.

28. **Quantoken**
March 15, 2005

Tony Smith:

Your calculation is pure numerology and has nothing to do with super string theory. It's not even an impressive numerology at all, to get a rough match of 75:20:4.5. I have looked at various forms of numerologies and I am getting good at recognize them when I see one. Unfortunately some times it is hard to make a distinction between real hard science and true numerology.

Quantoken

29. **Tony Smith**
March 15, 2005

Peter, you say:
"… there isn't … any string theory argument for the CC other than the landscape because one (Ooguri, Vafa, Verlinde certainly don't have one …) …".

If you allow "string theory" to include theories in which the strings are physically interpreted as world-lines (long strings as real particle world-lines, short strings as virtual particle world-lines), then you can get a path to a unique "string theory":

1 – 26-dimensions comes from usual string theory considerations;

2 – require Jordan algebra structure, which gives you the 26-dim traceless part of the exceptional Jordan algebra J3(O);

3 – require complex domain structures and Jordan symmetry, which give you the E6 Lie algebra with grading
8-dim g(-2) + 16-dim g(1-) + (28+2)-dim g(0) + 16-dim g(1-) + 8-dim g(-2)

4 – that graded structure, plus orbifolding, gives my physics model as described in my paper at CERN-CDS-EXT-2004-031 (note that it is in LaTeX, so those who are picky about style should be happy).
Once you are in contact with my model, you can then, using Segal's conformal ideas, calculate the ratio Dark Energy : Dark Matter : Ordinary Matter getting a result that is close to the WMAP observations.
If you interpret Dark Energy as CC, then the above is an example of a uniquely defined (without landscape) string theory model that quantitatively describes the CC. As to the WMAP calculations in detail, they are given in pdf form in a paper at http://www.valdostamuseum.org/hamsmith/WMAPpaper.pdf Such a paper was rejected by arXiv in 2004 because I am blacklisted, and by the time I got around to thinking about putting that paper on CERN EXT, it had been terminated, so neither arXiv nor CERN EXT has archived my work on the WMAP ratios.

It is interesting that no string theorist has ever communicated to me either:
A – a specification of a fatal error in the above (negative comments have not pointed to technical flaws, but only said that my work is complicated, that it involves orbifolding, or that it does not use the usual naive 1-1 supersymmetry of superstring theory, all of which are true);
or
B – said "thank you" for finding a specific string theory model that agrees qualitatively and quantitatively with gravity plus the Standard Model.

That indicates to me that not only are string theorists unable to do anything useful (re constructing physics models) with their stuff, but also that they don't like it when that is done by an outsider.

Tony Smith http://www.valdostamuseum.org/hamsmith/

30. **quantoken**
March 15, 2005

Aaron said:
"Do you agree or disagree with the statement that it's **conceivable** that there are lots of universes with differing fundamental constants in all of them?"

Peter answered: "Sure, it's **conceivable** that there are lots of different universes with different values fundamental constants. It's also **conceivable** that Ed Witten is an alien, that we live in a Matrix controlled by evil supernatural beings, and that the Reverend Moon is the son of God. But there's no evidence for any of these things (OK, maybe for the first one….)."

Please note the keyword I highlighted, "**conceiveable**", which is a string word since human brains are amazing thinking devices capable of **conceive** ANYTHING describable or even none-describable by human languages.

Put it simple, anything that is EVER uttered from any piece of mouth by any one, is certainly completely **"conceiveable"**, for if the idea has not been conceiveable by one of the brains, if would never haven been spoken out by one of the mouths.

But that is not physics at all. Physics strictly does NOT deal with conceivable concepts. That's the domain of psychology, not physics. Physics strictly deal with **Observable** evidences only, not conceiveable ones. Any one not understanding this needs to see a psychologist, not a physicist 🙂

There is not any evidence for multiverse and logic forbid the very existence of such evidences. THIS universe is all we can observe and physics deal strictly with what we can observe in THIS universe only, not what we can conceive out of this world.

Quantoken

31. **Peter**
March 15, 2005

Hi Aaron,

Sure, it's conceivable that there are lots of different universes with different values fundamental constants. It's also conceivable that Ed Witten is an alien, that we live in a Matrix controlled by evil supernatural beings, and that the Reverend Moon is the son of God. But there's no evidence for any of these things (OK, maybe for the first one….).

What really bothers me is not serious physicist's willingness to consider the "multiverse" idea, but their willingness to adopt it without a shred of evidence, or even a reasonable hope for ever having evidence. The only reason for doing this is to evade the fact that the string theory framework is a complete failure as a TOE. Being willing to trash the entire scientific enterprise to protect your failed ideology is just pathetic.

32.
March 15, 2005

"It's not impossible". Boy, we've really set the bar high haven't we. The same argument could be made in favor of reincarnation, or mental telepathy, or alien visitation (paging Dr. Kaku…)

33.
March 15, 2005

"I have a question to the phenomenologist who loves experimental data….actual high energy (I mean non-astro) data? If yes, what are the most interesting recent data and how did superstring theory help you or inspire your work on these data? "

Its not that there's new significant data (non-astro), it's that explaining the existing data, within the context of so called 'natural' solutions to the hierarchy problem is already very hard. These include SUSY, and all sorts of composite, technicolor like models of electroweak physics (here i just name 4d approaches).
Each of these approaches has predictions for what the LHC should see, which hopefully we can disentangle.
The problem is that all these approaches to stabilize the higgs mass introduce lots of junk that then has consequences on all sorts of precision measurements and predicts particle processes we have not seen. one winds up using all sorts of tricks to avoid this problem, some of them where inspired by string theory and
extra dimensional approaches (themselves inspired by string theory), though they are completely four dimensional. Most tricks involve cleverly controlling how symmetry breaking
is communicated to the SM.

The fact that none of these solutions/tricks are particularly simple or elegant, is part of the reason people are now looking
to anthropic explanations of the Higgs mass. I my self have not really bought into this, but here's how it goes:

for example, if you vary only the Higgs mass in the SM and nothing else, you can convince yourself that varying it even within a factor of ten or so of its current (estimated) value you quickly loose all of chemistry. you wind up with a universe containing nothin more complicated than hydrogen. obviously any life needs chemistry and stable elements besides hydrogen – therefore, the higgs
mass is roughly what it is. Since you dont need to worry about stabilizing the Higgs mass, you now motivate electroweak physics by requiring for instance that there be a stable particle at the weak scale which can be dark matter. Within SUSY such an anthropic scenario has very distinct predictions for physics at the electroweak scale. it might be very hard to see at LHC because there are very few predicted particles and none of them interact strongly, so you need to get clever. there could be some possible indirect signals though. so this is an example of how anthropics motivates concrete testable predictions. of course it depends highly on what you think can vary (in this case the higgs mass) and what cannot, so it is certainly arbitrary.

Also as I mentioned, QCD data (some of it really old) about
various strong particles perhaps can be explained using
AdS/CFT type of models – there has even been some
success in this – even quantatative: predicting particle masses
and decay rates. This is for those of you who doubt holography will ever make conact with experiment – maybe it already has and we just dont know it yet.

However I agree that thinking of string thy as TOE in the traditional sense is probably silly and that a lot of misleading marketing is being done by the popularizers of this field.
on the other hand, it is true that explaining to the public the various theoretical ideas to come out of string theory and motivating the existence of the field using those is really a difficult task, since of course we dont know which of these ideas will ultimately be useful in explaining data somewhere down
the line.

34. **Aaron**
March 15, 2005

"I'm not willing to admit any hypothesis about how the universe works unless I'm provided with evidence for the hypothesis. And by evidence I mean scientific evidence: the hypothesis explains something about nature that can be checked. So, if the "landscape" can't predict anything, it's in the same category as religious belief. Not my thing."

I didn't ask you to say that the anthropic principle *was* the correct answer, just that it's not impossible. Do you agree or disagree with the statement that it's conceivable that there are lots of universes with differing fundamental constants in all of them?

35.
March 15, 2005

I have a question to the phenomenologist who loves experimental data and has a high esteem of the accomplishments of superstring theory. Have you ever worked with actual high energy (I mean non-astro) data? If yes, what are the most interesting recent data and how did superstring theory help you or inspire your work on these data? Did it help you with understanding neutrino masses?

I agree with the comment that superstring theory was a reasonable thing to do while waiting for new experimental data. When the actual data starts coming, those string theorists who cared about "phenomenology" whatever it means these days, even if it's such a nonsense as the anthropic landscape, will have a headstart.
Being smart also helps: if worse come to worst, Witten will be remembered for his 1976 paper on deep inelastic scattering.

36. **Juan R.**
March 15, 2005

In the past, it was thought that string theory was correct.

At present many people opines that it is not even wrong.

Some of us already know that is wrong. In fact, string theory pointed (and still point) in the wrong way in each research topic that i have studied: irreversibility, time arrow, quantum gravitation, molecular dynamics, catalysis, decoherence, relativistic invariance, nanothermodynamics, TFD, etc.

When I read a recent "high-level" paper by Witten, Vafa, Schwartz, Greene, etc. I find stuff abandoned even decades ago in the sophisticated fields that I and others work!

When will "string" theorists understand that their research is NOT the last theory nor the most advanced formulation of Nature?

37. **Peter**
March 15, 2005

To make my statement precise and unambiguous: there is not now any viable superstring theory explanation of the CC other than the anthropic one. I'm talking about what is actually known to be true, not what is wishful thinking. String theorists seem to have lost the ability to distinguish these two things.

You're quite right that many string theorists hope that "one day" superstring theory will find another way to explain the CC. I didn't deny this, but I was talking about what is known to be true, not about some what some people would like to be true even though they have no evidence for it. They also hope that "one day" superstring theory will explain the parameters and structure of the standard model. But this is pure wishful thinking, with a huge amount of evidence now built up that it can't be true.

38. **Fyodor**
March 15, 2005

"You haven't provided any string theory argument for the CC other than the landscape because there isn't one (Ooguri, Vafa, Verlinde certainly don't have one, I've read that paper)."

So have I. They don't claim to have one. Nobody does, do they? I was just giving that as an example of a line of research that might, *one day*, lead to a theory of the CC that has nothing to do with the landscape. My point, which you are evading, is that many string theorists believe that there are other approaches than the Landscape. Your statement in the article was misleading.

"Susskind and many others are right about the fact that there isn't one."

How do you know? The truth is that you are engaging in wishful thinking: you *hope* that there is no alternative to the landscape because it makes string theory look bad. I repeat: a great many string theorists have no truck with the landscape. You know this, via your dear old friend Lubos. Why did you pretend to be unaware of it?

39. **Arun**
March 15, 2005

We know that some observations have anthropic explanations. The distance from the earth to the sun is the most common example. That's not a fairy tale; it's just how things are.

And this same anthropic theory explains Mercury, Venus, Mars, the asteroid belt, etc., how????????

The only thing the anthropic principle explains in this situation is why we live on earth and not on Mercury, Venus, Mars, etc.

40. **Thomas Larsson**
March 15, 2005

Wouldn't it be ironic if the correct ToE turned out to be essentially general relativity coupled to the standard model. After all, such a scenario would be in excellent agreement with experiments (though I realize that young theorists today disdain the E-word), and it would explain why so little beyond-the-SM physics is observed.

There are of course some theoretical problems with such a scenario, namely that QM and GR are incompatible and that the SM is somewhat ugly, especially in the Higgs sector. But it seems to me that these problems should be cured by some minor fix, rather than positing a plethora of unobserved phenomena and 10^500 unobservable universes totally different from ours. Finding the right fix is perhaps not so easy, but history teaches us that such a strategy has worked once before. In the 1930s people thought that QM was incompatible with special relativity, and especially electrodynamics. The small fix in that case was renormalization, which saved the situation without introducing any new physics.

41. **Peter**
March 15, 2005

Aaron,

I'm not willing to admit any hypothesis about how the universe works unless I'm provided with evidence for the hypothesis. And by evidence I mean scientific evidence: the hypothesis explains something about nature that can be checked. So, if the "landscape" can't predict anything, it's in the same category as religious belief. Not my thing.

Fyodor,

You haven’t provided any string theory argument for the CC other than the landscape because there isn’t one (Ooguri, Vafa, Verlinde certainly don’t have one, I’ve read that paper). Susskind and many others are right about the fact that there isn’t one. He’s also right that the whole field has been engaging in a huge amount of wishful thinking for years, although his wishful thinking is even more ridiculous.

Bottom line: you have an ugly, complicated, unfinished theoretical framework, and strong evidence that it either predicts nothing or something off by 120 orders of magnitude. You can bury your head in the sand and talk hopefully about how Ooguri, Vafa and Verlinde or some other recent paper will save the day, even though there is zero evidence for this. Why do you want to do this, other than to defend what looks like pure religious belief in an idea that doesn’t work?

42. **Fabien Besnard**
March 15, 2005

A child’s question to his mother : “why did you have a child ?”

Anthropic answer : “because if I had not you would not ask the question.”

This is easily generalized : “why…(put a question here) ?” answer : “because if things were not this way you would not ask this question.”

More seriously, I think some “anthropic” arguments about, say, the coupling constants needed for carbon production make sense but are only an indirect and not very accurate way of extracting these coupling constants from the experimental data.

43. **Fyodor**
March 15, 2005

“If you have a string theory argument for the value of the CC other than the anthropic landscape one, tell us what it is instead of just writing “Who says” in response to my post. I’m the one saying it. If you think I’m wrong, tell us why so we can learn something.”

No, you aren’t the one saying it, Lenny Susskind is. You are talking as though LS were the accepted spokesman for string theorists on this matter, when you surely know that most string theorists do not approve of his dogmatic announcements on the Landscape. Quite a lot of us think that it is too early to say where the cosmological constant comes from, because we don’t understant supersymmetry breaking and because we don’t have a good understanding of dynamical spacetimes in string theory yet. To take but one example, I have been speaking to someone who is working on the recent ideas of Ooguri, Vafa, and Verlinde; he thinks we may be able to get some sort of understanding of cosmology out of that, but he admits that it is too early to say anything definite yet. And that is the attitude of most people in this field : they are far from believing that the Landscape is the only way to go. The theory is still too primitive for that. I can imagine someone saying to Lagrange and Hamilton: this is just fancy mathematics, what does it predict beyond the Standard Newtonian Model? What could they have said, except: “Restrain your impatience!”?

44. **Quantoken**
March 15, 2005

Aaron said: “But that last bit’s where you’re wrong. We know that some observations have anthropic explanations. The distance from the earth to the sun is the most common example. That’s not a fairy tale; it’s just how things are.”

The anthropic principle is extremely vacuous. You think it explains why the earth-sun distance is the way it is, **ONLY BECAUSE** you have inappropriately narrowed possible forms of life to the one similar to what earth bound life forms look like. You think life is only possible as a carbon based devices that evolves with the help of a chemical called water. That’s too narrow a definition of life.

I think a more broader definition of life would be anything that can carry, process and duplicate any form of information. Further I would define intelligent life as any thing that can carry, process and duplicate a sufficnet amount of information. But that definition, as long as the universe is big enough to contain a large amount of quantum information, it would breed certain forms of life, using certain chemical or other forms of matter.

Life really does not have to be carbon-based devices only. Why can’t silicon based devices by life as well?

Quantoken

45. **Quantoken**
March 15, 2005

Mr “” said:

“For example, the idea … that a QM system with gravity in some number of dimentions is equivalent to a non-gravity system in a lower dimension might by itself merit 20 years of string thy.”

What? That a kindergarten problem is what you spent 20 years on? Let me show you how trivial it is. To describe trajectory of a particle in N dimentions requires exactly N-1 equations, not one more and not one less. That’s kindergarten math, alright?

Now you put in one constraint, be it gravity or whatever. The constraint can be described by one equation. Plug it in and you have reduced the equation set fro N-1 to N-2. So you have successfully reduced dimentions from N to N-1. Simple, right? The rests are just technical details. But the math is rather simple!

Took me 20 seconds to describe it. If string theorists have spent 20 years to figure the same thing out. Too bad that’s a waste of their lifetime.

Quantoken

46. **Aaron**
March 15, 2005

“If the string theory framework is not predictive, you’re right that doesn’t logically imply that it is wrong. But it does imply that it is something worse than wrong. It is not science, is completely useless, and is nothing more than a fairy tale.”

But that last bit’s where you’re wrong. We know that some observations have anthropic explanations. The distance from the earth to the sun is the most common example. That’s not a fairy tale; it’s just how things are. And, so it might be for the parameters of the standard model, if not through string theory, then through whatever is the correct theory of whatever.

If that’s true, there’s nothing you or I can do about it. No amount of dogmatism about the nonpredictiveness of the anthropic principle will change it. It’ll just be how the universe is.

And I agree with that dogmatism. The anthropic principle isn’t predictive. No amount of counting vacua will ever tell you anything about this universe. But I’m willing to admit that the anthropic principle may, nonetheless, be the right answer. Are you?

47.
March 14, 2005

Holography is a nice idea. My phenomenological ami — have you ever heard about Cauchy's theorem?
Unfortunately, physical systems are not necessarily analytic (unless you go far "beyond the standard model"), so holography will remain as a nice but a physically irrelevant idea. Take my word.
Jean-Paul

48. **Peter**
March 14, 2005

Certainly string theory has lead to a lot of interesting things, including a huge amount of interesting work on 2d CFTs, new insights into 4d QFT via AdS/CFT, quite a lot of beautiful algebraic geometry, and many other things. But what I object to is the entrenched ideology that a 10/11 dimensional string/M-theory can give you a TOE. This is the claim that is being used to sell the theory, and it motivates a large fraction of what string theorists do. But the evidence now is overwhelming that this idea doesn't work.

If you want to understand string theory better and really see what can be gotten out of it, the TOE claims are a huge impediment. People have got to admit failure and move on, or they will be stuck for the next hundred years lost in the "landscape". However you evaluate whether string theory is interesting, it is no longer interesting as a TOE. That idea has failed. But until people acknowledge that, the field won't move on to something more interesting, and remain stuck in the hopeless dead-end it has gotten itself into.

49.
March 14, 2005

I think you people are missing the whole point as to why string theory is interesting. It is interesting because it has been a great
toy model for many astounding theoretical ideas, whether or not it at the moment is directly linked to data.

I speak as a particle pheonomenologist who loves experimental data and certainly prefers writing papers about predicitve and testable theories. When confronted with difficult problems, theorists often try to solve much simpler, non-realistic examples which contain some features of the real problem but lack the complexity of the real world. This helps overcome the biggest challenge in theory – even knowing what is the right question to ask.

String theory has certainly proven to be a great laboratory for quantum gravity and QM in general, whether it is the right formulation for a full quantum theory of GR or not.

For example, the idea of holography (which you might have discussed) that a QM system with gravity in some number of dimentions is equivalent to a non-gravity system in a lower dimension might by itself merit 20 years of string thy. This idea (though first postulated indirectly without strings) was first fleshed out in certain concrete string theory examples where one can actually understand to some extent how it works. This idea has deep implications – changing our concept of dimension of a QM system, enriching our idea of how a QM system becomes classical, and changing our perspective on the data that QM gravity system requires.

Also, it has offered a method of building models which have qualitative agreement with features of QCD (a theory which has proven difficult to solve fully by any method thus far developed). My point is that string theory has changed our vocabulary for what are the right questions to ask in ways that we could not imagine before. And even if at some point this particular theory will be thrown out, i'm sure the toy examples it generates will further our understanding of fundamental issues.

As a side note, let me also mention that it has indirectly
influenced the creation of many mechanisms (completely garden variety four dimensional field theory ones) that are very useful in totaly predicitve, experimentally falsifiable theories of electroweak physics (which we might see at the LHC).

Of course this doesnt mean that all string thy research is useful – it is true that there are many people who dive into some techinical very specific aspects of the theory, rather than viewing it more as a lab for ideas, they take it very literally as a model for our world. i think though, that the really good string theorists are always seeking a general lesson, rather than some
specific solution in their work.

50. **Peter**
March 14, 2005

Phil Anderson been involved in the hiring of several generations of both particle theorists and condensed matter theorists at Princeton, and knows exactly what he is talking about.

Keep in mind that Anderson is about 80 years old, a Nobel Prize winner, and a good case can be made that he's the one who discovered the Higgs mechanism. He was applying QFT to condensed matter problems during the 50s and 60s when most particle theorists had abandoned QFT. Over the years I'm sure he has developed some resentment at how particle theory has gotten much more attention that condensed matter theory, especially at Princeton. He's famous for his opposition to the SSC and for his anti-reductionist "more is different" philosophical views. Over the last twenty years he has seen the Princeton particle theory group get completely taken over by string theory, a takeover facilitated by a huge amount of hype. He's knows what it is like to really understand something new about physics and knows that string theorists haven't been able to do this, despite all the hype. He has been upset and complaining about this situation for years.

Whatever you think about him, he's someone who definitely is honest and definitely is honestly telling you what he thinks. He's old enough and famous enough that he doesn't need to worry about who he might offend by speaking his mind.

51.
March 14, 2005

It's a very interesting article. Look at this quote from Philip Anderson: "we from outside the (string) field are disturbed by our colleagues' insistence that every new semi-adolescent who has done something in string theory is the greatest genius since Einstein and therefore must occupy yet another tenure track…". Has the Nobel Prize winning Dr Anderson ever participated in hirings of condensed matter theorists? They are usualy presented with even more hype because in addition to being genius they bring "million-dollar" grants. Do you believe that Dr Anderson criticism is honest? Welcome to the WWF physics of the XXIst century.
Jean-Paul

52. **Chris W.**
March 14, 2005

Quantoken said, *Science is not a part of nature, but the part of human culture which tries to interpret what we know about nature, in ways we human can understand and* *reason with*.

In a very real and non-trivial sense, science *is* a part of nature. It does not seem that arbitary changes to the known laws of nature (including apparently universal constants) are, in general, compatible with the existence of life. For many of the same reasons, it is not at all clear that science could get anywhere unless the physical universe had certain crucial properties.

Why might this be the case? If the evolution of life and science itself are fundamentally processes of trial and error, then one must ask: Under what circumstances can trial and error be effective? For example, suppose it was much harder than it is to construct and maintain isolated systems. How could one design and interpret experiments? What if effects like those assumed by astrologers were pervasive? One might have theories, but testing them would be much more difficult.

One of my objections to the more familiar formulations of the anthropic principle is that they short-circuit this question, although they implicitly or explicitly acknowledge its relevance. Aaron said, "On the other hand, I firmly believe that we have to practice science as if the anthropic principle were false." I would say that we must either do that, or reformulate the anthropic principle in a way that is truly fruitful, and doesn't subvert the whole enterprise.

I think this may well be possible. The key idea is to consider the logic of trial and error, and how it can lead to conclusions about the features and behavior of physical systems. In some sense I think the logic of trial and error, combined with certain very simple dynamical notions, may ultimately largely dictate these features and behavior, and the laws of physics themselves. I have been influenced in this direction by the writings of Karl Popper and Edwin Jaynes* and some younger (and still living) Bayesians, such as Carlton Caves. I'm not sure what they would make of the ideas I'm advancing here. (Incidentally, the relations between Popper's philosophy and that of the Bayesians is a topic for a long article or a book.)

53. **quantoken**
March 14, 2005

Peter said:
"I'm just completely amazed by the fact that **intelligent, trained scientists** are unwilling to admit an absolutely undeniable fact staring them in the face. If it really is true that string theory can't predict anything, it's not physics, and anyone claiming to be a physicist has to admit that, as physics, it's a worthless idea and has to be abandoned."

Peter, don't you see as I already pointed out. Doing so for string theoretist amounts to career suicide. **No amount of scientific training is enough to prepare one to commit suicide**. An intelligent scientist would want to continue to survive in the establishment camp. Science honesty is only secondary thought after survival is taken care of first.

For people old enough to have established tenureship and un-shakable credibility in the science community, but still young enough to have enough lifetime left to explore completely new exotic ideas. They may listen to you, Peter. Anything above or below that age threshold, I do not think they will ever listen.

Quantoken

54. **Peter**
March 14, 2005

Aaron,

Yes, nature is what it is, not what we want it to be, and this is a lesson string theorists are having a hard time facing up to.

If the string theory framework is not predictive, you're right that doesn't logically imply that it is wrong. But it does imply that it is something worse than wrong. It is not science, is completely useless, and is nothing more than a fairy tale. People who don't believe in the scientific method are free to promote it as having something to do with reality, but they have no business doing so from within university physics departments.

A few years ago, if you had told me that once it became clear string theory was a non-predictive framework, string theorists would refuse to abandon it and instead would start going on about how "we may have to rethink what it means for a theory to explain experimental data", and basically give up on the whole idea of science, I would have refused to believe you.

I'm just completely amazed by the fact that intelligent, trained scientists are unwilling to admit an absolutely undeniable fact staring them in the face. If it really is true that string theory can't predict anything, it's not physics, and anyone claiming to be a physicist has to admit that, as physics, it's a worthless idea and has to be abandoned.

55. **quantoken**
March 14, 2005

Aaron said:
"I think it's sometimes worth pointing out that just because the landscape isn't predictive, that **doesn't make it wrong**. It's not imaginable that that's how the world really is. It'd be disappointing, but nature is what it is, not what you want it to be."

You forget the title of this BLOG is "**Not Even Wrong**.", which is worse than being wrong 🙂

Nature is what nature is. Science is not a part of nature, but the part of human culture which tries to interpret what we know about nature, in ways we human can understand and reason with. Therefore, the whole idea of science is based on the philosophical belief that the nature is rationable, knowable and predictable. If you give up on the notion of nature's predictability, you give up your right of being called part of science.

Michael Nauenberg: What you said is wishful thinking. For string theorists to do what you said would be professional suicidal, to admit that the whole idea is a dead end and they have wasted tax payer dollars for decades on a futile pursuit, which is also a waste of their own intelligent lifetime. It simply would not happen.

Change is only possible, IMO, when the old generation die out and the idea of super string gradually fade out. Unless there is a up-down revolve of changes starting with re-appropriation of research fundings.

Quantoken

56. **Arun**
March 14, 2005

I think it's sometimes worth pointing out that just because the landscape isn't predictive, that doesn't make it wrong. It's not imaginable that that's how the world really is.

Aaron, how would you establish that the landscape is correct?

57. **Aaron**

March 14, 2005

I think it's sometimes worth pointing out that just because the landscape isn't predictive, that doesn't make it wrong. It's not imaginable that that's how the world really is. It'd be disappointing, but nature is what it is, not what you want it to be.

On the other hand, I firmly believe that we have to practice science as if the anthropic principle were false, but that's another story.

58. **michael nauenberg**
March 14, 2005

It seems to me that at this point it would be
best if string theorist would stop going public
with unscientific hyperbole. Whatever
merits the theory may have, announcing that the string "landscape' of 10^500 (or is it 10^501) multi-universes explains
the observed constants of nature
in "our" universe (anthropic principle) is nonsense characteristic of theologians of the middle ages.

59.
March 14, 2005

> So don't fool yourself that a
> tenure track position taken away from a string
> group will be used to hire a mathematical
> physicist working on (fascinating) Clifford
> modules — such a position will go to a nano-
> theorist..

.. which in fact is very good, isn't it? One can be a mediocre nanoscientist and still be useful. Obviously this is not so in string theory.

60. **Wolfgang**
March 14, 2005

> Parallel universes, time travel,
> miniature black holes

Your examples are all pretty old stuff and were "invented" before the superstring mania.
This shows that superstring theory did not even contribute to science fiction, like all other great theories before.

61.
March 14, 2005

These days it seems that obsessive hype-mongering is a prerequisite for any field to maintain a degree of public attention. With nanobots, age-reversing immortality drugs, hyperintelligent AI all just around the corner, according to the press releases, how is the physicist to compete? Parallel universes, time travel, miniature black holes- at least it gets your picture in the paper.

62.
March 14, 2005

The present problem of high energy phsyics started 30 years ago when the great success of theorists attracted many brillant students to theory, but at the same time closed main problems and ultimately slowered experimental progress. In this situation string theory was a good attempt of doing something. Passing to some other mathematics would not solve the problem.

Hopefully LHC will make real progress and indicate where good physics is. And a few more hyped theorethical books will indicate where it is not.

63.
March 14, 2005

Peter – Being at the math department of Columbia, you maybe not be aware of the situation outside Princeton, Harvard etc. Everywhere else any non-applied theory is under constant attack of the prophets of new physics who work on applications of such advanced theory as Hooke's Law to molecular motors etc and bring tons of money from NIH, DOD etc.
The criticism of some Very Famous theorists comes from jalousy how such an underfunded field like string theory could create so much hype and publicity. Unfortunately their response is to follow the bad example and to create their own hype — promise that physicists can find a cure for heart attacks, explain the origin of life etc.
So don't fool yourself that a
tenure track position taken away from a string group will be used to hire a mathematical physicist working on (fascinating) Clifford modules — such a position will go to a nano-theorist. Make your own call…

64. **Peter**
March 14, 2005

I'm actually not of the opinion that the amount of money spent on string theory is a problem. It's negligible compared to all sorts of other things, many of which are no more worthwhile.

The motive behind the string theory hype is not only to protect NSF and DOE grants, it is also to ensure the dominance of string theory in those physics departments where it has become entrenched (Princeton, Harvard, etc., etc….). The important question to me is about how to make it possible for good young people to try to do something new. In his comments in the article Phil Anderson also focuses on this saying he is

"disturbed by our colleagues' insistence that every new semi-adolescent who has done something in string theory is the greatest genius since Einstein and therefore must occupy yet another tenure track."

What can be done to change things so that young people don't feel their only hope of a permanent job in mathematically-based particle theory is to do string theory? I happen to think that puncturing the string theory hype is a necessary first step, but far from sufficient.

65.
March 14, 2005

String theory at Average Respectable University is a negligible fraction of the total funding as compared to bio, nano and neuro. While it is fair to criticize its scientific merits and outrageous self-promotion of some individuals, your over-funding criticisms should be directed towards other fields: these days hype and lies are everywhere (which of course doesn't justify saying that strings solve the dark matter problem or that you can see them in the sky or that LHC will be a black-home factory…)

66. **quantoken**
March 14, 2005

Dear Fyodor Uckoff,

Even if super string theory becomes the ONLY theory funded by public money, so you have no competitor funding wise, and even Lee Smolin et al., John Baez et al. all publicly acknowledge that their approches are wrong. Does that make super string theory a winner?

No, absolutely not. You are still a complete loser since you still can not predict anything. And you would be a very **sore** loser since you can't even acknowledge your failure, while Lee Smolin is at least ready and willing to acknowledge failure if they can not succeed.

Theory developments are not ball games in which there is always one and only one winner. Theories are not used for competition of funding, but for explaining the nature. So **there doesn't need to be a competition**. If none of the existing theories can make a verifiable prediction, then all of them are losers. If two theories can both explain the same thing, then both are winners and there may be a third winner unifying them behind.

So a good theory really doesn't need to compete against any one else. If it is correct it will be correct on its own merit. If it is wrong it is wrong on its own fault.

The fact that string theorists are SO jealous against other theories, really tells you that they simply can not establish their own validity on their own merit, but will have to base it on "competition" against other theories, and declare their victory based on their dominance on fundings. What a sore loser!

Quantoken

67. **Dave Bacon**
March 14, 2005

Fyodor said "And even if it were true that string theory has been "hyped", what harm would that have done?"

You can't be serious, can you? If a field of physics is hyped and turns out to be totally and completely wrong (or worse, totally unpredictive), then this has done serious damage to the progress of science. It slows down science by (1) maintaining a monopoly on what is considered acceptable for theorists to work on (and not just theorists in high energy physics), (2) stripping money away from other fields of physics and science which are more deserving of funding, and (3) reducing funding for all of science by reducing the public's trust in science. If the "great" leaders of string theory turn out to be following nothing more than a dead end, then it does serious damage to all of science.

Further, while there may be no alternatives to string theory at present, this does not mean that we should continue to fund it. It may be that the idea(s) necessary to reconcile quantum theory with gravity simply have not been put forward, and spending my money on a dead end (if that's what it is) doesn't seem to me like a very productive thing to do.

68. **Peter**
March 14, 2005

If you have a string theory argument for the value of the CC other than the anthropic landscape one, tell us what it is instead of just writing "Who says" in response to my post. I'm the one saying it. If you think I'm wrong, tell us why so we can learn something.

I have never said "there are lots of theories out there which would have explained everything by now if only they had not been starved of funding by the string theorists". There aren't any good ideas out there about how to go beyond the standard model, but having most of the community working on an idea that has failed and refusing to face up to the fact that it has failed doesn't help the situation. The reward structure of the field is still that if you are a smart young theorist and decide to work on a speculative idea that is not string theory, you probably are ruining your hopes for a career, whereas if you work on a speculative string theory idea (even if it is clear that it doesn't work), you can do quite well for yourself.

I don't expect Witten to announce tomorrow "String theory has failed as a TOE, people should go out and try and find other new ideas". But he really should do this, and it would have a very positive effect on the field if he did.

69. **Fyodor Uckoff**
March 14, 2005

Peter Said: The only way around this seems to be the "landscape" argument,

Who says?

And even if it were true that string theory has been "hyped", what harm would that have done? It's not like the theory has any competition. The notion that there are lots of theories out there which would have explained everything by now if only they had not been starved of funding by the string theorists is pure conspiracy theory at its most ridiculous. The people being starved of funds and attention are the kooks who tend to post in blogs like this one until Peter kicks them out. Where's the real competition? Does anyone really expect Ed Witten to announce: "String theory has failed. Go work on something else. What else? Go look at Tony Smith's or Quantoken's web pages….."

70. **Quantoken**
March 14, 2005

Peter said:
"…since it is well-known that superstring theory naturally leads one to expect a value for this energy density that is off by 120 orders of magnitude."

Sorry I must set the record straight here, although I am definitely not a defendent of super string theory. That 120 orders of magnitude is NOT the fault of string theory alone, but the fault of whole existing physics theories which could not unify QM with GR. You have the 120 orders of magnitude ever since vacuum energy was proposed and calculated, which is much earlier than the occurance of super string theory. You can not blamn super string theory for a problem that occurs before it, although super string theory does not lead to any thing better.

Put it simple, regardless of what theory you are in, any time you put the three constant hbar, C, G together, you naturally get the Planck Scale. Any time you deal with Planck Scale, the characteristic energy density, which is approximately one Planck mass per Planck volume, is automatically 120 orders of magnitude too larger, compare with the cosmological constant.

So any theory, not just super string theory, that talks about Planck Scale at all, inevitably runs into 120 orders of magnitude. That is, **unless** the math of the theory itself leads to a dimensionless number which happen to be very small but none zero, approx. 10^-120, which exactly cancel out the other 10^120 figure from Planck Scale. That is what the “landscape” business is all about. But that’s a complete failure as we know.

There are theories which discard the Planck scale and so get rid of the 10^120 as well as difficulty in explaining the cosmological constant. But obvioulsy Peter is not interested in hearing alternative theories and I am not going to elaborate here.

Quantoken

No Cosmological Constant?

March 16, 2005
Categories: Uncategorized

A paper appeared on the arXiv last night entitled Primordial Inflation Explains Why the Universe is Accelerating Today by Rocky Kolb of Fermilab, together with Sabino Matarrese, Alessio Notari and Antonio Riotto. There's also a Fermilab press release about it today.

I'm no expert on the subject, and would love to hear the opinion of someone who is. As near as I can figure out the idea is that what is really responsible for the effects that have been ascribed to a cosmological constant is a "cosmological perturbation" of the gravitational field. This is supposed to be a perturbation that expanded during the inflationary period so that its wavelength is now larger than the Hubble radius. According to the authors, this predicts a different magnitude vs. red-shift relation than the standard cosmological constant does, so their idea should in principle be testable.

If they're right, this certainly will cause a huge problem for the whole "Landscape" business, which has advertised as its greatest success the "prediction" of a non-zero cosmological constant of the right order of magnitude.

Comments

1. **steve m**
   March 19, 2005

   Well sometimes important ideas emerge in an incomplete or confused form but this paper of Kolb et al. is going along the directions of the sort of thing you really want–a clean conservative scientific resolution of the cosmic acceleration/dark energy problem. They are approaching the problem in a sober way and hopefully they can develop and refine more details. Seems to have generated much interest already. If it works out then the whole landscape business will look increasingly silly.

2. **Matthew**
   March 18, 2005

   Well, we had some discussion about this paper today in our lunchtime seminar. The consensus seems to be that they might be on to something, but there are a few things in the paper that don't quite make sense. For one, around equation (7) they make the claim that their formula for \Psi(x,t) is valid to all orders in perturbation theory. Yet the longer paper that they refererence only gives a result to second order.

   Of course, this is largely a function of the length of the paper (which is clearly intended for PRL), a lot of details get glossed over.

   Still, the cosmo people here were interested, but skeptical. One group member also had an email from a big time cosmologist who said this was an ineresting idea…

   It'd be nice if there was a longer paper, it's hard to get much from a PRL. For those who want to look into it more, it seemed fairly clear that you should track back at least one layer to their previous work.

3. **D R Lunsford**
   March 17, 2005

   This is the only blog that has much interest because it's about something other than the owner.

   -drl

4.
   March 17, 2005

   there is only one math blog by a professional mathematician and it is here http://www.matrix.ua.ac.be/ There are a number of other blogs by math instructors here

   http://talldarkandmysterious.ca/

   and here

   http://learningcurves.blogspot.com/

5. **anon**
   March 17, 2005

   This is an unrelated comment, but I'm just curious if you know any mathematicians who keep blogs? In general, I'd be interested in anything. Specifically, I'm interested in Algebraic Geometry, etc.

   Just curious.

6. **D R Lunsford**
   March 17, 2005

   Well Q has a point – this hair-pulling over the CC, inflation, dark matter and other miraculous genies seems silly in view of Mk205 and NGC7603 *inter alia*.

   -drl

7. **steve**
   March 17, 2005

   It is very nice to see this mixture of Ideas here.

   It will be nice to see what Sean has to say about this further.

8.
   March 17, 2005

Quantoken, this whole blog is built around an idea Peter does not believe.

9. **Quantoken**
March 17, 2005

Click on my name below to see why the experimental evidence for an “accelerating” universe is questionable and extremely weak, plus some useful links to the original ARXIV papers.

Peter does not allow discussion of ideas he does not believe, but said simply providing a link and discuss it some where else is fine. So go and see for yourself: http://quantoken.blogspot.com

Quantoken

10. **Lubo? Motl**
March 17, 2005

I would be happy, of course, if the true LAMBDA was zero after all, the expansion explained different, and we would return to the original C.C. problem “why it’s exactly zero”. But one must distinguish wishful thinking from reality. Longer-than-horizon fluctuations are always problematic.

If the effects like that are true, then these trans-Hubble oscillations may effectively be able to change many kinds of “constants” within the Universe. They’re frozen, they don’t change, yet the expectation values of the scalars may have an effect.

11. **ALejandro**
March 17, 2005

Cabi blogs on this issue too:
http://www.mit.edu/people/cabi/blog/2005/03/universe-accelerated-beyond-horizon.html#comments

12. **Alejandro Rivero**
March 17, 2005

Also Stephon Alexander pre-announces, in his blog
http://qd.typepad.com/17/, some advance in the question of the cosmological constant:

“Now I’m out of the cave with an answer.
Whats the answer?
I’ll tell ya tomorrow.”

13. **Matti Pitkanen**
March 17, 2005

For a different view about accelerated expansion and problem of cosmological constant see No cosmological constant after all?

Matti Pitkanen

14. **Serkan Cabi**
March 16, 2005

Even though I posted my blog on this issue I wanted to join the discussion here by pasting my opinion:

“I should say that I did not like the proposed solution. We always thought that solution of the acceleration of the universe puzzle will also solve two other great mysteries. Namely; why we do not see a cosmological constant in the Einstein-Hilbert action although the known symmetry principles allow it, and why the QFT vacuum condensates (ground state) do not gravitate; or why they cancel out. If the acceleration is due to super-Hubble fluctuations, I think we will be in a worse trouble. At least they are offering observable differences from a pure cosmological constant and nature will say the last word.”

15. **Haelfix**
March 16, 2005

These sorts of issue are very sensitive to gauge freedom issues in GR, and he is explicitly fixing a gauge in his model. That raised alarm flags in my head.

Regardless this is jsut as ugly as the landscape in principle, it again sticks us at a very unusual spot in phase space.

16. **Peter**
March 16, 2005

Hi Sean,

Thanks for sharing the quick opinion, I hope we’ll hear more when you’ve had a chance to look at this more carefully.

As to string theory. Sure, there are plenty of string theorists who aren’t happy with the landscape, and the disappearance of the CC wouldn’t change things for them. But they still would face the perennial two huge problems of

1. how to break Q (supersymmetry generator), but not Q^2 (Hamiltonian operator).

2. finding some argument why there aren’t at least 10^500 solutions to the theory, ruining its ability to predict anything.

By now a large number of prominent string theorists have publicly announced that they believe string theory has to have an exponentially large number of consistent ground states. They have argued this is a point in the theory’s favor since it allows the anthropic explanation of the value of the CC. If this disappears, and the existence of so many ground states becomes purely a bad thing for the theory, it will be interesting to see if they change their mind about them.

17. **A non-cosmologist.**
March 16, 2005

Sean wrote:

there's no way that the situation outside our Hubble patch can influence our observed dynamics.

But just because these modes have wavelength larger than Hubble radius, that doesn't mean they're entirely outside our Hubble patch — they still have some energy density inside our Hubble patch.

How the time-dependence arises is a bit of a mystery to me, but it is just scaling like a(t). They seem to claim to have derived this from Einstein's equations. (They don't seem to say phi is time-dependent, just that it has a time-dependent effect on the metric, parametrized by Psi.)

As a non-cosmologist, I was hoping you would have all the answers. Can you get Kolb to explain it all?

18. **Matthew**
March 16, 2005

Hi Peter,

This paper has caused some discussion around here already. You've got the gist of it, the crucial point seems to be that these super-hubble perturbations have some time dependance (constant perturbations could be scaled away). You can see this in equation (6), if the super-hubble part of the potential was constant, you'd get the standard result.

It's going to be the subject of our Friday lunch discussion, so I might be able to tell you what the experts around here think.

19. **Sean**
March 16, 2005

Oops, I left a link to my home page instead of my blog. For a minute there I was thinking like a scientist instead of a blogger!

20. **Sean**
March 16, 2005

My first impression, upon an admittedly brief reading, is that this mechanism won't work — there's no way that the situation outside our Hubble patch can influence our observed dynamics. But I have to think about it more to understand exactly what they are claiming.

Of course, if any cosmological observation shows that the dark energy is not a cosmological constant, people will abandon the landscape idea. String theorists, like everyone else, will go back to trying to understand a way to get a vanishing vacuum energy without unbroken supersymmetry. You try to build models that are consistent with the data, that's the way it works.

"The landscape" is not the same as "string theory"; it is an idea within string theory that may or may not be right. String theorists themselves argue back and forth about whether the landscape is the right way to think about things; the fact that there is so little experimental guidance makes it a difficult task, but not necessarily an impossible one.

21.
March 16, 2005

Common-sense suggests that super-Hubble fluctuations generically mimic an anisotropic expansion. While supernova data are consistent with an isotropic acceleration. Is this a problem? The text below eq. (12) seems a (plausible but not sound) attempt of addressing it.

Anniversary

March 17, 2005
Categories: Uncategorized

It's now been exactly one year since I first set up this weblog. At the time I thought the number of those sharing my interests would be very small and hardly anyone would be looking at whatever I put up here. Things have turned out very differently, with an ever increasing number of connections. I started gathering statistics in May of last year. Here's the average number of connections to the main page per day (there's a similar number of connections to other pages, from Google searches and links from elsewhere).

May 2004 146
June 2004 240
July 2004 281
August 2004 336
September 2004 315
October 2004 514
November 2004 514
December 2004 572
January 2004 735
February 2005 955
March 2005 (first half) 1109

I've enjoyed and learned a lot from many of the comments posted here (at last count there have been 2728, concerning 168 different postings), but a recurring problem has been that many people would like to turn the comment section into a discussion forum for their own personal speculative ideas about physics. This threatens to completely overwhelm discussion of the topics I'm actually posting about. This morning I had to delete several several such comments from different people. Please do not post comments here of this kind. Get your own weblog and do it there. If every so often you want to post a link here to something you've written of this kind elsewhere, that's fine.

It's been quite a year, especially as the story of string theory just gets weirder and weirder. I have no idea what will happen during the next year, but I'm looking forward to finding out.

Comments

1. **Narayanan**
   March 26, 2005

   sorry this is the right link I typed in 03 instead of 02 in the last comment

   http://arjunnarayanan.blogspot.com/2005/02/really-really-popular-science.html

   arjun

2. **Narayanan**
   March 26, 2005

   Hi,

   I am a student from India interested in math and physics. As you might be aware India has a large amount of string theory in its morning newspapers and in its research institutes. I am irritated by what I percieve to be an attempt at deluding students. after 20 pop sci lectures and even shiraz minwalla giving a couple of "involved " lecutres I am still in the dark(though now I know enough analogies/jargon to write my own pop sci book)
   recently I blogged in a fit of anger. could someone here please take a look at it and see if I need to tone down the youthful vitriol. it troubles me because i am really bright Till a couple of years ago I agreed, with every bright person i know, on why physics is Beautiful , But not any more. and its scary, makes me want to leave and go study economics or something(at least there are more girls in eco departments.)

   heres my post

   http://arjunnarayanan.blogspot.com/2005/03/really-really-popular-science.html

   thanks
   arjun

3. **J.F. Moore**
   March 19, 2005

   Happy anniversary! I just discovered your blog a few weeks ago, but it is now regular reading for me. Thanks!

4. **Aaron**
   March 19, 2005

   Maybe half thought about doing strings coming in, but it ended up just being six of us, I think, unless I'm forgetting someone….

5. **ali**
   March 18, 2005

   Congratulations Peter! (from a former Rabi sch.)

   As someone who spent a couple of years at princeton (1998-2000) and witnessed first-hand the mad rush to study string theory (half of my entering graduate class wanted to do a string thesis), I very much appreciate your willingness to confront this openly and intelligently. For a while it seemed like it was really just Glashow (and the ghost of Feynman)…

   By the way, I went to Lawrence Krauss's talk (to a lay audience) this week at the CUNY graduate center. He summarized pretty well the evidence for the standard model of cosmology, leaving the audience to contemplate the mystery of the dark matter and energy. At the end, he asked if there were any questions. A few people asked, but he seemed frustrated with the caliber of the questions. He said he'd take one more question, if someone had something GOOD. I shot my hand up and said (truly unsarcastically), "What about string theory? Isn't it supposed to explain everything?"

Krauss smiled and then said that string theory is a failed theory in his opinion. He got into cosmology in the 80's to understand the fate of the universe and the smallness of the cosmological constant. He said that string theory has completely failed in addressing either of these. He then plugged his book coming out in September.

In retrospect, the way that last question played out made me look like "jeff gannon" to Krauss's "George Bush". But I couldn't help myself! 🙂

6. **Daniel Doro Ferrante**
   March 18, 2005

   Congratulations on your first blog-bday, Peter! 🙂 It's a real "ritual of passage"… something along the lines: "If i made it so far, i can keep on making it!" 😉

   As for the "comments about the comments" (gotta love meta-statements… and G?del!), sometimes i think people forget that this is **your** space, not theirs… and, this pretty much gives you the right to do whatever you please. I guess some concepts are complicated…

   Anyway, if you ever need a boost on your stats, just let me know… i can easily make your blog beat the 1.000.000 hits/month! (Yeah, you're reading it right: one million. 😉

   Take care! []'s!

7. **quantoken**
   March 18, 2005

   Peter said: "many people would like to turn the comment section into a discussion forum for their own personal **speculative ideas about physics**."

   Oh Peter, that's a good one. **Speculative**. Don't you realize that ALL research into a new candidate theory unifying QM and GR are ALL **speculative ideas**? If something makes a solid prediction and then be verified by experiment beyond doubt. Then it's no longer just a speculation, but an accepted theory. Nothing like that happened yet to any of the theories being investigated.

   So every one is **speculative**, yours included. You just like your own speculation better than others'.

   And sometimes you erase purely **none-speculative**, but factual comments. Like last time when I talk about stars radiate energy and it acounts exactly for the right amount of CMB energy. It's a fact, not speculation. Even proponents of BB, like Edward Wright, accepts that there is a "coincidence" between the two. But to you, since you can not agree with that fact, you call it "speculative".

   Quantoken

8. **Nonspeculating man**
   March 18, 2005

   "many people would like to turn the comment section into a discussion forum for their own personal speculative ideas about physics."

   Good iniciative, bad methodology!!

9. **Kyle**
   March 18, 2005

   Thanks for your time and effort Peter!

10.
    March 18, 2005

    Peter, Congratulations. Your blog is very informative.

11. **rich_w**
    March 18, 2005

    Thanks, Peter, for blogging and keeping the site going – it's been informative, enjoyable and, on occasion, controversial. Thanks again, from this humble mathematician wannabe…

12. **Peter**
    March 17, 2005

    One trend I've noticed is that even a year ago many string theorists automatically assumed that anyone who criticized the theory was an idiot and/or did not know what they were talking about. I see less and less of that these days…..

13. **Not a Nobel Laureate**
    March 17, 2005

    How about graphing that data as a "landscape".

    😉

14.
    March 17, 2005

    Wow, hard to believe it's been a whole year. Out of curiosity I went back and looked at your first several posts and comments. I must say, as a former particle theorists, I feel embarrased on behalf of some of my former colleagues for some of knee-jerk abuse you've been subjected to. And that's even before the arrival of Lubos!

15. **Peter**
    March 17, 2005

Sorry, I don’t want to spend time massaging this data. Connections are from all over the world. Quite a few academic machines with a name that includes the string “strings”…..

And Lubos, if I really wanted to compete with you on traffic numbers, I could start expressing opinions about Columbia’s Mideast-related political controversy (don’t anyone even think of submitting comments on that here….)

16. **Lubos Motl**
    March 17, 2005

    Congrats, Peter your numbers are pretty encouraging for others. 😉

17.
    March 17, 2005

    Or how about some geographical visitor info? It would be nice to see from where the various visitors are.

18. **Steve**
    March 17, 2005

    How about posting a graph of that data with a fit curve?

Coleman Conference

March 20, 2005
Categories: Favorite Old Posts, Uncategorized

I spent the last two days up in Cambridge, mainly attending the conference in honor of Sidney Coleman. Sadly, Coleman is in poor health, suffering from Parkinson's disease, and was unfortunately unable to attend the talks in his honor. They were videotaped so that he could watch them later.

For me as for many particle theorists, taking Coleman's quantum field theory course at Harvard was one of the great intellectual experiences of my life. Another such experience was reading and learning from his great Erice lectures, both the late seventies ones as they came out, as well as going back to his earlier ones that started in 1966. These were collected in 1985 in the book "Aspects of Symmetry", allowing me and many others to replace a stack of dog-eared Xeroxes with a more durable volume. The fact that Coleman stopped giving these lectures after 1979 was to me one of the first indications that particle theory was entering a much less promising phase of its history. Coleman never really warmed to the topics of supersymmetry and string theory.

For much of his career Coleman played the role of guru for the particle theory community, generously sharing his unmatched insights into quantum field theory. He would sleep through the morning (famously announcing that he couldn't teach a 9am class because he couldn't stay up that late), get into his office late in the afternoon, then spend hours dealing with a long line of people waiting to talk to him to try and get some help with whatever problem they were working on. Steven Weinberg spoke for many people at the conference when he said that Coleman was the single person he had learned the most physics from.

The conference was extremely well-attended, with the large lecture hall in the physics building at Harvard overflowing on Saturday. I don't think I've ever seen so many Nobel prize winning particle theorists in one place. They included Gell-Mann, Glashow, Weinberg, 't Hooft, Gross, Wilczek, Wilson, as well as Fields medalist Edward Witten. One of the few living Nobel particle theorists who couldn't make it was David Politzer, who very much directly owes his prize to Coleman.

I won't describe the talks in detail, this has been done pretty accurately already by Lubos Motl (who I got to meet in person for the first time). Physics weblogging was very well represented at the conference: besides Lubos, Jacques Distler was liveblogging from one corner of Science Center B on Friday, and Serkan Cabi was also there. Sean Carroll also has some comments about Coleman.

Among the more historical talks, perhaps the most interesting was that of Gerard 't Hooft (Lubos seems to have missed 't Hooft's comment that he shouldn't be referred to as "Gerardus", a formal version of his name that appears on his Nobel citation and his passport, but is otherwise not much used). 't Hooft gave his version of the asymptotic freedom story. He said that he had computed the Yang-Mills beta function a couple years before Gross-Wilczek-Politzer, but didn't realize that this result wasn't known to the experts. He pointed out that everyone else had experience only in computing the scaling behavior of non-asymptotically free theories, whereas the first theory he did the computation for was an asymptotically free one, so he thought this was unremarkable. He did say that Gross-Wilczek-Politzer deserved the Nobel since (besides being the ones to publish the beta-function result) they had understood how to use this to explain Bjorken scaling, something that he hadn't known about. He said his advisor Martin Veltman had told him that the Yang-Mills scaling behavior wasn't relevant to experiment since experimentalists only cared about what happens on mass-shell. Luckily Veltman was one of the few Nobel theorists not in attendance, since he would likely have blown a gasket if he had been there to hear some of the things 't Hooft had to say about him. 't Hooft went on to say that he had learned one important thing from this episode: always immediately publish any new result you have.

There was significant mention of string theory in only two talks, those of Gross and Witten. Gross gave essentially the same talk he gave last October at the 25th anniversary of the KITP. He joked that he had managed to time the award of the Nobel with the KITP celebration by every year for the last thirty years writing to the Nobel Committee and asking them to wait a while before awarding him the prize, something they had been happy to do. At the point of his talk when he said that the question "What is String Theory" was one of the big questions for the future, he stopped to defensively note that since we don't know what string theory is, it is an idea that can't be killed, no matter how much certain members of the audience wanted to do this. He went on to claim that since AdS/CFT kind of connects string theory with QCD, string theory is in some sense part of the standard model, so it's importance is secure. This argument seemed to me pretty disingenuous, since presumably he's well aware that the problem most critics have with string theory is not with the idea of using it as a dual representation of QCD, but with the idea of getting a TOE out of it, a project which some have called a "colossal failure". He didn't have anything to say either about this failure or about the whole Landscape mania.

The last talk was Witten's, entitled "Emergent Phenomena in Condensed Matter and Particle Physics". He started by saying that he was afraid the title of his talk might be more exciting than the talk itself. By "emergent phenomena" he meant roughly non-perturbative phenomena in QFT, where the long distance degrees of freedom one observes are not directly related to the local degrees of freedom. He gave QED as an example of a non-emergent theory, QCD an emergent one, with the nature of the electroweak theory still up in the air until we know more about the origin of electroweak symmetry breaking.

He went on to say that gravity messes up this distinction between local and emergent phenomena, since one doesn't have diffeomorphism invariant local observables. He then quoted his 1980 work with Weinberg (and with help from Coleman) to the effect that you can't get a massless spin two bound state in a theory with a local stress-energy tensor, saying that this showed that you can't start with a local theory in Minkowksi space and generate Einstein gravity as an emergent phenomenon. For him the lesson is that if you want gravity as an emergent phenomenon, you need to find a way to first get space-time as an emergent phenomenon, and he believes that whatever the primoridial M-theory underlying string theory is, it should do this. While such a theory doesn't now exist, he went on to give the AdS/CFT correspondence as the kind of thing he had in mind. There the Weinberg-Witten argument is evaded since a QFT in 4 dimensions is related to a gravity theory in a different number of dimensions (5).

Comments

1. March 23, 2005

   Maybe I am missing something, but how can QCD be dual to a gravitational theory in AdS_5 when it doesn't have a massless spin-2 particle?

   Isn't that the same problem which killed string theory as a dual theory to QCD? The absence of a massless spin-2 particle?

   Or perhaps the graviton IS a QCD bound state? That would be shocking!

   But I guess if you don't have a Planck brane…

2. March 23, 2005

   I want to talk about Sidney Coleman. I met him almost 25 years ago. I was a 1st year grad student. He struck me as an "old" man. Now I know that he couldn't have been older than 45.

   Unlike the rest of the Harvard "superstars", he was not arrogant, he was not patronizing, and he was not abnoxious. He was approachable, and he very patiently listened to questions, then very kindly answered them. Sometimes his answer was an honest "I vaguely know these things. I'm not an expert".

In those days, there was a joke circulating around the physics dept at Harvard (I think it's attributed to Claude Bernard who was a grad student of Weinberg's at the time):

"How do you do physics at Harvard? You go to Witten to give you a problem to work on. You go to Coleman to tell you how to solve it. Then you go to Weinberg to write you a reference letter."

I'm saddened by his illness, and by the fact that he can longer teach physics and discuss physics.

3. **Arun**
   March 22, 2005

   "That's only possible for massive particles and the theorem doesn't apply."

   I thought the purpose of the theorem was to rule out massless bound states; not to assume that all bound states are massive. Anyway, I'm probably missing the point.

4. **Johan**
   March 22, 2005

   Regarding the "gravity as a gauge theory" issue, Deser's bootstrap approach does give you the Einstein-Hilbert action but I believe it only works for spacetimes that are topologically R^4. Penrose has shown that there is no map from Schwarzschild spacetime to Minkowski spacetime that takes lightcones into lightcones, which jives well with the Witten-Weinberg argument.

5.
   March 22, 2005

   P.S. you're describing a topological quantum field theory.

6.
   March 22, 2005

   That's only possible for massive particles and the theorem doesn't apply.

7. **Arun**
   March 21, 2005

   Probably a dumb question, but what happens to the Witten-Weinberg theorem if the particular QFT admits of no single-particle states except on length scales very much smaller than the natural scale of the theory?

8. **Peter**
   March 21, 2005

   No, Lee Smolin wasn't there. But a message from him was read at some point I recall.

9. **Anonymous**
   March 21, 2005

   Peter, was Lee Smolin there for this conference since he
   was Coleman's graduate student?

10.
    March 21, 2005

    Chris W.

    I seen this issue has people all tied up in knots?:)

    This idea is called emergence. It's a familiar phenomenon in the theory of condensed matter, which is Laughlin's background. Solids and liquids sometimes play host to strange entities that bear little resemblance to the atoms making up the substance. For example, in some materials there are things called spin waves. Every atom acts a bit like a small magnet, with a north and a south pole aligned along its spin axis, and spin waves are oscillations in the alignment of these spins. "Somewhat like what would occur if one took a supple picket fence and rapidly twisted one end back and forth," says Laughlin. Because this is the quantum world, waves can be considered as particles, and vice versa, so spin waves behave like a kind of emergent particle.

    http://www.fortunecity.com/emachines/e11/86/beneath.html

11. **Chris W.**
    March 21, 2005

    Maybe the views expressed a few years ago by Laughlin and Pines, and by Laughlin in his new book, have something to do with Witten's choice of the word "emergent". Chris Quigg's forceful response is contained in this article.

12. **Peter**
    March 21, 2005

    Quantoken,

    I talked to Lubos for more like 20 seconds, not 20 minutes. I often have perfectly civil e-mail exchanges with him, your guess is as good as mine why he adopts the obnoxious hyper-aggressive persona in his public internet writings.

    Tony,

    I kind of agree with you that Witten's choice of the buzzword "emergent" wasn't such a great idea. This is a subject that really would benefit from precision of language to combat the wide-spread fuzzy and wishful thinking going on. But his talk did give some insight into hows he thinks about this, which I found interesting.

13. **Tony Smith**
    March 21, 2005

Peter, a theorem is a theorem is a theorem is a theorem, and I have no problem with the Weinberg-Witten theorem (or any other theorem) IF it is clearly and correctly stated, thus making clear its domain of applicability (and inapplicability).

I DO have a problem with Witten's use of the popular buzz-word "emergent" in such a restrictive sense that it excludes such well-known formulations of gravity as Deser's (mentioned by jkg) and MacDowell-Mansouri,
and
then, having dismissed Deser, MacDowell-Mansouri, etc, by artful language, claiming that "… a theory [of]… gravity as an emergent phenomenon … doesn't now exist …"
and
then claiming that HIS pet AdS/CFT model is "… the kind of thing he had in mind …" to describe emergent gravity because in AdS/CFT "… the Weinberg-Witten argument is evaded …".

Tony Smith http://www.valdostamuseum.org/hamsmith/

14. **Quantoken**
March 21, 2005

Peter:
To me a bigger mistery than graviton and gravity is what you had to talk to Lubos when you meet. Does he seem like the same mean and aggressive Lubos we see on the blogs? What is your impression of him? You talked for 20 minutes. Is it aboout string theory, none-string theory? Or you could only talk about Boston's weather and global warming, and nothing in physics? Just curious.

Quantoken

15.
March 21, 2005

About Deser result, is it true? can we recover full Einstein gravity? There has been a recent paper which discusses that,

gr-qc/0409089
Title: From Gravitons to Gravity: Myths and Reality
Authors: T.Padmanabhan

any comment?

16. **Thomas Larsson**
March 21, 2005

Where does an emergent spacetime leave perturbative string theory? What is the meaning of maps from the worldsheet into spacetime, if the latter only exists as a long-distance approximation?

17. **Peter**
March 21, 2005

By the vague statement of Witten's I quoted, he was referring to something very precise: his 1980 work with Weinberg. There they show a problem with the idea of getting the graviton to appear as a non-perturbative effect, as a bound state of some more elementary constituents.

In MacDowell-Mansouri, I think the vierbeins are components of the gauge field, the graviton comes from them, not as a non-perturbative bound state. Anyway, you can argue with Witten's vague statement, but Weinberg-Witten is more or less a theorem.

18. **jkg**
March 21, 2005

I am also puzzled by Witten's comment that "you can't start with a local theory in Minkowski space and generate Einstein gravity as an emergent phenomenon. …". Perhaps this is because I do not understand what "emergent" means in this context. I always believed that it was clear since Deser paper of 1970 that if you want to couple spin 2 field to energy momentum, then you must end up with Einstein theory, with possible higher order corrections (that's why it is not surprising at all that in beta function calculation for heterotic string for example you get einstein action in the leading order.) Perhaps what he has in mind is that graviton is not fundamental and emerges only in low energy approximation (I remember that there were some works on this in 80s). But then again, whatever the fundamental theory is, Einstein gravity must emerge, just as a result of its universality.

Could anyone clarify this.

19. **Tony Smith**
March 21, 2005

Peter, you describe "… Witten's … saying that … you can't start with a local theory in Minkowksi space and generate Einstein gravity as an emergent phenomenon. …".

How is that reconciliable with the obvious existence of the MacDowell-Mansouri mechanism that produces the Einstein-Hilbert action from a Spin(2,3) = Sp(2) gauge theory in 4 dimensions ?

References to the MacDowell-Mansouri mechanism include Frank Wilczek's hep-th/9801184 in which he says that the MacDowell-Mansouri "… approach to casting gravity as a gauge theory was initiated by MacDowell and Mansouri … S. MacDowell and F. Mansouri, Phys. Rev. Lett. 38 739 (1977) … , and independently Chamseddine and West … A. Chamseddine and P. West Nucl. Phys. B 129, 39 (1977); also quite relevant is A. Chamseddine, Ann. Phys. 113, 219 (1978). …".
Other references include M. Botta Cantcheff who says in gr-qc/0010080 "… MacDowell and Mansouri proposed a gauge theory of gravity based on the group SO(3,2) …", and a number of other papers and books, including Mohapatra in section 14.6 of his book Unification and Supersymmetry, 2nd edition, Springer-Verlag 1992 and Freund in chapter 21 of his book Introduction to Supersymmetry, Cambridge 1986 and Ne'eman and Regge (Riv. Nuovo Cim. v. 1, n. 5 (1978) 1, at pages 25-28) and Nieto, Obregon, and Socorro in gr-qc/9402029.
It is true that construction of the MacDowell-Mansouri mechanism was motivated by using it in supergravity models, many of which involved dimensions higher than 4, but as some of the references (for example, Freund's book) explicitly show, the mechanism can
be applied and does work in the context of a 4-dim spacetime.

Am I stupidly missing something basic, or was this an error by Witten? If it were an error, then why was it not caught by Witten or a coworker before he made such a widely attended talk?

Tony Smith http://www.valdostamuseum.org/hamsmith/

20.
March 20, 2005

Found on the web: “PBS recently aired a series on ?string theory? which, to me at least, seemed to lay some groundwork for mathematical proofs of the existence of God ? not that God needs such a proof to exist. “

21. **Alejandro**
March 20, 2005

So extra dimensions are but a trick to formulate no local theories. Fine.

Recent Conferences

March 24, 2005
Categories: Uncategorized

Last week the 2005 International Linear Collider Workshop was held at SLAC, and the talks are available on-line. At the conference it was announced that Barry Barish of Caltech would lead the Global Design Effort for the International Linear Collider. The hope is to finish a design for the ILC in 2007, have a site chosen in 2008 and construction done by 2015, allowing the ILC to run at the same time as the LHC for several years, with each machine providing data that could help decide how best to use the other one.

This schedule seems overly optimistic to me. Because of the huge US deficits, getting the kinds of increases in the DOE budget needed to build the ILC in the US looks quite difficult, and, even if this were possible, funding constraints would probably stretch out the construction schedule. In Europe, CERN is devoting all its resources for a while to the LHC, and is backing an alternate, more speculative linear collider technology called CLIC. The most likely course of events seems to be that people will be waiting to see what the LHC finds and how the CLIC technology works out before fully committing to a new linear collider. If so, a decision about what to build and where to build it would probably not take place until almost 2010, with another decade probably required to actually construct the machine.

At the SLAC conference, the main theoretical talk was one by Savas Dimopoulos on New Models about his work with Arkani-Hamed on split supersymmetry and models where both the cosmological constant and the weak scale are anthropically determined aspects of the "Landscape". There increasingly seems to be a disconnect between the experimentalists planning experiments at the LHC and ILC, whose plans often revolve around the search for low-energy supersymmetry, and the string theory inspired theorists, who are spending their time wandering around the landscape. From the landscape point of view, it seems that low-energy supersymmetry is extremely unlikely.

For more from theorists wandering around the landscape, see the talks at this week's Workshop on N=1 Compactications at the Fields Institute in Toronto. The talks from this workshop are starting to become available on-line. Next week there will be even more of this at the Fields Institute, as it hosts a Workshop on String Phenomenology.

Comments

1. **Lubos Motl**
   March 25, 2005

   Peter, you should know that Savas and Nima are still leaders in SUSY phenomenology, and if they try something along different lines, it's because they have broad interests.

   Similar things apply to Shamit.

2. **quantoken**
   March 25, 2005

   DRL:
   I aready posted something about this paper a few days ago. I pointed out that:
   1.There is no credible experimental evidence for Casimir force or for vacuum energy. The calculated vacuum energy, which is 10^120 times too big so it does not exist.
   2.The Casimir force is supposed to be inverse proportional to the FOURTH power of the gap. Such a sensitive dependency on distance can not be measured by balance of force of balancing a torque, as the Lamoreaux method. His experiment, (which he himself) admittedly conducted using **$300** lab scratch materials, can not be trusted.

   Quantoken

3. **D R Lunsford**
   March 25, 2005

   Flash

   On SPR, T Larsson points to an interesting paper by Jaffe, which argues that the Casimir effect has no direct bearing on the issue of zero-point fluctuations:

   http://www.arxiv.org/abs/hep-th/0503158

   Some sanity! Hope! Of course, some of us are not surprised.

   -drl

4. **Aaron**
   March 24, 2005

   "From the landscape point of view, it seems that low-energy supersymmetry is extremely unlikely."

   Enh. The landscape can tell you pretty much whatever you want it to.

5. **Arun**
   March 24, 2005

   Maybe China could be persuaded to finance the ILC, instead of increasing its defence budget by 12.6% over last year? The increase amounts to about 3.4 billion dollars.

New This Week's Finds

March 27, 2005
Categories: Uncategorized

John Baez has just put out a new issue of his This Week's Finds in Mathematical Physics, dealing partly in more detail with the material about Clifford modules mentioned here a couple weeks ago. I've added as the first comment here something he had some trouble submitting as a comment to the older posting on this topic.

John briefly mentions a relation of all this to Bott periodicity in topology, using a very abstract homotopy construction involving spectra. A more concrete version of this can be found in Milnor's book on Morse theory. For the relation of Clifford algebras and K-theory, the standard refererence is the 1964 paper "Clifford Modules" by Atiyah, Bott and Shapiro published in the journal "Topology". The crucial fact they describe is how the Thom isomorphism in K-theory (which is essentially the same fact as Bott periodicity) is related to the structure of Clifford modules. Greg Landweber has recently worked out an interesting equivariant version of this story.

Greg also has a nice new paper with Megumi Harada about the K-theory of a symplectic quotient, that looks like it should imminently appear on the arXiv.

John also mentions some recent work of Dror Bar-Natan, Thang Le and Dylan Thurston on the Duflo isomorphism. This is a beautiful story, and also has a relation to Clifford algebras that John doesn't mention. For this, see Eckhard Meinrenken's talk at the 2002 ICM in Beijing.

Comments

1. **Peter**
   March 27, 2005

   Thanks, should be fixed now.

2.
   March 27, 2005

   Your link to the Landweber-Harada paper is broken.

3. **John Baez**
   March 27, 2005

   Dear Peter –

   I'm glad you liked week211 of This Week's Finds. I just put up week212, which digs deeper into the relation Clifford algebras and supersymmetry. A bunch of it comes from Deligne's talks at the Institute for Advanced Studies – the ones that became part of that book *Quantum Fields and Strings: a Course for Mathematicians*.

   I think there are still hopes for understanding the details of the Standard Model with the help of a deeper understanding of Clifford algebras, division algebras and related algebraic structures. Garrett Lisi mentioned Greg Trayling's work. I've never taken the time to properly understand that, in part because of some nonstandard terminology that it uses, but I'm encouraged by the fact that Garrett thinks it's good. I've spent a lot more time on Geoffrey Dixon's work. The SU(5) and SO(10) grand unified theories, and Jogesh Pati's work on left-right symmetric theories, also have something very beautiful about them. I suspect that they're all grasping various parts of some truth that we're not yet able to fathom, perhaps because we don't have the right language.

   If none of these theories are currently fashionable, well, that's in part because none of them quite hit the nail on the head – but also because the most influential particle theorists seem to have given up hope on the idea of staring at the Standard Model until something clicks and a beautiful theory takes form which explains all its baroque peculiarities. Most of the physicists who are really good at math seem willing to let the internal logic of string theory guide them where it will: either to a triumphant victory in physics, or to a journey through beautiful mathematics increasingly distant from the physical world. It's a pity that pondering the Standard Model is being left to mere "phenomenologists".

   Best,
   jb

Landscape Architecture

March 30, 2005
Categories: Uncategorized

The Perimeter Institute in Canada is known as a center for research in Loop Quantum Gravity. This week they have come up with an extremely clever way to make string theorists look bad. They've scheduled a week of talks on String Phenomenology, ending this Friday on April Fool's day. Most of the talks are related in one way or another to the "Landscape", with talks by Kachru on "Landscape Architecture" and DeWolfe on "More Landscape Architecture". If you're in the mood for a giggle, tune into these talks tomorrow: it will be all landscape, all the time, from Michael Douglas in the morning to a panel discussion in the evening moderated by Herman Verlinde on the topic "Landscape: What Is It Good For?". It's quite possible the panel discussion will be very short.

Comments

1. **D R Lunsford**
   April 3, 2005

   "Schroedinger's Cathouse.com"

   -drl

2. **Chris Oakley**
   April 1, 2005

   It is always a disappointment when one discovers that people are not acting from the best of motives. For example, when I discovered that Schr?dinger used to run a pay porn site using the University of Zurich's web space, I was devastated (I *think* that he was, anyway).

3. **Fyodor Uckoff**
   April 1, 2005

   "— Why is this all a good thing?"

   Why, that's a terrible thing to ask. I bet somebody will get the Nobel for this work some day, if indeed they haven't already.

   Meanwhile, I hear that Peter Woit is actually writing a popular book about Dirac Index Theory! Seems that his website was just to gain publicity for his new book, "Black holes, White holes, Worm holes, Bore Holes, The Bermuda Triangle, and Embeddings of SU(2) in SO(4)". And you thought it was only Stanford and Harvard profs who did that kind of thing. Michio Kaku, when asked to comment, would say only: "I think I'll wait for the movie."

4. **Arun**
   April 1, 2005

   "Remarkably, it is only in the modern context of the landscape that we can appreciate such a finely tuned theory. It would have been rejected out of hand by traditional effective field theorists only a decade ago. In the modern context, it is a strong competitor to other theories of physics at the weak scale."

   "…unlike traditional unwieldy model-building, in which additional fields are added and their phenomenological consequences studied, here we remove fields and their associated phenomenological problems."

   — Why is this all a good thing?

5. **Peter**
   April 1, 2005

   I get the joke about supersplit supersymmetry. But I still don't see why split supersymmetry isn't a joke. And please, somebody tell me that what has been going on at the Perimeter Institute this week is an elaborate hoax. It would be too depressing if this were not true…

6. March 31, 2005

   Wow. It's not often you see a theory paper with such a high author:page length ratio.

7. **Aaron**
   March 31, 2005

   Peter — there's a fascinating new paper on the ArXiV which shows how the landscape can lead to some radically new ideas in phenomenology.

   I find it hard to believe that people would have considered such a model before anthropic ideas became prevalent.

8. **Urs**
   March 31, 2005

   > People have been working on the idea that the
   > Dirac operator answers all our questions for 25 years now

   Beyond the intended parody it is maybe interesting to note that Dirac operators and index theorems and all that play a prominent role in superstring theory. The index of the heterotic worldsheet supercharge, which is a Dirac operator on loop space, is related to the elliptic genus, for instance.

   I think anyone interested in Dirac operators in general can hardly find a more fruitful area than superstring related topics.

9. **Quantoken**
   March 31, 2005

   I looked at the URL Peter provided, and find the LHC Stretch Exercise to be interesting. Any string theoretist up to the challenge of coming up with a theory to explain the presumed “data” from LHC, before Friday the April Fools Day?

I did a little bit calculation and find that the original author must had the figure of exactly 2000 GeV in his mind when he proposed that “data set”.

I guess the point they try to make is: If you can’t say anything about a clean set of hypersised data from your theory, then how could you say anything when the REAL data comes out, which is full of ambiguity and therefore much less useful? Isn’t it a complete waste of money if LHC doesn’t help you to tell which theory is right and which is wrong?

Quantoken

10. **Juan R.**
March 31, 2005

I agree with the “obscure instructor” on that “Dirac operator theory” is outdated. I’m sorry Peter, but your ideas are pure “speculation” (really is bad math)!

But, and this is an important point, string theory has derived lot of stuff outside of the standard model but **nothing inside it**!

String theory is so outdated as Dirac operator and *young and brilliant people would work in some more general and revolutionary theory*!

11. **Fyodor Uckoff**
March 31, 2005

Meanwhile, the 4/1/2005 issue of the SF Chronicle has an interview with a certain P Woit, a leading authority on Dirac Index Theory, the dominant theory in theoretical physics for the last quarter century. The theory has however attracted some criticism. Edward Witten, an obscure instructor at Princeton, says: “People have been working on the idea that the
Dirac operator answers all our questions for 25 years now, and the theory predicts absolutely nothing beyond the Standard Model. It’s long past time that people like Woit should admit that Index Theory is a failure; he should be persuading young people to try something new and radical. I’ve always thought that string theory deserves a lot more attention, attention it would have received if the Index Mafia had not dominated the physics departments of the great universities for so long.” Asked to comment, Woit said: “Talk to the Hand.”

12. **John Bell**
March 31, 2005

Spooky Processes

Scattering processes are always interesting and with regards to the Perimenter institute, Smolin retains a well balanced view of what needs to be done there.

We would need proof to this ascertion of Fool’s day. We know some people like to make use of it in regards to appling labels. Alan Sokal crying wolf?

This does not in anyway make the idea less tangible in what String Theorist think of. In regards to the information that comes out of the blackhole. They think they can get much closer theoretically.;)

New Institute at Stanford

April 1, 2005
Categories: Uncategorized

Stanford University will officially announce later today the founding of a new research institute, with major funding from the John Templeton Foundation. Many of the faculty and research staff of the new institute will come from the present Institute for Theoretical Physics which will be shutting its doors.

Co-directors of the new institute will be Stanford faculty member Leonard Susskind, and Gerald Cleaver, who is currently head of the Early Universe Cosmology and Strings Group at Baylor University. Susskind, who is one of the co-discoverers of string theory, has in recent years been the most prominent promoter of the theory of the "multiverse", which he describes in a recent interview. Later this month he will be giving the Einstein lecture at Brown University on the topic of String Theory and Intelligent Design. He is widely considered to be the leading candidate for next year's Templeton Prize. Cleaver, a prominent string theorist who was a student of John Schwarz (the co-discoverer of superstring theory) at Caltech, has published more than 40 important research articles on string theory. Like Susskind, his recent interests have been in the area of string cosmology.

Next year the institute will open its doors with a year-long program on the topic of the multiverse, led by theoretical cosmologist George F. R. Ellis visiting from the University of Cape Town. Ellis, the 2004 Templeton Prize winner, explains that the traditional view of an opposition between faith and science has been made obsolete by the latest research in string theory and cosmology. Says Ellis, "In the end, belief in a multiverse will always be just that — a matter of belief, based in faith that logical arguments proposed give the correct answer in a situation where direct observational proof is unattainable and the supposed underlying physics is untestable."

The new institute will be named the Stanford Templeton Research Institute for Nature, God and Science (STRINGS) and will collaborate with other related Bay Area organizations, including Stanford's own KIPAC (Kavli Institute for Particle Astrophysics and Cosmology) and Berkeley's CTNS (Center for Theology and the Natural Sciences). Steve Kahn, the director of KIPAC, welcomed the formation of the new institute saying "We're very pleased to have such a major institution on campus led by two such prominent physicists working on cosmology. In this era of declining NSF and DOE budgets, we need to branch out from traditional approaches to science. We expect to collaborate with the new institute to help us seek funding from sources such as the President's FBCI initiative." Besides the physicists, several faculty from other Stanford departments will be affiliated with the Templeton institute, including computer scientist Donald Knuth, author of the recent book Things a Computer Scientist Rarely Talks About.

According to Dr. John M. Templeton, Jr., president of the Templeton foundation, "the idea for the institute grew out of our involvement with a series of lectures at Stanford in the area of biology. At those lectures the biologists pointed out to us that it was the physicists on campus who were doing work most closely related to our foundation's interests, something we had already noticed through our Cosmology and Fine-tuning Research Program. As the latest cutting-edge research in physics has caused physicists to rethink what it means for a theory to explain experimental data, the wedge driven by Galileo between science and religion has begun to close. We're very proud to be able to support and encourage this trend."

Encouragement also comes from some other members of the Stanford physics department. Nobel-prize winning theoretical physicist Robert McLaughlin was quoted as saying "theoretical particle physics is just getting old and losing its youthful good looks. Even Ed Witten has given up on it. This latest plan for the cosmology/multiverse/string theory crowd to join up with Templeton reminds me of a woman deciding to become a nun when she gets too old to attract men. But if it gets them out of the physics department, I'm in favor of it. Don't let the door hit you on the way out, guys."

Comments

1. **Juan R.**
   April 7, 2005

   Hi J, thanks you by detail on Ellis and Hawking. I searched the old article by Ellis that I discussed below.

   Los l?mites de la Cosmolog?a

   George f. R. Ellis

   El enfoque epistemol?gico de la cosmolog?a lleva a recordar algunas perogrulladas y a plantear verdaderas dificultades. El hecho de que no exista m?s que un ?nico universo observable impide cualquier comparaci?n de este objeto con otro, una condici?n que sin embargo es necesaria en cualquier procedimiento cient?fico.

   That is, cosmology is not one of positive sciences.

   For fans of ?landscapes? and all stuff, simply to say that Ellis considered the vague discourses about ?multiple universes? like outside of physics.

2. **cvj**
   April 4, 2005

   BRILLIANT!!!!!!

3. April 4, 2005

   Quoting A. Nonymous
   "Nice one Peter! I only realized something's going on when I reached the "Stanford Templeton Research Institute for Nature, God and Science (STRINGS)" ;-)"

   Me too!!! Extremely good!

   Congrats!

4. **Not a Nobel Laureate**
   April 3, 2005

   "A few years ago I followed a good course of super-strings, together with good students. But at the end I found no way of applying the interesting things I learnt to physics, and it was too early for discussing this problem. So I sent an e-mail to students (modifying its header such that it seemed sent by the teacher) telling that, instead of a formal examination, they had to give seminars choosing from a list of topics. Each topic was a nonsense, obtained by combining in a pseudo-random order the usual words of stings papers.

It was an instructive joke.

Many of these students now are at major US universities."

So we are witnessing the final convergence between
String Theory and Post-Modernist Deconstructionist
Literature Theory.

My only question. Why did it take so long?

5. **Not a Nobel Laureate**
April 3, 2005

Excellent April Fool's parody.

6. **Matti Pitkanen**
April 3, 2005

Thanks for Quantoken for the comment concerning proton decay.

If quark and lepton numbers are conserved separately, the GUT mechanisms of proton decay are excluded. This is achieved in TGD framework since quarks and leptons correspond to different
conserved chiralities of M^4xCP_2 spinors induced
to space-time surface. The couplings of quarks and leptons to CP_2 Kahler gauge potential are n=1 and n=3 multiples which gives standard model quantum numbers correctly. Color is now not a spinlike quantum number but basically angular momentum like: this means a profound difference compared to QCD.

One can of course consider the possibility that quarks have lower mass states. TGD indeed suggest the existence of fractal hierarchy of QCD like theories for colored excitations of also leptons and there are some experimental findings giving some support for this conjecture. This picture is consistent with W and Z^0 decay rates if these QCD like theories are not asymptotically free and exist only in some finite energy and momentum transfer range. This framework allows to consider the transformation of ordinary baryons to baryons of scaled down hadron physics by a kind of tunneling mechanism.

Best,
Matti

7. **Quantoken**
April 3, 2005

Regarding proton decay. I must emphasis that it is not just the standard model that predicted that proton decays. The point is we all know proton is not the most fundamental building block. It clearly has internal or intrinsic structures. Because of that **ANY reasonable theory would have to lead to a prediction of proton decay**.

The current lack of evidence for proton decay can be interpretted in one of several ways:
0.Protons indeed does not decay ever.
1.Protons decay in ways predicted by the standard model, but its life time is too long to be detectable.
2.Protons decay in ways we do not know, and our current detection technology would not have registered any signal.

I think the 3 is the most possible scenary. There are plenty of things that the standard model can not explain. The most notable ones would be super high energy particles detected in nautral occuring cosmic rays. The energy is many times higher than the energy/mass of most elememtary particles, like proton and neutron.

What kind of cosmic process would leads to that kind of energy? There has been no plausible answer so far. You can throw in a black hole or things like that, but at most you can get something with an energy level of the same order of magnitude as

8. **Matti Pitkanen**
April 2, 2005

> The years when the proton refused to decay were very interesting. Everything indicated that it should. We had a beautiful, simple, elegant, minimal model, namely SU(5), that unified all three subnuclear interactions. It was proposed by our best and brightest. There was no hint of an inconsistency. It all looked great. However —- God refused to play along.

Perhaps God tried hard to tell us something very important but failed;-).

Matti Pitkanen

9. **Randall Rhodes**
April 2, 2005

I realize I just made an acronym mistake. I meant to say String Theory rather than SUSY (supersymmetry.)

10. **Randall Rhodes**
April 2, 2005

I've been reading the Greene book "Fabric Of The Cosmos." I like the way it was written. Of course, when you get to the SUSY part, the end of that chapter becomes murky in terms of resolving questions (In the same sense that the Standard Model explanations cannot continue to provide many more answers.)

I trust Greene's reasoning and articulate descriptions of SUSY, I understand the concepts better than I did before. But I can't help but feel that I'm reading about a framework that is literally being reverse-engineered to fit models that have already demonstrated tangible results. It will be interesting to see which will be discovered first: The Calabi-Yau shape predicting all particle charges, forces and masses or direct proof of the Higgs particle in the LHC.

P.S. The search for the right Calabi-Yau shape seems contrived in the way that re-normalization seemed contrived. Feynman invented re-normalization but was openly hostile towards it. Are any SUSY theorists at least showing mild skepticism about this issue?

11.
April 2, 2005

The years when the proton refused to decay were very interesting. Everything indicated that it should. We had a beautiful, simple, elegant, minimal model, namely SU(5), that unified all three subnuclear interactions. It was proposed by our best and brightest. There was no hint of an inconsistency. It all looked great. However —- God refused to play along.

12. **Quantoken**
April 2, 2005

Any one wants to comment on this recent discovery of a planet outside solar system? I think It **does NOT look like a April Fool's joke** at all:

First confirmed picture of a planet beyond the solar system

It is also reported on CNN:
http://www.cnn.com/2005/TECH/space/04/01/extrasolar.planet.photo/index.html

Within 5 seconds of seeing it, I became **skeptical** about this result and has an opinion. But I could be wrong:-) I want to see other people's response to this news.

Quantoken

13. **Peter**
April 1, 2005

Hi Tony,
The period when I was at Harvard (75-79) was after GUTs had already been formulated, but proton decay experiments were just getting underway. Glashow and Georgi especially were working on various GUT models.

There are plenty of people (for instance, besides Glashow, Georgi and Wilczek) who never thought much of string theory. But after the initial failure to see proton decay, most people felt that ruled out the simplest SU(5) GUT, which got them more interested in supersymmetric GUTs. I don't know anything about the background problems you mention, but I find it hard to believe Glashow would give up his ticket to another Nobel prize easily. As for why Glashow is the only one to complain publicly, you'd have to ask the others. But part of it is that Glashow has a stronger personality, is less likely to keep his views to himself than many others.

Milnor wasn't at Stony Brook when I was there, at the time he was a professor at the Institute in Princeton. I've never talked to him about string theory, but most mathematicians are pretty impressed by string theory, partly because Witten has pushed it so hard and he has a well-deserved Fields medal, partly because some very interesting math has come out of string theory.

14. **Tony Smith**
April 1, 2005

Peter, you say in your personal background blog entry at http://www.math.columbia.edu/~woit/blog/archives/000006.html
"… 1979: B.A. and M.A. in physics, Harvard University. …
1984: Ph. D. in theoretical physics, Princeton University, …
1984-87 Postdoc at the Stony Brook ITP Got interested in spinor geometry,TQFT and representation theory, started talking to a lot of the mathematicians at Stony Brook In 1987 it became clear to me that someone who didn't believe in string theory but wanted to apply mathematics to QFT didn't have much of a future in physics depts in the US. …".

So, you were at Harvard during or just after the birth of the Standard Model, and around the birth of GUT models, and you got to see all that up close and personal. It seems to me that would have been a fascinating experience, and here are some questions:

Did anyone other than Glashow keep the faith of the Standard Model, and if so where are they now and why don't they say something about the present state of superstring theory ?

It seems to me that models such as (non-susy) GUT that are based on the Standard Model have been (by most of the physics community) abandoned and that a lot of hype has gone to supersymmetry (with no experimental support) and to beyond-the-standard-model stuff (also with no experimental support). Is a reason for such abandonment the stated position of most neutrino laboratories that proton decay has not been observed within the predicted lifetimes of GUT models?
If so, then what if all those laboratories have been using incorrect background models in their data analysis ? Did ANY of the GUT founders (including but not limited to Glashow) do a detailed study of the background models used to refute GUT ? If not, why not ?
One reason that I ask about such background is that one study done independently of the big neutrino observatories was Experimental evidence for G.U.T. Proton Decay http://xxx.lanl.gov/abshep-ex/0008074 by Adarkar, Krishnaswamy, Menon, Sreekantan, Hayashi, Ito, Kawakami, Miyake, and Uchihori.
Roughly, they conclude that a different (and at least equally reasonable) choice of background, if applied to the raw data from many neutrino observatories, would produce results not inconsistent with GUT.
Even though this seems to me to be an important result reviving a class of models that are by construction quite consistent with the Standard Model, as far as I know NOBODY, not even the inventors of GUT, ever attempted to bring the discussion of background into a foreground of discussion in the world of physics of the viability of GUT models.
Why ?

One last question – was Milnor at Stony Brook math while you were there? If so, did he express opinions about superstrings, and if so, what did he say? (I have thought of him as a very reasonable (as well as brilliant) person whose opinion I would respect.)

Tony Smith http://www.valdostamuseum.org/hamsmith/

15. **Chris Oakley**
April 1, 2005

I must admit that I read "Dennis Overbyte"'s comment as humorous invention rather than a personal attack. In any case I would rather have Donald Trump rooting for me than any academic.

16. **Quantoken**
April 1, 2005

Anonymous below:
Go figure!

Quantoken

17.
April 1, 2005

quantoken, your link goes straight back to Peter's original blog. explain?

18. April 1, 2005

    Pretty weak.

19. **a**
    April 1, 2005

    A few years ago I followed a good course of super-strings, together with good students. But at the end I found no way of applying the interesting things I learnt to physics, and it was too early for discussing this problem. So I sent an e-mail to students (modifying its header such that it seemed sent by the teacher) telling that, instead of a formal examination, they had to give seminars choosing from a list of topics. Each topic was a nonsense, obtained by combining in a pseudo-random order the usual words of stings papers.

    It was an instructive joke.

    Many of these students now are at major US universities.

20. **Arun**
    April 1, 2005

    Hilarious!

21. **Quantoken**
    April 1, 2005

    That's a good April Fool's joke, Peter. You tricked quite a few people. But not me.

    Why? Not because I was smart. But because once I see your title, it looks so striking familiar: "New Institute at Stanford". I swear to God I just saw the same title and same story somewhere else on the netland! Just a few minutes ago!

    So I opened up my web browser, and went to the page I visited a few minutes ago. And Gosh! I caught you, Peter Woit! He copies the story eaxctly word by word. I can't believe even Peter Woit would commit pragiarism. I was shocked, shocked, shocked!!!

    Hurry before Peter has a change to erase my message. See for yourself. Here is the exact URL of the story Peter was copying from. I will forgive him because today is April 1st 🙂

    Quantoken

22. **Peter Woit**
    April 1, 2005

    A couple comments on the long posting about my ascension to the status of assistant professor from someone at MIT who doesn't have the guts to publicly put their name to what they write using the machine sci-22.mit.edu.

    Soon after I started writing this weblog I found that partisans of string theory almost uniformly preferred to personally attack me and my academic qualifications rather than to deal with any of the issues I was raising here. I wrote a posting explaining my academic background, it's at

    http://www.math.columbia.edu/~woit/blog/archives/000006.html

    Note that I held the title of assistant professor at Columbia for four years (1989-1993), a status I'm not interested in going back to. I'd turn it down if the department tried to reappoint me to it. Since I wrote that posting, my formal status at the university has changed slightly. The non-standard "Director of Instruction" title I held for many years was regularized a year ago when Columbia instituted a new system for permanent non-tenured positions. I'm now officially a full-time, non-tenured member of the faculty of arts and sciences with the title of Instructor. It's a permanent non-tenured position, with a schedule of reviews of my performance.

    Over the years I've often heard from particle theorists who tell me they agree with me about string theory but don't dare say so publicly due to the personal attacks they fear they would be subject to. This ugly atmosphere of intimidation is a disgrace and string theorists should be ashamed of being associated with it.

23. April 1, 2005

    If you wanna fun little diversion, look at today's hep-th abstracts and try to guess which ones are April Fool's jokes.

24. **J**
    April 1, 2005

    This is the same "Ellis" as in Hawking and Ellis of "Large Scale Structure of Space-time"

25. **Dennis Overbyte**
    April 1, 2005

    April 1, 2005, New York — Today Columbia University Mathematics Department chairman Prof. John Morgan announced the appointment of Dr. Peter Woit to the position of assistant professor.
    Prof. Morgan would make no further comment on the appointment, but several members of the mathematics department were willing to speak off the record. According to these sources, Prof. Morgan received a Monday morning call from Donald Trump, offering to make a substantial donation to the mathematics department. Apparently Mr. Trump was recently in San Francisco for the taping of an upcoming episode of "The Apprentice", "How to buy and sell the Golden Gate Bridge; a new approach to an old idea", when he happened upon a copy of the San Francisco Chronicle. He was immediately drawn to an article that was critical of the recent craze
    in string theory. Mr. Trump felt that string theory was being overplayed, saying "Come on! Brian Greene's name is in the Times, more than mine. How ridiculous is that?!" He read the article and was especially impressed by the comments of Dr. Woit who was listed as a member of the mathematics department at Columbia.
    Mr. Trump was impressed that Columbia's mathematics department had hired someone like Dr. Woit and wanted to show his appreciation. However, during their telephone call, Prof. Morgan
    informed Mr. Trump that Dr. Woit was not actually a member of the faculty, even though Dr. Woit might have given that impression to Keay Davidson, the author of the Chronicle article.

At this point, Mr. Trump, according to several sources, hurled more than a few expletives at Prof. Morgan, yelling "How can he not be on the faculty, he is your most famous guy?!!". When Prof. Morgan tried to explain that Dr. Woit had not published a paper since 1989 and that it would be impossible to get the rest of the faculty to agree to a promotion, Mr. Trump countered, "Who the f@*k cares about publishing in peer reviewed journals. That is so 20th century. Blogging is where the intellectual activity is at. Peter has shown how to cut through all the crap that passes for real science nowadays! If you want the donation you better f%!king hire him!"
At this point, Prof. Morgan promised to hold an immediate faculty meeting to decide on the appointment. After a fractious Tuesday afternoon meeting, the faculty voted to promote Dr. Woit. In order to save face, Prof. Morgan got Dr. Woit to promise that a paper on representation theory would be forthcoming within five or ten years. The title of the paper is purported to be "Quantum Field Theory and Representation Theory: An Etching"
After the announcement, Prof. Erick Weinberg, chairman of Columbia's physics department said, "I think it is great that the math department hired Peter. If nothing else it will piss off Brian."
Asked for comment, Gerald Cleaver, a recent victim of one of Dr. Woit's April Fools Blogs said, "I am actually happy for Peter. As Peter knows, I am a practicing Christian, but I am happy with who I am and I feel that I have done good, solid work in string theory and have the publication record to prove it. Anyway,
I am more of a `turn the other cheek' guy than an `eye for an eye' guy. But my wife thinks he is an asshole."

26. **Kyle**
April 1, 2005

I fully expected to click on the " Things a Computer Scientist Rarely Talks About" link and find a blank page.

27. **Fyodor**
April 1, 2005

I see that Cleaver's site refers to his
"PREPINTS".

I think that post-pints would have been more apt. Post quite a lot of pints in fact. Does whiskey still come in fifths, by the way?

PS: Peter, it is always a mistake to parody people like Laughlin, when he is so good at it himself, albeit unintentionally. Also it is bad strategy to draw attention to a string critic who is much more of a fool than any string theorist.

28. **Juan R.**
April 1, 2005

Is this "Ellis" not the same Ellis that openly critiqued the standard view of cosmology some decade ago?

If I am not wrong, Ellis thought that cosmology was not one of positive sciences. He tought that cosmology was more a semi phylosophical field, where scientific method was not applicable.

Interesting.

P.D: The "success" of string theory is independent of the number of centers devoted to it.

29. **Mark Trodden**
April 1, 2005

Wickedly funny post Peter. When I was out at LCWS04 at Stanford a couple of weeks ago I was dismayed to find out that there was a Templeton conference going on at the same time and that a number of prominent people were attending it rather than LCWS04.

30. **a**
April 1, 2005

From a theological point of view the landscape multiverse is an extension of Darwin natural selection. Therefore I do not believe that it can get much financial support. Your 1 April post seems unrealistic.

31. **anonymous**
April 1, 2005

Another good one for the day:

http://www.arxiv.org/abs/hep-th/0503249

32. **D R Lunsford**
April 1, 2005

Very funny. Unfortunately all those linked things are really happening. Somehow I don't feel like laughing.

-drl

33. **A. Nonymous**
April 1, 2005

Nice one Peter! I only realized something's going on when I reached the "Stanford Templeton Research Institute for Nature, God and Science (STRINGS)" 😉

34.
April 1, 2005

Jesus christ, this is the best April Fool's ever.

35.
April 1, 2005

I’m ashamed to say how far into this that you had me. Well done!

Witten in Canada

April 4, 2005
Categories: Uncategorized

Edward Witten is in Canada this week, giving a series of lectures at the Fields Institute, the mathematics institute at the University of Toronto. He's also giving a public lecture at the Perimeter Institute in Waterloo.

The first of his lectures in Toronto, on the topic of Relativistic Scattering Theory is now available on-line. It's a nice, simple explanation of scattering theory, using the "geometric quantization" point of view about quantum field theory. For a quantum theory of a real scalar field, one chooses a complex structure on the space of solutions of the free field equation, making this a priori symplectic infinite dimensional manifold actually a Kahler manifold. Witten's next two lectures in Toronto will be on "Gauge Symmetry Breaking" and "The Quantum Hall Effect".

I'm curious what he'll be promoting in the public event at Perimeter on Wednesday. Will it be string theory? Will anyone ask him about the appalling nonsense his fellow string theorists were spouting there all last week?.

Comments

1. **Quantoken**
   April 6, 2005

   Peter said: "The problem with the multiverse is not that it invokes things you can't **directly observe**, the problem is that it predicts nothing about anything."

   I would accept any **indirect observation** as a legitimate observation of nature, as long as the observation can be consistent and has the potential of rule-out/rule-in certain theoretical models. The problem is even INDIRECT observation of multiverse is impossible. It simply does not interact with our universe in any way that is observable, or at least not in a way that allows us to speculate any property of the multiverse.

   Anything that does not deal with objective observation of this particular universe we know, is not science.

   On a side note, I think it is pretty stupid for us to send THREE presidents to the funeral of a leader of a private religion, in a tiny country embbeded in a country full of street mafias and burglars, at a time when 100 other national heads also gather, in an era that we constantly face terriorist threats. It's recklessly dangerous and do no good other but provide a golden opportunity to those who want to do harm.

   Frankly I do NOT think they have any significant police force to provide any meaningful protection in any way, shape or form, in such a complete chaos when a couple million people around the world all want to rush in to see the Pope, including the one who attempted on his life in 1981!!! I can only hope that nothing bad happens.

   Quantoken

2. **Peter Woit**
   April 6, 2005

   Hi Mark,

   I took a look at the Spergel talk and the discussion afterwards. What really amazed me were the people trying to defend the whole multiverse nonsense by claiming it was just like the situation in the sixties with the quark model, which posited entities one couldn't see. This is really absurd. The quark model made all sorts of predictions which could be tested by experiment, most notably the prediction of the existence and properties of the Omega-minus. The problem with the multiverse is not that it invokes things you can't directly observe, the problem is that it predicts nothing about anything.

3. **robert**
   April 6, 2005

   Carl,
   1+2+3..=-1/12, surely. Still, what's a factor of two amongst theorists.

4. **Mark**
   April 6, 2005

   On an unrelated note see the discussions on anthropic
   principle, multiverse and string theory after David Spergel's
   talk at STSCI on dark energy
   http://www.stsci.edu/institute/center/information/streaming/archive/STScIScienceColloquiaSpring2005/

5. **D R Lunsford**
   April 5, 2005

   Dear ____,

   The point is, the existence of this Kaehler manifold is apparently equivalent to the need for antimatter. See this thread from SPR:

   http://www.lns.cornell.edu/spr/2003-04/msg0050057.html

   -drl

6. **Carl Brannen**
   April 5, 2005

   Dear Dr. Wotit,

   Around 5 years ago, I picked up a two-volume textbook on string theory that was supposed to be the most widely used text for graduate classes and figured that I would update myself on QFT. I was horrified to discover that the iffy premise of analytic continuation (as mentioned in the Witten lecture) had been replaced by dimensional regularization. Before I had gone more than a few dozen pages the author was had summed 1+2+3+… to get -1/6, which reminds one of divergent series analysis from the early 20th century, but was not what I expected a theory of everything to provide. So I decided to begin studying QFT again, to see if I could bring the standard model into a generalization of the Dirac wave equation.

The analytic continuation of the electron from past to future reminds me of the fact that the handedness of anti-particles are the opposite of the handedness of the spinors that represent them. In order to generalize the Dirac equation into a Clifford algebra equation, I had to use the handedness of the spinors rather than handedness as it is usually defined. Instead of conserving angular momentum, in this language, the usual interaction vertices conserve the difference between the handedness of the particle spinors and the handedness of the antiparticle spinors.

Anyway, it was interesting to see Witten's lecture.

Carl

7. **Alejandro**
   April 5, 2005

   BTW quantoken, I like the aspect of your new journal.

8. April 5, 2005

   An entire lecture by Witten (I was hoping for something more profound) leading up to… crossing symmetry? How anticlimatic!

9. **Quantoken**
   April 5, 2005

   No need for dark energy

   Have a look at my latest blog entry, where I talked about the possibility of "dark energy" being just the regular baryon matter, distributed throughout the voids of the universe, in the form of micro meteorites that's hard to detect but yet constitute 70% of the mass of the universe.

   Click here.

   BTW it is also a better blog server than username.blogspot.com. I encourage people, like Lubos et al. to use livejournal instead of username.blogspot.com.

   Quantoken

10. **DQOTD**
    April 5, 2005

    Dumb Question of the Day:

    If T, C or P is not a symmetry, then does the holomorphic S defined on Sym(r+s) H_C acquire additional interesting structure?

11. **Alejandro**
    April 5, 2005

    So, after all, Witten surrenders string theory and promotes barebones QFT.

12. **D R Lunsford**
    April 5, 2005

    Very interesting. Compare Feynman's Dirac Memorial Lecture (reproduced in the little Dover book "Elementary Particles and the Laws of Physics").

    -drl

High Energy Physics: Exit America?

April 6, 2005
Categories: Uncategorized

Science magazine has an article this week entitled High Energy Physics: Exit America?. It describes the US HEP budget situation, and gives details of the probable cancellation of the BTeV experiment. Evidently neither Michael Witherell, the Fermilab director, nor any of the physicists working on BTeV, had any idea this was going to happen until the day the FY 2006 budget was released.

The Science article is a lot more pessimistic about the future of high energy physics in the U.S. than any of the public reports you will read produced by the US high energy physics community, but it is also a lot more realistic. The underlying reality is that after the Tevatron stops operations in 2010 (because it can't compete with the LHC), for the first time in the history of modern physics there will be no machine operatiing at the high energy frontier in the US. Fermilab is planning an active neutrino physics program, but this will be much more limited in scope than what the lab is doing today and has been doing since its founding.

The only plan on the table for the US to get back into the high energy accelerator business is the International Linear Collider (ILC), but the question of how such a machine would be financed, and whether it would even be constructed in the US at all, remains up in the air. In a very real sense, the future of experimental high energy physics in the US after 2010 is a very large question mark.

Comments

1. **D R Lunsford**
   April 10, 2005

   Actually, it's far worse – because it's something that can't be fought – lack of curiosity.

   http://www.washingtonpost.com/wp-dyn/articles/A39175-2005Apr9.html

   An incurious society is a dead one.

   -drl

2. **Juan R.**
   April 9, 2005

   I think that high-energy physicists fail to recognize the point,
   I don?t agree with Quantoken about ?energy economics?. The reason of decreasing of interest by society is that High-energy physics is mainly a useless discipline.
   Therefore, it is rather natural to stop its funding.
   Dear Peter, I sincerely think that you are chosen the wrong discipline.

3. **Chris Oakley**
   April 9, 2005

   Re: The Pauli anecdote, the version I heard was this.

   Pauli dies and goes to heaven. Before St. Peter gets a chance to look through his records, Pauli demands to see God. St. Peter eventually gets tired of arguing and leads him to God.

   "WHY IS THERE A MUON!?" demands Pauli.

   "Well, my son, let me explain …"

   God then writes down the equations and just as he starts to wade through the deeper mathematics Pauli says,

   "THAT'S IT! THAT'S WHERE YOU'VE GONE WRONG!"

4. **Heinrich**
   April 9, 2005

   Another reason why the anthropic principle is wrong is the one given in some internet book I read recently (I think it was Schiller's): Also apes can claim that the laws of the universe are made in just that way that apes could evolve. It would therefore be equally correct to call it "simian principle".

   From the quality of the argument it might even be the better name.

   Heinrich

5. **D R Lunsford**
   April 8, 2005

   I'm a physicist, I can live with a result. *You* talk to God, I'm busy!

   -drl

6. **Alejandro**
   April 8, 2005

   Which remember me… was Pauli the one in the joke where G-d starts explaining how He waved the universe, and in the middle of the explanation he stands up, signals an equation G-d has just written, and shouts "Wrong!"

7. **Alejandro**
   April 8, 2005

Nonsense. Ask Him for a proof!

Of course, we are still interested on His Opinion about Continuum Hypothesis.

8. **D R Lunsford**
April 8, 2005

Yo G,

Please ask God if the the Riemann conjecture is true. Thanks in advance!

-drl

9. **G.H. Hardy**
April 7, 2005

Good Math is useless, EXCEPT, up here in heaven where I'm forced to work on applications!

And I take back my apology; if you people had let me asassinate Benito Mussolini like I wanted to…I wouldn't be stuck in this heaven-hole.

10. **Quantoken**
April 7, 2005

DRL said: "(PS: Did you notice how Q made my point for me? He instantly began a **good old Republican** harangue on power, and how I was "whining" to dare criticize my knotheaded neighbors.)"

DRL, you got me completely wrong. I am a registered democratic voter. My whole family is. I have never voted once for any republican candidate in my whole life. I have openly criticized Dana Rorabach, a republican congressman that I think chairs the science committee. But that does not stop me from supporting his view, as well as Lubos, another die hard republican, on the global warming thing. It's none partisan.

Quantoken

11. **D R Lunsford**
April 7, 2005

DMS –

It's peculiar because we were born as an *intellectual* experiment. Madison, Jefferson, Hamilton, even Washington, most of these folks were intellectual titans. The great divide for us came with Jacksonian populism. Like a gas expanding into a vacuum, the type of person needed to settle everything we bought or stole or inherited through Indian genocide was the "man of action", not the man of sober reflection. And right there, the intellectual experiment ended. A free republic can't exist with dolts at the levers. I submit that we've been in mental and moral decline since the first generation died out.

Now, we're just as natively smart as everyone else, but the populist/collectivist stain prevents us from individually enjoying knowledge for its own sake, for the most part. Knowledge and culture is for "sisses". Math is for "nerds". Science is for psychos.

Maybe someone with charisma will manage to get across to the hammer-headed people here that we're getting our ass handed to us by China. Mabye. It's possible.

Nah.

-drl

(PS: Did you notice how Q made my point for me? He instantly began a good old Republican harangue on power, and how I was "whining" to dare criticize my knotheaded neighbors.)

12. **DMS**
April 7, 2005

I have to agree 100% with what DRL wrote.

As a foreigner, who studied in the US, I was struck (and surprised) by the sharp intellectual divide in the US. In fact, a significant fraction of the US population is vehemently anti-intellectual (larger, I think than anywhere in Europe or emerging economies in Asia like India and China).

Of course, in every country popstars and sports people are always very popular. But at least in Asia, people with mathematical and scientific talent (like, say a Nevanlinna prize winner;)), are practically revered. History and tradition of learning plays an important role.

A couple of years back, BBC World did a worldwide poll on who the world thought was the greatest Briton. Thanks to the large population of India, the clear winner was Issac Newton. Interestingly, Charles Darwin also got the most votes from 'backward' India. (I suspect that if Germans were to hold a similar poll in India, Gauss would be on top.)

I think it is a safe bet to say that neither of the two Britons would be the top Briton in such a poll in the US.

13. **Quantoken**
April 6, 2005

DRL:

Quit whining. Frankly I think the American taxpayers have been pretty generous in funding science research. The problem is the return is disproportional to the investment. What we get for funding super string theory research for a quarter century? Nothing. You need to work out something useful to be worthy the public's money.

What is the name of the department you get most of your funding from. It's not DOE as you would call it, but "Department of **ENERGY**". The key word is **energy**. We are facing an energy crisis! Oil reserves are being depleted! The top funding priority for the department of energy should be to find a solution to the energy crisis. Particle physicists should be spending their time figuring out better ways of utilizing nuclear energy. That's where they can make some contribution!

But on second thought, forget about it. The research of controlable thermal nuclear fusion has been going on for four decades with little success. Even if it can be success it is probably too late to save us from the oil crisis.

Even if controled nuclear fusion can be put into industry production, I heard that one liter of sea water contains enough deuterium to produce the energy equivalent of 1.5 liter gasoline. The problem is extracting the deuterium would probably cost a couple thousand liters of gasoline in terms of dollar cost, and a few hundred liters of gasoline in terms of energy cost. So it's just not feasible.

I just realized that scientists have been singing the praise of one liter seawater equivalent to 1.5 liter gasoline, and talks about going to the moon to mine Helium 3 and bring back. But **no one ever meantioned the possible prohibitive cost**, which is orders of magnitude higher than the potential benefits. Why are scientists so dishonest?

The department of energy should shift the bulk of their fundings to researches of biodegration that may generate the replenisheable replacements of gasoline. Which is the only feasible solution to the energy crisis certain to come pretty soon.

Quantoken

14. **D R Lunsford**
April 6, 2005

Why is anyone surprised? Party conversation:

"What do you do for fun?"

"I work on my physics problems."

"OH! I'm just terrible at math!"

Americans loudly and proudly proclaim how bad they are at math. They pay lip service to science, but really they are afraid of everything, and particularly math. When we need to "whup the Russkies" we poured money into weapons research by the trillions, and this gave the surface appearence of interest in science. But in fact Americans have always seemed to actively hate science, "mad scientists", and the spirit of inquiry. Knowledge is the wrong palliative for their overarching fear.

American science is not about knowledge – it's about power. We care about science only insofar as it makes us seem powerful. We want to feel powerful because in fact we are trembling little children, afraid of each other, afraid of ourselves, our fellow inhabitant on this world, afraid of everything.

-drl

15. **Peter Shor**
April 6, 2005

The budget for fundamental science in other areas in the U.S. isn't good either. Despite the fact that theoreticians cost next to nothing, compared with experimentalists, the projected funding levels for theoretical computer science at NSF in the next few years look dismal. And I believe that NASA is being forced to cancel a number of its planned expeditions.

16. **Steven S**
April 6, 2005

Hi, Peter

Lower energies are not doing so well either. Recently the proposed Rare Isotope Accelerator was cut from the next federal budget with only $4 million remaining for research and development. It has officially been delayed but looking at future federal budget projections to me it looks effectively dead.

17. **a**
April 6, 2005

Since 20 years US colliders have not been competitive with SppS, LEP and hopefully LHC.
TeVatron is a poor substitute for SSC, and ILC will not reach an energy much above LEP. A likely scenario is that US will build TESLA (which is the original German name of ILC) in order to avoid the collapse of its hep experimental community.

18. **Peter Woit**
April 6, 2005

Hmmm, actually if theorists could confidently tell us what such a machine would see, that would be a really good reason not to waste the taxpayer's money on building it. But we don't know a fundamental fact about how the world works (how electroweak symmetry is broken) and such a machine might tell us. I don't think it is hard to argue that finding that out is worth the very small fraction of the US budget that would be required.

19. **Quantoken**
April 6, 2005

I think it is a good thing for the US government to stop throwing **money**, which is a linear term, towards **orders of magnitude**, which is an exponential term. It makes no sense especially when the current theoretical models clearly can NOT tell us what we will expect to find at certain specific reachable energy levels. When scientists can tell us without ambiguity, but with an adequate confidence, that we shall find particle X at energy level Y if we do experiment Z, then maybe we can give it a try. Before that happens I do not see why we need to do a multi billion dollar gamble regarding what we could or could not find, especially when the national debts are piling up.

Scientists are also members of the society and they need to respect the tax payer's money a little bit more, especially since they are supported by that money, frankly, I think. There are better usages of the money.

Quantoken

Not a Joke

April 7, 2005
Categories: Favorite Old Posts, Uncategorized

A week or so ago I wrote up as an April Fool's joke a posting claiming that the Stanford theoretical physics group was joining a new Templeton foundation devoted to religion and science. At the time I had no idea of the degree to which Templeton-funded pseudo-science has infected mainstream cosmology. This joke turned out to be much closer to reality than I had imagined. In my quick research before writing it, I had missed the fact that the Templeton Foundation two years ago organized a symposium at Stanford on the topic of Universe or Multiverse?. The participants, presumably funded by Templeton, included a large fraction of the senior Stanford ITP faculty (Dimopoulos, Kallosh, Linde, Susskind). Someone also wrote to me to tell me that Gerald Cleaver had spent a sizable amount of time at Stanford at Susskind's invitation, something I was completely unaware of when I picked him to co-direct the Templeton institute with Susskind. Finally, Mark Trodden reported in the comment section that "When I was out at LCWS04 at Stanford a couple of weeks ago I was dismayed to find out that there was a Templeton conference going on at the same time and that a number of prominent people were attending it rather than LCWS04."

One of the other attendees at the Templeton conference was Alexander Vilenkin, and yesterday Lubos Motl had a report on Vilenkin's talk at Harvard on "Probabilities in the Landscape". Lubos explains in some detail what a load of pseudo-scientific nonsense this all is, and I'm in complete agreement with him, down to his last paragraph about how "Finally, I am sure that various people who have a similar opinion about the anthropic thinking will use this admitted frustration as a weapon against string theory." Certainly. By the way, Vilenkin's research is funded by a Templeton grant.

It seems that Cambridge University Press will be publishing a volume this year also entitled "Universe or Multiverse?" based on the Stanford symposium. It's being edited by Bernard Carr, a professor of Mathematics and Astronomy at Queen Mary College in London. He's the recipient of a Templeton grant for a project entitled "Fundamental Physics, Cosmology and the Problem of our Existence". When he's not working on cosmology and religion, he is President of the Society for Psychical Research, which investigates poltergeists, parapsychology, survival after death, etc. You couldn't make this stuff up. "Universe or Multiverse?" will include as least one sensible article, Lee Smolin's Scientific Alternatives to the Anthropic Principle, which explains clearly why the Anthropic Principle is not science.

Another participant in the Stanford symposium was Robin Collins, and he's contributing an article on "A Theistic Perspective on the Multiverse Hypothesis" to the Cambridge volume. He's supported by the Center for Science and Culture of the Discovery Institute, a right-wing organization dedicated to promoting "Intelligent Design" research. The Discovery Institute has just started up a new weblog devoted to Intelligent Design called Intelligent Design The Future which has drawn scorn from (among others) Sean Carroll and Jacques Distler. Jacques claims to have fallen off his chair laughing at this posting with its claim that "mainstream physics is now quite comfortable with design in cosmology" and question "Why should inferring design from the evidence of cosmology be scientifically respectable, but inferring design from the evidence of biology be scientifically disreputable?", but again I'm with Lubos that this is not funny. Actually it's scary.

It's becoming increasingly clear that the theory of evolution is under concerted and well-funded attack in the United States by a wide array of religious fanatics and pseudo-scientists, who are doing everything they can to stop the teaching of evolution in US schools and promote the pseudo-science of Intelligent Design. This is a fight that scientists need to join, but the extent to which pseudo-science has already infected mainstream physics and cosmology is becoming dangerous and is going to make it very difficult to effectively answer the Intelligent Designers. Susskind is giving a talk at Brown soon entitled The Cosmic Landscape: String Theory and the Illusion of Intelligent Design. Unless he's gotten even crazier than I would have imagined, I guess he'll be claiming that the string theory landscape/anthropic principle stuff he has been pushing only appears to support Intelligent Design. Behind it all is not an intelligent designer, but a wonderful physical theory called string theory. But the reason Intelligent Design is pseudo-science is that it is a non-predictive framework. It doesn't predict anything, so you can't test it and show that it is wrong. This is exactly the situation that string theory is in these days, and, for the life of me, I have no idea what response physicists can now honestly make to someone who says: "Look, you have a non-predictive framework involving a very complicated and incomplete mathematical structure that you believe for emotional and sociological reasons. I've got a different non-predictive framework tracing everything back to an Intelligent Designer, and I think mine makes more sense than yours."

Comments

1. **Juan R.**
   April 11, 2005

   Modest scientists life:

   Take a problem (normally a problem important from application side); apply the scientific model developing the necessary number of new concepts and mathematical tools and obtain solution. If resolution is bad, begin again or abandon the problem. If it is good, then obtain ?fame? but continue working.

   Self-important scientists life:

   Choose a problem (a problem important for obtaining notoriety); apply the scientific model and obtain an adequate resolution. If this is not possible, choose one of several possibilities:

   – falsify the data forcing the fit with your ?marvellous? theory (e.g. Pons cold fusion).
   – ignore the failure and begin a new theory **named equal for notoriety purposes (e.g. string theory)**
   – take a good idea by other less recognized man (including coworkers and students) and publicly like if were your (e.g. Rutherford).
   – ignore the scientific method and use other methods like religious faith, trivial like antrophic, metaphysical, own ones (developed specifically for the occasion), etc.

   If you are critiqued by ?stupid? scientists, attack fierily them (this is familiar for us here 🙂

   Remember that you are right and other are stupid because you are… **The Best**.

   ****

   Comparison:

   – The first scientific searches true, the other searches notoriety.

   – The honest scientist talk using a firm discourse (of course can evolution), the other modifies his discourse (yesterday I said black, today I say white, tomorrow I say black again) when he needs.

   – The first helps to colleagues, the other puts obstacles to the progress of others.

   – The first is mainly interested in the solving of important society problems (e.g. energy crisis, cancer cure, etc.). The second ignores agony of people and just claim for more funding for studying a ?stupid thing? that only helps to him or herself (by obtaining notoriety).

– The first scientist admit that there is interesting disciplines and other scientists. The second think that just his/her field is important. The rest of science is trivial or only ?engineering?.

Of course, Weinberg, Witten, Hawking, Greene, Schwartz, etc. belong to the group of self-important physicists.

Note: Of course, initially both Weinberg and Witten did some significant progress in science, but now they are more focused on self-importance than in real progress.

Peter, I remember that you wait that Witten recognizes that all string endeavour is a waste of time. This will succeed on one of two possibilities: there is a new theory where he can be selfimportant once more (highly improbable because Witten is old enough for beginning again from zero even if you develop that theory), there are significant experimental data showing his theory is wrong (also highly improbable because current string theory is not falsifiable).

2. **Aaron**
   April 11, 2005

   Why not e-mail Nima and ask? I'm not so up on the phenomenological details.

3. **Jo?o Carlos**
   April 10, 2005

   I don't think medling religious point-of-view with scientific research can lead to any good. The best example I know is Lysenkoism in (now defunct) USSR. You know: atheism **is** a religious point-of-view…

4. **Peter**
   April 10, 2005

   Hi Aaron,

   It's the landscape I've been claiming is inherently unfalsifiable pseudo-science, not split supersymmetry. Sure, I'm willing to believe that split supersymmetry is in principle falsifiable, if you could go to arbitrarily high energies. But the question I can't get an answer to is whether it is falsiable at the LHC, although people seem to claim it makes predictions for the LHC. Sure, if the gluinos are high enough mass you're doing supersplit supersymmetry, which we know is a joke. But, to ask the same question I've been asking in a different form: if split supersymmetry predicts a gluino below a certain mass, what is that mass? You're telling me that if you push the mass too high, coupling constant unification won't work, but you've got a lot of free parameters to play with. At what mass does coupling constant unification conclusively fail? And, assuming the LHC runs for a few years at design luminosity, up to what mass will it see these gluinos? Will its reach be high enough to falsify the idea of split supersymmetry? I'm still not getting an answer to this….

5. **Aaron**
   April 10, 2005

   Peter, you're wrong about split supersymmetry. You may be unhappy with the philosophy — you'd certainly not be the only one — but, in and of itself, it is simply a fine tuned model. It *is* falsifiable. And fairly easily falsifiable, IIRC. I can't quote luminosities for you, but the gluinos cannot be too heavy as they're there to ensure coupling unification. I don't know the mass bounds offhand, but they're there. You either see them or you don't.

6. **Peter**
   April 10, 2005

   Hi JC,

   I think the landscape pseudo-science is a result of string theorists being unwilling to wave the white flag. By any sensible version of the scientific method, once you realize that the speculative hypothesis you've been investigating is, if consistent, unable to predict anything, you're supposed to abandon it and try something else. I find it just shocking that serious physicists are unwilling to acknowledge this, and would prefer to totally trash the subject and turn it into a pseudo-science.

7. **JC**
   April 10, 2005

   Peter,

   Why do you think otherwise legitimate physicists, end up indulging in pseudo-scientific activities like the anthropic principle? Do you think it's the equivalent of them waving a white flag?

   If I didn't know any better, one reason I would think that some physicists would use to justify indulging in pseudo-science, would be that all their personal "pet theories" were a washout and that they're engaging in desperate measures to salvage their life's work. Admitting defeat is a hard thing to do for many folks' egos.

8. **Peter**
   April 10, 2005

   Hi David,

   Well, you now seem to agree that Weinberg doesn't describe his bound as giving a "prediction". He's careful to avoid this misuse of terminology, even if many others with an agenda of passing off pseudo-science as science aren't so careful these days.

   Your example of falsifying split SUSY by finding something it doesn't predict is kind of silly. By the same reasoning I could argue that of course split supersymmetry is falsifiable because it is incompatible with angels with trumpets blaring being produced in a 14 TeV proton-proton collision, so if the LHC sees an angel with a trumpet blaring coming out of the interaction region, then split supersymmetry will be falsified. Clearly this is an abuse of what people generally mean by falsifiability: the theory is supposed to make a definite prediction that something will happpen if you do a certain experiment. The prediction should be definite enough that the theory must be wrong if you do the experiment and don't see the predicted behavior.

   Your example of long-lived gluinos is better. So now, what does split supersymmetry predict for the mass and lifetime of these gluinos? How much integrated luminosity will the experimenters at the LHC have to acquire and analyze so that they can rule out split supersymmetry?

   We still completely disagree about the significance of Weinberg's bound. Again: as far as the CC goes, the statement: "there's a multiverse out there with a random distribution of CCs" is experimentally indistinguishable from the statement "I don't have a clue what determines the CC". You're doing pseudo-science not science when you start going on about this statement giving scientific predictions. You're doing dishonest pseudo-science when you start crowing about "Weinberg's successful prediction" when it's:

1. Not really a prediction.
2. Off by at least an order of magnitude anyway.

There's nothing inherently wrong with people investigating the multiverse hypothesis, if they have an idea about how to get a legitimate scientific prediction out of it. All I've seen so far coming out of such investigations is pseudo-science, without even a plausible idea about how they'll ever get a real prediction. I've specifically asked people doing this kind of work to tell me what they expect to be able to really predict. For a while the answer was the scale of supersymmetry breaking, but recently they have given up on that, and they don't have any other answer.

You may not find very worthy my spending my time complaining that the landscape is pseudo-science. I happen to think it's a lot more worthwhile than trashing the field of theoretical physics by turning it into a pseudo-science, or allowing one's colleagues to successfully do so without raising any objection.

9. **Quantoken**
April 10, 2005

David said: "As for split susy, it's **very simple to falsify**: find squarks or sleptons at LHC. It also provides a distinctive smoking gun in the long lived gluinos."

It's neither simple nor easy. What if nothing is found? It is always much more difficult to prove something does **NOT** exist, **experimentally**, than to prove that something **DOES** exist.

Actually I would say it is **impossible** to prove something does NOT exist. For example there has NOT been a conclusive experiment proving that **ghost** does NOT exist, although no experiment showing it exists either. You can replace the keyword **ghost** with **sghost**, **ghostino**, **sghostino**, or **god**, **sgod**, **sgodino**. Same thhing can be said.

You could well claim that squarks exist at an energy unreachable by LHC, or claim that the cross section of interaction is too small that the LHC has not run for the many million years needed to detect a single event yet. Or the ultimate fudge factor would be the squarks do not interact with the rest of the world in any of the 4 know forces, since it only interacts using sforces. Certainly no one knows what a squark that only interact with sforces would look like.

All the new English words invented by super string theoretists are making me dizzle already.

Quantoken

10. **David**
April 10, 2005

Oops – sorry. Forgot to sign my name to the last post.

11.
April 10, 2005

I think everyone would accept the criticism that Weinberg's work on the cc isn't as predictive as his work in other areas. I'm happy that we're in agreement about this. A clear statement of his view can be found in the 97 paper where he says the calculation "suggests" a value of the cosmological constant at the bound, goes on to explain how this exceeds observational limits by an order of magnitude (this was before the supernovae observations) and then descends into bayesian analysis in an attempt to rectify the situation.

As for split susy, it's very simple to falsify: find squarks or sleptons at LHC. It also provides a distinctive smoking gun in the long lived gluinos.

But these details cloud the main point. We have a big problem with the cc. And Weinberg's calculation remains the only one to give a value in the right ballpark. Either you reject this fact and continue to look for a new mechanism. Or you treat it seriously and study the (potentially unpalatable) consequences. Both approaches are valid, presenting interesting scientific questions that can hopefully be answered with work. And both approaches are more worthy than launching polemics from the sidelines, denouncing anyone who dares consider the latter.

12.
April 10, 2005

"If you guess a theory and then seek money to test it, you are promoting belief"

Yep, you believe in Money.

(Which is a problem by itself. A reason for bankers to contract physicists instead of economists for market calculations, is that the physicist is more able to see money as a number, not as Money)

13.
April 9, 2005

If you guess a theory and then seek money to test it, you are promoting belief, hence you need religious faith. Mathematicians from Pythagoras onward have had faith in numbers, and can easily mislead people into weirdly believing that litmus paper is not a test for real acid:

?I don?t demand that a theory correspond to reality because I don?t know what it is. Reality is not a quality you can test with litmus paper. All I?m concerned with is that the theory should predict the results of measurements.? ? Dr Stephen Hawking in S. Hawking and R. Penrose, The Nature of Space and Time, Princeton University Press, Princeton, 1996, p. 121.

At the same time, this good guy publishes books with graphs admitting that his gamma ray emission rate from black holes is swamped by the background radiation noise and so cannot be confirmed by measurements.

14. **Peter Woit**
April 9, 2005

Hi David,

"Weinberg's prediction (and it is a prediction – you should read his papers)"

You must be a string theorist. They seem to think it is a clever put-down to tell people to read the basic papers of a subject. I've read Weinberg's papers and nowhere have I noticed him claiming a "prediction" of the CC. Maybe I'm wrong, but instead of stupidly telling me to read a paper I've already read, please provide a direct quote from

Weinberg in which he explicitly claims to "predict" the cosmological constant. He knows how to use the word, he uses it in these papers to refer to real predictions: supersymmetric GUT predictions of the weak mixing angle, and predictions of light-element abundances. In his first paper on this, he refers to his result, as I did, as a bound on the CC. In later papers he investigates possible probability distributions of the CC, but I've never seen him claim that the assumption of a flat probability distribution for the CC (whether motivated by the landscape or just the fact that we have no idea) allows him to "predict" the cosmological constant.

"Any complaint that their motivation is distasteful is frankly irrelevant."

I wasn't saying that their motivation was distasteful, I said they can't get a prediction out of the landscape. You should read their paper about this (hep-th/0501082), where they note that their assumptions can lead to the Standard Model, the MSSM, split-supersymmetry, or much else besides (their paper was written before the discovery of super-split supersymmetry, hep-th/0503249, which you could also get from their assumptions). They do claim to have "predictions", but they are seriously abusing the English language when they say this. If you think they have a prediction for the LHC, explain to me exactly what measurements the LHC experimentalists can do to show split supersymmetry is wrong. What integrated luminosity will the machine have to produce to test the Arkani-Hamed/Dimopoulos prediction and show that split supersymmetry is wrong?

15. **David**
April 9, 2005

That's the spirit Peter! Ask questions to see how this anthropic reasoning holds up. What happens if we allow the weak scale to change? What about the magnitude of density fluctuations? Are there some quantities which change and others which are fixed in this framework? Are there rules to this game, or are they arbitrary? Carry on like this and you'll soon be writing papers on the subject. It seems to me that these are interesting questions that may or may not have good answers. But I don't think it's reasonable to attack people for thinking about them.

The phenomenological interest in anthropics is due almost entirely to the cc and our complete failure to understand this in any other way. Weinberg's prediction (and it is a prediction – you should read his papers) is out by a factor of 30. And while everyone would like a beautiful mechanism that fixes the cc, this is the closest we've got and it deserves further attention. So what if the cc is fine tuned? Does this mean the Higgs mass could also be fine tuned? Is our understanding of naturalness wrong? What would be the consequences of this? I think asking these questions is much more worthwhile than simply being rude and shrill and insisting that it's all obviously wrong.

Finally: Arkani-Hamed and Dimopolous' split susy model does what every good model should: it offers a prediction for LHC. If it's not seen then their model is wrong. Very simple. Any complaint that their motivation is distasteful is frankly irrelevant.

16. **Peter Woit**
April 9, 2005

No, funding for particle theory is actually not doing that badly compared to many other things, with increases for FY 2006 in the proposed DOE and NSF budgets (although the DOE increase is negligible and doesn't make up for inflation). In particle physics recently, it's the experimentalists whose funding is seeing sizable decreases. Theory is relatively cheap, cutting that by a few percent doesn't save much, unlike the experimental funding.

17. **JC**
April 9, 2005

Peter,

Are there any ominous signs of the NSF and/or DOE planning to drastically reduce or outright cancelling funding for particle theory or theoretical physics in general?

18. **Peter Woit**
April 9, 2005

Hi David,

If people were actually getting legitimate scientific predictions out of the landscape that would be fine, but they're not. The Weinberg "prediction" is not a theoretical prediction. He's just noting that the observation of galaxies implies a bound on the CC. The so-called "prediction" that the CC is some randomly distributed number below the bound is:

1. Wrong. It seems that it is 10-100 times smaller than the bound, which is statistically unlikely. If you allow other parameters to vary, the bound is much much larger and the "prediction" is completely wrong.

2. Not a real prediction of a physical theory. It's exactly the same prediction I would make by saying "I have absolutely no clue whatsoever about what the physics of the CC is, so all I know is that, because I see galaxies out there, it's some random number below Weinberg's bound". There is no real link here between a physical theory and an experimental observation of the kind one normally means when one uses the word "prediction". The "landscape" is just an absurdly complex model that has exactly the same implications as not knowing a thing about what is going on.

As for Arkani-Hamed/Dimopoulos, I don't believe they have a legitimate prediction either. They make a large number of assumptions, of dubious relation to any underlying theory, then end up with some very vague "scenarios" of what might be seen at the LHC based on those assumptions. A real prediction would be to tell us something that the LHC should see based on the landscape, such that if the LHC didn't see this, the landscape was wrong. They don't have anything like this, neither does anyone else doing this kind of "string phenomenology", and I think anyone who thinks seriously about the implications of these 10^500 or more vacua should quickly realize that no one will ever be able to extract a legitimate scientific prediction from this framework.

While any individual can certainly justify taking Templeton money, I think anyone who cares about science should be worried to see funding decisions being made by people whose agenda is not to promote good science but to promote religion.

19. **Wolfgang**
April 9, 2005

Peter,

you wrote
> And I think it's a terrible idea for
> physicists to get into bed with them [Templeton]

In the past, physicists 'got into bed' with all
kind of supporters with irratinal motives,
e.g. cold-warriors (the DOE is a relic of this time).

In the end the motive is not important, only

the results and they should be scrutinized and
critized according to the scientific method.
It is the responsibilty of the scientific
community to keep the quality standards high.
(And you and your blog are very important contribution
to this effort by the way.)

20. **David**
April 9, 2005

Peter, it looks like there are two seperate issues here:

1) Emminent scientists with proven track records are attempting to think deeply about speculative ideas which appear completely crazy to others. Some, like Weinberg, even had the temerity to make an experimental prediction based on anthropic reasoning. Others, like Arkani-Hamed and Dimopolous, are continuing this ridiculous trend, making assumptions and following the reasoning through to extract experimental predictions from fine-tuning scenarios.

This abandonment of science from some of its leading stars is appalling. People simply shouldn't consider these ideas as it's obvious that they're wrong and won't lead anywhere.

2) Other leading scientists, like Dyson and Vilenkin, are receiving money from templeton, an organisation which clearly has an agenda that many of us disagree with. While there's no suggestion that templeton is dictating the research of these people, it's disgusting that they would accept the money, especially in the current climate where the DOE are showering departments with funds.

I agree that we should try to get serious scientists to sit up and discuss how we can stop this happening in the future. We must act now. Before it's too late.

21. **Quantoken**
April 9, 2005

Peter said: "I don't think anyone should be forcing anyone to work on anything in particular. But when leading figures in a scientific field decide to abandon science … serious scientists should sit up, take note, and start debating why this has happened and what can be done about it"

No one should be forced to accept or reject any ideas. It is essential to protect freedom of mind for science to be healthy. Freedom of mind means any different opinion, no matter how far fetched you think it is, shall be tolerated. Burning Bruno alive definitely is not protecting science.

Ever since I learned Darwin's evolution theory, I was deeply impressed by its simplicity and profoundness. It's not even science, it's pure logic. Survival of the fittest. Isn't it absolutely true logically, that the more fitting you are, the more surviveable you are? It's not even logic, it's linguistics, since what it means some spices are "fitting", is simply defined as that they are more likely to survive the environment.

So, while other scientific theories may need to be tested by experiments. There is not even a need to test the Darwin's theory by experiment or observation, because it is already correct by definition and logic. It's one of the few truely universal truth, as much true as "1+1=2".

You can applying darwinism to almost anything, including to the researchers' community. Scientists are paid scientists merely because they are the result of natural selection rules imposed by the **system**, only those fit the system survives. So if anything is wrong with the science community, we should look for problem in the system, instead of pick on individual person.

I think there are roughly two stages of development of the scientific inquiries. In the early stage, in which I envy, are the era from Newton to Einstein, where scientists' inquiry of science are free from interference by the daily chore of providing for a living and family. These scientists are either from prestigious family background, thus have sufficient resources to support their daily life and they can concentrate their mind on science research, like Newton. Or, in the case if Einstein, he had an alternative day time job and his scientific inquiry is independent from what he did to make a living.

That is the era when science research was much more healthier, because no financial factors play any role to impact scientific thinking in any way.

But that has changed completely, nowadays science is no longer just a curious inquiry of individuals regarding the nature. It has been turned into a huge industry where millions of people jump into it for the purpose of making a living of bread and butter.

Once that happens, science is no longer pure, no longer healthy. That's because at any given moment of time in history, bread and butter is always much more important than freedom of thinking. You can't think if you are hungry, and you can't survive if you do not eat. Simple, right? Put it simple, once your scientific research activity is connected to earning your bread, you are no longer a free thinker, since your research activity will have to earn your bread and allow you to survive in the system.

That is why so many people would blindly follow the main stream ideas. Each individual may believe that he/she had the freedom of thought and he/she independently chooses to trust the main stream, and there was never any pressure to force them to do so. What they don't understand is they are the result of natural selection. The system constantly eliminates people of dissident opinions so only the main stream will dorminate in the field.

What we need to change is minimize the natural selection power of the system. We need to look at the source of the funding. Appropriation of research funds are now dorminated by some powerful figures who has a very strong bias towards some particular ideas, and aginst some others. The more so, the more Darwinism will be in play in the field and more thorough the science will be purified that only the main stream voice remains. And that's dangerous.

The fundings must be appropriate by some one who does NOT have such bias. And it must be diversified in such a way that different ideas have equal opportunity of getting funded and supported. And let the scientific reasoning become the only eliminating factor in deciding what ideas is right and what is wrong, **let NOT the financial factors play a role**. This is the only way to allow science to return to a healthy state.

To this end, I would support and endorse Templeton, as well as other private sources, to donate money to support science research. The matter is, if there is just one Templeton, it's biased. But if there are different Templeton biased towards different directions, it is more likely cancel out and the whole system will have a better chance of being none-biased, due to such diversification.

Quantoken

22. **Peter Woit**
April 9, 2005

I don't think anyone should be forcing anyone to work on anything in particular. But when leading figures in a scientific field decide to abandon science (and are encouraged to do so by funding from a right-wing-financed organization devoted to promoting the interests of religion over those of science), serious scientists should sit up, take note, and start debating why this has happened and what can be done about it.

23. **David**
April 9, 2005

Freeman Dyson is another that has shamefully taken large sums of money from the templeton foundation. And, it appears that Vilenkin has combined his important work on cosmology with more speculative anthropic-like pursuits for years.

There's clearly a disease in the community. I think we have to face up to the fact that many of our leading scientists, despite making enormous contributions in their past, are now unable to decide for themselves which research path to follow. Something should be done about this. Do you think there's some way we can force these people to work on what we want? It's such a waste.

24. **robert**
April 9, 2005

More pedantry, I'm afraid. G.H. Hardy was a socialist, albeit of the etiolated Bloomsbury variety. He sported a picture of Lenin on the walls of his rooms in Trinity College and was, for two years, President of the Association of Scientific Workers.

25. **Carl Brannen**
April 8, 2005

I see that the spirit of G.H. Hardy chimed in here a few days ago, and that now we are talking about religion and physics. As far as I know, Hardy was neither Jewish nor socialist, but he did get a letter from Ramanujan (also neither Jewish nor socialist) with the formula 1+2+3+… = -1/12. Frankly, I'd rather prefer to take money from the Templetons.

Carl

26.
April 8, 2005

?Science is best defined as, in the first place, objective knowledge, plus, in the second place, the activity to enlarge, and make use of, this permanent and universal knowledge. Scientific objectivity stubbornly and irrefutably exists and refers to the permanent and universal knowledge of facts and phenomena of nature which are independent of any individual's whim: political affiliations, ideological persuasions, moral beliefs, and so on….

?The most striking instance of the mix-up between science and ideology, and that which generated the greatest harm is the following: Many elements of so-called 'modern physics' (relativity, quantum) were rejected by the Nazis because many (but by no means all) of their creators happened to be Jews or socialists or both. In this way, the military defeat of Hitler sealed the fate of twentieth century theoretical physics. As Hitler was (morally) wrong, those who criticise modern physics must be (scientifically) wrong, too.? ? Theo Theocharis, 'Science and Society', Wireless World, July 1981, p. 52.

27.
April 8, 2005

That the ID crowd would somehow glom onto the anthropic variants of string cosmology has been a fear of mine for quite some. It's a slippery slope when prominent physicists try to curry favor with the public by "sexing up" their work with references to God and stuff, however oblique. On the bright side, it will be entertaining to see Lubos Motl be sued someday for teaching non-creationist cosmology, under some "academic freedom" law devised by his political buddy David Horowitz.

28. **Quantoken**
April 8, 2005

Mark T.:

It is not news that **scientists ARE driven by the funds** that support them. It's the theory of evolution and darwinism applied to the scientists themselves. survival of the fittest.

The only reason that Templeton supported cosmologists is a small number, is that the Templeton money is a small porportion, comparing with the bigger cake. If say the Templeton money becomes the 99.9% of the whole cake, guess in which camp will you will find most of your cosmological colleagues in, Mark? And would you be able to continue to support your family and do research etc, with no funding? Would you???

And don't assume that as long as you accept Templeton money, without necessarily supporting their view of the world, you are OK. You do not automatically get funded by Templeton as long as you are willing to come forward timidly. There is a **natural selection process** here. They certainly are more inclined to support some one who are more likely endorse their agenda. And who can say that those in the power of controling public money, does not have an agenda, or some sort of bias, of their own, to endorse some particular point of views and suppress some others?

It doesn't matter if you think you can keep yourself clean by remaining scientifically honest, while receiving money from a source that has its agendas. The point is you are constantly being subject to the natural selection rules of the system and only those fit the system will manage to survive.

If you look at a gold fish in a fish tank. You wonder whether it likes or hates its fluffy tail, which makes it so hard for it to move around. But it doesn't matter, it rely on the food put in by the owner to survive. And it's up to the likeness of the owner to decide who shall survive and breed offsprings, and who shall perish. So a gold fish has a fluffy tail only because the one feeding it prefer to see a fluffy tailed gold fish.

That darwinism point of view explains why some of the crackpots, like super string theory, global warming, big bang, etc, could become fashionable and mainstream. Because these point of views obviously appeal to some of the people in power that has their own point of view and agendas.

Quantoken

29. **Arun**
April 8, 2005

In these times, restating the obvious perhaps may be forgiven.

Whatever our faith in science to probe all of Nature, science, in practice, has boundaries, that are set by the limits of our experimental abilities.

In the past, I think the boundaries of science were recognized. Today, we are under the illusion that there are no more boundaries.

Knowledge can be arrived at by any means – by super-Witten mathematics, or Kekule's dream of a benzene ring – but it enters the realm of science only when it becomes experimental. Otherwise, it is, at best, scientific speculation. Such speculation should never be mistaken for the real thing.

The whole enterprise of science was founded on not seeking explanations beyond our ability to experiment or observe, to abandon the unknowable as being outside the realm of science, and to hope that one day our capabilities would grow to be able to address these. Right now, Theories of Everything are outside our experimental reach, and are, at best, scientific speculation.

-Arun

30. **D R Lunsford**
April 8, 2005

"Does God's light guide us or blind us?"

-drl

31. **DMS**
April 8, 2005

Actually, the Templeton Foundation (and variants) as possible significant sources of funding in the future for physics is reminiscent of the situation in economics, where ideological think-tank funding and *visibility* for economics (like supply-side economics) far exceeds the unbiased sources of funding for economics in academia. Or the situation in medicine, where studies often promote the health benefits of their sponsor's products.

It will take only a few physicists, not hard to find in times of spending cuts, to cause great damage. Seems like there already are a few (who should know better) doing this.

Depressing times indeed.

32. **Mark Trodden**
April 8, 2005

I couldn't agree more with Peter's comments about the Templeton Foundation. There is no excuse for scientists taking their money and those who do so threaten the reputation of the subject and elevate Templeton's ideas by association.

I do think it is worth pointing out that the number of cosmologists involved in Templeton funded research is quite small. I wouldn't describe it as an extensive infection. By far the vast majority of my colleagues have never used Templeton money, never attended a Templeton conference, and find the whole thing silly. Nevertheless, Templeton activities are high profile (by design) because the Foundation has managed to get some well-known people to come to their conferences. I agree with you that these people are hurting science, particularly in the current climate.

I also worry that the disastrous federal funding situation in the US right now may tempt more people to consider taking Templeton money. They won't believe in the goals of the Foundation, but it won't matter – they'll be supporting it by association.

33. **Peter**
April 8, 2005

Fixed the bad URL.

34. **Auger**
April 8, 2005

I see there is a intelligent stance taken here about responsibility. Is there any chance for those who visit here to demonstrate what a responsible scientist would be and the view that would be acceptable to yourself Peter, Sean and others who comment here about our universe?

Somehow, I now see any attempts, as a explanation here supported by templeton foundation as a clear and present sanction of irresponsible scientists and theoretics.

I want to be responsible, and want to emulate responsble people

Help!

35. **Quantoken**
April 8, 2005

Steven why do you need to be on the slow side of wisdom? Is patching up a mis-spelled URL label really that hard?
Click here

Quantoken

36. **Matti Pitkanen**
April 8, 2005

I would be very cautious in labelling people as pseudo-scientists just because they are willing to consider the possibility that intentional action in some sense is a part of (even grand) design of the Universe. The undeniable fact is that Universe seems to be tailored for life.

To me the use of fine tuning to deduce "predictions" from a theory otherwise unable to predict anything, is bad science. But why not pose the fine tuning as an additional challenge for the theories of everything? And why not be even more ambitious and ask whether the gap between religious and scientific world views might be filled after all. Perhaps future physics could say something non-trivial about consciousness and about what is behind religious experience?

Biological evolution is accepted as a fact and the mere scale invariance of physical laws suggests evolution in all scales. The challenge would be to understand this evolution as part of properly generalized laws of quantum physics.

It is practical to start something from which is not understood. Quantum jump has remained more or less a complete black box thanks to the silly attitude of theoreticians towards clear thinking without formulas, which they label as "philosophy". It is perhaps not too weird a guess that the anatomy of quantum jump could provide a royal road to a deeper understanding.

For instance, could one imagine that the quantum state representing universe is gradually replaced by a new one in a continual re-creation (you can call it self-organization if re-creation makes you emotional)? This requires a new view about time distinguishing between experienced time having quantum jump as chronon and the geometric time of physicist. Of course, also the notion of time is one of the black boxes, or should I say taboos, of recent day methologically oriented theoretical physics.

Given infinite number of quantum jumps already occurred, a universe containing/possessing advanced intelligence could develop. Universe would be at the same time a theoretician able to even test his/her theories by patiently by jumping around the quantum landscape of states of the Universe, whereas the theoretician predicting and living in a single deterministic universe is strictly speaking not able to test his theory. The more general view about time would also resolve the problem about initial values at the moment of big bang. The problem disappears since the initial values change quantum jump by quantum jump.

Just a proposal, do not kill the messanger or, listen at least the message before doing it;-),

Matti Pitkanen

37. **Stephen**
April 7, 2005

Not Found
The requested URL /www.math.columbia.edu/~woit/blog/archives/000059.html was not found on this server.

Apache/2.0.53 (Unix) mod_perl/1.99_12 Perl/v5.6.1 mod_ssl/2.0.53 OpenSSL/0.9.7d PHP/4.3.10 Server at http://www.math.columbia.edu Port 80

38. **Quantoken**
April 7, 2005

Peter:

It's shocking to me. But never to me. A while ago I posted this comment on Lubos's blog, since I could not believe a famous guy like Leonard Susskind could be so **intelligently challenged**.

Since Sean is reading here, I am still curious to know whether he thinks a 3-torus universe should be Lorentz invariant or not. And why? 🙂

Quantoken

39. **JC**
April 7, 2005

Why do some physicists accept any of these Templeton grants in the first place? Is NSF and/or DOE funding so bad today that theorists are searching for other sources of funding?

40. **Peter Woit**
April 7, 2005

Hi Sean,

I suppose I should have made clear that the Templeton foundation has somewhat different goals than the evangelical anti-evolution right. While looking around the net, I did run into the following for instance

http://www.answersingenesis.org/docs2002/0806templeton.asp

But still, I don't think the Templeton foundation's goal is to promote good science, it is to promote Templeton's own view of religion and science. And I think it's a terrible idea for physicists to get into bed with them.

On the question of the role of the religious right in American politics, Templeton himself has made clear which side he's on:

http://www.newsmax.com/archives/articles/2004/6/26/111715.shtml

41. **Sean**
April 7, 2005

It might seem like splitting hairs, but it's probably worthwhile to distinguish between "people who believe in God and look for evidence in the universe we observe" and "people who deny the basics of evolution and cosmology." For the most part, the Templeton folks are in the former category and the Discovery Institute is in the latter, although of course there is some blending between them. I strongly believe that they are both *wrong*, but I wouldn't classify the Templeton folks as anti-science in the way the Discovery folks are.

(I can't believe I am actually defending the Templeton Foundation, however mildly.)

42. **Peter Woit**
April 7, 2005

Thanks for mentioning this. I did notice this, but forgot to include it in the posting. I'll add it now.

Peter

43. **Simplex**
April 7, 2005

Peter, last year when Smolin posted "Scientific Alternatives to the Anthropic Principle" which made Susskind so mad (and you blogged the ensuing fracas) the original arxiv posting said that Smolin's essay was intended for the book "Universe or Multiverse" to be published by Cambridge. It probably still says that. I dont know any more particulars.

This Evening's Finds in Theoretical Physics

April 10, 2005
Categories: Uncategorized

It's late tonight and I have to prepare a class for tomorrow, so I don't have time now to figure out what is going on here. But if you want to see something really strange, take a look at Susskind's latest, together with the revised version of an earlier paper.

Another new paper this evening is Witten's latest. This looks quite interesting, but definitely will take some serious effort to understand.

Comments

1. **Kyle**
   April 13, 2005

   DR Lunsford,

   At first I thought your link was too over the top to be funny. By the time I reached the meat of the page I found it quite amusing (quotes like "The details of the calculations have been (/are being/will be) presented elsewhere; here we give cartoons." I thought were rather ingenious).

   However, after thinking about it, I've decided it is much too serious to be funny.

   Just somenobodies opinion,

   Kyle

2. **Fyodor**
   April 13, 2005

   I'd just like to clarify that there is nothing wrong with making a mistake, and I too thought the way Susskind retracted was rather funny. But this attitude, that you can do research without reading the literature, that it's ok to write really stupid papers as long as you are prepared to retract the following week, gets my goat. Susskind should remember that despite all his recent vagaries he is still a very influential figure in this field. Subjects like wormholes [in which I've never published, by the way, and I have no intention of doing so] have a somewhat dubious status, and Susskind is propagating the notion that this field is not quite respectable: either you respect his "deep physical intuition" and assume that in some way he will turn out to be right, or you think he is an idiot and that only idiots work on wormholes. Either way, the subject loses. People should never underestimate the importance of these vague feelings about which things are respectable and which aren't.

   Finally, I note that everyone seems to think that Susskind's misadventures are funny, and that nobody should think the less of him because he has written a stupid paper. But nobody laughed when Chapline went off the rails. What's sauce for the goose ought to be sauce for the turkey.

3. **D R Lunsford**
   April 12, 2005

   I have a great sense of humor – this wasn't funny, rather, disgusting.

   THIS is funny:

   http://insti.physics.sunysb.edu/~siegel/parodies/next.html

   -drl

4. **Aaron**
   April 12, 2005

   Grow a sense of humor. It'll help you out a lot in life.

5. **D R Lunsford**
   April 12, 2005

   I suppose it was beyond his Holiness to simply admit "I made a bad mistake"

   Abstract: *In an earlier paper, the author made an elementary blunder, rectified here.*

   0 Overview

   The paper is question is wrong. Ignore it.

   1 References

   (none)

6.
   April 12, 2005

   It must have been humiliating for Susskind. Is that why he refered to himself as "the author of that paper"?

7. **Chris Oakley**
   April 12, 2005

   we can hope that the faint possibility that they are about to make utter fools of themselves will make them hesitate.

   The worrying thing is that they *don't* seem to make fools of themselves when they publish. Except, of course, to the majority of the followers of this web log.

8.
April 12, 2005

Good one Fyodor!

Susskind has obviously crossed into a higher plane of existence in which quaint academic formalities such as citations are obsolete. The obvious next step is for him to stop publishing altogether and transmit his thoughts directly via an EEG connected to a blog.

9. **Fyodor Uckoff**
April 12, 2005

First Chapline, then Susskind….
Surely, *surely* these gentlemen know that there are huge literatures on the subjects into which they have stumbled. And yet, in their vast ignorance and pride, they think that they can dispense with a little background reading, and revolutionize these fields with the help of a little freshman physics.

I would like to suggest a cure. The arxiv should automatically reject any submission with fewer than 20 references, not counting popular books or articles in the National Inquirer. True, this would not force the likes of Chapline and Susskind to undertake the onerous task of finding out what mere mortals have said, but at least it would acquaint them with the sheer volume of previous work. And then we can hope that the faint possibility that they are about to make utter fools of themselves will make them hesitate.

10. **Aaron**
April 11, 2005

Lenny knew about the flux through the cycle, although he didn't use those words to describe it in the original paper. As he says in the followup paper, the result depends on what initial state you prepare the flux through the wormhole in.

11. **Robert**
April 11, 2005

Everybody I talked to reagarding Susskind's paper (V1) agreed that obviously he missed the flux through the non-trivial cycle. So, why bother. This 'discovery' didn't make it into the popular press ("Stanford scientist disproved wormholes") so there was no need to send letters to the editior (as I did for newspapers reporting on Chapline's discoveries).

12. **Aaron**
April 11, 2005

Check out Kapustin's paper in conjunction with Witten's, I think.

13. **garrett**
April 10, 2005

Heh, nothing like an April fools joke on himself to boost his credibility. Bet he's sorry he left the topic of the anthropic principle to try and tackle something interesting.

New Quantum Field Theory Textbook

April 12, 2005
Categories: Book Reviews

I recently ran across a very good new quantum field theory textbook in the bookstore. It's called [Quantum Field Theory: A Modern Perspective] and is by my ex-Columbia colleague [V. Parameswaran Nair], who is now at City College nearby.

The first half of the book covers the sort of standard material about perturbative quantum field theory that appears in pretty much all quantum field theory books, including Peskin and Schroeder's [An Introduction to Quantum Field Theory] which seems to be the most popular one these days. But the second half of Nair's new book very much does live up to his "Modern Perspective" subtitle, containing a wealth of important material that anyone learning quantum field theory should know about, but that has not made it into the standard textbooks until now. This includes a very geometrical approach to gauge fields, anomalies and the index theorem, material on the WZW model and 2d fermion determinants, as well as an introduction to important non-perturbative ideas such as dual superconductivity and the 1/N expansion. Finally, Nair also includes a wonderful final chapter on the ideas behind geometric quantization and their application to the quantization of the Chern-Simons-Witten model.

I highly recommend the book for anyone who wants to seriously learn quantum field theory. Even if you've studied the subject already using a book like Peskin and Schroeder, the additional material in Nair's book makes it well worth reading.

Comments

1. **JC**
   April 17, 2005

   Carl Brannen,

   In terms of drinking the "poison" of string theory, I haven't really found many really good presentations of string theory in book or lecture note form. Though if I had to choose one, I would probably pick Kiritsis's "intro to superstring theory" lecture notes

   http://arxiv.org/abs/hep-th/9709062

   though it seems to be a bit dated these days.

   None of the books like Green/Schwarz/Witten, Polchinski, or Zwiebach seem to be satisfactory for the most part. I mainly learned string theory from reading various lecture notes and original papers, when Green/Schwarz/Witten was not being very clear.

2. **Alejandro**
   April 15, 2005

   Indeed. I never know if to refer the book as CdWD or BMB. Typical problem about married/maiden names. In any case, the underlying point is that for geometry and tensors one must have a index-based book and a index-free one.

   Off topic, some old news about Fomenko:

   http://jcolavito.tripod.com/lostcivilizations/id13.html

3. **D R Lunsford**
   April 14, 2005

   Ugh THAT book…

   -drl

4. **Peter Woit**
   April 14, 2005

   I assume Alejandro means

   Analysis, Manifolds and Physics by Yvonne Choquet-Bruhat, Cecile Dewitt-Morette and Margaret Dillard-Bleick

5. April 14, 2005

   "The book from the three girls"?

6. **JC**
   April 14, 2005

   Over the years I got the sense many field theory books seemed to fit into one of two categories:

   (I) books which are pedagogical and relatively "easy" to read and learn from

   (II) books which are hard to read and difficult to learn from

   Peskin/Schroder, Feynman's QED, Griffith's particle physics, Ryder, Bjorken & Drell, etc … seem to fit into the first category (I).

   Itzykson/Zuber, Zinn-Justin, Weinberg, Faddeev/Slavnov, Berestetskii/Lifshitz/Pitaevskii's QED, etc … seem to fit into the second category (II).

   The second category (II) books seem to be akin to Dirac's quantum mechanics book, which is very nice once one knows the subject but is terrible for a first book to learn from.

7. **Alejandro**

April 14, 2005

Dubrovin-Novikov-Fomenko I like too. I use jointly with the book from the three girls.

8. **h**
April 14, 2005

Is this new book readable for a mathematician? In any case, what book do you recommend for a mathematician (student) who wants to learn QFT?
I don't need the usual mathematical precision, but I do need clear indication of the underlying structures (imho the lack of this is what makes it very hard for math people to read physics literature) and also some sort of "big picture".
I know Weinberg, that's an example of what I cannot read. I also know the double IAS volume "Quantum Fields and Strings: A Course For Mathematicians", that is like chinese… (the target audience of that is very narrow I think)

9.
April 14, 2005

I on the other hand enjoyed Itzykson & Zuber's textbook. It was very clear, easy to read, complete and intuitive. Generally, I like old style textbooks, alot more physics unlike "modern" crap. Combine Itzykson/Zuber with Slavnov/Faddeev's book on gauge field theory + Dubrovin-Novikov-Fomenko for fantastic diff. geom. and you are good to go.

10. **Juan R.**
April 14, 2005

What are the main conflictive points of usual QFT (beyond renormalization of course)?

I used some textbooks like QED by Feynmann and the first two volumes by Weinberg, but I dislike with both.

Feynman is a calculation recipe based in many asumptions and "intuition".

Weinberg's manual is a unsatisfactory attempt to present us an axiomatic view of the field.

11. **Kristjan Kannike**
April 14, 2005

I find Robin Ticciati's "Quantum Field Theory for Mathematicians" a very good book. The title is somewhat misleading. It covers basically the same material as Peskin & Schroeder, but does not shy away from the fine points. In fact, for it being a bit more formal, I found it more clear than Peskin & Schroeder.

12. **Fabien Besnard**
April 14, 2005

I agree Itzykson/Zuber is hard, but it's interesting as a reference because they go into all sorts of details. (Incidentally I attended J.B. Zuber's lectures, and they were way clearer that their book)

What do you think of Ticciati's QFT for mathematicians ? I haven't been through all of it yet but I like its mathematical clarity.

13. **Alejandro**
April 14, 2005

What about Huang? His book on the Standard Model is very readable, and now I have ordered (not received yet) his QFT book, which seems to be a complement.

14. **JC**
April 13, 2005

Years ago I first learned quantum field theory from both T.D. Lee's "particle physics & intro to field theory" book, and Feynman's quantum electrodynamics book. They were not exactly the best books at the time.

Ryder's quantum field theory book seems to be one of those books which looks "deceptively simple". On the surface you think you understand what's going on, but you really don't.

15. **Alan**
April 13, 2005

Coleman's notes are very nice. Look here:
http://my.harvard.edu/icb/icb.do?course=fas-phys253a&pageid=tk.page.phys253a.dir.96baa9f5f565ad359215beb46d7685a9

16. **Carl Brannen**
April 13, 2005

I&Zuber: This really was horrid. Long and hard to understand.

B&Drell: Unfortunately, I lost my copy, which I
miss. But it is dated.

Weinberg: The concentration, at least at first, is on what can be deduced from Lorentz symmetry. I don't like this text. I should admit that I also doubt that Lorentz symmetry is exact, and that the assumption that it is has held physics back. For example:
http://arxiv.org/abs/astro-ph/0311576

Peskin&Schroeder: This is my favorite. It is widely used as a text and for good reasons. My only complaint is the part where they quantize the Dirac equation as bosons.

Zee: I have this, but I don't read it much. My belief is that one should have as many QFT books as possible because different ways of expressing the same theory help one in understanding. If I recall, it starts with a description of field theory from bed springs.

Ramond: I hated it when it was new, and now it's dated.

Brown: This book has some interesting expositions but I can't stand the typography. Dr. Brown is professor emeritus at a local school (UW), and his text is used there.

Ryder: This one I don't have. I guess I will order it and see if I learn anything.

We should put together a list of guage theory books, and for those of us who have tasted the poison, string theory texts.

17. **Alejandro Rivero**
April 13, 2005

DMS, I join in your gasp to "Itzykson and Zuber", the book that devastated one whole generation of physicists (mine). Perhaps the motivation to go (to escape) towards strings? I have also some doubts about Weinberg, because he, after all, is a disbeliever, seeing every QFT as an "effective theory", but no more.

I wished the Bjorken Drell were scanned somewhere, or at least cheaply reprinted. And wonder about these Coleman's QFT lectures.

Besides Schwinger, also some frenchies were into the source theory. A young Kastler, I believe. But they are better at Critical Phenomena. Zinn-Justin and Le Bellac. For Schwinger action plainly, the Dyson 1951 lectures. In the net:
http://hrst.mit.edu/hrs/renormalization/public/documents.htm

18. **D R Lunsford**
April 13, 2005

DMS – I highly recommend the Maggiore book, if for no other reason than the worked problems.

-drl

19. **DMS**
April 13, 2005

I am always interested in learning about QFT, and will pick up a copy of Nair's book. I have not seen Maggiore's book. Gone are the days when the only references were Bjorken and Drell and Itzykson and Zuber (Ugh!).

Three of my favourites on QFT are:

* Weinberg's QFT (of course). It is needlessly too complicated in parts (especially early on in Volume 1), but has excellent, modern discussion of various topics. Plus, it has the classic references, and is very to up-to-date (Weinberg having conferred with the experts on the topics). Even though his third volume on SUSY is not as complete, I found his discussion very well explained (despite his notation).

* Zee's QFT: It has very nice discussion of several topics.

* Siegel's Fields (on arxiv): Although highly idiosyncratic, it is an excellent book that teaches one a lot of things not found in many books. Plus, you cannot beat the price!

But I have also heard that Coleman's QFT lectures at Harvard (not to be confused with his classic Aspects of Symmetry) are exceptionally clear. I am not aware if it available online.

20. **pfedor**
April 13, 2005

Hello. Until now I only lurked your blog (which I find very interesting), but today's topic is of particular interest for me, since it's my ambition to really learn QFT at some point in life (sadly, I didn't manage to do it during my studies). So forgive me a few questions.

How does this book compare with Weinberg's "The quantum theory of fields"? Does it contain material that cannot be found in Weinberg's book? Is it easier? Harder? Which one would you recommend to someone who used to study heavily Peskin & Schroeder but only managed to understand about 2/3 of the material (I remember that most difficult were the parts about anomalies and operator expansion)? Does prof. Parameswaran's book include any discussion of bound states?

With best regards,

Aleksander

21.
April 12, 2005

Real men learn QFT from the collected works of Julian Schwinger.

22. **D R Lunsford**
April 12, 2005

On another level, a new book by Michele Maggiore replaces Sakurai as a beginner's book, and includes solved problems! I would recommend this book to any first-timer.

Maggiore, "A Modern Introduction to QFT" (Oxford)

-drl

23. **Alejandro**
April 12, 2005

There is a nascent new generation of books. Zee, Huang, now Nair. Curious.

24.
April 12, 2005

Off topic, but regular readers of this blog will want to see this exchange, where string theorist Eva Silverstein schools Lubos Motl for his overreliance on straw man arguments.

Dine on the Landscape

April 13, 2005
Categories: Uncategorized

Michael Dine from Santa Cruz was here at Columbia this afternoon to give a talk on "Branches of the Landscape". His talk more or less corresponded to his recent paper with the same title. He's following the philosophy pioneered by Michael Douglas of trying to look at the statistics of KKLT vacuum states, fixing the observed values of the cosmological constant and electro-weak breaking scales. The hope is that the distribution of supersymmetry breaking scales one gets would allow one to in some sense predict what this scale will be.

Dine finds three disconnected "branches" of the landscape, sets of vacua with different properties. The bottom line is that on two of them you have various problems getting something that looks like the real world, but you can do some kinds of counting. But on one of the branches you get lots of states with badly broken supersymmetry and the vast majority of states are in a region where there seems to be no hope to analyze what is going on. You can't even say whether the number of these states is finite or infinite. So, he isn't able to get the sort of prediction he and others were hoping for, but intends to keep working in this area nonetheless, with various ideas of what to try calculating. To me, he didn't seem to have even a glimmer of a hope of ever getting even the vaguest sort of prediction out of any of this.

He did say that the landscape is now the only idea on the table for getting physics out of string theory. Brian Greene was in the audience and somewhat objected to this. Brian's point of view appears to be the more traditional one that people should just try and cook up vacua with as many features as possible close to the Standard Model, and that once they've got such a thing it will have other implications for physics that can be checked. It seems to me that that kind of work has been going on for more than twenty years with no sign of success, but Brian still believes this will ultimately work out. Dine's ideas for the future are converging somewhat with Brian's older point of view. He seems to be giving up somewhat on the idea of counting all vacua in the Landscape, instead thinking about counting vacua satisfying some chosen conditions, e.g. being on one of his three branches. So he may be getting back to the older idea, looking at complicated constructions with some set of conditions imposed on them to make them look like the Standard Model, then hoping to extract something new, perhaps in terms of probability distributions rather than the more specific predictions people used to hope for.

Of course I find this whole thing pretty bizarre, since it's horrifically ugly, and appears to me to have not the slightest hope of success. It's discouraging that I don't see any way of having a rational discussion with the people doing this. They are motivated by a hope that somehow, some way, they will find amidst this complicated mess the Standard Model, in some context that allows them to predict something else. As far as I can tell this is the purest of wishful thinking. They aren't claiming to find anything encouraging, but they are pressing on, and convincing an increasing number of people to join them. One hopes that sooner or later they'll get tired of this and move on to something more promising.

Comments

1. **Eli Rabett**
   April 19, 2005

   The problem with Lubros Motl on climate is he knows neither the data nor the theory but tries to pull himself up by naive postulates based on a set of partial truths that he has been spoon fed by political allies. When he ventures out of his cozy blog to the real world he gets his ears pinned back and responds with vitriol.

   For a reasonable demonstration of his naivete take a look at http://tinyurl.com/77uqq.

   Since this is a high energy physics blog I won't press the point but in general I advise against asking oceanographers about string theory and visa versa. In both cases, it is not what you don't know that kills you, but what you think you know and is dead wrong.

2. **Peter Woit**
   April 19, 2005

   Hi JC,
   No, "overhead" (also known as ICR = Indirect Cost Recovery) is payments built into most grants for "Indirect Costs". The idea is that while the grant is specifically paying for costs to the university directly associated with the grant, there are also other costs the university is paying that indirectly support the grant (i.e. the lights in the building, the library, etc.). So universities typically add a charge for ICR as some specific fraction of the grant amount. The fraction is generally quite large.

   So, if you go out and get a grant that is supposed to pay for $100,000 worth of stuff, you actually have to ask the granting agency for more, say $150,000, to pay $50,000 directly to the university for ICR. Universities love this: this is money they can do anything they want with, and coming in in sizable amounts.

3. **JC**
   April 19, 2005

   Peter,

   What's the exact nature of these "overhead" payments? Are they like some form of matching funds?

4. **Peter**
   April 19, 2005

   Hi JC,

   Most theorists who are funded by grants are funded as part of a group. If the group were to lose funding, they'd typically lose funds to hire one or more post-docs, funds to support graduate students, a big chunk of personal cash ("summer salary"), and their university would lose large "overhead" payments.

   This is clearly extremely undesirable. If the NSF or DOE authorities funding work on the landscape ever realize that they are funding pseudo-science and shut off grants to people doing it, "landscape architecture" will quickly become a rather unpopular subject. This probably won't happen though as long as string theory peer reviewers keep telling the NSF and DOE what geniuses these people are.

5. **JC**
   April 18, 2005

   Peter,

   What normally happens to tenured theory professors who end up losing most or all of their grant funding? The cases I'm familiar with were of tenured experimentalists who lost their grant funding, and subsequently had things happen like their lab space being eventually taken away. Some "deadwood" theorists I can recall, seemed to be mostly guys who just sat around all day drinking coffee or reading a newspaper. (I don't know offhand if any of these "deadwood" theorists lost their grant funding).

If a bunch of tenured folks working on the string landscape (and/or string folks in general) end up losing their funding, do you think they will lose much of their influence in the physics world? (ie. Will the reference letters written by these guys be worth more than the paper they're written on?)

6. **Peter**
April 18, 2005

JC,

I'm curious how this will play out too. I would think a lot of physics departments would have trouble with this, but probably not those whose theory groups are dominated by string theorists. One thing I didn't mention about Dine's talk. He spent a lot of time going on about what a genius Michael Douglas is, along with some of his collaborators. People doing totally loony-tunes landscape stuff will probably end up having recommendation letters from people like Dine saying they are geniuses, and this may get them a job.

Fred,

One problem with your analogy of string theory to QFT or the measurement problem in QM is that those are both subjects for which there is a vast amount of experimental data. Figuring out exactly what is going on with rigorous QFT is hard, but you know you are doing physics since non-rigorous QFT describes the world so well. There's not a smidgen of experimental evidence for string theory, so why should you believe that
its problems can be solved, but for mysterious reasons this is incredibly difficult?

7. **Fred**
April 18, 2005

I don't agree, many theorists are very much against the anthropic principle. In fact those who subscribe to it I would say are in the minority. Of course a few bigshots do believe in it, and they have decent reasons, but its not the last word on the subject.

The Landscape is a generic feature of string theory, but it does not a priori exclude a selection mechanism. People are actively looking for such a thing, even though the problem is *hard*.

But then again many problems in physics are hard and have resisted years of study. Like trying to make mathematical sense out of quantum field theory, or the measurement problem etc etc. It doesnt mean that there isn't a good answer for all of them, its just we haven't found it yet.

8. **Quantoken**
April 18, 2005

Peter:

Well said! Good points made!

I might also add that most people discussing about Lubos's publicationsnever meant any personal attack at all. We LOVE lubos. He made this blog of Peter's so much fun to read because of his participation. It would have been very boring and no fun at all if only one voice can be heard on Peter's blog. Lubos had been outspoken on a lot of things and I like that, although I think that might hurt himself sometimes.

Talk about the peer review process. You do not publish a paper for your personal archival purpose, do you? You publish something to make it available for others to discuss it, comment on it, and criticize it, and pick it apart. Sometimes this leads to evaluation of the person who write papers. It's quite normal. And this peer review process can also be extended to the fact that one writes many papers, or one does not write at all for a while. What's wrong with that?

Quantoken

9. **JC**
April 18, 2005

Peter,

Do you think any untenured folks working on the string landscape stuff will get tenure in the near future (at a research university physics department, that is)?

On the surface it would seem a bit odd for a physics department to be awarding tenure to somebody who's working on an pseudo-scientific anthropic problem. Though awarding tenure for somebody working on anthropic stuff wouldn't be too surprising in another department like philosophy or one that does postmodernism stuff.

10. **Peter Woit**
April 18, 2005

Hi Jean Paul,

Sorry, but I'm not going to remove the comments, partly because I try to err on the side of letting people say what they want, partly because I think the discussion does have some relevance to the subject of Dine's talk.

As I've made clear, I think the whole landscape business is pseudo-science and the fact that it has become the latest fad in particle theory is a complete disgrace and disaster for the field. This disaster is happening because virtually no one in the field is willing to speak up against what is going on. Gross famously complained nearly two years ago at Strings 2003, invoking Churchill's words from the Battle of Britain, but he has been publicly quiet about this recently. Lee Smolin has written a paper about why this is not science, but he's a very polite sort. Lubos is the only string theorist I can think of who is forcefully making the case against this nonsense. I think it's brave of him, although perhaps foolhardy since he doesn't have a permanent job.

The whole issue of what a young, untenured theorist should be working on now is a very important one. If the latest fad is complete pseudo-science, can one survive professionally by arguing strongly against it, especially if you yourself don't have any really good ideas to work on?

As for his career prospects, I wouldn't worry too much. Historically, Harvard junior faculty generally don't end up getting tenure there, but do end up getting a good permanent job elsewhere.

One argument for removing the comments would be that some of them are inappropriate personal criticisms, which in many cases might be a good reason for removal. However, in this case since Lubos has never been shy about vociferously personally criticizing those who disagree with him (if they're not string theorists….), I'm not going to worry too much about his feelings getting hurt by this.

11. April 18, 2005

    Peter — please remove this whole chain of comments on Lubos' academic qualifications from amateur/aspiring scientists like Barry, N et al. It is inappropriate and completely
    off-track from Dine's talk. Poor Michael Dine — I wish his talk had stimulated more interesting comments, but it's very hard to believe in a genuine change of mind of a physicist who used to be one of the outspoken believers in the traditional dogma
    of predictability. I am still one of them, and instead of the landscape, I work on more formal things these days and spend more time in the garden, thinking about the next step.
    Jean-Paul

12. **Barry O'Genesis**
    April 18, 2005

    Well, "Michael" [what's your real name by the way?] you have managed to completely misunderstand my point. Suggest you re-read the part beginning with "I fear that he may be a victim …." I'm pro-Lubos, not anti. Just to spell it out, there are plenty of young string theorists out there putting out papers very regularly, and they are going to be the ones getting tenure. It's true that young profs are sometimes cut some slack at first. But not in string theory. And certainly not at Harvard. I hope by the way that nobody thinks that this discussion is off-topic. On the contrary, the real way to judge the state of a subject is to see what its prominent practitioners are, or in this case aren't, publishing. In that connection I think that all this stuff about the landscape gets more attention than it merits. Look at Vafa's papers over the last year to get an idea of what other people are doing.

13. **Thomas Larsson**
    April 18, 2005

    From here: "My efforts to get a strings course at MIT for next fall seem to be failing. At Harvard, too, it seems that there will also not be a strings course next year. How can this be?"

14. April 17, 2005

    N – your dislike for Lubos' style does NOT justify your cheap shots. Just stay away from his blog. I guess that you are not an academic, and even if you are, judging from your manners, certainly somewhere in the boondocks.
    Whatever your profession is, you are just a little mean and frustrated person, to avoid some French expletives that you deserve.

    Jean-Paul

15. April 17, 2005

    Let me ask you something Mike, are you Lubos' squire?
    You seem to know what Lubos' reaction was, can you please tell us why did he decide to shut down anonymous coments?
    This is funny, I wasn't pretending anything and Lubos' paranoia, which you describe and seem to be aware of, is frightening.
    Maybe he is really busy insulting people, or perhaps he doesnt have anything in mind, the true of the matter is that you are as contemptuous as he is and that's just sad. I dont even know who you are either (Michael Jordan? Michael Schumacher? Michael Jackson? oh god please dont!), and dont really care to be honest. If Lubos were in peace with himself he wouldn't have reacted as he did… 'say no more'

    best,
    N

    ps Sorry Peter, this stops here.

16. **M**
    April 17, 2005

    'I agree we should stop discussing about Lubos, but the landscape is so boring and hopeless …'

    This begs a question: Lubos doesn't embrace the landscape idea, but much of the action seems to be taking place there. Is his recent publication rate a symptom of a more widespread lack of other ideas outside of exploration of the implications of a landscape?

    M

17. **Chris Oakley**
    April 17, 2005

    I thought the tenure system is supposed to encourage academic freedom, but it seems to have exactly the opposite effect precisely because it is given too late and not easily.

    The tenure system is designed to protect useless, burned-out old men from being sidelined by those younger, brighter, and more dynamic than themselves.

18. April 17, 2005

    Perhaps Lubos hasn't published anything because he is too busy insulting on the internet?

19. April 17, 2005

    I thought the tenure system is supposed to encourage academic freedom, but it seems to have exactly the opposite effect precisely because it is given too late and not easily.

20. **Michael**
    April 17, 2005

    Dear N,

It is just too plausible that you are being dishonest. If you were so close to Lubos that you can casually ask him about his work, he'd probably know who you are even if you go by the name of "N". By only pretending this, you cause Lubos the discomfort of wondering who in his immediate environment is so disloyal towards him — transparently the strategy of a foe.

Also, if you had more experience with academia you'd know that your assessment of the situation is simply false. I have known many young professors and 95% of them did not publish in about a year after getting their first job. Tenure is going to be decided based on what is published eventually, even if it took more than a year.

Finally, if you were not a foe of Lubos', why would you drag this discussion into the public? There is absolutely no benefit in doing this, unless you hate the man — which you partially admit.

I don't know who you are, but I dislike your dishonest cowardly attitude. The same is true for that other "Barry" person who's posting here.

Michael

21. April 17, 2005

'This discussion is pathetic.'

Dear Mike,
There was no discussion to begin with, I just pointed out what Lubos reaction was to a simple question : "What are you working in, I havent seen a paper of yours in a year, is this your miraculous year?". The last part was a joke I hoped he would understand considering the amount of 'jokes' he produces per post.
Nobody reacts like that (shuting down anonymous comments) unless something is going on, and as Quantoken pointed out, I wonder what that is. I dont really care if Lubos gets tenure (although it would be very sad for him if not, which is the most likely scenario btw) I dont like his attitude in general but he deserves my respect as anybody else.
With respect to timing, the only time where a phyicisist stops production is 1) the year he/she graduates and gets his/her first postdoc, 2) The year of two applying for jobs and 3) the day he/she dies…and I am not sure about the later 🙂

I agree we should stop discussing about Lubos, but the landscape is so boring and hopeless that I cant find anything there to talk about 😉

best wishes,
N

22. **Michael**
April 17, 2005

Dear "Barry O'Genesis",

if I had jealousy dripping from my nose, I'd hide behind a silly nickname, too.

Best wishes,
Michael

23. **Barry O'Genesis**
April 17, 2005

Don't you worry, he's going to do just fine.

No, he isn't, not at this rate.
I fear that he may be a victim of the widespread superstition that one should only publish if one has done something really important. In reality one should let other people decide whether one's ideas are any good.

24. **Michael**
April 16, 2005

This discussion is pathetic. It is evidence of the fact that few people here have any experience in academia. When someone gets their first faculty job, they tend to publish everything they got right before that, in order to get the job. Then, starting with no work in progress, there are plenty of new responsibilities: teaching, advising, faculty meetings etc. pp. This slows down any project one might take on. In fact, it seems most young professors need at least a year to "get back on track" with research.

You guys are just jealous big weenies, admit it! Don't you worry, he's going to do just fine.

Michael

25. **dyspeptic**
April 16, 2005

Is this blog about theories that are not even wrong, or is it about Lubos Motl?

26. **Quantoken**
April 16, 2005

Some one said: "By the way, I asked Lubos why he didnt publish anything in a year and what kind of projects he had in mind. He always talks about others ideas, I gave him the chance to talk about his. After that, anonymous comments are no longer allowed in his blog."

I am also curious what happen to Lubos. It is nothing unusual that one has not published anything in one year. The guy who proved the Fermat big theorem did not publish in 9 years. And Peter doesn't publish in years also. Frankly I think scientists should spend more time doing REAL researches, and spend less time publishing papers, if you can write just one paper that really means something useful, in your lifetime, that's pretty good. 99.9% of publications on arxiv are rubbishes that only waste time of those people who read them.

But it is odd for some one like Lubos, who needs count of published papers to help him get tenureship. And it's especially odd that merely asking him about this would make him turn off anonymous comments on his blog, some thing he never did before. What's going on?

Maybe he really is NOT that much of a diehard string theorist as he wants us to believe. I feel his religious belief is probably shaking, evident from the fact that he became increasingly skeptical to point of views of other string theory people. I guess he is beginning to realize something is wrong.

He is a smart guy and certainly has an independent mind, evident from the fact that he is able to question and challenge the global warming theory, an establishment crackpot theory that's endorsed by 99% of experts in that field. If he can see something wrong in global warming, he surely is able to see something wrong with his own establishment camp.

He reminds me of Anakin Skywalker, a young and fearless jedi who has fatal weaknesses, and who was turned towards the dark side but ultimately his sould is salvageable.

Quantoken

27. **Thomas Larsson**
April 15, 2005

As a much more appropriate analogy than Hamilton-Jacobi theory, let us compare string theory with aether theory. 110 years ago, aether theory had accomplished great things, such as the unification of electromagnetism with acustics and the confirmed prediction of electromagnetic waves. Moreover, all good mathematics, at least PDEs, is really aether mathematics. It is true that aether theory has problems to explain the Michelson-Morley experiment, but the vanishing aether wind is just one number. In view of the other great successes of aether theory, should we really worry about just this one number, when aether theory does so much else? Why not just leave this one number for future generations who will know more?

One reason to worry about the vanishing aether wind is that it is incompatible with the basic postulates of aether theory: the logic goes
no aether wind => no aether => no aether theory.
The positive cosmological constant has a similar effect. We have all heard Lubos lecture about the S-matrix being the only observable in string theory. Well, if the cosmological constant is positive, the universe looks like de Sitter space, and there cannot be any S-matrix in de Sitter space. So here the logic goes
positive CC => no S-matrix => no string theory.
The same argument applies of course if you replace S-matrix by boundary CFT. No wonder that people do higher acrobatics to avoid this simple conclusion.

To see the problems, look e.g. at Jacques Distler's Supercritical post:

Two types of backgrounds have been proposed:
1. A flat background, with a linearly varying dilaton (varying along a timelike direction).
2. An AdS background.

To me, it sounds like he is really saying:

Two types of values for the cosmological constant have been proposed:
1. Zero.
2. Negative.

Anyone who has not been asleep for that past six years would realize that (2.13 +- 0.1) 10^-3 eV is neither zero nor negative by some 20 sigma.

Or look at Lubos' post Behind the horizon. The idea here is that because the cosmological constant is found to be positive in our universe, we posit by royal fiat that it is negative in an unobservable multiverse in which our universe is merely a bubble, and that everything worth knowning in fact lives on the boundary of this multiverse. Note that physics does not take place neither in our universe nor at the boundary of our universe, but rather at the boundary of the unobserved and unobservable multiverse. For some reason this appears to involve anthropic reasoning as well.

This work is apparently due to Steve Shenker, whom I admire for FQS. However, nowadays it is F rather than S who is doing the right thing by declaring defeat.

28.
April 15, 2005

'[Lubos] (has) made lots of really awesome blog posts. I'm sure the tenure committe will take that into consideration.'

Good, we started with peer review in journals, then we moved to the arxiv and lots of garbage per day, now do we also need to think on appearing in Lubos' blog the get tenure?
I am not against blogs, I do read the posts, but come on where are we gonna end! in friendster with a link to Ed?

By the way, I asked Lubos why he didnt publish anything in a year and what kind of projects he had in mind. He always talks about others ideas, I gave him the chance to talk about his. After that, anonymous comments are no longer allowed in his blog.

Genius…they dont understand us right Lubos? 🙂

best,
N

29. **Quantoken**
April 15, 2005

I agree there is absolutely NOT an analogy between the Hamilton dynamics and string theory. The Hamiltonian description of dynamics and the Newtonian description are completely identical in physics and completely equivalent in mathematics. They describe the same physics, but merely use different mathematical language. Hamilton dynamics is certainly useful even if it predicts nothing more than what Newtonian dynamics already predict, because the particular mathematics form may help our understanding. It's like "5-3=2" and "2+3=5". It's different ways of decribing the same thing, but to a kid he may find it easier to understand addition than subtraction.

And it is **ridiculous** to suggest that without Hamilton form of dynamics, there would be no quantum mechanics. It's a pure random happenedness of human evolution that we come across particular specific mathematical forms to describe nature. Mathematically there can be an infinite number of different but all equivalent ways of describe the same thing in nature. The Schrodinger Picture was invented because we have Hamiltonian dynamics. But we also have the Heisenberg Picture. And even we have not invented Schrodinger or Heisenberg picture, we would have invented some other pictures, use some other mathematics tools, to describe exactly the same thing in nature. As long as quantum effects are discovered experimentally, theory will be developed using whatever mathematics tool is available to describe them.

But super string theory is totally different. It does NOT describe the same world that existing theories are describing. Our world is 3+1 dimention and SS describes a 10 dimentional world. Any competent mathematician will have no problem deriving Newtonian dynamics from Hamiltonian one, or vise versa, but SS theory so far fails to figure out a way to "compactify 6 extra-dimentions" and derive our 4-D world.

There is also no experimental observation that leads to a postulation that leads to SS theory, the way equivalence principle leads to GR. Certainly there is also no theoretical calculation of SS that relates to anything to experimental observation.

So, so far, SS has nothing to do with physics, but is merely a pretty good **mind buggling mind exercise**, that we see has the good potential of **driving a few good mathematicians crazy or go nuts**. Same thing as the 3-torus world exam problem Sean Carroll gave to his students. He gave me zero point because I pointed out to him that it's merely good mind exercise but does not have anything to do with physics or GR, since 3-torus world does not describe our own universe.

Quantoken

30. **D R Lunsford**
April 15, 2005

String theory is being set up as a fundamental theory of nature, that matter "really is" made from umpteen-dimensional hair scrunchies. It should be obvious that there is no analogy. There *is* a direct analogy with Ptolemaic epicycles, or the elastic ether, or even say the Bohr-Sommerfeld atom (this gives too much credit to ST). It's easy to make outlandish models and push them to infinity. It's hard to have insights that lead to real physical progress.

-drl

31. **Fyodor**
April 15, 2005

Hamilton's reformulation of dynamics was not set up as a fundamental theory of nature.

Exactly — and yet it eventually led to great new discoveries. If the mentality at that time were similar to today's, somebody would immediately have suggested, "hey, let's complexify the Hamilton-Jacobi function and see where that goes!" And then I can easily imagine Maxwell coming up with the Schrodinger equation. And I can also imagine somebody in 1870 saying, "What a load of rubbish! You'd need an accelerator at least three feet long to check that!" But if they had pushed ahead, who knows, we might have had the Oprah show beamed into every home in 1914, and ghetto blasters in 1921. The possibilities are endless.

And it fact it does have observational consequences – geometric optics would have been impossible to discover from F=ma.

I hope you don't mean to say that Hamilton's work is only interesting from that point of view.

Look, jokes aside, all I am saying is that it took close to a century to get from Hamilton to Schrodinger. I don't see why string theory has to publish or perish so fast, particularly when there are no serious alternatives. If string theorists just come up with a new angle on general relativity from a quantum point of view, well, Hamilton did no more for Newton. And anyone who proposes to put a stop to string research had better have a damn good alternative on offer. Finally, the best way to prove that the landscape is crap is to work on it.

32. **D R Lunsford**
April 14, 2005

Well F, this is a non-point – sophistry. Hamilton's reformulation of dynamics was not set up as a fundamental theory of nature. And it fact it does have observational consequences – geometric optics would have been impossible to discover from F=ma. So what you've inadvertently done is to illustrate how real physics works with both reality and mathematical models.

-drl

33. **Arun**
April 14, 2005

Fyodor neglects the fact that String Theory does not have in its predictions any of the physics that we already know.

34. **Fyodor**
April 14, 2005

THE SCENE: Dublin, 1834
DRAMATIS PERSONAE : William Rowan Hamilton, natural philosopher, and Padraig Woight, a Peasant.

SIR WILLIAM: Behold, I have created a new form of dynamics, whereby all the motions of the cosmos are subsumed into one single function, determined by the following extremely simple relations…..

PADRAIG WOIGHT: Yirrah, BUT WHAT DOES IT PREDICT THAT GOOD OLD ISAAC DIDN'T KNOW ALREADY?????

SIR WILLIAM: Have patience, my boy. Meanwhile, let me borrow yonder trowel so I can carve this onto ye bridge here……

PADRAIG WOIGHT: Arrah, BUT IF IT DOESN'T PREDICT ANYTHING THEN IT ISN'T SCIENCE YOU KNOW!!!

SIR WILLIAM: Tell that to Lagrange……

35.
April 14, 2005

It's too serious to be funny. But most of "scientific" publications nowdays are not much better than computer generated random gibberish. – Quantoken

Take a look at http://www.mathematik.uni-muenchen.de/~bohmmech/BohmHome/sokalhoax.html

http://members.lycos.co.uk/nigelbryancook/

36. **Alejandro**
April 14, 2005

"Let's see if I've got this right: they're looking for SM properties amongst the vacua, like a three-leafed clover, and completely ignoring the wonderful work of Connes, Marcolli, Kreimer et al on the SM?"

It is because this work does not allow enough lateral publications. Even the experts on the field take a long time between paper and paper, and most of them include a review of the previous work.

I lived both the presentations of S T dualities by Witten (at Paris IAMP) and Renormalisation Trees by Connes (at Vietri), and the reception was clearly different. The excitation in Witten was not about the theoretical exposition, but about how many papers could be generated along his lines.

By the way, the Connes-Marcolli-Kreimer (and Moscovici, and Brouder) line is about renormalization theory in a general way, not only standard model. The line about the standard model is the one starting from Connes-Lott (and Coquereaux et al.) and taking seriously Weyl fermions as carriers of geometrical meaning. Both lines could be married in a unforeseen future.

37. **Quantoken**
April 14, 2005

Who said: "Did you notice that Lubos has not published a paper in 1 year!"

No paper? No problem! You can easily generate one paper with just a few mouse clicks, and it even gets accepted to a conference. No kidding:

http://www.pdos.lcs.mit.edu/scigen/

It's too serious to be funny. But most of "scientific" publications nowdays are not much better than computer generated random gibberish.

Quantoken

38. **Quantum_Ranger**
April 14, 2005

When is a Vacuum a State?..according to Brian Greene:When it can be described by a Non-Vacuum 'Dual' Solution.

What string theorists seem to be creating, is Two-Half holes out of one hole. You can Take a single brick from a completed or un-completed wall, thing is if one removes the brick prior to completion , you can classify the wall as being incomplete. Likewise if one removes the brick after completion one can never classify it as being complete!

If one imagines that the FIRST-FOUNDATION brick replaces a Vacuum/hole, the very last brick laid must not be constructed out of material/vacuum identical to the very first brick, it must be comparable to the W-HOLE wall.

So one arrives at:When is a brick a brick, and a hole a hole!

39.
April 13, 2005

"Did you notice that Lubos has not published a paper in 1 year!"

But he's made lots of really awesome blog posts. I'm sure the tenure committe will take that into consideration.

40.
April 13, 2005

Somewhere—I think in *Disturbing the Universe*—Freeman Dyson described Einstein as the savior of physics in the early years of the 20th century. Hyperbolic as this characterization may have been, I find myself thinking that we truly need another such person now—a synthetic intellect who can arrive at a radically simple, unifying perspective on all this, and has the technical chops and rhetorical gifts to make people pay attention and think hard about it. Sounds silly, I know.

For the time being sheer technical chops has outrun genuine* insight. We have lots of interesting ideas that lend themselves to mathematical development, but don't seem to get at the heart of the matter. Instead we hope that by exploring the mathematical landscape we'll stumble on something that somehow just works.

—-
(* I mean something distinct from, albeit akin to, mathematical insight—something like a new and fruitful metaphysical vision. I don't think physical insight, as most people now understand it, is up to the task.)

[*"…one should always know what the result of a calculation should look like before attempting it…"*]

41.
April 13, 2005

Hey Peter, I am sure you have probably realized by now but I'd like to point it out anyway. Did you notice that Lubos has not published a paper in 1 year! One day like today but of 2004….
Either this is his miraculous year or really the string comunity does not have anything promesing to do 🙂
I know this is unrelated to the landscape, or perhaps not….
Anyway, I like Ed's attitude, he got his fields medal, let's keep doing math 🙂

best regards,
N

42. **D R Lunsford**
April 13, 2005

JC – two words:

"Best Buy"

-drl

43. **JC**
April 13, 2005

Any guesses as the what the anthropic string folks will be doing if the entire landscape thing ends up as a total failure, and they actually give up?

44. **Quantoken**
April 13, 2005

Peter said:

“It’s discouraging that I don’t see any way of having a rational discussion with the people doing this. They are motivated by a hope that somehow, some way, they will find amidst this complicated mess the Standard Model, in some context that allows them to predict something else.”

Why, peter? This is their jobs, remember? What can they do if they do not do research on super string theory? Where do they get **funding** if they work on SOMETHING ELSE?

And don’t worry about they getting tired or bored of trying, as long as there continue to be funding for such research. You eat three meals a day and always seem to be eating the same food, do you ever get tired eating? No, you could never be tired of food. So scientists also will never be tired of trying certain ideas, as long as that particular idea continue to bring them funding and something to eat.

Quantoken

45.
April 13, 2005

Let’s see if I’ve got this right: they’re looking for SM properties amongst the vacua, like a three-leafed clover, and completely ignoring the wonderful work of Connes, Marcolli, Kreimer et al on the SM?

46.
April 13, 2005

Simple, the Intelligent Designer chose this particular vacuum, because he liked it.

47.
April 13, 2005

But even if they do find a vacuum which gives rise to the Standard Model — and given the astronomical number of vacua, it may even be likely — don’t they still have to explain why that particular vacuum was chosen?

Conferences Not To Go To

April 18, 2005
Categories: Uncategorized

This week in Santa Fe there's the International Conference on Science and Consciousness, where Michio Kaku will be giving a keynote address. He'll explain how "Many physicists today believe in the multiverse, i.e. Genesis is constantly taking place in a timeless ocean of Nirvana, creating Big Bangs even as you read this sentence" and will tell about experiments to confirm the multiverse theory. He's also running a workshop at the conference on "Visualizing Higher Dimensions" in which you can learn about how to capture different planes of existence (connected by wormholes) in simple pictures. His fellow speakers include Gary Schwartz, Ph.D. who will explain how new experiments involving deceased parapsychologists and Princess Diana provide evidence for life after death, Steven Greer, M.D. who "has taken teams around the world to make contact with Extraterrestrial Lifeforms", and a host of others. Kaku is also interviewed in this week's New Scientist, where he explains that the Standard Model is "supremely ugly" and string theory is "gorgeous".

This fall the Metanexus Institute, which is somehow part of the Templeton Foundation will be organizing a symposium honoring Charles Townes called Amazing Light: Visions for Discovery at which the Templeton Foundation will be announcing a "multi-million dollar, multi-year effort to catalyze research and dialogue at the boundaries of physics and cosmology" called Foundational Questions in Physics and Cosmology. Not clear exactly what this will be funding, but if you check the Templeton website you'll find that "we do not support what might be called standard or mainstream science research", so at least it won't be any of that. In case you're having trouble keeping them straight, this is real, this is a joke.

If you're wondering how Templeton has convinced 18 Nobel Prize winners to attend, Sean Carroll has a very interesting posting explaining how he decided to pass up the \$8000 + expenses he could have made by speaking at this conference. Also if you're wondering why Templeton gave Townes a \$1.4 million prize this year, you can read his remarks upon accepting it, where he explains that "Increasingly, science is showing how special our universe and we are, which has raised questions about whether it was indeed planned or influenced."

In other news, Susskind seems to have ruined his chances at the \$1.4 million today. In his talk at Brown, according to Daniel Doro Ferrante he "repudiated any connections with Intelligent Design".

Comments

1. **Juan R.**
   April 29, 2005

   Two comments,

   I would say canonical gravitodynamics for no confusion with canonical quantum gravity that is other thing.

   The concept of curvature is ambiguous in application of GR. Please remember that newtonian gravity is a consistent theory of gravity working on flat space.

   Remember also standard conformal transformations on cosmology. Perhaps the concept of curvature of GR is just a calculation procedure valid in some situations, somewhat like one can obtain a flat Mk -type metric for the universe from conformal transformations. This last discussion is still speculative. I continue to research.

2. **Juan R.**
   April 29, 2005

   Thanks drl

   Of course, perhaps I am wrong, but it is highly improbable. Only a "cosmic fluctuation" could do that I obtain the correct values for those relativistic values more the correct Newtonian limit from canonical gravity.

   You would be highly skeptical of my work, i understand that, but please be also of standard well-accepted theories.

   For example, recently it has been demonstrated (Phys Rev E) that usual LW potentials of electrodynamics does not verify Maxwell equations and therefore all computation based in LW are Maxwell violating ones!

   Curiosly canonical electrodynamics predicts that failure. Another coincidence?

   A crucial question for you.

   Who said that there is no tensors on canonical gravity?

   However, "complete" tensor calculus is not necesary for both light deflection and perihelion.

   In fact, the derived standard GR formulas for computing light deflection and perihelion anomaly are non tensorial, are scalars ones based in scalar magnitudes like M, R, e, G, and p.

3. **D R Lunsford**
   April 28, 2005

   The value of the light deflection and the sign and value of the perihelion precession force one to make a tensor theory, and curvature is inevitable. There is no way out of this.

   -drl

4. **Juan R.**
   April 28, 2005

   If this is of interest here, I have finisihed my research on GR.

   Effectively, I can derive well-know experimental phenomena from a new theory of gravitation on flat space.

   Next, some of experimental data explained from canonical gravity:

– Mercury anomalous perihelion.
– Light deflection.
– Radar time delay
– Redshifts

Moreover, the theory corrects the conceptual and thecnical problems of Einstein GR and can be quantized!!

More details will be shown on the non-technical paper (on strings) quoted below. Technical details will be shown in papers.

Thanks

5. **Juan R.**
   April 25, 2005

   Tom

   Thanks by your reply but I thik that are misunderstanding the point.

   My criticism is not conceptual, string theory formulas do not work. That will be shown in papers.

   I am not talking about toy models (e.g. cosmological 4D branes, idealized models for blackc holes, etc.) I am saiyng that complete string theory framework is wrong.

   I am not talking about fine-tunning of a almost good theory I am talking about a bad TOE. string theory is a waste of time. They will not work. It is not a problem for adjust one or two parameters or add a new correction to usual equations. All the framework is outdated!

   Moreover, when string theorists talked about Calaby-Yau they really thought that was the geometry of universe. When Scwartz talks about unitary he is not talking about a toy model. He is talking about string theory.

   "1) no-one has written down a Lagrangian for M-theory"

   Of course, it does not exist. Even I doubt that exists a hamiltonian for M-theory!

   "2) compactification has to be done by hand
   But they believe that the flaws/holes can be fixed."

   If compactification is done by hand then the derivation of GR from string theory is just the modification of string theory for adequately it for obtaining the correct reply (known before string theory): GR. This is not a **derivation** for a theoretical scientist I am.

   "This just shows that only referring to popular scientific books is not good enough."

   The ideas below my paper is not referring to popular literature are refering to string literature.

   "Yes, I still say that no string theorist says that they have the right theory."

   I'm sorry to say this but your statement is not honest and contrasts with own Greene statement opening my "paper": "String theory continues to show ever increasing signs of being the correct approach to understanding nature at its most fundamental level."

   I show that string theory is not fundamental.

   Yes it's like a photographer. One taking pictures of a dog and attemp to convince you that it is a car!! 🙂

   It is not true that basic concepts like extended objects and extra dimensions remain in all string literature. It is not fine-tuning of almost good theory, it is a disaster.

   In fact, some people claim that correct theory may based in poitlike particles (graviton, string are derived like aproximated concepts). Others claim that there is no extradimensions, and we are mising 4D versions of string theory!!

   "Again, misinterpretation. GR will still be a very very good model of gravity and very helpful in studying large scale behaviour of the universe. Even if there are some new experiments contradicting GR, they would be very small correction to GR. And actually everyone believes that GR will break down at small scales."

   No!! you are completely wrong!

   I am not talking about Planck scale modification of GR or similar. I am talking about the complete failure of GR like a theory of gravitation even in solar or cosmological scales. I am not talking about fine tuning of GR (that is an action with GR more corrections terms). I am talking about the failure of the concept of GR. E.g. the idea of that gravitation is not delayed, the idea of there is not gravitational fields, the idea of that curvature is not gravitation, the idea of Lorentz forces do not exist, etc.

   Almost all of this has been proved and now I am working in experimental tests. This is fascinating!!

6. **Juan R.**
   April 25, 2005

   Target does not work!

   Renormalisation, and the log jam in modern quantum field theory

   Canonical science project

7. **Juan R.**
   April 25, 2005

   Chris thanks by your comment on code. I used style code for the post but forgot to use it for the link itself.

   I?m sorry.

   ?TOM?, this demonstrates that I am not so intelligent as you believed 🙂

Suggestion: to use the attribute target=_blank, in the link for opening a new browser window. I do not check it still in this blog but I am checking now.

Renormalisation, and the log jam in modern quantum field theory

Canonical science project

An error (another? This guy…). The lower limit recently estimated for the speed of gravitational interactions is not I wrote. It is of the order 10^10 c. Since Brian Greene, as the rest of CST (crackpot-string-theorists), says us in his ?Elegant? that gravitational interactions between Sun and Earth travel at c I wonder how anyone with so incorrect understanding of physical reality can be so arrogant.

8.
April 22, 2005

What consciousness conference would be complete without… JackTheQuack

9. **Quantoken**
April 22, 2005

Some said: "Gavin Esler talks to Michio Kaku, one of the world's leading experts in theoretical physics, about the prospect that **the world will end in a 'Big Freeze'** and the possibility of organizing an **'exit strategy'** from planet earth."

Wow, I never thought about that:-) So what are their proposed salvation plan for the earth?

Maybe they should attach the ends of **some strings** to **galaxies** to prevent them from **flying away**. No, not the 25 pounds cheap fishing strings bought from Wal-Mart. That breaks too easily.

What we need to tire galaxies together is the **SUPER STRING**, something that is safe, **permanently safe**, so it could never fail and never break. How could it ever break if there are **10^500** of them? 🙂

Super String Saves the World. WOW! They probably should put that in their research funding proposals 🙂

Quantoken

10. **Tom**
April 22, 2005

Dear Juan,

>>> "it just shows a complete misunderstanding of what is really going on." It is easy talk, please (if you desire) demonstrate your words quoting real papers.

I dont need to quote papers. I only need to put forward arguments.

As I said you often make factually correct statements, but your interpretation of them in relation to every thing else is not correct. You constantly make subtle to moderate misinterpretations, which then lead to a complete misunderstanding of the area. AGAIN, you need to do physics formula by formula for a few years, just looking at the conceptual arguments is not enough. Remember the pope! 🙂

For example, in your non-technical paper, you say that there are too many models etc. Yes, but they are just toy models, and not realistic models. As I said "string theory" is more of a research program, where often theorists are creating simpler versions of the realistic model they really like to solve and play around with it. You often misinterpret them as the real models.

> I do not find "String theory is full of flaws" on my copies of CERN seminars, talks, papers, magazine reports (e.g. above quoted by Peter) or books (e.g. The elegant Universe)

Every "string theorist" would agree e.g. that
1) no-one has written down a Lagrangian for M-theory
2) compactification has to be done by hand
But they believe that the flaws/holes can be fixed.

And, for that matter, I had lunch with Brian Greene (Elegant Universe) in Erice five years ago and I proofread the German version of his book for him. He readily admitted that there are several open issues but that they could be fixed… And so did Michael Green and Mike Duff.

This just shows that only referring to popular scientific books is not good enough.

> If you state that no string theorist would claim to have the right theory is that you have not read string literature.

Yes, I still say that no string theorist says that they have the right theory. But they claim that they can make the string idea into a workable theory for unification.

>>> But even your words sound like "don't worry if 90% of string garbage is wrong because you can call string theory to everything you do." Also canonical science? Everything will be called string theory?

No, it's like a photographer. You take hundreds of pictures and take the one that fits. Same here, theorists are playing around to find the right models. But a subset (the string theorists) is led by concepts like extended objects and extra dimensions.

>>>Of course, there is NO unification even if GR in its actual status were correct. Recent experiments (by real people working in real 4D word and publishing in real journals not in ArXiv) suggest that gravitation velocity is bounded by 10^8 c invalidating "archaic" Einstein's thinking.

Again, misinterpretation. GR will still be a very very good model of gravity and very helpful in studying large scale behaviour of the universe. Even if there are some new experiments contradicting GR, they would be very small correction to GR. And actually everyone believes that GR will break down at small scales.

Tom

11.
April 22, 2005

Michio Kaku today on BBC World:

19:30 HARDtalk Extra (r)
Michio Kaku
Gavin Esler talks to Michio Kaku, one of the world's leading experts in theoretical physics, about the prospect that the world will end in a 'Big Freeze' and the possibility of organizing an 'exit strategy' from planet earth.

12. **Juan R.**
April 22, 2005

To all "blogers"

Do you see some strong failure on my ideas? Of course, I am sure of that will do errors, but it is difficult for a scientist see his/her own errors. Help?

To ""

"Hmm in page 12 a strange kink to chemistry is done. Alchemy somewhere around?"

Please define Alchemy.

My appeal to chemistry on page 12 means that if S. Weinberg has no idea of the implication of his own field of QWD for chemistry, how can he have idea of other fields of science that he newer studied? The problem with Weinberg is double:

– 1) He needs demonstrate that exists the TOE and we are close to it. Therefore he needs say that all of chemistry is already known.

– 2) He talk in popular media. I have no problem if Weinberg says barbage on Phys. Rew or in his own specialized manual on QFT because readers are scientist and can verify he says. THE PROBLEM is that Weimberg says garbage on popular media. So many policy makers retire funding to chemistry because Weinberg says thah all is already known? So many young students are leaving the field for more glamorous fields like string theory thanks to him?

Of course Weimberg idea of all of chemistry is studied with electrostatic interactions more QM is a complete garbage. Theoretical chemists say that in their papers and reviews but no in popular media.

Dear "TOM"

I am perplexed by your "high-quality" reply.

"I doubt anyone will endorse your "paper"."

First, it is not a paper, it is a nontechnical work. In fact I submited it to pop-phys category 🙂 Don't worry if it is not endorsed. He is just a popular magazine work.

Tecnical work cannot be contained on a single paper and will be published elsewhere. Around 20-30 papers * 25 pages per one = —-. I leave you this difficult computation. Note: can use renormalization tricks.

"it just shows a complete misunderstanding of what is really going on."

It is easy talk, please (if you desire) demonstrate your words quoting real papers.

You say: "String theory is not yet a scientific theory as such (and as a consequence full of flaws) but a research program led by ideas like extended objects like strings instead of particles, or extra dimensions, etc."

I do not find "String theory is full of flaws" on my copies of CERN seminars, talks, papers, magazine reports (e.g. above quoted by Peter) or books (e.g. The elegant Universe)

But even your words sound like "don't worry if 90% of string garbage is wrong because you can call string theory to everything you do." Also canonical science? Everything will be called string theory?

The idea of extradimensions is wrong because it arises directly from consistency (taquions, etc.) of basic string equation. Since basic string equation is wrong, (even is wrong the idea of a classical manifold R4, Calabi-Yau, G2, etc.) the extradimensions are not here!!

The idea of extended object is good but "like string" is also wrong. In fact, it is wrong the idea of branes including a theory of **pointlike** particles on 11D like Banks suggests.

I think (sincerely) either you have a distorted idea of real status of string theory or you are doing joke.

If you state that no string theorist would claim to have the right theory is that you have not read string literature.

As said, the idea of that "string theory" is the way forward to unifying QFT and gravity may be a joke. A joke during more than 30 years!!

Of course, there is NO unification even if GR in its actual status were correct. Recent experiments (by real people working in real 4D word and publishing in real journals not in ArXiv) suggest that gravitation velocity is bounded by 10^8 c invalidating "archaic" Einstein's thinking.

I continue working durely in this fascinating topic. Yesterday, I discovered that one well-known equation used by experimentalists for extragalactic dynamics (they do NOT use standard GR because don't work) can be derived like a special case of the canonical gravitation. Whow!!

I don't think that I have quantized gravitation satisfactory, but the distance between real equations I derive, which are used in real experiments, and the garbage, conjetures, bad mathematics, etc. used by "string theorists" and without link with nothing (even there is no real link with GR) looks your appeal to Pope and sex.

I agree with your three last points.

13.
April 22, 2005

Kaku waxes poetic because he is following his passion and he lives for the glimpses of beauty that his researches allow him to behold.

You mean he’s gay, too… ?

14. **Alejandro**
April 22, 2005

The paragraph below defies my understanding of English. It seems than on one side it is critiquing people who effortlessly uses others’ work, but on other side it is welcoming the possibility of effortlessly using other’s work, from Aliens or even from G-d Himself. Is it?

Funny that the same people that put down the efforts of others without themselves producing any significant work, are also the people so afraid of any challenges to their little materialist bubble that all they can do is sneer and scoff when mention is made of things like aliens, homeopathy, prayer, consciousness, etc.

15. **D R Lunsford**
April 22, 2005

How’s the dihydrogen oxide escribio:

Kaku waxes poetic because he is following his passion and he lives for the glimpses of beauty that his researches allow him to behold.

It’s not “poetic”, it’s self-indulgent crap designed to pump up his own inflated ego – he’s in denial because he doesn’t get it. Dirac is poetic.

On this blog, instead, the talk is all about putting others down, denigrating ideas that are daring and challenging, being petty. Little minds that dont seem to be finding much beauty or inspiration splurting sour grape juice at those who are.

We’re venting, because we’re pissed off to see the thing we love debased by these charlatans.

You dont like string theory? You think it sucks? Well, lets see your theory that does better.

http://cdsweb.cern.ch/search.py?recid=688763&ln=en

You think Steven Greer is a loony? Maybe he is, but have you seen the Disclosure Project material, testimony by dozens of retired military and intelligence officers, FAA officials, and military and civilian aviation experts and pilots, all willing to take an oath before congressional hearings as to their knowledge of the extraterrestrial presence and our government’s cover-up of their involvement in it? Have you heard of or read the COMETA report put out in France in 1999 by a panel of high ranking military and scientists concerning UFOs and the implications of a possible extraterrestrial presence?

Speaks for itself.

Funny that the same people that put down the efforts of others without themselves producing any significant work, are also the people so afraid of any challenges to their little materialist bubble that all they can do is sneer and scoff when mention is made of things like aliens, homeopathy, prayer, consciousness, etc.

Palliatives for the frightened mole.

Yes, there is more in heaven and earth than is dreamt of in any of our philosophies. I suppose the great divide is between those who recognize this and are actually excited by it, and those who react to this idea with fear.

The alien-mongers and water-talkers are the ones who are afraid.

I for one find the possible confluence of this new “Landscape” thingy, many worlds quantum interpretations, and the age old recognition of the existence of other dimensions and other consciousnesses found in many indigenous religions and philosophies, very exciting.

What’s the moment of inertia of thin disc about its symmetry axis? I thought so.

I’d like to read more here about what others find exciting, not just what they think sucks and are afraid of because it threatens their world-view.

I’d like to see my science get back its self-respect, and for the sensationalist carnival barkers to STFU.

-drl

16. **Como el Agua**
April 22, 2005

Reading the essay by Michio Kaku that is linked to from this latest blog entry, I was struck by the contrast between his writing and the kind of writing found here.

Kaku waxes poetic because he is following his passion and he lives for the glimpses of beauty that his researches allow him to behold.

On this blog, instead, the talk is all about putting others down, denigrating ideas that are daring and challenging, being petty. Little minds that dont seem to be finding much beauty or inspiration splurting sour grape juice at those who are.

You dont like string theory? You think it sucks? Well, lets see your theory that does better.

You think Steven Greer is a loony? Maybe he is, but have you seen the Disclosure Project material, testimony by dozens of retired military and intelligence officers, FAA officials, and military and civilian aviation experts and pilots, all willing to take an oath before congressional hearings as to their knowledge of the extraterrestrial presence and our government’s cover-up of their involvement in it? Have you heard of or read the COMETA report put out in France in 1999 by a panel of high ranking military and scientists concerning UFOs and the implications of a possible extraterrestrial presence?

Funny that the same people that put down the efforts of others without themselves producing any significant work, are also the people so afraid of any challenges to their little materialist bubble that all they can do is sneer and scoff when mention is made of things like aliens, homeopathy, prayer, consciousness, etc.

Yes, there is more in heaven and earth than is dreamt of in any of our philosophies. I suppose the great divide is between those who recognize this and are actually excited by it, and those who react to this idea with fear.

I for one find the possible confluence of this new “Landscape” thingy, many worlds quantum interpretations, and the age old recognition of the existence of other dimensions and other consciousnesses found in many indigenous religions and philosophies, very exciting.

I’d like to read more here about what others find exciting, not just what they think sucks and are afraid of because it threatens their world-view.

17. **L.E.J. Brouwer**
    April 21, 2005

    I must say I was pleasantly surprised with the delightful topics being presented at this conference until I stumbled across the photographs displaying the dreadful physical condition of the contributers.

    I have now surmised that they are all meat eating algebraists of the worst degree, unworthy, of further consideration.

    Tow,

    L.E.J.

18. **Torbjorn Larsson**
    April 21, 2005

    Tom:
    Your clarification is clear. However, I think responsiveness is redundant in your list.

    “You think that my examples are useless, because you think that consciousness is only awareness and self-identity, which indeed can be at least in theory programmed.”

    No. As you, I don’t have a ready definition. The definition I used was how I understood yours. I think your examples are useless since you ask what functions, including consciousness, will be observed by replacing with a functionally identical part.

    About consciousness in non-humans: Imagine a biological, mechanical or whatever species visiting. It would be rude and conflict creating to suggest to them that we had a monopoly on consciousness.

    If they act and describe themselves as conscious, we must accept that. Regardless of details like that they don’t see colors, dream, hurt or such minor differences. It is a different consciousness, but it is consciousness.

    If any couple of computers that we make start to act like this, they will be conscious by definition.

19. **Chris Oakley**
    April 21, 2005

    Dear Anonymous,

    As it happens, for me, writing software for investment banks comes a poor second as a career choice to doing quantum field theory research, but I had no choice other than to leave. No-one in authority seemed to like being reminded of the fact that their subject had roots about as substantial as Birnham Wood. I got tired of arguing, and they got tired of listening to me. Nothing much has changed in the subject in twenty years except that the fad that I thought would lead nowhere (String theory) actually *has* led nowhere. It seems that it is alright to be wrong, provided that you are part of a large team that is wrong. I could ignore it, but I choose not to as, like Peter, I actually care about the subject and find it offensive that custodians of such a valuable and important part of human knowledge should discard the principles of scientific investigation so lightly.

20. **D R Lunsford**
    April 21, 2005

    Chris O – ROFL!

    “The Message from Water” – So does the hydrogen speak 66% of the time, or the oxygen 89%? And is heavy water more boring?

    That reminded me of a New Yorker cartoon. Two toga-clad academics are standing at the blackboard, on which is written “AIR EARTH FIRE WATER”. One of them gesticulates at the four words and states excitedly – “What do you MEAN it’s a good start? That’s all there is!”

    -drl

21. April 21, 2005

    Chris Oakley-

    I notice from your site that you basically work helping rich people get richer and further skew the distribution of wealth on the planet. Kudos on your fine humanitarian work.

    The majority of the people on the page you take such pains to point to disparagingly are involved in exploring the frontiers of knowledge and helping people heal and grow.

    Funny that one such as you should so stridently make fun of ones such as them. Perhaps deep down you realize how shallow your work is, and thus seeing those folks pushes a few of your buttons?

22. **Chris Oakley**
    April 21, 2005

    OK – just so that you know, here’s how you put a link to a web page in the comments section here:
    Enter the wonderful world of pseudo-scientific bullshit

    Which appears as

    Enter the wonderful world of pseudo-scientific bullshit

    which you can of course click on to take you there.

23. April 21, 2005

http://www.canonical.chemicalforums.com/stringcriticism.pdf

Hmm in page 12 a strange kink to chemistry is done. Alchemy somewhere around?

24. **Tom**
April 21, 2005

To Pope Juan,

I doubt anyone will endorse your "paper".

Your statements might sometimes in itself be factually correct, but it just shows a complete misunderstanding of what is really going on. You are clearly intelligent, but you remind me of the pope talking about sex. You wont get it until you go through the motions i.e. spending several years doing physics formula by formula.

String theory is not yet a scientific theory as such (and as a consequence full of flaws) but a research program led by ideas like extended objects like strings instead of particles, or extra dimensions, etc. I am not a "string theorist" myself, but I would claim that no "string theorist" would claim to have the right theory, but they all claim that that's the way forward to unifying QFT and gravity.

The only thing I am a bit uncomfortable with is.
1) Too many permanent posts are being filled by "string theorists".
2) Too many PhDs in string theory, provoking a drain brain, leaving them unemployed (too few postdocs), and sometimes depriving bright people from a satisfactory career in other areas.
3) Too much publicity, which then leads to complete misunderstanding by the public on the subject.

Tom

25. **Juan R.**
April 21, 2005

For a point of view contrary to Kaku and why string theory is already **outdated**
see

http://www.canonical.chemicalforums.com/stringcriticism.pdf

This nontechnical article has been submited to ArXiv. I am waiting for endorsement.

26.
April 21, 2005

Unemployed Parapsychologist,

Blackmore's book "Consciousness: Introduction" is a textbook discussing the pro&cons of different views on many different aspects of consciousness, and not a description of her own views.

But I do like the ideas of memes.

Tom

27.
April 21, 2005

"… more adventurous minds."

http://bizspirit.com/science/sspeakers.html

Inspirational note for David Duval. There is falling really hard and then there is…KAKU

28. **Unemployed Parapsychologist**
April 21, 2005

Tom,

I think I can save everyone the bother of buying Ms. Blackmore's book…she states her position quite eloquently in her latest book on memes:

"The next chapter takes up the ancient questions as to what constitutes the self and consciousness. After a brief review of some of the current theories on the matter, Blackmore proposes her own idea: that consciousness or self is a memeplex: an agglomeration of countless memes which survive best as a group. If you call this a soul, you are being religious and that is bad. But call it selfplex, and you are talking science. The selfplex is formed by the coming together of so many memes which "form a self-organizing, self-protecting structure that welcomes and protects other memes that are compatible with the group, and repels memes that are not (p. 231)." We are not just a bunch of neurons, she reminds us, we are "a pack of memes too (p.235)." "

And check out the chapter entitled "An orgasm saved my life". Very scientific indeed.

http://www.metanexus.net/metanexus_online/show_article.asp?3056

Susan Blackmore ? Stopped lecturing and abandoned parapsychology altogether, because she could no longer endure the near fanatic and rude behavior of both believers and non-believers.

http://en.wikipedia.org/wiki/Parapsychology

…headin down to the memeplex to buy a case of beer and a pack of memes

29.
April 20, 2005

“Aperion” showed his ass by blithering:

“I don’t think I’ve ever seen a bigger collection of freaks, hucksters, lamiacs, charlatans and deviants than the hit-parade of losers on tap at the Consciousness Conference. Where are are the vitamin and juicer pitchmen and the no-money-down people when you need them? At least they drive expensive cars and are much better looking.”

Another example of a post by someone who no doubt has contributed little or nothing to humanity heaping invective upon more adventurous minds.

Please do us all a favor and immediately stop taking vitamins and vegetable juices, so your self loathing will be inflicted upon us for a lesser amount of time.

thanks.

30. **Apeiron**
April 20, 2005

I don’t think I’ve ever seen a bigger collection of freaks, hucksters, lamiacs, charlatans and deviants than the hit-parade of losers on tap at the Consciousness Conference. Where are are the vitamin and juicer pitchmen and the no-money-down people when you need them? At least they drive expensive cars and are much better looking.

Is that an intemperate or insensitive comment?

Tough shit.

31. **Chris Oakley**
April 20, 2005

Most HEP theorists today will happily “bend over and spread their legs” for whoever pays their bills.

I really doubt that that is true. No-one paying the bills would have agreed to dozens of their hirelings going off on a crazy tangent for decades.

32. **Not a Nobel Laureate**
April 20, 2005

Most HEP theorists today will happiliy “bend over and spread their legs” for whoever pays their bills.

So whoring at the Templeton Foundation comes as no surprise.

33. **island**
April 20, 2005

People put too much stock in what consciousness is about, since it essentially just enables a level of comparitive uniqueness when it comes to our contribution to the entropy of the universe.

Local increases in complexity and order necessarily equate to increases in the potential for disorder in an expanding universe, and this effect gets compounded as negative pressure increases to bring about emergent properties that enable the system to pay back the ever incresing entropic debt.

Our unmatched ability to process information and isolate the release of enough energy to directly affect the symmetry of our universe defines good physical reason why the expanding universe would “need” us into existence via the isolation of its forces, especially if the negative pressure component is increasing.

I agree with disgusted, string theroy utter bullshit, but I also think that everybody is so stuck on the either/or mentality of chaos vs. god that they can’t see the REAL importance of the most predominant freaking physical need in our universe.

The whole fanatical world is not even wrong…

34.
April 20, 2005

To Torbjorn,

I dont claim to actually have a definition for consciousness, as I dont know what it exactly is or whether it even is (like aether or elan vital).

I only want to clarify that by consciousness I dont mean responsiveness (i.e. being not unconscious), awareness (i.e. being conscious of something) or self-identity (i.e. knowing to be an entity). But rather what is left, what some call first-person experience, qualia, sentience. Someone can still have a conscious experience without being either responsive (lost all control of body), aware (of outside events), or self-conscious (some states of meditation)?

You think that my examples are useless, because you think that consciousness is only awareness and self-identity, which indeed can be at least in theory programmed.

Of course, computer might behave like us but are they mimicking us functionally or really experience like us i.e. “is there anyone home?”

All my examples are trivial if you believe that a standard computer can be conscious by just running the right software. But here I would quote you by saying “examples are useless since you can’t make them work.” Unless you implement the computer like a brain maybe?? 😉

Tom

35. **Thomas Larsson**
April 20, 2005

I still think that it is instructive to compare string theory to aether theory, because the parallels are so obvious. All the cool and smart people worked on these theories for decades, arguably with one successful physical prediction (electromagnetic waves/gravity), and they definitely led to some mathematical progress. However, both theories were eventually slain because one necessary thing was not observed (aether wind/low-energy supersymmetry), and one impossible thing was indeed observed (photoelectric effect/positive cosmological constant).

Does this mean that string theory is wrong? Perhaps it is aether theory that is not even wrong; in a sense, it was resurrected in the form of cosmic microwave background radiation.

36. **big bang**

April 20, 2005

completely disgusting.

37.
April 19, 2005

Is a theory that cant be proved right automatically (even) wrong?
I mean, if there is not a single experiment which can totally disproof the theory but nevertheless the theory predicts possible outcomes which might be realized in nature, is that science?
I have myself an answer for that question, but I wonder, we might have reached a point where perhaps many possible outcomes are allowed and we just happen to live in one of them.
That's not totally rubbish, and even though I dont like the uses of the anthropic principle and the landscape I wonder myself whether or not a GUT (something that I dislike too) could even make sense or not and what can we infer about the universe in either case. I do believe that philosophy is important for science, and asking deep questions have led us into new roads of understanding. I used to have discussions with historians, if what they do could be called 'Science'. If doing forensic studies and retrodiction is equivalent to predicting things. Certainly we can sort of disproof theories by an incorrect account of known results, but can we affirm their theory is 'correct' if leads consistently to what has been seen?
I myself spare my time between deep questions and hardcore science. I do believe that a real scientist should 'tink a little' about foundational issues once in while….after tenure preferably 🙂

best regards,
N

38. **Torbjorn Larsson**
April 19, 2005

Tom, to clarify: _all_ your examples are useless.

They amount to ask: "If one changes a part to a functional equivalent, will one see a functional difference?"

Computer consciousness can however be given an operational definition, as stated.

39. **Torbjorn Larsson**
April 19, 2005

Tom: Consciousness is (still) badly defined in general, and by you in particular. Your 'mental world' seems like awareness and selfidentity.

These things are observed in higher animals and humans. How do I know another person is conscious? I know I am, I observe them to behave like that and they describe themselves to be. One day computers will likely do the same to each other; they are then equivalently conscious as we are.

The other examples are useless since you can't make them work. They amount to asking "If I replace the Sun with an identical sun without you noticing it, will you notice it?"

40. **Tom Weidig**
April 19, 2005

To Travis,

I think consciousness is not as conceptually straightforward as you might think. Awareness and Self-identify seems acceptable to be created by the neuronal firing, but first-person experience (our mental world) seems very different.

Here are a few questions:

if you could duplicate the brain functionally on a computer, would the computer be conscious (in the sense of having an "inner life" or first-person experience)?

what if I train all Chinese to simulate the brain functionally like the computer, would there be a consciousness different to the billions of Chinese spread across China?

Suppose I step-by-step replace your brain with chips simulating each a neuron, will you loose your consciousness?

If I clone you physically identical, are there two yourselfs??

Buy the textbook, it's worth reading. As I said before, I dont know what to think, but it's certainly more intriguing than you think it is.

Tom

41. **Disgusted**
April 19, 2005

I guess the string nuts must already be chomping at the bit:

"it is clear that consciousness is equivalent to the abstract patterns formed by firing neurons and their dendrite connections in the brain"

to join the ranks of consciousness (whatever that is) "scientists" when their own funding dries up.

Should be good for another 20 years of worthless speculation and philosophy while avoiding real-world accountability.

42. **Peter Woit**
April 19, 2005

Hi Travis,

My comment about Susskind in this posting wasn't a criticism at all. It sounds like he was doing the right thing and disavowing any support for "Intelligent Design". When I said that by doing this he "ruined his chances at the $1.4 million", that was meant as a compliment. I think his behavior in pushing the landscape pseudo-science has been outrageous, but I never thought he was an Intelligent Designer and never accused him of this (OK, on April Fool's day, as a joke, I kind of implied it…..)

You and lots of others keep talking about predictions of Arkani-Hamed and Dimopoulos derived from the landscape and how they'll be testable before long. I've read their papers and see nothing there that looks like anything I would call a "prediction". I'll ask you the same question that I keep asking everyone who says this. Give me a prediction that they derive from the landscape (and a real prediction, one that if it is wrong, the landscape scenario is wrong), and tell me whether it is testable at LHC energies. If it isn't what energy is required? If it is, how much integrated luminosity will be required to falsify their prediction and show that the landscape is wrong?

43. **Travis Garrett**
April 19, 2005

Speaking a staunch atheist, I think the comment on Lenny was a cheap shot. There is an enormous difference between the landscape (it is certainly possible that chaotic inflation produces many different stable string vacua – and hopefully testable before too long along the lines of Savas & Nima…), and all of the obviously false religious/supernatural non-explanations. Of course Lenny doesn't have any connection with ID just because he finds the landscape intriguing – so do I – and it is disingenuous to imply otherwise.

Hi Tom – I also think that it is clear that consciousness is equivalent to the abstract patterns formed by firing neurons and their dendrite connections in the brain. And it works in the classical limit – each neuron adds up all the excitatory and inhibitory impulses and if the sum crosses a threshold then it fires too and so forth (and the Penrose stuff is crazy – the Chinese room and Godel arguments against classical AI are obviously wrong, and the decoherence time is tiny). Of course, more precise detail would be great (so that, say, you could study the topology of the connected graph that gives rise to the sensation of red and so on). In the end, consciousness will be nothing more than a (very interesting) subset of neuro & computer science.

44. **Jo?o Carlos**
April 19, 2005

I remember reading a science fiction story many years ago about a society which had highly advanced technology, but had lost the capacity to understand that technology scientifically and instead had shrouded the technology in religious mysticism. Could have been by Asimov, I'm not sure. Anyways, perhaps that's where we are headed.
Isaac Asimov's **Foundation**.

45. **Jo?o Carlos**
April 19, 2005

Well… I'm religious, I'm mystic, I'm a credulous person, and **I happen to know G?ddel's Theorem**. Trying to meddle religous beliefs with science is **stupid!** Faiht and science are opposites! Anyone who disagrees may email me. I've got a lot of *gold bricks* to sell…

46. **JC**
April 19, 2005

Anybody know where some of these pseudo/anti-science attitudes in America came from (whether government decreed or a general societal mentality)?

Only widespread case of a government and/or societal censure of a science in recent times that I can think of offhand, would perhaps be during Nazi times in Germany where various government decrees attempted to ban all "jewish physics" like relativity, quantum mechanics, etc ….

47.
April 19, 2005

I remember reading a science fiction story many years ago about a society which had highly advanced technology, but had lost the capacity to understand that technology scientifically and instead had shrouded the technology in religious mysticism. Could have been by Asimov, I'm not sure. Anyways, perhaps that's where we are headed.

48. **Steve m**
April 19, 2005

This will make you cringe Peter

http://www.web-books.com/GoodPost/Articles/Prologue.htm

Especially the line, "Physicists have discovered the laws that prevents us from seeing the Kingdom of God". The stuff on "String Theory and the Resurrection of the Dead" was especially BS.
While I am not antistring I am disturbed people abuse it in this rediculous way and a lot of scientists who should know better (eg Kaku and others) are as much to blame. I imagine this is the sort of stuff the so called "Templeton Foundation" will actually promote as real scholarship. Real scientists should not be whoring themselves out to these sorts of agenda-driven foundations. Maybe someone could submit a Sokal-type joke paper to one of these conferences? I get the very worrying sense these days that physics and science and the scientific method are getting trashed and undermined from all sorts of angles; that we are moving into some sort of "anti-enlightenment". All this metaphysical religious-motivated nonsense going on at these conferences simply does not "lie along the line of what we are able to understand if we devote ourselves to it" (to quote Jacob Bronowski, the great secular humanist). I worry greatly that science might ultimately not survive in the far (or perhaps not so far)future. I think it was Jacques D recently who made the analogy of a few threads coming off a sweater but eventually the whole thing can unravel. I really hope that in a hundred years from now we are not back in the dark ages burning witches once again.

49. **Steve Esser**
April 19, 2005

Folks like you Peter and Sean Carroll are convincing me that despite good intentions, Templeton and similar efforts do more harm than good and serious scientists should stay away. The line-up for the Science and Consciousness conference you linked to was particularly nutty (whereas the Consciousness conferences at Arizona mentioned earlier are mostly high quality with just a few sprinkles of nuttiness).
On the other hand I also fear that scientific explorations of consciousness probably suffer from this inability to keep the topic crackpot free and this is unfortunate.

50. **D R Lunsford**
April 19, 2005

I'm the one hurling epithets at Kaku, because he's a goofy charlatan. Don't blame the nice guy Peter. I'm not a nice guy and don't aspire to be one. My edge has been sharpened on the idiocy of my generation.

-drl

51. **Peter Woit**
April 19, 2005

Well, at least in this posting I'm not hurling epithets, making any ad hominem attacks, or using the words "crazy" or "bullshit". All I did was accurately quote Kaku's own words and link to information about the conference he is speaking at. If you drew the conclusion that the conference was devoted to crazy bullshit, I'm not going to argue with you.

You're right, this stuff does push buttons with me. I care very deeply about this subject and have been working in it for 25 years. I know exactly where these ideas came from and why they're getting so much attention, and I'm not happy with it. Kaku and others are devoting their lives to trashing something very important to me. They've gone a long way towards turning the field of particle theory from a serious and important science into a depressing pseudo-science, and in recent years this has gone from bad to worse. If you don't like this point of view and think Kaku's behavior is fine and I shouldn't be even implicitly criticizing it, you're really not going to enjoy this weblog.

52. **Todd Sieling**
April 19, 2005

I'm not a proponent nor an opponent of M-theory, Kaku etc., but I'm wondering why there is such a tendency to hurl epithets and ad hominems just because you disagree with someone. This stuff is obviously pushing some buttons with you, but is it so off the chart that it reduces you to labels like 'crazy' and 'bullshit' rather than triggering your curiousity about why ideas you hate are getting so much attention? I'm sure the latter would produce a much more interesting discussion than what I'm reading here.

53. **Tom Weidig**
April 19, 2005

**************** CONSCIOUSNESS SHOULD BE TAKEN SERIOUSLY, BUT MOST WHO STUDY IT NOT!!*********

Hi,

I have been reading your blog for a while, and finally I think I have something to say! My background is PhD theoretical particle physics, but I now work in finance.

Over the last two year, I have been looking at consciousness. I read some books and attending the Consciousness 2004 conference in Tucson last year.

Here are my impressions.

1. Consciousness is a very intriguing subject, and certainly a conceptual minefield for the weaker minds.

2. It should not be dismissed as irrelevant or unscientific. After 2 years, I still dont know what I should think of "consciousness". By consciousness, I mean "first person experience", not just "awareness". You sit at your computer, and ask yourself "Am I conscious now? What do I perceive". Can you build a computer that will ever have this experience? If not, what is special about consciousssness? If yes, explain me how the brain manages to create this experience, the unity of ourselves. Is it really just a big illusion?

3. Consciousness is really part of the neuroscience agenda. In the last decade due to new brain imaging technology, scientist can now look into the brain. And it is very fascinating. How can the brain work? How does it generate first person experiences?

4. Ultimately, it has to have an impact on physics/science itself, because effectively a bunch of neurons consisting of atoms study the universe and create theories (like standard model or string theory). The we-physicists you-nature separation is an extremly well working approximation, but does not really exist.

5. It is very unclear whether QM has any impact on the functioning of the brain, let alone is responsible for consciouness. But it is valid to ask whether there are QM effects in the brain. For example, the proteion folding can only be described QMically. but is it important?

6. The field of consciousness attracts a mix of people: serious scientists, "ufologists", and scientists "who have gone overboard". The last two give the field a bad name, as serious scientists themselves commit a logical fallacy by dismissing the whole field based on the theories propagated by a few people from the last two categories. The conference I went to was really strange. On the one hand you had very clever people from neuroscience, and on the other hand I had to listen to people telling me that Einstein was wrong and it's all a conspiracy.

7. A comment on Kaku and Penrose/Hameroff. I think Kaku is just doing PR for himself. I have read his quantum field theory book, a bit sloppy, and noticed that he semi-copied several parts from other books, like Rajaraman's book on solitons. He is a 20/80 person. Only do 80% of the quality but in only 20% of the time. I dont know what to think of Penrose, as he has such a high reputation. But I have met Hameroff, and the guy speaks of QM and the brain and a final theory. And he doesnt really understand QM at all.

Anyone seriously interested in the study of consciousness should read "CONSCIOUSNESS: An introduction". A textbook by Susan Blackmore. Very well written and very scientific, too. If you think I am wrong, then at least have a look at amazon.

Tom

54. **Chris Oakley**
April 19, 2005

Nec spec nec metu,

I do not agree. Scientific materialism is good. Mystical bullshit is bad.

Let me take a concrete example.

Your e-mail address would suggest that you are Tibetan. This is relevant because I spent most of yesterday trying to figure out the Tibetan calendar. It seems that the days are based on 12 degree segments of the lunar month and begin on the new moon, like the Hindu tithis, but despite searching the only book in English on the subject and Googling furiously, no-one seems to want to tell me (a) the criterion for matching lunar days to solar days and (b) the rule for inserting leap months. In the case of (a) somebody somewhere says that the lunar station has to occur between 5 AM on the solar day and the same time the following day, but he did not say which time zone is used, if any, and in the case of (b) the zodiac sign at the new moon determines the name of the lunar month, but it is not clear whether the tropical zodiac is used, like the Chinese or the sidereal one like the Hindus.

It would seem therefore that instead of one being in a situation where anyone with the ability to calculate or reference lunar/solar tables being able in principle to work out the calendar, as happens in the rest of the world, you have to rely on the Tibetan Medical and Astrological Institute, or whoever, to generate the dates for the calendar. Why is this good?

55. **D R Lunsford**
April 19, 2005

In the linked interview, Kaku proves that he is crazy (probably from spending too much staring out the window at Manhattan traffic..) He makes the fantastic statement that the Schroedinger equation “looks horrible” because it is not relativistically invariant! No one who understands a fig about the development of science and the theory of differential equations could make such a statement.

The problem these loons have is with their aesthetic judgment, mistaking their own screwy ideas of “beauty” for truth. Instead of staring out the window, Kaku should amble down to MOMA, or take a course in pottery. (What he says about music in the interview is also ridiculous. The implicit self-comparison with Beethvoen and Mozart made me cringe.)

-drl

56. **D R Lunsford**
April 19, 2005

I’m seriously considering throwing out my Kaku book. I saw a show last night on Discovery concerning Einstein (horrible) and as usual, the omnipresent Kaku was there spouting nonsense about both the man and the subject. He’s become a sort of Carl Sagan-san for physics. (I think Kaku is a clone. I swear he was also playing left field for the Mariners.)

The fact is that there is a rough consensus emerging among those engaged in research about how the future will evolve. Because the laws behind the quantum theory, computers, and molecular biology are now well established, it is possible for scientists to generally predict the paths of scientific progress in the future. This is the central reason why the predictions made here, I feel, are more accurate than those of the past.

What is emerging is the following.

The Three Pillars of Science

Matter. Life. The Mind. [what horseshit]

These three elements form the pillars of modern science. Historians will most likely record that the crowning achievement of twentieth-century science was unraveling the basic components underlying these three pillars, culminating in the splitting of the nucleus of the atom, the decoding of the nucleus of the cell, and the development of the electronic computer. With our basic understanding of matter and life largely complete, we are witnessing the close of one of the great chapters in the history of science. (This does not mean that all the laws of these three pillars are completely known, only the most fundamental. For example, although the laws of electronic computers are well known, only some of the basic laws of artificial intelligence and the brain are known.)

This to me is filled not only with mysticism, but also a sickly egoism and pessimism. I could not make such statements with a straight face. Has the large influx of Easterners into hard science since the war also brought along the underlying mysticism, even fatalism?

-drl

57. **Nec spe nec metu**
April 19, 2005

There is more in heaven and earth than is dreamt of in your philosophy.

“Official” science shows its arrogance when it either dismisses or “suddenly discovers” that which has been known to many throughout history. “Many dimensions” is old news to the vast majority of the world’s cultures and philosophies. The kind of scientific materialism that your attitude represents is just a blip in the history of human (and non-human) consciousness. Fortunately, you and those who think like you will also enjoy your multi-dimensionality when you overcome your fear and grow up a little.

It is not felicitous for those who have not made a mark at all to snipe at the brave explorations of greater minds who do not shrink from possibilities that may require major paradigm shifts.

The greater minds are too busy exploring to gossip, the lesser minds are too busy gossiping to explore.

58.
April 19, 2005

You ruined it Peter.

Lenny had it all planned out. He was gonna pocket the 1.4 mil, and then publicly repudiate ID as soon as the check cleared. Your little April Fool’s hoax forced his hand early.

59. **Matti Pitkanen**
April 19, 2005

To my opinion one should not lump together the desperate state of M-theory and the fact that leading physicists are finally beginning to realize that physics must sooner or later become also a theory of consciousness.

Personally I have spent 10 years working with quantum consciousness theory and the purely physical insights gained in this manner have been decisive for the mathematical formulation of quantum TGD proper. This vision about consciousness based on general principles of physics leaves only one conclusion: the standard view about consciousness as epiphenomenon fully deserves the attribute not-even-wrong. What else it could be when even the word “consiousness” has been and still steems to be a taboo for most physicists?

I have done this as unemployed and have not received a single coin of any kind of research money. Therefore I do like the light-hearted labelling of individuals who see farther than average colleagues as some kind of businessmen cheating money from religious organizations.

To avoid misunderstandings, I still emphasize that the open minded attempt to understand consciousness and intentionality using physics is something totally different from the desperate attempts to save M-theory by using anthropic principles. The recent miserable situation in M-theory is a logical consequence of the reductionistic and materialistic world view taken to its Planck length extreme, and the only way out is to jump out of the system and widen the scope of physics itself. My personal dream is to see the day when brightest theoretical physicists fully devote themselves to the problem of consciousness.

Matti Pitkanen

Taking a Break

April 22, 2005
Categories: Uncategorized

This week I'm quite busy so I'm taking a break from landscape-bashing. Instead I'll just quote someone else; it's up to you to guess who.

"Suddenly it's not too important whether a theory teaches us something new about the real world – either predicts new unknown phenomena or previously unknown links between the known phenomena and objects. It's more important that such an unpredictive scenario might be true and we should all work hard to show that the scenario is plausible because we should like this scenario, for some reasons that are not clear to me."

"The anthropic strategy is to pick as complicated Calabi-Yau manifolds as possible, to guarantee that there will be a lot of mess, confusion, and possibilities, and that no predictions will ever be obtained as long as all the physicists and their computers fit the observed Universe… This means that you don't want to start with Calabi-Yaus whose Betti numbers are of order 3. You want to start, if one follows the 2004 paper, with something like F_{18}, a toric Fano three-fold. That's a 3-complex-dimensional manifold that is analogous to the two-complex-dimensional del Pezzo surfaces, in a sense. But you don't want just this simple F_{18}. You take a quadric Z in a projective space constructed from this F_{18} and its canonical bundle. OK, finally the Euler character of the four-fold X is 13,248. Great number and one can probably estimate the probability that such a construction has something to do with the real world."

"Do we really believe that by studying the orientifold of the weighted projective space CP^{4}_{}, we will find something that will assure us (and others – and maybe even Shelly Glashow) that string theory is on the right track? … If we deliberately try to paint the string-theoretical image of the real world as the most ambiguous and uncalculable one, I kind of feel that it's not quite honest."

"Some people used to blame string theorists that they were only looking for the keys (to the correct full theory) under the lamppost. It's unfortunately not the case anymore: most of the search for the keys is now being done somewhere in the middle of the ocean (on the surface). Maybe, someone will eventually show that the keys can't stay on the surface of the ocean, and we will return to the search for the keys in less insane contexts."

Comments

1. **Juan R.**
   April 28, 2005

   Of course, I can be wrong. It is obvious that all man is wronmg some time, but I follow the philosophy of sending copies of my papers to people that is directly critiqued on my work.

   I sent a copy of Is this dynamics? to Weinberg and also to other people, specialist in different topics that I studied, and nobody find failures on the mathematical derivations. On specialist on molecular dynamics did a three page reply manuscript but I think that I show that his criticism was not satisfactory.

   Weinberg found no failure.

   My philosophy of scientific research is different and i am not proud of making mistakes.

   Your philosophy is very strange for me but i respect it!!

2. **Thomas Larsson**
   April 27, 2005

   Juan R.,

   If I make the same mistakes as Weinberg, I'm proud of it.

3. **Simplex**
   April 27, 2005

   According to the poster of 26April@1:44PM it was pointed out in 1957 by Wigner and Salecker that in QM there are no perfect clocks.

   Ideal classical time evolution is unphysical (just a convenient approximation) and at a more fundamental level is replaced by correlations between observationscorrelations between readings on real quantum clocks (which have finite lives and do not necessarily agree) and other observables.

   The poster cited 3 recent papers from Phys. Rev. Letters. I am wondering if anyone has looked at them? I found the arxiv numbers of the preprints and posted them also on 26
   April in case anyone might wish to have a look.

   I looked at the papers and I believe one of the main points is that if one describes time evolution by real-world clocks then time evolution can not, for theoretical reasons, be unitary. There is an interesting theoretical bound on how precise, and at the same time long-lived, a real clock can be.

   Here is an exerpt of the earlier post:
   "Unitarity is not needed to conserve probabilities. The existence of an 'ideal classical' time in which physics is unitary is simple 'wrong' within any QG framework. As emphasized originally by Salecker and Wigner (Rev.Mod.Phys. 29 (1957) 255) and more recently by Ng(Mod.Phys.Lett. A9 (1994) 335) and Camelia (Mod.Phys.Lett. A9 (1994) 3415-3422), there exist limits in nature to how 'classical' even the best possible clock can be. When one introduces realistic clocks, quantum mechanics ceases to be unitary and indeed, a fundamental mechanism of decoherence of quantum states arises…"

4. **Arun**
   April 27, 2005

   Thomas Larsson wrote:

   "You cannot give up unitarity without making a drastic modification of quantum mechanics. I didn't realize that this was controversial."

   It is not controversial that giving up unitarity will require modification of quantum mechanics. It is controversial that giving up unitarity means probabilities will not longer add up to 1. Recall –

"Unitarity encodes the idea that all probabilities must add up to unity. Although the technical requirement of unitarity might perhaps be relaxed in some way, I don't understand how this underlying idea could be wrong. At some point Dirac had some strange ideas about probabilities taking values between -2 and +2 (according to Dyson), but that evidently didn't work out either. "

In the Copenhagen interpretation of QM, measurements do not preserve unitarity. If all time evolution is indeed unitary, i.e., measurement itself is described in QM, then QM is able to provide excellent approximations to non-unitary time evolution. Maybe, just like free quarks are unobservable, unitarity is not directly observable when gravity plays a role.

5. **Juan R.**
April 27, 2005

Canonical science shows that unitarity is an approximated concept.

Unitarity –> conservation of probability and conservation of norm for closed systems.

But the inverse is not true. One can maintain non-unatirity with conservation of probability and well defined quantum states.

non unitary evolution is responsible for collapse of wave function and also for the increase of entropy (second law).

Usual mathematical proofs on the adequacy of unitary are simply wrong. E.g. Chapter three of Weinberg QFT is full of mathematical mistakes. I demonstrated time ago (manuscript sent to Weinberg) that there at least three mathematical mistakes in Weinberg derivation of master equation.

6. **Thomas Larsson**
April 27, 2005

I had pure states in my mind; I haven't thought about mixed states for a long time and really don't want to say anything about them. However, unitarity simply means that the norm and hence the total probability is preserved, so you cannot give up unitarity without making a drastic modification of quantum mechanics. I didn't realize that this was controversial. That it is difficult to define an invariant inner product without a background Minkowski metric is another matter.

One way to ensure unitary time evolution is to have a unitary rep of some symmetry group containing time evolutions as a subgroup, e.g. the Poincare or diffeomorphism groups. However, the only unitary, proper rep of the diffeomorphism group is the trivial one, so there seems to be no time evolution at all in quantum gravity; this is one version of the problem of time. One can avoid this paradox, at least in 1D, by allowing for projective reps, since the Virasoro algebra has many non-trivial unitary reps with positive central charge. The only alternative is apparently to formulate quantum gravity holographically using some sort of AdS/CFT, but that idea has serious problems since AdS is ruled out experimentally.

7. **D R Lunsford**
April 26, 2005

Thomas – I think I would say unitarity amounts to the possibility of setting up an isolated system and assigning it a wave function.

-drl

8. **D R Lunsford**
April 26, 2005

Arun said:

I thought unitarity encodes such a rule against probability amplitudes. Any process where a quantum mechanical pure state evolves to a mixed state would violate unitarity.

Well that's the whole point – a system that is fully described remains so. "Describe" means to give an exhaustive accounting of possibilities of measurements. Unitarity means new possibilities don't just appear or disappear from the blue. Insofar as quantum theory has a variational principle, something *has* to have this property.

-drl

9. **Arun**
April 26, 2005

Thomas Larsson wrote :

"Unitarity encodes the idea that all probabilities must add up to unity."

I thought unitarity encodes such a rule against probability amplitudes. Any process where a quantum mechanical pure state evolves to a mixed state would violate unitarity. Unitary time evolution cannot increase entropy, can it?

10. **Simplex**
April 26, 2005

"…exist limits in nature to how 'classical' even the best possible clock can be. When one introduces realistic clocks, quantum mechanics ceases to be unitary and indeed, a fundamental mechanism of decoherence of quantum states arises.
A concrete model has been recently put forward in discrete quantum gravity
(Phys.Rev.Lett. 90 (2003) 021301) which also has the appealing consequence of 'solving' the BH information paradox. (Phys.Rev.Lett. 93 (2004) 240401)
These ideas, including a consistent discretization of gravity, can be also applied to loop geometry (Phys.Rev.Lett. 94 (2005) 101302)."

I looked up the preprints for these articles
(Phys.Rev.Lett. 90 (2003) 021301) preprint is: gr-qc/0206055

(Phys.Rev.Lett. 93 (2004) 240401) is: hep-th/0406260

(Phys.Rev.Lett. 94 (2005) 101302) is: gr-qc/0409057

11.
April 26, 2005

"Unitarity encodes the idea that all probabilities must add up to unity. Although the technical requirement of unitarity might perhaps be relaxed in some way, I don't understand how this underlying idea could be wrong."

Unitarity is not needed to conserve probabilities. The existence of an 'ideal classical' time in which physics is unitary is simple 'wrong' within any QG framework. As emphasized originally by Salecker and Wigner (Rev.Mod.Phys. 29 (1957) 255) and more recently by Ng(Mod.Phys.Lett. A9 (1994) 335) and Camelia (Mod.Phys.Lett. A9 (1994) 3415-3422), there exist limits in nature to how 'classical' even the best possible clock can be. When one introduces realistic clocks, quantum mechanics ceases to be unitary and indeed, a fundamental mechanism of decoherence of quantum states arises.
A concrete model has been recently put forward in discrete quantum gravity
(Phys.Rev.Lett. 90 (2003) 021301) which also has the appealing consequence of 'solving' the BH information paradox. (Phys.Rev.Lett. 93 (2004) 240401)
These ideas, including a consistent discretization of gravity, can be also applied to loop geometry (Phys.Rev.Lett. 94 (2005) 101302).

all the best

12. **Thomas Larsson**
April 26, 2005

String theorists are still looking under the lamp post, in that I have not found many willing to concede that the universe might not be unitary. Loop quantum gravity theorists are much more open to non-unitarity.

Unitarity encodes the idea that all probabilities must add up to unity. Although the technical requirement of unitarity might perhaps be relaxed in some way, I don't understand how this underlying idea could be wrong. At some point Dirac had some strange ideas about probabilities taking values between -2 and +2 (according to Dyson), but that evidently didn't work out either.

Anyway, the LQG people evidently have a unitary rep of their C* algebra, called the Ashtekar-Isham-Lewandowski representation, so I don't think that they really give up unitarity. Unfortunately, this rep is not of type usually encountered in quantum theory, and apparently it leads to all kinds of trouble, like the energy not being bounded from below in the harmonic oscillator.

Being?very skeptical about all kinds of new ideas, I would like quantum gravity to keep all the key concepts in quantum theory and general relativity, i.e. unitarity, locality, causality and general covariance. There is of course a no-go theorem forbidding the combination of locality and diff invariance ("there are no local observables in quantum gravity"), but this is evaded by allowing for projective reps of the diffeomorphism group (this was a major motivation for finding these). With a positive cosmological constant, I don't really see what the alternative to locality could be, since there is no S-matrix and no good boundary on which a boundary theory could live.

For a while during the development of computer science, a ridiculously large fraction of the incoming students wanted to do the highly hyped field of Artificial Intelligence, while the number leaving with a thesis in AI was much smaller. They arrived, realized that AI was to a great extent a scientific failure, and changed fields at some point during their graduate career.

I spent a couple of years in my youth working for a startup company whose product was an implementation of the AI language Prolog. Like the rest of the AI industry, this company went bankrupt after the demise of the Japanese Fifth Generation project. We had a rather cool motto, though: Artificial intelligence is better than none.

13.
April 25, 2005

"For those physicists forecasting doom and gloom, missteps of artificial intelligence don't seem to have hurt the larger discipline of computer science much. "

Well, then, what do the Japanese have to say about that (re: The Fifth Generation Project)…if the parallel is to hold such that an deliberately overly ambitious grandious program leads to unforseen benefits along the way.

Did the Japs benefit or not?

14. **Aaron**
April 25, 2005

> I think this is because there's a narrative (I can't judge how true it is) that string theorists tell. This is that they start with the hypothesis that physics is unitary, is consistent with general relativity and quantum field theory, and they are led inexorably to a ten- or eleven-dimensional world with string theory.

I hope nobody's telling that narrative, because it certainly isn't true. String theory is simply the best guess out there for something that unifies gravity and QM. It certainly does not come close to following from just naively attempting to unify QM and GR; it's origin is much more convoluted.

I should also mention that the counternarrative by some LQG-types that string theorists are closeminded about other directions simply isn't true. I came into grad school well-versed in these myths and was disabused of them. This is not to say that everyone knows everything about the flaws in the other approachs, but the idea that LQG (as the only other major example, really) is rejected simply out of prejudice is just plain wrong.

15. **JC**
April 25, 2005

Perhaps Lubos' terminology of the "haystack" (instead of the "landscape") is more telling sign.

16. **Peter Shor**
April 25, 2005

String theorists are still looking under the lamp post, in that I have not found many willing to concede that the universe might not be unitary. Loop quantum gravity theorists are much more open to non-unitarity.

I think this is because there's a narrative (I can't judge how true it is) that string theorists tell. This is that they start with the hypothesis that physics is unitary, is consistent with general relativity and quantum field theory, and they are led inexorably to a ten- or eleven-dimensional world with string theory. But if it is true, then at some point, if they start coming to more and more unbelievable conclusions, they might want to start questioning their hypotheses.

17. **Peter Shor**
April 25, 2005

For graduate students, the relevant number is not the number of students going to grad school determined to do string theory, but the number of students leaving grad school having done string theory. For a while during the development of computer science, a ridiculously large fraction of the incoming students wanted to do the highly hyped field of Artificial Intelligence, while the number leaving with a thesis in AI was much smaller. They arrived, realized that AI was to a great extent a scientific failure, and changed fields at some point during their graduate career. In the defense of AI, let me say that since then it has reinvented itself, and now has much more modest goals which it is (to a large extent) truly succeeding in accomplishing.

I leave the parallels with string theory to the readers. For those physicists forecasting doom and gloom, let me note that the missteps of artificial intelligence don't seem to have hurt the larger discipline of computer science much.

18.
April 25, 2005

String theorists and Creationists give the anthropic principle a bad rap, which gets compounded by its only being a truism that's necessarily as incomplete as Dirac's Large Numbers hypothesis was flawed.

Tack on the entropic interpretation and you've got good reason for it if the universe is 'closest to Einstein's hand"

Based on the typical actions of humanity tho… it's the only thing keeping us alive!… 😉

19. **D R Lunsford**
April 25, 2005

Arun – that was a well-turned phrase 🙂

-drl

20. **CW**
April 25, 2005

It's worth remembering that even a physicist as great as [Wolfgang Pauli](#) was moved to despair at certain points in the development of quantum mechanics. During one such episode he declared that he wished he had become a magician instead of a physicist.

21. **Daniel Doro Ferrante**
April 25, 2005

JC: I don't have any particular stats on the number of grad students entering ST… however, something that i have been noticing over the years is that more and more folks are coming into grad school with their minds set into ST; as if there was nothing else in physics other than ST. That is, apparently these folks have "bought" the "marketing" without knowing the "merchandise" well enough.

22. **JC**
April 25, 2005

Anybody have any statistics on the number of graduate students entering string theory these days?

It would be interesting to compare it to the situation in the mid-late 1990's and/or mid-late 1980's.

23. **Thomas Larsson**
April 25, 2005

Two more Lubos quotes:

If string theory is gonna require an infinite sequence of refinements and increases of the complexity (and decreases of expected predictivity) of its vacua to match reality, then I would definitely love to know this fact as soon as possible, because in such a case I would consider string theory to be a wrong theory of physics – much like other incorrect theories in the history of science that were first corrected 100 times before they were abandoned (recall Lorentz's explanations of Morley-Michelson experiments) – and people should move on.
*Of course I don't believe that this is correct, but allowing something like that for a theory *does* mean to construct a scientifically unjustifiable theory.*

*More frustratingly, using the words of Steve Shenker, AdS/CFT now says that nearly every QFT may be assigned a "gravitational dual" – i.e. every quantum theory is, in some loose sense at least, a theory of quantum gravity (in a higher-dimensional space). We definitely don't want to be *this* broad because then the term "quantum gravity" would become pretty vacuous.*

Dan Friedan was quiet for decade before he declared that string theory was a complete scientific failure. Lubos hasn't published for a year. Let's see what happens in nine years.

24.
April 25, 2005

science will not be on a firm footing until it rocognizes the primacy of consciousness. All we experience is the awareness that cognizes shapes and colors, and the stories we tell ourselves about those shapes and colors. The stories are always changing, the shapes and colors are always changing. Only the awareness abides.

25. **Arun**
April 24, 2005

Science survived Sir Isaac Newton. It will survive the anthropic principle and superstring theory.

26. **island**
April 24, 2005

Somebody asked:
Is Lubos starting to doubt? Is he experiencing a conversion moment?

Nah, but he hates the anthropic principle enough to put his foot right into his own mouth. He thinks he's got a new and better idea:

http://motls.blogspot.com/2005/02/entropic-principle.html

… that he got from here:

http://www.arxiv.org/abs/hep-th/0502211

So now he's decided to jump on a bandwagon of denial instead, because the anthropic coincidences don't just go away because of this, and an "entropic" anthropic principle requires that the forces of the universe be constrained to produce intelligent life as a special and integral contributor to the thermodynamic process.

The probability that the cosmological evolution will end up as a Universe with a particular shape of the hidden dimensions (and particular values of the fluxes) is determined by the (exponentiated) entropy of a corresponding black hole whose geometry flows via the attractor mechanism to the given shape of the Universe near the horizon. Note that this contrasts sharply with the "anthropic principle" – which itself is not a principle, rather a lack of principles. In the anthropic principle, the corresponding probabilistic weight is determined by the ability of the Universe to support intelligent life.

Not quite, since Black holes and humans share a comparitively uniqueness when it comes to the entropy of the universe, since both can isolate the release of enough energy to make real particles from the negative energy of the vacuum, which serves to reverse the normally destructive consequences of the second law of themodynamics on a grand scale, while directly affecting the symmetry of the universe.

What they've actually managed to accomplish is to identify yet another of the vast and growing number of cosmic coincidences that define the many ecosystematic balances that are common to the anthropic principle, but in this context it's become a "biocentric" principle, where intelligent life is now more probably required on most every banded sprial galaxy that has a black hole at its center.

Way to go, Lubos, et. al… you've turned it into an epidemic… 😉

27. **Simplex**
April 23, 2005

I hadnt seen the most recent Lubos blog.
It's all there. Sorry for being dense
http://motls.blogspot.com/2005/04/kennedys-landscape.html

28. **Simplex**
April 23, 2005

everybody seems to know or assume that the quote is from Lubos Motl.
I didnt happen to see it on his blog or on the web anywhere.
can anyone give a link to where the quote can be found?

thx

29.
April 23, 2005

Is Lubos starting to doubt? Is he experiencing a conversion moment?

30. **ALejandro**
April 23, 2005

On other hand, excessive landscape-bashing can derive on empiricism-bashing.

31. **Robert**
April 23, 2005

Lubos must be gratified: (sincere) quotation is the sincerest form of flattery

32. **Simplex**
April 22, 2005

I guess Cumrun Vafa (basically because I admire him and this quote is admirably honest)

33. **FirstGuess**
April 22, 2005

Lubos Motl. These are remarkable thoughts indeed.

LHC Startup Scenarios

April 25, 2005
Categories: Uncategorized

Everyone in the particle physics community is avidly awaiting the startup of the LHC accelerator at CERN, scheduled for 2007. A new preprint by Gianotti and Mangano entitled [LHC physics: the first one–two year(s)] gives some idea of what to expect.

The design luminosity for the LHC is about $10^{34}cm^{-2}s^{-1}$, which is about 100 times the current luminosity of the Tevatron. Current plans are to first cool down the machine in spring 2007, followed by commissioning single beams over the next few months, with first colliding beams in the second half of 2007. During 2007, most effort will be devoted to commissioning the machine, followed by a shutdown for a few months. A seven-month long physics run at luminosities of up to 2 x $10^{33}cm^{-2}s^{-1}$ will take place during 2008. This is 20 times the current Tevatron luminosity and the Tevatron seems to be averaging a total of about 15 pb^{-1} per week, so one could expect a total luminosity of up to about 10 fb^{-1} to be collected during 2008. This is probably much too optimistic. Experience with the Tevatron when it was turned on at the beginning of its latest run was that for quite a while it was running at only a tenth of the hoped for luminosity. So perhaps 1 fb^{-1} during 2008 is a more realistic expectation.

According to Gianotti and Mangano, 1 fb^{-1} will be enough to see squarks and gluinos at masses of up to about 1.5 Tev. Seeing the Higgs is more demanding, especially if its mass is low. If its mass if above 180 Gev, it should require 5-10 fb^{-1}, if it is just above the LEP limit (114 Gev) it is likely to require more like 20 fb^{-1}.

Personally I think it's quite unlikely the LHC will be seeing supersymmetric particles, so, of the things it is looking for, it will require good luck to get the data required to see the Higgs during 2008. Even if this does happen, I'd guess that analyzing the data would take us into 2009. If the LHC has trouble getting anywhere near design luminosity, things could take longer. Of course everyone hopes that something completely unexpected will be found. If this is dramatic enough, maybe there will be some exciting news in 2008.

Comments

1. **D R Lunsford**
   April 27, 2005

   TL – fun paper!

   -drl

2. **Thomas Larsson**
   April 27, 2005

   Completely off-topic, but a nice historical paper anyway: [physics/0504179]. From the introduction one deduces that Ivan Todorov is not a big fan of string theory. But we knew that anyway, from some comment in one of Bert Schroer's assaults long ago.

3. **Juan R.**
   April 27, 2005

   Is there some theoretical posibility for a observed violation of SM in the LHC?

   Some time ago I read about the posibility for a four family!!

4. **D R Lunsford**
   April 26, 2005

   Neutrino experiment in Minnesota [mine..]

   -drl

5. **Peter**
   April 26, 2005

   Hi JC,
   As another commenter mentioned, I've written about this before in various places. Basically the minimal supersymmetric extension of the standard model (MSSM) is much more complicated and ugly than the standard model, introduces nearly 100 new free parameters, generically has problems with flavor-changing neutral currents and proton decay. The only real argument for it is the coupling constant unification calculation, and that's not that convincing. The popular "hierarchy stabilization" argument never seemed very convincing to me (and has technical problems besides, see the so-called "mu problem". Many of the landscape artists have already happily abandoned the idea of using supersymmetry to stabilize the hierarchy. Now of course anthropic reasoning explains all such things.

6. **Alejandro**
   April 26, 2005

   "If its mass if above 180 Gev, it should require 5-10 fb-1, if it is just above the LEP limit (114 Gev) it is likely to require more like 20 fb-1."

   This already means exciting times if the Higgs sector is not minimal. Say there is a particle at exactly 246 GeV… people would hail it as "the Higgs" and then when going down successive particles of the sector should appear. Funny.

7. **Alan R.**
   April 26, 2005

   Until Peter answers JC's question, readers might want to look at an earlier posting of Peter's entitled, "Attack on the Main Argument for Supersymmetry."

   http://www.math.columbia.edu/~woit/blog/archives/000025.html

   The fact that I can pick out this old citation off the top of my head tells me I'm probably spending too much time at this site, but I enjoy it!

8. **JC**
April 25, 2005

Peter,

What are your main objections to supersymmetric extensions to the standard model?

Ignoring GUTs, my main objection is that SUSY seems to be a symmetry that is just imposed by decree, with very little to no convincing experimental basis. The picture of the coupling constants converging to a "possible" single point with SUSY, isn't entirely convincing. All the arguments I've heard of over the years used to justify SUSY seem to be various aesthetic and/or theoretical "beauty" types of arguments, such as the coupling constants "converging" to a single point.

IF the universe was truly following some theoretical "beauty" type of principle, then why didn't the experiments confirm the simple SU(5) GUT model? (This is one counterexample to the many silly theoretical "beauty" types of arguments).

50 Years of Yang-Mills Theory

April 27, 2005
Categories: Book Reviews

There's a new book out, entitled 50 Years of Yang-Mills Theory, edited by Gerard 't Hooft. It contains some excellent review articles about topics related to Yang-Mills theory, together with short introductions by 't Hooft. Many but not all of the articles have already appeared at the arXiv as preprints.

The book begins with an article by DeWitt, unfortunately unfinished at the time of his death, about the space of gauge fields. 't Hooft's introduction and DeWitt's historical comments makes clear that "Fadeev-Popov" ghosts really should also have DeWitt's name attached to them. The full Faddeev-Popov paper is included in the book, a good idea since I don't think it was ever published. It appeared in Russian as a Kiev preprint in 1967, was translated into English and appeared as a preprint in 1972. While looking for information about this paper on the web, I noticed that Fermilab has put up scanned versions of their preprints, which is useful for the ones from the seventies and eighties that predate the arXiv.

There's an excellent review of the "Higgs mechanism" by Englert, where again Englert's name deserves equal time with that of Higgs. This paper has appeared as a preprint. Steven Weinberg contributes an interesting review article about the making of the standard model and his role in it. There are three articles related to renormalization of Yang-Mills: a detailed one by the master himself ('t Hooft), a mystifying one about Koszul complexes by Raymond Stora, and one about Slavnov-Taylor identities by Carlo Becchi.

Steve Adler has a long article about the history of what is now known as the "Adler-Bell-Jackiw" anomaly, and Jackiw has one about various topics related to Yang-Mills theory that he has contributed to, including anomalies, Chern-Simon terms, and gravitation. There's also an article by Frank Wilczek, mainly about asymptotic freedom, and one by Alexander Bais about magnetic monopoles in Yang-Mills theory.

On the non-perturbative side of things, there is Alexander Polyakov writing about string theory and confinement (he thinks string theory needs to have its head examined, see an earlier posting here). Pierre van Baal contributes a very interesting article on "Non-perturbative Aspects of Gauge Fixing", Michael Creutz a mainly historical article about lattice gauge theory. Peter Hasenfratz writes about chiral symmetry on the lattice. Both he and Creutz note that, while progress has been made, handling chiral gauge theories on the lattice remains somewhat problematic, so there is still no really satisfactory non-perturbative version of the electroweak part of the standard model.

Alvaro de Rujula has an entertaining discussion of events surrounding the "November Revolution" in 1974. Finally, there's a review article about supergravity by Peter van Nieuwenhuizen, and one by Witten reviewing the twistor space formulation of perturbative Yang-Mills amplitudes. Witten's article doesn't seem to have appeared on the arXiv (although there is a new review article by Cachazo and Svrcek which covers this material and much more).

Comments

1. April 28, 2005

   noninteracting spin-2 isn't gravity either

2. April 28, 2005

   massive spin-2 isn't gravity

3. **D R Lunsford**
   April 28, 2005

   To me, spin 2 connotes a background.

   -drl

4. **JC**
   April 27, 2005

   drl,

   What's a good counterexample, where spin 2 is NOT gravity?

5. **D R Lunsford**
   April 27, 2005

   Weinberg has the best line for years – "radial and azimuthal physicists" – ROFL!

   I was somewhat confused by the SG review. Apparently everyone just assumes spin 2 "is" gravity.

   -drl

6. April 27, 2005

   Off-topic, but I thought I should mention it: The BBC's "The Changing World" (broadcast here by PRI) is doing an hour tonight on the effects of visa restrictions on foreign researchers in the U.S.

   In the same vein see Sean Carroll's recent post.

Road Trip From Hell

April 28, 2005
Categories: Uncategorized

According to a new article in New Scientist entitled The Theory of Everything: Are we nearly there yet? (unfortunately not available for free on-line), "The hunt for the theory of everything is turning into a road trip from hell – and don't even ask who's reading the map." The article quotes Susskind and Weinberg as believing in the existence of a multiverse, even if this means that "all we can hope for from a final theory is a huge range of possibilities".

Witten is referred to as a "string grandee", and quoted as saying about string theory "More work has always given more possibilities – far more than anyone wanted… I hope that current discussion of the string landscape isn't on the right track, but I have no convincing counter-arguments." He's welcome to my counter-arguments if he wants them: there's not the slightest evidence for the landscape scenario pseudo-science, it's incredibly ugly, not based on any kind of well-defined theory, explains nothing, and holds out no reasonable hope of ever explaining anything.

The article goes on to discuss the wishful thinking surrounding "M-theory", quoting Witten as believing that M-theory may have a unique solution that fits our universe and explains the constants of the standard model. "Hope springs eternal" he says. Somebody seems to have given the writer the idea about M-theory that "theorists can prove that it exists as a mathematical construction, but they can't actually write down its equations and there is no clear route towards doing so", which is only true under a peculiar interpretation of the words "prove", "exists", and "it". Lisa Randall is quoted as follows about M-theory: "We probably need fundamentally new principles… it's not hopeless, but it's going to require some deep new insight that we don't really have." She promotes her own work with Mukohyama on an alternate explanation of the cosmological constant.

The only person quoted in the article as thinking that there may be any problem at all with the way particle theory has been pursued for the last twenty years is Lee Smolin, who takes the absolute lack of any experimental evidence for string theory as a sign that the field may be off on the wrong track. He notes that "If you look back over the last 200 years, every decade or two there's a dramatic advance, people always understand something new that couples theory and experiment… I suspect there is some right question that we're not asking."

Comments

1. **Juan R.**
   May 8, 2005

   Stephen,

   I don't understand your question very well. I don't know if you are claiming for some fractal behavior.

   In canonical gravitodynamics, gravitation depends of the scale with a parameter that is different for macro, astro, or cosmo scales.

   I still don't understand well that parameter but it appears compatible with certain cosmological requirements like expansion.

   Unification will be no achieved by usual high-energy methods. Sure!!

   My approach is not based in geometry or topology, just in AAAD.

2. **Stephen**
   May 3, 2005

   Peter and Thomas,
   Could it be that the critical mistake that is being made in the Unification theories is the assumption that gravity is not analogous to van der waals forces and is only representable at *all scales* as geometry/topology?
   I am aware that Sakharov's initial idea was fraught with problems, but is that sufficient to dismiss the entire class? I have reasons to think that current research in this direction is naive, cf: http://www.calphysics.org/articles/zpf_apj.pdf, but again my question remains.

3. **Simplex**
   May 2, 2005

   Stephen wrote: *How many more epicycles are going to be added to this theory before it collapses into some kind of Scholastic argument?*
   Might it be possible that a lot of money and brain power is being wasted on this theory that doesn't seem to be able to make any measureable predictions?

   Good questions! My personal guess is yes, a lot of time and research talent is being misdirected. To a large extent because leaders in the field have not been sufficiently frank and forthcoming. And then the hype—which distorts public and political support for science.

4. **Stephen**
   May 2, 2005

   T. Larsson wrote: "…we could have seen e.g. a light Higgs, proton decay, muon g-2 deviation, permanent electric dipole moment, WIMPs, and probably many other things…";
   Would it not be more correct to write "should" instead of "could". How many more epicycles are going to be added to this theory before it collapses into some kind of Scholastic argument?
   Might it be possible that a lot of money and brain power is being wasted on this theory that doesn't seem to be able to make any measureable predictions?

5. **Thomas Larsson**
   April 30, 2005

   There are many potential signals of supersymmetry, some of which should already have been triggered. Apart from direct discovery of sparticles, we could have seen e.g. a light Higgs, proton decay, muon g-2 deviation, permanent electric dipole moment, WIMPs, and probably many other things that I don't know about. An arxiv search for the keywords "tuning supersymmetry" gave 35 hits during the past year, the most recent one being hep-ph/0504246. Let me quote from the introduction

   "Another problem comes from the fact that LEP II did not discover any superparticles or the Higgs boson. In most supersymmetric theories, this leads to severe fine-tuning of order a few percent to reproduce the correct scale for electroweak symmetry breaking. This problem is called the supersymmetric fine-tuning problem".

I am no expert on SUSY phenomenology and never claimed to be. But if the experts say that there is a fine-tuning problem, I see no reason to doubt that.

It was, I believe, the need for SUSY fine-tuning that motivated the introduction of split supersymmetry. For almost 20 years, Witten used to say that string theory makes one prediction, supersymmetry (and one postdiction, gravity), but I haven't heard him make this claim for a couple of years. One cannot help noting that string theory apparently stopped predicting SUSY once this claim became accessible to experimental tests.

6.
April 29, 2005

SUSY requires fine tuning? Are you refering to the mu problem or the flavor changing neutral current problem?

7. **Peter Woit**
April 29, 2005

Sure, there was a lot of work on unification before 1984, including work on GUTs going back to Georgi-Glashow in 1974. But this all involved pretty well-defined QFTs, so people could fairly quickly see if realistic models that predicted anything were possible. GUT models made predictions about proton decay that were falsifiable, and the models were falsified in relatively short order. Witten showed fairly quickly that supergravity Kaluza-Klein couldn't produce a chiral spectrum (although now it has been revived as "M-theory" using singular compactification spaces).While Witten did a lot to promote supersymmetry, it was only one of many different research programs people were pursuing.

What changed in 1984 is that an overwhelmingly large number of people started working on string theory, abandoning and killing off many other research programs. String theory was and is so ill-defined that more than 20 years later no one can agree on what it is or extract any kind of prediction about it. This new phenomenon of the field being completely taken over by something this incoherent is what I had in mind in my remarks about how things changed in 1984.

8.
April 29, 2005

Peter,
Unification was big business before 1984. The name was supergravity Kaluza-Klein. It became really big business when Witten got into it around 1978. That's also when he introduced supersymmetry/supergravity in the USA. It was mainly a European thing till then.

9. **Thomas Larsson**
April 29, 2005

This pessimism about string theory over the last year or two, seems to be quite different and more ominous than what happened during the temporary lull string theory experienced around 1990 (before D-branes, duality, AdS/CFT, etc …).

Another reason why the present situation is much worse than 1990 is that we know more now. In particular, we know that the cosmological constant is positive (so AdS is ruled out) and that supersymmetry requires fine-tuning at the percent level (which in some sense means that the odds that SUSY is realized in nature is down to the percent level). Since SUSY and a non-positive CC are the main soft-predictions of string theory, it seems rather problematic that both are ruled out by experiments. Not surprisingly, it is precisely these two results that have triggered the recent anthropic excuses.

Hence I disagree somewhat with the premise of this blog. I don't think that string theory is not even wrong, but rather that it in fact *is* wrong.

10.
April 29, 2005

hey she's a fox

11.
April 28, 2005

Hi
Peter and other interested folks,
you might want to look at this talk
"Can cosmology test Stringly physicis" by Hiranya Peiris and one of her collaborators
is Brian Greene
See http://www.stsci.edu/institute/center/information/streaming/archive/HubbleFellows2005/HubbleFellows2005Overview

12.
April 28, 2005

Peter – nevertheless I thought it was interesting to get enough involved to see what was wrong. It was great fun to read all those papers.

-drl

13. **island**
April 28, 2005

I assume Peter doesn't mean that to mean that you can't challenge my statement.

… and don't feel bad, Michael, Dirac apparently didn' feel that he had need to consider that you have to condense Einstein's static vacuum energy over a finite region of space in order to achieve positive matter density and pressure, or his hole theory might work a lot better… 😉

14. **Peter Woit**
April 28, 2005

I've just deleted a bunch of comments from people insulting each other. Please stop doing this here and save me the time of having to delete these comments.

15. **island**
April 28, 2005

Lisa Randall… "promotes her own work with Mukohyama on an alternate explanation of the cosmological constant."

lol

16. **island**
April 28, 2005

Einstein didn' know that particle creation in his finite closed spherical near-flat positively curved static model, affects expansion.

"We probably need fundamentally new principles… it's not hopeless, but it's going to require some deep new insight that we don't really have."
-Lisa Randall

"I suspect there is some right question that we're not asking."
-Lee Smolin

"It never hurts to look in the basment"
-Danny Ross Lunsford

"I think it'll be something that we've all missed."
-John Baez

17. **Peter Woit**
April 28, 2005

In the late eighties it was clear there were potentially a large number of possible string vacua, but they all had the problem of unfixed moduli parameters. So, one could hope that whatever mechanism was found to fix these would eliminate all but a small number of possibilities. More recent work such as KKLT seems to show that you can fix the moduli, but the mechanism for doing this just makes things much worse.

In the late eighties, string theory was very popular, but still pretty new. People didn't have a huge amount invested in it, and if the 10^500 stabilized vacua had shown up then, quite possibly most would have been willing to abandon string theory altogther. Now you've got a whole field filled with people who have devoted either their whole careers or at least 20 years to working on string theory. What's remarkable is how they show no willingness to admit failure in the face of utterly overwhelming evidence.

18. **JC**
April 28, 2005

This pessimism about string theory over the last year or two, seems to be quite different and more ominous than what happened during the temporary lull string theory experienced around 1990 (before D-branes, duality, AdS/CFT, etc …). Despite the estimates of 10^100 possible Standard Model-like string vacuum states, nobody in the early 1990's was really seriously talking about using the anthropic principle. This time around it seems to be the "serious" use of the anthropic principle in string theory, which has been producing more ominous signs of decline in the field.

19. **Peter Woit**
April 28, 2005

Hi JC,
I don't think it's quite fair to label Einstein's efforts to extend GR to a unified theory as "semi-crackpot". They turned out not to work, but were not an unreasonable thing to work on, and he didn't go around claiming any great success. Until 1984 there were always plenty of people running around doing various incoherent work for which they made grandiose claims about unifying everything, explaining the big bang, etc. But serious people pretty much just ignored them since they weren't getting anywhere and there were plenty of more promising things to think about. The lack of any unexpected new experimental data, together with the lack of any good new theoretical ideas, is what has changed since 1984. Now the kind of pseudoscience that everyone used to scorn and ignore is being promoted by many of the most prominent people in the field.

20. **JC**
April 28, 2005

Peter,

How common were persuits of "theories of everything" in physics, during the time period after Einstein died and before string theory? It seems like most of Einstein's life at Princeton was largely in pursuing various semi-crackpot "unified field theory" ideas which ended up as failures.

Frist Filibuster

April 28, 2005
Categories: Uncategorized

For the last couple days students at Princeton have been protesting the Republican's plan to invoke the "nuclear option" and stop Democrats from filibustering a small number of Bush's judicial nominees. This protest has taken the form of organizing a "filibuster" in front of the Frist Campus Center at Princeton, which was underwritten by Senator Bill Frist (Princeton '74). Today Edward Witten and his wife, physicist Chiara Nappi, have joined the protest. I can't tell what Chiara is reading from, but Ed is using a bullhorn to regale the crowd with passages from Introduction to Elementary Particles by David Griffiths.

Many thanks to my correspondent who wrote to me today to tell me about this.

Update: It seems that Josh Marshall of the Talking Points Memo weblog had something to do with this. Ed and Chiara got awarded a Privatize This! Talking Points Memo t-shirt, and there are still two more available.

Comments

1. **JC**
April 30, 2005

   On the other side of the coin in an unrelated way, I've heard of many stories about graduate admissions committees in less "scientific" areas, from folks who have served on them. In some departments like economics, business, finance, law, etc … many folks have mentioned they actually prefer applicants with a math/science/engineering background, than a liberal arts or social science background. This is the case even if the applicants with the math/science/engineering background have very little to no background knowledge of economics, business, finance, law, etc …. My best guess is that they must think that folks with a math/science/engineering background have an easier time picking up a new field of study relatively quickly. (I don't believe admissions committes are doing this because of a lack of "qualified" applicants).

   I haven't heard as much about the coverse case, with somebody with a liberal arts or social science background applying for graduate school in math/science/engineering. The closest cases I've heard of over the years were folks who either minored in math, and/or took some more higher level math courses beyond freshman calculus, linear algebra, and statistics. A few other cases I've heard of were folks who worked in a university or government lab on some research projects, and were able to get some reference letters and/or even their name on some papers published in a half-decent peer-reviewed journal.

   Other than that, it seems to be more of a longshot in going from liberal arts/social science to math/science/engineering, than the other way around.

2. **Peter Woit**
April 30, 2005

   I don't personally know anything about how Witten was admitted to Princeton, but I'd guess he took some courses at Brandeis in math and physics, did well in them and did very well on his GREs. His father was a physicist, so he probably picked up a lot of math and physics growing up. From looking at a lot of graduate school admission folders, I can say that the kind of thing one looks for is

   1. some excellent letters of recommendation

   2. excellent performance in at least a few higher level courses

   3. excellent standardized test scores.

   Its quite possible Witten managed to put together these for his application, even though he majored in a non-scientific subject.

3. **anon**
April 30, 2005

   Yah, I'm sorta curious how that's possible too. (the previous poster's question)

4. **quantumhobby**
April 30, 2005

   How could Ed Witten have been accepted into the applied math program at Princeton without having majored in math or physics as an undergrad? I thought you needed to have recommendations and a demonstrated potential for research to get into a top-notch grad program in the sciences. I majored in business as an undergrad and I always assumed it would be impossible to get into a good graduate science program, coming from that background. I know Witten is a brilliant guy, but it seems like that must have been difficult, even for him.

5. **Alejandro Rivero**
April 30, 2005

   should we link this thread to the previous comments of Lubos blog about the left-windy side of physics?

6. **Peter Woit**
April 30, 2005

   I believe that Witten graduated from Brandeis in 1971 with a major in history and a minor in linguistics. He published an article in the Nation in 1968 when he was 17, but I don't think he ever worked for them. He did work on the 1972 McGovern campaign, then entered graduate school at Princeton in 1973 (he started in the applied math program, soon switched over to the physics department).

7. **R Gambi**
April 30, 2005

   Wilczek's contribution was pretty cool. He read a few excerpts from Einstein and Minskowski's original papers on relativity.

8. **anon**
April 30, 2005

I don’t know if this is true, but I heard Witten orginally graduated from Brandeis with a degree in Journalism (or a related field?), and only after a brief stint with the Nation, did he choose to become a physicist. In a way, it’s not terribly surprising, but still cool. (Is this story true, or just urban legend?)

He should do more of this stuff. Very cool guy.

9. **Alan R.**
April 29, 2005

Frank Wilczek has apparently joined in, too. His name appears on the list of speakers.

http://www.princeton.edu/~petehill/filibuster.html

10.
April 29, 2005

Does this make them liberals, or merely respectful of Senate traditions?

11.
April 29, 2005

Why didn’t he read from GSW? Too embarrassed?

12. **William Lynn**
April 28, 2005

Why is it not referred to as the “Nucular” option?

13. **D R Lunsford**
April 28, 2005

AWRIGHT ED!

“Somethin’ happenin’ here – what it is ain’t exactly clear”

-drl

14. **Chris Oakley**
April 28, 2005

Reminds me of this, which inspired a passage in *Brideshead Revisited*, although Witten’s recitals are IMHO of more value than Eliot’s *The Waste Land*. I have to say, though, that I probably would have chosen Weinberg, Vol. I in preference, but this is a quibble.

15. **tgl**
April 28, 2005

He must be demonstrating the nuclear option.

Wacka, wacka, as Fozzie The Bear would put it.

Lubos Leashed

April 30, 2005
Categories: Uncategorized

Lubos Motl has taken to signing some of his postings with "leashed", and Capitalist Imperialist Pig has speculated that "My dark suspicion is that he might have gotten caught in a PC violation in the Summers Affair, forcing him to do a T reversal to save his Lorentz invariant m ass". Lubos wrote in to tell him "Unfortunately your intuition is perfectly correct, but I am not sure whether your imagination is big enough to imagine the scale."

I have no idea who is responsible for the leashing of Lubos or what the reason for it is, but I figured this meant his blog would stop featuring the right-wing ideological political commentary he's fond of. But today he has a posting about the Frist Center "filibustering" in which he says "I currently do not enjoy the freedom to tell you what I think about these things." Actually he manages to make it pretty clear what he thinks about these things.

This leashing of Lubos is too bad, especially since I was finding myself more and more in agreement with his postings (not the ones about politics, but we seem to agree about the Landscape), and generally think the First amendment gives everyone the right to make a fool of themselves with crazed political rants if they feel like it. Lubos's blog has also played another important role for me. Whenever people won't believe me that string theorists can be smart and well-informed, but still crazed ideologues, all I've had to do is point them his way.

Comments

1. **Peter Woit**
   May 9, 2005

   Hi JC,

   Presumably physics theory groups worry about how whoever they hire will affect their grant, but this depends on the department's situation, and I only have direct experience with math departments.

   In my experience in hiring decisions the question of grants hasn't come up. The general assumption is that the criteria being used to hire people is similar to the criteria used by grant agencies, so the kind of person we're trying to hire is the kind of person who should do well in trying to get grants. In math departments like Columbia, few of the grad students are funded by grants, most are funded as teaching assistants by the university. So grants aren't that important: they aren't needed to fund labs and they aren't really needed to fund graduate students. The university and the department do make money off grants, but the amounts are not large or crucial to the department.

2. **JC**
   May 9, 2005

   Peter,

   When it comes to a physics department hiring new faculty for assistant professor jobs, what is the exact criterion for hiring new theorists?

   From stories I've heard over the years about experimentalists, I got the impression they're mainly interested in folks who they think will have a good shot at maintaining consistent grant funding year after year. On the surface I can understand perhaps why this is the case for an experimentalist, considering they're more or less out of business if their funding grants are not renewed. Kind of hard to pay for new lab equipment, facilities (ie. telescope time, accelerators, etc …), postdocs, etc … if one has no money. I remember several experimental particle guys mentioning they spend time writing a number of grant proposals every year or so.

   I haven't heard as much about what faculty search committees look for when they're hiring a new theorist, other than the obvious criteria of looking for somebody they think will have a good shot at maintaining consistent grant funding and not losing it so easily.

   Is this also true for math departments when they're hiring new faculty?

3. **Peter Woit**
   May 9, 2005

   Hi JC,

   Actually I think some mixture of 3. and 4. is a perfectly rational reason why people take these jobs. You get to work with the most prominent people in the business, and if things go well you have a bit of an inside track at a permanent position at Harvard or Princeton. Even if you're not going to get tenure there, you're in an excellent position for getting a permanent job elsewhere.

4. **JC**
   May 9, 2005

   Peter,

   Why do some people take junior faculty jobs at places like Harvard, Princeton, etc … in the first place, when they know very well that their chances at tenure are slim to none?

   Offhand, the few reasons I can think of are ones like:

   (1) It was the only job they were offered.

   (2) They are "overconfident" of their own abilities and/or they have a huge ego.

   (3) They see it as a means to an end, where Harvard/Princeton is just a step up along the way to a permanent tenured job somewhere else.

   (4) They produced a paper or two which got hundreds, if not thousands, of citations in a very short period of time, and that they're willing to "roll the dice" at the tenure game at Harvard, Princeton, etc …

   The only case which appears to have a half decent chance at getting tenure at Harvard, Princeton, etc … would perhaps be (4).

5. **JC**

May 8, 2005

Peter, Quantoken

The folks I know of who went the community college route all mentioned that their teaching loads were so heavy that they had very little to no time to do their own original research. For a few of these folks their mentality towards research became “why bother?”, when publishing original research papers wasn’t going to help them much anymore in their careers.

I’ve noticed some tenured professors who became “burned out”, eventually took on the same “why bother?” mentality towards research. One day they eventually reach the point where they don’t even bother publishing research papers anymore, and gradually become “deadwood”.

On the surface I can perhaps understand why some folks gradually fall into the “why bother?” mindset after they get tenure. During a postdoc, there’s the goal of getting an assistant professor job. During an assistant professorship, there’s the goal of getting tenure. Folks who are after these goals usually keep their eye on the “8-ball”. Once somebody gets tenure, what becomes the “8-ball” besides higher professor ranks? Teaching the same sorts of courses year after year, seems to become less and less interesting each successive time, especially freshman undergraduate courses. Some folks may start to question whether their research is going to mean anything in the long term, considering a large number of published journal papers become largely forgotten with the passage of time. How many folks today still regularly read papers from 100 year old physics journals, besides science historians? If string theory ever falls out of favor, I would imagine many string papers over the last 20 years will become largely forgotten with the passage of time. These days how many people still regularly read papers on the bootstrap analytic S-Matrix theory stuff? I would imagine that for every one “superstar” like an Einstein, Dirac, Schrodinger, Hilbert, etc … there must be easily thousands of researchers who were largely forgotten with the passage of time.

6. **Peter**
May 8, 2005

Hi JC,
At places like Harvard and Princeton, most junior faculty don’t get tenure, and the department gets most of its tenured people by hiring stars away from other places. I remember talking to someone I knew who had taken a junior job at Harvard, at the point when it was clear he couldn’t stay there and had started looking for jobs elsewhere. He told me he wasn’t sure whether it had been a good idea going to Harvard, because when he was just out of grad school, people may have thought he might be the next Witten, but after several years out, it was clear he wasn’t. So he thought he might have done better to go somewhere else where he would have been likely to get tenure. But things worked out for him, and he’s now a prominent member of the community tenured at a well-known place.

So, no matter how he behaves, I don’t think Lubos is a shoo-in to get tenure at Harvard, but he’s likely to be able to get a good permanent position elsewhere no matter what. Similarly for Sean Carroll: I was surprised to hear he didn’t get tenure at Chicago, but I’m sure he’ll have other offers elsewhere.

The people who are in trouble are the ones who don’t get tenure at less well-known places where one would normally expect tenure to be likely. They then have a much tougher time finding a permanent position, and may not be able to get one at a place where it is possible to do much research. Teaching a full load at a community college is hard work, and if you’re doing that it’s not easy to find the time and energy to do much original research. You also typically aren’t in a good environment in terms of colleagues, seminars, students etc. for research.

7. **Quantoken**
May 8, 2005

JC said:
“a few folks of the latter case I’m aware of did decide to go into teaching community college for a few years, but they **didn’t really publish any research papers** afterwards.”

Why so? Is that 1.They **do not want to** write papers any more, or 2. they **are unable to** write papers any more, or 3. their papers **unable to be accepted** for publication? (because their institution affiliation doesn’t seem impressive?)

A hundred years ago, a little 26 year old patent office clerk who also tutored high school kids half time for a living could publish papers on one of the best academy journals. Today, instituion affiliation is every thing.

I do NOT think Lubos will be denied tenure at Harvard. As smart as he is, I do not think he will **sit and wait** like Sean did till the day to be told that his tenure is denied, instead he will **beat the clock and deny Harvard a tenureship and find another job first, before he could be turned into a sitting duck.**

You have a much better chance if YOU take it to turn down a position at a prestigious institution to get a better job some where else, instead of you be turned down. Sean could have done that! There are plenty of good excuses to leave U of Chicago: It’s too cold a place to live, or U of Chicago is not good enough to keep me and I want to be in Harvard, I am not paid good enough, etc. etc. But as he sitted to wait for the UofC deny, his choices are now limited and he will have a hard time trying to explain why he would be fired, something his new boss must be curious to find out.

Fire the boss before the boss could fire you, Lubos :-)!

Quantoken

8. **JC**
May 7, 2005

Peter,

When somebody is denied tenure at one university, what is the likelihood of them finding another academic job (with chances at tenure) at another university?

Over the years I’ve noticed some folks who didn’t get tenure at places like Harvard, Princeton, etc … seemed to have been able to find another academic job at another university. For folks who didn’t get tenure at a less prestigious university (ie. state universities, 4-year liberal arts colleges, etc …), few seemed to have found another “half decent” academic job. Without mentioning any names, a few folks of the latter case I’m aware of did decide to go into teaching community college for a few years, but they didn’t really publish any research papers afterwards.

9. **Anonymous**
May 7, 2005

One thing is for sure. Should Lubos ever gets denied tenure, there won’t be any huge outpouring of sympathy for him, unlike Sean Caroll.

10.
May 7, 2005

Weather forecasts are done by simulations. Lattice QCD calculations are done by Monte Carlo simulations. etc.

11. **JC**
May 6, 2005

I wonder what would happen if academic tenure ceases to exist in America by congress passing a new law, where only existing tenured professor jobs remain while no new tenured jobs are produced in the future. On the surface one can perhaps see this being done by the republican congress, as a way of striking back at the "liberal left" in academia.

12. **Chris Oakley**
May 6, 2005

"Shut up, or I'll set Lubos on you." – That would have stopped Hoppe's persecutors in their tracks.

13. **Not a Nobel Laureate**
May 6, 2005

Academic freedom in the US is mostly an illusion.

"My Battle with the Thought Police – Hoppe"

http://www.mises.org/story/1792

The irony is that the academic left demands that same type of cultural conformaty as the religious right when it comes to their own set of beliefs.

A plague on both their houses.

As for global warming, it's become an quasi-religious meme that cannot be questioned to far too many people rather than a hypothesis. The fact that it's based on simulations should be the first warning flag.

14.
May 4, 2005

D R Lunsford May 2, 2005 05:53 PM said:
We should stick to physics here. LM isn't exactly a fascinating topic.

and again D R Lunsford May 4, 2005 09:46 AM
Interesting penta-quark news

http://www.sciencedaily.com/releases/2005/05/050502203532.htm

but the topic here is LM. the lead post is "Lubos leashed".
I think there is something to be learned from watching Lubos, as a sensitive compass needle.

Now (immediately after the leash episode) he points to Albert Einstein Institute at MPI-Potsdam where the director of string research is Hermann Nicolai.

http://motls.blogspot.com/2005/05/e10-billiards-and-m-theory.html

LM has some chips because of his visibility. IMO he won't come out a victim, nor be easily repressed: too agile.

Suppose one were to ask what is the top center for string research which is immune to the Landscape disease. If Harvard should be infected, to such an extent that one cannot criticize what LM calls the "haystack", where would a bright person want to go?

15. **D R Lunsford**
May 4, 2005

Interesting penta-quark news

http://www.sciencedaily.com/releases/2005/05/050502203532.htm

-drl

16. **ksh95**
May 4, 2005

"Sean Carroll was…just told that his tenureship was denied at UofC

Wow, that's horrible news. I wish him the best of luck in his upcoming search.

17. **Quantoken**
May 4, 2005

Sean Carroll was leashed, too. He just told that his tenureship was denied at UofC. That shocked many people. And I wish him the best in finding another job.

Peter would you not want to comment on that a bit? Here is a guy who wrote books and published a lot of papers, and he is still not good enough to earn his tenureship 12 years after his Ph.D. What's the odd for some one like Lubos on the expectation of tenureship? I guess this really gets a lot of none-tenured people worried.

Does it have to do with politics? Sean is an openly Atheist. Could that has something to do with it? No body knows how tough it is if you are one refusing to believe in God in this country.

Quantoken

18.
May 3, 2005

Why are you mistaking a superstring department for a physics department? Lubos realized his mistake and attended a real physics talk for the first time in his life.

19. May 3, 2005

    Lubos, if you are out there reading this, can you tell us what exactly is going on? I'd hardly expect this sort of thing in a PHYSICS DEPARTMENT

20. May 3, 2005

    Peter said:
    … Whatever it was, it doesn't seem to have slowed him down much since he is still posting political commentary not especially distinguishable from before. He does seem to have stopped criticizing the Landscape, maybe that's what all this is about.

    If so it is bad news. there are surprisingly few voices openly defending traditional scientific standards against attack from within

    Blank 10:26AM said:
    Isn't it strange that Lubos is being so secretive and tantalizing as to how he was "leashed" and by whom? And isn't it strange that he still posts his usual political views despite being "leashed"?

    I take that as an indication that the pressure came from someone in his own department.

    And why did he remove his "leashed" signature and other cryptic sentences like "I don't enjoy the freedom to tell you what I think"?…

    the quote about "don't enjoy the freedom" was at the end of some rather muted comment on a recent paper by Vafa et al called "Baby Universes in String Theory"

    the next poster sounds convinced by a smoke screen—a false explanation rumored by "Well-informed sources" in the department

    In response to Peter's
    "He does seem to have stopped criticizing the Landscape, maybe that's what all this is about."
    Blank 10:33AM said:

    *Well-informed sources 🙂 🙂 tell me that it was his climate stuff that finally went too far. Earlier versions of his latest post on that theme "raised ethical questions about the funding of climate research" — and what I've written is an extremely sanitized version….that was changed, with LM complaining that he was not allowed to say those things …. and now he's not allowed even to say that! I really doubt that his landscape stuff would result in any problems. People in this line are used to trashing each other's papers, no big deal. Though Eva Silverstein did blow a fuse, that's very exceptional. But raising questions about where money is going — well, that's *really* playing with fire.*

    I see, the real reason was his mentioning money (in the case of some climate research). It is normal to trash each other's papers. Eva did blow a fuse but that's very exceptional. Ha ha. Yes, it was really about funding in climate research.

    At least one subsequent poster seems to have bought the screen. The conversation turned to issues like ivy league liberals, Sean Carroll's views of women in science, and the superior analytical acumen of physicists.

    I think Lubos was getting out an important message about the Landscape school of pseudo-physics, that his language in his "Kennedy Landscape" piece was too effective, and that departmental authority, conceivably in the shape of Cumrun Vafa or someone equally august, stepped in. I think that we can expect anti-global-warming blogs as usual from Lubos, but nothing further that equals his "Kennedy Landscape" piece. I do not think the turn of events is humdrum (some have suggested it is boring to discuss these Motl matters) but, on the contrary, appalling.

21. **Arun**
    May 3, 2005

    There is nothing particularly "humanistic" or "scientific" in pointing out a logical fallacy. It also seems orthogonal to "aptitude for interpersonal relationships, social graces, understanding".

    Perhaps ksh95 should follow his/her own advice and not confuse "superior analytical acumen, for superior acumen in other more humanistic topics" (incidentally, physicists' superior analytical acumen is a bit of myth as well).

22. **ksh95**
    May 3, 2005

    "…I shouldn't speak for Sean Carroll, but what he has said is that the differences between men and women are not explanatory for the specific observed phenomena of the gender ratios in science…"

    Either way. My point isn't about Lubos or Sean. My point is, as physicists we should not mistake our superior analytical acumen, for superior acumen in other more humanistic topics.

23. **Arun**
    May 2, 2005

    I shouldn't speak for Sean Carroll, but what he has said is that the differences between men and women are not explanatory for the specific observed phenomena of the gender ratios in science.

24. **D R Lunsford**
    May 2, 2005

    We should stick to physics here. LM isn't exactly a fascinating topic.

    -drl

25. May 2, 2005

    ksh95 is very correct. It's sad, but true.

26. **ksh95**
May 2, 2005

Didn't your mom ever tell you…

" As brilliant as you are, you have no common sense"

Translation: An aptitude for Field theory is a very different thing than an aptitude for interpersonal relationships, social graces, understanding society…etc.

My mom clearly understands that free speech does not mean you have the right to challenge orthodox political or scientific views. Go to San Fransisco, scream I believe marriage is between a man and a women, and then try to open a corner store. Go to West Virginia, scream there is no god, and then try to find some fishing buddies.

Lubos was leashed…big surprise there!!!

As an assistant professor, in a physics department, in a flamingly liberal school, one can not expect to **PUBLICLY** attack liberal philosophy while simultaneously **PUBLICLY** declaring that the ayatollahs new theories are nonsense, and avoid consequences.

Brilliant, young, theoritician, completly lost when it comes to understanding people.

And just so you don't think I'm anti Lubos.

How did Sean Carroll convince himself that 4 billion years of evolution have resulted in a perfect equivilance between men and women (except for a few obvious differences).

A brilliant, theoritician, completly lost when it comes to understanding people.

It seems to me. At best social or interpersonal intelligence is completly uncorrelated to analytical intelligence. At worst social or interpersonal intelligence is negatively correlated to analytical intelligence.

27.
May 2, 2005

I have nothing against LM and wish him well.
I also strongly feel he should be allowed to express his views freely on physics and politics regardless of whether I agree with them or not.
Jumping between this blog and LMs there has been a lot interesting stuff debated and argued this past year. Nevertheless, I am getting really tired with all the conversation and attention that focuses upon him. Last week it was a debate about his publications for example. And I even am talking about him right now! But I think this is part of what is going on: the "leashing" has probably something to do with the fact that he has a very high profile among the online physics community and has now somehow become (inadvertantly) the voice and representation of Harvard's physics department. (If other Harvard physicists had blogs I guess this would balance things out though.) They probably want him to tone it down quite a bit.

Having been at an Ivy League school myself for a while I do know that you are there and are hired for the greater glory and image of the school more than you are to serve yourself and your own needs and views. LM is also probably realising that the US is not the land of the free and free speech it claims to be (at least not any more). I know young faculty can have a very hard time and are under a lot of pressure so I do wish him well and hope he gets through things ok if he is having a bad time. In the end though you have to be true to yourself and what you believe in and be in an environment where you are comfortable doing that and free to do that.

28. **island**
May 2, 2005

This is getting fanatically rediculous and ideological bias should be outlawed from science altogether.

Imagine how left-wing extremists would react to finding out that humans have a real practical function in nature that results from the growing "higher-level" physical "need" of our expanding universe.

http://www.taipeitimes.com/News/edit/archives/2004/09/30/2003204990

Imagine how right-wing fundie creationists would react to finding out that their "higher-power" is actually the second law of thermodynamics.

Science and humanity is doomed if it comes down to right-winged fanaticism vs. leftist liberalism

29.
May 2, 2005

Is the previous anonymous none other than Lubos himself? It certainly seems like it. If he really is Lubos, the whole "leashing" thing sounds extremely suspicious to me. If there is anything fishy going on with the funding of climate research, it's most likely to be coming from the side of global warming skepticism, but Lubos is on their side…

It certainly seems out of place for global warming researchers to "leash" a blogger with little credibility.

30.
May 2, 2005

I don't think it's no big deal to trash each others ideas. It may be the cultural norm in physics, but it can still be very hurtful, which is even the more so if the trasher is wrong.

And I don't think Silverstein blew a fuse. It seems more like she was defending herself from unfair attacks.

31.
May 2, 2005

"He does seem to have stopped criticizing the Landscape, maybe that's what all this is about."

Well-informed sources 🙂 🙂 tell me that it was his climate stuff that finally went too far. Earlier versions of his latest post on that theme "raised ethical questions about the funding of climate research" — and what I've written is an extremely sanitized version….that was changed, with LM complaining that he was not allowed to say those things …. and now he's not allowed even to say that! I really doubt that his landscape stuff would result in any problems. People in this line are used to trashing each other's papers, no big deal. Though Eva Silverstein did blow a fuse, that's very exceptional. But raising questions about where money is going — well, that's *really* playing with fire.

32.
May 2, 2005

Isn't it strange that Lubos is being so secretive and tantalizing as to how he was "leashed" and by whom? And isn't it strange that he still posts his usual political views despite being "leashed"?

And why did he remove his "leashed" signature and other cryptic sentences like "I don't enjoy the freedom to tell you what I think"? Isn't this a free country?

33. **Apeiron**
May 2, 2005

Which is scarier – that someone told him to cool it on his political beliefs or academic thoughts?

I think it's possible that someone advised him that his instantaneous trashing of papers and talks as soon as they came out within the small world of string theory physics is not how the game should be played.

I think he should behave as he sees fit, with an understanding that there are consequences to free speech as well – especially in closed communities like academia.

34. **Peter Woit**
May 2, 2005

I certainly support Lubos's right to say whatever he wants, but before protesting too loudly his "leashing" I'd like to know exactly what happened. Did someone tell him he should stop expressing right-wing political viewpoints? Or did they just tell him he was often making a fool of himself? Whatever it was, it doesn't seem to have slowed him down much since he is still posting political commentary not especially distinguishable from before. He does seem to have stopped criticizing the Landscape, maybe that's what all this is about.

35. **Arun**
May 2, 2005

I agree with Anonymous at May 2, 2005 06:53 AM that if Lubos is being leashed, his would be a case to defend strongly.

36. **Anonymous**
May 2, 2005

Well, I can't understand why Motl feels "leashed" (putting aside his psychological motivations for putting this show). And I think if people really believed in freedom of speech etc., if indeed he is serious in his being "leashed", his would be the case to defend vigorously. And that's precisely because his views are so obnoxious (he's basically the equivalent of a party hack here; old habits die hard).

So, if he indeed is feeling constrained in any way, I think this would be the situation to strongly defend his rights (however repugnant his stances). Especially relevant for Peter to do that – would be nice for Peter (he mentioned it briefly in this post though) to have a post staunchly supporting Motl's right to say whatever he has to say, plainly and unequivocally.

37. **JC**
May 2, 2005

Offtopic, but about today's (May 2, 2005) appearance of one new hep-th preprint. This sure seems odd for the otherwise normally "busy" hep-th board. Has there been any other days in the past which only had one, or even zero, new hep-th preprints?

(I don't believe this is a sign of reckoning).

38. **Simplex**
May 1, 2005

[i] But who would feel so threatened by a lone ravings of a scientist with no political clout?[/i]

If Lubos has been subjected to pressure or warnings about the content of his blog, I would guess it is not because of his reactionary politics but rather because of this:

http://www.math.columbia.edu/~woit/blog/archives/000184.html

that is, "leashing" if it has really occurred, was more likely prompted by his outspoken and effective criticism of string theory Landscape research.

His "Kennedy Landscape" posting of 22 April, which Peter quoted and gave a link to, would have been the last straw.

Everything else Lubos does on his blog is a "petty misdemeanor" by comparison and the authorities would be petty themselves, were they to discipline him for it.

39.
May 1, 2005

lubos does not seem to be the type of person who can be silenced easily. The "leashing" probably involves his tenure or something. If that's the case, I must say that's an awful abuse of the tenure system. But who would feel so threatened by a lone ravings of a scientist with no political clout?

40. **Alejandro Rivero**
May 1, 2005

I got it! "Politically Correct" (I was trying "Communist Party" in a first guess)

41. **Alejandro Rivero**
May 1, 2005

I missed something. I thought that PC was standing for "Parity times Charge".

42. **Apeiron**

May 1, 2005

Hmmm…..I guess loony political beliefs are those you disagree with?

Is it loony to megaphone nonsense in the campus courtyard? (cf Witten and Co.). I guess not if it conforms to the PC orthodoxy.

Pretty weak-minded stuff from smart people, but it's predictable from those who never made it off campus.

43. **JC**
May 1, 2005

Over the years I've heard of a few cases of untenured folks either saying all kinds of loony things, and/or burning all their bridges behind. In these particular cases, the untenured person in question knew they were never going to get tenure at their university nor at any other research university for that matter. With this realization their mindset is basically in a "lame duck" phase where nothing "academic" matters to them anymore, and they're already sending out their resume looking for another job outside of academia. During this "lame duck" stage these folks took on an attitude of goofing off and being an annoyance, before they get the eventual boot.

44. **Arun**
May 1, 2005

In what way has Lubos Motl's right to speak been curtailed? Has there been any formal sanctions? Or has been informal, some faculty member or official hinting at something?

My doubt being that if we interpret graphs of world ocean temperature, or the scientific value of string theory differently, then do we know that we would interpret whatever lead to being "leashed" in the same way? Without more facts, there is no objective way of judging the matter.

-Arun

45.
May 1, 2005

Fred,

When was Feynman "chastized on many occasions"?

I'm sorry if Lubos got into trouble. I thought Harvard was "The Crazy Academic's Last Stand", so to speak.

46. **Chris Oakley**
May 1, 2005

It is possible that Motl has been restrained because of this politics, but far more likely – in my opinion, at least – is that Harvard took a dim view of his generally behaving like a mad dog. If so, I will miss him. A least he put his head above the parapet, for example, in posting here – something that few other string theorists have been prepared to do, and certainly not to the degree that he did.

47. **Fred**
May 1, 2005

Fortunately scientists are somewhat immune to the politics of tenure/academic thought police with regards to someones particular idealogy.

This is not always the case, women had a great deal of difficulty in the past, as have notable conservatives (Edward Teller and Feynman both were chastised on many occassions). The same holds for liberals (the Colorado proffessor with the loony views on the WtC for instance).

However when you make enough of a buzz about yourself (say in lectures or on a blog) invariably you will end up with a backlash. Being a string theorist or most any high profile scientist kinda gives an intellectual edge to any conversation that might be seen as threatening to the average public or academic figure. I mean it wasn't long ago that scientists were sorta seen as high priests/priestesses of Ishtar when the nuclear age was dawning on us.

Still, its shocking that there is this 'leashing' taking place. I've seen it happen to other people too to a lesser degree (morally speaking).

48. **Tony Smith**
May 1, 2005

Peter, you say "… There certainly were plenty of people who had trouble in academia because of their political beliefs during the fifties. When I said I wasn't aware of any cases I meant during the last 25 years or so …".

Could that mean that a pendulum has swung over a 50-year period so that we are now entering a new era of politicized academia, in which the role of the McCarthy bad guys of the 1950s is now being played by the PC bad guys of today in the 2000s ?

(I admit that "bad guys" is a subjective term, but it is how I view them.)

Tony Smith
http://www.valdostamuseum.org/hamsmith/

49. **Peter**
May 1, 2005

Hi Tony,

There certainly were plenty of people who had trouble in academia because of their political beliefs during the fifties. When I said I wasn't aware of any cases I meant during the last 25 years or so that I've been around universities.

50. **Tony Smith**
May 1, 2005

Peter, you say that you have "… never heard of any case in math or physics where this … political beliefs … was in any way an issue in a tenure decision. …".

What about David Bohm's expulsion from Princeton?

According to the Bohm biography Infinite Potential, by F. David Peat (Addison-Wesley 1997) at pages 101, 104, and 133:
"… when his [Bohm's] … Princeton University … teaching … contract came up for renewal, in June [1951], it was terminated. … Renewal of his contract should have been a foregone conclusion … Clearly the university's decison was made on political and not on academic grounds … Einstein was … interested in having Bohm work as his assistant at the Institute for Advanced Study … Oppenheimer, however, overruled Einstein on the grounds that Bohm's appointment would embarrass him [Oppenheimer] as director of the institute. … Max Dresden … read Bohm's papers. He had assumed that there was an error in its agruments, but errors proved difficult to detect. … Dresden visited Oppenheimer … Oppenheimer replied … "We consider it juvenile deviationism …" … no one had actually read the paper … "We don't waste our time." … Oppenheimer proposed that Dresden present Bohm's work in a seminar to the Princeton Institute, which Dresden did. … Reactions … were based less on scientific grounds than on accusations that Bohm was a fellow traveler, a Trotskyite, and a traitor. … the overall reaction was that the scientific community should "pay no attention to Bohm's work." … Oppenheimer went so far as to suggest that "if we cannot disprove Bohm, then we must agree to ignore him." …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

51. **JC**
May 1, 2005

Anonymous (Posted at May 1, 2005 12:39 AM),

Sounds like stuff in Erich Fromm's "Escape From Freedom" book.

52.
May 1, 2005

It is a liberal illusion that people (as a whole) liberated from oppression understand freedom. The experience in most of the world is that freed peoples turn around and behave like their former oppressors; just as the abused individual is likely to become an abuser, like the harassed daughter-in-law becomes a terror of a mother-in-law; the victims of racism are racist; the persecuted dissenter becomes intolerant of dissent.

It is also both a liberal and a Christian misconception that suffering confers virtue.

53.
May 1, 2005

Conservatives love to feel sorry for themselves.

If he is denied tenure, I'm sure Lubos would find it much more comforting to nurse the illusion that he was persecuted for his political beliefs, than to face the fact that he has wasted his life on silly pretend physics.

54. **Daniel**
April 30, 2005

(I don't know Lubos Motl personally, or in any capacity except for his blog)I do know however, that he comes from a country that has in the past been "leashed" by communism, economic stagnation and stifling bureaucracy. A country where free speech and the individual were suppressed by corrupt people who thought that *they*, and *they* alone, knew what any given person *should* be doing at any given time. With this background Lubos, I would imagine, understands what makes *your* country, Dr. Peter Woit (assuming you are an american), a great country. His "right wing idealism" that you refer to, is an exaltation, a rejoicing cry, an exhausted relief in the simple fact that there *does* exist a place in the world where one man can rise above the rest, where reason isn't inhibited by political correctness and adherence to the majority belief. Given the stark contrasts that he has seen perhaps he and others like him who have emerged from behind the Iron Curtain know better than any of us what America really stands for and what we as members and visitors of this society should exalt in. (Forgive me for this slightly sarcastic final comment,…but Lubos stands in the extreme minority with his political views (in academic circles) and is unafraid to challenge the norm, and you stand in the face of the majority string theorists and are also unafraid to challenge the "standard" view. You are quite similar in many ways!!)

55. **Peter Woit**
April 30, 2005

Annoying your colleagues with loony political beliefs certainly doesn't tend to make them better disposed to your tenure case, but I've never heard of any case in math or physics where this was in any way an issue in a tenure decision.

56. **JC**
April 30, 2005

Peter,

How common is it for somebody to be denied tenure because of their political beliefs?

One extreme case of this sort I've heard of, though not having to do with tenure directly, was of some professor who was posting up Nazi white supremecist propaganda on his www homepage. The department eventually moved his office to some dark corner in the basement or the boiler room.

Various Mathematical Links

May 5, 2005
Categories: Uncategorized

I've recently run across various interesting mathematically oriented sites, each with some connection to physics:

Alain Connes now has a web-site. He's now a professor at Vanderbilt University as well as at the College de France. I can see him in Robert Altman's movie "Nashville". His site contains quite a few interesting things, including most of his research articles and some interesting survey articles about his work on non-commutative geometry. For instance, take a look at his "A View of Mathematics", which starts off with a wonderful description of doing mathematical research and some interesting history of geometry, before surveying his recent work relating non-commutative geometry and physics.

David Ben-Zvi at Austin is organizing a new lecture series to be made available over the web called GRASP (for Geometry, Representations and Some Physics), which sounds promising although it is just getting started.

The MIT math department sponsors something called the Talbot workshops. Last year the topic was elliptic cohomology, this year geometric Langlands. Notes from the lectures are available courtesy of Megumi Harada who also maintains a useful website of geometry conferences, many of which have some sort of physics component.

Comments

1. May 12, 2005

   Speaking of links, the readers of Peter can be marginally interested on the schedule of the Strings 2005 conference, early this July.

   http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/program.html

2. **stephen**
   May 11, 2005

   I learnt a lot of things in this blog of yours Peter. A you can see many links come from your posts.

   I have included a correction to previous post quotation. I might have given the wrong link. I had to give credit to Alain Connes for the statement, and it did not show this. I stand corrected here now.

3. **Juan R.**
   May 9, 2005

   I think that Connes idea of a "map" between geometrical items and operator ones is interesting, but I doubt that we can obtain some really fundamental.

   I am not sure of that noncommutative geometry solves some of the more difficult open questions in differential geometry. See, e.g. my criticism to infinitesimals on page 5 of Official launching

   Moreover, It is unlikely that noncommutative geometry alone can be the key for our understanding of quantum gravitation.

4. **D R Lunsford**
   May 8, 2005

   I followed Connes up through ds=1/D then he sort of lost me. But that's a great read. Anything with Desargue's Theorem in it is worth reading.

   -drl

5. **Juan R.**
   May 8, 2005

   Part of the ability of good mathematicians is to provide new language where our previous discriptions were lacking

   Part of the ability of good scientists is to provide new models of nature where our previous discriptions were lacking

   A mixture of bad math more bad physics is of litte interest for us.

   Humm. Bad math more bad physics. where do i read some similar?… perhaps in some 10D universe in my last travel?

   0 + 0 = 2?0 = nothing vibrating.

   An additional dimension does not change the result.

6. **stephen**
   May 7, 2005

   Most mathematicians adopt a pragmatic attitude and see themselves as the explorers of this mathematical world" whose existence they don't have any wish to question, and whose structure they uncover by a mixture of intuition, **not so foreign from poetical desire", and of a great deal of rationality requiring intense periods of concentration.**

   Each generation builds a mental picture" of their own understanding of this world and constructs more and more penetrating mental tools to explore previously hidden aspects of that reality.

   Like Lenny Susskind?

   Isn't this part of the ability of good mathematicians is to provide new language where our previous discriptions were lacking? New forms/models of math?

7. **Florian**
   May 7, 2005

   He.
   I'm reading your blog for some time now and enjoy it very much. Personally, I'm more on the "mathematical side" but with some interest in how ideas from physics motivate new mathematical questions and insights (like the paper of Witten on Morse-Theory or the one by Kontsevich on Deformation Quantization) and I appreciate the mathematical posts very much.

   Best regards,
   Florian.

8. **Levi**
   May 6, 2005

   Just wanted to say that I always appreciate the math links. Thanks.

9. **D R Lunsford**
   May 6, 2005

   Thanks. "Integral conformal invariants" sound interesting 🙂

   -drl

10. **Peter Woit**
    May 6, 2005

    Hi Danny,

    Kind of too big a topic, since "geometry" in one form or another covers maybe at least a third of all mathematics research. For some idea of the range of things people work on, there's a conference here at Columbia this week in honor of a great geometer, my colleague Masatake Kuranishi, who is 80 and retired this past year. See

    http://www.math.columbia.edu/%7Ewoit/kuranishi_schedule.html

    One hot topic is the curvature flow technique pioneered by Richard Hamilton, and used by Perelman to give at least an outline of a proof of the Poincare conjecture. Besides trying to nail down the details of this, people are applying this kind of technique to questions in complex geometry.

11. **D R Lunsford**
    May 6, 2005

    Could you give an overview of what is going on in geometry these days?

    -drl

The Stormy Onset of Group Theory in the New Quantum Mechanics

May 9, 2005
Categories: Uncategorized

When I first started studying quantum mechanics I read quite a bit about the remarkable history of the subject, especially about the brief period from 1925-27 when the subject grew dramatically out of the incoherent ideas of the old quantum theory to the full quantum mechanical formalism that is still taught today. This was the work of a small group of physicists: especially Heisenberg, Born and Jordan in Göttingen, Schrödinger in Zurich, Dirac in Cambridge, and Pauli in Hamburg. Recently I've been reading again about some of this history, but paying attention especially to the interactions of mathematics and physics during these years. An excellent very recent article that covers some of this is by Luisa Bonolis, entitled "From the Rise of the Group Concept to the Stormy Onset of Group Theory in the New Quantum Mechanics". (It seems that this link is inaccessible unless you're at a university site that has a subscription. The article should also be available at most physics research libraries as vol 27, numbers 4-5 of the 2004 issue of Rivista del Nuovo Cimento.)

I've written a bit about this history before, especially about the mathematician Hermann Weyl's role, but quite a few other mathematicians were closely involved, including Hilbert, von Neumann, Emmy Noether, and van der Waerden. Much of the interaction between mathematicians and physicists took place at Göttingen, where Hilbert was the leading mathematical figure, and Weyl was sometimes a visitor, with both of them lecturing on quantum mechanics. This period was very much a high point of the interaction of mathematics and physics, interactions of a sort that were not seen again until the 1980s. Heisenberg and his collaborators learned about matrices from Hilbert and the other mathematicians at Göttingen, and Weyl was responsible for educating physicists about group representation theory and turning it into an important tool in quantum mechanics.

The Bonolis article has some amusing quotes from physicists who were having trouble absorbing what the mathematicians were telling them. Heisenberg wrote to Jordan "Now the learned Göttingen mathematicians talk so much about Hermitian matrices, but I do not even know what a matrix is," and to Pauli "Göttingen is divided into two camps, those who, like Hilbert (or also Weyl, in a letter to Jordan), talk about the great success which has been scored by the introduction of matrix calculus into physics; the others, like Franck, who say that one will never be able to understand matrices." Pauli was scornful about this new, unphysical, mathematical formalism of matrices, drawing a testy response from Heisenberg: "When you reproach us that we are such big donkeys that we have never produced anything new physically, it well may be true. But then, you are also an equally big jackass because you have not accomplished it either."

Immediately after having to get used to matrices, physicists were confronted by Weyl with high-powered group representation theory, which they found even harder to understand than matrices. Famously, Pauli referred to the group theory that mathematicians were talking about as the "Gruppenpest", but the late twenties saw a very fruitful exchange of ideas between mathematicians and physicists around this topic. Weyl's proof of the Peter-Weyl theorem and von Neumann's work on representation theory grew out of quantum mechanics, and the Brauer-Weyl theory of spinor representations was inspired by Dirac's work on the Dirac equation.

It's also interesting to note how in the years just preceding this period, much interaction between math and physics had grown out of general relativity. Noether's work on what is now known as the Noether theorem came about because she was asked questions by Einstein and Hilbert who were trying to sort out conservation laws in GR. Weyl took up representation theory as a result of his work on the symmetries of the curvature tensor.

An amusing story I hadn't heard before that is in the Bonolis article was one told by Edward Condon about Hilbert. He claims that when Born and Heisenberg went to Hilbert to get help with matrices, he told them that "the only times that he had ever had anything to do with matrices was when they came up as a sort of by-product of the eigenvalues of the boundary-value problem of a differential equation. So if you look for the differential equation which has these matrices you can probably do more with that. They had thought it was a goofy idea and that Hilbert did not know what he was talking about. So he was having a lot of fun pointing out to them that they could have discovered Schrödinger's wave mechanics six months earlier if they had paid a little more attention to him."

Comments

1. **Juan R.**
   May 18, 2005

   No problem Peter,

   but by a question of education and consistency, please use always the same phylosophy for stoping/erasing any no relevant comments.

   It is my belief (and of others) that your meaning of "irrelevant" is rather flexible, specially when you open a topic on a "stringy theme" and people here finalize it attacking to Lubos Motl. I still wonder that you maintain several of those comments and personal attacks intact.

   Of course, this is your blog and I respect your decision. No problem by my part, this was only a comment.

2. **Chris W.**
   May 16, 2005

   The paper that is the topic of this post has been downloaded and may be found here:

   http://olympus.het.brown.edu/~danieldf/papels/math-ph/bonolis2004.pdf

   (..via It's Equal But It's Different)

   Regarding my previous comment (partly in response to one of Ben's), see Section **6. ? Einstein vs. mathematicians: Minkowski and the special theory of relativity** (p. 20-27).

3. **Peter Woit**
   May 16, 2005

   No, I was up in Boston at another conference, which I'll write about soon.

   Matti and Juan,

   Please stop using this weblog as a discussion forum for your own ideas that have nothing to do with the topics here. I'll delete any further comments of this kind.

4. **Anonymous**
   May 16, 2005

   Sorry, Peter to change the topic.

A question I have is, did you attend the
workshop on string cosmology at Columbia organized
on Friday 13th which Lubos Motl discusses in his blog. Maybe you can report on it if you did
thanks

5. **Juan R.**
May 16, 2005

Thanks Matti,

It has been a pleasure read your post. Still i may apologize because i do have studied TGD, but when I have some time free i will do.

I also work with a two-time formalism, therefore some times i talk about a 4+1D formalism. Humm, interesting! What is the status of the "geometric time appearing in field equations of physics" in your formalism?

I think that with apparently breaking of the second law in self asemmbly you mean integral decreasing of disorder or perhaps or on a diferential rate sense (negative production of entropy).

I found time ago that spacetime cannot be represented by usual differentiable manifolds (bye bye Calabi-Yaus), but still I cannot claim for fractal-like behavior, since i do know if a fractal description would be exact or only an approximation valid in certain regimes. I simply are not sure.

Really interesting, in my approach the failure of scattering theory is associated to density of matter. In particle physics with small effective densities, S-matrix work perfectly. for condensed matter situations, all the formalism breaks down and one works with phenomenological issues ad hoc.

The failure of usual relativistic description of bound states is more complex in my approach

Yes i also found that reductionsim fail, in fact it is a proven fact (usually ignored by physicists and by all string theorists) that string theory does not verify the equation for complexity level.

In my approach nature has a hierarchical structure and no one level is in deep more important that other. Upper levels are not totally reduced to simple lower levels, in fact there is information that is not contained in lower levels, e.g. particle. That is the failure of particle physics to explain upper structures for example biomolecules.

A priori my theory contain MOND approach like a limiting case for the explaining of anomalous acceleration and the missing matter problem.

6. **Matti PItkanen**
May 16, 2005

To Juan R.:

Thank you for interesting comments.

The most obvious apparent violations of second law relate to self assembly and behavior of phase conjugate light.

In my own theoretical framework they are apparent violations so that here we agree. I feel it necessary to distinguish between two times: the geometric time appearing in field equations of physics and the experienced time whose basic unit is quantum jump and which corresponds in average sense to some increment of geometric time which is however proportional to hbar so that there is a hierarcy for the geometric average durations of quantum jump (moment of consciousness) just like there is hierarchy of material systems: elementary particles, hadrons, nuclei, atoms…

The differences between these times are obvious: consider only reversibility (irreversibility) of geometric (experienced) time. With respect to the experienced time second law holds still true but since TGD predicts that both positive and negative arrows of geometric time (positive and negative sign of conserved inertial energy, two possible manners to select the fermionic Fock state in second quantization), processes such as self assembly for which controlling process proceeds backwards in the geometric time, apparently break the second law.

Concerning the "more quantal" issue. Increase of hbar means essentially fractal scaling: quantum coherence lengths and times are scaled up. For scattering cross sections in perturbative regime the effect is perhaps somewhat surprisingly just the opposite since higher order corrections come in powers of alpha= g^2/4*pi*hbar, which is reduced. For bound state energies which cannot be understood perturbatively the situation is different: in the case of hydrogen effects is simple scaling by 1/hbar^2 proportionality of binding energy. An interseting hypothesis is that hbar increases when the perturbative series for S-matrix fails.

Macrostructure indeed affects microstructure and reductionism fails: this is one of the main implications of TGD. This is already implied by what I call topological quantization: space-time surface has a many-sheeted structure with sheets having outer boundary (magnetic flux tubes, "topological light rays", etc..) identifiable as quantum coherence regions and forming a length and time scale hierarchy. Quantum classical correspondence together with the fact that these regions can have arbitrarily large but finite size suggests a generalization of quantum theory and dynamical and quantized hbar provides it.

Concerning strange unobserved matter: I am believer in TGD based variants of string like objects identifiable as magnetic flux tubes. Simplest of them are cosmic strings, 4-D surfaces X^2xY^2, where X^2 is string orbit in M^4 and Y^2 holomorphically imbedded 2-manifold of CP_2. A cosmic string traversing through the nucleus of galaxy in a direction transverse to the galactic plane (naturally assignable to the galactic jet) creates a Newtonian 1/rho potential, which explains the constant velocity spectrum of stars: no dark matter elsewhere would be the minimum option. I do not know how closely this relates to your explanation. Actually TGD allows to identify galactic black hole as a highly convoluted cosmic string. Galaxies would be pearls in a cosmic necklace.

I think that the limits of classical physics are encountered when one tries to understand intentional action and the coherent behavior of the matter in living organisms.

Matti Pitkanen

7. **Juan R.**
May 16, 2005

For Matti.

Some time ago I revised some of supposed violations of the second law in quantum regimes. The famous San Diego conference. Finally i discovered that all revised claims of violation of the second law were based in obvious misunderstanding of thermodynanmics and/or errors.

I also "showed" that formation of structures is compatible with a new generalized version of the second law for mesoscopic regimes.

Therefore, since that i known several of usual methods in laser physics, i doubt that any group can find real violation of the second law in laser phenomena.

8. **Juan R.**
May 16, 2005

Thanks,

I am not completely sure now of that the increase of hbar in Delta x*Delta p=about hbar–>hbar_s would make the system more quantal, just more uncertainty in coupled observables. I don't think that can be exclusively represented like more quantum character in all situations. Remember classical statistical mechanics, asumed to be classical but with Delta x*Delta p different from zero.

Therein my emphasis in that **perhaps** you are working with some like alpa?h, with alpha a system parameter, instead of with variable h.

Could variation of your h explain cosmological redsift like the effect of travel of light for different phases of universe? Or am i wrong?

I don't know the details of TGD and therefore I cannot do any serious comment still. However, I think that there is no real dark matter in the universe (this is another argument against ST and supposed dark matter explained from "cosmostrings").

My explaining of galaxies and cluster "dark matter" like a discrepancy in standard gravity appears to be supported by experimental data. In fact, i can derive the well-known (1/r) behavior without invoking to strange unobserved matter.

What is your opinion?

"The possibility of several values of hbar would allow interaction between widely different time and length scales."

It appears a fractal like behavior. Does macrostructure affects to microstructure in your TGD? If yes, this may be a violation of typical reductionism of particle physics.

Living matter is really interesting, still i found no sufficient time for doing research in that. Now i am working in gravitation and cosmology.

My point is that living matter is characterized by "long-range correlations", but i don't call that a "gigantic quantum structure", since formulas for understanding living phenomena are really classical ones, e.g. chemical kinetics.

Of course, perhaps i am wrong, but i don't know any macro-quantum effect violating classical laws usually applied, with success, in biology.

Cannot the movement of your hand be modelled with chemical kinetics (muscle) + transport theory (electrons, ions, etc.) + EM + classical mechanics (skeleton)?

9.
May 16, 2005

In reply to Ben:

I'm a mathematician (though unlike some – most? – people here, I like to think that math and physics are essentially the same; like two different sides of the same thing). What you describe/ask (abstract commutation relations vs. concrete PDSs) happens very often in mathematics. For example, consider finite groups; every finite group is a subgroup of some symmetric group (that is, all permutations of a set), and indeed that was the way people thinked of groups in the 19th century. Only in the 20th century started people think about abstract groups and their representation. Similarly, in the 19th century, continuous groups mostly meant groups of transformations of vector spaces; manifolds meant submanifolds of euclidean spaces (and again, every smooth manifold can be imbedded in a large euclidean space), etc. So why consider abstract objects instead "concrete" ones?
First, it is not always true that all abstract objects can be realized as "concrete" ones (like subobjects of some model object); and, even more significantly, the abstract viewpoint turned out to be very fruitful. For example, you can make a difference between intrinsic and extrinsic properties of an object: which are properties of the abstract object itself and which are consequences of the particular realization of it.

10. **Matti PItkanen**
May 16, 2005

To Juan R.:

The increase of hbar in Delta x*Delta p=about hbar–>hbar_s would make the system more quantal.

One can imagine that a system consisting of ordinary elementary particles can make a transition to a large hbar phase without an appreciable change in four momenta of particles but with an increase in quantal size hbar/m defined by the Compton length. Macroscopic quantum phase is a natural outcome due to the quantum overlap of particles.

For instance, suppose that hbar_s/hbar= about 2^11 (a preferred value for hbar_s for certain reasons). Ordinary IR photon with energy of 1.24 eV corresponds to wavelength of one micrometer whereas "dark photon" would correspond to a microwave wavelength of 5 millimeters.

There are good arguments (in TGD Universe) for believing that dark matter particles form analogs of Bose Einstein condensates and emit coherently BE condensates of dark photons behaving very much like laser beams and decaying to ordinary photons with wavelength shorter by a factor 2^(-11) in our example (decoherence).

The possibility of several values of hbar would allow interaction between widely different time and length scales. This kind of interactions characterize living matter. Consider only how my intentional action to raise my hand eventually boils down to *coherently* occurring interactions in molecular and atomic length and time scales.

Matti Pitkanen

11. **Juan R.**
May 16, 2005

Matti,

You claim for modifications of the Planck.

What is the interpretation of deltaE = hw in your theory for large systems?

Are you perhaps really claiming for the substitution

h –> alpha?h

in formulas, with alpha a parameter instead of asuming that h is not a real constant?

12. **Matti Pitkanen**
May 16, 2005

To Chris:

My purpose is not to propose any ad hoc modifications of quantum theory. The value of hbar remains free in quantum theory: this is a fact. Second fact is that TGD leads to a well-educated guess for the spectrum of allowed values of hbar based on mathematics associated with so called hyper-finite type II_1 factor of von Neumann algebras (see the chapter at my homepage). This appears naturally as the Clifford algebra of infinite-dimensional spaces (now the space of 3-surfaces in certain 8-D imbedding space) and are partially characterized by the requirement that infinite-dimensional unit matrix has unit trace (might be relevant for the finiteness of the theory). A considerable generalization of the structure of quantum theory is involved.

As far as energy conservation is involved, I am extreme conservative. TGD was born from the requirement that inertial energy is strictly defined and conserved: in general relativity this is of course not the case and has led to numerous difficulties discussed also in this blog. Non-trivial representations of Diff^4 required by the identification of momenta as Diff^4 generators have central extension and lead to Diff anomaly. The problem finds an elegant resolution if Poincare symmetries correspond to those of imbedding space rather than space-time surface.

To blame that free energy people have never heard of energy conservation and second law is of course cheap rhetoric. The anomalous effects involved with free energy effects and cold fusion (to name only few of them) represent the borderline of our knowledge, and it could be very rewarding for theoretical physicists to come down from the academic heights, and start to think more what refinements of the basic concepts are required by these anomalies if they are indeed real. There are also strange effects apparently breaking second law associated with phase conjugate laser beams. Self assembly in living systems is also very interesting in this respect. Of course, the conceptual problems of general relativity alone should provide enough food for original thought.

Matti Pitkanen

13.
May 16, 2005

To Ben:

Remembering the basics: QM algebra mumbo jumbo describes discrete values of observables not just a time-continuous evolution of probability waves (in the Born interpretation). The discrete values of an observable are nothing but the eigenvalues of a Hamiltonian acting as a hermitian operator on a Hilbert space which is spanned by orthogonal vectors representing solutions of the Schroedinger equation (the eigenvectors of the Hamiltonian). If hermitian operators of different observables cannot be simultanously diagonalized, measurements on the observable can not be measured accurately the same time. The Schroedinger-equation connects the Hamiltonian with continous time evolution and Hilbertspace/Matrix formalism connects the same Hamiltonian with observable spectra. Each element of the mathematical formalism has a clear physical interpretation.

To Matti:

If You want to vary physics on demand of some unobserved speculative effect, why not dispute energy conservation or entropy law (which makes people depressive)? The “free-energy” crowd that creates and sells perpetuum-mobiles would be grateful.

14. **Chris Oakley**
May 16, 2005

Hi Ben,

I think that the reason for wanting to base QM/QFT on commutation relations is simply that you have got to start somewhere. But I agree that it is a mistake. After all, it does not work for quantum fields other than spin zero, and even if one accepts mathematically meaningless constructs such as the differencing of divergent integrals, “quantization” of GR always fails. One might also add that the premise of quantization if nonsensical: we start with a classical theory and then “quantize” it – are we really saying that the classical theory is the fundamental thing here rather than just some kind of limit?

15. **Matti Pitkanen**
May 16, 2005

Thanks for Ben, the engineer, for very thoughtful comments. The canonical commutation relations for observables are often taken as a more or less sacred thing although there are of course generalizations. Symplectic geometry phase space gives a good justification for the general form of commutation relations apart from the value of hbar in the case of quantum mechanics. Bose (Fermi) statistics is behind the commutation (anticommutation) relations in quantum field theory, and in two-dimensional case braiding statistics leads to more general commutation relations.

What puzzles me is why the value of Planck constant is taken as sacred (and that also I took it as sacred for so long) so that it disappears from quantum physics formulas totally by the choice hbar=1,c=1. Dynamical, possibly quantized, Planck constant able to have large values, would be well-come to anyone attempting to understand living matter as a macroscopic quantum system since Compton lengths, etc. would be scaled up. A phase in which protons have atomic Compton lengths would be very different from ordinary condensed matter and might allow to understand some claimed anomalies such as cold fusion.

Variations of hbar do not lead to any dramatic effects in scattering of free particles if the classical cross sections representing hbar=0 limit are non-vanishing so that only higher order perturbative effects are affected (in fact reduced, since gauge coupling strengths are proportional to 1/hbar). Situation is different for processes like photon-photon scattering for which classical cross section vanishes. Also the spectrum of binding energies for say hydrogen atom scaling like 1/hbar^2 would be strongly affected.

One could play with the thought that the value of hbar must be such that classical bound states make sense also quantally. In the case of gravitational bound states of masses larger than Planck mass this would have rather interesting consequences. Planck constant would become gigantic and the black hole formation as a gravitational counterpart of infrared catastrophe for hydrogen atom would be prevented by the formation of quantum gravitational bound states. This line of thinking would mean a bottom-up approach to quantum gravity starting from gravitational wave mechanics (of dark matter perhaps) instead of not so successful top-down approach provided by M-theory.

For these and many other reasons I see the possible effects related to the dynamical hbar as worth of studying. More ponderings about this at my blog site.

Matti Pitkanen

16. **Ben the Engineer**
May 15, 2005

Chris W said: “In quantum field theory and general relativity we seem to have left this simple starting point (and its closely related predecessors) way behind, and modern mathematics has been absolutely essential in doing so. Have we lost something as well? ”

Hi, I’m Ben the (not terribly mathematical) engineer, and thanks to those who answered my post about Feynman vs. Schwinger. They’re both great of course!

Expanding on this discussion about abstract math in modern physics, please indulge me in a sweeping and naive question about quantum physics. Also, please forgive the length of this post, but this is the first (or rather second) time I have dared participate on a physics list and I wish to unload something that has been bothering me for a long time. The basic question is:

Why are the commutation relations so sacred?

This may seem like a strange question, but what I mean is, Why is a certain formal manipulation of symbols taken as sacred dogma by ALL physicists? Yes, I know it works in QED and QCD to upteen decimal places and all that, but why do the superstring and other quantum gravity people simply transplant it without any questions? Every other crazy idea can be entertained, but this is sacred. Why? At least, that’s the impression I get. Perhaps it is this blind faith in the commutation relations which is the reason that gravity and the other forces have not yet been unified. Perhaps they simply don’t work for gravity. Perhaps they don’t even work for the other forces in the regimes explored by unification theory.

If I could elaborate a bit, my question might make a bit more sense. As an EE (electrical engineer), I have no problem with wave equations. In fact, I love them. So naturally, I take a Schrodinger view of things, which I know is the simplest quantum approach. Hence, to me, the commutation relations simply express a straightforward relationship between partial differential operators. I know that the modern view is that the latter are only a *representation* and that the truth, following Dirac and others, is to be found in the abstract approach of a mere algebraic relationship between operators, which have themselves become quite abstract. (How ironic that Dirac started out as an engineer!)

Well, this clashes with a strong philosophical prejudice of mine, namely, that I want to be able to *visualize* everything. After all, whatever is happening down there is happening in space and time, so it must involve regions of space distorting and evolving in some way, and this means that it can be imagined in principle. Never mind what the regions are filled with, if anything at all. The point is that there must be regions in which whatever is happening is happening. (By ‘regions’ I am thinking primarily of boundaries in spacetime, though I realize that according to GR the spacetime itself may be distorting and evolving.)

So the bottom line is that I want to be able to visualize what is happening, even if there are profound ontological issues with *what* is being visualized. The idea of a probability wave may seem esoteric in some sense, but even engineers can feel happy with the complex exponentials or Bessel functions or Hermite polynomials or whatever that one gets when one solves the Schrodinger equation. At least we can *draw* them.

(Note that even Maxwell’s theory can seem a bit ‘metaphysical’ in that one may ask *what* is waving. This led to the idea of the ether and its subsequent repudiation. Yet we all feel comfortable with waves, whether classical or quantum, because we can at least imagine them, even if they have some ghostly ontological aspect.)

Things get even more mysterious for me when the commutation relations are applied to the electromagnetic field. How can something that was invented for particles be simply transplanted to the electromagnetic field? You may say that the EM field is ‘made up of’ particles called photons, but there is a difference with, say, the particle in a potential well for which Schrodinger derived his equation. The difference is simply that the ‘classical’ entity with which we begin is, in one case, a particle and, in the other, a wave. How can the same algebraic gimmick (if I might say so) simply apply in both cases? At least, why don’t physicists spend more time wondering about this? Every QED and QFT textbook I have seen simply ‘postulates’ the commutation relations for any pair of ‘conjugate’ variables, according to Lagrangian theory. I don’t deny that this somehow works in a number of cases, but the fact that it does should be profoundly puzzling to physicists. After all, not only is there the particle/wave difference in the classical starting point, but in one case we have a particle in an *external* potential and in the other case we have a *free* plane wave NOT in a potential. Yet the physicists just wave their magic wand and justify this magic by the pretext that it happens to work, as if that were a sufficient excuse! 🙂

Then when it comes to strings or the Planck regime, the questions and puzzles simply increase by orders of magnitude. I think I’ve said enough and you can see where I’m going. I’d like to dispense with algebraic abstractions like commutation relations and get back to good old partial differential equations describing waves that I can visualize evolving in space. If you argue that the commutation relations are in fact always equivalent to PDEs, then why not go to the PDEs directly? And the way I have seen the CRs applied in modern physics papers, in what seems like a mechanical and robotic fashion, makes me wonder if all contact with PDEs (and hence visualizability) has simply been lost, and THIS may be the problem. It may have all degenerated into quantum algebraic mumbo jumbo. Is this something any of you care about? Thanks

17. **Cy Cantrell**
May 15, 2005

Hi,

Interesting summary of Bonolis’ article… I’ll try to get a paper
copy.

The story about the conversation between Born, Heisenberg and Hilbert, in which Hilbert pointed out the connection between matrices and eigenvalue problems of partial differential equations, and Born/Heisenberg went away thinking that Hilbert was a very strange old man, is part of the lore of physics. I first heard it from Kurt Gottfried in a course on quantum mechanics at Harvard.

18. **D R Lunsford**
May 15, 2005

http://www.roadsideamerica.com/attract/images/mn/MNDARtwinelg.jpg

19. **D R Lunsford**
May 15, 2005

Hey! Here’s Ed Witten photographed with incontrovertible evidence of strings.

-drl

20. **Juan R.**
May 15, 2005

I sorry i had a problem with the axial flux-collector and with the catalyzer of supersimmetry. Now!!

O (closed)

S (open)

8 (cluster of two closed strings)

They look quiet at ambient temperature, but really are vibrating. Moreover, I am working in new glasses for seeing it in 10D and in full technicolor.

P.D: I forgot say that is just a math laboratory.

21. **Juan R.**
May 15, 2005

I have already sinthetized superstring and clusters in my laboratory. I post it below.

[b]O[/b] (closed)

[b]S[/b] (open)

[b]8[/b] (cluster of two closed strings)

22. **Quantoken**
May 15, 2005

Thomas Larson:

Yeah I saw Lubos meantioning that paper, too. I did not comment because I thought that's just a **joke** that some solid state physicists were trying to **ridicule** their counter part colleagues in super string research.

Certainly if super string simply does not exist, no one can make an experimental device to produce them. But that's not even the point.

We know, super symmetry, if it exists, must exist at an energy scale very high, much higher than the energy accessible by today's accelerators, i.e., above TeV, which corresponds above 1×10^16 degrees temperature.

On the other hand, the Bose-Einstein condensation of heavy metal atoms, as we know it, involves energy scale extremely weak, only at super cold temperature, sub mili degree absolute temperature, can those fermions condense into bosons. Therefore, any interaction withou the slightest amount of energy could easily thaw it out of Bose-Einstein condensation. Ther super symmetry interactions, certainly, is 1×10^20 times higher than that's required energy level to destroy the Bose-Einstein condensation. So such an experimental setting could not have produced anything even if super string theory is all correct.

Quantoken

23. **Thomas Larsson**
May 15, 2005

Note the standard caveats with if's and would's:

"If their idea can be put into practice, it would allow aspects of string theory to be explored in an experiment for the first time."

If arch-angles are produced at the LHC, it would allow aspects of Christianity to be explored in an experiment for the first time.

24. **D R Lunsford**
May 15, 2005

Hey, back to strings! In the lab!

http://physicsweb.org/articles/news/9/5/7/1

-drl

25. **Juan R.**
May 14, 2005

Quantoken

It appears obvious that when we improve our understanding of nature, it is necessary more and more mental power. In some sense you can explain GR, QFT or ST for public in usual, "cotidiane", terms, but the real understanding is only achieved from the mathematical formulation of those ideas and that math is each time more difficult and abstract.

I could offer you good examples of these topics but since Peter probably would erase my post, you would see my recent work in epsilon-calculus.
Recently, a mathematical research has done some good comments in my ideas on calculus and criticism of Connes, on an recent post (on GR) in a well-known forum. I think that you can find me easily.

It is rather probable that one day nobody can understand new mathematical formalism. In fact, that **fatal epoque** has already arrived to pure math, like some of us probably know.

In general, people has no idea of QM, GR, Strings or others. Since that was a teacher of young people, and some of my friends continue to be. I can say you that people has no best understanding of Newton gravity now that 100 years ago.

26. **Quantoken**
May 13, 2005

Some one said:

"This means that real physics is increasing **abstract** and **simplying** physics for the man on the onminbus is now impossible."

I completely disagree. The whole point of science is try to de-mystify nature and try to simplify and explain nature in ways we human can understand. If science grows more and more complicated and abstract and is detached from the comprehension ability of the people, eventually it will grow to the point that no one in the world understand science any more. Then science is no longer relevant to the development of human society if no one could understand it. I do not think that is the trend.

Back in stone age, nobody even knows how much is 1+1. Back in Newton's time, no one even realize the existence of gravity. But today even an average person knows a little bit of weird stuff of 10 dimentional super strings. And any weird field you publish your paper, there are at least a couple thousand people around the world who would like to read and can understand your stuff.

Science is becoming more accessible to people, not less accessible!

Imagine what will happen if the opposite is true. Einstein's GR was once said to be understood by only 12 people. Now a theory twice complicated and twice as advanced would probaly be understood by 6 people. Continue on and pretty soon the comprehensibility is reduced to zero. And the knowledge will be accessible to not a single living human being any more, zero.

Is that possible? Not at all. As long as science is still part of human culture, it will always remain accessible to at least a portion of the population, and this portion could only grow, not shrink. The information age makes it more likely that a vast majority of population will become very familiar with at least some partucular areas of science.

Quantoken

27. **Juan R.**
May 12, 2005

Like an expert in chemical questions, i can sure you that chemistry newer was reduced to/ explained by physics (either inside or outside of QM, SM, and ST).

I known the usual **(very wrong)** belief of that all of chemistry is already known (e.g. popular claims by Weinberg, Witten, etc). But they are so correct like Newtonian physicists claiming for an understanding of chemical reactions or 19th century physicists claiming for ultimate models of chemical bond based in classical electrodynamics.

For me it is so arrogant the claim by physicists of that all of chemistry is known like the claim by string theorist of that ST is the TOE. Somewhat like Peter considers useful to explain that ST is not a TOE (in fact one cannot predict anything) I consider good to present the current status of chemistry like an autonomous science.

28. **Selrach**
May 12, 2005

Hi Chris W,

Reading your post made me think about my
favourite issue with 21st century Physics.

What is to replace the Hierarchy?

What I mean is that Science upto now has explained the world thus: Geology then Biology then Chemistry then Physics then Mathematics.

However, with Quantum Mechanics, this clear delineation has broken down with superposition effects and with String Theory with mirror symmetries (proven as mathematics!).

So called paradoxes are paradoxes if you try to 'simplify' the problem at a higher Hierarchy level .

This means that real physics is increasing abstract and simplying physics for the man on the onminbus is now impossible.

An amateur mathematician.

29. **robert**
May 12, 2005

It's good to see Schwinger's rehabilitation in a couple of the posts following on from the 'can mathmos do physics and vice versa?' debate. His papers are models of clear exposition, whether they address the quantisation of gravity or the generation of synchrotron radiation, and, where possible, make direct numerical contact with physical reality. As is evident when reading his 'Classical Electrodynamics' text, he felt that the maths should emerge from the physics, rather than the other way round. This is particularly evident in his amazing 'On Angular Momentum' paper, which brings us neatly back to the Gruppenpest question.

Whatever; it is sadly true that those of us raised on Morse and Feshbach find it rather hard to penetrate the hep-th arxiv these days.

30. **Thomas Larsson**
May 12, 2005

Re Einstein's disregard for mathematics. While developing GR, AE did some absolutely marvellous inventions in mathematical technology, namely the summation convention and the idea to put contravariant indices upstairs (though I'm unsure whether this is really due to AE). In my experience, many mathematicians still seem unconfortable with these inventions, 90 years afterwards.

31. **Steve M**
May 12, 2005

Drl,
I recently had a look at old papers of Schwinger's from Physical Review in the 50s. They are an absolute calculational tour de force. He states what he is going to do and derive, then does the stunning calculation in complete detail. No excuses, no handwaving, no waffling, no speculation and no wishful thinking. The stark contrast with many of the modern arxiv papers kind of hits you.

32. **Chris W.**
May 12, 2005

Ben said, "By the way, even though Einstein developed respect for mathematics after laboring over General Relativity, I did read somewhere that later in life he complained that the mathematicians had so transformed (or veiled) his theory that he no longer understood it!"

Actually, I'm quite sure that he said this early in his career, in response to Minkowski's explicitly geometrical formulation of *special* relativity. Notwithstanding his initial misgivings, Minkowski's formulation set the stage (in part) for posing the questions that led to general relativity, ie, as Einstein reconsidered his relativistic theory of space-time measurement in the context of the equivalence principle and what was understood about gravitation prior to 1915.

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This is how new mathematical ideas can be so helpful; they can make it possible to state clearly and objectively notions and assertions that experience, physical intuition, ordinary language, and pre-existing mathematics can only motivate and sketch in an incomplete and sometimes contradictory fashion. Nonetheless the crude initial formulations are essential when new and fundamental problems are being confronted. They make it possible to sensibly discuss the problem of what new mathematics is needed for addressing *physical questions*, and why. My feeling about much recent work is that it relies on formal precision and rigor as a comforting and professionally rewarding refuge—a way of avoiding the crucial, difficult (and usually somewhat ill-posed) questions while still demonstrating admirable technical mastery and (perhaps) making contributions of substantial value to mathematicians and mathematical physicists.

In this context, consider Feynman's famous remark:

> If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generation of creatures, what statement would contain the most information in the fewest words? I believe it is the atomic hypothesis that
>
> *All things are made of atoms—little particles that that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into one another.*
>
> In that one sentence, you will see, there is an enormous amount of information about the world, if just a little imagination and thinking are applied.

In quantum field theory and general relativity we seem to have left this simple starting point (and its closely related predecessors) way behind, and modern mathematics has been absolutely essential in doing so. Have we lost something as well? That is, have we obstructed a path to a *lucid* reconsideration of this metaphysical starting point? It seems to me [see comments] that the fundamental issues raised in attempting to develop a theory of quantum gravity make this question more important* than it has been in a very long time.

—-
* In his recent lecture at Perimeter Institute Leslie Ballentine emphasizes what I believe to be a closely related point:

> Einstein's "God" talk was purely metaphorical, like "Mother Nature", or "Father Time". His objection to indeterminism was more serious, but has unfortunately been over-emphasized. [John Stachel has argued that it has also been badly misunderstood. -CW]
> His most powerful criticism of the "Copenhagen" interpretation did not involve determinism/indeterminism.
> Rather, it concerned realism (or *ontology*).

For some time now it has not been clear what physics at the most fundamental level is *about*. What it is about has certainly been clear enough to do much valuable research, up to a point, but I strongly suspect that in certain areas we have reached that point, and must now find a deeper, more lucid, and more unified answer to this basic question.

33. **D R Lunsford**
May 11, 2005

RE Klein – he at once understood the light cone, in fact he invented the theory of "light cones" insofar as projective geometry needs a quadric to become a metric geometry. How wonderful for Klein, in my opinion the Newton of mathematics, to see his program so gloriously realized.

-drl

34. **D R Lunsford**
May 11, 2005

Peter – you should check out Schwinger's book on electrodynamics, based on lecture notes and mostly reviewed by Schwinger himself. This book is one of my prized possessions. You really get a clear idea of how Schwinger did physics, and in this context it's just amazingly to the point.

http://www.amazon.com/exec/obidos/tg/detail/-/0738200565/qid=1115861218/sr=8-2/ref=pd_csp_2/104-0715732-6355125?v=glance&s=books&n=507846

Sorry for the long url.

-drl

35. **Peter**
May 11, 2005

Actually I don't think either Schwinger or Feynman had much use for mathematicians, although Feynman's style was more intuitive and pictorial and Schwinger's more formal.

I've always found Schwinger hard to read, whether or not you know the subject he is writing about, but Feynman can also be hard if you are just learning the subject. His lectures on physics for undergraduates famously baffled most of the students. I've heard that when they both presented their work on QED, people found Schwinger more understandable, since he presented coherent derivations, whereas Feynman seemed to be engaging in repeated leaps of logic. On the other hand, I've also heard about these early calculations that "Feynman made it look like anyone could do it, Schwinger that only he could have done it".

36. **Ben**
May 11, 2005

Hi. I'm an engineer by profession with an interest in physics, but my mathematical knowledge is not deep. Speaking of the overuse of math in physics, I have an impression regarding Schwinger and Feynman, and I am wondering if it is correct. Schwinger was supposed to be one of the most virtuosic mathematical physicists of his day, allegedly capable of calculations nobody else could do or perhaps even understand. Feynman, on the other hand, seems to have taken a relatively 'intuitive' approach to QED, which dispensed with heavy formalisms in favor of the famous Feynman diagrams. Yet, who had the greater impact on physics? It seems quite clear to me that it was Feynman. I suspect that this comparison is almost a cliche among physicists. I did read somewhere that Feynman once referred disparagingly to 'fancy schmancy differential geometry'. By the way, even though Einstein developed respect for mathematics after laboring over General Relativity, I did read somewhere that later in life he complained that the mathematicians had so transformed (or veiled) his theory that he no longer understood it!

37. **Peter Woit**
May 11, 2005

To expand on the previous comment:

Klein's point of view on geometry as being about Lie groups and Riemann's point of view as it being about metrics and curvature were unified by Cartan, who really was the first one to come up with the modern view of geometry, in which the connection on a principal G-bundle plays the central role.

Hilbert was definitely the major figure in mathematics in the early part of the century, since by this time Klein wasn't very active (he retired in 1913). Klein was the one who built up the great school of mathematics at Gottingen, but by the time quantum mechanics came around, he was dead and Hilbert was the leading figure there. Hilbert's influence is due partly to the fact that he worked in an amazingly wide array of mathematical areas, with geometry only one of many.

By 1925, Hilbert was getting old (63). It is Weyl who was really at the height of his powers during those years, and had the most influence on physics during that period.

38. **Walt Pohl**
May 11, 2005

Selrach: Klein's Erlanger programme was done in by changes in mathematical fashion more than anything else. Riemannian geometry simultaneously encompassed Klein's geometries and the classical differential geometry of curves and surfaces, so it has drawn most of the attention. The Erlanger programme just became a special case of highly-symmetric Riemannian manifolds.

DRL: Within mathematics itself, Hilbert is a more influential figure than either Klein or Weyl — probably the most influential figure on mathematics in the twentieth century.

39. **D R Lunsford**
May 11, 2005

Selrach,

In my opinion, there are Klein, Weyl, and a lot of second stringers 🙂

-drl

40. **D R Lunsford**
May 11, 2005

Thomas,

Your comment RE Lie is extremely interesting and exactly to the point.

-drl

41. **Thomas Larsson**
May 11, 2005

DRL, I don't think that one should regard Hilbert's remark as Einstein-bashing, but rather as an observation that mathematical skills and physical intuition are very different things. Of the two, the latter is more important. Your math can be improved if needed, but without an understanding of the physics you don't know which math is needed.

42. **Selrach**
May 11, 2005

Hi drl,

I am not sure that you can compare Klein and Hilbert in that way!

Both were mathematicians who created grand overarching philosophies which have proved influential but flawed. Klein's geometry is group theory (Erlanger program) was shown to be inadequate, when Peano/Weierstrass discovered continous curves which have no derivatives anywhere!

Hilbert's program was of course undone famously by K Godel.

My belief is that they are both great flawed mathematicians!

An amateur mathematician

43. **Thomas Larsson**
May 11, 2005

In my experience, the mathematical knowledge of string theorists is rather lopsided. Many of them are strong in algebraic geometry (which I am not), but it is unclear to what extent algebraic geometry is really needed in physics. Real physics, i.e. GR and SM, can be understood without even knowing about manifolds and bundles. This is obvious, since these theories were discovered before fiber bundles became popular in physics around 1980. If we want to calculate some QCD amplitude, does it really help to know that the gauge potential is a connection on some SU(3) bundle? Some problems, like the Dirac monopole and the Aharonov-Bohm effect, can perhaps be understood better in modern language, but I doubt that neither Dirac, Aharonov nor Bohm knew about it.

A good illustration of the strange selection of well-known math is given by simple Lie algebras of vector fields over the complex numbers. The Cartan-Killing classification in the finite-dimensional case (A_n, B_n, C_n, D_n, E_6, E_7, E_8, F_4, G_2) is common knowledge, but Cartan's classifictation of the infinite-dimensional ones (W_n, S_n, H_n, K_n) is not. Which is strange, because these algebras were well known to Sophus Lie himself, and they play a much more prominent role in physics than do the finite-dimensional ones (except A_1 and A_2).

44. **Juan R.**
May 11, 2005

Famous Feynmann criticism to math-oriented physicists continue to be true.

Some of my colleagues focus on mathematical research and believe that are doing physics when only are providing new mathematical views on old physical problems.

1?) Physics, after math.

Moreover, I believe that in some decades we will find limitations on the use of math for modelling nature.

When i read the GR manual from Wald, I wonder that only after several chapters one begins to see some of physics. What do you think?

45. **Kay**
May 11, 2005

“Einstein … often spoke against abusive use of mathematiccs in physics. Physics, he would say, is essentially a concrete and intuitive science”. “I don’t believe in mathematics”, Einstein is reported to have affirmed before 1910.

Einstein re-articulates here an old and somehow paradoxal intellectual relationship towards the use of language which can be dated back to Platon. It is very funny to read this citation of Einstein because remember that his enemies in Germany dispraised his physics as jewish and abstract. They bashed Einstein and all modern physics with exactly the argument that physics should be concrete and intuitive. The key-term in these philosophical polemics was the concept of “Anschauung” (in orig. german). “Anschauung” cannot be simply translated into “view” or “concept” but it includes a sensual and a contemplative aspect of “watching the true shapes of the ideas” and is not a mere technical reasoning. Remember also that Einstein was an opponent of Bohr/Heisenberg style of positivism which led the classical world of “Anschauung” completely behind and transformed physics into an interface-language for holding a conversation(!) with nature (later it was Prigogine/Spengers who insisted in this “dialog with nature”). This controversy was not less popular in mathematics and the most prominent proponent of positivism/formalism was no one else than David Hilbert.

The role of language in the mindset of “Anschauung” was that of a service. So it was mathematics to the physics-community of that time. Language had no different role than fixing vagueness and making ideas communicable and testable but it had no function of it’s own. The modern mindest is far away from the platonic ideal of “Anschauung” but it comes close to that of investigating language and it’s effects. It’s more Kabbalah than nature mystics.

Regards,
Kay

46. **Fabien Besnard**
May 11, 2005

“I don’t believe in mathematics”, Einstein is reported to have affirmed before 1910.
– p 3.

True, but he changed his mind later.

47. **D R Lunsford**
May 11, 2005

Wow, Einstein bashing even from Hilbert. Jealous I suppose. Of course it’s ridiculous. I read the papers, so I don’t give a fig about Hilbert’s lofty opinion of himself. Notice that one never finds Klein making such comments, and Hilbert was no Klein.

-drl

48. **Thomas Larsson**
May 11, 2005

Some quotes from physics/0504179 :

“Einstein … often spoke against abusive use of mathematiccs in physics. Physics, he would say, is essentially a concrete and intuitive science”. “I don’t believe in mathematics”, Einstein is reported to have affirmed before 1910.
– p 3.

“Every boy in the streets of Goettingen understands more about four-dimensional geometry than Einstein. Yet, … Einstein did the work and not the mathematicians”
– attributed to Hilbert, p 11.

49. **JC**
May 10, 2005

Alejandro,

I got the impression the more “hardcore” mathematically inclined string folks and other theorists seem to be fond of using a lot of “new math”. Papers written by folks of this sort seem to pop up frequently in various journals like J. Math. Phys. or Comm. Math. Phys., and other lesser known journals specializing in “mathematical physics”.

Some of these guys seem to be quite far away and disconnected from experimental data.

50. **Chris W.**
May 10, 2005

On the genesis and significance of Noether’s work in connection with general relativity, see this **review** by Nina Byer (UCLA, 1999).

From Hermann Weyl’s 1935 memorial to her:

“A stormy time of struggle like this one we spent in G?ttingen in the summer of 1933 draws people closely together; thus I have a vivid recollection of these months. Emmy Noether – her courage, her frankness, her unconcern about her own fate, her conciliatory spirit – was in the midst of all the hatred and meanness, despair and sorrow surrounding us, a moral solace.”

(For more, see this page.)

51. **Juan R.**
May 10, 2005

I agree in that in general new physics implies often new math.

The term new math would be taken on a broad sense, e.g. new applications in physics of old mathematical stuff. A example is the formulation of GR by Einstein and based in math done by mathematicians.

For science, the rule is first physics after math. This rule is violated in string M theory, where there is some advance in pure math but few or no advance in physics. This indicates, at least to me, that the entire endeavour is completely wrong. It is not the problem of finding some correction term or some new magical concept solving all problems, we may simply ignore the approach and focus in another theory/-ies.

Quantoken,

There are problems now intractable (but that wiil be solve in a future) and other may be totally intractable to practical effects forever.

One of that intractable forever problems is the formulation of a TOE. It does not exist.

I don't think that "intractability" is the source of time arrow, without a concept of entropy.

Time arrow is based in certain topological effects linked to intimate structure of spacetime. Entropy plays a fundamental role and it is the source for the well-known link between thermodynamics and gravitation.

52.
May 10, 2005

It's easy to let mathematical formalism bury physical insight (or to use it to disguise a lack of physical insight).

53. **Chris W.**
May 10, 2005

(That's Dawson and *Nielsen*. Sorry, Michael.)

54. **Chris W.**
May 10, 2005

This may stimulate some discussion:

?I have been impressed by numerous instances of mathematical theories that are really about particular algorithms; these theories are typically formulated in mathematical terms that are much more cumbersome and less natural than the equivalent formulation today?s computer scientists would use.? — Donald E. Knuth

[from D. E. Knuth. Computer science and its relation to mathematics. Amer. Math. Month., 81(4), April 1974.]

This quote begins the introduction of **The Solovay-Kitaev algorithm** (Nielson and Dawson, quant-ph/0505030, 6 May 2005).

55. **Robert**
May 10, 2005

Quantoken

There is significant contact between number theory, the Reimann hypothesis and quantum chaos, described in Chapter 11 of Marcus du Sautoy's 'Music of the Primes'. Perhaps it is not so strange that Bombieri's April 1st 1997 announcement of a proof of the RH described a physically motivated analysis that drew on insights from supersymmetric fermionic-bosonic systems – a near absolute zero ensemble of a mixture of anyons and morons.

56. **JC**
May 10, 2005

A lot of the math used in string theory before the 1984 Schwarz anomaly cancellation paper, didn't appear to be much more complicated than the sort of math one comes across in quantum field theory and general relativity. A lot of the "new math" seems to have surfaced quite quickly after Witten started to publish a lot of string papers, especially complex algebraic geometry related stuff like Calabi-Yau manifolds. (Some of it looks like it was carried straight over from supergravity compactification type of problems).

57. **quantoken**
May 10, 2005

Peter said:
"I think most people are resistant to learning a new abstract formalism unless there is good evidence that it **really does something useful**. One has a limited amount of time and energy, and learning a new formalism can be time-consuming."

Usefulness is really in the eyes of the beholder. Each individual math field is certainly considered some what useful at least to some people, otherwise there would be no one studying them. But out of all of possible math fields, which in principle could be an infinity, those applicable or useful to physics, which must be a finity since we are talking about a finited universe, such math applicable to physics must be a very small portion.

For example number theory is very useful. But it does not seem to be related to physics. What does it do with physics whether all prime numbers lies on a straight line in the Liemann Hyperthesis? Nothing. Similarly the P and NP problem is unrelated to physics, too. Whether there is an efficient way of cracking the RSA encryption would not tell us howto unify gravity and QM.

In mathematics you can surely imagine a 11 dimentional world, and derive tons of seemingly interesting mathematics out of it. You could also imagine what if the world is two dimentional. But it's really not relevant at all. The world is 3+1 D as we know it and there hasn't been any evidence it could be otherwise.

One math branch that interests me is the problem of tractability. Some math problems are seeming intractable. Are those truely intractable by nature, or are they merely due to our shallow knowledge of math in our era? If intractability can be proven as a natural occurance, then apply it to quantum computing, it could explain the emergence of the **time arrow**, without the entropy. In another word, our world could be constructed using a series of **quantum one way hash functions**, so it could only move forward in time but never backwards.

Quantoken

58. **Alejandro**
May 10, 2005

I dissent. String or not string, most theoretical physicists are fond of using new mathematics. It is only that their discovery path does not coincide with the one used by mathematicians; so the new math they use come mainly from other theoretical physicists.

59. **ksh95**
May 10, 2005

JC asked Peter:
"…Excluding the string theory crowd, why are some physicists resistant to using "new mathematics"?.."

Ksh95 will answer:

People are resistant to learning new mathematics for the same reasons they are resistant to sticking shards of glass in their eyes. Very painfull, lots of screaming, plenty of cursing…

60. **Peter Woit**
May 10, 2005

Hi JC,
I think most people are resistant to learning a new abstract formalism unless there is good evidence that it really does something useful. One has a limited amount of time and energy, and learning a new formalism can be time-consuming.

Heisenberg et. al. had some good reasons to be dubious about thinking of p and q operators in terms of matrices. It wasn't so clear how useful this was, and in the end Schrodinger ended up showing that representing these operators as differential operators was much more useful than thinking of them as matrices.

For more than twenty years, string theorists have been pushing a long list of proposed abstract formalisms, none of which have gone anywhere in terms of giving any insight into unification. By now, most everyone is pretty dubious whenever they hear about another such proposal.

61. **Juan R.**
May 10, 2005

Peter, since I am new on this blog I read now your previous historic post.

The usual presentation of history by physicists is usually wrong and omit important detailed well-proved. Perhaps the most radical manipulation was those of Newton, when recent research has demonstrated that his chemical career was omited…

There are several example of rewritings of history by physicists. This is not so strange for understanding. Think during one instant in string theory and the manipulation of mass media, the neglect of other schools (many laymen still think that string theory is the only approach to QG), and the rewriting of string theory history.

This is also true of usual history for group theory. For a more realistic view I recommend

Foundations of Chemistry 2001, 3, 55?78.

This paper traces the origins of Eugene Wigner?s pioneering application of group theory to quantum physics to his early work in chemistry and crystallography. In the early 1920s, crystallography was the only discipline in which symmetry groups were routinely used. Wigner?s early training in chemistry, and his work in crystallography with Herman Mark and Karl Weissenberg at the Kaiser Wilhelm institute for fiber research in Berlin exposed him to conceptual tools which were absent from the pedagogy available to physicists for many years to come. This both enabled and pushed him to apply the group theoretic approach to quantum physics. It took many years for the approach first introduced by Wigner in the 1920s ? and whose reception by the physicists was initially problematical ? to assume the pivotal place it now holds in physical theory and education. This is but one example that attests to the historic contribution made by the periphery in initiating new types of thought-perspectives and scientific careers.

More data

When Abraham Pais asked Wigner whether the vastly increased complexity of the calculations involved in the transition from three to four particles (in the Schr?dinger equation) marked his first full awareness of the power of group theory, Wigner replied that his first awareness of the power of group theory in facilitating calculations arose out of his work on the lattice structure of rhombic sulfur.

62.
May 10, 2005

Doesn't this article require a subscription?

63. **JC**
May 10, 2005

Peter,

Excluding the string theory crowd, why are some physicists resistant to using "new mathematics"?

I can perhaps understand why an experimentalist would be resistant to "new mathematics", when most "new math" doesn't really help them much in their day to day research work. I've noticed quite a number of particle phenomenology folks and even some string theorists who are particularly resistant to "new mathematics", unless the "new math" is "forced" upon them by the "experts" in the field (ie. like a Gell-Mann or a Witten).

64.
May 10, 2005

"What is a matrix?"
-Werner Heisenberg, 1925

"What is the matrix?"
-Keanu Reeves, 1999

Real World Strings

May 16, 2005
Categories: Uncategorized

Quite a few people have written in to point out to me a recent paper by some condensed matter physicists about the possibility of trapping a fermionic atomic gas in a vortex inside a Bose-Einstein condensate. As far as I can tell, about the only thing this has in common with superstring models of quantum gravity and elementary particles is that their abstract starts the same way as many superstring abstracts: "Supersymmetric string theory is widely believed to be the most promising candidate for a 'theory of everything'". This article has gotten wide attention in the press and on the internet at Slashdot which informs us that this will "(provide) the first experimental evidence to support superstring theory." At Slashdot you can also read comments from large numbers of confused souls who now believe that experimental confirmation of superstring theory is right around the corner. Obviously this is about as absurd as believing that the existence of my shoelaces provides excellent experimental confirmation of the existence of open strings.

Another weird related phenomenon is the wide-spread idea that violin strings somehow have something to do with superstring theory. For some reason it always seems to be violin strings rather than, say, electric guitar strings. Maybe string theory would be more popular if it would make the connection with a more popular music form. The violinist Jack Liebeck has been going around with physicist Brian Foster, with Liebeck giving concerts in which he "demonstrates superstring concepts on his violin." The performance ends "with a duet for two violins in which lecturer and soloist join forces to illustrate the production of mini Black Holes" at the LHC. I really think an electric guitar would be a lot better for this purpose.

These performances are taking place at dozens of locations around the world, are somehow part of "World Year of Physics 2005", and supposedly educating people about science. They invoke the memory of poor Albert Einstein, implying that he has something to do with superstring theory since he played the violin and searched for a unified theory. Unfortunately Foster and Liebeck don't seem to be coming to New York, although they were at Cornell this past weekend.

Along the same lines, for something truly weird, get a copy of Einstein's Violin: A Conductor's Notes on Music, Physics and Social Change, by Joseph Eger, the music director of the Symphony for United Nations. This book, besides also invoking poor Einstein, goes on in an extremely repetitive fashion about how superstring theory shows that music and fundamental physics are all the same thing. Eger has all sorts of original insights including for instance:

"Science had its heyday during Sputnik and then gradually faded until the eighties, when string theory came to the fore."

"Religious fundamentalists, big business, and politicians, especially of the neo-conservative variety, have been quick to appropriate quantum mechanics and a perversion of the new music to sell their fundamentalist religion, anti-Darwin ideologies, and biological nightmares."

"On this cosmological scale, and since we are postulating that the universe is music and that music expresses and explains the universe, then we can take the next logical step, that music could hold the key to a T. O. E."

Evidently Witten is guilty of at least not discouraging the author, a sin for which I hope he is punished by having to read this book:

"One day in the eighties, driving with Ed to New York from Princeton, he responded to my question about what he was working on by excitedly telling me about string theory and its ten or more dimensions. Bewildered yet emboldened by this brilliant scientist, I tentatively spoke of my theory that the universe is made of music. Half expecting polite derision, he thought for a few seconds and calmly responded affirmatively."

Comments

1. **alejandro**
   May 21, 2005

   more real strings

   http://www.interactions.org/cms/?pid=1019988

2. **Juan R.**
   May 18, 2005

   More music

   STRING THEORY: LYRICS AND STAGE DIRECTIONS

3. **Roy**
   May 18, 2005

   M.S. El Naschie is of course, the editor of Chaos, Solitons and Fractals. The rest of the gang(not say that they are tainted by association with their boss) are here, http://www.elsevier.com/wps/find/journaleditorialboard.cws_home/967/editorialboard

   In between chuckles, I am extremely disturbed that a reputed publisher would publish stuff that looks like it's excerpted from an anti-Sokal parody. Does Reed Elsevier publish more of these journals?

4. **ksh95**
   May 17, 2005

   …was making fun of the idea of theoretical physics thinking it had something to do with music, in this case a certain sort of popular music performance. If you can't make fun of popular music, what can you make fun of?
   …

   I thought it was the modern-day cyber incarnation of blackface comedy, but after a little research it turns out that "50 cent" is a highly successful rap artist…my fault I guess?

5. **Peter Woit**
   May 17, 2005

   Maybe I've got it wrong, but it seemed to me that whoever wrote that little bit of parody wasn't making fun of anyone disadvantaged or different than them, but was making fun of the idea of theoretical physics thinking it had something to do with music, in this case a certain sort of popular music performance. If you can't make fun of popular music, what can you make fun of?

6. **ksh95**
May 17, 2005

But beware of making fun of any of this. Surely Mr. El Nachie is a member of one disadvantaged group or another, so some commenters here will be offended.

Hmmm, Maybe I'm the only one who doesn't think that "…they're different than me, hahaha…" is funny.

And just for the record. I'm not a disadvantaged minority, gay, deaf, female, christian, or liberal. It just seems to me that the person who looks at different cultures and then proceeds to burst into gut-busting laughter…can't be very intelligent.

Maybe I'm wrong

7. **JC**
May 17, 2005

Just looking at El Naschie's listing of papers on SPIRES, it looks like he published around 56 papers over the last 4 years. This would be around 14 papers per year, or a paper every 3 or 4 weeks! When was the last time a bigshot, like an Ed Witten or John Ellis, published this many single-authored papers in one year?

On the surface it looks like El Naschie is using the journal "Chaos Solitons Fractals" as if it was his own personal "vanity" journal. In the publishing business the publishers with the lowest reputation seem to be the "vanity" publishers, who will publish just about anything for a fee. (These are usually the publishers of last resort for authors who have been turned down by just about every other publisher). Have some journal publishers stooped down to the point of offering "vanity" journals?

8. **D R Lunsford**
May 17, 2005

M. S. El Naschie = "Les Machines" – could it be those kwazy kwazy Bogdanoffs?

-drl

9. **D R Lunsford**
May 17, 2005

"Big Twang" – give that guy a gold star! (Still chuckling and the origin of the big U will never be the same…)

-drl

10. **Peter Woit**
May 17, 2005

Hi Kanex,

Thanks for the comment, those are extremely impressive papers. One might wonder how they got published in an Elsevier journal, since such journals are known for their high prices and correspondingly high editorial standards. The journal is online (if you are at an institution that sends big bucks to Elsevier) at http://www.sciencedirect.com/science/journal/09600779

The fact that the editor is named M.S. El Naschie may have something to do with why it publishes all those papers.

But beware of making fun of any of this. Surely Mr. El Nachie is a member of one disadvantaged group or another, so some commenters here will be offended.

11. **Kanex**
May 17, 2005

Hi Peter. I've been reading your blog for a long time and this is my first post 😉

I'd like to share with you some amusing papers. I think they are even better than warren siegel's parodies 😉

http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=ea+El+Naschie,+M+S

12. **ksh95**
May 17, 2005

Bump my new CD, homies, shit contains the world-formula, if you know what I'm saying. But it's LQG not string stuff, aaiight!?

Yes, I also think it's funny to parody disadvantaged urban youth. Maybe tomorrow we can parody gay people.

P.S.

When deaf people speak they sound funny and those Arab turbans look weird…maybe you could also touch on them.

thanks and keep up the good work

13. **Alejandro**
May 17, 2005

"The pleasure you get from math/physics is just like the pleasure you get from music really"

I am sure that string theoretists get much pleasure by doing their maths. But it doesn't follow they get much of real physics.

Personally I were never able to play the violin, Now I am learning to use a theremin.

14. **D R Lunsford**
May 17, 2005

“On the Analysis of Chopsticks and Attendant Phenomena” – Gang of Four^h^h^h^hEleven

-drl

15. **Steve**
May 16, 2005

This would be just a Green-Schwarz superstring analog? Interesting actually, if it could be done and string methods could prove powerful tools in condensed matter theory and in ongoing questions in qcd. Confusion among people though and again in the press, just like people thought black holes had actually been made at Brookhaven.

There is a definite and well-known connection between music ability and math ability. The pleasure you get from math/physics is just like the pleasure you get from music really. Both are a powerful symbolic language. I myself studied classical guitar in the 80s (and still do)and wanted to do music as as degree but ended up doing theo. phys/math instead (also my family did’nt consider music a “real degree” I remember)

The violin analogy has been used in just about every popular book on string theory: one vibrational mode or note corresponds to an electron, one to a quark etc. A nice enough idea but one that has never actually been made to work, at least with the particles we actually know to exist. Eddie Van Halen’s “Eruption” solo would probably much better describe the production of black holes at the LHC.

PS I see you are back Peter. I was wondering if you had gone off to join the cinema line a week early for the first showing of the new Star Wars film:)

16. **Anonymous**
May 16, 2005

Well, string theory was originally studied to understand strong interactions; what we now call QCD strings. Perhaps the study of Bose-Einstein vortices as a noncritical string is a return of string theory to its physical roots. As a phenomelogical model, the Polyakov action fits in with lattice simulations to a remarkably good accuracy. In fact, Polyakov came up with his action while trying to reformulate Yang-Mills theories using Wilson loops. Perhaps the same thing will hold true for vortices as well.

I think noncritical string theory is worth persuing to understand QCD. The theory where glueballs are closed strings and mesons are open strings with quark flavor Chan-Paton factors. The string network picture of gauge theories certainly adds weight to this idea with the interesting twist that strings can branch.

In fact, string theorists are trying to do the same thing. They are looking at string theory in AdS_5 as a dual description of QCD.

It certainly is plausible that there might be an S-dual description of superconductors and superfluids using vortices based upon noncritical string theory just as some gauge theories admits an S-dual description using monopoles. In fact, I think this has already been done by some theorists working on the Kosterlitz-Thouless phase transition.

The prediction of 26 or 10 dimensions only comes about, I think, if we restrict ourselves to the simplest conformal field theories and insist upon constant dilaton fields, etc..

17. **Ben the Engineer**
May 16, 2005

The idea that the ‘universe is made of music’ is hardly original. It goes back at least to Pythagoras. At least Pythagoras had a solid theorem to his credit.

18. **50 cent**
May 16, 2005

Bump my new CD, homies, shit contains the world-formula, if you know what I’m saying. But it’s LQG not string stuff, aaiight!?

19. **Maestro**
May 16, 2005

Open ‘E’,STRINGTHEORY, of course can be modified to incorperate the Universal ‘Big-Twang’, or Concerto for T.heoretical O.rchestra E.vents without any major Eighths?, where the Feynman Path integral is replaced by a Conductor Maestro Witten??.. “Marshal feedback”..or ‘My’ feedback theory!..I believe the Proton Decay is replaced with a ‘PHARTON-DECAY’ ideal G.A.S Law?

It made have its roots in an after dinner speech, which , due to digestive constraints, inflated out of all propotions, causing an ‘Echo’,signal Backreaction around the auditorium, filtered out into the Cosmos, and legend has it, if one was to gently place your Ear to the cosmic soundhole, one can recieve the correct Vacuum Signal, tuned of course to a perfect pitch and chord..G

20. **Wolfgang**
May 16, 2005

Peter,

I think you should have mentioned the band
“Superstring”. Their debut album is called
“artificial stupidity”.
http://www.quirkyworks.com/superstring/music/

JDG and Other Conferences

May 17, 2005
Categories: Uncategorized

This past weekend I was in Cambridge and attended many of the talks at the JDG conference held at Harvard. The conference was nominally in honor of Shiing-Shen Chern, who died late last year, so many speakers made some connection between their work and Chern's, especially his work on Chern classes.

Among the purely mathematical talks I attended was a very clear one by Victor Guillemin on Morse theory and convexity theorems on symplectic manifolds. The material he covered is quite beautiful, but rather old by now. His reason for covering it seemed to be that he has a new book on the topic (with Reyer Sjamaar) called "Convexity Properties of Hamiltonian Group Actions", soon to appear from the AMS in the CRM monograph series, but also available on Sjamaar's website.

Mike Hopkins gave an impressive talk on "Derived Schemes in Stable Homotopy Theory" which was based on very recent work by his student Jacob Lurie. This work involves defining a notion of a scheme which makes sense in the context not of the commutative rings of algebraic geometry, but instead the commutative rings of spectra in stable homotopy theory. It allows a new construction of the tmf (topological modular forms) theory of Miller and Hopkins.

Iz Singer reminisced about taking a class in geometry from Chern at Chicago in 1949, a class which he thought may have been the first one Chern taught in the US. Singer's talk was about "Projective Dirac operators" which have an index which is a fraction. One of the main motivations for Singer's original work with Atiyah on the Atiyah-Singer index theorem was to understand the integrality of the A-hat genus on a spin manifold as coming from the fact that it was an index. On a non-spin manifold the A-hat genus takes on fractional values, and one can use this to prove the non-existence of a spin structure. In work with Mathai and Melrose, pseudo-differential operator techniques are developed that allow one to define a sort of index in these situations where there is no spin (or even spin-c) structure.

There were several talks by physicists, or related to physics. One was by Kefeng Liu, half of which was about some new metrics on moduli space, the other half about some formulae coming out of work on topological strings. For this material, see his talk at last year's Yamabe Conference. Vafa gave a talk on "Topological M-theory", which he motivated by starting with the holomorphic anomaly in the topological string B-model. For quite a while it has been known that you can think of these topological string results as giving a vector in the Hilbert space one gets from quantizing H^3(M), where M is a Calabi-Yau. Topological M-theory is supposed to be something related to topological string theory in much the way the full M-theory is related to the full-string theory, so involves one-dimension higher. Thus it deals with 7-dimensional manifolds and tries to explain some of the phenomena related to topological strings on 6-d Calabi-Yaus in these terms. For more about this, there's a talk by Andrew Neitze on-line that covers some of the same material.

Nikita Nekrasov's talk was about "Z-theory", which is his own name for the same ideas about topological M-theory that Vafa was talking about. He drew a version of the standard picture of the M-theory moduli space, now for Z-theory and with all sorts of mathematical objects attached to the various cusps. Nekrasov gave a similar talk in Nagoya late last year, as well as one at Strings 2004.

While a lot of interesting mathematics has come out of topological strings, the idea that that there is some grandiose unification involving thinking about 7d G2-manifolds seems to me even less promising than the idea of 11d M-theory itself, which for years now seems to have gone nowhere. Just as M-theory has led many physicists to pointless wanderings in 11-dimensions, it now seems to be leading mathematical physics away from rather rich mathematical areas into the complicated geometry of seven dimensions. Undoubtedly this will lead to some new mathematics, but it looks to me like it will be much less interesting than the mathematics emerging from string theory during earlier periods. The interaction between mathematics and physics remains dominated by the ideology of string/M-theory, and this is harming both subjects.

One aspect of the sad state of the interface between math and physics is that virtually no one from the physics department at Harvard seemed to be attending the JDG conference lectures. I'd been expecting to see at least Lubos Motl there, but he was down at Columbia attending a meeting on string cosmology. He reports on the talks here, here, and here, as usual covering very critically a talk on loop quantum gravity, quite uncritically one about the landscape and absurdly baroque constructions that try to make some contact with the standard model. I'm beginning to believe that his "leashing" did have something to do with his criticizing the landscape ideology too vigorously, since he seems to have stopped doing that.

For the latest on the landscape, see a recent talk by Lubos's senior colleague Arkani-Hamed (whom he better not piss off too much) at the PHENO 05: World Year of Phenomenology symposium in Wisconsin, entitled The Landscape and the LHC. Arkani-Hamed's talk begins with the usual strained historical analogy, this time a long and bizarre description of the calculation by Aristarchos of the distance to the sun by the method of parallax. The point of this is highly obscure, but seems to be that since Aristarchos was wrong to find unreasonable the huge distances to the stars implied by the lack of visible parallax, we're wrong to find unreasonable the huge amounts of fine-tuning required by split supersymmetry.

He goes on much like Susskind for quite a while about the glories of the landscape idea, with the twist that supposedly split supersymmetry is "sharply predictive". The only "sharp" predictions he mentions concern a relation between some coupling constants which haven't been observed and likely never will, as well as that there may be a "long-lived" gluino. Not that he actually has a prediction for the mass or lifetime of this gluino.

Comments

1. **CapitalistImperialistPig**
   May 21, 2005

   Re: Lubos, I'm sticking with the PC violation story. But it's probably also true that he had best not offend his natural and most crucial constituency.

2. **D R Lunsford**
   May 19, 2005

   Back to "projective Dirac operators" this must imply giving up on an agreed upon length scale, so it's making a definite statement about mass (which is really an inhomogenous term in the Dirac equation). And there is an implicit statement about matter currents, or really charges, requiring a coupling that is consistent with no given scale. And so this is a definite statement about charges.

   -drl

3. **Juan R.**
   May 18, 2005

   G2 manifolds are not the true geometry of the universe. They continue to be a standard geometry, even when they are not well-understood still by mathematicians.

   I agree with you, Peter, on that the new folk will not provide any significant contribution to our physical ideas about nature in despite of so-many speculation and grandilocuents claims were posted by Schwartz and company.

4. **fysix**
May 18, 2005

You criticize the talk of Nima Arkani-Hamed at Pheno05 at UW-M . Checking the original paper (hep-th/0405159, title too long 😉 by N. A.-H. and Savas Dimopoulos I would rather say that this kind of “orthogonal” thinking should be welcomed rather than condemned.

5. **D R Lunsford**
May 17, 2005

OT – I’ve got to post this:

Pale Blue Dot

http://www.hq.nasa.gov/alsj/a11/images11.html

Read starting here:

“In March 2005”

-drl

6. **Carl Brannen**
May 17, 2005

I attended the talk by Arkani-Hamed on the landscape. There were quite a lot of talks having to do with landscapes at the conference.

Based on my observations of the audience, and the questions or lack thereof, I got the impression that most of the attendees were interested in more traditional elementary particle theory and experiments. Gia Dvali also gave a plenary talk on “attractors in Landscape”. To have plenary landscape talks in a phenomenology meeting was a bit silly.

My own talk was a speculation on the nature of the high energy cosmic ray “Centauro” events having to do with Clifford algebra and a hidden dimension.

Carl

7. **Peter Woit**
May 17, 2005

Note that I wasn’t claiming this stuff was related to physics. For one thing I don’t think Singer et.al’s “projective Dirac” operator is anything local. You need to look at their paper to see exactly what it is.

8. **D R Lunsford**
May 17, 2005

“Projective Dirac operators” – wow that implies a lot of questions…

What’s the adjoint? would probably be #1

How does it couple to mass and matter? #2 #3

etc.

-drl

String Theory and Theology at the AAAS

May 18, 2005
Categories: Uncategorized

Someone wrote to me today to tell me that Harvard's Nima Arkani-Hamed recently gave a lecture in Washington with the title "String Theory — Can We Test It?". Somehow, I suspect that his lecture didn't really give an honest answer to the question, since it would be hard to fill up an hour-long talk by just saying "No".

Looking into this more carefully, it turns out that the talk was part of a "Dialogue on Science, Ethics and Religion" sponsored by the AAAS. At first I thought it was unusual to see a "Science and Religion" program paid for by anyone but the Templeton Foundation (for more about them, see here and here), but it turns out that they are the first organization listed in the list of those providing financial support for the program. I wouldn't have guessed that the AAAS was in bed with Templeton and running programs on "Science and Religion", but this kind of thing doesn't surprise me anymore.

Arkani-Hamed's talk was entitled: Naturalness versus the Superstring Landscape, or, Why Does The Universe Appear Finely Tuned? (not sure why it was advertised with the "String Theory — Can We Test It?" title). The organizer and "respondent" was James B. Miller, an ordained Presbyterian minister with a Ph. D. in Theology from Marquette University. From the abstract it appears that the talk involved Arkani-Hamed's usual claims that split supersymmetry makes "sharp experimental predictions" for what the LHC will see (he seems to have a rather different notion of what an experimental prediction is than most scientists, much less what a "sharp" one is). He also seems to have implied that the superstring landscape scenario predicts split supersymmetry, something that actually isn't the case, or at least is only true in the sense that the landscape predicts nothing at all, and thus is consistent with anything.

Comments

1. **Chris W.**
   May 19, 2005

   To place the religious insinuations in context, see this jeremiad by T. M. Moore, former Executive Pastor of Coral Ridge Presbyterian Church (Florida).

   —
   [Thanks to Arun (I think) for tracking this down a few months ago and posting the link in a comment on Preposterous Universe (Sean Carroll's blog).]

2. May 18, 2005

   Here is an explanation. NAH is a champion of a branch of (meta)physics that can be summarized as "everything goes": large extra dimensions, ghosts, supersymmetry without supersymmetry,
   you name it. Indeed, in the absence of experimental data, such nonsensical, loophole-filling ideas cannot be disproved.
   But every human being needs structure: thus he turns to God for guidance. I am sure that ksh95 would provide an eloquent description of this supernatural phenomenon.
   Jean-Paul

Game Over

May 18, 2005
Categories: Uncategorized

Shamit Kachru (described by Lenny Susskind as the "master Rube Goldberg architect") and collaborators have a new paper out this evening on flux compactifications, one that in a rational world should finish off the subject completely. Recall that Kachru is one of the K's responsible for the KKLT construction of these flux compactifications that stabilize all moduli, and for the last couple years debate has raged over whether this sort of construction gives 10^{100}, 10^{500} or even 10^{1000} possible string theory vacuum states.

Susskind, Arkani-Hamed, and other anthropic principle aficionados have argued that the fact that this number is at least 10^{100} is a great triumph because it means that there are so many vacua that at least some will have small enough cosmological constant to be consistent with our existence. But if there are too many, all hope of getting predictions out of string theory disappears. With 10^{1000} vacua, you can find not only the cosmological constant you want, but probably any values of anything particle experimentalists have ever measured or ever will measure, and the theory becomes completely unpredictive.

Even so, the study of these vacua has become more and more popular over the last year or two, with many arguing that, no matter how big the number is, at least it's finite, so you have improved over the standard model, which has continuously tunable parameters. This argument was made in the panel discussion at the Perimeter Institute a month or so ago. Also, a finite number of vacua allows you to study their statistics, by assigning a weight one to each possible vacuum state and getting a probability measure by dividing by the total number. You can then engage in wishful thinking that this probability measure will be peaked about certain values, giving a sort of prediction.

The new paper gives a construction of flux compactifications of type IIA string theory, and in this case the authors find an infinite number of possibilities. This should kill off any hopes of extracting predictions from string theory by counting vacua and doing statistics. The authors try and put a brave face on what has happened, writing:

"we should emphasize that the divergence of the number of SUSY vacua may not be particularly disastrous. A mild cut on the acceptable volume of the extra dimensions will render the number of vacua finite."

but then they go on to puncture their own argument by noting that:

"one can legitimately worry that the conclusions of any statistical argument will be dominated by the precise choice of the cut-off criterion, since the regulated distribution is dominated by vacua with volumes close to the cut-off."

With this new result, the infinitesimally small remaining hope of getting predictions out of the string theory landscape framework has now vanished. It will be interesting to see if this slows down at all the ever-increasing number of string theorists working in this field.

Update: Lubos Motl has some comments about this same paper.

Comments

1. **Juan R.**
   May 22, 2005

   I said,

   I don't know the level of new students doing PhD and all of that in string theory. Do you know Peter?

   By "level" i mean "the number of"

2. **Juan R.**
   May 22, 2005

   Peter said:

   "No intelligent young person is going to go into this field as long as it is obviously an intellectual disaster area."

   I am not completely sure. Month ago, a joung math student contacted with me for solicintg more information in my open criticism of string theory.

   He was rather misguided in a lot of points. I explained my points, but he remained skeptic. For example, he said that was not true my claim that string theory was substituing the around 20 parameters of SM by more than 10000 new ones. He claimed that Brian Greene's "Elegant" book said that one single parameter was sufficient.

   I explained to him that string equation parameter was valid for "predictions" on 10D, for real 4D one need compactification and all, a priori, predictive power is lost.

   He remained skecptic. He said that if I was saiyng was true, then Brian Greene had said that in his book. !!!!!!

   Some people doing PhD and after leaving the field has contacted to me and agree with my valoration of string theory like a waste of time. I abandoned my string-brane research some time.

   I don't know the level of new students doing PhD and all of that in string theory. Do you know Peter?

3. **Dmitriy**
   May 22, 2005

   Hi Peter,

   What makes me especially nervous, it's the fact that almost everyone looks for physics _beyond_ the Standard Model. Everyone is eager to find its failure. This is nonsense to me. I always thought that a scientist wants to understand nature and I would never expect that he would like to see his model of the world failed. I'm afraid it's all about money.

4. **Alejandro Rivero**
   May 21, 2005

   Offhand I can't really think of many promising "new" fields that a post-1984 tenured string theorist could possibly defect to, other than maybe conventional *particle/astroparticle phenomenology or some area in condensed matter theory.*

Perhaps we should delete the last twenty years from memory and go back to the way we were then. What kid of things was people doing before the upsurge of string revolutions?

I think we were studying the non abelian anomaly in order to use it somewhere in QCD… was it because the glue? Or something of the chiral limit?

Also for sure there was someone there still thinking about flavour. Well, perhaps only H Fritzsch in Germany.

There was also detectability of new particles via their interaction with nucleus. Witten and Goodman on dark matter for instance. As well as some low-energy theorems for the Higgs boson.

5. **Peter**
May 21, 2005

I certainly agree that the whole string theory phenomenon is due to HEP being a victim of its own success. Until someone does an experiment which disagress with the standard model and gives a clue how to get beyond it, things are going to be very tough.

But this is no excuse for getting stuck in an endless investigation of one speculative idea about going beyond the standard model, one that has clearly failed. It is amazing to see how unwilling people are to abandon an idea they have a lot of time invested in, no matter how clear it is that the idea can't work.

6. **Dmitriy**
May 21, 2005

Hi everyone,

don't you think that all the problems of string theorists are mostly due to overall stagnation of HEP as a field. There is nothing really new found in an experiment (beside maybe the mass of neutrino, which hardly changed anything) and I doubt that anything can be found at LHC beside maybe Higgs. Without experimental data people are just doomed to either leave the field until any intersting data is available or do "math". It's clear that we are way too far from energies when quantum gravity effects can be of any significance. So overall I don't see why string theory approach is better or worse than any other quantum gravity theory.

But even this is not a big problem. What really looks bad is a typical career path that one has to follow to stay in physics. There is no room for mistakes. The competition for a permanent faculty position is so tough that hardly anyone who doesn't have tenure would risk to switch a field just because he thinks that the field is going in a wrong direction. It takes many years to get a certain level of compitence to do anything sensible. I'm afraid we just work too much and don't have much time to think.

7. **Tony Smith**
May 20, 2005

In a comment to his string theory blog "Game Over", Peter says "… string theory … is obviously an intellectual disaster area.".

I am reminded of a story mentioned in another of Peter's blog entries about Hilbert and Heisenberg. According to Thall's History of Quantum Mechanics, at http://mooni.fccj.org/~ethall/quantum/quant.htm :

"… Hilbert suggested to Heisenberg that he find the differential equation that would correspond to his matrix equations. Had he [Hesenberg] taken Hilbert's advice, Heisenberg may have discovered the Schr?dinger equation before Schr?dinger.
When mathematicians proved Heisenberg's matrix mechanics and Schr?dinger's wave mechanics equivalent, Hilbert exclaimed,

"Physics is obviously far too difficult to be left to the physicists …" …".

Since the string theorists seem to be becoming an obstruction to the advance of physics, and since JC said a comment on Peter's blog:

"… Only … scenario … which could possibly cause a huge landslide exodus from string theory, is if the government grant agencies (ie. DOE, NSF, etc …) all decide to stop funding …",

I have a suggestion. Perhaps the black-budget programs of the USA might be expanded to include a well-funded space exploration and colonization program in which a large human colony is sent into space to contact other civilizations on some of the newly discovered extrasolar planetary systems. Since the colonizers will be the ones who form the aliens' first impressions about humans, the colonizers should be only the best and the brightest so that the aliens will be dazzled by the brilliance of humanity. Obviously that means that the ship should carry all the most outstanding superstring theorists, who would be honored to be chosen. In order to impress the aliens even more, the aliens should be deceived into thinking that humanity back on earth included some who were even more brilliant than the best-and-brightest superstring theorists. Therefore, the ship carrying the superstring colonists should be modestly named something like, for example, the B Ark.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

8. **JC**
May 20, 2005

Peter,

Only other scenario I can think of offhand which could possibly cause a huge landslide exodus from string theory, is if the government grant agencies (ie. DOE, NSF, etc …) all decide to stop funding any string theory research.

It would be interesting to see what types of future papers will cite this particular Kachru et. al. paper hep-th/0505160

Offhand I can't really think of many promising "new" fields that a post-1984 tenured string theorist could possibly defect to, other than maybe conventional particle/astroparticle phenomenology or some area in condensed matter theory. I can't really see many string folks suddenly defecting to loop quantum gravity, nor to many of the other camps working on various partisan approaches to "quantum gravity".

9. **Peter Woit**
May 20, 2005

Hi JC,

What's really amazing to me is how string theorists refuse to give up, no matter how bad it gets. At this point it's completely clear that the idea of string theory based unification is dead, but no one seems to be giving up anyway. The attitude is "we're going to keep doing this until someone gives us a new fad to work on", but there is no encouragement at all for anyone to try and find something new. String theorists still will only hire string theorists.

I don't see anything changing unless Witten comes up with something new. Increasingly the people who got tenure pre-1984 and know about something other than string theory are getting old. Those who got tenure post-1984 more and more often don't know about anything except string theory, and have no interest in learning. No intelligent young person is going to go into this field as long as it is obviously an intellectual disaster area.

10. **Thomas Larsson**
    May 20, 2005

    *So I somewhat marvel that people aren't more interested in the old fashioned stringy models or even the more modern brane world stuff. No one has outputed a no go theorem on the small/positive cc for those classes of theories, so why aren't people still looking for whatever *that* may be.*

    I think that no-go theorems more or less exist. If you want to use AdS/CFT, it is certainly a problem if the CC is positive, since that rules out the AdS part. People have tried to invent some kind of dS/CFT, but AFAIU the CFT must then live at temporal negative infinity, which apparently is problematic and usually a discarded possibility.

    Don't forget that there are other problems, too. Without flux compactifications you have at least one unbroken SUSY and unstabilized moduli, i.e. new long-range forces, in disagreement with experiments, right?

11.
    May 20, 2005

    While not defending the Landscape approach to phenomenology, the point is that string theory contains these flux fields, they may be turned on, and so the number of possible supersymmetric vacua is much larger than previously imagined. As far as other approaches are concerned, people use flux vacua to construct brane worlds that are on a more solid theoretical footing, i.e. with all moduli stabilized and with an acceptable final cosmological constant. Once you've found the possibility of turning on flux, you can't pretend that it doesn't exist. What this indicates, to me at least, is that finding a useful (testable) string phenomonology will involve more than finding a proper vacuum, and will most likely involve cosmological dynamics. Finally, Occam's razor is fruitfully applied to starting points (two examples being Einsteins equations or the replacement of particles with strings) and really doesn't apply to solutions (all gravitational solutions in one case or all possible vacua in the other)

12. **Fred**
    May 20, 2005

    What I have never understood about the landscape business. Why exactly do people take flux compactifications so seriously if this is what it outputs, eg an unpredictive mess?

    There are many potential models in String theory, many Brane inspired models and what not. I take it the former is the only one (to date) that has outputed a small and positive CC. However typically the tradition in physics is, if its too messy move on and find something else. Typically (a variant of Occams razor) that turns out to be the simplest and invariably the correct answer (even if nothing has been discovered that fits the bill, we usually assume the universe is governed by simplicity).

    There have been many theories that have been discarded in physics b/c of this premise, even before whatever else replaces *it* has been discovered.

    So I somewhat marvel that people aren't more interested in the old fashioned stringy models or even the more modern brane world stuff. No one has outputed a no go theorem on the small/positive cc for those classes of theories, so why aren't people still looking for whatever *that* may be.

    It seems to me String theory is still alive and well, its just that many of its practioners have abandoned a perfectly sensible attempt at understanding qg, with a weaker phenomenological theory that clearly doesn't cut it.

13.
    May 19, 2005

    I have a theory that Quantoken is a monkey. For proof, look at his typing skillz.

14. **Quantoken**
    May 19, 2005

    Correction.

    I said: "Most of the monkey patents would look like completely garbage. But it's OK. We can eliminate most of them by the **anthropic principle**."

    On a careful thought, I would replace it with **Monkey Principle**. That would also be what I call what string theoretists call anthropic principle. I call it **monkey principle** 🙂

    Quantoken

15. **Quantoken**
    May 19, 2005

    Peter said:
    "Susskind, Arkani-Hamed, and other anthropic principle aficionados have argued that the fact that this number is **at least 10^100** is **a great triumph** because it means that there are so many vacua that at least some will have small enough cosmological constant to be consistent with our existence."

    Hey I would suggest something much better than 10^100 cavuas, I call it **the monkey theory**, a true **theory of every thing**. And this theory is **absolutely correct and permanently safe** from being falsified.

    Very simple, you just allow a monkey to hit a keyboard arbitrarily for a little while, and then repeat again, and try again. The possible number of monkey patents it may come up with is definitely limited, something between 4^4^4 and 6^6^6, which definitely look better than 10^1000 vacuas.

    Most of the monkey patents would look like completely garbage. But it's OK. We can eliminate most of them by the anthropic principle. The correct monkey patents would at least make some sense in English. And certainly, once in a while it may come up with something that reads like "F=MA" or "E=MC^2", which happen to be the correct physics theory.

    What's the odd of a monkey type 6 letters and it happen to be "**E-MC^2**"? It's certainly not zero, and roughly one out of 10^11. That's certainly a much better odd than one in 10^100 vacuas being the correct one!!!

Just allow the money keep typing, it eventually give you all monkey patents that is conceiveable. Any correct and incorrect theories we human can think about in the past and in the future million years, would all eventually come out of the monkey typing machine. Theory of every thing, every theory of thing, every theory of every thing, every every theory of thing, they will all come out with pretty good odds, not just a TOE.

And it is verifiable experimentally!!! Actually as I have demonstrated I can make **"very sharp predictions"** out of this theory. I predicted that "E=MC^2" can come out of it with an odd of roughly 1 in 10^11. It's not observed experimentally yet, but no one will doubt it. It is only that we do not have enough monkeys, (or in another word we do not have enough string theorietists to study all of the 10^100 cavuas).

You see, my monkey theory definitely beats string theories already 🙂

Quantoken

16.
May 19, 2005

there are more (vacua) in heaven and earth than are dreamt of in your philosophy.

17. **Juan R.**
May 19, 2005

JC,

"How fast do you think people will abandon the field?"

I replied to this in another part.

With each new failure of stringy endeavour, string adherents begin a new, more arrogant, folk tale.

The history that i write is not exact but one could say that with the failure of ST like a theory of the strong, they claimed that also was a theory of gravity. With the failure of first versions of "superST", claimed that was also a theory of rest of interactions and particles. With the failure, they claimed that M theory was the theory of everything, from particles or gravity to cosmology, big bang, and other universes newer observed but "living" in some "part". M-theory was also the basis for some eccentric topics like aliens (Kachu), extrasensorial perception, would reformulate QM eliminating its present weird state (Witten), etc.

I think that they will interpret the failure of M theory like a symptom of that the theory was conservative in excess. Probably it was a **theory of more than everything** and that would explain the current failure 🙂

18.
May 19, 2005

String Theory appears to be a toaster. M-theory however, though lacking in real physics, is nonetheless going somewhere.

19. **JC**
May 19, 2005

Peter,

If this is indeed "game over" and string/M-theory is not a mysterious space ship but really a toaster, how fast do you think people will abandon the field?

20. **Anonymous**
May 19, 2005

But these are vacua where SUSY is unbroken, right? Shouldn't one expect infinitely many, since there are moduli spaces of SUSY theories? Isn't what matters the number of non-SUSY vacua?

21. **D R Lunsford**
May 19, 2005

April is the cruelest month, breeding
Didacts out of the dead landscape, mixing
M-ory and D-sire, stirring squareroots with string pain.

22. **Not a Nobel Laureate**
May 19, 2005

Remarkable how much trouble one can get into by taking the simplest object beyond a point, a string and applying the constaints of quantization and relativity.

"This is how String Field ends,
Not with a Bang, but
With a Whimper."

[Sorry T.S. Elliot]

23. **D R Lunsford**
May 18, 2005

The CC is zero. It's an artifact of decoupling of light and gravity.

-drl

US HEP News

May 19, 2005
Categories: Uncategorized

The US High Energy Physics Advisory Panel (HEPAP) is meeting in Washington yesterday and today, and some of the presentations are already available on-line. These include one from the DOE Office of High Energy Physics which notes that, given budgetary constraints, the only way significant funds will become available for new projects (including significant work on the proposed ILC linear collider), is by shutting down operations at the Tevatron or PEP-II. The Tevatron is now scheduled to operate until 2009 (at which point it can't compete with the LHC), PEP-II at SLAC until 2008. The DOE is asking the P5 committee to advise about whether or not it might be a good idea to shut these facilities down early, and redirect the funds that are freed up elsewhere.

There are also reports on the status of PEP-II and the Tevatron. PEP-II and other accelerators at SLAC were shut down after an accident last October, only turned back on last month. The plan now is to run the machine steadily until July 2006, with only a one-month break in October. Presumably it's down today, since if you try and connect to the SLAC web-site, you get a message saying that power is out at SLAC due to a tree falling and severing the main power feed to the site.

The Tevatron is doing well this year, recently achieving record luminosity, and its integrated luminosity so far this year is running ahead of even optimistic projections. It seems highly unlikely to me that the P5 committee will suggest shutting it down early.

There's also a report from the ongoing National Academy of Sciences EPP2010 study of the future of US particle physics. Presentations from a meeting earlier this week at Fermilab are now available. These include presentations dealing with what is going on outside the US, including ones from DESY in Germany and KEK in Japan.

The biggest issue facing US particle physics is what to do about the International Linear Collider (ILC) project. In the presentation of Michael Witherell (ex-director of Fermilab), he notes that the world is in a transition from having five major labs running the largest accelerators to possibly only two: CERN with the LHC, and wherever the ILC is sited, if it is built. For US experimental high energy physics to remain a world leader, it is crucial that the ILC be built, and built in the US. Witherell recalls how the US HEP budget has declined by $100-150 million in real dollars over the last few years, but then gives a plan for the future that involves this budget increasing by 4% over inflation every year, something I find hard to believe is going to happen. The EPP2010 site also contains feedback they have received from various members of the community in response to questions about plans for the ILC.

In other experimental HEP news, the Experimental High Energy Physics Job Rumor Mill has been revived, joining the Theoretical Particle Physics Jobs Rumor Mill. Send them both your inside information!

Comments

1. **loser**
   May 21, 2005

   Speaking of pork, and directed to the first comment, the DeLay-Frist wing of the GOP is a lot of heat and flash, but is losing political capital rather quickly and will badly need to do horsetrading just to stay elected next year.

   Consider instead who IS speaker of the house, has been the speaker, and who will probably BE the speaker for the next 3-5 years. And what national accelerator lab is in his district? Hmmmm….

2. **loser**
   May 21, 2005

   Scuttlebutt is that since SLAC's high energy star is falling, BES will move in over the next couple years to completely take over SLAC, with LCLS as the star attraction. So that might accelerate their demise as a HEP lab.

   Fermi is clearly pushing hard to be in a strong position when siting for ILC is decided.

   Also, NANL: guess again. The NIH budget is tapering off now also. Pork is where it's at.

3. May 21, 2005

   Here we are now – entertain us

4. **Not a Nobel Laureate**
   May 19, 2005

   "Witherell recalls how the US HEP budget has declined by $100-150 million in real dollars over the last few years, but then gives a plan for the future that involves this budget increasing by 4% over inflation every year,"

   In your dreams, Mikey, in your dreams.

   That money is going to medical research now as the Boomers begin to confront their mortality.

5. May 19, 2005

   Well, for the ILC to be built in the US, it's clear that for political reasons it would have to be located in Texas, and in particular within one of DeLay's gerrymandered congressional districts, which would conflict with the "linear" requirement.

Stalking the Riemann Hypothesis

May 22, 2005
Categories: Book Reviews

My friend Dan Rockmore has a new book out, entitled Stalking the Riemann Hypothesis, which is quite good. Dan had the misfortune of starting work on this book at the same time as several other people had the idea of a popular book about the Riemann Hypothesis. For better or worse, his has appeared after the others, which came out last year. In solidarity with him, I haven't read the others, so can't directly compare his to theirs.

Dan's book begins with a mixture of history and explanations of the math involved. In the sections having to do with more recent work, he concentrates on one particular approach to proving the Riemann hypothesis, an approach that has interesting relations to physics. This involves an idea that goes back to Hilbert and Polya, that one should look for a quantum mechanical system whose Hamiltonian has eigenvalues given by the Riemann zeta-function zeros. Self-adjointness of the Hamiltonian then corresponds to the Riemann Hypothesis. This conjecture has motivated a lot of the research that Dan describes in detail, including relations to random matrix theory, quantization of chaotic dynamical systems, and much else.

Philosophically, I'm very fond of the idea that quantum mechanics is basically representation theory, and that the way to produce interesting quantum mechanical systems is by using geometric constructions of representations using cohomological or K-theoretic methods. While I'm no expert on the Riemann Hypothesis, my favorite idea about it is that proving it will require a mixture of the Hilbert-Polya search for a quantum mechanical system, together with the cohomological approach that worked in the case of function fields. In that case, the Weil conjectures famously were based on the idea of constructing an appropriate cohomology theory. This was carried through by Grothendieck and others during the fifties and sixties, with Deligne finally using this technique to get a proof in the early seventies.

For the number field case, the most developed conjecture that I know of about what might be the right sort of cohomology theory is due to Christopher Deninger. He has a very interesting recent review article about this, see also his lecture at the 1998 ICM.

Update: For another nice discussion of zeta-functions and the Riemann Hypothesis, see John Baez's latest This Week's Finds.

Update: There's a nice article in the Washington Post about Dan and his book.

Comments

1. **D R Lunsford**
   May 27, 2005

   In light of the Bump paper, this is fun:

   http://arxiv.org/abs/hep-th/0007199

   -drl

2. **D R Lunsford**
   May 27, 2005

   This is excellent:

   http://match.stanford.edu/bump/notes.pdf

   -drl

3. **Peter Woit**
   May 27, 2005

   I'm not sure exactly what Dan meant by "ill-defined" in this case. The Clay problem is trying to ask for a big breakthrough in what one can rigorously say about the solutions of Navier-Stokes, but since this is hard to define, they give four examples of specific theorems whose proof would count.

   I have looked recently at the de Branges paper. The first version had some intriguing mentions of quantum mechanics and listed Weyl's Group theory and quantum mechanics book as a reference (although the reference didn't correspond to anything in the text). These have been removed, but the rest of the paper looks much the same, although I don't have the old version to compare to.

4. May 27, 2005

   About De Branges proof….you say you tried to read it. Have you read it recently? it seems to be dated April 2005.

5. May 27, 2005

   Peter, you friend says on that post article that navier-stokes is "ill defined". What exactly does he mean by that?

6. May 25, 2005

   Since Riemann Zeta is perhaps the most fundamental function of the complex analysis, it would not be too surprising if conformal invariance would be involved with the proof of Riemann hypothesis.

   It is easy to find support for this intuition. The eigenfunctions of the scaling operator L0=t/dt with t in R+ come as functions
   t-s with eigenvalue given by λ=-s. For integration measure dt one obtains a counterpart of plane wave basis by assuming the complex conformal weight to be s= 1/2+iy implied by Riemann hypothesis. This easy to see by taking u=log(t) so that inner product transforms to an inner product of plane waves exp(iy1u) and exp(-iy2u). Zeros and also linear combinations of zeros of zeta with integer coefficients would define conformal weights for a discrete basis of a Hilbert space. If the real part of the conformal weight differs from 1/2, the functions in question diverge exponentially at the limit when u goes to +∞ or -∞. Obviously Riemann hypothesis is very natural in this context.

My own proposal for the strategy of proving Riemann hypothesis is however based on the idea that Riemann zeta at line Re[s]=1 defines an inner product for eigen functions of a modified operator L0 representing non-Hermitian operator analogous to an annihilation operator and having zeros of zeta as its complex eigen values. These functions define a generalization of coherent state basis rather than that of an orthogonal state basis as in the case of Hilbert-Polya conjecture.

This means that their inner products are non-vanishing and proportional to the values zeta (z1+z2*) and thus proportional to zeta(1+i(y1-y2)) for the eigen values at critical line (note that zeta diverges at s=1 but this is actually not a problem). Riemann hypothesis guarantees the orthogonality of states with conformal weights s=1/2+iy to a negative norm state having a vanishing conformal weight s=0. This condition would exclude non-critical complex zeros and I have proposed a reductio ad absurdum argument for a proof of Riemann hypothesis in the article Pitkänen (2003), *A Strategy for Proving Riemann Hypothesis*, Acta Math. Univ. Comeniae, vol. 72.

The article can be also found at my homepage

The complex conformal weights appearing as linear combinations of imaginary parts of non-trivial zeros of zeta have a key role in TGD. Since physical states must have real conformal weights this gives rise to conformal confinement forcing many particle systems to behave like single quantum coherent units. There are physical motivations for the proposal that dark matter corresponds to a conformally confined quantum coherent phase in which ordinary particles have complex conformal weight.

The gluonic color glass condensate observed in RHIC behaving like liquid rather than quark gluon plasma and having black hole like properties could be one instance of this phase associated with a highly tangled color magnetic flux tube in Hagedorn temperature defining the hadronic analog of black hole with effective gravitational constant corresponding to hadronic length instead of Planck length.

Matti Pitk?nen

7. **Chris W.**
May 24, 2005

I just emailed him about Week 216. He is still working on it, which is why it isn't linked yet as the latest edition.

8. **Levi**
May 24, 2005

Interesting stuff from Baez both in week216 and in week215. He loves to look at the big picture.
Hope he keeps writing on this subject.

9. **Peter Woit**
May 24, 2005

A bit like string theory, no?

Not very much, actually. Each of the many ideas floating around about the Riemann Hypothesis is being pursued by a small number of people, which is a quite healthy situation. In particle theory, one speculative idea (string theory) is being pursued by thousands of physicists, and it remains very difficult to get a job doing mathematically based particle theory research unless you are willing to do string theory. And if any speculative idea about the RH had failed as miserably as string theory unification has, it would have been long ago abandoned by everyone involved.

10. **Levi**
May 24, 2005

Two comments:

1/ For mathematically literate readers (e.g. nearly all of the readers of this blog), the best introduction to the Riemann Hypothesis is the book "Riemann's Zeta Function" by H. M. Edwards. Very readable, although you can't zip through it like a novel. And it's a Dover paperback so it's cheep.

2/ The approach Peter mentions is promising, but a lot of smart people have been pursuing it for quite a while without success. A bit like string theory, no? Maybe physicists will have to learn to be as patient as mathematicians.

11. **anon**
May 24, 2005

My guess would be that the proof of the Riemann Hypothesis would rely on something vaguely similar to Deligne's proof of the Weil Conjectures (the last conjecture,R.H. over finite (?) fields) in that some similar mathematics for Deligne's proof would be necc. for the RH. So I'd guess it'd demand a lot of stuff from Grothendieck. OR, something else…

My wild guesses are about as good as anybody else's out there…

12. **Walt Pohl**
May 24, 2005

Why cast aspersions, Tony? De Branges' paper is there for everyone to see. Download it, and see for yourself if the proof is valid.

13.
May 23, 2005

One of deBranges' former PhD students (Li) immediately posted a paper on the arXiv indicating an error in deBranges' argument.
D. Jerison at MIT read de Branges' paper carefully after his announcement, and if I remember correctly gave a seminar talk about it.
The subsequent lack of interest in the paper has to do with the contents of the paper and de Branges' failure to address or acknowledge the criticisms, not any particular prejudice of "Western" mathematicians (Li is Chinese, and Jerison an American, by the way).

There are certainly people who work publically or privately on the Riemann hypothesis, such as Sarnak, Connes, Cohen, Deninger, not to mention pretty much the entire analytic number theory community. The people other than deBranges who do this, generally manage to remain credible over the years.

Rockmore's book tries too hard to avoid formulas, in my opinion, and is interesting (though not necessarily to the general public) mainly for the academic gossip therein.

14.
May 23, 2005

Silly nitpick: While Wiles announced the proof at Cambridge, his official appointment at the time was at Princeton, not Cambridge as Tony asserted

15. **Peter Woit**
May 23, 2005

Hi Tony,

The situation of Wiles and deBranges is quite different. First of all, the math community didn't work as a team to fix Wiles' proof. He fixed it himself (with some help from his ex-student Richard Taylor, after a team of referees went over the manuscript, and one found the initial error). People would have taken any purported proof of Fermat by Wiles seriously, the fact that he was proving Taniyama-Shimura-Weil just made it more interesting. The significance of Taniyama-Shimura-Weil is that it is part of the "Langlands Conjectures". These are not a fad like string theory, many of them have been rigorously proved, and they give results about number theory that are accessible no other way (e.g. why Fermat is true). They are a set of very deep structural insights into how number fields behave, and there are a huge number of concrete, rigrorous results to back that statement up.

Dan's book doesn't have much at all about deBranges, from what I remember it is one of the others that has quite a bit about him, from a sympathetic point of view. Have you looked at de Branges's manuscript? I can't speak for why others haven't made a major effort to understand it, but I tried and I can tell you why I gave up. I found it completely impossible to follow. The manuscript is not written in anything like a standard mathematical form, with the structure of the argument outlined, together with a proof of each step. From the main manuscript itself, you can't even tell that there is supposed to be a proof of the RH in there. His separate document ("Apology….") has more of an explanation of what he is doing. Maybe if you spent a lot of time trying to read that first, you might have a fighting chance of figuring out what argument he is trying to make.

The case of Wiles is very different. His argument was laid out completely clearly and explicitly, so that anyone who was familiar with the technical tools he used could easily follow what he was doing. And if you weren't familiar with the technical tools, it was explicit what they were, and you could go out and read up on them elsewhere.

I have some sympathy for the idea that de Branges may actually have a proof, or enough of a good idea about one that maybe his ideas could be used to produce a proof. But he hasn't written up what he has in a way that others can understand, unless they are willing to do a huge amount of work to decipher what he is trying to say. There are already various reasons for people to be skeptical about whether he has a proof, and the combination of this with the fact that his manuscript is nearly impossible to follow is what explains the situation here, not any prejudice about Purdue or anything else.

16. **Tony Smith**
May 23, 2005

When I compare the Riemann Hypothesis efforts of de Branges with the Fermat efforts of Wiles, a question arises, and I wonder whether it is dealt with in the book by Rockmore.

According to a PBS interview at http://www.pbs.org/wgbh/nova/proof/wiles.html Wiles said: "… at the end of the summer of 1986 … this friend told me that Ken Ribet had proved a link between Taniyama-Shimura and Fermat's Last Theorem. … I knew that … to prove Fermat's Last Theorem all I had to do was to prove the Taniyama-Shimura conjecture. It meant that my childhood dream was now a respectable thing to work on. … in late May [1993] … Then I told ….[my wife]… I'd solved Fermat's Last Theorem. … I'd missed … an error … completely …".
The Nova interviewer said: "… Eventually, after a year of work, and after inviting the Cambridge mathematician Richard Taylor to work with you on the error, you managed to repair the proof. …".

In a June 10, 2004, blog post at http://www.math.columbia.edu/~woit/blog/archives/2004_06.html Peter said: "… Louis de Branges is a mathematician at Purdue who has had a long history of claiming proofs of the Riemann hypothesis. … de Branges has a checkered history as a mathematician, with several of his claimed proofs of the Riemann hypothesis and other problems turning out to be incorrect. On the other hand, he did produce a correct proof of one well-known problem, the Bieberbach Conjecture. In that case … after he explained his ideas to a group of Russian mathematicians … it became clear that de Branges really did have a proof. It looks like this one may also take some major effort to see what he really has. …".
In a 20 August 2003 review of Sabbagh's book on the Riemann Hypothesis at
http://www.maa.org/reviews/sabbaghRH.html
S. W. Graham says: "… The conventional wisdom is that de Branges' approach will not work …".

My question is:

Why was Wiles's erroneous proof treated sympathetically by the math community, leading it to work as a team to correct the proof, while de Branges's erroneous proof is treated by the math community with hostile skepticism ?

Is it because Wiles was hanging his hat on the fashionable Taniyama-Shimura conjecture ? If so, then another question is to what extent fashion (and social construction a la superstring theory) is a driving force in the mathematics community.

Is it because Wiles was at Cambridge while de Branges is at less-prestigious Purdue ?

Is it because the mathematics community somewhat resents de Branges because his Bierberbach work showed, with the help of USSR mathematicians, that skepticism of the Western mathematics community is fallible ? Since the USSR is gone now, is there really any somewhat independent (of the West) mathematics community that might work on approaches that are ignored by the Western mathematics community ?

In other words, why does the Western mathematics community not undertake a "… major effort to see what he [de Branges] really has …". ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

17.
May 22, 2005

"..what might be the right sort of cohomology theory"

topos theoretic descent theory

Running Scared

May 23, 2005
Categories: Uncategorized

Last Wednesday night, a paper appeared on the arXiv that spelled very bad news for the whole "Landscape" scenario of how to get physics out of string theory. This paper produced what appears to be an infinite number of possible vacuum states for string theory, ruining hopes for getting predictions out of the Landscape by doing a statistical analysis of vacuum states.

Tonight a new paper by a prominent Landscapeologist (Michael Dine) has appeared. The abstract gives no hint of trouble, claiming evidence of "distinctive predictions for the structure of soft breakings", but the beginning and the end of the paper tell a different story. The second paragraph of the paper admits that the infinite number of states destroys this research program, but deals with this by saying that the author will just ignore the problem for now:

"If this (infinite number of states) is true, many of the ideas discussed in this paper will have to be reconsidered…. the discussion of this paper will be predicated on the assumption that the number of relevant states in the landscape is finite and naive statistical ideas can be applied."

In the paper's conclusion, Dine states:

"There are many ways, as we have indicated, in which the ideas described here might fail. Perhaps the most dramatic is that the landscape may not exist, or alternatively that there might exist infinite numbers of states, whose existence might require signficant rethinking of our basic understanding of string theory and what it might have to do with nature."

I'm looking forward to Dine and others finally getting around to "rethinking what string theory might have to do with nature". It's about time.

Comments

1. **JC**
   May 30, 2005

   Alejandro,

   I don't know what would be a precise definition of "completeness", with respect to how our brains think.

   One way I've thought of it is how marketing and advertising folks are experts at manipulating people into thinking that they are "incomplete", and that the easiest way to feel "complete" is to buy their products.

2. **Alejandro**
   May 30, 2005

   Beyond the animal fear mechanism -which, btw, happens to be the title of this blog entry-, linguists have pointed out that the repression of some grammatically correct but unnatural phrases could be in the origin of religion. So for instance "you rain", "I died". In western culture, especially the later. This remark is interesting because it asks for some extra steps of sophistication in logic. Deictics must have evolved to be able to point to the speaker, and verbal tenses need be no trivial. Thus, when logic/language evolves to be sophisticated, the pretension of completeness creates a series of conflicts that are usually solved by religion. I do not know if this inability to cope with logical conflicts is a hallmark of modern theorists or just a naive comparison…

   Ah, Tony, here there is a taoist quote for our collection (-:

   "hence they hold that '*what is not*' is no less real than '*what is*'" [Arist. Metaphys. A 4 985b4]

3. **JC**
   May 29, 2005

   Alejandro,

   It's very well understood that for most people, uncertainty produces more anxiety in their minds than in scenarios of certainty. If for no other reason, ideological/fanatical type thinking brings about a sense of certainty to someone's mind (albeit, perhaps a false sense of security). More "certainty" seems to give many people a "peace of mind", even if only at a psychological level. If nothing else, it seems to be largely a psychological coping mechanism to flee away from "danger" in the form of "predators" and/or "barbarians".

   People who are major "control freaks" seem to fall into the pattern of only feeling "secure" in themselves if they have total certainty and control over the things in their lives which they have direct control or access over. Control freaks seem to go ballistic when their sense of self certainty and/or control becomes more uncertain.

   In the case of religion, perhaps the sense of a "god" looking over us and being the ultimate explanation of almost everything in the world, is what gives followers and believers a higher "certainty" outlook in life. Explanations of things happening by random chance and/or "shit happens" at random, doesn't quite have the same "warm fuzzies" feeling as explanations like "god's will".

   I'm not really sure how this would apply to something like string theory, unified field theories, etc … other than perhaps these "theories" produce a false sense of "theoretical" certainty in the minds of the people working on them. (Whether their theories are right in the end, is a completely different question altogether).

4. **Alejandro**
   May 29, 2005

   Yes Tony, this blur betwean creator and created is a hallmark of the approach to religion of a lot of scientists, and probably the only way for them to keep a religion while being productive in science. And this approach is already heretic for most established religions. But I believe that the acting scientist climbs a new step when s/he becomes aware that the scientific study of the description of the world has wiped away any fear of future or death; in such state religion does not have any role to fulfil anymore, and it becomes unnecessary.

   (Incidentally, and as a less ambitious example, I have always though that a reason for physicists to be good at Banking and Finance is that they do not have the same faith in money that economists have.)

Going back to topic, I wonder this view, of Faith as an analgesic against Fear, could be applied to analize String Theory from the positions claimed by Juan R.and JC. We should inquire which Fear are theorists trying to exorcise.

5. **Tony Smith**
May 28, 2005

Alejandro said "… that the continuous application of science and reason drives one towards Epicureim … while Western pre-chemists declared themselves as disciples of Leucipo and Democritus (thus Epicurus?), Eastern pre-chemists declared themselves as disciples of Lao-Tze, and then users of the Taoist inspiration. …".

I tend to agree with Prof. Cooper's course outline for Philosophy 335 on the web at http://www.princeton.edu/~johncoop/Phil335/Phil335.html where it says: "… the two main post-Aristotelian or (as they are more usually called) Hellenistic schools of philosophy, [are] the Epicurean and the Stoic. …", and I tend to agree with Stoicism as described at http://www.geocities.com/Athens/Delphi/8309/philosophical.html where it says: "… What is referred to in Stoic writings as "Zeus" in one place, may be referred to as "Nature" elsewhere. … there is still little distinction between creator and created, or between physical and spiritual. The Stoic worldview is thus closer to that of Daoism, Vedanta or some varieties of Sufism than to orthodox Christianity or Islam. …".

It seems to me that the "reading Nature book" approach has worked well in some instances, such as Einstein's development of special and general relativity, and that such an approach need not necessarily lead to Epicurus, but might as well lead one to Stoicism/Daoism, or also to the Hegelian Dialectical Materialism of the Nagoya school in Japan that motivated Kobayashi and Maskawa in their formulation of a 3-generation model that explains, among other things, CP violation.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

6. **Alejandro**
May 28, 2005

Well, probably the Spinozian/Eistenian view is heretic for most of the established religions. G-d aware scientists have usually tried the "reading Nature book" approach in order to justify their research in the inner works of Nature, but it has not worked very well. It seems that the continuous application of science and reason drives one towards Epicureim, in the sense preached by Lucretius.

Which is not bad, because if the goal of religion is to calm soul anguish and fears, it can be told that acting by Science and Reason also do a good work.

About symbolism, it is interesting that while Western pre-chemists declared themselves as disciples of Leucipo and Democritus (thus Epicurus?), Eastern pre-chemists declared themselves as disciples of Lao-Tze, and then users of the Taoist inspiration.

7. **Tony Smith**
May 28, 2005

If Alejandro is correct in saying: "… Probably the use of "religious" by JC there was implied to mean "based on faith, irrational, inaccessible to human reason"….", then JC's position would be in direct opposition to that of Einstein, to whom "religious" means "… at least to a certain extent, accessible to human reason. …".

Personally, I prefer Einstein's definition, but it may be that many people prefer the definition attributed to JC by Alejandro.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

8. **Alejandro**
May 28, 2005

Probably the use of "religious" by JC there was implied to mean "based on faith, irrational, inaccessible to human reason".

9. **Tony Smith**
May 28, 2005

JC referred to "… the mindset of people working on "unified field theories", resembling religion …" in the derogatory context of "… "physics envy" … "theory" in the social sciences …".
I think that such derogation of "mindset … resembling religion" in working on "unified field theories" is unfortunate and misguided.
For instance, it is well known that Einstein, who worked on "unified field theories", said: "I believe in Spinoza's God who reveals Himself in the orderly harmony of what exists … I have not found a better expression than 'religious' for the trust in the rational nature of reality that is, at least to a certain extent, accessible to human reason. …". Even though Einstein did not succeed in his search for a "unified field theory", he did accomplish some useful things, including general relativity, along the way.

Some other attempts at constructing "unified field theories", such as GUTs, supergravity, and superstring theory may have also failed in that they have not produced a model with the content of gravity plus the standard model, but they have at least been useful as no-go theorems, and the people working on such things (Glashow, Hawking, Witten, et al) vary widely in the extent to which they openly discuss "religion" in the context of their "mindset" with respect to their approaches to physics.

Perhaps it might be useful to distinguish between two different types of "mindset … resembling religion" with respect to physics:
an Einstein-type belief in the existence of a Platonic ideal orderly harmony
and
a cult-like belief in a particular approach that is embraced by a particular powerful bureaucratic social/political instituion, such as the Roman Catholic approach to astronomy hundreds of years ago that led to the burning of Giordano Bruno, and such as the current superstring bureaucracy.

In my opinion, the former (Einstein) view is good constructive motivation, while the latter view is obstructive to progress in physics.

In order to make clear my personal biases with respect to such things, I should say that my personal work, a summary of which is available on the web as a 15 Mb (about 300 pages) pdf web book at http://www.valdostamuseum.org/hamsmith/philophysicsbook/PhiloPhysics.pdf), is an attempt to unify gravity with the standard model and to connect that unified model with things "resembling religion" that have motivated me, including but not limited to the writings of Ibn Arabi, divination systems of IFA, Shinto, and I Ching, Tarot, and the Rig Veda.

Although I think that it would be quite fair to criticize my physics model with respect to objective criteria such as whether or not its calculated results are consistent with experiment and observation, and even with respect to subjective criteria such as whether or not its mathematical structure is perceived by a critic to be elegant or ugly,
I do not think that it would be fair to criticize it (or me) because it is to a large degree motivated by my perception of such things "resembling religion".

In other words, I am opposed to derogation of work on "unified field theories" that is motivated by a "mindset … resembling religion".

Tony Smith

http://www.valdostamuseum.org/hamsmith/

10. **JC**
    May 28, 2005

    Most “theory” in the social sciences such as economics, finance, etc … isn’t much more than the equivalent of “physics envy”. In some ways it is similar to the mindset of people working on “unified field theories”, resembling religion more than anything else.

    A hardcore Machiavellian type of person will use all kinds of “theory” to justify all kinds of things for their own “political” gain, when they know very well that the “theory” is largely BS for the most part. As far as they’re concerned, the “theory” provides a good “cover story” to justify their schemes, while “hiding” their ulterior movtives.

11. **D R Lunsford**
    May 28, 2005

    They wrench it out of its designed functional zone for the greater good of profit.

    -drl

12. **Jim**
    May 27, 2005

    “…although I’m the first to know what management does with code and coders”

    Ok I’m curious as to what they do with the code?

13. **pfedor**
    May 27, 2005

    I thought financial mathematics was only differential equations.

    I have recently stumbled onto a book called “Quantum Finance : Path Integrals and Hamiltonians for Options and Interest Rates”. Haven’t read it, though. Maybe it’s some kind of joke.

    With best regards,

    Aleksander

14. **ksh95**
    May 27, 2005

    Juan R. said
    Hum!! you sound a bit radical

    Hum, maybe I should stick to physics and leave the satire to the experts. I’m obviously not that talented.

15. **Eli Rabett**
    May 27, 2005

    Quantoken might be interested in Benjamin Franklin’s bequest http://www.mathsci.appstate.edu/~sjg/class/1010/wc/finance/franklin1.html

16. **D R Lunsford**
    May 27, 2005

    JC – there are a lot of smart people (math and physics PhDs who didn’t get academic jobs by choice or fate) on Wall St. and they aren’t all doing BS work. The idea behind derivatives is fine, although I’m the first to know what management does with code and coders.

    -drl

17. **Juan R.**
    May 27, 2005

    Well ksh95

    i disagree a bit with you.

    I think that the dead of SSC is not the cause of scandal of string theory in mass media. For example, LQG also suffer that many of their predictions cannot be tested (e.g. departures from energy momentum Einstein relationship to high energies) still i think that they are been rather honest when compared with the average of string theorists.

    I still think that a large part of humanity will feel distrut regarding science due to scandal of string theory, already known in specialized circles but still unknown for great public. Public still think that says Brian Greene Elegant universe is true and string theory is computing all.

    And Elegant universe was a best-seller, not a book for two or three fans of science.

    “When that happens the communists will take over and we’ll all be speaking Chinese.”

    Hum!! you sound a bit radical

18. **quantoken**
    May 27, 2005

    JC said:
    “If somebody can predict with 100% certainty how the future is exactly going to unfold, they can easily become a zillionaire with zero risk!”

    I know a way of getting rich with 100% certainty, and with zero risk. Just deposite $1 in your bank and never withdraw it for the first 500 years. You won’t get rich yourself but your offsprings will. At just 5% annual interest rate, you could easily beat Bill Gates when one of your offsprings withdraw the money 500 years later.

Seriously there are surely many ways one can get rich if that's all you want. Most people simply can not do it. Most people can not do simple things like save some money in the bank and keep their books balanced. Hey not even the president of this country can do that. What can you say?

Quantoken

19. **JC**
May 26, 2005

DR Lunsford,

Most financial "prediction" methods aren't much more than the equivalent of "snake oil", or in some cases outright fraud. If somebody can predict with 100% certainty how the future is exactly going to unfold, they can easily become a zillionaire with zero risk! A good test as to whether an economic theory and/or "prediction" is the real deal, is to see whether the author/promoter of it is richer than Bill Gates or Warren Buffet.

20. **D R Lunsford**
May 26, 2005

I thought everything was differential equations.

BTW it's likely that these financial industry predictive techniques will find their way into camcorder stabilization and so on.

21. **JC**
May 26, 2005

ksh95 said,

"BTW. I thought financial mathematics was only differential equations."

You'll probably get a different answer depending on who you ask.

In the case of Enron, former CEO Jeff Skilling jokingly referred to financial math as HFVA "Hypothetical Future Valuation Accounting", which "can add a kazillion dollars to the bottom line."

In principle, the "textbook" derivative securities valuation stuff is largely stochastic differential equations. This should already make things suspicious considering economics Nobel Laureates Myron Scholes and Robert Merton won the economics Nobel Prize in 1997 for the Black-Scholes equation stuff. A year later in 1998, the hedge fund LTCM (Long Term Capital Management) which Scholes and Merton were involved with, collapsed during the Russian ruble default. The Federal Reserve Bank of New York ended up strongarming LTCM's creditors in "bailing out" LTCM and dismissing the management, in order to prevent the credit markets from collapsing if LTCM ended up having to liquidate their portfolio in a "panic selling" manner in order to satisfy margin calls. (The Federal Reserve Bank seems to be very concerned about the credit markets collapsing, while not really paying as much attention to the stock markets).

The Black-Scholes stuff was also behind the "portfolio insurance" strategy which was popular in the 1980's. Some folks blamed the portfolio insurance stuff for really accelerating the downward crash of the stock market on Oct 19, 1987.

For the rest of practical everyday finance, a lot of it doesn't use much calculus for the most part. In principle things can be done without much more than a calculator or excel spreadsheet.

A lot finance seems to resemble religion a lot more than science, especially when it comes to the stock market and other investments.

22. **ksh95**
May 26, 2005

No one is interested in your facts Tony, they just cloud the waters. I think my argument is a perfect example US congressional logic.

Scientists usually predict or postdict experiments. Without experiments theorists are left without guidance and inevitably go off the rails. When theory goes of track the foundations of innovation are shaken. The pillars of capitalism are at stake. At this point the communists take over, terrorism runs rampant, and oil prices sour.

See? It all makes perfect sense.

If we could somehow relate the SSC to gay marriage and social security we may even secure the Super Duper Superconducting Collider (SDSC).

BTW. I thought financial mathematics was only differential equations.

23.
May 26, 2005

Tony Smith wrote: "[…] it is easier for the USA government to see through bullshit supporting a bad experimental project than it is for them to see through bullshit supporting a theoretical program […]"

More likely, the cost of supporting a theoretical program is negligible compared to the cost of building a superconducting supercollider. With a budget deficit in the billions, cutting a few millions on academic wages (such as they are) + pencils and paper won't get you very far. Cutting the largest hardware project ever, now that's something else.

24. **Tony Smith**
May 26, 2005

ksh95 described "… string theory as an object lesson describing what happens when congress kills large projects (the SSC comes to mind) …".

The relevant chronology is:
1984 – Weinberg and other influential physicists announced their support for Schwartz's Superstring theory;
1987 – Reagan announced plans for SSC and Green, Schwartz, and Witten published their 2-volume Superstring book;
1993 – SSC killed early in Clinton Administration
1994 – second superstring revolution (based on dualities etc) began

2005 – superstring theory is still dominant, devouring about 90% of theoretical elementary particle physics funding.

I fail to see how string theory is a consequence of the death of the SSC. It seems to me that the glory days of string theory, during which it became the dominant approach to theoretical physics, coincided with the glory days of the SSC rather than the death of the SSC.
Maybe ksh95 is referring to the fact that the second superstring revolution occurred shortly after the death of the SSC, but it seems to me that to the extent there may be a connection there, it may be that the superstring folks decided that they needed a second revolution to avoid the fate of the SSC, and that they hyped the dualities as the needed revolution.
Perhaps the fact that the SSC was killed off in 1993 while string theory continued to prosper financially is an indication that it is easier for the USA government to see through bullshit supporting a bad experimental project than it is for them to see through bullshit supporting a theoretical program involving mathematics that is even more sophisticated than the math used for financial derivatives etc.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

25. **ksh95**
May 26, 2005

Juan R. Said:

"…String theory has caused more damage to the public image of science that the last 100 scandals…"

The public image of science looks something like; a bunch of nerdy guys in thick glasses and white lab coats running around a lab full of beakers. The overwhelming majority of the public is blissfully unaware of the very existance of string theory…The image of science will be just fine.

If we're smart we'll use string theory as an object lesson describing what happens when congress kills large projects (the SSC comes to mind).

I would recommend pg. 429 from the Carl Rove playbook.

The sky is falling. China and India are passing us in science and we will lose our technological advantage. When that happens the communists will take over and we'll all be speaking Chinese. The only way to preserve our way of life is a new accelerator. It should be the greatest accelerator the world has ever seen because Americans are the greatest people in the world.

We should end with chants of USA…USA…

26. **Juan R.**
May 26, 2005

As said

The only correct ST equation

String theory = Faith

The Faith move mountains, therefore String theory move also mountains. In one or another form it remained with us in several centuries. Always someone in some place claimed that the theory is correct and the he/she has solved his current flaws.

The idea of string theory is correct is so scientific like the idea of that a new planet near Sun is the cause of anomalous orbit of Mercury. It was a hypothesis (beatiful?) but only that.

String theory was a hyphotesis, initially beatiful i believe, but with each new revolution the stuff was more and more uggly, inefective, and unelegant.

String theory has caused more damage to the public image of science that the last 100 scandals (e.g. Pons cold fusion, quantum telekinesy by Uri Geller, etc.) joined.

27. **D R Lunsford**
May 25, 2005

TL – very nice. But, GR is not strictly local (g_mn is in a sense reducible).

28. **Chris W.**
May 25, 2005

The industry purrs merrily along; see hep-th/0505232. (LS is among the co-authors.)

29. **Stephen Paul King**
May 25, 2005

String Theory and its M-theory progeny will continue on to attract the interest of pure mathematics; just as it should have from discovery of supernumbers.

30. **Alejandro**
May 25, 2005

For as long as there is money, there will continue to be super string theory and super string theoretists. Just as astrology has been in exist

Hmm the main use of astrology chit-chat does not involve money: it is about flirting.

31. **Thomas Larsson**
May 25, 2005

*Tell us what that Something Else *is*, and we'll gladly listen.*

hep-th/0504020. So now you are gladly listening? Didn?t think so.

32. **Kyle**
May 25, 2005

Hobbyists?? I bet that isn't what they tell the people funding them. I bet people like Kachru might even claim they do this kind of thing for a living. If they don't, it's more a time for a change in personel then ever.

As an aside, while in Alaska recently I talked quite a bit with many different people who made a living off fishing, and got to see some of how they worked. In any other circumstance I would consider it a shame to spend so much time on an analogy. Here, however, it seems quite appropriate, given how the mistake you are making in the analogy and the topic seem to be in the same vein.

33. **Barry O'Genesis**
May 25, 2005

If you are a hobby fisher, then you fish to see if there are fish. If you are professional, you have ways of determining whether there are fish before you fish, and you don't sit around a fishing hole that doesn't produce.

I see that you have never talked to a professional fisherman. The ones I know go out every night to try to catch fish, no matter what anyone tells them about the failings of the fisheries industry.

So are these hobby theorists we're discussing, or professionals?

Since 1998, we are all hobbyists . Jeez, guys, we are living at a time when we have one of the most sensational observations of all time, the acceleration of the universe, in front of us, and nobody has a clue as to why it is happening. Do you really think that "People should be trying Something Else" is an adequate response? Tell us what that Something Else *is*, and we'll gladly listen. Meanwhile I suggest that we all go off and try to write some papers. A paper showing how string theory constrains possible cosmological models would be good, even *if* string theory *is* a failure as a unified theory. It's still preferable to "Something Else" theory!

34. **Thomas Larsson**
May 25, 2005

Incidentally, Thomas Larsson, are you a physicist?

At least I once was. I ran out of funding after completing a four-year postdoc a decade ago. But then again, I didn't try very hard to hang around, since at that time I felt that starting a family and having a permanent income was a much higher priority than staying in academia. You can judge for yourself whether I'm still a physicist by looking at hep-th and math-ph.

Incidentally, are *you* a physicist? An anonymous poster doesn't have much credibility.

35.
May 25, 2005

Following the work of Georgi and Glashow on SU(5), there were lots of works in the early 80's on GUTs that became more and more complicated, lost any hope of being experimentally testable. Serious people like Callan, Coleman, Gross and Witten, never got into that.

There was also a lot of work at some point on the so-called technical fine tuning problem. Lots of papers were generated, but no serious person ever worked on it.

I'm afraid, you are right, I don't think Kachru, Arkani-Hamed, Dimopoulos and others that you mention, are of the same calibre as Gross and Witten. I also don't think that any of them would think of himself as being in the same calibre.

The only man in the landscape business that I take seriously as a very talented theoretical physicist is Michael Douglas. His work, at least from the late 80's on low dimensional strings and integrable models is great.

I know that I'm voicing strong opinions, but that's how I feel.

Incidentally, Thomas Larsson, are you a physicist?

36.
May 24, 2005

Actually, Q brings an interesting point. Phenomenology existed in particle physics long before strings. It meant fitting experimental data by simple theoretically-motivated formulas.
The highest point of particle phenomenology was the discovery of Bjorken scaling that led to the present formulation of QCD and last year's Nobel prizes.
The definition of superstring phenomenology is essentially "non-testable hypotheses, inspired by superstring theory, made by physicists who would not be able to compute the Veneziano amplitude".
Jean-Paul

37. **JC**
May 24, 2005

If this string landscape stuff dies a painful death, what is there left to do in string theory besides banging one's head harder and harder on KKLT type stuff or just plain giving up?

38. **Quantoken**
May 24, 2005

Aaron said:
"This is completely absurd. There are more jobs in **phenomenology** than in strings right now. Plenty of string theorists are moving in that direction (and towards astrophysics) to better position themselves for the upcoming job market."

What is absurd is the invention of the English word **phenomenology**, and the creation of a whole profession out of that hype? What exactly is a phenomenologist?

In all other branch of scientif research, you are either a theoretical researcher, or an experimental researcher. Either theory or experiment, or both. There is no third kind. There is no phenomenological biology. No phenomenological chemistry. No solid state phenomenologist, and no phenomenological mathematician.

Why should fundamental physics theory research be an exception and create the weirdness called **phenomenology**. If you start that route, you could also invent super string hypertheologist, or super string speculatologist, or landscapologist, or a bunch of other names. It's all vacuous!

I think that the very fact that they needed to invent a middle ground called phenomenology, reflect the truth that **they really can not make any prediction or connect**

their theory in any way with the reality. Otherwise, like in all other fields, theoretists do the calculation on their theory and make verifiable predictions, and experimenters simply do experiments and verify the prediction, and there is no need for any middle man called phenomenologist.

It remains that even phenomenologists are so far unable to provide any predictions so far. Time to create yet one another middle ground field between experiment and phenomenology, to bridge the wide gap between theoretical fantacy and reality, I guess? 🙂

Quantoken

39. **Aaron**
May 24, 2005

OTOH if you are a young theoretical HEP researcher the choice now is virtually to either work on string theory or get out altogether.

This is completely absurd. There are *more* jobs in phenomenology than in strings right now. Plenty of string theorists are moving in that direction (and towards astrophysics) to better position themselves for the upcoming job market. By my quick count on the rumor mill, there are 5 new string people and 15 or 16 new phenomenologists or astro-types. As we get closer and closer to the LHC turning on, most people expect this ration to tilt even more towards phenomenolgy.

I'm really not sure where this skewed view of the field comes from, but it certainly has little bearing on reality.

40.
May 24, 2005

"Any bets on what people will be talking about at the Strings 2006 conference?"

Shoe-laces?

41. **Chris W.**
May 24, 2005

Not that it means anything, but the chronology of Enron's rise is rather similar to that of string theory:

> Enron was formed in **1985** by the merger of Houston Natural Gas and InterNorth, engineered by HNG CEO Kenneth Lay. It was originally involved in the transmission and distribution of electricity and gas throughout the United States and the development, construction, and operation of power plants, pipelines, and other infrastructure worldwide.
>
> Enron grew wealthy through its pioneering marketing and promotion of power and communications bandwidth commodities and related risk management derivatives as tradable securities, including exotic items such as weather derivatives.
>
> As a result, Enron was named "America's Most Innovative Company" by Fortune magazine for five consecutive years, from 1996 to 2000. It was on Fortune's "100 Best Companies to Work for in America" list in 2000, and was legendary even among the elite workers of the financial world for the opulence of its offices.

42. **Kyle**
May 24, 2005

If you are a hobby fisher, then you fish to see if there are fish. If you are professional, you have ways of determining whether there are fish before you fish, and you don't sit around a fishing hole that doesn't produce.

So are these hobby theorists we're discussing, or professionals?

43. **Peter Woit**
May 24, 2005

Sorry, but the whole attempt to claim that cosmology will solve the problems of string theory just looks to me like another attempt to evade the undeniable fact that string theory unification has failed. It's only "common sense" that cosmology must be the answer if you start from the idea the string theory must be correct. The Hartle-Hawking stuff of Tye and others shows not the slightest sign at all of leading to anything that looks like the standard model.

If a small number of people want to base their research program on an extremely unpromising speculative idea, that wouldn't be objectionable, but this is being used to support a failed enterprise that dominates particle theory and keeps people from being able to work on other things. When work on a speculative idea is driven by sound scientific considerations, you're right that even if the idea doesn't work out, it may lead to something that does. When it is being driven by sociological reasons, it is very unlikely to lead anywhere interesting.

44. **Barry O'Genesis**
May 24, 2005

"You can put your faith in cosmology or some unknown non-perturbative effects, but this is purely wishful thinking and there isn't a shred of evidence for either hope."

Sure it's wishful thinking. That is what always happens when you embark on a research project and you can't see how it's going to pan out: you hope that something will work, it turns out that it fails in some interesting way, and that leads you in another direction, etc etc etc. This latest Kachru et al paper just shows that the uniform distribution cannot be the right one, something that many people, most loudly Lubos Motl, have suspected for a long time. That just means that we have to work on some kind of Hartle-Hawking gadget to tell us how points in the landscape are selected. Henry Tye and co have done some beautiful work on this, see
http://arxiv.org/abs/hep-th/0505104
Of course one works on such a thing hoping that it will lead somewhere — what's wrong with that? And the idea that cosmology will be relevant to this problem is not "faith", it's just common sense.
You can argue that a lot of people who should know better are working on silly stuff like [insert recent technical work of several famous people *here*] when they should be working on cosmology, and I would agree with that. But to say that there's no evidence that studying cosmology will get us anywhere is like saying that nobody should go fishing unless they have evidence that sufficient numbers of fish exist in the pond. You settle that question by proceeding to fish, not by waiting for somebody else to furnish "evidence".
Bottom line: string theory doesn't work without cosmology, and people are belatedly realizing that. Not very apocalyptic really.

45. **Quantoken**
May 24, 2005

For as long as there is money, there will continue to be super string theory and super string theoretists. Just as astrology has been in exist for thousands of years and will continue to be for some more thousands of years, although it's been long proven none-predictive. There continue to be a market for astrology and some people continue to believe they can predict something in astrology. It's all driven by money and people's fear of the unknowns.

Do you believe those super string researchers have actually been honest to themselves, and actually believed they wer doing scientific research, and never realizes they are researching crackpot theories? I actually don't think they have been honest to themselves in rejecting the notion that string theory is none-predictive.

It's all about money and survival within the circle.

Quantoken

46. **Peter Woit**
May 24, 2005

Meanwhile, back in the real world, there are plenty of string theorists who don't work on the landscape. And in fact don't believe in any of the statistical stuff.

Sure, but, as landscape advocates will point out, you still have to deal with the fact the the theory appears to have an infinite number of vacuum states, and is probably utterly non-predictive. You can put your faith in cosmology or some unknown non-perturbative effects, but this is purely wishful thinking and there isn't a shred of evidence for either hope.

47. **Peter Woit**
May 24, 2005

Any bets on what people will be talking about at the Strings 2006 conference?

I don't know about 2006, but I'm betting a lot of talks at Strings 2005 will start off (OK, after the initial "string theory is the most promising idea for unification") like Dine's, saying something like "We're going to ignore the problem of an infinite number of vacuum states, because that makes everything we're going to talk about useless".

On the Enron analogy: That's great, I've been fascinated by the Enron story, read the recent book and did see the film down at Lincoln Plaza. But I hadn't made the connection to string theory. I have been trying to figure out for years how to sell string theory short. One thing I've done is to try and get string theorists to put their money where their mouth is and enter into a bet with me. For some reason, none of them have taken me up on it.

I've witnessed such phases in theoretical high energy physics before.

I've never seen anything quite like this one, with people from the top institutions in the field embracing outright pseudo-science. And even those leaders of the field who aren't engaging in pseudo-science haven't been willing to step up and criticize what is going on (the only exception I can think of is David Gross).

48. **Thomas Larsson**
May 24, 2005

I've witnessed such phases in theoretical high energy physics before.

Are you old enough to have witnessed the demise of aether theory? Otherwise, exactly which phases are you referring to?

One of their main characteristics is that respectable people don't take part in them at all. Unfortunately, this time, one really good scientist is taking part in this, namely M Douglas, however, everyone that I have regard for is totally silent.

So you have no regard for Susskind, Dimopoulous, Arkani-Hamed, or Kachru?

Btw, Witten has not been totally silent. He has repeatedly been saying that he hopes that current discussion of the string landscape isn't on the right track, but has no convincing counter-arguments. Cf e.g. Peter's post about the recent New Scientist article from April 28:

Witten is referred to as a "string grandee", and quoted as saying about string theory "More work has always given more possibilities – far more than anyone wanted… I hope that current discussion of the string landscape isn't on the right track, but I have no convincing counter-arguments."

49.
May 24, 2005

I've witnessed such phases in theoretical high energy physics before. One of their main characteristics is that respectable people don't take part in them at all. Unfortunately, this time, one really good scientist is taking part in this, namely M Douglas, however, everyone that I have regard for is totally silent.

50. **Juan R.**
May 24, 2005

The only correct ST equation

String theory = Faith

51.
May 24, 2005

"No – that is the territory of the has-beens, crackpots and cranks who post here.
"
What–here? Well I never.

Meanwhile, back in the real world, there are plenty of string theorists who don't work on the landscape. And in fact don't believe in any of the statistical stuff.

52. **Chris Oakley**
May 24, 2005

Stringlover is neglecting to mention one important thing: if you work for Enron and don't like the corporate culture you can always go and work for someone else, doing the same job in a (hopefully) less annoying environment. OTOH if you are a young theoretical HEP researcher the choice now is virtually to either work on string theory or get out altogether. Enron also had the bullshit-defeating requirement to make money, a requirement that few are going to have difficulties understanding. But if the "top" HEP theorists are going round saying that the Landscape is the best thing since sliced bread, few among the general public are going to be able to prove them wrong. No – that is the territory of the has-beens, crackpots and cranks who post here.

53. **Stringlover**
May 24, 2005

Everybody following the progress of string theory must see the excellent documentary "Enron: The Smartest Guys in the Room". The title only gives a hint that it really tells the story of string theory, but the parallels between the Enron phenomenon and string theory are so remarkable that we can only expect string theory to share Enron's fate. It's on at Lincoln Plaza, which is within walking distance of Columbia if you like walking.

Anyway, it all starts off with a great accounting idea called "Mark to Market", in which you assume that the great idea that you have will work just fine, and write down in your accounting books that you already have the profit you expect to make. Great! Record profits straight away – the investors love it.

Step two: Lots of talking directly to the public; press releases, reviews in magazines saying that your company (or theory) is fabulously successful.

Step three: Create a macho atmosphere in which everybody involved in the company (theory) constantly talks about how smart they are. The public love it; everybody agrees that these guys are the most intelligent geniuses ever to walk the Earth. Suggesting that this might not be the case is immoral.

Step four: Make the leaders of the whole project objects of hero worship – they're the smartest guys in the room. Einstein had nothing on them.

Step five: Realise that perhaps some of things that you had earlier announced were sure to work out are not working as well as you had planned. Call another press conference and tell them that everything is working even better than you had hoped. Privately realise that you've staked too much on this to back out now.

Step six: Be aggressive and insulting to people who doubt your claims that your project is the greatest success in history. There was an excellent scene where Jeff Skilling gets asked "How does your company make its profits?", and responds with "If you'd done your homework you'd know how stupid that question is." To another guy who asks "Why is it that every company apart from yours can produce a balance sheet, but with you we just have to trust you?", Jeff calls him an asshole and hangs up.

Step seven: Finally acknowledge that the project is unsustainable. Declare bankruptcy and go to jail.

Well, string theory is up as far as step six, from what I can see. From the string theorists that I know who've moved to Wall Street, I can say that string theorists seem to fit in very well in an Enron-style environment, and have just the right mixture of technical ability, the inclination to soak up ideology and follow glorious leaders to the promised land, and macho insistence that since they're so extraordinarily clever, they could never be wrong, that the future looks quite grim for them.

54. **JC**
May 23, 2005

Any bets on what people will be talking about at the Strings 2006 conference?

News From SLAC and Elsewhere

May 26, 2005
Categories: Uncategorized

Earlier this week Jonathan Dorfan, the director of SLAC, announced a reorganization of the structure of the laboratory. The new structure involves four divisions, two scientific and two operational. One of the scientific divisions will bring together particle physics and astrophysics. It will be led by Persis Drell who also will be a deputy director of the laboratory, a position previously held by her father, particle theorist Sidney Drell. The other scientific division will be called "Photon Science", which will make use of the SLAC x-ray sources. At the moment SLAC produces intense x-ray beams at the SSRL, using synchrotron radiation from a ring which is a descendent of the original SPEAR electron-positron ring that was crucial in the "November Revolution" of 1974 (and which also provided me with a job one summer).

The main SLAC linac is being turned into a free electron X-ray laser to be called the Linac Coherent Light Source (LCLS), which will be operational in 2009. At that time the plan is for SLAC to be out of the accelerator based high-energy physics business, with the PEP-II collider also shut down. The last fixed target experiment using the linac, E158, recently reported the most accurate measurement of the weak mixing angle at relatively low energies (at LEP it was very accurately measured at the Z pole). This measurement shows the running of the ratio of coupling constants predicted by the renormalization group. For more about this experiment, see an article in the latest Nature magazine.

This week's Science magazine also has an article about particle physics. It reports on the HEPAP meeting mentioned here earlier where a plan to evaluate whether to shut down PEP-II or the Tevatron early was put forward. On a more positive note, the House Appropriations committee has restored some of the cuts in the FY 2006 DOE budget proposed by the White House. The House committe added $22 million to the high energy physics budget, bringing it back to the FY 2005 level (which, accounting for inflation, would still be a cut, but a smaller one).

An article in New Scientist about the same House bill explains that money is being taken away from the ITER international project to build a fusion reactor and used to bring funding for domestic fusion research also back to FY2005 levels. This may have something to do with the fact that the latest news about ITER is that a deal has been reached that will site it in France.

Comments

1. **Michael**
   May 30, 2005

   Quantoken,
   you are just obnoxious. Who talks about He3 from the moon? Deuterium from sea water is what the only sustained controlled fusion ever sparked on earth was using. You seem to think that everone except for yourself is a stupid moron. Some people tend to think the converse.
   Michael

2. **Quantoken**
   May 30, 2005

   Michael saked:

   "getting deuterium out of sea water does not kill the efficiency. Can you imagine that the people who work on fusion science, as well as those who grant billions of $$ funding for it, have checked this first thing?"

   Absolutely not! There is a huge gap between Fundamental research and industry application. Can any of the earlier scientists who were the first one probing the secret of atoms imagine that their research will one day lead to a deadly weapon that could easily wipe out whole cities? No. Did the inventor of pesticides like DDT and 666 realize the scope how his or her invention destroys the environment? Absolutely not.

   How hard or easy it is to extract fusable material from the environment is of absolutely no concern to those doing fusion research, or those grant research funds. So **NO**, they have NOT checked out this "first thing". I would be surprised if you could cite even one paper by thermal fusion research scientists talking about the techniques or cost of mining or extracting He3 from the moon.

   Quantoken

3. **D R Lunsford**
   May 29, 2005

   uh He3 that is 🙂

4. **D R Lunsford**
   May 29, 2005

   http://www.popularmechanics.com/science/space/1283056.html

   Lots of H3 on the moon.

   -drl

5. **Michael**
   May 29, 2005

   Quantoken,

   getting deuterium out of sea water does not kill the efficiency. Can you imagine that the people who work on fusion science, as well as those who grant billions of $$ funding for it, have checked this first thing? Do you really think that your rambling objections qualify as a contribution? I am afraid the answer to the last question is yes.

   Michael

6. **Quantoken**
   May 29, 2005

You still don't get the most important point I pointed out. The important thing is it may actually cost **MORE** energy to extract and refine the fusable elements, than the amount of energy you can obtain by the fusiom reactor. If that is the case, then it is a useless technology, regardless of the monetary cost.

The energy cost is due to the percentage scarcity of the fusable isotopics like deuterium and He 3. You would have to evaporate tons of sea water just to obtain one gram, or even a few miligram of the scarce isotopics you want. Such refinery process costs huge amount of energy. And that cost can not be reduced beyond a theoretical level no matter how sophisticated your technology might become.

If you disregard of the cost efective factor, sure, we have plenty of alternative energy source we can think about. There are plenty of Hydrogen on Jupiter for example. Just ship some Hydrogen from Jupiter to earth and it will can be used on fuel cells etc. But the energy cost to bring Hydrogen from Jupiter to earth is more than you can get out of it, so it is infeasible. Likewise, there are plenty of methane for you ti mine on Uranus. But it is in-practical.

The only feasible, economical, and environmental alternative energy source, is the **heat from the deep of the earth crust**. Just dig a hole a few KM deep, you let water down and high temperature steam comes up, allowing you to turn it into electricity and other energy form. It's actually nuclear energy since the source of heat is the natural decay of elements in the earth crust, like U238 and U235. That's a huge amount of energy, causing tectonic plate movements, volcanoes, earthquakes and tsunamis.

Quantoken

7. **Michael**
May 29, 2005

What is everyone here talking about? No closer to viability than a long time ago? That is simply wrong. Just a few years ago there's been the first self sustained controlled fusion for several minutes, and it put out a lot more energy than had to be put in.

I realize that science sceptics gather here. But why ignore well known facts?

Michael

8. **M**
May 28, 2005

In response to a couple of comments by Q:

"1.Feasibility. We have spent half a century on this without success. It may take another half century before the technology is finally achieved, and much longer before it becomes an economically viable energy source. The energy crisis will hit far before this becomes successful."

I believe one can make a very convincing argument that the relatively slow progress in fusion research is directly attributable to the lack of any feeling of urgency. If a true energy crisis looms, it is almost certain that enormous resources will be poured into whatever alternatives seem best. That would probably include fusion, but other viable alternatives would get serious attention as well. It is not reasonable to assume the same level of funding as today and then extrapolate more than about five years into the future.

" 2.Usefulness. The fuel used in thermal nuclear fusion, Helium 3, is a scarce resource on earth."

Helium 3 may look good in some ways, but deuterium-tritium as a fuel has a longer history of research and would almost certainly be more practical, if for no other reason than the scarcity of helium-3.

Of course, none of this means that fusion research will actually be practical as a large scale power source, but I wouldn't count it out either. SOMETHING has to eventually take the place of fossil fuels…

M

9. **steve**
May 28, 2005

Actually Mike, Quantoken raises a couple of fair points. With all due respect to the people who have worked on fusion and plasmas physics, we are no nearer a commercial or viable fusion reactor design –or even something for which you get more energy out than you put in–and may not even do so in this century. Its a tough, maybe intractable problem that only nature has solved in the sun and the stars. The political problems don't help either. Still, maybe someday it will work but 200 years is too long to wait.

Sometime this century a massive global energy crisis is going to start to kick in and no-one seems to want to face up to it. I remember once being at the top of the World Trade Centre observation deck (in the good old days before it had jet airliners flying into it) taking in the amazing view and thinking about the colossal amount of energy New York was using up. Not to mention the endless stream of cars. And that is just a tiny part of the US and the world. In the future, just where is all that power going to come from? And demand keeps going up and up.

I still think ITER is worth doing though and the cost is not that great when put in perspective. Yes, there is Helium 3 on the moon but how do you get it even if you could use it? We could not get back to the moon either even if we wanted to. We are not as technically advanced as we might like to think.

10. **Dmitriy**
May 28, 2005

Quantoken,
even if nuclear fusion will become practicle only in 200 years we have to do research now to make it possible in future.
Dmitriy

11. **Michael**
May 28, 2005

Quantoken,
you are underinformed and underexposed. Just keep your stupid ideas to yourself.
Michael

12. **Quantoken**
May 27, 2005

There is absolutely no point in wasting money on ITER or other thermal nuclear fusion research for several reasons:

1.Feasibility. We have spent half a century on this without success. It may take another half century before the technology is finally achieved, and much longer before it becomes an economically viable energy source. The energy crisis will hit far before this becomes successful.

2.Usefulness. The fuel used in thermal nuclear fusion, Helium 3, is a **scarce resource** on earth. Scientists have been telling us the total amount of He 3 stored on earth. That's misleading when you do not talk at the same time **what takes to extract the He 3**. For example He 3 contained in one litre of sea water contain the amount of energy equivalent to one litre gasoline. But to extract the He 3 contained in one litre sea water probably costs energy equivalent to a few hundred litre gasoline. Then it is totally worthless as an energy source even if you do not consider the economical factors.

Or there's plenty of He 3 on the moon, too. But to go to the moon and bring the He 3 back would also cost much more than what it is worth.

3.Cost, cost and cost. That includes econimical cost, environmental cost and resource cost.

Quantoken

Smart People

May 29, 2005
Categories: Uncategorized

Via Slashdot, an article that seems quite relevant to the current situation of string theory.

Comments

1. May 31, 2005

   Yes, Lubos Motl comes immediately to mind upon reading that article, as does Christopher Hitchens.

2. **stephen**
   May 30, 2005

   Taken from article

   "The lesson is this: ?Speed kills?. I was never very good at pool, but this one guy there was, and whenever we?d play, he?d watch me miss easy shots ***because I tried to force them in with authority****. I chose speed and power over control, and I usually lost. So like pool, when it comes to defusing smart people who are defending bad ideas, you have to find ways to slow things down.*

   Some mathematicains take years for preparation to solve a problem and the Clay Institue offers "objective mental measures" for consideration and money?

   Would this be like "intelligent design?" and the efforts to "funnel thinking" down to specific lanes. Mathematical models "assumed" as being right for "the situation?"

   All and all, it seems like good advice.

   No "streaming of consciousness" there, but patient deliberation?:)

3. **D R Lunsford**
   May 29, 2005

   Well it seems very condensed.

   -drl

4. **Peter Woit**
   May 29, 2005

   Hi Gentle Skeptic,

   I've posted at various points about some of the topics you suggest (see for instance the third post on the blog near its beginning, and many of the posts about mathematics that I think may have some relevance to physics).

   The questions that are left open by the standard model are few in number and well-known to everyone in the field. Basically the main ones are:

   What is causing electroweak symmetry breaking?

   What explains the pattern of groups, representations and coupling constants of the standard model?

   What about quantum gravity?

   My own best guess about which direction to investigate is laid out in a paper on the arXiv and on my website entitled "Quantum Field Theory and Representation Theory: A Sketch". I've been busy with other things this past academic year, but am again thinking about these topics this summer, and will try and write more of a positive nature about them here in the near future.

   But I really don't want to spend much of my time promoting my own ideas. They're there if you want to read about them (and I hope to write up something more detailed this summer), but I'm not going to repeat them endlessly here. The fundamental problem with particle theory these days is that things are hard and there are few if any good ideas around. But I feel the main reason for this is that all of the intellectual resources of the field are tied up in the failed string theory project. Until this situation changes, it is going to remain unlikely that things will improve.

5. **A_Gentle_Skeptic**
   May 29, 2005

   Peter, your detestation of string theory is almost everywhere in evidence on your blog. Please forgive me for asking, but have you posted somewhere on your blog your own analysis of what are the main questions that physicists ought to be addressing today and what are the promising techniques that they ought to be employing?

   Thanks and best regards…

6. **Anonymous**
   May 29, 2005

   Haha! That fits Lubos Motl perfectly

PITP Showcase Conference

May 29, 2005
Categories: Uncategorized

The Pacific Institute for Theoretical Physics, based at UBC in Vancouver, held a Showcase Conference a couple weeks ago, which was supposed to "celebrate the exciting new developments taking place in theoretical physics". According to the organizers there are lots of exciting new developments in string theory, since six of the invited speakers (Myers, Ooguri, Randall, Schwarz, Shenker, Susskind) spoke on that topic, but no one at all spoke about elementary particle physics. There were also quite a few talks on condensed matter physics.

The talk of John Schwarz consisted mainly of the standard recounting of the history and basics of string theory that anyone who has been to conferences like this has heard a hundred times. This part stopped with Maldacena's work more than 7 years ago. On more recent topics, about the anthropic explanation of the cosmological constant, Schwarz says: "Is there another explanation? I hope so." He ends by putting up a long list of questions about string theory, more or less the same list everyone has had for twenty years now.

Steve Shenker spoke on Emergent Quantum Gravity, with "emergent" the new buzzword of the field. There was a separate workshop on emergence overlapping with the Showcase conference, organized by Phil Anderson and others, with Susskind the only string theorist allowed to speak there. Shenker introduced a new terminology to justify string theory: it is "An algorithmically complete, consistent description of quantum gravity", although he does add the caveat "In certain simple situations (like flat space)". By this I guess he is trying to get around the problem of how to claim that your theory is complete and consistent when you don't know what it is. The idea is that at least you have an algorithm for doing computations. Perhaps he means perturbative string theory, although that is neither consistent nor complete (the expansion in the number of loops diverges). Perhaps he means a non-perturbative formulation like a matrix model, which works in 11 flat dimensions, but then he really should note that he's not talking about quantum gravity in four dimensions, which is what most people care about.

There was an interesting panel discussion on The Theory of Everything?, which was moderated by Steve Shenker. He seemed mainly interested in making the obvious point that string theorists weren't actually claiming that their theory explained anything about, say, biochemistry. The panel was actually balanced between string theory enthusiasts (Shenker, Schwarz, Randall), and skeptics ('t Hooft, Unruh, Wald). Some of Shenker's introductory remarks are inaudible, but he did repeat his claim about the "algorithmically complete" nature of string theory. "t Hooft had some quite interesting comments. He recalled that at a conference back in 1985 he had been the only one who didn't think that twenty years later string theory would have solved all the problems of particle physics, noting that it was now 20 years later, he had been right, everyone else at the conference wrong. He was making the point that string theory now is extremely far from solving any problems in particle theory, and one can't tell if this situation will change in 20, 200 or 2000 years. He tried to say some positive things about string theory, but they were pretty half-hearted. For instance he noted that dualities were very interesting, but they linked one ill-defined theory to another ill-defined theory. He also noted that in its present formulation string theory is only defined on-shell, which he takes as meaning that it doesn't give a true local description of what is going on. He has reasons for being suspicious of people who claim that all one needs is an on-shell theory.

Schwarz attributed the TOE terminology to John Ellis. He said that he feels string theory is very far from explaining anything about elementary particle physics, that it was "almost hopeless to find the right vacuum". He described what landscapeologists are doing in a skeptical tone, but didn't actually criticize this. Answering 't Hooft, he claimed that back in 1985 he and Mike Green were actually more pessimistic than most other people about the prospects for getting quick results out of string theory.

Bill Unruh made the standard criticism that what is wrong with string theory is that string theorists are motivated by beautiful math, not physics. He doesn't seem to have noticed that few string theorists are now doing math, since unfortunately most of them have taken to heart the criticisms of people like him. The failure of string theory has unfortunately reinforced the skepticism of many people like Unruh about the use of math in theoretical physics.

Wald quoted what sounded like a recent description of what string theorists think they are doing, then revealed that his quotes were from the 19th century, and referred not to string theory, but to the popular theory of the time that atoms were vortices in the ether. He deftly made the point that it is quite possible, if not likely, that string theory is just as wrong an idea as the vortex one.

Lisa Randall made some defensive comments about string theory as a guide for future research, even if it turns out not to work. These included the bizarre political analogy that it was wrong to worry about string theory ruining the credibility of physics, because, after all, the bogus WMD business didn't seem to have hurt Bush's credibility.

There were then some questions and comments from the audience. Susskind was in the first row, looking very peevish and defensive. He kept repeating that the field of theoretical physics had "no real choice but to track this down", meaning to investigate the infinite landscape, and that this would take the efforts of many physicists. He explicity worried that funding agencies would not give any grants to anyone working on the landscape, to which Unruh responded that the shoe was really on the other foot, with some NSF panelists refusing to fund anyone who wasn't doing string theory.

The conference web-site also includes an explanation of string theory which claims that in recent years string theory has "evolved very rapidly", that the reason it can't be tested is because of the small distance scales involved, and that it may be testable by observing a "5th force", all of which is a load of nonsense.

Lubos Motl has an interesting post going over all the possible ideas he can think of that might lead to the next superstring revolution. Needless to say, they all sound extremely unpromising to me. Judge for yourself. He also quotes the promotional material for Susskind's book due out late this year. It seems that "the Laws of Physics as we know them today are determined by the requirement that intelligent life is possible".

Comments

1. **Urs**
   June 14, 2005

   Hi Kea,

   I'd be interested to hear more about the formulation of non-standard analysis in the context of topos theory.

2. **Kea**
   June 12, 2005

   "one need more advanced math, like nonstadard analysis or better (epsilon calculus)"

   Hi Juan
   For anyone who is interested: non-standard analysis comes under the umbrella of topos theory

3. **Juan R.**
   June 12, 2005

I was not asking about total area of BH, i was talking about quantum of area (in the spirit of LQG).

Some of my questions were "redundant" because i already know the reply. For example the authors are interpreting incorrectly the "continuum limit". If really one takes the limit of area (or volume) –> 0 one obtains a kind of spacetime Zeno effect and nothing changes in spacetime.

This confussion is usual in quantum gravity, due to false statement of that GR is works with continuum spacetime. This cannot be studied from usual math, one need more advanced math, like nonstadard analysis or better (epsilon calculus). There one discovers that area is newer zero. This is not speculation, is well-proven, even there are experiments that can prove this. Moreover, mathematically one can prove trhat in the limit of area (quantum of) zero there is no Hawking radiation and BH are stationary ones.

In string M-theory, people still believes in differentiable manifolds (like new G2). I will always say that string M-theory is a waste of time because is being developed by – people. It is really interesting that each year string theorists agree with things that i said years ago.

4. **the original anonymous**
   June 11, 2005

   Juan, naturally I cannot speak for the CDT researchers. It is only recently that I have begun to watch this line of research carefully. I want to keep current some of your questions about CDT and perhaps respond (even though not authoritatively)

   You ask about CDT model of BH. This goes to some of the most recent work: a paper by Loll and Dittrich that just appeared, and the PhD thesis of Dittrich which unfortunately is written in German. So far there is almost nothing written about the way the BH should be modeled in CDT.

   I think this new BH research will be prominent at the October conference ("Loops 05") at Potsdam AEI. Dittrich is at AEI and is one of the local organizers, Loll is one of the invited speakers.

   You asked about the area. So far, in what I know of what is published, neither BH area nor entropy have been calculated in CDT.

   So that we do not forget the questions from your earlier post, I will copy some interesting ones of them:

   [[…

   …I have also my doubts about the mathematical machinery for taking the "continuum limit". Or there is continuum spacetime or one can measure quantums of area for example in a BH. Is not clear for me, What do authors say?

   At macroscopic distances, they "claim" for demonstration that geometry is 4D. On other paper, they say

   "Of course, we will never be able to show by computer simulations alone that the effective
   dimension is exactly equal to four. What we are assuming here is that this dimension is
   indeed an integer, because there are no classical theories describing the dynamics of geometries
   of dimensionality 3.9, say."

   Interesting concept of "derivation". It resembles to me the "derivation" of 4D GR from 10D string theory, i.e. when one knows the correct answer first and then modify/adapt the obtained reply from theory to the real, needed, reply.

   Moreover there is available other approaches (i know some very much) that provide outcomes for experiments and can be tested. Is there something about this outside of paper. Experimental data perhaps? Author claim that no check if
   Newton?s inverse square law can be recovered in an appropriate limit. Others have already proved this very important point.

   A dynamically generated scale dependent
   dimension from 2 to 4 is truly exciting news, i agree, but does work it in paper or in reality?

   ****

   And a joke for finish this large post. How many time will be necesary before string theorists claim that this is part of string theory? including the claimed reduction of dimension to short scales

   Posted by: Juan R. at June 9, 2005 07:02 AM]]

5. **the original anonymous**
   June 11, 2005

   I have to laugh, Juan, because it is just as you said here:

   [[You say,

   "But the idea of dimension being around 4 at large scale and declining (more or less continuously) to around 2 at small scale, how could this idea be accommodated in string theory?"

   A priori cannot be accommodated in today string theory, but like there is no string theory, just a big collection of conjeture, hypothesis, beliefs, etc. i wonder if future string theorists will claim that your ideas are really a part of string theory. String theory is great 🙂

   Simply revise the history of field and see like the theory was changing and adapting to new ideas from outside of string community. For example, one or two decades ago LQG was nonsense, one could compute "nothing", was called prerelativistic physics and garbage by string theorists. One decade ago, some (e.g. B. Greene) began to claim that perhaps LQG and string theory both were two sides of same reality. Now several string theorists claim that LQG may be a part of string theory…]]

   and ALREADY a string-believer here at this blog is making ludicrous attempt to imitate the outside result:

   [[String worldsheet has dimension 2, background space has dimension 4. 4->2 is a typical string effect. QED]]

   😀

   I can assure you he is not the real anonymous, only a second-rate imitation anonymous

   This thread may have expired but I will stay around a while in case there is more discussion.

6.
June 10, 2005

String worldsheet has dimension 2, background space has dimension 4. 4->2 is a typical string effect. QED

7. **Juan R.**
June 10, 2005

Dear "",

"You suggest string theorists like to absorb every interesting idea into their polymorphic theory as a possibility. Or at least this may have been their custom."

It is not my suggestion, it has been claimed by many people including own string theorists.

I can do is quote to string theorist Seiberg who said recently:

"*string theorists are arrogant enough that whatever comes up in their research, they will call it string theory.*"

You say,

"*But the idea of dimension being around 4 at large scale and declining (more or less continuously) to around 2 at small scale, how could this idea be accommodated in string theory?"*

A priori cannot be accommodated in today string theory, but like there is no string theory, just a big collection of conjeture, hypothesis, beliefs, etc. i wonder if future string theorists will claim that your ideas are really a part of string theory. String theory is great 🙂

Simply revise the history of field and see like the theory was changing and adapting to new ideas ***from outside of string community****. For example, one or two decades ago LQG was nonsense, one could compute "nothing", was called prerelativistic physics and garbage by string theorists. One decade ago, some (e.g. B. Greene) began to claim that perhaps LQG and string theory both were two sides of same reality. Now several string theorists claim that LQG may be a part of string theory.*

String theorists are very arrogant and don't love the idea of that anybody is working in more complex, sophisticated, and good theories than them (the most sophisticated that i know is non-criticial string theory in Lindblad form, a formulation totally outside of standard easy string theory. For instance, NCST is a nonunitary formulation violating Schwartz ineffective desires (an unitary theory is simply wrong). Curiosly there is not serious formal theory, just a generalization of usual (Schwartz, Greene, Witten, vafa, etc.) easy string theory to adapt it to theories known in other branches of physics since decades!!!!

The first versions of NCST (the 90s) used an old (really outdated) theoretical formalism developed by known chemist Ilya Prigogine. More recent versions are constructed from standard axiomatic theory developed by mathematical physics community. There six theories computing from different communities: chemists, decoherence physicsts, laser community, astrophysicists, etc.

From canonical theory one can derive all those efforts (decoherence, etc.) from a single advanced theory. The standard axiomatic theory developed by mathematical physics community arise like a simple trivial theory after of four succesive approximations. ***that is the theory used in NCST, therefore we know that is simply a toy model and not the more fundamental theory like people as B. Greene claim.***

8.
June 9, 2005

Juan_R, thanks for your reply! Your joke is also an interesting question:

"How many time will be necesary before string theorists claim that this is part of string theory? including the claimed reduction of dimension to short scales"

I also ask myself this, especially the finding about dimension (as measured by a diffusion process) declining at short scale.

You suggest string theorists like to absorb every interesting idea into their polymorphic theory as a possibility. Or at least this may have been their custom.

But the idea of dimension being around 4 at large scale and declining (more or less continuously) to around 2 at small scale, how could this idea be accommodated in string theory?

You have asked several questions that either I cannot respond to, or would take us rather far from Peter Woit's topic of this "PITP Showcase" conference blog.

I will come back to this if I see that it is not considered out of place.

9. **Juan R.**
June 9, 2005

Interesting posts "anonymous",

if you want talk about triangulations was innecesary your "hey guy that you think about".

If i don't love dynamical triangulations, you would not are hungry… but seeing your special interest in the topic…

****

It is really difficult for me to take seriously works, who authors still "believe" that wavefunction of universe may be a solution of the Wheeler-DeWitt equation

… and continue with Einstein-Hilbert (i wonder like will solve extragalactic problems from it), the use of piecewise Minkowskian geometries (like in "metric"-gravity approaches). It will be interesting that authors can say about "affine" gravity models and computation of astronomical orbits.

In one of papers they compute, wrongly, the wavefunction of the universe (eq 22). Fascinating for one so higghly estimated work (by its authors). The derived effective Euclidean
action is not correct.

The details of the renormalization mechanism (here open) were already solved in other methods (using also discrete structure).

What about the inclusion of matter fields? Are we claiming for a consistent quantum gravity or realistic quantum universes without matter or… only speculating? Will the method work fine with more realistic models?

The phase diagram is really interesting but does work it in paper or in reality?

What about the "fit" of used simulation method with standard QFT? I wait with impatience a rigurous study of this.

I have also my doubts about the mathematical machinery for taking the "continuum limit". Or there is continuum spacetime or one can measure quantums of area for example in a BH. Is not clear for me, What do authors say?

At macroscopic distances, they "claim" for demonstration that geometry is 4D. On other paper, they say

"Of course, we will never be able to show by computer simulations alone that the effective
dimension is exactly equal to four. What we are assuming here is that this dimension is
indeed an integer, because there are no classical theories describing the dynamics of geometries
of dimensionality 3.9, say."

Interesting concept of "derivation". It resembles to me the "derivation" of 4D GR from 10D string theory, i.e. when one knows the correct answer first and then modify/adapt the obtained reply from theory to the real, needed, reply.

Moreover there is available other approaches (i know some very much) that provide outcomes for experiments and can be tested. Is there something about this outside of paper. Experimental data perhaps? Author claim that no check if
Newton?s inverse square law can be recovered in an appropriate limit. Others have already proved this very important point.

A dynamically generated scale dependent
dimension from 2 to 4 is truly exciting news, i agree, but does work it in paper or in reality?

****

And a joke for finish this large post. How many time will be necesary before string theorists claim that this is part of string theory? incliding the claimed reduction of dimension to short scales

10. **Kea**
June 8, 2005

Who is the person loading us with CDT info?

11.
June 8, 2005

Juan R mentioned Dynamical Triangulations research. Instead of arguing with his notion of its significance and impact, I will post a short reading list for anyone who wants to find out what is going on in that field

Basic papers:

http://arxiv.org/hep-th/0105267
Dynamically triangulating Lorentzian quantum gravity
41 pages, 14 figures
Nucl.Phys. B610 (2001) 347-382

http://arxiv.org/hep-th/0505154
Reconstructing the universe
52 pages, 20 figures
Report-no: SPIN-05/14, ITP-UU-05/18

Abstract: "We provide detailed evidence for the claim that nonperturbative quantum gravity, defined through state sums of causal triangulated geometries, possesses a large-scale limit in which the dimension of spacetime is four and the dynamics of the volume of the universe behaves semiclassically. This is a first step in reconstructing the universe from a dynamical principle at the Planck scale, and at the same time provides a nontrivial consistency check of the method of causal dynamical triangulations. A closer look at the quantum geometry reveals a number of highly nonclassical aspects, including a dynamical reduction of spacetime to two dimensions on short scales and a fractal structure of slices of constant time."

—-

Recent short papers giving new results:

http://arxiv.org/abs/hep-th/0404156
Emergence of a 4D world from causal guantum gravity
11 pages, 3 figures; final version to appear in Phys. Rev. Lett
Phys.Rev.Lett. 93 (2004) 131301

http://arxiv.org/abs/hep-th/0411152
A semiclassical universe from first principles
15 pages, 4 figures
Phys.Lett. B607 (2005) 205-213

http://arxiv.org/abs/hep-th/0505113
Spectral dimension of the universe
10 pages, 1 figure
SPIN-05/05, ITP-UU-05/07

Lecture notes to introduce graduate students to the field

http://arxiv.org/hep-th/0212340
A discrete history of the Lorentzian path integral

R. Loll (U. Utrecht)
38 pages, 16 figures
Lect.Notes Phys. 631 (2003) 137-171

12. **Juan R.**
June 8, 2005

Yes, there has been a lot of discussion on this topic of dynamical triangulations.

Ambjorn, Loll and collaborators have provided some new material for strings theory, more correctly that a theory of membranes may be relevant for a background independent form of string theory.

These triangulations appear to be useful in some models of spacetime foam. I don?t know if are used in LQG. Possibly dynamical triangulations can be a computational approach to some basic aspects of quantum gravity. It is possible that finally it modifies some aspects of particle physics: high-energy behavior, cut-offs, etc. but I don?t wait for some really profound.

Thanks Tony, nice article

13. **Tony Smith**
June 8, 2005

Lee Smolin in an opinion article in the June 2005 isssue of Physics Today (pages 56-57) said in part:
"… In the present system, scientist feel lots of pressure to follow established research programs led by powerful senior scientists. Those who choose to follow their own programs understand that their career prospects will be harmed. …
Those who invent their own research programs … are often undervalued and underappreciated …
Several young string theorists have told me they simply have neither the time nor the freedom to ask their own questions or develop their own ideas. …
young theorists who pursue alternatives to string theory have had great difficulty finding any academic positions in the US. …".

Smolin proposes that "… Scientists should be penalized for doing superficial work that ignores hard problems and rewarded for attacking the longstanding open conjectures …
A research program should not be allowed to become institutionally dominant until supported by convincing scientific proof of the usual kind. …
A foundation or agency could create … fellowships, to go specifically to theorists under 40 who invent their own ideas and programs aimed at solving foundational problems in physics …".

Cynically, Smolin's article could be viewed as an attack on superstring theory and a plea for more funds for his LQG program, especially since he characterizes his Perimeter Institute as having a "… specific mandate … to be a home for independent foundational thinkers …".

Even though Smolin's article may be self-promoting, at least some of its points may have some validity.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

14. **Kea**
June 7, 2005

"Peter, what do you and other posters think about the
theory of dynamical triangulations…"

There has been a lot of discussion on this over at PF lately.

15. **Peter**
June 7, 2005

I'm glad people are working on dynamical triangulations, maybe this will give a viable verion of quantum gravity. But, like LQG, I don't see any way to connect this stuff to particle physics, which limits my interest in it, since my main motivation comes from that direction.

16.
June 7, 2005

Peter what do you and other posters think about the
theory of dynamical triangulations pioneered by
Jans Ambjorn and collaborators . From what I understand this is the third alternative approach
to quantising gravity besides string tehory and LQG.
do you think one can learn particle physics from this approcah?

17. **Juan R.**
June 7, 2005

Scott ,

Far from any interpretation, I simply cite your previous phrase.

?*I would suspect that any chemists who share this view do not trully understand what the implication of QM is*.?

The problem that I remarked is perfectly well known. I see no problem that you can easily find literature in the topic.The most easy violation of QM is to observe that full QM is not used in computational programs in molecular chemistry physics (e.g. Gaussian) only part of QM is used, whereas other part is ignored, when one reintroduces all of QM in the equation one observe that the concept of molecule disappear. This is not so strange since there is no structural aspects in QM, one cannot obtain the description of structures. There is no ?problem? in atomic physics.

In the reference that I provided you can find some easy (at undergraduate level) discussion regarding methyl acetylene, allene, and cyclopropene. See reference cited for more information.

Now you add ?*Also your point has to do with the concept of chemical structure in general whereas I was originally questioning why DNA's incomplete quantum explanation in particulare deserved special attention, which is a seperate issue entirely.* ?

Far from personal interpretation, I read your past post ?*Personally I am not aware that biochemists were having any trouble understanding DNA structure so I see no reason why the laws of physics would need to be changed to accomodate DNA.*?

I simply have pointed that we need change them for accommodate DNA since a pure QM description of DNA does not work. That was my point, only that.

Torbjorn Larsson

Simply I wanted state the stupid elitism in some members of academia.

Thanks by your mention to Baez?s index, we would agree that many string theorists are effectively crackpots.

You are right, these results were eventually published, but in some cases was a question of luck (there are well documented cases), in other occasion the publication was in an obscure journal and only after several decades that work recovered like excellent. Other works are rejected for publication during many decades (e.g. 25 years). All those cases I know are for ?simple? theories in a very specialized subfield of a part of science (e.g. Yukawa Meson theory), modifying only a 1% of accepted mainstream in that field (e.g. physics) and practically nothing in the rest of science (e.g. ecology). Therefore the initial rejection was from a small part of comunity believeing in mainstream.

I ask, what for a revolutionary theory modifying physics, chemistry, biology, etc. at the same time?

Previous editor-in-chief of Nature said that today Newton theory would be rejected for peer-review publication. What about a theory that violates the policies of compartimentalized journals, uses parts of different theories of physics, chemistry, etc., or even do not use "writting style" and "recomendations" (different for chemistry, physics, ecology, medicine, etc.)

Weinberg said that a new theory demonstrating that the concept of field is not fundamental would be a sensation. Imagine a hypotetical rigorous and advanced theory that modifies fields, GR, QM, thermodynamics, chemical kinetics, statistical mechanics, Maxwell EM, etc. Do you really believe that would be published by any editor of usual journals?

My previous use of words ?true? or ?TOE?, of course, was only prose for remarking the point.

About consciousness, I can assure to you that QM is not sufficient. I have no doubt. It is true Gell-Mann discusses an effect connected to specific brain structure, but he use a reformulation of standard QM that when properly improved (Gell-Mann/Hartle is non rigorous in several crucial points) offers to us a new formulation of nature generalizing QM.

18. **JC**
June 7, 2005

Many fanatics and some crackpots completely refuse to answer questions in plain english without any obfuscation and/or waffling. They also frequently get angry whenever hard questions are asked directly to them. Their tactics frequently involves ad hominem attacks against the questioner themselves, instead of answering the hard questions directly.

Perhaps there's some validity to the notion that "the truth hurts" for many people.

19. **D R Lunsford**
June 6, 2005

Meep! 173rd post! 40th prime post!

-drl

20. **Torbjorn Larsson**
June 6, 2005

Still not correct enough:

"distingush" – distinguish

"throw more money in" for experiments, even if new theories have superseeded the first.

21. **Torbjorn Larsson**
June 6, 2005

(Redfaced, again.) Umm, I thought I vetted my last commentary enough. Let's see:

"learn ST" – well, get to know some more about it, more likely.

"everythere" – everywhere.

"statistics" – probability models.

22. **Torbjorn Larsson**
June 6, 2005

Seems like most of the arguments I'm involved in borders to the content of blog and post, so I'm back:

Juan:
Crackpotism – Thanks for the interest! I think it's hard to measure alone due to different arenas (someone may be perfectly normal outside his eccentricity) and the consensus nature. Thomas mentions Baez measurement tool (http://math.ucr.edu/home/baez/crackpot.html) that lets one judge the most common crackpots outhand. But there remains social factors.

Publication – You mention difficulties, but these results were eventually published. I'm not sure what you mean by "true "TOE"" – something outright called "true" or "TOE" will not get published due to the implied crackpotism.

Consciousness – What I argued with Matti was that QM and consciousness cannot be fundamentally connected, if not TGD replaces todays QM. Gell-Mann discusses an effect connected to specific brain structure. If you ask me however if such QM (or ST) is needed to explain consciousness my answer would be 'no' from Occam's razor, in lack of further evidence.

QT:
First, I will offer my apologies for being such an arrogant bastard, whether you noticed it or not. Web posting seems to be an excellent social arena, and apparently while I can spot arrogant behavour in others, I need to put my nose in my own mess to recognise it.

Second, I'm not an "SSTer" or even STer. I'm probably going to try learn some of it though.

Third, on dimensions I didn't make myself very clear. Obviously I can misinterpret a lot, being next to ignorant on string stuff. But one has to distingush between _spacetime_ dimensions and others. ST concludes that it has a finite number of dimensions. Apparently they don't even need to have a geometry. Out of this, somehow, ST has to build spacetime.

So if you say 4 =! 11 or something similar, you are comparing apples and pears. You can't do that. So no contradiction.

Maybe ST has elicited true statements, but if so they were already known. Mostly it is Not Even Wrong – but so are its contenders for QG. I don't think you can call it crackpot – if you do, most will disagree.

However, you raise one very interesting point!!! How long may a theory be alive without any definitive answer?

Obviously, in math, a very long time. (Fermat's theorem, for example.) But I think of math as an internal logic game, very few measurements here. 🙂 Unfortunately, since I'm Baeysian, I think that you must sooner or later measure in statistics. Obviously, everythere else you must, too.

In lack of better measure, let's go for the money! So, I would say that 2-3 generations can be left hanging, since you probably want your children or grandchildren to benefit at least. So I would up your estimate to 3*30 years. After that it can be declared failed, and there is absolutely no use to throw more money in.

23. **Scott**
June 6, 2005

Also your point has to do with the concept of chemical structure in general whereas I was originally questioning why DNA's incomplete quantum explanation in particulare deserved special attention, which is a seperate issue entirely.

24. **Scott**
June 6, 2005

juan,
my point wasn't that the quote showed that they didn't understand QM it was that if I am going to doubt qm's ability to account for DNA I am going to need to see the evidence for myself untill then i am going to assume that it can account for it. I was hoping that someone might give me some details of this controversy as it seems very interesting and very helpfull if true. "the
view that structure is nothing but a metaphor"(and basically a meaningless statement anyways) is only one way to interpret the equations of QM and so that is why I suspected that there may not really be a controversy over this and requested actuall data showing some sort of violation of quantum mechanical principles. Still waiting for some info on this.

25. **Juan R.**
June 6, 2005

I said

"To list here the extensive research literature in the topic and the different models/proposals (by quantum chemists and molecular physicists) for improving QM would extenuate to me."

Of course, string theorists know nothing of this and string theory is a simple (probably unitary) elementary QM.

This is another of dozens of reasons of that ST has failed like a theory of everything.

The irrelevant attempt, from string theorists, to explain "everything" knowing almost nothing (e.g. what theories are used in other fields or like fit experimental data) is the best example of crackpot that i know.

26. **Juan R.**
June 6, 2005

Scott , the reference you solicited

When chemists disagreed with physicists? idea of that chemical reactions could be explained with classical mechanics. Physicists smile and claimed that didn?t understand the topic 🙂

When chemists disagreed with physicists? idea on that chemistry reactions could not conserve energy. Physicists smile and claimed that didn?t understand the topic, in fact the law of conservation of energy was rejected by top physics journal and published by first time in Annalen fur Chemie 🙂

When chemists disagreed with physicists? idea on that chemical bonds could be explained from classical electromagnetism smile and claimed that didn?t understand the topic. Please remember those childish models proposed by physicists (including several Nobel laureates for Physics like Stark). But chemical model (Lewis) was correct and was based in radical assumptions like the superposition principle BEFORE QM was developed by physicists 🙂

When chemists disagreed with physicists? idea on that all of chemistry could be explained with Schr?dinger equation. Physicists smile and claimed that didn?t understand the topic. Chemists shows that Dirac ideas on relativistic QM and chemistry was completely wrong. It is interesting that whereas many physicists (of course with no idea of chemistry) claim that famous Dirac?s quote is correct, Murray Gell-Mann said that ?of course it was an exaggeration?. I am sure that Gell-Mann thinking is the outcome of his chemical friends that explain to him how things are in reality 🙂

The quote that perturb to you is not from a top research journal is from an basic educative journal.

J. Chem. Educ. 2000, 77, 522?526.

To list here the extensive research literature in the topic and the different models/proposals (by quantum chemists and molecular physicists) for improving QM would extenuate to me.

You irrelevant claim, on that any chemists who share this view do not truly understand what the implication of QM is, would be remember in a future like today we remember the dozens of false claims of physicists from Newtonian ?poque. I?m sorry to say this.

Seeing your writing, especially your note on ?exact explanations? and ?complexity of DNA? suggests to me that you have no idea of the field that I was talking. I suspect that your knowledge of QM is very elementary one, perhaps at mathematical level of last Weinberg QFT manual?

Torbjorn Larsson said

A crackpot is "a person who is regarded as strange, eccentric, or crazy".

Humm!!! Really interesting, that is the definition of Einstein in his last years, when he was searching for a unified theory of fields and was considered an eccentricity (in his own words) caused by his old age.

?But the total risk is small for the really good stuff, so if you have great fears I would suggest that is because you have some other reason to be.?

Almost 35 of works after awarded by a Nobel Prize were rejected for peer review publication, and completely ridiculized by specialists. At least 18 of most cited works of all history of science were initially rejected for peer review publication.

The situation now is still poor due to increase of economic risks in usual journals, Editors prefer reject a good but problematic paper rather than lose one or two points in IF and similar measures.

Some editors simply admit that the true ?TOE? cannot be published in usual journals because editorial and peer-review policies are not good enough.

?I think decoherence and/or consistent histories make consciousness unrelated to QM.?

Murray Gell-Mann (crackpot?) considers that consciousness and other questions of mind modeling may be explained from his consistent histories like a kind of quantum effect amplified by special biomolecular structure of cerebrum cortex.

Perhaps the crackpots are a 90% of those wonderful members of Physics departments. Perhaps those intelligent guys that rejected his quark theory for peer-review publication?

27. **quantoken**
June 6, 2005

TL said:

"I think common logic tells the rest of us that even if SST isn't successful (Uups, yet) it doesn't contradict."

The only common logic I know says that 4D != 11D. Maybe you SSTers have invented a different set of math in which 4 == 11. You still have not bridged the gap between 4 and 11.

But it is recognized that so far SST has NOT been able to make a single statement that is true about nature, and that can be verified to be true. How many papers have you guys published in 20 years, yet you have NOT made one damn true statement about the nature.

It is about time to call SST a crackpot theory. If you think 20 years is not enough, then I can give you another 20 years, or maybe more. But at the end of day it can not be allowed indefinitely or forever, there needs to be **a finality to this craziness**. Let's call it what it is worthy: a crackpot theory.

Quantoken

28. **Thomas Larsson**
June 6, 2005

Larsson (Are there two Larssons?), can you tell us more about your theory of quantum gravity using projective representations of the diffeomorphism group? You keep mentioning to online but never spell out the details.

Yes, there are two Larssons. It is a quite common Swedish name, meaning the son of Lars (or Lawrence). I'm called so because my great-great grandfather's name was Lars Nilsson.

The most recent reference is hep-th/0504020; further references to published work by myself and others can be found therein. A very similar manuscript will appear as a chapter in a book on quantum gravity, probably scheduled to be released next spring. It has the same publisher as this book.

29. **Scott**
June 5, 2005

kea,
provide a link and i will look at it but i am in the middle of finals and am spending to much time dicking around as is to go find it myself.

anyways back on topic,

I find the fact that Susskind was worried that the landscape might stop being funded is a very good sign.

30. **Torbjorn Larsson**
June 5, 2005

Uups again, thet should be "successful yet".

31. **Torbjorn Larsson**
June 5, 2005

Uups, sorry Peter, I missed your commentary about the content stringency at first. Now, I would like to withdraw with a response to QT, string part of which may be acceptable, otherwise feel free to delete:

QT:

About 3section I think Scott made my point eminently. You could call my argument a retrap. 😉

"For as long as there hasn't been a successful attempt to reduce to 4-D from SST, the SST remains a contradiction to the known 4-D spacetime, and hence remain a crackpot."

I think common logic tells the rest of us that even if SST isn't successful it doesn't contradict.

32. **Torbjorn Larsson**
June 5, 2005

Anonymous:

"Are there two Larssons?"

Yup. I'm the ignorant one (on theoretical physics).

33. **Kea**
June 5, 2005

Scott:"there is no reason the structure of DNA should drive specualation over any other phenomenon that is only roughly described by QM due to its complexity"

At least read the paper referenced below.

34. **Peter**
June 5, 2005

I've been away much of the last week and haven't had time to do much with the weblog. This comment section has unfortunately been taken over by people who want to carry on discussions that have nothing to do with the original posting. Please do this elsewhere, from now on I'll start deleting such postings.

I'll also try and write something later tonight or tomorrow about the workshop where I spent most of last week.

35. **Scott**
June 5, 2005

ok will then can you point to more accurate observations of DNA that contradict current theory? If not there is no reason the structure of DNA should drive specualation over any other phenomenon that is only roughly described by QM due to its complexity.

36. **Kea**
June 5, 2005

Scott: "..check out about this issue somewhat quickly.."

No one said it was easy.

37. **Kea**
June 5, 2005

…which is true but no more then any other structure must be explained by the new physics unless DNA is not adequately accounted for in the old physics…

Let me spell this out: Newtonian mechanics has a perfectly adequate description for the orbit of Mercury, so long as one doesn't look too accurately. The situation here is analogous. Of course we know roughly what DNA is using standard QM. That doesn't necessarily mean that our understanding cannot be improved.

38. **Scott**
June 5, 2005

Quantoken,
read what TL said about trisection again,

"On 3secting angles, of course some angles can be constructed as required, that is known since the Greeks I think. The problem is to 3sect _all_ of them. (Or any randomly chosen one, if you prefer.) You know, most of the time we complain that you don't know, and don't want to learn, basic physics. Now it turns out you don't even know maths…"

he is saying not that you are wrong about being able to do this but that you lack the knowledge that this has been known for a long time(though of course from your wording that is not clear at all) So he was not nor ever did dissagree with your ability to trisect angles and hence did not fall into your "trap"

Kea,
I am in the middle of studying for finals right now, I don't have time to go to the library and find and read 53 pages of this book, if you have some sort link i can check out about this issue somewhat quickly it would be much appreciated. I do not think I am misunderstanding you, you said "As a pragmatist I simply recognise that the structure of DNA appears with tortile tensor categories, and hence must play a role in the new physics", which is true but no more then any other structure mus be explained by the new physics unless DNA is not adequately accounted for in the old physics for which i have seen no evidence.

39. **Kea**
June 5, 2005

"I was looking for something more specific I don't have the time to read through 20 books to find this supposed evidence against QM."

Scott – you appear to have misunderstood. We're not saying anything against QM per se. We're talking about theories of QG which go beyond QM.

40. **Kea**
June 5, 2005

Scott

How about

M.C. Shum
Tortile tensor categories
J. Pure Appl. Alg 93 (1994) 57-110

and references therein.

41. **Quantoken**
June 5, 2005

TL:

You made yourself a **very good example** to illustrate my point when I meantioned trisection angles. Go back and read my original word carefully, especially pay attention to the keyword **"SOME"** which I highlighted. The point is most people fall into such a mentality that they automatically close up at a first sight of something offending to their beliefs, and does not bother to carefully exam the fact. The fact is I have NOT said not a single thing that contradicts the conventional wisdom on trisection angles. But you thought I did and tried to argue with me.

You fall into the trap I set up to prove my point. Isn't it great!!!

Now, about 4-D spacetime and SST. There is no disagreement that macroscopically the spacetime is 4D. There are various attempts to reconcile SST with the 4-D spacetime. None successful or convincing so far.

For as long as there hasn't been a successful attempt to reduce to 4-D from SST, the SST remains a contradiction to the known 4-D spacetime, and hence remain a crackpot. So keep trying and trying harder to show me that you can get nothing but 4-D out of SST, until you do SST remains a crackpot theory!!!!!

As for microscopic scale, one that approaches the discreteness of spacetime. I think the dimentions would be zero, not 11. But I am not not going to discuss it here. The important things is any correct theory must tell us **without ambiguity and without a second guess of possibility** that the macroscopic spacetime is 4-D.

Quantoken

42. **Scott**
June 5, 2005

anon,

I was looking for something more specific I don't have the time to read through 20 books to find this supposed evidence against QM. Thanks anyways though.

43. **Kea**
June 5, 2005

"Every so often, Peter W has to drive all the crackpots out of his comment section, and clearly this thread is ripe for another such cleanup."

Perhaps you could clarify for us: precisely which posters on this thread are crackpots? Am I one? If so, then the lack of mathematical sophistication in your remark hardly makes my knees tremble.

44. **anon**
June 5, 2005

Scott

You could start with the books on the list

http://au.expasy.org/seqanalref/bioinfbo.html

45. **Scott**
June 5, 2005

Juan,

"Most chemists react with complete incredulity to the
view that structure is nothing but a metaphor, pointing out the
seemingly overwhelming evidence for structure that comes from spectroscopic and other structural studies. They suggest that if a deep quantum mechanical analysis reveals molecular structure to be a mathematical artifact, then the fault must lie with present-day quantum mechanics and not with the deeply entrenched chemical notion of structure."

where did this quote come from? I would suspect that any chemists who share this view do not trully understand what the implication of QM is. Show me an actual study with evidence that the observations of DNA can't be explained by QM(not that an exact explanation doesn't exist which it won't due to the complexity of DNA).

46.
June 5, 2005

Larsson (Are there two Larssons?), can you tell us more about your theory of quantum gravity using projective representations of the diffeomorphism group? You keep mentioning to online but never spell out the details.

47. **D R Lunsford**
June 5, 2005

TL – ok thanks.

-drl

48. **Torbjorn Larsson**
June 5, 2005

Damn, I meant reviewing, not refereeing, of course. Sigh, I haven't used my English part of the brain too much lately…

49. **Torbjorn Larsson**
June 5, 2005

QT:

Taking your own advise, please tell us how SST contradict 4D spacetime? AFAIK it doesn't, 4D may emerge and there are proposals how it does.

Nitpick: You can't say that anything is allowed in QG, you mention some restrictions.

On 3secting angles, of course some angles can be constructed as required, that is known since the Greeks I think. The problem is to 3sect _all_ of them. (Or any randomly chosen one, if you prefer.) You know, most of the time we complain that you don't know, and don't want to learn, basic physics. Now it turns out you don't even know maths…

If a guy has spent 27 years to insist on something without much out of it, au contraire, he _is_ totally nuts!!! I hope you will one day understand why…

You should not confuse nut with crackpot.

I know you don't understand the latter concept completely. (Please don't ask me why!) From the above, it seems you don't know how to measure nuttyness, either. But a nut is someone who, however intelligent, is "afflicted with or exhibiting irrationality and mental unsoundness". (Hmm, so me answering you once or twice may not be nuts. 😉 A crackpot is "a person who is regarded as strange, eccentric, or crazy". So you may seem crackpot without being nuts, or you may be nuts without being regarded as crackpot.

Specifically on Matti maybe my claim, in an earlier comment, on the problems with his consciousness claims will prove correct. Crackpot or nut or neither, I cannot however judge alone; at least there must be some consensus in regard of crackpotism, see the definition above.

50. **Torbjorn Larsson**
June 5, 2005

I am hesitant to join a thread that overwhelm my ability to handle all sub-subjects, but it is raining outside and you all seem to have fun. (BTW, is there a theory for the growth of complexity in threads, however complexity is defined? 🙂 My interest stems from merely trying to orient myself shallowly on todays front edge physics, so I am eminently equiped to make a longwinded fool of myself here:

Aaron makes a lot of good points. As I understand it, it has been and will be ever more difficult (expensive and timeconsuming) to make experiments. In lack of definitive experiments, string theory as well as the contenders currently hang out there. Obviously string theory has merits, as Aaron and others point out, and it's not the only theory out there, so the situation is normal.

Some posters seem to think that good science may be put down or will stay buried. I don't agree.

There are some risk that ideas or results will be rejected outhand by mistake or purpose (to close to researchers own products), I know that from my own publishing and refereeing as PhD, alas not on theoretical physics. (Of course, I am biased since the refereeing was a consequence of my own papers being accepted.:-) Or papers may never be read and supported. This is characteristics of filters and of social contexts.

But the total risk is small for the really good stuff, so if you have great fears I would suggest that is because you have some other reason to be.

Chris:
How do you make sense out of "the Laws of Physics are determined by the requirement that the laws of physics are discoverable"? Is it not possible that there are laws of physics that will never be discovered (to become Laws of Physics) or explained? I think Goedel has something to say about that in an even simpler context.

Matti:
I thought the view that QM should have anything to say about consciousness was shown to be a dead horse long ago.

There are still no good definition of consciousness, so how do you measure and experiment?

And when we can do that, there is the matter of QM interpretations, or 'different religions' that I saw someone refer to them as. I think decoherence and/or consistent histories make consciousness unrelated to QM. If that happens, you probably cannot do what you propose to do, unless you can make these interpretations faulty. Honestly, in my consciousness you appear nutty on this. 😉

51. **Quantoken**
June 5, 2005

A Rivero:

Surely there ARE crackpots who claim something that has long been proven wrong, like perpertual motion machine kind of thing. But more often there are alternative theories that were thought to be crackpots because they do not seem to be adhere to the main stream, but they actually adhere to all known and proven physics theories, and they only contradict the unknown and unproven of physics.

For example super string theory is crackpot, because it clearly contradict some known and proven physics, e.g. that **spacetime is proven to be 4-D**. And anything that does not agree with super string is probably NOT crackpot, as long as they have no inconsistency to classical and quantum mechanics and other known physics laws. As in quantum gravity, since it is totally unknwon, anything is allowe so far.

I hereby boudly claim that I find trisection angles is possible for some angles. Reading that most must think that such a claim is a crackpot because it's long proven trisection of angle is impossible. But it is actually possible for **SOME** angles. This is exactly what happens when a paper making some unusual, out of the main stream claims, is submitted. You would probably throw it away the first instance upon seeing the offending keyword trisection angle, but if you spend some time and taking it apart you find the claim actually does not violate any thing that is known so far.

Matti Pitkanen is an interesting guy. I do not think I can agree with his theory. But if a guy spend 27 years to insist on something, unless he is totally nuts he must have thought about something that no one has thought about.

My point is that if any one spend a little bit time study what his stuff is, and picking out just one claim he made which clearly violates one of the known physics law, then he can be called a crackpot. But if **no one** has done such a study, and no one has picked apart even **just one wrong claim** of Matti Pitkanen's, then he deserved no hat called "crackpot", regardless how weird his theory seems to be to the rest of us.

For civilized discussions can we all agree on that? That the word “crackpot” should not be abused as a four letter word used for personal attacks, but rather should be used together with facts and evidences. If you claim Joe is a crackpot, then show one case where Joe makes a claim which contradicts known physics? If you are unable to cite examples, then you should be refrain from using that word.

Quantoken

52. **Alejandro Rivero**
June 5, 2005

Every so often, Peter W has to drive all the crackpots out of his comment section, and clearly this thread is ripe for another such cleanup. But maybe PW should ask himself why his blog is so popular with nuts.

You are from outside academia, are you? Physics departments everywhere in the world constantly receive reports of squares of the circle and trisections of angles, so to say. With the advent of the internet, they also come via email or web; check any other forum on physics, from physicsforums to the forums of the official string-theory website. Bet the MIT librarian could name a complete collection of free books and preprints from nuts. Sci.phys… newsgroups are in the same situation. The internet is working as a publishing media, but it fails to be a research & learning media, and the nuts are always a good excuse for this failure.

53. **Juan R.**
June 5, 2005

at June 4, 2005 10:08 AM said

?Interesting article here on history of peer review by F. Tipler, including metion of Einstein’s 1905 papers,and how we came to the present system we have today. ?Peer Review. Does it ensure quality or enforce orthodoxy???

I have studied a bit the current scientific publication system, and could offer you dozens of convincing answer. Below two of them:

At least 18 of the articles next identified between the most cited of the history of science (according to the Science Citation Index) were initially rejected by editors and referees of scientific journals.

Nature rejected a Nobel class manuscript from Hideki Yukawa. The Physical Review also rejected similar manuscript in 1937

One of the referees of Physical Review found three ?errors? in Yukawa paper and recommend the reject. Yukawa won the 1949 Nobel Prize in Physics for his prediction of the existence of mesons on the basis of theoretical work on nuclear forces with that same paper.

This does not ensure quality

Thomas Larsson

Locality fails when one works in far from equilibrium situations. This is standard for anyone with a slight knowledge of that field (of course, outside the trivial applications of local QFT).

It is unnecessary the appeal to QG then.

Scott said

Personally I am not aware that biochemists were having any trouble understanding DNA structure so I see no reason why the laws of physics would need to be changed to accommodate DNA.

This contrast broadly with more recent chemical thinking.

?*Most chemists react with complete incredulity to the
view that structure is nothing but a metaphor, pointing out the
seemingly overwhelming evidence for structure that comes from spectroscopic and other structural studies. They suggest that if a deep quantum mechanical analysis reveals molecular structure to be a mathematical artifact, then the fault must lie with present-day quantum mechanics and not with the deeply entrenched chemical notion of structure*.?

I cannot post more data here because Peter would erase but the point is that QM is not sufficient for explaining mind (one even cannot explain DNA). In some restricted sense, Penrose is correct.

54.
June 5, 2005

Every so often, Peter W has to drive all the crackpots out of his comment section, and clearly this thread is ripe for another such cleanup. But maybe PW should ask himself why his blog is so popular with nuts.

In that connection I note that the name of Shamit Kachru regularly gets mentioned contemptuously here. I don’t see what SK has done to deserve such abuse. The truth, of course, is that anyone here, including me, would be delighted to have a publication list one-fifth as impressive as SK’s.

55. **Thomas Larsson**
June 5, 2005

DRL – local frame rotations generate a different group, isomorphic to gauge transformations with gauge group SO(3,1). This has nothing to do with diffeomorphisms, except that the vielbein transforms under both.

A rep is projective if it only holds up to a phase, eg. spinor reps of SO(3), which are in fact proper reps of SU(2). For infinite-dimensional groups, projective reps modify already the Lie algebra, e.g. in 1D the diffeo algebra becomes the Virasoro algebra (which is well known), and in several dimensions we get the multi-dimensional generalization thereof (which is new).

A key insight from CFT is that there are two qualitatively different types of reps; classical reps which act on primary fields (scalar densities), and quantum reps where L_0 is bounded from below. The higher-dimensional generalization of the classical reps act on things like tensor fields, connections, closed forms, etc. In fact, any concept in differential geometry can be formulated in terms of reps of the diffeo group, which is obvious once you think a little bit about it.

Another lesson from CFT is that it is the quantum reps that appear in quantum physics. The only proper (no Virasoro extension) quantum rep is the trivial one, which implies lack of locality. So if we want local general-covariant QFT, we have no choice but to consider projective reps. Considering that 't Hooft allows himself to think about hidden variables to achieve locality, I think that it is a rather big deal.

56. **Matti Pitkanen**
June 5, 2005

Kea's comment:

"I don't like the term 'consciousness' myself, but have a great sympathy for the direction of your work, as a category theorist. I prefer to talk about the Comprehension Scheme, following the work of Lawvere and others on the topos theoretic foundations of mathematics."

Thank your for an interesting and encouraging comment. My own strong conviction is that a real unified theory cannot contain logical contradictions in its basic structure, therefore consciousness. Instead of consciousness one could of course speak about the necessity of constructing theory of quantum measurement by making conscious observer part of the physical universe. Here von Neumann would probably agree.

I do not have any deep understanding about the technicalities of category theory and know only the basic concepts. It is however obvious to me that a systematic "structuralist" thinking is necessary if one really wants to construct anything resembling TOE. In M-theory this aspect seems to be absent. To me category theory seems to be tailor made to say something about consciousness ("Comprehension Scheme"). After all, the contents of consciousness remain hidden somewhat like the "real" structure of objects of category if only morphisms are known. Amazingly non-trivial things can be said about consciousness using only minimal starting assumptions consistent with quantum measurement theory. I have discussed possible applications to TGD in Category Theory, Quantum TGD, and TGD Inspired Theory of Consciousness and Equivalence of Loop Diagrams with Tree Diagrams and Cancellation of Infinities in Quantum TGD.

There were comments about locality somewhere in the thread. Space-time locality is the source of infinities in quantum field theories. If one identifies physical states of the Universe as modes of classical spinor fields in the world of classical worlds (briefly CH), situation changes. Physics is local and classical in CH but non-local at the level of 3-surface since K?hler function as "absolute minimum of Kahler action" is a non-local functional of the three-surface. As a consequence, the standard local divergences of path- and functional integral formalisms do not appear in TGD. This view about physics is almost unavoidable in "structuralist" mind set and I see the result as a demonstration about the problem solving power of this approach.

Matti Pitkanen

57. **Scott**
June 5, 2005

the Laws of Physics as we know them today are determined by the requirement that things we observe are possible.

If this is what you mean when you say you agree with Suskind's statement then yes of course. But this is not what Susskind is implying.

however you say "As a pragmatist I simply recognise that the structure of DNA appears with tortile tensor categories, and hence must play a role in the new physics"

Personally I am not aware that biochemists were having any trouble understanding DNA structure so I see no reason why the laws of physics would need to be changed to accomodate DNA.

58. **Kea**
June 4, 2005

Susskind: "the Laws of Physics as we know them today are determined by the requirement that intelligent life is possible"

The great irony in this quote is that the likes of Penrose, Matti and myself actually like the quote! Only, right out of context. My interpretation of it is completely different from anything resembling the Anthropic Principle. As a pragmatist I simply recognise that the structure of DNA appears with tortile tensor categories, and hence must play a role in the new physics. This is not to say that it is possible to say anything meaningful about the highly derived construct of intelligent life in the foreseeable future. I think that's stupid.

59. **anon**
June 4, 2005

I have a 2005-is-not-1905 experience to tell :

Our HOD copes well in the corporate culture of today. Every 6 months all PhD students have a meeting with the HOD and their supervisors. In one of my meetings it was carefully explained to me that, even if my work was as good as Maxwells no one would take the slightest notice if I did not have a PhD – so I had better put my head down and do what I was told.

60. **Kea**
June 4, 2005

Matti

I don't like the term 'consciousness' myself, but have a great sympathy for the direction of your work, as a category theorist. I prefer to talk about the Comprehension Scheme, following the work of Lawvere and others on the topos theoretic foundations of mathematics.

All the best…

61. **Kea**
June 4, 2005

I am constantly amazed by the number of physicists that seem to think an advance in QG will be made using mathematics with which they are familiar.

62. **Kea**
June 4, 2005

"Although not wishing to defend the custodians of arXiv in any way, it is hard, looking at your web pages, to see why your work is deserving of the term physics. I would see it more as a mathematical tangent that originated in physics…"

Quote: "What is a matrix?" – Heisenberg

63. **D R Lunsford**
June 4, 2005

So TL, are projective reps of the diffeo group just local bein rotations?

-drl

64. **Thomas Larsson**
June 4, 2005

Gerard t'hooft: …What seems to be missing presently, however, is a clear description of the local nature of its underlying physical laws. …

It is interesting that both 't Hooft and Unruh emphasized locality. 't Hooft himself even seems ready to dismiss quantum mechanics to obtain that goal (Planck-scale determinism). In this connection, it might be worth to recall item 9 in Baez' crackpot list:
9. 10 points for each claim that quantum mechanics is fundamentally misguided (without good evidence).
Actually, IMO the caveat applies, since locality is a very good reason for anything, so 't Hooft misses his 10 points.

He makes his point perhaps even clearer in the video from the same site, where he quotes David Gross saying "There are no local observables in quantum gravity, so forget it". Which of course is true, unless you allow for projective representations of the diffeomorphism group…

65. **Arun**
June 4, 2005

F. Tipler loses me when he suggests that Intelligent Design is a scientific theory and indulges in quote-mining Lynn Margulis against Darwinism (problems in the Darwinism do not open the door for Intelligent design). That suggests to me that he may be quote-mining all the other problems that various scientists have stated they have had in having radical new ideas accepted.

Anyway, in my opinion, let a million flowers bloom, and let there be a good search engine, and let people pick for themselves which ideas they find valuable, worth pursuing, and so on. Scientific respectability is like brand value, it is earned and maintained by providing high quality.

66. **stephen**
June 4, 2005

Thanks for link. It is important to have evidence of what t' hooft was thinking, contrary to other peoples thoughts.

Gerard t'hooft:String theory clearly appears to be strikingly coherent. What seems to be missing presently, however, is a clear description of the local nature of its underlying physical laws. In all circumstances encountered until now, it has been imperative that external fields, in- and outgoing strings and D-branes are required to obey their respective field equations, or lie on their respective mass shells. Thus, only effects due to external perturbations can be computed when these external perturbations obey equations of motion. To me, this implies that we do not understand what the independent degrees of freedom are, and there seems to be no indication that these can be identified. **String theoreticians are right in not allowing themselves to be disturbed by this drawback.** Pg. 79

67. **V**
June 4, 2005

Concerning ArXiv:
Note that in many countries researchers do not have access to journals, because they are too expensive. The ArXiv is then the only connection to the scientific world. (Well, sometimes we sent them the published version of the paper by e-mail, when asked.) I personally know one researcher who is regularly publishing in PRL and PRD but his university does not have funds to allow him to print the papers on the printer. He is thus reading papers only on the computer for years. This is what I call a real lack of funding.

68.
June 4, 2005

Interesting article here on history of peer review by F. Tipler, including metion of Einstein's 1905 papers,and how we came to the present system we have today. "Peer Review. Does it ensure quality or enforce orthodoxy?"

http://www.iscid.org/boards/ubb-get_topic-f-10-t-000059.html

69. **Chris Oakley**
June 4, 2005

?Also if you start using terms like "theory of consciousness", then AFAIC you are entering Crackpotsville (that favourite resort of disgruntled academics).?

Well, Matti can speak, study, research, etc. all that want do. There are not prohibited fields. Or perhaps there are? Past Pope recommended to Hawking do not study about Big bang and origin of universe!!

Gell-Mann is also interested in theories of consciousness. He offers talks and is collaborating with biologists, etc. Also Nobel laureate Watson (or was Crick?) did a "theory of consciousness", based in ?memory delay?.

Are Matti, Gell-Mann, Watson, neurobiologists, biochemists, quantum chemists working in anesthesia effects, etc. crackpots?

Sorry – I meant to qualify that. The connection with physics is the thing I think is tenuous.

70. **Thomas Larsson**
June 4, 2005

In my experience, mathematicians are much more open to ideas from outsiders than are theoretical physicists. Once one young string theorist independently made the same key observation that I did, namely that infinite-dimensional constraint algebras generically have quantum anomalies. Alas, he was only interested in this observation as a means to discredit LQG. When he realized that I agreed with him, he became so embarrassed that he declared that canonical quantization is invalid. Not only is this one of the weirdest claims I have even seen, but it is also completely irrelevant, since conformal anomalies also manifest themselves in the path-integral quantization of the Polyakov action.

How open-minded are people if they dismiss their own ideas just because I happen to agree?

71. **Juan R.**
June 4, 2005

mortain said

?Only if you are 'endorsed' by those already able to post on the arXiv.?

That is not true. From my personal correspondence with a physicist that know ArXiv very well

?*If administrators at ArXiv.org sent you a list of endorsers, most likely they would have been people who are strong supporters of string theory. It is probably not advisable to use the names they sent you. As an alternative you can go to the arxiv.org section where you are interested in posting and find papers by scientists which show that they do not necessarily support string theory and look on their abstract to see if they are qualified to endorse papers. If so, you could try sending to them. Perhaps you would have a better chance*.?

and he continues with

?*However, even if they endorse you, this does not guarantee that arxiv.org will allow you to post your paper, since they have a history of disobeying their own rules and have blocked people who have been legitimately endorsed (my own personal experience)*.?

Aaron Bergman said

?I'm just a guy doing fairly mathematical string theory. It's not a surprise I don't know something in phenomenology? But, it's out of my area so I can't really comment much more.?

Reading your comments on ArXiv and Swiss patent clerk. You appear to be ?out of my area? also here. Brian Josephson has studied the topic a bit (see also his letter in *Nature*). You simply are doing ?speculation? favoring your own views.

Probably you know that many administrators of ArXiv are oriented-string researchers. Perhaps would be more easy obtain endorsement and permanency on ArXiv for you 🙂

Please, write a short paper claiming that string theory is completely wrong and a waste of time and attempt again for endorsement and posting :-))

Your comments on that Einstein published his paper in usual journal may be understood in a pure 1905 perspective. He, today, would be rejected for peer-review publication, I am practically sure, I know a bit about editorial guidelines and publication policies.

?If Newton had published his theory today, it would be rejected for peer-review publication because was too ambitious one.?

From previous editor-in-chief of *Nature* journal.

D R Lunsford said

?Unaffiliated Individuals – Scientists, engineers or educators in the US and US citizens may be eligible for support, provided that the individual is not employed by, or affiliated with, an organization??

Also for outside of US? Have you read about rigidity of Spanish science administration system? Probably one of most rigid of the world!

In Spain it's not ?impossible? but more practical to play to Lotto 🙂

Chris Oakley said

?Also if you start using terms like "theory of consciousness", then AFAIC you are entering Crackpotsville (that favourite resort of disgruntled academics).?

Well, Matti can speak, study, research, etc. all that want do. There are not prohibited fields. Or perhaps there are? Past Pope recommended to Hawking do not study about Big bang and origin of universe!!

Gell-Mann is also interested in theories of consciousness. He offers talks and is collaborating with biologists, etc. Also Nobel laureate Watson (or was Crick?) did a "theory of consciousness", based in ?memory delay?.

Are Matti, Gell-Mann, Watson, neurobiologists, biochemists, quantum chemists working in anesthesia effects, etc. crackpots?

72. **Alejandro Rivero**
June 4, 2005

We are mixing two different problems: a) if/how foreigners to academy get a fair hearing. b) if/how foreigners to strings -or to the next mainstream- get a fair hearing.

73.
June 4, 2005

1. The arxiv is certainly very useful but also overrated. It is a double-edged sword. This year I needed detailed information on two topics and what I found on the arxiv in each case was pretty much utterly hopeless. Having consulted text books and various paper journals, going back a few years, I found the information I needed in detailed and substantial form. The arxiv is also now chock full of papers that have serious errors or are just superficial waffling and hand waving. I only read arxiv papers that were or are subsequently peer reviewed and published in a real journal. The Wittens and Weinbergs don't need peer review–everyone else does. Incidently, how many Landscape papers for example, would actually make it into Nucl. Phys. B or Phys. Rev. D? I read paper journals and will continue to do so. I would like to see a totally blind and unbiased system of peer review whereby there are no names or affiliations and the reviewers see only the physics content of "a manuscript".

2. I published 4-5 papers in the late 90s, and in my spare time, that had no academic affiliation.
(My stuff is more mathematical physics and math though.) Having a Phd certainly does help however. If your work is good enough or has any substance and it professionally presented, it at least gets a fair hearing. Editors can tell right away whether a manuscript has merit and should be considered or is just crackpot, and whether the author has a formal professional background in the subject. A lot of trained mathematicians and physicists work at companies and their own businesses. They still have a right to publish any ideas they still might have. I've seen published physics papers where the affiliation was a financial institution so maybe the next 'Albert' (male or female) is working at a bank rather than a patent office. My concern with the arxiv and physics publishing in general is that censorship and political motivations replaces fair and unbiased peer review.

74. **Matti Pitkanen**
June 4, 2005

Chris,

your earlier postings have not shown even a slightest indication that you have understood the idea of scientific argument. You produce just arrogant rhetorics typical for people who have never thought a single original thought and for some reason call themselves skeptics. You can perhaps scare some first year student but cannot cheat professionals.

Since you have obviously not yet learned what is the point of scientific discussion, I am willing to give some helpful advice. First ask yourself: do I have really something interesting to say? If the answer is yes, formulate clearly what your claim is. But do not stop here! You must also carefully develop arguments that you believe to justify your claim.

It is never too late,

Matti Pitkanen

75. **Scott**
June 4, 2005

I'm going to have to agree with Aaron, if your paper has any merit if and you are determined enough you can find a proffesor who will give you a chance whether this be your old advisor or anyone else. I would suggest looking outside of string theory for this professor though.

76. **Alejandro Rivero**
June 4, 2005

The funding problem is not about affiliated vs unaffiliated, it is about mainstream vs exploratory research. As I said before (upstream in the thread), even having tenure does not imply funds, at least in the European universities I know. A professor only has a limited fund for education, and zero euros for research. So he must ascribe to a research group, and the research group must send a proposal for funding to different institutions. Individual proposals have not encouraged (ie, they are unlikely to be accepted by the granting institution). And of course a group proposal outside mainstream is also unlikely to be accepted, as it is a huge assignation. Nice catch-22 here.

The way out is that a research group "looks towards other place" when one of their members do exploratory research. This ability to look to other place depends of a lot of factors from seniority of the researcher to his willingness to support the mainstream papers too, and of course of the risk for the prestige of the group (remember that a failure in prestige can imply loss of funding for all the group).

Low profile, not risky, research, is usually supported at the level of local resources even for not affiliated people keeping relationship with the field (as Aaron describes correctly). It is a bit more problematic to support travel and meetings of unaffiliated, but it can be arranged occasionally from minor allowances in the group funding. But at the end there are a group responsible for the funding, remember, and they have a limit in the risk they can take.

Now, beyond travels, chalk and blackboards, imagine the nighmare in *experimental* physics. I know of a (actual, I am not referring to Cavendish) young tenured professor leaving the campus because he foresaw that it was impossible to get funding for a different experiment; one needs to convince all the collaboration to resign from the current research line.

77. **Chris Oakley**
June 4, 2005

Matti,

Although not wishing to defend the custodians of arXiv in any way, it is hard, looking at your web pages, to see why your work is deserving of the term "physics". I would see it more as a "mathematical tangent that originated in physics", a classification that also applies to String theory, LQG, Twistor theory and dozens more ideas that have not, and may never lead to testable predictions. Also if you start using terms like "theory of consciousness", then AFAIC you are entering Crackpotsville (that favourite resort of disgruntled academics).

78. **Matti Pitkanen**
June 4, 2005

I have now worked almost 27 years with Topological GeometroDynamics. I have written four massive online books about TGD giving a detailed documentation of the theory and its applications(about 5000 pages). American Mathematical Society has in its subject classification table a link to my homepage. Penrose refers to my work relating to p-adic physics in US edition of his newest book.

Despite this it has been impossible to get material about TGD to arXiv.org during the last decade. Because TGD challenges the basic materialistic and reductionistic dogmas and is also a quantum theory of consciousness, I cannot expect invitations to physics conferences and the idea about getting some financiation is totally outlandish. I feel disgusting even the thought of forcing people in difficult position by begging for endorsement to arXiv.org. In practice this of course means professional death since no one takes seriously a person who cannot get eprints to arXiv.org.

This situation inspires a small thought experiment. Suppose for a moment that TGD indeed provides the bottle neck ideas necessary to unify the basic interactions. The physical interpretation of TGD was more or less established around 1982, 2 years before the first super string revolution, and collective effort to develop these ideas could have been launched then. On basis of this one can make rough estimates about the financial and human resources wasted during these 20 years as an outcome of my marginalization in physics community in the case that I am right. I am certainly not the only victim of censorhip and one of us might be right. Arrogance costs.

Matti Pitkanen

79. **D R Lunsford**
June 4, 2005

Aaron – getting funding is a challenge, regardless of publishing, if you're not affliliated:

Unaffiliated Individuals – Scientists, engineers or educators in the US and US citizens may be eligible for support, provided that the individual is not employed by, or affiliated with, an organization, and:

** the proposed project is sufficiently meritorious and otherwise complies with the conditions of any applicable proposal-generating document;*
** the proposer has demonstrated the capability and has access to any necessary facilities to carry out the project; and*
** the proposer agrees to fiscal arrangements that, in the opinion of the NSF Division of Grants & Agreements, ensure responsible management of Federal funds. Unaffiliated individuals should contact the appropriate program before preparing a proposal for submission.*

But, it's not impossible.

If you have something worth saying it will worm its way to the surface.

-drl

80. **Aaron Bergman**
June 4, 2005

Aaron, you mean your PhD "thesis adviser", right?
Suppose you are a patent clerk (or any other occupation) in 2005, with no physics PhD, and a BSc or MSc in physics. Do you think your undergraduate tutor or mentor (or anyone else) would endorse you to allow you to post three preprints on the arXiv? You could submit your work to journals, but in this century, how likely are the editors and referees to take your research seriously, when you have no academic affiliation?

If they were good, yeah. Good meaning more than just 'good physics'. They also need to be clear and well-written. If I were to try something like this, here is what I would do. I would pick a junior faculty member and send a polite e-mail noting your background, the problem you think you have solved and the techniques that figure in the solution. Note that you have a clear, publication quality exposition of the idea with lots of formulae and calculations. Say that you're not sure that what you've done is right and that you'd appreciate any advice they could offer. Some people will ignore you, but if you have a short, clear exposition, I'll bet someone will look at it. It might even end up being a graduate student. If they tell you it's wrong, let it go. If they tell you they don't understand it, ask how you can clean up the exposition. If they don't want to say anything else, let it go. Be polite and be willing to accept that you might just be wrong.

Needless to say, this is not how most things of this sort go. The vast majority of people writing from outside of academia have things that are either completely incoherent or you can tell that they are completely nutty in about three lines. And very, very few of them are polite. Remember, capital letters, boldface and exclamation marks are *not* your friends.

81. **mortain**
June 4, 2005

Aaron, you mean your PhD "thesis adviser", right?
Suppose you are a patent clerk (or any other occupation) in 2005, with no physics PhD, and a BSc or MSc in physics. Do you think your undergraduate tutor or mentor (or anyone else) would endorse you to allow you to post three preprints on the arXiv? You could submit your work to journals, but in this century, how likely are the editors and referees to take your research seriously, when you have no academic affiliation?

Quantoken: nice comment. It chimes with my own opinion on string theorist TV appearances. Walking on the street is a recurring theme in string theorist/theory documentaries, I believe. I recall Brian Greene sitting in a cafe and wandering around New York in his 'Theory of Everything' (U.K. title) series. That same series did repeat several sequences of CG imagery ad nauseum, especially those squiggly blobs on a grid. At least we weren't treated to a middle-aged man taking a dip!

Then there was that old BBC 'Horizon' documentary which showed Witten walking around a rocky landscape and gazing at the sky. Beautiful scenery, but surely not representative of the common working environment for theoretical physicists, and now perhaps illustrative of the quest of string theorists as they confront a barren Landscape.

82. **Aaron Bergman**
June 4, 2005

Do you really believe this? Wow. Do you mind if I ask you a personal question? How many years of your postgraduate life have you spent working outside academia?

Yes. I believe this. You know why? Because Einstein got his doctorate while outside of academia. He submitted his thesis in 1905, while working as a patent clerk since 1901. Einstein's papers were submitted to journals during that time, too and were accepted.

You know what else? There are people outside of academia publishing papers on the ArXiv (although the name of the person I'm thinking of escapes me right now.)

If you develop a good relationship with your thesis adviser and you write a good paper, you'll get endorsed. That's probably the easiest way to go. Really, being out of academia has nothing to do with anything. Not being a kook is the key.

83. **Kea**
June 3, 2005

Aaron: "This swiss patent clerk would have gotten endorsed by his old adviser and posted to his heart's content."

Do you really believe this? Wow. Do you mind if I ask you a personal question? How many years of your postgraduate life have you spent working outside academia?

84. **Quantoken**
June 3, 2005

Berry said:
""Even after 20 and more years, my feeling is that we do not understand string theory but that there is every indication that there is something there to understand."

This is the key point, isn't it? Is 20 years a long time or a short time? I would say that it is extremely short."

The first paragraph is a nonsense. Of course if you don't understand something then there is something to be figured out and understood. If you understand it all then there is nothing remaining to be figured out. But that says nothing about the thing you don't understand or whether it is relevant at all to the nature.

It is OK with me that some string theoretists feel they need a couple more 20 years to figure things out. But looks to me many of them have changed their professions to scifi novelists. These days you can not turn on the TV without seeing fancy stringy terminologies flying around, like landscape, bubbles, parallel universe etc etc.

What is that grey-haired Japanese guy who always shows up on TV, talking vividly, with mistery written all over his face? I see him more often than Dubya these days. The other night I was watching the Science Channel. Half of the night was filled with a series string theory programs, from "parallel universe" to "bubble universe" and "unfolding universe" and to "extra dimentions".

Can you believe it? They can talk the whole hours and show tons of colorful amazing computer animations, but at the end of day **they have NOT talked about a single thing that actually had anything to do with nature.** Not a single thing in string theory has been proven to be relevant to the nature in any way shape or form. Whole hours of TV program was filled with scenes of these famous guy writting something on the blackboard or on a piece of paper, typing on a computer keyboard, sitting, standing, sleeping, walking on the street, playing tennis, and swimming. There was a closeup of one of these guys swimming half naked, taken by an underwater camera.

What does it all has anything to do with science? It's ourageous they could propagate to the public in such a misleading way when they really have absolutely nothing to talk about about their research results after 20 year.

Reminds me of politicians who could talk for half an hour none-stop, and when you pick it apart you find that actually not a single thing has been said!

Quantoken

85. **Scott**
June 3, 2005

Peter,
You had me going there for a second. I had to go back to find other posts of Barry's in one of your other blogs, in order to figure out if you were joking or if you were actually complimenting him on a joke.

86. **Peter Woit**
June 3, 2005

Hi Barry,

Thanks for the parody of what string theorists are thinking these days. It was very funny.

87. **Alejandro Rivero**
June 3, 2005

Perhaps there are several "Einteins" in the world, but since they cannot post in ArXiv

ArXiV (and journal editors, generically) is not the problem, it just reflects the community (and note I have been myself censured/blocked some times in the past, sometimes due to content,
sometimes to -editor's opinion- "spamming"). Do you remember these poor guys in each macroconference trying to attract people into their poster? People knows that, beyond the content, the probability of immediate use of any of these posters is very low, and they do not care.

This guy, rejected the invitation because would pay the travel, etc. from his own money.

A related problem is that in most countries even **tenured** teachers do not have direct allowance for research expenses, and they need to get this money not from the university they are, but from external sources.

Of course a single travel is a minor issue, and sometimes I have been lodged in spare rooms of the conference lot, or in family houses. But as years go by, it becomes a real nuissance. So for the dreams of independent research at tenure.

88. **Alejandro Rivero**
June 3, 2005

I'm just a guy doing fairly mathematical string theory. It's not a surprise I don't know something in phenomenology

I acknowledge I was abusing a bit of empirical data by generalising from one guy to the whole community.

Still, it is a pity how divided we have come. Part of the problem is that almost nobody in hep-th (or math-ph) can remember why such or such topic is worthy of study.

Myself I was just a guy studying non commutative geometry and then two years ago some accident launched me to read phenomenologist papers. In the world of hep-ph, closer to experiment, it becomes more evident that in the route to success is not enough "calculation + publication + prediction"; you really need to fit in the mainstream. Following the previous example, another semiempirical formulae as the last year Minakata-Smirnov, which is neutrino related, have got in a short time a higher level of awareness -measured as citations from different authors and countries- that Koide's, which is for charged leptons.

89. **Quantoken**
June 3, 2005

Some one said:
"The arXiv is purportedly "a means for specific communities of scientists to exchange information." **One hundred years after 1905**, a Swiss patent clerk without a PhD in physics would surely have been locked out of the arXiv.

This swiss patent clerk would have gotten endorsed by his old adviser and posted to his heart's content."

I say **absolutely NO** and there is absolutely not a single piece of evidence for it. Notice we are now talking about **A swiss patent clerk in the year 2005**, whose name is most probably not called Einstein, **NOT the 1905 guy** called Einstein.

Can you name just ONE recent ARXIV paper which is authored by an actual real swiss patent clerk with no fame, with no Ph.D. and no academy institution association? Can any one name even just one such paper?

Not a single one I would say!

There is no scientific evidence to suggest that any swiss patent clerks can publish on today's ARXIV, and plenty of evidences to suggest otherwise.

Rememeber Einstein was NOT a famous swiss patent clerk in 1905, he was an **average and no-name patent clerk** back then, and needed to tutor high school students to supplement family income.

Quantoken

90. **Aaron Bergman**
June 3, 2005

Well, I have already made have of my point 🙂 *if nobody knows what papers are.*

I'm just a guy doing fairly mathematical string theory. It's not a surprise I don't know something in phenomenology. I notice that the PRD paper has 104 cites, though, which makes it hard for me to believe that nobody knows what it is. But, it's out of my area so I can't really comment much more.

The arXiv is purportedly "a means for specific communities of scientists to exchange information." One hundred years after 1905, a Swiss patent clerk without a PhD in physics would surely have been locked out of the arXiv.

This swiss patent clerk would have gotten endorsed by his old adviser and posted to his heart's content.

91. **Kostya**
June 3, 2005

For anyone who is interested here is a link to t'Hooft's lectures which he used in his string theory class.
http://www.phys.uu.nl/~thooft/lectures/string.html
It's a very nice introduction with many detailed computations that are usually skipped in most textbooks.

92. **mortain**
June 3, 2005

Peter, hasn't the length of this thread of comments set a record for comments on your blog? I don't recall seeing the number of comments on any of your other blog posts reaching 100. It seems notable and even remarkable.

Juan R., I too thought that the arXiv's 'endorsement system' is a terrible development. Why did they have to change their ways after a decade or so without such a system? They even had more funding directed to them in recent years! "The growth in number of submissions to arXiv necessitates an automated endorsement system" – well, why not use the additional funding to create more personnel positions and not shut out people who take a courageous step into the domain of academic physics? I think their reasons for the endorsement system are utter crap. After all, I don't recall the arXiv being swamped with crank 'preprints', and surely such 'preprints' only ever constituted a small fraction of the total content of the arXiv since its inception.

Yet they claim that "we can continue to offer free and open Web access to all." Yeah, right. Only if you are 'endorsed' by those already able to post on the arXiv. Maybe it has naturally become a mirror of the off-line world of selectivity on the 'it's who you know' basis in secular physics departments. Or a sad reflection on the state of the modern physics community.

The arXiv is purportedly "a means for specific communities of scientists to exchange information." One hundred years after 1905, a Swiss patent clerk without a PhD in physics would surely have been locked out of the arXiv.

93. **Juan R.**
June 3, 2005

Someone said

"Good luck being the next Einstein. If you're good enough to revolutionize the field while working outside of it, then you won't need any of my advice. But, for those of us who aren't as smart as Einstein, there are better paths to take."

Simply stupid and arrogant!!

"And, in the time since Einstein, can you name one physicist who wasn't at a research institution that made a revolutionary contribution?"

Perhaps there are several "Eınteins" in the world, but since they cannot post in ArXiv, as emphasized this year

Covert censorship by the physics preprint archive

by Brian Josephson (Nobel laureate for physics) (has you read his February open letter to community?

nor pay expensive print requirements from their own money for publish in a JCP, etc.

Curiously I know to one guy from outside of official academy that did a paper on nanothermodynamics, and was formally invited by a organizer (who read him) of an international conference on the topic.

This guy, rejected the invitation because would pay the travel, etc. from his own money. No because he could not write a conference. In fact, that guy gave a conference (in marine sciences) being an undergraduate student because its previous institution payed it.

94. **Alejandro**
June 3, 2005

Sorry the duplicate. Peter has put a "blocking system" that confused me; I was attending to a student (ah, exams) and at the same time pressing the "Post" button

🙁

95. **Alejandro Rivero**
June 3, 2005

There are 245 papers under 'Koide' on SPIRES. You're going to have to be a bit more specific.

Well, I have already made have of my point 🙂 if nobody knows what papers are. But the two more recent show the formula and references, so one can start from the references, for instance, of hep-ph/0505028

The earlier references online are Phys.Lett.B120:161,1983 and Phys.Rev.D28:252,1983

96. **Alejandro Rivero**
June 3, 2005

There are 245 papers under 'Koide' on SPIRES. You're going to have to be a bit more specific.

Well, I have already made have of my point 🙂 if nobody knows what papers are. But the two more recent show the formula and references, so one can start from the references, for instance, of hep-ph/0505028

The earlier references online are Phys.Lett.B120:161,1983 and Phys.Rev.D28:252,1983

97. **M**
June 3, 2005

I think it is worth echoing some of Aaron's words:

"So, if you want to have fun speculating occasionally, that's cool, but in the meantime you should also publish results that people can point at when they want to hire you. If you want to put in the time and energy that generating a new theory of quantum gravity will probably entail, it's probably a good idea to wait… The point being is that, before you have tenure, and to a lesser extent even afterwards, you have to produce. I'm sorry you find this offensive, but it's just the way things are."

Those are hard words, but in my opinion probably on the mark. I am somewhat of an idealist myself; if it isn't possible to look beyond the horizon, explore some ideas outside the mainstream and feel a passion about gaining understanding and (hopefully) making a real contribution to physics, then physics becomes "a job" and loses much of its appeal. As a grad student myself, I am sympathetic to Scott's and others' point of view. But, as Aaron indicates, you still have to live in a results-oriented society…

As in business, if you aren't producing you are a lot less likely to remain employed, and if you aren't succeeding at some level others are likely to ignore you and your ideas. If a research institution is looking to hire someone or a funding agency is looking at grant proposals, they will want to feel reassured that the people they choose will actually produce something. That is human; who wouldn't think that way? If you had two acquaintances who wanted to borrow money from you, would you rather lend to the one who had a track record of being conscientious and trustworthy or the one whom you knew little or nothing about in terms of their trustworthiness? Institutions and funding agencies have plenty of choices; why not go with the "safest" people? Idealism is good for your (and my) own motivation, but other people would rather take a risk on someone with a track record.

It's a harsh world out there for the seeker of knowledge and understanding for its own sake…

98. **Scott**
June 3, 2005

Barry,

Honestly if we aren't intelegent enough to realize that not all landscapers are adament about the antropic principle or to think that string theorists in general agree with peter that the game is over why would you care what we think as that would obviously make us retarded. Obviously the people doing research in string theory have different oppinions on it then peter.

20 years not long? its only one less then i've been alive. 20 years and still no testable predictions? Maybe you should look back on 20th century physics and see how much things progressed(by which I of course mean more and more predictions being made and verified more experimental results being explained) every 20 years.

"This is what an ambitious student should be aiming to do"

Isn't that what you guys have been trying to do for the last 20 years? Why should all ambitious students tie themselve down to one theory that has gone nowhere(aka made no verified predictions) in 20 years?

99. **Barry O'Genesis**
June 3, 2005

I'm very impressed by Aaron's original contribution to this thread and I would like to underline some of his points, especially for the benefit of any students or prospective students who might be reading this.

Aaron said:

"So, hopefully without sounding horribly condescending, I just want to point out that Peter's blog does not present a good idea about how things are."

This is absolutely correct. I do not see any signs of despair among the string theorists I know, including the landscapers. Maybe they *should* be despairing, but they aren't. So people reading this blog should not come away believing that the "game is over" as a result of Kachru's latest.

"Flux stabilization seems to have, much to the disappointment of many, led to an overabundance of vacua. This has led to a number of different proposals on how to deal with this, including anthropic arguments."

Actually very few landscapers are really convinced anthropoids. The anthropoids are just better at writing popular science books. Also note that not all landscapers are convinced that the landscape is really all that large.

"It is not correct, however, to say that the field is consumed with this. The number of papers that deal with anthropic arguments do not even approach one half or one quarter. Most of us are perfectly happy to go on with our own projects and leave the anthropic stuff to the occasional dinner table argument."

This is true, but by the way it is not so clear that it is such a good thing. I would much rather see more people working on the landscape than on chern-simons theory, for instance….

"Even after 20 and more years, my feeling is that we do not understand string theory but that there is every indication that there is something there to understand."

This is the key point, isn't it? Is 20 years a long time or a short time? I would say that it is extremely short.

"Some argue, then, that there should be research alternatives. There are two answers to that. In terms of quantum gravity, there just aren't that many games out there, and there's not much market for the sort of speculation that could lead to a new direction — that sort of thing, much like the interpretation of quantum mechanics, should be left to the tenured."

Another key point. Remember: *every* crackpot out there regards himself as an unjustly neglected iconoclast. By the time you get tenure, or indeed long before that, you should have grown up enough to realize that if you can make some tiny but real contribution to the mainstream you will be doing a lot better than most people.

"But, it's also possible that, as we understand the theory better and better, maybe some of those vacua fall away, or maybe we find universal predictions of the theory that apply for all physically realistic vacua. We'll never know if we don't try."

Exactly. This is what an ambitious student should be aiming to do, not to find a job as a patent clerk who will totally revolutionize physics by means of a simple application of the theory of left pseudo-heaps.

100. **D R Lunsford**
June 3, 2005

Back to the topic sort of,

This is a very good physicky lecture on Clifford algebraicana:

http://arxiv.org/abs/hep-th/0506011

-drl

101. **Aaron Bergman**
June 3, 2005

There are 245 papers under 'Koide' on SPIRES. You're going to have to be a bit more specific.

102. **D R Lunsford**
June 3, 2005

People will pay attention if you can surprise them with something they already know.

-drl

103. **Alejandro Rivero**
June 3, 2005

If you can calculate something people will listen

Not "something" but "almost everything". Look at Koide's work. Published in Phys Rev D and other journals, including a prediction (mass of tau) that was verified exactly (not within one or two sigmas, but just in the gaussian peak) ten years after the publication. But the original interpretation, in the context of preons, needs of a lot more of calculations to jump from "model" to "theory" and, not being mainstream, nobody is going to waste time on it. Nor to try another explanation.

You only get attention if you can calculate AND your calculations are reusable in current work of other people.

104. **D R Lunsford**
June 3, 2005

Ok thanks mike, if you wouldn't mind, could you drop an email telling me what was claimed and what is known?

-drl

105.
June 2, 2005

…and don't forget the free BBQs and afternoon teas.

106.
June 2, 2005

>>Aaah! But that's where you're wrong. I know of a >>number of smart people that have been doing PhDs >>for well over 10 years.

>And getting paid?

One can get by on remarkably little as a student. One can always earn a bit of money tutoring, waitressing or whatever.

107. **Aaron Bergman**
June 2, 2005

Aaah! But that's where you're wrong. I know of a number of smart people that have been doing PhDs for well over 10 years.

And getting paid?

So long as they keep coming up with good ideas that other academics can get credit for, they won't be kicked out.

The "good ideas" being very important there.

108.
June 2, 2005

Aaron: "If he had done that graduate school, there's a good chance they'd have thrown him out if he didn't have some good intermediate results."

Aaah! But that's where you're wrong. I know of a number of smart people that have been doing PhDs for well over 10 years. So long as they keep coming up with good ideas that other academics can get credit for, they won't be kicked out.

109. **Scott**
June 2, 2005

…like the development of relativity instead or…

110. **Scott**
June 2, 2005

well, ok I think I understand your position a little better now. Personally I am not going to worry that much about towing the line after I get a PHD because then I will be able to if nothing else at least publish on arxiv even without a job.

personally,I think that quantum gravity in the end will end up being not as hard as we think, just that the basic ideas that leads to it just hasn't been thought up and explored yet. Also I think it is because people are trying to solve the problem all at once like the development instead of by increments like the developments of quantum mechanics.

111. **Aaron Bergman**
June 2, 2005

so while you think non-tenured can i guess get away with writing some speculative papers(this being a strange definition of speculative that doesn't include stringtheory papers) you think they shouldn't do so.

Speculative in this case meaning outside the mainstream. And, generally, yes, young researchers won't get very far spending their entire time outside the mainstream. The reason why is not that this is somehow transgressive, but that generally you won't accomplish anything. It's easy to speculate vaguely, but it's very, very, very hard to get concrete results, ie, do calculations. Nobody wants to hear your ideas on what quantum gravity should be — everybody has them. If you can calculate something people will listen, but there's a reason that quantum gravity has remained unsolved for fifty years: it ain't easy.

So, if you want to have fun speculating occasionally, that's cool, but in the meantime you should also publish results that people can point at when they want to hire you. If you want to put in the time and energy that generating a new theory of quantum gravity will probably entail, it's probably a good idea to wait. Wiles, once he had tenure, was able to put aside seven years to work out Taniyama-Shimura. If he had done that graduate school, there's a good chance they'd have thrown him out if he didn't have some good intermediate results.

The point being is that, before you have tenure, and to a lesser extent even afterwards, you have to produce. I'm sorry you find this offensive, but it's just the way things are.

112. **Scott**
June 2, 2005

oops that last sentence should have ended in a question mark.

113. **Scott**
June 2, 2005

I didn't say I would be the next einstein my point was the non tenured often are the ones with the revolutionary ideas, I simply took the most extreme example of this that i could think of. Just a reminder of your origninal statement.

"there's not much market for the sort of speculation that could lead to a new direction — that sort of thing, much like the interpretation of quantum mechanics, should be left to the tenured."

so while you think non-tenured can i guess get away with writing some speculative papers(this being a strange definition of speculative that doesn't include stringtheory papers) you think they shouldn't do so.

114. **Aaron Bergman**
June 2, 2005

Good luck being the next Einstein. If you're good enough to revolutionize the field while working outside of it, then you won't need any of my advice. But, for those of us who aren't as smart as Einstein, there are better paths to take.

And, in the time since Einstein, can you name one physicist who wasn't at a research institution that made a revolutionary contribution?

Just as another random note, many people seem to have missed the part where I said that one can intersperse the more speculative papers with the mainstream stuff.

115. **Scott**
June 2, 2005

Aaron how about my example(though I didn't state it explicitly and just listed patent clerk as a job where you could still work on physics ideas at the same time) of Einstein who did not even work at a school at all let alone have tenure when he published his 1905 papers.

116.
June 2, 2005

er… "not"="now" in previous posting. I must use the preview button…

117. **Alejandro Rivero**
June 2, 2005

"When I was younger I had ideas but no skills, not I have skills but ideas are scarce". Heard to a tenured teacher time ago. Consider also that the tenure track is very long, some people getting it when they are near 40 years old.

118. **Aaron Bergman**
June 2, 2005

I will leave it to the readers of this blog to decide for themselves to what extent my brief summary is accurate or useful in the context of my comment to this blog, the point of which is:
If 't Hooft had waited for tenure to pursue an approach that his adviser Veltman "could not believe", then the acceptance of the electroweak model would have at best been delayed substantially.

You miss the point. 't Hooft was working in a well established direction. He was working on theories that had been around for years (if in somewhat disrepute for a period). This is exactly the opposite of what I said someone should wait for tenure to work on. Your problem seems to be that Veltman didn't believe that 't Hooft could do it. Fine, but I never said people shouldn't work on things because their advisers don't believe in them (or, more likely in this case, are threatened by them.) Your example is simply inapposite.

(And his current ideas on quantum mechanics are not Bohmian mechanics.)

119.
June 2, 2005

On that 6-sphere paper: While I'm definitely very far from being an expert on the subject, I heard the author (who is, by the way, a physicist) talking about it a month ago, and it seemed to be seriously flawed to me then (even without understanding the details). Of course this means just about nothing, but that was my impression.

120. **mike**
June 2, 2005

The 6-sphere paper was at math.DG/0505634, but has since been withdrawn.

121. **D R Lunsford**
June 2, 2005

Someone mentioned complex structure on 6-sphere – do you have a direct reference?

-drl

122. **D R Lunsford**
June 2, 2005

PS – kids probably still say "Thank God I'm done with density matrices!"

-drl

123. **D R Lunsford**
June 2, 2005

Peter said "I don't think smart people will go into physics any more…"

Well difficulties didn't stop **you**! You have no faith in these youngsters? It's the same stuff to figure out after all.

-drl

124. **Tony Smith**
June 2, 2005

Aaron seems to disagree with my brief summary of 't Hooft's experience with renormalization of the electroweak model. My summary was primarily based on the following quotes from The Second Creation, by Crease and Mann (Macmillan 1986):
"… Early in 1971, Veltman had a conversation with 't Hooft that he has never forgotten, the interchange went as follows:
M.V.: I do not care what and how, but what we must have is at least one renormalizable theory with massive charged vector bosons, and whether that looks like Nature is of no concern, those are details that will be fixed later by some model freak …
G.'t H.: I can do that.
M.V.: What do you say?
G.'t H.: I can do that.
"And this he could not believe," 't Hooft said years later …
…
To Steven Weinberg, 't Hooft's proof just seemed like hand-waving. Then he heard that his friend … Benjamin Lee … was working on it … Besides lending 't Hooft's work his considerable prestige, Lee spent most of August translating it into a form other theorists could comprehend. "I was really impressed with that," Weinberg recalled …".

I will leave it to the readers of this blog to decide for themselves to what extent my brief summary is accurate or useful in the context of my comment to this blog, the point of which is:
If 't Hooft had waited for tenure to pursue an approach that his adviser Veltman "could not believe", then the acceptance of the electroweak model would have at best been delayed substantially.

In other words, I feel that Aaron's "wait for tenure" prescription is bad for physics, although it may be a good strategy for achieving Aaron's stated primary goal: to get a job.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

125. **dan**
June 1, 2005

is it true that LQG has a good semiclassical limit, a large volume limit with a positive cosmological constant with the kodama state?

126. **Peter**
June 1, 2005

David,

I'm not convinced I'm the one that's confused about the Stony Brook workshop. It's entitled "Geometry of String Vacua", and the scientific description, in toto, reads "A deeper understanding of the geometry of string compacitification has become increasingly important in connecting string theory to the real world, in the context of early universe cosmology as well as collider physics" (by the way, the idea that studying these compactifications has anything to do with collider physics is pretty funny). Among the speakers listed are Denef, Douglas, Kachru, Kashani-Poor and Silverstein, all of whom work on flux compactifications. More of the speakers work in this area than work on topological strings. If this is a workshop mainly devoted to topological strings, why does it need to be advertised as one devoted to physical string backgrounds?

127.
June 1, 2005

Is this the same t'Hooft who had made some, uh, jokes about his former advisor much in the same way that Veltman would have done so about himself?

128. **stephen**
June 1, 2005

Drl,

[No 'Quantum Computer' will ever be able to out perform a 'scaled up classical computer.'](#) *by Gerard t'Hooft*

Yes. I tried to present this point before.

I think he is asking us to think differently for a reason?

129. **D R Lunsford**
June 1, 2005

Wow, t'Hooft uses Bohmian mechanics to gainsay quantum computing. Nice!

-drl

130. **stephen**
June 1, 2005

[Perhaps Quantum Gravity can be Handled by thoroughly reconsidering Quantum Mechanics itself?]Gerard t' Hooft

131. **david**
June 1, 2005

"The problem with landscape studies is….there are an ever increasing number of people studying the Kachru et al Rube Goldberg machines. Stony Brook's yearly workshop on mathematical physics will be devoted to this this summer."

No it won't. It will be devoted, as in the previous two years, to topological string theory. The phrase in the conference description that's confusing you refers to vafa's recent work in this direction.

132. **Juan R.**
June 1, 2005

Aaron said,

No. There just isn't. Go work on inflation. Go work on electroweak symmetry breaking. Go work on dark matter. Go work on large extra dimensions. Go work on baryogenesis. Go work on susy breaking. Nobody, and I mean nobody, is forcing anyone to go into string theory. I don't know where you get this idea that there is only one 'approved topic'. It's just not true.

I am not in the game directly, but heard people like Glasgow, Phil, or Peter claiming the contrary.

I read to Vafa claiming that ST may be present in **any** important physics departament. I heard to physicists claiming by brutal presure for following the last string fad in their laboratories.

I read a string theorist claiming that many string researchers do not believe in it!!

I read to Witten, B. Greene… Greene claims that ST is the only game in the city. An undergraduate student contacted with me saiyng exactly that, there is two options: ST or nothing.

If he is not informed probably will chose an string career.

Several people contacted with me and explained that leaved physics because either one studied ST or one leaved the field. That was said to me by a PhD on string theory now working outside of physics.

That and your posts on LQG, indicate that you may live in a different world. Alice?

133. **Aaron Bergman**
June 1, 2005

The problem with landscape studies is not just the loony-tunes people in high places who have abandoned doing science to promote anthropism. There are an ever increasing number of people studying the Kachru et al Rube Goldberg machines.

All I can do is point to the papers on hep-th. I just skimmed through the 'recent' search and counted a grand total of one paper on the landscape. A search on 'landscape' in the title or abstract gives 31 papers in the past year. 'Flux vacua' gives 37. Hardly a deluge.

The problem with the idea that young physicists should just keep their head down, and work on approved topics until they get tenure is that right now, in fundamental particle theory, there is only one approved topic.

No. There just isn't. Go work on inflation. Go work on electroweak symmetry breaking. Go work on dark matter. Go work on large extra dimensions. Go work on baryogenesis. Go work on susy breaking. Nobody, and I mean nobody, is forcing anyone to go into string theory. I don't know where you get this idea that there is only one 'approved topic'. It's just not true.

I think I've said this three times now. If you don't believe me, go to the [Rumor Mill] and click on the names of people offered jobs to see what they have worked on. It's all there in black and white (and some blue, too).

134. **D R Lunsford**
June 1, 2005

I think kids should spend a lot of time haunting the journal stacks in the local library – oh wait the Internet replaced all that – my bad.

-drl

135. **Peter**
June 1, 2005

My, my, I go away for the day and look what happens here.

This week I'm commuting down to a conference at Rutgers, and today gave a talk there on loop groups and QFT. After the conference is over I'll probably write something here about it.

Some comments on the comments:

I saw the preprint claiming to find a complex structure on the six-sphere. When I get a chance I'll check with an expert to see if this seems like it might be real.

Mortain:

I thought about mentioning Lax's Abel prize, but I don't know Lax, or really anything about his work, so skipped it since I didn't feel I had anything really to say about that.

I've been aware of the "not even wrong" phrase for longer than I can remember, I didn't get it from Veltman. As I sometimes point out, I don't necessarily have anything against speculative ideas that are "not even wrong", many good ideas start out this way. I'm glad you enjoy the non-string theory posts, some of which are about things that are not even wrong, some about things that are right. Actually, string theory is slowly moving out of the "not even wrong" category to the "wrong" category.

I'll stay away from fights about LQG, and just point out that if the number of people doing string theory now was the same as the number doing LQG, I wouldn't have a problem with string theory. And the idea that LQG hype is on anything like the scale of string theory hype is pretty laughable.

A few comments about what Aaron had to say (and I'm glad he's taking the time to give his point of view here):

While we agree string theory is in the doldrums, I don't think the nineties was a "fruitful" period for particle theory. Pick any decade during the past century and I think you can come up with many more important advances than what happened in the nineties. The nineties were the doldrums, what is happening now is much worse.

The problem with landscape studies is not just the loony-tunes people in high places who have abandoned doing science to promote anthropism. There are an ever increasing number of people studying the Kachru et al Rube Goldberg machines. Stony Brook's yearly workshop on mathematical physics will be devoted to this this summer. The infinite landscape provides an infinite number of relatively easy research problems that people can work on, happily thinking that they're on the cutting edge of research. While there are lots of easy problems to work on, none of them have the most remote chance of having anything to do with physics. Sometimes you don't need to do calculations like this to know that the result can't be interesting.

The problem with the idea that young physicists should just keep their head down, and work on approved topics until they get tenure is that right now, in fundamental particle theory, there is only one approved topic. And it's a topic that sensible young people quickly see is highly unappealing and won't ever lead anywhere. So, the danger is that, rather than spend ten years working on what they can tell is a bad idea, smart people just aren't going to go into particle theory anymore. I'm afraid I already see this happening.

136. **Kea**
May 31, 2005

"Work hard on some well-defined problems, write good papers, get your PhD's and only later, when you get tenure should you think about these big problems"

Some of us suspect that t'Hooft also knows perfectly well that anyone who loves Physics enough to think for themselves will probably ignore this advice.

137. **Kea - anonymous**
May 31, 2005

Aaron: "What I'll say is that's just how the world is: you need to get a job."

Hah! Not in my world. What a load of arrogant bullshit you sprout.

138. **jeff**
May 31, 2005

"If 't Hooft had followed Aaron's prescription, he ('t Hooft) would have submissively abandoned his proof and assumed that he was too dumb to even understand how flawed his work must have been."

Funny you should mention this. One of the best advice I've ever gotten was from 'tHooft: At a conference some years ago (when I just started as a grad student) a bunch of us young students were clamouring around the guy to talk to him and someone asked him about his (then recent) work on holography, quantum determinism etc. His said that he was more than happy to speak to us about it but first wanted to know where in our physics careers we were to which the response was "beginning graduate students". His reply was that before he tells us about this stuff he would just like to offer a little advice: work hard on some well-defined problems, write good papers, get your PhD's and only later, when you get tenure should you think about these "big problems".

139.
May 31, 2005

A paper was just posted on math.DG arxiv claiming to construct a complex structure on the 6-dimensional sphere (it was thought not to exist, but nobody could prove that…)

140.
May 31, 2005

Hi
Peter,
you mention,

"whereas I do see some hope that if one better understands the structure of the standard model, one may be able to get to quantum gravity from there."
whereas it maybe possible that the converse is true. i.e., only by trying to study gravity or
thinking of ways to marry gravity with other forces , can one understand the structure of standard model in detail. Heck we
still do not know if GR is the correct classical theory of gravity since it hasn't been tested as accurately as the other 3 forces in strong \
gravity limit. So I don't think there is anything wrong if people are not studying particle phenomenology and instead spending time on trying to unite gravity with quantum mechanics. Maybe
particle phenomenology has reached a limit.
In fact I would argue that there should be more people working on studying and understanding GR in detail, classical alternatives to GR, and other approaches to QG besides string theory and LQG.
In fact there are very few people working on
"gravitation phenomenology" i.e. trying to undderstand various gravitational based experiments such as data from binary pulsar, gravity probe B, lunar laser ranging etc.

141.

May 31, 2005

The problem with waiting until you get tenure is thas after years as a graduate school, years as a postdoc and years as a junior faculty making all sorts of compromises, you gradually and eventually change into becoming "one of them" without even noticing and assuming you are mainstream enough to get tenure, meaning you can act mainstream enough, decades of pretending will eventually get to you before then and you will start to believe…

That is the way the system works. By the time you get tenure, you have been throroughly brainwashed and completely orthodox. See, it's usually the younger generation which forms the leading edge.

142. **Aaron Bergman**
May 31, 2005

The key event leading to the acceptance of the electroweak component of the standard model was 't Hooft's proof of renormalizability. At the time, not only was 't Hooft merely a grad student (certainly not tenured), and his tenured adviser (Veltman) not only did not understand what 't Hooft had done, Veltman did not beleive that his student 't Hooft was correct or even capable of solving such a problem. If 't Hooft had followed Aaron's prescription, he ('t Hooft) would have submissively abandoned his proof and assumed that he was too dumb to even understand how flawed his work must have been.

Hardly. For a history, you can see Weinberg's recollections.

143. **Tony Smith**
May 31, 2005

Aaron said: "… Some argue, then, that there should be research alternatives. There are two answers to that.
In terms of quantum gravity, there just aren't that many games out there, and there's not much market for the sort of speculation that could lead to a new direction — that sort of thing, much like the interpretation of quantum mechanics, should be left to the tenured. …
But the second answer to the question is the more important, because it really belies much of the impression that one might get from this blog. The simple fact of the matter is that there are alternatives in high energy to working in strings … phenomenology or cosmology …".

With respect to Aaron's second answer, phenomenology and cosmology are not alternatives in fundamental theoretical physics, and are not in direct competition with superstring theory. Aaron only lists one competitor of superstring theory, that is, LQG, and he dismisses it by saying "… it is fraught with difficulties, not the least of which are the lack of a classical limit and a bizarre quantization procedure which, if applied to a theory like the standard model, gets physically incorrect answers. …".

That leads to Aaron's first answer, which is that a fundamental theoretical physics alternative to superstring theory "… should be left to the tenured …".

The key event leading to the acceptance of the electroweak component of the standard model was 't Hooft's proof of renormalizability. At the time, not only was 't Hooft merely a grad student (certainly not tenured), and his tenured adviser (Veltman) not only did not understand what 't Hooft had done, Veltman did not beleive that his student 't Hooft was correct or even capable of solving such a problem. If 't Hooft had followed Aaron's prescription, he ('t Hooft) would have submissively abandoned his proof and assumed that he was too dumb to even understand how flawed his work must have been.
Fortunately for physics, 't Hooft did not follow Aaron's prescription and maintained the correctness of his work even in the face of his adviser's contrary opinion, and Ben Lee did the hard work necessary to understand 't Hooft's proof and to recommend it to the physics community.
Aaron could say that the key element was the validation by tenured and respected Ben Lee, but the proof by untenured student 't Hooft was also necessary, and would not have happened had 't Hooft followed Aaron's prescription.
When you look at today's world of fundamental theoretical physics, it seems that there are no Ben Lee type people who are willing to work hard to understand anything outside of their own little boxes, and that amost all of their little boxes are superstring boxes.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

144. **Juan R.**
May 31, 2005

Aaron Bergman

said ?I don't believe a scattering amplitude has ever been computed in LQG.?

I am not sure of this because I not revised recent literature on the topic. I think that now graviton-graviton scattering amplitudes can be computed from LQG. Still is not that I said in my previous post.

I believe that computation of black hole entropy has ?been done? for black holes far more realistic than from ST.

Hawking radiation, elimination of ultraviolet divergences in QFT, cosmological inflation, unobserved supersymmetry, and others are best explained in LQG. String theory does not explain nothing of that, absence of ultraviolet is still debatable, and supersymmetry is, at best of my knowledge, a crucial piece of the theory, still nobody observed it in laboratory.

?This goes back to Wheeler and possibly before.?

Hum!! I was not speaking about geometrodynamics, I believe.

?Unitarity violations are also an old idea.? Of course!!

?String theory, on the other hand, really is unitary.? This is still debatable, the best **proof** by Schwartz is some like his recent ?I believe that string theory is unitary?.

anonymous

I said

?What if the failure of quantization of GR is on that GR is not correct after all??

Of course I wanted to be provocative. Let me rewritte.

We want quantize GR, are we sure of that GR is correct before our attempts to quantize it?

Penrose is a relativist. He claim that in some test GR is tested at order of 10^12 (I believe) and claim for an adaptation of QM to GR. I ask: are those test completely infallible? Is GR the correct approach to gravitation? Note that I am not talking about possible Planck scale modifications of GR.

Initially, I took as completely true GR and studied its basis. However, a problem of confrontation of symmetries obligated to me to reconsider my thinking.

Sincerely I am not sure still, but I think that GR is not so correct and verified like I thought only a year ago!! The surprising is that some specialists in gravitation think the same. For example in a recent PR-D of last year the author says

?[…]*the correct relativistic gravitational theory may be of a kind not generally considered hitherto*??

At what extension GR is correct or incorrect. I think that first we would study this before claim for quantization.

Quantoken said

?No, no one is a culpable of the stringy failure and no one should be held responsible for the “failure”. Nature is the nature way it is and it would not have changed anything because somethign the researchers have tried or have not tried. If the nature is not 10-D, then it is not 10-D, and you can get a super-Einstein to research string theory, and he would not get anything different. He would not be able to force the nature to become 10-D. Only God can do that.?

I was obviously being provocative. The emphasis was in that string theorist are unable to recognize, still today, that they choose a wrong way that instead of studyng like nature is, they choose obligate to nature to be like they want to be. I was referring to **scientific methodology**

145. **Scott**
May 31, 2005

man this has strayed far from peter’s blog hope he doesn’t mind.

this is the internet people get snarky(i’m not really even sure what that means) rude. My point was that people care about having a job more then they care about following the research paths that inspire them whether it be a new try at QG or any number of the cases of fine tuning and other problems to be explained The only advantage to having a job as far as I can see is being able to form close working relations with the other faculty which is nice don’t get me wrong but I won’t loose the relationships i will make in grad school completely if I don’t get a job, I can still think about physics and try to come up with new ideas even if i am working as a carpentor a schoolteacher a beggar a writer or even a patent clerk. Maybe I won’t come up with anything successful or maybe I will, who knows.

146. **Amused**
May 31, 2005

>What I’ll say is that’s just how the world is: you need to get a job.

Yeah, what is the alternative ? To join the club of crackpots and failed physicists who find their purpose in life in proclaiming failure of what thousands of other, hard working physicists are doing…finding comfort in mutual shoulder padding and the belief, all those others must be wrong.

147. **Aaron Bergman**
May 31, 2005

“your goal after graduate school is to get a job”

I think i understand what happened to particle physics now, thanks.

The temptation to be snarky here is almost irresistible. What I’ll say is that’s just how the world is: you need to get a job.

So, the question is how do people make their decisions on who to hire. They’re not going to give you a postdoc much less tenure based on nothing. You have to be able to show that you can produce real research. Now, you can spend graduate school trying to find a new theory of quantum gravity, but odds are you’ll fail. People have been failing at it for fifty years. It’s hard. And, then, you’ll be stuck with nothing but easy speculation and no job prospects.

What some people do is to work in the wacky stuff while generally doing mainstream stuff. To pick an example, Max Tegmark writes on the interpretaion of quantum mechanics, but if you check out his papers, you can see that the vast majority of his stuff is quite grounded.

What it comes down to is that, in order to succeed, you have to show that you can produce. And the way to do that is to work on tractable problems. But, I wish you luck in whatever you end up trying to do. Maybe you’ll succeed. We all hope someone will, eventually.

148. **Scott**
May 31, 2005

“your goal after graduate school is to get a job”

I think i understand what happened to particle physics now, thanks.

149. **Aaron Bergman**
May 31, 2005

It is arrogance of the highest order to conclude that the failure of string theory to predict means a failure of fundamental physics in general. We do, after all, have all the successful theories developed before string theory came on the scene.

Who’s doing that? In fact, the success of the standard model is precisely an example (if the landscape, god forbid, turns out to be true) of certain generic predictions that hold for all realistic vacua.

150. **Aaron Bergman**
May 31, 2005

While I can’t speak for mortain I can tell you that I didn’t first decide that i wouldn’t go into string theory from this blog, nor does all of my limited knowledge in strings come from it.

I’m not here to recruit anyone. There are more jobs in other fields. If you find them interesting, I highly recommend pursuing them.

I don’t like your assumptions that i am incapable of thinking for myself and was tricked by peter.

I’m not claiming anyone is ‘tricked’. I just recalled my experience about getting an impression of a field from an internet forum and noted that I certainly got a misleading impression.

This I also found rather unappealing:

"there's not much market for the sort of speculation that could lead to a new direction — that sort of thing, much like the interpretation of quantum mechanics, should be left to the tenured."

If there is no "game" in physics(particle or cosmology) that attracts me I plan, and already have a couple of spare ideas floating around my head that need development, on making my own "game" or I will fail trying untill someone else makes a "game" i'm interested in, whether the tenured want me to or not.

like chris said just cause this one model of how you think things might work is unpredictive doesn't mean there isn't another model out there that is, me I'm going to keep looking for that next model

I wish you luck because you will need it. The simple fact of the matter is that your goal after graduate school is to get a job, and the way to do that is to write papers that others read and cite. It might not be the ideal way for the world to work, but that's just life. The good news is that, if you can play the game well enough and get tenure, you can do whatever you want. That, after all, is the point of tenure. But until then, it's good to work on the tractable problems and get a record people can look at when application time comes around.

151. **Aaron Bergman**
May 31, 2005

Questions such as corrections to scattering amplitudes (via E-p relationships)

I don't believe a scattering amplitude has ever been computed in LQG

BH entropy (including computation of logarithmic corrections, etc)

LQG has not computed the black hole entropy either. There has not even, to my knowledge, been an identification of a black hole state in the theory.

Hawking radiation, elimination of ultraviolet divergences in QFT, cosmological inflation, unobserved supersymmetry, and others are best explained in LQG;

LQG has next to nothing to say about any of these.

CY is just a "standard" differential manifold. The substitution of macro spacetime by a kind of quantum foam was first introduced in LQG, etc.

This goes back to Wheeler and possibly before.

Even possible violations of unitary were first introduced in LQG and only recently in certain noncommutative geometries in brane theory.

Unitarity violations are also an old idea. String theory, on the other hand, really is unitary.

And this is what I mean by LQG hype….

152. **Scott**
May 31, 2005

Chris,

Thanks for the advice, motivating myself to work on stuff that I'm not really wanting to do is one of my biggests problems my plan is to apply to whichever grad schools will give me the most freedom in my research. Speaking of motivating myself to do things i'm going to go finish my lab write-up.

153. **mortain**
May 31, 2005

Previous comment was a mortain comment; accidentally omitted pseudonym.

154.
May 31, 2005

Yes, anonymous, I am frustrated. I'm glad I'm not the only one. Thank you for your comment on this (it's helped).

To Scott: yes, I think you are entering at a good time, too. Hopefully there will be lots of experimental discoveries soon, leading to new theories and a reshaping of fundamental particle physics. Maybe the LHC is just what we need to oust string theory, perhaps analogously to Michelson-Morley's experiment demolishing the aether. I got my PhD two years ago, by the way. I wish I'd been reading a blog like this as an under-graduate!

Hey, Chris, I did just that… and afterwards got nowhere very fast in physics or in life, sadly.

"What if the failure of quantization of GR is on that GR is not correct after all?" That is a worrying thought, at least for me, Juan R. I suppose the converse, as far as conventional approaches to QG go, is Penrose's kind of view, i.e. that QM requires modification, and not GR. Either way, I find possibilities like these very discomforting. One fact I've never gotten over is that some special materials (whilst in Earth's gravitational field) do not obey GR [see, e.g., Clifford's article in '300 Years of Gravitation']… what's that all about!? Just quantum corrections to GR, or a problem with GR?

Peter, was it Pauli or Veltman-quoting-Pauli who inspired your blog's name? If string theory did die, would you change the name of your blog? I believe that your blog would be no less interesting or worthy if string theory didn't exist. Incidentally, did you notice that Peter Lax won the 2005 Abel Prize this month? I'm almost certain that you made posts about the other previous Abel Prize winners (all three of them), and I wondered if you were planning a post about Lax.

155. **Ed Z**
May 31, 2005

I've been following a lot of the string theory news for years now in the press as well as in the arXiv (with very limited understanding) and by searching the internet for articles and blogs. It seems to me that there are a few camps out there with particularly strong views either in favor of this theory or that theory, and frequently they take their theories and any challenges to their theories quite personally, which I suppose is fine, as long as it contributes to the progress of science.

On the other hand, it seems that the politicization [sp?] that is so prevalent in the US now with the Conservatives and the Liberals squaring off in every possible forum is spilling over into high-energy physics theory. Is this how things were in the early days just before the quantum revolution, or is it somehow different?

To me the picture of an ideal physicist is someone who is deeply interested in understanding how nature works, and does so objectively, and realizes that despite the advances that science has made so far, nature always seems to throw a few curve balls in with every round of pitches, and we have to be prepared for them, if we hope to hit a home run.

156. **Quantoken**
May 31, 2005

Some one said:

"But the culpable of **failure of stringy endeavor** is not Nature, nor Lotto, nor Peter, nor Glasgow, nor Phil, nor… nor this ignorant called Juan R. The problem was the arrogance of string theorists."

No, no one is a culpable of the stringy failure and no one should be held responsible for the "failure". Nature is the nature way it is and it would not have changed anything because somethign the researchers have tried or have not tried. If the nature is not 10-D, then it is not 10-D, and you can get a super-Einstein to research string theory, and he would not get anything different. He would not be able to force the nature to become 10-D. Only God can do that.

Actually I think the word "failure" is not even the correct word to describe the current status of string theory. Failure is the kind of thing that has an outcome dependent on what you do. You do or behave in a correct way you succeed, and you do it a different way you fail. If there is a treasure island out there and you could not find it. It's a failure. But if there is no treasure island to start with, then it's not a failure but simply reality.

The string theory is the later case. It's not a failure. It is simply reality. The reality is the world is 4-D, not 10-D. Plain and simple. And some people can not accept that reality.

Quantoken

157. **Urs**
May 31, 2005

At least Aaron every now and then takes the time to drop a note here whose author is actually familiar with what he is talking about. He should be honored for that.

158. **Chris Oakley**
May 31, 2005

Scott,

Observations like the one I made below (at 05:16) are so obvious that no-one should need to make them. The fact that someone has to is indicative of the groupthink mentality of string theorists. I think that you are right to want to follow your own lights, stringy or not, and I applaud you for it, but my advice – for the sake of your own sanity – is to be prepared to go quietly and gracefully after your Ph.D. if you fail to find like-minded individuals within the establishment who are prepared to support you.

159. **Scott**
May 31, 2005

Wow Aaron way to fuel the fire. While I can't speak for mortain I can tell you that I didn't first decide that i wouldn't go into string theory from this blog, nor does all of my limited knowledge in strings come from it. I first decided that it definately wasn't for me after my freshmen physics seminar course where Schwartz's answers to my questions about predictions and testability were very disatisfying as were the reasons to pursue string theory that he gave(and all the ones i've read of sence then and before for that matter). I don't like your assumptions that i am incapable of thinking for myself and was tricked by peter. This I also found rather unappealing:

"there's not much market for the sort of speculation that could lead to a new direction — that sort of thing, much like the interpretation of quantum mechanics, should be left to the tenured."

If there is no "game" in physics(particle or cosmology) that attracts me I plan, and already have a couple of spare ideas floating around my head that need development, on making my own "game" or I will fail trying untill someone else makes a "game" i'm interested in, whether the tenured want me to or not.

like chris said just cause this one model of how you think things might work is unpredictive doesn't mean there isn't another model out there that is, me I'm going to keep looking for that next model.

160. **Juan R.**
May 31, 2005

Aaron Bergman, string theory has not been more successful that LQG.

Questions such as corrections to scattering amplitudes (via E-p relationships) BH entropy (including computation of logarithmic corrections, etc) Hawking radiation, elimination of ultraviolet divergences in QFT, cosmological inflation, unobserved supersymmetry, and others are best explained in LQG; CY is just a "standard" differential manifold. The substitution of macro spacetime by a kind of quantum foam was first introduced in LQG, etc. Even possible violations of unitary were first introduced in LQG and only recently in certain noncommutative geometries in brane theory. The absolute ?success? of LQG over string M hype is brilliant.

One woud also remember that the number of researchers in string M-theory is of the order of 10 times that of LQG practitioners. Therefore, the relative ?success? of LQG is still more impressive.

When Brian Greene claims, in public, that they can ?see? GR in their equations whereas LQG have many problems for obtaining the correct macroscopic limit, he is being no sincere, because one may know first GR for introducing *by hand* the corresponding modifications on original string description for ?convergence?.

Peter said:
?*I do see some hope that if one better understands the structure of the standard model, one may be able to get to quantum gravity from there*.?
I partially agree, but due to inconsistencies between GR and SM, at least one of both would be drastically modificated.

What if the failure of quantization of GR is on that GR is not correct after all?

Some question about this interesting possibility?

dan, string theory is not a quantum theory of gravity. Einstein gravity is 4D and nobody can derive that from 10D action without *ad hoc* assumptions. Moreover, string theory does not introduce an adequate quantum description of spacetime still. Non-commutative M-theory is just in a stopping way.
The description via gravitons is only ?formal? with no macroscopic correspondence and for the unification with particle physics, in the words of Daniel Friedan (now a ?renegade?):

?*At best, for each macroscopic background spacetime in the manifold of possibilities, string theory gives large distance scattering amplitudes that form a caricature of the scattering amplitudes of the standard model of particle physics*.?

Even if one see that the background may be chosen *by hand* previously.

mortain, after of several of my open criticisms to status of string theory, many people contacted with me, including previous string theorists. One of them, did a PhD in string theory and just leave the field (and physics) because insatisfction ith the way of high energy.

If instead of current dictatorial status in the field young people can research in other promising areas outside of string theory garbage, then several youngs promising physicists continue being today physicists and the high energy would today is full of new fascinating ideas.

You would be not angry if the ?best?, ?marvelous?, ?fascinating? ?theory? invented was simply a waste of time. You may be angry with Nature and its ?stupid? choosing of a macro 4D without string or D0-branes.

If string theorists were no crackpot theorists and had not killed other interesting stuff…

If you play to Lotto and you put all your money in a single combination, you can win or you can lose. String theorists were completely sure of that they couldn?t lose, but the ?Casino?, Nature, always win 🙂

But the culpable of failure of stringy endeavor is not Nature, nor Lotto, nor Peter, nor Glasgow, nor Phil, nor… nor this ignorant called Juan R. The problem was the arrogance of string theorists.

Peter, i think also that LQG is not the last word, still the research model chosed has been much more interesting. Supposed success of string theory, like the ?derivation? of BH entropy, are also achieved in LQG with 4D.
If S is the ratio success/publicly for strings and L is for loops then L >> S.

After of many years working in silent, LQGs (especially Smolin) are now playing to publicy of their ideas, this is a deffences attitude against so many papers, talks, and popular books by “stringys” claiming that string theory is the only approach to quantum gravity, which is, sure, completely false.

Moreover, still LQG is open and would introduce interesting ideas for particle physics. Smolin thinks one could see by first time gravity modification to standard model on next generation of accelerators. Note that in LQG
E^2 = m^2 + p^2 + corrections terms

161. **Chris Oakley**
May 31, 2005

In regards to the anthropic principle, there is, unfortunately, a rather depressing prospect. All this nonsense, for all that Peter can throw around words like ‘unscientific’, ‘unpredictive’ and ‘unfalsifiable’, could still just be how the world is. We might just be stuck with it. There could be countless vacua, and we just might live in one of them for no particular reason.

If this is true then in terms of theories that make precise statements about the world in which we live, we are a lot worse off than we were in 1979. What perhaps many will fail to appreciate is that string theory is not a superset of our pre-string view based on quantum mechanics, QFT and GR, but an alternative. The ability to properly reproduce the “old” theories has never been demonstrated. So if this alternative fails to make testable predictions it is not a failure of physics in general, it is just the failure of a particular idea. It is arrogance of the highest order to conclude that the failure of string theory to predict means a failure of fundamental physics in general. We do, after all, have all the successful theories developed *before* string theory came on the scene.

162. **Aaron Bergman**
May 31, 2005

Oh my. What a thread….

Certainly some people should be working on quantum gravity, especially if they are doing it in a non-overhyped way, trying to really seriously understand the technical issues involved. The LQG community appears to be doing this.

LQG is far from underhyped. They’ve had their full page spreads in the Times, Smolin gets quoted pretty much everywhere, and they’ve tiresomely put themselves forward as the great rebels against the overbearing and ignorant hordes of string theorists. Frankly, it’s just as offensive as anything that has ever come out of a string theorist’s mouth.

But that’s not really what I want to say. Judging from the comments, it looks like a lot of prospective physicists are reading this blog. I remember from not so way back when, as an undergraduate, in the days when Usenet mattered, I read sci.physics.research. What I didn’t know, then, was that it gave me a horribly skewed view of the field. When I showed up at graduate school, full of all the questions I thought string theorists had no answers for, I quickly discovered, sometimes to my embarrassment, that they knew all the questions and had good reasons for doing what they were doing.

So, hopefully without sounding horribly condescending, I just want to point out that Peter’s blog does not present a good idea about how things are. Now, I don’t want to lie: there is a sense of the doldrums in the field right now, especially after such a fruitful period as the nineties. Flux stabilization seems to have, much to the disappointment of many, led to an overabundance of vacua. This has led to a number of different proposals on how to deal with this, including anthropic arguments. It is not correct, however, to say that the field is consumed with this. The number of papers that deal with anthropic arguments do not even approach one half or one quarter. Most of us are perfectly happy to go on with our own projects and leave the anthropic stuff to the occasional dinner table argument.

My personal feeling on the subject is that trying to make predictions of the real world based on strings is like playing darts in a pitch black room without knowing which wall the dartboard lies on.

Now, Peter undoubtedly would think of that as a horrible indictment of string theory, but it isn’t. Even after 20 and more years, my feeling is that we do not understand string theory but that there is every indication that there is something there to understand. In the process of seeking that understanding, we have learned much about supersymmetry, nonperturbative gauge, the large-N limit, conformal field theory, gravity in higher dimensions, new scenarios for phenomenology and, not least of all, lots of cool mathematics. So, whatever mythical opportunity cost we may have incurred, I’d hardly say that these were years ill-spent.

Some argue, then, that there should be research alternatives. There are two answers to that. In terms of quantum gravity, there just aren’t that many games out there, and there’s not much market for the sort of speculation that could lead to a new direction — that sort of thing, much like the interpretation of quantum mechanics, should be left to the tenured. LQG is the most widely hyped alternative, but, their own PR aside, it is fraught with difficulties, not the least of which are the lack of a classical limit and a bizarre quantization procedure which, if applied to a theory like the standard model, gets physically incorrect answers. In addition, their claim to compute the black hole entropy relation appears to be quite ephemeral upon close examination. String theory, for all its ills, at least gets that correct. The simple fact of the matter is that no alternative theory of quantum gravity has come close to the success that string theory has had, however minor you may consider that success to be. If you think of yourself as a stubborn iconoclast, unwilling to follow the herd, realize that pretty much everyone who goes into physics is like that. It's hard enough to get a group of physicists to decide where to go out to eat, much less browbeat them into a particular direction of research. There's a reason so many people work on string theory, and it isn't peer pressure.

But the second answer to the question is the more important, because it really belies much of the impression that one might get from this blog. The simple fact of the matter is that there are alternatives in high energy to working in strings, and those alternatives are *more* lucrative as career paths. As a commenter pointed out, LHC is turning on in a few years. This is the golden age of cosmology. Look at hiring patterns: people who do phenomenology or cosmology are being snapped up by the dozens. Lots of string theorists on the market are trying to rapidly broaden their horizons so they can compete for these jobs. So, go work on those things. That's where the excitement is. There's no need for string theory to 'die'; there are plenty of other things to do with plenty of jobs to get.

And, why should it die, really? In regards to the anthropic principle, there is, unfortunately, a rather depressing prospect. All this nonsense, for all that Peter can throw around words like 'unscientific', 'unpredictive' and 'unfalsifiable', could still just be how the world is. We might just be stuck with it. There could be countless vacua, and we just might live in one of them for no particular reason. All the Poppers in the world can't change that. But, it's also possible that, as we understand the theory better and better, maybe some of those vacua fall away, or maybe we find universal predictions of the theory that apply for all physically realistic vacua. We'll never know if we don't try.

I've argued above that string theory has already had a string of successes even if the ultimate goal of a full theory of quantum gravity still lies beyond the horizon. Some people are antsy, to be sure, looking for things to say before the grand accelerator turns on, but, in my view, there are still many problems out there to explore. Once upon a time, in the late eighties and early nineties, people thought string theory was dying, too, but that was just the calm before the storm. Who's to say that now is the time to give up, that, from here, all is hopeless?

Because, really, that's just more hype.

163. **A.**
May 31, 2005

Peter,

if I understand you correctly, you consider string theory interesting and worthwhile because of its connections with QFT dualities, but not as interesting as the hype around it suggests. This is a reasonable point of view which I share, to some extent. I agree that the potential of string theory to give us a unified theory of all interaction has been somewhat overhyped. This is not a unique situation: when people discovered N=8 supergravity, some suggested that it is the final theory of everything, without much evidence. The amount of hype in string theory is about normal and much less than in LQG, imho.

There are at least two reasons that so many people work on string theory (and not, say, on LQG). First, it is very rich and consequently fun. It leads to all kinds of neat theoretical ramifications which make one forget about the original goal (unification). The second reason is that it apparently does provide a consistent theory of quantum gravity, including black holes. No other approach to quantum gravity can boast this. It is true that the matter fields do not quite come out right, but this should not be very important. By studying string theory one has a chance to understand how to describe physics in a space whose metric is fluctuating. This is the point which Witten likes to emphasize, and it is also very reasonable. IMHO, these two factors account for the "unreasonable popularity" of string theory, not the hype.

164.
May 30, 2005

First superstring revolution, second superstring revolution…..

my vote:
Categories and Logic revolution!

165.
May 30, 2005

"Unless, of course, there is a revolution."

Yeah! Let's go!

166.
May 30, 2005

Unless, of course, there is a revolution.

The first superstring revolution

The second superstring revolution

???

167.
May 30, 2005

"Just die, string theory – please! Die, die, die! I've had enough!"

Unfortunately, history runs at a slower pace than a mere human would like! Many of us share your frustration…but I'm not holding my breath.

168.
May 30, 2005

By the way, has anyone sorted out the relation between algebraic holography (Rehren duality) and the Maldecena duality yet?

169.
May 30, 2005

Did someone come up with a generalization of the AdS/cft duality? Any quantum field theory, not just conformal ones?

170. **Peter Woit**
May 30, 2005

It's not really true that I'm not interested in either string theory or LQG. I'm interested in string theory as a possible way of solving QCD, as a source of interesting mathematical ideas, and potential techniques that might be useful elsewhere. I just think the idea of getting a unified theory out of a 10d string or 11d M-theory doesn't work at all and people should recognize that and do other things.

I try and follow what is going on in LQG, also because I hope some of their techniques will be useful. The problem with quantum gravity is that you have no experimental guidance about what you should be looking for and no way to check whether your theory agrees with reality. Maybe this will change. But I'd be a lot more interested in quantum gravity if some way could be found to connect it to particle physics, where we have a huge amount of data. String theory hoped to do that, but it doesn't work.

Certainly some people should be working on quantum gravity, especially if they are doing it in a non-overhyped way, trying to really seriously understand the technical issues involved. The LQG community appears to be doing this. But, personally, I don't have any ideas about how to start from thinking about quantum gravity and get to particle physics, whereas I do see some hope that if one better understands the structure of the standard model, one may be able to get to quantum gravity from there.

What's not healthy about the current situation is how much effort is being put into one research program, especially now that it has failed. People should be trying a wide range of different ideas, starting from what they know best and have good ideas about how to extend in new directions.

Sure, I think ultimately we'll understand the correct relation of quantum theory and GR, but it's impossible to know on what time scale. Right now though, the discouraging indications from both LQG and string theory are that if they produce a consistent theory of quantum gravity, it will be one that isn't testable (Lee Smolin has some claims that contradict this) and doesn't connect up usefully with particle physics. At the PITP conference, I think Shenker was claiming that, by holography, every QFT is also a theory of quantum gravity. If that's true, you have a solution of the problem of quantizing gravity which is really a Pyrrhic victory.

171.
May 30, 2005

Peter , you mentioned that you are not interested
in either string theory or Loop quantum gravity. My question is which approach according to you will lead to the correct quantum theory of gravity?
also do you support research in quantum gravity
(i.e. non-string approaches to QG)?
also do you think that one day we shall find the correct quantum theory of gravity?

172. **Scott**
May 30, 2005

mortain,

As another person planning on going into theoretical particle physics, I like to look at the positive aspects of the situation today. By the time I get a Phd the LHC will have been up and running for a year or two(still an undergrad right now) and if there are interesting observations I will be entering the field at very interesting time, and if there are no interesting observations it is still a good bet that many will stop working on string theory and there will still be a wealth of new ideas that should have been thought of by now to tinker with. Personally I am really glad I am entering the field now instead of say any other time in the past 20 years.

173. **Peter Woit**
May 30, 2005

Hi Dan,

Sorry, but the idea of getting the structure of the standard model out of wormholes sounds like a complete pipe-dream to me.

The problem with perturbative string theory is that the loop expansion is divergent. So, while (conjecturally) you may be able to get finite answers for the contributions of n-loop superstring amplitudes to graviton scattering, you can't sum the series. String theorists like to say this isn't a problem, since the same thing happens in QFT where the perturbation series is only an asymptotic series. But, at least for non-abelian gauge theory QFTs, you have a well-defined non-perturbative definition of the theory, which seems to give finite results. Even conjecturally, the closest thing to this in string theory is something like Matrix theory, but that only works in some non-physical cases like flat 11d spacetime.

174. **dan**
May 30, 2005

thanks for replying Peter,

yes i am well aware of lubos anti-lqg crusade, though he does come "out of the closet" and admits he's spent "hundreds" of hours studying the subject.

john baez does hold out the hope that particles could be modeled on space-time wormholes through the fabric of space-time (as opposed to strings), though lqg is still in its infancy.

i am curious as to what you think of string theory as a quantum theory of gravity? string theory does allow calculations of s-matrix scattering for gravitons

175. **Peter Woit**
May 30, 2005

I believe Veltman's attitude towards string theory is much more negative than 't Hooft's. See the last few sentences in his book about particle physics, explaining why he hasn't mentioned string theory (or supersymmetry):

"They are figments of the theoretical imagination. To quote Pauli: they are not even wrong. They have no place here."

176. **mortain**
May 30, 2005

Just die, string theory – please! Die, die, die! I've had enough!

My apologies for such an unscholarly outburst. [Please note my outburst is not aimed at string theorists.] After reading recent posts by Peter about how ludicrous string theory already is (and is becoming), particularly about what should be recognized as the death blow to the 'landscape', I just couldn't contain myself. I agree with yourself, Peter, and always have. Juan R., I applaud you.

Damn string theory for being around at the time when I had hoped to begin a career in theoretical particle physics! Bad timing indeed; as if things, especially the availability of employment, weren't difficult enough already in theoretical physics!

I think Unruh's response to Susskind's claim regarding string theory funding extends to every Western nation engaged in physics research… perhaps to every nation engaged in physics research worldwide, such is the prevalence of string theory and its hype. Wizened senior theoretical particle physicists: just keep on sapping that funding for string theory – only you know it makes sense!

By the way, Peter, do you know what 't Hooft's former collaborator/supervisor, Veltman, thinks of string theory? I vaguely recall a post-lecture question about string theory being put to Veltman at a conference, but I cannot remember his answer (though I doubt it was as detailed as 't Hooft's public remarks).

Also, Peter, why should string theory's motivational 'qualities' concerning non-perturbative QFT be considered a success? String 'theory' seems to have motivated a lot of nonsense since its inception. As you point out, if more people had been working on non-perturbative QFT in the first place, and not on string theory, the progress already achieved to date would surely have been made, or made several times over, without the influence of string theory.

So… please, I beseech ye, just do the decent thing, string theory, and die. Forthwith, consign thyself to history!

177. **Peter Woit**
May 30, 2005

Hi Dan,

I wasn't thinking about LQG when writing about Lubos. He's very much skeptical about it. My own opinion is that it's a promising way of dealing with quantum gravity. But personally my main interest is not in quantum gravity, but in particle physics, and unfortunately LQG doesn't seem to have anything to say about this.

A.,

I've followed 't Hooft's public remarks about string theory carefully and corresponded about it with him. His own words should speak for themselves, but my interpretation of them is as follows:

1. He doesn't believe that string theory based unification is anywhere near explaining anything about the standard model. He explicitly says he was a skeptic about this in 1985 and now feels he has been proven right. He refers to the need for a completely new idea in this area.

2. He's interested in quantum gravity, and interested in what string theory has to say about that.

3. I think his words about dualities were carefully chosen. They have definitely led to interesting relations between QFTs and AdS/CFT is important progress towards finding a string dual of QCD, but precisely because 't Hooft has worked hard on large N, he's aware of how limited this progress is. 't Hooft is also a careful worker, taking great pains to be very precise about what he is doing. I suspect the very hazy nature of a lot of the string theory duality stuff, and the lack of a fundamental understanding of what the theory is and where the dualities come from, bothers him.

As for the progress in QFT due to string theorists that you mention. Witten has certainly contributed a huge amount to understanding non-perturbative QFT, but most of this has had little or nothing to do with string theory. String theory has motivated some progress in non-perturbative QFT, and this remains the best reason to keep doing string theory. In this area it's not a complete failure, like it is as an idea about unification. But I still think that if one-tenth the amount of work on string theory had gone into work on non-perturbative QFT itself, there would have been even more progress in this area.

178. **Alejandro**
May 30, 2005

CrisW says:
if Susskind's conjecture was reformulated as "the Laws of Physics are determined by the requirement that the laws of physics are discoverable," then I think it would stand a fighting chance of being fruitful,

Yep, I could buy it. Perhaps even "speakable" or "measurable" instead "discoverable", which is very broad.

179. **Fyodor**
May 30, 2005

"How exactly are Laughlin's "emergence" ideas very harmful to science in comparison to string theory, besides being a bit on the loony side?"

Homework exercise: write an essay for the Templeton Foundation explaining how Intelligent Design *is* after all scientific, once one takes into account the "emergent" nature of physical laws….[no money for this one I fear, because I bet that somebody has done it already…] Only in California. I hope.

String theory has its faults, but I have not seen a string theory paper anywhere near as downright kooky as Chapline's Laughlin-inspired twaddle about black holes. True I have heard string theorists say some silly things about GR. But nothing on that level. The tone of Chapline's stuff is frankly irrational: you pansies with your differential geometry — here, let me show you how a real condensed matter *man* handles a little problem like that!

By the way, I am impressed by Peter's ability to divine Leonard Susskind's emotional state by studying the bumps on LS' bald pate. Technology updates phrenology.

180. **Juan R.**
May 30, 2005

That is, like said in 2003 and repeat to the beginnig of this 2005, string theory is a waste of time.

The basic equation i said in March 3, 2005

Arrogance + wrong physics + elementary math = fiasco

All supposed experimental indirect verification in acellerators, and cosmology (beatiful cosmic strings "explaining" dark matter), etc. all of that proven to be wrong.

The supposed predictive power of string theory and its "only parameter" diluyed in the fact of that Nature is, at least macroscopically, four dimensional. Nature may be stupid after all!

From the hypotetical TOE to the real TON "Theory of Nothing"

I said that string theory was not a scientific hypothesis.
Now many of string theorists agree with desesperatly claims as "physicists may have to rethink what it means for a theory to explain experimental data". It sound somewhat as given that string theory does not agree with basic underpinnings of scientific method, we would change the method for adapting it to our nonscientific "Credo"

In recent years string leader Jim Gates like to use the term our “kind of a church”

Many string theorists have arrogantly ignored, furiously attacked (e.g. during decades claimed that LQG was wrong), or misunderstood other interesting approaches to quantum gravity.

On recent years Witten searched a link wth twistors, Vafa proposed that perhaps LQG (in past times a “heresy” for string theorists) could be a part of string theory, etc.

From the supposed leading of string theory durcing all of 20th century claimed in past years to the recent increasing number of canceling of lectures on the topic, halt publication of new undergraduate textbook on the topic; canceling of post-docs, summer programs, and conferences, etc.

Only five or six years ago, string theorists were claiming in public that the apparent 4D (“postulated” in standard model) was a consequence of string theory. What success!!

Recent work is less promising, as admited by Witten this year

“That’s a big problem that has to be explained. As of now, string theorists have no explanation of why there are three large dimensions as well as time, and the other dimensions are microscopic.”

Another of claims of string theorist was that string theory was the most fundamental and sophisticated theory newer imaginated. The math involved in string theory was imppresive and the concepts completely revolutionary ones.

We agree in that part of geometrical side of string theory is very advanced but in other parts string theory was always archaic, as stated from many people from many different fields: general relativity, advanced quantum mechanics, thermal fields, chaos, etc.

Even without experimental verification one already knew that, for instance, Schwartz superstring action was not the most fundamental approach to nature. But, and this is an very important point, string theorists have a general misunderstanding of that is being done in other fields of science. It is very arrogant claim that your theory is a TOE and explains ALL to most fundamental level, when you do not know equations, concepts used in other fields.

It is not so strange that one can obtain current string M-theory only after of many asumptions and simplications.

181. **JC**
May 30, 2005

Fyodor,

How exactly are Laughlin’s “emergence” ideas very harmful to science in comparison to string theory, besides being a bit on the loony side?

If not many people are really taking Laughlin, Anderson, etc … seriously about their “emergence” ideas as a “unified field theory”, then how harmful are they? How much more “dangerous” are they compared to the crowds who follow quantum gravity in general?

182. **Fyodor**
May 30, 2005

Peter, it is very clear that the “emergence” bullshit being propagated by Laughlin and co is infinitely more harmful to the cause of science and reason than string theory could ever be. I suggest that you direct your ire in that direction. It’s obvious that this whole field is generated by the gargantuan chips that fools like Laughlin have on their shoulders because anything beyond special relativity is also beyond their comprehension. He and Anderson are living proof that even Nobel prizewinners can let their personal resentments overcome their professional judgement — indeed, in Laughlin’s case, their residuum of common sense.

183. **A.**
May 30, 2005

Peter,

you say that ‘t Hooft’s positive remarks about string theory were “half-hearted”. I wonder how
you determined this. Perhaps this statement is simply a reflection of your anti-string bias. Why could ‘t Hooft not be genuinely excited about various dualities between gauge theories, or between gauge theory and string theory,
as in AdS/CFT? After all, he was the first to propose that in the large-N limit Yang-Mills theory
can be described in terms of strings. AdS/CFT makes this very concrete. (It also proves that superstrings are permanently part of theoretical physics, since they describe the strong-coupling limit of a very interesting QFT in 4d). String theory also enabled us to find or better understand many highly nontrivial dualities between QFT, by reducing them to geometric “dualities” or to the well-understood T-duality. Any QFT-lover would do well to study string theory.

I cannot resist noting that in the last 15 years there was a lot of progress in understanding nonperturbative QFT, and essentially all of it was achieved by people whom you would call “string theorists”, people like Witten, Seiberg, Vafa, etc.

184. **Chris W.**
May 30, 2005

..or to put it another way, such solutions look like *simulations*, based not on insight, but on sheer mathematical cleverness, with the promise that the results will ultimately justify the convoluted means. After all, Ptolemy’s account of celestial motions was of great practical use for centuries, and was overthrown by observations that were of little practical import but remained inexplicable in his system. Appearances might have been preserved with further refinement, but with explanation and not merely calculation as a goal, the effort began to seem pointless and futile.

185. **dan**
May 30, 2005

Peter,
In the link you provided to lubos’ website, “the next superstring revolution” he does cite loop quantum gravity, which he admits he’s spent “hundreds of hours on” as a possible basis for another superstring revolution. so when you suggest none of these directions seems promising, do you also believe LQG is a dead-end? that would be surprising to me, as you have posted Lee Smolin’s responses to string theorists on several occassions

-Dan

186. **JC**
May 30, 2005

At times one wonders whether something looking more and more like “spaghetti”, that something doesn’t look quite “right” about it. With more and more “epicycles” added in, it starts to look more and more like something that would impress Rube Goldberg.

If one has ever tried to figure out all of Enron’s accounting and obfuscation, it looks like the equivalent of “Rube Goldberg-ism” in the financial world. At times one wonders whether string theory, the old aether theory, and the many “unified field theories” over the years, are the scientific versions of “Rube Goldberg-ism”. Things look more and more complicated and more “spaghetti”-like.

187. **Chris W.**
May 29, 2005

See the **Conclusion** of this interesting new paper from John Stachel and Mihaela Iftime, posted on Friday.

188.
May 29, 2005

Well, your blog is one of the most widely visited physics blogs. You definitely ARE having an effect.

189. **Peter**
May 29, 2005

I don’t know exactly what effect I’m having, but the increasing number of physicists willing to speak out about what is going on, and the increasing amount of skeptical press may be having some effect.

As far as the landscape goes, I think I’m in a solid majority, with even most string theorists thinking that it is not science. Recall that even Lubos Motl and I agree about this, and David Gross has spoken out forcefully and publicly on the topic. It was interesting to see that Susskind has started worrying that the NSF and DOE will start refusing to fund landscape studies. I suspect a sizable number of people on the panels evaluating particle theory grants may soon start (or have already started) giving low marks to proposals to do landscape research.

190.
May 29, 2005

Is it possible that they are being so defensive because people like you are being so vocal in their dissent?

191. **Chris W.**
May 29, 2005

Regarding the last paragraph, if Susskind’s conjecture was reformulated as “*the Laws of Physics are determined by the requirement that the laws of physics are discoverable*,” then I think it would stand a fighting chance of being fruitful, although not by itself. Consider carefully what is implicit in the reformulated assertion.

Rutgers Workshop

June 5, 2005
Categories: Uncategorized

I spent most of last week commuting down to Rutgers to participate in a workshop on "Groups and Algebras in M-theory", organized by Lisa Carbone. Lisa was a student of Hyman Bass's here at Columbia some years back, and in recent years has been working on Kac-Moody groups and algebras over finite fields.

Much is known about one special class of Kac-Moody algebras, the so-called affine Lie algebras. These are basically Lie algebras associated to loop groups, with a central extension. The study of the representation theory of these algebras is closely connected to quantum field theory in 2d space-time dimensions, and my first talk was about this topic. For more details about this, from the point of view I was taking, see the remarkable book by Pressley and Segal called "Loop Groups", lecture notes from 1985 by Goddard and Olive at the Erice Summer School and Srni Winter school (see Int. J. Mod. Phys. A1:303, 1986), and Witten's paper "Quantum Field Theory, Grassmanians and Algebraic Curves" in Communications in Mathematical Physics, 113 (1988) 529-600.

An elaboration of these ideas in one direction leads to the concept of a "Vertex Operator Algebra" (first introduced by Richard Borcherds), and the study of these was pioneered by Jim Lepowsky, who also participated in the workshop, together with his ex-student and now Rutgers faculty member Yi-Zhi Huang. Several other current and ex-students of Lepowsky and Huang were also there and gave talks. For more about vertex operator algebras, see the recent short review by Lepowsky, or the materials on Huang's web-site. A VOA is essentially the same thing that Beilinson and Drinfeld call a chiral algebra, and these have applications in the geometric Langlands program.

What Lisa is really interested in is the non-affine case, where relatively little is known. Non-affine Kac-Moody algebras and groups seem to have no known tractable realizations, and many basic questions about both the algebras and the groups, as well as their representations, remain open. In recent years several of these algebras have been conjectured to have something to do with M-theory, most notably E_{11}, and the study of this connection has been the main focus of the work of Peter West, who gave a series of talks at the Rutgers workshop. For some more about this, see his recent papers, especially one on The Symmetry of M-theories. West's graduate student P. P. Cook also has a weblog, and recently wrote a posting explaining a bit about this topic.

Greg Moore was at many of the talks and kept the speakers honest. He gave a fast-pace talk covering some older work, roughly the same material as in his paper with Jeff Harvey entitled Algebras, BPS States and Strings. I gave a second talk explaining a bit about my point of view on the Freed-Hopkins-Teleman theorem and its relation to representation theory and QFT.

After the talks Thursday afternoon there was a discussion section on what is going on with string theory, supersymmetry, and mathematics. No one was willing to defend work on the "Landscape" and I was surprised to find myself pretty much in agreement with quite a few people there about the way string theory has been pursued in recent years. On the whole the mathematicians are kind of bemused by the whole string theory controversy. The subject has certainly led to some very interesting and important mathematics, and they are happy to concentrate on that, although interested to hear about the controversy surrounding string theory in physics.

Comments

1. **Matti Pitkanen**
   June 9, 2005

   Thank you for Thomas Larsson for a hint to look Segal's book. I will do it when I visit Helsinki.

   The Dynkin diagrams of simply laced algebras appear also in the Jones inlusions of hyper-finite type II1 factors of von Neumann algebras (see also the article V. Jones (2003), *In and around the origin of quantum groups*, arXiv:math.OA/0309199) and there are reasons to believe that minimal conformal theories with simply laced quantum groups with few exceptions correspond to Jones indices M:N1 factor. There are good reasons to hope that conformal field theory structure is in some sense an inherent feature of this kind of geometry. E10 and E11 seem to represent steps in this direction.

   In TGD framework the counterpart for this group is infinite-dimensional group of generalized canonical symmetries of δ M4+×CP2: the conformal structure is inherited from δ M4+. An interpretation as a Kac-Moody group obtained by localizing canonical transformations of S2×CP2 localized with respect to the radial lightlike coordinate of M4+ is in question.

   Matti Pitkanen

2.
   June 8, 2005

   Peter: "If one could find a simple symmetry principle underlying M-theory"

   Stone duality for higher descent

3. **D R Lunsford**
   June 8, 2005

   AR – I've never seen any explanation even of how gauge invariance as a main principle is supposed to emerge from ST. (Not that I really care 🙂 I saw a half-hearted attempt to pull the QED Lagrangian from such and so string confuguration but it was a real stretch, so to speak.

   -drl

4. **Peter Woit**
   June 8, 2005

   Mike,

   Glad you like the blog!

   Alejandro,

   If one could find a simple symmetry principle underlying M-theory, that would be very interesting, and make the whole idea much more well-defined and potentially useful. But I'm not really convinced by any of the attempts so far. Up to you to see what you think from West and other's papers. As they say, we report, you decide….

5. **Alejandro Rivero**

June 8, 2005

Is this post insinuating that "The Symmetry of M-theories" could be the next superstring revolution if it is explicitly approved and explained by some superstring boss?

6. **Michael Crowley**
June 8, 2005

Hi,
I just wanted to say thank you for this blog. I discovered it while Googling on the Penrose book "Road to Reality"–a book I am enjoying tremendously but am having an extremely difficult time with. I am a lay person but find this discussion incredibly fascinating and am desperately trying to learn enough math and physics to follow these discussions. It's probably hopeless : –). But thank you for this blog.

Sincerely,
Mike Crowley

7. **Thomas Larsson**
June 8, 2005

Matti, this is a good point. I had forgotten that the vertex operator construction of Frenkel-Kac and Segal only works for level 1 reps of simply-laced algebras (ADE). I think that Pressley-Segal has a chapter about vertex operators towards the end of their book, but they call them blips for some reason.

8. **CW**
June 7, 2005

In this connection see this new preprint by H. Nicolai:

> The purpose of this article is to highlight the fascinating, but only very incompletely understood relation between Einstein's theory and its generalizations on the one hand, and the theory of indefinite, and in particular hyperbolic, *Kac Moody* algebras on the other. The elucidation of this link could lead to yet another revolution in our understanding of Einstein's theory and attempts to quantize it.

(..link from **It's equal but it's different**.)

9. **Matti Pitkanen**
June 7, 2005

It would be interesting to known whether vertex operator construction exists for non-simply laced algebras, in particular G_2. Goddard and Olive proposed such a construction for all of them except G_2. I failed to find any definite answer to this question from web.

Matti Pitkanen

10. **Thomas Larsson**
June 7, 2005

That's Richard Borcherds, of course. Sorry.

11. **Thomas Larsson**
June 7, 2005

Robert Borcherds has done great things on Moonshine and Monsters, but he is too young to have introduced vertex operators. I think there is a difference between vertex operator algebras and more general vertex algebras, though. Maybe he had something to do with the latter.

According to Goddard's and Olive's IJMPA review, vertex operators were first introduced by the first generation of string theorists, refs 71,89,90,91: Fubini and Veneziano (1970), Nambu (1969), Fubini, Gordon and Veneziano (1969), Gervais (1970), although very similar things were already done by Skyrme (1961). Vertex operators were first applied to affine Kac-Moody algebras in refs 95,72,65: for SU(N) by Halpern (1975), and in the general case by Frenkel and Kac (1980) and Segal (1981).

Incidentally, the multi-dimensional Virasoro algebra, which I claim is the correct quantum constraint algebra of general-covariant theories, was first constructed as a vertex operator algebra by Eswara Rao and Moody, *Vertex representations for n-toroidal Lie algebras and a generalization of the Virasoro algebras*. Comm. in Math. Phys., 159 (1994) 239-264. It might be noted that Bob Moody is rather well known, e.g. as a coinventor of Kac-Moody algebras.

12. **Peter Woit**
June 6, 2005

I didn't really intend to make any claims about who was responsible for the idea of a VOA, just to comment on who was at the workshop. I had thought of mentioning Borcherd's work, since several talks referred to it. To avoid confusion, I'll rewrite the post slightly, adding a reference to Borcherds.

13.
June 6, 2005

Such silly revisionism. Vertex algebras were invented by Richard Borcherds; its hardly convincing to claim that it is a different notion when you add a Virasoro and the word "operator" in between "vertex" and "algebra"…

(Mind you, with a Fields' medal, he really doesnt need protecting.)

Future of Fermilab

June 9, 2005
Categories: Uncategorized

Nature this week has an editorial about Fermilab entitled All or Nothing at Fermilab associated with a news article Fermilab: High-risk physics. The article and editorial are about the fundamental problem facing Fermilab: in a few years the high energy frontier will move to the LHC at CERN, with many physicists leaving Fermilab. The future of the lab remains up in the air, as the only viable plan for a new high-energy accelerator is the ILC project, and this would require massive new funding which is still quite uncertain. While SLAC has diversified into X-ray physics, Fermilab remains committed to operating at the highest energies. Many people worry that if the ILC is not funded or delayed for many years, Fermilab will be in a difficult position, and a prime target for budget cuts.

This week the lab is hosting the annual "User's Meeting". Presentations about current and future activities at Fermilab are available on-line.

Comments

1. **Tony Smith**
June 9, 2005

The Nature Editorial abstract at
http://www.nature.com/nature/journal/v435/n7043/full/435713a.html
says in part "… If Fermilab builds the ILC, it will regain its position at the forefront of international science; failure could lead to staff reductions and intellectual atrophy. … the Large Hadron Collider (LHC) [is] due to enter service in 2008. …".

If Fermilab succeeds in getting the ILC, then it may be prosperous for many years.
However, if Fermilab's ILC efforts fail, Fermilab has only 2005-2008 of lifetime left before obsolescence.

In the latter event, there is in my opinion a way for Fermilab to go out in glory instead of dying with a whimper: consider non-consensus interpretations of existing Fermilab data.

The Fermilab T-quark data show 3 peaks at (roughly):
130-150 GeV (low)
160-190 GeV (middle – containing the usual consensus 173 GeV value)
200-240 GeV (high)

These values of T-quark masses can be seen in terms of a plot of the Higgs mass vs. the T-quark mass with vacuum stability and triviality boundary lines show, as done by Froggatt in http://xxx.lanl.gov/abs/hep-ph/0307138

A simple Nambu-Jona-Lainio model gives a 145 GeV T-quark, consistent with the low value. (See Yamawaki in http://xxx.lanl.gov/abs/hep-ph/9603293)
The corresponding point on the Froggatt diagram is well within the usual region with respect to triviality and vacuum stability.

An 8-dimensional Kaluza-Klein Nambu-Jona-Lasinio model gives a 172-175 GeV T-quark, consistent with the middle (and usual consensus) model.
The 8-dim Kaluza-Klein is similar to the model of Batakis in Class. Quantum Grav. 3 (1986) L99-L105, in which the compact 4 dimensions are CP2.
The corresponding point on the Froggatt diagram is on the vacuum stability line, possibly indicating that it is on the line between a 4-dim vacuum and an 8-dim vacuum, in which case the T-quark data might be used to study the higher Kaluza-Klein dimensions.

A Bardeen-Hill-Lindner model, similar to a gauged Nambu-Jona-Lasinio and four-Fermi model, gives a 218 GeV T-quark, consistent with the high value. (See Hashimoto, Tanabashi, and Yamawaki in http://xxx.lanl.gov/abs/hep-ph/0311165)
The corresponding point on the Froggatt diagram is the critical point at which the usual region is intersected by both the triviality boundary and the vacuum stability boundary.

How I view this stuff, with some pictures, are in a pdf file at http://www.valdostamuseum.org/hamsmith/YamawakiNJL.pdf which is a pdf version of my web page at http://www.valdostamuseum.org/hamsmith/Yamawaki.html

Roughly, it seems to me that further analysis of existing Fermilab data in the regions of all three peaks might give important insights into the mutual interactions of the T-quark, the Higgs, and the Vacuum, possibly shedding light on many important questions such as the origin of mass and possible higher Kaluza-Klein dimensions, all in terms of experiments that can be (and some of which have already been) done with realistic technology.

Please note that the above is only one proposal for how further analysis of existing Fermilab data might produce interesting physics insights that may or may not be seen at LHC, depending on how LHC selects its data by triggers, cuts, etc., and how LHC analyzes its data.
However, it is an example of how Fermilab might get some interesting and important results over the next few years even if the ILC is never built.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

Why No “New Einstein”?

June 9, 2005
Categories: Uncategorized

Lee Smolin has a piece in the latest Physics Today entitled Why no “new Einstein”?. Unfortunately it’s only available to Physics Today subscribers, although Lee tells me he will see if he can put it on-line on his web-page. Tony Smith previously mentioned this in a comment to an earlier posting.

The problem Lee addresses seems to me to be an extremely important one. Pretty much every knowledgeable particle theorist that I talk to these days, string theorist and non-string theorist, agrees that current ideas about how to go beyond the standard model are not working very well. Everyone hopes that some big new idea will come along and show the way forward, with people often wistfully speaking about how maybe some bright post-doc out there may be at this very moment working on the needed new idea. The problem with this is that what is needed is probably something quite different than any of the current popular research programs, and finding it may be difficult enough to require someone’s concerted effort over quite a few years. If this is so, it’s very hard to see how anyone on the standard career path in the US is going to be able to do this. A young post-doc here generally only has a couple years in between needing to apply for new jobs, and if he or she were to devote those years to working hard on a very speculative new idea, this would most likely be suicidal for their career.

Some will argue that young theorists should just try and work on speculative ideas in their spare time, spending enough time working on currently fashionable topics such as string theory to impress people enough to ultimately get a permanent job, at which point they can work more seriously on their speculative idea. The problem with this is that getting up to speed and participating in the latest trendy research in string theory is a very demanding task, one that isn’t likely to leave much time or energy for other projects. In addition, it’s not at all clear that being willing to work hard on an obviously failed research program like string theory is consistent with having the intelligence and drive needed to do something really new. Instead of working on string theory, a young theorist could try and work on one of the other popular topics such as cosmology or phenomenology, but these are very different subjects than fundamental work in quantum field theory. A young theorist would be more likely to be able to find the necessary time if he or she went to work as a night-time security guard.

Lee makes several excellent proposals about how to restructure the way hiring is done to encourage young people who want to try something new. I hope he has some success in getting the powers-that-be to realize what a serious problem the field is facing and take some of the actions he suggests.

Two completely unrelated topics:

Lubos Motl has a posting about the Harvard Commencement, where it seems they’re giving Witten an honorary degree (Columbia already did this in 1996). He also writes about a new web-site for the Sidneyfest, the conference in Sidney Coleman’s honor that was discussed here and on many other weblogs. The new web-site includes copies of letters to Coleman from people who couldn’t attend the conference. In one of them Greg Moore recalls and reproduces Coleman’s proof from the late eighties that string theory is the unique theory of nature.

For something pretty weird, see this from the latest Notices of the AMS. There’s more about the activities of its author on Robert Helling’s weblog. The new issue of the Notices also contains an article about the 2006 NSF budget request for mathematics.

Update: Lubos Motl has his own comments on Smolin’s article, together with a link to some site where someone seems to have posted the article without attribution.

Comments

1. **Peter**
   June 15, 2005

   Good idea, I’d been thinking of doing that.

   I have been deleting some comments, but am trying to err on the side of letting people say what the want to say.

2.
   June 15, 2005

   Peter,

   Could you put the text of Lee Smolin’s reply letter in your next blog entry? It is buried
   here among the postings of loonies and
   it deserves to be more widely known (at
   least parts of it) as it helps to understand
   his position and motives better. If that is
   impossible, can you at least remove the
   two postings right after that letter that are
   stupid, offensive and simply indecent.

3.
   June 15, 2005

   What’s wrong with anomalies? Sure, it turns first class constraints into second class constraints, but Dirac showed us how to deal with that.

4. **Juan R.**
   June 15, 2005

   Well, I agree with **Ruadhan**

   Now it is a usual popular claim that Witten is the new Einstein or even the new Newton.

   Sincerely, that is stupid. I?m sorry but Witten is not a 10% of a Feynman, regarding to physics.

   Dan said

   1- “there is no current experimental data that clearly goes beyond the SM and GR.” This is not true. There is experiments and data. SM was designed for typical accelerator physics experiments, if one continue to test it one probably fin nothing. Apply the SM to a molecule, for example, and after we will talk about that.

Has you computed the total orbit of Mercury using full GR (including non radial components)? Orbit programs use only some GR effects in perihelion and light deflection but ignore time delays in the computation of orbit. Why?

2- "theories that go beyond SM and GR such as ss/m-theory make predictions (10D-SUSY) that are not currently testable." Incorrect again. Alternatives to GR are doing predictions about the future Gravity Prove B, alternatives to SM also can be verified in molecular experiments. There are proposals in literature for verifying the SM several orders of magnitude more exact that usual tests.

3- "there is no experimental evidence to guide theory." False again, there are dozens and dozens of current anomalies in data that guide to us to new theoretical frameworks. For example, certain anomalies in tomahawk data arise from new forces do not predicted by QED.

In a recent Physical Review D:

"*may reflect departures from both Newtonian gravity and GR on galactic and larger scales. Now alternatives to GR are traditionally required to possess an Newtonian limit for small velocities and potentials… also raises the possibility that the correct relativistic gravitational theory may be of a kind not considered hitherto*."

A "new" einstein will make predictions like the universe is **not** 10D and SUSY, I am practically sure.

"same can be said for other approaches, such as LQG-volume and area operators." This is false; the existence of a quantum of volume and area can be proved. In fact, it has been theoretically proven that no existence of quantum invalidates some well-known experimental data. I mean an indirect verification of the quantum not one direct test (at least i don't know any), somewhat like curved spacetimes in GR are not directly measured but compatible with many data.

5. **dan**
June 15, 2005

I am not entirely certain how physics can benefit from a "new" einstein since the original einstein made specific predictions that were soon testable,einstein attempted to explain discrepiences with experimental results and known physics of his time, a new einstein would have the following problems

1- there is no current experimental data that clearly goes beyond the SM and GR and

2- theories that go beyond SM and GR such as ss/m-theory make predictions (10D-SUSY) that are not currently testable.

3- there is no experimental evidence to guide theory.

if a "new" einstein makes predictions like the universe is 10D and SUSY — there would be no way to confirm it.

(same can be said for other approaches, such as LQG-volume and area operators).

so i don't entirely understand smolin's point. maybe the "new" einstein is alive and publishing and his name is witten, but we don't have the technology to test witten's theories.

6. **Thomas Larsson**
June 14, 2005

Dear Lee,

Let me emphasize that I am not a string theorist – on the contrary, over the last years I have had strong disagreements with Lubos and others, especially over the role of diff anomalies. While initially a statistical physicist, the success of CFT made me interested in Lie algebras, where I discovered how to generalize the Virasoro algebra beyond 1D and developed its representation theory, together with mathematicians like Moody, Rao, Berman and Billig. In particular, the Virasoro algebra in 4D is the anomalous form of the algebra of 4-diffeomorphisms, which is the constraint algebra of GR in covariant formulations (in non-covariant canonical quantization the constraint algebra is modified).

So when I speak about diff anomalies, I do it as someone who has developed new mathematics which has not been absorbed by the physics community. You may wish to keep that in mind.

As to the issue of anomalies, i.e. the claim that we ignore the established knowledge that ?INFINITE-DIMENSIONAL CONSTRAINT ALGEBRAS generically acquire anomalies on the quantum level…? is simply false. It is contradicted by rigorous existence and uniqueness theorems in LQG.

Whereas I claim that this is true, it is not at all generally accepted. On the contrary, it is widely asserted that there are no pure gravitational anomalies in 4D, see e.g. Weinberg's QT of F II, ch 22. Nevertheless, the constraint algebra of GR contains many subalgebras isomorphic to the infinite conformal symmetry in 2D, generated by vector fields of the form f(z) d/dz, where e.g. z = x^0 + ix^1 or z = x^2 + ix^3. Upon Fock quantization, these conformal subalgebras will in general acquire anomalies for the usual reason, making the whole shebang anomalous.

The reason why these anomalies cannot be seen in conventional field theory is that the relevant cocycles are functionals of the observer's trajectory in spacetime. Unless this trajectory is introduced and quantized in conjunction with the fields, the relevant anomalies cannot be formulated. This is IMO the crucial obstruction to the quantization of gravity.

1) The approach to quantization of constrained systems is different in string theory and LQG. The former approach depends on a gauge fixing that refers to a fixed background metric. It results in the construction of a Fock space. The latter is background independent and involves no background metric, no gauge fixing and results in a state space unitarily inequivalent to a Fock space.

2) There is a body of rigorous results that support each kinds of quantization. Hence it cannot be a question of which is correct mathematically. Both are correct, within their contexts. It is a question only of which construction is appropriate for which theories and which describes nature.

Conventional quantization has turned out to describe nature in other contexts. I think this is a good reason to believe that it is the correct approach. In particular, CFT has been successfully applied to 2D condensed matter, where conformal anomalies have been measured experimentally. This is of course a different context and not directly relevant, but this fact has shaped my basic instinct that anomalies are very real things which cannot depend on the quantization method used.

3) The treatment of constraints in string theory depends on certain technical features of 1+1 dimensional theories, particularly the fact that there is a gauge in which L_0 plays the role of a Hamiltonian and therefore should, in that gauge, be quantized so as to have a positive spectrum. The anomalies are not generic, as asserted above, rather they depend on the additional condition that L_0 should be a positive operator.

Yes, this is the crucial point. In any physical theory, there should be some positive operator which can be interpreted as a Hamiltonian; there is a physical requirement that energy be bounded from below. Of course, in GR there is a Hamiltonian constraint rather than a genuine Hamiltonian. This is another reason to introduce the observer's trajectory; you can define a genuine Hamiltonian as the operator that translates the fields relative to the observer.

Anyway, in all applications of Lie algebras to physics so far, the reps have been of lowest-weight type. At least for finite-dimensional Lie algebras, all unitary irreps are of this type.

There are other reps of Diff(S^1) that are non-anomalous but in which L_0 is not positive.

If you consider the restriction to the algebra of polynomial vector fields, generated by L_m with m >= -1, then all irreps have a vacuum vector (or are dual to such a rep).

So a choice is made in the standard quantization of string theory, which his motivated by the physics. This does not mean it is the right choice for all physical theories.

OK. I disagree.

4) Conversely the existence and uniqueness theorems which support the LQG quantization work only in 2+1 dimensions and above for the reason that gauge fields don?t have local degrees of freedom in 1+1 dimensions. The existence theorems tell us that there are quantizations in 2+1 and higher of diffeo invariant gauge theories that have unitary, anomaly free realizations of diffeo invariance. The uniqueness theorem tells us that the resulting state space we use in LQG is unique.

Contrary to string theorists, I claim that anomaly freedom is not a necessary requirement. To illustrate this point, let me again use the bosonic string as an example and quote from GSW, subsection 2.4: 'Classical free string theory can be consistently formulated for any spacetime dimension, but quantization with a ghost-free spectrum requires D less than or equal to 26. […] In the special case of D=26 and a=1 the spectrum is entirely tranverse, with many decoupled zero-norm states.'

Thus, D=26 is special, but D less than 26 is not ruled out by consistency requirements. It is only in 26D that it is possible to pass to the reduced Hilbert space by imposing the physical state condition L_m|phys)=0, but when D less than 26 this is not necessary, because the full, unreduced Hilbert space is already positive-definite.

Thus, my position is that some diff and gauge anomalies are good, making it possible to break diff and gauge symmetry on the quantum level, such as the string in D less than 26 illustrates. This does not mean that all gauge anomalies are good, of course. On the contrary, I recently gave a simple algebraic argument why conventional gauge anomalies, due to chiral fermions and proportional to the third Casimir, indeed are inconsistent. This argument does not apply to observer-dependent anomalies, which are proportional to the second Casimir.

The idea that diff and gauge anomalies may be consistent is of course very controversial.

With regard to the non-standard quantization, in which holonomies, but not local field operators are well defined, it is of course true that when applied to standard systems this leads to inequivalent results. ?This apparently leads to unphysical consequences, such as an unbounded spectrum for the harmonic oscillator.? But, give me a break, do you really think someone is proposing to replace the standard quantization of the harmonic oscillator with the alternative one? What is being proposed is that the quantization used in LQG is well suited to the quantization of diffeo invariant gauge theories.
In case it is not obvious, let me emphasize that harmonic oscillators are not relevent here, and can play no role in a background independent quantum theory, precisely because the division of a field into harmonic modes requires a fixed background metric. Thus, the physics of the problem REQUIRES an alternative quantization.

Sorry, but here I flatly disagree. I find it very disturbing that LQG methods yield the wrong result for the harmonic oscillator.

I am frankly puzzled why someone who claims to know the literature well would throw up examples like the harmonic oscillator up in this context. I can try to understand their point of view, but it certainly reads as if they either are choosing to ignore the basic point, which is that background independent quantizations cannot use fock space, or they are looking to make debating points to impress ignorant outsiders.

I agree that a diff invariant quantization of gravity cannot use Fock space, and I am convinced that such a quantization does not exist. However, a diff *covariant* Fock space quantization of gravity may very well exist. By this I mean a quantization in analogy with the string for D less than 26: the unreduced Hilbert space is consistent in itself, and diffeomorphisms are promoted to a genuine but anomalous symmetry acting on the full Hilbert space.

A step in this direction was taken in hep-th/0504020. Sure, there are problems: the (manifestly covariant) regularization has not quite been removed, no invariant inner product has been found, and no hard predictions have been extracted. But there is a Hamiltonian which is bounded from below in the regularized theories, the analogous construction for the harmonic oscillator has a spectrum bounded from below (it is not quite right, and I discuss why), and infinities cancel best (though not quite, so I am doing something wrong) in 4D. Most importantly, since phase space variables are promoted to operators in the usual way, this is genuine quantization, which is witnessed by the presence of anomalies.

Finally, I didn't express myself very well on the sociological issues. I agree with you about the problems with string theory, and I did not mean that funding to LQG should be stopped. However, given what I feel is a major problem (the harmonic oscillator spectrum), and that LQG already is the second biggest player in QG, I cannot really think that it is badly underfunded at present levels.

7.
June 14, 2005

Indeed, string theorists truely are legends in their own minds.

8. **Ruadhan**
June 14, 2005

Thomas Larsson said:
>It would be very wrong to dismiss string theorists as misguided fools or corrupt villains. There might be exceptions, but many of them are among the smartest physicists on this planet.

I smell moral pressure from string theorists on this issue. It is considered immoral to say that string theorists aren't superhuman geniuses. Let me say this: I have met and talked to many of them, and they seem to me to be no more intelligent on average than a typical mathematician or theoretical physicist. Just as it is OK to exaggerate how bad Hitler or Saddam Hussein were, for example by saying that they ate babies, it is OK to exaggerate how clever string theorists are. A person who says that Saddam didn't eat babies can be attacked for being a Saddam-sympathizer, and a person who dares to say that all of this "string theorists are all geniuses" talk is mere propaganda can be attacked for claiming to be more intelligent than string theorists, which nobody is entitled to do unless they know more than all the string theorists about heteroskedastic fibrations over David-Letterman manifolds.

String theorists aren't the smartest guys on the planet. They're just enmeshed in a macho culture where they have to claim to be super-geniuses, and they can conceal their mediocrity behind a cloak of gibberish which one must become a string theorist to see through.

9. **Ruadhan**
June 14, 2005

Lee said:
>Then, because of the possibility that quantum computers could break codes, there has been a lot of support for the last few years. And a lot of progress has been made, both experimentally and theoretically on aspects of foundations of QM.

It seems to me that any progress that has been made has been in the realm of taking the previously existing understanding of QM and applying it to new systems. People have talked about qubits and Shor's algorithm and quantum registers and have demonstrated quantum teleportation, but these are all straightforward applications of the previously known and well-understood formalism of quantum mechanics.
I think that this is no more indicative of progress being made on the foundations of quantum mechanics than the successful factorization of a large number constitutes progress on the foundations of arithmetic.

An example of a genuine non-trivial thing that quantum mechanics says is the following:
Suppose there are N experiments with the following properties:
1. Each experiment has only two possible results.
2. If we perform the same experiment twice, then we get the same result.
3. If we know in advance with certainty that a particular experiment will give a particular result, then the probabilities of the possible results for all other experiments are 50%.

Then N is less than or equal to three.

Nobody has ever attempted to address questions like why this should be true. Instead, it seems that the quantum computation people have agreed amongst themselves that they like the many-worlds interpretation and have left the foundations there.

10.
June 14, 2005

gold

11. **Lee Smolin**
June 14, 2005

Dear Peter and colleagues,

I am grateful for the attention given to my essay. I only want to emphasize a few points here. The main thing is that the essay is carefully written. It does not advocate more funds to LQG or any other program. It explicitly advocates more support and positions for young, ambitious theorists pursuing their own research programs who are unaffiliated with any larger program. Several proposals are made for how to accomplish this. I would hope that the focus of the discussion could be on these proposals.

-String theory is criticized in the essay mainly because it is currently sociologically dominant, and so subject to the problems mentioned. It was necessary to do so as many readers of physics today will be unfortunately unaware that there are any problems with string theory, or any viable alternatives. Anyone with a long enough memory will know that the sociological issues in high energy theory predate string theory, and have hurt physics in the past, i.e. in the case of S-Matrix theory.

-I hope I don?t have to say that I am not anti-string theory. My current last paper on the ArXiv is a technical paper in string theory, and I have 14 more in past years, plus 8 papers on related topics such as the landscape. I wouldn?t have written these papers if I didn?t think there was a good chance string theory is relevant to nature. The fact that someone like me who contributes sometimes, but not exclusively, to string theory, is not considered ?a string theorist? is part of the sociological problems my essay criticizes. Similarly, the fact that one can elicit angry responses, and be called ?anti-string? for carefully and correctly recounting the actual status of various conjectures is a sign of an unhealthy sociology. No one calls someone anti-LQG or anti-QCD when they do a similarly honest summary of what is known and not known in those fields.

-I would claim that the sociological issues mentioned in the essay have hurt string theory even more than they have hurt the alternative programs, because they greatly limit the range of ideas worked on, and because people with a lot of imagination and intellectual independence are either selected out or choose themselves to work within communities which are more friendly to diversity and imagination. As a result, key issues such as the question of a background dependent formulation, or perturbative finiteness, don?t get a lot of attention, in spite of their centrality for the whole program.

-I was grateful that someone noted the range of subjects at the LQG meetings. This was not planned, it is a natural outcome of the more open and curious atmosphere among people who work on the subject. We don?t believe we should have a meeting without inviting people from alternative and rival programs to report to us what they are doing, as well as to serve as critics. At the meeting in Marseille last May we even invited a persistent critic of LQG-Ted Jacobson-an early contributor who is now very critical of the subject-to give a talk to lay out his criticisms. I think it would be very good for string theory if the organizers of their meetings took a similar attitude.

-Someone asked for a blanket term for LQG, CDT, causal sets etc. We use background independent approaches to quantum gravity. There is a lot of interchange of ideas, techniques and people among these programs, and many of us have contributed to more than one. There is a very different intellectual climate, in which diversity, creativity and independence are strongly encouraged.

-Someone is asking for what is ?LQG proper?? But the fact is that a lot of different things are now going on roughly under the name of or related to LQG. After all, this is now a community of > 100 people and there is no orthodoxy and no one trying to control what people work on. We agree generally on what has been achieved and what problems remain open, but not much beyond that. There is a healthy variety of approaches and attitudes towards the open problems. If there is one thing we all agree on it is that no approach is likely to achieve the right theory that is not background independent at its foundations. Come to the meeting and see what is happening.

-While the point of my essay was not to advocate more funding to any particular direction, if you ask me I will of course say that I think that people working on background independent approaches to quantum gravity deserve much more support. Among them are Loll and Freidel, that I am glad someone mentioned, but there are many others.

-I did not, as Lubos implies, advocate funding a large number of people who do nothing but think about the foundations of quantum theory. What I do advocate is much more support for the kind of person who might be inclined to work on foundational issues. These are deep and independent thinkers who believe that the road to progress in physics is confronting the hard problems directly. But there is no need to argue about whether more funding for foundations of quantum mechanics would be fruitful. The experiment has been done. For decades there was no support at all, and slow progress. Then, because of the possibility that quantum computers could break codes, there has been a lot of support for the last few years. And a lot of progress has been made, both experimentally and theoretically on aspects of foundations of QM.

-Although this essay was not written to advocate LQG, since it is attacked in response I should try to clear some things up. Someone asks for an accounting of the present status of the field. I among others, have given one in hep-th/0408048, shortly to be updated.

As to the issue of anomalies, i.e. the claim that we ignore the established knowledge that ?INFINITE-DIMENSIONAL CONSTRAINT ALGEBRAS generically acquire anomalies on the quantum level…? is simply false. It is contradicted by rigorous existence and uniqueness theorems in LQG. As a few people do nevertheless take this seriously let me start from a point we can agree about and see if we can clear this up for good. I would hope we can all agree that:

1) The approach to quantization of constrained systems is different in string theory and LQG. The former approach depends on a gauge fixing that refers to a fixed background metric. It results in the construction of a Fock space. The latter is background independent and involves no background metric, no gauge fixing and results in a state space unitarily inequivalent to a Fock space.

2) There is a body of rigorous results that support each kinds of quantization. Hence it cannot be a question of which is correct mathematically. Both are correct, within their contexts. It is a question only of which construction is appropriate for which theories and which describes nature.

3) The treatment of constraints in string theory depends on certain technical features of 1+1 dimensional theories, particularly the fact that there is a gauge in which L_0 plays the role of a Hamiltonian and therefore should, in that gauge, be quantized so as to have a positive spectrum. The anomalies are not generic, as asserted above, rather they depend on the additional condition that L_0 should be a positive operator. There are other reps of Diff(S^1) that are non-anomalous but in which L_0 is not positive. So a choice is made in the standard quantization of string theory, which his motivated by the physics. This does not mean it is the right choice for all physical theories.

4) Conversely the existence and uniqueness theorems which support the LQG quantization work only in 2+1 dimensions and above for the reason that gauge fields don?t have local degrees of freedom in 1+1 dimensions. The existence theorems tell us that there are quantizations in 2+1 and higher of diffeo invariant gauge theories that have unitary, anomaly free realizations of diffeo invariance. The uniqueness theorem tells us that the resulting state space we use in LQG is unique.

5) Now it is true that Starodubstev and Thiemann have found it an interesting exercise to apply the LQG techniques to free string theory. Not surprisingly they get a theory that is unitarily inequivalent to the usual one. This does not mean that the usual quantization of string theory is wrong, nor does it mean that the LQG techniques are wrong when applied to other problems, where the existence and uniqueness theorems together with a large number of results prove their worth. All we learn is that the two quantizations are inequivalent, which was to have been expected.

6) With regard to the non-standard quantization, in which holonomies, but not local field operators are well defined, it is of course true that when applied to standard systems this leads to inequivalent results. ?This apparently leads to unphysical consequences, such as an unbounded spectrum for the harmonic oscillator.? But, give me a break, do you really think someone is proposing to replace the standard quantization of the harmonic oscillator with the alternative one? What is being proposed is that the quantization used in LQG is well suited to the quantization of diffeo invariant gauge theories.

In case it is not obvious, let me emphasize that harmonic oscillators are not relevent here, and can play no role in a background independent quantum theory, precisely because the division of a field into harmonic modes requires a fixed background metric. Thus, the physics of the problem REQUIRES an alternative quantization.

The detailed motivation is, I think, well argued in the papers, and are supported by the results as well as the existence and uniqueness theorems. First, is well known that a complete coordinatization of the gauge invariant configuration space for a non-Abelian gauge theory requires the holonomies. Second, using them gives rise to the unitary non-anomolous reps of the spatial diffeomorphisms.

Nor is anyone proposing using non-seperable Hilbert spaces for the full theory, the point is that when one mods out by the piecewise smooth spatial diffeos one is left with a seperable Hilbert space.

I am frankly puzzled why someone who claims to know the literature well would throw up examples like the harmonic oscillator up in this context. I can try to understand their point of view, but it certainly reads as if they either are choosing to ignore the basic point, which is that background independent quantizations cannot use fock space, or they are looking to make debating points to impress ignorant outsiders. They must know comments like this are not going to influence experts, because they are, after all, taken from our own papers, written precisely because we wanted to clarify the difference between the new and standard quantizations and the limits of the applicability of each.

With regard to the sociology of the string-loop division, ?Roughly speaking, string theorists are fundamentally particle theorists with a strong understanding of quantum theory, whereas loop people are gravitists with a background in GR?, this is a myth. Rovelli, myself and many other people in LQG were trained as particle physicists, myself at Harvard in the late 70?s. Most of the physical motivation for LQG comes directly from ideas about formulating gauge theories in terms of loops that were studied by Polyakov, Wilson, Migdal, Mandelstam, Neilsen and others. LQG is squarely an outgrowth of their intellectual tradition. The only thing we added was to correctly treat the diffeomorphism invariance exactly in the quantum theory. This led to new results just as the exact treatment of gauge invariance in lattice gauge theory led to new results. I would claim that we made progress in LQG precisely because we had a very good grounding in QFT.

String theory, as it is practiced, makes much more contact with the general relativity tradition, especially the once discredited tradition of extending general relativity to add dimensions and degrees of freedom in the search for a unified field theory. You are much more likely to read a paper which studies solutions to a generalizationsof the Einstein equations, with hbar=0, by a string theorist than by someone working on a background independent approach to quantum gravity.

This of course does not mean that string theory is wrong. But I believe it does mean that by enforcing a narrowly restrictive notion of what constitutes good work, the community of string theorists has hampered progress in string theory by excluding from consideration the lessons learned by attempts to do what string theory must do eventually if it is to be a real theory: which is to find a background independent formulation of a quantum theory of spacetime.

12. **Curious**
June 14, 2005

Well said, M. I think that we have reached the
common ground, and the discussion was
useful. A revolution can start with a new Einstein —
or with less imaginative new Plank and Bohr —
or nonimaginative new Rutherford and Michelson. Eihter outcome is fine and useful
to physics. What is needed is a large
pool of technically savvy receptive people
who can carry it out when it finally starts.
Thanks to string theory we have this pool.
You may not like the theory but it keeps
able people busy and sharpens their skills;
it is superb mental gymnastics and it will
be useful in the end. Some diversification
would not hurt, but in all probability people
will end up exploring not one but several
dead ends. They will also stop understanding
each other completely, just like LQG and string people. Groupthink will not be breached; there will be more insular groups.
But that's nor here nor there.

M is also right that the best way to bring the true revolution about is, yes, to revise
the fundamental assumptions. My own prejudice which I share with Einstein is that GR is not the correct classical
theory of gravitation (and that is why it is so
difficult to quantize). Experimental verification

of GR is still a work in progress; we can wait.
It will all sort out in the end.

Thank you, M, for your contribution to this discussion, and thank you, Peter, for bringing the subject up.

13. **mortain**
June 14, 2005

In addition to the traits listed below as contributing to Einstein's success, wouldn't it be pertinent to also add that he – like many other pioneers and lone wolves, such as Newton, Wiles and Perelman – had the time to pursue problems which interested him? Unlike Newton's day and Einstein's day, perhaps the only way to ensure that one has the time to work on (and eventually solve) outstanding, significant problems in any subject in our era is to make the time for oneself. Einstein may have had time to theorize at the patent office. Newton had free time forced upon him by an outbreak of bubonic plague. As Peter asserted, it may take seven years to solve a major academic problem – but this would surely require full-time work upon it. Wiles may be an exception, but then again he was aware of the problem he wanted to solve from a young age.

Furthermore, I thought Einstein only graduated from the modern equivalent of a technical college with a diploma, and never went to anything approaching a graduate school.

14. **Quantoken**
June 14, 2005

Thomas Larson: "It would be very wrong to dismiss string theorists as misguided fools or corrupt villains. There might be exceptions, but many of them are among the smartest physicists on this planet"

BUT the SMARTEST physicists CAN ALSO BE completely misguided fools. In another thread some one said: "…that it was pity the Creator had not taken advice". The reason the **Creator** did not take advice was the pity fact that smart people like Edward Witten wasn't born yet to give the **Creator** some advices how the universe should be created and how many dimentions it should have.

Seriously, the nature is the way the nature is and you can not treak the nature to fit your theoretical model no matter how smart you could be. If the universe is not 10-D, Edward Witten can not turn it into 10-D no matter how hard he tried. The pity things is at the end of day it may very well turn out all wrong and Edward Witten's would have wasted some of the most intelligent minds on this planet in vain.

BTW, Einstein was never super smart or super intelligent. He was average in lots of aspects. But one thing that makes him great is he could **think outside the confinement of the box**, and he pick apart common ideas that other people took for granted, and find that these ideas were really not meant to be taken for granted, and he happened to be correct!

Quantoken

15.
June 14, 2005

…seem quite promising to me, e.g. dynamical triangulations. My comments referred to LQG proper. The papers I have looked at may be a few years old, but there has hardly been any changes in the fundaments since then.

Posted by: Thomas Larsson at June 14, 2005 03:16 AM

We obviously need better terminology, Thomas. Who is it that you are referring to when you say "LQG-people"? What current research to you mean when you say "more money to LQG"?

However, the suggested cure, which more or less explicitly reads more money to LQG, is problematic since many people feel that the LQG people are ignoring well-established facts.

Posted by: Thomas Larsson at June 12, 2005 06:13 AM

Two places–Hermann Nicolai's department at AEI and Smolin's Perimeter Institute–are centers where there are a lot of LQG people. I am not sure there is anyone at either place doing what you would call "LQG proper".

Perhaps it would give some substance to your comments, so I could better tell what you mean, if you would name some persons at AEI or PI who have posted "LQG proper" work on arxiv in the past year.

Since I hold you in high regard, I am quite curious to know what specific recent papers, say by AEI or PI people, you would pick to typify "LQG proper". This would typify the kind of research for which Smolin's essay, in your view, "more or less explicitly" advocates more funding.

16. **Thomas Larsson**
June 14, 2005

To the anonymous poster (Marcus?):

I have not said that LQG should be deprived of money. However, it is the second best funded approach to quantum gravity, and it has recently been well advertised in the press.

I think I have a reasonably good ideas what loop people are up to. There are several research programs present at Loops 2005, some of which seem quite promising to me, e.g. dynamical triangulations. My comments referred to LQG proper. The papers I have looked at may be a few years old, but there has hardly been any changes in the fundaments since then.

After the Loops meets Strings conference at AEI Potsdam, several string theorists have examined LQG closely from their viewpoint, Nicolai et al. but also Helling-Policastro and spr comments by Urs Schreiber. Although there might be some Schadenfreude in these papers, I believe that the critique voiced therein is serious and seriously meant, and it should not be dismissed easily. In particular, the second paper compares conventional and LQG quantization. To quantize in the usual sense, we start from some phase space and replace the q's and p's by operators and Poisson brackets by commutators. However, the q's and p's do not exist as proper operators in LQG (not both of them anyway), but only in exponentiated form. This apparently leads to unphysical consequences, such as an unbounded spectrum for the harmonic oscillator.

It would be very wrong to dismiss string theorists as misguided fools or corrupt villains. There might be exceptions, but many of them are among the smartest physicists on this planet (though I may have made some hasty comments sounding differently in the heat of debate). I have even found myself in agreement with Lubos Motl a lot recently (on physics, my opinions about politics are uninteresting). It is really only when he makes these weird statements about string theory, like it is immensely successful, predicts the standard model, and is already proven correct, that I disagree, and he has not made so many claims of that nature recently.

Roughly speaking, string theorists are fundamentally particle theorists with a strong understanding of quantum theory, whereas loop people are gravitists with a background in GR. So when string theorists say that LQG is quantum theory only in a weak sense, I think one should listen.

The importance of diffeomorphism group in GR is hardly in question, whether you call it general covariance, diff invariance or background independence. In fact, the lack of background independence is often used as an argument against string theory.

Perhaps I am barking up the wrong tree, but at least I have found my own tree to bark at. In a time when genuinely new ideas are scarce, this is at least something.

17.
June 14, 2005

M said:

[2] the key problem is that we are making flawed assumptions about the form (or ingredients) of a more fundamental theory, in which case the failure is one of imagination or insight rather than effort.

M, let's look more closely at your second cause which you call "[2]" and consider the possibility that it is especially operative in the US. I will explain but first look at this example of a paper by someone at Lyon (FR) and two people at Cambridge (UK)

http://arxiv.org/gr-qc/0506067

For context, glance at the abstracts of the four papers Laurent Freidel has written this year.

http://arxiv.org/find/grp_physics/1/au:+Freidel/0/1/0/all/0/1

I would say that the scientific establishments that support this are able to take general relativity seriously and pursue, with some of their resources, the idea that one should FIRST get a quantum spacetime geometry, a background independent, nonperturbative quantum gravity, and then, when one has quantum spacetime dynamics on a continuum, which will be a mathematically new continuum, THEN one can build on that a new field theory and a new standard model. So the program is to get a RELATIVISTIC QUANTUM PHYSICS but not special relativistic like earlier quantum field theory, not merely built on Minkowski space, but the real thing: a GENERAL relativistic quantum physics built on a continuum with no prior commitment to some fixed geometry.

For whatever reason that agenda seems to be understood by the establishments outside US and people like Freidel can work in Canada, Germany, France, UK.

Let us suppose that a scientific establishment which only knows to go directly for the UNIFIED THEORY therefore SCREWS UP because it is incapable of supporting work like Freidel's, because it only knows to try for a more fundamental theory in the way that STRING TRIED, on manifolds with a prior fixed metric, or on Minkowski space. We can entertain the idea that this kind of establishment screws up because of a flawed vision: because it DOES NOT TAKE SERIOUSLY the 1915 lesson of General Relativity of gravity as geometry. Because of this limited vision it is unable to support people like Laurent Freidel doing spin foam + matter, and people like Renate Loll doing CDT computer simulations of quantum universe. So Loll has to be at Utrecht (at Gerard 't Hooft's institute) and Freidel has to be in Canada or France, and his co-authors have to be at Cambridge UK and Loll's co-authors have to be at Copenhagen and Potsdam and Krakow Poland.

BECAUSE THEY AND THEIR LIKE CANNOT WORK IN THE USA because the establishment people cannot imagine to quantize Gen Rel FIRST and get a quantum spacetime dynamics and THEN on that new continuum to build a unified theory—-a relativistic quantum physics.

That is how I see your "cause [2]". there is no mystery about good ideas, progress towards a more fundamental theory is going WELL at the moment, there have been surprising developments in the past couple of years. But the US is somewhat retarded about noticing and lacks the preparation to hop on the bus. So Smolin essay is pointing out not that he or his postdocs are unhappy (they can always go back to Europe or find jobs in Canada) He is pointing out to US academics that the US is falling behind in theoretical physics (in quantum gravity and other branches that matter, not string theory)

The reason Smolin would say to diversify the programs in US departments, and to tune the support system more towards resourceful mentally independent people is NOT so that the fundamental problems of physics will be solved (that view is a bit US-centered, if necessary they really can do without us, if the US stays stuck on string there still are other establishments smart enough about placing their bets with young people who can eventually make whatever new physics is needed).

The purpose of doing what Smolin suggests and diversifying programs in US departments and institutes etc, and tuning the support system more for individuals IS SO THE US CAN CATCH
UP.

Well that is my take on it, M. At least for tonight.

18. **M**
June 13, 2005

Anonymous wrote:

Maybe by tinkering with the support system you can get more people who are intellectually independent, resourceful, inventive, motivated to solve fundamental problems, and who take seriously (possibly conflicting) well-established physical principles.

I don't know whether there is a clear answer to the question of whether an improved "support system" will lead to an influx of the kinds of new ideas that are needed. It certainly seems clear (to me at least) that a friendlier intellectual and funding environment will bring greater diversity, but whether this diversity actually accomplishes the desired goal of reinvigorating fundamental theoretical physics with productive new directions is much less clear. After all, the problem seems less one of "not enough new ideas" as one of "not enough productive new ideas."

A key question is what is the origin of the shortage of ideas today. Perhaps it is because

[1] the key problems are *hard*, and so the only failure (if it is to be called that) is insufficient effort;

or maybe it is because

[2] the key problem is that we are making flawed assumptions about the form (or ingredients) of a more fundamental theory, in which case the failure is one of imagination or insight rather than effort.

It seems impossible at this point to really know which of these (or maybe some an entirely different cause) is where the greatest problems lie. It seems clear that since the standard model solidified, many very smart people have attacked the problems from a (admittedly limited) number of angles, and thus far none has resulted in anything that looks like a genuine success. If explanation [1] is correct, then I would agree with Curious when he says,

Whether a testable physical theory is the result of tinkering of many or a protean tour de force by a single genius is of little importance.

Chris Oakley seems to concur when he says,

I would therefore advocate the relatively unambitious program of examining weaknessness in existing theories, or in one's understanding of existing theories and seeing if one can patch these up in any way.

Certainly it is hard to argue with the statement that as long as you stick close to what is known your effort is less likely to be wasted! But whether that will be enough to break the logjam is an entirely different question…

On the other hand, if explanation [2] is right then maybe it will take an original thinker to take the first steps. Once that is done, I believe there are enough very capable and willing physicists to flesh out a new theory, much as was done during the development of quantum mechanics or the standard model. In this sense, such an original physicist *would* resemble an artist of exceptional ability — an ability to see past what seems self-evident with the necessary insight to capture the essence of what is needed.

Personally, I think that some of explanation [2] is valid. Given the enormous effort by many people of very high caliber over a long time, pursuing a number of different paths, it seems to me that some significant assumptions are being made that aren't quite right. I would certainly argue that the theoretical edifice that has been empirically verified thus far will remain intact at least in the form of effective theories; one can't argue with success, and the successes of the standard model and GR, for example, are pretty evident. But it seems quite possible that some of the assumptions that went into the construction of these key theories were not entirely right or fundamental, and it is our mostly unquestioning assumption of the necessity of those assumptions that could now be keeping us from making significant new progress toward what will be an eventual new theory. This is speculation, of course. And even if it is correct it does not mean it would take another Einstein to take the first key steps.

19. **Chris Oakley**
June 13, 2005

I think Curious's comments are quite apropos. One reason why the inventors of quantum mechanics and relativity disappeared from view in the latter half of the 20th century was that they were looking for another revolution. Unfortunately for them, quantum mechanics and special relativity turned out to be pretty good, and not in any obvious need of revolutionising. And – whether the practitioners admit it or not – overambition has also been the bane of string theory. Try to do too much and you will end up doing nothing. And if they say, "At least we showed that XYZ cannot be made into a physical theory", one should simply point out that the universe of theories that do not work is potentially infinite, and creating new rays within it not an especially good use of one's time.

I would therefore advocate the relatively unambitious program of examining weaknessness in existing theories, or in one's understanding of existing theories and seeing if one can patch these up in any way. It may *sound* boring, but at least one is morely likely to have something to show for it.

20. **Alejandro Rivero**
June 13, 2005

And I think what M says he "doesn't buy into" boils down to the essay TITLE.
Yep, the title is misleading. One must notice that Smolin has suffered more pression about the concept of "New Einstein" because a popular magazine charged him with such title, some years ago. As I told before I had preferred "a new Sommerfeld" or something so.

21. **Curious**
June 13, 2005

I risk to repeat myself, but I would like to answer M and also once again turn the
discussion to two inter-related topics: (a) are any
administrative or reorganizational actions
on the part of physics community
like those advocated by Smolin
would lead to the emergence of a new Einstein? and (b) Do we need this new Einstein?

We all know that none of Smolin's suggestions will bring a new Einstein to
existence. Useful as his advice may be
in improving the climate in which the research
is done, there is no guarantee that such a
climate would help to bring this peculiar goal about.
Neither it will help to make people more
open to radical ideas, as that would require changing the human nature. Only a great crisis in physics
(say, nothing but SM from LHC) would do that.
Nobody and nothing stays in a way of creativity of today's generation of theorists: no gurus,
no cliques, no government policies, nothing.
It is all in the head. Independent thinkers
will overcome any pressure and any obscurity,
if their theories are correct. The best way
to bring Einstein is perhaps to cut federal
support completely; then only dedicated
people will remain and we will be back to the era of gentlemen scientists (when there was explosive development of science and true opennes to ideas); but I am not suggesting
that. The resistance does not matter.
Both quantum mechanics
and SR met tremendous resistance at
first. Did that stop Bohr or Heisenberg or Dirac? No, it did not. Just take my word, when the
correct theory emerges it will take us all by
storm, no matter what and who gets which grants
and how much, what cliques currently exist,
what opinion Witten has, etc. That happened
before and it will happen again.

M correctly points to the fact that highly talented, original thinkers make great advances in mathematics, music, etc. That whole new vistas can be opened by such people.

Symphonies and certain mathematical constructions would not emerge without them, ever. True.

But physics is not mathematics. Nor is it
artistry. Whether a testable physical theory
is the result of tinkering of many or a protean
tour de force by a single genius is of little importance. Quantum mechanics has been
developed in many small steps. So is QFT.
So is analytical mechanics. Laplace, Hamilton
etc were not Newtons or Einsteins but they
did a great job w/o which there would not be
Maxwells and Einsteins. I am not advocating team
work and doing TOE by a committee of
mediocrities. My point is different. We do not
need a new Einstein. We need 10 Schwingers, 5 Bethes, 6 Heisenbergs, 3 Bohrs, 7 Wigners, 4 Weyls and an occasional Landau. You can change this formula however you like. That would do, believe me, if these
people will pursue their interests with open minds. Please, do not wait for this new Einstein to appear and solve all the problems.
In physics, you do not have to be a genius to
do the work of a genius. Forget about the violin and the high moral ground; not needed for job.
You can do it too, just try.

22. **John Rennie**
June 13, 2005

Since we've mentioned physical intuition, can anyone tell me the physical interpretation of local gauge symmetry. Such Googling as I've done has failed to find anything beyond a comment (I'm afraid I forget the source) that "it represents hidden degrees of freedom in the theory".
Given how central the concept is to field theories I'm a bit surprised how much it is glossed over. Maybe the next Einstein could start by asking what physical mechanism is responsible …

23.
June 13, 2005

Thomas Larsson offered a nice concise argument here which I will try to respond to.

[[…For almost five years, I have publicly said that string theorists are barking up the wrong tree. Why should I now be afraid of saying that the much smaller LQG community is doing the same thing (a different tree, admittedly, but still wrong)? Basically I'm using the same argument. Diff anomalies simply must be relevant to the quantization of gravity, because GR has many constraint subalgebras isomorphic to infinite conformal symmetry in 2D, any the lesson from string theory is that these subalgebras generically have anomalies. And no, I certainly don't think that people at Loops 2005 neither appreciate diff anomalies nor know very much about them.

Posted by: Thomas Larsson at June 12, 2005 02:58 PM]]

"saying that the much smaller LQG community is doing":

Thomas, I do not think you know what the much smaller LQG is doing. I keep a casual watch on the literature and would say that those not involved with cosmology are mostly doing path integral-type stuff. You seem to think that your discussion of constraint algebra is relevant, but it is not clear why.

"Diff[eomorphism] anomalies simply must be relevant to the quantization of gravity, because GR has many constraint subalgebras isomorphic to infinite…"

Are you SURE that diffeomorphisms MUST be relevant? One center of interest in the LQG community is an approach where there is no differentiable manifold representing spacetime. No differentiable manifold means no diffeomorphisms. No diffeomorphisms means no anomalies.
In this approach (CDT) the continuum is approximated by piecewise linear manifolds. The behavior of the continuum is described in the limit as the size of the triangulation goes to zero, and the continuum in the limit is neither smooth nor of the same dimensionality at all scales.

It is not obvious why "diffeomorphism anomalies must be relevant to the quantization of gravity", especially in path integral approaches, or in approaches where there are no constraint algebras, or where there are no diffeomorphisms.

Since it is not obvious, perhaps you would like to give a detailed explanation.

Thomas you say Smolin's essay is "problematical" because following its results would result in more support for LQG people (doing whatever LQG people do these days).

You argue that these LQG people should not get more money because they do not know some things you know about diffeomorphism anomalies.

I get the impression that you do not know who the LQG people are or what they are working on. I have seen you sometimes citing some older papers of Thomas Thiemann and Abhay Ashtekar, papers which do not seem at all typical of either Ashtekar's current work or the research going on in the LQG community.

Maybe you should actually GO to the Loops 05 conference this October and see who actually are these people and what actually are their researches towards which you are making the blanket statement they should not get additional funding.

Then you might have a different idea of who is barking up what wrong tree. It might be you 🙂

24.
June 13, 2005

to return to the topic of Smolin's essay, M made a point on 11 June that I don't think got enough consideration

[[…

It seems to me that certain aspects of Einstein's personality are quite important to his ability to achieve the impact that he did. In my opinion the combination of several key traits was crucial:

1. Excellent physical intuition.
2. Great respect for established results. This characteristic strongly distinguishes serious scientists from crackpots.
3. Willingness to pursue an independent direction, but only when it was apparent to him that the conventional approach was inadequate.

4. Inwardly directed and motivated. He didn't let others decide for him what he should be working on or what approach was correct.
5. Tenacity. He didn't give up on problems he was interested in, neither because they were hard nor because others didn't buy into his program.

As for the connection with Smolin's article…

I think all of the above characteristics (and others) were very important to his success. I also think it is clear they came from inside him as an individual and were not the product of an especially nurturing environment or support system. None of his best work was done while he was at the IAS at Princeton, although at the IAS his environment offered an environment much closer to what Smolin advocates than the environment where he did his best work.

So, I guess I don't really buy into that part of Smolin's thesis that states "the system" shoulders much of the blame for "no new Einstein." If and when someone like Einstein comes along, he or she will succeed primary due to internal motivations and traits, not because Smolin's proposals have been put into place. Groupthink in physics departments and within funding agencies, hiring decisions based on having a clean record of "correct" research endeavors, and funding decisions based on their compatibility with fashionable research programs and directions are cancerous and harmful to the progress of physics, I agree. But do they prevent the appearance of a new Einstein? I really doubt it; I don't think obstacles in the system can stop someone like that…

Posted by: M at June 11, 2005 10:36 PM]]
****
My comment on M's post is
I would say that Smolin's essay has two distinct messages:

A. Diversify the theoretical research programs represented in departments, institutes, at conferences (incidentally giving young researchers more choice and better information about alternative lines of theoretical investigation). Arguments could be used similar to those applicable in the case of monopoly power and the suppression of economic competition.

B. Reallocate support more towards the individual researcher (with choice left open) and less by program. Perhaps this would mean longer-term postdoc fellowships awarded NOT on the basis of the pre-established program to which the recipient is indentured, but instead on personal criteria that allow more scope in the choice and invention of research goals.

And I think what M says he "doesn't buy into" boils down to the essay TITLE. And if so, I strongly agree. Part A, diversify programs with more competition between rival approaches, does not have any obvious connection with Albert Einstein in 1905.
Einstein is not a symbolic or informative example relevant to that.

Nor is his life a very good argument for Part B, says M, because Einstein represented a rare combination of intellectual and moral qualities which you can't get by tinkering with the system.

Maybe by tinkering with the support system you can get more people who are intellectually independent, resourceful, inventive, motivated to solve fundamental problems, and who take seriously (possibly conflicting) well-established physical principles. Maybe you can get more ordinary-caliber serious creative people, which would be great! Those people are not necessarily new Einsteins though. I am all for Part B of Smolin program. But the identification with Einstein seems corny.

25. **Kea**
June 12, 2005

Is that you, Marcus?

26.
June 12, 2005

Dan, you ask:
[[…let us suppose string theories' open problems continue to remain open, and that LQG is able to show it has a semiclassical limit in agreement with GR.
Would you predict a mass exodus from strings to loops? or is academia such that strings will remain dominant, and LQG marginal?]]

I take LQG broadly defined as "What LQG-people are doing" and as what will be the main focus at the Loop 05 conference. The Loop conference will feature CDT as one of its main attractions. For an idea of where it stands on "SEMICLASSICAL LIMIT" see this paper

http://arxiv.org/abs/hep-th/0411152
A semiclassical universe from first principles

For additional detail on this and other developments see this one:

http://arxiv.org/hep-th/0505154
Reconstructing the universe

Other basic CDT papers:

http://arxiv.org/abs/hep-th/0404156
Emergence of a 4D world from causal quantum gravity

http://arxiv.org/abs/hep-th/0505113
Spectral dimension of the universe

http://arxiv.org/abs/gr-qc/0506035
Counting a black hole in Lorentzian product triangulations

http://arxiv.org/hep-th/0105267
Dynamically triangulating Lorentzian quantum gravity

In this approach spacetime is not modeled by anything with a differential structure or smooth coordinates. For instance the largescale dimension can be 4D and the dimension decline continuously down to near 2D at very small scale. This is not what one expects with, for example, a differentiable manifold where the dimension must be a whole number corresponding to the number of coordinate functions.

this year we are beginning to see a shift of LQG-people's interest towards CDT. Actually it began around May 2004 with the Marseille LQG conference. this is a movement of interest within the general (broadly interpreted) LQG field. You speak of
an "exodus" from string but from my point of view this does not seem to matter as much as the shift of interest and resources inside LQG.

Dan you said:

[[I was thinking of spin network formulation, although one of Lubos' criticism is that there is no clear connection between the Hamiltonian formulation and spin networks. Is there are clear connection between canonical quantization and causal sets or dynamic triangulation?…]]

You may mean "spin foams" and not spin network since spin networks are basic to canonical-LQG (the formulation that requires a Hamiltonian constraint). Spin networks form the basis of the kinematic state space of vintage LQG.

On the other hand spin foams is a path integral approach and fortunately they are not equivalent. Lubos is quite correct that there is no clear connection! However this is hardly a criticism.
It just illustrates that the broad field of LQG contains several different approaches to quantum gravity.

Are their "clear connections" from spin foams to causal sets to dynamical triangulations? ABSOLUTELY NOT! It is just that similar skills and intuition apply and similar concerns and problems are important. And the same people (LQG-people) easily cross over and work on the various differnt approaches.
And the different approaches show up at the same conferences.

Vague connections yes. Formal logical provable connections no.

I should enter a disclaimer. I watch research in this field with considerable interest, as it is currently progressing rapidly and is exciting. But I am not an expert and dont do research in it. If I was to go into any kind of LQG (broad sense) research it would be CDT, without question. Very new so basically can get in at ground floor.

dan you say:
[[do you know what the current research in LQG is on the Kodama state, which according to Lee Smolin
http://arxiv.org/abs/hep-th/0209079,
has a good semiclassical limit with a positive cosmological constant. I am aware Witten has argued the state is unphysical.
http://arxiv.org/abs/gr-qc/0306083%5D%5D

these are old papers, a fair amount has been written about Kodama since then. probably arxiv search engine can find it. recent papers by Stephon Alexander and two people at UBC. use arxiv search to track them down.

27. **Peter**
June 12, 2005

Hi Dan,

I don't think that many string theorists will abandon string theory for LQG, even if everyone agrees that LQG has the right classical limit. For one thing, the whole landscape story shows that many string theorists would rather abandon doing science than abandon string theory. For another, many string theorists are, like me, basically particle theorists, and would take the attitude, that if LQG works it just means there are two ways to do quantum gravity, but since the string theory way is supposed to explain particle physics, it is better and they will keep doing it.

28. **dan**
June 12, 2005

Hello Pete,
short of a no-go theorem, I suppose the work on the semiclassical limit could continue on for an indefinite time. i agree with you though that the current commitment to string theory research is out of proportion to string theory's tangible physical results.

Based on Smolin's analysis of academia, let us suppose string theories' open problems continue to remain open, and that LQG is able to show it has a semiclassical limit in agreement with GR.

Would you predict a mass exodus from strings to loops? or is academia such that strings will remain dominant, and LQG marginal?

Hello At,

I was thinking of spin network formulation, although one of Lubos' criticism is that there is no clear connection between the Hamiltonian formulation and spin networks. Is there are clear connection between canonical quantization and causal sets or dynamic triangulation?

As for the spin network, do you know what the current research in LQG is on the Kodama state, which according to Lee Smolin
http://arxiv.org/abs/hep-th/0209079,
has a good semiclassical limit with a positive cosmological constant. I am aware Witten has argued the state is unphysical.
http://arxiv.org/abs/gr-qc/0306083

i know Smolin was workign on a paper in response to Witten, which John Baez pointed out, but what has become of this?

29.
June 12, 2005

Dan, here is the list of non-string QG topics selected for the October
"Loops 05" conference
http://loops05.aei.mpg.de/

Background Independent Algebraic QFT
Causal Sets
Dynamical Triangulations
Loop Quantum Gravity
Non-perturbative Path Integrals

It is not always clear how inclusively the term "LQG" is being used. Ten years ago LQG referred to a CANONICAL QUANTIZATION approach to GR, in which dynamics was to be implemented through the Hamiltonian constraint. In the mid-to-late 1990s LQG researchers experienced difficulty with the Hamiltonian and in particular with the associated constraint algebra. Largely on this account, many if not most LQG people moved over to path integral approaches including SPIN FOAMS, CAUSAL SETS, and DYNAMICAL TRIANGULATIONS.

Today, very few LQG are actively pursuing canonical quantization of GR. An exception is Thomas Thiemann at AEI who has something called the Master Constraint program.

There is a LQG program developed by Gambini and Pullin which looks somewhat like canonical LQG, but which has no constraint algebra and no Hamiltonian constraint. Its Hamiltonian is a discrete time-evolution operator.

Anyway we have a confusing abuse of language here. Almost everybody seems to use “LQG” as a blanket term for the non-string, mostly path integral, approaches to QG. There is no other widely recognized blanket term for what LQG people do!

But then some people, it would seem Thomas Larsson is a case of this, proceed to talk about the “LQG” in a much narrower sense, as if it were the canonical quantization program of the 1990s that has already been described in several books—the approach that encountered difficulty realizing Hamiltonian constraint dynamics.

Now I am wondering which “LQG” you mean, dan? Here is what you said:

[[..How many years are you willing to give “LQG” and its attempt to recover the classical limit before you deem it a failure? and if it is unable to find such a limit within a specified time period, would it be fair to say that LQG is as failed a project as string/m-theory.

Posted by: dan at June 12, 2005 03:08 PM]]

The non-string QG approach that many people would consider closest to “recovering the classical limit” would be CDT, I suppose (causal dynam. triang.) That was invented in 1998 (the year the first two CDT papers were published). It first produced a 4D spacetime in computer simulations in 2004. Smolin, who seems to represent “LQG people” for many of us, recently co-authored a CDT paper. There are indications that CDT will be one of the main points of discussion at Loops 05. I cannot say for certain that CDT has NOT “recovered the classical limit”.
Perhaps I could fetch some links to recent CDT papers and you could decide for yourself.

30. **Peter Woit**
June 12, 2005

Hi Dan,

First of all, unlike the case of string theory, I haven’t spent much time following the debates and arguments over LQG, so I don’t want to make any pronouncements about either its success or its failure. I’ve explained why elsewhere, basically it’s because I’m fundamentally a particle theorist. String theory has failed because it is now clear it can’t explain anything about unification in particle theory. LQG has never tried.

In the current situation, where there are no successful ideas about going beyond the standard model, everything people work on is not a success, and thus has experienced some degree of failure. This doesn’t mean people shouldn’t work on these ideas, maybe the failures can be overcome. But the amount of effort that has gone into string theory unification has been staggering, and has provided huge amounts of evidence that the idea can’t ever work. The effort going into LQG has been much more modest, and, although it hasn’t completely achieved its goals, it’s not implausible that with more work some new ideas will be found. I don’t think one can say the same about string theory any more.

If those working on LQG don’t make any progress during the next 5-10 years, and it really is true that they can’t get out GR in a the classical limit, I suspect most people working in the field will move on to something else. If instead they attract thousands of physicists to work on the idea, and have dozens of conferences each year, some with 500 theorists in attendance, if I’m still around I’ll be spending a lot of time complaining about their behavior.

31. **dan**
June 12, 2005

Hi Peter,
I do have a question for you,

I am sympathetic to your claim that String/M theory is currently a “failed” project.

What I wonder is whether you think LQG is a “failed” project as it has been unable to reproduce GR.

How many years are you willing to give “LQG” and its attempt to recover the classical limit befre you deem it a failure? and if it is unable to find such a limit within a specified time period, would it be fair to say that LQG is as failed a project as string/m-theory.

32. **Thomas Larsson**
June 12, 2005

Of course Smolin asks for a redistribution of funds from string theory to LQG. After bemoaning the string theory dominance, he complains how difficult it is to find positions for young people in technicolor, preon models, dynamical triangulations, causal sets, and LQG. How can this be construed otherwise than as a call for redistribution of funds from string theory to these areas, in particular LQG?

String theory surely has problems – there is no theory, no predictions and no experimental support. But this does not imply that large amount of funding should go to research aiming at disproving QM, GR or the standard model. OK, LQG does not aim at disproving QM, but claiming that anomalies can be arbitrarily dialled to zero by a new wonderful quantization method is not far from it. The existence of anomalies does certainly not depend on the quantization method; conformal anomalies arise also in the path-integral quantization of the bosonic string, in the form of Schwarzian derivatives that appear in the transformation law of the measure. Again, they only cancel in 26D.

In this connection it might be worth mentioning that I was originally trained in statistical physics, where stringy mathematics (read CFT) plays a fundamental role. Unless the situation in HEP, the application of CFT to 2D statphys is a success story, and it has be experimentally verified beyond reasonable doubt. In particular, the central charge has been measured, at least in computer simulations, and it has been found to agree with the non-zero CFT prediction. From this perspective, the idea that conformal anomalies can generically be avoided is absolute nonsense.

For almost five years, I have publicly said that string theorists are barking up the wrong tree. Why should I now be afraid of saying that the much smaller LQG community is doing the same thing (a different tree, admittedly, but still wrong)? Basically I’m using the same argument. Diff anomalies simply must be relevant to the quantization of gravity, because GR has many constraint subalgebras isomorphic to infinite conformal symmetry in 2D, any the lesson from string theory is that these subalgebras generically have anomalies. And no, I certainly don’t think that people at Loops 2005 neither appreciate diff anomalies nor know very much about them.

Implicit in section 5 of Nicolai et al. is a suggestion for a research programme: quantize GR in a similar way as the Polyakov action is quantized in bosonic string theory. This is more or less what I have tried to do for 15 years, and at the very least some of the mathematics – the anomalies and many representations – is now firmly understood. However, in my view this is strong evidence *against* string theory – if previously overlooked mathematics allows GR in 4D to be quantized, there is really no need for extended objects in 10 or 11D.

33.
June 12, 2005

Thomas Larsson faults Smolin's essay as follows:
[[… the suggested cure, which more or less explicitly reads more money to LQG, is problematic since many people feel that the LQG people are ignoring well-established facts.

Posted by: Thomas Larsson at June 12, 2005 06:13 AM]]

that is, in Larsson's view, Smolin's article is problematic because it advocates more money for LQG people
and LQG people ignore a well-established fact about constraint algebras which Larsson has discussed on several occasions.

1. the main thrust of the article is not advocacy of specific research lines. however several are mentioned (foundations of QM, technicolor, preons, QG phenomenology, causal sets, dynam. triangulations, Loop)

Loop (in the narrow canonical sense, or even the broadly inclusive sense) is not given priority in Smolin's list. It is hard to see how Larsson can criticize the essay for special pleading or favoritism.

2. In Larsson's view it would be problematical for unnamed "LQG people" to get more research funds. Who are "LQG people". A good working definition would be those who are organizing and speaking at the October Loops 05 conference at the Albert Einstein Institute
http://loops05.aei.mpg.de/index_files/Home.html

This link gives the topics to be covered at the conference.

This other link lists the invited speakers:
More about the programme
http://loops05.aei.mpg.de/index_files/Programme.html

Since you are accusing people of being ignorant of well-established facts, Thomas, which of these invited speakers to Loops 05 do you say are ignorant of well-established facts? I am just curious. Do you know any of them and have you talked with them to be sure that they are ignorant as you say?

I think maybe some were never ignorant of what you say and maybe some could have been ignorant perhaps a couple of years ago and may have learned something since then. But personally I don't know. You are making the blanket claim about the ignorance at present time of a group of people—-so that Smolin's essay is problematical if it leads to them being given more research funding—-and so I guess it is up to you to substantiate it.

34. **Quantoken**
June 12, 2005

It's worth noting that my discoveries must be ground shaking for figured out the strong and weak coupling constant. The exact moment I click the mouse on the "publish" button it triggered a 5.6 richter scale earthquake. No kidding.

35.
June 12, 2005

Thomas Larsson, it would help if you and indeed all of us (myself included) would look more objectively at what the Smolin essay actually says. I have used caps to emphasize relevant parts. Notice that most of what Smolin is talking about has little connection with what you are talking about (alleged ignorance of unnamed Loop people concerning anomalies and "infinite dimensional constraint algebras")

[[Alternatives to strings

More worrisome, young theorists who pursue alternatives to string theory have had great difficulty finding any academic positions in the US. This is true of those who pursue alternative programs in particle physics, like TECHNICOLOR and PREON models, and also true of those who pursue alternative approaches to quantum gravity, such as DYNAMICAL TRIANGULATIONS, CAUSAL SETS, AND LOOP QUANTUM GRAVITY. These subjects are all pursued much more vigorously outside the US, because leading researchers in these areas are drawn to leave US universities by offers of very good opportunities elsewhere.

One approach barely represented in the US is QUANTUM GRAVITY PHENOMENOLOGY, which studies how to test quantum gravity theories experimentally by means of high-energy astrophysics experiments such as the Gamma Ray Large Area Space Telescope and the Pierre Auger Observatory. The experiments are supported in the US, but most theorists who are developing the relevant phenomenology are outside the US.

Other examples show the hazards of too much concentration of resources on a few areas, to the exclusion of others. For decades, the FOUNDATIONS OF QUANTUM MECHANICS got virtually no support in the US; it was believed to be a direction without promise. In the last 10 years the fast-moving field of quantum information has shown that important experimental and theoretical results about foundations of quantum mechanics were always there for the finding…]]

The approach to quantum gravity which he mentions first, Ambjorn Jurkiewicz Loll CDT ("dynam. triangulations"), is a path integral approach that has nothing to do with "infinite dimensional constraint algebras".

Indeed if he is to be accused of special pleading, much of what Smolin mentions in the short passage about specific non-string lines of research do not involve "infinite dimensional constraint algebras".

However, you say:
[[… the suggested cure, which more or less explicitly reads more money to LQG, is problematic since many people feel that the LQG people are IGNORING WELL-ESTABLISHED FACTS.

Most of modern string theory – M-theory, AdS/CFT, flux compactifications, anthropic principle – is arguably built on loose sand, but some string theory insights are here to stay. LQG, and even more so the LQG string, violate probably the most fundamental insight of string theory: that INFINITE-DIMENSIONAL CONSTRAINT ALGEBRAS generically acquire anomalies on the quantum level…
…
…
Posted by: Thomas Larsson at June 12, 2005 06:13 AM]]

It has been pointed out here at Peter's blog, by Smolin himself in fact, that much of the work by LQG-people in the past 10 years has been in the path integral direction—spin foams and the like. This has not involved "infinite dimensional constraint algebras" or any constraint algebras at all.

I thought Smolin effectively answered Hermann Nicolai's
"outsider's view" article in his letter posted here, by pointing out that Nicolai's view of LQG research was overly narrow or else some 10 or 15 years out of date. Nicolai's view of Loop-and-related research has changed radically since that article, judging from the list of topics and speakers for the October "Loops 05" conference which Nicolai's institute is hosting.

I think if you take another look at your own post here, of 12 June, you will see that it is not Smolin who is self-serving and trotting out his own favorite hobbyhorse.

36. **Torbjorn Larsson**
June 12, 2005

Curious:

"GR, basically, hindered the progress in physics as much as it fostered it. Its own creator was not satisfied with this theory and its general validity has not been shown experimentally to this day."

I think this is incorrect. I seem to remember that you need GR, not only SR, to construct and maintain GPS satellite systems. Also, I think all the serious contenders have been shown wrong already. If I am mistaken I hope someone more knowledgeable will correct me.

37. **Alejandro Rivero**
June 12, 2005

A related question in some comments here is "Why a new Einstein", but it seems that most bloggers are driven by General Relativity. IE we want a new Einstein to unify the four forces. So we discuss about LQG (not rare in a thread about Smolin), fundamental strings, etc.

I a humbler way, let me suggest that a new Einstein taming the strong force could give already enough advance for some years: to be able to calculate particle masses *and effective coupling constants* (and from it, decay widths and branches) for all the hadronic interactions. I wonder how many of the support for strings comes still from the hope of being able to solve this particular point, independently of higher hopes.

38. **Juan R.**
June 12, 2005

Peter, i repeat my previous question for you if you have time for reply it.

would be more effective if you could post your comments in string theory (e.g. in Landscape stuff) directly like an attachment to each ArXiv preprint instead of here? That model did already exist in the CPS preprint (unfortunately closed) and worked very well, because one could see the preprint and in the same html page comments/discussion by people.

39. **Juan R.**
June 12, 2005

I agree with several, practically the whole, of Smolin's points and proposals.

Four additional comments:

1) I believe that Smolin article is, at some extension, a reclaim for attracting more people to "nonstandard" quantum gravity research like LQG. There is around 10 times more people working in strings, branes.

2) The real problem is about money. Past science was less expensive and moreover great advances were done by people with his/her own money. Today many parts of science are really expensive and one need funding or a "Templeton" grant :-). Money implies control of it and control implies directed research and directed research implies the control by mainstream. Less popular research programs are in the last part of the list for obtaining funding. That is a natural outcome of current scientific "democracy". History shows that in general questions mainstream knowledge is good, but in radical answers, mainstream is just wrong (remember revolutions of past begining in Newton).

3) A real Einstein, if any today, would not follow current popular research programs. Because a real Einstein (or Newton) would choose elegant, powerful approaches instead of irrelevant theories and ugly programs. A point is sure to me. Today Einstein would not follow current approaches to quantum gravity. **None** of them is really profound, several are ugly, and many of them are simply wrong.

4) Where is Wally (i.e. Lubos Motl)? I claim that the first "Einstein-like grant" was given to him for developing his impresive knowledge and physical intuition in quantum, gravity and other topics. All of we would favouring this plea. It is simple, if we leave to him study topic freely, he (great genious) choose the wrong way. The rest of people simply would follow the contrary way and the advance of science in next decades, would be impresive :-0.

40. **Thomas Larsson**
June 12, 2005

M wrote some posts ago
It seems to me that certain aspects of Einstein's personality are quite important to his ability to achieve the impact that he did. In my opinion the combination of several key traits was crucial:
…
2. Great respect for established results. This characteristic strongly distinguishes serious scientists from crackpots.
…

This is the crucial point, isn't it. It is easy to agree with Smolin's description of today's problems in physics, with string theory's experimentally unmotivated dominance. However, the suggested cure, which more or less explicitly reads more money to LQG, is problematic since many people feel that the LQG people are ignoring well-established facts.

Most of modern string theory – M-theory, AdS/CFT, flux compactifications, anthropic principle – is arguably built on loose sand, but some string theory insights are here to stay. LQG, and even more so the [LQG string](), violate probably the most fundamental insight of string theory: that infinite-dimensional constraint algebras generically acquire anomalies on the quantum level.

It is generally accepted that constraint algebras, like the infinite conformal symmetry of the bosonic string, must be anomaly free. (My opinions in this matter differ and are controversial, although IMO well-founded.) However, the fundamental observation is not really that the conformal anomaly cancels in 26D, but that it generically (D != 26) does not cancel. It is thus no surprise that string theorists (and not only them) reacted strongly when the LQG people claimed that it is possible to quantize the bosonic string without an anomaly for any D. For a detailed argument, see the [critical review of LQG]() by Nicolai et al., especially Section 5, Constraint algebra.

Thus, given that many people think that LQG is fundamentally misguided, it is hard to feel that it is really under-funded at the present level.

Incidentlly, my own work can be regarded as an attempt to treat GR along the same lines that string theorists treat the Polyakov action. This is evidently much harder (otherwise someone else would already have succeeded), but I find it rather striking that at least the relevant anomalies have been found, together with large classes of representations.

41. **Scott**
June 12, 2005

um my first sentence of my last post should have ended with "without Einstein" however curious seems to have made that point in a much clearer way then i did/ would have if i had finished the sentence.

42. **M**
June 12, 2005

Hi Curious,

I may be mistaken in my impression, but it seems like you are missing the main point of Smolin's article, based on what you wrote:

"It has been implicit in your discussion as well as in Smolin's paper that we desperately need new Einsteins to advance physics. The whole controversy then centers on how to get these proto-Einsteins out of obscurity, with or without the violin."

I didn't see that implicit assumption in Smolin's article. I doubt that his objective was to convince the world that we need a new Einstein to save us from our theoretical morass and lead us to a bright future (with the rest of the article then proceeding to explain how we should clear the roadblocks so this can happen).

It seemed to me that Smolin was using Einstein as a symbol for independent and creative thinkers who could (hopefully) make significant progress on really vexing problems. He proceeded to outline some systematic, serious impediments within "the system" and gave suggestions for making changes that would reduce them and nurture new approaches. He may be right. But it seems to me that someone like Einstein would find a way to work on the really difficult problems and get his or her work noticed even with the theoretical climate of today.

As an aside, is it controversial with you that there is a real need today for significant new ideas in fundamental theoretical physics, or that major advances are usually initiated by a small number of people? You seemed to imply that a team effort is a reasonable substitute for exceptional individual talent when you wrote,

"Except for GR I cannot think of any of Einstein's theories that would not be suggested by others 5-10-15 years later. … The task that was carried by a single person in 1905 could have been carried by several lesser minds w/o much loss for science."

Obviously it is impossible to disprove your statement, but there are many examples of activities where a team or committee of lesser minds is no match for exceptional thinking by a single individual. For example, think about difficult mathematical problems that have resisted long efforts by others until a single person with the requisite insight or creativity came along. Think about especially great works of visual art, literature or music. Can a team of excellent architects create a design that is equal or nearly equal to that of a truly exceptional architect? Would you feel as confortable fighting a battle (military or corporate) that is being led by a committee of excellent strategists as you would one that is led by a truly exceptional strategic thinker? (It also seems worth noting that Einstein played a very key role in the development of quantum mechanics; read Pais' biography if you doubt this. It is questionable that things would have turned out as they did in the 1920s without his help. Among "all those geniuses in the 1920s," most were working out consequences and difficulties in the new quantum theory, a much easier task than coming up with a new theory in the first place.)

43. **Scott**
June 12, 2005

Curious,
It is hard to understand cause and effect properly, were other people finally thinking new ideas because einstein broke the grip of the old ideas with his SR explanation of the photoelectric effect and brownian motion, or would have poincare finnally make the leap (actually i don't think he ever did either despite of or maybe because of einstein) to SR, and then maybe someone like bohr or heisenberg might have explained the photoelectric effect. The point is the field is in desperate need of some new people(don't get hung up on the "einsteins") to get the ball moving towards a new physics by making revolutionary steps toward the reconcilation of QM and GR as well as properly explain things like "dark matter" and "dark energy" among other things. It is these people who may not be quite the timesaver that einstein that need to be recognized and encouraged instead of pushed out of the field by its institutionalism. To do this we need to be able to recognize these thinkers better by changing arxiv and getting universities ect to diversify their speculation.

Also I would like to remind you that seeing as GR and QM have not yet been reconciled yet it would be foolish to assume the GR has somehow held physics back.

"He worked hard on these problems. And so should you"
yep, I am going to do that as soon as my summer starts. Speaking of which I am going to go finish up my labwritups so that I can start working on it.

44. **Curious**
June 12, 2005

It has been implicit in your discussion as well
as in Smolin's paper that we desperately need new
Einsteins to advance physics. The whole controversy then centers on how to get these proto-Einsteins out of obscurity, with or without the violin. I beg to differ. Except for GR I cannot think of any of Einstein's theories
that would not be suggested by others 5-10-15 years
later. He was, basically, a terrific time saver, most of the time. It is preposterous to suggest that w/o Einstein there will be no BE statistics, or photons, or statistical fluctuation & Brownian motion theories, or special
relativity – to name just a few topics. Think of all these geniuses in the 1920s; they did that and more. One thing
I am not certain about is GR. I do not think that
such a theory would be advanced without Einstein,
but it might have been better for physics if it weren't in the end; after 1930 nobody would even think about
suggesting a nonquantum, purely classical theory of
gravity. GR, basically, hindered the progress in physics as much as it fostered it. Its own creator was not satisfied with this theory and its general validity has not been shown experimentally to this day.

I think that we do not need new Einsteins. The task that was carried by a single person in 1905 could have been carried by several lesser minds w/o much loss for science. Perhaps, we just need to think a bit like Einstein and look at old problems with fresh eyes. One can hope that a
new Einstein will come and clear the slate for you.
Or one can look for new ideas w/o counting on that.

Surely, the second coming of A.Einstein would be helpful. However, delegating the responsibility to find new things to others is not dignified. Who
and what prevents YOU from seeing the obvious? Funding agencies? Peer opinion? Latest fads? Stop fooling yourself. Getting new ideas has always been difficult. It is YOU yourself. And I am no better than you.

Einstein did not write papers in the 50s that he
needs a new Newton to help him out of his TOE problems. He worked hard on these problems. And so should you. Enough of this new Einstein nonsense.

45. **Scott**
June 11, 2005

M,
Anonymous was the one trying to shut down an avenue of discussion, if he wanted smolins proposal's discussed, he should do more then say hey lets talk about these I think they are good instead of saying hey smolin said this I think this about it. Granted he did put some thoughts on it such as its good but all he said was I think these are good suggestion but I would like this suggestion checked out and think this other suggestion should be done carefully. Not knowing the answer to his one question and seeing nothing else to talk about I choose to explain why I thought my comments on changes to the arxiv and other comments about why were on topic in the fact they further the thinking in the question Smolin is attempting to answer thus in a way critiqueing for the points if overlooks if you need to look at it that way to justify the thread drift. So lets get the story straight anonomyous was trying to limit the direction of thought about why no new Einstein solely to discussing Smolins points and forcing us all to think(or at least restict our conversation) along only those lines, allowing no others. Do you see the parrallel? That parrallel to the main reason cited for their being no new Einstien was what i found so hilarious, i am sorry if it came off as insulting but it was really funny too me, i actualy did laugh out loud about it(granted I was really sleep deprived at the time). As far as I know Peter only gets worried about the thread going off topic when posters pet theories get brought up and discussed to much which hasn't happened in this thread.

That being said I think you are dead on with your description of his personality and how this would lead to his success even despite our current institutional problems. But what about the "miniEinsteins" those who are very similar to einstein and capable of thinking of revolutionary ideas but without as strong a drive to do so despite the institutional problems we have. Why should, many others who made creative and revolutionary ideas didn't have to succeed despite this. In that sense we should still try out these suggestions and Juan and my suggestions about changing the arxiv for better communication in order to facilitate these individuals. Of course all of this is mute if a genuinely "new einstein" in the sence you are speaking of blasts on to the scene as such an event would lead to a more revolutionary thinking among others as well as break the institutional hold. Personally I have odds on the real deal showing up and making this discussion mute, but just in case…(of course this just type of waiting for an "Einstein" really brings back my messianic complex comment)

46. **M**
June 11, 2005

As a follow-up to the anonymous poster:

"Let's not distract ourselves with details about the personality of Albert Einstein and his symbolic life experiences. Maybe there is hope of a generational shift and qualitative improvement in US research institutions."

It seems to me that certain aspects of Einstein's personality are quite important to his ability to achieve the impact that he did. In my opinion the combination of several key traits was crucial:

1. Excellent physical intuition.
2. Great respect for established results. This characteristic strongly distinguishes serious scientists from crackpots.
3. Willingness to pursue an independent direction, but only when it was apparent to him that the conventional approach was inadequate.
4. Inwardly directed and motivated. He didn't let others decide for him what he should be working on or what approach was correct.
5. Tenacity. He didn't give up on problems he was interested in, neither because they were hard nor because others didn't buy into his program.

As for the connection with Smolin's article…

I think all of the above characteristics (and others) were very important to his success. I also think it is clear they came from inside him as an individual and were not the product of an especially nurturing environment or support system. None of his best work was done while he was at the IAS at Princeton, although at the IAS his environment offered an environment much closer to what Smolin advocates than the environment where he did his best work.

So, I guess I don't really buy into that part of Smolin's thesis that states "the system" shoulders much of the blame for "no new Einstein." If and when someone like Einstein comes along, he or she will succeed primary due to internal motivations and traits, not because Smolin's proposals have been put into place. Groupthink in physics departments and within funding agencies, hiring decisions based on having a clean record of "correct" research endeavors, and funding decisions based on their compatibility with fashionable research programs and directions are cancerous and harmful to the progress of physics, I agree. But do they prevent the appearance of a new Einstein? I really doubt it; I don't think obstacles in the system can stop someone like that…

47. **M**
June 11, 2005

Scott wrote:

"No one is objecting to his suggestions here this indicates we all agree, and so we moved onto discussion about other things to incourage revolutionary thinkers and better ways to recognized them and their ideas in the arxiv."

I don't think anyone appointed you as spokesperson for what everyone else thinks. Peter's topic was Lee's article, 'Why No "New Einstein"?' It seems quite inappropriate to try to shut down discussion on that in favor of steering discussion to less on-topic areas (and in an insulting manner, at that). Recall Peter's explicit requests on a number of occasions to maintain discussion on the particular topic; that the anonomous poster was trying to do this is to be encouraged rather than disparaged.

48. **Scott**
June 11, 2005

anonymous,

No one is objecting to his suggestions here this indicates we all agree, and so we moved onto discussion about other things to incourage revolutionary thinkers and better ways to recognized them and their ideas in the arxiv. Personally I find the fact that you are simply wanting to simply discuss Smolins proposals instead of having us think of more proplems and proposals, is in the context of this topic extremely hillarious, I'll leave it to you to figure out why.

49.
June 11, 2005

I continue hoping for some discussion of these three substantive proposals of Smolin

quote from June "Physics Today"

To prevent overinvestment in speculatative directions that may end up as dead ends, departments should ensure that different points of view about unsolved problems, and rival research programs, are represented on their faculties.

Research groups should seek out people who pursue rival approaches, and include them as postdocs, students, and visitors. Conferences in one research program should be encouraged, by those funding them, to invite speakers from rival programs. Instructors should encourage students to learn about competing approaches to unsolved problems, so that the students are equipped to choose for themselves the most promising directions as their careers advance.

Funding agencies and foundations should take steps to see that at every level scientists are encouraged to freely explore and develop all viable proposals to solve deep and difficult problems. Funding should go to individual scientists for individual thought and not to research programs. A research program should not be allowed to become institutionally dominant until supported by convincing scientific proof of the usual kind. Before such proof is demonstrated, alternative and rival approaches should receive encouragement to ensure that the progress of science is not stalled by overinvestment in a direction that turns out to be wrong.

—-end quote—

Does anyone doubt that these three of Smolin's proposals should be put into effect? Could some governing body in NSF adopt them as resolutions, or is that too much to ask? Could someone with an agenda like Smolin's be appointed to a position within NSF from which leverage can be applied to the major physics departments and the relevant agencies and people at various levels. If that is too much to expect, then why, exactly, is it so?

If the US scientific establishment has reached a point at which such proposals actually need to be made (instead of being implemented per custom in normal academic practice) then can anyone here offer any objection to their adoption?

Let's not distract ourselves with details about the personality of Albert Einstein and his symbolic life experiences. Maybe there is hope of a generational shift and qualitative improvement in US research institutions.

50.
June 11, 2005

[[I thought this was about why their was no "new Einstein," silly me. Adding to Smolin and Peters comments on why this is…

Posted by: Scott at June 11, 2005 11:14 AM]]

Of course you are welcome to think that. Please allow me to suppose something different. I suppose that the headline question of "why no new Einstein" is essentially just good journalism.

It gets attention, dramatizes the issues, and I suppose energizes people by involving them in emulation fantasy.

One could also as "why no new Feynman" or "why no new Kepler".

but allow me to suppose that this is simply a journalistic hook and that the substance of what he is talking about is as follows:

1. he claims (dont know if true) that creative independent-minded talent that you would earlier have expected to stay in US is going to places like Europe and Canada.

this could be, e.g., a talented young European, proven innovator, who comes to US for postdoc and then instead of moving on to faculty at Columbia or Princeton (as he or she might have done in years past) goes back to Utrecht or Potsdam.

I dont know what statistics Smolin is talking about. he is saying US theory establishment is now COMPARATIVELY LESS ATTRACTIVE to bold ingenious workers and that the lucky ones are LEAVING. Maybe he is wrong. and maybe he is right. It needs to be checked.

2. He is saying that to make the US more attractive again, to foster and keep these people, certain re-diversification should be done by

a. department chairs, hiring committees, tenure review
b. research institute directors, conference organizers,
c. NSF committees, foundations that fund conferences, fellowships, etc.

This diversification is more along the lines of having SEVERAL RIVAL programs or theoretical approaches represented in the department, or the research institute. One of the best examples of this is the Albert Einstein Institute at Potsdam which has several quantum gravity approaches being pursued.

3. He may also be saying loosen the institutional grip which vested research programs have on theory in major departments, at least to the extent they turn the inventive young person into a brain-slave. But in my private estimation this should be with some care, if at all, since if applied heedlessly it could damage the coherence of valuable efforts.

In any case I think it is sentimental nonsense to think that one is going to find some policy change that will produce people that will be recognizably similar to those whom we revere as heros and saviours, "new Einsteins", or Keplers, or Feynmans or whatever. A practical objective would be to make US research institutions once more attractive to talented independent minds, if necessary by breaking the monopoly power of overgrown programs.

51. **Juan R.**
June 11, 2005

Quantoken now i am understand some of your topics.

You said "The real Einstein did not go to graduate school either." I think that he was.

"They can write to some of the most famous physicists at the time and very likely they will get some respose and discussion."

You also can. I say this by own experience. I have talked with leader physicists in several disciplines inclusding several Nobel laureates like Prigogine (for discussing about thermodynamics), Weinberg (for discussing about QFT), etc.

Almost all of my mails were read and correctly replied, including formulas, references, etc.

When i began to talk with Prigogine i clearly say to him that i was undergraduate, but my mansucripts were interesting for him and contacted to a post-PhD of his group with me and both collaborate in a hot thermodynamical topic.

I see that if your work is minimally serious, it is read by people with interest and comments/suggestions received.

On Edward Witten I can say nothing.

52. **Juan R.**
June 11, 2005

Quantoken,

I have hard often that Einstein was not studied and therefore any can do a revolution in science without go to the university.

I don't know if it is your case (i didn't understand well your post), but I thought that Einstein was a graduate (physicist) with good experience in scientific research. Moreover, I think he was did a PhD in a molecular hot topic. Am I wrong?

53. **garrett**
June 11, 2005

In talking about the arxiv, I said that it was "(mostly) open." I think this is pretty much the best way to go, with an automated, and hence near zero cost, system to screen out obvious cranks. A system of endorsements, with a very broad base of endorsers, does a good job of this. However, I think it's bad when the arxiv administrators mess with this submission criteria by hand — deleting articles or restricting endorsed posters, based solely on their opinions. It's bad because this sort of judgement should be carried out by the community, as the next step. By deleting articles or censoring posts, the reviewers are overstepping their bounds and potentially killing good and maybe even groundbreaking articles based on their opinions. This has unfortunately happened several times, to Tony Smith and Brian Josephson and others, and it's just not the administrator's place. If they take great pains to establish a good and fair system, the administrators should realize that in messing with it by hand they're doing something wrong — they're practicing exactly the kind of heavy handed censorship that an open forum is supposed to prevent.

54. **Scott**
June 11, 2005

I thought this was about why their was no "new Einstein," sill me. Adding to Smolin and Peters comments on why this is as well as discussing additional changes besides the general diversification ideas are in no way offtopic.

55.
June 11, 2005

this blog discussion is supposed to be about Smolin's proposals?

Here are 3 of them. they seem totally uncontroversial to me: no brainers. Any disagreement?
quote Smolin

To prevent overinvestment in speculatative directions that may end up as dead ends, departments should ensure that different points of view about unsolved problems, and rival research programs, are represented on their faculties.

Research groups should seek out people who pursue rival approaches, and include them as postdocs, students, and visitors. Conferences in one research program should be encouraged, by those funding them, to invite speakers from rival programs. Instructors should encourage students to learn about competing approaches to unsolved problems, so that the students are equipped to choose for themselves the most promising directions as their careers advance.

Funding agencies and foundations should take steps to see that at every level scientists are encouraged to freely explore and develop all viable proposals to solve deep and difficult problems. Funding should go to individual scientists for individual thought and not to research programs. A research program should not be allowed to become institutionally dominant until supported by convincing scientific proof of the usual kind. Before such proof is demonstrated, alternative and rival approaches should receive encouragement to ensure that the progress of science is not stalled by overinvestment in a direction that turns out to be wrong.

—-end quote—

Wasn't "portfolio diversification" in the theory section of a department approved and fairly common practice a few academic generations back? Isn't it still far from rare in major universities outside the US?

56. **Quantoken**
June 11, 2005

Scott said:
"One reason, there are no new einsteins is that many may quit before even finishing gradschool."

Please note: The real Einstein did not go to graduate school either. He finished colleage as an average and could not find a job for two years. Then only with some help from some one influential he got the job of a patent clerk.

The big difference is at Einstein's time science was still the activity of a small elite group, not a massive modern industry. As an average Joe, Einstein **and his friends** can access the academy easily: They can read the most advanced science journals and can underdstand and discuss the stuff. They can also submit papers and it is more likely than not that they can get their papers published. They can write to some of the most famous physicists at the time and very likely they will get some respose and discussion.

But all that is no longer possible today since science has grown into a huge huge massive full fleged modern industry. The kind of communication Einstein enjoyed is no longer possible, despite the availability of the internet.

Just try to think about a new Einstein, who is a college graduate, but who miraculously get it all figured out, write to Edward Witten, telling him Sir you are wrong, don't waste any more of your time studying super string theory, bla bla bla? What kind of response do you expect?

Now think about the real Einstein living today, and there hasn't been SR and GR yet, But QM and SST have been fully developed today. So the establishments includes the classical Newtonian mechanics, QM, SST, and the Universe was a static model, and of course the spacetime is absolute. Assuming that's what we got today.

Now this guy called Einstein, who nobody knows, comes forward in the year 2005, and tell everybody: It's all wrong, Newton got it wrong, there is no absolute spacetime, bla bla. I have a whole set of new theories which describes the experimental evidences better than the Newton Mechanics. And the universe could not be static at all. Depend on the critical density, **either all stars will fly away, or one day the sky will collapse on us**. Not only that, my new theory (GR) is also **incompatible with QM**, so I believe that QM could also be wrong or at least it is not a complete theory, bla bla bla. Would any one take him seriously? This guy doesn't even have a Ph.D., thank God, why would any one even bother to read his paper?

Think about that, wouldn't you say that Einstein was lucky to be born in 1879, not in 1979?

57. **Scott**

June 11, 2005

One reason, there are no new einsteins is that many may quit before even finishing gradschool. The amount of stuff needed to learn to catch up on the physics of today is alot longer then in einsteins day and someone of einseins character may not like having to spend all of this time learning what their school's say to learn instead of following wherever their thoughts bring them. My roomate quit school earlier this year for among other things. Would he have been a "new einsein" i don't know he definately loved thinking about physics and is smart as hell. I personally almost decided to quit this semester for the same reasons, and even though I didn't my constant thinking about things from how to get america cheap health insurance or getting rid of political parties, to thinking about fundamental problems in physics the reconciliation of GR and QM or the missing antimatter in the universe ect almost caused me to fail despite my decision to the contrary.

Another factor is the impression gotten that from most of the methods used today is that figuring out the new physics will take crazy mathematics skills and large research programs(ST and LQG) to work through and that there is no place for individual creative input that they could make despite not being a genius as far as mathematics is concerned. The only way for the creative individual to think otherwise is if they have a severe messianic complex like me. Luckily that is a common complex for smart people who can think up new ideas however it often gives them to much overconfidence in their particular ideas which can lead to crackpotism if the individual is not carefull.

58. **Scott**
June 11, 2005

Juan I like the idea of an open forum to talk about the paper in question, but a couple of points. Also a conveniant multiple ranking system to replace the how much is it cited model.

by which i mean seperate ranking for different categories such as:

scienceness,revolutionary/proggression of standard ideasness,effect on own workness(this one is measures the same thing that citations does but by person instead of by person weighted by papers),ect

then you would be able to see the number of people that gave each rating in each category as well as being able to sperate who(by category such as HEP and sub categories like string theorists) gave which ratings.

Also I would suggest having a seperate forum for pointing out actuall mathematical type errors.

59. **Juan R.**
June 11, 2005

Simply to say an important detail that i forgot!!

My old example of why a minimum review in science is needed, is based in a joung guy that a year ago claimed to be the new Einstein in certain science forum.

Some formulas of his "revolutionary" theory appear in the project page. You can value by yourself, why minimum review is needed. I wonder if Smolin proposal for detecting new Einsteins can effectively detect real geniouses from charlatans. I think that is possible but an really expensive task.

Probably from each 100000 hoaxes one find a single new Einstein. I agree with Smolin and think that humanity would waste time and money in this class of projects.

Only a last question, who will decide what is Einsteinian-like and what is hoaxers-like scientific work?

60. **Juan R.**
June 11, 2005

Below link don't work (i think that is a bug with preview buttom).

The link is Gonz?lez-?lvarez reply to "Science without denominations"

I think that current misunderstanding about string theory from outsiders would be eliminated if the system of publication was open with open discussion.
Any nonspecialist could simply read the criticism to string theory and see that the real status of string theory is not that of "Elegant Universe" by Brian Greene.

Students could obtain a good idea of that is being done in each field, if that field is open to debate and choose future careers.

I openly invite to any to discuss the future model of scientific publication and submit comments.

61. **Juan R.**
June 11, 2005

Garrett,

Your suggestion is exactly the posted by Shagaev, peer review may be substituted by open forum at journals pages. As said, I cannot post the page of project for reformulation of publication system here because Peter?s blog offers an error with the link; it is a Russian page and blog cgi says that is not "adequate" (?).

From Gonz?lez-?lvarez reply to "Science without denominations" you can link to Shagaev own suggestions in the page of the project for new model of publication.

I think that zero review is bad for science, and my specific proposal adds a minimum review process. It is compatible with some proposals at last 01 conference in electronic publishing. I added in the project page an example of why minimum review (split between adequate and inadequate) is necessary. The final validity of adequate papers will be based in open discussion within community.

I think that many anti-string theory (like Peter) would love i) an elimination of censure in some anti-string papers, ii) possibility for open forum of submitted papers. Now, one can see last papers in string fad in ArXiv and after one become here to read interesting comments.

Peter, a question for you, would be more effective if you could post your comments in string theory (e.g. in Landscape stuff) directly like an attachment to each ArXiv preprint instead of here? That model did already exist in the CPS preprint (unfortunately closed) and worked very well, because one could see the preprint and in the same html page comments/discussion by people.

Garrett on your *If your stuff is decent, you should be able to find an endorsement*." I did extensive comments in this. An expertise in ArXiv said to me that usually ArXiv administrators provide to you a list of endorsers pro string theory and is best ignore it and search for others endorsers. Moreover, even if your work is finally endorsed and accepted, administrators (pro string theory) can erase it without explaining to you why, and violating own ArXiv policies.

62. **D R Lunsford**
June 10, 2005

JE – is that a whine? Just put it out there.

-drl

63. **garrett**
June 10, 2005

I like the publishing model of the physics arxiv, as propounded by Paul Ginsparg. I think, instead of the peer review process, it's best if researchers post to a (mostly) open forum, and have their work rated collaboratively by the community. Right now citations act as this collaborative rating. The arxiv, as some point out, is not completely open, but I haven't encountered a problem with its barriers, as I usually have some affiliation or another with academic institutions. If your stuff is decent, you should be able to find an endorsement. It may be the case that I'm academically shooting myself in the foot by not publishing through traditional journals, but I like to make the future happen by embracing it.

Quantoken and J.E. — I always like reading good new stuff. But I can't comment on it unless it's out there to be seen.

64. **Quantoken**
June 10, 2005

Me, too! I am interested in seeing what J.E. has, although I will be quite skeptical.

garrett, how do you manage to get anything published at all? I don't think it's possible. Not that I have tried. I haven't, I am holding off publication of my research until I can figure it all out. But I am on a right track to getting pretty close to solve some of the most puzzling fundamental physics problems. So close that I no longer even bother to discuss my ideas on my own BLOG any more.

Quantoken

65. **Chris Oakley**
June 10, 2005

J.E.-

I would certainly be interested in your paper. Why not post a PDF version on your web site, or something?

66. **garrett**
June 10, 2005

I liked Smolin's article. Probably because, though no Einstein, I do fit Smolin's criteria for a young researcher on the fringe — working on hard fundamental problems, with only a few articles published because I thought they were good. And I've got a lot of good stuff I'm working on. But it's tough, and lonely, doing this all on my own. I looked at the web page for PI, thinking it might be a cool place to go spend some time, but it looks like it too is dominated by string theorists. I'd love if some other options were to open up, but I suspect Smolin's "modest proposal" for change will go over with the establishment about as well as Jonathon Swift's.

67. **Alejandro Rivero**
June 10, 2005

I agree with Aaron and Chris. Journals and ArXiV (that, btw, seems to be more relaxed this semester) policies are symptoms, not the cause.

As I told in Lubos's blog, my read of (/into?/) Smolin's article is a request for anarchy in modern science, opposed to the Order of "group thinking". Tony likes to speak of the "Golden Bars of Consensus". Consensus is a part of every assembly, of course, but it must be counterweighted by Initiative.

68. **Alejandro Rivero**
June 10, 2005

Read "The Dawning of Gauge Theory" by Lochlainn O'Raifeartaigh for a real happy shot of pure physics in contrast to all this bitching.

I was gonna send him email, but unfortunately he's dead. What is it about the Irish and Italians that makes them such great geometers? Catholicism?

Guinnes. I remember O'Raifeartaigh vividly describing us the smell of Dublin streets in the morning coming some days of the week, depending of the brewery works, and how the citizens were able to decide from it the best day to go out for a beer.

69. **J.E.**
June 10, 2005

Well, I'm no new Einstein for sure, only a theoretical physicist who works and lives as a translator. But in case it can help explain the current lack of new ideas in the field, let me say that several years ago I took the challenge of trying to remove infinities from QFT (specially from QED, which is where they first appeared), as I felt pretty uncomfortable with them during my student years (not to speak of renormalization). By realizing that something was missing and by introducing minor changes into the standard QED approach, I managed to reproduce all cross section values and to compute the electron MMA at third order with about the same accuracy as the standard value (and without any need for renormalization). Once the task was completed and the paper was prepared, I realized that I had no apetite to fail or succeed to publish it, gain recognition or anything like that. Why bother to face critics and indulge into endless discussions instead of turning my efforts into another problem which I felt more appealing?

70. **Juan R.**
June 10, 2005

Mike Crowley said

"Or perhaps breakthrough ideas were written about in a paper decades ago but never noticed."

There are well documented cases of breakthrough ideas ignored during decades and finally aknowledged like great. For example, Onsager Nobel Prize.

I think that situation now is poor with a order of 10^2 journals in a specific field. Nobody can read all. I suspect that really someone good idea is hidden in a non-top journal. Perhaps some great idea that permits to me solve some of unsolved problems that i am working now with few success:

71. **Peter Woit**
June 10, 2005

Perelman had no “departmental elders” to answer to. As far as I know he was essentially working outside the academic system in Russia, supporting himself on his savings.

Wiles was tenured at Princeton, and had made his reputation based on earlier work. Someone I know who saw one of his grant applications from that period claimed that although Wiles wasn’t explicitly saying so, reading between the lines you could tell that he was trying to prove Taniyama-Shimura-Weil. He was one of relatively few people with a strong enough reputation that he could be pretty sure he could get a grant based mostly just on his reputation, without having to give away what he was up to.

I’ve always thought the funniest part of the Wiles story was that near the end of his work on this problem, he scheduled an advanced graduate course, in which he went over in great detail one of the more obscure technical parts of the proof. The only one in the audience in on why he was doing this was Nick Katz, so slowly all the other students drifted away, since the course just seemed pointless and obscure to them. The funny thing is that this was normal enough for the Princeton math department that no one noticed anything unusual was going on.

72. **mortain**
June 10, 2005

Fantastic post, Peter. I’ve always tried to read everything Smolin writes (and also Rovelli). Once I was fortunate enough to meet Smolin in person, which despite the extremely high risk of intellectual embarrassment (at the time, I was a grad student), was like bathing in neural fireworks. Smolin opened up more new intellectual horizons for me in 15 minutes than innumerable string theory discussions ever did.

Still, no “new Einstein”? When it comes to gravity, perhaps we should be asking why there is no ‘new Newton’. If Einstein and Newton are on a par as regards their understanding of a force as elusive as gravitation, we might be better off reserving optimism for the 2160’s.

Seven years of intensive hard work seems like a small price to pay for relative immortality. It’s got to be worth it. But if so, why don’t more people try to solve the big problems? Anyway, just what did Wiles and Perelman do to satisfy their departmental elders that they were doing what mathematicians ‘usually’ do whilst patiently conquering major unsolved problems? Does any one remember?

73. **Mike Crowley**
June 10, 2005

One thought that sends shivers up my spine that there may indeed be another Einstein–or several–out there who simply cannot get the attention of the scientific community. Or perhaps breakthrough ideas were written about in a paper decades ago but never noticed.

Another thought–and I could be way off base here–is that a breakthrough of some sort might come from another discipline altogether, from dynamic systems or complexity (?). The interdisciplinary approach of complexity seems to have encouraged some new ideas in other fields.

Lastly, perhaps there is a possibility for “lone wolves” who have a limitless passion for physics (and intensely want answers) but, because they do not particularly care one way or the other whether they are accepted into the community of science, and have not been indoctrinated to think in terms of what is professionally acceptable or too risky, have nothing to lose by considering unconventional theories. These would most likely have to be individuals who are not dependent upon the scientific community itself for survival. But then the problem becomes one of being heard or taken seriously.

One hope I have is that the desire to know the truth about the origin of the universe, what came before, how the universe operates and why we are here would be so strong that it would eventually override the superficial considerations that might be obstructing new and better ideas. Discovering this and other blogs has been a true revelation because I had no idea until recently that these controversies existed.

74. **Juan R.**
June 10, 2005

I did comments in that, see references. See data extracted from papers, science citation index and own experience of Nobel laureates, whic work was rejected many times for peer review publication.

Scott, you forget that photoelectric articles was based in some ideas already thought for others, and a few of luck for the publication.

You forget that GR was rejected by Nobel commite like “speculation without many importance”.

75. **Chris Oakley**
June 10, 2005

I agree with Arun. ArXiv and the journals just reflect the attitudes and prejudices of the clique who decide the worthwhile research topics. With a few notable exceptions the rule is that if ArXiv do not want your papers then probably no-one will want to hire you either. This is not cause and effect: it is just two sides of the same coin.

76. **Arun**
June 10, 2005

Re: arxiv, and rejection policies – I don’t think an Einstein thought, oh, there is a journal that will accept my papers, so I’ll think revolutionary thoughts. arxiv problems are a symptom of a conformist culture, not the cause of such a culture.

77. **Scott**
June 10, 2005

your forgetting the explanation of the photoelectric effect as well. One of Einstiens greatest accomplishments and the his 1905 paper he thought was most revolutionary. Einsteins claim over poincare has mainly to do with the clear new conceptual breaks in his paper and his production of the Lorentz’s math from the two postulates that physics is the same in all reference flames and the speed of light is always c.

http://www-groups.dcs.st-and.ac.uk/~history/HistTopics/Special_relativity.html

78. **Juan R.**
June 10, 2005

Why no new Einstein? I did some comments about this in the past in this blog (e.g. last PITP Showcase Conference and in other sites.

Recently, Nobel laureate Laureate Brian Josephson critiqued the rejection model in ArXiv and in his web page claimed that today revolutionary ideas such as those by Einstein or Yang-Mills would be not considered for community. In the case of Einstein, he would find difficulties due to his lack of official affiliation.

Several editors, scientists (including several Nobel laureates), official science organizations, etc. are claiming in the last decade that current organization and publication system of science is the source of its clear stopping, without great advances or revolutions.

The problems are:

– **Oriented research**. Newer in the past the greatest achievement were obtained from rigid oriented research, specially Nobel laureated works. Nobel achievements are an elegant mixture of talent more luck.

– **Archaic publication systems favoring old ideas**. Usual peer review is the confrontation of your manuscript with established ideas. Referees are the guardians of standard knowledge. If your work is = establishment + small addition will be published. If your work breaks establishment at a great extension then will be rejected.

– **A rigid hierarchic (military?) organization**, where young promising science students are used like ?brain extension? by senior tenured scientists for working in the ? stupid? projects of later. It is really frustrating for a young scientist to work personally in some interesting (revolutionary) theme, comment with his chief and this say that your work is ?either wrong or uninteresting? (curiously highly respected specialists in the topic say, ?I like your approach? and similar after reading manuscripts). Peter did interesting comments relating high-energy physics in his legendary (physics/0102051).

There is available more information, (including information and open debate about recent ?Science without denominations? Chemistry and Life 2005, 5, 6?10) in next sites:

Gonz?lez-?lvarez reply to "Science without denominations"
—-
This site is "anticomunist" and say "Your comment could not be submitted due to questionable content:" quoting the Russian page of Shagaev and forum in the project of reformulation of publication system. For reading the content of Chemistry and Life and comments by editors and scientists, you may enter to Shagaev page from my web site or to Shagev comments from my above link
—-

Further information is available in other forms. For example, see the conclusion of the 7-month investigation of the House of Commons Science and Technology on the validity of current system of scientific publication and the no progress of science in ?(the centerl for canonical science? section (pag. 7) of

Official launching letter

79. **icecube**
June 10, 2005

>What is it about the Irish and Italians that makes >them such great geometers? Catholicism?

Fantastic! Looks like I have a new email signature…

80. **D R Lunsford**
June 9, 2005

This is OT, but he's dead and this is deserved:

Read "The Dawning of Gauge Theory" by Lochlainn O'Raifeartaigh for a real happy shot of pure physics in contrast to all this bitching.

I was gonna send him email, but unfortunately he's dead. What is it about the Irish and Italians that makes them such great geometers? Catholicism?

-drl

81. **D R Lunsford**
June 9, 2005

No new Einsteins? What does it matter? No one listens to their elders anyway.

-drl

82. **Peter Woit**
June 9, 2005

Hi Eric,

I was also thinking of Wiles and Perelman who are interesting evidence that it takes about 7 years to make progress on a big problem. Wiles did it after he had tenure at Princeton so could do what he wanted, Perelman by saving his money earned from several years of jobs in the states, and living cheaply in Russia.

Hi Tony,

I wasn't thinking of Vilenkin, but rather a college friend (not Eric….) who advocated this as a way of getting intellectual work done.

83. **Tony Smith**
June 9, 2005

Peter, you said "…A young theorist would be more likely to be able to find the necessary time if they went to work as a night-time security guard. …".

Were you thinking of Alexander Vilenkin, about whom a February 1996 Discover Magazine article by David H. Freedman on the web at
http://www.gradewinner.com/p/articles/mi_m1511/is_n2_v17/ai_17808131
says in part:
"… Vilenkin's fascination with cosmology dates back to high school in the Ukraine … few professors at the university could do anything to satisfy Vilenkin's curiosity about cosmology; his frustration grew worse when he was rejected by Soviet graduate schools. … Unable to get work as a physicist,[Vilenkin] took a job as a night watchman in a zoo and set about doing cosmology on his own. After being allowed to emigrate in 1976, he came across an advertisement for the graduate program in physics at the State University of New York at Buffalo. He had better luck getting accepted in Buffalo than in the Ukraine, and he whipped through the Ph.D. program in just one year. Eventually he landed a job at Tufts …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

84. **Eric Baum**
June 9, 2005

In mathematics Perelman (Poincare) and Wiles (Fermat) managed to devote many years hunkered down to solve big problems in surprising and deep programs. I bet there's hope in physics too.

85. **Peter Woit**
June 9, 2005

I feel compelled to point out that I don't endorse in any way the practice of using someone else's login to gain access to a subscription-only website. If you do this you are opening both yourself and the person whose login you are using to potential legal trouble that I want no part of.

86.
June 9, 2005

use http://www.bugmenot.com to access http://www.physicstoday.org

Multiverse, String Theory and Templeton

June 10, 2005
Categories: Uncategorized

A couple months ago when I was shocked to realize how close to reality my April Fool's parody had been, I'd unsuccessfully tried to find out some more information about the Templeton conference at Stanford that Mark Trodden had mentioned here. There's now something about it on the Templeton website. It was part of Templeton's Humble Approach Initiative which has as its goal "to bring about the discovery of new spiritual information."

The conference was called Multiverse and String Theory: Toward Ultimate Explanations in Cosmology, and brought together various landscapeologists (including most of the Stanford theory group). One of the other participants was the Rev. Dr. Rodney Holder, an Anglican priest who believes that science supports Christian belief, and that "modern cosmology has reinvigorated the traditional argument for the existence of God from design". He has written an article on miracles that won an award from the Templeton Foundation, and has a new book out entitled God, the Multiverse and Everything: Modern Cosmology and the Argument From Design, which argues for Intelligent Design.

Comments

1. **Tony Smith**
   June 15, 2005

   Peter, thanks very much for your honest statement that you are opposed to "… right-wing extremists …" such as Templeton who you view as "… mixing fundamentalist religion and politics in a dangerous way …".

   Although I cannot prove it by a rigorous poll or such, it is my opinion that most (probably almost all) of the academic landscape/superstringers have similar views.

   Assuming for the purpose of discussion here that such is the case, it raises the questions:
   Why would academic landscape/superstringers try to jump into bed with Templeton?
   Is their current 90% of the usual theoretical physics funding not enough for them?

   The obvious answers are that 90% is not enough and they want further funds so badly that they will sell their souls to get it.

   Why is 90% not enough?

   Probably because the system is designed to produce landscape/superstring PhDs each year at an exponential rate (if the number of PhDs produced is proportional to the number of current faculty, and the new PhDs become faculty), and the usual funding does not have corresponding exponential growth.

   Therefore, even 100% of conventional funding would not be enough to satisfy the exponentially growing landscape/superstring appetite.

   The resulting greed explains such superstringer actions as:
   selling their souls for Templeton money;
   and
   Witten's attempt (through Griffiths) a few years ago to grab Piet Hut's IAS post.
   With respect to the latter, see an 11 October 2000 statement by Richard Muller at http://muller.lbl.gov/pages/MullerStatement.html saying: "… I am very sad to see the lawsuit between the Institute for Advanced Study and Piet Hut … the Institute for Advanced Study … is doing enormous damage to itself by attempting to terminate him. …".
   and a Science magazine article by Constance Holden, dated 27 October 2000, at http://www.sns.ias.edu/~piet/lawsuit/science.html that says: "… Hut dismisses the negative job assessments, saying that there has been no formal evaluation of his work and that the visiting committee had little regard for his field of computational physics. He says the problem started with a 1993 dispute with string theorist Ed Witten over Hut's desire to buy an expensive supercomputer. Witten has declined to comment. …".

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

   PS – The links about Piet Hut above were roughly contemporary with the events around the year 2000, and may no longer be live links.

2. **Alejandro**
   June 14, 2005

   http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=find++t+consciousness

3. **Juan R.**
   June 14, 2005

   Hi "",
   I have not a machine for knowing that people is thinking when say phrases. I cannot know that Einstein mean when asked

   "Had God any choice in the creation of the world?"

   A joke? Perhaps a redundant question (Einstein was determinist)? I don?t know. He also said that time was an illusion, but in the last years of his life, he appeared to sustain a different view. I simply don?t know. If he was living now I could ask to him, but I cannot do it.

   I wonder is that scientist are discussing about things that science cannot say nothing and the hot debate here.

   Was Universe created by God:

   – Yes. It is possible

   – No. It is possible

   Science cannot say nothing. Therefore Rodney Holder's claims are undecible according to logical calculus. I cannot see serious problems with that (when my above opinion is explained). Quoted above Holder's claims are not antiscience, but some people here is anxious.

What **IS** antiscience is the mainstream belief of that SM or GR are explained by experiment. That is a great **MISTAKE**

After of several replies, manuscripts, sci posts, etc. still nobody has demonstrated to me that SM and GR are correct.

In SM the supposed proofs of my contenders (e.g. Weinberg) usually don't pass of the elementary stuff S-matrix theorems (many are wrong when one study the details).

In GR, nobody (including of course active workers as Carlip, Ehlers, Will…) has demonstrated rigorously that GR even fits to solar system data.

4. June 13, 2005

   Had I been present at the creation, I would have given some useful hints for the better ordering of the universe. [1]

   Note [1].
   Carlyle says, in his "History of Frederick the Great," book ii. chap. vii. that this saying of Alphonso about Ptolemy's astronomy, "that it seemed a crank machine; that it was pity the Creator had not taken advice," is still remembered by mankind,-this and no other of his many sayings.

5. **Arun**
   June 13, 2005

   Did you know "Although much of the public controversy over intelligent design has focused on the application of design to biology, it's important to remember that design theory itself reaches well beyond biology, and that some of the strongest evidence for design comes from such fields as physics, astronomy, and cosmology."

   Brought to you by the Discovery Institute.

   🙂

6. **Quantoken**
   June 13, 2005

   "Had God any choice in the creation of the world?"

   Probably the god had no choice on a lot of things that is needed to create this world. For example mathematics rules. For a very specific example, the numerical value of PI, 3.14159265…., you can list all the decimal digits of PI to infinity and it is a sequence that contains infinite amount of information, more than what the whoole universe can register and hold. But once you lay down the definition what PI is, all of its digits are **completely fixed**, even the god is powerless to alter even one digit of PI out of the infinite number of digits! You can not create a universe in which the PI is even slightly different from ours!!!!

   On other things, maybe it's just a matter of the way our thinking works. For example Newton's second law. It is actually just a Newton's second convention. Because that Newton's second law/convention actually **gives the definition** what is force. Without a definition of what is force, you can not lay out a relationship how force is related to other quantities. The same equation, F=MA, can not serve on two purposes simutaneously, one to give a definition what is force, one to define the relationship between force and mass and acceleration.

   So in that sense, force is really just a necessity of human thinking, not a necessity for how things works in this universe. The god could well create the universe using just mass, spacetime and movements, with no reference whatsoever to the notion of force.

   Now the question is, the 4-D spacetime, is it by choice or is it **a mandatory like PI is**? If it is mandated, then, certainly, only 4-D universe is possible, and the 11-D super string theory is not going any where.

   Quantoken

7. June 13, 2005

   Juan, I like your posts. But I think Einstein was being cute when he said

   "Had God any choice in the creation of the world?"

   Different ethnics have different funnies. English humor is not easily explained to French wit.

   It is utterly charming how Einstein put it, but it is not, I think, about God or choice.

   It is about the logical and mathematical form of physical laws. Can they only be of this form (but maybe with different constants?)

   If only Immanuel Kant had been a jew as well as a german he could have been more provocative and amusing and we might all have benefited.

8. **Juan R.**
   June 13, 2005

   In "A man's ethical behavior should be based effectually on sympathy, education, and social ties and needs; no religious basis is necessary. Man would indeed be in a poor way if he had to be restrained by fear of punishment and hope of reward after death."
   [Albert Einstein, "Religion and Science", New York Times Magazine, 9 November 1930]

   Einstein was talking about usual religion.

   Regarding

   "Religion without science is blind. Science without religion is lame."

   i simply don't know if he was talking about religion, in general, or about his own view of "religion" (phylosophy).

   I think that Einstein would not agree with many Templeton questions but, in some sense, he asked about "antrophic" questions in his famous query

   "Had God any choice in the creation of the world?"

Sorry Scott, I cannot post a link to that here. I follow rigorous Peter Woit's guidelines in what can be posted and what cannot be.

9.
June 13, 2005

I was glad to see Scott's quote of Einstein:
[[It was, of course, a lie what you read about my religious convictions, a lie which is being systematically repeated. I do not believe in a personal God and I have never denied this but have expressed it clearly. If something is in me which can be called religious then it is the unbounded admiration for the structure of the world so far as our science can reveal it.]]

I looked for the source and found something about the quote in
http://en.wikiquote.org/wiki/Albert_Einstein

Wiki says it comes from:
Letter to an atheist (1954); Albert Einstein: The Human Side (1981) edited by Helen Dukas and Banesh Hoffman, Princeton University Press

Must add agreement and approval for the distinction Scott draws between Einstein's view and Templeton's. Judging from the quote of Sir John, that gentleman wishes people to acknowledge an "intimate", actively interfering Creator-person, an idea I find mildly disgusting. The thought of an affectionate busybody transgressing laws of nature for personal reasons is something of comedown from the unqualified admiration for physical law expressed in Einstein's letter.

10. **Scott**
June 13, 2005

"It was, of course, a lie what you read about my religious convictions, a lie which is being systematically repeated. I do not believe in a personal God and I have never denied this but have expressed it clearly. If something is in me which can be called religious then it is the unbounded admiration for the structure of the world so far as our science can reveal it." -Einstein

"From a theological perspective it is indeed tempting to see this remarkable self-organizing tendency … of the cosmos … as an expression of the intimate nature of the Creator's activity and identification with our universe." -Templeton

Peter has already pointed out the difference of their views but I thought I would pick another more blunt quote of Einstein's to really drive it home. Templeton talks of the universes principles being an expression of the creator's "activity and identification" with the universe while Einsein is simply in awe of the nature of the world and for lack of a better word that awe can be termed religous. These are in no way the same thing and in no way indicative that Einsein would approove of templeton's actions. God was simply Einsteins word for order/harmony/ect in physical law.

"Religion without science is blind. Science without religion is lame"

knowing what Einstein considered religous, "a feeling of awe at the scheme that is manifested in the material universe," this means the second part indicates that the life of science is sucked out if you have no awe for the principles that define the universe.

"Phys. Rev. on a generalization of S-matrix theory to chemical dynamics for example? (Molecules are not in the infinite past and scatter to the infinite future :-)"

I for one would appreciate a link to that.

11. **Juan R.**
June 13, 2005

I said,

"*It is really interesting that Einstein would not have approved research in quantum field theory. It is well-known that his valuation of quantum field theory (QED) was more radical than Dirac own one*."

Peter said,

"*The situation of quantum field theory at the present time is very different than it was more than fifty years ago back during Einstein's lifetime.*

Similarly the threats to the scientific enterprise are different. Whether or not he would have approved of a lot of the Templeton verbiage…"

I posted extensive quotes of Einstein and his own ideas about physics and religion. You are open to interpret words of others in your own benefit, but his phrase "*Religion without science is blind. Science without religion is lame*." is simple and clear for understanding.

It is also clear that Einstein hated relativistic quantum field theory (as said his words were "hard") and when results of experiments show ?coincidence? at eleven (?) figure he remained impassible. Why? Eccentricity of an old man? Of course, no. Einstein was not an engineer, he was a physicist a pure physicists searching ?true? and ?elegance?.

He knew that relativistic quantum field theory (then QED) was an ugly subject (**and still is**). The words of Peter are not true. The situation of quantum field theory (e.g. QED) at the present time is the **SAME** than it was more than fifty years ago back during Einstein's lifetime.

There are two types of physicists: ?real? (authentic) physicists and ?engineers? (most of particle physicists). Unfortunately, a deep understanding of nature cannot be achieved from relativistic quantum field theory (a theory that does not work in molecular chemistry as is well-known) Would I post here a recent Phys. Rev. on a generalization of S-matrix theory to chemical dynamics for example? (Molecules are not in the infinite past and scatter to the infinite future 🙂

From a guy called Dirac

… When one tried to solve it, one always obtained divergent integrals… Rules for discarding the infinities [(renormalization) have been developed]. Most physicists are very satisfied with this situation. They argue that if one has rules for doing calculations and the results agree with observation, that is all that one requires. But it is not all that one requires. One requires a single comprehensive theory applying to all physical phenomena. Not one theory for dealing with non-relativistic effects and a separate disjoint theory for dealing with certain relativistic effects. Furthermore, the theory has to be based on sound mathematics, in which one neglects only quantities that are small. One is not allowed to neglect infinitely large quantities. The renormalization idea would be sensible only if it was applied with finite renormalization factors, not infinite ones. For these reasons I find the present quantum electrodynamics quite unsatisfactory. One ought not to be complacent about its faults. The agreement with observation is presumably a coincidence, just like the original calculation of the hydrogen spectrum with Bohr orbits. Such coincidences are no reason for turning a blind eye to the faults of a theory. Quantum electrodynamics … was built up from physical ideas that were not correctly incorporated into the theory and it has no sound mathematical foundation. One must seek a new relativistic quantum mechanics and one?s prime concern must be to base it on sound mathematics.?

When one studies QFT seriously, one knows **WHAT** is computed and **WHAT** is ignored. When one seriously studies quantum field theory onw know **WHY** quantum field theory being wrong offers the correct answer to scattering experiments and wrong answers in other questions.

As said in other posts in this blog, one would not take seriously Weinberg manual 🙂

12. **Peter**
June 12, 2005

Hi Tony,

Here are some clarifications about what I think.

First of all about politics: there are plenty of other blogs on the web with similar politics to mine, so I've tried to stay away from political topics. But, like a lot of people, I'm very unhappy about what I see happening to this country as its politics is taken over by right-wing extremists. One of them is John Templeton Jr., president of the Templeton foundation and chairman of "Let Freedom Ring", a right-wing political organization. These people are mixing fundamentalist religion and politics in a dangerous way. See for example:
http://www.newsmax.com/archives/articles/2004/6/26/111715.shtml
I don't think it is a coincidence that they also are trying to inject religion into science and to promote pseudo-science.

Sure, the main sources of funding of string/M-theory remain universities and government grants, and this is the biggest problem. But I think it is remarkable and worth making a fuss about that leaders of the field of particle theory are getting in bed with foundations run by people whose agenda in life is promoting extreme right-wing politics and trying to increase the influence of religion over all of American life, science included.

13. **eli**
June 12, 2005

Peter,

1. I apologize for placing comments on GR that did not directly pertain to the discussion that you have in mind.

2. However, it is the cosmological consequences of GR with its insistence on dark matter, energy and other mysteries that gave fodder to Templeton foundation and the like cults. These people did not get their agendas from the Bible. They've got it from the popular press that, in turn, reflects the current state of cosmology. The best way to deal with the problem is to seek for physical theories that do not invoke the forces of darkness.

Once more, please, accept my apology for bespotting your blog.

14. **Tony Smith**
June 12, 2005

Peter, you say that you are opposed to "… the unholy alliance of a well-financed right-wing organization and string-theory/multiverse pseudoscience …".

You choice of words puzzles me. "unholy" indicates a religious judgment. "right-wing" indicates a political judgment.

If "superstring/multiverse pseudoscience" does indeed threaten to bring about what Feynman called "a degeneration of ideas" that causes "vigourous philosophy …[to]… disappear", and I agree with you that such a threat is real and serious, then it seems to me
that ANY funding of "superstring/multiverse pseudoscience" enhances that threat,
and it is irrelevant whether the funding comes from "unholy" or "holy" sources or from "right-wing" or "left-wing" sources.
In fact, I would guess that the most serious problem is not that "unholy" "right-wing" Templeton money might go to "superstring/multiverse pseudoscience", but
that 90% of the academic (predominantly left-wing) and government (holy or not, I don't know) funding for theoretical physics might continue to go to superstring theory and that superstring theory might morph entirely into "superstring/multiverse pseudoscience", thus leading to Feynman's "degeneration of ideas".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

15. **Peter Woit**
June 12, 2005

The situation of quantum field theory at the present time is very different than it was more than fifty years ago back during Einstein's lifetime.

Similarly the threats to the scientific enterprise are different. Whether or not he would have approved of a lot of the Templeton verbiage, I think he would have recognized the threat to what he held most dear embodied in the unholy alliance of a well-financed right-wing organization and string-theory/multiverse pseudoscience.

16. **Juan R.**
June 12, 2005

It is really interesting that Einstein would not have approved research in quantum field theory. It is well-known that his valoration of quantum field theory (QED) was more radical than Dirac own one.

17. **Juan R.**
June 12, 2005

"If it is one of the goals of religions to liberate maknind as far as possible from the bondage of egocentric cravings, desires, and fears, scientific reasoning can aid religion in another sense. Although it is true that it is the goal of science to discover (the) rules which permit the association and foretelling of facts, this is not its only aim. It also seeks to reduce the connections discovered to the smallest possible number of mutually independent conceptual elements. It is in this striving after the rational unification of the manifold that it encounters its greatest successes, even though it is precisely this attempt which causes it to run the greatest risk of falling a prey to illusion. But whoever has undergone the intense experience of successful advances made in this domain, is moved by the profound reverence for the rationality made manifest in existence. By way of the understanding he achieves a far reaching emancipation from the shackles of personal hopes and desires, and thereby attains that humble attitude of mind toward the grandeur of reason, incarnate in existence, and which, in its profoundest depths, is inaccessible to man. This attitude, however, appears to me to be religious in the highest sense of the word. And so it seems to me that science not only purifies the religious imulse of the dross of its anthropomorphism but also contibutes to a religious spiritualisation of our understanding of life."

[Albert Einstein, "Science, Philosophy, and Religion, A Symposium", published by the Conference on Science, Philosophy and Religion in Their Relation to the Democratic Way of Life, Inc., New York, 1941]

18. **Juan R.**
June 12, 2005

The religion of the future will be a cosmic religion…

A. Einstein

19. **Juan R.**
June 12, 2005

"All religions, arts and sciences are branches of the same tree. All these aspirations are directed toward ennobling man's life, lifting it from the sphere of mere physical existence and leading the individual towards freedom."

A. Einstein.

20. **Juan R.**
June 12, 2005

"Religion without science is blind. Science without religion is lame."

A. Einstein

21. **Juan R.**
June 12, 2005

Had God any choice in the creation of the world?

A. Einstein

22. **Tony Smith**
June 12, 2005

Peter, you said "… Look not at what the Templeton people say (which is relatively innocuous), but at what they do. …".

OK, and when I do, I see things like providing publicity and support for things like landscape and failure to fund things like Smolin's suggested Einstein fellowships that might actually advance our understanding of physics (and, therefore, from an Einstein/Spinoza viewpoint, God/religion), so I see your point and agree with you.

If the superstring/landscape people prevail, then I think that we will indeed be entering an era of what Feynman called "…a degeneration of ideas, just like the degeneration that great explorers feel is occurring when tourists begin moving in …" and "… it gets very dull … the vigorous philosophy … will … disappear …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

23. **Peter Woit**
June 12, 2005

Please stop posting comments here about whether or not General Relativity is correct. This has nothing at all to do with the topic of the posting. Please stop using this weblog as a forum for this kind of discussion and do this elsewhere. I'm deleting all the comments about this.

24. **Peter**
June 12, 2005

Hi Tony,

First of all I read that quote differently, not as saying "Creator=Universe", but as saying whoever the Creator is, he/she/it "identifies" with the Universe and its problems, i.e. she/he/it feels our pain or something, is not detached, but remains involved on a day-to-day basis. But much of this religion/sciences stuff is all so ill-defined that you can make pretty much what you want out of it.

That's not what's objectionable. Look not at what the Templeton people say (which is relatively innocuous), but at what they do. They explicitly refuse to support serious science, and instead fund an incredible array of attempts to inject religion into scientific practice. In theoretical physics, they aren't ever going to fund serious research into quantum field theory, mathematics related to quantum field theory, or any serious work of the sort Einstein would have approved of. Instead they are heavily funding the one part of the field that most people consider dangerous pseudo-science and a serious threat to the whole concept of what it means to do science. I don't think this is a coincidence.

25. **Tony Smith**
June 11, 2005

Peter, you said "… Templeton and the intelligent designers believe there is some sort of intelligent "Creator" out there… who continues to intervene in the universe and the affairs of humans …".

Is that consistent with the plain meaning of the Templeton quote about "… the Creator's … identification with our universe …" ?

If Templeton says Creator = universe, then isn't that more consistent with Einstein/Spinoza, or pantheism, than with a human-father-type external Creator/God who built our universe as our playhouse and might from time to time punish us if we don't play nice in our playhouse ?

Further, Templeton's quote about "remarkable self-organizing tendency" seems to me to be far more consistent with Einstein's "rational nature of reality" than with the landscape view "that there is no mathematical reason for … fundamental facts about physics".

Templeton's actual words still seem to me to be pretty much consistent with the Einstein view.
Do you think that it is possible that Templeton himself is reasonable but that some people who purport to follow Templeton, and to try to get money from him, are not so reasonable ?

For instance, those known as fundamentalist Christians seem to me to insist that only their religious path is the correct one and that those who fail to follow that path will suffer in eternal literal hell,
while the web page at http://www.templeton.org/sir_john_templeton/index.asp indicates that Templeton himself "espouse[s] a non-literal view of heaven and hell" and "a multi-faith framework of … the diversity of gifts within the major religions of the world," … including "Buddhism, Christianity, Confucianism, Hinduism, Islam, Jainism, Judaism, Sikhism, Taoism, Zen and Zoroastrianism.".

Perhaps your disagreement is not so much with Templeton himself, but with some who purport to be his followers and try to get his money ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

26. **Peter Woit**
June 11, 2005

Hi Tony,

I think the two quotes you give represent quite different views. Einstein is emphasizing the "the rational nature of reality", that with hard work we can discover and understand the deep mathematical structures that govern how the world works. As far as I know he had no interest in "anthropic" explanations, or any sort of belief that the universe is designed the way it is in order to make human life possible. He didn't place human beings and their concerns at the center of things.

Templeton and the intelligent designers believe there is some sort of intelligent "Creator" out there who has set up the universe to produce human beings, one who continues to intervene in the universe and the affairs of humans. I don't think Einstein believed anything like this. The intelligent designers, together with the anthropic landscapeologists, are happy with the idea that the explanation for many fundamental facts about physics is that there is no deep mathematical reason for them, they just are the way they are because they make our existence possible. I think Einstein would have found this completely noxious, and most serious theoretical physicists still feel this way.

27. **Tony Smith**
June 11, 2005

From a Templeton web page at http://www.templeton.org/science_and_religion/index.asp :
"… From a theological perspective it is indeed tempting to see this remarkable self-organizing tendency … of the cosmos … as an expression of the intimate nature of the Creator's activity and identification with our universe.
– Sir John Marks Templeton …".

Einstein said:
"I believe in Spinoza's God who reveals Himself in the orderly harmony of what exists … I have not found a better expression than 'religious' for the trust in the rational nature of reality that is, at least to a certain extent, accessible to human reason. …".

I don't see much if any difference between the views stated above by Templeton and Einstein.
Also, I don't see much if any difference between Templeton's phrase "self-organizing tendency" and the usual meaning of the term "evolution".

So: Why can't we all just get along ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

28. **Nathan**
June 11, 2005

The fact that there is even a "debate" on teaching Intelligent Design angers me.

29. **Tony Smith**
June 11, 2005

There have been some comments mentioning Feynman's 1965 book The Character of Physical Law. Here are some relevant excerpts from that book:
"… The age in which we live is the age in which we are discovering the fundamental laws of nature, and that day will never come again. It is very exciting … but that excitement will have to go. … in the future either … all the laws become known … or … experiments get … more and more expensive, so that you get 99.9 percent of the phenomena … and it gets slower and slower and more and more uninteresting …
… ultimately, if it turns out that all is known, or it gets very dull, the vigorous philosophy … will … disappear … There will be a degeneration of ideas, just like the degeneration that great explorers feel is occurring when tourists begin moving in …".

To me, it seems that since GR plus the standard model does describe nature pretty well, and each advance in collider energy only produces data that is substantially consistent with GR plus the standard model, that superstring theory is a clear example of what Feynman called "a degeneration of ideas".

In my view, the only realistic way to get out of the degenerative rut of superstring theory is to try to construct a new model that embraces and extends GR plus the standard model. The same Feynman book gives his prescription for constructing such a new model:
"… 'guess – compute consequences – compare with experiment' … When you get it right, it is obvious that it is right … because … more comes out than goes in …
… we need … imagination in a terrible strait-jacket. We have to find a new view of he world that has to agree with everything that is known, but disagree in its predictions somewhere … and in that disagreement it must agree with nature. If you can find any other view of the world which agrees over the entire range where things have already been observed, but disagrees somewhere else, you have made a great discovery. …".

In my opinion, superstring theory fails to meet Feynman's criteria because it is not constructed by his 'guess – compute consequences – compare with experiment' method.

I feel Feynman's method can lead to a more fundamental unification of GR plus the standard model, and that my work is an example, because I constructed my model using Feynman's 'guess – compute consequences – compare with experiment' method, and every time I found a flaw in my construction, I found that the flaw was correctable and that each correction produced more and better results, so that as of now my model is in pretty good agreement with all accepted experimental observations, with one exception: the T-quark mass, which is the disagreement required by Feynman's criteria. My view about the T-quark mass is set out in my comment on Peter's 9 June 2005 blog entry about the "Future of Fermilab", so I will not repeat it here.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

30. **Juan R.**
June 11, 2005

It was said,

“Feynman hinted at the end of his 1965 BBC lectures, The Character of Physical Law, that when a radical revision occurs next it may be quite different from what is wanted by today’s practitioners.”

Humm, it sounds like “string theory is not the Final formulation” since ST is just modified QFT and its practitioners use just traditional physics like S-matrix, Hilbert-Fock space, wavefunctions, armonic strings, reductionism, etc.

31.
June 10, 2005

Joao: they have to turn out speculations because they work in theoretical physics. John Horgan’s claim in “The End of Science” (1997) that science has ended, just because there is the big bang and standard model, pushes them into stringy multiverses.

If Horgan is right, even in just the sociology of physics, then the enlightenment really is over. We then have to dig deep in the history to discover what happens when the lights go out in science.

Everyone who claimed that the elegant theory of phlogiston was rubbish was laughed off stage. Dead end science claims it is right because it fiddles its theory to fit all the facts. You can’t debunk it. You cannot get anyone to listen to a radical, simpler and more accurate approach.

Feynman hinted at the end of his 1965 BBC lectures, The Character of Physical Law, that when a radical revision occurs next it may be quite different from what is wanted by today’s practitioners. He referred to an influx of tourists, the silly guys on the outside always making silly comments. To him, it would be tragic for tourists to pollute prestine mountains by looking for easier paths to the top.

32. **Jo?o Carlos**
June 10, 2005

Those ID people are pathetic: they lack the science to “prove” their creeds, and lack the faith to believe their creeds without “scientific proof”.
Bad “science” and worse “religon”…

33.
June 10, 2005

http://www.google.com/search?q=multiverse&ie=UTF-8&oe=UTF-8

Response From Smolin

June 15, 2005
Categories: Uncategorized

Lee Smolin wrote an interesting responses to comments in the comment section of my posting about his [Physics Today piece entitled "Why No New Einstein?"](). I'm reposting it here.

"Dear Peter and colleagues,

I am grateful for the attention given to my essay. I only want to emphasize a few points here. The main thing is that the essay is carefully written. It does not advocate more funds to LQG or any other program. It explicitly advocates more support and positions for young, ambitious theorists pursuing their own research programs who are unaffiliated with any larger program. Several proposals are made for how to accomplish this. I would hope that the focus of the discussion could be on these proposals.

-String theory is criticized in the essay mainly because it is currently sociologically dominant, and so subject to the problems mentioned. It was necessary to do so as many readers of physics today will be unfortunately unaware that there are any problems with string theory, or any viable alternatives. Anyone with a long enough memory will know that the sociological issues in high energy theory predate string theory, and have hurt physics in the past, i.e. in the case of S-Matrix theory.

-I hope I don?t have to say that I am not anti-string theory. My current last paper on the ArXiv is a technical paper in string theory, and I have 14 more in past years, plus 8 papers on related topics such as the landscape. I wouldn?t have written these papers if I didn?t think there was a good chance string theory is relevant to nature. The fact that someone like me who contributes sometimes, but not exclusively, to string theory, is not considered ?a string theorist? is part of the sociological problems my essay criticizes. Similarly, the fact that one can elicit angry responses, and be called ?anti-string? for carefully and correctly recounting the actual status of various conjectures is a sign of an unhealthy sociology. No one calls someone anti-LQG or anti-QCD when they do a similarly honest summary of what is known and not known in those fields.

-I would claim that the sociological issues mentioned in the essay have hurt string theory even more than they have hurt the alternative programs, because they greatly limit the range of ideas worked on, and because people with a lot of imagination and intellectual independence are either selected out or choose themselves to work within communities which are more friendly to diversity and imagination. As a result, key issues such as the question of a background dependent formulation, or perturbative finiteness, don?t get a lot of attention, in spite of their centrality for the whole program.

-I was grateful that someone noted the range of subjects at the LQG meetings. This was not planned, it is a natural outcome of the more open and curious atmosphere among people who work on the subject. We don?t believe we should have a meeting without inviting people from alternative and rival programs to report to us what they are doing, as well as to serve as critics. At the meeting in Marseille last May we even invited a persistent critic of LQG-Ted Jacobson-an early contributor who is now very critical of the subject-to give a talk to lay out his criticisms. I think it would be very good for string theory if the organizers of their meetings took a similar attitude.

-Someone asked for a blanket term for LQG, CDT, causal sets etc. We use background independent approaches to quantum gravity. There is a lot of interchange of ideas, techniques and people among these programs, and many of us have contributed to more than one. There is a very different intellectual climate, in which diversity, creativity and independence are strongly encouraged.

-Someone is asking for what is ?LQG proper?? But the fact is that a lot of different things are now going on roughly under the name of or related to LQG. After all, this is now a community of > 100 people and there is no orthodoxy and no one trying to control what people work on. We agree generally on what has been achieved and what problems remain open, but not much beyond that. There is a healthy variety of approaches and attitudes towards the open problems. If there is one thing we all agree on it is that no approach is likely to achieve the right theory that is not background independent at its foundations. Come to the meeting and see what is happening.

-While the point of my essay was not to advocate more funding to any particular direction, if you ask me I will of course say that I think that people working on background independent approaches to quantum gravity deserve much more support. Among them are Loll and Freidel, that I am glad someone mentioned, but there are many others.

-I did not, as Lubos implies, advocate funding a large number of people who do nothing but think about the foundations of quantum theory. What I do advocate is much more support for the kind of person who might be inclined to work on foundational issues. These are deep and independent thinkers who believe that the road to progress in physics is confronting the hard problems directly. But there is no need to argue about whether more funding for foundations of quantum mechanics would be fruitful. The experiment has been done. For decades there was no support at all, and slow progress. Then, because of the possibility that quantum computers could break codes, there has been a lot of support for the last few years. And a lot of progress has been made, both experimentally and theoretically on aspects of foundations of QM.

-Although this essay was not written to advocate LQG, since it is attacked in response I should try to clear some things up. Someone asks for an accounting of the present status of the field. I among others, have given one in hep-th/0408048, shortly to be updated.

As to the issue of anomalies, i.e. the claim that we ignore the established knowledge that ?INFINITE-DIMENSIONAL CONSTRAINT ALGEBRAS generically acquire anomalies on the quantum level…? is simply false. It is contradicted by rigorous existence and uniqueness theorems in LQG. As a few people do nevertheless take this seriously let me start from a point we can agree about and see if we can clear this up for good. I would hope we can all agree that:

1) The approach to quantization of constrained systems is different in string theory and LQG. The former approach depends on a gauge fixing that refers to a fixed background metric. It results in the construction of a Fock space. The latter is background independent and involves no background metric, no gauge fixing and results in a state space unitarily inequivalent to a Fock space.

2) There is a body of rigorous results that support each kinds of quantization. Hence it cannot be a question of which is correct mathematically. Both are correct, within their contexts. It is a question only of which construction is appropriate for which theories and which describes nature.

3) The treatment of constraints in string theory depends on certain technical features of 1+1 dimensional theories, particularly the fact that there is a gauge in which L_0 plays the role of a Hamiltonian and therefore should, in that gauge, be quantized so as to have a positive spectrum. The anomalies are not generic, as asserted above, rather they depend on the additional condition that L_0 should be a positive operator. There are other reps of Diff(S^1) that are non-anomalous but in which L_0 is not positive. So a choice is made in the standard quantization of string theory, which his motivated by the physics. This does not mean it is the right choice for all physical theories.

4) Conversely the existence and uniqueness theorems which support the LQG quantization work only in 2+1 dimensions and above for the reason that gauge fields don?t have local degrees of freedom in 1+1 dimensions. The existence theorems tell us that there are quantizations in 2+1 and higher of diffeo invariant gauge theories that have unitary, anomaly free realizations of diffeo invariance. The uniqueness theorem tells us that the resulting state space we use in LQG is unique.

5) Now it is true that Starodubstev and Thiemann have found it an interesting exercise to apply the LQG techniques to free string theory. Not surprisingly they get a theory that is unitarily inequivalent to the usual one. This does not mean that the usual quantization of string theory is wrong, nor does it mean that the LQG techniques are wrong when applied to other problems, where the existence and uniqueness theorems together with a large number of results prove their worth. All we learn is that the two quantizations are inequivalent, which was to have been expected.

6) With regard to the non-standard quantization, in which holonomies, but not local field operators are well defined, it is of course true that when applied to standard systems this leads to inequivalent results. ?This apparently leads to unphysical consequences, such as an unbounded spectrum for the harmonic oscillator.? But, give me a break, do you

really think someone is proposing to replace the standard quantization of the harmonic oscillator with the alternative one? What is being proposed is that the quantization used in LQG is well suited to the quantization of diffeo invariant gauge theories.

In case it is not obvious, let me emphasize that harmonic oscillators are not relevent here, and can play no role in a background independent quantum theory, precisely because the division of a field into harmonic modes requires a fixed background metric. Thus, the physics of the problem REQUIRES an alternative quantization.

The detailed motivation is, I think, well argued in the papers, and are supported by the results as well as the existence and uniqueness theorems. First, is well known that a complete coordinatization of the gauge invariant configuration space for a non-Abelian gauge theory requires the holonomies. Second, using them gives rise to the unitary non-anomolous reps of the spatial diffeomorphisms.

Nor is anyone proposing using non-seperable Hilbert spaces for the full theory, the point is that when one mods out by the piecewise smooth spatial diffeos one is left with a seperable Hilbert space.

I am frankly puzzled why someone who claims to know the literature well would throw up examples like the harmonic oscillator up in this context. I can try to understand their point of view, but it certainly reads as if they either are choosing to ignore the basic point, which is that background independent quantizations cannot use fock space, or they are looking to make debating points to impress ignorant outsiders. They must know comments like this are not going to influence experts, because they are, after all, taken from our own papers, written precisely because we wanted to clarify the difference between the new and standard quantizations and the limits of the applicability of each.

With regard to the sociology of the string-loop division, ?Roughly speaking, string theorists are fundamentally particle theorists with a strong understanding of quantum theory, whereas loop people are gravitists with a background in GR?, this is a myth. Rovelli, myself and many other people in LQG were trained as particle physicists, myself at Harvard in the late 70?s. Most of the physical motivation for LQG comes directly from ideas about formulating gauge theories in terms of loops that were studied by Polyakov, Wilson, Migdal, Mandelstam, Neilsen and others. LQG is squarely an outgrowth of their intellectual tradition. The only thing we added was to correctly treat the diffeomorphism invariance exactly in the quantum theory. This led to new results just as the exact treatment of gauge invariance in lattice gauge theory led to new results. I would claim that we made progress in LQG precisely because we had a very good grounding in QFT.

String theory, as it is practiced, makes much more contact with the general relativity tradition, especially the once discredited tradition of extending general relativity to add dimensions and degrees of freedom in the search for a unified field theory. You are much more likely to read a paper which studies solutions to a generalizationsof the Einstein equations, with hbar=0, by a string theorist than by someone working on a background independent approach to quantum gravity.

This of course does not mean that string theory is wrong. But I believe it does mean that by enforcing a narrowly restrictive notion of what constitutes good work, the community of string theorists has hampered progress in string theory by excluding from consideration the lessons learned by attempts to do what string theory must do eventually if it is to be a real theory: which is to find a background independent formulation of a quantum theory of spacetime.”

Comments

1. **Thomas Larsson**
   June 21, 2005

   Actually, I wasn’t talking about spatial diffeos, but rather the group of 4-diffeos, which is the full constraint algebra in covariant formulations. The Dirac algebra is an artefact of the foliation of spacetime which nobody likes, but phase space is intrinsically a covariant concept. This observation apparently goes back to Lagrange and has been emphasized e.g. by Witten, Ashtekar and Rovelli. My suggestion for how one can use cohomological methods to construct the covariant phase space and use it for quantization can be found on the arxiv.

   The reps do not depend on a reference metric or foliation. They do, however, depend on a privileged curve, which I call the observer’s trajectory. The key idea is to expand all fields around it before quantization. The trajectory is not a background structure, however, because must be quantized together with the fields.

   I recently tried to initiate a discussion about this on spr. Since I have already abused Peter’s hospitality by promoting my own ideas, maybe the discussion should be moved.

2. **Lee Smolin**
   June 21, 2005

   Re the last comment, on recovering ordinary QFT from LQG, let me stress again that there are explicit known semiclassical states, and ordinary QFT is recovered at long wavelengths by studying excitations of them. Hence, we know that the physics of flat, or DeSitter spacetime is in the theory.

   The problem is to go beyond these results to
   i) show that the ground state, subject to some appropriate boundary or asymptotic conditions, is such a state, ii) show whether classical spacetime emerges from a generic physical state and iii) show whether lorentz invariance is preserved, broken or deformed by Planck scale corrections in the ground state, and hence predict what should be seen in AUGER, GLAST, ICECUBE and other upcoming experiments.

   In recent papers, Freidel et al show how deformed Poincare invariance arises as the limit of LQG coupled to matter in 2+1. See also hep-th/0501091 for an admittedly heuristic argument that this is true also in 3+1.

   For these see sections 4.4 and 5 of hep-th/0408048 and the references provided there.

   As to whether it might be easier to obtain certain results in a different formulation with anomalous reps of the spatial diffeo’s, perhaps, and this could be worth trying, but only if one does not put in what is to be shown, which would be the case if those reps are constructed with reference to a background metric.

3. **Haelfix**
   June 19, 2005

   “But surely, the new quantization should in some way resemble a Fock type quantization in the limiting case where there is a classical metric. So some limit of the new quantization should recover the familiar harmonic oscillator.”

   Well isn’t thats the big unanswered question though in LQG, that they can’t seem to recover minkowski space? In many ways it makes sense if you think about it, the anomalous reps constrain ones freedom considerably, whereas the generic formalism they use doesn’t contain that structure, or rather it subsumes it so completely it washes it out. Finding a mechanism to retrieve that as some sort of limit strikes me as a *hard* problem in asymptotic behaviour. Everyone who does QG knows how hard that can be, in one guise or another.

4.
   June 19, 2005

The results are very preliminary. I don't think it's in anyone's best interest to make sweeping pronouncements.

good caution. I will try to avoid doing that

5. **Aaron Bergman**
June 19, 2005

thank you Aaron, your words are partially reassuring. I remember the parameters which they discuss tuning—-essentially G (or the inverse kappa) and an asymmetry parameter Delta (or the related alpha) which intuitively tells the "squatness" of 4-simplex (its foreshortening of timelike edges).

Those are the parameters are inputs to the model. They don't tune them so much as describe the phase diagram of their model in the plane of those two parameters. They find one phase which seems to describe a macroscopic universe.

This is a different issue than the continuum limit where, as you take the spacing to zero, you tune various parameters to achieve a phase transition.

Also, I never said that the continuum limit of this theory (if such a thing exists) is something like only lives on S^3 x S^1. Who knows what will happen. They have some interesting ideas on the effective dimension as a function of length scales, but this doesn't necessarily mean some bizarre spacetime topology. Rather, it could describe some sort of fuzzy graviton, or something else. Beats me.

It's not even clear that these models describe anything really. The results are very preliminary. I don't think it's in anyone's best interest to make sweeping pronouncements.

6.
June 19, 2005

Aaron:*You can't just take the lattice size to zero to get a continuum limit. Things are generally more complicated than that and involve tuning various couplings so as to achieve a phase transition in the limit. Ambjorn et al comment on this on pp. 12-3.*

thank you Aaron, your words are partially reassuring. I remember the parameters which they discuss tuning—-essentially G (or the inverse kappa) and an asymmetry parameter Delta (or the related alpha) which intuitively tells the "squatness" of 4-simplex (its foreshortening of timelike edges).

but I am still not fully confident that even with the correct tuning of the parameters for a "continuum limit" —obtained by computing the path integral for simplexes of size 'a' repeatedly as 'a' goes to zero—-I am still not confident that in the limit there is a continuum with topology of R x S3

Or, as you say, S1 x S3, since they sometimes make it periodic so it fits in the computer and replace R by the circle S1, in which case what we are talking about may really be S1xS3.

I appreciate the clarification, but I still have a suspicion that the spacetime of Renate Loll is not topologically either S1xS3 or RxS3 but may instead be a mathematical novelty.

(my doubts arose partly because the dimension varies with scale and tends to be quite a bit less than 4 at close quarters)

7. **Aaron Bergman**
June 19, 2005

the basic CDT spacetime is the set R x S3, but I am not sure that the topology is R x S3. In fact I am unsure how to define the topology on it, except as the cumulative effect of many histories, or in other words as a somewhat uncertain topology.

The topology of each element in their path integral is always the same, usually S^1 x S^3.

the CDT continuum is constructed as the limit of PL 4-manifolds as the size of the simplex goes to zero. Any PL 4-manifold is a topological 4-manifold. In this case each of the approximating PL 4-manifolds is topologically R x S3.

You can't just take the lattice size to zero to get a continuum limit. Things are generally more complicated than that and involve tuning various couplings so as to achieve a phase transition in the limit. Ambjorn *et al* comment on this on pp. 12-3.

8.
June 19, 2005

Chris W: *From Quantum general relativity and the classification of smooth manifolds (Hendryk Pfeiffer):*

…The diffeomorphism invariance of the classical observables then implies in the language of the triangulations that all physical quantities computed from the path integral, are independent of which triangulation is chosen. The discrete formulation on some particular triangulation therefore amounts to a complete fixing of the gauge freedom under space-time diffeomorphisms…

(I'll leave it at that. It's been raining in Vermont for several days. I've spent a good portion of the past 36 hours whitewater kayaking, and could use a good night's sleep.)

—
Chris, that was a very interesting paper by Hendryk Pfeiffer. I have a glaring point of mathematical ignorance that perhaps you might help me with.

the basic CDT spacetime is the set R x S3, but I am not sure that the topology is R x S3. In fact I am unsure how to define the topology on it, except as the cumulative effect of many histories, or in other words as a somewhat uncertain topology.

the CDT continuum is constructed as the limit of PL 4-manifolds as the size of the simplex goes to zero. Any PL 4-manifold is a topological 4-manifold. In this case each of the approximating PL 4-manifolds is topologically R x S3.

But I am not sure in what sense, if at all, the limit of such things has to be a topological 4-manifold. This is Hendryk Pfeiffer's term—-locally homeomorphic to R4. In a topological 4-manifold, as Pfeiffer says, the transition (i.e. coordinate change) functions are C0 continuous but not necessarily differentiable.

So I am not sure that Pfeiffers paper would apply to the CDT spacetime, for instance.

BTW I notice he posted the first version 21 April 2004. A couple of weeks later Pfeiffer was at the "Loops 04" conference that Carlo Rovelli organized at Marseille, and talked about it. On the second day of the conference, Tuesday 4 May, Renate Loll gave a talk on CDT in the morning and Hendryk Pfeiffer spoke that afternoon. It would have been an appropriate time to ask this kind of question, I suppose.

There may be some embarrassingly obvious answer to my doubts about this. If you are aware of one please let me know after you have rested up from kayaking.

9. June 19, 2005

this is not intended to elicit a response from Lee Smolin (if he happens to be still reading the thread) but simply to acknowledge that I stand corrected by him on the subject of LQG and spin foams spacetime discrete structure. Accordingly I remain undecided on how to view the CDT lack of discreteness declared by Loll et al for instance on page 2 of hep-th/0505113. They say their probing by Monte Carlo-style simulation has so far not uncovered evidence of “fundamental discreteness…[or]…a minimal length scale.”

1. I can, on the one hand, set aside notions of reconciliation and consider LQG and CDT to be incompatible models, each consistent and supporting calculation, of quantum spacetime dynamics. Presumably, each is able to make testable predictions (though CDT is at a somewhat earlier stage of its development) and empirical observation will eventually distinguish between them.

2. On the other hand, I can surmise that Loll et al have not probed sufficiently and that further computer simulation or analysis might uncover discrete spacetime structure in their model. For instance, if I understand correctly, no one yet has constructed an operator in CDT corresponding to an observer measuring a physical area or volume. If this is eventually done, further along in CDT development, I cannot see how to rule out the possibility that such an operator have discrete spectrum. Thus some discrete structure might appear at a later stage.

sorry for misstating Smolin’s role in PI, back a ways in this thread—he cleared that up

10. **Juan R.**
June 19, 2005

Some remarks,

For the history of early “eigenvolumes” and the use of quantums of volume (in the same sense that quantums of energy) see

Length Scale for the Constant Pressure Ensemble: Application to Small Systems and Relation to Einstein Fluctuation Theory. J. Phys. Chem. 1996, 100, 422-432.

One can prove that those “quantum of volume” and related to quantum of area of standard BH. One can also prove that in the limit quantumA –> 0 the BH is stationary (constant total A).

You have claimed in several articles that departures from standard (string theory)

E^2 = p^2 + m^2

predicted from LQG could be “directly” verified in the next generation of acellerators (2007?). Have you considered an “indirect” verification of quantum of volume at macroscopic scales? I am working in that.

What is the status of Lorentz invariance and frame independence in today LQG?

You claim a consistent classical limit. Can LQG obtain departures from GR at the extragalactical scale (e.g. TF law for anomalous galaxies)?

11. **Juan R.**
June 19, 2005

Lee Smolin,

Regarding discrete structure of spacetime, i think that can be proved indirectly with usual experiment.

In fact, some well proven mathematical theorems show that if area and volume are really zero one obtains wrong answers for macroscopic questions. It is an usual error to believe that classical gravitation is based in a pure diferential manifold. It is a error conected with some wrong asumptions of usual calculus (non standard analysis and hiperreal numbers solve that partially)

I can prove that if quantum of area –> 0 then there is no Hawking radiation, there is no dissipative effects at astro or cosmo scale, etc. Therefore there is quantum sure!

The quantum of area/volume is related to departures from standard (string theory)

E^2 = p^2 + m^2

Moreover, the existence of quantum of volume is needed in the computation of partition functions, since that many standard computed PF have dimension of volume. That is well known. In fact the first “quantums” of volume were named eigenvolumes by Guggenheim.

I think that you are not replied M query. I am sure that one cannot obtain a pure consistent classical state from LQG, due to mathematical incongruencies. Someone derived GR from LQG?

I am not talking about obtain certain spectra that like how. The same situation arises in usual limits of QM. Somewhat like one need “decoherence” effects (h^2 terms to Schrodinguer like in standard Calderia-Legget equation) we need add news terms to usual LQG. This also solves the old problem of time in HQG; Wald recent proposal is unnecesary.

“So the attitude is rather different from other approaches. Some string theorists admit they do not know what string theory is, but they nevertheless are sure it is right.”

Great!!! One day Witten admits that nobody know that is really string or M theory, and another day he claims that it beatiful and elegant!

12. **Thomas Larsson**
June 19, 2005

Lee: I realize that what I wrote could be construed as a personal attack, and I apologize for that. My excuse is that I feel a great sense of frustration for not being noticed (by physicists, mathematicians were always much more open-minded). I started to work on multi-dimensional generalizations of the Virasoro algebra back in 1987 and published the first paper in 1989. Although not not explicitly stated, it was always obvious to me that this algebra must have applications to quantum gravity, for the same reason that the ordinary Virasoro algebra is relevant to string theory.

My funding ran out in 1993, which made sense at that time because my program was stuck. But after the key obstacle was removed by Rao and Moody a few years later, it was possible to develop a representation theory, which may be regarded as the quantum analogue of tensor calculus (tensor fields carry classical reps of the diffeo group). It was totally obvious to me that this must be to quantum gravity like tensor calculus is to classical gravity, i.e. very important. The complete lack of interest was extremely frustrating in that situation.

Thus, I behaved nicely throughout the 1990s, and it didn't do me any good. After more than a decade, one can start to lose patience. I only started to receive feedback after I criticized string theory in math-ph/0103013 (from a quite original viewpoint). This taught me that being nice is something that the physics community simply does not award. So be it.

Given my present age and family situation, I am not really personally interested in funding anymore; not unless it comes in the form of a permanent position in Stockholm without teaching duties anyway. But I would very much like to see an influx of people thinking along these lines, because I believe it is a promising idea, but I myself am stuck at this time.

On a different note, I think that your proposals have some serious problems. In what way would your Einstein fellowships differ from a MacArthur genius grant except that string theorists are excluded? Who would decide who should get these grants – you, Ed Witten or maybe Lubos Motl? It seems to me that funding must ultimately be decided by tenured professors, i.e. the same people who decide about funding today. Finally, should a paper like hep-th/0412325 be regarded as string theory or not?

13. **Arun**
June 19, 2005

" So long as the resulting theory is well defined, I don?t see the force of an argument from a priori grounds. that experience shows Fock type quantizations must be right in all cases because they only work when there is a fixed background metric, while the whole point of the new quantization is that it provides an answer to the question of how to construct a well defined QFT in the absence of any background metric."

But surely, the new quantization should in some way resemble a Fock type quantization in the limiting case where there is a classical metric. So some limit of the new quantization should recover the familiar harmonic oscillator.

14. **Kea**
June 18, 2005

"Also see gr-qc/0311055, gr-qc/0407094, and gr-qc/0407093"

Cool – more category theorists.

15. **Chris W.**
June 18, 2005

From **Quantum general relativity and the classification of smooth manifolds** (Hendryk Pfeiffer):

> This suggests that the path integral of general relativity in d ≤ 5 + 1 admits a discrete formulation on such triangulations. General relativity in d ≤ 5 + 1 is therefore related to what we call a PL-QFT, i.e. a TQFT based of piecewise-linear manifolds. In particular, a path integral quantization of general relativity in d ≤ 5 + 1 is related to the construction of invariants of piecewise-linear manifolds. From the classification results, we will see that this is most interesting and in fact an unsolved problem in topology, precisely if d = 3 + 1. It is the decision to take the diffeomorphism gauge symmetry seriously which singles out d = 3+1 this way.
>
> The diffeomorphism invariance of the classical observables then implies in the language of the triangulations that all physical quantities computed from the path integral, are independent of which triangulation is chosen. *The discrete formulation on some particular triangulation therefore amounts to a complete fixing of the gauge freedom under space-time diffeomorphisms. The relevant triangulations can furthermore be characterized by abstract combinatorial data, and the condition of equivalence of triangulations can be stated as a local criterion, in terms of so-called Pachner moves*. [emphasis added] ?Local? here means that only a few neighbouring simplices of the triangulation are involved in each step. A comparison of Pachner moves with the block-spin or coarse graining renormalization group transformations in Wilson?s language reveals what renormalization means for theories with dynamical geometry for which there exists no a priori background geometry with which we could compare the dynamical scale of the theory.
>
> —
>
> **If quantum general relativity in d = 3+1 is indeed a PL-QFT, the following two statements which sound philosophically completely contrary,**
>
> - **Nature is fundamentally smooth.**
> - **Nature is fundamentally discrete.**
>
> **are just two different points of view on the same underlying mathematical structure: equivalence classes of smooth manifolds up to diffeomorphism.**

Also see gr-qc/0311055, gr-qc/0407094, and gr-qc/0407093.

(I'll leave it at that. It's been raining in Vermont for several days. I've spent a good portion of the past 36 hours whitewater kayaking, and could use a good night's sleep.)

16. **Lee Smolin**
June 18, 2005

Thanks again for all the insightful comments. Perhaps I can add something to a few of the threads of discussion.

-On LQG and discrete structure. First, do we agree that even though electrons move in space the spectrum of the hydrogen atom being discrete means that quantum mechanics of the atom has discrete structure? In a very similar sense, since all the geometric observables including volume, area (and yes length) have discrete spectra, corresponding to a discrete basis (of diffeo classes of embeddings of labeled graphs) then the quantum geometry of space has become discrete. The key point is that the discreteness scale-roughly L_Planck, cannot be taken to zero, otherwise black hole entropy comes out wrong, and semiclassical states do not correspond to classical metrics.

-But it is true that if you derive a version of LQG from a strict quantization of GR, there is a fixed background, which is the bare differential manifold. There is no background metric but there is a background topology and differential structure, defining the diffeo classes of embeddings of the spin networks.

-Hence, Markopoulou followed by Freidel and others, proposed dropping the embedding and basing the theory just on combinatorial spin networks. These models are then discrete in a stronger sense. There are some advantages to this (reformulation in terms of a matrix model, cleaner relation to causal sets) but one can non longer claim the theory is a precise result of a quantization of GR. Both frameworks, with and without embeddings, continue to be studied.

-The discreteness of length was shown in T. Thiemann, gr-qc/9606092, J.Math.Phys. 39 (1998) 3372-3392. Angles also have disrete spectra: S. Major, Class.\ Quant.\

Grav.\ {\bf 16}, 3859 (1999) gr-qc/9905019; gr-qc/0101032.

-On spin foam models and discreteness. There are several different spin foam models under study. In all of them a history is a discrete labeled combinatorics structure (for example branched 2-complex.) In some of them the label sets are continuous because they come from the rep theory of Lorentz or Poincare and areas are not discrete. But these have not been shown to correspond to evolution amplitudes for canonical states. Others (Reisenberger, Markopoulou, etc) do give evolution amplitudes for spin networks and have discrete areas.

– M asks, is there a suitable correspondence principle where known physics can be recovered? The answer is yes. There are several results that show that excitations of certain LQG states reproduce, for momenta small in Planck units, the spectra of conventional QFT?s including gravitons, photons etc on flat space or de Sitter spacetime. Some are cited in section 4.4 of my review hep-th/0408048. See also hep-th/0501091. See recent papers by Freidel, Livine and others that show in full detail how standard Feynman perturbation theory emerges from a spin foam model for gravity coupled to matter in 2+1 when G_Newton goes to zero.

As to what people in non-string approaches to quantum gravity are doing, I agree, why not look at the conferences? Here are some recent ones, some with talks available.

http://www.cpt.univ-mrs.fr/%7Erovelli/program2.html
http://www.ws2004.ift.uni.wroc.pl/flash.html
(talks at: http://www.ws2004.ift.uni.wroc.pl/html.html)
http://www.perimeterinstitute.ca/activities/scientific/PI-WORK-2/

-Several of the comments ask, why quantize as in LQG? Why not quantize with another approach (such as one that uses anomalous reps?)

I do not see how there can be an apriori reason to prefer one quantization scheme over another one. Our job is to construct candidate quantum theories of gravity, compare their results and learn from them. In LQG there are existence and uniqueness theorems that prove that the approach exists, and theorems that guarantee uv finiteness. Thus, the approach leads to a structure that mathematically exists and within which computations can be done. Many computations have been done.

We are thus no longer at a stage where it is interesting to ask why do or why not do questions. There are now a different class of questions which include: Does the theory make predictions? How do they compare with experiment? What properties have been shown? What remains to be shown? There are certainly several key open issues to discuss, and we are not shy to discuss them.

No one is claiming that we know LQG is the right theory of nature. We are claiming that it is a well developed approach, that gives an apparently consistent answer to what we think is a necessary question, which is how to construct a diffeo invariant QFT in the absence of a fixed background metric. This gives a rich arena with many open problems and many things to do either to understand it better, make predictions, or as a jumping off point for the invention and study of new theories.

So the attitude is rather different from other approaches. Some string theorists admit they do not know what string theory is, but they nevertheless are sure it is right. In LQG we study well defined theories, which have many good properties, but most of us feel no need to ?believe in them? pending experimental confirmation.

So our attitude is if someone like Thomas Larsson has a different approach that?s great. We know what its like to be starting something new other people don?t understand or support, and we will support you, so long as you don?t waste your and our time attacking us on a priori grounds. We suggest you should try to develop your ideas to at least the point where we can compare the results.

For example, someone asks, ?What's wrong with anomalies? Sure, it turns first class constraints into second class constraints, but Dirac showed us how to deal with that.? Fine, we only insist that this is not the only way. The LQG results and theorems show that you can find diffeomorphism invariant states through a different procedure, involving only first class constraints, which is

a. Construct a kinematical Hilbert space, which is a rep of a Poisson algebra that coordinatizes the phase space, which carries a unitary and non-anomalous rep of the spatial diffeo?s.

b. Use that non-anomalous unitary rep to construct explicitly another Hilbert space, which is the space of diffeomorphism invariant states.

c. Compute many observables of interest representing diffeo invariant classical quantities as finite operators on this space, leading to predictions of physical interest, an ultraviolet finite theory etc.

There are by now so many rigorous results supporting this construction that the burden of proof is on the other side: given that this procedure works and leads to a well defined finite physical theory, why not explore its consequences as a possible quantum theory of gravity?

So when Urs says, ?It seems to me that the reason to drop weak continuity in the quantization of gravity in 3+1 dimensions is that it makes an otherwise intractable problem tractable – but possibly at the cost of having oversimplified a hard problem,? fine, but lets discuss the results. Does this lead to a space of states with enough physical states and with a well defined dynamics? YES. Are some states interpretable as semiclassical states? YES. Does that dynamics have all the properties we require for a quantum theory of gravity? YES to some questions such as uv finiteness, other questions are still open, such as a proof that the ground state is semiclassical.

-Aaron says, ?It is, in fact, a radically different approach to quantization that, when applied to current theories, gives experimentally incorrect answers.? Thomas Larsson argues that ?I find it very disturbing that LQG methods yield the wrong result for the harmonic oscillator.? I don?t understand the logic of their arguments at all. Yes, it is a different quantization, i.e. one based on representations of the algebra of Wilson loops and electric flux?s rather than local field operators. Yes, it is unitarily inequivalent to Fock space. That is good, as Fock space knows about a particular fixed background metric. If a background independent Hilbert space, which quantizes the whole space of metrics, were unitarily equivalent to a Fock space based on a single fixed metric, something would be wrong.

The claim is precisely that this is a new class of QFT?s which is available to quantize diffeo invariant gauge theories in 2+1 dimensions and above, and which has novel features and leads to novel results. So long as the resulting theory is well defined, I don?t see the force of an argument from a priori grounds. that experience shows Fock type quantizations must be right in all cases because they only work when there is a fixed background metric, while the whole point of the new quantization is that it provides an answer to the question of how to construct a well defined QFT in the absence of any background metric.

-If you still want to have an argument on a priori grounds as to why representations of non-canonical algebras will be required to have a background independent quantum theory of gravity, please go back to the papers of Chris Isham from the late 70?s and 80?s where he made a detailed and convincing case for this. These papers, together with the work of Polyakov, Wilson, Midgal etc on formulating quantum gauge theories directly in terms of Wislon loops were the major motivation for LQG. What we did was construct the non-canonical algebras Isham called for from Wilson loops. Also, please note that lattice gauge theory is not based on Fock space.

-Finally, I am not a director of PI, just one of the scientists, so PI is very far from ?Smolin?s institute?. Also, when I am defending LQG I try to discuss the whole research program, not my own personal work, which departs in some papers quite a bit from that of many of my friends.

17.
June 17, 2005

Chris, another thought in connection with what you said earlier:

It should be mentioned that at least some string theorists seem to have a particular antipathy to conjectures about a possible discrete substructure for spacetime, which often play a role in alternative approaches to quantum gravity.

Your reference is a bit vague, but you may have heard TALK about a discrete structure of spacetime in canonical-LQG.
In fact the model of spacetime used in that approach is a differentiable manifold, as is the LQG model of 3D space. The basic variables and observables of that theory are constructed on this smooth manifold (not on a lattice or set of discrete points, a mistaken impression easy to get.)

As canonical-LQG theory develops, the spectra of the area and volume operators turn out to be discrete. This means that even though space and spacetime are represented by smooth continuums, when one comes to actually measure some physical area or volume the outcomes of measurement must in theory be confined to a discrete set, which can be calculated in planck units. (The points in the set are very close together, on the order of a planck unit area or unit volume apart, but they are nevertheless separate points). I find this puzzling, and can't say I quite understand how observation of area and volume can have discrete spectrum. But that is how it turns out.

Not so with length. In canonical-LQG the operator corresponding to measuring a length, if I remember correctly, has not as yet been shown to have discrete spectrum.

Popular accounts of canonical-LQG ordinarily make much of the discrete spectra of area and volume. So also do non-technical survey articles by LQG pioneers such as Ashtekar and Rovelli. That is understandable (the discrete spectra of certain measurement operators are very interesting and have far-reaching consequences) but how far one wants to go towards interpreting that as spacetime's "discrete structure" is a somewhat matter of taste.

Since the late 1990s the interest of QG researchers has shifted noticeably away from canonical-LQG towards the spin foam approach, whether rightly or wrongly remains to be seen. In the spin foam approach, AFAIK, there is so far no proof that area and volume operators have discrete spectrum.

In my non-expert judgment as an observer, if you put together CDT (e.g. Loll et al) research with Spin Foams (e.g. Freidel et al) you'd get at least 60 percent of current work on spacetime dynamics. And therein would be no discrete spacetime, nor even discrete spectra of area and volume!

But some of the remaining percentage of the work would be canonical-LQG with its discrete spectra albeit constructed on a smooth continuum.

I don't have statistics on this, only an impression from following the literature, and I am excluding quantum cosmology research (e.g. Bojowald et al) where the model has only a finite number of degrees of freedom. This is a quantum analog of the usual Friedmann equation of classical cosmology—quantum cosmology is symmetry-reduced so it can deal with a finite number of parameters instead of a whole spacetime geometry. Several of the operators in loop quantum cosmology have discrete spectra.

In short, the situation is complicated and it is not at all clear that "QGATS" (quantum gravity alternatives to string) research is moving in the direction you suggest, namely "discrete substructure of spacetime". Nor is it clear what discrete substructure means, in general.

Probably those you mention as expressing their "particular antipathy" can safely be ignored since they could well be saying more than they actually know about non-string QG. It is no use arguing with them, I should guess, about discreteness, or Lubos Motl's "aether", or anything else, for in my experience the antipaths can always find more reasons for antipathy.

18.
June 17, 2005

[Chris W. at June 17, 2005 12:36 PM]:
It should be mentioned that at least some string theorists seem to have a particular antipathy to conjectures about a possible discrete substructure for spacetime, which often play a role in alternative approaches to quantum gravity.

"often play a role" is vague, Chris, and could be misleading. Aaron Bergman just brought up recent work of Ambjorn and Loll that interested him. This is a prominent example of QG alternatives to string. There is no discrete structure or any suggestion of a minimal length. Here is a quote

http://arxiv.org/hep-th/0505113
Spectral Dimension of the Universe

—quote from page 2—
We have recently begun an analysis of the microscopic properties of these quantum spacetimes. As in previous work, their geometry can be probed in a rather direct manner through Monte Carlo simulations and measurements. **At small scales, it exhibits neither fundamental discreteness nor indication of a minimal length scale**.
—end quote—

Chris, I urge you to keep abreast of the research in QG alternatives to string, which is moving rapidly and does not accord with common "hearsay" that one may get from string experts. I hope you are not relying on hearsay.

The paper which Aaron gave a link to, also recent and by the same authors, is
http://arxiv.org/hep-th/0505154
Reconstructing the Universe

It would be a good place to get firsthand impressions about this particular non-string development (called CDT, causal dynamical triangulations)

Chris, you say:
Lubos Motl has suggested that such ideas are tantamount to misguided attempts to resurrect the classical aether in the context of quantum gravity.

I suspect if we were to go through on a case by case basis we would find that you have received a number of erroneous impressions from Lubos Motl.

19. **Thomas Larsson**
June 17, 2005

Anonymous June 17, 2005 10:16 AM:

It is clear that some of us attach more significance to the harmonic oscillator than you and Smolin do. Fine. But let us at least agree that the Helling-Policastro result deserves to be widely known, just as the fact that string theory is not background independent is well known. Then people can make up their own minds.

20. **Chris W.**
June 17, 2005

It should be mentioned that at least some string theorists seem to have a particular antipathy to conjectures about a possible discrete substructure for spacetime, which often play a role in alternative approaches to quantum gravity. Lubos Motl has suggested that such ideas are tantamount to misguided attempts to resurrect the classical aether in the context of quantum gravity.

It would be interesting to read a thoughtful exposition of the philosophical presuppositions underlying the string theory program (or programs). I haven't come across one so far, and given the impatience of many string and particle theorists with philosophical discussion of any kind I don't really expect to see one any time soon.

This intersects in my mind with the issue of how to evaluate and *interpret* quantization methods. Here again is a subject for which thoughtful philosophical discussion could be most illuminating. One might wonder why the formulation of a quantum theory should depend on "quantizing" a classical prototype at all. A key notion appears (to me) to be that in our currently accepted understanding of quantum theories, discreteness is a feature of *certain* solutions, and does not really reside in the fundamental assumptions shared by quantum theories in general. Indeed, quantum field theory is supposed to largely explain the origin of the discrete substructure of matter that was taken as a given* in 19th century and early 20th century physics.

—
(* ..with notable exceptions, of course.)

21.
June 17, 2005

I did not yet get an answer to my question to Urs Schreiber and Robert Helling about rival lines of QG research alternative to string (which I will call "QGATS" for lack of a better term).

So I will repeat the question, just to be clear what i am asking.

The people contributing here (including the string experts Urs Schreiber and Robert Helling) seem to mean several different things by "LQG", so that confusion gets into this thread of discussion very easily.

I would like to know what Schreiber and Helling would identify as the currently active lines of research that are QUANTUM GRAVITY ALTERNATIVES TO STRING.

Which people would you gentlemen say are leading QGATS researchers? What papers have been posted recently, say in the past twelve months, that are significant QGATS papers?

It would be very helpful to know who you think leading people are, especially the younger crop just getting established.

It would be useful to have some specific arXiv numbers of research articles posted in the past twelve months that could serve to typify for us what you think are the main rival directions to string.

[Posted by: at June 16, 2005 01:24 PM]

I am grateful to Aaron Bergman for his brief response, with link to a paper by Renate Loll and her co-authors Ambjorn and Jurkiewicz.

But it is unfortunate that Urs and Robert have not yet replied (or perhaps they will choose never to reply to this question.)
I will explain why I think it is unfortunate.

The discussion of the issues raised in Smolin "New Einstein" essay is clouded by people naively equating
Smolin = LQG = QGATS
So people in this thread dismiss Smolin's call for more program diversity in US instutions, including QG rivals to string, as self-serving. They assume it would simply result in more support for LQG *whatever that is* (none of our critics seem to have an accurate idea of Smolin's research interests, or what should be called LQG, or what the range is of rival alternative lines of research).

What is even more unfortunately misleading is an oversimplification often suggested by posts like those of Schreiber and Helling:

QGATS = LQG = Thomas Thiemann's January 2004 paper.

Whether or not this is intentional, it has seemed to me that as soon as the possibility is raised that money or attention might be reallocated from string to some alternative QG lines of research, we immediately begin to hear references Thiemann's Loop-String paper. 🙂

22.
June 16, 2005

I can't believe the way people are falling all over themselves over this Smolin guy. Why not read Thomas Larsson?…
Posted by: D R Lunsford at June 16, 2005 09:49 PM

That is an interesting question, drl. I think the answer in part is that Smolin is a leader in "Quantum Gravity Alternatives to String" research. Call it "QGATS" if you like acronyms 🙂
the older term "LQG" is no longer sufficiently inclusive or adequately descriptive.

"QGATS", or whatever you want to call it, is now a hot group of research lines. Postings on arXiv have been increasing sharply over the past 2 or 3 years, while string-related postings (and citation standings) have stagnated or declined.

Smolin's institute (PI) is one of the few places in the world where there is a rough balance between research in string and Quantum Gravity alternatives.

Two other places I can think of are Hermann Nicolai's branch of Max Planck Institute (AEI-Potsdam) and it seems now also Gerard 't Hooft's institute at Utrecht.

Interesting things are happening at all these places. And they are the exception—only a handful of such institutes worldwide. So Smolin is not only a leader in an interesting area, "QGATS" :-), but he is also playing in an interesting league. Accordingly, it is not unusual for someone in touch with current developments in theoretical physics to take note of Smolin's point of view.

23. **D R Lunsford**
June 16, 2005

I can't believe the way people are falling all over themselves over this Smolin guy. Why not read Thomas Larsson? He's an order of magnitude more interesting. And I'll bet he's right.

-drl

24. **M**

June 16, 2005

From Juan:

You can continue erasing my posts Peter, since that your "arguments" (to say) for erasing them is not consistent i continue to post here because information is important. If your argument is simply "i erase because i want do it", then please explicit it in "your" blog philosophy. I don't post my last three or four erased posts, simply the later of minutes ago (with minor modifications). (This was followed by a reposting of a comment that Peter had apparently previously deleted.)

Juan,

Your insistence on violating Peter's guidelines bothers me a lot, and probably many others as well. This blog is Peter's project, something that takes some of his precious time to build and maintain, and willfully violating his requests and standards so that you can push your own views, your own idea of what is important, is simply improper. A blog is not an unmoderated public forum; it is not your given "right" to say whatever you want just because you personally think it is important. If you think certain things need to be said, then find another forum (e.g., sci.physics.* Usenet groups) where you can raise and discuss those issues. It is important to remember we are all guests here, and should treat the host (in this case, Peter) with the same respect we would want to be treated with if we were the host or hostess.

It should be clear that explicitly violating Peter's guidelines for keeping comments on topic does not impress others. More likely, it gives you more the appearance of a crank, and makes it much less likely that others will take you seriously. It gives the appearance of a guest who acts rudely to the host or hostess; even worse, the guest apparently complains loudly when the host protests their rude behavior.

So please, Juan, respect Peter's repeated requests to keep posts on the topic. For an umoderated environment, take advantage of the Usenet groups.

25.
June 16, 2005

Aaron: *Speaking for myself, I find the recent results of Ambjorn et al intriguing, although I don't claim to understand them all that well right now.*

Since the link you gave in your post is
http://arxiv.org/hep-th/0505154
Reconstructing the universe

you might also be interested by another recent paper by one of the same authors
http://arxiv.org/gr-qc/0506035
Counting a black hole in Lorentzian product triangulations>

26. **Alejandro Rivero**
June 16, 2005

Hmm Aaron I can imagine in three minutes three arguments to relate d=2 and d=4 in the context of quantum gravity.

– That the curvature tensor is basically a 2-dimensional object,

– That path integrals have the custom of having fractal dimension two.

– That Polyakov action in NonCommutative geometry comes from the four dimensional integration of a two dimensional object (NCG integrates via dixmier trace, you can do strange things).

-that a string has worldsurface d=2

(did I said three arguments? Well, the last one does not score)

27.
June 16, 2005

Aaron, I just saw your post about the work by Ambjorn, Jurkiewicz and Loll. I don't have time to change my own post, which follows, to accord with yours but I agree—find the Dynamical Triangulations work intriguing.

It is to be one of the main topics at the Loops 05 conference at AEI in October, and Renate Loll is on the invited speakers list.

—-what I was going to post before I saw Aaron's was this—
it seems to me that as a first approximation a good map of the current active research in QUANTUM GRAVITY ALTERNATIVES TO STRING is given by the non-string topics posted at the "Loops 05" conference website

http://loops05.aei.mpg.de/

Background Independent Algebraic QFT
Causal Sets
Dynamical Triangulations
Loop Quantum Gravity
Non-perturbative Path Integrals

A representative list of researchers would be the list of invited speakers for that conference, which has been posted although the conference is still several months away.
http://loops05.aei.mpg.de/index_files/Programme.html

I think that the underlying concern shown by several of the posters here is with MONEY. Does string get to keep it or does it have to share some with its rivals? Therefore we cannot afford to be vague about who the rivals are. Funding for young researchers is an especially important topic in this thread and in the earlier "New Einsteins" thread. Thanks to Smolin for bringing issues of support allocation for young theory people (grad students, postdocs, young faculty) to the fore.

Because rival research lines are at issue, we must be clear about what the main QGAS efforts are. If we include some non-string QG in the physics department of a US university, what kind of research would it be? Name some papers from the past twelve months. Name some young researchers featured in the Loops 05 list of invited speakers.

28. **Aaron Bergman**
June 16, 2005

Speaking for myself, I find the recent results of Ambjorn *et al* intriguing, although I don't claim to understand them all that well right now.

29. June 16, 2005

The people contributing here (including the string experts Urs Schreiber and Robert Helling) seem to mean several different things by "LQG", so that confusion gets into this thread of discussion very easily.

I would like to know what Schreiber and Helling would identify as the currently active lines of research that are QUANTUM GRAVITY ALTERNATIVES TO STRING.

Which people would you gentlemen say are leading QGAS researchers? What papers have been posted recently, say in the past twelve months, that are significant QGAS papers?

It would be very helpful to know who you think leading people are, especially the younger crop just getting established.

It would be useful to have some specific arXiv numbers of research articles posted in the past twelve months that could serve to typify for us what you think are the main rival directions to string.

30. **Urs Schreiber**
June 16, 2005

Hi Ummm,

OK, I am not 100% sure what your point is, now.

Fact is that in many discussions about that anomaly issue people get confused by the fact that in string theory there is a gravitational theory on parameter space and one on target space.

In the context of what has become known as the 'LQG-string' one is concerned exclusively with the issue of the quantum gravity theory on parameter space. All background dependence of string theory is a red herring for this particular discussion.

Which of course does not mean that there is nothing to discuss concerning the quantization of the target space gravity theory. But that's not the issue of the 'LQG-string'.

Best,
Urs

31. **Ummm**
June 16, 2005

Hi Urs,

I am in agreement that the worldsheet theory is not background dependent. My point is that when an LQG enthusiast gets a bug up their bum about background dependence, they're talking about strings as perturbation theory against a particular space-time. It's not necessary to be too technical about this.

32. **Peter**
June 16, 2005

To Juan and others:

Do not post here attempts to carry on a discussion about alternatives to GR, QM, etc. This is not a general physics discussion forum. Please do this somewhere else. I will continue to delete all such posts.

33. **Urs Schreiber**
June 16, 2005

Hi Ummm,

no, the issue I commented on is the quantization of the worldsheet theory and how its LQG-like quantization relates to the standard one. This is 2D gravity coupled to scalar fields and it is irrelevant for its discussion whether you want interpret these scalar fields as embedding fields into a target space or not.

So, if it helps, you can forget about the idea of fundamental strings for the moment and just consider the quantization of 2D gravity coupled to scalar fields. This is well understood. In the standard formalism, which uses weakly continuous representations of operators, the ADM constraint algebra of this gravitational system always has an anomaly.

34. **Thomas Larsson**
June 16, 2005

The Thomas mentioned in Robert's post is Thiemann, not me. Also, I am pretty sure that Stone-von Neumann only applies to QM with finitely many degrees of freedom, so it might not be directly relevant to gravity.

35. June 16, 2005

Ummm, the fixed background that Smolin refers to is the target space-time, fixing this is part of the definition of the CFT. Surely you realize that this is an important open problem?

36. **Urs Schreiber**
June 16, 2005

Concerning the different approach towards anomalies in the LQG-like quantization of the string and the ordinary quantization, Lee Smolin wrote:

> The approach to quantization of constrained systems is different in string theory and LQG. The former approach depends on a gauge fixing that refers to a fixed background metric.

I guess this refers to the quantization of the Polyakov action after the conformal gauge has been fixed. It is worth pointing out that precisely the same constraint algebra is obtained by taking the Nambu-Goto action or the Polyakov action, regarding them as (background free) gravity on the worldsheet coupled to scalar fields on the worldsheet and compute the ADM constraints of these. One finds a Hamiltonian constraint L + bar L and a diffeomorphisms constraint L – bar L. No gauge has to be fixed at any time.

(This can easily be checked. The calculation is for instance given in Henneaux's old lecture notes on string theory.)

So the quantization of the string is precisely about the quantization of a background independent gravitational system in two dimensions. And any weakly continuous quantization of the resulting constraint algebra does feature an anomaly.

The resason that 'oscillators' make an appearance in this background free quantization is merely due to the special property of the constraint algebra in 2D to have structure constants instead of structure functions. This makes Fourier decomposition in parameter space a useful tool.

It seems to me that the reason to drop weak continuity in the quantization of gravity in 3+1 dimensions is that it makes an otherwise intractable problem tractable – but possibly at the cost of having oversimplified a hard problem.

37. **Robert**
June 16, 2005

As Lee goes into some details, I would like to mention our preprint hep-th/0409182 where we discuss Thomas' approach to the quantization of the string (and also the harmonic oscillator).

There, the upshot is, that the GNS-state of Thomas is not (weakly) continious, a property that is up to discussion but I consider quite suspicious. In the case of the oscillator it leads to a state that is that of an oscillator coupled to a heat bath of infinite temperature. This does not look too promising.

Furthermore, (although this argument might not be rigorous) this treatment suggests to me, that his state is not only independant of diffeomorphisms but also under any map that maps spacetime points bijectively (pointwise, not necessarily even continious) and thus produces a theory that only sees space-time as a set of points and forget completely even about topology.

As I understand, the reason to take such drastic steps are taken is the misconception that one should start from state that is invariant under diffeomorphisms. However, there is no physical motivation for this: It is enough to start with a state that is covariant, i.e. a state in which the action of diffeomorphisms is defined as operators in the corresponding hilbert space. The usual Fock quantization provides exactly this and due to the Stone von Neumann theorem is the only one that does this continiously.

At least in the classical theory, GR is diffeomorphism invariant but any solution (i.e. metric) sponaneously breaks this invariance to the isometry group of that space-time (which is generically trivial). So all the classical states are not invariant but only covariant.

38. **Alejandro Rivero**
June 15, 2005

I can not see the problem with the harmonic oscillator. Well, first of all, I missed the preprint number where such problem is discussed. I didn't know that LQG was already able to support and quantise another force fields jointly with gravity. But in anycase, given that an harmonic oscillator force is not included as one of the forces in the Standard Model, I am happy if it can not be supported under LQG. I would be even happier if the unique supported forces where SU(3)xSU(2)xU(1).

39. **Aaron Bergman**
June 15, 2005

The point about LQG is not that it's wrong — after all, experiment is the ultimate arbiter of that — but that is not a conservative approach to quantum gravity as is often claimed. It is, in fact, a radically different approach to quantization that, when applied to current theories, gives experimentally incorrect answers.

40. **M**
June 15, 2005

Thomas Larsson gave a detailed response to Lee's post in the previous thread. Since it is more likely to elicit a response in this thread than the earlier one, I think it is worth repeating it in full. I will add my own comment at the end, in agreement with one of Thomas' comments.

* _ * _ * _ * _ * _ * _ * _ * _ *

Dear Lee,

Let me emphasize that I am not a string theorist – on the contrary, over the last years I have had strong disagreements with Lubos and others, especially over the role of diff anomalies. While initially a statistical physicist, the success of CFT made me interested in Lie algebras, where I discovered how to generalize the Virasoro algebra beyond 1D and developed its representation theory, together with mathematicians like Moody, Rao, Berman and Billig. In particular, the Virasoro algebra in 4D is the anomalous form of the algebra of 4-diffeomorphisms, which is the constraint algebra of GR in covariant formulations (in non-covariant canonical quantization the constraint algebra is modified).

So when I speak about diff anomalies, I do it as someone who has developed new mathematics which has not been absorbed by the physics community. You may wish to keep that in mind.

As to the issue of anomalies, i.e. the claim that we ignore the established knowledge that ?INFINITE-DIMENSIONAL CONSTRAINT ALGEBRAS generically acquire anomalies on the quantum level…? is simply false. It is contradicted by rigorous existence and uniqueness theorems in LQG.

Whereas I claim that this is true, it is not at all generally accepted. On the contrary, it is widely asserted that there are no pure gravitational anomalies in 4D, see e.g. Weinberg's QT of F II, ch 22. Nevertheless, the constraint algebra of GR contains many subalgebras isomorphic to the infinite conformal symmetry in 2D, generated by vector fields of the form f(z) d/dz, where e.g. z = x^0 + ix^1 or z = x^2 + ix^3. Upon Fock quantization, these conformal subalgebras will in general acquire anomalies for the usual reason, making the whole shebang anomalous.

The reason why these anomalies cannot be seen in conventional field theory is that the relevant cocycles are functionals of the observer's trajectory in spacetime. Unless this trajectory is introduced and quantized in conjunction with the fields, the relevant anomalies cannot be formulated. This is IMO the crucial obstruction to the quantization of gravity.

1) The approach to quantization of constrained systems is different in string theory and LQG. The former approach depends on a gauge fixing that refers to a fixed background metric. It results in the construction of a Fock space. The latter is background independent and involves no background metric, no gauge fixing and results in a state space unitarily inequivalent to a Fock space.

2) There is a body of rigorous results that support each kinds of quantization. Hence it cannot be a question of which is correct mathematically. Both are correct, within *their contexts. It is a question only of which construction is appropriate for which theories and which describes nature.*

Conventional quantization has turned out to describe nature in other contexts. I think this is a good reason to believe that it is the correct approach. In particular, CFT has been successfully applied to 2D condensed matter, where conformal anomalies have been measured experimentally. This is of course a different context and not directly relevant, but this fact has shaped my basic instinct that anomalies are very real things which cannot depend on the quantization method used.

3) The treatment of constraints in string theory depends on certain technical features of 1+1 dimensional theories, particularly the fact that there is a gauge in which L_0 plays the role of a Hamiltonian and therefore should, in that gauge, be quantized so as to have a positive spectrum. The anomalies are not generic, as asserted above, rather they depend on the additional condition that L_0 should be a positive operator.

Yes, this is the crucial point. In any physical theory, there should be some positive operator which can be interpreted as a Hamiltonian; there is a physical requirement that energy be bounded from below. Of course, in GR there is a Hamiltonian constraint rather than a genuine Hamiltonian. This is another reason to introduce the observer's trajectory; you can define a genuine Hamiltonian as the operator that translates the fields relative to the observer.

Anyway, in all applications of Lie algebras to physics so far, the reps have been of lowest-weight type. At least for finite-dimensional Lie algebras, all unitary irreps are of this type.

There are other reps of Diff(S^1) that are non-anomalous but in which L_0 is not positive.

If you consider the restriction to the algebra of polynomial vector fields, generated by L_m with m >= -1, then all irreps have a vacuum vector (or are dual to such a rep).

So a choice is made in the standard quantization of string theory, which his motivated by the physics. This does not mean it is the right choice for all physical theories.

OK. I disagree.

4) Conversely the existence and uniqueness theorems which support the LQG quantization work only in 2+1 dimensions and above for the reason that gauge fields don?t have local degrees of freedom in 1+1 dimensions. The existence theorems tell us that there are quantizations in 2+1 and higher of diffeo invariant gauge theories that have unitary, anomaly free realizations of diffeo invariance. The uniqueness theorem tells us that the resulting state space we use in LQG is unique.

Contrary to string theorists, I claim that anomaly freedom is not a necessary requirement. To illustrate this point, let me again use the bosonic string as an example and quote from GSW, subsection 2.4: 'Classical free string theory can be consistently formulated for any spacetime dimension, but quantization with a ghost-free spectrum requires D less than or equal to 26. […] In the special case of D=26 and a=1 the spectrum is entirely tranverse, with many decoupled zero-norm states.'

Thus, D=26 is special, but D less than 26 is not ruled out by consistency requirements. It is only in 26D that it is possible to pass to the reduced Hilbert space by imposing the physical state condition L_m|phys)=0, but when D less than 26 this is not necessary, because the full, unreduced Hilbert space is already positive-definite.

Thus, my position is that some diff and gauge anomalies are good, making it possible to break diff and gauge symmetry on the quantum level, such as the string in D less than 26 illustrates. This does not mean that all gauge anomalies are good, of course. On the contrary, I recently gave a simple algebraic argument why conventional gauge anomalies, due to chiral fermions and proportional to the third Casimir, indeed are inconsistent. This argument does not apply to observer-dependent anomalies, which are proportional to the second Casimir.

The idea that diff and gauge anomalies may be consistent is of course very controversial.

With regard to the non-standard quantization, in which holonomies, but not local field operators are well defined, it is of course true that when applied to standard systems this leads to inequivalent results. ?This apparently leads to unphysical consequences, such as an unbounded spectrum for the harmonic oscillator.? But, give me a break, do you really think someone is proposing to replace the standard quantization of the harmonic oscillator with the alternative one? What is being proposed is that the quantization used in LQG is well suited to the quantization of diffeo invariant gauge theories.
In case it is not obvious, let me emphasize that harmonic oscillators are not relevent here, and can play no role in a background independent quantum theory, precisely because the division of a field into harmonic modes requires a fixed background metric. Thus, the physics of the problem REQUIRES an alternative quantization.

Sorry, but here I flatly disagree. I find it very disturbing that LQG methods yield the wrong result for the harmonic oscillator.

I am frankly puzzled why someone who claims to know the literature well would throw up examples like the harmonic oscillator up in this context. I can try to understand their point of view, but it certainly reads as if they either are choosing to ignore the basic point, which is that background independent quantizations cannot use fock space, or they are looking to make debating points to impress ignorant outsiders.

I agree that a diff invariant quantization of gravity cannot use Fock space, and I am convinced that such a quantization does not exist. However, a diff covariant Fock space quantization of gravity may very well exist. By this I mean a quantization in analogy with the string for D less than 26: the unreduced Hilbert space is consistent in itself, and diffeomorphisms are promoted to a genuine but anomalous symmetry acting on the full Hilbert space.

A step in this direction was taken in hep-th/0504020. Sure, there are problems: the (manifestly covariant) regularization has not quite been removed, no invariant inner product has been found, and no hard predictions have been extracted. But there is a Hamiltonian which is bounded from below in the regularized theories, the analogous construction for the harmonic oscillator has a spectrum bounded from below (it is not quite right, and I discuss why), and infinities cancel best (though not quite, so I am doing something wrong) in 4D. Most importantly, since phase space variables are promoted to operators in the usual way, this is genuine quantization, which is witnessed by the presence of anomalies.

Finally, I didn't express myself very well on the sociological issues. I agree with you about the problems with string theory, and I did not mean that funding to LQG should be stopped. However, given what I feel is a major problem (the harmonic oscillator spectrum), and that LQG already is the second biggest player in QG, I cannot really think that it is badly underfunded at present levels.

Posted by Thomas Larsson at June 14, 2005 10:44 PM

* _ * _ * _ * _ * _ * _ * _ * _ *

I am not at all qualified to comment on either Lee's or Thomas' statements. However, like Thomas, it bothers me a lot that LQG apparently cannot reproduce the known spectrum for the harmonic oscillator in some limit where it should. Historically it has been a requirement for a new theory to reduce to an established earlier theory in the limit of the earlier theory's applicability, as, say in the well known example that quantum theory gives the classical result in the limit hbar->0, or GR gives the Newtonian result in the flat space limit. Why should LQG be exempt from such a consistency check? Given that there aren't too many ways to test a theory of quantum gravity, it seems like a failure here should be taken very seriously. It seems like Lee is a little too dismissive when he says,

But, give me a break, do you really think someone is proposing to replace the standard quantization of the harmonic oscillator with the alternative one? What is being proposed is that the quantization used in LQG is well suited to the quantization of diffeo invariant gauge theories.

But then again, this is just the perspective of one who has only superficial knowledge about LQG…

41.
June 15, 2005

here is a footnote to Smolin response relating to something in the original thread:

http://loops05.aei.mpg.de/

>

this bears on Smolin's paragraph 5:

>

42.
June 15, 2005

this is quite possibly a trivial sidecomment but I want to mention a consideration from the standpoint of a "physics watcher" which is that I also benefit from the range of quantumgravity research options that gradstudents and postdocs have at AEI-Potsdam and Perimeter/Waterloo.

when a US grad student who has only the choice string or nothing chooses a research topic this does not tell me anything, I am watching a stock market where investors have only one choice or very limited choice. so I do not benefit from watching his or her behavior.

but by contrast I watch the career of, for instance, Bianca Dittrich at the Albert Einstein Institute with great interest
BECAUSE HERE IS AN INTELLIGENT HIGHLY MOTIVATED PERSON ON THE GROUND who is free to make choices between, say, Thiemann's Master Constraint canonical approach and Loll's Triangulation approach (which are on radically different mathematical ground).

I think a lot can be learned by watching intelligent gamblers, or intelligent investors investing precious capital (young research time).

So to me it is very exciting to watch Dittrich's career because the system she is in gives her choice of programs—-Hermann Nicolai directs AEI "Unified Theories and Quantum Gravity" division in a way that is VERY DIFFERENT FROM VAFA'S HARVARD. Wow, is it different! Nicolai branch of AEI has string but also LQC with bojowald, and canonical-LQG with thiemann and also dynamical triangulations work and a lot of other stuff. If you are there, or at 't Hooft"s Utrecht institute, or at Perimeter in Canada then you really have some interesting options. So to me it is more informative to watch what the postdocs do in those places.

And this is not just pure frivolity on my part, I really think you get scientifically valuable information by giving graduate students exposure to various things and a choice of radically different rival approaches, because the scientific enterprise benefits from their special perspective of what attracts them to devote their careers to. We all benefit from their intuitive hunches of what will pay off.

Let us hope that this does not make Artem Starodubtsev and Etera Livine and all of them self conscious knowing that bystanders get a clue of what is happening by seeing what they do.

Quantum gravity is at an intensely exciting stage and a LOT is happening. Onlookers can be glad for the instututes that are run with several alternate programs, and also (in my experience as an observer) they tend to be where the progress is being made.

43. **D R Lunsford**
June 15, 2005

Yawn.

-drl

Suspicious Dualities

June 15, 2005
Categories: Uncategorized

These days some of the strongest criticisms of what is going on in string theory are coming from Lubos Motl's weblog. His latest post asks what would have happened if currently fashionable ideas about string theory had appeared in the sixties before the standard model. They would have led to claims that many things about particle physics were inherently unpredictable, or dependent on the details of the earliest moments of the big bang. But when the standard model appeared in 1973, it made a wide range of detailed predictions of this type.

He also makes some remarkable statements about string dualities:

"Virtually all conjectured non-supersymmetric dualities (except a few exceptions in the topological context) are suspicious, and even those that are true may be true only because we define one of the sides to be dual to the other – while other equally consistent definitions may exist, too."

and claims that in the mid-90s Tom Banks described how research on string dualities was being done as follows:

"If you can't show that a conjectured duality is wrong in 5 minutes, it must be correct."

Funny, I'd always suspected that was what they were doing, but I'd never have dared to suggest it. And by the way, I'm wondering if Jacques Distler has given up on string theory. It's been several months since he's written anything about strings, and a month since he's written anything at all.

Comments

1. **Torbjorn Larsson**
   June 17, 2005

   Sure. But Lubos says "Although string theorists are obviously capable to study any of these things, …"
   which I criticize; as hubris if you like.

2.
   June 17, 2005

   I kow that Lubos did not have this in mind, is is awfukky lowbrow

   Harsh, but just.
   🙂

3. **Torbjorn Larsson**
   June 16, 2005

   I don't believe I did that! awfu*ll*y of course, my fingers slipped.

4. **Torbjorn Larsson**
   June 16, 2005

   "We need to define what string theory is *not*…"

   I kow that Lubos did not have this in mind, is is awfukky lowbrow :-), but there is a constraint of sorts right here, in his model of theories:

   "… construct a bound state of these electrons called the feminist …"

   There is no obvious one-to-one correspondence between different brain constructions and states (ie male and female) to feminist actions.

   So we should really know to stop here. (Apart from that nerve interactions are through ions, transmitter substances, reinforced/inhibited couplings and growth/death, so electrons should not describe them.)

5. **Peter Woit**
   June 16, 2005

   I've been moving to a new apartment today, so have been away from e-mail and just recently came into the office. I've deleted some of the more egregiously off-topic posts just now, so if you're wondering what that discussion is about, to some extent it's about posts that are no longer there.

   M has it right about why I don't just let anyone post anything they want to. To put it in more personal terms: I want this weblog and its comment section to be something that I'd actually like to read. In the past when I've let people post whatever they want, the comment section has degenerated into something I found not worth reading. When this happens, I assume that many other sensible people will decide the comment section here is worthless, and stop reading it or writing in to it. I'm not going to let that happen.

6. **Kostya**
   June 16, 2005

   Dear Peter,
   Why do you let crackpots to post on your blog?
   I guess it's amusing to read their comments but it gets kind of annoying at the end of the day.
   Lubos, please don't waste you time answering to their stupid comments.

7. **M**
   June 16, 2005

   From Quantoken:

Not only Peter will delete any thing about alternative ideas, he will also delete arguments against those alternative ideas he deletes just as well. He is against string theory. But basically he is only interested in arguments either for or against string theory, and for or against LQG, and nothing else.

I have not noticed Peter selectively deleting comments just because they weren't about string theory or LQG. I have seen him repeatedly request that people keep their comments related to the topic at hand. People who post comments that push their own particular flavor of a "theory" that supposedly explains all kinds of wonderful things that standard theory does not are usually not adhering to Peter's guidelines, and thankfully he is not overly shy about removing such comments.

If you want to see how free-for-all physics discussion forums operate, visit any of the unmoderated Usenet physics groups, like sci.physics.relativity for example. A relatively small number of people actually try to hold intelligent discussions, but the great majority of the traffic consists of discussions of nonsensical alternative "theories," insults, and off-topic items. In an unmoderated public forum, it just doesn't seem to happen that intelligent discussion of carefully thought out alternative ideas will take place over any length of time. Turkeys flock together, and the turkeys will be attracted to a forum where "alternative theories" can be discussed like fruit flies are attracted to a piece of overly ripe fruit (especially if they think serious scientists might read their stuff).

I really hope Peter continues to hold standards for keeping comments on-topic. It is really nice to have a few forums where people can discuss serious physics without the clutter of unsubstantiated (or poorly supported) claims posing as an alternative theory. It think Peter already errs on the side of keeping comments that are at best marginal, although some people whose comments were deleted would probably disagree. Perhaps he will occasionally have a topic where discussions of far out ideas will be on-topic, and then there would be no need to suppress even the nonsense.

Finally, I think that anyone who carefully thinks through an alternative idea and plays by the rules that all serious physicists play by — and that means making quantitative predictions, having predictions that agree with known empirical results, and not making unsubstantiated wild claims — can expect a respectful hearing by at least some people. Peter has said more than once that occasional references to personal web pages that discuss alternative ideas are fine; he just doesn't want to have people filling the comment section with that kind of stuff. For those who want to stimulate a more vigorous discussion of their ideas, take it to the unmoderated physics groups on Usenet and it will probably generate some traffic there.

8. **Lubo? Motl**
June 16, 2005

Quantoken, your comments are irrelevant because you know nothing about the subtle questions that are discussed.

It's just trivially true that the 1/2 BPS sector of type IIB string theory on AdS5 x S5 is equivalent to Quantum Hall System.

http://arxiv.org/abs/hep-th/0505129

A natural question is whether this can be extended to similar systems used in condensed matter, and it is a technical question that can't be ultimately answered by your passionate guesswork.

9. **Lubo? Motl**
June 16, 2005

"Fascist and antif*ist" – have not you wondered that this is an oxymoron?

10. **Quantoken**
June 16, 2005

Lubos meantioned that he had done some research relating "Fractional Quantum Hall Effect and string theory"

It is absolutely ridiculous for a string theoretist to even waste one second thinking about the connection between the two subjects. There are as unrelated as trying to use Euler's law to try to forecast the weather. They belong to completely different domains. The FQHE is problem of manybody of quantum theory in condensed matter physics, and problem that has been well studied and well understood, and does not need any external explanations. There is no physical fractional charge whatsoever.

Quantoken

11.
June 16, 2005

Fascist and antifeminist

"The crudest approximations will be enough for this calculation because the physical object under investigation is pretty simple"

12. **Lubos Motl**
June 16, 2005

Well, I can't deny that our two blogs have a significant overlap in the readership, but one may still argue that my blog is being read by many serious people, and even many of those who read both of these blogs have very different reasons why they read one as opposed to the other.

My comment comparing the situation of theoretical particle/string physics to biodiversity climate science was not predominantly about the anthropic principle although I mentioned some anthropic examples. Let me say more examples what I had in mind – something related to my recent research that seems increasingly less attractive:

Fractional Quantum Hall Effect and string theory.

Non-singlet states in various matrix models.

Calogero models and string theory.

And so forth. One may propose many dualities between string theory and these conventional theories and constructions and their generalizations. But do we really believe that these connections are deep? Is there a reason why string theory should be helpful or illuminate either of these things? It has illuminated Calabi-Yau manifolds, for example, but it does not mean that the success can be extended anywhere.

As far as I know, string theory has not given new insights about the FQHE and my bet is that it won't give any in the near future.

I personally doubt that string theory is deeply connected with all these (and other) things, and even the speculative idea that it *is* connected is not attractive to me in any deep way. String theory should be a theory of everything, but not everything in this sense. I would find it more rational and satisfactory if string theory showed that some of these otherwise nice things – like the Calogero model – can't have any relation with the most fundamental laws. We need to define what string theory is *not*, and undoubtedly some of the things that string theory is not have been proposed as examples what string theory *is*.

Of course, any idea can be embedded in string theory, including feminist social theory. Take a Calabi-Yau space, heterotic strings, compactify, discover electrons, construct a bound state of these electrons called the feminist, let it evolve, and decode the brain hologram. The crudest approximations will be enough for this calculation because the physical object under investigation is pretty simple, and what you will get is feminist social theory.

But in this example, the theory is not connected to the characteristic stringy phenomena. Although string theorists are obviously capable to study any of these things, I doubt whether we should pretend that it is a part of string theory.

13.
June 16, 2005

Lubos' posts in this blog are legendary ones.

Many of us know to him and his fascist style.

14.
June 16, 2005

"what your low-brow blog and its low-brow readers enjoy"

"- (Lubos). You and you readers are stupid."

Hmm, it appears to be true then that, no matter how
smart you are, how righteous your cause is or how
strong your arguements are, in the end it'll come
down to namecalling!

15. **Arun**
June 16, 2005

I postulate a (Peter blog) (Lubos blog) duality and can confirm it in 5 minutes 🙂

16. **Juan R.**
June 16, 2005

– (Lubos). You and you readers are stupid.

– (Peter). Thanks for the clarifications.

Great!

17.
June 16, 2005

From the perspective of time, all discoveries seem trivial. The "duality revolution" was certainly a big step. However, one can ask what is the true physical origin of various dualities and why they work so beautifully for superstrings while not much can be done without supersymmetry. In my opinion, dualities have no dynamical content. All what they represent are very strong constraints imposed by supersymmetry, and they hold only in BPS (and almost-BPS) sectors. Same thing about AdS/CFT: when two structures have the same symmetry, you will certainly find some matching observables.
In non-supersymmetric theories, BPS-like configurations — instantons play some minor role
but the important dynamical effects like confinement have a different origin.
If you agree, then here is no point in talking about non-supersymmetric dualities. We are stuck with very difficult dynamical problems, like in QCD. I am curious what others think about it…
Jean-Paul

18. **Mike Ros**
June 15, 2005

I have the impression that a sizeable fraction of Peter's readers are also Lubos' readers

I anticipate that it works both ways; I always read both.

19. **Torbjorn Larsson**
June 15, 2005

It is worse; Lubos included himself. May I suggest Vicodins? 😉

20. **Just another low-brow reader**
June 15, 2005

From Lubos:

… or about simple-minded destruction of string theory which is what your low-brow blog and its low-brow readers enjoy.

As one of Peter's blog readers, this would make me low-brow. But I also read Lubos' blog, so I guess that makes me one of his low-brow readers too. In fact, I have the impression that a sizeable fraction of Peter's readers are also Lubos' readers, so does this mean that Lubos' blog is therefore infested by low-brow readers? Such a deep, difficult question almost makes my head hurt.

Lubos can be so *endearing* sometimes…

21. **Peter**
June 15, 2005

Hi Lubos,

Thanks for the clarifications. I thought from the context of my post and yours it was clear that your gedanken experiment referred to the whole anthropic principle line of thinking.

I don't want to discourage people from thinking about anything, but I do want to discourage them from thinking about one particular thing (string/M-theory based unification in 10/11 dimensions) since, despite twenty years of effort, it has failed completely and has now led a sizable part of the community off into pseudo-science. It would be good for physics if people thought more about other things than this, just about any other things…

22. **Lubo? Motl**
June 15, 2005

Dear Peter,

what you have not understood is that the rule "if you can't rule it out in 5 minutes, it's true" has really worked in all the important cases during the duality revolution, and in some cases it also worked afterwards.

Non-supersymmetric dualities have never been a part of the basic established structure of string theory. For example, type 0A is, in some sense, dual to M-theory on a Scherk-Schwarz circle. Except that such a statement is physically unverifiable because in order to connect the two limits, one has to go through spacetime that is completely unstable.

A more modern example is the description of non-SUSY configurations obtained by adding things such as anti D3-branes to supersymmetric AdS spaces; it's questionable whether these configurations may be seen in the dual boundary CFT.

My gedanken experiment about the 1970s was not about putting the whole string theory as we know it to the test – or about simple-minded destruction of string theory which is what your low-brow blog and its low-brow readers enjoy. It was about checking ideas how to go beyond the current state of affairs. Unfortunately, you're extraordinarily capable to distort statements and facts in such a way that you want to discourage as many people as possible from thinking about anything.

All the best
Lubos

Bogdanovs Gain a New Supporter

June 18, 2005
Categories: Uncategorized

If you didn't follow this a couple years ago, you can read John Baez's detailed description of the Bogdanoff Affair. For more about my dealings with them, see here., here, and here. If you want to read their stuff, go to the website of the Mathematical Center of Riemannian Cosmology which purports to be in Latvia.

In brief, the Bogdanovs are two brothers in France with a TV show who got Ph. D.s based on work which on the whole was complete nonsense. I'm not surprised that they managed to get Ph. D.s, and one of them was failed on his first attempt. It's not unusual in academia to be faced with having to decide what to do with students who seem to be enthusiastic and work hard, but don't perform at an acceptable level. There are lots of reasons to just pass them with the lowest possible grade (for one thing, this gets rid of them). One of my colleagues refers to this as the "infinitely elastic C-minus".

What was disturbing about the Bogdanov story was that they managed to get papers published in six refereed journals, some of which were quite respectable. Some of these papers were essentially identical. After this story became public, the editorial board of one of the journals (Classical and Quantum Gravity) issued a statement saying that the paper they published shouldn't have been accepted, and that they were taking (undisclosed) steps to change their refereeing process so this wouldn't happen again. This seemed to me strong evidence that there is so much nonsense now in the theoretical physics literature that the refereeing system has broken down. Many referees are now either unwilling or unable to identify nonsense when they see it.

I had discussions about this with quite a few physicists at the time, and many of them took the position that this wasn't such a big deal. Their attitude was roughly that "So what if these guys managed to get something nonsensical past some lazy referees? Everyone in the community can tell that what they wrote is nonsense and just ignores it. It's not true that we can no longer tell nonsense from serious work". I became somewhat convinced that I was being too harsh on string theorists and others when I thought that they had completely lost the ability to identify nonsense. Maybe the only scandal here was the laziness of referees, not the infection of the whole subject by nonsense to the point where lots of people can't tell the difference. Well, I just changed my mind, clearly at least some string theorists can't.

I feel somewhat constrained in what I can say about the details of this, due to the fact that someone I've been in contact with tells me of being threatened with a lawsuit by the Bogdanovs for having criticized them publicly. I'm also not about to engage in discussion of the details of their nonsense, which is what they, like all crackpots, really want. It's just a complete waste of time.

Comments

1. **Peter Woit**
   June 20, 2005

   Funny, that last comment came from an internet address that looks a lot like the ones of Roland Schwartz and Prof. L. Yang…..

   The idea that topological gravity has something to do with the full quantum gravity is a reasonable conjecture and isn't due to the Bogdanovs. I'm sure people will keep working on it.

2. **Me**
   June 20, 2005

   From my point of view, one must leave to time and history the care to decide wether this fields of research was effectively killed by Bogdanovs.

   I will rather say, on the contrary, that it was born from Bogdanovs.

3. June 20, 2005

   In his criticism of LQG Lubos states, "It assumes the metric tensor is a good variable at all distances and is the only relevent variable…it even assumes Einstein equations are more or less exact at the Planckian regime…these assumptions are challenged in a general enough theory of QG, for example all models that emerge from string theory…assumptions that have no theoretical or experimental justification".

   I agreed with these statements actually, but how come he now thinks it is ok perhaps if the above variable "the metric" has signature fluctuations in the Planckian regime, a la Bogdanov, and is a viable variable in this regime?

   Even if you think TQFT has something deep to say about the initial singularity of spacetime(and who knows)the Bogdanovs have effectively killed this as a respectable research direction.

4. June 20, 2005

   BTW the first place where Lubos signed "lumo (leashed)" was his blog entry commenting on
   http://arxiv.org/abs/hep-th/0504221
   Dijkgraaf, Gopakumar, Ooguri, and Vafa
   Baby Universes in String Theory

   that was on April 30 and in the preceding segment of that day's blog he had said "Unfortunately, I currently do not enjoy the freedom to tell you what I think about these things."

   I tend to view Lubos protestations of tolerance for the Boganoff article as an ironical reproach to somebody who told him to ease up and stop the vitreolic attacks on other stuff. It puts him in position to say, well you told me not to be so ferocious so you must favor relaxing the intellectual standards applied to scholarship, so look what happens! I will just (to spite you) go and approve of the Bogdanoffs on the same basis that I tolerate specious research by [unnamed].

   Yeah that is a tortured overinterpretation, and who cares about the adventures of Lubos melodrama, except he is entertaining sometimes. Anyway I dont take his apparent acceptance of the Bog paper at face value, it is a travesty just like the Bogs (or so I tend to suspect)

5. June 20, 2005

It’s interesting that Lubos is pushing “not complete nonsense” as the new standard for scientific publication, at least for certain types of physics. Of course for, say climate science, he expects a level of rigor exceeding pure mathematics literature.

I can’t help but notice a parallel with certain defenders of Bush administration foreign policy who have adopted “not as bad as Saddam” as the new standard for acceptable moral behavior.

6. June 20, 2005

on the other hand, Dijkgraaf is an eminently sensible choice).

a propos Dijkgraaf, he is one of the invited speakers on the programme of Loops 05 conference in October
loops05.aei.mpg.de/index_files/Programme.html

since Dijkgraaf’s public lecture in July is about black holes and time, I was reminded of his recent paper

http://arxiv.org/abs/hep-th/0504221
Baby Universes in String Theory

Abstract: “We argue that the holographic description of four-dimensional BPS black holes naturally includes multi-center solutions … This provides a concrete realization, within string theory, of effects that can be interpreted as the creation of baby universes…”

in that context I cannot imagine what he could mean by his jazzy phrase “the end of time” except that continuation through a black hole implies a branching of time which could be said to dispose of the classical single-track unitary-evolution notion and thus be “the end” of time as we have thought of it up til now. I certainly agree Dijkgraaf is a great choice (maybe “eminently sensible” could be supplemented to read “inspired” 🙂

7. **Peter Woit**
June 20, 2005

I’d noticed that too. It speaks volumes that the organizers of this conference feel that Susskind’s pseudo-scientific nonsense is worthy of being half of the public face of string theory (on the other hand, Dijkgraaf is an eminently sensible choice).

I wouldn’t read too much into the lack of a definite program for the conference. Scheduling such a program is always difficult, with all sorts of last minute changes.

8. June 20, 2005

this may or may not bear on the general topic of attention-seeking and marginal science
please erase if insufficiently topical

The schedule posted for Strings 05 is still (as of Monday 20 June) almost blank, but it now lists a Leonard Susskind public lecture and two other events.

http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/program.html

Public Talks
Saturday July 16, 2-5p.m.
Robbert Dijkgraaf U. Amsterdam
“Strings, Black Holes, and the End of Space and Time”

Leonard Susskind Stanford U.
“Cosmic Landscape: String Theory and the Illusion of Intelligent Design”

Panel discussion: The Next Superstring Revolution
Tuesday, July 12, 19:00-21:00

the conference goes July 11-16, so is only 3 weeks away. It seems curiously suspenseful on the part of the Toronto organizers that the posted schedule of talks is mostly blank (no regular talks shown, only the two public lectures.) Maybe we can take it as providential sign that Susskind’s title mentions the illusion of intelligence in connection with string theory (kidding)

9. **Mike**
June 20, 2005

I apologize if this is somewhat off topic (please delete if it does not belong), but every day I learn something new here. Matti Pitkanen wrote: “In some other summary brothers identify Big Bang singularity with a point rather than singularity analogous to the boundary of future light cone: this mistake of course appears again and again in popular literature.”

Exactly! Until recently everything I have learned has come from popular literature and that is how we are told to “visualize” the Big Bang. For someone who is not a mathematician or physicist, where do we turn for a more accurate understanding of these issues? Something more advanced than popular books (Kaku) but not as complex as the text books by Polchinski, Zwiebach, Zee, etc.., which are presented as introductions but are too complicated for someone without an advanced understanding of math.

I’m trying to work my way through Penrose’s new book and almost threw in the towel on the chapters on Manifolds and Symmetry Groups. It’s very frustrating. In the introduction he nonchalantly advises readers to feel free to skip the math. 1) I don’t want to skip the math and 2) It’s ALL math.

10. **Fyodor**
June 20, 2005

Between that demonstration of ignorance and Lubos’s new one, I think it is at least safe to say that on the whole string theorists don’t even know what a TQFT is.

Yeah, PW, people who don’t really understand things ought to abstain from commenting on them, don’t you agree? I also find that there are way too many people who engage in heated denunciations of certain theories and yet have absolutely nothing to suggest as alternatives. That, too, is rather feeble, is it not?

11. June 20, 2005

“1.These brothers are surely intelligent. They got an IQ of 200.”

That only comes out to 100 each, assuming it’s evenly split. Nothing to brag about.

12. **Fabien Besnard**
June 20, 2005

Peter,

>What to do about this is a hard problem.

to this I answer : a national anonymous competition. In France this is the way the “grandes ?coles” recruit their students and it put them way ahead universities. The problem is that now it is criticized also (for one thing because this system is very different from what is done everywhere else in the world).
I am not saying that this system should be considered as perfect : it selects people with very peculiar qualities. Nevertheless I do believe it is the less unfair, and I think it should be used along with more traditional academic systems in a balanced way.

Regards.

13. **Matti Pitkanen**
June 20, 2005

I would be happy if the discussion about Bogdanov affair could be based on real arguments rather than uneducated crackpot guesses and unjustified non-sense claims so familiar also from earlier discussions.

Thanks for Lubos for seeing the trouble for performing a real analysis of one of papers. I looked the reports about the thesis of G. Bogdanov and found that Majid gave a positive statement saying that the work was original. Brothers emphasize that they are mathematicians, not physicists. By looking the abstracts of their papers, one gets the impression that also their understanding of Riemannian geometry could be better.

For instance, look just the abstract of THERMAL EQUILIBRIUM AND KMS CONDITION AT THE PLANCK SCALE

Considering the expected thermal equilibrium characterizing the physics at the Planck scale, it is here stated, for the first time, that, as a system, the space-time at the Planck scale must be considered as subject to the Kubo-Martin-Schwinger (KMS) condition. Consequently, in the interior of the KMS strip, i.e. from the scale ℬ = 0 to the scale ℬ = ℓplanck, the fourth coordinate g44 must be considered as complex, the two real poles being ℬ = 0 and ℬ = ℓplanck. This means that within the limits of the KMS strip, the Lorentzian and the Euclidean metric are in a “quantum superposition state” (or coupled), this entailing a “unification” (or coupling) between the topological (Euclidean) and the physical (Lorentzian) states of space-time.

Brothers seem to have a rather vague view about Riemannian geometry (“the fourth coordinate g44 must be considered as complex”). Could a real mathematician really write something like this without feeling a deep pain in his guts.

In some other summary brothers identify Big Bang singularity with a point rather than singularity analogous to the boundary of future light cone: this mistake of course appears again and again in popular literature. Taking into account what brothers propose, this mistake looks strange since singularity is very much like the boundary of the future lightcone and by its metric 2-dimensionality is indeed an excellent candidate for serving as a seat of conformal (and thus also topological) field theory.

I see nothing crackpottish in idea about possibility of Euclidian and Lorentzian signatures. If space-time is identified as a 4-surface (or brane) this is an unavoidable prediction with deep implications. The idea that pre-Planck era corresponds to TQFT is also interesting. Personally I would rather view it as a phase during which the analogs of long cosmic strings dominate and string models become a good approximation.

Matti Pitkanen

14. **Peter Woit**
June 19, 2005

Hi Deane,

I agree that through grade inflation and passing students who don’t perform acceptably the value of undergraduate degrees has been seriously debased. To a lesser degree the same has been true of graduate degrees. What to do about this is a hard problem.

In the Bogdanov case I still think the root of the problem is the failure of the refereeing system. If you look at their theses, Grichka’s is not egregiously non-sensical, and his committee passed him with the lowest grade, which was not unreasonable. Igor’s was nonsense, and his committee did the right thing the first time and failed him. My understanding was that he was told that if he could get his work published in refereed journals, he could come back for a second try. If the referees had done their job correctly, there would have been no problem.

Another unusual part of this story is that the person who agreed to supervise their work was Moshe Flato, and he died unexpectedly before their theses were completed. It’s quite possible that had he lived he would not have allowed Igor to submit such a thesis. Moshe was a wonderful, generous and extravagant man, one whom I was quite fond of, having enjoyed meeting and talking to him on half a dozen occasions or so over the years. I suspect his generosity was what led him to take on the Bogdanovs, but had he lived his devotion to good science probably would have kept something like Igor’s thesis from getting approved.

15. **Deane**
June 19, 2005

Quote:
It’s not unusual in academia to be faced with having to decide what to do with students who seem to be enthusiastic and work hard, but don’t perform at an acceptable level. There are lots of reasons to just pass them with the lowest possible grade (for one thing, this gets rid of them). One of my colleagues refers to this as the “infinitely elastic C-minus”.

Peter,

It’s about time that we professors recgnize the damage we do to ourselves and our community by doing this. It’s certainly an expedient action, but it does tremendous damage to our credibility.

It is an open secret in many places (Wall Street, for example) that a degree in mathematics— undergraduate, masters, or doctorate—even from a good school, tells you absolutely nothing about a person’s ability to use mathematics. The Bogdanov affair is only a particularly public example of something happens quite often. Is this really how we want it to be?

16. **JC**
June 19, 2005

Some folks completely refuse to believe they're wrong, regardless of how many times they're hit over the head with a stick?

17.
June 19, 2005

quantoken, do you ever tire of being wrong?

18. **Quantoken**
June 19, 2005

Peter:

It was easy to call the other side logically incoherent or complete idiot or things like that when they have published something that you can not make sense of. But consider that:

1.These brothers are surely intelligent. They got an IQ of 200. Admittedly they have practiced and trained for it (well, you can say that for virtually all student exams, as well). But Peter or any one try to get an IQ test score of 200. I bet you can't. I can't. No one I know can.

2.They clearly have spent a lot of time, energy working on the stuff. As Lubos said, all the formulas and equations they cited are correct. And they make correct references to names of people and all that.

If they have better intelligence than you, and have spent **10 years** to come up with the ideas. And you have spent **10 minutes** reading their stuff and could not understand it. The more plausible case is they may really have some good ideas that you are simply not capable to appreciate, versus the other possibility that they are complete idiot with 0 IQ, and that their brain can not think coherently.

Althought scientifically, all those are still nonsense regardless how the logic works. If you can not explain anything in nature, your theory is still worthless as far as science is concerned. This has nothing to do with intelligence.

Quantoken

19. **Peter Woit**
June 19, 2005

Quantoken,

There is a difference between the Bogdanov papers and most of the string/M-theory literature. A more or less typical paper on hep-th these days starts from a set of assumptions about string/M-theory, then goes on to make various arguments and do various calculations based on the assumptions. Most of the time these arguments and calculations make sense, if you accept the assumptions. The problem is that the assumptions are incoherent and don't really make sense.

As an example, consider the Douglas et. al. statistical counting arguments and calculations. You can definitely follow the logic of their argument and calculations, even though the starting point, a mish-mash of ideas about string theory, branes and the anthropic principle, is completely incoherent and there is no rational reason to believe that any prediction about physics can ever be derived from it.

The Bogdanov papers are different. There are huge leaps of logic from one sentence to the next, clear examples of misunderstandings of basic ideas by the authors, etc. The problem isn't their assumptions, but that they can't construct a coherent argument and don't understand the technical tools they are using. This makes their papers look a lot like Sokal's hoax paper, except that their papers aren't (intentionally) funny.

I don't think I'd have any problem winning a court case against the Bogdanovs (for one thing, I think I have strong cause for legal action against them since they published an intentionally mistranslated version of my words). But getting involved in something like that would be a huge waste of time. And now the fact that a Harvard faculty member is going around claiming that their papers make sense and are of value would make the whole thing much more difficult.

Similarly, I'm not going to waste time pointing out all the problems with the Bogdanov's manuscripts. This has been done elsewhere, by John Baez and others. When people like the Bogdanovs publish papers containing wrong arguments and leaps of logic, what they most want is for someone more knowledgeable than them to take their ideas seriously and devote time to figuring out exactly what is wrong with their arguments and thinking hard about whether or not their logical gaps can be filled in. They're lazy, convinced they are geniuses because of their vague conjectural ideas, and hoping that someone else will do the hard work necessary to turn their incoherent conjectures into something that makes sense. In this case I don't believe it is possible to do this, and any time spent on showing this would be completely a waste.

20. **Fabien Besnard**
June 19, 2005

Hello.
I think a clarification is needed here. The brothers have been harshly criticized for a very precise reason : in their last book they misquoted some people (including Peter Woit) as if they supported them, or at least as if they thought the brothers' work were meaningful. I first pointed this (here), then a french journalist talked about it and the brothers suited him, I just can't imagine for what motive. I don't know if the procedure is still on its way.

I agree with Peter that talking about this affair now only serve the brothers' commercial interests, so I won't make any further comment.
Best regards.

21. **steve**
June 19, 2005

As far as I know, complaints from readers about the paper started coming in to the Editorial board of J. Classical and Quantum Gravity before the whole affair blew up, which is unusual. If the brothers are going to start sueing people now for criticising them then any sympathy that I (and other people) might have had for them is gone because this is a good way to help kill science and open scientific debate. However, I don't see how they could realistically expect to sue someone in this way. I imagine though (or hope) that it is the 'crackpot' or 'fraud' label they don't like and not impersonal criticisms of the work itself. This could probably be considered defamation of character and grounds for a lawsuit.

Even if there are seeds of really good ideas in their papers (which there might be) there is such a stigma attached to this whole affair now that no-one is going to continue with or explore the questions they address using the methods they suggest, even if that person(s) could develop or improve the whole approach. Do you think any career-minded person(s) is ever going to write a paper with both "topological field theory" and "initial singularity" or "KMS states" in the title and have the brothers' papers in their reference list? Anyone doing such a thing is going to be instantly labelled crackpot. Also, I doubt any journal (esp CQG) or even the arxiv would accept it. Not that I am saying this is right but this is just the way things are.

22. **Quantoken**
June 19, 2005

Peter said:
“I feel somewhat constrained in what I can say about the details of this, due to the fact that someone I’ve been in contact with tells me of being **threatened with a lawsuit** by the Bogdanovs for having criticized them publicly. I’m also not about to engage in discussion of the details of their nonsense, which is what they, like all crackpots, really want. It’s just a complete waste of time.”

You repeated that several times. Are you really this weak-nerved that you really take such threat seriously? If they sue then let them sue. I do not see you have anything to lose just for being actually sued.

Frankly I do not see any ground for a legitimate law suit just because you discuss and criticize the content of their paper. On the contrary, the fact that you repeatly call them crackpots, without discussing any of the facts that leads you to do so, could well be used as a ground for suing you for defamation.

So just for the purpose of avoiding being sued, you should start criticizing and analysing about their papers in more details, instead of just blanketly accusing them to be crackpots.

Quantoken

23. **Quantoken**
June 19, 2005

Peter:

I do not think Lubos become a supporter of the brother just because he made some comments that looks fair, reasonable and balanced. He made some good points that the brother’s papers raised some important questions. A paper could be well worth publishing even if just for asking the important right question along.

On the other side I do not understand why you have to **single out** these brothers for some harsh criticism, at an era that 99% of academic papers can already be accorded the same evaluation: nonsense and worthless.

The whole super string business is already nonsense since it does not explain a single thing we observe in nature. Does it make the brothers’ paper more nonsense than nonsense just because the presentation of their ideas does not seem very coherent to you? Or if they present the same idea, but in a more logically coherent and more readable ways, then you would not consider it nonsense in that case? **Why pick on unimportant details if the fundation of the whole thing of the whole field is already wrong and none-scientific, regardless of the mathematical details**?

The whole thing started when some one thought the brothers pulled a hoax, and it should end when the brothers point out that it was not a hoax. There is nothing especially wrong or especially bad in the paper of the brothers **comparing with the quality of the rest of academic papers**. The brothers even excelled in asking important questions and provided novel ideas, something that Lubos praised rightfully!!!

The important matter in the status of the affair, is the fact that nowadays in the field of theoretical physics research, it is no longer possible to distinguish between a serious paper, or a hoax. You could no longer tell if some one is dead serious or if he is joking around. That is the status of affair today.

Now, my own evaluation of the brother’s work: The whole idea is total nonsense to start with, regardless of the details. But that would also apply to many of other academic papers I see. So the brothers do not deserve any harsher criticism than the rest.

Quantoken

24.
June 19, 2005

I personally don’t find the paper terribly valuable, but I insist that its vagueness and strangeness is comparable to the vagueness and strangeness of other works about equally difficult and unknown subjects.

such as, for example?

25. **Lubo? Motl**
June 19, 2005

Well, I don’t need to comment on the description “a new supporter of Bogdanovs’ brother” because Peter Woit is obviously the only person here who thinks that this description is anything else than a stupid and childish exaggerated game.

The paper is not a complete nonsense. It is a mathematically detailed clarification of a vague but intriguing idea about a very difficult subject of the initial singularity of the Universe. It is a paper whose details have been improved for 7 months or so, to say the least, to satisfy the real physicists much more than at the beginning. And the local fragments of the paper now mostly reflect the correct definitions of the mathematical concepts and their basic relations.

If Peter Woit thinks that most sentences of the paper are wrong or that the paper has no idea, then it only shows that Peter Woit is uncapable to understand any paper published after 1900.

I personally don’t find the paper terribly valuable, but I insist that its vagueness and strangeness is comparable to the vagueness and strangeness of other works about equally difficult and unknown subjects.

26. **A Scott Crawford**
June 19, 2005

In all fairness, there is a problem evaluating any number of categories of doctorial level physics research, as advanced work commonly delivers raw data based on apparatus that lack a standardized metrology or operation.

The most obvious example of this is “quantum computing” work with ion traps. Because there’s not a high enough degree of consistancy (over 90%) between apparatus performance in different labs, there’s still not an established frequency standard for the time being. (I’m over simplifying). Thus only a casual review of claims or results out of a given lab are possible, strict confirmation and verification will only be possible a couple of years from now. Should doctorates be withheld in the meantime? Of course not.

Yet it's quite obvious that given the number of labs, and the wide spread of resulting research that's been published using standard equipment, that everyone cannot possibly be representing their work accurately (to put it gently). I think it's probably safe to assume that a large number of phd's given for quantum computing (ion trap) research are gifts, pure and simple. So what? By the time the dust settles (so to speak), the frauds will have moved on, the public won't know or care, and no fuss will be made by the physicists and engineers that know.

Just because it's clear there must be some frauds, there's no practical way to determine the genius from the cranks as of yet, so there's little one can do. And because one genius is worth ten cranks, the best course of action is to hold ones nose and wait. If its genius it'll persist.

27. **Juan R.**
June 19, 2005

Sincerely, I am not sure if it is a Lubos trick.

Often one revise literature that is really wrong but contain one or two ideas really interesting that one would investigate in deep.

How could i know if Lubos is talking seriously or simply doing a joke?

Instead of title of this blog

Bogdanovs Gain a New Supporter

i would prefer

Does Bogdanovs Gain a New PARTIAL Supporter?

28. **Juan R.**
June 19, 2005

"Lubos deleted one of my comments about this on his blog, so I won't bother writing there, but will comment here. It's true that I did accuse him of joining the crackpots, and he has every right to delete things he considers personal attacks, but given his general style of dealing with people he disagrees with, this is kind of funny."

Also is funny your erasing of my past post here!!

29.
June 19, 2005

As a post doc, at a leading university, in the 80's, I was witness to at least one PhD thesis on string theory that was refereed by well known people and approved by them, while those who actually knew something about how these theses produced realized that there was nothing new in them, and that at least one was plagiarized.

The problem at the time, was that the subject was developing very fast, there were very few experts with sufficient overview to referee every thesis, and almost all senior people simply had no clue.

I was also witness to how some leading string theorists with physics background, were totally taken by younger ones who used the jargon of algebraic geometry so heavily that no onen could tell if anything they said made sense, but one thought "It must make sense, because it's highly unlikely that someone can make all this up". Not true.

I realized already at that time that it's entirely possible that someone can master the jargon of a highly technical subject and manage to produce papers that get published in leading journals, while these papers have no content whatsoever.

I would be very happy to give detailed examples with names, but I don't want Peter Woit to be hit with legal action.

Another problem that I noticed is that, if any starts to make waves or to question the authorship and/or content of a paper by a PhD student and/or young researcher, people around him, and particularly his supervisor may not like that at all, and the person who questions things ends up being the bad guy. People just don't want this kind of headache. They want students to graduate and go away.

30.
June 18, 2005

Speaking of LQG, Lee Smolin posted a new response this morning in the topic, "Response From Smolin."

31. **quantumhobby**
June 18, 2005

Peter wrote:

"It's not unusual in academia to be faced with having to decide what to do with students who seem to be enthusiastic and work hard, but don't perform at an acceptable level. There are lots of reasons to just pass them with the lowest possible grade (for one thing, this gets rid of them). One of my colleagues refers to this as the "infinitely elastic C-minus".

One of the reasons why I like to read this blog is that Peter does not pull any punches. I had a few professors in college who gave me 'C's that I probably didn't deserve. I used to think they were just being generous — now I know the truth. 🙂

32. **Wolfgang**
June 18, 2005

> You have written simplicial gravity
> papers yourself
Indeed. I am one among the many who failed
to solve the puzzle (so far).
At this point I hope that somebody will find
the solution and do not care who will find it,
Lubos, Renate or anybody else.
But I would expect that some new experimental
evidence will be needed as guidance.

33.
June 18, 2005

Lubos attacked the CDT approach several month ago.
You can follow this link
http://yolanda3.dynalias.org/tsm/tsm02.html#20041218
for (one of) the discussions about it.

Wolfgang Beirl! You have written simplicial gravity papers yourself, if I remember. I visited your family website quite some time ago, an attractive picture of life in Bahamas.

Thanks for the link.

I wonder if Lubos will use similar arguments the next time he attacks CDT, or if he will try different.

34. **Wolfgang**
June 18, 2005

Just one more remark.
Quantization of 2+1 gravity is possible
without super-strings and Regge-Ponzano
is an example of lattice gravity in 3d.

So, whenever Lubos rides a general attack against
LQG, CDT or other non-string approaches, check
first whether his argument distinguishes between
3d and 4d (the ones I heard from him usually do
not).
If not the argument cannot be valid …

35. **Wolfgang**
June 18, 2005

Dear "",

Lubos attacked the CDT approach several month ago.
You can follow this link
http://yolanda3.dynalias.org/tsm/tsm02.html#20041218
for (one of) the discussions about it.

36.
June 18, 2005

Hi Aaron,
I remember your mentioning Ambjorn et al CDT as an interesting quantum gravity rival to string, in the other thread.
I guess in any research line there are some who are generous towards rival efforts (and permit themselves to be sincerely interested in them) and there are others who habitually attack or condescend. I value the former a lot.

my simplified model of Lubos is that whenever he talks about quantum gravity alternatives to string he will tend to attack whatever he sees as the most dangerous threat to string prestige. (clearly unfair to Lubos and an oversimplification, he would not be so amusing if he were predictable, but that's my model)

So I am expecting Lubos to attack CDT presently, because I see it as the most threatening from his standpoint. I would not be surprised if he were getting into position.

BTW one of the CDT leaders is a woman named Renate Loll, as you doubtless know since you've read one or more of her papers. Apparently she and Ambjorn invented the approach in 1998—their first CDT papers are from then.

37. **Aaron Bergman**
June 18, 2005

I think it is at least safe to say that on the whole string theorists don't even know what a TQFT is.

Nice of you to generalize from two examples.

38.
June 18, 2005

Lubos refers to Majid as "One of the co-fathers of quantum groups". With all due respect, that's plain silly, and shows how little he knows about the subject and how inclined he is to hyperbole.

39. **Peter Woit**
June 18, 2005

Lubos deleted one of my comments about this on his blog, so I won't bother writing there, but will comment here. It's true that I did accuse him of joining the crackpots, and he has every right to delete things he considers personal attacks, but given his general style of dealing with people he disagrees with, this is kind of funny.

One point I made in my comment is that Lubos's claim that "this is one of the punch lines that shows that they're either pretty smart or someone helped them: the observables are replaced by homology cycles on the moduli space of gravitational instantons" is ridiculous. That homology classes on a moduli space are the observables is nothing but the definition of this kind of TQFT. He's impressed that they can repeat the definition????

When this all happend in 2002, another string theorist circulated e-mail to his colleagues attacking John Baez and demonstrating an impressive lack of understanding of TQFT (he believed that a TQFT doesn't depend on the Lagrangian). Between that demonstration of ignorance and Lubos's new one, I think it is at least safe to say that on the whole string theorists don't even know what a TQFT is.

40.
June 18, 2005

Alejandro: *It is a trick, isn't it?*

same thought occurred to me, on first reading Lubos post.
setting up to go thru the motions of applying similar criteria to
some serious work that he wants to discredit
but I would not have voiced my misapprehension
(thinking it too mistrustful on my part) except
that you mentioned it. Yes, it could be a "set up".

41. **Alejandro**
June 18, 2005

It is a trick, isn't it?

Various Math and Physics Links

June 21, 2005
Categories: Uncategorized

Here's a collection of interesting things I've run across recently:

A website devoted to Hermann Weyl. The author is a religious sort, but of the good kind.

A movie taken at the 1927 Solvay conference. It is on the website for "The End of the Certain World", which is a biography of Max Born. I've read the book and some of it is interesting, but I have little sympathy for one of its themes, that Born felt he didn't receive enough recognition for his work (he got his Nobel Prize in 1954, long after many other Nobel prizes were given for quantum mechanics). Frankly I think any physicist like Born who had the incredible luck to be at Gottingen in 1925-26 should have spent the rest of his life thanking his lucky stars and not complaining about his career.

Harvard mathematical physicist Arthur Jaffe has a website. In particular the site has some interesting expository papers, including an autobiographical memoir about the IHES in the early 60s, a survey of constructive quantum field theory, and a work in progress, an introduction to quantum field theory from a rigorous point of view.

There's a recent lecture by Eric Zaslow on Physics and Mathematics which he gives the supremely ugly name of Physmatics.

Some of the lectures from String Phenomenology 2005 in Munich last week are on-line. Lots of talks about flux vacua and the landscape, nothing that seems to have the remotest connection to physics. For a report from the conference, see Robert Helling's weblog.

The talks from a conference held at Potsdam in April on Geometry and Physics after 100 years of Einstein's Relativity are available.

The Bonn Arbeitstagung is a summer conference that was started by Friedrich Hirzebruch in the late fifties, and which often has been the site of announcements of important developments in mathematics. The 2005 Arbeitstagung ended last week and notes from the talks are on-line.

Comments

1. **D R Lunsford**
   June 28, 2005

   Yes, Peter, and in fact the "Dreimaennerarbeit" was really the beginning of QM, so he's got a point.

   -drl

2. **Nathan Lanier**
   June 23, 2005

   Nice collectin, thanks. 🙂

3. **Peter Woit**
   June 23, 2005

   Hi Tony,

   Those stories are in the book. Born's complaints weren't so much about his problems fleeing the Nazis as that Heisenberg got a Nobel prize early on and he didn't until much later. I guess he thought the first prize for QM should have recognized the Born/Heisenberg/Jordan work, not just Heisenberg. One problem with this is that Jordan was a Nazi, and the Nobel committee wasn't enthusiastic about recognizing him.

4. **Tony Smith**
   June 23, 2005

   Peter, you say that you "… have little sympathy for one of its themes, that Born felt he didn't receive enough recognition for his work …".
   I have heard (no scholarly references, just gossip) that Born was as you indicated, somewhat insecure and feeling unappreciated, and that he felt such an insecure chord of emotion when he arrived (fleeing Hitler) at the Cambridge train station. One of the buildings most visible from the station was a movie theater whose marquee showed the movie title "Born to be Hanged".

   Further, he expected that he would stay in Cambridge as a professor there, and when he realized that Cambridge was only a first stop at which those fleeing Hitler were sorted out and matched up with available positions, he felt yet another chord of emotional insecurity and feeling unappreciated.

   (Maybe all that is in the book, but I have not read it, and I don't know.)

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

5. **Godspell**
   June 22, 2005

   "You can see that Dirac was rather genial as a youngster…"

   Did I miss something or you can tell it from the bright of his eyes?

6. **Nat Whilk**
   June 22, 2005

   It would make more sense for the author to have a pair of sites.

   With any luck, it will be trichinella.

7. **robert**
   June 22, 2005

   Thanks for that Peter – the Solvay ‘home movie’ made my day.

8. **D R Lunsford**
   June 22, 2005

   “Physmatics” – utter idiocy. How can people write such drivel with a straight face?

   The Weyl site is nice, but vastly understates his influence, and somewhat misrepresents his work on gauge invariance.

   The Solvay movie was great! You can see that Dirac was rather genial as a youngster, while Pauli seems to have always been a five-foot scowl. Ehrenfest seems almost giddy – no evidence of his impending suicide.

   -drl

9. **Levi**
   June 21, 2005

   I always appreciate it when you post such links. I bookmarked the Jaffe site. The Weyl site is certainly a strange combination of things though. It would make more sense for the author to have a pair of sites.

10,500 vacua!

June 26, 2005
Categories: Uncategorized

According to today's New York Times, "string theory allows for a vast number – 10,500 – of possible 'worlds' wth different self-consistent sets of laws and constants." If 10,500 is "vast", I wonder what 10^{500} is? I also wonder how long it will take for the news to get through that actually this number is infinite, which is somewhat bigger than "vast".

The Times piece is an extract from a recent Scientific American article on variable constants of nature. It also includes the standard claim that M-theory is "the best candidate for a theory of everything", but perhaps an editor is editorializing by pairing this with the news from an astrologer that Saturn is going from Cancer to Leo and Mars is in Taurus. At least the astrologer is able to make predictions: "Many born during this summer of '05 … will impact the world greatly in years to come."

Comments

1. **Kea**
   July 4, 2005

   "Lubos Motl and I would imagine pretty much everyone who works on string-m-theory (i.e witten randall susskind) would disagree with you"

   Somehow I don't think so.

2. **dan**
   July 4, 2005

   "This simply isn't possible. Nature is far more sublime than this simplistic and overly classical view."

   lubos motl and i would imagine pretty much everyone who works on string-m-theory (i.e witten randall susskind) would disagree with you. obviously if the world as it exists in itself is NOT 11D SUSY then all the *physics* work on string-m theory is a waste, but at present we have no way of knowing/testing for that. of course the same can be said of non-SUSY spin networks/spin foam.

3. **Kea**
   July 3, 2005

   "what if the world as it exists is 11-dimensional SUSY and our world is one example of an actual Calabi-Yau manifold of 10^500 possible examples"

   This simply isn't possible. Nature is far more sublime than this simplistic and overly classical view.

4. **dan**
   July 3, 2005

   here's a what-if question i have for anti-stringy

   — what if the world as it exist is 11-dimensional SUSY and our world is one example of an actual kalabi-yau manifold of 10^500 possible examples, and that particles are 1-D strings vibrating in 11D as described by the mathematics of string theory.

   wouldn't this scenario justify continued research into string theory for as long as there is physics?

   of course, i've read lubos motl's objections to LQG on his website, and the same what-if applies there (what-if space-time is only 4d non-SUSY, and is described on the planck scales as discrete spin networks would justify continued research into LQG despite its current difficulties)

5. July 3, 2005

   quoted

6. **Alejandro Rivero**
   July 3, 2005

   Phys.Lett.B149:351-356,1984 is sometimes quotes as the initial commentary on axions in string field theory.

7. June 30, 2005

   thanks for the correction, you were right. it now looks to me like 5 landscape talks

   Frederik Denef, Rutgers University
   Constructions and distributions of string vacua (review talk)

   Michael Dine, Santa Cruz Insitute for Particle Physics
   Branches of the Landscape

   Michael Douglas, I.H.E.S., Rutgers University
   Is the number of string vacua finite?

   Shamit Kachru, SLAC, Stanford University
   A classical type IIA landscape

   Fernando Quevedo,University of Cambridge
   Exponentially large extra dimensions and soft supersymmetry breaking in type IIB flux compactifications

   Plus add to that Susskind's which is one of the two public lectures, to get an overall idea of what sort of showing.

8. **Peter Woit**
June 29, 2005

Well, presumably Denef will be talking about the landscape since that is what he has been working on. And Quevedo's work is also part of the KKLT program, see http://www.arxiv.org/abs/hep-th/0505252

The only other talks related to particle physics unification are Arkani-Hamed's and de Roeck's. de Roeck isn't even a string theorist and presumably will have nothing to say about string theory. Unless Arkani Hamed has figured out some way of using string theory to say what will happen at the LHC, his also won't be about string theory.

So, the only talks that actually involve the idea of using string theory for particle physics unification are the landscape ones. Possible exceptions are Witten, depending on what he has to say about string theory axions, and Yau, who presumably is talking about non-Kahler backgrounds, something which would really make the number of backgrounds infinite…

Looks to me like string theorists have de facto given up on using string theory to do particle physics unification, with the only people still pursuing this the crazies lost in the landscape.

9.
June 29, 2005

in String 05 programme the title of Arkani-Hamed's talk has been posted as about the state of "HEP in year 2010", so not explicitly about landscape but more about impact of future experimental results, presumably.

so now I count 36 titles and only 3 are explicitly about landscape.

I think I counted 44 speakers, with 8 titles "TBA" and so 36 titles listed and only 1/12 of them about landscape.

of course there is the hoopla public lecture by Leonard Susskind with a title to whet the itch of religious curiosity, but that is in the media window of string, which I think is different from the talks.

10. **D R Lunsford**
June 29, 2005

Dear F. Uckoff,

What do you find remotely interesting? There isn't a single idea in the entire lot. It's the pounding of hammers on a steel plate.

-drl

11.
June 28, 2005

To continue the "content analysis" of Strings 05 lineup, the breakdown could be that, while a few are in Landscape, many of the others are in Escape

in other words, to avoid thinking about basic problems, which are a headache, think about black holery/cosmology instead, or read Lord of the Rings.

I counted only three titles about Landscape (out of the 35 titles listed so far)

Michael Dine, Santa Cruz Insitute for Particle Physics
Branches of the Landscape

Michael Douglas, I.H.E.S., Rutgers University
Is the number of string vacua finite?

Shamit Kachru, SLAC, Stanford University
A classical type IIA landscape

admittedly there are the 8 remaining TBA talks, some of which could turn into landscape, like

Nima Arkani-Hamed, Harvard
TBA

12. **Peter Woit**
June 28, 2005

I don't know Douglas personally, and I wasn't accusing him of "making up" stuff he doesn't believe in. I have no reason to believe that he is cynically saying one thing and believing another.

But, you can read about the last time I saw him in person here. I think the fact that he is pursuing a research program that has no plausible hope of ever producing a prediction of anything means that he is doing pure pseudo-science, and the fact that many others are joining him in this is extremely disturbing. And I don't think this is a minority opinion in the physics community. I've found very few string theorists willing to defend what Douglas is up to. People like David Gross and Lubos Motl have made their views about this publicly known, and many, many others privately feel the same way.

Douglas is not the only one whose utter refusal to acknowledge the failure of the string theory has led him to work on things that make absolutely no sense. He, Susskind, and others though, have gone beyond just the standard string theorist's wishful thinking and have completely abandoned the usual norms of what it means to do science.

13. **Kyle**
June 28, 2005

That's funny, I read "Well, Douglas has to either come up with a reason for the number to be finite…" to mean that there has recently been an argument that the number is actually infinite, and he'll have to have a better argument to make his case.

That seems less like a strong acusation and more like common sense to me.

14. **Chris Oakley**

June 28, 2005

I'm not sure about that, Fyodor. Cosmology is more a topic of conversation for those participating in the use of banned substances rather than an exact science.

15. **Fyodor Uckoff**
June 28, 2005

"they are all bullshit nevertheless"

What struck me about the list of titles was how interesting many of them look. Unlike the dreary drivel that is posted here whenever PW goes on holiday and the lunatics take over the asylum.

16. **D R Lunsford**
June 28, 2005

Good point, _____. Those papers are not about string theory, but they are all bullshit nevertheless. Getting shed of strings will not solve very much.

-drl

17. **M**
June 28, 2005

Many people who read what you said will read it as: "Oh, of course he will MAKE UP SOMETHING rather than admit what he did was a failure." I think this is a very strong accusation, and if it is not one you want to make, you should be more careful.

I agree this would be a strong accusation, if that were what Peter is implying. It is certainly possible interpret his words that way, but such an interpretation would itself be a form of accusation that Peter is carelessly assuming Douglas has less-than-noble motives. Implied accusations can work both ways…

Assuming that Douglas will probably prefer a rationalization over admitting his program is wrong just seems like a recognition of common human nature — if you have invested a lot of time, energy and professional "capital" into a program, it can be very hard to admit to yourself that it was a mistake, and usually even harder to say it to others. It is very human to believe and rationalize what one wants to believe, and recognizing that is a far cry from implying questionable ethics. And so far, Douglas hasn't seemed to openly question the idea of statistically "analyzing" the landscape, at least that I have heard…

18.
June 27, 2005

that other anonymous said*Many people who read what you said will read it as: "Oh, of course he will MAKE UP SOMETHING rather than admit what he did was a failure." I think this is a very strong accusation…*
heh heh now we have an argument amongst the nameless, because I think what other anonymous says sounds silly. Landscape is speculative mush, so IMO there is no chance for someone to distort findings. no way can one impute reprehensible scholarly misconduct to anybody.
Mike Douglas seems to me (on admittedly very little clues) to be a totally nice sincere person and I attribute to him scholarly integrity in spades, but the whole thing is so iffy that how could he NOT look on the bright side and emphasize reasons to hope it's finite.

19.
June 27, 2005

"Well, Douglas has to either come up with a reason for the number to be finite, or admit that the research program he has promoted heavily for the last couple years is a complete failure. I'm guessing he'll go for the first alternative."

May I ask if you know Douglas in person? Or, more to the point, what you think of his academic integrity? Do you think he honestly explores and says what he thinks is true, or do you think even when he thinks something doesn't work, he spins things in a way that would make string theory good?

Many people who read what you said will read it as: "Oh, of course he will MAKE UP SOMETHING rather than admit what he did was a failure." I think this is a very strong accusation, and if it is not one you want to make, you should be more careful.

20. **Torbjorn Larsson**
June 27, 2005

Nitpicks: "good we are halfway there" – is of course correct in orders of magnitude if they started out at ~ 1 watt (not Watts; see SI measures).

21.
June 27, 2005

I did some "content analysis" (simple counting) of the titles of the talks on the String 05 program. 43 speakers were listed but 8 titles were TBA leaving 35 titles of talks.

Of these 35, a rather large number seemed to be about cosmology, or about black holes/rings. So I counted those up and they came to 14.

In other words 40 percent of the scheduled talks were about black holes or some cosmic topic. Many had appealing titles like Strominger "Fun with Black Holes" and Tye "Wavefunction of the Universe". In case anyone might be interested, here are those 14 titles:

Melanie Becker,University of Maryland
M-theory Cosmology

Iosif Bena, UCLA
Geometric Transitions, Black Rings and Black Hole Microstates

Atish Dabholkar,Tata Institute of Fundamental Research
Going Beyond Bekenstein & Hawking Exact and Asymptotic Degeneracies of Small Black Holes

Henriette Elvang,UC Santa Barbara
Black rings

Gary Horowitz, University of California Santa Barbara
A new endpoint for Hawking evaporation

Renata Kallosh, Stanford
String cosmology and the index of the Dirac operator

Per Kraus, UCLA
Attractors, Anomalies, and Black Hole Entropy

Joseph Polchinski , KITP, UCSB
Update on cosmic strings

Ashoke Sen, Harish-Chandra Research Institute
Extremal black holes in higher derivative gravity

Eva Silverstein, SLAC, Stanford University
The Tachyon at the End of the Universe

Andrew Strominger, Harvard
Fun with Black Holes

Henry Tye, Cornell
Wavefunction of the Universe

Erik Verlinde, ITF, Universiteit van Amsterdam
A Matrix Big Bang

Bernard de Wit, Institue for Theoretical Physics & Spinoza Institute, Utrecht University
Supersymmetric Black Hole Partition Functions

Here is the full list:
http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/speakers.html

I suppose the above count could be meaningless, or it may simply confirm what has already been said as regards shifting emphasis in string research.

22.
June 27, 2005

Quantoken wrote: "God is not capable ot doing that, since he created just one universe"

You know this for a fact? 🙂

23. **Quatoken**
June 27, 2005

Anonymous said "I can only assume that everyone at the New York times is **mathematically illiterate** and don't know what 10^500 actually means"

Most people ARE mathematically illiterate in failing to appreciate how big 10^500 actually is, that include any one who may believe there is a difference between 10^500 and infinity. 10^500 is a finite number in strict mathematical sense, but for all practicality purpose that is physically meaningful, 10^500 IS an infinity, and there is physics difference between the two.

To appreciate how big the number is, let say we just do a simple enumeration and simply list all the 10^500 vacuas, each entry costs one English letter to list. How big will the entry book be, will it be larger than the Encyclopedia Britannica?

The answer is the total quantum information the whole universe can register is merely 10^120. So even if we turn the whole universe into a huge quantum memory chip, it is 10^380 times short of just to enumerate the list of possible vacuas. It would take 10^380 universes just to do that. Even the almighty God is not capable ot doing that, since he created just one universe.

So string theory ceased to have any remote connection to science once up to 10^500 different vacuas were proposed to be possibly "exist". Go to the other 10^380 universe to research it. Our universe is simply **pitifully too small** to allow the kind of crap like 10^500 vacuas.

Quantoken

24.
June 27, 2005

10,500 is a lot of vacua! I can only assume that everyone at the New York times is mathematically illiterate and don't know what 10^500 actually means! Would not surprise me in this day and age.
I remember hearing about a some idiot senator commenting on a 'star wars' laser system. He was told they had a system that currently operates at 10^4 Watts or something like that but for actual defence purposes would have to be 10^8 Watts (don't know the exact numbers), to which he replied, "good we are halfway there". Don't know if this anecdote is true but it probably is.

25. **Peter Woit**
June 27, 2005

Well, Douglas has to either come up with a reason for the number to be finite, or admit that the research program he has promoted heavily for the last couple years is a complete failure. I'm guessing he'll go for the first alternative.

But from everything I've heard, he makes the number of vacua finite by introducing a cut-off, and one expects things then to depend strongly on the cut-off. The interesting question will be whether he has a plausible argument for cut-off independence.

Also note that a couple years ago the big question he and others were pursuing was "how many vacua?": if the number was 10^100 that was good, if 10^500 that was bad (no predictions possible). Now he seems to have given up on that and is worrying about finiteness. I predict that at Strings 2007 he'll speak about "Is the number of string vacua countably or uncountably infinite?"

26.
June 27, 2005

Douglas is going to give a talk called

Is the number of string vacua finite?

The word I hear is that the answer is going to be, effectively, *yes*.

27.
June 27, 2005

I now see there is a list of the titles of the talks here:

http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/speakers.html

the other link gives the schedule:
http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/program.html

28.
June 26, 2005

The tentative programme schedule for July 11-16 "Strings 05" has now been posted. However so far it just gives the speakers' names without titles of talks:

http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/program.html

29. **FineStructure**
June 26, 2005

I wonder when people would realize the difference between 10,500 and 10^500. I wonder which of the prediction would turn out to be correct. I predict none

30.
June 26, 2005

Poor english. The word "fanatic" does not necessarily mean crackpot. I would call anyone who has spent a considerable amount of time on a particular subject a fanatic.

31. **JC**
June 26, 2005

I've always wondered what makes a particular topic attractive to fanatics, whether it's astrology, Marxism, Ayn Rand, string theory, supply-side economics, evangelical Christianity, extreme Islam, etc … The only common feature I can think of in these particular topics of interest to fanatics, is that they all seem to promise and/or portray a precise worldview "vision" of some sort. It's as if the particular topic's "doctrine" is portrayed as a concise map to a "promised land" of some sort, whether metaphorically or literally.

In the real world, most things seem to be a lot more uncertain and messy to deal with. ("Utopia" is rarely ever found in this world). Fanatics in general don't seem to be attracted to things which cannot be viewed in a "black and white" manner. They don't seem to like subjects which requires one to view things in "shades of grey".

Awhile ago I read Eric Hoffer's book "The True Believer – Thoughts on the Nature of Mass Movements", which discusses the mindset of fanatics and the dynamics of the mass movements they run.

Witten on CNN

June 28, 2005
Categories: Uncategorized

Via David Goss and Lubos Motl, the news that CNN's Candy Crowley has a piece about Witten. Unlike Lee Smolin's Why No "New Einstein"? piece, CNN more or less identifies Witten as the new Einstein. Witten is quoted as giving the following rather defensive statement about string theory: "I just think too many nice things have happened in string theory for it to be all wrong… Humans do not understand it very well, but I just don't believe there is a big cosmic conspiracy that created this incredible thing that has nothing to do with the real world." He's kind of defending against a straw man, since virtually no one is saying string theory is "all wrong" or "has nothing to do with the real world". It quite possibly can provide some sort of dual description of QCD, and that is what much research in string theory these days is aiming for. On the other hand, the conjecture that you can make a unified TOE using string/M-theory in 11 dimensions at this point shows every evidence of being all wrong.

Comments

1. **D R Lunsford**
   July 4, 2005

   Having mass means having a rest frame. In such a case the idea of "left" and "right" neutrinos disappears, and so any Lagrangian built only on (1+y5), which projects out the heretofore unphysical right-handed neutrinos, is going to be wrong. So I assume one adds in some terms in (1-y5) which go to zero with the neutrino mass. But then the mass has suffered from "scope creep". It's complicated.

   -drl

2. **dan**
   July 3, 2005

   i've wondered if neutrinos have mass, does this imply velocity can change if forces act on it?

   "GUTS, both SUSY and non-SUSY, only achieve "unification" by introducing a whole new very problematic Higgs sector to break the new unification symmetries they introduce. This has never been very convincing."

   i am aware that GUT's make predictions like proton decay that have not been verified. Do you think the strong nuclear force can be unified with the electro-weak force if you find the higgs sector to be unconvincing?

3. **Juan R.**
   July 2, 2005

   The situation with string M-theory is specially simple.

   All, exactly all physics, predicted in the past by string theorists was wrong. This indicate, at least to me, the profound misunderstanding of nature that they have.

   Initially, I studied string M-theory, including very recent material like stwing or non-commutative M(atrix) theory. All was wrong, and often the math involved simply ridiculous.

   Seiberg-Witten paper in non-commutative geometry and string theory is very simplistic from a mathematical point of view (specially when one already worked with the non commutative star products in the phase space representation of quantum mechanics). Thus, I remained perplexed of the low level of string literature (when compared with popular claims on books and conferences). In many aspects, string theory is outdated. This is claimed in many published papers and conferences. For example, in the last conference Quantum future, Claus Kiefer and Erich Joos said

   "*this is even true for tentative frameworks such as GUT theories or superstring theory. Although the latter may seem 'exotic' in some of its aspects (containing D-branes, many spacetime dimensions, etc.), it is very traditional in the sense of the quantum theoretical formalism employed*."

   Very traditional may be read "outdated" regarding, for instance, sophisticated experiments with fullerenes.

   I am astonished that the self-proclamed ultra-advanced NC string theory (a radical modification of usual string theory in fixed background/cosmologies) used, in the last decade, advanced math developed in other fields of science by Prigogine and the Brussels School in the **60s**. The delay of the ?ultra advanced? theory is of most than 30 years for a supposed "profound" theory that, in the words of Brian Greene, is providing us the most basic understanding of nature! This is, obviously, false, simply propaganda.

   The same situation arises in recent TFD Dp-brane theory. Today, string theorists are very excited with the new formalism (was unknown for them), but people that developed TFD in the past (were not string theorists) now are developing TFD II. Again, string M-theory is outdated.

   What is correct in string theory (like a TOE)?

   Simply nothing!! It is a waste of time.

   Of course, the claim of that string theory quantizes gravity or predicts GR is false propaganda.

   String theorists are well known due to their falsification of true. This is the reason of this blog.

   For example, in his Elegant Universe, Brian Greene explained to many people that the observed 4D geometry of universe was explained from string considerations or that the concept of pointlike particles was substituted by the concept of strings, for example.

   Curiously, he forgot to comment to public that string is an approximation and that the only known formulation of M-theory is a quantum mechanics of pointlike particles (D0-branes).

   Ok, but can string theorists explain why universe look like 4D? According to Brian Greene popular book, that was explained by Brandenberger y Vafa.

   To the question

   – If these extra dimensions exist, does string theory offer any explanation of why there are apparently three space dimensions larger than the rest?

Witten recently replied

– That’s a big problem that has to be explained. As of now, string theorists have no explanation of why there are three large dimensions as well as time, and the other dimensions are microscopic.

4. **Peter Woit**
July 2, 2005

There’s very strong evidence at least for neutrino oscillations, and I don’t think there’s any known way to explain this withough giving neutrinos mass.

The argument that the neutralino could be a dark matter candidate is the kind of thing that I had in mind in saying the arguments for supersymmetry are very weak. For one thing, you have no idea what the mass of a neutralino in these theories is, choosing it to solve the dark matter problem just looks to me like wishful thinking.

GUTS, both SUSY and non-SUSY, only achieve “unification” by introducing a whole new very problematic Higgs sector to break the new unification symmetries they introduce. This has never been very convincing.

5. **dan**
July 1, 2005

thanks for answering some of my questions. as for SUSY, it’s been said the strongest candidate for cold dark matter is a SUSY-photon/higgs boson called the neuatralino.

the standard model does not yet unify electro-weak with the strong nuclear force, and most such unification scenarios predict proton decay and magentic monopoles. it’s my understanding that non-SUSY unification scenarios predict unobserved short half-lives of protons, so what is left is SUSY-unification scenarios of the standard model.

by the way, do you think neutrinos have rest mass?

6. **D R Lunsford**
July 1, 2005

Strange. Fourth order equaitions fill me with horror, although you can see how the string guys with a world tube have to deal with the sort of things ruled by 4th order equations in elasticity. So this must be why anyone would even consider it. Weyl’s theory in 4d died from 4th-orderness. Again, it’s like mangling some colossal Scrabble Lagrangian so that it churns up something resembling QED.

7. **Peter Woit**
July 1, 2005

Hi Danny,

R^2 theories do have fourth-order equations of motion in the metric variables, thus problems with unitarity and causality. Some people (e.g. Hawking) at times have claimed that maybe these can be dealt with. More positively, these theories are renormalizable.

All known approaches to quantum gravity have one problem or other.

8. **Peter Woit**
July 1, 2005

The standard model isn’t a theory of everything, in particular it isn’t a theory of cosmology. It’s a theory of what particles and fields there are and how they interact (excluding gravity). As for the specific questions you ask:

matter/antimatter asymmetry: where this came from is a cosmological question. In conventional cosmological models, you can take advantage of various features of the standard model (CP violation, electro-weak symmetry breaking) to get mechanisms that produce this asymmetry. Whether within such cosmological models these mechanisms are sufficient to produce the observed amount of asymmetry I believe is still an open question.

particle masses: this is perhaps the main weakness of the standard model. You have to put these numbers in by hand, whereas you would like the theory to predict them.

magnetic monopoles: there are no stable, finite energy magnetic monopoles in the standard model. This agrees perfectly well with the fact that they aren’t observed.

dark matter: again this is a question of cosmology and astrophysics. There is no particle in the standard model whose abundance in standard cosmological models would explain what dark matter is. Maybe there’s a new particle not known to the standard model, maybe something else is causing this.

dark energy: you can set the vacuum energy to whatever you want in the standard model, including the observed cosmological value. The vacuum energy has the same status as particle masses in the standard model, it’s an undetermined parameter.

SUSY: there’s no good reason to extend the standard model to a supersymmetric version. The positive arguments about what this buys you are weak, and supersymmetric models are much more complicated, have a hundred or more extra undetemined parameters, and a host of other problems.

9. **dan**
July 1, 2005

Peter, when you speak of the standard model working so well, how does it explain the matter/antimatter asymetry? does it predict masses? why are there no magnetic monopoles? what is dark matter? what is dark energy? do you think the standard model needs to incorporate SUSY?

10. **D R Lunsford**
July 1, 2005

Peter, what are R^2 theories? Aren’t those fourth order in some context (and so loaded with unphysical solutions)?

-drl

11. **Kyle**
June 30, 2005

“This said, String Theory in it’s various forms and sects is a very useful theoretical tool,”

I’ll ask the question on everyone’s mind:

Useful for what?

12. **pseudo string fan**
June 30, 2005

Hi Chris,

Is it true that the LHC (Large Hadron Collider) will be used to find the super-symmetric particles that string theory has proposed?

A little background about me: I am a physics graduate in Hong Kong. String thoery appears too profound to me. But I am interested in its latest development.

13. **Chris Oakley**
June 30, 2005

Do String theories contribute? Yes. Isn't that enough?

No it isn't, and if I had known 25 years ago that the majority of particle "physicists" were apt to go on this quasi-religious quest just because Ed Witten told them to, I would have gone into molecular biology instead.

There is no, repeat, no experimental evidence to support string theory or even to suggest that it might be a promising idea.

14. **A Scott Crawford**
June 30, 2005

Juan,

I had to knock on wood to ward off the bad mojo that comes from slandering Newton! When it came to understanding the universe Newton was wise enough to defer to "angels" as a viable force when pressed;)

This said, String Theory in it's various forms and sects is a very useful theoretical tool, and I think those who'd pooh pooh them for their inability to be all things to all people are missing the point. There's an infinite variety of ways to view collections of data gathered across the spectrum of experimental apparatus, but there's very few ways to accurately identify generally observed trends for future practical refinement and advance of our total body of scientific understanding. Do String theories contribute? Yes. Isn't that enough?

15. **Chris Oakley**
June 29, 2005

Penrose seems to think highly of something called Twistor Theory.

This is a bit like saying "the pope seems to think highly of a religion called Catholicism".

16. **Peter Woit**
June 29, 2005

No, I don't want to elaborate on this. There have already been long discussions about this here, see for instance Lee Smolin's response I posted on June 15, 2005, as well as his exchange here with Larry Yaffe (August 19 and August 22, 2004).

In general my impression is that neither LQG nor string theory has yet given a completely satisfactory theory of quantum gravity. But the LQG program tends to work with well-defined concepts so you can at least see exactly what their problems are, whereas the string theory arguments often involve very ill-defined concepts and loads of wishful thinking.

17.
June 29, 2005

"…although arguably at least one of them (LQG) does a better job of it than string theory…"

Can you elaborate on the ways you think that this is true?

18. **Mike Crowley**
June 29, 2005

Hi Pseudo String Fan,

Your question is one I was asking myself two months ago. I'm not a scientist, just an interested layperson. The thing that helped me grasp some of the issues involved was reading Roger Penrose's chapters on String Theory in his book "Road to Reality." He deals with some of the difficulties with grand unification, and while I can't pretend I understood every word I did digest enough to recognize that there's more to the story than we get from a book like Michio Kaku's "Parallel Worlds." For me at least it was a real wake up call.

Penrose seems to think highly of something called Twistor Theory. He also includes a chapter on Loop Quantum Gravity and other potential avenues of pursuit.

Mike

19. **Peter Woit**
June 29, 2005

Hi pseudo string fan,

Despite what you might hear in some places, string theory doesn't actually now provide a consistent unification of quantum theory and gravity. The string perturbation series is divergent and no one knows what non-perturbative theory it is supposedly asymptotic to.

There are quite a few other attempts to unify quantum theory and gravity, none of them completely satisfactory, although arguably at least one of them (LQG) does a better job of it than string theory. Some of them are:

1. Loop quantum gravity

2. R^2 theories

3. “Induced gravity”

4. Various versions of triangulated or lattice gravity

5. Topological quantum gravity

and there are probably others…

20. **Peter Woit**
June 29, 2005

Hi Robert,

I voted yes on the CNN poll. Modulo the standard caveats about what one means by a “theory of everything”, I do believe that sooner or later we’ll have a theory that ties up the remaining loose ends of the standard model. Personally I think the fact that the standard model works so well means we’re actually rather close to what Weinberg calls a “final theory”. We might even be there now if so much of the effort of the last twenty years hadn’t been wasted…

21. **pseudo string fan**
June 29, 2005

If you don’t do string theory, with what theories can you bring general relativity and quantum mechanics together?

22.
June 29, 2005

I just wish Michio Kaku would calm down a bit.

23. **Robert**
June 29, 2005

So Peter, did you participate in the poll on the CNN web page?

24. **Juan R.**
June 29, 2005

String theory is, in the words of its more popular practitioners, the Final Theory: **the Last True**.

It was not designed like a dual representation of QCD, like an attempt to perturbatively quantize gravity alone or like a generator of mathematical ideas, it was studied, extended, thought in some High Schools, and popularized to public like the Final theory: the Theory Of Everything.

String theory is wrong; it has failed for explaining everything. It has failed for quantize gravity (perturbative series is not well defined and nonperturbative regime is unknown), it cannot explain GR (contrary to popular Witten claims string theory does not predict gravity, really string theory is adapted to previously known gravity), etc.

I partially agree with the idea of Witten like a new Einstein. Of course, the contribution of Witten to physics is irrelevant (Murray Gell-Mann advanced physics 10 times more than Witten). But I agree in that the emphasis of Witten on “his” string theory sound like the emphasis of Einstein in his unified field theory. Einstein didn?t understand other best theories developed in his time and Witten is doing the same error now.

String, stwing, M-theory, and all that stuff are a waste of time. *Since that their research is based in irrelevant mathematical formalisms and outdated concepts*. String M-theorists (here and thereafter SMt) are so arrogant that with a superfitial mathematical knowledge of other fields of science, they claim for a Theory Of Everything. *The list of outdated concepts and irrelevant mathematical formalisms is greater than number of estimated vacua* (-:

– Usual quantization of the classical bosonic string violates cosmological boundaries. (I wait that SMts will recognize this error before 2050 :-).

– The spacetime used (CY, G2, etc.) do not account for the non-differential character of stochastic processes. It will be funny like string theorists will attempt to model spacetime-foam noisy contributions to a triple D0-brane collision. I wait to see his faces then!

– In the usual unitary vectors space of string mathematics the L-product of two elements is not defined. Only in the L-space the relation *I+(rs)I+(tu) = I+(ru)delta(st)* is defined. No similar product relation exists in the H-space (dimension n) because L-space (dimension nn) is more general. That is, the supposed TOE cannot explain, for instance, Ernst?s work in NMR (that received the Nobel prize) 🙂

– The fixed background S-matrix is, undoubtedly, a funny caricature of real-word processes. There are dozens of well-known papers on the topic and even a new branch of string theory developed!

– Vector states used in the standard spectral decomposition of strings (branes) are of course valid only in the limit *T* –> 0. This was known for decades in other fields of science like plasma physics (quark-gluon plasma). Only the last 5 years, after of three decades of totally wrong research and funny claims, SMts fixed this sound error and developed the very recent Dp-branes theory with the (~) operators, which only work in the linear regime. Far from eq. one cannot use the tilde (~) operators due to well-known presence of dynamical bubbles coupling spacetime events. Of course, all this advanced mathematical stuff was/is ignored by leading researchers like Witten, Greene, Vafa, Schwartz, etc. Witten, the great genius, the great theoretician, the new “Einstein”, did **NONE** contribution to recent doubled space Dp-branes theory. *In fact, his great mind did not know the problem with the use of standard states because he like other string theorists study the topics just superficially*. He is a great string theorist sure 🙂

– String theorists still claim for the derivation of an unitary theory, whereas people in other experimentally proved theories are working with LPS theory in Gelfand triplets. The theorems used are outside of the simple and outdated string mathematics, and one needs a lot of recent mathematical work in rigged spaces and involutive Banach algebra of bounded operators. These non C*-algebras are, of course, ignored by SMTs and their irrelevant TOE. It is impossible to explain recent models for neutral Kaons and its counterparts in higher-flavor-generations from the basic mathematical framework of string, M theory. Concretely the models developed in the last decade by Sudarshan (e.g. generalizing the LOY model) does not fit to string M-theory because are more general.

– Non-critical string theory is more advanced that usual critical (Witten-Schwartz-Vafa-Greene-etc.) one in fixed backgrounds but again irrelevant for a TOE. The most advanced formulation today in non-critical theory simply use ?Lindblad?like? operators (which is only valid if one take the zero limit of the correlation functions for the different vacua) to take into account quantum transitions between different critical string vacua. Moreover, the non-critical string theory has unsolved problems. One can show (with the aid of mathematical methods unknown for SMts and still don’t applied to noncritical formulation) that non-critical string theory formulation is just a shadow to more consistent and generalized theories.

M-theory is "*the best candidate for a theory of everything*", sound like that old claim of "*all universe is understood from Newton mechanics*".

All of us know how accurate was the claim 🙂

25. **D R Lunsford**
June 29, 2005

String theory is wrong because it utterly fails inside its context. Likewise, Weyl's theory is a total failure, notwithstanding Weyl 1929 where phase is made the cuckold of gauge (see O'Rafairtaigh). Failure in physics means insufficiency to the expected context. On this level, string theory is an absolute failure.

-drl

26. **Peter Woit**
June 28, 2005

Not sure what your simple SPIRES search is, but looking at the SPIRES list of most heavily cited papers in 2004, the top 5 string theory ones are

1. Maldacena on AdS/CFT 446 citations
2. Witten on AdS/CFT 258 citations
3. Seiberg-Witten on non-commutative geometry and string theory 231 citations
4. Gubser, Klebanov and Polyakov on Ads/CFT 225 citations
5. KKLT paper 190 citations

Only 5. has anything directly to do with getting a unified theory out of string theory. We can argue about what use 3 is, but 1, 2, and 4, are about AdS/CFT, an explicit relationship between a 4d QFT closely related to QCD and a 5d string theory dual to it, something which has nothing to so with string theory as a unified theory.

27. **Ohne**
June 28, 2005

"It quite possibly can provide some sort of dual description of QCD, and that is what much research in string theory these days is aiming for."

By much I guess you mean about four percent, as a simple SPIRES search can verify?

Ohne

New Top Quark Mass?

June 28, 2005
Categories: Uncategorized

There's an article in the Chronicle of Higher Education about the Quantum Diaries Webloggers. It points out that since this project is organized by the high energy physics labs, it "presents a sanitized version of life in high-energy physics", with one of the participants quoted as saying "None of us wants to be responsible for saying anything negative." Well at least there is one weblog dealing with high-energy physics where things are not sanitized….

From Gordon Watts, one of the Quantum Diaries webloggers, I learned about the Tevatron Connection Program, which brings together theorists and people from the CDF and D0 experiments at the Tevatron. It seems to me that new results on the top mass were first made public this past weekend at this conference (see here and here). The latest combined CDF/D0 result is ("pending final CDF/DO review"):

top quark mass = 174.3 +/- 3.4 Gev

compare to the previous result using Run I data of

top quark mass = 178.0 +/- 4.3 Gev

In the standard model, the new top quark mass implies a value for the Higgs mass of 94 +54/-35 Gev. Note that much of this range is excluded by the LEP result that the Higgs mass must be above 114 Gev. In the minimal supersymmetric standard model with this top quark mass, getting the Higgs mass above 114 Gev requires making the superpartner of the top quite heavy, introducing a certain amount of fine-tuning into the theory. For more about this, see a posting by Jacques Distler.

Comments

1. **Matthew**
   July 5, 2005

   Why is the matrix so large in the first place, here as opposed to elsewhere

   Because you can't do local updates. For the gluon action, you're locally updating the gauge field. So at any point, you only need to know the nearest neighbours. Whereas with the quarks, you've "done" the path integral, so it cares about every gluon field, at every point.

   Put more simply, you're not updateing the "quark field at point x" you're computing the entire effect of the quark field at once.

   Also, it seems to me if you get good precision in the higher mass calculation, the scaling would naively appear to be linear in the eigenvalues.

   The compute time scals as the inverse. And the physical chiral limit is determined by Chiral PT, which shows that the extrapolation is not linear.

   Which again, computing power seems to have vastly superceded since I first heard about Lattice QCD (Moore's law et al). So, obviously it cannot be linear, what exactly big O() is it?

   For a fixed volume compute time scales roughly as the inverse 6th power of the lattice spacing, times the inverse of the lightest pion mass. 4 powers of the lattice spacing are just the number of points on the grid. One is critical slowing of the gluon update algorithm, and the remaing mass*spacing is the slowing of the quark matrix inversion.

   In "real life" it's even worse than this. But this sums it up pretty well.

2. **Fred**
   July 5, 2005

   "So you end up having to compute the determinant of a very large matrix."

   Yea I never quite understood this. Why is the matrix so large in the first place, here as opposed to elsewhere (where other lqcd calculations seem to be so reliable). Is it merely b/c of the smallness of the lattice spacing necessary that leads to such huge matrices? Also, it seems to me if you get good precision in the higher mass calculation, the scaling would naively appear to be linear in the eigenvalues. Which again, computing power seems to have vastly superceded since I first heard about Lattice QCD (Moore's law et al). So, obviously it cannot be linear, what exactly big O() is it?

3. **Matthew**
   July 4, 2005

   What exactly is the nature of the problem in getting the up/down mass light enough?

   Okay, in lattice QCD you write the fermion action as

   S = \bar{\psi} M \psi

   where M is some huge (but finite and totally well defined) matrix that depends on the gauge fields. Now you can't really do grassman variables on the computer, so instead you perform the path integral over the fermions exactly, which gives you

   det(M)

   So you end up having to compute the determinant of a very large matrix. The major cost of doing this is computing the inverse of M. The cost of the algorithim you use to compute the inverse (conjugate gradiant) goes like the inverse of the smallest eigenvalue. And the smallest eigenvalue of the matrix M is the quark mass. So as you take the quark mass smaller and smaller, the cost of your simulation goes through the roof.

   If one is calculating the meson and/or baryon spectrum with heavier quarks (ie. charm, bottom, etc …), is it reasonable to treat them non-relativisticly?

For the bottom quarks the answer is yes. A non-relativistic effective theory is what we use. For the charm it's somewhat less clear. The charm is "heavy" but it's not quite "heavy enough" to let you trust non-relativistic expansions in the same way you trust them for the b quark.

It still worries me if the calculation is about mass splittings instead of absolute.

That is entirely due to the fact that the chiral perturbation theory for the absolute masses still needs doing. The simulations don't care one way or the other. The omega mass was computed, see the paper by Davies and Bernard.

Indeed it is already a bit regrettable the need to recourse to chiral extrapolation there. In some sense, we are not testing QCD anymore but QCD plus the effective theory we are expected to fit to.

I disagree. Chiral perturbation theory, just like heavy quark effective theory, is a consquence of QCD. Using fits based on chiral perturbation theory is a test of full QCD.

Or is the neutral pion one of these problematic low mass objects?

That I don't know actually. One rarely makes a distinction between the charged and neutral pions.

The \eta' is hard though, I know that.

4. **Alejandro Rivero**
July 2, 2005

You tune the 4 bare quark masses (up and down are degenerate, and you ignore the top) and the bare lattice spacing to reproduce 5 experimental numbers

Hmm I have taken a look to the paper; the small error in 5 experimental numbers (plus the four ones "exact" from tuning) are impressive, really more impressive than quenched approximations. It still worries me if the calculation is about mass splittings instead of absolute.

The light quark masses are the key problem in the modern simulations.

Indeed it is already a bit regrettable the need to recourse to chiral extrapolation there. In some sense, we are not testing QCD anymore but QCD plus the effective theory we are expected to fit to.

Acutally calculating meson masses is easy.

In surprises me that also the width for QCD-stable objects seems to be easy to calculate. In particular for the pseudoscalar, this means that the chiral anomaly is handled correctly, does it? Or is the neutral pion one of these problematic low mass objects?

5. **JC**
July 1, 2005

Matt,

What exactly is the nature of the problem in getting the up/down mass light enough?

If one is calculating the meson and/or baryon spectrum with heavier quarks (ie. charm, bottom, etc …), is it reasonable to treat them non-relativisticly? One would guess that mesons and/or baryons with light quarks would have to be treated relativistically.

6. **Matthew**
July 1, 2005

The results of quenching QCD, I dont know if it is a cause of optimism or the contrary. It seems a bit of everything goes.

Modern large scale simulations are unquenched. The paper I mentioned is an unquenched calculation.

Hmm but, besides a fundamental mass, how many free parameters do you need

You tune the 4 bare quark masses (up and down are degenerate, and you ignore the top) and the bare lattice spacing to reproduce 5 experimental numbers. That's it, there are no "free parameters" beyond that. This is not a model.

Are the tuned values published elshewhere?

Yes, you'd have to dig a bit through the references. The papers by the MILC people (Bernard et. al.) are the relevent ones for light mesons.

could you inform us of which other groups are obtaining sucessful predictions for the hadronic spectrum?

Well, every group does the meson spectrum to some degree, if only to fix parameters. Large scale calculations are being done by the CP-PACS group, see hep-lat/0409124 for example. It's not clear if they will be able to get to light enough quark masses though.

The light quark masses are the key problem in the modern simulations. That's where the difficulty is, getting the up/down mass light enough. Acutally calculating meson masses is easy.

7. **Alejandro Rivero**
July 1, 2005

The results of quenching QCD, I dont know if it is a cause of optimism or the contrary. It seems a bit of everything goes. After all, also the purely kinematical cross section of e+ e- —> muons is within a ten percent of the QED prediction, isnt it?

*3 * mass_cascade – mass_nucleon*

agrees with experiment at the few percent level. The paper hep-lat/0304004 has some other things. We've computed a few other light hadron quantities since then, the \Omega mass, for example.

Hmm but, besides a fundamental mass, how many free parameters do you need, and how sensitive the results are to veriations of these parameters? In 0304004, all the quark masses are tuned to reproduce the most important mass predictions (Are the tuned values published elshewhere?).

And now you are here, I wonder… could you inform us of which other groups are obtaining sucessful predictions for the hadronic spectrum?

8. **Matthew**
June 30, 2005

Regarding Lattice QCD,

The CP-PACS group reproducde the light hadron spectrum to within around 10% using quenched (i.e. not entirely physical) lattice QCD in the mid-nineties. There's a plot of this, which is what you probably saw in Wilczek's talk. For modern full QCD simulations we're not quite at the full hadron spectrum, but some things are done. In order to reduce systematic errors one often computes mass differences, for example, the combination

3 * mass_cascade – mass_nucleon

agrees with experiment at the few percent level. The paper hep-lat/0304004 has some other things. We've computed a few other light hadron quantities since then, the \Omega mass, for example.

It's better in the heavy quark sector. Apart from a couple of lingering problems, the charm and bottom meson spectrum has been totally computed, and agrees with experiments. There are also calculations of various decay constants and form factors, which are a bit harder than masses.

My opinion, I think it is fair to say that the meson spectra has not been calculated.

You're wrong, as even a cursory glance at the lattice liturature would show.

Note also we are speaking of unstable particles, so the "pole mass" spectra includes both mass and decay width

Many particles are stable, for example, you can get very clean pion masses. You are correct that unstable particles (such as the \rho) are harder, but it can be done.

Montecarlo errors are high, finite size effects etc

Actually, those errors are fairly well under control for spectrum calculations. What really bites you is discretization errors, and errors in the chiral extrapolations.

Of course, when you try to do harder things (glueball masses, K \to (2,3)\pi) statistics and finite volume are much worse. But for meson masses, it's pretty well under control.

9. **Peter Woit**
June 29, 2005

Hi JC,

I believe the calculation Robert mentions for K a Calabi-Yau gives a simple result: the low mass particles are massless and supersymmetry is unbroken. For obvious reasons this result is not heavily promoted.

A problem he doesn't mention is how to give dynamics to the moduli parameters for the compactification manifold. Naively the effective action doesn't depend on them so you end up with massless scalars, less naively you can believe in fixing them a la KKLT, then you have the landscape to deal with.

10. **JC**
June 29, 2005

Robert,

I'm familiar with what the string people do to obtain an effective action of the sort you have described. What I'm skeptical about concerning this particular effective action procedure, is that I have never seen anybody producing a convincing calculation of the quark and lepton masses in this manner which agrees with the experimental data.

Also what criteria is used to select the K in the 10K spacetime product R^4 x K, besides just trying out many different K's (ie. a torus, K3, Calabi-Yau, etc … which may or may not break some of the SUSY)? With 10^100 or more possible Calabi-Yau's (depending on who you ask), what criteria is used to choose one besides trying out every single one in an exhuastive manner? What would happen if 10^20 or 10^30 of the possible Calabi-Yau's all produce similar quark and lepton mass spectrums to within the experimental error bars? (Though it would be very impressive if exactly ONE Calabi-Yau actually could produce the correct quark and lepton mass spectrum, with all other Calabi-Yau's excluded).

11. **Peter Woit**
June 29, 2005

For the record, Tony Smith points out to me that his prediction for the neutrino mass and mixing angles are at:

http://www.valdostamuseum.org/hamsmith/snucalc.html#asno

and he also has predictions for the top quark and other quark masses at his web site.

I don't have any objections to people posting here a simple link to their predictions of this kind, but keep in mind that I don't want this weblog to be used as a forum for discussing the details of these arguments.

12. **Matti Pitkanen**
June 29, 2005

p-Adic thermodynamics for super-Virasoro generator L_0 taking essentially the role of mass squared operator provides an alternative view about elementary particle and hadron mass spectra explaining fundamental mass scales number theoretically. Also CKM matrix can be deduced to a high degree from number theoretic constraints. Since the possible Higgs contributes only a small shift to fermion masses, the production cross section for Higgs can be by a factor of order 1/100 lower than in standard model.

The five chapters in the second part of “TGD and p-Adic Numbers” contain the detailed calculations.

Matti Pitkanen

13. **Alejandro Rivero**
June 29, 2005

My opinion, I think it is fair to say that the meson spectra has not been calculated. Note also we are speaking of unstable particles, so the “pole mass” spectra includes both mass and decay width. But even mass alone, from QCD plus lattice, is not really got. Montecarlo errors are high, finite size effects etc.

14. **Robert**
June 29, 2005

Tony Smith can also compute the mass of a proton. And this at tree level. Which is remarkable for a composite object in a strongly coupled theory. Or numerology.

Seriously, it is much more convincing to argue that the mass scale of the proton is given by the QCD scale (where the running coupling is of order unity). This gives at least the right ballpark. Anything more precise requires really hard work.

Re particle masses from string theory: This comes from the effective action. Assume you have a massless scalar field in 10D. It obeys some wave equation like Box phi = 0. If your 10D space-time is the product of R^4 x K for some compact six manifold K, you can look for solutions of the Schroedinger type equation Laplacian psi = k psi on K. For such an eigenvalue k make take the original phi to be phi = psi(K) x f(R^4) where I indicated the dependance on the coordinates in parenthesis. Then the above wave equation implies that in R^4, f obeys the Klein Gordon equation for mass squared m^2 = k.

This works not only for scalar fields and thus once you know the K and the spectrum of the Laplacian on it, you know the masses of particles in 4D. The problem to identify the correct K for the real world remains.

15. **D R Lunsford**
June 29, 2005

Tony Smith can calculate particle masses as ratios of volumes of (homogeneous symmetric) spaces. (But, I can’t get him to write down an integral I can do.)

He says the top quark is something like 138 GeV.

-drl

16. **JC**
June 28, 2005

Peter,

I vaguely remember in the 80’s some people making claims of attempting (in principle) to calculate various “particle” masses using string theory. So far I haven’t seen any convincing results of anybody being able to do this successfully. I could never get anybody to explain precisely how to get these “particle” masses. Hopefully they weren’t referring directly to the Regge trajectories stuff from the late 60’s/early 70’s, which don’t seem to be very convincing.

17. **Peter Woit**
June 28, 2005

Hi JC,

I don’t have any references at hand, but I’ve certainly seen papers and talks (e.g. Wilczek’s at the Sidneyfest) where people have shown the results of impressive lattice qauge theory calculations of the masses of these states that come out correct to within the errors expected in the Monte-Carlo calculations. The size of the errors I vaguely recall as being in the range of at most a few percent.

So the fact that you can compute these hadronic masses from first principles makes numerology of them pretty pointless. Of course no one knows how to calculate quark masses from first principles, so there’s lots of room for speculation there. But please don’t do that here….

18. **JC**
June 28, 2005

Peter,

(Slightly offtopic).

Have the lattice gauge researchers been able to calculate the spectrum for any meson and/or baryon families yet, with a precision that makes it possible to compare with the experimental data? If this is possible to do, I would guess it would possibly shut-up most of the people who look at the “numerology” particle masses.

19. **Peter Woit**
June 28, 2005

The difference between the old 178 number and the new 174 number is not statistically significant. It’s very hard to measure this mass accurately since you have to accurately measure the energy contained in a jet. Please spare us further numerology involving quark or lepton masses, since these have all been at least approximately known for a while. Now if someone has a plausible prediction of the Higgs mass or absolute neutrino masses, they should get that on the record since during the next few years these things may finally get measured.

20. **Quantoken**
June 28, 2005

Peter:
Any comment on the fact that a certain John Martin predicted the 174GeV figure correctly one year ago? He made a very good point that the electron mass is a very important fundamental mass unit. That is in line with my assertion in QUITAR that electron mass equals to alpha times the fundamental mass unit, which is about 70MeV.

Quantoken

Latest on Poincare

June 29, 2005
Categories: Uncategorized

The status of Perelman's proof of the Poincare conjecture is still somewhat confusing. For some background see a previous posting and a later follow-up. Last week the ICTP in Trieste issued a press release entitled Poincare Conjecture Solved, which states that Perelman's proof "has been confirmed by an international group of mathematicians whose findings were presented to participants at a conference" at the ICTP. The conference was a summer school, and the press release goes on to claim that "The 60 participants, more than half from the developing world, reaffirmed the approving judgement of the mathematicians."

If you look at the write-ups of the talks, the only relevant thing in print is in the lecture notes of Carlo Sinestrati where he states "The details of the proof are still being checked by the experts in the field; however, the main ideas of the papers are by now widely understood." So, it's not clear who exactly is supposedly now willing to vouch for Perelman's proof, and the comment that the students at the school "reaffirmed" that it is a proof is kind of silly, given the difficulties involved.

There's a month-long summer school going on right now at MSRI in Berkeley, sponsored by the Clay Mathematics Institute. Many relevant materials are available at the web-site of the summer school. As far as I know, none of the experts there is yet quite willing to claim that a complete proof using Perelman's techniques has been written down and checked, although they do seem to be getting close to this point.

Comments

1. **Juan R.**
July 6, 2005

Not Nobel laureate said,

"*Aside from the various theoretical dead ends, a more fundamental problem with string theory is that it operates at an energy scale many orders of magnitude beyond what is measurable today. Thus it's non-testable, hence meta-physical*."

This is one of popular miths of string M-theory.
It, like almost all of popular claims on string M-theory, is simply false.

String theory is perfectly testable. The first version of string theory was tested in the strong force regime and abandoned.

The introduction of 26D was forced because the unobserved tachions predicted by 4D version of the bosonic theory.

The next version (+ fermions) were also (un-)tested, for example so many times was claimed the inminent discover of supersimmetry and the first verification of supersimmetric string theory by crackpots!

In cosmological issues, string M-theory has been also tested. A complete failure in the words of Krauss, possibly the poor theory of history of physics according to Woit (discrepancy with experimental data is around 50 orders of magnitude).

The attemtp to explain inflaction and dark matter from string theory also has failed. Regarding cosmological brane theory, the recognized specialist Linde showed that "popular" (i.e. string theory is marvellous, string theory solves the most difficult open problems, etc. etc.) papers by string theorists were completely wrong and predicted the contrary to observed "inflationary" data.

Moreover, it is often ignored that before to explain NEW physics, string theory may explain known physics. In this simple, elementary, step, string M theory does not explain nothing already known. In fact, even the derivation of GR from string theory is based in heuristic reasoning and ad hoc hyphotesis.

It can be shown that string M-theory is incompatible with known experimental data.
The incompatibility with certain statistical mechanics data has forced to some string theorists to develop the so-called non critical approach that violate basic principles of usual "critical" string theory.

The incompatibility with thermal phenomena has forced to abandon all of standard Hilbert-Fock quantization of branes in favor of the new doubled space approach (so-called tilde operators) and the new thermal states of TFD-Dp-brane theory.

In the past, the "derivation" (not in the rigorous sense of term) of GR from 10D superstring action was one of main popular claims of string community. Curiously, now physicists and astronomers maintain doubts in the validity of GR at cosmological scales and well-known modifications like MOND, AQUAL, etc. are being studied and tested. Since that string theory claims just small-scale modifications for GR, one may see that also here string theory is a failure here. For example, string theory is incompatible with standard TF law.

From unitary Schwartz string action one cannot explain experimental data which IS explained by Lindblad semigroups axiomatic theory.
Etc, etc, etc.

All i am saying is standard, it is not speculation. For example, TF law is the basis for one of standard methods in gauging distance in spiral galaxies, Fourier thecniques in L-space (string theory and standard (e.g. Weinberg manual) QFT both work only with H-space) received Nobel Prize for chemistry in 1991, etc. specialists in quantum mechanics have said in many occasions that string theory is not fundamental (see me previous post on Witten for a quote extracted from the conference Quantum Future. It is well-known that true specialists (i.e. people that has really advanced the field) in quantum theory durely critiqued last Witten claims on the generalization of quantum mechanics from M-theory. In fath, it is aknowledged by many recognized specialists in the field that Witten (with no contribution to the field) misunderstands quantum mechanics.

The last year, a Nobel laureate (Freeman J. Dyson) did a similar criticism to Brian Greene in his review of book the Fabric of Cosmos. Dyson reacts atonished to Greene string interpretation of quantum mechanics saying "He rejects [standard view] without any serious discussion". Dyson ignores that IS precisely string theory literature: no serious discussion, only speculation, conjetures, bad math, superfitial insight, etc.

This is the reason why i claim that string theory is wrong like a TOE. It is amazing that questions that i said some years ago (then i was ridiculized) begin to be supported by string theorists now.

In fact, i said in the past that string theory was rather standard (simple) and people ridiculized. In fact, one guy contacted with Lubos Motl and this "hiring" to me.

Fortunately, after of his Nobel Prize, David Gross has said a phrase very similar to i said two years ago: that string theory is not revolutionary.

The guy said to Lubos Motl 🙂

****
If all i am saying about failure of string theory to explain known data is standard, why do string theorists ignore it?

Because are arrogant people and think that understand things when have only a superfitial knowledge of things.

For example some of them are very excited with TFD generalization of brane theory. TFD theory was know decades ago. Now we are working in more geernla stuff TFD II, NESOM-TFD, etc.

Other example, the most recent and radical modification of NC string theory posted in ArXiv by Nanopoulos uses mathematical tools in projected dynamics mathematics developed by Brushels School in the 60s but abandoned by the own School in favor of the recent LPS formalism in Gelfand triplets in the 90s!!!

It is more, not only again string ideas are outdated, even copying the interesting work done by others (e.g. Prigogine), they copy incorrectly!!

For example, the equation (10) of arXiv:hep-th/9403133 is simply wrong for anyone with a minimum insight in generalizations of quantum mechanics (e.e. Solvays conferences, etc.)
Note, i contacted with Nanopoulos for explaining it but he ignored to me, now i does not explain to him that almost of next sections of that and other papers are misleading. It is waste of time contact with string theorists.

My criticism to that preprint is correct, in fact it is supported by one of members of the School, prof. Gonzalo Ordo?ez from Ilya Prigogine institute on Texas that i contacted for verifying.

2. **D R Lunsford**
July 5, 2005

Matti said:

Batakis introduces electroweak structure more or less by hand without noticing that CP_2 spinor connection possesses naturally electroweak gauge group as holonomy group

I think this latter is Finkelstein and Jauch's (and Speiser and Schiminovich's) quaternion quantum mechanics. This theory also had a rationale for the Higgs mechanism other than expediency.

-drl

3. **Not a Nobel Laureate**
July 4, 2005

Aside from the various theoretical dead ends, a more fundamental problem with string theory is that it operates at an energy scale many orders of magnitude beyond what is measurable today. Thus it's non-testable, hence meta-physical. Having said that, the same can be said about LQG, spin foams and any other theories whose predictions, if they even make any, lie outside the energies attainable at the Tevatron and the LHC or perhaps astrophysically observable.

"Pure reason" has never been a productive form of scientific enquiry.

4. **Matti Pitkanen**
July 3, 2005

Tony Smith mentioned in his posting the paper of Batakis about H=M^4×CP_2 Kaluza-Klein theory. We had a discussion about the paper with Tony for a couple of months ago.

Batakis introduces electroweak structure more or less by hand without noticing that CP_2 spinor connection possesses naturally electroweak gauge group as holonomy group. The problems of KK scenario become obvious when one looks for the spectrum of Dirac operator in H.

a) The two chiralities of H-spinors allow an identification as quark and lepton type spinors when one couples leptons/quarks to n=1/n=3 multiple of Kahler gauge potential of CP_2. The holonomy group U(2)_ew has a natural identification as electroweak gauge group. Separate conservation of lepton and baryon numbers is predicted.

b) The problems are that only right-handed covariantly constant neutrino is massless whereas other states have mass scale defined by CP_2 size. Also the correlation between color and ew quantum numbers for the spinor modes is wrong: only right-handed neutrino corresponds to color singlet. Thus Kaluza-Klein type theory as a limit of something more general is out of question.

Batakis does not notice that CP_2 already unifies color and electroweak symmetries. If one considers space-time as a 4-surface in H and induces spinor structure to the space-time surface (bundle induction is mentioned in the 20 first pages of any text book about bundles and means in recent case projecting of the gamma matrices of H to space-time surface), one obtains electroweak gauge field as classical gauge fields inheriting their dynamics from the dynamics of space-time as 4-surface. Color gauge potentials can be identified as projections of Killing vectors of color isometries.

From this it is a long way to a generalization of string model predicting correctly the massless sector of the theory and mass spectrum of elementary particles and hadrons. A profound generalization of conformal symmetries is needed and emerges naturally when one formulates quantum theory as a theory of free classical spinor fields in the "world of classical worlds" consisting of 3-surfaces in H and endowed with Kähler geometry. The spectrum of Dirac operator and mass calculations see the five chapters in the second part of p-Adic TGD.

Matti Pitkanen

5. **Curious**
July 3, 2005

Thank-you, Peter. I think I've got it.

6. **Peter Woit**
July 3, 2005

Hi Curious,

I think I mostly agree with you, but a few comments.

The string theory community IS wasting time. They are sitting around waiting for someone else to come up with a new idea. Sure, if someone comes up with a wonderful, compelling new idea with lots of evidence for it, they'll take it up. But as you say, they've set the bar very high. The present situation allows them to get jobs, awards, grants, etc, etc, based on worthless work on string theory, but not for work on other ideas (unless these ideas are quickly successful). I still think that if people start publicly acknowledging that string theory has failed, this will pull the plug on this unhealthy situation. If people can't get a grant proposal to do string theory funded, but have to try and come up with something else, this would have a huge positive effect.

The leaders of the string theory community are well aware that if they start publicly talking about how badly things are going, they're quickly going to get into this kind of trouble. I think that's why many of them will privately agree that string theory is in bad shape, but say very different things publicly. I hope to have some role in not letting them get away with this.

I agree with your three ideas of things to do, and am trying to do them. I hope that reading this weblog will help students think independently. A lot of what I have to say is aimed at them. I also do try and identify and point to new work that might be promising. Unfortunately it's rather discouraging how little of this there is from my point of view. I personally think that a deeper mathematical understanding of the standard model is what is needed for progress, but virtually no one is working on this. The interaction between particle theory and mathematics has narrowed down in recent years to virtually just topological string theory and enumerative problems involving Calabi-Yau 3-folds. This work is interesting, but it is mathematically very narrow.

7. **Tony Smith**
July 3, 2005

Curious said "… I have had several occasions to discuss your blog with our (U Chicago) string theorists — and they read it and they agree with it, and then they ignore it. … The best alternative ideas around are not good enough to recruit string theorists. That is not the result of their obstinacy. These alternative ideas are simply not good enough, period. …".

I disagree, and here is a concrete example of a model that unifies gravity and the standard model and is (afaik) ignored by the conventional string theory community.

N. A. Batakis, in Class. Quantum Grav. 3 (1986) L99-L105, wrote a paper entitled Extra gauge field structure uncovered in the Kaluza-Klein framework. In it Batakis said: "… In a standard Kaluza-Klein framework, M4 x CP2 allows the classical unified description of an SU(3) gauge field with gravity. … the construction of an additional SU(2) x U(1) gauge field structure is uncovered. The construction involves a properly modified 'gravitoweak connection' and supplies a mechanism analogous but not redundant to the Kaluza-Klein ansatz … As a result, M4 x CP2 could conceivably accommodate the classical limit of a fully unified theory for the fundamental interactions and matter fields. …".

If Curious is correct, then I would think that his U. Chicago string theorists would see the Batakis paper as a "spark to inflame" them into developing and completing the work of Batakis.

If, on the other hand, Peter is correct, then those string theorists would feel threatened by a competing non-string (not even LQG) theory and dismiss it without careful evaluation, perhaps even attacking anyone who is seriously interested in it.

I am curious to see what reaction the string theory colleagues of Curious might have to the Batakis paper.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

8. **Curious**
July 2, 2005

Peter,

I do not want to distract you with endless arguments going to and fro etc
inasmuch as I think we are on the same side of the issue. Thereof I'll comment on only one point of
your answer; it is a very important point — to you. The question is how to stop the wastage of
time on ST; that's what we both chiefly want.

You say that you deeply care for the field and you cannot just sit back and see how it disintegrates and how young people become demoralized. I know you do feel the pain. So do I. But critisizing ST in the way you do it – and you do a jolly good job of it – it just does not work. You are convincing the wrong people; those that do not waste time anyway.
I have had several
occasions to discuss your blog with our (U Chicago) string theorists — and they read it and they agree with it, and then they ignore it. It is too late in the game for your arguments to work on them.
One cannot turn a tide on a completely negative message. I do not say that YOU have to be positive about something. But someone better be; otherwise ST is unstoppable. You are trying to combat a belief system using rational arguments against it; that is as futile as Sean's struggle against creationists. Painful as it is to watch the wastage of effort by first-class minds, the only way to stop it is to be creative; that's where we all collectively failed.The best alternative ideas around are not good enough to recruit string theorists. That is not
the result of their obstinacy. These alternative
ideas are simply not good enough, period.

I do not believe in great men visions either but
even more am I sceptical of
the "fertile soil" model of which you and Smolin equally partake. The community of German physicists from which Heisenberg emerged was not at all like a healthy community. It was more like a viper's nest,
with duelling schools, less then enthusiastic welcome of radical ideas, and a community riddled by mutual distrust, anti-semitism, and typically German professorial anti-everything. The community that you have in mind did exist but it was initially marginal and had
not more than 20-30 people at any time. It is always possible to
create such a mini-community of like-minded people inside a larger hostile community.
The power of Heisenberg was not that he had roots
in some healthy worldwide community; initially, there was tremendous opposition to QM. It was the truth of his ideas; it is the truth that won the people against their worst selves. ST does not strangle new thought; this thought is simply not there.

We can only do three things to stop ST:
(i) generate this new all-winning idea by ourselves (ii) foster independent thinking in our students and
(iii) spot this new idea and champion it when it emerges. I cannot think of anything else that might work towards the goal.

You look at the ST community and see people wasting their time on a dud. I see it as a barrier this new idea has to jump over;

it is a mighty high barrier and it works as an excellent
obstacle w/o which the race is meaningless and a perfect deterrent to bad ideas. The ST community is also a potential pool of people that will be able to develop and complete this new idea when its time finally comes. This community is not wasting time,
it is waiting for a spark to inflame.

Perhaps, all of the above is not really news to you and you have heard such arguments many a time. By no means I suggest that you should stop what you are doing; you would not listen anyway. All that I am asking from you is to think once in a while: Are the means that I chose helping to bring about the goal that I chose? If the means are not helpful, another tactics is needed.

I enjoy reading your honest, principled, and intellectually challenging blog but it mainly
serves to vent the frustration; it does not go to the root of
the problem and it does not suggest a winning strategy. Nobody ever became healthy by
a realization of one's sickness, using your analogy. We need a diagnosis, a treatment plan and medicine — if not a cure. You offer a death sentence to a terminally ill patient. Is that enough?

9. **Tony Smith**
July 2, 2005

Curious asked "… Could it be that people rage against the ST because the dominance of this admittedly failed programme painfully reminds them their own failure to produce a great new idea? …".

I don't think so. I think that the rage is directed at the massive publicity campaign to present conventional superstring theory as the only possible program for unification of gravity and the standard model.

For example, the 1 July 2005 issue of Science listed the 25 most important questions in Science today. About the number 5 question, Can the Laws of Physics Be Unified?, Charles Seife wrote:
"… Gravity clashes with quantum theory so badly that nobody has come up with a convincing way to build a single theory that includes all the particles, the strong and electroweak forces, and gravity all in one big bundle. But physicists do have some leads. Perhaps the most promising is superstring theory. Superstring theory has a large following because it provides a way to unify everything into one large theory with a single symmetry?
SO(32) for one branch of superstring theory, for example
?but it requires a universe with 10 or 11 dimensions, scads of undetected particles, and a lot of intellectual baggage that might never be verifiable. It may be that there are dozens of unified theories, only one of which is correct, but scientists may never have the means to determine which. Or it may be that the struggle to unify all the forces and particles is a fool?s quest. …".

It seems to me that Seife and Science are saying that conventional superstring theory, their "most promising" approach, may or may not produce a unique unified theory, but if it fails to do so, then "the struggle to unify all the forces and particles is a fool's quest".

In other words, anyone who pursues any approach other than conventional superstring theory is characterized as a "fool".

That attitude, which is prevalent not only in the scientific media such as Science, but also in the popular media and in the culture of conventional superstring theorists themselves, is where my rage is directed.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Just for the record, the number 1 question listed by Science was What Is the Universe Made Of?, as to which Charles Seife wrote:
"… Ordinary matter and exotic, unknown particles together make up only about 30% of the stuff in the universe; the rest is this mysterious antigravity force known as dark energy … at the moment, the nature of dark energy is arguably the murkiest question in physics?and the one that, when answered, may shed the most light. …".
Thankfully, Seife's article did not present conventional superstring theory as the "most promising" answer to that question, but I have little doubt that members of the conventional superstring community will declare that their theory is the "most promising" approach to an answer.

10. **Peter Woit**
July 2, 2005

Hi Curious,

I've got no idea by what you mean in claiming that Hilbert's research program dominates mathematics. Hilbert was a very broad mathematician, and did a wide variety of things. Some of the areas he worked in that were most identifiable with him are relatively unpopular now (e.g. the formalist approach to foundations of mathematics). Mathematicians work on a wide variety of problems, from a variety of points of view, and they've been making real progress (e.g. the Wiles proof of Fermat, the possible proof of Poincare using ideas of Hamilton and Perelman). A lot of what they do is unproductive, but the field is healthy. Theoretical physicists could learn a lot from this, and not just possibly useful mathematical ideas.

I don't think I'm criticizing string theory in order to find out what's wrong with it. In the end, that's very simple: it abandons the successful core mathematical concepts at the foundation of the standard model and adopts an extremely speculative and not very promising alternative, one that has now conclusively failed. From the beginning of the fad in 1984, it was clear to me and to many other people that string theory wasn't an obviously promising idea. It predicted nothing and there were clear reasons for this.

In 1984 it wasn't unreasonable to work on string theory. The theory was not very well understood and one could hope that further work on it would show a way around its problems. In 2005 the situation is very different. It is completely unreasonable to now believe that these problems can be overcome. Current attempts to create a unified theory out of string theory are both mind-boggling ugly and utter failures.

I won't disagree with you or anyone else who tries to argue that I should devote more of my time to working on positive alternatives and less to criticizing the current situation. Maybe this is right. But I find what has happened to the field I care deeply about extremely disturbing. Each year it has gone from bad to worse, and is increasingly dominated by pseudo-scientific garbage, heavily promoted to the public. I don't find myself able to ignore this.

I'm not a big fan of the "great man" view of science, the idea that progress depends on one brilliant person. More commonly, scientific progress comes from a community of people working on promising ideas. When Heisenberg came up with quantum mechanics he didn't do this all by himself. He was part of a healthy community in which many people were trying many different things. The theoretical physics community is not healthy. It is sick and has been getting sicker. When you're sick, the first thing you need to do is acknowledge this, then figure out what you need to do to get better.

11. **Curious**
July 2, 2005

Peter,

Thank-you for your thoughtful remarks. I've been reading
this blog for some time and I still cannot determine what exactly is your problem with the ST:
you give so many different reasons and none of these various reasons look entirely
convincing, hence your need to repeat yourself time and time again, in order to convince yourself and others. I have a hunch that you have started this blog to find out precisely that — what is the problem with the ST as a programme, at the deepest level — something that you intuitively feel — and you write this blog for yourself — not for the benefit of the others. You suggest various answers to this one question, but these answers do not fully satisfy you. And so is the case with your readers, many of whom are string theorists. They read your blog, they agree, and then keep on doing what they are doing. If the answer were right, they would stop doing ST and start doing something else.

I do not know what that right answer is. You have to find it out for yourself and, please, keep on trying; it is important.

One thing I am sure of is that the problem with the ST is not that it is a fad or that it dominates HEP or that it is unprecedented in physics or mathematics or that it is unpredictive or plain wrong. The problem is US.

Hilbert's research program had dominated and still
dominates mathematics to a much greater extent than
string theory dominates high energy physics. It was viewed as a passing fad by many leading
mathematicians at the turn of the 20th century. It was
enormously sucessful, several excesses aside. Its
success is not and was not self-evident, as it does not logically follow from any rational argument; it is one man's vision. By all criteria it was a fad. It could've been a misguided fad as well. Fad or no
fad, tangible results started to stream out immediately
and still do. There was no time lag between the
promise of a wonderland and the result.

The news about the string theory is not that a certain fad dominates a certain field but how unproductive that fad turned out to be. Perhaps, one's man intuition
is not enough for physics. Or maybe we listened to the wrong man.

I believe that it is entirely appropriate for a fad to dominate the field, and it happended repeatedly, in both physics and mathematics. But most of these previous fads were productive and that's why we do not call these fads "fads" anymore. For lack of better ideas and guiding experimental results,
ST manages to dominate the field without the benefit of being productive.

And yet still, if every string theorist
will read this blog and agree that ST is not even wrong that would not change the situation one bit, because it is unclear what's right. The dominance of ST is not the result of groupthink or worldwide conspirancy as some people believe. It is the looming, annoying testimony to
OUR failure to come up with better ideas
and decisive experiments. The problem is US not THEM. Never before physicists were unable to come up with a good idea for such a long time — and that is certainly not the fault of the ST community.

Could it be that people rage against the ST because the dominance of this admittedly failed programme painfully reminds them their own failure to produce a great new idea? My feeling is that at the bottom, that's the right answer, and it is very sad and disconcerting.

12. **Peter Woit**
July 2, 2005

While physics and math, like all human endeavor, have always experienced a certain number of misguided fads, I think the superstring theory story is without parallel. In physics the closest analog I can think of is the craze for S-matrix theory during the 60s, but that only lasted a decade or so.

No one research program has ever dominated mathematics the way string theory dominates particle theory. The excessively formal style of Bourbaki is more an issue of bad pedagogy and exposition than bad research. While the members of Bourbaki were writing the very formal Bourbaki textbooks, they also were doing a wide range of different wonderful mathematics research. Take a look at the Bourbaki seminar writeups during the fifties and sixties. There's all sorts of exciting new mathematics there, being written up often in an accessible way, with very little evidence of bad effects of too much formalism.

13. **Walt Pohl**
July 2, 2005

Curious: Your comments about axiomization are not true. Axiomization is not as big a deal now as it was in the early twentieth century mainly because it was so successful: the current foundations are adequate for most purposes. Foundational matters still arise, though. For example, a long-open problem in abelian group theory (the Whitehead problem) has turned out to be independent of the usual axioms of set theory (ZFC).

Foundational techniques are beginning to be used to settle purely mathematical questions. There are some results in analysis whose only known proof requires "nonstandard analysis". Model theory was used to prove the Mordell conjecture in the function field case.

14. **Kea**
July 1, 2005

Curious: "…axiomatization of mathematics peaked in
popularity in the first half of the century and
then faded out."

Actually, this isn't true. The field simply evolved into something qualitatively new. Foundations of Mathematics is a big subject today. And as much as String theory bugs me, I have a terrible feeling that history will look at Strings in a similar light. A lot of the maths of M-theory has to be important to physics, as Witten says. The fact that what wins out might not have anything to do with M-theory from certain points of view doesn't alter the fact that detailed arguments connecting the old ideas to the new will inevitably turn up. And all history will see is another turning tide in the ocean – not the swirl of water washing over into the tiny lagoon at the end of the beach.

15. **Curious**
July 1, 2005

Peter then Tony asks an interesting question:
has it ever been an analog of superstring theory in mathematics? Perhaps, they mean:
was there a school of thought that captured a lot of attention, hype and effort and produced very or relatively little after many years of persistent study? The answer is

surely, yes,
though we may disagree on details. Say,
axiomatization of mathematics peaked in
popularity in the first half of the century and
then faded out. It is crushingly boring and
not a very good way to come with interesting
math. Another program, N. Bourbaki used to be as influential as Witten, but few people still care. One can come with more examples.

Observe also that in mathematics the logic
proof stands on its own; there is no emprical
input. By contrast no physical theory is self-
consistent or logically complete. We accept
these theories not because these theories are
rigorous (they may be actually highly dubious)
but because they work. Such a situation is
acceptable for any physical theories because
their difficulties are delegated to a yet
unknown more general theory. However,
such a situation is not acceptable for a TOE;
it should be as consistent as a mathematical
proof. I think that the problem with the string
theory is exactly that: it aspires to be what
no physical theory had ever been: a domain
of pure logic and mathematics, with acceptance criteria peculiar to these fields.
It cannot live to that self-imposed standard.
There could be in this sence no analog of the string
theory in mathematics: the complete analog would
be a mathematical theory that needs a
physical experiment to decide the truth of
the theorem. So far, even computer experiments are not exactly welcome.

16. **Peter Woit**
June 30, 2005

Hi Tony,

The huge groups and large sums of money necessary to do some kinds of experiments have certainly had a bureaucratic effect on some areas of experimental physics, but this doesn't really explain what has been going on in particle theory, where large groups aren't needed.

Particle theory is much, much, much more faddish than mathematics, and attitudes are very different. To oversimplify, in mathematics someone who quickly publishes a not really great paper on what seems to be the hottest topic would generally be thought of as someone shallow, unwilling to take the time to become a real expert in something and do some serious work. In particle theory, someone who doesn't jump on the latest fad is often thought of as an intellectual lightweight who isn't smart enough to quickly absorb something new and work on it.

The roots of this attitude in particle theory come from the days when experiments were producing unexpected new results. The most ambitious people would then jump into trying to explain them. The problem now is that there haven't been any especially new unexpected experimental results in particle physics for a long time, so this kind of attitude has become dysfunctional. Mathematicians have never had experiments to feed them new clues as to what to think about, so they have a tradition of people spreading out and digging in for much longer term research projects.

17. **Tony Smith**
June 30, 2005

Peter, you say "… The way the particle theory community has refused to acknowledge …[that]… over the last 20 years a huge amount of evidence has accumulated that, as an idea for unification … Superstring theory … doesn't work … is something that has no analog I can think of in mathematics.

What is the relevant difference between the math and physics communities?

First, it seems to me that superstring theory is not the only example in which the physics community refuses to recognize obvious failure.
For example, huge amounts of money and manpower have been spent over several decades on magnetic confinement fusion machines. At first, it was worth exploring (like superstrings in 1984), but now, decades later, even though it is clear to any reasonable person that magnetic confinement fusion is not going to be a significant source of energy, the ITER project is getting under way.
For another example, it was clear to any reasonable scientist that the International Space Station would not produce results that would come close to justifying its costs, but it was built anyway.

Those projects (superstring theory, ITER, ISS) all have something in common: they are in fact promoted by growth-seeking bureaucracies.

It seems to me that a distinguishing factor between physics and math is that
the physics community has evolved into a group of bureaucracies that use committee/consensus to enforce uniformity of thought because they feel that any independent thought threatens the fundamental bureaucratic goal of growth,
while
the math community is a lot of individuals, with uniformity of thought being enforced primarily by relatively objective standards of logical proof.

In my opinion, it is no accident that the last big success in theoretical particle physics was the Standard Model of the 1970s, which roughly coincided in time with the rise of large collaborative physics institutions like Fermilab and SLAC.
It also may be no accident that the last big experimental particle physics success was Fermilab's taking of data about the T-quark, and that the most significant data was taken over a decade ago, during its first run.

On the other hand, the math community has continued to make substantial advances, such as Wiles's proof of Fermat and Perelman's possible proof of 3-dim Poincare. Both of those advances were made by individuals, not large collaborations. It may be that collaborations are necessary to validate their work, but the initial work was by individual initiative.

Maybe the problem with physics is its current sociological structure, something that Burton Richter (former SLAC director) tried to point out in his paper at http://xxx.lanl.gov/abs/hep-ex/0001012 in which he said "… In the 500-strong collaborations of today, we … have a bureaucratic overlay to the science with committees that decide on … speakers, paper publications, etc. The participating scientists are imprisoned by golden bars of consensus …”.

Perhaps particle physics might advance more if it loosens the golden bars of consensus, and allows 1000 flowers to bloom (and perhaps lets committees be evaluators rather than enforcers of consensus thought).
It seems to me that astrophysics has come closer to following that path, as it has been successful by taking interesting data such as WMAP and not attempting to enforce any consensus interpretation of the data (being tolerant of interpretations of dark energy and dark matter ranging from conventional cosmological constant dark energy to MOND and many others).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

18. **Peter Woit**
June 30, 2005

Perelman is kind of a special case since the problem is that he hasn’t actually written down a proof, just an outline. Turning his outline into a real proof is a lot of work, especially since he is using some techniques few people are familiar with. The classification of finite groups proof is very long, but at least supposedly all the details are there.

I know people have been working on shorter versions of the four-color and finite group classification theorems, not sure what the status is. For another related story, there’s the claimed proof by Hales of the Kepler conjecture. This one involves both issues of computer calculations and such a complex argument that there have been problems getting it refereed.

19. **DMS**
June 30, 2005

The classification theorem for finite simple groups is likely to remain impossible for any one individual to verify. Perelman’s proof should be ‘easier’ since it requires verification of new mathematical tools.

Also, is there a proof of the Four-Colour theorem that does not need computers??

Seems like proof in the strict traditional sense is not always possible.

What do you think?

20. **Peter Woit**
June 30, 2005

Quantoken,

First of all, stop submitting large numbers of comments, this is not a forum set up for you to go on and on. I’ve deleted all except the first one.

About your points: there’s never any absolute logical certainty of the validity of a proof of the complexity of the one Perelman has outlined. But if several people who really have a lot of experience dealing with the techniques involved put in the time necessary to go over the proof with a skeptical eye and announce that they are convinced by it, it’s extremely likely that the proof is valid. But you don’t have to take their word for it, that’s the whole point of the culture of mathematics. The mathematics community historically won’t recognize a proof until it is written down in sufficient detail to allow anyone who wants to to check it.

How well a proof is checked is a function of how much people care about the result. There are lots of proofs in the literature that are probably invalid if looked at carefully, but no one does this because hardly anyone cares about the result. For things like the Wiles proof of Fermat and this proof, the result is so important that you can be sure the proofs will be carefully checked. The whole process is also to some degree self-correcting. If a mistaken argument gets into the literature, and if it is an important one, other people will sooner or later try and use it to do other things, and if it is wrong it will give them wrong results which will ultimately be noticed.

21. **Peter Woit**
June 30, 2005

Hi Tony,

The smooth 4d Poincare conjecture is still open, no significant progress that I know of.

I think the way mathematics deals with conjectures like 3d Poincare and the way physics has dealt with superstring theory are completely different. Over the years lots of good evidence accumulated for 3d Poincare: all attempts to construct spaces that violated it failed, it was shown to be equivalent to other conjectures about topology for which there was significant evidence, and, as Morgan points out, it fit into a larger web of conjectures for which there was a lot of evidence.

So, the general assumption has been that the conjecture is true, but that new mathematical ideas were needed to prove it. It seems that, together with Hamilton’s work, Perelman has come up with these ideas. But still, until there is a complete proof written down and experts have gone over it and vetted it (under normal circumstances this would happen in a formal refereeing process before publication), mathematicians are not going to be comfortable saying there is a proof. From experience they know that a plausible sounding outline of a proof and a proof with the details worked out are two different things. Until you work out the details you can’t be sure a subtle problem has been missed (e.g. for instance recall the problem with the initial proof of Fermat by Wiles).

Superstring theory in 1984 was just a somewhat wild conjecture with little evidence to back it up. At the time you could argue it was an idea worth working on, but over the last 20 years a huge amount of evidence has accumulated that, as an idea for unification it doesn’t work. The way the particle theory community has refused to acknowledge this is something that has no analog I can think of in mathematics.

22. **Quantoken**
June 30, 2005

Interesting.

“and the press release goes on to claim that “The 60 participants, more than half from the developing world, **reaffirmed** the approving judgement of the mathematicians.”…”

Peter raised a good point on how these 60 guys supposed to **“reaffirm”** the judgements of the mathematicians? By what? Maybe by their faith or confidence in the authority and credibility represented by the hats these mathematicians wear? If they have not spent 5 years trying to rigorously check every single step of the logic in Perelman’s proof, they probably have run it through a spell checker to make sure it contains no English grammar error. Maybe that’s what they mean “reaffirm”.

Suppose Perelman is already a well known and well established guy, He is the No.1 guy in math, and he made this unusual claim that he proved the Poincare Conjesture just today. And the proof is 1000 pages long and extremely hard to read and go through? How does the No.2 guy in math supposed to believe in this No.1 guy? He has two choices:

1.He believes it because he trusts the No.1 guy and because of the authority power that the No.1 guy is smarter than him and whatever No.1 says must be true, and he is not going to waste time questioning the No.1 guy.

2.He question it until he can convince himself that the logic really works. So he need to spend time, tremendous amount of time, maybe several years, 5, 10 years? Rigurously going through every step. After exhaustive search, he could not find a single logical fraud that could render the original proof invalid. Thus the No.2 guy claim to the world that he has gone through the No.1 guy's proof, and he discovered nothing wrong. Thus the proof is valid.

But certainly there is always the possibility that despite of the honest effort No.2 put in, he might still have missed something. Or even the remote possibility that No.2 gets a little bit lazy after spend one year going through 2/3 of the paper. How are we supposed to know that is the case or not. How are we even supposed to know that No.1 wasn't being dishonest and he took the first approach since he was lazy.

So that is the question the next guy in line, No.3, has to ask and answer. Does he believe in No.1 and No.2? And just like No.1, he has two choices, either he believes in No.1 and No.2 by faith and confidence, and save himself some time. Or the No.3 has to do the hard work and going through the whole rigorously thing himself. Being less smart than No.1 and No.2, it would be even harder and take him more time if he wishes to go through the whole thing himself. So there is an even better chance No.3 gets lazy and become dishonest, and just make the "re-affirmation" claim before he finish reading 1/2 of the paper.

And what about the No. N guy, after all the previous N-1 guys claimed the proof was valid? Would he choose to nod his head with the rest of the gang. Or would he want to become the first child who point out the fact that the emperor could actually be naked, and all previous N-2 guys are all just blindly follow their trust in the judgement of the "smart guys" before them?

That's an interesting philosophy question. And one that really worth thinking about, in the light that 60 physicist all collectively choose the first approach, i.e., "re-affirm" purely based on their faith in their math colleagues, instead of actually spend time proof reading the actual proof. And I bet 99.9% of mathematicians, 99.999% of the physicists community, and 99.999999% of the human population, would all very likely put their faith in the top 3 math guys and "re-affirm", instead of wasting their own time in trying to figure out the thing themselves. And I guess Peter, me myself, all readers of this blog, are all probably belong to this majority group like the 60 physicists.

Now are we really so sure the emperor is not naked?

There are two kinds of believe systems, One, faith based, you believe therefore you believe. That we call religion. Another, you believe based on evidences and based on rigorous logic and reasoning, that's science.

Unfortunately when it comes to extremely difficult math or physics problems, virtually in-accessible to the majority of people even in the field, evidence based religions system could be easily replaced by faith based believe system.

What happens a couple hundred years down the road. When the most difficult math problems become so difficult that no human being can resolve them and we have to just trust the proof spitted out of the printer from a super computer. Will we then become a civilization with a religion that worship machines? And science cease to exist? Will that happen eventually?

Quantoken

23. **Tony Smith**
June 29, 2005

Peter, you cited in an earlier post Morgan's paper at http://www.ams.org/bull/2005-42-01/S0273-0979-04-01045-6/S0273-0979-04-01045-6.pdf
In that paper, Morgan said: "… After Thurston?s work, notwithstanding the fact that it has no direct bearing on the Poincare Conjecture, a consensus developed that the Poincare Conjecture (and the Geometrization Conjecture) were true. Paradoxically, subsuming the Poincare Conjecture into a broader conjecture and then giving evidence, independent from the Poincare Conjecture, for the broader conjecture led to a firmer belief in the Poincare Conjecture. …".
That is a fascinating commentary on the process of acceptance of conjectures within the mathematics community.

Can you comment on comparing that process of acceptance of the 3-dim Poincare Conjecture by the math community with the process of acceptance of superstring theory as the only possible Theory of Everything by the physics community?

Also, what is the status of the smooth Poincare conjecture in 4 dimensions?
I may be out of touch, but if I recall correctly as of a few years ago it was still unsolved, and Donaldson and Kronheimer say in their book The Geometry of Four-Manifolds (Oxford 1990): "… Smale's h-cobordism theorem: if X and Y are h-cobordant then they are diffeomorphic … breaks down in four dimensions … ". Also, Milnor's paper at http://www.math.sunysb.edu/~jack/PREPRINTS/poi-04a.pdf (updated June 2004) says "… In particular, if M4 is a homotopy sphere, then … M4 is homeomorphic to S4 . It should be noted that the piecewise linear or differentiable theories in dimension 4 are much more difficult. It is not known whether every smooth homotopy 4-sphere is diffeomorphic to S4 … As one indication of the complications, Freedman showed, using Donaldson's work, that R4 admits uncountably many inequivalent differentiable structures. …".
Has the smooth Poincare conjecture in 4 dimensions been solved, or has significant progress been made?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

Renaissance Technologies

July 1, 2005
Categories: Uncategorized

Via [Steve Hsu](), the news from the Wall Street Journal is that Renaissance Technologies is about to launch a new hedge fund that could end up managing $100 billion, 10% of the total managed by all hedge funds today.

Renaissance Technologies is run by mathematician Jim Simons, perhaps the most successful hedge fund manager ever. His current Medallion hedge fund manages $5 billion, and his personal net worth is estimated to be about $2.5 billion. Renaissance employs many Ph.D. mathematicians and other scientists, but is quite secretive about their investment strategies. If you ask people who are working there about what they are doing, you get answers like "I could tell you, but then I'd have to kill you."

Simons was an undergraduate at MIT, got his Ph.D. from Berkeley in 1961, then was a junior faculty member at MIT and Harvard. He worked for a while at the Institute for Defense Analyses in Princeton, but was fired over criticism of the Vietnam War. He then went to SUNY Stony Brook, where as chair he built up a great math department, one especially strong in geometry. His work with Chern in the early seventies led to an extension of Chern-Weil theory involving "Chern-Simons forms", which have been of great importance in physics.

During the seventies he started trading currencies and commodities with his own money, leaving Stony Brook in 1978 to form his own investment fund. Over the years he has been generous to the mathematics community, supporting MSRI, the IAS, the Stony Brook and MIT math departments, and many conferences and workshops. I have no idea what his long term plans are for his current billions or the additional ones his new hedge fund may generate, but if even a fraction of them end up financing pure math research, this could have a very dramatic effect.

Comments

1. **M**
   July 14, 2005

   Hi Anonymous,

   Thanks for your well thought out comment regarding free markets and central planning. Before commenting on some of your specific points, some clarification is appropriate. In my initial comment about free markets vs. central planning, in an effort to try to avoid the kind of misunderstanding that seems to have occurred, I mentioned early on that

   First, I agree that overall, free markets are much preferable to centrally planned economies, and probably for similar reasons to yours.

   Given that, you will hopefully agree that I will give an emphatic "No" to answer your question,

   So, just to make sure that I got you right: states, i.e. the entities which use theft and slavery to start and run wars, are inherently good; companies, which offer goods on a free market for voluntary exchange, are inherently bad. Did I get that quite right?

   I think my agreement with you on various other points in the past should support my disagreement of your assessment of my overall view. You may have gotten your impression from examples I gave to support the idea that there are times when "central planning" can have overall positive results (sometimes unintentionally!). Generally I try to avoid holding more extreme views since they are usually the product of ideological thinking rather than well thought-out arguments, and to cast free markets vs. central planning is "always good" vs. "always bad" seems somewhat extreme…

   Overall, I think there *can* be a role for regulation or outside intervention in at least three general conditions:

   1. Abusive practices create an unfair advantage for an individual or company. Examples are laws against bribery or racketeering, some regulation of insider trading, and regulations against some kinds of monopolistic practices (e.g., blatantly predatory pricing).

   2. Laws and regulations to prevent indentured servitude or abusive working conditions.

   3. Seeding or nurturing of basic science research, or even new basic technologies that are speculative or have very start up costs without any positive return on investment in an agreeable time frame. This is probably the most controversial area, and I will try to defend it below.

   In a truly free market, government would not take action in any of the above areas. Hopefully we can both agree that government intervention in at least the first two areas above is justified by significant actual abuses that have occurred in the past, even if we disagree on how much government action is appropriate.

   Now, on to some specific points you made.

   This is essentially Marx' old argument for how free market capitalism would abolish itself. It has a couple of big holes in it.

   I wasn't thinking about Marx at all when I made the comment, and I don't think that his agreement or disagreement with it has anything to do with whether the argument is correct. Marx probably thought periodic bathing was a good idea too; I doubt anyone will stop bathing because Marx thought that. 🙂

   One boils down to the difficulty of precisely defining "the market in X".

   This isn't something I had thought about, but I agree with your argument here. I would probably call much of it "market segmentation" rather than "different markets," but I don't know if this is purely a matter of semantics or not. The kind of monopolies I am concerned about are the ones relating to basic needs, where a monopoly has a lock on a very important market and there are no available alternatives, whether it is a scarce resource, a distribution channel, or a manufactured product. On the other hand, many markets are probably not prone to monopoly; for example, trees grow in a lot of places, one company could probably never own a sizable fraction of all the farm land, and you can always drive a basic car if you don't want to pay for a high end sports car.

   As new technologies emerge, old ones are abandoned, and even if a monopolist had managed to establish itself in the context of the old technology, it is very rarely able to retain its position – or even just to survive – when the technology changes… Xerox and IBM are two examples which I particularly like…

   Here I will both agree and disagree, but in different ways. I disagree because this only works in many cases if the market is sufficiently free. A new competitor must be able to establish itself enough to become a serious competitor, and new technology won't necessarily allow that if the established monopolist can use predatory pricing to prevent the new competitor from making a profit, or if the monopolist closes distribution channels to the hopeful competitor by using its market power. Probably most importantly, there is nothing to prevent a monopolist from seeing the need to adapt to the technological change by adopting the new technology itself and then proceeding to use anticompetitive practices to put the new competitor out of business before raising prices again. The reason this works is that adoption of new technology is rarely immediate, so the monopolist can use its power to buy time while adapting to the change.

The way I see it, your argument about technological change is primarily valid if (1) the monopolist is incompetent and refuses to adapt to maintain dominance; or (2) another company with a lot of resources and clout decides to enter the monopolist's market. I admit to grossly oversimplifying things by completely ignoring international trade, and that probably throws into question a lot of what I am arguing since a monopolist can't exert much control in a country where it has little presence. But the international market is even less free than the US market, so I think it is hard to fully take that into account. And wage disparity won't be permanent in the presence of a free market.

If monopolists are effective in using the above anti-competitive practices, it seems safe to say that there will be less and less interest by investors in funding a new company that will compete primarily on the basis of new technology. I think Microsoft is an excellent example — venture capitalists tend to steer clear of funding companies that will compete with Microsoft, one reason being that Microsoft has been very good at incorporating new ideas and thus depriving the new competitor of a market.

Finally, I think the examples you gave of how new technologies can end monopolies don't really support your argument too well. They are great examples of how companies can lose dominance or markets can dramatically change due to new technology, but they all happened in a relatively free market. If IBM hadn't been restrained by the US government in the 1960s to 1980s, I think there would be a lot more mainframe usage today. (As far as Xerox goes, their inability to make money from great new technologies they invented outside their core market is legendary; that says more about their incompetence than anything else). Finally, a lot of the new technologies and markets are offshoots of earlier ones, and they probably wouldn't exist if the earlier ones hadn't established themselves. I don't think you can come up with good supporting arguments for your position that apply to a truly free market, because as far as I know there is no history of that kind of free market existing, at least in "modern" times. It is just "belief" on your part…

*No, a government monopolist will squeeze much harder, in the absence of *any* counterforce.*

I was thinking of the Soviet Union when commenting that a government monopolist will probably not be as inclined to try to extract the maximum possible price for goods. Basic goods were often cheap; it's just that their supply was limited or nonexistent (a different problem). I was not trying to use that as an argument in favor of central planning…

I think this has gotten long enough. I want to respond to some of your other points, especially where you tried to argue that among my examples of government involvement, none actually had an overall positive economic benefit. (At least that seemed to be what you were claiming.) Some of my examples were better than others (the farming one is probably questionable), but I think you are extremely optimistic to think that the aircraft, electronics or communications industries, for example, would be anything close to what they are now if the government hadn't supported their development and acted as a large, reliable customer while the technologies established themselves and costs declined. But I admit that is speculation.

Anyway, thanks for taking the time to reply to my past comments. I am definitely learning from it, and it is getting me to think more about these issues.

2. **Alejandro Rivero**
   July 13, 2005

   But seriously, your definition, and the subsequent extinction of "planned economy" species only proofs, from the point of view of evolution theory, that planned economies had a disadvantage in the niche of "XXth century countries".

   Darwin's theory does not have a concept of progress, just extinction.

3. **Alejandro**
   July 13, 2005

   I am imagining the sexual intercourse of two countries… well, mitosis seems a more frequent process.

4. July 13, 2005

   A.R. wrote:
   Evolution theory is about species and niches, not about individuals. It is a shame that divulgated evolution theory fails to stress this.

   So what prevents me from defining the species of free market economies and watching it outcompete the species of planned economies, with countries in the role of individuals?

5. July 13, 2005

   M wrote:
   as Maynard mentioned a little while back, "friction" could be introduced into the system to decrease that attractiveness of quick, low profit trades, for example a tax on certain kinds of transactions

   Such friction is already in place: it's known as the spread, i.e. the difference between bid and ask prices which ultimately lands in the pockets of brokers and exchanges as payment for services rendered. No need for a tax. 🙂

6. **Alejandro Rivero**
   July 12, 2005

   what does in fact, prevent a country from being considered a biological organism?

   You need species. Evolution theory is about species and niches, not about individuals. It is a shame that divulgated evolution theory fails to stress this.

7. **M**
   July 12, 2005

   Hi Anonymous,

   Thank you for your well reasoned reply to my comments about the Ren Tech stragegy. I really appreciate your informed comments — I am learning from them, and trying to learn something and test/modify my views is my motivation for putting some energy into this topic.

At this point it seems like our views are not really in conflict, at least in matters that are well understood. Just a couple of comments about what you wrote:

What actually seems to happen is that the competing technical systems quickly start pre-empting and therefore neutralizing each other, as noted above. Even if this weren't the case, it's hard to see how regulation could help.

I think JC mentioned something about this neutralization early on. It certainly makes sense; one could probably predict this would happen. If this is indeed how things play out, then there probably wouldn't be important, negative long term consequences, meaning that this kind of trading didn't offer significant new efficiencies (i.e., that it was a short term tactical move borne of opportunity).

If, on the other hand, the trading strategy has meaningful negative consequences then I believe regulators will try to step in and "fix" it. Note that I am not arguing that they *should* step in, nor that such an attempt would be successful (as you noted). On the other hand, I am not an absolutist; I think *some* kinds of regulation do more good than harm, so I don't have an opinion at this point. But whatever I (or you) may think, if there is enough disruption and it is perceived as negative, then I am confident that regulators will *try* to clamp down.

I agree that the kinds of regulation that you mentioned, such as limits on trades, computer based trading and limits on trading volume would be either ineffective or counterproductive. However, as Maynard mentioned a little while back, "friction" could be introduced into the system to decrease that attractiveness of quick, low profit trades, for example a tax on certain kinds of transactions (at a minimum this would eliminate the benefit of using multiple brokers). Please note that I am *not* advocating taxation of this sort, but mention it only as an example of the many different tools a creative regulator could use. We'll see how it all plays out…

8.
July 12, 2005

A.R. wrote:
Please let me apologise

No need to apologise.

A.R. wrote:
pricing mechanisms depend on availability, and that any market system, willing to have exchange prices, must restrict availability by using ownership. The

same applies, intriguingly, in Estate regulated economies, except that ownership is not exercised by individuals.

I agree, prices depend on availability; if you don't want to run into scarcity and oversupply problems, there's no way around that. The problem when trying to dictate prices, rather than letting the market find them on its own, is that you are trying to perform and enormously complicated optimization task. You don't have all the relevant data to do it; if you did have all the data it would overwhelm you; and if it somehow didn't overwhelm you, you would just end up finding the same optimal price point set by the market. So what was the point of all that work, then?

A.R. wrote:
I fail to see how Spanish America was a *internal* colonisation.

Before they became independent countries, the Spanish, Portuguese (and Dutch, and French, and British) colonies in the Americas were run as territories of the respective coloniser, remember? And for a long time, they pretty much plodded along according to the usual European model of the time, with kings, aristocrats and priests running the show. The US got rolling only after it threw tea, taxes and Britons back into the Atlantic and decided to start doing things its own way. And not a minute too soon, I say!

A.R. wrote:
As for "evolution=confrontation", I am pretty sure this is not a theory of evolution as applied by the biologists

Watch countries, economies and social systems evolve, interact with their environment and compete with each other over historical timescales (centuries, millennia). How does this process differ from that of biological organisms evolving, interacting with their environment and competing with each other? (Now that you got me started, what does in fact, prevent a country from being considered a biological organism in its own right?)

9.
July 12, 2005

M wrote:
I am not aware of any truly free market in any developed, industrialized country

I must unfortunately agree. Such is the state of our world that we must choose among the lesser among evils. 🙁

M wrote:
a truly free market, i.e. a completely unregulated one, is in a kind of unstable equilibrium. Left alone, some competitors will emerge as the strongest, and monopolies form because there is nothing to prevent the most powerful from becoming powerful enough to completely dominate their market.

This is essentially Marx' old argument for how free market capitalism would abolish itself. It has a couple of big holes in it.

One boils down to the difficulty of precisely defining "the market in X". A silly example: is there just one market in cars, or do Ferraris and SEATs really trade on separate markets? Methinks they are really separate, and guess what – the people who make the world's best sport and luxury vehicles turn out to be really bad at competing in the low end mass market, where they get handily beaten not only by Seat but by a whole bunch of Japanese (and other) manufacturers (behind the various corporate logos, it's all FIAT, sometimes spelled out as Failed Italian Attempt at Transportation… which is just plain wrong, at least if your name happens to be Schumacher). Markets stratify and specialize, and competition tends to reward those who concentrate on one segment. Even then, it's rarely the case that a single company completely dominates its segment for long.

The second big hole has to do with the last two words in the previous paragraph: "for long". Schumpeter coined the phrase "creative destruction" to describe the effects of technological development on companies. As new technologies emerge, old ones are abandoned, and even if a monopolist had managed to establish itself in the context of the old technology, it is very rarely able to retain its position – or even just to survive – when the technology changes. When you get a little bit older you start realising that the average time period involved – the duration of a dominating position – is really quite short. You start thinking about it, and you can rattle off one company after the other which totally dominated its market when you were a child or a teenager, and now it's gone – along with the market! Are you old enough to remember mechanical watches from world-renowned Swiss manufacturers (what were they even called?), mechanical calculators (Facit), typewriters (Olivetti), Polaroid instant cameras, Xerox copiers, IBM mainframes?

Xerox and IBM are two examples which I particularly like, because of all their irony: the Palo Alto Research Center run by Xerox developed the mouse & GUI computer interface which we all take for granted these days. Apple "borrowed" it, using it first in a clumsy contraption called "Lisa" (after Job's daughter, I think – or was it

Wozniak's?) which bombed in the market, but which was followed by the (need i say it?) wildly successful Mac line. All the while Xerox kept churning out paper copiers, eventually waking up in reconstruction as the market for paper copiers was largely killed by digital documents stored, processed and viewed on (and occasionally printed from) computers with mouse & GUI interfaces – their own technology! Meanwhile, IBM – king of corporate computing – outsourced the task of developing an operating system for its newfangled personal office computer to a two-bit startup calling itself Microsloth or something… and in so doing created the largest computer market in history, only to be pushed out of the hardware business by Dell and other low cost manufacturers (do you even remember the time when those were called "clone makers"?), while MS took over the software business (but for how much longer?). Did you ever notice when IBM recently sold off the last remnants of its PC business to a Chinese outfit? Probably less so than Apple's announcement that they are finally dropping IBM's processors in favour of Intel's. What IBM does these days? Mainly "services", which to a large extent seems to be about keeping those old mainframes running at the premises of die-hard corporate customers.

It's not easy being a dominatic actor these days… not to mention a monopolist, which strictly speaking is something else, a company which isn't allowed to have competitors (typically by political mandate, as in the common cases of tobacco, alcohol and telecommunications), rather than one which is just very good at beating them in the market (which must mean it's doing something right in the eyes of the consumers, or they would indeed favour the competition).

Now, I'll give you this much: it's conceivable that technologically mature markets, where little further development is possible, may fall in the hands of a dominating company, that such a company may abuse its dominating position in various ways, and that the lack of creative destruction due to the lack of technological competition will cement this dominating position for a long time. I agree that such extraordinary circumstances may call for extraordinary measures, presumably the breaking up of the company in smaller independent units, as was done in the US with Standard Oil and with Bell. But such cases are truly few and far between, and even the two big, historical ones which I just mentioned are often taken as examples of the questionable wisdom of such imposed breakups. Were they really necessary, or would technology have done its job soon enough? Were they really not politically motivated, a la Lukoil? Were ties between baby oils and baby bells really severed, behind the scenes? And what was the net impact on the national economy? There's plenty of material for debate there.

M continued:
Society suffers as these monopolies extract the maximum price their markets will bear without collapsing. Innovation suffers because as long as the monopolist has a captive market there is little external economic pressure to improve efficiency.

I most definitely disagree. The first sentence in the above quote describes a situation of extreme economic duress for the customers, who will therefore be extremely motivated to innovate their way out of their dependence on the monopolist.

M. hoped that:
A government monopolist is probably not going to squeeze the consumer as hard either.

No, a government monopolist will squeeze much harder, in the absence of *any* counterforce. When the same people who run the businesses are the ones who run the schools, the media, the courts and the government, exactly where are you going to turn? And if there's nowhere to turn, why would they not squeeze you as hard as they can? Out of the goodness of their hearts?

M asked:
Was their inefficiency and ineffectiveness at meeting consumer needs due to the fact that the businesses were monopolies (and hence had little motivation to improve), or was it primarily attributable just to their central planning?

How can there be competition without a free market? How can there be central planning with a free market? You can have one or the other, but not both.

M opined:
China seems to maintain a significant amount of central economic planning while gradually freeing its markets by moving away from government monopolies and toward privately owned businesses.

No. China maintains a political dictatorship, i.e. one party rule, state control over the media, censorship, jailing and execution of dissidents, but has been dismantling state run businesses and letting private ones run pretty much undisturbed as long as they make money and nothing else (i.e. trouble, as in free media). It's not the first time the world sees this kind of mix between steel-fisted political dictatorship and pretty free market economics; another releatively recent example which comes to mind is Chile under Pinochet. As far as the economy is concerned, it's not more centrally planned than the US.

M then really made my day by reminding us all that:
The aircraft and electronics industries were extensively funded and nurtured to maturity due to their usefulness for war.

Yes. States with lots of resources – ultimately always derived from taxation, known as theft when any other entity engages in it – always do that, plow those resources down into military expenditures of one kind or another. After relieving their citizenry of the means needed to develop and build the weapons, they draft – a practice known as slavery when any other entity engages in it – their sons (and occasionally daughters) and send them out to kill other expendable young people like themselves using said weapons. I guess that's why you assumed, a little while ago, that those who run state monopolies are inherently good and won't squeeze as hard as private "monopolists", correct?

So, just to make sure that I got you right: states, i.e. the entities which use theft and slavery to start and run wars, are inherently good; companies, which offer goods on a free market for voluntary exchange, are inherently bad. Did I get that quite right?

M claimed:
The Internet was originally a military project.

You are persumably thinking of ARPA-net, just one of the many – mostly academic – networks which grew independently and then fused into the Internet as we know it today. What made it into a mass phenomenon, hence relevant, was (1) the WWW, which as we all know was originally dreamed up at CERN (on European tax money, I'll hand you that much) and (2) the mass availability of PCs, for which we can thank Xerox, Apple, IBM, MS, Dell et.al., in various degrees (see above).

M claimed:
The space industry would probably not exist yet without government funding.

My guess, which is at least as good as yours, is that communication, earth resource and weather satellites – all most commercially viable – would be orbiting the earth now even if not one dime of tax money had ever gone into politically motivated moonwalks and other such nonsense, which historically absorbed most of the funds thrown into the "space" bin. I agree we would most probably not have those amazing videos of astronauts playing golf on the moon. The loss!

M claimed:
The railroad industry was given a significant push due to government grants in the 19th century.

The thought which never seems to enter the mind of those talking about "government grants" is that the money in question came from somewhere – and no, it wan't from the government, it was from something known as taxes. So, let's see; first the government relieves workers and companies of their money, then (after taking its "own" ample cut) it hands some of that money back in various ways. That makes the government the giver of all good things? Let's imagine the unimaginable, that companies and workers had been allowed to keep their money to do things like – oh, I don't know, invest in railways, perhaps? Do you seriously believe that profitable activities wouldn’t be undertaken without the “aid” (actually, it should be properly called interference) of the government?

M continued:
Government funding of the intrastate and interstate highway systems involved huge outlays of funds

Same illogic as for railroads.

M reached new highs with this:
Central planning has been important in maintaining the health of the farming industry, for example in price supports to prevent boom and bust cycles.

Surely even you are aware that the farming “industry” in both the US and the EU is an unmitigated scandal, living off subsidies and protected by trade barriers against third world countries, all amounting to far more money being poured on fat Western farmers than what goes in “aid” to starving ones in the third world, the whole revolting state-run business resulting in Western consumers (yes, the same ones who finance the farm subsidies with their tax money) paying far more than they would in a free market for products which could be imported cheaply from people who are literally dying for a chance to sell their crops?

The “health” of the US, EU (and to some extent Japanese) farming industry is nothing but. Farming is – or rather, should be – a business like all others. If it can’t be run profitably, it shouldn’t be run at all. Those “healthy” farms which you are looking at zombies. The sooner they are put to rest for good, the less further damage they will do.

M continued:
The Federal Reserve Board uses its power over the credit markets to try to keep things stable

The artificially low interest rates dictated by the Federal Reserve Board (which, needless to say, should not be allowed to dictate any interest rates at all – there is no reason why the optimal price for borrowing money shouldn’t be set freely by the market, no matter what the duration of the loan) have created a whole series of bubbles, too long to even start rattling it off here. If you really believe the current situation is stable, I suggest that, as a first step, you peek at a chart over total US debt.

M then surprised me with:
and many would say that currently its policy of maintaining artifically low interest rates continues to encourage unnaturally rapid price increases in many housing markets.

Yes, that’s the most well-advertised of the current Fed-induced bubbles. I thought you were talking about stability?

M correctly summed up thusly:
All in all, I think one could make a good argument that even the US has significant elements of central planning

I must unfortunately agree.

M then again lost me with:
and that overall it has been to the good of the economy and society

The US is great because it’s less regulated and central planned, i.e. more free, than most other countries. The US could be far greater still if it really lived up to the spirit of what got it started in the first place: that little tax revolt known as the Boston Tea Party.

10.

July 12, 2005

M wrote:
there is something about the use of blind, automated trading, based only on recent activity without regard to any kind of fundamentals, that bothers me

There, now you put your foot right in the ever-raging debate between fundamental and technical traders. 🙂

Most technicians would probably tell you that all available information is already priced into to market, so studying fundamentals is just a laborious and error-prone way of finding out what you can learn immediately from the price and volume charts. The fundamentalists would counter this by asking: ah, but what information can there be in a chart if every trader just looks at market action and nobody bothers to analyze the fundamentals any longer? My (obvious) answer is that, sooner or later, such behaviour will result in a bubble, with financial markets detaching themselves from the realities of the underlying real economy and taking on a brief but spectacular life all of their own.

But, and here we come full circle, if you study the past behaviour of markets – as you must, if you want to be a technical trader – you can’t avoid learning about the existence of bubbles. In fact, you’ll get to know all kinds of interesting historical examples (tulip mania, South Sea Bubble, DJI ~1929, NDX ~2000…). If you are hard core enough, you will even find technical bubble signatures like the log-periodic oscillations of Sornette et.al.; but what you’ll defintely learn is the necessity of always keeping a watchful eye at least on a few fundamental indicators like price over earnings growth. Not to trade on from day to day – that’s futile – but at the very least as rough cuts to keep you out of the worst trouble, and more generally to give you a sense of overall direction over the longer term.

Now, I don’t know what RenTech is doing – I guess nobody outside the company does, or is allowed to tell, at least – but if they are half as successful as claimed, I find it extremely hard to believe that they are not throwing fundamentals into the longer term part of their statistical analysis.

M continued:
This, in a sense, is indeed an efficiency improvement, although it is only of benefit to Ren. Tech’s clients rather than the investment community as a whole.

It’s so tempting to quip “what’s good for RenTech is good for the markets”, but as long as they don’t pay me to do that, I’ll try to take a broader view.

So, let’s remember who’s at the other side of trades entered by speculators. Another speculator? That’s one possibility. In that case, each individual trade may be a zero-sum game between speculators, but the sum of all those trades provides much of the previously mentioned liquidity and price discovery.

Consider for instance the passage of Dennis through the Mexican Gulf. Last Friday, nobody could tell for sure whether the oil rigs and ports in the region would be hit, and if so how badly, and what the consequences would be for oil production and availability.

Companies which depend on a steady flow of oil were looking at a risk. Now, the first thing which you want to do when dealing with a risk is quantify it. That means answering the question: how bad can it get? The activities of speculators in the oil futures markets provided an educated answer to that question, based on weather reports and publically available information about the daily volume of oil produced and off-loaded in the Gulf, the positions and repair status of oil rigs and ports, the safety measures (like taking personnel off platforms and suspending port operations) of companies in the region and so on. This amounted to a pretty sophisticated analysis, performed in real time in a distributed fashion by a large number of independent agents, all acting in – and sharply motivated by – their own self-interest. The consensus emerging from this operation, weighted by degree of conviction (a good measure of analysis effort spent) and speculative resources (a good measure of analysis resources) could be looked up as it evolved, tick by tick, by anyone with nternet access. That’s a pretty good societal service.

But returning to the question of who's at the other side of the trade: the other major possibility, if it isn't a speculator, is that it's a commercial trying to insure against a risk, a.k.a. a hedger. That's the second thing you want to do when dealing with a risk: after
you've quantified it, if you determine that circumstances call for it, you buy insurance against it. Last Friday, if you were a commercial depending on the price of oil for your operations, you could have bought insurance against supply disruptions in the form of oil futures or call options. The counterpart would have been a speculator, willing to take on the risk in exchange for the prospect of a profit.

And here we find the answer to your question: if this speculator is successful, it's because it has good risk management and analysis. From your point of view as a commercial seeking insurance, good risk management means that your counterpart won't go bust and prove unable to pay you when it's time to close the trade; good analysis means that it can take on larger volumes and offer you a better price than other speculators, who are less confident about the future direction of oil prices. Remember, when a commercial goes to the futures or options market to buy insurance, the speculators have to bid for the commercial's business.

M correctly observed:
it seems the game now starts to change. Trading activity is now influenced by the same trading considerations as before, but an additional influence also starts to become important: the behavior of the automated trading systems themselves

True. It's often observed by technical traders that once an indicator becomes widely known and used, its efficiency declines rapidly, as everybody starts pre-empting its signals. To the extent that trading is a zero-sum game between speculators, this inevitably kills the profitability of the indicator, since all traders can't possibly make money from it at the same time. The same reasoning applies when you employ statistical analysis to find patterns in past market action. You may not know the explicit form of the indicator(s) being used by your speculating counterparts, but if you can deduce how they'll respond to future moves, that's close enough. You'll still have neutralized their advantage. So off they go to the drawing board (or, more likely, to the computer) to cook up something new.

And so do you.

I believe biology has plenty of behaviours and physical characteristics evolved in such a manner, with predators and preys, or even males and females of the same species successively adapting to each other's adaptations in previous rounds.

M continued:
Over time, if blind, automated trading systems become the norm (as they would in an unregulated environment if they are more efficient than older systems), then I would expect an important shift in the rationality of affected kinds of speculative activity.

What actually seems to happen is that the competing technical systems quickly start pre-empting and therefore neutralizing each other, as noted above.

Even if this weren't the case, it's hard to see how regulation could help. Are we going to prohibit thinking up new trading strategies? How would such regulation be enforced? Prohibit computer-based trading? Bye bye NASDAQ – and just about every modern exchange in the world (even the last few strongholds of open outcry run electronic trading networks and will probably be fully screen based in a a few more years). Impose a limit on the number or speed of trades? Just sign up with more brokers, trade through more exchanges or make larger block trades (the latter really bad news for anyone wishing to see smooth, liquid, continuous market action). Impose a limit on daily volatility? Done already, after the crash of 1987 – automated trading systems were conveniently blamed, so automatic circuit breakers were put in place which would prevent a repeat performance (and proved pretty useless in 2000 and 2001; all they did was protact the agony rather than have it all go down in one full swoop).

Ultimately, I just don't see with what *right* a third party could come in and impose such regulations on the participants in a free
market. Those who want to trade a market do, those who don't want to don't; those who want to start up a new market with their own rules can do so, too (and do, like the bunch of ECNs for stock trading which started up in the late 90s; the flavor of the day seems to be emission rights trading).

11. **Alejandro Rivero**
July 12, 2005

Dear anonymous,

Please let me apologise by the translation problem in the word "value", and blame myself with Machado: "Solo un necio confunde valor con precio". It seems to me, and this was my whole point, that pricing mechanisms depend on availability, and that any market system, willing to have exchange prices, must restrict availability by using ownership. The same applies, intriguingly, in Estate regulated economies, except that ownership is not exercised by individuals.

I fail to see how Spanish America was a *internal* colonisation. As for Siberian, as you say, there is a component lacking: the willingness to go there and to stay there.

Of course I do not like the old Spanish (et al) system of imposed values. Your suggestion surprised me, and it shows that I did not got to make myself clear in the previous posting.

As for "evolution=confrontation", I am pretty sure this is not a theory of evolution as applied by the biologists. Discussion could be perhaps a bit off from the blog entry topic, economy+physics.

12. **M**
July 12, 2005

Hi Anonymous,

You wrote:

There is a system of resource allocation which is theoretically understood and empirically known to work very, very well: the free market. There is an alternative system of resource allocation which is theoretically understood and empirically known not to work well at all, leading to all sorts of imbalances (scarcities of needed goods and surpluses of unwanted ones) and typically collapsing after a few decades: central planning.

First, I agree that overall, free markets are much preferable to centrally planned economies, and probably for similar reasons to yours. However, I am not aware of any truly free market in any developed, industrialized country, even though the U.S. probably comes as close to that "ideal" as any.

I enclosed "ideal" in quotation marks because I think it is hard to argue that a truly free market is desirable for society as a whole, just as a centrally planned economy (the other extreme) is not desirable. From what I understand, a truly free market, i.e. a completely unregulated one, is in a kind of unstable equilibrium. Left alone, some competitors will emerge as the strongest, and monopolies form because there is nothing to prevent the most powerful from becoming powerful enough to completely dominate their market. At the point of monopoly the market is no longer free; the central planners aren't in government, but they are in the company headquarters. Society suffers as these monopolies extract the maximum price their markets will bear without collapsing. Innovation suffers because as long as the monopolist has a captive market there is little external economic pressure to improve efficiency. And so on. I don't think any of this is very controversial; it seems to be the way things have gone in times past when there was little regulation. I also imagine that you would favor government regulation to keep it from happening, provided it is kept to the minimum necessary to keep the markets as free as possible.

The centrally planned economy you mentioned most prominently, the Soviet Union, is interesting because it also had government ownership of production. Thus, according to my understanding the government was also the monopolist in all key markets, which may be worse than having the different monopolists that emerge in a truly free market, but if so it seems more a matter of degree. (A government monopolist is probably not going to squeeze the consumer as hard either.) So an interesting question arises: Was their inefficiency and ineffectiveness at meeting consumer needs due to the fact that the businesses were monopolies (and hence had little motivation to improve), or was it primarily attributable just to their central planning?

Also interestingly, China seems to maintain a significant amount of central economic planning while gradually freeing its markets by moving away from government monopolies and toward privately owned businesses. Since it continues to grow at a strong rate and is emerging as increasingly powerful on the world stage, it seems their example argues more against monopolies than central planning as the primary economic evil. But since giving up their monopolies means the government has less direct control, it may be hard to distinguish between the two.

Even in the United States, government involvement has interfered with the natural course of free markets, but at least sometimes it has had very positive economic consequences. For example, consider the aircraft, space, electronics, railroad and automobile industries in the United States, which have been extremely important economically; at different times at least the automobile and electronics industries have been the single largest manufacturing employers in the US. The aircraft and electronics industries were extensively funded and nurtured to maturity due to their usefulness for war. The Internet was originally a military project. The space industry would probably not exist yet without government funding. The railroad industry was given a significant push due to government grants in the 19th century. Government funding of the intrastate and interstate highway systems involved huge outlays of funds and made the automobile more desirable. Central planning has been important in maintaining the health of the farming industry, for example in price supports to prevent boom and bust cycles. The Federal Reserve Board uses its power over the credit markets to try to keep things stable, and many would say that currently its policy of maintaining artifically low interest rates continues to encourage unnaturally rapid price increases in many housing markets. All in all, I think one could make a good argument that even the US has significant elements of central planning and that overall it has been to the good of the economy and society.

13. **M**
July 12, 2005

Hi Anonymous,

Well, am finally getting around to responding to you about your last comment in the discussion about computer-based trading of the kind Ren Tech seems to use, and whether it contributes any additional value to society or is just opportunistic. You seemed to take the view that it contributes new value in terms of improved efficiency:

Just like technological competition between car manufacturers results in better cars for their customers, techonological competition between speculators results in better risk reduction, liquidity and price discovery for theirs (i.e. the "respectable" market participants).

You may be right, but there is something about the use of blind, automated trading, based only on recent activity without regard to any kind of fundamentals, that bothers me. Recall that I agree speculators provide an important service. I also agree with you that efficiency improvements are important; they improve productivity. If an automobile manufacturer introduces significant efficiency improvements, competitors will notice and introduce their own improvements or probably lose market share. Similarly, if a large investment management firm produces superior returns due to rapid, automated trading systems, then its competitors will take notice and either introduce their own similar efficiencies or suffer competitive loss.

Where I think the efficiency argument starts to break down is as follows. If the comment by "R" is indicative, i.e.,

RenTech has a large historical database of stock market price movements. The price movements have a lot of random noise, but also significant signals. The job is to identify the signals…

then the trading strategy includes analyzing historical indicators together with current activity and then making trades accordingly. This, in a sense, is indeed an efficiency improvement, although it is only of benefit to Ren. Tech's clients rather than the investment community as a whole. Assuming they maintain superior returns, others can be expected to employ similar tactics. However, it seems the game now starts to change. Trading activity is now influenced by the same trading considerations as before, but an additional influence also starts to become important: the behavior of the automated trading systems themselves. Historical data and analyses don't fully reflect the trading patterns of these computer systems; trades by them are done for different reasons than the historical ones. So the trading systems must adapt to this new trading force; automated trading systems must factor in the activity of competing automated trading systems. Over time, if blind, automated trading systems become the norm (as they would in an unregulated environment if they are more efficient than older systems), then I would expect an important shift in the rationality of affected kinds of speculative activity. Trading activity may become less comprehensible to human traders, since blind, automated trading is at least one step removed from the historical forces that drive speculation (e.g., natural disasters, changes in political climate, changes in economic indicators).

Left unchecked by regulation, I can imagine several kinds of undesirable consequences. Having rapid, automated trading as a significant force in speculative markets could make these markets less stable. People may be relatively slow, but this slowness can improve stability; significantly reducing this time by automation reduces feedback. Second, if having an automated trading system is the price of entry into some speculative markets, smaller investors except for the very brightest will probably be driven out. It is hard to see how the resulting concentration of power would be a good thing. Finally, if the nature of speculative trading changes significantly enough, it seems it will be less effective in providing its historical value to the economy.

I guess I'm saying that I don't see more than a superficial parallel between traditional efficiency improvements (e.g., in car manufacturing) where economic benefits are fairly clear, and efficiency improvements in speculative trading due to technical data driven automated trading. If the hazards I mentioned become reality then I would expect regulators to step in. If none of those hazards became important then it would probably be because the automated trading systems weren't all that effective after all. Either way, I still don't see how the enconomy as a whole will benefit from them; it still seems more like opportunism than additional value to society.

14.
July 12, 2005

A.R. wrote:
proving that a system/theory/whatever is incorrect does not prove that an alternative id./id./id is correct.

No, but pitting alternative systems against each other under varying conditions and seeing which one does better is a perfectly valid way to find out which one works best (biologists know the concept well as "evolution"). By now, only somebody incredibly (read willingly) ignorant of history could claim other than this kind of competitive stress test has been done many, many times over, always with the same result: planned economies fail (typically killing millions in the process), free market economies thrive.

A.R. also wrote:
As for free markets, the problem to me is not freedom, but market. In the sense of valuation, because I feel that the methods to impose value upon objects are defective.

This statement shows that your problem is very much freedom. Freedom means that each individual chooses for him- or herself what constitutes value, and acts accordingly. There is no such thing as "methods to impose value" in a free market, only methods to peacefully achieve mutual, voluntary agreement on the correct *price* of X in terms of Y (and vice versa), leading to the optimal availability of *both*. If you value X more than Y, you give som Y in exchange for some X. If you value Y more than X, you give some X in exchange for some Y. Free market economies don't impose preferences and values.

Imposing values, i.e. telling others what they should like and dislike, want and not want, demand and abhor – that's the business of dictatorships, which centrally planned economies must inevitably be.

It's obvious from your statements that your real reason to dislike free market economies is that they extract the fair price for the things which *you* value – let's venture a guess, time to think about physics. It would be much nicer to get all that time for free, wouldn't it? Except there is no such thing as a free lunch. If you don't pay for it, somebody else has to do so instead.

That's what imposing values ultimately means: forcing others to pay for what you, not they, want. No longer voluntary agreements, a.k.a. trade, but brute force. That's what you are advocating.

A.R. also wrote:
internal colonisation -go go west- was really a very special prerrogative of the USA

No dear, it wasn't. The medieval kingdoms of Spain and Portugal, incidentally societies much more in line with your preferences of imposed values, had the same prerogative in Latin America, and see how well that served them. Or if you'd prefer to talk about the Soviet Union, there was plenty of internal colonisation going on there too. Of course, those invaluable Siberian oil fields were not exactly created by voluntary workers, now were they?

15. **Alejandro Rivero**
July 11, 2005

Note that proving that a system/theory/whatever is incorrect does not prove that an alternative id./id./id is correct. I believe this has been already argued in this blog when discussing about Quantum Gravity (or in physicsforums?) and I do not see apropiate to repeat all of it.

As for free markets, the problem to me is not freedom, but market. In the sense of valuation, because I feel that the methods to impose value upon objects are defective. Time ago I dreamt of developing a "Physics of Ownership" by studying first valueless, scale invariant, markets as if they were fixed points, and then taking value -either via private or public retention, er, ownership- as a perturbation, analysing relevant and irrelevant parameters etc.

(PS: internal colonisation -go go west- was really a very special prerrogative of the USA, and it distorts the whole perspective)

16.
July 11, 2005

Bah! To this day, even after the loss of the oil-rich, former Soviet republics in the south, Russia remains one of the planet's largest oil producers and exporters. The Soviet Union of old controlled one sixth of Earth's surface and was essentially self-sufficient in natural resources – or would have been, had it not been utterly unable to manage its natural wealth due to its dysfunctional central planning system of waste and corruption.

So much for oil.

As for colonisation, it was certainly neither invented by nor a prerogative of free market economies. Historically, every kingdom and empire which had the capacity to engage in that practice did so, right up to the last one of them, the former Soviet Empire, whose adventures in South East Asia, Africa and Latin America surely aren't unknown to you. But needless to say, even that couldn't save its fundamentally flawed system.

17. **Alejandro Rivero**
July 10, 2005

There is a system of resource allocation which is theoretically understood and empirically known to work very, very well: the free market.

I deny any scientific meaning to "empirically" above. Such empirical source was not under controlled conditions, and a blank experiment does not exist. Most free markets economies have enjoyed an external resource inflow from external (EU) or internal (US) colonisation process, and this inflow could cause the delusion of efficiency.

Additionally, the availability of oil during the XXth century, sometimes becoming almost a free energy source, cast doubts about the equilibrium state.

18.
July 9, 2005

There is a system of resource allocation which is theoretically understood and empirically known to work very, very well: the free market.

There is an alternative system of resource allocation which is theoretically understood and empirically known not to work well at all, leading to all sorts of imbalances (scarcities of needed goods and surpluses of unwanted ones) and typically collapsing after a few decades: central planning.

So, you have a plate of good food and a plate of poison. Which mix of the two are you suggesting is optimal? 50% food and 50% poison? 90% food and 10% poison?

In a free market, the allocation between "planners" (i.e. investors, speculators, traders and what have you) and producers of other goods is self-regulated by it just like the allocation between producers of any set of goods. Supply goes where there is demand to support it, or it will quickly be starved out and move on to other businesses.

In a planned economy, even just a "partially" planned one like European social democracies, allocation decisions are made by politicians and bureocrats, i.e. people who have neither the analytic capabilities nor the information necessary to make sensible decisions, who are not constrained by the feedback mechanisms which make free markets self-correcting and whose own self-interest always calls for one thing only: steal more, spend more.

In one word, poison.

19. **Maynard Handley**
July 8, 2005

What good does trading provide?

In a certain sense the financial markets are the world's planning department; they sift through various proposals for how to invest money and choose those that appear to be most likely to generate profit. This is very obvious in the case of venture capitalists, it is somewhat more roundabout in the case of buying stock at the NYSE and thereby "voting" for or against Apple computer.

Now one can levy at least two different types of criticism against this activity.
* The first is generic "anti-money" sort of criticism, saying things like "the value of a new product is more (eg perhaps some medicine) or less (a newer bigger SUV) than just how much money it will make". There's probably validity to this argument, but beyond this overall agreement, people seem unable to make useful progress on either how to measure this beyond-money value, or how to construct a society (based on real human beings subject to all the corruption and evil of real human beings) to put into

place these "beyond-money" ideas. So I find this line of criticism rather sterile.
* A second line of criticism is to accept the necessity of the current system, but to quibble with details of it. The drugs vs SUVs issue can be tinkered with using taxes and CAFE standards, for example. Specifically, nnow, the issue is, yes these people are doing useful work in planning how the world should invest its money BUT
(a) don't reach a limit in terms of the value of this planning? Having 1% (to pick a number) of the world's brain power spent on the task may be worthwhile, but perhaps the extra 1% spent taking this to 2% of the world's brain power is better spent on making the things we've already planned to do work better?
(b) isn't most of the energy thrown at the problem actually involved in a zero sum game of attacking other participants in this space, meaning that reducing the amount of effort (brain power and computational power) thrown at the problem even by a factor of 10 would lead to precious little difference in the actual planning outcomes?
I personally think both of these are perfectly valid criticisms, and I think society would be better off acting on them. For example small amounts of friction thrown into the system (transaction taxes, higher income taxes etc) would, IMHO, have pretty much zero influence on the actual outcomes for society, but would reduce the value of the exercise for many participants, thereby encouraging them to go off and do something more useful for society.
(Naturally, of course, those in power would disagree with this claim. As always, use some common sense and ask yourself "cui bono"?)

These ideas are explored at great length in Doug Henwood's book _Wall Street_, available for download at
http://www.wallstreetthebook.com/

20. **r**
July 7, 2005

Since I have known a few experimental physicists who have gone to RenTech, I thought a few late comments might be in order. But – warning – I have also heard a few math/physics and Wall Street talks, so I might be confusing some of these talks with some of the private RenTech discussions, or with discussions with others who know the RenTech people.

RenTech has a large historical database of stock market price movements. The price movements have a lot of random noise, but also significant signals. The job is to identify the signals. It seems the technical people, who come from a variety of backgrounds and thus have different perspectives on how one identifies signals, basically are running an ongoing research project in which they put proposed signals into the model, and then test these against the historical data.

I know little about the details of what they do. They do mention signing confidentiality agreements. From either them or others I recall terms like scale invaiance, Wigner random matrix theory, and Markovian statistics. My impression was that they just tend to be a little smarter / faster than other people, rather than being simply contrarian. But a friend's impression was that they are simply better at hedging their risks, as opposed to really finding signals. So draw your own conclusions from this.

21. **Alex R**
July 5, 2005

M wrote: *The contrarian approach can evidently be successful if properly used, but one needs to be intelligent about it. … For example, I observe that nobody is building portable compact disc players with vacuum tube circuitry, but I also predict that if you decide to act contrary to this "majority opinion" and start building vacuum tube-based CD players that you won't make billions of dollars from it.* 🙂

While I, too, have problems with "contrarianism" as an investment philosophy (for example: what happens when *everyone* is trying to be contrarian?), I can't resist pointing out that even your off-the-cuff "observation" about vacuum-tube based CD-players is incorrect. (OK, strictly speaking, I don't know how many of these are portable, and I doubt anyone is making billions of dollars in this market, but the market is there.)

The point here is not so much that one should be contrary for the sake of being contrary, but that many people make unquestioned assumptions about things based on conventional wisdom or common knowledge, and that it can pay to *discover* what those unquestioned assumptions are (often the hardest part!) and to *question* the conventional wisdom they are based on.

22.
July 5, 2005

Yeah, right.

23. **Arun**
July 4, 2005

There are too many errors in the previous to correct on a blog devoted to Not Even Wrong physics theories. So I'll say no more.

24.
July 4, 2005

Arun wrote:

"Profit is the market economy's measure of the value of the good being provided" is simply wrong.

No. It depends on how you interpret "good being provided". Using your implied interpretation, the statement would be technically correct if it said "added value" instead of just "value". If you separate out "the good being provided" as what's added by the provider, and the rest of it (e.g. materials required to produce it) as just being passed along, the "added" is implied and the statement is technically correct as is.

Not that any of this silly marking of words is of any relevance to the point made.

Arun also wrote:
Secondly, there would be lot of profits being made by some traders if the USA had fifty currencies instead of one. The economic benefit of having one currency however outweighs all these profits.

Perhaps. So what? Given any set of currencies, it's better to have speculators trading them freely than not having speculators trading them freely. That's the point.

Arun the illogically added:
Similarly, it is not at all obvious that speculators are doing something useful

First of all, there is no similarity between the arguments; and secondly, it's quite obvious that speculators are indeed doing something useful, as previously pointed out. And, I may add, they are doing it of their own free will, with their own money, coercing nobody into trading with them – so what's your problem, exactly?

Arun continued:
or whether they are simply living off an inefficiency of the market economy

Elementary knowledge of economics would tell you that it's exactly the process of "living off inefficiencies", a.k.a. arbitrage, that minimizes those inefficiencies.

Wherfeafter Arun surprisingly did admit that:
The market economy is simply an optimizing machine.

The best one there is. And incidentally the only moral one, since it's the only one based on participants acting out of free mutual consent for mutual benefit. What more could you possibly ask for?

25. **Tony Smith**
July 4, 2005

M said "… speculative activity is … important in other fields like science or art, notwithstanding the many, inevitable dead ends …".

Is conventional superstring theory analogous to a dead-end speculative bubble ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

26. **Arun**
July 4, 2005

"Profit is the market economy's measure of the value of the good being provided" is simply wrong. The market economy's value for a good being provided is the price of the good. The value of a United Airlines flight is the same as the value of an equivalent Southwest Airlines flight – that is why the ticket prices cannot be substantially different. That one company makes a profit while the other is in bankruptcy is a measure of the different costs incurred by the respective airlines, and not a difference in the value of the good to the market economy.

Secondly, there would be lot of profits being made by some traders if the USA had fifty currencies instead of one. The economic benefit of having one currency however outweighs all these profits. As a matter of fact, the market economy is ***more efficient*** by having one currency instead of fifty. Similarly, it is not at all obvious that speculators are doing something useful, or whether they are simply living off an inefficiency of the market economy similar to that of having fifty currencies.

The market economy will also yield efficiences and profits in the sale of slaves or narcotics just as well as in derivatives. The market economy is simply an optimizing machine. The economic activity, profits, etc., or making hte optimizing machinery work better are not a justification for what we ought to or ought not to trade in, or whether some forms of trading have any social value or not.

27.
July 4, 2005

The improvement of speculative trading methods (e.g. by automation and more refined statistical analysis) means by definition that the speculator's business becomes more profitable. Profit is the market economy's measure of the value of the good being provided. Hence, the improvement of speculative trading methods increases the speculator's value to the economy.

See it like this: a bad (i.e. unsuccessful) speculator doesn't survive as a speculator for long, and can therefore not keep providing the aforementioned valuable services (risk reduction, liquidity, price discovery) to others. A successful speculator can; a more successful speculator can do it more, and more efficiently.

Just like technological competition between car manufacturers results in better cars for their customers, techonological competition between speculators results in better risk reduction, liquidity and price discovery for theirs (i.e. the "respectable" market participants).

28. **M**
July 4, 2005

Hi Dick and Anonymous,

Actually, I wasn't thinking about speculators in general, just the kind of rapid, computer-based trading systems such as what Sean mentioned Renaissance uses. I fully agree with you that speculators play a valuable role in the economy, provided it is not excessive (and speculative activity is likewise important in other fields like science or art, notwithstanding the many, inevitable dead ends).

On the other hand, my understanding is that this kind of computer-based speculative trading does not provide a service that didn't already exist — market makers of various kinds already have established their niches and seem to have efficiently performed their services to the markets for some time now. Thus, it seems that automated, proprietary trading systems that offer superior returns to a single investment company's customers do not provide a similar benefit to the economy as a whole, unless of course that company is the sole occupant of its niche. Without providing new value, the net effect of this strategy on the overall economy would basically be to redistribute existing wealth rather than create new wealth. This seems more like opportunism, and I don't see that as something to admire…

29. **carl**
July 4, 2005

As far as physicists being impressed by money, I recall an invited talk by S. Thorpe at a particle physics seminar on the subject of how he made $93 million.

That would have been about 1984 or 85. I read his book on black jack, so after this many years I can't recall if his talk included that subject, or was entirely on how he was making money based on options pricing.

Carl

30. **Andreas**
July 3, 2005

Of course, there are societal benefits to expect from speculators if society itself is perceived as a market. While many intelligent and respectable individuals indeed see society as a great market place, such a perception is a bizarre delusion of human reality. Unfortunately, this distorted vision leads global society into crisis.

31.
July 3, 2005

There are several well known answers to the question of market speculation's societal value.

1) Speculators assume financial risks in exchange for future returns (hopefully). Those risks don't come out of the blue; they come from the balance books of people who

do not want or can not afford them. By taking over those risks, speculators perform a service.

2) The presence of speculators in the market makes ir more liquid, improving its ability to accomodate long term investors and commercial operators when they need to perform market operations in the course of their respectable business.

3) Closely related to point 2: speculators improve the market's ability to function as a price discovery mechanism, i.e. a way to determine the current value of an asset you hold. If you are a commercial company or long term investor, you feel a lot happier knowing not only that there are many buyers and sellers of the stuff on your books, but also knowing at what price they are willing to trade right now, not a week or a month ago.

So yes, unlike string theorists (just to pick a random example) speculators perform very useful services for the economy and therefore for society as a whole.

32. **Dick Thompson**
July 3, 2005

M, you said

… The Code actually does the buying and selling, much faster than a human ever could. And it doesn't know, or care, what it is buying or selling; all it knows are the statistics of past performance.

The question that comes to mind is, where the value to society in that? No value is being produced except to those who invested in Rennaisance or work there (at least that I can see), and even there, such a strategy basically seems like a zero sum game.

The value that arbitrageurs, hedgers and such provide to the generality is making a market. Providing opportunities for others to buy and sell where they might not otherwise have had the opportunity. The fact that the code can do this at blinding speed is all to the good.

33. **Arun**
July 3, 2005

M, I too wonder. If there is, e.g., social value in trading currencies, currency futures, hedging against currency rate fluctuations, then the US could do itself great good by letting each state have its own currency, creating so much more opportunity to play these games. The Euro was then a step backwards and so on.

34. **M**
July 3, 2005

Regarding my earlier question about differences in viewpoint between physicists/mathematicians and the population as a whole in the respect/awe given to those who have accumulated great wealth:

It seems like most people who gave an opinion don't see a lot of difference between those who are highly educated in analytical fields like physics and mathematics and the general population when it comes to views about great wealth. There is good food for thought in the responses, at least for me. I especially liked Andreas' thoughts about the limited correlation between wisdom and intellect/education, and how that might limit significant differences from the general population; it makes sense to me.

I also agree with Walt that having $2.5 billion would eliminate the need to worry about income, potentially freeing up time to do physics or mathematics. But I think this potential is kind of an illusion. If you have that much money and you are like most people, your wealth is going to have a major impact on what you do with your time. If you are conscientious you will try to manage your money carefully, which takes time; even if you let others do the managing, you will still need to pay attention, and it seems like that would interfere with creativity. If you are more carefree, you will spend a lot of that money freely, and that too will be a distraction from intense, creative thought. Either way, it seems like great wealth would be more of a hindrance than a help to doing good physics or math.

However, I do agree that financial *independence* is desirable for doing good work for its own sake. Worries about money kill creativity and lead people to direct their energies to meet practical demands, like career growth or just getting by. As Peter and others have repeatedly pointed out, the need to follow what is popular just to survive is not healthy for science.

But… How much money is needed for that independence? Billions? Hardly — it is hard to see how anyone could productively use more than $100 million or so for their own needs. That would buy a *very* nice house, a yacht, maybe a private island, and leave plenty of money left over to live comfortably. I guess that is part of the reason I would naively hope that highly educated, analytical people might have less envy or awe of billionaires than the general population — they could see that beyond a certain point there is little practical advantage to having more wealth. Beyond that point, it seems like its main benefit is to keep score, to have bragging rights to making the most dough. It seems like there is no place for worrying about being in the really big financial leagues if a person loves science, the attainment of knowledge and understanding, and who strives to make a contribution there. (I am omitting philanthropy as a major issue. It is great when wealthy people spread it around, but I can't imagine that very many people become wealthy just so they can be philanthropists.)

Something Sean said about a strategy of Rennaisance Technologies disturbs me at another level:

…The Code actually does the buying and selling, much faster than a human ever could. And it doesn't know, or care, what it is buying or selling; all it knows are the statistics of past performance.

The question that comes to mind is, where the value to society in that? No value is being produced except to those who invested in Rennaisance or work there (at least that I can see), and even there, such a strategy basically seems like a zero sum game. Because there is no new value, wealth is merely being redistributed rather than created. Society as a whole does not benefit; only the names of the winners and losers change. I guess I don't see what is admirable about playing games like that, and even if the players are rich I feel differently about them than those who become wealthy by making society richer as a whole due to their efforts. (I'm referring to the movers and shakers, not the people who are just trying to make a living at what they do well.) This is in contrast to those who build businesses that create new markets, or long term investors who allow companies to grow and become strong, even if it's partly at the expense of weaker companies that will probably wither later anyway. Regardless of whether they are doing it to earn bragging rights, at least society gains value in the process. And if they make some serious money in the process, that's fine with me.

I wonder if I am in a small minority in these views…

35. **Chris Oakley**
July 2, 2005

Mathematics should be an extension of common sense rather than a replacement. This is something that most people wowed by mathematics PhD's in finance don't seem to appreciate.

The LTCM disaster was just maths/physics PhDs doing dumb things, the sort of dumb things that no-one who didn't have a cocky belief that he was smarter than everyone else would do.

And all Joe Jett was doing was arbitraging the internal accounting system. It was so ridiculously simple that Kidder/Peabody should have been, and probably were ashamed of themselves for allowing it to happen.

36. **Andreas**
July 2, 2005

M,

to realize that envy for wealth (i.e., economic power) and wealth worship are pointless requires a certain level of personal wisdom. Wisdom, however, is usually uncorrelated with intellect or education, much in the same manner as technology is uncorrelated with culture. Hence, I expect any highly educated individual in the analytic fields be as much susceptible to money as anybody else. For instance, I think of mathematicians like John Nash, De Branges and, surely, Jim Simons.

37.
July 2, 2005

In order to be a success at anything or make huge amounts of money you have to love what you do; in fact, love it so much that it's not actually a job. A lot of physics and math people are like that, myself included. I can relate to the guy who said he worked in a financial institution and hated it. I tried to get into financial math at one point since people I knew were doing it and wanting to go in that direction. I just found it incredibly boring. I am not saying it is boring–some people will get really into it and that's fine–but for me personally I could'nt do that stuff no matter how much I was being paid. I find it really soulless.

I was also under the impression though that the financial world is more wary of math and physics types due to a number of high-profile disasters in the 90s involving "rocket scientist" types,
like the LTCM meltdown and the Joseph Jett/Kidder Peabody affair.

38.
July 1, 2005

Yeah, unless the $2.5 billion and the people you hire to help manage it end up running your life and taking up most of your time. Some people can handle that kind of wealth and remain creative and happy, and some can't. I'm sure Simons took plenty of time figuring out where he fell in that spectrum.

39. **Walt Pohl**
July 1, 2005

M: I think it's an ordinary human impulse, not something that requires special explanation. Even if you only care about mathematics or physics, you could do more of either with 2.5 billion dollars than you could without.

40.
July 1, 2005

"Are people who are highly educated in analytical fields like physics and mathematics similarly susceptible to the wealth envy or wealth worship?"

Of course, some are and some aren't. When I was younger, at the insistence of my supposedly intelligent family and friends, I got a job at a financial institution dealing in the fixed interest derivatives market. I worked with another mathematically trained individual. I started on $40 000 a year. I estimate that he was on about $500 000 a year. He made the company lots of money. I hated every minute of it. I lasted a year and a half – I don't know how. I left on the dot of 5 o'clock every day, and the only people I was friendly with were the secretary and a woman in accounts. I'm guessing that I was the only technical staff member that was ever expected to actually stay for the one month after I gave notice, because they had no doubts whatsoever that I wasn't simply moving to another investment firm.

Now I'm a little older, and probably no wiser, but I don't let people boss me around any more. I know many in Maths and Physics like myself. Then again, the guy in the office next to me is currently applying for jobs in banks. It takes all types to make the world….

41. **Quantoken**
July 1, 2005

Mr. M:

Doing something other than what majority of the gang is doing is ONLY a necessary condition for success, NOT a sufficient condition. Clearly if you follow the gang and do identical things the end result could only be failure, not success.

Now you ask why not build vacuum tube based CD players, just because no one else does? Well if you look around this world there are too many of this kind of hyperthetical "no body else does" things to enumerate. You could not try all of them.

Actually vacuum tubes are still being used today as **advanced technology**. The largest vacuum tube is 25 miles long and burned underground in Geneva. They are still building it and it's called LHC. It's very different from your TV tube, certainly, but it is a vacuum tube nevertheless and it works on the same physics principle with allow particles to travel in the vacuum and electromagnetic fields are used to accelerate them and bend them. You might ask maybe you can build an accelerator without the vacuum tube, just like you can build a CD player without vacuum tube.

Quantoken

42. **JC**
July 1, 2005

M,

The first "group" in your categorization would perhaps fit into the background of somebody like an Einstein and/or Paul Erdos. Only other people I can think of who could possibly fit into a similar type of categorization (albeit in a different field), would perhaps be people who take a vow of poverty and enter the clergy.

43. **M**
July 1, 2005

This topic has the potential to generate a lot of interesting comments, so I hope Peter will relax his standards a bit for being "on topic" and let well thought out comments remain. It appears that the financial industry is an important employer of physicists and mathematicians, so this seems like a useful topic.

I have what is more of a sociological question about perceptions of mathematicians and physicists about wealth and wealth creation, if anyone knows. The basic question: Are people who are highly educated in analytical fields like physics and mathematics similarly susceptible to the "wealth envy" or "wealth worship" that the general population seems to have? For example, Peter's mention of Jim Simons as managing a $5 billion fund and having $2.5 billion in personal wealth would undoubtedly evoke awe in a very sizable fraction of the population, regardless of whether he donated any of it for increasing human knowledge. Would physicists and mathematicians have that same awe in similar proportions?

If mathematicians and physicists are similarly awed, a secondary question is, "Why?"

I can speak with more understanding of physicists, but perhaps the comments apply equally well to mathematicians. On the one hand, many of us are attracted to physics for idealistic reasons. We want to make a contribution to human knowledge, and hopefully make society better as a result. We also like the challenge of difficult problems and enjoy the excitement of making discoveries or gaining insight. Perhaps a relatively small subset of this group is driven by a deep need to understand and make sense of the physical world; this goes beyond just doing something that is fun and interesting. Certainly the route to becoming a physicist involves a lot of hard work, and hopefully most realize that in the end there is little likelihood of becoming fabulously wealthy, and hopefully most realize that employment in the field is definitely not guaranteed.

There also seems to be another fraction who see physics and mathematics as very challenging fields that are beyond the ability of most of the population to become active participants. Thus, by entering these fields they can not only do something they are good at but they can (perhaps unconsciously) also impress others by their mental acuity, thus gaining respect (and maybe even some awe) of others.

I guess I would expect the second "group" to be more likely to have great respect for a very wealthy person, simply because these people place some premium on success according to traditional societal standards. But what of the first group?

44. **M**
July 1, 2005

From Quantoken:

The secret is do the EXACT OPPOSITE thing of what other people would be doing. Make sure you are always the minority. That way, you make money?

The contrarian approach can evidently be successful if properly used, but one needs to be intelligent about it. Just because you're in the minority doesn't mean you'll make money, just as the early bird doesn't always get the worm (especially if the worm starts roaming around *after* the bird has hunted for its meal). For example, I observe that nobody is building portable compact disc players with vacuum tube circuitry, but I also predict that if you decide to act contrary to this "majority opinion" and start building vacuum tube-based CD players that you won't make billions of dollars from it. 🙂

45. **Dick Thompson**
July 1, 2005

The contrarian strategy is natural for a hedge fund like Renaissance because the idea of a hedge is to invest in something that will pay off if the main investments go sour. I saw the word commodities pop up in these comments. Hedging commodities is a big deal. For example if you are a technology industry with a need for gold in your processes, then you will want to hedge against price swings in gold. You will play around with futures, other metals, things that historically go up when gold goes down and vice versa.

46. **JC**
July 1, 2005

– Sean,

Sounds like an exercise in "data mining". The biggest problem is determining whether a correlation in a data set is just spurious (and meaningless) or if there's a genuine causality with particular "events". Many spurious correlations seem to come and go in a "fly by night" manner.

– Quantoken,

This is better known as the strategy of being a "contrarian". Though being a contrarian isn't always the easiest strategy either, especially if one is into short selling overvalued stocks.

47. **Sean**
July 1, 2005

I visited Renaissance once to give a colloquium. A fun place, full of smart ex-scientists making boatloads of money. The basic idea is to have a computer program ("The Code") look at past performance of various commodities, and use that info to guess what will happen next. The Code actually does the buying and selling, much faster than a human ever could. And it doesn't know, or care, what it is buying or selling; all it knows are the statistics of past performance.

Of course, going from past performance to future behavior is highly nontrivial. That's what they aren't able to talk about. Some day almost all trading will be done by dueling algorithms (or is that already true?).

48. **Quantoken**
July 1, 2005

Oh no, Chris Oakley. "Buy low, sell high" is NOT the secret. It is not followable, most times the market will force you or allure you to do exactly the opposite thing, because market can not be forecasted, without future forecast, you really don't know what is high and what is low.

OK, here is the real secret. I do not need to kill you. But I bet majority of the audience here would not be able to follow it, except for a few lucky ones. Ready?

The secret is do the **EXACT OPPOSITE** thing of what other people would be doing. Make sure you are always the minority. That way, you make money?

Why? Because no system could support a model where the majority of people, 99%, becomes billionares, and only a minority, 1%, go bankrupt. **The only plausible model that's sustainable is one where 99% of people lose money and 1% becomes billionaire**.

So if you see 99% of people doing the same thing and the other 1% doing the opposite, you can bet that 99% is a losing deal and you should join the 1%.

The slogan is be crazy and be different from an average person.

Quantoken

49. **Chris Oakley**
July 1, 2005

Actually I know the strategy that Simons is following. I am going to share it with readers of this blog, but if I discover that anyone has leaked it to any outsiders, then I will personally come round and kill them.

Are you ready?

OK – here it is: *Buy low, sell high*

50. **JC**
July 1, 2005

The fund's MO isn't that surprising. It's very well understood that as soon as "everybody" in the financial world knows about a particular trading strategy, its effectiveness at producing "above average returns" on investment greatly diminishes.

Various and Sundry

July 6, 2005
Categories: Uncategorized

One of the big experimental HEP conferences, the Lepton-Photon Symposium, has just ended and many of the talks are on-line. This is the 22nd of these conferences which happen every two years. New data from the Tevatron about the top quark was discussed, and a paper with the new top quark mass results has been released.

There's a new web-site with news about the LHC.

Last month there was an Einstein Symposium in Alexandria, and presentations are on-line. They're for the general public so pretty content-free, but it is interesting to see what Witten's latest view of string theory is: "I'd like to believe — but of course I don't know — that string theory is on the right track…" Michio Kaku begins his presentation with advertisements for his books, then tells the audience that testing string theory would require creating a "baby universe", that "Mind of God = music resonating through 11 dimensional hyperspace" and that the standard model is "supremely ugly" (which strikes me as something supremely stupid to say).

If you can't wait for next week's Strings 2005 in Toronto, there's a summer school on strings going on at Perimeter, and a meeting in Crete that just ended, along with many more string conferences to come. The one series of talks I won't be able to make it to, but would love to hear would be Graeme Segal's talks in Oporto on 2d QFT.

The DOE has just announced the award of seven new Outstanding Junior Investigator grants in high-energy theory and experiment, one of which is going to Lubos Motl.

Comments

1. **Matthew**
   July 13, 2005

   OK – so to answer Arun's original question [is there a purely Quantum Field Theoretical treatment of the hydrogen atom], the answer is "No",

   Not to be combative, but I pointed you to one, effective field theory methods. Indeed this is the *sane* QFT treatment, since you have things in the problem you can treat exactly.

   Put another way, it doesn't make much sense to restrict your "pure" QFT treatment of the proton to

   S = \bar\psi (Dslash – m) \psi

   as your action. You know you're in the non-relativistic limit, it makes sense to use that fact to the maximum.

2. **Chris Oakley**
   July 13, 2005

   OK – so to answer Arun's original question [is there a purely Quantum Field Theoretical treatment of the hydrogen atom], the answer is "No", you're better off with your undergraduate text books.

3. **Peter Woit**
   July 13, 2005

   There's very good evidence that asymptotically free theories like QCD are non-trivial in the continuum limit. If you ignore fermions, the lattice Monte-Carlo calculations work quite well. There's a mass gap and the extrapolation ot zero lattice spacing and infinite volume look fine. Non-asymptotically free theories like QED are the ones that appear to have a problem in the contiuum limit, since they are becoming strongly coupled at the cutoff scale.

4. **Chris Oakley**
   July 13, 2005

   The notion that any RQFT becomes free in the limit of lattice spacing going to zero ties in with other results, Haag's theorem in particular. People seem to be prepared to do the most amazing things, such as abandoning special relativity, to avoid accepting this.

5. **Peter Woit**
   July 13, 2005

   You can certainly formulate QED on a lattice, and even do perturbative calculations (although they're a lot harder since your regularization is not Lorentz invariant). The lattice technique that doesn't work well for QED is that of Monte-Carlo computer calculations on a finite lattice. These work best in theories with a mass gap, since finite-size effects will go away for lattices much bigger than the inverse of this mass.

   As far as I know, no one has really understood what happens non-perturbatively to QED, whether regularized by a lattice or by any other method. By analogy with phi^4 theory, the fact that it's not asymptotically free suggests that no matter how you try and take the continuum limit, you'll end up with a non-interacting theory. Whether this or something more interesting happens is not known as far as I'm aware, but I haven't followed work in this area, maybe more is known.

6. **Matthew**
   July 13, 2005

   Is this not just saying that lattice techniques simply do not work for QED?

   No, it's saying you have to be careful.

7. **Chris Oakley**
   July 13, 2005

   …at finite lattice spacing QED confines just like QCD. It has been demonstrated (numerically at least, I don't think it's been proven) that QED has a phase transition as the spacing goes to zero, which takes it to a non-confining phase.

Is this not just saying that lattice techniques simply do not work for QED?

8. **Matthew**
July 13, 2005

It may be worth listing out the highest-precision tests of each sector of the Standard Model and of General Relativity.

QED — electron g-2
Electroweak — LEP precision data
QCD — Running of \alpha_s or evolution of structure functions
GR — Hulse Taylor Binary pulsar timing data

For the electroweak theory, I'm not really sure there's a single "showstopper" number. But the LEP data as a whole is impressive confirmation of the standard model.

For QCD, you can measure \alpha_s in a number of different experiments, and run each value to a common scale. The fact that they all agree when run to this scale is strong evidence for QCD.

There's also the evolution of structure functions in deep inelastic scattering (i.e. deviations from Bjorken scaling). This is impressive evidence, but I'm not sure if it relies on any "non-QCD" input.

9. **Matthew**
July 13, 2005

In regard to the the first two: no doubt, but this is not actually a quantum field theoretical treatment of bound states. It is more like saying, "If we could do bound states in QFT, then we would expect the following".

It's an effective field theory analysis. It's more rigorous than just guesswork, it's a systematic expansion. These are used all over modern particle physics.

It's as field theoretical as using a Schwinger-Dyson or Bethe-Salpeter type approach, just different.

In regard to lattices, is it actually feasible to do lattice gauge theory for QED in the same way as one does it for QCD (& I am talking about QFT rather than QM) – ?

Yes, but there are some subtlties peculier to QED. The major one is that at finite lattice spacing QED confines just like QCD. It has been demonstrated (numercially at least, I don't think it's been proven) that QED has a phase transition as the spacing goes to zero, which takes it to a non-confining phase.

From a practical standpoint, I'm not sure how hard this makes it to do QED on a lattice. People do do it though.

IIRC there was some interest in 2+1 dimensional QED in the condensed matter community.

10. **Arun**
July 13, 2005

It may be worth listing out the highest-precision tests of each sector of the Standard Model and of General Relativity.

11. **Chris Oakley**
July 12, 2005

Hi Matthew,

In regard to the the first two: no doubt, but this is not actually a quantum field theoretical treatment of bound states. It is more like saying, "If we could do bound states in QFT, then we would expect the following".

In regard to lattices, is it actually feasible to do lattice gauge theory for QED in the same way as one does it for QCD (& I am talking about QFT rather than QM) – ?

12. **Matthew**
July 12, 2005

The usual answer is that one uses the Bethe-Salpeter equation to do bound states in QFT, but one has to ignore the infinities detonating all over the place, an inconvenience not present in the first-quantised treatment of the one-electron atom.

You can also use a non-relativistic effective theory (NRQED) properly matched to relativistic QED. The Lamb shift has been done this way.

Also, no-one has ever satisfactorily answered my question as to how a technique that only covers scattering processes can be used for bound states anyway.

There are a few techniques. The effective field theory analysis is probably the clearest. For an example of what you can do with this approach there's http://arxiv.org/abs/hep-ph/0003277.

Then of course there is Lattice Gauge Theory. The last time I enquired – relatively recently – I was told that no-one has used the technique to calculate the Hydrogen atom. I am surprised at this as I would have thought it would provide a useful check, especially as they obviously do not balk at the much harder problem of q-qbar bound states.

It's not a terribly useful check since lattice QCD is much more complicated than lattice QED. You can do the H atom using (numerical) path integrals in Quantum mechanics though. That's a fun undergraduate level excercise. However, lattice field theory is not a precision tool. You could easily do the H atom, and get 5/10% accurate results. To get a ppm/b determination of the lamb shift would be very hard.

13. **Alejandro Rivero**
July 11, 2005

Some of these checks are to ten digit accuracy

Well, as I have remarked in other places, by using quenched QED and some experimental data an efriend and myself got to fit -or to fake- six or seven of these same digits in hep-ph/0503104. I guess the real value of QFT is about the generality of its application, and not about a particular, experiment driven, quantity.

14. July 11, 2005

James, the problem with QCD is essentially asymptotic freedom; as momenta grow smaller and distances larger, the coouplings between quarks grow *stronger*. So you can use the well-developed techniques of perturbation theory to compute the outcomes of processes where quarks are banged into each other at very high energy, but the "perturbations" grow too large to handle that way when you try to compute the behaviour of quarks assembling into stable, bound states involving small average momentum transfers. (And as if that weren't bad enough, existing techniques for handling bound states in QFT are quite unsatisfactory; I have little doubt that good progress on that front would open up whole new frontiers for detailed parton model building – now there's something *useful* for all those talented mathematica physicists now slaving away at strings to look at!)

Not being able to use perturbation theory means having to solve a much, much harder problem; in practice you can only do it numerically, with lattice techniques, and then you run into all sorts of technical problems, starting from hardware limitations (a 4-dimensional lattice eats up memory and CPU time very quickly as you increase its size – double the linear size and you get 2^4=16 more points – and ideally you want to be able to take the limit of zero lattice spacing…).

Pentaquarks are such a non-perturbative, and therefore computationally (but maybe not conceptually, depending on how you want to draw the line between the two) hard problem. The 10-digit-precision claims which you quote on the other hand are from realms amenable to perturbation theory.

15. **James Graber**
July 11, 2005

Peter Woit wrote, in response to my comment, ?It's just bizarre to talk about "how unpredictive the standard model" is.? Once again Peter, thank you for your response. Well Peter, you know this stuff and I don?t, so I?m just going to have to take our word for it, or spend years studying it. And I have heard and read that ten digit accuracy claim many times. That?s why the inability to predict or even agree on the interpretation or even existence of whole classes of particles at this late date seems so bizarre to me. Once again thank you.
Jim Graber

16. **Peter**
July 10, 2005

It's just bizarre to talk about "how unpredictive the standard model" is. It makes an infinity of predictions, and every single one that experimentalists have been able to check comes out correctly within experimental errors. Some of these checks are to ten digit accuracy. It's probably the most predictive of all scientific theories known to man. Every particle physics experiment done during the last 30 years has generated reams of data exactly predicted by the standard model. Pick any experimental high energy physics conference, look at the data reported and its precise agreement with standard model predictions.

You seem to not understand the difference between not being able to predict everything (standard model) and not being able to predict anything (string theory). There are some things the standard model inherently can't predict, like fermion mass matrices, others it can predict in principle, but our calculational methods are not good enough to extract the answer. If the standard model predicted everything and we knew how to extract every prediction, high energy physics would just close up shop.

If you want to understand which things the standard model inherently can predict and which it can't, that's not very hard, just learn exactly what the theory is. If you want to understand which things are easy to calculate and which are hard, you have to spend some time really understanding what the known calculational methods are in QFT, and what are their limitations. This takes some serious work.

But it's just completely absurd to claim that there's any similarity between string theory and the standard model from the point of view of predictivity.

17. **James Graber**
July 10, 2005

Peter,
Thank you for your response to my comment. I was not trying to imply that neutrinos were directly related to pentaquarks. My main point is the surprising (to me, at least) lack of predictivity of the standard model. Not being an expert on either the standard model or on string theory, I am continually surprised at how unpredictive both of them are. I will leave criticism of string theory to you and others. I realize that the standard model is not ?not even wrong? because with solid evidence of neutrino oscillations, and thus indirect but solid evidence for neutrino masses, ?everyone? agreed that the standard model needed to be changed and expanded.

(By the way, I was somewhat astounded at how long acceptance of this change took, but that?s a different issue. I am also amazed at the apparent preference for Majorana over Dirac masses. No majorana particles are yet known, so why should neutrinos be Majorana? Of course, it would be cool if they were, but it would seem like Dirac is a much more likely bet.)

And also, people think that by accurately measuring the top quark mass, they can partly predict the Higgs mass. But on the other hand, the pentaquark or the tetraquark can exist or not and the standard model seems fine either way. This seems very unpredictive to me. I have wondered about this for some time. My comment was triggered not just by the recent press release concerning Jlab?s failure to confirm the pentaquark, but also by hep-ph/0507025 which I just happened to read. In addition to your response, another anonymous poster also suggested the reason for this lack of predictivity was the difficulty of solving QCD. I can accept that, but I still wonder why some predictions are possible and others are not. It makes the standard model seem much shakier than it is usually presented as being.
Jim Graber

18. **Arun**
July 10, 2005

Chris,

Thanks! Do you think we can count this as one of the "mass of unsolved problems" that physicists have left behind "in a pursuit of an *a priori* vision of what a simple world would look like" ?

-Arun

19. **Chris Oakley**
July 10, 2005

Hi Arun,

The usual answer is that one uses the Bethe-Salpeter equation to do bound states in QFT, but one has to ignore the infinities detonating all over the place, an inconvenience not present in the first-quantised treatment of the one-electron atom. Also, no-one has ever satisfactorily answered my question as to how a technique that only covers scattering processes can be used for bound states anyway. Then of course there is Lattice Gauge Theory. The last time I enquired – relatively recently – I was told that no-one has used the technique to calculate the Hydrogen atom. I am surprised at this as I would have thought it would provide a useful check, especially as they obviously do not balk at the much harder problem of q-qbar bound states.

20. **Arun**
July 10, 2005

Question born of ignorance: is there a purely Quantum Field Theoretical treatment of the hydrogen atom?

21. **Peter Woit**
July 10, 2005

Neutrinos have nothing to do with pentaquarks. For no obvious reason you're conflating two of the unsatisfactory aspects of the standard model: the fact that it doesn't predict fermion mass matrices, and the fact that one doesn't know how to exactly solve QCD. If one could exactly solve QCD in the infrared, this should tell you whether or not pentaquark states exist and what their properties are.

Actually this will be a good test of the idea of using string theory to solve QCD. If anyone ever comes up with a workable string theory dual to QCD, it should predict what happens with pentaquarks.

22.
July 10, 2005

Jim Graber wrote:
Why can?t the theoreticians even agree on a prediction?

Because non-perturbative QCD is pretty damn hard to solve? (And no, while I'm certainly no expert, I very much doubt that neutrinos, whether massive or not, can be relevant to pentaquarks.)

23. **James Graber**
July 10, 2005

Perhaps this ?Various and Sundry? thread is a good place to enter this question, which I have been wanting to ask for some time.

The pentaquark has been searched for for over twenty years. Recently, the pentaquark seems to have reappeared and then disappeared, but other weird particles, or particles with weird decay modes are still being seen. The arxiv is full of conflicting explanations for these particles, mostly based on the standard model. Of course, the old standard model, 321Z with _Zero neutrino masses has just been replaced with two competing new standard models, 321M with _Majorana neutrino masses and 321D with _Dirac neutrino masses. So does any of these models predict the pentaquark or not? It seems that the standard model of particle physics (SMOPP or just SM) is almost as non predictive as string theory (ST) at least as far as pentaquarks are concerned. Or maybe it does predict the pentaquark (albeit broader than the recent mirage) and is just plain wrong.

The experimentalists once again seem to be converging on the position that the pentaquark does not exist at any significant level. Why can?t the theoreticians even agree on a prediction? Or is the absence of pentaquarks and tetraquarks some kind of ?superselection rule? to be axiomatically included in the SMOPP?

Jim Graber

24.
July 9, 2005

"To the men and women who create the accelerators, the detectors and the experiments from which the concepts of particle physics spring."

Ah yes, it's in "Concepts of Particle Physics" (Volume II, at least), by Kurt Gottfried & Victor F. Weisskopf. Not so recent though (1986). I remember being struck by that dedication, many years ago, as quite paternalizing for all its political correctness. It's nice to see that I evidently wasn't the only one.

25.
July 9, 2005

Joseph Schwartz in *The Creative Moment : How Science made itself alien to Modern Culture*. Please, oh please shoot the following down.

"In the late Victorian period Planck was an unusual physicist. Physics then was like biology today, with theorists being objects of scorn for their lack of contact with experimental realities. Other physicists from the period who today are known for their theoretical work were skilled experimenters. Gustav Kirchoff (1824-1887) is known now for his theoretical contributions to heat radiation and the analysis of electrical circuits and not for his experimental work with George Bunsen on optical spectra. Maxwell, famous today for his partial differential equations of the electromagnetic field, was professor of *experimental* physics at Cambridge. H.A. Lorentz, the first theoretical physicist in Holland, was an active experimentalist doing work in optical spectra. Max Born, one of the leading pioneers of quantum theory, was an expert in experimental optics.

And Einstein, known as the foremost theoretician of all time, was experienced in laboratory techniques and interested in technology. With the Habicht brothers he patented a precision volt meter in 1914. In the 1920s, in collaboration with aerodynamicist Rudolf Goldschmidt, he invented a hearing aid. With the Dutch firm, N. V. Nederlandsche Technische Handelsmaatschappy he held a patent for a gyrocompass. And with Leo Szilard he patented several refrigerating devices designed to reduce the noise levels of existing commercial machines.

But today the physics community is deeply divided between the theorists and the experimenters. {resulting in a two class system, with theorists at the center and experimentalists circling around them}

Sensitive theoreticians have been careful to soothe ruffled feathers by making it a point to acknowledge the role played by experimental work. A recent textbook concentrating on theoretical developments is dedicated to the experimentalists, "the men and women who create the accelerators, the detectors and the experiments from which the concepts of particle physics spring".

Although these words are a much-needed acknowledgement of the fundamental source of physics in observation, they nevertheless confirm the existence of inequality. Their unavoidably patronizing connotations are reminiscent of the dedications made by male professionals of all kinds to their wives and secretaries…..

The potentially explosive tensions caused by the inequality between experimental and theoretical physicists are kept within bounds by an exceptionally strong belief in inherited intelligence. Experimental physicists accept second-class status because they feel they are not as bright as theorists. Theorists take their superiority for granted as due recognition of superior intelligence. Physics meetings are famous for their stilted, tense atmospheres as each person is afraid of asking a “stupid” question. Even informal contacts can be dominated by a competitive proving of who understands more of a subject under discussion.

{As a result of the necessarily industrial size undertaking that experimental particle physics has become} “theorists have become an elite within an elite with little or no accountability to an outside audience. There is the occasional theoretical briefing “to the experimentalists”, but in the main, theoreticians have, not unnaturally, become ingrown in their approach to physics.

…Theorists today are so divorced from real experience of nature they have no unconsciously absorbed perceptual knowledge to draw upon. Everything they know has been learned from books. And the understandings based on this secondhand knowledge have inevitably been derivative and second-rate.

—

“Once one believes that spin is a consequence of the Dirac equation, it is only a short step to try to make physics out of the thin air of mathematical guesswork….

{Previously}

“Dirac, perhaps because of his engineering training, was one of the few physicists who remained clear about where things came from. In 1962, Thomas Kuhn…spoke to Dirac about his life and work. Among other things, Kuhn was interested in how Dirac came to write down his equation for a relativistic electron with spin.

Dirac said: “I was playing around with equations and I found that Pauli’s matrices were quite a nice thing to play with. It needed quite an effort to make the further generalization (from 2×2) to 4×4 matrices, but that work did come about from playing about with the three dimensional scalar product.”

Kuhn said: “Was it a surprise that what came out were spin terms?”

Dirac said: “No, I don’t think so. Because one had the Pauli matrices in it [to begin with.” }

…what students of the 1950s were not taught was the understanding of James Franck, one of the leading experimentalists of the 1920s: “What one doesn’t put into the equation will not finally be given by the mathematics”. In its place, students absorbed a theoretical sensibility inspired, not by an attempt to understand, express, and describe, or in Bohr’s word, communicate real physical experience, but by spectacular deductions from a few well-chosen equations.…

{resumes} “The patterns of thinking that now dominate theoretical physics approach the classical definition of autistic thinking: thought that is solely determined by the subject’s wishes and fantasies without reference to the environment or to realistic considerations of space and time”.

26. **Alejandro Rivero**
July 9, 2005

About strings 2005, any clue about these axions from Witten? Are they still related to a possible two-higgs doublet (remember it is also my bet 🙂 ?

27. **D R Lunsford**
July 8, 2005

LM has never had a single idea about physics that amounted to pouring piss from a boot, and our wonderful, wonderful academic structure rewards him this way. THAT is why string theory, a vile and idiotic lie, has managed to slough itself along for 20 years, leaving a shiny trail behind it.

-drl

28. **Peter**
July 8, 2005

While I like Chris’s interpretation, I suspect that what Kaku had is mind was that in 1968 what was discovered was not a theory, but a formula for amplitudes. Only later was it understood that these amplitudes come from a (first-quantized) string theory. The continuing hope is that the current (inconsistent and unable to reproduce physics) version of string theory is an approximation to some wonderful but yet to be discovered M-theory.

29. **Chris Oakley**
July 8, 2005

Does anyone know what Kaku meant by “String theory has been moving backwards”

I presume that what he means is that the more they work on it, the more that they realise that it is not going to be of any use as a physical theory.

30. **pseudo string fan**
July 8, 2005

Hi,

Does anyone know what Kaku meant by “String theory has been moving backwards” in his PowerPoint statement

= String theory has been moving backwards, since it was accidentally discovered in 1968.
?

31. **Alejandro Rivero**
July 8, 2005

currently accepted

is not different of the typical forms to request funds in some project, where you are basically asked what are you to discover, and when. I call this part of science, very botanic-wise, the “classification” side. The (also botanic) counterpart, “exploration”, is always more problematic. Smolin article on “New Einstein” was about this, wasn’t it?

32. **Peter Woit**
July 7, 2005

I’m tempted to delete the previous comment, but am leaving it since I think that, if accurate, it is interesting to see that the editor of PRL is resorting to an indefensible argument in dealing with nonsense submitted to him (although the “…” may hide a more defensible argument). Please discuss this with the author of this comment on his weblog, not here. I'll be deleting any further comments about this.

33. **Nigel**
July 7, 2005

Editor of Physical Review Letters says

Sent: 02/01/03 17:47
Subject: Your_manuscript LZ8276 Cook
MECHANISM OF GRAVITY
Physical Review Letters does not, in general, publish papers on alternatives to currently accepted theories ? Yours sincerely, Stanley G. Brown, Editor, Physical Review Letters

Now, why has this nice genuine guy still not published his personally endorsed proof of what is a ?currently accepted? prediction for the strength of gravity? Will he ever do so?

?String theory has the remarkable property of predicting gravity?: false claim by Edward Witten in the April 1996 issue of Physics Today, repudiated by Roger Penrose on page 896 of his book Road to Reality, 1994: ?in addition to the dimensionality issue, the string theory approach is (so far, in almost all respects) restricted to being merely a perturbation theory?. String theory does not predict for the strength constant of gravity, G! However, the Physical Review Letters editor still ?believes in? Edward Witten and Physics Today.

http://einstein157.tripod.com/

34.
July 7, 2005

There was a short presentation on E Witten on CNN recently. And no, it's not the one that's really on Jim Simons. Seiberg, Nappi and Kaku were interviewed and asked what they think of Witten. Witten's son was also interviewed. I think it was nice.

35. **Alejandro Rivero**
July 6, 2005

I can not but agree with DOE about labeling Lubos as one of the Outstanding Junior Investigators in the USA research network. I can not tell if I am happy about it. In any case, at least Lubos is worried about the spectrum.

36. **Chris Oakley**
July 6, 2005

Kaku, slide 45:

The Unfinished Theory

= String theory has a new Picture: strings and membranes.
= String theory lacks a physical principle, like the equivalence principle.
= The full symmetry and mathematics of string theory are also unknown.
= String theory has been moving backwards, since it was accidentally discovered in 1968.

I am not sure how these observations lead one to the conclusion that String theory is a good thing to work on. Could someone please explain?

Strings 2005

July 10, 2005
Categories: Strings 2XXX

Strings 2005, the latest in a series of yearly huge string theory conferences, will be taking place this week in Toronto. This series began in 1997 in Amsterdam, and in recent years has attracted 445 participants to Cambridge in 2002, 392 to Kyoto in 2003 and 477 to Paris last year. So far there are about 415 people already signed up, so it looks like this year's conference should be similar in size to ones of the last few years.

I expect that some string theory bloggers wil be reporting from the conference. In particular Jacques Distler will be there, chairing a session that should include two of the loonier talks of the conference (Kachru and Douglas on the landscape), and presumably we'll be hearing from him. Last year there were several people reading "Not Even Wrong" on their laptops using the wireless connection in the lecture hall in Paris, this year I hope anyone there who doesn't have his or her own weblog will let us know what is going on by posting comments here.

The conference will end next Saturday with a public lecture by Lenny Susskind. His talk has the same title as his forthcoming book on the landscape pseudo-science. The theme of the public lectures is listed as: "If String Theory's the Answer, What's the Question?"

Update: Slides from the conference have already started to appear, including Ooguri's survey talk on topological string theory, one of the few subjects in string theory which seems to still be alive. Ooguri makes a valiant effort to try to answer the question "If topological string theory is an answer, what is the question?" He does answer the question "If string theory is an answer, what is the question?", but the answer is disappointing: "What is string theory?"

Jacques Distler is blogging from the conference. In his coverage of this morning he ignores the topological string theory stuff and describes Eva Silverstein's talk. She seems to me to be getting into Bogdanov territory with an obscure mechanism that somehow is supposed to say something about the initial singularity of space-time. Jacques says he doesn't really understand this, and I'm in agreement with him there.

And my logs are starting to show some connections from user37-*.wireless.utoronto.ca. Hi guys! Come on, there are at least nine of you reading this from the lecture hall, so at least one of you can tell us what is going on. String theorists seem to prefer Macs, so far the wireless connections are coming from 6 different Macs and 3 different Windows machines.

Comments

1. **FineStructure137**
   July 13, 2005

   Thank you everyone for your valuable comments. Thanks to Peter for pointing out that I need to look at QFT and the standard model further and "anonymous" for taking time to find the links of the CDT papers.

   Originally, I had an inclination to look at string theory as I was told that string theory is the only way to explain nature. When I asked what if its wrong, the string theorist I talked to explained that the contribution that it had made in mathematics is already so incredible that even if the whole thing turns out to be wrong its contribution would not go as a waste.(not a very satisfactory answer).I dont consider that I have reached the stage where I can declare to myself what is right and wrong. But I dont want to be a string theory fanatic (for that matter any theory) and hopefully be able to look at all the things with proper guidance.

2. **Mike Crowley**
   July 13, 2005

   Hi FineStructure,

   I'm also trying to tackle this material on my own (even if it takes ten years). That is impressive you were able to do Polchinksi and Zwiebach. I took one look at Polchinksi and realized there were about six other subjects I had to learn first.

   I wish you luck on your studies.
   Mike

3.
   July 13, 2005

   You write:"Jacques Distler … describes Eva Silverstein's talk.
   She seems to me to be getting into Bogdanov territory.."

   I wonder…have the Bogdanovs ever dared to give a
   talk about their 'scientific work' with experts as audience?
   At a conference?

4. **D R Lunsford**
   July 12, 2005

   "All I want to find is the truth" said a youngster. Yes, we were all like that. Sooner or later you've got to develop some ability to think for yourself.

   -drl

5.
   July 12, 2005

   dan: *will you be blogging loop 05?*
   friendly question, encouraging of you to ask!
   October is too far away to say for sure.
   Probably not, but hopefully someone will be.

6. **dan**
   July 12, 2005

thanks for the discussion “at”.

sounds very promising.
will you be blogging loop 05?

7. **Urs Schreiber**
July 12, 2005

By way of another shameless self promotion let me just mention that two news correspondents of the String Coffee Table are currently liveblogging at least some of the Streetfest.

8. **Matthew**
July 12, 2005

By way of shameless self promotion let me just mention that I intend to liveblog at least some of lattice 2005 (the Plenary sessions), starting July 24th.

9.
July 12, 2005

dan:*do you think, given its good semiclassical limit, there will be an exodus from LQG and spin foams to CDT? (i.e smolin thiemann markopoulou?)*

I will tell you what I think. You may look at the same bunch of researchers and see quite a different picture. In my view, (non-string) Quantum Gravity is a single research community and divsions within that are fairly fluid. Loll has written LQG papers, both Smolin and Markopoulou have written CDT papers. The spin foam sum over histories approach has some resemblance to the CDT path integral.

the people who do (nonperturbative background independent) Quantum Gravity are allied by having similar concerns and familiarity with each others’ methods—you might think of the different methods represented at the Loops 05 conference as a menu or as a repertory, and anyone who wants to do nonperturbative quantum gravity can choose from that menu of tactics.

Probably this year Loll’s new results will attract a lot of attention and the others will see if they can get similar results (with causal sets technique or spin foam) or if they can adapt CDT to what they do, or they will move over and try CDT approach proper.

Loll is just beginning to do black holes with CDT, and there are others who seem further along with that. Loll is also just beginning to include controlled topological variation in the path integral (so that it sums over topologies instead of just over the geometries of a fixed topology). People who use other methodology will want to know about that.

Collectively the (non-string) Quantum Gravity community is in a period of rapid growth, each year more people and more papers. I presume that will continue. I cant predict much about the demographic shifts WITHIN that community because people change back and forth and may even work several lines.

10. **dan**
July 11, 2005

hello “at”

i read the links on loll’s CDT, and i see a claim is made it has a good semiclassical limit (unlike LQG, spin foam, etc)

while i understand LQG is based on asketar’s new variables quantized version of GR, it’s not clear to me how CDT starts and how it quantizes GR, what sort of predictions it makes at high energies/planck scale etc it seems clear to me it is unrelated to LQG.

do you think, given its good semiclassical limit, there will be an exodus from LQG and spin foams to CDT? (i.e smolin thiemann markapolou?)

can you enlighten me?

11.
July 11, 2005

dan: *I am curious as to whether anyone thinks the Loop 2005 conference will show progress and/or results,..*

Personally, I think it will show progress and results and can point you to papers that have appeared since the last conference (which was May 2004) which I consider significant.

But first notice that the Loops 05 website does not define the conference as confined to LQG but rather says:
“the annual international meeting on **non-perturbative/background independent** quantum gravity takes place from 10-14 October 2005 …”

http://loops05.aei.mpg.de/
And notice the topics listed on the homepage, where it says:

“The topics of this conference will include:

Background Independent Algebraic QFT
Causal Sets
Dynamical Triangulations
Loop Quantum Gravity
Non-perturbative Path Integrals
String Theory”

As it happens, both Laurent Freidel (a paper of whose Peter mentioned here earlier this year) and Renate Loll are invited speakers and members of the organizing committee. Loll is associated with research in CDT (causal dynamical triangulations) which is a nonperturbative path-integral approach to QG.

If you want a sampling of recent progress and results bearing on the Loops 05 conference, just go to arxiv and look up the papers of Loll and of Freidel which appeared since May 2004. Then you can form your own estimate of the significance of the work.

12. **dan**
July 11, 2005

I am curious as to whether anyone thinks the Loop 2005 conference will show progress and/or results, esp in comparison to String 2005, both from what has happened before, and in comparison to one another.

13. **Peter Woit**
July 11, 2005

QFT is in some sense a very large subject, being used in condensed matter physics as well as high energy physics, as well as mathematics. So it might be too broad a topic for a conference.

But there's also an attitude among particle theorists that QFT is a completely understood subject, and that the only people who still think about it are those that are just not smart enough to do string theory, which is the real cutting edge stuff.

14. **mortain**
July 11, 2005

I can tell you what will be going on at Strings 2005 without even being there, Peter: same old, same old.

Why should it be otherwise? The expected, wonderful breakthroughs in string theory have been failing to materialize since the event began in 1997. When the time comes, perhaps comparing the content of Strings 2007 and Strings 1997 may demonstrate what actual useful 'progress' string theory has made in the preceding period.

I've wondered for some time why there never was a large annual conference on QFT (named, perhaps, 'QFT'). Is it because QFT is considered a dormant, completely understood topic in theoretical HEP? Is a large portion of the theoretical HEP community implicitly promoting this erroneous opinion?

15. **Tony Smith**
July 11, 2005

John Baez has written a useful web page entitled How to Learn Mathematics and Physics at http://math.ucr.edu/home/baez/books.html

A nice book (based on Sidney Coleman's Harvard lectures and Robert Brandenberger's official homework solution sets) is Quantum Field Theory for Mathematicians, Cambridge University Press, Cambridge, 1999, by Robin Ticciati. John Baez describes it as "… Quantum field theory – so even mathematicians can understand it …". However, be aware that an Amazon review says that the book has misprints, so be warned and take care reading it (of course, any book should be read carefully because of the possibility of misprints).

An older book that I like is Quantum Mechanics and the Particles of Nature: an Outline for Mathematicians, Cambridge University Press, Cambridge, 1986, by Anthony Sudbery, but it is hard to find (Amazon says that it is not available now). As John Baez says, it is "… Not just for mathematicians! …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

16.
July 11, 2005

To anyone who shows up at Strings2005 with a laptop: yes, please post comments. Put "connectivity" to a good use.

17. **Alejandro Rivero**
July 10, 2005

what should I do before I go too far

My personal experience, I went first far into another branch, non commutative geometry, and now I have found myself looking at traditional QFT, the standard model, model building and all that. (Somewhere in the middle I read the Green-Swartz-Witten, but it did not convince me).

QFT books do not always provide a decent instint in model building, nor on HEP scattering, but they are always a better startpoint that strings or other mathematically minded setup. After managing them, you can confidently venture into more theoretical enterprises. I hope to retake NCG, but by now I have understood how different is the third generation from the two first ones, or how naive is to blindly hope a single higgs doublet. Nor to speak of surprising mass regularities, or the lack of some a priori expected energy scales (eg why the muon and the tau live at well known hadronic energy scales, the muon so near of the pion?), or the Nambu "alpha" principle to hierarchise masses… a lot of things you only learn by playing with the data, and man, I just wished to be more fluent on standard QFT when juggling these balls.

18. **Peter**
July 10, 2005

If you're an undergraduate, after you have a solid background in quantum mechanics, EM, classical mechanics, general relativity and thermodynamics, you should start trying to seriously study quantum field theory (using books like Peskin and Schroeder, Zee, Weinberg, Ramond, others), and the details of the standard model. Really understanding QFT and the standard model is more than enough for even the most ambitious of undergraduates. You should also learn some modern geometry and topology.

It's kind of absurd to be studying string theory if you don't already have a solid understanding of quantum field theory, and that's a very non-trivial project which can take a few years.

19.
July 10, 2005

Fine, I said I'd get some links, on the off chance you wanted to check out some CDT papers
here's the entire CDT output for the past 12 months, it is only 8 papers
http://arxiv.org/find/grp_physics/1/OR+OR+abs:+AND+triangulations+AND+Lorentzian+dynamical+abs:+AND+triangulations+AND+causal+dynamical+ti:+AND+gravit

and you can actually narrow it down to those co-authored by R. Loll

http://arxiv.org/find/gr-qc/1/au:+Loll_R/0/1/0/all/0/1

and of those, the most recent summary of results is
http://arxiv.org/abs/hep-th/0505154

20.

July 10, 2005

Fine asks *However well if string theory is not the answer what should I do before I go too far and become not willing to see anything else*.

no authoritative answer, but my personal response would be you should check out the picture of quantum spacetime coming out of CDT (causal dynamical triangulations, one of the quantum gravity alternatives to string)

there are only a small number of papers to read, most of which appeared in the past year and a half.

I will get some links. It is a path integral "sum over spacetime geometries" approach where they have both analytical and computer spacetime simulation results.
There is evidence that their spacetime agrees with classical GR in the large, and with prevalent semiclassical "quantum cosmology" near the cosmological singularity. And also that their spacetime is highly unclassical at small scale. (may be fractal-like or topologically complicated at very small scales — probably not describable as a differentiable manifold)

So at least it is different. and immediately accessible.

the "question", I suggest, is how should quantum spacetime be represented mathematically? because only once there is a satisfactory spacetime foundation can particles/fields be reconstructed on that basis.

21. **FineStructure137**
July 10, 2005

If String Theory's the Answer, What's the Question? I am an undergraduate and I have already put in lot of energy and time trying to understand string theory from Polchinki book and Zwiebach book to a lesser extent. Also, I check most conferences in String Theory. Recently,I have been following Peter's blog (for a month now)I have been skeptic of string theory for a while. All I want is to find the truth. I guess ultimately I have to find the answer to this question for myself. However well if string theory is not the answer what should I do before I go too far and become not willing to see anything else.

22. **Alejandro Rivero**
July 10, 2005

About the Landscape, I was thinking… if instead the anthropic principle, we use the humbler "Standard Model at Low Energy Limit" principle, how large is the remaining landscape? Still infinite dimensional? Finite? Null?

23. **Chris Oakley**
July 10, 2005

If String Theory's the Answer, What's the Question?

Suggestion:

"How does a bright, sceptical young scientist turn into a raving, pseudo-religious moron?"

Bad Craziness

July 13, 2005
Categories: Uncategorized

I'd been wondering why Lubos Motl seemed rather subdued in recent months, now one of his recent postings makes some reasons for this clear. Evidently since April he's been the victim of someone who has been sending him grotesque anonymous death threats. Luckily the person responsible for this has now been identified by the police. In the comment section of the next entry in his blog, Eva Silverstein tells of also being the victim of similar threats, and again the police had to be called in.

Lubos's participation in the controversy over the president of Harvard's remarks about women in the sciences seems to have earned him both warnings from senior colleagues (I guess this is what his "leashing" was about), as well as harassment by some other anonymous figure. There's no excuse for people trying to hide behind anonymity to engage in personal attacks. While Lubos is not known to completely refrain from personal attacks himself, at least he has always put his name to them.

In his latest posting he discusses in detail Silverstein and McGreevy's claims that one can understand something about the initial singularity of the big bang in terms of tachyon condensation in string theory. Like Jacques Distler, he says that he doesn't understand these claims, which to me seem to be a lot more coherent, but not of a significantly different nature than those of the Bogdanovs.

Comments

1. **Alejandro Rivero**
   July 14, 2005

   I still hope Witten will come around, start thinking about something else, and lead the field in a more promising direction.

   The talks in the last year have been more of a general review of HEP than specific stringy work, so perhaps he is already starting thinking about something else.

   I can not see yet the slides of his talk.

2. **Peter Woit**
   July 13, 2005

   I don't think I'm bullying Lubos, and actually have never personally attacked him in the ways he has attacked me. I even agree with him about a lot of things, for instance I think we see eye-to-eye about the landscape. At least when he's talking about string theory he knows what he is talking about, which is not true of lots of other people.

   I do have the deepest admiration for Witten. He's extremely talented, works very hard, is a nice guy, has helped me in the past, and has accomplished truly amazing things. Some of the high points of my intellectual life have been reading his papers or listening to his talks. I happen to think that his deep belief in string theory is a mistake, just as Einstein was mistaken in his ideas about unification. Unlike Einstein, I still hope Witten will come around, start thinking about something else, and lead the field in a more promising direction.

3.
   July 13, 2005

   Peter, why don't you stop beating around the bush and take on Witten, instead of bullying Bad Boy Motl?

   He's the ring leader of the string circus yet Peter Voit has nothing but the deepest admiration for him.

Panel Discussion in Toronto

July 13, 2005
Categories: Strings 2XXX

Comments

1.
   July 17, 2005

   Steve mentioned this link to Sakurai's blog
   http://makotosakurai.blogspot.com/2005/07/2nd-day-of-strings-2005.html

   and then you added it, in an update, to the main post.

   But we never followed up, it seems. Sakurai actually does give some sense of things:

   —quote from Sakurai blog—
   OK. Again from the panel discussion. "Why is string theory worth trying even today?" (It was phrased in a different way.) Some said "it is mathematically beautiful but phenomenologically poor" and some said "it is the only known consistent theory of gravity" (how do you prove its consistency?). My viewpoint is not equal to them. What do you think?
   —endquote—

   If anyone has any ideas it might be nice to post them at his blog. So far he has only one very brief reply. It is interesting that he appears to paraphrase the real question being asked by everybody (not what will be the next revolution but)
   "Why is string theory worth trying even today?"

   If Sakurai's paraphrase reflects the sense of the discussion and the responses he heard people give were generally as weak as he suggests then it is not surprising that Florian Gmeiner wrote to Distler's blog complaining of depression.

2. **Peter**
   July 15, 2005

   Sen's slides do seem to have disappeared. There were about half a dozen of them, and they didn't say very much, actually.

3. **Peter**
   July 15, 2005

   Hi Anonymous Graduate Student,

   I didn't mean to imply that all graduate students are impressionable, I'm well aware that some aren't. Only for the impressionable ones would Jacques's hyping of the prospects for string theory be educational malpractice.

   Actually I'm more and more noticing the last few years that a larger and larger fraction of graduate students are well aware that there's a problem in the field, no matter what people like Jacques are telling them. I hope that by reading what I have to say, what Jacques has to say, what Lubos has to say, etc., that students will have enough information to make up their own minds about what is going on.

4.
   July 15, 2005

   Must say that a virtual blackout of reporting on the panel discussion leaves the field open for people to invent their own scenarios, like for example

   MODERATOR'S OPENING REMARKS

   Steven Shenker (moderator of "The Next Superstring Revolution"):

   "Ladies and gentlemen, I think everyone realizes that if we don't want to start looking silly, we have to decide what we're going to say about all these vacua…"

5.
   July 15, 2005

   Peter you say
   Update:It looks like Ashoke Sen's slides from the panel discussion are on-line.

   but I couldnt find anything from the panel discussion there, what i found related to a talk on extremal BH scheduled for later in the week.

   was there something from the panel discussion posted there earlier?

6. **Anonymous**
   July 15, 2005

   "Saying things like this to impressionable graduate students is really educational malpractice."

   Why the assumption that graduate students are impressionable? I'm pretty damn opinionated and also well-informed, if I do say so myself, as are many other grad students I know.

   — An anonymous grad student

7. **Peter Woit**
   July 14, 2005

   Hi Quantum,

Quite a while ago I wrote a short piece about this:

http://www.arxiv.org/abs/physics/0102051

Physics Today refused to publish it, ultimately American Scientist did.

Don't believe everything you read about the beauty of string theory….

8. **quantum**
July 14, 2005

Is there any basic book or article or post that would list the reasons why you anti-string theory partisans are so sure of yourself? I want to learn. I thought that string theory is so beautiful it must be right! I'm a layman tho.

I've pimped your site over at my blog quantuum.blogspot.com

9.
July 13, 2005

dan:
some thoughts
1- paradigm shift from LQG to LOLL's CDT
2- implementation of LOLL's CDT into LQG
3- CDT reproduces many results expected of a QG such as black hole entropy

Dan I am a great fan of Loll and the CDT approach she and co-workers have developed and my comments may be responsible for what you said here, so I must apologize for somehow giving the wrong impression.

Loll is just beginning to work on Black Hole. She does not have black hole entropy formula. Look at her recent paper with Dittrich. Other QG people (e.g. LQG) are farther along on that.
Loll approach is comparatively new and has catching up to do.

it is too extreme, or maybe premature, to speak of a "paradigm shift". I think some notion of quantum spacetime is gradually emerging as people work on these various methods like LQG and CDT. It all contributes to the same understanding. I am frankly delighted with Loll CDT recent papers and results but it is too early to say that one approach to nonperturbative QG will "win the race" and beat the others.

I probably suggested the idea of "implementation of CDT into LQG" and it is my fault for having such an awkward notion. I do believe that there will be some kind of assimilation of methods. But I cannot picture how that will work out! I think some of the other people are going to have to match CDT results or else (as you mentioned earlier) start using Loll's methods. I am confused as to how this will actually happen and I am afraid that I may have communicated my confusion.

It is pretty clearly too early to try to look ahead to this October.
My bad, for doing this. I cant resist saying however that I think it is an exciting time in QG largely because of the past year's results in CDT and the organizers of the October conference have shown this by how they organized it. Last year it was a "Loop and Spin Foams" conference and Renate Loll just happened to be there and gave one paper. this year the outlines seem to have changed significantly. I will try to post something about this when the full Programme is posted (they say something this month but it could be longer)

10. **dan**
July 13, 2005

"No I don't think that something new and exciting will happen at one of these conferences."
Hello Peter,
I am curious as to whether you think (or expect) the same for Loop 2005.

some thoughts
1- paradigm shift from LQG to LOLL's CDT
2- implementation of LOLL's CDT into LQG
3- CDT reproduces many results expected of a QG such as black hole entropy

11. **Peter Woit**
July 13, 2005

Hi Gadfly,

Thanks for that link. The discussion over there was triggered by the cover story of the latest Discover magazine. I just went out and got a copy and will post some comments later.

12. **Peter Woit**
July 13, 2005

To whoever is anonymously posting uninformed attacks on my motivations:

I'll delete any more nonsense of this kind that you submit, but for your information let me just explain that, for various reasons including a clever choice of parents, I'm in excellent financial shape. I don't need either government money or anyone else's mil, and could live comfortably without working at this or any other job if I felt like it.

This financial freedom means that I can afford to piss off people if necessary, and I feel it gives me some responsibility to say things that other people think but are too afraid to say because of fear of repercussions. Witten's role in this whole story is a central one, and I say exactly what I think about it.

13.
July 13, 2005

Quantoken,

Actually there is a simple explanation why Peter is obsessed with the string landscape loons on the fringes instead of hammering away at their leader, Witten, which is that he shares a common goal with those same loons.

Its all in anticipation of the end of public financing for certain science activities that will be deemed nice intellectual pursuits but of dubious practical relevance.

There has been such a massive wealth redistribution and a shrinking middle class will no longer be able to provide the funds.

So its all about getting the attention of the rich aging kooks largely ignorant of science looking for some deep meaning in life before they croak.

Thats why Peter runs a Mockracy to generate as many page hits as possible. Just like the string landscape fruitcakes, its all about finding a Templeton with a mill burning a hole in his pocket.

14. **gadfly**
July 13, 2005

Apparently some economists are concerned about the state of string theory. they fear that is getting worse than their "dimal science"

http://economistsview.typepad.com/economistsview/2005/07/string_theory_h.html

15. **Quantoken**
July 13, 2005

Peter:

The point is any one who is more likely to stand up in those panel discussion and criticize things will most likely NOT attend such conferences. At least not some one who has some minimum self-respect. You would stand up and criticize things, I think, but you will NOT go to such conference in the first place.

The CNN news is still relevant. Medical research is certainly a different dissipline. But the scientific dishonesty discloser is NOT limited to just medical research, but is rather widespread in virtually all other scitific research areas. If scientists in a certain theoretical research area is bold enough to go to TV and announce to audience of the whole world that their theory "explains every thing" while in fact they have not explained a single thing in nature, do you think they have been more honest than the medical researchers?

Scientists are as human as any one and given chances they can all likely be **dishonest** if it serves some personal benefits, whether they are medical researchers or researchers in other fields.

Quantoken

16. **ksh95**
July 13, 2005

Hmmm, no conference participants want to talk about the panel discussion. I don't understand this. Perhaps some one could explain to me why string theorist's opinions about string theory are deemed top secret.

17. **Peter Woit**
July 13, 2005

Hi Steve,

Thanks for pointing out that link, I'll add it to the posting.

18. **Aaron**
July 13, 2005

I'm not going to put words in Jacques's mouth. You can, for example, see his comments about the paper.

I think I'll also follow Jacques's lead, for now at least, in not talking about the panel. I'm sure you'll find out somehow, anyways.

19. **hack**
July 13, 2005

I couldn't help but think of string theorists when I read this article.

20. **Peter Woit**
July 13, 2005

Hi Quantoken,

No I don't think that something new and exciting will happen at one of these conferences. What amazes me though is how the whole subject gets crazier and crazier all the time. I admit to being fascinated by seeing supposedly smart people behave like this and can't believe that no one ever stands up at one of these panel discussions and acknowledges how bad things have gotten. I think this is a truly bizarre and amazing time in the history of science and can't wait to see what happens next. Right now it's kind of like watching lemmings, hard to believe what one is seeing.

And no, bad medical research is a different story, off this topic.

21. **Peter Woit**
July 13, 2005

Hi Aaron,

I'm not surprised by the vote. For the last year or two every string theorist that I've talked to about this has had nothing but scathing things to say about the whole landscape business, many quite unprintable. What I find weird is that despite this, the landscape has become such a popular thing for people to work on. Even people who profess to not believe in it feel compelled to work on things related to it. For instance, which alternative did your colleague Jacques raise his hand for? We know what his latest paper was about.

Still curious about the panel discussion….

22. **Quantoken**
July 13, 2005

Peter:

It’s curious to note even you clearly belong to the anti-string camp, every time there is a string sort of conference going on some where, you showed a great interest and stick your neck out long to find out and you openly beg for insider information about “what’s going on” in those conferences.

You have been inconsistent, Peter. Are you so interested in those meetings because you **worried that they may make some some surprise announcements** of some great news that they made some great discovery and explained everything in nature by SST? Do you really believe there will be something interesting at all?

To me, string theory has clearly failed to demonstrate that it has anything to do with nature at all. And that is unlikely to change any time soon. So **it is un-interesting, and it is boring**. And I would not be interested in any related string research activities, until they can announce something that seem to be relevant. So I really don’t know where Peter gets so enthusiatic in the stuff.

Might as well discuss [this CNN news](), which I think is actually interesting, and shows why sciece fraud is widespread in today’s research community.

Quantoken

23. **Steve**
July 13, 2005

Here’s one I’ve found:

http://makotosakurai.blogspot.com/2005/07/2nd-day-of-strings-2005.html

24. **Aaron**
July 13, 2005

I won’t say much about what else happened, but at the end, there was a vote of hands on whether the cosmological constant was ‘environmental’ or ‘physical’. The results: 4:1 or so against.

And you didn’t believe me….

25. **dan**
July 13, 2005

lubos asked the same question on his blog awhile back.

among the suggestions lubos himself offered……..

LQG!

would you believe it?

Is String Theory About to Snap?

July 13, 2005
Categories: Uncategorized

The August issue of Discover magazine is out, with a cover story entitled "Is String Theory About to Snap?". The editors of the magazine describe how they recently became aware of the controversy over string theory when they organized a celebration of Einstein in Aspen last summer. They quote Lawrence Krauss as telling them "String theory may be in a worse position now regarding being testable than it has been at any time in the past 20 years." To get a response to this, they asked Michio Kaku to write something for them. They refer to him as a "cofounder of string theory", which I suspect some people might object to. Presumably they meant to repeat what is in their profile of him, which calls him a "cofounder of string field theory."

Kaku's article is entitled Testing String Theory, and is a thoroughly intellectually dishonest piece of writing, designed to mislead anyone without expertise in what is at issue here. He succeeded in misleading whoever wrote the blurb for the article which goes: "No experiment has ever allowed us to test whether any of the assumptions of string theory are true. That is about to change." No it's not. None of the experiments Kaku mentions will "allow us to test whether any of the assumptions of string theory are true".

As I've explained in detail on other occasions, the simple fact of the matter is that string theory does not make any predictions, unless one adopts a definition of the word "prediction" different than that conventional among scientists. A scientific prediction is one that tells you specifically what the results of a given experiment will be. If the results of the experiment come out differently, the theory is wrong. String theory can't do this, since it is not a well-defined theory, but rather a research program that some people hope will one day lead to a well-defined theory capable of making predictions.

At places in the article Kaku qualifies his claims of "predictions", for instance saying near the beginning of the article that certain experiments "could provide significant evidence that would support string theory" (note all the qualifiers in this phrase: "could", "significant evidence", "support") but that "the rub is that all the new evidence, no matter how compelling, will still provide only indirect proof." He soon abandons his qualified language and starts talking about the following topics:

1. Gravitational waves: He says of gravitational waves created in the Big Bang: "String theory predicts the frequencies of such waves", and that this prediction will be tested by LISA. I don't know specifically what he has in mind here, but I know of no way to use string theory to make a specific prediction of the spectrum of gravitational waves that LISA will see. The only things he mentions are inflation and epkyrotic scenarios, the first of which has nothing to do with string theory, the second very little.

2. The LHC: Kaku discusses the possibility that superpartners exist, but does note that you don't need string theory to have these. He also discusses possible Tev-scale particle physics effects of extra dimensions, without mentioning that string theory makes no predictions at all about what these extra dimensions are like, or even what their size is. There is absolutely no reason other than wishful thinking to expect extra dimensions in string theory of a size invisible until now, but visible at LHC energies.

3. Laboratory tests of the inverse-square law: Kaku claims: "according to string theory, at small scales like a millimeter, gravity might hop across higher dimensions and perhaps into other, parallel universes". This is a load of nonsense. String theory predicts no such thing. It may be consistent with this, purely because it is consistent with anything. He does go on to say "Perhaps the additional dimensions would show up only on smaller scales — string theory is still somewhat vague about this prediction." "Somewhat vague"??? As far as I know string theory makes no prediction about this at all, except that most string theorists expect effects to show up below 10^{-33}cm, not 10^{-1}cm.

4. Dark matter searches: according to Kaku "Once particles of dark matter are identified in the laboratory, their properties can be analyzed and compared with the predictions of string theory." Only problem is string theory makes no such predictions. He's talking about neutralinos, but in string theory the neutralino mass could be absolutely anything. After discussing these string theory"predictions" about dark matter, he goes on to speculate that maybe there is no dark matter anyway, just "huge clumps of shadow matter in a parallel universe, causing our galaxies to form in mirror-image locations", then admits that such an idea is incapable of ever being experimentally tested.

After going through all this, he saves the real kicker for the end: "Some theorists, myself among them, believe that the final verdict on string theory will not come from experiments at all". So he doesn't even believe in any of the nonsense he has been spouting. He admits that "The principal reason predictions of string theory are not well-defined is that the theory is not finished." So the earlier talk of "predictions" is now no longer operative. He goes on to invoke the pipe dream that someday someone will come up with a finished version of string theory that will predict precisely the standard model, neglecting to mention that there's not the slightest evidence that this is a realistic possibility. On the contrary, all the evidence now points to the conclusion that, if string theory makes sense at all, it has an infinity of different vacuum states, and is probably a radically non-predictive theory. Impressive that Kaku could write a whole article about the prospects of string theory, and somehow neglect to mention the huge and very relevant controversy surrounding the idea of the landscape. Do you think he hasn't heard about it?

Comments

1. **Anonymous**
   July 19, 2005

   Alejandro wrote: "Hmm, anonymous, could it be said that a fat graviton is something as a colored gluon, and then asymptotic freedom strikes? Or is it another mechanism?"

   I don't think there's *any* concrete model, really. From the Sundrum papers it seems one really wants to realize the graviton as an extended object on the scale of 20 microns, while the photon and other known particles remain pointlike. Needless to say, this is a strange picture that doesn't look like any known field or string theory.

   I have wondered about the asymptotic freedom idea you suggest, but I don't know how it could be realized.

   I think whoever first comes up with a good field theoretic or string theoretic framework in which a fat graviton seems plausible will have made major progress.

2. **Torbjorn Larsson**
   July 18, 2005

   Alejandro, thank you, I enjoyed the info.

   It seems like I have one or two years to read it all thoroughly. 🙂 It's a pity if they want to sit on their results until they themselves can verify with differing experiments. Hopefully the other groups work faster…

3. **ks**
   July 18, 2005

   @Anonymous

   Are you shure that string theory is NOT a kind of (nevertheless constrained) meta-language of physics that is able to include/express anything? What is blurred at least to an outsider of string theory like me is the relationship between expressivity as a language and it's own constraints and assumptions. As an analogy: in software development we distinguish between frameworks and applications. A framewok by itself is vacuos (it can be useless or clumsy but it never maps an application domain in a falsifiable way). It just defines how applications can be customized with few effort to run under the constraints of the framework. The applications ('theories') are performing the real stuff. In this sense 'string theory' may be a kind of doubling of theoretical physics within physics, creating a framework for particular theories to work with.

4. **Alejandro Rivero**
   July 17, 2005

   Torbjorn, the information is identical to a rumour started in Lubos blog one month ago, if you read it slowly.

   http://motls.blogspot.com/2005/06/deviations-from-newtons-law-seen.html

5. **Torbjorn Larsson**
   July 17, 2005

   I'm sorry about the confusement; I was reading too fast, I guess.

   The summary article was from April and with full references, so it didn't contain the information you were privileged to have.

   Thanks for the info, it looks interesting!

6. **Alejandro Rivero**
   July 17, 2005

   Hmm, anonymous, could it be said that a fat graviton is something as a colored gluon, and then asymptotic freedom strikes? Or is it another mechanism?

7. **Anonymous**
   July 15, 2005

   Torbjorn, your repeated posts confuse me, but I'm sure what I said was right, so let me elaborate:

   — Down to 100 microns, the tests show no deviation.
   — Below 100 microns, they do begin to see some deviation. So far this is unpublished, and might go away. If it does not, it poses a serious problem for string theory. The reason is that string theory can easily explain gravity getting *stronger* at short distances (due to more dimensions being accessible), but not getting *weaker*. It doesn't immediately invalidate the theory, but no one seems to have any model of how string theory could account for this.
   — The fat graviton, to be related to the cosmological constant, implies gravity will be modified below some scale that cannot be much larger than 20 microns. In other words, if the tests find no deviation below about 20 microns, the fat graviton is ruled out. But if the current deviation is confirmed, it's provides a compelling reason to think about the fat graviton.

8. **Torbjorn Larsson**
   July 15, 2005

   I have a bad day; Anonymous said the reverse, and it fat gravitons aren't ruled out.

9. **Torbjorn Larsson**
   July 15, 2005

   Oops, sorry; they seem also to have ruled out the fat graviton as Anonymous said.

10. **Torbjorn Larsson**
    July 15, 2005

    "I think it's worth noting that tests of the inverse-square law stand to actually *invalidate* string theory,…"

    I'm no expert but I don't think they invalidate ST; it has as usually covered all bases. According to http://physicsweb.org/articles/world/18/4/6/1 the inverse-square law is fine to small scales. What they started to rule out is some, but not all, brane-world scenarios.

11.
    July 15, 2005

    dan mentions
    http://motls.blogspot.com/2004/12/ny-times-20-years-of-strings.html

    which is Lubos blog about the Overbye NYT article. There was a Notevenwrong blog too about the same article, if I remember right.
    It was an interesting article partly because of the anecdotes, and gems of triumphant rhetoric. I dont happen to have a subscription to the NYT so I used these alternative links

    http://www.iht.com/articles/2004/12/07/news/string.html

    http://pmbryant.typepad.com/b_and_b/2004/12/string_theory_d.html

    the second has a part of the original Overbye article that the first omits, namely the exchange between Brian Greene and Steve Shenker at the conclusion, about "If it's wrong, don't you want to know?"

    dan also quotes A. Garrett Lisi from earlier this thread, the comparison with microsoft, posted at July 14, 2005 01:05 PM.
    I would like a direct link to that. Has Garrett published that comparison somewhere else? it is too apt to lose. I don't like having to hunt for it by scrolling down though the comments.

    A propos overblown rhetoric, there is Overbye's final paragraph:

    String theory's biggest triumph is still its first one, unifying Einstein's lordly gravity, which curves the cosmos, and the quantum pinball game of chance, which lives inside it.

“Whatever else it is or is not,” Harvey said in Aspen, “string theory is a theory of quantum gravity that gives sensible answers.”

should be an anthology of grandiose string pronouncements. It would make entertaining reading now, and more so in a few years, I suspect.

12. July 15, 2005

    I’ve always had the impression that string theorists roll their eyes upward at Kaku’s attempts to exaggerate his role in the development of string theory, but are loathe to diss him in public in fear of negative publicity for the field.

13. **dan**
    July 15, 2005

    Does Lubos Motl work for Microsoft?

    http://motls.blogspot.com/2004/12/ny-times-20-years-of-strings.html

    “The article also includes an appropriate portion of a text about loop quantum gravity – a few percent. The most relevant recent discovery in loop quantum gravity was done, according to the article, by Vafa, Neitzke, Gukov, and Dijkgraaf, and Cumrun explains that if loop quantum gravity is correct, it must be a part of string theory. I am sure that our loop quantum gravity colleagues will agree wholeheartedly. ;-)”

    as a response to

    “I see string theory as the microsoft of physics — all marketing, with technological “development” coming only from the principle of “embrace and extend” leveraged to succeed via its monopoly. If I use this model to speculate as to the future, I don’t think string theory will have a spectacular crash, as some might hope and as it certainly deserves. Rather, some non-string researcher, or research group, will make some real progress and the string theory mob will embrace it and claim it as their own. The resulting fun will depend on how adamantly the successful “confounders of string theory” protest that their stuff has nothing to do with strings — but my pessimistic view would be that this voice would be lost amid the torrent of stringy activity and papers tied to their work. Time will tell.”

14. **Anonymous**
    July 15, 2005

    I think it’s worth noting that tests of the inverse-square law stand to actually *invalidate* string theory, or at least force one to consider very different scenarios than the conventional ones, in the very near future. Eotwash apparently sees a weakening of gravity at around 20 microns, where Raman Sundrum predicted one would see this if a “fat graviton” explains the cosmological constant. This is exciting stuff, and I hope they are able to confirm the tentative evidence of a signal.

15. **Quantoken**
    July 15, 2005

    Kyle said:

    “In fact, I’ll go a step further. There are high school REU students out there who hold greater claim to having advanced physics than Kaku has in the past twenty years. If only they were doing work on things that sound suitably star-treky, then maybe they could join him in such grand publications as Discover.”

    Actually I will make the observation that any random walking soul on the streets would have a **perfectly legitimate claim** that he/she has advanced physics in the past year than what Kaku did in the past 20 years.

    Because even if the random soul has not worked in physics, he/she has very likely paid taxes and thus provided support by the public funding to support physics researches. So that’s a positive contribution. Whereas Mr. Kaku, if he had achieved nothing, he would have wasted 20 years worth of research funding and thus contributed negatively to science.

    But it’s even worse. By going to TVs and publish popular readings that utterly lie about and mislead the public about scientific facts and realities, Kaku not only wasted tax payer money, he also contributed negatively to the progress of real science.

    Quantoken

16. **Ben**
    July 15, 2005

    I’m no expert, but it boggles my mind that someone who has published the highly technical books that Kaku has, by major technical publishers (Springer, Oxford), could be incompetent, which seems to be the implication here. On the other hand, that seems to be the implication about all of string theory. I am not saying you are right or wrong, since as I said I don’t know. But something really weird is going on.

17. **A Scott Crawford**
    July 15, 2005

    How much did the LIGO facility down south cost? $600 million or something? And now we eagerly await a signal that’ll EITHER be a wrinkle in gravity, OR a car backfiring on a not so nearby highway! Just as Einstein predicted!

    Just to play the devil’s advocate… the LISA system is going to adjust the position of it’s three SV’s using the coronasphere model. This assumes there is ALWAYS solar weather, or varying degrees of both proton and photon density interacting in a particular volume of “euclidian” space (at one AU), &etc. (CQ Magazine has monthly solar weather charts for the curious)

    The first observation is that solar weather is, in practical terms, aether. One might almost want to look at chromatic aberrations, or (echm) other signal characteristics (like spherical aberrations or x) to see if an interferometer inside a spaceship can allow one to determine lots of stuff without looking out the windows. That’d be pretty cool, as it’d be nice to zoom up past ‘c’.

    The second observation is that the LISA SV’s beams aren’t traveling through a shielded vacuum or a stable magnetic fiield. The SV’s will tell us a lot of things about Sol. But how one distinguishes between a space jiggle due to cloud of charged dust from the sun and one due to a cosmic wave of gravity is beyond me.

    The last observation is actually a question. Is the scientific principle commonly referred to as Ockhams Razor, dead, or alive? If the former, what’s replaced it? If the latter, how well does String Theory (xyz) hold up if the Razor is applied?

18. **ks**

July 15, 2005

It is at least the mass audience who is willing to spend money into an enterprise without any practical use (former generations may have believed in new high energy wappon systems as a side-product of accelerator research). It will be sold as a part of “culture” starting with greek intellectuals speculating about the arch? and ends up with string theorists doing the same thing collaboratively with more advanced mathematical tools. Maybe Kaku has understood better than the mainstream mathematical physicist that whatever theory will be finally accepted by the community it will end up in physics-actors like him and books about timetravel, wormholes and parallel universes i.e. pop-culture that justifies the luxury of HEP research. Therefore Kaku goes well beyond John Horgans claim that scientists practice ‘ironic science’ but deny this circumstance.

19. **Andy Dabydeen**
July 15, 2005

I think Kaku is pop-scientist, that is what offends a great deal of his critics. Regardless of his position, lack thereof, and the liberties he’s taken, what he has managed to do is communicate to a mass audience — which is not such a bad thing — even if some say he’s delving into the realm of science fiction.

20. **D R Lunsford**
July 15, 2005

Kaku’s QFT book is not bad. In fact it’s very conservative, which is what you really want in a textbook. Of course there is no single tome that will do, since the theory is broken to begin with.

Peter’s comment perplexes me. As I understood it, string theory emerged from an attempt to explain nuclear forces and in particular resonances, in the sense of literally banding together nucleons and using Hooke’s law (!). So how could Kaku have been an evangelist before there was anything to get really worked up about?

-drl

21. **Kyle**
July 14, 2005

Why would that irk you? It doesn’t take much to pass up someone standing still. Spend a couple of years helping out in a good lab and you’ll have done more to increase the scope of physics’ predictive power than some others have in decades of mathematical meanderings.

In fact, I’ll go a step further. There are high school REU students out there who hold greater claim to having advanced physics than Kaku has in the past twenty years. If only they were doing work on things that sound suitably star-treky, then maybe they could join him in such grand publications as Discover.

22. **ksh95**
July 14, 2005

Hack said
I seriously doubt it! What kind of demented instructor would choose Kaku as a primary text for a QFT course? Aside from Kaku himself, of course.

Agreed. I think it’s a horrible introductory text. The quality of Kaku’s book, however, was not the point of my post.

I just get irked by goofy statements like, “…I contributed more than Kaku when I was an undergrad…”.

23. **hack**
July 14, 2005

“Respect your elders young grasshopper. One day you may be taught QFT from Kaku’s books.”

I seriously doubt it! What kind of demented instructor would choose Kaku as a primary text for a QFT course? Aside from Kaku himself, of course.

24. **Peter Woit**
July 14, 2005

Hi Garrett,

I think you’re being unfair to Microsoft, their product actually works. But I think you’re right that what will happen in the future is that anything new and successful will be dubbed a “version of string theory”. In his Discover article, Kaku refers to all sorts of different things as “versions of string theory”.

25. **garrett**
July 14, 2005

I see string theory as the microsoft of physics — all marketing, with technological “development” coming only from the principle of “embrace and extend” leveraged to succeed via its monopoly. If I use this model to speculate as to the future, I don’t think string theory will have a spectacular crash, as some might hope and as it certainly deserves. Rather, some non-string researcher, or research group, will make some real progress and the string theory mob will embrace it and claim it as their own. The resulting fun will depend on how adamantly the successful “confounders of string theory” protest that their stuff has nothing to do with strings — but my pessimistic view would be that this voice would be lost amid the torrent of stringy activity and papers tied to their work. Time will tell.

26. **Anonymous**
July 14, 2005

A few years back I was at a conference where Kaku was a guest of honour. I was looking forward to meeting the guy and managed to pull strings to join him for dinner. He was not the person I expected. He was one of those annoying scientistic scientists – one of those who thinks that if it’s been published in a paper it must be “absolutely and scientifically proved”, even, surprisingly, from the ‘softer’ sciences like biology. He’d quote stuff on a wide variety of topics as if he were some kind of oracle with perfect knowledge of The Truth. Anyway, the next day he gave his talk. It was more of the same with him laying down The Law as to what was and wasn’t physically possible. He stated some ‘fundamental’ limit on computing: maybe it was a limit on switch size, something like that. Anyway, there was a bit of noise coming from the back of the auditorium. It got a bit louder. A bunch of people were conferring. And eventually they said something. This group had already exceeded his so called ‘fundamental’ limit in the lab. They had intended to make no announcement about it yet but after the arrogance of Kaku’s speech they couldn’t contain themselves any longer!

27. **ksh95**
July 14, 2005

Kyle said
I'm proud to say that by the time I graduated with a B.S., I had contributed more to physics than Kaku has before or since.

Respect your elders young grasshopper. One day you may be taught QFT from Kaku's books.

28.
July 14, 2005

I found the statement "cofounder of string field theory" on his web site.

That's very clever, actually. The layman will not notice the difference between "string theory" and "string field theory", so it practically reads "cofounder of string theory". But to the expert, Kaku can always say "Hey, I said string FIELD theory", which is a relatively obscure subject that very few people know or care about its history anyway. Very clever!

29. **Mike Crowley**
July 14, 2005

Peter,

I found this entry very helpful in providing a more realistic context for Kaku's book "Parallel Worlds". The article you described is very similar to the arguments he sets out in this book.

I reviewed the chapter where he discusses testing String Theory. He devotes a lot of space to gravity waves but I'm still unclear how they confirm String Theory; however, he writes "One important goal of LISA is to provide the smoking gun for the inflationary theory… Some, such as Kip Thorne of Cal Tech, believe that LISA may be able to tell whether some version of string theory is correct. As I explain in chapter 7, the inflationary universe theory predicts that gravity waves emerging from the big bang should be quite violent, corresponding to the rapid, exponential expansion of the early universe, accompanied by much smoother gravity waves. LISA should be able to rule out various rival theories of the big bang and make a crucial test of string theory."

At the end of the chapter he writes, "My own view is that verification of string theory might come entirely from pure mathematics, rather than from experiment. Since string theory is supposed to be a theory of everything, it should be a theory of everyday energies as well as cosmic ones. Thus, if we can finally solve the theory completely, we should b able to calculate the properties of ordinary objects, not just exotic ones found in outer space. For example, if string theory can calculate the masses of the proton, neutron, and electron from first principles, this would be an accomplishment of first magnitude."

He then quotes Einstein and concludes the chapter dramatically: "If true, then perhaps M-theory will make possible the final journey for all intelligent life in the universe, the escape from our dying universe trillions upon trillions of years from now to a new home." (After learning what I've learned here, it's no longer possible to read paragraphs like that with a straight face.)

Yesterday morning on a local talk show in Phoenix I heard the anchor praise Kaku, describing him as "one of the fathers of String Theory" and also "like the rock star of theoretical physics–he has a huge following and a web page."

30. **Peter Woit**
July 14, 2005

Kaku was one of the co-developers of string field theory, see his 1974 papers with Kikkawa. Of course there was little interest in this at the time, since a year earlier Gross et. al. had discovered asymptotic freedom and the best people in the field were all doing gauge theory.

31.
July 14, 2005

It's also a fact that there is (almost) no such thing as a (serious) "string field theory", and if there is, then Witten is the founder (probably Warren Siegel should also be mentioned). I recall that Kaku wrote a paper or two on the subject in the 80's (as so many other people), but nothing like a major contribution.

32. **Joe Bolte**
July 14, 2005

I would encourage you to write something similar to what is here as a letter to the editor of Discovery. No matter who owns it, or what its standards are, I think it's important to point out to readers who may not have a sophisticated knowledge of what constitutes science that Mr Kaku is being extremely slippery here.

33. **Kyle**
July 14, 2005

I'm proud to say that by the time I graduated with a B.S., I had contributed more to physics than Kaku has before or since.

I believe this is so because though he has done much work (of what quality?) in mathematics, I have never seen any evidence of Kaku having produced anything in physics at all.

34.
July 14, 2005

Kakuphony!

35. **Alejandro Rivero**
July 14, 2005

I fail to find the updated version of "Comments On String Theory", if it is http://arxiv.org/abs/hep-th/0212247

36.
July 14, 2005

Peter,

For pete's sake, Discover Magazine is a rag owned by Disney which is far below the standards of Scientific American (which itself has been going downhill for the past two decades). Who cares what they have to say pro or con.

Some of your concerns in your post can be put to rest by Witten himself in his paper entitled “COMMENTS ON STRING THEORY” which was updated as of June 2005.

but no you keep chasing after kooks like kaku because you’re runnin a mockracy.

37. **M**
July 14, 2005

Maybe a fine-tuned version of your post would be an appropriate “letter to the editor” of Discover, or even a rebuttal to Kaku’s piece? Or do you think Discover would ignore it, preferring sensation over accuracy along the lines of “We are on the cusp of discovering great things; isn’t that really exciting?”

38. **Quantoken**
July 14, 2005

MICHIO KAKU. I wonder what kind of scientist this guy is and what kind mind he has. These days I can not turn on television without seeing the face of this big mouthed MICHIO KAKU, especially when I watch science related channels.

Does any one know what kind of research he does? A search in ARXIV turned up just 6 of his papers. And **nothing since 1999**. Has he turned himself into a TV actor or SciFi novelist or sort of thing?

Quantoken

39.
July 13, 2005

Kaku, Kaku. What can I say! Where do I begin!

The Landscape in Toronto

July 14, 2005
Categories: Uncategorized

Most of the talks at Strings 2005 about the landscape have now taken place, although there's at least one more this afternoon by Dine. Frederik Denef gave a survey talk entitled Constructions and distributions of string vacua. One amusing thing he does is note that even in toy models with these exponentially large numbers of states, counting the number of states with vacuum energy less than some bound is computationally an NP-hard problem. He describes a wide range of constructions that people have come up with to fix the moduli, concluding that you can "throw enough ingredients together to get sufficiently complicated potential, and this will fix moduli, at least at effective field theory level", but that these constructions are "ugly". He then goes on to survey various results about the statistical distributions of these states, and ends by announcing a workshop in Trieste next spring on "String Vacua and the Landscape."

The talk on Is the number of string theory vacua finite? by Michael Douglas makes Denef's survey of distributions of vacua kind of pointless. The number of such vacua is definitely infinite, which ruins ones ability to get a probability distribution by counting vacua. Douglas hopes that by putting in a cutoff on the diameter and volume of the compactification space, as well as the size of the vacuum energy, he can make the number of vacua finite. He explains this conjecture, for which the evidence is not very compelling.

Even if he gets the finiteness he hopes for after imposing these cutoffs, the problem then is that the distributions of vacua depend strongly on the cutoff and are peaked at the cutoff value. This is what happens in examples that Kachru talked about at the conference. Douglas is reduced to arguing that "it seems a priori plausible that cosmological selection could depend on the volume of the extra dimensions", i.e., that somehow the Big Bang would get rid of the problem that his program is predicting large compactification spaces when he wants small ones. There seems to be no reason for this other than wishful thinking. One thing is clear though now: it makes no sense to spend time computing distributions of these vacua, since this gives a result you don't want. In this game though, it's not like you give up on your research program when it gives results that don't look at all like the real world.

Comments

1. **Peter Woit**
   July 19, 2005

   Hi jkg,

   The Douglas program is based on the idea that we live in a randomly chosen (by not understood dynamics of the early universe) part of the multiverse, randomly chosen among those in which life is possible. If this is true we would expect to be in vacua corresponding to high probabilities, not low ones (consistent with anthropic principle). Douglas et al hope (hoped?) to use this idea to make some predictions. It is these hopes that have collapsed.

   Susskind has written a whole book about why this is a great idea, you can read it later this year. Personally I always thought this was just nuts.

2. **Torbjorn Larsson**
   July 19, 2005

   I'm not qualified to say much in this except that the difference between Peter and Bob (and me) is split by 'among which String methods can not _yet_ distinguish'.

   The 'not yet' is wide open to be realised as 'soon' to 'never'. I'm not sure how we would be certain of any of these extreme cases.

3. **jkg**
   July 19, 2005

   Hi,

   Sorry for asking naive question, but I do not quite get what could be the relevance of probability distribution of universes in multiverse (if you can calculate it, and I understand that you cannot in the landscape scenario.) Suppose in string theory I you calculate the probability of having our universe to be 99.9% and in another string theory II this probability is 10^{-10}%. So what? Should I say that the second theory is wrong and the first is right? Certainly not! Leibniz said that we live in the best of all possible worlds, which is perhaps right, but why am I to believe the we live in the most probable one (or most improbable, or whatever?)

4. **Peter**
   July 19, 2005

   Robert,

   If you think about it for a minute, you'll see there's an obvious problem with your analogy between QFT and string theory. You can use QFT to make an infinite number of precise experimental predictions, then go out and check them and they work. This is called doing science. Sure, QFTs don't predict absolutely everything, but they come with a precise understanding of what they predict and don't predict. There are lots of QFTs, but one of the simplest ones seems to work perfectly.

   The problems with string theory in general are complex, but the problem with the landscape is very simple: it's not science. The one hope for getting predictions out of it (one for which there was never any evidence, just pure wishful thinking), Douglas's statistical program, has now collapsed, although its practicioners seem to be having trouble clearly admitting this.

   The people investigating the infinite number of vacuum states that make up the landscape can't tell us how they're going to ever get any sort of prediction out of this. What physical quantity are you going to calculate, use to make a prediction such that it can be checked and if it is wrong, you'll admit that the idea is wrong? It's fine if you can't do this calculation yet, but it's not fine if you don't have a plausible program for getting there.

   What string theorists are doing now is launching into an endless investigation of an infinitely complicated structure, with no scientific justification for doing this. The only justification is the idea that string theory must be right, and this is what string theory leads to. This is no longer science, it's a cult.

5. **Bob McNees**
   July 19, 2005

   "that is, there were a whole bunch (one followed by 100 zeros) of quite different versions of nature—with all different basic features and constants—among which String methods could not distinguish!"

This is a bit of a disingenuous comment. I know, it's hard to believe that anyone around here would offer a gross oversimplification of the state of things in string theory, but it seems to have inadvertently happened.

First of all, the notion of "the landscape" has been met with more than a healthy amount of skepticism in the string community. That's not to say that it's entirely wrong, but it's certainly not the universally accepted source of communal shame that Peter makes it out to be. But let's suppose, for the sake of argument, that all of the arguments surrounding the landscape go through, or even that Douglas' speculation about the number of vacua being infinite holds up. Claiming that this dooms string theory requires that we already have a near-complete technical mastery of the subject, so that the notion of large (or infinite) families of vacua becomes an irreparable flaw in the theory.

That's just not true.

Consider the following analogy: write down every field theory you can think of. I'll even spot you four dimensions…go ahead, start writing. How many do you have? Is it a few? A few dozen? 10^100? 10^500? An infinite number? What does this have to do with the landscape, you say? Pretend for a moment that you're ignorant of every mechanism by which you might effectively reduce the number of theories you've just written down, to those which you feel qualify as "realistic". How will we ever describe nature? My lord, with so many theories, how will we ever make progress? Field theory was supposed to unify quantum mechanics and special relativity, but all it has done is land us in this embarassing morass of theories consistent with quantum mechanics and special relativity. Let's all go home…field theory is a bust.

Of course, now I'm the one who's being disingenuous. You know lots of physical principles that narrow down the number of realistic field theories. We've even singled one out as describing most of what we see. But based on your reasoning, why should you even start to narrow down the field? Such a large task, even if it was only made large because you began with an incomplete knowledge of what principles you should apply, is apparently reason to just give up.

The point is, we have no reason to believe that string theory isn't full of selection principles that deal precisely with Peter's (and many string theorists') objections to the landscape. Personally, I'm not sold on the landscape. But I am aware that, left unaddressed, the idea of lots and lots of vacua for string theory might pose a problem for predictability. Do I abandon the field? No. In somewhat novel fashion, I conclude that perhaps I should work on the problem. Quixotic, I know, but I've heard that actually working on a problem usually gets better results than complaining about the problem.

"But one should always have hope."

Yes, one should. String theory is hard. We're working on understanding it. We run into hard problems all the time, and what to make of the landscape is one of them. As discussed earlier in the post, progress on such problems is often made by working on them.

6.
July 18, 2005

Torbjorn, at the time I was talking about, they could not distinguish the right one. I cannot speculate about future String methods and what they will be able or not able to do about the Landscape. You probably remember it was described as a needle in a haystack—or Lubos Motl compared Landscapeology to a drunken man looking for the keys to his house not in the light of a street-lamp but in the middle of the ocean, on the surface. But one should always have hope.

7. **Alejandro Rivero**
July 18, 2005

And, every time I look in there, full of inaccurate information.

Hey, our thread on numerology tries to keep a high standard of 99.9% numerical accuracy and above! Perhaps you mean *nonsensical* or *misleading*. But we are accurate.

8. **Torbjorn Larsson**
July 18, 2005

Thanks for the physicsforums tip.

Since we laymen already lay about, perhaps you can clarify why "…among which String methods could not distinguish" is necessarily true.

The KKLT model is but a very special case of a huge class of more general compactifications. It is made, as I understand it, primarily to have 4 large dimensions and a small positive cosmological constant. (It also entails other good benefits.)

Why do we discount other compactifications and methods to constrain vacua just because this particular case failed?

9. **Aaron Bergman**
July 18, 2005

did you already try physicsforums.com with your question?
it is more oriented toward layfolk (general reader-type) questions.

And, every time I look in there, full of inaccurate information.

10.
July 18, 2005

pseudo

did you already try physicsforums.com with your question?
it is more oriented toward layfolk (general reader-type) questions.

the gist of the "Landscape" business is that around January 2003 there was a massive setback in the String research program. Some people at Stanford (Kachru, Kallosh, Linde, Trivedi) came out with a paper showing that String doesnt predict just one version of physics—this is what had been hoped for: a theory that would explain why a lot of the basic constants in physics are what they are.

In fact the KKLT paper showed that String doesnt even narrow it down to just 2 or a dozen or a thousand different versions. KKLT found that the number of possible String "ground states" or "vacuum states" was more like ten-to-the-hundred. that is, there were a whole bunch (one followed by 100 zeros) of quite different versions of nature—with all different basic features and constants—among which String methods could not distinguish!

This became known as the "Landscape" of string versions of physics, or (to put it in a more technical language) the Landscape of String Vacua. (Vacua or vacuums, in this case, just means the possible ground states or basic blank conditions of nature.)

Later researchers (after january 2003) determined that the number was more than ten-to-hundred, it was maybe more like ten-to-500, or maybe infinite. This year infinite seems to be emerging as a favorite estimate.

This development has been interpreted by some as ending String hopes of being a useful scientific theory (able to predict unique outcomes and explain a lot of stuff). One possible response to the existence of the Landscape is to say that String is over and it is time to look for a different theory.

Another possible reaction which some String theorists have shown is to try to "save" the program by abandoning the expectation that a physical theory should be testable by making unique predictions. This has lead to talk about "Anthropic Principle" or as some say, the "Anthropic Lack of Principles".

We heard a lot of talk about that in 2003 and 2004 but the word has fallen out of favor. The idea was that the world is just the way it is and we dont have to explain it, because if it were very different then life wouldnt have evolved and we wouldnt be here talking about it. You wouldnt have Conscious Life-forms Like Us ("anthropic" is just a technicalsounding word for "Like Us" in a very broad sense, here).

Now String theorists seem embarrassed by the word "Anthropic" and they are using the word "Environmental". they sometimes say that some basic key physical constants **don't have to be explained because they just are what they happened to be**.

This is a way of saving String by excusing it from having to predict or explain basic features of nature. It is an abdication from the traditional scientific quest. String theory is all right as long as we decide we dont expect a fundamental theory to predict unique values of the constants of nature. Like, why, on the macroscopic level that we can see, does spacetime have 4 dimensions? Well one answer is let's not try to explain that, it just is. It is **environmental** which here is a code-word for "how it happened to turn out".

Maybe the number 4 dimensions is a bad example and i should be taling about something more technical like the cosmological constant or the fine structure constant, but maybe you get what I'm trying to say without that.

11. **pseudo string fan**
July 18, 2005

Hi guys,

I often meet the following terms in string theory's discussion:landscape and anthropic principle

I think landscape in string theory is quite different from that found in a dictionary. I also can't find the explanation of anthropic in my dictionary. Could someone tell me what they mean in string theory? Thank you in advance.

12. **Alejandro Rivero**
July 16, 2005

What is depressing, is the following slide
http://www.fields.utoronto.ca/audio/05-06/strings/sugimoto/index.html?2;large#slideloc

The author seems to think that his audience is not going to be interested in anything related to experimental input, and he uses a full slide to justify the interest on QCD. Pitier if one thinks that string QCD is the main silver bridge for young graduates to return to reality.

13. **Alejandro Rivero**
July 16, 2005

Not rare, if you have sometime seen these panels, which are rather formal. Panelists either took the opportunity to keep selling his own product, or try to come when an enthusiastic, politically correct, speech to please the public. I think it should be funnier, and more interesting, if some place were left to think aloud, instead of reading aloud.

14.
July 15, 2005

In terms of the physics, the panelists gave their ideas for about an hour. Then there were some comments and ideas from the audience. There was some discussion of the anthropic stuff, but it didn't come close to dominating the two hours. The moderator ended with the vote and that was it.

Thanks for the report, Aaron.

Impressive lineup:

Raphael Bousso (UC Berkeley)
Shamit Kachru (SLAC & Stanford)
Ashok Sen (Harish-Chandra Research Institute)
Juan Maldacena (IAS, Princeton)

Andrew Strominger (Harvard)
Joseph Polchinski (KITP & UC Santa Barbara)
Steve Shenker (Stanford), moderator.
Eva Silverstein (SLAC & Stanford)
Nathan Seiberg (IAS, Princeton)

The organizers' announced purpose of the session was "to explore, in an informal and interactive way, possible directions that may lead to major new progress in our field."

Judging from your account not much worth telling about except a show of hands 4:1 in favor of the cosmological constant being "physical" as opposed to "environomental".

If you remember anything else that struck you as interesting, later on, hope you will share it with us. Thanks again for the reportage.

15. **Torbjorn Larsson**
July 15, 2005

So no ideas worth mentioning and a vote not worth explaining?

16. **Aaron**

July 15, 2005

In terms of the physics, the panelists gave their ideas for about an hour. Then there were some comments and ideas from the audience. There was some discussion of the anthropic stuff, but it didn't come close to dominating the two hours. The moderator ended with the vote and that was it.

17.
July 15, 2005

Aaron:*There's just not all that much to say about it, physicswise*.

In that case, if it's no big deal, then there's no reason not to report your impressions and what you got of the sense of the discussion, is there Aaron? So please report.

18. **Aaron**
July 15, 2005

I'd be careful, if I were you, about ascribing beliefs on the anthropic principle without talking to the actual people. (One of the advantages of a conference like this is that you actually get to do that. I don't feel it's appropriate for me to talk for other people, however, so I'll just leave it there.)

I also wouldn't get too excited over the panel discussion, either. It wasn't some great string theory Waterloo or anything. There's just not all that much to say about it, physicswise.

19.
July 15, 2005

It's hard to know where string theorists are on the Landscape issue collectively (if there is a common viewpoint) because of the failure of anyone to report Tuesday's panel discussion
http://www.math.columbia.edu/~woit/blog/archives/000218.html

In the comments on your blog #218, I remember Aaron did say there was a show of hands at the end of the session:

"I won't say much about what else happened, but at the end, there was a vote of hands on whether the cosmological constant was 'environmental' or 'physical'. The results: 4:1 or so against.
…
Posted by: Aaron at July 13, 2005 03:53 PM"

He doesnt say against what. presumably "environmental" = resorting to Anthropic Principle (approx.) and the showing was "4:1 or so" against appealing to the Anthropic Principle. Hope I am not misconstruing.

Don't know if Aaron actually counted or if he or someone just eyeballed it and said 4:1 because it felt right. Not sure if I am interpreting it right when I think of it as a strawvote that went against Susskind and the (Dine Douglas Denef) consulters of the entrails. Maybe it doesnt matter how the vote went and what the issue really was.

But at least it sounds like the folks Tuesday were concerned about "environmental versus physical" and worried enough that none of them want public notice of what went on in their discussion.

Letter to New York Review of Books

July 14, 2005
Categories: Uncategorized

A colleague informs me that the latest New York Review of Books contains a letter from one of the most well-known mathematicians in the U.S.

Comments

1. **Alejandro Rivero**
   July 20, 2005

   Assuming it is him and not someone with the exact same name.

   … and living in the same maximum security prison at Florence, Colorado.

2.
   July 20, 2005

   I was thinking Pauly Shore for the role of Lubos.

   And Michael Douglas as Michael Douglas, of course.

3.
   July 18, 2005

   I am sure the editor was very nervous opening a letter from this infamous mathematician. Assuming it is him and not someone with the exact same name. As for the string movie mentioned below, Antonio Banderas could play the part of J. Maldacena and Leonardo diCaprio that of L. Motl…maybe :).

4. **Torbjorn Larsson**
   July 18, 2005

   No no! _Dead_ people got no reason to live; _short_ people got less reason to live; long people take up unnecessary space. Long live mediocracy!

5. **String, Strang, Strung**
   July 17, 2005

   From: http://physicsmathforums.com/showthread.php?t=56

   All Tied Up & Strung Along: Hollywood String Theory Movie!!! Looking For Extras!!!
   FOR IMMEDIATE RELEASE:

   ALL TIED UP & STRUNG ALONG, a movie about String Theorists and their expansive theories which extend human ignorance, pomposity, and frailty into higher dimensions, is set to start filming this fall. Jessica Alba, John Cleese, Eugene Levie, Jackie Chan, and David Duchovney of X-files fame have all signed on to the $700 million project, which is still cheaper than String Theory itself, and will likely displace less physicists from the academy.

   “As contemporary physics is about money, hype, mythology, and chicks,” Ed Witten explained from his offices at the Princeton Institute for Advanced Study, “The next logical step was Hollywood, although I thought Burt Reynolds should play me instead of Eugene Levy.”

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   “At first I am reluctant to stop, but when my love interest Jessica Alba is sucked into the black hole, I search my soul and find Paul Davies there, played by John Cleese. I ask him what he’s doing in my soul, and he explains that the answer to that is containined in the mind of God, which only he is privy too, but for a small fee, some tax and tuition dollars, a couple grants here and there, and an all-expense-paid book tour, he can let me in on it. But as soon as he points out that we can make more money in Hollywood than printing more coffee-table books that recycle Einstein, Bohr, Dirac, Feynman, and Wheeler, adding little else, I am converted. I agree to turn my back on String Theory’s hoax and save Jessica Alba.”

   But it’s not that easy, as standing in his way is Michio “king of pop-theory” Kaku, played by Jackie Chan. Kaku beats the crap out of Greene for alomst blowing the “ironic” pretense his salary, benefits, and all-expense paid trips depend on. “I have better hair,” Kaku argues as he delivers a flying back-kick, ” I should be String Theory’s front man!!”

   How does it all end? Does physics go bankrupt funding theories that have expanded our ignorance from four dimensions into ten, or twenty, and thirty dimensions? Do tax payers revolt? Do young physicists overthrow the hand-waving, contortionist bullies and revive physics with a classical renaissance favoring logic, reason, and Truth over meaningless mathematical abstractions? Does Moving Dimensions Theory (MDT) prevail? We’ll all just have to wait!

   From: http://physicsmathforums.com/showthread.php?t=56

6. **fooltomery**
   July 15, 2005

   From the Michael Mann version of the movie *Manhunter*, a memorable (and not inapposite) exchange:

   > **Will Graham:** “I know that I’m not smarter than you.”
   >
   > **Hannibal Lecter:** “Then how did you catch me?”
   >
   > **Will Graham:** “You had disadvantages.”

Hannibal Lecter: "What disadvantages?"

Will Graham: "You're insane."

7. **Randy Newman**
July 15, 2005

Short people got no reason to live.

8. **Dick Thompson**
July 15, 2005

One notes that Kascynski himelf, while in high school, was small enough to be shut in a school locker by bullies. Perhaps his interest in the history of short people is personal

9.
July 15, 2005

At least he doensn't post comments on your blog.

Or does he? Any IP addresses out of Florence, Colorado?

10. **Joe Bolte**
July 15, 2005

The Unabomber! What do I win?

Cosmic Variance

July 18, 2005
Categories: Uncategorized

Sean Carroll, of Preposterous Universe, has joined forces with Mark Trodden (of Orange Quark), and new bloggers SLAC particle phenomenologist JoAnne Hewett, USC string/brane theorist Clifford Johnson, and Chicago cosmologist Risa Wechsler. They'll be collaborating on a new weblog entitled Cosmic Variance, and I'm looking forward to following what they do with it.

This may be part of a new trend of consolidation in the physics weblogging industry, following the lead of the String Coffee Table and the massive, multi-national, government-subsidized Quantum Diaries site. Will small, independent, artisanal producers like myself be able to compete with huge combines like Cosmic Variance, with their professional software and expensive ($6.95/month!) web-hosting services? Or will we be driven out of business as our profit margins are squeezed to the vanishing point? Wait a minute, I'm not making a profit at this anyway…

Actually, today I'm in Austin, Texas on personal business. I suppose I should be looking up Jacques to see if he, Lubos and I can organize an even bigger competing organization.

Comments

1. **D R Lunsford**
   July 22, 2005

   If Peter and Lubos combined efforts, it would be called "Not even close".

   -drl

2. **Bob McNees**
   July 19, 2005

   "Saying things like this to impressionable graduate students is really educational malpractice."

   Yes. You should totally start a blog with Jacques.

3. **Arun**
   July 18, 2005

   As long as you don't compromise your independence…

4. **Matthew**
   July 18, 2005

   I'll second the Peter/Jacques/Lubos blog idea. Reading that would take entirely too much of my time 🙂

5. **Sean**
   July 18, 2005

   Peter, you should definitely team up with Jacques and Lubos. I would certainly read that blog.

6. **Levi**
   July 18, 2005

   You are forgetting the economies of scale in their operation. $6.95/5 = $1.39. You don't stand a chance.

Hawking Paper

July 18, 2005
Categories: Uncategorized

It has been almost exactly a year since Hawking gave a talk in Dublin claiming to have found a resolution of the black hole information paradox. Tonight a preprint giving some details of his argument has appeared.

I'll leave to the quantum gravity experts the evaluation of exactly how convincing Hawking's argument is. It is based on using the Euclidean quantum gravity framework, which Hawking refers to as "the only sane way to do quantum gravity non-perturbatively". I've always been fond of the idea that you have to think about QFTs using a Euclidean signature for the background, so I wouldn't argue with him about this point, but I assume others will.

Comments

1. **Lubo? Motl**
   July 23, 2005

   My comments about the article are available on my blog.

2. **D R Lunsford**
   July 22, 2005

   I say, so what? The Clifford algebra of spacetime is *not* H(2), because in the non-relativisitic limit the Dirac theory has to reduce to the Pauli 2-spinor theory. The H(2) theory would lead to Majorana fermions, and these are not seen. Dirac spinors are *not* quaternionic.

   -drl

3. **Tony Smith**
   July 20, 2005

   Peter Woit discusses "… Euclidean signature and with Minkowski signature, especially when there are spinors …", saying
   "… Mathematically the simplest way of saying what you have to do is to formulate the theory in Euclidean space and analytically continue. …".

   Danny Ross Lunsford then commented: "… Yes, but this introduces yet another complex structure that requires interpretation. …".

   John Baez at http://math.ucr.edu/home/baez/symplectic.html made an observation that may be relevant:
   "… Fermions are quaternionic …".

   Therefore, if you want fermions/spinors in your (1+3)-dim spacetime, you want quaternionic structure.
   Since you want fermions/spinors, look at the relevant Clifford algebras, which for (1+3)-dim spacetime can have signature either -+++ or +—.
   Although their spin groups are isomorphic to each other (and to SL(2,C)), the entire Clifford algebras are distinct, one being M(4,R), the 4x4 real matrices, and the other being M(2,Q), the 2x2 quaternionic matrices.

   If you follow John Baez and go to the quaternionic M(2,Q), you see that the vector spacetime is not a real 4-dim Minkowski space as to which you must add a complex structure to get analytic continuation,
   but
   is in fact a 1-dim quaternionic space which has its own built-in machinery for analytic continuation.

   Therefore, if you follow John Baez's observation to its logical conclusion, you get the benefits of Peter Woit's analytic continuation without the introduction of Danny Ross Lunsford's extraneous ad-hoc complex structure.

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

4. **D R Lunsford**
   July 20, 2005

   Peter,

   Yes, but this introduces yet another complex structure that requires interpretation. The existing complex structures (e.g. electromagnetic duality, Dirac algebra and spinors, 2-spinors..) have tight geometric interpretations, much as the "circular points at infinity", the solution to R^2=0, characterize Euclidean space. In fact if there is going to be any interpretation of this analytic continuation it will amount to mutating the light cone, which characterizes propagation, with the circular points at infinity, which charactize distance.

   -drl

5. **Peter Woit**
   July 19, 2005

   I'm certainly not sure what is the exact relation between QFTs in curved spaces with Euclidean signature and with Minkowski signature, especially when there are spinors, but something interesting is going on. Even in flat space, free field QFT, if you try and directly formulate it in Minkowski space you run into trouble (the propagator is given by an integration contour that goes through poles) and you have to do something. Mathematically the simplest way of saying what you have to do is to formulate the theory in Euclidean space and analytically continue.

6. **D R Lunsford**
   July 19, 2005

   Peter – it doesn't bother you pretending that the signature of spacetime is definite?

   -drl

7.

July 19, 2005

I think that Hawking, as well as Penrose, are upset that Hollywood has overlooked them in their latest venture.

Has anything Hawking predicted been experimentally verified?

Has anything Penrose predicted been experimentally verified?

http://physicsmathforums.com/showthread.php?t=56

Tied Up & Strung Out: Hollywood String Theory Movie!!! Looking For Extras!!!
FOR IMMEDIATE RELEASE:

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Brian Greene, the famous String Theorist who will be played by David “the truth is out there” Duchovney, explained the plot: “String theory’s muddled, contorted theories that lack postulates, laws, and experimentally-verified equations have Einstein spinning so fast in his grave that it creates a black hole. In order to save the world, we String Theorists have to stop reformulating String Theory faster than the speed of light. We are called upon to stop violating the conservation of energy by mining higher dimensions to publish more BS than can accounted for with the Big Bang alone.”

Greene continues: “At first my character is reluctant to stop theorizing, but when my love interest Jessica Alba is sucked into the black hole, I search my soul and find Paul Davies there, played by John Cleese. I ask him what he’s doing in my soul, and he explains that the answer is containined in the mind of God, which only he is privy too, but for a small fee, some tax and tuition dollars, a couple grants here and there, and an all-expense-paid book tour with stops in Zurich and Honolulu, he can let me in on it. But as soon as he points out that we can make more money in Hollywood than printing more coffee-table books that recycle Einstein, Bohr, Dirac, Feynman, and Wheeler, I am converted. I agree to turn my back on String Theory’s hoax and save Jessica Alba.”

But it’s not that easy, as standing in Greene’s way is Michio “king of pop-theory-hipster-irony-the-theory-of-everything-or-anything-made-you-read-this” Kaku, played by Jackie Chan. Kaku beats the crap out of Greene for alomst blowing the “ironic” pretense his salary, benefits, and all-expense paid trips depend on. “I have better hair!” Kaku argues as he delivers a flying back-kick, “There can be ony ONE! I WILL be String Theory’s front man!!”

But Greene fights back, “Kaku! Kaku! (pronounced Ka-Kaw! Ka-Kaw! like Owen Wilson did in Bottle Rocket) It is theoretically impossible to build a coffee tables strong enough to support any more coffee-table physics books!!! And what the #&#%&$ does M stand for in M theory???”

How does it all end? Does physics go bankrupt funding theories that have expanded our ignorance from four dimensions into ten, twenty, and thirty dimensions? Do tax payers revolt? Do young physicists overthrow the hand-waving, contortionist bullies and revive physics with a classical renaissance favoring logic, reason, and Truth over meaningless mathematical abstractions? Does Moving Dimensions Theory (MDT) prevail with its simple postulate? We’ll all just have to wait!

But in the meantime, how do you think it will play out?

MDT’s postulate: THE FOURTH DIMENSION IS EXPANDING AT A RATE OF C RELATIVE TO THE THREE SPATIAL DIMENSIONS IN QUANTIZED UNITS OF THE PLANCK LENGTH, GIVING RISE TO TIME AND ALL QUANTUM MECHANICAL AND RELATIVISTIC PHENOMENA.

http://physicsmathforums.com/showthread.php?t=56

Two Cheers for String Theory

July 21, 2005
Categories: Uncategorized

Over at the new Cosmic Variance blog, Sean Carroll has posted a defense of string theory against what he sees as disdain, resentment and disparaging remarks from other physicists, a defense he entitles Two Cheers for String Theory. I've written a couple of comments over there, and maybe this will lead to an interesting discussion. But I'll be traveling a lot of the time during the next week and a half, so my ability to participate in such a discussion, here or over there, may sometimes be limited. We'll see what happens….

Comments

1. **Thomas Larsson**
   July 29, 2005

   Wolfgang,

   I am not talking about conventional anomalies proportional to the third Casimir, which indeed are inconsistent – the anomalous algebra does not possess any unitary lowest-weight reps. If you introduce the observer's trajectory and quantize it together with the fields, there are also new anomalies proportional to the second Casimir. This is necessary to do canonical quantization in a manifestly covariant way, see hep-th/0501043.

   It should be possible to describe these new anomalies also in the conventional, non-covariant Hamiltonian formalism, although I have not thought so much about it. The YM gauge algebra can be cast in the form

   [J^a(m_0,m_i),J^b(n_0,n_i)]
   = f^abc J^c(m_0+n_0,m_i+n_i)
   + k \delta^ab m_0 \delta(m_0+n_0)\delta(m_i+n_i),

   where four-momentum m = (m_0,m_i) has been split into temporal and spatial components. The extension can be expressed covariantly, but this form is suitable to make my point. Note that this is a 4D generalization of the affine algebra, and it is easy to show that it is indeed a Lie algebra.

   The spatial subalgebra, generated by J^a(0,m_i), is anomaly free. This means that you can construct the Hilbert space as usual, and mod out spatial gauges. However, a rarely observed fact is that the temporal gauges are implemented as time-dependent canonical transformations. If the extension is non-zero, you will run into serious trouble with this.

   This does not happen for the free Maxwell field, because the adjoint rep of U(1) is trivial. But it does happen in interacting theories, where the second Casimir k != 0. I do not understand this in detail, because I have only quantized interacting theories in a formal sense, but it must be related to renormalization.

   Anyway, the second-Casimir extension is simply there, and it always arises when you build lowest-energy reps of the gauge algebra, see math-ph/9810003.

2. **Alejandro Rivero**
   July 28, 2005

   I have been reading this review of string theory, very complete including susy, D=26, D=10 etc…
   J. Scherk An introduction to…
   http://prola.aps.org/abstract/RMP/v47/i1/p123_1?

   Now I think about, they missed the opportunity to stab the muon as a susy partner of the pion.

3. **WL**
   July 28, 2005

   Hi Thomas,

   I did not say nor imply that anomalous local gauge theories would be consistent, rather that you'd be warmly invited to try to prove this 😉

   In fact, it is standard knowledge that it is not possible; unitarity will be lost (and renormalizabilty: the anomalous graphs scale not in the way the non-anomalous ones do, and there is no renormalization scheme where you could cancel the divergences of both types of graphs simultaneously).

   If nature is any guidance: the spectrum of the standard model is supposed to be anomaly free, and this had in the past led to the prediction that given the tau lepton there should be a bottom quark (and similarly a top quark given the tau neutrino). This prediction has been experimentally verified later, and one may view this as a spectacular triumph where theory made a prediction based on consistency, ahead of experimental data.

   Summa summarum, I don't see why one would insist on abandonding gauge invariance and giving up consistency for no good reasons.

   As for gravity, things may be more subtle and it looks indeed that background (in-)dependence may have something deep to do with anomalies. In topological strings there is a beautiful story relating certain anomalies to an apparent background dependence, and this could be a prototype for something more general; there are some recent, extremely interesting papers on that. However, the involved "holomorphic" anomalies are not crucial for the consistency of the theory, so there is no parallel to local gauge symmetry; rather, it is more the other way around, namely the holomorphic anomalies arise _because_ one insists on a consistent geometric interpretation of the theory.

4. **Thomas Larsson**
   July 27, 2005

   Wolfgang,

   I thought we agreed that gauge anomalies cannot in general be dismissed on the grounds of unitarity violation – the subcritical free string. The chiral Schwinger model in 2D is another example, at least according to Roman Jackiw. As for 4D gravity, here is what I have and have not done.

   I don't see how to use path integrals, so I prefer a version of canonical quantization which is more directly connected to representation theory. But any correct quantization method should do.

   Moreover, I don't know how to prove unitarity, because I don't know what the invariant inner product is. But I do know that every non-trivial, unitary rep of the diffeomorphism algebra must be anomalous (in 1D, c=0 implies h=0), and I know how construct anomalous reps. So at least I satisfy a necessary condition.

   The problem, both for path integrals and unitarity, is this: In order to avoid infinities, I must first expand all fields in a Taylor series around the trajectory q(t), and truncate at some finite order p. This gives me a classical, non-linear realization on finitely many fields of a single variable, which is exactly when the normal-ordering prescription works. Without this step, normal ordering gives infinities and diffeomorphisms do not act in a meaningful way.

   Thus everything is expressed in terms of Taylor data instead of field data. Classically, this is nothing, but I don't see how to make sense of a path integral over Taylor data. And although one can readily write down inner products, I haven't found an invariant one. Another problem is that truncation to order p is a regularization, which must be removed at the end. Infinities resurface in this limit, and can only be cancelled with a clever choice of field content.

   I don't claim to have quantized gravity. However, I have quantized (on a linear space rather than a Hilbert space) a regularized form of gravity, while maintaining manifest diffeomorphism covariance, constructed the relevant anomalies, and derived conditions when the regularization can be removed. I think that that is a rather significant achievement, in particular since there is no abundance of good new ideas around.

5. **Juan R.**
   July 26, 2005

   Thanks Quantoken,

   I contacted with Eotvos group and are waiting for reply.

   I cannot "rebate" your arguments now, since that i discover this posible weakness of Newtonian potential today.

   However, let me say that like in some well-known LED models (e.g. mm-scale string extradimensions), perhaps the reduction of dimensionality could be undetected with usual non-gravitational thecniques being real.

6. **WL**
   July 26, 2005

   Thomas,

   the issue of background independence in QG is a subtle one and I prefer not to get drawn into this. But as far as local gauge anomalies are concerned, I wouldn't see how an anomalous theory would make sense from a path integral point of view. And I would expect important basic properties like unitary getting violated – so in order to be convincing, why don't you cook up a proof that anomalous gauge theories are unitary, that would help your case !

7. **Quantoken**
   July 26, 2005

   Juan said:
   "Basically the idea is as follow if recent sugestion of experimental verification of weakenes of Newton force to short scales is correct. This would be a final knock to String M theory."

   Not to defend the string M theory, but it should be clear that the "recent experimental verification" of weaker gravity force at shorter distance is very weak in credibility, and does not say anything either way. Further, it has **nothing to do with reduction of dimentionality, even if the reduction of force is credible and verifiable. Clearly the distance scale at which the allerged force reduction happens is well above atomic scale, and we have plenty of solid evidence that at atomic scale everything is just as 4-D as the macroscopic scale. Should dimentions start to reduce, which could be possible, it should start at a much much smaller scale.**

   Quantoken

8. **Juan R.**
   July 26, 2005

   Recent post in cosmic variance

   Basically the idea is as follow if recent sugestion of experimental verification of weakenes of Newton force to short scales is correct. This would be a final knock to String M theory.

   Ignoring possible dependence on relative velocity, one obtains strong effective gravitational interaction to shorter distances, I take like good the rule 1/r^(2+d) for d extra dimensions (some recent RS brane model introduces Yukawa like exponential correction from extra 5th dimension), we can observe that smooth behavior is obtained formally with

   d 0 imply formally elimination of divergencies on (1/r^2) force strengh since (1/r^2) ?-> (1/r^0) at short scales without appeal to an arbitrary (by hand) add cut-off.

Are not these exciting news?

9. **Thomas Larsson**
July 26, 2005

Wolfgang,

Although people know that subcritical strings are fine, they still seem to believe that gauge anomalies are inconsistent, and that gauge symmetries are redundancies of the description. Lubos has repeated that phrase for five years. Moreover, in a discussion a long time ago, Jacques Distler implied that there is a fundamental difference between conformal gauge symmetries, relevant in string theory, and conformal global symmetries, relevant to 2D critical phenomena. Why would he do that if he realized that gauge symmetries can become global upon quantization?

So one thing I want to do is to eliminate the widespread myth that all gauge anomalies are inconsistent and must be cancelled.

This issue comes up in the context of diffeomorphism symmetry in QG. It was always obvious to me that the diffeomorphism group will acquire anomalies in 4D QG, which is pretty obvious since already 2D QG has gauge anomalies. (The anomaly can be traded between the Weyl and diff sectors, but it can not be removed.) With this is mind, I generalized the Virasoro algebra to higher dimensions (in particular 4D), and worked out its Fock representations. Fortunately, the same problem was simultaneously addressed by mathematicians Rao and Moody (of Kac-Moody fame), which helped me overcome the crucial obstacles.

In case you think that there are no pure gravitational anomalies in 4D, it is only true if you quantize the fields alone. A crucial insight is that one must also explicitly specify where observation takes place, and quantize the observer's trajectory together with the fields. This is mandatory because the relevant Virasoro-like cocycles are functionals of this trajectory.

One can view the controversy between ST and LQG in the light of this result. The key lesson of GR is background independence, and the key lesson of QM is that it is QM, in the Fock sense. However, Lee Smolin has informed me that a rigorous theorem rules out anomaly-free Fock quantization of background-independent theories, and I see no reason to doubt that assertion, partly because I have proven similar (but very non-rigorous) theorems myself. Locally, this leaves three possibilities:

1. QG is not background independent. A lot of people would dislike this possibility, because it would violate the spirit of GR, but it is a logical possibility.

2. One should not quantize in the Fock sense, but only in the weaker LQG sense. A lot of people would certainly dislike this, especially the part of the unbounded harmonic oscillator spectrum.

3. The diffeomorphism symmetry is anomalous. This neither violates the spirit of QM nor GR, it is known to happen in 2D, and much of the math is now here. However, the anti-gauge-anomaly myth prevents this idea from being taken seriously.

10. **WL**
July 26, 2005

Thomas,

that strings with c less than 26 are fine and make sense is known since a long time, there are hundreds of papers (I guessimate) on this issue called non-critical strings. This includes the well-investigated c=1 model, etc. What is what you want to convey – that professional physicists wouldn't know about this ?

As for desinformation, it is one of the unfortunate virtues the internet has brought to us, namely that laymen can just go ahead and spread nonsense, and other laymen are sadly influenced by this as they have no way to distinguish crap from serious science. This is what I meant when I was referring Q's statements, in relation to someone expressing appreciation for them.

11. **Thomas Larsson**
July 26, 2005

Wolfgang,

The most notorious desinformation spreaders are those highly educated people who claim that a gauge symmetry necessarily is a redundancy of the description, despite the fact that 2D gravity coupled to D &lt 26 scalar fields is neither inconsistent nor anomaly free. It has taken me a long time to reeducate Lubos, but now I think that he finally understands this point.

It is not surprising that quantizing 4D gravity fails if you don't know about the relevant diff anomalies.

12. **WL**
July 26, 2005

"Quantoken,

Thank you for the explanation; much appreciated. That would be a bitter disappointment if the LHC were not to get up and running.
"

Indeed. Fortunately I see many thousands of highly educated people around here working hard to make sure it will work. They know what they are doing, in contrast to those notorious desinformation spreaders who have not the faintest clue of what they talk about and who would still live in caves if all of mankind would think like them.

13. **Juan R.**
July 25, 2005

Mike Crowley said

Please forgive my ignorance, but I was recently watching a lecture for non-scientists on particle physics that seemed to suggest that the discovery of super-partners for the elementary particles might be a validation of string theory, and that this is something potentially achievable at labs like CERN and FERMILAB. Is this a misunderstanding?

Well, personally i suspect that supersimmetry will be not observed (as already suceeded in the past).

Supersimetry is not a prediction of String theory. It is a requirement put by hand for admending tachionic behavior (experimentally unobserved). The observation of supersimmetry will be not a verification of ST.

Moreover, ST predicts exact supersimmetry and if observed in a future experiment we will observe only high-energy supersimetry behavior, but still low-energy non supersimetric one. ST **fails** to provide a mechanism for the observed low energy behavior and, therefore, is incorrect.

14. **Michael**
July 25, 2005

Hi Peter,

“you can’t reliably calculate anything at generic values of the moduli and couplings”

That’s what I said when I wrote that it’s hard to compute things in the middle of moduli space absent a more unified definition of string/M-theory. But it’s not so terribly discomforting as you claim. Certain points in moduli space can be shown to have low-energy limits which are exoctic things like E_8 gauge theory or massless “gauge strings”. We have no idea how to right down an effective Lagrangian in such cases. On the other hand, in the string theory, many things remain computable in principle. If string theory is worth nothing because it can’t give better answers to these questions yet, then QFT is even worse off. We would have never even known that we *should* be able to make sense of things like E_8 gauge theory. Understanding this means realizing that it would be more than amazing if we had been able to understand all of the moduli space from the out start.

But we’re getting there…

15. **Mike Crowley**
July 25, 2005

Quantoken,

Thank you for the explanation; much appreciated. That would be a bitter disappointment if the LHC were not to get up and running. I remember how disappointing it was when the Superconducting SuperCollider project was canned–it would probably be operational by now.

16. **Peter**
July 25, 2005

Michael,

No, you don’t have a “good non-perturbative formulation” and you can’t reliably calculate anything at generic values of the moduli and couplings. If you look at the discussion over at cosmicvariance you’ll see the professional string theorists admitting this. You’ll also note that they and Sean agree with my intial point that you found so ignorant. As Sean points out, one way of seeing that non-perturbative string theory may have amplitudes that look like field theory ones, not stringy ones, is just to look at the 11d supergravity corner of the conjectured moduli space for M-theory.

I’m kind of charmed at the idea that I’m bullying the likes of Jacques Distler, Lubos Motl, or other serious string theorists. But you should know a lot about bullying,since you seem to think the way to deal with someone who makes serious arguments that threaten your beliefs is not to answer them but to personally insult and attack the person making the arguments.

17. **SocialRetardTeenager**
July 24, 2005

Peter,

The suspenders holding up my pants broke and I had to use a *string* to tie them back up.

18. **Quantoken**
July 24, 2005

Microly said: “….a theorist saying that the worst thing that could happen in particle physics at higher energies would be to discovery “only” the Higgs.”

That’s because the Higgs particle is the only missing piece of the standard model that has yet to be confirmed by experiments. People wants to go beyond the standard model so they need some clue by finding something that the standard model failed to predict. If Higgs and only Higgs is found, then the picture of standard model is complete but they still can not move beyond it, so that’s the worst of possible outcomes.

The problem is nobody said anything specific about how these supposed super partners look like. Certainly that’s true for Higgs particle or Higgs particles as well. If you can’t tell specific details you can’t really say you have predicted something. They know absolutely nothing about the allerged new particles they expect to discover, except for the names. So you bet they have prepared a bunch of printed labels so anything that jump out of LHC will be automatically assigned a label and be claimed as something they predicted beforehand.

I predict that the most likely outcome of LHC is it be killed before producing anything useful, either because
1.it run far over budget, or because
2. they fail to achieve the design specifics, or
3. everything work as expected but still no new discoveries except for maybe a couple of high energy resonance states of known particles.

Quantoken

19. **Mike Crowley**
July 24, 2005

One other quick question: I remember reading a quote from a theorist saying that the worst thing that could happen in particle physics at higher energies would be to discovery “only” the Higgs. Is this related to the super-partner search? This comment bewildered me, because I came away from these lectures thinking that the discovey of the Higgs would be an important triumph.

20. **Mike Crowley**
July 24, 2005

I really appreciate this discussion that has been going on across several blogs, and appreciate also entries like this that take into account that laypersons are interested in this difficult topic as well.

Please forgive my ignorance, but I was recently watching a lecture for non-scientists on particle physics that seemed to suggest that the discovery of super-partners for the elementary particles might be a validation of string theory, and that this is something potentially achievable at labs like CERN and FERMILAB. Is this a misunderstanding?

Thanks!

21. **Juan R.**
July 24, 2005

Lubos has written a reply to Sean article in his own [blog].

Curiously, he agrees with me on that the article is outdated and shows very wrong ideas about real status of string theory. As said in several occasions, research ST is not the same that layman version of ST.

My several years claim of that string theory is a theory without strings and the name is maintained by marketing purposes are supported by Lubos now.

22. **Quantoken**
July 24, 2005

Michael below:

Do you realize that your response below had NOT disputed a single point Peter raised. If any thing you enhanced Peter's arguments where ever you can. You seem to be pretty weak in logical skills and must look quite dumb if been spotted walking on the street.

I especially like this:

Peter said to Michael:
"You seem very proud of knowing what is in the first chapter of Polchinski, but **do you know anything else about physics**."

I would have removed the unnecessary word "else" above. Michael may have wasted decades training himself in being able to recite the whole book of Polchinski in reverse order without a mistake, but he surely had NOT picked up any idea what is science and what physics is all about. Physics is all about being able to explain and predict observables. Not a single thing in Polchinski book, or any other super string theory stuff has proven itself relevant to anything in nature.

You may continue to wish that there will be more super string revolutions to come. But over 2 decades of research so far yields absolutely nothing that is valuable, and can be summed up in one word "failure". In light of that, any wish that the situation could change in the near future, can only be described as wishful wishes.

I think some one must have got to be incredibly stupid to be so willing to plunge his/her intelligent lifetime into such a hopeless pursuit, like dropping a stone into water, knowing full well how fruitless this could be. While people pursuing some other un-explored ideas may probably be gambling for success, SSTers can only be described as suicidal.

People like Edward Witten must have an IQ far exceeding Einstein. But I view them as Einstein's wasted because they are pursuing the wrong path. As I said already if the nature is not co-operating and is not 10-D, then not even god can create a 10-D theory to describe a none 10-D universe, unless it's either a wrong or an useless theory.

Michael, thousands of much more qualified researchers before you have wasted a collective one thousand intelligent lifetimes without figuring out something useful. What makes you think you are any different from them, in terms of luck? Or you consider this whole enterprise as just a job to earn livelihood? Then there are much easier ways to do it.

Quantoken

23.
July 23, 2005

Some analogy:

String = WMD
String Theory = War in Iraq
String Theorists = NeoCon

24. **Torbjorn Larsson**
July 23, 2005

Michaels:

"What I dislike is pretenders."

I understand the general feeling. This time you didn't drag in bystanders into that sentiment by falsehood.

Thomas:
"I don't think that neither you nor Michael grasped the discussion over at Lubos' blog."

It is correct that I do not grasp the technical part. What I meant was probably correct was when Michael accused you of bringing up a seemingly detail as "the most important lesson of string theory" as trying to seem important.

Now you have explained your inspiration in a much different light. I am sorry if I hurt your feelings.

Nigel:
"Torbjorn ? please realise it is personal when the rejection is made because I am not contributing to mainstream string speculation."

I sincerely does not understand your thinking. If you had contributed 'to mainstream' you had not been rejected, you say so yourself. There is absolutely nothing personal (about you as a person) in that.

Maybe you mean that you take it personally? As per above reasoning you should not.

25. **Juan R.**
July 23, 2005

I add my reply to Sean here

Dear Sean, the problem with current unpleasant status of string theory into the community was built by own string theory community. Let me first remember to you some basic points ?the list, of course, is not exhaustive?, which will help to you to rewrite your ?cheers?.

– String theory did born like a failure to explain strong force and since it has been always a complete failure. Nothing predicted and all past claims shown to be false. String theory is a theory without laws or postulates because they are modified with time. Please, let me remember to you the history of dimensions: 4D -> 5D -> 26D -> 10D -> 11D -> 12D (some people is working in more than a time dimension) -> 4D (Segal has claimed that we may find a 4D version for solving compactification problems), etc. The claim of string theory is ?open? is, of course, a complete nonsense when claim is properly interpreted on both epistemological and ontological terms.

– It is well known that string theorists have manipulated public opinion about string theory. In fact, no popular string theory writer has still convincingly explained to public that string theory failed like a TOE since is being substituted by still unknown M-theory. The popular dissemination of string theory to non-experts violates the most basic ethic guidelines.

– The arrogant attitude of many string theorists is also very well known. Please talk with some critics of them like Peter Woit or Glashow and learn the true sense of the word ?pressure?.

– String theory is a mathematical goulash and a dishonest copy of formalisms developed by others. For example, after of decades of very wrong claims about the supposed TOE, now string theorist recognized that were wrong since usual quantization of string was not exact. Now they are launching the TFD version of string theory BUT TFD was previously developed outside of string theory. Again, non-string theorists were correct and ?smart? string theorists (e.g. Witen Greene, Vafa, Schwartz, etc.) completely wrong. In fact, none string theorist did contributions to TFD. Even some string theorist has recently recognized that string theorists usually copy the work of others and after ?rename? it like string theory.

– All past claims by string theorists were shown to be false, absolute all! In fact, the popular idea of that pointilike particles would be substituted by one-dimensional strings has been superseded by recent M(atrix) formulation by Banks and others, which is basically a quantum mechanics of pointlike particles (D0-branes). People again are ignorant of that, since that, like openly admitted by some theorists, the old name ?string theory? is maintained by marketing purposes.

– It is also well knonw and denunciated in several occasions that string theorists have ignored other approaches to quantum gravity. It is very hard for a loop theorist to hear in a popular talk ?given by a string theorist? that string theory is the only approach to quantum gravity. The only game into the city!

– It is also well known that young researches were forced to research into string theory because financial support of other theories was stopped in departments, funding agencies, and others due to aggressive string marketing activities. Many young physicists begin a PhD on string theory, discovered that string theory was a waste of time (real string theory is not the same that popularised version of string theory), and leaved the field. Some of them feel…

– Let me take a simple example from chemistry. According to ?ignorant? and very arrogant people like Ed Witten, string theory is a promising TOE and reduces all of others sciences, e.g. chemistry. A moment, chemists know that is false, the reduction of chemistry to physics is a myth, as brilliantly explained in innumerable papers in Foundations of chemistry and others journals. Interestingly, 30 years ago some chemists were working in advanced formalisms for explaining behaviour that cannot be studied with usual methods. If you compare the very advanced theoretical work developed in the 60 and 70s with corresponding string theory status you found that string theory was wrong like a TOE even a joke. String theorists, arrogant as they are, ignored all of that and claimed that all of chemistry was an application of string theory. String theory was so advanced that no one other theory could provide to us an explanation of nature more profound, they said. Of course, chemists smiled, like they smile in the 20th century, when physicists (including Nobel laureates like Stark) attempt to convince to them that chemical bond was modelled by classical electrodynamics and that Lewis bond theory was, in simple words, nonsense. Now in the last part of 90, some string theorist discover that all past claims were wrong and are developing a new version of string theory called non-critical one. It is interesting that all past quantum methods and basic stuff is abandoned whereas work developed in the 60s by chemist Ilya Prigogine (see for example his Nobel lecture) used for a radical generalization of old string theory. But the ideas used NOW in string theory were developed in the 60s by other people! Prigogine and others were correct, string theorists again wrong. Interestingly, the ideas of the 60s have been updated in the 90s by the Prigogine and co-workers. Therefore, the current ?radical? generalization of string theory by string theorists is, again, an outdated theory. This is real status of string theory; an authentic revolution if one read that masterful piece of marketing called the Elegant Universe (by Brian Greene) and focused to laymen, but claimed to be ?very conservative? and outdated in the recent conference Quantum future by expertises that know stuff. Said I again once more? The first step for any serious theorists is to read previously published literature and then develop a better theory, but crackpots are specialist in ignoring the scientific method. If string theorists continue to develop a really outdated theory at one hand and arrogantly claim that are doing (they believe that in their infinite ignorance) the most important, the most powerful, the most fundamental theory at the other, then they would feel comfortable with the mocking of their colleagues. If Brian Greene, offensively claim in his talks that we may quantize everything, and Dyson convincingly reply him saying that Greene is providing no solid arguments in his belief, the problem is not with Dyson, the problem is with Brian Greene, that would first study serious stuff before doing irrelevant claims surrounded by a halo of pomposity.

– Etc.

Sincerely, I believe that non-string theories have been very generous with string theory community. String theorist would please to us our kindly attitude.

Once ?refreshed? your memory, let me now comment some of your points.

The idea of that string theory is the most promising way to reconcile gravity and quantum mechanics, is one of well-established myths of literature. In an absolute sense, loop quantum gravity is so ?successful? like the strings but in a relative sense (successes / total number of researchers), the loop approach has been around 10 times more satisfactory. Please, let me remember to you again that string theory has been substituted by M-theory.

It is false that ?string theory? is based in one-dimensional loops. In fact, you appear to unknown the current joke on Internet that say that ?string theory is now a theory without strings?. Yes, you obtain a remarkably rich structure, but just at mathematical level.

It is false that string theory predicts or explains gravity (this is another myth). ?To predict? a massless spin-two particle is not the same that quantum gravity. In fact, causality is defined on a flat fixed metric with graviton modes arising in the perturbation, which violate GR basic idea of that full causality is defined in the full metric. This has been the main criticism of general relativists and loop theoreticians during decades. Now, string theorists are recognizing that great mistake (in the past they claimed that one would not take GR ?too seriously?) and are unsatisfactorily searching for a background independent version of the old (outdated) string theory.

?In string theory, you just say the word ?strings,? and gravity leaps out at you whether you like it or not.? This is not true, in fact one use previous ideas from GR, like to leave ?freedom? to the metric into the string action. Somewhat like we need know previously that universe looks 4D and then introduce an arbitrary (that is by hand) compactification 10D -> 4D x 6D.
?At this point it?s a little unclear what the fundamental building blocks of string theory are? It is clear that string is NOT the fundamental entity into the non-perturbative regime.

?One often hears that string theory simply makes no predictions, but that?s clearly false. If you scatter two particles together, string theory unambiguously predicts that the cross-section should look stringy, not like that of fundamental point particles.?

Humm, even ignoring that prediction really mean, this is another myth. Let me simply quote to D. Friedan:
?Even if some particular macroscopic background spacetime is chosen arbitrarily, by hand or by ?initial conditions,? string theory still fails to be realistic at large distance. The large distance limit of string theory consists of the perturbative scattering amplitudes of the low energy string modes, which are particle-like. But the particle masses are exactly zero, and the low energy scattering amplitudes are exactly supersymmetric. String theory fails to provide any mechanism to generate the very small nonzero masses that are observed in nature, or to remove the exact spacetime supersymmetry, which is not observed in nature. More broadly, string theory is incapable of

generating the variety of large characteristic spacetime distances seen in the real world. At best, for each macroscopic background spacetime in the manifold of possibilities, string theory gives large distance scattering amplitudes that form a caricature of the scattering amplitudes of the standard model of particle physics.?
The problem, of course, is not the popular statement of the difficulties for testing Planck-scale physics as you incorrectly argue; the problem is that nobody has obtained the successful standard model from string theory. A first step of any new theory is obtain that is already known before predict any new (including Planck-scale physics). So string theory is not compatible with available experimental data. ?it?s just that we are as yet unable to test them.? As explained above, this is wrong.

?Recently there has arisen another sense in which string theory purportedly makes no predictions, associated with the ?landscape? of possible string vacuum states.?

?Well, too bad. It would have been great to make such predictions, but the inability to do so doesn?t render string theory non-scientific.?

No comment!

?? and possesses a mathematical beauty that is so compelling that the theory simply must be correct.?

The world is as it is, no that we like we want that it was. Mathematical beauty is a guide newer a justification. Moreover, string theory is rather ugly, at least for me. About his supposed ?beauty?, sceptics suggest that string theorists try to colourfully camouflage the well-known theory's flaws, like "a 50-year-old woman wearing way too much lipstick?.

?These kinds of arguments just don?t carry that much weight with the non-converted.?

Yes, I believe that ?converted? is the correct word to use here. String community looks like a sect or, in the words of one of its most famous members, ?a kind of a church?. It is not science.

?it?s the most promising way we know to quantize gravity. If there were multiple very successful ways to quantize gravity, it would be important to distinguish between them experimentally; but so long as the number of successful models is less than or equal to one, it makes perfect sense to make every effort to understand that model.?

This marvellous piece of promotion just emphasize the myth of string theory is the only game in the city. Please read literature in semi phenomenological approaches to quantum gravity and recent advances in other theories like LQG and predictions for the future LHC.

Yes, the comparison between Microsoft and Apple Linux is correct!! Microsoft is a layman-oriented business, whereas Apple or Linux are more specific but more serious. Moreover, it is well knonw that windows OS is a copy of graphical Apple OS and certain kernel properties of Linux/Unix. The success of Windows is in marketing and layman orientation, somewhat like string theory. It is interesting remark that when Microsoft presented the revolution of the trash icon, graphical copy and paste, multitasks, and others features in his first versions of windows, all of that was already known for decades for Mac users. Remember the famous Windows blue display. Yes, your comparison is really good!!

?It didn?t have to work out that the entropy of a black hole calculated from semiclassical gravity ala Hawking would be equal to the entropy of a corresponding gas of strings and branes, but it is.?

Another myth!! Loop quantum gravity obtains the entropy of Schwarzschild black holes. In ?string? (really brane) theory, one traditionally has worked with BPS and idealized models of black holes. Strictly speaking, the ?traditional? results in string theory do not concern, precisely, black holes, as they are found in a limit in which the gravitational constant is turned off. But they concern systems with the same quantum numbers as certain black holes. There is a kind of analogy instead of an identity with GR black holes.

26. **Michael**
July 23, 2005

Dear Peter,

"You seem very proud of knowing what is in the first chapter of Polchinski"

It's chapter 6.4, Peter. Get a copy and try to catch up.

"The point I was making that you seem to have trouble following was just that if you don't really know what your theory is, and all you have is a perturbation series which you hope is asymptotic"

What you say is true: the different perturbative formulations are probably only asymptotic series. On the other hand they are dual to one another. OK, that's not a very concise definition, and it's hard to do calculations in the "middle" of moduli space. In the realm of each perturbative formulation a wealth of information on non-perturbative aspects is available (worldsheet instantons etc. pp.). Sure, we are quite anxious to tie this all together and find the underlying fundamental symmetry principle. But until then we have a good non-perturbative definition that consists of these many parts and aspects. Claiming that, absent the final concise definition, nothing can be done is simply false.

I personally think that there is a lot to do. Along the way we'll have to kill off the landscape and pay more attention to certain mathematical details that didn't seem to matter too much up to now. It will be fun and worth every hour of work.

"you don't know what the value of the coupling "

Well, we haven't found the way string theory selects the vacuum we live in. But we are able to consider various backgrounds, some of which are semi-realistic, and we know precisely what the coupling is in all of these cases. Furthermore, remember that there are stringy features common to any and all backgrounds. It's still in Chapter 6.4 of Polchinski…

"you can't reliably compute anything"

Look, Peter, You know it better. In QFT you have to choose a gauge group and fix a variety of parameters by hand before you can compute things. In string theory, at the present level of our understanding, the equivalent procedure is to choose a point or region in moduli space and a background. Once you do that, you can reliably calculate almost anything you want, at least in principle. The great advantage of string theory is that it naturally includes quantum gravity and that there likely is a dynamical mechanism that should eventually tell us what the background and moduli are (at the present age of the universe 😉). QFT has no hope of doing either. That's why we take the stringy extension of it so seriously.

"He's a real poster boy for string theory."

Thanks for the flowers. I wasn't even so kind to you as to deserve this kind of compliment.

"let's first hear your qualifications"

No, thanks, I don't need this silly kind of publicity that you use as a substitute for an academic career.

"Are you the socially retarded teenager you appear to be? Maybe an undergrad?"

No, but I wish I was that young. My social abilities have no bearing on the present discussion, do they? I have already conceded that it is reasonable on your part to consider me an a*hole and, if you want, a nerd.

"My qualifications include a 1985 Ph.D. in particle theory from Princeton, postdocs in physics at Stony Brook and math at Berkeley (MSRI), four years as an assistant professor at Columbia, and currently I'm a non-tenured full-time faculty member at Columbia with the title of Lecturer. I regularly teach graduate courses here, including one in quantum field theory recently."

Congratulations, that's a bunch of nice accomplishments. So why do you act like a chief bully now instead of being proud of yourself?

27. **Thomas Larsson**
July 23, 2005

Torbj?rn,

I don't think that neither you nor Michael grasped the discussion over at Lubos' blog. For five years, I and Lubos have disagreed about the consistency of gauge anomalies. His argument was always "A gauge symmetry is a redundancy of the description, you idiot". With my last post, I think I convinced him that he was wrong, since a counterexample can be found in the most elementary chapter of GSW. Boy, that must have hurt

28. **D R Lunsford**
July 22, 2005

In other words, Peter is a professor who has accountability instead of tenure. I like it.

-drl

29. **Peter**
July 22, 2005

I've been traveling, so internet access is intermittent. I'll probably soon try and delete some of the more off-topic posts. I'll leave the ones from Michael, whoever he is. He's a real poster boy for string theory.

Michael:

You seem very proud of knowing what is in the first chapter of Polchinski, but do you know anything else about physics. The point I was making that you seem to have trouble following was just that if you don't really know what your theory is, and all you have is a perturbation series which you hope is asymptotic to the real, unknown thing, and you don't know what the value of the coupling is, you can't reliably compute anything, and so you shouldn't be claiming to have predictions. Did you follow that? Do you have a counterargument?

Before we hear your counterargument, let's first hear your qualifications. Are you the socially retarded teenager you appear to be? Maybe an undergrad? My qualifications include a 1985 Ph.D. in particle theory from Princeton, postdocs in physics at Stony Brook and math at Berkeley (MSRI), four years as an assistant professor at Columbia, and currently I'm a non-tenured full-time faculty member at Columbia with the title of Lecturer. I regularly teach graduate courses here, including one in quantum field theory recently. OK, what about you?

30. **Levi**
July 22, 2005

Michael,

If you are such a hotshot string theorist, why aren't you over there discussing it with the other big boys and girls?

31. **Michael**
July 22, 2005

Hi Torbjorn,

I think it's great if "laymen" take interest in physics and string theory. I personally welcome everybody to talk and discuss such things no matter what their level of education or background might be.

What I dislike is pretenders. Why would Peter Woit, who's academic career ended in 1989, be in a position to lecture people like Sean Carroll and even Jacques Distler on string theory? This is disingenuous. If he were to ask or to try and understand, that would be great. Instead he is putting out bold claims debating professional physicists. In the process he reveals gaping holes in his knowledge of the subject's very basics. For example, the soft behavior of string scattering amplitudes is covered in any 1st semester grad course on string theory and is explained in detail in Polchinski's 1st volume. Yet Peter Woit essentially bases one of his arguments on the assumption that such behavior does not exist. His comments on perturbative vs non-perturbative physics are seriously confused, too. This is something he ought to know, having received a PhD in theoretical physics in the 80s. You know, there is a huge disparity between what he knows and is capable of, and what he pretends to be.

Please do not think that I do not welcome reasonable thoughts and arguments by everyone — even Peter Woit.

32. **Torbjorn Larsson**
July 22, 2005

Maybe Michael is the same as who attacked Thomas Larsson over at Lubos blog.

That Michael was probably correct; this one is seriously wrong.

Obviously there are non-knowledgeable people who can find interest in the discussion; I am one of them.

We will discuss ST whether or not practicioners take part. If they don't the discussions will be poorly informed.

And to try to supress the discussion is Not Even Wrong; it will lead to resentment and bad-will.

Nigel:
It was not false when they said that ST is the currently accepted theory, and it was not personal. It would be best if you can accept that.

Science is not democratic. It needs a democratic society, but nothing in the process of science itself is using democracy.

33. **Nigel Cook**
July 22, 2005

The Higgs boson explains inertial mass, which by Einstein's equivalence principle is the same as gravitational mass.

34. **Nigel Cook**
July 22, 2005

"Super string theory is even worse than astrology because it does not make any prediction at all. It hasn't even made even an astrology type of ambiguous and bad predictions. "

Quantoken, you have hit the nail on the head. The 'soft stringy' theorists are charlatans, always hoping to one day make a 'prediction'. Other people have to produce results to earn a living.

35. **Quantoken**
July 22, 2005

Michael said:
"How can I insult what does not exist? You are not a string theorist, wake up. Your pathetic obvious mistakes over at cosmicvariance.com are evidence enough. You don't know about the uniquely stringy "soft" behavior of string scattering amplitudes. And you are unable to see that non-perturbative aspects don't matter in the IR. Among many other things, this shows how little you know."

It does NOT matter a bit at all whether Peter has intimate knowledge about super string theory or not. It works the same way that Peter can confidently criticize astrology **without** necessarily knowing any thing about astrology at all. Any one with the least bit grasp of what astrology is can safely criticize astrology as superstition, because it does NOT make good predictions. Super string theory is even worse than astrology because it does not make any prediction at all. It hasn't even made even an astrology type of ambiguous and bad predictions.

So, knowing just that little bit of knowledge of the fact that super string theory has not made a single prediction, is sufficient enough to allow any one to criticize super string theory. I believe Peter knows at least that much of the facts, so he is fully qualified to be a criticizer of SST.

My point is one can waste decades of time to pick up tons of intimate math skills needed to "research" super string theory, but it still does not add you a single ounce of knowledge about the nature, since so far super string theory has proven no relevance to the nature and does not go beyond a mere mind exercise so far.

An obvious thing that I want to point out is no one is smart enough to change natural laws. **If the nature is inheritantly not a 10-D world, there is no way you can correctly describe nature using a 10-D theory**, no matter how smart or how hard you try. It's a dead end.

Quantoken

36. **Nigel**
July 22, 2005

'String theory' doesn't exist, what does exist is speculation. Serious people have better things to do than mudslinging over trivia of no consequence, that predicts nothing, leads nowhere, and cannot be tested. By Popper's definition of science as being merely tested speculations, 'string theory' is not science. What part of this can't the 'string theorists' grasp?

37. **Michael**
July 22, 2005

"Are you honest and realistic enough to know that you're a gutless asshole?"

Yes, I guess I'd feel that way if I were you. And I understand that my blunt ways must be painful for you. Judging by the vulgar expressions you use: very painful, indeed.

"I wonder why you're insulting my competence"

How can I insult what does not exist? You are not a string theorist, wake up. Your pathetic obvious mistakes over at cosmicvariance.com are evidence enough. You don't know about the uniquely stringy "soft" behavior of string scattering amplitudes. And you are unable to see that non-perturbative aspects don't matter in the IR. Among many other things, this shows how little you know.

38. **Levi**
July 22, 2005

Wonderful discussion over there!

39. **Peter**
July 22, 2005

Hi Michael,

Are you honest and realistic enough to know that you're a gutless asshole?

Either you're the 12 year old you appear to be, in which case I wonder why you're insulting my competence, or you're an adult string theorist, in which case you may (or may not) know more about string theory than me, but are definitely a really sad case.

Posting this kind of shit from the protection of anonymity is pathetic. Grow up, whatever age you are.

40. **Michael**
July 22, 2005

Why do you think the discussion might be interesting? You are not exactly knowledgeable in the area. I hope you are honestly and realistic enough to know that.

41. **D R Lunsford**
July 21, 2005

I guess it's postmodern cool to offer a defense of abject idiocy.

[physics, strings] = ih

-drl

42. **Carl Brannen**
July 21, 2005

"Two Cheers for String Theory" wasn't exactly a ringing endorsement. It was lukewarm at best.

One of the quotes was: "If true, this puts a damper on the hope that string theory would predict a unique vacuum state, and we could explain (for example) the ratio of the muon mass to the electron mass from first principles."

It turns out that if you square the sum of the square roots of the masses of the electron, muon and tau, you will end up with a mass that is exactly (to exp. error) 1.5x the sum of the masses of those three leptons.

I suggested this as evidence that the standard model was incomplete to an older physicist. His attitude was, "so what?" From his point of view the masses were arbitrary and it really didn't matter if they happened to fall in some sort of pattern.

Funny that he didn't feel the same way about the coincidences that the standard model managed to explain.

Carl

Some Quick Links

July 23, 2005
Categories: Uncategorized

Several months ago Erick Weinberg had told me that his recollections of the story of the calculation of the Yang-Mills beta function were different than David Politzer's. Erick actually did independently do the beta function calculation (for the case with scalars). At the time we talked he thought he had gotten the sign right, but the coefficient wrong, but now he has checked it and says the coefficient is right. He has posted his thesis on the arXiv, equation 6.68 is the beta-function. From the comments after this equation, you can see that he was aware that this meant that perturbation theory would break down in the infrared. Like 't Hooft though, who also did this kind of calculation, he wasn't aware of the significance of asymptotic freedom in the ultraviolet for explaining the SLAC deep-inelastic scattering results.

Fabien Besnard has a new blog (in French), which is quite interesting. His latest post is a report from a Paris conference celebrating the Einstein centenary. He's shocked by the comments of string cosmologist Thibault Damour that Popper was wrong, scientific theories don't need to be falsifiable.

The New York Times has an article about the actress Danica McKellar and her work in mathematical physics. She was working with Lincoln Chayes while an undergraduate at UCLA. Lincoln and his then-wife Jennifer (also a mathematical physicist, now at Microsoft Research) were graduate students with me at Princeton. I have many happy memories of them and their impressive leather outfits, and our joint trips down to the punk-rock club City Gardens in Trenton.

Comments

1. **Torbjorn Larsson**
   July 30, 2005

   stevep: That was much clearer and illustrative! I agree that questions of falsifiability should not immediately throw out ad-hocs. And I fully agree that there are other criteria to throw stuff out, as you illustrate nicely.

   Since we are using illustrations I too will take the opportunity to show how falsification can work. I was working in a group where we wanted to set up a model for a particular class of plasma processes to make thin films. This class has an hysteresis effect.

   Earlier models assumed the hysteresis (!), so they could not be falsified. We set up a model from surface mass balances that showed this effect without assuming it. That model was widely accepted because of that (and because it fit nicely without ad hoc parameters). I saw it work, so that is a reason why I feel strongly that falsification can be a tool in the tool box too.

2. **stevep**
   July 29, 2005

   Torbjorn: I can be less vague by talking about stuff I work with. Economic models often start by trying to characterize an individual's or organization's optimization problem. Then they solve for the agent's optimal policy (say, an investment decision) as a function of some exogenous parameters (say, a set of prices and technical features of the input/output relationship).

   There are plenty of potential points of empirical falsification in these situations–we could have the wrong objective function, the wrong exogenous description, or the agent may not optimize but do something else. All of these turn out, in general, to be hard to observe and test. Nevertheless, some of these models are useful and others are not, even when their falsifiability is held constant (say, at a very low level). When they are useful, they help us understand what is likely to be going on in a given problem and give us a start on figuring out what advice to give to someone in such a situation.

   Since you prefer to think about throwing theories out, the bad models have some combination of a) definitions of the agent's choice set and constraints that are hard to map intuitively to real-world problems, b) lots of technical detail that obscures the primary mechanism, c) strong assumptions about functional forms that are hard to characterize intuitively, d) omitted variables that are of equal or greater importance to those included (although abstraction is OK if done consciously and without fooling oneself). There's probably some other stuff, too, but those are the main bad things to look for. Usually, you can trade some of these off against each other, too, i.e. stronger assumptions can cut out extraneous technical detail. But lots of models get weeded out at review (or published but ignored) based on these kinds of considerations.

3. **Torbjorn Larsson**
   July 28, 2005

   stevep: I must be dense (or vacuumheaded 😉 since the details of your argument are vague to me. But it is interesting.

   Problem are that: You seem to discuss philosophy. I prefer to discuss use. You are interested in how to make a theory useful. I try to find out what makes it useless, IMO it is what we discuss and it is much easier to find.

   You discuss predictive formal theories that may have trouble being falsified. But their predictions are tested for falsification by large errors. The objection you make about small errors should go into the 'slippery notion' category.

   Your discuss nonpredictive formal theories, which are logical and mathematical theories and perhaps prototheories. The domains of logic/mathematics are tested then parts of them are used in theories on the real world. Prototheories should eventually be falsifiable.

   You discuss a floor tiling model that I can't agree with. It is the tiling model we are working on, not arithmetic that have been plenty verified earlier. This is exactly the matter of trust and why falsification is good.

   "Sorry to be so long-winded."

   It's OK as long as the ride is interesting.

4. **stevep**
   July 28, 2005

   Torbjorn: Perhaps I can clarify.

   You said: "Information theory is a widely tested theory. It is used and verified in data communications, data compression, and other fields, sometimes directly on channel capacity and error rates. I can't find the reference, but IIRC Shannon did verifications in his first paper. Shannon entropy corresponds closely to thermodynamic entropy which you may see as another field to falsify it in."

I agree that the theory is widely used because it is useful–that was my premise. My point was that these "verifications" are not empirical tests in the Popperian sense, driven by observation. They are all done by the application of formal logic. The conclusions of communication theory follow deductively from the assumptions, with no room for contingent empirical tests. There is no conceivable set of observations of communications systems that could cast doubt on the correctness of communication theory. That's not "slippery," it's the nature of deductive logic. If you think you've empirically falsified the theory, it must be because you inappropriately applied it.

The only "empirical" aspect to Shannon's theory is that his assumptions about the abstract transmitters, channels, and the rest map directly and cleanly onto their real-world counterparts. The correctness of that mapping is intuitively obvious and no one bothers to "test" it–if we found that the infomation counts didn't add up, we'd look for missing bit generators or absorbers, not worry that the theory is in danger of "falsification." The intuitive correctness of that mapping is also why the theory is so useful.

You expressed puzzlement about how logical omniscience (or its lack) relates to falsifiability. My point about logical omniscience is that a theory can make a contribution by clearing up logical inconsistencies in our thinking, even if it has no direct empirical falsifiabiltiy. The proof that there is no greatest prime number is valuable because we don't necessarily grasp it intutively before we see the proof. Yet the theorem is not empirically falsifiable. Contradictions in logic are not empirical tests, because we can postulate any abstract system we like without it having any necessary correspondence to the world we live in.

You say: "I think you allude to Peters 'slippery notion' of falsifiability. This is a indeed problem. One could also say that the theory is falsified and that a new one is needed. If you redefine your categories or their application, I could say that it is a new theory already."

No, I was not talking about modifying the theory (although that is an interesting set of issues). I am saying that if you count the tiles on your floor, discover that there are 5 rows and 4 columns but that when you count them you end up with 22 tiles, you are not going to modify your theory of arithmetic. Instead, you are going to postulate that extra tiles pop up when you're not looking, or some other modification of the application situation–not the arithmetic theory that appears to be "falsified."

You say: "These are criteria out of vacuum. Verification of theories can be done without them. c) is actually contrafactual; since you want general theories they tend to be simple instead of consisting of several particular ones. (Due to Occam's razor if you wish.) Instead they may be, and usually are, complicated to use in the particular case."

My head may bear some resemblance to a vacuum, and these criteria are indeed not the result of prolonged analysis. But there is a logic to them. I'm interested in what makes a theory useful.

If a theory is like arithmetic or communication theory or queuing theory–mathematically derived from a set of premises not subject to test–then it can't be useful because it rules out contingent states of the world. Rather, it helps us because we can use it to figure out the non-obvious consequences of our premises. If we're interested in real-world problems, then we need the theory to be clear about how to map the real world into those premises, and ideally it should be easy to figure out the conclusions of those premises.

Your point about Occam's razor is a good one. Simple theories of great generality may be hard to use in specific situations, even though their parsimony increases the chances that they are predictive. Their is thus a tradeoff in theory usefulness between these two considerations. But with theories whose predictivity is not in question, like arithmetic or communication theory, there is no real gain to Occam's razor (unless simplicity makes a theory easier to remember).

Sorry to be so long-winded.

5. **Alejandro Rivero**
July 28, 2005

I am happy with E. Weinberg uploading his old papers. Some moths ago, in his duty as PhysRev editor, he rejected one of my papers, and a couple weeks later I become really disturbed: I discovered that in the rejection he was using an standard template… instead of suggesting pointers to his old work, which could be of some value to improve mine!

What world do we live in, if the bureaucratic roles of seniors researchers block them of guiding the younger ones?

6. **Torbjorn Larsson**
July 27, 2005

stevep:

I have several problems with your commentary.

"One problem with falsification as a criterion is that it rules out something like Shannon's communication theory. Shannon's theory is the foundation of network engineering, but an empirical "test" of it is obviously ridiculous–it follows mathematically from assumptions and definitions about "sources," "channels," "coding," etc."

Information theory is a widely tested theory. It is used and verified in data communications, data compression, and other fields, sometimes directly on channel capacity and error rates. I can't find the reference, but IIRC Shannon did verifications in his first paper. Shannon entropy corresponds closely to thermodynamic entropy which you may see as another field to falsify it in.

"A big reason why they can be useful is because we are not logically omniscient–we do not instantly know all the deducible conclusions of the things we believe."

I fail to see that this means re falsifiability.

"Some of these conclusions may be impossiblity theorems, but they still are not empirically falsifiable."

By observing a contradiction to impossibility.

"Communication theory tells us that certain things are impossible, but they are logically, not contingently, impossible; if you ever saw an event that seemed to violate the theory, you would have to go back and (assuming no errors in data) redefine how you applied the categories of the theory to the phenomena."

I think you allude to Peters 'slippery notion' of falsifiability. This is a indeed problem. One could also say that the theory is falsified and that a new one is needed. If you redefine your categories or their application, I could say that it is a new theory already.

"A theory that is not empircally falsifiable must be judged by how much a) the entities it proposes to reason about are operationally definable, b) it includes the factors important to distinguishing situations, and c) it is easy to use in situations we care about."

These are criteria out of vacuum. Verification of theories can be done without them. c) is actually contrafactual; since you want general theories they tend to be simple instead of consisting of several particular ones. (Due to Occam's razor if you wish.) Instead they may be, and usually are, complicated to use in the particular case.

7. **stevep**
July 27, 2005

One problem with falsification as a criterion is that it rules out something like Shannon's communication theory. Shannon's theory is the foundation of network engineering, but an empirical "test" of it is obviously ridiculous–it follows mathematically from assumptions and definitions about "sources," "channels," "coding," etc. The nature of Shannon's contribution was to draw out the surprising conclusions of mundane assumptions and definitions, not to make bold predicitons about contingent facts. Communication theory establishes the basic logical constraints on signal transmission under different noise regimes, etc. (Warning: I am not an electrical engineer or an expert in communication theory–just an interested bystander.)

Falsifiability is a fine thing when you can get it, but lots of useful intellectual frameworks need not be falsifiable and can still say useful things about the world. A big reason why they can be useful is because we are not logically omniscient–we do not instantly know all the deducible conclusions of the things we believe. Some of these conclusions may be impossiblity theorems, but they still are not empirically falsifiable. Communication theory tells us that certain things are impossible, but they are logically, not contingently, impossible; if you ever saw an event that seemed to violate the theory, you would have to go back and (assuming no errors in data) redefine how you applied the categories of the theory to the phenomena.

A theory that is not empircally falsifiable must be judged by how much a) the entities it proposes to reason about are operationally definable, b) it includes the factors important to distinguishing situations, and c) it is easy to use in situations we care about. There are probably other criteria, too, but I can't think of them right now.

I am not qualified to judge whether string theory is on the road to becoming a good non-falsifiable theory like communication theory (or queuing theory, or accrual accounting), but the discussions I have read so far make me wonder. It doesn't sound like the three criteria listed above are going that well. And certainly most of the people pursuing it have the ambition to be empircially falsifiable, so even if it did turn out the way I've described, they wouldn't be too happy.

8. **Torbjorn Larsson**
July 26, 2005

Thank you Clark, it was educational.

"Merely to suggest that its problems can't just be seen as simple falsification. From my limited knowledge I'd say it has more than enough problems to still be questionable though. Let's just not buy into Popper to attack Superstrings."

However, here I don't agree. If falsification is useful at all, it should be used. Some of the anthropic principles that has been used falls for that requirement IIRC. But here I am quickly going offtopic because I don't know enough about the current subject to take it any longer. I think Lubos Motl says similar things on his blog, though…

9. **Clark Goble**
July 26, 2005

Torbjorn, I think Kuhn ends up being the most problematic of the philosophers, if only because of the problems of "paradigm" as having a stable meaning. (He acknowledges this problem in his later writings, but doesn't completely fix the problem) Still, I think the later neoKantians like I think most take Kuhn to be do recognize the categories that we are presented the world through are in part socially determined. That means that falsification is partially socially determined. Kuhn wouldn't go as far as Feyerabend. But I think all of those people have some points – although one can debate how much of an impact it makes in practice. But I think Kuhn's right in that this is because we have some dominate paradigms (or frameworks if we are to adopt the more positivist approach that I think Quine still favors somewhat)

The problem is that Superstring theory is, as I understand it, attempting to be one of those frameworks. As such, discussions of falsification become rather difficult. Especially when for the phenomena in question there isn't a dominant system it is competing against. instead there are other frameworks with at least as many problems as Superstring theory. Given that fact, I'm not sure how falsification even in practice makes much sense.

One might instead just ask for some novel predictions of something unexpected. Yet thus far they can't offer this.

Please note I'm not suggesting falsification isn't useful, although I think the Popper view of them is hard to buy into. Rather falsification, testability, predictions and much else all work together. It just isn't as simple as Popper presents it unless one of comparing a small theory against a dominant overarching one. Even there I think it can run into problem. (Experimental error, some other phenomena at work, etc.) So I think simplicity issues always enter in.

This isn't to defends superstring. (And I'm not enough of a theorist to be able to) Merely to suggest that its problems can't just be seen as simple falsification. From my limited knowledge I'd say it has more than enough problems to still be questionable though. Let's just not buy into Popper to attack Superstrings.

10. **$tringer**
July 26, 2005

From:

http://physicsmathforums.com/showthread.php?p=200#post200

The Inmates ($tring Theorist$) Are Running The Asylum

$tring theory has done far more damage to physics than just $tring theory itself.

$tring theory's central postulate is that there is nothing more to be asked, and that all government funding for all of science should thus go to $tring theory.

$tring theory has fostered a class of fundamentally dishonest, hand-waving, conjecturing, posing, preening, vogueing pretenders. Corruption allows them to make more money from lying than seeking the truth, so they have no incentive to do physics. The fashionista class has bled over into other fields–even the experimentalists raising millions upon millions to test $tring theory's hoax–they're in on the con too.

They're all in on the joke, and should you speak out against them, they laugh at you. They call you a crank when you question their ridiculous theories that as someone pointed out here have no laws, nor postulates, nor any predictions that can be tested. When you ask them to draw a cube in dimensions 8-10, they jeer, sneer, and put you on the blacklist so you'll never be a peer. And everyone lives in fear of not being a peer, because peer review is how they further the untrue.

$tring Theory is about one thing–money. Book deals, government grants, TV shows–it's a big-time tax-funded circus. A theory of nothing–an elite insider's club for those smart enough to learn the rules of accepting and living the lie, but to stupid to ever think on their own. The worst have risen to the top. It's not the first time in all of history, and it never lasts long.

I have dated many beautiful, elegant women, but none of them were subsidized by NSF nor the government nor student loans.

–caltechpostdoc

http://physicsmathforums.com/showthread.php?p=200#post200

11. **Torbjorn Larsson**

July 25, 2005

Gosh, sorry! Of course I mean Clark, not Michael!

12. **Torbjorn Larsson**
July 25, 2005

I am not doing research anymore and foundational questions did not matter much at the time. But I am still curious, so here goes:

'Undermine' seems to be a vague term in english, I found both both 'weaken' and 'destroy'. I would agree with the former but not the later. Which did Michael mean?

Feasible falsifiability seems to be good tool to make us trust theories and debunk much junk or faithbased reasoning. There are a lot of problems as Peter and Michael mentions, but it is still doable.

Kuhn, Lakatos, Feyerabend and Quine critisizes falsifiability. From a very short overview of the later two I get the feeling they are confused philosophers with critiques that does not mean much if one want to use falsifiability.

Kuhn's and Lakatos' criticism are more social and practical, about the problems of not allowing ad hoc hypotheses. Enforcing falsifiability would diminish social influence on theories. And since we can't enforce falsifiability on each and every statement in non-formal theories ad-hocs would survive as long as they are needed.

In short, I agree with Peter and can agree with Michael if he means the weaker version of his statement. Applied to string theory it seems that most or all of the anthropic principles used on several occasions would be immediately out. To the benefit of the theory and lessening of criticism until the whole theory can be verified as more than a new part of QFT, or whatever the conclusion was on Cosmic Variance.

If anyone can explain more on the problems of falsifiability I would certainly appreciate that.

13. **Clark Goble**
July 25, 2005

Alas, I didn't do grad work in superstring theory. (Although I did for a while consider doing work in spinors and clifford algebra which I guess would have led me towards loop theory given my inclinations)

As to whether Quine or Kuhn styled critiques apply. I suppose it would depend. I think both of them would argue that there are competing interpretations that would undermine falsification. That is one community would say things have been falsified while the other hasn't. The problem with string theory is that I don't see that they've even *gotten* that far yet. But I can see why some would suggest this as a problem in the future.

14. **Peter**
July 25, 2005

Clark,

I think the kinds of critique of Popper that I assume you have in mind (Kuhn, Quine?) aren't really relevant in this context. While falsifiability can sometimes be a slippery notion, up until now physicists haven't had any trouble agreeing what it means in this particular case and that it is an important criterion for distinguishing whether a proposed idea about theoretical particle physics is vacuous or not. The only physicists challenging this now are ones whose pet theory is failing the test, and all evidence is that this is just because they don't want to admit failure.

If you can identify how one of the standard problems with falsifiability is relevant to this particular case, that would be interesting.

15. **Clark Goble**
July 25, 2005

Surely critiques of Popper have been around long enough that someone shouldn't be surprised that many reject his overly simplistic views of scientific theory. I don't see why someone would be shocked on this. This has been widely discussed for probably 30 – 40 years.

16. **quantum**
July 23, 2005

check out http://physicsmathforums.com/showthread.php?t=56

17. **Carl Brannen**
July 23, 2005

Re: [Peter Woit]I'm certainly not sure what is the exact relation between QFTs in curved spaces with Euclidean signature and with Minkowski signature, especially when there are spinors, but something interesting is going on. Even in flat space, free field QFT, if you try and directly formulate it in Minkowski space you run into trouble (the propagator is given by an integration contour that goes through poles) and you have to do something. Mathematically the simplest way of saying what you have to do is to formulate the theory in Euclidean space and analytically continue.[/Peter Woit]

The short way of describing the interesting thing that is going on is to note that QFT is equivalent to a quantum statistical mechanics for a space-time where time is imaginary (i.e. carries the same signature as the space coordinates) and cyclic. Quantum statistical mechanics is simply classical statistical mechanics for waves (which are not distinguishable) as opposed to particles.

The problem, of course, is that time is not cyclic. However, if you start with a space-like metric (such as would be used as the path length ds along the track of an observer or particle) and promote proper time from being a parameter to a coordinate, you will have a Euclidean space-[proper]time. If you assume that proper time is not classically observed as a coordinate because it is small and cyclic, you will have exactly what you need to establish a quantum statistical mechanics as the theory underlying quantum field theory.

Now all this has VERY IMPORTANT implications on particle theory. As it is currently written elementary particles and fields are specifically designed to avoid any possiblity that a non Lorentz compatible theory will slip through. This is partly due to the success of this technique, and partly due to the fact that Lorentz symmetry has not been experimentally disproved. If the gravitation people begin moving into Euclidean relativity, rather than Einstein's relativity, the particle people will have to eventually follow.

I find it very gratifying that Stephen Hawking is moving into Euclidean gravitation. I find it even more gratifying that the particle physicists gave me a 3 year head start on figuring out what the Dirac equation looks like in a world where Lorentz symmetry is only an accidental:
http://brannenworks.com/PHENO2005.pdf

Carl

[Apologies for pumping a personal theory on your blog. I believe that you allowed each of us one such post. It won't happen again without your permission. Feel free to edit for length.]

18. **Suresh K Maran**
July 23, 2005

I have posted a reply to Lubos Motl critique of loop quantum gravity in my weblog http://universalwatch.blogspot.com/

19.
July 23, 2005

at the end, Fabien Besnard uses an expression I hadn't heard before which I think is a reference to this fable of La Fontaine
http://www.lafontaine.net/lesFables/afficheFable.php?id=122

to let the prey go and chase the shadow
l?cher la proie pour l'ombre

or, as in this story, not the shadow but the reflection in a pond: the image or appearance being taken for the real goal

I wish he would correct his spelling of Karl Popper's name

Seattle Conference

July 26, 2005
Categories: Uncategorized

This week I'm in Seattle, among other things attending a Summer Institute in Algebraic Geometry sponsored by the AMS. This is the latest in a series of large summer conferences on algebraic geometry that have taken place about every ten years. The last one was in Santa Cruz in the mid 90s, the one before that at Bowdoin in the mid 80s. This one is being billed as "the largest algebraic geometry meeting in the history of the world", with about 320 mathematicians here this week, and a total of around 600 planning on showing up for at least part of the three weeks during which the conference is taking place. The full schedule of talks is on-line, and copies of speaker's notes and transparencies should soon be appearing there.

The main topic of the first week is billed as "interactions with physics", but there's actually not a whole lot of that going on here. The organizers originally hoped that Robbert Dijkgraaf would be lecturing this week, but that didn't work out. Kentaro Hori of Toronto is giving a series of three talks on mirror symmetry, and some of his lecture notes are already on-line. Rahul Pandharipande started off the conference with the first in what looks like it will be a very interesting series of lectures on Gromov-Witten invariants. This has now become a huge subfield of algebraic geometry, with many ramifications, some of which have been inspired by physics, and there continues to be active interaction between math and physics around this subject. Many of the talks in the afternoon parallel sessions are also related to this topic.

An unrelated note: Lee Smolin has a rather philosophical, but interesting, new preprint out entitled The case for background independence

Comments

1.
   July 26, 2005

   You mentioned the new Smolin article ("what's wrong with string theory, how to fix it with background independence, and other thoughts" heh heh) http://arxiv.org/hep-th/0507235

   He is probably right and I hope string theorists read and heed this article.

   The new article also provides a survey of the various nonperturbative/backgroundindependent approaches to QG (Loop, CDT, causal sets…) same ones represented at Loops 05 conference. It has thumbnail accounts of various approaches and lots of references. So for some purposes it is a de facto review article (as well as proposing a cure for string theory).

   Here, for instance, is the description of CDT, beginning on page 21,

   —quote from Smolin's paper—

   5.3 Causal dynamical triangulation models

   These are models for quantum gravity, based on a very simple construction[26]-[32]. A quantum spacetime is represented by a combinatorial structure, which consists of a large number N of d dimensional simplexes (triangles for two dimensions, tetrahedra for three etc.) glued together to form a discrete approximation to a spacetime. Each such discrete spacetime is given an amplitude, which is gotten from a discrete approximation to the action for general relativity. Additional conditions are imposed, which guarantee that the resulting structure is the triangulation of some smooth
   manifold (otherwise there is a severe inverse problem.) For simplicity the edge lengths are taken to be all equal to a fundamental scale, which is considered a short distance cutoff. One defines the quantum theory of gravity by a discrete form of the sum over histories path integral, in which one sums over all such discrete quantum spacetimes, each weighed by its amplitude.
   —end quote—

   ===================

   Does anyone have a subscription to "contemporary physics"?

   http://www.ingentaconnect.com/content/tandf/tcph

   I understand that the May-June issue will have a commissioned article on CDT. or some issue soon to appear.

2. **Jack**
   July 26, 2005

   I very much doubt that Smolin's article will convince anyone. He talks about the Newton/Leibniz [etc] dispute over absolutism/relationalism, decides that relationalism is obviously right, and then argues that what ails string theory is that it isn't relational. He even mentions Mach's principle and worries that general relativity doesn't really make acceleration relative [doh!]. Something tells me that string theorists [and others] are unlikely to slap their heads and say, "Goddamnit! Of course! The way to understand a small cosmological constant is to make everything relational!!" I'm afraid that the response is more likely to be, "So string theory is absolutist and not relationalist? Who gives the proverbial rat's ass?"

3. **Aaron Bergman**
   July 26, 2005

   Four equations in forty-six pages. Positively Banksian.

   I rather think it's quite an act of hubris, myself, to say that the "correct quantum theory of gravity must be" anything. It'll be what it is, and we'll have to live with it. Regardless, I think generally most people think that the end theory will be background independent, so I really don't why various loopy types spend so much time arguing for it.

   And just on a quick glance, it is certainly *not* always true (p. 24) that nonsupersymmetric theories contain a tachyon. The SO(16)xSO(16) heterotic (discussed briefly in Polchinski, I believe) is an example. As for his discussion for the meaning of the 'height' function in the landscape, it's not energy; it's just a term in the action. The stationary points are the vacua and they're stable if they have no massless scalars of tachyons.

4. **Juan R.**
   July 27, 2005

For the string theorist

“Not only, we are told, is string theory a consistent theory of quantum gravity, but it\uffffs a theory of everything, gives us wonderful new insights into gauge theories, and possesses a mathematical beauty that is so compelling that the theory simply must be correct.”

Sean.

It is many times more bold than Smolin claims, but i don’t see your efforts for critizing bold string theory claims.

Your

“I think generally most people think that the end theory will be background independent”

>was no true into string theory comunity only 12 years ago. Then string theorists claimed that one would not take GR “too seriously”. In fact, a theorist (James Graber) still maintain (three days ago in Cosmic variance) the old posture that one would ignore background independence and formulate causality in a flat metric.

He claims that we would ignore experimental data on favor of string theory!

But string theory community has a special facility for forgeting the history of the field.

Fortunately, history shows how wrong string theorists always were.

A waste of time!

5. **ksh95**
July 27, 2005

Aaron Bergman said:
“…As for his discussion for the meaning of the ‘height’ function in the landscape, it’s not energy…”

Which is precisely the point Smolin was trying to make.

The sad thing is that string theorists feel so embattled and animosity between camps runs so deep that enjoyable, non-technical, profound papers by very smart people are only given a “*quick glance”*.

The propaganda and indoctrination (from all camps) is amazing. It seems the more a camp is criticized, the more ardent in their beliefs they become. Often to the point that they’re unable to even consider the merits of another camps arguments.

…If you’re not a fellow string theorist everything you say is *a priori* wrong…If you don’t believe in Causal sets I don’t want to hear anything you have to say…

But then again, I shouldn’t be surprised. This same phenomena is present everywhere else (see modern day american policital discourse).

As I’ve said before, the same rules that govern the schmucks in the street, govern us in the Ivory towers.

6.
July 27, 2005

Wow. I’m sorta surprised that an alg. geometry conference billed as one of the largest gatherings doesn’t have some of the leading men in that area as part of their speakers–ie. Drinfeld, Beilinson, Deligne, etc etc.

7. **Aaron Bergman**
July 27, 2005

*The sad thing is that string theorists feel so embattled and animosity between camps runs so deep that enjoyable, non-technical, profound papers by very smart people are only given a “quick
glance”.*

Yes, that must be it. I feel embattled and angry. The fact is, a quick glance is more than the vast majority of the papers on the ArXiv get form me. There’s just too damn many of them, and only some percentage of them are about things I find interesting. This paper doesn’t particularly interest me because, generally, I have very little patience for philosophy in physics. As I said, I don’t think it is our place to say that an unknown theory “must” be anything. I just glanced at it this time because of this comment thread.

On the other hand, I do happen to believe that a final theory of quantum gravity will be background independent, so I don’t see where you’re getting that I’m going after Smolin on this point.

8. **Chris W.**
July 27, 2005

Has anyone in the string theory community systematically set forth some requirements or expected features of a background independent formulation of string theory? Should it be considered understood that M-theory will be the hoped-for background independent formulation, or is this a matter of disagreement?

9. **Aaron Bergman**
July 28, 2005

M-theory is a dream, not really any particular theory. String theory really has been developed in a form of a bottom-up approach. No one has gone and said, “this is what we need from the theory; let’s put it in.” Rather, people have just explored more and more and various things have fallen out of the theory. The eleventh dimension, for example, came about from investigating the strong coupling limit of the IIA superstring.

The one real nonperturbative theory we have, AdS/CFT, seems to strongly depend on the asymptotics of the spacetime. This is somewhat of an in-between case: the only part of the background that is fixed is what happens at infinity. What happens in the middle is background independent. This has led a lot of people to think that perhaps that’s the best we’re going to be able to get.

One principle that a lot of people think will be important for some final theory of quantum gravity is the holographic principle. One expression of this is that the theory of quantum gravity will be encoded in some nongravitational theory in one less dimension. In AdS/CFT, the boundary is the holoraphic screen, but in more general spacetimes, there will be other possible screens. Bousso has a proposal for this. Because this still fundamentally requires the idea of a real spacetime geometry, the principle is only semiclassical, but many people think that it will be a general feature of whatever form of emergent geometry appears in the end.

Really, it might be that the whole idea of background independence is a non sequitor. Geometry will arise only in some limit of the theory, and the fundamental theory will have some completely different set of degrees of freedom. As I said, I don't think we get to tell the theory what to be. We'll just have to see (hopefully in our lifetimes) how it turns out.

10. **Peter**
July 28, 2005

Hi meh,

It's a rather young conference, with relatively few of the the organizers, speakers and participants in their fifties or sixties. I'm kind of slumming here, since I'm not an expert in algebraic geometry, but I do notice that the people here include quite a few of those my department identified as the best young people in the field in internal discussions about who to try and hire.

11. **Doug**
July 28, 2005

Smolin's characterization of modern theoretical struggles, as really only a continuation of the old debate on the true nature of space and time, is not explicitly recognized very often these days, but I think it's right on. However, if the reason why string theory cannot meet the challenge of the landscape is that there is no background-independent formulation of it, then what does that imply for *any* background-dependent theory?

Juan Maldacena just posted a comment on Cosmicvariance stating his enthusiastic support for the study of the landscape. However, Smolin's argument that because the static probability distributions live on the "space of possible backgrounds," and the dynamics of the theory are to be done on this space, and thus must employ a fixed background in the study of the landscape, makes this effort an illogical endeavor, and he calls for "an alternative methodology for treating [it]"

Nevertheless, his alternative is not a background-independent formulation of string theory, but a background-independent quantum theory. I think this is very significant, because, as he points
out, the reason string theory fails is because it is background-dependent, which is what helps make it untestable and less explanatory, while background-independent theories are "more constrained" and therefore more "subject to law."

Ironically, then, this implies that the strenuous opposition to string theory found on this and other blogs appears to be a little like someone throwing stones while living in a glass house, doesn't it? I mean, if background-dependence is an indictment of accepted physical theory in general, which Smolin seems to think it is, then the sociological pathology, identified as the maddening fascination with string theory that so many physicists are caught up in, with the accompanying refusal to recognize its futility, turns out not to be due to the philosophical error of the misguided, pursuing a non-falsifiable theory, but the epistemological error of all of us, perpetuated now for generations.

This error is the assumption that we "know" what reality is, and that the grand edifice that has taken centuries to build, and that is built upon the assumption that space and time are non-dynamical structure, or even partly dynamical structure, is unassailable, is doing us in, just as surely as the old farmer was done in by his false assumptions.

As I have pointed out before, Einstein warned us of this very peril. Epistemologically speaking, assuming forces can exist autonomously, apart from the underlying motion from which they must proceed by definition, just because we cannot find such motion, is the fundamental error that has lead to the present predicament, because force requires a fixed background. Fields must evolve over time. Taking force out of the picture of gravity separately, by making the background partly dynamical, just avoids the issue in GR.

The fact is, gravity is a force, and as such must be the property of an underlying motion, but gravitational force is one that cannot be defined in terms of a quantum field on a fixed background of three space, for reasons that are clearly related to dimensions of the motion involved. But, unfortunately, since we think we know what space and time are, we can only talk in terms of the one-dimensional motion of objects "through" spacetime. Oh well.

Ironically, recognizing our errors in the understanding of the nature of space and time opens the door to formulating a truly background-independent theory, which is what we need, but how are we going to go that far back now?

12. **Thomas Larsson**
July 28, 2005

It is unlikely that ST possesses a background-independent formulation. Lee Smolin has informed me that a rigorous theorem rules out anomaly-free Fock quantization of background-independent theories, and I see no reason to doubt that. This explains why the conflict between LQG and ST is so fierce. ST gives up background independence, which to LQGists is the most profound lesson from GR,
and LQG gives up Fock QM, which string theorists can never accept. Hence the conflict will persist.

The loophole is that you can combine Fock QM with background independence, provided that you give up anomaly freedom. Most people think this idea is absurd, since classically a gauge symmetry is a redundancy of the description. However, there are examples where quantum anomalies break gauge symmetry, without violating unitarity. The subcritical free string is the best known example. This example is of course also very relevant to QG, since the free string is nothing but 2D gravity coupled to scalar fields.

13.
July 28, 2005

the paper is enlightening in general but doesn't treat CDT adequately.

CDT is more background independent than LQG (independence is not an all-or-nothing feature of theories, there are degrees)

Smolin schematizes degrees of background independence and discusses striving for a greater extent of independence as a strategy. but his paper does not acknowledge that dimension becomes a dynamical variable in CDT rather than being determined in advance as it is in LQG.

14. **Aaron Bergman**
July 28, 2005

It is unlikely that ST possesses a background-independent formulation. Lee Smolin has informed me that a rigorous theorem rules out anomaly-free Fock quantization of background-independent theories, and I see no reason to doubt that.

I'm not sure I want to steal someone else's thunder, but I have heard that one of the premises of that theorem is seriously flawed.

15.
July 28, 2005

Thomas Larsson: It is unlikely that ST possesses a background-independent formulation. Lee Smolin has informed me that a rigorous theorem rules out anomaly-free Fock quantization of background-independent theories, and I see no reason to doubt that.

Aaron: *I'm not sure I want to steal someone else's thunder, but I have heard that one of the premises of that theorem is seriously flawed*.

Aaron, you've gotten me curious. Could you please be more specific? What theorem has the questionable premise? What is the premise and in what way is it flawed?

16. **Aaron Bergman**
July 28, 2005

I've been under the impression that the theorem requires a sort of quantization that doesn't even apply to ordinary gauge theories, but I haven't looked at it — I don't even know the reference — so I'd rather not pontificate too much on it.

17. **Juan R.**
July 29, 2005

"*It is unlikely that ST possesses a background-independent formulation*."

Yes, M-theory is not so simple as a nonperturbative version of ST. It is something different (there the new name) that is not based in the old string theory framework. This is the reason that nobody formulate still M-theory.

M-theory is not a dream, it is the last hope for some guys 🙂

But string community is not being sincere. Since there exist not M-theory (matrix formulation does not work), it looks like a blanck paycheck.

Next the formula of everything

” ” = ” ”

that i discovered today. You can verify by yourself that reduces to M-theory in the joke regime (i.e. when jokes –> infinite).

Would string theorists not explain to public that after of 30 decades of futile efforts finally we know that **string theory will be not the final theory** and theoreticians need of a new theory that nobody (including Witten) know what is?

18. **Juan R.**
July 29, 2005

Uppps!

I mean 3 decades!!

Well exactly are already 4 (or 38 years).

19.
July 30, 2005

Motl wrote 12 pages on his blog about the new Smolin paper and, I thought, missed the point. Here in these comments we havent even restated Smolin's main message.

He announces this quite explicitly in italicized passages at the beginning and then at the end (with reference to the previous) so it's difficult to overlook.

–quote from Smolin introduction–

The reason that we do not have a fundamental formulation of string theory, from which it might be possible to resolve the challenge posed by the landscape, is that it has been so far developed as a background dependent theory. This is despite there being compelling arguments that a fundamental theory must be background independent. Whether string theory turns out to describe nature or not, there are NOW few alternatives but to approach the problems of unification and quantum gravity from a background independent perspective.

This essay is written with the hope that perhaps some who have avoided thinking about background independent theories might consider doing so NOW.

–end quote–

I have added the emphasis on the word NOW which I think is an essential part of the message. It is friendly advice. Should not raise the hackles of sensitive string theorists. It is trying to be constructive and reasoned. And it is urgent. Theorists have been saying for years that the eventual string theory should be background independent—but have postponed such reformulation. He encourages them to go ahead with it—also gives examples of background independent theories suggesting possibility, and points to his own recent work towards a background independent formulation of M theory.

Then right at the end he comes back to this theme

–quote Smolin conclusions–

…The former are more constrained, hence harder to construct. More of what is observed is subject to law, as there is no background to be freely chosen. Hence, it appears that relational, background independent theories are more testable, and more explanatory.

This is the reason for my provocative hypothesis. If it is true then *the reason that string theory finds itself in the situation described in the introduction is that no background dependent theory could successfully solve the five key problems mentioned there*. *If this is true, then the only thing to do is to go back and work on the less studied road of relational theories*.

At the same time, I have tried here to explain the key problems still faced by the relational road…

–end quote–

20.
July 31, 2005

Lubos Motl wrote a 12-page blog reacting to Smolin's "The case for background independence" that you mention here, and it contains misleading statements like this:

—quote Motl—
On the other hand, there are no successes whatsoever of the approaches that Lee wants to call "non-perturbative approaches". The main problem is that they don't care about physics, experiments, and the new principles that are revealed by them; they prefer philosophical dogmas from the 16th century. It is a waste of time to discuss these "non-perturbative" speculations in detail. In all cases (causal set theory, loop quantum gravity, triangulation models), the speculations are based on the naive picture of space as being composed of infinitely sharp points – like in the classical theory – which are moreover exactly discrete. All these approaches make incredibly strong assumptions about the physics at the Planck scale whose probability to be incorrect safely exceeds 99.9999999999%; all of them belong to the discredited category of "gravitational aether theories"…
—end quote—

This is at the end of the section "Background Independence of GR" right before the section "Background independence in string/M theory".

The nonperturbative QG approaches Smolin focused on and devoted sections of the paper to were Loop, Causal Sets, and CDT (currently the leading triangulations method).

Lubos blanket statement about spacetime discreteness in the nonperturbative approaches is in contrast to what the CDT developers say. They explicitly say they have found no evidence of spacetime discreteness or a minimal distance scale. So Motl is attributing to them what they neither postulate nor find as a result.

To illustrate, this from the introductory section, page 2, of a recent CDT paper hep-th/0505113

"The alternative we will advance here is based on new results from an analysis of the properties of quantum universes generated in the nonperturbative and background-independent CDT (causal dynamical triangulations) approach to quantum gravity. As shown in [5, 6], they have a number of appealing macroscopic properties: firstly, their scaling behaviour as function of the spacetime volume is that of genuine isotropic and homogeneous four-dimensional worlds. Secondly, after integrating out all dynamical variables but the scale factor a( ) in the full quantum theory, the correlation function between scale factors at different (proper) times is described by the simplest minisuperspace model used in quantum cosmology. We have recently begun an analysis of the microscopic properties of these quantum spacetimes. As in previous work, their geometry can be probed in a rather direct manner through Monte Carlo simulations and measurements. **At small scales, it exhibits neither fundamental discreteness nor indication of a minimal length scale**."

Also it is misleading to say as Motl does, that "*All these approaches make **incredibly strong assumptions** about the physics at the Planck scale whose probability to be incorrect safely exceeds 99.9999999999%*"

This evokes the canard that nonperturbative approaches are somehow wedded to precisely one version of the Einstein Hilbert action and cannot modify the action by additional terms. In CDT, to take one example, researchers are free to explore modifications of the microscopic dynamics and do indeed perform (monte carlo) computational experiments involving different versions of the action.
I fail to see what is "incredibly strong" about their assumptions.

It may also be that Lubos blanket statement (which he applied to all three approaches discussed by Smolin) is inaccurate as regards Loop gravity, as well as CDT. It may also be wrong for Causal Sets, but I cannot tell about that because I don't know much about that approach. Here I am using CDT to illustrate the misleading, over-the-top nature of his comments. They sound as if he does
doesn't know what he is talking about and is foaming at the mouth.

What is it about Smolin's central thesis (that it is now time for string/M researchers to get a background independent formulation together—that it is urgent given the present situation of string/M) — that disturbed Motl so much that he had to respond like this?

And why doesn't he address the main thesis of Smolin's paper, which is basically some friendly advice, coupled with the urgency?

21.
July 31, 2005

To all interested folks in string theory/LQG.

here is a link to Lee Smoln's video archived colloquiam at CFA.

http://cfa-www.harvard.edu/colloquia/spring05/smolin.html

Conference Roundup

July 28, 2005
Categories: Uncategorized

Lots of conferences are going on right around now, here's some of them, many with on-line versions of the talks.

Lattice 2005 in Dublin, with blogging from Matthew Nobes.

SUSY 2005 at Durham. See some comments by Clifford Johnson. There was also a Pre-SUSY 2005 workshop aimed at graduate students.

Also at Durham, a workshop on Geometry, Conformal Field Theory and String Theory, blogging from Paul Cook.

Introduction to Collider Physics, a summer program aimed at graduate students, taking place at the Institute in Princeton.

This year's SLAC summer institute is on Gravity in the Quantum World and the Cosmos. Sean Carroll is lecturing there and may have more to say about it over at Cosmic Variance.

There's a Summer Institute going on in Taipei, and a summer school in Dubna.

The summer meeting in Oporto has taken place. I'd love to hear from anyone who was there about Graeme Segal's lectures.

The Simons Workshop in Stony Brook has started, leading off with a talk by Cumrun Vafa on The Swamp Surrounding the Landscape. He seems to be suggesting that theorists should be spending their time investigating the "swamp" of possible effective field theories for which it is unknown whether they can be the low energy limit of a string theory. Why he thinks its a good idea to try and lead the field into a "swamp" is very unclear to me, although one could argue it is already there anyway….

Update: A commenter properly takes me to task for ignoring what's going on down under. There's been a Conference in honor of Ross Street's 60th birthday, together with one workshop on categorical methods and another one on noncommutative geometry and index theory, all covered extensively by bloggers over at the String Coffee Table.

Comments

1.
   August 1, 2005

   Hi Lubos:
   So in addition to string prediction making no testable predictions, you now say you haven't much of a clue what it says is impossible? Great work.
   Nigel

2. **Lubo? Motl**
   July 29, 2005

   Cumrun's goal is to define what string theory is – and especially what string theory is not. For example, pure N=1 supergravity in 4 or 6 dimensions does not seem to arise in any stringy vacuum we know of. It would be great to find a kind of proof that various things are impossible according to string theory; we need these things to fully distinguish the predictions of string theory, even outside our Universe, from generic effective field theories.

   I think Cumrun would agree that we may be currently in swamp, and he may become the savior who will take us away from it. 😉

3. **Tony Smith**
   July 28, 2005

   In his paper The Swamp Surrounding the Landscape, Cumrun Vafa says "… The main difficulty in describing nature using string theory comes from the multiplicity of string vacua. …".

   David Gross said, in Chapter 11 of the book The Rise of the Standard Model – Particle Physics in the 1960s and 1970s, edited by Hoddeson, Brown, Riordan, and Dresden (Cambridge 1997):

   "… The bootstrap idea was immmensely popular in the early 1960s … it rested on the solid principles of causality and unitarity … It promised to … provide a unique value for all observables … This is of course false. We now know that there are an infinite number of consistent S-matrices that satisfy all the sacred principles. …".

   Is it fair to modify Gross's quote as follows:

   "… The superstring idea was immmensely popular for the past two decades … it rested on the solid principles of supersymmetry and gravity from loop gravitons … It promised to … provide a unique value for all observables … This is of course false. We now know that there are a huge (possibly infinite) number of consistent vacua that satisfy all the sacred principles. …"

   Is Edward Witten the present-day counterpart of Geoffrey Chew?

   Compare this Chew quote (from Capra's Tao of Physics)

   "… Our current struggle with the hadron bootstrap may thus be only a foretaste of a completely new form of human intellectual endeavor …"

   with this Witten quote (from Greene's The Elegant Universe)

   "… Understanding what M-theory really is – the physics it embodies – would transform our understanding of nature at least as radically as occurred in any of the major scientific upheavals of the past …"

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

4. **Kea**

July 28, 2005

There's also a southern hemisphere.

5.

July 28, 2005

leading off with a talk by Cumrun Vafa on The Swamp Surrounding the Landscape…
—an evocative title, reminiscent of a passage by WB Yeats:

And certain men, being maddened by those rhymes,
Or else by toasting her a score of times,
Rose from the table and declared it right
To test their fancy by their sight;
But they mistook the brightness of the moon
For the prosaic light of day –
Music had driven their wits astray –
And one was drowned in the great bog of Cloone.

http://www.csun.edu/~hceng029/yeats/yeatspoems/TheTower

6. **P P Cook**

July 28, 2005

Sean, I suspect the hiring process encourages bloggers to only report "nice" things about the people who may effect their careers. Of course you will have to worry about the "untouchable" bloggers who already have a secure career, oh and the insane 🙂

Peter, thanks for the link.

Best wishes,
Paul

7. **Sean**

July 28, 2005

Soon it will be impossible to make fun of anyone in a talk, as the chances will be good that whatever you say will show up on a blog within minutes.

Change in Software

August 1, 2005
Categories: Uncategorized

Since I accidentally deleted a post, so had to spend some time fixing things, I decided to go ahead with a long-planned project to try and change around the software here. The old Movable Type software is gone, replaced by Word Press. It seems to be working, but I still need to add in the old links, and fiddle with the configuration a bit. Let me know if anything seems to be broken.

Comments

1. **J.F. Moore**
   August 1, 2005

   Next page link at the bottom is "not found".

   I'm glad the comments are going the 'right' way now! It was hard trying to read them backwards.

2. **Ijon Tichy**
   August 1, 2005

   I find the font too small. It's not very kind to middle-aged eyes.

3. **Peter**
   August 1, 2005

   My eyes are also unfortunately getting rather middle-aged. You should be able to adjust the font size on your browser (I've checked and this works, at least on Mozilla and Internet Explorer). It's not so easily done from this end. This particular "theme" is one of the more legible ones.

4. **Ijon Tichy**
   August 1, 2005

   I think all you have to do is edit this file:

   http://www.math.columbia.edu/~woit/wordpress/wp-content/themes/default/style.css

   specifically, the first section after the comments, i.e.:

   /* Begin Typography & Colors */
   body {
   font-size: 62.5%; /* Resets 1em to 10px */

   Change 62.5 to 70.0 (or thereabouts) and it should look much better. And I'm pretty sure all text elements (including links) will scale accordingly.

5. **Peter**
   August 1, 2005

   OK, I made the change you suggested. I'll leave it like that for now (unless there are any complaints that it's now too big…)

6. **Ijon Tichy**
   August 2, 2005

   Thanks.

7. **Nigel Cook**
   August 2, 2005

   How about starting a thread for a discussion of how to replace string theory with more rational, testable physics? At some stage string theory will have to give way to more productive work.

8. **Ivo**
   August 2, 2005

   One thing I find annoying in this theme (Steam IIRC) is that there is no 'Home' button, apart from the title in the header image. People tend to miss that one. You may want to consider adding that somewhere.

9. **Daniel Doro Ferrante**
   August 2, 2005

   Congratulations, Peter!

   I myself have been dwelling on whether i should move from Movable Type and replace it with WordPress… but, after some "time investment" in building my blog the way it is right now, i'd really like to avoid any kind of "transition". 😉

   Anyway, this "real time preview" is pretty sweet!

   Rock on!

10. **James Graber**
    August 2, 2005

The new look is ok, maybe even better than the old one. I hope your links come up soon; I really miss them because I used them a lot. Let me take the time to say I really appreciate your blog.
Jim Graber

11. **Dick Thompson**
August 2, 2005

    I really like the new look. It's very professional. Rock On!

12. **Clark Goble**
August 2, 2005

    I like the new look – especially the graphic. Nice.

New York Times on Toronto Panel Discussion

August 2, 2005
Categories: Strings 2XXX

I didn't have much luck when I tried here to find out exactly what had happened at the panel discussion in Toronto at Strings 2005 last month. One graduate student (Florian Greimer) commented on Jacques Distler's weblog that he felt quite depressed after listening to it, earning a slap-down from Jacques, who evidently found it so upsetting that he got up and left halfway through it, and later wrote about why such discussions were a waste of time.

Today's New York Times has a report on the panel discussion by Dennis Overbye entitled "Lacking Hard Data, Theorists Try Democracy", which makes it clear why many of the people in attendance were depressed and/or upset. The title of the piece refers to the previously reported fact that the audience voted overwhelmingly against the idea that the anthropic principle was what explains the value of the cosmological constant. What I hadn't heard before is that the panel itself, representing the leadership of the field, voted rather differently, splitting evenly (4 to 4, with abstentions) over the issue. It looks like Susskind's point of view has gone from being a minority one among leading string theorists to one that half of them are willing to publicly sign on to. I can see why the audience was depressed. Overbye reports the reaction to the audience vote as "'Wow', exhaled one of the panel members, amid other exclamations too colorful to print here."

The article also includes some truly bizarre and delusional quotes, which it is hard to believe were not taken out of context. Michael Douglas is reported as saying that "We've done very well for the last 20 years without any experimental input", which is just so weird I don't know what to say about it. Andy Strominger deplored the increasing pessimism about string theory, trying to rally the faithful with the promise of glory in the after-life: "Sooner or later we will get there, and when we do we'll all be heroes."

Susskind gave his vision of the immediate future of the field: "there's nothing to do but just hope the Bush administration will keep paying us", and Amanda Peet has stolen one of my favorite lines, saying that string theory should be trying to get government funding as a "faith-based initiative".

Comments

1. **Robert**
   August 2, 2005

   So Peter, what are we supposed to do, completely forget about quantum gravity? Don't you agree, that physical scenarios where both quantum theory and gravity matter likely involve Planck scale energies and are thus out of reach for a very long time?

   The loopy people claim they make predictions that are mostly about violating relativistic dispersion relations for high energy gamma rays. Do you believe those? I cannot make up my mind if those predictions are just due to a 3+1 split that breaks Lorentz invariance.

2. **LM**
   August 2, 2005

   'Andy Strominger deplored the increasing pessimism about string theory, trying to rally the faithful with the promise of glory in the after-life: "Sooner or later we will get there, and when we do we'll all be heroes.??'

   Ha! Suicide theorists! Sacrifice yourself and there will be 71 Nobel prizes awaiting you in paradise!

3. **woit**
   August 2, 2005

   Hi Robert,

   First of all, I don't think there is any one thing that people should be doing, they should be trying a lot of different things. The big problem of the last 20 years is that almost all of the effort in the field has gone into one very speculative idea about quantum gravity.

   As for other things to try, if you want to just think about how to quantize gravity, you are kind of stuck without having experiment to help you. In that case all you have to go by is mathematical consistency and elegance, and you need to behave a lot more like a mathematician. By this I mean you need to be very clear about exactly what your theory is, what you understand about it, and what you don't understand. You can't get away with just sweeping problems under the rug the way you can when there are experiments around that tell you whether you're on the right track or not. You shouldn't go around claiming you have a "consistent theory of quantum gravity" when you don't, e.g. when all you have is a divergent series that you hope is asymptotic to some unknown, but consistent theory.

   I've always personally felt that the real question is not how to quantize gravity, but how to quantize gravity in some way that tells us how the geometry of space-time is related to the geometry of the standard model. It would be disappointing if these two things have nothing to do with each other, and the danger is that there may be lots of ways of "quantizing gravity", and with no connection to experiment you could never choose amongst them. String theory became so popular partly because it held out hope for being able to put the standard model and gravity into the same structure. But there's no reason to believe it's the only way of doing that, and people should be trying different things in order to come up with some new ideas.

4. **LM**
   August 2, 2005

   "Theoretical calculations suggest it should be 1060 times larger than what astronomers have measured. "

   Wow, they've gotten the discrepency down to three orders of magnitude? That's pretty good.

5. **R.R. Tucci**
   August 2, 2005

   YA GOT TROUBLE

   Well, ya got trouble, my friend.
   Right here, I say trouble right here in Jersey City
   Why, sure, I'm a stringy player
   Certainly mighty proud to say,
   I'm always mighty proud to say it
   I consider the hours I spend pulling out all my hair are golden

Help you cultivate horse sense and a cool head and a keen eye

Now, folks, let me show you what I mean
You've got one, two, infinitely many stringy vaccua
Vaccua that mark the difference between a gentleman and a bum
With a capital 'B' and that rhymes with 'Stree' and that stands for 'String'

And all week long, your Jersey City youth'll be fritterin' away
I say, your young men'll be fritterin'
Fritterin' away their noontime, suppertime, choretime, too

Ya got trouble, folks, right here in Jersey City
with a capital 'T' and that rhymes with 'Stree'
and that stands for 'String'

May I have your attention, please? Attention, please
I can deal with this trouble, friends,
with the wave of my hand, this very hand
Please observe me, if you will I'm Professor Harold Hill
and I'm here to organize a quantum computer band

Oh think, my friends, how can any stringy guess
ever hope to compete with a gold Q comp
Rah, rah, rah-da-da-da-da, rah-rah
Remember, my friends, what a handful of Apple players
did to the famous, fabled walls of I B M
Oh, corporation walls come a-tumblin' down

Oh, a band'll do it, my friends, oh yes
I said a Q C band, do you hear me?
I say Jersey City's gotta have a Q C band
and I mean she needs it today
Well, Professor Harold Hill's on hand
and Jersey City's gonna have her Q C band
Just as sure as the Lord made little green apples
and that band's gonna be in uniform

6. **Tony Smith**
August 2, 2005

Peter, in reply to Robert's question
"… So Peter, what are we supposed to do … ? ",
you said
"… I don't think there is any one thing that people should be doing, they should be trying a lot of different things. … people should be trying different things in order to come up with some new ideas. …".

With respect to "different things", some commonly mentioned such as LQG and Dynamical Triangulation do have some institutional support, although nowhere near as much as conventional superstring theory, so they are actually in the process of being evaluated by the physics community.

Others such as the models of Quantoken, Matti Pitkanen, Jack Sarfatti, and others, and my model, are (afaik) pretty much individual works with little or no institutional support, and may be regarded by many as crackpot.

Even if some of the individual works contain crackpot elements, it may be that other elements of those individual works might contain seminal insights that might, with further development, grow into a useful unification of gravity and the standard model.

Therefore, my proposal would be that a chunk of superstring money and manpower should be diverted from the abyss of the landscape/swamp and used to do detailed evaluation of all aspects of the individual models. My guess is that even if none of the individual models were totally free of flaws, there would be enough germs of truth that the evaluators would come up with at least one, or maybe more, serious alternative candidates for realistic unification of gravity and the standard model.

My proposal would require agreement of the powers-that-be in physics that any grad student or postdoc doing evaluative work should get credit for work done in evaluating, no matter how many or few (if any) positive results emerge.

Politically, I feel that my proposal is unlikely to be implemented, but I feel that the alternative is to watch fundamental physics sink into the quicksand of the landscape/swamp and remain for the forseeable future in a New Dark Age.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **simplex**
August 2, 2005

the report said the panel split 4 to 4 with abstentions, but the panel was advertised as consisting of these 8 people (plus the moderator Shenker):

Raphael Bousso
Shamit Kachru
Ashok Sen
Juan Maldacena
Andrew Strominger
Joseph Polchinski
Eva Silverstein
Nathan Seiberg

maybe they added more people to the panel at the last minute, but if it was just those 8, plus Shenker, it doesnt quite add up to have 4+4 + abstentions.

My guess is that they foresaw that issue coming to the fore and they handpick BALANCED the panel on that issue. So automatically they got a 4+4 vote. the only thing meaningful, or not prior arranged, was the unbalanced way the audience voted.

8. **Peter**
August 2, 2005

I think one can pretty accurately guess who voted how:

Pro-Landscape:
Bousso, Polchinski, Kachru, Silverstein (note, all from West Coast)

Anti-Landscape:
Maldacena, Seiberg, Sen, Strominger (not from West Coast)

noncommittal: Shenker

Maybe there were one or two other people there not listed (Douglas and Witten are quoted in the article), but I'd bet this division is more or less right.

I doubt the panel was chosen specifically to balance landscape/anti-landscape, but since the topic was the future of the field, I'd guess an attempt was made to choose younger people
working on the latest, hottest topics (I think just about everyone on the panel is under 50, or at most, barely over it). For better or worse, the landscape is the one new idea in the field (the KKLT paper is the only recent one with a large number of citations), so that's probably why the landscape side of the argument was well represented.

9. **Aaron Bergman**
August 2, 2005

Douglas and Witten were both in the audience. I don't remember anyone else on the panel off the top of my head. Somehow, I seem to remember Juan being the fourth anthropic vote, but I very well could be wrong.

10. **Thomas Larsson**
August 3, 2005

Robert,

Nobody expects you to care about Peter's opinion any more than Smolin cares about what you and Policastro write.

Nevertheless, if you seriously want an advice, mine would be to get rid of some excess baggage. The dominant ideology dictates that every gauge anomaly is inconsistent. Of course, we know where this prejudice comes from: the chiral anomaly in Yang-Mills theories leads to unitarity violation and it is indeed inconsistent. Alas, to generalize this result to rule out every gauge anomaly is manifestly wrong; it is clearly stated in GSW that the free subcritical string is consistent, despite its conformal gauge anomaly. Incidentally, it is funny to see how Lubos wriggles when I point out this well-known fact. Last time he started to babble about flux compactifications and Eva Silverstein, as if either had anything to do with the no-ghost theorem.

Do with this whatever you like. But to believe that every gauge anomaly must lead to an inconsistent theory is manifestly false, and I don't see how a manifestly false prejudice could be a good guiding principle.

11. **Ingemar**
August 3, 2005

Think the numbers in the NYT article should be 10^500 and 10^60 right?

//Ingemar

12. **Thomas Larsson**
August 3, 2005

I note that Joe Polchinski voiced one of my favorite theses: That the third string revolution has already happened and it was anthropic.

13. **Nigel Cook**
August 3, 2005

"Others such as the models of Quantoken, Matti Pitkanen, Jack Sarfatti, and others, and my model, are (afaik) pretty much individual works with little or no institutional support, and may be regarded by many as crackpot.

"Even if some of the individual works contain crackpot elements, it may be that other elements of those individual works might contain seminal insights that might, with further development, grow into a useful unification of gravity and the standard model.

"Therefore, my proposal would be that a chunk of superstring money and manpower should be diverted from the abyss of the landscape/swamp and used to do detailed evaluation of all aspects of the individual models." Tony Smith

Tony, this is not going to happen even over the dead bodies of string theorists. What will happen when string theory sinks will be a reversion to the situation of the late 19th and early 20th century, with personalities like Maxwell and Kelvin speculating and ignoring criticisms and new evidence. Kelvin never accepted Maxwell's displacement current or his theory of light, nor did he accept Rutherford's interpretation of radioactivity. I don't see how radical new ideas can be treated in any other way than as crackpot nonsense in the commercialised science of today. There is too much money at stake for democratic let alone liberal attitudes.

14. **Quantoken**
August 3, 2005

Peter said:

"As for other things to try, if you want to just think about how to quantize gravity, you are kind of stuck without having experiment to help you. In that case **all you have to go by** is **mathematical consistency and elegance**, and you need to behave a lot more like a mathematician"

Peter, you sounds like beginning to sing the praise songs of super string theoretists. Those are the exact arguments repeated many many times by super string theoretists:

1.QG is very hard because the technology is not available to do experiments, and super string theory is the clear winner, being the most hopeful theory. It just will take much longer time.

2.Super string theory is the only self consistent quantum theory of gravity, and it is so elegenat, so beautiful, so rich in mathematical structures and it is impossible that it could turn out to be wrong and irrelevant to nature.

I see no one ever attempted to dispute that two arguments, despite they being reiterated many times. And I see Peter is clearly nodding his head and hence ready to join the pro-super-string camp. Looks like the only complaint he still has is that "give some more money to none-super-string alternative approaches."

I say No to both accounts. First on the self-consistency and elegance. A theory has to be logically self consistent to even begin to be considered. If it contradicts itself logically, then it's automatically disqualified without having to do any experiment. So self-consistency is really a minimal necessary condition, and is far from being sufficient to say whether a theory is right or not. As for the elegance, it is NOT even a necessary condition. In history many more elegant theories are replace by counter parts that's less elegant, but agree with experiment better. For example Newton Mechanics is certainly more elegant than Einstein's SR and GR. Newton needs just one universal clock and one ruler for measurements. But Einstein needs lots of clocks and rulers in every corner of the room. Only when all things else considered are equal, then we prefer the more elegant one due to our natural human nature of appreciation of beautiful and elegant things.

Now on the experiment end, I say NO, too. There ARE very accessible, and already done experiments to check against theories. For example we have measured the cosmological constant, and know its value. So that's one experiment evidence that's out there, and none of the existing theory can explain why they can not come up with the correct value of CC. They try to get around that piece of hard experimental evidence by the anthropic principle and landscape craps of nonsenses, and then turn around to say there is NO experimental data available to further their theory research. That's ridiculous!!!

Please try to explain the **currently KNOWN, and UNEXPLAINED experiemental facts**, before demanding experimentalists to discover more unknown and unexplained experimental facts! Please explain CC, explain why the supposedly ridiculously high vacuum energy density is not observed and none-exist, and explain other un-explained observational facts. Until you do that, we are in a situation where experiments far lead the theory, instead of theory being far more advanced than experiments, as some theoretists claim.

Quantoken

15. **Scott**
August 3, 2005

It seems the times piece would have been better titled "Lacking Hard Predictions, Theorists Try Democracy.?? It is interesting that string theories inability to reproduce the predictions of quantum mechanics let alone make a precise prediction on what energy supersymetric particles or other stringy effects appear can be blamed on lack of hard data especialy when hard data such as the CC exists as Quantoken pointed out.

The Science writer also seemed to be under the impression that in 1984 "it was shown that a consistent theory of all the forces of nature could be constructed from strings." Which is of course not even really true today let alone in 1984. Also the writer seems to not understand what Smolin means, or at least doesn't point out to his readers with little knowledge of the anthropic principle, when he says "I'm not sure it will be the next revolution, but I am sure it will be the last," and the same goes for the faith based initiative comment, which, along with the defensive comments about not everyone haveing to be a string theorist, lead me to wonder if there was a lot more negativity then implied at the end of the panel when it was opened to comments from the audiance.

16. **Zelah**
August 3, 2005

Reply to Quantoken rant!

I have looked at your attempts at explaining the universe at:

http://www.livejournal.com/users/quantoken/

and frankly, if String Theory is Not Even Wrong well you are Not Even Right!

Let point out some facts. QFT does not explain the CC data or the asymptotic zero energy of the vacuum either! Nor does Loop Gravity or anything else. String Research ashould definately continue until some better comes along

Any SERIOUS suggestions!

17. **simplex**
August 3, 2005

hi Scot,
you mention a lot of evidence of pessimism but you include Smolin's remark (about reformulating string/M to be background indep would be the last) which is extremely optimistic and hopeful, so it doesnt belong with the other cases. this is just a minor correction

you said…*writer seems to not understand what Smolin means, or at least doesn't point out to his readers with little knowledge of the anthropic principle, when he says "I'm not sure IT will be the next revolution, but I am sure it will be the last,?? … a lot more negativity*

IT does not mean Anthropery here, it means B-indep. Smolin's message is that string has great potential but is stuck now, can't be predictive and falsifiable, because they havent grappled with the problem of making it background indep

and that might not be the NEXT because there is no limit to how long they can or will procrastinate, but Smolin's opinion is that it will be the FINAL reinvention of the field because that will be what it takes to bring it to a predictive falsifiable condition, which is all one can ask of a theory—the rest is testing.

he spells this out in "The Case for Background Independence"
http://arxiv.org/hep-th/0507235
where the main point is that background DEpendence is what is wrong with string and moving in the direction of INdependence makes theories have fewer assumptions and be more predictive. he says it is the way out of the Landscape.

this is a radical message, but it might be right. if it is right, then it is hopeful because there IS a clear way to proceed.

so there was nothing gloomy about Smolin epigram comment from audience, about B-indep not being the next but being the last.

he wasnt talking about Anthropistics. he was talking about weaning the field off the prior metric.

18. **Scott**
August 3, 2005

oh my bad, I thought the last comment by smolin was about anthropic and was disconnected from his previous comment on string theory not being background independent. Also I didn't really think it was possible that string theory could be made background independent, that string theory was fundamentally background dependent, and so that helped add to my confusion.

I am going to disagree with you that the idea that a quantum gravity theory must be background independent, which i think to be highly likely, is in some way radical as I think the idea has been around since people were first trying to formulate a theory of quantum gravity.

19. **simplex**
August 3, 2005

that's a friendly disagreement, Scott. I grant you could well be entirely right!

20. **Tony Smith**
August 4, 2005

In his 2 August 2005 New York Times article, Dennis Overby says that "… string theory …" has achieved "… success in formulating a mathematically consistent theory that unifies gravity and the rest of nature …".

If the Standard Model is included in "the rest of nature", then where is the concrete example of a string theory model that does in fact unify gravity and the Standard Model ?

Although my E6 string model at http://www.valdostamuseum.org/hamsmith/E6StringBraneStdModelAR.pdf does unify gravity and the Standard Model, it is not supersymmetric, and so does not qualify in Dennis Overbye's mind as a "string theory" because Dennis Overbye says "… Supersymmetry is predicted by string theory …".
As far as I know, there does not now exist an example of such a supersymmetric "string theory" model that in fact "unifies gravity and the rest of nature".

Further, Dennis Overbye's article says "… What physicists most expect to discover with the Large Hadron Collider is a new phenomenon called supersymmetry (which would manifest itself as a passel of new particles) …". In my opinion, at least a substantial number of physicists don't believe in supersymmetry at all, because it has never been observed in searches that have seen all the particles of the Standard Model other than the Higgs. As to the Higgs, I thought that the primary motivation for the LHC was to study Higgs phenomena.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

21. **Alejandro Rivero**
August 4, 2005

Tony, I'd not put the hand over the fire to swear that a given model is not supersymmetric. Just check Catto and Lichtenberg works; they are following the scent of SUSY between quarks and diquarks, Which has sense by two reasons: one from, cof, strings: that diquarks are allowed to sit in an extreme of the QCD strings, as quarks do. And other from the standard model spectrum: if you allow for "quark antiquark" diquarks (aka mesons), then the mu lepton is very near in mass to a family of diquarks, and the tau lepton is near to other one, so this SUSY extents to leptons.

22. **Tony Smith**
August 4, 2005

Alejandro indicated that I should not swear that my model is not supersymmetric, citing possible supersymmetries more subtle than the 1-1 fundamental fermion – fundamental boson supersymmetry that is conventionally used in superstring theory.
I agree. In my comment, I should have said that my model does not have the simple 1-1 fundamental fermion – fundamental boson supersymmetry that is conventionally used in superstring theory. I expect that the simple 1-1 supersymmetry is what Dennis Overbye meant when he mentioned "supersymmetry" in his NYT article, so i t probably remains true that my model would not fall into the class of (super)string theories described by Dennis Overbye in that NYT article, and my point that I know of no Overbye-type (super)string theory model that does in fact unify gravity and the Standard Model remains unchanged.
I should also have pointed out that my model does have a correspondence between fundamental fermions and fundamental bosons. Although it is not a simple 1-1 correspondence between fundamental fermions and bosons (I have sometimes referred to it as a subtle supersymmetry), it is well-defined and may be useful in ultraviolet finiteness calculations.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

23. **xpinor**
August 4, 2005

Peter,

I'm curious whether you have any comment on the paper:
"Violation of Quantum Gauge Invariance in Georgi-Glashow SU(5)"
by Martin Ambauen, Gunter Scharf
http://arxiv.org/abs/hep-th/0409062

Strings for Dummies

August 6, 2005
Categories: Uncategorized

Joe Lykken just finished giving a series of talks at the SLAC summer school, now entitled "String Theory for Physicists". This was changed from the original title, "String Theory for Dummies" (still on the poster). Presumably somebody realized that the title could be taken the wrong way, giving the impression that string theorists think non-string theorists are stupid.

Lykken's talks are actually unusual for this kind of exercise in expounding string theory to non-string theorists. They begin with a long list of the pros and cons of string theory. I'd disagree with him about some of the "pros" he lists, but it is remarkable that he gives a detailed discussion of the problems with string theory. I've never seen a string theorist do that before. During the last few months I've been sensing a definite change in the atmosphere surrounding string theory. String theorists are on the defensive, and many science journalists and members of the general public are starting to get the idea that there might be something funny going on. For the first time there was open pessimism and defensiveness expressed at the panel discussion at Strings 2005 and the recent New York Times article about it had a somewhat mocking tone.

There's a posting at cosmicvariance.com by JoAnne Hewett about the panel discussion and Times article, and many comments, including some from yours truly. Jacques Distler proves that he thinks anyone who doesn't agree with him about string theory is just ignorant (OK, maybe he just thinks that I'm the only one who is ignorant) with his trademark tactic when he's on the losing side of an argument: take something perfectly accurate that your opponent writes, change the wording to something else that can be interpreted as inaccurate, then use this as evidence to back up a sneering put-down of your opponent. Jacques seemingly can't help himself from doing this. For an all-time classic, check out his contribution to one of the first postings here, where he attacks me for saying that the standard model is a chiral gauge theory.

Unfortunately, Jacques isn't the only string theorist who thinks that this is an intelligent way to behave. Besides another well-known string theory blogger I could mention, at one point I had a remarkable experience with an unknown "prominent string theorist" (I'm pretty sure it wasn't Jacques) who was asked to referee something I'd written about string theory. This referee wrote a report saying that I was just so wrong it wasn't worth explaining why, but that they would give one example. Their example was constructed by taking a sentence out of context, then changing a singular to a plural to allow the sentence to be construed as saying something inaccurate. Some string theorists seem to be willing to go to any lengths to preserve their belief that any criticism of the theory is based on ignorance. My impression is that a lot more criticism is coming their way, and it will be interesting to see how long they try and keep claiming that their critics are just dummies.

Update: Lubos Motl is back from vacation, with a posting about the Toronto panel discussion.

Comments

1. **Nigel Cook**
   August 6, 2005

   'String theorists are on the defensive, and many science journalists and members of the general public are starting to get the idea that there might be something funny going on.'

   Like a conspiracy of string theorists to put over stuff that would look like science fiction if it wasn't so boring? There is little physics coverage because it is becoming so awful. There is a decline in physics A-level uptake in Britain, and a large number of university physics and maths departments have shut here.

   'Children lose interest … because a natural interest in the world around them has been replaced by an unnatural acceptance of the soundness of certain views, the correctness of particular opinions and the validity of specific claims.' – David Lewis, You can teach your child intelligence, Book Club Associates, London, 1982, p. 258.

2. **cvj**
   August 6, 2005

   Peter,

   Respectfully, I must point out that you're misrepresenting several things here, in this post and others. (You are of course entitled to do this because it is your blog). First: The fact that you have not seen a string theorist point out the problems with string theory does not mean that it does not take place quite regularly. Second: As was discussed over at Cosmicvariance on several threads which I know you've read, there are several string theorists all around the world working on many things other than the landscape and anthropic issues that you have made central to your argument that string theory is in a crisis. Third: You fail to point out that there is at least one example of a string theorist blogger (perhaps not as prominent as the others you mention) who has been extremely welcoming to you, your comments, and who has taken the time out to address some of your objections (and clear up some of your misconceptions). I refer to myself.

   Your comments here on your own blog seem to be constructed to perpetuate your preferred mythology that all string theorists are on the defensive, and that they are all arrogant and think that all non-string theorists are dummies. I know that you used the phrase "some string theorists" in some places, but it was somewhat lost in the overall thrust and tone of your post.

   In summary: There is a much wider program of activity in string theory than you seem to acknowledge, and a much wider range of motivations. There is a much wider range of temperaments of string theorists than you seem to allow for. Oh, and the theory is not in a crisis, and there is nothing "funny" going on, assuming that you mean "peculiar".

   Finally, The New York Times' attitude to a body of scientific activity cannot be used as evidence of anything, in view of the fact that even when they are supportive of something, they often get the content, emphasis, and overall point woefully wrong. (There are several articles on string theory which fit that description. It is, however, good that they at least try to cover this activity, I must add.)

   Finally finally, it would be nice to think that the courtesy we've shown you over at Cosmicvariance would result in you making an effort to paint a less narrow and bitter characterization of the valuable discussions that have taken place there. There were several people addressing points you'd made besides Jacques, some of them also active string theoriest with a valid point of view, who quietly explained things to you, and listened to what you had to say.

   Best,

   -cvj

3. **woit**
   August 6, 2005

   Hi Clifford,

First of all, I was in no way referring to you in my complaints about string theory bloggers. You're right that you've always been welcoming to me and willing to engage in very reasonable discussion on all issues. I appreciate that. I also should say that in many of my discussions at cosmic variance as well as in hundreds of private discussions over the years I've found the great majority of string theorists to be reasonable people willing to respectfully discuss issues we disagree about (and we often find we agree about more things than we would have expected). I take your point that I should acknowledge this more often.

That said, until you came on the scene, the two most prominent string theory bloggers have been Jacques and Lubos, and the behavior of both of them is atrocious. I can forgive Lubos a lot, I too was once young and foolish, but Jacques is a middle-aged man and there is absolutely no excuse for the kind of bullying behavior he chooses to engage in. I'm not going to put up with it.

While Jacques is one of relatively few string theorists who behaves this way, he's not the only one who chooses to react to the problems of the subject by engaging in intellectually dishonest and bullying behavior. When I first posted an article criticizing string theory I got a lot of positive responses, a small number of highly hostile responses personally attacking me, and zero reasonable, intelligent reactions from professional string theorists. What most surprised me were how many people complimented me on my courage, saying they agreed with me, but didn't dare say so publicly for fear of retribution. There's a really ugly side to the way string theory has come to dominate particle theory, and the Jacques Distlers of the world are a big part of the story.

As for whether the field is in crisis, that's a matter of perspective, and is something on which I think we do fundamentally disagree. But I assure you that my perspective is not purely a personal one, I've found that many graduate students, postdocs and more senior people agree with me about this. A few years ago I found it wasn't uncommon for string theorists I talked to to agree about this. They often felt the lack of new ideas or forward motion was reaching the point of crisis. More recently I've found an even larger number of people agreeing with me, with very many sharing my point of view that a sizable and increasing fraction of the string theory program has been taken over by research which is pseudo-science, not science. Looks to me like a crisis. By definition a crisis doesn't last that long, so we'll see what things look like in few years, and then may be able to evaluate which one of us was right.

4. **Urs Schreiber**
August 6, 2005

While I don't know if it does any good to try to discuss it, I am wondering where you think your arguments were distorted.

I had the feeling your comments received pretty good replies. Seems like you tried to argue that all valuable progress attributed to string theory is actually progress in field theory. It was pointed out that this is an odd point of view, and a pretty good analogy was given to illustrate this.

5. **Peter**
August 6, 2005

Hi Urs,

I don't have any problem with any of the replies I received to my comments except those from Jacques. I didn't actually say that "all valuable progress attributed to string theory is actually progress in field theory", but certainly anyone who felt that was what I was saying and wanted to challenge it was more than welcome. In response to some of the replies, to Aaron and to Clifford I explained in more detail what I meant to say. In particular I made the completely accurate and unobjectionable statement that topological string theory at a fixed genus is a QFT, and that the string theory sum over genera is not a QFT.

Jacques then began attacking me, first putting the words "topological string theory is just 2D QFT" in my mouth and saying I was "flat-out wrong". When I copied for him my earlier comment explicitly saying that it was only at fixed genus that it was a QFT, he then made an analogy with the QFT perturbation expansion and in that context again put the "topological string theory is just QFT" argument in my mouth, using it to proudly announce that I was "not a serious interlocutor".

At this point, I was pretty pissed off by Jacques's endless tactic of trying to find some way of twisting words that I write so he can find an interpretation of them in which they say something incorrect, and use this to attack my professional competence. I've had to put up with a large amount of this offensive behavior from him (and others), and am really sick of it. I pointed out to him that his own argument was worded incorrectly (he wrote "precisely equivalent" when he meant "precisely analogous"), but that I wouldn't accuse him of being ignorant about QFT based on this.

His rsponse was quite offensive (hint, in case this ever happens to you: if you have a Ph.D. in particle theory and someone carefully explains to you what a Feynman diagram is, then asks for a response that they announce will be "diagnostic", you're being insulted), so much so that he himself tried to retract it as "needlessly inflammatory".

Undoubtedly that's more detail than anyone wants, but that's what my posting and comments to Clifford are referring to.

6. **Aaron Bergman**
August 7, 2005

I can't help but find this:

> In particular I made the completely accurate and unobjectionable statement that topological string theory at a fixed genus is a QFT, and that the string theory sum over genera is not a QFT.

disingenuous. The discussion at hand was whether there was anything beautiful in string theory learned in the last 20 years (or something along those lines.) I responded with AdS/CFT and the topological string. Your response was that AdS/CFT dealt with field theory dualities and that the topological string at a fixed genus was just a field theory. The former is, as Clifford pointed out, wrong. The latter may be technically correct, but if construed that way, completely nonresponsive to the point. Given the context, it's fair to infer that you were attacking the topological string as being just field theory in disguise. Otherwise, why mention it?

7. **Jacques Distler**
August 7, 2005

Remarkably thin-skinned, coming from someone who, in a **single** recent post referred to me as

a) "completely delusional"
b) engaged in "educational malpractice."

I, fortunately, am rather thick-skinned. So, please, don't change your style just for me. Continue to inveigh away at what an evil, mean-spirited (and completely delusional) person I am.

I gather you have a loyal following, who just lap that stuff up.

8. **Quantoken**
August 7, 2005

I am not interested to make a judgement whether anything Peter said is right or wrong. Because it doesn't matter and doesn't change the fact that SST has so far unable to explain a single damn thing in nature. And that so far as it is unable to do that, it is a complete failure, and people like Jacques etc are a complete waste in their invain efforts to pursuit a goal that is simply wrong.

Lubos is OK. He some times behave a bit silly and a bit arrogant. But he is young and he does show evidence of intelligence some times, like on the matter of global warming. As for Jacques, I have lost every little bit of respect for him after this. Clearly he has zero IQ on matters unrelated to SST, and could not think for himself.

9. **Peter**
August 7, 2005

Aaron,
This is beating a dead horse, but let me try one more time:

First of all, you've changed what initially led to this exchange, which was Jacques's claim that

"string theory has turned out to be a vastly more beautiful and intricate subject than anyone suspected 20 years ago."

not

"whether there was anything beautiful in string theory learned in the last 20 years."

These are two very different statements. By changing what this discussion was about you're constructing a straw man argument I never made and don't agree with and putting it in my mouth. It's an annoying debating tactic which Jacques also loves to use, but at least you don't then go on to personally insult me.

I have no problem with the second statement, and recent work on the topological string is a good example of something beautiful coming out of string theory.

The first statement however is simply delusional (the beautiful part, not the intricate part, the subject sure is complicated). 20 years ago there was a conjectural idea about how to use string theory to produce a TOE, and many people made many public claims that this was an extremely beautiful idea. 20 years of work have shown just the opposite. All attempts to get a TOE this way lead to hideously complicated constructions which don't even work. As a TOE, which is the main way string theory has been and continues to be sold, the situation is the precise opposite of what Jacques said, with the theory vastly uglier than anyone suspected 20 years ago.

Some beautiful things have come out of string theory, but many of them are purely 2d QFT. Examples of 2d QFT results that I have in mind are the mid-late 80s work on CFTs, and the early 90s work on mirror symmetry (which involved topological sigma models with a fixed genus of the world sheet, often genus zero). In recent years there have been two related classes of results that truly are string theoretical and that truly are beautiful. They both come out of the idea of a having a precise duality between a gauge theory and a string theory. To be more specific they are:

1. AdS/CFT. Of course I'm well aware that this is a duality between a string theory and a QFT, not a duality between two QFTs. It was my mistake to try and make the debating point that often this duality is checked by doing a QFT (supergravity) calculation on the string theory side. My bad, that muddied the waters and is pretty much beside the point.

2. Topological string theory: As I've repeatedly said, there are certainly very interesting mathematical results about the full topological string theory expansion, and this full expansion is not a QFT. Among the most impressive things I've seen of this kind are the Gopakumar-Vafa results, and, correct me if I'm wrong, but part of the story is that these also come out of the idea of looking for a string/gauge theory duality, in this case using Chern-Simons as the gauge theory.

So, when I look at the beautiful mathematics coming out of the last 20 years of string theory research, two sorts of things seem to me the most striking: some older results that are purely 2d QFT, and some more recent results that are based on the idea of looking at precise string theory duals of a gauge theory QFT. From this perspective, what is interesting about string theory is not the idea that was initially used to sell it, that it could give the standard model QFT + supergravity as a low energy limit, but the fact that it can provide an alternate formulation of QFT.

If you want to challenge any of the above, go right ahead, I'm happy to discuss it further. If instead you want to ignore this elaboration of my earliest comments and make complaints about them based upon a misinterpretation of what I was saying, I don't really see the point of continuing.

10. **Peter**
August 7, 2005

Jacques,

Because of your continual insults of me as professionally incompetent, at some point I stopped worrying about whether my references to you were sufficiently polite or not. Perhaps this was a mistake, I hear they have a saying down in Texas about what happens if you start wrestling with pigs.

If you want to continue your tactic of trying to deal with my criticisms of string theory by making up things I haven't said and using them to insult me and to try and convince people that I don't know what I'm talking about, go right ahead. But it's slimy, pathetic, bullying behavior and you should know better. It may help you maintain your delusions about string theory, but the obnoxious way you and Lubos behave in the face of criticism makes clear to most people how little of a legitimate case you have on your side of the argument.

11. **Jacques Distler**
August 7, 2005

Well, then, you probably don't care that we were referring to the Gopakumar-Vafa papers on the duality with M-theory (I,II) — a highly nonperturbative rewriting of the Topological String free energy (summed over genera) — rather than to large-N Open/Close Topological String duality.

And, no, in the latter case, the Open String side only reduces to Chern-Simons theory in the special case of T^*S^3.

Anyway, I had forgotten why I avoid responding to your attacks. Thanks for reminding me.

12. **Peter**
August 7, 2005

You know Jacques, I think this is a mania, and you should consider seeking professional help.

Let's look at this and see the trademark Distler behavior in action. Recall how it works: first ignore the points at issue, and pick out one sentence in which some sort of reference is made to a particular technical idea that he knows something about. Then go on to interpret that reference in a way that can't be supported by the actual text, but that allows him to attack me as not knowing something that he knows.

In this particular case, the the technical thing at issue is results of Gopakumar-Vafa, about which I said, in toto:

"part of the story is that these also come out of the idea of looking for a string/gauge theory duality, in this case using Chern-Simons as the gauge theory"

I wrote "part of the story" specifically because I was only referring to, well, part of the story, the part that Jacques properly recognized as large N Open/Closed Topological string duality, and in particular the special case of the cotangent bundle of the 3-sphere. Note that I was very careful to not be claiming that this was a complete description of all of the Gopakumar-Vafa results.

It's pretty tedious to have to spend time writing comments like this very carefully, since I know Jacques is going to be spending his time searching them for something he can use to attack me as an incompetent. The thing that most amazes me about Jacques is that he just can't stop himself from doing this kind of thing. You'd think that once anyone had publicly made a fool of himself so many times this way they'd learn their lesson.

13. **Jacques Distler**
August 7, 2005

I can't resist point out that, in the same recent post in which you called me "completely delusional" and "[engaged in] educational malpractice," you *also* complained that, "This is the first time I can think of that he has actually used my name, even if as an insult."

So much for my "continual insults" of you and/or your professional competence.

Now, I *really* feel the need to shower off and stop responding to your … *umh* …. well-reasoned critiques.

14. **woit**
August 7, 2005

Sorry Jacques, you've won and caught me in an imprecision. For "used my name", I meant "used my name in his blog".

Your mania for avoiding the point and desperately trying to find something inaccurate in what I write continues. Get help.

15. **Urs Schreiber**
August 7, 2005

Even though this has become a flame war, I would like to make a comment that I hope is recognized as a technical comment, not intended to attack anyone personally.

It was Peter who wrote above:

> As a TOE, which is the main way string theory has been and continues to be sold, the situation is the precise opposite of what Jacques said, with the theory vastly uglier than anyone suspected 20 years ago.

'As a TOE' = 'as concerns it's quasi-realistic solutions found so far', yes indeed. Nobody can deny that and as far as I can see nobody around here is denying that.

The discussion over at cosmicvariance was however about the theory, not about any of its solutions (or about any way (anthropic or what not) to pick its solutions). This is a big difference. Newtonian mechanics is beautiful. Specifying the solution to some billard problem, say, in Newtonian Mechanics is generically not so.

It seems to me that the argument here became heated because the distinction has been blurred between statements *like*

> There are many beautiful aspects to be discovered once certain CFTs are interpreted as describing strings propagating in certain target spaces.

on the one hand side and

> Such interpretation has however so far not led to a nice derivation of the standard model.

on the other side.

I believe there is no disagreement on the truth of either of these two statements between Peter and anyone else. The whole point is that Peter feels that the truth of the second statement totally undermines the usefulness of the first statement, while many others feels that the truth of the first stament is ample indication that we can eventually remove the 'so far' from the second statement.

16. **Chris Oakley**
August 7, 2005

I believe there is no disagreement on the truth of either of these two statements between Peter and anyone else. The whole point is that Peter feels that the truth of the second statement totally undermines the usefulness of the first statement, while many others feels that the truth of the first stament is ample indication that we can eventually remove the 'so far' from the second statement.

Not proven, I'm afraid. Just because something is "beautiful" (a very subjective thing anyway) it does not that mean that it is likely to be right. This obsession with mathematical beauty has led precisely nowhere, and it is high time that you people stopped pretending that what you do has more than a passing resemblance to physics.

You can fool all of the people some of the time, etc.

17. **Quantoken**
August 7, 2005

Jacques:

You clearly do not know anything you talk about. Are you **really sure** you have READ the Vafa paper carefully? I think you have NOT! To show how ignorant you are, please tell me, without go a read that paper again, exactly how many equations have occured and exactly how many pages that paper contains. You can't answer that without cheating, can you?

The point I want to make is the **technicality details are totally un-interesting and unimportant**. The important thing is even if you memorize everything Vafa ever published and down to the detail of page numbers and word counts, you are still a complete idiot and not knowing what you are talking about. The whole SST business is completely meaningless except it could be otherwise interesting to a few paid nerds like your kind. Clearly SST is unable to make any prediction so it belong to a category worse even than astrology, which at least makes predictions, right or wrong.

Have you figured out which end of yours emit the kind noxious smell that contributed greatly to global warming, Jacques? I never thought that's an important detail to know but you clearly had a different opinion.

18. **Aaron Bergman**
August 7, 2005

> "string theory has turned out to be a vastly more beautiful and intricate subject than anyone suspected 20 years ago.??
>
> not
>
> "whether there was anything beautiful in string theory learned in the last 20 years.??

I'm sorry, but there is almost no difference in the plain wording of these statements besides the word 'vastly'. That you choose to interpret the former one as somehow implying that the use of string theory towards finding a realistic vacuum (and things along those lines) has become more beautiful is bizarre. It's just not there in the sentence. At the risk of putting words in Jacques's mouth, the sentence means precisely what it says, that string theory, as a subject, has turned out to be more beautiful and intricate, again, as a subject, that anyone suspected 20 years ago. The proliferation of candidate vacua in the past few years does little to negate this statement.

> From this perspective, what is interesting about string theory is not the idea that was initially used to sell it, that it could give the standard model QFT + supergravity as a low energy limit, but the fact that it can provide an alternate formulation of QFT.

Nobody has said, Peter, that the vast areas of beauty are all part of the idea that was initially used to sell it (although many of these new understandings hav gone into the constructions of vacua.) You're so fixated on this point that you cannot respond to the actual words that people are writing.

And, you're still wrong on the topological string. You can carefully word your statements as much as you like; you're still using them to support your conclusion:

> So, when I look at the beautiful mathematics coming out of the last 20 years of string theory research, two sorts of things seem to me the most striking: some older results that are purely 2d QFT, and some more recent results that are based on the idea of looking at precise string theory duals of a gauge theory QFT.

Now, when someone points out that your statements don't actually support this conclusion, by pointing out that they involve things beyond field theory and field/string dualities, you appeal to your careful wording. In other words, you *claim to be aware* of the fact that your statements, precisely parsed, do not support your conclusion. Given your claims to greater knowledge, your careful wordings come across as simply deceptive.

19. **JKG**
August 7, 2005

Hi,

On his first slide Lykken says „string theory is a consistent theory of quantum gravity??. Lee Smolin uses to stress that there is no proof that string theory is renormalizable to all orders, so this statement seems to be not exactly correct, but I have even more naïve question. Namely could anybody explain, what are the arguments that string theory is (contains) a theory of gravity, and not merely just a theory of spin 2 field moving on some backgrounds. Could one derive equivalence principle from strings? Is it possible to derive Newtonian potential (with some corrections perhaps) for point mass(es)?

Thanks

JKG

20. **Aaron Bergman**
August 7, 2005

One can find the consistent backgrounds on which one can do string perturbation theory. It turns out that these backgrounds are exactly those that satisfy the Einstein field equations (plus higher order corrections). String perturbation theory also gives rise to a spin 2 field, ie, perturbative gravity, and this contains the Newtonian potential.

21. **Chris Oakley**
August 7, 2005

Aaron,

This was the question:

Could one derive equivalence principle from strings? Is it possible to derive Newtonian potential (with some corrections perhaps) for point mass(es)?

Your answer seems to be "we hope so" rather than "yes". For some of us, this is not good enough.

22. **Aaron Bergman**
August 7, 2005

In what way does my answer seem to be "we hope so"? The equivalence principle (for various definitions thereof) fails for strings, but this isn't a big deal. As I said, you can compute perturbative gravity and show that any background has to satisfy the EFEs. What more do you want?

23. **woit**
August 7, 2005

Hi Urs,

I'm glad to have someone commenting here who seems interested in having a reasonable discussion. A couple comments of my own:

First of all, it's not so clear in this case what is "the theory" and what is "the solutions to the theory", so the ugliness of the latter is not irrelevant to the question of the beauty of the former. To discuss the issue of the beauty of "the theory" that is supposed to be a TOE we have to first agree about what "the theory" is. If you want to very precisely tell me what "the theory" is, we can try and have a discussion about its aesthetic properties, positive and negative.

Secondly, about the two statements you give, I'm not completely happy with your formulation of either one. In the first statement, you need to specify precisely what CFTs and what target spaces you're talking about before I'll sign on. I'll happily agree with this statement if we're talking about the topological string and target spaces where string theory has led to calculations of the full sum over genera. But I suspect you have in mind other CFTs and other target spaces. Your second statement is misleading: not only hasn't work on string theory led to a "nice" derivation of the standard model, it hasn't led to any derivation of the standard model, nice or not nice.

24. **JKG**
August 7, 2005

Hi Aaron,

You say

"String perturbation theory also gives rise to a spin 2 field, ie, perturbative gravity, and this contains the Newtonian potential"

I understand that you compute entirely within string theory the scattering of two particles mediated by graviton, go to the static limit and get Newtonian potential. This sounds reasonable. Is it then obvious that from the first principles it follows that the relevant charge is to be mass (inertial one because you do not have anything else to start with), or you just put it by hand, somehow? In the first case you would have more-or-less the equivalence principle.

Thanks

JKG

25. **Aaron Bergman**
August 7, 2005

The violation of the equivalence principle comes from the fact that string theory really gives dilaton-gravity.

26. **woit**
August 7, 2005

Hi Aaron,

First of all, look at what you are doing in defending Jacques's statement. You argue:

"the sentence means precisely what it says, that string theory, as a subject, has turned out to be more beautiful and intricate, again, as a subject, that anyone suspected 20 years ago."

I don't have any problem with your changing "string theory" to "string theory, as a subject" (it actually doesn't clarify the tricky point of what "string theory" refers to), but the "vastly" you decided to drop is a big part of the problem.

In standard use of the English language the two questions of

whether string theory is "vastly more beautiful … than anyone suspected 20 years ago"

and

"whether there was anything beautiful in string theory learned in the last 20 years.??

are quite different and have very different truth values. To make the second one true you just need to demonstrate one beautiful thing about string theory learned in the last 20 years, and there are plenty of candidates.

To make the first one true, you have to first understand what people thought about the beauty of string theory 20 years ago. Note that Jacques didn't just say that string theory is more beautiful than the average string theorist thought, he says that it is "more beautiful than anyone suspected" back then, so you have to identify the maximal amount of beauty that any string theorist suspected the theory might have back in 1985.

I have a bit of an advantage over you here in that I was there, spending lots of time talking to people and going to talks about string theory. Unlike many people who have problems with string theory, my problem is not that I object to being guided by mathematical beauty, especially when you don't have experimental results to help you. Many people at the time were going on about the beauty of string theory and I was having trouble seeing this. The quantization of the string never seemed to me a beautiful business (and still doesn't). Part of what people generally seemed to mean about the beauty of the theory was the way anomaly cancellation conditions picked out 10d and a gauge group like SO(32) or E8xE8, together with the way conformal invariance picked out approximately Calabi-Yau spaces among all possible 6d spaces. Some people were quite taken with the beauty of the geometry of these Calabi-Yau complex threefolds. Algebraic geometry is a beautiful subject.

So, 20 years ago, at least some practicioners would have claimed that string theory was a very beautiful subject, with one of their arguments being the beauty of the Calabi-Yau condition in picking out an attractive class of algebraic varieties, one of which would soon explain all of particle physics to us. Fast forwarding 20 years, much of this "beauty" has collapsed, as it has become clear that to get anything that looks like physics, you can't just use a Calabi-Yau, but have to add in all sorts of ugly, complicated and poorly understood structure.

Sure, some beautiful things have been discovered about parts of string theory having nothing to do with its use as a TOE, but I still think you have to be delusional to think that these are "vastly" more beautiful than the maximalist claims of beauty for the theory that were being made back in 1985.

Perhaps like Urs you're also of the opinion that one can consistently claim that the ugliness problems of string theory come just from its solutions, not the theory itself. If so, see my response to him about this.

As for the rest of your comment, you seem to be objecting to my conclusion describing what, to me, are the most beautiful things coming out of work on string theory: things coming from 2d QFT and things that come from the discovery of various string/gauge theory dualities. That was a personal statement describing my aesthetic reaction to those things I have learned about by following research in string theory. No I don't claim to understand everything going on in string theory, and if you know of something you find significantly more beautiful than the things I mentioned and want to tell me about them, I'd be happy to learn something.

27. **Aaron Bergman**
August 7, 2005

Again, you miss the point. Our understanding of string theory has expanded. Expanded vastly, even. And, there has been a whole lot of beauty in those new areas that have been discovered. These are areas and directions that no one suspected back when string theory first became popular. As best I can tell, you don't disagree with any of these facts. Nonetheless, you seem so obsessed with the vacuum situation that you automatically interpret the statement that the field is vastly more beautiful as referring to that. Did the people in the early 80s suspect AdS/CFT? Did they suspect mirror symmetry? Did they suspect dualities? I could go on. Are these ugly ideas? You're attacking as 'delusional' statements that nobody has made.

> As for the rest of your comment, you seem to be objecting to my conclusion describing what, to me, are the most beautiful things coming out of work on string theory

I was objecting to your implication that various areas of beauty I mentioned were, in fact, just field theory and not string theory. You're welcome to find beauty whereever you care to look. What I dislike is when you misrepresent the facts of a situation (even when carefully worded to be technically correct) in order to score cheap points.

28. **Peter**
August 7, 2005

Aaron,

You're just repeating yourself and showing no signs of even bothering to read anything I write. You're continuing to go on about how I'm claiming that certain subjects are just field theory, long after I've repeatedly wasted a lot of my time acknowledging that some of these subjects are definitely string theory, not QFT and trying to be precise about which is which. I'll take your lack of response to my last question as at least indicating that we're in agreement that the most beautiful things to come out of string theory are various ideas about 2d QFT, and various examples of string/gauge theory duality.

You seem to agree that what I'm saying is technically correct, but you find it a misrepresentation of the "facts" of the situation. What's going on is that I have a very different interpretation of the significance of and lessons learned from a lot of the undeniable advances achieved in work on string theory over the last 20 years. If you want to argue against it, you're welcome to do so and we might both learn something. But you first have to at least read what I write and pay some attention to it.

29. **Aaron Bergman**
August 7, 2005

> One would naively expect that the effective potential strengthens at short distances due to the extra dimensions appearing; Do you know a quantitative estimate for the deviation from Newton and would experimental evidence that the potential actually weakens (relative to Newton) be a hint that string theory is wrong?

I don't know of any way to explain it with what we currently know about string theory (or field theory, for that matter.) Is it a hint that string theory is wrong? I don't feel that we understand string theory well enough to say. That's one of the reasons I'm not particularly enthusiastic about the attempts to do string phenomenology these days.

It would, however, pretty much rule out all the large extra dimension scenarios that I know of.

Peter,

George Bush never actually said outright that Saddam Hussein and Al Qaeda were linked, but he damn well implied it. That's the feeling I get from your argument here. Your careful wordings seem tactical and designed to give a wrong impression while remaining technically correct.

I've already mentioned what I find most beautiful these days: AdS/CFT and the topological string. In fact, it's exactly a subject on the intersection of those two that is frustrating me right now.

30. **Aaron Bergman**
August 7, 2005

No. String theory is the only known consistent *quantization* of a gravitational theory in more than three dimensions. There are plenty of different theories of gravitation. We know that the vanilla perturbative string gives supergravity in ten dimensions. Compactifications of this give gravity in lower dimensions.

The theory is not well-developed enough to be falsifiable (to my knowledge) by anything less than a probe of the Planck scale.

31. **Gordon**
August 7, 2005

Aaron,

You may always regulate gravity in the same way string theory is,
when you take the low-energy limit. The GR theory is sensible, and
even models string theory. What do you expect string theory to
be at scales below the Planck scale? You are not going to see all
of the states, but rather some theory that we call gravity (which
is regulated the particular way).

32. **Peter**
August 7, 2005

Hi Aaron,

If you're concerned about people who use wording designed to give the wrong impression, check out hep-th/0508034 by Michael Douglas, which just came out and which I just started looking at.

"we begin with compactification of the heterotic string on a three-complex dimensional Calabi-Yau manifold. This was the first construction which led convincingly to the Standard Model"

And, by the way, referring to string theory as giving a "consistent" quantization of gravity, somewhat strains the conventional meaning of the word "consistent".

If I'm behaving like George Bush, some others have to be compared to the Iraqi information minister.

33. **Michael Sanford**
August 7, 2005

Dr. Woit and Dr. Distler,

For the most part, I enjoy reading the differing opinions found on this blog. I have learned some interesting things as well. However, this flame war between the two of you is unprofessional. It does not put either of you in a good light. It is obvious that neither of you are going to agree on the subject of String Theory or about each other personally. So, can you two take the high road and agree to disagree?

I look forward to more spirited (but polite) discussion here.

34. **Gavin**

August 8, 2005

Peter,

I've been following your comments on several threads, and just want to make sure I understand. I would welcome any corrections.

QFT makes a ton of predictions about all sorts of low energy experiments once you pick the particle content, masses and couplings. There are many choices you can make, but there are some restrictions, the most obvious being that the theory has to be renormalizable and anomaly free. We actually have a set of particles and couplings, the standard model, which matches very well with experiment, and the theory is renormalizable and anomaly free. All very nice.

The only bad news is gravity, which is not renormalizable. Based on our current understanding of QFT, gravity should be left out. Opps.

String theory looks like QFT at low energies, so if we could find a "standard vacuum" that gives the right particle content and interactions, then string theory would be as good as QFT, although needlessly complicated. There is one advantage, however, which is that string theory predicts gravity where QFT recoils.

If we lived in ten dimensions, then we could do a lot better. With only a handful of string theories to chose from, it would be pretty exciting if the low energy spectrum matched the massless modes of one of the string theories. If the spectrum was different, then string theory would be out.

Living in four dimensions means there are tons of vacuums, so practically anything is possible. This makes it hard to see how string theory is any better than QFT.

Am I on track with this?

Gavin

35. **Aaron Bergman**
August 8, 2005

Wolfgang, do something about your wrapping, please. There's a preview right below the entry box.

> By the way, does anybody understand why and how M-theory compactifies to 4D ? And also why this compactification "stops?? at small distances? (Roger Penrose raises this question in his book.)

The better question to ask is, why do four dimensions become large, not why do however many become small. Everything was small at one point if you believe in the big bang, after all. Is there a definite answer to this question? No. There are plenty of attempts at answering it (Brandenberger-Vafa being the most famous, probably), but nothing is close to definite. It's an open problem. There are lots of them. I don't understand the second question.

If I understand Gordon's comments as being against the idea that you can get too many funky modifications to gravity in the IR, my response is that string theory has surprised us in the past and will probably do so again in the future. Given that there already are weird linkages between the UV and the IR in the theory, I'm not comfortable making definite pronouncements about anything.

For Peter, I'm not here to defend Douglas (whose philosophy on landscapy things I generally disagree with). The statement does seem weasely to me. I'll stand by 'consistent', however, unless you can point out anything inconsistent in string theory. Goedel's theorem, of course, tells us that anything consistent must be incomplete and string theory certainly is that.

(joke)

36. **Nigel Cook**
August 8, 2005

Gavin: string theory doesn't 'predict' gravity. General relativity is testable, string theory isn't. Eddington in his 1920 book Space Time and Gravitation counted 200 other speculative theories for gravity. None made testable quantitative predictions, so none were really science. If they don't make testable predictions, they are crackpot. Witten in Physics Today, April 1996, claimed that string theory 'predicts gravity' which is extremely misleading, as Penrose points out in The Road to Reality. This is the real crime of string theory, these distortions of the facts. They make it heavy going for everyone trying things outside string theory.

37. **Arun**
August 8, 2005

Something that Peter Woit wrote, about not trusting mathematical beauty to be a guide to the world, resonated with something I just read by biologist Sean B. Carroll. The context is how structure arises, and in particular varoius regular patterns arise, in a biological organism that starts off from a single cell.

> For several decades, mathematicians and computer scientists were drawn to the periodic patterns of body segmentation, zebra stripes and seashell markings. Heavily influenced by a 1952 paper by the genius Alan Turing (a founder of computer science who helped crack the German Enigma code in World War II), "The Chemical Basis of Morphogenesis", many theoreticians sought to explain how periodic patterns could be organized across entire large structures. While the math and models are beautiful, none of this theory has been borne out by the discoveries of the last twenty years. The mathematicians never envisioned that modular genetic switches held the key to pattern formation, or that the periodic patterns we see are actually the composite of numerous individual elements."

(From "Endless Forms Most Beautiful : The new science of Evo Devo".)

Not precisely on the topic of string theory, but I hope relevant nonetheless.

38. **woit**
August 8, 2005

Hi Gavin,

"Am I on track with this?"

In a word, yes. However, one important difference between the standard model and string theory is that in the standard model case we have a non-perturbative formulation of the theory (although there is an interesting caveat about chiral gauge couplings), whereas there is no workable non-perturbative formulation of string theory that includes the standard model as a limit.

39. **Aaron Bergman**
August 8, 2005

I don’t have Penrose’s book, so I can’t comment.

Bogdanovs on Wikipedia

August 6, 2005
Categories: Uncategorized

I've never really understood how Wikipedia works, especially how it protects itself from getting filled with nonsense. The entry about the Bogdanovs has evidently recently been the subject of repeated attempts by the Bogdanovs to modify it, one can follow the history here. This sort of thing seems to be enough of a problem that steps are being taken to prevent this kind of abuse.

Comments

1. **Nigel Cook**
   August 6, 2005

   Wikipedia stores previous versions of itself so changes can be indentified and reverted to the original easily if necessary. There is also a discussion facility for each entry where the rationale for a change can be argued. The battle is won by those nerds who have nothing else to do but keep changing or reverting entries to the way they want. So the Bogdanovs will probably win by sneaky tactics.

2. **Stan Seibert**
   August 7, 2005

   I also suspect the vast majority of Wikipedia articles are not controversial or "interesting" enough to attract the sort who would engage in such edit wars. Articles on slime molds and Happy, Texas aren't go to be targets for this sort of abuse. But when it's bad, it's really bad.

3. **JF Moore**
   August 15, 2005

   Never underestimate the penchant for control among some people.

   It's what keeps many forums moderated.

Strings on the Beach

August 6, 2005
Categories: Uncategorized

The KITP in Santa Barbara, a few steps from the beach, is running a semester long program on Mathematical Structures in String Theory that started this past week. Some of the talks are already on-line.

On the other coast, as part of the Simons Workshop at Stony Brook, Witten will be giving a talk at Smith Point beach on “Gauge theory and the Geometric Langlands Program”. This sounds like it might be very interesting and mercifully off the main topic of the workshop, which seems to be swamps not beaches.

No Comments

Mathematics and Narrative

August 6, 2005
Categories: Uncategorized

A group called Thales and Friends, based in Greece and sponsored by MSRI, has organized a recent conference about Mathematics and Narrative that took place last month on Mykonos. Their web-site has abstracts of the talks and some other interesting material.

Update: There's an article about this in the Independent.

Comments

1. **Chris Oakley**
   August 6, 2005

   Hi Peter,

   Re: Mathematics generally: on my web site I say something to the effect that String theory belongs in the mathematics department (rather than physics). Obviously this is not the first time anyone has said this, but I am not confident about saying this: you have suggested on at least one occasion in the past that the study does not belong here either. Could you elaborate?

2. **woit**
   August 6, 2005

   Hi Chris,

   There are some parts of string theory, e.g. mirror symmetry, topological strings, conformal field theory, which could reasonably be characterized as mathematics. The people working on these subjects might even benefit from being in a math dept. There are many others though that certainly aren't mathematics. Some of these are perfectly good physics (e.g. AdS/CFT), others aren't even really physics at all (landscape studies), and there are some where you could argue about whether they're physics or not (string phenomenology, string cosmology, etc.), but they're certainly not mathematics.

3. **David Corfield**
   August 7, 2005

   The meeting is reported in a 2 page article in this week's edition of the journal Nature (4 August).

New Theoretical Physics Institute in Florence

August 8, 2005
Categories: Uncategorized

A new institute devoted to theoretical particle physics has been organized, the Galileo Galilei Institute for Theoretical Physics, which will be located in Florence. The purpose of the institute is to organize advanced workshops, the first of which will take place next spring. There will also be an inaugural conference next month. The institute is clearly modeled after the KITP in Santa Barbara, and its web-site design looks very familiar…

No Comments

Panel Discussion Video

August 8, 2005
Categories: Strings 2XXX, Uncategorized

Video of the panel discussion at Toronto is now available, so one can hear some of the context of the comments that were reported in the recent New York Times article. As reported, the audience voted 4 or 5 to 1 against the anthropic principle. Unfortunately the camera was not on the panel during the vote, so one can't tell from this video how the panelists voted.

Some other things that weren't reported: while Andy Strominger commented that he saw no reason for pessimism, he also said he thought the odds were against any data relevant to quantum gravity or string theory coming out of the LHC. Steve Shenker said that he was very much bothered by the fact that it was starting to look as if one could associate some sort of "quantum gravity" dual to any quantum mechanical system whatsoever, so any notion of uniqueness was completely gone.

There were several skeptical questions from the audience. Someone with a Russian accent pointed out that it was becoming increasingly difficult to argue the case for string theory in the physics community, and asked what argument he should use in its favor. The panel didn't seem to want to address this, but Shenker finally said "Only consistent theory of quantum gravity". The next question wasn't really audible, but had something to do with "it's been 20 years". Shenker's response was something like "most of us don't want to think about this, we haven't done as well as in other 20 year periods". Later on someone asked "Can you imagine any experiment in the next 20 years that will falsify string theory", getting no real response except "You're not supposed to be asking that" from Shenker. Another question from the floor was about why none of the panelists had mentioned M-theory, which didn't get much of an answer except from Nathan Berkovits who commented that in particle theory problems not solved in five years stop being discussed.

In their speculation about the future, many of the panelists invoked the possibility of having to change quantum mechanics. From the floor Witten speculated that quantum mechanics was only valid in asymptotic regions of space time, with something different needed to understand the interior. Also from the floor Susskind speculated that the splittings into different universes of the many-worlds interpretation of quantum mechanics were the same as the cosmological bubbling off of different baby universes. Several panelists responded that they had no idea what he was talking about.

The emphasis on vague ideas about the foundations and interpretation of quantum mechanics led Martin Rocek to point out that there was one field of study in physics that had gone nowhere in the last eighty years: the study of the interpretational issues in quantum mechanics. Lee Smolin rose to the defense of this field, claiming that it had led to recent ideas about quantum computers.

Also now available online are videos of the public talks by Dijkgraaf and Susskind. Susskind tells the audience that there is a "War" or "battle of intellects" going on between two groups of physicists, which he describes as being "like a high-school cafeteria food fight". The two groups are the "As" (A for anthropic), and the "Es" (E for elegant). He describes the belief by the Es in mathematical elegance as "faith-based science", and says that they are in "psychological denial" about the existence of the landscape, then goes on to give the standard arguments for the landscape and the anthropic use of it to "explain" the value of the cosmological constant. He refers to belief in the existence of a vacuum selection principle as analogous to belief in the Loch Ness monster. He ended his talk by claiming that the As were winning the war, with the Es in retreat.

Dijkgraaf's talk was completely standard string evangelism, and except for a couple slides mentioning D-branes and black holes, could easily have been given, completely unchanged, twenty years ago.

Comments

1. **Kea**
   August 8, 2005

   "Steve Shenker said that he was very much bothered by the fact that it was starting to look as if one could associate some sort of "quantum gravity?? dual to any quantum mechanical system whatsoever…"

   Why should he be bothered? It's wonderful. That's the whole point. They just need to give up the idea that String theory is physics and there wouldn't be any anthropic problem.

2. **Dave Bacon**
   August 8, 2005

   While it is certainly true that David Deutsch began dreaming of quantum computers as a way to "test the many-world's interpretation" I'm not sure whether the connection between interpretations and quantum computers is all that tight. After this initial major step (Deutsch is the first to seriously consider the computational advantages of quantum computers over classical computers. His result with Richard Jozsa in 1992 was the result which set the quantum algorithm ball rolling.) there really has not been a huge amount of interaction between those who study interpretation and those who study quantum computation (with notable exceptions, for example, Chris Fuchs and Robert Spekkens)

   It's very interesting to hear people comment on modifying quantum theory. From a computer science prospective this seems very dangerous. The reason is that many of the modifications of quantum theory which we have been able to dream up, lead to modified quantum computers with pretty astounding computational abilities. Now, it could be that our universe does provide such strong computation, but there are definitely reasons to be very troubled by such a turn. See for example, Scott Aaronson's article, quant-ph/0502072: "NP-complete Problems and Physical Reality" for interesting thoughts on this matter.

3. **Wolfgang**
   August 8, 2005

   Just a really naive thought about this issue:
   The wavelength of macroscopic bodies is typically much smaller than the Planck length. One would thus assume that modifications of quantum theory for macroscopic bodies a la Penrose are possible if not likely.

4. **Quantoken**
   August 8, 2005

   Peter said:

   "In their speculation about the future, many of the panelists invoked the possibility of having to **change** quantum mechanics. From the floor Witten speculated that quantum mechanics was only valid in asymptotic regions of space time, with something different needed to understand the interior. "

   I wonder what makes them think so? And if QM is to be changed at certain small scale, what they are going to change it to? A QM with a different value of hbar, or no

QM at all but return to classical limit at small scale? I really don't know what Witten was thinking. QM is all about uncertainty principle. You either HAVE an uncertainty principle, which is QM, or you DON'T have an uncertainty principle, which is classical. It's a yes or no and there is no third possibility, so there is really nothing to be modified of QM, even at small scales.

It is also true that QM received far more rigorous experimental tests at microscopic scales than GR does. What needs modification at small scale is GR, not QM. Graviton, as a naive prediction of **quantized** gravity force, does NOT exist and does not need to exist. When people DO realize that graviton can not exist, then that's a death sentence for super string theory, since super string theory would have **wrongly** predicted the existence of graviton, something that just does not exist in nature.

Quantoken

5. **Robert**
   August 8, 2005

   From what I can tell from listening to the realaudio is that someone asked "Can you imagine any experiment…, that someone is Jan de Boer and the person with the Russian accent is Sergei Ketov oder Djordje Minic.

6. **Nigel Cook**
   August 8, 2005

   "… Susskind speculated that the splittings into different universes of the many-worlds interpretation of quantum mechanics were the same as the cosmological bubbling off of different baby universes. Several panelists responded that they had no idea what he was talking about."

   What a character Susskind is. Feynman, 'QED', says wavy electron orbits occur due to path integrals interference in the small atomic space, and in a footnote he says path integrals get rid of the problems with Copenhagen Interpretation. He could have added that it also gets rid of the many-worlds. Path integrals is useful, even if Feynman didn't derive the Schroedinger equation directly from it. Bohm did that using hidden variables in 1952, using chaotic brownian motion of electrons, caused by wave interference.

7. **Lee Smolin**
   August 8, 2005

   If, as is true, quantum computers were invented by someone seeking a way to test ideas about the foundations of quantum mechanics, then that is a pretty tight connection. What I see is that quite a few of the people working in quantum information theory (QIT) were and still are motivated by issues in foundations,besides those mentioned these include Anton Zeilinger, Lucien Hardy and many others. And while there are QIT people with no interest in foundations and foundations people with no interest in QIT, I observe quite a healthy interaction among people working in the two fields. But the point I wanted to make to Martin Rocek is much simpler. When we were students together people advised us to go into high energy theory rather than foundations of quantum theory because there would be much more chance for contact with new experimental discoveries. Looking back on the last 30 years, one would have to say that there were more opportunities for a theorist to propose new phenonomena that were confirmed experimentally in foundations than in high energy theory. And if the field turned out to spawn a whole new technology, then supporting the few theorists with the courage to think about foundations will have turned out to have been a good investment.

   And I agree that Scott Aaronson's paper is well worth reading.

8. **D R Lunsford**
   August 8, 2005

   Wow, I leave for a week and the Blog is filled with string propaganda. Peter's become a shill for blighted stringers 🙂

   -drl

9. **Fyodor Uckoff**
   August 8, 2005

   Somebody should really call Susskind on his deliberate confounding of the existence of a landscape [generally accepted, and no big deal — did anyone criticize Maxwell or Einstein because their theories admit lots of solutions?] and the anthropic principle. The idea that there exists a probabilistic vacuum selection principle just seems like the kind of thing quantum mechanics does for us all the time, and shouldn't be controversial. The Tye group at Cornell even have concrete ideas about how it might work, though of course their models are very basic so far. [See Tye's presentation at Strings 2005]

10. **Peter**
    August 8, 2005

    I think Susskind did address this in his public talk. He referred to the vacuum selection principle as "the Loch Ness monster", something that lots of people claimed to have seen, but no one actually knows anything about. If you know what the selection principle is, let's hear about it and see you use it to, like, select a vacuum. The Tye stuff is very far from being able to do anything like this, and I see no good reason to believe it ever will.

    Susskind would say that people like you are just in denial and engaging in wishful thinking. For once, I'd agree with him.

11. **Dave Bacon**
    August 8, 2005

    Hey Lee! I don't disagree that there isn't more talk between the interpretation crowd (whatever that is!) and quantum information science (whatever that is!), but when I think of the main results of quantum information science (Peter Shor's quantum algorithm for factoring, Lov Grover's quantum algorithm for searching, the theory of fault-tolerant quantum computation, quantum cryptography, and the main results of quantum information theory (Schumacher quantum data compression, entanglement concentration, etc.)) the results aren't "directly" related to foundational questions. On the other hand, most of the results and the people who invented them are extremely interested in foundational questions, I think. This is, I think, because a healthy understanding of foundational questions means leads one to a very good understand of what quantum theory is and what it is not. For example, in quantum teleportation, understanding what role a quantum wave function plays seems essential to making the jump to the protocol of transfering that quantum state using entanglement and classical communication. So the main results are unconnected to foundations, but by studying foundations it's easier to come up with results that cut to the heart of what quantum theory is! (And I say this with the caveat that some of my work has been motivated by interpretation questions, but this lead directly to something which feels very non-interpretational: the communication cost of simulating quantum correlations.)

    Peter, sorry for turning your comments into a quantum computing discussion, but, you know, I just couldn't resist 😉

12. **Dave Bacon**
    August 8, 2005

Oh, and as for my personal views, I'm very sympathetic to the notion that the foundations of quantum theory have something important to say about the problems of quantum gravity. Just what it has to say, however, is something else!

13. **Tony Smith**
August 8, 2005

Peter's blog entry said "… Witten speculated that quantum mechanics was only valid in asymptotic regions of space time, with something different needed to understand the interior …" and that "… Lee Smolin rose to the defense of … the foundations and interpretation of quantum mechanics … claiming that it had led to recent ideas about quantum computers. …".

Perhaps quantum theory does NOT need to be modified, but is actually a good alternative to conventional superstring theory as a "framework" for a unified physics theory.

For instance, in http://xxx.lanl.gov/abs/quant-ph/9512022 , Negative entropy and information in quantum mechanics, N. J. Cerf and C. Adami said:
"… Quantum information theory … allows for negative conditional entropy even though this is forbidden classically. This leads us to propose that such quantum informational processes can be described by diagrams — much like particle physics reactions — involving particles carrying negative (virtual) information. By analogy with anti-particles, we refer to them as anti-qubits. … This analysis suggests the possibility that a qubit (the fundamental quantum of information) could have an analogously defined anti-qubit (a quantum of negative information), formally equivalent to a qubit traveling backwards in time … this leads us to conjecture that these processes can be recast into reactions involving information quanta … described by diagrams, much like particle physics reactions. …".

Taking such an information – particle physics correspondence seriously is one of the primary motivations I have had in constructing my Clifford-algebra related physics models. It is a puzzle to me that Clifford-algebra physics models have not become a well-funded school of physics. AFAIK, the only work on Clifford-algebra physics models closely related to information theory (as opposed to just reformulating existing physics models in terms of Clifford/Geometric algebra) has been done by David Finkelstein and his students (I am one of his former students, and that is where I learned my Clifford algebra stuff).
Maybe there might be interesting socio-political reasons why Lenny Susskind's anthropic superstring stuff is well-funded with a horde of rabidly devoted followers, while David Finkelstein's Clifford-algebra stuff is largely ignored (and in the case of some of those who learned from him, such as me, even blacklisted).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

14. **rof**
August 8, 2005

One question: Dan Friedan, a man who is, arguably, responsible for the present-day understanding of the relationship between string theory and gravity, says that string theory neither predicts gravity nor spacetime quantum field theory (see hep-th/0204131, which is cited on your home page, Peter). I understand that the relationship between string theory and gravity is "a matter of interpretation", meaning that the occurrence of something which looks like Einstein's equations as the condition for the vanishing of the beta function of string theory (prescribing reparametrization scale invariance of the world sheet) , has yet to be universally accepted as a secure indication of a consistent theory of quantum gravity (indeed, the successful reproduction of the Hawking entropy formula would hardly have been considered an additional successful accomplishment, if the "vanishing of the beta function implies general relativity" argument had already been fully accepted as a proof that the theory in question is a coherent theory of quantum gravity). Nevertheless, my question is instead about the claim that string theory reproduces spacetime quantum field theory as a consequence, at low energies. Dan Friedan claims that this is unproved. I have not seen anything which even claims to be a proof of this rather important statement. A senior string theorist (who shall not be named here, unless he cares to step forward and name himself) has told me that a proof was to be found in Green, Shwartz and Witten. I inspected the said tome and instead found a section which claimed that a proof would be too unwieldy to provide in such a slender volume, and that the correct procedure was to assume in advance that there was a QFT which corresponded to the low-energy limit of a string theory, and that merely matching the S-matrices of each sufficed (obviously, duh), to establish that one was the low-energy limit of the other. Now, you've read Friedan's article, and presumably GSW, so what's your opinion? With all due respect to everybody involved, I have never heard a coherent articulation of both the question and the answer from a single individual, apart from Dan Friedan, who advocates studying his own personal theory instead of string theory.

15. **Matti Pitkanen**
August 9, 2005

A comment concerning possible modifications of quantum mechanics mentioned by Witten and therefore belonging to the topics of discussion (as I dare hope).

At algebraic level the modification could reduce to a choice between different types of von Neumann algebras. Von Neumann algebras allow hyper-finite factors of type II_1 which differ from factors of type III encountered in Poincare invariant relativistic quantum field theories and from type I_n and I_infty factors encountered in non-relativistic quantum theory.

Hyperfinite Type II_1 factors emerge when you provide infinite-dimensional separable Hilbert space with spinor structure such that unit operator has unit trace. Von Neumann thought that probability interpretation requires this property and regarded factors of type III pathological. Unit trace could be defended also by the finiteness of quantum theory of infinite systems. These algebras emerge naturally when you have an infinite-dimensional configuration space with Kaehler metric and spinor structure (the space of 3-D surfaces in certain 8-D imbedding space, the "world of classical worlds", in the case of TGD).

The mathematics of inclusions of hyper-finite factors has a close relationship to that of conformal field theories, quantum groups, braid groups, knot and 3-manifold invariants, etc… The sequences of Jones inclusions of these algebras have interpretation in terms of sub-system-system inclusions. A model for topological quantum computation led to the idea about the quantization of hbar in terms of Beraha numbers so that hbar would become a characterizer of Jones inclusion and would be dynamical and quantized. Large hbar phase would be of obvious interest from the point of view of quantum computation.

A generalization of braid diagrams to Feynman diagrams suggests strongly itself together with a symmetry principle generalizing string model duality. Diagrams would be classified by the topology of the lowest genus two-surface allowing the imbedding of diagram and all diagrams with homologically trivial loops at this genus are equivalent to a minimal diagram characterized by its homology class for the miminal genus. The absence of homologically non-trivial loops has in TGD framework straightforward interpretation: there is no path integral over all possible 4-surfacace since configuration space geometry assigns an almost unique space-time surface to a given 3-surface identifiable as a generalized Bohr orbit. Almost uniqueness means the failure of strict classical determinism: this makes it possible to assign space-time correlates not only to quantum states but also quantum jump sequences.

I have been working out the consequences of this picture for more than year year with a particular emphasis on macroscopic and macrotemporal quantum coherence and understanding of dark matter as large hbar phase. See 'What's New' links of various books about TGD at http://www.physics.helsinki.fi/~matpitka/ and my blog site http://matpitka.blogspot.com.

Matti Pitkanen

16. **Fyodor Uckoff**
August 9, 2005

If you know what the selection principle is, let's hear about it and see you use it to, like, select a vacuum. The Tye stuff is very far from being able to do anything like this, and I see no good reason to believe it ever will.

Well, if you actually bother to read their papers, they do argue that their version of Hartle-Hawking selects the KKLMMT vacuum. Of course they are open about the fact that their methods are very primitive, but their results are sure a hell of a lot more impressive than Susskind's….which are what, exactly?

Susskind would say that people like you are just in denial and engaging in wishful thinking. For once, I'd agree with him.

So now you find yourself in eager accord with Lubos Motl and Susskind. If that isn't a warning sign, then what is?

17. **Thomas Larsson**
August 9, 2005

Vacuum selection is only a problem if you accept the premise that string theory has anything to do with quantum gravity. If the right problem rather is to quantize gravity coupled to the standard model in 4D, as experiments indicate, vacuum selection is not an issue anymore. But you have to do it right, of course, and not ignore anomalies, which we know exist already in 2D gravity.

Besides, I observe that mr FUckoff is either too incompetent to publish even a single paper on the ArXiv, or too coward to use his real name. Either way, I don't understand why anybody should care about his comments.

18. **Alejandro Rivero**
August 9, 2005

I think we should praise Toronto Committee, they started very slow, but they have made a very good work on availability of the talks.

As for these "20 years periods"… can anyone inform me what kind of milestom do they use? I count between 30 and 35 years since last batch of significant theoretical events.

19. **Alejandro Rivero**
August 9, 2005

(milestone)

20. **Peter**
August 9, 2005

Alejandro,

Everyone is using "20 years" to refer to the fact that it was the fall of 1984 – early 1985 when the huge amount of effort going into the study of string theory started. The last really big, successful new piece of our understanding of particle physics was probably the discovery of asymptotic freedom, over 32 years ago.

Fyodor,

In this war I agree with both sides. The As aren't doing science and the Es are in denial about what has happened. There's an obvious point of view under which they are both right….

Matti (or anyone else),

Please do not post here attempts to start off-topic discussions about things like your favorite alternatives to quantum mechanics. In this case, if you inside information about what Witten had in mind, it would be interesting to hear it, but otherwise please don't do this.

21. **Thomas Larsson**
August 9, 2005

From Peter's post I in fact got the impression that Witten was contemplating giving up QM, perhaps replacing it by something like 't Hooftian Planck-scale determinism. Instead, he pointed out the rather obvious fact that if your quantum theory is defined in terms of asymptotic data, things become murky if your spacetime does not allow for the right kind of asymptotia. He is probably worrying about the positive CC, which leads to de Sitter spacetime, which indeed has precisely this kind of problems. Witten made this point in [hep-th/0106109](hep-th/0106109), and I doubt that anything really has changed since then.

22. **Luboš Motl**
August 9, 2005

On my blog, http://motls.blogspot.com/ , you may find a discussion of the achievements of the interpretation of quantum mechanics research, and about uniqueness of quantum gravity and its compatibility with holography.

23. **Ben**
August 9, 2005

Please pardon my ignorance as a non-physicist reader of this blog, but WRT Shenker's comments that one can associate a quantum gravity model with any quantum field theory, should one take out of this that: (a.) there is no unique quantum gravity theory, (b.) one just needs to find the right field theory, or (c.) there is a unique theory of quantum gravity, but it won't be coming from string theory? I'm a bit confused…

24. **Peter**
August 9, 2005

Hi Ben,

You're not the only one who is confused about this, I think everyone is, thus Shenker's worries. See Lubos's blog posting that he mentioned for some comments about this, although I don't think they'll really answer your question.

The problem right now for the string theory program is that their best argument, that strings give a quantum gravity theory, suffers from the embarassment that, if correct, it seems to give an infinite number of quantum gravity theories. I think the standard hope from 20 years ago that string theory will lead to a unique TOE has now become essentially untenable (although Lubos does his best to come up with a scenario to rescue things). People have reacted to this in two ways

1. The anthropic/landscape scenario: there's an infinite number of possibilities out there, all we can do is study them all and see which ones can support life, then try and

get some prediction of something out of that.

2. Deciding that any attempt to connect string theory to a TOE is just premature, arguing that the existence of these string duals to interesting QFTs is one thing that makes string theory very much worth continuting to invest time in. Maybe once one learns more about string theory a path to a TOE will become clear.

25. **Lee Smolin**
August 9, 2005

Regarding quantum gravity and the foundations of quantum mechanics: it can only be good news if Witten, Lobus and other string theoirsts are finally coming to terms with the possibility that the problems of extending quantum theory to cosmology may force us to revise the foundations of quantum theory. This is a welcome development, as those in the quantum gravity world have been thinking about this for decadees and have already published several concrete proposals for how quantum theory may be modified to include cosmology. These include 1) 't Hooft, in his original formulation of the holographic principle and subsequent work. 2) Penrose's proposals for non-linear modifications of the Schrodinger equation motivated by quantum gravity, which by the way leads to real experiments. 3) Gell-Mann, Hartle, Butterfield ,Isham and other attempts to formulate a generalized quantum theory for quantum cosmology. 4) The proposal of relational quantum theory, by Crane, which in some ways anticipated the holographic principle, developed by Rovelli and others. 5) Quantum causal historeis, proposed by Markopoulou as an alternative formulation of quantum cosmology. 6) a hidden variable theory inspired by LQG, published by Markopoulou and myself, 7) reinterpretations of matarix models, including the BFSS model, as hidden variables theories, by Adler, Starodubtsev and myself and 8) proposals by Dowker, Sorkin et al related to causal sets.

Perhaps if string theorist get involved in this question they can do better; that would be very welcome.

So we don't have to debate how useful foundations research has been, so long as we agree that the problem of quantum cosmology forces us to revisit the issue. I would only suggest that anyone wishing to think about this problem woud do well to study the literature, as in any field of sciience, to learn about the good ideas already under development and to prevent repeating mistakes already made.

Lee

ps references as usual in my "Invitation…"

26. **Chris W.**
August 9, 2005

A reminder: Stephen Adler presented his ideas on an alternative foundation for quantum mechanics and quantum field theory in a book published about a year ago. A draft appeared on arXiv.org in 2002.

27. **rrtucci**
August 9, 2005

In my opinion,

(1)There is a good chance that Quantum Computing/Quantum Information (QC/QI)
can shed some important insights into Quantum Gravity

(2)QC/QI can be practiced in an interpretation neutral (i.e., shut up and calculate) way, following the standard rules of QM (i.e., no tinkering with its foundations). 99% of the QC/QI papers in arxiv are like that. So, it's misleading to imply at this point in history a big overlap between (QC/QI) and (Foundations of QM or Interpretations of QM)

(3)QC/QI can and will be tested in the lab MUCH sooner than String theory. Furthermore, unlike String Theory, QC/QI promises to yield useful devices.

28. **Aaron Bergman**
August 9, 2005

I completely agree with #2 and #3. But, especially #2. #1, who knows?

29. **Peter**
August 9, 2005

rof,

I realized I didn't respond to your question. I'll leave that one to the string theory experts, admitting that I'm not intimately familiar with every argument in Green-Schwarz-Witten. This isn't something I've ever thought much about, largely because the claim that at low energies string theory reduces to a QFT seems quite plausible. The time I've spent learning about string theory has been more devoted to trying to understand the claims about the theory that seem to me implausible, not the plausible ones.

30. **Scott Aaronson**
August 9, 2005

Regarding the question of whether quantum foundations has gone anywhere in the past 80 years: I think one needs to distinguish carefully between "results-oriented" and "non-results-oriented" research. The "results-oriented" side of quantum foundations typically involves either experiments or nontrivial theorems (even if the theorems seem trivial in retrospect — like Bell's Theorem, the Kochen-Specker Theorem, or the dense quantum coding theorem). The "non-results-oriented" side typically involves assertions that a particular stance toward the double-slit experiment is the correct one, and that if other people fail to see that, then it must be because they're too dense. I would characterize the results-oriented side as having made phenomenal progress (especially from the interaction with quantum information over the last 15 years), and the non-results-oriented side as having made zero progress (except when it 'accidentally' stimulates a result, similarly to how Mach stimulated Einstein's work on GR). I know that sounds like a tautology, but I think it's a useful tautology for choosing quantum foundations problems!

31. **rof**
August 9, 2005

Thanks, Peter. It would be good if a string expert can shed some light on the question. It seemed almost trivial to me as well that string theory would produce something like QFT at large distances until I read the article and thought about it a little harder. To say that it will look like QFT at long distances is really to extrapolate from short-distance physics to long-distance physics.

On the subject of revising the foundations of quantum mechanics, there are two senses in which one can read the word "revise". The writings of Heisenberg and Von Neumann are still among the clearest expositions of what quantum mechanics is all about. Somewhere over the last eighty years, a shift occurred in the attitude of physicists to the role of the observer. It's now considered common sense that the observer should be treated as just another part of the system, but for the founders of quantum mechanics, the observer played a crucial role.

Von Neumann said, in "Mathematical Principles of Quantum Theory" that one must always split the world into observer and observed, or else one proceeds vacuously, since the purpose of quantum mechanics is to provide relationships between the results of measurements, and without an observer, there are no measurements to relate to one another, and all that is left is empty formalism.

One important development in the foundations of quantum mechanics over the last eighty years has been the almost universal rejection of Von Neumann's (and Heisenberg's and Bohr's) understanding of what quantum mechanics is fundamentally about. It didn't come about because of a research program, though, but rather because of the gut feeling that every physicist has that the observer should play no special role in the theory.

32. **rof**
August 9, 2005

One other point: The purpose of examining the foundations of any subject isn't to produce new results and new technologies, but is rather to develop a clear understanding of what the subject is and why it is the way that it is.

If the foundations of quantum mechanics are dismissed as unworthy of study because studies of them haven't produced any new results lately, then this is like saying that elementary calculus shouldn't be studied because it hasn't produced any new results. The measure of value is inappropriate.

It might be argued that physicists have done very well over the last eighty years without knowing much about the foundations of quantum mechanics, so why start thinking about them now? Lee provides the answer in his post above: when it comes to cosmology, there can't be an outside observer, and so the present formulation of quantum mechanics is incompatible with the idea of taking the whole universe as a single quantum system.

33. **Nigel Cook**
August 9, 2005

Lee's seven promising options include one of causality and two of hidden variables. It would be nice to have a convergence toward consensus, which is vital to avoid a disintegration of the research-education infrastructure. Without consensus, it's very hard to teach with interest at lower levels, because it looks a bit like speculative gambling or the disintegration of central ideas. It would be nice to see some things unified carefully, like path integrals and some hidden variables. This is not regression to determinism, because you still have uncertainty, you just have a cause for it like chaos due to wave interference. 'Caloric', fluid heat theory, eventually gave way to two separate mechanisms, kinetic theory and radiation. This was after Prevost in 1792 suggested constant temperature is a dynamic system, with emission in equilibrium with the reception of energy. The electromagnetic field energy exchange process is not treated with causal mechanism in current QFT, perhaps if it was it would turn out to be the missing hidden variable needed.

34. **Kea**
August 9, 2005

"From the floor Witten speculated that quantum mechanics was only valid in asymptotic regions of space time, with something different needed to understand the interior"

I don't understand why people find this surprising. Asymptotic regions are, after all, what we are used to. Of course we want QM to hold in this domain, but we should also expect it to be extended (or 'altered' if you like) in a theory of quantum gravity. I don't think Witten meant anything complicated. Maybe someone could ask him.

35. **Dave Bacon**
August 9, 2005

Kea: Not surprising. Just (1) hard to do, and (2) not the main path advocated by the majority of theoretical physicists.

36. **Chris W.**
August 9, 2005

Nigel,

Lee Smolin referred to *quantum causal histories* and *causal sets*. He didn't use the loose term "causality".

37. **Not a Nobel Laureate**
August 9, 2005

This talk of the "need" to modify QM reminds me of the one-upon-a-time talk of the "need" to modify QFT to describe the Strong Nuclear force. Before the discovery of quarks/partons and the development of QCD and asymptotic freedom.

Will String Theory suffer the same fate as Regge Pole Theory?

It would certainly be interesting if someone could come up an alternative to QM that would survive the battery of possible experiments.

I'll admit to being rather "old-school". I still think that physics is an experimental science.

38. **garrett**
August 9, 2005

Hey Peter,
Have you considered starting a group weblog for physics phd's who don't like string theory and are actively pursuing alternatives?
-Garrett

39. **Tony Smith**
August 10, 2005

Not a Nobel Laureate commented:
"… This talk of the "need?? to modify QM reminds me of the one-upon-a-time talk of the "need?? to modify QFT to describe the Strong Nuclear force. Before the discovery of quarks/partons and the development of QCD and asymptotic freedom.
Will String Theory suffer the same fate as Regge Pole Theory? …"

As I have commented in another thread on Peter's blog:
"… David Gross said, in Chapter 11 of the book The Rise of the Standard Model – Particle Physics in the 1960s and 1970s, edited by Hoddeson, Brown, Riordan, and Dresden (Cambridge 1997): "… The bootstrap idea was immmensely popular in the early 1960s … it rested on the solid principles of causality and unitarity … It

promised to … provide a unique value for all observables … This is of course false. We now know that there are an infinite number of consistent S-matrices that satisfy all the sacred principles. …??. …".

It seems to me that the 20 years of Witten's superstrings (1984-now) is similar to the 20 years of Chew's Bootstrap (1950s-mid1970s).

Chew's Bootstrap fad did not end until the Standard Model emerged in the mid-1970s.

I doubt that Witten's superstrings fad will end unless or until an alternative model that is as connected to experiment as the Standard Model is recognized and accepted. Barring that (unlikely in view of the blogs and comments over the past few weeks) event, I expect that bureaucratic inertia will be sufficient for conventional superstring theory to maintain its dominant position for the forseeable future (at least through Lubos's retirement). From my point of view, that amounts to a New Dark Age of physics.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

40. **Matti Pitkanen**
August 10, 2005

Tony,

I am again and again astonished by the extreme orthodoxy of both string theorists and also many of those who do not like string theory. To me it looks utterly inconceivable that a theory created during first five centuries of physics and containing obvious logical paradoxes (mention only quantum measurement problem) could be final and that the only challenges would relate to purely technical problems of M-theory as Lubos argued in complete seriousness in his blog.

What also surprises me is the extremely hostile attitude towards new well-formulated ideas and theories making precise predictions when the authority provided by a formal academic position is lacking. It seems that all that we are allowed to do is to try to interpret casual remarks made by Witten or some other name. This extremely authoritarian atmosphere really brings in my mind dark middle age.

Quite concretely, for some time ago Science reported a statistical survey which suggests that rate of technological discoveries per capita was at maximum at thirties and has now reached the level of 1600 century. Although information technology was not included in the study, I tend to believe that the survey reflects the reality. Perhaps it is not accident that the shut-up-and calculate philosophy was fully established in physics after thirties. Together with the reductionistic and materialistic world view and heavy censorship this makes intellectual breakthroughs impossible at the level of collective.

Matti Pitkanen

41. **Fyodor Uckoff**
August 10, 2005

Anomalies Larsson said:

"Vacuum selection is only a problem if you anomalies the anomalies that string theory has anything to do with anomalies. If the right anomalies rather is to quantize anomalies coupled to the standard anomalies in 4D, as anomalies indicate, vacuum anomalies is not an anomalies anymore. But you have to anomalies it right, of course, and not ignore anomalies, which we know anomalies already in 2D anomalies."

What is the sound of one pot cracking?

Anyway, back in the real world, most string theorists do think that the landscape idea should be explored — see the very sensible remarks by Denef and Maldacena over at cosmic variance.com. That is a long, long way from endorsement of anthropic ideas. People are looking for a vacuum selection principle; the search has only just started, precisely in response to this situation. Prof Tye is outspokenly "misanthropic", and says so in his papers. No crisis, no big problem, unless someone can prove that there is something *technically* wrong with the whole vacuum selection idea.

42. **Quantoken**
August 10, 2005

"…unless someone can prove that there is something *technically* wrong with the whole vacuum selection idea"

Sure there is something technically wrong with the vacuum selection business. The problem is there is a huge gap of magnitude difference between the expectation value of these vacuas, and the value of cosmological constant. It's 10^120 order of magnitude difference. 10^500 vacuas is NOT nearly enough to allow at least one vacua with small enough value to be the correct one. For an analogy, there are 10^10 global population, but I can safely bet that you can NOT find even one single human being who is less than 1/4 the height of the tallest person, despite the huge population.

Let's say the order of magnitude of the energy density of the 10^500 vacuas observe a normal distribution, centered some where around Planck Scale, and the sigma is one order of magnitude. What's the order of one of them being 120 orders of magnitude smaller than "usual", i.e., deviate from the norm at 120 sigma? It would be roughly exp(-(120)^2/2) = exp(-7200) = 10^-3127. A value which is effectively zero when times 10^500.

And maybe some one can prove none of the vacuas can be lower than a certain threshold, which is well above CC. Then you are done with the vacua and landscape business for good.

Quantoken

43. **Alejandro Rivero**
August 10, 2005

"From the floor Witten speculated that quantum mechanics was only valid in asymptotic regions of space time, with something different needed to understand the interior??

Er, early string theoretists (pre-1985) used to say that their origins are rooted in S-matrix theory, which after all was supposed to be the Theory of asymptotic states.

44. **Thomas Larsson**
August 10, 2005

Alejandro,

As I pointed out before, Witten expressed similar concerns already in hep-th/0106109. The keyword "asymptotia" is used frequently both in this paper and in his comment from the floor.

It is remarkable that people like ‘t Hooft, Smolin and now perhaps Witten express doubts about QM. It must be a sign of some kind of crisis, mustn’t it? Let me emphasize that my opinion differ – I do not believe that QM needs any fundamental revision. No matter how wacky my comments may seem, they are in fact a logical consequence of taking both Fock QM and background independence seriously.

45. **Nigel Cook**
August 10, 2005

Thomas: the doubts are probably due to the untestable wacky ideas that some formulations of QM lead to, many-worlds etc. Nobody is going to disprove any part of QM which is experimentally established, but the frontier is at the interpretative boundary. To view Coulomb’s law with QFT as photon energy exchange delivering momentum to produce force is not serious physics, but to have baby universes bubbling off everytime a wavefunction collapses it!

46. **Peter**
August 10, 2005

Fyodor,

It’s just not true that “the search has only started” for a vacuum selection principle. For 20 years it has been clear that some principle was needed to explain which of the many Calabi-Yaus corresponds to the real world, and people have been looking for it, failing utterly.

47. **Peter**
August 10, 2005

Garrett,

Unfortunately I think I’m already spending too much time on the weblogging stuff, but I encourage other people to start other blogs, perhaps even group ones. It’s not that hard, but does take up time.

48. **garrett**
August 10, 2005

Peter,
I can sympathize on the lack of time. Right now you are the strongest voice in the world speaking out against string theory. But, as only one person, you have to carry this off and do all the work yourself. It’s not surprising that it takes a huge chunk of your time to constantly be debating the legions of string theorists. But, you don’t have to carry this flag yourself — there are other theorists out there who think string theory is a red herring. I’ll bet a few would be happy to contribute to a group weblog if you were to host it. Plus, right now, blogs are hot in the media, and group blogs get more attention than lone voices. So, I think you could host a group blog with minimal effort that has a better chance of influencing the direction of physics research a bit. On the technical side, it would just mean mirroring your existing blog and picking out a few researchers to post top level threads. Oh, and it would need a name.
-Garrett

49. **Alejandro Rivero**
August 10, 2005

One year ago in physicsforums we threaded a “quantum gravity haiku”, which can be interesting to mention here again… (on topic now!)

We asked, using classical gravity, for which radius will a test particle to sweep one unit of Planck Area in one unit of Planck time, when orbiting around a particle of mass M.

50. **cvj**
August 10, 2005

Garrett, I’m puzzled and saddened. Why is there a need for a group blog speaking out against string theory? Is there a group blog out there speaking out in favour of string theory? No. There are maybe three sites that are dedicated to regularly discussing results in string theory, and other blogs (such as cosmicvariance) that has some discussion of strings from time to time, none devoted to promoting any particular theory – there are certainly no group blogs of that nature.

You’re promoting the business of doing science by seeing who can shout the loudest? That’s very sad. Wouldn’t it be better to form a blog to do something *positive* where you might often calmly and rationally discuss the alternatives approaches to string theory, rather than existing purely in opposition to something? Come on. Let’s not demean ourselves as a community in this way.

-cvj

51. **Gordon**
August 10, 2005

I agree, Garrett, there is no need for another blog, especially
another site speaking against the string theory, however important
that it is.

52. **cvj**
August 10, 2005

By all means have another blog. Just have it for positive reasons, not negative ones. -cvj

53. **simplex**
August 10, 2005

here is Garrett’s original suggestion posted 9 Aug around 11PM

Hey Peter,
Have you considered starting a group weblog for physics phd’s who don’t like string theory and are actively pursuing alternatives?
-Garrett

I dont think such a blog, of people doing Quantum Gravity alternative to string, would necessarily “demean us as a community” as Clifford evidently fears.

I can see why Peter would not want to set up such a blog, it would have to be set up by one or more people who are ACTIVELY PURSUING ALTERNATIVES on a professional basis. One or more postdocs or young faculty who are involved with the main nonstring lines of QG research.

It would be better, if there were going to be one, for it to be set up by someone like Etera Livine who is a postdoc at Perimeter engaged in nonperturbative/backgr. indep. QG, also possibly in quantum computing.

Maybe if Livine happened to be friends with Willem Westra or Dario Benedetti (both at Utrecht) they could set up something like "Nonperturbative Coffee Table" analogous to String Coffee Table.
This would report on conferences and research gossip. Someone
in Loop cosmology at the postdoc level, maybe Parampreet Singh.

this is just a pipedream. not a real idea. but it would be nice to have
some regular source of news and views from young researchers in QG analogous to string coffeetable.

criticism of string, which Clifford seems to fear would dominate such a blog, would I think hardly be an issue. these people tend in my experience to be too busy with their own interests to bother criticising the string-that-be establishment. have to run, so must leave this
post in rough shape

54. **cvj**
August 10, 2005

Simplex, Do read my post again. You seem to have misunderstood it. Perhaps it was because you were in a hurry. That happens. Let me help you by repeating: I am advocating blogging postitively about alternative approaches and cautioning against blogs set up purely to be in opposition to a specific approach, which is rather different. You use the word "fear". Yes, I fear pointless and wasteful negativity taking over from positive and constructive discussion. I do not fear constructive criticism of string theory. I hope that your positive suggestions are implemented in some shape or form, since I for one would like to learn more about alternative approaches, for my own education about what other people are doing, if nothing else. This is an important quest we're all on; no good idea -in any field- should be wasted.

Best,

-cvj

55. **Peter**
August 10, 2005

About Garrett's suggestion:

First of all, in any case I can't do this. For one thing, while I think it's appropriate for my department to be hosting my current weblog, it probably wouldn't be an appropriate place to host a group weblog like Cosmic Variance. More importantly, the time and energy to coordinate the activities of several very different people would be significant. Especially if they're chosen for not wanting to follow the most popular path in particle theory, they're likely to have strong opinions about how things should be done, and dealing with this would strain my rather limited political skills.

What I 'd really like to see is just a lot more good people setting up their own weblogs, perhaps cooperating to the extent of coordinating with a site that aggregates content from many different places. Alejandro Rivero's physcomments.org is one version of this kind of thing.

As for Clifford's comment: I didn't interpret what Garrett had in mind as an "anti-string theory" site, but rather as a place for people interested in alternatives to string theory to be able to have a forum to exchange ideas. There's a strong feeling of frustration among the people I talk to who aren't happy with string theory about the lack of an active intellectual community that they can participate in. The only one that really seems to exist is the LQG community, to the extent that Simplex just assumed that this is what an alternatives to string theory weblog would be about.

Anyone should soon realize if they follow my postings on this site that I'm fundamentally a particle theorist, not a relativist, and don't see quantum gravity as the most fruitful thing to think about. There's not a shred of experimental evidence about quantum gravity, and little in the way of prospects for getting any during my lifetime. If you believe the most extravagant claims discussed at the Toronto panel, every QFT is dual to a consistent quantum gravity theory, so there is no lack of those. The real problem is not finding a quantum gravity, but finding a quantum gravity that simultaneously explains what is going on in the standard model. String theory became so popular because it promised a solution to this problem, a promise which has not been fulfilled.

I think Garrett and others share in varying degrees my point of view that what is needed is a much deeper understanding of the mathematical structure embodied in the standard model, and a frustration at how difficult it is to sometimes even get other theorists to acknowledge that this is a reasonable thing to be thinking about. Over the past twenty years the reaction I keep getting from many string theorists is that anyone who is still thinking about QFT itself is probably just not smart enough to be working on string theory.

Simplex, whoever he or she is (and by the way, I'm really tired of having my professional qualifications challenged by people who cowardly hide behind the cloak of anonymity) contrasts me to "people who are ACTIVELY PURSUING ALTERNATIVES on a professional basis" who he or she identifies with "postdocs or young faculty who are involved with the main nonstring lines of QG research". Well, believe it or not, there are people out there, actively pursuing alternatives on a professional basis, who aren't involved with the main nonstring lines of QG research. It's a difficult path to follow, largely because there is no active intellectual community to participate in, and much of the physics community, like Simplex, seems to believe that something that doesn't fit into string theory or LQG, like hep-th/0206135, doesn't count as "real" research.

So I think I understand what Garrett would like to see, and I would too, but the difficulties are very large. I've devoted so much time to making the case that string theory has failed as a way of understanding the standard model because I believe that the physics community is not likely to be willing to take an interest in speculative alternative approaches as long as the perception remains that "string theory is our best hope for a unified theory". Why should anyone waste their time on anything but the best hope?

56. **simplex**
August 10, 2005

sorry Peter, my mistake
didnt for a moment intend to challenge your prof. qualifications!
I see how my careless assumption that your research is
not on alternatives to string may have seemed insulting

57. **Peter**
August 10, 2005

My last comment crossed Clifford's, and his reminded me of some other points I wanted to make, especially since I think I'm very much in agreement with him. I don't think that setting up a purely "anti-string theory" blog is a good idea, and despite appearances, that's not what I intend this one to be. As I periodically remind people, "Not Even Wrong" refers to speculative theories that are not well-defined enough to be testable, and I'm all in favor of work on such theories. Such work is the necessary starting point for getting to a theory that is testable. My objection to string theory is not to its speculative or mathematical nature (I'm in favor of those), but to the way in which the field has both immunized itself against any honest evaluation of whether it is getting anywhere or not, and simultaneously made it difficult for other speculative research programs to get any attention.

I try to cover here newsworthy things that seem important to me. These include any news about experimental high energy physics, both any current experimental results and prospects for possible ones in the relatively near future. They also include a lot of material about mathematics, concentrating on those aspects of mathematics that seem likely to me to ultimately have something to do with particle theory. I'm frustrated that there's not more of this kind of thing to write about, unfortunately the area of overlap between mathematics and particle theory that seems most promising to me has had very few people working in it in recent years.

Probably about half the material here is about string theory, and much of that is negative. In my last comment I explained why it seems important to me to try and make the case to the physics community that string theory is not the "best hope" around for unification. It's also true that this is a form of journalism and the string theory story is one in which a lot of amazing things are happening. A novelist couldn't make up things like Lenny Susskind and the "high-school food fight" he sees himself engaged in. It's a fascinating story, I can't wait to see what will happen next, and years from now I think people will look back on this as a truly remarkably weird era in the history of physics.

Well, as an example of the kind of positive thing I think people should be paying attention to, a few days ago I reported that Witten would be speaking today at Smith Point Beach about "Gauge Theory and the Geometric Langlands Program". I should stop this and go listen to his talk, the audio of which has just appeared on the net. Also, should go and check to see if there's anything worth reading on the arXiv, and, yes, finish doing my laundry.

Simplex, just saw your latest comment. Sorry I misinterpreted your earlier one, my error for being overly sensitive about some things.

58. **garrett**
August 11, 2005

I've been out all day, and in coming back I see the suggestion I made has been digested, thought about, and in the end properly interpreted, which — just being a random commenter here — I'm very pleased to see. To elaborate a bit, and since Peter has a good idea of what I was suggesting but it isn't super clear, I want to give the background I had in my head when I proposed it.

I have a group of friends I talk with regularly who are a-religious, and we have a mailing list for random discussions. Sometimes the discussions that pop up are pointing out, making fun of, and/or just shaking our communal head at crazy religious stuff happening in our world. This provides a bond for the group, and some dark humor value. But the group overall is very upbeat and constructive. And more often discussions arise about new technological or interesting scientific developments. It's a good time. But the community wouldn't hold together without its common, and uncommonly held, bond of appreciation for science and complete lack of tolerance for religion in its worst forms.

This is what I had in mind when envisioning an a-string-istic group weblog. Peter's posts exactly capture the dark humor inherent in the dominance of string theory and it's inability to explain our world. But there needs to be a constructive side too. And there are many people, including but not restricted to people doing LQG, CDT, etc., working on alternatives. The sane ones hold two things as sacred: quantum field theory and general relativity. These, I think, include many of the people reading this weblog. And, looking among them, I see a lot of people holding threads that, optimistically, may be hanging off the same piece of cloth. Peter's manifesto suggests everything has to be about connections. Tony Smith sees Clifford algebra behind the standard model. Matti Pitkanen sees everything as related to CP2. I've seen how fermions can arise naturally as the BRST ghosts conjugate to gauge degrees of freedom in a certain formulation of GR. And Gordon, who I've known since we were undergrads, has a different angle on QFT. And all of these people (and I'm sure some reading this whom I've left out), working completely independently, have things that overlap significantly in their models.

And Peter, perhaps the strongest critic of string theory, does post constructive things in this weblog, so I thought he would best get the idea of what I'm suggesting. But, I'm afraid Peter is right and it would be intractable to moderate a group weblog involving such vigorously independent folk. It would be like trying to herd cats. The postings would more than likely digress to each person espousing their work and not looking into the work of the others. The same spark that drives these people to strike out on their own is the one that keeps them from playing well with others. So I can see it would probably just be ugly to try to gather a group of fierce individualists.

We can only hope that someone out there on their own working on crazy ideas will come up with ideas crazy enough to be true.

59. **D R Lunsford**
August 11, 2005

Somewhere in the above mass Smolin wrote:

"..yada yada extending quantum mechanics to cosmology yada yada…"

Pray tell, how is one going to prepare the entire Universe for observation? By who? In what lab?

Talk like this is just meaningless. Word salad.

-drl

60. **cvj**
August 11, 2005

Don't abandon the idea entirely. Let it be a bit incoherent, that's ok. I think it would be great to have a place where someone (one or a few people) simply make posts on what is going on in the "alternative approaches" world, describing for the benefit of everyone what the idea is behind a given peice of work that someone has published, how it fits into the scheme of things, what it is trying to do, and whether there are connections with anything else. Nobody has to moderate it per se – let the cats do what they want in the comments; the posts will speak for themselves – it would still be of value to the entire community to have a place where people can know that alternative ideas are being showcased and discussed. It would relieve some of the frustration that people have because they percieve that string theory is running a closed shop – there'd be a place you can "gather" and have your own chatter, on your own terms – and it would be good for the stirng theorists too, since there'd be less of that random posting of alternative theories right in the middle of a discussion of something else. No matter the value of that alternative, it is hardly ever useful to do that – it just derails any discussion because it would take too long to digest the new material…but people do it. Now they can place a link to a discussion of it on another blog. Everybody wins! Garrett – just do it! It would be a valuable service if you keep high the scientific integrity and the quality of discussion. Start out doing it yourself and simply invite guests to blog from the blog platform every now and again. Some of them might then stay on if the fit is right.

-cvj

61. **Quantoken**

August 11, 2005

I disagree with the notion that Peter is the strongest anti-string critics. Actually I think it's exactly the opposite. He seem to be rather strange in his stands. The strongest argument he ever presented against super string theory research, is that 20 years passed and it doesn't seem to be able to predict anything yet. That argument is rather weak and it hardly even convince himself that SST is wrong, and is easily refuted by noting that QG itself is a difficult topic and 20 year may just not be enough to make progress. There is indication that Peter himself believed **as much as most SST researchers** that there is possibility one day STT **MIGHT** come out all right and all figured out. And Peter nod his head in agreement and sing the praise song with the mob when SSTers point out that there's too much math beauty found in SST that simply could not be wrong. Clearly Peter believes that the argument of math beauty is so strong that he is persuaded by it, too. At the end of day Peter is really not anti-string, but merely complaining the lack of progress, which all but make him a member of the SST camp.

It is rather strange that Peter paid a tremendous interest in every single act of the top SST researchers, and every single word they utter, down to very details. For example he desperately wanted to know how 8 of the SST guys voted in a **behind door settings** (which to me sounds like a silly childish **religious ritual**.) Another example is he paid extremely close attentions to every thing Witten ever says or does.

If one truely believes that SST is the wrong approach that leads no where, then Witten etc are simply wasting their time and they are destined to go no where, and there is no chance they ever discover anything physically significant, then who would one pay **so much close attention** to Witten et al.? To the point that Peter almost looks like a dissiple of Witten as much as Tom Cruise is a dissiple of Ron Hubbard. Why?

62. **Tony Smith**
August 11, 2005

Garrett says:
"… an a-string-istic group weblog … would be intractable to moderate … It would be like trying to herd cats. The postings would more than likely digress to each person espousing their work and not looking into the work of the others. The same spark that drives these people to strike out on their own is the one that keeps them from playing well with others. So I can see it would probably just be ugly to try to gather a group of fierce individualists. …".

cvj says:
"… Nobody has to moderate it per se – let the cats do what they want in the comments; the posts will speak for themselves – it would still be of value to the entire community to have a place where people can know that alternative ideas are being showcased and discussed. It would relieve some of the frustration that people have because they percieve that string theory is running a closed shop … and it would be good for the stirng theorists too, since there'd be less of that random posting of alternative theories right in the middle of a discussion of something else. … Everybody wins! …".

As one of Garrett's cats, my opinions may be relevant, so here they are:

Garrett is correct that it is likely that the cats would "digress to each person espousing their own work". For example, although Matti (another cat) and I agree in using M4xCP2 as a fundamental spacetime geometry, Matti wants to vary hbar and I want to fix hbar as a constant. There is no way that Matti and I can devise a single model that does both at once, so on that point at least Matti and I could never arrive at a consensus model that makes both of us happy.

cvj says such a blog would "relieve some of the frustration" that I have because I do in fact "percieve that string theory is running a closed shop", which closed shop even extends to me being blacklisted from posting on arXiv.
I disagree, because even if such a blog resulted in the production of a consensus alternative model that all the bloggers (and the blog moderators) considered to be realistic, it could not be posted on arXiv if it involved
elements due to blacklisted people (including, but not limited to, Matti and me), and my frustration level might even be increased.

cvj also says "… it would be good for the stirng theorists too, since there'd be less of that random posting of alternative theories …". I do not disagree with cvj that conventional superstring theorists would be happier if alternative theories were hidden from their sight in a ghetto blog that they felt free to totally ignore.

In other words, cvj's remark "Everybody wins!" really means only that "Conventional Superstring Theorists win!".

In my opinion, the main real problem is that there is no mechanism in the physics establishment to evaluate alternative theories, such as, for example, by giving alternative theorists opportunities to defend their work before an establishment forum that would actually consider, criticize, and evaluate the alternative theories.
Given the current political structure of the USA physics community, I feel that it is unlikely that any such forum will ever be set up.

As I have said before, current conventional superstring theory has much in common with Chew's bootstrap theory, which was so socially powerful that even Gell-Mann was intimidated into apologizing that his quarks were not real, just mathematical abstractions that might be useful.
Chew's bootstrap theory remained powerful until the acceptance of the Standard Model, which emerged from Harvard and Princeton with the help of Kobayashi, Maskawa, and 't Hooft.
Until an alternative to conventional superstring theory is accepted by a similarly powerful group of institutions and individuals, it is clear to me that inertia will keep conventional superstring theory in power.
A ghetto blog such as suggested by cvj is highly unlikely to get any alternative accepted by such institutions and individuals, so I agree with Garrett that it is an impractical idea given the culture of the current physics community.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

63. **Alejandro Rivero**
August 11, 2005

drl, your point is additional motivation to rethinking QM in the context of gravity, is not?

64. **Chris W.**
August 11, 2005

Tony,

There can be advantages to being in a ghetto, if you have intelligent company, and time to think about, develop, and discuss your ideas. Maybe this assertion reflects a prejudice of mine, which is that the problem (quantum gravity/unification) is suffering from a surfeit of formalism combined with a shortage of truly significant insights, lucidly expressed. In the end such insights may also be precisely what highest on Peter's agenda — a deeper understanding of QFT and gauge theory. Indeed, how could they not be?

(PS: I wonder if Perimeter Institute would be willing to host such a group blog?)

65. **rof**

August 11, 2005

DRL said:

> Pray tell, how is one going to prepare the entire Universe for observation? By who? In what lab?
>
> Talk like this is just meaningless. Word salad.

Alejandro said:

> drl, your point is additional motivation to rethinking QM in the context of gravity, is not?

DRL is more accurate here, because the distinction needs to be made between gravity and cosmology. I can drop a book in a lab and see it fall and so it is necessary to incorporate gravity if I want to predict the results of experiments.

On the other hand, the quantum formalism, which essentially tells us to represent the state of the system under investigation as a formal sum of measurement results (hence a|up> + b|down> etc), is inapplicable to the universe as a whole, considered as it is in itself, rather than as a set of measurement results obtained by an observer.

Something must yield – either the notion of considering the universe as a whole system seen from the point of view of no observer, or quantum mechanics. Witten, 't Hooft, Penrose and so on seem to be suggesting that it is quantum mechanics that will yield. No doubt they have carefully considered the alternative and rejected it for good reasons which they consider too obvious to need mentioning.

66. **simplex**
August 11, 2005

getting back to the toronto panel video, it occurs to me to ask what happened after each time Lee Smolin spoke. I will go back and check. If there's a pattern, it might be interesting to notice as a way of gauging the receptiveness to ideas from outside, on the part of string theorists in a group. Maybe there's something quite different to learn from the reaction (who can say in advance?).

besides the exchange about quantum computing and foundations (connections between those two lines of research) there were two main instances I can remember.

One was when Smolin urged string researchers to put more effort into background independent reformulation–giving arguments why such a reformulation was urgent, possible, and could lead to testable predictions. this came quite early in the open discussion — around 1:09 to 1:12 (I checked–it was 1:08:45 to 1:12:25 by my counter.)

The other occasion I recall was, I think, after Jan de Boer raised the issue of falsifiability. this was about time 1:44. (I checked–it was 1:43:45) Replying to de Boer, Smolin spoke about predictions of modified Lorentz invariance coming out of LQG and possibly other non-string QG. And he mentioned two tests: one (AUGER) already collecting data and one (GLAST, Smolin didnt name it) scheduled for orbit in a year or so.

This is all I remember, may have to correct or add some significant detail after another hearing.

67. **Chris W.**
August 11, 2005

Another reminder, regarding the search for testable QG consequences: There is some recent work indicating that violations of Lorentz invariance associated with quantum gravity may be generically more acute than previously assumed, unless some sort of fine tuning — or symmetry — suppresses them. A paper following up on this work examines a well known supersymmetric model (Wess-Zumino) and finds evidence that supersymmetry can provide the desired suppression.

See hep-ph/0502106 and its cited preprints.

68. **Alejandro Rivero**
August 12, 2005

About that group blog… it is in principle possible to rent cheap web space with php scripting included, for instance my physcomments.org is lodged in aruba.it, a italian company, just to avoid local institution problems of the sort Woit insinuated above. Now, is there some interest on having such blog backed by a strong entity, as Chris seems to be suggesting?

Geometric Langlands on the Beach

August 11, 2005
Categories: Langlands

I've written a bit about the Geometric Langlands Program and its relation to physics here late last year, confessing to being confused about what it was supposed to have to do with N=4 supersymmetric Yang-Mills. Yesterday Witten gave a talk on the beach at the Simons workshop going on at Stony Brook. I've just finished listening to it, and it clarified things quite a bit for me.

Only having audio and no video is a bit frustrating, since not all the details of the equations get spoken, so sometimes you have to guess what the equation really is. In this case it's a bit charming since you get to listen to the seagulls, waves and kids playing on the beach in the background. Maybe at some point lecture notes will be posted, and presumably Witten is writing up a paper on this material that will appear sooner or later, at which point I'll try to get a better understanding of the details of this.

The idea seems to be to use a TQFT given by a twisted version of N=4 supersymmetric Yang-Mills, a slightly different one than the one studied by Vafa and Witten back in 1994. Then one does dimensional reduction using as 4-manifold a Riemann surface times the upper-half-plane, and ends up with a sigma model of maps from the upper-half-plane to the Hitchin moduli space of flat connections on the Riemann surface. The boundary degrees of freedom are branes, and the S-duality of the 4-d theory is supposed to give a duality at the level of the sigma model that corresponds to the fundamental duality one is trying to understand in the geometric Langlands program. The Hecke eigensheaves studied by mathematicians in this language are related to "magnetic eigenbranes". Witten makes use of Wilson and 't Hooft operators studied in this context by Kapustin, and also mentions some related purely mathematical results of my colleague Michael Thaddeus and his collaborator Tamas Hausel.

No Comments

Jean Dieudonne

August 11, 2005
Categories: Uncategorized

Pierre Cartier has written a short biographical article about the remarkable French mathematician Jean Dieudonné. Cartier estimates that Dieudonné wrote about 80,000 pages of mathematics over the course of his career. He was a driving force behind Bourbaki, often taking on the bulk of the writing tasks. With Alexandre Grothendieck he co-wrote EGA, the huge foundational text on algebraic geometry (some people note that, considering the French meaning of his name, this text could be described as "God-given").

Comments

1. **D R Lunsford**
   August 12, 2005

   I was told in such a way as to believe it, that one of those "new math" Bourbaki tomes boasted about the lack of diagrams and heuristics to be found within – as if to say, knowledge must be gained in some exquisite way in order for it to have value. This is the heart of the string debacle. Academics have become exquisites.

   -drl

2. **Alejandro Rivero**
   August 12, 2005

   Yep, Bourbaki in some introduction boasts a bit about this, ie about not using didactics as a substitute of proofs. It aims, as Streater/Wightman, for precision.

   I can not think about any string book similar in style (nor precision) to the ones of Bourbaki, so perhaps it is a bad comparison.

KITP Weblog

August 11, 2005
Categories: Uncategorized

The KITP program on Mathematical Structures in String Theory has a new weblog associated with it where Andrew Neitzke has been posting summaries of the talks given there. The idea of having such weblogs attached to programs like the one at the KITP seems to be an excellent one, it will be interesting to see how it works out.

Comments

1. **Chris W.**
   August 11, 2005

   FYI — I got a warning about an untrusted security certificate when accessing this site. (The URL's protocol is https.) I guess it's because the issuer is KITP, which is not a recognized certificate authority.

2. **Quantoken**
   August 12, 2005

   Chris:

   No no no. What you said is not true. I happen to know all the intimate details of HTTPS and have actually worked on a HTTPS based product that's being widely used. So let me explain what it is.

   The certificate your web broser received is not trusted because it is a root certificate (self signed certificate) sent over the wire. Which is WRONG way of implementation. Because any self signed certificate can pretty much be replaced by any machine in the middle, allowing easy man-in-the-middle attack and allow password etc be stolen.

   And it is not even a well formatted self-issued certificate!!! The certificate identity is not spelled correctly. The people who implemented this web site is a complete idiot. Even a dummy would know to buy a certificate from recognized RootCA, and do things in proper ways. This is the most bizzar HTTPS web site I ever see!!!

   Quantoken

3. **rof**
   August 12, 2005

   It's probably just because the site is new. A lot of sites use self-issued certificates during testing phases and only pay up to a third company later. I was at the KITP at the end of last year and their computer people seemed reasonably competent, although the wireless signal wasn't great.

4. **Quantoken**
   August 16, 2005

   Wrong again. See my latest comment on Lubos's blog.

   Quantoken

John Horgan's New York Times Op-Ed Piece

August 12, 2005
Categories: Uncategorized

Today's New York Times contains an Op-Ed piece by science writer John Horgan entitled In Defense of Common Sense. In it, Horgan takes an iconoclastic view of this year's many celebrations of the 100th anniversary of Einstein's great work of 1905, writing "In the midst of all this hoopla, I feel compelled to deplore one aspect of Einstein's legacy: the widespread belief that science and common sense are incompatible."

Horgan stirred up controversy in 1996 with the publication of his book The End of Science, where he claimed that most of the big discoveries in science have been made, forcing scientists to instead engage in what he calls "ironic science". By this he means science done in a "speculative post-empirical mode", something more like literary criticism, where claims are made that can never be shown to be right or wrong. While he saw this phenomenon taking place in many different areas of science, theoretical physics was where he had his strongest argument, pointing to controversies over the interpretation of quantum mechanics which seem to never be resolvable by experiment, and especially to string theory, which he describes in a later book as "science fiction in mathematical form." Publication of his book made him rather unpopular in the science community and ultimately led to his leaving his position as an editor at Scientific American. At the time I thought he somewhat overstated his case, especially in trying to see the same pattern in a wide range of different sciences, but he had the insight and courage to put his finger on something very important that was going on in physics. Since 1973, the field has been a victim of its own success, suffering greatly from the fact that the Standard Model is just so good that it has been impossible to find experimental results that disagree with it, as well as impossible to find any convincing improvement on the model that would address any of the issues it leaves open. Horgan's critique of string theory was forceful and on target, although to me his depiction of Witten seemed unnecessarily personally unkind.

I have mixed feelings about this latest piece of his, both strongly agreeing and strongly disagreeing with his defense of "common sense". This comes down to what one means by "common sense". One aspect of common sense is basically the standard scientific method and the norms traditionally used by scientists to evaluate what is good science and what isn't. The sub-headline on Horgan's piece "Beware of scientific theories that can't be tested" involves this aspect. It's just common sense to be skeptical of people who are making grandiose and radical claims unless they've got some good evidence for them, and string theory violates this notion of common sense. But I'm afraid Horgan conflates this kind of common sense with a different kind of "common sense", the common sense about how the physical world behaves that is built into us based on the evolution of our species and our growing up in an enviroment where we interact with the world on a very specific scale of distances. This kind of common sense may not help us at all to understand how nature behaves at the atomic scale, near a black hole, etc., instead quantum mechanics and relativity are required, and these are subjects that don't fit well with our notions of "common sense", in the second of the two meanings. But even if they were initially counter-intuitive, both quantum mechanics and relativity were based on a wide range of detailed experimental evidence, something that overcame people's qualms about whether they were violating "common sense". Absent this kind of evidence, string theory is a very different story….

On a completely different topic, Lubos Motl has a posting about amazon.com censoring any criticism of a certain crackpot physics book. He's started a contest, grand prize $3.00, which people may want to participate in.

Update: Lubos seems to have trouble telling apart John Horgan and John Hagelin….

Comments

1. **Nigel Cook**
   August 12, 2005

   'It's just common sense to be skeptical of people who are making grandiose and radical claims unless they've got some good evidence for them…'

   Eddington, who verified GR in 1919 wrote in his 1920 book Space Time and Gravitation, p. 152: 'The great stumbing-block for a philosophy which denies absolute space is the experimental detection of absolute rotation.'

   The best example of Einstein's problems was the twin's paradox, which is resolved by absolute acceleration, requiring general relativity, not special.

   It's the popularisations of relativity, based on out of date stuff, which discredit common sense. Or else they go with one 'interpretation' and quote Bohr (defending Copenhagen Interpretation wavefunction collapse) saying: 'Anyone who is not shocked by quantum theory has not understood it.'

   This means that commonsense approaches, like the simple mechanism for gravity that Feynman discussed in a 1965 BBC lecture (click my name), are not encouraged, even if they give more testable predictions than string theory, and are consistent with general relativity!

2. **GWB**
   August 12, 2005

   More and more pundits grab upon the oxymoronical phrase "common sense" to justify their weak arguments. Wisdom is valued precisely because, in the face of complexity, sense is never common. So when I read Hogan's Op/Ed this morning, I was turned off almost immediately by the use of the phrase "common sense". Advances in science must be arrived at through the scientific method. This isn't common sense, it's science.

3. **Arun**
   August 12, 2005

   "But I'm afraid Horgan conflates this kind of common sense with a different kind of "common sense??, the common sense about how the physical world behaves that is built into us based on the evolution of our species and our growing up in an environment where we interact with the world on a very specific scale of distances."

   I read Horgan differently. He's saying because scientists found that physics doesn't follow "common sense" in the above sense, scientists discard common sense altogether. That is, Horgan is saying that he recognizes the difference, while super string theorists, among other scientists, no longer do so.

   Namely, he says the following is the common exchange:

   Horgan: "Your theory is cannot be tested by experiment and so common sense leads me to say it is dead-end speculation"

   Scientist: "Quantum mechanics and relativity have taught us not to rely on common-sense".

4. **Nigel Cook**
   August 12, 2005

   String theorist: "Quantum mechanics and relativity have taught us not to rely on common-sense??.

5. **LM**
August 12, 2005

Ha! When I looked at Lubos's crackpot book on Amazon, it was paired with Weinberg's 'Dreams of a Final Theory'.

6. **rof**
August 12, 2005

"…a more convenient method of being defiant without any insight, viz., the appeal to common sense. It is indeed a great gift of God, to possess right, or (as they now call it) plain common sense. But this common sense must be shown practically, by well-considered and reasonable thoughts and words, not by appealing to it as an oracle, when no rational justification can be advanced. To appeal to common sense, when insight and science fail, and no sooner-this is one of the subtle discoveries of modern times, by means of which the most superficial ranter can safely enter the lists with the most thorough thinker, and hold his own. But as long as a particle of insight remains, no one would think of having recourse to this subterfuge. For what is it but an appeal to the opinion of the multitude, of whose applause the philosopher is ashamed, while the popular charlatan glories and confides in it?" – Immanuel Kant, Prolegomena

7. **Nigel Cook**
August 12, 2005

'Bohr was notorious for the obscurity of his writing. Yet … Bohr's obscurity is attributed, time and again, to a "depth and subtlety" that mere mortals are not equipped to comprehend. Perhaps disclosure of another editorial oversight will demonstrate my point. In a widely used compendium of papers on quantum theory, edited by John Wheeler and Wojciech Zurek, the pages of Bohr's reprinted article are out of order. This paper (Bohr's response to the famous 1935 Einstein-Podolsky-Rosen critique of the standard Copenhagen interpretation) is widely cited in contemporary literature by physicists and philosophers of science. Yet I have never heard anybody complain that something is wrong with Bohr's text in this volume. The mistake, it seems, is rarely noticed, even though it occurs in both the hard- and the soft-cover editions.

'When physicists failed to find meaning in Bohr's writings, no matter how hard they tried, they blamed themselves, not Bohr.'

– Mara Beller (Barbara Druss Dibner Professor in History and Philosophy of Science at the Hebrew University of Jerusalem, Israel):
http://www.mathematik.uni-muenchen.de/~bohmmech/BohmHome/sokalhoax.html

8. **Stephen**
August 12, 2005

"There is nothing new to be discovered in physics now, All that remains is more and more precise measurement."

http://zapatopi.net/kelvin/quotes.html

Note the date. Here we go again!

9. **Tony Smith**
August 13, 2005

Peter, you said:
"… quantum mechanics and relativity … don't fit well with our notions … of "common sense??, the common sense about how the physical world behaves that is built into us based on … our growing up in an enviroment where we interact with the world on a very specific scale of distances. …".

I disagree.
Quantum mechanics is absolutely required to explain such aspects of our environment on our scale as interference patterns from double slits, not to mention the radioactivity of glowing watch dials and the change of color (red to orange to yellow) of heated iron.
Relativity is absolutely required to explain such aspects of our environment on our scale as the constant speed of light.

The only difference between the concepts of QM and R and concepts recognized earlier in human history, such as a sphere, is that children were playing with balls long before humans did double slit and speed of light experiments.

In fact, I believe that QM and R could be taught to children very early if their parents and teachers would show them how to experience the relevant phenomena.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

10. **Nigel**
August 13, 2005

Tony: when they observe interference in the double slit experiment, how do you explain it? Young's explanation is that two light waves collide at a dark fringe, cancelling one another. Feynman says that nothing arrives at the dark fringes because the interference is caused by path interference with wave effects going through both slits even when just a single photon is used. So Young's interpretation is false and path integrals are right. This also disproves Copenhagen since they would have the wavefunction collapse when someone sees the light from the interference fringes, when in fact you have to account for the two slits affecting a single photon. I don't think Feynman used path integrals to explain the interference in his 1963 lectures, but he did after 1965.

11. **Arun**
August 13, 2005

Lubos Motl now has a post on John Horgan's essay; and it almost perfectly illustrates my point made a few posts above.

Horgan wrote :

"The strings (or membranes, or whatever) are too small to be discerned by any buildable instrument, and the parallel universes are too distant. Common sense thus persuades me that these avenues of speculation will turn out to be dead ends."

and as expected, to evade this, Lubos Motl goes into a paean of how well-accepted physics defies common sense (i.e., the physical intuition that everyday life gives us). He does not address the common sense idea that the reason we accept common-sense (of the physical intuition type) defying physics is because the experimental evidence is overwhelming; all that Horgan is pointing out is that string theory is deficient in the common sense idea of having experimental support.

IMO, Lubos Motl has also confused John Hagelin with John Horgan; and in general does everything he can, casting a wide net to examine all of John Horgan's beliefs, to avoid facing John Horgan's central point in this essay. The problem as always is that a person can be wrong on many things, but still right on one thing; the wrongness of all the other things does not provide any evidence on the one thing. This way of arguing is distasteful and resembles politicking more than scientific discourse. It resembles using Newton's theology to discredit his physics.

12. **Simon**
August 13, 2005

I agree with the points Arun just made. Can't think of anything to add.

13. **Chris W.**
August 13, 2005

On [Cosmic Variance]() Sean Carroll summarized some recent remarks of David Z. Albert which seem relevant here:

> After some hesitation, David decided to go, and thought very carefully about the talk he would give. I can't do justice to the precision with which he worded his presentation, but the basic message was essentially this: "When you are trying to figure out how the world works, there are two ways to proceed. One is to invent a story about Nature which serves to say something flattering about yourself. The other is to listen to the story that Nature itself tells, no matter what it may turn out to be. What you are doing is the former; science is the latter.??

Most appeals to common sense sound like self-flattery, or shallow appeals to conventional opinion. There is a core to common sense which is not self-flattery, and is also at the core of science: When you suppose some assertion to be true, ask yourself, "how could I check this, ie, how could I know if this statement was false?"

John Horgan is starting to sound like Gregg Easterbrook. When he says "I have also found common sense – ordinary, nonspecialized knowledge and judgment – to be indispensable for judging scientists' pronouncements, even, or especially, in the most esoteric fields" for what exactly does he take common sense to be indispensable?

14. **Alejandro Rivero**
August 13, 2005

"Common sense" is a buzzword in philosophy of the language, a science that usually gets interesting politic scents. It is not to be confused with "lore" or with "culture" (the "conventional opinion" Chris mentions), and it implies the ability to travel thought logic inferences. To put an extreme example, to swim across Euclid book should be considered an ability of common sense.

15. **Chris W.**
August 13, 2005

From Albert Einstein:

> The years of anxious searching in the dark, with their intense longing, their alternations of confidence and exhaustion and the final emergence into the light – only those who have experienced it can understand it.

This does not describe the experience of the confident, self-assured professional, or the ordinary man settled in his beliefs and secure in his application of common sense and generally accepted knowledge to the problems of living.

However, it describes the nature and depth of the problems currently confronting fundamental physics. Much (most?) of the general public is unimpressed and even repelled by such intellectual struggle, and prefers the calm effectiveness of competent professionals acting within their domains of expertise. Ask any physician, attorney, or business executive why they are often so anxious to present such a front, although the underlying realities of medical care, law, and business, not to mention many other fields, are quite different. The professionalization of science has put scientists in the same predicament.

16. **Peter Shor**
August 13, 2005

I'd like to defend common sense a little bit. Einstein's original derivation of the theory of relativity seems to me to use a lot of common sense (at least it uses intuitive reasoning of the type that somebody as smart as Einstein might consider common sense, as well as several decidedly non-intuitive already-verified facts about physics). And I suspect that people with enough experience in doing quantum field theory calculations can also make some progress using intuitive reasoning. If string theory is so far removed from our intuitive reasoning ability that we can't make head or tail of it, this may explain the current impasse in string theory.

17. **Luboš Motl**
August 13, 2005

Yes, I confused Horgan and Hagelin. That's painful but hopefully, others will understand that neither of these two Gentlemen is on the top list of my interests.

Sorry for the error and tell me if there are other errors.

18. **Arun**
August 14, 2005

Chris W. asked:

"John Horgan is starting to sound like Gregg Easterbrook. When he says "I have also found common sense – ordinary, nonspecialized knowledge and judgment – to be indispensable for judging scientists' pronouncements, even, or especially, in the most esoteric fields?? for what exactly does he take common sense to be indispensable?"

It would be nice to be able to ask John Horgan directly this question. The common sense as applied to the most esoteric fields is – where is the empirical evidence? – otherwise, those esoteric fields degenerate into theology. "I'm doing super-duper something which you ignorant rubes can know nothing about" is acceptable from a scientist only if said scientist can point to experiments that bear him out.

19. **finestructure137**
August 14, 2005

Hogan says"
"In the midst of all this hoopla, I feel compelled to deplore one aspect of Einstein's legacy: the widespread belief that science and common sense are incompatible."

Its preposterous to claim that this was Einstein's legacy. Horgan uses common sense to justify his argument while my common sense tells me that common sense is a term that doesnt have any definite meaning. If common sense is what is generally and appropriately called intelligence, it is the most important virtue of a scientist and no rational person can defy that in their study of nature. Nonetheless, one can acquire higher understanding and profound insight after persistence. If John Horgan is claiming that to be a bad thing then I must say that that he cannot really distinguish between science journalism and evolution of scientific ideas.
P.S I posted the same comment in Lubos's blog

20. **Andre**
    August 16, 2005

    You can read a response by Susskind on Edge.org.

21. **Alejandro Rivero**
    August 16, 2005

    A poor response. Susskind substitutes “common” by “intuitive”, or implies this translation in mouth of Horgan. Can anyone confirm or deny if Horgan does explicitly this equivalence? Susskind corrects himself two paragraphs below, after the main argument has been done under this equivalence.

    Worse, Suskind seems to tell that concepts as force and acceleration are in the intuitive range. (he uses the verbe “to grock”). I could buy about acceleration, but force?

22. **Alejandro Rivero**
    August 16, 2005

    On other hand it is a clue about what is happening with science. Instead of following mathematical and logical deductions, which is possible with common sense, they need to rewire towards the “uncommon sense” invoked in the title. No math anymore, finally the promised High Road that Archimedes denied us is available, just rewire yourself.

23. **Alejandro Rivero**
    August 17, 2005

    Oh my, finally I have got to read Horgan beyond the introduction and it is horrible, it changes his view of “common sense” two or three times along the article. And the best one he gets is from a notorious non-sensical defender of Darwin (poor Darwin gets bad defenders, that is politics).

Edgy Attacks on Horgan

August 16, 2005
Categories: Uncategorized

A commenter points out that the Edge web-site has put up John Horgan's recent New York Times Op-Ed piece about science and common sense, together with some quite hostile [responses to it](). I've already explained [what I think about Horgan's piece](), and I agree with some of the points of his critics, but I think their reaction to his quite accurate point that string theory is untestable is pretty remarkable.

John McCarthy, a computer scientists, writes the following bizarre paragraph:

"When Horgan says that string theory is untestable, he is ignoring even the popular science writing about string theory. This literature tells us that the current untestability of string theory is regarded by the string theorists as a blemish they hope to fix."

Ignoring the peculiar characterization of the untestability of a theory as a "blemish" rather than a serious problem, does this make any sense to anyone? McCarthy seems to be trying to make the argument that one isn't allowed to point out a problem with a scientific theory if the scientists involved agree it is a problem and say they wish they could do something about it.

McCarthy at least has figured out that string theory is currently untestable, unlike Lenny Susskind, who invokes the heavy artillery of big names to (seem to) claim that it is:

"Finally I must take exception to Horgan's claim that "no conceivable experiment can confirm the theories as most proponents reluctantly acknowledge." Here I speak from first hand knowledge. Many, if not all, of the most distinguished theoretical physicists in the world — Steven Weinberg, Edward Witten, John Schwarz, Joseph Polchinski, Nathan Seiberg, Juan Maldacena, David Gross, Savas Dimopoulos, Andrei Linde, Renata Kallosh, among many others, most certainly acknowledge no such thing. These physicists are full of ideas about how to test modern concepts — from superstrings in the sky to supersymmetry in the lab."

First of all, his parenthetical elaboration "" isn't quite right, Horgan never said anything about "cosmological eternal inflation", although he did criticize as untestable claims for the existence of "parallel universes". Susskind attacks Horgan's claim that string theory is untestable by claiming that he and lots of illustrious physicists have ideas about how to test "superstrings in the sky to supersymmetry in the lab". Note that Horgan never said anything about supersymmetry not being testable. The "superstrings in the sky" presumably refer to Polchinski's claims that in some of the infinite variety of possible string theory scenarios there are cosmic strings that might be observable. I fail to see how this counts as a "test" of string theory, since if, as is likely, astronomers don't see these things, that in no way shows that string theory is wrong.

After this piece of intellectual dishonesty, Susskind ends with the favorite tactic of string theorists on the losing side of an argument, the ad hominem attack:

"Instead of dyspeptically railing against what he plainly does not understand, Horgan would do better to take a few courses in algebra, calculus, quantum mechanics, and string theory."

Comments

1. **Simon**
   August 16, 2005

   I think there is something of a misunderstanding here. Maybe some string theorists claim that we can make testable predictions based on our current understanding of string theory, but some certainly acknowledge that at present we cannot.

   Perhaps I can explain why string theorists in the latter class react the way they do when some critics point out that there are currently no testable predictions. The reason is that this is a typical criticism of nonsense theories and religious explanations of phenomena – systems of thought where predictability is *not even acknowledged as a virtue*. String theorists are not such people, and understandably do not want to be associated with them; who wouldn't love to find a definite observational consequence of string theory?

   One may then decide (as you seem to have done Peter) that this wish is unlikely to be fulfilled, and theorists might better spend their time thinking about other approaches to particle physics if not quantum gravity. Maybe that's right …

2. **LM**
   August 16, 2005

   I think Lenny is now practicing what a prominent theorist once described to me as "Hollywood style physics".

3. **rrtucci**
   August 16, 2005

   Who cares about what is said on the Edge? I consider the Edge hilariously pompous, elitist trash, because it doesn't allow readers to post comments. That is THE WAY of the Internet, and any Internet publication that doesn't understand this, is certainly not on the Edge.

4. **D R Lunsford**
   August 16, 2005

   Well Susskind is behaving just like a scholastic philosopher. GSW is their *Summa Theologica*.

5. **Peter**
   August 16, 2005

   Hi Simon,

   In my experience most string theorists are well aware that they don't know how to make testable predictions, and that this is a big problem. I can understand this, as well as their getting upset if someone seems to be accusing them of not even being interested in having a testable theory. You're right, I happen to believe that the program they're pursuing isn't going to lead to testable predictions, so think people should give up and do other things, but it's a matter of judgement and reasonable people can disagree.

   But I have real trouble understanding the behavior of Susskind, Kaku, and some others, who bald-facedly deny the problem, and/or start twisting the meaning of what it means to make a scientific prediction to evade the problematic position they're in. Fundamentally I guess I think this is just dishonest, and shouldn't be tolerated in a scientific community.

6. **Tony Smith**
August 16, 2005

Peter, you say that you guess you think that "… the behavior of Susskind, Kaku, and some others … is just dishonest, and shouldn't be tolerated in a scientific community. …".

If you are correct (and I agree with you), then:
Does the fact that,
although they may be criticized internally as Lubos criticizes Lenny,
they are NOT disavowed by the conventional superstring community with respect to the general public and the politicians who provide funding,
imply that the conventional superstring community is NOT a scientific community, but is a political club whose purpose is to maintain its status quo of dominance of theoretical physics jobs and funding.

In turn, does that not imply that scientific methods, such as honest intellectual criticism of conventional superstring theory, are doomed to be ineffective in challenging the dominance of conventional superstring theory.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **The Original Simon**
August 16, 2005

Tony,
good analysis. indicates an important job for the press
(getting internal criticism out to general public and politicians)

8. **The Former (now Ex-) Simon**
August 17, 2005

Simon,

I'll take a guess that you are Simon Ross, welcome.
I hadn't realized you were posting here at Peter's blog.
Glad to hear your views.

9. **Peter**
August 17, 2005

Nope, not Simon Ross, a different Simon….

10. **Simon**
August 17, 2005

Well it's nice to be confused with a physicist as smart as Simon Ross!

Peter – I haven't followed Kaku's or Susskind's opinions on the future of string theory too closely. But I don't find Susskind's response to Horgan objectionable.

String theory is not a theory we understand well enough right now to make a prediction. I believe the physicists he lists are expressing their confidence that one day that will change and their ideas for how it might happen. The string theory they refer to is the hypothetical complete theory, rather than our present glimpse of it. One may not share their confidence in the existence of such a theory, or in our ability to find it.

As for Horgan, his argument against the testability of string theory is essentially that one can't do accelerator experiments at the relevant energy scales. Well this is really a bad argument. There are many ways even in QFT that observations far below the characteristic scale of the theory can tell you lots.

A spectacular example of this is the electroweak theory. The existence of strong interaction bound states allows one to observe beta decay without building an accelerator. From the kinematics one can deduce the existence of a massless neutral fermion, and then it's not such a big leap to the Fermi 4-point theory, which describes scattering very well at energies less than 300 Gev. At this energy, the theory violates unitarity, so one can wonder how to alter it so as to conserve probability. One starts adding particles with charges and couplings chosen appropriately. To be consistent with the success of the Fermi theory, one can show that you have to add a massive charged spin 1 particle to mediate the 4-point interaction (W). But then scattering of longitudinally polarized W's causes new unitarity violations, which require the introduction of at least 2 more particles. If one wants to add only 2, then there are just 2 options. One is a neutral massive spin 1 (Z) and a neutral scalar (Higgs). The other is a neutral fermion, and a neutral scalar. Picking the first option, one then chooses the couplings and masses to avoid unitarity violation in WWZ and vvZ scattering, and amazingly the result is the electroweak theory, with exactly the masses and couplings found from the spontaneously broken gauge symmetry analysis! The other option predicts the W mass to be about 50 GeV, which is ruled out again by experiments well below the electroweak scale.

I left out most of the details, but the argument is spelled out beautifully in a book called 'Introduction to Electroweak Unification: Standard Model from Tree Unitarity ' by J. Horejsi. Of course this isn't how the electroweak theory was found, but it does show that by thinking hard enough, one can sometimes learn about high energy physics from observations at lower energies. I imagine this is what people have in mind when they suggest that we can learn about string theory by looking at the CMB, or cosmic strings if they are found.

11. **Who**
August 17, 2005

I will be Who then.

(that is not a question)

12. **Who**
August 17, 2005

I liked Simon's statement very much and I thought that he might be a string theorist I could respect and trust. So I wanted to ask him a question.

for me a kind of test of a QG researcher is the quality of awareness and interest he or she shows in other alternative-QG lines of investigation.

A while back Aaron Bergman (in spite of the fact that I often dislike and disagree with what he says) impressed me positively because he was volunteering real interest in recent Ambjorn-Loll work in CDT type nonperturbative QG.

the kind of string theorist one wants to listen to is one who is not contemptuous and dismissive of other QG, especially (since string is perturbative) the background independent approaches. so many of them have some stock response.

So I wanted to ask this Simon (whether or not he is a real Simon and doing string, which is merely my guess) what QG alternative(s) to string do you find especially interesting or promising, if any?

13. **Simon**
August 17, 2005

Who – I'm sorry to disappoint you, but I'm just a lowly graduate student, and mainly I'm interested in the more mathematical side of string theory. I agree that one shouldn't be contemptuous or dismissive of other approaches to quantum gravity. But in all honesty I know very little about them. Perhaps someone can clarify for me why background independence is so important?

14. **Nigel**
August 17, 2005

'… what QG alternative(s) to string do you find especially interesting or promising, if any? '

What if nature turns out to appear boring or unpromising? What do you do then, if your expectations of an elegant or beautiful mathematical structure can't be fulfilled? If you look back, you see that nature is often unexpected. People didn't want evolution because it lacked elegance and beauty of creationism. If you don't know the answer in advance, can you afford to have prejudices?

An approach to QG which is the opposite of string theory, just a fluid Higgs pressure shielding mechanism, offers many testable (tested) predictions and solutions to other issues. It's amazing that people are so dismissive of alternatives to string theory.

Andrew M Wray, publishing editor of Classical and Quantum Gravity, wrote me a letter dated 14 July 1997 stating: '… we do not publish this type of article…' PRL, Nature, arXiv, and New Scientist didn't either, but Electronics World (60,000 circulation) and the cern server did 6 years later. Nobody in physics knows about it. The problem is, people were then and still are obsessed by superstrings.

Dr Bob Lambourne of the Open University wrote me on 23 April 1997: '… the ultimate "cause" of gravity is an ill-defined concept… Rather the issue should be "what kind of theory is best suited to describing a certain range of phenomena in broad conformity with our current knowledge…" At the present time such a discussion would probably focus on superstrings.'

Strings are therefore the only really rational way to even discuss quantum gravity. If you don't want to talk strings, you aren't serious. The physics community is sure about this for some reason. I won't bore you with my predictive, well tested model.

15. **Peter**
August 17, 2005

Hi Simon,

I agree that Horgan's argument, as stated, is too crude, or incomplete. His two sentences about the lack of testability

"My problem is that no conceivable experiment can confirm the theories, as most proponents reluctantly acknowledge."

and

"The strings (or membranes, or whatever) are too small to be discerned by any buildable instrument"

are both true, but the implication that the second sentence implies the first isn't correct. In Horgan's defense, the true situation here is complex enough that explaining it correctly to a general audience would take up the entire length of his Op-Ed piece. In this kind of very short piece, it's hard to not sweep a lot of complexity under the table.

Of course, if string theory really were a TOE, it should imply the full range of phenomena we see at low energies and you should be able to calculate them and compare these calculations to experiment. Horgan does ignore this point.

The true situation of course is that there is no well-defined theory, so one really can't calculate anything, at low energy or high energy. In the approximate versions of a theory that one has, one can do calculations of what the low energy excitations of the theory look like, and, basically, the problem is they come out wrong. I'll not here go into the long story of attempts to get the standard model out of string theory constructions, but the bottom line is that no one has gotten the full standard model in detail yet, no matter how complicated a construction they use.

Here's a more detailed version of what I take to be Horgan's argument, one that would avoid the incorrect implication in his simpler version:

"After more than twenty years of effort, no one has managed to come up with a consistent well-defined version of string theory that reproduces what is known about physics and makes further predictions that can be tested. One of the main reasons for this is that the conjectured fundamental excitations of the theory are too small to ever be directly measured, so we can't directly check the idea of higher dimensional fundamental excitations by going out and looking for them. String theorists have hoped that the existence of these higher dimensional excitations will have characteristic effects at low, observable energies, but so far this hasn't worked out at all. The most recent work on the theory implies that these hopes can never be realized, since the theory, if it makes sense at all, seems to have an infinite variety of possible low energy behaviors."

On the other hand, what Susskind writes is clearly designed to give the impression that a long list of distinguished people are full of ideas for feasible tests of string theory. This is simply untrue and Susskind knows it. You have to parse his words very carefully to see that he's not quite saying this. Again, I think this is just dishonest.

16. **Who**
August 17, 2005

Simon, thanks for your response. You say:
Who – I'm sorry to disappoint you, but I'm just a lowly graduate student, and mainly I'm interested in the more mathematical side of string theory… Perhaps someone can clarify for me why background independence is so important?

I would be happy if one or more others would discuss background independence. I will take a whack at it from the standpoint of a physics-watcher. There are several ways to respond, some of which you may find less persuasive than others. I won't try to fit the argument to the listener—I will just lay out several responses.

1. Gen Rel is background independent. So it is likely that any successful QUANTUM spacetime dynamics will also have to be.

2. Go read Smolin's recent paper "The Case for Background Independence" hep-th/0507235, which argues that it would help string theory resolve the landscape confusion and become more predictive if an effort were made to formulate it in a less background dependent way. I won't summarize his argument unless you ask. Ignore Motl's hostile paraphrase. Just read the introduction and conclusion sections of Smolin.

3. There are degrees of background independence. Gen Rel is B.I. in the sense that does not need a prior metric. (By contrast, stringy perturbative calculations require a prior metric to start from.) But Gen Rel is NOT as B.I. as the Triangulations approach that recently became prominent (Ambjorn-Loll, CDT, for instance hep-th/0505154) in the nonperturbative quantum gravity community.

Why? Because CDT does not assume that spacetime is a differentiable manifold, only that it can be APPROXIMATED using a path integral sum involving (triangulated) differentiable manifolds. This means that the continuum does not have to have uniform dimensionality at all scales.
It is not required have to have an atlas of coordinate charts. One can calculate in CDT and there are indications that it reproduces Gen Rel at large scale. But it DOES NOT ASSUME the manifold structure that Gen Rel assumes. Because it assumes less prior structure it is more background independent than General Relativity.

On the other hand, the various Stringy models, seem all to be LESS background independent than General Relativity. Indeed they seem on the whole to be PERTURBATIVE. So, well, maybe that does not interest you Simon but it is a stark contrast and, speaking personally, it gets my attention.

4. As a general rule, theories which assume less prior structure are on that account more predictive. If a theory is having trouble making unambiguous falsifiable predictions, one obvous helpful suggestion is to make it more background independent.

5. There ain't no absolute spacetime. Newton invented that idea (he called it the sensorium of God) and it prevailed since it is a technical convenience.

As soon as I post this I will think of something I said wrong or left out. But regardless of that, you are asking a good question and some sort of response is needed, so here is one to get started with.

yours truly,
Who

17. **Simon**
August 18, 2005

Peter – let me divide your (rather complimentary) paraphrase of Horgan's argument into two pieces. The first piece ends at "so far this hasn't worked out at all". The second piece is just the next sentence.

Imagine if in piece one, the expression "20 years" was changed to "3 months". I'm guessing you would no longer think the criticism was reasonable. It's really a judgement as to how long it's worth working on an approach that hasn't so far yielded concrete predictions. Presumably this depends on many other things, e.g. whether the theory has interesting mathematical spinoffs, whether other approaches are promising, etc. I don't want to get into those issues – I honestly have no idea how to decide what is the right amount of time to work on a theory like string theory. And I'm pretty sure Horgan has no sensible idea either. I don't find 20 years a shockingly long stretch of time.

The second piece concerns the landscape – the apparent plethora of consistent string backgrounds. You are right that it isn't too encouraging, and I think if it's correct (i.e. if these are indeed consistent string vacua) it could force us to change our idea of what we hope to get out of string theory. As far as I understand, this is the content of the 'east coast — west coast' debate. I don't understand this subject too well, but I don't see any reason to rush to the most pessimistic position.

To emphasize just how little is currently understood about the space of string vacua, note the tension between the statement that standard model-like vacua seem to be very difficult to find, and the statement that vastly many of them exist. To me this indicates that it's premature to ditch string theory because of the landscape.

By the way – I think I understand why Susskind tells Horgan to go take some classes. I suspect that people who say that extra dimensions are 'preposterous' would also think quantum mechanics and special relativity were preposterous if they understood them.

18. **Who**
August 18, 2005

Simon, glad to see you are still around! Have to leave (day at the ocean) in a moment, no time to read or respond to posts. However you asked about Background Independence issue earlier and I gave you my response—for contrast or whatever reason you might also like to glance at very different views on this at String Coffee, starting with a post by Robert Helling.
http://golem.ph.utexas.edu/string/archives/000621.html

Different notions of what background independence means arise there, defenses are thought of against Smolin's urging that string theorists work out a B.I. formulation. Concern is expressed that outsiders will be (naively or mistakenly) critical of string because of its background dependence, and so on. Interesting, if rather intricate, set of reactions to Smolin's hep-th/0507235.

19. **Scott**
August 18, 2005

Simom,

If you noticed Horgan did imply several times that QM and relativity are preposterous but that a wealth of experimental data makes that not matter. That was his main point that common sence reasoning should not be abandoned without good experimental reasons. The reason people started studying string theory was not because of some experimental result like the inability to discover motion throught the ether, the photoelectric effect, and the spectrum of blackbody radiation, but because the mathematical construct of string theory produced an unobserved massless spin 2 particle which previous failed theories to quantize gravity had also predicted to move particles along the geodesinc(or however its spelled) determined by the metric which the mathematical construct of string theory had no mechanicsm to produce. This slim "evidence" that string theory has something to do with nature has led to many "crazy" predictions of particles being one dimmensional loops, then the world having a bunch of extra curled up dimmensions and now that all sorts of branes exist. All of which, while possible, has no basis in the real world other then the fact that some people think the way to reconcile relativity and QM is to quantize gravity and string theory happens to contain a particle similar to the one usualy predicted when someone tries to do this and fails.

20. **Simon**
August 18, 2005

Who – thanks for the references. I'll do some reading!

21. **Peter**
August 18, 2005

Simon,

You're right that if people had only been working on string theory for 3 months no one would be complaining about its present state. Personally I waited about 16 years before starting to publicly complain about what was going on. But, sorry, 20 years (actually 21 and counting since the "First Superstring Revolution") is a shockingly long time for thousands of the smartest people to work on a speculative idea, given that not only have they not made any progress on using it to come up with a TOE, but they're much farther from their goal than when they started. I don't think anything remotely like this has ever happened in the history of physics.

If the landscape is correct it could "force us to change our idea of what we hope to get out of string theory"? How about calling a spade a spade and changing that to "force us to admit this idea has failed completely and we have to do something else"? One of the most annoying things about the way many string theorists express themselves is that they act as if it is completely inconceivable that string theory as a TOE is simply a wrong idea that will need to be completely abandoned.

Susskind's telling someone who was a senior writer at Scientific American for many years that he should go take a high school algebra class is just a stupid, juvenile insult, one that I recognize all too well from personal experience as a standard kind of thing some string theorists say when challenged about the problems of string theory.

22. **Simon**
August 18, 2005

Peter – I don't think it's really fair to compare current research to previous research in physics. It's not the same problem, and it's not such a surprise to me that as experiments at higher energies become more and more expensive, theoretical progress slows down too. I have a vague recollection that this slow-down of theory was even predicted by Feynman.

I agree with you that it could turn out that when we fully understand the landscape (if it exists), we'll have to give up on string theory as a TOE, but I have no idea if that's how things will go. In the meantime, I'm glad some people are trying to figure out if that's the situation.

I'm sorry you've had unpleasant experiences dealing with some string theorists. The reason I defended Susskind a little is that I also found Horgan's article obnoxious. Not because of anything he said about string theory though. Rather it's because he rather deviously uses 'common sense' as a rhetorical device when lost for a logical argument. As you said above, he makes a couple of statements about string theory which are true if interpreted charitably, but the implied logical links between them are absent. That works for me as a description of the whole article. It's just not good writing.

It's also arrogant and self-aggrandizing:
"Needless to say, I reject that position, and not only because I'm a science journalist (who majored in English). I have also found common sense — ordinary, nonspecialized knowledge and judgment — to be indispensable for judging scientists' pronouncements, even, or especially, in the most esoteric fields."

23. **Scott**
August 18, 2005

Simon I am not sure what you find arrogant and self-aggrandizing in saying that ordinary, nonspecialized knowledge and judgement (which is what he defined common sense as) is indespensible in judging various ideas even in esoteric fields. He goes on tell which parts of ordinary knowledge and judgement(common sense) are usefull, namely that things involving how humans behave are unlikely to be dirrect effects of a single cause and that extroardinary claims should require extraordinary proof(expirmental results).

24. **Dr. Cal**
August 21, 2005

Hollywood Physics

http://physicsmathforums.com/showthread.php?p=403#post403

FOR IMMEDIATE RELEASE:

ALL TIED UP & STRUNG ALONG, a movie about String Theorists and their expansive theories which extend human ignorance, pomposity, and frailty into higher dimensions, is set to start filming this fall. Jessica Alba, John Cleese, Eugene Levie, Jackie Chan, and David Duchovney of X-files fame have all signed on to the $700 million Hollywood project, which is still cheaper than String Theory itself, and will likely displace less physicists from the academy.

"As contemporary physics is about money, hype, mythology, and chicks," Ed Witten explained from his offices at the Princeton Institute for Advanced Study, "The next logical step was Hollywood, although I thought Burt Reynolds should play me instead of Eugene Levy."

Brian Greene, the famous String Theorist who will be played by David "the truth is out there" Duchovney, explained the plot: "String theory's muddled, contorted theories that lack postulates, laws, and experimentally-verified equations have Einstein spinning so fast in his grave that it creates a black hole. In order to save the world, we String Theorists have to stop reformulating String Theory faster than the speed of light. We are called upon to stop violating the conservation of energy by mining higher dimensions to publish more BS than can accounted for with the Big Bang alone, and I win the Nobel prize for showing that M-Theory is in fact the dark matter it has been searching for."

Greene continues: "At first my character is reluctant to stop theorizing and start postulating, but when my love interest Jessica Alba is sucked into the black hole, I search my soul and find Paul Davies there, played by John Cleese. I ask him what he's doing in my soul, and he explains that the answer is contained in the mind of God, which only he is privy too, but for a small fee, some tax and tuition dollars, a couple grants here and there, and an all-expense-paid book tour with stops in Zurich and Honolulu, he can let me in on it. And he shows me God in all her greater glory, as he points out that we can make more money in Hollywood than writing coffee-table books that recycle Einstein, Bohr, Dirac, Feynman, and Wheeler. I am quickly converted, and I agree to turn my back on String Theory's hoax and save Jessica Alba."

But it's not that easy, as standing in Greene's way is Michio "king of pop-theory-hipster-irony-the-theory-of-everything-or-anything-made-you-read-this" Kaku, played by Jackie Chan. Kaku beats the crap out of Greene for alomst blowing the "ironic" pretense his salary, benefits, and all-expense paid trips depend on. "WE MUST HOLD BACK THE YOUNG SCIENTISTS WITH OUR NON-THEORIES!! WE MUST FILL THE ACADEMY WITH THE POMO DARK MATTER THAT IS STRING THEORY TO KEEP OUR UNIVERSE FROM FLYING APART, OUR PYRAMID SCHEMES FROM TOPPLING, AND OUR PERPETUAL-MOTION NSF MONEY MACHINE FROM STOPPING!!" Kaku argues as he delivers a flying back-kick, "There can be ony ONE! I WILL be String Theory's GODFATHER as referenced on my web page!! I have better hair!"

But Greene fights back as he signs his seventeenth book deal to make the hand-waving incoherence of String Theory accessible to the South Park generation, senior citizens, and starving chirldren around the world. "Kaku! Kaku! (pronounced Ka-Kaw! Ka-Kaw! like Owen Wilson did in Bottle Rocket)," Greene shouts. "It is theoretically impossible to build a coffee tables strong enough to support any more coffee-table physics books!!!"

"Time travel is also theoretically impossible, but there's a helluva lot more money for us in flushing physics down a wormhole. Nobody knows what the #&#%&$ M stands for in M theory ya hand-waving, TV-hogging crank!!! Get it?? Ha Ha Ha! We're laughing at the public! We're the insider pomo hipsters! Get with the gangsta-wanksta-pranksta CRANKSTER bling-bling program!!"

How does it all end? Does physics go bankrupt funding theories that have expanded our ignorance from four dimensions into ten, twenty, and thirty dimensions? Do tax payers revolt? Do young physicists overthrow the hand-waving, contortionist bullies and revive physics with a classical renaissance favoring logic, reason, and Truth over meaningless mathematical abstractions? Does Moving Dimensions Theory (MDT) prevail with its simple postulate? We'll all just have to wait!

But in the meantime, how do you think it will play out?

Will theories with postulates ever be allowed in physics again? Or will the well-funded, tenured pomo String Theory / M-Theory (Maffia-Theory) Priests send their armies of desperate, snarky postdocs and starving graduate students forth to displace and destroy all common sense, logic, reason, and physics in the academy? It must be so–for the greater good of physics, the individual physicist, and thus physics, must be sacrificed.

http://physicsmathforums.com/showthread.php?p=403#post403

25. **Suresh K Maran**
August 23, 2005

If you guys want to talk about quantum gravity please drop by blog (click on my name). I just started the blog recently and learning to keep it steady. (I deleted some comments left by some of you accidently-sorry)

Electric Dipole Moments

August 16, 2005
Categories: Uncategorized

Chad Orzel has interesting posts here and here about electric dipole moment experiments and their implications for particle physics. He claims that these experiments will ultimately be capable of getting down to three to four orders of magnitude below the current limits, and since they already put constraints on beyond the standard model physics, these results could be very significant.

Comments

1. **Thomas Larsson**
   August 17, 2005

   There was an article about "The Search for a Permanent Electric Dipole Moment" in the June 2003 issue of Physics Today. It is available online here, but you need to be able to log in. The results seem already to be difficult to reconcile with SUSY.

2. **Not a Nobel Laureate**
   August 21, 2005

   Is it an accident or a reflection of the field that the one post by Dr. Woit referring to actual experiments attempting to probe "beyond the Standard Model" generated the least comments.

3. **Peter**
   August 21, 2005

   I do try and write a significant number of posts about experiments relevant to particle physics, but you're right that these generally don't attract many comments, unlike the ones about the string theory controversy. However, one recent post did get even fewer comments, the one about Witten's talk on geometric Langlands. And that one was definitely not about something relevant to experiment….

4. **D R Lunsford**
   August 22, 2005

   Well I was going to comment, so now I will!

   One can think of the magnetic field as the "small" correction to the electric field required by Lorentz invariance and necessary for propagation. Likewise, one can think of the "bottom half" of the Dirac spinor in the std rep (which encodes the idea of antimatter) as the "small" correction to the Pauli 2-spinor required by Lorentz invariance and the necessity of propagation of the Dirac field (Klein paradox).

   The "small" part of the Maxwell field has no direct sources (monopoles) and so the lowest order manifestation is the dipole. At low energy it is possible to treat magnetic dipolar phenomena almost independently. Thus, the illusion of two theories, electrostatics and magnetostatics. What is the analogy in the Dirac theory? You can think about this for a while. (It should be clear that there is *no* actual separation of the world into matter and antimatter independently.)

   -drl

Snowmass Workshops

August 18, 2005
Categories: Uncategorized

This week and next there are workshops at Snowmass on the particle and accelerator physics aspects of the proposed International Linear Collider (ILC). There's a new weekly newsletter and a new website devoted to the ILC project which has twice-daily updates from the Snowmass workshops. Kind of like blogging, except done by professionals. Soon every conference or workshop will have their official blogger (two ongoing mathematical physics ones that link to blogs on their website are at the KITP and at Oberwolfach. I really should write more about twisted K-theory here sometime….)

For more about plans for the ILC, and for a presentation about CERN's plans for the future, see the talks from the EPP2010 meeting at Cornell earlier this month.

In other particle physics news, the RSVP project at Brookhaven has been terminated.

Update: There's another collider physics workshop going on in the Colorado mountains, this one is at Aspen and is concentrating on LHC physics.

Comments

1. **Quantoken**
   August 19, 2005

   Peter:
   This is an academic **scandal** related to some in the Math department of Columbia University. Since you are at Columbia, you must know the persons involved, and probably know about this incident, too. Would you like to comment on it? True? False? Your opinion?

   http://www.stat.rutgers.edu/~shepp/smm.pdf

   Quantoken

2. **Urs**
   August 19, 2005

   > I really should write more about twisted K-theory here sometime….)

   I'd enjoy reasding it.

   BTW, I was wondering about the following, but couldn't find the answer here in Oberwolfach:

   Given a space X, its K-theory is the set of homotopy equivalence classes from X to the space of Fredholm operators [Map(X,Fred)].

   One gets twisted K-theory from that by allowing these maps to really be sections of a Fredholm-*bundle* E associated to a PU(H)-bundle, [Gamma(X,E)].

   But noting that a map is just section of a -1 gerbe and a bundle-section is really the section of a 0-gerbe, it almost seems like there is a pattern emerging here.

   What would we obtain if we considered homotopy classes of sections of a Fredholm-structure associated to a PU(H)-*(1-)gerbe* (or a PU(H)-n-gerbe)?

   (Of course I am aware that it might not be totally obvoious what an associated gerbe is supposed to be.)

3. **Chris Oakley**
   August 19, 2005

   Re: the academic scandal – Ma seems to have failed to acknowledge that a graduate student has no status and that putting up with shabby or perceived shabby treatment by one's supervisor is part of the deal. Of course, when he threatened them with a lawsuit, they just closed ranks. What did he expect?

4. **Peter**
   August 19, 2005

   Quantoken and Chris,

   This is completely off-topic and I feel highly constrained in commenting in a public forum about something that has been the subject of a court case, since the last thing in the world I would want to do is to stir up renewed litigation over this issue. So I won't say anything about Shepp or Ma other than that my personal opinion, knowing most of the facts of the case, is that they're the ones in the wrong here.

   I can comment in more detail about Phong, who I know quite well and whose interactions with his students I've seen quite a lot of over the more than fifteen years I've been at Columbia. Phong is one of the best advisors of Ph. D. students our department has, one could easily make the case that he is the best we have. He works extremely hard at advising students, and is incredibly generous to them with his time and his ideas. He has had very many students, quite a few who have gone on to successful careers. He's a very straight-forward guy, and the accusations made against him are highly implausible if you know him and the student involved.

   The department did look into Ma's accusations carefully, and the people who did this are also people I know well and who have high ethical standards. Repeated attempts were made to find some way for Ma to complete a Ph.D. here.

   I'm afraid that's all I can say here about this story. Please don't post anymore about it here. It is off-topic, and I find it very frustrating not to be able to say what I think about the details of this and the people involved.

5. **Peter**
   August 19, 2005

   Hi Urs,

   Sorry, but I can't help you with your question, since I haven't thought much at all about the abstract point of view that you're asking about. The cases I've found twisted K-theory and gerbes useful involve specific equivariant projective vector bundles, not the abstract definition you ask about.

Peter

6. **Nigel**
August 21, 2005

String theorist Lubos Motl has a new post on the beauty of using maths to fight cancer: http://motls.blogspot.com/2005/08/beauty-of-math-vs-cancer.html

It makes a lot more sense to describe useful and potentially beneficial uses of maths with emotional words, than the usual propaganda of calling useless string theory elegant/beautiful. String theorists could become cancer theorists and actually do something useful. Hopefully they will when they finally grasp that strings are a dead end, wasting time and money! It is terribly tragic that the finest brainpower in the world has been wasted for over 20 years.

7. **Luboš Motl**
August 21, 2005

Dear Nigel,

let me admit that the word "beauty" was mostly referring to something else that you had to miss.

Of course I think that the beauty of string theory is much more striking than any beauty that can be found in the models you mention; on the other hand, these models may be more useful for practical applications.

All the best
Luboš

8. **Nigel**
August 21, 2005

Dear Luboš,

I am glad you are honest and admit that work in other areas are more useful for practical applications than string theory.

If beauty lies in the eye of the beholder then you are entitled to see strings like that. But my teachers told me the arts are subjective, since beauty is not really a testable scientific concept. This is because everyone sees beauty differently. But science is different, being based on quantitative facts, and no theory is worth anything until it has after survived experimental tests. This is why extra dimensions/strings cannot be viewed as science. There are other possibilities out there which are not treated as serious science because of the excessive attention on strings. Crackpot theories are crowding out some genuine alternatives to string theory because string theory itself is so hard to distinguish from a crackpot theory. One positive claim about string theory is that it has brilliant people working on it: but if you look back to the 19th century, many brilliant people worked on Kelvin's theory of vortex atoms, which turned out to be a dead end. So mistakes happen.

Best wishes,
Nigel

9. **Luboš Motl**
August 21, 2005

Dear Nigel,

it may be difficult for you to distinguish string theory from a crackpot theory. But one should notice that it is difficult to distinguish any correct theory from any wrong theory for anyone who simply does not have a sufficient intellectual capacity to understand the relevant questions.

So unless you're Nigel Hitchin, I highly recommend you to think about the implications of your ignorance. If you will think enough, one of your conclusions will be that you should be silent – or at least modest – about things that you have no chance to understand.

Best
Luboš

10. **Nigel**
August 22, 2005

Dear Luboš,

I'm not saying mathematics is not beautiful, just that it is ugly for you to keep sounding off emotionally about string theory. Science is fragmenting because most professors are locked into one particular idea which they defend emotionally like a girlfriend!

(Is string theory really more 'beautiful' than Franziska Michor, who you interviewed about the maths of cancer?) Applying the term 'beauty' to useless string theory shows a corruption of science.

If string theory is not tangible or even testable. It is like ESP and UFOs or aliens. To use propaganda like 'beauty' is just missing the point: beautiful equations can be wrong.

Please be aware I studied QM and general relativity and the beauty of those theories occurs where they have been checked by experimental tests. Einstein's views on cosmology were disproved experimentally, and his fault was pushing general relativity too far, by adding unobservables to make it model his prejudiced view of a static universe.

Luboš, I do not understand why string leaders become dictators of 'beautiful' speculative science, but that ignorance on my part is no reason to be silent. Any dictator can say his critics are ignorant of the hidden dimensions of his philosophy. Really, it is the not the critics who are really the ignorant ones. I've done some research and string theorists will not be objective enough to read it and let it be published and debated properly. It might be just as well, because they are so emotional about strings they aren't objective.

Best wishes,
Nigel

11. **Nigel**
August 22, 2005

http://news.bbc.co.uk/1/hi/education/3580742.stm :
'There are more pupils taking A-level psychology than physics – and if current trends continue, the declining science subject will be overtaken by sociology.'

I wrote about the fall in A-level physics uptake in Britain in the editorial/opinion piece for Electronics World, Oct 03. It coincides with the rise of superstring speculation at the top. Teachers can't do anything because nobody at the top listens, they say all critics are ignorant. At that time, the physics department of my local university, Essex, had just closed because there were too few students. All the string theorists who wrote letters in said (rudely) that Kaku's popular string theory book sold well, so I was ignorant. But that book had no maths or physics and may have been selling to people who also like crop circles and the paranormal, and whenever pushed, Kaku tries to use Dirac's tested equation – which is so beautiful he says it made him cry when he first saw it – to defend strings (entirely different). To have a dead end untestable theory at the top of physics sends out the wrong message to students: the whole subject seems like a dead end.

12. **Lubos Motl**
August 22, 2005

Dear Nigel,

string theory is beautiful in a different way than Franziska Michor – although it may be less different than one would a priori think. 😉 I've already written some texts about the beauty of string theory but let me add a few words.

String theory is beautiful because it makes the union between a diverse family of mathematical concepts and theorems inevitable. It is beautiful because all the potential inconsistencies always evolve in the right way so that all problems eventually disappear; something like that can't happen in a generic theory that would use the same or similar fragments to do calculations.

It is beautiful because if one studies an extreme limit, any potential singularity is always smeared out and fixed by a new kind of effect, new massless modes – much like if one follows the ideal curves of an ideal lady. 😉 It is beautiful because it unifies all good ideas in physics and all good ideas are naturally melting into each other in its framework. It is beautiful because it has been the generator of new striking physical paradigms such as holography. It is beautiful because it is so incredibly unique. Your comparisons with the other "beautiful but wrong" theories are completely ridiculous. The physicists have never had a theory as beautiful as string theory, and the only thing that you may show by disagreeing with it is your ignorance.

One may sometimes be confused what a beauty of physical theories means. But if one is not confused, then it's almost true that so beautiful theories just can't be wrong. Your attempts to make your opinions about string theory relevant – although, as you say, your knowledge does not go beyond elementary QM and GR – are completely ludicrous.

Thanks for your understanding
Lubos

13. **Nigel**
August 22, 2005

Dear Luboš,

Holography was invented by Dennis Gabor in 1947 and was not generated by ST. You are being misleading. Similarly, Witten in 1997 claimed ST 'predicts gravity', which is vacuous. ST doesn't give us testable predictions like GR. Everyone knows that any real success of ST would be in the news, and it is not happening.

To give a specific example, ST cannot give any information on the strengths of fundamental forces. I've seen defensive, arm-waving talk that gravity must be weaker than electromagnetism because it is spread over 10 dimensions not 4, but this vague argument gives no quantitative prediction. The earliest use of extra dimensions in GR in the 1920s spilled out solutions which seemed to predict Maxwell's equations (in tensor form) and give a unified electromagnetism-gravitational theory, but this turned out to be useless. Any unification requires an explanation of why the forces are different. The difference in strengths of weak nuclear and electromagnetism for instance is predictable from electro-weak theory, which is why that is a tested scientific theory.

ST shows no signs of being on the right lines. It has not a single tested prediction. Luboš, you are being misleading when you say: 'It is beautiful because it unifies all good ideas in physics and all good ideas are naturally melting into each other in its framework.'

This is the sort of emotional trash you find being used by crackpots to defend nonsense. ST doesn't unify anything properly, let alone everything. Quantitative tests are required to claim unification.

It is therefore fact, not my ignorant 'ludicrous' opinion, that ST is untested speculation. GR with a fluid source (Higgs field) and viewing Hubble's law as a spacetime recession (not just recession with distance but as time past, since both are equivalent in spacetime we observe) predicts gravity quantitatively using Feynman's pressure shielding mechanism. You demonstrate a love for ST, but that just makes it seem more crazy to most students, who see nothing but a love for mathematics detached from reality.

Best wishes,
Nigel

14. **woit**
August 22, 2005

NIgel and Lubos,
Stop using this place to carry on this kind of argument that leads nowhere. I'll delete any further similar comments here.

WMAP Status

August 22, 2005
Categories: Uncategorized

The WMAP mission has now been in place and taking data near the L2 Lagrange point for four years, with two more years still to go. Spectacular results from the analysis of the first year's worth of data were reported in Feb. 2003, and the second year's data was initially supposed to appear a year later, in Feb. 2004, but they're now a year and a half late. For some reporting on this, see this site with cosmology news. Just recently the WMAP team has put up something new on their mission status page, where they state:

While the first-year results were based mainly on temperature measurements, the continued mission operations are now primarily focused on the much weaker polarized signals – an invaluable "stretch" goal of the extended mission. Analyses of these weaker signals are more difficult and continue with steady progress. The data and results will be provided as soon as calibration and systematic error analyses have been completed, and the data files have been adequately documented for use by researchers.

Are they seeing the effects of gravitational waves in this polarization data? Anyone with inside information want to take advantage of the ability to post here anonymously and tell us what is going on? Or e-mail me, I promise to protect the confidentiality of my sources, even going to jail with Judith Miller if necessary.

Comments

1. **D R Lunsford**
   August 22, 2005

   Why are they "keeping secrets"? Cosmologists just can't be trusted 🙂

   I'm assuming that light and gravity are related, and the effects are galaxy-local and solar-system local, and superimposed.

   -drl

2. **WMAP boss**
   August 22, 2005

   Let me use the anonymous character of this forum. We've looked at the polarization and gravitational waves data in detail and they perfectly confirm the string-theoretical scenarios of inflation. In fact, when we read the data carefully, they say "the opponents of string theory are incredible morons".

   We are a very careful team so we must still double-check all of our procedures and calculations before we publish the results. 😉

3. **woit**
   August 22, 2005

   OK, Lubos, thanks for the inside information. I'll carefully protect your anonymity….

4. **WMAP boss**
   August 22, 2005

   Thanks for giving me an alias and for protecting my anonymity, and happy wishful thinking!

5. **D R Lunsford**
   August 22, 2005

   It's obviously dipole moments associated with the various planes determined by life on Earth. It's the anthropic principle for real!

   -drl

6. **Fyodor Uckoff**
   August 22, 2005

   Jokes aside, it is simply incredible folly for the WMAP people to carry on like this. Far from establishing their credibility, they are undermining the credibility of the 2003 data release. Way to go guys. Not.

7. **D R Lunsford**
   August 23, 2005

   If there were simply a correlation with the ecliptic one would have reason to doubt their methods. But correlation with both the ecliptic and galactic planes makes one believe they are afraid to release the data because the Big Bang Inflator priesthood would be offended and they would be shown the instruments of torture.

   "E pur sono correlato."

   -drl

8. **Quantoken**
   August 23, 2005

   Obviously Peter is more interested in crackpot theories that explain the microwave radiation background of the milky way galaxy using gravitational waves, rather than the obvious fact that why some of the WMAP team members had to leave. There is no way they would allow release of data that could challenge the orthodox theory, nor would Peter tolerate it either.

   It's the natural **local** background, stupid!

   Quantoken

9. **woit**

August 23, 2005

I have no idea why some people are posting comments here about members of the WMAP being forced to leave or being unwilling to release data because it disagrees with one theory or another. If anyone has specific information about WMAP scientists being forced out, that would be interesting, but otherwise, stop repeating nonsense.

As for whether these scientists are willing to release data that doesn't agree with a theory, remember that they aren't theorists, so have nothing invested in any particular theory. If their data could kill the standard consensus cosmological model, I think they would be elated, since this would surely win them a Nobel prize. But before releasing this data, they might want to take the time to check carefully for other explanations.

I have no idea whether things are taking so long because the WMAP people are seeing something unexpected and exciting in the data, or whether they aren't and it is just a lot of work to carefully analyze the possible systematic errors in their results before releasing them. If anyone actually knows anything about this, let's hear from them. Completely uninformed attacks on the scientific ethics of the WMAP scientists will be deleted.

10. **D R Lunsford**
August 23, 2005

Two words: open source.

-drl

11. **mikejones**
August 27, 2005

The lack of openness about what is going on with the WMAP project is a problem. To their credit, the WMAP team gave some explanation for the delay. I'd be even happier if they gave even a rough time table for the next data release, however. Many other space science projects give weekly or at least monthly status reports. Take the other big player in cosmology these days, the SDSS; the SDSS team announce when new data releases will take place and the previous releases have been on time.

The WMAP satellite has finished taking all four years of observations as of this August. There is even talk of an extension of the mission for another four years. I predict that the reason for the delay is two-fold: (i) there is much more data to be analyzed, and (ii) the amplitude of the polarization signal is ~10% of the temperature signal. I am a fan of the standard cosmological model. After years of being labeled a branch of metaphysics (cosmology) it is nice to see that humanity is getting gaining a substantive understading of the size, shape, and evolution of the universe. If I had to bet, I'd put my money on the next WMAP release providing us with a stunning and resounding confirmation of LCDM.

12. **Kea**
August 27, 2005

"If I had to bet, I'd put my money on the next WMAP release providing us with a stunning and resounding confirmation of LCDM."

How much money?

13. **woit**
August 27, 2005

By the way, just to show that I'm not much of a cosmologist, what does "LCDM" stand for?

14. **Quantum_Ranger**
August 28, 2005

Just to throw a spanner in the workings and terminology:
http://www.roe.ac.uk/japwww/pust/cargese03/sld082.htm

Not Even Wrong: The Book

August 23, 2005
Categories: Favorite Old Posts, Not Even Wrong: The Book

There's a project I've been working on for the last couple years that I haven't wanted to write about here until it was further along, but now seems to be a good time. I've written a book, also entitled "Not Even Wrong", and the British publisher Jonathan Cape is bringing it out in England, publication date March 16th from what I last heard. It will presumably appear later in the U.S., with the publisher here still to be arranged. Right now I'm putting some final touches on the manuscript, and hope to have a final version within the next week or so. You can take a look at the latest version of the cover art, and someone last night wrote to tell me that Random House in Canada has a catalog entry for the book.

The book contains material on several related topics, including a history of the standard model from a mathematically-informed perspective, a description of the history, current status and prospects of high energy accelerators and particle physics experiments, some of the history of recent interactions between mathematics and physics, a history of supersymmetry and string theory and attempts to use them to get beyond the standard model, comments on the notion of "beauty" in theoretical physics and on the sociology of how particle physics is pursued and supported, especially in the U. S.. There's also a section explaining exactly what the problems with supersymmetry and string theory are, making the case that these are ideas that have failed conclusively, together with an explanation of what the whole "landscape" controversy is about.

The story of how the book came to be is roughly as follows. I started writing it in 2002, and had something pretty well finished by the end of that year. Early in 2003 an editor from Cambridge University Press heard about what I was writing and stopped by to see me when he was visiting Columbia. He got interested in the idea of having Cambridge publish the book, but I think he had no idea of how controversial this topic was. During 2003 the manuscript went through a couple iterations of refereeing at Cambridge. The first round of referee reports included a very positive report from a non-string theory particle theorist, a non-committal report from a mathematician who works on things related to string theory, and an extremely negative report from a string theorist.

I'd been quite curious to see how a string theorist referee would respond to the manuscript, since I was pretty sure all my facts were right, and I assumed that they would have trouble recommending against publication of something without being able to show that it said something incorrect. This first string theorist referee was described to me as a "well-known mainstream string theorist". He or she dealt with the problem of not being able to find anything wrong with what I had written by claiming that arguing against string theory was like arguing against teaching evolution, and that "I think that you would be very hardpressed to find anybody who would say anything positive about this manuscript", using this as an excuse for only coming up with one example of something incorrect in the manuscript. By now I'm pretty used to the tactic that was used to do this, but at the time I was pretty shocked by it. A sentence I had written was taken out of context and one of the words was changed from a singular to a plural, allowing the referee to construe the sentence in a way that allowed him or her to claim I wasn't aware of some important developments in physics.

This experience convinced me that at least some string theorists were in far worse shape than I had imagined, suffering from the delusion that no one who knows what they are talking about could possibly criticize string theory, and willing to stoop to pathetic levels of dishonesty to maintain this point of view. I had off and on been worried that I was being too harsh in some of my criticisms of the behavior of string theorists, but after seeing this report I stopped worrying about this.

The Cambridge editor seemed to believe that the negative referee report lacked credibility, and that it even gave some evidence for the problems I was claiming existed in the string theory community. But for Cambridge to publish a book, a board of academics who act as advisors have to sign off on any decision. The editor felt that this round of referee's reports would not be enough to convince them, so the manuscript was sent out to two more referees, both theorists who have worked on string theory. It took quite a while for these reports to come back, and when they did, one of them was very positive and recommended publication. The second however was quite negative. This referee found nothing inaccurate to complain about, but said that while he or she agreed with many of my critical comments about string theory, basically string theorists were the ones who should be evaluating the theory, and Cambridge shouldn't be publishing the opinions of the likes of me. I couldn't really disagree with this; string theorists are the ones who should be critically evaluating what has happened in the field, but the problem is that they're not doing it.

At this point the editor still felt that he would have trouble getting approval to publish the book, and offered to try another round of referees, but this seemed to me a waste of time. String theorist referees were clearly willing to strongly recommend against publication even when they couldn't point to anything inaccurate in the book, and the way the Press works, it was unlikely to publish something over the strong objections of some very prominent people. I then circulated the manuscript to editors at several other university presses. Two of them wrote back that while they found the book very interesting and well-written, a university press just could not publish something so controversial.

A friend of mine then put me in touch with a prominent New York literary agent. Her advice was that, if the manuscript was extensively rewritten to remove some of the more technical discussion, she thought she would be able to easily sell it to a trade publisher. I had mixed feelings about this idea, since if I removed some of these more technical chapters, I would be in the position of criticizing string theory, while not giving the details of what the problems with it were. I had also sent the manuscript to a few quite prominent mathematicians and physicists to ask them for advice about what to do with it. This led to some very interesting e-mail exchanges that I learned a lot from. Finally I heard from Roger Penrose, who offered to put me in contact with his publisher, Jonathan Cape. The editor at Jonathan Cape decided that they would like to publish the book, and that they were perfectly happy with it having some technical parts (which, after all, were quite a bit less technical than much of Penrose's recent book, which has been a great success).

So, that's the story until now of the book. I'm certainly curious what reaction it will get when it is published, and of course hope that it will stir up a serious debate on the issues currently surrounding string theory. I also hope the book will provide some explanations of what has been going on at the interface of particle physics and mathematics that a wide range of people will be able to get something out of, from members of the general public with an interest in science and math to professional researchers in both fields.

Update: Commentary on this here, here, here, and here.

Comments

1. **Quantoken**
   August 23, 2005

   Peter:

   Do you really want to sell your book? Listen your cover art needs a total re-design. From an artistic point of view it's totally un-attractive. The color is monochrome and boring. There is no balance of the values and shapes, no harmony, no contrast. It's simply un-appealing and looks ugly. I must admit that blue is my most favorite color. But even to me, the pale blue just look so ugly to me that I wouldn't even touch it if I see it in a book store.

   Now, the physics part. You think those spiral shape resembles what people gets on a high energy collider? It's **laughable**. The Spiral shape, if it spirals inward, describes a picture where the charged particle loses energy by EM radiation and hence spiral inward. That's a classical picture which is proven wrong and be replaced by QM. And the small curvature radius tells the energy is low, not high. Get some thing real! Or use something totally different.

   Also, do not use the word "failure" on the cover. Negative words on the cover is a turn off. I would rather use something like "controversy", "debate", "paradox", or other words that ring a bell. Helps you sell your book.

And hide your name on the hinge side, not on the cover. Make the big title bold and red, not black.

Quantoken

2. **woit**
August 23, 2005

Hi Quantoken,
Thanks for the marketing and design advice, but I'm mostly leaving those issues to the professionals. If you look at the Random House link, you can see the original subtitle for the book, but I agreed with my editors that a more descriptive subtitle was needed. It's true that part of this book is about a not very inspirational story, which may limit its readership, we'll see.

The graphic is essentially a bubble chamber photograph, the designer didn't make up those spiral shapes.

3. **Quantoken**
August 23, 2005

OK, that surely looks like a bubble chamber photograph, in which particles do lose energy in decaying spiral orbits. But bubble chamber is an instrument for low energy study, it has nothing to do with today's high energy collider, which is done in vacuum chambers, not bubble chambers. And it surely has even less to do with string theory, which today still makes no connection whatsoever to anything in the particle world, low energy pr high energy.

So a bubble chamber really does not match the topic you want to describe.

Better to use a favorite **toten** or icon the string theorists love to use. I do not know what it could be. But presumably something that must be drawn in 10-D space, not a 3-D image 🙂 And maybe an image of a alchemist sort of thing, too?

Quantoken

4. **sunderpeeche**
August 23, 2005

Quantoken is correct in both postings above. I recognized the cover as a bubble chamber picture and my first thought was "that 1960's stuff" ~ out of date. You should be able to get a nice picture of tracks from a collider event. While it is true that you may not have (much) control over the graphic art, who decided to use a bubble-chamber picture anyway? (probably a file photo?) Probably someone not as well-informed as you on modern physics.

It is also true that words like "failure" will be a strong negative. Are you trying to write a negative or positive viewpoint? Your post above has a much better subtitle, why not use it? "… a history of the standard model from a mathematically-informed perspective, a description of the history, …"

Subtitle ~ "A history of the Standard Model of particle physics from a mathematically-informed perspective with sociological commentary and prospects for the future" (this may be too long … omit "with sociological commentary")

Remember that it is the cover that sells the book, whatever the merits of your text may be. People won't open it if the cover doesn't appeal.

One further point — the preface must be well-written (you don't have a preview of the preface, so one cannot comment on your text). For those who pick up the book, the next step is to read the preface. The preface is the second selling point of the book. It must be good.

5. **Chris Oakley**
August 23, 2005

Well done, Peter! I am proud that it was my nation that was able to help you get this into print. It's about time.

<aside>Mind you … your experience does show you how the system works. Get one of the "big guns" to support you (Penrose in this case) and you'll be fine … otherwise, forget it. </aside>

The fears about the preface, by the way, are unfounded, if the January 2005 copy of the manuscript I have is anything to go by, anyway … the book is compelling reading from beginning to end.

6. **The Anti-Quantoken**
August 23, 2005

Don't listen to them, Peter; the cover art is cool, and the negativity is *precisely* what you are going for. This is an attack on string theory! You're done mincing words; the theory is a failure, a failure so far removed from valid science that it's Not Even Wrong, right? I don't know what planet Quantoken is from, where seeing a negative word on a book cover makes people cry and wet themselves, but when I see a book call a popular and well-regarded theory a "failure," I want to read that book.

Congratulations on your success. I'll be buying a copy.

7. **Tony Smith**
August 23, 2005

Peter, you say, about your book:
"… The first round of referee reports included a very positive report from a non-string theory particle theorist, a non-committal report from a mathematician who works on things related to string theory, and an extremely negative report from a string theorist. …
… the manuscript was sent out to two more referees, both theorists who have worked on string theory. It took quite a while for these reports to come back, and when they did, one of them was very positive and recommended publication. The second however was quite negative. …".

With respect to that I will quote from an e-mail message that I received some time ago:
"… retrospective studies have shown that the folks who went on to make truly big breakthroughs, get Nobel prizes, etc. , usually did NOT get all excellents when their ideas were first coming in for review at NSF. More typically, they get a MIX of excellents and poors. Much more can be said… but excluding work based on a kind of "min norm" aggregation of comments is one of the best ways to move towards total mediocrity and zero real research productivity. … The problem of Local heresy and Local conventional wisdom is very serious in every branch of science and engineering I have tracked. …".

Tony
http://www.valdostamuseum.org/hamsmith/

8. **Cameron**
August 23, 2005

Peter,

Last month's issue of Discover spotlighted spring theory. The editors asked Michio Kaku to respond to criticisms that string theory is not testable. In the article, Michio describes several upcoming experiments that would provide indirect evidence in support of string theory, depending on what kind of data is reported. Have you read it, and if so, what is your opinion? Looking forward to your book,

Cameron

9. **woit**
August 23, 2005

Cameron,

I wrote about that article when it first came out, see

http://www.math.columbia.edu/~woit/wordpress/?p=219

I've talked to some people associated with Discover magazine who suggested I contact the editor there about them publishing some sort of rebuttal to the Kaku piece, but, to be honest, I just haven't had the time to follow up on this.

10. **Scott**
August 23, 2005

I aggree with the anti-quantoken, the cover art looks cool, and most people won't really care that it is a bubble chamber and not a vacuum chamber, the point is that it is experimental data(at least i am guessing thats the point) which is what science should be based on. Anyways, I can't wait to buy a copy of your book.

11. **Who**
August 23, 2005

this is really great news
also i like the cover art and the revised title a lot

12. **woit**
August 23, 2005

Sunderpeeche,

A significant part of the book is about history, so the bubble chamber photo seemed appropriate to me. For a while Cape was suggesting a cover with a geometric sort of figure of the kind that Calabi-Yaus tend to inspire. I argued against that and for an image coming from a particle physics experiment, partly because my point of view is that the wealth of experimental evidence and successful theory concerning particles is likely to be much more important for future progress than untestable ideas about quantum gravity. I pointed them to a database of these kinds of images, and the designer chose the one they thought would work best as a book cover.

The book has both negative and positive aspects and I think the current subtitle reflects that. It's far from simply a criticism of string theory. But the editors and I felt that the subtitle should both indicate what the book was about and not be mealy-mouthed. The current version agressively leads with the controversial part of what I am saying, but I'm happy to stand behind that.

13. **a.spring**
August 23, 2005

So, who have you signed for the movie version?

14. **rof**
August 23, 2005

I agree with Anti-Quantoken. The occurrence of the word "Failure" in the title will help sales in this case. String theory has become a household name, so news of its failure will be more striking to bookshop browsers than the idea of yet another history of physics.

A google search for "Woit" and "Failure" produces a coincidentally relevant result.

Incidentally, it would be nice if somebody (you're somebody, Peter) collected the reactions of string theorists and non-string theorists to the book and presented them side by side on the web. Not for the sake of rebutting their criticisms, but just to see if there really is a statistically significant correlation between being a string theorist and hating the book. Presenting the reactions of non-string theorists who are competent to address the issue (Penrose is good for a start) will allow you to refute the argument which will be presented by string theorists, namely that only string theorists are intelligent enough to make judgments about these matters.

15. **D R Lunsford**
August 23, 2005

Well done Peter!

-drl

16. **woit**
August 23, 2005

rof,

I edited your comment so it has the correct link to the page I think you were mentioning. Pretty funny. As far as I know, D. Woit is not a relative.

17. **Chris**
August 23, 2005

I’m with the Anti-Quantoken. The cover art looks good and the word “failure” guarantees sales! I’m buying a copy when it comes out.

Do you plan on providing an online version before book publication? Seems the thing to do nowadays.

18. **woit**
August 23, 2005

Chris,

Cape is a commercial publisher and trying to make some money at this, so I’m sure they don’t want me making the book freely available. When it gets closer to the publication date perhaps I’ll get together a web-page for the book which may include some material from it, such as the table of contents and preface.

19. **Johannes Weickert**
August 23, 2005

I’m very pleased to learn you’re working on this book! It’s going to be near the top of my reading list.

20. **anon**
August 23, 2005

Lubos:

Mr. President, you know what a good Junior Republican I’ve been, what with voting for you (if I could), and poo-pooing global warming and women’s brains and liberal commies… Now, it is I who needs your help. Could I borrow Carl Rove for a small job? The background of a certain book author needs some clarification in the media. I don’t mean to take Carl away from the Cindy Sheehan case. It would just be a part-time loaner.

Bush:

Well, of course, Lubos. You’ve earned it. Maybe, in return, you could give a talk at my Bible Studies class about the latest Physics theories: String theory, Intelligent Design, etc.

21. **Alejandro Rivero**
August 23, 2005

Following the horizontal line between NOT and EVEN, and about 1/8 from the right margin, there is a decay event of a neutral particle into a pair of charged ones. The V of the pair points towards the main collision point, so one can assume the particle has been produced there and travelled by about one half of the width of the word WRONG. Just for curiosity, can anyone identify it?

(ah, does compulsive means the same that compulsory? I guess not.)

22. **Tom Weidig**
August 23, 2005

Hi Peter,

I like the title page fine.

But I am not sure putting up a comment by Roger Penrose is appropriate.

In fact, he is a good example of exactly what you seem to be fighting against namely theorists that go wild and spread theories that are not even wrong.

Roger Penrose has put forward a theory of consciousness that is not even wrong (to a greater extent than string theory), and especially his “partner-in-crime” Stuart Hameroff has been pushing their agenda with a lack of any reasonable sense of scientific enquiry.

On the other hand Roger Penrose’s achievement in standard theoretical physics are of course outstanding.

Best wishes,
Tom

23. **Who**
August 23, 2005

(ah, does compulsive means the same that compulsory? I guess not.)

no, it means “Compelling reading”
but it does not sound good to some people to have two consecutive gerunds (“…ing”).

Penrose made a bad literary choice to get away from “two ings in a row”

He should have bitten the style bullet and said
“Compelling reading”

24. **woit**
August 23, 2005

Hi Tom,

On the whole, how the book gets promoted is mainly up to the publisher, although if I really disagreed with what they were doing and told them so, I’m sure they’d change it. In this case I’m happy to have Penrose’s support and think it will probably be effective in getting many people’s attention.

A point I continually have to make is that I’m in no way opposed to people working on or promoting speculative ideas that are “not even wrong”. At this point in history, particle physics desperately needs new ideas. Any really new idea is likely to start off in a form where it is so poorly understood that no one knows exactly what its implications are or whether it will ever lead to solid, testable, permanent scientific knowledge. Penrose engages in lots of different sorts of such speculation, and I suspect that most of this ultimately won’t lead to anything, although in some cases he may end up having really been on to something (my best bet would be on twistor theory, where some day someone may figure out that it is a very important piece of the story of how internal and space-time symmetries get unified).

The problem with any particular “not even wrong” speculation only arises if it completely takes over a field and drives out other competing attempts to come up with new speculative ideas. I don’t think any of Penrose’s work shows any signs of causing this sort of problem.

25. **LM**
August 23, 2005

Hey Peter, is your publisher going to send Lubos a review copy?

26. **woit**
August 23, 2005

I was going to make sure they sent the other LM a review copy, but he has just posted a review of the book on his blog, so I guess I don’t need to. At least I get two stars….

27. **Lubos Motl**
August 23, 2005

Congrats, Peter. It’s a great idea to earn some bucks, although not exactly the most moral one. Based on the discussions on your blog, it is pretty clear that there are thousands of morons who are dumb enough that they will like the kind of arguments like “string theory is like intelligent design” and clap their hands. Well, I’ve encountered many of them already, and assuming that at least 10% of them can waste 50 bucks for an apparently useless book, you’re gonna be a bit rich. 😉

One more thing: you don’t believe that the string theorists are never asking important, conceptual, and philosophical questions, do you?

28. **woit**
August 23, 2005

Hi Lubos,

Judging from traffic on my weblog, a large fraction of the people who buy the book may be string theorists, and while there are a lot of them, I fear that there aren’t enough to make me rich.

Not sure what your last question is about. Sure I think string theorists are asking important, conceptual and even philosophical questions, but just don’t think they’re coming up with good answers or even promising ways of getting good answers.

29. **Wolfgang**
August 23, 2005

Peter,

I see your book listed in the category: Social Science and Popular Culture.
I find this pretty funny, but I doubt that you have any influence on that ?

30. **Lubos Motl**
August 23, 2005

Dear Peter,

if you were relying upon string theorists as your future customers, your chances to become rich would be poor indeed. 😉 Maybe you should lower your idea about the readers; they will have IQ lower by a few orders of magnitude than what you would like to believe. Your readers will be likes of DR Lunsford. 😉 In other words, complete morons.

As you know, I consider your opinions about the mathematical framework of string theory and its ability to generate new physical insights and predictions to be roughly as important as the noise generated by the chimps in zoo. But what I want to comment on is your proposal that the physicists should study “many other ideas” such as Chern-Simons theory – to go beyond the framework of the Standard Model.

You seem to misunderstand the difference between mathematics and physics completely. Chern-Simons theory is simply not a theory that is designed as a competitor of string theory to unify the known physics.

Chern-Simons theory is a theory that admits a similar type of (quantum-field-theoretical) description as some physical theories, but that apparently lacks the physical strength to have anything to do with the observed particle physics.

Chern-Simons theory is just an effective description of D-branes in topological string theory. A small subsector of an unrealistic vacuum of string theory. A few numbers. Discrete math. Nothing.

The main problem of yours is that you have completely lost your knowledge of physics and especially the idea which mathematical ideas may be relevant for which physics. When you talk about the “wide range of ideas” that physicists should be talking when they try to go beyond GR+SM, you obviously don’t know what you’re talking about.

There exist no general conceptual frameworks to surpass the existing theory except for string theory, and if someone tries to force people to work on these non-existent ideas, he is doing the same job as the Intelligent Designers. It just can’t work. There exist “small” ideas how new phenomena behind the Standard Model could look like, and this is what phenomenologists work on. But there is no unifying deep structure except for string theory. It’s not a theorem yet but it may well become one next year.

You will never be capable to understand why these alternatives to string theory can’t work – because you’re probably just too old for these things and you have not learned these important technical things in time. But you should at least try to understand that there is a crucial gap in your knowledge that makes all your “big conclusions” totally worthless.

You just can’t judge string theory without knowing anything about its math, its physical implications, and its uniqueness, and if you try to make big conclusions anyway, then you’re a crackpot.

Best wishes
Lubos

31. **Who**
August 23, 2005

oh hello Lubos,
Please tell us again why the loop and dynamical triangulations approaches to quantum gravity cannot possibly be right. It sounds better and better each time you explain.

waiting in rapt attention,

Who

32. **Lubos Motl**
August 23, 2005

Dear "who",

we have wasted roughly 100 times more time with these stupidities than what I would find appropriate.

See some standard texts such as

http://motls.blogspot.com/2005/01/very-meaningful-paper-on-loop-quantum.html

or

http://motls.blogspot.com/2004/10/objections-to-loop-quantum-gravity.html

and the preprints cited therein.

Best
Lubos

33. **The Anti-Lubos**
August 23, 2005

Does anyone, anywhere like Lubos? Just as a person, I mean. I can't imagine him as anything except universally despised, on a personal level.

34. **Kea**
August 23, 2005

Peter

Could you give us a sneak preview, or is that also in the hands of the publisher?

35. **.**
August 23, 2005

no.

36. **Kea**
August 23, 2005

"Does anyone, anywhere like Lubos? Just as a person, I mean."

I like Lubos. He seems like an honest guy.

37. **Lubos Motl**
August 23, 2005

Thanks, Kea. You'll always find a place in my heart. 😉

38. **Lubos Motl**
August 23, 2005

And yes, I definitely want my copy (and 10% of the royalties for making advertisements to you and encouraging the readers of limited intelligence to be interested in the debate). However, I am not sure whether you're brave enough and ready to see your work being deconstructed, Peter. 😉

39. **Quantoken**
August 23, 2005

Lubos:

Unlike many of the peers in your camp, like Kaku, who must have made tons of money selling books and going to TV programming claiming super string theory explains everything, my judgement is this is a money losing deal for Peter to spend all these time and effort to write a book that not many would buy. He could have spent the time doing something else and make more money. But he is doing a public service by salvaging a few pity souls who could otherwise waste a lifetime pursuing something unfruitful.

Why don't you write your own book and make a few quick bucks, too, Lubos? It's fashionable every one else in your camp is ready doing it? Mean while, by Peter's own admission, through intelligent selection of parents, he probably doesn't give a damn about the small amount of royalty from selling a few thousand books. Go get a better life if you are envy of that, Lubos.

Quantoken

40. **Luboš Motl**
August 23, 2005

Let me say that the ideal primary reason to write a book should be that the author has something new and interesting to say. Money may be fun, Quantoken, but you can't buy the most important things for them. On the other hand: yes, when certain conditions are gonna be satisfied, it would become irresistable to write things for which a book is the only appropriate format.

41. **Arun**
August 23, 2005

Congratulations! I look forward to reading your book!

42. **Gordon**
August 23, 2005

Thank you for writing another book on the subject, and I will
read it.

But I am curious. If there is not string theory, what does one
expect to find at energies of 10^30 eV or so…

43. **D R Lunsford**
August 23, 2005

Lubos, you omitted "physical" questions (of course).

-drl

44. **Interpretor**
August 23, 2005

Lubos said:

"You will never be capable to understand why these alternatives to string theory can't work – because you're probably just too old for these things and you have not learned these important technical things in time. But you should at least try to understand that there is a crucial gap in your knowledge that makes all your "big conclusions?? totally worthless.

You just can't judge string theory without knowing anything about its math, its physical implications, and its uniqueness, and if you try to make big conclusions anyway, then you're a crackpot."

Interpretation:

I have wasted 10 years of my life learning this crap and now you are trying to spoil the party. I will be left without funding, hype or hope. You are a worthless old whistle blower.

45. **Luboš Motl**
August 23, 2005

Dear Interpretor,

Peter Woit himself knows that what you write is complete rubbish. I have absolutely no reasons to fool myself, and your comments about the career and hype are irrelevant because I am planning no career whatsoever – especially because of other reasons to change the environment. My opinions are as pure and independent as you can get in this partially corrupt world.

I know that it may be annoying for many to hear it so often, but it is really incredible what kind of trash – both intellectually and morally – is contributing similar anonymous and sometimes less anonymous comments to Peter Woit's blog. I don't want to idealize Peter himself, but I am sure Peter Woit himself must feel to vomit when he reads comments like yours all the time. You should be ashamed.

Best
Luboš

46. **Peter**
August 23, 2005

Well, I'm able to easily control any nausea generated by the endless Lubos/anti-Lubos comments, but I do wish there wasn't so much of this. Lubos brings a lot of this on himself with his tirades, but you shouldn't encourage him.

We both have strong beliefs, and aren't in this for the money (you'd have to be pretty stupid to go into theoretical physics for the money). For better or worse, this new internet technology allows us to make the case for our opposing viewpoints (which aren't even always opposing, we seem to at least partially agree about the anthropic/landscape stuff). Up to you to decide who is making the most sense.

47. **Suresh K Maran**
August 23, 2005

If you guys want to talk about quantum gravity please drop by blog (click on my name). I just started the blog recently and learning to keep it steady. (I deleted some comments left by some of you accidently-sorry)

48. **Quantum_Ranger**
August 24, 2005

Peter, the cover picture is actually cover art!
This picture has been used on many occations, it's abstract..it's convergence by Jackson Pollock!

http://www.soho-art.com/cgi-bin/shop/shop.pl?fid=1056115418&cgifunction=form

I have the same image on the cover of a book entitled:How the Universe Works, which is an Open university course here in the UK.

I also believe the picture was dubbed “Particle Pollack” by Marcelo Gleiser?..used in the Stephen Hawkings ‘Universe’ documentry..or it may have been Franck Close lecture:Cosmic Onion.

It is a definate must to invoke abstract thinking into a very “abstract” Subject.

49. **ppcook**
August 24, 2005

Dear Peter,

Congratulations on finishing your book. It’s very important that important scientific debates are carried over to the mainstream public press. There are many popular science books that engage their readership with the mysterious implications of string theory, amongst others, but there are few that are willing to talk about the 30+ year struggle to get it to the shape it’s in now. It’s my opinion that popular science is very important, future scientists can get a beginner’s picture of the field they might wish to devote their lives to, and generally popular science books focus on the exciting and positive aspects of a story. Consequently all the popular science I have read about string theory gives the impression that it is all but a fait accompli and while I feel string theory is the best candidate to date, it’s important to emphasise that it isn’t complete, and that there is not a consensus amongst the informed (no matter how you define informed). So long as your opinions do not impinge on facts, which I’m sure from reading your blog they don’t, then you’re doing a very decent thing in taking the time to popularise your opinions about string theory. The truth will out eventually (either way), but besides this it’s important that the state of play is reported fairly, and I admire your stance of honestly presenting your doubts about string theory. There must be at least 3 anti-string theory popular science books now, versus 10+ or so in the implicitly pro-string camp; of course when it comes to text books the gap is wider ;).

With regard to the pro/anti-Lubos debate, I must say that I also admire Lubos very much for almost all the same reasons that I congratulate you, i.e. honesty and a desire to communicate ideas. Keep up the good work.

Best wishes,
Paul

50. **ksh95**
August 24, 2005

Peter: Congrats

Quantoken: Your comments are absolutely disgusting. Jealosy is the ugliest of all traits.

Anti Lubos: I like Lubos for the same reasons I like Peter. They believe what they believe and they stick to their guns.

51. **Luboš Motl**
August 24, 2005

Concerning the spirals, Brian Greene had a similar picture on the first French edition of The Elegant Universe.

http://www.amazon.fr/exec/obidos/ASIN/2221090659/402-4337920-0695356

See the other 25 or so covers of The Elegant Universe at:

http://www.physics.rutgers.edu/~motl/brian/

52. **Wolfgang**
August 24, 2005

> [..]
> I must say that I also admire Lubos very much for almost all the
> same reasons that I congratulate you, i.e. honesty and a desire to
> communicate ideas.
Very well put. This is exactly my opinion.

53. **woit**
August 24, 2005

Hi Paul,

Thanks for your comments. I’m glad to see that at least some people studying string theory recognize that the popular literature on the subject is rather one-sided, and that the other side of the argument deserves some exposure.

After this project is done, maybe I’ll start work on the anti-string theory textbook (AKA, a book about mathematics and QFT…)

54. **Alejandro Rivero**
August 24, 2005

The spirals (aka trajectories of charged particles) in Green book are to me a bit more strange, perhaps due to the enhancement. I am not able to were the big spiral starts and where it ends. Does it says what kind of collider it is?

As for the event in Woit picture, I am whinking about a kaon, could it be?

55. **woit**
August 24, 2005

The original source for the image is:

http://www.interactions.org/imagebank/search_detail.php?image_no=CE0057

I’ll leave it to you to do the analysis…..

56. **cvj**

August 24, 2005

Dear Peter,

I posted these two next comments on the cosmicvariance.com thread about your book. They were addressed to you since you had a comment there, but I am not sure if you are rreading that thread any more, so I will put them here too. I hope you don't mind. Come over to CV and give an answer at your leisure, and feel free to copy it here…or the reverse….put a pingback.

So they follow.

Cheers,

-cvj

57. **cvj**
August 24, 2005

Dear Peter,

As a result of several discussions on other comment threads on this blog, I was under the impression that we'd all made some progress in sorting out what were well-posed disgreements you have with some approaches research in string theory, what were "gut-feelings?? that you have (over which we can simply agree to disagree), what were misconceptions based on not being an active researcher in the field, and -very importantly- what were simply your misattributions of a minority view to that of the whole field. Recall that I spent a fair amount of time trying to clear these up. I refer you to the comment thread of the Landscape post, for example. I thought we arrived at some agreement that your views about what is actually going on in the field need a bit of re-balancing. If so, will these refinements be incorporated into the book before it is published? Or will your pre-cosmicvariance position be published? I do hope that these "finishing touches?? might involve significant rebalancing some of your emphasis to reflect the outcome of the enlightening discussions that have taken place here. Otherwise, it will be a missed opportunity for you to put out a book that is a useful alternative view, and not just a view based on an exaggerated chariacature of research in string theory.

I'd like to ask you to please make the effort. It probably won't delay publication at all, and even if it did, it will be worthwhile: It will improve your book, and thereby enhance your reputation. If it comes across as an uninformed rant, however, you'll do service to nobody's cause at all, which would be sad, at the very least.

Cheers,

-cvj

58. **cvj**
August 24, 2005

Dear Peter,

I refer particularly to your comment # 67 in that thread, although it is worth reminding yourself about the discussion that led up to that point. Quoting you entirely:

> Peter Woit on Aug 15th, 2005 at 8:42 pm
>
> Clifford,
>
> Sorry for harassing you into stating the obvious that once one has shown a theory is unpredictive, it's wrong (or not even wrong…) and one has to give up on it, but I think this discussion was worthwhile, it certainly helped me clarify some things for myself. And it's helpful to see that we share fundamental criteria for evaluating science. I'm afraid that I sometimes share what I take to be Lee's perception that for some string theorists, the possibility that the idea of string-based unification is just wrong seems to be something they won't even admit to be a possibility.
>
> No, I'm not going to take you up on your suggestion and devote myself to working on string theory. There are already many, many smart people doing this, and they appear to me to be doing a good job of slowly accumulating evidence that the string theory unification idea doesn't work. I don't think I could significantly speed that process up. I'll stick to pointing out what other people have already found, and trying to develop what seem to me to be more promising ideas.
>
> The main thing this clarified for me is the whole issue of falsifiability. You and Sean are right that it's a good idea to think about the analogy between the gauge theory and string theory frameworks, although I draw different conclusions from this analogy. I guess I do think that the difference is one of degree, but that differences of degree are crucial. Whatever theoretical framework one has, one can generally find some way of making it fit the facts. If it's a good theoretical framework it's easy, if it's not you have to engage in all sorts of ugly contortions. Thus, in evaluating theoretical frameworks, a sense of aesthetics is crucial, and claims like those that Susskind is making that it doesn't matter if things are really ugly are dangerous. I've been thinking a lot in recent years about this kind of "aesthetic?? issue, and the connection to falsifiability is something I hadn't thought about before.

So, will the refinements of your views mentioned by you in the above be reflected in the book? (Not to mention other points I mentioned which you agreed with elsewhere on the thread?)

Cheers,

-cvj

59. **cvj**
August 24, 2005

Link to comment thread of the Landscape post, in which many valuable exchanges were had:

here.

Cheers,

-cvj

60. **Scott**
August 24, 2005

I find it interesting that although I followed the discussion clifford is talking about I did not get the impression of peter substantially changing his oppinion at all, just admitting that not all string theorist are luny and or very worried about the landscape(something which I have no knowledge of him ever implying) and admitting that the falsifiability of a theory is obviously a matter of degree.

personally I think String Theory is in more danger of being not even wrong because there is no actuall theory that meets all of the assumptions/conjectures that string theory is built on rather than having too many consistant theories.

61. **woit**
August 24, 2005

I'm copying here my response to Clifford over at cosmicvariance.com. There should be a better way of carrying on some of these cross-blog discussions….

Hi Clifford,

Yes, the discussion here has had an effect on some of the changes I'm in the middle of making, specifically the new insight into the falsifiability issue that discussion here helped me with is one of those changes.

As for the other issues you mention, I should point out that I have a somewhat different point of view about parts of our discussion. In some cases what to you may have appeared to be a clearing up of misconceptions on my part to me seemed to be just my clarifying some things that I hadn't written carefully enough, allowing them to be too easily misunderstood or misconstrued. In any case, the book manuscript is written more carefully and at greater length than my web comments, so it shouldn't have so much of this kind of problem.

One thing you've properly taken me to task for is sometimes attributing to all string theorists views held only by a minority, or at least appearing to do so. To some extent this is hard to avoid. The sheer complexity of the range of different opinions is hard to do justice to in any piece of expository writing about these issues, so one has to oversimplify to some degree. I'm well aware that many if not most string theorists are eminently reasonable people I can agree with about most things, who don't hold unreasonable or indefensible views. Some of my best friends are string theorists, and I never have trouble talking about the subject with them.

On the other hand, there are a significant number of string theory partisans out there who seem to me to be unwilling to engage in rational discussion of the issues surrounding string theory, and often engage in the offensive behavior of assuming anyone skeptical about the theory is just stupid and ignorant. I've had a lot of this to put up with in the last day or so since publicly announcing my book project. These people are presumably overrepresented in internet forums, and range from fools hiding behind pseudonyms like F. Uckoff, to Harvard junior faculty, to respected senior faculty at major research institutions. I've just wasted some of my time trying to respond on Dave Bacon's blog to Greg Kupferberg, a mathematician string partisan who holds the unshakeable belief that my objections to string theory are of the same sort as Intelligent Designers' objections to the theory of evolution and that my only motivation is unwillingness to do the hard work necessary to learn string theory. I think Lubos Motl's comments here and elsewhere speak for themselves.

So, while I'm willing to believe that the majority of string theorists are reasonable sorts, that's not so clear from what goes on on the internet, and some of my experiences somedays leave me feeling not especially charitable. While there are certainly some stupid comments left on my weblog by people bashing string theory, I'd like to think that if any of these were coming from serious people in respected positions (e.g. Harvard faculty members), I'd be taking them to task for their behavior and I can't help noticing that this doesn't seem to be something any string theorists are willing to do.

About the landscape: my own view of the issue is extremely simple. Any theorist working on a theory who ends up deciding the theory leads to something that ugly and that unpredictive has to just acknowledge failure and do something different. I understand that there's a wide range of opinions about this among string theorists, but don't think this is a subtle issue. The book was largely written in 2002 before the landscape controversy got going, so material about it is kind of added on, and given the way I see this, I haven't had the interest or energy to go into too much detail about the various issues that people often get into when talking about this.

Finally, I don't want to put you on the spot in public, but will soon contact you privately with a proposition about the issues you raise. Maybe you can help me out…

62. **Luboš Motl**
August 24, 2005

Dear Peter,

if someone if being stupid or ignorant, then it's important to point out this fact, especially if the person is arrogant enough that he wants to decide about the direction of theoretical physics as much as Cumrun Vafa or Edward Witten, to say the least.

Let me emphasize that you don't know even the basics of the theory and the idea that this is a good starting point for a rational discussion about string theory is simply stupid. You seem to think that because your superficial insults against the whole field are supported by a gang of incredibly dumb readers, you have the right to expect that the leading theoretical physicists will discuss with you as with a peer. But that's completely crazy.

It's also important to say that F. Uckoff much like the senior string theorists you mentioned are very fine and smart people.

Sincerely Yours
Luboš

63. **Thomas Larsson**
August 24, 2005

It may be worth pointing out the Lubos Motl has proven to be quite ignorant about basic string theory himself. For the past five years, he has repeated, with the perseverence of a drunken parrot, that gauge symmetries are redundancies of the description. If he had understood chapter 2 of GSW he would have realized that this is not a general truth; the subcritical free string has a ghost-free spectrum despite its conformal anomaly. That consistency singles out 26D for the **free** bosonic string is just the lies-to-children (or lies-to-junior-Harvard-faculty) version of the no-ghost theorem.

64. **cvj**
August 25, 2005

Well Scott. Amusingly, Peter seems to disagree with you -see his post below yours- so it's probably a good idea in future to let him speak for himself, don't you think?

-cvj

65. **Nigel**
August 25, 2005

Dear Luboš,

You write above 'if someone if being stupid or ignorant, then it's important to point out this fact' and then you say 'you don't know even the basics of the theory'. Right, see how you like this.

Luboš, you don't need to eat a whole cow just to decide if the meat is bad. ST is past its sell by date. People don't need to sample the entire package to discover that it is poisoning physics. You just can't grasp this, although you string theorists have plenty of hypocrisy.

Notice that all I have to do is ask 'what does ST predict quantitatively?' If you say 'wait a year and I'll answer' you're wasting my time. Why should anyone study stuff which has led physics nowhere in over 20 years?

Now I don't have a postdoc in ST so I'm not 'intellectual' enough to comment, or well qualified enough to be serious, or just perhaps too juvenile in sticking to simple, testable ideas which work 🙂
Best wishes,

Nigel

66. **Alejandro Rivero**
August 25, 2005

http://www.cerncourier.com/objects/2004/cernbub10_7-04.jpg is a picture of the machine where the picture was done. The BEBC was not a old machine as some post could suggest; it worked during the seventies.

67. **Scott**
August 25, 2005

clifford,

Actually it is not that surprising that three different people would have three different perceptions about what transpired, I just figured that while you waited for peter's responce, I would point out that you were describing your perception and not neccessarily peters by stating my own perception (which was much closer to peters that most of discussion was just clarification of what you both thought). I find it interesting that you thought I was trying to speak for peter instead of just pointing this out especially when i started the comment by saying "I find it interesting…"

68. **The Statistical Mechanic**
August 25, 2005

" [..] Peter Woit thinks that superstring theory is not a scientific theory in the usual sense, being incapable of making concrete, testable predictions and he announced that he is finishing a book with the same title as his blog: Not Even Wrong. [..]"

69. **Who**
August 26, 2005

I followed the link given here to The Statistical Mechanic, to see what more Wolfgang had to say about the book, and found a strong recommendation of Capitalist imperialist Pig, and this link:
http://capitalistimperialistpig.blogspot.com/2005/08/bad-vibrations.html

to the "Bad Vibrations" blog entry by said Pig.
Gist: Pig tried to have a conversation about Peter's book and it ended unsatisfactorily. Pig then reflected on this.

Wolfgang's recommendation of the Pig blog is as follows:
"One of the better blogs is written by CapitalistImperialistPig. He usually has an independent, yet sane, point of view and discusses mostly US and world politics, but sometimes also physics and other stuff."

70. **anonymous idiot**
August 26, 2005

Who says: I read the colloquy between Pig and Lubos slightly differently. Pig teased Lubos – Lubos said "you are stupid, stupid, stupid, and an idiot moron," whereupon Pig retreated to his own blog and bitchslapped LM.

71. **Nigel**
August 26, 2005

http://motls.blogspot.com/2005/08/book-that-is-not-even-wrong.html :

Luboš resorts to the argument that Witten is best to judge if ST is right, as he is the most qualified in ST. (By the same argument, the best way to find out if a criminal is guilty is to ask him.) Luboš then repeats Witten's misleading claim (disproved by Penrose) that ST is proved by predicting graviton right (see my home page for quotes).

Quantoken replies to Luboš: 'Einstein says there is no distinction between acceleration or gravity attraction. If gravity is exchanged by a boson called graviton, then the two cases can in principle be distinguished by observing whether any graviton has actually been absorbed by the object or not, breaking the equivalence principle.'

I think that gets rid of gravity nonsense coming from the ST lobby! 😉 So now we know ST is as vacuous as cold fusion. 🙂

72. **pablo mora**
August 26, 2005

Dear Peter,

I found it very interesting your 'enigmatic and delphic' comment on the'quantumpontiff' blog about Chern-Simons theory, which provokedan furious andoutraged answer from LM.
In fact i am interested in CS gauge andgravity theories in higher dimensions. I would very much like it if you can elaborate on that, either in your blog or a personal e-mail.Thanks,

Pablo

73. **A Theorem, You Say?**

August 26, 2005

Lubos,

do you mean anything specific by “it is not a theorem now, but may be so next year”? (re: canonicity of ST).

If so, what did you have in mind? Is there some specific conjecture rather than a warm fuzzy feeling about the majesty of string theory?

74. **woit**
August 26, 2005

Hi Pablo,

Sorry to be enigmatic, but what I had in mind probably won’t help you. Right now I’m way behind on everything I’m supposed to be doing, but once the new semester here gets going, this fall I hope to spend time writing some long postings about things like the Chern-Simons idea I mentioned. People sometimes quite legitimately complain that there’s too much negativity on this weblog, which is probably right, and I should be spending more time writing about positive ideas that I find interesting.

75. **MC**
August 26, 2005

Peter,

Your cover art closely resembles that of my copy of “What is the World Made Of: Atoms, Leptons, Quarks, and Other Tantalizing Particles” by Gerald Feinberg (1977). Unfortunately, I can’t seem to find a picture of it online to show you.

76. **woit**
August 26, 2005

Hi MC,

Thanks for letting me know. I think the Columbia library has several copies of this (Feinberg was on the faculty here), so I can take a look at one of them on Monday. Maybe it’s just that many bubble chamber photos look alike, maybe both used the same CERN image. In any case I know where the Cape designer got the image and it wasn’t from Feinberg’s book. But if they’re too similar I’ll let Cape know and they can decide if they want to do something about that.

77. **woit**
August 29, 2005

Took a look at the Feinberg book. The cover may or may not be the same CERN bubble chamber, but definitely is a different picture.

More on WMAP

August 23, 2005
Categories: Uncategorized

In reference to my recent posting about the status of the WMAP experiment, an anonymous (but as far as I can tell, well-informed) source writes:

Hi Peter,

*I am *not* a WMAP person, and would appreciate you not mentioning my name or my institution, but here is the story in the interests of keeping things sane:*

1. WMAP is fine.

2. They are being very, very careful with their analysis.

3. Polarization foregrounds are difficult to model.

4. I doubt WMAP has detected GWs. Someone would have leaked that by now.

5. Note that WMAP does not have the sensitivity to detect the GWs predicted by inflation, it is hard to see how any simple, reasonable models could produce a GW signal much larger than that, and so a GW signal would be truly revolutionary if WMAP saw it.

6. The conspiratorial "Cosmology News" that you have linked to looks pretty slanted to me. They are talking about the famous missing power in low multipoles, discussed in the first year data release. Note that COBE also saw this missing power. IMO, I very much doubt this is due to systematics, as that site alleges, and statements that the team thinks – at the late date of 2004 — otherwise are almost certainly made up.

Comments

1. **D R Lunsford**
   August 24, 2005

   http://www.arxiv.org/abs/astro-ph/0403353

   http://www.cerncourier.com/main/article/44/10/4

   -drl

2. **Who**
   August 24, 2005

   DRL, you might be interested in this
   http://www.arxiv.org/abs/astro-ph/0508047/
   which just came out this month.

   it is by the same people and reviews earlier results,
   drawing stronger conclusions

3. **Quantoken**
   August 24, 2005

   hmm…VERY VERY **INTERESTING**!

   After reading the above paper. I completely changed my opinion regarding the mistery surrounding the WMAP data. I originally thought that it's a known fact that many local astronomical objects do emit microwaves, so what's big deal the data is contaminated by some foreground signal, which shows some characteristics ligned up with the solar system?

   But in reality it is a totally different story. Seeing something you do not expected see is one thing (which can be explained away by signal contamination), but seeing something you **expected** to see **COMPLETELY MISSING** from the data, is quite a different story, and it can not be explained away by data contamination!!!)

   Quote:
   "To conclude, using the multipole vector decomposition we have shown that the quadrupole and octopole of the microwave
   background sky are correlated with each other at a level that is excluded from being chance in excess of 99%. This comes about from a preponderance of peculiar correlations and is statistically independent of their observed lack of power. This observation is **in bold contradiction** to the predictions of pre-existing cosmological model, and **argues against an inflationary origin** for these fluctuations. In addition, there is strong evidence (again of greater than 99% confidence) that the microwave background at these multipoles is correlated with the geometry and direction of motion
   of the solar system. The observed signal is most **unlikely** to be due to residual contamination of the full-sky microwave background maps by known Galactic foregrounds."

   Any comment?

   Note the authors are NOT the WMAP team members, who would be too timid to make such bold and politically incorrect statements!

   Quantoken

4. **D R Lunsford**
   August 24, 2005

   Thanks Who, that is a real paper.

   -drl

5. **Matti Pitkanen**
   August 24, 2005

   The lack of correlations for angular scales above 60 degrees implies the smallness of quadrupole and octupole moments: see http://www.cerncourier.com/main/article/44/10/4.

   I have proposed an explanation in terms of many-sheeted space- time. The space-time sheets along which incoming photons arrive have finite size. For large angular separations it is probable that they arrive along different space-time sheets so that there is no correlation.

   See the subsection "Fluctuations of the microwave background as a support the notion of many-sheeted space-time" at http://www.physics.helsinki.fi/~matpitka/tgd.html#cosmo .

   Matti Pitkanen

6. **Artem Khodush**
   August 28, 2005

   Could this be explained by non-uniform distribution of mass in the universe? I.e. what if all the supposed dark matter is inside some giant black hole lurking somewhere, could that black hole affect CMB photons in a way to produce such anomalies?

7. **D R Lunsford**
   August 29, 2005

   Could this be explained by non-uniform distribution of mass in the universe? I.e. what if all the supposed dark matter is inside some giant black hole lurking somewhere, could that black hole affect CMB photons in a way to produce such anomalies?

   This is the problem with teaching and endorsing fantasies. Students start to think of fantastic (that is, absurdly unreal) scenarios instead of sticking to sane physics.

   The issue is that the data indicate an unexplained local contribution to the MB, after which the C in CMB is practically irrelevant, the BB plain wrong, and inflation an opium smoker's pipe dream.

   -drl

8. **D R Lunsford**
   August 29, 2005

   This is off topic but important.

   Today we had an authentic physics miracle – once the eye was past New Orleans, the equally strong west winds pushed the storm surge back out to sea. It's tempting to think of this as simple superposition of waves but wave motion is essentially different in even vs. odd spatial dimensions – in any case the equal and opposite winds on the south side of the eye tended to mitigate the storm surge.

   Now the amazing thing is, the storm turned right by just the right amount to be able to use the storm's own rotational energy against itself. This is, to all purposes, a near miracle.

   -drl

9. **Artem Khodush**
   August 30, 2005

   The issue is that the data indicate an unexplained local contribution to the MB

   I dont' get why it's assumed local. Just because it's aligned with ecliptic? Or are there other reasons in the article which I overlooked?

10. **D R Lunsford**
    August 30, 2005

    Because the "horizon", the ultimate non-local, defines a (projective) metric, which in turn imposes an order – of magnitude – on influences. There are two scales – cosmic and local – because of the cross-ratio, which essentially is a quotient – and is the basic invariant of projective geometry. The claim so far is that the MB is cosmic only.

    -drl

11. **D R Lunsford**
    August 30, 2005

    I see I misread your question – I actually answered the question "Why you can't assume it's cosmic". To answer the actual question – if aligned with the ecliptic then, since the final radiation field can be expanded in spherical harmonics, there is a pre-defined relation between all the various multipole terms, because the symmetry is now planar and not spherical – a favored direction. That is, space is not isotropic. In fact the most interesting part of this work is that the octopole and quadrupole are aligned, with each other, regardless of the ecliptic.

    -drl

12. **Artem Khodush**
    August 31, 2005

    Thank you DRL for the explanation. Looks like I misunderstood the term "local" – actually it can be anything anywhere that defines a favored direction. And doing wild guesses is pointless unless I could put that in the equations, solve, and compare result, which I can't. Sigh. Thanks again.

13. **D R Lunsford**
    August 31, 2005

    Well people are imagining bizarro worlds with complicated topologies instead of just admitting the simple answer, that not only does light fall – gravity glows.

-drl

14. **island**
August 31, 2005

So, Einstein was right if the universe is rotating, then it has a center of rotation, and a center of gravity. If the assumption about the universe being unbounded can be demonstrated to be false, then the Copernican Cosmological principle takes a big hit and BB theory gets more realisitic applied to a universe that has volume when a big bang occurs without a singularity.

I'll buy all of that even if I did have to say it myself… 😉

15. **D R Lunsford**
September 1, 2005

"The Universe is rotating"

With respect to what?

How can you even WRITE such a sentence?

-drl

16. **island**
September 5, 2005

I know what you're saying, but it falls out of Quantoken's quote, maybe as more of a figure of speach… or wouldn't this result in quadrupole and octopole moments?

> In addition, there is strong evidence (again of greater than 99% confidence) that the microwave background at these multipoles is correlated with the geometry and direction of motion
> of the solar system.

The pattern indicates a rotating universe, since this incoherence manifests via octopole and quadrupole components in a bound universe, so there should be a center of gravity at the center of the visible universe with the universe rotating as a black hole might… with respect to what… I have no clue.

Oberwolfach Workshops

August 24, 2005
Categories: Uncategorized

There have been two quite interesting Oberwolfach workshops this summer with some relation to my favorite ideas about K-theory and quantum field theory. The most recent was a workshop on Gerbes, Twisted K-theory and Conformal Field Theory, with blogging from Urs Schreiber at The String Coffeee Table. Jouko Mickelsson gave a talk on “Twisted K-theory and the index on G” which from Urs’s description was mostly about the material in Mickelsson’s paper Families Index Theorem in Supersymmetric WZW Model and Twisted K-theory. This is closely related to the Freed-Hopkins-Teleman theorem, and their construction of a twisted K-theory class using Dirac operators on a circle, parametrized by connections on the circle.

Urs wasn’t sure what to make of this talk or how to connect it to string theory. My own point of view is that this is very interesting not because of the relation to strings, but because one can think of it as a possible new way of describing the Hilbert space for 2d chiral gauge theory. Perhaps this can provide a 2d toy model to test out new approaches to gauge theories in 3 and 4 dimensions. From this point of view, the QFT involved is best thought of not as the supersymmetric WZW model, but as a chiral fermion coupled to a gauge field, with BRST gauge fixing. In some sense what is going on here is an index-theoretic version of BRST.

Earlier in the summer there was an Oberwolfach workshop on Geometric Topology and Connections With Quantum Field Theory. One of the main topics there was recent work on elliptic cohomology, with a survey talk by Graeme Segal and Jacob Lurie speaking on a new “derived algebraic geometry” approach to the related theory of “topological modular forms”. Greg Moore’s talk looked interesting, especially his comments on various QFTs which he thinks of as special cases of AdS/CFT, and generalizations of the Chern-Simons/CFT correspondence. In a footnote he writes “It would constitute a major step forward in mathematics if someone could state the AdS/CFT correspondence in a mathematically precise way.”

The same Oberwolfach workshop also had a talk by Nitu Kitchloo on “The Baum-Connes Conjecture for Loop Groups”, which really was also about Freed-Hopkins-Teleman in disguise. I’ve talked a little bit with Paul Baum about this idea that FHT is Baum-Connes for loop groups, but Kitchloo has tried to do something with it. The general idea behind Baum-Connes is that one can study the representation theory of a group in terms of the topological K-theory of a classifying space for the group. In the case of loop groups, the classifying space is the space of connections on a trivial bundle over the circle, and the topological K-theory is FHT’s twisted K-theory of the group. The information about the loop group representation theory is encoded in the Verlinde algebra. An ongoing project of mine is to try and sort out the relations of this story to 2d QFT (see comment above about Mickelsson’s work), hoping that if one gets the right point of view on the 2d case one can use this to define gauge theories in 3 and 4 dimensions in terms of some sort of K-theory, implementing some sort of Baum-Connes correspondence for higher dimensional gauge groups.

Comments

1. **John Baez**
   September 6, 2005

   How come whenever you talk about something with some mathematical substance, nobody ever comments on it, while they are happy to argue endlessly over X being rude to Y in Z’s blog?

   Sigh….

   I think there’s got to be a much slicker way to formulate and prove the relationship that the Freed-Hopkins-Teleman result addresses, but so far my best attempt is the paper I wrote with Alissa Crans, Urs Schreiber and Danny Stevenson relating the fundamental gerbe on a compact Lie group G to a 2-group (= categorified group) that one can also build from the central extension of the loop group of G.

   I guess I need to go further and study how the representations of this central extension are connected to the K-theory of vector bundles on G twisted by the fundamental gerbe.

2. **woit**
   September 6, 2005

   Hi John,

   I’ll take a close look at the paper you mention, and see if you can convince me that I really need to think about Lie 2-algebras and 2-groups. My main interest in this is still QFT; I think there’s a beautiful QFT story going on here, one which I’ve only partly worked out, but have yet to see how the 2-group point of view helps.

   You’re right that it’s kind of discouraging that my more substantial posts don’t get many comments. Partly my fault I think, for just referring to things without taking the time to write out some explanations of what is going on with this stuff (the kind of thing you do in TWF). This fall I hope to have more time for this and to get organized to be able to put formulas in these postings in some simple way. Maybe that will help.

   I can’t really blame people though. The ongoing train wreck that is string theory and the bizarre behavior it is leading to is pretty fascinating to watch and hard to resist commenting on.

arXiv Trackbacks

August 24, 2005
Categories: Uncategorized

As discussed here, here, here, and here, the arXiv is now putting on each abstract page a link to trackbacks from weblogs which contain a link to the paper in question. This is an interesting mechanism for integrating the discussion of various papers on weblogs with the arXiv site.

I remember more than ten years ago Paul Ginsparg talking about the idea of setting up a mechanism for having commentary on papers on the arXiv, but this idea seems to have not gotten off the ground at the time. Part of the idea was that the author of the paper would be able to delete any posted commentary he or she didn't like. When asked about whether this would stop people from being able to use the commentary section to point out that a paper was wrong, Ginsparg noted that if there was no commentary on a specific paper, did you really care whether it was because the author had deleted the comments, or because no one thought the paper was worth commenting on?

My latest posting from earlier this evening contained a couple links to arXiv papers (I didn't know about this trackback business at the time). Jacques Distler explains that one's weblog has to be on a list of "serious physicist-bloggers" in order for one's trackbacks to appear. So far mine haven't, so I guess I'm not a "serious physicist-blogger" by the standards of Jacques (or whoever is managing this thing).

Update: The trackbacks are there now, as pointed out by Sean Carroll. Not sure when this happened, partly because there seems to be a bug in their system. The abstract page for the Mickelsson paper I linked to counts only one trackback, when there really are two (the other, from Urs Schreiber was there yesterday).

Comments

1. **Steve Thorsett**
   August 24, 2005

   > Jacques Distler explains that one's weblog has to be on a list of "serious physicist-bloggers?? in order for one's trackbacks to appear. So far mine haven't, so I guess I'm not a "serious physicist-blogger?? by the standards of Jacques (or whoever is managing this thing).

   In my experience, WordPress doesn't support trackback autodiscovery, so a casual link to an abstract isn't enough: I have to deliberately ping the http://arxiv.org/trackback/… address. I've verified this in experiments from my own page with arxiv. Manual trackbacks register (after a short delay), but simple links don't. Of course, we're using different versions of WordPress.

2. **woit**
   August 25, 2005

   Thanks Steve. But I'm still confused. Notices of trackback pings from me appear in other weblogs when I post something with links to them, even if I don't put the trackback in manually. I just tried putting in the two arXiv link trackbacks manually, but it looks to me like I'm getting a message that these were "already pinged". Anyway, I'll check after a while to see if the manual trackbacks had any effect.

3. **Suresh**
   August 25, 2005

   the trackback mechanism is semi-reviewed, in that it has to go thru some processing before the trackbacks are registered. this is probably why you haven't seen it yet.

4. **Sean**
   August 25, 2005

   Looks like your trackbacks are there now.

5. **woit**
   August 25, 2005

   Thanks Sean,

   I'd checked earlier this morning, but the fact that their trackback counter doesn't seem to be working may have kept me from realizing that mine was there.

6. **Steve Thorsett**
   August 25, 2005

   The page headers at arxiv aren't being set correctly with a new "modified" date when the trackback counter changes, so you are pulling the page with your cache with an old value. Force a refresh (shift-reload in firefox) and the counter will be correct. This caught me last night.

A First Course in Modular Forms

August 29, 2005
Categories: Uncategorized

I recently got a copy of a very interesting new textbook entitled A First Course in Modular Forms by Fred Diamond and Jerry Shurman. Fred was a student of Andrew Wiles at Princeton, and came here to Columbia as a junior faculty member at the same time I did. He now teaches at Brandeis.

The title of the book is a bit deceptive, what it is really about is what used to be called the Taniyama-Shimura-Weil (or some subset of those names) conjecture, but now is often known as the Modularity Theorem. Most of this theorem was proved by Andrew Wiles (with help from Richard Taylor), who famously used his result to prove Fermat's last theorem. More recently, the proof of the full theorem was completed by Fred, together with collaborators Christophe Breuil, Brian Conrad and Richard Taylor. Stating the modularity theorem precisely requires some serious mathematical technology, an imprecise statement is the "All rational elliptic curves arise from modular forms". This fits into the Langlands program of establishing a correspondence between arithmetic objects (in this case elliptic curves over the rational numbers), and analytic objects (in this case modular forms). If one can do this, typically the fact that the analytic objects are pretty well understood allows one to get a vast amount of very deep information about the more mysterious arithmetic objects (e.g. being able to count solutions to equations over the rationals or integers).

The book takes an interesting approach to the Modularity Theorem, not trying to actually prove it. The proof involves highly sophisticated mathematical technology, and really understanding it is still the province of experts. If one wants to try and learn this technology, two places to look are the volumes Modular Forms and Fermat's Last Theorem and Arithmetic Algebraic Geometry, which are the proceedings of two different instructional conferences. Instead of trying to give a proof, Diamond and Shurman's book explains exactly what the various related versions of the Modularity Theorem say. This covers a range of beautiful mathematical ideas, much of which hasn't before had a particularly readable exposition. Until now, the main reference for some of this material has been Shimura's Introduction to Arithmetic Theory of Automorphic Functions, a famously difficult text.

The book is advertised as "A First Course" and attempts to minimize the prerequisites necessary to read it, making it conceivable to even use the book with advanced undergraduates. This is a worthy goal, but may be a bit over-ambitious. I suspect most people will get more out of the book if they already have had exposure to some of this mathematics at a slightly more basic level. One place to get this is Neal Koblitz's Introduction to Elliptic Curves and Modular Forms. But this really is a wonderful book, making accessible parts of the really beautiful mathematics which mathematicians have been making great progress in understanding over the last decade.

Comments

1. **owen**
   August 29, 2005

   I'm not qualified to comment on the mathematics contained in this book, but I would like to mention that I had Jerry Shurman for an introductory math class when I was a freshman. He was a fantastic teacher – possibly the best I had (along with David Griffiths) during four years at Reed. I consider this to be an impressive achievement, considering the general caliber of professors at Reed. He had a knack for conveying information clearly and concisely without being dull in the least. It's no surprise that this book succeeds at "making accessible parts of really beautiful mathematics."

2. **iso42**
   August 29, 2005

   I have a naive question.
   If only a handful of people understand and can check a proof a la Wiles, how can we be sure that there is no hidden problem with it?
   Is there a formalized, automated or semi-automated way to do this?
   I know that long reviews were done on Wiles' proof but I wonder if the mathematics could be formalized enough to check it (to some extent) on a computer.

3. **woit**
   August 29, 2005

   As far as I know, at the level that Wiles is working the arguments are difficult to completely formalize so that a computer could check them. One thing to keep in mind though in a case like this is that his argument was gone over with a fine-toothed comb by some very good people, including some who wish they had solved this problem themselves, so were highly motivated to find something wrong with it. Also, many other people are now using his techniques to try and do other things. If there were a problem with his use of one of them, it's quite likely someone would notice this when they tried to use it to do something else.

   When not many people care much about a result, it is quite possible for a wrong argument to get in the literature and be accepted. In this case it seems extremely unlikely.

Warped Passages

August 30, 2005
Categories: Book Reviews

A couple days ago I got ahold of a copy of Lisa Randall's new book [Warped Passages: Unraveling the Mysteries of the Universe's Hidden Dimensions](), and finished reading it last night. It's a book intended for a popular audience, containing an overview of modern physics, but concentrating on the idea of extra dimensions beyond the standard four we know about. The last part of the book attempts to explain at a non-technical level work by Randall and others that generically goes under the name of "braneworld scenarios", and involves various versions of the idea that our four dimensional space-time is embedded in some higher dimensional space. The specific ideas she describes in some detail are:

1. Work with Raman Sundrum (hep-th/9810155) on solving the flavor-changing problems that occur in supersymmetric models by "sequestering" the supersymmetry breaking sector on another brane, separated from ours.

2. The Arkani-Hamed, Dimopoulos and Dvali idea (hep-ph/9803315) of large extra dimensions, which explains the weakness of gravity as due to the large size of some of the extra dimensions, with gravity propagating in them, but not the other forces.

3. The Randall-Sundrum warped geometry with two branes (hep-ph/9905221).

4. The Randall-Sundrum warped geometry with an infinite extra dimension, using AdS geometry (hep-th/9906064).

5. Work with Karsch on "localized gravity" (hep-th/0011156).

I afraid I've never found these brane-world scenarios to be at all compelling. They don't really seem to me either aesthetically appealing or able to explain in a convincing way any of the things we don't understand about the standard model. They're not derived from any fundamental theory, so the rules of what branes you're allowed to postulate and what properties you can assign to them seem very loose, allowing all sorts of things. At one point Randall writes:

Other branes might be parallel to ours and might house parallel worlds. But many other types of braneworld might exist too. Branes could intersect and particles could be trapped at the intersections. Branes could have different dimensionality. They could curve. They could move. They could wrap around unseen invisible dimensions. Let your imagination run wild and draw any picture you like. It is not impossible that such a geometry exists in the cosmos.

which I guess is meant to be inspiring, but makes me worry there's not enough structure to this game to make it useful. One virtue of some of these models is that they lead to new phenomena at potentially accessible energy scales. If the LHC sees the kinds of effects predicted by these models, there will be some well-deserved Nobel prizes for the people involved in this story, but this seems to me highly unlikely. Randall says in her book that she really does believe in these sorts of extra dimensions, but most particle theorists I know of (string theorist and non-string theorist) tend more to the opinion that while these are models worth investigating (since you may learn something, and it gives experimentalists something more specific to look for), there's only the most outside chance that they correspond to what the LHC will see.

The one problem of the standard model that braneworlds do provide an interesting answer for is the hierarchy problem, that of why the weak and Planck scales are so disparate. In these scenarios, the fundamental gravitational scale is not the Planck scale, but something closer to the weak scale, so (unlike in the standard picture) gravity is not weak because the Planck scale is so large, but because braneworlds provide various mechanisms for making the gravitational force much weaker than the others. The idea that the gravitational scale may be closer to and maybe even directly related to the weak scale, and that this is somehow related to the electroweak symmetry breaking mechanism that we still don't understand, is an appealing one, but the ways braneworlds accomplish this removes much of the appeal (at least for me). The choices just seem too arbitrary, and while there is some geometry involved, it is geometry of a crude sort. The standard model involves fascinating and beautiful spinor geometry and the geometry of Yang-Mills fields, which is pretty much ignored in these scenarios, which try and get everything out of simple Riemannian geometry and general relativity sorts of considerations.

There's a lot about string theory in the book, with Randall clearly skeptical about many of the claims made for the theory. I remember a few years ago at a debate over string theory held at the Museum of Natural History here in New York, she scornfully responded to the argument that "string theory predicts gravity" with "sure it does, gravity in ten dimensions." Here she says *I'm an agnostic on this subject – I don't know what string theory will ultimately be or whether it will solve the questions of quantum mechanics and gravity it sets out to address*. She's similarly agnostic about GUTs: *Although unified theories have some appealing features, I'm not really sure whether studying them will lead to correct insights into nature. The gap in energy between what we know and what we extrapolate to is huge..*

Randall describes being a student in 1984 at Harvard, seeing the field split into two camps that were at odds with each other: Gross/Witten doing string theory at Princeton, Georgi/Glashow doing model building at Harvard. About Princeton she says :

Physicists there were so certain that string theory was the road to the future that the department no longer contained any particle theorists who didn't work on string theory – a mistake that Princeton has yet to correct.

She tells the story of the relation between model builders and string theorists over the last twenty years as follows;

Early on, the battles between the merits of the two opposing viewpoints – string theory and model building – were fierce, with each side claiming better footing on the road to truth. Model builders thought that string theorists were in mathematical dreamland, whereas string theorists thought that model builders were wasting their time and ignoring the truth.

Fortunately, things have now changed. ….many of us now think about string theory and experimentally oriented physics simultaneously. I have continued to follow the model building approach in my research, but I also incorporate ideas from string theory…. The communities are no longer so rigidly defined, and there is more common ground. Both scientifically and socially, there are now strong overlaps between model builders and string theorists.

The fact that branes are an important part of modern string theory meant that string theorists took an interest in this kind of model-building, with Randall noting that:

In fact, because our research didn't directly challenge string theory models, the string theory community actually accepted and recognized the significance of our work sooner than the model-building community.

In particular, the fact that the Randall-Sundrum model uses the same AdS geometry and has interesting relations to AdS/CFT has drawn a lot of interest from string theorists. Whatever you think of all this as physics, as academic politics it was an absolute stroke of genius, defusing a bitter conflict. I confess to finding this unholy alliance between the model-builders and string theorists rather problematic. I'd much prefer to see the model-builders holding string theorists accountable for the theory's inability to actually predict anything or even lead in any well-defined way to a specific class of models that could be tested. By reaching an accomodation with string theorists and agreeing on a central role for string theory in particle theory research, the model-builders have made the string theory juggernaut pretty much impregnable, leaving anyone interested in alternatives to string theory very much marginalized within the particle theory community.

In the acknowledgements, she prominently thanks one of her Harvard colleagues:

Lubos Motl, a brilliant physicist and dedicated science communicator (whose specious ideas about women in science we'll ignore), read everything, even before it was readable, *and gave extraordinarily useful suggestions and encouragement at every stage*.

Update: Lubos has a new posting about Randall's book. He ends by referring to some forthcoming book containing "dumb insults against the physicists". I guess the rumors that he's written something for publication must be true then.

Comments

1. **Chris Oakley**
   August 30, 2005

   In the absence of any evidence for hidden dimensions, especially those in need of unraveling, the title, "Warped Passages: Unraveling the Mysteries of the Universe's Hidden Dimensions" is somewhat misleading.

   I would prefer, "Exploring the possibility that extra dimensions might solve some problems in physics".

   But, hey, when did honesty sell books?

2. **Very**
   August 30, 2005

   >The choices just seem too arbitrary, and while there is some >geometry involved, it is geometry of a crude sort. The standard model >involves fascinating and beautiful spinor geometry and the geometry >of Yang-Mills fields, which is pretty much ignored in these scenarios, >which try and get everything out of simple Riemannian geometry and >general relativity sorts of considerations.

   Crude??? The "beautiful" spinors you write about are one of the renormalizable local representations of the Poincare group, which is why they appear, not because of their underlying "beauty". Similarly, gauge theories arise because when you try to describe massless spin-1 bosons using a vector field, we come up with unphysical gauge degrees of freedom. The only consistent way to make those massless spin-1 bosons self-interacting is if they happen to arrange themselves into a Lie algebra. There's no underlying mathematical beauty here either.

3. **Chris Oakley**
   August 30, 2005

   Very-

   Beauty is very much in the eye of the beholder, but I agree to the extent that the beauty of Yang-Mills evaporates as soon as one tries to quantize. If gauge fixing, ghosts or renormalization is beautiful then the Incredible Hulk ought to win Miss America.

4. **woit**
   August 30, 2005

   Very,

   Some of us happen to think that the fact that gauge fields are connections and connections are the fundamental objects in modern geometry means that they're mathematically beautiful. You're welcome to the philosophy that this is just a coincidence, that what's important is what gives consistent quantization of a massless spin-1 field, but my philosophy is different. We can't really have a rational discussion about what is mathematically beautiful and what isn't, but I'll point out that I just spent a year teaching our graduate course in geometry in the math department here, so I have some idea what mathematicians consider the deepest and most beautiful constructions in the subject.

   Similar remarks apply to spinors. As to "crude", Riemannian geometry is 19th century mathematics, spinor geometry and the geometry of general connections is 20th century mathematics.

5. **woit**
   August 30, 2005

   Chris,

   I agree with you that the way gauge invariance is handled in quantum Yang-Mills theory is not a pretty sight. It gets the Feynman rules right, but opens all sorts of other questions. I suspect there are better, more beautiful ways of doing it, and these are very much worth working on.

6. **Shantanu**
   August 30, 2005

   Peter, does this book discuss Loop quantum gravity , dynamical
   triangulations or anything else besides string theory?

7. **woit**
   August 30, 2005

   Shantanu,

   Nothing about loop quantum gravity, dynamical triangulations, etc., and very little about quantum gravity. Randall makes it clear she's fundamentally a particle theory model builder, interested in making models of particle physics that have some hope of being tested at future accelerators or other particle physics experiments.

   I undoubtedly overemphasized how much there is about string theory in the book, since I was interested in her relation to string theory, and she does write extensively about this. But most of the book is basically a particle physics book, aimed at getting to and explaining her work on braneworld scenarios in particle physics.

8. **Who**
   August 30, 2005

   Shantanu that is a great question
   **does [the Randall] book discuss Loop quantum gravity , dynamical
   triangulations …?**

I'm inclined to think that at this point any book that pretends to be about contemporary views of spacetime and does not give an adequate description of the loop and triangulation pictures is appealing to self-indulgent fantasists rather than to interested lay readers.

So I am curious too. Maybe Peter will tell us. (Must say I dread the answer.)

9. **woit**
August 30, 2005

Who,

Already answered that question above. This is not in any way, shape or form a book about quantum gravity, and actually I think that's fine. The problem of quantum gravity is not the only important one out there!

Although after writing this comment I thought better of it. The assumption that the gravity scale is much lower than the Planck scale does mean one is saying something about quantum gravity in these braneworld scenarios. But Randall isn't much interested in any of the standard conceptual problems about quantizing gravity, or in the question of what happens at very high energies, whether the theory is finite, whether there's a background independent theory, etc. She's taking a very pragmatic approach, not asking fundamental questions about space and time of the sort that normally are part of the subject of quantum gravity.

10. **dan**
August 30, 2005

"Lubos Motl, a brilliant physicist and dedicated science communicator (whose specious ideas about women in science we'll ignore)"

i'm kinda curious as to what lubos' ideas about women in science, as we all clearly know his views on LQG.

11. **woit**
August 30, 2005

He seems to have gotten himself into trouble with his colleagues over this. Basically he was strongly supporting Summers in the recent controversy at Harvard over women in science. Like Summers, he seems to believe that the reason there are a lot fewer women than men in science is that, on the whole, they're dumber.

Oh, and please don't start up a discussion of this whole controversy here, it's been done to death already many places on the internet. So unless someone has a really informative and original comment on this (or Lubos wants to defend himself against mischaracterization of his views), I'll probably delete more comments about this as off-topic.

12. **Gordon**
August 30, 2005

You should realize, that none of theories you advertised are quantum consistent. They explode upon quantization. I still
dont know what to do about this, besides many months of work.

13. **Jean-Paul**
August 30, 2005

You described the book without any comments on its quality — very diplomatic.
So is it a good book? A possible best-seller?

14. **Chris Oakley**
August 31, 2005

Well, Gordon, don't quantize, then.

15. **Thomas Larsson**
August 31, 2005

Very,

What you say about spinors and connections is technically correct, but why don't you think that this is beautiful? Mathematical beauty is usually connected with symmetry, and groups (and infinitesimally Lie algebras) are the language of symmetry.

You can regard differential geometry (DG) as the representation theory of the group of diffeomorphisms. All objects of interest in DG, like tensor fields, connections, exterior and covariant derivatives, etc., have a natural formulation in diffeomorphism group language. This is obvious, since DG is about well-defined objects, and an object is well-defined precisely when it transforms as a representation under arbitrary coordinate transformations.

You are of course free to think that DG is trivial or ugly. However, you cannot coherently argue that DG is beautiful and that diffeomorphism group representations are not, because the former is a special case of the latter.

A major insight of conformal field theory is that in addition to the classical reps relevant to 1D DG (primary and secondary fields), there are also quantum or lowest-energy reps, with energy = L_0: Verma modules, Fock modules, minimal models, etc. These quantum reps are directly applicable to the physics of 2D phase transitions, and also play an important role in string theory; the special role of D=26 follows immediately from the Virasoro algebra.

Algebras of diffeomorphisms and gauge transformations admit similar quantum representations also in higher dimensions. I have tried to educate the physics community about this remarkable fact for several years, so far in vain. People unwilling to learn are simply not susceptible to edification.

16. **a**
August 31, 2005

From a phenomenological point of view the AdS/CFT duality means that the Randall-Sundrum model is the same thing as "walking technicolor". So, it is curious that almost no phenomenologist likes to work on technicolor (because disfavored by precision electroweak tests), while its dual Randall-Sundrum version attracted a lot of attention. Luckily LHC will start in a few years, allowing us to restart doing real physics.

17. **woit**
August 31, 2005

Jean-Paul,

No clue whether it will be a best-seller. Intellectually, I think it's a better book than many recent ones of the same genre, because there's less gee-whiz evangelizing for very speculative ideas that probably don't work. In this case the author only does a bit of this, and mostly only for her own work. Every scientist should have the right to write something overly optimistic about their own work for the general public, and I think anyone reading such a thing does so well aware that people tend to have an exaggerated opinion of how wonderful their own children are. About ideas like supersymmetry and string theory, Randall is even-handed, saying the jury is out and explaining what some of the severe problems with these ideas are.

The book is written so as to be in principle understandable by someone with no knowledge at all of math or physics, which is a worthy goal, but I'm not sure how realistic this is. Some simple ideas are explained in great detail, with several different analogies and related stories. I confess to having skipped most of this, since it wasn't aimed at me, and it's hard to tell how effective these parts of the book will be. I just finished my own book, and spent a lot of time thinking about how to explain some sophisticated ideas to as wide a range of people as possible, without using equations. In my case I decided to mostly avoid analogies and just give the clearest short explanation I could come up with. People with more background hopefully will get something out of these explanations, people with less will find some paragraphs baffling, but hopefully just move on to the next paragraph.

The danger with writing a long explanation, with lots of analogies is that people can get lost in these things, and if not chosen very carefully the analogies can do more harm than good. They can confuse people who focus on the wrong part of the analogy and miss the point, or convince people they understand a concept when all they've learned is that X is like Y, where they know what Y is, but still don't have a clue as to what it is about X that is like Y. Anyway, I can't really tell how well Randall succeeds at communicating these ideas to someone who starts not knowing much at all about this stuff, for that you need to hear from a different reviewer.

18. **rrtucci**
August 31, 2005

I don't like analogies too much either; but, physics/math figures (including graphs), I consider vital. (e.g. Penrose's latest book is brimming with figures) Pedro, how many figures will your book have? Even before you answer, I will ask my next question: Do you think such a paltry number of figures is enough? If it's still possible, I think you should add a few more.

19. **woit**
August 31, 2005

Hmm, the book has a few figures, but not enough. How did you know?

Actually I'm about to start dealing with the figures next week, will take your excellent advice to think about smuggling in more.

The figures in Penrose's book are truly amazing, definitely one of the best parts of his book. He's an excellent draftsman, did them himself I hear. Just thinking about the amount of work that went into them scares me.

20. **plato**
September 1, 2005

He's an excellent draftsman, did them himself I hear.

With his tessellations and influence of Escher one understands why I think.

21. **basho.**
September 1, 2005

Hi,

I have been a reader of your blog for some time (as a graduate student in particle physics).

Recently, I began writing too.

I won't be writing for some time due to other commitments.

However, I would like it if you read a few things I had written in the past.

Best,

basho.

22. **Arun**
September 1, 2005

I just wonder why he feels so threatened by a book by the ignorant catering to an undemanding audience.

23. **D R Lunsford**
September 1, 2005

It's utter horseshit.

Markarian 205. WMAP. Connect the dots.

-drl

24. **Shantanu**
September 2, 2005

I just browsed through this book at a nearby bookstore and in the
acknowledgements Lee smolin has been thanked.

25. **Nigel**
September 17, 2005

Just read Warped Passages and think it brilliant. Especially where Lisa writes on page 295 that ‘even if string theory is correct, we are unlikely to find the many additional particles it predicts. The energy of current experiments is sixteen orders of magnitude too low. … because the string length is so tiny and the string tension is so high, we won’t see any evidence to support string theory at the energies achievable in accelerators, even if the string description is correct.’

In addition, she admits the fact that not only are these speculations impossible to test convincingly, they are also extremely vague because there are many variations of the extra-dimensional theories. She remarks on page 456: ‘We now know that extra-dimensional setups can come in any number of shapes and sizes. They could have warped extra dimensions, or they could have extra large dimensions; they might contain one brane or two branes; they might contain particles in the bulk and other particles confined to branes. … Which, if any, of these ideas describes the real world?’

Quantum Gravity Commentary

September 2, 2005
Categories: Uncategorized

During the last couple days, some interesting commentary on quantum gravity has appeared at a couple places on the web. One is at John Baez's latest edition of his proto-blog This Week's Finds in Mathematical Physics. John is mainly writing about operads, but he begins by saying a bit about why he's working on pure math rather than quantum gravity these days:

Work on quantum gravity has seemed stagnant and stuck for the last couple of years, which is why I've been turning more towards pure math.

He mentions the "landscape" and the problems it is causing for string theory, suggesting a reason Susskind's "anthropic" nonsense is getting attention:

perhaps it's because nobody really knows how to get string theory to predict experimental results! Even after you chose a vacuum, you'd need to see how supersymmetry gets broken, and this remain quite obscure.

But instead of spending time bashing string theory, John admirably also has a critical take on his own side of the LQG/string theory controversy, noting that

it has major problems of its own: nobody knows how it can successfully mimic general relativity at large length scales, as it must to be realistic! Old-fashioned perturbative quantum gravity failed on this score because it wasn't renormalizable. Loop quantum gravity may get around this somehow… but it's about time to see exactly how.

Jacques Distler also has an interesting posting about quantum gravity, based on his introductory lecture to the string theory class he is teaching this semester. He explains what some of the generic problems with quantum gravity are, from an effective field theory/renormalization group point of view, and how string theory gets around them. There are also some interesting comments about observables in quantum gravity and the signficance in this context of non-trivial gauge transformations at infinity. Unfortunately, unlike John, Jacques doesn't believe in being very explicit about the problems his side is having (to be fair, maybe that's the topic of another lecture). He does mention background independence and refers to discussion elsewhere, where students could learn about the lack of a non-perturbative formulation of the theory. But his claim that string theory "provides a unique, or nearly unique UV completion" seems to me seriously misleading, and deserving of elaboration lest the uninitiated get the wrong idea.

Jacques does deal in a somewhat peculiar way with a commenter named Jason who is happy with the idea of a quantum gravity theory that can't predict anything at all at the Planck scale. Instead of making the obvious point that believing in a theory that can't predict anything is not what scientists do, Jacques writes

Careful, Jason. A certain self-anointed String Theory gadfly might hear you.

Perhaps Jacques meant to write "self-appointed", since I'd never thought of myself as a "gadfly" until Sean Carroll recently referred to me as such. If I were the sort to self-anoint, I suppose I'd prefer something more serious sounding than "String Theory gadfly", maybe "String Theorist's worst nightmare"…..

Comments

1. **garrett**
   September 3, 2005

   This quote is also from John Baez's TWF post:
   "Math is (at least for me) a less nerve-racking pursuit, since the truths we find can be confirmed simply by discussing them: we don't need to wait for experiment. Math is just as grand as physics, or more so. But it's more wispy and ethereal, since it's about pure pattern in general – not the particular magic patterns that became the world we see. So, the stakes are lower, but the odds are higher. "

   This seems like such a cop out! I mean, yes the stakes are high and the odds low when working with the fundamentals, but if the most qualified people don't work on this — and it does desperately need to be worked on — who will? And what is tenure for if not working on risky endeavors? I do agree the math itself is beautiful, but it's so much better and more important when it's the universe's math one is trying to figure out.

   I guess this just emphasizes the fundamental dilemma with pure math from a physicists point of view, and the dance theoretical physicists and mathematicians do around one another. Quite often mathematicians wandering away from physically motivated math and into their own creations will make something up that is later found to apply to the physical world, but even more often it doesn't. For mathematicians this is not a gamble at all, but for a physicist this is the biggest gamble there is.

   Gadfly — heh, yah, you're a gadfly the same way the kid who said the emperor had no clothes was a gadfly.

   Hey Peter, I don't know if I should ask you this via email, or in comments, or where, but do you know much about the geometry of BRST transformations? I figured you would and I wanted someone to talk with about it. Email would be my first choice.

   -Garrett

2. **Alejandro Rivero**
   September 3, 2005

   First time I head (read) the word "gadfly". So you bite. Well.

3. **Thomas Larsson**
   September 3, 2005

   gadfly: n. a fly that bites horses and cattle
   – Oxford paperback dictionary

   Swedish: broms. Norvegian and west Swedish dialects: klaegg.

4. **Lee Smolin**
   September 3, 2005

   Dear Peter,

   Thanks for this. It is good to lay out the strengths and weaknesses of all the approaches. It is true that the big challenge facing all background independent approaches is showing that the classical spacetime geometry is recovered in the low energy limit. But my own view is that John is too pessimistic. There have been for years results which show that LQG has semiclassical states, and that predictions, such as for the possible deformation of Lorentz invariance, can be gained by studying their excitations. (See summary and references in section 4.5 and 5.1 of hep-th/0408048.) So it is wrong to give the impression there are no results supporting the conjecture that the low energy limit of LQG is GR. And we should not forget the rigorous results that show that LQG and spin foam models are finite theories, so the question of the low energy limit is well posed, something not achieved in earlier theories.

Of course if the theory is right-and we never assume so-we must show more. We must show that the ground state is semiclassical, by solving the dynamics. This is a hard problem, analogous to showing that the ground state of water is a solid. But as this is the focus of attention there are beginning to be significant, non-trivial results on how classical spacetime can emerge from a background independent quantum theory. The best so far are not in LQG, they are the Ambjorn- Jurkiewiczcy-Loll results on CDT, hep-th/0404156. More is coming, I know of 3 papers in preparation by different authors that contain interesting new approaches or results on this problem. So stay tuned and (to John) don't loose heart.

Having said this, I should also say that my own view is that it is not likely that LQG, CDT or anything else now on the table will simply be the right thing. I believe these are all models, necessary steps from which we learn how to do non-trivial calculations in background independent, diffeomorphism invariant quantum field theories. As we gain control over them we are beginning to use the new language and tools gained to address not only quantum gravity but the other big problems such as unification and quantum cosmology. The right theory will solve all of these. And I believe it will do so by featuring a genuine emergence of classical spacetime geometry from something more fundamental.

As for string theory being the unique UV completion, the claimed uniqueness requires imposing two physically unjustified assumptions, 1) that it makes sense to an arbitrarily high energy to separate the spacetime geometry into a fixed background and gravitons of arbitrarily high energy and 2) those graviton states transform under the ordinary Poincare transformations, no matter how high the energy. The first appears false in any consistent non-perturbative unification of gravity and quantum theory including CDT and LQG. The second is much less compelling since we learned that Poincare invariance may be deformed, as in deformed or doubly special relativity theories. These allow the relativity of inertial frames to be consistent with energy and/or momentum cutoffs. At least in 2+1 gravity coupled to matter, we know this is how the theory achieves consistency. And there are indications (far from proofs) that the same will be true in 3+1 when we get the low energy limit sorted out.

Of course, the best news is that AUGER and GLAST will in only a few years tell us the fate of Lorentz invariance. The need to firm up our predictcions before the experiments report is what keeps us working hard on these problems.

-Lee

5. **woit**
September 3, 2005

Garrett,

John Baez is a very good mathematical physicist who knows a lot of physics, so I've been kind of sorry to see that recently he hasn't been directly working on physics. It's a loss for the field. But for most mathematicians, I think it's a good thing if they learn some physics, but then instead of directly working on it, use what they have learned to come up with some new mathematics. What physics is really suffering from these days is a lack of needed new tools, and when mathematicians pursue good new mathematics, they often end up generating the kind of tools physicists need, even if that's not what they were trying to do.

About BRST: I've been thinking a lot about this in recent years, but it's a complicated subject and here's not really the place to say much about it. From the Hamiltonian point of view, BRST is basically Lie algebra cohomology, but in a somewhat exotic "semi-infinite" context. From the Lagrangian point of view, it's related to the whole Mathai-Quillen formalism for constructing Thom forms, explicit representatives of the Poincare-dual of a slice of the group action. In this context I suspect there's more to the relation between the Hamiltonian and Lagrangian point of view than many people think. If you're interested in talking about this stuff, we should do it by e-mail for now, although I should also write more about this here or in another form. I'm hoping once the semester gets started, I'll finally have time for this sort of thing.

Lee,

Thanks for you comment, it's very interesting and helpful. The contrast between the evangelism and refusal to acknowledge problems that characterizes many string theorists, and the much more straightforward and scientific attitude of the LQG community is really remarkable.

6. **Arun**
September 3, 2005

Distler wrote:

http://golem.ph.utexas.edu/~distler/blog/archives/000612.html#AdviceF1

> The principle of Universality — that the same infrared physics allows for multiple, distinct, ultraviolet completions — is what makes effective field theory possible. But it also, ultimately, dooms any attempt to study quantum gravity in a field-theoretic context. Anyone who tells you, "First, I'm going to quantize pure gravity, and then we can add whatever matter and gauge interactions we need, later.?? is trying to sell you a bill of goods. Nothing sensible can come from such an approach.

Comments?

7. **woit**
September 3, 2005

Arun.

I've always felt that ultimately a successful theory of quantum gravity has to also explain where the standard model comes from, partly for the reason Jacques explains, partly for the reason that I don't see how a quantum gravity theory can ever be tested unless it also has something to say about particle physics (I'm less optimistic than Lee about finding tests of quantum gravity using experiments like AUGER and GLAST).

I don't think Lee or other LQG people really disagree with this. If you look at his comment here, he is clear that he sees LQG and related approaches to quantum gravity not as a final theory, but as worth studying to get a better handle on background-independent theories, something that might then point the way to the right theory, one which would tell us about unification. Similarly, the more sensible among string theorists acknowledge that current ideas about getting unification out of string theory don't work, but see further investigation of string theory and its relation to QFT as the most promising thing to think about that might lead to new ideas that do work.

Jacques is right to point to a basic problem that LQG must face up to, but he ignores even more serious basic problems that string theory unification faces. The fact that the vacuum energy is the order parameter for supersymmetry breaking is an even less subtle problem that one could say dooms any attempt to quantize gravity using any supersymmetric theory, including the superstring.

If you believe Jacques that his argument "dooms any attempt to study quantum gravity in a field-theoretic context", so you should give up on field theory, you should also give up on string theory because of the vacuum energy argument (and a host of others….). But once you've given up on field theory and string theory, you may not have any ideas left to work on, which creates kind of a problem.

8. **Wolfgang**
September 3, 2005

Lee,

Lubos Motl wrote a long comment to the same two opinion pieces by John Baez and Jacques Distler on his own blog. Would you dare to comment on Lubos' remarks against LQG, especially the argument(s) that it cannot get the BH entropy right ?

Thank you,
Wolfgang

9. **Lee Smolin**
September 3, 2005

Hi Wolfgang,

Thanks for asking. It is always good to have critics and a few of the points Lubos mentions are correct. But not most. I 'll avoid the temptation to get into a point by point rebuttal and would just ask those interested to read the section on frequently asked questions in my review hep-th/0408048, which covers most of the points. On recent developments, he appears to mischaracterize Rovelli's new paper, gr-qc/0508124, which is to my understanding significant progress. There is a lot in that paper and it also depends on a series of technical developments that directly address some of the issues Lubos raises, for example about observables, that Rovelli and his collaborators have carried out over the last few years. Rovelli et al impose a boundary to define a convenient set of observables, but this is not, as Lubos seems to think, the same as giving up background independence.

The situation with regard to black hole entropy, Immirzi, and quasi normal modes is still evolving and I don't think Lubos's characterization is correct. My own current understanding is contained in a new version we just posted of hep-th/0409056.

I agree that background independent quantum theories of gravity, including LQG, must address the problem of unification. We have some new ideas and results about this that I'm pretty excited about which will be announced at the Loops 05 conference.

-Lee

10. **Arun**
September 3, 2005

So, e.g., that 2+1 general relativity has been successfully quantized is a red herring (done without constraining the matter content of the 2+1 theory); success in a similar program in 3+1 general relativity without constraining the matter content of the 3+1 theory would simply yield another toy model with conceptual and technical insights that might apply to a physical theory? That is, a "quantum general relativity" may exist, but by the Georgi/Distler argument, cannot be a theory of physics?

11. **Gordon Chalmers**
September 3, 2005

Censored again. Ever tried holography, Woit. It is not science fiction, and believe me it does require string theory. Your book panning string theory has got the biggest denial of my entire life.

12. **woit**
September 3, 2005

Gordon,

I deleted some of your comments because they didn't make a lot of sense or add anything to the discussion here. Sorry, but doing this seems to be necessary to keep you and others from filling up this forum with off-topic or non-sensical stuff. If you don't like the fact that this space is moderated, and want to be able to say whatever you want, get your own blog, it's free and easy to set-up.

Before criticizing the book, you might want to first read it.

Unless you have comments that make sense and are on the topic of the posting, please don't submit them, I'll continue to delete them.

13. **ksh95**
September 3, 2005

I don't know much about deformed lorentz symmetries, diffeomorphi-la-shi-siscms, or ringing black holes (although I do have vague memories about some BRST ramblings).

Anyway. . . as a condensed matter theorist I do know about discrete topological objects, spin networks, verticies and edges, etc. I also know that it is impossible to get long-range-anything unless there are extra terms in the lagrangian/free energy. Meaning you have to add interactions or some background pressure or temperature fields. . .something. (I don't need any theorems to tell me that, I know it's true).

Now, I speak condensed mattereeze fluently but I've only taken quantum graviteeze 101. Nevertheless, I will attempt to translate.

You can have spin networks/foams and discrete topological objects all day long, but until you add interactions (whatever that means) or some other background stuff you will never have long range order.

14. **Moshe Rozali**
September 3, 2005

Peter,

Quick factual correction: the vacuum energy is an order parameter for SUSY breaking only in globally SUSY theories (where this does not matter since it is not measurable). In supergravity this is no longer the case, as there are some positive and some negative contributions when SUSY is broken, and vacuum energy can be anything.

15. **Nigel**

September 4, 2005

Peter,

If you click on my name you go into a disussion of the fifth dimension as being the spacetime fabric responsible for gravity:
http://eskesthai.blogspot.com/2005/08/fifth-dimension-is-spacetime-fabric.html

This looks like a more sensible approach than the usual 'consistent theory of quantum gravity' that ST is supposed to provide, since this approach unlike usual ST actually seems to predict things.

Nigel

16. **woit**
September 4, 2005

Moshe,

The argument I gave was certainly over-simplified, you're right that it's only for global supersymmetry that supersymmetry breaking implies positive energy and you get a direct connection between the supersymmetry breaking scale and the vacuum energy.

But if you try and get around this by breaking supersymmetry using supergravity, it's true that you no longer have the above argument, and the vacuum energy is not necessarily positive, but its scale is now typically the Planck energy, no? So, to get a small enough vacuum energy you have to fine-tune the theory for no good reason to one part in 10 to the 120th or whatever, right? Correct me if I'm wrong, I've spent a fair amount of time trying to understand models of supersymmetry breaking, but still find the subject very complicated with all sorts of possibilities. But from what I have understood of the subject, as far as I know no one has an idea for a supersymmetry breaking mechanism that gives a vacuum energy of anywhere near the right scale. As far as I can tell the reason this is so hard comes from the fact that, depending on how you break supersymmetry, the scale of the vacuum energy involves the supersymmetry breaking scale and/or the Planck scale.

Is there some known way to get an appropriately small energy scale when you spontaneously break supersymmetry in supergravity (i.e. without fine-tuning or anthropism)?

17. **Moshe Rozali**
September 4, 2005

Peter,

The statement you had before, about having a positive CC in theories where SUSY is spontaneously broken, would have amounted to falsifying SUSY back in the days when the CC was thought to be exactly zero. Also troubling is the relation between SUSY breaking scale and the vaccuum energy. In any event, one has no choice but to think about things in SUGRA, the vacuum energy is not measurable otherwise (and also, of course, we do have gravity in our universe).

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Sure, no good way of solving the CC problem dynamically is known, in SUGRA or otherwise, as I said the vacuum energy could be anything. If I knew how to answer this I would not post it as a comment on a blog…
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In the context of SUGRA there is really no direct relation between the SUSY breaking scale and the vaccum energy. Also, none of the scales is Planckian- the SUGRA modifications to the vacuum energy are suppressed, not enhanced, by the Planck mass (so that one gets the right limit in the globally SUSY case).
\
In that context there is the old idea of no-scale models where you get zero CC as a consequence of some symmetry (till you break the no-scale structure, which you have to do…). These are typically the models one gets as the low energy limit of flux compactifications.

best,

Moshe

18. **woit**
September 4, 2005

Hi Moshe,

I wasn't trying to imply that the direct relation between vacuum energy and supersymmetry breaking in the global supersymmetry case falsified the idea of supersymmetry, that would be just about a mathematical theorem, and too easy. Of course you have to couple to gravity somehow, which makes the question much more complicated. But this still seems to me analogous to Jacques's renormalization group argument: it's not a rigorous no-go theorem, but shows that generically you've got a big problem of principle, one you have to find a way around somehow.

Thanks for your clarifications, but I'm still confused about some things. For any given supersymmetry breaking mechanism the vacuum energy should be computable in terms of the various energy scales in the problem, including ones we know about (Planck scale, weak scale…), as well as hypothetical ones for things like messenger particles and supersymmetry breaking in various sectors. Do you know of a good reference that explains how this works out in various possibly realistic scenarios? The papers I've looked at where people try and get implications for phenomenology out of various supersymmetry breaking scenarios seem to mostly ignore the vacuum energy problem.

Finally, I'd assumed that KKLT sort of scenarios led to vacuum energies generically at the Planck scale, just because of the often-heard argument that the existence of more than 10 to the 120th of these things implied that some were likely to have small enough vacuum energy. What does set the vacuum energy scale in these scenarios?

19. **Moshe Rozali**
September 4, 2005

The CC is of course a "big problem of principle", and there is always fine tuning involved. In the context of SUSY it is not immediately obvious the required fine tuning is allowed, but it is. Given that, I am not sure if SUSY makes the problem any better or worse.
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In KKLT, or earlier SUGRA models, the CC comes as a difference of two positive definite quantities, each of which is of the order of the SUSY breaking scale (which is typically low in these scenarios). Most of the effort in these directions is to be really careful that EFT methods are justified, so among other things there are no Planckian energy densities involved.
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For reviews you can look at Nilles' famous one, and there is a physics reports on no-scale supergravity (both have over 500 citations, so they are easy to find).

20. **woit**
September 4, 2005

Hi Moshe,

Thanks a lot for the references, that's very helpful and I'll take a look at them.

From what you say about KKLT and earlier SUGRA models, they give a vacuum energy of order the SUSY breaking scale unless you fine-tune, no? That's what I'd very naively expect, if you've managed to avoid introducing Planck-scale effects. Granted it's only in the SUSY/SUGRA context that the whole question of the vacuum energy is well-posed, but this has always seemed to me evidence that there's something fundamentally wrong with the idea of SUGRA based unification, or, less negatively, it's missing something very important.

21. **John Baez**
September 4, 2005

Garrett writes:

>This quote is also from John Baez's TWF post:

> "Math is (at least for me) a less nerve-racking pursuit, since
> the truths we find can be confirmed simply by discussing them:
> we don't need to wait for experiment. Math is just as grand as
> physics, or more so. But it's more wispy and ethereal, since it's
> about pure pattern in general – not the particular magic
> patterns that became the world we see. So, the stakes
> are lower, but the odds are higher. ??
>
>This seems like such a cop out! I mean, yes the stakes are high
>and the odds low when working with the fundamentals, but if the
>most qualified people don't work on this — and it does
>desperately need to be worked on — who will?

It's not clear that quantum gravity (or particle physics) "desperately needs to be worked on". There are a lot of problems, like finding a vaccine for AIDS, that desperately need to be worked on. Quantum gravity and particle physics are different.

First of all, nobody is dying for lack of a solution to these problems. Secondly, these problems will only get *easier* as time passes. Right now our technology lags far behind our knowledge of physics. We are nowhere near making the kinds of machines our current knowledge of physics would let us build. If we let technology catch up for a few decades – or centuries – we'll be in a better position to do experiments and make astrophysical observations that could give us some extra clues. Also, our understanding of mathematics will keep getting better, and it's quite possible there's some math we don't know that's holding us back.

Mind you, I'm *not* saying everyone should wait before working on quantum gravity. I dived in and worked on it for about 10 years, and other people should too – and other people will regardless of whether I think they should! But, there's no need for anyone to work on this stuff who doesn't want to.

So, I've decided that before I get too old I'd like work on some stuff that I'm *sure* will be good. In my work on quantum gravity I came up with some quite nice math, but most of this math will only be *really* exciting if the physical theories I was pondering turn out to be right, or at least a step in the right direction. Luckily, I also have the option of working on math that I'm *sure* is exciting, regardless of how things go in physics.

>And what is tenure for if not working on risky endeavors?

Tenure is for working on big projects that you wouldn't dare start without having job security. This does not imply you have to pick projects that are unlikely to succeed.

> I do agree the math itself is beautiful, but it's so much better
> and more important when it's the universe's math one is
> trying to figure out.

Certainly this is what physicists think, but I'm in a math department, and mathematicians are allowed to hold quite different opinions on this subject.

Mathematicians tend to feel that the most important math is math that would be important *regardless* of what the laws of physics turn out to be: fundamental stuff that applies all over the place. Certain patterns are so powerful that calling them merely "beautiful" drastically understates their importance. We haven't found all of them yet – there are a lot staring us in the face that we're just beginning to recognize – and tracking them down is incredibly exciting.

So, I don't feel it's a copout for someone to work on math instead of physics, as long as they're good at it, they like it, and they try to tackle the biggest, most important problems they can.

In short: it may be less important to work on physics when there's a high chance one is barking up the wrong tree and ones work will wind up in the dustbin of history, than to do math that's clearly good.

This issue, of course, is part of what Peter's blog is all about.

But, I understand the disappointed feelings you are expressing, because physics is a wonderful quest. It's very hard to give it up, even in times like ours when it's hard to tell if real progress is being made.

22. **John Gonsowski**
September 5, 2005

John, could you perhaps take a longer look at Tony Smith's math? The patterns do not get more beautiful. There's even still some physics.

23. **Alejandro Rivero**
September 5, 2005

*First of all, nobody is dying for lack of a solution to these problems. Secondly, these problems will only get *easier* as time passes.*

Now you mention it, it could be worth to point out that, actually, everyone is dying because time passes.

24. **JC**
September 5, 2005

John Baez,

What things could possibly convince you to go back to quantum gravity research?

At this point the only major thing which could possibly convince me to ever go back to doing any quantum gravity research, is if somebody ever finds a way of getting around the anthropic/landscape stuff in string theory without making things any worse. From a quantum field theory perspective, it would be impressive if somebody ever found an easy way to get around the Sagnotti nonrenormalizable 2-loop result for "pure" quantum gravity, without resorting to any paradigms like strings and/or loops.

25. **Yacine**
September 5, 2005

There's no such thing as the anthropic/landscape principle. Since when G is interpreted as an anthropic argument in Newton's Law of gravitation ?? Cause sure you can find an anthropic reasoning for about everything. It's just like numerology, if you look for it you'll find it everywhere.

If you fine tune your theory using so called parameters and you're still able to make predictions that's nothing of a short coming. Sure you won't get the ultimate-theory-of-I-don't-know-what, but you'll do some physics.

26. **Who**
September 5, 2005

JC: *impressive if somebody ever found an easy way to get around the Sagnotti nonrenormalizable 2-loop result for "pure?? quantum gravity, without resorting to any paradigms like strings and/or loops*.
If you haven't already seen it, you might be interested in some work of Martin Reuter and others. This is a recent paper with references going back.

http://www.arxiv.org/hep-th/0508202

This is an earlier paper
http://www.arxiv.org/hep-th/0112089
Towards Nonperturbative Renormalizability of Quantum Einstein Gravity
Abstract: "We summarize recent evidence supporting the conjecture that four-dimensional Quantum Einstein Gravity (QEG) is nonperturbatively renormalizable along the lines of Weinberg's asymptotic safety scenario…"

27. **Who**
September 5, 2005

John Baez: *in short: it may be less important to work on physics when there's a high chance one is barking up the wrong tree and ones work will wind up in the dustbin of history, than to do math that's clearly good*.

Historians might consider the image of a dustbin unfortunate because it makes them out to be Dumpster Divers. And others as well, including physicists, who learn from the successes and failures of the past. Maybe history is not exactly a dustbin.

Apparently it was Leon Trotsky who coined the cliché. I found this account in a NY Times review:

…mutinies in the army, land seizures in the countryside and intrigue and conspiracy in the cities. The Bolsheviks were the chief beneficiaries of the disorder, and in November armed Bolshevik detachments in St. Petersburg and Moscow dealt the tottering provisional Government its death blow. When the Mensheviks protested, a onetime Menshevik turned Bolshevik, Leon Trotsky, scornfully consigned his former comrades to "the dustbin of history."

http://www.nytimes.com/books/98/01/18/reviews/980118.18issermt.html

Aside from the bad press that history gets in John's post, I see a lot of truth and little to object to. It's great when people are free to work on what they are enthusiastic about and perceive as going forward. There are good reasons why a person who sees quantum gravity research as stalled and stagnant for the past couple of years should work on something else. I think that how one sees it must be to some extent a personal vision reflecting one's individual point of view. In my case, I see the field as having been especially active since the May 2004 Marseille conference which John reported in
http://math.ucr.edu/home/baez/week206.html
and therefore as not at all stagnant.

28. **Carl Brannen**
September 5, 2005

Alejandro Rivero: *Now you mention it, it could be worth to point out that, actually, everyone is dying because time passes*.

It's my understanding that neither quantum mechanics nor relativity possess a notion of "now". I recall reading that Einstein made reference to this in a letter he wrote within a few months of his death: *"For those of us who believe in physics, this separation between past, present, and future is only an illusion, although a persistent one."*

Carl

29. **John Baez**
September 5, 2005

JC asked: *What things could possibly convince you to go back to quantum gravity research?*

Well, first of all I'd be unable to completely quit quantum gravity research even if I wanted to, because I have two grad students working on spin foam models: Derek Wise and Jeffrey Morton. But this is a good thing, because I don't want to completely lose track of this field.

Among other things, we're working on Freidel, Louapre, Barrett et al's ideas on how to describe particles in 3d quantum gravity as "spin networks with loose ends" – a wonderful realization of Wheeler's old dream of "matter without matter". Spin networks with loose ends are also mathematically related to D-branes, especially in topological string theory. We want to clarify these ideas using n-categories, following the strategy outlined in last year's quantum gravity seminar at UCR (see my website). Crudely, there's a 2-category with:

particles as objects
spin networks going between particles as morphisms

spin foams going between spin networks as 2-morphisms

and this fact, when worked out in detail, gives a rather beautiful new picture of how matter, gravity and spacetime could fit together – at least in 3 spacetime dimensions! Building a realistic theory along these lines would be a lot harder, since many of the details use mathemagical features special to 3d spacetime.

But, to answer your question, what could get me working *harder* on quantum gravity is some evidence that we can find a mathematically elegant background-free quantum theory that can reduce to general relativity in a suitable limit. I see no reason why such a thing can't be found if we drop the restriction on "mathematical elegance" – but I like things that use beautiful math.

This is precisely why I mentioned Carlo Rovelli's new paper. Getting the two-point function for gravitons on Minkowski spacetime out of loop quantum gravity would be a marvelous bridge between the background-free approach and perturbative quantum gravity. Carlo does it in a rough-and-ready way: can we fill in the details? I'll be in Marseille next February talking to him about this.

I just don't want to burn myself out staying up all night struggling with these issues when I could be having fun doing cool math. If it works, it works. If it doesn't, it doesn't.

From a quantum field theory perspective, it would be impressive if somebody ever found an easy way to get around the Sagnotti nonrenormalizable 2-loop result for "pure?? quantum gravity, without resorting to any paradigms like strings and/or loops.

Someone has pointed out the papers of Lauscher and Reuter, which are quite fascinating and fit together suggestively with the work of Ambjorn, Jurkiewicz and Loll. I urge you to check them out.

30. **Who**
September 6, 2005

John Baez: *Someone has pointed out the papers of Lauscher and Reuter, which are quite fascinating and fit together suggestively with the work of Ambjorn, Jurkiewicz and Loll*. *I urge you to check them out*. That someone was I. Here are the links again:
http://www.arxiv.org/hep-th/0508202
http://www.arxiv.org/hep-th/0112089
Towards Nonperturbative Renormalizability of Quantum Einstein Gravity
Abstract: "We summarize recent evidence supporting the conjecture that four-dimensional Quantum Einstein Gravity (QEG) is nonperturbatively renormalizable along the lines of Weinberg's asymptotic safety scenario…??

Ambjorn Jurkiewicz and Loll point out the connection of CDT results with Reuter's QEG in their new survey paper
http://www.arxiv.org/hep-th/0509010
on the first paragraph of page 24, right before the conclusions.
the connection is that both QEG and CDT get a macroscopic 4D space with an approximately 2D fractal structure at very small scale. It is a striking convergence since the two approaches appear very different.

31. **Wolfgang**
September 6, 2005

Another reason I find these results interesting is because it also works the other way around. If you start with 2D gravity you find that the fractal dimension of your lattice is actually 4. This is an old result of dynamical triangulation/lattice gravity.
Of course it is also true that in 4D gravity the action is concentrated on the 2D triangles.
What is perhaps interesting from this for the spin-foam guys is the idea that collapsing 4-simplices do not necessarily mean everything is wrong.

32. **Wolfgang**
September 6, 2005

I am sorry to post twice in a row.
But I would just like to add that in Regge Quantum Gravity and other lattice gravity approaches (in the Euclidean sector) one finds two phases. In one the 4-simplices tend to collapse and in the other they do not (this is usually called the "well-defined" phase). Changing the coupling parameters gets you from one phase into the other.
If the phase-transition would be 2nd order, the problem of quantum gravity would have been solved 15 years ago.
Recently it has become clear that the phase structure of lattice gravity theories is quite complicated, but the CDT results are definitely encouraging.

33. **Who**
September 6, 2005

IMO you should not worry about posting twice-in-a row Wolfgang. These are interesting things and if you have more to say I hope you will not always wait for others to reply!

34. **John Gonsowski**
September 6, 2005

Two interesting posts in a row by one person is certainly better than two posts in a row by two people that say nothing except it is OK to have two interesting posts in a row by one person.

35. **Not a Nobel Laureate**
September 6, 2005

>Work on quantum gravity has seemed stagnant and >stuck for the last couple of years, which is why I've >been turning more towards pure math.

Some pedantry.

Physics, unlike math, is an experimental science.

The probability of someone discovering the nature of quantum gravity by "pure thought" is on the order of someone guessing the "right" vacuum of the string landscape or all of the molecules in the room where I'm typing this comment ending up in a cubic centimetre in the corner.

Theoretical particle physics was at it's most productive when there was a flood of new experimental data in need of explanation.

When there's a similar flood of quantum gravity experiments, then we may have similar progress.

36. **Arun**
September 7, 2005

I'd put it the other way – we won't know we've had progress until there is a flood of quantum gravity experiments.

37. **Fabien Besnard**
September 7, 2005

"When there's a similar flood of quantum gravity experiments, then we may have similar progress. "

Well when Einstein discovered GR he was not driven by experiments. It is only when the theory was there that experiments could be proposed. Who would have think of measuring the difference in time flow in the gravity field before ?
I think that quantum gravity phenomenon are likely to be even more remote from our intuition. Maybe they are plenty of them we already know of but don't reckognise as such. Einstein said something like "it's the theory which tells what we can observe".

Furthermore, trying to make quantum theory and GR compatible in some way is something someone ought to do, if only for "the honour of the human mind".

38. **Wolfgang**
September 7, 2005

> Well when Einstein discovered GR he was not driven by
> experiments.
An important input to GR was the equivalence principle, derived from empirical evidence.

In contrast, one important input to M-theory/string theory is supersymmetry and unfortunately there is no empirical evidence for it (yet). This may change with the LHC.

I assume that one or more important idea, based on empirical evidence, is needed to find a quantum theory of gravitation.
Maybe we have such evidence already and just do not understand how to turn it into a basic principle, but maybe we have to be more patient until somebody finds the missing clue.

39. **dead kurt**
September 7, 2005

Come on, now: Entertain us

This has gone on for too long.

Move on

40. **Who**
September 7, 2005

Fabien: *Furthermore, trying to make quantum theory and GR compatible in some way is something someone ought to do, if only for "the honour of the human mind??.*

Hello Fabien! I have often enjoyed reading your excellent blog. An extra pleasure to have the opportunity both to read French and get some comments on conferences taking place in Paris from the local perspective.

I agree that upholding the honor of the human mind is a good aim. Have the highest admiration for those rare researchers whose work does this.

Peter has a link to Fabien's blog at the righthand margin, so I wont paste it in here

41. **woit**
September 7, 2005

Will try and come up with further entertaining material, preferably unrelated to quantum gravity, real soon. The semester has just started here and my first class (multi-variable calculus) met yesterday. That and other beginning of semester business has been keeping me pretty busy. Soon we'll return to your regularly scheduled programming….

42. **island**
September 8, 2005

Yacine Says:

> There's no such thing as the anthropic/landscape principle. Since when G is interpreted as an anthropic argument in Newton's Law of gravitation ?? Cause sure you can find an anthropic reasoning for about everything. It's just like numerology, if you look for it you'll find it everywhere.

I'm NOT advocating string theroy, but I disagree, and this is why:

The common denominator in every case is that life only occurs almost exactly between whatever relevant spectrum of "coincidental" potential.

For example, and without **assuming** stuff about inflation and whatnot:

The Big Bang produced numerous principles and laws that have yet to be broken in spite of a lot of projections and theoretical speculation about the eventual and final fate of the usable energy of our expanding universe.

The inevitable heat death of the universe is one of the more obvious projections of an expanding "entropic" universe, but this conclusion doesn't completely justify the fact that the extremely small positive value of the cosmological constant means the big bang actually resulted in a near perfect balance between runaway expansion and gravitational recollapse, which actually puts the universe about as far away from the tendency toward heat death as you can possibly get, and yet still be heading in that direction. The principle of least action says that it is no coincidence that this near-perfectly symmetrical configuration is also the most energy-efficient means for dissipating energy, because this means that tendency toward "heat-death" is most economically restricted to the most-even distribution of energy possible.

The universe actually expresses a grand scale natural preference toward the most economical form of energy dissipation, so if the second law of thermodynamics is telling us that the entropy of our expanding universe increases with every action, then the anthropic principle is telling us that this will occur by the most energy efficient means possible, since the flatness of the universe is one of the many coincidentally **ecobalanced** requirements of the principle.

If the second law of thermodynamics points the arrow of time in an expanding universe, then the anthropic principle determines that time is maximized.

The anthropic principle is relevant!

43. **dan**
September 8, 2005

Hello John Baez,

Since Loll's causal dynamic triangulation appears to have a well-behaved semi-classical limit, with non-trial predictions on the planck scale, shouldn't that excite you to doing research in QG?

dan

44. **Who**
September 8, 2005

dan: *Hello John Baez,*
Since Loll's causal dynamic triangulation appears to have a well-behaved semi-classical limit, with non-trial predictions on the planck scale, shouldn't that excite you to doing research in QG?

that is such a fascinating question it's no fair not to open it up and let others besides JB answer

my guesses are that (1) mathematicians need to refurbish and revitalize their imagination by regular treks into pure math

if it is time for that it wouldnt matter what was going on with modeling spacetime dynamics

(2)I don't think anyone has yet seen how to apply elegant high algebra and categories to Loll's approach.

Loll's picture of spacetime does not even have a uniform-at-all-scales DIMENSIONALITY. Indicators of what dimension you are in correspond to observables. When you are in that place and you want to know what dimension it is in your surroundings, you take a reading from some selfadjoint operator.

so there is no off-the-shelf differentiable manifold to build structure on.

probably there is a new category of object in the briarpatch

I'm just speculating irresponsibly. I think it might take a while before any categorist or algebraist "discovers" Loll CDT

a creative mathematician should never have to justify the motions of his imagination and curiosity

45. **Who**
September 8, 2005

in http://arxiv.org/hep-th/0508202 it looks like you can get away with using a differentiable manifold if you allow it to have an infinite sequence of metrics, all describing the same system but at different scales or energies

quote Reuter et al:*But since the quantum spacetime is characterized by the infinity of equations (1.1) with k = 0 to infinity, it can acquire very nonclassical and in particular fractal features*.

I suppose there is a category of such manifolds equipped with infinite sequences of metrics.

well whether there is or not, Reuter gets similar results to Loll, like largescale 4D going down to near 2D at small scale. and maybe what Reuter is talking about is more amenable to elegant treatment.

46. **h**
September 9, 2005

Some comments, from the perspective of a (more-or-less) mathematician:

"Physics, unlike math, is an experimental science."

No. Math is experimental, too (and always was). Especially since we have computers (labs of the mathematicians…).

"so there is no off-the-shelf differentiable manifold to build structure on."

It would be *very* naive to expect that spacetime is anything remotely resembling to a manifold (of course it resembles, in large scales; but i'm not talking about that), or even that it is off-the-shelf. For starters, it "is" noncommutative (math speak for quantum :). And i like to think that spacetime is an emergent phenomenon, whatever it means.

If Loll's spacetime does not have an uniform dimensionality, i look that as a promising sign…

47. **Who**
September 9, 2005

the program for the Loops 05 conference, October 10-14,
has been posted
http://loops05.aei.mpg.de/

click on "programme" for a list of the talks

48. **Quantoken**
September 9, 2005

h said:

"No. Math is experimental, too (and always was). Especially since we have computers (labs of the mathematicians…)."

Well, computers are made of matters that follow quantum mechanics, so there is randomness and uncertainty, and there is a possibility the computer makes a mistake, like say in one out of every 10^17 instructions. It's an extremely small possibility, but nethertheless computers are not 100% error-free, due to quantum mechanics.

We humen are made of quantum mechanical matters, too. And so we could make mistakes, too. And we make far more mistakes than a computer does. How do you avoid that? You build redundances. If several computers or several person come to the same output, it is very unlikely all simutaneously make the same mistake. But the possibility is still not zero.

Because of quantum mechanics, nothing is completely certain. Therefore, that renders mathematics **an experimental science**.

For example, Perelman proved the Poincare Hypothesis. But we do not know for certainty whether his proof is correct or not. So we invite a couple of experts to exam his proof. But the process is difficult and takes two years for a complete proof read of the paper. And there is always a chance that you made at least one mistake during two years, and draw the wrong conclusion. Several experts draw the wrong conclusion at the same time is more unlikely, but it is still something of none-zero possibility. So, proofs like Perelman are quantum mechanical: you can approach the classical limit of 100% certainty, but you can not reach 100% certainty.

And without 100% certainty, mathematics is rendered an **empirical experimental science**.

I must also point out that the odd of a large group of people make mistakes simutaneously is actually much higher than you would expect. Just look at the 2004 US election, or look at how people at various levels reacted in this labor day hurricane of 2005.

Quantoken

49. **Doug**
September 9, 2005

Perhaps related to Quantum Gravity are two relatively recent articles:

[1] 'Wave acceleration of electrons in the Van Allen radiation belts by Richard B. Horne et al [http://www.nature.com/nature/journal/v437/n7056/abs/nature03939.html]

[2] 'Observational Evidence for Extra Dimensions from Dark Matter' by Bo Qin, Ue-Li Pen, Joseph Silk [http://xxxDOTarxivDOTorg/abs/astro-ph/0508572]

These articles led to the following question that Quantum Gravity and Superstring Theory strive to answer.

How would one recognize dark matter or dark energy if or when detected?

Clearly the torus [a folded doughnut] described in [1] by Richard Horne [et al] is a three dimensional unseen entity of non-optical electromagnetic energy generated by the Earth's magnetic core. These radiation belts trap particles that apparently do not react with optical electromagnetic energy. Some sources state that the primary populations are protons for the inner belt and electrons for the outer belt. [http://farside.ph.utexas.edu/teaching/plasma/lectures/node22.html]

Joseph Silk [et al] refer to dark matter 'betrayed by the gravitational tug' [2] which does not seem to rule the possible association with electromagnetic fields, sequestered protons or electrons.

Could these two papers be related?
Are non-optical electromagnetic fields within the realm of dark energy?
Are 'naked' protons and electrons in the realm of dark matter?

50. **Matti Pitkanen**
September 10, 2005

TGD based interpretation of dark matter relies on dynamical hbar having a spectrum of values and various hierarchies predicted by TGD, in particullar the hierarchy of space-time sheets characterized by p-adic length scales.

The first guess was that dark matter represents a new quantum coherent phase of matter with large value of hbar. Even laser beams could represent Bose-Einstein condensate of dark photons which through decoherence transform to ordinary photons. Large hbar would not mean in perturbative context any change to classical predictions.

During the last year it has however become clear that entire hierarchy of (relatively/partially) dark matters accompanied by a hierarchy of electroweak and color physics is predicted. This hierarchy provides interpretation for the predicted classical long ranged weak and color fields in all length scales as correlates for corresponding scaled down fractal copies of standard model physics. Since the particles in question are very light and interact only via gravitation with ordinary matter, contradictions with experimental facts are avoided.

One could say that TGD Universe is like Mandelbrot fractal which has suffered inversion. Zooming is replaced by its inverse and reveals endlessly new worlds with scaled down particle mass spectra and scaled up Compton lengths.

Light dark matter is predicted to be a grey eminence even in nuclear and condensed matter physics. In particular, in the physics of living matter scaled down color interactions and electro-weak interactions with weak length scale of order atomic length scale or longer would explain among other things large parity breaking effects in living matter. Needless to say, this picture makes sense only in many-sheeted space-time.

Various aspects of this developing vision are discussed here (astrophysics), here (elementary particle level), here (nuclear physics), here (condensed matter), and here (general vision).

See also my blog page TGD diary at http://matpitka.blogspot.com.

Matti Pitkanen

51. **woit**
September 10, 2005

Please, no more comments about your favorite ideas about physics, unrelated to the original posting. Stop doing this here, I'll delete anything more of this kind.

52. **Shantanu**
September 10, 2005

Hi
Peter
Probably you know this already but a video of Joe Lykken's talk
at this year's SSI is online.

53. **John Baez**
September 10, 2005

dan writes:

>Since Loll's causal dynamic triangulation appears to have a
>well-behaved semi-classical limit, with non-trivial predictions
>on the planck scale, shouldn't that excite you to doing research
>in QG?

It does excite me; I think it's one of the most exciting things to come along in quantum gravity during the last few years! Everyone should read this for a less technical description of what Ambjorn, Jurkiewicz and Loll have done – or these for more detail. I talked about this stuff in the issue of This Week's Finds covering the 2004 Marseille conference on loops and spin foams, so you can also read that.

Unfortunately the most important work being done by these authors isn't the sort of thing I'm good at. It involves lots of computer calculations. I've tried to get some computer whizzes interested, but so far nothing has come of it. So, I expect I'll watch from the sidelines for a while.

There's one place I can *imagine* helping out. Their theory makes crucial use of a time coordinate. This should wash out when they take the continuum limit, but it might not – in which case they would be studying not quantum gravity, but some other theory in a different "universality class".

One can investigate this issue numerically. But it would be nice to find a variant of their model which did not make use of a chosen time coordinate, to simply sidestep this issue.

That's the sort of thing I can *imagine* being able to do… but I haven't actually been able yet. Since I'm making so much more progress on various kinds of math, I've been doing more of that.

54. **dan**
September 11, 2005

"There's one place I can *imagine* helping out. Their theory makes crucial use of a time coordinate."

Perhaps you should help them out. To what extent can CDT be applied to LQG or spinfoam, or to what extent can LQG-spinfoam, including calculations of BH entropy, be applied to CDT?

55. **Chris W.**
September 11, 2005

Follow-up on JB's comment: See this lovely set of slides by Renate Loll, from a talk apparently given in 2002.

56. **Chris W.**
September 12, 2005

Slightly off-topic, but *very* interesting:

The Information Geometry of Space and Time
gr-qc/0508108 (Ariel Caticha, SUNY-Albany)

Cosmic Hype

September 9, 2005
Categories: Uncategorized

The latest issue of Astronomy magazine has two articles hyping the landscape/multiverse/anthropic principle and cosmic superstrings. Many well-known theorists are quoted supporting the anthropic principle and the multiverse, including Joe Polchinski, Nima Arkani-Hamed, Martin Rees, Max Tegmark, Alexander Vilenkin, Alan Guth and Lenny Susskind. The only negative quotes are from Paul Steinhardt and David Gross (whose quote is just one word: "virus", that he used to refer to the anthropic principle a couple years ago).

Max Tegmark is quoted as saying the kind of thing that motivated John Horgan's recent NYT Op-Ed piece: "I fully expect the true nature of reality to be weird and counterintuitive, which is why I believe these crazy things." Funny, I thought scientists were supposed to believe things, crazy or not, because of experimental evidence for them. At a Templeton Foundation sponsored conference on the multiverse, supposedly Martin Rees "was confident enough of the multiverse's existence to stake his dog's life." And Andrei Linde "went further, claiming he would put his own life on the line." Horgan might point out that neither Linde nor Rees's dog are in much immediate danger since no one has any plausible idea of how one could ever show that there is no multiverse.

Linde, Vilenkin and Susskind acknowledge that they don't know how to use the anthropic principle to predict anything, with Linde noting the problem of an infinite number of possible vacuum states: "There are many different ways of counting infinities, and we don't know which methods are preferable." Vilenkin is quoted as saying the problem has to do with the lack of the right "statistical techniques", which is kind of misleading, since the problem isn't one of mathematical technique. Susskind actually sounds the most sober of the lot, saying it will be a long time before the anthropic principle can be used to predict anything and "At the moment, it's telling us more about what not to do than what to do." Polchinski on the other hand, goes for maximum hype value, claiming that "The value we now measure for the cosmological constant is precisely what Weinberg predicted." Of course, by "precisely", he means "off by one to two orders of magnitude, much more if you allow not just the cosmological constant to vary."

The article on cosmic superstrings also contains quite a lot of hype from Polchinski, who not only is pushing the idea that the "CSL-1" object is a galaxy lensed by a cosmic string, but that "We're likely to go from one event to 1,000 events in 10 years" and "We're really at the dawn of a new era of science." For more about this, see the latest posting on Lubos Motl's weblog.

Over at Cosmic Variance, Clifford Johnson has a posting about an article in the Guardian about wacky science stories in the media by Ben Goldacre, who runs the Bad Science weblog devoted to this topic. Clifford is quite critical of media coverage of science, but the only examples he gives are ones related to health scares. String theory inspired wacky science stories like the ones in Astronomy aren't mentioned, neither is the fact that here the problem may not be incompetent science journalists, but the fondness for hype of some of his prominent colleagues.

In other popular science magazine news, I learned from David Appell's weblog that the New York Times is reporting that Discover magazine is being sold by Disney to Bob Guccione Jr. Guccione says that he intends to add a humor column to Discover and to create two new print magazines devoted to science. He claims that scientists are kind of like rock stars: "a bunch of people with strong egos and God complexes. That sounds like rock 'n' roll to me." I guess he liked Michio Kaku's recent cover article.

Comments

1. **Dick Thompson**
September 9, 2005

   The bas science media stories I always look for is the annual one where some lab or other has "demostrated faster than light communication" and thereby "overthrown Einstein". The New York Times, continuing its great tradition ("A rocket in space won't work because it has nothing to push against") is a regular venue for these stories.

2. **woit**
September 9, 2005

   Actually, in recent years I think the New York Times science coverage has been much better than what I remember from many years ago. On stories that I know something about they generally don't get all that much wrong. These days, at least in theoretical physics, when there's nonsense in the Times articles, it's often because they're quoting, accurately, some prominent physicist.

3. **cvj**
September 9, 2005

   Hi, Peter, I have to say that I'm less concerned about hype (alleged or otherwise) by string theorists about string theory research in the media than I am about hype concerning health issues. I think that misinformation about the latter is a more immediate concern with regards innaccurate science stories. I'm not obliged to mention string theory in everything I talk about, right? There's more to this universe than just strings…There's branes, for example. … 😉

   -cvj

4. **Maynard Handley**
September 10, 2005

   Perhaps journalists wouldn't write quite such stupid articles on physics if physicists writing textbooks, pop physics books and blog articles didn't think it a point of pride to go on and on about how "wacky" QM is, how "no-one in the world understands it", and similar drivel.
[I'm not attacking Peter here, he's pretty good about this. On the other hand Cosmic Variance goes in for this big time.]

5. **cvj**
September 10, 2005

   > On the other hand Cosmic Variance goes in for this big time.

   What?! Speaking of drivel….

   -cvj

6. **woit**
September 10, 2005

   I agree with Maynard that there's a lot of drivel out there from people who should know better about how wacky and incomprehensible quantum mechanics is (actually I take the extreme point of view that QM is simpler, mathematically deeper and more coherent than classical mechanics). But promoting this kind of drivel isn't something I'd ever noticed anyone at Cosmic Variance being especially guilty of.

Clifford,

Nothing wrong with being concerned about the health-related nonsense in the media. Personally I think I stopped reading such articles about thirty years ago, when I realized that most of them weren't based on any serious science. Maybe I'll start paying attention again as I get older and develop more health problems.

But I still think you should take the problem of hype in physics more seriously. Ideally I think we'd all like the average person to understand what real scientific evidence is, and to be able to evaluate any news article they read about science in terms of whether there is real evidence to back up the claims being made. The danger of physicists promoting highly speculative ideas based on virtually no evidence that are most likely wrong, and continually announcing that "we're at the dawn of a new era of science" is that the credibility of the field can be destroyed. I don't see how you can both explain to people that they should reject ID, astrology, etc. as wishful thinking not backed up by any evidence, while at the same time promoting the multiverse as the latest serious scientific advance.

7. **Not a Nobel Laureate**
   September 10, 2005

   More pedantry.

   In their day,
   dispersion relations, Mandelstam representation, Regge poles, S-matrix theory, N/D method, and bootstrap dynamics, were the fashionably dominant theories.

   In each case, they were superseded by, what was then, "obscure work".

   Progress was possible because the "obscure work", for example Yang-Mills fields, were able to explain some aspect of the flood of experimental HEP data that the popular theories of the day could not.

   The difference with string theory along any current "obscure work" is that there is a drought of experimental observations that could differentiate between the various approaches.
   That is, any of the current "theories" could actually make experimental predictions to begin with.

   Thus we have the sad, if not pathetic, situation of very clever people confusing their opinions with physical reality.

8. **Not a Nobel Laureate**
   September 10, 2005

   The truly interesting work today is instead being done in condensed matter physics and optics.

   Disclaimer: don't work in either field.

9. **island**
   September 10, 2005

   > Linde, Vilenkin and Susskind acknowledge that they don't know how to use the anthropic principle to predict anything, with Linde noting the problem of an infinite number of possible vacuum states: "There are many different ways of counting infinities, and we don't know which methods are preferable.??

   phhht… Try, none. In order to use the anthropic principle to predict anything, you must first give up trying to apply it to anything other than the observed universe where it belongs. Evobiologists could use it as it stands right now in its incomplete and tautologous form to make testable predictions about life, if they weren't anti-fanatically motivated to *believe* that it represents proof of god's existence if we're here for any other reason than by pure accident. Cosmologists could use it in its biocentric form to predict that we won't find life on Mars or Venus, and that we will find life on the bands of spiral galaxies that are about the same age as ours… etc…

   Peter wrote:

   > I don't see how you can both explain to people that they should reject ID, astrology, etc. as wishful thinking not backed up by any evidence, while at the same time promoting the multiverse as the latest serious scientific advance.

   'Dear Holiest man on Earth…

   I appeal to your highest mortal authority in order to ask that you please tell your peeps not to say stuff that hurts the popularity of my crackpot theories… '

   Thanx in advance…

   Luv, Larry
   http://genesis1.phys.cwru.edu/~krauss/papalletttxt.htm

10. **Peter Shor**
    September 10, 2005

    This is pretty off-topic, so I'll apologize, but what happened to all the popular science magazines anyway? There were quite a few more a decade ago, but now, at least on newsstands, it seems to have dwindled to Scientific American, Discover (which is being sold!), American Scientist, and a handful a specialized ones. I don't think Scientific American is as good as it used to be, either. Is this a symptom of the decline of the popularity of science in America?

11. **Not a Nobel Laureate**
    September 10, 2005

    P. Shor wrote: "This is pretty off-topic, so I'll apologize, but what happened to all the popular science magazines anyway? . . ."

    I've noticed the same.

    Scientific American was worth reading every month, now it's worth avoiding.

    New Scientist used to be an excellent British science news publication (it's actually where I first read an article by Penrose about his Twistor program) but is now the National Enquirer / World Weekly News of science reporting.

Science has not only lost popularity in the US, but also respect.

Interesting article here about math and science education in N. America:

http://globeandmail.workopolis.com/servlet/Content/fasttrack/20050906/COASIA06?section=Education

I'll also apologize for going off topic.

12. **woit**
September 11, 2005

Not really off-topic, the question of what if anything is happening to popular science magazines is interesting, and I'm certainly curious what the significance of Guccione buying Discover and getting into the science magazine business will be. Scientific American was one thing that got me interested in science when I was a kid, and part of that was the fact that it was quite serious and intellectually demanding, something I fear is less true these days.

One thing that has affected all magazines in one way or another over the last decade is the internet, and many people are getting information this way that they used to get from magazines. So it's not clear a declining number of magazines means less interest from the public. It's my impression that there are actually a lot more popular science books being published than there used to be, and books aren't affected in the same way as magazines by the internet (just about nobody wants to read a book online). So maybe there is more popular interest in science, but magazines are just less important.

13. **cvj**
September 11, 2005

Peter. I agree with your comment that it is very important. I just happen to think that misleading health-science stories are a lot more damaging to far more people. But these things are all connected, of course. But in an article or post that I'm writing, if I want to mention a few examples of dangerously misleading science reporting, I am more likely to mention examples such as the misrepresenting of the results of drug trials than I am the results of string theory. Call me old-fashioned if you want to, but this will always be the case. This has nothing to do with me being a stirng theorist, (perhaps trying to hide some non-existent theorectical physics community conspiracy to delude the public) and everything to do with me trying to be a responsible citizen of the planet first and foremost.

Cheers,

-cvj

14. **J.F. Moore**
September 11, 2005

Spin and Gear were OK for what they were. Guccione the younger's main problem seems to be keeping his publications in the black. Frankly, Disney managing anything is more financially viable, but they have no compunctions about stong-arming producers, editors, etc to enforce the party line, which is a moderately conservative, family values, thoroughly corporatist, although secular viewpoint.

Given the growing storm over ID, and the opportunity to make science "cool" to a new generation, this change might not be that bad. Incidentally, my favorite articles in Discover have consistently been the "Vital Signs" pieces, because of the way they are written and the fact that I always learn something reading them (I'm not a medico).

I will also point out that while SA has been watered down, they threw down the gauntlet rather dramatically over the assault on darwinism with the "april fools" editorial.

New Scientist has become an embarrassment, almost as bad as Wired now.

15. **J.F. Moore**
September 11, 2005

Sorry, but I have to respond after reading that op-ed piece "Not a Nobel Laureate" linked. Pedagogy is infinitely debatable – personally I think hers is terrible – but the author seems either ignorant or willing to ignore the basic facts involved in her argument about outsourcing. US companies are not outsourcing due to any shortage of talent or because US science students are lousy. They are outsourcing primarily programmers and clerical technical workers because they can be had for much less salary in India and China. This is not only obvious, but can be thoroughly sourced in business literature. To the extent that there is scientific outsourcing, a significant number of those scientists, you will find, were educated in the US.

These comparative tests that rank various countries are a joke. One thing they don't account for is the much more eglatarian basis of US education compared to many other countries, which essentially doom the majority of their population to trade labor at an early age. So our average sample is pitted against a German gymnasium (for example). Administrators and politicians keep their mouths shut about this outrage because they get more money being ranked low than they would if we were near the top.

The difficulty to get students to go to grad school or take a career in science in the US exists because the opportunities just aren't there afterward. Other professions are a much safer bet. Again, that should be obvious.

16. **JC**
September 11, 2005

I agree with J.F. Moore on this issue.

The job market has always been dictated by supply and demand. Brainpower, skills, abilities, etc … have always been a secondary issue when it comes to hiring decisions and/or the clueless HR drones. As long as the demand for particular jobs is significantly greater than the supply of those particular jobs, the PHB's (ie. "pointy hair bosses" in Dilbert) will always pick and choose whoever they want to hire. For a long time engineers, scientists, computer programmers, etc … have been a "dime a dozen" as far as the PHB's are concerned. (The only significant exception to this in the recent past was during the dotcom bubble of the late 1990's). With major outsourcing overseas, scientists and engineers have become more like a "nickel a dozen" or even a "penny a dozen". Many PHB's are mainly interested in their own paycheck and bonuses, and how much their stock is worth.

It's not entirely encouraging for freshman university students, when they see so many computer programmers, engineers, scientists, etc … out of work and/or significantly "under-employed" (ie. an engineer working as a clerk at a Wal-Mart). Parents may be less likely to dole out the cash for tuition when they see the poor job opportunities awaiting their kids. Increasing the number of science/engineering degrees will only compound the problem even further. This would be like the equivalent of significantly increasing the supply WITHOUT a corresponding increase in the demand.

Whether there is a significant correlation between the declining numbers and quality of scientific magazines with the decline of science/engineering jobs, I don't really know at this point. Though it sure seems like many kids are going into areas where the money and jobs are (ie. they're following the money). At the present time it seems to be areas like accounting, real estate, oil/energy industry, etc …

17. **Amitabha**
September 12, 2005

Hope you had a wonderful birthday, Peter! And hope you continue to debate the future of theoretical physics for many years to come.

On the off-topic comments above, J.F.Moore is right in pointing out that outsourcing is based solely on financial reasons. There isn't enough reason to believe that science and technology work is being outsourced to India/China because the workers there are better qualified. In fact many, if not most, of the top science and engineering students in India (and perhaps also China) end up on Western shores for graduate education and further research anyway. On the other hand, some Indian recipients of the outsourcing work do employ top-of-the-class science and technology graduates.

There is a recent trend among the school boards in India to make school education more fun and also easier. I guess time will tell if this leads to more students interested in serious science.

18. **D R Lunsford**
September 12, 2005

"Astronomy" is to "Sky and Telescope" as "Lubos Motl" is to "Socrates". It's been a miserable fishwrapper since the day it first appeared.

-drl

October AMS Notices

September 9, 2005
Categories: Uncategorized

The October issue of the Notices of the AMS is now available on-line. It has an interesting historical article about Henri Poincare, and a short expository article called WHAT IS… a Pseudoholomorphic Curve by Simon Donaldson. Counting these pseudo-holomorphic curves is what topological sigma models do, and they have turned out to have many different kinds of mathematical applications, including the new field of so-called Gromov-Witten theory, as well as several others.

There's also an extensive interview with Fields medalist Heisuke Hironaka. I've heard that Hironaka is a celebrity in Japan, with one of my colleagues once telling me that during a trip to Japan he was surprised to see Hironaka on a billboard selling something or other.

Comments

1. **Not a Nobel Laureate**
   September 10, 2005

   Is there a good exposition about Hironaka's singularity work for the non-specialist. It sounds very interesting and I'd like to learn more about it.

2. **Walt Pohl**
   September 11, 2005

   There was a good introduction in the Bulletin of the AMS, which is available online at http://www.ams.org/bull/2003-40-03/S0273-0979-03-00982-0/home.html.

3. **Chris Oakley**
   September 11, 2005

   HAPPY BIRTHDAY, PETER!

   I was just thinking … if the publishers don't like the title of your book, how about "Born on September 11" as an alternative?

4. **woit**
   September 11, 2005

   Hi Chris,

   Thanks for the birthday wishes!

   I think I'll stick with the current book title…..

5. **Not a Nobel Laureate**
   September 11, 2005

   Walt Pohl wrote:

   "There was a good introduction in the Bulletin of the AMS, which is available online at http://www.ams.org/bull/2003-40-03/S0273-0979-03-00982-0/home.html."

   Great.

   Thank you.

Comedy

September 11, 2005
Categories: Uncategorized

I realize that this is a low form of entertainment, but reading Lubos Motl's blog today has definitely livened up my birthday, which in recent years has been a rather sad occasion. It's hard to say what is the funniest thing there since it's all great stuff, including:

1. Crazed, heavily ideological attacks (here and here) on climate scientists, who unlike Lubos, actually know something about the subject. The comment sections feature mathematician Greg Kuperberg, who has the hilarious idea that it's possible to try and have a rational discussion with Lubos on this subject.

2. Kuperberg's attempts to endear himself to Lubos by attacking the evil Peter Woit, announcing that even though he doesn't understand string theory (something he has shown a perverse interest in demonstrating publicly, besides his comments on Lubos's blog, see here and here) he believes it because "string theorists seem credible, seem talented, and have appointments at top universities."

3. Lubos's response to said attempts, comparing Kuperberg to some of his more "out-there" commenters.

4. Lubos's claims that neither Lee Smolin nor I know what we're talking about when we point out that perturbative finiteness of the superstring is not yet proved beyond two loops, followed by his claim that QFT perturbation series are Borel-summable, nonsense that Jacques Distler then writes in to correct.

Some may object that it's highly unfair to use the fact that some of its practitioners and supporters are out of their gourds to make fun of string theory, but, hey, it's my birthday, so I can do what I want today, right?

Comments

1. **icecube**
   September 11, 2005

   happy birthday!

2. **J.F. Moore**
   September 11, 2005

   Happy birthday Peter. I've enjoyed reading your blog, with the exception of Motl's insult-laden comments, and I look forward to buying a copy of your book when it comes out.

   Internet Schadenfreude is OK, but be sure to have some real life fun as well today!

3. **cvj**
   September 11, 2005

   Happy Birthday Peter!

   -cvj

4. **Who**
   September 11, 2005

   Happy Birthday, Peter!
   Kind of a special one since you have a book coming out shortly.
   Thanks for your blog–the intellectual quality and energy.
   Enjoy the next year, whichever number it is for you.

5. **Luboš Motl**
   September 11, 2005

   Happy birthday! I am very pleased that you like the gift(s) from me. Next year – assuming that there will be something like 2006 (because you probably believe that the last one will be The Day After Tomorrow) – I may try to explain you why the perturbative finiteness of string theory is a proved fact. But be prepared that Lee may be a bit faster in learning this elementary material.

6. **The Statistical Mechanic**
   September 11, 2005

   Peter,

   if I am not mistaken, September 11 is also a special day for Lubos (the day of his thesis defense).
   You two should celebrate together – oh I see that you do already …

   Happy Birthday,
   Wolfgang

7. **Luboš Motl**
   September 11, 2005

   Hi Wolfgang,

   yes, we already celebrate together. 😉 Sorry, in 1 hour we go for a dinner with FM and a renowned climate scientist.

   9/11/2001 was a day that I will never forget.

Best wishes
Lubos

8. **Tom Weidig**
September 11, 2005

You forgot to mention that Lubos also censures i.e. deletes relevant posts / opinions that he doesn't like.

He should better stick to physics than politics. It is amazing to see how his mind falls for logical fallacies once politics kicks in. Not to speak of his boundless naivety regarding policies and its implementability.

I think we should make him a politician for a week and let him leave his protected academic ivory tower…

9. **Moshe Rozali**
September 11, 2005

Happy Birthday!

Moshe

10. **Kasper Olsen**
September 11, 2005

Happy Birthday, Peter!

I'm looking forward to reading your book (even though I'm not sure I'll agree with your conclusion, well…, let's see 😉

Kasper

11. **iccutrr**
September 11, 2005

some entries from Lubos' diary

Sept/10/05: W called me again at 1 in the morning. Wanted me to hear his newest Biblical evidence for String Theory. Says Carl Rove too busy to work on Woit case right now. Carl in a frenzy; frantic with spin control for W, Brownie, FEMA, Condolezza, Chertoff. Et tu, Barbara.

Sept/11/05: Another bad 9/11. Got punched in the mouth, again, after telling dinner companion, a world renowned climate scientist, what a moron he is. His IQ must be less than 90. I think he doesn't like me. How can that be. My arguments were flawless, as usual.

12. **Arun**
September 11, 2005

Happy Birthday!

Here's something to chew on. Over at Panda's Thumb,
http://www.pandasthumb.org/archives/2005/09/dembski_quote_m.html

they're talking about a Richard Dawkins quote:

Richard Dawkins wrote:

"Instead of examining the evidence for and against rival theories, I shall adopt a more armchair approach. My argument will be that Darwinism is the only known theory that is in principle capable of explaining certain aspects of life. If I am right it means that, even if there were no actual evidence in favour of the Darwinian theory (there is, of course) we should still be justified in preferring it over all rival theories??.

Apart from the "(there is, of course)", don't the defenders of string theory sound exactly like the above?

13. **Dick Thompson**
September 11, 2005

Happy Birthday, a little late. Long may you wave and be an irritant to Lubos and his ilk. Sto Lat!

14. **Arun**
September 11, 2005

Yes, this is probably the funniest September 11 since 9/11. Maybe things are looking up, even though we still haven't caught the ultimate responsible for 9/11.

15. **woit**
September 11, 2005

Thanks to all for the birthday greetings, one thing I didn't expect today was to have so many string theorists wishing me a happy birthday. Thanks!

Spent part of the morning cleaning my apartment, which was unusually exciting, since it involved trying out my birthday present from my brother, a little "Roomba" robotic vacuum cleaner. The thing runs around the apartment vigorously vacuuming, doing its best to get into every possible nook and cranny. It only gets into trouble with electrical wires and the fringes of oriental rugs, otherwise does a great job, while providing a certain amount of entertainment.

My brother is in town this weekend, so after playing with the robot, we went out to brunch at a nearby Provencal sort of bistro (Cafe du Soleil). It was a gorgeous day here in New York, and late in the afternoon I rode my bike along the river downtown to meet my brother and mother for dinner. We went out to Bonelick Park, a barbeque place on Greenwich Ave. Had an excellent meal including some wonderful baby back ribs.

After dinner, looking downtown one could see the "Tower of Light" lights rising from the World Trade Center site. These have been turned on specially tonight to commemorate the date. Rode back uptown along the river, quite a beautiful ride at night. From my apartment now, looking out the windows to the south, I can see the World Trade Center lights, although much dimmer up here than from downtown.

Well, I'll stop with this sort of thing now, don't want anyone to get the idea that this blog is going to start having anything like Clifford's charming postings at Cosmic Variance. Enough's enough.

16. **Shantanu**
September 12, 2005

Hi
Peter
belated happy birthday. Always wanted to ask you one thing.
Does your colleague Brian Greene read your blog and your
critiques of string theory?

17. **Michael Sanford**
September 12, 2005

Happy Birthday Dr. Woit.

18. **fooltomery**
September 12, 2005

Happy Birthday, Peter…may your book become a best-seller.

And don't mind Luboš…his conviction that no aroma accompanies his defecations won't survive his denial of tenure at Hahvud.

19. **Robert**
September 12, 2005

I'm late but nevertheless: Happy birthday Peter!

20. **Eric Baum**
September 12, 2005

Peter,

Happy Birthday.
I can't help remarking on the apparent irony between your first two points.
How are readers to distinguish your respect for climate scientists from
the possibility that it derives from their seeming credible, talented, and with
appointments at top universities?

BTW, since I know you are interested in the sociology of Science, I highly
recommend reading Bjorn Lomborg's Reply to Scientific American, which
you can find at http://www.greenspirit.com/lomborg/ScientificAmericanBjornLomborgAnswer.pdf

Eric

21. **woit**
September 12, 2005

Hi Eric,

I actually have no idea who is right in the various climate science debates. What I found laughable was Lubos's absurdly ideological discussion of the issue. From the way he goes on about Bush, communism, etc., etc., it's clear that rationally weighing the evidence is the furthest thing from his mind. Perhaps a sizable part of the climate science community is so politicized that you have to evaluate all research in the field very skeptically, and in any case one certainly shouldn't rely on reputation to decide who is right. But if you want to try and do so, you have to approach the data and the subject seriously, not just engage in ideological ranting.

And please folks, don't start up a discussion of this controversy here….

Shantanu,

As far as I know Brian doesn't read the blog.

22. **Luboš Motl**
September 12, 2005

Dear Eric,

that's really bizarre what you say. Climate science and climatology has always been – and it still is – attracting the weakest students of physics and related fields in most universities I know; and for a very good reason. So if Peter Woit supports the mainstream climate science because it is done by the weak people, it is absolutely consistent with his approach to physics.

Best
Lubos

23. **Eric Baum**
September 12, 2005

Actually, Peter, Lomborg's reply to Sciam does a remarkable job of clarifying
issues all by itself. Lomborg's book went through a long series of environmental issues (GW, species diversity extinction, etc etc) and argued in each case that the popular understanding, the press version, and the pronouncements of many scientists on the one hand were wildly at odds with the actual results in the scientific papers on the other, and described the actual state of knowledge, simply quoting relatively authoritative sources such as UN reports and reviews by top scientists. Scientific American then asked 4 of these top experts to review relevant sections of the book.

Lomborg's reply goes through each of these reviews, answering it line by line. The interesting punchline comes when he gets to the end of the review,

and it's the same in each case. The punchline is: "Now I've gotten to the end of the critique, and you can see that scientist X nowhere in his critique challenged my main conclusion of Y."
Each of these critiques are tendentious in the extreme, seemingly throwing up as much dust as possible to cloud the issue, but none of them even challenges Lomborg's main scientific conclusions (e.g. the best estimates are that species diversity is vanishing at a ridiculously tiny rate, not a vast rate). This is perhaps not surprising, since all Lomborg did in the first place was to quote the scientific papers of these very scientists, but that's what emerges.

I find it highly interesting from a social standpoint that these scientists are happy, perhaps not even fully realizing what they are doing, to make all kinds of comments in public media of dire political outcomes that seem to contradict their actual scientific understanding, but I find it extremely positive that they are nonetheless unwilling to actually make clear false statements in scientific publications, even sciam, and that they still seem to be able to treat the science fairly carefully.

Anyway, since they don't even challenge his conclusions, you can maybe assume they are mostly valid as far as scientific understanding goes, even without trying to sort it out in detail, and his conclusions in every case are rather ridiculously more positive than much of what you may believe.

Eric Baum

24. **Scott**
September 12, 2005

Peter,

Happy birthday yesterday. I find it funny that he invokes Feynman about junk science while feynman himself thought string theory was a waste because of it disconnect from experiment.

Eric,

I remember reading both the scientific american reviews and his online responce and then the authors of the critiques responces(online somewhere) to his online responce. I remember seeing tons of logical errors in his responce before reading the responces which frankly blew him out of the water. Have you read those responces?

25. **Quantoken**
September 12, 2005

Peter:

I could not believe you are so narrow minded. I posted a comment which is on the very topic that you discuss whether Lubos's objection to GWT is reasonable or not. And as always, you erase it because of fear that **TRUTH** be told.

Remind you, this is NOT a crackpot theory that the earth's oil will be depleted. You must have thought it's a numerology number that the gas prices are over $3 a gallon. But it's not numerology and people are paying if from their wallets. And it's just the beginning.

I find it ridiculous that no one is even interested in talking about it. Am I the only one who still lives on the earth, and ever one else, including Peter, has moved to extra dimentions, multiverse, or ont of the the other 10^500-1 landscapes? All the number I cited are official ones and no one even disputed those numbers.

Quantoken

26. **woit**
September 12, 2005

Quantoken,

The question of how much extractable oil is still out there is completely off the topic. The only relation to what I wrote is that Lubos undoubtedly has some ill-informed views on the matter, but he's got ill-informed views on lots of things and I don't want this weblog filled up with an endless discussion of them.

About climate science discussions in general: honestly I know nothing more about these controversies than what I've gathered from reading a few articles in the newspaper. This has convinced me it's a complicated subject, full of people who have a political ax to grind and no real dedication to objectively looking at the science. I'm also convinced that if one wants to, one can read up on this and learn enough to figure out who is doing serious science here and who isn't. But this is a time-consuming business, and I don't have the time for it. If people want to discuss this, I think they should do so in a forum run by someone who actually knows something about this subject, and there seem to be several of those around.

27. **Kyle**
September 12, 2005

Happy Birthday Peter, hope your next year is a fine one.

28. **Eric Baum**
September 12, 2005

Scott,
don't know if I've read *all* the responses, in fact I just did a quick web search, and found a response that I hadn't read by Holdren to Lomborg's response to his Sciam critique. I don't have to get past the paragraph 2 to feel strongly that its more of the same tendentious obscurantism. Paragraph 1 has no content except insult. Paragraph 2 is:

"As my review for Scientific American acknowledged, Lomborg�s energy chapter does contain a number of propositions that are correct (such as the observation that there is large potential in renewable energy sources and energy-efficiency improvements).The problem with the chapter–and the rest of the book as well–is that, as a famously brief review of a long paper submitted to a professional journal once put it, `What is right in this document is not new, and what is new is not right.' "

Yeah, Lomborg couldn't have put it better himself. He doesn't claim anything new. He just claims to be surveying the literature, contrasting popular and political statements with the contents of the scientific papers. Sounds to me like Holdren still doesn't want to challenge anything of content in what Lomborg is saying.

Eric

29. **LM**

September 12, 2005

Lubos Motl says: “Climate science and climatology has always been – and it still is – attracting the weakest students of physics and related fields in most universities I know”

A-ha! Now we see the reason for Lubos’s hostility- he sees the climate scientists as poaching prime recruits for string theory!

30. **Scott**
September 12, 2005

eric this is peter’s blog so I am not going to get into this with you but you read the first paragraphs of a 7 page responce, and decided how the rest of the article was be. Your retarded.

31. **Scott**
September 12, 2005

oops, i meant, “was going to be”. so i guess i’m retarded too.

32. **woit**
September 12, 2005

This kind of argumentation about scientific issues that sheds no light whatsoever on them and degenerates into personal attacks just annoys me. Please discuss this topic elsewhere, I’ll delete any further comments here about this.

33. **island**
September 12, 2005

> This has convinced me it’s a complicated subject, full of people who have a political ax to grind and no real dedication to objectively looking at the science.

As Arun’s ‘food-for-thought’ indicates, this also stereotypically includes most of the same ideologically pre-inclined that commonly argue both sides of the creation/evolution debate.

Politics destroys honesty in science.

Happy birthday.

34. **Eric Baum**
September 12, 2005

Scott,

Not really. If it was going to have content, it wouldn’t have begun with paragraph of gratuitous insults. In fact, it would be hard to imagine a
presumably respected scientist writing a paragraph of gratuitous insults, much less beginning a paper with it, if I hadn’t seen it.
And it follows a previous critique (Holdren’s first one) which was basically
the same deal. And it’s not like Lomborg didn’t have content. He said:
A is my message, and X didn’t anywhere object to it. If X wanted to
come back and say yes he did, and if he was a scientist used to communicating to other busy scientists, he would have placed a topic sentence somewhere near the beginning that said “A is in fact wrong for the following reasons, as the remainder of this paper will show”.

The fact that your correspondance is about on the level of Holdren’s should either make you proud, or serve as further evidence of the intellectual weakness of the position, which do you think?

Eric

35. **woit**
September 12, 2005

Eric,

I’ll leave this comment in place since Scott is the one who started the name-calling. But please, stop. I didn’t want discussion of this topic here because I was pretty sure that it would quickly deteriorate, but I had no idea of how quickly….

36. **Theo**
September 13, 2005

Happy birthday Peter from someone who has read your blog quite a lot recently but been lax in writing any comments so I just wanted to say thanks for the entertainment and insights.

37. **misslemon**
September 13, 2005

I wonder how most “string theorists”* or physicists would feel about having the verdict drawn up on their research by climate scientists, chemists or biologists. Get my point?
*note separate category

38. **CapitalistImperialistPig**
September 19, 2005

Happy Birthday Peter – very belated. Even though I don’t know enough string theory to have a strong opinion, I think it’s very valuable to have critics like yourself to try to keep them honest.

And Lubos – happy Phd-averssary!

The Equation That Couldn't Be Solved

September 13, 2005
Categories: Uncategorized

I just finished reading an interesting new book by astrophysicist Mario Livio. It's called The Equation That Couldn't Be Solved, and the subtitle is "How Mathematical Genius Discovered the Language of Symmetry". Livio's topic is the idea of a symmetry group, concentrating on its origins in Galois theory.

The first part of the book contains a wonderful detailed history of the discovery of the formulas for the roots of third and fourth order polynomials, and the much later proofs that no such formulas existed for general fifth order polynomials. The romantic stories of the short and tragic lives of Abel and Galois are well-told, in much more detail than in other popular books that I've seen. Galois was the one responsible for first really understanding the significance of the concept of a group, and using it to get deep insights into the structure of the solutions of polynomial equations.

The latter part of the book deals with the important role of symmetry in modern theoretical physics, and this is a topic treated in many other places in more detail. Livio gives the standard party-line about string theory, but he does do one very interesting thing. He notices that while string theory implies various sorts of symmetries, e.g. supersymmetry, it lacks a fundamental symmetry principle itself, and this leaves open a very important question. Does physics at its most fundamental level involve a symmetry principle, or are symmetry principles an artifact of our throwing out complexity and only focussing on simple situations that we can understand? Perhaps symmetry is not fundamental, but only an artifact of our limited abilities to understand things. Livio asks several people this question, and gets the following answers:

Weinberg: symmetry might not be the most fundamental concept in the ultimate theory, and "I suspect that at the end the only firm principle will be that of mathematical consistency". (I don't think I really understand what Weinberg has in mind here)

Witten: "there are still missing, or unknown ingredients in string theory" and "some concepts, such as Riemannian geometry in general relativity, may prove to be more fundamental than symmetry."

Atiyah: "We come to describe nature with certain spectacles… Our mathematical description is accurate, but there may be better ways. The use of exceptional Lie groups may be an artifact of how we think of it."

Dyson: "I feel that we are not even at the beginning of understanding why the universe is the way it is."

There is one interesting thing that Livio gets wrong. He explains Klein's Erlangen program of identifying the notion of symmetry with the notion of a geometry, but then says that this is precisely what Riemannian geometry is. This isn't really right, since the non-Euclidean geometries Klein was using are basically homogeneous spaces of Lie groups, whereas Riemann's notion was more general, just insisting that the geometry be locally Euclidean. To unify these two points of view, you need the later ideas of Elie Cartan about Cartan geometries and connections. A related distinction is that Klein was considering finite dimensional symmetry groups, whereas in Riemannian geometry you don't have a global symmetry group. You do have infinite dimensional groups of local symmetries, e.g. the diffeomorphism group, and the gauge group of frame rotations. By the way, a nice article about the early history of gauge theory has just appeared on the arXiv.

My main problem with Livio's book is that he only discusses the groups themselves, and doesn't even try to explain what a representation of a group is. For the applications to quantum mechanical systems and to particle physics, it is this notion of a representation of a group that is absolutely crucial.

Comments

1. **Andreas**
   September 13, 2005

   Interesting review, and maybe Livio avoids to write about specific representations because he intuitively knows that the truly interesting symmetries of nature do not admit any non-trivial representation?

2. **Quantoken**
   September 14, 2005

   Weinberg said symmetry might not be the most fundamental concept in the ultimate theory, and "I suspect that at the end the only firm principle will be that of mathematical consistency??. (Peter said: I don't think I really understand what Weinberg has in mind here)

   Well, I understand. On first thought you would think that mathematical consistency is a necessary condition. If the math is inconsistent then the theory is useless regardless of anything else.

   Wenberg probably belong to the group of people who believes that the ultimate theory can explain everything from **pure mathematical principles**, like an exact formular to calculate alpha.

   If the ultimate theory is pure math, then, probably, being mathematically self-consistent is all that is required to be correct in mathematics. That must be what Wenberg had in mind.

   I do not believe the ultimate theory will be pure math. There will be at least one thing unexplainable in pure math, and that remains physics.

   Quantoken

3. **D R Lunsford**
   September 14, 2005

   The paper mentioned misses the point (as usual) about Weyl's 1918 work – namely that Weyl does NOT find a suitable action at all, because the equations for g are 4th order. In fact it is not possible in 4 dimensions to find a suitable action in this theory. Only first in 6 dimesions does it become possible to find an action leading to 2nd order equations for the g's and have the g and A fields essentially coupled, without arbitrary constants.

   http://cdsweb.cern.ch/search.py?recid=688763&ln=en

   See

   -drl

4. **John Baez**
   September 15, 2005

   Peter Woit writes:

   > My main problem with Livio's book is that he only discusses the groups themselves, and doesn't even try to explain what a representation of a group is.

   It's forgivable not to explain what a representation of a group is. It's unforgivable not to explain what an action of a group is.

   Groups naturally arise as symmetries of sets with extra structure, and in this situation we say the group **acts** on the set. The first and most important way people discovered groups was by finding them acting on sets. We call these "concrete groups". This is the sort of group Galois ran into: the group of symmetries of a field fixing some subfield. I don't see how Livio could discuss Galois theory without at least implicitly talking about group actions, or at least concrete groups.

   Only later was the concept of "abstract group" achieved. This is an incredibly powerful concept. But now, horribly, there are some abstract algebra texts that discuss group theory without a good discussion of group actions – of which representations are a special case.

   Of course, anyone working on quantum theory needs the concept of group representation.

5. **Richard**
   September 15, 2005

   Yes, and sadly if group actions are discussed at all within the context of abstract algebra, it's action on an abstract set with no additional structure. Much interesting theory arises when you attach a topology to an infinite set and consider notions such as orbital almost-periodicity and proximal and distal relations under action by the group. Everyone should know how concepts of periodicity can be supported within the context of co-compact (syndetic) subsets of the acting group, even when the spaces involved are not even necessarily metric spaces.

   Concepts of group actions really should be woven more completely into the math curricula, and much theory is readily accessible even to undergraduates.

6. **JM**
   September 21, 2005

   Given your critics, what book does give a good introduction to group theory? Let's assume for example the reader to be an engineer with only trivial knowledge of quantum mechanics.

7. **woit**
   September 21, 2005

   Unfortunately I don't know of a good book that explains group theory and how to use it in quantum mechanics at an elementary level. Quite possibly such a thing is out there, but I just haven't run across it. If anyone has any suggestions, I'd like to hear about them.

8. **D R Lunsford**
   September 21, 2005

   Not to butt in, but I love recommending books 🙂

   There are any number of good books but the all-time classic for physicists is "Theory of Groups and Quantum Mechanics" by Weyl. This reprint suffers from horrible typesetting but is otherwise a masterpiece.

   For a great introduction to the entire worldview of group theory, read "Elementary Mathematics from an Advanced Standpoint" by Felix Klein, both volumes (short but very intense).

   For a standard textbook, try "Group Theory and Quantum Mechanics" by Tinkham.

   A little formal algebra couldn't hurt, for which I recommend "Abstract Algebra" by N. Herstein.

   There must be modern texts that are readable but I don't know of any.

   -drl

How Much Mathematics Does A Theoretical Physicist Need To Know?

September 13, 2005
Categories: Uncategorized

Mathematician Dave Morrison is giving a colloquium talk tomorrow at the KITP with the provocative title [How Much Mathematics Does A Theoretical Physicist Need To Know?]() It should soon be available for viewing on the KITP web-site, and I'm looking forward to seeing what he has to say.

I'm not at all sure myself how much mathematics a theoretical physicist needs to know, it certainly depends on what they're trying to do. But there does seem to me to be a well-defined list of what mathematics goes into our current most fundamental physical theories, and anyone who hopes to work on extending these should start by learning these subjects, which include (besides the classical mathematical physics of PDE's, Fourier analysis, complex analysis):

Riemannian geometry
More general geometry of principal and vector bundles: connection, curvature, etc.
Spinor geometry
Lie groups and representation theory
deRham cohomology

I'm sure others have different ideas about this….

Update: Dave Morrison's talk is now on-line here. He began his talk my noting that it had been advertised here on "Not Even Wrong", and he put up a slide of my posting and people's comments as an example of people's lists of what mathematics theoretical physicists should know. He did say that that his talk wasn't intended to provide such a list, but rather various comments about how physicists can fruitfully interact with mathematicians.

He began by giving several examples of people who had to construct new mathematics to do physics: Newton, Fourier, Heisenberg, and Gell-Mann. David Gross correctly objected that SU(3) representation theory was already known before Gell-Mann started using it, even though at first Gell-Mann wasn't aware of this. As for more recent interactions, he mainly mentioned the connection between the index theorem and anomalies, as well as various math related to the quantum hall effect. For some reason he decided not to go into the relation of string theory and mathematics, which has been quite fruitful. He did say that he still believes there is some unknown more fundamental way of thinking about string theory that will involve now unknown mathematics. His general advice to physicists was that they should be willing to acquire mathematical tools as needed, but should be aware that if they ask a mathematician questions, they are likely to get answers of too great generality. He ended his talk early, opening the floor to a long discussion.

Comments

1. **Haelfix**
   September 14, 2005

   I would say you have the majority of what is necessary (you probably want some functional analysis as well). Then again there have been several successful theorists who's knowledge in certain of those subjects is sorely lacking (read nonexistant).

   In general people adapt pretty quickly in physics to what they need to know for the problem at hand. For instance, (a subject close to your heart), I have a few friends working on the landscape and they are more or less pouring over extremely sophisticated books on probability and analysis (lots of Bayesian statistics etc).

   So what does a theoretical physicist need above all else? Well, pretty much he/she needs to be a quick learner.

2. **Fabien Besnard**
   September 14, 2005

   I agree that physicists are quick learners. I come from the mathematical side and I'm often amazed how quick physicists assimilate concepts that took me ages to understand. The problem is that they often turn these concepts into something I don't recognize 🙂
   But of course it is most useful to have both insights. By the way, I am very grateful to the physicists who occasionally write formulae in clean mathematical notations, it saves me to do the job myself and helps me a lot in understanding what they say.

3. **Wolfgang**
   September 14, 2005

   I see a problem if Bayesian statistics is considered extremely sophisticated 😎

4. **Pindare**
   September 14, 2005

   As you said Peter, depends very much on what we want to do, but I'd like to add 'dynamical systems', a glaring omission IMHO. There's a nice little book "A First Course in Dynamics" by Hasselblatt & Katok for example.

5. **robert**
   September 14, 2005

   Times have moved on from when J.M. Ziman claimed to have 'contrived to give the appearance of doing research in theory .. with little more serious analytic equipment than could be learnt from … Dirac's Quantum Mechanics', for more than twenty years. Is this an altogether good thing?

6. **Brett**
   September 14, 2005

   Frankly, this choice of topics rather surprises me. I am a theoretical particle physicist, but I was trained as both a physicist and a mathematician. Although I understand all the topics listed, I have almost never found the geometrical ones useful in my research. (I do agree with Lie groups and algebras are ubiquitous though.) If one is interested in incorporating gravity into a theory, then there is surely reason for working with differential geometry. However, there is plenty of interesting work that can be done with no (or almost no) reference to gravitational interactions. What I work on is undoubtedly "physics beyond the standard model," yet it is done mostly within the context of quantum field theory. In fact, the only time I have ever had occasion to use any tools of differential geometry, it was of a much deeper and more complex variety than anything listed here. (And that line of investigation turned out to be a dead end anyway.)

This brings me to my second point. I wonder what exactly it should mean in this context to "know" a subject. My experience has certainly not been that physicists have an easy time mastering sophisticated concepts in mathematics. Most theoretical physicists possess a set of tools drawn from higher mathematics; however, their understanding of the underlying structure of what they're doing is usually minimal. For example, take Lie theory, which is something every theoretical physicist really does need to be familiar with. In graduate school, I took three classes in this subject; however, for the practicalities of physics research, I could have gotten away with just one, the most elementary class in Lie algebra structure. That is about the level of understanding that I see in most physicists.

There is one area of topology that I think theoretical physicists do need to learn, and that is homotopy theory. This has applications in standard model physics and is a much more general subject area than DeRham cohomology. The mathematician in me says that ideally, physicists working on topological issues should master the full machinery of homology theory as well, but this is just not going to happen. Homotopy theory is very intuitive though, and it can be used to analyze just about all the issused that are likely to occur in physical theories living on manifolds.

7. **Hektor Bim**
   September 14, 2005

   This is an interesting idea, because lots of things that condensed matter theorists might consider important aren't here. For example, there is no discussion of numerical techniques, optimization, graph theory, etc.

   I've used all of that a lot more as a condensed matter theorist than I have Riemannian geometry.

   Does theoretical physicist in this context really mean theoretical particle physicist – that is, the only theoretical physicists are those working to extend the standard model?

8. **island**
   September 14, 2005

   robert Says:

   > Times have moved on from when J.M. Ziman claimed to have 'contrived to give the appearance of doing research in theory .. with little more serious analytic equipment than could be learnt from … Dirac's Quantum Mechanics', for more than twenty years. Is this an altogether good thing?

   Only if you believe that anybody's really going anywhere… or aren't they still just trying to rationalize-away the flaw that prevented Dirac from unifying QM and GR…?

   http://www.lns.cornell.edu/spr/2005-06/msg0069755.html

9. **Kaveh**
   September 14, 2005

   Why did you omit dynamical systems and topology?

   I would encourage knowing (real) analysis (spectral theory in particular and maybe advanced probability theory) and modern PDE's but I understand that they have so much overlap with everything else that you might not need to take a course to learn them.

10. **Brian**
    September 14, 2005

    I assume there is a mantra for theoretical physicists:

    Must … learn … more … math!

11. **Dick Thompson**
    September 14, 2005

    A little symplectic topology; how to calculate characteristic classes, what is an almost complex structure and when, and so on. Goes with the homotopy theory. Any finitely presented group is the fundamental group of some symplectic manifold; stick that in your landscape and smoke it.

12. **woit**
    September 14, 2005

    Just to clarify. I think different kinds of theorists studying different aspects of theoretical physics need different kinds of mathematics. The list I gave was one for the specific case of particle theorists interested in how to better understand the standard model and find some way of improving upon it.

    Some people mentioned more topology, especially Brett who mentioned homotopy theory. I'm very fond of topology myself, and had to resist putting K-theory on the list. I still think though that deRham cohomology is the basic thing most theorists should know about. It gives specific integral formulas for topological invariants, and most of the topological invariants that come about in standard model related QFTs can be computed in terms of cohomology, not needing homotopy (especially important is Chern-Weil theory, which computes topological invariants of bundles in terms of connections and curvature). There are some places where the fact that a fundamental group is Z2 is important, but otherwise cohomology covers most of the topology physicists need.

13. **John Baez**
    September 15, 2005

    I'm surprised that nobody has mentioned this story yet:

    When Weisskopf was asked "how much mathematics does a theoretical physicist need to know?", his answer was:

    More!

14. **Tony Smith**
    September 15, 2005

    What "new mathematics" was "constructed" by Heisenberg in order "to do physics" ?
    Bear in mind that according to http://mooni.fccj.org/~ethall/quantum/quant.htm
    "… Hilbert suggested to Heisenberg that he find the differential equation that would correspond to his matrix equations. Had he taken Hilbert's advice, Heisenberg may have discovered the Schrödinger equation before Schrödinger. …".

    Tony Smith

http://www.valdostamuseum.org/hamsmith/

15. **Hongbao Zhang**
September 15, 2005

On Differential geometry(DG) in particle physics, I think tranditional particle physicist's language is not easy to understand. DG can make many topics more clear. The examples include
Neother theorem's presentation
angular momentum for spin nonzero particle
which both involve the Lie derivative concepts and Lie group

16. **Nigel**
September 15, 2005

"… several examples of people who had to construct new mathematics to do physics: Newton, …"

Peter, Newton uses standard geometry of 300 BC (Euclid, "Elements of Geometry" stuff) in his Principia, not calculus. There is no evidence he invented calculus for the physics in Principia. Newton also delved into Biblical numerology and alchemy. Archimedes' "Method" shows that Archimedes used a type of calculus to work out the volume of a sphere and cylinder, but merely used that as scaffolding to help him work out a geometrical proof. Although both Archimedes and Newton worked on calculus, both were careful to express all results for physics classically. They didn't need to provide critics with ammunition to sneer at by taking a sum of an infinite number of infidesimal slices to get a result. It was better to use geometry which people understood and respected, not a newfangled approach.

The same happened with Einstein. Professor Morris Kline describes the situation after 1911, when Einstein began to search for more sophisticated mathematics to build gravitation into space-time geometry:

'Up to this time Einstein had used only the simplest mathematical tools and had even been suspicious of the need for "higher mathematics", which he thought was often introduced to dumbfound the reader. However, to make progress on his problem he discussed it in Prague with a colleague, the mathematician Georg Pick, who called his attention to the mathematical theory of Ricci and Levi-Civita. In Zurich Einstein found a friend, Marcel Grossmann (1878-1936), who helped him learn the theory; and with this as a basis, he succeeded in formulating the general theory of relativity.'

(M. Kline, Mathematical Thought from Ancient to Modern Times, Oxford University Press, 1990, vol. 3, p. 1131.)

17. **Matti Pitkanen**
September 15, 2005

I would add to the list of interesting mathematics some new items (I confess immediately that they relate to my personal favorite theories;_)).

No one has mentioned number theory yet. The zeros of Riemann's Zeta seem to be closely related to quantum chaotic systems and conformal symmetry and Riemann hypothesis could relate in a very deep manner. The generalization of quantum physics to p-adic number fields and fusion of quantum physics in different number fields to single super structure is highly attractive idea with which I have worked for a more than decade now. The notion of primeness is incredibly general and the direct analogy with the notion of elementary particle might serve as an inspiration for an imaginative theoretician. For instance, the construction of infinite primes can be interpreted as an iterated second quantization of super-symmetric arithmetic quantum theory.

There has been no mention of infinite-dimensional geometries, which might be of interest for string model builders. Loop space Kaehler geometries are essentially unique from the mere requirement that they exist mathematically as shown by Freed. Kac Moody symmetries as isometries guarantee the existence of Riemann connection. Curvature scalar is however infinite which suggests that strings are quite not enough.

This inspires the idea that physics might be unique from the mere existence of Kaehler geometry and corresponding spinor structure for infinite-dimensional configuration space, "the world of classical worlds". My own bet is that the space of 3-surfaces in certain uniquely determined 8-D imbedding space is the correct guess and leads to a physics unique from its mere mathematical existence. This approach is a diametric opposite for the M-theory approach where imbedding space can be almost anything and leads to landscape problem.

A third fascinating branch of mathematics not yet mentioned relates to von Neumann algebras. The so called hyper-finite factors of type II_1 are obtained by requiring that the infinite-dimensional unit matrix has unit trace. The Clifford algebra of a separable Hilbert space realizes this algebra. The Clifford algebra of spinors of an infinite-dimensional "world of classical worlds" can be regarded as direct integral of these factors. Hyper-finite type II_1 factors have fascinating connections with conformal field theories, knot and braid theory, 3-manifold invariants, etc..

In wave mechanics factors I_n, n=1,..,infty, appear. In algebraic quantum field theory factors of type III_1 appear and possess very counter-intuitive properties. Could it be that hyper-finite factors of type II_1 provide the solution to the problems of QFT via the geometrization of the fermionic Fock algebra in terms of gamma matrix algebra for the world of classical worlds?

Matti Pitkanen

18. **Not a Nobel Laureate**
September 15, 2005

Another question.

"How much experimental physics does a theoretical physicist need to know?"

19. **Eric Baum**
September 15, 2005

This brings to mind the tale of the 3 physicists in the hot air balloon who realize they are lost. They come over a hill and see some campers. They shout down: "where are weeeeee".
No answer, while they continue to drift, until finally they are just about out of sight, comes the reply "in a balloooon".
One physist says to the others, "they must be mathematicians. It took them forever to answer, the answer was manifestly true, but it was totally useless."

20. **fooltomery**
September 15, 2005

When asked why he became a mathematician, Polya is said to have replied:

> "I am too good for philosophy and not good enough for physics."

21. **Thomas Larsson**
September 16, 2005

It seems appropriate to mention this well-known quote, which I have seen attributed to Dirac, Wigner and Einstein, and perhaps others:

"God is a mathematician."

Of course, this statement has a dual formulation, which I came up with myself:

"Physics is divine mathematics."

22. **Arun**
September 16, 2005

"Like the crest of a peacock, like the gem on the head of a snake,
so is mathematics at the head of all knowledge."

-Vedanga Jyotisa (c. 500 BC, India)

23. **Chris Oakley**
September 16, 2005

Not really on topic, but did any of the UK readers see the Horizon program about Stephen Hawking on BBC2 last night? It featured, amongst many others, Leonard Susskind as Hawking's bete noir in disagreeing about information loss in black holes. The producers of the program seemed to be expecting Hawking to keep churning out brilliant ideas despite (a) being over 60 and (b) having a nasty wasting disease. This seemed a bit unfair to me.

24. **Wolfgang**
September 17, 2005

Maybe we just need to learn a different type of trigonometry …

25. **robert**
September 19, 2005

Advice from a bygone age: J.E. Littlewood responded to a stduent's request for a background reading list with a curt
'Nothing is neccessary – or sufficient'

Serge Lang 1927-2005

September 15, 2005
Categories: Obituaries

I just heard that mathematician Serge Lang passed away this past Monday. Lang was a well-known number theorist and algebraist, a member of Bourbaki and recipient of the 1960 AMS Cole Prize. He was a professor here in the Columbia math department for fifteen years, leaving in 1972 for Yale, where he spent the rest of his career. Lang was an amazingly prolific author of mathematics textbooks, and famous for his outspoken views and "files" on various controversies. In recent years some of these had become increasingly cranky, especially on the topic of AIDS. He was truly one of the most remarkable characters of the mathematics research community.

Update: There's an obituary at the Yale Daily News (thanks to David Goss for pointing this out).

Update: The New York Times ran an obituary of Lang today.

Comments

1. **icecube**
   September 15, 2005

   Oh boo…he was quite a character. His algebra book is, like, the best algebra book in the world ever (reading one of his differential geometry books at the moment as well, it's quite nice).

2. **John Baez**
   September 15, 2005

   When I was a postdoc at Yale sometimes I would stay up late working in my office. Serge Lang was the only person I'd see, regularly working past 2 am, sometimes rushing off to the copier room to print out copies of his "files" to send to people.

   He was quirky but fascinating – incredibly energetic, too.

   When I was his TA for calculus he insisted that derivatives could be taught in 15 minutes: explain the idea, give the definition, show how to compute some examples… done.

   I heard he would take a trans-Atlantic cruise each summer and produce a book during this time. I'm not sure that's true, but it would help explain his prolific writings.

   It's a pity he's gone.

3. **Daniel Doro Ferrante**
   September 15, 2005

   I thought myself Linear Algebra, Complex Analysis and some Differential Geometry from Lang's books.

   As an undergrad, S. Lang, M. Spivak and T. Apostol were my "mathematical icons"!

   It's really sad to know that he's gone…

4. **James Borger**
   September 15, 2005

   This is sad. I've been spending a bit of time lately looking at some of his books, and he was on my mind a lot. I remember one time when I was a grad student, I was standing next to him at tea while he was explaining to a first-year that analysis is just "number theory at infinity". I said Come on, that's not true. He immediately turned up the volume, challenging me to stop bullshitting and give an example. I said OK, p-adic analysis, and then walked away. But I've always wished I had stayed to see what his reaction would have been. We need more trouble makers like him.

5. **Walt Pohl**
   September 15, 2005

   I guess that I'm in a minority here, in that I hated most of his books. I thought his Real Analysis was okay, but his Algebra is by and large a horrible book (it does have a few good chapters). I'm told his Differentiable Manifolds book has a truly outstanding number of mistakes in it.

   I'm sorry to hear that he has passed, though.

6. **Edgar van Tuyll**
   September 16, 2005

   He told me that when he read about a subject, he would write down everything he could find out about it. At the end, this could be made into a book. When he saw that you were impressed by how quick he was, he said that he did not have the gift of physicists who could watch an experiment and immediately visualise the equations that explained it. His was an uncompromising mind, and in non-mathematical conversation, he often asked you to define the words you used, or to perform a calculation in the field in the middle of a pop science explanation he was giving at a dinner. I have to thank him for the gift of a lifetime passion for mathematics he gave me, and I miss him and to know that I shall never stumble upon a new book by him in a bookshop makes me sad.

7. **AJ**
   September 17, 2005

   Lang used to spend every summer at Berkeley. Arguing with him at tea time was enormous fun. I'm going to miss him.

8. **Yevgeny Vilensky**
   September 17, 2005

   I was an undergraduate at Yale. I never had Lang for a prof., but I knew him through the math club, of which he was an advisor. He was a tireless advocate of precision and rigor even in every day speaking. While I think that sometimes went too far, it was good that there was someone pushing things in that direction in a world in which people were losing rigor in their everyday speech more and more. I used to argue with him all the time. His defense of Shafarevich when Shafarevich was repressed by the Soviets and a second time, when the NAS to its discredit, decided to expel him for his views was highly laudable. I think that he was a bit unfair to Sam Huntington, and I sometimes disagreed with his views on pedagogy (he thought that his way to understand a subject is the only correct way). But surely, he will be missed by everyone who knew him (save for Sam Huntington, perhaps).

9. **Christian Claiborn**
September 21, 2005

He was at Berkeley this summer, too, and gave a lecture on analysis to undergraduates before he left. He was in good spirits and was cheerfully dismayed by our ignorance. I was honored to meet him.

10. **John C. Howe**
September 21, 2005

This past year, Professor Lang gave a wonderful presentation to the San Jose Mathematics Circle at San Jose State University.

He led the circle in an inspiring discussion of the numerical constant Pi. He opened new avenues of thought on this very old topic. In his leading the Circle, we could feel the presence of his greatness.

While Professor Lang was one of the world's great mathematicians, he was also at ease in working with the youngest students in the Circle. He had a natural sense of humor that immediately caught the attention of young people.

A student that I mentor, Bowei Liu (who was then a gifted sixth grader), was influnced by his gentle nature and fine-tuned art of using a version of the Socratic Method to gain understanding of a deep mathematical topic. Bowei went on to win the Mu Alpha Theta Achievement Award at the Synopsis Silicon Valley Science and Technology Championship, across all grade levels 6-12, for his researched-based project on Pi entitled "Experimental Mathematics: The Relative Efficience of Estimating Pi."

Whether it was Bowei, or someone like myself, who struggled to understand his "Algebra" textbook, we have all benefitted by his remarkable lifetime of achievement in mathematics. He was a "magister."

My life has been enriched by having had the opportunity learn both about mathematics and dissent in a free society from the late Professor Serge Lang.

11. **david friedman**
September 26, 2005

Serge was my advisor as an undergrad back in the early 1980's. He was a tremendous moral, intellectual and musical influence. I'll never forget that after we first met over dinner at the Law School, upon hearing that I knew no Baroque lute music, he walked me over to Cutlers records on Broadway and bought me an album of Julian Bream and Peter Pears.

Sadly, I was out of touch with him for many years, although I tried to get in touch with him at the beginning of the Summer. I will really miss him.

One of my favorite of his rejoinders: "that is a true statement that happens not to be relevant."

12. **Alexander Russell**
September 26, 2005

Some ill-informed and ignorant person sated above: "In recent years some of these had become increasingly cranky, especially on the topic of AIDS." Lang did not become cranky: he just asked questions concerning the anomalies amd contradictions regarding the redundant and unproven 'HIV/AIDS' hypothesis. Richard Horton of The Lancet could never answer any of Lang's questions regarding the 'HIV' fraud: Horton, as well as Robert Gallo, Luc Montagnier and David Ho have never proven that 'HIV' exists or causes 'AIDS'. In fact no one has: all the theories on 'HIV' are bassed assumptions and suppositions which have never been scientifically proven. Lang will be remebered for 'Challenging' the current 'HIV' myth making mathematical models!

13. **M. H. K.**
September 30, 2005

I suspect that I am the only person who had Professor Lang wait on the telephone. Early in the 90s our Departmental secretary left me a message Professor Lang wanted me to call him. We never met and I was excited that he knew me to call me! At that time making a long distance call from my office was a little hassle. I went to the secretary and told her that I wanted make a long distance call and gave her the number. I thought that she would inform me when she was making the call. A little after she called me in my office to tell me that Professor Lang was on the phone in her office. I was stunned that I had him waiting on the phone. I think he was a little annoyed with me but his attitude changed right away and he asked me for a reprint of one of my articles.

Later I received an e-mail from one of his students stating that Professor Lang received the reprint I sent him and wanted me to send him other reprints as well. Professor Lang, I think, immediately had the student e-mail me again that he wanted preprints as well as reprints. I will always remember and be appreciative of his interest in my work.

14. **Marvin Jay Greenberg**
October 3, 2005

I got my B.A. at Columbia in 1955 and went to Princeton for graduate study in math. In my first year I studied with Lang's thesis advisor Artin (among others). Artin returned to Germany after that year and asked Lang, who was then on the Columbia faculty, to commute to Princeton to continue teaching algebraic geometry, which Artin had introduced to a few of us – an extraordinary request by Artin and an extraordinary acceptance by Lang.

At that time algebraic geometry was not yet fashionable. and after a while, I was Lang's only student in that subject, which he continued to teach. Two years later, I wrote a Ph.D. thesis under his direction, proving a conjecture he had made. He was a perfect advisor for me, sensing exactly when to be nice and when to yell at me, inspiring me with his passion for the subject and his uncompromising intellectual honesty.

I will forever be grateful to him.

Dangling Particles

September 18, 2005
Categories: Uncategorized

Lisa Randall has an Op-Ed piece in today's New York Times entitled [Dangling Particles](). The title seems to have little to do with the piece, but I suppose it is a play on words on "dangling participle", a term for a sort of faulty grammar. Randall's topic is the difficulty of communicating scientific topics, and her comments on the problems caused by scientist's different use of words and by the complex nature of much science are true enough and unobjectionable.

But I still find the sight of a string theorist lecturing the public on how to properly understand science to be a bit jarring. Randall tries to claim that the difference between the colloquial usage of the word "theory" and the way it is used by scientists is a source of problems with the public understanding of science. She writes

For physicists, theories entail a definite physical framework embodied in a set of fundamental assumptions about the world that lead to a specific set of equations and predictions – ones that are borne out by successful predictions.

Yet she keeps on referring to "string theory", although the subject is distinctly lacking in specific equations and predictions (she does note that "theories aren't necessarily shown to be correct or complete immediately", but the problem with string "theory" is not that we don't know whether it is correct or complete, but that it isn't really a theory, rather a hope that one exists).

Instead of devoting their time to writing for the public about the scientific status of issues that they're not really experts in (e.g. global warming), it seems to me that string theorists would do better to first address the outbreak of pseudo-science now taking place in their own subject. When the intelligent design people get around to noticing how much of the highest level of research in one of the traditionally most prestigious sciences is now being conducted without any concern for falsifiability or traditional norms of what is science and what isn't, the fallout is not going to be pretty.

Update: Sean Carroll has a posting about the Randall Op-Ed piece over at [Cosmic Variance](). He quotes approvingly Randall's claim that Intelligent Designers don't make a distinction between the colloquial usage of "theory", meaning an idea not necessarily better grounded than a hunch, and the way real scientists use the term. As for whether string theory deserves to be called a "theory", here's a quote from Gerard 't Hooft (from his book *In Search of the Ultimate Building Blocks*):

Actually, I would not even be prepared to call string theory a "theory?? rather a "model?? or not even that: just a hunch. After all, a theory should come together with instructions on how to deal with it to identify the things one wishes to describe, in our case the elementary particles, and one should, at least in principle, be able to formulate the rules for calculating the properties of these particles, and how to make new predictions for them. Imagine that I give you a chair, while explaining that the legs are still missing, and that the seat, back and armrest will perhaps be delivered soon; whatever I did give you, can I still call it a chair?

Update: Lubos Motl has some [comments]() about Randall's Op-Ed piece and about my posting. As usual, I come in for a fair amount of abuse, but at least this time I'm in good company ('t Hooft's views are characterized as "just silly").

Update: John Baez points out that the article is now up at the [Edge web-site](). Over at Pharyngula, there's a posting about [Danged physicists](). Evidently biologists are not amused at all about Randall's comments about evolutionary biology. They seem to think that string theorists are arrogant and prone to going on about things they don't really understand.

Comments

1. **Nigel**
   September 18, 2005

   Peter, she does comment on this issue in chapter 4 ('approaches to theoretical physics') of her book 'Warped Passages' although you didn't focus on that in your review of the book. Lisa there says candidly: 'The choice could also be phrased as "Old Einstein vs. Young Einstein".' She then launches into a discussion of string theory, saying that it seems the only way to consistently unify quantum mechanics and general relativity.

   The two issues with string theory, that there is no hope for tests it because the energies required are 16 orders of magnitude too high, and that there are no end of differing versions of string theory (different brane models, different explanations for the different strengths of gravity and the other fundamental forces). Thus string theory with its 10/11 dimensions and fanciful claims is a bit like Alice in Wonderland, as Lisa admits (it was her inspiration).

   Witten is the man who seems to be responsible for the current mess, and it would be nice to see him defend his arm-waving propaganda claim that string theory 'predicts gravity'.

2. **logopetria**
   September 18, 2005

   Of course, there's another distinct technical use of the word "theory", namely in mathematics, where *X theory* can just mean 'the general field of study that addresses X'. For example, graph theory is the study of graphs, not a particular theorem about graphs. Maybe this is a better way of understanding the name 'string theory' – it's just the whole mathematical field of study of strings, and what they'd be like if they existed. (Although I'm not suggesting this is how string theorists think about it themselves).

3. **Nigel**
   September 18, 2005

   This 'string theory' issue was the problem 100 years ago with 'ether theory'. There was no single ether theory, there were multiple versions, all contradictory ad hoc models, and not one predicted quantum theory or radioactivity. This was why 'it' was really abandoned. You had to have faith to believe in all the ad hoc models, because they were impossible to experimentally prove (as per Michelson-Morley test), just as string theory can't be proved today. The genius of relativity was bucking the mainstream of his day, by making testable predictions.

4. **Scott**
   September 18, 2005

   > And string theory is not about strings that you tie around your finger that are made up of atoms; strings are the basic fundamental objects out of which everything is made.

   and she was saying something about better communication?

5. **dan**
   September 18, 2005

Would you consider the dectection of SUSY-particles or extra dimensions at LHC a successful prediction of string theory? what about deviations from GR some forms of string theory predict?

6. **woit**
September 18, 2005

No, the things you mention may be consistent with string theory but aren't predictions of string theory. String theory doesn't tell us what the properties of superpartners will be, how many extra dimensions of observable size there are, what the deviations from GR are, etc. etc. It makes no real predictions: zero, zip, nada.

7. **jobhunter**
September 18, 2005

Given how it is easier today to get a professorship at a major American university as a string-theorist than as a non-string theorist (witness Sean Carroll's recent public praise for string theory just before entering the job market), I do believe that a genuine legal case could be made that this constitutes discrimination on the basis of religion.

8. **woit**
September 18, 2005

Hi Jobhunter,

That's very funny. String theorists will whine that they don't get all the jobs, phenomenologists and cosmologists get some too. The one religious belief that now makes you truly unemployable in physics departments in the US is Einstein's belief in the existence of a deep mathematical and geometrical structure to physical reality.

9. **Arun**
September 18, 2005

Motl is now using the Dawkins argument, namely

Richard Dawkins wrote:

"Instead of examining the evidence for and against rival theories, I shall adopt a more armchair approach. My argument will be that Darwinism is the only known theory that is in principle capable of explaining certain aspects of life. If I am right it means that, even if there were no actual evidence in favour of the Darwinian theory (there is, of course) we should still be justified in preferring it over all rival theories??.

10. **anon**
September 18, 2005

A confusing writer criticizing confusing writing? I find myself re-reading Lisa's convoluted sentences and saying, heck, she could have expressed the very same thought with a much simpler, clearer sentence. Also, I find that adjacent sentences in her article are often curiously disconnected. I'd say she has a problem with dangling sentences and dangling thoughts. The title of the piece is emblematic of this problem: it's left dangling and unexplained.

11. **absolutely**
September 19, 2005

Perhaps string theory is more of a "system of theory" than a single theory. By that I mean a generator of theories for specific domains.

12. **Thomas Larsson**
September 19, 2005

Dan,

Would you consider the **non**-dectection of SUSY-particles or extra dimensions at LHC as an indication that string theory is wrong? Remember that for almost 20 years, Ed Witten kept stating that string theory makes one prediction, supersymmetry (and one postdiction, gravity). For some reason, he seems to have stopped making these statements now that the LHC draws near…

Personally, I would consider the discovery of sparticles a strong hint that string theory may be on the right track. However, I would like hard evidence; a sign-bug in the Schoonship program is not good enough for me.

13. **Chris Oakley**
September 19, 2005

IMHO the New York Times and Lubos are going to be the main agents for bringing about the end of Superstrings. The former runs articles on Superstrings with alarming regularity, but in each case it will be evident to anyone who can be bothered to read it (i) that there has not been any significant progress since the previous article and (ii) that after more than 20 years the subject still has not made contact with reality. Lubos's contribution to the demise of Superstrings will just be that if the most vocal advocate of their subject is a petulant, raving brat, and if they continue to make no more than token efforts to rein him in, then something has got to be fundamentally wrong.

14. **dan**
September 19, 2005

re: thomas larrsson,

I personally would think the non-detection would rule out certain forms of string theory, those with a relatively low SUSY-breaking value, just as the non-detection of proton decay in SuperKakomoke rules out certain forms of GUT such as SU5.

almost everyone agrees that the non-detection though would not be fatal to all string theories, just as the absence of proton decay has not ruled out all GUT's.

personally such non-detection, though I hope will stimulate research in other QG programs, esp CDT.

15. **dan**
September 20, 2005

re: The one religious belief that now makes you truly unemployable in physics departments in the US is Einstein's belief in the existence of a deep mathematical and geometrical structure to physical reality.

Peter,

Witten and Lubos and Randall would describe string M theory as a vindiction of Einstein's belief in the existence of a deep mathematical and geometrical structure to physical reality. the top universities all employ string theoriests, although personally, i wish they would employ quantum gravity theorists as an independent category to string theoriests.

16. **Tony Smith**
September 20, 2005

dan said:
"… Witten and Lubos and Randall would describe string M theory as a vindiction of Einstein's belief in the existence of a deep mathematical and geometrical structure to physical reality. …".

Would Witten and Lubos and Randall have the gall to "describe string M theory as a vindication of Einstein's belief" that a really good physics model should meet the criterion of being:
"… a theorem which at present can not be based upon anything more than upon a faith in the simplicity, i.e., intelligibility, of nature: there are no arbitrary constants … that is to say, nature is so constituted that it is possible logically to lay down such strongly determined laws that within these laws only rationally completely determined constants occur (not constants, therefore, whose numerical value could be changed without destroying the theory). …".

See Wilczek's article in the winter 2002 issue of daedalus.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

17. **D R Lunsford**
September 20, 2005

Chris O, amen!

-drl

18. **John Baez**
September 20, 2005

Lisa Randall's article "Dangling Particles" has appeared on The Edge, a chat forum for the self-proclaimed "digerati":

Lisa Randall – The Edge

So, there will probably be some discussion of this article there soon.

19. **D R Lunsford**
September 22, 2005

Bravo! It is correctly pointed out that Randall (and other STers) behave exactly like the IDers, and their schoolmen forebears, in forcing the argument into a pattern designed to produce the predetermined conclusion.

Nice find Peter!

-drl

20. **John Baez**
September 23, 2005

Apparently Brockman doesn't expect discussion of Lisa Randall's piece on his Edge website. He just included her essay there because the New York Times version left out parts, which somehow contributed to evolutionary biologists becoming incensed at it. I haven't compared the versions, so I don't know what this is all about.

Here are a few remarks I was going to contribute to that discussion, had it taken place:

…………………………………………………..

Lisa Randall mentions how different uses of the word "theory" provide a field day for advocates of "intelligent design".

True – but it's not only here that the definition of "theory" has gotten pulled into the rhetorical struggle over a scientific issue. The same thing happens with the phrase "string theory".

Here's what James Watson of DNA fame says in an essay introducing the book "Darwin: The Indelible Stamp":

> Let us not beat around the bush – the common assumption that evolution through natural selection is a "theory" in the same way as string theory is wrong. Evolution is a law (with several components) that is as well substantiated as any other natural law, whether the law of gravity, the laws of motion or Avogadro's law. Evolution is a fact, disputed only by those who choose to ignore the evidence, put their common sense on hold and believe instead that unchanging knowledge and wisdom can be reached only by revelation.

Compare what the physicist Gerard 't Hooft says in his book "In Search of the Ultimate Building Blocks":

> Actually, I would not even be prepared to call string theory a "theory" – rather a model or not even that: just a hunch. After all, a theory should come together with instructions on how to deal with it to identify the things one wishes to describe, in our case the elementary particles, and one should, at least in principle, be able to formulate the rules for calculating the properties of these particles, and how to make new predictions for them. Imagine that I give you a chair, while explaining that the legs are still missing, and that the seat, back and armrest will perhaps be delivered soon; whatever I did give you, can I still call it a chair?

The point here is that string theory does not yet make any specific predictions about what we might see in experiments.

So, “theory” lies on a rhetorical continuum that ranges from “not even a theory” to “not just a theory” – and even real scientists fight about where a given “theory” lies on this continuum.

Jaron Lanier's Review of The Road to Reality

September 18, 2005
Categories: Uncategorized

A correspondent points out to me that the latest issue of American Scientist has a wonderful review of Roger Penrose's new book The Road to Reality by computer scientist, author, artist, etc. Jaron Lanier, much better than my own effort along these lines. Despite not being a theoretical physicist, Lanier does a great job of recognizing and explaining what is great about Penrose's book. He also is dead-on about string theory ("mob mentality", "pompous triumphalism").

The same issue of American Scientist also has a very good review by Lee Smolin of Gravity's Shadow: The Search For Gravitational Waves by Harry Collins. It also contains a nowhere near as good review by yours truly of Sneaking a Look at God's Cards, a book about interpretational issues in quantum mechanics by Giancarlo Ghirardi.

Comments

1. **Nigel**
   September 19, 2005

   Penrose's book reminds me of Maxwell's Treatise on Electricity and Magnetism, being a mixture of ingenious physical ideas and really awful mathematical methods. Quaternions feature prominently in both Penrose and Maxwell, without any real justification.

   Whenever I see the picture of Penrose's twistor, I automatically wonder what the lines are, electric field, magnetic field, or Poynting vector of energy transfer? The same happens with field lines in electromagnetism. If you have a Poynting vector going around in a simple circle, you get a dipole magnetic field which looks similar to Penrose's twistor, while the electric field lines spread out radially.

2. **andy.s.**
   September 19, 2005

   I didn't think I had the background to fathom the book. Now I'll have to take a look at it. So you guys think a mere math B.S. can digest the thing?

3. **woit**
   September 19, 2005

   My own impression of the book was that to fully understand everything Penrose is doing, you probably need a graduate level background in math and physics. But large parts of the book are quite accessible, especially if you have some basic university level mathematics background, so if you're willing to skip some things that seem too difficult, you should find much of the book well worth time spent with it.

4. **John Gonsowski**
   September 19, 2005

   Maxwell was just thinking ahead to the weak force and Penrose is just thinking ahead to conformal gravity. I attended a presentation by Penrose once, it was great but as usual I felt like the AFLAC duck wanting to yell out "TONY SMITH". Smith upgrades quaternions to octonions and gravity to conformal gravity.

5. **Eric Dennis**
   September 19, 2005

   Hi Peter. I've been a lurker here for a while and am looking forward to your book. Good luck! In your review of Ghirardi's book, you write:

   "Most physicists generally believe that quantum mechanics, in its relativistic version as a theory of quantum fields, is a complete, consistent and highly successful conceptual framework. They assume that there must be some well-defined way of describing the entirety of a physical system, experimental apparatus and human observer, appropriately dealing with the confusing interpretational issues. As a result, the study of the sorts of questions examined in this book has often been considered somewhat of a backwater."

   I can understand that belief as contributing to the view of foundational research as a backwater, but I would suggest something more sociologically profound is going on. The belief amounts to the non sequitur "we are sure there is an explanation, but we don't know exactly what it is, so we look askance at anyone who tries to explicitly figure it out." Not exactly a normal, scientific attitude. The high energy frontier is perhaps not the only unstable orthodoxy in physics.

   Given the above quote, I'm curious if you think that the foundational issues are somehow attenuated in (relativistic) field theory. I'd say they just get harder. When field theory is introduced into the discussion, it's usually intended to downplay the significance of Bell Inequaltiy violation, since it's not clear how to formulate Bell's argument in terms of in/out state scattering. But this is just an inadequacy of the standard field theory measurement formalism, not a solution to the problem.

   Regards,
   Eric

6. **Nitin**
   September 19, 2005

   When I saw how big the book is, I thought, and still think, it will take me some time to read it (also, I am busy with graduate work, so time for leisure is quite restrained these days). So I bought the book as a present for a good friend. Hopefully, he'll tell me a nice story at some point (he has not given any feedback so far). I did majors in pure maths, applied maths and physics, so I think the necessary background is there. ;)

   From Down Under

7. **woit**
   September 19, 2005

   Hi Eric,

   I'm really not an expert on interpretational issues, so from one point of view wasn't the best person to review that book. But I'm interested in the topic, and think my prejudices about it are pretty common ones among theoretical physicists, so part of what I wanted to do was to explain what those prejudices are.

One thing that this kind of QM and string theory seem to me to have in common is being largely insulated from experiment, and full of people who have an ideological ax to grind. What I've seen of the QM literature is often as depressing as the string theory literature. Without the discipline of experiment, theoretical physics seems to easily degenerate into nonsensical blather. I'd like to believe that QM will get more interesting as people figure out how to manipulate more interesting quantum mechanical systems. Maybe this is overly optimistic, we'll see.

I have no idea whether the resolution of interpretational problems requires thinking about quantum field theory. But quantum field theory is far and away the best fundamental theory we have. It seems to me that, absent any interesting experimental data to chew on, theorists of all kinds should be spending a lot more time thinking deeply about QFT. My own approach to this is mathematical, I suspect there are mathematically much deeper ways of thinking about QFT than the ones we know about. If and when we figure these out, maybe they'll shed some light on the interpretational issues, maybe not.

8. **fooltomery**
September 20, 2005

Here's a paragraph from Lanier's review of Penrose's book, Peter:

> Reading this math section is eerily liberating. It is shocking that so much can be explained so well. The obvious comparisons are to *The Feynman Lectures on Physics* or George Gamow's *One, Two, Three . . . Infinity*, but the achievement here is greater, because the book starts at such an elementary level and soars to such heights, without any glitches along the way. It's a magical escape from the bounds of gravity.

Perhaps you feel that you're obligated to say nice things about Lanier's review since your own book review was published in the same issue of the magazine and you might like to publish there again, but, really, if your BS detector doesn't peg after reading that paragraph, I'm going to have to reconsider my estimate of the seriousness of your devotion to debunking. Comparing the Feynman lectures to Gamow's little trade book? And then suggesting that Penrose's book (which I've read, BTW) is superior to both?

If Lanier even understood most of the words in Penrose's book, I'd be quite surprised.

9. **Nigel**
September 20, 2005

Penrose undertakes a difficult task and I was surprised how enlightening his treatment of electromagnetic theory is in tensor notation, chapter 19. If you look at Maxwell's theory in Feynman's lectures, you find that Feynman avoids tensors altogether, sticking to vector calculus (divergence and curl, which do what their labels say). Where Feynman tries to explain GR without tensors, he comes up with the ingenious physical contraction of space in the radial direction only around a mass, which indeed is precisely the special feature of the contraction term that Einstein introduced. However, the Feynman lectures by keeping to simple mathematics, are limited. Gamow's 'One Two Three … Infinity' falsely debunks the idea that the Lorentz contraction can be considered a physical pressure effect of moving against the spacetime fabric. Gamow tries to obfuscate by claiming different materials would contract differently, when in fact the compression forces involves are always electrical and the contraction would be similar. So for all their skills in physics, I don't see how anybody can learn sufficient physics from the popular books of Gamow and Feynman. The Penrose book excels at sticking to mathematics and physical facts, without the self-opinionated interpretational baggage…

10. **fooltomery**
September 20, 2005

Nigel, my point was that Lanier sets up a sort of parity between the Feynman Lectures and Gamow's book, which is silly (and leads me to suspect that he's not spent a lot of time reading either book). Then, lumping these two very unlike texts together, he asserts that Penrose's book is superior to both.

As a loose analogy, consider equating the acting skills of Sir Laurence Olivier and Jon Lovitz, and then going on to say that Russell Crowe is better than both. The initial equation calls one's judgment into question, even though it might well be the case that at least in some respects Russell Crowe *is* a better actor than both (although the standard Crowe must beat is much higher in the case of Olivier than in the case of Lovitz).

With apologies to the Master Thespian, of course…

11. **woit**
September 20, 2005

Sorry, but I was really legitimately very impressed by Lanier's review, and wasn't saying nice things about it because the folks at American Scientist just paid me a fee in the high zero figures. His language may be a bit over the top, but Penrose's book is something very unusual, so this level of enthusiasm seems to me not inappropriate. I don't remember Gamow's book, I think I probably read it more than thirty years ago. The comparison to Feynman is appropriate, that's one of the few other books of at all similar scale and ambition. Penrose is much more of a mathematical physicist, so his book is much more mathematical. If your taste runs more to mathematical than physical arguments, Penrose's is definitely a better book than Feynman's. For purely physical arguments though, you're better off with Feynman.

I don't know Lanier personally, but do know people who do, who had told me he's a very impressive character, with knowledge of a wide range of subjects. His review certainly demonstrated this. He correctly pointed out many of the best and most striking things about the book, made a wonderful analogy to the book that made Camille Paglia famous, and did a fantastic job of situating Penrose, his point of view and what he was trying to accomplish with the book in the context of contemporary theoretical physics.

Lanier's comments about string theory certainly did endear him to me, so if you want to accuse me of being seduced into feeling obligated to say something nice about the review, it would be those that did it, not the American Scientist connection. But I can assure you that I thought the review was great and at no time felt I had to say nice things about it for any other reason.

12. **fooltomery**
September 20, 2005

Okay, I'll let you slide on this one. Heck, I wouldn't have sold out for $0 either. They would've had to pay me at least $&epsilon for some ε>0.

And for the record, I, too, read Gamow's book decades ago and have little recollection of its details. But I'm not the one who mentioned it in a published review, so I'm cutting myself (and you) some slack on that score.

Regards…

13. **fooltomery**
September 20, 2005

Lost a semicolon after that first epsilon, I see. Dang.

14. **John Gonsowski**

September 20, 2005

Nigel, where is Feynman limited by not having tensors? He can't unify GR and electroweak/color I suppose but tensors can't do that either. You need something like SU(5) GUT which from a spacetime point of view could be like adding Kaluza-Klein-like dimensons.

Some interesting history from Tony Smith's site:
http://www.valdostamuseum.org/hamsmith/HeisHist.html

15. **Dan P**
September 20, 2005

How many times does this book mention Godel's theorem?

16. **Carl Brannen**
September 20, 2005

I was a grad student at U. Cal. Irvine back when Joe Weber taught there (he was also teaching at U. Maryland, if I recall). Most of the small amount I know about gravitation I learned in his class, which he taught out of his book. He died quite recently.

Carl

17. **woit**
September 21, 2005

The book has a couple pages about Godel's theorem.

18. **Nigel**
September 21, 2005

John: Feynman pulls his simple formula for contraction of space around a mass out of thin air, like magician pulling a rabbit from a hat. Penrose goes into mathematical physics a bit deeper. In your link Tony Smith deals with Feynman's lectures on gravitation (1995 publication), not the lectures on physics.

19. **hongbaozhang**
September 22, 2005

Today, I received a copy of Sir Penrose's The Road to Reality. After a brief glimpse, I think it is the graduate's Feynmann's Lectures on physics!
It tells us with caution what we did know, what we are now doing and in what directions shall we go?
But here is a question in Page 659: why the commutation(anticommutation) relations involve the imaginary number i?

20. **Maynard Handley**
September 24, 2005

Peter,
I found your review extremely frustrating because it didn't answer the single most important thing anyone reading a review wants to know: should I devote some of my precious and very limited time to reading this book. The book, apparently, covers material we have all seen many times and, as least as far as your review goes, says absolutely nothing that wasn't said thirty years ago.
Bohr, EPR, entanglement, Bell, Aspect, it's all very mysterious, blah blah.

It sounds like the only thing they have to offer is the sort of stupid ad hoc modifications people were making in the 50's to non-relativistic QM. I honestly don't know why people waste their time with this crap — it's so clearly not going to fit into the larger structure. It's like approaching rotation as something algebraic, and dicking around with the algebra, rather than viewing it as something geometric and making modifications that match that geometric view.

So the answer to my question would appear to "don't waste your time, unless you've never, in your life, met this material before". Is there a single thing they say that is not either a waste of time, or that would come as news to an intelligent 2nd yr grad student?

Is this a fair conclusion, and it was politeness and the conventions of the review format that prevented you from saying so outright?

21. **woit**
September 25, 2005

Hi Maynard,

I suppose I should have made this clear in the review, but the Ghirardi book is aimed at people who don't know much about interpretational issues in quantum mechanics. If you've thought about this stuff, read about it elsewhere, and have a graduate-level background in QM, the book isn't aimed at you. It has a bit more about Ghirardi et. al.'s own approach than you'll find in other books, but not that much. If you have some background and want to know about what they have done, you should read their papers.

I thought the best thing about the book was the way it wasn't either too gee-whiz, or pushing a specific viewpoint. Some introductory books like this act as if QM is some complete mystery, Ghirardi doesn't. He also doesn't push his own research strongly, acknowledging that it has problems. It's basically an even-handed introduction to the subject, aimed at the non-expert.

22. **dan**
September 28, 2005

speaking of roger penrose, what do his peers think of his suggestion that gravity is what divides the quantum realm to the classical realm?

2005 Physics Nobel Prize

September 21, 2005
Categories: Uncategorized

After my initial success last year, I've retired from the business of predicting who will get Nobel prizes. This year's physics prize will be announced in less than two weeks, on Tuesday, October 4. Anyone else want to make a prediction?

Last year there was a Nobel Prize Market, but it doesn't seem to be in operation this year.

For the last few years Thomson Scientific has been issuing Nobel prize predictions based on citation counts. They're not doing very well in physics, basically because every year they predict it will be Green, Schwarz and Witten. This year's prediction is here. In 2003 they rather petulantly commented:

Most observers believe the Nobel Prize will not be awarded for theoretical work. If, however, citations reflect real influence and prizes ought to be awarded for influential work, the Nobel Committee should consider recognizing string theory and M theory, whose leading figures have been Green and Schwarz, the pioneers, and Witten, who extended their work. Witten, it should be noted, is the most-cited physicist of last two decades.

Their idea that the Nobel prize is not awarded for theoretical work is kind of strange, and wrong. Last year's award was to theorists. The people at Thomson seem to not be able to tell the difference between theoretical work that is confirmed by experiment, and work which isn't. So far the Nobel committee seems to be able to make that distinction, and doesn't just count citations. Presumably this will still hold true for this year. While I won't predict who will get the prize, I will predict that Green and Schwarz won't get it, and if Witten does, it won't be for his work on string or M-theory.

Comments

1. **LM**
   September 21, 2005

   To Leonard Susskind, for pioneering work in the field of Quantum Tautology.

2. **Wolfgang**
   September 21, 2005

   To Lubos Motl for his contribution to the physics of global warming,
   determining the heat capacity of ice.

3. **Belizean**
   September 22, 2005

   David Deutsch, one of the fathers of quantum computation.

4. **Haelfix**
   September 22, 2005

   I hate to be biased, but we do have some astrophysicists who are worthy of four of five nobel prizes by now. If theoretical work in the absense of 11 decimal precision is ever to be awarded, I would think that field would and should be the first one to be signaled out.

   Yes we do have experimental verification, no its unfortunately nothing like detector confidence, yes our work is pretty important.

5. **a**
   September 22, 2005

   Nobel prize of not-this-year:

   M.W. Goodman and E. Witten for
   DETECTABILITY OF CERTAIN DARK MATTER CANDIDATES (1985)
   and the experimentalist that will possibly do
   DETECTION OF CERTAIN DARK MATTER CANDIDATES (200?).

6. **Zelah**
   September 22, 2005

   Hi,

   I have a courageous prediction for the nobel prize.

   I believe that it should go to Vera Rubin of Dark Matter fame, with relevant coawardees.

   The reason is that Sir Martin Rees, and James Peebles won the Crafoord prize in 2005 for Dark Matter! I consider this scandalous! I have nothing against Rees or Peebles, who have done wonderful work regarding Dark Matter, but ignoring Vera Rubin is quite astonishing!

   I do not have much hope in this regard but who knows?

   To be fair to the nobel committee, there are plenty of experimentalist Physicist who are just as worthy.

   An Amateur Mathematician

7. **hongbaozhang**
   September 22, 2005

   Time flies fast, this year's Nobel prize will come!
   I guess this year will be given to some quantum optics physicist:)

8. **D R Lunsford**
September 22, 2005

How about BRST? (Becchi, Rouet, Stora, and Tyutin) That's real physics at least. If Witten gets it I'll go on a ten-day drunk out of despair for my lost civilization.

-drl

9. **Tony Smith**
September 22, 2005

For physics Nobel, how about Kobayashi and Maskawa ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

10. **Quantoken**
September 22, 2005

I do not know who will receive it, but if the Nobel is awarded to string theory related research, it would be the unprecedent because this will be the first time a Nobel price is awarded to some **highly speculative ideas** that has **no confirmation** whatsoever by experiment. It would be a joke and could make Nobel a **laughing stock** and respected scientists would refuse to be recognized by the Nobel prize, if at the end of day super string theory is proven to be totally wrong and irrelevant to the nature.

I do not think that will happen.

Remember Nobel prizes are not awarded to mathematics?

Quantoken

11. **Nigel**
September 22, 2005

Einstein got the prize for the maths of the photoelectric effect, which was experimentally first noticed by Edison. Edward Witten could get it for superstrings, on the basis of being the only mathematical approach which ties up the standard model.

12. **hong-bao zhang**
September 22, 2005

If Nobel prize give string theorist, how about Hawking and Penrose?
Hawking's radiation gives the most important hints on string theory.
Penrose's spinor technique much influences Witten's work.

13. **blank**
September 22, 2005

Noticed there weren't any MacArthurs awarded to physicsts this year.

14. **D R Lunsford**
September 22, 2005

Tony – good suggestion, and it's classic "this works but God knows why" phenomenology!

-drl

15. **ali**
September 22, 2005

I agree with Haelfix:

I'll be betting on Rashid Sunyaev (too bad Zeldovich isn't still around). They might also give it to some experimental cosmologists.

Someone deserves credit for showing the universe is flat based on CMB measurements…

16. **Not a Nobel Laureate**
September 22, 2005

One hopes that the Nobel Prize in Physics will be awarded for physics.

Offhand;

Goldhaber, Grodzins, and Sunyar: neutrino helicity

Sajeev John et al: photonic bandgaps and photonic localization

David Awschalom et al: spin Hall effect

Unfortunately Chien-Shiung Wu (parity violation) died in 1997.

On the other hand, Susskind et al are highly qualified candidates for the Ubu Prize in Pata-Physics or at least the IgNobel Prize.

17. **biophysics**
September 22, 2005

Here are a couple theorists who most might've passed over. How about giving the prize to Edward Lorenz for opening up the study of chaos, which has had both immense mathematical and physical research associated with it?

Ok that's one idea. The other one I got by looking at the list of winners of the Dirac Medal – how about J.J. Hopfield for his pioneering work in theoretical biophysics?

Yeah, yeah totally biased and all, but the physics of complex systems is definitely one of the most exciting fields of modern theoretical physics and it would be a fine idea, in my opinion, to recognize some pioneers in the arena.

18. **misslemon**
September 23, 2005

Guth? Tyson (J.A.)?

19. **Matthew**
September 23, 2005

No way will it go to particle physics again this year. If I had to guess, something to do with the CMB seems a reasonable bet.

20. **Mike Bacon**
September 23, 2005

I second the nomination of David Deutsch.

21. **Jeremy**
September 23, 2005

What about Jim Peebles? or Peebles and Sunyaev?

I don't think there has ever been a prize for theoretical cosmology, which is scandalous. Well, Bethe won, but that was for stars. It's too bad Dicke and Wilkinson aren't with us any more.

22. **Not a Nobel Laureate**
September 23, 2005

I "second" the CMB.

Regarding J.J. Hopfield, interesting work, but what is it's predicitive ability in what I agree is the very exciting field of biophysics?

23. **Chris**
September 23, 2005

Fowler got it in 1983 for his work in big bang nucleosynthesis.

c

24. **misslemon**
September 23, 2005

David Deutsch?!? Really? And seconded?!

25. **Eric Dennis**
September 23, 2005

Why don't we wait until a quantum computer is actually built — or at least something with more than like 5 qubits — before we start getting behind Deutsch (or better Peter Shor) for a Nobel.

Not at all to diminish the relevant achievements, but 1) there is no new physics here, it's all quantum engineering; 2) after the initial breakthrough in error-correcting codes around 1996, nothing much has happened in this field of fundamental importance or significant help to experimentalists in building anything remotely resembling a useful machine; 3) at present, building a quantum computer seems about as likely as performing an experimental test of string theory.

While at least in quantum information the theoretical side has not divorced itself from real physics, the hype surrounding the whole enterprise (and generated just as much by the experimentalists in this case) is not that different in terms of its calculated half-truthfulness from that of string theory.

26. **Quantoken**
September 23, 2005

Dennis:

Who said that enginnering work can not be awarded a Nobel prize? Last year's Nobel Chemistry prize was awarded to a Japanese for inventing a measurement trick that gained wide usage. He didn't even have a Ph.D. and was just a technician.

There are quantum computers around. Any quantum system is a quantum computer in a broad sense. According to [Seth Lloyd at **http://www.edge.org**](), the whole [universe is a quantum computer]().

Quantoken

27. **Scott Aaronson**
September 24, 2005

Eric: as someone who works on the theory side of quantum computing, I agree with you that the Swedes should wait for more experimental successes before awarding any prizes in this field. That would be in keeping with their past practices.

But if it's true that "there is no new physics here, it's all quantum engineering," then it's strange that at least two Nobel physicists ('t Hooft and Laughlin) think building a quantum computer is fundamentally impossible! Maybe the right way to say it is this: quantum mechanics is 1920's physics, which becomes 21st-century physics when taken completely seriously.

28. **Pseudo-string-fan**
September 24, 2005

I watched a TV program which said Susskind was the first one who proposed string theory. Therefore, he should be awarded the Nobel prize if string theory can be proved true. Right?

29. **biophysics**
September 24, 2005

Ok I think I agree that Hopfield getting a Nobel physics prize might be a stretch (for the same reason that other Dirac medal winners don't get Nobels, like Daniel Freedman, for example..while experimentalists righly get Nobels for inventing devices that have furthered physics, theorists do not to get prize for their 'theoretical devices' until at least some experimental substantiation has come about to validate the use of those 'devices').

But Lorenz I think is eminently qualified for the Nobel – chaos and nonlinear dynamics have become a huge part of the modern theoretical frontier, and the existence of deterministic chaotic motion has been borne out by grade school experiment 🙂

30. **Zelah**
September 24, 2005

I find the pushing of Mr Lorenz for the nobel as extremely strange!

The truth of the matter is that Chaos was well know to mathematicians! KAM Theory anyone! It was the physicists who needed to catch up!

Essentially, Chaos Theory if you have studied it is just a subsection of Condensed Matter Physics in disguise!

The only person who could POSSIBLILY win the nobel prize for Chaos is Feigenbaum of Feigenbaum constant fame!

An Amateur Mathematician.

31. **Luboš Motl**
September 25, 2005

I have not written the first comment by "LM" – but actually it is not such a bad comment. 😉

32. **Aswin**
September 25, 2005

Penzias and Wilson got a nobel.. and the WMAP team deserves one too! The Supernova team ??.. may be later.
These days, Weinberg (and many others) seems to be interested in comparing Cosmology-of-today to PP of 70s. With support of such powerful voices, I guess cosmology is in for some good time 😕

33. **Aswin**
September 25, 2005

oops..I meant COBE.

34. **Nitin**
September 25, 2005

What about having a group of researchers getting the Nobel? Maybe the guys who found out that the expansion of the universe is accelerating (Aswin's supernove team), or the WMAP team (again Aswin thinks the same)? Alan Guth would be a good choice. My guess is this year's Physics Nobel is going to a cosmologist/astrophysicist (see how the picture of the universe on the large-scale has changed in the past 2 decades). It's hard to say who though, hard. hmm…

35. **Thomas Larsson**
September 26, 2005

Nitin, the Nobel is traditionally awarded to at most three individuals. That's probably why Ward wasn't recognized with Glashow-Salam-Weinberg for the electroweak theory.

36. **Nitin**
September 26, 2005

Ok.. maybe the conditions are different for different Nobels? Because I remember the Medecins Sans Frontieres getting the Peace Nobel.. maybe I'm wrong on this point as well.
Toutes mes excuses.
Nitin

37. **Thomas Larsson**
September 26, 2005

Yes, I was thinking about the sciences (including economy). Peace is different. So is literature, which AFAIK has never been shared.

38. **Sergei Popov**
September 26, 2005

Last year on the Nobel prize Market I suggested Michel Mayor for the discovery of exoplanets. Still, I think, he's a good candidate.

Prizes are not given for unprooved theories, so nothing for Witten, but I have to mention Linde & Co. for inflation theory.

39. **Eric Dennis**
September 26, 2005

Scott,

I didn't know 't Hooft was a skeptic. Do you have a reference? As for Laughlin, while I have great respect for some of his heterodox views, from what I've seen his polemics against the possibility of quantum computing are pretty flimsy, and quite poorly articulated besides. I think we would agree that if someone actually demonstrated the impossibility, or exhibited a currently unknown physical effect that would somehow in general prevent a quantum computer from being realized, that would be entirely unexpected and almost certainly itself merit a Nobel.

Perhaps you could be more concrete though. Could you give an example of important new physics that has been discovered in connection with quantum information research? (Again, I don't think the absence of such is necessarily a bad thing, I just think it's relevant for something like a Nobel decision.)

Regards,
Eric

40. **Mike Bacon**
September 26, 2005

My 'second' of Deutsch had to do with his groundbreaking work in quantum computing. Pariticularly now, with rapid progress being made in "cluster quantum computing" (i.e. robust and scalable quantum computation using both stationary qubits (e.g. single photon sources made out of trapped atoms, molecules, ions, quantum dots, or defect centers in solids) and flying qubits (e.g. photons)). It's the case that theorists do not to get the prize for their 'theoretical devices' until at least some experimental substantiation has come about to validate the use of those 'devices.' While quantum computation has been 'proved' in theory (and carried out with a limited number quibits), it does now seem that this fundamentally new form of information technology is about to blossom. Whether this supports Deutsch's philisophical views is another matter — however, the work is turning out to be hugly significant.

41. **Not even a physicist**
September 26, 2005

I agree that CMB is a good candidate. Will it go to COBE, WMAP or both?

How about neutrino oscillations? Davis and Koshiba were awarded for detection of cosmic neutrinos, but not for oscillations. Is there a possibility of another prize for neutrino physics?

Is there any chance for Nambu, either for his work on symmetry breaking, or for introducing colors?

42. **Scott Aaronson**
September 28, 2005

Eric,

I completely agree with you about Laughlin's arguments. A reference for 't Hooft is gr-qc/9903084 (especially pages 12-13). My response to some of the skeptics (mainly computer scientists) is at quant-ph/0311039.

As for whether quantum information is "new physics": would you say the uncertainty principle was new physics? What about Feynman's sum-over-histories picture, or the Bell inequality? All of these were "mere" mathematical consequences of the quantum formalism, yet today they're almost inseparable from QM itself.

I would argue that the following results from quantum computing and information have deepened our understanding of QM in an analogous way:

* Shor's and Grover's algorithms
* The optimality of Grover's and other quantum algorithms (showing why QM often *doesn't* yield exponential parallelism)
* Quantum error-correction and fault-tolerance
* Adiabatic computation and QMA-completeness of the Local Hamiltonians problem
* Teleportation and dense quantum coding
* Holevo's Theorem, lower bounds for random access codes, and other results in quantum communication complexity
* Ideas from entanglement theory (like entanglement swapping and purification)

But maybe it's just a matter of taste — I'd also give Aspect et al. a Nobel for confirming the Bell inequality violations, but it hasn't happened yet.

Best,
Scott

43. **Eric Dennis**
September 28, 2005

Scott,

I don't claim something is disqualified from being "new physics" just because it's ultimately a mathematical consequence of standard QM. Presumably high temp superconductivity is completely understandable in terms of some many-body Hamiltonian, and that's certainly new physics. But high temp superconductivity — as opposed to Grover's algorithm or channel bounds or the other things you list — actually exists. It's not just some idea we have about something we could make.

The actual physics in quantum computing, the ion traps or QED cavities, is not really part of quantum computing reserach itself. It's condensed matter or quantum optics or whatever. Things like adiabatic or anyonic computation may at some far future date straddle this boundary, but that's only when something computationally non-trivial is actually built.

Eric

P.S. I definitely don't regard Bell's inequality as new physics. It's really great, but it has no special connection to any theory of physics (including QM) other than providing a necessary condition for locality. I won't bother commenting further on path integrals etc.

44. **Scott Aaronson**
September 28, 2005

Eric,

This seems like a semantic debate — e.g. were Bose-Einstein condensates not a legitimate part of "physics" until someone actually created one?

But I don't think we disagree that much in practice — we both think the Nobel committee should continue to award prizes only for theories that have actually been confirmed, or things that have actually been built. That acts as a useful brake on unrestrained speculation.

–Scott

45. **Eric Dennis**
September 29, 2005

I don't think it's semantic, and that's why we seem to disagree about the status of Bell's inequaltiy. I am saying that if an experimentalist were to succeed tomorrow in assembling a bunch of coupled quantum dots or something and implement fault-tolerant codes and factor 10^100 + 1, that would be miraculous, but there's no new physics. Perhaps the superconductivity comparison wasn't clear. Each of the little quantum dot widgets in the computer, and the cavities, and etc. — that's the physics, and that's not really part of QC research. Having built this computer, no basic new aspects of nature would have been discovered. Something wonderful would have been built, but that's not the same. Perhaps you could say some mathematical aspects of the world would have been discovered, in the same way that some mathematical aspects of the world are discovered in the course of designing and running a classical silicon-based computer.

The reason I persist here, is that the physics-ization of QC research strikes me as similar in certain respects to the physics-ization of string theory. In terms of physics rather than math or CS, both of these things are constructions, not discoveries. QC isn't trying to be a discovery, and it need not be one, but some of its practitioners find it useful to talk as if it is one. String theory is trying to be a discovery, and it's failing badly, but some of its practitioners also find it useful to talk as if it is one.

Anyway, I suspect we won't agree. Nice arguing with you.

46. **kaushik**
September 30, 2005

James Yorke…..pioneer of chaos. Its such a huge field in itself, and is an integral part of physics now and has application is so many fields.
Yorke, and few other pioneers of the subject deserve a nobel

47. **Jack**
October 2, 2005

I predict Kosterlitz and Thouless for the KT transition. It really opened up the area of 2D physics and its originality and applicability make it Nobel Worthy. Future prizes that will be awarded have to eventually include Michael Berry for his geometric phase. that would also be high time to recognize Aharonov.

48. **"A guy who does not believe in String theory"**
October 3, 2005

A prize for cosmology would be nice! Two guesses: either Bennett, Page(?) and Spergel (???) from WMAP, or Guth, Linde & Steinhardt for inflation (although I think they will wait for the detection of GW, the B modes of CMB, to award inflationists).

49. **Neil**
October 3, 2005

It is time to recognize the theoretical and practical achievements in photon localization and photonic band gaps (Sajeev John and Eli Yablonovitch). The visionary discoveries in the field of light localization and photonics will lead to revolutionary real-world applications, greatly exceeding what was achieved in the past century using the discoveries in electronics.

50. **Muthiah**
October 3, 2005

I think Benoit Mandelbrot, deserves a Nobel prize for his work on fractals?
Im not sure.

51. **Bumerang**
October 4, 2005

When you talk about Chaos theory, you completely forget russian school. Arnold, Sinai and Chirikov, for example.
Unfortunately, the Chaos theory does not have a strong power for implementations.

52. **James**
October 4, 2005

My predictions:
Shuji Nakamura and Yoshinori Tokura for diodes and semiconductors resp.
or
Exo planet searches — Marcy and Butler

Dont expect Guth to win — his intial inflationary proposal was flawed (but he realised this) and had to be modifed be Linde and Steinhart. Also, beyond the superfical level, inflation makes predictions, e.g. a flat universe, in terms of probabilities, which mind you are exponentially close to 1 or 0. It is not a "confirmed" theory as some of you suggest above, merely an exciting theoretical idea. I believe the theory is correct from a phemonological prespective, but it is far from a finished theory, and the mechanisms that drives inflation in the very early universe is unknown.

In line with the usual criteria Guth and co will not be awarded a nobel.

As many of you said above, string theory will not be awarded a nobel in the near future. Until the theory can be understood at a level to make firm experimental predictions, and those predictions tested, string theorists will have to wait.

Cheers

James

53. **Jimbo**
October 4, 2005

If anybody who has worked on superstring/M-theory gets it, then a terrible & irrevokable precedent has been set, namely the negation of the 3 centuries-old covenant with the scientific method for establishing the veracity of physical theory thru observation.
However, if this radical leap `must' be made, it should clearly go to Hawking, for two obvious reasons. One, the universal respect & awe in which his work is held, and two, the tragedy the world of physics will have to live with, if we wake up one morning & find that ALS has finally claimed Stephen's life, without him getting the Nobel

prize he so truly deserves.

54. **OJP**
October 4, 2005

I think actually hongbaozhang actually guessed correctly (at least the field). See Sept. 22 entry.

OJP.

Dirac’s Hidden Geometry

September 24, 2005
Categories: Uncategorized

There’s an interesting article by Graham Farmelo in last week’s Nature, entitled [Dirac’s Hidden Geometry](). Most people think of Dirac as a brilliant algebraist, but he himself claimed that his motivations and way of thinking were much more geometrical than algebraic. Farmelo’s article contains an amusing account of how Roger Penrose tried to get Dirac to explain how projective geometry had influenced his work in quantum mechanics. Dirac gave a talk about this at Boston University in 1972, but, after giving a presentation about projective geometry, stopped before explaining the relation to quantum mechanics. Penrose, the moderator, asked Dirac about the relation to quantum mechanics, and in answer “Dirac gave his trademark shake of the head, and declined to speak.”

Several historians of science have tried to figure out what Dirac’s geometrical motivations were. This question is dealt with in Olivier Darrigol’s very interesting book (which is now available on-line) [From c-numbers to q-numbers: The Classical Analogy in the History of Quantum Theory](). The material about Dirac and projective geometry is in chapter XI. On the same topic, there’s also an article by Peter Galison published in 2000 in the journal Representations, entitled [The Suppressed Drawing: Paul Dirac’s Hidden Geometry]().

Comments

1. **icecube**
   September 24, 2005

   Wow..that’s really fascinating…never knew about that before.

2. **Fabien Besnard**
   September 25, 2005

   Unfortunately I don’t have access to the first and third link, but nevermind, the Darrigol’s book seems very interesting, thanks a lot for this.

3. **Alejandro Rivero**
   September 25, 2005

   Hmm, at least in one paper Dirac was explicit about the geometric character of his non-commutative p, q.

4. **Nigel**
   September 25, 2005

   ‘Most people think of Dirac as a brilliant algebraist, but he himself claimed that his motivations and way of thinking were much more geometrical than algebraic.’

   What strikes me in reading chapter XI of Darrigol is Dirac’s love of relativity. This must make it strange for many that Dirac followed Einstein into ’embarrassing’ ideas about a 3-D fabric of spacetime:

   ‘… with the new theory of electrodynamics we are rather forced to have an aether.’ – P.A.M. Dirac, ‘Is There an Aether?,’ Nature, v.168, 1951, p.906. See also Dirac’s paper in Proc. Roy. Soc. v.A209, 1951, p.291.

   ‘Looking back at the development of physics, we see that the ether, soon after its birth, became the enfant terrible of the family of physical substances. First, the construction of a simple mechanical picture of the ether proved to be impossible and was discarded. This caused to a great extent the breakdown of the mechanical point of view. Second, we have to give up the hope that through the presence of the ether sea, one co-ordinate system will be distinguished and lead to the recognition of absolute and not only relative motion. … After such bad experiences, this is the moment to forget the ether completely and to try never to mention its name. We shall say our space has the physical property of transmitting waves and so omit the use of a word we have decided to avoid. The omission of a word from our vocabulary is of course no remedy; the troubles are indeed much too profound to be solved in this way. Let us now write down the facts which have been sufficiently confirmed by experiment without bothering any more about the ‘e—r’ problem.’ – Albert Einstein and Leopold Infeld, Evolution of Physics, 1938, pp. 184-5

   ‘The idealised physical reference object, which is implied in current quantum theory, is a fluid permeating all space like an aether.’ – Sir Arthur Eddington, MA, DSc, LLD, FRS, Relativity Theory of Protons and Electrons, Cambridge University Press, Cambridge, 1936, p. 180.

   ‘It has been supposed that empty space has no physical properties but only geometrical properties. No such empty space without physical properties has ever been observed, and the assumption that it can exist is without justification. It is convenient to ignore the physical properties of space when discussing its geometrical properties, but this ought not to have resulted in the belief in the possibility of the existence of empty space having only geometrical properties… It has specific inductive capacity and magnetic permeability.’ – Professor H.A. Wilson, FRS, Modern Physics, Blackie & Son Ltd, London, 4th ed., 1959, p. 361.

   ‘To deny the ether is ultimately to assume that empty space has no physical qualities whatever… Recapitulating, we may say that according to the general theory of relativity, space is endowed with physical qualities… therefore there exists an ether. According to the general theory of relativity space without ether is unthinkable.’ – Albert Einstein, Leyden University, 1920. (Einstein, A., Sidelights on Relativity, Dover, New York, 1952, pp. 15, 16, and 23.)

   ‘But if, meanwhile, someone explains gravity along with all its laws by the action of some subtle matter, and shows that the motion of planets and comets will not be disturbed by this matter, I shall be far from objecting.’ – Isaac Newton, Letter to Leibniz, 1693.

   ‘The Michelson-Morley experiment has thus failed to detect our motion through the aether, because the effect looked for – the delay of one of the light waves – is exactly compensated by an automatic contraction of the matter forming the apparatus.’ – A.S. Eddington, Space Time and Gravitation, Cambridge, 1921, p. 20.

   Weird.

5. **andy.s**
   September 25, 2005

   I can’t see those links, but the Geometric Algebra folks have been going on about that for a while. See:

   http://modelingnts.la.asu.edu/html/GAinQM.html

6. **D R Lunsford**

September 25, 2005

Dirac's book on GR is one of his miraculous works. It's only 69 pages long and reads like one of his papers. I remember it used to sit next to MTW on my shelf – here was this burlesque routine filled with hot air and dubious leaps of faith, and there was Dirac.

This is good:

http://arxiv.org/abs/gr-qc/0301097

The papers about deSitter space are fascinating and should be read by anyone interested in extra dimensions.

-drl

7. **plato**
September 27, 2005

Interesting…..

These pictures were not for pedagogical purposes: Dirac kept them hidden. They were not for popularization—even when speaking to the wider public, Dirac never used the diagrams to explain anything. Astonishing: across the great divide of visualization and formalism that has, for generations, split both physics and mathematics, we read here that Dirac published on one side and worked on the other.

http://www.representations.org/article.php?article=72.7

And I Thought My Office Was Bad…

September 24, 2005
Categories: Uncategorized

Via For God, for Country and for Your Name Here, it seems that Alan Guth had the winning entry in a Boston contest for the messiest office. He won an office make-over, check out the before and after photographs.

Comments

1. **Chris Oakley**
   September 24, 2005

   It looks like the "before" office was an ingenious attempt to model the early universe – before galaxies started to form.

2. **Steve**
   September 24, 2005

   The very early office probably started out in a low-entropy ordered state with just a little disorder, eventually undergoing about 64 efoldings in clutter.

3. **Chris Oakley**
   September 24, 2005

   There also seems to be an information loss issue, as Janet of Go Simple made him donate his Physical Review collection to the MIT library. Maybe Hawking's original view of Black Holes, as applied to the initial singularity, was correct after all.

4. **Mark Trodden**
   September 24, 2005

   Fantastic! I've been in that office many times and it is by far the messiest one I've ever seen. As a postdoc Ionged for this day.

5. **Brett**
   September 24, 2005

   Keep in mind, that's Guth's NEW office. The old one, as I recall, worse.

6. **Kellstrom**
   September 24, 2005

   I give him 4-6 weeks, the office will return to its previous messy state. In other words, it will return to equilibrium, as is the eventual and inevitable fate of all perturbed systems.

7. **fantomas**
   September 25, 2005

   Theoretical physics is so passé. These days, it's all about Feng Shui.

   But wait a second, is Feng Shui even falsifiable ? Cause we all know the answer for inflation.

8. **Quantoken**
   September 25, 2005

   Kellystrom said:

   "I give him 4-6 weeks, the office will return to its previous messy state. In other words, it will return to equilibrium, as is the eventual and inevitable fate of all perturbed systems. "

   No it's not equilibrium. How do you go from zero entropy to a huge amount of entropy in virtually no time? **Inflation theory** is the answer 🙂 You bet inflation will happen in Guth's new office, which would quickly grow into a bubble universe 🙂

   His old office is really impressive. My guess is the amount of entropy probably has broken the Beckenstein Entropy Bound of blackholes.

   Quantoken

9. **Quantum_Ranger**
   September 25, 2005

   Guth would have noticed that, in his previous office he could tolerate things being moved to about 15 decimal places, things could be moved around his office freely, at least fifteen times before he could no longer retrieve any of his things, and would have to re-calculate where he last remembered where he seen what he was looking for!

   I calculate, in his new office he would have to fine-tune his sloppy filing nature down to about 1 decimal place, due to the fact that his office space is more restrictive, and thus moving any object from A to B would be a near improbable event!

10. **Chris Oakley**
    September 25, 2005

    Theoretical physics is so passé. These days, it's all about Feng Shui.

    Are you suggesting that if Alan Guth had a north-facing office behind a mountain, and no knives in his drawer (or whatever), then his office would not have got so messy? Or would that just be the condition for people to take inflation theory more seriously?

11. **fantomas**
September 25, 2005

Since inflation was about free lunch, I guess everything is possible.

Seed Magazine

September 26, 2005
Categories: Uncategorized

The science magazine Seed is being relaunched, and the first issue of its new incarnation is now on the newsstands. Their motto is "Science is Culture", and Clifford Johnson over at Cosmic Variance has an enthusiastic appreciation of what they are doing. The magazine is strikingly attractive, with impressive photography and graphics. One photo essay pairs photos with important equations.

There's a piece by Lisa Randall promoting her recent work with Andreas Karch on what she calls the "relaxation principle". I guess this is meant to be a sort of vacuum selection principle, contrasted to the "anthropic principle". In her Seed article she describes what she is doing as follows :

The challenge for physicists, and the problem I tackle in my own work, is find all possible qualitatively different universes — and to search for principles that determine which of these universes is most likely to exist.

Unfortunately there seem to be an infinite variety of possible such universes, and examining them all could easily take up the efforts of all particle theorists for the next few centuries. There's zero evidence for any sort of vacuum selection principle that will pick out the standard model from this infinite array of possibilities, so setting out on this path means probably abandoning any hope for ever explaining much of anything about particle physics. Karch and Randall try to give an argument for why there are 3 space dimensions, ending up with an argument for the survival of both 3 and 7 dimensional branes if one starts out with branes of all dimensions. This is a very, very long way from getting any non-trivial information about particle physics.

This issue of the magazine also has a short piece entitled "A New Force? How blogs are revolutionizing physics" by Joshua Roebke, an ex-string cosmology graduate student who now works at Seed. Joshua devotes a sizable part of his piece to telling about "Not Even Wrong" and some of the effects it has been having. Earlier this summer I had lunch with him here in New York and was encouraged to see that Seed has someone on staff with a good theoretical physics background.

Update: Lubos Motl also has a posting about the new Seed magazine. He comments on the Karch-Randall "relaxation principle", saying that he "kind of worked on it", but

frankly, I don't really believe it – because of the devil hiding in the details that just don't seem to work – much like many other proposals that have appeared in recent years.

Comments

1. **D R Lunsford**
   September 26, 2005

   What utter nonsense! As if Newton or Kepler or even Aristotle or Belushi had the complete hubris to enumerate all possible uni…

   Wait! I am suddenly filled with the Randallian spirit! I too can enumerate the possible universes!

   ONE.

   I tire of these loathsome exercises in narcissism.

   -drl

2. **dan**
   September 26, 2005

   Hello Peter,
   I forget if i've asked this before, but apart from SUSY, higher-dimensional geometries, and string theory, what are plausible ideas for going beyond the standard model (or explaining the SM) that do not involve string theory, SUSY, and higher-dimensions? Why is it we never hear about them?

   I know I did ask about other QG's such as LQG and CDT, but are there other ideas in particle physics that do not involve string theory?

3. **Chris Oakley**
   September 27, 2005

   I forget if i've asked this before, but apart from SUSY, higher-dimensional geometries, and string theory, what are plausible ideas for going beyond the standard model (or explaining the SM) that do not involve string theory, SUSY, and higher-dimensions? Why is it we never hear about them?

   One reason why you have never heard about them is that they have been starved out. When I was applying for post-docs around 84-85 just about everyone seemed to be switching to Superstring theory, and if you did not want to join the 10/26-dimensional circus yourself, you were unlikely to be shortlisted for a job.

4. **Matti Pitkanen**
   September 27, 2005

   I forget if i've asked this before, but apart from SUSY, higher-dimensional geometries, and string theory, what are plausible ideas for going beyond the standard model (or explaining the SM) that do not involve string theory, SUSY, and higher-dimensions? Why is it we never hear about them?

   For plausible ideas for goung beyond the standard model and explaining it see my homepage at

   http://www.physics.helsinki.fi/~matpitka/

   and my blog site TGD diary at http://matpitka.blogspot.com/.

   Matti Pitkanen

5. **Thomas Larsson**
   September 27, 2005

   Dan, given that essentially all experiments are well described by the standard model coupled to gravity, perhaps the ultimate ToE will turn out to be SM coupled to GR. If so, the quest for something beyond the SM (except for gravity) would turn out to be fundamentally misguided.

Sure, this theory has some well-known mathematical problems – QM and GR are incompatible, the SM is somewhat ugly in the Higgs sector, QED breaks down at the Landau pole, etc. But these problems are mathematical rather than physical in nature, and may very well be resolved with a better mathematical understanding of field theory. At least we know that the mathematics underlying field theory is related to real physics.

6. **Dissident**
   September 27, 2005

   How about the most conservative approach of all, yet another parton layer, i.e. quark and lepton substructure?

   http://cosmicvariance.com/2005/09/05/dark-matter-and-extra-dimensional-modifications-of-gravity/#comment-4091

7. **Thomas Larsson**
   September 27, 2005

   Adding another parton layer is hardly conservative. As in string theory and supersymmetry, you have to posit the existence of an invisible world for which there is zero experimental evidence. But unlike string theory, you cannot argue that field theory infinities are absent, so GR will still be unrenormalizable. So you sacrifice the connection to reality without gaining anything mathematically.

   Technicolor is a variant of this idea. Here it is the Higgs boson which is a bound state of fermions (unless my memory fails me here), much like Cooper pairs in superconductivity. The experimental status is similar to that of SUSY and strings: the simplest and natural models are ruled out by experiment, but by adding new parameters you can avoid confrontation with experiment forever.

8. **D R Lunsford**
   September 27, 2005

   TL, that's more or less the end of that idea 🙂

   I wonder why such simple reasoning is in such short supply? …

   -drl

9. **dan**
   September 27, 2005

   re Thomas Larsson Says:

   one of the motivations for going beyond SM+GR is that certain astronomical phenomena appear to be unexplained, and i am referring specifically to dark matter, dark energy, cosmological constant, hierarchy problem, matter/antimatter asymmetry, BH information paradox, etc., that string theory and LQG are attempting to address.

   is your position that SM+GR can eventually explain the above phenomena and will not require new physics? if so why all the effort being poured into string theory?

10. **Chris Oakley**
    September 27, 2005

    One of the things – no, *the* thing I enjoyed most about Peter's forthcoming book (he kindly gave me a copy of the manuscript in January) was an honest survey of the various schemes, stringy and non-stringy, for going beyond or fixing the problems in the standard model. All too often the state of each particular art is explained by one of its enthusiastic exponents, and one gets somewhat of the Monty Python Pet Shop Owner effect ("No, this parrot's not dead, he's resting … pining for the fjords, etc.")

11. **anon**
    September 27, 2005

    WoW! Off topic but eye popping:
    a post by Martin Rocek at Lubos' website.

12. **Dissident**
    September 27, 2005

    > Adding another parton layer is hardly conservative.

    Yes it is.

    > As in string theory and supersymmetry, you have to posit the
    > existence of an invisible world for which there is zero
    > experimental evidence.

    You miss the little detail that unlike strings, an underlying parton structure would not need to wait until the Planck scale in order to reveal itself. As for zero experimental evidence, what do you make of the anomalous magnetic moment of the muon? It's already been pointed out that it can easily be accomodated by a composite muon.

    > But unlike string theory, you cannot argue that field theory
    > infinities are absent, so GR will still be unrenormalizable.

    Who said partons are supposed to fix GR? By the same logic, quarks are no good since they don't fix GR either.

    > So you sacrifice the connection to reality without gaining
    > anything mathematically.

    Seems to me somebody is confusing mathematics with reality here…

13. **Thomas Larsson**
    September 27, 2005

    GR+SM as formulated today is certainly not the end of the story. Astronomical observations are a minor problem here, compared to the fact that the two theories are mutually incompatible. However, there are good reasons to expect that a deeper understanding of the mathematics underlying QFT can lead to a unification of QM and GR, without adding much on the physics side.

More specifically, I am thinking about gauge symmetries. Recently I had a look at Rudolf Haag's book on Local Quantum Physics. A striking claim there is that no intrinsic quantum formulation of the gauge principle is known, despite its central importance in the SM. The gauge principle is of course well understood on the classical level, and that's what matters in the path-integral formalism. An exception, where a gauge symmetry is understood on the quantum level, is conformal symmetry in string theory.

The lack of such a quantum gauge principle is quite obvious from my point of view. In order to understand how a gauge symmetry acts on the (kinematical) Hilbert space, we must first understand how it can act at all, and this is precisely the subject of the representation theory of gauge algebras. The most striking observation is that all interesting quantum representations have anomalies, and that one must explictly introduce and quantize a clock's worldline in order to formulate these representations. This appears to me as the key missing idea.

I have of course no real idea how the astronomical observations should be understood, and I think that it may be premature to address that problem without a theory of quantum gravity. However, i would guess that the CC is somehow related to anomalies – this is at least the case in 2D and 3D gravity.

14. **Thomas Larsson**
September 27, 2005

Dissident, quarks are good because they explain many experiments. An extra layer of partons would be equally good if they fixed some glaring cracks in the SM. Last time I checked, there were no such cracks.

Besides, I thought muon g-2 was explained by a sign bug in the Schoonship program.

15. **Dissident**
September 27, 2005

Really. I guess you should tell these guys then:

http://www.g-2.bnl.gov/

16. **woit**
September 27, 2005

Thomas is right that there was an error in the muon g-2 calculation (I don't know if Schoonschip was to blame). Fixing the error reduced the difference between the theoretical and experimental values. At this point as far as I can tell, given the uncertainties in the theoretical calculations and in the experimental results, there is no convincing conflict between theory and experiment.

17. **Matthew**
September 27, 2005

Just for the record, the g-2 for the muon is indeed out by over a sigma from the standard model prediction. However, the errors and stability of the hadronic part of the theory are "questionable". There's no reason to expect the hadronic number to stay where it is. In fact, there's two different hadronic numbers, and they disagree with each other. hep-ph/0402206 is a good place to start.

In short, the hadronic part of the muon mm is way too uncertain to go about claiming a 2 sigma difference is "evidence" of anything.

18. **John Baez**
September 27, 2005

Thomas Larsson writes:

> Dan, given that essentially all experiments are well described by the standard model coupled to gravity, perhaps the ultimate ToE will turn out to be SM coupled to GR. If so, the quest for something beyond the SM (except for gravity) would turn out to be fundamentally misguided.

This ignores the 800-pound gorilla: dark matter! Barring drastic revisions of our theory of gravity, about 23% of the energy density in the universe is dark matter, while only 4% is ordinary matter. (The rest is dark energy – these are the WMAP findings.)

So, while I think it's *crucial* to pay close attention to the details of the Standard Model, I think that someday we'll find a beautiful theory that includes the Standard Model particles *and a bit more*.

Looking for this theory is one of my hobbies….

19. **Dissident**
September 27, 2005

Dark matter? Nah! 🙂
As I have pointed out elsewhere

http://cosmicvariance.com/2005/09/05/dark-matter-and-extra-dimensional-modifications-of-gravity/

there are now several interesting alternatives:

http://arxiv.org/abs/hep-th/0410119

http://arxiv.org/abs/astro-ph/0506370

20. **Dissident**
September 27, 2005

Almost forgot: the current g-2 deviation from the standard model prediction is 1.6 sigma.

http://www.npl.uiuc.edu/exp/g-2/g-2Main.html

By all means, pooh pooh it all you want. 😉

21. **woit**

September 27, 2005

No matter what I write about, sooner or later people want to start discussing their favorite alternatives to GR. Please don't do this here, it's off-topic, and the last thing in the world I want to do is to try and moderate a discussion of this subject.

I've had to delete a bunch of such comments, then deal with people complaining about this and accusing me of censorship. This is not sci.physics and I'm not going to let it turn into that.

22. **Nigel**
September 28, 2005

John Baez wrote:

'This ignores the 800-pound gorilla: dark matter! Barring drastic revisions of our theory of gravity, about 23% of the energy density in the universe is dark matter, while only 4% is ordinary matter. (The rest is dark energy – these are the WMAP findings.)'

All the evidence for dark matter and dark energy is from cosmology models, so why can't the basis of cosmology be re-examined? It is entirely possible to incorporate into general relativity a model for gravity which eliminates the dark matter problem (which is due to the false assumption that gravity has no cause within the universe, correcting that just leaves invisible dust contributions), and the dark energy problem results again from ad hoc force-fitting the theory to the observations. This is not the way to do physics. The theory should not keep getting fiddles to make it fit the facts, that is what went wrong with epicycles in ancient cosmology.

23. **Who**
September 28, 2005

Vafa article posted today—-relates to earlier post by Peter on this blog.
Here is the article:
http://www.arxiv.org/abs/hep-th/0509212
The String Landscape and the Swampland
Cumrun Vafa
9 pages

since this post is not on topic, I'd hardly object were it deleted. Just wanted to let folks know in case of interest.

Into the Swampland

September 28, 2005
Categories: Swampland

Last month Cumrun Vafa gave a talk at Stony Brook entitled The Swamp Surrounding the Landscape. Tonight he has a new paper on the arXiv entitled The String Landscape and the Swampland. Vafa appears to be suggesting that, faced with the huge landscape of possible string vacua and the attendant inability of string theory to predict anything about physics, the thing to do is not to abandon string theory, but to head off into the even larger "swampland" of effective field theories that may or may not correspond to string theory vacua. He gives various arguments for why certain effective field theories may not correspond to string theories, but most of these are just something like "the string theory constructions we have looked at so far can't give this kind of effective field theory". Since one still doesn't know what string theory really is, one probably can't do much better than this. He also assumes that the rank of the cohomology groups of Calabi-Yau threefolds is bounded, which is a conjecture that at least some algebraic geometers don't believe in.

Throughout his article, Vafa assumes that string theory must be true, asking "how" it will connect to experiment, not "whether" it will. For more than twenty years, string theorists have led particle physics deep into a swamp. It seems peculiar in the extreme that Vafa is now suggesting that, instead of hiking back out of the swamp to dry land, particle theorists should push on deeper into the swampland.

Update: Lubos Motl has a posting about the Vafa paper. It includes the news that Andy Strominger
believes that the program has two basic flaws: the conjectures are trivially correct in every theory of quantum gravity independently of string theory; and moreover they are wrong.

Some of my commenters claim that what Vafa is doing is designed to make string theory falsifiable. I don't see this, and Vafa doesn't make this claim himself. This seems to me an example of a common phenomenon. People take a string theory paper that already is going way out on a limb with not very solid arguments, then make a wild extrapolation that goes far beyond what the author claims and use this to promote the importance of the paper in a completely unjustifiable fashion.

To falsify string theory along these lines, one would have to show that it can't lead to the standard model as an effective low energy theory. Vafa doesn't claim this is conceivable, and his arguments can't possibly do this. Most of the examples he gives of effective theories that may not be low energy limits of a string theory are gauge theories of high rank. It's certainly conceivable that one can argue for something like a bound on the rank of the effective field theory gauge group if it comes from string theory, but there's absolutely no reason to believe that such an argument can rule out the rank 4 case we care about (SU(3)xSU(2)xU(1)). I suppose one can argue that, if say Vafa can show the rank must be less than 500, and the LHC discovers a new gauge theory sector with rank 501, string theory would be falsified. But that's kind of like saying that string theory is falsifiable, because if dragons emerge from the LHC interaction regions, string theory would be wrong.

Update: Jacques Distler also has a posting about the Vafa paper. He says he'll wager that it is "far, far from true" that "'anything' is realizable somewhere on the Landscape", and that "we will learn much" if we investigate this swamp. He doesn't explain why it's a good idea for the particle theory community to enter this swamp to investigate it carefully.

Comments

1. **A.J.**
   September 29, 2005

   What Vafa is suggesting is utterly reasonable. Even if you are string theory true believer, you can at least admit that studying models which are at least very similar to string theory is a good way to learn more about string theory.

2. **Anonymous**
   September 29, 2005

   Don't you think you're being unreasonable? You press for string theory to be good science, and argue that it isn't falsifiable. Someone outlines a program that could *make* it falsifiable — granted, a difficult program to carry out — and you complain. Would you be satisfied by anything less than Vafa completely renouncing string theory?

3. **Luboš Motl**
   September 29, 2005

   Dear Peter,

   the fact that some people don't believe that the number of Calabi-Yau topologies is finite does not mean that the physicists are not allowed to investigate physics with the opposite assumption that is consistent with all known facts.

   If one felt pressure not to investigate a scientific theory (or conjecture) just because there exists someone who does not like it, we could also stop doing physics or string theory because some people at Columbia think that physics beyond the Standard Model is not even wrong.

   Such a thing will certainly play no role whatsoever for me. I leave this method of deciding about science to the numerous crackpots who like to visit your blog assuming that it is a serious reading.

   You should be pleased if people start to study swampland – it is a step towards the research of f**kland. I explain on my blog why this should really please you. 😉

   All the best
   Luboš

4. **Wolfgang**
   September 29, 2005

   It seems that the main argument of this paper is that the "landscape" of truly consistent vacua is of zero measure compared to the "swampland" of models only seemingly consistent as semiclassical effective theories. In this sense (far away from the Planck scale) M-theory is predictive and I am waiting for ref [8].

5. **Arun**
   September 29, 2005

   There is a point to this – it does open a way to falsification of string theory. If one understands the types of effective field theories string theory cannot produce, and shows that e.g., our universe is governed by an effective field theory that is not stringy, then we can move string theory entirely to the math. department.

6. **woit**
September 29, 2005

I added something to the posting about the falsifiability issue.

A.J.: I don't understand your argument why this is utterly reasonable. The kind of models Vafa is talking about are atrociously complicated, far more so than the ones in the landscape, (thus the "swamp"). Why is it reasonable to spend time looking at these things? They're hideously ugly and have nothing to do with the Standard Model or the real world. The only justification for entering this yucky swamp is the belief that understanding just how yucky string theory can be is important, given that we already know that it can be very, very yucky. I don't understand why anyone thinks this is a sensible way to spend one's time.

Anonymous: If string theorists like Vafa want to say that string theory is too poorly understood to believe in the landscape, so they intend to keep working on it, that is not unreasonable, and such work sometimes even leads to interesting things (e.g. work on topological strings). But once they accept that the landscape exists and string theory is a radically non-predictive framework, if they are honest scientists, they should admit that the idea should be abandoned.

Lubos: people are welcome to investigate whatever assumptions they want, I was just pointing out that this particular assumption is not one where the experts on the subject (algebraic geometers) are agreed about what is likely to be true. And as to the crackpot level of readers of my blog, have you taken a look a look at your own comment section recently?

Wolfgang: What's a specific experimental prediction of M-theory that comes from Vafa's work?

7. **Wolfgang**
September 29, 2005

Peter,

as I understand it, Vafa's paper outlines a research program on how to make string theory predictive far away from the Planck scale. It argues that there should be a large number of field theories which are not consistent with M-theory and thus should not be observed in nature. The "swamp" of such models should be very large compared to the "landscape" of consistent vacua.
This should/could provide for testable predictions at some point.
The paper does not yet provide examples of such models;
I expect subsequent papers (e.g. ref[8]) to be more specific.

8. **woit**
September 29, 2005

Hi Wolfgang,

I don't see anything in Vafa's paper that comes even close to giving a plausible way of coming up with a legitmate prediction from M-theory. He's assuming the existence of the landscape, and once you do this there seems to be no way to predict anything. The question of how big the swamp is seems irrelevant to making real predictions. I don't see why if he had a serious idea about how to predict something, he wouldn't put it in his first announcement, keeping it secret until the later paper.

9. **A.J.**
September 29, 2005

Hi Peter,

All I was attempting to say — and I hope you'll forgive the vagueness; I spent most of yesterday typing — was that I think studying a far wider class of models is a good idea. I don't care whether people do it with the intent of learning more about string theory, or with the intent of showing that the Standard Model actually lies somewhere in the swamp. The bottom line is that (I'm speaking here as a mathematician) we don't really understand that much about any of these theories. Having a broader selection of examples might teach us a few things.

10. **Brett**
September 29, 2005

I think it's a perfectly reasonable idea that there should be vastly more field theories that cannot exist as string theories than those which actually could arise as string theory vacua, say. However, given the complexity of either kind of theory, I would expect that that boundary between the two in parameter space is probably not going to be anything simple; it will likely have a fractal struture, and it is my guess that any randomly selected theory, even if it exluded as a string theory, will nonetheless be "close" to an string theory version.

11. **Renormalized**
September 29, 2005

I sometimes wonder if Lubos Motl is working on any publishable material or has any original ideals about physics. His real talent is in making useless arguments about crackpots and those less intelligent than himself. This includes the whole world since he thinks of himself as the smartest of all. It is easy to insult others work and ideas when you have none of your own to be criticized. His pompous imperturbable egoism is as worthless as his contributions to science.

12. **Not a Nobel Laureate**
September 29, 2005

You're all wrong.

The Theory of Intelligent Falling is the Answer.

Evangelical Scientists Refute Gravity With New 'Intelligent Falling' Theory

http://www.theonion.com/content/node/39512

August 17, 2005 | Issue 41•33

"Closed-minded gravitists cannot find a way to make Einstein's general relativity match up with the subatomic quantum world," said Dr. Ellen Carson, a leading Intelligent Falling expert known for her work with the Kansan Youth Ministry. "They've been trying to do it for the better part of a century now, and despite all their empirical observation and carefully compiled data, they still don't know how."

13. **Matti Pitkanen**
September 29, 2005

The proposed theoretical activities in infinite-dimensional infinite-volumed swampland unavoidably create an association to monkey, typewriter, and Shakespeare's sonnet. Another association is a story in Mathematical Intelligence about Baron Monty Carlow and his ingenioius method of measuring the area of lake by randomly bombing the county and counting the fraction of bombs hitting the lake.

The pioneers of Monte Carlo method of TOE building
deserve all encouragement in their challenging task,

Matti Pitkanen

14. **Arun**
September 29, 2005

If our universe is indeed hidden somewhere in the landscape, what is the rational reason for abandoning string theory? Nature could be both subtle and malicious.

15. **Aaron Bergman**
September 30, 2005

Some of us continue to hope otherwise, though.

16. **woit**
September 30, 2005

Arun,

Before you start believing in something completely absurd like the landscape, how about first asking for some experimental evidence? I can't believe people are abandoning the most elementary aspects of the scientific method. This is something we are teaching to children in elementary school these days, why can't professional physicists understand this?

A less elementary aspect of the scientific method is that you can always try and make a wrong idea work by making it more and more complicated, desperately trying to avoid confrontation of the idea with experiment. It should be completely obvious by now that this is what is happening with string theory.

17. **Wolfgang**
September 30, 2005

Peter,

Jacques Distler conjectures on his blog that only models with 3 fermion generations are consistent (in Vafa's sense) but not 1 or 2.
Would you consider this a valuable post-diction ?
(It would have been really nice as a prediction, but sometimes the calculations just take a looong time.)

18. **rrtucci**
September 30, 2005

Peter, just had an idea for "the special-edition cover" for your book: a picture of Lubos' head (wearing a harvuhd tie), the head superimposed on the body of a tiny ant, which is at the focal point of a large magnifying glass, which in turn is being illuminated by the sun. Maybe also a smarter ant, with Witten's head, running away. And an ant with Vafa's head caught in a swamp.

19. **woit**
September 30, 2005

Wolfgang,

No, this is not a prediction or post-diction, since you have no idea of what the masses of the particles are. Remember, experimentally all we know is the number of light generations (more specifically, the number of generations with a neutrino less than half the mass of the Z). It's entirely possible that the number of generations is really >3. If we had only observed 1 or 2 generations, and string theory really needed at least 3 generations, we wouldn't falsify string theory because more generations could just be at higher mass.

In general, a "prediction" of the number of generations is operationally meaningless without also knowing at what mass scales they are going to occur.

I'm no expert on these constructions, so I'm not sure which difficulties in constructing models with one or two generations Jacques is referring to. My guess is that it is the fact that in the standard way of using Calabi-Yaus, the number of generations is the Euler characteristic, and it is hard to construct Calabi-Yaus of Euler characteristic one or two. But I suspect there are many ways around this, and that an expert in these constructions could come up with one way or another of getting one or two generations if they really wanted to. Again, since you don't know the masses, one or two light generations, the rest heavier than we can observe, is no problem.

20. **Arun**
September 30, 2005

Peter,

Why don't I ask for experimental evidence? Because I know there is none. But if we're into dealing with reality, then it is a given that Vafa, Motl, Douglas, etc., are not going to abandon string theory any time soon, or resign from the physics department. Then, all I can ask for is that they pursue the approach that is most likely to produce an experimental signature for string theory. Is this approach the right one? I don't know, but until the string theorists explore it a bit more, we won't know.

-Arun

21. **woit**
September 30, 2005

Arun,

I think by now it's completely clear that the things that are known about string theory are insufficient to make any contact with physics and predict anything. Under these circumstances, it's not unreasonable to try and come up with some better understanding of string theory, e.g. to work on trying to figure out what M-theory is. It is unreasonable to deny that this is the situation and promote the idea that you can get physics out of the Landscape.

22. **Shantanu**
September 30, 2005

    Peter have you read hep-th/0509157 ? any comments on that?
    It has been discussed at http://web.mit.edu/cabi/www/blog/

23. **woit**
September 30, 2005

    Shantanu,

    Yes, I saw that. I was thinking of putting together a posting very soon of various links including that one. I'm not sure what one can sensibly say about it though. Do you see anything in that paper that looks at all like a promising idea about how to connect string theory to real world particle physics? I didn't.

24. **Not a Nobel Laureate**
October 1, 2005

    Looks like Vafa et al have some serious competators . . .

    Two female gorillas have been photographed using sticks to get through the swampland . . .

    http://images.ctv.ca/archives/CTVNews/img2/20050930/160_ap_gorilla_tool_050930.jpg

    http://www.ctv.ca/servlet/ArticleNews/story/CTVNews/20050930/gorillas_tools050930/20050930?hub=CTVNewsAt11

Thinking Big

September 30, 2005
Categories: Uncategorized

Philip Anderson has a piece in the latest Nature entitled Thinking Big. It's about the interpretation of quantum mechanics, and in it he claims that Fritz London was the first one to really have the right idea about the problem. Commenting on the Bohr-Einstein debates on the subject, Anderson says "In reading about these debates I have the sensation of being a small boy who spots not one, but two undressed emperors." Instead of the Bohr or Einstein positions, Anderson promotes a point of view he attributes to London, who wrote a paper about it in 1939 with Edmond Bauer. He says "Taking London's point of view, one immediately begins to realize that the real problem of quantum measurement is not in understanding the simple electron being measured, but the large and complicated apparatus used to measure it" and that "The message is that what is needed is an understanding of the macroscopic world in terms of quantum mechanics."

I take Anderson's point to be that the classical physics of a measuring apparatus is an "emergent phenomenon", and understanding this is the real problem of interpreting quantum mechanics. He ends with his favorite slogan: "more is different!".

Comments

1. **Eric Dennis**
   September 30, 2005

   Interesting that your paraphrase of Anderson's comments is exactly the position of Einstein and exactly the opposite of Bohr's position. Specifically, Einstein demanded that the previously known physics on the large scale emerge deductively (at some appropriate level of rigor) from the new microscopic theory. Bohr wanted to retain classical objects (measurement devices) as irreducible elements of the new theory. As I understand it, this, and not any question of indeterminism, was at the heart of Einstein's criticism of Copenhagen QM.

2. **Who**
   September 30, 2005

   Not on topic and may fairly be erased: thought NEW readers might like to look at the Freeman Dyson article on Feynman
   http://www.nybooks.com/articles/18350
   occasioned by the publication of a collection of Feynman's letters.
   Dyson sketches his own ideas of Feynman's style of thought, approach to physics, accomplishments, character.
   Free download—not restricted to paying subscribers of the NYRB.

3. **woit**
   September 30, 2005

   Just added that Dyson article to a new posting containing a list of assorted interesting links.

4. **Maynard Handley**
   September 30, 2005

   Anderson (like Robert Laughlin) seems to following in Chomsky's footsteps. For thirty years he has been peddling emergence (like Chomsky's deep structure), yet I still have no concrete idea what he is actually talking about. My understanding is that what he means is something like saying "a computer can look at the waveform of speech, but it does not *understand* speech, ie there is something more to speech than just the waveform" to which one can reply
   * the theological position: yes there is some sort of magic fairy dust that animates speech and macro-physics
   * the Dawkins position: there is no fairy dust, the waveform is all there is, you just lack the appropriate way (so far) to extract info from the waveform

   Anderson and followers irritate me because they seem to speak in the theological mode, but when pushed revert back to the Dawkins mode. If you accept Dawkins, then WTF is the big deal? Yes, we don't have all the appropriate tools yet, but no-one, not even Weinberg or any of the other Anderson demons claims that, so what it their beef. Yes, they want more money for solid state research, I get that, but this mystical mumbo-jumbo is not the way to get there.

5. **Eric Dennis**
   September 30, 2005

   Maynard — I agree with you about the slipperiness of this "emergence" concept as used by Anderson and Laughlin. The latter's popular book does a very poor job at trying to spell out the ideology and his papers aren't much better.

   It's a shame because the emergent quantum gravity idea (the geometry of space-time as a mathematical convenience for describing the physical effects of a condensed matter ether) is intriguing and fully consistent with your "Dawkins" position.

6. **Arun**
   September 30, 2005

   The "appropriate way" to extract information from the speech waveform is the emergent property.

7. **Chris W.**
   October 1, 2005

   To expand on what Arun said, the problem of interpreting (extracting information from) a speech waveform is very much like interpreting a data stream coming over a wire. If one knows nothing about network protocols and encoding methods one will have a hell of a hard time getting beyond a crude physical and statistical description of the signal. (Ditto for interpreting signaling in the nervous system.)

   What is emergent, in the sense that biological complexity is emergent, is the scheme for speech generation and processing that our biological and cultural evolution has worked out over several million years. At the physical level what is being produced is still just some aperiodic acoustic signal (often supplemented by hand gestures and facial expressions). We *homo sapiens* have a grasp of this scheme the way a batter has grasped a solution to the problem of connecting his bat with a baseball coming at him at 80+ mph. Objectifying this grasp of the problem, and implementing it in a computing device, is inherently difficult; nobody has or will supply us with the relevant technical specs or theoretical background.

8. **Chris W.**

October 1, 2005

Reinforcing the first comment (posted by Eric Dennis) John Stachel asserted 15 years ago in his contribution to Conceptual Problems of Quantum Gravity (Ashtekar and Stachel, editors) that Einstein's position on the indeterminism of quantum theory has been widely misunderstood. He said that Einstein objected to the fact that the theory offers no explanation for the *extent* of its statistical description. If the physical world is not (in effect) a deterministic machine, then why shouldn't it be so totally chaotic as to utterly defeat any attempts to rationally investigate it?

Fortunately for us (and for that matter, all living things) such radical chaos does not reign, but Einstein felt that we still don't understand this as well as we can and should; quantum theory is not the last word on the subject.

(By the way, in the last few months Stachel has produced some *interesting* papers on foundational questions in quantum gravity.)

ICM 2006

September 30, 2005
Categories: Uncategorized

The International Congress of Mathematicians (ICM) takes place every four years and is the most important international conference in mathematics. The 2006 ICM will take place next August in Madrid. One thing that happens at each ICM is the announcement of the winners of the Fields Medal. This has traditionally been considered the most prestigious award in mathematics, and the closest analog to a Nobel prize in math, although the recently instituted Abel prize may now compete for this honor. The Fields medal is awarded to between two and four people at each ICM, and recipients must be under the age of 40 on Jan. 1 of the year of the ICM. I have no inside information about who will win this year, but in gossip with mathematicians two names that tend to come up are those of Grigori Perelman (for his work on the Poincare conjecture), and Terence Tao.

The other important thing about the ICM is the list of invited talks. The speakers are carefully chosen and are supposed to be people who have done the most important work in mathematics during the past four years. Looking over the list of speakers gives a good idea of who the most prominent names in the business are, as well as what are the hottest topics. It's an especially great honor to be chosen as a plenary speaker, and the names of these have been recently announced. The invited speakers in the various sections have also been announced, one section covers mathematical physics.

Comments

1. **Zelah**
   October 1, 2005

   I have been reading your blog regard ICM2006.

   Now Grigori Perelman is problematic, as I believe he may be 40 next year! This has come up before! Infact, this problem is quite serious for mathematics as frankly, essentially all of the TRUELLY great mathematics in the last decade has been done by the over forties (e.g. Oded Schramm of Brownian Frontier Fame!) So much for math being a young man's game!

   Now, you have really got my interest with Terence Tao. Now as an amateur mathematician I am a sort of a math junkie and have been looking at the work of various mathematicians over various fields
   I investigated Terence Tao because of his work on harmonic analysis!

   But I must admit he was not on my list of Field Medallists! I am interested in why Terence Tao work crops up! Please explain!

   Here is my list of Field Medallists.

   1. Warwick Tucker

   Warwick Tucker has given a rigorous proof that the Lorenz attractor exists for the parameter values provided by Lorenz. This was a long standing challenge to the dynamical system community, and was included by Smale in his list of problems for the new millennium.

   I must add that He is working within a new computational field called interval analysis which allows one to solve simultaneous ODE RIGOUROUSLY. Now if you have tried to solve Nonlinear ODE, you will realise this is one of the holy grails of Applied Mathematics!

   2. Elon Lindenstrauss

   Already, in joint work with Katok and Einsiedler, he has used some of the ideas in this work to prove the celebrated conjecture of Littlewood on simultaneous diophantine approximation for all pairs of real numbers lying outside a set of Hausdorff dimension zero

   What I love about this work is that it continues the pathbreaking work of in my opinion, the greatest female mathematician of the 2nd half of the 20th Century, Maria Ratner and her theorem about unipotent flows.

   I believe that the work on unipotent flows will be the next revolution in function theory. In particular, I believe it will lead to solutions to many problems in Quantum Field Theory eventually! Watch out!

   3. Mark Groves

   Someone who has no chance in reality, but his work is something I fully understand!
   Winner Richard Von Mises prize in applied mathematics.

   What did he achieve?

   Solutions to something called the Water Wave problem. But what is important to me is that they confirm an intuition about Nonlinear Wave I have always held.

   Essentially, for a large class of multidimensional nonlinear systems, the behaviour moves from linear to solitons to chaotic along various semigroups!

   This very simple statement actually provides a small piece of a puzzle about chaos, which has been mysterious for a long time. Basically how does chaos create order in nature? Well, it appears that certain nonlinear systems generate semigroups spontaneously, which does not align itself with the natural semigroup time! This means that chaotic functions can be thought of as acting like solitons in a highly nonlinear environment.

   Unfortunately, I do not have any women for the Field Medal at this time. Do not get me wrong, there are many female mathematicians doing great work, but difficult to discern if it is of Field Medal Caliber.

   An Amateur Mathematician

2. **Zelah**
   October 1, 2005

   I have had a look at the ICM2006 site, and it appears that there is a new prize called the Gauss Prize.

   Sound interesting.

An Amateur Mathematician

3. **John Baez**
   October 1, 2005

   An Amateur Mathematician exclaims:

   > I investigated Terence Tao because of his work on harmonic analysis!
   >
   > But I must admit he was not on my list of Field Medallists! I am interested in why Terence Tao work crops up! Please explain!

   He and Ben Green showed there are arbitrarily long arithmetic progressions of prime numbers.

   And, he's won some prizes already for other things, including his work with Allen Knutson, in which they proved Horn's conjecture. This says what the eigenvalues of a matrix A+B can be, given the eigenvalues of two hermitian matrices A and B. It's equivalent to a lot of other nice conjectures.

   So, he's cracked some hard problems that are easy to state.

4. **a graduate student**
   October 2, 2005

   Just to keep the gossip going, Dennis Gaitsgory (recently tenured at Harvard) and Manjul Bhargava are two other names that have popped up during tea at my department.

5. **Pindare**
   October 2, 2005

   The only plenary speaker under 40 (if my google checks are ok) is Terrence Tao, so that's in the bag IMHO. Besides he's really a good bet as John said, with lots of prizes and big invited lectures already (next are his Chern lectures at Berkeley.)

   In fact, he's got such a wide array of expertise that there's already only a few people left who could possibly introduce all his work (Bourgain, Gowers…) so the Fields commitee can't really wait much longer anyway… 😉

   As for Perelman, well if his birthdate is indeed the one given on wikipedia, then he's definitely *done* his work before being 40…
   http://en.wikipedia.org/wiki/Grigori_Perelman

   Personally I'd expect Artur Avila to win it one day, barely 26 and already quite impressive…

6. **Zelah**
   October 2, 2005

   Hi Everyone!

   Thanks for all of the wonderful tips!

   Unfortunately from my point of view, it appears that the bias against applied mathematics will continue. I am hoping that the Gauss Prize will correct this obvious problem and they will pick someone really wonderful like Kiyosi Ito of Ito Calculus fame.

   I am also hoping that the Abel prize commitee pick a Woman next time as there are in my opinion women who are deserving. Karen Uhlenbeck or Maria Ratner would do fine!

   An Amateur Mathematician

Is N=8 Supergravity Finite?

September 30, 2005
Categories: Uncategorized

Zvi Bern gave a talk yesterday at the KITP in Santa Barbara entitled The S-Matrix Reloaded: Twistors, Unitarity, Gauge Theories and Gravity. He surveyed recent progress on computing perturbative amplitudes in QCD and N=4 supersymmetric Yang-Mills, some of which involves using twistor methods. The most striking thing though were his last few transparencies (here, here, and here). He notes that all previous studies of divergences in supergravity rely only on power-counting and supersymmetry, assuming that if these two principles allow a divergence to occur, it will. Actually doing the full computation to see if the divergences are there is too hard and no one has done it. Bern notes that in these arguments the extra structure seen by the recent twistor methods is not taken into account, and when one does this, so far all complete calculations show that N=8 supergravity has exactly the same degree of divergence as N=4 Yang-Mills, even though one would naively expect the supergravity amplitudes to have worse behavior. He ends by suggesting that "Serious re-examination of the UV properties of multi-loop N=8 supergravity using modern tools is needed."

If N=8 supergravity turns out to be renormalizable, this raises an interesting question about string theory….

Comments

1. **Ben Compson**
   September 30, 2005

   I don't understand — supergravity has been around for at least 25 years. How can we only now be asking if N=8 supergravity is renormalizable??? What else have theorists been up to all these years?

2. **woit**
   September 30, 2005

   "What else have theorists been up to all these years?"

   Hmm, have you heard about this idea called string theory?

   More seriously, divergences in N=8 supergravity don't occur until at least 5 loops. Ever tried to do a 5-loop calculation in N=8 supergravity?

3. **Wolfgang**
   September 30, 2005

   Since 3 loops is good enough for superstrings, 5 loops should be more than sufficient ?

4. **logopetria**
   October 1, 2005

   Sorry to inject such a basic question, but this is something I've never been able to find out: in these "N=4", "N=8" etc theories, what does the 'N' denote? Is it a variable that can take on arbitrary integer values? Is it like a coupling constant, or what?

5. **Urs**
   October 1, 2005

   > Is it like a coupling constant, or what?

   N is the 'number of supersymmetries'. Very roughly and schematically, this means there are N different odd-graded quantities that square to the generator of (time) translation.

6. **logopetria**
   October 1, 2005

   Thanks. I'm not sure I understand much, but at least I know *what it is* that I'm not understanding! Does "the number of supersymmetries" dictate how many superpartners each ordinary particle has? That is, does "N=1" say that the electron is partnered with the "selectron", while "N=2" say that there are 2 flavors of selectron? Or am I way off target here?

7. **Field-Theorist**
   October 1, 2005

   Even if N=8 happen to be finite, it is not clear that we can embed the standard model in it. Moreover, in contrast to string theory there is no deep reason why it should be finite. Historically, until the first superstring revolution it was considered as an interesting theory, but soon after the superstring revolution people abandoned it.

8. **woit**
   October 1, 2005

   logopetria,

   Basically, yes. When you have multiple supersymmetries, particles come in multiplets more complicated than just fermion-boson pairs.

   Field theorist,

   If Bern is right, N=8 supergravity may be renormalizable because of a combination of supersymmetry and twistor geometry, which would be arguably a deeper reason (symmetry) than the reason for the (conjectured) finiteness of the superstring.

   Yes, it's hard to get the standard model out of N=8 supergravity, which is one reason people gave up on it. But this may be a much more fruitful starting point than string theory, where the problem is not that you can't get the standard model, but that you can get anything. And working with a well-defined theory instead of a vague hope that a theory exists might also be a good idea.

   The reason people gave up on N=8 supergravity was not just the problems getting the standard model, but everyone believed it was non-renormalizable. At this point, about the only argument you can give for string theory is that it is the only way to combine gravity and quantum mechanics. If that collapses, I suppose string theorists will keep on arguing for string theory unification, but they won't have any legitimate arguments left.

9. **Moshe Rozali**
   October 1, 2005

   This is fascinating, I was under the false impression that Bern and company proved that theory to be non-normalizable, but apparently that was for 11dim SUGRA. Zvi has a review in living reviews in physics (which presumably gets updated regularly) on the subject, and conjectures about N=8 SUGRA being more finite than it should be apparently already appeared before the twistor connections.
   \
   In case N=8 turns out to be finite because of some deep relation to twistor space, that would be absolutely spectacular. Right now it is not clear though (to me) what could be this deep structure (maybe a string theory? just kidding…).
   \
   I am wondering though how can one break SUSY and get more or less realistic spectrum (I was in high school when these things were all the rage). If one has to add matter and thus break the symmetry, the magic is likely to be gone.

10. **woit**
    October 1, 2005

    Hi Moshe,

    I was in grad school at the time, so I did learn a bit about this, but I've forgotten most of it and would have to go look this stuff up. From what I remember, unless you start adding other things in, two big problems are that

    1. you get an SO(8) gauge theory, and SO(8) isn't big enough to include the standard model gauge groups.

    2. the spectrum is vectorlike, not chiral as needed for the standard model.

    As for supersymmetry breaking, that's always been a huge problem for any supersymmetric theory.

    Even if the theory is renormalizable, you're right it's not clear this would survive whatever one did to try and get around the above problems.

11. **John Baez**
    October 1, 2005

    Thanks for pointing out this talk by Zvi Bern – it's really interesting!

    For one thing, it shows that you should never trust a physics "folk theorem" until you see how people convinced themselves of it, and know what the loopholes are.

    For another thing, it's more evidence that there's a lot left to understand about perturbative quantum field theory. Connes and Kreimer have been doing cool work on this subject for a while, and now comes this twistor business. I wish I understood more about both!

    If you read Predrag Cvitanovic's autobiographical remarks entitled Search Without a Plan, you'll see he's one of the few people who have added up thousands of Feynman diagrams in QED… and you'll see he discovered something interesting!

    While field theorists typically guess that the sum of all nth-order diagrams grows like

    n^n (alpha/pi)^n

    with the whopping nn term coming from the combinatorial explosion in the number of diagrams, he found empirically that there were lots of cancellations, leaving a result more on the order of

    n (alpha/pi)^n

    If this were actually true, one could sum all Feynman diagrams in QED and get a finite answer! – contrary to the usual folk wisdom.

12. **Moshe Rozali**
    October 1, 2005

    John,

    I am confused, I thought we already know by the existence of non-perturbative effects (in asymptotically free theories) that perturbation theory cannot converge, regardless of any diagram counting. Maybe there is something special about QED, or maybe one cannot really check explicitly any statements about asymptotic values of n…

13. **Robert**
    October 2, 2005

    I think the world expert regarding counter terms for gravity theories is Anton van de Ven. He worked for several years to show that the two loop divergence for (N=0) gravity is in fact there but was beaten by Goroff and Sagnotti:

    QUANTUM GRAVITY AT TWO LOOPS.
    By Marc H. Goroff (Caltech), Augusto Sagnotti (UC, Berkeley & LBL, Berkeley),. CALT-68-1263, UCB-PTH-85/18, LBL-19512, Apr 1985. 13pp.
    Published in Phys.Lett.B160:81,1985

    and

    TWO LOOP QUANTUM GRAVITY.
    By A.E.M. van de Ven (Hamburg U. & SUNY, Stony Brook),. DESY-91-115, ITP-SB-91-52, Oct 1991. 51pp.
    Published in Nucl.Phys.B378:309-366,1992

    The first paper is not covariant whereas van de Ven uses covariant methods (background field gauge, heat kernels etc). The last twenty years, he has worked on the N=1 version of this, where the divergence is supposed to appear at three loop level. As an intermediate result, he (and his student Jan Peter Börnsen) did a three loop YM calculation.

Anton supevised my master thesis (Diplomarbeit) which was about some minor property of heat kernels in superspace. I should not forget to mention that he now lives in Utrecht and has a teaching position at some high school type institution.

14. **John Baez**
October 3, 2005

Moshe Rozali writes:

> I am confused, I thought we already know by the existence of non-perturbative effects (in asymptotically free theories) that perturbation theory cannot converge, regardless of any diagram counting. Maybe there is something special about QED, or maybe one cannot really check explicitly any statements about asymptotic values of n…

Yes, it's confusing – as Cvitanovic himself admits, his observation is "heretical". His calculations only apply to QED, not asymptotically free theories. But even here, an old argument by Dyson suggests that the power series in the coupling constant can't really converge: if it did, there should be a sensible version of electrodynamics for particles of small imaginary charge, which would *attract* each other… and Dyson argues that this is implausible.

Maybe someday someone will compute the magnetic moment of the electron in QED up to a really huge order in the fine structure constant, and we'll see if Cvitanovic's observations hold water.

Or, maybe someday people will discover some math that sheds more light on these issues.

15. **Arun**
October 3, 2005

Why can't the QED perturbation series sum to something that has a branch cut going from zero to negative infinity?

16. **Moshe Rozali**
October 4, 2005

Arun,

Such function is not analytic at zero, in other words does not have a convergent power series expansion in small coupling.

Assorted Links

September 30, 2005
Categories: Uncategorized

Some assorted things I've run across recently that may be of interest:

Talks from the annual meeting of the SLAC Users Organization.

A dialogue between Barry Mazur and Peter Pesic about imagination and mathematics.

An Op-Ed piece in today's New York Times by my colleague Brian Greene called That Famous Equation and You.

Some sensible comments by John Baez about string theory.

A survey of the state of string field theory by Leonardo Rastelli.

A "description of some important issues in supersymmetry and string phenomenology" entitled Twenty-five Questions for String Theorists. The authors think these questions may have answers that will help connect string theory and phenomenology, although this seems to me unlikely. Serkan Cabi also has some comments on this paper.

An article about Feynman by Freeman Dyson in the latest New York Review of Books.

A talk about theoretical physics in the Netherlands.

Update: One more, a report by Paul Cook on an interesting talk by Roman Jackiw.

Comments

1. **Arun**
   September 30, 2005

   John Baez wrote (from the URL provided)

   > The saddest the LHC could find is a complete
   > confirmation of the Standard Model, Higgs particle and all. Then we're
   > back to pondering these puzzles without benefit of extra clues.

   Could the absence of extra clues be a clue? The good thing would be that one doesn't have to wait for the LHC to start working on that. 🙂

2. **Wolfgang**
   October 1, 2005

   I have to assume that in the current environment any deviation from the standard model observed at the LHC would be (initially) understood as indication of supersymmetry.
   A good example was the deviations recently observed for g-2 experiments.

3. **Alejandro Rivero**
   October 1, 2005

   After two years in the swamp, my position is that we already have extra clues. Wasting the "single self-advertisment post" rule that Woit gave time ago, here come three examples from my own:

   hep-ph/0405076 (and related preprints) indicates that a "lamb balance" could be hinting us the points where massive particles are.
   hep-ph/0505220 (and Dr. Koide's work cited there) hints that quarks and leptons should be, if not composite, supersymmetric to diquarks and mesons.
   hep-ph/0507144 hints that the above is related to the properties of the Z0 particle. Note also that the decay of Z0 has properties usually derived from GUT theories

   If LHC comes with confirmation of standard QFT (eben with non-minimal higgs), I'd hopw theorist to start looking seriously for missing extra clues. Also, at these times the LEP colaboration files should already be open in the field for everyone to explore, should them?

4. **John Baez**
   October 1, 2005

   Wolfgang writes:

   > I have to assume that in the current environment any deviation from the standard model observed at the LHC would be (initially) understood as indication of supersymmetry.

   Yes, I worry about that too, but I believe (and hope) that the experimentalists are more careful about these things than the superstringers.

   I recently participated in a PhD oral exam of a physics student who is building detectors for the LHC. He spoke about signatures of nonminimal Higgs bosons. One of the physicists on the committee noted that there are ways to tell the difference between various kinds of nonminimal Higgs bosons and the specific sort which appears in the MSSM (the minimal supersymmetric extension of the Standard Model). So, while superstringers will initially proclaim any hint of a nonminimal Higgs as evidence for the MSSM and "therefore" as evidence for string theory, experimentalists are less likely to jump on this bandwagon without good evidence.

   The student also claimed that among the 105 (!) adjustable parameters in the MSSM, only a couple affect the behavior of the Higgs. So, it may not be quite as much of a morass as I'd thought. I'm still a bit concerned, though.

   You probably know the old saying:

   With 7 parameters you can fit an elephant.

I had worried that with a 105 adjustable parameters, even the LHC short-circuiting and exploding could be explained by the MSSM.

People interested in this issue may enjoy reading about a computer program called Fittino that's supposed to do a best fit of 24 MSSM parameters based on LHC data.

5. **Aaron Bergman**
October 1, 2005

There are not 105 adjustable parameters in the MSSM. There are ~100 (109 is the number that comes to mind, but whatever) adjustable parameters in the soft susy breaking lagrangian. One expects many relations amongst them from however susy ends up being broken.

6. **woit**
October 2, 2005

But until someone figures out what the susy breaking mechanism is, John is right that there are at least around 100 adjustable parameters. The idea that SUSY exists in the real world and that SUSY is broken by some physics that can be parametrized by a small number of parameters looks more and more like wishful thinking. People have been trying to do this for more than 25 years, and there still is no theoretically convincing model for supersymmetry breaking, or even a shred of experimental evidence.

7. **Aaron Bergman**
October 2, 2005

I'm curious about "more and more" there? SUSY itself has some trouble with the Higgs mass, but SUSY breaking looks just as ugly as it always has. I don't see any change there.

Regardless, the weak susy breaking lagrangian is a phenomenological lagrangian. There are plenty of real problems with susy, but the number of parameters there isn't one of them.

8. **woit**
October 2, 2005

By "more and more" I mean that, besides the increasing amounts of fine-tuning needed, as people have learned more and more about supersymmetry breaking it's become more and more clear what a gory business it is. 25 years ago one could have thought that this was just because there hadn't been enough work on the subject, so people hadn't had time to discover a simple way to break supersymmetry that would lead to some predictions. As time goes on, it get more and more clear such a thing probably doesn't exist.

9. **John Baez**
October 2, 2005

Aaron Bergman writes:

> There are not 105 adjustable parameters in the MSSM. There are ~100 (109 is the number that comes to mind, but whatever) adjustable parameters in the soft susy breaking lagrangian. One expects many relations amongst them from however susy ends up being broken.

Maybe so (or maybe not). But, I was talking about experimentalists with the the practical chore of looking for hints of supersymmetry in the LHC data. Unless someone figures out some relations before data analysis begins (optimistically around 2008), these folks have about 100 adjustable parameters to fiddle with. And they're busy trying to figure out how.

I'm no expert on this stuff. I got the figure of "105" from various including the "Fittino" program I mentioned – check 'em out! This figure apparently excludes the roughly two dozen parameters already built into the Standard Model.

10. **Alejandro Rivero**
October 3, 2005

One of my interests last year has been to try a measure of information content beyond "free parameters". Of course I haven't got any succes. The only reasearch I am aware of comes from a bayesian statistics, I.J.Good. The point is that the act of selecting a theory, or a method of symmetry breaking, or a special set of islands, can also be considered as information you input in, and it should be counter between the parameters if there are in the town other theories (or symmetry breaking patterns, or sets of islans) equally sound.

11. **Arun**
October 3, 2005

Hmmm:
http://arxiv.org/abs/hep-ph/0412012

> It is impossible to determine all 105 possible parameters of L_MSSM simultaneously. Therefore, assumptions on the structure of L_MSSM are made. All complex phases are set to 0, no mixing between generations is assumed and the mixing within the first two generations is set to 0. Thus the number of free parameters is reduced to 24 (MSSM-24).

24 doesn't seem so bad.

Mucking About in the Swampland

October 1, 2005
Categories: Swampland

A little while ago I wrote about the recent Vafa paper on [The String Landscape and the Swampland](), as well as about postings on the subject by [Lubos Motl]() and [Jacques Distler](). Lubos's contribution to the subject was introducing the new terminology of "s**tland" and "f**kland". Jacques's was to claim that you can't get anything you want out of string theory, his main example being the supposed impossibility of getting one or two-generation models. This didn't sound right to me, but I'm no expert on the subject. Well, it turns out Jacques had no idea what he was talking about, which Volker Braun [pointed out to him in a comment]().

Given the high quality of the comments by Lubos and Jacques, I was surprised to see that if you look at the [trackback page for the Vafa paper](), you'll note that [trackbacks]() to their postings are there, but not to mine, which evidently has been censored. Not all my trackbacks have been censored, but it appears that, as far as papers about the Landscape and the Swampland are concerned, the arXiv policy is that trackbacks to postings about the subject that are ignorant or scatological will be allowed, but not ones critical of the whole idea.

Update: I've heard from someone associated with the arXiv that it's not their intention to allow trackbacks to my postings to be censored and that part of the problem has been both difficulties they've been having with new software and with deciding how to handle moderation of trackbacks. A trackback to my posting on the Vafa paper is now there. Jacques Distler has [updated his posting]() to include an explanation of Volker Braun's proposed construction of a one-generation model.

Comments

1. **Bryan**
   October 1, 2005

   ' … arXiv policy is that trackbacks to postings about the subject that are ignorant or scatological will be allowed, but not ones critical of the whole idea. '

   Those guys have a right to find a way of having some fun. Be kind to them and they might love you more.

2. **anon**
   October 1, 2005

   Guess who the moderators for string theory at arxiv are:
   physics.HT High-Energy — Field Theory and String Theory (Jacques Distler, Paul Fendley).
   I hope arxiv's policy is to remove unethical moderators

3. **Wolfgang**
   October 1, 2005

   I guess this was just one (among many) comjectures …

4. **Not a Nobel Laureate**
   October 1, 2005

   Two female gorillas have been photographed using sticks to get through swampy areas . . .

   http://images.ctv.ca/archives/CTVNews/img2/20050930/160_ap_gorilla_tool_050930.jpg

   http://www.ctv.ca/servlet/ArticleNews/story/CTVNews/20050930/gorillas_tools050930/20050930?hub=CTVNewsAt11

   Can't wait for the first "Stick Theory – How to Navigate the Swampland" pre-prints

5. **Not a Nobel Laureate**
   October 1, 2005

   Looks like Vafa has some serious competators . . .

6. **D R Lunsford**
   October 1, 2005

   Astounding.

   -drl

7. **Luboš Motl**
   October 1, 2005

   Dear Peter,

   as some readers have pointed out, I am not the first one who wrote about Scotland and the Falkland Islands.

   It would be interesting if you constructed a one-generation or two-generation model. I don't claim that no one will be able to do it, but I am pretty sure that *you* won't be able to do it. 😉

   Incidentally, Cumrun is saying so incredibly nice things about you. Too bad that you can't reply in a similar fashion. And Cumrun deserves it so much!

   All the best
   Lubos

8. **woit**
   October 1, 2005

Hi Lubos,

Sorry for misinterpreting your island terminology….

I've had the highest respect for Cumrun's intellect since we were both graduate students, and also for his many achievements since then. It's precisely because he is such a talented physicist and mathematical physicist that I find this swampland business upsetting. Seeing fools doing this kind of thing wouldn't be surprising or worth much comment, but seeing the best people in the field do it is shocking and depressing.

I think physics has suffered immensely during the last few years as people like Cumrun, the best theorists in the world, have devoted their impressive talents and energies to trying to somehow make a failed idea work, instead of giving up on it and trying to come up with something new. I'm not happy to be spending my time criticizing what Cumrun and others are doing, but very much wish things were otherwise.

So, I think you're right I should try and balance my criticisms and say some of the nice things about people that I think. The above applies not just to Cumrun, but somewhat to you too Lubos (although renormalized a bit…)

Peter

9. **Luboš Motl**
October 1, 2005

Dear Peter,

thanks for having said nice things about Cumrun. It was the easier part of the goal of my comment, of course.

If you think that this kind of research – and probably not just this one – is not useful, you may imagine that such a criticism will only feel material – as opposed to hot air – once you offer some new vision that may have a chance to be more useful.

Sorry to say, but to worship the divine power of the Dirac operator for 24 hours a day is not yet the right solution.

All the best
Luboš

10. **D R Lunsford**
October 1, 2005

Lubos, I love the way you tell everyone what they can and can't do, when we don't have one original line from you yet. One of these days you'll have to stop living off your boyish charm and have an idea.

-drl

11. **Quantum_Ranger**
October 2, 2005

After reading the Vafa paper, one can only conclude that the Swampland is actually more like Quicksand! and the island made out of Quicksand rather than any stable material.

Any theorist venturing forth had better make sure they have all the equipment needed to survive?

Clampons for attaching some pretty heavy/lightweight Strings, a self-preserving life jacket, patented of course, and made from "bubble-wrap" as opposed to B-O-N.

Oh yes, and no theorist should venture forth unless they have the latest up to the minute pair of "Anti-Anthropic Gravity boots", to get out of the quicksand, they are going to have to do a lot of Anthropic Walking..which is really a transform of 'Talking' , as opposed to any physical reality act of actually "walking".

Talk is cheap, it seems that many stringtheorists are having "closing-down-sale"s !

12. **Thomas Larsson**
October 2, 2005

One claim in physics/0102051, namely that string theory has been spectacularly successful on public relations, does not seem to be true anymore. The anthropic landscape, the swampland and Lubos Motl can hardly be viewed as very successful inventions from a PR point of view.

Words matter. When Vafa and Motl use words like swampland, s**tland and f**kland, they are, consciously or not, sending a message both to budding young theorists and to future grant committees.

13. **woit**
October 2, 2005

Hi Thomas,

Yes, I think over the last year or two string theory is finally starting to have problems on the public relations front.

There are two things that strike me as bizarre about the Vafa paper. One is the whole idea and that he is encouraging others to work on it. But the second is the name he chose. If one wanted to promote a piece of one's own work and get others to take it seriously, why choose the "swampland" terminology? This whole subject just gets more and more weird all the time.

14. **Haelfix**
October 2, 2005

Peter, I don't see anything wrong perse with what Cumrun is doing. He's looking for defacto features of the landscape that are theoretically universal and hence presumably falsifiable at some point down the road. He is no advocate for anthropic principles etc

This seems to me to be the *right* direction to take, rather than studying statistics on what generic feature is more *likely*.

I am also of the mind that you can't just get anything in the world out of the landscape, in fact im not even convinced the 'real world' is sitting in there either, at least with the current toy models. No one has shown that, or picked out a minimal standard model. All generic models tend to have exotics or as yet to be seen experimental phenomenological points, about the only thing people can do is push those to arbitrarily high energies in a somewhat adhoc manner.

15. **woit**
October 2, 2005

Haelfix,

I suppose one can argue that the swampland stuff is better than the anthropic nonsense, but that's not saying much. I'm still not seeing any plausible hope of making string theory falsiiable by the kind of thing Vafa is doing. The kind of constraints he's looking at are so far from the standard model that I don't see any relevance to the real world. If he has an idea about how to show that string theory can't ever reproduce the standard model, I'd of course think that was well worth pursuing.

The other thing that strikes me about all this is that Vafa and people who think this is a great idea like Distler are specialists in the more formal aspects of string theory. They don't have much experience with the wide range of constructions that "string phenomenologists" have come up with. My suspicion is that as they try and come up with conjectured "universal" consequences of string theory, they'll just find that these are all violated by some sufficiently clever and complex string theory construction. This has already happened to Distler. People seem to have an endless optimism that the current string theory framework has some rigidity to it, despite ever increasing evidence to the contrary.

16. **J.F. Moore**
October 2, 2005

It is a standard strategy to use a perjorative on oneself, to take ownership of that term and devalue it for the opposition.

It does seem strange that they would not be concerned about appearances of being closed to thoughtful criticism, at the least.

17. **Tony Smith**
October 2, 2005

Haelfix said "… you can't just get anything in the world out of the landscape, in fact im not even convinced the 'real world' is sitting in there either, at least with the current toy models. No one has shown that, or picked out a minimal standard model. …".

There does exist a string theory model that has the minimal standard model.
It can be found on the CERN preprint server at CERN-CDS number EXT-2004-031.

Peter says "… Vafa and people … like Distler are specialists in the more formal aspects of string theory. They don't have much experience with the wide range of constructions that "string phenomenologists?? have come up with. My suspicion is that as they try and come up with conjectured "universal?? consequences of string theory, they'll just find that these are all violated by some sufficiently clever and complex string theory construction. …".

The model at CERN-CDS number EXT-2004-031 is probably complicated enough that it fits Peter's description of something beyond what sting formalists such as Vafa are considering.
That model was constructed based on usenet discussions in 2004 on sci.physics.research and sci.physics.strings, beginning with an spr thread "photons from strings?" started by John Baez. Participants in the subsequent discussions included Lubos Motl, Urs Schreiber, Aaron Bergman, and others.
To build the model, I started with a suggestion by Urs Schreiber to consider a 25-brane in 26-dim bosonic string theory as giving the U(1) photon gauge group, and I proceeded by modifying it by several steps until it gave the standard model.

As to evaluation of that model by string formalists, Lubos Motl attacked it by saying "It is not true" that String theory is fundamentally 26-dimensional because, according to Lubos: "String theory" is a shorthand for "superstring theory" which is at most 10-dimensional – and its extension "M-theory" is 11-dimensional. …".

On the other hand, another contributor to the discussion said "A Matrix theory such as Tony Smith's would then be a nice formulation of (bosonic) M-theory (as Susskind refers to the 27-dimensional theory), from where we work down dimensionally … to recover fermions. …".

Why do the Vafa-type string formalists fail to consider in detail the relevant string phenomenology work?

What would be so bad about a non-super string model being successful?

———-

Unfortunately, while composing the above message, I discovered some disturbing facts.
Both of the preceding remarks, as well as the bulk of the discussion about the model that is now at CERN-CDS number EXT-2004-031, occurred in May 2004 on a sci.physics.strings thread entitled "Re: Speculation: E6 and 26-dim. string theory",
and all but 2 messages in that thread seem to have been removed from sci.physics.strings (there had been at least 6 messages in the thread),
as have
all but 1 message in a related thread "Re: Calculation of Standard Model parameters" (there had been at least 12 messages in the thread).

I am particularly unhappy with the fact that of the 2 messages remaining in the thread "Re: Speculation: E6 and 26-dim. string theory", one (dated 18 July 2004) is from Lubos Motl (an sps moderator) who says in part: "… This is my favorite speculation. I want to start with a theory in 26 dimensions that already has a E8 gauge group … A superstring spacetime would be a sort of codimension-16 brane in the bosonic string theory. …".
It seems to me that Lubos is trying to use the structure that I described with respect to my E6 model for his E8 model, but the messages describing my work have been removed from the relevant sps threads.
Further,
the removals got rid of the sps record of Lubos's rather embarrassing statement that 26-dim bosonic string theory is not a part of string theory.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

18. **D R Lunsford**
October 2, 2005

Tony – there are many mirrors of spr posts I think. Find and demonstrate the revisionist disappearence here.

-drl

19. **Quantum_Ranger**
October 3, 2005

There appears to be an increase in the scientific community, of war-cries..rallying-of-troops, and a general dissagrements spilling out into Academic Castles such as Stanford-v-Rest of the World?

In a recent documentary about Susskind v Hawking:
http://www.bbc.co.uk/sn/tvradio/programmes/horizon/hawking_prog_summary.shtml

the programme shed some light on Susskind's affiliation with a "used car salesman" :Werner Erhard.

A little skeptic has written page here: http://skepdic.com/est.html

It is quite amazing that the straglehold of the String Community is precariously teetering upon a total system breakdown, instigated by the proponants themselves? from the BBC documentary above, Susskind spoke of the first early days of "self-hypnosis" seminars he and other "later-to-be stringtheorists", attended.

To quote from Susskind himself talking about the early days before stringtheory:They would put people in a room, keep them there for 16 hrs, not let them go to the bathroom, harrang them harrass, them and as a consequence of this they would emerge completely different people!

A lot of Academic pressure has built up from Stringtheorists, who are insistant upon arguing with "Counter-Information" Anthropic reasonings?

A ventriloquist can only throw his voice so far, thats why there are strings attached to "dummies", and not to the audience of listners.

At Stanford, there are a lot of "Twangs-n-Bangs" being heard as the strings finally snap, and the dummies fall by the way-side?

20. **Bryan**
October 3, 2005

In the current issue of New Scientist, Susskind is quoted (on the issue of the reality of the Casimir force) as suggesting he wants to outlaw all use of the word 'real' by physicists.

21. **D R Lunsford**
October 3, 2005

There is just no shame any more. Shame is the critical missing ingredient in our corrupt civilization.

Susskind is a blithering moron, who does physics the EST way. Hey! Don't knock if you haven't tried it!

-drl

22. **JE**
October 3, 2005

DRL – is that a whine? Just put it out there.

23. **Who**
October 3, 2005

Peter wrote–
Given the high quality of the comments by Lubos and Jacques, I was surprised to see that if you look at the trackback page for the Vafa paper, you'll note that trackbacks to their postings are there, but not to mine, which evidently has been censored.

On the question of arXiv trackback censorship favoring one side of a controversial issue, please have a look at this:

http://www.arxiv.org/tb-display/hep-th/0507235

Concerning Lee Smolin's "The Case for Background Independence", the only two trackbacks are to Distler and Motl blogs. It seems to me that there was discussion elsewhere, to which Smolin may have contributed.

Perhaps more to the point, look at this:
http://www.arxiv.org/abs/hep-th/0501114
http://www.arxiv.org/tb-display/hep-th/0501114

There was discussion of the Nicolai et al paper "Loop quantum gravity: an outside view" here at N.E.W., to which Smolin contributed a letter replying to Nicolai. A trackback could be valuable and shed additional light. But the only trackback link at the Nicolai abstract is to Distler's own blog.

24. **D R Lunsford**
October 3, 2005

JE, what, me whine? Fulminatio ergo sum!

This retinue of loser-exquisites gets on my last engram – sorry.

-drl

25. **Who**
October 3, 2005

DRL and JE, about whining and fulmination. do you consider my concern over possibly biased screening of trackbacks on the arxiv to be exaggerated or misplaced? Trackbacks are new (at least to me) and I don't know if they matter much.

Nicolai's paper was the first of two topics in Peter's blog #145, in January.
http://www.math.columbia.edu/~woit/wordpress/?p=145
There was a letter from Smolin replying to some points in Nicolai's paper. I don't know if Peter would want to put in a trackback to that. My question is this: if he sent one in would it be erased by the censor?

The only trackback now is to an entry in Distler's blog which doesnt seem to say much of anything—it is some 5 lines long and essentially just says "LQG is no good and I approve of this criticism of it."

By contrast, Smolin's letter replies to specific points in the paper and goes into considerable detail.

26. **Wolfgang**
October 3, 2005

Dear Dr. Who,

it seems to me that Lee Smolin and friends need to get their own blogs and into this trackback business.

27. **D R Lunsford**
October 3, 2005

Who, I don't think either Peter or Tony would chase chimeras. And remember, just because I'm paranoid doesn't mean I'm wrong!

-drl

28. **Who**
October 3, 2005

Dear Wolfgang,
you write:
it seems to me that Lee Smolin and friends need to get their own blogs and into this trackback business.

How would that address or remedy, or address the issue of, biased censorship of the arxiv.org trackbacks?

Or would it?

29. **Wolfgang**
October 3, 2005

> How would that address or remedy, or address the issue of, biased censorship of the arxiv.org trackbacks?

If there really is censorship it would become obvious. You need more than 1 or 2 incidents, which could be just mistakes.

30. **Who**
October 3, 2005

>If there really is censorship it would become obvious. You need more than 1 or 2 incidents, which could be just mistakes.

I see what you mean. thanks for clarifying Wolfgang.

DRL you say:
>I don't think either Peter or Tony would chase chimeras. And remember, just because I'm paranoid doesn't mean I'm wrong!

I dont think they would chase chimeras either. If Peter chases something it is probably a rat and not a chimera. thanks for your reply, and I will try always to remember that.

31. **Volker Braun**
October 4, 2005

I do not agree with what you make out of my comment. When I am not sugarcoating my questions then this does not mean any disrespect. I know Jaques Distler since i took his string theory class many years ago, and I know that you can ask him scientific questions and expect an honest answer.

Now we made serious progress this year to find compactifications with three generations, and no anti-generations. It is still true that nobody succeeded there in the previous decades. Moreover, we never published that we might also be able to find one and two generation models, so it is hardly fair to blame Jaques for not knowing about them.

If anything, this shows that there is progress string theory every year. Even though you probably will not agree 🙂

32. **woit**
October 4, 2005

Hi Volker,

I'm certainly no expert on this kind of question and didn't realize you were referring to unpublished work.

Still, I'm in no particular mood to apologize for pointing out the fact that Jacques's posting on this subject was misinformed, for several reasons:

1. While he may respond to certain scientific questions honestly, his behavior towards me in the past has repeatedly been disgraceful and dishonest. He has on several occasions taken things I have written, changing them around and then using these fraudulent constructions to attack me as professionally incompetent. When presented with evidence that this is what he has done, he has not once apologized for his behavior. While he may be a technically competent physicist, he's a complete ideologue when it comes to string theory, to the extent of being willing to repeatedly engage in nasty and dishonest behavior.

2. I'm not sure why trackbacks to my postings related to string theory are not appearing at the arXiv. The system is completely untransparent and the person I tried to contact there hasn't responded to my e-mail. As far as I can tell though, it seems likely that Jacques is the one responsible for moderating these trackbacks. If so, it is rather rich that he is censoring links to my accurate but critical postings, while allowing them to his own inaccurate posting.

3. I think this story gives strong evidence for part of my own reaction to the Vafa paper, which was that claims that "one can't get low energy effective field theories with property X out of string theory" are likely to just indicate that either the person saying this isn't very well informed, or no one has tried all that hard to do this.

That every year there is new progress towards showing that string theory is inherently vacuous and one can get anything one wants out of it is something on which I guess we can agree…

Best wishes,

Peter

33. **alex**
    October 7, 2005

    Your trackback seems to have appeared.

Pauli and Not Even Wrong

October 1, 2005
Categories: Uncategorized

When I first started thinking about using "Not Even Wrong" as the title of a book, I did some research to try and find out where the supposed Pauli quote came from. No one seemed to have any information about this, other than the attribution to Pauli, and various different stories existed about the context in which he had used the phrase. I started to worry that these stories, like many of the best ones about Pauli, might be apocryphal, so I contacted a few physicists who had some connection to Pauli to ask them about this. Prof. Karl von Meyenn, the editor of Pauli's correspondence, wrote back to tell me that the phrase doesn't occur in his correspondence. He pointed me to a biographical notice about Pauli written soon after his death by Rudolf Peierls as the best source for the story of Pauli using the phrase.

Peierls writes

No account of Pauli and his attitude to people would be complete without mention of his critical remarks, for which he was known and sometimes feared throughout the world of physics…

No doubt many of the stories of this kind circulated about him are apocryphal, but the examples below come from reliable sources or from conversations at which the writer was present…

Quite recently, a friend showed him the paper of a young physicist which he suspected was not of great value but on which he wanted Pauli's views. Pauli remarked sadly 'It is not even wrong.'

The Peierls article is in

Biographical Memoirs of Fellows of the Royal Society, Vol. 5 (Feb. 1960), 174-192.

It is on-line via JSTOR.

Just recently, Oliver Burkeman wrote a short piece for The Guardian about the Pauli phrase and its recent uses. I talked to him on the phone about this and his article contains some accurate quotes from me, together with a link to this weblog.

Comments

1. **Luboš Motl**
   October 1, 2005

   That's interesting. Exactly a few hours before you, I was writing roughly 30 new requested Wiki articles about physics, and one of them was

   http://en.wikipedia.org/wiki/Not_Even_Wrong
   http://en.wikipedia.org/wiki/Special:Contributions/Lumidek

   where I had to solve the questions about Pauli's quote, too.

2. **The Anti-Lubos**
   October 2, 2005

   Exactly a few?

   Spoken like a true string theorist!

3. **John Gonsowski**
   October 2, 2005

   Apparently Pauli is still into Jung's synchronicity.

Nobel Prize Announced

October 4, 2005
Categories: Uncategorized

Well, it looks like I was right to not try and guess this year's Nobel Prize, since it has been awarded for work in an area of physics I know nothing about. None of the commenters here managed to guess correctly either. The prize goes to Glauber, Hall and Hänsch for work in the field of optics.

Roy Glauber is 80 years old now, and taught the first quantum field theory course I ever took. At the time I was an undergraduate at Harvard and the course was way over my head. All I remember from it now is that it involved a lot of writing down and manipulating long formulas involving mode expansions and annihilation and creation operators. I did end up with some facility in doing this, but didn't much understand what it all meant. Buried somewhere in my office should be notes for that course, perhaps I'll try and dig them up and take a look at them, since I suspect I can probably now appreciate much better what Glauber was trying to teach than I could way back then.

Congratulations to Glauber, Hall and Hänsch!

Update: Hongbao Zhang didn't guess the prize winner's names, but he did correctly guess that it would go to physicists in the field of quantum optics. Congratulations!

Comments

1. **MathPhys**
   October 4, 2005

   I thought I knew about the physics Nobel prize before the news is posted on "Not Even Wrong", but I was wrong!

   Thank you, Peter, for a very informative, and up-to-the-minute blog. I regularly find information here that I would otherwise almost surely miss.

2. **D R Lunsford**
   October 4, 2005

   I remember reading some interesting papers by him regarding some issue in the Dirac theory, long ago..

   -drl

3. **hongbaozhang**
   October 4, 2005

   I guess right!:)

4. **Zelah**
   October 4, 2005

   Hi Hongbaozhang,

   Please provide evidence for your assertion!

   An amateur mathematician

5. **Aswin**
   October 4, 2005

   Was Mandel unfortunate to miss out ??

6. **Zelah**
   October 4, 2005

   Hi everyone,

   Although I am disappointed that Astrophysics and in paticular Dark Matter did not win, I have been educated about a little known field called Quantum Optics.

   Personally, I am now trying to see how QOptics have effected the other fields of physics!

   Congratulations

   Glauber Hall and Hansch.

7. **Eric Baum**
   October 4, 2005

   Hi Peter,
   I took Glauber's course, with similar comments to you. (Maybe the year before? Or were we in the same course?) But aside from the annihilation and creation op calcs that went on for months which I was struggling to follow, what I remember clearest was Ginsparg's reaction. He was a year or two older, and may well have completely understood the stuff (he definitely aced the tests) and rather pointedly read the newspaper in class every day by way of hinting to the old prof that he should move on to something more current ;^)

8. **woit**
   October 4, 2005

   Hi Eric,

   Probably the same course. You were a year ahead of me, but I was taking this course at a point I was ridiculously unprepared for it. That's what happens when you have an undergraduate advisor who will sign anything you put in front of him.

I also remember Ginsparg and his newspaper. But I was never sure whether or not he did that in all his classes.

9. **Eric Baum**
October 4, 2005

I was helaciously unprepared for it too. I think I must have been a soph, because I know Ginsparg was a year ahead of me, and we took Glashow's group reps together the next year (which I also was not sufficiently prepared for and struggled in.) You couldn't have taken Glauber as a frosh, could you?

10. **woit**
October 4, 2005

No, I was a sophomore, I think you must have been a junior. It was the academic year 1976-77.

I just found my old notes together with problem sets, exams, etc. The things Glauber did in the course that weren't part of a normal course these days included some many-body physics, as well as more about the physics of electromagnetic fields, coherent states, resonances, etc. No path integrals, all done by canonical quantization of oscillators.

Looking at my problems sets and exams, I'm surprised to see that I didn't do that badly, considering how little I knew at the time.

11. **D R Lunsford**
October 4, 2005

Peter – funny 🙂

I had an odd thought just now – I think equations have literally "lost their value". That is, when people write the kind of things you see on the archive, things that are neither coherent nor interesting, and get away with it, and get promoted because if it, there has to be a deep reason. I think it started with the "coordinate-free" mania of some years ago – equations in a sense "lost their value" and one had (as in chess) both to keep in mind the trail leading from the symbol to the world, as well as the possible uses of that symbol, which, having been disconnected from numbers, is at a loss to express itself clearly. This is really why physics is (supposed to be) different from math, that is, the equations are supposed to imply a value of some sort – really "an evaluation", while the equations in most of these papers are really just short-hand expressions for a hand-waving argument of arbitrary dubiousness – one doesn't write equations and explain them, one explains one's viewpoint and then draws equations to embody it. (This also explains why these people are so loud – they're working!) The equations have no more connection, the one to the other, than does the torrent of verbiage that attempts to express the unreal – in real physics, one tends to be quiet and push on from point to point (see Einstein and Dirac).

That is why I think string theory is dead, and Nobel Prizes keep going to those people with evaluations in their papers.

12. **MathPhys**
October 4, 2005

Ginsparg was a postdoc at the time. He was many years older than any of you. He was a PhD student of Wilson at Cornell.

13. **woit**
October 4, 2005

Just checked Ginsparg's web-site. He got his undergraduate degree from Harvard in 1977, so Eric is right and he was one year ahead of him (Eric graduated in 78, I was in 79). Ginsparg then went off to graduate school at Cornell, came back to Harvard as Junior Fellow, later faculty member 1981-90.

14. **ksh95**
October 4, 2005

Peter Woit Said:
Looking at my problems sets and exams, I'm surprised to see that I didn't do that badly, considering how little I knew at the time

That always scared the hell out of me. Classes I was unprepared for, where I knew I didn't understand the material on a deep level…but somehow managed to get an A.

I guess that says something about a physicist's (at least this one's) abilty to mass produce problem solutions, regardless of deep understanding.

15. **Luboš Motl**
October 4, 2005

"..Nobel Prize, since it has been awarded for work in an area of physics I know nothing about…"

Wow, cool – it seemed finally like a Nobel prize for string theory. 🙂

16. **woit**
October 4, 2005

Hi Lubos,

Any predictions about something you might really know about, the IgNobels to be awarded Thursday evening at Sanders theater?

I'm predicting that string theory will be awarded an IgNobel before it gets a Nobel.

17. **JC**
October 4, 2005

Peter,

Who was your undergraduate advisor, who was willing to "sign anything put in front of him"?

18. **woit**
October 4, 2005

Hi JC,

My undergraduate advisor was Glashow, and he was the one who happily told me this was his policy.

19. **JC**
October 4, 2005

Peter,

I remember when I was an undergrad, my advisor was a bit of a "hard ass" where he wouldn't let anybody take courses for credit for which they did not have the prerequisites. Though I ended up sitting in on some graduate quantum field theory course (ie. not taking it for credit). I studied some basic field theory stuff on my own previously, so I wasn't completely unprepared. Though in the end I got lost after awhile. (It was a course which covered canonical quantization of scalar, fermion, and Maxwell fields, along with basic tree level computations. The next term covered Yang-Mills and renormalization stuff, but I didn't even bother sitting in on it after my experience of getting lost half way through the first term). Then again at the time, the courses I was actually taking for credit took priority over the field theory course. I suppose if I didn't go to as many parties in those days, I could have worked harder at understanding the field theory stuff. At the time it was easy to let things "slide", when one wasn't taking it officially for credits.

Besides quantum field theory, what other "advanced" courses did you take in undergrad?

20. **woit**
October 5, 2005

Actually I arrived at college with "sophomore standing" since I had passed a lot of advanced placement tests. I ended up staying four years and getting both a B.A. and an M.A. Beside's Glauber's field theory class, some of the other graduate classes I remember were a course on gauge theory from Weinberg, on the standard model from Alvaro de Rujula, and on particle physics from Carlo Rubbia. I also sat in on Coleman's field theory course one year and courses on groups and representations from Howard Georgi and on constructive field theory from Arthur Jaffe. All of these were great experiences and I learned an incredible amount. The standard model was very new in those days and there was a lot of excitement around Harvard as new confirmations of the model kept coming in.

21. **Chris Oakley**
October 5, 2005

Peter,

Your advisors/lecturers seem to be a "who's who" of particle physics. It is a pity you missed out on being taught by Feynman and Gell Mann. Or did you?

22. **Nigel**
October 5, 2005

He certainly has Feynman's views about ST. 😉

23. **Nigel**
October 5, 2005

My information for Feynman being critical of strings is the book Davies & Brown, 'Superstrings' 1988. I haven't read it since 1988 so cannot quote it, but seeing that Feynman died soon after, I thought it accurately reflected his view that ST is not even wrong.

However, searching the internet for Feynman and ST brings up claims that Gell-Man indoctrinated Feynman with it. However, I distinctly remember the interview of Feynman in 'Superstrings' where he point out it is useless.

24. **Tony Smith**
October 5, 2005

NIgel said: "… My information for Feynman being critical of strings is the book Davies & Brown, 'Superstrings' 1988. I haven't read it since 1988 so cannot quote it …".

Here are some excerpts from Feynman's statements in that book, at pages 194-195:
"… I do feel strongly that this is nonsense! … I think all this superstring stuff is crazy and is in the wrong direction. … I don't like it that they're not calculating anything. … why are the masses of the various particles such as quarks what they are? All these numbers … have no explanations in these string theories – absolutely none! … ".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

25. **Nigel**
October 5, 2005

Thank you very much, Tony! I thought so…

26. **Hongbao Zhang**
October 5, 2005

Thanks Woit for your congratulations.:)
Here are my naive reasons:
On one hand, as is well know, Einstein published his famous papers on photon and special relativity in 1905. When we treat the quantum optics, we must put it in the framework of special relativity, unlike the eletron, we have a FW approximation, which is essentially taken seriously in condensed matter physics. I think this year's Nobel Prize should give quantum optics in memory of our great Einstein. Cerntainly, it is the best way!(Einstein's own Nobel prize seemed to be late for his contribibutions to the fundamental phsics)
On the other hand, quantum optics deserves this Nobel prize in its own right. quantum optics not only plays a special role in demonstrating the striking properties of quantum mechanics, but also initiatiates many new technologies and possess wind applications.

27. **goeppert**
October 5, 2005

I think string theory was still in its earliest state of inception, which makes feynman's conclusion about ST rather premature . It should be taken with a grain of salt.

28. **woit**
October 5, 2005

In the mid-80s when Feynman was criticizing string theory it still seemed possible that the theory had only a small number of vacuum states, one of which would give the standard model. Even so, Feynman was skeptical that this would work, and I think the idea that the Landscape and other evidence of string theory's failure over the last twenty years would change his mind and make him favor the theory is kind of laughable.

29. **Tony Smith**
October 5, 2005

goeppert said "I think string theory was still in its earliest state of inception, which makes feynman's conclusion about ST rather premature . It should be taken with a grain of salt. ".

The date of Feynman's interview statement in the Brown and Davies book:
"… I do feel strongly that this is nonsense! … I think all this superstring stuff is crazy and is in the wrong direction. …"
was probably 1987, which seems to me to be long enough after Schwarz's talk at the 1984 APS DPF Santa Fe meeting for Feynman (who was at CalTech with Schwarz during that time) to have formed a NON-premature conclusion about superstring theory.

Here are excerpts from the Brown and Davies book that support a 1987 date for the Feynman interview: "… In 1987 we decided to review the state of superstring research by making a documentary on the subject of BBC Radio 3. … The programme … was broadcast in early 1988 … we felt that it would be worthwhile publishing the interviews in a fuller and more permanent form. …".

As to whether or not Feynman changed his mind at a later date, bear in mind that, according to the Brown and Davies book, Feynman "…died in early 1988.".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

30. **scott**
October 5, 2005

Just to corroborate Feynman's distaste for string theory. In the book Feynman's rainbow, by some dude who postdoced at tech, it was mentioned that string theory upset him so much that his doctor encouraged him not to discuss it. According to the to rumor, according to this book, schwartz was kept on(before the string fad caught on) as faculty because of Gell-Mann's influence and that he did this primarily to agravate Feynman. Also it seems his criticism was mostly about string theory's disconnect from experiment something which hasn't changed at all.

31. **Renormalized**
October 5, 2005

" I do feel strongly that this is nonsense! … I think all this superstring stuff is crazy and is in the wrong direction. … I don't like it that they're not calculating anything. … why are the masses of the various particles such as quarks what they are? All these numbers … have no explanations in these string theories – absolutely none!"

What a bomb up Motl's butt! Thats the price you pay for following the crowd. String physics should be reclassified as Speculative Physical Interpretation Translation. SPIT

32. **JC**
October 5, 2005

Besides Feynman, who else was anti-string in those days? I vaguely remember Glashow being somewhat vocal against string theory in the 1980's. Don't know about today if he's still anti-string.

33. **woit**
October 5, 2005

Glashow is as opposed to string theory as ever. He and Feynman are the only very well-known theorists I can think of who were very vocally anti-string theory in the mid-eighties. Many others were privately skeptical, but not about to complain publicly.

34. **D R Lunsford**
October 5, 2005

Finkelstein was of the opinion that it was "horseshit" in the mid 80s. (He was right.)

-drl

35. **Tony Smith**
October 5, 2005

Peter, you knew Weinberg through "… a course on gauge theory from Weinberg …" as a Harvard undergraduate in the 1970s, and you got your Ph.D. under Curtis Callan in 1984.

By any chance, did you attend the 1984 APS DPF Santa Fe meeting (October-November 1984) ? Curtis Callan gave a talk on "Anomalies and Fermion Zero Modes on Axion Strings", but my question is whether you have any recollection about how that meeting affected the course of superstring theory.

IIRC, at the meeting there was a lot of uncertainty about whether or not superstring theory should be accepted or not. Schwarz (with his colleague Hamidi) was to give a superstring talk, and Weinberg was at the meeting. Weinberg's influence then was HUGE, and almost everybody was wondering (1) would Weinberg attend Schwarz's talk and (2) if he did, would he like the superstring theory or would he just read newspapers*.
Schwarz's talk was in a large room, with front and back doors. Just before the talk, Weinberg was not seated, but just as the talk began Weinberg appeared, standing in the back doorway. At the end of the talk, Weinberg announced that superstring theory would be the approach that he would follow and (almost) everybody there immediately followed suit.

Around 1987, Weinberg also gave an interview that was published in the Brown and Davies book, in which Weinberg confirmed his enthusiasm for superstrings, saying "… string theory … is very beautiful, very promising and it's had qualitative successes so far in making a lot fof things come out right when it wasn't clear how they could ever come out right – things having to do with gravity. …".

Why do you think that Weinberg's enthusiasm had more influence over the late-1980s physics community than the skepticism of Feynman and Glashow?

IIRC, in the 1980s there was some rivalry between Weinberg and Glashow with respect to how much money they might be offered to move from Harvard to Texas (Weinberg) or Texas A&M (Glashow).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

* – IIRC, Weinberg was famous for reading newspapers during lectures by others. Could Ginsparg have picked up that habit from Weinberg?

36. **Peter**
October 5, 2005

Hi Tony,

Don't know where Ginsparg got his reading habits, but, with a few exceptions, the Harvard physics faculty consisted of people not exactly known for being considerate towards others.

There was a rumor that Glashow might go to Texas A and M, getting paid as much as the football coach ($500,000), and more than Weinberg. Supposedly Glashow's friends had a t-shirt made up, saying on one side $500,000, on the other "and worth every penny of it". Glashow and Weinberg were famous for not getting along, with Glashow referring to the Higgs mechanism in the Weinberg-Salam model as "Weinberg's toilet".

I didn't go to the 1984 Santa Fe meeting. That fall I had left Princeton and started a postdoc at Stony Brook. At that time, the person everyone was looking to for leadership was Witten not Weinberg. You just can't overestimate how influential Witten was at the time. He had for several years been producing a stream of amazing work, and the appearance of each of his papers was a huge event. By then he was working full time on string theory and telling everyone else it was the way to go. I'm sure Weinberg was heavily influenced by Witten in his decision to work on string theory. The fact that people like Weinberg fell in line behind Witten certainly added to the momentum behind string theory, but Witten was the driving force. By then I don't think too many people cared what Feynman thought, and Glashow was also no longer that influential. I think most people just saw Feynman as too old, and Glashow as getting old and not capable of learning the sophisticated mathematics needed to do string theory.

37. **Arun**
October 6, 2005

Q – is there a Nobel due for neutrino physics?

38. **MathPhys**
October 6, 2005

If I remember correctly, Green and Schwartz' SO(32) anomaly cancellation superstring paper appeared in August 1984. They wrote it during the summer meeting at Aspen.

Witten's first paper on superstrings, which starts with "In a stunning development", appeared about 2 weeks later, if not less. So that must have been September, at the latest.

The 3rd paper on the subject was by Green, Schwartz and West, and that was a couple of weeks after Witten's. It was really only after Witten's paper that everyone dropped whatever they were doing and jumped on the superstring bandwagon.

It should be easy to check these dates. All 3 papers appeared in Phys Lett B.

39. **John Baez**
October 6, 2005

Chris Oakley wrote:

> Peter,
>
> Your advisors/lecturers seem to be a "who's who?? of particle physics. It is a pity you missed out on being taught by Feynman and Gell Mann. Or did you?

These guys taught at Caltech, not at Harvard like the guys Peter mentioned.

Tony Smith writes:

> Why do you think that Weinberg's enthusiasm had more influence over the late-1980s physics community than the skepticism of Feynman and Glashow?

My guess is that particle physics had to go *somewhere*, and the people saying where it *shouldn't* go were unable to say where it *should* go.

Despite all the flaws of string theory, it's clearly been an incredibly rich research program, with lots of interesting things for lots of people to do. Feynman and Glashow presented no comparably rich research program.

Most of the really interesting results of string theory have been mathematical in nature: it may never win a Nobel prize, but there have already been at least 3 Fields medals based on work related to string theory (Witten, Borcherds and Kontsevich). So, it's possible that in the long run string theory will be seen as a research program that lured a bunch of very smart physicists into mathematics – not necessarily a bad thing, given the somewhat stagnant state of experimental particle physics.

40. **Chris Oakley**
October 6, 2005

John,

The bad thing is not that String theory lured physicists into mathematics; it is that these people pretended and continue to pretend that what they do is not only physics, but the most important physics anyone was ever doing. This is just dishonest.
It is also damaging, as it blocks those who have their own ideas about QFT but refuse to sign up for quasi-religious mathematical cults.

41. **Tony Smith**
October 6, 2005

John Baez said "… particle physics had to go somewhere, and the people … were unable to say where it should go. … string theory will be seen as … not necessarily a bad thing, given the somewhat stagnant state of experimental particle physics. …".

Chris Oakley said "… String theory … people … pretended and continue to pretend that what they do is not only physics, but the most important physics anyone was ever doing. This is … damaging, as it blocks those who have their own ideas about QFT but refuse to sign up for quasi-religious mathematical cults. …".

In fact, there were (and are) some possibly fruitful lines of work in physics, both experimental and theoretical, that were (and are) suppressed by the string-dominated physics community during the past 20 years. In other words, some of what John Baez considers to be stagnation, I consider to be caused by repression.

I will list a few examples, which I think support the position of Chris Oakley. Although some of them are related to my work, they are just as interesting if you totally disregard my work, so this list is NOT intended as a complaint about consideration (or lack thereof) of my work.

1 – Supergravity was abandoned, on the grounds that it was thought not to be finite at high orders. Ironically, the superstring people have quite recently sought to revive supergravity. If the superstringers had not suppressed supergravity work 20 years ago, maybe we would already know the answers to the supergravity finiteness questions now being raised by superstringers. (See Peter Woit's recent blog entry "Is N=8 Supergravity Finite?".)

2 – With respect to experiment, consider the analysis of experimental data about the T-quark. The book "The Evidence for the Top Quark", by Kent W. Staley (Cambridge 2004) says (here XXX represents the name of someone who afaik has not participated in Peter Woit's blog discussions – you can see who XXX is by reading the book): "… XXX … objected that "[w]ith the Godparents for the top analysis becoming a part of the closed analysis group the principle of independent internal review has been abandoned … XXX … thought … a top signal … was marginal and being railroaded through … XXX also complained that discrepancies appeared in some distributions … XXX … described "a few hints that the simplest hypothesis that the top candidate events are just the t tbar events and SM background may not be entirely correct" …". Perhaps non-consensus analysis of T-quark data might shed light not only of the nature of the T-quark, but also on whether or not the Higgs might be a T – Tbar composite, or closely related to such a composite.

3 – Work attempting to explore the connections between the math/geometric structures of bounded symmetric domains and particle theory has been suppressed, for example by a December 1989 Physics Today Reference Frame article by David Gross that ridiculed the work of Armand Wyler. I am not saying that Wyler found a good physics model, but I am saying that he found coincidences between the fine structure constant and math/geometric structures that should have been seen as good reason to do further work exploring the possible utility of such an approach, and that such exploration should be worthy of funding and support.

4 – SU(5) GUT models were and are totally abandoned, based on statements that proton decay neutrino observations showed a too-long proton lifetime, even though reasonable alternative definitions of background might give a proton lifetime within the range of GUT models. (See for example hep-ex/0008074.)

Of the above 4 examples (NOT an exclusive list, I regret to say), only 1 seems to me to be likely to get significant funding in the near future, and that is only because some superstringers have become hopeful that it might rescue their foundering attempts to connect with experimental results.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

Townes Symposium

October 5, 2005
Categories: Uncategorized

The Townes Symposium will be taking place in Berkeley starting tomorrow, and if you've got \$500 burning a hole in your pocket, you might want to help subsidize the Templeton Foundation in its efforts to bring science and religion together. If you want dinner on Saturday that will be another \$300, although you could buy a whole "Laureate Table" for \$10,000, and presumably get to dine with one or more of the 18 Nobel Laureates that Templeton has convinced to attend.

Among those in attendance will be string theorists Raphael Bousso, who will promote the Landscape pseudo-science, David Gross, who won't be promoting the Landscape pseudo-science (I hope), Michio Kaku, who will speak on science fiction, and Leonard Susskind, who will promote the Landscape pseudo-science and his forthcoming book. One physicist that attendees won't get to hear from is Sean Carroll.

At some point during the symposium the new fq(x): Foundational Questions in Physics and Cosmology project will be unveiled. About all I know about this project so far is that it "is a multi-million dollar, multi-year effort to catalyze research and dialogue at the boundaries of physics and cosmology that are related to really big questions" and is based on the idea that "positivistic, deterministic, or materialistic philosophies no longer have secure places" because of modern physics and chaos theory. It will answer questions like "Why existence? What makes meaning?", and its domain name is registered to Max Tegmark.

Update: The fq(x) website has just appeared. On the whole the project seems more sensible and free of religious nonsense than I had feared. It is being run by Tegmark, assisted by astronomer Anthony Aguirre. The advisory board consists of real physicists (Barrow, Rees, Silverstein, Smolin, Wilczek and Zeh), not religion and science people. It looks like the Templeton Foundation has provided $5 million in seed money, to be spent over 4 years, with the idea that after 4 years the project would have attracted funding from elsewhere. They will announce the first competition for grants on December 1. Grants will be awarded based on "a competitive process of expert peer review similar to that employed by national scientific funding agencies, and will target research unlikely to be otherwise funded by conventional sources." They hope to "Expand the purview of scientific inquiry to include scientific disciplines fundamental to a deep understanding of reality, but which are currently largely unsupported by conventional grant sources." I wonder what kind of research they have in mind to fund that isn't getting funded by the current sources of funding, that will be interesting to see.

Comments

1. **Bryan**
   October 5, 2005

   It is good to combine string theory with religion in my opinion. The advantage is that heretics can be imprisoned if they publically ridicule an accepted religion.

   (A few centuries ago, they'd be burned at the stake for doing what you are doing! Can't you understand how dangerous you are?)

   One thing a free democracy prevents is ridiculing religion, because a purely belief system can't be defended rationally. People have a right to believe in witchcraft or 10 dimensional creation without a shred of evidence.

   In the Soviet Union, criticism resulted in being sent to a mental hospital. Too bad it collapsed. I say, string critics should be locked away so that everyone else can be brainwashed with claptrap.

   What is at stake is the reality of the world and the future of physics. How can you be so cold and rational, so heartless in dismissing the work of great mainstream men and women, who did their best and continue to do so, despite difficulty.

   Please buck up and start supporting your fellow scientists!

2. **ali**
   October 5, 2005

   It's good to know that in the event that supersymmetry isn't found at CERN and the NSF et al. decide to defund string theory, fq(x) will make sure that string theorists don't starve (or end up on Wall Street).

3. **Nigel**
   October 5, 2005

   "… Michio Kaku, who will speak on science fiction…"

   I'm confused. Do you mean he will be talking UFOs or string theory? Please be more lucid, Peter.

4. **Not a Nobel Laureate**
   October 5, 2005

   "I'm confused. Do you mean he will be talking UFOs or string theory?"

   As both have the same predictive ability to predict physical phenomena, what does it matter he if talks about strings, UFOs or both.

5. **Chris W.**
   October 5, 2005

   A little zinger from **A. Zee**:

   > We could suppose either that the entries in [the neutrino mixing matrix] V represent a bunch of meaningless numbers possibly varying from village to village in the multiverse landscape as advocated by some theorists of great sophistication or that they point to some deeper structure or symmetry as some theorists with a more traditional faith in the power of theoretical physics might dare to hope for. It is natural to imagine that there is a family symmetry [2] linking the three lepton families.

   (See hep-ph/0508278.)

6. **Fabien Besnard**
   October 6, 2005

I'm really worried about famous scientists being involved in an entreprise funded by Templeton fundation. Don't they realize that their mere presence will almost certainly end up being used to give credit to stupidity and obscurantism ?
Something else worries me. The subjects fq(x) deal with have a priori nothing to do with religion. The funds by Templeton will contribute to the wrong but often quoted idea that highly conjectural but science-based ideas and religion converge. I'll take the example of the big bang, which is not conjectural anymore but was so in the past. The first opponents of big bang models (Hoyle for instance) did not like the religious flavour of it. Recently the Pope (the last one) said that the big bang was compatible with the religion (and in fact gives credit to it). Well the Pope and Hoyle were both wrong : the big bang has really nothing to do with a divine creation. If one wants to believe it has something to do with it, of course one has the right to do it, but there is zero scientific evidence for it. Moreover, a few century ago one could have been burned on the spot by the religious autorities just to say that the world was more than a few thousand years old… I think this example shows well that science and religion should be kept completely separate, and that scientists who wish to promote a religious point of view should do it on a strictly private basis without giving their scientific aura to an entreprise which has really nothing to do with science.
I should add that I'm really disappointed to see Smolin involved in this.

7. **Steve**
October 6, 2005

You don't have to try to separate religion from science. They are already separate as physics is separate from literature.

Now, I don't think Templeton Foundation is trying to use religious method in seeking scientific facts. I think the Templeton Foundation is doing the converse; They are trying to seek religious truth by scientific methods; And this is more a threat to religious people than to the scientific community because it is as stupid — I didn't say it's wrong, but it's not smart — as trying to prove that my mom loves me by quantifiable numbers and experiment. There are things in this world when seeking truth or facts where scientific method is not the best way; for example, mathematics.

But of course, when seeking the physical law of nature, there is nothing like the scientific method. So if you ask me why I believe in God, as a scientific person I cannot tell you anything; But as a human being, I might be able to convince you.

8. **Fabien Besnard**
October 6, 2005

Steve, of course you're right, but you assume that the Templeton people are intellectually honest and will tell it if they fail in their attempt. I can predict the exact opposite. I can predict it on the ground that it has always been this way : people who already have faith (be it in religion or in a political idea for instance) will never give up on their faith because of scientific evidence, but on the contrary will try to bend scientific evidence so as to match their views, even if they end up with gross distortion of the facts. A political example is the Lyssenko affair.
You say : "this is more a threat to religious people than to the scientific community because it is as stupid…". On this point I don't agree with you : first because religious people are long immunized against this sort of threat, they are even immunized against logical contradiction. Second, it can be a threat to the scientific community because the scientific people embarking on this sort of mixing between science and religion will end up being manipulated. This has already happened in France with something called "Université interdisciplinaire de Paris" which managed to "trap" some well known scientists.

9. **Who**
October 6, 2005

Fabien, thanks for warning that the category "interdisciplinary" can be a codeword or an euphemism for putting theo-spin on cosmology. You say:
Second, it can be a threat to the scientific community because the scientific people embarking on this sort of mixing between science and religion will end up being manipulated. This has already happened in France with something called "Université interdisciplinaire de Paris?? which managed to "trap?? some well known scientists.

I think I saw in the program of the Townes conference that there was someone talking on the subject of "interdisciplinary studies". Those inside the tent must carefully inspect each new hairy object that appears to discover if it is the Nose of the Camel.

To me, it seems that FQX is different from this already compromised Townes Conference and FQX is not necessarily a tool of Templeton, even though Templeton has given the seed grant. It may be that FQX will "take the money and run"—-that is, it may operate in an intellectually independent manner, which I believe is completely ethical.

Something I find interesting is that Smolin, on the advisory board of FQX, is indirectly associated with a cosmology model in which the classical "big bang" singularity has been removed and time extends back to a prior contraction phase—this is the Loop Quantum Cosmology model which Smolin's work indirectly supports and which he has discussed in his survey paper "Invitation to LQG".

This raises the possibility that the "big bang" has nothing to do with any creation at all—Divine or otherwise. It was simply not a moment of creation but rather a continuation of the cosmological model. So the issue of whether it was "purposive" or accidental does not arise.

This might have disappointed the late pope, if what you say is true:

Recently the Pope (the last one) said that the big bang was compatible with the religion (and in fact gives credit to it). Well the Pope and Hoyle were both wrong : the big bang has really nothing to do with a divine creation…

I remain undecided about the validity of FQX as a scientific research/education project. The presence of people like Smolin on the board gives it potential legitimacy. The fact of $5 million Templeton seed money potentially compromises its integrity. Have to wait and see.

10. **Fabien Besnard**
October 6, 2005

About the UIP (Université interdisciplinaire de Paris), I just learn from a friend of mine that it is funded by… the Templeton fundation ! The people engaged in this UIP (apart from those who have been lured into it) are well known for their irrational beliefs.
Who says : "It may be that FQX will "take the money and run??—-that is, it may operate in an intellectually independent manner, which I believe is completely ethical." Well this is this particular point about which I'm most worried. I don't think such an attitude would be ethical, because basically I don't think it is possible to "take the money and run". If you take the money you owe something, it's as simple as that.
"The presence of people like Smolin on the board gives it potential legitimacy. The fact of $5 million Templeton seed money potentially compromises its integrity." Precisely ! I really wish Smolin would reconsider working on this project.

As a sidenote : any cosmological model, with or without big bang, is unable to give credit/destroy a cosmogonical belief, for the latter is irrefutable (unfalsifiable). If the biblical tales were to be taken as scientific statements they would have been refutated long ago. This is why they are now said to be metaphorical, and for this reason can't have anything to do with any scientific theory whatsoever.

11. **Nigel**
October 6, 2005

Who, on big bang religion, please note Erasmus Darwin (1731-1802), father of Charles the evolutionist, first defended the big bang seriously in his 1790 book 'The Botanic Garden':

'It may be objected that if the stars had been projected from a Chaos by explosions, they must have returned again into it from the known laws of gravitation; this however would not happen, if the whole Chaos, like grains of gunpowder, was exploded at the same time, and dispersed through infinite space at once, or in quick succession, in every possible direction.'

Weirdly, Darwin was trying to apply science to Genesis. The big bang has never been taken seriously by cosmologists, because they have assumed that curved spacetime makes the universe boundless and such like. So a kind of belief system in the vague approach to general relativity has blocked considering it as a 10^55 megatons space explosion. Some popular books even claim falsely that things can't explode in space, and so on.

In reality, because all gravity effects and light come to us at light speed, the recession of galaxies is better seen as a recession speed varying with known time past, than varying with the apparent distance. Individual galaxies may not be accelerating, but what we see and the gravity effects we receive at light speed come from both distance and time past.

So the acceleration of universe = variation in recession speeds / variation in time past = c/t = cH where H is Hubble constant. The implication of this comes when you know the mass of the universe is m, because then you remember Newton's 2nd law, F=ma so you get outward force. The 3rd law then tells you there's equal inward force (Higgs/graviton field). When I do the simple LeSage-Feynman gravity shielding calculations, I get gravity within 1.7%.

It is suppressed like Tony Smith's prediction of the top quark mass by arXiv.org

12. **Who**
October 6, 2005

Nigel that Enlightenment (1790) Big Bang story is fascinating, so he was writing a Gunpowder Genesis right around when Mozart was writing the Magic Flute, great days.

Fabien I like several of your points and will not try to argue against those I disagree with, but rather wait to see if other people want to argue.

What I disagree with is what you say here: *If you take the money you owe something, it's as simple as that.*. I think that someone who accepts funding for a scientific project is in fact ethically obligated to be intellectually independent.
It is the scientist who thinks he "owes" to his donors to put some bias or nice spin on his findings who is the unethical one, in my view.

13. **Fabien Besnard**
October 6, 2005

"It is the scientist who thinks he "owes?? to his donors to put some bias or nice spin on his findings who is the unethical one, in my view. "

He does not necessarily "thinks" he owes something. It may be unconscious. There's a french saying "on ne crache pas dans la soupe" (you must not spit in the soup).

14. **Eric Dennis**
October 6, 2005

There is an American saying. "One must not dip one's quill in the company ink." But that is an entirely different matter.

I think Fabien's admonition is correct and prescient. Of course a scientist must maintain independence. But by accepting a grant from an organization whose explicit purpose is profoundly anti-scientific, one has already compromised that independence.

15. **Nat Whilk**
October 6, 2005

[Steve:] "*it is as stupid . . . as trying to prove that my mom loves me*"

[Fabien Besnard:] "*The people engaged in this UIP (apart from those who have been lured into it) are well known for their irrational beliefs*."

Just so I get the terminology straight, is Steve's belief that his mom loves him irrational?

16. **Alain Riazuelo**
October 7, 2005

[Fabien B.] *Recently the Pope (the last one) said that the big bang was compatible with the religion (and in fact gives credit to it).*

As far as I know the last Pope was not involved in this. After Hoyle invented the word Big Bang in 1948 or 1950, the Big Bang model became known to Pope Pius XI who officially declared on 22 november 1951 that the Big Bang model was in agreement with the biblic *Fiat Lux* (probably after having realized that the model had been initiated by Catholic priest Georges Lemaître). Lemaître then requested an audience with the Pope, who later retracted his first statement on 7 september 1953 at some IAU meeting. I never heard John Paul II having had such ambiguous statements.

17. **Who**
October 7, 2005

Alain what you say about Pope Pius XI is interesting. Do you have any sources that you could give us.

Pope Pius XI who officially declared on 22 november 1951 that the Big Bang model was in agreement with the biblic Fiat Lux (probably after having realized that the model had been initiated by Catholic priest Georges Lemaître). Lemaître then requested an audience with the Pope, who later retracted his first statement on 7 september 1953 at some IAU meeting

If I understand you, Pius XI went in person to an IAU meeting in 1953 and retracted his official statement made in 1951.

Or did he perhaps send an emissary to read a retraction statement at the meeting for him?

It would be fascinating to read the two statements. Are they by chance online at the peternet site? Some historical documents are online there, if I remember correctly.

18. **Fabien Besnard**
October 7, 2005

Alain : in “a brief history of time” Hawking talks about is meeting with JP2 :
“He [the pope] told us that it was all right to study the evolution
of the universe after the big bang, but we should not inquire
into the big bang itself because that was the moment of Creation
and therefore the work of God. I was glad then
that he did not know the subject of the talk
I had just given at the conference – the possibility
that space-time was finite but had no boundary, which means that it had no beginning, no moment of Creation. I had no desire to share the fate of Galileo, with whom I feel a strong sense of identity, partly because of the coincidence of having been born exactly 300 years after his death!”
This implies that JP2 identified the big-bang with the moment of divine creation.

19. **Alain Riazuelo**
October 7, 2005

> Who

I got this from Jean-Pierre Luminet and also read it in several popular science books. Luminet is definitely reliable on this issue, I think. I will investigate and try to find online references. From what Luminet says in his website Pope Pius XII (and not Pius XI) went in person at the IAU meeting in 1953. See (in French, sorry) http://www.obspm.fr/savoirs/contrib/debat.fr.shtml .
More later.

20. **Who**
October 7, 2005

Alain’s link has this:
“…A côté de ces justes critiques, de faux procès sont intentés à la cosmologie. L’un d’entre eux a injustement gâché la renommée scientifique du plus grand cosmologiste de ce siècle: Georges Lemaître, inventeur du concept de big bang avec le russe Alexandre Friedmann. On lui a reproché de vouloir confirmer par la science le récit de la Genèse. Il n’en était rien: abbé, certes, mais brillant scientifique, Lemaître tenait à une distinction radicale entre science et religion, pensant que l’on ne pourra jamais réduire l’Être suprême au rang d’une hypothèse scientifique – comme le disait à Napoléon le mathématicien français Pierre Simon de Laplace. Cependant Lemaître joua de malchance : le 22 novembre 1951, le pape Pie XII déclarait devant l’Académie Pontificale : “Il semble en vérité que la science d’aujourd’hui, remontant d’un trait des millions de siècles, ait réussi à se faire le témoin de ce Fiat Lux initial. Vers cette époque, le cosmos est sorti de la main du Créateur”.

Farouche adversaire d’un tel “concordisme”, Lemaître demanda audience au pape et remit respectueusement les choses en place. Le 7 septembre 1953, devant l’assemblée générale de l’Union Astronomique Internationale, Pie XII tint effectivement un discours radicalement opposé : la cosmologie scientifique ne parlait ni de Fiat lux , ni de création.”

21. **Who**
October 7, 2005

Someone with better French please help. this is from JeanPierre Luminet and is posted at the Paris Observatory website. Maybe you think this exchange between Lemaître and the Pope is unimportant and merely involved a trivial misunderstanding, but I think it might be indicative of a longstanding problem and would like to have an approximate English translation.

False indictments (as well as these just criticisms) have been brought against cosmology. One of these unfairly damaged the scientific reputation of the greatest cosmologist of the century: Georges Lemaître, inventor of the concept of the big bang, with the russian Alexander Friedmann.

I have to go, back later to continue

22. **Who**
October 7, 2005

continuing with the passage from Luminet
‘…He was accused of wanting to confirm the story in Genesis by science. There was nothing in that: although certainly a priest, Lemaitre was a brilliant scientist and held to a radical distinction between science and religion, believing that one could never reduce the supreme Being to the level of a scientific hypothesis—as the French mathematician Laplace put it to Napoleon.

Meanwhile Lemaître had some bad luck: on 22 November 1951, Pope Pius XII declared in front of the Pontifical Academy “It seems true that today’s science, going back over a tract of millions of centuries, has succeeded in witnessing the initial Fiat Lux. Around that epoque the cosmos emerged from the hand of the Creator.”

Lemaître, who was a fierce enemy of this kind of “concordism” [my comment: we might say “Templetonism”], **requested a papal audience** and respectfully set things right. On 7 September 1953, before the general assembly of the International Astronomical Union, Pius XII took the radically opposite line: **scientific cosmology refers neither to Fiat Lux nor to creation.**‘

Nature Physics

October 6, 2005
Categories: Uncategorized

A new physics journal was launched this week, it's an offshoot of Nature called Nature Physics and will cover research in pure and applied physics. In an opening editorial, the editors of the new journal explain what its goals are. Back over at their mother publication, in their own editorial, the editors of Nature welcome the new publication, although they can't help pointing out that "Nowadays, thanks to the allure of biology's progress and benefits, physics is just another discipline."

Comments

1. **andy.s**
   October 6, 2005

   Umm… what was it before?

2. **TJ**
   October 6, 2005

   I loved the editorial at Nature:

   The enduring conjugal relationship between physics and mathematics continues to stimulate both.

3. **dan**
   October 6, 2005

   loop 2005 is almost here. is anyone blogging on this?

4. **Urs**
   October 7, 2005

   > loop 2005 is almost here. is anyone blogging on this?

   I'd bet you'd get to see a TWF or two about it 🙂

5. **dan**
   October 7, 2005

   i think it is next week. hopefully peter woit will offer comments/insights. i wouldn't mind lubos but he isn't very objective. do you blog urs?

6. **Urs**
   October 7, 2005

   > do you blog urs?

   Not from loops05, if that's what you are after.

   The next meeting that I plan to blog-report on is a MathPhys Colloquium here in Hamburg.

   However, my new colleague D. Bahns will be in Potsdam, giving a talk on 'Pohlmeyer invariants' (which are classical invariant observables of the Nambu-Goto and Polyakov string) (even though).

   So with a little luck I might be able to report on some second-hand information. Perhaps.

7. **Ken Muldrew**
   October 7, 2005

   Andy.s:

   A previous Nobellist in stamp collecting once proclaimed that, "All of science is either physics or stamp collecting."

   Physics used to be a big thing.

8. **Who**
   October 7, 2005

   andy.s congratulations on the plainspoken forthrightness of your remarks here
   http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/panel.html

   especially in the interval 1:26–1:29, as well as your panelmember talk starting at 52:00. I say this simply in the interest of giving credit where due.

9. **QWERTY**
   October 7, 2005

   WHY OH WHY OH HAVE YOU FAILED TO REVIEW THE NEW NOTICES OF THE AMS IN A TIMELY FASHION? RSS FEEDS MEAN THAT I NOW PROCRASTINATE WITH SURPASSING EFFICIENCY, AND I RELY ON CONTENT PROVIDERS (YES, THIS MEANS YOU WOIT) TO PROVIDE ME WITH THEIR OH SO DISTRACTING CONTENT POST-HASTE. I FEEL NO SHAME IN MAKING THESE DEMANDS OF VERY BUSY PEOPLE BECAUSE OF MY NEO-MODERNIST UPBRINGING. YOU SHOULD ALSO PROVIDE SPECULATIVE REMARKS ABOUT THE NEW MATHEMATICS THE QUANTUM HALL EFFECT EXPERIMENT WILL GENERATE AND COMMENT ON THE RECENT WORK OF DENCKER REGARDING NIRENBERG-TRENES. HURRY.

10. **andy.s**

October 7, 2005

Wow. This guy thinks I'm a Harvard string theorist.

Of course, on this blog that might not be a compliment.

11. **Who**
October 7, 2005

my mistake. hope you had a look at some of the toronto show.
you could do worse than be a harvard string theorist btw.

12. **Who**
October 7, 2005

dan wrote (October 6th, 2005 at 10:33 pm)
loop 2005 is almost here. is anyone blogging on this?

John Baez said he asked about videotaping and was told that the Loops '05 people plan on recording the talks and putting them online.

My understanding anyway is that we should eventually be able to download some or all of the invited talks (e.g. by Rovelli, Smolin, Baez, Loll, Reuter, Ashtekar and others)

Urs is right that one should expect Baez to report in TWF—most likely when he gets back from the conference, sometime after 15 October.

the slides for Baez talk, scheduled for Tuesday 11th, are already posted at his site, with links to related work.

13. **dan**
October 8, 2005

incidentally,
i am curious as to the purpose of these conferences, since presumably the material could all be retried from physics journals like arix. will there be any new material introduced, not reflected in journals

14. **Who**
October 8, 2005

dan writes:*incidentally,*
i am curious as to the purpose of these conferences, since presumably the material could all be retried from physics journals like arix. will there be any new material introduced, not reflected in journals

I will answer you as best I can. there certainly will be unpublished stuff brought out in the talks, and IDEAS for future papers. To see this, Just look at the abstracts of the talks: for example
Laurent Freidel says explicitly that he is going to extend something to 4D which he has not yet published—-John Baez has a new abstract, not at the conference website, and he will be talking mostly about stuff he has not published. Rovelli will be putting stuff together from half a dozen recent papers by him and others, and projecting from that to future research.
So there will be plenty talked about that is unpublished. but maybe that is not the point.

I think the point is that if you are a Gradstudent or a Postdoc then it is very important for you to know personally some of the other 150 people in the field so you know WITH WHOM TO TEAM UP IN COLLABORATION and also WHERE TO GO FOR YOUR NEXT POSTDOC and if you are a Faculty then you want to size up the horseflesh and see who of the postdocs looks good, then maybe you can bring good ones to your department. Everything is done by teamwork and interaction and people stimulating each others ideas. So it is terribly important to personally know all the promising people. when you know someone, then you know more than what quality papers they HAVE written. when you listen to someone you also get an impression of what quality papers they WILL write, and you get a sense if they would be simpatico to co-author with.
The people at Loops '05 come from several different QG approachs, canonical LQG, spinfoam, CDT, causal sets, also cosmology/phenomenology like Maartens, and numerical relativity, also consistent discretizations approach of Pullin/Gambini. They have to meet in order to trade ideas and postdocs and coallesce into a "nonperturbative quantum gravity" community. Ultimately they have to cross lines and converge results. there must be a lot that will happen besides what you get from reading each other's papers in isolation.

15. **Chris Oakley**
October 8, 2005

I agree with Who (World Health Organisation?) in that the function of conferences is mainly for networking, and what one gains in talking to people between sessions is actually far more valuable than the talks themselves. I don't know whether this is just me, but a general problem with the talks at conferences (or elsewhere) is that they always seem to assume that you too have been working on their particular problem for the last six months. After about the second transparency you therefore tend to not be able to follow the details, and switch off. When I gave talks on my own work I was well aware of this problem, and so tried to keep it interesting for the entire audience for the duration. The success of my methods was demonstrated by the fact that at a talk I gave at Harwell Laboratory in 1987, only one person fell asleep (although, admittedly, he was snoring loudly).

16. **Nigel**
October 8, 2005

'…the function of conferences is mainly for networking, and what one gains in talking to people between sessions is actually far more valuable than the talks themselves…'

Chris, what about the function of publicity which science conferences are sometimes used for (and not just those of political parties which pevent heckling using the Prevention of Terrorism Act).

String theorists have cried wolf so many times that physics has lost popular credibility. Can you imagine the media forever reporting endless speculations in ST without any hope of being tested? Then look at the decline in A-level physics students over the last decade (since M-theory hype in 1995), it's now behind social sciences, as everyone knows it is a dead end discipline.

17. **Chris Oakley**
October 8, 2005

Nigel,

… what about the function of publicity which science conferences are sometimes used for [?]

Having been at CERN relatively recently, you would probably know more about this than I do [the last international physics conference I attended was at ICTP nr. Trieste in 1983. I don't remember journalists being there … my clearest memory, in fact, is Abdus Salam's Mercedes being parked on a dais next to the main building – as director he did not seem to be obliged to use the car park like everyone else].

18. **Nigel**
October 8, 2005

Chris,

It's sad that physics is so dull that journalists don't report the conferences anymore, and while ST dominates, there will be no news from the theory end of physics (unless you want sci fi).

I've not worked for Cern, although their preprint server hosts an article on cosmology/gravity.

On QTF controversy, do you think it might be possible to find an easier way around the maths, at least for the first coupling correction? I'd like a simple explanation of magnetic moment factor 1 + 1/(2 x Pi x 137) = 1.00116 Bohr magnetons. All the renormalisation problems from the abstract maths look bogus to me, surely there is a more simple solution behind it? The 2 Pi is going to be a geometric correction and the 137 also has a physical explanation like the shielding factor of the charge of the electron core by the polarised virtual charge surrounding it? I can't exactly see the solution, but surely it doesn't need a vast amount of abstract theory? I can't see why QED is so applauded for trivia like the 10 decimals of the magnetic moment of an electron, when it does not address the physical mechanism of EM forces. OK, it has survived experimental tests (if you accept renormalisation), but it is hardly complete.

19. **Chris Oakley**
October 8, 2005

Hi Nigel,

I agree that the jewels in the crown of QED (the Lamb Shift and anomalous MM's) are not much more impressive than Ivan Boesky's miraculous prescience in regard to stock prices in 1980's, but at this stage I don't have much to add to what I have already said on my web site. Numerical coincidences are no more than that unless there is a meaningful theory behind it.

20. **dan**
October 8, 2005

thanks who.

was there a loops 2004, and is there a loops 2006 scheduled? personally i wish lubos attended, as he is a very punctual blogger

21. **Who**
October 9, 2005

dan wrote (October 8th, 2005 at 11:08 pm)
thanks who.

was there a loops 2004, and is there a loops 2006 scheduled?

Here is the main webpage for last year's conference:
http://web.lpta.univ-montp2.fr/users/philippe/quantumgravitywebsite/

Last year it was called
Non Perturbative Quantum Gravity:
Loops and Spin Foams

It took place 3-7 May at Marseille. Before that conference it was not clear that there was going to be a yearly Quantum Gravity conference, so it was not called "Loops '04". The international organizing committee was much the same people as for Loops '05. They saw that the 2004 conference was a success, and enough was happening, so they decided to make it annual.

Last year there were 101 registered participants
http://web.lpta.univ-montp2.fr/users/philippe/quantumgravitywebsite/

This year there are 156 participants.
http://loops05.aei.mpg.de/index_files/Participants.html

The name "Loops '05" is very much a shorthand expression. On the main webpage for this year's conference
http://loops05.aei.mpg.de/
they say: "Loops '05…[this year] the annual international meeting on non-perturbative/background independent quantum gravity takes place from …"

In other words they just started the tradition of having an annual meeting of researchers in **non-perturbative/background independent quantum gravity** and they call it by the shorthand "Loops" although there are several different approaches, not just LQG but also spinfoams, CDT, QEG, causalsets.

dan, you ask about next year's conference. I will risk a guess that it has not been decided yet where to have it, but that there will be one, that it will called Loops '06, and that it will be in Utrecht. It could just as well be at Penn State, or at Perimeter Institute. The reason I think Utrecht is that Ashtekar will be there at least one semester in 2006—it tips the balance in that direction. But the location is just a wild guess.

Notes for Witten Lecture

October 8, 2005
Categories: Uncategorized

Witten gave a lecture on the beach at Stony Brook on the topic of gauge theory and the Langlands program two months ago, and lecture notes are now available. Lubos Motl has a posting about this, where he promotes the idea that people should stop referring to the "Langlands Program" and just refer to "Langlands duality". Somehow I suspect that mathematicians will keep doing what they have always done, using "program" to refer to the general, well, program, and "duality" to refer to the more specific, well, duality, that one would like to prove as part of the program.

An earlier posting of mine contains a lot of relevant links, to which should be added the notes from David Ben-Zvi's talk in Seattle this summer.

Comments

1. **Aaron**
   October 8, 2005

   It should probably be pointed out that what seems to show up in the physics is Geoemetric Langlands. The full Langlands program is a lot more than just that.

2. **plato**
   October 8, 2005

   I am trying to synopsize this conversation so I understand this issue.

   I was actually looking for "culminating mathematical visualizations" that could take us directly to the geometric design, and work backwards.

   Any idea here?

3. **QWERTY**
   October 8, 2005

   DID YOU NOT LISTEN TO ME? WHO GIVES A CRAP ABOUT GEOMETRIC LANGLANDS WHEN WE COULD BE TALKING ABOUT THE "IMAGING OF THE BRAIN" ARTICLE IN THE NOVEMBER NOTICES OF THE AMS. WITTEN HAS BETRAYED PHYSICS WITH HIS SUPPORT OF STRING THEORY THEREBY DIVERTING GLOBAL ATTENTION FROM BETTER THEORIES LIKE LQG. YOU SHOULD STOP GIVING UNDUE PUBLICITY TO WITTEN HOWEVER INTELLIGENT AND PRAISEWORTHY HIS MATHEMATICAL WORK HAS BEEN, FOR TRULY HE IS METHUSELAH INCARNATE. HE IS THE FIGUREHEAD OF THE STRING THEORY CULT AND MUST NOT BE GIVEN UNDUE PUBLICITY BY RIGHT-MINDED FOLKS.

   NOW DO SOMETHING USEFUL AND BLOG ABOUT THE AMS MATHEMATICAL SOCIETY NOVEMBER NOTICES PLEASE. I NEED VALIDATION FOR MY INSECURITY BY HAVING MY NEEDS ATTENDED BY PROMINENT PUBLIC MATHEMATICIANS. THANX.

4. **woit**
   October 8, 2005

   Sorry qwerty, but if you're looking for someone who knows something about imaging the brain (or prominent public mathematicians for that matter..), you're in the wrong place. And I'd much prefer that commenters attend to my needs and insecurities than expect me to do that for them.

   As for Witten, I don't think he's old enough to be Methuselah, and doesn't really deserve to be compared to Mephistopheles, which I think is what you had in mind. His recent work on geometric Langlands mercifully has nothing to do with string theory unification or the Landscape, and a lot to do with the relation of quantum field theory and mathematics, which is a hopeful sign.

5. **Luboš Motl**
   October 9, 2005

   Dear Peter,

   one of the reasons why I don't see why it's "program" is that it is not clear what kind open questions and eventual "big goals" i.e. future in general does it offer. Can you write something about it?

   Let me remind you that N=4 super Yang Mills, for example, is exactly equivalent to type IIB string theory on AdS5 times a five-manifold, a completely standard string theory with 10 dimensions, strings, D-branes, and all other objects that you so fervently dislike. So maybe your idea that something goes away from strings is confused after all. 😉

   All the best
   Luboš

6. **A.J.**
   October 9, 2005

   Hi Lubos,

   The Langlands Program is (amongst other things) an attempt to understand the Galois groups of number fields. The Galois group of a field extension, you'll recall, is the group of automorphisms of the extension field which fixes the original field. They're the central objects of number theory; understand all the Galois groups and you can answer almost any question in number theory. Understanding the Galois groups would be _huge_; it would completely change number theory.

   The nice thing about Galois groups is that they fit together into hierarchies, which mirror the hierarchies of field extensions — and even better, there is, for every number field, an uber Galois group which governs this entire hierarchy. This is the absolute Galois group, the Galois group of a number field's maximal field extension.

   Unfortunately, the absolute Galois group G is a pretty hard object to get your hands on. So mathematicians usually approach it by trying to understand its category of finite dimensional representations. This is an easier problem: representations are nice linear objects, and we can sort them by their rank. So, we can look at a simpler problem: trying to understand all representations G -> GL_n

Langlands insight was one can understand the n-dimensional representations of the Galois group by relating them to certain representations of the group of adeles of the original number field. By "certain" above, what I really mean is automorphic; we study the representations which exhibit a kind of modular behavior. A huge amount of work has gone into understanding these conjectures; the case of two-dimensional representations was basically the Taniyama-Shimura conjecture which implied Fermat's Last Theorem. We don't have anything resembling a complete solution for number fields yet though.

So we try to learn more about this Langlands correspondence by translating it into other fields. We can realize a number field as the field of rational functions on an arithmetic curve. When we do this, the Galois group gets reinterpreted as the fundamental group of our curve! So maybe we can learn something by studying the n-dimensional representations of the fundamental groups of algebraic curves? But these are the flat connections on principal GL_n-bundles on our algebraic curve! They're nice geometric objects. In particular, they make sense even when our curve is not an arithmetic curve. So we can study the Langlands phenomena over the complex numbers for instance. We don't even need to restrict ourselves to maps from pi_1(curve) to GL_n. We can replace GL_n by G. At this point, things start to look physics-y. Flat connections on a Riemann surface are instantons of Yang-Mills theory, the automorphic representations can be thought of as sections of certain line bundles living on the space of G-bundles, and the Langlands map starts to look a lot like mirror symmetry.

All of which is a long winded way of saying, we're borrowing ideas from physics to understand questions in algebraic geometry that originally arose in number theory. For myself, the main fun is that I get to think about relations between quantum field theory and the theory of moduli spaces.

Hope that was a satisfactory answer.

–A.J.

7. **woit**
October 9, 2005

Thanks A.J. for the explanation of the Langlands program.

Some other comments: while the Langlands program for number fields is a central idea in modern number theory and has proved its importance in many ways (e.g. the proof of Fermat), what geometric Langlands is good for is less clear. I know of number theorists who are dubious that it has anything useful to tell them, and other kinds of mathematicians who are also skeptical about it. Personally I've always found it fascinating because of its relationship to QFT. It involves many of the same mathematical structures that come up in 1+1 d QFT, including affine Kac-Moody representations, the moduli space of flat connections on a complex curve, etc. The kind of relationship to 4d QFT that Witten is talking about is yet a very different kind of relationship to QFT.

What has always fascinated me about the Langlands program in general is that one of the basic ideas is to look at the cohomology of a moduli space (actually a Shimura variety in the number field case), which gives you both an automorphic representation and a representation of the Galois group, establishing a relation between these. The TQFTs that come out of 2 and 4d gauge theories also are basically all about the cohomology of a related moduli space. And these TQFTs are just twisted versions of supersymmetric gauge theories not that different than the standard model QFT. There's some still not understood relationship here between perhaps the deepest ideas in physics and one of the deepest ideas in mathematics, and neither side of this story is well understood. If we understand this relationship better, we're likely to learn something new about QFT, number theory, or both.

8. **A.J.**
October 11, 2005

Hi Peter,

I have to admit: I don't care too much about the number theory side of Langlands. It's interesting in so far as its geometry, but the relations to physics are a lot more interesting. It's quite tempting to think that quantum field theories are in fact the cohomology theories of derived stacks.

Assorted Links

October 8, 2005
Categories: Uncategorized

An assortment of news and links that may be of interest:

The Tevatron has achieved a record luminosity for a hadron collider: $1.41\times10^{32}cm^{-2}sec^{-1}$. This is higher than the best luminosity at the ISR at CERN, and that was a proton-proton collider. Getting to high luminosity at the Tevatron is a lot harder since one need to create and store an intense beam of antiprotons.

The proceedings of this year's Lattice 2005 conference are now online.

Prior to the summer's big algebraic geometry conference in Seattle, there was a Graduate Student Warm-Up Workshop at which there were some excellent expository talks, for which lecture notes are online. A couple of these talks were specifically relevant to physics (Jim Bryan's and Ron Donagi's), but they are all interesting and worth reading.

The Bulletin of the AMS has a new editor and will soon have a new cover. One article soon to appear is a short piece by Michael Atiyah on Mathematics: Art and Science which contains a very interesting explanation of his views on mathematical beauty. Another is a review article Floer Theory and Low Dimensional Topology by Dusa McDuff. Floer theory has its origins in Witten's work on supersymmetry and Morse theory. McDuff goes over this, and explains recent results on Heegard Floer theory due to Peter Ozsvath and Zoltan Szabo. Ozsvath is my colleague here in the math department, and he has recently been joined by Mikhail Khovanov who moved here from Davis. The relation of Khovanov's new homology theory for knot invariants and the Heegard Floer theory is the subject of recent work by several mathematicians, including a second new Columbia faculty member, Ciprian Manolescu.

There's a fantastic new set of introductory lectures on quantum field theory by Luis Alvarez-Gaume and Miguel Vazquez-Mozo. In less than a hundred pages they cover a wide range of subjects including the basics of quantum field theory, anomalies, renormalization, external field problems and supersymmetry. Page for page it's by far the best introduction to the subject I've ever seen. For some other similarly excellent introductions to the subject, see one by 't Hooft and one by Pierre van Baal.

The last two items come from links on Gerard 't Hooft's excellent web-site which includes a useful page on How to Become a Good Theoretical Physicist. He has just put up a new page on How to Become a Bad Theoretical Physicist, where he notes that "It is much easier to become a bad theoretical physicist than a good one." This page is still under construction, I fear that he has a large amount of potential material for it.

Comments

1. **Jean-Paul**
October 8, 2005

   Atiyah's piece is very nice. It is very interesting that mathematicians' connection to art is always made through beauty, harmony etc. Well,
art historians would tell you that these are

2. **Tony Smith**
October 8, 2005

   't Hooft says on his web page at http://www.phys.uu.nl/~thooft/theoristbad.html:
"… Young and inexperienced students could surf the web and get seriously confused by what they find. Therefore … Here is my BLACK LIST. …".

   My personal opinion is that "Young and inexperienced students" should be exposed to all kinds of stuff – right, wrong, crazy, sane, etc – so that they can form their own ability to judge stuff. Further, such concern about "Young and inexperienced students" is what led to the death of Socrates.

   Tony Smith
http://www.valdostamuseum.org/hamsmith/

   PS – Although I have not yet made 't Hooft's BLACK LIST, 't Hooft goes on to say:
"… There are many more to add. I'll be back when I found their names.
G. 't Hooft …".

3. **D R Lunsford**
October 8, 2005

   I've done almost everything on t'Hooft's "good" list. This proves that I am a good theorist! Why didn't I know this 20 yrs ago?

   The primer on QFT was indeed very good, nice find.

   -drl

4. **Dissident**
October 8, 2005

   No D R L, at best it proves that you SHOULD be a good theorist… 😉

5. **plato**
October 9, 2005

   I like Atiyah's article as well…..to bad Jean-Paul didn't finish.

   *If theory is the role of the architect, then such beautiful proofs are the role of the craftsman. **Of course, as with the great renaissance artists, such roles are not mutually exclusive**. A great cathedral has both structural impressiveness and delicate detail. A great mathematical theory should similarly be beautiful on both large and small scales.*

   As the current story of the interaction between geometry and physics shows, the feedback from science to mathematics can be extremely profitable, and this is something I find doubly satisfying. Not only can we mathematicians be useful, but we can create works of art at the same time, partly inspired by the outside world.

Mathematics: Art and Science

Impressive, and always lots to learn.

It's always good to learn some history, as John Baez relates here

6. **Nigel**
October 9, 2005

'… There's a fantastic new set of introductory lectures on quantum field theory by Luis Alvarez-Gaume and Miguel Vazquez-Mozo…'

Thanks for this link, Peter. I like the explanation on p71: '… we find the electromagnetic coupling grows with energy. This can be explained heuristically by remembering the effect of the polarisation of the vacuum … these virtual pairs behave as dipoles that, as in a dielectric medium, tend to screen this charge, decreasing its value at long distances (ie at lower energies).'

If so, then shouldn't people be going all the way here, and developing a physical model of the polarised virtual field that will allow an alternative, more classical-type, calculation of the usual QED results, to avoid renormalisation? If only there was more of this heuristic explanation in physics, and less abject speculation (ST)…

7. **MathPhys**
October 9, 2005

Personally, I would have liked to see an introduction to non-perturbative aspects of QFT. I feel that students are sort of cheated by a purely perturbative approach. I thought that Luis A-G, more than almost anyone else, is qualified to write such an introduction.

8. **ks**
October 9, 2005

When mathematicians start to talk about the beauty of their discipline they end up returning to a 19-th centurys classicistic discourse: cathedrals, Bach fugues, white marble… *sigh*. I would wonder how indian or chinese mathematicians describe their discipline in aesthetic terms which conventional comparisons they use to describe their most inner feelings. Do japanese mathematicians compare proofs with cherry blossoms or do they find such comparisons as annoying then myself?

9. **Pseudo-string-fan**
October 9, 2005

It is quite amazing to know there is a how-to-be-a-good-theoretical-physicist recipe on t Hooft's webpage. Then, can we massively produce the good theorectical physicists by his magic suggestions?

10. **Arun**
October 9, 2005

t Hooft provides necessary, but not sufficient conditions.

11. **plato**
October 9, 2005

I would wonder how indian or chinese mathematicians describe their discipline in aesthetic terms which conventional comparisons they use to describe their most inner feelings.

Maybe as a Taoist figure topological expressed Ying and Yang, defined, as a Calabi Yua?

Tony Smith, might have an answer for you?:)

On my speculation could General Relativity understood these "momentum occasions" as inclinations of circles and ellipses in martial art forms?:)

Last comment I will make like this Peter, as I know you like to run a tight ship.

12. **Jean-Paul**
October 9, 2005

I see that ks is exactly on the same track as what I was going to say.
The connection from mathematics to art is usualy made through beauty and harmony, using the pre-XIXth century concept of art.
Take music: The tonal harmonic system ended (I would say Mahler gave it a final blow) more or less at the same time as quantum mechanics was born, and it was replaced by atonal, dodecapohony etc. Are mathematicians ready for such a radical step? Can anybody tell me whether there was any radical change in mathematics methodology except for the same old proofs getting longer and longer?

13. **plato**
October 10, 2005

Can anybody tell me whether there was any radical change in mathematics methodology except for the same old proofs getting longer and longer?

Now what does this have to do with the math?

Such a comparison I would think, was from the perspective of the artist himself and , not the completion of the 10th, although attempts as seen were made to do this?

http://en.wikipedia.org/wiki/Gustav_Mahler#Symphonies

Any way, was it meant as, "the shift to new mathematics" and not the death of tonal qualities in relation to perspective views on "sound " and views on the cosmo respectively? We know how this shift happened if held to Wayne Hu.

Would mathematicians concurr?

14. **sunderpeeche**
October 10, 2005

Did anyone read the Fermilab link? The peak luminosity is 1.41 x 10^30 (not 10^32 as reported in the blog). But still a record.

15. **woit**
October 10, 2005

    Actually the number was right:

    141 x 10^30=1.41 x 10^32

16. **D R Lunsford**
October 10, 2005

    On the assorted link front, we see this:

    http://xxx.lanl.gov/abs/astro-ph/0507619

    GR explains what NG can't. No dark matter!

    -drl

17. **ali**
October 10, 2005

    D R L:

    You might want to look at this response to the paper you mentioned. Apparently, their metric and assumptions aren't self-consistent:

    http://arxiv.org/abs/astro-ph/0508377

18. **blank**
October 10, 2005

    The above link was slashdotted today for some reason. I knew it sounded too good to be true.

19. **D R Lunsford**
October 10, 2005

    ali and blank,

    The rebuttal paper is by no means convincing. The posited matter distribution is not at all unreasonable, nor is it the only possibility. The remarkable thing is, someone actually bothered to solve the equations instead of invoking yet another hand-waving argument. I'm sure we'll see a "re-rebuttal" soon!

    -drl

20. **Arun**
October 10, 2005

    The idea that General Relativity provides only tiny corrections to Newtonian Gravity as far as galaxies are concerned is not a hand-waving argument, but a result from a well-developed expansion for an arbitrary source (e.g., see chapter 39 of Misner, Thorne, Wheeler). The expansion obviously doesn't hold if there is a singular mass distribution, and perhaps singular mass distributions are reasonable.

    The point is that it was not handwaving exercise to write off GR for galaxies.

21. **Nigel**
October 10, 2005

    The question is what the dark matter is, it could be mostly asteroids, comets, planets, small dim dwarf stars, or black holes. I've not seen any suggestion that most of the mass of a galaxy is dark matter to justify the rotation. You can't justify having 10 or so times as much dark matter as visible star mass in a galaxy without massaging the model to fit the 'critical density' prediction… So the rest of the dark matter is then assumed to be in the intervening spaces as exotic particles, a nice ST-type untestable prediction.

22. **D R Lunsford**
October 11, 2005

    Arun et. al, Copperstock and Tieu explicitly state

    "The absolute value of z must be used to provide the proper reflection of the distribution for negative z. While this produces a discontinuity in Nz at z = 0, it is important to note that this has no physical consequence since Nz enters as a square in the density and Nz does not play a role in the equations of motion. Moreover, the metric itself is continuous. This is analogous to the Schwarzschild constant density sphere problem that leads to a discontinuity in metric derivative across the matter-vacuum interface in Schwarzschild coordinates. In principle, other coordinates could be found to render the metric and its first partial derivatives globally continuous but this would be counter-productive as it would unnecessarily complicate the mathematics. As in FRW, our co-moving coordinates simplify the analysis."

    where of course FRW means Friedmann-Roberston-Walker cosmology.

    This IMO is conclusive. There must be something wrong with the rebutters' Killing vector argument.

    -drl

23. **Nigel**
October 11, 2005

    D.R. Lunsford, thanks for this analysis 🙂

24. **Arun**
October 12, 2005

It should be fairly easy to decide.
The z-dependence of N is

N= exp(- k |z|) , k is some constant

Now what is limit as y-> 0 of Integral(Nz^2 dz, { -y, + y))?

If the limit is 0 then Cooperstock has a point. If it is not zero,
then there is a delta function there, and that is the singular disk.

25. **DMS**
October 12, 2005

To the list of QFT notes, I would also add the tome Fields by Warren Siegel (hep-th/9912205).

26. **Nigel**
October 12, 2005

I've recently added more stuff to my page in the hope of making t'Hooft's bad list! (All publicity is good when you're suppressed!) 😉

27. **Arun**
October 12, 2005

Re: mine of 8:18 AM, upon reflection, that is not a good way of looking for a density singularity.

28. **Arun**
October 12, 2005

See this on why Cooperstock et al. are wrong:

http://cosmicvariance.com/2005/10/04/most-exciting-discovery/#comment-4996

29. **D R Lunsford**
October 12, 2005

Arun,

See thread on SPR. Story is not over 🙂

-drl

30. **Arun**
October 13, 2005

I see 5 messages on SPR and nothing new. What am I missing?

31. **N.R.**
October 13, 2005

RE: art and science

Saw this on slashdot:

Art of Particle Physics

http://www.symmetrymagazine.org/cms/?pid=1000198

look at the pdf, as apparently, their is an error in the basic page

Physics Strings Us Along

October 11, 2005
Categories: Uncategorized

A commenter here wrote in to point out that Margaret Wertheim, a science columnist for the Los Angeles Times, has a new piece entitled [Physics strings us along](). She discusses [Lisa Randall's new book]() as an example of physics that has become completely unmoored from empirical evidence and instead "has become in effect a form of speculative literature" (much like John Horgan's characterization of this sort of thing as "science fiction in mathematical form"). Wertheim notes that it is becoming hard to distinguish theoretical physics from religion and magic, supposedly less rational practices, claiming that "in recent years science itself has been showing increasingly magical tendencies", concerning itself with "entire landscapes of universes for which there is no empirical evidence whatever. "

She is writing a book about "the role of imagination in theoretical physics", and she seems overly enthusiastic about how "Unchained by the fetters of verification, string theorists are free to dream, articulating through their equations vast imagined domains in which almost anything that is mathematically possible is deemed to be happening 'somewhere.'"

Comments

1. **Chris Oakley**
   October 11, 2005

   I saw the article. I also saw one in a March 2005 Physics Today where they interview Kaku. Someone seems to have likened him to Jerry Springer. Unfortunately, though, it seems increasingly unlikely that String Theorists will ever host a Reality Show.

2. **Lubos Motl**
   October 11, 2005

   I was personally not too happy with this article either. But let me mention that a physicist mentioned in your article 😉 may have had a different opinion.

3. **Rob Scott**
   October 11, 2005

   Chris, according to the article there must be many universes where string theorists do indeed host Reality Shows, after all it is mathematically possible:) It's just not this one.

4. **Egbert Humplebody**
   October 11, 2005

   It seems there's [another]() story in the news about how string theory can explain everything.

5. **Quantum_Ranger**
   October 12, 2005

   Rob Scott says "Chris, according to the article there must be many universes where string theorists do indeed host Reality Shows, after all it is mathematically possible:) It's just not this one".

   And thus, there is another Universe stringtheorists keep ignoring? This one!

   For if it is so that there are a multidude of Universes, then they have to admit that there is one Universe wherby the theory of strings does not exist?

   A stringtheorist who does NOT admit this fact, by their own scientific arena, methods of madness, must be 'string-calculating', inside this Universe, but the resulting evidence is elswhere!

   How can stringtheorists be so sure that stringtheory EXISTS in this Universe, and not in another perticular Unverse? by their own evidence of MW's, the evidence appears to be that Strings are elswhere, even though the theorists themselves, appear at first glance, to be residing in the same referenced Universe as the Human Race?

6. **MathPhys**
   October 12, 2005

   I'm not shocked by the rise and rise of M Kaku in the US media, afterall, we have Jerry Springer. What I find shattering is that a salesman who starts every talk by plugging his most recent paperback, actually gets invited, along with respectable people like Vafa and Witten to conferences such as the Alexandria Einstein symposium.

7. **Ranger**
   October 12, 2005

   http://physicsmathforums.com/showthread.php?p=403#post403

   FOR IMMEDIATE RELEASE:

   ALL TIED UP & STRUNG ALONG, a movie about String Theorists and their expansive theories which extend human ignorance, pomposity, and frailty into higher dimensions, is set to start filming this fall. Jessica Alba, John Cleese, Eugene Levie, Jackie Chan, and David Duchovney of X-files fame have all signed on to the $700 million Hollywood project, which is still cheaper than String Theory itself, and will likely displace less physicists from the academy.

   "As contemporary physics is about money, hype, mythology, and chicks," Ed Witten explained from his offices at the Princeton Institute for Advanced Study, "The next logical step was Hollywood, although I thought Burt Reynolds should play me instead of Eugene Levy."

   Brian Greene, the famous String Theorist who will be played by David "the truth is out there" Duchovney, explained the plot: "String theory's muddled, contorted theories that lack postulates, laws, and experimentally-verified equations have Einstein spinning so fast in his grave that it creates a black hole. In order to save the world, we String Theorists have to stop reformulating String Theory faster than the speed of light. We are called upon to stop violating the conservation of energy by mining higher dimensions to publish more BS than can accounted for with the Big Bang alone, and I win the Nobel prize for showing that M-Theory is in fact the dark matter it has been searching for."

Greene continues: “At first my character is reluctant to stop theorizing and start postulating, but when my love interest Jessica Alba is sucked into the black hole, I search my soul and find Paul Davies there, played by John Cleese. I ask him what he’s doing in my soul, and he explains that the answer is contained in the mind of God, which only he is privy too, but for a small fee, some tax and tuition dollars, a couple grants here and there, and an all-expense-paid book tour with stops in Zurich and Honolulu, he can let me in on it. And he shows me God in all her greater glory, as he points out that we can make more money in Hollywood than writing coffee-table books that recycle Einstein, Bohr, Dirac, Feynman, and Wheeler. I am quickly converted, and I agree to turn my back on String Theory’s hoax and save Jessica Alba.”

But it’s not that easy, as standing in Greene’s way is Michio “king of pop-theory-hipster-irony-the-theory-of-everything-or-anything-made-you-read-this” Kaku, played by Jackie Chan. Kaku beats the crap out of Greene for alomst blowing the “ironic” pretense his salary, benefits, and all-expense paid trips depend on. “WE MUST HOLD BACK THE YOUNG SCIENTISTS WITH OUR NON-THEORIES!! WE MUST FILL THE ACADEMY WITH THE POMO DARK MATTER THAT IS STRING THEORY TO KEEP OUR UNIVERSE FROM FLYING APART, OUR PYRAMID SCHEMES FROM TOPPLING, AND OUR PERPETUAL-MOTION NSF MONEY MACHINE FROM STOPPING!!” Kaku argues as he delivers a flying back-kick, “There can be ony ONE! I WILL be String Theory’s GODFATHER as referenced on my web page!! I have better hair!”

But Greene fights back as he signs his seventeenth book deal to make the hand-waving incoherence of String Theory accessible to the South Park generation, senior citizens, and starving chirldren around the world. “Kaku! Kaku! (pronounced Ka-Kaw! Ka-Kaw! like Owen Wilson did in Bottle Rocket),” Greene shouts. “It is theoretically impossible to build a coffee tables strong enough to support any more coffee-table physics books!!!”

“Time travel is also theoretically impossible, but there’s a helluva lot more money for us in flushing physics down a wormhole. Nobody knows what the #&#%&$ M stands for in M theory ya hand-waving, TV-hogging crank!!! Get it?? Ha Ha Ha! We’re laughing at the public! We’re the insider pomo hipsters! Get with the gangsta-wanksta-pranksta CRANKSTER bling-bling program!!”

How does it all end? Does physics go bankrupt funding theories that have expanded our ignorance from four dimensions into ten, twenty, and thirty dimensions? Do tax payers revolt? Do young physicists overthrow the hand-waving, contortionist bullies and revive physics with a classical renaissance favoring logic, reason, and Truth over meaningless mathematical abstractions? Does Moving Dimensions Theory (MDT) prevail with its simple postulate? We’ll all just have to wait!

But in the meantime, how do you think it will play out?

Will theories with postulates ever be allowed in physics again? Or will the well-funded, tenured pomo String Theory / M-Theory (Maffia-Theory) Priests send their armies of desperate, snarky postdocs and starving graduate students forth to displace and destroy all common sense, logic, reason, and physics in the academy? It must be so–for the greater good of physics, the individual physicist, and thus physics, must be sacrificed.

MDT’s postulate: THE FOURTH DIMENSION IS EXPANDING AT A RATE OF C RELATIVE TO THE THREE SPATIAL DIMENSIONS IN QUANTIZED UNITS OF THE PLANCK LENGTH, GIVING RISE TO TIME AND ALL CLASSICAL, QUANTUM MECHANICAL, AND RELATIVISTIC PHENOMENA.

http://physicsmathforums.com/showthread.php?p=403#post403

8. **Bryan**
October 13, 2005

On the topic of Dr Lisa Randall, I think it sad that Scientific American made fun of her in the Oct issue, pp20-22.

The article is headed ‘The Beauty of Branes’ and has a big picture of her in front of a blackboard.

Underneath the picture is the caption: ‘Lisa Randall: Warped Thoughts.’

The article concludes by quoting her collaborator Andreas Karch of University of Washington: ‘I often don’t understand her. When she says things, they don’t make sense and I first think ‘she is crazy’. But I don’t say anything…’

With friends like these, who needs enemies?

9. **Rainey**
October 16, 2005

um actually i dont think the article was making fun of her at all. the actual article, on pages 38-40

concluded with the sentance:

“But I don’t say anything, because she is usually right. Lisa just knows the answer.”

Just pointing this out because if you read the above post Karch’s comment seems pretty patronizing. Add the final sentance and he just sounds ignorant.

Usually taking things out of context “warps” their meaning…;)

10. **Bryan**
October 17, 2005

‘…because she is usually right. Lisa just knows the answer.’

This fear of pointing out potential problems ‘because she is usually right’ and ‘just knows the answer’ in my opinion makes Lisa sound patronising, which is why I left it out. It has nothing to do with the point being made, that someone senses a problem, yet does not have the courage to say so.

11. **Nigel**
October 17, 2005

I hate the idea of personally ridiculing string theorists just because they are wrong scientifically, but who gave the idea to Laughlin for the following analogy in the San Francisco Chronicle?

“… skeptics suggest it’s the latest sign of how string theorists, sometimes called “superstringers,” try to colorfully camouflage the theory’s flaws, like “a 50-year-old woman wearing way too much lipstick,” jokes Robert B. Laughlin, a Nobel Prize-winning physicist at Stanford…”

http://sfgate.com/cgi-bin/article.cgi?file=/chronicle/archive/2005/03/14/MNGRMBOURE1.DTL

Loops ’05, Again

October 11, 2005
Categories: Uncategorized

This week there’s a large conference in Potsdam on non-perturbative/background independent quantum gravity called Loops ’05. The programme is on-line, and there is live-blogging from Robert Helling.

Update: String theorist Robert Helling has more coverage of the conference. This includes the hilarious criticism (quoted approvingly by Jacques Distler) that too many of the talks were “so vague and speculative that they are not even wrong.” Helling does notice that the string theorist speaker (Stefan Thiessen) delegated to talk about what is going on in string theory had nothing to say, and just repeated the failed dogma from more than 20 years ago. Maybe at Strings 2006 they’ll even let someone from the LQG camp speak, or at least there will be live-blogging from an LQGer.

Note: This is a reconstruction of my original posting. Unfortunately I accidentally hit the wrong button when trying to edit a typo in a comment, deleting the posting and all comments. Thanks to Steve and Aaron for helping me retrieve the content of the posting, but unfortunately most of the comments were lost. If you have copies of them, please send them to me or resubmit them yourself.

Update: For something truly bizarre, see Lubos Motl’s comments on Loops ’05, where he attacks the talks there as “not even wrong”, while in the same posting respectfully reporting on a talk by Cumrun Vafa at Radcliffe on the Swampland, a talk at which several people evidently expressed the opinion that it could never lead to an explanation of anything about physics.

Update: There’s a bit more about Loops ’05 in the latest issue of John Baez’s “This Week’s Finds”.

Update: The talks from the conference are now on-line.

Comments

1. **Who**
   October 11, 2005

   great of Robert.
   meets the hunger for news from the conference and
   also the need always for a critical outside perspective.
   real gift. thanks Robert! Looking forward to more
   (e.g. Laurent Freidel on Tuesday, Loll and Reuter on Wednesday, and to Roberts choices of what talks to hear and his commentary on them)

2. **Shantanu**
   October 11, 2005

   Peter (in case you didn’t know) next week at Stony Brook there is an interesting symposium on geometry and universe which predominantly consists of relativists.
   http://insti.physics.sunysb.edu/itp/conf/GR2005/ were you planning to go for this?
   Maybe you can tell the readers about this conference and also let us know what the traditional GR community thinks of string theory (which will definitely be discussed at this conference).
   Shantanu

3. **a**
   October 11, 2005

   “traditional GR community thinks of string theory??

   Shantanu, what does the traditional GR community think of string theory?

4. **Robert**
   October 12, 2005

   I cannot reconstruct all I commented earlier. My main point was that Theissen gave a standard “strings for non-experts” talk rather than addressing the loop people and their prejudices directly. This way, they had seen it all before and he gave away the possibility of telling them something that is new for them and maybe even thought provoking.

5. **Lee Smolin**
   October 12, 2005

   Hi, if I can put a comment here I didnt figure out how to put on Robert’s blog:

   Thanks for the comments. If I’d known you were here I could have answered some of these questions in person.

   Let me emphasize first that the point of my talk was to emphasize important open issues. We are not afraid of emphasizing open problems but we do hope that people notice when they are solved. Hence, I would have hoped you noticed and reported that major progress was described concerning the problem of showing that classical spacetime honestly emerges from background independent theories. Rovelli derived the graviton propagator and hence Newton’s law. Freidel and Livine showed in detail that 2+1 quantum gravity with matter has a low energy limit which is an effective QFT on a non-commutative geometry with deformed Poincare invariance. Both results were derived from spin foam models.

   Hence, one can no longer say that there is no understanding of how classical spacetime and low energy qft emerges from these theories.

   There was still more. Perez showed how regularization ambiguities in the Hamiltonian constraint may be resolved. Markopoulou discussed a new approach to the low energy liimit based on her new paper with Kribs. Loll announced major results showing that 3+1 spacetime emerges from causal dynamical triangulations and that at short distances the theory scales as a 1+1 dimensional theory. Livine and Terno showed how to derive the log(area) corrections to black hole entropy and estimate the rate of Hawking radiation. Starodubtsev reported on work with Freidel in which quantum gravity is defined by a perturbation expansion around a topological quantum field theory where the expansion parameter is G Lambda. This is a very promising direction as there are indications (no proof yet) that this new pert. theory is renormalizable and the low energy limit reproduces QFT in DeSitter spcetime……

These, and other results were based on detailed calculations and are solid results. I would have hoped that your report would have focused on the presentation of these major result.

In the face of this kinds of impressive progress, I was not embaressed to emphasize open issues or speculate a bit about future directions. So my talk was certainly not representative. By the way, I would have thought that as a particle physicist you would have recognized that what I presented was just a preon model. It was translated into the language of LQG with the help of recent work by Bilson-Thompson. This is new stuff and much remains to be done. But you can also no longer say that there is no proposal for unification of matter and geometry in LQG.

Still to come are new results on rigorous formulations of the theory and at least one striking new results on quantum cosmology, of relevence for upcoming observations.

6. **Who**
October 12, 2005

a friend of ours was in Berlin and happened to attend Lee Smolin's public lecture. Here is Ratzinger's report:
—quote—
I'm in Berlin right now and found out that Lee Smolin is giving a public lecture, named "The unfinished revolution: finishing what Einstein started". So how could I resist?

He started with the question "what is at stake?". Answer: all the big questions (what is time, space, physical law? why is the universe hospitable to life?). After going through the three revolutions (Aristotle, Newton, Einstein) and their notions of space, time, etc., he stressed the importance of relationality in present-day world view, both in qm and gr.

(He said Leibniz was right about relationality, but he had no workable physical theory, so scientist followed Newton for 200 years. Mach and then Einstein rediscovered relational thinking.)

So what are the approaches to attack quantum gravity? There are two, according to Smolin.
1. Einstein's way (rethinking the concepts of space and time, and especially reworking qm)

2. everybody else's way (string theory, loops, etc.)

He admitted that he researches everybody else's way, but pointed out the need for radical and rebellious thinking the Einstein way. He also praised Penrose in that context.

He ended with saying that physicists are standing in the lights and shadows of Einstein. Lights: Einstein's theories, that they work with. Shadows: ignoring what is at stake and not sharing Einstein vision of a complete understanding of the universe by rejecting qm.

Edit: 1. I think Renata Loll was also sitting in the audience.
2. Smolin was very excited by the coming representation of Winkler and another guy on Friday.
—endquote—
http://www.physicsforums.com/showthread.php?p=785298#post785298

[my comment: the other guy is probably Abhay Ashtekar, whose talk on Friday will discuss what replaces the hole and bang singularities when the classical singularities are removed by quantum gravity methods. This would probably interest Lee Smolin since he has conjectured that the two regimes could be related—black hole pit expanding to form another spacetime region.]

7. **Who**
October 12, 2005

or the other guy, whose name Ratzinger didnt remember, could be Viqar Husain—-Husain is also giving a talk Friday and he and Winkler have a new QG way to remove the BH singularity.

8. **Nigel**
October 13, 2005

a: '…what does the traditional GR community think of string theory? '

I earlier posted that Sir Roger Penrose who is the only person that actually understands GR physically – or at least the only person who lets on that he does – thinks Witten is talking out of his hat where Witten says in a leading journal in April 1996 that ST: 'has the wonderful property of predicting gravity' (not an exact quote, but close enough).

Penrose has this issue that ST is at best just a perturbative calculating procedure which cannot predict – or lead to – the prediction of the strength of gravity.

Witten's praise of his own work on M-theory by calling it a wonderful prediction of gravity reminds me of the controversy when Edward Teller around 1983 hyped the 'wonderful' space-based nuclear explosion pumped x-ray laser as a thing the size of a suitcase which can shoot down the entire Soviet missile force, 'if in the field of view'. (After checking the equations, it turned out that the x-ray laser would need to be about 1000 metres long, and that as such it would only be able to shoot one thing at a time, and would be hard to send into space anyway.)

Sorry if I've gone off topic a bit 😉 On my weblog, I've got all kids of rubbish I can't delete because you have to keep pressing 'confirm to proceed to deletion' and 'are you really sure you want to delete this post, it looks so brilliant? please click here again before it can be deleted!' So I just give up trying.

9. **Robert**
October 13, 2005

Lee,

please reread what I wrote. I said I liked Rovelli's talk an recommended reading his paper. I gave some extended coverage of what you were saying but you have to admit your remarks on 'quarks', your remarks on CMB and the Poineer anomaly were quite bold to say the least. I expect to read some popular texts on LQG where these topics will be mentioned as 'understood/solved by the canonical approach'.

What Markopoulou said sounded to me (but that might be my fault) as an introductory lecture in block spin transformations and RG group in the 1D Ising model. The remarks on how evolution is like a channel in quantum information theory again were not very concrete.

And, as you will find stated in my second post, I had to leave Golm during Friedel's lecture to return to teaching duties so I could not report on what happened after Tuesday 10:40.

As far as the announced results on quantum cosmology go, I have to admit, I am sceptical. Loop quantum cosmology had one big result in the past, the fact, that the initial singularity is not there and the inverse area operator was bounded (i.e. the universe cannot get arbitrarily small). This result was celebrated as a huge step forward and all the publicity was collected in favour of LQG.

In the meantime however, Thomas Thiemann has shown, that this result was an artefact of the minisuperspace like approximation and that it the inverse area is unbounded in the full theory. As always everybody is encouraged to draw their own conclusions.

10. **Pindare**
October 14, 2005

Off-topic: there's a short article on string theory featuring Gabriele Veneziano (now chair at Collège de France) in today's edition of Le Monde at http://www.lemonde.fr/web/article/0,1-0@2-3244,36-698934@51-699012,0.html

Another one features Brian greene and Stephen Hawking and their popular science books.

This pops up because this week is the nationwide Sciencefest in France, and there's "a large public eagerly interested in theoretical physics" who read (part of) those books.

Of course this is just a tiny aspect of the Sciencefest, most other things are on less controversial topics, all the activities are listed at http://www.fetedelascience.education.gouv.fr/

11. **Arun**
October 14, 2005

If one goes to amazon.com and looks up David Lindley's book on Boltzmann ("Boltzmann's Atom: The Great Debate That Launched A Revolution In Physics"), then, at the end of the editorial reviews is an excerpt from the book. We learn of James Waterston who discovered some of the ideas of the kinetic theory of gases, but was ignored.

And the following contains probably useful advice for the 21st century as well:

> Waterston's achievement finally came to light in 1891, when the English physicist Lord Rayleigh, then secretary of the Royal Society, discovered the lost manuscript in the course of tracking down some old citations. By that time the kinetic theory amounted to a sophisticated and well-known body of knowledge, and Rayleigh immediately perceived the true merit of Waterston's ideas. He arranged for its belated publication as the first item in the first issue of the Philosophical Transactions for 1892, along with a brief commentary on its tortured history.
>
> Acknowledging that Waterston had submitted his work at a time when scientists thought very differently than they were accustomed to doing just a few decades later, Rayleigh admitted nevertheless he was surprised that the Royal Society's expert reviewers were so dismissive of the paper. "The omission to publish it at the time was a misfortune, which probably retarded the subject by ten or fifteen years," he wrote. Rayleigh suggested that Waterston might have done better to mention that he was working to elaborate ideas previously suggested by Daniel Bernoulli, whose reputation was unarguable; that might have made a reviewer hesitate. But Bernoulli's work had itself been forgotten, and it is the strength of Waterston's claim to unjust treatment that he indeed came up with his reasoning entirely by himself. On that score Rayleigh had another observation: **"Perhaps…a young author who believes himself capable of great things would usually do well to secure the favourable recognition of the scientific world by work whose scope is limited, and whose value is easily judged, before embarking on greater flights."** The reliable route to scientific fame, in other words, requires brilliance judiciously combined with careerism. Just as well, perhaps, that the already bitter Waterston didn't live to see this endorsement of his unrewarded endeavors.

12. **Dissident**
October 15, 2005

Great advice Arun. Surely it would have been much better for everybody involved if young Albert Einstein had concentrated on establishing a solid track record in molecular physics till he was 40 or so, to then tackle the big vision task of unifying mechanics and electromagnetism.

BWAHAHAHAHAHA!

13. **Chris Oakley**
October 15, 2005

Unfortunately Arun's advice is all too sound: if you concentrate your mind on how not to be a threat to the has-beens or never-weres who run the physics establishment, then they might, after forcing you to jump through a lot of hoops, offer you a permanent job. If your brain has not completely atrophied by then, then who knows, you might even have some infinitesimal contribution to make to the subject.

Relativity and Quantum Mechanics only happened because there were enough bright 23-25 year olds around in the early part of the last century brave enough to think for themselves. Although the research establishment then was just as much a gerontocracy as it is now, it was one of those situations where the old timers simply could not afford to ignore the youngsters.

14. **Dissident**
October 15, 2005

Arun's advice is sound only if the goal is to get a job in academia. Why anyone would be willing to sacrifice his or her intellect and creativity for such a lowly reward is beyond me. There are far more rewarding occupations which will let you exercise both to the fullest *and* which offer the very real possibility of reaching financial independence well before 40 (especially if you don't mind living on a theoretical physicist's budget). And then it's off to the races. 🙂

15. **Chris Oakley**
October 15, 2005

Actually, I *did* want a job in theoretical physics. I was prepared to put up with the low salary (less than a schoolteacher, by the way) in exchange for doing what I love doing. I will never have moral qualms about going in search of £££ in the City of London as leaving academia was a question of being pushed rather than jumping.

16. **The Statistical Mechanic**
October 15, 2005

"[..] Of course, Peter Woit followed up on this line, because it is his argument that superstring theory is not even wrong. [..]"

17. **Arun**
October 15, 2005

Dissident,

Albert Einstein did concentrate on molecular physics – that was his Brownian motion paper. Anyway, considering that Boltzmann committed suicide in 1906, in part we think because of opposition by Mach and Ostwald to atomic theory in general and Boltzmann's work in particular, molecular physics was not the place to be, either.

Anyway, Einstein's Nobel citation was "for services to Theoretical Physics, and especially of the law of the photoelectric effect." Perhaps another sign of the conservative nature of the establishment.

-Arun

18. **Dissident**
October 15, 2005

That's why I mentioned molecular physics, Arun (if memory serves, it was his thesis subject). But for how long did he "concentrate" on it?

And yes, that silly Swedish academy used the photoelectric effect to motivate his Nobel prize, so as to avoid openly endorsing relativity. It wasn't really controversial in the world at large by then, but apparently there were some local "heavyweights" in Uppsala who just didn't get it.

19. **MathPhys**
October 15, 2005

I'm disappointed that the coverage of Loops '05 has stopped after the first couple of days of talks. I would have liked to know what happened next. Can one find that anywhere?

20. **Lee Smolin**
October 16, 2005

Dear Robert,

Thanks, perhaps I misread you. I was perhaps over-reacting to your opening: "I sneaked into the Loops 05 conference…" I hope this was not how you really felt. Certainly, all were welcome, no one was checking badges.

But I also don't understand why, after doing a good job of just reporting, you feel the need to add comments like, "I expect to read some popular texts on LQG where these topics will be mentioned as 'understood/solved by the canonical approach'?" Perhaps you are used to an atmosphere in which people are not careful to distinguish conjecture, evidence and proof, but this is not us.

As to the other points, why not read the papers? Kribs and Markopoulou is gr-qc/0510052. Regarding singularity avoidance in the full QFT, the papers are by Johannes Brunnemann and Thomas Thiemann, gr-qc/0505032 and 033. Not surprisingly, the situation is quite a bit more complicated in the full theory than it is in the models, but their conclusion is not pessimistic. They do find states on which the inverse volume is not bounded. But they show it is bounded on a large class of coherent states. The last line of their abstract states, "After outlining what would be required, we present the results of a calculation for LQG which could be a first indication that our criteria at least for curvature singularity avoidance are satisfied in LQG."

If I may add, the atmosphere in the quantum gravity community is pretty open. Most of us know and easily acknowledge that what we are doing is high risk. Most of us are very self-critical and any honest critic will be made to feel very welcome because you cannot be more critical to us than we are to ourselves.

Looking forward to seeing you next time,

Lee

21. **Who**
October 18, 2005

John Baez TWF #222 is out.

Scroll 3/4 of the way down the page for his report on a couple of things of interest from Loops '05.

http://math.ucr.edu/home/baez/week222.html

22. **Who**
October 20, 2005

Unofficial word about when the VIDEO from Loops '05 will be made available at the website:

My understanding, based only on indirect report, is that they plan to have the videos of the talks up by beginning of next week.

Worth keeping an eye out for it. Thanks to John Baez for originally mentioning something about this.

23. **dan**
October 22, 2005

will there be a loop '06? incidentally, shouldn't all LQG researchers work on the semiclassical limit problem, for if it doesn;t reduce to GR, then it is not a viable theory of QG?

24. **Who**
October 23, 2005

will there be a loop '06? incidentally, shouldn't all LQG researchers work on the semiclassical limit problem,…?

Normally it might be better to wait until John Baez responds—since your question is one he can answer best. But he might not still be reading this thread, now that it is overlayed by other discussion. So I will take a shot at the question.

I havent heard anything official about a conference next year. It seems to me that enough is going on in the various allied research lines to make people want to have one next year. Last year it was at Marseille so it probably would not be there again so soon. It could be at Penn State (Ashtekar's institute) or a Perimeter in Waterloo, or it could be at several places in the UK, or possibly at Utrecht.

My private guess is that there will be Loops '06 and it will be at Utrecht. (Ashtekar seems to be taking his sabbatical there for at least part of 2006—the place has become a QG center.)

about the semiclassical limit problem, dan—-it does seem to me that there is a lot of focus on that now in several of the main QG research lines. I think your guess that they should is approximatley right and that they ARE focusing effort on that.

To get an overview, go to Loops '05 programme and take a quick look at several leading people's ABSTRACTS. Look at the abstracts of Rovelli, and Baez and Freidel and Loll.

All these people are focusing on this and related issues like including matter. Rovelli is explicit about this, also Baez, as you can see from the abstracts. Freidel has just finished showing that the 3D version of his spinfoam model DOES include matter and have the right largescale behavior. His program is to achieve success in 3D and then extend the results to 4D. He talked about the prospects for 4D at the conference. Still other nonperturbative QG approaches are those of Loll (CDT) and of Reuter (QEG)—where considerable effort is focused on these same issues.

So on the whole you are right, but there are exceptions. LQC (loop quantum cosmology) is almost a separate field. It may be possible to test it separately. There the semiclassical limit has already been shown. So the emphasis has shifted towards phenomenology—what might be some observable effects of LQC? Cosmology involves symmetry-reduced degrees of freedom (assuming isotropy and homogeneity simplifies the picture). One thing that can be explored is the relation to the full QG theory—-how similar will the predictions of full LQG (say in Thiemann's Master Constraint version) turn out to be to the "toy model" symmetry-reduced LQC?

Another complication is that, as is often pointed out, the field of (nonperturbative, non-string) QG includes several approaches—it isnt just the canonical LQG (described, say, in Hermann Nicolai's "outside view" paper). Offhand I can't think of anyone at Loops '05 whose abstract indicates they were talking about canonical LQG. The guy from Beijing Normal was discussing Thiemann's Master Constraint which is really a new QG approach. If you look over the programme and happen to see someone who is actually doing LQG proper, please let me know!
The terminology is really frustrating. Can of worms! People should really not ever say LQG. They should say "nonperturbative QG" (meaning Rovelli's spinfoam, Freidel's spinfoam, Loll CDT, Reuter QEG, Gambini CD, Thiemann MC,…) and when all these people get together they call it "Loops".

25. **dan**
October 23, 2005

"So on the whole you are right, but there are exceptions. LQC (loop quantum cosmology) is almost a separate field. It may be possible to test it separately. There the semiclassical limit has already been shown."

What is LQG and how is it there's so little word on its semiclassical limit? So it reproduces GR with quantum corrections on the planck scale?

26. **John Baez**
October 24, 2005

dan said:

will there be a Loops '06?

Probably; the idea of calling it Loops '05 was to make this an annual thing. However, we need someone to agree to run Loops '06 – and I don't think it's gonna be me!

Some obvious possibilities include Penn State, the Perimeter Institute, and Marseille, but they've all run conferences like this quite recently. So, Mexico and Utrecht are being mentioned.

I'm not sure Renate Loll will want to run something called Loops '05, since she considers her own approach – causal dynamical triangulations – quite distinct from loop quantum gravity, and more successful! Personally I think this year's conference should have been called something like QG '05, since there were talks on almost every approach to quantum gravity. Or maybe NOT VERY MUCH STRINGS '05. 🙂

Anyway, we'll see what happens.

dan said:

incidentally, shouldn't all LQG researchers work on the semiclassical limit problem, for if it doesn't reduce to GR, then it is not a viable theory of QG?

I think all loop quantum gravity researchers should work on this problem. That's why I keep talking about it every time I get a chance! I spoke about it at Marseille, at the Perimeter Institute, and at Loops '05. However, it's hard to get people to work on a very hard problem, when there are easier problems out there.

Similarly, I think all string theorists should be working on a background-free approach to this theory, and on finding a way for it to make specific predictions about particle physics. But at any given moment there are lots of easier things to do.

27. **Who**
October 25, 2005

I'm not sure Renate Loll will want to run something called Loops… since she considers her own approach… quite distinct from loop quantum gravity… Personally I think this year's conference should have been called something like QG '05…

QG '05 would have been a better choice (it seems now)
Names matter (you pointed this out somewhere not long ago) and a little thing like a name can be a bigger obstacle or source of trouble than one would expect—-I will spare everybody the Shakespeare quote.

For the first time I understand why they didnt simply announce at the end of Loops '05 that Loops '06 would be held at Utrecht Spinoza Institute. Damn. It is a shame. But I understand how Loll might object.

To me it looks different. If the Utrecht people would simply have Loops '06 at Utrecht and call it Loops, this would prove to the world that CDT IS LOOPS. It would show everybody that there is a collective research drive towards the goal of non-perturbative quantum theory of gravity which is not confined or characterized by one specific approach like canonical loop quantum gravity, or any particular path integral tactic either. So I see it for Loll as a good opportunity to establish a broad inclusive definition of the term "Loops".

28. **dan**
October 26, 2005

hello john & "who"

re: “However, it’s hard to get people to work on a very hard problem, when there are easier problems out there.”

from the standpoint of physics, if LQG & friends do not reproduce GR as its semiclassical limit, then is there any reason to continue researching it? so i’m not sure if it is wortwhile to work on the easier problems if LQG does not reproduce GR. Perhaps no-go thereoms that show background-independent QG does not produce GR might be an easier problem everyone should work on! has lubos published to this effect?

“who” would you call SST/m-theory “qg”?

incidentally John, you once wrote about de-emphasizing QG in your research, has loops 05 changed that?

Physics Demographics

October 17, 2005
Categories: Uncategorized

To some extent, if one wants to understand some of the recent history of physics, one should take into account important demographic trends in the subject. For particle physics in the U.S., in recent years the Particle Data Group has been conducting an annual Census of U.S. Particle Physics. The American Institute of Physics has a collection of reports available on-line. The NSF and other various other organizations periodically issue hysterical reports about there being too few physics students getting Ph.D.s. For some perspective on this, in 2003 there were a bit more than 1100 physics Ph.D.s awarded in the U.S., and during 2001-2002, about 230 retirements per year of permanent faculty. Due to large recent increases in graduate student enrollment, the number of Ph.D.s is expected to increase significantly during the next few years. There doesn't seem to be much danger that anytime soon U.S. universities will see any change in the current situation of having vastly more qualified candidates for academic jobs than actual permanent jobs available.

In the specific case of particle theory, the Particle Data Group figures show roughly 450-500 tenured faculty, and 400-450 graduate students. So, the entire U.S. tenured particle theory professoriate could be just about replaced by one 4-5 year cohort of graduate students. The theoretical particle physics job market will remain extremely competitive for the forseeable future.

Unfortunately, the main hope for young physicists who want an academic job is that current tenured faculty are getting old and have to retire or die sooner or later. The latest data I've seen (from a 2000 AIP membership survey) indicated that the average age of tenured physics faculty had reached nearly 60. If anyone knows of more recent data I'd be interested to hear about it. I don't know of any good on-line sources for historical data, but the December 1995 issue of Physics Today had an interesting article about demographic trends in physics entitled "What future will we choose for physics?". The authors of that article claimed that before 1970 the median age of physics professors in the U.S. was relatively stable and under 40. In 1970, the number of physics Ph.D.s awarded hit an all time high of nearly 1600, and faculty hiring essentially fell off a cliff. According to the Physics Today article, from 1970 on the median age of tenured faculty increased linearly at the rate of about 8 months/year.

One effect of the aging of the physics community is that Physics Today has been running an increasing number of obituaries, since it has a long-running policy of printing a picture and several paragraphs about each of their members for whom obituaries are submitted. As of this month, facing the prospect of having to devote an increasing fraction of space to this purpose, they have abandoned this policy, announcing that from now on they will only publish obituaries in special cases, setting up a separate web-site for on-line obituaries, since these won't be appearing in the magazine itself.

Update: Andre Brown wrote in to point out that the 1995 Physics Today article is available on-line.

Comments

1. **blank**
   October 17, 2005

   I think the main hope for young theorists hoping to get an academic job, is some sort of natural disaster at the location of one of the big Strings or SUSY summer conferences.

2. **Andre**
   October 17, 2005

   "What future will we choose for physics?" is available online along with some other career related articles on the physics today website:

   http://www.physicstoday.org/pt/vol-54/iss-4/archive.html

3. **Chris Oakley**
   October 17, 2005

   These statistics are even more shocking than I expected.

   I am not saying that I have all the answers to these problems (not in detail, anyway), but it is useful to examine some of the significant events in our subject. Off the top of my head … Gell Mann discovered the eightfold way at age 23; Einstein, photoelectric effect & SR, aged 26; Heisenberg, matrix mechanics, aged 25; Dirac, the relativistic wave equation, aged 26. De Broglie, wave-particle duality at 24 (or thereabouts). Schrodinger, an old man at 38 – mind you, he was only following up on his young student de Broglie's work. <shameless plug> Also, my great uncle W J van Stockum, Axially Symmetric solutions of Einstein's equations – the work was done before he was 26 – it is still regularly cited &lt/shameless plug>

4. **Chuck**
   October 17, 2005

   And, yet Edward Witten just announced on PBS's show, Nova, that he did his best work in his 30's and 40's.

5. **SteveM**
   October 17, 2005

   "Science advances funeral by funeral."
   Max Planck

6. **blank**
   October 17, 2005

   Perhaps next time some billionaire philanthropist wants to help advance physics, instead of spending $100mil on some fancy new research center, he should offer $100mil in incentives for 50- and 60-something professors to retire early.

   Granted, it's not as sexy as having a building named after you.

7. **JC**
   October 17, 2005

   Is there a specific reason why physics funding fell off a cliff around 1970? From what I read over the years, it seems like the government decided to stop funding a lot of things in the early 70's, such as the Apollo missions.

8. **woit**
October 17, 2005

JC,

From what I remember it was a combination of a bad recession, putting an end to somewhat of a bubble in research and academic spending. Throughout the 50s and 60s the economy was expanding, and especially after Sputnik in 57 ever larger amounts were going into funding research and expanding the size and number of universities. When the recession hit, and state and federal spending had to be cut, the universities and research funding were among the hardest hit. Since universities had just expanded greatly in size and hired a lot of new tenured people, when all of a sudden they stopped expanding there were virtually no jobs, since the faculty was so young that very few older professors were retiring. This effect has continued for much of the period since 1970, only starting to change recently as many people hired during the 60s are finally starting to retire.

9. **A.J.**
October 17, 2005

Gell Mann discovered the eightfold way at age 23; Einstein, photoelectric effect & SR, aged 26; Heisenberg, matrix mechanics, aged 25; Dirac, the relativistic wave equation, aged 26. De Broglie, wave-particle duality at 24 (or thereabouts). Schrodinger, an old man at 38

Yeesh. What's the point of retelling this myth about young genius? That we should drive established thinkers out of academia? Or maybe that we should refuse to hire anyone who's done great work before the age of 30, on the grounds that their best years are behind them? 🙂

10. **D R Lunsford**
October 17, 2005

On a recent visit to 's physics department, I saw among the grad students and postdocs, not a single American face. Mostly Chinese, a smattering of Russians, Indians, and Pakistanis, a token German or two.

This is the direct result of the Faustian bargains struck among American deans and registrars, and those who desired wonder weapons. In the US, physics is done for power or profit, not for knowledge. Physics = weapons.

-drl

11. **weichi**
October 18, 2005

This is a bit off-topic, but being outside academia, the whole idea of tenure seems a bit odd. As I understand it, the idea is to give talented people the freedom to work in unfashionable/unpopular areas. This sounds great, but given human nature, how realistic is it to expect this to lead to improved results? Are there a lot of scientific results from, say, the past 50 years that were achieved by tenured professors, but would likely *not* have been achieved if tenure did not exist?

Maybe Andrew Wiles & Fermat might be an example, though that's not really science.

Perhaps my question boils down to this: I understand the benefit to professors of tenure. But what is the benefit to universities and to society at large?

12. **Doran**
October 18, 2005

There is some statistical correlation between ground-breaking work and how young a scholar is, though that is no reason to ship 40 & 50 year-olds off to the old folks home just yet. For those of us (myself only 21) still yet to turn 30, the young genius motif is even more frustrating because we only have those few years left before our brains turn to mush, though I imagine this isn't the care.

What is the most important characteristic of the giants in any scientific discipline is the ability to think differently. To head off the reservation and still keep your wits about you. Yes it may sound like a worn out platitude but it still makes sense. Just have to think outside of the ten dimensional box is all.

Being an undergrad in physics & history, I ended up being interested in the history of scientific theories and the many curiousities that abound in that study. Does anyone still do research along the lines of John Bell and hidden variable theories? I would really love to hear a physicist actually discuss this subject rather then snear at it for being "too much philosophy."

13. **Chris Oakley**
October 18, 2005

On the age issue, what I think can be changed is the emphasis. At the moment the most important thing is that a group of middle-aged and old men (the tenured professors) should have secure – if relatively poorly-paid – jobs. Demonstrably, this group is not the one most likely to be of most benefit to the subject. This is something that falls, in the vast majority of cases, to the under 30s. The emphasis could be changed so that the latter group is allowed more free rein. This would of course be at the expense of the old timers, but I simply do not accept that university professors are unemployable elsewhere. The bottom line is that they can always become schoolteachers (which would be some kind of poetic justice given how many of their graduate students will have had to find employment in this profession aged about 24).

Here is the kind of thing I would like to see:

(i) No tenure (at least not in research posts)
(ii) Research jobs/grants awarded on a 5 year rolling basis
(iii) Decisions about allocation of jobs and grant money made democratically, involving *all* engaged in research, i.e. from 2nd year graduate student upwards.

This is not ridiculously ageist anyway, as someone who continues to be a good researcher into their 30s and 40s is likely to continue to be re-elected to their research post.

The point is simply that, as history has shown, the young person's scepticism is the most valuable thing that science has. Instead of treating this as a dangerous threat (as happens at present), it ought to be harnessed and used in ways that actually advance the subject.

14. **Nigel**
October 18, 2005

With all due respect to them, a few words about the influence of Susskind, t'Hooft, and Hawking. Although Hawking radiation is undoubtedly a major contribution, Hawking has possibly made a couple of crackpot errors elsewhere. First, his application of Penrose's black hole math to the big bang, which seems to ignore the possibility that gravity might have a mechanism within the universe. Second, in 1976 he published 'The Breakdown of Predictability in Gravitational Collapse.'

This was taken seriously by Dr Leonard Susskind and Dr Gerard t'Hooft who were in the audience of Hawking's talk on the subject a few years later in San Francisco. Susskind is quoted in some BBC documentary saying that the lecture shocked him into believing that the Hawking 'breakdown of predictability' completely threw causality out of physics! Then in July 2004, Hawking announced he was wrong all the time…

15. **Thomas Larsson**
October 18, 2005

On young geniuses. Older people are usually better in normal science than youngsters because of their greater experience, but too much experience is a disadvantage when new thought patterns are necessary. Of course, people who did successful radical thinking in their youth can do great conventional science when they grow older. Just think of Dirac's wonderful work on constrained Hamiltonian systems, done at an age close to 60.

Witten is no counterexample. He was 33 when he, for better or worse, pulled the physics community through the first superstring revolution, and many others who jumped the string bandwagon were even younger. And most old-timers who did become string theorists, like Susskind, Veneziano and Gross, had once done dual resonance models or S-matrix theory, so they didn't really have to think in radically new ways. That Witten kept doing original mathematics for many years afterwards does not change this. He seems to have slowed down in recent years, though.

What 33-years-old today could, like Witten in 1984, change the direction of theoretical physics? Lubos Motl? 😉

16. **Chris Oakley**
October 18, 2005

Thomas,

We will perhaps never know what today's generation of youngsters engaged in particle physics research are capable of. At least, not as regards physics. Since the majority have been duped into believing that following the Superstring bandwagon is the only thing likely to be of any benefit to their careers, and since – on their own admission – this is still all light years away from actually *being* physics, we will never know what they might have achieved.

17. **Arun**
October 18, 2005

First, youthful creativity is most pronounced in mathematics and theoretical physics. This criterion should not be used in other areas where it is less true, for instance, great experimentalists have done crucial experiments at more advanced ages (and physics is primarily an experimental science, the discussion here shows that people say it a lot, but pay lip service to it, behaving as though Hawking and Witten and Einstein is all there is to physics).

Second, many of the breakthroughs wouldn't happen without the plodding work of actually dealing with messy reality. Who is going to do the actual work of measuring and building the enormous set of experimental results which are the bulwarks of theory?

Physics needs a balance of people – mess with that balance and you'll destroy the enterprise of physics.

18. **Not a Nobel Laureate**
October 18, 2005

It's certainly a truism that Physics is an experimental science. Pure reason will get you "proofs" like Hegel's that there can only logically exist seven planets.

However, given the how the number of HEP experiments has concentrated over the decades,
the usual suspects are still in charge.

Making bricks for the Pharoah, waist deep in mud and straw, is probably a better career choice.

19. **Dissident**
October 18, 2005

Doran, re. hidden variables, there's always this "old" guy:

http://www.phys.uu.nl/~thooft/

(scroll down to where he brings up "Fundamental aspects of quantum physics", check out the publications). His take is that both QM and GR will have to be modified in order to be reconciled. My own half cent's worth is that he's on to something. Penrose may or may not agree; his view seems to be more along the lines of QM, but not necessarily GR, being in need of revision.

Arun, " the actual work of measuring and building the enormous set of experimental results" is always going to be carried out by the same sort of people who've always done it: technicians, starving grad students and post-docs barely scraping by.

20. **Stephen**
October 18, 2005

Is anyone sure the supposed superiority of young physicists isn't just an example of [regression toward the mean]?

21. **Not a Nobel Laureate**
October 18, 2005

Always a pleasure to read about Physics

http://www.eurekalert.org/pub_releases/2005-10/jhu-neh101705.php

as an antidote to the Meta-physics of strings.

22. **Nigel**
October 18, 2005

QM and GR: the "stupid" physicist may be at an advantage in spending more time on this stuff, when brighter students are getting stuck into the delights of renormalised QED and ST.

"Dissident", are you sure that QM and GR need modification? Could be the maths is fine but GR can't be applied to cosmology (universe) because there is a gravity mechanism within the universe. This keeps GR maths intact, and perhaps makes the maths more clear physically.

As for QM and QFT, we know the fabric of space is filled with virtual particles. Normally particles like gas molecules can't carry transverse waves, only a solid normally allows transverse waves. But suppose the virtual particles have a spin, like real ones? Then you get transverse waves. Dr John Baez has some ideas on this for quantum gravity here http://math.ucr.edu/home/baez/loops05/ which seem to account for the light speed of gravity while preserving a pressure mechanism?

My understanding is that this is reinventing the wheel, since Maxwell's 1873 Treatise section 822-3:

"The … action of magnetism on polarised light [discovered by Faraday not Maxwell] leads … to the conclusion that in a medium … is something belonging to the mathematical class as an angular velocity … This … cannot be that of any portion of the medium of sensible dimensions rotating as a whole. We must therefore conceive the rotation to be that of very small portions of the medium, each rotating on its own axis… The displacements of the medium, during the propagation of light, will produce a disturbance of the vortices … We shall therefore assume that the variation of vortices caused by the displacement of the medium is subject to the same conditions which Helmholtz, in his great memoir on Vortex-motion, has shewn to regulate the variation of the vortices of a perfect fluid."

Am I alone in thinking we are going round in circles here?

23. **Quantoken**
October 19, 2005

Peter:

You described some observation of facts that we all know already. But what's your point? Are you saying it's a good thing or a bad thing? Are you complaining about something you don't like?

Yes it's going to be hard nowadays if a young man wants to get into an academic position and study physics. But is that a problem? I don't think there is any problem at all. We have a quite adequate number of physics professors teaching in universities, and actually, if anything, maybe a little bit too many of them, and if there is any shortage of professors, we can quickly replenish from freshly minted graduates. So what's the problem?

If a young man wants to become a physicist and he can't because the society doesn't need any more phycists than what it already has, then that's not a problem of the society, it's a problem of the young man himself. There are too many important things in the world that needs some young intelligent men to work on, so there could never be an over-abundance but shortage of the suply of real talents.

A while ago Mark Troden complained about the fact that not all of his students can get a faculty position, and he thought that's bad. I showed to him that if he educated just 12 students and each all becomes a faculty, and each of them also each educates another 12 students who all become physicists as they wished, so on and on. Then the simple math shows that the multiplication took less than 9 generations to exceed the earth's population. Such **exponential growth** is simply not possible.

So I told him a realistic expectation is he should expect at most one, and probably zero, of his students who could eventually become a faculty, and the rest evaporate, regardless how many students he put out, and regardless how hard his students work.

Quantoken

24. **Thomas Larsson**
October 19, 2005

QT, the physics community grew exponentially for almost century. In the US, the PhD production rate grew from about 1/year in 1870 to 1,000/year in 1970. It is unlikely that such a growth rate is sustainable for another century.

25. **Dissident**
October 19, 2005

QT, the problem is that physics professors are supposed to do two things: teach and research. As you point out, there is no major problem as far as the teaching goes. But if it is true that most breakthrough research is done by young people, the current situation with faculty averaging 60 means that breakthroughs are heavily disfavoured. This demographics is a a recipe for stagnation. And it does indeed seem to work pretty well…

26. **Somebody**
October 19, 2005

There is a saying that groundbreaking work seems to be mostly achieved by young physicists (say at age X), but on the other hand, if you look at any single, given physicist, his or her "best work" tends to be done at an age significantly greater than X.

It's population bias.

There are vastly more people in their 20s and early 30s working in particle physics than in their 40s and 50s, so it stands to reason that the former group produces a large fraction of the best work. On the other hand, they also produce a lot of not-so-great work, too, but those that do don't usually stick around into their 40s and 50s.

27. **Dissident**
October 19, 2005

Somebody wrote:
if you look at any single, given physicist, his or her "best work?? tends to be done at an age significantly greater

Maybe true of average physicists. Definitely not true of the true standouts like Newton, Maxwell, Einstein and the QM crowd.

28. **Paul Houle**
October 19, 2005

Senior physics think that a tight market for young physicists means they can skim the cream of talent. The reality might be the opposite: a bright young person who wants a career in science should choose any field BUT physics. Young people who stumble into graduate programs of physics today are the most clueless people about opportunities in science — the "gauntlet" doesn't select for the best people, but just for people who can take the most abuse.

29. **Dissident**
October 19, 2005

Paul, I remember an interesting piece of statistics from a few years back (maybe it's from the "What future" article?) showing that the fraction of top undergraduates in physics who choose to go on to grad school (in physics) has been declining since the 70s.

Nobody in the real world would be particularly surprised; it's a well known fact of business crises that the best people are the first ones to leave. They have the brains to see what's going to happen and they have the ability to find other lines of work. The last ones to leave are the clueless and those without alternatives.

30. **Doran**
October 19, 2005

Paul, pray tell how these physics undergrads are clueless of other opportunities in science, when we have been hearing about the horrible job market for academics in physics and watch our friends in biochemistry, engineering, and computer science pick up high paying jobs doing industry research for years. Also, why are departments still encouraging those who actually want to go to graduate school in physics despite more lucrative options in other fields.

Now physics is hard, and I can attest that the undergraduate curriculum does resemble a weeding out process, but thats how all the sciences at a top-level school are like. And who stick it out, those with the skills and the determination to master the material. Dissident, the first ones to jump ship are often those who came in thinking physics was easy and got a rude awaking.

I must admit my grades are not stellar, but I cannot help but feel a bit miffed by this. I believe most physics students who decide to go to graduate school are fully aware of the limbo that awaits them, but never-the-less are drawn to an intense and rewarding study of physics. To degrade that desire is only hurting the field, by dissuading anyone from even considering the subject to begin with.

I must agree with QT, that if one cannot find employment in academia, there are numerous other avenues that need intelligent technically trained individuals. My personal point is that maybe it would be better to encourage those interdisciplinary connections as well as the rigorous physics studies, rather then accusing the next generation of researchers as being adrift without a clue.

31. **Quantoken**
October 20, 2005

Doran asked: "why are departments still encouraging those who actually want to go to graduate school in physics despite more lucrative options in other fields"

Physics departments NEED to recruit and train large amount of graduate students, regardless whether the academy community needs that many new Ph.Ds or not. That's what university departments do and that's how they get fundings and survive at all. Can you imagine a Harvard physics department which no longer admit any graduate student for the next 10 years? Or can you imagine that Mark Troden trains just one student of his, instead of 12, knowing that the realistic expectation is at most one of his students can one day become a faculty? No he can't. Training students is part of Mark Troden's job that he gets paid for. So he will continue to train as many as he can, regardless of how his students will end up be.

It's like a fish would lay a couple million eggs at a time, most simply got eaten up by other fish and the realistic expectation is only one egg out of the millions will grow up to an egg-laying fish. Would a fish then lay less number of eggs so as not to be wasteful, or not to feed its enemies? No, a fish will still try to lay as many eggs as it can. Just like a physics professor will always try to train as many students as he can.

It's all Darwinism whether it's the nature or the human society.

Quantoken

32. **Thomas Larsson**
October 20, 2005

Chris,

Gell Mann did not discover the eightfold way at age 23. He was rather something like 33; I think he was born in 1927. Maybe you thought of the Gell-Mann and Low theorem from 1953.

33. **Chris Oakley**
October 20, 2005

Hi Thomas,

According to this:

http://www.answers.com/topic/murray-gell-mann

He was born in 1929 and invented the Strangeness quantum number (not the Eightfold Way as I originally said) at the age of 24.

Well, I did say that the information was off the top of my head.

Somebody called "Somebody" pointed out that the majority of people engaged in research are in fact quite young, i.e. PhD students and post-docs. This may be true but they definitely do not have the whip hand. Their relationship with the relics who actually run their departments is more like Apprentice/Jedi Master, and my point is simply that this is completely wrong, as the young person is the one who is far, far more likely to come up with the goods provided that he/she is given the space to do so. I have to be honest here – my D Phil (PhD) supervisor (between 1982 and 1984) was great about letting me do what I thought best. The problems only started to arise when I wanted to do post-doctoral work. At that point the old timers were able to demonstrate the consummate ease with which young, dissenting voices could be silenced.

34. **Dissident**
October 20, 2005

Sorry Doran, but you're looking at this from an undergraduate's perspective. The old guys contradicting you have been where you are now, and well beyond.

Sure there's a always a whole bunch of fresh undergrads going into physics, finding it too hard and "jumping ship". I don't think anyone (other than you) taking part in this discussion ever considered them. They really don't count. The people I and the other oldies here are writing about are those who do graduate; in particular, the top of the class.

I can not underscore enough what Quantoken already told you: physics departments run on grad students and post-docs. They are extremely cheap labour who do everything from teaching labs and correcting homework to doing the actual research which professors put their names on. This system is in essence the medieval one of apprenticeship. In modern terms, it's a system of exploitation, by the old of the young.

As if that weren't enough, funding also tends to be directly related to the number of students. If you can demonstrate a large demand for whatever it is that you teach, you will get more funding; if you can't fill all seats, you will see cuts and eventually be forced to fire faculty, maybe even shut down the whole department.

If you understand all this and still want to try for a "career" in academia, you have admirable guts and may even deserve to be cheered on. But do not delude yourself about what you're up against.

35. **weichi**
October 20, 2005

There seems to be a subtext here that physics is a particularly poor choice for people who want to go into science. But are job prospects in other fields of science really that much better?

My guess is that in academia the situation is no better, but there are more opportunities in industry for other fields – biosciences in particular, perhaps also for chemistry? Does anyone have any numbers for other fields?

We should also keep in mind that for certain fields of physics, there will never be job opportunities in industry. My understanding is that industry hires condensed matter experimentalists to do work that is at least similar to academic research in condensed matter, but I'm sure that no one gets hired by industry to do theoretical research that is similar to string theory. So choice of field within physics also has an effect, perhaps a large one.

36. **D R Lunsford**
October 20, 2005

About age: I suspect every physicist has (at least) one good idea, and when this shows up is not a function of age, rather insight (a combination of experience, technique, and intuition). And, one may have a good idea, but not realize for years. Certainly intuition is highest when inexperienced, because you don't know what you can't know!

So it's more like a Heaviside function, theta(X – age) where X is when you have your idea, and age could be anything between diapers and senility.

-drl

37. **Not a Nobel Laureate**
October 25, 2005

Anecdotally speaking.

It's unfortunate that the culture in academic physics is such that any other path than the academic one leading to a professorhips is seen as failure when the reality is that that most graduates will go on to do something else – the few perceptive ones by choice, the rest by default – often with considerable, but pointless, bitternes.

I've met many exceptionally bright people outside of academic physics – sorry to say, but these days the best and the brightest are no longer in HEP. HEP today "reads better than it lives".

38. **Eli Rabett**
October 25, 2005

A bit late, but a short comment on why the job market in physics dropped dead in the early 70s:

1. The costs of the Vietnam war, amplified by a recession limited the Federal budget for research

2. Universities that had been expanding like gangbusters to meet the demands of the baby boom, found there was a baby bust.

3. New universities established to meet the growth in enrollment (e.g. UMass Boston as one example) had hired complete departments of relatively young faculty in the mid-sixties, but then hired no one else for thirty years. This cohort is now retiring.

From one who was a bit late.

Hiding in the Mirror

October 18, 2005
Categories: Book Reviews

I've just finished reading Lawrence Krauss's new book Hiding in the Mirror: The Mysterious Allure of Extra Dimensions, from Plato to String Theory and Beyond, and it's very, very good. Scientifically, the book covers a lot of the same material as Lisa Randall's Warped Passages, but it's about half as long and has a wider perspective, with writing that is pithy and entertaining. Krauss's topic is not just the science of extra dimensions, but the history of various ways the idea has turned up in art and literature, and the whole question of why people find it so fascinating.

He begins by telling the story of an episode of the *Twilight Zone* TV program that had quite an impact on him when he was very young. It involved a little girl who falls into another dimension and is saved by intervention of a physicist. Krauss notes that "We all yearn to discover new realities hidden just out of sight", but that "Ultimately our continuing intellectual fascination with extra dimensions may tell us more about our own human nature than it does about the universe itself." He writes about a wide range of different writers and artists who have been fascinated by the idea of extra dimensions, and some of the historical and cultural context for their work. Much of this I didn't know anything about, although his description of the science fiction short story "And He Built a Crooked House" by Robert Heinlein brought back memories of my childhood, since I had found that story very striking, but hadn't thought about it in a very long time (it involves a house based on a tessaract, a 4d version of a cube). Another interesting piece of history he unearths is that Marcel Duchamp's famous piece *The Bride Stripped Bare by Her Bachelors, Even* (also known as the *Large Glass*), was heavily influenced by ideas about projecting from four dimensions, and that Duchamp spent a lot of time trying to learn about this, including reading Poincare.

Krauss writes that, while fascinated by the idea, he himself remains a skeptic (or at least agnostic) about the actual existence of physical extra dimensions. He tells the history of attempts by theorists to use extra dimensions, from 19th century conjectures that atoms were points where a four-dimensional etherlike field leaked into three-dimensional space, to Kaluza-Klein models and the heterotic string, ending up with recent braneworld scenarios. He describes the ideas behind this research concisely, and also explains exactly what some of the problems with these ideas are. Along the way he comes up with various obscure and interesting pieces of the history of physics I'd never heard before, for instance that in 1928 an English experimentalist named R. T. Cox found evidence of parity violation, but his results were not taken seriously.

On the topic of string theory and braneworlds, Krauss promises to be not like Fox News (i.e. actually "Fair and Balanced"), but he has truly scathing things to say:

But in the ever-optimistic string worldview, there are no embarassments… For these 'true believers', every new development provides an opportunity to confirm one's expectations that these ideas ultimately reflect reality.

… string theory might instead do for observational cosmology what it has thus far done for experimental elementary particle physics: namely, nothing.

In short, the as-of-yet hypothetical world of hidden extra dimensions had, for many who called themselved physicists, ultimately become more compelling than the world of our experience.

This embarassment is solved in the way other similar confusing aspects of string theory and M-theory are sometimes dealt with: Namely, it is assumed that when we fully understand the ultimate theory, everything will become clear.

Over the past five years, hundreds if not thousands, of scientific papers have been written considering cosmological possibilities that might be associated with Braneworld scenarios. One cannot do justice to all of them, but the greatest justice I could probably do to many of them is to not mention them here.

What the notion of large or possibly infinite extra dimensions has done is borrow some of the facets of string theory while ignoring the bulk of the theory (forgive the pun), about which, as I have explained, we have only the vaguest notions. It seems to me to be a very big long shot that an apparently ad hoc choice of what to keep and what to ignore will capture the essential physics of our universe.

This has resulted in yet another fascinating sociological metamorphosis of the theory, with warts becoming beauty marks.

… the anthropic principle is something that physicists play around with when they don't have any fundamental theory to work with, and they drop it like a hot potato if they find one.

This finally brings up back to M-theory. Faced with the prospect that the theory may ultimately predict a virtually uncountable set of possible universes, some string theorists did a 180-degree about-face. Instead of heralding a unique Theory of Everything that could produce calculable predictions, they are now resorting to what even a decade ago they may have called the last refuge of scoundrels. But, when string theorists take a position, they do it with flair.

…if the landscape turns out to be the main physical implication of the grand edifice of string theory or M-theory… we might be left with the mere suggestion that anything goes. What was touted twenty years ago as a Theory of Everything would then instead have turned quite literally into a Theory of Nothing.

Krauss ends his book with an epilogue describing conversations with Gross, Wilczek and Witten about string theory. Wilczek is a skeptic, annoyed by the excessive claims made for the theory. Witten is quoted as saying that string theory "is a remarkably simple way of getting a rough draft of particle physics unified with gravity. There are, however, uncomfortably many ways to reach such a rough draft, and it is frustratingly difficult to get a second draft." He justifies work on string theory partly through progress it has led to in the understanding of strongly coupled gauge theories.

Gross is described as convinced "that the theory is simply too beautiful *not* to be true", an attitude that strikes Krauss "as sounding like religion more than science." With this, Krauss ends his book by quoting Hermann Weyl:

My work always tried to unite the true and the beautiful, but when I had to choose one or the other, I usually chose the beautiful.

and concludes:

So it is that mathematicians, poets, writers, and artists almost always choose beauty over truth. Scientists, alas, do not have this luxury, and can only hope that we do not have to make this choice.

Here, to some extent I part ways with Krauss. As I've explained elsewhere, I don't find the 10 dimensional heterotic superstring compactified on a Calabi-Yau to be in any sense beautiful, and attempts to connect string theory with physics lead to appallingly ugly constructions, strong evidence that they are on the wrong track. Absent useful experimental results, the pursuit of compelling new mathematically beautiful insights into fundamental physics is one of the few promising ways forward. But to go down this road successfully you have to be honest about what is mathematically beautiful and what isn't.

All in all, this is by far the best book that I know of on the topic of recent speculative work on fundamental particle physics, and I strongly recommend that anyone who enjoys reading about this should get themselves a copy.

Update: An interview with Krauss about the book just appeared in the Cleveland Plain Dealer.

Update: It was pointed out to me that the way I compared Krauss's book to Randall's here wasn't really fair to hers since she was trying to do something different, so for non-specialist readers, the books have different functions. I submitted this review to Amazon, and edited it a bit so that it would be more appropriate for people looking for a comparison of the two books. The main change was the addition of the following paragraph:

"While they are ultimately concerned with the same speculative ideas about extra dimensions, Krauss and Randall's books are in many ways different. Randall is writing about her own research work, so on the one hand she is a partisan for these ideas, on the other she gets to tell the inside story of exactly how she came up with them. She goes to a lot of trouble to dig in and try and explain in as simple terms as possible the details of the physics that motivates this research, as well as exactly what it is trying to achieve, how it has evolved in recent years and where it seems to be going. Krauss also covers these topics, but is (justifiably in my view) more of a skeptic, and sets the whole story in a wider context of the long history of this kind of speculation. If you've read Randall's book, you should seriously consider reading Krauss for a different point of view. If you read Krauss and want a much more extended exposition on some of these topics, Randall is the place to go."

Comments

1. **MathPhys**
   October 18, 2005

   "I don't find the 10 dimensional heterotic superstring compactified on a Calabi-Yau to be in any sense beautiful."

   I've read the above statement many times on this blog, but now it strikes me that I agree with you. The whole structure, looked at from a distance, seems unnatural and contrived: You start in 10 dimensions with lots of symmetries, then you have to get rid of 6 dimensions and most of the symmetries.

   On the other hand, starting with a type II string that has no internal symmetries in 10 dimensions, and obtaining these at the expense of the extra dimensions looks better. At this moment.

2. **Lubos Motl**
   October 18, 2005

   Dear Peter,

   are you serious that this book covers "very much the same material" as the Warped Passages? I find such a statement incredible. First of all, Krauss understands physics of extra dimensions roughly as well as you do, he's never worked or thought about them seriously, so it is physically impossible for him to write anything meaningful about them. Maybe you wanted to say that it tries to cover very much the same material as Abbott's Flatland?

   Best wishes
   Lubos

3. **woit**
   October 18, 2005

   Hi Lubos,

   Yes, I'm very serious that the book covers the same material as Warped Passages, but does a better job. I was going to put something in the posting about how string theorists' reaction to the book would be to denounce Krauss as incompetent, even before they read it…

4. **blank**
   October 18, 2005

   "… they are now resorting to what even a decade ago they may have called the last refuge of scoundrels."

   That sums it up pretty well. Particle theory over the past decade has been marked not by objective progress, but rather receeding standards.

5. **Anonymous**
   October 18, 2005

   The piling up of symmetries and dimensions that one has to then discard to match the real world resembles the cycles upon epicycles needed in the Ptolemaic theory of the solar system, 'cause circular motion was sacrosanct. Oh, for a Kepler!

6. **Tony Smith**
   October 18, 2005

   Peter, as you and Krauss noted, "… Marcel Duchamp's famous piece The Bride Stripped Bare by Her Bachelors, Even (also known as the Large Glass), was heavily influenced by ideas about projecting from four dimensions …". Here are some (in my opinion) relevant quotes from Duchamp, as found in the book Dialogues with Marcel Duchamp, 1966 interview with Pierre Cabanne, Plenum (Da Capo) (1971, 1987):

   "… I never was the scientific type. … What we were interested in at the time was the fourth dimension. … Povolowski … was a publisher, in the rue Bonaparte. … He had written some article in a magazine popularizing the fourth dimension, to explain that there were flat beings who have only two dimensions, etc. … That was working in my head while I worked, although I almost never put any calculations into the "Large Glass". Simply, I thought of the idea of a projection, of an invisible fourth dimension, something you couldn't see with your eyes. … I thought that … the fourth dimension could project an object of three dimensions, or, to put it another way, any three-dimensional object, which we see dispassionately, is a projection of something four-dimensional, something we're not familiar with. It was a bit of sophism, but still it was possible. "The Bride" in the "Large Glass" was based on this, as it were the projection of a four-dimensional object. … Only the "Large Glass" interested me, … I wanted to be free of any material obligation, so I began a career as a librarian, which was a sort of excuse for not being obliged to show up socially. … I … went to take courses at the School of Paleography and Librarianship. … I knew very well that I would never be able to pass the examination at the school, but I went there as a matter of form. It was a sort of grip on an intellectual position, against the manual servitude of the artist. At the same time, I was doing my calculations for the "Large Glass". … The "Large Glass" constitutes a rehabilitation of perspective, which had then been completely ignored and disparaged. For me, perspective became absolutely scientific. … It's a mathematical, scientific perspective. …".

   Could the "… completely ignored and disparaged …" perspective in art correspond, in today's world of physics, to the 1970s Standard Model plus Gravity view of physics based on detailed contact with experimental results ?

   Has the superstring community completely ignored and disparaged the building of physics models based on detailed contact with experimental results ?

   Does today's world of physics need a new Duchamp to rehabilitate the theoretical physics by building models based on detailed contact with experimental results from a

"… mathematical, scientific perspective …" ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **steve**
October 18, 2005

My only problem with Krauss was in one of his popular books on physics in science fiction. He claimed that the giant flying saucers in the movie Independence Day wouldn't have been able to levitate over the Earth without flattening everything beneath. I have no problem with F=MA, but hasn't he heard about neutrino drives? All the propulsive efficiency with none of that annoying matter interaction! (He also didn't stop to consider that the saucers could be made of single-molecule-ply superstrong materials with ultra-high strength to mass, but I wouldn't expect a particle guy to think about advanced materials.)

8. **D R Lunsford**
October 18, 2005

Most talk of extra dimensions is idle speculation, because the spine of physics, irreducibility, is optional.

-drl

9. **Aaron**
October 18, 2005

"string theorists"

Be careful with those generalizations, please.

10. **woit**
October 18, 2005

Hi Aaron,

Point taken, I should have mentioned that I was thinking of the reaction of certain string theorists, not all of them. I'm certainly curious to see how string theorists in general will react to Krauss's book. Will they respond to it with the seriousness it deserves, or like Lubos, just attack the author as not knowing what he is talking about? So far the count is 0-1. But the book isn't even quite in stores yet….

11. **Tung**
October 19, 2005

RT Cox, as mentioned here, had done important work on the foundation of probability theory, which has become the cornerstone of the view that probability is a kind of generalisation of aristotelian logic.

12. **Quantoken**
October 19, 2005

Lubos said: "…so it is **physically** impossible for him to write anything meaningful about them"

Absolutely correct! It is **physically** impossible for **ANYONE** to say anything meaningful about extra dimentions. Because extra dimentions are simply imaginative and none-physical. How could any one say anything physical about something that's pure imaginative? There is not the slightest evidence that any extra dimention exists in nature.

And I object to Peter using the term "science of extra dimentions". There is no "**science**" of extra dimentions. Because extra dimentions are simply not qualified to be scientific. You can say "research of extra dimentions", but it's not science. Not all research activities are science.

Quantoken

13. **Shantanu**
October 19, 2005

FWIW, Spires hepnames page indicates that Lawrence Krauss was the thesis
advisor of Raman Sundrum (of Randall/Sundrum)
See http://www.slac.stanford.edu/spires/hepnames/

14. **Richard**
October 19, 2005

So, Lubos, it is not possible to say meaningful things about things you are not professionally working on? Oh. So tell me, which is your latest paper about global warming?

15. **The Anti-Lubos**
October 19, 2005

I've asked it before, but I'll ask it again.

Does anyone like him? And if so, how and why?

16. **plato**
October 19, 2005

I for one, find this a very important blog entry from the perspective of responsibility, in explaining these extra dimensions.

I sense this in Peter's blog entry, and of course, the underlying skepticism about this is still no secret. There have been attempts to define this issue much clearer and directly, in experimental fashion.

So it is not without some historical implications that early thoughts could have been exceeded, to relate this issue, and try to make sense of it. It is still a responsible function that we recognize as necessary.

The artistic implicatons of "cubism" is one I found related to discrete measures, and to see this developing aspect in relation to science, is no less important as we engage how such extra dimensions might be seen as a continuity in topological form?

Would this be incorrect?

17. **Wolfgang**
October 19, 2005

Anti-Lubos,

> I've asked it before, but I'll ask it again.
> Does anyone like him? And if so, how and why?

Are you saying you do not like his songs ?
http://schwinger.harvard.edu/~motl/sf/frames.html

PS: I like Lubos.

18. **plato**
October 19, 2005

Dissident:*If you perform an experiment in which some of the energy you put in seems to disappear somewhere, unaccounted for, then yes, you have some explaining to do. Conservation of energy is not something we'd give up lightly; rewriting all those textbooks would be exhausting… but large extra dimensions would certainly not top the list of things to consider.*

First of all, "missing energy?? is a normal feature of collider experiments, since you can't expect to catch all the stuff that comes out of them. You have two particle beams banging into each other inside a tunnel of finite width; any decay products flying off into the tunnel are lost. Around the collision point, you have detectors which, while huge and most impressive, also have blind angles and – most importantly – finite size.

http://cosmicvariance.com/2005/09/05/dark-matter-and-extra-dimensional-modifications-of-gravity/#comment-4160

19. **Dan**
October 19, 2005

Quantoken,

your extra-dementia clearly determines your opinion about extra-dimentions. How's your GUITAR?

20. **Quantoken**
October 19, 2005

Dan:
I am making pretty good progress in GUITAR but I have decided not to talk about it until I can figure out the whole thing in one piece.
Privately I wish large extra dimentions exist. It will make life much easier. Surgical incisions will not be needed for a doctor to operate on a patient's internal organs, for example. They can be reached through extra dimentions, without having to cut up the patient's chest. But there may be other inconveniences. Prison inmates may escape through extra dimentions without having to dig a secret tunnel. And compressed air within a tire could leak out through extra dimentions, even though the surface of the tire is perfectly tight.
Super String Theorists could save their time and energy in their invain effort to search for extra dimentions, by humbly begging the teaching of magicians, who are the only people who have figured extra dimentions out already. For there is no other rational explanation how they managed to cross one metal ring into another one and then remove them freely, without breaking the rings to do that. The topology simply does not allow that unless there is extra dimentions 🙂
Of course, that is not science.

Quantoken

21. **ksh95**
October 19, 2005

Quantoken said:

For there is no other rational explanation how they managed to cross one metal ring into another one and then remove them freely, without breaking the rings to do that. The topology simply does not allow that unless there is extra dimentions.

Hmmm, that's not obvious to me. It's obvious that you could escape from prison through extra dimensions, but I'm not sure you could pull rings apart in any dimension.

I'm sure some one here has some insight into this.

22. **D R Lunsford**
October 20, 2005

IIRC knots are only possible in 3d (Klein and others).

-drl

23. **ks**
October 20, 2005

Maybe L. Krauss and Peters books will be the last critical examinations of ST by ST outsiders before dropout literature enters the market? Until than ST does not have to harm about an "objective enemy" (Stalin). By the way I find Lubos attitude towards Krauss somehow strange for a person who criticises climate research as a well informed ousider. If sectarianism or closed society counts so much one may ask for the amount of scientific papers he published in this area.

24. **Thomas Larsson**

October 20, 2005

Hmmm, that's not obvious to me. It's obvious that you could escape from prison through extra dimensions, but I'm not sure you could pull rings apart in any dimension.

Manifolds of dimension m and n embedded in D-dimensional space generically intersect along an (m+n-D)-dimensional manifold (i.e. codimensions add). They are knotted if m+n-D = -1. E.g., two curves (m=n=1) are knotted in D=3 dimensions, two 2D surfaces are knotted in D=5 embedding dimensions but intersect along a 1D curve in D=3, etc.

25. **Bryan**
October 20, 2005

'Maybe L. Krauss and Peters books will be the last critical examinations of ST by ST outsiders…' KS

I disagree, because when details come out next March there may be a Congressional inquiry into what went wrong with ST…

26. **Kris**
October 20, 2005

Brian, what are you expecting in March? Seems you have something specific in mind…

27. **Bryan**
October 21, 2005

Kris: was thinking of the publication of a book about ST being "Not Even Wrong". Think it might stir up trouble!

28. **Juan R.**
October 21, 2005

ks Said:

By the way I find Lubos attitude towards Krauss somehow strange for a person who criticises climate research as a well informed ousider. If sectarianism or closed society counts so much one may ask for the amount of scientific papers he published in this area.

Well, perhaps, you forget that many particle physicists, and by extension string theorists, believe that they can talk about everything since are -they believe- more intelligent that other guys.

Newer have you heard abut fist class and second class scientists? Unfortunately some physicists still believe that a chemist is a kind of 'second-class physicist'.

This would imply -of course, it is a completely nonsense- that a physicist 'can' talk about physics, chemistry, biology, environment whereas chemists or biologists cannot talk about physics.

Some time ago, when i was in the *Colegio official de químicos de Galicia*, an 'iluminated' group of physicists proposed that, in Spain, physicists could gave courses of both physics AND chemistry in Schools but chemists can only gave courses on chemistry!

Of course, was a nonsense and the corresponding law newer was approved…

When people like Gross, Witten, Greene, Weinberg, Anderson, or even Gell-Mann talk about chemistry only say either irrelevant/wrong things or outdated stuff.

Juan R.

Center for CANONICAL |SCIENCE)

29. **Bryan**
October 21, 2005

http://www.indiadaily.com/editorial/5061.asp

The article above reports the discovery of extra dimensions. Is this premature, like cold fusion, or real. It mentions that the extra dimensions are really big so they will enable us to do the things Quantoken suggests, like operations without any need to cut skin or tissue at all, and it may account for punctures in tyres where you can't immediately see a hole. 😉

30. **Nigel**
October 21, 2005

Juan – I think Lubos is not the bad guy really, he has just got mixed up with the wrong crowd. The only reason he asserts views on every aspect of everything is that he is qualified to do this, since string theory – his expertise area – is a 'theory of everything'!

31. **Arun**
October 22, 2005

Now that I have Roger Penrose's "Road to Reality" in hand, I see that he has a plethora of objections to string theory. Are there available any coherent replies to Penrose's doubts?

32. **Wolfgang**
October 22, 2005

Arun,

Lubos Motl stated that the objections of Roger Penrose are "obvious nonsense", but from what he wrote it is clear that he never read the relevant parts of the book.

Some of the doubts have been expressed also by Lee Smolin and Peter (e.g the question if string theory is indeed finite and consistent) and have provoked some responses from string theorists.

I am not aware that the main argument of Penrose (M4xCY is unstable against perturbations, in particular in a classical approximation) has ever been seriously discussed or shown to be incorrect.

I am personally not too impressed by his argument(s), but I cannot say if his classical approximation is relevant or not.
Also, I am not convinced of his discussion of quantum theory in general.

33. **Arun**
October 23, 2005

Wolfgang,

Motl's remarks don't help me, they don't count as a coherent reply. Obviously, there is plenty of room for disagreement with Penrose.

What I'd like is the following type analysis, hopefully that addresses the same audience that Penrose is addressing; or if not, to Penrose himself, and he can tell us if he is adequately answered.

1. For some objections, Penrose is making an unwarranted assumption about the nature of reality, or he has utter misunderstood string theory.

2. For some objections, Penrose is on the mark, and this is an area where string theorists have to do further research to meet his challenge.

3. For some objections, there is a good physical argument, even if no mathematical proof, that resolves Penrose's objection.

34. **Steve Myers**
October 24, 2005

It's long been known that adding dimensions drives volume to the surface (it's called "The Curse of Dimensions"). So how does that affect a field?
Also remember you can always get a symmetry by adding a dimension — but there's no reason it is physically real.

35. **bryan2**
October 24, 2005

Thomas Larsson Said:
"Manifolds of dimension m and n embedded in D-dimensional space generically intersect along an (m+n-D)-dimensional manifold (i.e. codimensions add). They are knotted if m+n-D = -1. E.g., two curves (m=n=1) are knotted in D=3 dimensions, two 2D surfaces are knotted in D=5 embedding dimensions but intersect along a 1D curve in D=3, etc."

An easy way to visualize this is the case where m=n=0, and D=1. m and n are each 2 points separated by a fixed distance. They are knotted when a point from one is in between the point of the other. When D=2, m and n are no longer knotted since they have another direction to move in.

36. **Juan R.**
October 26, 2005

Nigel note i do not said that Lubos or other string theorists were "bad guys". I simply said that are a bit confounded.

Yes they believe that are working in the TOE, but this is false.

See my recent post on sci.physics.research.

I wait a hot debate on this

Juan R.

Center for CANONICAL |SCIENCE)

37. **Juan R.**
October 31, 2005

Sorry, i made a typo.

Instead of

"See my recent post on sci.physics.research."

would read

"See my recent post on **sci.physics.strings**."

Juan R.

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Deligne Conference

October 20, 2005
Categories: Uncategorized

Yesterday I went down to the Institute in Princeton with my friend Oisin McGuinness to attend one day of a conference in honor of Pierre Deligne that is going on there this week. Deligne has spent most of his career at the IHES and at the Institute, and this conference was in honor of his 61st birthday (I suspect they initially planned it for last year, but it got pushed back).

Deligne worked with Grothendieck at the IHES during the late sixties, and is perhaps best known for his proof of the Weil conjectures completed in 1974, an achievement which won him a Fields medal in 1978. The Weil conjectures motivated much of the work by Grothendieck and others in algebraic geometry during the fifties and sixties, and Deligne was able to finish a proof using Grothendieck's machinery as well as some different ideas of his own. For more about Grothendieck, visit the Grothendieck Circle web-site. Grothendieck left the IHES around 1970, and later became a recluse, increasingly hostile towards his former colleagues, especially Deligne, who he attacked in his long unpublished manuscript "Recoltes et Semailles." Several people told me that at the conference banquet held Tuesday night, after an array of different speakers rose to praise Deligne, especially for his generosity with his ideas and help to others, Deligne himself spoke and said that he was only repaying the debt he owed to Grothendieck, who himself was famous for such generosity.

One of the conference talks I heard was by Gerard Laumon, who described some of his work with Ngo Bau Chau on the Fundamental Lemma. Various lecture notes on this subject are available here. Laumon emphasized the role of equivariant cohomology in the proof, a method pioneered by Goresky, Kottwitz and MacPherson. Equivariant cohomology techniques are also crucially behind much work on topological quantum field theory, although of course the context is quite different there.

Comments

1. **Dave Bacon**
   October 21, 2005

   "Several people told me that at the conference banquet held Tuesday night, after an array of different speakers rose to praise Deligne, especially for his generosity with his ideas and help to others, Deligne himself spoke and said that he was only repaying the debt he owed to Grothendieck, who himself was famous for such generosity."

   Wow. That is very touching.

2. **Tom**
   October 21, 2005

   Thanks for reporting and for the lecture notes. Do you know by any chance if they made some videos of the talks ?

3. **MathPhys**
   October 21, 2005

   Grothendieck was very generous with his ideas (and 1000's of pages of published manuscripts) up to his breakdown in the late 60's and break up with the mathematical community. Things changed after that point, as he became increasingly bitter, and started accusing people of stealing his ideas and/or not giving him credit. Some of the things he said in his latter (autobiographical)writings were mean and paranoid.

4. **Geon Oh**
   October 21, 2005

   Hi there,
   I am an undergraduate physics student in New Zealand.
   I'm not sure if this is right place to post this but I was wondering if you 'expert' physicists could have a look around my weblog and if there are misunderstandings in the articles that I write, to correct me. I'm only a junior in University so I have a lot more to learn and I think your comments or criticism of feedback will help me learn A LOT more. Thanks!
   http://precondition.blogspot.com

5. **Geon Oh**
   October 21, 2005

   criticism of feeback ==> criticism or feedback
   typo sorry : D

6. **Prime**
   October 21, 2005

   Actually, the 61st birthday was celebrated as opposed to the 60th because Pierre likes prime numbers..

7. **Wolfgang**
   October 21, 2005

   Geon Oh,

   I tried to comment on your blog. but it is restricted to team members.

8. **Geon Oh**
   October 21, 2005

   Hi Wolfgang
   Thanks for your comment
   I corrected the setting so that anyone can make comments.
   I appreciate your interest in my 'newbie' weblog.
   Thanks!
   http://precondition.blogspot.com

Wilczek on Weyl

October 21, 2005
Categories: Uncategorized

The latest issue of Nature has an essay on Hermann Weyl by Frank Wilczek. The essay mainly advertises Weyl's book *Philosophy of Mathematics and Natural Science*, originally published in 1926, but updated for the English translation in 1949. I'm embarassed to say I've never read this, despite my fascination with Weyl, so I guess I better go out and get ahold of a copy.

Comments

1. **plato**
   October 21, 2005

   "*The question for the ultimate foundations and the ultimate meaning of mathematics remains open; we do not know in which direction it will find its final solution nor even whether a final objective answer can be expected at all. "**Mathematizing" may well be a creative activity of man, like language or music, of primary originality, whose historical decisions defy complete objective rationalization***." — Hermann Weyl (Gesammelte Abhandlungen)

   http://en.wikipedia.org/wiki/Hermann_Weyl

2. **D R Lunsford**
   October 21, 2005

   I would sure like to see that article.

   -drl

3. **D R Lunsford**
   October 21, 2005

   In honor of Weyl:

   What is the analog in distant parallelism, of Weyl's ansatz in distant measure (Riemannian geometry)? Has anybody investigated? A first guess is

   L(A) g_mn = Am An

   where L(A) is the Lie derivative. It looks to me like "distant parallelism" gets changed to "distant parallelism with respect to an arbitrary quadric" (just a guess, haven't investigated yet). It would be very interesting if the covariant derivative in this looked like dm + i Am.

   -drl

4. **biophysics**
   October 23, 2005

   Peter, I've seen a couple references to Nature on the site – how much of the journal do you manage to read? As a theorist in biophysics I actually feel like I have to get through most of Nature and Nature Genetics in addition to the physics journals (mostly PRL and PRE and, most of all, arxiv; of course I still indulge my particle hobby and glance at PRD and hep-th)..but I always wondered how many people (especially in the particle community) read Nature.

5. **woit**
   October 23, 2005

   I confess that my reading of Nature has always been kind of like just reading the cartoons in the New Yorker, i.e. I ignore the research articles (except on very rare occasions), and just skim the news items and things like book reviews and essays. Few technical articles in the fields I really know about (math and particle physics) ever appear in Nature, and when they do, they've often been placed there by people who are more interested in getting their names in the popular press than in communicating with their fellow physicists or mathematicians (who generally don't read Nature). Maybe "Nature Physics" will be different.

   Until a few months ago I used to skim copies of Nature when I saw them appear down in the library. More recently I tend to look at the online version.

6. **Anonymous**
   October 25, 2005

   Nature 437, 1095 (20 October 2005) | doi: 10.1038/4371095a
   An explorer and surveyor

   Frank Wilczek1

   1. Frank Wilczek is at the Center for Theoretical Physics, Massachusetts Institute of Technology, Cambridge, Massachusetts, 02142, USA.

   Top of page
   Abstract

   Hermann Weyl made prescient contributions to both mathematics and physics, but also strove to understand reality as a whole.

   Hermann Weyl was, according to Fields medallist Michael Atiyah, "one of the greatest mathematicians of the first half of the twentieth century". Every great mathematician is great in their own way, but Weyl's way was particularly special. Unlike most modern scientists who choose one or a few specific areas to explore and look neither sideways nor back, Weyl surveyed the whole world. He sought truth and beauty with a discriminating and far-seeing eye.

   Weyl's most unique work is Philosophy of Mathematics and Natural Science. No one else could have written it, and no other book I know is like it. I have consulted it many times, and each time I've come away enriched. My main purpose here is to direct readers to that book. But since space remains, let me add some background information.

Weyl, who died in 1955 at the age of 70, was a student of David Hilbert at the Georg-August University in Göttingen, Germany, and thus stood in the line of intellectual descent from Carl Gauss, Bernhard Riemann and Lejeune Dirichlet. Upon Hilbert's retirement, Weyl was invited to take up his chair, but conditions in 1930s Germany and an attractive offer from the new Institute for Advanced Study conspired to bring him to Princeton, where he stayed. With Albert Einstein and John von Neumann, Weyl made the trinity of refugee stars that gave the institute its inimitable scientific lustre.

Einstein and von Neumann had both grown up in the grand German literary and pan-European cultural tradition that was rocked and then shattered by the two world wars. But more than the rebellious Einstein or the protean von Neumann, Weyl embodied that tradition, and his Philosophy of Mathematics and Natural Science reflects it in focus, style and erudition.

The main body of text was written in German in 1926, as an article for R. Oldenburg's Handbuch der Philosophie. In 1949, for the English translation, Weyl altered many details and added seven appendices, comprising almost a hundred pages. These centre on relevant scientific events in the intervening years — his passages on Gödel's theorem, and on quantum mechanics and causality are especially brilliant. But the core of the book had its genesis in the vanished handbook tradition of magisterial reviews in natural philosophy.
Unfortunately we are unable to provide accessible alternative text for this. If you require assistance to access this image, or to obtain a text description, please contact npg@nature.com

ARCHIVES OF THE INST. OF ADVANCED STUDY

Hermann Weyl touched on questions that are relevant today.

In the introduction, Weyl declares the roots of his literary style: "I was bound by the German literary and philosophical tradition in which I grew up." As an example of that style, let me quote a passage that I think ranks among the most beautiful and profound passages in all literature: "The objective world simply is, it does not happen. Only to the gaze of my consciousness, crawling along the lifeline of my body, does a section of this world come to life as a fleeting image in space which continuously changes in time."

Weyl's work is also remarkable for its erudition. René Descartes, Gottfried Leibniz, David Hume and Immanuel Kant enter into the discussion as familiar friends. From time to time important literary or philosophical passages are quoted in the original French, German, or even Greek. Weyl's erudition is not vain display — he is far above that — but a touching assumption of shared culture. Of course the reader will feel at home in this milieu, he seems to assume.

In his biographical memoir of Weyl, Atiyah also talked about Weyl's work in mathematics, saying: "The last 50 years have seen a remarkable blossoming of just those areas that Weyl initiated. In retrospect, one might almost say that Weyl defined the agenda and provided the proper framework for what followed." Weyl's contributions in physics were more sporadic, but they were significant, and likewise remarkably prescient. In particular, Weyl proposed, and named, the concept of 'gauge invariance', which came to dominate fundamental physics during the second half of the twentieth century.

Evidently, Weyl's intuitions have an excellent track record. By now, parts of the Philosophy of Mathematics and Natural Science are dated, of course. Those parts retain interest as intellectual history, because Weyl's understandings, being close to the best that was possible in their time, serve as benchmarks. But beyond that, Philosophy of Mathematics and Natural Science touches great questions that remain very much alive. Near the end of the book, after a penetrating critique of the concept of causality, Weyl turns to what he calls the body–soul problem — what we know today as the problem of consciousness.

"It is an altogether too mechanical conception of causality that views the mutual effects of body and soul as being so paradoxical that one would rather resort, like Descartes, to the occasionalistic intervention of God or, like Leibniz, to a harmony instituted at the beginning of time.

The real riddle, if I am not mistaken, lies in the double position of the ego: it is not merely an existing individual which carries out real psychic acts, but also 'vision', a self-penetrating light (sense-giving consciousness, knowledge, image, or however you may call it); as an individual capable of positing reality, its vision open to reason; 'a force into which an eye has been put', as Fichte says."

The fullest enlightenment comes only to those who, like Weyl, discern its absence, seek it, and recognize it when it arrives.

FURTHER READING

Atiyah, M. in Biographical Memoirs Vol. 82 , 1–13 (Natl Acad., Washington DC, 2002).

Weyl, H. Philosophy of Mathematics and Natural Science (Princeton Univ. Press, Princeton, 1949).

Talks at KITP

October 21, 2005
Categories: Uncategorized

There have been several recent interesting talks at the KITP in Santa Barbara as part of their program this semester on [Mathematical Structures in String Theory]. Last week Greg Moore gave a beautiful talk on [Mathematical Aspects of Fluxes]. It's a sad commentary on the state of the field that he felt it necessary to begin his talk by apologizing that the work he was describing didn't seem to have anything useful to say about the Landscape.

This week Graeme Segal spoke about [On the Locality of the Statespace of Quantum Field Theory]. He is trying to understand the right way to axiomatize the way in which the Hilbert space of a QFT depends on the boundary of space-time in a local fashion. In his talk he worked out things for the case of a free scalar on an arbitrary manifold. Also this week, [Peter Teichner] and [Stephan Stolz] spoke on their work on generalized cohomology and QFT, motivated by trying to understand the relation of elliptic cohomology and conformal field theory. Teichner gave the [first part of the talk] on Tuesday, Stolz gave the [second part] on Thursday. For more about their work, see some [earlier comments of mine], their paper [What is an elliptic object?], and an [earlier survey talk by Teichner at the KITP].

Comments

1. **Dimitri Terryn**
   October 24, 2005

   Hi Peter,

   You might be interested in this. I recently translated an interview with Renate Loll of Utrecht. The article has a fairly critical view of String Theory, along with comments on her own research and the position of women in science (Lubos is going to love this 🙂).

   Here is an exerpt. If you want the entire article, give me word.

   Superstring Theory

   According to superstring theory the most elementary particles in the universe do not consist of points, but of a kind of vibrating ellastic bands, whose vibrations manifest themselves as particles, like electrons or photons. Although the theory initially seemed to be a promising candidate to bridge the gap between relativity and quantum theory, it seems to be more and more clear that the theory has her own share of problems. The most serious complication is that according to this theory our world is part of a ten dimensional universe, without us noticing in our everyday lives. Possibly is our threedimensional universe floating through higher dimensions, in the same way as a two dimensional flying carpet is flying throug three dimensional space, separated from a shadowworld that may be only a few tenths of a milimeter away, as Spinoza winner Robert Dijkgraaf recently decribed.

   Altough Renate Loll is careful with her formulation as to not antagonize any of her colleagues, it is clear that she does not think much of this line of research. "Initially, superstring theory looked to be very simpel and therefore attractive, but graduately there emerged more and more complications, making me to find it quite a far fetched theory now. In addition it is unclear wether the string approach will lead us somewhere. That's why I favour my own approach. That at least has produced some concrete results.??

   Gerard 't Hooft, just like Renate Loll, isn't at all convinced by string theorists. But wether the approach of his colleague from Utrecht is correct, is still a question according to him. "It is clear that Renate has made progress the last few years, but she's not there yet. It's even the question wether she is on the right track concerning QG. Although personally I tend to look in the direction of black holes I think that string theory still has the best hand. We have hit a number of obstacles, but none the less is that approach still more concrete and structured than other attemps to reconsile GR and QT. But that doesn't mean that Renate couldn't be right. My philosophy is, let everyone muddle on. She should continue with what she is doing, because the resolution of this problem will probably come from an unexpected direction.??

2. **MathPhys**
   October 24, 2005

   It's very interesting to hear that 't Hooft says that "string theory still has the best hand". Very interesting.

3. **woit**
   October 24, 2005

   Hi Dimitri,

   Sounds like an interesting article. I see you have a blog, perhaps you should post the full translation there?

   Peter

4. **Dimitri Terryn**
   October 27, 2005

   Peter,

   I've posted the full article on my blog. You can find it on my blog.

Atiyah Talk at Santa Barbara

October 25, 2005
Categories: Uncategorized

Sir Michael Atiyah is here in the United States this month. Evidently he was at the Institute in Princeton last week during the Deligne conference, talking to Witten. Last Friday he gave a public talk at an AMS conference at the University of Nebraska on The Nature of Space, and will be giving another one tomorrow in Santa Barbara on the same topic.

Yesterday at the KITP he gave a talk on his own very speculative ideas about physics entitled Does the Universe Have a Memory?. He began by subtitling his talk “Crazy thoughts of an old man”, and noting that when he was at the Institute Witten had listened to him politely and then after a micro-second given him four reasons why his ideas wouldn’t work. One motivation he gave was that the current situation of string theory was somehow like Ptolemaic epicycles, with a fundamental idea that would drastically simplify everything still missing.

The speculative idea he was promoting was that perhaps quantum mechanics should be changed so that the future depends not just on the present, but on the history of the system during some short period before the present. So dynamics would be somewhat non-local in time. He hopes for some connection to the Connes version of the standard model, but this was all very vague. All in all, I fear that I wish Atiyah would go back to working on the relation between K-theory and physics….

Update: In a comment Doug provides a link to Atiyah’s public lecture.

Comments

1. **Chris W.**
   October 25, 2005

   That Atiyah would even give such a talk strikes me as a significant indicator of the level of disquiet that exists among leading figures in the string theory community and their allies in mathematics about the current state of work in the field, that is, about its presuppositions and goals.

   He adopted a clear position on the debate between those who would abandon continuity in favor of discrete models, and those who see too much power in the mathematics of continuous structures to accept taking such a step. I think this dichotomy is suspect; I’ll say more about it later.

2. **Arun**
   October 25, 2005

   If the theory of gravity induced by string theory has arbitrarily high degree of derivatives, then what’s the problem?

3. **Chris W.**
   October 25, 2005

   [*Oops! Sorry, I forgot to escape a < with a character entity…*]

   More on the above-mentioned dichotomy: *Please* read this discussion by Cosma Shalizi, and note the following:

   > The other reason for using symbolic dynamics is that there are important cases where one can find generating partitions — mappings into symbols where there is a one-to-one correspondence between continuous states and the symbol sequences they generate. In these cases, studying the symbolic dynamics is completely equivalent to studying the original dynamics. Remarkably enough, even with a generating partition, the symbolic dynamics can be stochastic for an underlying deterministic system. In this way, for instance, one can show that some kinds of (sufficiently chaotic) deterministic dynamics are in a sense completely equivalent to sources of independent, identically-distributed random variables.
   >
   > To go off on a tangent, there’s something of a movement in cognitive science which sets up an opposition between computation, conceived in the usual symbol-manipulation sense, and continuous nonlinear dynamics. This seems to me quite wrong-headed, if only because the existence of generating partitions shows how symbol-manipulating computation can be completely equivalent to a dynamical system. Computation is intrinsic to dynamics, but that’s another topic.

   Also, from Hendryk Pfeiffer (gr-qc/0404088):

   > It turns out that differential topology distinguishes the space-time dimension d=3+1 from any other lower or higher dimension and relates the sought-after path integral quantization of general relativity in d=3+1 with an open problem in topology, namely to construct non-trivial invariants of smooth manifolds using their piecewise-linear structure. In any dimension d<=5+1, the classification results provide us with triangulations of space-time which are not merely approximations nor introduce any physical cut-off, but which rather capture the full information about smooth manifolds up to diffeomorphism.

   ..and this from the body of the paper:

   > If quantum general relativity in d = 3+1 is indeed a PL-QFT, the following two statements which sound philosophically completely contrary,
   >
   > – Nature is fundamentally smooth.
   > – Nature is fundamentally discrete.
   >
   > are just two different points of view on the same underlying mathematical structure: equivalence classes of smooth manifolds up to diffeomorphism.

4. **Chris W.**
   October 25, 2005

   I must quote a bit more from Pfeiffer:

   > The diffeomorphism invariance of the classical observables then implies in the language of the triangulations that all physical quantities computed from the path integral, are independent of which triangulation is chosen. The discrete formulation on some particular triangulation therefore amounts to a complete fixing of the gauge freedom under space-time diffeomorphisms. The relevant triangulations can furthermore be characterized by abstract combinatorial data, and the condition of equivalence of triangulations can be stated as a local criterion, in terms of so-called *Pachner moves*. ‘Local’ here means that only a few neighbouring simplices of the triangulation are involved in each step. A comparison of Pachner moves with the block-spin or coarse graining renormalization group transformations in Wilson’s language reveals what renormalization means for theories with dynamical geometry for which there exists no *a priori*

background geometry with which we could compare the dynamical scale of the theory.

5. **Peter**
October 25, 2005

   Please remember this is NOT a forum for discussion of people's favorite ideas about quantum gravity. This is not what the topic of the posting was about.

6. **Kea**
October 25, 2005

   Chris W

   Atiyah did not deny this dichotomy between continuous and discrete. On the contrary, he endorses it (a) by indicating a preference for Connes' picture and (b) by saying explicitly that this Hale formalism for past dependent equations incorporates aspects of both.

   I enjoyed the talk! Forget the physics – it's full of subtle jibes and the sort of jokes that only very respectable old men can make.

7. **Nitin**
October 26, 2005

   Hi Peter

   Sorry to go out of subject here. I am wondering about something. I see a rather surprising number of physicists and mathematicians having weblogs these days. Even more surprising to me is that these people get the time to write about things they have heard, talked or thought about. Now, some of these postings are rather long (I cannot recall any unreasonably long posting of yours so far. I see a lot of this on Lubos' blog). Tell me… hasn't blogging affected your productivity at work? Clifford at Cosmic Variance writes frequently, you too… It seems to me that a lot of interesting things are being said most of the time, but aren't you better sometimes being busy at work or spending some time with friends away from the pc, or doing something different..?

   I would like to hear from you on this matter.

   Thanks
Nitin, Down Under

8. **Nitin**
October 26, 2005

   By the way.. I like the links you provide to interesting papers, notes, articles.

9. **Bryan**
October 26, 2005

   Hi Nitin: I think the problem is that guys like Peter have wireless connectivity to the internet all the time from their laptop or brain implant. It never gets turned off. Fortunately, they are able to do more than one thing at the same time…

10. **Ben Compson**
October 26, 2005

    Let's face it, if a theoretical physicist does 2 hours of good work in a day, that's a good day and the rest is his to do what he wants, like blog.

11. **Daniel Doro Ferrante**
October 26, 2005

    Following up on Ben's response: for a high energy theoretician, ~4h of good work in a day is a GREAT day.

    But, regardless, the sheer exercise of writing up, commenting, providing links, etc, etc, etc, is really good; it's a win-win situation: it's good for the audience (because they hopefully learn something new, leave interesting comments, etc) and it's good for the writer, beucase he gets a chance to clear his mind, making sure that his thoughts are cohesive and so on.

    So, say you teach, get 3h of good work and then you blog for ~1h, you may just have hit a pretty well rounded day: You can go home with a much better view of physics than when you came in.

    But, maybe it's just me… 😉

12. **woit**
October 26, 2005

    Hi Nitin,

    You're right that the weblog takes up a fair amount of time, and sometimes I do worry that it is taking away time I could be spending doing something more worthwhile, especially research. For now I think the time spent on this is worth it, but at some future point I may very well change my mind.

    I don't tend to write very long postings (I don't know how Lubos does it….), so the actual writing isn't that time consuming. Often I'm writing about things found by spending time looking around on the web, so in some sense the fact that I do a lot of that is the big time-waster, and there I'm probably not alone.

    I keep intending to try and write some postings that are more explicitly about the research work I'm doing. Then time spent on those would be in some sense time spent on research. One thing I need to do first is to try out various methods for putting equations on WordPress, I may get to this pretty soon.

    Peter

13. **John McCrone**
October 26, 2005

    A fun talk but – from a systems science point of view – it makes the familiar mistake of focusing on a "single-scale" discrete~continuous dichotomy and not the more fundamental "scalefree" dichotomy of local~global.

Atiyah suggests incorporating the uncertainty of QM by making just a single Planck-sized step into the past. A scale-free approach to memory would want to model a powerlaw distribution of "pasts". In this view the Planck scale defines not the fundamentally small (and discrete) but instead the fundamentally cogent (a web of interactions/gone to equilibrium).

Start by asking what is memory in a system. It is the general context within which a succession of particular states form. It is an ambience – a prevailing equilibrium in a system of interactions. So it is the "frozen" global view. And in hierarchical fashion, it exerts a downward constraint on any localised particulars. But of course, global constraint cannot "see" everything and so the local events (particle interactions, etc) are left with degrees of freedom. What we call their inertias.

So the memory of a system is its global coherent state which reflects some average of its past and – because the coherence is self-stable – also serves to predict the future for some reasonable distance. A universe that has developed certain constants, like the speed of light, is likely to continue to roll with those constants (barring a slow underlying evolution that eventually allows some further phase change to a new global vacua).

The point is that this globalised system memory is spread over all spatiotemporal scales. It is "scalefree" because the issue of scale has been thermalised.

So rather like a hologram, a very small "bite" into the past – like Atiyah's suggested Planck-scale step – would give some kind of snapshot of the whole, but it would be very fuzzy. By definition, it would seem to give the most uncertain view!

By being the closest to the localised freedoms of the system, it would see the least of the global constraints, the global memory. It would be like trying to observe the convergence nature of the system, its tendency towards a global remembered balance, from exactly the most open and divergent viewing position. To offer another analogy, it would be like trying to gauge the temperature of a gas by measuring some particular passing molecule.

Prigogine has some arguments along these lines in his modelling of QM in "The End of Certainty".

14. **Tony Smith**
October 26, 2005

In the .mov version of Atiyah's 24 Oct 2005 KITP talk at http://online.kitp.ucsb.edu/download/strings05/atiyah/snd/Atiyah_KITP.mov

Atiyah says (at about 1:12:24) that his class of models is based on "… the past history of a particle moving as a real particle …", which seems to me to be the past world-line of the particle.

An audience member describes to Atiyah (at about 1:05:02) "… a common thread between the class of models you are suggesting, Connes class of models, and some unsolved problems in string theory.
So,
one simple way to think about the class of models you are talking about is just to do a power series expansion of x(t-r) in t and … the higher derivatives of t so then you have an infinite order differential equation.
Similarly,
quantum field theory on a noncommutative spacetime can be expressed in terms of a star product which is an exponential of derivatives and therefore is also in some sense a differential equation with an infinite number of derivatives
and
the best nonperturbative formulation of string theory we have is string field theory which is expressed in terms of Witten's star product on strings which is also expressed in terms of some exponential of derivatives, but which we don't understand how to grapple with as well …".

It seems to me that a natural physical interpretation of that "common thread" is that strings should be interpreted as world-lines, NOT as individual particles or precursors of individual particles.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

15. **Quantum_Ranger**
October 28, 2005

Tony Smith says:"Atiyah says (at about 1:12:24) that his class of models is based on "… the past history of a particle moving as a real particle …??, which seems to me to be the past world-line of the particle".

An audience member describes to Atiyah (at about 1:05:02) "… a common thread between the class of models you are suggesting, Connes class of models, and some unsolved problems in string theory.

re:"So,
one simple way to think about the class of models you are talking about is just to do a power series expansion of x(t-r) in t and … the higher derivatives of t so then you have an infinite order differential equation".

The problem with this is that for evolving systems, specifically, from Quantum to Macro and equally, from Present to Future, the systems would have to be equivilent?

From Atyiah's talk of a "planck-memory" as representing some "past-initial-evolving-state", a little "it" from bigger "bit" if ever I heard one!, but there has to be a same/equivilent comparable 'worldline' from Present-time to Future-time?..this very notion would condem stringworld lines to the "Non-Renormable-Un-re-cyclable-Bin" correct me if I am wrong, but Aytiah notion of Relative/Quantum domains seems to be teetering on insane?..this I take to be the deliberate interuptions(spoiler in audience?) in the said lecture.

Aytiah's idea that a planck length is a Maximum Minimum "past-memory", of a "present-time", is no defferent from stating that the Present-time, is but a past memory of the 'yet-to-exist' future!

Just as a memory for any relative observer cannot contain 100% information (Reality is preserved in the Present-time), any worldline evolving from a planck domain, will contain very relevant Uncertainties?..you just cannot predict or construct, any present-time event from its past history!..if the speed of information recieved in any present-time event frame, exceeds the natural frequency of entropy, then theoretically, the Future, events that have not yet occured, could be manipulated directly from the Quantum past?

Slowing down an ordinary Photon would invoke a transformation in its "present-time", it would no longer have any Uncertainty association, you could design the Future with complete absolute accuracy?

The Quantum Mechanical domain would collapse, by the fact H.U.P would be nullified?
If one combines Atyiah's two-particle systems of [Past-Planck+Present-Time] with [Present-Time + Future Events], their equivilence would mean there is actually NO evolution whatso-ever.

16. **Tony Smith**
October 28, 2005

Quantum Ranger said, about the “common thread” comment by an audience member at about 1:05:02 in Atiyah’s KITP talk online at
http://online.kitp.ucsb.edu/download/strings05/atiyah/snd/Atiyah_KITP.mov

“… ..this I take to be the deliberate interuptions(spoiler in audience?) in the said lecture. …”.

That comment was made in the question and answer period following the lecture, so it was not a “deliberate interruption”.

Based on the context (Atiyah’s reply was “I agree with what you say” followed by extensive further discussion), it did not appear to me that the commenter was considered by those at the KITP talk to be a “spoiler”.

If you watch the .mov of Atiyah’s talk, you can see who made that comment. The camera shows him at about 1:07:58. I did not recognize him (but then I don’t know a lot of KITP people by sight).
Perhaps someone reading this can identify the one who made the comment.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

17. **Quantum_Ranger**
October 28, 2005

Tony, my appologies, I had watched the lecture, but found the constant interuptions by the “same” audience member quite annoying, I do so admire Atyiah, but actually felt a little sorry for him being so obviously “heckled”.

I am going to re-watch the ‘whole’ talk, and hopefully the Q/A I missed would enlighten me, a question I had been contemplating form Atyiahs idea, is what happens to the missing “planck-memory”, it seems to be forever evolving “backwards”, as for sure, even the Planck-memory has to been formated from a previous “past”?

18. **Who**
October 28, 2005

much of the interruption came from David Gross

during the QandA someone pointed out that there had really been two lectures going on, one by sir Michael and one by David, and reasonably enough that person asked David Gross a question related to HIS part of the lecture

the interruption was almost frantic and I think not malicious so much as instinctively defensive. If Atiyah ideas had been simply crazy it would not have been necessary to resist so hard. but the ideas, for good or ill, were somehow perceived (consciously or not) as plausible enough to be disturbing.

19. **Tony Smith**
October 28, 2005

Quantum Ranger asked “… what happens to the missing “planck-memory??, it seems to be forever evolving “backwards??, as for sure, even the Planck-memory has to been formated from a previous “past??? …”.

I agree that is a good question, and it is also something that nagged in the back of my mind (in the form of why should the memory / past world-line be cut off at the Planck scale).

If there were no past time cutoff, then the basic entity would be the entire (back to the big bang?) past history world-line of each particle. Maybe such a model would be like that of Andrew Gray, who said in http://xxx.lanl.gov/abs/quant-ph/9712037 (in the abstract) “… probabilities are … assigned to entire fine-grained histories. The formulation is fully relativistic and applicable to multi-particle systems. It shall be shown that this new formulation makes the same experimental predictions as quantum field theory …”.

The same Andrew Gray proposed a “Quantum Time Machine” in version 1 of http://xxx.lanl.gov/abs/quant-ph/9804008v1, but he withdrew that proposal on 8 Aug 2004, the same day that he posted version 2 of http://xxx.lanl.gov/abs/quant-ph/9712037. Therefore, it seems to me that although Andrew Gray felt his “Quantum Time Machine” was flawed, he still feels that his formulation of quantum theory in terms of “entire fine-grained histories”, which sounds to me a lot like Atiyah’s model without the Planck-scale cutoff, is valid.

I wonder whether Atiyah knows of Gray’s model, and, if so, how he (Atiyah) thinks it compares with his (Atiyah’s) model.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

20. **Doug**
October 30, 2005

For those who have been as frustrated as I have in trying to find Atiyah’s Nature of Space talk, here’s a link that actually works:

The Nature of Space

21. **Doug**
October 30, 2005

I have an innocuous question to ask: where would theoretical physicists generally tend to place Aityah, and his contributions, in their list of world-class mathematicians important to theoretical physics? And, BTW, what is the correct pronunciation of his name?

22. **Kea**
October 30, 2005

“where would theoretical physicists generally tend to place Aityah, and his contributions, in their list of world-class mathematicians important to theoretical physics?”

Very high.

23. **woit**
October 30, 2005

Hi Doug,

I agree with Kea. I'd put Atiyah among the top three mathematicians of the last half of the 20th century (with Serre and Grothendieck), and more important for theoretical physics than any great mathematician since Hermann Weyl. His work on instantons and anomalies during the late seventies and early eighties was wonderful, and his conjectures made in 1987 about the existence of various TQFTs were influential in Witten's amazing work in this area. Atiyah became less active in research mathematics after around 1990 when he became president of the Royal Society and master of Trinity College.

The pronunciation is Ah-Tee-Yaah.

24. **woit**
October 30, 2005

Oh, and thanks for the link to Atiyah's lecture!

25. **Tony Smith**
October 31, 2005

Peter, I agree with you that Atiyah is "… among the top three mathematicians of the last half of the 20th century (with Serre and Grothendieck), and more important for theoretical physics than any great mathematician since Hermann Weyl. …".

How does that square with the treatment of Atiyah by David Gross and Edward Witten?

As to David Gross, "who" said in a comment here: "… much of the interruption came from David Gross … the interruption was almost frantic and I think not malicious so much as instinctively defensive. If Atiyah ideas had been simply crazy it would not have been necessary to resist so hard. but the ideas, for good or ill, were somehow perceived (consciously or not) as plausible enough to be disturbing. …".

As to Edward Witten you (and Atiyah) said: "… Witten had listened to him politely and then after a micro-second given him four reasons why his ideas wouldn't work. …".

If I am correct in my opinion that any evaluation given after only a micro-second is NOT a considered evaluation (nobody, not even Witten is that smart), then it seems to me that
1 – Gross and Witten are very insecure about the superstring theory in which
they have invested their lifework
and
2 – Gross and Witten are incapable of fair evaluation of any alternative ideas.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

26. **woit**
October 31, 2005

Hi Tony,

My take on Gross and Witten's reaction to Atiyah was rather different. Atiyah has taken pains to not be especially critical of string theory (I've corresponded with him about this, and, much as I would love to count him as a string theory critic, he makes clear that's not at all how he sees himself), so I don't think they were worrying about defending string theory from Atiyah. Gross did interrupt and make extensive comments at Atiyah's talk, but I took this as evidence that he was taking Atiyah seriously and trying to engage with him on his idea, even though it was pretty off-the-wall. I suspect Atiyah was exaggerating about the "microsecond", and in any case Witten probably had plenty of time to think about Atiyah's proposal while Atiyah was explaining it to him, enough time to come up with several pretty obvious problems.

Both Atiyah and Witten are extremely quick on their feet. I remember one time at MSRI talking to Raoul Bott, who had just walked away from Atiyah and Witten, shaking his head. He told me he found listening to the two of them "scary" since they were so much quicker than he was. Bott is a great mathematician also, but one who has to think everything through slowly and carefully to understand it, quite different than Atiyah or Witten.

27. **Jack Sarfatti**
October 31, 2005

What about Wheeler-Feynman & Aharonov 2-State QM in which present is a self-consistent global double loop 1/2(Advanced + Retarded)?

28. **Quantum_Ranger**
October 31, 2005

Thanks to Doug, I have just sat through the whole talk:The Nature Of Space.

My initial annoyance of the talk with D Gross was based on the lack of acoustics, not being able to hear what Gross was asking produced my unfounded accusation of "spoiler", this self-evaluation is made from after the above linked talk.

What becomes clear from the Atyiah talk, is that there are numerous area's of interpretations in nailing down what constitutes 'Memory'?..for instance a mathematician who calculates a simple formula, is performing a repetative function based on "memory", its an act of reproducing a "Time" and "information" event. In its simple way it is an act of "time-travel", via a thought function contained in the bio-mass of braincells.

Relativity, in its conception gives presedence to the Observer in Time, we are the only forms that can perform "time-travels" mentally?..this is what a memory is.

Now interestingly Atyiah is stating that a planck-memory, is a decomposition of a real-time event,(a past-moment) located at some instant in the present. Matter has a specific evolutional path, it tends to move from one location to the next in constant fashion. If one take's the electron and follows its trajectory, one finds that there is no continuation of its path, it can transfer itself from one location to another, without trancending the intervening space, from A to B, it can appear at B from the Future, without trancending the Present.

Feynman I believe made the "electron" statement:It is never located in the present-time, it always jumps from Past/Future or Future/Past?

The QM roots of H.U.P, contends that an Atyiah "planck-memory" must, at some moment along its timline evolution, make a transition from the Present-Time, to a Past-

Time, it makes a 'Jump' into the Past?..the "moment" this occurs, it loses its memory of where it jumped from, it is re-configured, without any memory at all!

The Virtual Quantity associated with any energy of Quantum status, means that any finite Planck-Memory (any componant with all its paramiters), after it reaches the transitional 'gap', it has no choice but to reconfigure, as if it is appearing from the Future, and it has a new set of Quantum Numbers!!

Calibrating a decomposed planck-memory from a 'present' Spacetime, with that of a 'future' Space,(there has to be particles contained within present-time space, that are scaled from the future) means that at in a finite limit, energy and its memory of its evolution, exchange with that of particles emminating from the Future, there would therefore be Particles comparable to that of Planck Scale, appearing with a lot more Energy, future re-tarded Quantums?

Where is Planck-Memory stored?..is it within the present or future, ordinary memory by a conscious observer is stored within the mind, somewhere in the Observers present-time, scaled down and stored, never to be recalled as 100%, it always has some missing information, else it would reproduce reality events 100%, is this information lost to the Future or Present?..it has to go into the construction of the future, the future always needs more information than the Present, just as the Present always needs more informaiton than the Past!

29. **Juan R.**
October 31, 2005

Almost all said by Atiyah is pure nonsense in the best string tradition.

A basic discussion of why most of Atiyah's remarks are completely *outdated* and some of them **completely wrong** is available at

http://www.physicsforums.com/showpost.php?p=806910&postcount=10

Jack Sarfatti said

What about Wheeler-Feynman & Aharonov 2-State QM in which present is a self-consistent global double loop 1/2(Advanced + Retarded)?

We are still waiting the promised Wheeler's conference 🙂

Juan R.

Center for CANONICAL |SCIENCE)

30. **Quantum_Ranger**
October 31, 2005

juan r, the context of Atyiah's "planck-memory" is surely an inquisitive inquiry into cause and effect, does a past memory effect present events?..can the present,'whole', be created out of a finite memoric value, a minimum (planck) value?

At some moment the Present-time becomes the Past, and at the same instant the Future also becomes the Present?

If there is a "planck-memory", a Quantum value of energy at miniscule scales, then there is also a comprable Macro value at the intesection moments of Present to Future. The scale difference increase's at Present/Future boundaries, this is because the Future needs more energy/information, due to fact of increase in size, by volume?

If the future surely contains the same amount of energy(in discrete bundles of planck memory bits) as the Present, and consequently the Past energy, then symmetry could not be broken in a previous era?

At least 50% of 'present-time', is distributed into a past and future tense virtual domains. If planck scale "memory" is continueous in its attachment to a present spacetime location,then there is no cut-off point. If it is discrete at finite volume's, then it is "seperate" and isolated from the present-time, therefore it no longer has a memory of its origin, it could take,choice? of a "memory" of infinite proportional value.

Virtual particles have virtual histories?.. accountable in terms of no memory of a previous existence, which I believe Feynman asscerted to certain particles mentioned by Jack Sarfatti above.

Again Atyiah seems to be poking little 'peek-holes' into certain "canned" models and interpretations.

Some canned models, contain worm-holes.

I think there is a whole new can of Wormholes being inderectly opened/presented by Atyiah, some of the nagging 'worms' are going to be hard to swallow, especially for certain string models.

If you do not want to shoot yourself in the foot, then controlled experimental particle physics is the sure way to avoid using "elephant-guns" to exterminate "M", Theoretical mice!

I rate Atyiah as joint "number-2" in the context of greatness in contributing to scientific knowledge. There is no number one, this is yet to be filled, and he share's this position with a vast number of other greats, I have a personal distrust of league tables, as the importance of stature can be over-rated by association and personal tastes.

31. **Tony Smith**
October 31, 2005

Peter, as you say, "… Both Atiyah and Witten are extremely quick on their feet. … Raoul Bott, who had just walked away from Atiyah and Witten, shaking his head … told me he found listening to the two of them "scary?? since they were so much quicker than he was. Bott is a great mathematician also, but one who has to think everything through slowly and carefully
to understand it, quite different than Atiyah or Witten. …".

It is interesting that your characterization might be that Atiyah is a Hare and Bott is a Tortoise, yet working together they produced wonderful results. In an interview at http://www.ams.org/notices/200104/fea-bott.pdf Bott described his work with Atiyah, saying: "… In most of my papers with Atiyah he would write the final drafts and his tendency was to make them more abstract. …".
Bott went on to say: "… I like the old way of presenting things with an example that gives away the secret of the proof rather than dazzling the audience. … on the whole I like the problems to be concrete. I'm a bit of an engineer. For instance, in topology early on the questions were very concrete – we wanted to find a number! …".
Bott's "we wanted to find a number" remark sounds to me a Feynman-like attitude toward physics.

As to physics and physicists, Bott made an observation about the Princeton IAS under Oppenheimer: "… Oppenheimer had taken over, and he was very dominant in the physics community. He had a seminar that every physicist went to. We mathematicians always thought they ran off like sheep, for we would pick and choose our seminars! …”.

Maybe superstring theory under Witten and Gross is a the contemporary manifestation of physicists’ sheep-like behaviour.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

32. **woit**
October 31, 2005

Hi Tony,

Your characterization of Atiyah and Bott as the Hare and the Tortoise is apt, as is the Bott quote. Together they did some truly wonderful things.

It’s always been true that physics is much more faddish than math, with particle theorists generally desperate to somehow contribute something to whatever the latest, hottest thing is, mathematicians more interested in digging deeply into one particular thing that they can become one of the few experts in and make their own. In the past, when experimentalists were regularly providing theorists new and surprising results that pointed out the right direction to go, a somewhat faddish concentration on figuring out the significance of these new results made sense as a way to make progress. The problem is that particle theory still has that sociology in place, but the experimental impetus that made it work is gone.

33. **Doug**
October 31, 2005

Regardless of the merits of Atiyah’s ideas on how to approach the QM challenge, the merits of his assessment of the current situation in theoretical physics are priceless, in my opinion. As an icon in the coummunity (gathered from the respect expressed here), his emphasis of the role of simplicity and elegance in nature’s secrets is crucial. From this perspective, he indicts string theory with one devastating observation:

“If a final theory emerges soon from string theory, we will discover a universe built on fantastically intricate mathematics.”

His point is made in the “conudrum” of imaginary numbers that he describes. The conundrum is perplexing because, if, fundamentally, the origins of mathematics are found in nature, then the “fantastically intricate mathematics?? of string theory reflects something ugly and unsatisfying in nature, which would be so surprising, given humanity’s historical experience with her.

On the other hand, if mathematics is just a mundane tool for studying the physical structure of the world, and is no more than an invention of the human mind, how is it that its “biggest, single, invention,” imaginary numbers, show up in observed physical phenomena?!

He says that while his position is more moderate than Kronecker’s, he still believes that the origins of math are to be found in nature’s fundamentals, which then man develops and elaborates upon. Clearly, he’s implying that we have strayed too far from the origins of mathematics; that the vastness of string theory’s mathematical complexity has now taken on a life of its own, which abandons the vital reciprocal relation of math and physics.

It reminds me of Hestenes’ observation that

There is a tendency among physicists to take mathematics for granted, to regard the development of mathematics as the business of mathematicians. However, history shows that most mathematics of use in physics has origins in successful attacks on physical problems. The advance of physics has gone hand in hand with the development of a mathematical language to express and exploit the theory…The task of improving the language of physics…is one of the fundamental tasks of theoretical physics.

Coming from such eminent mathematicians, these are sage words indeed. Is Atiyah on a mission?

34. **John McCrone**
October 31, 2005

“From this perspective, he indicts string theory with one devastating observation: “If a final theory emerges soon from string theory, we will discover a universe built on fantastically intricate mathematics.??

String theory seems pretty natural if you have an organic metaphysics in mind – where something emerges as the constraint of a vaguer everythingness, rather than a creatio ex nihilio ontology.

So at the fundamental level, there is a chaos of potential with no mathematical structure. Then structure self-organises as resonances that lock into place. As this SO reduces dimensionality, the structure becomes more robust. Far out, you have only sporadic flashes of order – like the Monster lie group. Then as you get down to just 10 or 11 dimensions, the structures become increasingly robust.

Everything finally locks up pretty solid at three dimensions (with their emergent “fourth dimension” of a flow of time).

This is why we should expect a swamp as the fundamental ground for physical theory – it describes the vagueness, the potential, that would be a realm of “everythingness”.

The issue is then to understand whether the reduction of this raw potential to crisp (mathematically resonant and self-organising) structure has just one outcome, or a variety of possible outcomes. We could be the only possible kind of universe.

These points may seem off-thread but the metaphysics is important once you start asking whether maths is mechanically constructed or Platonically given. There is a third road which may be unfamiliar but was in fact widely prefered even in the time of Plato and Democritus.

35. **Tony Smith**
November 1, 2005

Peter, you said “… All in all, I fear that I wish Atiyah would go back to working on the relation between K-theory and physics …”. Here are some related quotes and comments:

Bott, in his interview at http://www.ams.org/notices/200104/fea-bott.pdf , said “… the start of my long and wonderful collaboration with Michael Atiyah. We first of all gave a new proof of the periodicity theorem which fitted into the K-theory framework … Then Grothendieck, in the purely algebraic context, gave a … proof …[of]… the index theorem … using his K-theory in the formal, algebraic way. … Before, we had taken complex analysis or algebraic geometry as a given, so that the differential operator was hidden … here, suddenly the topological twisting of the differential operator came into the equation. Of course, Atiyah and Singer immediately realized that this twisting is measured with the homotopy groups of the classical groups, by the so-called symbol. Eventually the whole development of index theory fitted the periodicity theorem into the subject as an integral part. Atiyah very rightly chose Singer to collaborate on this project. …”.

In their book Spin Geometry (Princeton 1989 at page 277), Lawson and Michelsohn said: "… In 1982, E. Witten found a different approach … through consideration of symplectic geometry and supersymmetry. … he outlined a proof of the index theorem for the Atiyah-Singer operator … however … none of these methods [including Witten's] applies to prove the index theorem for families or the Cl_k – index Theorem (in their strong forms). These theorems in general involve torsion elements in K-theory which are not detectable by cohomological means. …".

In his book Introduction to Superstrings and M-theory (Second Edition, Springer 1999, 1988 at page 338), Michio Kaku said: "… new developments in supersymmetry have now made it possible to prove the Atiyah-Singer index theorem from a simple Lagrangian. Traditionally, the proof of the Atiyah-Singer theorem has been inaccessible to most physicists because of the intricacies of the mathematical formulation. …".

Reading those excerpts in sequence leads me to think that a reason that superstring physicists are so attached to supersymmetry is that it is only through Witten's supersymmetric approach that they can understand the Atiyah-Singer index theorem.

However, by restricting themselves to the Witten supersymmetric construction, the supersymmetry physics people are cutting themselves off from possibly very fruitful avenues of constructing new and possibly realistic physics models.

For instance, Lawson and Michelsohn, at page 270 of their book cited above, said [I have omitted some tildes etc from notation due to ASCII limitations]:
"… Given a real operator … in the basic case, no information is lost under complexification. This is not true, however, if one passes to the index theorem for families. The index of a family of real operators takes its value in the group KO(A), and .. for example … KO(Sn) = Z2 for n = 1 (mod 8) but K(Sn) = {0} in these dimensions.
For this reason Atiyah and Singer established a separate index theorem for families of real operators. It is a more subtle and profound result … the appropriate theory is not KO-theory … It is the more general KR-theory …".

If Kaku's assessment of physicists' inability to understand a KR-theoretical index theorem is correct, then I share Peter's sense of loss if Atiyah is not now "working on the relation between K-theory and physics".
However,
I respect Atiyah's right to follow his own intuition and to pursue his physics model based on Hale's book Functional Differential Equations. It may even turn out that there is some connection between the two areas, and that Atiyah is smart enough to sense such a connection.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

36. **Tony Smith**
November 1, 2005

My immediately previous post seems to suffer from an inadvertent smiley.

The smiley should be the number 8 and the equation should read

KO(Sn) = Z2 for n = 1 (mod 8) but K(Sn) = {0} in these dimensions.

As I indicated in the immediately previous post, the equation also suffers from suppression of such things as tildes etc due to ASCII limitations.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

37. **Juan R.**
November 1, 2005

quantum ranger, As already said Atiyah's talk is outdated.

Curiously the current tendency in the topic -Atiyah is of course NOT an expert- is **the contrary** of Atiyah's proposal.

Retarded diferential equations have proved to be computationally intractable during more than 40 years (moreover do NOT solve the famous problem of the arrow of time). Today, there is emphasis on the **abandonment** of this way by either Marklovian equations in a generalized space functional -e.g. 90s Prigogine proposal in a RHS- or the use of sophisticated memory kernel techniques eliminating the convolution (the 'memory') from the evolutor, transforming the retarded equation into a local time equations that can be computationally solved.

Literature in this topic is very extensive and complex.

Atiyah's talk is both outdated and wrong in several fundamental aspects. See above link for a dissection.

That universe has a memory is already very well-known, for example the standard Zubarev equation with an infinitesimal memory -Abel kernel-. Equations with memory are standard in many condensed matter disciplines. This is not new.

The discussion of Atiyah on causality structure of the universe is completely wrong and very outdated. It is unnecesary the appeal to Planck scale physics -see Prigogine theory for example, or CSM approach, or etc, etc, etc.-.

You would read *The End of certainty* by Prigogine

http://www.amazon.com/exec/obidos/tg/detail/-/0684837056/103-6383925-0546230?v=glance

for some recent views on the topic. Atiyah proposal would be good in the 50s and 60s. In fact, the first basic equation -generalizing mechanics- proposed by Prigogine was a retarded equation with a memory term and a complex diagranmatic technique acounting for all that cannot be explained via mechanics (see above book) or field theory. All of that is well-known in speciliazed literature. But further work did Prigogine and other abandoned that way. We are in the 2005!

Juan R.

Center for CANONICAL |SCIENCE)

38. **Adrian Heathcote**
November 2, 2005

I've read through the discussion above I can't resist expressing a point of disagreement.

I found Atiyah remarkably charming and urbane—but I was kind of expecting that—but I thought the continual interruptions from Gross rude to the point of obnoxiousness. Atiyah couldn't get two sentences out without being pulled up and forced to engage in a dialogue. This is not how one should behave in a talk, whether threatened, not threatened, malicious, not malicious. There is just no excuse for this boorishness. And it was very easy to sense Atiyah's frustration.

Sorry, but I thought this was pretty much a disgrace. I can't believe that the person chairing the talk didn't exert some control over his audience.

39. **Doug**
November 2, 2005

I felt the same way, but the person causing the disruption is the head of the Institute, so the chair of the talk wasn't about to try to exert any control. Which, of course, makes it all the more boorish.

40. **Adrian Heathcote**
November 2, 2005

Thanks Doug, that info does go some way to explaining it! I now see what others have been saying regarding his sensing Atiyah's talk as a threat. Any suggestion of even the possibility of a new idea, or the need for one, seems to have been taken as a five alarm fire—to be extinguished immediately.

Poor Atiyah had the hose directed at him the entire time!

41. **Adrian Heathcote**
November 2, 2005

I have a more substantial thought on Atiyah's talk which I think is on his side.

Modern explanations of EPR-like entanglement suggest that the composite system is in a pure state, whereas the component systems are in mixed states. The mixed states of the components do not uniquely determine the composite pure state—so some information resides in the state of the pair of particles (there from their anti-correlation origins) which does not reside in the component states. This is the so-called quantum holism, and it gives rise to the entanglement because of the non-commutativity of the operators that apply to the individual systems.

But then there is at least room for saying that the pure state contains information about the past of the system, which must be factored in when looking at the correlations at the time of measurement. I know it's sketchy but it does suggest that Atiyah's idea may have some kind of role to play.

Pardon the pun, but if string theory could go some way to explaining entanglement it would have done something interesting. But it doesn't even appear to be in the right ball-park. Or am I wrong here?

String Theory and Intelligent Design

October 26, 2005
Categories: Uncategorized

The latest Cosmic Log column on msnbc.com concerns Lawrence Krauss's new book Hiding in the Mirror and the author asked Krauss a question I'm expecting that physicists will be hearing more and more often as time goes on: "Why is string theory science but intelligent design isn't?"

Krauss gives a response that isn't completely convincing. He says that "the difference is that Ed Witten and the other good string theorists will, if an experiment comes along that demonstrates that supersymmetry isn't discovered in a definitive way, be the first to say the theory is wrong." This isn't really true. Since the scale of supersymmetry breaking is unknown, one can't hope to experimentally definitively show supersymmetry is not there. And the question at issue is string theory, not supersymmetry. Will string theorists abandon the theory when supersymmetry is not found at the LHC? We'll see in a few years, but I already see them hedging their bets and many undoubtedly will not see the lack of supersymmetry at LHC energies as proving string theory wrong.

The behavior of string theorists that Krauss identifies as most like religion is the argument that "the theory is so beautiful it must be true." I actually don't hear many string theorists making this argument these days. If the theory actually were beautiful in the sense of providing some impressive new understanding of physics in terms of some simple, compelling mathematical or physical idea, that actually would be a good reason for believing in it, although not a completely conclusive one. All attempts so far to connect the theory to real physics lead to hideously complicated and ugly constructions. Some string theorists such as Susskind, argue that one should believe in string theory anyway, and it is this argument which seems to me to be more like religion than science. It's my impression that Susskind and others are believing something for sociological and psychological reasons, something for which they have no rational, scientific argument. This behavior is not distinguishable from that of many of the intelligent designers, and if it becomes more widespread it ultimately threatens to do real damage to the public perception of science in general and theoretical physics in particular.

Krauss gets closer to the real difference between string theorists and intelligent designers when he says that string theorists "are trying to come up with predictions that actually do something". More sensible string theorists are well aware that what they are doing isn't going to be part of science until they figure out a way to use it to make real predictions that can be tested. In general, given a new speculative idea, it will not be obvious how to figure out all of its implications and see whether it can lead to real predictions. It can take years of work for this to become clear, and this sort of work is definitely science. On the other hand, if after a lot of work, there still is no indication that an idea can produce predictions, the continued pursuit of it at some point stops becoming science and starts becoming something more like religion. Susskind and other anthropic landscapeologists have already gone past this point: they have no plausible idea about how to ever get real predictions out of their framework. String theorists who argue that the theory is still too poorly understood, that more work is needed to understand whether there is some way around the radical non-predictivity implied by the landscape, are nominally still doing science. But at some point, as years pass without any progress in this direction, and evidence mounts that hopes for ways to get predictions aren't working out, this activity stops being science and it too starts being a non-scientific activity pursued for sociological and psychological reasons. We're close to that point, if not already past it.

Update: There's a defense of string theory against the charge that it's like intelligent design over at Kasper Olsen's blog. I don't find it very convincing, since it doesn't address at all the question of how string theory is ever going to do what a real science is supposed to do: make falsifiable predictions. Much of Olsen's list actually strikes me as a recitation of a catechism of supposed reasons why string theory is so wonderful, rather than a serious scientific argument. Some of these are also highly dubious (e.g. "the Standard Model can be reproduced in a very simple way"), they're things that one has to be a true believer to say, since they really don't accord with reality.

One commenter (Gavin), gave a very good reason for distinguishing string theory from intelligent design: "the former is trying to explain something that is already explained, while string theory is trying to solve a mystery" and he correctly notes that while string theory's scientific credentials may be weak, the problem is that there aren't really good alternatives (LQGers may argue with this…). John Baez's comment about the relationship of math and physics was also quite nice.

Comments

1. **BD**
   October 26, 2005

   "A non-scientific activity pursued for sociological and psychological reasons" seems an overly broad characterization, if it encompasses both religion and — as most people would characterize non-empirical string theory — abstract mathematics. Do you really want to be arguing that non-physics mathematics (which string theory may prove to be) is equivalent to religion? Even totally absent "predictions that actually do something," string theory as mathematics would still be a very different type of activity from religion, and would deserve a very different treatment in our schools (were a high school ever to attempt to teach totally abstract math).

2. **Arun**
   October 26, 2005

   "Why is string theory science but intelligent design isn't???

   Even if string theory is not physics, it is mathematics. Depending on your point of view, mathematics is the handmaiden or the queen of sciences.

3. **Subhash**
   October 26, 2005

   Now that it has been mentioned, one cannot deny that if "beauty" is the justification to do string theory then the same can be invoked for Intelligent Design (ID) also. The supporters of ID claim that it is more elegant than evolution, stressing that it is the messiness of probabilites that they are trying to banish.

   If some people are doing string theory because of its mathematics, then perhaps it should be done in the Math Departments. There is no question that falsifiability should remain the primary criterion of any physical theory.

4. **jack**
   October 26, 2005

   Well, it could be worse. At least the ID people propose an alternative to evolution, however crazy. They don't just tell us, year in, year out, that evolution is not science, that evolutionists are pursuing the theory for purely sociological reasons, etc etc etc, without ever proposing anything that even looks like an alternative.

5. **Quantoken**
   October 26, 2005

   Arun said:

   "Even if string theory is not physics, it is mathematics. Depending on your point of view, mathematics is the handmaiden or the queen of sciences."

I am sorry. Mathematics is not science. Mathematics is just a language, a language that is suitable to be used to describe science. But it is not itself a science. Mathematics do not make predictions and do not experiment with nature. So mathematics is NOT science. You can easily figure out how to calculate the surface area of a sphere in a hypothetical 26 dimentional space, using pure math, but such math constructs have nothing to do with nature, which is certainly not 26 dimentions.

Quantoken

6. **dan**
October 26, 2005

playing devil's advocate, SST/m-T does predict SUSY & 11-D which *could* be falsible.

also, one contact SST does make is reproducing the BH entropy

7. **an interested observer**
October 26, 2005

I agree and disagree with Quantoken. Yes, mathematics is not a science. No, mathematics is not just a language to describe science.

Modern mathematics, the way I look at it, is a creative endeavor in which we study abstract well-defined objects that we find intriguing, in a certain language (set theory, category theory, …) that's internal to mathematics. In particular, a priori, there's no connection of mathematics with any language that describes how the world works. That mathematics itself happens to be the best language to "do" science in, is co-incidental and a consequence of the fact that (historically) SOME PARTS (calculus, say) of mathematics were invented to solve problems external to mathematics, and SOME PARTS (representation theory, say) of modern mathematics, besides being of great interest within mathematics, simply HAPPEN to describe the real world spectacularly well. — it's a gross generalisation to say that all of mathematics is just a language to describe the real world. And yes, mathematics is not a science since, as defined above, it serves its own ends and does not attempt to describe how the "real world" works .

One may conclude from the above that, as BD does with Peter's post, that I believe mathematics is "equivalent" to religion. This seriously depends on the definition of equivalence. If two subjects become equivalent simply because they both arise from psychological motivations, then yes — mathematics and religion are equivalent. In this case, however, I think your definition of equivalence needs rethinking. After all, physics and biology (or even economics) arise from similar motivations — they both try to describe a certain aspect of the real world. Does this make them "equivalent"? I think not.

8. **Subhash**
October 26, 2005

I disagree with the notion that math is not science. Mathematics relates to properties of objects in a formal system (that may or may not correspond with a physical system) together with rules of association amongst those objects. Since such formal systems are a product of our mind, mathematical truths are of fundamental importance and they may be viewed as extensions of logic. Obviously claims in a formal system are falsifiable excepting in cases where the system of axioms is not rich enough for one to prove or disprove an assertion.

9. **Doran**
October 27, 2005

Jack, I am really hoping your being sarcastic, for you have attributed to stringers what ID proponents have been doing since 1987 when Pandas and People was first published. Please go check out Pandas Thumb or Pharyngula if you need a refresher course on the sadness that is the Intelligent Design movement.

I believe Peter has made this analogy before, and I cannot help but wince. String theory has numerous problems, especially with regards to experimental testing. Intelligent Design is a socio-religious movement that wishes to repackage old school creationist trash in a form that will squeeze by the establishment clause. I doubt stringers have hired their own international PR firm, and are attempting to teach Gauge theory to fourteen year olds.

If there was any "religious quality" about string theorists it reminds me more of New Agers, whose theories while holistic, are absolutely worthless. I find Planck's original derivation of his radiation law beautiful due to its ingenuity, but its wonderful agreement with experimental evidence is just as aesthetically pleasing as the mathematics he used.

ID is far more dangerous and near term, for its proponents have sold their souls and intellectual integrity to promote a defunct faith and the bastardization of the scientific method.

10. **Arun**
October 27, 2005

What I wrote was intended in the following spirit:

"Mathematics is the Queen of Science, and Arithmetic the Queen of Mathematics. – C. F. Gauss"

"The Handmaiden of the Sciences. Eric Temple Bell (1883-1960), [Book by that title.] "

"Mathematics serves as a handmaiden for the explanation of the quantitative situations in other subjects, such as economics, physics, navigation, finance, biology and even the arts." – H. F. Fehr

Intelligent Design and "is mathematics science?" make for sterile arguments.

11. **Clark**
October 27, 2005

The problem with the claim mathematics is what distinguishes religion from science is that it runs into trouble when you consider numerology or pythagoreanism. There's been a lot of mathematical mysticism from the early days of Pythagoras on up to even the present. Go down to that goofy isle in Barnes and Noble that sells "metaphysical" books and you're bound to find at least one book that is complete silly gibberish but very mathematically.

The relationship between math and religion is quite old. Remember that Plato saw geometry as one of the best illustrations of his rather odd notion of an immortal soul and remembering. Since we weren't creating the geometric proof we have to be "perceiving" them in some sense or remembering them.

Complete balderdash, of course, but hugely significant. This rebirth of mathematics as religion can even be found in fairly prominent scientists. Read some of the discussions of Newton as a hermeticist to see this.

Anyway, I don't have much to say vis a vis ID or string theory. I reject ID and have my doubts about strings, as interesting as they are. But I didn't go far enough down that line of physics to be able to say much of worth to the science.

12. **Quantoken**
October 27, 2005

Subhash:

Has it been falsified, or not falsified, in mathematics, the statement that two parallel lines shall never cross? You've got to know neither case is right or wrong.

There is no empirical truth in mathematics. All math are logical derivations from a few fundamental rules which we probably took for granted, but which do not need to be taken for granted as truth, like the parallel line hypothesis. As such, all you could ever say is certain statement is either consistent, or inconsistent, with the set of hypothesises that your math is based on. There is no absolute truth.

Another example, you may take it for granted that 1+1 surely equals 2. But there is a hidden presumption here that you discuss the problem within the arithmatics rules built on the set of natural numbers. You could well establish an arithmatics system which is self consistent, for example one where the only numbers exist are 1 and 0. In that case, 1 + 1 gives 0, and 2 doesn't exist.

You might think an arithmatic system with only 0 and 1 and no other number may sound silly. But remember, for a long time, our math of geometry only dealt with 3 dimentional space, not anything more than 3-D, and certainly not infinite dimentionality. And it is still difficult for our limited mind to comprehend higher dimentionality than 3. What is true in a 3D world may not be true in other dimentionality.

So, there is no absolutely truth in math. All you can ever show is some statement are consistent or inconsistent with other statements.

Quantoken

13. **woit**
October 27, 2005

Thanks for all the interesting comments.

I've said this many times before, but I should be more precise about what aspects of string theory I'm criticizing as unscientific. What I have in mind is specifically the idea that one can unify the standard model and gravity using a 10/11-d string/M-theory. Things like AdS/CFT, where strings are used to construct a dual to a strongly coupled gauge theory, are certainly physics. Things like topological strings and their relations to Gromov-Witten invariants and much else are definitely mathematics. But the 10d superstring compactified on a Calabi-Yau is a very complicated mathematical structure, one that doesn't seem to have any special deep significance. Studying aspects of this complicated structure can lead to a lot of interesting mathematical work, but the full structure itself is not mathematically compelling, a good reason to believe it isn't going to be a successful TOE.

As for the mathematics/religion comparison, well they're quite different pursuits, although I'm sure one can find some relations between them. The mathematics/physics relationship is a very deep question, and I don't think we know enough about either subject to yet know precisely what the relation is. My own inclinations are kind of hyper-Platonist, thinking that ultimately we will see that the deepest mathematical structures and the deepest physical structures are very closely related.

14. **Amitabha**
October 27, 2005

Through a strange coincidence I received the following quote from Aldous Huxley in the mail yesterday:
Where beauty is worshipped for beauty's sake as a goddess, independent of and superior to morality and philosophy, the most horrible putrefaction is apt to set in. The lives of the aesthetes are the far from edifying commentary on the religion of beauty.

15. **John Baez**
October 27, 2005

Peter writes:

The mathematics/physics relationship is a very deep question, and I don't think we know enough about either subject to yet know precisely what the relation is. My own inclinations are kind of hyper-Platonist, thinking that ultimately we will see that the deepest mathematical structures and the deepest physical structures are very closely related.

It's interesting how your post about Lawrence Krauss' book provoked this almost totally irrelevant discussion about whether mathematics is a science, etcetera. I think there's a lot of what the economists would call "pent-up demand" for better understanding the relationship between mathematics and science – it bursts out at inappropriate moments, like just now.

The problem of course is that in the standard modern picture, science is empirical, based on induction, and tends to favor a materialistic ontology, while mathematics is non-empirical, based on deduction, and tends to favor a Platonist/Pythagorean ontology… yet somehow they need each other!

So, mathematics is not only the queen and handmaiden of the sciences – it's the secret mistress as well, a source of romantic fascination but also some embarrassment.

The hard-nosed physicist is supposed to treat mathematics as a mere "language", but finds himself becoming fascinated by its "beauty". He finds that the pursuit of beauty can lead to practical results that a merely pragmatic approach would never obtain – the "unreasonable effectiveness" of mathematics.

This is already puzzling enough, but the full story is even trickier: beauty – *or at least our possibly mistaken* ***notion*** *of beauty* – can seduce us and lead us astray! Is string theory beautiful and correct, beautiful and false, or actually ugly but pretending to be beautiful, like Cinderella's elder sister, struggling to fit the glass slipper on her oversized foot?

It's all so confusing. 🙂

16. **A.J.**
October 27, 2005

A shorter answer: String theorists are sincerely interested in extending known and tested science. Intelligent design "theorists" are interested in undermining the public's faith in known and tested science.

17. **Thomas Larsson**
October 27, 2005

Natural supersymmetry has **already** been ruled out by experiment, AFAIU. If no sparticles are found at the LHC, slightly unnatural SUSY will be ruled out as well, leaving very unnatural (split or supersplit) SUSY as the only possibility.

SUSY has long been invoked as a solution to the hierarchy problem, i.e. why the weak scale 100 GeV is so small in units of the Planck mass. Although SUSY reduces this problem, recent experiments (in particular limits on the Higgs mass and permanent dipole moments) rule out the possibility that SUSY will solve it completely. This is usually expressed by the phrase "SUSY requires fine-tuning at the percent level".

Let me add that this does not make me happy because I dislike SUSY. Rather, it's the other way around: I dislike SUSY because it seems very much to disagree with experiments.

18. **dan**
October 27, 2005

"A shorter answer: String theorists are sincerely interested in extending known and tested science"

tell that to one of lubos motl's critics!

19. **D R Lunsford**
October 27, 2005

Well it seems to me that yes ST is religion, but its 11th century religion, not Baptist fundies.

-drl

20. **david g**
October 27, 2005

If we were to take an Hegellian approach and consider the theory of evolution, the thesis, and the intelligent design theory, the antithesis, then perhaps we need to seek a synthesis of the two. If we turn to physics, we find that the atom, indeed, any atom, is almost a perfect vacuum, closer to perfect than outer space. All objects in our physical universe are made up from molecules, which are in turn consist of atoms. As atoms are vacuums, then molecules would be vacuums, then so too is the substance of our universe. So our physical universe is almost a perfect vacuum, an absence of matter. There is next to nothing there. Of course, I am not suggesting that the physical universe is an illusion, but rather that our perception of it is illusionary. The universe is one big Disneyland! And it is simply the coarseness of our senses, that makes us believe that the physical universe is substantial.
Our eyes respond to visible light, which is electro-magnetic radiation with wavelengths very much larger than the diameter of an atom and at the same time the frequency of these radiations is very much lower than the frequency of electron orbits forming the shell of the atom. This means that visible light will bounce off an atom rather than pass through it. Sound and touch involve much much larger wavelengths than visible light, so they too would tend to find a physical object impenetrable. On the otherhand, cosmic rays have a wavelength much smaller than the diameter of an atom, and scientists going down into deep mines have found that cosmic rays can penetrate thousands of metres into the Earth.
It is one's minds which interprets his senses and passes it on to him, the inner self. The conscious, intelligent part of us. Oh dear! It would seem I'm making a case for intelligent design. Oops!

21. **JKG**
October 27, 2005

"Why is string theory science but intelligent design isn't??? its not really the question, the question is "Why is standard model (hot Big Bang cosmology, etc) science but intelligent design isn't???

All these questions refer to metaphysical foundations of sciences and are actually the core of the so called demarcation problem (of Popper). There is no good answer to that. One can presumably DEFINE science so that the standard model is science and intelligent design is not, though it is not trivial at all (for example by using Popperian conjectures and refutations program). One can also presumably DEFINE science so that the string theory is science and intelligent design is still not. One can also try to device a demarcation line between sciences and religion, for example by assuming that there is no transcendental knowledge, see what is left and call it science. One can also take the Spinozian pantheistic stand, and assume that the physical universe is just god (I am simplifying things here, of course), and then intelligent design is nonsense, logically.

Of course, intelligent design is just fiddling with judeo-christian-muslim image of god: an old man who treats the physical universe as his playground. If you accept this I can hardly see how you could do science at all since causality does not work any more (or at least there are uncontrollable exceptions).

But whatever you do, this is a metaphysical exercise, and not a scientific one (ie. one cannot define science from within the science itself.) So the question "Why is string theory science but intelligent design isn't??? is basically not even wrong

22. **AG**
October 27, 2005

It is notoriously difficult to draw the Popperian demarcation line, but surely there is a fundamental difference between string theory and intelligent design. When embarking on a scientific research program, it is not always possible to tell from the start what falsifiable predictions or practical applications might emerge, and theoretical scientists have to go on well-formulated hunches and search for ways to corroborate their hypotheses; nonetheless, their efforts can still be called science. Intelligent design, however, is not science– not because it cannot be falsified, but because it cannot, even in principle, be corroborated either. How can an ID proponent move forward with his "research program??? All he can do is attempt to expose places where evolution supposedly fails as an explanation… but this is not how science works. A scientist who spends his time trying to reveal gaps in loop quantum gravity is not doing string theory by default. Science doesn't work in the negative—you have to try to corroborate claims of your own hypothesis, not simply attack the claims of others. And clearly ID has no means of corroborating their "hypothesis?? in a self-contained, positive manner. Of course, it may prove to be impossible to test string theory's hypotheses directly, but actually this is where mathematics can step in and render string theory legit science. The power of mathematics is that it can reveal equivalences among physical statements that are in no way evident in science alone – and that's because 'science alone' consists of physical interpretations we impose upon mathematical structures. These interpretations are limited by imagination and physical intuitions, but mathematics is not. AdS-CFT is a prime example, and if string theory's structure can be shown to be equivalent to something we can better understand physically, and therefore test, its proponents' efforts will not have been in vain. Surely there are many mathematically equivalent physical theories all expressing the same basic truths about nature, so it can't hurt to pursue multiple lines and hope some clever scientists (or mathematicians) unveil their hidden relationships. On that note, I'm really curious to know if there have been any interesting developments in the search for a dS/CFT?

23. **Arun**
October 27, 2005

Prof. Motl's review of the book on amazon.com says that superstring predicts fermions.

To me that is stretching the meaning of prediction to an extreme, but your mileage may vary.

24. **Gavin**
October 27, 2005

One important distinction between intelligent design and string theory is that the former is trying to explain something that is already explained, while string theory is trying to solve a mystery. Intelligent design is useless because evolution is the right explanation for the origin of species. However, we don't have a theory of quantum gravity. String theory may be wrong, or even untestable, but I think that we should have a lot of freedom to speculate when we are trying to solve an open problem.

The biggest threat to string theory isn't experiment, it is somebody coming up with a straight forward way to quantize gravity without all of the extra dimensions, branes, etc. of string theory. The draw of string theory isn't its strong scientific credentials, it is the weakness of any competition.

Gavin

25. **Juan R.**
October 27, 2005

A couple of replies

Woit
I think that you are almost right in your personal valuation. Almost all of string theorists have spent much time on string theory research. But this is not the true problem; the problem has been the premature publicly in mass media. String theory was presented as 'fundamental', as 'the last theory'. This has generated a lot of hype around it. For example, it is usually thought by arrogant people that any guy working in string theory is a genius and *any* outsider is a 'mo – – n', etc. It is usually thought that any other theory is derived from string theory as a special case, etc. Of course, nothing of this is true.

I am rather sure that string theory community can be splinted into three parts:

– Workers. E.g. graduate students doing PhD because they did not find other area of research due to pressure of string physicists on research programs.

– Leaders. People who is a fanatic of the theory. For example Kaku, who has no problem in claiming, "*Absence of aliens in the universe is a sign of that string theory may be correct*??. The basis for such one bizarre idea is that they used string theory for travelling, via a hidden dimension, to other universe!

– Rest.

I think that only 'leaders' will continue to work in the theory even if they is experimentally proved to be incorrect. In fact, there is a very good basis for this thinking: the own history of the field. String theory was *always* refuted by experiment, and each new experiment or internal contradiction did that string theorists developed a new version or reparameterised previous one –curiously are always called 'string theory'-. There is a joke circulating in the Internet saying that if string theory is shown to be experimentally false like a theory of everything, the 'leaders' will explain that is due that string theory is really a theory of more than everything.

For anyone who do not know the history of the field, the joke is based in that when string theory failed in the nuclear regime and was substituted by QCD, string theorists as Schwartz claimed that the problem was that string theory was more that a theory of strong force and included also gravity and then began a new -more general- formulation. Curiously, that more general theory has not still explained the strong force like QCD has done during decades…

Regarding beautiful arguments, I simply want to say that beauty always was a subjective feature. I personally find all string theory research an ugly subject, with lot of irrelevant math and wrong physical stuff.

At least, some string theorists openly state that his belief on string theory is only that. James Gates recognizes "*the analogy to a religion has been noted by a number of people. In a sense that's right; it is kind of a church to which I belong. We have our own popes and House of Cardinals*.??

Arun

It is not true that string theory was mathematics.

People working in string theory is doing mathematical research because some parts of string theory need of mathematics still do not developed. That is VERY different from claiming that string theory 'is' mathematics:

Mathematics is Calabi-Yau manifolds and G2s, topology, K-theory, etc.

Chemists and mathematicians working in molecular structure have developed some useful techniques on graph theory, but chemistry is not math not graph theory a branch of chemistry.

John Baez

I respect you opinion but I follow Newtonian philosophy. Math is an idealized construct for the description of the physical word. Feynman explaining between geometrical lines and light 'lines' was brilliant.

Your claim about that "mathematics is non-empirical?? is being debated by own mathematicians. Have you heard about experimental mathematics?

Will be math sufficient? Any reply will be speculative. However, I begin to believe that for a 'real' understanding of world, math alone will be not sufficient. In my opinion, there is not a kind of marriage between math and physical world in the end. Math is a beautiful kind of simple intermediate language that contains many features of that we call physical world…

Probably the great failure of string theory is directly related that its main practitioners are people with mathematical-oriented minds… and therefore unable to really understand physical word. One only need the many paper on the subject.

We need is not another Witten in physics, we need a new Feynman 🙂

Juan R.

Center for CANONICAL |SCIENCE)

26. **Mr Jones**
October 27, 2005

John wrote
"It's interesting how your post about Lawrence Krauss' book provoked this almost totally irrelevant discussion about whether mathematics is a science, etcetera. I think

there’s a lot of what the economists would call “pent-up demand?? for better understanding the relationship between mathematics and science – it bursts out at inappropriate moments, like just now.
”

I guees this partly has to do with a somewhat naive application of Popper’s falsification method. Remember Popper wanted to distinguish between science and pseudo-science. Now we obvioulsy don’t want maths to be on the wrong side of the line, but a naive application of Popper runs precisely that risk. Has anyone checked what the great philosopher himself wrote about this? He was a clear thinker!

27. **Joao Leao**
October 27, 2005

Commenting on John Baez comment:

Funny on how a science/religion argument closes in on the math/physics question, indeed! A recent paper by Hut, Alford and Tegmark dives fully into this puddle curiously without touching string theory! The target happens to be **Penrose** himself a thorough critic of String Theory along Peter’s lines, seems to me. What strikes me is the tone of the discussion which is entirely set in religious terms advocating a plurality of “mind sets” rather than any self-critical departure. How postmodern!

Something for your one of your “This Week’s” entries, John? I would be curious to know your opinion on this gem…

-Joao

28. **Ygorff**
October 27, 2005

Peter, you say “topological strings and their relations to Gromov-Witten invariants and much else are definitely mathematics”, “dual to a strongly coupled gauge theory, are certainly physics” which is fine. Then you claim “But the 10d superstring compactified on a Calabi-Yau is a very complicated mathematical structure, one that doesn’t seem to have any special deep significance”.

This seems a contradiction to me, since all is the same.

GW invariants of topological strings are most interesting on CY, they do describe 4d strongly coupled gauge theory and in reverse most of the well studied 4d strongly coupled gauge theories are dual to (GW on) CY. Topological string on CY is nothing but a low energy sector of superstring on the same CY. So how can the same subject be relevant mathematics and physics and without significance at the same time.

29. **woit**
October 27, 2005

Hi Ygorff,

There’s no question that you can get some interesting mathematics or physics to study by looking at certain limits or subsectors of superstring theory. The examples I gave are what seem to me the most interesting ones for mathematics and physics. But just because something has a very interesting low-energy limit doesn’t mean it is necessarily especially interesting itself. By now there’s a huge variety of different things that people have looked into motivated by superstring theory. They’re not “all the same”, even if they have the same motivation. People need to carefully see which things have led to good mathematics and physics, pursue those, and abandon those parts of superstring theory that haven’t led to good math or physics.

If you want to promote the idea of working on superstring theory because it has a topological subsector that gives interesting invariants of certain complex three-folds, or because it might give a useful string dual to QCD, that’s fine. But that’s not what people promoting this research are doing; they keep pushing the failed idea that the full 10d superstring theory will give a unified TOE. It is this idea that not only doesn’t work, but leads to ugliness like the landscape, not to anything particularly mathematically deep.

30. **Blue Fog**
October 27, 2005

an interested observer said

…..That mathematics itself happens to be the best language to “do?? science in, is co-incidental and a consequence of the fact that (historically) SOME PARTS (calculus, say) of mathematics were invented to solve problems external to mathematics, and SOME PARTS (representation theory, say) of modern mathematics, besides being of great interest within mathematics, simply HAPPEN to describe the real world spectacularly well. — it’s a gross generalisation to say that all of mathematics is just a language to describe the real world. And yes, mathematics is not a science since, as defined above, it serves its own ends and does not attempt to describe how the “real world?? works ….

The above statement is an absolutely ridiculous idea. Math CANNOT be invented, it can only be discovered. New math was always a result of a need for explanation of observable fenomena in reality.
Calculus was NOT “invented” by Newton, he actually disovered that Euclidian geometry was not adequate to describe his physical observations. That process continue to happen now too, whenever physics is not able to describe an observable phenomena with available mathematical tools, we are going to see attempts to find new, adequate tools.

To think the opposite one would have to admit that there is no difference between math and fiction writhing. The only reason that math is the best language for science is that math indeed describes the real world because mathematical descriptions of reality are representing real physical qualities of matter.

Try to use your “invention” process and create a new math to describe any already settled physical theory, lets say, Carnot cycle, and tell me what you come up with. If you say that math is “invention”, you should not have any difficulties to do so.

31. **ygorff**
October 27, 2005

Peter, the point was, that if you compactify 10d string theory on 6d CY, you get a strongly coupled 4d gauge theory (very similar to QCD) and the internal part of the superstring in addition gives you the non-perturbative action, which you could hardly get from field theory.

A field theorist, who indeed would be strong enough to get the action from scratch, would necessarily reconstruct 6 extra dimensions in the form of a CY, whether she likes 10d strings or not. Even if she refuses to take these dimensions as physical, she would find that dynamical questions in the gauge theory have a mathematical structure that describes certain dynamics of strings on the internal 6d dimensions.

Whether or not you take the internal space and these strings on it as physical, this structure of 4d x 6d CY emerging from gauge theory is rather simple and efficient and this seems at odds with attributes like “complicated” and “without deep significance”, at least for me.

As for coupling to gravity, the very same argument also gives you the coupling of gravity to this field theory, by considering contributions from GW at higher genus. So the internal part of the 10d superstring is worth of a lot of interesting physics and mathematics.

Not too many string theorists would seriously claim that string theory as it is today is already the TOE of THIS universe, but most of them would agree on that unifying gauge theory and gravity is one of the main motiviations to study strings. The above example shows that the (pretty sketchy) ansatz of 10d superstrings on CY does already a surprisingly good job, a better job then any other known ansatz. A lot of work and ideas will be necessary to apply strings to this universe, but there is no reason whatever to worry about anything else but the reach of human brain power. So far things work surprisingly well and I don't see why you say the idea is "failed" or "doesn't work".

32. **Arun**
October 27, 2005

I think it is a reasonably well-defined question mathematically to ask what a quantum theory of extended objects would look like, and a reasonably well-defined mathematical answer is it would look like string theory. Hence string theory is at least mathematics.

Does such a theory have any physical relevance is the question, and the answer is, yes, it does, except perhaps not as much as the most rabid hypemeisters of theory proclaim.

33. **woit**
October 27, 2005

Ygorff,

Sure, you can try and study various strongly coupled supersymmetric 4-d gauge theories by constructing string duals using 10d superstrings compactified on a CY. But this is a complicated business and in general these are complicated constructions of unclear mathematical signficance. The physical significance is that you'd like to solve QCD this way. This hasn't yet happened. Maybe this will ultimately be the path to finding a string dual of QCD, maybe some very different idea using neither 10d superstrings nor CYs will be required.

Sorry, but to describe the current situation of attempts to unify the standard model and gravity using superstrings as "so far things work surprisingly well" is just absurd. We're at 21 years of work by thousands of smart people and counting, without a single prediction of any kind, and the best guess as to where this all leads is the landscape framework which is horrifically ugly and can't predict anything. By any reasonable accounting, this is an idea that has failed and doesn't work.

34. **woit**
October 27, 2005

Arun,

String theory is of course mathematics in the sense that it is a collection of mathematical formulas.

As for its physical relevance, it's not just "rabid hypemeisters" who are claiming you can unify gravity and the standard model this way. If most string theorists have actually given up on this idea, I think they should say so.

35. **Zimmermann**
October 27, 2005

God said to Abraham: Kill me a son
Abe said man: you must be puttin' me on

Has theological debate progressed any more than has science?

36. **Clark Goble**
October 27, 2005

Excuse the typos on my earlier comment. (It was late at night) Anyway, a few more comments on this very interesting discussion.

John Baez *The problem of course is that in the standard modern picture, science is empirical, based on induction, and tends to favor a materialistic ontology, while mathematics is non-empirical, based on deduction, and tends to favor a Platonist/Pythagorean ontology… yet somehow they need each other!*

Is mathematical platonism still dominant? I know that the quasi-realism of folks like Putnam has intrigued a lot of people. But I thought that the logicists and the constructivists were now dominant. Of course that might be more among the philosophically well read. So I couldn't even guess in the typical math department let alone physics department.

Woit *My own inclinations are kind of hyper-Platonist, thinking that ultimately we will see that the deepest mathematical structures and the deepest physical structures are very closely related.*

Doesn't that verge upon being more of a religious belief? (Not that there is anything wrong with that – so long as it isn't taught as science the way ID tries to portray itself) I just bring it up since it seems, as John Baez seems to suggest, that at this level where we're so far removed from empiricism many things more religious or aesthetic seem to dominate. I think that's true of ID and perhaps is an influence in some thinking about string theory.

"Blue Fog" *The above statement is an absolutely ridiculous idea. Math CANNOT be invented, it can only be discovered.*

That's true only in some formulations of mathematics. Other formulations say it is all invented/constructed. To say that math can't be invented is akin to saying a nuclear reactor can't be invented, only discovered. Unless one is clear about how to distinguish invention from discovery in an unambiguous fashion, I'm not sure this approach is useful.

37. **woit**
October 27, 2005

Hi Clark,

I don't think that my views on math or physics have much to do with religion, although they do have something to do with aesthetics. That deep mathematics and deep ideas about physics are sometimes closely related is an empirical fact about the history of physics and math. My belief that there is more along these lines is just a working hypothesis about where to look for promising ideas. It is based both on extrapolating from past history and some very specific conjectures about the relation of math and physics that look compelling to me, although much work needs to be done to see if they really make sense.

38. **Arun**
October 27, 2005

Peter,

Certainly string theory is more than just a collection of mathematical formulas. It has led to one if not two Fields medals.

-Arun

39. **Scott Aaronson**
October 27, 2005

Hi Peter,

Thought I'd contribute a "manual trackback entry" (since my blog software doesn't support trackbacks):

http://www.scottaaronson.com/blog/2005/10/insert-string-pun-here.html

[…] In short, if the ID'ers are armed squatters in the apartment building of science, openly scorning the materialistic concept of rent, then the string theorists are model tenants who often drop by the landlord's office to say good afternoon, and by the way, that check from 20 years ago should clear any day. (In their defense, the other tenants' checks haven't cleared either.) To me, this raises an interesting question: does science need a notion of "resource-bounded falsifiability," which is to Popper's original notion as complexity is to computability?

40. **woit**
October 27, 2005

Hi Scott,

Thanks. I just saw your blog posting and was about to link to it from here but you beat me to it. I love your analogy of science as apartment building and scientific predictions as rent.

Interesting to hear that Frederik Denef has been consulting with you on Landscape/computational complexity issues.

41. **Quantoken**
October 27, 2005

Blue fog said:

"Try to use your "invention?? process and create a new math to describe any already settled physical theory, lets say, Carnot cycle, and tell me what you come up with. If you say that math is "invention??, you should not have any difficulties to do so. "

Hey you make invention sound **so easy and trivial**. Let me ask you have you ever patented **anything** at all during your lifetime? I bet you have not applied patent for even one damn little silly invention, all your intelligence notwithstanding.

Mathematics is a language, invented and developed over thousands of years by the whole of human intelligence. It is a great invention so please do NOT trivialize it by challenge any individual to come up with an invention that is in par. We saw great civilizations in human history that built great palaces and cultivated plantations, but failed to develope modern science because they have not invented the necessary language to describe science.

Peter wonders how mathematics and physics are so close in describing nature. Maybe you should also wonder why English and French, two completely different language, happen to describe exactly the same thing in the reality world? The languages, whether it is English or mathematics, are invented to describe the world we observe. So they have got to be closedly related to nature.

Besides, mathematicians who study fields that more or less has some association with the reality world have a better chance of getting support and being able to forward the study, versus stuff that sounds totally silly and totally detached from reality. I predict that all the mathematics tools that folks developed in super string theory research will pretty soon be **completely forgotten**, once it's been shown it has no resemblance to the reality world.

Quantoken

42. **AJ**
October 27, 2005

ARUN: Yeah, but none of those Fields Medals have been related to the way string theory uses Calabi-Yau manifolds. CY manifolds are nice spaces, but they don't seem to be of fundamental significance in mathematics. It doesn't bother me that the Standard Model makes use of an object as random as SU(3)xSU(2)xU(1), but string theory purports to be fundamental. It's aesthetically disappointing it seems to require an arbitrary choice of Calabi-Yau manifold.

43. **Dan**
October 27, 2005

ygorff's point is very strong, and I might add, some people are strong enough to get the internal dimensions and their geometry from purely field theoretical considerations. Namely, Seiberg and Witten have shown that in their exact solution of d=4, N=2 SQCD a rather mysterious curve appeared, the period matrix of which described the effective couplings of the gauge theory. Soon it was realized that this curve is precisely the internal manifold on which you need to compactify string theory if you want to realize this SQCD theory on branes.

I'm kind of surprised Peter doesn't understand this point. So far I thought he was a hard boiled skeptic of the relevance of string theory to nature, but distinguished from pure crackpots by his realization that string theory does lead to highly non-trivial and useful results for a lot of semi- or non-realistic models — such as N=2 SQCD. Now it seems that Peter has given up his last piece of wisdom. I will not go as far as Lubos who labels him an "empty head", but…

44. **Chris W.**
October 27, 2005

The comments on this post haven't said all that much about Intelligent Design, despite its mention in the post's title. I thought I would add this modest contribution (with thanks to William Saletan of *Slate*, and Monty Python).

45. **woit**

October 27, 2005

Dan,

I think I've written that string theory is useful for studying supersymmetric gauge theories and and is a promising approach to understanding QCD several times already in this posting and somewhere between 10^2 and 10^3 times on this weblog.

There's some sort of weird and very specific disease that afflicts string theorists who read the words I write. Anything critical I write about string theory they interpret as meaning that I am not aware of the achievements of string theory methods in dealing with 4d supersymmetric gauge theories. This is really tedious. They also especially like to do this from behind the cover of anonymity.

Your notion that I used to understand this, but don't anymore, at least has the charm of novelty.

46. **plato**
October 27, 2005

John Baez:*The problem of course is that in the standard modern picture, science is empirical, based on induction, and tends to favor a materialistic ontology, while mathematics is non-empirical, based on deduction, and tends to favor a Platonist/Pythagorean ontology… yet somehow they need each other!*

This is a very interesting comment to me. No, not because of my name :), but because it really defines the process, doesn't it?

Putting ID aside, isn't this what would make string theory suitable?

If such inductive and deductive processes are indeed followed, as in any other model that you chose to use, furthers insight and development are indicative of advancement, in physics and mathematics?

I think this has already been pointed out, that modest gains may have some value in string theory as a model.

47. **Dan**
October 27, 2005

Hi Peter,

I'm afraid you don't get off the hook so easily. You said:

"Sure, you can try and study various strongly coupled supersymmetric 4-d gauge theories by constructing string duals using 10d superstrings compactified on a CY. But this is a complicated business and in general these are complicated constructions of unclear mathematical signficance. The physical significance is that you'd like to solve QCD this way."

This is plain wrong in the case of N>=2 SUSY. In particular, the meaning of the Calabi-Yau is *physically* clear from the *field theory* perspective. Your lip service that you understand the significance of string theory to gauge theories is contradicted by your own statements. It's not a matter how many times you claim you understood…

48. **Arkadas Ozakin**
October 27, 2005

John Baez said:

> The problem of course is that in the standard modern picture, science is empirical, based on induction, and tends to favor a materialistic ontology, while mathematics is non-empirical, based on deduction, and tends to favor a Platonist/Pythagorean ontology

I am not sure if you agree with this "modern picture", but here is a quote from the book *Complex Manifolds and Deformation of Complex Structures* by Kunihiko Kodaira that I find interesting in relation to the "induction/deduction" issue:

> The process of the development [of the theory of deformation of compact complex manifolds] was the most interesting experience in my whole mathematical life. It was similar to an experimental science developed by the interaction between experiments (examination of examples) and theory.

I have the feeling that this was not an isolated incidence in the history of mathematics, but that inductive/experimental approaches in mathematics are alive and well. I also remember reading strongly worded statements by V. I. Arnold on this issue (which I can try to dig up).

49. **plato**
October 27, 2005

I have the feeling that this was not an isolated incidence in the history of mathematics, but that inductive/experimental approaches in mathematics are alive and well.

I would say so. If you have situations in physics such as particle reductionism and you are reaching limits, why would you not say, okay, let's try something different and quite profound in our general concept makeup. Let's shake the foundation up?

Did string theory succeed? Did they point to a time where we knew good scietntists were working in conjunction with views on the cosmo?? It further refined our views on how we view the early cosmo? Guth's was the first three minutes, refinement, meant something else here.

50. **woit**
October 27, 2005

Dan, sorry but you're engaging in the really obnoxious behavior I've experienced time and time again from string theorists. The sentences you quote are not "plain wrong"; they are perfectly accurate. You're putting your own construction on them which has nothing to do with what I was saying, then using this to attack me as not knowing what I'm talking about, using your anonymity in a cowardly fashion. If you don't think something I wrote is correct, how about writing in to ask me to elaborate? Then if I don't know what I'm talking about, my elaboration should make this very clear and you can joyfully point this out. Or you might find out I just meant something different than what you thought.

I can't be sure, but I assume you're objecting to my final sentence, which had nothing at all to do with the "physical significance" of the Calabi-Yau in the constructions you're thinking of. I was referring to the potential significance to a real physical problem: that of getting non-perturbative information about 4-d gauge theories relevant to the standard model. I realize this is kind of strange behavior you may have trouble understanding, but when I use the term "physical", I'm just about always referring to the physics of the real world. By "physical significance" I meant "significance for physics", real world physics. In the real world the main reason you want to better understand non-perturbative gauge theory is QCD (a secondary reason is that you might believe in something like technicolor and want to use non-perturbative gauge theory behavior to break electroweak symmetry).

That's the elaborated version of the last sentence you quoted. If you think that it's "plain wrong" and evidence I don't know what I'm talking about, let's hear why. If your problem was with one of the other sentences, we can go over those too….

51. **dan**
October 27, 2005

playing devil's advocate here, if there was a house fire, one of two hypothesis as to the origin of explusion may be possible

1- it is of human origin (accident, or intentional)
2- it is the result of natural processes (i.e faulty wiring)

how would we tell apart 1 from 2? could such methods apply to biological organisms?

52. **Renormalized**
October 27, 2005

Dan said:

"A shorter answer: String theorists are sincerely interested in extending known and tested science??

String theorists are sincerely interested in extending known and tested science, into unknown and untested science.

"playing devil's advocate here if there was a house fire, one of two hypothesis ….."

Huh? Should have put this where most biological material coming from a back hole belongs.

53. **Steve Myers**
October 28, 2005

Math deals with all possible worlds; physics tries to select the one we live in. Math is Jules Verne; physics is Tolstoi. Intelligent design is situation comedy; string theory is Gertrude Stein turning Joyce"s "Finnegan's Wake" (without consulting Gell-Man) into an opera (music by John Cage).
Math is not only a language in the way French, English, etc., are languages since I can analyze the structure of languages with mathematics. As Russell (and Frege before him) showed, math is neither mental nor physical — it deals with an objective world (in the sense that the axis of the earth is real but neither mental or physical). Math ideas can start out as generalizations of the particular (Gauss' technique) or figues and systems created from basic abstract concepts (invariance, symmetry, etc.). The math I do has direct physical applications (temperature profiles of elastic extruder dies; power requirements of a splicer; gear ratios,etc.) but it all has a basis in fundamental concepts.

54. **Juan R.**
October 28, 2005

Next i will write a list with all correct physics has been already obtained **from** string, brane, and M theory.

– [ ]

– End.

P.S1: Witen Fields medal was not awarded for work in string theory.

The 1990 Medal was for Vladimir DRINFELD, Vaughan F.R. JONES, Shigefumi MORI (University of Kyoto), and Edward WITTEN. None of awards was for work in string theory.

Regarding Witten the award was for "*Completed an amazing proof of the classic Morse inequalities and gave a proof of positivity of energy in Einstein's theory of Gravitation*."

None of them directly based in string theory. In fact *a posteriori* one can search some link between string theory and part of Field Medals Witten's work, but the claim that Witten received the Medal for string theory is just an abuse of language, somewhat as "*string theory predicts gravity*". Both Newtonian gravity and general relativity were discovered before string theory!

P.S2: The myth says that Ed Witten is the new Newton. This is rather difficult to believe. Newton was a real mathematician with an insight beyond rest mathematicians of his epoque. Witten is more a particle physicist with a very good command of math.

String theory community over-popularizes Witten's contributions claiming that Witten is doing 'real' math and stating that string theory 'is' math. This distorted view is neglected by own mathematicians. The correct place of Witten in math is brilliantly explained as [*]:

Although mostly not in the form of completed proofs, Witten's ideas have triggered major mathematical developments by the force of their vision and their conceptual clarity, his main discoveries soon becoming theorems. His Fields Medal at the 1990 International Congress of Mathematicians acknowledged the growing impact of his work on contemporary mathematics.

Reading above, I understand Witten's work as a kind of 'pre-mathematics'. Newton did not "pre-mathematics", Newton did mathematics. From the mathematical side, string theory is a kind of 'pre-mathematics'.

[*] ICM-90 Kyoto, Japan, Notices Amer. Math. Soc. 37 (9) (1990), 1209-1216.

Juan R.

Center for CANONICAL |SCIENCE)

55. **Dr. Ranger McCoy**
October 28, 2005

FOR IMMEDIATE RELEASE:

http://physicsmathforums.com/showthread.php?p=788#post788

Tied Up & Strung Out: Hollywood String Theory Movie!!! Looking For Extras!!!

ALL TIED UP & STRUNG ALONG, a movie about String Theorists and their expansive theories which extend human ignorance, pomposity, and frailty into higher dimensions, is set to start filming this fall. Jessica Alba, John Cleese, Eugene Levie, Jackie Chan, and David Duchovney of X-files fame have all signed on to the $700 million Hollywood project, which is still cheaper than String Theory itself, and will likely displace less physicists from the academy.

“As contemporary physics is about money, hype, mythology, and chicks,” Ed Witten explained from his offices at the Princeton Institute for Advanced Study, “The next logical step was Hollywood, although I thought Burt Reynolds should play me instead of Eugene Levy.”

Brian Greene, the famous String Theorist who will be played by David “the truth is out there” Duchovney, explained the plot: “String theory’s muddled, contorted theories that lack postulates, laws, and experimentally-verified equations have Einstein spinning so fast in his grave that it creates a black hole. In order to save the world, we String Theorists have to stop reformulating String Theory faster than the speed of light. We are called upon to stop violating the conservation of energy by mining higher dimensions to publish more BS than can accounted for with the Big Bang alone, and I win the Nobel prize for showing that M-Theory is in fact the dark matter it has been searching for.”

Greene continues: “At first my character is reluctant to stop theorizing and start postulating, but when my love interest Jessica Alba is sucked into the black hole, I search my soul and find Paul Davies there, played by John Cleese. I ask him what he’s doing in my soul, and he explains that the answer is contained in the mind of God, which only he is privy too, but for a small fee, some tax and tuition dollars, a couple grants here and there, and an all-expense-paid book tour with stops in Zurich and Honolulu, he can let me in on it. And he shows me God in all her greater glory, as he points out that we can make more money in Hollywood than writing coffee-table books that recycle Einstein, Bohr, Dirac, Feynman, and Wheeler. I am quickly converted, and I agree to turn my back on String Theory’s hoax and save Jessica Alba.”

But it’s not that easy, as standing in Greene’s way is Michio “king of pop-theory-hipster-irony-the-theory-of-everything-or-anything-made-you-read-this” Kaku, played by Jackie Chan. Kaku beats the crap out of Greene for alomst blowing the “ironic” pretense his salary, benefits, and all-expense paid trips depend on. “WE MUST HOLD BACK THE YOUNG SCIENTISTS WITH OUR NON-THEORIES!! WE MUST FILL THE ACADEMY WITH THE POMO DARK MATTER THAT IS STRING THEORY TO KEEP OUR UNIVERSE FROM FLYING APART, OUR PYRAMID SCHEMES FROM TOPPLING, AND OUR PERPETUAL-MOTION NSF MONEY MACHINE FROM STOPPING!!” Kaku argues as he delivers a flying back-kick, “There can be ony ONE! I WILL be String Theory’s GODFATHER as referenced on my web page!! I have better hair!”

But Greene fights back as he signs his seventeenth book deal to make the hand-waving incoherence of String Theory accessible to the South Park generation, senior citizens, and starving chirldren around the world. “Kaku! Kaku! (pronounced Ka-Kaw! Ka-Kaw! like Owen Wilson did in Bottle Rocket),” Greene shouts. “It is theoretically impossible to build a coffee tables strong enough to support any more coffee-table physics books!!!”

“Time travel is also theoretically impossible, but there’s a helluva lot more money for us in flushing physics down a wormhole. Nobody knows what the #&#%&$ M stands for in M theory ya hand-waving, TV-hogging crank!!! Get it?? Ha Ha Ha! We’re laughing at the public! We’re the insider pomo hipsters! Get with the gangsta-wanksta-pranksta CRANKSTER bling-bling program!!”

How does it all end? Does physics go bankrupt funding theories that have expanded our ignorance from four dimensions into ten, twenty, and thirty dimensions? Do tax payers revolt? Do young physicists overthrow the hand-waving, contortionist bullies and revive physics with a classical renaissance favoring logic, reason, and Truth over meaningless mathematical abstractions? Does Moving Dimensions Theory (MDT) prevail with its simple postulate? We’ll all just have to wait!

But in the meantime, how do you think it will play out?

Will theories with postulates ever be allowed in physics again? Or will the well-funded, tenured pomo String Theory / M-Theory (Maffia-Theory) Priests send their armies of desperate, snarky postdocs and starving graduate students forth to displace and destroy all common sense, logic, reason, and physics in the academy? It must be so–for the greater good of physics, the individual physicist, and thus physics, must be sacrificed.

MDT’s postulate: THE FOURTH DIMENSION IS EXPANDING AT A RATE OF C RELATIVE TO THE THREE SPATIAL DIMENSIONS IN QUANTIZED UNITS OF THE PLANCK LENGTH, GIVING RISE TO TIME AND ALL CLASSICAL, QUANTUM MECHANICAL, AND RELATIVISTIC PHENOMENA.

http://physicsmathforums.com/showthread.php?p=788#post788

56. **Arun**
October 28, 2005

Juan R.

That is why I said one if not two Fields medals arose from string theory. Borcherds’ work, according to the blurbs, used methods from string theory.

57. **ks**
October 28, 2005

Juan, I heartly disagree that mathematics has to be reduced to euclidean style theorem proving machinery in order to be labled as maths. But this is an old discussion that can be rooted back into ancient times of old greeks who didn’t accept Archimedes as a mathematician due to his heuristic style argumentation. I do think the distinction between what is mathematics and physics in the work of Witten is small-minded hairsplitting and philistine. It does not have any cognitive value at all and is a science-political one.

By the way I do not understand Peters argument against ST that it will be perpetuated for social and psychological reasons? This is obviously true for the whole endeavour of finding a “theory of everything” that fits well into a certain academic-cultural pattern that promises high payouts of symbolic capital and intellectual influence to those who make any progress. Denying progress and deevaluating the symbolic capital of researchers will naturally force them into defense and closing the lines against attacks – whether the research program gets stuck or not. This is not very special to ST.

58. **AJ**
October 28, 2005

That is why I said one if not two Fields medals arose from string theory. Borcherds’ work, according to the blurbs, used methods from string theory.

Borcherds? I thought you meant Kontsevich. Borcherds work belongs properly to 2d chiral conformal field theory. He’s studying a kind of sigma model, but there’s no coupling to worldsheet gravity, and hence no sum over Riemann surfaces. Not very stringy. Kontsevich’s work, on the other hand, does make use of such sums; he’s actually using the CFT to make string theory computations.

Kontsevich’s work on the other hand, does actually involve integrating sigma model correlation functions over the space of conformal structures.

59. **Juan R.**

October 28, 2005

Arun Said:

"*Juan R.*

That is why I said one if not two Fields medals arose from string theory. Borcherds' work, according to the blurbs, used methods from string theory."

I agree that Borcherds' work is related to 'string theory' in some sense, but your suggestion that "*Fields medal arose from string theory*" is another exageration.

In 1998 the Fields Medal was for

W. Timothy GOWERS, Maxim KONTSEVIC, Curtis T. McMULLEN, and
Richard E. BORCHERDS.

BORCHERDS was **explicitely** awarded for work in Kac-Moody algebras and automorphic forms.

b = d·h is math

F = m·a is physics

kc, of course, math is an 'independent' discipline. My emphasis was on the link math-reality, which cannot be done without physical sciences playing the role of 'intermediary'. Paraphrasing Feynman, "*Euclidean geometry lives in a mathematical word*" aplication of Euclidean geometry to real word is an approximation, strictly speaking there is no real object following rules of Euclidean geometry.

Even if string theory was correct (it is not of course). Nothing in the physical word could be identified with a string unnless one used certain methaphysical considerations outside of pure physics.

Juan R.

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60. **Arun**
October 28, 2005

Juan,

I know nothing of the math. involved; I'm merely quoting from things like this, from a mathematical org. website:

http://www.icm2002.org.cn/general/prize/medal/1998.htm

" In his proof, Borcherds uses many ideas of string theory – a surprisingly fruitful way a making theoretical physics useful for mathematical theory."

Borcherds was awarded the medal for proving the "moonshine conjecture" which has something to do with the monster group.
Is that Kac-Moody & automorphic forms? I have no idea.

Of general interest, here is Faddeev on Witten:

http://www.mathunion.org/General/Prizes/Fields/1990/Witten/page1.html

Witten is cited for several contributions, of which item 3.
"Rigidity Theorems" is from string theory.

> 3. Rigidity Theorems : Witten [7] produced an infinite sequence of such equations which arise naturally in the physics of string theories….

61. **Arun**
October 28, 2005

http://arxiv.org/abs/math/9808136

62. **woit**
October 28, 2005

Arun,

All the work you're citing is 2d QFT, not string theory. I think AJ is right to point out that you're only really doing string theory when you integrate over the space of metrics on your surface (and sum over genera). If you're doing 2d QFT on a riemann surface with a fixed metric, that's just QFT, not string theory.

Witten's rigidity theorems come from looking at a supersymmetric 2d QFT on a torus. Borcherd's vertex algebras are basically an algebraic version of conformal field theory. Again, these are 2d QFTs, and he's not summing over the 2d metric or genus. It's certainly true that a lot of work on 2d QFTs has been motivated by hopes it will be useful in string theory (especially in the case of conformal field theory), but the things you're quoting are purely QFT results.

63. **John Gonsowski**
October 28, 2005

There would be some beauty if say it was obvious that all of physics came from something like Fibonacci numbers. Then you could say perhaps physics was coming from the most natural math. Fibbonacci numbers come from Clifford Algebra and CLifford Algebra may be the most natural math for physics, spiritual geometry, personality models, etc. The crack in the door that lets ugly things like SUSY in is that no matter what theory is right, there probably is some ugly symmetry breaking somewhere but SUSY seems too ugly. Symmetry breaking is supposed to reduce possibilities while SUSY adds a bunch of not needed possibilities before eventually reducing them.

64. **D R Lunsford**
October 28, 2005

This beauty issue is a canard. Things like GR and Dirac eqn are beautiful because they are actual solutions to standing problems, not imagined ones, moreover, they don't have the defect of overstaying their welcome when they are exhausted.

This issue isn't beauty – it's metaphysics, philosophy. The entire idea of string theory was obviously false from the first minute, because it was out of context all the way back to Democritus, and because it adopted whole cloth a discredited physical idea, Kaluza-Klein theory, also in disrespect to the tradition that says, when it's wrong, it's wrong.

-drl

65. **Kasper Olsen**
October 28, 2005

Dear Peter,

Of course I'll have to object to your objection 😉 But you should consider the fact, that there is (at least) a 6 hour difference between your continent and mine (so, now it's around 3.00 AM). I'll update my comments as soon as possible…

66. **Daniel Elander**
October 29, 2005

Regarding platonic mathematics:

The foundation of math is a messy business. Typically one starts by developing a meta theory describing how to prove theorems. Each proposition is viewed just as a typographical string of symbols, which can be manipulated according to so-called rules of inference. You start out with certain strings which you call axioms, and if you can manipulate them according to the rules of inference, eventually reaching some more involved proposition (which is just another string of symbols), you call that new proposition a theorem. The meta theory describes how all this is done. The problem is of course that the meta theory really needs a meta meta theory to be rigorous, and so on ad infinitum. Also, all mathematics, including set theory, has to be formulated with the help of the meta theory, but the meta theory actually assumes some set theory! Namely, it is talking about the set of allowed symbols. So it's really quite messy if you think about it. In the end, you may even come to the conclusion that mathematics presupposes language, in some sense. Platonism seems to me like just a suspension of whatever doubts and concerns you might have about these kinds of issues ("the mathematical truths are alive and happy in math heaven" ;-)), rather than a real resolution.

67. **Gavin**
October 29, 2005

Peter,

Thanks for mentioning my earlier comment in your update. The same distinction I made between intelligent design and string theory can also be make between string theory as a theory of particle physics and string theory as a theory of quantum gravity.

String theory as a theory of particle physics is up against very strong competition from quantum field theory. There is nothing from particle physics experiments to suggest that quantum field theory is wrong. There was a hope at that string theory might predict certain features of the particle spectrum and interactions, but that path has not emerged.

However, string theory as a theory of quantum gravity is quite another story. There is no established theory of quantum gravity to unseat; the field is wide open. LQG is in the running, perhaps other ideas offer promise as well, but none of the theories are the "evolution" of quantum gravity. They are all speculative and untested.

This brings me to my concern about the "not even wrong" criticism of string theory, which is that it will apply to *any* theory of quantum gravity. Short of a complete miracle (like detectable large extra dimensions), the Planck scale is going to be out of experimental reach for a very, very long time. Maybe LQG has better prospects that string theory, but it is probably too young to tell. Even if there are certain regions of parameter space where quantum gravity theories offer low energy predictions, there will also be large regions of parameter space with nothing but QFT at attainable energies. If experiments fails to produce direct evidence for any of these theories, they will all be able to retreat to high energies and avoid falsifiability.

So my question, Peter, is should we be thinking about quantum gravity at all? Since no theory is going to be falsifiable, should we just call the whole thing off? If looking for a theory of quantum gravity is something that we should be doing, then certainly string theory is part of the program.

I'm concerned that the attack on string theory as unscientific is going to taint the entire pursuit of quantum gravity, a pursuit that I think is worth while. String theory is not a strong competitor for a theory of particle physics, but bad player in the major leagues can be a great player in the minors. We should make sure the player is in the right league, not kick him out of the sport. String theorists certainly have confused this issue as well. Having taken the lead in quantum gravity, we act like string theory is up 3-0 in the world series of physics.

I hope that we can turn the debate away from the unproductive question about whether string theory is star or a failure and toward a consensus about where string theory belongs.

Gavin

68. **Arun**
October 29, 2005

Peter:

Whatever. I don't have a stake in this either way. If Faddeev and Atiyah say that Witten derived something from string theory in their talk felicitating Witten on his Fields Medal, and AJ and you say its not string theory; or the Mathematical Congress puts out a press release saying that Borcherds used methods from string theory (the press release also says that string theory is controversial among physicists, so they weren't trying to get on a bandwagon) and here folks say nay, then I'll leave it for the historians of science to figure out who is correct.

Perhaps the mathematicians have a different understanding from physicists of what string theory is, and maybe it is the wideness and imprecision about what string theory is that is contributing to our collective irritation with it.

69. **Arun**
October 29, 2005

BTW, while I don't have a stake in the discussion, I do appreciate it. How else would I learn the fascinating fact (Borcherds, 1992) that "The monster Lie algebra is the simplest example of a Lie algebra of physical states of a chiral string on some orbifold."

70. **woit**
October 29, 2005

Hi Arun,

Lots of people claim that 2d conformal quantum field theory results are string theory results, and this is one of the main reasons mathematicians are so impressed by string theory. Personally I think this completely muddies important issues, and that people should be careful to keep straight what is really string theory and what isn't. Others undoubtedly disagree.

71. **Juan R.**
October 29, 2005

Gavin

String theory NEVER was a valid theory of particle physics. Never wad a real competition! Physical states in string theory are *supersimmetric masless* states. There is not such one thing is the standard model of particles.

Of course, all current models of QG are speculative because experimental QG does not exist still. However, string theory is not comparable to rest of alternatives. In general, the rest of alternative rely on 'direct' modifications of the already known. String theory is based in an unending introduction of speculations and unobserved things; with each new failure -since 40 years ago, experimental disproval, etc, string theorists introduce some new unobserved thing claimed to be fundamental.

Moreover it is not clear that any quantum gravity theory cannot be falsifiable. Remember that the scale of the string in string theory is introduced *by hand* and choosed to be the Planck scale. Perhaps some QG effect can be detected at usual but high energies in other formulations or perhaps QG was UNOBSERVED. In fact, this was the point of Dyson some time ago. He argued that gravitons may be unobservable and therefore any QG are outside of physics.

Is he right? I still unknow.

People does not attack string theory 'as unscientific'. In fact, this is the reason that other approaches to QG are NOT usually atacked. That people attacks is the arrogance of string theorists and premature -wrong- claims by guys like Kaku, Greene, Witten, Schwartz, etc.

People attack heavy marketing for public, loop theoreticians attack when string theorists claim in public that string theory is the ONLY approach to quantum gravity. Honest physicists attack books as "The Elegant universe" by Greene, Profesors attack the attempt to giving courses on string theory to students of physics, etc.

People attack the lack of honesty of many string theorists, who popularize string theory hidding all of its flaws. In fact, in personal communications and talks string theorists agree that string theory is not now a theory of strings -in M theory the D0 brane appears to be fundamental- but public still beoieve thst string theory claim that universe is done of small vibrating strings.

If you ask to string theorists why the name string theory is maintained in popular books and magazines they reply "*because marketing purposes*".

Juan R.

Center for CANONICAL |SCIENCE)

72. **Nigel**
October 29, 2005

'If you ask to string theorists why the name string theory is maintained in popular books and magazines they reply "because marketing purposes".' – Juan R.

I sadly agree. Marketing is now deemed to be the problem of adapting a product to fit the customer.

73. **David Corfield**
October 29, 2005

Without wanting to be harsh on anyone, some philosophical input may be timely.

AG writes:

It is notoriously difficult to draw the Popperian demarcation line, but surely there is a fundamental difference between string theory and intelligent design. When embarking on a scientific research program, it is not always possible to tell from the start what falsifiable predictions or practical applications might emerge, and theoretical scientists have to go on well-formulated hunches and search for ways to corroborate their hypotheses; nonetheless, their efforts can still be called science. Intelligent design, however, is not science– not because it cannot be falsified, but because it cannot, even in principle, be corroborated either. How can an ID proponent move forward with his "research program???

For Popper, corroboration is the survival of an attempt to falsify, so you cannot have the possibility of one without the other. Popper doesn't develop a notion of positive evidence until his later rather ugly theory of verisimilitude. This is what marked Popper off from the logical empiricists who saw the need for a logic of confirmation. But AG is using the language of someone you generally teach after Popper, namely Imre Lakatos. The last sentence I quoted is thoroughly lakatosian. A program has a heuristic spirit. It isn't fully worked out. If it encounters problems, it needs to have the resources to move in progressive direction. It mustn't make *ad hoc* moves by, say, artificial exclusions of events from falling under a law, or unnatural borrowings from rival programs.

Lakatos is also interesting for adapting and welding ideas from Popper and the mathematician George Polya (with a sprinkling of Hegel). Polya had developed a probabilistic (Bayesian, without using the word) logic of confirmation for mathematics. Lakatos takes on a case suggested by Polya, the V-E+F=2 formula for polyhedra, and argues that mathematics is 'quasi-empirical'. He would disagree with:

Subhash wrote:

Has it been falsified, or not falsified, in mathematics, the statement that two parallel lines shall never cross? You've got to know neither case is right or wrong.

There is no empirical truth in mathematics. All math are logical derivations from a few fundamental rules which we probably took for granted, but which do not need to be taken for granted as truth, like the parallel line hypothesis. As such, all you could ever say is certain statement is either consistent, or inconsistent, with the set of hypothesises that your math is based on. There is no absolute truth.

Lakatos's idea was that Polya had been right to find a strong parallel between mathematical and scientific activity, but had erred in thinking that what was common was probability-based inductive reasoning. Instead, Lakatos suggests that what's common is a passage from certain observed facts to an explanatory framework for them, driven by the proposal of theories and the provision of counter-examples. The important point is that the language changes through this process. In the case of the Euler conjecture, the term 'polyhedron' is transformed from an imprecise, intuitive term to a mathematical definition. This courts the danger that something of the original intuitively understood domain may fail to be captured. About Subhash's mention of parallel lines, a Lakatosian story would tell the history of mathematical conceptions of space. If progressive, we're achieving an ever less partial understanding of space. This is clearly unfinished business. Read the second half of Pierre Cartier's great article '

A Mad Day's Work'. Any comments about my own attempt to move beyond Lakatos, explained in a paper 'How Mathematicians May Fail to be Fully Rational' (link form my webpage), would be welcome.

Peter wrote
My own inclinations are kind of hyper-Platonist, thinking that ultimately we will see that the deepest mathematical structures and the deepest physical structures are very closely related.

The closest to a modern philosophical account of this is Roland Omnes' 'Converging Realities'. Omnes (a physicist) calls it physism.

David

74. **Nigel**
October 29, 2005

David – I've read some of Lakatos' views which I quote on my webpage, namely his essay in a 1974 Open University book "Philosophy in the Open". He is critical of Popperian critical experiments and of Kuhnian revolutions because both always "turn out to be myths" caused by historical revisionism. Thus, caloric, phlogiston, and ether died out as their proponents died, not in a blinding flash of revelation when people like Priestley did experiments. Still today the FitzGerald 1889 interpretation of the Michelson-Morley experiment is heresy and cannot be discussed because it is actively defended by a group of crackpot dissenters.

While I respect Lakatos for pointing out that Popper's and Kuhn's views are total nonsense, I fear he is simplistic himself in claiming that revolutions occur when "progressive research programmes [British spelling – sorry] replace degenerating ones". In the case of string theory, which ultimately stems to Kaluza 1919, the whole speculative endeavour has been sustained by propaganda and make-believe, and people have done very well out of it. Is this progressive or degenerative? A-level uptake of physics and maths is falling off in the UK, but that just leads to demands for more "popularisation of string theory" to make the subjects more appealing. With no real rival, string theory will run physical science and maths into the ground, without "degenerating" in the despondent way Lakatos imagines.

Therefore, string theory need never collapse. When Peter's book "Not Even Wrong" appears next March, will ST collapse? No! Basically, Peter is doing the ST lobby a favour by pointing out to them areas which need to be dressed up more effectively, or worked on harder. His own ideas like http://www.arxiv.org/abs/physics/0102051 have not caused much impact yet: he says a good way to advance the standard model is SO(4) spinors and Clifford algebras, and analysing path-integrals. When you dig into the complexity of this, it is as technical as string theory but is in the opposite direction. It is also more realistic, and therefore further from science fiction of parallel universes and M-theory.

Is string theory going to shut up shop next March when his book "Not Even Wrong" comes out? I doubt it. Personally, I'm impatient with string theory. Not because I don't like 6 dimensional manifolds or vibrating strings, but because they are not building on what is known. If it turns out there is some evidence for any of this, OK, it would deserve some allocation of research. But it is just crackpotism, and then these people dismiss others who are struggling without any grants in bad conditions as being "crackpots". They have no shame but too much egotism.

75. **Nigel**
October 29, 2005

Whoops, the Woit ref I meant was http://www.arxiv.org/abs/hep-th/0206135 'Quantum Field Theory and Representation Theory: A Sketch'.

76. **A.J.**
October 29, 2005

Arun: Go read Borcherd's original paper. http://math.berkeley.edu/~reb/papers/monster/monster.pdf
He does say string theory in several places: He's studying a vertex algebra which describes the chiral states of a sigma model to an orbifold, and he's using a no-ghost theorem that was originally proved by string theorists who wanted to think of the sigma model's Fock space as the space of single string states for some string theory.

But Borcherds is not making any essential use of the string theory picture. He never needs to think of the string as interacting. So saying his result is inspired by string theory is true in a sociological sense, and it's also true in a rather weak technical sense: It's like saying a result in relativistic quantum mechanics is a result in quantum field theory.

I'm about done with this topic.

77. **ks**
October 30, 2005

In the case of string theory, which ultimately stems to Kaluza 1919, the whole speculative endeavour has been sustained by propaganda and make-believe, and people have done very well out of it. Is this progressive or degenerative?

In case of ST it resided a niche i.e. it did not have strong competitors to subdue. Lubos insistence in ST to be the only attempt of a unified theory reflects this (of course he is well aware about LQG but asserting that it is not a competitor is part of the same rhetoric). From Poppers analysis we can conclude that ST will wiped out only in presence of a better approach (simpler, more concise, fruitfull and falsifiable). From Lakatos we can conclude that history will be written and re-written by the victors who present a rational reconstruction (rationalisation of their victory).

Peter et al describe the crisis of ST, it's inability to return to the real. But in absence of a valid theory it might be the only possibility to survive in theoretical physics – as abstract art. Some find it beautifull.

78. **Thomas Larsson**
October 30, 2005

Arun, AJ, Peter:

The physically successful application of 2D CFT is in the theory of 2D phase transitions. This is part of condensed matter physics and has nothing to do with strings as a theory of quantum gravity. It is true that this discovery was made mainly by string theorists, who happened to have access to the relevant mathematical machinery. However, string theory is not a ToE even in statistical physics – it says everything worth knowing about 2D phase transition but nothing about the physically more relevant 3D case. In fact, this was my original motivation for generalizing stringy mathematics to higher dimensions, which led to my discovery of multi-dimensional Virasoro algebra and its representation theory.

Gavin:

Whereas string theory is not a successful theory of quantum gravity in 4D, there is no doubt that the worldsheet theory of the free string correctly describes gravity coupled to scalar fields in 2D. The most striking thing from this viewpoint is that gauge components of the metric becomes physical after quantization. This manifests itself in different ways: the Hilbert space in lightcone quantization cannot be reduced due to the conformal anomaly, or one must explicitly keep the trace of the metric (the

Liouville mode) as a dynamical field.
Analogously, one may expect that the metric field in 4D has more components after quantization than the two transverse graviton polarizations, due to diffeomorphism anomalies. This was another motivation for my generalization of stringy math beyond two (or rather one complex) dimension.

79. **David Corfield**
October 30, 2005

Nigel,

Your points are well taken. I think we must remember that Kuhn and Lakatos were in the business of describing historical cases of one paradigm/programme (I'm British too) superseding another, events seen as rational with hindsight. Leaving aside the point that Lakatos's own rationally-reconstructed 'histories' are often very far from historical reality, of the two Lakatos was more interested in isolating the rational component of the decision, and at one point he hoped that this could help with decision-making in current science, but by the end of his career he'd given up on the idea that conditions could be given which would rationally warrant one switiching programme. Instead, he just insisted that any scientist should be honest enough to acknowledge where his programme was on a scale running from degenerate to progressive.

When you say, "While I respect Lakatos for pointing out that Popper's and Kuhn's views are total nonsense, I fear he is simplistic himself in claiming that revolutions occur when "progressive research programmes [British spelling – sorry] replace degenerating ones?? ", pointing to string theory as a counter-example, he could reply in a number of ways. First, that rational change might need to be judged over lengthier periods of time. Nowhere does he say that progress must occur in a given period. Further, he could agree with you that ST is in a (permanent) degenerate phase. For the latter he would want us to judge how ST is doing according to his 3 criteria of progress: (1) Is it making new predicitions?; (2) Are these being confirmed, or can it explain already observed phenomena for which ST was not designed to explain; (3) when it encounters problems, does it engage in problem shifts leading to theoretical devleopment in the spirit of the programme.

I guess you'd argue that ST was not progressive according to these criteria. You might also same the same about other candidates for QG. I don't recall Lakatos suggesting what one should do in terms of institutional support in this situation. Presumably there are approaches which haven't been given enough support to reveal their potential. All he suggest is honest score-keeping.

I think there are problems with Lakatos, as I outline in the paper I mentioned. There I looked at the ideas of Alasdair MacIntyre. He would suggest that given an acknowledgement of weakness/resourcelessness of their own programme, some of its members should be encouraged to go and learn the languages of other programmes, as second first langauages, to see if they have the resources to understand these weaknesses. Perhaps this is one small piece of advice we philosophers can pass on.

David

80. **Nigel**
October 30, 2005

David,

Thank you for this detailed reply. I saw a couple of weeks ago a piece in New Scientist by Appleyard, where the historical revisionism in science was blamed for Marxism and Nazism, because Marx used a cranky scientific analysis of history to defend his ideas, while Mein Kampf utilised crackpot genetic theory. It seems absurd that science is to blame for Hitler and Stalin via crackpot official science. I'd better not push my luck here by mentioning the ST propaganda book "Warped Passages"… perhaps that will be used to justify future warped ideas.

What I'm curious about is what Peter intends doing to sort out physics. He says SO(4) can link with U(2) to produce the generations of leptons, etc., in the Standard Model. How far can this approach go toward replacing ST research?

81. **woit**
October 31, 2005

Gavin,

Thanks for another interesting comment. My own view on quantum gravity research is that it is worth doing, but needs to be done differently than most of the rest of physics, precisely because there is now no experimental data, together with the real possibility that there won't be any in this or even the next century.

Without experimental data to keep a field honest, research needs to be conducted more like in mathematics, with an emphasis on being completely precise and clear about what you have and what you don't have. If you have experimental results, you can afford to string together (that was an unintentional pun…) a bunch of not very solid arguments and see what you get. If you get something that agrees with experiment, you're on the right track, if not you try something else. I personally don't see the point of stringing together a lot of dubious arguments, and at the end claiming you've reached the holy grail of a quantum theory of gravity.

So, I think pure quantum gravity research is legitimate, but it really needs to be done with a higher standard of precise thinking than is usual. There's also a very serious danger that in the end one will discover that one has found an infinite array of different "quantum gravities", and, without any experimental way of telling them apart, the whole thing becomes not that interesting.

To me the "holy grail" of quantum gravity is not finding a consistent quantization of the classical theory, but finding some sort of useful relation between the (pseudo)-Riemannian geometrical degrees of freedom of GR and the geometrical degrees of freedom (gauge fields, spinors) of the standard model. If one could do that, one might be able to even make predictions about particle physics, in which case whether or not one had a rigorously consistent way of "quantizing gravity" wouldn't be the most important thing.

Put differently, I think the real problem is how to unify GR and the standard model, not so much how to unify GR and the general principles of quantum mechanics. Of course the latter may be a good thing to think about to get an idea about the former.

82. **Jack Sarfatti**
October 31, 2005

"String theorists are sincerely interested in extending known and tested science. Intelligent design "theorists?? are interested in undermining the public's faith in known and tested science."

This is false. While you can find SOME fundamentalists who say things like that, it is not true of people like Tipler & Barrow, or Fred Hoyle (his book "The Intelligent Universe" (1986). Remember Hoyle predicted a nuclear resonance (I think in Carbon) using WAP. So WAP is falsifiable. WAP of course is not enough for ID. I don't mean to suggest that.

83. **Juan R.**
October 31, 2005

Woit,

I completely agree with you that in absence of experimental data, research would be more careful and far from the media popularization until solid results are achieved. In fact, this was more the program of rest of quantum gravity researchers -except exceptions-. due to marketing and popular presure loop theoreticians began to popularize his own views.

It was a true scandal that a string theorist began one talk or a article on a popular magazine with the typical "string theory is the only approach to quantum gravity…"

But the question is: **is quantum gravity testable?**

Nobody know the reply still. *A priory* one may wait relevant phenomena at Planck scales, but this is not clear.

It is posible that in some configuration very small effect can cause a big measurable effect. I sincerely think that particle physicists continue thinking in the old 'linear' manner:

small cause ==> small effect

which is not true.

The best example are PV effects. ALL particle physicists -including important ones as Weinberg- claimed in the past that parity violating effects were so small that would be unimportant for all chemistry purposes.

Yes, they are very small, but chemistry is mainly a nonlinear discipline and some molecular configurations *amplify* those initial ultraweak effects even at level of being measured by us. Particle physicists were wrong and electroweak quantum chemistry has arised as a proper discipline.

Sincerely, i wait some kind of 'nonlinear' phenomena doing posible some quantum gravity effect visible at available high energies. Of course, i have **not** proven this still.

About your own insight on the "holy grail?? i think that you are in the wrong way.

1) By definition, quantum gravity is a quantization of classical gravity.

2) The Riemannian geometrization of GR is NOT fundamental (in fact, the Cartan extension proves that a pure Riemann geometry is not suficient and teleparallel gravity thought us that the traditional GR spacetime curvature view is a mere 'mathematical' artifact)

3) Far from current claims about success of the standard model i would solicit a bit of caution about the correctness of the model. In fact, recent published research in EM suggest existence of experimental data cannot be explained via traditional EM theory. Some recent research in this topic -e.g. Weber electrodynamics or mixed approaches- suggest that QED is NOT correct.

Recent research by Hoyle/Narlikar theory suggests that standard model is not completely correct. For instance, one of most fascinating outcomes of Hoyle/Narlikar theory is that the photon does not really exist, and, however, Hoyle/Narlikar EM reproduces all experimental effects of traditional QED and BEYOND. It appears that some extensions also explain recent experimental data on longitudinal EM forces, Marinov motor, railguns explosions, and some other experimental data -it is posible that some discrepancies on tokamaks reactors was also due to failure of usual EM, QED, and QCD theory (there is some relevant literature on this also published)-.

It has been also published some recent discrepancies of QED in chemistry [Radiation Physics and Chemistry 71 (2004) 611–617]

However, this may also suggest that the current
perturbative expansion is showing some limitations, or
that the renormalisation approach needs reinvestigation.
Of course, it is also possible that the approach of QED
has some particular flaws or limitations which experiment
might be beginning to illuminate.

4) It is not still clear that GR was the correct approach to gravitation even at the classical level. Many of GR tests are also passed by rival theories (e.g. FTG). And as emphasized by David Gross in one of his recent talks,

http://qd.typepad.com/24/2005/01/the_future_of_p.html

"does GR work for strong fields?"

5) The main problem of formulation of a quantum theory of gravity is that GR is clearly incompatible with QM. Therefore a direct -consistent- unification with the standard model is NOT possible. In fact, there is a large history in 'direct unification attempts' begining from Kaluza-Klein that you may know better than my. so far i know all direct atemps were abandoned.

Juan R.

Center for CANONICAL |SCIENCE)

84. **Ranger**
October 31, 2005

Does string theory have any postulates or laws?

85. **Urs Schreiber**
October 31, 2005

> Does string theory have any postulates or laws?

Perturbative string theory does. The postulate is that the S-matrix is computed by summing correlation functions obtained from any one 2-dimensional superconformal field theory of central charge c=15 over genera (in a way that follows from a 2D gravity point of view).

The study of this postulate has shown that it has precisely all features expected of the perturbative expansion of a nonperturbatively defined theory. The totality of the latter is still unknown, though the existence of several hints of properties of this theory has made people come up with the working title 'M-theory' for it.

86. **Nigel**
October 31, 2005

Urs, just because you have postulates or laws does not mean you have science. Moses wrote down postulates and laws from God, but that was intelligent design, not science. Your belief in ST is irrational and pseudoscientific. Who told you 11 dimensional M-theory is the best way forward? Some authority figure I suppose?!

87. **Urs Schreiber**
October 31, 2005

> Your belief in ST

I don't believe in anything. I just answered Ranger's question. (Which for some reason appears below my reply.)

88. **Peter**
October 31, 2005

Sorry about that, WordPress is too stupid to keep track of Daylight savings time. My fixing this around noon today messed up the order of comments a bit.

89. **Gavin**
October 31, 2005

Peter,

I agree that many theorists have a "if you don't believe me, just do the experiment" attitude that is not appropriate in a field where experiments are not on the horizon. Can you point me to some quantum gravity research that does not suffer from this illness?

I'm also intrigued about the idea of "finding some sort of useful relation between the (pseudo)-Riemannian geometrical degrees of freedom of GR and the geometrical degrees of freedom of the standard model." However, I'm not quite sure what that means. Can you point me to any work being done in this area?

I am curious about these things because I am looking for a research direction myself. I earned my Ph.D. in 1999 and spent the next several years as at-home dad while my wife launched her career as a doctor (a path that seemed more practical than string-theorist). My son is in school now, so this summer I went to Strings05 and a couple weeks ago I was at Stanford (my alma mater) learning about what they are doing there. I learned that in six years away from the field, I didn't miss much.

Bad as things are for string theory, it is hard to see much else in the quantum gravity race that looks better. People wanting experimental results are hoping for a miracle. People who want rigor can't prove that Yang-Mills exists. People who want simplicity have to wave their hands just to find flat space. Quantum gravity is an exciting problem, but I can't find a strong horse to put my money on. If I shouldn't bet on string theory, then I should probably walk away from the race.

Gavin

90. **Peter**
October 31, 2005

Gavin,

The kind of thing I had in mind as quantum gravity pursued with an insistence on precise formulations is a lot of the things I've seen coming out of LQG, some of which are really rigorous mathematics. For a random example look at some of the papers of Thomas Thiemann.

I said I think the problem of relating the geometry of GR and the standard model is the big problem, I didn't say I have any good ideas about this… One I wrote about long ago involved trying to understand the electroweak U(2) as a subgroup of the Euclideanized Lorentz group. Some day I'll get around to working on this idea again and trying to come up with a more sensible version of it.

To my mind there is no really strong horse to put one's money on in the field of quantum gravity. More interesting things seem to be happening in LQG than in string theory these days and the subject is being pursued in a much healthier way than string theory, so if you want to do quantum gravity, LQG seems to be a more promising direction. There's also no strong horse for the other huge problem: electroweak symmetry breaking. There at least, maybe in 2008 they'll be new experimental results, but if one wants to think about this during the next few years, you have to be willing to deal with the situation that there aren't any good ideas around.

91. **Juan R.**
November 1, 2005

Urs Schreiber, perturbative string theory is ill defined and is incompatible with GR.

The scattering processes do not correspond to anything known of our world, AND, in complex disciplines like chemistry, the trivial S-matrix theory does not work. See

PRA 1996, 53(6) 4075-4103

Of course, ST S-matrix is not exactly equal to QFT S-matrix but above reference continues to hold.

As would i explain that string theory is both wrong and outdated?

Juan R.

Center for CANONICAL |SCIENCE)

92. **Kea**
November 1, 2005

Juan

Once you said that physicists were guilty of grossly underestimating the sophistication of chemists, and others, in their attempts to advance their understanding. Are you not being rather hypocritical?

93. **Gavin**
November 2, 2005

Peter,

The recent papers by Thiemann are too focused to be accessible to me at this point. However, they do point to a couple recent reviews, "Background independent quantum gravity: a status report," by Ashtekar and Lewandowski and "An invitation to loop quantum gravity" by Smolin. I also found a recent critical review, "Loop quantum gravity: an outside view" by Nicolai, Peeters, and Zamaklar. I hope that these in combination will be a fair introduction to the subject.

Your last post brings us back to the original topic of this thread. We agree that "there is no really strong horse…in the field of quantum gravity." Therefore, it seems very premature to declare any of the contenders "moribund." None the less, your informed and well reasoned assessment of the quantum gravity field has been extremely helpful to me. I wish that all of the discussion of this challenging topic could be carried out with the civility I found here.

Gavin

94. **Juan R.**
November 3, 2005

Kea,

Nature is multidisplinar. If you want understand nature you cannot do only physiucs. This is the reason of existence of Institutes as Santa Fe of Center like our.

It is not the paper in Physical Review A i cited in Woit weblog an elegant example of how ideas from chemistry -Prigogine group- have been applied to a generalization of QFT?

Remember P.W. Anderson's words in his famous *Science* article:

Biology is not applied chemistry.

Chemistry is not applied physics.

Juan R.

Center for CANONICAL |SCIENCE)

95. **Tony Smith**
November 3, 2005

Peter, I think that the question "Why is string theory science but intelligent design isn't???
is unfairly disparaging to "intelligent design".

I am NOT an advocate of or apologist for "intelligent design", but I note that "intelligent design" seems to be something that certain groups have adopted to replace "creationism" because they found it hard to defend "creationism".

In my opinion, the current landscape/anthropic/superstring theory is actually very similar to the "creationism" that has been abandoned even by the "intelligent design" people.

I use as a definition of "creationism" the doctrine that the world and everything was created in 4004 BC with all the fossils, stars, ratios of radioactive elements, etc being carefully set up by G-d to produce exactly what we observe today.

The "landscape" of possible superstring models
corresponds to
the set of all possible states of the world and everything as of 4004 BC.

The anthropic principle that selects "our world" out of all the possible "landscapes"
corresponds to
the divine creation by G-d that selected/constructed "our world" instead of any other of all the possible states of the world and everything as of 4004 BC.
Both are quite vacuous in that they say no more than that our world is as it is because it is as it is.

The landscape/anthropic/superstring stuff and the divine creation stuff both exactly describe "our world", and each is equally useful (i.e., totally useless) in making predictions (or even interesting postdictions) of particle physics data.

However,
since "intelligent design" implies some use of some intelligence by G-d or something
and
since no substantial intelligence whatsoever is required for landscape/anthropic/superstring stuff or "creationism" stuff,
I think that "intelligent design" is far superior to both landscape/anthropic/superstring stuff and "creationism" stuff.

Even so, for the record lest I be misunderstood, I should reiterate that I am NOT an advocate of or apologist for "intelligent design".
In other words,
I am not saying the "intelligent design" is good.
I AM saying that BOTH landscape/anthropic/superstring and "creationism" are EVEN WORSE.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

96. **Ranger**
November 4, 2005

Are postulates allowed in modern theoretical physics?

It seems that string theory has no postulates.

Correct me if I'm wrong but it seems that these are the rules of contemporary theoretical physics:

1) postulates are not allowed
2) experimental evidence is not necessary
3) only theoretical research programs that disallow posulates and experimental tests are allowed, as said research programs are best suited to greasing the perpetual-motion NSF money machine.

With no hope of proof one way or the other, and with nothing to prove in the first place, string theory is firmly ensconced as the anti-theory.

I hope I'm wrong, but this seems to be the case.

Please name a postulate or law of string theory.

Or just give me an equation.

Newton, Einstein, and Faraday all gave us postualtes, laws, and equations, and their physics received a helluva lot less funding than string theory.

so what's going on here?

97. **D R Lunsford**
November 4, 2005

Hi Ranger,

I have lots of equations for you! Highly non-linear! Fun for years! Gravity, charge, and light!

-drl

Cosmological Frontiers

October 31, 2005
Categories: Uncategorized

Last week the Perimeter Institute ran a Workshop on Cosmological Frontiers in Fundamental Physics and someone wrote in to point out to me that the talks are now available on-line. Much of the workshop was about mainstream cosmology, especially the more speculative ideas about inflation and what signals might be found in the CMB.

The particle physics component was heavily weighted towards Landscapeology (talks by Denef, Kachru, Kleban), with a workshop-ending talk entitled “50 Years since the LHC” by Nima Arkani-Hamed. Arkani-Hamed’s talk was supposed to be a prediction of what things would look like 50 years after the LHC, and he ended it with the prediction that “the LHC will put the last nail in the coffin of mono-vac theories” and that “the Landscape will be with us to stay”. Along the way he went over various possibilities for what the LHC might see and their implications for whether the cosmological constant and the weak scale are anthropically determined. He said that he believed the cosmological constant was anthropically determined, and half the time he thought the weak scale was too, the other half of the time he thought it wasn’t. He argued for giving up on coming up with new mechanisms for electroweak symmetry breaking since “working on the (N+2)nd variant on the (N+1)st model of EWSB is not worth it”, saying that instead people should participate in the LHC Olympics and work on the “inverse problem” of figuring out from LHC data the 105 parameters of the MSSM or some other model of beyond standard model physics.

Near the beginning of his talk he gave a graph representing (as a function of time) the average string theorist’s view of the probability that string theory could be used to calculate standard model parameters. This started out near 1 in 1985, dropping to a small number in 1995 after the duality revolution showed that strongly coupled strings didn’t get rid of the wide range of possible backgrounds, and further dropping to “a number close to the fine tuning of the cosmological constant” in 2000 after the advent of the Landscape and the non-zero cosmological constant. The same graph also included a plot for the views of “clueless popularizers and science journalists”, which had only recently started to head down slightly from 1. He claimed that such people are ten years behind the times and that it is “not going to be pretty” when they catch up with the current views of string theorists and realize the theory can’t predict anything.

While at Perimeter, (according to Lubos Motl) Arkani-Hamed was talking to LQGer Laurent Freidel about Doubly-Special-Relativity in three dimensions. It’s going to be a lot of fun to watch what Lubos has to say if and when some of his senior colleagues start working on LQG, something that seems entirely possible since string theory is now moribund, whereas LQG is in a much livelier state.

Landscapeology still seems to be making headway at taking over particle theory and ensuring that it becomes a pseudo-science. If you’ve got $250 and can get to an access grid facility, next week you can participate by video conferencing in a workshop at Ohio State on Strings and the Real World, which will have one day out of three devoted to the Landscape. I would have thought the title of the workshop would get into trouble with false advertising laws, since one thing that is clear is that absolutely none of the talks will be even slightly relevant to the real world.

For a horrific vision of where particle theory is headed, check out the website of the String Vacuum Project. The idea seems to be to get particle theorists spending their time developing software to do numerical computations searching amongst the infinite variety of the Landscape to find something or other. The section of the web-site on the connection of any of this to real particle phenomenology remains to be written.

Comments

1. **D R Lunsford**
   October 31, 2005

   Heat death via mathematicians.

   May they expand forever into nothingness!

   -drl

2. **Arun**
   October 31, 2005

   When the money and public support runs out, unemployed physicists can start landscaping businesses – “Calabi-Yau lawn service”, and so on. I’m sure there are some catchy names to be had.

3. **Quantoken**
   November 1, 2005

   No no no. How could they do landscaping business. They have too many choices for customers, 10^500, and they have not built a single landscape yet.

   The money will run out **much sooner** than one expects. There is already a US Congressional House Resolution about this huge paradigm shift. See also Apollo Alliance.

   What these string theorists can do, is pitch the research on **dark energy** in hidden dimentions as a viable research on sustainable alternative energy source to replace fossil fuel. And there will be huge fund for it. Not that I believe in these stuff. But people like Kaku have proven themselves to be good salespersons and good advertisers.

   Quantoken

4. **hack**
   November 1, 2005

   This is an uplifting post. I’ve never felt better about leaving physics behind.

5. **logopetria**
   November 1, 2005

   “[Arkani-Hamed] said that he believed the cosmological constant was anthropically determined…”

   Can I just check what people like Arkani-Hamed mean by this? To say that some parameter X is ‘anthropically determined’ means that out of lots of different worlds (or universes), each having a different value of X, we know that only those with particular values of X will be habitable – and so obviously that’s what we observe. Is that right?

So, for example, the Earth's distance from the Sun is 'anthropically determined', because there are other planets, some are closer to their stars, some further away, and life will only arise on those that are at roughly the right distance. So 'anthropic determination' is a statistical selection effect (selecting from a large, already existent set of possibilities), rather than anything resembling a *mechanism* for fixing a parameter to a value?

6. **Nigel**
   November 1, 2005

   "The idea seems to be to get particle theorists spending their time developing software to do numerical computations…"

   Monte Carlo methods and even just numerical integrations of hard to solve anayltic functions are fun. Also, why not just fit a wave equation to the group behaviour of particles (molecules in air) and talk sound waves? Far easier than dealing with the fact that the sound wave has an outward pressure phase followed by an equal under-pressure phase, giving an outward force and equal-and-opposite inward reaction which allows music to propagate. Nobody listens to music, so why should they worry about the physics? Certainly they don't listen to explosions where the outward force has an equal and opposite reaction, too, which in the case of the big bang tells us gravity. Far better to stick to horseshit computing.

7. **Chris Oakley**
   November 1, 2005

   Hi logopetria,

   Right.

   Anthropically determined = Not determined by our theories.

   Landscape = We give up. Tell us, God, how *does* your universe work?

   Spacetime has N dimensions, where N > 4 = Our theory doesn't work, but we don't want to admit it.

   (You get the general idea).

8. **Dissident**
   November 1, 2005

   Speaking of moribund string theory, I assume everybody's noticed that Clifford Johnson over at comsmicvariance has now given undead theory a face:

   http://cosmicvariance.com/2005/10/30/chewing-things-over/

9. **Juan R.**
   November 1, 2005

   In standard particle physics exists something called vacuum. In the opinion of some of us that 'vacuum' is irreal and NEWER experimentally verified. This is easily proven via a number of recent published advances (e.g. Rev. Mod. Phys. 1995 67(1) 113-155). Then what play the role of the traditional vacuum of QFT? Hoyle/Narlikar theory explains us that the vacuum is really the 'thermal-bath' generated by the rest of the universe. Hoyle/Narlikar theory is a generalization of Wheeler/Feynman absorber theory.

   In Hoyle/Narlikar theory, a single 'quantum vacua' means a single environment. Since there is only a single environment in the observed universe, i see difficult how one could claim the existence of multiple vacua!

   About anthropy insanity, the best criticism i found on this topic was from M. Gell-Mann. I do not remember exactly the quote -i think that i read in his *The quark and Jaguar* book- but was some like

   I newer found a version of the anthropic principle that was not trivial or absurd

   P.S: Hoyle/Narlikar theory has been already extended -in press- for accounting some recent work in experimental failures of standard field theory and further mathematical research

   see PRE 1996 53(5) 5373, Phys. Lett A 1990, 146 (1,2) 6, etc.

   Juan R.

   Center for CANONICAL |SCIENCE)

10. **Kyle**
    November 1, 2005

    Why would this post make you feel better about leaving physics, Hack? It isn't as if HEP or theoretical HEP is particularly important to physics anymore.

11. **Chris**
    November 1, 2005

    It is difficult to avoid the conclusion that Arkani-Hamed and many theoreticians of his generation are primarily technicians. They have mastered a huge array of sophisticated formalisms and techniques, and they want to exercise them.

    "So, we have a MSSM with 105 free parameters? Great, get us some data, and we'll fit them to it. String theory leads us to the Landscape? Don't worry about it, and certainly don't waste time reflecting on what it means; get on with developing techniques to explore and characterize it. We impressed our elders and established our careers by getting good at this stuff, so let's keep doing it and expand our repertoire."

    That's what theoretical particle physics is becoming, five years into the 21st century. The bigger the labyrinth the better; it just gives everybody more to do.

12. **Adrian Heathcote**
    November 1, 2005

    This anthropic stuff continues to baffle me and I feel a little bit like Gell-Mann.

    Isn't the point that it is simply an example of our being able to rule out some theories because they will conflict with a pretty obvious fact about the universe, namely that we are here to make observations and theorise. It's a crude prediction that any true theory must make.

So the AP is just a coarse sieve to winnow out obviously false theories.

Which makes it baffling: how have people been able to invest it with an almost mystical significance? And why did anyone swallow it in that form?

cheers

13. **Syksy Rasanen**
November 2, 2005

Peter, what did you think of Burt Ovrut's talk on building realistic models in string theory?

14. **woit**
November 2, 2005

Adrian:

"Which makes it baffling: how have people been able to invest it with an almost mystical significance? And why did anyone swallow it in that form?"

Good questions. I'd claim it's because the string theory unification program has failed, but its proponents refuse to admit this. With a theory on their hands that can't predict anything, they've turned to anthropism in desperation to avoid admitting failure.

Syksy:

I didn't listen to the whole talk. Every so often I try and check out what progress has been made on this problem of getting the standard model. The kind of thing Ovrut is talking about has been pursued by many, many people for 20 years. My understanding of the current state of affairs is that they can, through a rather complicated construction, get something which at low energies is just the MSSM and lots of moduli fields. The problems with the moduli fields are well known. Their methods don't seem to allow them to calculate a single one of the 125 or so parameters of the MSSM, or predict anything about supersymmetry breaking. This all looks to me like what you expect to happen if you pursue a wrong idea: by complicating things enough you can reproduce some of the gross features of nature, but no matter what you do you can't come up with a real prediction, or even a convincing postdiction.

15. **logopetria**
November 2, 2005

"[String theorists have] turned to anthropism in desperation to avoid admitting failure."

This is something I still don't understand. As Adrian (implicitly) asks, what is there to the "anthropic principle" aside from "agreement with particular observed facts"? How does the fact that the universe is habitable rule out any theory, except to the extent that the theory has already failed to agree with other observations (about the coupling constants, particle masses etc)?

OK, so here's a way it could be different. Given a theory that fails to predict some quantity, one thing you could do is just put in the exact value of that parameter by hand, and show that the theory can at least accommodate it. But instead of that, you could do something different – you could just put in by hand the fact that these parameters lie *within a certain range* (i.e. the range required for life). That's a weaker constraint, but it might force some predictions out of the theory. So, for example, your theory might say: "If the speed of light has to be within a range compatible with life, then the only allowed value turns out to be exactly *c*". Then, it seems, something anthropic-looking would appear to be doing some predictive work. Is that what's going on?

16. **D R Lunsford**
November 2, 2005

Chris – re "technicians" – yes indeed, this was pointed out to me by my advisor – the machinery is so dense that you can crank out work without even thinking. Of course the latter is what goes missing, and without that one gets boredom.

These technicians also tend to rely on cults of personality, lacking as they are in the necessary mental wattage.

-drl

17. **dan**
November 2, 2005

"possible since string theory is now moribund, whereas LQG is in a much livelier state."

so what is the current story on
1- string theory entropy for "ordinary" BH's
2- LQG on BH (with the discussion of transcedental numbers and ln2 versus ln3)
3- do LQG still predict wavelength-dependent photon velocity deviations of c? — something that could be "tested"

18. **Juan R.**
November 3, 2005

Adrian

If you are devoted the best years of your life to a head horse.

If you are popularized your wrong theory in mass media before verifying that was correct -somewhat as cold fusion scandal- and moreover have used **dishonest** actions - as saying not the true, hidding the flaws of the theory, etc.-

If you have claimed in almost every talk, paper, popular book, etc. that people against string theory was either stupid or ignorant.

Then you have only two 'exists'.

1) Intelectual suicide

2) Continuous negation of evidence

It is interesting how some string theorists begin to claim that the problem of prediction is not with string theory. It is with our incorrect understanding of prediction.

Since string theory does not fit basic requirements of science. String theorists, who do not choose option 1 of above, are claiming that we may change scientific method!!!

This is a pure nonsense and reflects desesperation of the field

Ironic question: When Kaku will propose the travel of all humanity to an alternative ‘elegant’ universe where string theory fits data?

Juan R.

Center for CANONICAL |SCIENCE)

Nekrasov, Pure Spinors and the Berkovits Superstring

November 1, 2005
Categories: Uncategorized

There's a new paper out tonight by Nikita Nekrasov entitled [Lectures on curved beta-gamma system, pure spinors, and anomalies](). Motivated by questions about the covariant superstring quantization method being studied in recent years by Berkovits, Nekrasov considers a sigma model with target space the space of "pure spinors". For more about pure spinors I suggest consulting "Spin Geometry" by Lawson and Michelson, but in general they are a subspace of the full spinor space with remarkable properties. In $\mathbf{R}^{2n}$, a pure spinor determines a complex structure on $\mathbf{R}^{2n}$, one that doesn't change when you multiply the spinor by a complex scalar. Furthermore, modding out by the action of the complex scalars, the space Q(2n) of projective pure spinors is a Kahler manifold, isomorphic to O(2n)/U(n). This is a projective algebraic variety, and geometric quantization of it gives back the space of spinors. There's quite a lot of beautiful geometry in this story.

Unfortunately, in the Berkovits story the target space of the sigma model is not Q(2n), which is smooth and has every nice property one could ask for, but the space of pure spinors themselves which is a cone over Q(2n), and has a singularity at the origin. How to handle this singularity is the problem Nekrasov is addressing. This is a rather technical business, one about which I'm no expert (and I'm not sure there are many experts out there on this topic other than Berkovits and Nekrasov).

At the end of his paper Nekrasov makes what appear to be some remarkable comments. He describes two ways to deal with the singularity. The first is to just remove it and work with a non-compact target space. In his paper he shows that this removes certain potential anomalies, but he comments that doing this causes "some unclear issues with the definitions of string measure". The second way to deal with the singularity is to blow it up, working with the total space of a complex line bundle over Q(2n). Nekrasov claims that if you do this the superstring "would cease to be consistent beyond tree and one-loop level, thereby killing at once the landscape problem." The reference is to Susskind's [anthropic landscape paper](), although Nekrasov refers to Susskind as "Sussking".

I'm assuming this is some sort of perverse joke, since if the superstring is inconsistent on flat ten-dimensional space, there's every reason to believe it's also going to be inconsistent on curved 10d spaces and what gets killed is not just the landscape, but the whole idea of unification based on the 10d superstring. Nekrasov goes on to end with the comment that "This is of course one of the unrealized, so far, hopes to solve some pressing predictive issues of string theory by capitalizing on its unusual, from the conventional quantum field theory point of view, perturbation theory", referring to a 1987 paper of Greg Moore that I don't have access to at the moment.

I'm curious to hear what people more expert in this subject think of all this. There are various relevant blog entries: [Robert Helling]() and [Urs Schreiber]() on Nekrasov's talk a couple weeks ago about this in Hamburg, a recent posting by [Jacques Distler](), and a report on a talk by Berkovits at the KITP in August by [Andrew Neitzke](). For some relevant papers on the arxiv, see a paper by [Berkovits and Nekrasov]() from earlier this year as well as quite a few papers by Berkovits and other collaborators written over the last few years.

Update: A commenter wrote in to point out that the Moore paper is available [on-line as a scan of the preprint at KEK]().

After my post appeared, there were later posts on this topic by [Jacques Distler]() and [Lubos Motl](). Lubos seems to agree with me that Nekrasov's comment about an inconsistency in the quantization of the superstring in flat 10d killing the landscape is rather bizarre, since such an inconsistency would probably then hold in all backgrounds.

Funny, but if you look at [trackbacks for the Nekrasov paper](), they're there for Distler and Motl's blog entries but not mine, even though mine appeared earlier. I guess whatever the moderation policy is for trackbacks these days, I'm in a separate category.

Update: After inquiring with the arXiv about what was going on about this trackback, I just heard that it has been posted. It's still unclear to me what their moderation system is.

Comments

1. **Anonymous**
   November 2, 2005

   Spires has a link to a KEK scanned version of the 1987 Greg Moore lecture.

2. **D R Lunsford**
   November 2, 2005

   That is fascinating.

   Here is some interesting work by Trautman, which is fascinating in its own right:

   http://www.fuw.edu.pl/~amt/gaspin.ps
   http://www.fuw.edu.pl/~amt/gps.ps

   They seem to be generalizations of twistors to other spaces. It sounds a little like line geometry (Pluecker).

   -drl

   -drl

3. **Wolfgang**
   November 2, 2005

   > Nekrasov claims that if you do this the superstring "would cease to be consistent beyond tree and one-loop level

   I thought it was explicitly shown that two-loop is consistent.
   So this would indicate that something is wrong with the PS approach ?

4. **Mafra**
   November 2, 2005

   Last month it was explicitly shown that the four point two-loop amplitude computation in the pure spinor formalism agrees with the RNS result (when all states are NS).
   So, there is no indication that the PS approach is wrong up to 2-loops.

5. **Urs Schreiber**

November 2, 2005

One needs to deal with the singularity of the pure spinor space. This is a technically issue of correctly working out the formalism.

One way one might *guess* to deal with it is to 'blow it up'.

If you'd blow up the singularity of the pure spinor space, *then* there'd be incosistencies above one loop.

So it's the wrong thing to do.

Unless you argue like Nekrasov mentions one could argue. You could argue that you *redefine* your theory to be given by the beta/gamma model on the blown up space of pure spinors, by definition. This is now a different gadget than the original theory. Since it is inconsisztent on flat target space, it is apparently more constrained than the original theory. So maybe (that's Nekrasov's idea), maybe there are only very few target spaces which would make a pure spinor string with a blown-up space of pure spinors consistent.

6. **anonymous**
November 2, 2005

it appears that the "moderation policy is that you have to submit your own trackbacks.

http://www.arxiv.org/help/trackback/

7. **woit**
November 2, 2005

OK, I just tried submitting a manual trackback and will see what happens. The documentation does seem to claim that auto-discovery of trackbacks is supposed to be supported.

8. **Matt**
November 2, 2005

Peter, it's not a secret that they won't post your trackback on the arxiv. The arxiv is for scientific exchange only, to which you have not contributed at least since 1989.

9. **woit**
November 2, 2005

Hi "Matt" (aka "Michael") from Brandeis,

Whoever you are, you're as much of a cowardly asshole as ever. What is it about string theory that causes its proponents to behave like this?

10. **D R Lunsford**
November 2, 2005

Peter, that's a more interesting question than anything about string theory proper. I have my own theories about it, which I will keep to myself.

-drl

11. **Who**
November 3, 2005

I see where Steven Weinberg has bet the life of Andrei Linde plus one dog, apparently on the string theory Landscape.

see the conclusions on page 13 of a paper by Weinberg posted today
hep-th/0511037

"As for me, I have just enough confidence about the multiverse to bet the lives of both Andrei Linde and Martin Rees's dog. "

Do I hear two dogs?

12. **D R Lunsford**
November 3, 2005

That is really distressing. "Gravitation and Cosmology" is still the best gravity textbook.

-drl

13. **D R Lunsford**
November 3, 2005

It reminds me of Hegel's "proof" that there were no more planets – just before Ceres was discovered.

-drl

14. **D R Lunsford**
November 4, 2005

OK I read the entire paper – what a mess. Apparently Weinberg has the now common attitude, "since I'm the smartest guy in town, and I can't figure it out, we have to change the definition of science so that I can". You'd think a little embarrassment would creep in.

-drl

15. **Chris Oakley**
November 4, 2005

Weinberg's paper is depressing, and ultimately, irresponsible. As one of the most influential people in particle physics it is his duty to try to make the subject attractive to bright young mathematicians and physicists currently at school or university. The promise of nothing better than the application of anthropic reasoning to research problems certainly would not have drawn *me* into the subject, and I rather suspect that the same applies to the majority of young people today.

Anthropic reasoning is in any case circular. We work out what we believe to be the physical processes necessary for life and then study the range of values of physical constants that allow this to work. But all we are doing is finding out what works for *our particular* form of life. What if a different set of constants led to varieties of life vastly different from our own? It is arrogant to assume that the set of nuclear and chemical reactions that led to us are the only ones possible.
I do not agree with his analogy concerning Kepler's unfulfilled wish to be able to predict planetary distances. At least Kepler and Newton had a full dynamical theory. With the string theory landscape *everything* is up in the air.

16. **Dissident**
November 4, 2005

That paper has a truly remarkable cringe factor.

17. **Arun**
November 4, 2005

Weinberg does say that it would help to know what string theory is (w.r.t. the work of classifying string vacua). So, we have to accept as a philosophical truth (as it is unverifiable scientifically) some conclusion from a theory which we cannot formulate completely?

18. **Juan R.**
November 4, 2005

Weinberg's paper is fascinating.

It is an open oportunity for any new 'Weinberg'.

Solve the problem that Weinberg cannot. This may be the message to the young generations.

I, at least, am sure that 'Weinbergs' born each 50 years.

Juan R.

Center for CANONICAL |SCIENCE)

19. **Wolfgang**
November 4, 2005

who,

I mentioned Weinberg's latest opus on my blog (just click on my name).
I would certainly not bet my dog on this …

20. **Chris Oakley**
November 4, 2005

I would happily bet my dog, car, house, life, your dog, your car, your life, anyone else's life or anything else in a bet where it is never possible to say whether either party has won.
What I would rather do, though, is to sell an option. Someone can pay me a million dollars now, and if it is established that a multiverse exists in some given time frame – twenty years, let's say (that is, twenty years in this particular universe for an observer not moving at relativistic speeds relative to this planet), then I pay ten million dollars back.

21. **MathPhys**
November 5, 2005

Is it possible that a certain arXiv moderator just doesn't like you, Peter? Seriously, I can imagine that certain string theorists would prefer not to divert too much traffic to this site.

Incidentally, what's the current job situation like, for string theorists, in the US, nowadays? In the mid 80's, only string theorists could get postdoctoral positions, in the mid 90's, no string theorists could get positions. What's it like it in the mid 00's?

Weinberg Goes Anthropic

November 4, 2005
Categories: Uncategorized

As a commenter here noted last night, and other commenters have discussed in the last posting, Steven Weinberg has just put on the arXiv an article entitled [Living in the Multiverse](). In it, he correctly points out that theoretical physics was immensely successful during the twentieth century as it adopted a fundamental paradigm of exploiting symmetries and quantum mechanical consistency conditions, using these to develop extremely powerful and predictive theories. Initial hopes for superstring theory were that it would lead to further progress along similar lines, but these have not worked out at all.

Faced with the failure of superstring theory to provide any new predictions based on a useful new symmetry principle or consistency condition, instead of drawing the obvious conclusion that it's just a wrong idea about how to get beyond the standard model, Weinberg instead proposes to dump the lessons of the success of twentieth century physics:

Now we may be at a new turning point, a radical change in what we accept as a legitimate foundation for a physical theory. The current excitement is of course a consequence of the discovery of a vast number of solutions of string theory, beginning in 2000 with the work of Bousso and Polchinski.

What Weinberg sees as "excitement" is what some others have characterized as "depression and desperation". His "radical change in what we accept as a legitimate foundation for a physical theory" seems to be to give up on the idea of a fundamental theory that predicts things and instead adopt the "anthropic reasoning" paradigm of how to do physics. Weinberg goes through various examples of his own recent work of this kind, announcing that the probability of seeing a vacuum energy of the observed value is 15.6% (this seems to me to violate my high school physics teacher's dictum about not quoting results to insignificant figures, but I'm not sure how you'd put error bars on that kind of number anyway). He also quotes approvingly recent anthropic work of Arkani-Hamed, Dimopoulos and Kachru, as well as that of his colleague Jacques Distler. All he has to say about the underlying string theory motivation for all this is that "it wouldn't hurt in this work if we knew what string theory is."

In his final comments he acknowledges that this new vision of fundamental physics is not as solidly based as the theory of evolution. Describing the strength of his belief in it, he says "I have just enough confidence about the multiverse to bet the lives of both Andrei Linde *and* Martin Rees's dog." One can't be sure exactly what that means without knowing how he personally feels about Andrei Linde, or cruelty to innocent dogs.

Weinberg's article is based on a talk given at a symposium in September at Cambridge on the topic "Expectations of a Final Theory". I haven't been able to find out anything else about this symposium, and would be interested to hear any other information about it that anyone else has. The article will be published in a Cambridge University Press volume *Universe or Multiverse?*, edited by Bernard Carr (the president of the Society for Psychical Research), about which I've posted earlier [here]().

I'm curious whether this Cambridge symposium was one of the infinite number of such things funded by the Templeton Foundation. Next week the Vatican will be sponsoring a Templeton-funded conference held in the Vatican City on the topic of [Infinity in Science, Philosophy and Theology](). It will feature a talk by Juan Maldacena on "Infinity as Simplification", and is part of a larger Vatican/Templeton project called [Science, Theology and the Ontological Quest](). This project is designed to promote the vision of scientific research outlined by Pope John Paul II in two encyclical letters, including the rule that scientific research must be "grounded in the 'fear of God' whose transcendent sovereignty and provident love in the governance of the world reason must recognize."

Update: Lubos Motl has some [comments on the Weinberg article](). This is one topic on which we seem to be in agreement.

Update (much, much later, May 2022): Rereading this posting many years later, I decided to check on the question of Templeton funding raised here. The Weinberg article was published in the volume [Universe or Multiverse?](), and the Acknowledgements section there has:

> First and foremost, I must acknowledge the support of the John Templeton Foundation, which hosted the Stanford meeting in 2003 and helped to fund the two Cambridge meetings in 2001 and 2005.

Comments

1. **Ranger**
   November 4, 2005

   (edited version)

   As a young physicist, I was looking for some career advice.

   After teaching all the labs and classes for years in grad school, writing all the grant proposals, and doing all the research, it would be fun to have a job and get paid someday.

   Should we completely cut off all relations with physics and reality, and instead fully devote ourselves to ingratiating the tenured dinosaurs who now say we must abandon the scientific method so as to turn their failures into success?

   Should we hide the fact that we have theories with postulates that are based in logic and reason?

   Should we bury all independent thought, and discard our theories which unify disparate phenomena with simple principles?

   What would you do if you wanted a job?

   Are there any journals out there taht favor logic, reason, and postulates over cronyism and nepotistic jargon?

   Thanks all!

   Having fun working on some lowly patents here. 🙂

2. **DMS**
   November 4, 2005

   Note also that
   The STOQ Project is aimed at developing the dialogue and the confrontation between Science and Religion, to face many theoretical, ethical and cultural challenges posed by current developments in science to the Christian vision of world, the human person and society.

   What possessed the Fields medallist Enrico Bombieri also to talk at such a forum? I wonder what these scientists think of Giordono Bruno, Galileo and Copernicus. This is very depressing indeed (not just silly, as in the case of Weinberg).

3. **woit**
November 4, 2005

Hi DMS,

One motivation for people to attend these Templeton sponsored things is that they tend to pay very well. Actually I found the STOQ Project and conference really silly, but the Weinberg article extremely depressing.

4. **R**
November 4, 2005

To me, this just seems like (justifiably) eminent thinkers who realize they are approaching the end of their usefulness in physics and want to think that they know what the next big thing will be — in order for them to believe they still have some small part to play.

5. **D R Lunsford**
November 4, 2005

What's perhaps most distressting – W wrote this paper using *quantum optics* grants. I'll bet he can't even point a telescope!

-drl

6. **JC**
November 4, 2005

Speaking of funding issues, has anybody with Weinberg's stature ever had their grants cancelled by the government funding agencies (ie. NSF, DOE, etc …)?

7. **Thomas Larsson**
November 4, 2005

I have heard Weinberg twice, I think. The first time was during the Nobel lecture in 1979. Back then, I was a sophomore and basically didn't get anything. The second time was when Rubbia and van der Meer won the prize in 1984, and Weinberg was on the panel during the Nobel lecture. This was the first time I realized that people were excited about string theory (I had previously tried to read Schwarz' 1982 review on my own, without success). In particular, I remember Weinberg making one prediction: that the 1991 Nobel prize would go to one of the smart young string theorists. So S.W. has evidently made at least one manifestly wrong prediction.

Perhaps growing up means that you realize that the heroes of your youth have become pathetic old men. It is nevertheless tragic.

8. **D R Lunsford**
November 4, 2005

TL – W was basically completely wrong about gravity as well (although he did it perfectly in his book).

-drl

9. **woit**
November 4, 2005

Hi Thomas,

One of my colleagues this morning, after being shown the Weinberg article, commented that Weinberg must just be senile. Unfortunately I don't think that's what's going on. Weinberg wants to be part of whatever the hot topic in particle theory is, and the landscape is the hot topic these days. It's being driven mainly by younger people, not by seventy-year-olds, and you can't put their behavior down to senility.

10. **Ranger**
November 4, 2005

Is everyone getting the feeling that String Theory is the Enron of physics?

Wouldn't it be fun to do a documentary and ask Brian Greene, "Yes–those were some very pretty animations in your show, and yes, we might well live in ten or twenty or thirty dimensions, but what exactly do you mean? What are the laws of string theory? What are the postulates? Besides TV shows and tenure and millions of tax payer dollars, what are string theory's hopes and dreams? Finally, could you please draw the intersection of the seventeenth dimension with the twenty third dimension? I have no idea how to picture this, but I imagine it must be very beautiful. Please share. Einstein and Feyman and Farady and Dirac and Newton and Ohm and Ampere and Gauss shared their wisdom in simple equations, so could you please do the same? Thanks! And keep on rockin'!!!!"

11. **elmer fudd**
November 4, 2005

what is pathetic is being unable to even consider a paradigm shift. The history of science is littered with pathetic scientists of all ages who just could not let go of old ways of thinking.

The current situation seems to be littered with spectators who would rather consider distinguished older scientists, and the most creative younger ones, wrong rather than let go of their little box of intellectual familiarity.

12. **elmer fudd**
November 4, 2005

Isn't the classic scenario that older scientists cling to the theories of their own generation that they helped create? Now you're saying that older scientists want to jump on the bandwagon. Which is it?

13. **woit**
November 4, 2005

Hi Elmer,

Nothing against paradigm shifts myself, I actually think particle theory is in desperate need of one right now. But if the paradigm shift is to completely trash the intellectual traditions of physics in order to prop up a failed research program that some people have a lot invested in, count me out.

In this instance, Weinberg is both jumping on a bandwagon and clinging to a theory he helped create (the anthropic "prediction" of the CC is generally attributed to him).

String theory is no longer a cutting edge new idea only embraced by the young. It has been around since about 1970. Young Turks who first came up with it (Susskind, Schwarz, etc.) are now old enough to collect Social Security. Those who came of age at the time of the First Superstring Revolution (1984) are now deep into middle age and losing their hair (I know a lot about this, this was my generation). Younger people trying to make a career for themselves in recent years have to do so in the context of an ossified ideology older than they are. That they end up doing things to prop it up in order to get ahead is not particularly surprising.

14. **Ranger**
November 4, 2005

Whoah elmer.

The only thing people are saying is that science does science and religion does religion.

You can use a fork to dig a grave and eat your spaghetti with a shovel, but we're allowed to laugh.

As a young scientist my greatest fear is that the old guard is pulling up all the ladders of objectivity, logic, and reason, and surrounding their NSF funded castles with useful-idiot grad student guards who will shoot you on site if you ask questions such as, "how many millions of dollars has each new dimension cost the NSF?"

The first three dimensions came relatively inexpensively.

And the fourth dimension was a bargain too.

But now it seems that each new dimension costs billions.

And can we really use them?

Can I take my date there tonight?

If so, then rock on, as there aren't any good bands playing tonight.

15. **Aaron**
November 4, 2005

"Weinberg wants to be part of whatever the hot topic in particle theory is, and the landscape is the hot topic these days"

Actually, Weinberg's mostly working on cosmology these days. I really doubt Weinberg does much of anything because of fashion. As you note, he was making anthropic arguments before any of the stringy types jumped on the wagon.

16. **Kris Krogh**
November 4, 2005

Hi D R Lunsford,

"W was basically completely wrong about gravity as well (although he did it perfectly in his book)."

Could you elaborate? I'm familiar with the book, which I admire very much, but not the mistakes about gravity.

17. **Shantanu**
November 4, 2005

Peter , I believe this conference was held at Cambridge university in 2001
and was discussed in Physics world. Smolin also has contributed to this volume
(hep-th/0407213), so has Wilczek (hep-ph/0408167). some other contributions
for this conference on preprint archive are by Aguirre (astro-ph/0506519)

See http://space.mit.edu/home/tegmark/photos/science/anthrofest_010830.jpg
which I believe is a photo of this conference (although I see neither Weinberg nor Wilczek in the photo)

18. **woit**
November 4, 2005

Hi Shantanu,

Weinberg's article says his talk was on Sept. 2 of this year. There have been lots of "multiverse" conferences over the last few years, often sponsored by Templeton, at least a couple of them at Stanford.

19. **blank**
November 4, 2005

I'm not a big historian of science, so could somebody please tell me, are there any past instances of succesful paradigm shifts, driven not to kill off an old theory which cannot explain empirical data, but instead to save an existing theory which cannot explain empirical data? This would seem truly to be a paradigm shift among paradigm shifts.

20. **D R Lunsford**
November 4, 2005

KK – he claimed that the geometrical picture that makes gravity sensible, is unimportant. He then wrote a 500 page book that is thoroughly geometrical!

-drl

21. **Kris Krogh**

November 4, 2005

Hi DRL,

Point well taken. Meisner, Thorne and Wheeler have said the same. But maybe Weinberg's heart was really in the right place. I don't think it's unreasonable to look for alternatives to complicated geometries to explain gravity, particles, etc… That was the road to this string theory mess!

Here's an alternative to to general relativity which is quantum-mechanical rather than geometrical: http://arxiv.org/abs/astro-ph/9910325

22. **Jean-Paul**
November 4, 2005

The whole story is very weird. Within 1 week, I see Weinberg's article in Physics Today on Einstein's mistakes and then his own arXiv paper in which he essentially disintegrates as a physicists, quotes cardinal Schoenberg and cracks silly jokes. My suspicion was that he was getting senile, but then I felt embarassed by such disrespectful thoughts…
So let's simply forget about the whole incident, drop the curtain and remember Steven Weiberg at his best…

23. **Tony Smith**
November 4, 2005

Are there sociological pressures in the U. Texas physics department that might force Weinberg to conform or be ostracized ?
My view is that ostracism by one's "peers" (read fellow members of the U. Texas physics department, in the instant case) can be a powerful force inducing conformity of thought.
Consider, for example, "silent treatment" in military academies.
If Weinberg is stuck in such an environment, it is possible that even someone of his mental ability (evidenced for example by his excellent 1972 book Gravitation and Cosmology mentioned by DRL and his more recent 3-volume treatise on Quantum Theory of Fields) might be so "brainwashed".
If so, U. Texas should be ashamed of its physics department. Further, if the U. Texas physics department does have such an environment, it might even tarnish the highly favorable reputation now enjoyed by that university due to its currently successful football team.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

24. **Aaron**
November 4, 2005

"Are there sociological pressures in the U. Texas physics department that might force Weinberg to conform or be ostracized ?"

That may well be the funniest thing I've read in years.

And no, he's not senile. Just because you disagree with someone — and I'm against the anthropic principle — it doesn't make him nuts.

25. **D R Lunsford**
November 4, 2005

"smoking or not? Multi or uni verse? May I take your coat?"

-drl

26. **Moshe Rozali**
November 4, 2005

Aaron, That also brought a smile to my face, I'd like to see someone try giving Weinberg "the silent treatment"… also noteworthy, for earlier and more serious comments, is the fact that Weinberg's first paper on the subject is from 1987, so it is not clear who is jumping on which bandwagon.

The paper itself is really nice, as usual. I liked the clean separation between the anthropic principle (which is a tautology) and the "principle of mediocracy" which is much more problematic.

27. **Tung**
November 4, 2005

There's also an article by Weinberg in the latest issue of Physics Today, titled "Einstein's Mistakes".
http://www.physicstoday.org/vol-58/iss-11/p31.html

28. **D R Lunsford**
November 4, 2005

I should write out a detailed destruction of these (anhistorical) "arguments". "It is a pity that Einstein gave up on Kaluza-Klein.." etc. etc. And what about Pauli? Dirac? Everyone gave up on KK theory because it was demonstrably content-free.

-drl

29. **Quantum_Ranger**
November 5, 2005

Whatever stringtheory is?..it's not part of this Universe, no matter how wrong we are in our understanding of our Universe.

It's apparently quite clear that the only saving grace of the stringtheory era, is choosing the right 'get out clause'. Anthropic reasoning is quite obvious the only way out, as embarassing as it is, its the one and only way out.

Finding 'multi-universe' reasons to which they can catogorize a Universe, where they know they would be absolutely correct in stating that stringtheory would finally exist somewhere!..although not in this universe..there is a more than likely probability that it exists elswhere?

Saving grace future headline replacing the previous ST headline of wondering how they discovered stringtheory?>,.. (you know the one:where a monkey left some mathematical clue from the future, and it suddenly was discovered in the 20th century, explaining the discovery of ST-MTheory?)..HEADLINE:We had the correct theory..but it was in the wrong Universe!..oops!.. sorry wrong place and wrong time!

Even to mathematically show this to be 'almost' true..we would all have to pack our weekend overnight suitcase's, and visit the correct Universe to see for ourselves.

Problem?..why go to another Universe just to proof ST!

30. **MathPhys**
November 5, 2005

"Those who came of age at the time of the First Superstring Revolution (1984) are now deep into middle age and losing their hair (I know a lot about this, this was my generation)."

Speak for yourself, Peter!

31. **Arun**
November 5, 2005

Off-topic for this thread, but suitable for Not Even Wrong?

http://www.guardian.co.uk/science/story/0,3605,1627424,00.html

followed by

http://xxx.arxiv.org/abs/physics/0507193

32. **Quantoken**
November 5, 2005

I only have this to say: Weinberg is getting old now and he no longer say anything intelligent.

I could not bear to read on when reaching the end of the third paragraph, which he claimed never before was a symmetry principle involved. One who made such a statement should return whatever prize he was once rewarded for being intelligent.

I thought it's taught to every kindergarten kid who has learned any physics at all, that **Each one symmetry principle is ALWAYS associated with one corresponding conservation law**, and vise versa.

By Weinberg's extraordinary claim, he is suggesting there had been no conservation law proposed before Einstein. He must have forgot all the physics he leaerned. For example, what is the corresponding symmetry that caused Galileo to believe that an object shall continue to move at constant speed, if left free of inference of any external force? Weinberg should have known better.

Quantoken

33. **island**
November 5, 2005

I liked the clean separation between the anthropic principle (which is a tautology) and the "principle of mediocracy?? which is much more problematic.

Has anybody but me ever tried to actually answering the obvious and begged question of what good physical reason might exist for the implied "specialness", rather than to automatically assume that it's a circular reasoned tautology.

FYI: The principle of mediocrity only applies to banded spriral galaxies that are on the same evolutionary "plane" as us. SETI is wasting its time looking elsewhere.

34. **Nigel**
November 5, 2005

Feynman, Character of Physical Law, BBC 1965, page 57-8:

"It always bothers me that, according to the laws as we understand them today, it takes a computing machine an infinite number of logical operations to figure out what goes on in no matter how tiny a region of space, and no matter how tiny a region of time. How can all that be going on in that tiny space? Why should it take an infinite amount of logic to figure out what one tiny piece of space/time is going to do? So I have often made the hypothesis that ultimately physics will not require a mathematical statement, that in the end the machinery will be revealed, and the laws will turn out to be simple, like the chequer board with all its apparent complexities. But … it is not good to be too prejudiced about these things."

Earlier in the book he had reviewed the LeSage ether theory, concluding on p. 39 that it didn't do anything right as of 1964:

" 'Well,' you say, 'it was a good one, and I got rid of the mathematics for a while. Maybe I could invent a better one.' Maybe you can, because nobody knows the ultimate. But up to today [1964], from the time of Newton, no one has invented another theoretical description of the mathematical machinery behind this law which does not either say the same thing over again, or make the mathematics harder, or predict some wrong phenomena. So there is no model of the theory of gravitation today, other the mathematical form."

These lectures were given at Cornell in 1964, filmed and transmitted on BBC2 in Britain in 1965. It is a pity they are not available on DVD or on the internet. People like Weinberg could learn from them.

35. **Tony Smith**
November 5, 2005

1986 – Weinberg said "… In the last two years, theoretical physicists have become intensely excited over the idea that the ultimate constituents of nature … are … strings. … [each of]… these theories … has no free parameters in it … 'solve the string theory' … mean[s] to find out what these theories predict at much lower energies than 10^18 GeV …
The aim today is to try to find out whether the theory does in fact predict the standard model of the weak, electromagnetic, and strong interactions.
If it does then the second question is, what does it predict for those seventeen or more parameters of the standard model … the mass of the electron, the mass of the quarks, and so on?
If it does, then that's it. …". (from his 1986 Dirac Memorial Lecture)

1992 – Weinberg said "… Physicists will certainly keep trying to explain the constants of nature without resort to anthropic arguments. My own best guess is that we are going to find that in fact all of the constants (with one possible exception … the cosmological constant …) are fixed by symmetry principles of one sort or other and that the existence of some form of life will turn out not to require any very impressive fine-tuning of the laws of nature. …" (from his book Dreams of a Final Theory)

2005 – Weinberg said "… when the effort to extend the Standard Model to include gravity led to widespread interest in string theory, we expected to score the success or failure of this theory in the same way as for the Standard Model: String theory would be a success if its symmetry principles and consistency conditions led to a successful prediction of the free parameters of the Standard Model.
Now we may be at a new turning point, a radical change in what we accept as a legitimate foundation for a physical theory. …
Unless one can find a reason to reject all but a few of the string theory vacua, we will have to accept that much of what we had hoped to calculate are environmental parameters, like the distance of the earth from the sun, whose values we will never be able to deduce from first principles. … Theories based on anthropic calculation certainly represent a retreat from what we had hoped for: the calculation of all fundamental parameters from first principles….". (from his paper Living in the Multiverse at hep-th/0511037)

Why has Weinberg relaxed his standards for a well-founded physical theory from
"predict … parameters of the standard model" in 1986
to
anthropic principle only for the cosmological constant / vacuum energy in 1992
to
"much of what we had hoped to calculate are environmental parameters" in 2005 ?

If Aaron is correct that Weinberg is "not senile" and that he is not responding to sociological pressures, then:
What accounts for the deterioration in Weinberg's standards for a well-founded physical theory ?
Why does Weinberg not even mention the possibility that the "retreat" should be a retreat from string theory itself rather than a retreat from high standards for a well-founded physical theory ?

My only remaining guess is that, as Weinberg said in his 1986 Dirac Memorial Lecture, "Many of us are betting the most valuable thing we have, our time …" on string theory, and that Weinberg, having invested a lot of time in string theory, is, like some monetary investment losers, unwilling to cut his losses and move on.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

36. **Juan R.**
November 5, 2005

The problem of being smart is when one begins to believe it is true.

Juan R.

Center for CANONICAL |SCIENCE)

37. **Juan R.**
November 5, 2005

Kris Krogh,

Weinberg did an atempt to quantize gravity based in previous work by Feynman. Now we know that gravity is nonrenormalizable as a field theory.

Juan R.

Center for CANONICAL |SCIENCE)

38. **John Gonsowski**
November 5, 2005

Tony, yes it may be more that Weinberg has blinders on than a "brainwashing" but I guess it's the same thing really. It's not like the leaders in the field haven't come up with good ideas. You've mentioned Susskind and Smolin both having nice 27-dim M-theory related papers. Nobody seems to get excited even over their own good ideas. You seem more interested in SU(5) GUT than its creators. You've talked with Schreiber and Baez about Feynman lattices and exceptional algebras but the excitement doesn't seem to last past the conversation. People don't seem to get excited enough about their own good ideas to pursue them far enough. You would make for a nice refreshing Discover magazine article kind of for the same reasons the Penrose article was refreshing (even if it was not all correct).

39. **Nigel**
November 5, 2005

I read Weinberg's "Dreams of a Final Theory" when it came out, but can't remember a word from it. I think he had his day with "The First Three Minutes" in 1977. Dreams can easily turn into nightmares with science.

40. **MathPhys**
November 5, 2005

I heard that S Coleman used to say that the reason why "The First Three Minutes" sold so well is that people thought it was about sex.

41. **Kris Krogh**
November 5, 2005

Juan R.,

"Weinberg did an atempt to quantize gravity based in previous work by Feynman. Now we know that gravity is nonrenormalizable as a field theory."

Do we know that? I think it's only been shown for gravity theories exactly equivalent to general relativiity, as Weinberg's was. The Yilmaz theory is renormalizable, for example.

Feynman called renormalization a "dippy process." And he said he didn't know whether renormalizability is a valid test of a theory's correctness or not.

Ever look and look for something and can't find it — then it turns up in a place you didn't look because you "knew" it wasn't there?

42. **Chris W.**
November 5, 2005

There is a subtle but noticeably subversive tone in Weinberg's article. (Why not bet his own life on the multiverse?) Think of Shostakovich's work while Stalin was still alive. I'd say the man knows exactly what he is doing; he is hedging his bets, and gently pointing out that the foundation is hardly stable.

(Einstein said, "To punish me for my contempt for authority, fate made me an authority myself.")

43. **MathPhys**
November 5, 2005

My understanding from the comments of people like John Baez and Steve Carlip is that Yilmaz' theory is simply wrong.

Einstein's theory is a cultural phenomenon: A major, far reaching theory and a pillar of modern physics that was created single handedly by one man (unlike quantum mechanics), in the absence of any pressing experimental motivation (unlike both quantum mechanics and special relativity). It is elegant and deep, and has stood the test of time for almost 90 years now, during which time it remained a focal point of research.

44. **D R Lunsford**
November 6, 2005

MP,

Yilmaz may be wrong, but he's not "simply wrong". His motivation is legitimate but he's trying to solve his problem in the wrong context (analogous to, say, the contracted electron flying through the ether, of Lorentz).

-drl

45. **D R Lunsford**
November 6, 2005

Any comments from Glashow about this business?

-drl

46. **JP**
November 6, 2005

Of course, given the enormous number of consistent CFTs, as well as the large number of moduli in each, string theory will not have real predictive power until the dynamics that SELECTS the vacuum is understood.

47. **Adrian Heathcote**
November 6, 2005

Elmer F. above implicitly appealed to Kuhn and his Structure of Scientific Revolutions to say that W. is not being irrational (he's going with the paradigm shift despite being old) but that others here were (for not going with the paradigm shift despite being young).

I doubt that Kuhn had anything useful to say about irrational belief in science and his book is notably free of hard data. What arguments there are are pretty naive. Truth to say irrationality comes in a lot of shapes and forms and is probably by its nature hard to classify. Unlike rational belief it is inherently disordered.

But one form that the irrationality of old scientists *can* take is to want, before they die, to see that the current situation, whatever it is, is good—that we are on the right path. After all, they know that they won't get to see the final answer—but they want to assure themselves that their life's work was not wasted. So whatever is currently going on is *in the right direction*. And they are prone, in this stage, to retreating to philosophy, as though that might be where the answer really lies, rather than slaving away at the math to come up with something that really fits nature.

Einstein was a great figure IMO because he never gave up on the math—he was always trying to find what he (justifiably) thought was missing. Even when the orthodoxy was complacent.

Weinberg has done a lot of kicking of philosophers in the past (particularly in the NYRB) in my view with perfect justification. How ironic that he's now turned into one of them! "The problem my friends is that we had the wrong philosophy of physics: we expected experimental predictions. How naive! How Positivist of us!"

cheers

48. **Nigel**
November 6, 2005

Adrian,

Kuhn's "Structure of Scientific Revolutions" book, at least the edition I read (a red paperpack) has a preliminary page thanking Niels Bohr and the Copenhagen people for editing or whatever. Kuhn was having to err on the side of a defence of Bohr's Copenhagen Interpretation. Hence Kuhn saw the issue as one in which physicists like Planck, Einstein, and Schroedinger were old fools being irrational, while Bohr and Heisenberg were right. He then dug up those arm-waving bits of the history of phlogiston and caloric which fitted in with his prejudices.

You say "Einstein was a great figure IMO because he never gave up on the math—he was always trying to find what he (justifiably) thought was missing."

I agree, although as Feynman suggested in the November 1964 lectures on physical law, if the math is so complex it takes an infinite series of coupling terms to calculate the magnetism of the something as tiny as just an electron exactly, perhaps God is not a renormalised mathematician as such. The automatic assumption that nature is abstract mathematics because there is a mess of equations is just like the joke of the great biologist JBS Haldane who said God must be a lover of stars or beetles, because he made so many of them.

Not very anthropic!

49. **Adrian Heathcote**
November 6, 2005

Hi Nigel

Yes, I read that same red paperback! I agree with your remarks of course—its a pity so many people took that book as a reliable guide to the sciences. (Sociologists took it to mean that all scientists were irrational but themselves!)

Weinberg, ironically, was one of its greatest critics.

But on math: surely there is no problem in which the answer is: *less math*. More elegant, yes, more powerful, yes. Different, yes. But never less.

Less only makes the computations longer!

cheers

50. **Elmer Fudd**
November 6, 2005

Isn't it possible that there is a nested approximation within string theory that will predict the realities of our own universe close enough to build further machines, even as Newtonian physics exists as a useful approximation/limited case within relativity/quantum physics?

People seem to want "the final answer" to mean complete description of our own universe with its physical laws. Now that "the final answer" may turn out to be talking about something different, many are unable/unwilling to listen since it does not accord with their expectations.

If the Landscape shows that there are infinite universes, it is up to the sentient beings (in those universes that support their existence) to come up with versions of physics that work in their particular universe, as useful appoximations. Two different things- one speaks of the large picture, the other focuses on one small picture at a time.

51. **island**
November 6, 2005

There has never been logical proof the universe is infinite.

Relativity extends to a finite closed universe.

So who listens Einstein, his theory, or anybody else, further than they really want to…?

stars or beetles, because he made so many of them.

Not very anthropic!

Stars and beetles are entropy-efficient… only, they're not as efficeint as humans… "pound-for-pound".

How practical.

52. **Mr Jones**
November 6, 2005

Kuhn tried to understand aspects of (as Popper also did) how science works in practice, instead of prescribing how it ought to be done (as was common among philosophers of science up to that time roughly). What's so offensive about that? He got some of it right but not all, but then who gets everything right anyway?

53. **Kris Krogh**
November 6, 2005

MathPhys,

It's come to light in Einstein's collected papers he did have a pressing experimental motivation. First there is a previously unknown manuscript, coauthored with Besso, on his next-to-last gravity theory. There he calculates Mercury's precession and gets the wrong value. Then there are two letters on the final theory, to Lorentz and Planck, explaining he abandoned the previous one because it didn't fit Mercury's orbit. Why hasn't this been noted by the general relativity community?

The "elegance" of general relativity, reminds me of string theory hype. If it were really elegant, would it be necessary to say so? Shouldn't such a theory include quantum mechanics? Some say it's elegant because it's based entirely on general principles. Weinberg (bless his heart) says that isn't true, because Einstein arbitrarily assumed second-order partial differential equations.

Maybe general relativity stands the test of time because the only tests counted are the ones passed. Does it pass the test of the velocity curves of the stars in galaxies? Or the motions of the Pioneer space probes? A failure only means we've discovered "new physics," like strange dark matter and energy.

I agree Einstein's theory is a cultural phenomenon — but so are myths.

54. **Yraste**
November 6, 2005

W- "…we will have to accept that much of what we had hoped to calculate are environmental parameters, like the distance of the earth from the sun, whose values we will never be able to deduce from first principles."

-Did he forget Titus-Bode law which can be explained from first principles.

When the math gets too hard, then you are left with the philosophical option only.

55. **island**
November 6, 2005

I guess I crossed that line but I thought it was just a natural extension of einstein's theory, rather than one that I'd dreamt-up.

I'm sorry.

56. **D R Lunsford**
November 7, 2005

KK,

A theory need only include its “referents” in the observable world. If that theory is inherently macroscopic, then quantum mechanics is both 1) unnecessary and 2) impossile – you can’t arrange the experiment that nails down |psi>. That GR has no direct quantum statements is neither unexpected, nor troublesome.

GR is compelling because it deals with matter *even when Tmn = 0*. This is exactly analogous to light, even before the inverse square law.

-drl

57. **Not a Nobel Laureate**
November 7, 2005

What Weinberg neglected to mention is that that success of theoretical physics in the 20th century was due in no small part to rapid advanced in technology enabling physicists to generate a flood of experimental data

Theories were rapidly tested and those found wanting rejected.

58. **Nigel**
November 7, 2005

The “flood of experimental data” still exists and remains to be analysed, the mass ratio of muon to electron and other particles, and the coupling constants of electroweak to strong nuclear and gravitation.

Theories were not “rapidly tested and those found wanting rejected”.

Take the case of Gell-Mann’s quarks versus Zweig’s aces. Zweig wrote a more detailed paper and was suppressed by a big American journal while he was in Europe at CERN, while Gell-Mann in America from experience was shrewd enough to submit his briefer and less substantiated paper to a small European journal which printed it. (Let’s not get involved in the issue of Zweig never getting a Nobel prize, as Gell-Mann officially got it for symmetry work.)

Arthur C. Clarke once said that any sufficiently advanced technology is indistinguishable from magic. This is the fate of any revolutionary idea, which by definition (being revolutionary) is in conflict with preconceived ideas like string theory. It is very easy to weed out reality, to flush the baby away with the bath water. It is a different matter to take a heretical idea seriously. Everyone can see it is absurd and obviously wrong. I think this is why the Soviet’s having lost an enormous amount in WWII still managed to beat America into space with Sputnik. The mainstream is always too prejudiced in favour of yesterday’s methods to be really serious about science.

59. **Juan R.**
November 7, 2005

Kris Krogh,

Yes, Weinberg worked with a GR spin-two field. i wanted say that GR is nonrenormalizable as a field theory.

Effectively, there is not proof that other approaches to gravity cannot be renormalizable.

So far as i know Yilmaz theory has been proven to be incompatible with experimental data, including some Newtonian-like tests.

Feynman called renormalization a “dippy process.?? And he said he didn’t know whether renormalizability is a valid test of a theory’s correctness or not.

Well, i completely agree with Feynman and also with Dirac and Landau who expressed similar claims before. Moreover, the trick on renormalization is on infinite less infinite equal to any thing that i want, because mathematically the operation is not defined. In some sense renormalization has worked but ONLY in scattering experiments, where renormalizable terms do not modify the dynamics. It appears that renormalization does not work in recent test of QED in bound states in He atom.

It is often claimed by particle physicists that renormalization is rigorous, which is false since it has been only proven for scatering states with free fields doing lot of asumptions in the ‘proof’.

Precisely, i am working in a promising line of research where renormalization is not needed and the theory is defined also for bound states. My work is a further generalization of Fokker/Dirac/Wheeler/Feynman theory. A first version of a non-perturbative quantum gravity is already available at our center but we are doing consistency tests and consulting with external colleagues. Until 2006, we will not publish first results which would be -if our work is correct- a serious candidate to string or LQG approaches.

Juan R.

Center for CANONICAL |SCIENCE)

60. **Juan R.**
November 7, 2005

MathPhys Said,

Einstein’s theory is a cultural phenomenon: A major, far reaching theory and a pillar of modern physics that was created single handedly by one man (unlike quantum mechanics), in the absence of any pressing experimental motivation (unlike both quantum mechanics and special relativity). It is elegant and deep, and has stood the test of time for almost 90 years now, during which time it remained a focal point of research.

This is part of the mith around einstien he developed his theories alone. This is not true. I proved in sci.physics.research General relativity is mainly the work of four or five guys. Einstien was just one of them.

The only contribution of Einstein to GR was his proposal of substituting the scalar potential by a 10-component metric.

The geodesic equation of motion was proposed in an early paper of 1909 by Harry Bateman and the field equations of GR were obtained by Hilbert at least nine days before Einstein. See

http://canonicalscience.blogspot.com/2005/08/what-is-history-of-relativity-theory.html

for some aditional comments and historical analysis of recent data including Einstein-Hilbert correspondence. I am updating the page with a more extensive and rigorous version.

Juan R.

Center for CANONICAL |SCIENCE)

61. **Juan R.**
November 7, 2005

Nigel,

Even submiting his work on quarks, Gell-Mann's obtained a rejection form one of referees and practically was rejected for publication but finally he was able to convince editor for publication.

It is really interesting as string theorists were rejected by stablished colleagues four decades ago whereas now string theorists are the "stablishment" and reject any theory -even promising- if do not fit string theory preconceived ideas.

The difference is that in the past the criterion was 'mainly' fit of experimental data now it is a supposed mathematical elegance

Juan R.

Center for CANONICAL |SCIENCE)

62. **Kris Krogh**
November 7, 2005

DRL,

"GR is compelling because it deals with matter even when Tmn = 0."

Does Tmn = 0 refer to the instant of the Big Bang? Has it been proven there was such an instant, before which nothing existed?

63. **MathPhys**
November 7, 2005

Juan R,

Thank you for the correction. I actually tend to believe your version of history rather than the popular one. I'm not surprised at all to learn that there were priority disputes for both special and general relativity. Einstein was never an easy man, particularly in his youth, and from what I know of his character, I'm not so very surprised by what you say.

64. **D R Lunsford**
November 7, 2005

KK, not here.

-drl

65. **Tung**
November 7, 2005

Juan R,

It is an interesting account of "actual" history that you are promoting. True, Einstein's contribution is not really understood by the public, but for a more accurate and more objective account of what he did, I think I would recommend the works by real historians of physics like John Stachel, Arthur Fine, John Earman, Don Howard etc. Some of them are the first to read Einstein's then unpublished manuscripts and letters, and seems to me to be more believable.

66. **Tony Smith**
November 7, 2005

Actually, IIRC, neither Gell-Mann nor Zweig were the first to invent/discover quarks.

Around 1960, Liu Yao-Yang invented the Ceng Zi (Quark) model of particle physics. He was working at the University of Science and Technology of China, which was then located at Beijing, when he invented the Ceng Zi model. He wrote a paper and submitted it to a Chinese journal. It was turned down because the editors thought the paper was not correct. After the quark model had been independently re-invented a few years later, with most of the credit going to Murray Gell-Mann, the editors apologized for rejecting the paper. The latest information I have is that Liu Yao-Yang was working at the University of Science and Technology of China, which is now at Anhui, in the fields of atomic and molecular physics, quantum field theory, and quantization of gravity.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

67. **Eric Dennis**
November 7, 2005

Tony,

That's interesting. Do you have a reference for your account of Liu Yao-Yang?

68. **Chris W.**
November 7, 2005

Over on Cosmic Variance, Sean Carroll [reminds us]() to consider what extraterrestrial intelligences might have to say about the "anthropic" principle.

Seriously, does anyone—other than those motivated by certain religious doctrines—seriously consider the anthropic principle to be specifically, much less exclusively, about the relevance of life *on Earth* to understanding the structure and evolution of the universe?

69. **Kris Krogh**

November 8, 2005

Hi Tung,

If you can find a mistake in someone's work — that's good. But only to question their credentials? That's a page from the book of some string theorists.

70. **Tony Smith**
November 8, 2005

Eric Dennis asks "… Do you have a reference for your account of Liu Yao-Yang? …".
My information is from personal communications with Chinese friends.
If you want authoritative confirmation of those facts, I suggest that someone with Stony Brook connections contact Frank Yang (who moved from Stony Brook to China in the recent past), as he has close connections with Anhui and is probably quite familiar with more details than I know.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

71. **Quantoken**
November 8, 2005

Chris W. said:
"Over on Cosmic Variance, Sean Carroll reminds us to consider what **extraterrestrial intelligences** might have to say about the "anthropic?? principle."

He might as well ask what god would say on the topic, or what the hypothetical green men on mars would say.

I mean the whole thing is complete fictitious and hypothetical. How could you use something purely imaginary and hypothetical, instead of something based on solid evidence, to back up a scientific argument? We have not made contact with any alien and no one is able to ask an alien how it will answer the question regarding anthropic principle. So no one really knows how an alien will answer. Anything you say is then just your own personal opinion, not opinion by aliens.

Quantoken

72. **Adrian Heathcote**
November 8, 2005

Quantoken says: "So no one really knows how an alien will answer. Anything you say is then just your own personal opinion, not opinion by aliens."

I would have thought that the only thing they might insist on is a name change! ('Anthopos'—how terracentric!)

But seriously, surely the AP is just a way of eliminating theories that are so wide of the mark that they predict a universe in which life is impossible. Life exists, ergo such theories are false. String theorists seem to have taken it over as a way of putting back in by hand some of the predictive power that the theory itself has left out.You might just as well talk about a Planetary Principle: any theory which is so wide of the mark that it says that there are no planets can be ruled out from the beginning. *Anything* we know about our universe can be used as a filter on String theory models, or *any* theory.

BTW Tony: really interesting information on Liu Yao-Yang. Someone should write an article to give him his due.

A.H.

73. **Chris Oakley**
November 8, 2005

surely the AP is just a way of eliminating theories that are so wide of the mark that they predict a universe in which life is impossible. Life exists, ergo such theories are false.

I think that this is a good way of putting it. The AP is not worthless, but neither is it especially valuable. To expect a full dynamical theory just from this principle is crazy.

74. **Quantoken**
November 8, 2005

AH:
Too many people meantioned Liu Yao-Yang and his Ceng Zi model that a few words are worth saying. The so called Ceng Zi model was but vapourware. It was nothing scientific. The idea was a politically motivated one, inspired by the philosophy thought of then Chairman Mao, who held the belief that matter can always be divided no matter how small they become. It's the same intuitive belief since ancient times. For example, imagine you cut a stick in half, the remaining half is shorter, but you can cut it in half again, and you can repeat it infinite times and there is always a little bit left.

The Ceng Zi model believes, therefore, it must be divisible within the most fundamental particle known by then, proton and neutron. There were already experimental evidences that there are intrinsic states of protons and neutrons, implying that they have structures. But beyong suggesting that baryons are divisible into Ceng Zi's, the model says nothing specific about how the Ceng Zi should look like, how they interact. How many kinds there are, etc. Therefore it's nothing beyond just some philosophy ideas.

The problem with Sean Carroll, and other people in the same camp. Is that their "research" have been long detached from reality, that they probably believe that solid experimental data is not necessary and can be substituted by pure imagination in one's mind, and that such imagination can be used to establish or support a scientific argument.

I am not trying to argue for or against the AP. But hypothetical questions have no weight in making any argument. Sean already counted it as a fact that aliens exist and that in principle we could ask an alien. In reality, we simply do NOT know it as a fact whether aliens even exist or not. There is no evidence.

Quantoken

75. **Who**
November 8, 2005

An**slop**ic

76. **Quantoken**

November 8, 2005

Chris O. said:
"surely the AP is just a way of eliminating theories that are so wide of the mark that they predict a universe in which life is impossible. Life exists, ergo such theories are false.

I think that this is a good way of putting it. The AP is not worthless, but neither is it especially valuable. To expect a full dynamical theory just from this principle is crazy."

It must be point out that the AP is completely useless. That is, zero usefulness. AP does NOT eliminate any theory. Experimental evidences do. If you believe that AP helps to eliminate some theories, then you belong to the same camp of people who believe solid experimental evidences can be substituted by pure mind hypothesis. The fact of matter is experimental data is ultimately the only thing that can eliminate or establish a theory. **Data is the only judge in the court of science**. Principles, no matter how sounding, can not be used to make a judgement regarding a theory. You may have a theory which is at odd with the AP principle but fits the data, but the fact that your theory conflicts the AP only means that AP is probably wrong, not that your theory is wrong.

As far as experimental data is concerned, the AP is completely useless because it has no predictive power whatsoever. Anything we already know, by definition becomes part of the AP and be rationalized by the AP. But anything we do NOT already know, the AP leaves it wide open and anything could be possible. So the AP is unable to say a single thing about the unknwns that could be later checked against the facts. AP is as useless a princple as thereligious idea of rationalize everything by simply claiming "because God made it so."

Quantoken

77. **island**
November 8, 2005

The anthropic principle, when extended to become a biocentric principle is valid science that makes predictions about what aliens might say. Failure to look for evidence why the anthropic principle is valid, does not constitute a lack of evidence.

This is what I wrote in his thread concerning how it applies to his question about what aliens would say about the principle:

Space aliens would tell you that we humans are an arrogant bunch, and modern science is wrongly prejudiced against it for that reason, because the principle is actually biocentric in nature, extending to every banded spiral galaxy that is on the same evolutionary "plane?? as us, in terms of its implications that fall from the observation that we inhabit a preferred "place and time?? in the history of our universe. We're far from alone in that… is anybody awake?

Methinks that modern science is gonna be sorry that they didn't try to answer the begged question of what good physical reason exists for why the implied "specailness?? might be for-real, rather than to automatically assume that we are so detatched and insignifcant to the thermodynamic process that the principle is no more than a circular reasoned tautology, that's easily explained-away if we simply make a few leaps of speculative theoretical faith that aren't even close to being justified in origins science, which is dominated by empiricism.

Insignificance in not a valid argument either, when the principle is biocentric, due to the cumulative high-energy physics contributions that intelligent life is capable of making to the process, which is unmatched in terms of energy-efficiency, pound-for-pound, so to speak. Fred Hoyle proved that it only requires a few particles anually from each galaxy to account for expansion. What a coincidence NOT!

So this indicates that particle creation from negative vacuum energy holds the universe flat… What an anthropic coincidence! No, wait… my mistake, it's actually an anthropic prediction that theories which don't derive this are cluelessly screwed up about how the physics actually works!

In other words, space aliens will tell you that "Free-thinkers?? are every bit as arrogant as creationists for thinking that space aliens could be much more or less advanced technologically than us… although they might actually trust us to figure out why that might be for ourselves, assuming that we gave them a clear indication that we were finally actually getting a clue as to how the principle actually works… and applies.

78. **Nigel**
November 8, 2005

"Data is the only judge in the court of science. Principles, no matter how sounding, can not be used to make a judgement regarding a theory." – Quantoken

What about the principles of Copenhagen quantum mechanics, like Bohr's beloved Correspondence and Complementarity? Or Einstein's principles? The maths of Bohr and Einstein is very elegant and of course consistent numerically with reality. What people argue is that there are other ways of getting the same results without using the same principles, at least regarding SR and Copenhagen principles.

It seems that Bohr and Einstein didn't notice that there were other ways of getting the same maths without using the same philosophy. FitzGerald, Lorentz and Larmor, had the testable formulae of SR. Einstein still didn't notice this when he gave his "Ether and Relativity" lecture at Leyden in 1920. His biographer Pais wrote that he (Pais) first gave Einstein the Poincare's relativity papers of 1904 in the early 1950s. Einstein asked Born to acknowledge Poincare's work. Pais says he (Pais) was angry with Born for praising Poincare too highly, since Poincare used 3 postulates and Einstein used only 2. The whole story makes me nauseous. The love of monk Ockham's razor is just absurd. You don't find biologists or chemists banning biological or chemical mechanisms as superfluous or unnecessary difficulties. The lack of mechanism for forces allows string theorists to claim they are copying the guessed principle philosophy of Bohr and Einstein.

79. **Tony Smith**
November 8, 2005

Quantoken said "… The so called Ceng Zi model was but vapourware. It was nothing scientific. The idea was a politically motivated one, inspired by the philosophy thought of then Chairman Mao …".

Since I have not personally read the paper that Liu Yao-Yang wrote around 1960, I cannot deal directly with Quantoken's assertion that the Ceng Zi model was "vapourware" and "nothing scientific". Perhaps some Chinese sources might respond to that part of Quantoken's criticism.

However, I can say that it should be quite irrelevant whether or not the Ceng Zi model was "politically motivated" or "inspired by the philosophy thought of then Chairman Mao". Further, given the political climate of China around 1960, it should not be surprising that ANY paper about ANYTHING might well be phrased in terms that are connected with "the philosophy thought of then Chairman Mao".

Not only is that part of Quantoken's criticism invalid, it is unfortunately typical of the attitudes of some Western European / American people about physics motivated by philosophical schools to which they do not ascribe.
For instance, it is interesting that the Nobel prize have yet to recognize Kobayashi and Maskawa even though it is experimentally clear that there are three generations of fermions. Perhaps that might be related to the facts that their work was closely related to that of the Nagoya group (they both got their Ph.D.'s at Nagoya) and that the fundamental philosophy of the Nagoya group was Dialectical Materialism. As Kent Staley says in his book "The Evidence for the Top Quark" (Cambridge 2004): "…

some japanese physicists felt strongly that Western physicists, especially in the United States, systematically ignored their work. …". Staley's book contains some details about the philosophical underpinnings of the physics ideas of the Nagoya group around the 1960s. I do not know much about the flow of ideas between China and Japan during that time period, but it might be an interesting bit of history.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

80. **Adrian Heathcote**
November 8, 2005

Quantoken

Just some thoughts to try to clarify the idea of the AP.

The normal situation in science is to have T –>P, a theory making a prediction. Verifying the prediction increases the confidence that the theory may be true. The AP takes a range of theories/models and filters out those that are inconsistent with some known obvious fact (life exists). The AP leaves those theories/models that are consistent with the facts. It's perfectly true that a theory being consistent with some fact is not the same as a theory predicting that fact—the relation is much weaker. But filtering is not nothing either: Evolution is essentially a filtering process.

One could say that prediction is to forensic investigation as the AP is to a police line-up. (Or worse: to a line-up in which the person knows only what the suspect *didn't* look like!)

cheers

81. **island**
November 8, 2005

The constantly growing number of anthropic coincidences clearly indicates that the principle predicts that life only occurs fixed *near*-exactly between diametrically opposing runaway tendencies, like the near-flatness of the universe, or the balance between the relevant cumulative runaway tendencies that the earth has toward Milankovitch predicted glaciation… that gets offset by the innate tendency that humans have for warming the climate… etc, etc, etceteras… anthropic *Ecobalances*make a statement about uniform energy dissemination in a flat yet expanding universe.

Evobiologists could also make valid predictions from the AP if they weren't so knee-jerk conditioned and pre-prejudiced against any and all use of the principle because its religious abuse.

…cept for maybe the fringe like the templeton bunch.

http://www.templeton.org/biochem-finetuning/

82. **Quantoken**
November 8, 2005

Adrian H. said:
"The AP takes a range of theories/models and filters out those that are inconsistent with some known obvious fact (life exists). The AP leaves those theories/models that are consistent with the facts."

Wake up, **checking theories against observational facts is all science research is doing**. That's the day time job of all scientists. You do NOT need a middleman called AP to do such reality checks. People have been doing it for thousands of years without AP.

What other great usefulness of AP can you propose, other than your suggestion that without AP we will not be able to exam theory against known facts, or that we probably will not know for a fact that life exists if it were not for the AP? How absord!

On the same token of AP, we may also propose Hydrogen Principle or Oxygen Principle, Or Snake Oil principle. We know these things, hydrogen, oxygen, or snake oil, exists, so any correct theory must leads to the existence of these items. If a theory leads to the conclusion that snake oil does not exist, we know its wrong because snake oil exist, especially in the field of fundamental physics research. I would so propose that we rename Anthropic Principle as **"Snake Oil Principle"**, which may be more appropriate.

Quantoken

83. **island**
November 8, 2005

I didn't know that intelligent life was necessary to the existence of hydrogen or oxygen.

Snake oil however…

84. **Adrian Heathcote**
November 9, 2005

Hey Quantoken

woah, woah, woah!

I am not arguing for the AP by any stretch of the imagination. Essentially I am arguing on *your* side—but also trying to show what the AP is doing (the very little that it is doing!) In fact you are repeating my argument and pretending that it refutes what I said.

Perhaps I can suggest the principle of counting to ten before one hits the submit comment button.

cheers

85. **Juan R.**
November 9, 2005

Some authors have said that Weinberg was already anthropic decades ago. I think that is another exageration.

I do not really know those previous Weinberg works on cosmological constant and similar, if i am wrong please correct me.

I think that in the past, Weinberg used some kind of statistical methods for computing some average value for cosmological constant or similar. I think that his motivation would be obtain some, even approximated, value. Probably Weinberg's hope was to obtain the correct value from some future theory.

It appears that now is usually thought that those 'future theory' does not exist and all we can do is 'obtain' (so say) those anthropic values. I think that **now** Weinberg is anthropic therein Woit's title for this thread *Weinberg Goes Anthropic*

MathPhys, thanks! Works, manuscripts, and those are always subjected to certain personal interpretation (history is not one of exact sciences) but correspondence with Besso, Seelig, Hilbert, etc. proves that Einstein copied work of others and after do not cite them in his papers. For instance, in his correspondence to Hilbert said not the true and in his posterior 1915 (day 25) paper on GR, newer cited to Hilbert.

Also the history of string theory is completely distorted and that is the basis of popularity of people like Brian Greene, or Witten.

Tung, thanks by the advice. In the new extended version more authors are cited. Stachel coincides with me on that "*Einstein's proposal of the quanta of light was not revolutionary*" and that contributions to SR were pionnerized by Poincaré.

Regarding GR, in his broadly critized article on *Science* Stachel accused Hilbert of plagiarism. On a new article -more serious- now he argues that Hilbert had already obtained the correct GR Lagrangian…

Juan R.

Center for CANONICAL |SCIENCE)

86. **Adrian Heathcote**
November 9, 2005

Juan R.

I read your article on Einstein and found it very interesting. If the dates that you infer for who knew what when stand up then you've made a pretty strong case. The thing that I hadn't heard anything about was the relation with Hilbert and his work. Pretty damning!

It would be good to see a more polished version of this come out somewhere.

cheers

Krauss New York Times Essay

November 8, 2005
Categories: Uncategorized

Lawrence Krauss has an essay in today's New York Times about science, religion and string theory, covering much the same material discussed here in a recent posting. There are postings about this from Mark Trodden at Cosmic Variance and Lubos Motl on his blog. In comments at Cosmic Variance, Lubos tries to make the rather bizarre claim that the status of the theory of evolution is much the same as that of string theory. I don't notice any string theorists writing in there to tell him that he is full of it.

Meanwhile, in the real world, the Kansas Board of Education has voted to change the definition of science. Krauss has been very involved in this controversy in recent years, fighting the good fight against Intelligent Design and Creationism. I suspect he's all too aware of the danger posed by string theorists like Lubos intent on muddying the waters about the question of what is solid, testable science, and what isn't.

Update: Over at Cosmic Variance, see some of the reaction Krauss is getting to his criticisms of string theory.

Comments

1. **Ranger**
   November 8, 2005

   String theory is no different from religion, except that while religion finds many practical applications in civilized society, string theory has none.

   String theory is based on blind faith.

   And not only blind faith, but tenured snarkiness, taxes, and vast student loans.

   But nevertheless, the renaissance that takes us beyond postmodernism to truth's beauty will yet be.

2. **Quantoken**
   November 8, 2005

   Peter:

   There are something in parallel between the theory of evolution (TOE) and that of Super String Theory (SST). Both, in my opinion, are not falsifiable.

   But there is a huge difference. TOE is unfalsifiable because it is the result of pure logic. Just like mathmatics, you know 1+2=3 is absolutely correct, but you can't design an experiment to test to see if 1+2=3 is true or not. Because the possibility it's wrong simply do noit exist.

   TOE is pure logic. Natural Selection Rule and Survival of the Fittest. They come from logic and nothing more than logic. How could you refute that. How could you possibly design an experiment that could potentially defy logic. Fitness of a spiece is defined as the likelihood how it will survive. So of course a better fitting spiece has a better chance of surviving and spreading offsprings. It can not be falsified by any experiment, but it is correct by logic.

   On another hand, SST is un-falsifiable because it has no relevance whatsoever with the reality world. There is no connection whatsoever so there is no way any experiment can be proposed to prove or disprove it. So SST is at the completely opposite end, the wrong end, of un-falsifiability.

   Quantoken

3. **Eugene Stefanovich**
   November 8, 2005

   I think Krauss put his finger on a very important issue. I have a feeling that some parts of theoretical physics were hijacked by formal mathematicians who place mathematical "beauty" above sober observable reality. I am not even talking about strings. For better or for worse, this started 100 years ago with the invention of spacetime. This idea quickly became a favorite toy of mathematicians.
   Think about many different ways you can play with the spacetime: you can bend it, twist it, "foam" it, tear it apart. You can apply numerous branches of mathematics to it: differential geometry, topology, … What if you add some extra dimensions? Oh! The joy is endless! There is just one thing you cannot do with the spacetime. You cannot observe it!

   This seems to contradict one important lesson we learned from quantum mechanics. This lesson says: "never ask questions about something that you do not (can not) observe." If you want to get a verifiable answer from your theory, then specify the experimental conditions and the measuring apparatus. You can say: I have an electron gun at point A, a two-slit screen at point B, and a photographic plate at point C. Then the theory would correctly predict what is the image on the photographic plate. If you don't want to get into trouble, never ask which slit the electron passed through. This question has no counterpart in your experimental design.

   Now back to the spacetime. I don't think there is experimental apparatus that can measure spacetime curvature or topology, or whatever.
   All these "properties" are hidden, or maybe even non-existent.
   I think there should be a very very high threshold for introducing non-observable concepts in physics. Wouldn't it be much safer to limit the vocabulary of physics to things that we can directly observe: like electrons, photons, protons, their positions, momenta, spins, wave functions, etc.

   I see a tendency to move away from this basic stuff to some esoteric high-dimensional never-observable purely mathematical rather speculative things. Some may say that the logic of science led us there. I am not sure. I have a suspicion that pursuit of pure "mathematical beauty" played a part in misleading us there. Sorry for harsh words directed to mathematicians, but this tendency worries me a lot.

   Eugene.

4. **Luboš Motl**
   November 8, 2005

   Although it may sound strange, Quantoken is quite clearly closer to the truth than Peter Woit. The very evolutionary framework or the string theory framework are not falsifiable by a single experiment.

They're much more fundamental approaches to broad classes of questions that are supported by very general arguments – such as the billion-year-long history of Earth and life and/or the existence of gravity in the quantum world. In the case of string theory the arguments are much more mathematical in nature, but the basic reasons are very similar.

There is a belief system that is primary. It underlies another layer of insights which are viewed as technical details to be answered in the future. It is true about evolution much like about string theory.

Any particular experiment designed to falsify either evolution as such or string theory as such – before the exact vacuum etc. is localized – could only result in the modification of some subtle technical features of these theories. Only very unrealistic people claim that there exists a single experiment whose result would convince everyone in science that the whole Darwinian picture of species is wrong.

Something similar holds for string theory because we don't have the complete picture either: the absence of SUSY at the LHC won't kill string theory (although it may convince many to work on other problems) – and some people in fact claim today that SUSY is not a prediction of string theory.

Let me not go into details but string theory as such is a much more robust framework than SUSY at 1 TeV which is a mere quantitative technicality.

Peter's celebration of Krauss' anti-religious activity sounds like a prayer itself. Krauss may have obtained the right binary answer to the question whether creationism is wrong, but as far as his texts in the New York Times and elsewhere can indicate, his reasons are misguided. The reason why a theory in science is wrong is definitely not that the theory is too counter-intuitive for a layman or that it uses too complex maths or that the religious people may like the theory, and whoever thinks that these are the reasons that decide about the truth in science is confused about very basic features of the scientific method.

Also, I find Peter's suggestion that biology of the early life forms is more rigorous or more scientific or more well-established than theoretical high-energy physics to be rather absurd. We're continuing the same physics whose insights have been tested with the accuracy of 13 decimal places – the only difference is that we focus on harder questions that are also less accessible to cheap experiments.

At any rate, theoretical physics is more rigorous and reliable than biology – especially theoretical physics that follows from well tested frameworks. This includes the Bekenstein-Hawking entropy mentioned at Cosmic Variance and used as a non-trivial check of self-consistency of the theories of quantum gravity. Of course that the physicists are much more certain about the magnitude of the entropy of large black holes than the biologists can ever be sure about the hierarchy of very early life forms on Earth. If you disagree, Peter, are you just joking or are you serious?

5. **Luboš Motl**
   November 8, 2005

   I think that Eugene's example is excellent. There are simply people around who deny not only spacetime curvature – something that has been tested in several different types of experiments (they deny it because the theory behind it normally involves differential geometry that they hate) – but even the spacetime itself. They have problems with special relativity from 1905 whose 100th anniversary we just celebrated.

   Our colleagues like Lawrence Krauss and Peter Woit are qualitatively similar; the difference is merely quantitative. Maybe they don't deny the insights made by Einstein in 1905, but they almost definitely deny all insights in theoretical high energy physics made after 1975. Because they don't understand it and they don't want to understand it.

   Even if a majority of the population on Earth does not care about relativity, there are still people who care, and there is a subset of people who care about even more advanced questions, and so forth. What Peter Woit and Lawrence Krauss are doing is nothing else than a gigantic anti-scientific crusade. They want to destroy whole branches of science just because they personally find the questions and the concepts considered as solutions to be too difficult and too abstract.

6. **woit**
   November 8, 2005

   Hi Lubos,

   I was comparing string theory to the theory of evolution, not theoretical high energy physics to the theory of evolution. String theory is not equivalent to theoretical high energy physics, much as you wish that to be true.

   You're really out of your gourd to compare string theory and the theory of evolution. Sure, scientific theories are generally frameworks that can't be easily falsified by a single experimental result. But the framework of the theory of evolution does something the string theory framework can't do: it makes lots of testable predictions, ones that have been tested and come out as predicted. Wasting my time by arguing about this is against my personal religious convictions, as is responding to your foolish belief that people who have a problem with string theory just don't understand it.

7. **scott**
   November 8, 2005

   quantoken, I will repeat for you here a post I made on the Mark's thread over at cosmic variance.

   The theory that all organisms evolved is in no way the same postion as string theory. Microevolution, the prerequisite for the theories of speciation and common being general among all species, has actually been observed in the lab as well as selection pressures and other things. Corresponding things in string theory strings, compactified dimmensions, and susy have not been observed. Furthermore direct evidence that doesn't depend on the observation of microevolution exist.

   > In the following list of evidences, 30 major predictions of the hypothesis of common descent are enumerated and discussed. Under each point is a demonstration of how the prediction fares against actual biological testing. Each point lists a few examples of evolutionary confirmations followed by potential falsifications. Since one fundamental concept generates all of these predictions, most of them are interrelated. So that the logic will be easy to follow, related predictions are grouped into five separate subdivisions. Each subdivision has a paragraph or two introducing the main idea that unites the various predictions in that section. There are many in-text references given for each point. As will be seen, universal common descent makes many specific predictions about what should and what should not be observed in the biological world, and it has fared very well against empirically-obtained observations from the past 140+ years of intense scientific investigation.

   http://www.talkorigins.org/faqs/comdesc/

   String theory meanwhile has not been subjected to any tests.

8. **Sakura-chan**
   November 8, 2005

   If the LHC and colliders of the future fail to find evidence of the graviton, will Standard Modelers and Stringers still search for it?

9. **andy s.**
November 9, 2005

On TOE. Natural Selection and Survival of the Fittest exist obviously enough, but Darwin's insight is that these phenomena are the ONLY mechanisms behind the differentiation of species.

So it is falsifiable. If you proved that, say, species bifurcate because endoparasites diddle with their germ lines – for their own benefit, not their hosts, that would disprove Darwin by making Selection irrelevant.

(and don't laugh at the example; the endoparasites in your body get away with tons of weird shit we don't even know about).

10. **Eugene Stefanovich**
November 9, 2005

Lubos,

it is not correct to say that "spacetime curvature … has been tested in several different types of experiments." As far as I know, curvaturemeters have not been invented yet. What have been measured are observable effects with real physical systems, like planet Mercury, or light rays passing near the Sun, or binary pulsar systems. Spacetime curvature is a theoretical tool that our current model of gravity employs to explain these effects.
There is no theorem stating that GR is the only possible theory that can explain these effects.
Surely, GR is very beautiful mathematically. Surely, no other successful theory has been suggested yet. But I think we should better keep an open mind and try to avoid as much as possible those non-observable theoretical ingredients Krauss was talking about.

I think it is important to keep an open mind, because, in my opinion, quantum gravity project was a complete failure so far. In order to make it right, we should exercise discipline and logic, and try not to be distracted by beautiful, but physically empty, mathematical ideas.

I don't hate differential geometry or algebraic topology. To the contrary, I find them very enjoyable. I am just not sure what is their place in physics.

If (keeping the discipline of logic and an open mind) we rewind 100 years back and analyze foundations of special relativity, we may find that
4D Minkowski space-time unification does not follow from two Einstein's postulates (which are based on solid observations, of course) by straight logic. In fact, the idea of Minkowski spacetime is an additional postulate.

Moving some 20 years closer to our times we may discover that this idea is in a deep contradiction with basic quantum mechanics where position is an observable described by 3 Hermitian operators, and time is a numerical parameter. This just does not work well with the 4D spacetime unification. That's where the seeds of modern quantum gravity controversy were planted. In my humble opinion.

I am convinced that this controversy will not be solved until we make a deliberate choice between sober reality-based formalism of quantum mechanics where each component has a directly measurable counterpart in the real world and abstract non-observable (but mathematically so sweet!) postulate of the 4D spacetime unification.

Note that rejecting the 4D spacetime does not require us to reject the principle of relativity or the Poincare group properties of transformations between inertial observers. These two ideas have overwhelming support in experiments.

Eugene.

11. **wiseguy**
November 9, 2005

While I get some people are pissed at string theory, people outside the community (physicists in neighboring disciplines, like me) have I think a pretty accurate idea of what string theory is. It's a set of mathematical ideas, with some intriguing analogies, that a number of very smart people have worked on for more than twenty years.

Back in the day, string theory made some big claims. It still makes some big claims, but it tends to make them in the New York Times these days, and not in serious journals.

There are competitors to string theory. Even some of the great string theory gurus have taken up research into those areas.

Today, string theory is in trouble. It has provided no significant input into other areas of physics in a long time. All of its predictions are either trivial (fundamental Lorentz invariance, which may yet be hidden) or ambiguous (different versions say different things.) Sexy things like string cosmology may be "inspired" by string theory, but in practice are mostly statements in classical GR with a dose of field theory and a perhaps a nod to Hawking radiation.

Fields in trouble are not new for Science. Science will survive despite the occasional ideologue.

That's how we see it. IMO, Krauss should not be saying silly things about Intelligent Design and science. ID is, intellectually, somewhere up there with Dick Cheney's 2001 napkin theory of Iraq.

12. **Nigel**
November 9, 2005

"There are simply people around who deny not only spacetime curvature – something that has been tested in several different types of experiments (they deny it because the theory behind it normally involves differential geometry that they hate) – but even the spacetime itself. They have problems with special relativity from 1905 whose 100th anniversary we just celebrated." – Luboš Motl

Luboš, the spacetime curvature is physically required to keep Pi intact when the contraction term of GR reduces the radial distance around mass without affecting transverse distance, circumference. One way to account for this reduction of radius while circumference isn't reduced is to say spacetime is curved by the fourth dimension.

This is equivalent to radial pressure from the spacetime fabric virtual radiation, causing the contraction in the same way as the FitzGerald-Lorentz contraction in the direction of motion. I've shown how each contraction is related to the other, on my home page.

If you are going to worship Einstein religiously, relativity is going to forever remain stuck in either 1905 or 1915. How are you going to prove it is a theory so special that it can't be worked on any more? How are you going to defend it by saying it is so useful that nobody is allowed to use it? Differential geometry shouldn't be used to simply cover up ignorance, nor should any other mathematical tool.

13. **Arun**
November 9, 2005

Speaking of curvature meters – we measure the curvature of space-time in exactly the same way as we can measure the curvature of earth – by observing and measuring the non-parallelism of geodesics.

While there is no theorem that General Relativity is the only theory that can explain all the things we observe, there is the Parametrized Post Newtonian formalism, which is a systematic set of corrections to Newtonian gravity with a number of free parameters. Any theory of gravity sets values for these parameters; these parameters are obtainable by experiment or observation; and as I understand it, GR is the only theory left standing.

14. **Nigel**
November 9, 2005

Arun,

GR is not a scientific theory as Popper views such a thing, it is right because it ties up two empirical facts, Newtonian gravity (put in as the weak field/slow speed limit) and mass-energy conservation (the contraction term correction).

This is why GR is not a speculative Popper-type guessed theory. Einstein was not being arrogant when he said to a student in 1919 that it is right regardless of Eddington's experimental result. GR is not fallible provided the two inputs, Newtonian gravity plus the mathematical compensation for energy conservation, are correct.

Since the two 'arbitrary' inputs are empirical facts at least within the solar system, GR is logically correct on that basis alone. You don't need to test a mathematical prediction based entirely on empirically determined facts. Where Einstein ran into problems was in fiddling the basic field equation to give cosmological predictions, such as the cosmological constant fix to give a steady state universe. In addition there is the issue of whether gravity (thus the Newtonian G factor) is due to a mechanism like some kind of gauge boson pressure, which causes the contraction in gravitational fields, which is determined by the surrounding matter in the universe. Because rotational motion appears to be absolute with respect to the surrounding stars, ultimately there is empirical evidence for Mach's principle over relativism. GR is, as Einstein recognised in his 1920 Leyden university inaugural lecture "Ether and relativity", an absolute motion theory because it describes accelerations – which are and have always been absolute. Restricted or "special" relativity (SR) is incomplete. So much applauding is given to SR having only 2 postulates that people forget Einstein added a third postulate in GR in 1915, and GR is the universal theory.

As an analogy to GR, consider the heuristic interpretation of QFT, with virtual particles polarised around particle cores, forming a shielding veil which reduces the bare core charge by the 137 factor. In turning the maths of GR into a physical mechanism, you do the same thing, looking for a mechanism which fits the facts and then using the mechanism to make some testable predictions.

15. **Juan R.**
November 9, 2005

I completely agree Eugene!

Great physicists like Isaac Newton or R. Feynman understood perfectly the diference between physics ('science') and math. There is a current tendency in theoretical physics to use the pure mathematical formalism without a clear underlying physics.

Not only nobody has newer measured spacetime curvature. Even as i said in sci.physics.relativity and sci.physics.research time ago, in others theories as FTG or torsion gravity one verifies the same tests that GR with a **flat spacetime**.

Perhaps the most interesting of last days in this topic was my proof in spr of that in the nonrelativistic limit of GR, the metric of spacetime goes to (1 -1 -1 -1) and spacetime looks flat.

Some specialists like Carlip were rather sckeptic and initially did rather bold statements (as "your metric is wrong", when is a recomended metric) but finally Carlip agreed with me own proof that in the nonrelativistic limit one can obtain zero connections and still there is Newtonian-like gravity: a = -grad(phi).

Obviously curvature, R_ab, R, or \gamma^i_{ab}, etc. of spacetime is zero in that limit, but gravity is not.

If A is cause of B, then elimination of A may eliminate B…

My emphasis on this is because this is one of keys on why until now GR has been not unified with other forces and quantized: the geometric view of gravity has been the great failure of 20th century physics.

String theorists just hereditated this geometric view and the result has been… 40 years…

Juan R.

Center for CANONICAL |SCIENCE)

Baez on the Geometry of the Standard Model

November 8, 2005
Categories: Uncategorized

John Baez has a very interesting new paper on the arXiv this evening entitled Calabi-Yau Manifolds and the Standard Model. In it he points out that the standard model gauge group (which he carefully defines as SU(3)xSU(2)xU(1)/N, where N is a six-element subgroup that acts trivially on the standard model particles) is the subgroup of SU(5) that preserves a splitting of $\mathbf{C}^5$ into orthogonal 2 and 3 dimensional complex subspaces. Furthermore, if you think of SU(5) as a subgroup of SO(10), then the spinor representation of SO(10) on restriction to the standard model group has exactly the properties of a single generation of the standard model.

Baez would like to think of SO(10) as the frame rotations in the Riemannian geometry of a 10d manifold X. The SU(5) is then the holonomy subgroup picked out by a choice of Calabi-Yau complex structure on the manifold. One way to get such an X is as the product of $\mathbf{R}^4$ and a compact 6-manifold M^6, picking Calabi-Yau structures on both manifolds in the product. What is happening here is related to an old idea I wrote a paper about a very long time ago (see Nuclear Physics B, vol. 303, pgs. 329-342, from 1988). By picking an orthogonal complex structure on $\mathbf{R}^4$, one picks out a U(2) in SO(4) (the Euclideanized Lorentz group), and it is tempting to identify this with the electroweak U(2). This is one part of what is happening in Baez's construction. It's very hard though to see what to do with this within the standard gauge theory framework; this is true both for my old idea and for Baez's newer one. Maybe string theorists can come up with some way of implementing this idea of thinking of the standard model gauge group in terms of the Riemannian geometry of the target space of a string. If so I might even get interested in string theory…..

I don't immediately see from Baez's paper why the hypercharge assignments come out right. I need to sit down and work that out, but it's getting late this evening. There are some other issues his paper raises that I'd like to think about, and maybe I'll finally get around to doing some work to see whether what I've learned about spin geometry in recent years has any use in this context.

I also noticed today that Baez is advertising for students to come to UC Riverside to study Quantum Mathematics. I like the term, and for many students who really care about mathematics and fundamental physics, this would be worth thinking about.

Please, commenters who want to write about their favorite ideas about standard model geometry, try and stick to any aspects of this directly related to Baez's paper.

Comments

1. **Luboš Motl**
   November 8, 2005

   As far as I understand, John:

   * rediscovered that the Standard Model group is SU(3) x SU(2) x U(1) divided by a certain Z_6 group

   * rediscovered that 16 of spin(10) is a good representation for a single generation of quarks and leptons – i.e. rediscovered one reason behind grand unified theories

   * rediscovered that manifolds with SU(5) holonomy are called Calabi-Yau five-folds

   * wants to study, for a very incomprehensible reason, manifolds whose holonomy coincides with the Standard Model gauge group

   The last point seems completely crazy because holonomy is exactly the symmetry – a part of the tangential group – that is broken by the manifold's shape, while the gauge group of the Standard Model is a group that must be, on the contrary, completely unbroken.

   Comparing the dimensions 4+6 of the large and hidden dimensions in string theory with the (doubled – real) dimensions of the fundamental reps of SU(2) and SU(3) is pure numerology. The four dimensions of the space we know do not transform under the electroweak SU(2), and the six hidden dimensions do not transform under SU(3).

2. **John Baez**
   November 8, 2005

   Sometimes it takes work to ignore Motl, but it always pays off. If anyone has anything interesting to say about my paper, I'll be glad to discuss it here.

3. **D R Lunsford**
   November 9, 2005

   I'm going to read it now – this sounds fascinating. It's great to get results 🙂

   -drl

4. **Thomas Larsson**
   November 9, 2005

   There is one thing about GUTs that has bothered me for some time. We know that SU(5) is out because it predicts too fast proton decay, whereas SO(10) does not. However, it seems to me that two-step breaking, SO(10) -> SU(5) -> SU(3)xSU(2)xU(1) is also ruled out because of the SU(5) in the middle. Doesn't this mean that any kind of GUT where SU(5) plays an important role, apparently including John's, has trouble with proton decay?

   Alas, I have heard people talk about non-minimal SU(5), which apparently evades this problem.

5. **Quantoken**
   November 9, 2005

   John:

   I am not interested in discussing your paper, but in your description of QM in UCR, some dangling words really seem sexy:

   What exactly is the relationship between **number theory** and quantum mechanics? I have no idea. Can you explain? For example, is there anything particular that the inverse of the fine structure constant necessarily happen to be very close to the 26th odd prime number?:-)

6. **Robert**
November 9, 2005

The fact that the hypercharges work out has been checked already many years ago by people working on SO(10) GUTs.

I like the observation about 10=4+6 although I don't see an obvious way to turn it into something that resembles a model.

And note (besides Lubos' point about the holonomy being the part of the isometry that is broken which I think is valid) that the CY2 x CY3 is not what we have in nature: At least the large four dimensions are very likely to have full SO(3,1) holonomy as there seem to me arbitrary curvatures around. Of course one could make the approximation (or mumble something about 'groundstate' or whatever) that one should take 4D Minkowski space (or (A)dS if one worries about the cosmological constant) for the large dimensions but those in turn have too small a holonomy group.

7. **Urs**
November 9, 2005

> the holonomy being the part of the isometry that is broken which I think is valid

I you wanted to, you could argue that the standard model has only been tested in flat space. The proposed model would then presumeably predict deformations of the standard model group in strong gravitational fields.

I'd like to note that people interested in what they call 'geometric algebra' have played around with similar ideas (I'll look up concrete papers when I find the time).

In particular, note that if one wants to adopt the idea that the standard model particles arise as components of a spinor, one has to address the question what then happens to the 'usual' spinor degrees of these fermions.

The only natural answer to that which I am aware of is "Dirac-Kaehler" formalism. Assume the particles are not spinors, but "bispinors", i.e. inhomogeneous differential form, with the Dirac operator really being the Dirac/Kaehler/deRham operator d + del. The form bundle is Lamda M = S x bar S the product of two spinor bundles. One can interpret the left factor as the observed spinor degrees of freedom, while using the right factor to model families of fermions. That has been proposed several times.

I could remark that the Dirac operators arising in string theory actually are Dirac/Kaehler operators (on loop space). The reason this is usually not seen is chiral splitting of left- and rightmovers in WZW backgrounds.

8. **X**
November 9, 2005

Can this neat geometrical picture provide any insight into electroweak symmetry breaking? Or the existence of 3 generations of quarks and leptons?

9. **Tony Smith**
November 9, 2005

John Baez, in hep-th/0511086, says:
"… a G-manifold where G is the Standard Model gauge group is precisely a Calabi–Yau manifold of 10 real dimensions whose tangent spaces split into orthogonal 4- and 6-dimensional subspaces, each preserved by the complex structure and parallel transport. In particular, the product of Calabi–Yau manifolds of dimensions 4 and 6 gives such a G-manifold. Moreover, any such G-manifold is naturally a spin manifold, and Dirac spinors on this manifold transform in the representation of G corresponding to one generation of Standard Model fermions and their antiparticles. … For example, we can take M to be Euclidean R4 and K to be any 6-dimensional Calabi–Yau manifold. …[and then]… M x K is a G-manifold….".

Penrose and Rindler, in their book Spinors and Spacetime, v.2 (Cambridge 1986) say (at pages 307-308):
"… projective twistor space PT … has the structure of a complex projective 3-space CP3 … the points of CM# [compactified complexified Minkowski space] represent the lines of PT. Such a correspondence, in which the lines of a projective 3-space are represented as the points of a quadric in a projective 5-space, is known as the Klein representation …
Even more relevant to twistor theory was the observation by Sophus Lie in 1869 … that oriented spheres in (complex) Euclidean 3-space could be represented by lines in CP3, with (consistently oriented) contact between spheres represented by meeting lines in CP3. These spheres can be regarded as the intersections of light cones, of points in (complex) Minkowski space, with a constant time …".

Is it consistent with John Baez's physical model picture to use for Calabi–Yau manifolds of dimensions 4 and 6:

6-dimensional CP3 for a twistor picture of space-time
and
4-dimensional CP2 for the (10-6) dimensional space, related to SU(3) in the sense that CP2 = SU(3)/S(U(2)xU(1)) ?

Would the Klein correspondence allow you to use a conformal spacetime based on the 6-dimensional space of signature (2,4) upon which the Conformal Group SU(2,2) = Spin(2,4) acts ?

Could the CP2 then be related to the standard model groups in a way similar to that described by Batakis in his paper at Class. Quantum Grav. 3 (1986) L99-L105 in which the CP2 plays the role of the compact extra-dimensional space of a Kaluza-Klein model ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Thomas Larsson, in this same blog thread, said: "… We know that SU(5) is out because it predicts too fast proton decay …". Although that is clearly the conventional view as of now, there are alternative definitions of background so that the experimental results may be interpretable as consistent with proton lifetimes not so far from SU(5) predictions. See hep-ex/0008074 by Adarkar, Krishnaswamy, Menon, Sreekantan, Hayashi, Ito, Kawakami, Miyake, and Uchihori, entitled Experimental evidence for G.U.T. Proton Decay.

10. **Kasper Olsen**
November 9, 2005

Personally, I don't find Baez' paper that interesting,
– that the SM gauge group is not SU(3) x SU(2) x U(1), but rather this group divided by Z_6 has been observed **many** years ago, for example by H.B. Nielsen like 20 years ago (but it is anyhow, a rather trivial observation, since for most purposes we are just interested in the *Lie algebra*)
-as Lubos also observed, the inclusion of SU(5) in SO(10) has been known for ages – sure enough, SO(10) has some nice features, but if he's looking for beauty he could as well have chosen E_8 x (something), or whatever else that thas his SM group as subgroup. Spin(10) is the double cover of SO(10) (which is known for anybody who have heard about Clifford Algebras) and there is basically nothing new in noting, that the Dirac representation is single-valued in SU(5); this is basically just a

mathematical ‘accident’
– his comments about representations ‘on’ the exterior algebra Lambda(C^5) which he observes includes d-quarks, u-quarks etc. is also rather trivial since it is – as far as I can see – just another ‘accident’ related to how Clifford Algebras, Spin-groups and Exterior Algebras of C^n are related (or more precisely, representations of those)
– his comments about relating this to Calabi-Yau manifolds is also rather fictitious since his observation about the ‘real” SM group preserves the splitting of C^5 into a three- and a two-dimensional subspace is just another ‘accident’ related to the structure of Clifford Algebras, for which he — out of the blue — chose R^10 = C^5
– and in the same vain – the observation, that if M^4 and K^6 are Calabi-Yau manifolds, then M^4 x K^6 is a G-manifold (where G is *his* SM group) follows trivially as above. I guess he want’s it to be conneted to string theory (by choosing R^10 = C^5, i.e. the magic number 10 coming out of string theory), but actually it is not (at least not in any non-trivial way) . Sorry, I have to **agree** with Lubos, he is just playing with numbers 😉

Kasper

11. **Urs**
November 9, 2005

The paper I had in mind is this. Not that I have looked at it closely enough, but it does build (see the references) on the idea of embedding standard model particles into spinor reps. These people tend to use a slightly non-standard language when talking about spinors, but it’s mostly easy to sort it out.

I guess if you took these ideas and tried to embed them into something string-motivated in 4+6 dimension it would amount to trying to interpret the closed type II string’s RR states (the p-form fields) collectively as spinor bilinears (what is called ‘algebraic spinors in papers of the above sort’) with interpreting one spinor factor as the observed spin degree of freedoms and decomposing the other in terms of standard model particles of one generation.

I don’t know if that could work out to a sensible model. But it seems to be a natural way to try to implement the idea of realizing particles species as components of a spinor representation.

12. **Quantum_Ranger**
November 9, 2005

6 Quarks , 4 Forces, = 1 Gravity in 10 dimensonal space?

Great paper.

13. **Urs**
November 9, 2005

Sorry, it’s me once again. Might be that the link I gave above does not work. The paper it was supposed to lead to is

G Trayling and W. Baylis
A geometric basis for the standard model gauge group
J. Phys. A: Math. Gen. 34 (2001) 3309-3324

14. **Tony Smith**
November 9, 2005

Urs, it may be that the Trayling and Baylis paper you are mentioning can be found at hep-th/0103137.

Note, for fun, that the arXiv number decodes as number = 137 (the rest of the code is only year and month).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

15. **John Gonsowski**
November 9, 2005

“the inclusion of SU(5) in SO(10) has been known for ages – sure enough, SO(10) has some nice features, but if he’s looking for beauty he could as well have chosen E_8 x (something)”

The idea of someone well know bringing up ideas prematurely pushed to the sidelines and suggesting nice new things to do with them is not so bad. Funny, I pretty much said the same thing a couple days ago in the Weinberg thread. I even mentioned SU(5) GUT and Baez but I had Baez mentioned in relation to the exceptional algebras… so yes why not start at E8, there’s those E6 orbifolded fermions just waiting to suck up the SO(10) bosons. For details of the algebraic glue see Tony Smith, that’s his forte.

16. **Kasper Olsen**
November 9, 2005

John – the fact, that Baez’ observation – that SU(5) is a subgroup of SO(10) – is quite old, does not in itself logically make it “bad” (or irrelevant, unjustified, or whatever). The thing is this – virtually all his assumptions are not substantiated in any way, and this is why he could have decided to consider E_8 or E_8 x E_8 or U(1)^496, or ….

And I don’t think he is suggesting any nice “new things” with this. Basically, he is only expressing well-known fact in terms of Clifford Algebras, which might seem fancy and ‘new’ to some people; talking about Clifford Algebras is roughly just the same as talking about Dirac matrices, which for obvious reasons are related to Spin-groups, as for example Spin(n) is a certain subgroup of Cl(n,0) – but I guess you know all this.. 😉

Best,

17. **Nick Warner**
November 9, 2005

While Lubos is sometimes not as diplomatic as one might like, this does not mean you should ignore his comments – which are essentially correct.

It may be useful to explain the last one in more detail because the Baez article makes an elementary mistake in the physics underlying compactifications from higher dimensions. Unbroken gauge groups *do not* come from holonomy, and generally the exact opposite is true. The point is that if a field transforms under the holonomy then it is responding to the curvature tensor of the compactifying manifold and as a result fields that transform under the holonomy group will have masses that are set by the compactification scale. This means that a compact manifold, no matter how big it is, will not give rise massless fields that transform under the holonomy group.

In compactification one finds the low mass/massless fields in the sectors with trivial (generalized) holonomy, and as Lubos says, the holonomy group is precisely what is broken at the compactification scale.

18. **woit**
November 9, 2005

Nick,

Lubos's first four comments are just offensive and stupid. John was obviously not claiming to have discovered any of these well-known facts, he was just stating them. Today seems to be my day for pointing this out repeatedly, but I'll do this here again: you can't imagine how much damage Lubos has done to the cause of string theory by this kind of behavior. He's convinced large numbers of people that string theorists are arrogant, ill-informed fanatics. One reason for this is not just his behavior, but the fact that other string theorists seem to think it is all right to behave this way towards non-string theorists, just perhaps "not as diplomatic as one would like".

As for the last point, John wasn't making "an elementary mistake in the physics underlying compactifications from higher dimensions", since his only comment about the relation of the two mathematical propositions he states to physics was that "it would be nice to find a use for these results". He was pointing out a characterization of the standard model group that isn't hard to see but that I hadn't thought about before, as the subgroup of SO(10) preserving certain geometrical structures.

There are lots of problems with coming up with any sort of dynamics that make use of this, from the ones I pointed out about the U(2) piece, to the ones you and Lubos note If you try and do standard dynamics on a compactification. John isn't getting very far, but what he is addressing are two of the biggest problems the standard model leaves unanswered:

1. Why SU(3)xSU(2)xU(1)/N?

2. Why the certain specific representations of this group that occur in the standard model?

What he's saying is that if you start with 10 dimensional Riemannian geometry and certain specific geometrical structures, from the distinguished 10-d spinor rep you get an answer to 1. and 2. This just changes the problem to

Why 10 dimensions, and why these specific geometrical structures?

I'm not at all convinced it's much of an advance, but it seems to me more worth thinking about than what currently seems to be the preferred answer to questions 1. and 2.: "because of some horrifically complicated lowest energy state of some ill defined infinite dimensional minimization problem we know hardly anything about". At least it's different and has a bit of mathematical beauty.

19. **Kasper Olsen**
November 9, 2005

Dear Peter,
I agree with you, that 1. and 2. should be answered in a more 'fundamental' theory. But I don't actually think that Baez answered these questions. He starts with SO(10), but for no obvious reason, except that of course SO(10) has SU(5) as subgroup which has the SM group (= G) as subgroup. But of course SO(10) has many other subgroups which as well could be 'interesting' for the same reasons. The group G has the nice feature observed by Baez, that representations of Spin(10) restricted to G includes the fermion representations given by the exterior algebra *Lambda(C^5)*. But this fact seem (at least to me) to be implicitly included in the old grand unified SU(5) theories. I don't think this *really* answered the question #2 about the representations.

20. **woit**
November 9, 2005

Hi Kasper,

As I said, he leaves unanswered the question "Why 10 dimensions?" If you decide that 10 dimensions is fundamental and you want to do Riemannian geometry in it, you've got an SO(10) structure group.

For a mathematician, if you're doing geometry in an even number of dimensions, picking an almost complex structure is a very natural thing to do (i.e. you're identifying the tangent spaces with complex vector spaces). Doing this reduces the structure group to U(5). The Calabi-Yau structure that further reduces to SU(5) is a bit less of an obvious thing to do.

If your're working with SO(10) (or, better, its double cover, Spin(10)), the spin representation is a very special one, and you can build all other reps out of it (i.e. you can build all tensors out of spinors, not vice versa). The fact that this spinor rep gives you a generation of standard model particles is well-known. In general, for SO(2n), once you pick a complex structure and reduce to U(n), the spinor rep becomes the complex exterior algebra rep, up to a certain projective factor, one which is trivial if you just look at SU(n). So that's why John can explicitly construct his spinor rep as an exterior algebra.

21. **Kasper Olsen**
November 9, 2005

Dear Peter – It's of course correct what you are saying. If you just start by assuming D=10, then the structure group is SO(10), and since D is even, it reduces naturally to U(D/2)=U(5). And then – out of the blue – comes the assumption that the manifold should be a Calabi-Yau which is unique in the sense of having a SU(5) holonomy (while in string theory, as you know, a SU(n) holonomy comes from the requirement of the existence of a covariantly constant spinor). But here, the assumption is not justified – you could as well have chosen the manifold to be Kahler with first Chern class c_1 *different* from zero, and therefore not CY. But again, only with SU(5) holonomy here can the spinor representation be identified (up to isomorphisms) with the exterior algebra Lambda(C^5), which (roughly) is identified with a generation of fermions. How did that "explain" the #1 on your list — that our Standard Model gauge group *should* be exactly SU(3)xSU(2)xU(1)/Z_6? (Which has some nice geometrical features observed by Baez and many others) And how did the coincidence, that the fermions 'fit into' the exterior algebra here "explain" that our Standard Model fermions should transform as they do? (I.e. answer your question #2, except from the trivial observation, that the representation of Spin(10) decomposes as determined by the exterior algebra mentioned above). If I'm missing something here, please explain 😉

Best, Kasper

22. **somebody**
November 9, 2005

I'm simply amazed at all LM's ignorant stuff about holonomy groups and "symmetries". Holonomy groups are not symmetries and have absolutely nothing to do with isometries….jeez….

The only problem I see with JB's stuff is this: is there a manifold with holonomy that is not just *contained* in G, but actually *isomorphic* to it? I think the answer is no, so the actual holonomy group would be a subgroup of the standard group. And that is much less interesting. Still a stimulating paper though.

23. **woit**
November 9, 2005

Hi Kasper,

To me the not very well-motivated stuff is the extra structure needed to get from U(5) to the standard model group. This is the Calabi-Yau structure on the 10-manifold and the splitting of the tangent space into 2 and 3 d complex spaces. That the standard model particles fit precisely into an SO(10) spinor rep has always been perhaps the most appealing feature of SO(10) grand unification. This is just representation theory. Given any irreducible rep of G, you can ask how it decomposes into H irreducibles for H a subgroup of G. Here G=SO(10), H the standard model group. It's quite striking that the simplest SO(10) irreducible, the spinor, decomposes as precisely the sum of irreducibles that makes up a standard model generation.

24. **john baez**
November 9, 2005

Peter has tried his best to clarify certain issues, but I can't resist adding my two cents, since it's my paper…. 🙂

Thomas Larsson said:

> However, it seems to me that two-step breaking, SO(10) -> SU(5) -> SU(3)xSU(2)xU(1) is also ruled out because of the SU(5) in the middle. Doesn't this mean that any kind of GUT where SU(5) plays an important role, apparently including John's, has trouble with proton decay?

You raise an important question here, and I think we should talk about it. But for now, I just want to make something clear:

My paper *does not propose a grand unified theory*, or any sort of physical theory. It simply points out some facts relating the Standard Model and Calabi-Yau manifolds.

There's a reason I'm doing this. So far most approaches to deriving the Standard Model from a more "beautiful" or "unified" theory seem rather artificial. One reason is that the math of the Standard Model looks, to our eyes, ugly and arbitrary. To address this problem, I think some of us should play around the math of the Standard Model… until maybe we see that it's not so ugly and arbitrary after all!

In other words, some of us should try to connect the Standard Model to ideas that seem natural and inevitable. (Other people should explore other ideas, including the "landscape" scenario which has the Standard Model as a completely undistinguished inhabitant of a vast panoply of theories.)

Connecting the details of the Standard Model to truly beautiful mathematics is a tough challenge. Georgi and Glashow's SU(5) and SO(10) grand unified theories make a promising start. So does Alain Connes' ideas on the Higgs boson. So does Victor Kac's work on the exceptional Lie superalgebra E(3,8). So does work on the heterotic Standard Model and the recent attempt to get the minimal supersymmetric Standard Model from the heterotic string. Various people who post here have also done their bit, with greater or lesser success.

I think all these efforts are praiseworthy, even if it turns out that none of them are "the right theory".

Indeed, some of this work, like Victor Kac's work and my own little paper, does not even propose a theory of physics! It still might come in handy.

We need to explore. We need to play around. We need to stop being so grandiose that we think we're gonna write a paper that proposes The Theory of Everything and gets it right the first time.

Robert wrote:

> the CY2 x CY3 is not what we have in nature: : At least the large four dimensions are very likely to have full SO(3,1) holonomy as there seem to me arbitrary curvatures around.

Right: had I been proposing a theory, I would not have proposed one in which the visible 4 dimensions of spacetime are modelled by a Calabi-Yau manifold.

The connection on the Calabi-Yau 2-fold (=CY2) in my paper is clearly related to the electroweak force, not gravity.

Kaspar Olsen writes:

> […] the Baez article makes an elementary mistake in the physics underlying compactifications from higher dimensions. Unbroken gauge groups *do not* come from holonomy, and generally the exact opposite is true.

Since I wasn't proposing a physical theory, I was not in the position to make this elementary mistake. You may have thought I was proposing something like a Kaluza-Klein theory, or perhaps planning to use my CY2 x CY3 as a string theory background. I wasn't. I was doing exactly what I said I was doing in the first sentence of the paper. I wrote:

> The purpose of this note is to point out a curious relation
> between the mathematics of the Standard Model and the geometry of Calabi–Yau manifolds.

No more, no less.

25. **D R Lunsford**
November 9, 2005

Does this have anything to say about parity in Minkowski space? (i.e. a direct statement about weak interaction)

-drl

26. **john baez**
November 9, 2005

Whoops! I just said that Kaspar Olsen wrote:

"… the Baez article makes an elementary mistake…"

It was Nick Warner who wrote this.

27. **Who**
November 9, 2005

in case anyone's interested, this posted today:
http://arxiv.org/abs/quant-ph/0511096
A Polynomial Quantum Algorithm for Approximating the Jones Polynomial

Dorit Aharonov, Vaughan Jones, Zeph Landau
26 pages

"The Jones polynmial, discovered in 1984, is an important knot invariant in topology, which is intimately connected to Topological Quantum Field Theory (TQFT). The works of Freedman, Kitaev, Larsen and Wang provide an efficient simulation of TQFT by a quantum computer, and vice versa. These results implicitly imply the existence of an efficient quantum algorithm … Unfortunately, this important algorithm was never explicitly formulated. Moreover, the results of Freedman et. al are heavily based on deep knowledge of TQFT, which makes the algorithm essentially inaccessible for computer scientists.
We provide an explicit and simple polynomial algorithm to approximate the Jones polynomial …Our algorithm does not use TQFT at all. By the results of Freedman et. al, our algorithm solves a BQP complete problem.
The algorithm we provide exhibits a structure which we hope is generalizable to other quantum algorithmic problems…"

28. **john baez**
November 9, 2005

Quantoken writes:

> What exactly is the relationship between number theory and quantum mechanics? I have no idea. Can you explain?

There are lots of relationships. I think it would be quite a digression to explain them here, but I've begun explaining some in various issues of This Week's Finds, especially week199, week216, week217 and week218.

I'm just getting started – in part because I'm just getting started on learning number theory. The really interesting ideas on the border of quantum mechanics and number theory, like the Langlands program, the relation between zeta zeroes and quantum chaos, and Connes' work on the Riemann hypothesis, are pretty darn deep.

Every week I spend a few hours studying this stuff in the UCR coffee shop with James Dolan. Right now we're trying to understand the Taniyama-Shimura hypothesis. As a start, we're trying to see why automorphic forms that are eigenvectors of Hecke operators give Dirichlet series with Euler product expansions. It's great fun!!!

29. **Kasper Olsen**
November 9, 2005

Peter – it's correct what you are saying. But I'll have to ask again: 1) how did the coincidence between the standard model group H and the SU(5) holonomy explain that the interesting group is actually H? – it seems this is what you claimed had been answered.(Except from the obvious thing that you get a splitting of T(M) as C^2 x C^3?) 2) And how did the representation of H in G=SO(10) actually explain why the standard model fermions are in certain representations? Apart from the fact, that SO(10) potentially could be a good grand unified theory group since the standard model fermions fit into representation of SO(10), which has been known for decades…

Actually I think John himself answered above: he is just observing some 'nice' mathematical 'coincidences' between the standard model group H and the Calabi-Yau geometry. And this is of course completely legitimate.

You can always find thousands of reasons for 'why' our SM gauge group should be exactly H, when you actually know what it is. You can also find millions of reasons for why our spacetime seem to have 3+1 dimensions. 4 is an interesting number – there are for example the exotic R^4's – on which you have an uncountable number of differentiable structures. And actually, we should look at not R^4, but rather R^{3,1}. But, that's also interesting; the Clifford Algebra Cl(3,1) is isomorphic to H(2), and the Spin group Spin(3,1) is even more beautiful….

John – no reason to worry; I don't mind you confused me with Nick
😉

Best, Kasper

30. **woit**
November 9, 2005

Hi Kasper,

I should leave this to John, especially since I'm having trouble explaining this, but here's a last stab at this:

Given 10d, John is not explaining "why H", he's showing that "why H" is equivalent to "why certain specific geometrical structures. He has just reformulated the problem, not solved it.

The important point about SO(10) representations is that you get precisely the standard model multiplet, no more no less, from choosing a single SO(10) irreducible, the most basic one, the spinor. That choice is very simple and compelling, whereas the standard model multiplet looks complicated. This idea isn't John's.

31. **john baez**
November 10, 2005

I could spend all night commenting on comments that criticize me for not achieving grandiose goals that I never claimed to achieve, but I won't.

I'll just say this: particle physicists are a touchy bunch! Give 'em a 4-page paper with two propositions and a corollary – I save the word "theorem" for things that are hard to prove – and they'll complain you *didn't explain why the universe must be the way it is*.

Reminds me of the guy on sci.physics who's been viciously attacking me for… *not being the next Einstein!* Hard to argue with that. But there's some kind of "I've gotta save the world today or it ain't worth getting out of bed" attitude going on here, and I don' t like it.

Anyway, back to physics:

Everyone always says how the Georgi-Glashow SU(5) theory predicts too high a rate of proton decay, and that kills it. This is one reason my paper is not about the SU(5) theory: it uses some math from the SU(5) theory, but it's really just about G-manifolds where G is the Standard Model gauge group.

However: I am ashamed to admit, but I must admit here, that I've never carefully gone through the calculation that predicts the rate of proton decay in the SU(5) theory. This is bad, because it's dangerous to take the conventional wisdom on faith: the conventional wisdom tends to leave out the assumptions behind the results. For example: compare how many people know the conclusions of the Coleman-Mandula theorem to how many people remember its assumptions!

So, I should strap myself to a chair and work through this calculation. I should also see exactly how supersymmetry helps. I should see how flipped SU(5) changes things. And, I should see how things are different for SO(10). I know what people say about this stuff. But, I should check it for myself.

Unfortunately, I always seem to have something more fun to do.

In case anyone here is even worse at this stuff than me, I recommend the Wikipedia articles on the Georgi-Glashow SU(5) GUT and other grand unified theories including SO(10), the Pati-Salam model and flipped SU(5). They don't compute proton decay, but they're really not bad for starters!

For more detail while keeping things pretty easy, there's chapter VII of Zee's book *Quantum Field Theory in a Nutshell*.

For even more detail, I recommend Ross' *Grand Unification* and Mohapatra's *Unification and Supersymmetry: The Frontiers of Quark-Lepton Physics*.

These old review articles are also good:

D. V. Nanopoulos, Tales of the GUT age, in Grand Unified Theories and Related Topics, proceedings of the 4th Kyoto Summer Institute, World Scientific, Singapore, 1981.

P. Ramond, Grand unification, in Grand Unified Theories and Related Topics, proceedings of the 4th Kyoto Summer Institute, World Scientific, Singapore, 1981.

But, given how fundamental the topic is, I'm surprised there are not more books on grand unified theories – books that cover every popular theory in excruciating detail, computing the proton lifetime and the running of coupling constants in each one, etcetera.

Maybe I'm overlooking some?

32. **John Gonsowski**
November 10, 2005

"virtually all his assumptions are not substantiated in any way, and this is why he could have decided to consider E_8 or E_8 x E_8 or U(1)^496, or …And I don't think he is suggesting any nice "new things?? with this. Basically, he is only expressing well-known fact in terms of Clifford Algebras"

Yes I know a Clifford Algebra bivector when I see one so that's not the new I was thinking of. It's kind of as you've been discussing, just looking at spacetime geometry and standard model bosons and having this be a potential area for "new". Also as you've been discussing, if you want spacetime geometry, SO(10) is kind of on the short list of known places to look and Baez is not suggesting it's the only place to look. Smith by the way has Clifford Algebra reasons for starting with a single E8.

33. **john baez**
November 10, 2005

Kasper Olson writes:

> […] the inclusion of SU(5) in SO(10) has been known for ages – sure enough, SO(10) has some nice features, but if he's looking for beauty he could as well have chosen E_8 x (something), or whatever else that [has the] SM group as subgroup.

Of course for my "G-manifold" idea one wants a nice small orthogonal group O(n) containing the given group G, to get some reasonably simple description of Riemannian n-manifolds whose holonomy group lies in G. When G is the Standard Model gauge group, O(10) is the smallest one that will work. So, I consider 10-dimensional G-manifolds. And, as you know, I prove a couple of things about them: 1) they're 10d Calabi-Yau manifolds whose tangent spaces split invariantly into 4d and 6d parts, and 2) spinors on them form one generation of the Standard Model fermions.

But speaking of E8….

My most recent spell of thinking about this stuff was last summer, when I was stuck in the Beijing Friendship Hotel. My wife was attending an international congress on the history of science. Whenever I got sick of talks on that subject – held in small, miserably hot and stuffy rooms – I went back to my nicely air-conditioned hotel room, took a shower, turned on the BBC news, and worked on finding patterns relating the Standard Model to the octonions and exceptional Lie groups. It was especially fun because I hadn't brought any notes with me, so I had to rework everything from scratch. Endless hours of amusement!

I found some neat stuff which I will develop further when I have more time, and eventually write up. The quickest bit to describe was this thing about G-manifolds, so that's what I wrote up first. Exceptional groups will make their appearance later.

34. **Quantoken**
November 10, 2005

John:

I'd be interested in knowing what you come up with, after you strip yourself to a chair and calculate the proton decay lifetime.

Mean while, I am making this observation from what I know. All particle decaying phenomena has a decay life time spanning a great range of order of magnitude, from as short as femto second to as long as billions of years. However the longest decay lifetime observed do not exceed a norminal value times the age of the universe scale. We have not seen anything that decays at a time scale like say a thousand times the age of the universe, or a million times, or 10^10 times. **A few billion years seems to be an upper limit**.

Any one know a counter example that the decay lifetime is actually observed to be significantly higher than a norminal multiply of the age of the universe? Please let it be known if such counter example exist. If not, then there is a reason why it can go from femto second to billions of years, a dynamic span of more than 40 orders of manitude. But it doesn't go beyond that. I have a perfect reasonable explanation why that is the case. But I shall refrain from discussing it here.

Quantoken

35. **Passby**
November 10, 2005

Quite interesting topic and discussions, although I don't understand what your guys talked about!

36. **a**
November 10, 2005

Just to quickly answer the question above: there are no 10-dimensional CY manifolds with the standard group as holonomy group. Once you reduce as far as that, you have to go right down to SU2 x SU3. There is an obvious 12-dimensional non-CY manifold with a holonomy group very much like the standard group [namely SU5/G] but I'm not sure what the exact holonomy group is after you factor out the center of SU5. I guess it's ok.

37. **Quantum_Ranger**
November 10, 2005

Did not Ed Witten:http://arxiv.org/abs/hep-th/0211269

http://arxiv.org/abs/hep-th/0304079

get pretty close to moving the 'proton-decay' goalposts?

One major problem of a precise proton-decay rate, is that you first have to have a precise "vacuum".

I do recall that Witten tried his best a number of years ago, and I recall that I could only conclude that the/our PROTON must have its decay mode from another Universe, this is to say that the appearence of the standard model 'proton', must have decayed into that fundemental state "prior" to it appearing at the finite Vacuum in the early Universe.

This other "Universe" may for all intensive purpose be contained at Blackhole singularity 'terminal' junction, as the primordial/first particle in certain cosmological bounce models, aka Turok?

38. **Haelfix**
November 10, 2005

As far as model building in this scenario, I really don't think it would buy you much. You can't deform the CY2 or CY3 at all obviously since you would break the all fundamental gauge group.

Perhaps it might be useful *somehow* to analyze various subtelties of gauge theory using CY machinery, but that seems to me to be somewhat backward. We tend to know more about gauge theory than we do about CYs, and obviously those two specific calabi Yaus picked out aren't terribly illuminating on the general case.

Maybe im missing something.

39. **Tony Smith**
November 10, 2005

"a" says "… there are no 10-dimensional CY manifolds with the standard group as holonomy group. Once you reduce as far as that, you have to go right down to SU2 x SU3. …".

Arthur L. Besse (pseudonym for a group) says as author of the book Einstein Manifolds (Springer 1987) at page 284: "… Hol(g) = U(m) for the canonical Kahler metric on the complex projective space CPm. …".

Green, Schwarz, and Witten, in v. 2 of Superstring Theory (Cambridge 1987) say at page 440: "… a metric of SU(N) holonomy is the same thing as a Ricci-flat metric. This … is our first taste of he fact that in finding states of unbroken supersymmetry we actually are finding vacuum states that obey the equations of motion of string field theory. …".

So, if the superstring theory requirement of unbroken supersymmetry were removed, could John Baez find the 10-dimensional standard model holonomy model that his paper describes ?

If the unbroken supersymmetry requirement of Ricci flatness were removed, could holonomy U(3)xU(2) (which includes the standard model) be achieved by using 6-dimensional twistor-and-conformal-related CP3 along with 4-dimensional CP2 ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

40. **Nick Warner**
November 10, 2005

Dear All

This discussion raises an interesting question about when one should pay attention to numerical coincidences. I get a large number of e-mails claiming to have solve the mysteries of the universe based upon numerology. For my own part I have stared for hours at tables of counts of rational curves in Calabi-Yau manifolds in the hope of relating them to characters of Lie Algebras and thereby see interesting dualities. I did this with the odd success until a colleague told me that he just found his complete eight-digit birthday in a particular divisor set ….

So here is the question: When is numerology pointless and when is it valuable? (There are lots of examples of the latter…. so I am not asking for a catalog.) When do we say "enough!"? There are some obvious criteria for when it is useful:
(i) When the number of numerical coincidences is outrageously large
(ii) When there is a conjectured underlying mechanism for which one is trying to gather evidence
(iii) When the setting suggests many possible mechanisms and the task is to find the correct one ….
(iv) When your numerology satisfies (i), (ii) or (iii) and does not have an elementary explanation that is already well understood.
Conversely, numerology is probably pointless if you satisfy none of the conditions above.

Put more practically, Physicists and Mathematicians have lots of demands on their time and if you are going to attract their attention you have to get beyond the "crank-mail filter" which means that your numerical coincidence better have more than a few matching numbers.

So we come to my earlier post. John's article (from my perspective and, I think, Lubos') fails to meet (i), (ii) and (iv). One might hope that it meets (iii), and the burden of my original post was to examine this possibility in more detail, and expalin why it actually fails (iii). That is, based upon standard approaches and mechanisms of compactification the expressed hope behind the numerology cannot work. Given this, it is especially important to postulate, even very vaguely or roughly, some new proposed mechanism by which the numerology could be given meaning. To hide behind statements like

"Since I wasn't proposing a physical theory, I was not in the position to make this elementary mistake."

simply makes me (and probably most other physicists) move the idea to the pile of amusing but insubstantial numerical coincidences that arrive in the mail each week. It is possible that this approach will indeed cause me to throw out the solution to life, the universe and everything, but the practical fact is that we all have to make judgements of what will be profitable to pursue and for me John's paper does not cross this threshhold.

More constructively, it might be useful to have a discussion on what kinds of conjectures and what kind of evidence is needed in order to engage the average theorist more deeply ….. The criteria will have some interesting universal properties, and probably a number of really quirky individualistic ones …. I suspect that the ones I have given above are pretty universal (but that might just be the arrogance of a particle physicist….)

41. **Michael**
November 10, 2005

Hi Nick,

nice post. I would like to add one 'negative' criterion: If the mechanism suggested by numerology is obviously not operating in nature, it shouldn't be taken seriously.

In the present example, quoting Lubos, the "four dimensions of the space we know do not transform under the electroweak SU(2), and the six hidden dimensions do not transform under SU(3)." This seems good enough to dismiss John's idea as not relevant to physics, doesn't it?

42. **DMS**
November 10, 2005

Nick,

What you say is interesting and thoughtful. It is a matter of personal taste what one works on, of course.

However, there is a LOT of string-inspired "numerology" (think "realistic brane models")that is out there, that do not explain ANYTHING about the physics beyond the standard model(and in my opinion, never will). So for *some string theorists* to be going it bullying and insulting anyone coming up with any idea that is not completely string-inspired is a bit rich.

Peter,

Seems like you had a busy day yesterday 🙂 Thanks to the arrogant attitude of some(I think they believe they are the next Pauli; well, they are, minus the contribution to physics), I and others I know have changed our attitude from:

"(Ok, some)String theorists are arrogant, but they may have a point."
to
"(Ok, some)String theorists are arrogant and **liars**, who thrive on delibrately distorting other people's work, likeley because they have nothing useful to say."

It is amusing to note that Clifford was upset about Krauss' opinions. Many physicists I know have a **much worse opinion** of the field(unfair, in my opinion).

DMS

43. **Kasper Olsen**
November 10, 2005

Dear John,

I guess we can all agree that the standard model is not the *final* story – it is basically just a low-energy effective field theory. So, some of its mathematical 'coincidences' might be important and some might not. Therefore, as long as you do not have a more 'fundamental' theory, I doubt you will be able to tell which of these coincidences are important and which are not.

Best, Kasper

44. **X**
November 10, 2005

Modern physics seems to be mostly about piling up beautiful symmetries; with the symmetry breaking required to match the real world as an ugly and arbitrary and an afterthought – this is my subjective impression. Is there some framework in which symmetry breaking is more elegant?

45. **Chris Oakley**
November 10, 2005

Hi X,

I completely agree. I remember asking "Is there anything better than the Higgs mechanism?" when studying QFT for the first time in 1980/1981 in Cambridge, UK. The lecturer – Peter Goddard, now running the IAS – answered with a forceful "No". He could justifiably give the same answer today. Whilst I can see the attraction of spending most of one's time doing classical field theory – it is easier and more elegant, after all – ultimately *quantum* field theory is the thing that actually counts and I have no doubt that more focus here would quickly lead to constructs greatly superior to the Higgs mechanism.

46. **john baez**
November 10, 2005

a writes:

> Just to quickly answer the question above: there are no 10-dimensional CY manifolds with the standard group as holonomy group. Once you reduce as far as that, you have to go right down to SU(2) x SU(3).

Well, that would be a bit sad. Could you point me towards a proof?

47. **Who**
November 10, 2005

the recorded talks from Loops '05 conference are now available

http://loops05.aei.mpg.de/index_files/Programme.html

in many cases the notes are also available for download as PDF or ppt.
just click on the speaker's name

48. **Alejandro Rivero**
November 10, 2005

John, I am unsure it is related to your paper, but if you are going towards using GUT SU(5) or SO(10) groups without running up to GUT scale as traditional, it could be worth to bring into attention my unpublished note on extracting GUT angles from Z0 decay only. It is at

http://dftuz.unizar.es/~rivero/research/gut.pdf

(Referee told that it could do a good footnote to a more important paper. I am a writer of footnotes, you see)

49. **andy.s**
November 10, 2005

Summing up all the contributions, it looks as it John B's first comment in the thread was right on the money.

It took a lot of effort, but it was worth it.

50. **woit**
November 10, 2005

Nick,

John's idea isn't really numerology, more just a geometrical characterization of the standard model group. The only real numerology is the standard model multiplet as SO(10) spinor, which isn't his idea.

From the way you refer to Lubos, I take it you don't see much wrong with his behavior. Suit yourself. But do read DMS's comment. I don't think you or most string theorists have a clue how much damage Lubos and Susskind together are doing to how string theory is now viewed by other physicists.

51. **a**
November 10, 2005

JB wrote: a writes:

Just to quickly answer the question above: there are no 10-dimensional CY manifolds with the standard group as holonomy group. Once you reduce as far as that, you have to go right down to SU(2) x SU(3).

Well, that would be a bit sad. Could you point me towards a proof?

OK let me back off a moment and say that I was talking about the simply connected case. Back to that in a minute. S[U2 x U3] is a subgroup of U2 x U3, so we [as you know] are talking about a *Riemannian product* of Kaehler manifolds. But for a product of two manifolds to be Ricci-flat, each one separately has to be Ricci-flat. Hence the holonomy group has to be contained in SU2 x SU3. Somehow writing it out like this makes it look trivial, but there is something highly non-trivial going on here; see Besse's book.
All this is in the simply connected case. In the more general case you might, by cleverly taking a Riemannian quotient that affects both factors non-trivially, get a holonomy group which is a *disconnected* group which sort of interpolates between SU2 x SU3 and the standard group. That might be interesting but it still wouldn't be the standard group itself of course.
Let me conclude by saying that although I have no clue as to how your idea can be used, at least I opened the paper and looked at it; which is more than I can say about the dry technical exercises which, particularly over the last couple of years, have clogged the arxiv. And several of the responses in this thread made my jaw drop after the manner of one of the skeletons in "Corpse Bride". I do hope that people won't be discouraged from posting papers like yours. God knows, we don't exactly have an oversupply of interesting papers these days……

52. **Ruadhan**
November 10, 2005

I think Nick's question is answered by the minimum description length criterion. Essentially, the log probability that a particular conjecture, C, which happens to explain some data, D, is just a coincidence, is given (more or less) by the amount of information needed to specify the conjecture (the complexity of the conjecture) minus the amount of information needed to specify the data. To apply this in practice you need a way to "encode" conjectures, or rather, you need to make a list of all conjectures, and then the complexity of a conjecture is the log of its position in the list. (Be careful, though – you can't simply choose an ordering which puts your conjecture at the top; you need to choose a way of generating the list which doesn't require much information to specify).

I've never seen anybody apply this procedure to the problem of classifying physical theories, but I think it would be a great idea for somebody who isn't me to work on. It's exactly the kind of procedure that will tell us how surprised we should be that this apparently simple group mod that one looks like the standard model. It's numerology done properly, so to speak.

53. **Tony Smith**
November 10, 2005

"a' refers to Besse's book and says "… In the more general case …[than]… the simply connected case. you might, by cleverly taking a Riemannian quotient that affects both factors non-trivially, get a holonomy group which is a *disconnected* group which sort of interpolates between SU2 x SU3 and the standard group. …".

However, Besse says at pages 323-324:
"… 11.21 Theorem. Let M be a compact Kahlerian manifold with vanishing real first Chern class. Then M admits as a finite holomorphic covering the product of a complex torus by a simply-connected Kahlerian manifold, again with vanishing real first Chern class.
…
Theorem 11.21 allows us to restrict the study of the compact Kahlerian manifolds with vanishing real first Chern class to the study of simply-connected ones. …".

In light of that, it may not be feasible for John to evade "a"'s statement that "… there are no 10-dimensional CY manifolds with the standard group as holonomy group. Once you reduce as far as that, you have to go right down to SU(2) x SU(3). …", unless somehow by using noncompact things Besse's Theorem 11.21 might be evaded.

However, please do NOT take this as being critical of John for having written the subject paper. It is, as "a" says, a truly "interesting paper" and such things are indeed all too rare nowadays.
To me, even if John's paper's construction does not apply to Ricci-flat Kahler manifolds, it is doubtless applicable to some non-Ricci-Flat Kahler manifolds,
and
since Ricci-flatness is means (in string theory context) unbroken supersymmetry,
maybe the lesson to be learned from John's paper is that the standard model, and probably nature itself, does not like currently fashionable string supersymmetry (no matter how attached to it some physicists may be).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

54. **Quantum_Ranger**
November 11, 2005

Supersymmetry?

Surely Riemannian Geometry is the crux of General Relativity?.. Relativity is Observer dependant? ..Stringtheory needs a continuious string field background as stated by TS above 'ricci-flatness'? is exactly what you get when observers are removed, an observer is nothing more than a curveture imposed by the presence of an 'observer' being that observers are composed of tangable matter?

Baez paper is really a very complex "Hand-Wavy" investigation into discrete notions of continued or, broken dimensions that energy and fields exist within.

In fact, it is really the bases of how matter and energy are contructed and manupulated, using the techniques of "seperation" or "extended" continuations of inquiry?

55. **Quantum_Ranger**
November 11, 2005

Should have stipulated:Reductionism v Separation
as the standard definition:continuous = Reductionism
"a continued decomposition" from a prefered or real location.

56. **Urs**
November 11, 2005

> since Ricci-flatness is means (in string theory context) unbroken supersymmetry,

That's just the simplest case. When for instance (p-form-)fluxes are turned on susy no longer comes with plain Ricci-flatness in general.

Heuristically this is because further fields have further energy-density which back-reacts on the geometry by its gravitational field.

More technically it is because the susy one is talking about here is the existence of a spinor which is constant with respect to a given covariant derivative. That covariant derivative receives contributions from essentially all of the background fields. For instance Kalb-Ramond flux gives rise to a torsion term, etc.

57. **MathPhys**
November 11, 2005

Agressive behaviour has always been part of the theoretical high energy physics scene in the US. Motl's behaviour is silly and childish. He's so predictible, that by now he's boring.

58. **Tony Smith**
November 11, 2005

Urs says that "Ricci-flatness is means (in string theory context) unbroken supersymmetry" is "just the simplest case. When for instance (p-form-)fluxes are turned on susy no longer comes with plain Ricci-flatness in general. Heuristically this is because further fields have further energy-density which back-reacts on the geometry by its gravitational field. …".

Then, could John's construction be seen as indicating
that "simplest case" Ricci-flat Calabi-Yau superstring theory may be physically unrealistic
and
that perhaps realistic string theory models should be based on non-Calabi-Yau, non-Ricci-flat, Kahler manifolds for 6+4=10 dimensional string "background" (maybe even including my favorite pair , CP3 and CP2) ?

If so, might that mean that the "landscape" based on Calabi-Yau manifolds may have a flawed basis, and that work like John's might be a good way to search for a physically realistic string theory model ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

Tony Smith

59. **Urs**
November 11, 2005

> could John's construction be seen as indicating
> that 'simplest case' Ricci-flat Calabi-Yau superstring theory may be physically unrealistic

Isn't that essentially asking if any spacetime which is not a manifold of S(U(2)xU(3))-holonomy may be physically unrealistic? Sure it may. But unless you find some mechanism why spacetime should have S(U(2)xU(3)) holonomy it's hard to see a relation between one and the other.

> might that mean that the 'landscape' based on Calabi-Yau manifolds may have a flawed basis

The landscape arises precisely due to the freedom of turning on all sorts of discrete values for these fluxes.

But I am no expert on landscape issues, so that's all I am going to say on that point.

There is little point, I think, in trying to see any relations between standard string phenomenology and an approach that tries to find particles in spinor reps.

In ordinary string phenomenology, the fermions arise from the R-NS and the NS-R sector of the string. This means that they are really just spacetime spinors. Now, if you identify the components of that spinor with particles, you are left with something like scalars, no?

That's why I said above that – if you wanted to identitfy spacetime spinor components with spinning particles, you'd need to supply another set of ordinary spinor degrees of freedom. For instance by using "bispinors" or whatever you'd call them.

That's just a very vague observation. It is not new, but has been investigated in papers like the one I mentioned above. Being totally untrained in phenomenology, I cannot judge the viability of such ideas. All I can do is make the general observavtion that such bilinears in spacetime spinors appear in the string's spectrum, too. Namely in the RR sector, where they are known as RR p-form fields.

Make of that what you wish.

60. **Juan R.**
November 11, 2005

What is your opinion on

the relationship between Cosmology and quantum electrodynamics claimed on Hoyle F. and Narlikar, J. V. *Reviews of modern Physics* **1995**, *67*(1) 113-155?

Juan R.

Center for CANONICAL |SCIENCE)

61. **Nick Warner**
November 12, 2005

Peter,

From my comments about Lubos you can only reasonably infer that I consider him to be "somewhat undiplomatic". I have no wish to be drawn into a more personal discussion. The purpose of my posts was try to explain, more diplomatically, some of the substance underlying Lubos' post and why his particular post here, but *not* his mode of expression, probably represents the views of many people who work on physics from higher dimensions.

I read DMS's comment. It is hard to determine whether his comments were directed at me or Lubos (perhaps this ambiguity was your point, Peter). At any rate, I agree that there is a lot of nonsense under the "string inspired" banner. Indeed, the worst of it is actually fundamentally inconsistent with string theory. However, this is beside the point here. Holding me responsible for the excesses of the "string inspired" is comparable to holding me (a US resident) responsible for George Bush's supreme court nominations ….. . While you may be justified in disliking the messenger and his other messages, one should keep focussed on the actual content of a particular post within a thread. In this instance the subtantial content of Lubos' post is correct, but the way he chose to express it was not.

62. **woit**
November 12, 2005

Hi Nick,

Yes, my point was that the way in which you choose to describe and deal with Lubos's comments here gives the strong impression that you think his behavior is acceptable (if "undiplomatic'), and this will influence people and cause them to draw conclusions you're not going to be happy with.

I understand that there was a substantive point in what Lubos wrote that may have been worth discussing. There also were substantive points in his comment (his claims that John thought he had discovered all sorts of well-known facts), that were stupid, dishonest and offensive, not just undiplomatic.

63. **D R Lunsford**
November 12, 2005

two words: Charm School!

-drl

64. **Nigel**
November 13, 2005

Luboš Motl of Harvard University once wrote something that was sensible: '… quantum mechanics is perhaps the deepest idea we know. It is once again a deformation of a conceptually simpler picture of classical physics.' – http://motls.blogspot.com/2005/07/measuring-depth-of-ideas.html

I can't understand why he seems to think string theory fits into this. String theory is not a deformation of conceptually simpler ideas, it is a real mess with no empirical basis at all. I wish he would grasp that you need to go back and review the evidence, discard obsolete speculations (ST), before making progress.

I know that Motl's being paid some grant by Uncle Sam or whoever to pontificate on crank ST, or he wouldn't be under such pressure. He complains about Baez rediscovering things, but this is what happens in real science! Darwin rediscovered the evolution theory of Anaximander while Copernicus rediscovered the solar system of Aristarchus. Cranks who deny rediscoveries are just creationists believing in an earth-centred world. You have to feel sorry for Motl.

65. **Juan R.**
November 14, 2005

Nigel,

In *some sense* i agree with Motl at this point.

String theory 'born' (of course after its failure on strong-force) from a very simple idea: a string vibrating could offer us a two-spin vibration mode resembling the hypotetical graviton.

Since then string theorists unable to understand gravity (or any other part of real science) have claimed that string theory was "important", "fundamental" or even "elegant". Since 40 years ago, the topic had been modified, radicalized, and today 'string' theory is a 'deformation' of conceptually simpler ideas.

Today, there is not simplicity, there is not elegance, there is not empirical basis, there is not fundamental point of view, there is not unified theory. Is there nothing? Well i think that ego remain and this is the reason that string theorists -specially smart ones -are unable to recognize that they have failed 🙂

In fact, today there is not theory or 'strings'. Now, the conceptually simple idea of reducing 'all' (of course, this was always an exageration from arrogant people as Schwartz or Greene) to a simple vibrating string in 4 dimensions is abandoned.

Now, one finds 11D that reduce to 10D in weak coupling limit, those 10 contain 16 'inner' dimensions in that unelegant mixture of bosonic and fermionic stuff, all aderezed with zero mass and none connection with real particles. The breaking into 4×6 is done by hand, it is not well defined, etc.

The initial single parameter transformed into a mixture of more than 10000 parameters in the best of cases. It is true that initially the posibility for reducing the near 20 of standard model to 1 was really good, but if my math is correct 10000 >> 20 and the situation now is worse.

The theory of everything turned into the theory of nothing.

From the posibility for understand 'all' of our universe they are passing to the unability for understand even one of multiple (posibly infinite) predicted 'universes'.

What is more, from the elegant reduction of dozens and dozens of different particles to a single item. Now there are p-branes (0, 1, 2, 3, 4, 5, 6, 7, 8, and 9). All the theory (so say) offering towers and towers and towers of unobserved effects.

From

> NO, the standard model is incorrect. There is not pointlike objects on Nature. The basic item of nature is a small unidimensional object

Now some of them are claiming

> NO, string theory is incorrect. There is not real unidimensional object on Nature. The basic item of nature is a small pointlike object: the D0-brane

Etc, Etc.

Well i could agree that was a 'distorsion'. One very very great 🙂

Juan R.

Center for CANONICAL |SCIENCE)

66. **Chris W.**
November 17, 2005

This is interesting. I've just skimmed it:

Non Abelian gauge symmetries induced by the unobservability of extra-dimensions in a Kaluza-Klein approach (gr-qc/0511100)

Latest Freed-Hopkins-Teleman

November 10, 2005
Categories: Uncategorized

A wonderful long-promised paper by Dan Freed, Mike Hopkins and Constantin Teleman entitled Loop Groups and Twisted K-theory II has just appeared. They have advertised it in the past under various names such as "K-theory, Loop Groups and Dirac Families", but their latest way of organizing their work seems to be to relabel the two-year old Twisted K-theory and Loop Group Representations (which recently has been updated, improved and expanded with new material) as "Loop Groups and Twisted K-theory III". Working backwards it seems, they now advertise a "Loop Groups and Twisted K-theory I" as still to appear, hopefully in less than two years.

I don't mean to give them a hard time about this. They are doing wonderful work, continually refining and improving on their results, and the paper is worth the wait. At the moment I don't have time to do them justice by explaining much about their results or the conjectural relations that I see to quantum field theory, but I wrote a little bit about this a while back in another context. In the future I'll try and find time to write some more entries about this material.

Also related to this is a new paper of Michael Atiyah and Graeme Segal called Twisted K-theory and cohomology which discusses the relation of twisted K-theory to twisted and untwisted cohomology.

Teleman has also recently made available on his web-site a preliminary version of notes from his fascinating talk at the algebraic geometry conference in Seattle this past summer, entitled Loop Groups, G-bundles on curves. He starts off with some philosophy he claims comes from lessons learned in working with moduli of bundles:

(i) K-theory is better than cohomology
(ii) Stacks are better than spaces
(iii) Symmetry

The first and third points I'm well aware of, and he has convinced me to spend some more time learning about stacks by his next point, which I hope may clarify some issues that confused me when I was writing my notes on Quantum Field Theory and Representation Theory. According to Teleman, the fundamental K-homology class of a classifying stack BG gives a notion of "integration over BG" in K-theory that corresponds precisely to that of taking the G-invariants of a representation. This idea has been a fundamental motivation for me for quite a while. It seems to me that one fundamental question about the path integral formulation of the standard model is "why are we looking at the space of connections and trying to integrate over it?" The K-theory philosophy gives a potential answer to this: we're looking at the space of connections because it is the classifying space of the gauge group, and we're integrating over it because we want to be able to pick out the invariant piece of a gauge group representation. I'll try and write up more about this later, especially if learning some more about stacks ends up really clarifying things for me as I hope.

On a somewhat different topic, Teleman recently gave a very interesting talk at Santa Barbara entitled The Structure of 2D Semi-simple Field Theories.

Comments

1. **Anonymous**
   November 11, 2005

   we're looking at the space of connections because it is the classifying space of the gauge group, and we're integrating over it because we want to be able to pick out the invariant piece of a gauge group representation.

   Hmm. I, for one, would like to see you elaborate on this. I know you wrote some of this up in your notes on the arXiv, but it's nice to see it written up in words. I would like a summary of the idea in "physics language." As I understand it you want to recast the ill-defined path integral formalism as some sort of sophisticated (and currently not well-understood) representation theory. Is that the gist of it? It sounds like this might make QFT more rigorous but it doesn't seem that it would have calculational advantages. What do you think would be the physics applications if your program worked?

2. **AJ**
   November 11, 2005

   What do you think would be the physics applications if your program worked?

   Peter, I'm sure, will provide his own answer, but let me pipe up anyways. Providing a definition of the path integral in terms of the algebraic topology of a stack would almost certainly allow us to introduce new computational tools into quantum field theory. Algebraic topologists have a fascinating collection of Really Powerful Computational Gadgetry, and I for one, would love to see what they can do to QFT. It'd be nice, for instance, to compute correlation functions using simplicial resolutions.

   There would probably also be conceptual advantages. Right now, the status of the path integral is a bit murky. We write down some classical action, and then we define the path integral measure in a rather ad hoc fashion, choosing some regularization. But our computations are supposed to be independent of our choice of regularization. It would be nice to interpret this by saying that each regularization is a different way of representing a fundamental class.

   Or, less trivially, it would be nice to explain anomalies as obstructions to the existence of a fundamental class, just like we think of the Stieffel-Whitney class as an obstruction to the existence of an orientation. This would provide a very clean explanation for the fact that they tend to be measured by characteristic classes.

3. **Anonymous**
   November 11, 2005

   AJ, what you say sounds very nice, but it's unclear to me how one would ever hope to compute correlation functions using algebraic topology. Probably this is because of my elementary understanding of algebraic topology. In what sort of calculation does one really capture the sort of local information we are after in QFT correlators? I'd like some example of an algebraic topology calculation where one gets back a continuous function. (I can see that a calculation might for instance tell one about a DeRham class, but one still has to figure out what is the functional form of the form, yes?) It seems to me that the question of computing correlators where perturbation theory does not apply is currently formulated as a difficult analysis problem (solving some infinite coupled set of Schwinger-Dyson equations, subject to certain constraints), and I don't see how the proposed approach would do any better than reformulating it as a different difficult analysis problem.

4. **woit**
   November 11, 2005

   Hi Anonymous,

   Let me start with an analogy. You can write down the Schrodinger equation for an atom, even a simple one like the hydrogen atom, but then you have a difficult PDE to try and solve. Without exploiting the rotational symmetry group and what you know about its representations (i.e. using angular momentum operators and corresponding decompositions of states), it's hard to get anywhere, but if you exploit what you know about representations of SU(2), all sorts of calculations become possible. Perhaps if we understood better how to reformulate certain QFTs from the ground up in terms of representation theory, we would get all sorts of new insights and possible calculational methods. Maybe some of these would involve the kind of algebraic topology AJ mentions (the Teleman idea I described directly relates representation theory and algebraic topology: you study the representation ring by looking at the K-theory of the classifying space). If you're an incurable optimist, you might hope these new insights will give you a new idea about how to solve one of the big problems of particle theory: e.g. how is electroweak symmetry broken? how do you unify gravity and the standard model?

This is in some sense just a fancy way of saying something well-known: as Weinberg in his latest article points out, physics in the 20th century made progress by finding symmetries and exploiting them. He thinks this idea is played out and we need to change paradigms and abandon it. I don't. I think physicists need to understand that "exploiting symmetry" = representation theory, and mathematicians know a vast amount about representation theory that physicists have never absorbed. Before abandoning hope like Weinberg, people should try and understand this kind of mathematics and see what can be done with it.

The Freed-Hopkins-Teleman stuff is relevant to 2d QFT, but precisely 2d chiral gauge theories, which are the 2d analog of the standard model in 4d. My specific hope is that if you properly sort out the 2d story, you'll get a new idea about how to approach things in 4d. From a more physical point of view, my claim would be that, at a non-perturbative level, chiral gauge theories are still extremely ill-understood. Sure, we know how to use BRST to get a consistent perturbation expansion and to cancel anomalies, but we don't know much about what happens non-perturbatively. There are all sorts of problems with the BRST formalism (Gribov ambiguities just for a start), my hope is that the FHT kind of stuff will allow a reformulation of BRST that makes more sense non-perturbatively, and might expose some new and interesting structure we can use to do real physics.

Anyway, at the moment, this is all a pipe-dream, but I see quite a few reasons to believe that it is a promising direction to pursue.

5. **Lubos Motl**
November 11, 2005

The fact that K-theory is "better" than homology to classify possible cycles (on which D-branes can be wrapped) has been known to the string theory community since 1998.

http://arxiv.org/abs/hep-th/9810188

I placed "better"in quotation marks because there is of course no universal adjective "better". Some things may be described by homology, some things may be described by K-theory, and only more specific statements are meaningful.

Also, the D-brane people have been using stacks, sheaves, and other things for quite some time.

Nevertheless, I am happy to see that some mathematical insights ignited by string theory have become so powerful that even Peter Woit was forced to notice and admit the reality.

6. **woit**
November 11, 2005

Despite what certain string theorists think, K-theory and sheaves have been around since the 1950s, stacks since around 1970. It's just absurd to claim that they are "mathematical insights ignited by string theory". Atiyah has been promoting the idea that K-theory is sometimes more fundamental than cohomology since the early 1960s.

7. **AJ**
November 11, 2005

AJ, what you say sounds very nice, but it's unclear to me how one would ever hope to compute correlation functions using algebraic topology. […] I'd like some example of an algebraic topology calculation where one gets back a continuous function

Short answer: Use a _really_ big space, something that bears the same resemblance to the space of connections as a sphere does to the smallest complex computing its cohomology. Let me be clear: I don't believe the tools exist yet to treat spaces this huge. But I think there's good reason to hope that they could be developed within the context of homotopical algebra, and I believe that algebraic topologists have been taking steps in the right direction. The elliptic cohomology theories, for instance, give us formal power series, instead of just elements of a finite-dimensional vector space.

Moreover, I do expect that, if we could reinterpret path integrals in this fashion, a lot of the usual gadgetry from algebraic topology would carry over, giving us new methods of computing. Maybe we'd run afoul of conservation of trouble, and computations would still be next to impossible. I don't know. But it's amusing to speculate.

I should probably emphasize that this what I'm talking about here is even more pie-in-the-sky than Peter's suggestion, which (if I understand him correctly) could be crudely characterized as "develop an analogue of geometric quantization which works for the group of gauge transformations". I'm focusing more on the connection between fundamental classes and integration; he's looking more at the representation theory side. What he's suggesting is probably actually a more promising idea — certainly closer to being not-a-fantasy. My only complaint is that I don't know how we're supposed to think about regularization and renormalization in that context. There should be some conceptual explanation for the fact that the choice of regularization doesn't matter in well-defined theories.

8. **Lubos Motl**
November 11, 2005

Dear Peter, I am not talking about K-theory being universally more fundamental than homology because, as I've indicated, this statement is meaningless. I am talking about a very specific example where homology played (and plays) a role, which are wrapped D-branes. Apologies to Prof. Atiyah, but some general statements that a legitimate object is universally more interesting than another legitimate – and very natural – object have no testable meaning.

String theory is the only known context in which the question "is K-theory more faithful or fundamental than homology" can have a well-defined meaning.

9. **woit**
November 11, 2005

Lubos,

"String theory is the only known context in which the question "is K-theory more faithful or fundamental than homology?? can have a well-defined meaning."

Your delusional mania that things only make sense in the context of string theory is getting weirder and weirder, suggesting organic brain damage. If you want to know why Teleman is saying this, read his papers, he has well-defined technical reasons for what he says, reasons which have zero to do with string theory. Similarly, Atiyah back in the early sixties had very good reasons for what he was saying (hint: there's this thing called the Atiyah-Singer index theorem….)

10. **Anonymous**
November 11, 2005

Perhaps by “only known context”, Lubos means “only context known to Lubos”.

11. **Anonymous**
November 11, 2005

So all this talk about using representation theory to define a path integral seems to raise the question: what about scalar field theory? If there’s no gauge group, shouldn’t the representation theory all be trivial? And yet we have no nice mathematically satisfying definition of the path integral for, say, phi^4 theory. I must be missing your point.

12. **woit**
November 11, 2005

I’m not conjecturing that all QFTs can be understood in terms of representation theory, just the fundamental one that has to do with the real world (i.e. the standard model or some extension of it). In 4d, you can rigorously define phi^4 theory, but all evidence is that you end up with a trivial theory in the continuum limit. Which may explain why you don’t see elementary scalars. Actually I think there are things you can say about scalar field theory using representation theory, but they’re pretty minimal (just as in QM for any Hamiltonian you can invoke the Heisenberg algebra, but things get more interesting when there are other symmetries around).

13. **Moshe Rozali**
November 11, 2005

Point that confuses me about this, in topological field theories usually the space of physical states tends to be finite dimensional and the number of observables quite small compared to that of a generic QFT. In generic QFT there are new issues to do with renormalization that are crucial in its understanding (e.g. the difference between asymptotically free and trivial theories will only show up then). Is the statement that some infinite dimensional version of K-theory useful for understanding generic field theories, with all their infinities?

14. **woit**
November 11, 2005

Hi Moshe,

In the kind of 2d QFT that I think Freed-Hopkins-Teleman is relevant to (basically a chiral fermion coupled to a gauge theory), there’s only a finite dim space of observables and it’s topological, when you’re looking at the gauge invariant sector. But the constructions are infinite-dimensional and involve the infinite dimensional gauge group, and infinite dimensional space of connections. Freed-Hopkins-Teleman are working in infinite dimensions. But the end result is a finite dim piece they describe in terms of finite-dimensional K-theory. The only renormalization sort of trickery involved here is the standard sort that gives the anomaly, i.e. you only need normal-ordering to get rid of infinities.

I certainly don’t know much about how to do things in 4d, where you do have to face coupling constant renormalization and infinite dimensional spaces of gauge invariant observables. I don’t believe that this kind of abstract mathematics will give insight into renormalization. Rather the opposite: that what physicists have learned about renormalization will be needed to understand exactly when it even makes sense to talk about things like representations of gauge groups in 3 or 4d, and what kinds of constructions of these actually can work.

Again, I don’t think of these ideas as a general tool that will be applicable to generic QFTs. Rather, I’d hope they will pick out a very specific QFT or set of QFTs, one’s which are very simple in representational-theoretic terms (just as certain special QM systems are special in purely involving representation theory, e.g. the quantization of a co-adjoint orbit in the compact Lie group case.) A.J. is right to say what I’m trying to understand is how to do geometric quantization in certain special infinite dim cases. In my pipe-dream this class of special cases will include the standard model. Anyway, the 2d case is something with a lot of beautiful mathematical structure, so it’s well worth sorting out for its own sake and to see what it teaches you.

15. **Moshe Rozali**
November 11, 2005

Thanks Peter, one of the things that I liked about the topological string on twistor space story is the (probably well known) fact that the sheaf cohomology of the appropriate twistor space is infinite dimensional and gives you the fields of N=4 SYM, that is pretty.

16. **Urs**
November 13, 2005

> what I’´’m trying to understand is how to do geometric quantization in certain special infinite dim cases

BTW, the question recently raised by John Baez on the String Coffee Table seems to be closely related to the desire to understand a field theoretic analog of geometric quantization, I think.

There the question was, in a way, if we can understand something defined by a symplectic 2-form on an infinite-dimensional space in terms of a 2+n form on a finite dimensional space.

In particular, for 2D QFT it seems that it should be possible to re-express geometric quantization in terms of bundles over loop space in terms of gerbes over target space. Maybe, somehow, sort of … 🙂

Polyakovfest

November 11, 2005
Categories: Uncategorized

Last weekend Princeton held a Special Symposium in honor of Alexander Polyakov's 60th birthday. Witten talked about his recent work on Langlands duality. He'll also be speaking about this next week at Rutgers and December 1 and 2 up in Boston.

I hear from someone who attended the symposium that Gross gave a talk with title officially still TBA, but for which he said he'd use the title "Strings and Instantons", since that is what all of Polyakov's titles are. His theme was irresponsibility and he recalled around 1990 having dissuaded Polyakov from going to Santa Barbara and spending his time on the beach, getting him to come to Princeton instead. Of course Gross himself then soon left Princeton for Santa Barbara. Gross also said that, unlike his usual practice, he would end his talk on time since he didn't have much to say due to being busy with the events of the past year.

On a completely unrelated topic, Fermilab recently held a celebration of the tenth anniversary of the discovery of the top quark, and the talks are on-line. Also on-line at Fermilab are some on-going lectures by Chris Quigg on The Electroweak Theory and Higgs Physics.

Comments

1. **Quantum_Ranger**
   November 11, 2005

   Any chance that Atiyah will be present at Gross talk?

   It sound like Gross is expecting some tricky questioning, thus the reason given for the lack of available 'Time' at the upcoming talk, tells me Gross is aware of how interuptions of speaker's can be grossly mis-interpreted.

2. **Thomas Larsson**
   November 12, 2005

   Belavin's talk is probably about hep-th/0510214. Pokrovsky's article about Polyakov's contributions to condensed matter in the same e-print explains why I consider Polyakov to be one of the greatest physicists alive (Einstein, Dirac and Feynman are all dead).

   Jackiw's talk is probably about hep-th/0511065. This is what I'm currently reading.

3. **Amazon.com**
   November 12, 2005

   A book about this blog finally appeared here.

4. **Chris Oakley**
   November 12, 2005

   Re: the above, I found a list of treatments for autism:

   Nutritional Therapies
   Hyperbaric Oxygen Treatment
   Fibroblast Growth Factor 2
   Live Cell and Stem Cell Therapy
   Anti-fungal Treatment
   Antibiotic Therapy
   Naltrexone (NTX) Treatment
   Intensive Educational Therapies
   Detoxification for Heavy Metals
   Craniosacral Therapy

   Would any of these work on String Theorists, I wonder? One could could start the experiments on Landscapers (the useless ones, that is – not the ones who re-design your garden).

5. **Dr. Ranger McCoy**
   November 12, 2005

   does string theory have any laws or postulates?

6. **Alejandro Rivero**
   November 12, 2005

   While Chris Quigg is based on hep-ph/0204104, its companion paper 0404228 is a must.

7. **Juan R.**
   November 14, 2005

   Chris Oakley,

   A 10^120 volts electro-shock therapy?

   Perhaps one could then observe supersimmetric strings…

   I sincerely doubt that works with Landscapers. At least we would search them between the multiple universes first.

   Dr. Ranger McCoy

Of course!!

God is a string theorist. If God is not one then he is not so smart.

String theorists are smartest people of this universe. Landscape corolary: and of the rest of infinite others.

String theory explains more than everything

If anyone is against string theory, then kill her/him (academically, of course).

String theory is not defined, therefore you can define string theory as you want

Remember if you are a string theorist, you are smart. Corolary: this also apply if your contribution to science was zero

Any rival theory, if good, is part of string theory if not, then it is another example of how smart you are

Publish papers claiming contrary things. For example, in a paper string theory is unitary in other it is not and in other paper it is a mixture of two last ones. Then string theory always win. Corolary: you are more smart still

Energy spectra is bounded due to dualities. But 'smart spectra' is unbounded. You can always become more smart still

Rival people is so 'stupid' that when they say a thing they are not saying it, then you can claim that they are saying a different thing they really said. Corolary: if they claim that you are an ignorant, then that mean that they are saying that you are smart. Remember (1/R) => R

If string theory is incompatible with Standard model, the problem is with Standard Model. It is ugly

If string theory is incompatible with experiments, the problem is with the experiment. It was incorrect, or done to a very low energy. Corollary: by each time that energy level to observe effects is sited to more higgher level, smart level is also

If string theory is incompatible with scientific model model, the problem is with scientific model. It is so 'old' and string theory so new and revolutionary…

If string theory is incompatible with string theory, then there is not problem

If string theory is unelegant, then change the definition of elegant

If string theory is nonscientific, then change the definition of scientific

Name 'theory' to any thing that you can think or imagine even if you cannot write anything about it. Example: M-theory. Corollary: in private sure to young impresionable students that M-theory does not exist by the motive of being more smart when you write it by the first time

Public is…

If you are wrong, do not worry, wait to next duality

Maintain the name string theory by marketing purposes

This list is time-dependant, author-dependant, and context depdendant

Juan R.

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Superstrings at Princeton

November 14, 2005
Categories: Uncategorized

Yesterday, Princeton University, as part of an effort to bring physics to a wide audience during the centennial of Einstein's great work of 1905, sponsored a performance of Superstrings. This event featured a lecture by Oxford physicist Brian Foster as well as a performance by violinist Jack Liebeck, and it was one in a series of such events that have taken place around the world.

I've always wondered what non-physicists come away thinking after being exposed to things like this. There's not much real scientific content, lots of wonderful music that has no real connection to the physics at issue, and many impressive analogies that could easily confuse listeners as to what the point of the analogy is. This time, one can see some of the effect the event had by reading an article about it in the Daily Princetonian.

The report recounts how the performers explained superstring theory:

"The concept of superstrings can be illustrated with a demonstration of quantum cookery," Foster said, as Liebeck helped him into an apron. A mesh colander modeled the universe with very fine holes corresponding to fluctuations in the space-time continuum. Foster poured flour through the holes, exemplifying how point-like particles cannot be contained in the universe, making a "delicious mess" on the floor of the stage.

Foster proposed circumventing this problem by making the particles long, rather than point-like, a concept known as particle supersymmetry.

To complete the analogy, Foster introduced uncooked pasta in three different varieties, one for each generation of matter, which he nicknamed "quantum pasta" or "superpasta." Although composed of the same ground-up grain as the flour, these "particles" avoided the problem of the point-like particles, staying contained within the colander.

Besides convincing at least some of the audience that supersymmetry is the idea of using uncooked spaghetti instead of flour, Foster did admit there was no evidence for any of this: "Superstrings may be purely philosophical and may have no measurable contributions to our universe". It might have been more helpful if he'd mentioned that "purely philosophical" here really means "wrong".

Some other facts about physics that the reporter learned yesterday are that:

gravity distorts the smoothness of Einstein's continuum, a problem he attempted to resolve through quantum mechanics.

There are *three "generations" of matter — the quark, lepton and boson.*

Superstring theory will resolve *the large discrepancies in the masses of these elementary particles.*

All in all, it seems to me that these performances are not helping the public understanding of science, but rather signficantly setting it back. I'm sure that those bloggers who are highly concerned about the public understanding of science in general, and string theory in particular, will want to address this issue and demand the immediate cessation of events like this.

Comments

1. **MathPhys**
   November 14, 2005

   This remind me of Brian Greene's last Nova on strings. Educated laymen told me that (unlike his book) they found it shallow and boring. Simplifying things beyond a certain point, coupled with absence of real punchline, gives the impression that string theorists have money and time on their hands, but it doesn't help the cause. I think the public wants to see solid results, like "There was this very big puzzle, now it's solved".

2. **blank**
   November 14, 2005

   Mission Accomplished!

   The less the public actually understands, the better.

   I wonder if Big Ed attended the performance?

3. **cvj**
   November 14, 2005

   Hi Peter,

   I note that the article that you pointed us to ends with the following:

   > "Superstrings may be purely philosophical and may have no measurable contributions to our universe,?? Foster said. However, their existence could be proved if they are large enough to create new, detectible particles when they collide.
   >
   > "If supersymmetry exists, the subatomic particles physicists found over the past 50 years are like various notes that can be played on superstrings,?? the final slide read, as Foster and Liebeck performed a duet believed to have been played by Einstein.

   About his other analogies, I can't comment. I don't fully understand them!

   But he does seem to allow for (and clearly point out) the possibililty that the enterprise may come to naught. Is that not significant? The extracts in your post don't mention these lines. I'm puzzled about that. Please help me.

   Cheers,

   -cvj

4. **woit**
   November 14, 2005

   Hi Clifford,

   I don't think it's difficult to understand what the analogies were trying to explain (perturbative superstrings better behaved than perturbative quantization of GR as a QFT in the ultraviolet), or that they were unlikely to actually transmit any information about this and would instead just confuse people.

   In my posting I did quote Foster's line that "Superstrings may be purely philosophical and may have no measurable contributions to our universe", and commented that this is highly misleading and gives people the wrong idea about what science is. If superstring theory ends up involving "no measurable contributions to our universe", it's not "purely philosophical", it's wrong.

   The last line that you quote is something the reporter says that she copied off the final slide, it would be interesting to hear in her words what she thought it meant. I actually think it is again quite misleading: as you're well aware, the existence of supersymmetry does not imply the existence of superstrings (although I'll readily agree it would be some evidence that superstring theory might be on the right track).

   So I take it you have no problem with this kind of performance and think it should be encouraged?

   Peter

   This discussion is now taking place in parallel here and at CV, which doesn't seem like a great idea. I never know how to handle this. If you want to continue it, pick one of the two places, your choice, and one of us can put a pointer at the other place to the continuation of the discussion.

5. **cvj**
   November 14, 2005

   Whoops, I see a flaw in the last sentence…. susy does not imply strings, of course. that is certainly wrong.

   Cheers,

   -cvj

6. **Alejandro Rivero**
   November 14, 2005

   Well, if SUSY gets to imply QCD, then as QCD implies strings…

7. **cvj**
   November 14, 2005

   Sorry Peter. I replied to your last question over on cosmic variance, since when I posted the comment above (correcting my earlier remark), your comment had not yet appeared here. It seems that we posted here at precisely the same time.

   Anyway, my mistake…just was not sure where you were looking!

   Cheers,

   -cvj

8. **woit**
   November 14, 2005

   Hi Clifford,

   That's fine, comments crossing each other in cyberspace seems to be part of the joys of blogging. If there's more of this particular discussion, I think it will be over at Cosmic Variance.

9. **Tony Smith**
   November 14, 2005

   As I read the Daily Princetonian article about "… "Superstrings," a lecture and concert event with Oxford physics professor Brian Foster, violinist Jack Liebeck and pianist Charles Own, … was sponsored by the physics department as part of the World Year of Physics, I am, as a graduate of Princeton (AB 63 mathematics), ashamed and embarrassed.
   A silver lining in the black cloud is that it validates my decision to change majors from physics to math.

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

10. **Dr. Ranger McCoy**
    November 14, 2005

    Hello!

    I attended Princeton and worked with Professor John Archibald Wheeler.

    It was there that moving dimensions theory was born.

    But because it has a Postualte, is based in logic and reason, and does not use cooking utensils, moving dimenisons theory is banned. But you can google it's postulate which unifies relativity and quantum mechanics.

11. **Dr. Ranger McCoy**
    November 15, 2005

    Actually moving dimensions theory isn't banned–I just haven't submitted it to any journals.

Not too long ago one of the faculty of the physics department here was retiring and throwing out all of his journals, saying it was all “useless rubbish” these days.

Are there too many physicists just publishing bs to get tenure and government grants these days?

12. **Plato**
    November 17, 2005

    Now I understand your kitchen analogy over at Cosmic Variance. 🙂

13. **Plato**
    November 17, 2005

    While you had deleted such comments before, I would hope you will reconsider.

    If the analogies stand for anything then it had by nature as Jacque noted, taken us to a certain point. Now it becomes complicated. Yes for me very much so.

    But as you read on, you must consider how Susskind or others like Wolfgang, or yourself, might have inflicted the evilness of what is “inhernetly right” with observations of the physics involved.

    Do you understand that in context of Krauss’s article, while there is great divergence from it, we might have inflicted our own makeup in this bashing? I am trying to make this point clear.

    I’ll save this entry

    Thanks

Lawrence Krauss at Cosmic Variance

November 14, 2005
Categories: Uncategorized

There's an interesting guest posting from Lawrence Krauss over at Cosmic Variance. I think I'll turn off the comment section on this posting here, since if people want to discuss this, it is probably best done over there.

No Comments

More Swampiness

November 14, 2005
Categories: Swampland

Jacques Distler has a new posting about the Swampland, based on hearing a talk by Cumrun Vafa and discussions with him in Eugene, Oregon. Vafa seems to have made clear to Jacques that what he had in mind was just what he wrote about in his paper, investigating qualitative issues such as what gauge groups could arise in string theory. Jacques notes correctly that if string theory is ever to make contact with experiment, it has to have detailed, quantitative things to say. Vafa didn't think that such things were currently addressable, an attitude Jacques found perhaps overly cautious, although it just sounds to me realistic.

Jacques enlarges Vafa's swampland question to make it include the obvious crucial problem for string theory: given some arbitrary choice of the 120 or so parameters of the MSSM, can you get this out of the string theory framework? He makes much of the fact that current constructions of flux vacua are parametrized by sets that are not continuous, but discrete, although of such a huge if not infinite number that it is unclear whether this is of any practical significance.

Jacques and many others seem to be of the opinion that the thing theorists should now be doing is studying the details and physical implications of these huge numbers of flux vacua. Besides the fact that this is a horribly complex and ugly business, without the slightest indication from physics that it is a promising thing to do, it seems to me to be something inherently doomed to failure. Without knowing what the non-perturbative formulation of the theory actually is, the reliability of the perturbative string approximation one is using is unclear, with wishful thinking the only reason to believe that the real world will correspond to a region where the approximation is sufficiently reliable. Furthermore, these flux vacua constructions have been accurately described by Susskind as "Rube Goldberg mechanisms", and it seems to me likely that one can get just about whatever one wants by further complicating the mechanism. This is the completely conventional way wrong scientific ideas often fail: the simplest version of the idea doesn't work, so people keep trying to fix it by adding more and more ugliness and complexity. Sooner or later the whole thing collapses or fades into deserved obscurity when people finally give up hope of getting anything out of it.

To make the whole question of calculating anything in this framework even worse (something hard to imagine), it seems that there is an inherent theoretical problem with the computational complexity of the question of figuring out which flux vacua correspond to specified observable quantities. Frederik Denef mentioned this in some of his recent talks and Michael Douglas will be giving a talk about this on Wednesday at the KITP, entitled "Computational Complexity of the Landscape". I guess perhaps the new line about all this will be that string theory is the TOE, but it can be rigorously shown that one can't ever actually calculate anything with the theory.

Update: The Douglas talk is now on-line. As far as I can tell he has now given up on the idea of doing statistics of vacua, and is instead concentrating on the problem of whether you can show that, given one of the known flux vacua constructions, some flux vacua give you what you want, e.g. a cosmological constant of the right magnitude. Given how poorly string theory on these flux vacua is actually understood, I don't see that he can even formulate a calculation that makes any sense. But he doesn't actually calculate anything, engaging instead in a long meta-discussion about computability. Kind of a weird performance. Gross seems to have been in the audience, but not spoken up. I hope he hasn't given up.

Comments

1. **MathPhys**
   November 15, 2005

   If they run into computational complexity issues, then that's probably the end of the story. They definitely need a new brilliant idea.

2. **Who**
   November 15, 2005

   MathPhys Says:
   November 15th, 2005 at 7:46 am
   If they run into computational complexity issues, then that's probably the end of the story. They definitely need a new brilliant idea.

   Here's a new idea Ma.Ph., I don't know whether it is brilliant.
   It approaches TOE (joining gravity and particle physics) by deriving features of the Standard Model from beefed-up spin-networks (the LQG states of geometry) so that a quantum description of the geometry includes a description of matter (various numbers of different kinds of fermions)

   I don't know any source for this except a set of slides from the recent Loops '05 conference. If you want to check it out, start reading at around slide #35 in this bunch of transparencies for Smolin's talk

   [url]http://loops05.aei.mpg.de/index_files/PDF_Files/smolin.ppt[/url]

   The abstract has links to both the slides and the recorded talk (video).
   [url]http://loops05.aei.mpg.de/index_files/abstract_smolin.html[/url]

   If you want to watch the video allow a quarter of an hour or so for it to download:
   [url]http://loops05.aei.mpg.de/index_files/Video/smolin.wmv[/url]

   the (so far speculative) model being described here draws on work by Bilson-Thompson
   http://arxiv.org/abs/hep-ph/0503213
   A topological model of composite preons
   ===============
   so whether or not the specifically stringy approaches are bogged and need a new idea (as you suggest) that could be true but string IS NOT THE ONLY PROMISING APPROACH TO JOINING GRAVITY WITH PARTICLE PHYSICS, and this seems to be a new idea in the Loop approach—-in case you or anyone might be interested.

3. **Chris W.**
   November 15, 2005

   Who,

   For a different take on this, still involving spin foams, see hep-th/0403137 and other papers by the same author.

4. **Quantoken**
   November 15, 2005

“Computational Complexity of the Landscape” is an utter understatement of the problem, which makes it sound like a technicality issue of computer science. It is NOT. 10^500 vacuas is a HUGE HUGE HUGE number that few super string theorists even have enough brain cells to comprehend how big the number actually is. To put it in procpsective the univerve has roughly 10^80 various particles and it’s Hawking entropy is roughly 10^120, far shy from the 10^500. You can construct a quantum computer using the whole energy of a gogoplex of universes, and still has not even been able start to enumerate even just a small fraction of the 10^500 vacuas. For all practical purposes and intents, it’s an intractable math problem. Computer science is not the limit, physics is.

Put the computability issue aside, **what makes any one to believe there is ONE, and ONLY ONE vacua, and no more, out of that total of 10^500, that happen to match up to the Standard Model**? There may be more than one “correct answser”. There may be a dozen. Actually there could be much more.

An arbitrary vacua, or any arbitrary theory of any sort, has approximately 1 in 10^12 random chance of matching a particular numerical result to a discrepancy of no more than one part in 10^12 disprepancy, purely by random chance. If we consider one part in 10^12 as a reasonable precision requirement before we consider the experimental result as a matching one, then in average we have one “correct” vacua out of every 10^12 of them.

In all 10^500, lots of them, about 10^488 of them, will be deemed to be “correct” in providing the correct parameters to the Standard Model. When a theory gives 10^488 equally good and “accurate” descriptions of the nature, it is as good as useless.

Quantoken

5. **Scott Aaronson**
   November 15, 2005

   Quantoken: “You can construct a quantum computer using the whole energy of a gogoplex of universes, and still has not even been able start to enumerate even just a small fraction of the 10^500 vacuas. For all practical purposes and intents, it’s an intractable math problem. Computer science is not the limit, physics is.”

   No, that’s not really true. Supposing there were 10^500 vacua, it wouldn’t follow that you’d have to enumerate all of them to find out whether there exists a vacuum with desired properties. Maybe there’s a much faster method. Granted, we *believe* there’s no general way to speed up combinatorial search by more than a small amount; even quantum computers are known to yield exponential speedups only for a few highly-structured problems (such as factoring and discrete logarithm). Right now, there doesn’t seem to be a good reason to think that the problem of (say) finding a minimum-energy vacuum given a collection of scalar fields has the requisite structure. But you need computer science to tell you that. 🙂

6. **Dr. Ranger McCoy**
   November 16, 2005

   Does anyone see the corruption and celine of Wall Street pension plans, Hollywood, and culture tied directly to the decline of Physics?

   Elite groups of snarky insiders have replaced truth and beauty with trash and bureaucracy, all to benefit themselvs at the expense of the public.

   But less people trust Wall Street. Less people are going to see movies. And less people are believing the string theory hype.

   A renaissance is around the corner.

Templeton on ID

November 16, 2005
Categories: Uncategorized

I've criticized the Templeton Foundation in the past for their endless attempts to blur the line between science and religion, supporting some of the most dubious research in cosmology and physics. To be fair to them, at least they are not promoting Intelligent Design, something they make clear in a statement released on Monday. The statement challenges a front-page Wall Street Journal story that referred to Templeton as a supporter of ID. Evidently one of the main pieces of evidence that the Wall Street Journal gave for this was Templeton's support of IDer Guillermo Gonzalez as part of their Cosmology and Fine-Tuning Research Program.

So, if you're interested in seeking funding from Templeton, you'd be aligning yourself with an organization controlled by right-wingers that wants to bring religion into science, but they're not IDers. If you decide to go for it, it looks like Dec. 1 is the day when fq(x), a Templeton funded program run by highly reputable physicists, will announce how to apply for money from them. If you just want to extract money from Templeton for something completely flaky, I'd suggest considering another new program they are funding, Science and Theology Advanced Research Series (STARS), devoted to research "on the ways science, in light of philosophical and religious reflection, points towards the nature, character and meaning of ultimate reality." It appears that, if you play your cards right, you can get a free winter break in Cancun, as well as grants of \$20,000 in walking around money and multiples of \$100,000 to look into this ultimate reality thing.

Comments

1. **Wolfgang**
   November 16, 2005

   Peter,

   > So, if you're interested in seeking funding from Templeton, you'd be aligning yourself with an organization controlled by right-wingers

   would this be (currently) also true of every researcher who seeks government funding in the US ? The last time I checked the US government was "controlled by right-wingers".

2. **woit**
   November 16, 2005

   Wolfgang,

   Very good point. Even worse, I think the US these days is "controlled by right-wingers who want to bring religion into science". At least the Templeton people don't go around starting wars.

3. **Fabien Besnard**
   November 16, 2005

   They say they don't promote ID… But just look at that .

   Staune is in the board of advisors of the Templeton Foundation .

   He is the director of the UIP (université interdisciplinaire de Paris) which received funding from the Templeton foundation. On their website they promote a little documentary I've seen on the french tv which was clearly anti-darwinian (apart from being utterly stupid). So they don't promote ID, they promote anti-darwinism and let others do the dirty work…

4. **Geoff**
   November 16, 2005

   Why the air of ridicule?
   It's a private foundation
   They don't claim to be a source for basic science funding
   Issues of "this ultimate reality thing" actually matter to people, even scientists.

5. **woit**
   November 16, 2005

   My main concern about Templeton is about having a very well-funded foundation trying to inject religion and pseudo-science into science, especially into theoretical physics. The anthropic landscape pseudo-science is already a big problem for theoretical physics, and the fact that it is being promoted and funded by Templeton is part of the story.

   There's a nearly infinite amount of idiocy about religion, science, "ultimate reality" out there and in general I'm not about to waste my time discussing it here, unless it happens to come into contact with mainstream theoretical physics research, which is unfortunately what is happening with Templeton recently.

6. **Dr. Ranger McCoy**
   November 16, 2005

   Is String Theory intelligent design?

   Or unitelligent design?

   For the tax payers who fund the indecipherable smorasborg of snarky, condescending crap, it would appear to the latter.

7. **DMS**
   November 16, 2005

   The situation is some areas of physics is beginning to be a lot like the situation in economics(see **Peddling Prosperity** by **Paul Krugman**). The funding (and media prominence) given to supply-siders (called **charlatans and cranks** by Greg Mankiw), who are basically frauds(unlike conservative economists) dwarfs the money from fed. govt. sources in economics.

Looks like we will see something similar happening in parts of physics that areas that they want to control/shape public perceptions for religious or ideological reasons(cosmology and ID). Their share of the overall funding is only going to get higher thanks to the reckless economic policies currently being pursued.

Obviously, they have been very successful in economics: most think tax cuts, especially for the rich, are the cure for all economics problems. One shudders to think what will happen if they have similar success with cosmology and evolutionary biology. It is sad that there will be physicists, perhaps even some prominent ones judging by recent events, who will gladly take this Faustian bargain.

DMS

8. **Tony Smith**
November 16, 2005

The Templeton statement described by Peter said in part:
"… the Templeton Foundation refuses in its programs to blacklist scholars based on their ideological positions. … Blacklisting is ethically inappropriate in academic contexts. The Foundation believes that proper academic adjudication of important and controversial issues is not by censorship but rather by open scholarly debate and consideration of positions and arguments on the merits or lack thereof. Research scholarship does not proceed by processes of censorship and inhibition of debate. …".

As one who is blacklisted from posting on the Cornell arXiv, I am happy to see that there exists an influential institution that opposes blacklisting.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

9. **Joao Leao**
November 16, 2005

The statement from the Templeton Foundation is perhaps worth reading to the end in order to understand why such unwarranted
"confusion" could have arisen in the mind of the WSJ reporter:

Indeed, it should clearly be recognized that some perspectives that scholars associated with the ID movement have brought to scholarly attention involve matters of very considerable public importance. ID scholars have been prominent critics of the abuse of evolutionary biology today by prominent philosophical interpreters arguing for modern science to be considered as if it provided a clear coherent scientific foundation for philosophical atheism. (Which it most certainly is not: such grandstanding does science a grave disservice in the United States). They also have most unfashionably, but importantly, brought to attention the catastrophic abuse of evolutionary biology by Nazi intellectuals in the 1930's and 1940's in support of racist "master race?? eugenics, leading clearly and directly to the justification of genocide against the Jews.
If this is not an endorsement of ID what exactly is it? Darwinism uncheked leads to either nazism or "philosophical atheism"!? Ho-oh! Which could be worse, I wonder? My guess is that the ID guys and the Templetons are here to prevent such "abuses" to which science is so "prone" and unwilling to denounce… (Give me a break!)

10. **James**
November 17, 2005

Re-read their statement carefully. It is not an endorsement of ID. It says that some of the ID proponents' critiques of the abuse of evolutionary biology have merit. It does *not* say that their proposals to correct the abuses have merit.
This evaluation seems quite accurate: the combination of philosophical reductionism (never questioned) and evolutionary biology *is* used as a foundation for atheism. The place to address this abuse is not evolutionary biology, as we all agree; but instead is the philosophical assumptions used to interpret it. We never bother to teach our kids anything about philosophical questions, we just let them absorb the Zeitgeist. Kids need at least a little introduction to the big questions.
So let's fill that gaping hole and introduce them to the debates about meaning. Just not in science class, even though evolutionary biology makes a great example for discussing a couple of the important concepts.

11. **John Gonsowski**
November 17, 2005

Templeton does not seem any worse than the Quantum Mind 2003 conference that Roger Penrose and the University of Arizona co-hosted. There were things like Hindu cosmology and Jungian Psychology at that conference as well as physics. It was fun watching Henry Stapp show up and do a little debate with one of the Hindu cosmologists who quite enjoyed it and her family took pictures afterwards. The world could use more of that. I don't think Penrose and the U of A are particularly right wing. Here's a nice Henry Stapp quote:

"Pauli`s idea of a regulative principle lying beyond the mind-matter distinction is intertwined with the Jungian concepts of archetypes and synchronicity. Synchronicity refers to the occurrence of representations of archetypes in meaningful coincidences that defy causal explanation … behind the process of nature that we already know and understand there lies another, which acausally weaves meaning into the fabric of nature."

12. **Joao Leao**
November 17, 2005

To James:

I hate to nitpick but the sentence refers *"some perspectives that scholars associated with the ID movement have brought to scholarly attention"* leaving open whether what they (Templeton) applaud is part of the ID dogma or of the ID critique of Evolution, which is already telling. In either case putting atheism and genocide in the same basket (=abuses of Evolution!) seems quite OK for Templeton . Is this what you teach your kids? It sounds much less like a big question than an already-made-up answer…

To John:
I am not sure whether Templeton funded the Quantum Mind conference or the "Toward A Science of Consciousness" conferences in Tucson, but I doubt they did since they don't take credit for it in their Web pages. But I don't think that these are quite the same thing! Unlike Jung, Pauli, Penrose or Stapp, who may have dabbled in mystical speculation on the side, Templeton has a lot of money and an agenda which is not so benevolent or amusing, namely the eradication of secular humanism.

13. **John Gonsowski**
November 18, 2005

Actually to me secular humanism seems at best silly and at worst spiritually draining, kind of like creationism seems at best silly and at worst scientifically draining. One of Peter's links mentions CTNS and the "about us" for CTNS sounds quite fine in a Tucson conference kind of way:

"CTNS is an international non-profit organization dedicated to research, teaching and public service. The central scientific focus of CTNS is on developments in physics, cosmology, evolutionary biology, and genetics, with additional topics in the neurosciences, the environmental sciences, and mathematics. With regard to the theological task, CTNS engages in both Christian and multi-religious reflection. The Christian theological agenda focuses on the various doctrinal loci of systematic theology. The multi-religious agenda attends primarily to theological issues arising from the engagement between the sciences and religious traditions such as Islam, Hinduism, Judaism, Buddhism, Confucianism, Taoism, and indigenous spiritualities. This engagement is best reflected in the work done in the Science and The Spiritual Quest Program (SSQ). "

CTNS seems partnered with the Vatican Observatory which is run by the Jesuits who are quite liberal overall. The Jesuits also operate telescopes at the University of Arizona. I not only don't see a problem, it actually sounds interesting though as a disclaimer I should mention I'm Catholic.

14. **Nigel**
November 18, 2005

Tony:

The Templeton statement on 'blacklisting' is meaningless. Open debate sounds very nice, and was promised also by Britain's Prime Minister who stated we were going to war to defend liberty.

When people wanted to raise the issue of liberty in Iraq during the last Labour Party conference, the Government used its power over the police to have an old member held under the 'Prevention of Terrorism Act' to prevent free speech. He was lucky compared to the guy who was repeatedly shot in the back until dead 'by accident'.

Templeton will have to turn similarly paranoid when it runs into the real world. The whole point of 'blacklisting' seems to be accusing people of terrorism or being unethical, without foundation.

They will simply take the moral high ground like religion, Tony, and when you start asking questions or providing ideas they'll fabricate some irrelevant excuse to suppress you, saying you're a cowboy.

15. **Nigel**
November 18, 2005

Hitting the delete key is not just a string theorist or religious problem, either, as Quantoken says:

'Peter is one of those few scientists "in the circle" who is able to speak out about something wrong with the state of theoretical physics research nowadays. For that I admire him. But while he sees problems with the establishment theories. I am a bit take back that he is NOT willing to look at some alternative approaches and make some judgement.'

http://quantoken.blogspot.com/2005/03/dont-become-scientist.html

(Of course, if Peter does look at alternatives, he steps outside of the circle. Quantoken's GUITAR is a bit like Eddington's claim to have predicted the ratio of proton/electron mass from a quadratic containing 10 and 137, without explanation. Eddington defended himself saying modern physics is based on guessed equations!)

16. **Joao Leao**
November 18, 2005

To John:
Catholic, huh? It figures. The Catholic Church has had a less inimical attitude towards science than the fundamentalist protetstant hordes, at least since that pesky Giordano Bruno affair, and such. Yes, and you are right, jesuits have even, on occasion, made significant scientific contributions, say, as in Boscovitch, Lemaitre or Pat Heelan. Yet these were pursued as part of genuine scientific inquiry goals, not as a "theological task" or "a spiritual quest program" as your CTNS (Catholic TempletoNS?) puts it. Unlike the good brothers, these novel "spiritual sponsors" are about getting scientists to peddle religion for money. Considering the latest financial drainage that Catholic Church is currently attending to, I would estimate the prospects of the CTNS in this crowding field at worst, as you would put it,… silly!

My disclaimer: I find all religions equally objectionable and uncalled for in what concerns scientific matters. Now let us get back to the One True Church of String Theory!

17. **island**
November 18, 2005

Eddington defended himself saying modern physics is based on guessed equations

Dirac did a much better job defending large numbers, and he was thoroughly convinced that nature provided these numbers in a meaningful manner that theory should explain.

He had the same kind of belief about higher structuring in nature that Einstein had.

It's a good thing that we got so much smarter than them… so fast.

18. **Stan**
November 19, 2005

Have a look at this statement from the Discovery Institute. Note the following sentence:

"The Institute does not favor requiring that students learn about the scientific theory of intelligent design."

A new tactic, perhaps?

19. **island**
November 19, 2005

That's only for high school students, not higher-education.

The DI has never endorsed teaching ID in high school, because they don't think that they can win a First Amendment challenge, and they are afraid that losing would have the effect of legally ruling Intelligent Design a form of religious creationism, and greatly diminish any possibility of the movement ever achieving its goals set forth in their "Wedge" strategy.

The ding-bats that *were*on the Dover school board had to revise their original statement to meet the state educational guidelines to include Darwin's "alleged" feelings about the random nature of speciation in order that they had some basis by which to challenge his theory.

Kansas went to Oz with Dorthy… surely, they're in violation of the constitution and every federal educational guideline in existence.

20. **Bryan**
November 20, 2005

"The Institute does not favor requiring that students learn about the scientific theory of intelligent design.??

A new tactic, perhaps? -Stan

Stan, this whole thing is just weird to us in Britain. As a Catholic I see no contradiction between ID and evolution. ID was taught in religious education classes and evolution in biology. It must be a thing peculiar to America that there are rows on this. Brother John, a monk, taught us evolution as integrated in biology. I think he glossed over the differences between ID and evolution by an analogy to modern physics where wave theory and particle theory were taught on alternate days, and the two are not really contradictory, but complementary! Thus, you use ID when thinking religiously, but evolution when thinking scientifically. America only has problems because the religious fanatics try to corrupt biology.

21. **Bryan**
November 20, 2005

String theory fanatics only get into trouble because they try to brainwash the whole of physics before they have any empirical evdence they are right. If string theory was a small backwater of crackpotism, people like Peter would leave it alone. Religions only become dangerous when they try to destroy scientific integrity.

22. **island**
November 20, 2005

Religious fanatics are the only ones that recognize goal oriented design in nature, while ideologically motivated evobiologists anti-fanatically knee-jerk react to deny that any such evidence is even possible, citing multiverses and uncertainty as their chaotic anti-gods of choice.

Ya'll are gonna love me for this, but I see ID as necessary for it's anti-political relevance, since honesty in science has absolutely nothing to do with the debate whatsoever.

One of the most respected members of the evolutionary biologists community, Lynn Margullis, called these anti-fanatics "neo-dawinian bullies"… which illuminates that this is no normal dispute among peers. That was a direct shot at the exact form of "anti-fanaticism" that runs rampant throughout the field and comes about as a reactionary response to the constant barrage of BS from creationists fanatics that would wrongly hold up evidence for design in nature as evidence for intelligent design.

Lynn said what she said as the honored guest speaker at the last evolutionary conference.

… or could this be an IDist…?

The problem with neo-Darwinism is that Random changes in DNA alone do not lead to speciation…

> The problem with neo-Darwinism is that Random changes in DNA alone do not lead to speciation. It was like confessing a murder when I discovered I was not a neo-Darwinist. I am definitely a Darwinist though. I think we are missing important information about the origins of variation. I differ from the **neo-Darwinian bullies** on this point.
> -Lynn Margulis
>
> The only watchmaker in nature is the blind forces of physics, albeit deployed in a very special way.
> -Richard Dawkins
>
> Not just any universe would be one in which Darwinian evolution would work. For example, if a tiny reduction in the early cosmic expansion speed would have made everything recollapse within a fraction of a second while a tiny increase would quickly have yielded a universe far too dilute for stars to form, then such changes would have been disastrous to Evolution's prospects.
> -John Leslie

Don't be so sure any of them on the side of science.

23. **CWB**
November 25, 2005

Ah, yes random mutations alone do not cause spectiation. I don't think anyone claims that – there are such things as natural selection and genetic drift.
I don't think anyone is saying that there is no design in nature, that would be stupid, the arguement is that there is no reason to believe the unsupported IDist position that some vast hyper-intelligent being created life.

24. **Alexander Hellemans**
November 25, 2005

To CWB:

What do you mean by "design in nature?" Is that not the ID position? There are even weaker ID forms, derived from Teilhard de Chardin's idea that evolution is a fact, but it is directed, and was always directed towards the appearance of man. This form of ID has recently raised its head in France, with the help of the Universite Interdisciplinaire de Paris (wich is funded by the Templeton Foundation).

25. **John Gonsowski**
November 25, 2005

The idea of Teilhard de Chardin (a Jesuit) has a modern home in the person of Alexander Wendt.

http://en.wikipedia.org/wiki/Alexander_Wendt

It is also consistent with the transaction models of Andrew Gray, Tony Smith and Chris King where the probabilities for the transactions are for worldlines extending far into the future.

26. **island**
November 27, 2005

Ah, yes random mutations alone do not cause spectiation. I don't think anyone claims that

No, that's false.

What do you mean by "design in nature?" Is that not the ID position?

Yes… idists hold up evidence for design in nature as evidence for intelligent design, which it cannot be without direct proof, and that's all that needs to be said in order to rebut the point, since design can be perpetually inherent to the energy of the universe.

http://www.anthropic-principle.ORG

Design in nature usually gets twisted around by neodarwininans to refer to patterns that fall accidently from what is assumed to be a random process and they reach to unprovable theoretical speculation in order to support this **belief**… so they're equally religious in this respect, since this is origins science that we're talking about… not speculative cutting-edge theory 101. Empiricism rules this debate, and so crackpot theories about multiverses and whatnot are not valid arguments against the implications for "specialness" that Brandon Carter, Robert Dicke and Fred Hoyle found in the constants of nature.

Neodarwinians aren't scientists… they are ideologically motivated antifanatics.

Further reading:

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27. **CWB**
November 27, 2005

By design in nature I mean patterns – not design as in something that was created by any sort of intelligent being. For instance evolution is a process by which things can be "designed" – in a sense but design, or the presence of patterns does not neccesarily mean an intelligent designer.

And Island, where do you get this assertion:

> Ah, yes random mutations alone do not cause spectiation. I don't think anyone claims that
>
> No, that's false.

Perhapes you have some obscure little reference but I've never heard biologist advance the position that evolution is only the mutation of genes and nothing more. To me it seems like your asserting biologists have abandoned natural selection.

28. **CWB**
November 27, 2005

No I do not accept the idea of "evolution with direction", by design I mean pattern and form.

29. **island**

November 28, 2005

You mean… no, you refuse to recognize evidence for evolution with direction.

Are you a string theorist?… cause they don't much care about evidence either.

30. **island**
November 28, 2005

And Island, where do you get this assertion:

Ah, yes random mutations alone do not cause spectiation. I don't think anyone claims that

> No, that's false.

Well, it you'd read what was written you'd know, since it was a credited direct quote of a statement made by Lynn Margulis when she was the honored guest speaker at the last evolutionary conference… this past summer.

Not that it's any big secret, you can also find it in books and about a million other places:

http://www.biology-online.org/2/14_gene_pool.htm
Mutations are random occurrences which change the genome of an organism. They greatly increase genetic diversity, where advantageous mutations are favoured by natural selection and disadvantageous ones are phased out.

31. **CWB**
November 28, 2005

> Mutations are random occurrences which change the genome of an organism. They greatly increase genetic diversity, where advantageous mutations are favoured by natural selection and disadvantageous ones are phased out.

Well that's basically what I'm saying. There you have natural selection acting on populations in which genetic mutations have created advantages and disadvantages. Neo-Darwinian Evolution was just the marriage of genetics with the old natural selection based theory of evolution. Genetics provided a framework in which to understand how characteristics can be inherited and how new features in organisms can arise. This is the first time I've ever heard a non-Creationist claim that evolutionary biologists think that biological diversity is a result of the purely random process of genetic mutation. That's a very standard creationist claim – they totally disregard the more or less deterministic process of natural selection.

As for evolution with direction, that's the stance of many biologists who are also religious but I don't think that would be something you'd want to include in a formal scientific theory. I think your claims of ideology on the part of biologists is unwarrented.

No, I am not a string theorist.

32. **island**
November 28, 2005

Well that's basically what I'm saying.

And I made no creationist claims, but, like Lynn, I believe that we are missing important guiding information about the origins of variation. Lynn's approach is symbiosis, which she's had enough success with to get her the medal of science, (which is commonly called the U.S. Nobel Prize), for her 'outstanding contributions to the understanding of living cells, their structure, and their evolution, as well as for her extraordinary abilities as a teacher and communicator of science to the public'.

Her son's approach, (Dorion Sagan), as well as mine, (independently derived), is that we are driven to higher orders and enabled by the second law of thermodynamics, but mine includes an entropic interpretation of the weak anthropic principle via the life-practical environment in an expanding universe that has an increasing negative pressure component. In case you don't know it, and independent derivation serves as hard supporting evidence.

http://www.taipeitimes.com/News/edit/archives/2004/09/30/2003204990

http://www.intothecool.com/

James Kay was instrumenatal in the above, arrived at in conjuntion with Eric Schneider before he, (very unfortunately), died at a young age, and Dorion Sagan stepped-in.

"Many" biologists are also religious"…

No, actually, a very small minority may be, but "for some reason" evobiologist like to pretend that this is not the case. Why is that?… like I don't already know.

I cannot tell you how many times that I've been called a creationist by evolutionary biologists at the mere mention of the anthropic principle, and I'm an atheist, but this clearly defines the preconceived prejudice that runs rampant throughout the field.

There is no denying this fact to me as I have too much personal experience with evolutionary biologists to believe that you aren't in willful denial of these facts, and Lynn does not call them **Neo-Darwinian bullies** because she is having a normal dispute with her peers. Lynn's statement was clearly a direct shot at exactly the kind of antifanaticism that I speak of.

It's not entirely their fault, because creationists constantly provoke them, but that does not justify the willfully ignorant nihilistic "devil's advocate" approach to the manner that they will interpret evidence. Rather, they refuse to recognize said evidence while citing speculative and unproven cutting-edge theory in order to *explain-away* any implied significance.

In point of supporting fact, there is a big movement in the field right now to quit using mechanistic terminology to describe mechanistic function and systems in nature in order that they can **hide from this evidence.**

But of course they deny this, since it's strictly a matter of interpretation, they can simply rationalize their way to the denial of my interpretation. Denial is very big with them, as they remove the layers of the onion until they can claim that there's nothing there.

These people are not honest scientists, and are led by the mentality of liberal extremists like H. Allen Orr, so do not be fooled by the fact that they are technically "in the right"… where the ID debate is concerned, because evidence for "design in nature" does not and cannot prove that there is an intelligent agent involved without **direct** proof, so hiding from the evidence is completely un-necessary and only hurts their position while furthering their lack of credibility.

I know the game too well… it's about downplaying the significance while selectively ignoring any and all evidence that runs contrary to their motivations.

I don't buy your denial… from experience.

33. **John Gonsowski**
November 28, 2005

Island, multiverses with transactions looking far into the future is actually an arguement for your "specialness". A single particle going one way instead of another to avoid annihilation is a form of intelligence and looking into the future. Extend that to a large spacetime scale and one could get your "specialness".

34. **island**
November 28, 2005

Keep talking like that and Peter's gonna send us packin!… 😉

… but he also doesn't like us to discuss our pet theories here, so please click on my name and goto my website and see about half-way down why I think that multiverses aren't necessary, because I don't believe that anybody ever proved Einstein to be wrong in the first place.

This article and the three that are attatched to it that were made to the spr group also illustrates the point:

http://www.lns.cornell.edu/spr/2005-06/msg0069755.html

The ball is not in Einstein's court… in other words, and the significance of the implications of the anthropic principle increases by orders of magnitude if the universe if finite and bound.

35. **CWB**
November 28, 2005

But what do you want here? You want biologists to throw away the theory of evolution for what exactly, Intelligent Design? Do you want them to include "guidance by a superior being" as part of biological evolution?
I don't understand what you're upset about.

36. **CWB**
November 28, 2005

I don't see how Intelligent Design is useful science, actually I don't see how it's science period.

Also, I am not a biologist.

37. **island**
November 28, 2005

I want them to fight the battle on the right front, which is the ludicrous distance that you have to go in order to attain scientific plausibility for something like alien intervention, which is the actual "science" IDists hide behind.

Distance to plausibility kills it as a comparible and viable theory, and there is no valid reason to fight the battle based on the denial of evidence for goal oriented design in nature, so it won't be a matter for interpretation, which, you would likely lose in court, unless you can prove what the motivations behind it were… even though that's not a valid arguement as far science is concerned.

My understanding is that the physics derives that punctuated equilibrium is analogous to a *near* static flat universe, whereas a big bang convolves characteristics forward in the same manner and by the exact same mechanism, so why would I want to be foolish enough to throw the "writing-on-the-wall" theory of evolution away?

38. **island**
November 28, 2005

As far as I can tell… The TOE… is the ToE.

39. **Tony Smith**
November 28, 2005

With respect to discussion of how IDers and Evolutionists etc stand on the issue of whether speciation can be fully explained by random mutations, what about epigenetic inheritance, such as described by Gail Vines in an article at http://www.ifgene.org/vines.htm ?

Could such epigenetic inheritance be embraced by Evolutionists as just another scientific process that supplements random mutation ?

Could such epigenetic inheritance be embraced by IDers as a way that a purposeful design could influence evolution ?

Could such epigenetic inheritance be a common ground embraced by both Evolutionists and IDers, so that they could "just get along" with each other ,
or do they hate each other so much that they don' t ever want to see any common ground ?

There is a wikipedia article at http://en.wikipedia.org/wiki/Epigenetic_inheritance
but please bear in mind that some wiki contributors have axes to grind, and it is not always useful to take a wiki article on a controversial subject at face value without looking closely (in great detail) at the history of that article.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

40. **CWB**
November 28, 2005

I think the fight against ID is based mostly on the fact that it's creationism in disguise and that it doesn't have any hard science behind any of it.
Now, you're upset that biologists don't accept goal-oriented design in nature? I think this is more of a philisophical position than one that has any real applications to biology.

41. **CWB**
November 28, 2005

> With respect to discussion of how IDers and Evolutionists etc stand on the issue of whether speciation can be fully explained by random mutations

Why do people keep leaving out natural selection and genetic drift? Those are extremely important to evolutionary development, genetic mutation isn't everything you know.

42. **CWB**
November 28, 2005

> Could such epigenetic inheritance be a common ground embraced by both Evolutionists and IDers, so that they could "just get along" with each other

No, IDers won't be happy until Evolution is vanquished. Take a look at the "Wedge Document", the goal of the ID proposed by the Discovery Institute is to be used to discredit science and allow supernatural (particularly Christian/Biblical) explanations to be used in scientific theories. They really don't give a damn about advancing human knowledge or solving scientific problems.

43. **woit**
November 28, 2005

I'm closing this thread, since I'd rather people carry on the Evolution/ID debate elsewhere. There are many, many much more appropriate places for this. This was a posting about Templeton, its policies and funding of theoretical physics, not about evolution vs. ID.

Seed Magazine On-line

November 16, 2005
Categories: Uncategorized

The recently relaunched science magazine Seed has a new web-site. You can read their article on physics blogs, and it will be interesting to see what they do in coming months with the new site.

Comments

1. **MathPhys**
   November 16, 2005

   They couldn't find a less seedy name for a science magazine?

2. **Plato**
   November 17, 2005

   I am sure it will blossum into something quite nice 🙂

3. **Pindare**
   November 17, 2005

   off-topic (but on-topic for the blog istself): apparently this year members of the french math society will get a supplement on physics, and 3 articles out of 8 are on ST:
   http://smf.emath.fr/Publications/Gazette/2005/EditionSpeciale/

4. **Ben Compson**
   November 17, 2005

   Seed has a reputation for not paying its writers–many of them had to wait many months to be paid in the first incarnation of Seed. I suspect it has scared some writers off and wonder if their content will suffer in this second incarnation.

5. **D R Lunsford**
   November 17, 2005

   May it fail as hard as it deserves (even OMNI was less credulous).

   -drl

6. **J. Ellenberg**
   November 18, 2005

   I wrote for them, and I got paid.

7. **Nigel**
   November 19, 2005

   One of the most notorious physics blogs is Peter Woit's Not Even Wrong—the first site this summer to discuss the gravity wave experiment. Although Woit has been critiquing string theory since 2000, he had difficulty finding an audience for his ideas: Journals rejected him, theorists attacked his credentials as an untenured mathematician, and a book he was writing on the subject was "too radical?? for publishers. In 2004, he launched Not Even Wrong and engaged string theorists with pithy and scientifically rigorous posts. A young Harvard string theorist named Lubos Motl was one of the first to defend his intellectual territory on Woit's and others' blogs, often peppering his comments with invective, and eventually starting his own blog "as a balance against Peter Woit.?? With traffic and links to his site expanding, Not Even Wrong carved out a unique space for Woit's criticism that broke through previously impenetrable walls, and helped his ideas gain credibility. "People have told me that they have changed their career plans because of my criticisms,?? Woit told us, adding, "They used to think I was crazy. Now I think they're warming to me.?? –
   http://www.seedmagazine.com/news/2005/10/a_new_force.php

   What made Peter think Lubos Motl and other string theorists are 'warming'? Getting overheated would be be more honest!

8. **Chris W.**
   November 21, 2005

   Let's hope *Seed* doesn't publish much more **crap** like **this**.

Jim Simons in the New York Times

November 19, 2005
Categories: Uncategorized

There's an article (unfortunately not available for free) in today's New York Times based on an interview with the normally publicity-shy mathematician Jim Simons of Chern-Simons fame. Simons runs the incredibly successful hedge fund Renaissance Technologies, and I've written something about this earlier. The New York Times article describe his mathematical career as follows: "A former crypt analyst – a code breaker, that is – he did important work in mathematics that helped lay the foundation for string theory."

Comments

1. **Cameron Peters**
   November 19, 2005

   Off topic, but did you see the mention of your blog in the latest issue of Science?

2. **woit**
   November 19, 2005

   Hi Cameron,

   No, I hadn't seen it. Thanks for pointing it out. For anyone interested, it's at the bottom of:

   https://science.sciencemag.org/content/310/5751/1099.5

3. **Tony Smith**
   November 19, 2005

   The science web page cited by Peter also mentions John Baez, saying:
   "… For nearly 3 years, mathematical physicist John Baez of the University of California, Riverside, has discoursed on books and papers that catch his interest …".

   In fact, John has been writing "This Week's Finds" since 1993. His first entry can be found at http://math.ucr.edu/home/baez/week1.html

   Could the description of the time period between 19 January 1993 and now as "nearly 3 years" be indicative of the lack of mental discipline that has crept into science reporting during the superstring era?

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

4. **Chris W.**
   November 19, 2005

   Maybe that was supposed to be "nearly 13 years", ie, a copy-editing oversight rather than sloppy reporting and fact-checking.

5. **Tony Smith**
   November 19, 2005

   Chris, you say, about the science web page description of John Baez's This Week's Finds that says it has been written by John "… For nearly 3 years …",
   that
   "… Maybe that was supposed to be "nearly 13 years??, ie, a copy-editing oversight rather than sloppy reporting and fact-checking. …".

   Are you suggesting that the reporter/writer wrote "nearly 13 years" and that a copy-editor changed it to "nearly 3 years" ?
   If so, it would appear to me that such copy-editing, which changes correct writing to incorrect publication, is far worse than a mere "oversight".
   It might even indicate that editors, seeing a 13-year time frame, are so ignorant, lazy, and stupid that they just cannot believe that something similar to a blog could have possibly existed in the far distant past beyond 3 years ago, and that they are so confident of their wrong concept (no blog-type stuff in such distant past) that they are too arrogant and lazy to even look at John's web site to which the article itself refers.
   If so, such editors would, in my view, be much like what I regard as typical superstring theorists.

   In the alternative, if the reporter/writer did initially write "nearly 3 years", then maybe the editors would be guilty of no more than oversight (due, probably, to a mixture of laziness and ignorance with a little stupidity thrown in). However, the reporter/writer would then indeed be guilty of what you describe as "sloppy reporting and fact-checking", to say the least.

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

6. **Nigel**
   November 20, 2005

   The arrogance of untestable string theory makes the whole of physics look like a time-wasting fantasy, so why should editors waste time checking the details?

7. **anon**
   November 20, 2005

   Let's face it. Science journalism at present is attrocious. Often, 90% of an article consists of a press release from a university, full of disingenous exaggerations. I also particularly dislike the cliche story trying to portray a particular scientists as an amazing individual that young ones should admire and emulate. Well, I've know a lot of Physicists, having a Phd in physics myself, and very few were worthy of my admiration as human beings. I love physics, but physicists I could happily do without. Paul Farmers they are not.

8. **frank flaherty**
   November 29, 2005

jim simons is a co-author in chern-simons. he also bet the right way on the british pound when maggie thatcher was PM.

A Particle Theorist's Perspective on String Theory

November 19, 2005
Categories: Uncategorized

There's a new posting over at Cosmic Variance by JoAnne Hewett of SLAC about string theory, entitled A Particle Physicist's Perspective. It gives a good idea of what I believe most non-string-theorist particle physicists think about string theory.

She does express some very controversial views, ones that are widely held in the physics community, but rarely publicly expressed:

I find the arrogance of some string theorists astounding, even by physicist's standards. Some truly believe that all non-stringy theorists are inferior scientists. It's all over their letters of recommendation for each other, and I've actually had some of them tell me this to my face.

and she describes string theorists as holding the arrogant belief that

String theory is so important that it must be practised at the expense of all other theory. There are two manifestations of this: string theorists have been hired into faculty positions at a disproportionally high level not necessarily commensurate with ability in all cases, and the younger string theorists are usually not well educated in particle physics. Some literally have a hard time naming the fundamental particles of nature. Both of these manifestations are worrying for the long-term future of our field.

I suspect that some of Hewett's strong feelings about this come from being at Stanford, where the theoretical physics group is made up mostly of members of the looniest wing of the string theory enterprise. The logo of the new web-site of the Institute for Theoretical Physics there is a representation of the multiverse, and Stanford is probably the major center for landscapeology in the world (and perhaps in the multiverse).

My alma mater, Princeton, is rather different in that landscapeology is not popular, but the particle theory groups both at the university and at the Institute have only hired string theorists for the last twenty years, displaying the kind of attitude that Hewett finds disturbing.

While most string theorists demonstrate no more than the usual theoretical physicist's helping of arrogance, it has certainly been my experience that some of them display a degree of arrogance that is pretty astounding. This includes some of the earliest and most prominent string theory bloggers, where the phenomenon is pretty much off-scale. When it comes to purely intellectual arrogance and confidence in one's own beliefs, I'm no paragon of humility, but I don't take the attitude that people who disagree with me are idiots who don't know what they are talking about, an attitude I've encountered amazingly often from more than one string theorist.

Comments

1. **Bryan**
   November 19, 2005

   I think it is very sad that people like JoAnne Hewett are queuing up to put kick string theorists' because of their arrogance.

   There have already been abuses of string theorists leak into the media, like http://www.theonion.com/content/node/41454

   It's a good thing the media think this is just a storm in a teacup.

2. **Plato**
   November 19, 2005

   > I'm no paragon of humility, but I don't take the attitude that people who disagree with me are idiots

   Thanks Peter 🙂

3. **JoAnne**
   November 19, 2005

   Bryan,

   Hmmm….I'm not quite sure what a `put kick' is, but am going to assume it is similar to a punt kick in American football. So, no, if you read my post, you will see that I am most certainly not lining up to punt kick string theory. I most emphatically said that I believe the enterprise is worth pursuing. It's all a matter of balance.

4. **Bryan**
   November 19, 2005

   Hi JoAnne,

   Do you just think Lubos Motl should be kicked, then? 😉

5. **D R Lunsford**
   November 19, 2005

   How's this for irony – the server that hosts the HETG at Harvard, loaded with stringers, is named "democritus" 🙂

   -drl

6. **Aaron**
   November 19, 2005

   Princeton has made many offers to non-string theorists.

7. **Bryan**
   November 19, 2005

   D. R. Lunsford, mind your step! Motl will probably investigate you and find you're major paper http://cdsweb.cern.ch/search.py?recid=688763&ln=en stems to NASA astronaut training, http://www.hq.nasa.gov/office/pao/History/alsj/DRLunsford.html so you are just a nerd with a chip on your shoulder about string theory!

8. **Eli Rabett**
November 19, 2005

Any field such as sting theory starts from a few places. How much of the arrogance is a reflection of those who founded the field, which was picked up by their students?

BTW, I think there was a confusion betweeen the American term punt (something) as in be ready to abandon or leave, and the English term "put the boot in", ie kick someone when they are lying on the ground, as in rugby. Both may be appropriate

9. **Nigel**
November 19, 2005

Bryan, in replying to Lunsford, should write 'your major paper' not 'you're'. It's a shame that there's no automatic grammar check/correction on blogs.

Eli, surely you mean 'string' not 'sting' theory, although in England a 'sting' is slang for a rip-off or overpriced commodity, so it is certainly right to call string theory a sting theory! 🙂

10. **Rafael**
November 19, 2005

I saw a talk given by Robert Laughlin which was very interesting. Mostly it was about emergent properties but he did talk a bit about his criticisms of string theory. Especially, not being able to make any predictions which can be measured. I think he was sad (or angry) that theoretical physics has gone off the rails. I guess that would be a condensed matter theorist's perspective?

11. **Eli Rabett**
November 19, 2005

The best laid puns of mice and men, oft go awry. Thank you Nigel.

12. **D R Lunsford**
November 19, 2005

Wow, they actually *credited* me on the ALSJ? For revising a transcript? Hey! This is easy!

-drl

13. **dan**
November 19, 2005

So what kind of non-stringy particle physics research directions are being pursued these days? By non-stringy I mean fundamental particle physics that (1) do not involve string theory, (2) do not invovle SUSY, (3) do not involve higher-dimensions?

Incidentally, how do particle physics explain the transformation of kind of fundamental particle, the electron/position, into another, such as photons? Do they recourse to string theory or are there non-stringy ways of explaining this?

curious
Dan

14. **Thomas Larsson**
November 20, 2005

Incidentally, how do particle physics explain the transformation of kind of fundamental particle, the electron/position, into another, such as photons? Do they recourse to string theory or are there non-stringy ways of explaining this?

Hopefully particle physicists don't explain the transformation of a single electron into a photon at all, since it does not happen. The transformation of one electron + one positron into two photons is explained by a theory called QED. Believe it or not, but it does not involve string theory, and it makes some predictions in reasonable agreement with experiments (to ten decimal places or so).

15. **Aaron**
November 20, 2005

So what kind of non-stringy particle physics research directions are being pursued these days? By non-stringy I mean fundamental particle physics that (1) do not involve string theory, (2) do not invovle SUSY, (3) do not involve higher-dimensions?

That's a very odd definition of 'stringy'. I'm sure the vast legions of phenomenologists who work on those subjects would be surprised that they're doing 'stringy' stuff. Some might even object rather vociferously.

Anyways, there's stuff involving little higgs models, dark matter, baryogenesis, neutrino masses, some GUT stuff (although most of that is supersymmetric), lattice stuff and probably plenty more that's not coming to mind.

BTW — in case it wasn't clear from context above, I meant that Princeton has made job offers in the past to high energy theorists who were not string theorists.

16. **D R Lunsford**
November 20, 2005

TL,

I think his point was, does anyone work on something you can't find in a Michio Kaku book..

And as for his comment about matter/antimatter, I think he meant – is there a mechanism for this beyond just positing the interaction? And the answer is – not one that is generally accepted.

-drl

17. **andy s.**
November 20, 2005

Dan: Loop Quantum Gravity addresses many of the same issues are String Theory. Google Lee Smolin or the Perimeter Institute.

18. **Aaron**
November 20, 2005

And as for his comment about matter/antimatter, I think he meant – is there a mechanism for this beyond just positing the interaction? And the answer is – not one that is generally accepted.

It's called gauge invariance.

19. **D R Lunsford**
November 20, 2005

Yep, that's the old posit right there Aaron.

-drl

20. **Aaron**
November 20, 2005

Positing gauge invariance is far from positing an interaction. I invite you to write down a quantum theory of a massless vector (we do want electromagnetism, right?) without gauge invariance.

21. **D R Lunsford**
November 20, 2005

This isn't the place to discuss it. As it happens a post on SPR will soon address it, so go there (gauge invariance determines the interaction, and for 4d they are practically synonymous).

-drl

22. **Who**
November 20, 2005

Dan asked
"dan Says:
November 19th, 2005 at 9:57 pm
So what kind of non-stringy particle physics research directions are being pursued these days?"

to expand on an earlier comment
"andy.s says
November 20th, 2005 at 1:50 am
Dan: Loop Quantum Gravity addresses many of the same issues are String Theory. Google Lee Smolin or the Perimeter Institute."

you might like to watch this 40 minute video of a recent Smolin talk, and download the set of slides that goes with it.
http://loops05.aei.mpg.de/index_files/abstract_smolin.html

in this talk Smolin tries out a certain a particle physics scheme in the context of Loop QG. the quantum state of the gravitational field is given by a kind of graph called a "spin network" and families of particles (electrons, quarks, etc.) are are built into the gravitational field as kinds of links in the graph. the "preon" model which he uses is due to Bilson-Thompson and it reproduces some features of the standard model.

there must be several cases of non-string QG research which strives to unify particle physics with quantum gravity, by a variety of authors, but I can't think of a survey paper specifically about this. andy.s suggestion to use search engines is a good one. This talk, that smolin gave last month (10 October) is at least something like what you were asking about, to start you off.

the effort is bound to look preliminary and insubstantial compared with string

23. **Dr. Ranger McCoy**
November 20, 2005

http://physicsmathforums.com/showthread.php?t=56

Tied Up & Strung Out: Hollywood String Theory Movie!!! Looking For Extras!!!
FOR IMMEDIATE RELEASE:

ALL TIED UP & STRUNG ALONG, a movie about String Theorists and their expansive theories which extend human ignorance, pomposity, and frailty into higher dimensions, is set to start filming this fall. Jessica Alba, John Cleese, Eugene Levie, Jackie Chan, and David Duchovney of X-files fame have all signed on to the $700 million Hollywood project, which is still cheaper than String Theory itself, and will likely displace less physicists from the academy.

"As contemporary physics is about money, hype, mythology, and chicks," Ed Witten explained from his offices at the Princeton Institute for Advanced Study, "The next logical step was Hollywood, although I thought Burt Reynolds should play me instead of Eugene Levy."

Brian Greene, the famous String Theorist who will be played by David "the truth is out there" Duchovney, explained the plot: "String theory's muddled, contorted theories that lack postulates, laws, and experimentally-verified equations have Einstein spinning so fast in his grave that it creates a black hole. In order to save the world, we String Theorists have to stop reformulating String Theory faster than the speed of light. We are called upon to stop violating the conservation of energy by mining higher dimensions to publish more BS than can accounted for with the Big Bang alone, and I win the Nobel prize for showing that M-Theory is in fact the dark matter it has been searching for."

Greene continues: "At first my character is reluctant to stop theorizing and start postulating, but when my love interest Jessica Alba is sucked into the black hole, I search my soul and find Paul Davies there, played by John Cleese. I ask him what he's doing in my soul, and he explains that the answer is contained in the mind of God, which only he is privy too, but for a small fee, some tax and tuition dollars, a couple grants here and there, and an all-expense-paid book tour with stops in Zurich and Honolulu, he can let me in on it. And he shows me God in all her greater glory, as he points out that we can make more money in Hollywood than writing coffee-table books that recycle Einstein, Bohr, Dirac, Feynman, and Wheeler. I am quickly converted, and I agree to turn my back on String Theory's hoax and save Jessica Alba."

But it's not that easy, as standing in Greene's way is Michio "king of pop-theory-hipster-irony-the-theory-of-everything-or-anything-made-you-read-this" Kaku, played by Jackie Chan. Kaku beats the crap out of Greene for alomst blowing the "ironic" pretense his salary, benefits, and all-expense paid trips depend on. "WE MUST HOLD BACK THE YOUNG SCIENTISTS WITH OUR NON-THEORIES!! WE MUST FILL THE ACADEMY WITH THE POMO DARK MATTER THAT IS STRING THEORY TO KEEP OUR UNIVERSE FROM FLYING APART, OUR PYRAMID SCHEMES FROM TOPPLING, AND OUR PERPETUAL-MOTION NSF MONEY MACHINE FROM STOPPING!!" Kaku argues as he delivers a flying back-kick, "There can be ony ONE! I WILL be String Theory's GODFATHER as referenced on my web page!! I have better hair!"

But Greene fights back as he signs his seventeenth book deal to make the hand-waving incoherence of String Theory accessible to the South Park generation, senior citizens, and starving chirldren around the world. "Kaku! Kaku! (pronounced Ka-Kaw! Ka-Kaw! like Owen Wilson did in Bottle Rocket)," Greene shouts. "It is theoretically impossible to build a coffee tables strong enough to support any more coffee-table physics books!!!"

"Time travel is also theoretically impossible, but there's a helluva lot more money for us in flushing physics down a wormhole. Nobody knows what the #&#%&$ M stands for in M theory ya hand-waving, TV-hogging crank!!! Get it?? Ha Ha Ha! We're laughing at the public! We're the insider pomo hipsters! Get with the gangsta-wanksta-pranksta CRANKSTER bling-bling program!!"

How does it all end? Does physics go bankrupt funding theories that have expanded our ignorance from four dimensions into ten, twenty, and thirty dimensions? Do tax payers revolt? Do young physicists overthrow the hand-waving, contortionist bullies and revive physics with a classical renaissance favoring logic, reason, and Truth over meaningless mathematical abstractions? Does Moving Dimensions Theory (MDT) prevail with its simple postulate? We'll all just have to wait!

But in the meantime, how do you think it will play out?

Will theories with postulates ever be allowed in physics again? Or will the well-funded, tenured pomo String Theory / M-Theory (Maffia-Theory) Priests send their armies of desperate, snarky postdocs and starving graduate students forth to displace and destroy all common sense, logic, reason, and physics in the academy? It must be so–for the greater good of physics, the individual physicist, and thus physics, must be sacrificed.

http://physicsmathforums.com/showthread.php?t=56

24. **WL**
November 20, 2005

As usual, things are turned upside down. The supreme form of arrogance is not the one of experts, but of fools who claim to know better than the experts.

25. **secret milkshake**
November 20, 2005

I know lumo a bit. It's not the Strings – he is like this on any subject. He cannot help it.

26. **Quantum_Ranger**
November 20, 2005

Peter, the interesting thing is that the 'new' webpages here:
http://www.stanford.edu/group/sitp/
specifically the bottom copyrighted macromedia image at the bottom,
have been extracted from the website of here:
http://www.perimeterinstitute.ca/activities/scientific/seminarseries/index.php

look closer at the bottom starcluster image, there are Aztec and Toltec "gods" embedded into the image?

!!..the Perimiter webpages had the inca/toltec statues embedded into the starcluster picture sometime ago, this was tackled here:
http://www.physicsforums.com/showthread.php?t=48248

reply#3

The institutes appear to be having a "bat-ball" exchange, the Cosmic-String theorists at Stanford are obviously having fun with the symbolic inclusion of theory of the "Gods"?

I twould be interesting to find out when the 'new' stanford webpages became active, as the smae Aztec/Toltec images from perimiter were present Oct04?..they are no longer there today, but the reason for their removal must have a significance at Stanford webpages?

27. **Juan R.**
November 20, 2005

Arrogance could be supported if string theorists really were smart people, but unfortunately only one or two of them are really good. The rest is a group of…

This is the reason that people working in other areas -biophysics, quantum measurements theory, arrow of time, quantum gravity, complexity and chaos, etc.- often do jokes (even denigrating) with string theorists.

One of lasts, i know was a joke from F. Dyson (the Nobel winner) who argued against Brian Greene (that 'SMART' string theorist 🙂 regarding several conceptual issues on the quantum measurement problem the last year.

Dyson had studied the problem with some deep, whereas Brian Greene only speculated (his true speciality) that string theory would revolutionate our current views…

Dyson words were crystal clear:

> He rejects without any serious discussion

This is the best (mean) definition of string theorists activity i know

…Without any serious discussion…

Juan R.

Center for CANONICAL |SCIENCE)

28. **secret milkshake**
November 20, 2005

To Juan: Freeman Dyson was left out of Nobel for QED, actually. What he got was Templeton.

29. **dan**
November 20, 2005

Now that someone meontioned “preons”, are strings (D0 branes) are said to be “fundamental” and make up other fundamental particles, including gravitons, photons, etc cetera.

according to string theory, do strings make up quarks, or do strings make up preons, which then make quarks?

incidentally, if the project of spin networks giving rise to preons seems to work, then LQG would be relevant to particle physics, right?

30. **D R Lunsford**
November 20, 2005

Juan,

This is the impression I get – that stringers are not very bright.

-drl

31. **Adrian Heathcote**
November 20, 2005

Dr Ranger McCoy

Someone’s got to say it. Bravo! Author!

I might mention that a famous winner of the Templeton (whom you mention) once said that the way to understand mathematics was to stare at the page for a long time until it looked about right.

(Forget about doing those pesky exercises!)

I’m amazed that physics has fallen into this hole. It’s like the malaise in the humanities has spread like an ink-stain outwards.

A.H.

32. **Who**
November 20, 2005

dan, I think you draw the right conclusion but put it a bit strongly
“… if the project of spin networks giving rise to preons seems to work, then LQG would be relevant to particle physics, right?”

In his talk that I gave the link for, Smolin was drawing on this Bilson-Thompson preon paper
http://arxiv.org/abs/hep-ph/0503213
A topological model of composite preons
you might enjoy this paper and get something from it

you must judge for yourself how far along is Smolin’s project of relating spin network to these twisted-ribbon preons of B-T.
it strikes me as very tentative but in Smolin’s Loops ’05 talk
http://loops05.aei.mpg.de/index_files/abstract_smolin.html
he is saying that networks are a hospitable place to find these braided triplets of preons as links, and putting them in the picture could address several problems efficiently.
Best if you listen to smolin’s talk and look over the slides yourself

I see that both Bilson-Thompson and Renate Loll (CDT) are now at Perimeter. Also Xiao-Gang Wen. B-T just gave a talk on preons a few days ago and Loll and Wen were giving a workshop this weekend.

the talks and/or slides of some of these are available on line at
http://streamer.perimeterinstitute.ca:81/mediasite/viewer/

select “Seminar Series” and go to page 14 to find the Bilson-Thompson preon talk, if you are interested.

to find the Loll and Wen workshop scroll the menu down to
“Emergence of Spacetime Workshop” and click that

anyway it sounds too definite to say non-string QG relevant to particle physics but there certainly are connections being explored and it is not too hard to keep track of the various initiatives people are pursuing

glad you are interested

33. **dan**
November 21, 2005

hi who,
well i would love to see QG within my lifetime, as one of the final frontiers of science. his talk spoke of fermions, what about interger spin bosons, such as higgs, photons, and gravitons?

as for string theory, does string theory predict preons? does string theorists predict an inner structor of quarks much in the same way as preons do?

i am curious in lqg how is GR dynamics to be recorded, by gravitons, or by the large-scale behavior of spin foams?

34. **We Pretty**
November 21, 2005

Hi,

scientists are in general more interested in facts than attitutes of other people. But as a representative of the public I think that the way string theorists trumpet their theory as a solution to everything is cheating. I suppose that if you ask a random person what is quantum theory, if he or she knows anything about it, he or she will usually say that it is something that explains the atoms and inderminancy and stuff. If you currently ask a random person what is string theory, if he or she knows anything about it, he or she will usually say that it is some new theory that explains everything.

To help to change string theorists and especially young physicists but also the public attitude towards string theory, you should add section of string theory to the physics FAQ (though strictly speaking it does not belong there). In the beginning of the FAQ about physics in general Q: "What is a prediction" ? Q: "What is a nontrivial prediction" (A: "A prediction that differs from the currently accepted theories such that if the predicted thing does not happen then the theory that makes the prediction is false"). In string theory sections Q: "Does ST make non-trivial predictions ?" A: "No". Q: "Does string theory make predictions ?" A: "No" (I suppose). Or make this information otherwise available.

There are also other issues that would be of interest like how long string theory has predicted nothing, are there any other theories and stuff. I know also that there are positive side in string theory, cool mathematics and such.

Whatever landscapes and multiverses and extra dimensions sound cool. It sounds also very cool that the universe is 2 or 4 everything between dimensional.

We Pretty

35. **Tony Smith**
November 21, 2005

The arrogant narrowmindedness of superstring theorists may not be unprecedented.

According to an Opinion article by Gregory Benford (not only a scifi writer, but also a professor of physics at UC Irvine) in Physics Today (November 2005 at pages 48-49):
"… In the late 1940s, George Gamow, Ralph Alpher, and Robert Herman worked out element formation and the entire scenario that led to the now-famous 3-K background radiation. Yet the steady-state model held sway, and their work had faded from view by the mid-1950s. …
Rather than testing the Gamow-Alpher-Herman model, cosmologists spent more than a decade falsifying steady state's predictions. No one followed on the nucleosynthesis path, let alone thought of observations. …
To outsiders, such events might seem a scandal. … We didn't treat both models fairly, and lost more than a decade before discovering the truth. … what were the scandal's roots? …
Alpher and Herman … said they suspected a cultural bias was at work: "It is possible, but regrettable, that Gamow's fun-loving and irrepressible approach to physics led some scientists not to take seriously his work. …" … Gamov, Alpher, and Herman committed a minor social sin: They weren't in the club. …
Are there similar scandals in our own era? …
Despite 30 years of their drum-beating, the string theorists never suspected that the dark energy could comprise the majority of the energy density of out universe. …
When a bug of this size hits your conceptual windshield, it makes a big splash. The dark energy scandal is that the bug was the size of an eagle.
String theory is an idea that functions as … no predictions, yet widespread acceptance.
Theorists can fall in love with mathematical beauty. Philosophical elegance, which steady state had, is even more glamorous. Gamow, Alpher, and Herman had to fight steady state's shiny splendor, which blinded our field.
What causes such scandals?
… An unspoken timidity lurks in the close-focus grant-approval process – small steps are rewarded as more reliable than conceptual leaps. Possibilities beyond our current conceptual horizons get little attention. In academia, we maximize publicaton numbers rather than originality. This approach also gets us through the incremental mindset of review committees, which are seldom noted for their leaps of insight. ….
Do we suffer from anxiety over imagination?
Rigor is reassuring, but it should come at the end of that powerful chain that starts with intuition and proceeds to experimental checks – not at the beginning. To set our work in motion, we reason mostly by analogy, not by rigorous deduction. Imagination is not incremental.
Yet among reviewers, "speculation" is a work mostly deployed as a pejorative. We should not allow it to be. …".

I think that the lesson here is that there should be some sort of reasonable evaluaton mechanism for nonstandard physics models (not just mine). However, since I am more familiar with my work than with the work of others, I will state that I wrote a paper calculating the Dark Energy : Dark Matter : Ordinary Matter ratios and got results pretty close to the WMAP observations. When I submitted the paper to the Cornell arXiv in 2004, it was rejected (probably due to me being personally blacklisted by them). I then posted a similar paper to the CERN EXT preprint series as CERN CDS preprint EXT-2004-013, where it was accepted and is still available. However, later in 2004 (October 2004) the CERN Scientific Information Policy Board (SIPB) closed the CERN CDS EXT preprint series. A speculative opinion of mine is that EXT was closed due to pressure from the Cornell arXiv to prevent work blacklisted by them from seeing the light of day.

Just as the steady state model, in Benford's words, "… could not explain helium formation …", (afaik) superstring theorists have yet to explain the Dark Energy : Dark Matter : Ordinary Matter ratios observed by WMAP.

Tony Smith

http://www.valdostamuseum.org/hamsmith/

36. **woit**
November 21, 2005

I just accidentally deleted this entry, and had to restore the whole blog database from a version of early this morning. This happened partly due to my own carelessness, partly due to the bad user interface of WordPress, and partly due to the fact that I've had to spend a lot of time deleting repeated attempts by people to post long discussions of their own completely off-topic ideas about physics.

Please stop doing this, it is getting really, really, annoying, wasting my time, and now causing damage. This is not a forum for you to go on about your favorite ideas about physics that have nothing to do with the topic of the posting. Do that elsewhere. From now on, I'll be taking steps to shut off access to this weblog from anyone who repeatedly tries to post such material.

37. **Matti Pitkanen**
November 21, 2005

Dear Peter,

you definitely cannot argue that my comment relating to Tony's posting was a long discussion about off topic idea. It was precisely about the immense arrogance of super string theorists and about its tragic consequences: I am not talking about myself here but about the development of the theoretical physics as a whole.

It seems that superstring theorists are not the only ones to be blamed of extraordinary arrogance and willingness to censorship. This is a disease plaguing the entire comunity. This blog could be much more than it is by allowing open discussion of new ideas (in my own case not only new ideas but extensive life work) rather only allowing boring repetition of the well-known failures of string theory.

With Best Regards,
Matti Pitkanen

38. **woit**
November 21, 2005

Matti,

Your comment was not deleted by me intentionally, it was accidentally deleted when I accidentally deleted the whole posting. The latest backup of the blog database was made automatically early in the morning after Tony's comment and before yours. I had to restore from that backup and your comment was lost.

I was not about to delete your posting, but the problem is when you, Tony and others start discussing your own ideas, many others then want to join in with their own pet theories, and the comment section rapidly degenerates into a long string of comments by people promoting their own ideas (many of which make no sense). This leaves me with the job of trying to moderate such a forum. I don't have the time or energy for it and I resent having to do it, especially when it causes me to accidentally damage things here (e.g. by irretrievably deleting your comment). If you have a copy of your comment, you're welcome to resubmit it.

39. **D R Lunsford**
November 21, 2005

I'm personally a little sick of your whining about the status of your damn blog. Your own work in this field is pretty thin, but you've carved out a niche for yourself by calling a spade a spade. If you can't handle the heat, get out of the damn kitchen. And if you need some computer help, drop me a line, I'm pretty damn good at it after 20 odd years without a career in physics, that I worked very hard to secure from an early age. I don't "lose things".

-drl

40. **Who**
November 21, 2005

The topic is Joanne's forthright expression of a perception that is shared by others—–about string hubris.

I think the arrogance or pretention she speaks of is based on a (questionable) fundamental premise which Joanne herself repeats.
In other words, Joanne's own assumption is part of the problem.

In her original blog, Joanne pays tribute to a dubious cliché (which is at the root of string complacency). I don't object to her conclusion that string is worth studying. i object to her premise, which people seem to repeate unquestioningly:

"…string theory worth studying? I would answer yes, for two reasons. One has already been given, namely, that **string theory is currently our best idea about how to quantize gravity.**"

Why can't people say yes string theory is worth developing **even though it is not** necessarily the best idea for quantizing gravity that has been proposed?
And why not say yes we should support string research, among other approaches **even though it is not** necessarily the most promising avenue being explored?

I think the claim that it is the "best" or most promising is not to be taken for granted but is, in fact, controversial. There is legitimate difference of expert opinion. The only string theorist I recall acknowledging this is Andy Strominger. I hear people like Steve Shenker and David Gross use phrases like "our one best hope" like an article of faith or a party line—but the Strominger attitude i heard is that it aint so clear: String is worth studying but it's not clear that it is the best and there may be other avenues worth exploring as well.

41. **Who**
November 22, 2005

I think complaining about Peter losing some comments is off topic.
It is fine for him to occasionally lose stuff, most of what we write is not for the ages—-and it keeps the threads shorter and easier to read.

for better or worse, in this thread the topic is Joanne Hewett having the guts to say "arrogant"—-and she is a nice person too. so we better talk about that while we have the chance and not get off quarreling about minor blog mishaps

42. **plato**
November 22, 2005

I think complaining about Peter losing some comments is off topic.

I second that. I keep getting my posts deleted repeatly you think I am going to whine about?:) Non!

I just wonder though about the fervor with which a group view on anti- can become, could be affected by a lone person who might disagree, with what you people go on about string/M theory.

Peter you took a stand.

While it is not totally comprehensible to me, I do try to understand the models that are being adopted? And see them, as models.

Do you think Lee's comments arise just in cosmic varaince?

http://cosmicvariance.com/2005/11/14/our-first-guest-blogger-lawrence-krauss/#comment-7514

43. **Aaron**
November 22, 2005

Why can't people say yes string theory is worth developing even though it is not necessarily the best idea for quantizing gravity that has been proposed?

Maybe people have actually looked at what's out there and actually think that string theory is the best game in town?

Just a thought.

44. **Matti Pitkanen**
November 22, 2005

Peter,

I am glad to hear that this was an accident. And I want to emphasize that we were not going to discuss about details of our theories. Unfortunately I do not have the copy of posting. The ingenious definition of Who for what is off topic allows unlimited censorship without complaints. Congratulations!

Matti

45. **Nigel**
November 22, 2005

'…others then want to join in with their own pet theories, and the comment section rapidly degenerates into a long string of comments by people promoting their own ideas (many of which make no sense). …'

Sorry for causing problems by being arrogant. My arrogance is just copied from the string theorists who lead physics, which JoAnne Hewitt writes about. If they're arrogant, others follow.

BTW, some people want to have a go at stringing other people's stuff together, to sort out what makes sense.

46. **Juan R.**
November 22, 2005

Well, i will 'repeat' my post (since i think that was on-topic and, moreover, i do not speak on my own theories here)

secret milkshake

YES!! What great mistake!! Thanks by correction. Since Dyson unified the 'different' QED of the époque (and even people as Feynman was initially unable to see that Tomonaga or Schwinger, were just other 'pictures'), do you know why do not received the Nobel? Perhaps the lack of PhD?

dan

Since there is not still formulation for M-theory, nobody can reply you now. However, some brane theorists as Banks claim that D0-branes (which are NOT strings; a string is a 1-brane) are "fundamental" blocks of nature. It has been already proven, for example, that supersimmetric gravitons can be derived from 'excitations' of D0-branes.

For me, the most 'beatiful' result from M(atrix) theory is that fundamental building blocks are again pointlike particles. Probably the four decades 'stringy' excursion around extended object (and the popularization of how 'stupid' poinlike particle were) will finalize again in that the pointlike behaviour of particle physics was sufficient, when divergences and some other mistakes are corrected.

D R Lunsford

I personally think that one can find clever people in any field (scientific or not). Due to my multidisciplinary interest, I found genial papers on math, particle physics, quantum gravity, ecology, irreversible thermodynamics, electromagnetism, chemical dynamics, philosophy, etc.

However, I agree with you that current 'hidden' law saying that *the most brilliant guys of the planet are string theorists*, is a pure assumption without sound basis. Simply by reading published literature on string theory and comparing it, one already detects that. In my opinion, there is not a single Feynman on the current string theory community and this is the reason that physics has not advanced.

Moreover i think that the comparative of Witten with Einstein (or even with Newton!!) is, I believe, one of multiple distorted outcomes of the heavy stringy marketing.

We Pretty

> But as a representative of the public I think that the way string theorists trumpet their theory as a solution to everything is cheating. I suppose that if you ask a random person what is quantum theory, if he or she knows anything about it, he or she will usually say that it is something that explains the atoms and inderminancy and stuff. If you currently ask a random person what is string theory, if he or she knows anything about it, he or she will usually say that it is some new theory that explains everything.

A scientist is one who solves problems via generating scientific knowledge (i.e. acquiring knowledge via scientific method). String theory would be fantastic and string theorists become heroes if string theory was a scientific solution to only a 10% of the claimed in media. Unfortunately, it is all pure marketing. String theory is not a theory of physics, it is not a theory of science, it is an excellent piece of marketing, just that. I completely agree with Woit here. The only success of string theory has been on mass media.

Many people think that string theory does not predictions about nature. I partially agree. However, string theory claims that universe IS 10-D AND supersymmetric. Our observed universe is 4D and non-supersimmetric, therefore there is 'some' that string theorists are ignoring… just the scientific method!

Juan R.

Center for CANONICAL |SCIENCE)

47. **Juan R.**
November 22, 2005

Who

Could you explain why you think

> that string theory is currently our best idea about how to quantize gravity

?

During my travel on 'quantum gravity lands' i have known many people who believe 'that' because other people also believe 'that'.

I have even heard that the 'best' argument for studying string theory is because Witten believes on it!!!

If you really sustain you are writting here, could you write your real name too?

Is not part of the typical arrogance of string theorists to begin their talks on string 'theory' (so say) claiming that string theory is the ONLY possibility for quantizing gravity?

Where is the paper, book, talk, etc. where this was supposedly proven? Or is just another piece of the propaganda?

Juan R.

Center for CANONICAL |SCIENCE)

48. **Who**
November 22, 2005

Juan R you totally misunderstand me. Please carefully read JoAnne Hewett orig. blog
http://cosmicvariance.com/2005/11/18/a-particle-physicists-perspective/

Notice where she says

> string theory worth studying? I would answer yes, for two reasons. One has already been given, namely, that string theory is currently our best idea about how to quantize gravity. We obviously need to explore that link further, no matter how tenuous.

I am pointing to where she says "best idea" as if it were taken for granted and not controversial. Well-informed people can differ, but she does not not indicate this. It is something that even nice intelligent sensible people like JoAnne PARROT. This item of unquestioned dogma is a large part of the problem.

Suppose JoAnne had said this:

> string theory worth studying? I would answer yes, for two reasons. String theory is one of several promising ideas about how to quantize gravity. We obviously need to explore that link further, no matter how tenuous.

then the conclusion (worth further support) would remain the same but the grounds for what she objects to—the privileged sense of entitlement—would be removed.

If people could make that simple substitution, I think it would be a real step towards solving the problem she is talking about.

BTW think we should acknowledge who JoAnne is and what she puts on the line. correct me if I am mistaken but i think she is a type of nice levelheaded young scientist that people in the research establishment pick to be on advisory boards and to organize panels and committees and workshops. She is someone you trust to be constructive and have a sense of humor and be normal according to the highest standards. I've worked with people in an NASE/NRC study and I respect them and their careers are partly political and based on people-skills that some of the rest of us lack. And I have to say that I think JoAnne has something to RISK

49. **Aaron**
November 22, 2005

It is something that even nice intelligent sensible people like JoAnne PARROT.

Not to speak for JoAnne or anything, but I hope you realize that you're being rather insulting here. How do you know that anybody is 'parrot'ing anything?

And this idea you have that there is this string mafia who seek out to destroy the careers of those who oppose string theory is just silly. Lots and lots and lots of people criticize string theory.

50. **Bee**
November 22, 2005

Folks, today there is probably no particle physicist more tired by stringers than I am. If I have to listen to one more heterotic twisted whatever type string talk with compactification on manifolds that I don't know how to pronounce w/ or w/o flux or any connection to the real world, I am sure my head is going to implode.

However, I don't think string theory is such a stupid thing to do. It's just not plausible to me what it should have to do with nature and I find it is time that stringers admit they are mathematicians.

String theory has some beauty on its own, there are for sure many points worth investigating and the properties of the theory are amazing. Just why that is physics, I can't see. Maybe it would help, if any of the guys giving these headache-causing talks would at least TRY to explain why the physics community should be interested in that topic.

Also, string theory in the US is definitly completely over-funded, whereas there is lack of funding in equally or more important fields – that's for sure.

How come the NSF hasn't yet noted that?

51. **woit**
November 22, 2005

Hi Bee,

The problem with your suggestion that "stringers admit that they are mathematicians" is that, on the whole, the math community doesn't agree and doesn't want them.

52. **Aaron**
November 22, 2005

We're inspirational to the mathematicians. They like us. We give them things to prove.

53. **secret milkshake**
November 23, 2005

Bee: their attitudes are awful, not the field. The fluff – not the stuff.

54. **JC**
November 23, 2005

Peter,

Is there any particular reason why the math community doesn't really want string theorists?

55. **woit**
November 23, 2005

JC,
Would you want Leonard Susskind ranting about the anthropic landscape in your department?

Mathematicians are interested in some parts of string theory, e.g. conformal field theory, mirror symmetry, topological strings, etc. These are pretty well-defined ideas that have led to very non-trivial conjectures about interesting mathematical objects. Some people working on these things are happily working in math departments. Other parts of string theory, where no one knows precisely what the object under study even is (e.g. M-theory), or where there's no new insight into interesting mathematics (e.g. most of the work on the landscape or string cosmology) aren't things mathematicians want much to do with.

56. **Juan R.**
November 24, 2005

Ok **Who**! I agree

Juan R.

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All Sorts of Links

November 22, 2005
Categories: Uncategorized

Here's a collection of things I've run across recently that may be of interest:

The Tevatron is doing quite well, with sizable increases in luminosity in recent months. There are some articles telling about this in Fermilab Today, and you can get up to date information about how things are going here. At the moment they're doing better than their "design" projection, which is meant to be quite optimistic.

On December 1 there will be a live 12 hour webcast called Beyond Einstein, which will feature many different groups and individuals talking about physics.

December 1 will also be the opening of the 23rd Solvay conference in Brussels. These conferences have a very illustrious history. This year the topic is The Quantum Structure of Space and Time, and most of the invited participants will be string theorists. Of the 60 participants there seems to be exactly one physicist from the LQG camp, Abhay Ashtekar. There will also be an event for the public, with talks by string theorists Brian Greene and Robbert Dijkgraaf, and a debate featuring five string theorists and Gerard 't Hooft.

Witten has been giving talks about his new work on gauge theory and geometric Langlands. Notes from a talk at Penn last month are on-line, and video from a talk at Rutgers last week should soon appear.

A conference was held earlier this month at Queen Mary College in London entitled From Twistors to Amplitudes, with many interesting talks on using twistor techniques to study gauge theory amplitudes.

There's a new site called Mixed States which does a good job of aggregating blog entries about physics.

There are all sorts of links relevant to research in number theory at the Number Theory Web.

Robert Wald has an article on teaching general relativity. Until I taught our graduate differential geometry course I hadn't realized just how tricky the definition of a tangent vector can be. Most of the difficulty with teaching GR has to do with the large amount of sophisticated geometry needed.

This is one of the funnier things I've read in a while. It seems that, like all non-string theorists, internet con artists are really stupid.

Update: Two recent talks by Alain Connes at the KITP in Santa Barbara are now online. One is entitled Non-Commutative Geometry and Space-Time, the other, discussing his ideas about the Riemann hypothesis, is called Noncommutative Motives, Thermodynamics, and the Spectral Realization of Zeros of Zeta.

Comments

1. **Tax Payers Revolt**
   November 22, 2005

   http://physicsmathforums.com/showthread.php?t=56

   I think the taxpayers aare getting tired of funding String Theory.

   Perhaps Hollywood is better suited?

   Tied Up & Strung Out: Hollywood String Theory Movie!!! Looking For Extras!!!
   FOR IMMEDIATE RELEASE:

   ALL TIED UP & STRUNG ALONG, a movie about String Theorists and their expansive theories which extend human ignorance, pomposity, and frailty into higher dimensions, is set to start filming this fall. Jessica Alba, John Cleese, Eugene Levie, Jackie Chan, and David Duchovney of X-files fame have all signed on to the $700 million Hollywood project, which is still cheaper than String Theory itself, and will likely displace less physicists from the academy.

   "As contemporary physics is about money, hype, mythology, and chicks," Ed Witten explained from his offices at the Princeton Institute for Advanced Study, "The next logical step was Hollywood, although I thought Burt Reynolds should play me instead of Eugene Levy."

   Brian Greene, the famous String Theorist who will be played by David "the truth is out there" Duchovney, explained the plot: "String theory's muddled, contorted theories that lack postulates, laws, and experimentally-verified equations have Einstein spinning so fast in his grave that it creates a black hole. In order to save the world, we String Theorists have to stop reformulating String Theory faster than the speed of light. We are called upon to stop violating the conservation of energy by mining higher dimensions to publish more BS than can accounted for with the Big Bang alone, and I win the Nobel prize for showing that M-Theory is in fact the dark matter it has been searching for."

   Greene continues: "At first my character is reluctant to stop theorizing and start postulating, but when my love interest Jessica Alba is sucked into the black hole, I search my soul and find Paul Davies there, played by John Cleese. I ask him what he's doing in my soul, and he explains that the answer is contained in the mind of God, which only he is privy too, but for a small fee, some tax and tuition dollars, a couple grants here and there, and an all-expense-paid book tour with stops in Zurich and Honolulu, he can let me in on it. And he shows me God in all her greater glory, as he points out that we can make more money in Hollywood than writing coffee-table books that recycle Einstein, Bohr, Dirac, Feynman, and Wheeler. I am quickly converted, and I agree to turn my back on String Theory's hoax and save Jessica Alba."

   But it's not that easy, as standing in Greene's way is Michio "king of pop-theory-hipster-irony-the-theory-of-everything-or-anything-made-you-read-this" Kaku, played by Jackie Chan. Kaku beats the crap out of Greene for alomst blowing the "ironic" pretense his salary, benefits, and all-expense paid trips depend on. "WE MUST HOLD BACK THE YOUNG SCIENTISTS WITH OUR NON-THEORIES!! WE MUST FILL THE ACADEMY WITH THE POMO DARK MATTER THAT IS STRING THEORY TO KEEP OUR UNIVERSE FROM FLYING APART, OUR PYRAMID SCHEMES FROM TOPPLING, AND OUR PERPETUAL-MOTION NSF MONEY MACHINE FROM STOPPING!!" Kaku argues as he delivers a flying back-kick, "There can be ony ONE! I WILL be String Theory's GODFATHER as referenced on my web page!! I have better hair!"

   But Greene fights back as he signs his seventeenth book deal to make the hand-waving incoherence of String Theory accessible to the South Park generation, senior citizens, and starving chirldren around the world. "Kaku! Kaku! (pronounced Ka-Kaw! Ka-Kaw! like Owen Wilson did in Bottle Rocket)," Greene shouts. "It is theoretically impossible to build a coffee tables strong enough to support any more coffee-table physics books!!!"

   "Time travel is also theoretically impossible, but there's a helluva lot more money for us in flushing physics down a wormhole. Nobody knows what the #&#%&$ M stands for in M theory ya hand-waving, TV-hogging crank!!! Get it?? Ha Ha Ha! We're laughing at the public! We're the insider pomo hipsters! Get with the gangsta-wanksta-pranksta CRANKSTER bling-bling program!!"

How does it all end? Does physics go bankrupt funding theories that have expanded our ignorance from four dimensions into ten, twenty, and thirty dimensions? Do tax payers revolt? Do young physicists overthrow the hand-waving, contortionist bullies and revive physics with a classical renaissance favoring logic, reason, and Truth over meaningless mathematical abstractions? Does Moving Dimensions Theory (MDT) prevail with its simple postulate? We'll all just have to wait!

But in the meantime, how do you think it will play out?

Will theories with postulates ever be allowed in physics again? Or will the well-funded, tenured pomo String Theory / M-Theory (Maffia-Theory) Priests send their armies of desperate, snarky postdocs and starving graduate students forth to displace and destroy all common sense, logic, reason, and physics in the academy? It must be so–for the greater good of physics, the individual physicist, and thus physics, must be sacrificed.

MDT's postulate: THE FOURTH DIMENSION IS EXPANDING AT A RATE OF C RELATIVE TO THE THREE SPATIAL DIMENSIONS IN QUANTIZED UNITS OF THE PLANCK LENGTH, GIVING RISE TO TIME AND ALL CLASSICAL, QUANTUM MECHANICAL, AND RELATIVISTIC PHENOMENA.

http://physicsmathforums.com/showthread.php?t=56

2. **andy.s**
   November 22, 2005

   How many times are you going to post this spoof.?
   It was only funny the first time.

3. **AJ**
   November 22, 2005

   It was only funny the first time.

   I'm not sure I'd go that far…

4. **Jason**
   November 22, 2005

   How many times are they going to publish the string theory spoof?

   How many more coffee table books?

   How many more films?

   How many more conferences?

   How many more millions of dollars to further the postmodern hoax that is string theory?

   How many more hoaxters are going to receive tenure for doing absolutely nothing positive for physics?

   How does it feel to be famous, to be revered, and yet,

   not one iota
   of your thought
   matters
   because it ain't true.

   what does it profit a man
   to gain the world
   and lose his soul?

   so c'mon!

   let us in on it!

5. **Jason**
   November 22, 2005

   I think it's hilarious. Especially with the pictures:

   http://physicsmathforums.com/showthread.php?t=56

6. **David Corfield**
   November 22, 2005

   Peter wrote: "Until I taught our graduate differential geometry course I hadn't realized just how tricky the definition of a tangent vector can be."

   Much simpler than equivalence classes of paths, derivations, or whatever, is synthetic differential geometry's idea of a tangent vector as a map from an infinitesimal object into the manifold.

   Urs Schreiber discusses SDG and gives references here.

7. **Elmer Fudd**
   November 22, 2005

   apparently assistant professors at haavard aren't too bright either.

8. **Shantanu**
   November 22, 2005

Peter , Jim Hartle is another non-string theorist whose name appears in the participants of teh quantum structure of space-time conference.

9. **woit**
November 22, 2005

Shantanu,

I didn't mean to imply that all except Ashtekar were string theorists, just that Ashtekar is the only LQGer. Besides Hartle, there are also various cosmologists (Linde, Steinhardt, etc.), and various other non-string theorists (Wilczek), or even historians (Peter Galison).

10. **Shantanu**
November 22, 2005

yes you are right. sorry I misundertood your article.
hope the video of the talks are archived

11. **Nigel**
November 22, 2005

Thanks Peter for the link to Robert Wald's paper. I like his emphasis on Einstein's great discovery in GR, p3:

'Gravity isn't a force at all, but rather a change in spacetime structure that allows inertial observers to accelerate relative to each other.'

Well done Robert Wald! That is accurate, and emphasises the spacetime fabric of general relativity. General relativity is better tested than string theory. Students should be allowed to know that there is FAR MORE evidence for a spacetime fabric than there is for string theory 😉

12. **andy.s**
November 22, 2005

Mega-dittoes on Robert Wald. I didn't realize until reading the paper that a lot of my hangups in understanding GR are due to my attachment to the Sophmore level Linear Algebra concept of a vector.

Maybe I should do some re-reading. Got nothing else to do while my spacetime fabric is at the cleaners.

13. **ks**
November 23, 2005

The definition of a tangent vector becomes tricky only in context of modern DG because algebraic techniques are introduced right from the start. The concept itself is nothing that one can't explain a high school student or even an ancient mathematician of a Pythagorean academia in a pure geometric terms without coordinates or algabraic clutter. With little additional investment of very basic topology it goes as follows:

Start with a sphere S (in arbitrary dimensions > 0) . Draw a line L from the center of S to a point p on S. The hyperplane H orthogonal to L with p in H is the *tangent space* of S in p. The points q!=p on H are called *tangent vectors*.

This concept can now be applied to smooth surfaces. First we need a definition of smoothness: let F be a surface of dim n and p a point on F. F is *smooth* in p if p is a point in an open set of S (not a boundary point) and n-dim spheres S1, S2 exist which suffices the requirements that p is the only common point of S1, S2 and F (no cusps). F is smooth iff it is smooth in each point (it is left to the reader to proof that choosing a certain pair of S1 and S2 is no loss of generality).

In particular a non-intersecting sphere S(p) exists in each point p on a smooth surface F. The tangent bundle is the set of tangent spaces of such spheres in the points p of F.

14. **ks**
November 23, 2005

One thing is missing in the little discussion above. The n-spheres S1 and S2 should be "parallel" i.e. embeddable in an n+1 dim space. Otherwise the tangent-spaces in their common point p would be different.

15. **Anonymous**
November 23, 2005

I took a one-quarter GR class from Bob Wald and didn't think it was so great. As he mentions in those notes, it was almost all spent on mathematical background. If I remember correctly, he only got to the Schwarzschild solution on the last day of class. Rather than cutting out material, perhaps he could save time if he didn't write everything out longhand on the blackboard and spend a great deal of time formulating every sentence in the most precise way possible. People are capable of getting all of the details from his book without him spelling it all out on the blackboard, after all. I felt that his class involved very little physics, so for those of us with a solid mathematical background it was largely a waste of time.

Do people really get to grad school at Columbia without knowing what a tangent vector is? This was covered in my first year of undergraduate mathematics (an analysis course, where we learned about calculus on manifolds and differential forms). Granted, not everyone has such a class so early, but surely most well-educated undergrads in mathematics learn some basic things about differential manifolds?

16. **woit**
November 23, 2005

Certainly most grad students at Columbia have seen some geometry and know what a tangent vector is. My comment was from the point of view of teaching this material in a graduate level math class where you want to be able to rigorously prove things. The issue here is how to define a tangent vector intrinsically, not assuming your manifold is embedded in Euclidean space. There are at least three kinds of different definitions you can use:

1. tangent vector = derivation
which is one the algebraic geometers love because it can be expressed algebraically.

2. tangent vector = equivalence class of curves

3. tangent vector = something that transforms correctly under change of coordinates.

Each of these definitions works, and can ultimately be shown to be equivalent. When preparing this material, I was just surprised to realize that rigorously showing this is not completely trivial.

17. **Anonymous**
November 23, 2005

"The issue here is how to define a tangent vector intrinsically, not assuming your manifold is embedded in Euclidean space. There are at least three kinds of different definitions you can use"

Sure. We saw each of these definitions, and proved their equivalence, in that undergrad class I was talking about. And they came up again in at least one other undergrad class I took. So I would have expected graduate students in math to be familiar with this already.

18. **woit**
November 23, 2005

This was covered as one part of the first couple weeks of foundational material in the class, it wasn't a big part of the course. Graduate classes often go over some of the same material as in undergrad classes, just much faster. Some undergraduate geometry classes don't really do much intrinsic geometry, so some of our graduate students probably hadn't seen some of this before.

19. **Walt**
November 25, 2005

It's not *that* unusual for an undergraduate math major to never have seen differential geometry, particularly at a smaller school. Most will have heard about tangent vectors from a differential equations course or just because the idea is in the air, but it's not unusual to not see it formally.

I think the place where starting with manifolds embedded in R^n really pays off is in tensor calculus. Picturing the tangent bundle as the manifold with little vectors attached to it isn't that hard, but general tensors are sufficiently abstract that the extrinsic point-of-view helps ease the pain.

20. **Florifulgurator**
November 29, 2005

Isn´t one of the most basic things you learn in math phys class that COORDINATES ARE OF NO PHYSICAL SIGNIFICANCE?
But then it looks physicists (and many diff geometers too) don´t want to do linear algebra without coordinates.
A field is a Christoffel symbol. A tensor is a monstrosity with indices. Wrrrrrrrrrrr. 90% of your IQ wasted! I´ve seen eminent minds fail at the calculus product rule while doing "tensor" calculus. No wonder without abstract tensor product.

Them Christoffel symbols and index notation have their place in computations with concrete coordinates. In general expositions they are more than a superfluous nuisance. O microsoftified math!

21. **woit**
November 29, 2005

Funny thing is I've known several physicists who have told me what a revelation it is to realize that you can think about these things in a coordinate-invariant way. I've also known mathematicians who have told me it was a revelation to realize they could actually make a choice of coordinates and use them to do calculations.

Seems to me, what one should aim for is understanding what you are trying to calculate in a coordinate invariant way, but also being able when necessary to know how to choose appropriate coordinates and work with them.

22. **Florifulgurator**
December 7, 2005

A veeery subtle reason for avoiding coordinates/bases where possible: TO MAKE CLEAR WHERE THEY ARE NEEDED. This point is way above almost all math/phys writ I encountered.
Indeed you sometimes need bases in abstract linear algebra. Example: Compute the adjoint of
[tex]T\bigotimes\bigwedge T \longmapsto\bigwedge T[/tex],
[tex]t\otimes a \mapsto t\wedge a[/tex]
This might occur in computations with (co)differential.
——————-
One more pearl of my wisdoms thrown into this fine black hole:AVOID DIRECTIONAL COVARIANT DERIVATIVES where possible. Otherwise, you will one day get stuck at a Laplacian.Exercise: Formulate the product rule for the induced cov. derivative on the tensor product of vector bundles without using a directional derivative. (Hint: Use the flat map permuting the factors).
=============
Sigh. Last century one of the final drops into my bucket was when I caught myself translating Lichnerowicz & Tachibana stuff (P Petersen, Riem.Geom, Springer GTM 171, Ch. 7.5) into my own calculus. Shortly after I gave up on serious math…
—————-
One of my DaDaistic dreams is writing a book titled THE CALCULUS OF PHYSICS, based on Th. Frankel´s book "The Geometry of Physics", putting most of his writ into a large appendix titled "Computational Reference"

Slate on Krauss

November 23, 2005
Categories: Uncategorized

Slate today has an article entitled [Theory of Anything?]() about Lawrence Krauss's [recent book]() and the controversy over string theory. The article begins by describing Krauss as having "a reputation for shooting down pseudoscience." and goes on to say:

Yet in his latest book, Hiding in the Mirror, Krauss turns on his own—by taking on string theory, the leading edge of theoretical physics. Krauss is probably right that string theory is a threat to science, but his book proves he's too late to stop it.

The article ends with the following summary:

Hiding in the Mirror does a much better job of explaining string theory than discrediting it. Krauss knows he's right, but every time he comes close to the kill he stops to make nice with his colleagues. Last year, Krauss told a New York Times reporter that string theory was "a colossal failure." Now he writes that the Times quoted him "out of context." In spite of himself, he has internalized the postmodern jargon. Goodbye, Department of Physics. Hello, String Studies.

Update: Lubos Motl [deals with the Slate article]() with the all-too-familiar favorite tactic of many string theorists when faced with criticism of the theory: don't respond to the argument being made, but instead attack the intelligence and competence of the person making the argument. After all, they're not a string theorist, so how bright can they be? In this case Lubos informs us that "Boutin's intelligence resembles that of dogs", while repeating his favorite claim that the status of string theory is much like that of the theory of evolution.

Comments

1. **Dr. Ranger McCoy**
   November 23, 2005

   String theory is starting to get on my nerves.

   I see hundreds of sheep getting tens of millions of dollars to polay pomo pattycake games with tax dollars, while physicists, poets, and philosophers, who rely on logic, reason, truth, and beauty, are exiled.

   I am shooting a documentrary entitled "Call the Bluff."

   I am looking to interview any string theorist, pomo artist, pomo Wall Street analyst, or pomo politician on video so as to get them to explain why think it's cool to steal tax and tuition dollars as well as pensions for their arrogant friends.

   The same kind of people who're stealing your pensions on Wall Street are furthering String Theory.

   They are anti-theory.
   Anti-truth.
   Anti-beauty.

   Pro-money for their rizty homes.
   Pro-tenure for their friends.
   Pro-BS as long as it keeps paying.

   It is time for a Renaissance.

   Let theories with postulates, truth, and beauty take the forefront of physics.

   http://physicsmathforums.com

2. **scott**
   November 23, 2005

   in case anyone is interested i found the article's author's resume.

   http://www.paulboutin.com/PaulBoutin.html

3. **Jose**
   November 23, 2005

   Beyond the standard model, is there any theory confirmed with experiment? No, we should conclude that all those theories aren't scientific theories?

4. **woit**
   November 23, 2005

   Jose,

   The problem with string theory is not that its predictions aren't confirmed, the problem is that it doesn't make any predictions.

   In the initial stages of working on a very speculative idea, it's not surprising if you can't yet figure out how to get predictions out of it. But after more than twenty years work, with no progress towards predictions, you start to get suspicious that they aren't possible. And much string theory research is now based on the idea that the theory can't inherently predict things, which seriously raises the question of whether it is scientific.

5. **Tony Smith**
   November 23, 2005

   It seems to me that this modification of part of Rudyard Kipling's "The Gods of the Copybook Headings" gives an optimistic (from my point of view) vision of the future of SuperString Theory:

As I pass through my incarnations
in every age and race,
I make my proper protestations
to the Gods of the String Theory Place.
Peering at reverent Stringers
I watch them flourish and fall.

And the Gods of Experiment Results,
I notice, outlast them all.
We were living in trees when they met us.
They showed us each in turn.
That water would certainly wet us,
as Fire would certainly burn:
They denied that Wishes were Horses;
they denied that a Pig had Wings.

So we worshiped the Gods of String Theory
Who promised these beautiful things.
But, though we had plenty of Strings,
there was nothing our Strings could predict,
And the Gods of Experment said:
'That means that String Theory is sick.'

Then the Gods of String Theory tumbled,
and their smooth-tounged wizards withdrew,
And the hearts of the meanest were humbled
and began to belive it was true
That All is not Gold that Glitters,
and Two and Two make Four—
And the Gods of Experiment Results
limped up to explain it once more:
As surely as Water will wet us,
as surely as Fire will burn,
The Gods of Experiment Results
with inevitable truth will return!

Tony Smith
http://ww.valdostamuseum.org/hamsmith/

6. **DJ**
   November 23, 2005

   Just found that Peter's name has been listed after P. W. Anderson, Sheldon Glashow, L. Krauss as "Prominent critics" on string theory in Wiki's "string theory" item.

   See:
   http://en.wikipedia.org/wiki/String_theory#endnote_critics

7. **secret milkshake**
   November 23, 2005

   1) There is the dark energy and dark mass we don't know anything about. It will be hard to cook up a grand unification scheme until the bulk ingredients are found.

   2) Maleable frameworks with lots of internal adjustable parameters are unhelpful. It is like Freudist psychoanalysis – you cannot prove them wrong. Invoking extra 7 unobservable dimensions having whatever-you-want properties seems like lotsa degrees of wiggle freedom to me.

   The particle zoo was there when the gauge theories were developed and one was able to predict the pattern of interactions and the properties of unknown particles – even as the numbers were hard to extract.

   String theorists keep adjusting their framework to the observable world – weeding out the alternatives that would not produce it – but apart from their requirement for SUSY and predictions about black holes, not much is realy coming out. Their swamp is big and deep and maybe they will never landscape (or dredge) it into a productive field.

8. **MathPhys**
   November 23, 2005

   They should have included G 't Hooft amongst string theory's prominent critics, for he certainly is (prominent and a critic!).

9. **Dr. Ranger McCoy**
   November 23, 2005

   M-theory is in fact the dark matter it has been searching for.

   The dark energy can be accounted for by the mysterious drive that compels young grad students to devote their lives to bs, by studying string theory.

   Oh wait–meaningless, indecipherable bs is the best way to get tenure.

10. **Aaron**
    November 24, 2005

    't Hooft taught a [course]() on string theory. You can find some of his views on the last page.

    Like most people, string theorists included, I think you'll find that he has more nuanced views than "string theory bad" or "string theory good".

11. **We Pretty**
November 24, 2005

What I as a representative of the public find wrong in string theory is the status that it has. Here is a comparison of some features between string theory and other theories
(CDT, LQG, Christianity):

String theory is cool (extra dimensions etc).
Applies also to Causal Dynamical Triangulations (2 and 4 and between dimensions), loop quantum gravity and Christianity (eternal life, heaven).

String theory does not yet make testable predictions.
Applies also to Causal Dynamical Triangulations, loop quantum gravity and Christianity.

String theory has been trumpeted as the theory of everything for a long time but is not yet scientific theory.
Applies also to Christianity.

String theory is known by the public as a theory that tries to explain everything.
Applies to Christianity

String theory is generally believed by the public to explain everything.
So is Christianity by large proportion of the public

12. **Ugo**
November 24, 2005

I think the problem String Theory, and to a certain extent Quantum Gravity Theory have is that they aren't born out of necessity.
For instance special relativity, general relativity, quantum physics, quantum field theory even qcd and electroweak theory were all born out of necessity to explain physical phenomena which couldn't be explained within the framework of the accept theories of the time.
They were doing catch-up to experimental results and that is the way work theoretical physics should proceed.
Not the other way around.
I mean, the lack of any kind of observable phenomena at the planck scale for the foreseable future (maybe one-two centuries) will make string theory, quantum gravity and a host of other "speculative" theories completely irrelevant.
Were they even in mesure of giving testable predictions there wouldn't be any type of realisable experiment to test those predictions.
Today, high energy theoretical physicsts are just playing with toy theories, they aren't doing physics.
Anyway wouldn't it be great if once the LHC is online, the experiments demonstrate that the higgs boson doesn't exist ?
Theretical physicists abandoning string theory and going back to the "drawing board" to come up with a good physical theory ?
Yep, we need experimental data to advance theoretical physics.

13. **Jose**
November 24, 2005

In XIX century Boltzmann introduces statistichal mechanics and the hypothesis of atoms to explain at a deeper level thermodynamics. He was strongly criticized, for example by Mach, because atoms were abstractions no forced by facts. Match thought that Boltzmann was not doing physics.

The same can be said about many other theories (quarks, Yang-Mills theories,…).

I think that explorations in theoretical physics shouldn't be confined by experiment. Experiments will decide what is the better theory but there is not a logical path from facts to theories.

With respect to string theory I think is a possibility and we shouln't discard it.

14. **Wolfgang**
November 24, 2005

> String theory is generally believed by the public to explain
> everything.
> So is Christianity by large proportion of the public

Would Lubos Motl be the Jerry Falwell of string theory in your analogy?

15. **Juan R.**
November 24, 2005

And do not forget others string theory's prominent critics, Feynman was one!

Jose. There is a **big** difference between string 'theory' and Boltzmann kinetic theory. The idea of atoms was rather 'popular' between physicists and chemists, and the theory was mainly testable even if atoms were not detected. Once atoms were experimentally found, Boltzmann theory was completely accepted.

The first step of string 'theory' would be offer us testable predictions and after, only after, we could search the strings. However, and this *may be you unknow* string theory has failed in all tests done until now.

As explained by Woit in his American Scientist's article *Is string theory even wrong?* string theory is probably the hypothesis/theory with the biggest discrepancy with experimental data (10[sup]55[/sup]).

> This is perhaps the most incorrect experimental prediction ever made by any physical theory that anyone has taken seriously.

Moreover, string 'theory' have appeared -during 3 decades- totally incompatible with observed data. For example, particle spectrum derived on string theory are massless supersimmetric states, laboratory data says that states are non-supersimmetric and with nonzero mass.

Do not forget that even the initial idea of an unidimensional object on 10D has failed and TODAY people is working with others things (such as D0-branes, which are pointlike objects) in 11D.

In the past, was claimed that string theory was a theory of physics with posibility for quantizing gravity, explaining the Standard Model, solving the measurement problem of QM, the divergences of R-QFT, and the singularities of GR (e.g. Big Bang). And all of this based in a single parameter!

Reality **TODAY** is as follow:

– The theory cannot predict anything, even statistically, due to the infinite Landscape trouble!

– The theory does not quantize gravity. In fact, it is already recognized by string theorists that string theory failed and then launched M-theory, that is not formulated still and nobody know what is.

– The theory can only deal with supersimmetric masless states, which are not the states of the Standard Model. Nothing of the sucesfull Standard model has been explained and/or derived from first principles.

– The theory does not solve the measurement problem of QM. In fact ideas proposed by people as Witten has been durely critized by specialists on quantum measurement as 'nonsense'.

– String theory does not cure the divergences of R-QFT. In fact, there exist not single paper, lecture, or book where was proven that string theory is NOT divergent, just a partial theorem and some conjetures. In fact, some authors think that string theory is as divergent (therefore unuseful) as covariant quantum gravity.

– Cosmological models based in string and brane theory have failed. A. Linde and other cosmologists proven as 5D-brane models was theoretically inconsisten and violating many experimentla data.

– From the initial single parameter on 10D we obtain a theory with more than 10000 unknown parameters when one compactificates to 4Dx6D!

– Etc.

Once string theory was a marvellous hipothesis but become a pure nonsense. Today many string theorists (or even some particle physicists as Weinberg) openly admit that theory is not predictive.

Juan R.

Center for CANONICAL |SCIENCE)

16. **Juan R.**
November 24, 2005

The discrepancy between string theory 'prediction' and experimental data is 1055.

That is,

10000000000000000000000000000000000000000000000000000000

For **B**oltzmann theory the typical errors are of order of 1% or less. For **Q**ED, there are discrepancies on the eleven figure…

Juan R.

Center for CANONICAL |SCIENCE)

17. **Christine Dantas**
November 24, 2005

Ugo said:

"the lack of any kind of observable phenomena at the planck scale for the foreseable future (maybe one-two centuries) will make string theory, quantum gravity and a host of other "speculative" theories completely irrelevant."

I think that discussions on astrophysical probes to quantum gravity are severely missing… There is already some relevant and interesting literature on this! I have listed some papers in my blog page. Tests of quantum gravity are already happening now, and maybe a lot will be learned with the GLAST experiment (Gamma Ray Large Area Space Telescope), in a near future. Or — maybe I am too optimistic.

Christine

18. **Who**
November 24, 2005

neat blog!
http://christinedantas.blogspot.com/2005/11/astrophysical-probes-of-quantum.html

I especially approve the BACKGROUND :))

19. **mclaren**
November 25, 2005

Been saying this for years and years. Common sense tells us that in order to qualify as a scientific theory, job one is that it must make objectively falsifiable predictions. Ufology makes none, so it's not science. String theory makes none either. In what meaningful way does ufology differ from string theory?

The big question that comes to mind: how long? How long will the public and the government (via tax breaks for educational instutitions, including NSF and DOE grants to private institutions like Stanford) continue to pay the proponents of a belief system like string theory which persistently fails to produce any objcetively falsifiable predictions?

20 years? 30 years? 50 years? 100 years? 200 years? 500 years? At what point do we decide string theory is a fancier more mathematically sophisticated version of alchemy or phlogiston?

How long do we continue to shovel money and time and energy, most importantly the intellectual abilities of some of our brightest young scientists, into a scheme that yields not a single testable prediction?

This proves a painful question to ask, since we all know the gummint spends precious little money on basic research — and the amount continues to plummet yearly. Plenty of gummint cash for "faith-based" initiatives nowadays, but none for basic research. It's to weep. But string theory just provides more ammo for those who want us to spend less. When no-neck members of the great unwashed talk about shutting down the NIS, string theory is the poster boy for that position.

Let's ask another question — would you object if the government suddenly started funding scientology? How about if all the brightest young scientists started to take lucrative tenure positions at major universities teaching scientiology? Would you have a problem with that?

If so, why don't you have a problem with the same situation going on for string theory? How does string theory differ from scientilogy in any signiifcant way? Both use impenetrable jargon…both systematically fail to make objectively falsifiable predictions about observed reality…both have churned away for many decades without generating any meaningful new areas of knowledge. Nobody ever built a new kind of power plant or invented a new kind of transistor based on the alleged "science" in scientology…but then again, nobody built a new kind of power plant or invented a new kind of transistor based on the alleged "science" in string theory either. The only thing the string theorists lack is an e-meter.

What makes this especially significant is the fact that the field effect transistor was a direct result of some of the earliest work done in quantum emchanics. Abstruse and arcane they might be, but quantum mechanics and special relativity both generated a cornucopia of practical real-world technologies from LEDs to transistors to nuclear power. Which practical new real-world technologies have arisen from string theory? Can you name one?
Just one?

After 30-plus years of work on string theory, there ought to be at least _one_ new technology coming out of all this intellectual ferment. When there isn't, I start to smell a Unarius saucer cult.

String theory and Intelligent Design are twins separated at birth. Both fail Occam's Razor. When I ask ID proponents, "Show me a single disease you can cure using ID theory that can't be cured using standard Darwinian molecular biology," they fall silent. When I demand of ID proponents, "Which new vaccines has ID theory allowed us to create that we couldn't create with the standard Darwinian model of evolution?" the response is…zero. Nada. Dick. Diddly. Zip. Zero. Squat. Bupkiss. The ID proponent stands there with his mouth opening and closing, like a guppy in a fish tank.

So let's hear it, string theorists — which new device can you build using string theory that you can't build using the standard electroweak model? Power plant? Computer? Sensor? Detector? Anything? Show me one device. Just one. Let us see it. Put it here on the table.

Put up or shut up. I'm tired of the 50-dollar words and the incoherent equations. Show me the hard objectively falsifiable evidence or shut up, sit down, and stop wasting my time.

The real tragedy of the superstring fiasco is that we're living in an incredibly anti-intellectual era. Arguably it's the New Medievalism. Rational skeptical thinking is getting rolled back on every front, from the denial of global warming to the denial of Darwin to the redefinition of "torture" into something (anything) less offensive… Yes, kiddies, we're rocketing back into the year 1100, and it's not pretty. What with the catastrophic drop in the funding of basic research and the wholesale destruction of skeptical critical thnking and the ever-growing public disdain for evidence and logic, America's in serious trouble. 23% of the Americna public *still* believe Saddam used chemical weaopns on our troops in Iraq.

And the big tragedy is that string theory adds fuel to the fire. String theory (so-called) gives the irrational 51% of American society (who think God created human beings in their present form and American forces found WMDs in Iraq and Saddam parachuted out of one of the 757s just before it hit the twin towers on 9/11) all the ammunition they need to _further_ remove logic and observed facts from American society. After all, the ID(iots) can argue, if ID makes no objectively falsifiable predictions, well, no matter: neither does string theory…and string theory is accepted at major U.S. universities, isn't it?

Somtimes I pinch myself and say, "What the hell happened to this country?" 40 years ago we were building particle accelerators and sending men to the moon. Now, we waste our time arguing about vacuous twaddle like ID and string theory. What next, angels dancing on the head of a pin? Will we get into a civil war about tridentine transubstantiation? Has an epidemic of highly infectious brain parasites over the last 40 years destroyed the population's frontal lobes? Why is anyone even spending time arguing about unbelievable tripe like ID or string theory or whether waterboarding is torture?

I feel like Ripley in the sequel to Alien, when she looked at the corporate officers and asked, "Did IQs drop sharply while I was away?"

20. **Juan R.**
November 25, 2005

Christine Dantas,

I agree with you that we are far from claim that quantum gravity is unobservable at usual scales. Similar errors were done in the past by particle physicists, first regarding relativistic quantum effects in chemistry (Dirac wrong quote claiming that relativistic effects would be unobservable in chemistry, today there exists relativstic quantum chemistry) and recently regarding parity violating effects.

Of course PV effects are very small for a single particle, but due to 'acumulative' effects can be observed in certain molecules thanks to modern high-precision spectroscopy. Weinberg's explicit claim that QM + electrostatic CB potential was sufficient for **all** chemistry purposes turned wrong, today there exists a electroweak quantum chemistry.

Quantum gravity, *a priori*, lives on the Planck scale. But has anyone shown that 'acumulative' effects in a molecule cannot be detected, for instance?

However, i would add a note on your comment.

Since there is not complete and satisfactory theory of quantum gravity, what is people 'proving' on astrophysical tests?

We would agree that they are only 'testing' partial truths, hypothesis, ideas. For example, if finally 'spacetime foam-like' contributions to energy spectra in future acellerators are detected, then one 'prediction' of LQG would be verified, but that does not automatically mean that full LQG was correct.

Juan R.

Center for CANONICAL |SCIENCE)

21. **Juan R.**
November 25, 2005

mclaren,

there is no comparison of string theory with alchemy or phlogiston. Alchemy (preparative alchemy) was based in experimental work and worked! It was basis of early medicine (iatrochemistry) of Paracelso, etc, etc.

Regarding phlogiston theory, it also worked, in fact explained many experimental data: chemical reactions, odor, color, etc.

Just a day plogiston theory was unable to explain combustion. well combustion was explained, but at expenses of negative masses. Then was subsituted by Lavosier theory.

Being wrong phlogiston theory could still be used today in a basic course of chemistry.

Eugenio Portela, 1999, Historia de la Ciencia y de la Técnica, 33-La Química ilustrada, ediciones Akal S.A. Madrid pag 19.

Somewhat as initial Dirac theory, being wrong (as explained in many quantum field theory textbooks), is still used in elementary courses on relativistic quantum mechanics.

Juan R.

Center for CANONICAL !SCIENCE)

22. **CWB**
November 25, 2005

How did Alchemy work? Who has ever turned lead into gold?

23. **Zelah**
November 25, 2005

CWB,Juan R means that Alchemy was success because it LEAD TO CHEMISTRY. At the moment in time, it is unclear if String Theory will lead to new theories. It seems to have lead only to improving old ones.

Juan R, if relativistic effects are being noticed in chemistry, then is this not in some sense an quantum gravitation effect? Especially as you support looking at Quantum Gravity using Flat connectives and putting the physics in gauge effects?

An amateur mathematician.

24. **Arun**
November 25, 2005

CWB,
Turning lead into gold was only one of the objectives of alchemy. A good recent book is
Distilling knowledge : alchemy, chemistry, and the scientific revolution / Bruce T. Moran.

The publisher’s blurb is

> The traditional grand narrative of the scientific revolution styles it as a decisive rejection of magic and mysticism in favor of rationality and empiricism. This engaging study of early modern science insists there was no such sharp break. Historian Moran traces the gradual evolution of alchemy to chemistry through a wide array of texts from the 15th through 18th centuries, including classical alchemical treatises, handbooks of practical alchemy, early chemistry textbooks and the writings of Newton and Boyle, both of whom considered alchemy a perfectly legitimate scientific discipline. He finds in alchemical thought intriguing precursors of modern ideas about the particulate nature of matter, the biochemical paradigm of life and disease, and Newtonian gravity. Moreover, he considers alchemy, which boasted a vast amount of lore on everything from metallurgy to medicine and was practiced not just by adepts but by doctors, artisans and housewives, to have been an important catalyst in the development of the scientific mindset; while alchemical theories may have been wrong, alchemical practice schooled society at large in everyday habits of observation and experimentation. Conveying a wealth of historical detail in an accessible, jargon-free style, Moran provides a fascinating corrective to simplistic notions of the origins of modern science.

25. **Juan R.**
November 25, 2005

Sorry to say this CWB, but your two questions are irrelevant. I clearly emphasized “preparative alchemy”. And YES! in the 20th century physicists were able to convert lead atoms into gold atoms via a nuclear reaction.

Zelah,

About if quantum gravity is observable or not a current energies, Penrose believes that mind may be based in some quantum gravity effect amplified by chaotic processes in human neocortex structure. I personally doubt, but nobody can afirm that still.

Yes relativistic effects are noticed in chemistry but are NOT gravitatory ones! For instance, chemical difference between AgH and AuH, the famous lanthanide contraction, optical properties for 5d band –> Fermi level in metallic gold, etc.

My emphasis on this topic was simply for illustrating that in the past physicists (even smart ones as Dirac, Weinberg, etc) said *this is unobservable this sin unimportant for chemistry* and now we **can measure** effects in the laboratory.

Can anybody claim that quantum gravity effects will be unobserved in physics, chemistry, or biology laboratories? Can someone really assure us that quantum gravity effects are observable only with a particle accellerator of Jupiter size?

Since this is going out-topic i will nothing more about this. Simply remark that string theory is **not** testable in many ways (due to ill-defined formalism, due to Landscape, due to lacking detailed models, etc.) and in others is **clearly incompatible with experimental data**.

Juan R.

Center for CANONICAL |SCIENCE)

26. **Lubos Motl**
November 25, 2005

If you’re interested in 15 reasons why Paul Boutin is a complete ignorant, read

http://motls.blogspot.com/2005/11/ignorance-of-paul-boutin.html

27. **Juan R.**
November 25, 2005

After read and re-read Paul Boutin Slate article i can resume it:

MISINFORMED.

Next some details.

> String theory, which stretches back to the late 1960s, has become in the last 20 years the field of choice for up-and-coming physics researchers.

Due to presure! Many young people interested in fundamental science is doing a PhD in string theory because other doors are closed to them. In fact, several graduate students confirm this to me personally! Today i think that two great topics are condensed matter and biophysics. ‘Stringy’ is only followed by 10^3 people or similar.

> Many of them hope it will deliver a “Theory of Everything”—the key to a few elegant equations that explain the workings of the entire universe, from quarks to galaxies.

In the past this was pure speculation, after turned into marketing point and now is pure nonsense. It is curious that nobody explain NOW why believe that string theory is a TOE. You can personally reply me on sci.physics.strings:

String theory is not a TOE. Juan R. (21 oct)

> Elegance is a term theorists apply to formulas, like E=mc2, which are simple and symmetrical yet have great scope and power. The concept has become so associated with string theory that Nova’s three-hour 2003 series on the topic was titled The Elegant Universe (you can watch the whole thing online for free here).
>
> Yet a demonstration of string theory’s mathematical elegance was conspicuously absent from Nova’s special effects and on-location shoots. No one explained any of the math onscreen. That’s because compared to E=mc2, string theory equations look like spaghetti. And unfortunately for the aspirations of its proponents, the ideas are just as hard to explain in words.

The BIG problem is that there is NOT elegance in string theory, i.e. mathematical simplicity, basic postulates, and explanation of phenomena. There are not postulates on string theory, there are a mathematical goulash and *ad hoc* procedures, and none experimental prediction or even ‘postdiction’.

> Einstein dismissed this probabilistic model of the universe with his famous quip, “God does not play dice.” But just as Einstein’s own theories were vindicated by real-world tests, he had to adjust his worldview when experimental results matched quantum’s crazy predictions over and over again.

Einstein NEWER accepted experimental results and was guided by pure mental experiments the rest of his life. For example, some biographers remarked as Einstein simply ignored the good concordancy of relativistic quantum mechanics to data.

> Step 3: String theory (1969-present). String theory proposes a solution that reconciles relativity and quantum mechanics.

This is flagrantly wrong. String theory reconciles nothing.

> To get there, it requires two radical changes in our view of the universe. The first is easy: What we’ve presumed are subatomic particles are actually tiny vibrating strings of energy, each 100 billion billion times smaller than the protons at the nucleus of an atom.

Even asuming as true that strings are ‘vibrating energy’. The idea of strings is already very outdated. All popular recent advances -for example computation of BH entropy- are based in BRANE theory. Modern views point to the abandon of the string (really superstring) as fundamental. In fact, the only version known of M-theory is a theory of **poinlike particles** (D0-branes).

> That’s easy to accept. But for the math to work, there also must be more physical dimensions to reality than the three of space and one of time that we can perceive. The most popular string models require 10 or 11 dimensions.

But for explaining world we look, we may do 10 –> 4×6 and then all initial ‘elegance’, hyphotetical predictive posibility, etc. is lost.

> What we perceive as solid matter is mathematically explainable as the three-dimensional manifestation of “strings” of elementary particles vibrating and dancing through multiple dimensions of reality, like shadows on a wall. In theory, these extra dimensions surround us and contain myriad parallel universes. Nova’s “The Elegant Universe” used Matrix-like computer animation to convincingly visualize these hidden dimensions.

Nothing of this paragraph has been even minimally proven. In fact, it is proven just the contrary, none of string theory spectra is compatible with Standard Model. Using computer animation i can prove that universe is 0 D and that earth is really flat. I can prove anything with a computer package 🙂

> Unlike relativity and quantum mechanics, it can’t be tested. That is, no one has been able to devise a feasible experiment for which string theory predicts measurable results any different from what the current wisdom already says would happen.

Typical marketing! String theory has been tested during decades and changed due to failures. Begin from failure on strong force (before QCD) and finalizing by most recent claims that dark matter was cosmic strings…

Do not forget that string theory has been succesive changed due to experimental failure.

For example, the first post-strong-force version was bosonic and due to violation of *experimental* LI, string theorists passed from 4D to a new 26D version, then this is again *experimentally incorrect* and ad hoc corrected via KK, but then the new version of string theory predicted tachions, since they are *experimentally* unobserbed, the theory was again modified via ad hoc supersimmetry, and the new theory was again inconsistentet, and etc. String theory newer worked. Now, since string theory is again considered to be not 100% compatible with GR, it has been proposed the *new* M-theory that nobody know that is.

> The string theorists blithely create mathematical models positing that the universe we observe is just one of an infinite number of possible universes that coexist in dimensions we can’t perceive. And there’s no way to prove them wrong in our lifetime.

But then ‘the 101’ says that a hypotesis cannot be tested is not a scientific hypotesis!

> That's not a Theory of Everything, it's a Theory of Anything, sold with whizzy PBS special effects.
>
> Last year, Krauss told a New York Times reporter that string theory was "a colossal failure." Now he writes that the Times quoted him "out of context."

String theory pressure again?

Juan R.

Center for CANONICAL |SCIENCE)

28. **Christine Dantas**
November 25, 2005

Juan R. Says:

Since there is not complete and satisfactory theory of quantum gravity, what is people 'proving' on astrophysical tests?

We would agree that they are only 'testing' partial truths, hypothesis, ideas.

Yes, it would be illogical to test something that one does not know what it is. Some quantum gravity approaches suggest the existence of effects that could be verified in principle. Therefore some limits are being searched for using astrophysical signals: gamma ray bursts, ultra high energy cosmic rays, the microwave background radiation, etc. One of the main effects being investigated is Lorentz violation. There are already quantitative limits to this. More information can be found in, e.g., Mattingly´s review. I have posted a partial list of papers in my blog that I think is a reasonably good entry to the relevant (astrophysical) literature on this matter. The bottom line is that these are not really tests of quantum gravity, we are just probing Nature the best we can. This is better than nothing.

29. **John Gonsowski**
November 25, 2005

The best way for string theory to pick up a bunch of predictions would of course be to actually include the Standard Model in the theory. I could see them having a good connection to the Standard Model and still having a bunch unneeded extra stuff but that would be an improvement on no Standard Model. What does the all-intelligent Lubos have to say on connecting to the Standard Model? He told me this back in 2001:

"Yes, there are hundreds papers about realistic heterotic string models;this is a very well known issue. Throughout the 80s and the early 90s, everyone believed that there was a single realistic stringy way to descibe our world: E8 x E8 heterotic string theory on a Calabi-Yau space where the Standard Model is embedded into one of those E8s. For instance, you can start with an E6 subgroup of E8 – a good Grand Unified Theory that automatically gives you correct representations of fermions etc. if you start from string theory. Today we have some new implementations of the real world into string theory: Horava-Witten on Calabi-Yau, M-theory on G2 manifolds, F-theory on Calabi-Yau four-folds, brane worlds where the Standard Model lives on branes etc. but heterotic strings are still the most classical example."

I personally think they should have declared more of a victory with that E6 GUT and then keep the new ideas (branes, M-theory, F-theory) as related as possible to that GUT.

Lubos also had this to say about a good E6 GUT related M-theory:
"People are excited about the possibility that the 26-dimensional bosonic string is connected in one way or another with supersymmetric string theories (see also papers by Englert et al.). But I repeat that no convincing arguments have been found – excitement is one thing, evidence is another; at least 95% of string theorists (especially the leaders) agree with this claim of mine. From this point of view, those three names you mention consistute none. Physics – and string theory especially – is not a free arena for your fantasy. It has very tough rules and things that are not justified do not become part of it. Once again, all such things are just idiosyncratic speculations. Concerning nonperturbative behavior of the bosonic string, many more renowned names than those you mentioned – namely Lenny Susskind and Gary Horowitz – suggested various solutions (a 27-dimensional Bosonic M-theory in this case) but it is fair to say that their proposal has not been accepted by the community and there are serious reasons."

The serious reason is probably the bosonic part of Bosonic M-theory but if you have the E6 GUT (with fermions) then why is bosonic such a problem? It's not like there are actually any supersymmetric particles around that need an explanation.

30. **Bryan**
November 26, 2005

The supersymmetric partners in the vacuum are part of the aether of string theory. You can't disprove aether very easily…

31. **Juan R.**
November 26, 2005

Christine Dantas Says,

> Yes, it would be illogical to test something that one does not know what it is. Some quantum gravity approaches suggest the existence of effects that could be verified in principle. Therefore some limits are being searched for using astrophysical signals: gamma ray bursts, ultra high energy cosmic rays, the microwave background radiation, etc. One of the main effects being investigated is Lorentz violation. There are already quantitative limits to this. More information can be found in, e.g., Mattingly´s review. I have posted a partial list of papers in my blog that I think is a reasonably good entry to the relevant (astrophysical) literature on this matter. The bottom line is that these are not really tests of quantum gravity, we are just probing Nature the best we can. This is better than nothing.

I completely agree that many quantum gravity approaches suggest the existence of effects that could be verified in principle. Take for example Lorentz violation.

I do not know details of the history of LQG approach, but from reviews i obtained the idea that violation of Lorentz invariance is an early claim of that approach. Moreover, it appears that recently they found some links with double spacial and other ideas. Then if one tests Lorentz violation at 2007 via verification of LQG formula

E^2 = p^2 + m^2 + f(L_p)

are we verifying LQG?

Well the reply may be NO.

The obtaining of the formula may a lucky 'coincidence' (just as Dirac predicted antiparticles using his hole theory which is wrong according to R-QFT where E is > 0).

LQG may be still wrong. Remember that after of many decades still nobody has found the correct classical limit of LQG. Therefore, the theory cannot be correct even if just the part used for computation of Lorentz violating terms appears correct.

Situation is similar to Gerber gravity; it predicts the same perihelion anomaly for Mercury that GR and, however, is an incorrect theory.

Now take string 'theory'. Of course, there is not theory, just mathematical time-dependent gulash of ugly formulas with inconsistent ideas. In the past, Lorentz was claimed to be exact and then people introduced extradimensions and other stuff. After LQG was durely critized by string theorists because was not purely Lorentz invariant.

See for example

http://en.wikipedia.org/wiki/Lorentz_invariance_in_loop_quantum_gravity

Curiously, now string theorists admit that string theory is not the *last word* and are searching a M-theory.

😉 just M(atrix) theory is NOT Lorentz invariant in general. From Banks

> Full Lorentz invariance is not obvious and will arise, if at all, only in the large N limit. It also follows from this that Matrix Theory is not background independent.

Therefore one can chose 'stringy' formulations where Lorentz invariance is fundamental and other where it is not, but derived in certain limits.

Scenario A

Imagine that one finds violation of Lorentz invariance in a future experiment, string theorists will say:

> Yes we predict that via M-theory. This is the first quantum gravity prediction of M-theory verified. LQG, if correct, may be a part of M-theory

Scenario B

Finally no violation of Lorentz invariance is measured. Then string theorists will say:

> Yes string theory predicts that since decades ago since Lorentz invariance is one of basic 'simmetries' of the theory. During last decades we critize LQG, now it is experimentally found that LQG is incorrect, string theory is the only way to quantize gravity

String M-theory, as a whole, is a theory of nothing, but taking some parts of the formalisms developed during 3 or 4 decades, one can make a theory of anything.

Personally, i believe that Lorentz invariance is approximated and we can test it (In fact, it appears that recent cosmic ray observations suggest the possibility for a preferred rest frame. No?).

Juan R.

Center for CANONICAL |SCIENCE)

32. **Wolfgang**
November 26, 2005

> don't respond to the argument being made, but instead attack the intelligence and competence of the person making the argument.

Sounds familiar
http://iso42.blogspot.com/2005/11/lubos-motl.html

33. **joe**
November 26, 2005

Peter,

With which of lubos's statements do you take issue?

34. **secret milkshake**
November 27, 2005

Slate should have hired actual physicist to pen the Krauss review. Boutin does not have the understanding – he parrots what he thinks he got from the book and he is confused about basics. It is a lot like Rush Limbaugh explaining Bjorn Lomborg to the popular audience. Add Motl to the mix and you get a troll-fest.

35. **Jim Wills**
November 27, 2005

As the author of a novel whoes protagonist concludes there will never be a coherent theory of quantum gravity, I find all of this interesting.

36. **dog**
November 28, 2005

It is funny when Motl says "Boutin's intelligence resembles that of dogs"

Did he not recently sign his posts as "Lumo leashed" ?

37. **ark**
November 28, 2005

Those who criticize string theory for not producing results simply forgot the fact that it did produce conformal field theories known in condensed matter physics and fully supported experimentally. It produced Gromov-Witten invariants-extremely useful in algebraic geometry. It produced mirror symmery traces of which can be seen experimentally already. It affected theory of exactly integrable systems used in many practical applications, etc. etc The fact that some people do not like extra dimensions cannot be considered as fundamental flaw of string theory since even simplest models of classical mechanics are multidimensional. The problems with string theory originate from the fact that it is based on the Regge theory which itself is not well founded. No string theory texbook mentions this fact. No texbook also mentions about the fact that for each parent Regge trajectory there is a countable infinity of daughter trajectories which actually never were observed….Instead for each parent trajectory one sees just a few daughters…Why this is so….? Or how string theory can reconsile itself with the Froissart theorem (providing some known bounds on the total crossections) in the case of a graviton? I would like string theoreticians to think about this…and the rest who are critics of string theory too…

38. **Thomas Larsson**
November 29, 2005

Those who criticize string theory for not producing results simply forgot the fact that it did produce conformal field theories known in condensed matter physics and fully supported experimentally.

Nobody has denied that CFT is the correct theory of 2D critical phenomena; this has been verified beyond reasonable doubt, and I have argued on several occasions that BPZ deserve a Nobel prize for this discovery. However, this does not mean that string theory is the correct theory of QG in 4D. There is little doubt that string theory and its cousins are the correct way to describe 2D things – strings in spacetime (obviously), membranes in space, 2D gravity, 2D phase transitions. This is valuable, because 2D systems can be readily manufactured in the laboratory, but it is also a limitation, since systems in 3D and 4D are physically more interesting.

In fact, my own critique of string theory follows closely the LQG critique voiced by Nicolai-Peeters-Zamaklar in hep-th/0501114. In section 6.1 they essentially argue that

1. The free bosonic string is 2D gravity coupled to scalar fields, and as such it is a useful toy model for 4D gravity coupled to matter.

2. The constraint algebra generically (except for D=26) acquires an anomaly, but the theory is nevertheless consistent for D less than 26.

3. By analogy, the constraint algebra of quantum gravity in 4D, which in covariant formulations is the spacetime diffeomorphism algebra, should generically acquire an anomaly.

4. No such diff anomalies in 4D appear in LQG, which hence is probably wrong.

I have twisted their argument a little, but only very little, in order to make the punchline:

5. No diff anomalies in 4D appear in string theory neither, which hence is probably wrong, too.

The idea to treat 4D gravity in analogy with 2D gravity is not new; it was proposed by Roman Jackiw and collaborators, see e.g. hep-th/9501016, gr-qc/9511048. What gives myself a competitive advantage is that I know the generalization of the Virasoro algebra to higher dimensions and its representations. It seems clear that if anomalies arise in 4D gravity, which they must in analogy with the 2D case, it is a good idea to know how to construct such anomalies. Otherwise is like trying to do string theory without knowing about the central charge.

39. **ark**
November 29, 2005

The existing string/brane theory is not going to succeed even if the swampland problem is resolved. This is so because of the Froissart bound (theorem) mentioned earlier. I completely agree with items 1 and 3. Moreover, I am about ro submit paper where items 1 and 3 are fully developed …

40. **Thomas Larsson**
November 29, 2005

Moreover, I am about ro submit paper where items 1 and 3 are fully developed …

You may wish to study the literature, lest you rediscover something which is already known.

41. **ark**
November 29, 2005

This is precicely what I've said in my 1st message: some things are either intentionally or unintentionally deleted from the string-theoretic textbooks. New generations of interested young people begin their study of string theories without proper historical background believing that foundations are corect and only the roof needs some repair.

42. **andy**
November 29, 2005

Professor Woit,
The link to the article does not appear to work anymore.

43. **woit**
November 29, 2005

Andy,

The Slate site seems to be down. Presumably it will be fixed soon.

44. **Anonymous**
December 1, 2005

http://www.techcentralstation.com/120105C.html

Leonard Susskind in 1970 'got stuck in an elevator with Murray Gell-Mann' who ridiculed Susskind's string theory: 'Gell-Mann responded with loud, derisive laughter.'

'… Some scientists regard string theory as an unjustified and over-hyped speculation. Peter Woit, who teaches mathematics at Columbia University, has a blog and upcoming book criticizing string theory as "Not Even Wrong." Moreover, contrary to longstanding hopes, string theory has not provided a concise formula — something like Einstein's E=mc2 — giving a deep mathematical explanation for why the cosmos is as it is. Instead, string theory increasingly has seemed compatible with diverse universes. That's something celebrated by Susskind but disturbing to some of his fellow string theorists; and to critics such as Woit, it's a sign the theory makes no sense.'

Two Conferences

November 26, 2005
Categories: Uncategorized

Two recent interesting conferences that have some materials available on-line:

Last week the Perimeter Institute held a workshop on Emergence of Spacetime. Some of the talks are available at the Perimeter streaming video site (scroll to the bottom of the list for the Emergence of Spacetime workshop). The first day talks which are available online cover a very wide variety of points of view, including Petr Horava on string theory, Renate Loll on causal dynamical triangulations and Seth Lloyd on “Computing the Universe”.

Earlier this month there was a program in Lisbon on Algebraic Geometry and Topological Strings. The program included mini-courses by Jim Bryan, Marcos Marino, Albrecht Klemm and Rahul Pandharipande.

Comments

1. **Who**
November 26, 2005

   that link doesnt get to it either
at least this should work to get the program of the workshop, with its schedule of talks:
http://perimeterinstitute.ca/activities/scientific/PI-WORK-5/schedule.php

   to download the slides and recording you probably do have to
go thru the steps Peter mentioned, scrolling down the menu and all.

2. **Chris W.**
November 26, 2005

   Xiao-Gang Wen has collected the papers most closely related to the ideas presented at the PI workshop on this page.

   I just finished watching the presentations by Markopoulou and Seth Lloyd. The latter summarizes his quant-ph/0501135, which was submitted to *Science*, and also touches upon the subject matter of quant-ph/0505064.

   In case you haven’t read much of or about Lloyd’s work, it can be very briefly summarized as follows:

   He starts by setting the problem (after an introduction by Lee Smolin). Forget trying to arrive at a quantum theory of gravity by quantizing the classical theory in some fashion. Instead, make a guess at a quantum theory that (one hopes) captures essential features of general relativity (as geometrodynamics!), and see how much headway can be made. So what’s the quantum theory? It’s a system consisting of simple two-way quantum gates — two inputs and two outputs. Feed an output of one gate into an input of another, constructing a simplicial lattice *and a causal order*. As in causal set theory one relies on the fact that the causal order determines the metric structure of spacetime up to a conformal factor. Getting this to work with two-way gates constrains the dimensionality of the spacetime to 4. (Lloyd spent some time on this in his talk.) The structure of system — the quantum computation — contains more information however, and this makes it possible to formulate an analog of spacetime’s energy-momentum content as well as a Regge-calculus-like construction and reproduce Einstein’s field equation.

   Of course there is fine print. In particular he makes the point that the computational universality of even such a simple system is a problem — his version of the string theory landscape, as it were. (He mentioned this; I’m not just throwing it in.) It can simulate too much; we would like to identify a principle that constrains the simulation to something closely approximating the Standard Model in a classical spacetime governed by $G_{\mu\nu} = 8\pi T_{\mu\nu}$.

   Markopoulou’s talk complements Lloyd’s in some respects, and she makes the connection explicitly; she also employs ideas from quantum information theory, with emphasis on the ramifications of coarse-graining procedures applied to a quantum computational “pre-geometry”.* Furthermore there are strong connections between spin networks as employed in LQG and string-nets as employed by Wen and, more generally, several points of contact with condensed matter models of fundamental physics, which is what led the organizers to invite Xiao-Gang Wen and G. E. Volovik to this workshop.

   ——————————
   * Remember John Wheeler’s “pregeometry as the calculus of propositions” in Chapter 44 of *Gravitation*?

   Also compare with the ideas outlined in physics/0505040.

3. **Wolfgang**
November 27, 2005

   Chris W.,

   interesting post about an interesting idea. Other people (e.g. Finkelstein) have made similar proposals and the difficulty is always to get GR in a classical continuum limit. Lloyd tries to use Regge calculus to do this, but the devil is in the details as you have noticed. In particular the details of the ‘microscopic elements’ of the quantum network; There sems to be an infinite number of possibilities how such an element could look like.
But perhaps we are lucky and the ‘simplest element’ can reproduce our world (and nothing more).

4. **Fabien Besnard**
November 28, 2005

   CDT is a very interesting theory also. However there is something that puzzles me. You are in a 4 d world, but you can show that at short scale it is 2 dimensional. Now you use 4 d building blocks to prove this. But suppose you were living at the 2 d scale : you would never think of using 4 d building blocks, you would use 2 d blocks instead. But now I think I remember from a paper of Loll et al that if you use 2 d blocks you get degenerate geometries, and anyway you don’t get 4 d at large scale. I think this is problem. You should be allowed to use 2 d building blocks if the short scale effective dimension is really 2. It’s a bit like supposing you have an acid solution and finding that the pH is 8. Now it’s an effective dimension, and maybe this is why I’m mistaken. Does Loll address this question in the conference ? I watched the loops 2005 one but unfortunately the sound was too bad in the part where she talked about this dimension issue.

5. **Wolfgang**
November 28, 2005

Fabien,

the fractal dimension of 2d euclidean dynamical triangulation is 4
or very close to it. It seems to work in both directions.
At this point I would emphasize "it seems".

6. **Who**
November 28, 2005

Fabien, to respond without being able to give more than my own interpretation:

I think one has to make a sharp mental distinction between the simplex dimension (a feature of the regularization) and the quantum observable which is the observed dimensionality at a given scale.

I think the simplex dimension is a combinatorial formality that helps to determine the network in which the blocks are assembled and how to shuffle and permute their interconnections, and the form of the Lagrangian.

the simplex dimension has no direct tie to the resulting spacetime dimension—–indeed prior to 1998, when they used plain DT, they found that when they used 2D and 3D simplices the resulting dimensionality was usually wrong. It would either be unbounded—essentially infinite—or too small. the resulting dimension was always too big (much too big) or too small.

in 1998 they started doing CDT and by 2003 they had found that in the case of using 2D and 3D simplexes the resulting expectation value of the largescale dimensionality was CORRECT. Actually the pathological behavior of too large or too small dimensionality can occur, but it has vanishing probability.

It took several years to do the computer simulations for the 2D and 3D case, so only in 2004 they got around to the 4D case.

I think in the CDT approach they do not pretend that the simplexes actually exist—they are just a combinatorial formalism that provides a framework for the dynamics and a regularization for the path integral.
When the model is compared to physical scale my understanding is that it is assumed the size of the simplexes is much smaller than planck length, the idea is that the size of the simplexes should go ideally to zero.
So I think of these simplexes as an imaginary formality, for performing the random Monte Carlo moves that implement the spacetime dynamics and for computing the path integral and expectation values of various things. I try to detach the idea of the simplex dimension from my expectations about the real spacetime dimension observed at various scales.

The result is that although I find your questions intriguing I nevertheless disagree when you say: "You should be allowed to use 2D building blocks if the short scale effective dimension is really 2."

You could be right though. I can't speak with any assurance and what you are pointing to does seem paradoxical. No time right now to try to shorten or clarify this post so have to leave it confused. maybe someone else can clarify.

7. **steven jones**
November 28, 2005

I'm visiting Duke this week, and they have this talk:

String Theory Seminar

2:45pm, 120 Physics Building (DUKE)

Katrin Wendland (University of North Carolina, Chapel Hill)

"Z_4 orbifold limits of K3 and a family of smooth quartic K3s: A
nonclassical duality"

Smells like bullshit to me… but perhaps someone can explain what the title means?

8. **woit**
November 28, 2005

Steven,

Not clear why you think Wendland's talk is any more "bullshit" than any other. She works on conformal field theories with target space a complex surface (i.e. 2 complex dimensions, 4 real dimensions). A K3 is such a surface, they come in families.

I think it's hard to make the case this is interesting for physicists. It seems to me to be the kind of thing many physicists have in mind when they say it belongs in math departments, not physics departments. Wendland does work in a math department, so physicists can't complain. There's a lot of interesting math involved here: conformal field theory and algebraic geometry. I don't know any particular reason to be interested in the specific geometry and models she is studying, presumably experts in this are could tell you more of the motivation.

9. **Chris W.**
November 28, 2005

New paper (25 Nov 2005) by Lauscher and Reuter reviewing their recent work and the similiarity of its conclusions to those of the recent work in CDT:

Asymptotic Safety in Quantum Einstein Gravity: nonperturbative renormalizability and fractal spacetime structure (hep-th/0511260)

10. **Fabien Besnard**
November 29, 2005

Wolfang :

>the fractal dimension of 2d euclidean dynamical triangulation is 4

This is very interesting. Can you tell me in which paper do they speak about this ? I could not find it. It is strange that such a consistency result is not more emphasized.

Who :

>So I think of these simplexes as an imaginary formality

Yes, this is precisely why it is important to prove it. In fact you seem to think about an even more stringent consistency condition : that the building blocks topology (and in particular dimension) have no importance whatsoever. I don't think that something that strong could be true, but maybe I'm wrong.

11. **Wolfgang**
November 29, 2005

Fabien,

http://xxx.lanl.gov/abs/hep-th/9806241

Already in the abstract they state that:

"the intrinsic Hausdorff dimension of usual 2d Euclidean quantum gravity is four, and not two. However, certain aspects of quantum space-time remain two-dimensional, exemplified by the fact that its so-called spectral dimension is equal to two"

The paper discusses the relationship between 2d Euclidean and Lorentzian dynamical triangulation and may shed some light on your question.

12. **Fabien Besnard**
November 29, 2005

Thanks a lot, Wolfgang. It seems a very interesting and important result, although I still don't understand why the spectral dimension would remain equal to two. I'll have to read and see.

13. **Chris W.**
November 29, 2005

By the way —

From hep-th/0505113; Ambjorn, Jurkiewicz, Loll; June 2005 (abstract):

> We measure the spectral dimension of universes emerging from nonperturbative quantum gravity, defined through state sums of causal triangulated geometries. While four-dimensional on large scales, the quantum universe appears two-dimensional at short distances. We conclude that quantum gravity may be "self-renormalizing" at the Planck scale, by virtue of a mechanism of dynamical dimensional reduction.

Compare with gr-qc/9310026; G. 't Hooft; October 1993 (abstract):

> The requirement that physical phenomena associated with gravitational collapse should be duly reconciled with the postulates of quantum mechanics implies that at a Planckian scale our world is not 3+1 dimensional. Rather, the observable degrees of freedom can best be described as if they were Boolean variables defined on a two-dimensional lattice, evolving with time. This observation, deduced from not much more than unitarity, entropy and counting arguments, implies severe restrictions on possible models of quantum gravity. Using cellular automata as an example it is argued that this dimensional reduction implies more constraints than the freedom we have in constructing models. This is the main reason why so-far no completely consistent mathematical models of quantum black holes have been found.
> Essay dedicated to Abdus Salam.

14. **Matti Pitkanen**
November 29, 2005

I think that I have good reasons to hope that this comment might not be classified as off topic but still I have unpleasant feeling in my gut(;-).

The effective 2-dimensionality is an interesting phenomenon which appears also in TGD framework. Apart from vacuum functional which carries information about 3-surface or equivalently about corresponding 4-surface analogous to Bohr orbit (exponent of Kahler function in the configuration space of 3-surfaces, the "world of classical worlds"), the quantum state carries only information about certain rather special 2-dimensional sub-manifols of 3-surface having interpretation as partons.

These partonic 2-surfaces represent cross sections of 3-D lightlike causal horizons of 4-surface meaning that partons are analogs of shock waves. It is not difficult to guess that a generalization of super-conformal invariance so that it applies to metrically 2-dimensional but topologically 3-dimensional lightlike causal horizons is involved. This superconformal invariance is present only for 4-dimensional space-time surfaces so that the theory predicts space-time dimension correctly from the requirement of generalized super-conformal invariance alone.

Matti

15. **ali**
November 30, 2005

Off-topic, but did anyone else see Brian Greene on The Colbert Report last night?

Brian's heart just didn't seem in it. He trotted out the usual talking points though anyway. I really wish that string theorists wouldn't always immediately think in terms of strings when addressing unification. Don't they see a need to introduce the problem first and then say why strings are nice? From the interview you didn't get really any idea of why quantum theory and gravity were so hard to put together. Brian just said that there's this great theory out there called Superstring theory that may be the final theory that describes everything. I would've far preferred Brian giving a physical basis for why unifying gravity and quantum mechanics is so hard. And not one based on non-singular, stringy Feynman diagrams, please. (I realize that Colbert's Q&A sessions can be a bit daunting, but that's all the more reason to really figure out what it is you want to say beforehand.)

Case in point: I attended a talk by Seth Lloyd this week where he described a very conceptual approach to quantum gravity. His springboard was the GPS system. Satellites with clocks all tracking one another and mapping out spacetime. That kind of explanation could capture the imagination of the public much more than non-singular Feyman diagrams or extra dimensions.

To string theorists, I say, please step back a little bit when addressing the public before launching into your talking points.

16. **Anonymous**
November 30, 2005

god, ali, string theorists like that should be strung up…

17. **woit**
November 30, 2005

Anonymous,

Please stop with the content-free abuse of string theorists, both ones who are my colleagues and ones who aren't. It has been justifiably pointed out to me recently that there's sometimes a Motlesque tone to many of the comments here, which is not a good thing. Leave the personal abuse tactic to Lubos and colleagues since it's often the only argument they have.

18. **Who**
November 30, 2005

" Leave the personal abuse tactic to Lubos …"
Amen to that. Self-defeating.

BTW of course like everyone else, I would imagine, I have have the highest respect for Frank Wilczek, but I was disappointed by parts of his article with Tegmark and others

http://arxiv.org/astro-ph/0511774

"Dimensionless constants, cosmology and other dark matters"

the reasoning involving the "prior" probability distribution on the vector of 31 dimensionless constants seemed nebulous or fishy.
the assumption of a particular eternal multibubble inflation to give a prior. Bayesian reasoning can be a very good thing but unexamined foundations here could be rotten.
wonder if anyone else felt worried by this paper

however nothing adhominem—only highest respect for Wilczek and his fellow authors.

19. **anon**
November 30, 2005

"Case in point: I attended a talk by Seth Lloyd this week where he described a very conceptual approach to quantum gravity."

Seth Lloyd's "theory of quantum
gravity" has much less predictive value than String Theory, and very little original content (it consists mainly of the ancient Regge ideas). For all his faults, Lubos has given a very intelligent assessment of Lloyd's extremely naive, wishful thinking ideas about quantum gravity. It's sad that some of the people (not Woit or Wolfgang) commenting in this blog seem to think that Lloyd's ideas are a respectable alternative to string theory or even a serious foundation for a theory of quantum gravity. I'm afraid that those highly gullible people are guilty of judging a book by its cover, in this case judging Lloyd by the fact that he works at MIT.

Even within quantum computing, Lloyd's contributions have been minimal. (if you disagree, please tell me what they are). If somebody asked me who is the quantum computation counterpart of Michio Kaku, I would say Seth Loyd. Lately, Seth has decided to make a foray into quantum gravity, and he is way out of his depth.

20. **Andreas**
November 30, 2005

What I do not understand about Lloyd's approach is that at some point he ad hoc introduces a continuous manifold in which he embeds his QC-graph. What is the motivation for this and "where" does the manifold come from? It does not seem to be a truly "emerging spacetime" concept.

21. **Chris W.**
December 1, 2005

anon,

It might help others in assessing Lubos Motl's comments if you could point to some approaches to quantum gravity — other than string theory of course — that he has *not* dismissed as a naive product of wishful thinking and half-baked speculation, or a reasonably well developed but conclusive failure. "String theory is the only game in town and everything else is a waste of time, talent, and research funding" doesn't cut it.

After watching his antics for more than a year and a half I find it difficult to care about anything Motl says anymore. If I want a usefully critical perspective on any idea I happen to find interesting there are plenty of other places I can find it.

22. **Wolfgang**
December 1, 2005

anon,

the proposal of Seth Lloyd is an interesting idea, just 'some details' need to be worked out; as in the story of Pauli who claimed that he can paint like Tizian once he has worked out 'some details'.
By the way there are many other great ideas which just need 'some details' be worked out (LQG, causal sets, etc.)

With superstring theory it is almost the other way around. A lot of details have been worked out and it all looks very promising.
I am just not convinced it is such a great idea 😎

But what do I know…

23. **Wolfgang**
December 1, 2005

By the way, there is a fresh paper related to this discussion:
"Spin networks, quantum automata and link invariants"
http://xxx.lanl.gov/abs/gr-qc/0511161

24. **Chris W.**

December 1, 2005

The aforementioned paper is one of several included in the proceedings of QG05 (September ’05). The list of participants is impressive, and many of the abstracts are very interesting and relevant to this post.

25. **Brett**
December 1, 2005

I happened to be in Seth Lloyd’s first ever quantum computing class, and in the last lecture, he presented some of his first ideas about quantum gravity. To me, they seemed hopelessly naive, built almost entirely on dimensional analysis (something that is not necessarily useful in nonperturbative GR, which was what he wanted to talk about). His ideas have gotten a little more sophisticated since then, but they are still quite unimpressive.

26. **Shantanu**
December 1, 2005

Peter (and others) what do you people think about Finkstein’s aproach to
quantum gravity based on plexors ? sometime back I remember reading
on usenet that his work (though heroic) has received very little attention.
I don’t recall it discussed even once here or on cosmicvariance

27. **Quantum_Ranger**
December 1, 2005

As good as the talk by Lloyd was, PI emergence of spacetime, I was not surprised that during this talk he admitted he was at the same “car-salesman” seminar as detailed here:

http://www.physicsforums.com/showthread.php?t=89240

in the early eighties!

This San Francisco seminar series of the early eighties, has really a lot of explinations for the emergence of String Theory and Anthropic Rationale.

28. **Matti Pitkanen**
December 2, 2005

Personally I cannot take seriously the discretization of space-time continuum. A much more elegant manner to introduce
dicreteness at the basic level consistent with basic ideas fo algebraic geometry emerges if one accepts all number fields, also p-adic number fields and their extensions as building blocks of physics.
This more or less forces the view that real and various p-adic physics (possibly representing physical correlates for intention and cognition) are obtained by analytical continuation from rational physics both at classical spacetime level and quantum level.

A possible application is the definition of fermionic determinant playing fundamental role in QFT:s and also in TGD and being plagued by divergences. The idea is that the restriction to rational eigenvalues of the appropriate Dirac operator could make the determinant finite. Entire hierarchy of determinants corresponding to various algebraic extensions of rationals emerges perhaps defining a hierarchy of finite Dirac determinants. This hierarchy would not be a calculational trick in TGD Universe but represent an actual physical hierarchy having natural identification in many-sheeted space-time of TGD Universe.

Matti Pitkanen

Sciencegate

December 2, 2005
Categories: Uncategorized

The editors at Seed magazine have started a new blog about science called sciencegate, which contains a wide variety of interesting material. One of the recent postings is called Strung Out on the Couch; it's by Joshua Roebke and not exactly complimentary about string theory. Here's his analogy for the current situation of string theory:

Think of it this way, a precocious little genius, who everyone has been touting would do great things in the world, finally grows up. Now imagine he's 30 years old, living at home having not accomplished much, and his mom keeps going on about how great he is and is still going to be. You'd probably just want to tell him to grow up and make something of all that potential instead of just talking about how he's going to get off the couch.

Before Lubos and others start the usual personal attack on any string theory critic as not knowing anything about the subject, it's worth pointing out that Roebke spent several years as a graduate student working on string cosmology before leaving academia.

Comments

1. **ks**
   December 2, 2005

   Sciencegate – sounds like a big scandal these days…

2. **A.**
   December 2, 2005

   Boy, who is this Joshua Roebke? Please present list of his publications, otherwise its difficult to judge from his repetitive and, I may add, boring piece whether he came to these conclusions himself after working hard many years in " string cosmology", or he is just this blog's big fan and read a couple of articles in new york times and such. As far as I checked arxiv.org produced no records of Joshua Roebke.

   I am in no way trying to "personally attack" you, but , in my opinion, unless you Joshua Roebke have actually wokred in the area on the level to produce something more or less deserving attention of the community, your comments have zero value. Who are you to comment on string theory?

   What is it with you people? Why everyone thinks starting his own blog is a good idea? Everyone is confident that his thoughts are the correct ones, "everyone else is stupid, I am smart"
   Dont start a blog, there are more than enough blogs, Peter doing his job quite well already. Just go home , or better go to your office, sit there from 8am to 8pm, and work on a problem. Maybe you will feel better.

3. **Ralph**
   December 2, 2005

   Apparently stringists can produce any number of possible universes from their theories, except it seems, the one we live in.

   I think it is time to formulate this as a scientific hypothesis "the anti-string hypothesis":

   Our universe is uniquely determined, by the inability of string theory to approximate it.

4. **woit**
   December 2, 2005

   "who is this Joshua Roebke?"

   Well, first of all "A.", who are you? Don't you think it's kind of disgraceful behavior to attack others from behind the cloak of anonymity?

   It's funny how predictable string theory partisans are these days. When Roebke wrote to tell me about the Seed blog and what he'd written for it, I wrote back to tell him I'd be happy to mention it here, but I could confidently predict his professional competence would be immediately attacked by Lubos and various others of his ilk. If you actually had any arguments to answer the ones Roebke is making, you'd be making those and not instead attacking his fitness to make those arguments. But go right ahead and behave like this, by doing so you and others are doing a great job of convincing ever increasing numbers of people that you are both intellectually and morally bankrupt.

   If he's in the mood I suppose Roebke can defend himself. I have met him and can tell you that his understanding of string theory appears to be consistent with what you would expect given his background, that of a good physics Ph.D. student who did research for a while in the subject, but ultimately decided to do something else. Your criterion that no one can criticize string theory unless they have written papers on the subject is that of a member of a cult convinced that only the opinions of true believers are of any value. By now lots of smart people have gone through the experience of spending years of their lives learning about string theory, finally reaching the point of realizing that the theory hasn't lived up to its promise, and that devoting the rest of one's life to working on it might not be such a great idea.

5. **secret milkshake**
   December 2, 2005

   A reason to post here anonymously: M O T L never forgives

6. **AJ**
   December 2, 2005

   A reason to post here anonymously: M O T L never forgives

   This is nonsense. Lubos and I have had loud public disagreements in the past; check out the sci.physics.research archives. I don't believe he bears me any ill will.

7. **garrett**
   December 2, 2005

   Ralph said:

"Our universe is uniquely determined by the inability of string theory to approximate it."

Classic! I loved this quip so much I had to applaud it with this comment. Thanks Ralph. It's as if the blind men are feeling everywhere the elephant isn't.

8. **Kool Kevin 8-)**
December 2, 2005

Loop Quantum Gravity is teh suck!!1! It is for the losers and the nerds.

Kool kids do teh strings. They might have lots of vacua but they also have lots of funding money and womens LOLOL. Also they use kool words liek heteroteric so they are the real daddy. And they have simetries in hodgy-podgy mirrors too!!

9. **Christine Dantas**
December 2, 2005

Dear A.:

You say:

What is it with you people? Why everyone thinks starting his own blog is a good idea? Everyone is confident that his thoughts are the correct ones, "everyone else is stupid, I am smart"
Dont start a blog, there are more than enough blogs, Peter doing his job quite well already. Just go home , or better go to your office, sit there from 8am to 8pm, and work on a problem. Maybe you will feel better.

Internet is just like that: you will find a broad spectrum of material, from garbage to excellent. The same is valid for blogs of course. I have started my own blog even though I know several of them, from garbage to excellent. At the same time, I never thought that "everyone else is stupid, I am smart". Maybe a lot of people think like that, but I am sure many do not.

I am not here to defend one approach to quantum gravity or another (much less the people involved), nor to say that I have a good blog. I am interested in all of the ideas towards a quantum theory of gravity as long as they seem sufficiently sound or look interesting enough to me. That is my blog. Yet, I do work from 8am to 8pm, or even more, and that involves several responsabilities as well as my own personal projects. So having a blog is not incompatible with doing hard work. This is valid for many people, as much as it is not valid for many others.

There is a little more I would like to comment on blogs. We are living in an epoch were ideas on quantum gravity are being constructed. It is a formidable problem. I see this as a very exciting period in the history of physics, and the various discussions I read daily in the (good) blogs are, to my opinion, very illustrative of our times. So these blogs are useful, and in some sense will serve as unique testimonies of our struggle towards quantum gravity.

Now, concerning the problem of who should be considered competent to "judge" a given approach. It seems clear to me that "judgement" is a strong word and an inappropriate one in science, except from the point of view that nature is always the final judge, since it is obviously nature what we want to probe, describe and ultimately explain.

Intelligent and constructive criticism is what is necessary in order to make progress. But this is hard to find. String theory in particular does not "help", and it is a good example in that sense. It requires years and years of very hard work in order to gain some knowledge of its basics. So, it seems inevitable you must be an expert to say something technically valuable considering the huge amount of mathematical material involved.

At the same time it is an obviously tricky or paradoxical situation, specially when one finds out that apparently there are no predictions of the "theory". So, considering this state of affairs, it seems natural that one needs not to be an expert to have the strong impression that it might be "not even wrong".

I must say I share this overall impression, although I am not saying I have a final position on this. I am not competent enough to offer a reasonable or more technical criticism on this matter, so I stop here to go for my daily hard work.

A final, perhaps also obvious thing: technique or knowledge is nothing without wisdom, but that is another story.

10. **Aaron**
December 3, 2005

I am in no way trying to "personally attack" you, but , in my opinion, unless you Joshua Roebke have actually wokred in the area on the level to produce something more or less deserving attention of the community, your comments have zero value. Who are you to comment on string theory?

Is this a parody? I hope so….

The difference between string theory and ID, BTW, is that string theorists dream of making positive statements. IDers never will.

11. **Adrian Heathcote**
December 3, 2005

I think the problems with String Theory go way back to the beginnings of Quantum Mechanics or Quantum Field Theory. There was a misconception as to what was needed to bring them together with General Relativity.

The fundamental difference between QM and GR is that, in the former, observables are operators (or at least are the results of operators acting on states to yield eigenstates). In GR they are pretty much just functions—functions of many variables, but still just functions, just as in classical physics. Getting a unification means, at the very least, having a mathematical formalism that represents the observables in one single way. But then, going backwards, it would be possible to recast QM in that same formalism, but just neglecting gravity. We would then have the revised mathematical structure for QM with the measurement problem—just what so many now want.

But I think this got fudged way back at the beginning. The Copenhagen interpretation effectively hid the interesting features of QM, and masked the important mathematical structures: the state space K of pure and non-pure states, the self-adjoint operators and their awkward formal properties. (Just how much of what is true of bounded operators really carries over to the unbounded case?)

These things got buried. And quantum field theory seemed to adopt a purely heuristic attitude to QM. It took the little bits that it liked—Heisenberg Uncertainty, transition probabilities, the Planck length—and downplayed the rest. You could read what particle physisit's wrote and think for all the world the universe was still basically Newtonian. And as time has gone on this cavalier attitude to QM seems only to have gotten worse.

String Theory is just the culmination of a long period of not getting the fundamental problem and sweeping the real difficulties under the rug. The renormalisation problem was the real clue that something had gone very wrong at a basic level. The calculational sucess of QED just allowed the sleepers to stay asleep.

Now we have a very elaborate mathematical structure that has no contact with reality. But given the history this was bound to happen. Physists started out fudging the conceptual issues and caring only about the calculations—the irony is that we have no more calculations to care about, but we have a whole lot of bad concepts.

Just my 2 cents.

12. **Adrian Heathcote**
December 3, 2005

Sorry, that should have said:

”We would then have the revised mathematical structure for QM without the measurement problem—just what so many now want.”

13. **Christine Dantas**
December 3, 2005

Anonymous Says:

Is this kind of crackpotism, which does make tested and testable predictions by the way, would be easy to write off as crazy.

I think there is a perceptible difference between “crackpotism” and “theory under construction”. Baez index list on the former is a good reference to unmask potential crackpots; scientific method on the latter is usually enough, and the result is progress or refutation.

A “grey zone” however is always possible and that is where the danger lies. This is specially symptomatic when theorizing about nature at regimes where experimentation is very difficult or observation relatively inaccessible. So you have a “theory under construction” that cannot be adequately or promptly investigated under the conventional rules of the scientific method. One may even end up with a mathematically consistent formulation, but in what sense it is satisfying? One may think it is “beautiful”. One may think is the only proposal for a given problem. So this is the grey zone because it is all under the umbrella of personal taste.

I had the curious experience a few years ago of giving a talk on the basics of braneworld scenarios to a mixed audience of astrophysicists ranging from specialists on gravitational waves, CMBR, high energy, extragalactic, galactic and stellar astrophysics. This is a much harder audience than a general public audience, because these people are well versed in the scientific method. They deal with very, very hard problems, and even problems that may need new physics. Although I did my best and they enjoyed the talk, most of them thought there was something quite disturbing on the overall idea. I am sure this is symptomatic of the grey zone.

As I see it, the only obvious way out is to work as hard as possible on the problem of identifying possible observable effects of a given quantum gravity proposal as it is being constructed and look for these effects, otherwise one is hopelessly stuck in the grey zone, sometimes without even being aware of it.

14. **Arun**
December 3, 2005

In reply to Michael at 7:14 PM, just what constitutes a research program in ID? There would be scientists receptive to ID if there existed any sch thing. ID is intellectually sterile. String theory is far from sterile, its problem is that its productions don’t yet make contact with the real world.

15. **Arun**
December 3, 2005

Peter, I think you did Joshua Roebke a disservice by not including the lines just previous to what you quoted.

> Don’t get me wrong. I actually love string theory and think it’s light years ahead of other theories. I follow the papers, and excitedly read them in earnest. I just think perspective and critique are necessary for progress.

16. **Quantoken**
December 3, 2005

Joshua Roebke said:
“I actually love string theory and think it’s light years ahead of other theories.”
Oh sure, how could you not be light years ahead of others, if you started several decades before other theories were first proposed, and you have several orders of magnitude more researchers working on the fields, and many times more public funding invested in supporting you. Surely you are light years ahead of other approaches. But it doesn’t make you look good. What matters is are you on the right direction? **You are on the wrong approach**. So the further light years you go forward in your dead end, the further away you are from the ultimate answer!!! Look at the 10^500 landscapes figure. You may be light years ahead but you have another 10^500 light years to cross in your front. Mean while the theory that is on the correct approach doesn’t need to go a light year: It only needs to reach the moon.

17. **ks**
December 3, 2005

I think there is a perceptible difference between “crackpotism” and “theory under construction”.

Unfortunately cargo-kult-science according to Feynman does anything right, is a true slave of scientific ritual and jargon but is nevertheless not science. This reminds me somehow to the old catholic problem off finding a true saint or a real representative of god obtaining all the priviliges of god including the permission to kill. Both science and religion are hunted by their own double or shadow.

It is interesting that you find the difference “perceptible”. Ususally strong differences do not follow from something as subjective as perception in particular the perception of strange social behaviour but from a certain axiomatic or precise and objective diagnosis. Misusing authority is therefore not so much an accident in evaluating competing researchers work but has to be expected. On the other hand the theory of social systems provides a self-referential answer presupposing a running science business: viable is what gives rise to connectivity. As an addition of my own I would guess that connectivity is not only a conservative mechanism of affiliation in modern societies but can be understood more dynamically like trading commodities on a stock-market.

18. **Juan R.**
December 3, 2005

A said,

> I am in no way trying to “personally attack” you, but , in my opinion, unless you Joshua Roebke have actually wokred in the area on the level to produce something more or less deserving attention of the community, your comments have zero value. Who are you to comment on string theory?

If this argument is correct then string theorists can only talk about string theory since they have seriously studied nothing of real word. Please using your ‘argument’ explain to Schwartz he cannot talk about BH-paradox because has provided none work on the topic, please explain to Witten that he cannot talk of chemistry since has published not a single paper on the topic, please explain to Brian Greene that he cannot write on measurement problem of QM since has published a single paper, please explain to Lubos that cannot write reviews of books on environment science since he never published a paper on the field,

etc,

etc,

etc,

etc.

Juan R.

Center for CANONICAL |SCIENCE)

19. **Juan R.**
December 3, 2005

If it is of interest, i have posted directly some basic comments on the Joshua Roebke post highlighted by Peter Woit

I basically agree that string theory is a disaster.

Juan R.

Center for CANONICAL |SCIENCE)

20. **woit**
December 3, 2005

Please stop with the attempts to turn this into a forum for discussion of religion, intelligent design, the anthropic principle, your favorite ideas about physics, etc., etc., none of which have anything to do with the topic of the posting. I've just deleted about half of the comments on this thread and will keep doing so if I feel it is necessary to keep this comment section from turning into something completely depressing and unreadable.

21. **Christine Dantas**
December 3, 2005

ks Says:

It is interesting that you find the difference "perceptible". Ususally strong differences do not follow from something as subjective as perception in particular the perception of strange social behaviour but from a certain axiomatic or precise and objective diagnosis.

Perhaps "perceptible" was not the right word for what I meant to say. In any case, if I understand you correctly, I agree that it is unfortunate that researchers are not usually evaluated according to a well defined, precise criteria, but mostly from his/her "social resources" ("colleagueship") — or lack of them. Be a colleague of an "authority" and you have good career chances, whatever the value of your research. I have been through this before. Sorry if I misunderstood you on this matter. Also, I should be more careful with the words I use. In any case, perhaps this is becoming off-topic, and I fear Dr. Woit will not like it.

22. **God**
December 3, 2005

'Think of it this way, a precocious little genius, who everyone has been touting would do great things in the world, finally grows up. Now imagine he's 30 years old, living at home having not accomplished much, and his mom keeps going on about how great he is and is still going to be. You'd probably just want to tell him to grow up and make something of all that potential instead of just talking about how he's going to get off the couch.'

Peter, explain how someone can make a cheerful comment 😉

23. **A. nonymous**
December 3, 2005

I think Joshua's article takes approximately the right tone towards string theory. It is not yet certain that srting theory will never be able to make a prediction, although there are some signs that this might be the case. The real problems for physics are the position that string theory has taken in the common view of theoretical physics as the only option available and the fact that young theorists don't really have any career options apart from string theory or condensed matter, while faculty positions are increasingly being occupied by string theorists at the expense of other legitimate research programs.

There is, of course, a separate problem with the string theorists themselves, namely that they are arrogant and narcissistic, to the degree that they dismiss all non-string theorists as idiots and all non-string theory physics as irrelevant. String theory shows practically all the signs of groupthink, namely:
1. Illusion of invulnerability
2. Unquestioned belief in the inherent morality of the group
3. Collective rationalization of group's decisions
4. Shared stereotypes of outgroup, particularly opponents
5. Self-censorship; members withhold criticisms
6. Illusion of unanimity (see false consensus effect)
7. Direct pressure on dissenters to conform
8. Self-appointed "mindguards" protect the group from negative information

Lubos would no doubt be pleased to know that he is a "mindguard".

On the subject of intelligent design, it might be instructive to see how A's comment looks from that point of view:

I am in no way trying to "personally attack" you, but , in my opinion, unless you have actually wokred [sic] *in the area on the level to produce something more or less deserving attention of the community, your comments have zero value. Who are you to comment on intelligent design?*

24. **Pseudo-string-fan**
December 3, 2005

Hi,

What is ID that some of you mentioned?

Thanks

Pseudo

25. **woit**
December 3, 2005

ID=Intelligent Design, but please, don't take this as an excuse to start discussing the topic here.

26. **D R Lunsford**
December 4, 2005

Intelligence Deficit, which by definition is

ID = (100 – IQ)/IQ

A negative ID is a sure sign that IQ

27. **Jose**
December 4, 2005

String theory predicts the BH entropy formula, string theory predicts a 10-dimensional space-time, string theory predicts how particles interact at [tex]10^19[/tex] GeV. STRING THEORY MAKES PREDICTIONS. The difficult task is going from [tex]10^19[/tex] GeV to the actual energy that can be reached in experiments.

In any case, if string theory is a wrong physical theory, string theory has given a great amount of mathematical results, and it is a respectable endeavour of the human mind.

28. **woit**
December 4, 2005

Jose,

1. It would take too long to go into the exact status of black hole entropy calculations in string theory, but there is no solid "prediction" for physical black holes, at best an argument that you get the semi-classical limit that you expect for any thing that deserves to be called a quantum version of gravity.

2. String theory doesn't predict 10d spacetime (have you heard of M-theory?). And if it did, there's the problem that we only see four dimensions.

3. String theory doesn't predict how particles interact at the Planck scale (have you heard about M-theory?).

STRING THEORY MAKES NO PREDICTIONS. NONE WHATSOEVER, NADA, ZIP.

29. **Juan R.**
December 4, 2005

Jose Said,

> String theory predicts the BH entropy formula, string theory predicts a 10-dimensional space-time, string theory predicts how particles interact at 10^19 GeV. STRING THEORY MAKES PREDICTIONS. The difficult task is going from 10^19 GeV to the actual energy that can be reached in experiments.

String theory does **not predict** BH formula because i) compactification is done *by hand*, ii) string theory 'BH' are highly idealized objects are not general relativity BH, iii) etc.

String theory does **not predict** a 10D spacetime. The 10D spacetime is introduced *by hand*, with a strange 'compactification' of the rest 16D. Not forget that supersimmetry is not correct at low energies. Not forget that all 10D are equivalent and for comparison with observed 4D universe, 6D may be differentiated by hand. Not forget that a new dimension is hidden in the perturbative regime. Not forget that still nobody know if 11D is the correct dimensionality for M-theory, etc.

The 'prediction' (of course there is **not prediction** because topology and geometry of background spacetime and others features are choosed by hand) of interactions at 10^19 GeV are just scattering amplitudes with the wrong large distance limit. There is not dynamics, there is not adequate classical limits (GR causality structure is broken when taken classical limit of gravitons), etc.

Etc,

Etc,

> In any case, if string theory is a wrong physical theory, string theory has given a great amount of mathematical results, and it is a respectable endeavour of the human mind.

Many other theories give mathematical results. Moreover, the real impact of **string theory** on math is easily sumarized:

epsilon, when epsilon –> 0.

I, at least, am able to distinguish between math and physics. For example between NC geometry (math) and M(atrix) theory (physics).

Juan R.

Center for CANONICAL |SCIENCE)

30. **God**
December 4, 2005

Woit, religion makes more predictions than string theory!

Religion predicts a day of judgement! Religion predicts sinners will go to hell. This is perfectly testable: die and you'll see… Religion predicts all kinds of things that you can test if you are desperate enough…

String theory merely predicts 10^500 universes, a landscape of universes, that there could be any number of branes, and so forth.

I think you've got to start saying that string theory is not 'as bad as religion', but rather that religion is a lot more scientific than strings. Be honest!

31. **Wolfgang**
December 4, 2005

Peter,

> STRING THEORY MAKES NO PREDICTIONS.

a standard argument (I guess E. Witten is the author) is that string theory predicts supersymmetry [*]. Of course, we do not know at which energy scale we may see it…
My question to you is this:
Assume that the LHC (or ILC) will provide strong empirical evidence, e.g. for a MSSM or NMSSM model.
What would be your reaction?

[*] Lubos became quite angry when I stated once that string theory 'assumes' supersymmetry in order to be consistent; It is the same IMHO, string theory/M-theory requires supersymmetry at some energy level to be consistent, at least at our current understanding.

32. **anon**
December 4, 2005

Wolfgang,

Where you say 'string theory predicts supersymmetry' you are really saying that Witten and others developed string theory so that it would do this. It is an ad hoc 'prediction', like epicycles.

Lubos gets angry all the time, you should know. Being a string theorist, he has a good excuse, because the news keeps getting worse.

If and when empirical evidence is found for strings, that will be the time for it to be taken seriously. The whole problem is that it is being taken seriously by the world now, when there is no evidence.

33. **woit**
December 4, 2005

Wolfgang,

It doesn't really make sense to "predict supersymmetry" if you then say it may be broken at arbitrarily high scale and be indistinguishable from no supersymmetry. The real question is whether it is broken at LHC energies and can thus do one of the main things it is supposed to, solve the hierarchy problem. My guess is that the LHC won't see supersymmetry and I'll be surprised if it does, especially if it sees some random MSSM spectrum. If this does happen, that's certainly some encouragement for the ideas that have led people to work on string theory, although far from solid evidence for string theory.

34. **Wolfgang**
December 4, 2005

anon,

I am not very interesting in the syntax or semantics of the word 'predict'.
Also I do not care if Lubos is angry (but he is sometimes entertaining when he is).
But I would like to hear Peter's opinion, since he is a strong critic of string theory (like many here), but seems to really know QFT and particle physics. And my question is simple and straightforward:
How would you react if the LHC (or ILC) provides strong evidence for supersymmetry?

I would see this as strong evidence that superstring/M- theory is at least on the right track.

35. **Wolfgang**
December 4, 2005

Peter,

ooops, seems that posts have crossed here.
Thank you for the response.
When you say

> If this does happen, that's certainly some encouragement for the ideas that have led people to work on string theory

I interpret this as you saying "with strong evidence for supersymmetry I would eventually change my opinion on superstring theory" ?

36. **woit**
December 4, 2005

Wolfgang,

Our comments crossed. I'd agree that supersymmetry at the LHC would be some sort of evidence that superstring/M-theory might be on the right track, although I wouldn't go so far as to call it "strong evidence".

It would also depend a lot on exactly what was seen and what sort of hints this gave about the mechanism for supersymmetry breaking.

37. **Chris W.**

December 5, 2005

> I am not very interest[ed] in the syntax or semantics of the word 'predict'.

That's unfortunate. I think the semantics of this word is badly in need of review under the current circumstances.

It used to mean something very much like this:

An experimentalist finds a way to reproduce a particular effect, which they characterize with one or more measurements. That is, they identify those features of the experimental situation that must be controlled to bring about the same effect (within certain error margins) in multiple runs of the experiment.

The experimentalist then says to a theoretician: "I can reproduce this effect, but I can't explain it. Can you tell me why the effect occurs in this situation, ie, given the features I have identified, and also calculate the results I have measured using the features of the experimental setup as inputs to the calculation?"

Now, I would assert that the theoretician's job is to do this, and do it with a *minimum* of auxiliary assumptions, ie, in addition to the theory (the explanation) being tested. Any auxiliary assumptions that are made should be *independently* verifiable by the experimentalist, so they cannot be freely used to fit the prediction to the result.

It appears to me that string theory in its current form must make extensive use of auxiliary assumptions that can't be independently tested in order to make anything like a definite prediction of any given physical effect. Of course one could say, "well, I'm sorry if you don't like it that world is this complicated, but this appears to follow from the basic principles of the theory. The world can be as complicated as it likes; we can't dictate the structure of physical reality for our methodological and epistemological convenience."

I would assert that in fact we *must* "dictate the structure of physical reality for our methodological and epistemological convenience". In doing so we may find that those dictates are repeatedly violated, but nonetheless our theories must be as restrictive and unambiguous in their predictions as we can make them. If one thinks the world can be arbitrarily complicated then one might as well abandon any attempt at testable explanation, and simply accept the *prima facie* complexity of the observed world, along with whatever interpretive mythology one finds most comfortable.

It is not noted often enough that the deep symmetries of the physical world, and moreover, the decomposability of physical systems into largely independent parts, have great epistemologic as well as aethestic significance.* In a world like that of the astrologer, in which physical influences of arbitrary form and magnitude can be propagated over arbitrary distances at arbitrary speeds, one can hardly imagine any effective dialogue between experimentalists and theoreticians, or even among observers trying to reach intersubjective agreement about what they perceive. We have not been merely discovering what the physical world is like for the last several centuries; we have also been gradually discovering how it is possible for us to know the world at all. The anthropic principle offers an answer to this question that is perhaps suggestive but ultimately scientifically vacuous. There is yet reason to believe we can do better.

38. **Christine Dantas**
December 5, 2005

According to Amelino-Camelia et al. (gr-qc/0501053), "string theory may predict many new low-energy effects, but it can also be easily tuned to avoid all of them."

I have just highlighted their paper today in my blog page (see their very nice fig.5).

39. **Colin Rosenthal**
December 5, 2005

That is a somewhat unusual usage of the word "predict".

40. **anon**
December 5, 2005

"string theory may predict many new low-energy effects, but it can also be easily tuned to avoid all of them"

What beautiful flexibility! Those who love string theory will be enchanted. Lubos will be delighted. Why bother testing string theory when it is so vague any result is consistent with it?

41. **Christine Dantas**
December 5, 2005

Amelino-Camelia et al. also write (page 11 of that same paper):

"The low-energy physics of String theory, as presently understood, could be very rich, with the presence of new fields and the possibility of a variety of new effects. This may provide the basis for a large phenomenological effort, even though one should keep in mind that **the new effects are not genuine 'predictions' of String theory**: String theory could make room for these effects but it could equally well suppress them all. **It appears at present not possible to falsify String theory on the basis of low-energy phenomenology** , but it would nonetheless be very exciting if any of the new effects that String theory may host was actually found."

So it is clear enough that the motivation that Amelino-Camelia et al. focuses on is the search for new low-energy physical effects per se. They clearly state that string theory does not actually predict them, nor can be falsified by them, it just can accommodate them if one wishes.

I really look forward to a technical, non-passionate (if possible) opinion of string theorists and non-string theorists on this matter (specially on Amelino-Camelia et al.´s paper). If considered off-topic, please apologize and feel free to redirect to my blog page.

42. **Juan R.**
December 5, 2005

Wolfgang Said

> Lubos became quite angry when I stated once that string theory 'assumes' supersymmetry in order to be consistent.

Since string theorists like to claim that string theory does predictions, a brief historical review of 'predictions' would be interesting…

String theory began from his **first failure** on explaining strong force on a 4D world (and substituted by QCD), then predicted **wrong** Lorentz symmetry when extended and for correcting this **wrong prediction** 22 additional dimensions were introduced by hand.

Then, **wrong** dimensionality was predicted and for correcting this **wrong prediction** KK was introduced by hand.

Then, tachionic modes were obtained and this **violated again** experimental data, and for correcting this novel **wrong prediction** supersimmetry was introduced by hand.

Of course, string theory continued doing **wrong predictions** and evolutioned until the modern M-theory nobody know that is!

However, supersimmetry is a fascinating example of how string theory is wrong.

String-superstring theory predicts either

superimmetric non-tachionic world

or

non-superimmetric tachionic world.

Curiously our observed universe (at least at low energies) is

both non-superimmetric and non-tachionic.

Therein the extended criticism in relevant literature that string theory predicts **nothing of this world**.

If one follows scientific method (string theorists do not) then one may develop a first theory for:

– 4D Standard gravity and

– Compatible with non-superimmetric and non-tachionic known Standard Model.

At future HLC-data is supersimmetry found?

No problem, to generalize above theory adding supersimmetry to HIGH energies (NOT at all energies).

At future 'cosmic-acellerator' are hidden dimensions found?

No problem, to generalize above theory adding more dimensions to HIGH energies (NOT at all energies).

The 40 years failure of string theory is because string theorists want say how universe may be.

Science is about we can observe, NOT about we want observe and complete ignorance we really observe.

Juan R.

Center for CANONICAL |SCIENCE)

43. **Alejandro Rivero**
December 5, 2005

Juan, we have actually found [supersymmetry 🙂] Perhaps Lubos is right after all.

44. **Juan R.**
December 5, 2005

Alejandro Rivero,

Since i said is crystal clear, it appears that after all Lubos is again wrong 🙂

If anyone want help to Lubos in his research, then may try to find a well-defined mathematical limit where superstring actions turn, at least, **non-supersimmetric**, all looks 4D at large distances, and masses are non zero.

Juan R.

Center for CANONICAL |SCIENCE)

45. **dan**
December 5, 2005

is m-theory, are strings supposed to make up quarks (and electrons) directly, or do they make up preons which then make up quarks?

46. **dan**
December 5, 2005

lubos has claimed m/string theory predicts BH entropy, but LQG does not. apparently john baez claims LQG does predict BH entropy. what is the status of BH entropy in LQG (and BI-like CDT) gravity?

47. **Juan R.**
December 5, 2005

dan,

see Lee Smolin said. Question 4 of page 60 of arXiv:hep-th/0303185 v2 11 Apr 2003.

BH entropy is 'solved' on LQG, whereas only partial results and many open questions remain on the string side.

I do not follow this topic in detail now (i discovered that string theory is a waste of time) , but some few years ago, string theorists were working with certain idealized states that they 'believe' that *could* explain GR black holes.

Yes! almost all in string theory are beliefs and conjetures…

See also section 6.3

> The results in string theory do not concern, precisely, black holes, as they are found in a limit in which the gravitational constant is turned off.

Since LQG arises directly from quantization of GR field equations, the BH studied are more close to GR (Schwarzschild) BH.

Juan R.

Center for CANONICAL |SCIENCE)

48. **Chris Oakley**
December 5, 2005

I would like to draw everyone's attention to the prefix "pre" in the word "predict". It means "before". In other words, it describes a situation where you guess it is going to happen before it actually happens. Examples of this are:

1. Maxwell guessed that it would be possible to observe a displacement current before anyone actually did. It was therefore a "prediction".

2. On the basis of observed patterns of quantum numbers for baryons, Gell Mann "predicted" a baryon of strangeness -3 and mass of about 1680 MeV. In 1964, a particle fitting this description was observed. A fine prediction.

One can, of course, predict things which turn out to be wrong. But there are also things which never were predictions in the first place. For example:

1. Special relativity does not "predict" four dimensions. Four dimensions are assumed at the outset.

2. Superstring theory does not "predict" gravity. General relativity – a tested theory of gravity – is one of the inputs (although it is not clear that one can get it back).

3. Superstring theory does not "predict" supersymmetry. Supersymmetry is assumed at the outset.

I am happy to answer any further questions that anyone might have about the usage of this word.

49. **Jose**
December 5, 2005

"Superstring theory does not "predict" gravity. General relativity – a tested theory of gravity – is one of the inputs (although it is not clear that one can get it back)."

This is not true Chris if we suppose that particles are strings then we are led to the existence of massless spin 2 particles, that is gravitons, without more assumptions.

50. **A. nonymous**
December 5, 2005

Jose is more or less right, but one could also say that there is a selection bias; mathematical formalisms in which Einstein's equations appear are going to be selected and studied more intensely than those which don't 'predict gravity'. Thus, the fact that the currently most intensively studied theory has the field equations inside it is not particularly surprising.

The question at hand – whether string theory has earned the attention it has received – is to be decided by asking what predictions it has made which were not simply the reasons for paying attention to it in the first place. The result seems to be that string theory has produced dualities, AdS/CFT and so on, which are interesting, but which don't actually qualify as experimental predictions.

It seems likely to me that in the distant future, string theory will occupy a position in relation to gauge theories similar to that presently occupied by group theory in relation to quantum mechanics. That is, it will be regarded as an invaluable tool, although it is really a branch of pure mathematics which on its own makes no statement about nature.

51. **Aaron Bergman**
December 6, 2005

what is the status of BH entropy in LQG (and BI-like CDT) gravity?

There is some debate on this point. There has been a calculation done that obtains a number proportional to the area with an unfixed constant of proportionality. I don't think there's a real sense that this calculation was done in LQG; it's done in a sort-of hybrid formalism.

From my reading of the calculation, I think that the proportionality to the area is something assumed by the particular formalism used. In other words, I don't think it is true to say that black hole entropy has been derived to be proportional to the area in LQG. Others disagree.

I discussed this a bit with Lee Smolin here — scroll down a bit.

I discussed this a bit with John Baez here.

I know of no discussion of black hole entropy in CDT.

52. **Chris Oakley**
December 6, 2005

I do not understand why, when talking about String Theory, I always have to choose the optimistic version, whereas when talking about most other scientific theories, I am allowed to tell it like it is.

If we have to select, among the various candidate String theories, the ones that contain Einstein's equations, then Einstein's equations are not a "prediction" of String theory.

Please explain why it is more complicated than this.

Although I find it hard to believe that ST will ever be a useful tool in doing anything other than duping funding agencies, I do at least agree about group theory – indeed, if you can be bothered to look through the archives of this blog, you will find me advocating teaching the theory of continuous groups to physics undergraduates. Also, in my own attempts to understand QFT – e.g. here, I try to give group theory as central a role as I can.

53. **Thomas Larsson**
December 6, 2005

Wolfgang,

Assume that the LHC (or ILC) will provide strong empirical evidence, e.g. for a MSSM or NMSSM model.
What would be your reaction?

I think one should formulate the answer symmetrically.

Low-energy SUSY would not prove string theory, and absense of low-energy SUSY would not disprove it.

Low-energy SUSY would be a strong argument in favor of string theory, and absense of low-energy SUSY would be a strong argument against it.

Do you agree?

I have an analogous question about LQG, which somebody (Christine perhaps) may be able to answer. LQG apparently predicts (or may predict) Lorentz violation. If this is observed in Auger or GLAST (or whatever the relevant experiments are called), it would be a great triumph for LQG. But is it a real, falsifiable prediction, in the sense that no Lorentz violation at Auger or GLAST would disprove LQG?

54. **Christine Dantas**
December 6, 2005

Thomas Larsson Says:

I have an analogous question about LQG, which somebody (Christine perhaps) may be able to answer. LQG apparently predicts (or may predict) Lorentz violation. If this is observed in Auger or GLAST (or whatever the relevant experiments are called), it would be a great triumph for LQG. But is it a real, falsifiable prediction, in the sense that no Lorentz violation at Auger or GLAST would disprove LQG?

According to Lee Smolin, there are two scenarios that specify how lorentz invariance is treated in LQG, see: "An invitation to Loop Quantum Gravity" (hep-th/0408048; section 5, "The near term experimental situation"). From a reading of this material, it is not really clear that LQG offers clean-cut, distinguishable low-energy physical predictions at the moment, exactly because of these 2 possible scenarios. But that is my understanding.

I think Smolin offers a somewhat optimistic view, which is not something particularly bad, I even share it. That is acceptable if you are not stuck by it and are able to contrast it with, e.g., Amelino-Camelia et al.'s (gr-qc/0501053) more direct words expressing that the main difficulty is:

"*the fact that **the techniques for obtaining the classical limit of the theory have not yet been developed**. Since our phenomenology will usually be structured as a search of corrections to the classical effects, this is a very serious issue. However, several authors [16, 17, 18, 19], guided by the intuition from working with some candidate quasiclassical states, made analyses that led to the expectation that Lorentz symmetry should be broken in Loop Quantum Gravity, and as a result the Maxwell and Dirac equations should include extra terms of higher derivatives. But clearly these violations from Lorentz symmetry still cannot be viewed as a "prediction" of Loop Quantum Gravity because of the heuristic nature of the underlying arguments, and indeed there are some authors (see, e.g. Ref. [20]) who have presented arguments in favour of exact Lorentz symmetry for Loop Quantum Gravity."*

Two more papers I find relevant are:

* "Falsifiable predictions from semiclassical quantum gravity" (by Lee Smolin, hep-th/0501091).

* "On Loop Quantum Gravity Phenomenology and the Issue of Lorentz Invariance (by Martin Bojowald et al., gr-qc/0411101).

I have a somewhat biased view that LQG is — or will eventually be — a falsifiable approach/theory, and the current problems of LQG concerning the issue of predictability are of a very different nature as those already pointed out in the case of string theory. That is my biased view but I am attempting to broaden my understanding of this matter of course.

The overall conclusion I have at the moment is that **LQG phenomenology is a very, very incipient one, but a possible one**. I have a much more pessimistic view concerning ST, since, as already stated, *"string theory may predict many new low-energy effects, but it can also be easily tuned to avoid all of them"*. I do not think that is the case for LQG, but I may be in error of course.

55. **Juan R.**
December 6, 2005

Ed Witten often has said in open forums and media that string theory pre-dicts gravity.

Of course, this is a nonsense and then Witten openly admits that that really with that phrase is that he means a **post-diction** because

> the experiment was done before the theory.

Naturally, an 'uniportant' detail for stringys!

Source: Interview with John Horgan on the in-famous book *The End of science*.

Note1: i obtained the Spanish version (pag 97), perhaps the english quote is not exactly equal.

Note2: perhaps i would remember readers as Jose that only 'gravity' contained in superstring theory is a perturbative spin-2 (masless) graviton over a flat classical spacetime. String states are masless and, therefore, even a basic description of e-e gravitational scattering is absent.

Of course, a spin-2 perturbation over a flat background is NOT general relativity.

How many times, will string theory be mistified until it was abandoned as a complete failure?

Juan R.

Center for CANONICAL |SCIENCE)

56. **Juan R.**
December 6, 2005

Christine Dantas, it appears that LQG already offers expressions for alpha coefficient and some bounds for beta, but I have not clear still is at what extension the proporcionality constant (L_p) is fixed on LQG.

Moreover, not forget that still nobody has proven that LQG was the correct classical limit. Why would we speculate about future AUGER, GLAST, etc. predictions being unable to explain current data (classical gravity)?

Even if tomorrow LQG offers corrects values for possible Lorentz violations, will continue to be an incorrect theory if anybody does not prove that when h–> 0, LQG –> GR or similar.

The same about 'stringys', very anxious waiting indications of supersimmetry at HLC without worry that string theory has not the correct low energy limit?

Is this science?

Juan R.

Center for CANONICAL |SCIENCE)

57. **Christine Dantas**
December 6, 2005

Juan R. Says:

> I have not clear still is at what extension the proporcionality constant (L_p) is fixed on LQG.

I would like to know that too. I have a lot of information yet to process. Could someone answer that?

> Even if tomorrow LQG offers corrects values for possible Lorentz violations, will continue to be an incorrect theory if anybody does not prove that when h–> 0, LQG –> GR or similar.

Undoubtedly yes. I have never thought otherwise.

> Is this science?

According to Frank Wolfs' text,

> The scientific method is the process by which scientists, collectively and over time, endeavor to construct an accurate (that is, reliable, consistent and non-arbitrary) representation of the world.

So the answer would be yes if we believe we are in the process of construction without forgetting the 3 magic words: reliable, consistent and non-arbitrary. However, of course this is not all. Let us review some concepts. He continues:

> In physics and other science disciplines, the words "hypothesis," "model," "theory" and "law" have different connotations in relation to the stage of acceptance or knowledge about a group of phenomena.
>
> *1. An **hypothesis** is a limited statement regarding cause and effect in specific situations; it also refers to **our state of knowledge before experimental work has been performed and perhaps even before new phenomena have been predicted.***
>
> *2. The word **model** is reserved for situations when it is known that the hypothesis has at least limited validity.*
>
> *3. A **scientific theory or law** represents an hypothesis, or a group of related hypotheses, which has been confirmed through repeated experimental tests.*

So it is my opinion that ST and LQG are both hypotheses for sure (at least in the sense highlighted above). I am optimistic that LQG can evolve towards item 3, because item 2 seems within its reach, if not already. I am not sure about string theory and other approaches, but one obvious thing that none of them can be seen as theories.

Christine

58. **Aaron Bergman**
December 6, 2005

If we have to select, among the various candidate String theories, the ones that contain Einstein's equations, then Einstein's equations are not a "prediction" of String theory.

We don't have to do so. It's very hard to write down a string theory that doesn't contain gravity. Sometimes it's even annoying. Much of the excitement about the twistor string, for example, was tempered when it turned out that it was computing amplitudes in conformal supergravity rather than ordinary super Yang-Mills.

59. **Juan R.**
December 8, 2005

Aaron Bergman Said;

> We don't have to do so. It's very hard to write down a string theory that doesn't contain gravity. Sometimes it's even annoying. Much of the excitement about the twistor string, for example, was tempered when it turned out that it was computing amplitudes in conformal supergravity rather than ordinary super Yang-Mills.

Any inexpert reader would be convinced that string theory works except by a 'small' detail you forgot.

As explained above on reply to Jose, the "gravity" contained in string 'theory' is not the gravity contained on general relativity not Newtonian gravity

Juan R.

Center for CANONICAL |SCIENCE)

60. **Juan R.**
December 8, 2005

Christine Dantas,

In science a hypotesis is a premise can be falsified via laboratory. The hypothesis, beyond any distance reachable by present or future telescopes stars are pink cubes is not scientific since is not testable. It would a metaphysical hypotesis.

Therefore string theory is not a scientific hypotesis since it is not (in rigor) testable; in fact, some leading string theorists openly call it a kind of church (i can obtain this cite).

A priori, i think that LQG may be a scientific hypotesis. However, my emphasis on the classical limit of LQG was not superfitial.

The failure for finding the limit is not due to thecnical difficulties. I mean that the classical limit obtained is not compatible with correct classical limit and this is the basis of the general criticism to LQG: it has not the correct classical limit.

Therefore, as one scientist, i consider 'stupid' the construction of detailed models and predictions for that "we will observe at HLC" when TODAY we cannot explain already know and explained by general relativity, for example Mercury perihelion or binary pulsar data.

That is, first to develop a candidate to quantum gravity with the correct classical limit and then **only then** predict it will be observed at HLC. Do not follow this basic scientific methodology is, in my opinion, a waste of time.

Juan R.

Center for CANONICAL |SCIENCE)

61. **Christine Dantas**
December 8, 2005

Juan R. Says:

> A priori, i think that LQG may be a scientific hypotesis. However, my emphasis on the classical limit of LQG was not superfitial.

Yes, I agree with you. I do not consider it (the classical limit of LQG) a minor problem, on the contrary. It is a very serious one of course.

Best wishes
Christine

LatexRender

December 3, 2005
Categories: Uncategorized

I've just added LatexRender support to this weblog, using Steve Mayer's WordPress plugin. In principle you should now be able to add formulas in TeX in the comments by putting the TeX in between and (without the space).

Here's an example of the output:

\displaystyle{\int_{-\infty}^{\infty} e^{-x^2}dx=\sqrt{\pi}}

I fear this will undoubtedly require some further debugging, but no time for that right now.

Update: This is now long out of date. I've replaced LatexRender with MathJax support. The opening and closing delimiters for tex math are now '\$' and for displayed math are "\$\$".

Comments

1. **woit**
   December 3, 2005

   Let's see if this works in the comments:

   [tex]\displaystyle{\int_{-\infty}^{\infty} e^{-x^2}dx=\sqrt{\pi}}[/tex]

2. **D R Lunsford**
   December 3, 2005

   [tex]
   \begin{equation}
   R_{\underline{mn}}=\left(\frac{2R}{W}\right)T_{mn}-\left(\frac{1}{2W}\right)(D_{m}D_{n}+D_{n}D_{m})W\end{equation}

   \[
   \frac{1}{\surd}\partial_{n}\left(\surd RF^{mn}\right)=\frac{5}{4}\: D^{m}W\]
   [/tex]

   Did it work?

   -drl

3. **Daniel Doro Ferrante**
   December 3, 2005

   I know the post is in pt_br, but these "TeX" plugins for WP are really cool: Testing MathML.

4. **antonio**
   December 3, 2005

   [tex]e^{e^{e^{e^{e^{e^{e^{e^{x}}}}}}}}[/tex]

5. **Aaron**
   December 3, 2005

   Hmmm. Without the spaces, I bet.

   [tex]\mathbb{R}Hom_\mathcal{D}(\mathcal{E}_i,\mathcal{E_j}[k]) = 0 \quad \forall k \mbox{ and } i>j[/tex]

6. **Anonymous Dude**
   December 3, 2005

   Copy catting…

   [tex]\mathbb{R}Hom_\mathcal{D}(\mathcal{E}_i,\mathcal{E_j}[k]) = 0 \quad \forall k \mbox{ and } i>j[/tex]

   Neat! (assuming it worked)

7. **D R Lunsford**
   December 4, 2005

   Just an egregious editorial comment,

   Markup languages are clunky and unreliable. This is a much better idea.

   -drl

8. **D R Lunsford**
   December 4, 2005

   Quick! What's the derivative of [tex]e^{e^{e^{e^{e^{e^{e^{e^{x}}}}}}}}[/tex]?

-[tex]\delta\rho\lambda[/tex]

9. **Thomas Larsson**
   December 4, 2005

   Maybe formulas must fit into a single line. Let’s see what happens if we break up the offending input.
   [tex]{[}L_\mu(m), L_\nu(n){]} = [/tex]
   [tex]n_\mu L_\nu(m+n) – m_\nu L_\mu(m+n) -[/tex]
   [tex](c_1 m_\nu n_\mu + c_2 m_\mu n_\nu) m_\rho S^\rho(m+n)[/tex]

10. **anon**
    December 4, 2005

    [tex]E = m c^2 .[/tex]

11. **FP**
    December 4, 2005

    [tex] c = \sqrt{a^2 + b^2} [/tex]

12. **Aaron**
    December 4, 2005

    I’m getting the feeling this new toy might need a preview button….

13. **anon**
    December 4, 2005

    [tex]G = (3/4)H^2 /(\pi \rho e^3)[/tex]

14. **Ugo**
    December 4, 2005

    Its like christmas eve on midnight when the kids are busy playing with the new toys 🙂

15. **ksh95**
    December 4, 2005

    this works or I give up
    [tex]\sum_{\bm{G^\prime}}\sum_{w=1}^{2}M_{\bm{k}}^{vw}\left(\bm{G},\bm{G^\prime\right)h_{\bm{k},n}^{\bm{G^\prime},
    (w)}=\frac{\omega_{\bm{k},n}^{2}}{c^2}h_{\bm{k},n}^{\bm{G},(v)}
    [/tex]

16. **vk**
    December 4, 2005

    [tex] \dot{\rho}(t) = \frac{i}{\hbar}[\rho , H] [/tex]

    hope it works .

17. **woit**
    December 4, 2005

    Unfortunately I don’t have time to debug what went wrong with people’s failed attempts to use this, so I just deleted them, to keep my server from continually having to run failed latex commands. I’ll leave the rest as examples of what can be done with this.

18. **D R Lunsford**
    December 4, 2005

    Don’t know if it’s related, but another post had all the text after a left angle bracket (less-than sign) deleted…

    -drl

19. **Carl**
    December 4, 2005

    How about arrays?

    [tex]\left(\begin{array}{cc}0&i//-i&0\end{array}\right) \;\;\textrm{sure enough}[/tex]

    Carl

20. **Thomas Larsson**
    December 4, 2005

    Eggs on my face. Tonight I used instead of [ ], which was eaten as unrecognized HTML tags, of course. Apologies to Peter for filling his comment section with nonsense, but I hope that he will clean up after me. A previewer would be much appreciated.
    [tex]{[}L_\mu(m), L_\nu(n){]} = [/tex]
    [tex]n_\mu L_\nu(m+n) – m_\nu L_\mu(m+n) + [/tex]
    [tex](c_1 m_\nu n_\mu + c_2 m_\mu n_\nu) m_\rho S^\rho(m+n)[/tex]
    [tex]{[}L_\mu(m), S^\nu(n){]} = n_\mu S^\nu(m+n) + \delta^\nu_\mu m_\rho S^\rho(m+n)[/tex]
    [tex]{[}S^\mu(m),S^\nu(n){]} = 0[/tex]
    [tex]m_\mu S^\mu(m) \equiv 0[/tex]

21. **Daniel Doro Ferrante**
    December 5, 2005

    Some problems happen when you type < or &: Those are "special HTML entities" and do not get properly parsed by the plugin. In all fairness, i have not had any trouble with the <, but i have had the exact same trouble with tables when using &s in order to separate the fields.

    Religious wars aside: DRL, MathML is pretty good… and if you use the plugin i mention on the link i sent [see my previous comment above], you won't have much trouble.

22. **Alejandro Rivero**
    December 5, 2005

    Speaking of plugins, there is a podcast plugin around. http://aleph.llull.net/post-to-speech/

    You could try to include TeX to Speech features 🙂

23. **J.F. Moore**
    December 5, 2005

    D.R. Lunsford,

    Latex is a markup language.

    Peter,

    This is very neat, haven't seen it before in a comment section.

24. **D R Lunsford**
    December 5, 2005

    JF, not in the context of a browser, any more than Postscript is. "Everyone knows this". Congrats on being the third person this morning to say something idiotic!

    This thing generates images and puts them in image tags.

    -drl

25. **woit**
    December 5, 2005

    drl,

    Please stop with the abuse of other commenters as idiots, save it for special occasions.

    What is going on here is that the webserver is running latex, using a graphics manipulation program to turn the result into a gif graphics file, which then gets included in the page. Advantages: you don't need any special fonts or plugins for your browser. Disadvantages: doesn't scale properly, a search engine can't get at the content, probably others.

26. **Henrik**
    December 5, 2005

    [tex] \coprod_{2}=\biguplus_{1} [/tex]

27. **D R Lunsford**
    December 5, 2005

    Peter, the other two were offsite.

    -drl

28. **Kasper Olsen**
    December 5, 2005

    Nice, Peter – but I'm sure somebody already "proved" that it works…

    Peter and everybody else: any similar software for use at blogger.cóm?

    Best, Kasper

29. **Who**
    December 5, 2005

    Chris Beasley has posted a new Beasley-Witten paper
    in case anyone is interested
    http://arxiv.org/abs/hep-th/0512039
    New Instanton Effects in String Theory

    I assume some other blogs mentioned it but am having trouble
    opening them, so can't be sure. Is it news that Edward Witten
    has coauthored a stringy paper? Don't recall many such from him
    recently.

30. **D R Lunsford**
    December 7, 2005

Another supremely uninteresting paper without a millisecond expended on physical thought. To witness a genius like Witten produce this effluvia is analogous to watching some hypothetical Beethoven scribble down advertising jingles for an orchestra of detuned kazoos.

Not only is there no physics, but the paper's English style is as deadly monotonous as VCR clock-setting instructions.

-drl

31. **God**
December 7, 2005

Lunsford, I notice that Woit comes in for criticism on Wikipedia for being needlessly kind to Witten: http://en.wikipedia.org/wiki/Peter_Woit

32. **woit**
December 7, 2005

Ugh,
It's pretty annoying to see the Wikipedia entry containing Witten-bashing and descriptions of my research I don't really agree with. Too busy right now, but I'll have to see if I can get this changed.

33. **logopetria**
December 7, 2005

I've had a go at making a few (minor) improvements. Here's a snapshot of the page as I left it:
http://en.wikipedia.org/w/index.php?title=Peter_Woit&oldid=30491221

Took out the Witten-bashing, deleted the stuff about your research that was, I guess, unrepresentative, and replaced it with a couple of sentences from a post at this site. Hope that's OK. Can't claim that I've made the page better, but I hope I've made it less bad.

34. **God**
December 7, 2005

Logopetria,

Good, but you deleted the bits saying:

'He argues that from the path integrals approach to quantum field theory, Euclidean four dimensional space makes the most sense, and Unitary group U(2) is a proper subset of Special Orthogonal group SO(4). Using a spin representation Spin(2n), U(2) gives the quantum numbers of a Standard Model generation of leptons. For a generation of quarks, the 'vacuum' vector transforms under Unitary group U(1) with charge 4/3, which makes the overall average U(1) charge of a generation of leptons and quarks zero.'

(that just summarises the ideas arrived at in hep-th/0206135) and

'Witten, however, has his own share of crackpotism as the inventor of the mainstream string theory, 'M-theory', in 1995: 'String theory has the remarkable property of predicting gravity.' (Witten, April 1996 Physics Today.) Peter Woit avoids an outright condemnation of Witten.'

then you inserted a link to 'The Holy Grail of Physics', http://www.math.columbia.edu/~woit/wordpress/?p=3 which is a mud-wrestling argument between Woit and professor Srednicki!

35. **Florifulgurator**
December 7, 2005

Badly programmed thing! Last try, no & now. This is first time I TeX this century… Yay…
[tex]T\bigotimes\bigwedge T \longmapsto\bigwedge T\cr t\otimes a \mapsto t\wedge a[/tex]

36. **Florifulgurator**
December 7, 2005

How to get \cr work??? 🙁

37. **D R Lunsford**
December 7, 2005

The Wikipedia is just more Internet-Googly trash on the level of flag-waving right-wing war manifesti and porn sites. How surprising that it should contain questionable material!

A venerated reporter, John Siegenthaler I believe was his name, was falsely accused on Wikipedia of participating in assassination plots! I would sooner read the National Inquirer than that pile of horse dung.

It's remarkable how so-called "freedom of information" has corroded intellectual rigor so rapidly. Take a look at the stink wafting from Usenet if you're not convinced.

-drl

38. **Juan R.**
December 16, 2005

A simple proof

[tex]g^{ik} = \eta^{ik} + h^{ik}[/tex]

Roadmap for Fermilab

December 6, 2005
Categories: Uncategorized

Over at Quantum Diaries, Rob Gardner has an interesting report on a lunchtime informal brown bag talk given yesterday by Pier Oddone, the director of Fermilab.

He includes some telegraphic comments about prospects for financing future accelerators (the ILC and upgrades to the LHC):

Scenarios are complex to me! What will CERN and Asia do? Looking at 8B for ILC? How to share the cost? CERN is in debt till 2010! And, note that LHC will need to be upgraded, and will cost 1.5B or so. Can CERN chip in 1B + other Europe 1B? Can we then claim 50% from abroad (ask for 4B)? But what if the ILC RDR comes back at 12B?

Comments

1. **anon**
   December 6, 2005

   What we need is Intelligent Design (of colliders). Hmmm…IDC

2. **scott**
   December 7, 2005

   off topic question: Peter, what happened to that review of Susskind's book you said you were working on over at cosmicvariance? Are you still going to write that?

3. **woit**
   December 7, 2005

   Hi Scott,

   Hope to have it done later today. Got behind schedule on a bunch of things partly due to a day lost to a migraine headache.

4. **Chris Oakley**
   December 7, 2005

   Peter,

   Do any String theorists have access to your kitchen?

   One can't be too careful …

5. **Quantoken**
   December 7, 2005

   Sure they have access to Peter's kitchen. Through extra dimentions!!! Kaku, the guy who always brag about extra dimentions, has no problem access the TV screen in my family room, no matter what channel I switch to. I guess he has plenty of extra dimentions to use 🙂

The Cosmic Landscape

December 7, 2005
Categories: Book Reviews

Susskind's new book, The Cosmic Landscape: String Theory and the Illusion of Intelligent Design is now out. It's basically a lengthy version for the general public of the argument that he has been, with some success, trying to sell to the physics community for the last few years. In short, the argument is that the compatibility of string theory with an essentially infinite variety of different physics is not a bad thing (because it can't predict anything), but a good thing (because it allows an anthropic argument for the small size of the cosmological constant).

Susskind devotes quite a lot of space to attacking the argument that the string theory picture of unification is "elegant", instead promoting the idea that the properties of the universe come from some more or less random very complicated "Rube Goldberg" construction of a vacuum, one whose nature is just constrained by the anthropic principle. He asks:

But is String Theory beautiful? Does String Theory live up to the standards of elegance and uniqueness that physicists demand? Are its equations few and simple? And, most important, are the Laws of Physics implied by String Theory unique?

He answers these questions by first making fun of the supposed mathematical elegance of the theory:

Elegance requires that the number of defining equations be small. Five is better than ten, and one is better than five. On this score, one might facetiously say that String Theory is the ultimate epitome of elegance. With all the years that String Theory has been studied, no one has ever found even a single defining equation! The number at present count is zero. We know neither what the fundamental equations of the theory are nor even if it has any.

He goes on to argue that the laws of physics implied by string theory have turned out to be highly non-unique:

During the 1990s the number of possibilities grew exponentially. String theorists watched with horror as a stupendous Landscape opened up with so many valleys that almost anything can be found somewhere in it.

The theory also exhibited a nasty tendency to produce Rube Goldberg machines. In searching the Landscape for the Standard Model, the constructions became unpleasantly complicated. More and more "moving parts" had to be introduced to account for all the requirements, and by now it seems that no realistic model would pass muster with the American Society of Engineers — not for elegance in any case.

From this he draws the following bizarre conclusion:

Judged by the ordinary criteria of uniqueness and elegance, String Theory has gone from being Beauty to being the Beast. And yet the more I think about this unfortunate history, the more reason I think there is to believe that String Theory is the answer.

He remarks with surprise that no one has drawn the obvious conclusion that these arguments just imply that string theory is wrong:

What I have never heard is criticism based on the unfortunate inelegance or the lack of uniqueness of String Theory. Either of these tendencies might be thrown back at the string theorists as evidence that their own hopes for the theory are misguided. Perhaps part of the reason that the enemies haven't pounced is that string theorists have kept their Achilles heel under wraps until fairly recently. I suspect that now that it is becoming public, partly through my own writings and lectures, the kibitzers on the sidelines will be grinning and loudly announcing, "Ha, ha, we knew it all along. String Theory is dead."

adding a footnote to this paragraph in proof:

This remark was written in the spring of 1994, but by the time I completed writing 'The Cosmic Landscape' a year later, the vultures had descended in force.

He seems to have forgotten about at least one particular vulture, who back in 2003, tried to make this point at the question session after one of his colloquium talks.

Susskind's argument that string theory's compatibility with just about anything is actually an advantage is based on the fact that this makes a place for Weinberg's 1987 anthropic principle argument for the size of the cosmological constant (which from what I've seen gets it wrong by at least one to two orders of magnitude if you only vary the CC, more if you vary other parameters). He addresses criticism of the anthropic principle as unscientific by denouncing the field of philosophy of science, and the criterion of falsifiability in particular:

Frankly, I would have preferred to avoid the kind of philosophical discourse that the Anthropic Principle excites. But the pontification, by the "Popperazi," about what is and what is not science has become so furious in news reports and Internet blogs that I feel I have to address it.

He then goes on to quote from something he wrote for a debate with Smolin at the Edge web-site. For the bizarre story of how this debate came about, including the rejection by the arXiv of a submitted "paper" by Susskind about this, see here. He begins with:

Throughout my long experience as a scientist I have heard unfalsifiability hurled at so many important ideas that I am inclined to think that no idea can have great merit unless it has drawn this criticism. I'll give some examples:

The examples he gives are:

1. Behaviorist psychologists like B. F. Skinner who argued that statements about emotions were unscientific. Here I think Susskind is confusing positivism (the philosophy that science should just deal with directly observable quantities), with falsifiability, which is different. A theory may be based on quantities that are not directly observable, and still make falsifiable predictions about experimentally observable quantities. In any case, physics is supposed to be a much "harder" science than that part of psychology dealing with human emotions, and it is pretty strange for a physicist to be arguing that what he is doing really is science using this as an example.

2. The theory of quarks. Again, Susskind completely confuses positivistic objections (that if quarks are not directly observable you shouldn't talk about them), with falsifiability. While quark theory was problematic until 1973 since there was no workable dynamics, it was taken seriously because it made some very impressive, highly falsifiable predictions. The best known example is the quark theory prediction of the mass, spin and charge of the Omega-Minus particle. If string theory had made some predictions like this, few people would be criticizing it.

3. The theory of inflation. Susskind claims that after Guth first came up with this in 1980, it was attacked as unfalsifiable. I don't recall ever having heard such a criticism, although it was always clear and remains true to this day that experimentally distinguishing between the predictions of different mechanisms for inflation is difficult. From what I remember, there was actually a lot more optimism in the early 80s about this than now, since people were pretty enthusiastic about GUT models, and there seemed to be a good chance that one of the scalar fields in a simple GUT model would do the trick. Susskind writes: "It took 20 years to do the experiments that confirmed inflation." As far as I know people were calculating the effects of inflation on the CMB and starting to design experiments to see them within a few years after Guth's work. I don't see the relevance of the fact that it took a while to get a sufficiently sensitive experiment working.

4. The theory of evolution. Susskind joins other string theorists like Lubos Motl and an anonymous Cambridge referee I dealt with in believing that the status of string theory is much like that of the theory of evolution. He seems to believe that fossil evidence is irrelevant to testing Darwin, writing:

And it took more 100 years or more for to decisively test Darwin (some would even say that it has yet to be tested).

I'll leave it to a professional biologist like P. Z. Myers to argue this point with him, but it seems to me both nutty and irresponsible, given the ongoing battles over the teaching of evolution (which Susskind is getting himself involved with in the very subtitle of this book).

After attacking falsifiability as a criterion for a scientific theory, Susskind does admit that a theory has to make some predictions, even if they're not the sort that could falsify the theory. He acknowledges difficulty in coming up with any predictions:

Is there any way to explain in which of these anthropically acceptable vacuums we live? Obviously, the Anthropic Principle cannot help us predict which one we live in — any of these vacuums is acceptable.

This conclusion is frustrating. It leaves the theory open to the criticism that it has no predictive power, something that scientists are very sensitive about.

He discusses the idea of using statistical arguments, acknowledging that there are severe problems with this due to the "measure problem" of not being able to compare sizes of infinite sets, as well as the problem of not knowing what a priori probability to assign to any given vacuum state. Finally he does try and come up with some suggestions of how the theory might be tested, they are:

1. Evidence in the CMB that our universe was formed by bubble nucleation:

If we are very, very lucky, the largest lumps in the CMB might date to a time just before the usual Inflation got started — in other words, just as the universe was settling onto the inflationary ledge…

If we are that lucky, then the Inflation did not go on long enough to wipe out evidence for the curvature of space… If our pocket universe was born in a bubble nucleation event, the universe must be negatively curved.

At the level of accuracy that the curvature of space has been measured, there is no indication of such curvature. This idea may fail because standard Inflation probably has been going on for a long time when the largest visible lumps were formed.

So, this is both very unlikely to be something we can observe, and even if we did it would only tell us that the universe was born in a nucleation event, still giving us just about zero information about the supposed landscape and none whatsoever about string theory.

2. Cosmic superstrings. Here Susskind is referring to claims by Polchinski and others that amidst the infinity of possible physics due to string theory, one can cook up special cases where certain kinds of superstrings of astronomical dimensions exist and have properties precisely such that we wouldn't have seen them yet, but could see their effects in gravitational wave experiments like Advanced LIGO. As far as I've ever been able to tell, these are contrived constructions, with no reason at all that the vacuum state of the real world should be such as to support them. These are highly non-falsifiable "predictions" of string theory. No string theorist is going to give up on string theory just because Advanced LIGO doesn't see these effects.

3. High energy physics. Susskind talks about the LHC and the question of whether the fine-tuning problem of the Higgs mass will be resolved by supersymmetry or is anthropic. He acknowledges that, based on Landscape arguments:

My original guess was that supersymmetry was not favored, and I said so in print. But I have changed my mind — twice — and probably not for the last time.

This isn't much of a argument for predictivity on this issue, but I guess his point is that sufficient study of the Landscape might somehow resolve this question, although all the evidence so far is that this is not possible.

The bottom line here is that Susskind is unable to come up with any remotely plausible way of ever getting any scientific predictions out of the string theory landscape framework, and yet he thinks it is a good idea to write a popular book designed to sell it to the public. He makes clear that he is doing this because he sees himself at war with that part of the theoretical physics community which still believes in the idea of continuing to try and do what theoretical physicists have always done: find a more mathematically beautiful, more compelling, more predictive theory than the one we have now. In one chapter he surveys the state of the ongoing political battle for the hearts and minds of his fellow theorists. He crows (with some justification) that Weinberg agrees with him, saying physicists have to give up the paradigm of how to do physics they pursued during the last century, that Witten is facing defeat and getting depressed, that Joe Polchinski says there's no alternative, that the entire Stanford theoretical physics group are his allies, that 't Hooft won't rule out anthropic explanations, that Maldacena believes in the Landscape, that Michael Douglas is on his side, that cosmologists Linde, Vilenkin, Rees and Tegmark are in his camp, and that Alan Guth is at least a fence sitter. One of the few active opponents that he sees left on the scene is David Gross, whose reasons for opposition he describes as "more ideological than scientific." He sees Gross as dead meat: "the field of physics is littered with the corpses of stubborn old men who didn't know when to give up."

In coming weeks, it will be interesting to see how the physics community deals with the challenge presented by Susskind's publicity campaign for changing how theoretical physics is done. So far the initial signs are depressing. Michael Duff's review in Physics World just more or less respectfully repeats Susskind's argument, not challenging it in any way. In a review of the Duff review, Clifford Johnson answers the question of whether this sort of thing is still science with "I have not yet made up my own mind whether it sits well with me or not…" He makes a distinction between postdiction and prediction that I don't quite agree with (if Susskind's framework accurately postdicted even a few of the known Standard Model parameters, I'd be a believer). He takes the usual stance favored by most sensible string theorists who want to keep working on the theory that they don't understand the theory well enough yet to know whether they are stuck with the Landscape or not. Finally he thinks there's a chance that maybe the structure of the Landscape is such that once one anthropically fixed the CC and some other constants, the remaining set of vacua would actually predict something. I don't see the slightest evidence for this, but it's the argument many are now using to justify exploring the Landscape and surrounding swampland instead of giving up on string theory and trying to find a better idea.

Update: I should have mentioned a recent well thought out review of Susskind's book at Tech Central Station by Kenneth Silber. It's quite sensible and worth reading if you're following this story. Another review of the book has just appeared, this one by George Ellis at Nature. Ellis is much more critical of Susskind than Duff was, realizing that the crucial issue is that Susskind has no evidence for his claims, and writing in his final paragraph:

Physicists indulging in this kind of speculation sometimes denigrate philosophers of science, but they themselves do not yet have rigorous criteria to offer for proof of physical existence. This is what is needed to make this area solid science, rather than speculation. Until then, the multiverse situation seems to fit St Paul's description: "Faith is the substance of things hoped for, the evidence of things not seen." In this case, it is faith that enormous extrapolations from tested physics are correct; hope that correct hints as to the way things really are have been identified from all the possibilities, and that the present marginal evidence to the contrary will go away.

One peculiar thing about Ellis's review is that he accuses Susskind of ignoring the fact that there is no experimental evidence for negative curvature of the sort one might get if the universe was formed by bubble nucleation. In Susskind's defense, he does address this point, saying it is very unlikely we can see this negative curvature since inflation is likely to have gone on long enough to make it unmeasurably small.

Comments

1. **Chris W.**
December 7, 2005

   One hardly knows what to say. I find myself wondering whether this isn't some kind of setup, with Susskind accusing dozens of people—some years from now—of allowing themselves to get sucked in by what was, in the end, just a ruse.

   Also, I sense an ironic twist in many of Weinberg's comments on this subject that suggest he isn't buying. He has merely made a modest investment which he would be happy to lose. (He hasn't actually done much work on the Landscape himself, right?)

   Great post, Peter.

2. **D R Lunsford**
December 7, 2005

   (aside) In contrast to this garbage, Cooperstock and Tieu have a followup paper on galactic rotation curves:

   http://arxiv.org/abs/astro-ph/0512048

   -drl

3. **D R Lunsford**
December 7, 2005

   Speaking of Guth, one might well point out that the modern idiocracy rose to power along with the general acceptance of the intellectually bankrupt idea of "cosmic inflation".

   We physicists need to take our subject back from these bright boys.

   -drl

4. **Arun**
December 7, 2005

   A great essay, but profoundly depressing.

5. **MP**
December 8, 2005

   Can anyone who knows these things comment on the braneworld scenarios? The claim is that braneworld predictions are testable in the not so distant future.

6. **hack**
December 8, 2005

   "What I have never heard is criticism based on the unfortunate inelegance or the lack of uniqueness of String Theory"

   If he has never heard this criticism, it's because his fingers were plugging his ears while he said "na na na na na…"

7. **Kasper Olsen**
December 8, 2005

   @ MP: how braneworld theories can be tested: read Lisa Randall's new book, Warped passages, or look at xxx.lanl.gov…

   regards, Kasper

8. **Adrian Heathcote**
December 8, 2005

   Peter

   A great article, and for my money you are spot on in your critisisms of Susskind's claims about falsifiability. Susskind seems to not have a clue about these things. And it's interesting that he didn't mention the one theory that has repeatedly faced claims of unfalsifiability, namely Freud's theory of dreams and repression. And he doesn't mention it because it would upset this silly idea that a theory could be perfectly good yet unfalsifiable.

   (Why do referees do such a poor job? they block the good and interesting, and let through confused stuff like this. The world's going to hell in a handbasket.)

9. **Jose**
December 8, 2005

   It seems to me that the book of Susskind is the best allegation against string theory!.

10. **Chris Oakley**
December 8, 2005

    Why do referees do such a poor job? They block the good and interesting, and let through confused stuff like this.

    This may have something to do with the fact that LS has a prestigious job at a prestigious university.

    The same comment I made about Weinberg also applies here: if one is in a position of influence then one is duty bound to try to encourage people to do one's subject rather than to try to scare them off. Telling people that String theory is nonsense but still the only idea in particle physics worthy of study is not going to make anyone want to sign up.
    Of course, there *are* alternatives to String theory, but the String-theory-dominated research establishment does its utmost to make sure that these particular suckers never get an even break. In the long run, though, if leaders like Susskind and Weinberg convince taxpayers that theoretical particle physics is not worth funding, then who are the

real suckers?

11. **Juan R.**
December 8, 2005

Peter Woit said,

> Susskind's new book, The Cosmic Landscape: String Theory and the Illusion of Intelligent Design is now out. It's basically a lengthy version for the general public of the argument that he has been, with some success, trying to sell to the physics community for the last few years. In short, the argument is that the compatibility of string theory with an essentially infinite variety of different physics is not a bad thing (because it can't predict anything), but a good thing (because it allows an anthropic argument for the small size of the cosmological constant).

That is, we can choose between two options:

– **Theory A**. The theory predicts many things with great precision. By "predict" i mean that one knows the result of an experiment before it was done. For instance, one known approximation to theory A is the pair standard model more general relativity, even if both are incompatible, since both provide excellent result on the very big and the very small! Of course the true theory A may be internally compatible. String theorists often misunderstand the word "predict" and use it instead of "post-dict". By "post-dict" i mean that one may know first the experiment and then *modify*/*adapt* the theory to the obtained value. Since we may know first the experimental outcome, of course, the practical utility of this kind of theories is zero. The theory cannot scientifically explain the cosmological constant.

– **String 'theory'**. Or its M unknown generalization! *The 'theory' predicts nothing of this world and is incompatible with physics we observe in laboratory*. Since string 'theory' is not useful, people of the real world continue using well-tested scientific theories. For example, astronomers continue using general relativity on the prediction/explanation of phenomena. Chemists continue using quantum mechanics of particles on the computation of reaction constants, biologists continue using thermodynamics to understand metabolism, etc. Using antrophic principle, string theorists can 'explain' the cosmological constant. The antrophic principle basically says that cosmological constant has value we measure because if has other value life would be imposible and we would not measure it! Of course, this is not a scientific explanation. At the best, it is phylosophical one, but of no real utility on science.

Therefore, the option opened by string theory reduces to either scientifically understand/predict/explain a 'billion' of things or scientifically understand/predict/explain nothing and just phylosophically explain ONE single thing.

Of course, the phylosophical antrophic principle can be used with theory A!

Since my position is well-known i add no more comments!

Juan R.

Center for CANONICAL |SCIENCE)

12. **DMS**
December 8, 2005

Nice, but depressing review.

It is quite pathetic to see some of the arguments used by Susskind, like comparing string theory to QCD, in terms of falsifiability. Whom does he think he is fooling with this blatant lie?

I am afraid the post-War (generally very favourable) perception of the public of physics(and physicists in general), not just string theorists, will be severely damaged. This will not be without consequences.

It is quite amusing to see the kid glove treatment of Susskind by other string theorists and contrast it with the reaction to Krauss; he obviously touched a nerve. The irony is that Susskind's book(right from the title onwards) is more damaging to the field than Krauss'. I suppose, intellectual dishonesty is in all fields, but the extent of it in string theory may be unrivalled.

I was quite surprised to note the many eminent physicists who are behind this anthropic nonsense, or at least not outright rejecting it. I was under the impression that it was merely a fringe in the community. No longer, it seems…

DMS

13. **a**
December 8, 2005

I think you are discussing two different issues.

1) The anthropic issue. Here you seem too negative: experiments still have something important to say. If LHC will find that the Higgs mass hierarchy problem is solved in some "natural" way, anthropic interpretations will be mostly abandoned. If LHC will find nothing, anthropic intrpretations will be a good alternative to harakiri.

2) The string issue. Here it is easy to guess the impact of LHC results: whatever LHC will find (nothing? supersymmetry? extra dimensions?) will be used to argue in favor of string theory, and you are doing a good work to make the situation more healthy.

14. **Dumb Biologist**
December 8, 2005

Dear. Dr. Woit,

I've been an admiring lurker for quite some time*, but this review prompted me to contribute. Superbly written, and I am always deeply appreciative of scientists who can not only think clearly, but also communicate their thoughts with equal parts clarity and cogency to a lay audience (in physics, at least). I suppose the depressing starkness with which Susskind's bereft position is revealed is also due, in no small part, to his efforts to reach a broad audience as well.

I've no doubt Dr. Susskind is my intellectual superior, as are most, if not all of the String "theorists", but all the powers of all the greatest physicists joined together will never conjure experimental verification out of thin air, and for the assay and its fruits, logic is simply no substitute in the realm of skeptical inquiry. Now that we've seen a preeminent physicist's attempt to herald a new paradigm shift, by eschewing testability altogether in the name of "progress", I fear for the integrity of the natural sciences in general, and physics in particular. I'd laugh it off in happier times, but particularly the ID "theorists'" faith-based pollution and perversion of the public scientific discourse has revealed how truly vulnerable the edifice of skeptical inquiry, built as it has been, brick-by-torturous-brick since the Renaissance, can be battered by the rams of fundamentalist faith and dogmatic philosphizing on nothing. It's painful to see science assaulted from without, but positively excruciating to see it eat at itself from within.

Despite the sometimes overly-pejorative nature of the conversation here (which it seems you've recently taken more assertive steps to limit, which I appreciate), I'm glad people like you are willing to stick their necks out and protest this corruption of the scientific method. I hope the message gets through someday.

*I chose my handle to preempt any insults hurled my way by Dr. Motl, should he deign to swat at a biognat such as myself if something I might ever say touches a nerve.

15. **woit**
    December 8, 2005

    a,

    All I was doing here was reporting on what Susskind is saying, he's the one bringing claiming that string theory both allows and requires anthropic arguments, and that, perversely, this is a good thing.

16. **Chris Oakley**
    December 8, 2005

    I chose my handle to preempt any insults hurled my way by Dr. Motl, should he deign to swat at a biognat such as myself if something I might ever say touches a nerve.

    No-one is going to take you seriously in this forum until you have been savaged by Lubos Motl. Look on it as a rite of passage.

17. **Brett**
    December 8, 2005

    While I disagree with Susskind's ideas about anthropic reasoning, I have to say that I find Smolin's arguments on the subject equally unconvincing. Smolin's (and everyone else's) main example of how the anthropic principle is used is Weinberg's examination of the cosmological constant. Smolin's seems to mistake the fundamentally inductive character of this argument. The deductive stage is unambiguous, that because there are galaxies, the cosmological constant must be small. But this tells us nothing about why it is small. Part of the purpose of science is to determine causes for what is observed, and what Weinberg wanted to argue was that the size of the cosmological constant could provide evidence that the cause of that smallness was purely anthropic. While I do not consider his evidence to be all that strong, the problem is a numerical, not philosophical one.

    I believe that Susskind is correct that bringing heavy philosophy into things is simply misleading, and Popper's ideas, in particular, do not really match with how many real scientists have always done science. Popper rejected almost entirely the notion of positive evidence, which is really one of the cornerstones of the scientific method. Falsifiability is fundamentally a deductive criterion; we can deduce which theories are false, but not which are true. The earliest modern philosophers of science, Descartes and Bacon, ridiculed the deductive reasoning of the scholastics. Scholastic reasoning assumed known (and theological) causes and tried to work of predictions from that. There was no apparatus for determining any new causes. But finding new causes was exactly what a scientist like Newton had to do. This is a fundamentally inductive process, and it is based, ultimately, on the intuitive Bayesian fact that an observation of a cause's effects makes that cause more likely. This kind of framework allows anthropic causes, and so there is no a priori reason to reject them as unscientific.

18. **Dumb Biologist**
    December 8, 2005

    I think if you simply substitute "testability" for "falsifiability", many these pernicious philisopical issues simply vanish. Science, simply put, involves putting ideas to the test experimentally. Perhaps there are scientifically relevant concepts which cannot be falsified by any means available to mortals, but we become comfortable (or only ought to, anyway) with such assertions only after the most vigorous experimental assault. That's how science keeps itself honest. Surely scientists utilize everything from induction to deduction to dumb luck in the process of discovery, but the process becomes rather suspect when the very meaning of "discovery" is subverted, and at the end of the day one no longer feels beholden to nature to demonstrate the predictive power of one's ideas. Perhaps we invent hypotheses, but we verify the accuracy of those hypotheses and build theoretical frameworks through observation. THAT is the scientific method.

    If what I have read in the "popular science media" about String "theory", it is right to say it's not a theory at all, but a hypothetical model. There's nothing wrong with hypothetical models, but mistaking or conflating such a thing, no matter how rigorous or ambitions, with the TESTED predictive power of a true theoretical framework is irresponsible. Look no further than the "controversy" over Intelligent Design to see why respecting the meaning of words like "theory" is vitally important.

    And yet: Because of its purported beauty and profundity, what started as hope does seem, at least in the minds of some, to have become a kind of faith among some physicists. String Theory simply must be true, because of all its wonderful symmetries, and so forth. Ergo, if it cannot be falsified (or worse, even tested), the need for anything remotely resembling experimental verification is shown to be superfluous, quaint, even obstructive. Such is the "progress" we've achieved with deductive reasoning, it would seem. Perhaps the sciences are coming full circle to mere philosphy once again. It's especially rich when the very definition of science, speciously derided as some "Popperian dogma", is being deliberately reworked to defend a so-called theory that apparently can't predict anything (or rather, predicts everything?). The scientific method isn't just any philosophy we can discard when no longer fashionable (like postmodernism). At least, I hope it isn't, because we've tried promulgating "truth" after bouts of deep thought, and that program of "discovery" has rather failed to deliver.

19. **Dumb Biologist**
    December 8, 2005

    Oh, and apologies for some of my typos and other poor writing. I failed to see the preview unfolding below the entry field (first time posting on a blog and all…simple use of the scroll bar would have revealed the Preview), and hence didn't proof-read my posts very well. I'll may just shut up now, but, anyway, thanks again Dr. Woit for your contributions to a worthy subject of debate.

20. **Chris W.**
    December 8, 2005

    From Leonard Susskind, in his email exchange with Lee Smolin published on Edge.org:

    > Good scientific methodology is not an abstract set of rules dictated by philosophers. It is conditioned by, and determined by, the science itself and the scientists who create the science. What may have constituted scientific proof for a particle physicist of the 1960's—namely the detection of an isolated particle—is inappropriate for a modern quark physicist who can never hope to remove and isolate a quark. Let's not put the cart before the horse. Science is the horse that pulls the cart of philosophy.

    A quote from Popper, via this page:

    > "We all take our philosophies whether or not we are aware of this fact, and our philosophies are not worth very much. But the impact of our philosophies upon our actions, our lives, is often devastating. This makes it necessary to try to improve our philosophies by criticism. This is the only apology for continued existence of philosophy which I am able to offer." (from *Objective Knowledge*)

(I don't think this transcription is completely accurate, but it's very close to the original. I remember the passage well from my own reading of *Objective Knowledge*.)

21. **Christine Dantas**
December 8, 2005

As a somewhat newcomer to the subject of quantum gravity, and an astrophysicist by formation, I find this all exceedingly interesting. Congratulations to Dr. Woit for making this very relevant and timely debate possible.

I would like to take the opportunity, if not considered a diversion, and quote the following paragraph from Barrow & Tipler – The Anthropic Cosmological Principle, page 275:

> At present there is no theoretical understanding of why just three dimensions have expanded to a large size if the others are indeed confined to minute extent. However, the Anthropic arguments we gave concerning the special properties of three-dimensional space and four-dimensional space-time show that there would be a Weak Anthropic explanation for this observation also; but, for all we know, it may also be a consequence of the unique topological properties that four-dimensional manifolds have recently been found to possess. The fact that only they admit more than one distinct differentiable structure may well turn out to have something to do with the fact that observable space-time has four dimensions.

Is that view compatible or incompatible with the current anthropic landscape issue? In any case, how can this really be useful?

Thank you,
Christine

22. **Clark Goble**
December 8, 2005

Good post, although I'd quibble with your discussion of logical positivism. Logical positivism, at least none of the figures I've read, would say science can "just deal with directly observable quantities." Rather logical positivism was more trying to take the typical science of the day and move it towards philosophical problems. That is, if it can't be verified empirically, it is meaningless. That tendency goes back well into the 19th century. In America we have figures like C. S. Peirce espousing a theory fairly similar, for instance. All would admit theoretical entities, although they'd say our justification for theoretical entities comes from this verification principle.

Once could adopt *both* a verification principle and a principle of falsification. Peirce does this, for instance, although many more argued that one could verify directly through empirical knowledge.

Logical positivism does start diving kinds of science when we get into psychology, history and related disciplines. But with a few caveats, I think one could be a logical positivist and not have trouble with most physics.

Where the logical positivists part ways is with people who tend to espouse a coherency theory of truth. The positivists were very much tied up into a correspondence theory of truth.

23. **Quantum_Ranger**
December 8, 2005

The clarity of your post peter, portrays a just confusion with respect to Susskind's "state of mind".

It is by no coincedence that the 'boom and bust' mentality of the 80's, was perfectly matched with stringtheory's creation and evolution.

The sober thinking these days, albeit with hindsight, needs the guiding voice and eye's such as you attain in your varied writings.

I do not think it is all doom'n'gloom though, but it is apperaing more and more that there is a lot of "dis-information" contained within the Anthropic Landscape, by fact of his positional stance, Susskind rule's supreme, over the scientific kingdom that is apparently untouchable as well as unaccountable?..if one takes the vacuum background as literally de-facto?

24. **Chris W.**
December 8, 2005

Christine,

Regarding the significance of the special features of 4-dimensional manifolds, see the excerpt from a paper by Hendryk Pfeiffer quoted in this comment on an earlier post on this blog.

There is much of interest in this post and many of the ensuing comments.

25. **Who**
December 8, 2005

when Susskind's views on the String Landscape are discussed, together with Anthropic, and related, reasoning, I think you miss half the argument if you dont mention this article by Smolin (which upset Susskind quite a bit when Lee sent him a copy)

http://arxiv.org/abs/hep-th/0407213
Scientific alternatives to the anthropic principle
Lee Smolin
Contribution to "Universe or Multiverse", ed. by Bernard Carr et. al., to be published by Cambridge University Press.

"It is explained in detail why the Anthropic Principle (AP) cannot yield any falsifiable predictions, and therefore cannot be a part of science. Cases which have been claimed as successful predictions from the AP are shown to be not that. Either they are uncontroversial applications of selection principles in one universe (as in Dicke's argument), or the predictions made do not actually logically depend on any assumption about life or intelligence, but instead depend only on arguments from observed facts (as in the case of arguments by Hoyle and Weinberg). …
…
We show however that it is still possible to make falsifiable predictions from theories of multiverses, if the ensemble predicted has certain properties specified here. An example of such a falsifiable multiverse theory is cosmological natural selection. It is reviewed here and it is argued that the theory remains unfalsified. But it is very vulnerable to falsification by current observations, which shows that it is a scientific theory.
The consequences for recent discussions of the AP in the context of string theory are discussed."

26. **Christine**
December 9, 2005

Chris W. wrote:

see the excerpt from a paper by Hendryk Pfeiffer

Thank you very much!

Best wishes
Christine

27. **JC**
December 10, 2005

Chris W.,

In your link "http://www.math.columbia.edu/~woit/wordpress/?p=206#comment-3795" in a previous post, check out the talk of one of the authors (Prakash Panangaden) of the paper you linked to gr-qc/0407094. The last page of the talk is hilarious.

28. **island**
December 11, 2005

Lenny doesn't realize it, but he is advocating a natural design theory if he's using the landscape to rationalize away the significance of the fact that *the appearance of [intelligent] design is undeniable*… because it isn't just an "appearance" if multiverses don't exist to lose the significance in.

He isn't just correct about that, he's…

NOT EVEN RIGHT

29. **Chris W.**
December 11, 2005

The same talk is available as a PDF file.

— — — — — — — — —
It seems that a number of computer scientists studying distributed systems have formulated theories of what might be called *asynchronous causality*, and some have noted the potential value of connecting these ideas with Lorentzian geometry. In Panangaden's recent papers with his collaborator Keye Martin this point of view has been well developed.

Also see Discrete Quantum Causal Dynamics (gr-qc/0109053).

[By the way, asynchronous processor designs have been the subject of a growing R & D effort in the computer industry for the last several years. (Sun Microsystems' effort has been led by Ivan Sutherland.) Synchronous clocking of highly integrated digital circuits is threatening to become non-viable in the fairly near future, for performance and power consumption reasons as well as the sheer difficulty of implementation at very high clock rates.]

(* professor of CS at McGill)

30. **island**
December 12, 2005

Finally, he thinks there's a chance that maybe the structure of the Landscape is such that once one anthropically fixed the CC and some other constants, the remaining set of vacua would actually predict something. I don't see the slightest evidence for this

Universes with other exaples of the commonly speculated forms of life, like; Silicon based life, Nitrogen and Phosphorus based life. Chlorine or Sulfur based life, where differently tuned constants produces a different set of eco-balances, and a different ratio in the cosmic abundance of any of the mentioned elements to carbon, of 10 to 1 in their favor, rather than the other way round like it is in our universe… for examples of how far your imagination can run if…

… if ours is an anthropic universe that undergoes an evolutionary-like leap/bang to a higher order of symmetry and efficiency, in terms of more uniform form of energy-disseminating capability…

…but that's not going to happen in the one that exists this go-round, and there certainly isn't the slightest bit of evidence that a "pocket" of naturally selected near-identical bubble-universes in a "would-be" multiverse, would or should be preferred over an evolving biocentric universe, for which there is pre-existing observed justification for all the way down to your ancestors, the apes.

David Gross Admits String Theory is in Trouble

December 9, 2005
Categories: Uncategorized

The latest issue of New Scientist has an article entitled [Nobel Laureate Admits String Theory is in Trouble](). It describes remarks by David Gross at the recent Solvay conference in Brussels, mentioned [here]() earlier.

Gross described the current state of string theory as "We don't know what we are talking about", and also admitted:

"Many of us believed that string theory was a very dramatic break with our previous notions of quantum theory," he said. "But now we learn that string theory, well, is not that much of a break."

He said the field was in "a period of utter confusion", and compared the current situation to that at in 1911, at the time of the first Solvay conference, when no one had any idea what was causing radioactivity.

"They were missing something absolutely fundamental," he said. "We are missing perhaps something as profound as they were back then."

The same issue of New Scientist also has an editorial on the subject entitled [Physics' greatest endeavour is grinding to a halt](), which ends as follows:

For decades, string theorists have been excused from testing their ideas against experimental results. When astronomers discovered the accelerating expansion of the universe, which string theory fails to account for, many string theorists took shelter in a remarkable excuse: that their equations describe all possible universes and should not be tied to matching data in just one of them.

But when the theory does not match the one data set we have, is it science? There is a joke circulating on physics blogs: that we can, after all, call our universe unique. Why? Because it is the only one that string theory cannot describe. Should we laugh or cry?

There is a growing feeling that string theory has run into the sand. Gross thinks we are missing something fundamental. We need a leap in understanding, though where it will come from is not clear. Many of the greatest minds in physics were there at last week's conference, and none had an answer.

We are approaching the end of Einstein's centennial year – a celebration of physics. While some lesser-known areas of the subject are flourishing, the search for a theory of everything is in a sorry state. Unless string theory gets a radical shake-up, gifted but frustrated minds will begin to drift into other areas of science. And if that is what makes biology the subject of the century, it will be depressing reason indeed.

Update: Lubos Motl has some [comments]() on this. He compares the current devastation of string theory to the effects of hurricane Katrina and me to an Islamic extremist, while arguing against the terrible danger to physics if all this leads to study of a "diversity of approaches" other than string theory.

Update: Gross now [claims]() his words were misinterpreted.

Comments

1. **blank**
   December 9, 2005

   Ouch!

   Now is Lubos going to tell us that Gross has s**t for brains?

2. **Who**
   December 9, 2005

   for me the big question is whether David Gross acknowledged the existence of other interesting approaches to quantum spacetime or quantum gravity that might reasonably be pursued.

   there has been a consistent tendency for string leaders to say things "our one best hope" and "the only consistent quantum theory of gravity". Steve Shenker used that phrase at an embarrassing moment in Strings '05 when someone in the audience asked why study string theory and the panel members didn't have a ready answer.

   It is a kind of monopoly delusion—-of being God's chosen research program—-which is maintained by repetition, like a creed. The only time I heard any string notable break from it was in what Andy S said during the same discussion. He outright said there were other interesting approaches that you could reasonably investigate. No sneer or any hint of condescension. Apparently it takes courage and honesty to say that.

   For me the question is if David Gross went that far. If his message was merely "Oh my we are the only game in town and look how confused we are!!! This must be an exciting historical moment, like 1911, when relativity and quantum mechanics were waiting in the wings."
   If his message was merely that, then it lacked integrity.

   But if he said "we are confused right now and there are alternatives—we aren't sure of being the one right approach", then I would put that on par with the Andy S statement at Strings '05. It is the kind of healthy realism that leadership has to show before things can start getting better.

3. **Who**
   December 9, 2005

   a propos "physics greatest endeavor" grinding to a halt, I seem to remember that in past years Michael Peskin's Topcite Review of previous year has been out by around September.

   but if you look here
   http://www.slac.stanford.edu/library/topcites/
   you see that it promises 4 items of information for 2004 but has delivered only two, not including Peskin's review

   —-quote—
   2004 Edition

(The 2004 edition covers all HEP papers from January 1, 2004 and December 31, 2004. The following articles are available for this edition.)

Review of top cited HEP articles 2004
List of top cited HEP articles in 2004
List of all-time top cited HEP articles (as of 12/31/2004)
List of top cited articles from the E-print archives 2004

—-endquote—

only the two bolded items appear to be actually available

Is Michael Peskin going to write the review this year, in fact, or is he reluctant to continue performing that ceremonial function?

has the annual Rose Bowl of cite-ranking become so embarrassing to string theorists that the Stanford librarians have tactfully decided to let the practice lapse?

4. **Doug**
   December 9, 2005

   It would be really interesting to know what Gell Mann, t'Hooft, Atiyah, Weinberg and the rest had to say. Does someone know of any plans to make the proceedings available?

5. **woit**
   December 9, 2005

   Who,

   I doubt it's the Stanford librarians deciding this, but suspect that, given the discouraging situation of virtually no new ideas in particle theory to report, Peskin has been avoiding the task of sitting down to write up the kind of thing he's done in past years. Maybe he'll get to it at some point, maybe he'll give up on it for good.

6. **Christine Dantas**
   December 9, 2005

   Is it really a consensus in the string theory community that this enormous so-called landscape of vacua is in fact an unavoidable result from the "theory"? Or there are people that do not really accept this result as a final word and attempt to find other possibilities (within the "theory", I mean)?

   Now one more thing concerning this apparent dead-end for string theory. This question is ashamedly basic and possibly a nonsense, but I will ask anyway.

   – This landscape thing. Has anyone thought about some "meta"(?)-variational principle or something analogous so that a unique vacuum could be selected from such "principle"? I have not the slightest idea if that makes any sense, but I appreciate any comments on this.

   Christine

7. **Aaron Bergman**
   December 9, 2005

   Is it really a consensus in the string theory community that this enormous so-called landscape of vacua is in fact an unavoidable result from the "theory"?

   Yes and no. There seem to definitely be an infinite number of vacua — toroidal compactifications at various radii, for example. The new idea is the production of vacua that have no massless scalars. These are all AdS vacua. The techniques involved in the production of these vacua aren't completely rigorous, but persuasive.

   Because they're AdS, none of these vacua are realistic. One needs to lift them to de Sitter vacua with a positive cosmological constant. This step is much more sketchy.

   Even when you accept all that, one has to ask how many vacua are there that have a reasonable matter content. For any useful definition of reasonable, that's a difficult question. It has been joked by various string theorists that people have extrapolated the existence of a huge number of realistic vacua when string theory hasn't yet produced a single one.

   One scenario for predicitivity of string theory is that there turns out to be very few vacua consistent with current observations. If we can pin down the precise vacuum that we live in, then the theory becomes predictive.

   Lots of people have thought about selection mechanisms for vacua, but — in that we don't actually understand string theory — it's all pretty speculative.

8. **A. nonymous**
   December 9, 2005

   I think the variational principle involved is that the background should be a (meta-)stable vacuum. This will ensue that you have a discrete set of candidate backgrounds. The problem is that, although there are discrete vacua, there are a very large number of them. You need some principle which selects a good element from a very large and discrete set, and those kinds of principles are much harder to come by than principles which let you choose a special element from a continuous set.

9. **Who**
   December 9, 2005

   christine, before people start discussing the "landscape" I want to make one point of information about the 23rd Solvay CONFERENCE. It was myopically organized, to a disappointing extent I think. Ostensibly, the conference was about research areas where the non-string quantum gravity community has been especially successful recently —and by contrast little of significance has been happening in string.

   When this was realized, it may well have left a residue of embarrassment—-either in David Gross himself or in some of the others. It would have been a better conference had it been less parochial. I will illustrate how 23rd Solvay shot itself in the foot.

   To see the program follow links at:

   http://tena4.vub.ac.be/23Solvay/qsst/

   The subject was the
   QUANTUM STRUCTURE OF SPACE AND TIME

broken down into several main foci of discussion
each with a "rapporteur" and a panel. I will list the rapporteur
for each of several main topics:

A. singularities (Gibbons)
B. mathematical structures (Dijkgraaf)
C. emergent spacetime (Seiberg)
D. cosmology (Polchinski)

the page I mentioned has links to lecture notes for three of the four rapporteur talks—-A. C. and D. Gibbons, Seiberg, Polchinski
I was impressed by how insubstantial these lecture notes were, and how much they missed by way of active research and recent results in these areas

In each of these three cases, significant advances are being made outside string, to which the rapporteur does not refer.

A. in the case of singularities the removal of the black hole and cosmological singularities in the Loop context, by Martin Bojowald and others should have been highlighted (in any but a narrowly string-centered context) and apparently was not even mentioned

C. as for emergent spacetime, that has been exactly the point of recent work by Loll and others—the emergence of a 4D spacetime in Causal Dynamical Triangulations. In fact it was surprising that Loll was not on hand.

D. as for cosmology, I assume this means Quantum Cosmology since the overall subject of the conference was the quantum structure of space and time. Abhay Ashtekar, who was at 23rd Solvay, could well have pointed out the rapid growth in activity in Loop Quantum Cosmology, with connections beginning to be made with observational astronomy. But quite possibily he would not have done so in public, since the organization made it awkward. Ashtekar was not included on the cosmology panel. In fact there was no LQC representation on the cosmology panel at all! It consisted of Banks, Guth, Kachru, Kallosh, Linde, Steinhardt, Weinberg! I see that not as a recipe for quantum cosmology discussion, but instead for speculative talk about eternal inflation, bubbles, multiverses, and the landscape.

maybe some discouraging noises in the Solvay wake is due to unwise organization, I'm thinking.

10. **Not a Nobel Laureate**
December 10, 2005

Radioactivity was an experimental observation that no one knew how to explain in 1911.

What experimental data requires a string theory explanation?

Physics is still an experimental science.

Deduction by pure reason is the domain of meta-physics or, in the case of strings, Ubian pata-physics.

11. **secret milkshake**
December 10, 2005

M-theory => Marshland Monstrosity

12. **Aaron Bergman**
December 10, 2005

Physics is still an experimental science.

Would that we could do experiments that probe quantum gravity.

13. **D R Lunsford**
December 10, 2005

Would that we could do experiments that probe quantum gravity.

What would we learn? I don't think anyone can realistically formulate what you would measure.

Sometimes I'm amazed at how bright people such as you can defend these labyrinthine thought constructions. No successful theory of anything was ever so complicated. On the contrary, the good theories like Dirac and GR are simple and involve a bare minimum of structure. Isn't it just intuitively obvious that when things get so complicated, it's time to try something else? Even if you choked some kind of answer, even a wrong one, from such a theory, who would be happy?

-drl

14. **Christine Dantas**
December 10, 2005

Thank you all for the instructive comments on my landscape question an to "Who" for the important comments on the 23rd Solvay conference.

All the best,
Christine

15. **FP**
December 10, 2005

> we could do experiments that probe quantum gravity

Let's assume that somebody can show that the Pioneer anomaly is real and
measure it exactly.
How would this help string theorists? (And how would this help LQG?)
As far as I know "the only consistent theory of quantum gravity" cannot estimate
deviations from Newton's potential.

16. **Not a Nobel Laureate**

December 10, 2005

NNL: Physics is still an experimental science.

AB: Would that we could do experiments that probe quantum gravity.

NNL: Since we can't then what's the point of constructing theories that can't be falsified, be they stringy or loopy.

17. **Aaron Bergman**
December 10, 2005

Since we can't then what's the point of constructing theories that can't be falsified, be they stringy or loopy.

Because they might be right? I don't know about loopiness, but we've also learned a lot of things about susy gauge theories and mathematics from the study of string theory, too.

18. **Anonymous**
December 10, 2005

Peter,

Lubos Motl is a clown, and a rather pathetic one at that. That much is obvious to anyone who's had a glance at his blog. What I don't understand is why you are so concerned about his silly tirades and opinions (e.g., the update to this post). If he has'nt anything substantiative to say, why not just let him rot in this own dimensions?

Just curious.

-Anon.

19. **woit**
December 10, 2005

Anonymous,

Sometimes I think Lubos's antics are just hilarious, and don't want anyone needing entertainment to miss them, thus the update to the post. OK, this is not very high-minded behavior on my part, I admit it.

20. **Anonymous**
December 10, 2005

I thought I would share this link demonstrating that even if the older members of the string community are becoming more pessimistic about the current incarnation of string theory, younger people certainly aren't. The website:
http://www.interactions.org
Has an article written by a grad student describing the field to a lay person:
"String theory a very exciting prospect for physicists"
http://www.theithacajournal.com/apps/pbcs.dll/article?AID=/20051207/LIFESTYLE18/512070307/1025

21. **D R Lunsford**
December 10, 2005

In the day, we usually had (or taught ourselves) a course in theoretical physics (Sommerfeld, Landau, etc.) which included not only QM and relativity, but things like hydrodynamics, elasticity, kinetic theory of gases, optics, etc. etc. I doubt this student has much acquaintence with these things, and therefore he is not in a position to know what he's missing with ST.

-drl

22. **Aaron Bergman**
December 10, 2005

younger people certainly aren't

Like older people, there's a wide array of opinions among younger people.

23. **Chris Oakley**
December 10, 2005

Another interesting point you might have heard about is that string theory predicts that there are really ten dimensions — the three big spatial ones and time, which we are used to, and six more tiny dimensions.

This is a bit like saying, "I'm a qualified brain surgeon: all I need to do is to get into medical school and study for seven years"

24. **Not a Nobel Laureate**
December 10, 2005

NNL: Since we can't then what's the point of constructing theories that can't be falsified, be they stringy or loopy.

AB: Because they might be right?

NNL: The odds of finding the right theory be it string or loopy or something entirely different in the space of all possible theories is vanishingly small – the history of pure reason is the history of dead ends.

If some people want to pursue this fine – they should, but it is inappropriate that it should come to dominate theoretical HEP.

AB: I don't know about loopiness, but we've also learned a lot of things about susy gauge theories

NNL: Great. Now what's the experiment evidence for SUSY?

AB: and mathematics from the study of string theory, too.

NNL: That's also great. And it reinforces why strings belong in the math dept not the physics dept – until they can predict some physical phenomena.

25. **Aaron Bergman**
December 10, 2005

The odds of finding the right theory be it string or loopy or something entirely different in the space of all possible theories is vanishingly small – the history of pure reason is the history of dead ends.

Like GR?

If some people want to pursue this fine – they should, but it is inappropriate that it should come to dominate theoretical HEP.

It doesn't. There are more posts on hep-ph than on hep-th. String theory, for better or worse, just gets most of the press. In the future, there's going to be even more of a trend towards hep-ph jobs. You can see that happening right now.

Great. Now what's the experiment evidence for SUSY?

SUSY for years was impressive in that it provided a dark matter candidate, unified the couplings and made things more natural. It's not looking as pretty as it once did these days, but we'll see in a few years.

That's also great. And it reinforces why strings belong in the math dept not the physics dept – until they can predict some physical phenomena.

Who cares what department it's in?

26. **Not a Nobel Laureate**
December 10, 2005

NNL: The odds of finding the right theory be it string or loopy or something entirely different in the space of all possible theories is vanishingly small – the history of pure reason is the history of dead ends.

AB: Like GR?

NNL: Thanks. I was expecting this reply.

1. The validityof inertial and gravitational mass as physical concepts and the WEP were already established by experiment – although Einstein states that he was unaware of the Eotvos experiement until later.

The concept of strings is not established experimentally.

2. Even if we allow that you're right, GR is atypical of the way the progress in physics is usually made:

Technical advance in instrumentation -> observation of new and unexplained phenomena -> candidate theories -> new predictions -> experimental tests.

27. **dan**
December 11, 2005

before we write off string theory shouldn't we wait for LHC to come online?

I think SUSY ans SUSY-m-theory are important research programs for *physics* in that they help
1- encourage investments into particle accelerators, including LHC
2- tell experimentalists what sort of particles to look for (Higgs, extra dimensions, SUSY-particles)

if LHC does find SUSY-particles such as the neutralino, such a finding would strongly support the string theory research program.

as difficult as the landscape problem presents, m-theory may be worth pursuing if LHC finds evidence of SUSY particles and/or higher dimensions.

if LHC does not find SUSY-particles or extra dimensions, i would agree with Woit we should more aggressirvely pursue other research programs, including LQG.

28. **Chris W.**
December 11, 2005

(Taking off from NNL's comment —)

Both special relativity (SR) and general relativity (GR) are atypical, inasmuch as they were motivated in Einstein's mind by *facts about certain relations of existing theories to well-established experimental observations and to each other*.

In the case of GR the relevant fact was the apparent equivalence of gravitational and inertial mass, which was relied upon but not explained in the formulation and application of Newton's theory of gravitation. The significance of this fact was amplified for Einstein by Ernst Mach's acute analysis of the difficulties that universal gravitation created for any effort to give operational meaning to the idea of inertial motion.

In the case of SR the relevant fact was the equivalence of the currents induced in a wire (eg, in the form of a coil) when (1) the wire was held stationary relative to the laboratory and a magnet was moved near it, and when (2) the magnet was held stationary and the wire was moved such that the relative velocity of wire and magnet was the same in the two situations. The measured and calculated currents (in Maxwell's electrodynamics) were known to be the same in the two situations, but the description of the two situations in Maxwell's theory as it was understood at the time differed markedly. The significance of this fact was amplified in Einstein's mind by the *apparent theoretical possibility*—given Newtonian kinematics—of overtaking an electromagnetic wave and rendering its oscillation stationary in the observer's frame of reference.

Other physicists of the time could have responded to both of these concerns this way: "Well, *so what?* What observation is being contradicted here? I don't see any real difficulty, certainly not one that justifies reconsidering the foundations of our science, or the assumptions behind our current investigations into the properties of the luminiferous aether."

The point in the case of SR is that the theoretical issue restated here was as much or more of a preoccupation for Einstein than any of the specific observational oddities that were perplexing his contemporaries. They were concerned with specific discrepancies between theoretical expectations and observation. He was concerned with what he saw as a *deficiency in explanatory power in situations where no specific discrepancy between calculated consequences and corresponding observations was at issue*.

His genius lay in the fact that he had latched onto genuine problems within the overall theoretical framework of classical physics that had not been clearly seen before, proceeded to resolve them, and in so doing showed that observational anomalies already known could be understood and resolved.

This echoes our current predicament in essential respects. The fact that string theory in its early formulations did not follow directly from supporting observations is beside the point. (We should be so lucky…) We *do* have fundamental inconsistencies within the overall structure of late 20th century theoretical physics, and we do have observations and experimentally determined quantities which play a fundamental role in our theories but which have no deeper explanation. Our problem, as it was for Einstein, is to understand these difficulties, see how they can be resolved, and see how the resolution can be tested, ie, how it manifests itself in the world we now observe or might observe if we are sufficiently clever.

There is no guarantee that this can be done in a way that doesn't amount to an epistemological capitulation, an abandonment of essential methodology of science—that we find things out by having ideas and trying very hard to see where they might fail as well as where they succeed. To do this we must have ideas that are *not* designed at the outset to be immunized against empirical failure. Such immunization must itself be regarded as a failure; this point is what this weblog has become largely about. Einstein had a profound faith that one could adopt this attitude and yet not be hopelessly stymied in understanding the world.

29. **D R Lunsford**
December 11, 2005

Chris W makes excellent points, and just to emphasize, GR was no more (or less) a product of "pure reason" than was the Dirac equation, and it fit in nicely with the historical development in its time, no matter how surprising the form*. Antimatter and horizons are concepts that *emerged* from the two formalisms – they were not inputs. In contrast, string theory *puts in by hand* an assumed form of matter. To make an analogy, one cannot derive the Dirac equation from the existence of antimatter, because the Klein-Gordon equation also leads to the same idea. String theory is always trying to derive what we already know with an assumed form of matter, which is backward – the traditional way is to model what we don't know as competently as possible and see what new ideas emerge, which inevitably correspond to real facts.

(markus: that is an intriguing idea 🙂

30. **Aaron Bergman**
December 11, 2005

The program of quantum gravity — not just string theory — is certainly an act of hubris. That nobody has made it very far in twenty-five years is humbling. The hope of everyone in string theory, for years, was that it would uniquely predict our world. At best, in now seems more likely that string theory has a huge multitude of vacua, and the best hope of obtaining predictions would be to overconstrain the particular vacuum we live in or discover some way to probe the universal aspects of the space of vacua.

I don't know if that's possible. One can argue aesthetics all night, but string theory's still the only idea out there that has derived a black hole entropy. Until we've got some experimental data, we're stuck with our hubris. It's either that or go work on something with the prospect for data — which a lot of people are doing these days.

31. **FP**
December 11, 2005

Aaron,

> Until we've got some experimental data, we're stuck with our hubris.

I have asked this question before. How would experimental data help you (or any other string theorist) ?

E.g. If the Pioneer anomaly is real and we could measure it exactly,
how would this help with superstrings (or LQG) ?
You cannot calculate corrections to the Newton potential and thus you cannot decide if the Pioneer anomaly confirms or falsifies string theory.

I am just using this as an example, the same would be true for any other (gravitation) experiment.
What if Gravity probe B data show a small (or large?!) deviation from GR? How would a superstring theorist react to such news?

32. **Arun**
December 11, 2005

Chris W., thank you!

FP, if the Pioneer anomaly is real, and cannot be accomodated in Einstein's GR or similar geometric idea, then the idea of gravity as the effect of the geometry of space-time may be overthrown. Who knows what will happen to blackholes and their entropy? The idea of space-time fluctuations at the Planck scale, etc., may be discarded and so on. The correct classical theory of gravity is an essential ingredient to a theory of quantum gravity and thereby to string theory as well. For instance, whatever the corrected theory of gravity turns out to be, it had better show up in the string theory derivation of the consistency condition on the background space-time that looks like Einstein's equations.

33. **Aaron Bergman**
December 11, 2005

I have asked this question before. How would experimental data help you (or any other string theorist) ?

It depends on the type of the experiment. If one could (unlikely) directly probe high energy quantum gravity, that would be immensely useful.

Low energy things, on the other hand, are much more difficult to exploit. The cosmological constant, for example, is a bit of low energy experimental data that has drived people to ~~despair~~ anthropic explanations.

Nonetheless, it's still better than nothing. And the more stuff we have, the more likely it is that someone might be able to guess some unifying theory.

34. **FP**
December 11, 2005

Arun,

> The correct classical theory of gravity is an essential ingredient to a theory of quantum gravity and thereby to string theory as well

I always understood it the other way around. The quantum theory of strings, or
M-theory if you will, contains necessarily a theory of gravitation and there is no freedom to begin with different ingredients …

35. **theon**
December 13, 2005

Did anybody question whether we really have to quantize gravity?
If not, then a lot of the current problems need no answer.

36. **Aaron Bergman**
December 13, 2005

Did anybody question whether we really have to quantize gravity?

Yes. It's very hard to write down anything that makes sense that way. Jacques talked a bit about it here.

37. **CapitalistImperialistPig**
December 13, 2005

Question especially for Peter and Aaron (or anyone else who knows): I have been hearing for many years some variation on "we don't really know what String Theory (or sometimes M-Theory) is." Clearly string theorists know how to compute a lot of things, so what do they mean by that? If it's not the theory of these little string like things, why not?

Is some key conceptual or dynamical piece still missing? If so, isn't it likely that discovering it might simplify landscape, swamp, etc.?

38. **woit**
December 14, 2005

CIP,

Basically what is understood is "perturbative string theory", which is conjectured to be a series expansion of some underlying "non-perturbative string theory, AKA M-theory". Many string theorists certainly hope that when M-theory is finally understood, it will solve the problems currently plaguing string theory. But some things are known about M-theory, and what is known gives no evidence at all for this to be true.

39. **Aaron Bergman**
December 14, 2005

If it's not the theory of these little string like things, why not?

Damned if I know.

Really, it's not at all clear that we understand what the fundamental degrees of freedom actually are in string theory. We have control over a few things like AdS/CFT and a perturbation expansion, but beyond that, it's a lot of patchwork approximations.

It'd be cool if understanding all this stuff got rid of the multitude of vacua — Tom Banks has argued that many of the approximations break down for subtle reasons having to with asymptotics in cosmology — but it's hard to say that it's likely.

You ask if some key conceptual piece is still missing. I think, even after who knows how many 'revolutions', we still understand next to nothing. The anthropic and string phenomenology types get a lot of the press, but there's still a whole bunch of us who would prefer to understand just what string theory is before trying something as audacious as applying it to the real world.

Of course, that's hard, so I'm just working on trying to understand what a quiver gauge theory on a D-brane is….

40. **Who**
December 14, 2005

Theo asked a good question.

Did anybody question whether we really have to quantize gravity?

there is a thread at PF about this question, in case anyone has ideas about why it could be useful to quantize gravity.

http://www.physicsforums.com/showthread.php?p=856677#post856677

41. **Wolfgang**
December 14, 2005

> Did anybody question whether we really have to quantize gravity?
Yes of course. And the answer is, yes we do have to quantize gravity,
as explained e.g. by Jacques Distler (the part about): http://golem.ph.utexas.edu/~distler/blog/archives/000639.html

Princeton Center for Theoretical Physics

December 9, 2005
Categories: Uncategorized

Princeton University has just announced the formation of a new Princeton Center for Theoretical Physics, to be led by Curtis Callan (who was my thesis advisor).

Interestingly, the concept of this new center seems to be to move away from Princeton's traditional emphasis on particle theory (and more recently, string theory) as the central topic of theoretical physics, in favor of a much broader concept, bringing "together faculty, postdoctoral fellows and students from science departments across campus to study topics ranging from the Big Bang to quantum computing to evolution."

In recent years Callan has been spending much of his time working on biology, and the idea seems to be for the center to encourage this sort of work by theoretical physicists in other disciplines. Callan says:

"A motivation for the center is the growing realization that some very exciting challenges in theoretical science arise when we ask what theoretical physics can do to help comprehend the new phenomena and enormous amounts of high-quality data that other disciplines are now producing."

"In discussions among a group of faculty over the last year, we came to the conclusion that Princeton is remarkably well-placed to foster such developments: It is a leader in theoretical physics and it has an unusual number of faculty in other departments — including chemistry, engineering, molecular biology and genomics — who are trained in theoretical physics," he said. "The purpose of the center is to create a framework in which these people can work together to expand the boundaries of theoretical science."

The associate director will be cosmologist Paul Steinhardt, and the other faculty associated with the center include condensed matter theorists Ravindra Bhatt and Shivaji Sondhi, string theorist Igor Klebanov, astrophysicist David Spergel, biophysicist William Bialek, and materials scientist Salvatore Torquato. The center will open in the fall of 2006 with thematic programs in cosmology and quantum computation starting in 2007.

Across town at the IAS they're not branching out into other subjects but sticking to pure string theory, recently announcing that next year's Prospects in Theoretical Physics summer program will be devoted to training graduate students and postdocs in string theory.

Comments

1. **Aaron Bergman**
   December 9, 2005

   They're putting the thing *inside* Jadwin?

   That's not very nice.

2. **anon**
   December 9, 2005

   Will they recruit Lubos to research climate change? 🙂

3. **secret milkshake**
   December 9, 2005

   Only if they desired to bring about a precipitous change in research climate there… (Seriously, I do think that LuMo should start doing something with his Misanthropic Principle.)

4. **Dave Bacon**
   December 9, 2005

   Woot! Quantum computing! This coming from theoretical physics? Woot!

5. **andy**
   December 9, 2005

   What if the chemists already have their own theorists?

6. **secret milkshake**
   December 10, 2005

   Chemistry theorists: Only theorists that work in physics-related part of chemistry (in applications like such as material science or NMR spectroscopy) are realy helpful. Chemistry is run by experimentalists – because they know about chemicals and theorists do not. Study of any real problem very quickly gets out hand from the theory standpoint. You can use physicist insights to explain something after observing it but problems in chemistry and biology (and generaly in any complex system) are pretty impossible to deal with from first priciples.

7. **anon**
   December 10, 2005

   Maybe Princeton will find an application of SuperString Theory to Chemistry!

8. **biophysics**
   December 10, 2005

   secret milkshake – you are definitely correct that complex systems are pretty impossible to deal with from first principles in the sense that it's pretty useless to write down a Hamiltonian and then expect to describe the rich dynamics. But I think the programme (in biophysics as I understand and practise it at least) is more toward phenomenological model building to start with and then go back and see if the models point toward some kind of overarching framework. In particular (again for me) it will be interesting to see whether good quantitative models in biophysics will fulfill the promise of research into self-organisation, etc. A lot of beautiful mathematics from nonlinear dynamics seems not to have lived up to its hype vis a vis applications to physics but perhaps the search for new physics in biology will be the place where they shine. In fact I'm guessing that is what Princeton is betting on (of course as my pseudonym indicates I'm betting my career on this as well 🙂).

9. **Richard**
December 10, 2005

I would definitely welcome more involvement by physicists (and mathematicians) with biology, especially regarding molecular biology. Having been born with a flakey immune system, and done a lot of reading about the immune system, I realize how amazingly complex it is (probably more complex than the brain) and how difficult it is to model its dynamics with its very large array of cell types with complex functions, feed forward and feed backwards chemical signaling via a large number of cytokine types, and genetic variations. Understanding the immune system is a highly nontrivial problem that needs very bright people. And a related and worthwhile pursuit would be attempting to understand the relation and interaction between the immune system and evolution. This is something I've never noticed anyone discussing, but I think it could be fascinating.

10. **Chris W.**
December 10, 2005

Richard,

From the May 2005 addendum to the biography of **Gerald Edelman** on NobelPrize.org:

> Dr. Edelman has made significant research contributions in biophysics, protein chemistry, immunology, cell biology, and neurobiology. His early studies on the structure and diversity of antibodies led to the Nobel Prize for Physiology or Medicine in 1972. He then began research into the mechanisms involved in the regulation of primary cellular processes, particularly the control of cell growth and the development of multicellular organisms. He has focused on cell-cell interactions in early embryonic development and in the formation and function of the nervous system. These studies led to the discovery of cell adhesion molecules (CAMs), which have been found to guide the fundamental processes by which an animal achieves its shape and form, and by which nervous systems are built. *One of the most significant insights provided by this work is that the precursor gene for the neural cell adhesion molecule gave rise in evolution to the entire molecular system of adaptive immunity.*
>
> Most recently, he and his colleagues have been studying the fundamental cellular processes of transcription and translation in eukaryotic cells. They have developed a method to construct synthetic promoters and have also been able to enhance translation efficiency by constructing internal ribosomal entry sites of a modular composition. These findings have rich implications for the fields of genomics and proteomics.
>
> Dr. Edelman has formulated a detailed theory to explain the development and organization of higher brain functions in terms of a process known as neuronal group selection. This theory was presented in his 1987 volume *Neural Darwinism*, a widely known work. Dr. Edelman's continuing work in theoretical neuroscience includes designing new kinds of machines, called recognition automata, that are capable of carrying out tests of the self-consistency of the theory of neuronal group selection and promise to shed new light on the fundamental workings of the human brain. A new, biologically based theory of consciousness extending the theory of neuronal group selection is presented in his 1989 volume *The Remembered Present*. A subsequent book, *Bright Air, Brilliant Fire*, published in 1992, continues to explore the implications of neuronal group selection and neural evolution for a modern understanding of the mind and the brain. His book published with Giulio Tononi, entitled *A Universe of Consciousness: How Matter Becomes Imagination*, presents exciting new data on the neural correlates of conscious experience. In his latest book, published this year, entitled *Wider than the Sky: The Phenomenal Gift of Consciousness*, Dr. Edelman offers a model of the biology of consciousness.

11. **Juan R.**
December 11, 2005

The Princeton NEWS read,

> This is an exciting venture for the University that we believe will be immediately recognized as a site where some of the most important theoretical work around the world is being done

This sound a bit to "Santa Fe Institute". We still are waiting for some useful science from there!

Or maybe sound a bit to "string theory is the last formulation of nature. Everything will be understood from string theory."

I am seeing that by "interdisciplinary" methodology Princeton means

joint many people in a single room and wait

As proved by "Santa Fe Institute" and explained next this does not work for obtaining "new physics".

biophysics said,

> secret milkshake – you are definitely correct that complex systems are pretty impossible to deal with from first principles in the sense that it's pretty useless to write down a Hamiltonian and then expect to describe the rich dynamics.

This is a myth! There is not Lagrangian or Hamiltonian description! this is one of billion of scientific reasons that string theory always was a wrong approach to theory of everything (see "String theory is not a TOE" in sci.physics.strings).

The literature on the topic is huge and i cannot cite here, but see below some basic works and references cited therein.

I am talking of real science. NOBODY studies electron transfer in biological systems, for instance, using a Hamiltonian approach. In fact, **standard** approaches as Redfield-like or Lindblad-like one are not based in Hamiltonian approaches.

Lindblad approach is axiomatic and NOT derived from Hamiltonian dynamics. I have updated a xml page [http://www.canonicalscience.com/en/researchzone/time.xml] with last tendencies on the topic. Since it is not accesible to old browsers i add next some relavant literature cited.

Prigogine, Ilya. The End of Certainty: Time, Chaos, and the New Laws of Nature; The Free Press: New York, 1997.

Wang, G. M; Sevick, E. M; Mittag, E; Searles, D. J; Evans, D. J. Phys. Rev. Lett. 2002, 89(5), 050601/1–050601/4.

González-Álvarez, Juan R. Has thermodynamics been violated in the quantum domain? In press.

Zwanzig, Robert. Nonequilibrium statistical mechanics; Oxford University Press, Inc: New York, 2001.

Eu, Byung Chan. Nonequilibrium Statistical Mechanics, ensemble method; Kluver Academic Publishers: Dordrecht, 1998. In Fundamental theories of physics; van der Merwe, Alwyn (Ed.); vol 93.

In Chan monograph *is clearly explained that Hamiltonian/Lagrangian equations are not sufficient* and a new equation (Eu equation) explaining experimental data (that Schrödinger or classical Hamiltonian cannot explain) is postulated as a complement to “old” physics.

Eu equation (10.14) has the formal structure

drho/dt = (H + B) rho

H is Hamiltonian dynamics.

B is a new dynamics!

This expliting is formally similar to Prigogine one (see eq 7.19 on http://nobelprize.org/chemistry/laureates/1977/prigogine-lecture.pdf) but experimentally more useful and theoretically sound that early Prigogine approach.

That is NEW exciting physics!

String theory is garbage (Note: some particle-string theorists are attempting to expand string theory via that new physics. See arXiv:hep-th/9406016) But their approach is tecnically wrong and lof ot advanced math proves that contains lot of wrong stuff, even if eq (45) ‘looks’ as Eu approach. Equation 37 was incorrectly copied!

biophysics said,

> perhaps the search for new physics in biology will be the place where they shine.

Precisely, on my study of biological systems via Keizer modelling (eggs metabolism, membrane fluctuation currents in Ca^2+ transport in muscle membranes, etc.) i obtained one of first formulations of nanothermodynamics [*].

New physics, experimentally verifiable and contradicting some last wrong papers on PRL and PRE!

Juan R.

Center for CANONICAL |SCIENCE)

[*] CPS: physchem/0309002 and references cited therein. The work is freely HTML-accesible via goggle Scholar, but it renders incorrect. A new extended version is in press.

12. **anon**
December 11, 2005

This is very bad news. Princeton monopolized and politicized Plasma Physics and destroyed it, exorcizing all honesty from it. It did the same thing with Theoretical High Energy Physics, spreading its superstring minions throughout all American universities. And now, Quantum Computing is in its sights. Yikes! Run for your lives

13. **Dissident**
December 11, 2005

Secret Milkshake, I guess LuMo was just scooped on a promising (?) application of the Misanthropic Principle:

http://arxiv.org/abs/astro-ph/0512204

😉

14. **Richard**
December 11, 2005

Chris,

> One of the most significant insights provided by this work is that the precursor gene for the neural cell adhesion molecule gave rise in evolution to the entire molecular system of adaptive immunity.

If so, this is very interesting! Thanks!

While walking in the snowy woods with my dog today, I was thinking about related issues. There does seem to be some tight relationships between the nervous system and the immune system. The brain (especially the hypothalamus) is highly responsive to cytokines emitted by immune cells, and we have even discovered neural receptors on immune cells. One of the most difficult problems in immunology has been the self/non-self issue; that is, how do immune cells distinguish between tissue which is myself and that which is not myself. We only partially understand how the immune system is self-regulated so that it is aggressive enough to disable invaders but controlled enough so that it doesn’t begin attacking ourselves or overwhelming us with inflammation. [All of this breaks down with autoimmune diseases, of course, such as Lupus. And ironically, some people with immune deficiencies also have autoimmune diseases.] At any rate, it occurred to me that there could be (at least weakly) analogous issues with the brain. There must be a problem of self/non-self there, and at the actual neuronal system level, not just a philosophical issue. Well, my brain started spinning even further, and began to wonder how fundamental physics could be approached from a kind of self/non-self viewpoint. Is there any analogy there to what I’ve been talking about with biological systems? A quiet walk in the woods with your dog is conducive to free association! Now, back to math.

15. **Dave Bacon**
December 12, 2005

“And now, Quantum Computing is in its sights. Yikes! Run for your lives ”

Resistance to our factoring machines is futile!

Wilczek Goes Anthropic

December 12, 2005
Categories: Uncategorized

A few weeks ago one Nobel prize winner put out an article promoting the idea of adopting anthropic reasoning as a new paradigm of how to do theoretical physics. More recently another Nobelist, Frank Wilczek, has to some degree followed suit. Wilczek is one of four authors on a new paper entitled Dimensionless constants, cosmology and other dark matters which first appeared on the arXiv November 29th, then in a slightly revised version on December 8. The other authors are Tegmark, Aguirre and Rees, with Tegmark's name appearing first indicating it's more his work than that of his co-authors.

I wasn't sure quite what to make of this paper when it first came out, especially how much it reflected Wilczek's own point of view on anthropism. Last Friday I attended talks by Wilczek and Tegmark at the 6th Northeast String Cosmology Meeting organized by the Institute for Strings, Cosmology and Astroparticle Physics here at Columbia.

Wilczek's talk was entitled "Enlightenment, Knowledge, Ignorance, Temptation". He explained that these corresponded to categorizing parameters of physical theories according to whether life depended on them or not and whether we have a good idea for what determines them or not. Choosing the two possible answers to these two questions gives four cases:

Enlightenment: Parameters that life depends on, and we think we have a good idea about what determines them. Here his example was the proton mass, very small on the Planck scale, but we think we know why: logarithmic running of coupling constants.

Knowledge: Parameters that life doesn't depend on, and we think we have a good idea about what determines them. One example he gave was strong CP violation, which is irrelevant to life, but very small, perhaps because of axions.

Ignorance: Parameters that life doesn't depend on, and we don't have a good idea about what determines them. This includes most of the standard model parameters, as well as just about all parameters in theories that go beyond the standard model.

Temptation: Parameters that life depends on, and we don't have a good idea about what determines them. The examples he gave were the electron and up and down quark masses.

He said that his talk would concentrate on "Temptation", the temptation being that of using anthropic argumentation. He noted that David Gross believes this is a dangerous opiate, causing people to just give up instead of really solving problems. The one anti-anthropic point he made was to put up a graphic showing agreement of the lattice QCD spectrum calculations with experiment, saying the lesson was that sometimes real calculations turned out to be possible even though people had at times doubted this. So one should try and "limit the damage", not go wild and use anthropics inappropriately, trying to save as much beautiful physics as one can even when anthropic reasoning is forced on us.

The rest of his talk though showed a significant amount of enthusiasm for the new anthropism. He referred to people like his co-author Rees who have been promoting the anthropic point of view for years as "unhonored prophets". Given the paucity of experimental data relevant to explaining where things like standard model parameters come from, he said that at least anthropics gives lots of new questions so one has something to do when one gets up each day which might be fruitful. He attacked the idea of using "pure thought", without consulting the physical world, saying this hasn't worked, not 20 years ago, not now, not in the future. I presume he had string theory in mind when he said this, noting out loud that it might annoy some people in the room.

The main idea about anthropics he was trying to push is that anthropic calculations were "just conditional probability", making much of the equation

$f(\mathbf{p})=f_{prior}(\mathbf{p})f_{selec}(\mathbf{p})$

for the probability of observing some particular value **p** of parameters, given some underlying theory in which they are only determined probabilistically by some probability distribution $f_{prior}(\mathbf{p})$. The second factor $f_{selec}(\mathbf{p})$ is supposed to represent "selection effects", and it is here that anthropic calculations supposedly have their role. In the paper the authors argue that "Including selection effects is no more optional than the correct use of logic". The standard way physics has traditionally been done, one hopes that the underlying theory determines **p** (i.e. $f_{prior}(\mathbf{p})$ is a delta-function), making selection effects irrelevant in this context. The authors attack this point of view, writing:

to elevate this hope into an assumption would, ironically, be to push the anthropic principle to a hedonistic extreme, suggesting that nature must be devised so as to make mathematical physicists happy.

At no point in his or Tegmark's talks, or anywhere in their paper, do they address the central problem with the anthopic principle, that there's a huge issue about whether you can get falsifiable predictions out of it, and thus whether you're really doing science. In this context, the nature of the problem is that if $f_{prior}(\mathbf{p})$ is not peaked somewhere but is flat (or more or less flat), then everything just depends on $f_{selec}(\mathbf{p})$, but if you calculate it anthropically, all you are doing is seeing what you can conclude from known laws of physics and the fact that we exist. In the end what will come out of this kind of calculation is some probability distribution that better be non-zero for the values of the parameters we observe, otherwise you've done the calculation wrong.

There is a particular sort of physical model one can hope to falsify this way. If one assumes our universe is a randomly chosen point in a "multiverse" of possibilities, and looks at an observable that is supposed to have a more or less flat probability distribution in the ensemble given by the multiverse, then one can argue that we should be at some region of parameter space containing the bulk of the probability in the anthropically determined $f_{selec}(\mathbf{p})$, not far out in some tail where the probability distribution is vanishingly small. There are plenty of examples of this already. The proton lifetime is absurdly long compared to bounds from anthropic constraints, so any model of a multiverse that doesn't have some structure built into it to generically sufficiently suppress proton decay is ruled out. This includes the string theory landscape, so one of the many mysteries of the whole anthropic landscape story is why its proponents don't take their own arguments seriously and admit that their model has been falsified already. It also applies to Tegmark's favorite idea, that of the existence of a Level IV multiverse of all possible mathematical structures, an idea he also promotes in the paper with Wilczek.

Wilczek also discussed one particular axion cosmology model in which $f_{prior}(\mathbf{p})$ can be calculated. In these models one has the relation
$$\xi_c\sim f_a^4\sin ^2\frac{\theta_0}{2}$$
for the axion dark matter density in terms of the Peccei-Quinn symmetry breaking scale and the misalignment angle of the axion field at the Peccei-Quinn symmetry breaking phase transition. To make this agree with the observed dark matter density, if one assumes the misalignment angle is some random angle then the Peccei-Quinn scale has to be about 10^{12}GeV. If one wants to make the Peccei-Quinn scale the GUT or Planck scale, one has to find some reason for the misalignment angle to be very small. The proposal here is that this happens for anthropic reasons, since if the angle were not small it would cause an amount of dark matter incompatible with our existence. For these small angles the above formula implies that the probability distribution for the dark matter density caused by such axions satisfies
$$f_{\text{prior}}(\xi)\sim \frac{1}{\sqrt \xi}$$

The Tegmark et. al. paper contains an elaborate calculation of f_{selec} for the dark matter density, involving all sorts of "anthropic" considerations which goes on for eleven pages or so and involves a bafflingly long list of considerations about galaxy, star and planet formation, as well as many possible dangers that could have disrupted the evolution of life, such as disruption of the Oort cloud of comets. I'll freely admit to not having taken the time to follow this argument. The end result for f_{selec} as a function of $\sqrt\xi$ is a probability distribution with the measured dark energy corresponding to something close to the peak.

I'm not sure exactly what conclusions one can or should draw from this calculation. So many different facts about our specific universe are being folded into this that it's not clear to me that there isn't some circular reasoning going on. This is a general problem with "anthropic" arguments: if you assume that life couldn't exist if the universe was much different than it is, you smuggle all sorts of information about the way the world is into your "anthropic" calculation, after which it is not too surprising that it "predicts" the universe has more or less the properties you observe.

What we really care about in these arguments is whether they can be used to extract any information whatsoever about f_{prior}, the physics we are trying to get at. In this axion cosmology case we have a prediction for this distribution and the calculation shows this is consistent with the observed dark energy density, but as far as I can tell, all sorts of other quite different distributions would work too. So, I'm still confused about exactly what this calculation has told us about the underlying axion cosmology physics that it is supposed to address, other than that it is not obviously completely inconsistent.

Tegmark's talk at Columbia was titled "Measuring and Predicting Cosmological Parameters". The "measuring part" was a summary of some of the impressive experimental evidence for the standard cosmological model. The "predicting" part was pretty much pure promotion of anthropism, including a long section on reasons why the electroweak symmetry breaking scale is anthropic and some comments making fun of David Gross ("even he couldn't predict the distance from the earth to the sun. Laughter…"). The only actual "predictions" mentioned were the results about the axion cosmology model mentioned above and described in detail in the Tegmark et. al paper, as well as the well-known Weinberg anthropic "prediction" for the cosmological constant.

All in all, I found these two talks and the Tegmark et. al. paper pretty disturbing. They seem to me to be part of a highly ideological effort to sell the Anthropic Principle as science. The paper devotes two pages to a detailed list of standard model parameters, and makes various statements about the probability distribution function on this large number of parameters, even though it has nothing to say about almost all of them, and I think there's a strong argument that the anthropic program inherently will never have anything useful to say about most of these parameters. Many of Wilczek's remarks were more modest, but the paper he has signed his name to is highly immodest in its claims for anthropism. Together with Weinberg and Susskind's anthropic campaigns, it seems to me that more and more theorists are going to join this bandwagon. Neither Wilczek nor Tegmark are string theorists (and Wilczek is clearly somewhat skeptical about the whole idea), but there seems to be an unholy alliance brewing between them and Susskind and his followers. The only prominent person in the field standing up to this publicly is David Gross, and it is very worrying to see how little support he is getting.

Update: A preprint by Frank Wilczek corresponding to his talk last week entitled Enlightenment, Knowledge, Ignorance, Temptation has appeared. It is a contribution to the same conference as the one Weinberg contributed Living in the Multiverse to, I gather in honor of Martin Rees. Wilczek's preprint announces a "new zeitgeist", that anthropic arguments are in the ascendancy. One quite strange thing in the preprint is that he suggests an anthropic explanation for the long proton lifetime in terms of doing anthropic calculations involving future observers.

He does say there are drawbacks to the new order (a loss of precision and of targets to calculate), but on the whole he seems to embrace the new anthropic paradigm rather whole-heartedly, seeing it as a lesson in humility for those who had the hubris to believe it was possible to understand more about the universe through "pure thought."

Update: Two of the authors of the paper discussed here (Aguirre and Tegmark) wrote in with some comments that are well worth reading (as well as those from Smolin and others about his own proposal). Aguirre points to an interesting paper of his On making predictions in a multiverse (see also an earlier paper with Tegmark), which addresses some of the conceptual issues that were bothering me about this sort of calculation. It points out many of the problems with this kind of calculation, and I don't really share the author's optimism that they can be overcome.

Lee Smolin mentioned to me a somewhat related workshop that was held this past summer at the Perimeter Institute, on the topic of Evolving Laws, especially "do the laws of nature evolve in time?" Audio of the discussions at the workshop is available

Comments

1. **Aaron Bergman**
   December 12, 2005

   God, I'm so sick of this crap. And people wonder why I do math these days….

   The proton lifetime, by the way, has been often mentioned by people against anthropic reasoning. It is not alone in Wilczek's "ignorance" category. The existence of such parameters is one of the reason that Nima would like to claim that only superrenormalizable couplings are anthropically selected. It's also was some of the motivation for his "friendly landscape" paper.

   Remember, BTW, the vote in Toronto. Most people don't go for this stuff.

2. **Anonymous**
   December 12, 2005

   The anthropic arguments remind me of the ones you will encounter from people into bible codes. Typically, they have found some code, and then the calculate the probability that a book like the bible would at random produce this code. Of course, it turns out to be really small, thus "proving" divine intervention. The point is of course that they have singled out precisely the code they already found, and in any random string of letters, they might very well not have found this particular code, but they probably would have found another code, which they then would have argued was of divine origin.
   So the anthropic people have found the code that is life in the universe, and in particular the kind of life that exists here on Earth. And it seems really unlikely to them, implying not a divine origin of the universe, but an almost infinite amount of existing universes. But life, and sometimes really bizarre forms of life, are known to flourish in the most unlikely conditions here on Earth. Why not in another kind of universe? All needed is some process of replication, variation, and some kind of selection, maybe plus minus some conditions on the mutation rate, et cetera. Have the anthropic people been able to produce anything towards a proof that no kind of life is possible in some hypothetical universe? If they did, they'd at least have a methodology.
   Finally, why single out life? Why not single out the Iraq war or the act of seeing a squirrel? Or why not concern oneself with universes in which it is possible to go out and drink beer?

3. **JE**
   December 12, 2005

   It may be worth mentioning that besides a Nobelist, Wilczek is editor in chief of Elsevier's Annals of Physics. Let's just hope that his anthropic leanings do not permeate the journal's content.

4. **Fabien Besnard**
   December 12, 2005

   Apart from the points Peter mentions against anthropic reasoning I see two others. The first : anthropic reasoning necessarily begins this way : there is intelligent life–> there is life –> there is carbon chemistry. After this, one can make so-called predictions which I think are just very indirect measurements of some observables which remind me (in the best case) of the very clever way Einstein measured avogadro number thanks to the fact that the sky is blue. To call this sort of thing a 'prediction' one has to be confident in the chain of implications I mentioned above, which is very weak indeed since it is based mainly on our lack of imagination and on our very poor understanding on what is intelligence. One also has to assume the existence of a multiplicity of uninhabited universes and use the indifference principle, which is well known to lead to crazy conclusions on some occasions. For instance, suppose I am struck with sudden amnesia about who I am. I could argue that I am chinese by using this principle. However looking around me would soon provide me with clues that I'm in fact french. I feel that it's the same with anthropic argument. That is, you assume the standard model+GR is the end of the story, you vary parameters as if they were independent, knowing it will give crazy results if they happen not to be so etc… Perhaps this is the only thing to do when you give up any hope of finding a more precise theory. Except that 'giving up' is not precisely the right attitude for a scientist, I think.

5. **Haelfix**
December 12, 2005

Everyone is tired (in cosmology at least) of talking about the anthropic principle. Everytime it comes up everyone rolls their eyes and mutters something about philosophy and that its 'boring'.

When approaching a problem as a working scientist, I see no valid reason to think of fprior as anything other than a delta function. If I can't calculate something, I then assume its either b/c im stupid or b/c I have insufficient information about the system, such that it might naively appear that fprior is smeared out into a nontrivial ensemble.

Historically the amounts of time a physics problem did not involve a fprior delta function and instead was something else is vanishingly small compared to the times that there is a pure deterministic or selection effect at play.

So it seems to me, from a historical perspective, this is just another example of people jumping the gun and giving up too early.

And perhaps its really not so surprising is it? We are implicitly making an intellectual leap of tens of orders of magnitude between observable physics and intellectual gaming, does it astonish anyone then that there is a understanding gap, such that you could seperate unknowns away into the prior distributions?

6. **FP**
December 12, 2005

It would be interesting if anthropic reasoning could predict something we do not yet know. E.g. the exact Higgs mass.
As long as anthropics only 'predict' what we know already, the whole philosophy is not very convincing.

7. **Wolfgang**
December 12, 2005

Does anybody want to comment on this paper?
http://xxx.lanl.gov/abs/physics/0512062

From the abstract: "I make some comments about brane world scenarios and their potential to strengthen the Fermi Paradox."

In the text: "My solution to the Fermi paradox is compatible with the
speculations that some UFO's could be true alien spacecrafts."

8. **Alejandro Rivero**
December 12, 2005

The classification Wilczek does is a good idea in order to distinghish anthropicism from empiricism. To me, antropic reasonment is a trick to introduce empirical measurements (under the disguise of "life normal conditions") inside the theory.

I join Gross' objections; any ad-hoc theory is an opiate if it is not perceived as ad-hoc, and it can cause retirement from the fight. Mattews' pill killing Barrow. I was not surprised by Weinberg take on anthropicism; his book promotes Effective Field Theory, which is a lesser narcotic but with similar effects. I am not sure about Wilczek, even after reading your report.

9. **woit**
December 12, 2005

Please, Wolfgang and others, no space aliens and UFOs here. The anthropic stuff is bad enough.

10. **weichi**
December 12, 2005

Aaron,

Which crap are you sick of? Peter's crap? The anthropic crap? Wilczek's crap?

11. **Aaron Bergman**
December 12, 2005

Anthropic "just so" stories.

12. **Wolfgang**
December 12, 2005

Peter,

OK. But I think this type of paper (about the 'subanthropic principle') is the logical next step in the line of anthropic reasoning.
By the way, when will Frank Tipler and the 'Physics of Immortality' re-appear?

13. **Jason Douglas Brown**
December 12, 2005

It's disappointing that so many otherwise smart people are caving into this landscape rubbish. For one it will not ever yield one prediction whatsoever. The only real hope string theory or its new incarnation as the yet to be discovered M-theory is for it to have enough symmetry to find a unique vacuum. I am very partial to many of the ideas and discovers surrounding string theory but if I came across it now I'd think twice before putting in all the effort.

I think the reason for all this landscape crap is that string theorists have yet to make the next big discovery in superstrings. They say one comes along every ten years or so. Since AdS CFT not much has happened from a fundamental standpoint. It seems to me that string theory is foundering because we're reached an impass. String/M-theory will not move forward until the next major piece of the puzzle has been discovered. The next major step is to create a consistent second quantized covariant version of string/m-theory. String theory in the 60's gave way to QCD. Perhaps the current string theory/M-atrix theories will give way to a local field theory with enough symmetry to give a single unique true vacuum state. Or perhaps not. It really gives me the willies when some of the most intelligent men who've walked the earth invoke anthropic arguments in the context of some supposed string landscape.

14. **AJ**
December 12, 2005

JDB,

Field theories with lots of symmetry tend to have more vacua, not fewer. Very crudely, more symmetry equals more representations.

15. **Michael**
December 12, 2005

I don't understand Wilczek's categories. He claims to know that life doesn't depend on the smallness of CP? How could he possibly know this? Does it get any more pathetic than this? I am sad to see Wilczek go in this direction.

16. **Quantoken**
December 12, 2005

Jason, what makes you think that once Super String Theory is able to find a unique vacuum and make predictions, things will be OK, then? You guys have worked on the crackpot theory for too long and forget that a science theory not only needs to make predictions, but more important, make **CORRECT** predictions. The day that super string theory makes predictions will also be the day that the idea is dead. Because there is only one in 10^500 chance your "unique vacuum" will actually match the reality world. The world is 3+1 dimensions! There has been nothing suggesting anything different from that fact.

It will take a truely wrong time before super string theory will eventually be able to make a prediction at all. The **real tragedy** is by then you will realize how wrong you guys were and what a waste of time and effort it had been, to dwindle on a crackpot idea for so long, just because it was a paid day time job to research crackpots.

17. **Wolfgang**
December 12, 2005

Off topic:
Lubos just posted about Riemann's hypothesis and
states that "a proof may possibly follow from string theory".

18. **secret milkshake**
December 12, 2005

I have greatest respect for Wilczek but I don't understand where he is going with this. This material would be acceptable as a speculative afterthought chapter in a popular book or as a dinner discussion.

There was a discussion on anthropic stuff between Dyson and Gould (the biologist) many years ago and it went like this:
Dyson: "The numerical coincidences are striking. All the fine balancing in QED, for example. Universe must had expected our arrival – I have this hope, I feel this way."
Gould: "If you look on astrophysics models from the early 20th century, you can see now that the models were completely wrong. But then (as now) their authors were saying – 'what an incredible coincidence, if this aether behaved a bit different and comets were less populous, we would would not be here, no life possible, so these parameters must have been pre-arranged!' So there seems to be a strange alure to fall back on anthropism whenever we lack a real understanding."

19. **Kris Krogh**
December 12, 2005

milkshake,

I had an Earth Science teacher who gushed about how amazing the world is. If the oxygen content of the atmosphere were sligthly different, we couldn't exist. The world is perfectly tuned to our existence. Somehow it never occurred to him life existed here before oxygen.

20. **Quantoken**
December 12, 2005

Some how it never occured to any one that instead of the world being perfectly tuned for our existence, it is actually the case that instead our existence is ferfectly tuned for this world, not the other way around, just like the case of oxygen on earth.

21. **Adrian Heathcote**
December 12, 2005

Peter

Your point about the circularity of Anthropic reasoning is dead right I think. People complain about philosophy—but this is *bad* philosophy, bad logic. Interestingly I don't know of a single professional philosopher who has backed this idea.

And as for explaining anthropic reasoning using conditional probability—that is truly a risky business. There are more abuses of conditional probability out there than all of us have had hot dinners.

cheers

22. **Chris W.**
December 12, 2005

Speaking of the conversation between Freeman Dyson and Stephen Jay Gould, and the gushings of Kris Krogh earth science teacher, NPR's *Speaking of Faith* is starting a two-part series on Albert Einstein this week (tonight on some NPR stations). Guests include Freeman Dyson and Paul Davies. The latter is described as an astrobiologist.

As I write this, Dyson is saying that "nature is much more than a set of equations"—more like the rain forest surrounding a mountain peak than the pristine sterility of the peak itself. This immediately followed a turn of the discussion to Einstein's break with his contemporaries on quantum mechanics.

(For what it's worth, Dyson is on record as saying that he finds the complexity and diversity of nature more interesting than any prospect for the unification of its laws.)

23. **Not a Nobel Laureate**
December 12, 2005

Anthropic "Principle"?

One thing that is certainly unbounded is human hubris.

24. **Chris W.**
December 12, 2005

Even if one suspects that the existence of life could teach us something important about fundamental physics, it seems to me that the formulation of the Anthropic Principle as a selection effect is a woefully inadequate way to approach the problem. Given what we have learned about the basis of heredity and evolutionary change, why not ask a question like this:

Life depends for its continued existence on the fact that the laws of physics allow metastable configurations of matter, and also on the possibility of such configurations repairing and reproducing themselves, albeit with variations (errors) that produce changes of form and patterns of behavior in successive generations. To what extent does the allowed existence of such systems, *understood computationally*, constrain or dictate the laws of physics?

This problem formulation is reminiscent of questions about fundamental limits of computation, which have received [considerable attention] from workers in quantum computation and others. Of course it is by no means unexplored territory, and would require the input of some novel ideas to throw any additional light on our current problems. I don't know whether this is possible, but the current approach seems only marginally better than numerology. In fact, the main motivation for it, aside from the simple logic of selection effects, seems to be that it allows string theorists, particle physicists, and astrophysicists to continue applying familiar concepts, techniques, and arguments in this new context, unsatisfying though the results may be. That's easier than a deep reconsideration of fundamental assumptions and the development of fundamentally new ideas.

25. **Christine Dantas**
December 12, 2005

Talking about predictions (that is a strong word these days!), I am doing a survey on what quantum gravity models predict (again, the ugly word) on the GZK cutoff. I could not find (up to now, but still investigating) not even one single string theory paper solely dedicated to this problem. On the other hand, I did find at least 2 LQG papers on it (from the same authors, though):

> Alfaro, J., and Palma, G.A., "Loop quantum gravity effects on the high energy cosmic ray spectrum", hep-th/0501116.
>
> Alfaro, J., and Palma, G., "Loop quantum gravity and ultra high energy cosmic rays", Phys. Rev. D, 67, 083003, (2003). hep-th/0208193.

For those who want to know more about this issue, just go to my blog page. In there, I list today several references of interest on this matter.

What does the Anthropic Principle say about the GZK cutoff? Does the cutoff exist because we are here to observe it? Or does the cutoff not exist because we are here to not observe it? (note: I am being sarcastic here. The AP never convinced me as a valid scientific approach. It really bores me).

Best wishes
Christine

26. **Jenny Attiyeh**
December 12, 2005

Hi all,
As the blog post I'm responding to is now archived, I thought I'd weasel in on this one… I will be interviewing Lisa Randall shortly on her "Warped Passages" for my podcast program on authors, academics and intellectuals, and I'm eager for your input!
In the spirit of open-source research, I'm looking for good questions to ask her that are appropriate for an intelligent but mainstream (ie. non-scientific) audience. Please do pitch in, and of course I will credit you (and your blog) when I ask her your question(s)!
If you'd like to take a look at ThoughtCast, please go to http://www.thoughtcast.org, and thank you!
— Jenny

27. **ksh95**
December 12, 2005

Life depends for its continued existence on the fact that the laws of physics allow metastable configurations of matter
Here's something else that bothers me about the AP and similar arguments. Who says that the above is true (stable matter, carbon based life, etc). It seem to me that simple complexity may be a neccessary and sufficient condition for life….Can complexity arise in quark-gluon plasma. How about gravitational based complexity in a universe where big G is much much larger….
If life is more diverse than our carbon based examples appear, Quantokens point becomes very relevent…*.instead of the world being perfectly tuned for our existence, it is actually the case that instead our existence is ferfectly tuned for this world*…Here, the anthropic principal is even less "predictive" than before.
The point being that all of this is a never-ending discussion, built on likelyhoods, I-believe-thats, it-could-be-possilbe-thats, and what-ifs.

We would do well to remember that congress only funds nuclear missiles, semiconductors, space craft, and productivity enhancements.

28. **Kea**
December 12, 2005

Well, I guess, on the bright side: at least they're *starting* to think about logic.

29. **Dissident**
December 12, 2005

Some of us began suspecting that all's not well with Wilczek already after reading his review of "The Road to Reality". Post-Nobel stress syndrome, maybe?

30. **Michael Bacon**
December 12, 2005

Peter,

Your right that Tegmark's favorite idea – the existence of a Level IV multiverse of all possible mathematical structures – is subject to the same problems. What do you think generally about a Level III multiverse like that supposed to be underlying the physical computations performed by quantum computers?

31. **woit**
December 12, 2005

Michael,

I'm not really very fond of many worlds interpretations of QM, and not sure exactly what Tegmark has in mind here. The formulation you give "underlying the physical computations performed by quantum computers" doesn't seem to me to necessarily correspond to anything except QM itself, and I don't see the necessity of bringing the multiverse into discussions of QM.

Jenny,

An interesting question for Randall might be to ask her what she thinks about the whole "Anthropic Principle" controversy, discussed in several blog entries here recently, including this one. No need to credit me or the blog with this question.

32. **Chris W.**
December 12, 2005

[Jenny: The following might prompt some questions for Lisa. It reinforces my suspicion that higher-dimensional theories lead into a quagmire not unlike the multiverse and anthropic arguments.]

New paper on arXiv.org:

Extra symmetry in the field equations in 5D with spatial spherical symmetry (gr-qc/0512067)

> [ABSTRACT] We point out that the field equations in 5D, with spatial spherical symmetry, possess an extra symmetry that leaves them invariant. This symmetry corresponds to certain simultaneous interchange of coordinates and metric coefficients. As a consequence a single solution in 5D can generate very different scenarios in 4D, ranging from static configurations to cosmological situations. A new perspective emanates from our work. Namely, that different astrophysical and cosmological scenarios in 4D might correspond to the same physics in 5D. We present explicit examples that illustrate this point of view.

From the text:

> All these theories face the same challenge, namely the prediction of observable effects from the extra dimensions [8]. The success of this mission depends on the "correct" identification of the physical or observable space-time metric from the multidimensional one. This is not a trivial task in higher-dimensional cosmologies, like braneworld and STM, where the extra dimension is noncompact. In this scenario all the coordinates are alike, in the sense that the metric tensor is allowed to depend explicitly on the extra coordinate, usually called fifth coordinate.
> In this regard, the crucial question is: given an arbitrary 5-dimensional metric, that depends on all five coordinates, how do we decide which one is the "extra" coordinate? The answer to this question seems to be far from obvious. Even in the simple case of spherical symmetry, in ordinary three space, there are various possible options leading to different scenarios in 4D.

33. **Lee Smolin**
December 12, 2005

To Christine Dantas: What LQG implies for the GZK cutoff depends on whether the symmetry of the ground state shows broken Lorentz invariance or deformed Poincare invariance. The former is what Alfaro and Palma and other authors assume through their choice of an ansatz for the ground state. But there is increasing evidence (but no proof so far) for the latter conclusion. There is proof that Poincare invariance is deformed but not broken in 2+1 dimensional quantum gravity coupled to matter and there are heuristic arguments such as hep-th/0501091. If the ground state has deformed Poincare symmetry the expectation is that GZK threshold does not differ measurably from that of ordinary Lorentz invariant theories, as shown in several papers, including gr-qc/0312089, astro-ph/0008107 and gr-qc/0312124.

I agree its strange there are no papers making the obvious point that as string theory is constructed to be Poincare invariant it predicts that ordinary special relativity should hold to arbitrarily small distances. This would appear to be the only testsible predictions string theory is capable of making. The reason is perhaps that they would prefer to keep open the option of finding a consistent string vacuum with any symmetry that is observed experimentally.

Thanks,

Lee

34. **Michael Bacon**
December 12, 2005

Peter,

Thanks for the response. I'll take not corresponding to "anything except QM itself" as a compliment.

I guess then that you would not agree with Deutsch and others that the quantum theory of computation is the clearest and simplest language and mathmatical formilism for setting out quantum theory itself?

35. **Aaron Bergman**
December 12, 2005

I agree its strange there are no papers making the obvious point that as string theory is constructed to be Poincare invariant it predicts that ordinary special relativity should hold to arbitrarily small distances.

It's easy to break Lorentz invariance by turning on a background field.

36. **woit**
December 12, 2005

Michael,

Sorry, but I've never spent much time reading about or thinking about the quantum theory of computation (or is it the theory of quantum computation?), so I don't really have any views about it.

My views about quantum mechanics are different than some people's. I think the basic formalism itself, embodied in the path integral and in the idea of describing the world in terms of a Hilbert space, with self-adjoint operators as observables, is something closely connected to representation theory, and extremely mathematically deep. Sure, the interpretational issues that arise as one tries to understand how classical behavior emerges from the quantum formalism are very tricky, but that's no reason to search for a replacement for the QM formalism itself. There may be interesting issues arising from quantum computing, I'm just not very well informed about them.

37. **Michael Bacon**
December 12, 2005

Peter,

Thanks again. I agree that there is no reason to search for a replacement for the QM formalism itself.

38. **JC**
December 12, 2005

Peter,

(slightly offtopic)

Do you know of any references which try to put path integrals on a more mathematical rigorous footing, in the field theory context without going to the euclideanized version of the path integral?

39. **Jenny Attiyeh**
December 13, 2005

Thanks for the suggested questions — I really appreciate it. Also, I read an article about how this blog is quite a lightening rod. Hooray!!
-Jenny

40. **Aaron Bergman**
December 13, 2005

I guess then that you would not agree with Deutsch and others that the quantum theory of computation is the clearest and simplest language and mathmatical formilism for setting out quantum theory itself?

The clearest and simplest language for setting out quantum theory is quantum theory. Quantum computing is just an application of quantum theory.

Do you know of any references which try to put path integrals on a more mathematical rigorous footing, in the field theory context without going to the euclideanized version of the path integral?

Ugh. I suppose there's Glimm and Jaffe, but the impression I get from afar is that constructive field theory has been a spectacularly unsuccessful endeavor.

For the last, I suppose you could ask Lisa about how she fell off a mountain a few times, but it looks like that's actually fairly widely covered. You could ask her how she feels about top-down vs. bottom-up approaches to physics. You could ask what she feels might happen when the LHC turns on in a few years.

I actually think the middle one might be the most interesting.

41. **Adrian Heathcote**
December 13, 2005

JC

There is a talk on this at KITP site by Graeme Segal of Oxford. You can watch the entire thing (and there are not so many interruptions in this one).

Cheers

42. **Chris Oakley**
December 13, 2005

Jenny,

I do not think that you should be interviewing String theorists, or apologists for String theory at all. The public are not completely stupid and the more exposure the String theorists get the more the public will realise that their hugely speculative idea has very little chance of connecting with reality. Those who are content to pursue ideas simply because they find them mathematically intriguing, and who show little or no interest in finding experimental verification, should not be held up as examples of application of the scientific method.

Find some genuine scientists. Molecular biology, for example, is an exciting area now – I am sure that you will find much more interesting material here.

43. **Lee Smolin**
December 13, 2005

Aaron says, "It's easy to break Lorentz invariance by turning on a background field."

In standard field theories, like Maxwell, it costs energy to break Lorentz or translation invariance, which is why for these theories the vacuum is the only Poincare invariant state. Are there explicit examples in string theory where it doesn't cost energy to break Poincare invariance of the uncompactified dimensions? Also, are there consistent string vacua with deformed Poincare invariance? With Magueijo we began to investigate this and found partial results in hep-th/0401087.

Lee

44. **Fabien Besnard**
December 13, 2005

Adrian Heathcote said :

>Interestingly I don’t know of a single professional philosopher who has backed this idea.

Well there is at least Nick Bostrom who apparently is writing a paper with Tegmark. Incidentally, Bostrom also support the simulation argument which according to me is subject to the very same kind of logical problems that anthropic reasoning.

As to Tegmark and his level IV multiverse, it’s an idea I find more interesting that level 1 to 3 multiverse for the following reason : you don’t care at all about what happens in multiverse 1-3, it does not constrain you at all, not more than what happen in a fiction, whereas you do care about what “happens” in the mathematical world. Our world would be very different if you changed some math formula/theorem. This is why I think of mathematical structures as more real than parallel universes.

45. **Christine Dantas**
December 13, 2005

Dr. Smolin,

Thank you very much for your comment on the GZK cutoff issue.

Best wishes
Christine

46. **Adrian Heathcote**
December 13, 2005

FB said

”Well there is at least Nick Bostrom who apparently is writing a paper with Tegmark. Incidentally, Bostrom also support the simulation argument which according to me is subject to the very same kind of logical problems that anthropic reasoning.”

I can’t read French but I trust you to be right about this one.

Hadn’t heard of Nick Bostrum previously but having looked at some of his stuff I feel no better about the AP.

BTW a sure sign that someone is talking unmitigated nonsense with the AP is when they say such things as ”the cosmological constant’s value is *caused* by the existence of humans.” This is the most foolish abuse of the concept of causation I know. There should actually be a law against it.

47. **JKG**
December 13, 2005

Hi,

I just want to understand something about anthropic reasoning. Suppose I find that the probability of finding value of X close to what we observe is 99%. So what? Suppose I find that the probability of finding value of X close to what we observe is 10^{-100}%. Then what?

Is it that beneath this kind of reasoning is the metaphysical assumption that our universe is probable? If so how to justified this assumption?

48. **Aaron Bergman**
December 13, 2005

Those who are content to pursue ideas simply because they find them mathematically intriguing, and who show little or no interest in finding experimental verification, should not be held up as examples of application of the scientific method.

And what exactly makes you think that Lisa Randall fits this description?

Are there explicit examples in string theory where it doesn’t cost energy to break Poincare invariance of the uncompactified dimensions?

I was thinking of the D-branes with a Moyal product which is a limit of a background with a B-field.

Also, are there consistent string vacua with deformed Poincare invariance?

Not that I know of. Some people have thought about q-deformed de Sitter symmetries, though.

49. **Chris Oakley**
December 13, 2005

Those who are content to pursue ideas simply because they find them mathematically intriguing, and who show little or no interest in finding experimental verification, should not be held up as examples of application of the scientific method.

– And what exactly makes you think that Lisa Randall fits this description?

Not as guilty as many, I agree, and not to be bracketed with those who advocate “anthropic” capitulation. But I would still classify her as an apologist for ST.

50. **Aaron Bergman**
December 13, 2005

But I would still classify her as an apologist for ST.

How depressing, that we must classify everyone.

51. **Fabien Besnard**
December 13, 2005

Adrian said :

>I can’t read French but I trust you to be right about this one.

you don't have to : there is a link to an english version at the top of the page.

52. **Arun**
December 13, 2005

Yes, Aaron, since we have little hope of saying "correct" and "incorrect" about people's work, we are reduced to classifying the people, sort of like what happens in any humanities department. The level of objectivity that characterized particle physics is being eroded.

53. **Who**
December 13, 2005

Arun:…**since we have little hope of saying "correct" and "incorrect" about people's work, we are reduced to… level of objectivity … is being eroded.**

maybe there is a ray of hope regarding falsifiability. judging by what Smolin said earlier, string ideas were constructed to have thorough Poincaré invariance, naturally, and so in 2008 if GLAST finds some energy dependence of the speed of gammarays, that will falsify string. I presume that string can then be discarded as physics for that reason, if for no other.

conversely there are several non-string approaches to quantum gravity which do naturally and unavoidably predict energy dependence of the speed of photons—I think Smolin-style LQG does, for one. If GLAST does NOT find energy dependence in arrival time of gammaray burst photons that would seem to me like a good reason to discard Smolin-type LQG and any other QG approach with that feature.

By the way this does not affect CDT as far as I know.

I looked at the Stanford GLAST website recently and it appeared that it was still on schedule for a launch in 2007. Perhaps someone will wish to correct me, but that does seem to introduce a little "correct" and "incorrect" into the picture—which Arun found lacking.

54. **island**
December 13, 2005

Anthropic "Principle"?
One thing that is certainly unbounded is human hubris.

I think that it's equally arrogant to think that we can detatch ourselves from nature to presume that we can remove ourselves from the natural process to conclude that nature doesn't specially produce intelligent life for the thermodynamic contributions that intelligence enables. As soon as you do that, then you realize how stupid that it is to think that we humans could be alone in that effort if there's a real thermodynamic need for it.

Especially when there is writing-on-the-wall evidence that the leap from apes to humans enabled us to take advantage of energy sources that we would not be able to tap into otherwise. Etceteras and so on, the list of stuff that humans do in this effort is about as endless as the growing list of anthropic coincidences.

And then start looking from there… ASK the questions, don't *automatically* knee-jerk react with lame representations that are designed to ignore the full implications of the principle.

Some how it never occured to any one that instead of the world being perfectly tuned for our existence, it is actually the case that instead our existence is ferfectly tuned for this world, not the other way around, just like the case of oxygen on earth.

That means that we're a part of the local ecobalance, and you're right, so don't assume that we're any different than squirrils that bury nuts when we warm the climate, this counterbalances the *cumulative* accelerating runaway tendency toward glaciation that is predicted by Milankovitch models, which have been verified spectrographically from ice core samples taken in Greenland.

This balance between cumulative runaway tendencies, the greenhouse effect, vs the tendency toward glaciation… is yet another anthropic coincidence for which we are *innately* contributing players… go figure?

Deal with it, you don't have a choice, so don't be so arrogant as to presume that we can fool mother nature, the tug-o-war between "big business" and the "green movement" self-regulates the process, not the biggest brains in science, who *would* have us stagnate and die. Deal with it, we're not detatched from the process, we're specaially required 'fungi' that design "fairy-rings" per our inherent predisposition so do so. Nothing more… NOR LESS CONNECTED.

Bostrom introduced the Self Sampling Assumption in an attempted extension of the Copernican Principle into the time domain, but it has been proven that this is of no real-world significance to cosmology, because theories of everything that have constrained prarameters guarantee the emergence of man in a world in which man is known to exist. And that's where the ill-considered idea from skeptics comes from, they then conclude that "just so" isn't a significant factor, because that's a given or we simply aren't going to be here. I say that it is ill-considered reasoning because this line of thinking fails to take into account the fact that "just so" falls between cumulatively runaway tendencies.

The anthropic principle fell from only one of these weirdly balanced coincidences, the vast number of these life-permitting "ecobalanced" conditions that have been discovered since that time serve to compound the significance exponentially by orders of magnitude with each additional coincidence that is discovered. So it isn't even close to rational to offer up, multiverse-like "what if" scenarios about other forms of life and conditions that MIGHT exist elsewhere.

One thing that string theory teaches is that you can lose touch with real physics in imaginary universes, so be very careful when you project beyond what is observed to "what-if" things are different elsewhere in the universe scenarios, because you're appealing to religion when you do that. Just because we don't know something isn't an excuse to assume whatever you want.

Nick Bostrom's ideas are interesting and relevant where apparently chaotic, (subtly determined?), scenarios are applicable, but he fails to take into account the fact that an anthropic explanation for the fine-tuning of the universal constants is supposed to be embedded into humans by the universal scale mechanism that enables or requires human existence. That justifies the selection effect, in this case, because anthropic bias is supposed to be an innate characteristic of the universe.

He correctly notes that there can be areas of low entropy, (which are necessary to life), within the greater whole of our entropic, expanding universe, but he failed to equate the predominant entropic prejudice of our universe to the anthropic bias, as supported by conventional Big Bang Theory and The Standard Model of Particle Physics.

The anthropic principle notes that "Anthropic Bias" is, by definition, the natural expression for universal scale favoritism toward humans.

The principle becomes a powerful form of support for our best theories if we are thermodynamically connected to the process and there is no way to set yourself apart from this because the underlying direction of all action in a big bang induced expanding universe is ultimately entropic, per conventional Big Bang Theory as supported by the latest confirmed observational evidence.

Barring quantum fantasies… any occurrence within the system is, therefore, a result of the tuning of the constants that were set at t=10^-43. This includes humans in all their glory, and the weak entropic anthropic argument would support this via the fact that it is observationally proven that the human is comparitively one of nature's more preferred methods for efficiently dissipating energy at a regulated but increasing rate.

Dicke got his coincidence from Dirac's Large Numbers Hypothesis, but Dirac's cosmological model was wrong… gravity falling off with expansion. It is logical to think that repairing Dirac's cosmolgy would necessarily complete and clarify the anthropic principle, while removing the tautologous nature of it. Oh that's right we've accepted assumptions that take us beyond all that, no wonder Einstein died believing what he couldn't prove, rather than to accept that crap.

If you stick to the observed universe like the AP is telling you to do, then you can only derive that a multiverse is for dreamers, and so are infinities, uncertainty, absolute cosmic singularities… etc. idealizations. We have to distinguish between an idealized vacuum and the lowest actual energy state, which is non-zero.

It's equally arrogant to assume that there isn't some good reason for the anthropic principle in one universe as it is to intentionally bury OBVIOUS significance, (as Lenny pointed out), in many imagined universes.

Quit pretending that you don't see the writing on the wall.

At least Lenny has the guts to admit that the "appearance" of purpose in nature is real.

Deal with it.

55. **Michael Bacon**
December 13, 2005

It's far too skeptical to say that when you project beyond what is observed you're appealing to religion. What we know about the world is far greater than what we observe. More interesting is the idea that if GLAST finds some energy depedence of the speed of gammarays, that will falsify string theory. If not, non-string approaches like Smolin-style LQGs, which can accomodate energy dependence, are faced with a similar situation. I was wondering what folks thought of that?

56. **island**
December 13, 2005

It's far too skeptical to say that when you project beyond what is observed you're appealing to religion.

That's true. I worded it improperly and should have said that the anthropic principle is telling us that we shouldn't project beyond what is observed to assume that idealizations can actually exist… or you're appealing to religion.

57. **Michael Bacon**
December 13, 2005

Not sure what you mean by "assume" and "idealizations". Nevertheless, regardless of the meanings, there is much beyond what is observed that actually exists.

58. **Chris W.**
December 14, 2005

Hey, look at the papers linked by 'Dissident' in his comment on Cosmic Variance ("Where the dark matter is"). Many of you are aware of the recent work of Lauscher and Reuter (latest in hep-th/0511260). Reuter, et al, are arguing for observable manifestations of renormalization effects in QG at galactic and larger scales. Along with the connections of the same work to recent results in CDT, I find all this very encouraging. It seems to me that Wilczek of all people should be paying close attention to this stuff instead of wasting his time with the Multiverse and AP.

59. **Chris W.**
December 14, 2005

PS (credit where credit is due): I believe Aaron Bergman has been closely following work in CDT as well as the papers of Lauscher and Reuter. Any comments, Aaron?

60. **Aaron Bergman**
December 14, 2005

I haven't been following very closely — I have my own research, after all. I read a bit on CDT, and while it's intriguing, it's still feels very preliminary and somewhat *ad hoc* to me. A lot of people believe that any such UV fixed point is going to be extremely sensitive to the matter content in the theory, so it's not clear what significance a pure GR calculation would have. In that, as best I know, this is the first time anyone's every found something that's not horribly crumpled up, I'll be paying attention to where it's going. It'd be nice to see some black hole stuff with it.

I haven't looked at the Reuter stuff myself, but Jacques has, and it seems pretty persuasive to me.

61. **island**
December 14, 2005

…there is much beyond what is observed that actually exists

Like what, goblins? or did I stumble through a side door into the church of infinities?

I'll leave a tip in the plate when they pass it around, the point that I made was that the observational evidence indicates that you'd have to fix Dirac's Cosmology to see if it repairs his Large Numbers Hypothesis before you can make that claim, since we are talking about the affect that an entropic anthropic cosmological principle would have on this model.

A failure to look for evidence is popular where the AP is concerned, but that's all starting to change now that the string theorists have started abusing it to support their belief system, like creationists do.

Better lose the lame arguments that are designed to downplay its significance though, because they won't fly, they just make you appear as antifanatics who don't care what the actual science is.

62. **Christine Dantas**
December 14, 2005

It is not clear to me whether the framework below can be implemented in string theory. If this could be so, then will string theory accommodate Lorentz violation as well? Does it depend on how far string theory reproduces the standard model?

This is taken from Mattingly's review (gr-qc/0502097):

> The most conservative approach for a framework in which to test Lorentz violation from quantum gravity is that of effective field theory (EFT). Both the standard model and relativity can be considered EFT's and the EFT framework can easily incorporate Lorentz violation via the introduction of extra tensors. Furthermore, in many systems where the fundamental degrees of freedom are qualitatively different than the low energy degrees of freedom, EFT applies and give correct results up to some high energy scale. Hence following the usual guideline of starting with known physics, EFT is an obvious place to start looking for Lorentz violation.

And:

> "(…)the first place to look from an EFT perspective is all possible renormalizable Lorentz violating terms that can be added to the standard model. In [Ref] Colladay and Kostelecky derived just such a theory in flat space – the so-called (minimal) Standard Model Extension (mSME).

[Ref] Colladay, D., and Kosteleck´y, V.A., "Lorentz-violating extension of the standard model", Phys. Rev. D, 58, 1160

Thank you,
Christine

63. **Brett**
December 14, 2005

Christine-

The original motivation for developing the standard model extension came from string theory! Alan Kostelecky was an expert in string field theory, and he and Stuart Samuel wrote several papers in which they found minima of the string potential with odd properties. Perhaps the oddest property was that some of the minima had spontaneous Lorentz violation. (I think it turns out that none of the Lorentz-violating minima are actually the global vaucua of the theories, though, unfortunately).

Moving to effective field theory was motivated in large part by the fact that there are no realistic string theories. (There are no sufficiently realistic quantum gravity theories of any type.) But, knowing that string theory or some other high-scale physics could break Lorentz violation, it was immediately natural to consider how this could be encoded in an effective field theory. Having an effective field theory of Lorentz violation allows us to translate empirical verifications of relativity into bounds on specific coefficients, and these effective field theory bounds should not be sensitive to the underlying physics that gives rise to the Lorentz violation.

The effective field theory has now been developed for both quantum field theory and GR. The GR part is very interesting, because it apparently does not allow for explicit Lorentz symmetry breaking; any Lorentz violation must be spontaneously induced. There are a few other conditions on what form the Lorentz violation can take, but there's still a lot of very simple questions whose answers are unknown.

64. **Who**
December 14, 2005

about falsifiability, something interesting came up in QG recently

there is this different but related question which is sometimes asked string thinkers
What would make you give up string theory?
It was e.g. asked in the Panel Discussion at Toronto Strings '05 and Steve Shenker replied "You're not supposed to be asking that!"
From string theorists I've never seen a serious answer.

but Laurent Freidel recently gave a serious answer to that as regards LQG—he works on the spinfoam approach—in his invited talk at October Loops'05.

spinfoam is a kind of path integral, or sum over histories, treatment of spacetime. Freidel has introduced matter in the 3D case and shown that an effective QFT appears as the zero-gravity limit. he gets feynman diagrams and vertex amplitudes out of the spinfoams in the limit as newton's G goes to zero.

he was reporting on the first steps of an effort to extend this to 4D.
the requirement of getting the right effective QFT as a flat limit, of getting the correct feynman diagrams out of the spinfoam, should according to him determine a unique spinfoam QG model. IF IT DOES NOT he says, then he would stop researching spinfoam QG.

There is a video of Laurent Freidel talk at Loops '05. I downloaded it earlier and just watched it again. I was impressed—there is a kind of "falsifiability" here. It is not so much empirical (except that feynman diagram vertex amplitudes are supported empirically) as theoretical consistency. but he is obviously serious about it. for people who think like that and expect that, it is an exciting time in spinfoam.

something has worked in the 3D case and they are starting to check it and if it does not work in 4D that means, for them, that spinfoam goes on the scrap heap. but if it does work then it passes a kind of test.

see if you can download the Freidel talk from the Loops05 site. I think they may have taken the videos offline because I can't download it now.

the relevant preprint is
http://arxiv.org/hep-th/0512113
and references therein

65. **Christine**
December 14, 2005

Dear Brett,

Thank you for your comment.

Best wishes
Christine

66. **Who**
December 14, 2005

Wilczek posted this today
http://arxiv.org/abs/hep-ph/0512187
Enlightenment, Knowledge, Ignorance, Temptation

Frank Wilczek
10 pages, 5 figures. Summary talk at "Expectations of a Final Theory", Trinity College, Cambridge, September 2005
"I discuss the historical and conceptual roots of reasoning about the parameters of fundamental physics and cosmology based on selection effects. I argue concretely that such reasoning can and should be combined with arguments based on symmetry and dynamics; it supplements them, but does not replace them."

67. **Chris W.**
December 14, 2005

I just read through Wilczek's preprint. It is beautifully written; whatever one may think of the ultimate fate of these notions one could hardly ask for a better introduction. String theory and its current travails plays only a supporting role in it.

Some thoughts occurred to me after I finished it:

The so-called anthropic coincidences are derivable and interesting only because so much is governed by dynamical laws. We could hardly argue that a certain small set of parameters must be close to their measured values for life to exist unless those parameters were embedded in a theoretical structure that is largely unique, precisely formulated and testable, and broadly applicable. That is, if the structure of the world, and its changes from one moment to the next, were completely arbitrary then the notion of selection effects would have no meaning; we couldn't sensibly reason about them. So the possible need to invoke cosmological selection effects, unless we approach it in a glib and superficial way, should force us to think deeply about the nature and basis of physical laws, as well as their relationship to the possibility of discovering them.*

For these reasons I am deeply suspicious of assertions that are tantamount to saying that in the multiverse *all physical laws are possible*. This implies that the dynamical laws that we have formulated and successfully tested are in fact purely accidental—just aspects of one "point" in the multiverse—so that the very basis for analyzing the anthropic coincidences threatens to collapse. Everything about the observed universe becomes a selection effect, and we are forced to conclude that the laws, the fundamental constants, and the existence of life are one big arbitrarily selected package.

(Of course I'm setting aside the fact that defining life, and identifying the features of the observed universe upon which life really depends, is still a fairly dicey enterprise.)

————————
[* Note that, in some sense, all living things must discover something about physical laws, through evolution as well in the adaptive behavior of individuals, in order to effectively negotiate their environments and survive long enough to reproduce.]

68. **Chris W.**
December 14, 2005

By the way, Sean Carroll's latest post on Cosmic Variance nicely complements this discussion.

69. **Michael Bacon**
December 14, 2005

If spinfoam does work in 4D it adds evidence that it fits the quantum formalism and that the math surrounding the sum over histories approach is appropriate in the circumstances. I'm curious how the information representing the physical objects constituting the spinfoam, whatever its ultimate appearance, commute with their nearest neighbors. Perhaps quantum computation theory has something to add in this regard.

70. **Arun**
December 16, 2005

Which fundamental physical parameters, when changed to within a factor of 2, would make carbon-based life impossible?

71. **Arun**
December 16, 2005

Further question – would the "Standard Model" with a single generation of leptons and quarks instead of three (hence the scare quotes) be as consistent as the Standard Model? Would such a universe with such physics support life? While one generation instead of three would change details of the big bang and nucleosynthesis and so on, presumably a carbon atom doesn't change all that much; planets and chemistry might still be possible? If the answer is yes, then doesn't that put paid to the anthropic principle? i.e., if the anthropic principle can't distinguish between one generation and three, then it isn't of much use, is it?

72. **Max Tegmark**
December 17, 2005

Hi Mark,

I recently discovered your blog entry about our paper, and found it both interesting and amusing. Next time you spot me at a meeting, please stop by and say hello during a coffee break, as it would be fun to discuss these issues.

My opinion is that this anthropic debate is so spirited and long-lived because the two sides are largely talking past each other, addressing different questions. In this spirit, I think it would help of we both state clearly what points we're trying to make. The essence of what you write seems to be "the anthropic principle is unscientific and I don't like it" -how would you phrase your core points, and what precisely do you mean by "the anthropic principle"?
Specifically, how would you classify your critique?
a) The calculations in our paper are uninteresting and a waste of time.
b) Our paper contains incorrect statements.
If it's (a), then I respect your viewpoint and merely disagree with it.
If it's (b), then which specific statements to you object to?

This may surprise you, but I don't like using the phrase "anthropic principle", because there are many different definitions of
it floating around, all of which I believe to be either tautological or false. The question that I'm personally interested is how to confront a mathematical theory of physics (given by some Lagrangian or whatever) with observation, which I think you agree involves computing its predictions with selection effects taken into account.
For example, what does the pre-inflationary axion model predict, and is it ruled out? A key goal of our paper was to addresses these questions.

Cheers,
😉

73. **Who**

December 17, 2005

Dear Max,
I thought it was a serious flaw in your paper that it failed to address Smolin's cosmological natural selection proposal (CNS) and the accompanying arguments presented in

Scientific Alternatives to the Anthropic Principle
http://arxiv.org/abs/hep-th/0407213

—exerpt from abstract—
…We show however that it is still possible to make falsifiable predictions from theories of multiverses, if the ensemble predicted has certain properties specified here. An example of such a falsifiable multiverse theory is cosmological natural selection. It is reviewed here and it is argued that the theory remains unfalsified. But it is very vulnerable to falsification by current observations, which shows that it is a scientific theory. ,,,
—endquote—

Since the CNS proposal is the simplest and most directly testable multiverse model, and it has not yet been refuted by observation as far as I know. I think it is incumbent on you to describe CNS and to say why you think it can be ruled out. Or explain why it is not testable by current astronomical observations, if you believe it is not.

It might further the discussion if you did this.

Cheers 😉

74. **woit**
December 17, 2005

Hi Max,

Thanks a lot for taking the time to write here, I'd certainly enjoy talking to you in person about these issues sometime. Here are some comments, I hope they address the questions you asked.

First, some context. My background is in mathematics and particle physics, not cosmology, and what motivates me is the idea of trying to find an improvement of the standard model, using new mathematical ideas. I think Wilczek would classify me as the sort who is trying to get somewhere by "pure thought", something which he doesn't believe will ever work. I'd claim that, given the lack of any new helpful experimental input, particle theorists don't really have any choice at the moment except to go this admittedly much more difficult route. I've never found string theory a particularly appealing idea for unification (although it gives interesting insights into strongly coupled gauge theories and has led to some important new mathematics), and think the way it has completely dominated mathematically-minded particle theory for the last twenty years has been a disaster for the subject. By now it should be clear that it is simply a highly speculative idea that failed, and the field desperately needs to acknowledge this and move on to trying other things. The whole string theory landscape program, especially in its "anthropic" version, seems to me to be a retreat from the very idea of doing science, motivated by a refusal to admit failure. For more about this, read my recent review here of Susskind's new book.

Like just about any particle theorist, I've spent some time looking at what is going on in cosmology and hoping it will provide some new insights into how to get beyond the standard model. So far it seems to me cosmology has provided some interesting hints, but unfortunately they're no more than hints. We're agreed that what needs to be done here is to find ways of confronting our mathematical models with the real world. When dealing with models that involve a statistical ensemble of universes and observables that are only probabilistically determined, sure, one has to take into account selection effects.

The scientific part of my critique of your paper was not that I thought any of it was incorrect. The major part of it, your anthropic calculation of the selection effect for the dark energy density was more complicated than I have the time or interest to follow. You end up with a probability distribution for the dark energy, with the observed value near the peak. What was unclear to me was exactly what conclusions you are claiming can be drawn from this. Ignoring the axion cosmology prior, it seems you're just claiming that there's an anthropic window in the dark energy density, and observations show we're in the middle of it.

But, as a particle physicist, what I really want to know is what is left over when you remove the selection effect. Exactly what does your calculation says about the axion cosmology model? Have you provided any new evidence for it other than that it's not obviously inconsistent? Can you rule out a flat dark energy density prior in favor of the axion cosmology one? Your paper didn't seem to seriously address these kind of questions, or the more general question of when you can expect to get genuine, falsifiable predictions out of this kind of calculation.

That's the scientific critique, there's also a more general critique, one I made in my posting. Especially the early part of your paper seemed to me a heavily ideological push for the idea of anthropic determination of the standard model parameters, without anything to really back this up. Given the on-going disaster in particle theory these days due to the string theory landscape, I think this is really unhelpful.

Anyway, thanks again for writing in. If you'd like to write something more than a short comment in response, I'd be more than happy to put it up as a new posting, so people would be more likely to see it and comment on it if they wish.

Peter

75. **Aaron Bergman**
December 17, 2005

For my part, I think probabilistic arguments across multiverses are nonsensical. Otherwise, I don't see a satisfactory response to the doomsday argument. So, you can file me under (a).

76. **Who**
December 17, 2005

Peter to Max: "If you'd like to write something more than a short comment in response, I'd be more than happy to put it up as a new posting, so people would be more likely to see it and comment on it if they wish."

That would be great! Please consider doing this, Max!

77. **Max Tegmark**
December 18, 2005

Hi Peter,

Thanks for your thoughful response. I'm glad to hear that you didn't feel any of it was incorrect.

I agree that it will be interesting of one can make stronger statements about the axion model – as described in the paper, this will require improved astrophysics calculations that I hope somebody will make. You described the early part of the paper as a heavily ideological push for the idea of anthropic determination of the standard model parameters. The intent was rather to push for an open mindset on the issue, since we frankly don't know how many parameters will ultimately be computable from others.

Regarding the CNS-critigue by "Who": The pre-inflationary axion model is a complete physical theory, whereas Smolin's "Cosmological natural selection" is not (we lack a mathematical description of how black holes spawn universes; see also http://arxiv.org/abs/hep-th/0407266). Moreover, my guess is that the hypothesis is ruled out by the low observed fluctuation level (~1/10^5), since raising it would lead to more black holes.

Finally and most importantly, I'm glad that many of you disagree with my views! As argued in http://arxiv.org/abs/physics/0510188, I think that diversity in the physics community is more useful than an ideological monoculture, since it motivates physicists to tackle unsolved problems with a wide variety of approaches.

😉

78. **Who**
December 18, 2005

Hi Max,
personally I view your "low observed fluctuation level" not as a fundamental physical constant but more of an ad hoc *result*. It could be symptomatic of more basic parameters established prior to the beginning of expansion. But this is largely a matter of preference. It is personal opinion on your part (as you say) and equally on mine!

I would have expected you to mention CNS in your paper and to say why, in your opinion, it is probably ruled out. This would have given others a convenient opportunity to argue that it has not yet been ruled out.

There have been quite a few papers in the past year relating to the question of how black holes might spawn universes. Much of the work (QG modeling black hole collapse) was by people who reported in the Friday (14 October) session of Loops '05. The main topic was LQC, but the same people work on quantum models of gravitational collapse and, for what it's worth, the bounce mathematics has turned out to look rather similar in both cases.

You say "we lack a mathematical description" and cite a 2004 paper by Leonard Susskind responding to Smolin, but, as I say, there has been quite a bit of mathematical description since then. I also do not believe Susskind's rebuttal stands unchallenged.

Be that as it may, my point is that the CNS idea has not been disposed of and should have been discussed. If you think it is not falsifiable, or that it has already been falsified, then you should at least give your reasons.

In case you would like to look up the past year's QG papers modeling gravitational collapse and bounce, I will post some arxiv numbers later. Some of the relevant authors, if you want to look up their recent work yourself, would be Abhay Ashtekar, Martin Bojowald, Viqar Husain, Oliver Winkler, Leonardo Modesto, Parampreet Singh.

To be fair, one should note that Smolin's CNS proposal is testable without reference to any specific mathematical description of black hole collapse and bounce. The CNS conjecture is that some reproductive/evolutionary mechanism has fine-tuned the constants for black hole production. CNS challenges us to find even one fundamental constant (hope you will pardon me if I decline to view your "low observed fluctuation level" as a fundamental constant) which if it were better tuned would result in substantially greater black hole abundance.
That is something one can use to test—and possibly falsify—CNS, even before one has a complete mathematical theory of the conjunction of black hole and big bang events.

Thanks,
😉

79. **Lee Smolin**
December 18, 2005

Dear Max and Who

Thanks for mentioning the CNS proposal. The issue of the level of delta rho/rho-the fluctuation level-was discussed in detail and resolved in the first paper published on the model, Did the universe evolve?, Classical and Quantum Gravity 9 (1992) 173-191, summarized in Life of the Cosmos, ps 309-10.

Given that this was the first theory based on the "landscape*" I would hope that people would look at the literature before dismissing it.

The point is that in simple one field inflation models the fluctuation level is proportional to the inflaton self-coupling constant. However the number of e-foldings in inflation is related to (if I recall right) the inverse of the square root of the inflaton self-coupling. The result is that the cost of raising the self-coupling to produce primordial black holes or make more galaxies is that there are fewer e-foldings in inflation and the resulting universe is exponentially smaller. The result is that CNS predicts that the self-coupling should be as small as possible consisent with some steady level of black hole production. i.e. many more black holes are produced by a slow, but steady rate of black hole production in an exponentially larger universe than by a burst of primordial black hole production in an exponentially smaller universe.

This seems to characterize our universe where most black holes are made as supernova remnants. It also leads to a prediction, which is that inflation is governed by one parameter and not by a complicated potential with more than one parameter in which the fluctuation amplitude and number of e-foldings would be independent. So were there any evidence for an inflaton potential governed by more than one parameter the theory would be ruled out. I made this prediction in 1992 and so far it has held up.

I can endorse WHO's comments: recently we have very good detailed support from the quantum theory of gravity that black hole and cosmological sinigularities bounce. There is not yet a detailed description of the parameters mutate in a bounce, but this is certainly plausible given current views of the "landscape".

Max, if these were your main objection to CNS, would you now agree that the theory is viable?

I would go further, CNS is the only landscape theory proposed so far that makes falsifiable predictions, Should this not make it the leading candidate for an explanation of the choices of parameters in the case that the landscape is real?

Thanks,

Lee

*and indeed the origin of the term, which comes from "fitness landscape".

80. **Anthony Aguirre**

December 19, 2005

Hi Lee,

This is a nice argument, but does not terribly ease my misgivings about CNS, which are approximately 5-fold.

First, the initial field value will also come in to the number of e-foldings, and (absent a concrete model) that may or may not be correlated with the inflaton coupling.

Second, other things could lead to ultra-numerous black holes, e.g. a very high baryon/photon ratio. This physics should be unrelated to inflation, and variable by tuning the coupling constants. Or, we could introduce tilt into the perturbation spectrum to create scads of primordial black holes. Or is there a size requirement?

Third, I've never been clear on the 'number of black holes': per unit what? If per unit photon (or dark matter particle), we can easily maximize this by increasing the baryon (or dark matter) to photon ratio. If per unit volume, this is of course time-dependent. And in all cases we must worry about infinite universes (note that a universe starting out finite does not mean that it cannot spawn an infinite universe either, as in open inflation). These are more-or-less the same semi-intractible issues eternal inflation has to deal with.

Fourth, CNS still requires, even if we accept the nascent models for creating baby universes from black holes, some way of passing-down constants with small variations.

Last, CNS does not answer the question of why the universe supports life, unless black holes are alive. It merely transforms the question into 'why does a high black-hole formation rate correlate with life?'. An alternative that *would* explain things, I would say, were if advanced civilzations were responsible for creating the baby universes. But that it pretty science-fictiony and I suspect you would rather avoid going down that path.

Thus I would be very pleased if CNS could be made to work and create strongly-peaked probability distribution for the observed parameters. (Note, in regard to our recent paper that CNS is not, as I see it, an alternative to the scenario we present, but a special case of the 'multiverse' explanation in which p_prior is strongly peaked around certain parameter values related to black hole formation. Unless axions are involved in black holes, I'm not sure it would impact the argument). But it seems to me that there are even more missing pieces, and about as many very difficult problems of principle to deal with, than in the eternal inflation scenario.

cheers,

Anthony

81. **Anthony Aguirre**
December 19, 2005

Hi All,

An interesting discussion, and I agree with Max that it is good for parties with very different viewpoints to discuss this in a way that is "not talking past each other", i.e. making assumptions about what the other parties are actually saying. Those interested in a more detailed (and, I think, not particularly pro-'anthropic') discussion of what responses one might take to having non-unique constants, by one or two of the authors of the paper under discussion, might enjoy hep-th/0409072 or astro-ph/050651.

Anthony

82. **Chris W.**
December 19, 2005

Anthony,

Was that was supposed to be astro-ph/0506519 ("On making predictions in a multiverse: conundrums, dangers, and coincidences")?

83. **woit**
December 19, 2005

Anthony,

Thanks for your very interesting comments. I'm sure the paper Chris mentions is the one you had in mind. It looks like it goes into much greater detail in addressing the kinds of questions that your more recent paper raised in my mind. I'll add something to the main posting pointing to it.

84. **Aaron Bergman**
December 19, 2005

I'll ask my usual question for people who make arguments along these lines? Is the human race going to end soon? Or, if you don't like the Doomsday argument, then there's Olum's argument that we should be in some star spanning civilization.

I just don't see how the notion of probability makes any sense in these situations and, even if it did, what we gain from thinking about it.

85. **Lee Smolin**
December 19, 2005

Hi Antony,

Thanks for your criticisms. Briefly, CNS works well for the parameters of the standard model of particle physics, and I don't think this can be disregarded. Every variation of those parameters for which definite conclusions could be reached, the conclusion was that the number of black holes made decreased. The case is not as strong for some of the cosmological parameters. I don't take these as important as we don't which if any cosmological parameters are determined by parameters of a theory. But there are some answers to your points, which I'll repeat and then address:

"… the initial field value will also come in to the number of e-foldings, and (absent a concrete model) that may or may not be correlated with the inflaton coupling…."

Fine, but the mechanism that determines the initial field may be independent of the inflaton coupling to leading order, if so then this decouples, i.e. for any initial field value my argument works.

What about "… a very high baryon/photon ratio…?"

If I recall correctly, this affects nucleosynthesis, and many other things, as this is a cold or tepid big bang. Ill check.

"…Or, we could introduce tilt into the perturbation spectrum to create scads of primordial black holes.."

The question again is what the consequences are for other processes, Ill think about this.

"…I've never been clear on the 'number of black holes': per unit what?"

The answer is per unit bounce, which means per unit black hole in the previous universe.

"Fourth, CNS still requires, even if we accept the nascent models for creating baby universes from black holes, some way of passing-down constants with small variations…."

Granted. This remains to be filled in. But still it is possible to deduce falsifiable predictions. This is not surprising, Darwin could make predictions in spite of being ignorant of DNA and its replication.

"…Last, CNS does not answer the question of why the universe supports life, unless black holes are alive. It merely transforms the question into 'why does a high black-hole formation rate correlate with life?'…"

There is a clear correlation. You only get a lot of black hole production in universes with many massive stars. You only get many massive stars when the initial mass function (IMF) is power law out to many solar masses rather than exponentially damped. While this is incompletely understood, there is evidence the high end of the IMF is power-law because there are processes which efficiently cool giant molecular clouds (GMC's) and because the GMC's are well shielded from star light. Both the cooling and shielding involve carbon and oxygen. (This is described in some detail in Life of the Cosmos). Hence the chemical complexity life requires may exist because it is needed for a universe to efficiently produce stars massive enough to leave black hole remnants.

"…An alternative that *would* explain things, I would say, were if advanced civilzations were responsible for creating the baby universes. But that it pretty science-fictiony and I suspect you would rather avoid going down that path…."

I don't. But Harrison (of Harrison-Zeldovich) has written a paper which goes down that path, as has Louis Crane.

I hope this helps,

Thanks,

Lee

86. **ksh95**
December 20, 2005

"…Darwin could make predictions in spite of being ignorant of DNA and its replication…"

Darwin had evidence that during a "species-bounce" a chicken would not mutate into a donkey. The important word here is small. Without small changes one is left with a random mess.

"…CNS does not answer the question of why the universe supports life…"

Why should CNS (or any theory for that matter) answer *that* question. An equally valid question could be: why does ksh95 like soup?

87. **Anthony Aguirre**
December 20, 2005

Chris W.:

That is indeed the article; sorry about the typo.

Lee:

This is probably a discussion that we should have over a nice meal sometime, and hopefully I can make it out to PI soon to do so. I would love to understand how CNS could work, and would be happy to be convinced. Perhaps my biggest stumbling block now, as I think about it, is the invocation of inflation. If inflation is involved, then it is easy for inflation to be future eternal, and if this were possible for *any* set of constants, then universes involving eternal inflation, which spawn infinitely many black holes, would immediately dominate the ensemble, and we are back where we started. If you have a good idea for allowing inflation while precluding the possibility of eternal inflation, that would be wonderful (as much as I enjoy thinking about eternal inflation, I would not mourn it's passing, I have to say 😉 But, for example, in the case with possible multiple vacua, I see no way to avoid it from occuring at least for some parameter values — and I think evolution would find a way to exploit this mechanism for infinite reproduction!

cheers,

Anthony

88. **Moshe**
December 21, 2005

Peter, since this discussion is going in your territory, I am curious what is your opinion of cosmological natural selection as an alternative to anthropic reasoning. Feel free to ignore this if you haven't looked at it in sufficient detail.

Happy holidays!

best,

Moshe

89. **woit**
December 21, 2005

Hi Moshe,

I haven't looked at CNS in enough detail to have an intelligent comment on it. The crucial question for anything like this is whether it generates testable predictions. Here

Smolin claims it does, and his discussion here with Aguirre looked like it could be very illuminating, but I haven't taken the time to really understand the arguments on both sides (life is short, and it's exam period here these days…)

In general, what I really care about and am willing to invest time in trying to carefully understand, are new physical ideas that explain something about particle theory, or new mathematical ideas that might somehow be useful in better understanding particle theory. This means there are a lot of topics in cosmology and quantum gravity that I've never studied in a really serious way, CNS is one of them.

90. **Lee Smolin**
December 21, 2005

Hi,

To Antony on eternal inflation. I understand the arguments that go from inflation to eternal inflation but I find myself unconvinced of them, especially in the context in which inflation would arise in a small region to the future of the bounce of a black hole singularity. I would trust inflation in general much more if we had solved the cosmological constant problems. The cosmological constant problem arises not in a fundamental theory but only at the effective field theory level and, as pointed out by Dreyer in hep-th/0409048 there may be a physical mechanism such as proposed by he or Volovick which dynamically adjusts the vacuum non-perturbatively close to zero, in which case there is no inflation. Of course then I would lose the argument I made, but there is no problem in principle as the horizon problem is already solved by the fact that the universe arises from a bounce.

On the other hand, WE KNOW that there are many black holes-perhaps as many as 10^19 in our Hubble volume. And we have good theoretical evidence that black hole singularities bounce. So only one element is missing for the CNS scenario-that the changes in vacuum resulting results in small changes in standard model parameters.

For the time being we must assume this-although it is possible this assumption may be tested soon. But once we do we get quite a bit.

My impression, if I can say so, is that many cosmologists undervalue the positive successes of CNS. It EXPLAINS otherwise mysterious features of our universe such as the setting of the parameters to make carbon and oxygen abundent-not because of life but because of their role in cooling GMC's. It also EXPLAINS the hierarchy problem and the scale of the weak interactions-because these can also be understood to be tuned to extremize black hole production. Further, it EXPLAINS two otherwise improbable features of glaxies: why the IMF for star formation is power law and why disk galaxies maintain a steady rate of massive star formation.

Moreover CNS makes a few real predictions: that the upper mass limit of neutron stars is less than 1.6 solar masses and that inflation is governed by one parameter. Made in 1992 these predictions could easily have been falsified but they have stood up.

CNS makes these genuine explanations and predictions without having to invoke the AP, whereas eternal inflation requires invoking strongly the AP just to be plausible.

It seems to me CNS has a much longer list of successes than eternal inflation. which so far as I can tell explains NOTHING about the particle physics standard model parameters and which makes no fallsifiable predictions.

There are always many theories with attractive features, my understanding of how science is supposed to work is we are to pick out of the list of otherewise attractive theories those few that genuinly explain and predict over those that don't.

The problems I set out to solve were 1) why the neutron is a bit heavier than the proton, 2) why their difference is comparable to the electron mass, 3) the value of the fermi constant, 4) the value of the strange quark mass, 4) the hierarchy problem. These are real problems which have been around for decades. So far as I know, CNS is the only explanation proposed so far that is both genuinly explanatory and genuinly falsifiable in terms of ongoing observations.

If you ask me to discount all this because of a highly speculative theory of the very early universe that explains nothing and makes no falsifiable predictions, that may easily dissapear when the cosmological constant problem is solved, I am afraid I don't think this is consistent with the methodology of science as I understand it. I understand reasonable people may differ, and I have the greatest respect for you, Max and other cosmologists, but I must admit also I am puzzled by your apparent judgements of what is more and less reliable in present theory.

Also, to Michael Bacon: Methods from quantum computation are being used to solve the problem you mention, which is how distinct particle states emerge from a spin foam. See D. W. Kribs, F. Markopoulou, Geometry from quantum particles, gr-qc/0510052.

Thanks,

Lee

91. **Moshe**
December 21, 2005

Thanks Peter, and good luck with finalizing the semester.

92. **Who**
December 21, 2005

in his preceding post Lee Smolin says

The problems I set out to solve were 1) why the neutron is a bit heavier than the proton, 2) why their difference is comparable to the electron mass, 3) the value of the fermi constant, 4) the value of the strange quark mass, 4) the hierarchy problem. These are real problems which have been around for decades. So far as I know, CNS is the only explanation proposed so far that is both genuinly explanatory and genuinely falsifiable in terms of ongoing observations.

[MY COMMENT] this is a key paragraph and I want to connect the dots and see what I understand and what I dont. Maybe get some help explicating, if possible. The above connects with page 31 of the paper (Scientific Alternatives to the AP, hep-th/0407213). Here is an exerpt from Smolin's paper:

The crucial conditions necessary for forming many black holes as the result of massive star formation are,

1. There should be at least a few light stable nuclei, up to helium at least, so that gravitational collapse leads to long lived, stable stars.

2. Carbon and oxygen nuclei should be stable, so that giant molecular clouds formand cool efficiently, giving rise to the efficient formation of stars massive enough to give rise to black holes.

3. The number of massive stars is increased by feedback processes by which massive star formation catalyzes more massive star formation. This is called "selfpropagated star formation, and there is good evidence that it makes a significant contribution to the number of massive stars produced. This requires a separation of time scales between the time scale required for star formation and the lifetime of the massive stars. This requires, among other things, that nucleosynthesis should not proceed too far, so that the universe is dominated by long lived hydrogen burning stars.

4. Feedback processes involved in star formation also require that supernovas should eject enough energy and material to catalyze formation of massive stars, but not so much that there are not many supernova remnants over the upper mass limit for stable neutron stars.

5. The parameters governing nuclear physics should be tuned, as much as possible consistent with the forgoing, so that the upper mass limit of neutron stars is as low as possible.

The study of conditions 1) to 4) leads to the conclusion that the number of black holes produced in galaxies will be decreased by any of the following changes in the low energy parameters:

• A reversal of the sign of Δm = m_neutron – m_proton.

• A small increase in Δm (compared to m_neutron will destabilize helium and carbon.

• An increase in m_electron of order m_electron itself, will destabilize helium and carbon.

• An increase in m_neutrino …, will destabilize helium and carbon.

• A small increase in α will destabilize all nuclei.

• A small decrease in α_strong, the strong coupling constant, will destabilize all nuclei.

• An increase or decrease in G_Fermi of order unity will decrease the energy output of supernovas. One sign will lead to a universe dominated by helium.

[my comment: not sure the Delta and alpha are going to print. Some of the above seems readily understandable. Some, not. In one case where I failed to understand, I shortened the quote by elision. Refer to the original. Would be glad if anyone volunteered to clarify, instead of my having to, even the more self-evident points here.]

93. **Frank Wilczek**
December 26, 2005

Hi,

I'd like to encourage readers interested in my views on these subjects to read to original papers, which are rather more nuanced and substantial than the summaries presented here. Thanks, and happy holidays.

94. **woit**
December 26, 2005

I'd like to second Wilczek's encouragement to all to read the original papers discussed here. In this posting, as in just about all others, what you are getting here is not an accurate, objective summary of a talk, article or book, but my own commentary on one or more specific aspects, written under the assumption that readers will read the original source for themselves.

In addition, in cases like this particular one, it should be taken into account that my commentary is very much not objective, but highly colored by certain specific concerns which the regular readers of this blog should be well aware of.

95. **Who**
December 26, 2005

Hello Frank Wilczek,
I'm a long-time fan of your writings—like the combination of ideas and style.

Here I am responding directly to the Tegmark et al paper
http://arxiv.org/abs/astro-ph/0511774
"Dimensionless constants…"
of which you are co-author.

What disturbs me about this paper is that it does not include a discussion of Smolin CNS, as described in
http://arxiv.org/abs/hep-th/0407213
"Scientific alternatives to the anthropic principle"

Would you please explain why the CNS hypothesis does not merit consideration when one discusses the explanation or non-explanation of basic dimensionless constants? Has CNS, in your view, already been falsified by observation?

96. **woit**
December 26, 2005

Who,

I probably shouldn't try and answer for Wilczek, who I gather from his comment is not particularly happy with my summary of parts of his papers and talk, but there's an obvious answer to your question. His paper with Tegmark is not a review of the various attempts to get scientific predictions out of anthropic arguments. It not only doesn't mention Smolin's CNS hypothesis, but it also doesn't discuss the far more popular string theory anthropic landscape program being pursued by many string theorists. The paper is specifically about the dark matter density, looking at axion cosmology and WIMPs as hypotheses for the origin of dark matter, as well as relevant anthropic selection effects. Given what they are trying to do in that paper, I don't see any particular reason the authors should be addressing the CNS hypothesis there.

97. **Who**
December 26, 2005

Peter,

let me be a bit more specific about where I think the omission occurred and then either you or Wilczek can judge if my objection is valid. In the Tegmark et al paper, on page 1, in the introduction section1B, quote:

—quote from Tegmark et al—

B. The origin of the dimensionless numbers

So why do we observe these 31 parameters to have the particular values listed in Table1? Interest in that question has grown with the gradual realization that some of these parameters appear fine-tuned for life, in the sense that small relative changes to their values would result in dramatic qualitative changes that could preclude intelligent life, and hence the very possibility of reflective observation. As discussed extensively elsewhere [9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22], there are four common responses to this realization:

1. **Fluke: Any apparent fine-tuning is a fluke and is best ignored.**

2. **Multiverse: These parameters vary across an ensemble of physically realized and (for all practical purposes) parallel universes, and we find ourselves in one where life is possible.**

3. **Design: Our universe is somehow created or simulated with parameters chosen to allow life.**

4. **Fecundity: There is no fine-tuning, because intelligent life of some form will emerge under extremely varied circumstances.**

Options 1, 2, and 4 tend to be preferred by physicists, with recent developments in inflation and high-energy theory giving new popularity to option 2.

Like relativity theory and quantum mechanics, the theory deepening our understanding of the nature of physical reality. First of all, inflation is generically eternal [25, 26, 27, 28, 29, 30, 31], so that even though inflation has ended in the part of space that we inhabit, it still continues elsewhere and will ultimately produce…

…More dramatically, a common feature of much string theory related model building is that there is a "landscape" of solutions, corresponding to spacetime configurations involving different values of both seemingly continuous parameters (Table 1) and discrete parameters…

—endquote—

Peter, you say an obvious answer to why CNS was not mentioned is that
" **His [Wilczek's] paper with Tegmark is not a review of the various attempts to get scientific predictions out of anthropic arguments.**" But I would reply that CNS is *not* an attempt to get scientific predictions out of anthropic arguments. The theory has no anthropic character at all—it doesnt refer to life or consciousness, or depend logically on their existence.

However the paper does review 4 possible ways of reacting to the "realization that some…parameters appear fine-tuned for life."

I believe that until or unless CNS is refuted it should be included among the possible responses, and that it does not fit neatly into any one of the four listed by the paper. I suppose that according to the CNS view it can be considered a **fluke** that conscious life can arise where evolution has tuned the constants for abundant black holes. And that in this view there is a single **universe** capable of branching where, as in the case of a stellar mass black hole, gravitational collapse leads to a bounce. So it is perhaps closest to responses 1 and 2—but not adequately represented by either!

98. **Aaron Bergman**
December 26, 2005

CNS is subsumed in #2.

Hope this helps!

99. **Anthony Aguirre**
December 26, 2005

To "who":

I agree with Aaron Bergman that CNS is subsumed in 'multiverse', (and this is why I did not think it required separate listing either in the present paper or my earlier one with a similar list) though I also agree with you that, as I noted before, there is an aspect of 'fluke; that the conditions for life must coincide with the conditions for black-hole formation (or else life all lives in rare universes and we are back to anthropics to explain why we do not observe the most common type of universe).

Lee:

For me, and I think for many, the argument goes:

1) observations imply inflation.
2) Inflation implies at least the possibility of eternal inflation.

breaking eather link would be extremely interesting. Breaking the first, by creating a viable alternative to inflation for explaining the CMB fluctuations, etc., would obviously be interesting, but has not, in my opinion, been even nearly done, with all due respect to the cyclic and VSL folks.

Breaking the second link, which you suggest, would also be interesting and I would be happy to entertain such a possibility — I just do not see how to do it. You may be right that somehow eternal inflation will 'go away' if we understand vacuum energy, but I'm not sure how this could happen without inflation itself going away.

There are some, I think, who entusiastically embrace the eternal inflation picture. But I think many others others, like myself, find it interesting that observations seem imply it, perhaps despite our wishes.

cheers,

Anthony

100. **Dissident**
December 27, 2005

Anthony, what about Luminet's "hall of mirrors"?

http://arxiv.org/abs/physics/0509171

101. **Lee Smolin**
December 27, 2005

Dear Anthony,

Thanks, I am aware of this point of view but I haven't found the literature completely convincing. Perhaps you could tell me the paper where the argument "inflation implies eternal inflation" is most clearly and convincingly presented, and I'll study it.

Worry about the realness of the vacuum energy is not the only issue I've had with this argument. Another is how well defined are the frameworks in which calculations are done. Some are very heuristic, others depend on assumptions about the interpretation of the wave function of the universe and measures on infinite numbers of universes that don't seem to make sense when scruitinized.

Another kind of worry is that extending from inflation to internal inflation requires believing that the mechanism is reliable at scales presently outside our horizon that were formerly way below the Planck scale. If there is a universal planck scale cut off, as in DSR, it could alter the physics presently outside of our horizon. In fact, there are apparent anomalies in the CMB spectra near present Hubble scales-both the low power and the axis of evil. If real they support the idea that Planck scale effects have a non-trivial effect on inflation, which would change things outside our present horizon.

On the other hand, we know our universe produces large numbers of astrophsyical black holes, a fact that depends on features of star formation and galactic dynamics that do not seem otherwise necessary.

Thanks,

Lee

ps to Who, thanks for pressing the argument.

102. **Anonymous**
December 27, 2005

I tried to get a response over at CV and none so far, so I might as well point it out here: there's a new paper from Reza Mansouri, http://arxiv.org/abs/astro-ph/0512605, which claims that properly accounting for inhomogeneities in the universe eliminates the need for dark energy or a cosmological constant to explain the data. It looks reasonable to me and much more convincing than the Kolb stuff earlier this year. Any thoughts? This is somewhat off-topic, but on the other hand if this sort of reasoning is shown to be correct I think much of the reason for supporting anthropic arguments will go away fairly quickly.

103. **Anthony Aguirre**
December 27, 2005

Hi Lee,

Absolutely agree that many of the arguments for eternal 'stochastic' inflation (driven by quantum fluctuations) are somewhat unrigorous, even at the level of effective field theory in curved spacetime. I find them reasonably compelling but consider it possible that there is something fundamentally wrong with them. The argument for eternal inflation with multiple minima, however, seems extremely strong to me — the spacetime can be described more-or-less exactly, and a very straightforward computation implies that the (physical) inflating volume increases when spatial sections are chosen so as to make the background spacetime homogeneous. One must only accept the Coleman-De Luccia picture of the decay of the false vaccum for this to be true for at least some multiple-well potentials.

Now, the questions of how *generic* eternal inflation is, is I think an interesting one: are there non-fine-tuned inflation models that explain the observations and are not eternal? I don't know of any decent study on this question (and may well undertake one myself). In the context of CNS, however, even the *possibility* of eternal inflation is very dangerous, however, because unless eternal inflation is *forbidden* by the 'meta' laws that govern which types of inflation are possible in the landscape of possibilities, it must only be realized once to 'take over' the ensemble by creating an infinite number of black holes out of one. The only escape from this, it would seem, would be to have other channels to create infinitely many black holes from one 'parent' — but then you would be in the same extremely thorny boat of comparing infinities that eternal inflation is already in; further, which type of universe would come out 'winning' in this competition between infinities would seem quite independent of mundania such as the IMF…

One interesting way that EI might be essentially wrong is if the QFT in curved spacetime description is essentially wrong, for example for 'holographic' reasons (see the recent Banks paper). In this view, all of the zillions of 'other universes' are just refigurings of the same degrees of freedom inside the horizon (this realizes, in some sense, a version of the 'hall of mirrors' mentioned by 'dissident' above.) I find this view very hard to understand, but perhaps it is right and would change the way we think about EI, perhaps in a way less troubling (to me at least) in regard to CNS.

cheers,

Anthony

104. **Chronos**
December 28, 2005

Would it be fair to say that the Anthropic Principle rules out the impossible, but is not predictive beyond that?

105. **Shantanu**
December 30, 2005

To
Anthony and others,
what do you think about Penrose 's objections to inflation?
I have not seen a good counter-argument to them.

Yet More Assorted Links

December 14, 2005
Categories: Uncategorized

Various and assorted things that may be of interest:

Joshua Roebke at Seed has accumulated opinions about the landscape from various physicists. No real surprises; Witten wins the award for most non-committal:

I just don't have anything incisive to say. I hope we will learn more.

There's a mailing list called philphys devoted to "philosophical and foundational problems of modern physics."

The web-site for the ICFA Seminar held in Korea a couple months ago has several interesting presentations on-line, including one by John Ellis, and several about future plans at various accelerator laboratories world-wide.

There was a conference recently in Geneva celebrating the 100th anniversary of the birth of E.C.G. Stueckelberg. Some anecdotes about Stueckelberg are here.

There is a bit of a controversy about another E.C.G., Sudarshan, who some (including himself) feel should have been part of this year's Nobel prize in physics. Stories about this here, here, and here.

Some talks given at the recent conference at OSU on Strings and the Real World are on-line. See if you can find anything in them about the real world.

According to this article, string theory is now being marketed to the 6-11 year-old age group, appearing as test questions in the Flashcard Fishing game on the GoGo TV game system.

Barry Mazur has an article about category theory entitled When is one thing equal to some other thing?.

Penn State mathematician Adrian Ocneanu has designed a sculpture representing an interesting four dimensional figure, the Octacube.

Update: Sean Carroll has a new preprint out entitled Is Our Universe Natural?, and some commentary about it at Cosmic Variance. Unlike certain Nobel prize winners, Sean recognizes that before throwing in the trash the paradigm of how to do theoretical physics that has had such success for many centuries, one should at least have a shred of scientific evidence for one's proposed alternative. He explains what the problems are with the one supposedly successful "prediction" of the anthropic principle, that of Weinberg for the cosmological constant, noting that it makes three assumptions, and :

The first of these is a guess, the second is likely to be fantastically wrong in the context of eternal inflation, and the last only makes sense if all of the other parameters are held fixed, which is not how we expect the multiverse to work.

Even with these dubious assumptions, the "prediction" one gets is off by more than an order of magnitude.

Comments

1. **Kea**
   December 14, 2005

   Thank you for the very beautiful article by Barry Mazur.

2. **D R Lunsford**
   December 14, 2005

   It's high time Stueckelberg got his due. There is a beautiful article here about Stueckelberg's 1934 paper:

   http://arxiv.org/abs/physics/9903023

   Stueckelberg was, like Pauli, a student of Sommerfeld. I'd still recommend Sommerfeld's 6-volume course any day, even alongside Landau. (Sommerfeld's teacher was Klein. This was a lusty vein of physics and mathematics.)

   -drl

3. **Aswin**
   December 15, 2005

   More on the Sudarshan issue :

   A report in Frontline about the whole issue
   Sudarshan's Letter

4. **Tony Smith**
   December 15, 2005

   Peter, you say "… Penn State mathematician Adrian Ocneanu has designed a sculpture representing an interesting four dimensional figure, the Octacube. …".

   When I went to the link you gave to
   http://www.science.psu.edu/alert/Math10-2005.htm
   I found material including the following:

   "… The subject of the projection is a regular 4-dimensional solid of intermediate complexity, which Ocneanu calls an "octacube." It has 24 vertices, 96 edges, and 96 triangular faces, which enclose 24 three-dimensional "rooms."
   …
   No good rendering of any 4-dimensional object existed anywhere in the world before the Octacube, either in solid or virtual form, according to Adrian Ocneanu, the Penn State professor of mathematics who designed the sculpture.

…
The sculpture represents a three dimensional map of the surface of a four dimensional regular solid. Adrian Ocneanu developed and copyrighted the map, called windowed radial stereographic projection. … Linear edges of the solid become circles in the projection. …".

I am appalled by those statements, and I wish that I could attribute them solely to Penn State PR bureaucrats, because Ocneanu has done interesting mathemematics, such as work on von Neumann algebra factors.

First:
The "octacube" is nothing new. It has been around in mathematics for decades, and it already has a well-accepted name (the 24-cell).

Second:
"Good renderings" of the 24-cell have been in existence for a long time. In 1932, Hilbert and Cohn-Vossen wrote the book Anschauliche Geometrie, which has a nice rendering of a 24-cell that is figure 172 in the English translation, which is entitled Geomety and the Imagination (Chelsea 1952). There have been so many other renderings over the past decades that I will not attempt to list them here.

Third:
Radial stereographic projection of the 24-cell is nothing new either. For a very nice description with beautiful illustrations, see the web page of Frans Marcelis at http://home.wanadoo.nl/f.marcelis/24-cell.htm
If you go to the PSU web page cited above, you can see an image whose caption reads "Dr. Ocneanu, pictured at the inauguration of the Octacube, sharing his first apparition of the octacube as a wire model.". If you look at the "apparition", you will see that it looks almost identical to the work of Frans Marcelis.

I think it is disgusting that the PSU people are trying to rename the 24-cell and claim priority on renderings and projections thereof.

The sculpture itself is quite beautiful and is a good visualization tool. It is a shame to degrade such a nice work of art by associating it with attempts to rewrite history and rename the 24-cell.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

5. **nitin**
December 15, 2005

I read about this "controversy" of who between Stueckelberg and Feynman first came up with the now famous diagrammatic perturbative approach to doing QED calculations about 2 years ago, when I was in my second year of undergraduate studies at Melbourne University. Some of the anecdotes related to this matter that were brought to my attention shocked me, especially Feynman's comment after his CERN lecture. If the story is true and Stueckelberg did actually work things out well before Feynman did, then we wonder why Feynman, who reportedly was aware of Stueckelberg's works before he was awarded the Nobel Prize, did not say anything explicitly about it. But maybe more facts need to surface for a consistent picture to emerge… Funnily, I remember being rather angered on hearing the story, more so because I respect Feynman a lot, and such a behaviour of his would be condemnable. Then I forgot about it, until I bought a copy of Schwinger's "Selected papers in QED", and found no paper by Stueckelberg. Now, if Schwinger knew that Feynman did something morally wrong, as the remark during the phone call would suggest, then why did he not say anything about it in this book? Maybe Stueckelberg did not write any paper about his approach, so would this be reason why Schwinger did not include any of Stueckelberg's papers? I wonder if these questions are legitimate…

6. **Tony Smith**
December 15, 2005

Crease and Mann say in their book The Second Creation (revised edition, Rutgers 1996) (page 143) that Stueckelberg "… in the army, was almost totally isolated from physics. Nonetheless, he apparently wrote up a lengthy paper – in English, for once – that outlined a complete and correct description of the renormalization procedure for quantum electrodynamics. Sometime in 1942 or 1943, he apparently mailed it to the Physical Review. It was rejected. "They said it was not a paper, it was a program, an outline, a proposal," Stueckelberg remembered. … Stueckelberg was not a bitter man. … We asked if he had the manuscript, which would help him establish priority. "I never cared much about that question," he replied. "I don't know what happened to the original copy. I lost it, it completely disappeared. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **secret milkshake**
December 15, 2005

It is normal that several people have the same key insight but it matters what they do with it.

I take it that Feynman was just being very nice to Stueckelberg, mentioning him (for getting on the right path before anybody else). F was always bending backward to aknowledge people like Jehle, Wheeler and Bethe from whom he got the helpful ideas – S. was not a source of F inspiration but he deserved the honorable mention. Also, had Tomonaga not published in wartime Japan, he would hardly get more credit then Stueckelberg.

8. **D R Lunsford**
December 15, 2005

Stueckelberg had a German name and tried to get a paper published in the pre-eminent American physics journal. In 1943. Before D-Day.

Read the paper I linked. Weisskopf states:

Already in 1934 […] it seemed that a systematic theory be developed in which these infinities [divergent radiative corrections] are circumvented. At that time nobody attempted formulate such a theory […].There was one tragic exception and that was Ernst C.G. Stueckelberg. He wrote several important papers in 1934-38 putting forward a manifestly formulation of field theory. This could have been a perfect basis for developing of renormalization. Later on, he actually carried out a complete renormalization procedure in papers with D. Rivier, independently of the efforts of other authors. Unfortunately, writings and his talks were rather obscure,and it was difficult to understand them or to make use of his methods. He came frequently to Zurich in the years 1934-6, when I working with Pauli, but we could not follow his way of presentation. Had Pauli and I myself been capable of grasping his might well have calculated the Lamb shift and the correction the magnetic moment of the electron at the time.

There is little to argue about. There is no doubt at all that Stueckelberg had the whole program down.

-drl

9. **sunderpeeche**

December 15, 2005

Many Nobel Prizes are controversial
Anthony Hewish (pulsar) – no prize to Jocelyn Bell
Ken Wilson (RG) – many people (incl Wilson) feel Leo Kadanoff + Michael Fisher should have shared prize
prize for (integer) Quantum Hall Effect ….
prize for MRI ….
no prize John Bahcall solar neutrino
no prize Lise Meitner etc discovery nuclear fission (because of WW2 + bomb)
The list will not end anytime soon

10. **Dissident**
December 15, 2005

Could somebody please remind me why we attach such significance to a prize awarded by a bunch of Swedes, apparently expecting the choice of recipients to reflect superior competence? Because of all the outstanding contributions to physics made by the Swedish Cook…?

(I can hear somebody in the backrows shouting "Klein!". Sorry, no. Klein was a mathematician – and as the story goes, Alfred Nobel explicitly excluded mathematics from the list of awardable subjects because he was after the same lady as Klein, whom he knew would have been a strong contender for the prize. Hardly an auspicious start.)

11. **sunderpeeche**
December 15, 2005

Nobody knows how the prizes attracted such prestige, even from the early years. As for Nobel and mathematicians and ladies, there is much hogwash on the subject. Don't believe most (all?) of it. Nobody really knows either.

12. **Tony Smith**
December 15, 2005

DRL mentions the paper at http://arxiv.org/abs/physics/9903023 which (in addition to the very interesting part quoted by DRL) also states:
"… Stueckelberg came to the University of Zurich in 1933 … Stueckelberg was … Privatdozent at the University of Zurich with professor Gregor Wentzel …".

Crease and Mann, in their book The Second Creation cited above by me, quoted Stueckelberg as saying, about his 1942 or 1943 submission to the Physical Review:
"Afterward, I was told that our friend and teacher, Gregor Wentzel – he was the expert [referee] – he got my paper."

So, to me it appears that Stueckelberg's 1942 or 1943 paper was not rejected by American Physical Review due to wartime hostility against Stueckelberg (who after all was in the army of neutral Switzerland),
but was in fact torpedoed by his former teacher Wentzel.

I also find it interesting that Stueckelberg's submission to the Physical Review in New York could have been floating around in the USA physics community (as a rejected but possibly interesting program) after its submission in 1942 or 1943, well in advance of the 1948 Shelter Island conference at which Feynman and Schwinger both presented their approaches to QED.

According to the paper physics/9903023 cited by DRL
"… The main innovation of the 1934 paper [by Stueckelberg] is the introduction of a new perturbative scheme yielding manifestly relativistic expressions for the matrix elements. This is achieved by performing a four-dimensional Fourier transformation of the wave-function, thus eliminating space and time variables … Stueckelberg's procedure is thus the first departure from the "older (Dirac) form of the perturbation
theory … The approach proposed by Stueckelberg is far more powerful, but was not adopted by others at the time. …".
As DRL has indicated, Weisskopf felt that Stueckelberg's 1934 ideas would enable calculation of "the Lamb shift and the correction of the magnetic moment".
In my opinion, it is almost certain that Stueckelberg's 1934 key ideas would be set out in his 1942 or 1943 submission to the Physical Review.

As to the time of Feynman soving the QED problem, in 1941 (according to Mehra's Feynman biography The Beat of a Different Drum (Oxford 1994)) Feynman had the inspiration from Dirac's paper of using the Lagrangian method, which led to Feynman's 1942 Ph.D. thesis. As to that thesis, Mehra says "… Feynman mentioned that "the problem of the form that relativistic quantum mechanics, and the Dirac equation, take from this point of view, remains unsolved. …". So, Feynman's Shelter Island relativistic QED solution was developed after his 1942 Ph.D. thesis.

Therefore, it seems to me that Feynman's solution of the relativistic QED problem was probably achieved during the time when Stueckelberg's 1942 or 1943 paper could have been circulating in the USA physics community as a manuscript rejected by the Physical Review,
and it is probable that Stueckleberg's rejected manuscript did in fact contain the key to solving the relativistic QED problem.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

13. **robert**
December 15, 2005

Like Stuekelberg, Sudarshan can claim to be multiply mis-used, as he was, with Marshak, pre-empted in the announcement of V-A by Feynman (again) and Gell-Mann, under slightly questionable circumstances. This too caused considerable rancour and unpleasantness, which MGM, for once, did his best to minimise. Marshak's 'Conceptual foundations of modern particle physics' makes very scant reference to F-G in its discussion of this topic.

14. **JE**
December 15, 2005

"Alfred Nobel explicitly excluded mathematics from the list of awardable subjects because he was after the same lady as Klein…"

Fairly speaking, it seems Mittag-Leffler was the contender A. Nobel had in mind when he decided against a Nobel prize in math. Mittag-Leffler was Swedish, unlike Klein, who was German.

15. **Chris Oakley**
December 15, 2005

I am looking at the paper about Stueckelberg linked by Danny(http://arxiv.org/abs/physics/9903023), and am amazed that I was never aware of it – 4D Fourier transforms, power series expansion in the coupling constant – all the stuff I was playing with 20 years ago, and 50 years after Stueckelberg first dreamed it up. I am also surprised that none of the referees for my papers picked it up either. Maybe they too were unaware of his work. If it is leading where I think it is leading, then, yes, you can do manifestly covariant perturbation theory, and you can get agreement with tree Feynman graphs, but you still get infinities exploding all over the place from second order upwards. IIRC Stueckelberg's solution was to start assigning meaning to meaningless quantities (infinity minus infinity) – as everyone in the subject but me seems to be happy to do. A better solution, though, is just to choose equations of motion where the infinities do not appear in the first place. I am going to find out more, and will update the QFT part of my web site once I have studied his work properly.

16. **Dissident**
December 15, 2005

JE, Klein really was a Swede (the common misunderstanding about him being German is probably due to the German-sounding family name). See e.g. http://www-groups.dcs.st-and.ac.uk/~history/Mathematicians/Klein_Oskar.html
(just happens to be the first link that comes up on Google).

Yes, I've seen the mittag-Leffler version of the Nobel story, too. Admittedly, the dates make more sense in that version. But who knows…

17. **JE**
December 15, 2005

Dissident – You're right, for chronological reasons I thought you were referring to Felix Klein (who was contemporary with Nobel).

18. **Brett**
December 15, 2005

The story about Nobel not giving a prize in mathematics because of competition for some lady's affections is a many-times debunked urban myth. (Nobel never married, but he had a regular girlfriend for many years, and she was never involved with any mathematicians.)

If you actually read the terms of Nobel's will, the reason there is no mathematics prize is self-eveident. Math just doesn't fit into the framework; Nobel wanted to reward much more applied and immediate research than the committees that have actually presented the awards. In fact, the committees consistently violate the rules Nobel set down. (At least one of the prizes–chemistry, I believe–is specifically supposed to be for discoveries made in the last year, for example.)

19. **Dissident**
December 15, 2005

Oh, come on Brett! So he had this one girlfriend – and never ever laid eyes on another woman? And he was smart enough to come up with dynamite and build a business empire – but not to write terms for the prize excluding mathematics for socially acceptable reasons?

20. **Dissident**
December 15, 2005

JE: yes, having now checked the dates, I see Oskar Klein was 2 years old when Alfred Nobel died, so I think we can safely debunk his having had any role in the story at least!

21. **fh**
December 15, 2005

There is a moral here. It is not important who first dreams up the idea, it is important who first contributes it.
That is, it is important to formulate this ideas in a language that makes them accessible to non-geniuses (or in Stückelbergs case to the co-geniuses of the time like Pauli and Weisskopf). To get the ideas "out there".

22. **Chris W.**
December 15, 2005

Since this post mentions a miscellaneous collection of things, I thought I would throw in something a bit out of left field, from today's Wall Street Journal:

> **A Hedge-Fund Titan's Millions Stir Up Research Into Autism**
>
> In their quest for answers, the Simonses aren't just another family seeking comfort. Audrey's father, world-class mathematician James H. Simons, runs Renaissance Technologies Corp., one of the world's most successful hedge funds. With little notice, the family's charitable foundation has in the past two years committed $38 million to find the causes of autism. The money manager says he and his wife will spend $100 million more in what is rapidly becoming the largest private investment in the field.

23. **Chris Oakley**
December 16, 2005

fh,

In the case of Stückelberg, the HEP community seems to have retained the chaff (his work on renormalization) while throwing out the wheat (his covariant perturbation theory). There really ought to be more of a clue about the latter in mainstream QFT literature. I am in the Bodleian (Oxford U) today to chase up the references, and when I am finished I will hopefully be able to say something about his work on my web site, at least.

24. **Juan R.**
December 16, 2005

Well, i do not know personally to Sudarshan but know his interesting work on Kaon systems and his generalized quantum mechancis presented at Solvay conference of last 97. My colleague Gonzalo Ordonez at Texas always said that Sudarshan was a "very good profesor" there, at the Prigogine Institute.

Also i may say that i am :-0 by the history of Feynman and Stückelberg.

For anyone interested in the politics beyond the Nobel Prize can read fascinating and extensively documented (The author, who had access to the Nobel archives, spent 20 years researching) book by recognized historian Robert Marc Friedman: Politics of Excellence: Behind the Nobel Prize in Science.

Available at amazon

An interesting review (from Kauffman) was published in the top journal [Angew. Chem. Int. Ed. 2003, 42(11), 1194].

An excerpt:

> the archives show that from their inception the awards have reflected the changing priorities, arrogance, racism, hostility, sexism, inconsistencies, politics, ambitions, open and hidden agendas, biases, rivalries, vanities, pettiness, prejudices, and narrow personal, scientific, and cultural self-interests of comittee members who evaluate nominations. He concludes that the process is nowhere nearly as impartial or objective as generally believed

Juan R.

Center for CANONICAL |SCIENCE)

25. **sunderpeeche**
December 16, 2005

Alfred Nobel invented dynamite, built a successful business empire (and never married, had a girlfriend etc). It is also a documented fact that his will took EVERYONE by surprise. It was hastily written and the terms of the prizes were vague (but it did say "work in the previous year" for physics, chemistry, medicine not just chemistry). It took 3 years (I think) for the lawyers to sort it out. Also the academicians usurped the prizes for themselves. So for example Thomas Edison never won a prize even though his technological contributions to society were many and profound. In fact it is unusual that Guglielmo Marconi was awarded a prize for the wireless.

Many NPs have been controversial since the early days.
All the stuff about Kein, Mittag-Leffler etc is urban legend.

26. **Chris Oakley**
December 16, 2005

Hi Danny,

Thanks again for the tip about Stueckelberg. I managed to get copies of his papers. The 1934 one seems to be key, but my German was not up to reading it properly, so I mainly relied on the pre-digested version supplied by Lacki, Ruegg and Telegdi.
The QFT pages on my web site now take account of his work.

<my personal view>
I am gobsmacked. There he was with a much better way of doing things than Feynman (Gell Mann apparently even referred to Feynman diagrams as "Stueckelberg diagrams") and yet he seems to have spent much of his time messing around with renormalization.

Did he not realise that minimal substitution is just *a guess based on classical field theory* and that there is no physical principle that requires it – ??? Tamper with the field equations just a little and the need to renormalize goes away.

</my personal view>

27. **D R Lunsford**
December 16, 2005

Chris, excellent, I'd be happy to translate the whole paper and perhaps you could fill me in on what I'm missing 🙂 Just mail a scanned copy.

-drl

28. **D R Lunsford**
December 16, 2005

Chris, I'd have to say miminal coupling is the trace of conformality, but we could discuss it later perhaps.

-drl

29. **Chris Oakley**
December 16, 2005

Hi Danny,

Thanks for that. A translation would certainly help, although maybe one of Lacki, Ruegg and Telegdi has already done it.
Remind me of your e-mail address & will send scans (19 small journal pages, BTW).
BTW: What about follow-ups on this work? Was it just that he never bothered to to get stuff published?

30. **Tony Smith**
December 16, 2005

As to later publications by, or about the work of, Stueckelberg:
According to The Second Creation by Crease and Mann:
Stueckelberg "… … struggled to carry out the program rejected by the Physical Review. By the end of the war, in 1945, he seems to have done it. … he wrote up bits and pieces of his ideas. Eventually they were presented in a complete form in a chapter of the thesis of one of his students, Dominique Rivier. … HPA 22, no. 3 (1949):265 …".
HPA refers to Helvitica Physica Acta. According to an ICTP Trieste web page it may no longer be published, but have been superseded by Annales Henri Poincare.

Crease and Mann say: "… by then [1949 when Rivier's thesis was published] Schwinger had come out with his program, and Stueckelberg, who had the ideas first, published afterward …".
I guess that the idea that publication, and only publication, establishes priority is the (in my opinion flimsy and unjust) basis for denying Stueckelberg a prominent place in the history of physics.

If a world-wide physics e-print archive had existed back in the 1930s and 1940s, maybe it would have allowed Stueckelberg's priority to have been recognized by the physics establishment. However, if such an archive had blacklisted Stueckelberg, then maybe it would not have been helpful to him.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

31. **Chris Oakley**
December 16, 2005

Hi Tony,

Thanks for that information. What bothers me is not Stueckelberg publishing after Schwinger so much as Stueckelberg not publishing certain things *at all*. I am not sure when Helv Phys Acta became Ann Henri Poincare, but either way I am sure that I will be able to find the reference you give, which I will check. I am sure that Stueckelberg would always have been able to publish in *this* journal, being the local boy, even if the likes of Ann Phys and Phys Rev were reluctant …

32. **JC**
December 16, 2005

Chris Oakley,

Did you look up the citation indices for Stueckelberg's 1934 paper and/or it's english translation? It would be interesting to see what other papers cited it before Schwinger, Feynman, etc …

33. **D R Lunsford**
December 16, 2005

CO: antimatter33 -@- yahoo -.- com

Thanks in advance.

-drl

34. **Chris Oakley**
December 17, 2005

Danny … thanks.

JC,

This article http://arxiv.org/abs/physics/9903023 tells the story of Stueckelberg's covariant perturbation theory (BTW: Peter, why can't I seem to get <a> … to work any more? Am I doing something wrong?)
Although I have not checked in any citation index, it looks as though, citation-wise, Stueckelberg's 1934 Ann. Phys. article did not do much better than my (independent) re-creation of some of the arguments more than fifty years later … I know for a fact that one can take the arguments much further than he does in this paper, and I would very surprised if he or someone else had not done so. But I will have to check. The Lacki/Ruegg/Telegdi paper sounded fairly convincing, though.

35. **Juan R.**
December 17, 2005

Chris Oakley,

probably you are already addressed this, but let me note that Gell-Mann may be not a neutral source regarding Feynman contributions, due to heavy rivalry between both and Gell-Mann public accusations of 'plagiarism'.

Any case your discussion is very interesting since i unknow this part of history of physics: Stueckelberg vs Feynman.

Juan R.

Center for CANONICAL |SCIENCE)

36. **Chris Oakley**
December 17, 2005

Hi Juan,

I agree about the Feynman/Gell-Mann rivalry.

Bjorken and Drell, vol. 1 talks about "Stückelberg-Feynman positron theory", giving references Helv. Phys. Acta **14**, 32L, 588 (1941) for Stückelberg and Phys. Rev. **76** 749, 769 (1949) for Feynman. I have not yet studied the Stückelberg paper, but I am guessing that he does not use the covariant perturbation theory he worked out in 1934 here.

37. **Chris Oakley**
December 21, 2005

Following on from the above, and after a day ferretting around various Cambridge libraries, it seems that Lacki/Ruegg/Telegdi have done a fine job in assessing the impact of Stueckelberg's Covariant Perturbation theory. The only cites of his 1934 paper between 1945 and 2002 are the Weisskopf review article in *Physics Today* (1981) and a list of all his publications to date at the start of Helv. Phys. Acta **38** in 1965 when they were celebrating his 60th birthday.
I do not think that he followed up on it much at all.

Susskind Interview at New Scientist

December 15, 2005
Categories: Uncategorized

There's an interview with Susskind in the latest issue of New Scientist by Amanda Gefter, entitled Is String Theory in Trouble? Susskind makes many of the same points as in his recent book The Cosmic Landscape: mixing up positivism and falsifiability, attacking those who ask for falsifiable predictions as "Popperazi", and saying that the best he can come up with as a prediction from his ideas is the very long-shot that the negative curvature of space due to its origin in bubble nucleation has not been made vanishingly small by inflation.

There's a discussion forum about the article on the New Scientist site that people might want to contribute to.

Update: Ken Silber writes in to point out that William Dembski, one of the most prominent Intelligent Design ideologues, has now latched on to the string theory controversy as evidence that mainstream science is no better than ID. Dembski has both comments on Susskind and comments on David Gross's admission that string theory is in trouble.

I've been pointing out to string theory partisans for a while that they need to publicly confront Susskind and his followers over their abandonment of the scientific method, otherwise they will have no argument against Intelligent Design. Susskind is making all this much worse with his dismissive comments about the falsifiability of evolutionary theory, as well as the following from the New Scientist interview:

If, for some unforeseen reason, the landscape turns out to be inconsistent – maybe for mathematical reasons, or because it disagrees with observation – I am pretty sure that physicists will go on searching for natural explanations of the world. But I have to say that if that happens, as things stand now we will be in a very awkward position. Without any explanation of nature's fine-tunings we will be hard pressed to answer the ID critics. One might argue that the hope that a mathematically unique solution will emerge is as faith-based as ID.

Update: Susskind is fast becoming the darling of the IDers. A new posting on the web-site "Intelligent Design the Future" run by the Discovery Institute links to a review by IDer and nuclear physicist David Heddle entitled Susskind's Sophie's Choice.

Heddle concludes:

Susskind has presented the physics community with what is, for some (not this writer), a Sophie's Choice: a hidious, complictated, unfalsifiable String-Theory Landscape, or Intelligent Design.

Susskind rocks.

Comments

1. **Dissident**
   December 15, 2005

   So New Scientist has this moderated "discussion forum" with an invitation to post and highly visible links leading to it both from here and from Cosmic Variance. As I write this there are 127 views, a handful of negative comments in the Cosmic Variance thread – and 0 (that's zero) posted comments in the "discussion forum". Shall we guess that they are waiting for a few positive responses for balance? 😉

2. **Tony Smith**
   December 16, 2005

   In the New Scientist interview, Susskind says: "… Even most of the hard-core adherents to the uniqueness view admit that it looks bad. …".

   If Susskind is restricting his universe of "adherents" to superstringers, then indeed the difficulty of superstring theory in predicting anything does make it look bad for uniqueness emerging from superstring theory.

   However, the fact that the basic structure of gravity plus the minimal non-supersymmetric standard model is so good at explaining experimental results, even though it is a remarkably restrictive and predictive structure that permits only SU(3) color, SU(2) weak, and U(1) electomagnetic forces along with gravity, and permits only 3 generations of leptons and quarks with clearly defined charges and helicity,
   seems to me to be an indicator that nature IS probably aligned with a unique model,
   even though it is becoming increasingly clear that superstring theory will not be the way to get to it.

   Just because there is now no widely accepted unique model that specifies all the observed force strengths, particle masses, etc, does not mean that such a model does not exist (it might even turn out to be a lot like my currently-much-ignored model).

   In short, it seems to me that, although Susskind's remark that things look bad for the uniqueness view does apply to superstring theory,
   it does not apply to other approaches based on gravity plus the standard model.

   That, in turn, indicates to me that physics would be advanced if some of the army of people and flood of funding now being spent on superstrings were to be applied to such other approaches.
   Failure of the physics community to make such changes indicates to me a failure of imagination, and fear of trying to think creatively and deeply about gravity plus the standard model, on the part of the people in the now-dominant superstring community.
   Perhaps the leaders of the superstring community actually fear the possibility that an unconventional approach might be successful, because it would disrupt the current rigid sociopolitical "pecking order" of the present superstring establishment.

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

3. **nitin**
   December 16, 2005

   "We can hope to get an answer from string theory and we can hope to get some information from cosmology." Surely Susskind is a bit misled here in his beliefs. A possible answer would naturally come from (observational) cosmology and hopefully some information from string theory (which is, so far as the theory presents itself, rather unlikely)!

4. **secret milkshake**
December 16, 2005

"Ask not what you can do in Physics – ask what you can do to it."

5. **Christine Dantas**
December 16, 2005

If the criteria for publishing in a high-impact journal, e.g., the Physical Review Letters, is taken at face value, for instance (see the guidelines in their site):

> "(…) The paper must satisfy criteria of validity, importance, and broad interest. The work must be sound, free of detectable error, and presented in reasonable detail. (…) Papers advancing new theoretical views on fundamental principles or theories must contain **convincing arguments** that the new predictions and interpretations are distinguishable from existing knowledge, at least in principle, and do not contradict established experimental results. (…)"

(my boldface) then how anything related to the anthropic reasoning or the landscape issue can be possibly accepted there? I didin´t search to see. But of course, it seems clear that it depends on how "convincing" your arguments are.

6. **Juan R.**
December 16, 2005

String theory always was in trouble. Is cosmological constant explained via graviton mass?

The problem of string theory is NOT that it 'predicts' multiple universes and, therefore, one may use previous knowledge for fixing undetermined parameters of the theory (more than 10000 in some models of compactification 10D –> 4Dx6D). The serious problem of string theory is that none of those 'billions' of possible universes coincides with real universe we observe on Nature. For example, string theory works with supersymmetric massless states, whereas our observed universe is non-supersymmetric and non-massless. Imagine that we observe supersymmetric at next HLC data. It does not mean that string theory is correct, because any plausible theory of supersymmetry (if finally observed) may reduce to the experimentally verified non- supersymmetric theory we name the Standard Model, somewhat as general relativity reduces to special one when gravitation vanishes. Simply we may admit string theory is incompatible with all data we know. In fact, there is a joke circulating by the Internet saying that the best definition of observable universe is "that string theory cannot explain".

What was the main motivation for string theory in the past? Simple: that "*it predicts gravity*". Well again this is not correct, the 'gravity' contained in string theory is **not** Newtonian gravity **not** relativistic gravity.

Newtonian gravity describes direct interactions between non-massless particles, but current string theory spectra are massless states and there are not electrons or protons defined. Precisely, since the graviton is massless, it can be defined in string theory. However, if finally the graviton has some mass as some theorists recently argue [see for example Class. Quantum Grav. 20 No 6 (2003) L67-L73 or recent 2004 talk by Vainshtein here] then string theory could not explain it.

What is more, the relativistic gravity contained in string theory is a 2-spin graviton over a flat background metric. That is not general relativity, as explained in textbooks on matter, and the reason that all of us know that 10D string theory -even if some day was internally consistent and compatible with experimental data- would be improved by its background-independent 11D generalization currently called M-theory.

Many other criticism on why string theory is not good enough is addressed in moderated newsgroup sci.physics.strings in the Oct 21 post "String theory is not a TOE"

Juan R.

Center for CANONICAL |SCIENCE)

7. **Dissident**
December 16, 2005

Juan: http://arxiv.org/abs/hep-th/0512177

The problem isn't that string theory can't produce something that looks like our universe. It's that it can produce just about anything you want, so it doesn't predict anything.

8. **Juan R.**
December 16, 2005

Christine Dantas,

perhaps this link to recent work published in PRL by string theorists Karch and Randall, may be interesting.

I do not read the letter, but it appears that they are doing 'convincing' arguments 🙂 seeing i am finding on the net

> They believe the way our universe started and then diluted as it expanded what they call the relaxation principle favored formation of three– and seven–dimensional realities. The one we happen to experience has three dimensions.
>
> The only assumptions were that it started with a generally smooth configuration, with numerous structures called membranes, or "branes" that existed in various spatial dimensions from one to nine, all of them large and none curled up.
>
> Other realities, either three– or seven–dimensional, could be hidden from our perception in the universe, Karch said.
>
> "There are regions that feel 3D. There are regions that feel 5D. There are regions that feel 9D. These extra dimensions are infinitely large. We just happen to be in a place that feels 3D to us," he said.
>
> "We know there are people in our three–brane existence. In this case we will assume there are people somewhere nearby in a seven–brane existence. The people in the three–brane would have a far more interesting world, with more complex structures," Karch said. With gravity diminishing rapidly with distance, a seven–dimensional existence would not have planets with stable orbits around their sun, Karch said.
>
> "I am not precisely sure what a universe with such a short–range gravity would look like, mostly because it is always difficult to imagine how life would develop under completely different circumstances," he said. "But in any case, planetary systems as we know them wouldn´t form. The possibility of stable orbits is what makes the three–dimensional world more interesting."

AMEN

I only wait that Kaku can someday built a multidimensional teletransporter and all those ‘stringy guys’ can travel to other parallel universe full of nine-branes.

Juan R.

Center for CANONICAL |SCIENCE)

9. **Juan R.**
December 16, 2005

Dissident,

thanks by the link to the recent preprint. But please note that does not invalidate i said. Perhaps, i would resume and emphasize i said:

1) String theory **cannot** predict.

2) There is **not** model in string theory compatible with the experimentally verified non-supersymmetric and non-massless **Standard model** of particle physics.

3) The ‘gravity’ contained in string theory is **not** Newtonian gravity and is **not** compatible with relativistic (general relativity) gravity.

4) brane, stwing, or M theories does **not** solve the problems opened by string theory.

Juan R.

Center for CANONICAL |SCIENCE)

10. **Dissident**
December 16, 2005

Juan: Agree about (1). About (2): the MSSM is an extension of the SM compatible with current experimental data. The LHC may be able to rule it out, but that’s yet to come. About (3): any quantum theory of gravity will deviate from the classical theory at some point (or what would be the point of quantizing it?). Agree about (4).

11. **Jean-Paul**
December 16, 2005

I think that there is a large social/psychological component of the recent anthropic endorsements. There are made by senior, very accomplished physicists who are at the end of their productive lives (or like Polchinski ready for early retirement at 55) and want to ride away into the sunset with “this is the end, my friend” — nothing left to do, we solved all important problems, we won’t miss anything. It is very sad that among younger generation, only Lubos dares to scream foul play…. while the more distinguished people like Vafa and Witten who are clearly opposed to this nonsense hide their heads in the sand instead of energizing their junior colleagues…

12. **Ken Silber**
December 16, 2005

William Dembski has now latched on to Susskind’s landscape-intelligent design dichotomy.

http://www.uncommondescent.com/index.php/archives/583#more-583

But shouldn’t life evolve easily in a “fine tuned” universe?

13. **woit**
December 16, 2005

Hi Ken,

Thanks for pointing that out. Dembski has definitely latched on to string theory, also see:

http://www.uncommondescent.com/index.php/archives/570

14. **Who**
December 16, 2005

Peter quoted Susskind as follows:
Without any explanation of nature’s fine-tunings we will be hard pressed to answer the ID critics. One might argue that the hope that a mathematically unique solution will emerge is as faith-based as ID.

this “hope” is a straw-man. the mainstream scientific enterprise exhibits confidence in the gradual INCREMENTAL explanation of nature—not in the appearance of a complete “mathematically unique solution” explaining everything once and for all. Moreover this confidence is not analogous to religious faith, but has been born out by gradual progress over the course of several centuries and many generations. It is the fruit of long experience.

At least for physicists, the string venture may have been a mistake, and its present quandary should not be interpreted as a portent of anything. One cannot draw wider conclusions from the confused frustration of this one theoretical initiative—-it does not mean anything as regards intelligent design, or the determination of several dozen fundamental numbers, or the cause of the big bang or the fate of science. If string thinking fails to explain the current list of basic numbers, it simply means that a large bunch of theoreticians were misled, perhaps self-deluded, for several decades—this is no reason to start imagining a “sea-change” in the “zeitgeist” (that is portentous over-dramatizing). String is just a hiccup in the history of science.

Susskind’s phrase “without any explanation of nature’s fine-tunings” is also a straw man—at least a rhetorical exaggeration. We have explanation for SOME of nature’s fine tunings, and can reasonably expect to get more. I mean real physical explanation, not anthropic selection from a postulated ensemble based on stringy conjecture.

the the Old Guard show is increasingly embarrassing, I’m disappointed with Wilczek—expected better.

15. **David Heddle**
December 16, 2005

That’s “Heddle” not “Weddle”.

16. **Juan R.**
December 16, 2005

Dissident said,

> Juan: Agree about (1). About (2): the MSSM is an extension of the SM compatible with current experimental data. The LHC may be able to rule it out, but that's yet to come. About (3): any quantum theory of gravity will deviate from the classical theory at some point (or what would be the point of quantizing it?). Agree about (4).

Thanks by your comment. Unfortunately i was not able to express my thinking correctly and i may correct one thing.

Of course, any quantum theory will deviate from the classical one (in the contrary case its quantization would be irrelevant) but i was not saying that. i was saying that the classical limit of superstring theory is not general relativity. I mean that gravity contained in strings and superstrings is a quantum spin-2 (graviton) in a flat classical background defining causality structure (e.g. S-matrix). The classical limit looks as a classical gravitational 'field' propagating on a flat background. But general relativity is not that (in general), and this is the reason that superstring theorists are now anxiously searching a background free generalization called M-theory. Precisely the perturbation formalism around a flat spacetime metric structure has been always the criticism of loop theoreticians to string/superstring theories.

In fact, i think that it is a common misconception the claim that general relativity is equivalent to a theory for a spin-2 field **on** a Minkowski background.

[tex]g^{ik} = \eta^{ik} + h^{ik}[/tex]

I think that this error was perpetuated in literature 'thanks to' Misner, Thorne, Wheeler 1977 popular book and by some popular authors as Feynman or Weinberg.

The equivalence between full general relativity and a [tex]h^{ik}[/tex] field theory is valid only in the weak regime, in fact, Deser [Gen. Rel. Grav. 1970, 1, 9.] proved the equivalence between the 'geometric' and the ' tensor field' view only to third order in the tensor field recurrence series. But this did that some authors claimed that the two views are fully equivalent to any order when are not. In fact, there is crucial differences in the strong gravity regime between both approaches.

Juan R.

Center for CANONICAL |SCIENCE)

17. **blank**
December 16, 2005

I've always thought it was only a matter of time until the IDers latched on to Susskind. In fact I'm surprised it took them so long. He actually had to go so far as to publish a popular book with "intelligent design" written on the cover to get their attention.

18. **woit**
December 16, 2005

David,

Sorry about that. It's fixed now.

19. **woit**
December 16, 2005

Juan,

Please stop posting so many comments that don't have anything to do with the topic of the posting. In this case, if people want to discuss the interview with Susskind, that's great, but the question of the technical problems of string theory as a theory of gravity is off-topic here.

20. **Hans de Vries**
December 16, 2005

Of all contradictions I've never come upon a greater contradiction as this one:
–
Firstly:

The idea that constants of nature, reproducible in 15 digits, going to 16 digits (Frequency Comb work) are represented by 500+ hole clonable Calabi Yau pretzels. (16 digits means an object the size of the earth accurately reproduced with single atom precision)
–
Secondly:

That these Calabi Yau spaces live at Planck's length in a wildly bubbling space-time chaos at Planck's temperature of 1.417 10^32 degrees…

–
Total chaos meets total perfection….

–
Regards, Hans

21. **Eric Dennis**
December 16, 2005

It's important to keep in mind that rejecting anthropic reasoning in the context of the landscape and string theory does not imply a rejection of anthropic reasoning as such. Given apparently fine-tuned aspects of nature, anthropicism is certainly a legitimate sort of explanation if the necessary ensemble emerges naturally. The problem is just when anthropic reasoning becomes the *pervasive* explanatory mechanism of a theory, leaving it without any distinguishing quantitative predictions.

In the context of a healthier theory, I don't see the problem with e.g. the anthropic prediction for bounds on the CC, which I think Peter has criticized (but perhaps I misunderstood his intent). Indeed I think an unwillingness to accept this kind of limited anthropism is rooted in the same epistemic error of the IDers. Limited anthropism recognizes that Nature is just the way She is — She isn't designed so that our theories will be pretty and convenient and maximally deductive — just like She isn't designed so that Jesus could die for our sins. She just plain isn't designed.

22. **Peter**
December 16, 2005

No time for a long answer, but here's a short one:

A legitimate scientific theory has to make strong enough predictions that you can rationally decide whether there is good evidence for it by looking at the world. The string theory anthropic landscape doesn't do this now and there's no good reason to believe it ever can, so it's not science.

One can come up with legitimate scientific theories in which certain quantities are only probabilistically determined, e.g. because they are "environmental", depending on the history of the universe and one doesn't accurately know the initial state. It's certainly possible the CC is such a quantity, but there are lots of problems with the Weinberg "prediction" of it, including:

1. It comes out an order of magnitude or so too high.

2. It's done by taking known values of other similar cosmological parameters, fixing them and just varying the CC. Looking at the joint distribution for several variables, the CC is way, way smaller than one expects.

There's also a conceptual problem that bothers me with this specific piece of anthropic reasoning. The assumed a priori probability distribution for the CC is taken to be flat. Such a "theory" of the CC says about it exactly the same thing as my favorite theory: "Hell, I have no idea what determines the CC, so, a priori any value is equally likely." There's something funny about a "theory" that makes exactly the same "predictions" as deciding that anything is equally likely since you have no idea what is going on.

23. **michaeld**
December 16, 2005

Susskind's comments strike me as very strange.

"But I have to say that if that happens, as things stand now we will be in a very awkward position. Without any explanation of nature's fine-tunings we will be hard pressed to answer the ID critics."

It sounds as if he thinks physicists should be opposing ID just for the sake of it. To me, the reason to oppose ID is that the (scientific) arguments for ID are completely ridiculous. If that ever changed… if it happened that there were strong scientfic arguments for ID, then why should physicists be influenced by this and alter their theories simply for the sake of remaining in opposition to ID?

24. **anon**
December 17, 2005

Michael,

the reason to oppose something just for the sake of it is to blend into the mob. Susskind months ago was quoted on the issue of the reality of the casimir force as saying the problem should be solved simply by banning physicists from using the word "real".

It is just fashionable to sneer at laymen and reality by coming up with speculative 11 dimensional super duper theories which are so value that there are 10^500 different dynamical interpretations, and which predict nothing, and are untestable. Different versions of string theory produce different predictions naturally, so there is no clear prediction to be tested by experiment. That is in fashion.

String theorists have no grasp of science, and think the reason ID is a scientific failure is because it is unpopular in science, and the reason string theory is a success is that it is popular in the media.

String theory is lucid. Everyone knows what it claims: everything is string. They also know it is popular, so you get the herd instinct … so many people can't all be wrong, that sort of thing.

String theorists can't afford to use scientific criteria or they'd sink.

25. **Juan R.**
December 17, 2005

Peter Woit,

I am sorry, often i write in a random manner and click the submit bottom too easily. Please receive my congratulations about your blog. Let me write some few comments about New Scientist interview *Is string theory in trouble?*

i)
The first i want say is that recent Landscape trouble has emphasized the problems of string theory. I have noted in many magazines, media and talks this 2005 the emphasis to claim that string theory is in trouble. It appears that all was fantastic in previous years but we know that is not true: as said above **String theory always was in trouble** and i think that people may know this. Therefore, i would personally rename New Scientist title to *Is today string theory more in trouble that yesterday* or similar one.

ii)
I leave criticism to inflation for other thread.

iii)
About cosmological constant, we already discuss about anthropic reasoning and Weinberg last preprint (or would say last nonsense?) here. One of arguments of anthropic symphatizers is that experimental value for the cosmological constant cannot be explained by usual Standard Model more GR, therefore our best option -they claim- is Landscape. However, that is not true and this is reason i cited above some works where the cosmological constant is computed scientifically from hypotesis of a small mass for the graviton. I repeat again, by no means the Landscape argument is the only possible way to explain cosmological constant. In fact, Landscape way is not a scientific explanation, just metaphysics one.

iv)
About Steven Weinberg recently comment that Landscape is one of the great changes in fundamental science because changes the nature of science itself. I think that Weinberg has definitively lost the North. Landscape is just a return to so-called magic era before modern science born. The only "radical" that i can see is "to call that science", when we could call it "proto-science", "meta-science", "no-science" or similar. Weinberg attitude is so stupid (and i am sorry by the word) as if your cat does not look like a dog and then Weinberg argues that one may change the traditional definition of dog! If string theory does not fit science requirements, then call to string theory "no-science" or "meta-science" but never change the traditional mean of the word "science" for fitting string requirements.

v)
About last Susskind comment

> it seems increasingly likely that the constants of nature are more like the temperature of the Earth – properties of our local environment that vary from place to place. Like the temperature, many of the constants have to be just so if intelligent life is to exist. So we live where life is possible.

I believe it is an excellent way to illustrate anthropic nonsense.

Simply compare replies to scientific questions how "estimate the temperature here", "compute the temperature when", "draw the temperature profile for" using equations and the model-data from scientific literature [see Thermodynamics of Atmospheres and Oceans basic monograph] with **fact** anthropic theorists cannot compute anything and their "explanation" looks like

> the temperature have to be just so if intelligent life is to exist.

It is more, what if there is not life? Curiously the scientific models presented in above monograph can be applied (changing parameters of course, for example to flow radiation received from Sun due to different distance, etc.) to other planets where is not life. See Chapter 14.

vi)
Let me finalize saying that Susskind comment

> No more than when physicists discovered that the radii of planetary orbits were not determined by some elegant mathematical equation, or by Kepler's idea of nested Platonic solid.

Is not accurate since it appears that radii is determined. I have seen scientific models on that.

> There is a philosophical objection called Popperism that people raise against the landscape idea. Popperism [after the philosopher Karl Popper] is the assertion that a scientific hypothesis has to be falsifiable, otherwise it's just metaphysics. Other worlds, alternative universes, things we can't see because they are beyond horizons, are in principle unfalsifiable and therefore metaphysical – that's the objection. But the belief that the universe beyond our causal horizon is homogeneous is just as speculative and just as susceptible to the Popperazzi.

Of course! but Susskind perhaps forgets that serious scientists carefully invented the term *observable universe* for referring to the part of the universe accessible to our current telescopes. No serious scientists claim what there exists beyond is today observable: perhaps pink stars? Nobody knows.

Susskind reply to question "Is it possible to test the landscape idea through observation?"

> So the landscape, at least in principle, is testable.

is, of course, incorrect.

Juan R.

Center for CANONICAL |SCIENCE)

26. **ks**
December 18, 2005

Michael says: *It sounds as if he thinks physicists should be opposing ID just for the sake of it.*

But it is striking that some of Susskinds arguments are mimicking the structure of the conventional ID/creationist ones.

From the Susskind interview:

At first, string theorists thought there were about a million solutions [for string vacua]. Thinking about Weinberg's argument and about the non-zero cosmological constant, I used to go around asking my mathematician friends: are you sure it's only a million? They all assured me it was the best bet.

But a million is not enough for anthropic explanations – the chances of one of the universes being suitable for life are still too small.

Religion is not the only way of reasoning that creates "gap gods" these days.

27. **Alejandro RIvero**
December 18, 2005

http://del.icio.us/url/61f55137a3ed1109f6bef9f59669635d

28. **Tony Smith**
December 18, 2005

Susskind's book "The Cosmic Landscape" is reviewed by George Ellis in Nature (8 December 2005, pages 739-740. The review is titled

"Physics ain't what it used to be".

In that review, Ellis says: "… The particular multiverse version proposed by Susskind, however, has the great virtue of being testable in one respect.
It is supposed to have started out by quantum tunnelling, resulting in a spatially homogeneous and isotropic universe with negative spatial curvature, and hence with a total density parameter OMEGA_o less than 1.
The best observationally determined value for this parameter, taking all data into account,
is OMEGA_o = 1.02 plus or minus 0.02.
Taken at face value, this seems to contradict the proposed theory. … These data are not discussed in the book – a symptom of some present-day cosmology, where faith in theory tends to trump evidence.
Presumably the hope is that this observational result will go away …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

29. **Chris Oakley**

December 19, 2005

[Re: String theory] About Steven Weinberg's recent comment that the Landscape is one of the great changes in fundamental science because it changes the nature of science itself:
I think that Weinberg has definitely lost his bearings. This is just a return to the so-called magic era before modern science was born.
The only "radical" that I see is calling it science, when we should be calling it "proto-science", "meta-science", "no-science" or similar.
Weinberg's attitude is so stupid (and I am sorry to use this word) that if your cat does not look like a dog then Weinberg would argue that one needs to change the traditional definition of "dog"!
If string theory does not fit the requirements of science, then we should call string theory "no-science" or "meta-science". But we should never change the traditional meaning of the word "science" just to accommodate string theory.

Sorry, Juan R. I don't normally go around correcting people's English, but what you say here is so sensible that I felt compelled to do so in this case.

30. **Juan R.**
December 19, 2005

Absolutely no problem with that!

Anyone is free for correcting all my failures from English to math.

Juan R.

Center for CANONICAL |SCIENCE)

31. **Eric Dennis**
December 19, 2005

Peter,

OK, I think we agree on the underlying issue then — the potential legitimacy of some kind of anthropic argument in the context of some independently confirmed theoretical framework. Your point about what happens for the specific case of the CC when you look at the joint distribution given other variables sounds reasonable (if that's in fact the case — I'm not familiar with the nitty gritty here).

But I don't understand your other objections. It's a bound obtained by imposing some galactic nucleation constraint, right? The "prediction" is just that the bound is satisfied, not that it's the most informative conceivable bound. About the flatness of the priors, I guess there's the question (like there always is when choosing priors) about what variable (CC or CC^2 or…) is the right one to have a flat prior distribution on. But aside from that standard Bayesian conundrum, the point of the argument is that we want to impose as little prior information as possible. We're admitting that we have no clue what are all the causal factors in the "environmental" determination of the CC. But at least we might resolve the discrepancy of it's (order of) order of magnitude.

It's inevitable that some physical processes are gonna be just too complicated to calculate, with whatever theory we settle on. If the hypothetical historical dynamics that determined the CC is one such process, que sera.

32. **woit**
December 19, 2005

Eric,

I have no problem with the idea that maybe the CC is something "environmental", depending on the history of the universe, in a way such that we'll never be able to calculate it. What I object to is people running around saying they have a "prediction" of it, based on nothing more than the fact that if it were 100 times bigger, all else being equal, galaxies wouldn't form. Even worse, people go on about this being a "successful prediction", write popular books about how great it is, and use it as an excuse to successfully push a research program which is pure pseudo-science.

33. **Eric Dennis**
December 20, 2005

I agree with that.

34. **Juan R.**
December 22, 2005

How much nonsense does one find in the anthropic idea?

Perhaps reply was in a simple example.

Look the Moon, why is it there?

Scientific reply: It is there (effect) because it is the result of the motion guided by gravity force (cause). We can predict the Moon position at some future instant *t* via mechanics.

Antrophic reply: It is there (effect) because we are looking it ("cause") here. We can**NOT** predict the Moon position at some future instant *t*.

Of course, some questions cannot be today solved from science, but then the (honest) scientist admits that does not know the reply. The rest is meta-science, religion, philosophy, etc.

Juan R.

Center for CANONICAL |SCIENCE)

35. **D R Lunsford**
December 25, 2005

Check this one out:

http://xxx.lanl.gov/abs/physics/0510102

Pornography is more legitimate.

-drl

36. **Juan R.**
December 26, 2005

Here an extract from Lunsford cited preprint

> In one of our favorite scenarios, our universe is a school-assigned science experiment [1, 2] carried out by a high school student in a meta-universe. Perhaps he or she or it even started an assortment of universes like ant farms and stashed them away somewhere in the basement, out of his or her or its parent's way. Perhaps by now he has lost interest and forgotten about the universes, leaving some to expand, others to collapse, in complete futility and silence. But, perhaps not without leaving a message for the occupants….

The interesting is the affiliation:

– Institute of Theoretical Science (University of Oregon)

– Kavli Institute for Theoretical Physics

🙂

Juan R.

Center for CANONICAL |SCIENCE)

37. **anon**
December 28, 2005

Are those dope-testing labs, or mental institutes? 😉

Interview With Alain Connes

December 17, 2005
Categories: Uncategorized

Someone wrote in to tell me about a very interesting interview with Alain Connes, conducted at the IPM in Teheran at the time of the Workshop on Non-Commutative Geometry held there this past September.

As always, Connes has quite a few provocative things to say, including some harsh criticisms of the way string theory research is conducted (this in spite of the fact that the Institute hosting him is dominated by string theorists):

The only thing I resent in string theory is that they put in the mind of people that it is the only theory that can give the answer or they are very close to the answer. That I resent. For people who have enough background it is fine since they know all the problems that block the road like the cosmological constant, the supersymmetry breaking, etc., etc.. But if you take people who are beginners in physics programs and brainwash them from the very start it is really not fair. Young physicists should be completely free, but it is very hard with the actual system.

Connes also has many interesting comments about non-commutative geometry and about his own career, including the fact that he went off in the direction he did because he was put off by the arrogance of the algebraic geometers at the IHES. He also has a lot to say about the importance of having a system like the French CNRS system that allows talented young researchers to develop a long-term research program without too much pressure to achieve quick results. He is quite scornful about the US university system, which he sees as emphasizing money and subjecting young people to huge pressures to work in well-established areas instead of trying to do something new and ambitious.

The interview also contains quite a few amusing stories. In one of them Connes tells about a well-known string theorist who walked out of his talk at Chicago because he wasn't very interested, but two years later was paying rapt attention to the same talk when Connes gave it at Oxford. When Connes asked him about this, the physicist told him that the difference was that in the meantime he had heard that Witten had been seen reading Connes's book in the library at Princeton.

On a completely different topic, there's a nice review article by Edward Frenkel on the Langlands program and conformal field theory. Witten has new ideas about this subject and how it is related to S-duality in four-dimensional gauge theory. I hear he has been working on a paper on the subject since this summer, and that it should appear imminently.

Comments

1. **Aaron Bergman**
   December 17, 2005

   The paper on Langlands is by Kapustin and Witten and should hopefully appear soon, but I don't know when.

2. **QWERTY**
   December 17, 2005

   YOUR LINKS ARE TEH DED!!1!

   I WANT TO READ THE INTERVIEW WITH ALAIN CONNES AND I CANNOT. I AM DESPARATE TO LEARN ALL ABOUT NON-COMMUTITIVE FROBENIUS ALGEBRAS AND HOW TO VISUALIZE ASOCIATED COBORDISMS IN 2D QFT, AND ALSO GENERALLY DRAW ALEPH-ONE INSPIRATIONS FROM HIS WISE FRENCH WORDS AND REFINED YET SARDONIC DEMEANOR.

   PLEASE ADVISE.

3. **Adrian Heathcote**
   December 17, 2005

   I've been a great admirer of Connes for some years. I think he has great intellectual honesty and courage—and judging by his KITP talks a good sense of humour. (And speaking of the KITP talks he took on that audience (trial by interruption!) and won.)

4. **Thomas Larsson**
   December 18, 2005

   What caught my eye in Frenkel's lecture was the statement on the top of page 27:

   "Before we get to that, we want to comment on why is it that we only consider curves and not higher dimensional varieties. The point is that while function fields on curves are very similar to number fields, the fields of functions on higher dimensional varieties have a very different structure. […] At the moment no one knows how to formulate an analogue of the Langlands correspondence for the field of functions on an algebraic variety of dimension greater than one, and finding such a formulation is a very important open problem."

   Since geometric Langlands is evidently closely related to the affine and Virasoro algebras, perhaps their higher-dimensional analogues have something to do with a higher-dimensional Langlands. Or perhaps not.

5. **Adrian Heathcote**
   December 18, 2005

   QWERTY

   Try now.

6. **Tony Smith**
   December 18, 2005

   In the IPM interview at http://www.ipm.ac.ir/IPM/news/connes-interview.pdf Connes said:

   "… The true question is whether or not string theory has anything to do with reality … That key question begins by supersymmetry; whether or not nature is supersymmetric … If one would already have found 3 or 4 super partners by now, then I would believe they would find the others but in reality they haven't found any and because of that I'm very skeptical. … The supersymmetric standard model is an horrible thing … with more than a hundred free parameters and an ugly mechanism to

break that “beautiful” unseen supersymmetry! …
I think it’s very important to construct other competing models which are not necessarily based on supersymmetry. I think it is crucial for the development of physics that there are people who are courageous enough not to follow the main dogma, heretics that develop a different model.
…
From my point of view the actual system in the US really discourages people who are truly original thinkers … The US are successful mostly because they import very bright scientists from abroad. For instance they have imported all of the Russian mathematicians at some point.
…
I believe that the most successful systems so far were these big institutes in the Soviet union, like the Landau institute, the Steklov institute, etc. Money did not play any role there, the job was just to talk about science.
…
the way the young people … in the US … get their position on the market creates “feudalities” namely a few fields well implanted in key universities which reproduce themselves leaving no room for new fields. … Beginners have little choice but to find an adviser that is sociologically well implanted … so that at a later stage he or she will be able to write the relevant recommendation letters and get a position for the student … all these letters look alike in their emphatic style. The result is that there are very few subjects which are emphasized and keep producing students and of course this does not create the right conditions for new fields to emerge. …”.

Since I am a patriotic citizen of the USA, I wish that I could disagree with Connes, but as one who works on heretical non-supersymmetric physics models and has been blacklisted by the Cornell arXiv, I cannot.

Tony Smith
http://wwww.valdostamuseum.org/hamsmith/

7. **Chris Oakley**
December 18, 2005

Tony,

Although I could never possibly agree with Connes’ attitudes towards QFT, I think that his analysis of the research machine, especially in the U.S., is right on the button. In view of this, if you are being blackballed by the ArXiv you should should regard it as flattery of the most sincere kind.

8. **Doug**
December 18, 2005

I would like to know who the top 10 “heretics” are. It appears that Danny and Tony are considered “heretics,” but would they make the top 10 list? What is the difference between “crackpots” and “heretics?” If mainstream string theory is “not even wrong.” then is it heretical?

9. **anon**
December 18, 2005

Doug, string theorists are crackpots. Real scientists are always heretics to religious dogma which has no evidence.

All those who believe in the mainstream because it is fashionable are crackpots. Dawkins states you have to be open minded in science, but not so much so that your brains fall out completely

10. **Anonymous**
December 18, 2005

QWERY IS THE BEST COMMETNAR EVAR LOLLERSKATES

But seriously, I think there is an important issue here regarding the US university system, and it’s much more important than your typical string theory complaints, Peter. Or perhaps your usual string theory complaints are just a specific instance of a broader problem. And that problem is that young people have to sustain a high rate of publications. As soon as one reaches the point that they begin publishing, one has little freedom to spend a long time delving into a hard problem. If one wishes to work on very difficult problems, one needs lots of publishable intermediate results. Also, one should be sure to publish things mainstream enough to help one get jobs.

This isn’t a completely terrible situation; academics, even young ones, still have considerable freedom, and some sort of politics and job pressures are unavoidable. But I think the culture could change in some beneficial ways.

Of course, it doesn’t help that a lot of the much-touted less mainstream ideas have blatantly obvious problems, so that nearly everyone outside the mainstream is crackpot or borderline crackpot.

11. **woit**
December 18, 2005

Hi Anonymous,

Thanks for your comment. I do think the problem of young scientists being forced by the job market to stick to research with a quick payoff is a significant part of the problem with string theory (and have written about this in various places, including my first public critique of string theory, from about 5 years ago). Particle physics is suffering more than most subjects from this because it is a victim of its own success: the standard model is too successful and it is very hard to see how to get beyond it. Progress in this field will probably require a major conceptual leap in a new direction, not just small incremental advances, and the way the US academic system is organized makes it much harder for a young theorist to try and do this without committing professional suicide.

The physics community needs to acknowledge this problem and start thinking about ways to deal with it. But one reason I’ve focused on the issue of getting people to acknowledge the failure of string theory based unification is that as long as the perception is that string theory is a viable idea making progress towards its goals, people are unlikely to agree that this kind of action needs to be taken.

While making it easier for young people to work on more ambitious research programs would help, that by itself probably won’t do the trick. A French string theorist friend correctly points out to me that young theorists in France with its CNRS system, and in some other similar European systems, haven’t done much better than those in the US in terms of coming up with something new.

It’s very difficult to figure out how to properly structure a reward system to encourage the kind of difficult, long term speculative work in new directions that particle physics needs. But I believe before this can be addressed, first the physics community has to realize how big a problem this is, and that some dramatic changes in traditional ways of doing business are needed.

12. **Steve**
December 18, 2005

I do not wish to go into philosophy or ethics, but I think another problem causing young people to rush into string theory is the desire to "show off" how smart they are; and reading Alain Connes' interview was such a comfort;

"I think in mathematics it is extremely important to be persistent. The point is not being brighter or faster. Forget it! What is important is to never abandon a problem"

— He is truely a great human being.

13. **Anonymous**
December 18, 2005

Peter wrote

Particle physics is suffering more than most subjects from this because it is a victim of its own success: … Progress in this field will probably require a major conceptual leap in a new direction, not just small incremental advances

Err, yes and no? I'm not quite sure what you mean by "progress in this field," but the LHC is almost certainly going to move particle physics forward in a big way. If it finds supersymmetry then maybe we can start paring down the huge space of SUSY models to something workable. If it finds new strongly coupled physics then the whole focus of the field will shift, I think. And with any luck, the whole industry of extra dimensional models will vanish (except to whatever extent they prove to be useful as duals). I think a lot of nonsense is going to get swept away in the next decade.

On the other hand, certain problems like quantum gravity almost certainly *do* need a major conceptual leap (as I'm sure most string theorists would agree). Other problems, like a better nonperturbative understanding of gauge theories, might actually be amenable to incremental progress, if people were to really focus on them. There are a lot of tools around, after all. On the other hand, a lot of them aren't trendy at the moment, so such work can be risky for young people.

It's very difficult to figure out how to properly structure a reward system to encourage the kind of difficult, long term speculative work in new directions that particle physics needs.

Agreed. To think out loud a bit: we certainly don't want to encourage just *any* speculative work. At this point I think various "alternative" ideas in quantum gravity (*cough*Reuter*cough) have *far more* partisans than they deserve, for instance. Truly promising new approaches are rare, and while it might be hard to know if they will be successful, it's pretty easy to see that many ideas will be *un*successful. So I think a few high-paying prestigious postdoc fellowships for people outside the mainstream could make a huge difference, *provided* that they were selected by well-respected and intelligent people who could filter out only the very best candidates, and that it could somehow be ensured that if these people's ideas didn't pan out that it does not mean the end of their career. It seems like the Clay Institute's fellowships in mathematics might be the analogue of the sort of thing I have in mind. But generally I get the impression that independence is more highly valued in mathematics than in physics.

14. **Tony Smith**
December 18, 2005

Peter Woit said "… young theorists in France with its CNRS system, and in some other similar European systems, haven't done much better than those in the US in terms of coming up with something new …".

Connes said "… the most successful systems so far were these big institutes in the Soviet Union, like the Landau institute, the Steklov institute, etc. Money did not play any role there, the job was just to talk about science. …".

Maybe both of the above statements might be correct. Two points then come to mind:

1 – Perhaps "big" is an important word, and a critical mass size is necessary for such institutions to be successful.
That is, maybe the "big institutes in the Soviet Union" were so large that the (probably small) percentage of useful creative innovators gave a reasonably large number of useful creative innovators, with the larger number of relatively unproductive people being tolerated as inevitable overhead,
while
the French CNRS and other similar European systems might be too small to produce a reasonably large number of useful creative innovators.

2 – Perhaps the French/European systems might be overemphasizing USA-type "efficiency", and concentrating too much on the idea of Anonymous that the process should "filter out only the very best candidates". If Anonymous's "filtering" were to be done by "well-respected and intelligent people" who are determined to be so by their status in the physics community, then it seems to me that in the case of the present-day USA physics community, no candidates outside superstring theory (90%) and LQG (10%) would be chosen.

Since the USSR is gone, there are no contemporary examples to support or refute 1.

As to 2, it seems to me that the demographics of North American institutes such as IAS, KITP, Perimeter, etc., are consistent with 2.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

15. **secret milkshake**
December 18, 2005

Since I had some exposure to the science establishment in Eastern Europe in late 80s, I can say it was not a model how science should be done. Please note how many people in USSR went into physics and math – and how little came out of it.

16. **Adrian Heathcote**
December 18, 2005

When I first heard of Non-Commutative geometry I enthusiastically told my wife about it, explaining as much as I understood. She thought for a minute and then said, 'No'.

So I had a T-shirt made up for her, in Soviet-style lettering (she's Russian): "Just say 'Non' to Non-Commutative Geometry". When she wears it out she is often asked what it means!

I have a feeling that Connes would like the joke.

17. **Tony Smith**
December 18, 2005

secret milkshake said ".. Please note how many people in USSR went into physics and math – and how little came out of it. …".

I do take note of "how many people in USSR went into physics and math", which is the point that maybe "big" is important,
but
I disagreee that "little came out of it".
For just a few examples:
the proof of the Bieberbach conjecture was validated in the USSR;
the geometry of Lie groups (for example the works of Boris Rosenfeld);
translation from Chinese of the work of L. K. Hua on geometry of classical domains;
works on singularity theory;
works on von Neumann algebras;
invention of supersonic cavitation torpedoes;
early work on nonlinear dynamics (Joe Ford, a founding father of the nonlinear chaos group at Georgia Tech, used to remark that in the early days some of his most productive contacts were with people in Novosibirsk) ;
… etc … the list is far too long to continue here.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

18. **Anonymous**
December 18, 2005

Tony Smith says:

If Anonymous's "filtering" were to be done by "well-respected and intelligent people" who are determined to be so by their status in the physics community, then it seems to me that in the case of the present-day USA physics community, no candidates outside superstring theory (90%) and LQG (10%) would be chosen.

And the alternative is — what? Funding every person who claims to have an interesting new idea? Clearly not. Most such people are just no good.

Also, please note that most of the theoretical physics community does not do string theory or LQG. Even in high-energy physics, string theory is only about half of the community.

secret milkshake said:

Please note how many people in USSR went into physics and math – and how little came out of it.

In high-energy physics: Gribov, Polyakov, Shifman, Vainshtein, Zakharov…. Others as well. A large fraction of important work on gauge theories from the 70s and 80s came from the USSR. (Another large fraction came from 't Hooft.)

19. **Tony Smith**
December 19, 2005

About filtering people in theoretical physics, Anonymous said "… Funding every person who claims to have an interesting new idea? Clearly not. Most such people are just no good. …".

Aside from being unable to restrain myself from remarking that "most people are no damn good" was a line by Lex Luthor (Gene Hackman) in the original Christopher Reeve movie Superman,

I will note that I have heard that with respect to NSF grants the most innovative and productive grants were NOT for proposals that received "excellent" overall ratings, but they came from proposals that:

received "fair" overall ratings resulting from a mix of "poor" and "excellent" reviews.

In other words,
successful innovation usually comes from controversial proposals that are strongly opposed by many "well-respected and intelligent people".

Anonymous and I might be able to agree that, in a system composed of reviewers who were NOT substantially universally biased (for example, toward superstring theory as the "only game in town"), a proposal that received "poor" from ALL reviewers should be rejected. (Unfortunately, I believe that the present system in the USA is in fact so biased.)

I would advocate approval of proposals with a mix of "excellent" and "poor".

I am not sure from the posted comments (and I have no other basis for knowing the views of an anonymous commenter such as Anonymous) whether Anonymous would insist on all "excellent" ratings for approval.

I do agree (as should be evident from my comment) with Anonymous that the USSR system produced a lot of high-quality results.

Tony Smith
http://ww.valdostamuseum.org/hamsmith/

20. **garrett**
December 19, 2005

I can provide one data point regarding what it was like to be a young physicist in the US who wanted to work on fundamental problems:

In 1995, as a physics graduate student at UCSD, I found a soliton solution to the Maxwell-Dirac equations that no one — especially me — knew what to do with. I was working in nonlinear science at the time, but my real motivation has always been to understand and figure out the universe — it's the best puzzle around. I showed my work to the department chair, Roger Dashen (a great guy), and he liked what I'd done enough to take me on as an advisee. (And it didn't hurt that I was a straight A student) During this same time, string theory was growing in popularity and there was a strong push to move in this direction. I took a nascent string course, as well as read up on my own — but… I just couldn't buy it. There were too many wild assumptions, and in the end they bought you nothing. Using QED to calculate the results of experiments to twelve decimal places — THAT is good physics. But I was in the minority. And when Roger tragically died, I had nowhere to turn for a high energy physics advisor. I finished up my dissertation in nonlinear science under my previous advisor, and hit the dilemma. I wanted to work in GR and QFT — they have always interested me the most. But I had nobody to introduce me to opportunities in either field, and the main community was going for strings in a big way.

However, I had been lucky enough to have another wild option. My graduate fellowship had paid me on top of the money I earned as a TA, and I'd invested that in stocks while the market was booming. So I had a nice little nest egg built up — enough to last five years or so at my graduate student spending level. And, thanks to the net, I figured I could work anywhere, on the physics I wanted. So I wandered a bit, and settled in the most beautiful place I could find — Maui. I've been finding my own way ever since, working on what I want, and publishing only when I've thought I figured out something significantly cool. Well, after five years, and less than stellar stock market performance, the money ran out. So I've had to find money making projects to work on here and there while dedicating most of my time to working on physics — traveling down one theoretical path after another.

It's been a hard road to walk alone. I spend way too much time wading through arxiv articles that I can't know are bunk until I've invested time to figure that out myself. (I wish other people didn't have publishing pressure — it results in garbage.) And, in my own work, I've had to be very conservative and careful, since the only person checking my ideas and giving me feedback is me. But I've had the opportunity to work on what I want, and enjoyed it immensely. Now, much to my own surprise, I found that some of what I've been working on meshes amazingly well with recent work in quantum gravity. It was the type of thing that seemed too much of a coincidence to ignore, so I wrote it up. This, to me, may present a good opportunity. Because I miss hanging out with other physicists — I used to be a very social guy back in grad school, and I miss the active interchange of ideas. And now, as string theory appears to be collapsing under the weight of it's own broken promises, this seems a good time for me to come back and interact with other people working on alternative approaches to fundamental questions in physics.

So… young American physicists who want to work on fundamental questions in physics will do it themselves, because they want to, any way they can.

21. **D R Lunsford**
December 19, 2005

Gratuitous aside for Doug:

I am neither distressed by my absence from arXiv, nor would I want it any other way.

-drl

22. **Christine**
December 19, 2005

> Please note how many people in USSR went into physics and math – and how little came out of it.

See, e.g.,

http://www.hssonline.org/teach_res/essays/graham/graham.html

Best wishes

Christine

23. **Pat Szuta**
December 19, 2005

I'm curious to learn more about his attitude towards the American system of producing PhD's. Are we really only spitting out 'technicians'?

24. **anon**
December 19, 2005

'But if you take people who are beginners in physics programs and brainwash them from the very start it is really not fair. Young physicists should be completely free, but it is very hard with the actual system.' – Alain Connes.

Pauli wrote to Fierz, 12 August 1948: 'I think the important and extremely difficult task of our time is to try to build up a fresh idea of reality.'

'Bigoted technicians', would be more precise!

25. **Tom Weidig**
December 19, 2005

I think you are quoting him out of context. He also said a lot of nice things about string theory in the interview!

26. **Dissident**
December 19, 2005

Tom, after noting that there is not one shred of evidence that either supersymmetry or strings have anything to do with physics, he kept saying again and again that string theorists are great for mathematics. Draw your own conclusion.

27. **Who**
December 19, 2005

> I miss hanging out with other physicists — I used to be a very social guy back in grad school, and I miss the active interchange of ideas. And now, as string theory appears to be collapsing under the weight of it's own broken promises, this seems a good time for me to come back and interact with other people working on alternative approaches to fundamental questions in physics.

Garrett, I have to cheer for your determined independence and also deplore the narrowness of QG option open to US grad students. I hope that the string monopoly is indeed breaking and that research will open up some, so that you will be able to re-connect somewhere in an active interchange of ideas.

> Tom, after noting that there is not one shred of evidence that either supersymmetry or strings have anything to do with physics, he kept saying again and again that string theorists are great for mathematics. Draw your own conclusion.

LOL

To Peter, very glad to see the "Latest Comments" feature in the righthand margin. Big help. Much quicker now to see in a glance
if there has been some new discussion on an interesting thread.
Thanks.

28. **Attila Smith**
December 19, 2005

Esteemed Adrian Heathcote:

“I’ve been a great admirer of Connes for some years. I think he has great intellectual honesty and courage”

Yes, for a Frenchman to be scornful in Tehran about the U.S. University System(?) is the height of bravery indeed.

29. **Dissident**
December 19, 2005

Maybe on that particular occasion he was merely being honest, Attila…

30. **sbar**
December 20, 2005

The soviet system that produced these great scientific institutes also sent millions to their death in the Gulag .Let’s not forget them . Let’s not forget the Mendelian geneticists who perished in the Kolyma because they disagreed with Lysenko. Let’s not forget that 30 years after Stalin’s death Sakharov was still under surveillance and in exile in Gorky.
There is nothing to praise in such a system..

31. **Dissident**
December 21, 2005

sbar, while I certtainly sympathize with Connes on much he says, I tend to agree with you about the old Soviet way of doing science. When you look at the list of weel-known Soviet physicists, the vast majority seem to have come of age during the first decades of the revolution, pre-WWII, before all of Russian society was reshaped by it. What came out of those great Soviet institutes after 1960 or so?

32. **Zelah**
December 21, 2005

To Dissident,

You are correct that in terms of economics, the Russian institutes failed miserably, but post 1960’s Science?

You should do some homework son.

To help you along here are 3 in mathematics:
Perelman, Efim I Zelmanov, Vladimir Gershonovich Drinfeld.

The reason for the sucess of Soviet education is the ironies of ironies. Unlike Anglo Saxon failed one size fits all education systems, the Soviets let COMPETITION RIP!!!

That is the main point, students had to compete on a regular basis starting from 5 years old if they were to advance!

An amateur mathematician

33. **Dissident**
December 21, 2005

Ahem, I did write “Soviet physicists”, not “Soviet mathematicians”.

34. **Not a Nobel Laureate**
December 21, 2005

“Yes, for a Frenchman to be scornful in Tehran about the U.S. University System(?) is the height of bravery indeed.”

— Attila Smith

“There is no national science just as there is no national multiplication table; what is national is no longer science. ”

— Anton Chekhov

35. **Dissident**
December 22, 2005

Speaking of the devil: http://news.scotsman.com/international.cfm?id=2434192005

36. **Juan R.**
December 22, 2005

Connes said

> […] the most successful systems so far were these big institutes in the Soviet Union, like the Landau institute, the Steklov institute, etc. Money did not play any role there, the job was just to talk about science. […]

Today, my Russian colleagues would not say the same. See for instance the current policy of the High Certifying Commission (Russia) on foreign electronic journals here.

In fact, I know that Alexander Shagaev suffered hard personal pressure from some rival Russian scientists some months ago. Georgiy Vasilievich Lisichkin editor-in-chief of the Journal of Russian Chemical Society said us in public that the reviewer of the journal would be anonymous because was known tragic cases of murder of reviewers by scientific mafias at Russia!

Moreover, i find interesting the attitude of Connes regarding alternative approaches to NC geometry. In fact, Connes has been traditionally very hard with non-standard analysis and popularized his definition of infinitesimal via limits of non-compact operators (which i believe is totally inconsistent).

Is Connes critizing string theory attitude but following a 'similar' one regarding his own geometric approach?

It is a query, not an affirmation…

Juan R.

Center for CANONICAL |SCIENCE)

Raoul Bott 1923-2005

December 22, 2005
Categories: Favorite Old Posts, Obituaries

I was deeply saddened to hear this morning that Raoul Bott died a couple days ago, on December 20th, in San Diego, of cancer. Bott was one of the greatest mathematicians of the twentieth century; for some details about his life see the commemorative web-site set up by the Harvard math department. The article by Loring Tu gives an excellent outline of Bott's mathematical career and accomplishments.

I first encountered Bott when I attended his graduate course on differential geometry at Harvard. The course was over my head since I was an undergraduate, and the fact that it met at 9am (or was it even 8:30?) didn't help at all since I was sometimes not awake that early. But the course was extremely inspirational, giving a beautiful and revelatory view of geometry in terms of Lie groups, Lie algebras, connections and curvature. I hope someday someone who has a complete set of notes from that course will write them up. I never took Bott's course on algebraic topology, but learned much of the subject from the book Differential Forms in Algebraic Topology, which was a write up of the course notes done with Loring Tu. Bott's point of view on algebraic topology is a perfect one for physicists interested in the subject, starting very concretely with manifolds, deRham cohomology and Poincare duality, and only later getting to more abstract material. Later on in the 1980s when I spent another year at Harvard, one of the most rewarding experiences of that year was sitting in on an advanced course on the index theorem (especially the heat equation proof) that Bott was teaching.

During my undergraduate years I lived at Dunster House, and my last year there was livened up by Bott's presence, when he took up the position of "Master" of Dunster House, living there with his wife and often interacting with the undergraduates. Bott was an extremely warm and friendly person, a truly wonderful human being and a pleasure to be around. He gave the outward appearance sometimes of not being all that swift, demanding that one explain things to him slowly and in as simple terms as possible. His greatest mathematical achievements came not from any ability to quickly master difficult formalisms, but from a talent for seeing deeply into a problem, getting at its very core and finding new ways of understanding what was going on in the simplest terms possible. Many of his results have given new insights into mathematics at the deepest levels that we currently are able to understand.

Some of Bott's earliest work involved dramatic new applications of Morse theory, especially his discovery and proof of Bott periodicity, an unexpected fundamental new fact about topology that lies at the foundation of topological K-theory. Bott was intimately involved with Clifford algebras and spinors from early on, and his extremely important paper with Atiyah and Shapiro shows how crucial these are for understanding K-theory. While the proof of the general index theorem is due to Atiyah and Singer, Bott periodicity and the central role of the Dirac operator made clear by the Atiyah-Bott-Shapiro work are critical parts of the story. Bott also worked out with Atiyah an amazingly powerful fixed point theorem that also fits into the index theory story.

Despite being nominally a topologist, Bott had a big influence on representation theory, with his Borel-Weil-Bott theorem showing how to extend the Borel-Weil theorem to understand the way in which irreducible representations can occur not just as holomorphic sections of line bundles, but also in higher cohomology. He worked out the Dirac operator version of this result, an early check of the index theorem, and he was among the first to promote the idea that the notion of geometric quantization is best understood in terms of the index theorem and integration in equivariant K-theory.

His work with Atiyah on the moduli space of flat connections on a Riemann surface opened up a whole new field of mathematics, one whose implications are still not fully understood, especially the connections with quantum field theory. Over the years Bott took a great interest in physics and in communicating with physicists. He often gave lectures at physics conferences, and it was at one of these that Witten first learned about Morse theory, leading directly to his extremely influential work on supersymmetry and Morse theory. The fact that advanced age and ill-health reduced Bott's mathematical activity in recent years has been a huge blow to the whole subject of the interaction of mathematics and physics.

Most of Bott's papers have appeared in a four-volume set of his collected works, together with some commentary on them by him and by other mathematicians. Reading through these volumes has been a significant part of my mathematical education and I heartily recommend them to everyone interested in mathematics and physics. Bott was an exceptionally lucid thinker and thus a very clear expositor. His death marks a great loss on many levels for both mathematics and physics.

Update: Other blog entries about Bott can be found here, here, and here.

Comments

1. **Jeremy Henty**
   December 22, 2005

   Oh, this is sad. 20-plus years ago when I was trying to be a theoretical physicist I attended some of his seminars. Your description of him fits perfectly with my memories of a large, genial, barrel-chested man, not obviously fast in movement or thought but clearly having truly great insight. Amusingly Atiyah attempted to disrupt things by playing the irritating schoolkid ("Please sir! Please sir!"). They could have been a nice comedy double act, though it wouldn't have been worth the crushing loss to mathematics if they had changed careers.

2. **john baez**
   December 22, 2005

   I took a course on differential geometry with Bott when I was a grad student at MIT. It was great! He was explaining, and trying to simplify, Witten's work on supersymmetry and Morse theory.

   He always had a benign twinkle in his eye. I remember him starting one sentence with the phrase "Before I was such a bigshot…" – and the whole class cracking up in laughter.

   I also remember him saying "So, if thinking doesn't let us solve the problem, what should we do? *Superthink!*" This was sometime around 1985, when mathematicians were just getting into supersymmetry, so it suited the spirit of the times.

   Like Dirac, Bott started out as an electrical engineer. He said that he worked his way to algebraic topology through Kirchoff's laws, which are a nice introduction to the ideas behind deRham cohomology and Hodge theory. My friend Robert Kotiuga, who is an electrical engineer at Boston University, talked to Bott about this and got really fired up about it – he's written a lot about electromagnetism and algebraic topology. Just another of the many people influenced by Bott….

3. **garrett**
   December 22, 2005

   Thanks for posting this Peter. It's good to pass along bits of personal history. I went ahead and ordered his book (sounds like it might help improve my poor knowledge of algebraic topology) — you should consider becoming an Amazon Associate so you can get a small percentage from books you link to. Sadly, Bott won't get his share either, but it sounds like he lead a good life.

4. **Deane**
   December 22, 2005

   Peter,

   Beautifully written and well said.

5. **D R Lunsford**
   December 23, 2005

   Peter, this was inspiring.

   It's a great loss.

   -drl

6. **Lee Smolin**
   December 23, 2005

   This is very sad news. If I can add a reminiscence, Raoul Bott showed a great deal of generosity to me and other physics students who found a warm environment where he would give freely of his time to talk about physics and math. After he expressed interest in the singularity theorems in GR, another student, he and I met regularly to work our way through the key chapters of Hawking and Ellis's book. He required that every formula be translated into index free notation and insisted we gain an intuitive understanding of each term in each equation before moving on. I don't know if he got anything from it but it was an education to me.

   I learned from his example that one could do good science in an atmosphere that was as intellectually probing as it was warm and light hearted. At the end I felt sufficiently grateful to him that I arranged to receive my Ph.D. from him in the graduation ceremony at Dunster House, where he was master.

7. **MathPhys**
   December 23, 2005

   I met him only one time, when I attended one of his lectures. He was indeed a very warm man, very communicative. He talked about Morse theory.

8. **Andrew Sutter**
   December 27, 2005

   Very sad news about a warm and generous man. I confess to a pang of jealousy that I was in Dunster House a couple of years too soon to experience him as house master. Instead, I met him when I was a clueless freshman taking linear algebra. I was too young and ignorant to fully appreciate my luck at being plopped into the one section taught that year by tenured faculty: Shlomo Sternberg in the first semester, and Raoul Bott in the second.

   It was springtime, and my attention wandered. I tried to fake it through much of the final exam. My blue book came back with one comment written exuberantly across many of my answers: "SALAD!" Decades later, this is still the phrase I hear in my head — with its author's accent and enthusiasm — whenever I'm editing my most awkward prose.

Atiyah and Witten in Nature

December 22, 2005
Categories: Uncategorized

This week's issue of Nature has short articles by Atiyah and Witten, both addressing the issue of the current state of string theory.

Atiyah's piece is an interesting review of Lawrence Krauss's new book Hiding in the Mirror entitled Pulling the strings, and it concentrates on what Krauss has to say about the relation of mathematics and physics. Krauss ends his book quoting the mathematician Hermann Weyl as choosing beauty over truth, remarking that physicists don't have this luxury. Atiyah points out the story of Weyl's work on gauge theory, which Weyl published over Einstein's strong argument that it was physically wrong. The idea was just so beautiful that Weyl felt there had to be something to it, an opinion that turned out to be amply justified as the concept of a gauge theory has turned out to be among the most fundamental ideas in theoretical physics.

Witten's piece is entitled Unravelling string theory and it tells the story of how he got interested in string theory and offers a defense of its continued study despite the lack of progress during the past 21 years in using it to come up with a unified theory. His defense consists of three points:

1. It appears to be a consistent generalization of QFT, and is worth study on that grounds alone.

2. It incorporates general relativity and provides a "rough draft" of particle physics.

3. Research on string theory has led to all sorts of spin-offs: insights into confinement, black-holes, mathematics.

Those are certainly the strongest arguments for working on string theory, but I find it disappointing that Witten chooses to ignore much of what has been happening in string theory over the last few years. He addresses only by indirect allusion the whole issue of the landscape and the strong possibility that the string theory framework for unification is inherently incapable of predicting anything. Witten would do particle theory a huge favor by at least acknowledging that if the string theory landscape really exists, it is not, as many seem to think, a new paradigm for how to pursue theoretical physics, but instead the end of hopes for this idea about how to achieve unification.

Comments

1. **Dumb Biologist**
   December 22, 2005

   I have a question for you, Dr. Woit, if you don't mind:

   What's so great about beauty? It seems one clear distinction between your assessment of strings, and that of, say, Witten, is the opinion regarding its purported "beauty". Obviously Witten and those who share his belief find their perception of the beauty of strings so compelling they've devoted their professional lives to it. You don't their aesthetic judgement on the matter.

   Either way, I have to wonder if the beauty criterion has produced as many red herrings as it has fruitful research programs, given that "beauty" is, in the end, in the eye of the beholder (with many philosophers arguing as only they can about its significance).

   I don't remember the precise wording, but Feynman asserted essentially that "beauty" pales next to experimental verification. I tend to agree, only because I distrust the aesthetic judgements of others, as well as my own, when it comes to describing what is vs. what I'd like reality to be. I wouldn't want to deny anyone the pleasure of deriving human pleasure from the perceived elegance or gorgeousness of theoretical models any more than I'd want to tell them their joy in experience art or music is worthless. However, it seems to me, in science, evidence trumps everything and anything else, precisely because good experimental data is not generated as a matter of oppinion.

   If the theory does it's job the way a scientific theory should, whether it strikes some as beautiful and others hideous is completely irrelevant. Isn't it?

2. **Juan R.**
   December 22, 2005

   Some comments on Witten's *defense of its continued study despite the lack of progress during the past 21 years in using it to come up with a unified theory*:

   1. Witten says "It appears". Yes, i agree, only that…

   Is Witten ignoring that there are others more interesting, consistent and realistic ways for generalizing standard QFT: via direct curved spacetime, nonlinear, NC-QFT, TFD (generalized vectors on doubled space), time-symmetric AAAD (Hoyle/Narlikar), via Gelfand triplets (Brussels School), etc.

   2. String theory does not incorporates general relativity. This is one of more dishonest claims of string theorists i know. In fact, the supposed background-independent version is not a string theory and is called M-theory. M-theory is not formulated and nobody know what is, if it really exists. M is for mistery…

   3. The comment that string theory provides a "rough draft" of particle physics can be easily replied via Woit's appeal to Pauli

   > one is reminded of another quote from Pauli. Annoyed by Heisenberg's claims that modulo some details he had a wonderful uniffied theory (he didn't), Pauli sent his friends a postcard containing a blank rectangle and the text "This is to
   > show the world I can paint like Titian. Only technical details are missing."

   4. Research on string theory has led to all sorts of spin-offs: insights into confinement, black-holes, mathematics.

   Yes, but like lot of other disciplines with minor publicity in mass media. Or has only string theory provided advances? For example, i estimate the impact of string theory on math less than 1%. The real impact of string theory on disciplines as chemistry, biology, or geology for instance, is 0%.

   Bad numbers for the so-named TOE; really bad numbers for the theory that, in words of Brian Greene, was revolutioning our views…

   Juan R.

   Center for CANONICAL |SCIENCE)

3. **woit**
   December 22, 2005

Dumb Biologist,

One problem with beauty is that it is not precisely defined, so one has to be very careful about what exactly is being claimed to be beautiful, and what isn't. In Witten's article, note that he isn't actually claiming that string theory is beautiful. I've written extensively elsewhere about string theory and what about it is or isn't beautiful, so won't here.

By "beauty", theorists often mean the property of getting a huge amount of explanatory power out of a very small number of assumptions, or if you like, a simple equation or equations. Most people agree that the Dirac equation is "beautiful" for this reason. This is the sort of thing many theorists are hoping to find again. They haven't found this in string theory, since no one knows what the equations are. Experimental evidence is of course ultimately the arbiter of what is true and what isn't, and it can also often provide hints to theorists about what directions to be looking in. Beauty is another source of hints, especially important when experimental guidance is lacking. Atiyah's example of gauge theory is an excellent one of an idea that was so beautiful there had to be something right about it. But in its initial form, the idea wasn't quite right, input from another direction (QM) was needed to get it right.

4. **Dumb Biologist**
   December 22, 2005

   OK.

   Being myself a mathematical unsophisticate (with none of the virtue of earnest purity that word might imply), it's clearly a property of mathematically deep theoretical frameworks that is lost on me, but for the qualitative features you describe. Semantics may be as much the issue as any other, I suppose. "Beauty" here seems more-or-less interchangeable with notions like "elegance", "economy"… To the extent that this more specialized use of "beauty" is understood with broad consensus, then the problem could very well be, as you say, not one of naive reliance upon beauty as a "hint", but rather its misidentification or abandonment.

   Again, the beauty, as defined, of the Dirac equation is over my head, and perhaps I ought to do my best to understand the basics more (buy Penrose's book?) before I get too suspicious. Because I know I'm no genius, I've never trusted my own impressions enough to have ever said to myself, in my work, "that's just so darn beautiful it's GOT to be…" I suppose that allows me to always be pleasantly surprised when I'm right, though perhaps it also means I lack "common sense". I guess I've always been far more impressed with, say, the accuracy to so-many decimal places of QED and GR than any other feature those theories posess…probably because I'm too dumb to understand those other features well enough. But, because even the brightest among us can be misled, it seems, by personal biases, it's always seemed best to toss aesthetics if it looks too much like intellectual baggage.

   If one cannot observe, even in principle, my (perhaps ignorant) suspicion turns to alarm, and what little trust I have goes out the window. I find it difficult to resist the urge to dismiss considerations of "every possible universe" that lies beyond our horizon out of hand, no matter how beautiful the theory that describes them might be. If the theory tells us nothing specific about our own universe (or vacuum, or whatever), my dismissive reflex turns to a sense of betrayal of the scientific method.

5. **Dumb Biologist**
   December 22, 2005

   I must say, after re-reading Witten's essay, in light of the notion of beauty meaning "getting a huge amount of explanatory power out of a very small number of assumptions, or if you like, a simple equation or equations", he certainly seems to be saying ST is beautiful, in not-so-many words. Witten says String Theory "leads in a remarkably simple way to a reasonable rough draft of particle physics unified with gravity", and that it "has proved to be remarkably rich, more so than even the enthusiasts tend to realize", citing the insights it provides to quark confinement, BH entropy, and so forth. He outright asserts that its equations are profitable (so to speak), in this physically "beatiful" fashion, since that they give back more than what is put in. This seems, allegedly, to be congruent with what is said of the Dirac equation (which looks simple enough, I suppose, though it's been a long time since I've had to contend with diffy-q's and matrices), as it explains electron spin, magnetic moment, charge quantization, (where earlier work had to incorporate those properties "by hand"), predicts the existence of positrons, as well as agrees with all the experimental data of the day.

   I can't help but interpret Witten's words as an endorsement of String Theory's "beauty", and that this beauty is the very reason why it is to be taken seriously. Perhaps I'm just reading it all wrong.

   Again, its distant relation to empirical inquiry seems not to bother.

6. **woit**
   December 22, 2005

   I think Witten is being extremely careful in what he says, trying to find the absolute best argument he can make for string theory, without going too far and saying something that can't be supported. I don't think he is saying string theory is too beautiful to not be true, largely because he is saying we don't yet know what string theory is. He is marshaling evidence for the idea that there is something beautiful going on, but we don't yet know what it is. Until he actually knows what string theory is, he can't claim that it is beautiful, and I think he is being careful not to do this in his article.

   You're quite right to be suspicious of arguments based on universes we can't observe, but this has nothing to do with questions about beauty. Susskind forcefully makes the point that the string theory landscape picture of the multiverse is a quite ugly one. He doesn't say you should believe because it is beautiful, quite the opposite.

7. **Chris W.**
   December 22, 2005

   D. B.,

   I think the use of the word "beauty" in science is confusing precisely because the meaning summarized by Peter—"getting a huge amount of explanatory power out of a very small number of assumptions"—does not play much of a role in discussions of aesthetics in other fields (with the possible exception of music, given an appropriate recasting of "explanatory"). When non-scientists see the word used they naturally tend to think of the uses of it that are familiar to them, in connection with the visual arts, architecture, music, etc. Those uses tend to emphasize subjective responses of appreciative non-specialists to works of art.

   The best analogy in biology to the deep principles of physics are the deep structural principles of biology, and of course the principles of evolutionary biology. (Needless to say many of these principles have important connections to physics.) These principles place the staggering body of detailed empirical knowledge in biology in an intellectual framework without which much of that empirical knowledge would be far less significant, ie, far less worth knowing.

   Of course one might be inclined to say the same of various mythologies. The problem with myths is that, considered objectively and critically, they don't stand up to close scrutiny as explanations. Seeking after beauty in physics only becomes pernicious if it is used to argue for the irrelevance of testability and empirical verification in science. The existence of this potential should not be turned around and used to argue that theoretical beauty (in the sense given above) is itself irrelevant. We not only want but *need* more than an enormous catalog of verified but largely disconnected facts.

   In short, great theories (and for that matter, great engineering) elicit a kind of objective relief that a cacophony of disconnected and apparently contradictory facts (or problems) can be reconciled and understood in a unified, lucid, and robust way. To a scientist *that* is beautiful. Mathematical formulation can be be tremendously helpful, and sometime essential, in achieving this relief, although without key informal insights into the relevant problems formalization may simply augment the confusion.

That said, I think we are still in the process of figuring out what beauty in scientific theories *ought* to mean. The word is merely a label for a quality we are struggling to identify and articulate with ever increasing depth and clarity. That quality is best conveyed by lucid explanations of well-chosen examples, and the fertile insights they have provided.

8. **Dumb Biologist**
December 22, 2005

Oy. I must simply give up and say beauty gives me a migraine, especially when I try to contemplate what it ought to mean. I guess I'll never be comfortable with it, and must leave substantive discussion of the matter to those wiser and smarter than myself.

I guess it's a small comfort that I've got comparisons with objective reality to compensate for my philistine lack of aesthetic sense or interest. And no, I don't mean that sarcastically.

9. **pax**
December 22, 2005

Justifiying millions of (brilliant) man hours based on beauty alone is definitely questionable. After all, the very notion of beauty is most likely a result of evolution. The human mind has been selected to differentiate between ugly and beautiful as a survival mechanism. Harsh, dry desert terrain – ugly. Waterfall and greenery – beautiful. To now say that the key to understanding the way the world is put together lies in concepts like beauty and its younger sibling symmetry is, at the very least, a circular argument.

10. **Chris W.**
December 23, 2005

D. B.,

There is another issue here, I think. "Comparisons with objective reality" in physics have probably been more problematic* in physics than in biology for much of the last hundred years, if not before. Theories in physics set the very terms of the discussion to a much greater extent; they tell us what we can expect to observe, and how to go about observing it, which may require tremendous effort and technical skill. Many (most?) physicists' conception of beauty in a theory is also associated with its consistency, clarity, and comprehensiveness when used in this role.

Trying to follow the Baconian ideal and observe the world without preconceptions or prejudices—whatever you see in the microscope or the telescope,* as it were—is hopeless. You always have to assume something and have some problem in mind; the question is what these presuppositions are, how one would know if a given assumption or problem formulation was leading one astray, and what presuppositions are absolutely central to the enterprise. It obviously helps to have several alternatives in play, which may cut up the alleged objective reality in different ways. Some of these alternatives may be more or less "ugly"—unappealing on logical, philosophical, or "aesthetic" as well as empirical grounds—while still contributing something valuable to critical discussion of a problem. (Consider the case of MOND.)

(I should point out that philosophers of science have discussed this issue at great length, starting in the first third of the 20th century.)

(* Actually, I wonder if this is really the case, that is, if biological observation is really so unguided by theoretical presuppositions.)

11. **Anonymous**
December 23, 2005

pax: you have to consider that people are not just making that argument from out of the blue. look at it more as a historical obervatoin: physical theories have an uncanny tendency to be beautiful/symmetrical.

12. **anon**
December 23, 2005

Dumb biologist:

The Feynman quote on the benefits of beauty versus those of experiment is:

'You can recognise truth by its beauty and simplicity. … The inexperienced, and crackpots, and people like that, make guesses that are simple, but you can immediately see that they are wrong, so that does not count. … We have to find a new view of the world that has to agree with everything that is known, but disagree in its predictions somewhere, otherwise it is not interesting. And in that disagreement it must agree with nature.' (Character of Physical Law, 1965, p171.)

13. **sunderpeeche**
December 23, 2005

Dumb biologist:
Chris W gave you an excellent reply. ("The best analogy in biology to the deep principles of physics are the deep structural principles of biology, …") — this is right on the money. Think of the DNA double helix (coupled with what I believe is called the "Standard Dogma" in biology), how much it has explained based on a few simple ideas. That is beauty. The double helix is an icon of popular culture now — it is on the cover of Dec issue Scientific American and many other places.

14. **Juan R.**
December 23, 2005

i find really interesting the discussion about the supposed beauty of the Dirac equation addressed above.

Of course, beauty is a subjetive concept and whereas some theoreticians as Kaku claim that the Dirac equation is specially beatiful, others theoreticians such as the own Dirac (i already cited his words several times in this blog) or myself think that the equation is NOT beatiful.

Since string theorists like a wrong concept of 'beauty' and are rather unrigorous in her/his academic discussions, it is not so strange the *three decades stringy failure for finding a consistent theory of nature*.

Still more interesting is that nobody here has still expressed that she/he really understand by "Dirac equation":

– The original Dirac wavefunction, which is wrong and was ABANDONATED in modern quantum field theory by a Schrödinger equation for funtionals.

– The modern Dirac operator identity appearing in QED, which is not fundamental since does not apply to two-electron systems (in fact, still nobody know what is the full two-electron equation). I would note that also Dirac rejected the modern QED view in his last publications.

– Dirac-like equation obtained on string-brane theory when [tex]D_{10}[/tex] is splinted into “compact” and “non-compact” dimensions [tex]D_{6}\psi_{n} = m_{n} \psi_{n}[/tex].

I see also some incongruencies about the supposed ‘predictions’ of the original Dirac equation. For example, it is simply untrue that Dirac equation predicted the correct magnetic moment for electron and the prediction of antiparticles would be discussed because the concept of antiparticle on the original Dirac-hole theory is NOT the antiparticle of the modern QED.

For a simple review of the original Dirac equation and its obvious flaws see pages 13-14 of Weinberg manual.

Juan R.

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15. **Dumb Biologist**
December 23, 2005

Well, there’s a lot of public discussion these days about the beauty of Darwin’s insight, as the very simple program of random variation coupled with selective pressure is all that is needed to explain the complexity and diversity of life. (Of course modern evolutionary theory incorporates other mechanisms like genetic drift, takes into account that mutation isn’t always completely random, etc., but those necessary additions to the theoretical framework in no way diminish the importance of Darwin’s proposed mechanism of natural selection).

Logically, evolution is a slam dunk, there’s no denying. I can’t think of a single good argument against the notion that, given a population of replicating, mutating entities in a dynamic environment, evolution simply MUST happen. Its inevitability does seem to make experimentation to empirically demonstrate the point essentially superfluous. What else could possibly happen? I certainly can appreciate the perception of beauty in a concept so simple, yet so powerful and, probably, _necessary_.

Then I look at the ID debate, and any confidence I might have had in the reliability of human perception crumbles. Platonic solids and epicycles have beguiled the minds of many a brilliant thinker in the past. Who knows what today’s equivalent might turn out to be. Of course, without some initial bias, sensible scientific investigation is impossible; and indeed most of those biases are entirely justified. But by the example humanity has provided time and again, it seems ultimate reliance upon anything but the well-tested hypothesis will yield rapidly diminishing marginal returns, and eventually complete detachment from reality.

16. **pax**
December 23, 2005

Dear Anonymous: (Out of the blue guy…)

Has it occured to you that you regard the physical theory as beautiful
simply because your thoughts, aesthetic sensibility and yourself are a to some extent a product of that construct ?

It’s like saying “This candy is sweet because it tastes of sugar…”

17. **Shantanu**
December 25, 2005

Peter, one interesting thing in Witten’s article is that he makes no mention of LQG or CDT (when he talks of critics of string theory) but does mention twistor theory, non-commutative geometry .

18. **woit**
December 25, 2005

Shantanu,

Witten says something like “when critics of string theory have good ideas, they tend to be absorbed as part of string theory”. His failure to mention LQG and CDT could be interpreted as meaning he doesn’t think they are very good ideas. I found that sentence of his kind of strange: he seems to be claiming for instance that black hole entropy and twistor theory have been absorbed as part of string theory, which is a somewhat peculiar way to phrase the situation of the relation between these subjects.

19. **Samuel Prime**
December 26, 2005

The string theory bashing here is amazing. Why is this? Are such feelings directed towards researchers of string theory (and what they say) or towards the theory itself? (Or both?) They sound like a hint of jealousy.

Okay, so there is competition going on. What’s the deal about that? Physics has always had competition in its endeavor to find the truth (or the most successful theory to date). From what i have read by Witten he has been more careful than how some seem to portray him. He frequently speaks in tentative terms like “if string theory is correct”. And why would he bother to mention the problems with string theory if he’s that biased? He does mention them, but those who want to bad mouth him and string theory choose not to be fair.

Instead of engaging in negative interpretations of Witten’s not mentioning LQG, isn’t it best to settle the matter by asking Witten himself via a friendly e-mail (how he feels about LQG)? On a couple of occasions I have asked him some questions and Ed politely answered me to clear a speculation of mine. The reason Witten mentions noncommutative geometry is because he used it in some of his papers. And perhaps he doesn’t refer to LQG is because he’s either not convinced of it or does not see the need to use it. Do we do the same for LQG researchers when they don’t use string theory?

20. **D R Lunsford**
December 26, 2005

Homo biologicus non sapiens,

Beauty involves economy, in all cases. The Dirac equation
[tex]
\begin{eqnarray*}
(\gamma^{\mu}\partial_{\mu} + im)\phi & = & 0\end{eqnarray*}

[/tex]

is very economical. Another way of saying it is

P psi = M psi

the mass is the eigenvalue of the momentum. This equation is really the generalization of E = m to moving electrons. It explains not only the otherwise incomprehensible phenomenon of spin, but also gives a direct statement about the electron's magnetic moment. To top it off, this little equation implies the existence of a new kind of matter, that was hitherto unsuspected. Not bad for 8 symbols.

21. **Chris W.**
December 26, 2005

Follow-up on "Theories in physics set the very terms of the discussion to a much greater extent; they tell us what we can expect to observe, and how to go about observing it" (in my comment on 12/23): See this recently posted preprint:

Observables in effective gravity (hep-th/0512200)
Authors: Steven B. Giddings, Donald Marolf, James B. Hartle

This paper indicates the extent to which the above statement has been realized in quantum field theory and general relativity, and how its significance is further magnified in quantum gravity. The theme is pervasive in the vast literature on these subjects.

22. **woit**
December 27, 2005

Samuel Prime,

"why would he bother to mention the problems with string theory if he's that biased?"

I don't think Witten actually does mention any of the serious problems with string theory, other than that we're not quite sure what the theory actually is. By now there is strong evidence of problems with string theory as a framework for unification, and Witten doesn't address these, instead just making the positive case for continued research into the theory. He's within his rights to do so in this kind of short essay, but a serious examination of the problems with string theory is not what he is doing.

You'll not find coming from me personal attacks on leading researchers in string theory. I think there are serious scientific problems with the theory, which I've repeatedly explained in detail, as well as problems with how research into the subject has been conducted. Discussing these issues is not a personal attack on anyone. As for jealousy, I have immense amounts of respect for both Atiyah and Witten, for their talents, their hard work and their incredible accomplishments. Sure I wish I could have accomplished a fraction of what they have, but I'm well aware I'm just not as smart and hard-working as they are.

About string theorists in general, I don't envy them at all their current situation, and, on the contrary am quite happy with my professional life, as well as with my decision many years ago to not go into research in string theory.

23. **Samuel Prime**
December 27, 2005

Peter, but Witten does mention the serious problems with string theory. For example in his expository article "Reflections on the fate of spacetime" (which is available on his homepage) he states the good news with string theory, and then later asks "what is the bad news?" And he proceeds to discuss them—the first of which he calls "glaringly unsatisfactory" (see page 26, Ibid). He says clearly the concepts are unclear and the principles underlying string theory unknown or not yet understood. Later on in the article (page 28) he says "We are far from cing to grips fully with this paradigm, and one can scarcely now imagine how it will all turn out." These are a lot more than saying we are not sure what the theory is, but points to problems with it—and certainly leaves the reader (or me, at least) with the impression that it could all turn out wrong (not that it is "not even wrong"—which is actually unclear at this point). (Unless you say that you have a disproof of string theory!)

By the way, and respectfully, I do not think anyone is in a position to say what Witten is not doing. He may be doing privately what we don't know from his work publically. No one knew Wiles was working on Fermat's Last Theorem when he was; many mathematicians like to work on some things and keep them to themselves.

It's okay with me that there are serious problems with string theory. They could reflect that ultimately it is the wrong, but ingenious, approach. Or that there is still a lot of work ahead; work which might point to a different direction to something closer to the truth.

Anyway, in the end, string theory could be wrong in describing nature (or could be right), but whatever the outcome it has left its mark on the history of Physics it seems to me (having been around a couple decades or more). Let's remember that quantum theory didn't come to us on a silver platter.

24. **biophysicist**
January 5, 2006

Dumb biologist,

I think if you stop worrying about popular ideas of what beauty is and focus on the beauty we as scientists find in the remarkable simplicity and necessity of Darwin's law of natural selection, then you'll appreciate the physicist's view of beauty as well. It seems to me that the only difference (right now at least, this could of course change) between beauty in purely biological theory and physical theory is the presence of symmetry principles in the latter. But the psychological satisfaction in the Dirac equation and Darwin's natural selection are what I at least mean by beauty (both are beautiful in unique ways though, since the former is conjectured and the latter, as DB points out, just seems inevitable – but remember it still took a human being to point out what we take for granted as inevitable, that's why it is still beautiful!).

Trackback Censorship

December 23, 2005
Categories: Uncategorized

Over the last month or so I've sent repeated requests to the arXiv to have trackbacks posted for several of my weblog postings. The first of these requests, back in early November, was answered positively by "mjf", and the trackback (to a paper by Nekrasov) soon appeared. Since then I have sent repeated requests to the arXiv that they list trackbacks to my postings about papers by Weinberg, Baez, Freed-Hopkins-Teleman, and Tegmark et. al.. I've received no response whatsoever to these requests, including to requests that they inform me of what the arXiv policy on trackbacks is and why my postings don't seem to qualify.

Of the four postings involved, two of them (about the papers of Baez and Tegmark et. al.) led to discussions here involving some of the authors of the papers themselves, and I think seeing this discussion could be valuable to people interested in those two papers. I can think of no legitimate reason why trackbacks to those postings should not be allowed. The posting about Freed-Hopkins-Teleman was intended to point physicists to some very interesting new work in mathematics; I also believe some people might find that valuable. Finally, the posting about Weinberg's article was perhaps more controversial, but I believe it raises legitimate issues that the particle theory community needs to allow public discussion of, and censorship of this is inappropriate.

I've looked a few times at the list of recent trackbacks to see if I can figure out from that what the arXiv policy about this is. Today's list of trackbacks from the period December 20-23 has 6 listings, one from cs.unm.edu and five from golem.ph.utexas.edu. As far as I can tell, at least as far as particle theory is concerned, the arXiv trackback system is being run mainly for the benefit of the owner of golem.ph.utexas.edu, who sits on the arXiv advisory board, and may or may not have something to do with the censoring of trackbacks to my postings. Of course I have no way of actually knowing what is going on here or who is responsible. This situation seems to me to raise questions which the arXiv advisory board needs to address, and I am simultaneously contacting them about this.

Update: I still don't know exactly what is going on at the arXiv, but from what I've heard so far, it is clear that the problem is not a technical or administrative one, so I'm removing the question mark that used to be there in the title of this posting.

Time now to ignore this for a while and start celebrating the holiday with family and friends. Happy holidays to all!

Update: Still no word from the arXiv. Commenter Jose notes that trackbacks to comments at physcomments.org do generally appear on the arXiv. There are instructions at physcomments.org that say:

If and only if a blog annotation starts with an identification paragraph "A Comment by …", and its subject line starts with the preprint it is commenting about, then it is submited to the ArXiV for consideration. Note that currently the ArXiV reserves its right to reject the trackback ping.

Evidently Alejandro, who runs physcomments.org, has been successful in not only communicating with the arXiv, but getting them to post his trackbacks. He comments here that "It is only via Distler that it has been finally incorporated, and it is partial, experimental etc." From this I take it that the key to getting arXiv trackbacks posted is, as commenter Chris Oakley suggests, to not have pissed off Jacques Distler, something I seem to have done long ago with my criticisms of string theory.

I'm more and more convinced that what is going on here is all due to the simple fact that Distler doesn't like me. I was surprised and saddened to note that in his recent posting about Raoul Bott, he links to other postings about Bott by Sean Carroll (who credits my blog as where he learned of Bott's death) and Lubos Motl, but not mine. I also didn't realize that Distler was a couple years behind me at Dunster House, an experience that perhaps he has yet to get over.

Update: While I was writing the above, Alejandro submitted a comment noting that, after initial problems getting his trackbacks accepted "now it seems I am allowed to send trackbacks. Of course, one never knows when this permission can change again."

Comments

1. **sunderpeeche**
   December 23, 2005

   "Do not ascribe to conspiracy that which can be explained by incompetence" — Napoleon Bonaparte

2. **woit**
   December 23, 2005

   Sunderpeeche,

   Good advice in general. And I should make clear I have absolutely no idea how these issues are handled at the arXiv. All I know is that repeated polite requests to arXiv administrators over the past month and a half have been met with no response of any kind whatsoever. I'm having trouble at this point ascribing this to incompetence.

3. **Chris Oakley**
   December 23, 2005

   (Obviously the notion that you might have p***ed off Jacques Distler can be ruled out from the start)

4. **Dumb Biologist**
   December 23, 2005

   Seem's a safe bet whoever's responsible thinks you're a crank, and your views on physics don't merit inclusion in collegial discourse.

   If that's the case, it's kind of sad that there's presently no way to prove them right or wrong, there probably never will be, and you'll just have to accept their opinion on the matter.

   If this is the sorry state of affairs, it ought to give anyone pause before excluding degreed academics from the discussion, as it is itself a symptom of a major problem in a field that purports to be science, but maybe they don't see it that way.

5. **andy**
   December 23, 2005

   I'll point out at this juncture that you seem to censor your blog here. I posted a comment that I thought was germane and you deleted it. I point this out in as friendly a way possible. I'm a big fan of what you're doing here.

6. **ksh95**
December 23, 2005

Peter,

Please let us know how things turn out. I sincerly hope there is a legitimate explaination for this.
It is almost unthinkable to me that anyone on the arXiv advisory board would exibit such juvenile tantrum-like censorship.
I really hope this is not the case.

7. **woit**
December 23, 2005

Hi Andy,

Some form of censorship is necessary, both here and at the arXiv. I don't remember what your comment was that I deleted, but I do delete a lot of comments that seem to me off-topic. Glad to hear you remain a fan despite it all! How stringent I am about this depends on lots of random factors, including how much time I have to look at comments and decide what to do with them, whether I'm in a good or bad mood, etc. The difference with the arXiv case is that I take full responsibility for what I'm doing and will respond with an explanation to anyone who politely asks why I have deleted a comment.

Right now I don't even know that I am being censored since I've been unable to get anyone at the arXiv to respond and tell me what they are doing with requests that I submit. For all I know, my requests are sitting in a queue of things to be dealt with by some overworked arXiv staff member. No way to tell as long as no one there is willing to contact me and let me know what is going on.

ksh95,

I have heard from one advisory board member who promises to look into what is going on and get back to me. Of course, with the holidays now starting it may be a while before relevant people are back at work and willing to deal with this. The bad timing is my fault.

8. **Dumb Biologist**
December 23, 2005

Glad you got in touch with someone.

You might hear back it was an anti-spam snafu. That might even be the real reason.

9. **Anonymous**
December 23, 2005

I'm amused by the thought that "Dumb Biologist" is Paul Ginsparg in disguise.

10. **Dumb Biologist**
December 24, 2005

Ouch, poor Paul Ginsparg. No, absolutely not; I am quite what I say I am.

I will say minds of his caliber, and the knowledge they bring to bear on the increasingly relevant physical and computational problems important to a richer understanding of life science are precisely what the field can always use more of. It's interesting to see he's teaching a physics course on biological applications (which I doubt I'd survive). That's to be applauded.

11. **Quantum_Ranger**
December 24, 2005

Having a number of posts deleted is no big deal,(having encountered this myself) you have to maintain a selective balance of the subject being blogged. The "control" factor for reasons that are "total" and to a specific site namely Peter Woit's?..can only be guessed as someone "up there" does not like what you do?

Up there , being those whose, powers that be, far exceed their authority on the subject?.. they obviously have no knowledge about the subjects in question, or do they wish to promote any correspondance forthwith.

12. **secret milkshake**
December 24, 2005

Peter,

Say, you were an editor of a rather important journal like arXiv and you got lots of trackback request from somebody with extreme view and polemic bend towards a major fashionable enterprize that monopolized high energy theoretical physics – how would you react?

Being born in east-Eurepean contry (LUMO!) I know how ideological establishment works – but these things do not make much difference in the long run. In few years time, there will be new unexplained data from CERN and most of what is happening in the theoretical physics in the last few years will be mercifully forgotten.

"The Nature will come out the way she is – it does not matter how famous you are, how beatiful the theory is – if it is wrong, it is wrong. It's all there is to it."

Either way, I think you are wasting too much time and energy on things that are much like politics – in few years, somebody else will deploy troups and who cares? Stick close to people that do the actual experimental work.

13. **Juan R.**
December 24, 2005

Welcomed to real life… and science,

Perhaps you would contact with

archivefreedom

explaining your case.

A public criticism to ArXiv censure by Nobel winner Brian Josephson was recently published on Nature journal and is openly available here.

Juan R.

Center for CANONICAL |SCIENCE)

14. **FP**
December 24, 2005

Maybe it is a problem that arXiv has a quasi-monopoly as pre-print storage?
Some competition is always healthy, but apparently nobody outside Cornell wants to do the job.

15. **Tony Smith**
December 24, 2005

secret milkshake said "… In few years time, there will be new unexplained data from CERN and most of what is happening in the theoretical physics in the last few years will be mercifully forgotten.
… I think you are wasting too much time and energy on things that are much like politics …".

As to "new unexplained data from CERN" LHC, for all we know any new LHC data may NOT be "unexplained" by the Standard Model.
Indeed, Fermilab has produced a lot of data over the past decades and ALL of it can be "explained" in terms of the Standard Model.
So, maybe it is not only possible, but actually likely, that LHC might find a Higgs around 100 – 150 GeV, with everything else very similar to Fermilab data, extended to somewhat higher energies and luminosities.

That being the case, theoretical physics should not sit on its thumbs for the next few years, but (in my opinion) should look closely at constructing models that include the Standard Model, without supersymmetry or other exotic extensions.

The way to do that is to actively work NOW on theoretical physics, and one way (of the many ways that should be pursued) would be to look at the K-theory math that might lead to a deeper understanding of Quantum Field Theory as suggested by Peter in his blog at http://www.math.columbia.edu/~woit/wordpress/?p=292 about "Latest Freed-Hopkins-Teleman". Unfortunately, as Peter indicates, arXiv seems to have censored Peter's K-theory ideas.

If arXiv (which grew during its Los Alamos days, beginning in the early 1990s, to have an effective world-wide monopoly on distribution of contemporary theoretical physics information among the physicists of the world) censors possibly useful ideas such as (but not limited to) Peter's possibly very useful K-theory ideas, then the loss to physics is IMMEDIATE and SERIOUS, and should not be minimized as "things that are much like politics".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Peter, thanks for the Happy Holidays wishes. I am also celebrating Holidays through next week and the calendar New Year, and I join you in wishing everybody Happy Holidays of whatever kind they prefer.

16. **D R Lunsford**
December 24, 2005

Merry Christmas everyone!

-drl

17. **Not a Nobel Laureate**
December 25, 2005

Dr. Woit – thank you for your efforts in running this blog.

Wishing everyone a Merry Christmas and a Happy and Productive New Year.

18. **andy s.**
December 25, 2005

Why not approach the matter scientifically? You have a theory, namely that arxiv has a bug up its collective ass on the subject of Peter Woit.

Test this hypothesis by having someone other blogger make the same comments you do and see if they get deep sixed as well.

19. **Juan R.**
December 25, 2005

FP Said,

> Maybe it is a problem that arXiv has a quasi-monopoly as pre-print storage?
> Some competition is always healthy, but apparently nobody outside Cornell wants to do the job.

Perhaps you would click on the two links that i provided above. The quasi-monopoly of ArXiv administrators is explained in the letter recently published in Nature journal. Whereas that the Nobel laureate and some other people has launched an free alternative to ArXiv without censure (it is the reader who decides what is 'good' and what is not).

Juan R.

Center for CANONICAL |SCIENCE)

20. **Juan R.**
December 25, 2005

andy s. Said:

> Why not approach the matter scientifically? You have a theory, namely that arxiv has a bug up its collective ass on the subject of Peter Woit.

> Test this hypothesis by having someone other blogger make the same comments you do and see if they get deep sixed as well.

Interesting proposal. Do you believe that Peter Woit is exagerating?

I doubt it, but if your own reply is affirmative, please read first the material contained on links that i provided before obtaining your conclusions. For example the Nobel Prize for physics Brian D. Josephson (University of Cambridge) says in Nature

> The exclusion of particular individuals
> and particular ideas from arXiv appears
> to me to be deliberate.

Curiously some scientists have done experimental studies on ArXiv administration policies proving the premise. One of most interesting i know is the submiting of identical preprints to ArXiv from two different e-mails. Curiously the works submitted from *black listed* e-mails are automatically erased from the ArXiv at few minutes, whereas their copies were stored during days (until manually detected and erased). How can a preprint service reject works on function of e-mail. Would not works be rejected just in basis to scientific issues? For example if a preprint is flagrantly wrong (e.g. Lynds preprint) then would be erased.

Some empirical studies show a correlation coefficient of near 1 for the thesis of deliberate exclusion.

In this intersting blog i know at least three (if i count now to Woit) authors suffering 'censure' from ArXiv.

Juan R.

Center for CANONICAL |SCIENCE)

21. **D R Lunsford**
December 25, 2005

Why not just sue them? Something should be done to rein in those petit-Napoleons. Suing is the best option.

-drl

22. **anon**
December 25, 2005

Lunsford, a turkey breeding (or string theory) community can't afford a diversity that would make the most popular theory (strings/turkeys) look silly. I suggest you give up on trying to reform dictators; they just try to suppress all criticism and shoot the messengers. Instead of telling them what they don't like to hear and having them doing the shooting, you need to adopt more sturdy methods and go on a turkey cull. So I suggest you try building on alternatives to strings until one succeeds in doing more than string theory, then shoot, stuff and slowly roast the turkey.

Merry Christmas

23. **andy s.**
December 25, 2005

Juan R. I have no idea if it's going on or not, which is why I suggested the experiment.

Looks like somebody already thought of my idea. Story of my life.

24. **woit**
December 25, 2005

Danny and others,

I'm not the litigious sort, and this trackback issue is not one of great importance anyway, so I'm not about to sue anyone.

I don't think I should need to try to run experiments on their system to try and figure out what decisions it is making. The arXiv administrators and their advisory board have every right to decide that the kind of commentary that I've requested they post trackbacks to isn't part of legitimate professional discussion and that links to it should not be allowed on the arXiv. All I'm asking is to be informed about whether that's the decision they have made and if so to be assured that they take responsibility for and stand behind such a decision.

If that is their decision, I have a perfectly good venue here for saying what I think about it, and the rest of the community that is familiar with these matters can draw their own conclusions about what this means for the current state of one of the most important institutions in particle theory these days, and by implication, for the health of the field in general.

25. **D R Lunsford**
December 25, 2005

Peter,

I basically don't care what they do – as far as I'm concerned, they have about as much credibility as tachyons. I'm happy to be dissociated from them and have no plans to submit future work there.

But the stories are too similar to be discounted – e.g. what happened to Paul LaViolette happened to me as well (and my paper was *already* published), just as he stated – no answers, no explanations. Being unable to answer any questions, physics or administrative, must be habit-forming.

-drl

26. **Adrian Heathcote**
December 25, 2005

I think it's obvious that the reason they provide no explanation of their reasons for rejection is precisely that they fear being sued. If they don't explain then they provide no ammunition for a skilled attorney. I think it's unlikely that you will ever get anything out of them other than pro forma comments to the effect that 'the decision of what to accept is entirely in the hands of the administrators, blah blah blah'.

Merry Christmas to all

27. **ObsessiveMathsFreak**
December 25, 2005

When you were young, you assummed that scientists were a magnamous, logical and rational bunch. You had great faith in their ability to be imparital and to discern the truth through the application of scientific rigour. You also thought they had great integrity and were above petty actions as they aspired to the greater goal that was The Truth.

Then you grow up and hear about things like this and see that scientists are just as human as everyone else, pettiness included. It's still very disappointing though.

28. **Jose**
December 26, 2005

I've visited sciprint.org, a pretenended competence to arxiv.org, and there are very few papers. I've seen those of the mathematical physics section and none of them can be called a scientific paper, the authors expalin his theories in words, not with mathematics as should be for a physical theory.

29. **Alejandro Rivero**
December 26, 2005

Please note that we are not speaking in this thread about ArXiv articles, but about an ArXiV comment feature. As far as I understand, the comment feature has been suggested if not since the origins, at least since the web doorway of the ArXiV. It is only via Distler that it has been finally incorporated, and it is partial, experimental &c. We do not know how favourable to comments the rest of the ArXiV management are.

30. **woit**
December 26, 2005

Adrian,

The peculiar part of this story is the fact that I've been unable to get out of the arXiv even the sort of pro forma rejection that you describe.

ObsessiveMathsFreak,

I never had any great illusions that scientists were above human pettiness. But I must say that the lengths certain people are willing to go to avoid acknowledging the failures of string theory continues to surprise me, even now.

31. **anon**
December 26, 2005

Once they define string theory as being such a beautiful piece of genuine science, they are actually defending science by censoring out criticism. You have to see this from their warped perspective!

32. **D R Lunsford**
December 26, 2005

It all sounds like groupthink (make up your own examples):

1) Having an illusion of invulnerability
2) Rationalizing poor decisions
3) Believing in the group's morality
4) Sharing stereotypes which guide the decision
5) Exercising direct pressure on others
6) Not expressing your true feelings
7) Maintaining an illusion of unanimity
8) Using mindguards to protect the group from negative information

8) is particularly relevant.

Groupthink seems to be at the bottom of much of our (USA) current dysfunction. Countless examples could be adduced. The Shuttle Columbia and World Trade Center disasters come to mind immediately. (Iraq? New Orleans? the list is endless…)

-drl

33. **Dumb Biologist**
December 27, 2005

Sorry to see this blog has been excluded, though not surprised.

A totally shameful event in the field of biology has been the revelation of fraud in the human embryonic stem cell field, which everyone has heard about already. Amid all the legitimate beating of breasts and rending of garments shall come, I hope, a grateful recognition of the fact that the fraud was revealed, and corrective measures could be taken. Nature being the final arbiter, and all, the truth will aways out. I'm not sure if the fact the outing took place essentially outside of the conventional system of peer review (which appears to have completely broken down, and in Science, of all journals) is something to be happy or unhappy about. Again, I guess it's nice to know smacking into the "real world" will eventually correct for the failings of human investigators, if and when no human mechanism is up to the task.

Unfortunately, at least in some branches of theoretical physics, it seems investigators have forged ahead so far away from attainable real-world checks and benchmarks that the system of peer review is all they've got, or perhaps will have, for a very, very long time. How could the tyranny of groupthink not prevail in such an environment? It's functionally equivalent to a church. Very sad.

34. **Jose**
December 27, 2005

A question:

does http://www.physcomments.org/ pertain to arxiv.org?

I ask this question because there appears a link to this blog, so perhaps censorship is not an appropriate word.

35. **Alejandro Rivero**
December 27, 2005

Jose,

No, it does not pertain to the ArXiV. I pay from my pocket money both the domain and the hosting, as it is explained in the About…

The status of PhysComments about trackbacks has been evolving. It started bad, because of my so-called "spam" to people in the references of each preprint, and because this system included the possibility of anonymous commenters. I have agreed to identify the people commenting in the blog (this is the goal of the line "A comment by…" and now it seems I am allowed to send trackbacks. Of course, one never knows when this permission can change again.

36. **MathPhys**
December 27, 2005

Peter,

While I agree with you that Jacque Distler's behaviour is petty (he's normally a nice man, actually), the current status of string theory is serious business to certain academics: It makes it very hard to apply for promotions, get grants, students, etc, things that materially affects people's lives. I can understand the bitterness (but not condone it).

37. **Peter Wilson**
December 27, 2005

Peter,

After privately censoring me three times (before the cock's call, I might add), you lambast the powers-that-be for censorship.

Nice work!

38. **woit**
December 27, 2005

Peter Wilson,

As I wrote in another comment, I'm not opposed to censorship per se, and some sort of moderation (or censorship if you will….) is necessary in many contexts to maintain a useful forum for information and discussion. The difference here is that I take full responsibility for what I am doing, and will respond to any polite request asking to know why I've deleted a certain comment. I don't remember yours in particular. I do delete a sizable number of comments submitted here, virtually all on the grounds that they are off the topic of the posting. If I don't do this, experience shows that such off-topic posting overwhelm other discussion.

The arxiv has every right to not include trackbacks to my postings. But I believe that they should respond in some way to my polite requests asking what is going on. I have been having a hard time finding out even whether they have decided not to include my trackbacks. The only evidence I have at the moment that they have made such a decision is based on private communications from people who have partial knowledge of what is going on, and insist that I not use their names, since this is evidently a highly contentious issue there.

39. **Matthew**
December 27, 2005

"what happened to Paul LaViolette"

Right. What happened to him is that he got ignored, as he deserved to be, after bugging a ninety-three year old to try to get his paper published.

40. **Arun**
December 27, 2005

I would understand a arxiv policy that disallowed trackbacks to any forum that allowed anonymous posting. But whatever the policy is, it should be made explicit.

41. **JC**
December 28, 2005

MathPhys, and others,

(slightly offtopic)

I wonder how much of the "defense" for the legitimacy of string theory these days has to do with maintaining the gravy train of things like funding grants, promotions, etc … I'm sure it would look pretty bad if all the untenured faculty hired to do string theory over the last few years, all suddenly had most or all of their grant funding yanked before tenure decisions are made. Most people don't want to have to "save face" for their bad decisions. For the case of a physics department who hired string folks for assistant professor jobs, I can imagine things would not look too good for them if their "star" string theorists were denied tenure.

For an untenured string person from a more down to earth and/or selfish perspective, I can imagine that maintaining the string gravy train until a "day of reckoning" or falling into obscurity, as more of a "stalling" tactic until most or all of the present untenured string folks are awarded tenure. Then afterwards if the entire string enterprise collapses under it's own anthropic weight, it wouldn't matter anyways since they already got tenure.

42. **Eli Rabett**
December 28, 2005

ArXiv is partially funded by NSF. A call to NSF, or even an Email to ArXiv asking whether their policy conforms to their NSF funding requirements might be a lot more successful. It seems to me that they would be allowed to reject postings and trackbacks but would have to have a stated policy that was applied uniformly.

The most interesting thing to me about ArXiv is that it represents a fundamental change in policy for the US. When I was going to university in the 1960s, several people who were in a position to know told me that after WWII the question arose as to how the government should, could or would encourage scientific publication. Two models were proposed, direct funding of publications sponsored by organizations such as APS or ACS, or providing page charges as part of grants. The latter was adopted because it would encourage for profit publishing organizations as well. ArXiv and PubMed are direct challenges to the previous policy.

43. **Chris W.**
December 28, 2005

Eli,

Speaking of the for-profit publishers, how do they feel these days about arXiv.org? How often (if at all) do preprints get taken offline because a publisher insists upon it? Given the current contention between the big publishers and the library community, and the litigiousness of the AAP, I would think that preprint servers would be as sensitive a topic as ever right now.

(NOTE: I'm not a professional academic, so I'm a bit out of touch with the on-the-ground realities in this area.)

44. **Tony Smith**
December 30, 2005

Since censorship and Cornell both seem to be involved in this thread, here is a quote from the book Cosmos (first edition, Random House, 1980, at page 91) by Carl Sagan, then "Director of the Laboratory for Planetary Studies and David Duncan Professor of Astronomy and Space Sciences at Cornell University":

"… The worst aspect of the Velikovsky affair is not that his hypotheses were wrong or in contradiction to fimly established facts, but that some who called themselves scientists attempted to suppress Velikovsky's work.

Science is generated by and devoted to free inquiry: the idea that any hypothesis, no matter how strange, deserves to be considered on its merits.

The suppression of uncomfortable ideas may be common in religion and politics, but it is not the path to knowledge; it has no place in the endeavor of science.

We do not know in advance who will discover fundamental new insights. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

45. **Rien**
December 31, 2005

Chris W,
Preprints don't get taken offline, all the relevant publishers allow arxiving. If they didn't, nobody would publish in their journals. Some of them actually say you're not allowed to update the eprint to reflect the published version but I don't think anyone cares about that. At least I don't.

In fact, preprints can't be removed once they're submitted, even if they are withdrawn the older versions are still available.

46. **John Gonsowski**
December 31, 2005

Wonder what former Cornell and current Columbia string theory guru Brian Greene would think about this thread?

47. **FS**
December 31, 2005

There is an International Mafia in Science which controls ideas, awards
felowships and prizes,
and ignores or even boycotts people who have different opinions than theirs.

A Few New Links

December 27, 2005
Categories: Uncategorized

Eckhard Meinrenken has been teaching a course at Toronto on Lie groups and Clifford algebras, and he has lecture notes available. This is beautiful mathematics and brings together several of my favorite mathematical topics: Clifford algebras, spinors, and representation theory of Lie groups. Some of this material is quite new, and very possibly has interesting applications in physics.

Dennis Gaitsgory, a new young tenured member of the Harvard math department, has been teaching a course on Geometric Representation Theory, and also has lecture notes available. These explain the "D-module" point of view on the subject.

My Columbia math department colleague D. H. Phong and fellow ex-Princeton grad student Eric D' Hoker have a new paper on the arXiv called Complex Geometry and Supergeometry. It is based on Phong's recent lecture at this year's Current Developments in Mathematics conference at Harvard last month. D' Hoker and Phong have been able to explicitly write down and show finiteness of superstring amplitudes at two loops, with the problem still remaining open at higher loops.

The arXiv web-site has a new look today. Gone are Paul Ginsparg's "skull and cross-bones" icons like this.

Comments

1. **QWERTY**
   December 28, 2005

   I CANT BELIEVE IT, CLIFFORD AGLEBRAS! IN FACT I WAS JUST PERUSING """ CLIFFORD MODULES """ BY """ ATIYAH BOTT AND SHIPARO """ YESTERDAY, ITS FUN FOR ALL THE FAMILY!!1! IN MORE FACT, BTW, I REMEMBER WHEN I WAS YOUNG AND I FIRST LEARNT ABOUT UNIVERSAL PROPERTY AND FILTRATIONS AND CONJUGATION AUTOMORPHISMS, I JUMPED UP AND DOWN WITH GLEE. AND THEN I WAS AMAZED ALL OVER AGAIN ONCE I READ ABOUT GENERALIZING THEM USING DIFFERENT IDEALS, AND FRACTIONAL CLIFFORD STRUCTURES AND THEIR REPRESENTATION THEORY. IT WAS SO SUPER-LATIVE THAT IT MADE ME TINGLE ALL OVER, EXCEPT I DIDNT REALLY BECAUSE THAT WOULD BE VERY SILLY INDEED. BTW I DIDN'T REALLY JUMP UP AND DOWN WITH GLEE EITHER. HMMMMM.

   PETER WHY ARE YOU TALKING ABOUT STRING THEORY AND SUPERANYTHING WITHOUT ACCOMPANYING DISPARAGING COMMENTS? A Z2 GRADING IN SPINORS IS ELEGANT BUT ON SUPERSPACES IT JUST OFFENS MY SENSIBILITIES. WHY NOT SWITCH TO VENERATING ABHAY ASHTEKAR – LQG WILL BEATS STRINGS FOREVER!!

2. **garrett**
   December 28, 2005

   Here's a recent paper relating this stuff to physics:

   http://arxiv.org/abs/gr-qc/0511120

   It shows how the fields and dynamics of gravity and the standard model can be represented as a single Clifford bundle connection and its curvature.

3. **anon**
   December 28, 2005

   Garrett, well done, it looks far more sensible than string theory.

4. **Arun**
   December 28, 2005

   http://insti.physics.sunysb.edu/~siegel/parodies/misanthrope.html

5. **Samuel Prime**
   December 28, 2005

   These Clifford algebra notes look good and are worth having. Thanks for mentioning it. I had a colleague long ago give seminar lectures on them, which i really enjoyed. Admittedly, I have used Clifford algebras only sparingly in relation to K-theory of C*-algebras (through Connes' work). (Particularly, C^{p,q} = R^p + R^q.)

6. **D R Lunsford**
   December 29, 2005

   It's very dry, what? (When did lively style go out of fashion, then?) For physics, this sort of approach is hopeless. He took a lively idea and made it seem dull. That took skill.

   Why (in these papers) is it always about restating the known?

   (Arun: extremely funny 🙂

   -drl

7. **D R Lunsford**
   December 29, 2005

   Siegel has the best line of all time about ST:

   "Cosmology is the opposite of string theory: Instead of starting with 10 or 11 dimensions, and compactifying most of them, you start with no dimensions (except time), and the Big Bang uncompactifies 3 of them."

   ..and a candidate for the best footnote ever:

"500 – No, it's really an exponent, not a footnote. You don't seriously think there could realistically be 500 footnotes, do you?"

-drl

8. **Levi**
   December 29, 2005

   I printed out all 96 pages of the lecture notes by Meinrenken. Nice stuff. Reading them carefully could take a while though…

9. **Tony Smith**
   December 30, 2005

   Peter, you said "… The arXiv web-site has a new look today. Gone are Paul Ginsparg's "skull and cross-bones" icons …".

   Just to be nit-picking historically, the "skull and cross-bones" icons were NOT the ORIGINAL logo/icons of what is now called the Cornell arXiv. IIRC, the first web site was at http://MENTOR.LANL.GOV/ and its logo/icon was the image that I have attempted to preserve for posterity at http://www.valdostamuseum.org/hamsmith/Mentor.gif (as you can see, it is more related to Ancient Egyptian Pyramids than to the more recent ocean pirates).

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

10. **jeffrey collins**
    January 11, 2006

    String theory offers promsie, but we do have as of yet a fundamentally satisfying way to falisfy it, or prove it true. The LHC will certainly give us some answers, but sheer philosophy about the string is simply not science. Something is missing, and we do not know-yet–what we are missing.

Jerusalem Winter School

December 29, 2005
Categories: Uncategorized

The 23rd Jerusalem Winter School in Theoretical Physics started yesterday. The topic is "String Theory: Symmetries and Dynamics", and it is organized by David Gross and Eliezer Rabinovici.

Some of the talks are already available on-line, with the quality of the video and audio very good, although you need the latest version of Apple's Quicktime player. In his opening talk, Gross mentioned the recent New Scientist article quoting him as admitting string theory was in trouble, saying that the article misrepresented what he said. At the recent Solvay conference he had said something like "In string theory we don't know what we are talking about", and the New Scientist reporter interpreted that as meaning there was trouble, an interpretation Gross disagreed with. He was annoyed by the New Scientist editorial about the sorry state of string theory, and says he has been offered the opportunity to write a rebuttal and may do so. Gross went on to claim that really string theory is a vital subject and that it is in a wonderful period. He didn't mention the Landscape.

Update: Another recent particle theory conference was the Christmas Meeting at Durham. There's a report from the conference by blogger Paul Cook. Evidently Herman Verlinde is taking bets that string theory is the correct unified theory. Those who want to make some easy money might want to contact him. Then again, it's unclear when you would get paid.

Update: The lectures by my Princeton classmate Igor Klebanov on using string theory to study strongly coupled gauge theories are particularly clear and interesting. Near the beginning he mentions this blog quoting it as saying "String theory is not good for anything." I'd like to emphasize that that is not something I wrote or something that I think, I assume he is referring to one of my commenters.

Comments

1. **Alejandro Rivero**
   December 29, 2005

   I had already suggested that it was just a misinterpretation of a very repeated slogan about M-theory.

2. **Chris Oakley**
   December 29, 2005

   I suppose that this means that the plea, "Never, never, never, never give up!" will have to be changed to "Never, never, never, never, never, never, never, never, never, never, never, give up!"

   I am sure that Winston Churchill would have admired their determination, if not their judgement.

3. **garrett**
   December 29, 2005

   And here I thought De Nile was only near Jarusalem, and not actually in it.

4. **Danger**
   December 29, 2005

   NSF crackpot fund.

   Once you realize how many millions are at stake, it's easier to understand the String Theorist's supreme commitment to inceipherable nonesense.

   http://www.nsf.gov/awardsearch/piSearch.do;jsessionid=12140160808182889516AF72BFC8F425?SearchType=piSearch&page=1&QueryText=string&Search=Search#results

5. **Quantoken**
   December 30, 2005

   It doesn't matter what Gross says about super string theory. SST already has the endorsement of the greatest mathematician of all times and greatest physicist since Sir Newton, Einstein notwithstanding, Mr. Edward Witten, it also has the endorsement of the most famous physicist since the invention of Television, Mr. Michio Kaku, and the endorsement of a number of other big guys. Who is Gross to say either way about SST?

   At the end of day, only facts speak the truth. The true is still that SST is in trouble after more than 2 decades, whether Gross agree or disagree with that notion.

6. **MathPhys**
   December 30, 2005

   I think the problem with a web site such as this, is that it attracts more than its share of crackpots. That's probably inevitable.

7. **Tony Smith**
   December 30, 2005

   Danger said "… Once you realize how many millions are at stake, it's easier to understand the String Theorist's supreme commitment to inceipherable nonesense. …" and then cited a search link at http://www.nsf.gov/awardsearch/ for the word "string". I did a similar search for the word "superstring", and here are four large grants on the resulting list:

   0098527
   Theoretical Physics
   PHY CENTRAL & EASTERN EUROPE PROGR, ELEMENTARY PARTICLE THEORY
   08/15/2001
   Smith, John
   NY

SUNY at Stony Brook
$1,552,181.00

0354776
Theoretical Physics
PHY
ELEMENTARY PARTICLE THEORY
08/01/2004
Smith, John
NY
SUNY at Stony Brook
$986,000.00

0098395
Problems in Theoretical Physics
PHY
ELEMENTARY PARTICLE THEORY
08/15/2001
Sugar, Robert
CA
University of California-Santa Barbara
$2,068,257.00

0098840
PHYSICS: THEORY OF ELEMENTARY PARTICLES
PHY
ELEMENTARY PARTICLE THEORY
09/01/2001
Murayama, Hitoshi
CA
University of California-Berkeley
$2,242,047.00

Of course, superstrings get funding from agencies other than the NSF, so these may not be the largest government grants to superstrings, but it seems to me that they indicate that a slogan for present-day physicists might be (to use a word from Danger's comment):

NONSENSE PAYS .

Tony Smith
http://www.valdostamuseum.org/hamsmith/

8. **A.J.**
December 30, 2005

Tony Smith — Nice research finding the grant figures, but your interpretation is a bit off. Please note that none of the grant recipients are actually string theorists. Jack Smith and Hitoshi Murayama are well-known phenomenologists, and Robert Sugar does lattice gauge theories. They might have tossed string theory into their grant applications as a buzzword, but I can guarantee that only a small fraction of that money is supporting string theory research. Most of that money is being spent on down-to-earth particle physics.

(Frankly, if the NSF were dumping as much money into strings as you guys imagine, then there would be far more funding for string theory graduate students. As it is, at all but the wealthiest schools, there's usually not enough to go around.)

9. **woit**
December 30, 2005

A.J. and Tony,

One thing to remember is that many of these grants are group grants: they fund all or most of the particle theory group at an institution. So, even if only part of the group is doing string theory, string theory will appear in the proposal. Even if the P.I. doesn't do string theory, many of these group grants do include string theorists.

If you want to figure out how much is actually going to string theory, you need to get ahold of the detailed budgets and see where the money goes. If you can do that, you have the added bonus of being able to compute the actual salaries of most of the people involved…

10. **Anonymous**
December 30, 2005

Another thing to keep in mind is that these grants generally cover multiple years. Paying the salaries of a few postdocs over a few years adds up to a reasonable amount of money, but it's not as if anyone is making millions each year doing string theory! String theorists earn approximately the same amount of money as other physicists, generally speaking, but their research requires comparatively little money. Most of the money going to physics goes to experiment (as it should!), where the research itself can require astronomical amounts of funding. There is always much more money to support experimental graduate students than theory graduate students (again, this is as it should be!).

Tony, I suggest you think twice about what you label as "nonsense." As A.J. says, much of what is being funded from those numbers is not string theory. Hitoshi is a good example of a particle theorist whose work is mostly guided by experiment. It might be true that much of what comes out of particle theory these days isn't very good, but that's because it's hard to work without experimental guidance. Wait five or ten years for LHC results and then we'll see who's still producing nonsense and who isn't. (Maybe even some of the hardcore stringers will switch!)

Peter, thanks for the link to the Jerusalem school. The lectures are nice and pedagogical, and steer clear of the more dubious parts of recent string theory research. It's interesting that Kutasov speculates there might be different universality classes of string theories and that so far everyone might be focusing on ones that don't describe the real world.

11. **ksh95**
December 30, 2005

MathPhys says:
I think the problem with a web site such as this, is that it attracts more than its share of crackpots. That's probably inevitable.

Hmmm, I wonder if that's true. This anti-string theory site seems to attract a fair number of well-known reputable scientists. I wonder what the mean education level is here and at other science blogs.

12. **Tony Smith**
December 30, 2005

Anonymous suggests that I should "… think twice about what …[I]… label as "nonsense." As A.J. says, much of what is being funded from those numbers is not string theory. …".
That is a point well taken.
Although I do regard much of conventional superstring theory to be nonsense as physics (and all the grants I listed were for funding for physics, and not interesting related mathematics etc), l do agree that any part of those funds going to experiment might be well spent, at least if the experiments are truly fair searches for unexpected new phenomena, and not narrowly focussed by triggers and cuts to look only for those phenomena that are expected by conventional superstring theorists.

I fear that LHC might not turn out to be as useful as it might be if all the cuts and triggers are set by consensus expectations of currently popular theoretical models. I certainly hope that Anonymous is correct in stating "… Wait five or ten years for LHC results and then we'll see who's still producing nonsense and who isn't. (Maybe even some of the hardcore stringers will switch!) …".

I also wish that Anonymous were not Anonymous, so that I would know more about the background of a person whose comments seem reasonable to me. However, anonymity can be interesting – I have often toyed with the idea of submitting comments, etc, through a sock puppet so that I could make a comparison of responses, but I have not yet done so.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

13. **Anonymous**
December 31, 2005

LHC triggers will find events with high-energy leptons or photons, with large missing transverse energy (a sign of invisible stable particles, as expected in SUSY, but maybe in other things as well), or events with sufficiently many high-energy jets. It's hard to think of new physics that wouldn't be found by decays to high-energy Standard Model particles, so this is pretty flexible. Even more bizarre scenarios with long-lived charge particles have been studied and can generally be found. It's hard to think of how to make the triggers accept more things without overloading the available bandwidth; accepting more low-energy jets is impossible, for instance, because of QCD background. The limitations on the hardware and software for triggering are very strict, so it is pretty highly optimized. Of course, if you have ideas of plausible new physics that wouldn't be found by the triggers, you should start bugging people about it!

14. **Tony Smith**
January 1, 2006

Anonymous said that if I "… have ideas of plausible new physics that wouldn't be found by the triggers …", then I "… should start bugging people about it! …".

My main concern is that observation of T-quark events may limited by cuts to the energy region within a few GeV of the 173 GeV peak that was observed at Fermilab, whereas
it seems to me (in my unconventional opinion) that Fermilab's T-quark event data also shows two other "peaks", or "regions of interest" within 10% or so of about 130-140 GeV and 225 GeV, and therefore I would like to see cuts set so that those regions are also studied at least to some degree.

I think that a set of 3 peaks around 130-140, 173, and 225 GeV might indicate that the Fermilab observed T-quark events are NOT simple decay of an isolated quark, but are due to an interesting 3-element system involving the Vacuum, the Higgs, and a T-quark condensate, described by Nambu-Jona-Lasinio and variants thereof. Some material about those ideas can be found at http://www.valdostamuseum.org/hamsmith/YamawakiCP2KKNJL.html with more details being available on other pages of that web site.

I did present this idea at the APS DPF meeting in Tampa in April 2005 in a session chaired by Joe Lykken, and he seemed to have some interest in it at the time, but I have heard nothing further, so I assume that it is not being pursued.

Unfortunately, my status of being blacklisted by the Cornell arXiv affects my reputation in the physics community adversely, and a probable consequence thereof is that if I were to "start bugging people about it", it might do more harm than good.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

15. **anon**
January 1, 2006

If you don't bug other people, nothine ever gets done.

"… the innovator has for enemies all those who have done well under the old conditions, and lukewarm defenders in those who may do well under the new. This coolness arises partly from fear of the opponents, who have the laws on their side, and partly from the incredulity of men, who do not readily believe in new things until they have had a long experience of them. Thus it happens that whenever those who are hostile have the opportunity to attack they do it like partisans, whilst the others defend lukewarmly…" – http://www.constitution.org/mac/prince06.htm

16. **Anonymous**
January 1, 2006

Tony, the good thing about complaints like yours is that they are issues of analysis, not of triggering. The data are there, you just have to convince someone to analyze the right mass windows. And as anon says, the only way to do that is to bug people in the experiments.

17. **Tony Smith**
January 1, 2006

Anonynmous is quite correct in saying "… The data are there, you just have to convince someone to analyze the right mass windows. …".
Also, Anonymous is probably correct in saying "… as anon says, the only way to do that is to bug people in the experiments. …".
However, my capability of bugging people has severe limitations, including the following:

1 – I have no institutional affiliation through which to "bug" LHC people by contacting them through institutional channels;
2 – I did speak at APS DPF April 2005, but such talks alone seem to be ineffective (most people at such big conferences seem to me to spend almost all their time with like-minded people, lining up support for grants, jobs for their students, etc);
3 – I can speak my mind on a forum such as this blog or on my web site (and as you can see I have done so), but blacklisting prevents me putting a series of papers about the issues on the relevant parts of Cornell arXiv, thus eliminating a very effective way to "bug" LHC people;
4 – The fact that I am blacklisted not only diminishes my reputation in the physics community, it also apparently frightens some in the physics community so much that they seem to be afraid to be associated with me.

As to anon's advice to me in comment 16 hereinabove:

It is sad enough that there has been very little advance in elementary particle theory since the 1970s (when the Standard Model was developed),
but it is far sadder still that
back in 1980, in his book Cosmos, Carl Sagan could say "… The suppression of uncomfortable ideas may be common in religion and politics, but it is not the path to knowledge; it has no place in the endeavor of science. …",
while
now in 2005/2006, Machiavelli is considered to be the teacher who must be followed in order to succeed in elementary particle physics.

Maybe the leaders of today's theoretical particle physics establishment should be asked the McCarthy question:

HAVE YOU NO SHAME ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

18. **Shantanu**
January 1, 2006

Peter, here is a website of a school on early universe cosmology
involving topics in BOTH string theory and LQG , with back-to-back
lectures in both topics.
http://www.iucaa.ernet.in/~scveu
Hope the slides get archived.

19. **Anonymous**
January 1, 2006

Tony, I have one more suggestion. Your website is somewhat disorganized, full of long quotes, and hard to approach. Also, it advocates a number of very speculative ideas. I, for one, find it hard to isolate the parts of it that I might think are interesting. I hope you will not be offended if I suggest that in part the reason you are not listened to is that you are not effective in communicating in the typical language of physics and physicists.

It is possible for someone to be interested in your ideas about the top quark data without being interested in the bulk of your website. An experimentalist you might convince to take a closer look at the data would likely fall into this class.

Thus, if you could write up a clean (preferably TeX'ed, available as PDF) account *soley of the top quark data*, with general remarks about why one might expect multiple apparent masses to show up in data (top condensates, etc….), and indications of what you think are the hints that these in fact *do* show up in the data, I think you would be *far* more likely to get a response from someone. Divorce your idea about re-analyzing the data from particular ideas about new physics. The RG arguments for a particular heavy top mass are fairly well-known, but the general attitude is that since we found it somewhere else, those models must be too simplistic. You really need to argue from the data, at least partly. Also, reliance on NJL models is not so great; qualitatively they're very interesting, but as non-renormalizable field theories one doesn't tend to trust exact numbers from them, since one tends to view them as effective theories modified by other new physics we don't understand.

If you produced a clean, well-argued document focusing *only* on top quark physics, I think at least a small number of people would read it. If it's *convincing*, one of those people might be able to do something about it.

One last note: you would be wise to not take the tone that you are certain to be right. It grates on the people who have done difficult experimental work over the past decade or more. You should suggest this as interesting, potentially overlooked physics, not as truth. And it would help if you exhibit some willingness to admit you might be wrong and the experimentalists so far might be right. (Even if there are hints of something at other masses, the amount of data is likely too small to say anything definitive at this point. Don't mistake caution on the part of Fermilab experimentalists for conspiracy.)

Best of luck.

20. **Tony Smith**
January 1, 2006

Anonymous is quite correct that my website "… is somewhat disorganized, full of long quotes, and hard to approach. Also, it advocates a number of very speculative ideas. …".
However, I view my web site as my home, containing all sorts of stuff from oil and politics to Jesus and Mary Magdalene to math and physics, and a lot more. It is not intended to be a clear exposition of a single topic (such as LHC cuts for T-quark candidate events).

Anonymous also said that it might be good for me to "… write up a clean (preferably TeX'ed, available as PDF) account *soley of the top quark data*, with general remarks about why one might expect multiple apparent masses to show up in data (top condensates, etc….), and indications of what you think are the hints that these in fact *do* show up in the data …".
That is substantially what I did for my 10-minute talk at APD DPF 2005 in Tampa, and, as I said, the chair (Joe Lykken) seemed interested at the time, and there is a pdf version of that talk on my web site at http://www.valdostamuseum.org/hamsmith/YamawakiNJL.pdf However, it is not in LaTex because I was using the pdf file projected from my computer onto the screen as the basis for my talk. Even if I did convert it to a standard-academic-LaTeX look, there is no place that I can post it where LHC folks can read it (without it being on my web site, which as you point out they might dislike due to it containing a lot of controversial stuff) because I am blacklisted by the Cornell arXiv and the CERN EXT preprint series has been terminated.

Anonymous also said "… reliance on NJL models is not so great … as non-renormalizable field theories one doesn't tend to trust exact numbers from them, since one tends to view them as effective theories modified by other new physics we don't understand …".
That is true of pure NJL (which might decribe one of the 3 peaks from my point of view), but the variants of NJL which might describe the other 2 peaks may include renormalizable structures that would in fact be the "new physics" of which pure NJL is a corresponding effective theory, as has been indicated by the work of Yamawaki in hep-ph/9603293 and of Hashimoto, Tanabashi, and Yamawaki in hep-ph/0311165. All this is work in progress, but a lot of work has been done including some numerical calculations that can be compared with experiment, and I find it sad that some people who advocate conventional superstring theory (which seems to me

obviously to be far more incomplete) might attack Yamawaki-type models because they may not yet be completely perfected.

Anonymous also said that I "… would be wise to not take the tone that you are certain to be right. … Don't mistake caution on the part of Fermilab experimentalists for conspiracy. …".
If I have ever said anything to the effect that I am "certain" to be "right" about ANYTHING, I apologize and retract it. My whole life and everything I do is only a work in progress, and I NEVER expect to achieve "certainty" in this lifetime.
However, at any given time I do have a certain amount of enthusiasm for the ideas that I hold in the moment, and it is hard to suppress that enthusiasm, but over the years I have always been happiest when I found a flaw in a model and then reworked it, either correcting it and making it better, or finding that the model was irreparably flawed, and thereupon throwing it out and not worrying with it any more. I do apologize if that enthusiasm appears to be certainty.

As to "conspiracy" of "Fermilab experimentalists", I think that "conspiracy" is not a correct description, but, based on years of personal experience, and also on the book The Evidence for the Top Quark: Objectivity and Bias in Collaborative Experimentation, by Kent W. Staley (Cambridge University Press 2004), I feel that the organizational structure of Fermilab has produced some results that might, from the outside, reasonably be perceived as conspiratorial, although they were actually probably produced by dysfunctional aspects of Fermilab bureaucracy.

I don't think that Peter's blog is the place to go into any further detail on such things, but I would be willing to participate in e-mail discussion of further details.

Tony Smith
current, but probably not permanent, email address can be found at
http://www.valdostamuseum.org/hamsmith/tsemailaddress.html

21. **JC**
January 2, 2006

Tony,

(I'm not saying this to be belligerent).

Most folks seem to already have a lot of stuff on their plates, and don't really want anymore stuff to deal with. Whether it's from physical and/or mental exhaustion, there's only so much stuff which can be done in a day. In some fields, such as string theory, just keeping up with the current literature is almost equivalent to a full time job.

For many folks when they are young and full of enthusisam, it's easy to want to read a lot of books and papers, and working out a lot of calculations. When the same folks get older and into academic (ie. professor) type jobs, there's a lot more stuff on their plates such as teaching, bureaucratic, etc … type duties. With all kinds of stuff on their plates, there's only so much time which can be allocated to reading new papers, that one has to pick and choose and be more selective. There isn't enough hours in the day to still work like a grad student or postdoc, especially if one also has more outside commitments like a family.

Whether somebody's research is right or wrong in the long term, is largely irrelevent when it comes to short term funding issues. (Just have to ask some older professors who worked on analytic S-Matrix theory in the 1960's). It's unfortunate the system works this way, with funding issues governing the direction of research and the "herding" mentality which comes with it.

22. **D R Lunsford**
January 2, 2006

I love Tony's website, it's full of interesting things. The 3-d diagrams of various polytopes alone are worth the time spent visiting.

-drl

23. **John Gonsowski**
January 3, 2006

Some experiments seem to have low signal to noise ratios that make them easy targets for the "herding". I would not want to have my bets on physics decided by "herded" experiments.

Those A-D-E series polytopes (and the associated Triality) are the main reasons I became certain Tony is right. Not actually being Tony, I'm allowed to say that, right? I'll finish making my own page about this some time soon but it's hard to put intuition into words so hopefully the pictures talk well.

One visitor to Tony's site told me he found the linear organization of Tony's downloadable e-book easier to use. I think you can get the same effect by using the outline you get to through Tony's "Keywords" link.

What Is Your Dangerous Idea?

January 2, 2006
Categories: Uncategorized

John Brockman's Edge web-site has an annual feature where he asks a wide array of scientists and others how they would answer a hopefully thought-provoking question. Last year the question was What Do You Believe Is True Even Though You Cannot Prove It? This year it's What Is Your Dangerous Idea?

There are responses to this question from 117 different people, a large fraction of them psychologists or cognitive scientists. Among the responses from physicists, several deal with the Landscape as a dangerous idea. Susskind takes credit for it, noting "I have been accused of advocating an extremely dangerous idea", and that some of his colleagues believe it will lead to the end of science, leaving no way to defend physics as a truer path to knowledge than religion. He proudly describes the anthropic Landscape idea as "spreading like a cancer."

On the opposite side of the issue, Brian Greene emphasizes the dangers of the Landscape philosophy:

When faced with seemingly inexplicable observations, researchers may invoke the framework of the multiverse prematurely — proclaiming some or other phenomenon to merely reflect conditions in our bubble universe — thereby failing to discover the deeper understanding that awaits us.

Paul Steinhardt is more emphatic about these dangers:

I think it leads inevitably to a depressing end to science. What is the point of exploring further the randomly chosen physical properties in our tiny corner of the multiverse if most of the multiverse is so different. I think it is far too early to be so desperate. This is a dangerous idea that I am simply unwilling to contemplate.

He also has his own "dangerous idea", about a cyclic model of the universe explaining the small size of the cosmological constant. Lawrence Krauss gives his own version of an explanation of the danger that the Landscape will lead to an end-point for theoretical physics:

… all so-called fundamental theories that might describe nature would be purely "phenomenological", that is, they would be derivable from observational phenomena, but would not reflect any underlying grand mathematical structure of the universe that would allow a basic understanding of why the universe is the way it is.

Some other interesting contributions from physicists come from Philip Anderson, who has some speculative comments about dark matter and dark energy, Lee Smolin, who discusses the possibility of natural selection having something to do with fundamental laws, and Carlo Rovelli, who remarks that we have still not completely absorbed the revolutionary ideas of 20th century physics:

I think that seen from 200 years in the future, the dangerous scientific idea that was around at the beginning of the 20th century, and that everybody was afraid to accept, will simply be that the world is completely different from our simple minded picture of it. As the physics of the 20th century had already shown.

What makes me smile is that even many of todays "audacious scientific speculations" about things like extra-dimensions, multi-universes, and the likely, are not only completely unsupported experimentally, but are even always formulated within world view that, at a close look, has not yet digested quantum mechanics and relativity!

Comments

1. **Adrian Heathcote**
   January 2, 2006

   I think Carlo Rovelli is right on the money. Physics moved ahead too quickly in the last century, without properly understanding the mathematics of quantum mechanics. The big advances in understanding QM came after 1960 when QFT had more or less been finalised. Parables about building on quicksand come to mind.

   Happy New Year

2. **Who**
   January 2, 2006

   posted today

   [url]http://arxiv.org/abs/hep-th/0601001[/url]
   [b]The String Landscape, Black Holes and Gravity as the Weakest Force[/b]
   Nima Arkani-Hamed, Lubos Motl, Alberto Nicolis, Cumrun Vafa
   20 pages, 5 figures

   ionesco's rhinoceros
   et tu Lube?

3. **Chris W.**
   January 2, 2006

   The String Landscape, Black Holes and Gravity as the Weakest Force
   Nima Arkani-Hamed, Lubos Motl, Alberto Nicolis, Cumrun Vafa
   20 pages, 5 figures (hep-th/0601001)

   > If true, our conjecture shows that gravity and the other gauge forces can not be treated independently. In particular, any approach to quantum gravity that begins by treating pure gravity and is able to add arbitrary low-energy field content with any interactions is clearly excluded by our conjecture. Of course in string theory all the interactions are unified in a way that makes treating them separately impossible. In particular, if we take the standard model gauge (augmented by SUSY or split SUSY or other particles leading to precision gauge coupling unification), we have perturbative gauge couplings at a very high energy scale, and our conjecture then implies that there must be new physics at a scale beneath the Planck scale, given by …. which is close to the familiar heterotic string scale [of order] 1017 GeV.

4. **Chris W.**
   January 2, 2006

   Susskind offers this curious remark:

> Another danger that some of my colleagues perceive, is that if we "senior physicists" allow ourselves to be seduced by the Anthropic Principle, young physicists will give up looking for the "true" reason for things, the beautiful mathematical principle. My guess is that if the young generation of scientists is really that spineless, then science is doomed anyway. But as we know, the ambition of all young scientists is to make fools of their elders.

I say curious inasmuch as this remark suggests that Susskind has been concealing his motivations. Perhaps he anticipates being deemed not crazy, but rather crazy-like-a-fox, at some point in the not-too-distant future. More precisely, perhaps he has decided that the best way to promote the intellectual independence of the post-1990 generation of theoretical physicists in the face of these deep problems is to convince them that their erstwhile mentors have become depressed, foolish, demented, or outright raving lunatics.

5. **Kasper Olsen**
   January 3, 2006

   Adrian quoted Rovelli as of saying:

   > Physics moved ahead too quickly in the last century, without properly understanding the mathematics of quantum mechanics.

   This is a really odd statement; you cannot blame physics for moving too fast, but maybe rather Rovelli for not having understood quantum mechanics; what math is it which is so complicated? Hilbert spaces? Linear equations? Or matrices?

6. **hack**
   January 3, 2006

   "But as we know, the ambition of all young scientists is to make fools of their elders."

   Susskind is an old scientist, who wishes to be forever young, therefore it is his ambition to make a fool of himself.

   Mission accomplished!

7. **D R Lunsford**
   January 3, 2006

   It's interesting the see Carroll go apoplectic over a supposed attack on the holy balance of gender, while he calmly entertains the most absurd and twisted physical ideas.

   Here's my "dangerous idea" (what a load of dung for a topic) – Western man is committing intellectual suicide by allowing the entire estate to be tended by such hysterical stewards.

   -drl

8. **D R Lunsford**
   January 3, 2006

   Bravo KO!

   -drl

9. **Charles Daney**
   January 3, 2006

   Greene, Steinhardt, and Krauss all make reasonable arguments against Susskind's Landscape idea, or similar ideas associated with any concept of a "multiverse", eternal inflation, etc. The arguments are generally of the form: It's a cop-out to accept that the important physical constants are simply random variables. It would discourage continuing efforts to find fundamental principles that explain the values of these constants. And it's not a testable or refutable idea anyhow.

   But then I'm reminded of another theory which posited that fundamental aspects of reality are random variables — quantum theory. That has turned out OK, even though Einstein didn't like it.

   And further, I don't see how the idea that there are basic principles determining the fundamental constants (as Einstein and many others have supposed) is refutable either. The idea that there really is a scientifically sound "theory of everything" may be testable if/when we have a candidate to test — but not before.

   And what if we did happen to find a satisfactory theory that fully determined all the fundamental constants, as Einstein expected? Then we would have to face a rather striking conclusion — that the fundamental principle(s) together with pure mathematics require that all the physical constants have specific values which just happen to allow our kind of life to exist. Wouldn't that itself be a conclusion which demands an explanation?

   Just asking.

10. **Chris Oakley**
    January 3, 2006

    > Another danger that some of my colleagues perceive, is that if we "senior physicists" allow ourselves to be seduced by the Anthropic Principle, young physicists will give up looking for the "true" reason for things, the beautiful mathematical principle. My guess is that if the young generation of scientists is really that spineless, then science is doomed anyway. But as we know, the ambition of all young scientists is to make fools of their elders.

    I wonder what exactly Susskind expects young researchers to make of this?
    Currently, the academic system rewards obedience and punishes independent thought, unless the thinker in question is already well established (Susskind or Weinberg, for example). Young researchers who are prepared to snub their noses to the fashionable trains of thought do not tend to last more than a year or two, unless they have obtained a permanent job by some fluke. Even then, they will be ostracised. Although elderly gentlemen like Susskind may say that they relish the prospect of being proved wrong by some 24-year-old, they are in fact conveniently forgetting that the hierarchical structure they sit on will silence most dissenting voices long before they get a chance to be heard.

11. **Adrian Heathcote**
    January 3, 2006

    Kaspar said

    "what math is it which is so complicated? Hilbert spaces? Linear equations? Or matrices?"

Unbounded self-adjoint operators with their domain restrictions (see Reed and Simon vol 1); State spaces of operator algebras (see Alfsen and Schultz, 2 volumes); Hyperfinite Factors; Gleason-Kochen-Specker proofs; C^* Algebras and Information theory; etc.

Time to update beyond your Dirac or von Neumann texts from the 1930s. A lot has happened. (The Copenhagen interpretation really did chloroform the physics community! I think that is what Rovelli is complaining about.)

12. **Tony Smith**
January 3, 2006

Chris Oakley et al quoted Susskind as saying that "… if the young generation of scientists is really that spineless …[as to]… give up looking for the "true" reason for things, the beautiful mathematical principle … then science is doomed anyway … ".

Since Susskind himself is, by advocating his Landscape, "… … giv[ing] up looking for the "true" reason for things, the beautiful mathematical principle … ",
it seems to me that he himself is advocating that "science" be "doomed", and
that his comment seems to be an attempt to shift the blame for any resulting Dark Age from himself to the "spineless … young generation".

As one who has a model that does allow calculation of the fundamental constants, which model is therefore testable, I find the questions of Charles Daney:

"… what if we did happen to find a satisfactory theory that fully determined all the fundamental constants, as Einstein expected? Then we would have to face a rather striking conclusion – that the fundamental principle(s) together with pure mathematics require that all the physical constants have specific values which just happen to allow our kind of life to exist. Wouldn't that itself be a conclusion which demands an explanation? …"

to be interesting.

Tony Smith
htttp://www.valdostamuseum.org/hamsmith/

13. **Kasper Olsen**
January 3, 2006

Unbounded Ignorance…
Ups! This is of course something which I should have known about, sorry. But I'm sure Adrian will help me. For starters: what is an unbounded operator? One without boundaries? (It's hard for me to imagine what this could mean). How can such an OP have domain restrictions when it is unbounded? What is a state space of OP algebras? (I've never heard about an operator being in some specific "state"). And C^* algebras? What does the C stand for – and more interestingly the * – is it ultimatively related to cosmology? And Information Theory? Does that apply both to modern quantum mechaincs and the media? Seems like I've go a lot to learn…

14. **Adrian Heathcote**
January 3, 2006

Hi Kaspar

Yes, it looks like it's going to be a long winter over those books. Enjoy! 🙂

A.H.

15. **Jody**
January 3, 2006

Kasper said

"what is an unbounded operator? One without boundaries? (It's hard for me to imagine what this could mean). How can such an OP have domain restrictions when it is unbounded? What is a state space of OP algebras? (I've never heard about an operator being in some specific "state"). And C^* algebras? What does the C stand for – and more interestingly the * "

An unbounded linear operator on a normed vector space (e.g. Hilbert or Banach space) is a _not necessarily continuous_ linear operator that is _not necessarily_ defined on the whole space! Bounded (= continuous) linear operators are then (confusingly) a special case of unbounded linear operators! Domain restrictions are a big thorn in the analysis of unbounded operators, which is why some mathematicians feel physicists are a bit dodgy in their calculations. See the nice paper:

F. Gieres, Mathematical surprises and Dirac's formalism in quantum mechanics (quant-ph/9907069)

for a discussion of the sutleties of unbounded ops in QM.

For a more basic general discussion see the Wikipedia article:

http://en.wikipedia.org/wiki/Unbounded_operator

As for C*-algebras, see: http://en.wikipedia.org/wiki/C%2A-algebra

The * stands for the involution or "adjoint" operation on a C*-algebra (which plays the role of a generalized "conjugate transpose" from matrix theory). Physicists tend to use a "dagger" symbol for adjoints, while mathematicians use a "star" symbol. There is also the C*-identity: ||T*T|| = ||T||^2. Thus, a C*-algebra is a normed algebra, which is complete with respect to the induced norm topology, with an involution operation * that happens to satisfy the C*-identity.

A "state" on a C*-algebra A is a positive linear functional f : A -> C from A to the complex numbers C. (Thus, the "state space" of A is a subset of the dual space A* of all bounded linear functionals, but that is another use of *…) When A = B(H) is the C*-algebra of bounded linear operators on a Hilbert space H, every unit (= state) vector v gives a "state" for B(H) by the "expectation" formula:

f(T) = (T(v), v)

for T in B(H), where (v , w) denotes the inner product. By the Gelfand-Naimark-Segal (GNS) construction, every state on an abstract C*-algebra A can be (essentially) viewed in this way.

Hope this helps,
Jody

16. **Kasper Olsen**
January 3, 2006

Sorry guys – but it was obviously a joke from begining to end 😉 Of course I know these things very well indeed.

The point here is, that for many applications of quantum mechanics (for example if you are doing atomic physics) you don't need to know what an unbounded self-adjoint operator is. Or what a C^* algebra is. Or what Information Theory is.

So, it was not *unbounded ignorance* from *my* side…

17. **Lee Smolin**
January 3, 2006

HI everyone.

Carlo Rovelli did not say or imply that "Physics moved ahead too quickly in the last century, without properly understanding the mathematics of quantum mechanics." Kasper, this comment was made by Adrian. Please read what he actually said, http://www.edge.org/q2006/q06_9.html#rovelli. Rovelli's point was entirely different.

What Rovelli is complaining about is clear from his last sentence. It has nothing to do with the math, it is the extent to which string theory has become a study of cataloguing solutions to classical or at best semiclassical equations, ignoring both the background independence of general relativity and the challenges of understanding what a spacetime is within quantum mechanics. His point, with which I sadly have to agree, is that those who think of themselves as doing fundamental physics but have avoided wrestling with the implications of combining the principles of GR and QM have not appreciated the radical conceptual and technical innovations imposed on us just by those principles.

Best wishes to everyone for the new year, Lee

ps To avoid the inevitable misunderstanding, I do NOT imply here that the Einstein equations are fundamental, only that the principles of GR are.

18. **Aaron Bergman**
January 3, 2006

And, yet, Rovelli was still condescending and wrong.

19. **rof**
January 3, 2006

We must understand that Carlo has his own interpretation of quantum mechanics, the so-called relational interpretation, and his statement that others have not understood quantum mechanics simply means that others don't share his interpretation.

(Incidentally, his interpretation is that which we should say that a thing is in one state "relative to" something else, rather than saying that a thing is in a particular state. He doesn't answer the question: Can a cat be dead relative to one person and alive relative to somebody else? If the answer is yes, his interpretation is just many worlds, and if the answer is no then there's nothing relational about it – the cat is absolutely alive or dead. Consequently, his interpretation is incoherent.)

The remarks about background independence are more widely known, and have more justification, although the appeals to Leibniz's theory of space should not be made, since Leibnizian space was disproved by Kant in the 1780's.

Aaron, would you care to say which of Rovelli's statements you consider to be wrong and whether it is just your opinion or whether it's an established fact?

20. **Anonymous**
January 3, 2006

Rovelli said:

What makes me smile is that even many of todays "audacious scientific speculations" about things like extra-dimensions, multi-universes, and the likely, are not only completely unsupported experimentally, but are even always formulated within world view that, at a close look, has not yet digested quantum mechanics and relativity!

rof asks:

Aaron, would you care to say which of Rovelli's statements you consider to be wrong and whether it is just your opinion or whether it's an established fact?

I don't presume to speak for Aaron, but the quote above seems wrong to me, and it's *Rovelli* who needs to justify the opinion. A perusal of the literature on extra dimensions, for instance, will reveal plenty of both relativity and quantum mechanics. It is, as Aaron said, condenscending of Rovelli to claim that other physicists do not understand these things. They are the bread and butter of all of high-energy physics.

21. **Aaron Bergman**
January 3, 2006

Aaron, would you care to say which of Rovelli's statements you consider to be wrong and whether it is just your opinion or whether it's an established fact?

The idea that people don't understand background independence, the lessons of general relativity, yadda, yadda, yadda. People are quite capable of understanding the metaphysics and still rejecting Rovelli's favorite theory.

22. **Dumb Biologist**
January 3, 2006

Has anyone of the "bigwigs" (a scientist of stature on the order of a Weinberg, say) proposed the dangerous idea that if quantum gravity theorists can't figure out how to formulate a falsifiable theory, they should either make the ethical leap to mathematics*, where their imagination need not be fettered by external referents, or abandon quantum gravity models altogether for fields that contain problems which are testable, at least in principle?

(*This is not to denigrate mathematics, being perhaps the only worthy "philosophy", to which the greatest human minds contribute, just to acknowledge there's a difference, and that difference can be important.)

23. **Lee Smolin**
January 3, 2006

Aaron and Anonymous,

Let's lower the temperature and discuss the substance of what we disagree about. You say, "People are quite capable of understanding the metaphysics and still rejecting Rovelli's favorite theory." But, as Carlo makes clear, his favorite theories are general relativity and quantum mechanics. He claims that the principles of these are ignored by approaches to unification that study only semiclasical effects in fixed classical spacetime backgrounds.

You make it clear that you disagree, i.e. you reject the view that a quantum theory of gravity should be background independent. This is a substantial disagreement. So lets discuss why we disagree.

Certainly, no insult is intended. But it is hard to avoid the impression that some people who reject the case for background independence have not thought carefully through the issues in the interpretation of general relativity that lead us to adopt it. Perhaps this is not true of Aaron, but it is true of many who confuse the methodlogy used to interpret very symmetric solutions to GR with the very different issue of describing observables for the generic solution.

The issue is not about knowing how to write the Einstein's equations down or find simple solutions. It is about the interpretation of observables on the space of solutions. Untill one has struggled through the issues raised by the hole argument, or the problem of specifying diffeomorphism invariant observables, you cannot have absorbed the full meaning of general relativity. My impression is that once people have absorbed this it changes their taste as to what kinds of solutions to the problem of quantum gravity they are willing to entertain. Specifically it seems impossible to have understood these things and still to believe in the possible viability of any background dependent approach to quantum gravity.

Of course the equations of general relativity are used in Kaluza-Klein inspired higher dimensional unifications. But, because of these subtle interpretational issues, this is not the same thing as taking the princiiples of the theory seriously.

A related issue is that all the higher dimensional unifications require that some degrees of freedom be frozen. Otherwise, as Penrose argues in his recent book, there are instabilities that lead quickly to collapse to singularities. These are to some extent avoided by the flux compactifications but, as we know, the cost is the need to appeal to anthropic arguments.

But the point is that from a background independent point of view, any result that requires the specification of a specific solution rather than a generic prediction of the theory seems unreliable exactly because the symmetric solutions are in many ways non-generic.

Even Einstein saw this was a potential trap, when he commented, ""It is anomalous to replace the four dimensional continuum by a five dimensional one and then subsequently to tie up artificially one of those five dimensions in order to account for the fact that it does not manifest itself"

Thanks,

Lee

24. **woit**
January 3, 2006

Dumb biologist,

There seems to be a misconception that if what string theorists are doing isn't falsifiable and thus not physics, it must be mathematics. Not so. Some sorts of work in string theory involves non-trivial new mathematics that is of interest to mathematicians, and there already are some people doing this successfully working in mathematics departments. But most of the non-falsifiable stuff going on in string theory does not involve any new mathematics, and mathematicians want even less to do with this kind of thing than physicists.

25. **Dumb Biologist**
January 3, 2006

Ah, I see.

Well, that's kind of depressing…

26. **Moshe**
January 3, 2006

Lee, back to that story. I believe you are talking about string perturbation theory when you claim we expand around a classical solution, which are very symmetric, and all the rest. I would call it lack of *manifest* BI., but never mind that.
\
Since it is already 2006 we don't have to have that conversation. We have, since 1997, a background independent formulation of quantum gravity in spaces with negative cosmological constant. Yes, it is not quite as BI as you would like it to be, but as I mentioned before there are good reasons, independent of string theory, to believe that is the best one can do. Indeed, one of them is precisely the struggle to define diffeoemorphism invariant observables, which at least so far require one to have appropriate asymptotia.
\
Finally, not too get too deeply into that, but lowering the temperature seems like a good idea if one is to have a useful conversation, derogatory or misleading statements about each other's research are not helpful in that.

27. **woit**
January 3, 2006

Since I kind of started all this by posting the material from Rovelli, let me say a bit about why it struck a little bit of a chord with me and I included it. I suspect that my own ideas about this are quite different than Rovelli's, but perhaps there is some overlap.

As everyone in the field is aware, combining QM and special relativity inescapably leads us to quantum field theory. My version of Rovelli's comment that our picture of the world is too simple-minded and that we haven't fully absorbed the implications of QM and special relativity would be not that physicists don't understand the standard formalisms of these two subjects, but that the full implications of QFT have yet to be fathomed or absorbed. Starting in the late 1970s, people have found some very deep new mathematics by thinking about QFT, and I suspect there's a lot more of this to be uncovered, with major implications both for mathematics and physics. 200 years from now, people may very well see the late 20th century as a period when we were just starting to get a clue about what QM+SR=QFT really was, with a large part of the field ignoring the very challenging issues posed by trying to get to the bottom of this, instead spending its time on ideas about multiverses and extra dimensions that turned out to be shallow and besides the point.

28. **Aaron Bergman**
January 3, 2006

You make it clear that you disagree, i.e. you reject the view that a quantum theory of gravity should be background independent.

I have made absolutely no such claim whatsoever. We've covered this ground *ad nauseum* on Cosmic Variance, so I'm not going to repeat myself here.

After all of that discussion, that you still don't understand my position is inexplicable to me.

29. **Lee Smolin**
January 3, 2006

Dear Moshe,

Thanks. Two small points. You say, "there are good reasons, independent of string theory, to believe that is the best one can do." No, we know that one can do much better and get results from genuinely background independent methods. This is shown by recent results in causal dynamical triangulations and LQG in both 2+1 and 3+1 dimensions. Indeed, the recent results are based on methods which deal successfully with the problems of diffeo invariant observables (read Rovelli's papers leading up to the recent results on the graviton propagator-as that was their purpose.)

As for AdS/CFT, let me insist again that except for special cases where BPS symmetry is used, the evidence supports a weak form of the conjecture-which is the one in WItten's 97 paper. This conjecture gives a map between CFT's on the boundary and classical or semiclassial field threories on fixed backgrounds that are asymptotically AdS. Most of the very impressive results in this subject support this weaker form of the conjecture. This is far from sufficient to support the much bolder conjecture of Maldacena that there is an equivalence between a string theory in the bulk and the boundary CFT. Given that there is not even a complete formulation of free string propagation on AdS5 X S5 one cannot claim that there is strong evidence for Maldacena's form of the conjecture-except again in very special cases where BPS symmetry allows constructions to be done that may not otherwise exist.

Given this, I see little support for the even stronger form of the conjecture that posits that results of the boundary CFT are equivalent to a fully background independent (except for asymptotic boundary conditions) quantum gravity theory in the bulk. There are many papers that assume that the conjecture is true and deduce consequences, but the only evidence I know of that supports a strong as opposed to a weak form of the AdS/CFT conjecture makes strong use of BPS symmetry, which means it may not be reliable outside of a tiny sector of the Hilbert space.

So I do not agree that "We have, since 1997, a background independent formulation of quantum gravity in spaces with negative cosmological constant." I cannot object if you personally believe this strong form of the conjecture, but I do object if you write as if it is an established fact.

thanks, and happy new year, Lee

30. **Lee Smolin**
January 3, 2006

Aaron, I'm sorry, but I looked on CV and you say there that: "… to say that any quantum theory of gravity must be background independent smacks of hubris….Nature is going to work how it's going to work." And, "If a nonperturbative formulation of string theory exists, we should find it. If it's background independent, that's great. … But, if the nonperturbative formulation isn't background independent, then so be it."

If these are representative then you appear to " reject the view that a quantum theory of gravity SHOULD be background independent". which is what I said.

Thanks, Lee

31. **Aaron Bergman**
January 3, 2006

Again, Lee, look at the paper of Berenstein, Maldacena and Nastase and the **795** papers the cite it.

32. **Aaron Bergman**
January 3, 2006

I reject there the idea that a quantum theory of gravity **needs** to be background independent. But, as the whole rest of that thread points out, string theory looks background independent. Nobody's rejecting the "case for background independence". You continually set up this straw man, and it's tiresome.

33. **Moshe**
January 3, 2006

Lee, Aaron reminds me there was a reason for my deja vu… Let me just say I strongly disagree with your characterization of AdS/CFT as applying just to the BPS sector, or only in the supergravity limit, this is just wrong. I also think a conjecture that faced thousands of attempts for refutation over 8 years is as established as we will ever acheive in this business, just my opinion. If you prefer we can refer to anything that was not rigorously proven as a conjecture…
\
Peter, interesting interpretation of Rovelli's remarks, I am afraid though that he refers to general and not special relativity… I have to say that this struck a chord with me as well, the thing I find most useful about string theory is that it is a quantum mechanical model which naturally incorporates general relativity at large distances. So maybe we don't fully incorporate the deep ideals of QM and GR, but at least we incorporate the theories themselves, that is a good start.

34. **Lee Smolin**
January 3, 2006

The pp waves studied by BMN are an extremal limit of the theory. While these are very impressive results they are special cases that rely on special structures and so do not prove the general conjecture.

And Aaron, please clairfy: you reject the idea that a quantum theory of gravity NEEDS to be background independent. But you say nobody rejects the case for background independence. Isn't this a contradiction? My reading of the "case" is that it consists of arguments that REQUIRE background independence.

I am sorry if this is tiresome, but you claim that "string theory looks background independent." What is meant by background indepedence is a formulation of the theory that makes no reference to any classical background geometry. Even granting the strong Maldacena conjecture, it would not do this because there is a fixed Minkowksi geometry on whcih the SYM theory is defined.

Were there in fact a backgound independent formulation of string theory it would have a Hilbert space and observable algebra whose specificaion made no reference to any classical metric. There simply is no such formulation of string theory. All the matrix models considered to be part of string thoery, including BMN and BFSS have a fixed metric in their Hamiltonian. There were a few attmepts to make a truly background independent formulation of string theory, but not enough is known about them to claim success.

Lee

35. **Aaron Bergman**
January 3, 2006

Lee, you said “As for AdS/CFT, let me insist again that except for special cases where BPS symmetry is used, the evidence supports a weak form of the conjecture”. Now, when presented with such evidence, you say “they are special cases that rely on special structures and so do not prove the general conjecture”. This strikes me as goalpost shifting.

In general, I reject metaphysics. You can make all such arguments you want, but nature is the ultimate arbiter. I don’t think we can ever ultimately learn anything about nature except through experiment.

As for the rest, pretty much everything I want to say has already been said in the CV thread, but “Even granting the strong Maldacena conjecture, it would not do this because there is a fixed Minkowksi geometry on whcih the SYM theory is defined.” is really a very strange statement. You’re complaining about background independence for a nongravitational theory?

36. **Anonymous**
January 3, 2006

You know, there is always the possibility that quantum gravity *really does* depend on asymptotics. Background independence will be nice, but we just don’t know for sure if it is a necessary feature of quantum gravity. I think Banks has argued that it cannot be a feature of quantum gravity, for instance.

Lee, is the following true from your perspective? Every known attempt to start from background independence and construct a theory of quantum gravity relies on the *assumption* of a UV fixed point for gravity. It’s true of all the examples I know, but I know of no convincing reason for that underlying assumption.

So if we have to choose a starting point, either a UV fixed point and background independence, or a lack of a UV fixed point and a lack of manifest background independence, I would go with the latter. But string theory, though lacking *manifest* background independence, certainy appears to have *at least some degree* of background independence. It would certainly be nice to clarify the exact status of background independence in string theory, but the lack of manifest background independence is no reason to scrap string theory in favor of other approaches starting from much shakier foundations.

37. **Adrian Heathcote**
January 3, 2006

Hi Peter

Since Kasper pulled my leg by asking me ”which new mathematics” for QM, can I ask you to ‘fess up and say what new maths for QFT you have in mind?

(I’ve long been curious what happened to the Glimm and Jaffe program. At a certain point I just no longer heard of it. Did it face some insurmountable problem or did people just drift on to other things? For a while there it looked like it was the bomb.)

happy new year to you

38. **Peter**
January 3, 2006

Adrian,
Some of my speculations about the kind of mathematics behind quantum gauge theory is in the paper about QFT and representation theory I wrote a few years ago that is on the arXiv.

I haven’t heard much in recent years about the kind of rigorous constructive QFT that Glimm and Jaffe were working on. Not sure whether there just are very few people working on it, or really insuperable problems. My own guess is that trying to rigorously construct very general classes of QFTs is too hard, you need more structure to make the theory tractable. If one really could better understand the relation between QFT and representation theory I was writing about, for certain specific QFTs one might be able to do much more in terms of understanding the theory rigorously.

39. **Lee Smolin**
January 3, 2006

Hi Anonymous,

Thanks, your statment about background independence and UV fixed points is interesting but does not agree with my understanding of the results. Ambjorn and Loll, who have good results which show the emergence of geometry from a background independent theory, SHOW in detail that fixed points exist, they don’t assume it. As for the LQG results, the theory is shown to be uv finite, in both the hamiltonian and path integral formulations. So there is no uv scale invariance at all.

As to the role of asymptotics: in GR, which is a background independent theory, the phase space splits into disconnected sectors, depending on the asymptotics. I see now reason the same should not be true in
the quantum theory. The evidence, for example, from 2+1, is that it is.

To Moshe, I don’t dispute there is an AdS/CFT relation, I am just trying to find out precisely which possible version is true. Don’t you agaree this is a good thing to do, given that there are different possible conjectures? I am following a basic rule of logic which is that if two conjectures explain some evidence, only the weaker of them can be considered to have support from that evidence. Especially in a subject that has been well explored, where, as you say, there have been thousands of papers over 8 years, isn’t it reasonable to believe the weakest conjecture that I need to explain the evidence in all those papers?

I can easily imagine results that would support a stronger conjecture, given the subject is well studied, their absence is also at least indicative.

But perhaps we should agree to disagree till there are fresh results,

Thanks, Lee

40. **Moshe**
January 3, 2006

Lee, I am curious about your statement about the phase space of GR having disconnected pieces, do you regard that as evidence for weaker form of BI such that asymptotics are kept fixed?
\

As for AdS/CFT, all I am saying there is lots of evidence for much stronger form of the relation than the form you presented (only BPS objects in the supergravity limit).
\
I also think that the mode of operation of physicists tend to be taking whatever seems to work as a hint and try to apply it to further interesting problems (for example in the present case gravity in deSitter space), we don't tend to stick around and sharpen and prove conjectures, maybe that is regrettable but this is the way things are.

41. **Tony Smith**
January 3, 2006

I pretty much agree with Aaron about the tiresomeness of objections to superstring theory based on lack of background independence.

If you go back to the days of N=8 supergravity, IIRC the gravity sector got the Einstein-Hilbert action from a MacDowell-Mansouri process applied to the anti-deSitter group Sp(2) = Spin(2,3), and IIRC people back then were not attacking supergravity based on lack of background independence.
IIRC, the attacks back then were based on fears that UV finiteness would not be maintained at high orders.
Was the idea back then that somehow "enough" background independence for GR would probably "emerge" through the MacDowell-Mansouri process (a la Deser-type formulations)?
Since such supergravity might be a low-energy limit of some superstring formulations, could it be that a similar "emergence" of "enough" background independence for GR might occur?

Doesn't that mean that, unless and until such an "emergence" is conclusively ruled out (which I do not think has been done or is likely to be done in the near future), it is silly to attack superstring theory on the basis of lack of background independence?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – If my recollections above (designated by IIRC) are seriously flawed, then, as Gilda Radner used to say, "Nevermind".

42. **D R Lunsford**
January 3, 2006

Lee Smolin said:

The issue is not about knowing how to write the Einstein's equations down or find simple solutions. It is about the interpretation of observables on the space of solutions.

This is absolutely not the issue! and it is flat wrong for you to annouce it as such. I think you people are not very bright – you completely miss the point about GR, which is how you end up abusing it so much.

The issue is the measurement problem without assuming an apparatus = a background. This does honor to both GR and QM. You will instantly understand if you think for 10 minutes that the entire ethos of the measurement problem is antithetical to the idea of background independence. Any attempt to go farther than this is doomed. You must either change one, or the other. Your crowd ignores the actual physical import of GR because it is easier to hide one's canoe in the metaphyical tributaries of the of "interpretation".

"Wave function of the universe" – case closed!

The only people who take both GR and QM seriously are Finkelstein, Dirac, Einstein, Pauli, and Schroedinger. The very people who get ingored now.

-drl

43. **Chris W.**
January 4, 2006

(..and the actual physical import of GR is…)

Are you saying that assuming a background is equivalent to assuming that measurement apparatuses are physical realizable, which is obviously an essential precondition for doing physics, hence the demand for background independence is self-defeating and pointless?

44. **Chris W.**
January 4, 2006

[correction: *physically* realizable]

45. **Anonymous**
January 4, 2006

Lee,

OK, it sounds like we are in agreement that asymptotics are potentially important.

You say "Ambjorn and Loll, who have good results which show the emergence of geometry from a background independent theory, SHOW in detail that fixed points exist, they don't assume it. As for the LQG results, the theory is shown to be uv finite, in both the hamiltonian and path integral formulations. So there is no uv scale invariance at all."

I'm puzzled by the Ambjorn/Loll result, but I have to admit to only having skimmed the relevant papers. It doesn't seem the lattice theory is known to have a good continuum limit, which is what I would think you mean by "SHOW" that fixed points exist. Are you saying they have definitively established such a limit? In which papers?

As for LQG: you're saying the theory is UV finite but there is no UV fixed point? How does this mesh with the nonrenormalizability of GR? What's the extra structure that fixes the infinitely many independent constants one naively expects in quantum GR?

One last question: in all of these non-string theoretic approaches, quantum gravity is proposed to be just a *quantum field theory* in the usual sense, yes? If so the claims conflict with my field-theoretic intuition, but if you can answer or give references for answers for my questions above it should help me understand exactly what you're claiming. Maybe others have the same confusion? (I, for one, thought LQG was predicated on assuming a UV fixed point of some sort.)

Thanks!

46. **Lee Smolin**
January 4, 2006

Hi Moshe,

Yes, GR satisfies a weak form of BI (background independence) in which several things are kept fixed including dimensions, topology and asymptotics. So hence does any direct quantization of GR. We might posit a deeper theory that dispenced with these background structures, which takes us beyond the strict quantization of GR. Several people in the BI quantum gravity world have advocated doing so, for example, Markopoulou and Freidel both advocated forms of spin foam models in which there is no fixed embedding space for the spin foams.

I agree also its regrettable that at least some people don't dig in and sharpen and prove conjectures. We can certainly do something about this, for example make sure that people who do so, like Berkovits, are honored and rewarded.

Hi Anonymous, for Abjorn and Loll, you can see clearly the demonstration of a fixed point in their 1+1 dimensional case, hep-th/9805108. Another way to see it is in the paper of Ansari and Markopoulou, hep-th/0505165. There are results also in 3+1 but those are numerical, whereas in 1+1 they are analytic and clean. As for LQG I tried to explain in an intuitive way for quantum field theorists the basics in my review hep-th/0408048. It evades perturbative non-renormalizability because there simply are no excitations with wavelength below the planck scale.

But there was an intuitive idea which indeed was motivational for LQG at the very beginning, which is that one expected from the ideas of asymptotic safety that a uv fixed point would require a scaling dimension of 2. This suggested that when you probe to Planck scales you should see a distribution of 1+1 dimensional excitations on a background where the metric vanished. Crane and I studied this heuristically in 84-85. This suggested initially looking at states made by finitely distributed WIlson loops, which was adopted then to the Ashtekar connection. But the actual results indicated there is just a uv cutoff rather than a scaling with reduced dimension.

However it is interesting to note that both Amborn and Loll and recent works on asymptotic safety see evidence for a reduction in scaling dimension to 2.

Thanks,

Lee

47. **rof**
January 4, 2006

DRL,

Can you elaborate a bit about GR and measurement? It is a project of mine to take GR as it is presented at the moment (namely, as an ontological theory – "There exists a manifold, with this and that tensor and these geometrical properties") and reword it so that it makes mention only of experimental results and their relationships from the points of view of observers. Have you attempted something similar?

48. **Moshe**
January 4, 2006

Lee, just to clarify my opinion, I think that proving conjectures is not a particualrly effiecient search startegy. Sure, once the correct language and issues are identified this should be done. I am glad for example that the people who developed the standard model did not stop to prove their points. Many of the ingredients going into that theory, such as confinement and the existence of chiral gauge theory, remain on shaky mathematical ground to this day. But, of course this is just a matter of judgement, though this view I think is wide-spread among theoretical physicists of all stripes.
\
And about background independence, I am a fan of the weak form, which fixes the asymptotics, at least for now. I take it as another encouraging sign that Einstein's classical gravity obeys precisely this form.

49. **Fabien Besnard**
January 4, 2006

I would like to go back to what Rovelli said in the first place. I have heard him say something similar, and he also talks about it in his book : it's the need to be conservative to open the way for a scientific revolution. What Rovelli says, and he's surely right about it, is that finding a quantum theory of gravity is not just a very difficult technical problem but a conceptual one that will involving a complete reinterpretation of our old ideas about the world. Perhaps even the words "quantum theory of gravity" are inappropriate for what will come out. But that does not mean that new concepts need to be introduced by hand before solving this problem : Rovelli also argues in his book that it had never worked that way. The only thing we can do is taking seriously the theories we have, without any wild guess about additional structures, and follow the thread until something appears that need to be understood in a new way. This is exactly what Einstein did with special relativity : he took Mawxell theory and galilean relativity seriously, and follow the thread until the Lorentz transformations followed. What's interesting is that these were already around, but no one understood them, no one took them really seriously. Einstein did because they followed logically from two well established facts : the invariance of c and the relativity of inertial motion. Thus he was compelled to take them seriously, and it led him to a new conceptual framework. Before that, wild theories about how the way aether interacted with matter had been made, but of course they couldn't get anywhere. Now suppose string theory is the right direction to follow to quantize gravity. To what conceptual revolution does it hint to ? This is not only a rhetorical question, I am frankly curious about that. As far as I can see, Maldacena conjecture if it turns out to be true seems to me the only candidate for a change in concepts, but I don't know enough to be sure about that. On the other hand, the disappearance of time in LQG does really sound like a conceptual revolution to me. But what makes the difference between a new concept and what is just a clever idea ? Can several very clever ideas lead to a new concept ? I don't know for sure, but perhaps one could say that a clever idea is an answer to a specific question (how many dimensions does spacetime has ? what is the shape of the universe ? what do particles look like at very small scales ?), even one that has never been put forward, whereas a new concept does not answer any question but tells you what questions you are allowed to ask.

50. **Juan R.**
January 6, 2006

Rovelli exactly says:

> We still haven't digested that the world is quantum mechanical, and the immense conceptual revolution needed to make sense of this basic factual discovery about nature.

That is not true and perhaps it is the basis of the typical "*we do not understand quantum mechanics*". Maybe people who claim to be doing fundamental physics would revise their knowledge of basic stuff.

As brilliantly highlighted by Bohr -a century ago- there is two visions of the world. At the one hand, the quantum vision, mainly applicable to atomic scale. At the other, the classical vision, mainly applicable to macroscopic scale. Both are complementary between them and none view can be *reduced* to the other.

Precisely, that is the reason that Schrödinger equation is totally inefficient when dealing with macroscopic phenomena, and that the old quantum measurement problem is not solved still. This is reason that Gell-Mann/Hartle 'histories formalism' [1] has not predictive power. It is the reason of great failure of the decoherence approach to solve measurement problem (far from P. W. Anderson unjustified claims [2]), this is reason that multiple-worlds Weinberg 'interpretation' sounds (see for example Penrose's criticism on why multiple-worlds is wrong in both philosophical and technical details in one of his last books [3]) to joke for people doing serious stuff (i.e. people who is doing top-research in quantum measurement), etc.

All **realistic** models of quantum measurement published in specialized literature are based in generalizations of the quantum: axiomatic generalizations of QM (e.g. Martingale models based in Lindblad semigroups that permit us *derive* Born rules), nonlinear equations violating superposition principle at large scales (e.g. 'Caldeira-Leggett' ones), LPS systems outside of Hilbert space (Austin-Brussels School Rigged-QM), etc.

Simple equations of the kind Rovelly loves (H phi = 0 or ADM or similar ones) are rough approximations valid just in certain specific situations.

Probably, the general misunderstanding about quantum mechanics is the explanation for the four decades futile effort about geometrodynamics, LQG, and so. I agree that superstring theory does rather poor our approach to quantum gravity, but LQG is not better. One can agree that both are complementary approaches. LQG does mistakes A, B, and C, whereas superstrings incorrectly deals with D, E, and F points.

Unfortunately, Rovelly (as many others LQG theorists) does not understand time and this is the reason of his last decade unfounded claims on a pure relational structure for the universe.

Of course, the superstring perturbative series around a fixed classical spacetime background is wrong and really trivial (one can use elementary R-QFT techniques); I agree with the LQG philosophy here. However, sorry to say this Lee Smolin, the principles of GR are not fundamental.

Lee Smolin said:

> Yes, GR satisfies a weak form of BI (background independence) in which several things are kept fixed including dimensions, topology and asymptotics. So hence does any direct quantization of GR. We might posit a deeper theory that dispenced with these background structures, which takes us beyond the strict quantization of GR.

I do not agree. We might posit a deeper theory explaining available (present and future) experimental data. Only that!

There are no single experimental data suggesting us that topology of spacetime may change at Planck scales even if anyone in quantum gravity community think so. Therefore, you (as Rovelli) are expressing how you want universe was…

—

[1] M. Gell-Mann and J. B. Hartle, Quantum Mechanics in the Light of Quantum Cosmology, in Complexity, Entropy, and the Physics of Information, W. Zurek, ed., Addison-Wesley, Reading (1990), p. 425; also in Proceedings of the 3rd International Symposium on Quantum Mechanics in the Light of New Technology, S. Kobayashi, H. Ezawa, Y. Murayama, and S. Nomura, eds., Physical Society of Japan (1990); Phys. Rev. D 47, 3345 (1993).

[2] http://arxiv.org/abs/quant-ph/0112095

[3] The Large, the Small and the Human Mind

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Juan R.

Center for CANONICAL |SCIENCE)

Nekrasov on String Perturbation Theory

January 3, 2006
Categories: Uncategorized

Nikita Nekrasov is giving a very interesting series of talks at the Jerusalem Winter School on the topic of "Introduction to modern covariant superstring theory." The first of his talks was yesterday and is now on-line. In it he outlined the two main formalisms for superstring theory and discussed their advantages and drawbacks, while also giving a beautiful discussion of the quantization of the superparticle, and the use of twistor and pure-spinor methods in 10d super-Yang-Mills.

One of these two formalisms, the NSR formalism, uses supersymmetry on the world-sheet, with target space a usual (bosonic) space (i.e. 10d space-time). The advantage of this is that amplitudes are computed using a linear theory, supergravity on the worldsheet. One disadvantage of this is that spacetime supersymmetry is not manifest, only recovered after GSO projection. A very serious technical problem is that, while one ultimately wants to construct amplitudes by summing over spin structures and integrating over the moduli space, the formalism gives one for each spin structure an amplitude on the super-moduli space, not the moduli space (and these super-moduli spaces are different for different spin structures). In recent years D'Hoker and Phong have been able to deal with this problem for genus 2 (and they have some results for genus 3), but for higher genus how to consistently get amplitudes on moduli space remains an open problem. Note that the problem with these multi-loop amplitudes is not only that you aren't sure they are finite, but you aren't sure that they are even well-defined. Presumably this is purely a technical problem, not evidence of an inherent inconsistency problem with such amplitudes, but one can't be sure of this until someone finds a way of resolving the problem.

The other formalism, the so-called Green-Schwarz formalism, uses a bosonic worldsheet, but takes the target space to be a supermanifold. This has the advantage of making space-time supersymmetry manifest, and avoiding the problem of integrating over super-moduli space, but it carries its own disadvantages. The world-sheet theory is now a highly non-linear, constrained theory, with both first-class and second-class constraints, constraints that Nekrasov describes as "hard to separate in a covariant way". No one knows how to quantize this theory preserving super-Poincare invariance, so one typically uses a non-covariant gauge-fixing like light-cone gauge, something that runs into trouble at genus 2 or higher.

In recent years, Berkovits has been developing an improved version of the Green-Schwarz formalism, sometimes called the Berkovits formalism, and this is the main topic of Nekrasov's lectures. Presumably Nekrasov will be discussing in his next two lectures how this works and some of the interesting problems with it, problems that he wrote a paper about a couple months ago, one which was discussed here. In his talk, Nekrasov seemed rather nervous that he would get into trouble because people might think he was raising the possibility of superstring perturbation theory being inconsistent. At one point he said that his "policy statement" was that he hoped that things could be made to work at any genus. He also seemed concerned in his talk yesterday about how his remarks might be reported today, saying:

There are really conc… well… I don't want to call them conceptual problems because these days everything is recorded. If I say something is a conceptual problem, tomorrow there will be a blog on that, or a paper. So, there are some technical difficulties….

Comments

1. **MathPhys**
   January 3, 2006

   I think that this website has helped educate more people about the most recent developments in string theory (while keeping a sober view of its problems and shortcomings) more than any other resource on the internet, excluding Ginsparg's archives.

   I think that this website is doing string theory as a discipline a great service. I doubt more than a fraction of the readers of this website read J Distler's blog (for example).

2. **woit**
   January 3, 2006

   Thanks MathPhys,

   I've certainly noticed that a sizable fraction of the traffic here comes from machines at academic sites with "string" part of the machine name, and have also heard from several string theorists that, while they disagree with my point of view, they do find a lot of interesting material here.

   Recently there have been roughly 5000 people a day looking at this weblog, a number that really astounds me.

3. **Luboš Motl**
   January 3, 2006

   Dear Peter and MathPhys,

   try to guess what error you have done that allows an intelligent reader, after going through all occurences of "MathPhys" on this blog, determine that you are the very same person. 😉

   All the best
   Luboš

4. **MathPhys**
   January 3, 2006

   Dear Lubos,

   Would you be willing to put your money where your mouth is and bet anything above US$1,000 that Peter Woit and I are the same person?

   If not, would you be willing to apologize in public for insinuating that Peter Woit lacks the integrity that puts a stunt such as you proposed below him?

   I am willing to accept any method of verification by independent observers. I do not accept any bets below $1,000 because that makes them not worth my time.

   Thank you.

5. **Luboš Motl**
   January 3, 2006

Dear Peter or MathPhys,

no, I don't intend to make a bet with you because I don't believe that you would pay me the money.

All the best
Luboš

6. **Peter**
   January 3, 2006

   Lubos,

   You really are mad as a hatter, I can't begin to guess what you have in mind here, do tell. I do know who MathPhys is, and no he's not me. From his previous comment he has less of a sense of humor about your antics than I do.

7. **MathPhys**
   January 3, 2006

   Peter, Please! I can use the money!

   Lubos, It shouldn't be too difficult to find someone that both of us accept who can administer the bet. I know at least one person right here that both of us trust. Or you can nominate anyone from your side that I know.

8. **Arun**
   January 3, 2006

   Looking forward to hearing about the technic-conceptu-al problems.

9. **WorldRen**
   January 3, 2006

   Luboš Motl
   I visited your blog before using the link woit provided. But I don't think I would visit your blog again.

10. **Peter**
    January 3, 2006

    Enough about Lubos, please! Unless he has an entertaining explanation of why he is sure I am MathPhys, I don't want to hear any more about it. Further comments should be about higher order calculations in superstring perturbation theory.

11. **Luboš Motl**
    January 4, 2006

    Dear "WorldRen",

    indeed, I would appreciate if you'll never come closer than 5 miles from my blog because I have roughly 10 times more of such WorldRens visiting the blog than what would be appropriate and their market value is heavily negative. Thanks! 😉 Incidentally, it is not hard to see that this is the first time that "WorldRen" appears in the physics blogosphere. What do you think is the conclusion?

    Peter,

    your comments about the stringy perturbative expansions are pathetic. I have explained you why it works in such a transparent way that a better than average high school student must have already understood the proof of the finiteness.

    Nikita is trying to get people's attention and attract the people into the worldsheet because it is his world. Of course he will never say that there are conceptual problems with string perturbation theory because he knows that such a statement is unjustifiable, and if he said such a thing, everyone would be asking him what he means (and he would have nothing to answer) because – as we correctly observe – everything is recorded these days.

    Best
    Luboš

12. **David**
    January 4, 2006

    Peter, it creates a strange impression when on the one hand you bemoan the current dominance of string theory in particle physics (or at least the formal theory end of it) and on the other hand use your blog to publicise the work of string theorists. I can understand it when the purpose is to point out and discuss major problems with the program (landscape etc), but that doesn't seem to be the case here. The admiring tone of your discussion of Nekraskov's "very interesting series of talks" is hardly offset by the mention of the "conc-technical difficulties" bit at the end. Are you a closet string theory fan? (Perhaps analogous to a homophobe whose homophobia is really a reaction to doubts about his own sexuality…)

    In a previous post/comment you said that you thought LQG is advancing much more than string theory at present and that research efforts should be shifted from strings to LQG. Why is it then that you write admiring posts on string theory work but have never done the same for LQG work? (I know you mentioned the talks at Loops 05 but that isn't the same.) Another area where interesting developments have occured in recent years, and which would have been natural for you to mention given you background in it, is lattice gauge theory. E.g. one of the developments in this area made the AIP's `Top stories of 2005′ (www.aip.org/pnu/2005/split/731-1.html); other developments include index theory on the lattice and have led to major progress on the very topic of your PhD work (see, e.g., hep-lat/0108009, hep-th/0407052). Is the reason why you don't mention work in these areas that the people doing it are not high-powered or illustrious enough for your liking?

13. **Alejandro Rivero**
    January 4, 2006

    Well I can think of three justifications for Lubos first comment here:
    1) A naive "social engineering" attemp to discover (for free) the identity of MathPhys

    2) A misread of an early intervention of MathPhys, where he appears as first commenter of a post titled "Hiding in the Mirror". Lubos could had missed a previous

apparition of this commenter some days before and thought that this one was the first one, then being a subtle play of Peter.

3) I really thought a third reason, but I have forgotten it while I was writting this note.

14. **hobgoblin**
January 4, 2006

The issue is about finiteness, and the arguments on one side seem to consist of statements like 'There's no evidence that it isn't finite and no reason to suppose that something magical will step in at higher orders and ruin everything,' while the argument on the other side is 'There's no proof that we have finiteness at all genera and the assumption that we do have it without further evidence is wishful thinking.'

A lot of the debate in theoretical physics seems to be like this lately. There's an awful lot of politicization and taking sides going on, and that isn't healthy for science. In fact, most of the side-taking seems to relate specifically to string theory (either 'string theory is the best achievement of the human race since speech' versus 'string theory is a massive waste of resources that are better spent elsewhere', or 'the landscape is stupid' versus 'the landscape is the best achievement of the human race since string theory').

As for Lubos, he has been diagnosed as a narcissist.

15. **WorldRen**
January 4, 2006

Peter,
In your posts, you try to publicize the most recent development in string theory and modestly criticize it at the same time. Well, the key problem with string theory is, of course, that there is no experimental evidence to prove it or disprove it yet. So string theory is special. I suggest everyone should be careful when making posts and comments in case not to mislead people. Maybe we need some humility in string theory.

Dear Luboš Motl,
You seems to be confident. But if one becomes too confident, he/she may ignore others' existence and may somewhat reluctantly trusts anything.

16. **Ben Compson**
January 4, 2006

Lubos, presumably you are going to apply for a faculty position at some point, then hope to be awarded tenure. Have you thought of the consequences of having written so many nasty comments and ad hominem attacks on this career path?

Or do you think yourself so smart that that all your online activities will be overlooked?

17. **secret milkshake**
January 4, 2006

hobgoblin: I have a problem with what you wrote:
1) such a diagnose requires some experienced psychiatrist
2) who needs to be at least as smart as the patient.
3) and who does carefull examination of the case history + interviews + tests with the patient.

The results are not likely to be posted on a blog. To the point 1) and 2): I am not sure you can find somebody qualified, Dr. Lecter has been travelling.
You better re-phrase your statement to something like this: "Lubos has been widely conjectured to be a piece of work".

Also, check out the most recent Motl, Vafa paper on arXiv; they are trying to drasticaly narrow the permisible lansdscape by introducing a new condition (by hand). If you ask me, the new hand-drawn line between the landscape and swampland on Fig.1 looks a lot like map of Czech Rep…

18. **woit**
January 4, 2006

Hi David,

You're probably right that I take too great an interest in following exactly what string theorists are up to. One justification I can give is that if I'm going to criticize them I should at least understand what they are doing, but sure, that's not sufficient.

I should try and make more clear what this blog is and what it isn't. It's my own take on what I for one reason or another take an interest in, it's not in any way an attempt to make uniform judgements about what is or isn't worth pursuing in theoretical physics. The fact that I don't take enough of an interest in something to write much about it doesn't mean it's not interesting, it just means that for one personal reason or another it's not something I've spent much time thinking about, virtually always simply due to a lack of time. There are many, many, many topics I wish I had more time to look into and think about.

About LQG: I have written some about it, but one of the main reasons I haven't written more is that it is an active, healthy subject that already has some really excellent expositors (such as Smolin and Rovelli). I encourage people to read what they have to say, and if quantum gravity is your main interest, you should be paying close attention to what is going on in LQG. But quantum gravity is not my main interest, particle physics is, so I'm mostly spending my time thinking about other things than LQG. While I think what the LQGers are doing is great and should be encouraged, I also want to draw attention to ideas closer to my own interests that are getting virtually none.

About lattice gauge theory: Again, this is an important, healthy subject, with a lot of good work going on. But it's not something I actively think much about these days. Some of my current interests grew out of first thinking a lot about spinor geometry in an attempt to find better ways to put spinors on the lattice. This is a problem I keep hoping to find time to come back to. If I ever do, I'll probably be writing more about lattice theories here.

As for my interest in string theory. First of all, while I do bemoan endlessly its current dominance, the formal theory end of it is actually the part that I think is worth paying some attention to. What seems to me really a complete waste of time are some of the phenomenological ends of the subject. Unlike "string phenomenology", the more formal, mathematical ends may very well lead somewhere, at least in terms of producing spin-offs of good new mathematics or new ideas about quantum field theory. The Nekrasov talks are a good example. If you look at his second talk, which I just watched, it's all about an interesting 2d qft, one with target space one of great geometrical significance, the space of pure spinors (=space of orthogonal complex structures). This is a nice QFT story, involving some spinor geometry that deserves to be much better known. Ignoring the string perturbation theory context, on its own it's a nice piece of work on quantum field theory. Some of the other topics Nekrasov discussed in his first lecture involve again some beautiful ideas that are independent of string theory, especially the use of twistor methods to study Yang-Mills.

One of my reasons for taking an interest in the multi-loop issue is purely idiosyncratic: I have friends and colleagues who have worked on this, and have enjoyed discussing their work with them and follwing this story over the years. Finally, I think it is also an excellent example of what's wrong with how string theory is often being pursued. Instead of honestly evaluating what is known and what isn't, and trying to push forward and learn more (as Nekrasov, D'Hoker, and Phong are doing), string theory partisans loudly make untrue claims and personally attack anyone who disagrees with them. Lubos is just the most extreme case, others like Distler aren't much better.

19. **anon**
January 4, 2006

Lubos is symptomatic of the bigotry of many theoretical physicists.

In a way you could say it is refreshing honest to see a person behaving so outrageously instead of quietly suppressing opposition.

Lubos loudly proclaims without a fig leaf of evidence that string theory must be right, and that alternatives are a waste of time.

This in my opinion is more honest than the usual "conspiracy of silence" which theoretical physicists use. Without people like Lubos, there wouldn't be much evidence of the misery caused!

20. **Michael**
January 4, 2006

"[…] if I'm going to criticize them I should at least understand what they are doing […]"

Peter, do you believe you do understand what string theorists are doing? May I quote you as saying that you believe you understand recent developments in string theory?

Thanks,
Michael

21. **secret milkshake**
January 4, 2006

since the Bogdanov affair, it is clear that that some PhD comitees and article editors do not have much clue about string theory. (At least at one minor French university and several less-known journals, they were accepting/publishing 100% balooney stringy thesis + several articles. This was done on the face value of the summary of this bogus work – because the comitees/referees were unable to understand the gibberish.)

22. **woit**
January 4, 2006

Michael,
I have no idea who you are, but from your behavior I'm quite sure that I understand a lot more about recent developments in string theory than you do. And yes, you can quote me on that.

23. **shantanu**
January 4, 2006

Peter , what about neutrino physics. It is one field in particle physics in which
there have been lots of exciting developments in the past 7 years both in
experiments (neutrino mass discovered, solar neutrino problem solved,
direct evidence for tau neutrino, etc.) IS it something which can impact rest of particle physics and what do you think?

24. **MathPhys**
January 4, 2006

Michael,

If Peter Woit had no clue what he's talking about and/or what's going on in string theory, people like you (and most notably Motl) wouldn't even bother to log onto his web site, let alone take the trouble to write comments on it.

I just don't see what your problem is with someone who says that the formal/technical aspects of string theory are great, but that the theory has made no contact with physics.

It seems like a very reasonable point of view (also made by people like 'tHooft, and in different ways by Witten) and one that any of us may wish to disagree with but can also live with.

Logging onto this site simply to write insulting comments anonymously is something that doesn't become a scientist.

25. **woit**
January 4, 2006

Shantanu,

There certainly has been a lot of progress in neutrino physics in recent years, and it looks like there will be more in years to come (MiniBoone and Numi-Minos should soon be reporting results). It's one piece of particle physics where the limitations on the energy to which we can accelerate particles is not very relevant.

But the problem is that we still have no idea where the entries in fermion mass matrices come from, neither the ones we already knew about, nor the new ones we're learning about in the neutrino sector. So what is being discovered about neutrinos is just adding a new puzzle to solve, pretty similar to one we've already failed at solving for quite a while now. Maybe there will be some hint hidden in the structure of these new numbers that gives an important clue…

26. **dan**
January 4, 2006

Peter,
Does theory predict neutrinos travel at a constant speed (presumably less than c due to mass) at all times, or can they vary, and can gravity slow down (or accelerate) their speed?

i wonder if dark matter is nothing else but slow moving neutrinos?

does string theory have anything to say about neutrinos?

27. **woit**
January 4, 2006

Dan,

I don't want to get into a discussion of this here, it's off topic. But very quickly: yes, neutrinos travel at variable speed less than c, string theory says nothing about neutrinos, and dark matter doesn't seem to be just neutrinos (their mass is too small).

28. **hobgoblin**
January 5, 2006

Dr. Milkshake,

You're right about needing a qualified psychiatrist to make a diagnosis, but you don't need a psychiatrist to see that something is abnormal. Psychiatrists have crazy people brought to them by non-psychiatrists – they don't have to prowl around the population looking for candidate crazies who others just aren't qualified to spot.

I'll have a look at the landscape paper today. Best of luck to everybody for 2006 from the goblin.

29. **David**
January 5, 2006

Hi Peter,
Thanks very much for your detailed, balanced reply to my somewhat knee-jerk comment. I understand your points. The problem I have with the original post, while sympathising with the reasons you gave in your comment, is that it adds to the impression that string theory is where the action is and that if someone wants to make an impact in particle physics then that is what they should be working on. I know this wasn't really your intention, but still, it's an impression that someone reading it could easily get.

More generally, when attempting to counter the "sociological" problem of string dominance I think it is crucial to emphasise and discuss breakthroughs and interesting developments taking place in other areas. I do understand though that there is only so much that you as an individual can do in finite time. And for what it's worth I think you have generally been doing an excellent job of pointing a critical spotlight on string theory. There was a real need for someone to step in and counter the string hype, and you have been performing this role admirably imho. It's too bad there aren't other people to help you out with this. (Sure there are other string critics but as far as I can tell no one else has been prepared to immerse themselves in the details in the way that you have done.)

Best wishes,
David

30. **Arun**
January 5, 2006

I had technical difficulties in getting to Nekrasov Lecture 2, and I wouldn't understand most of it anyway. Was there anything that throws new light on the superstring perturbation expansion?
Thanks in advance!

31. **woit**
January 5, 2006

Arun,

Nekrasov's second and third lectures were rather technical, mainly about the sigma model discussed in his recent paper. Unfortunately the video cut off at the end of his third lecture while he was about to answer questions from the audience. He describes his approach as a more geometrical version of that of Berkovits, I don't think he has actually used it to do multi-loop calculations himself. He did say that Berkovits claimed to be able to use his formalism to reproduce some of the 2-loop results of D'Hoker-Phong and even come up with some new ones. But nothing in his talk that I noticed dealt explicitly with the issue of what happens at higher order than 2-loops.

32. **mathjunkie**
January 7, 2006

Peter appeared as MathPhys to attack the stringy guys?

Outrageous Fortune

January 4, 2006
Categories: Uncategorized

There's an article in this week's Nature by Geoff Brumfiel entitled [Outrageous Fortune] about the anthropic Landscape debate. The particle physicists quoted are ones whose views are well-known: Susskind, Weinberg, Polchinski, Arkani-Hamed and Maldacena all line up in favor of the anthropic Landscape (with a caveat from Maldacena: "I really hope we have a better idea in the future"). Lisa Randall thinks accepting it is premature, that a better understanding of string theory will get rid of the Landscape, saying "You really need to explore alternatives before taking such radical leaps of faith." All in all, Brumfiel finds "… in the overlapping circles of cosmology and string theory, the concept of a landscape of universes is becoming the dominant view."

The only physicist quoted who recognizes that the Landscape is pseudo-science is David Gross. "It's impossible to disprove" he says, and notes that because we can't falsify the idea it's not science. He sees the origin of this nonsense in string theorist's inability to predict anything despite huge efforts over more than 20 years: "'People in string theory are very frustrated, as am I, by our inability to be more predictive after all these years,' he says. But that's no excuse for using such 'bizarre science', he warns. 'It is a dangerous business.'"

I continue to find it shocking that the many journalists who have been writing stories like this don't seem to be able to locate any leading particle theorist other than Gross willing to publicly say that this is just not science.

For more about this controversy, take a look at the talks by Nima Arkani-Hamed given today at the Jerusalem Winter School on the topic of "The Landscape and the LHC". The first of these was nearly an hour and a half of general anthropic landscape philosophy without any real content. It was repeatedly interrupted by challenges from a couple people in the audience, I think David Gross and Nati Seiberg. Unfortunately one couldn't really hear the questions they were asking, just Arkani-Hamed's responses. I only had time today to look at the beginning part of the second talk, which was about the idea of split supersymmetry.

Update: One of the more unusual aspects of this story is that, while much of the particle theory establishment is giving in to irrationality, Lubos Motl is here the voice of reason. I completely agree with his recent comments on this article. For some discussion of the relation of this to the Intelligent Design debate, see remarks by David Heddle and by Jonathan Witt of the Discovery Institute.

Comments

1. **JoseIRS**
   January 5, 2006

   I don't understand how string theorists don't realize that the idea of the landscape go against of the former philosophy of his theory: "standard model contains multitude of adjustable parameters, etc…". With the landscape we don't need string theory: we live in a multiverse and in each pocket universe the parameters of the standard model take different values, so we don't need to explain that particular values of our universe.

   It's a shame for people that, like me, had some hopes in string theory, to see how it is transformed in a ridiculous sci-fi story by Susskind and company.

2. **MathPhys**
   January 5, 2006

   I think David Gross is a hero.

3. **anon**
   January 5, 2006

   "I continue to find it shocking that the many journalists who have been writing stories like this don't seem to be able to locate any leading particle theorist other than Gross willing to publicly say that this is just not science."

   This is called the "tactiturn problem":

   "An old Cambridge story, concerning a person who found himself sitting next to the taciturn Prof. X (X being variously named as Dirac, Stokes ..) at dinner.

   "Person sitting next to X: "someone has bet me I won't get more than 2 words out of you tonight"

   "Prof. X: "You lose!"

   – Josephson, see http://electrogravity.blogspot.com/

4. **Dumb Biologist**
   January 5, 2006

   I just don't get it.

   During the course of a conversation at Cosmic Variance, I've learned of something like four or five testable hypotheses for extending the SM, some even involving extra dimensions with or without strings/branes, most very difficult to distinguish from one another, and it seems like in a decade or so there will be plenty of new stuff about our own universe to argue about. Stuff we've actually *seen*, because we *can*. Once we've seen what we will see, we'll build a bigger machine to pin down some of the observed "BSM" physics. It'll take time, but it looks like particle theorists have their hands very full already just preparing to interpret the LHC's discoveries.

   I just don't understand why, in a universe so full of unsolved, yet conceivably tractable problems, a scientist would not recoil from a path that leads inevitably to the unobservable. I don't understand the arguments for exploring the landscape because "it might be right" when one cannot *ever* assay causally isolated regions of the putative multiverse. What's even more bizarre is the apparent notion that String Theory can incorporate all the plausible hypothetical models of TeV physics that have been constructed in anticipation of actually doing that TeV physics. Little Higgs? Well that's just this version of String Theory with d branes on orbifolds. Low energy SUSY? String Theory predicts it. High energy SUSY? Same thing! (why not? there's a landscape, after all…) UED? My guess is since it's got a KK tower and extra dimensions, naturally it's just a facet of string theory…

   It's everything, it's everywhere, it's bigger than we can ever know. Whatever you think it is, it's strings. It can't be killed!

   Must be fun doing calculating God…

5. **David Heddle**
January 5, 2006

Very strange (maybe not) that we reach virtually the same conclusion coming from seemingly orthogonal directions.

6. **island**
January 5, 2006

Speaking of the "g" word… lol

7. **secret milkshake**
January 5, 2006

yeah, He lives… somewhere – He has created a dacha in Landscape for Himself. (Santa probably sometimes goes and visits Him there, too.)

8. **Adrian Heathcote**
January 5, 2006

"I continue to find it shocking that the many journalists who have been writing stories like this don't seem to be able to locate any leading particle theorist other than Gross willing to publicly say that this is just not science."

Journalists the world over are boosterists for the current big thing—whatever it is. They swim in the same pond as publishers who are looking for the next big pop science book. At present it looks like Susskind has been the one canny enough to create the very wind that he is volplaning in. Tomorrow it will be someone else—with an outsized ego and zero intellectual responsibility.

"The modern world is rubbish"—Blur

9. **Elmer Fudd**
January 5, 2006

JoseIRS wrote:
"With the landscape we don't need string theory: we live in a multiverse and in each pocket universe the parameters of the standard model take different values, so we don't need to explain that particular values of our universe."

I understand that some find this disappointing or irritating, but what is the objection to the possibility that this happens to be exactly the way things are?

10. **Adrian Heathcote**
January 5, 2006

EF says "what is the objection to the possibility that this happens to be exactly the way things are?"

What is it that "makes" the values of the parameters take on different values?

11. **woit**
January 6, 2006

Elmer,

The objection to this possibility is not that it is inherently impossible or inconceivable, but that there is no scientific evidence for this at all. No one has a consistent theory that makes some predictions that can be checked and also predicts this kind of multiverse. What is going on here is that string theory has failed as an idea about unification, and instead of acknowledging this, its proponents are giving up on the standard idea of how to do physics. They are turning theoretical physics into a pseudo-science whose only purpose is to come up with untestable explanations for why string theory should be believed, despite the lack of any evidence for it, and the impossibility of ever getting any evidence for it.

12. **Anonymous**
January 6, 2006

I suggest you check out Nima's talk "Possible and Impossible In Effective Field Theory (2)." He gives field-theoretic arguments (to appear in a paper soon, I gather) that certain effective field theories used in phenomenological models are sick. They have a secret inconsistency with Lorentz invariance + causality + unitarity, which is only visible when you turn on a nontrivial background (so in that sense, it is a nonperturbative problem). This together with the recent paper by Nima, Lubos, Alberto Nicolis and Cumrun Vafa seem to be very interesting directions. They're ruling out huge classes of effective field theories all at once, based on very solid general arguments. We can hope that continued study along these lines might reveal hidden inconsistencies in string vacua as well, but even if it's limited to effective field theory this is powerful stuff. I have a feeling that a fairly large class of extra-dimensional models are ruled out on similar grounds.

13. **Chris W.**
January 6, 2006

> I understand that some find this disappointing or irritating, but what is the objection to the possibility that this happens to be exactly the way things are?

Haven't we had enough of this particular line of bullshit? The reason that so many people have come to accept the strangeness of quantum mechanics, notwithstanding its challenges to our intuitions and pre-existing philosophical prejudices, is that it is supported by a vast array of *empirical evidence*, and it has been extremely difficult to find empirically viable alternatives that are any more palatable. Do you remember what "empirical evidence" means? It means confirmation by actual observations that *could easily have contradicted the theory*, but once made, were found to be consistent with it.

If you want the Multiverse to *deserve* to be taken seriously as a scientific hypothesis then you have to do a hell of a lot better than saying, "well, after all, that *could* be the way things are". You might object by pointing out that we were led in the first place to inflation, and then eternal inflation and the multiverse, by attempting to apply quantum field theory and general relativity to the whole universe. (I'm acknowledging the argument that a form of the multiverse predates the string theory 'landscape', although the landscape can be [and should be??] interpreted as an even bigger and messier multiverse.)

Well, to the extent that this has led fundamental physics into its current quandary, we ought to be questioning the theoretical (and philosophical!) assumptions that got us here, rather than just saying, "that could just be the way things are" in the total absence of genuine empirical evidence that they are that way. We could have settled for such a pseudo-solution to any number of problems in the last 350+ years. It's as much of a copout now as it ever was. It's like saying, "well, the ether could exist, and the properties of matter and electromagnetism could just happen to conspire to make its presence undetectable by any means we have been able to imagine. In fact, thanks to H. A. Lorentz and others, we have models for this. So let's keep right on creating mechanical models of the ether itself and try to understand it better."

— — — — — — — — — —
— Peter: The above is little more than a polemical elaboration on your comment, but I felt the need to post it anyway. I too have felt for quite a while that the “things could actually be that way” argument is about as worthless as assertions that astrological influences *could* be real.

14. **Adrian Heathcote**
January 6, 2006

”What is it that ‘’makes’’ the values of the parameters take on different values?”

Maybe that was putiing my point too rhetorically. So to be clearer: I doubt that there is *any possible explanation* of how, or why, the free parameters assume the values that they are meant to assume. They can’t be taken as eigenvalues of some operator—which would be the QM path—for there would be no explanation of how the operator came to act; and it would not constitute an explanation to say that they can’t be taken as the solutions to some equation—the classical way: this would not tell you how any particular universe has the properties that it does. So the landscape idea is an explanation-free idea posing as an explanation.

15. **island**
January 6, 2006

Peter: The above is little more than a polemical elaboration on your comment, but I felt the need to post it anyway. I too have felt for quite a while that the “things could actually be that way” argument is about as worthless as assertions that astrological influences could be real.

I really am ashamed to say that neo-darwinists do the same thing on a regular basis. They think that speculative theories are fair game in origins science just because they’re popularized.

‘maybe conditions are different somewhere else’

Prove it, or shut up.

16. **Juan R.**
January 6, 2006

Heroes save people (scientists) from difficulties/Monsters (string/M-theory) without thinking twice.

Heroes do not belong to the *dark side*…

Juan R.

Center for CANONICAL |SCIENCE)

17. **Urs**
January 6, 2006

If we forget for a moment string theory, the landscape speculation, our disdain for string theory or the disdain for the way some of it is being performed, then what remains is the following trivial fact.

Fact: We have *no clue* if the final theory, whatever it is, will fix the parameters of the standardmodel uniquely.

Claiming that we do know that the parameters should be unique solutions of some equation is just as much astrology as claiming the opposite.

The problem with the landscape discussion is not that it is a priori pointless to think about a theory in which the parameters of the standard model are not uniquely specified.

18. **Who**
January 6, 2006

I think I agree with Urs here:

The problem with the landscape discussion is not that it is a priori pointless to think about a theory in which the parameters of the standard model are not uniquely specified.

The validity of Urs position was demonstrated by Smolin in “Scientific Alternatives to the Anthropic Principle” which exhibits an EXAMPLE of a falsifiable theory in which SOME of the parameters are locally optimal for black hole formation.

Urs seems to be saying “it is not a priori pointless to think about” theories in which some of the parameters are variable.

In fact one can be more definite than Urs was, and say it can be entirely POINTFUL to think about such theories—because the set of such theories that are predictive and falsifiable is non-empty.

Smolin CNS is a kind of “existence proof” example showing that one can have a theory in which perhaps some of the parameters are uniquely determined but where others are variable and assume values near local maxima for black hole formation. Then if some astronomer or experimentalist ever happened to observe that one of the variable parameters of this theory is NOT near-optimal for black hole abundance, this would be evidence against the theory.

To elaborate on what Urs said: I think maybe what IS the problem with landscape discussion (Urs says what it is not) is that when one does science one has a MORAL OBLIGATION TO USE TESTABLE THEORIES because the scientific community depends for its unity on the possibility of resolving differences of opinion by empirical methods.

(rather than by rhetoric or showbiz or war or burning-at-the-stake, or all the other ways people have of arriving at consensus)

19. **Tony Smith**
January 6, 2006

Anonymous says that Nima gives “… field-theoretic arguments (to appear in a paper soon …) that certain effective field theories used in phenomenological models are sick. They have a secret inconsistency with Lorentz invariance + causality + unitarity, which is only visible when you turn on a nontrivial background … I have a feeling that a fairly large class of extra-dimensional models are ruled out on similar grounds. …”.

How “sick” do the “ruled out” effective field theory models appear to be?
Can they me “cured” by modifying them in some reasonable way, just as some unifications of gravity plus standard model were “cured” of “inconsistency” with Coleman-Mandula by using Lie superalgebras instead of ordinary Lie algebras ?

Even if “a fairly large class” of models were to be conclusively “ruled out”, wouldn’t that still leave the landscape subject to the same type of objections that are being made now, because the landscape after removal of such a “ruled out” part might still be a very large landscape,
unless
the “ruling out” process were able to “rule out” all but one unique physically realistic model ?
Is that the goal of Nima et al in that line of work?

What about, instead of starting with a landscape full of models that may or may not, as Urs says, “fix the parameters of the standard model uniquely”,
approach the situation by spending some time and effort studying models that do “”fix the parameters of the standard model uniquely”, no matter how unconventional they might appear at first glance. At least one such model (mine) can be formulated in terms of (non-supersymmetric) string theory.
Since it is not supersymmetric, would it not be included in the landscape in which Nima et al are searching?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

20. **woit**
January 6, 2006

Urs,

I’m not claiming at all that one has to believe that a final theory will fix the standard model parameters. Maybe it will, maybe it won’t, maybe it will fix some and not others. The problem with the anthropic landscape is that it not only predicts absolutely nothing whatsoever, it does not even come with a remotely plausible idea about how it will ever be able to predict anything. The best people like Susskind can do is say that if we devote thousands of physicist-years to investigating the landscape, maybe we’ll find some way to make a prediction, even though there is zero evidence so far of any such thing. As far as I can tell, the justification for doing this is pure wishful thinking and desire to avoid admitting failure. This isn’t science.

The issue of whether or not certain parameters are environmental is a red herring. Right now, we don’t know and our job as scientists is to figure out what determines them. If we’re successful at that, then we’ll know whether they are uniquely calculable or not. The real issue here is one of scientific ethics: we’re faced with the shocking phenomenon of leading figures in the field abandoning honest science. What do you do about this? Just say that maybe they’re right, and this has to be done? People like my commenter David Heddle and the Templeton foundation, who would love to mix religion and science together, think what is going on is great. I think it’s appalling.

21. **David Heddle**
January 6, 2006

Peter,

This is the second time (once on a previous post) you mischaracterize my view. You claim I think it is “great.” I do not. In terms of the landscape similarities to ID, I think it is *amusing*, not great. I don’t spell that out in my posts, but I don’t think you have Fellini to figure it out.

If I promoted ID as science, then I might find it great. But I take no pleasure, as a physicist, that some of the brightest in HEP are telling us, in effect, that physics as we know and love it is, ultimately, a fool’s errand.

22. **woit**
January 6, 2006

Anonymous,

I did look at Arkani-Hamed’s talk you mention, as well as his recent paper. It’s the same idea as Vafa’s “Let’s investigate the Swampland”, and I have the same objections. If one manages to show that some sorts of exotic brane-world scenarios can’t be embedded in our current understanding of string theory, so what? There’s zero evidence for either them or string theory, and our understanding of string theory is so primitive one can argue that when we understand it better it will accomodate even these exotic scenarios.

Amidst the swampland hype, you’re missing the main point. This has nothing to do with the problem of the landscape. The infinite number of known vacuum states of the landscape are still there and those mucking about in the swampland have nothing to say about them. You are attributing to this work some interesting possiblities that even its promoters don’t claim.

23. **woit**
January 6, 2006

David,

Sorry for mischaracterizing your views. Sounds like we’re more in agreement than I thought, although “amusing” is not quite my reaction to Susskind.

24. **island**
January 6, 2006

David Heddle said:
If I promoted ID as science, then I might find it great…

Ah, but you do, (make an unfounded leap of fath to) promote the anthropic principle as scientific evidence for the existence of god, so you must, (like me, but for different reasons), be happy to see that Lenny has admitted that these special implications do exist if there is no multiverse?

25. **David Heddle**
January 6, 2006

island,

This is a great blog and I don’t want to be a troll so I’ll clarify my position just once. In the sense that Susskind is motivated by the problem of fine-tuning I am pleased because he has provided a service. I have spent so much time arguing that there is fine-tuning that I admit Susskind has been helpful in demonstrating that it is real and requires an explanation. You have no idea how many of the Panda’s Thumb folks have argued with me that only religious wingnuts claim there is fine tuning, and that the cosmological constant is off by 120 OoM only because the calculations are naïve (or because of the units one uses). I am now sensing, thanks in part to Susskind, fewer objections along those lines. For that you can't fault Susskind—after all there really is a fine tuning problem.

But two points are obvious:

1) If Susskind is correct, then fine-tuning is an illusion
2) If Susskind is correct, it's a dark day for physics

Still, if there were a way to test the landscape, I'd be all for it.

Similarly, if black-hole natural selection is correct, then once again fine tuning is an illusion–but hey, let's do the science and see how it plays out.

I am always for doing the science. If it ever explains the fine-tuning, then I'll stop talking about cosmological ID. Strangely, the best way to preserve the fine-tuning and to preserve cosmological ID would be a fundamental theory that explains the constants. That is where I hope progress in HEP is forthcoming. If no progress is ever made, I fear we'll reach a point where undetectable multiverses are accepted as self-evident. That would be very bad.

This period we are in now–where cosmology is so metaphysical and really smart folks are ready to redefine science–well that's not good news for anyone.

26. **island**
January 6, 2006

I just want to say that I do not think that David is a troll, and I actually have a fair amount of respect for the man as a mostly honest with themselves), scientist, regardless of our departure.

You have no idea how many of the Panda's Thumb folks have argued with me that only religious wingnuts claim there is fine tuning,

Yes, I do, because I've done the same, myself, and I've made your point to this group on numerous occassions, without pulling any punches as to how I feel about their willfully ignorant, Devil's-advocate approach.

I make this point so emphatically because it is so relevant to this whole mess that it isn't even funny!

This attitude goes well beyond neodarwinians… and it is killing science!

27. **Leon Hart**
January 6, 2006

It seems to me that (finally) scientists, string theorists in this case, are reaching the limits imposed by Godel's theorem (the Godel Wall). One can get arbitrarily close to the wall, but it takes an infinite amount of time to actually touch it. The closer you get, the larger the number of Lanscapes you see. I find this debate extremely interesting indeed.

28. **Hail Multiverse full of Grace**
January 6, 2006

Main entry: **faith**
Pronunciation: 'fAth
Function: noun
(1) : firm belief in something for which there is no proof

29. **Tony Smith**
January 6, 2006

With respect to a "final theory" (perhaps maybe it should be called "next model" because there might be further and even better ones), Peter said:
"… Maybe it will, maybe it won't … fix the standard model parameters …
… our job as scientists is to figure out what determines them …".

How should scientists figure that out?

As to any parameters that might be unfixed by a "next model", maybe the anthropic/landscape approach is the only way to go.

As to parameters that might possibly be fixed, what methods might be used to fix their values? Here are some possibilities that come to my mind (not necessarily in order of how useful they might or might not be):

1 – Renormalization techniques – For example, Alvarez-Gaume, Polchinski, and Wise (Nuclear Physics B221 (1983) 495-523) said "… Weak interaction breakdown occurs for top-quark masses between 100 and 195 GeV … The renormalization group equation … tends to attract the top quark towards a fixed point of about 125 GeV …".

2 – Volumes and measures related to symmetry groups – For example, Wyler's calculation (see his IAS papers at http://www.valdostamuseum.org/hamsmith/WylerIAS.pdf) of the fine structure constant.

3 – Combinatorial methods – For example, in their book Combinatorial Physics (World Scientific 1995), Bastin and Kilmister offer an extension of Eddington-type calculations.

4 – Chaotic structures of quantum fields – For example, the work of Christian Beck in hep-th/0207081and other publications.

5 – P-adic structures – For example, the work of Matti Pitkanen.

6 – Algebraic structure of quantum field theory – For example, the work of Connes and Krein in hep-th/0201157.

7 – Phenomenological structures in string theory – For example, the work of Lust in hep-th/0401156 (since this is a part of string theory and is well-funded, there are many other examples of this).

8 – Clifford algebra structures – For example, the works of Traying and Baylis in hep-th/0103137 and of Chisolm and Farwell in J. Phys. A: Math. Gen. 20 (1987) 6561-6580.

I am not saying that the above list is exhaustive, nor that all the examples are the best that might have been chosen, and I am not saying that any or all of the listed methods may or may not be useful (in fact, I use more than one of the listed methods in my model, which does fix the parameters of the standard model).

I am saying that it would be a good idea if some funding and support were available for work on all such methods (not just the "popular" or "superstringy" ones) and that none of them should be banned, or people who work on them blacklisted, just because some influential people don't like them.
If any of them produce calculations, then comparison with experiment can determine how useful they are.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

30. **Chris W.**
January 6, 2006

Kind of off-topic, and kind of not: Sharon Begley of the WSJ has this to say about recent experiments in nano- and mesoscale quantum mechanics:

> But I confess that interests me less than what producing cat states and entangled particles says about the world. In an essay at the Web site Edge.org, astrophysicist Piet Hut of the Institute for Advanced Study muses that quantum advances are making conventional understanding about what exists and what is real start "to melt away." With "avant-garde insights" such as entanglement, he writes, the next scientific revolution "could be a dissolution of the strict distinction between reality and fiction."
>
> To which Dr. [Dietrich] Liebfried* adds, "I'm with Niels Bohr [a founder of quantum physics]. If you're not outraged by the implications of quantum physics, you don't understand it."

(* of NIST)

31. **Urs**
January 7, 2006

I feel like listing more trivial facts.

Fact: If you don't fully understand your theory yet, it is speculative to talk about its space of solutions.

Fact: Interesting speculations tend to play an important role in the development of physics (like conjectures do in mathematics).

Fact: Debate over speculations tends to be fruitless unless concrete substantial questions can be addressed with non-philosophic means.

Once we agree on these fact we can move on with the really interesting questions. (That was my optimistic statement for today.)

32. **Who**
January 7, 2006

David Heddle 6 January 1:18P
Similarly, if black-hole natural selection is correct,…–but hey, let's do the science and see how it plays out.

the science IS being done whenever the mass of a neutron star is measured.

the theory is empirically testable and it is being tested—-although more could be done by way of working out detail and deriving other predictions (other means of testing the theory).

Urs 7 January 8:57A

Fact: If you don't fully understand your theory yet, it is speculative to talk about its space of solutions.

I don't think this is right. A famous counterexample is Darwin's theory of evolution by natural selection. In the mid-1800s, genes were not known—the mechanism of heredity which allowed for small variations in offspring was not understood—but one could still test the theory.

If a theory makes a prediction, then one can test the theory, and "address substantial questions" by empirical (non-philosophic) means even though the theory is not completely understood.

**…
Fact: Debate over speculations tends to be fruitless unless concrete substantial questions can be addressed with non-philosophic means.**

I believe what you say here is true and it implies that discussion of black hole natural selection is NOT speculative, since the theory can already be addressed by empirical (nonphilosophic) means.

As reminder, a core prediction of black hole natural selection is that you cannot point to any parameter of the standard model which, if it were changed slightly, would lead to a greater abundance of black holes.

33. **Juan R.**
January 7, 2006

I wonder why there is debate about nonsense (i.e. Landscape).

It remember me ancient discussions about the sex of angels…

Perhaps people would take again a course on scientific methodology:

> A scientific hypothesis is one can be scientifically tested.

The 'Landscape' is not pseudo-science as Gross admits. It is pure philosophy or best religion (since God appears in the debate of some 'stringers').

The problem of particle physics and string theorists is that do not understand the world we can see. But is is **their** problem…

I am a bit tired of all this nonsense; that Landscape here, that Landscape there; that the cosmological constant only can be undertood via antropic reasoning [arXiv:hep-th/0511037], etc.

Stop nonsense, I am a scientist!

—

Juan R.

Center for CANONICAL |SCIENCE)

34. **Who**
January 7, 2006

there's a danger that I'm being misunderstood and need to clarify. If you disagree with these please correct me:

BH natural selection has nothing to do with the "Landscape" of string vacua.

BH natural selection has nothing to do with "God" or "cosmological ID"

BH natural selection is not a "Multiverse" theory in the usual sense.

Essentially this is meant to respond to what Peter said:

woit 6 January 12:12P
The issue of whether or not certain parameters are environmental is a red herring. Right now, we don't know and our job as scientists is to figure out what determines them. If we're successful at that, then we'll know whether they are uniquely calculable or not.

BH natural selection does not assume the cosmology/standard model parameters are apriori environomental. If I understand correctly, it is an appeal to the usual principle of MEDIOCRITY.

We live in a part of the universe that is not atypical

There is a single connected universe—-all part of the same uniform process. Some parameters are allowed to vary slightly during a BH bounce. These will evolve to favor BH formation. A TYPICAL region of the universe will have values of those constants which are at or near local maxima. The **prediction** is, since we live in a typical region, **we will not be able to find any parameter which could be varied slightly so as to make BHs more abundant.**

Because this theory is simple, explanatory, and testable, I am arguing that it should be examined FIRST before imagining multiple (causally disjoint) universes, or anthropic selection effects, or the Landscape of 10^500 string vacua, or intentional manipulation by a conscious "Fine Tuner".

Given that this theory may explain simply some of the parameters in question, for Occam Razor reasons, the research priortiy should be to determine WHICH of the parameters affect BH abundance and then TEST whether their values local optima. This offers a chance to invalidate the theory, whereupon more complex and far-fetched theories could be considered. Basically I am saying that the "Landscape" discussion is out of order until this simple matter has been cleared up.

35. **Juan R.**
January 9, 2006

Who,

> BH natural selection has nothing to do with the "Landscape" of string vacua.

I would not say "nothing to do". Thecnically both are different, but both are related in a conceptual way. In some sense both deal with a multiverse.

> BH natural selection has nothing to do with "God" or "cosmological ID"

It may depend of author. I do not remember Smolin specific arguments but i believe that you are right.

> BH natural selection is not a "Multiverse" theory in the usual sense.

What do you mean by 'usual sense'? Multiverse means "more than one universe" and if i remember correctly, Smolin dealed with multiple universes, each child being more optimized than previous one for the generation of BH. If i remember correctly, Smolin even explicitely used the term multiverse in his writtings…

> We live in a part of the universe that is not atypical

What is the definition of "atypical"? If by atypical i mean far from mean values then i (personally) live in a very atypical region of universe:

– typical temp, 3 K. Vigo temp today, 288 K.

– typical dens, 10^(-25) kg m^(-3). Earth dens. 5,52 kg m^(-3)

> There is a single connected universe—-all part of the same uniform process.

Nobody know anything of that. That is pure speculation (as string landscape)

> Some parameters are allowed to vary slightly during a BH bounce. These will evolve to favor BH formation. A TYPICAL region of the universe will have values of those constants which are at or near local maxima.

Idem

> The prediction is, since we live in a typical region, we will not be able to find any parameter which could be varied slightly so as to make BHs more abundant.

That is not a prediction, no scientifc one at least!

> Because this theory is simple, explanatory, and testable, I am arguing that it should be examined FIRST before imagining multiple (causally disjoint) universes, or anthropic selection effects, or the Landscape of 10^500 string vacua, or intentional manipulation by a conscious "Fine Tuner".

I do not call simple a theory with a landscape number of **unobserved** universes in a great cosmological evolutionary family. 'That' has explained nothing of *this* world. I think that it would be ignored like the rest of nonsense you cite next.

—

Juan R.

Center for CANONICAL |SCIENCE)

36. **Who**
January 9, 2006

Juan thanks for your response. Smolin's hypothesis saves the idea of the universe as a single connected deterministic process evolving everywhere by the same laws.

This is not the usual idea of Multiverse. The usual idea involves many DISCONNECTED causally disjoint items.

The usual Multiverse is not encompassed in a a single uniform evolution.

So there is a clear difference, even if you or Smolin wishes to call his picture a "multiverse".

I think it was simply a bad choice of word on Smolin's part, to call his picture a "multiverse", because it is distinct from the usual in so many basic ways.

I do not know whether BH natural selection is correct or not—-I do not even know if the "Bigbang" event was actually a bounce, with a prior contraction (as some approaches to QG suggest). But the prior contraction should be regarded as JUST AS OBSERVABLE as the first instants of expansion because it is connected to us by a lawful progression.

In the same sense that we can infer things about the first three minutes (of expansion) using independently testable theory, we could also in principle infer things about the final stages of contraction prior to the bounce again using independently testable QG theory.

All observation (at least in cosmology) requires assuming some theoretical model, which must be independently tested, in order to interpret and only in this sense can one say that one can observe the early stages of our expansion—it is an extrapolation of causality.

Granted that **we do not yet have an independently testable model of a BH bounce!** and so I can only say that the prior stage is IN PRINCIPLE observable.
At the present time we cannot infer back because we have no model of what occurs during the bounce.

I disagree with you here:

W: The prediction is, since we live in a typical region, we will not be able to find any parameter which could be varied slightly so as to make BHs more abundant.

JR: That is not a prediction, no scientifc one at least!

It is clearly a prediction which is able to falsify the hypothesis of BH natural selection. If one can find a parameter of the standard model which could be varied in such a way as to make BHs more abundant it would shoot down BH natural selection. The hypothesis is highly exposed to falsification and it amazes me that it has survived for over 10 years without serious challenge.

Thanks again for your critique,

Who

37. **David Heddle**
January 9, 2006

Who wrote:

> If one can find a parameter of the standard model which could be varied in such a way as to make BHs more abundant it would shoot down BH natural selection.

Why? Why could you not counter that our present universe is merely good at producing black holes? Maybe there haven't been many generations. Why does it have to be *optimal*? No, I don't think your falsification test is legitimate.

38. **Who**
January 9, 2006

good objection Heddle,
what I said needs qualification with some vague words like "substantial" and there is a judgment-call involved

if a parameter were found to be within ONE PERCENT of local optimum then I personally would not discard the BH natural selection hypothesis.

but if it were discovered that a gradual change in some parameter by as much as TEN PERCENT

39. **Who**
January 9, 2006

sorry, I struck the wrong key while typing the above, so it got entered prematurely. Here's what I was trying to say: If it turned out that you could vary some parameter gradually and get a steady monotone improvement in BH abundance until the parameter was changed by some ten percent or more, then I would consider BH natural selection refuted and forget about it.

So less than ONE percent from a local maximum seems to me IMHO to be all right, and more than TEN percent from a local maximum would seem in my opinion to be fatal.

In between seems to me to be a grey area and I would listen to expert opinion arguing pro and con. It is a judgment call and I wouldn't try to make up my own mind about it.

It has to do with the primitive (partly intuitive) idea of MEDIOCRITY. I picture a typical region as having gone through enough iterations to get to within one percent of local maximum—-if this process has been going on at all.

if parameters are not within one percent, then my feeling would be that there is something wrong and maybe this process has NOT been occurring. But I would still listen to experts if they disagreed about it—maybe they know something I haven't thought of. And if it is a worse than ten percent fit, then forget it.

thanks for your critical response Heddle. I hope you see the falsification test as legitimate (as qualified) now, or can give me some more definite reasons why you think it isn't.

40. **David Heddle**
January 9, 2006

Well I can see a couple objections.

One is that falsification of BHNS should ultimately be related to an experiment, not to consistency with the standard model.

Second, I don't think one can tweak the parameters of the standard model and reliably calculate the resulting density of black holes.

41. **Who**
January 9, 2006

…falsification of BHNS should ultimately be related to an experiment, not to consistency with the standard model.

to experiment, that is, OR astronomical observation.
For that, see
http://arxiv.org/abs/hep-th/0407213
and also references therein going back to 1994

… I don't think one can tweak the parameters of the standard model and reliably calculate the resulting density of black holes.

Must take up that with Lee Smolin. See the discussion of neutron star mass in the same paper
http://arxiv.org/abs/hep-th/0407213

The paper gives several examples where a few percent change in some standard model parameter is argued to reduce BH abundance

42. **David Heddle**
January 9, 2006

Who,

Thanks for the references. Perhaps my assertion was incorrect. I'll read the papers

43. **Leon Hart**
January 10, 2006

… so the Theory of Everything will be a Web Page where anybody – the Inteligent Designers – will be able to build Toy Universes by guessing the 31 or so parameters (I bet the final number will be 42) that according to Tegmark and coworkers define the Laws of Physics. The rest of science will continue feeding technological innovation as usual to feed. Perhaps research in Extra Dimensions will even lead to spaceships capable of populating the Landscape of all the anthropically valid Universes constructed by the Inteligent Designers.

44. **Juan R.**
January 10, 2006

I am sorry to say this but Lee Smolin paper [http://arxiv.org/abs/hep-th/0407213], even ignoring several thecnical mistakes and acepting (as true) several unproved premises, is NOT a scientific paper.

In no way, the S premise our universe is optimized for production of BH is scientific one. In fact, i ask,can even Smolin rigorously prove the existence of BH?

Question: is S from Smolin?

—

Juan R.

Center for CANONICAL |SCIENCE)

45. **Who**
January 10, 2006

Juan, ***S*** is not a premise, but a statement which Smolin challenges us to show is false—-by empirical means.

S is defined on page 29 of the paper

section 6.3 (page 33) is
"How a single heavy pulsar would refute ***S*** "

section 6.4 (page 34) is
"How observations of the CMB could refute ***S*** "

section 6.5 (page 35) is
"How early star formation could refute ***S*** "

Smolin challenges us to make astronomical observations and find a neutron star above a certain mass, which will falsify the hypothesis he has offered us.

Some people believe that science is done this way, can you explain why you think this is not the case?

——————————

Peter, I want to emphasize the aspect of falsifiability to support and elaborate your quote from David Gross. You wrote:

> The only physicist quoted who recognizes that the Landscape is pseudo-science is David Gross. "It's impossible to disprove" he says, and notes that because

> we can’t falsify the idea it’s not science.

I believe you and others have pointed out that it is unethical for anyone calling himself or herself a scientist to traffic in unfalsifiable suppositions (as do the Landscapers) because the community of scientists depends in an essential way on the ability to resolve differences by empirical test. Scientists a moral obligation to propose only testable theories. David Gross is saying that the Landscapers are not behaving as morally responsible members of the community.

This theory ***S*** may very well be wrong—that is a secondary issue. I think it is being offered, in some measure, to SHOW WHAT A SCIENTIFIC THEORY WOULD LOOK LIKE as an implicit criticism of Landscapery. This supports David Gross’ objection by illustration. It shows that one can have a theory with some explanatory power as regards the values of fundamental dimensionless constants, which nevertheless is not in any sense anthropic and which is highly (one might say almost indecently) exposed to refutation.

thanks,
Who

46. **Juan R.**
January 11, 2006

Who,

Thanks by your opportunity to explain better my point.

In my opinion Smolin Natural selection view is a camouflaged version of anthropic arguments.

About the nomenclature for S, simply to state that, in logic, a premise is “*One of the propositions in a deductive argument*.”

For people who has not read cited Smolin preprint, S is defined as follow

> If p is changed from the present value in any direction in P the first significant changes in F(p) encountered must be to decrease F(p).

where P is the ‘phase’ space of dimensionless parameters of the standard models of physics and cosmology, and the ‘phase’ point characterizing our universe is denoted by p. F(p) is a fitness function which is equal to the average number of descendents of a universe with parameters p.

Yes, it is true that Smolin propose certain observations for the refutation of S and this would be an advance over the untestable anthropic reasoning of stringers, but i continue to say that *S is not a scientific premise*. In fact, i think that Smolin misunderstand the measuring of certain observables *with* the scientific testing of his premise S.

Take, as illustration, the next hypotetical J premise

> Child universes are optimized for 10^(5) value of Lambda(x)

where Lambda(x) is the value of the cosmological constant for our universe.

Then, following with this hypotetical example of a ‘theory’ of “natural cosmological evolution” we could imagine that each universe generates baby universes via a process similar to Linde chaotic inflation.

Now well, could the J premise be refuted?

Reply is negative.

If we measure the actual value for Lambda in OUR universe (order of 10^(-52)) and it is not the value J ‘predicted’, we am **not** refuting J. Imagine now a hypotetical premise J2 claiming a value of 10^(-52). Any measuring of Lambda for OUR universe does **not** verifies or refutes J2.

To your question

> Some people believe that science is done this way, can you explain why you think this is not the case?

I may say that i practice a very strict definition of science and, as many people, I consider that cosmology is not a pure scientific discipline since is based more in observation that in experiment.

I agree with George F.R. Ellis when says that the core of cosmological questions form more a metaphysical discipline that physical one.

Moreover, even if Smolin Cosmological Selection was a scientific theory, I would still wait that Smolin presents us an astrophysical BH.

Each x time all of us heard of a confirmation of a BH, but all previous candidates failed in more detailed test…

—

Juan R.

Center for CANONICAL |SCIENCE)

47. **Peter H**
January 12, 2006

In view of the possibility of alien life somewhere out there, isn’t the use of the term anthropic a bit anthropomorphic? Why not generalize it to “panthropic”?

48. **Jack Lothian**
January 12, 2006

I read this month’s Discovery & followed this story here. Very interesting BLOG. I have been hearing about BLOG’s for a while but this is the first BLOG I have visited. Really interesting – more interesting than the last 10 years of Scientific American.

It has been 25 years since I left physics (my field was statistical mechanics – very far from this debate) so I am a bit rusty but I would like to comment.

First – Who:

You say, “S is not a premise, but a statement which Smolin challenges us to show is false—-by empirical means” and then you say “Some people believe that science is done this way, can you explain why you think this is not the case?”

Proving a negative is a high bar to jump – much more difficult than proving a positive. This has the appearance of being a reasonable request but in reality it puts the onus on the refuter to disprove any claim made by the author rather than having author show that the theory suits our reality. These so-called empirical tests seem very fuzzy to me. Most of the tests build on measurements, assumptions and theories that can be challenged – thus ultimately they are false tests.

Secondly in economics they distinguish between “empirical facts” and “stylized facts”. Empirical facts are facts that can be verified in repeatable and controlled experiments while stylized facts are observed consistencies that are observed in non-repeatable and non-controlled systems. Often stylized facts are observed in a limited number of data sets. I believe what you are calling empirical facts are actually stylized facts.

Thirdly as a general comment on the issue. I am avid reader of sci-fi & in the last few years I observed a phenomena that troubles me. More & more sci-fi writers are claiming their fictional ideas are supported by current physics theories. To support these scientifically outlandish ideas they usually point to articles or talks from well-known string or landscape theorists. When the line between sci-fi & science blurs I worry about where science is going.

Lastly, for over 30 years I have been an avid reader of Scientific American and my impression is that starting in the 80s, outside of genetics, fundamental breakthroughs and discoveries have really slowed down in many sciences, especially in physics. Where there use to be 1 or 2 really interesting and thought provoking articles each months, recently 1 or 2 a year is the average. I personally don’t think the decline is due to a decline in the quality of the writers – rather it is a reflection of a decline in the quality of the science being produced.

A layman’s thoughs.

49. **woit**
January 12, 2006

Hi Jack,

Glad you like the blog. I certainly share your perception that fundamental breakthroughs in physics have slowed down (although there are some subfields like astrophysics and cosmology, which have been very active). In particle physics, the problem is that the field has been a victim of its own success. The standard model, developed by 1973-74, is just too good, and addressing some of its remaining problems requires energies that are technologically very hard to reach. As a result, things have gotten strange in particle theory…

50. **Thomas Larsson**
January 13, 2006

Jack,

It has been 25 years since I left physics (my field was statistical mechanics – very far from this debate)

If you had stayed on for another five years, the gap would have been much smaller. In 1984, conformal field theory, which counts as stringy math, was applied to 2D phase transitions, certainly a subfield of statphys. Unlike the situation in string theory, this application of CFT has been experimentally confirmed beyond reasonable doubt. But that something is relevant to 2D statphys does not mean that it is relevant to 4D gravity, or even 3D statphys.

51. **Who**
January 13, 2006

Hi Jack,
I will explain why showing S to be false is not as complicated as you might think.

for starters, in his paper Smolin explains why simply observing a neutron star above a certain mass would falsify S.
Just take a look at the paper
http://arxiv.org/hep-th/0407213
around page 33

he has worked out one way of falsifying the theory, saving us the trouble.

but more generally, look at what S actually says and you will see that it is rather straightforward to falsify it by empirical observation

——here’s part of your comment—-
You say, “S is not a premise, but a statement which Smolin challenges us to show is false—-by empirical means” ….

Proving a negative is a high bar to jump – much more difficult than proving a positive….
——–endquote———

I will explain that in THIS case proving the negative of S is NOT a high bar to jump! It is more like proving a positive.

Here is what S says:

> If p is changed from the present value in any direction in P the first significant changes in F(p) encountered must be to decrease F(p).

P is the parameter space—-if there are 31 dimensionless constants going into the standard models of particle physics and cosmology then P is just a 31 dimensional space —-a point p in P is a list of 31 numbers.

F(p) measures the abundance of black holes. To falsify the statement all you need to do is FIND ONE DIRECTION IN WHICH TO CHANGE p SUCH THAT F(p) INCREASES.

The statement says that changing p (from the measured value) in ANY direction will cause the abundance of BH to DECREASE.

To refute that you just need to find ONE direction in which a change will make it INCREASE. Just find **one uphill direction**
and you’ve done it! The theory is toast!

The changes involved have to be significant in the sense of exceeding noise or measurement uncertainties, that is discussed somewhat in the paper, but this is the broad outlines.

52. **Juan R.**
January 14, 2006

Who,

> F(p) measures the abundance of black holes. To falsify the statement all you need to do is FIND ONE DIRECTION IN WHICH TO CHANGE p SUCH THAT F(p) INCREASES.

It appears that you are misunderstanding direction in usual 3D with any direction in Smolin P space which is very, very different.

Directions, gradients, etc. in P space cannot be measured, therein Smolin premise is so not scientific as antrophic string.

Juan R.

Center for CANONICAL |SCIENCE)

53. **Who**
January 14, 2006

I thought someone might be amused by the suggestion that Smolin's parameter space is R^31. The dimensionality is taken from the yearend 2005 paper **Dimensionless constants, cosmology and other dark matters** by Tegmark, Aguirre, Rees, and Wilczek.
http://arxiv.org/astro-ph/0511774
There was some discussion of that paper here at Peter's blog including posts by Smolin, Aguirre, and Tegmark.
http://www.math.columbia.edu/~woit/wordpress/?p=310

Smolin doesn't say how many independent fundamental constants there are in the combined standard models of particle physics and cosmology—naively speaking the number of knobs on the universe-machine. But Tegmark et al give a list of 31 dimensionless numbers.

Anyway, suppose ***P*** is that space of dimensionless parameters, and suppose ***p*** is the list of parameters which we measure. Then the statement of the theory that you have to falsify—-the hypothesis—is this:

you can't find a small change in *p* that makes F(*p*) increase

(as in all such situations, there is the usual need for reasonableness and good-faith because measurement and calculation involve uncertainty, you would have to show that F(p) increases *significantly* as some parameter is varied)

The most puzzling objection to Smolin's proposal which I have seen so far is the one offered by Anthony Aguirre in posts #101 and #105 of the thread referred to above. They are easy to find because the thread has 107 comments.

Here is Aguirre's main point from #105:

> …Now, the questions of how *generic* eternal inflation is, is I think an interesting one: are there non-fine-tuned inflation models that explain the observations and are not eternal? I don't know of any decent study on this question (and may well undertake one myself). In the context of CNS, however, even the *possibility* of eternal inflation is very dangerous, however, because unless eternal inflation is *forbidden* by the 'meta' laws that govern which types of inflation are possible in the landscape of possibilities, it must only be realized once to 'take over' the ensemble by creating an infinite number of black holes out of one. The only escape from this, it would seem, would be to have other channels to create infinitely many black holes from one 'parent' — but then you would be in the same extremely thorny boat of comparing infinities…

Surprisingly, since this comes from Aguirre, I do not see that the objection is relevant. Aguirre argues that SOME black hole might accidentally have an infinite number of offspring, but he does not contend that this is TYPICAL. On the other hand, Smolin's hypothesis concerns a local maximum, assuming mediocrity. It is not affected by rare instances of "eternal inflation", should they occur. So Smolin's fitness function F(p), the number of black holes in a single generation, is typically finite. The hypothesis is that it evolves towards a local maximum and (rare instances of eternal inflation notwithstanding) will typically BE at a local maximum. But this can be CHECKED. So the challenge stands: **can you show a change in some parameter which would result in our spacetime having more black holes?**

54. **Jack Lothian**
January 14, 2006

Who

Thanks for your comments. I downloaded the paper. I will read it but I suspect it will take me a week or two to get through it.

55. **Who**
January 14, 2006

Hi Jack,
I am glad you have downloaded the paper and intend to have a look at it. I may be able to save you time by giving some specific page references.

Only a part of the paper is about Smolin's black hole natural selection idea and the opportunities to test it. We can narrow the focus some.

To recap, the paper I referred you to is:
http://arxiv.org/abs/hep-th/0407213
[b]Scientific alternatives to the anthropic principle[/b]
(Contribution to "Universe or Multiverse", ed. by Bernard Carr et. al., to be published by Cambridge University Press)

The section on cosmological natural selection begins on page 28
with the section "5.2 Natural Selection"

Section "6 Predictions of Natural Selection" begins on page 30
and runs through page 36.

So I believe that everything relevant is contained in pages 28 thru 36. I look forward to any comments you have, particularly about the testability.

The testability issue speaks to the main point made by David Gross in the remarks which Peter quoted at the beginning of this thread.

> The only physicist quoted who recognizes that the Landscape is pseudo-science is David Gross. "It's impossible to disprove" he says, and notes that because we can't falsify the idea it's not science. He sees the origin of this nonsense in string theorist's inability to predict anything despite huge efforts over more than 20 years: "'People in string theory are very frustrated, as am I, by our inability to be more predictive after all these years,' he says. But that's no excuse for using such 'bizarre science', he warns. 'It is a dangerous business.'"

"It's impossible to disprove" he says, and notes that because we can't falsify the idea it's not science."

Smolin has offered an hypothesis with some potential for explaining the values of key constants which, however, IS subject to disproof. So it IS science by David Gross standards. This testable hypothesis ought to be mentioned along with the prospect (which Weinberg and Wilczek appear willing to contemplate) of GIVING UP amid string landscape's welter of possible vacua. The hypothesis may indeed be incorrect and it may be possible to falsify it—perhaps that should be first on our list of things to do.

56. **Juan R.**
January 15, 2006

Each time the discussion becomes more interesting,

Who said,

> F(p) measures the abundance of black holes. To falsify the statement all you need to do is FIND ONE DIRECTION IN WHICH TO CHANGE p SUCH THAT F(p) INCREASES.

and now adds

Anyway, suppose P is that space of dimensionless parameters, and suppose p is the list of parameters which we measure. Then the statement of the theory that you have to falsify—-the hypothesis—is this:

you can't find a small change in p that makes F(p) increase

Therefore, I admit now that you are informed that P is not 3D space. Then the you may aware that experimental methodology would be:

1) Measure the number of BH in our universe (or region). So far i know still nobody measured one and, however, Smolin curiously talk how if we found BHs each day.

2) Take p_1 = G for our universe. Then [i]vary[/i] the value of G in our laboratory and the point p in 'phase' space changes to p'.

3) Now measure again the number of BH in this new universe

4) Repeat 1)-3) for rest of parameters from p_2 to p_31

5) If our universe is optimized for the production of BH then the S premise is correct.

May I continue with this nonsense?

—

Anthropic nonsense: universe is optimized for human life.

New 'BHolic' nonsense: universe is optimized for BHs.

—

Juan R.

Center for CANONICAL |SCIENCE)

57. **Who**
January 15, 2006

Hello Juan, you say

> Anthropic nonsense: universe is optimized for human life.
>
> New 'BHolic' nonsense: universe is optimized for BHs.

I am not sure you have correctly stated the Anthropic position (defeatists like Susskind may not actually claim "optimized for", they may merely be asserting "compatible with") human life.

The point made by Peter, in quoting David Gross at the beginning of this thread, is that whatever they are claiming is not science because it is not falsifiable. There is no way to demonstrate that the universe is NOT compatible with human life.

By contrast the BH natural selection conjecture IS FALSIFIABLE. This is the significant difference, and the main point I am stressing here.
I am waiting for you to read and comment on pages 28-36 of the paper because several ways to falsify it are discussed there.

The proposed tests of BH natural selection depend on some established physics that has been independently confirmed by experiment, such as Gen Rel, QED, QCD. I trust that you will grant that it is legitimate to use older well-established branches of physics in designing ways to test new theories.

58. **Who**
January 15, 2006

> 5) If our universe is optimized for the production of BH then the S premise is correct.

the aim in not to prove that the theory is correct, by testing for optimality in every direction

the aim is to show that the theory is INCORRECT, by finding just ONE direction in which one can vary the parameters so as to increase BH abundance.

Scientific theories are never proven to be correct because they can never be tested in infinitely many cases. In 1919 when Eddington tested Gen Rel, he did not prove that Gen Rel is correct. He made an unsuccessful effort to show that it was incorrect. If he had observed the Pleiades to be in a substantially different position this would have falsified Gen Rel.
But apparently he didn't and Gen Rel survived that test. And continues to survive. That is what scientific theories do: they CONTINUE TO SURVIVE.

And that is what Smolin's BH natural selection theory is doing today. It continues to survive astronomical testing. The theory predicts that you cannot have a two solar mass neutron star—because if you could, then the topquark mass could be revised so as to make neutron stars more apt to collapse (this is discussed in the paper). But astronomers are constantly finding neutron stars and measuring their masses. Some day they may find one which can be reliably shown to have a mass of 2 solar or more. That would invalidate the BH natural selection theory.

This is only one of what I suspect are a great many possible ways to test, and potentially falsify, the theory.

Just as people have thought of several different ways to test Gen Rel itself, so they can invent a variety of ways to test this theory, and i hope they do 🙂

Your steps 1) through 5) are essentially an argument that optimality can never be proven and that BHNS can never be shown to be correct. Right! you can't show that. This is not the issue. The point is that there are ways to show that BHNS is incorrect.

59. **Who**
January 19, 2006

Urs,
I want to make sure I understand the sense of what you said in this thread earlier
http://www.math.columbia.edu/~woit/wordpress/?p=321#comment-7237

"The problem with the landscape discussion is not that it is a priori pointless to think about a theory in which the parameters of the standard model are not uniquely specified."

I think this means that there IS a problem with the landscape discussion. But the problem is NOT that it involves considering a theory in which the standard model parameters are undetermined.

As I interpret (please correct me if I misunderstand) this rather complicated sentence it suggests that it is OK and quite scientific for a theory not to determine the fundamental constants.

Maybe this is is drifting towards a kind of "apologia" for string theory that says "hey string theory is OK and quite scientific, don't criticize it because it fails to determine the fundamental constants! Lots of other OK theories do not do that!"

In the Susskind NYT Book Review thread,
http://www.math.columbia.edu/~woit/wordpress/?p=329#comment-7668
you blurred the distinction between testable scientific theories and string thinking by arguing that having an infinite space of solutions is GENERIC, so what is especially deficient about string?

Urs 18 Jan 3:28P
"I tried to point out how a theory generically has infinitely many solutions. So it's not clear to me why it should be a problem if some theory has only finitely many."

I think the message is: why should string be considered unscientific if theories generically have an infinite space of solutions?

It helps to keep examples of alternatives in mind. General Relativity is an example of a theory with instant falsifiability. If it had happened to predict the wrong light-bending angle, checked in 1919, then it is simply wrong. If it predicted the wrong clock speeding with altitude, constantly checked by the GPS, then it is simply wrong.

As you pointed out, Gen Rel has an INFINITE SPACE OF SOLUTIONS—I believe these are associated with different possible initial conditions and the key point is SO WHAT? because that does not affect the instant falsifiability. The theory can still make predictions that refute it if they are not observed.

Maybe we are dealing with a sort of apologetics here and some of this could be smoke or red herring. What matters is the testability/falsifiability and not the "infinite solution space" or the "undetermined parameters"

A theory can have infinite solution space and undetermined parameters and still commit to decisive predictions. If string is non-committal and accomodating to any future experimental result then that is probably the problem. And it doesnt help, or is at best only a distraction, to point out that other scientific theories have undetermined parameters and infinite solution spaces.

Basically this amplifies the David Gross quote in the original post.

60. **Urs**
January 19, 2006

> General Relativity is an example of a theory with instant falsifiability.

Only once you have detectors that are sensitive enough to see the required effects.

61. **Who**
January 19, 2006

Only once you have detectors that are sensitive enough to see the required effects.

I am not sure how to interpret this, Urs. I think that when Gen Rel was published in 1915 there WERE already detectors able to see the effects.

Or, if there were none already sitting on the shelf, one could see a clear way to build them. One could say in good faith that it was reasonably practical to test—it was in actual practice VULNERABLE to empirical disproof.

I see a moral obligation for theoreticians to make their theories be vulnerable. Because the unity of the scientific community depends on the ability of members to settle their differences of opinion by empirical means.

If a theoretical construct is not vulnerable, it does not seem like proper science to me. This depends on people being reasonable and discussing in good faith. If one can see how to build an instrument within reasonable time and cost, then as long as one can see the way to do it then it does not have to be already available when one makes the theory. One wants to see that the theoreticians are at least sincerely TRYING to make their theory vulnerable to practical tests.

At this point it would help to compare string with some other QG approaches—because it involves a judgement call of what is reasonable to expect. Currently my testcase example is Laurent Freidel paper hep-th/0512113. It looks to me that if the result is extended to 4D then it is falsifiable with instruments that are planned or underconstruction. Please say if you disagree, since i would value your point of view on that very much.

the reason for considering alternatives is because one has to judge what is reasonable to expect (pure logic outside the context of alternatives does not seem to make for a successful discussion)

best wishes

62. **Urs**
January 19, 2006

> I think that when Gen Rel was published in 1915 there WERE already detectors able to see the effects.

Sure. But in 1815 there were not.

Today, entire high energy physics is suffering from the lack of good detectors. Theory is far ahead of experiment, unfortunately.

63. **anonymous**
January 19, 2006

"Theory is far ahead of experiment, unfortunately."

Indeed! I thought it was the other way round, with theory being unable to catch up with experiments. Perhaps I've missed a paper on arXiv.org that predicted the masses of quarks, coupling constants, and other Standard Model parameters. How careless of me.

Yet More Links

January 6, 2006
Categories: Uncategorized

An assortment of interesting things I've run across recently:

There's something called Multiversal Journeys that seems to organize lecture series on theoretical physics, with a special interest in the multiverse (at least to the extent of using it as an inspiration for the organization's name).

UC Davis particle physicist John Terning has a weblog. Also a new graduate-level text book on supersymmetric field theories, entitled Modern Supersymmetry: Dynamics and Duality, soon to be published by Oxford.

Ever since 2001, the physicists in Paris have been running a Seminaire Poincare, modeled after the mathematicians famous Seminaire Bourbaki. The latest Seminaire Poincare was on the topic of Quantum Decoherence, and texts from the older meetings are available.

Harvard philosopher Hilary Putnam has an updated version of his 1965 article "A Philosopher Looks at Quantum Mechanics" in the latest issue of the British Journal for the Philosophy of Science. It is entitled A Philosopher Looks at Quantum Mechanics (Again). Part of my misspent youth involved taking several philosophy courses as an undergraduate at Harvard, including ones from Quine and Putnam.

In November, Joe Lykken gave a particle physics seminar at Princeton entitled Is particle physics ready for the LHC? His talk explains some of the challenges particle physics will face at the LHC. The next-to-last slide is a none too subtle dig at the lack of any particle phenomenology going on at my alma mater. It is entitled "is Princeton ready for the LHC?", and lists the titles of the particle theory seminars going on at Princeton during the period before his talk.

The International Committe for Future Accelerators (ICFA) has a new web-site.

The Tevatron has recently achieved new luminosity records, both for peak luminosity and integrated luminosity over a week. You can follow the status of the Tevatron here.

A beautiful new paper by Greg Landweber and Megumi Harada has just appeared. It is entitled A comparison of abelian and non-abelian symplectic quotients and uses equivariant K-theory methods to get the relation between the K-theories of the symplectic quotients M//G and M//T, here T is the maximal torus of a compact Lie group G.

Update: One more. Slate today is advertising Meaning of Life TV, where various people, including some physicists, do things like promote the idea that the anthropic principle shows religion has a lot to do with science. This site has been around for a while, but just now has affiliated with Slate. Looking at it I thought "funny, this is the only thing like this trying to inject religion into science that doesn't seem to be a Templeton Foundation project." Then I saw the About Us link.

Comments

1. **Alan Reifman**
   January 6, 2006

   Also to consider for one's reading list, there's a new book on the standard model. It's entitled "The Theory of Almost Everything," written by Robert Oerter, a professor at George Mason University. Further information on the book can be accessed by clicking on Oerter's webpage:

   http://physics.gmu.edu/~roerter/

   (Info on the book is underneath where he lists his course pages.)

2. **QWERTY**
   January 6, 2006

   THAT IS A RATHER NICE PAPER AT THE END. I FIND GIT VERY DIFFICULT BECAUSE THE SURJECTIVITY ARGUMENTS ARE A LITTLE BIT SUBTLE.

   APOLOGIES FOR GOING VERY SLIGHTLY OFFTOPIC, I AM PRONE TO DO THIS OCCASIONALLY AND HOPE YOU WILL NOT THINK LESS OF ME FOR IT.

   I HEREBY DECLATE THAT I, QWERTY, SUPPORT ISRAEL GELFAND FOR THE ABEL PRIZE 2006. I WAS APPALED AND DISGUSTED WHEN PETER LAX WON IT LAST YEAR. I THINK GELFAND DESERVES IT BECAUSE HE IS VERY CLEVER.

   PS I HAVE NAMED MY PET HAMPSTER GELFAND IN HIS HONOR.

   PPS I THINK YOU SHOULD TALK MORE ABOUT MATHEMATICS AND LESS ABOUT PHYSICS

3. **MathPhys**
   January 6, 2006

   Peter,
   Why do you consider your time attending Putnam's and Quine's classes misspent? I have my own ideas about that, but I wish to hear yours.

4. **woit**
   January 6, 2006

   I took a class from Quine on mathematical logic, which mostly was him reading from ancient lecture notes (the notes were on cards from what I remember). I don't remember as much about Putnam's class, I think I was just auditing it.

   When I started college I was quite seriously interested in philosophy, especially philosophy of science and mathematics. While, outside of his class, I greatly enjoyed reading some of Quine (and to a lesser extent Putnam), and found his view of how physics fits into philosophy quite congenial, in the end it seemed a lot more interesting to deeply study math and physics themselves than to study "philosophy of" math or physics. The deepest questions about these subjects seem to me not the ones that are addressed by philosophers, but ones that our lack of sufficient knowledge of these subjects still leaves us unable to answer.

   For example, the current debates over whether the multiverse is science, falsifiability, etc. aren't truly deep problems in these subjects. They are coming up because people are refusing to honestly go after the real problems. Philosophers may be useful in getting people to think clearly and honestly, and they may contribute to these issues now.

But they can’t help much in creating the kind of new math and new physics needed to make progress on fundamental problems.

5. **secret milkshake**
January 7, 2006

Feynman: “Physicists are explorers, philosophers are tourists.”

6. **Sakura-chan**
January 7, 2006

"“A Philosopher Looks at Quantum Mechanics” in the latest issue of the British Journal for the Philosophy of Science. It is entitled A Philosopher Looks at Quantum Mechanics (Again).”

Hmmmm, I think there’s like five people in the world who have subscribed to The British Journal for the Philosophy of Science Online, and I’m sure not one of them. =D Is it worth my $$$?

7. **mgimo**
January 7, 2006

While agreeing essentially with Woit, I’d say that mathematical logic provides an interesting blend of philosophical and mathematical questions which can be studied from a mathematical point of view. As tools of mathematical logic develop, one hopes that they enable to answer philosophical questions by meaningful mathematical theorems. For example, now there are meangful (mathematical) tools to tackle the question why “why we study what we study”…

8. **logopetria**
January 7, 2006

“Philosophers may be useful in getting people to think clearly and honestly, and they may contribute to these issues now. But they can’t help much in creating the kind of new math and new physics needed to make progress on fundamental problems.”

In most periods of science, this is probably true. When we have lots of data, and multiple theories competing to explain it, with new theories and variations being spun off and explored all the time, there’s probably little that a philosopher can add to the party. But high-energy physics generally isn’t in a period like that at the moment. Not least in quantum gravity, new experimental data is relatively sparse, and progress is mostly in pure theory.

That (arguably) means that new developments will come from two sources: new mathematical techniques and ideas, and new ways of thinking about and understanding our old theories. There’s a decent chance, then, that some of the useful new ideas that will help move the field forward might come out of philosophy of physics, rather than physics alone.

Of course, philosophers can only be helpful in these areas when they have relevant knowledge of the subject in question. Fortunately, there are quite a few researchers in philosophy departments who have a very solid background in physics as well. A good cross-section of this work is given in the book “Physics Meets Philosophy at the Planck Scale” edited by Craig Callender and Nick Huggett (many articles from which can be found online: click on “logopetria” above).

9. **woit**
January 7, 2006

Sakura-chan,

I mentioned the Putnam article because I think it is historically interesting: Putnam is one of the more well-known philosophers in the US and his 1965 article was often-cited as influential. Like the old one, the new article is very clearly written, and it also has some interesting asides about Einstein’s views and those of an unidentified “world-famous physicist” (I’m guessing Feynman, may be wrong). But I don’t think the article is especially good or original in terms of getting at the real problems of the interpretation of quantum mechanics (e.g. decoherence is not even mentioned).

10. **Juan R.**
January 7, 2006

secret milkshake said,

> Feynman: “Physicists are explorers, philosophers are tourists.”

From what perspective?

Well, Feynman was a physicist, one great but only was that. Feynman philosophical insight was very superfitial. For instance his “Character of physical law” and similar works are often cited in philosophical courses as an example of superfitial approach to serious epistemological and ontological stuff.

Therefore,

Physicists are explorers, philosophers are tourists in the physical land

AND

Philosophers are explorers, physicists are tourists in the philosophical land

“A Philosopher Looks at Quantum Mechanics” again 🙂 has no real interest for science. In fact, one can easily see many ideas about scientific hot topics launched by philosophers are completely wrong. For instance, one can read all nonsense published by philosophers about quantum measurement, irreversibility, absence of time in last decades.

I agree with Gell-Mann that philosophers would do just philosophy and leave physics (and probably epistemology) for physicists…

—

Juan R.

Center for CANONICAL |SCIENCE)

11. **Chris Oakley**

January 7, 2006

From the Slate web site:

John Polkinghorne: *"The laws of nature were just exactly finely tuned to allow [life] to happen. It couldn't happen in just any old world. That's a very striking fact about the world …"*

Statistical inferences based on a sample of one.
Wear a "D" cap and stand in the corner.

12. **Attila Smith**
January 8, 2006

Dear Qwerty,
Thanks for your capital post.
Although some party-spoilers might find your opinions controversial,
at least they are not smug and boringly PC , like ***'s or ****'s or above all *****' s.
Greetings to Gelfand (your hamster, needless to say).
Abelian greetings,
Attila.

13. **fooltomery**
January 8, 2006

The Templeton Foundation never stops trying to buy a stairway to heaven…

14. **Adrian H.**
January 8, 2006

Well, I can't resist a moderate defence of philosophers.

It is true that some philosophers of science have done little more than add noise to the debates, but some others have also contributed very usefully. It was philosophers who were the primary sceptics about the Copenhagen interpretation through the 40s and 50s. It was philosophers who were the main enthusiasts for the quantum logic approach of Birkoff and von N. and it was they who were the primary sceptics when it looked like it wouldn't work. And now that physicists are rushing as one (the herd instinct seems quite well developed!) to think of decoherence as the solution to the measurement problem it is philosophers who are trying to point out to them that it just won't work.

Physicists are often condescending about philosophers but when they attempt to to do philosophy themselves (because they think they can do it with their eyes closed, with no training at all) it is often embarassingly sophomoric. Witness Susskind, Weinberg, etc.

When philosophy of science is done well it is done by people who know the maths, know the physics, and look at the theorems as external scrutineers. They are trying to answer the question "Yes, but what does it all mean?" in a more sceptical and frank way than physicits often manage unaided. Philosophy is (as some politician once said) there to keep the bastards honest. And in quantum mechanics it has been remarkably effective, particularly over the last thirty years.

I wouldn't expect much from Quine or Putnam. Putnam I think has never kept up with the literature. He listens to his friends, puts together his ideas from conversations with the *people who matter*. His ideas veer all over the map because he thinks quickly but no longer particularly well. Quine had the same arrogant attitude—but gave the impression of having even less contact with the outside world.

I should add that I think Peter W. is an exception here: his philosophical instincts seem judicious and right, to this writer anyway. Maybe those few courses he audited had a salutary effect. 🙂

Philosophers, it is true, have largely ignored string theory. But that also is to their credit. I think they can tell vapourware when they see it.

15. **j**
January 9, 2006

As a student looking to grad school, the contentious/complementary relationship between modern science and philosophy is both fascinating and troublesome. To pursue a more humanisitic approach to physical science or a more rigorous bend in philosophy? I'm pleased to see the topic arising here (if only tangentially) and that Prof. Woit faced similar considerations as an undergrad.

Out of curiousity, can anyone cite examples of philosophers [of science/math] that appear to "get it"? Is anyone familiar with the work being done by Hans Halvorson at Princeton?

16. **Adrian H.**
January 9, 2006

Yes, I'm familiar with Halvorsen's stuff. And he's good. Very good. So also is Jeff Bub at Maryland. N.P. Landsman is a very philosophically astute mathematical physicist in Holland. Michael Dickson of Sth Carolina is good. Guido Bacciagaluppi of Paris is good (they seem to have quite a few good people in Paris—go figure?) John Earman of Pittsburgh is also good—and interested in quantum field theory.

But I don't agree that there is a "contentious/complementary relationship between modern science and philosophy"—mostly the relationship (at least from the philosophers' side) is good. Philosophers who b*tch about science are an ignored minority, mostly social constructivists of one kind or another.

17. **j**
January 9, 2006

Adrian, I appreciate the list of names.
I didn't mean to imply a signficant rift between the two disciplines or any fundamental tension, but rather my recurring experience with the cliche of the specious philosopher and the myopic physicist. (The opinion(s) of Feynman and Gell-Mann cited above being superficial, if not outdated examples.)

18. **Juan R.**
January 9, 2006

Echeverría and Rábade Romeo in general theory of knowledge, Kleene in metamathematics.

Personally, i am very influenced by Prigogine (not by thecnical details of his theory); Scerri in philosophy of chemistry…

Etc.

I do not know Jeff Bub work but i do not agree with him in [http://carnap.umd.edu/philphysics/bub_research.html]. In fact, he appears to have a serious misunderstanding of basic issues of both quantum and classical mechanics.

I do not agree with some Landsman's work, specially his 'wavefuntions of universe' in quantum gravity. He has not adressed the problems of WdW and ADM quantum gravity in detail.

I think history of philosophy is the history of continuous loosing of fields of aplication in favor of rival: science. In some sense, current philosophy is the study of those fields cannot be *still* studied in a scientific manner.

—

Juan R.

Center for CANONICAL |SCIENCE)

19. **Adrian Heathcote**
January 9, 2006

Hi j—just a little more information. Halvorson at Princeton seems to have the aim of reviving a Bohrian philosophy of QM, partly tied to the idea that QM is "really" about information. A number of people are riding this horse at present, including Jeff Bub. Personally I'm pretty unconvinced, and I think many people share that scepticism. But Halvorson seems to be exerting a strong influence at present.

From a different direction N.P. Landsman also seems to be looking to resuscitate Bohr. I think his 100 page essay Between Classical and Quantum is one of the best surveys around at present. Look for it on arXiv for 2005 as quant-ph/0506082.

There are many other good philosophers around that I didn't mention: Craig Callender at UCSD, van Fraasen at Princeton, harvey Brown at Oxford, David Albert at Columbia(!) and many more.

On Feynman: apparantly he had a run-in with the neo-Wittgensteinians at Cornell in the 50s and couldn't stand philosophers ever after.

20. **Juan R.**
January 10, 2006

I do not think that N.P. Landsman seems to be looking to resuscitate Bohr in his recent quant-ph/0506082.

In fact, i think that he is ignoring the main premise of Bohr the classical is not reducible to the quantum.

Moreover his analysis of the situation is not rigorous (there are several sound technical errors in the h-limit, N-limit, decoherence, and histories sections) and he just forget the most realistic and fascinating option in his 'review'; the third way: that both quantum and classical are two sides of a more complex nature.

Precisely this third way is taken by Martingale models, Penrose, Prigogine theory, etc.

I myself have done some recent advance in the topic. In brief, i will publish a **derivation** of Martingale models and Born rules from first principles. Prigogine theory for example appears like a special case of a more general model. No other model (e.g. the popular decoherence) has done similar things.

—

Juan R.

Center for CANONICAL |SCIENCE)

21. **Levi**
January 10, 2006

Since the title of this post is "Yet More Links", I hope this isn't considered off topic. There is an interesting new paper out today by Connes and Marcolli entitled "A walk in the noncommutative garden". It's a long survey paper consisting mostly of an exploration of examples. Some of the examples are from physics, including string theory. The final section sketches the function field approach to the Riemann Hypothesis that Peter is fond of. The link is

http://arXiv.org/abs/math/0601054

22. **Adrian H.**
January 12, 2006

Juan

"I do not think that N.P. Landsman seems to be looking to resuscitate Bohr in his recent quant-ph/0506082.

In fact, i think that he is ignoring the main premise of Bohr the classical is not reducible to the quantum."

In his use of Raggio's therem on p. 20–1, he is certainly defending Bohr's response to Einstein, and is using the idea that the classical is fundamentally different from the quantum realm (it has a commutative C^* algebra rather than a noncommutative one). However this is all much more explicit in his other 2005 article on arXiv (quant-ph/0507220) and there the resuscitation of Bohr is the central aim of the paper rather than a side issue. Again Raggio's theorem is the primary technical result.

23. **Tung**
January 17, 2006

Hi peter, it seems that the message i post just now cannot be seen after i click the submit button

Follow-Ups

January 8, 2006
Categories: Uncategorized

Follow-Ups to two recent postings:

Michael Duff has written a letter to New Scientist complaining about their recent editorial Physics' greatest endeavour is grinding to a halt. Duff begins by claiming:

History has shown that rapid confirmation by experiment is a poor guide to the eventual value of a physical theory

but backs this up in a rather bizarre way. You might think he would list some physical theories whose experimental confirmation took awhile, instead he lists various theoretical ideas that have been around for a long time, still haven't been experimentally confirmed, although lots of people are still working on them. Evidently for Duff the value of a physical theory is how many people are working on it (he also points out that about 500 people go to Strings 200X), not whether there is any experimental evidence for it. The examples he gives range from cases where there is zero experimental evidence, and probably never will be any (extra dimensions, supersymmetry) to ones that it is very plausible we will soon see evidence of (Higgs boson, gravitational waves) to ones that arguably we already have some evidence for (cosmological constant).

He notes that gravitational waves were predicted in 1916 and have yet to be confirmed, that string theory is more ambitious than GR so it should take longer to confirm, and that one should only really start counting in 1995, when M-theory came along. So I guess his prediction is that by 2074, we still won't yet be anywhere near confirming string theory. Like many string theorists, he make highly tendentious claims about the relation of the standard model to experiment, writing:

decades required to knock the standard model into a shape that could be confirmed by experiment

I assume he's not talking about the QCD part of the standard model, which was born in 1973, already making verifiable predictions, and within ten years had accumulated a huge amount of evidence in its favor. The electroweak theory was first written down by Weinberg and Salam in 1967, and by ten years later the evidence for it was overwhelming. I suppose you could try and argue that the history of attempts to put together the standard model go back to Glashow in 1960 or Yang-Mills in 1954. Even using 1954, it was 19 years later that the full standard model was in place with a lot of experimental evidence already there and more pouring in. And that period would quite likely have been shorter if most of the theory community hadn't given up on QFT and been working on the bootstrap, dual models or string theory during that time. In the case of string theory, taking Veneziano in 1968 as a starting point, nearly 4 decades later there is not a glimmer of a piece of experimental evidence for string theory. Comparing the history of the standard model to the history of string theory is just absurd.

On another recent topic, the New York Times finally today carried an obituary for Raoul Bott.

Comments

1. **Wolfgang**
   January 8, 2006

   Peter,

   the example of gravitational waves is not so bad in my opinion.
   If experiments are difficult for classical gravitation it is no surprise that
   quantum gravity is even more difficult.

   But I would make a different point: We may already have some evidence for new physics, e.g. the Pioneer anomaly or perhaps in a few years deviations from Newton's potential at small distances.
   The problem is that string theory currently makes no concrete statements about such effects.

2. **JK**
   January 8, 2006

   The slow down in orbital period of binary pulsars constitutes good evidence for gravitational waves (Taylor et al. 1978). I would put gravitational waves in the same category as the cosmological constant, "arguably we already have some evidence".

3. **woit**
   January 8, 2006

   Wolfgang,

   Yes, the problem is that after almost 40 years, string theory predicts nothing. This is very different than the case of GR, where precise predictions about gravitational waves were there from the beginning (and, as JK points out, we already have indirect evidence).

   Sure, true quantum gravitational effects may always be beyond the reach of observation, but string theory is supposed to include all other interactions too. The fact that it can't actually predict a single thing about those after almost 40 years is what conclusively shows it is on the wrong track.

4. **dan**
   January 8, 2006

   "Yes, the problem is that after almost 40 years, string theory predicts nothing. "

   String theory does predict SUSY, which may turn up at LHC ca 2010

5. **A.J.**
   January 8, 2006

   String theory does predict SUSY, which may turn up at LHC ca 2010

   It's probably more accurate to say that string theory _assumes_ or perhaps _requires_ supersymmetry. It's a necessary part of the theory rather than a consequence of it.

   Now with that said, the discovery of supersymmetry among the elementary particles would constitute indirect evidence that our universe can be described by a string theory. Coupling a supersymmetric gauge theory to gravity more or less requires us to consider supergravity, and supergravity theories basically all arise as low energy limits of string theories.

The reasoning above isn't iron-clad, so take it with a grain of salt. For one thing, it might well be that susy appears in particle physics, but only as an approximate symmetry, indicating further substructure. (Something like this actually happens in nuclear physics.)

6. **Peter**
January 8, 2006

Dan,

The problem is that supersymmetry is broken and string theory doesn't tell you how or even at what energy scale. If it's broken at very high scale (which landscapeologists seem to prefer), it's indistinguishable from a non-supersymmetric theory at any energy scale we can ever hope to access, and you certainly won't see anything at the LHC. I don't see many string theorists these days willing to stand behind the idea that string theory implies supersymmetry at the LHC scale.

7. **Anonymous**
January 9, 2006

A.J. says:

It's probably more accurate to say that string theory _assumes_ or perhaps _requires_ supersymmetry.

To what extent is this clear? The type 0 theories, for instance, have worldsheet SUSY but not spacetime SUSY. True, they have a tachyon, but in at least some cases it's pretty clear that the tachyon condenses and doesn't really cause problems. Example: whatever the dual of pure Yang-Mills theory is 😉

Of course, these things probably can't describe a realistic universe with gravity and all of the other fields we know about, but I'm not sure there's an airtight case that realistic string theory backgrounds require SUSY….

8. **Fabien Besnard**
January 9, 2006

Peter, I think you would not complain against string theorists if they were a small community and made more humble claims about what their theory has achieved so far and about how promising it is. Duff is right when he says that "rapid experimental confirmation" is not a good guide : what is a good guide is the own prejudice of gifted researchers about the way nature works. Well it's a good guide only 1 percent of the time so what we need is a diversity of ideas. The academic institution should encourage such a diversity as long as no theory has won the competition… and as long as no theory destroys the rules of the competition, that is scientific method.
I develop a little bit more here .

9. **Chris Oakley**
January 9, 2006

Mike Duff is just digging his own (and his fellow Superstringers') grave by making these comments. To adapt the kamikaze motto: The requirement to understand the physical world is as heavy as a mountain, but individual theories as light as a feather.

If you had invested in a company that failed to make a profit after 40 years of trading, would you put up with being told by one of the directors that all of this is your fault for not being patient enough? I think not.

10. **Fabien Besnard**
January 9, 2006

"If you had invested in a company that failed to make a profit after 40 years of trading, would you put up with being told by one of the directors that all of this is your fault for not being patient enough? I think not. "

Do you really think science should be "managed" like a company ?

11. **anon**
January 9, 2006

Physics already is managed like a company. It is big business: compare the costs of particle accelerators to other business costs.

Outsiders passionate about science are written off automatically as amateurs. The professionals are those who make money out of it.

12. **Chris Oakley**
January 9, 2006

> Do you really think science should be "managed" like a company?

They need our money to operate, so why should they not be accountable?

There is in any case nothing wrong with operating like a company. In my experience research in companies is better managed than academic research. More emphasis on results, less on personalities, and much more willingness to give up on things that are leading nowhere.

13. **Quantafyzyx**
January 9, 2006

Wanting definite experimental predictions from a theory is so twenty-first century! Knowledge itself has a quantum behavior, like electrons and photons. Without 24th century mathematics, it is hard to explain but a rough analogy can be provided. Please remember, these are analogies only.

A definite theory with predictions that can be compared to experiment is roughly analogous to a quantum system with close-to-classical behavior (see Schrodinger's cat). Low Energy Physics is classical – there is a definite theory and definite experiments. However for physics of 1000 TeV and above, theory is in a quantum state. This analogy is rough, too, but the theory of physics is (in your language) a wavefunction over the space of string theory landscapes. Without knowledge that you all don't have right now, the best LHC can do for you is collapse the theory wavefunction onto one of two branches of the landscape – roughly those with low-energy SUSY and those without. Even a rough estimate of amplitudes is currently beyond you.

Now, knowledge of physics itself being quantum is extremely inconvenient and virtually intractable. Therefore the effort is to make it as "classical" as possible (the so-called incoherent regime where theories do not interfere with each other). Rough analogy again- there are two ways of collapsing the theory wavefunction – the first is by suitable experiment, and the second is by suitable theoretician. The cost of education, salary and retirement benefits for a theoretician is four-to-five magnitudes cheaper than any conceivable experiment, and therefore, we must produce more theoreticians. All this is dealt with in the Theory of Incoherence, though the upper bound on reduction of the landscape per theoretician is still undetermined upto a multiplicative constant. One of the important open questions, this is.

On the practical side, The Harvard Breeding Program of 2316 has been instituted to optimize production of theoreticians.

Clearly, more string theorists the more rapidly we approach the Incoherence regime, and Peter, your blog has been identified as a key blocker to production of such.

Please consider this a polite request from the 24th century to cease and desist.

14. **Chris W.**
January 9, 2006

Actually, business itself is considerably messier and hard to manage than investor newspeak* would lead one to believe, especially when technology is involved. The analogy is forced only because nobody would invest in high energy physics as a pure business proposition. If one tried to turn it into a business one would end up selling snake oil to dupes.

(* intentional reference to *1984*)

15. **dan**
January 9, 2006

I recall Lubos expecting to see SUSY @ LHC. I agree with you that if no SUSY is found, it would not falsify string theory as it now understood, but if SUSY is found, then the question is not where it breaks (obviously at TEV scale) but how. it would provide strong experimental support to string theory i think.

" I don't see many string theorists these days willing to stand behind the idea that string theory implies supersymmetry at the LHC scale."

16. **Fabien Besnard**
January 9, 2006

"I recall Lubos expecting to see SUSY @ LHC. I agree with you that if no SUSY is found, it would not falsify string theory as it now understood, but if SUSY is found, then the question is not where it breaks (obviously at TEV scale) but how. it would provide strong experimental support to string theory i think."

So it's a game where you can't lose. Cool !

If you can't kill a theory isn't it a strong indication that this theory is not alive ?

17. **Chris Oakley**
January 9, 2006

Hi Quantafyzyx,

You are probably right, but for the majority of the contributors to this blog, bringing ourselves round to your point of view would just cause too much distress.

Is there any chance of you developing and marketing brain implants that would enable us to to see the beauty of the Landscape without painful electric shock treatment?

18. **Chris W.**
January 9, 2006

The dialogue with experiment, Multiverse/Landscape edition:

"The answer is yes. What was the question?"

(This is an exaggeration, but not as much as one would hope.)

19. **AJ**
January 9, 2006

Hi Anonymous,

As you say, it's not entirely clear that string theory really requires supersymmetry for its formulation. But I think this supports my claim, that strings don't really predict SUSY, except in so far as one includes SUSY from the start.

Out of curiosity, is there really any evidence that pure YM theory has a gravity dual?

20. **Urs**
January 9, 2006

> But I think this supports my claim, that strings don't really predict SUSY, except in so far as one includes SUSY from the start.

But if you don't include SUSY from the start, you get something which seems to be incompatible with observation, namely the bosonic string. One is tempted to say that the bosonic string is ruled out by experimental data, namely by the observed existence of fermions.

So from the available string models only those with SUSY are not immediately excluded by observation. In this sense (high energy) SUSY is a prediction.

Of course it might still be that the endpoint of closed string tachyon condensation is nothing but the superstring. I suppose nobody can yet decide this one way or another.

If true, we'd have an even more robust implication of supersymmetry from the assumption of strings.

But certainly, as noted by others, worldsheet SUSY and hence SUSY of 10D flat space does not imply much about the SUSY we might hope to actually observe…

21. **anon**
January 9, 2006

SUSY is required for a hypothetical unification at an energy beyond that which can ever be measured. The 1-1 boson:fermion superpartners are no more scientific a prediction than predicting aliens, because no specific energy is predicted, there is no detailed dynamics, no numbers to be tested.

If you don't want precise calculations and numerical predictions that are testable scientifically, you find that religion can be interpreted as making many "testable predictions" about the afterlife, heaven, the day of judgement, etc.

As Lunsford would say, it's not science, it's horseshit even before you start looking at the landscape of 10^500 universes in ST.

22. **Marty Tysanner**
January 9, 2006

So from the available string models only those with SUSY are not immediately excluded by observation. In this sense (high energy) SUSY is a prediction.

I don't think I will easily understand this reasoning, unless the word "prediction" is redefined in just the right way. If I understand your comment correctly, you have a set of possibilities (the space of all string models) that can be grouped into two subsets: one depending on something that gives you the bosonic string, and another that includes SUSY. So far, there is no prediction. Then you add experimental input (i.e., that the first subset is incompatible with observation), and claim that what now remains is a prediction. A naive counting indicates that the ratio of outputs to inputs is less than or equal to one.

That doesn't look like a prediction to me.

23. **woit**
January 9, 2006

Marty,

Urs is just giving you an argument that unification inherently requires the superstring, not the bosonic string, which is a not unreasonable argument. There are lots of reasons to rule out the bosonic string. The problem is that even if you just look at the superstring, in the simplest version it has unbroken space-time supersymmetry. So it predicts exact supersymmetry, which is a very bad prediction, since we don't see that in nature. You can break this supersymmetry in various ways, but the problem is that the theory doesn't tell you what the energy scale of the supersymmetry is. Urs is being slippery when he says "(high energy) SUSY is a prediction", because "high energy" can be any thing: 1 Tev, an intermediate scale, the GUT scale, the Planck scale. For all I know maybe it can even be way above the Planck scale.

So what is bogus is saying that "SUSY is a prediction" when you know (from experiment) that it is broken, but string theory has nothing at all to say about how it is broken, not even at which scale.

24. **A.J.**
January 9, 2006

Hi Urs,

But if you don't include SUSY from the start, you get something which seems to be incompatible with observation, namely the bosonic string.

Right. Experimental results rule out the non-supersymmetric string theory. That's not quite the same thing as saying that supersymmetric string theory predicts supersymmetry.

25. **dan**
January 10, 2006

Peter,
Do you think though that with enough researchers and enough intelligence and studies thrown at the problem, that string theory could come with a mechanism of how it is broken, and at what scale? or do you think that the problem is insurmountable, even for string theorists?

So what is bogus is saying that "SUSY is a prediction" when you know (from experiment) that it is broken, but string theory has nothing at all to say about how it is broken, not even at which scale.

26. **Tony Smith**
January 10, 2006

Urs said "… if you don't include SUSY from the start, you get something which seems to be incompatible with observation, namely the bosonic string. One is tempted to say that the bosonic string is ruled out by experimental data, namely by the observed existence of fermions. …".

However, Englert, Houart, and Taormina said, in hep-th/0106235:
"… supersymmetric and nonsupersymmetric ten-dimensional fermionic closed string theories stem from the compactified closed bosonic string theory. … Space-time fermions … arise from bosonic degrees of freedom … Compactification must generate an internal group SOint(s) admitting spinor representations … the spinor representations of SOint(s) describe fermionic states … the highest available space-time dimension accommodating fermions is … s + 2 = 10 … the truncation … to SOint(8) + ghosts transfers modular invariance from the 26-dimensional bosonic string to ten-dimensional fermionic strings …
a manageable non-perturbative approach to the bosonic string is mandatory. An attempt towards formulating such an approach in a classical limit has been recently proposed … G.T. Horowitz and L. Susskind, Bosonic M Theory , hep-th/0012037 … and the present analysis suggests that such efforts should be further pursued. …".

In hep-th/0012037, Horowitz and Susskind said:
"… We … try to interpret bosonic string theory as a compactification of a 27 dimensional theory. We will refer to this theory as bosonic M theory. …".

In hep-th/0104050, Lee Smolin said:
"… "A new matrix model is described, based on the exceptional Jordan algebra, J3(O). … There are 27 matrix degrees of freedom, which under Spin(8) transform as the vector, spinor and conjugate spinor, plus three singlets, which represent the two longitudinal coordinates plus an eleventh coordinate. Supersymmetry appears to be related to triality of the representations of Spin(8).".

In hep-th/0110106, Yuhi Ohwashi said:
"… … Smolin's matrix model [is] based on the groups of type F4. … the actual world requires complex fermions without doubt. … In accordance with this complexification, the groups of type F4 are upgraded to the groups of type E6. … we consider a new matrix model based on the simply connected compact exceptional Lie group E6 …".

The result, which has much in common with my E6 model described on the CERN preprint server at http://cdsweb.cern.ch/ as EXT-2004-031, is that bosonic string theory CAN be interpreted in a way that is consistent with experimental observations.
As Smolin suggests, Spin(8) triality produces fermion-boson relationships that are a subtle form of supersymmetry in which the familiar fermions of the standard model are the subtle-supersymmetric partners of the familiar bosons.

In short, the bosonic string is NOT "ruled out by experimental data", because fermions emerge naturally in bosonic string theory.
Maybe that is what Urs had in mind by saying "… it might still be that the endpoint of closed string tachyon condensation is nothing but the superstring …" and that Urs supposes "… nobody can yet decide this one way or another …". I think that the above line of work can decide this in favor of realistic fermion emergence from bosonic string theory.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

27. **Thomas Larsson**
January 10, 2006

I recall Lubos expecting to see SUSY @ LHC.

According to himself, Lubos has bet several thousand dollars on the experimental discovery of SUSY by 2006. Unfortunately, I am not among the ones who accepted his bet, but I wonder whether the deadline was January 1 or December 31…

Actually, the deadline might be quite appropriate. When the Tevatron has collected 2 fb^-1 of data, which should happen this summer, it might be able to rule out a light Higgs, and thus indirectly SUSY, at the 95% CL.

But this prediction is of course not watertight. We could be dealing with split (or supersplit) SUSY, which has been intelligently designed to avoid confrontation with experiments forever.

28. **Urs**
January 10, 2006

Tony,

thanks for listing all these references. I know people are trying to see if the closed bosonic string is related to the superstring. One idea has always been that, since it contains a tachyon and is hence instable, it will reduce to the 10D superstring when closed tachyon condensation is over.

I am no expert at all on the state of the art of studying this. The impression I got from hearsay is that it is not really understood yet. But very likely much more is understood than I am aware of.

As a gut feeling, I would *expect* the bosonic string to be related to the fermionic one.

29. **Urs**
January 10, 2006

Some elements on how a theory gives rise to predictions:

Say we have an assumption A about the world. Say we can show that A implies either B or C.

A => (B v C)

Say we find from measurements that in our world B is violated

not B

Then we combine our assumption above the world with our knowledge of the world and deduce

(A /\ not B) => C .

Hence we conclude:

"Given available data, our assumption predicts C".

An example:

Let A be the assumption that the universe on large scales is described well by an FRW model in Einstein GR. As you know, this implies

(C v D v E)

where C is a universe of positive, D one of vanishing and E one of negative curvature.

Now you make measurements. These rule out two of these possibilities.

not C and not E .

(This example might be a little outdated. Modify as you deem appropriate according to current experimental data.)

Hence combining our assumption (FRW universe) with the experimental data allows us to make the prediction

(A /\ not C /\ not E) => D .

And that is in fact a prediction, as everybody knows. The successful standard model of cosmology follows from

– assuming a theory (FRW model of GR)

– excluding solutions which do not match observation at all

– predicting that the only remaining solution is the one observed in nature, which then again implies predictions for further observations (like CMB fluctuations, etc).

Critics of the fact that string theory seems to admit more than one solution should note in this example how one observation about the world reduces the number of candidate solutions which in turn allows to predict outcomes of more observations.

Similarly, one might hope that if there are only a few (or even just a single) string vacuum matching basic properties of the standard model (like gauge group, number of generations, etc), then this would imply predictions on outcomes of other observations, like for instance the existence and configuration of compactified dimensions at high enough energies.

Since string theory is incompletely understood one might feel that this is unlikely to happen, but it is not by itself a procudure alien to science at all.

Finally, to apply the above to the prediction of high energy supersymmetry that we talked about, we have the following:

The assumption A is that strings with the CFT dynamics considered currently in string theory do exist.

This assumption implies B or C

A => B or C,

where B is the bosonic string and C is the superstring.

Say B were ruled out by observation (we have discussed caveats to that).

Since

(A and not B) => C

we get the prediction C of worldsheet supersymmetry. Finally, since

C => D

where D is (generically broken) target space SUSY, we get the prediction of (broken) target space SUSY from the assumption that strings exist at all and that fermions are being observed.

30. **Tony Smith**
January 10, 2006

Urs said: "… As a gut feeling, I would expect the bosonic string to be related to the fermionic one. …".

I agree, and I think that a possibility that is often overlooked when people say things like "SUSY has never been observed" is that, while the superpartners of naive 1-1 fermion-boson supersymmetry have indeed never been observed,
it may be that supersymmetry is more subtle, and related to Spin(8) triality (as Lee Smolin suggests in hep-th/0104050) in such a way that the "ordinary" fermions of the standard model are in fact (subtle) supersymmetric partners of the standard model gauge bosons.

If that is the case, bosonic strings might be "fundamental parents" of a fully realistic model with fermions, and SUSY might be of the subtle-triality type rather than a naive 1-1 correspondence,
and
one might expect that the LHC would see Higgs, more details about the T-quark, etc, but no "new" superpartner particles,
and
that failure by LHC to observe any "new" superpartner particles would not rule out string theory models such as those described by Smolin and others and cited in my previous comment 27. on this blog entry.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

31. **woit**
January 10, 2006

Urs,

You're still completely evading the main point: Saying that you have a "prediction of (broken) target space SUSY" is completely vacuous unless you can say something about how SUSY is broken. This isn't even in any sense a prediction of SUSY since SUSY with breaking scale pushed off to infinity = no-SUSY.

Dan,

I think the supersymmetry breaking problem is the fundamental problem for supersymmetry, both just in QFT, and in string theory. The fact that no one has come up with a way to break supersymmetry which agrees with experiment and doesn't just about completely ruin the ability of the theory to predict anything indicates to me that there is a serious problem with the whole idea as it stands. My own guess is that there is something to supersymmetry, but it will require some major new idea and a serious revamping of how we think about supersymmetry to come up with something that actually works.

32. **Juan R.**
January 10, 2006

Michael Duff said,

> History has shown that rapid confirmation by experiment is a poor guide to the eventual value of a physical theory.

This may be a joke!

> Consider the following theoretical ideas that have yet to be empirically confirmed: gravitational waves (1916); the cosmological constant (1917); extra dimensions (1926); the Higgs boson (1964); supersymmetry (1971).
> All were dismissed in their time by impatient naysayers as theories going nowhere.

Were those 'naysayers' wrong? There is theoretical evidence that gravitational waves are not real (this is focused to the problem of localization of energy in GR). There are experimental discrepancies (order 2%) in last binary pulse tests suggesting longitudinal components cannot be explained via GR-waves and this has open the doors to reevaluation of energy problem of GR again.

There exist serious theoretical evidence that cosmological constant is not one. In some recent works, even the cosmological constant is a geometrical effect with no vacuum interpretation.

There are studies against existence of addittional dimensions, etc.

Precisely, the existence of **alternative** points of view and theories is the REASON for experimental checks selecting the correct theory or view. There are huge collections of beatiful scientific ideas that experiments ruled out from science. String theorists know a lot of that. String theory NEWER worked. For example, Robert B. Laughlin, 1998 Nobel Prize-winning physicist says

> People have been changing string theory in wild ways because it has never worked.

Do you remember string theory fiasco on strong force? They was substituted by QCD.

In SCIENCE, any interesting idea is not true before experimentally verified. But string theorists have introduced the non-scientific idea of a 'beatiful' idea is true even if there is no more basic experimental evidence for it. They believe are Good…

> String/M-theory is much more ambitious and far-reaching than any of the above ideas. It is a bold attempt to fuse gravity with quantum mechanics and explain all physical phenomena.

Yes! and FAILED. String M-theory does not unify QM and gravity (in fact is even unable to obtain GR equations without appeal to fixed backgrounds) and, of course, being a reduccionist approach string M-theory is UNABLE to explain emergent phenomena. As said P.W. Anderson "More is different".

I already explained that string theory is not a TOE in sci.physics.string. In fact, it is not even a 1% of a realistic TOE.

> The debate is not between those who believe that string/M-theory should be judged by its agreement with experiment and those who do not. The debate is between those who demand instant gratification and those who recognise that a theory of everything does not happen overnight.

No! The debate is between those (scientists) who study the world as it is and those (stringers) who want that universe was as they want. Stringers want that universe was 10-11D, supersimmetric, quantum, reduccionist, and formed by dozens and dozens of unobserved 'particles': 0-branes, 1-branes(strings), 2-branes, … 5-branes, … (and some even claim that may be background dependent and that GR may be ignored!)

They claim all of above (and more) even if SCIENTIFIC disciplines experimental data, and theories claim and prove the contrary.

For example, there is well studied cases in complexity theory and published in specialized journals that prove that reduccionism does not work in complex systems as biological societies due to nonlinear features. It has been proven in very recent articles published in the journal *Foundations of chemistry* that chemistry has been **not** reduced to microphysics and that discipline is emergent one, with own concepts and theories *cannot be reduced* to ontological objects of lower class, e.g. atomic physics.

People who has worked in complexity problems (e.g. Anderson, Laughling, Gell-Mann in Santa Fe institute) has expressed the point that string theory is NOT a theory of everything even if was correct.

Stringers simply ignore data and proofs and continue to call to a 'theory' of microscopic strings a theory of everything…

That is not a scientific attitude and remember to me classical physicists searching for a TOE whereas ignored the new QM.

> Many pioneers of the incredibly successful standard model of particle physics, including Nobel laureates Murray Gell-Mann, Abdus Salam, Steven Weinberg and David Gross, turned their attention to superstring theory and continued to pursue its ramifications notwithstanding its lack of empirical support.

And by each one of those scientist we can cite another smart scientist (including lot of Nobel laureates) rejecting string theory.

Do you remember Feynman evaluations of string theory? Yes he used the word nonsense.

Moreover the scientists supporting string theory did some mistakes in the past. Take the example of Gell-Mann or Weinberg. Weinberg has done several wonrg stuff on gravitation, and unification. He openly claimed that QM and Coulomb forces is "all one requires for chemistry" and he failed. Today, we have electroweak quantum chemistry. If the 'father' of eletroweak theory was unable to understand all details theory he did. How can we are sure of his evaluation of string theory.

Take Gell-Mann case, ignore the errors he did in the past and focus in his evaluation of string theory. I have an early work where he clearly explain why heterotic superstring is the *only* serious candidate to unified theory of elementary particles and others string versions being incorrect. In one of his last works, he now carefully talk about the action of M-theory since *now* he know that all string theories are related via dualities.

If Gell-Mann failed then to understand string theory how do you know that now he is understanding M-theory?

> They no doubt recalled the decades required to knock the standard model into a shape that could be confirmed by experiment – for example the 25-year time lag between the prediction and discovery of non-Abelian gauge bosons upon which the standard model is based.

How many alternative theories were rejected in those 25 years? Yes a huge number…

> M-theory solves, among other things, the 1974 puzzle posed by Hawking concerning the microscopic origin of black-hole entropy.

No! There is a mathematical conjeture in brane theory, not a verified scientific explanation.

> Nor is M-theory incompatible with an accelerating universe as your article implied.

Of course, since M-theory is a misnomer because M-theory is not formulated!

Nobody know it but stringers use the term 'theory' how if really exited a theory. In fact, nobody has proven even that the theory exist. We use the term "M-theory conjeture" for refering to those conjetures extracted from non perturbative studies of string theory suggesting existence of a full theory for different values of parameter g.

Some candidates to M-theory as Matrix model violate previous asumptions of string theory. For example, Banks formulation is based in pointlike particles, when during decades all stringers attempted to convice us as stupid the concept of pointlike partiles of the standard model was. Anyone in the street has heard about small unidimensional object (the famous string) but practically nobody heard of 0-branes.

> This is hardly deserving of your description as a theory in "a sorry state". The annual international string theory conference continues to attract an increasing number of participants, now about 500.

Perhaps do you misunderstand between state of theory and state of the community? The community is rather well, the theory is in a sorry state with lot of no-go theorems and zero advance in fundamental issues in the last two decades.

—

Juan R.

Center for CANONICAL |SCIENCE)

33. **Urs**
January 10, 2006

Peter,

I don't think I am trying to evade any point. I am trying to state the obvious elementary facts in a hope to increase the productivity of the discussion.

If your point is that the prediction of SUSY by strings is vacuous for almost all conceivable practical purposes then I fully agree.

I just went through this little exercise in order to show that susy is not really put in by hand in string theory, but follows from assumption A (when stated with all due qualifications).

This is true, albeit not very useful. That happens…

34. **Urs**
January 10, 2006

One more thing: When talking about 'breaking of SUSY in string theory' one is mostly thinking of spontaneous breaking in the effective 4D theory obtained by compactifying 10D on M^4 x CY6.

But, as far as I know, there is no known mechanism that would favor compactification on CY. I'd expect the generic string vacuum 'breaks' susy by not being of the form M^4 x CY6.

What remains true, however, is that flat 10D space will look supersymmetric. So in as far as on small enough scales (i.e. above the compactification scale) spacetime looks locally flat, perturbative string theory would seem to predicts it to look supersymmetric. If it still applied there, that is…

But I am obviously not anywhere near being an expert on this.

35. **Urs**
January 10, 2006

Tony,

this sounds all very interesting. I wish I would better understand the ideas on algebraic patterns that you have (and be able to safely distinguish them from your more voodoo-like ideas, if you know what I mean).

36. **Tony Smith**
January 10, 2006

Urs, about your comment 36. on this blog, here is an effort to describe in voodoo-free terms some aspects of my view of SUSY:

Start with Smolin's hep-th/0104050 statement: "… Supersymmetry appears to be related to triality of the representations of Spin(8). …".

Spin(8) has three 8-dim representations:
two mirror-image half-spinor representations; and
one vector representation.

Half-spinor representations naturally correspond to fermions, so it is natural for one 8-dim half-spinor rep to correspond to fermion particles and for its mirror image 8-dim half-spinor rep to correspond to fermion antiparticles.

Vector representations naturally correspond to vectors.

Triality shows that each half-spinor representation is isomorphic to the vector representation.

So, for Spin(8), triality gives a 1-1 correspondence between half-spinor fermion particles and vectors.

Since gauge bosons come from bivectors, or antisymmetric pairs of vectors, and since there are 8/\8 = 8×7/2 = 28 bivector gauge bosons for Spin(8),
the triality SUSY for Spin(8) is NOT a 1-1 fermion-boson correspondence, but is rather a correspondence between 8 fermion particles and 28 gauge bosons, which is a 2 to 7 ratio, NOT a simple 1 to 1 ratio.

Since I like voodoo, and Spin(8) is natural for voodoo, if I were to advocate my voodoo stuff here I would say in detail how those fermions and gauge bosons look realistic and how the dimensionality of factors in the Lagrangian density in 8-dim spacetime works out to be nice, and how a quaternionic calibration breaks the 8-dim spacetime into 4-dim spacetime plus 4-dim internal symmetry space, etc.
However,
I promised here to stay away from voodoo, so I will do so as follows:

Many people in string theory like to use gauge groups with Lie algebras such as Spin(10), E6, Spin(16), E8, and products thereof, so please note that each and every one of those Lie algebras contain a nice Spin(8) subalgebra with triality, and (calculation details vary with whichever algebra is your favorite) roughly you can find a subtle-supersymmetric correspondence in each case such that the fermions are related to gauge bosons by using the triality of the embedded Spin(8), which means that:

The natural SUSY of such string theory models is NOT naive 1-1 requiring unobserved stuff like selectrons and gluinos.

The natural SUSY of such string theory models is not only more subtle, but, very importantly, THE WELL-KNOWN STANDARD MODEL FERMIONS CAN ACT AS THE SUBTLE-SUPERSYMMETRIC PARTNERS OF THE GAUGE BOSONS,
so that
A FAILURE OF THE LHC TO FIND SELECTRONS, GLUINOS, ETC WOULD BE IRRELEVANT WITH RESPECT TO (actually, supportive of) THE VALIDITY OF SUCH STRING THEORY MODELS.

This may be the "new idea and … serious revamping of how we think about supersymmetry" that Peter mentioned in his comment 32.

I thank you for the opportunity to say this here, because I used to wish that some group somewhere would let me participate in a seminar or workshop etc about such stuff, but I guess my status (being blacklisted by the Cornell arXiv) made me an outlaw-persona-non-grata to be shunned by all decent physicists, and I guess as of now I just have to live with that.

Although I do like the voodoo = Clifford algebra / generalized hyperfinite II1 von Neumann factor / Wyler geometry / strings-as-world-lines model that I use (and I do hope that someday others might come to like the voodoo stuff),

I also hope that it is clear from the above that some parts of it might be very useful in non-voodoo string theory models, and that I am quite happy to discuss such parts in non-voodoo contexts.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

37. **Alejandro RIvero**
January 11, 2006

Just a note: any revamping of supersymmetry should contemplate the hierarchy problem, just as usual susy does; cancellation of fermionic and bosonic loops and all that.

Of course the simplicity of the 3-familiy standard model is amazing, 12 spin 1/2 particles (and another 12 antiparticles thus) and 12 spin 1 particles seem to be claiming for some special symmetry or a cancellation mechanism, or perhaps even a Galoisian justification. But here comes just the same thing we are critiquising in this thread: to build an structure needing these particles is not the same
that to predict the existence of these particles; exactly as to build a metatheory needing susy is not the same that to predict susy.

38. **Urs**
January 11, 2006

Tony,

thanks for the response.

It is certainly true that triality of SO(8) has a lot to do with supersymmetry.

The fact that the superstring has to live in 10 dimensions (to cancel the Weyl anomaly) is closely related to this. Precisely in 10 dimensions does a string (a 2D worldsheet) have precisely 8 transverse directions and its fermionic and bosonic transverse oscillations a chance to be related, thnaks to SO(8) triality.

This is particularly manifest in the procedure which relates the Green-Schwarz formulation of the superstring (the one which is bosonic on the worldsheet and manifestly susy in target space) with the Ramond-Neveu-Schwarz formulation (which is manifestly susy on the worldsheet but not manifestly so in target space).

In order to show that these two formulations are in fact equivalent one rewrites the worldsheet spinors of RNS as worldsheet bosons ("bosonizing" them) and checks that the result is the GS string. This procedure only works due to triality and hence only in 2+8 dimensions.

Similarly the 11 dimensions of "M-theory" arise as 3-dimensions of the worldvolume of a supermembrane plus 8 transverse ones.

So in this sense there is a relation between triality and supersymmetry which is well-established.

But it seems that you are arguing for a different relation.

I am not sure yet if I follow the details, though. Let me see: are you arguing that any system with a spin(8) symmetry has bose/fermi symmetry?

Certainly this is not true without further qualifications. The point of triality above is rather along the lines that in 8 dimensions Weyl spinors have the same number of components as vectors. But the spin(8)-representation still acts inside the spinor space or the vector space seperately.

Maybe I am missing the obvious. Could you maybe sketch the form of one of these Lagrangians that you seem to have in mind which contains 2x fermions and 7x bosons such that it is invariant under a symmetry operation which mixes these 7x-bosons and the 2x-fermions?

39. **Urs**
January 11, 2006

> voodoo = Clifford algebra / generalized hyperfinite II1 von Neumann factor / Wyler geometry / strings-as-world-lines model

I should clarify what I meant by "voodoo" in a previous message. I don't consider Clifford algebra and von Neumann algebra theory as voodoo at all.

I assume what you have to say on your website concerning algebras etc. is correct and often interesting. What looks like voodoo to me are some of your more liberal free associations of structures with physics. It might be that there is deep truth hidden there, but then it is not easy to see.

Hoping that you don't mind me being honest, I have to say that much of what you present (and certainly the style in which you present it) looks like an extremely sophisticated version of numerology.

Maybe it's not. But I bet the reason why you are encountering problems with the arXiv is due to that.

40. **Urs**
January 11, 2006

> strings-as-world-lines model

If you had said "worldsheets as planar Feynman-diagrams" I would agree that there is something very deep about it.

41. **Santo D'Agostino**
January 11, 2006

Hi Urs,

You said about predictions:

"… Hence combining our assumption (FRW universe) with the experimental data allows us to make the prediction

(A ∧ not C ∧ not E) => D .

And that is in fact a prediction, as everybody knows. The successful standard model of cosmology follows from

– assuming a theory (FRW model of GR)

– excluding solutions which do not match observation at all

– predicting that the only remaining solution is the one observed in nature, which then again implies predictions for further observations (like CMB fluctuations, etc). …"

This is, to me, an unusual use of the word "prediction." Sticking with the context of GR, consider the prediction of the perihilion shift of the orbit of Mercury, or the prediction of the bending of distant starlight by the sun. In these two cases, one can derive formulas from GR, then one substitutes experimental values for independently-measured constants (such as the gravitational constant), and the formulas then produce definite, numerical values for the desired quantities. These definite numerical values can then be compared to the corresponding observed values. To my mind, this is what is meant by prediction.

Contrast these predictions with the curvature of spacetime in the FRW models, as you discuss. The curvature of spacetime cannot be calculated by GR, and so does not qualify as a prediction of the theory. True, any experimentally measured value of the curvature is consistent with GR, but that does not make any particular measured value a prediction of the theory.

Using such a traditional interpretation of the word "prediction," are there any predictions of string theory?

Best wishes,
Santo

42. **Urs**
January 11, 2006

Santo D'Agostino,

the example you describe is indeed a special case of what I
sketched in

http://www.math.columbia.edu/~woit/wordpress/?p=323#comment-7364

(There seems to be something wrong with hyperlinks here, at least in the comment preview.)

In your example "A" is the assumption that Einstein GR describes gravity.

Your statement that "one can derive formulas from GR" is the implication

A => (B1 or B2 or B3 or …)

where the Bi for all i in some set I are all the solutions of these equations.

Your statement "then one substitutes experimental values" is precisely the important point which I tried to emphasize in my post. It eliminates classes of the Bi, those that do not fit these experimental values, so that we have

for all j in a subjet J of I : not Bj .

Given sufficient experimental input (large enough set J) we obtain a small enough set

K = I without J

of candidate solutions and
then your statement is true that "the formulas then produce definite, numerical values".

In my notation this was the logical deduction

(A and not Bj for all J in J)
=>
Bk_1 or Bk_2 or … for k_i in K

Please read again carefully through what I wrote to see this.

So the point is this: We make an assumption about the world. This usually is compatible with more than one state of the world. Hence we look at only those consequences of that assumption which do not contradict known facts about the world. Then we check what our assumption implies on the remaining set of solutions.

It's a very simple fact. I stated it because in the heat of string-theory-bashing some simple facts tend to be forgotten. 🙂

String theory is based on an assumption A which says that perturbative scattering amplitudes are given by evaluating certain 2D CFTs over Riemann-surface diagrams.

You can work out consequences of this assumption and go through the above procedure sketched above.

So in particular, you can take experimental input (like the gauge group of the standard model or the number of generations) and use this to exclude certain a priori possible consequences of assumption "A", i.e. certain "string vacua"

Unfortunately, the set K of remaining candidate solutions is thus far still too large to say almost anything definite about it.

Another problem is that assumption "A" itself may not be sufficiently well understood yet.

So, that's how it goes in science. Assumptions are made, checked, reworked, etc. The problem with string theory that Peter is fond of pointing out is not that assumption "A" is by itself an assumption without any chance of being useful. If we perfectly understood the consequences of "A" (say at the level at which we understand the consequences of the assumption that GR describes gravity) we'd be all much happier.

It makes sense to complain about the progress that has been achieved in understanding “A” and to urge people to look at other viable assumptions “A1”, “A2”,…

43. **Santo D'Agostino**
January 11, 2006

Hi Urs,

Although the traditionally standard use of the word “prediction” may indeed logically be a special case of the process you describe (and also call by the name “prediction”), the two processes are sufficiently different in character that they ought to be labelled by different names to prevent confusion.

To clarify the difference in character between the two concepts, consider this passage from your previous message:

“In your example “A” is the assumption that Einstein GR describes gravity.

Your statement that “one can derive formulas from GR” is the implication

A => (B1 or B2 or B3 or …)

where the Bi for all i in some set I are all the solutions of these equations.

Your statement “then one substitutes experimental values” is precisely the important point which I tried to emphasize in my post. It eliminates classes of the Bi, those that do not fit these experimental values, so that we have

for all j in a subjet J of I : not Bj .”

Suppose that the Bi are possible values for the perihelion shift of Mercury. Then by substituting experimental values INDEPENDENT of the perhilion shift, one eliminates possible values of Bi and obtains a single value, called the predicted value of the perihelion shift, which then one can compare to the observed value.

Contrast this with the situation of curvature. That is, suppose that the Bi represent possible values of the curvature of spacetime. If one had a formula for the curvature of spacetime, and substituted experimental values into the formula that were INDEPENDENT of the curvature, then once again one could say that the theory predicts the curvature. However, using observed values of the curvature itself to select values of Bi, which are also values of the curvature, is a process that you refer to as prediction, but which I would not call as such.

With your usage of the word “prediction,” it seems that one could say that GR predicts the curvature of spacetime no matter what its observed value is. This represents a departure from the traditional use of the term, and it would be better to coin a new term for the process you describe.

Best wishes,
Santo

44. **Urs**
January 11, 2006

> With your usage of the word “prediction,” it seems that one could say that GR predicts the curvature of spacetime no matter what its observed value is.

No. Please read what I wrote. In the example of cosmology the observed curvature of space on large scales is experimental input. Only given that input can we select out of all available FRW models the one which we then use to make further predictions.

The whole point is that the assumption GR+FRW model does *not* predict the curvature of space. There is a “landscape of solutions” to GR+FRW which contains precisely three solutions. We pick the one not excluded by experiment and use it to predict further observations.

45. **Santo D'Agostino**
January 11, 2006

Hi Urs,

I’m relieved to read that you agree that experimental input cannot be considered a prediction of a theory. But your usage of the word prediction in an earlier post is still unjustified if we stick to the traditional meaning of “prediction.”

Quoting you:

“Let A be the assumption that the universe on large scales is described well by an FRW model in Einstein GR. As you know, this implies

(C v D v E)

where C is a universe of positive, D one of vanishing and E one of negative curvature.

Now you make measurements. These rule out two of these possibilities.

not C and not E .

(This example might be a little outdated. Modify as you deem appropriate according to current experimental data.)

Hence combining our assumption (FRW universe) with the experimental data allows us to make the prediction

(A $\wedge$ not C $\wedge$ not E) => D .

And that is in fact a prediction, as everybody knows.”

I am not arguing with the validity of the methodology you describe. I am only saying that D should not be referred to as a prediction of the theory, since D is observational input, as you seemed to agree with in your immediately previous post. In other words, I am disputing your sentence, “And that is in fact a prediction, as everybody knows.”

A prediction of a theory is a definite thing, and is not contingent on experimental or observational data. What if in the future, more acceptable measurements rule out D and E, but not C. Would you then say that the theory now predicts C?

Santo

46. **Urs**
January 11, 2006

Ah, now I see what you mean. That was sloppily formulated. The point is, once you have fixed D this implies other things

D => D1 and D2 and …

(like details of nucleosynthesis, CMP spectra, etc.) and *this* are the predictions.

47. **Santo D'Agostino**
January 11, 2006

Hi Urs,

Agreed.

All the best wishes,
Santo

48. **John Gonsowski**
January 11, 2006

Urs, Tony's model uses E6 much like the E6 GUT one gets from the E8xE8 heterotic string. I think for both Tony and the heterotic string the two half-spinors of D4 relate to E6 orbifolding. The difference may be in the use of the D4 vector part of the Triality. If you think of D5 in between D4 and E6 or the SO(10) GUT in between the SU(5) GUT and the E6 GUT. That SO(10) as John Baez mentions in his recent paper is a nice way to see a 10-dim spacetime. That 10 is a D5 vector and is a nice way to see why Tony uses the 8-dim vector of the D4 Triality as spacetime. The two extra dimensions Tony has for an SO(10) spacetime makes the spacetime complex. D5/D4xU(1) has 16-dims or the 10-dim has 2-dim added to the 4 big dimensions to get a 6-dim CP3 twistor. So Tony has spacetime as a substructure of the String. That Gross comment about space and time needing to go away (as the superstructure of the string) seems related to this. Gross asks what then replaces spacetime. For Tony it's the Clifford algebra. Kind of makes sense to think of Lie Algebras as a substructure of Clifford algebras. It kind of bothers me that so many people seem to have trouble with the general idea of Tony's model, it's not that complicated. If you were looking at something like Tony's Lagrangian after dimensional reduction I could see where a discussion is needed, there are tons of places I just take Tony's word for it, he seems rather good at math. As for Tony giving credit to ancient civilizations for finding Clifford Algebra and Lie Algebra patterns, lots of the patterns like D-series root vectors are rather simple and it is not so hard to imagine ancient people noticing them. I personally believe like Tony that some ancient people were quite good at noticing patterns that aren't all that easy to see.

49. **Tony Smith**
January 11, 2006

Urs asked (in comment 39.) "… are you arguing that any system with a spin(8) symmetry has bose/fermi symmetry? … Could you maybe sketch the form of one of these Lagrangians that you seem to have in mind which contains 2x fermions and 7x bosons … ? ..".

No, I am not arguing that ANY system with a spin(8) symmetry has bose/fermi symmetry, but I am arguing that some interesting systems do have what I call subtle supersymmetry due to the spin(8) triality.

The 7/2 boson/fermion ratio stuff is peculiar to my voodoo stuff, so I don't want to say much about it here, but (since yesterday (10 Jan 06) was National Voodoo day in Benin – see the BBC article at http://news.bbc.co.uk/1/hi/world/africa/4599392.stm) I will mention that the key is that in a Lagrangian density over 8-dim spacetime, the dimension of fermion/spinor PSI is 7/2 (as opposed to dimension 3/2 over 4-dim spacetime), and 8 fermion particles x 7/2 dimension = 28 = 28 gauge bosons x 1 dimension.

For a non-voodoo point of view, I will quote from Lee Smolin's hep-th/0104050:
"… the local structure of a 9+1 dimensional spacetime may be expressed in terms of J(O,2), which is the Jordan algebra of 2×2 hermitian matrices of octonions
z1 O0
J(O,2) =
O0 z2
… Similarly, the relevant Spin(9,1) spinors may be parameterized as 2 component octonionic spinors, which we may write as,
O2
PSI =
O1
… The main idea of supersymmetry, in the context of a theory of spacetime, is that there should be some fundamental unification of the spacetime geometry with the fermionic degrees of freedom which live in the spinor representations of the local invariance group.
…to unify the coordinates of the tangent space of a 9 + 1 dimensional spacetime with its spinor degrees of freedom, in a way that naturally includes an eleventh spatial coordinate z3 … incorporate all of them in the algebra of 3×3 hermitian matrices of octonions, which is called the exceptional Jordan algebra. This is given by
z1 O0 O2*
J = O0* z2 O1
O2 O1* z0
… sixteen of the dimensions of bosonic string theory are transmuted from bosonic to fermionic by a dynamical mechanism that involves the decay of the tachyonic degree of freedom … the exceptional Jordan algebra is exactly the unique mathematical structure we seek. … The automorphism group of J is known to be F4. This group has several Spin(8) subgroups, one of which acts on the components of J in the following way: the za are scalars,
O0,O1,O2 are, respectively, the 8 dimensional vector, spinor and conjugate spinors. …
One way to … make an action … is to construct a cubic action … which we will call the exceptional cubic matrix model … Spin(8) acts differently on each of the three slots of the cubic product. … The three particular Spin(8) subalgebras that act on each slot are related to each other by triality …
It is interesting to conjecture that the supersymmetries … can be understood as arising from the components of F4 that live in the 8 dimensional spinor and conjugate spinor representations S and Sc. These act bosonically on the degrees of freedom in J however it is possible that after the compactifications that turn the …[O1 and O2]… into fermions they may imply a fermionic symmetry of the reduced action …".

I hope that the quote is extensive enough to give Lee Smolin's basic idea, and not too extensive as to be burdensomely long in a blog comment.

As you can see from language like "conjecture", full calculations were not done at the time of hep-th/0104050, and, as far as I know, they have yet to be done.

I am sort of puzzled (and disappointed) that both Lee Smolin and Lenny Susskind, authors of papers about such stuff (hep-th/0104050 and hep-th/0012037) seem to spend most of their energy nowadays on other matters, even though they both doubtless have grad students / postdocs /etc who could be assigned some of the calculation work that remains to be done.

Lee's Jordan algebra approach lends itself to LQG / spin foams with the exceptional Jordan algebra at the spin foam nodes,
while
Lenny's matrix model lends itself to string theory models with Lie algebras that are related to that exceptional Jordan algebra,
and
it would be nice (in my opinion) to work on both approaches, and maybe it would result in showing that the two approaches are equivalent (sort of like Heisenberg and Schroedinger quantum models were shown to be equivalent). Maybe they might even also be equivalent to my voodoo model.
Therefore,
I am somewhat saddened that Lee seems to mostly run around talking about relatively general topics such as background independence and Lenny seems to be running around Lost in his Landscape. Such topics are easy to use as PR hype for the public and funding agencies, but in my opinion they are nowhere near as useful as the hard work of doing some detailed calculations about triality and subtle supersymmetry.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Urs, I am quite OK with your honest expression of opinion (in comment 40.) that "… much of what …[I]… present (and certainly the style in which …[I]… present it) looks like an extremely sophisticated version of numerology. …".

As to style, if you are referring to my web site, it is not intended to look like a standard-format physics paper, and it is true that it contains a lot of associations (IFA divination = Clifford algebra, etc) that some people might find unusual.
However,
my pure-physics papers that were put on the XXX archives before Cornell blacklisted me might look a bit less unconventional. For example: hep-th/9302030 deals with Jordan algebras and force strengths and hep-ph/9501252 is a model overview. Although those older papers contain some ideas that I have changed/corrected as of nowadays, they were written in conventional LaTeX format, and I tried to make them look conventional in style. If you find them to also look strange, then I can only apologize for my limited expository ability.
As to some of the associations that I make seeming strange or unusual, that is one of the topics that I discuss in therapy with my psychiatrist. So far, his diagnosis seems to be that the unusual associations are not problematic, although depression is a problem for which I am undergoing therapy.

50. **Urs**
January 11, 2006

> I will mention that the key is that in a Lagrangian density over 8-dim spacetime, the dimension of fermion/spinor PSI is 7/2 (as opposed to dimension 3/2 over 4-dim spacetime), and 8 fermion particles x 7/2 dimension = 28 = 28 gauge bosons x 1 dimension.

If this is really more than numerology it might be interesting. It's hard to say, for me.

> sixteen of the dimensions of bosonic string theory are transmuted from bosonic to fermionic by a dynamical mechanism that involves the decay of the tachyonic degree of freedom

That's what one would hope. What concrete evidence do we have?

> I am somewhat saddened that Lee seems to mostly run around talking about relatively general topics such as background independence and Lenny seems to be running around Lost in his Landscape. Such topics are easy to use as PR hype for the public and funding agencies, but in my opinion they are nowhere near as useful as the hard work of doing some detailed calculations

Well said. We definitely agree on that.

It's not that there is nobody out there looking hard for new grand (algebraic) structures. P. West and H. Nicolai come to mind with their attempts to integrate everything into E10 and E11. Judging from the abstracts of Nicolai's papers there seems to be slow but steady progress and mounting evidence that indeed all of 11D supergravity is (a small subset) of the geodesic motion on something like exp(E10/K(E10)).
(http://golem.ph.utexas.edu/string/archives/000353.html)

But things like that are certainly, and unfortunately, in the minority at the moment.

P.S.
If you make John Gonsowski write a comprehensive and coherent pdf exposition of your ideas, I promise to take a detailed look at it. More importantly, others might, too.

51. **ksh95**
January 11, 2006

> If you make John Gonsowski write a comprehensive and coherent pdf exposition of your ideas…

I second that, especially the part about **coherent**.

> As to some of the associations that I make seeming strange or unusual, that is one of the topics that I discuss in therapy with my psychiatrist. So far, his diagnosis seems to be that the unusual associations are not problematic, although depression is a problem for which I am undergoing therapy.

Whoa, slow down there Tony…Way to much information. You were on a roll, why didn't you quit while you were ahead? You wrote one paragraph to many.

52. **woit**
January 11, 2006

This discussion of Tony Smith's ideas is way off topic, and this kind of thing gives lots of people the idea that this is a forum for discussion of their own work. I then have to spend increasing amounts of my time deleting people's comments in order to keep this comment section from turning into sci.physics. Please continue this discussion with Tony privately or in some other forum.

53. **Tony Smith**
January 11, 2006

Urs, "… sixteen of the dimensions of bosonic string theory are transmuted from bosonic to fermionic by a dynamical mechanism that involves the decay of the tachyonic degree of freedom …" is a direct quote from Lee Smolin's paper at hep-th/0104050, so he would be the one for you to ask: "… What concrete evidence do we have? …". It could probably be answered with some of the hard calculations that in older times would be assigned by someone like Lee to his grad students / postdocs / etc.

Maybe (and this is just speculation from one who has been out of academia for a long time) grad students / postdocs / etc nowadays don't see doing such assigned hard work as "the" way to advance, but instead just write a lot of relatively shallow "me-too" papers, or even follow the example of Lubos and advance by acting as mindless advocates (a la talking head political spin-masters on Fox News etc) of whatever the powers-that-be say.

As to writing a “comprehensive and coherent pdf exposition of …[my]… ideas”, I wouldn’t demand that of anyone, much less a friend like John Gonsowski. The closest thing to such a single pdf file might be the one on my web site at http://www.valdostamuseum.org/hamsmith/TQ3mHFII1vNFadd97.pdf which is about 550 KB, and about 65 pages, in size. It has my ideas up to around 2003, but it is not in LaTeX, and it does not cover my more recent calculations of neutrino masses and mixing angles and of the Dark Energy : Dark Matter : Ordinary Matter ratios observed by WMAP.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

Photos of Mathematicians

January 8, 2006
Categories: Uncategorized

For many years C. J. Mozzochi has been taking photographs of mathematicians, primarily at mathematics conferences and lectures in Princeton or in the New York City area. With help from Mark Goresky, he now has a web-site where you can view and download many of his photographs. The site will be periodically updated with additional photographs.

Comments

1. **C. J. Mozzochi**
   January 9, 2006

   Thanks Peter!

2. **Dissident**
   January 9, 2006

   I'm surely missing something really obvious, but… what's the point? Ogling supermodels I might understand, but mathematicians?

3. **woit**
   January 9, 2006

   Dissident,

   As I've said before, the interests of this blog are pretty idiosyncratic. Lots of places to go on the internet if you want to ogle supermodels. Those that want to ogle mathematicians need a much more specialized site like this to point them in the right direction….

4. **Rob Scott**
   January 9, 2006

   Sigh, if there were only supermodel mathmaticians!

5. **MathPhys**
   January 9, 2006

   There are.

6. **John Gonsowski**
   January 9, 2006

   Just got my February Discover magazine and it has a mathematician photo. Is Columbia's math department interested in having a 2nd TV/movie star?

7. **woit**
   January 9, 2006

   Hi John,

   No, I think one of those is enough. I still haven't seen the magazine yet, will write something about it soon.

8. **Chris Oakley**
   January 9, 2006

   Maybe Mozzochi should have three sets of photos: mathematicians in casual attire, mathematicians in their national costume and mathematicians in swimwear, like the Miss World pageant. The latter would not necessarily be a pretty sight, but one could always just make one's excuses and leave at that point.

9. **Robin**
   January 10, 2006

   Andrej Bauer also maintains a collection of photos of mathematicians; mainly people working in category theory and/or logic.

Discover Interview

January 9, 2006
Categories: Uncategorized

The February issue of Discover Magazine contains an interview of me by writer Susan Kruglinski. I haven't seen the magazine itself, and it's not yet on the newsstands here, but a friend who subscribes was kind enough to send me a copy of the article. There's a picture of me sitting underneath the blackboard outside my office taken by a photographer. The day he came I had a migraine headache, so was looking rather grim, not my usual cheery self.

Comments

1. **mathjunkie**
   January 10, 2006

   So, what was the interview about?

2. **MathPhys**
   January 10, 2006

   What do you think it's about?!

3. **woit**
   January 10, 2006

   String theory and the blog, mainly. The title is "The Dean of Debunkers"….

4. **MathPhys**
   January 10, 2006

   Great title, Peter. Could you please make it accessible here?

5. **Dumb Biologist**
   January 10, 2006

   Perhaps the mantle can be shared with James Randi?

6. **woit**
   January 10, 2006

   Sorry MathPhys, but I think Discover wouldn't want me putting up their copyrighted content on my web-site.

7. **John Gonsowski**
   January 10, 2006

   Peter, your photo seems pensive as well as stern and it kind of goes with the folded arms and title. Discover seems to like slightly surreal photos of people. For the interview, you must not have had a migraine or too many off topic comments that day cause it was great. Respectful and understanding of why the field went in the direction it did, but very clear that more people need to be thinking in more directions. I feel like I'm giving an opinion on a movie while not wanting to give away too much.

8. **Quantoken**
   January 10, 2006

   Peter:

   It is **NOT** true that just because it's Doscover copyrighted material you can not re-produce it here on your blog. You have a right to **fair use** of copy-righted materials, persuant to the law, **Title 17 U.S.C. Section 107**

   As a matter of fact, you and your colleagues are already routinely exercise your title 17 section 107 rights, by photo copying published articles without having to contact the publisher.

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   Clearly if you quote the article here for purpose of education and discussion, and not for making money, you qualify for the fair use clause.

   Quantoken

9. **Quantoken**
   January 10, 2006

   Peter:
   All you need to do for such fair use, is attach such a declaration note, in its exact format as below:
   In accordance with Title 17 U.S.C. Section 107, this material is distributed without profit to those who have expressed a prior interest in receiving the included information for research and educational purposes.
   As a matter of fact, if you search on the web that **exact sentence**, you find many occurances.

10. **woit**
    January 10, 2006

    Quantoken,

I'm not completely convinced by your legal argument, and in any case I'd rather just not get involved in this. There's nothing much in the interview that isn't already said many times elsewhere on this blog. The Discover people, the writer, the photographer are all trying to make a living by putting together pieces like this, hoping someone will pay some money for them. I'd rather encourage people to support them by at least buying a copy of the magazine every so often. It would be a shame if magazines like this all go out of business because their market dries up as everyone expects everything to be freely available on the internet.

11. **Thaed**
January 10, 2006

I loved the Discover article, but I'm a lawyer, not a math guy, so I was curious what the equation is on the blackboard above your head?

Michael Crichton is also fond of the expression "Not even wrong." I first read that phrase in an interview with him in some magazine.

12. **Peter**
January 10, 2006

Thaed,

The photographer asked me to write something on the blackboard, that was the best I could come up with at short notice. The top line is just the path integral for a quantum field theory like the standard model. The bottom line is the integration map in equivariant K-theory. What I've been working on is understanding the relation between this physics and this math, can't say that I've figured it out yet, but there's something there….

13. **MathPhys**
January 11, 2006

Some of us have seen a photograph of a very well known physicist standing facing a blackboard, arm extended with a piece of chalk in hand, chalk touching blackboard, blackboard is covered with equations. Looks very impressive. A great physicist in action. Too bad the handwriting on the blackboard is that of a longterm collaborator.

14. **Chris Oakley**
January 11, 2006

What – so were the Feynman Lectures a put-up job then? I know that Leighton and Sands helped write the books, but this is a revelation. So were the students looking at a hologram of the great man navigating around Leighton's equations while the real Feynman was playing bongos or testing out cheesy chat-up lines in hostess bars … ?

15. **woit**
January 11, 2006

Chris,

I asked MathPhys privately what he was referring to (no, despite what Lubos thinks, I'm not MathPhys). It wasn't Feynman, but a more obscure picture of a more obscure physicist, one I wasn't aware of. I'll respect his desire to keep his comment nearly a private joke, but thought I'd mention that it wasn't about Feynman (which would have been my guess too).

16. **MathPhys**
January 11, 2006

Definitely not Feynman. Besides, Feynman never had long term collaborators, let alone any scientific collaborators in any sense (I don't think of those who wrote his books on the basis of his lectures as collaborators). Only those who personally knew the man that I have in mind will recognize him from my comment, so I'll leave it as a private joke.

17. **Thomas Love**
January 12, 2006

We on the West Coast get the magazine late so I just read the article last night. I agree that string theory is not even wrong, but I said that about the standard model years ago (unpublished) so I don't understand your love affair with it. I was at the AMS summer meeting in Salt Lake City in 1987 when Witten gave a week long series of talks on string theory. I laughed out loud, it was so absurd. Another phrase I like: It's crazy, but is it crazy enough? In a letter to the editor of Physics Today I wrote: Nature comes with no strings attached.

18. **anon**
January 12, 2006

Nature has no strings attached! How does that translate into Latin? It should be adopted as the motto of objectivity. 🙂

19. **Thomas Love**
January 18, 2006

In describing string theory to a friend, with thousands of little strings wiggling around, I realized "This is a bucket of worms."

Current Science

January 10, 2006
Categories: Uncategorized

Mathematician David Goss wrote to tell me about the latest issue of the journal Current Science, which contains quite a few articles on Einstein's legacy, including an interesting one on Einstein and the search for unification by physicist David Gross. Perhaps I'm being a bit churlish, but surely I'm not the only person who is at least a bit happy that 2005 is over and done with, so that attention can begin to be paid to some other topic than that of Einstein. He was a true giant, but I bet he'd be pretty tired of the hoopla by now.

Gross discusses Einstein's goal of finding simple universal laws from which all physics can be deduced, and tells about how this inspired him at the age of thirteen to decide to become a theoretical physicist. He tells about Einstein's failed attempts to find particle-like solutions to the non-linear equations of GR, unified in various ways with electromagnetism, and notes that in the early 80s, with Malcolm Perry, he found magnetic monopole solutions that were a bit like what Einstein was looking for.

Getting to topics of current interest, Gross talks about "The discovery of supersymmetry, which we all hope and some expect in a few years from now at the Large Hadron Collider…" I don't think I'm the only one hoping not for this, but for something more interesting. I'd also be curious whether Gross puts himself among those who "expect" to see this at the LHC. There's the usual string theory propaganda, including the incorrect claim that string theory provides "a consistent and finite quantum theory of gravity" (no, the sum of the perturbation series is not finite, and Gross is one who often says we don't know what the theory even is). Gross also as usual stresses that he thinks we need to give up on space and time, but doesn't know what will replace them. He concludes that Einstein was wrong to refuse to accept quantum mechanics and to ignore nuclear and particle physics, but that he was right to try and unify gravity with the other forces, saying "this we know today is the central issue in fundamental physics", something I don't really agree with (I'd go for understanding electroweak symmetry breaking).

This issue of the journal also contains interesting articles by Michael Atiyah on Einstein and Geometry, and by Abhay Ashtekar on The winding road to quantum gravity. There's also a completely uncritical piece of string theory propaganda by Ashoke Sen entitled String theory and Einstein's dream that could easily have been written ten years ago.

For more uncritical promotional material on string theory, here are two things from UC Davis. They have something there called the "High Energy Frontier Theory Initiative (HEFTI)", and on their website you can read a report written by an external committee for the Dean at Davis promoting the idea of hiring more string/brane theorists of the phenomenological sort. The report is a few years old, but shows exactly what the consensus thinking of just about the entire high energy theory community has been for the past few years about what is the hot area to hire in. They promote the idea that particle theory is doing extremely well, so much so that

The last period of comparable experimental and theoretical ferment occurred in the early 1970s, swiftly culminating in the development of the Standard Model of particle physics.

and that the "pace of new developments is accelerating" in both theory and experiment.

Also from UC Davis is a new paper entitled Space and time from translation symmetry by Albert Schwarz. It starts off by claiming "In some sense string theory today is in very good shape", but seems to be empty of content.

Finally, there's a new paper out tonight reviewing the status of the string theory Landscape. It looks like landscapeologists are now ready to abandon even the idea that the number of phenomenologically viable vacua is finite, and the last part of the paper contains some impressive contortions about why this isn't necessarily a bad thing. They also seem to have given up on Douglas's idea of making predictions by counting vacua. They're still counting the vacua, but now the reason given is not to make predictions, but to see if there are enough to be sure that one of them will reproduce the standard model.

Comments

1. **MathPhys**
   January 10, 2006

   I hope that when putting the world together, God had more imagination than to attach a superparticle to every particle, then push it to very high energies.

2. **anon**
   January 10, 2006

   "… He concludes that Einstein was wrong to refuse to accept quantum mechanics and to ignore nuclear and particle physics,…"

   I don't this well represents the concerns Einstein was expressing. It is possible to accept the mathematics of quantum mechanics and particle physics as being a useful working approximation without religiously proclaiming that it is infallible.

   Einstein's reservations were not directed towards the validated empirical equations of QM, but towards Bohr's brainwashing philosophies which sweep inconsistencies between classical and quantum theories under the carpet.

   It is not good science to invent philosophies that make questioning look heretical or silly (complementarity and correspondence principles), although it had to be done back in 1927 to overcome the charge that QM was incompatible with classical physics.

3. **Juan R.**
   January 10, 2006

   A bit of history,

   Einstein was not the father of the idea of unification. This one of multiple miths. In fact, as historically proven, Hilbert already was working in an unified field theory before Einstein. In fact, Einstein contacted with Hilbert before the formulation of GR expressing his interest in Hilbert axiomatic unified field theory.

   Einstein rejected QM as pure nonsense and wait that some alternative equations (e.g. somewhat in the spirit of Bohmian mechanics) could REPLACE Schrodinger one. Einstein become a member of continuum guys, whereas Heisemberg, for example, was member of quantum school.

   Einstein rejected the math of QM (as most of classical physicists who found repugant the new mathematical tools). Einstein durely critized Dirac formalism as a kind of 'nonsense' when was presented to him.

   Einstein completely ignored nuclear physics advances since he waited that nuclear forces would be a kind of combination of EM + gravity. Somewhat as he waited that QM would be explained via some 'modification' of classical mechanics. He was not alone, some physicists (e.g. Stark) waited that a chemical bond could be explained via classical EM forces, for example.

In fact, when physicists said Einstein that via relativistic-QFT, they had obtained the highest precision in experimental predictions never seen, Einstein said “Bah that is not impressive for me”.

—

Juan R.

Center for CANONICAL |SCIENCE)

4. **anon**
January 10, 2006

Juan,

OK, so Einstein had his own share of crackpotism…

5. **Ugo**
January 10, 2006

@ anon :

Einstein’s crackpotism is excusable since he developped a correct theory of gravitation.
It was not a small endeavour, and even if he didn’t accept QM and Bohr’s interpretation of quantum mechanics so what ?
It doesn’t diminish at all all the great discoveries he did : in statiscal mechanics, brownian motion, special and general relativity etc….

6. **Chris W.**
January 10, 2006

Einstein rejected QM as pure nonsense …

This is, to put it mildly, a gross overstatement. Also, to characterize Einstein’s views on quantum mechanics in the 1930s and later as crackpotism strikes me as ludicrously ahistorical and revisionist. (I suspect Anon was just throwing Juan R a bone.) Were his views in several respects wrong, or at least questionable and self-defeating? Undoubtedly, although they stimulated a level of critical examination of quantum mechanics that might not have occurred otherwise.

Before criticizing Einstein for his deeply held positions in the mid-20th century the current generation of physicists should consider how their own views might appear with the benefit of hindsight several decades from now.

From Richard Feynman:

> We have a habit in writing articles published in scientific journals to make the work as finished as possible, to cover up all the tracks, to not worry about the blind alleys or describe how you had the wrong idea first, and so on. So there isn’t any place to publish, in a dignified manner, what you actually did in order to get to do the work.
> Nobel Lecture, 1966.
>
> I think that I can safely say that nobody understands quantum mechanics.
> *The Character of Physical Law* (Cambridge, USA, 1967)
>
> What I am going to tell you about is what we teach our physics students in the third or fourth year of graduate school… It is my task to convince you not to turn away because you don’t understand it. You see my physics students don’t understand it… That is because I don’t understand it. Nobody does.
> *QED, The Strange Theory of Light and Matter*, (London, 1990) 9.
>
> Where did we get that [Schrödinger’s equation] from? It’s not possible to derive it from anything you know. It came out of the mind of Schrödinger.
> *The Feynman Lectures on Physics*

(..via this site)

7. **QWERTY**
January 10, 2006

THE FEBRUARY NOTICES OF THE AMS ARE FULL OF DOO-DOO TOO.

THERE IS NO MATHEMATICAL CONTENT WHATSOEVER. THE INTERVIEW WITH LAX IS ODIOUS AND VILE. REMARKS LIKE “INCIDENTALLY, I HAVE A VERY GOOD MEMORY” ARE SELF-SERVING AND CRASS.

8. **Who**
January 10, 2006

Dear QWERTY
to save me having to walk over to the math library, since i don’t have a subscription (not an AMS member), could you say very briefly what the articles are about that you are objecting to?

Based on impressions gathered in the 1970s I have a very high regard for Peter Lax. It would be amusing and surprising to find him guilty of making doo-doo with no mathematical content whatsoever, though less astonishing if he were merely odious crass and vile, since that is a familiar part of all our human makeup.

Anyway please be a little more specific for the edification of all us AMS non-members. Unless you are totally out to lunch, what are you talking about?

9. **woit**
January 10, 2006

Qwerty and Who,

Peter Lax and the Feb. AMS notices is off-topic, discuss this elsewhere. The AMS site does require you to set up a web-account to access it, but I believe much of the content (like the Notices) can be accessed even if you are not an AMS member.

10. **Who**
January 10, 2006

I stand corrected, Peter. I should never have inquired of QWERTY what in the world he was talking about (so pungently and with such feeling.)

11. **Haelfix**
January 10, 2006

Actually Einstein liked Dirac's formalism, particularly when it was boosted into field theory. What he hated was vanilla quantum mechanics with no special relativity and the collapse principal, he felt that limit was very poorly defined.

People often say he was off on a island, yet very often great physicists would go to see him to talk about interpretational issues in QM (this years after Copenhagen was well entrenched) and he could still with the power of his mind alone convince them otherwise (see Bohm for instance).

Note, he was one of the first people to be supportive of the path integral approach, this near the end of his life when everyone was praising Schwingers methods

12. **StrungOut**
January 10, 2006

A bird once whispered in my ear that the Gross-Perry monopole solution was first found by Sorkin.

13. **MathPhys**
January 10, 2006

That's a bird with a good memory. 1982? 1983?

14. **Fabien Besnard**
January 11, 2006

"Einstein had his own share of crackpotism… "

Geniuses and crackpots share an enormous amout of self-confidence that allow them to keep on following their ideas when everyone else tell them it's a dead end. In fact I see two main differences : the first is that geniuses try to overcome the difficulties, where the crackpots generally just ignore them.

The second can be seen in what Feynman said :

"We have a habit in writing articles published in scientific journals to make the work as finished as possible, to cover up all the tracks, to not worry about the blind alleys or describe how you had the wrong idea first, and so on. So there isn't any place to publish, in a dignified manner, what you actually did in order to get to do the work."

Crackpots can't go through this cleaning process. Their ideas tend to remain muddy.

So what I would like to convey is this : someone is not a crackpot because he has this idea that sounds crazy, like Einstein's ideas about QM or nuclear interactions, but because of his behaviour in case he is opposed to a counterargument or an empirical difficulty. Did Einstein push his own ideas against empirical evidence ? You can't accuse him of crackpotism just because he felt the urge to follow his inner track, his "good passions".

15. **anon**
January 11, 2006

"We have a habit in writing articles published in scientific journals… to cover up all the tracks, to not worry about the blind alleys or describe how you had the wrong idea first, and so on. …" – Feynman

"Crackpots can't go through this cleaning process. Their ideas tend to remain muddy.…" – Fabien

So covering up errors en route to success is called "cleaning up"!

16. **Juan R.**
January 11, 2006

Anon, Ugo, Chris W, Haelfix, and Fabien Besnard,

Einstein rejected both quantum and probabilistic nature of QM and becomed a member of the deterministic continuum school togheter Schrodinger. Born, Heisemberg and others belonging to the nondeterministic discontinuum (matric formulation) school. Einstein named QM nonsense, consider the Dirac formal formulation of QM, ugly and irrelevant for the future of theoretical physics, and ignored experimental success of R-QFT, atomic, and nuclear physics.

There is a lof of hype around Einstein figure somewhat comparable to current hype about string theory. Historical facts are very different from current believings…

A bit more of history,

> Einstein never had a good word for the relativity version of quantum mechanics knows as quantum field theory. It successes did not impress him.[41].

[Abraham Pais. Subtle is the Lord. Oxford University Press, New York, 1982.]

> The quanta really are a hopeless mess.

[Albert Einstein, On doing Quantum Theory calculations with Pauli p58]

> Einstein thinks he has a continuous field theory that avoids 'spooky action at a distance', but the calculation difficulties are very great. He is quite convinced that some day a theory that does not depend on probabilities will be found.

[Albert Einstein to Max Born, p158 Mar 1947]

Einstein did not obtain special relativity or general relativity by their own means as more recent historical studies prove. In a recent 2005 meeting at the American Physical Society, Renn was quoted saying

> I had personally come to the conclusion that Einstein plagiarized Hilbert.

[Winterberg, F. On The Hilbert-Einstein Priority Dispute]

Renn is the director of the Max Planck Institute for the History of Science.

In a recent work in the American Mathematical Society

> His books on Maxwell theory contain the germs of special relativity and led him to analyze, correct, and name the Lorentz transformations. Poincaré published in 1905 a note (followed by an extended memoir) on the dynamics of the electron, containing the whole mathematics of special relativity. Historians of science still passionately discuss the priority between Einstein and Poincaré, and if one follows some recent publications, one might conclude that Hercule Poireau might be the only one able to uncover the whole story. Curiously, the mathematician Poincaré reached relativistic kinematics via Maxwell's electromagnetic theory,
> while the physicist Einstein used an axiomatic
> method. But it is unquestionable that Poincaré anticipated
> the so-called Minkowski space-time.

[Mawhin, Jean. Henri Poincaré. A Life in the
Service of Science. NOTICES OF THE AMS (Oct 2005) p1039]

But some very recent historical discoverings suggest/prove that Einstein copied worked of others. From the Wiki

> Many believe that Einstein and his wife were both aware that the famous Frenchman Henri Poincare had already published Relativity, a few months before Einstein.

[http://en.wikipedia.org/wiki/Albert_Einstein]

One of lattest i know is the historian of science Stephen G. Brush who in his recent 2001 Physics Today review of "Einstein, Picasso: Space, Time, and the Beauty That Causes Havoc" recognized that Einstein was not sincere in his priority issues

> Einstein read Poincaré's Science and Hypothesis (French edition 1902, German translation 1904) and discussed it with his friends in Bern. He might also have read Poincaré's 1898 article on the measurement of time, in which the synchronization of clocks was discussed–a topic of professional interest to Einstein as a patent examiner.

All of this is of direct relevance to the understanding of the history the current status of string theory and social issues of stringy community, since there is a great paralellism between Einstein and string theorists. In fact,

1) String M theorists ignore experimental predictions or discrepancies of many orders of magnitude, just as Einstein ignored success of R-QFT.

2) String M theorists ignore any advances in disconected fields just as Einstein ignored nuclear physics by a 'beatiful' idea: search of unification via classical EM+GR

3) It has been denunciated in several sites including this blog that string theorists copy the work of others and after rename it as new. I am not wrong M. Pittaken has said here in ocassions that some of mathematical ideas usually attributed to M theorists were formulated earilier by mathematicians, even if mathematical relevant papers are not cited and names ignored.

And if i remember correctly, Peter Woit has often shown here that mathematical insights usually claimed to be discovered in the string theory field were already known or belonging to the mathematical side.

—

Juan R.

Center for CANONICAL |SCIENCE)

17. **Juan R.**
January 11, 2006

I have an interesting question for you. Woit said,

> He [Gross] concludes that Einstein was wrong to refuse to accept quantum mechanics and to ignore nuclear and particle physics, but that he was right to try and unify gravity with the other forces […]

In [Albert Einstein to Max Born, p158 Mar 1947] one read

> Einstein thinks he has a continuous field theory that avoids 'spooky action at a distance', but the calculation difficulties are very great. He is quite convinced that some day a theory that does not depend on probabilities will be found.

As it is well-known Nobel laureate Sheldon Glashow has compared many times the real failure of string theory to the failure of Einstein unified field theory. Now subsitute in my above Born quote the keywords "Einstein", "continuous field theory", etc. such as

> Gross (or Witten or Kaku…) thinks he 'has' a unified string M-theory theory that avoids 'the ugly standard model more GR', but the calculation difficulties are very great. He is quite convinced that some day a unified theory will be found.

Yes, Einstein was rigth in his attempt to unify forces (a topic that Einstein learned from Hilbert axiomatic attempt to unified theory of gravitation and EM as proved in correspondence Einstein to Hilbert of 15 November 1915) but only on that Einstein was right. In the rest, Einstein was VERY wrong.

Seeing the impressive difference between Einstein UFT and current way of the Standard model or string M-theory, is not the parallelism between above quotes Born original and modified a clear signal that probably a new 'Gross' would say by the 2050 some like next statement?

> 'New Gross' concludes that string theorists were wrong to refuse to accept string theory was wrong and to ignore other alternatives to quantize gravity and explain the Standard Model, but that they were right to try and develop a unified theory of all known phenomena […]

What is your personal opinion?

–

Juan R.

Center for CANONICAL |SCIENCE)

18. **anon**
January 11, 2006

Juan,

Your point is very relevant. People wants to find a unified theory if it exists. How much longer should they try to achieve that goal by flogging a dead (actually stillborn) horse, "string theory".

I agree with the views of many here, namely that it would be more dignified for science to stop flogging string theory in the hope of making it produce numbers, and to focus on other approaches.

19. **Fabien Besnard**
January 11, 2006

Juan R, if Einstein really plagiarized Poincaré and Hilbert, why haven't they protested (as far as I know) ?

20. **woit**
January 11, 2006

Please, enough of the off-topic material about Einstein, e.g. "Was Einstein a Plaigiarist?". I don't know about anyone else, but 2005 has left me thoroughly sick of endless Einstein discussions and I wish that a moratorium on some of this stuff could be instituted for 2006. If anyone has anything new and interesting to say on the topic at issue here, the relation between Einstein's work on unification and current ideas about unification like string theory, that would be great.. But given the immense of amount of attention this topic has had over the last year, I find it hard to believe people can come up with something new to say.

21. **V H Satheesh Kumar**
January 11, 2006

Hi

In fact, it is my first visit to ur site, n seems tht u have got pissed off with all new advances in Physics like Strings,Branes, Supersymmetry, Cyclic universe and kinda stuff. Then wht do u think is relevant to study? Why dnt u suggest something?

22. **dan**
January 11, 2006

Since Universities employ string theorists, and string theorists cite string papers, Witten Greene and others have a high H-index.I personally agree with Peter Woit and string theory critics that alternatives, such as LQG, should be pursued vigoriously, and that Universities should hire more LQG-theorists. Were this to happen, I would imagine the H-index of LQG-theorists would rise.

23. **woit**
January 11, 2006

Kumar,

If you look at my web-site or on the arXiv you'll find a paper by me explaining ideas that I think are a promising way forward. But I have never wanted this to be a site just promoting my own personal research. I do try and mention interesting new ideas when I learn about them, but unfortunately I think the current situation is that there are few good new ideas around. The people pursuing strings etc. are not stupidly doing so despite there being obviously much better things to work on. Under the circumstances, my view is that people need to be encouraged to come up with new ideas, and one thing that is stopping this is the overwhelming hype being used to promote failed ideas and keep people working on them.

24. **anon**
January 11, 2006

"…unfortunately I think the current situation is that there are few good new ideas around. The people pursuing strings etc. are not stupidly doing so despite there being obviously much better things to work on.…"

Loop quantum gravity is the best known alternative, although the physical picture (spin foam vacuum) is less tangible to most people than strings (albeit extra dimensional ones!).

I think you have to accept that to get funding you have to explain physically what loop quantum gravity says, but nobody does that.

25. **Shantanu**
January 11, 2006

Peter what do you think of Narlikar's article in the same issue(especially the last para?) Do you agree that research in non-standard cosmology should also be supported/encouraged even today?
Probably you already know this, but the rivalry/bitterness between steady state theorists and supports of the Big bang model (especially before 1965) was somewhat(actually much worse) than between string theorists and people
pursuing other approaches to QG.

26. **ark**
January 11, 2006

I wonder if somebody was thinking about the fact that mathematics exist for at least 2500 years and is still thriving while physics exist for about 350 years or so and is in swampland already. Is there in physics literature some kind of equivalent of Bourbaki-type style monographs detached from particular individuals? Or physicists-all of them- only like to glorify themself, to listen only to themself, to promote only their own works -just like in any other religious sects. In my opinion, instead of thinking about who did what, when and how, may be one should think about what, after all, will survive, say, 2500 years (e.g. like a Pithagorean theorem) in physics.
David Hilbert asked A.Einstein to become one of the editors of the Annals of Mathematics and Einstein had accepted his invitation. Everybody can find in the library

volumes of Annals which have Einstein as one of the main editors. I suppose, this fact should put to the rest the dispute about relations between Einstein and Hilbert. As for the path integrals, they vere discovered by L. Bachelier in “Theorie de la speculation”, Ann.Sci.Ecole Norm.Sup.17 (1900) pages 21-86. Incidentally, H.Poincare was one of the members of the PhD committee when Bachelier was defending his dissertation, later published in Ann.Sci. Ecole…Hence, one can go on and on about who did what and when…E.g. the paper by Cameron and Martin on path integrals have been published in Annals of Mathematics in 1944 (just take a look at the math.sci net!). Given that Mark Kac was at Cornell at the same time as Feynman, introduction of path integrals into physics later on looks quite natural….. And so on and so forth…

27. **woit**
January 11, 2006

Shantanu,

I'm fundamentally a particle physicist, of a mathematical sort, not at all a cosmologist. So I don't have any definite opinions about what cosmologists should be doing, or about what value there is in "non-standard" cosmology.

28. **Walt Pohl**
January 11, 2006

ark: *Physics* is in fine shape. It's *particle physics* that has run into difficulty. And there are thousands of works where physicists describes the works of other physicists (what do you think is in all of those textbooks?).

29. **Shantanu**
January 12, 2006

Peter, today cosmology is very much intertwined with particle physics
and vice-versa. for example (though all of this has been dicussed here):
1) the most probable candidate for cold dark matter is the supersymetric
neutralino and/or axion. (lots of experiments currently searching for WIMPs
and/or axions would thus solve two problems in particle physics AND cosmology). Also notice the plethora of papers on how one's favorite
model beyond SM leads to a dark matter candidate. I have seen lots of
talks that one of the motivations behind building the linear collider is to investigate the properies of dark matter.

2) the first evidence for number of "active " neutrino flavors came from theory
of big bang nucleosynthesis and observed Helium-4 abundance.

3) Evidence for non-zero neutrino mass implies that amount of density contributed by neutrinos is as much as the visible stars.

4) Concept of cosmic inflation , which is determined by potential energy of
a scalar field governed by particle physics.

I bet that every participating particle physicist must have worked or thought about (1) and (4) above

By non-standard models, I meant models of cosmology based on alternate
theories of gravity (such as B-D theory and others), or alternate theories of
gravity which explain dark matter (such as MOND)., or things like steady-state
theory of cosmology which rely on slightly different points of view.
do you think studying these could lead to insights in particle physics ?

or are you sufficiently confident in current standard Big-Bang picture
of universe consisting of ~ 23 % dark matter (for which there is no laboratory
evidence but lots of candidates from particle physics) ~70 % dark energy
(for which we have no clue), the concept of inflation (for which we have no idea
about the energy density of scalr field which caused it) and 5 % baryons(again
which we have no idea on how they came about) ?

30. **woit**
January 12, 2006

Shantanu,

Sure, I'm aware of the ideas relating cosmology and particle physics that you describe. But having an informed opinion about the kinds of alternatives to GR or to the standard cosmological model that you mention would require knowing much more than I do about exactly what the astrophysical evidence says. I'm just not expert enough in cosmology to know precisely how strong the evidence is for various aspects of the standard picture, or exactly what this evidence rules out and what could still be consistent with it.

From everything I've seen the standard cosmological model looks very solid and there isn't any unambiguous evidence that I'm aware of for alternatives. But what I've read are generally expository articles for non-experts, and I'm well aware from my experience with string theory that such things can sometimes gloss over problems, and it requires a more serious level of expertise to understand exactly what is really known. Since I don't have this level of expertise, my opinions aren't worth much on this topic.

31. **dan**
January 12, 2006

Peter,
as a particle physicist, i am curious as to whether you think dark matter could plausibly be anything other than SUSY-particles such as the axion or neutralino? an alternative to LHC are detectors which hope to detect dark matter passing through earth.

32. **Alejandro Rivero**
January 12, 2006

Re #23, #24, #27: It seems that the problem is not about having new ideas, but about the energy scale where we expect such ideas to be. It is very interesting that the most suggested alternative to strings is LQG, no GUTs. I believe this comes because of the "effective theory" view of particle phenomenology, so that people aspiring to Unification disregards anything near the standard model.

33. **Jackson Riszemoi**

January 12, 2006

LOL, Juan R., you call a wikipedia article "a historical discoverings suggest/prove that Einstein copied worked of others."?

34. **woit**
January 12, 2006

Dan,

Dark matter could be all sorts of things, we know virtually nothing about it.

Looking for dark matter other ways is not an alternative to the LHC. What the LHC is going to be important for is understanding electro-weak symmetry breaking.

Others:

I've just deleted a bunch of comments of people going on about their favorite non-standard idea about cosmology. This is off-topic: I'm no expert on this, and the people writing these comments don't seem to be either. I don't see the point of having an uninformed discussion on this topic here. Again, this is not sci.physics.

35. **nitin**
January 12, 2006

Hi Pete
I have just started a blog (which is going to be a hell of a challenge for me). I am a student in Physics from Mauritius. Maybe, if you are so inclined, you could add a link to my blog (http://pioneerthis.blogspot.com/) on yours. That would be kind.
Thanks
Best,
Nitin

36. **Dissident**
January 12, 2006

For an informed discussion, you could do worse than visiting the astro-ph section at Cosmocoffee:

http://cosmocoffee.info/viewforum.php?f=2

37. **woit**
January 12, 2006

Hi Nitin,

I've added a link. Good luck with the blog!

38. **robert**
January 12, 2006

However off topic pre/re discoveries might be

"EW Montroll, Communs. Pure Appl. Math. 5, 415 (1952)."

merits a quick look.

39. **ark**
January 12, 2006

Robert, I believe, most of Elliot Montroll's papers merit more than a quick look. They are classic examples of how papers should be written. But, again, are you aware of some kind of detached from particular individual Bourbaki-style monograph in physics? As you probably know, in books by Bourbaki there is always some historical short discussion regarding to how this or that particular idea dicussed in the main text had evolved. Sometimes this is done in the form of exercises. I bet that such an idea will newer cross physics community mind since they love themselves too much to share freely ideas with others. Most typically, they cannot agree on anything between each other even without such a free sharing of ideas. The discussions above my comment are good examples. Is this a sociology of theoretical physics or it comes along with the individual who chooses theoretical physics as a profession ? The great Sh.Chern at the begining of his collaborative work with Ellie Cartan (which happened to be at the very height of the physics sucsesses in quantum mechanics, etc) had asked his mentor if it is worthy to do something in physics to which he got a reply : "stay away from this". So Chern stayed away… letting swampland to be developed by others. Any comments?!

40. **anon**
January 13, 2006

"… they cannot agree on anything between each other even without such a free sharing of ideas."

Mathematics is less controversial because the distinction between fact and speculation is clearer. In physics there are always different interpretations of the facts, or a scatter in the data, which leads to controversy before new ideas become accepted. Generally in mathematics, once a theorem is proved, that's it…

41. **ark**
January 13, 2006

Dear Anon, this fact is well known to me as it is known to you. This is what always you can hear from physicists (I am one of those too!!). Fact of the matter is that if you look a bit closer at things, theoretical physics as much a discipline as it contains a meanigful mathematics. Hence, the controversy lies, apparently, in use of this or that mathematics to achieve the same physical goal. But, as you know, you can go from Chicago to Washington DC via Seattle or you can go straight. As far as the final destination is concerned, both ways will lead there. Hence, the intitial task of finding the only ONE right path cannot be achieved. Yes, mathematicians prove theorems and that is it…But then, they pay attention to what had been proven by others. To come to the point, I would like to rase the following question. Suppose, that in physics somebody published an important result in some journal, say X, will this imply that those who submit their papers to journal Y should be aware and comply with the result published in X ? Or should they ignore result published in X and try to push their own agenda in journal Y ? Mathematicians will comply if the result is proven rigorously while physicists will ignore. Othervice, why it is so important to publish things in PRL or Nature ?! etc. If you do not publish your work in the "right" journal it will be ignored by those who manage to publish their works in such journal. Hence, what is the use for the rest of journals which are "wrong" from the beginning ?! Controversy in physics is synonimous with the trend NOT to be precice, NOT to pay attention to work of others (does not matter how rigorous it is), to push ones own agenda without any regard to even one's own prior work which can be easily forgotten, abandoned, etc. Who among respectable mathematicians had published 650 or more papers during their lives? Does all these 650 papers contain rigorous proofs ?! Is it possible for the very same person to ignore its own proven results in mathematics ?

42. **anon**
January 13, 2006

Dear Ark, in physics many accepted ideas and laws at any one time are always controversial, so the mainstream is decided to a far greater extent by the whims of the leaders and by fashions, than by hard proof. Popper even says that a hard proof (such as the physical proof for the law of buoyancy given by Archimedes) is not useful, because mainstream physics should be fallible. This is the source of the bickering in physics. In mathematics, people can be nicer to one another. Physicists are fallible, mathematicians aren't.

43. **ark**
January 13, 2006

Dear Annon, if you've read my previous comments, you've noticed that I have indentified physics(theoretical physics in particular) with various religious sects. I've provided some facts (other than reference to Popper) justifying my claim. However, I should say that mathematicians are also NOT saints whatsoever! We are talking only about the degree to which one can convert science into religion. Mathematics has proven record that the system of values was chosen correctly from the outset. Physics also have proven record of fighting between Newton and Hook, between Newton and Leibnitz, etc… So, physics had deep roots in religion and Newton in some circles known more for his religious writings than for his scientific…Clearly, if you feel that you are religious person, then follow the pre selected leaders thus going to a pre selected sect. But, please, do not make claims that this has something to do with science. I am talking not to you personally but to those who know to what extent the existing system of values is working for their benefits. Surely, unless some REAL emergency appears at the horizon, things will flow as usual non stop.

44. **anon**
January 13, 2006

Dear Ark, the only things I can reply to are those I've some experience of. In mathematics, alternative theories are welcome if they are useful. In physics, alternatives are fought with rigor. One insidious method of suppressing alternatives is to say that the authors must be ignorant of the details and beauty of the mainstream model, while another is to dismiss the alternative as "speculative" (when in fact it is the maintream model that is overly speculative or even untestable, resulting in the newer model which is not so speculative and is more testable). Outsiders would imagine that such hypocrisy is easily exposed, but it isn't, because the media seeks authority on scientific matters, which means only listening to the mainstream. So the farce just goes on.

45. **ark**
January 13, 2006

Anon, I would be cautious talking about farce. One cannot undo the human nature…One cannot fight against what is intrinsic. However, just like with different countries, they are like people-all different and all the same. If there is something proven to be good in one country why not to borrow the idea and not to bring it to another country…Have you seen this ever happen? For the same reason, physics will stay away from mathematics even though all the ingredients of how to make things to work better are actually known.

46. **anon**
January 14, 2006

Dear Ark, yes in the end useful bits and pieces will have to be put together. The mainstream will first resent any interference from "outsiders" who try to change their foundations, then eventually the mainstream will make a big deal out of how "kind and generous and open-minded" they are to eventually allow publication or discussion of a radical suggestion. The farce is the double standards; they publish worthless speculation, but are scared numb (or become "angry") about "radical" ideas which are less speculative than the string theory ideas they already have!

47. **Juan R.**
January 14, 2006

Jackson Riszemoi

Rather irrelevant question. Reply is, of course, NOT!

Fabien Besnard

Correct, as far as YOU know…

Any other comment in my sugestion that string theorists are playing the role of Einstein, i.e. searching in the wrong way?

—

Juan R.

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48. **xpinor**
January 14, 2006

For more uncritical promotional material on string theory, here are two things from UC Davis. They have something there called the "High Energy Frontier Theory Initiative (HEFTI)", and on their website you can read a report written by an external committee for the Dean at Davis…

Now, isn't it rather interesting that Gunion puts **this** report on his website…

49. **xpinor**
January 14, 2006

A while ago someone here suggested a potential analogy between the string theory establiashment and Enron. I have been seeing it this way for quite a while. I wonder whether string theorists might be found legally (and financially) liable for fraudulently soliciting funding if they were to be proven to have known (or not honestly believed) that string theory cannot describe nature. If so, how could this be brought effectively to the attention of prosecutors at the office of the Attorney General or the respective office in other counties ?

50. **ark**
January 14, 2006

xpinor, can you, please, provide an exact web link to HEFTI. I was able to find on GOOGLE 34 entries none of which match what you've said. I assume, you are not referring to L.Randall public lecture…

51. **woit**
January 14, 2006

ark,

I think HEFTI at Davis is more a plan than a reality, see

http://higgs.ucdavis.edu/gunion/theorygroup.html

xpinor,

While I've seen a certain amount of dishonesty and unethical behavior by certain string theorists in regards to how they have responded to criticisms I've been making of the theory, I think the great majority of them honestly believe in what they are doing, or at least honestly can't think of anything better to do. I don't think there's any case for intentional fraud to me made here.

52. **xpinor**
January 14, 2006

ark,

The report I was refereing to is at:
http://higgs.ucdavis.edu/gunion/particle_report.pdf

53. **xpinor**
January 14, 2006

ark and Peter,

Lisa Randall made that remark about the analogy to the
administration not losing credibility by making up evidence. A smoking gun? Sufficient evidence to justify an investigation, maybe?

54. **Aaron Bergman**
January 15, 2006

Yes. That's precisely it.

In fact, we need a congressional committee.

No. This is international. We should get the UN involved.

Oy vey.

55. **Charles Moeller**
January 15, 2006

On first principles, the continuous may contain (a subset which are) the discrete (quantized values) but never the reverse. A strictly quantized regimen can't be modified enough by expansion or subdivision or by any other means to become continuous. Relativity is couched in the Calculus, which is a continuous mathematics, suitable for continuous space and time. QM is, by definition and by practice, a strictly quantized system. So QM will never encompass Relativity, whereas Relativity could encompass QM.

56. **ark**
January 15, 2006

Thank you very much xpinor for a useful link. As to what encomass what, this is very much of the same quality as wether Sun is rotating around mother Earth or Earth is rotating around Sun. Thanks God, hopefully, nobody is going to be burned alive for taking the sides in this futile dilemma.
Physics is NOT about what comes from what but about how new is connected with old. Correctness of General relativity can be proven in many ways as much as correctness of quantum mechanics. When I say "correctness" I mean 2 things: the predictive power and agreement with earlier, older theories.These older theories work well within some known(agreed upon)domain.There were many serious papers in the past in general relativity which questioned existence of gravitational waves and yet if one looks at GOOGLE one finds that gravitational waves are going to be discovered the next day…Keep in mind, that the whole business of quantum gravity (and, hence, string/brane theory) is about how to quantize gravity correctly. But for this, gravitational waves should be solidly discovered ! But they are not ! This is a typical case (characteristic for string theoreticians as well as of others in physics) of sincerely believing in things which may or may not happen ever. If this is not a religion, then what this is ?! Surely, for the progress to exist, one should believe in something strongly. But this is a personal business (e.g. read Einstein) and cannot be a subject of debates. In mathematics also there are conjectures which need proofs. Nevertheless, general public is NOT involved in debates about correctness/validity of mathematical conjectures ! Why then this happens in physics where, some people spread around false ideas that new string theory requires new not yet developed mathematics. My own experience tells me just opposite: whenever there is a serious use of established mathematics in string theory,there are at least some results of some value. In the opposite case we all know what happens…

The First Evidence For String Theory? Not.

January 12, 2006
Categories: Uncategorized

Over the last couple years there has been a large amount of hype about cosmic strings, including press releases from Santa Barbara, and a story in New Scientist about The First Evidence for String Theory?.

The Santa Barbara press release from June 2004 concerned a paper by Polchinski and others about potentially observable fundamental strings of cosmic size. It stated that "LIGO… could provide support for string theory within two years." There are five months left for this prediction to work out.

The New Scientist story was about an astronomical object optimistically given the name "CSL-1" (for Capodimonte-Sternberg-Lens candidate), that supposedly might be a galaxy lensed by a cosmic string, causing it to appear doubled. Of course the much less exotic and much more likely possibility was always that it was just two similar looking neighboring galaxies. Recently the Hubble space telescope was used to take a closer look at CSL-1, and as reported here and here the Hubble image clearly shows that it's not a cosmic string, just two nearby galaxies.

Comments

1. **anon**
   January 12, 2006

   Peter, can't you stop reporting string theory hoaxes?

   O.K. Pauli made a point of exposing the empty claims of Heisenberg's grand unified theory and many others, but we all know string theory doesn't hold water now!

   Just because the string theory band is still playing loudly, doesn't mean anything. (The band on the Titanic continued playing until the end.)

2. **Dumb Biologist**
   January 12, 2006

   It's interesting that the initial hype was over the image being evidence for either cosmic strings OR string theory. Given the Borg-like ability of string reasearch to lay claim to virtually any and all phenomena hinting at new high-energy physics (as low-energy manifestations of some-or-other version of string theory, I guess), I'm surprised CSL-1 wasn't touted as potential evidence of both at once. Then again, maybe it was…

3. **woit**
   January 12, 2006

   Dumb Biologist,

   The hype was for both. It will be interesting to see how much this effect this negative result will have on the hype going on in this particular area.

4. **Quantoken**
   January 12, 2006

   Peter:

   Predictably this negative result will have virtually zero effect on the enthusiasm of cosmic string theorists. The hype will continue on, so will the search.

   One has got to understand the universe is **pretty big** and there are more number of galaxies than the number of sand grains on a beach. You can find anything you are looking for out of this huge collection of samples. Look a little bit harder and I am sure you will find many more of such similar galaxies in close proximity so it's predictable there will be many more CSL-x type of cosmic strings "discovered" and be hyped by string theorists.

   This time in this particular case HST is **JUST** slightly sensitive enough to be able to make a small distinction between the two close by galaxies. Interesting question to ask is what if it doesn't, or what if the CSL-2 or CSL-3 is slightly beyond HST's the ability to resolve. How long will the hype continue on. Or are they going to claim "String Theory Confirmed by HST"?

   Quantoken

5. **Adrian H.**
   January 12, 2006

   anon said "Peter, can't you stop reporting string theory hoaxes?"

   I don't think there is any danger of Peter acceeding to this request, but I'd like to vote for him continuing as he is. It is very useful to have an honest broker in a field where there is so much…well, I guess we've all agreed to call it 'hype'.

6. **matt**
   January 12, 2006

   What's the difference between a "cosmic" string and a "regular" string?

7. **woit**
   January 12, 2006

   Matt,

   Cosmic strings are strings of macroscopic size, perhaps even big enough that you can see them or their effects in a telescope. It turns out that in some kinds of gauge theories with Higgs fields, there are topologically stable string-like configurations of the classical fields (basically solitons). You don't get these in the standard model, but you do get them in some GUT theories. If they were produced in the big bang, they might still be around since they can't decay for topological reasons. But most kinds have been ruled out since if they existed they would have had effects on cosmology that disagree with observations.

The standard fundamental strings are quite different. They are fundamental dynamical variables, not classical configurations of a Higgs field, and their size is generally assume to be the Planck scale (or, if you believe in some extra dimensional models, they could be as big as the TeV scale, but this is still extremely small). In recent years, Polchinski and others have found variants of string theory where the fundamental strings can get to macroscopic sizes, and there has been a lot of hype promoting these models as ones where strings would be experimentally visible. There's no experimental evidence whatsoever that such models have anything to do with the real world.

8. **Luboš Motl**
   January 12, 2006

   Dear Peter,
   half of what you write is a complete nonsense.

   The fact that strings – if they carry enough energy – can be macroscopic has been appreciated for several decades, certainly since the mid 1980s and probably since the early string theory in the late 1960s. It's just you and your naive readers who has infinite problems to realize that the shape and size of a string is a dynamical observable.

   There's a lot of experimental evidence that these models have something to do with the real world because there is a lot of evidence for the Standard Model and gravity which is implied by these models.

   But your comments are addressed to likes of Quantoken – who obviously thinks that all string theorists are really "cosmic string theorists" 🙂 – so the fact that the quality of your comments is 3 orders of magnitude beneath the quality that would be acceptable in science does not really matter.

   Best
   Luboš

9. **woit**
   January 12, 2006

   Lubos,

   What I wrote is obviously not an exhaustive treatment of the subject, but it is perfectly correct. Your delusion that string theory implies the Standard Model is just wishful thinking.

   In the years before 1984, there was quite a lot of discussion of string-like solutions in gauge theories and whether they might be produced in the early universe, discussions which I was well aware of since I was working on topological aspects of gauge theories. No one was talking about fundamental strings in this context, actually virtually no one except Green and Schwarz was talking about fundamental strings at all.

   In 1985, Witten wrote a paper about the possibility of fundamental strings produced in the early universe reaching cosmic sizes as the universe expands, but he identified three problems with the idea. I'm no expert on this, but my understanding is that it is those three problems that Polchinski et. al. claim to be able to overcome.

   Again, there's not a shred of evidence for the particular scenario Polchinski et. al. work with, as far as I can tell it was developed purely in order to overcome the problems Witten identified and allow cosmic size strings to be produced and survive.

10. **Dumb Biologist**
    January 12, 2006

    Oh well. Though I can't quite get my head around the idea of a fundamental constituent of matter blown up to cosmic scales, I have absorbed as much as my pea brain can from sundry popularizations about cosmic strings, and found the idea of some hyper-dense filament of primordial matter/energy floating around out there to be pretty wild and fun. Too bad this ain't it.

11. **Urs**
    January 13, 2006

    There are some statements which are hard to prove, but easy to disprove. Others are hard to disprove but easy to prove.

    A statement of the form

    "For all x in S we have A(X)".

    Is usually harder to prove than to disprove. Proving it requires knowledge about all x of S. Disproving it requires knowledge only about one x.

    A statement of the form

    "For at least one x in S we have A(X)".

    Is usually harder to disprove than to prove. Disproving it requires knowledge over all x of S. Proving it requires knowledge only about one x.

    I gather this is well known to philosophers of science.

    The situation with string theory is currently that it makes statements of the second kind.

    Up to some well-known caveats, the claim of perturbative string theory is something like

    "There exists at least one x in the set of real numbers such that at an energy scale of x eV we see stringy effects."

    As Peter is fond of pointing out, this is hard to disprove. You can check for higher and higher x (and we cannot check really high). But finding nothing at these x does not disprove this statement. Simply because it is a statement of the form "there is at least one x…".

    Proving the statement (if true) would in a sense be much easier. Observe at least one stringy effect and you are done.

    Driven by the desire to make contact with experiment, people like Polchinski began scanning the theory for possibities of such stringy effects that might be observed.

    They argue that one such possible effect could be cosmic fundamental strings.

    It should be noted that nobody is claiming that string theory demands that we observe cosmic strings.

Just like GR does not demand that we observe a Goedel universe, or Einstein's instable static solution.

But if you want to prove an existence statement of the form "there is at least one x in S" what you usually do is scan the space of all elements of S for those that might be candidates which make the statement true.

So people investigating stable cosmic fundamental strings could be said to be studying the claim:

"There is at least one x in the set of stringy effects such that x can be observed with contemporary technology."

Disproving this is hard. Proving it (if true) would be easier. It suffices to find one example.

So people are checking plausible candidates for x. They argue one good candidate is "x = cosmic fundamental string".

I guess one could have a scientific argument about if this is a "good" candidate or not. But the simple fact is that, indeed, if we did observe a cosmic string which could be identified as a "fundamental string" the above statement would be proven.

I don't think this is likely. But I do think it is scientific.

Of course, one can state many existence theorems which are highly unlikely but nevertheless very hard to disprove.

"There exists at least one extraterrestial life form with a genome the same as terrestrial groundhogs", I claim.

You don't believe that? Prove me wrong!

So if you feel the same about the statement that there is at least one observable stringy effect, there is nothing much one can do about it except that you should feel free to study some other theory.

But whoever does see a chance that a certain existence statement is true should, in order to prove it, try to find an example that solves it. That's what people interested in cosmic fundamental strings are doing.

You may feel that it is hopeless and that money should be spent elsewhere. Fine. But it is not non-scientific.

12. **woit**
January 13, 2006

I haven't said that thinking about cosmic strings is non-scientific (that applies to something else Polchinski promotes, the anthropic landscape). What I object to is the way this work has been over-hyped. Working on something extremely speculative, something for which you don't have any evidence and that is very unlikely to really exist is fine, but I don't think you should be issuing misleading press releases about it.

13. **D R Lunsford**
January 13, 2006

I don't know why but this strikes me as being very funny! I couldn't help but think of the Star Trek movie where Kirk dies saving the Universe from a superstring.

-drl

14. **anon**
January 13, 2006

That's sad. I always thought superstrings were at least nice fiction, but horrible murderers!

15. **Urs**
January 13, 2006

> I haven't said that thinking about cosmic strings is non-scientific (that applies to something else Polchinski promotes, the anthropic landscape).

While it is hard to say what "anthropic landscape" really refers to, I could make a similar discussion as the above on cosmic string solutions but with respect to the space of all global solutions. It's not a priori and by itself non-scientific to study the space of solutions of a theory.

But I agree that there tends to be too much hype about things too little understood.

16. **Juan R.**
January 13, 2006

cosmic string = superstring?

Sure? 🙂

—

Juan R.

Center for CANONICAL |SCIENCE)

17. **Dissident**
January 13, 2006

DRL wrote: "I couldn't help but think of the Star Trek movie where Kirk dies saving the Universe from a superstring."

Oh my God! The Nexus ín ST:Generations (1994)!

http://www.startrek.com/startrek/view/series/MOV/007/synopsis/87.html

It wasn't a superstring, it was quite clearly a brane intersection. So THAT is where Randall got the braneworld idea!

I hereby nominate writers Brannon Braga and Ronald D. Moore for the Nobel Prize in physics.

18. **dan**
January 13, 2006

Urs, "Driven by the desire to make contact with experiment, people like Polchinski began scanning the theory for possibities of such stringy effects that might be observed."

Wouldn't one of the most impressive way for string theory to do this, would be as Lubos Motl says, to show you can derive most, if not all, of the particles of the standard model from string tension?

BTW: Lubos states it is possible to derive the standard model from first principles of string theory, Peter says it is not. Who is right on this specific issue?

19. **Urs**
January 13, 2006

> Wouldn't one of the most impressive way for string theory to do this, would be […] to show you can derive most, if not all, of the particles of the standard model from string tension?

It would certainly be impressive. It would also be impressive to have a theory which would allow to derive most, if not all, distances of the planets from our sun. We do, however, have no particular reason to assume that such theories exist.

We may hope such theories exist. Kepler hoped a theory exists which explains the distances from the planets to the sun as given by spheres inside perfect polyhedra.

Some people hope that the parameters of the standard model are unique solutions to a theory beyond the standard model. It might be so, or it might not be so. Who knows.

What I am already impressed, by, however, is that string theory is a theory which in principle allows to address this question. String theory may well be wrong. But it is the only theory we have ever known which has solutions that determine these parameters.

On this blog I shouldn't say something like this without giving an example from outside of string theory which illustrates this by analogy:

Analogously, it is impressive how GR is a theory which, while it does not fix the curvature of spacetime, it has solutions which determine it. If we perfectly understand the space of all solutions of FRW-models in GR we can check if it contains any solution which matches the observed structure of the cosmos at large scales. The theory does not predict it, but it puts a structure on the space of all possibilities.

20. **Zelah**
January 13, 2006

Hi Urs,

Does String theory predict the Standard Model from first principles?

The stringy attempts I have seen are adhoc!

An amateur mathematician

21. **Urs**
January 13, 2006

> Does String theory predict the Standard Model from first principles?

No.

22. **Urs**
January 13, 2006

That was the short answer. There is a longer one which mentions how there is an issue of perturbative versus nonperturbative, how things are too little understood to fully answer this question, and so on. But I have to run now.

23. **woit**
January 13, 2006

> What I am already impressed, by, however, is that string theory is a theory which in principle allows to address this question. String theory may well be wrong. But it is the only theory we have ever known which has solutions that determine these parameters.

The problem with this is that the evidence is that there are an infinite number of solutions, enough to give you almost any values of the standard model parameters. If this is really the case, the theory has zero predictive value, can't be falsified, and continuing to pursue it doesn't make any sense. Some string theorists believe the theory is still not well-enough understood and maybe there is some way around this to get a predictive theory. I think they're engaged in wishful thinking, but at least they haven't given up on science. What I really don't understand are those who accept the idea that all these solutions are out there, can't come up with any evidence that there is enough structure on the space of these solutions to allow predictions, and yet keep working on this.

24. **Urs**
January 13, 2006

> ST is great because it places structure on what possibilities exist, you are just cutting out alternatives and introducing prejudice into science.

That prejudice is called a hypothesis. The structure it implies is called a prediction, implied by the hypothesis.

There is every reason to be unsatisfied with the progress string theory has made on the phenomenological side and with some of its sociological aspects. What I find somewhat deplorable is, however, that on this blog the party line has established that "string theory is not science". That's unfruitful anti-hype (co-hype, for the mathematicians) and not suited to bring anyone closer to the truth than the equally unfruitful string hype is.

25. **Urs**
January 13, 2006

> The problem with this is that the evidence is that there are an infinite number of solutions,

This is, by the way, the generic situation for any theory. The irony of history is that the "landscape affair" was ignited by an argument that there are only *finitely* many solutions to string theory (under some conditions).

> enough to give you almost any values of the standard model parameters.

This might be the expectation of some people, concerning an unknown space known as the "landscape". But if you check with those people who actually construct solutions which come close to the standard model, the picture is rather the opposite.

26. **woit**
January 13, 2006

Urs,

I don't think there's a party-line here, except that this blog does reflect my views. I've never said "string theory is not science", but have time and time again made a quite precise point about the particular thing that I see some string theorists doing that is not science: See my review of Susskind's book for the details of this.

By the way, I'm starting to get annoyed by what I have been hearing and seeing in how some quite prominent string theorists are dealing with the existence of this blog and the arguments I have been making. Instead of dealing with my arguments, people have been attributing to me stupid or mean blanket statements about string theory or string theorists that I have never made. I try to be quite clear and precise in what I write here, and I wish that anyone who wants to discuss any of this would make an attempt to deal with what I actually write, not something else.

27. **Urs**
January 13, 2006

> deal with what I actually write

Sure. I think I was engaged in discussion with other commenter's here and certainly had them in mind.

I get the impression that your personal view, while often expressed in a provocative form ("string theory is not even wrong" is the intended provocative title of this blog, no?) is pretty much that people should stick to doing solid research. Nobody could disagree with that.

But it is also true that the comment sections here display a lot the point of view that "string theory is simply plain nonsense", which I would like to see rectified a little.

And I do think it is good to criticize things which ought to be critized. It is, not the least, helpful for those being criticized.

28. **woit**
January 13, 2006

Urs,

The problem isn't that there are an infinite number of solutions, the problem is that there is no evidence that the structure of the set of solutions has any useful predictivity. If there were an infinite number of solutions, all with different CCs, but all with a specific neutrino mass matrix, you would have a highly predictive theory.

Sure, it is hard to construct particular examples that do exactly what you want. The main reason for this is that simple examples that you can analyze all don't look like the standard model. To get something that looks more and more like the standard model, you have to make your construction more and more complicated. This is an obvious clue that you're on the wrong track.

Correct me if I'm wrong, but of the 20 odd parameters of the SM, and its choice of gauge groups and fermion representations, I know of no plausible proposal of a particular piece of structure on the landscape that will allow you to predict a single one of them. The closest thing to an attempted prediction I have seen was the attempt a year or two ago by Douglas and others to predict whether the supersymmetry breaking scale was low or not, and that effort seems to have quickly collapsed in failure.

29. **woit**
January 13, 2006

Urs,

There certainly is a sizable amount of nonsense in the comment section here, including uninformed blanket denunciations of string theory.

I already delete some of this sort of thing, in the future may start deleting more, to keep the noise level down to a more tolerable level.

30. **ksh95**
January 13, 2006

urs said:

> It would also be impressive to have a theory which would allow to derive most, if not all, distances of the planets from our sun.

Yes, it would be impressive. Let me propose such a theory…F=ma… As input we need the position and momenta of the particles comprising the gas cloud surrounding our proto-sun.
-or-
In principal we could use the SM+GR, using the conditions shortly after the big bang, and derive the radii exactly.
The point, of course, being that the planetary radii are not anthropically-determined random parameters and are entirely derivable from other quantities.

31. **JE**
January 13, 2006

Urs said:

"But it is also true that the comment sections here display a lot the point of view that "string theory is simply plain nonsense", which I would like to see rectified a little."

As a noiseless reader of this blog, I'd say what Peter and others mostly do here is criticizing the hype and unjustified funding and support that string theory is receiving from the Establishment compared to its physical achievements. It's this absurdly large imbalance that is often denounced as plain nonsense in this blog, not the whole string theory program.

32. **BC**
January 14, 2006

String theory has always been a joke.

33. **anon**
January 14, 2006

No, let's be clear. String theory provides one possible frame for quantum gravity since it would allow a spin-2 gauge boson (graviton) if that theory could ever be found (like the empty frame which Heisenberg called his grand unified theory), and it provides a landscape of 10^500 vacua, one of which might be real.

Whatever you do, don't laugh. It is not supposed to be a joke.

34. **Juan R.**
January 14, 2006

dan said,

> Lubos states it is possible to derive the standard model from first principles of string theory, Peter says it is not. Who is right on this specific issue?

It is easy. Cite a single paper, manual, talk, etc. where the Standard Model is **derived** from string theory.

Of course, Lubos Motl is not correct. The whole string M-theory framework is very outated and wrong in many technical details.

P.S: "Derived" here means that all mathematical and conceptual structure of the SM is a special case of the axiomatic and conceptual structure of ST. Of course derivation does not mean to know previously the answer and use it as input in the 'derivation', not obtain models simlar but not equal to SM, for example models with certain simetries resembling SM but based in massless states, etc.

—

Juan R.

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35. **D R Lunsford**
January 14, 2006

Even if string theory worked as advertised, the resulting world-picture would be so repulsive that no one would get interested in science. That it fails so thoroughly is evidence that the world is not repulsive, and people will remain curious about it.

-drl

36. **dan**
January 14, 2006

Lubos is on record stating " claim such nonsenses such as that LQG only has the same problems like string theory"

His position as well as Witten Greene Weinberg, is that the alternatives such as LQG suffer from such serious problems that the only research program worth pursuing is string theory. the argument seems persuasive — given a speculative theory, if the alternatives suffer from even more serious problems, work on the one with the least (in this case string theory).

While i cannot answer Lubos' criticisms of LQG, i cannot help but wonder, if the greatest physcists like Witten worked on LQG's oustanding issues for the past 30 years, and abandoned string theory in the early 80's, where would LQG be today?

37. **Nigel**
January 14, 2006

Some kind of loop quantum gravity is going to be the right theory, since it is a spin foam vacuum. People at present are obsessed with the particles that string theory deals with, to the exclusion of the force mediating vacuum. Once prejudices are overcome, proper funding of LQG should produce results.

38. **Lee Smolin**
January 14, 2006

Dear Dan,

Actually LQG and related research programs (such as CDT or programs based on quantum information ideas) are progressing rapidly, see for example the talks at the recent Loops 05 conference: http://loops05.aei.mpg.de/

Lobus's criticisms have been aswered in detail several places, you might look for example at the FAQ section of my review hep-th/0408048 (although this review needs updating in view of some developments since then.)

While we would of course be happy with anyone's contribution to the subject, the fact is that there is a community consisting of about 150 mostly young physicists and mathematicians working in the general area of LQG and other background independent approaches and the results show that many of them are of equal quality scientifically to the best young people in other fields. The only concern I have for the future of quantum gravity is that these people be rewarded for their contributions with positions that will let them continue to contribute; for the present people in these fields have much harder careers than equally accomplished or talented string theorists. This does mean that those who have gone into the field have significantly more courage and intellectual independence, which I believe is one reason so much has been accomplished by relatively few people.

Thanks also to Nigel for those supporting comments. Of course more support will lead to more results, but I would stress that I don't care nearly as much that LQG gets more support as that young people are rewarded for taking the risk to develop new ideas and proposals. To go from a situation where a young person's career was tied to string theory to one in which it was tied to LQG would not be good enough. Instead, what is needed overall is that support for young scientists is not tied to their loyalty to particular research programs set out by we older people decades ago, but rather is on the basis only of the quality of their own ideas and work as well as their intellectual independence. If young people were in a situation where they knew they were to be supported based on their ability to invent and develop new ideas, and were discounted for working on older ideas, then they would themselves choose the most promising ideas and directions. I suspect that science has slowed down these last three decades partly as a result of a reduced level of intellectual and creative independence avaialble to young people.

Thanks,

Lee

39. **matt**
January 14, 2006

Thanks for that description, WOIT

40. **dan**
January 14, 2006

Hi Lee,
Thanks for the response. I do think your suggestion is admirable in principle, but i wonder to what extent it could be realized in practice. For example, Igor and Grichka Bogdanov would be an example of two intellectual creative young people who developed and invented new ideas, but whether they created some kind of Sokal hoax remains disputed. If a young researcher publishes string theory, the value of the article can be appreciated by other sting theorists. If a young researcher publishes an article suggesting a new program, it may be difficult to find qualified individuals to make a proper evaluation.

I am curious: how close is LQG on its semiclassical description, dynamics and contact with standard model particle physics?

41. **Who**
January 14, 2006

Lee,
you say

> Actually LQG and related research programs (such as CDT or programs based on quantum information ideas) are progressing rapidly, see for example the talks at the recent Loops 05 conference: http://loops05.aei.mpg.de/
>
> …there is a community consisting of about 150 mostly young physicists and mathematicians working in the general area of LQG and other background independent approaches and the results show …

I note that this research community does not have much visibility on the Web: for instance there is no QG blog and I don't know of any central bulletin-board for QG news information and comment.

You mention the archived Loops '05 talks as a good indication of current progress. That would be an excellent resource except the recorded talks are now off-line. They were up for a month or so around late-November-early-December but are now inaccessible. One can still download slides in some cases.

I would particularly like to be able to refer people to the video of the talk by Laurent Freidel, which I found quite impressive. But there are no slides and the recording (like the others from Loops '05) is no longer available. I have my own copy, but nowhere to post it for others.

You mention the intellectual independence of the best young QG researchers. It is a strong point. None of them, that I can think of, are merely working on some senior person's program. They tend each to have a definite individual research direction.

I don't see why universities are not scrambling to hire these people in junior faculty positions. It looks like where the future action is, but apparently deans and department chairmen haven't realized this.

It sounds good, in principle, to say that the academic rewards and support should go to those who make independent progress and not be attached to this or that program. However it would probably help to create prospects for the present crop of postdocs if non-string QG approaches were validated in visible ways.

QG could probably use a web-magazine of some kind. Not limited to scholarly papers but open to comment, news items, usual blog-stuff.
And I mean non-string QG.

There isn't a good collective name for it—-simply saying LQG can lead to confusion (people cite Nicolai et al thinking that is a critique of current LQG work)—-and I see that you resort to circumlocutions such as **LQG and related research programs (such as CDT or programs based on quantum information ideas)**

or as you say elsewhere **LQG and other background independent approaches**

and when you even just say LQG you mean, I believe, to include spinfoams—which many people don't seem to realize under that heading. The Nicolai et al paper purported to review LQG but did not discuss spinfoam—or any path integral approach—as i recall.

So there is a bothersome communication problem. To improve prospects for young researchers you need to publically validate the field and point out the rapid progress being made. but you do not even have a NAME for the field!

Much of the progress is in Path Integral approaches but if you say LQG then stringy-people will make a lot of noise about strict canonical LQG (as defined 5 or 10 years ago and reviewed by Nicolai et al) and this will tend to drown out whatever you say about LQG.

You need a name for this allied group of QG approaches which is short and immediately recognizable and which people cannot intentionally or unintentionally confuse with circa 1995 canonical LQG.

Something like QG*

where the asterisk indicates non-string, background independent, non-perturbative, and that spinfoam and other path-integral approaches are included

and you need a blog with a name like QG*, which reports all the progress being made and emphasizes all that is happening in the field, what different postdocs are doing and where they are going, what the most active centers are. (which turns out to be mostly outside the US, so less visible to people in US habituated to think of Harvard and Princeton etc. as where the important physics research happens)
and I think maybe you need to drum into people the message that
if a physics department is not strong in QG* then it is apt to lose standing the next time round.

But agreeing with your point that QG* is not one specific program but a hodge-podge of independent directions which various independentminded individuals have gone in, off the beaten path, just (this is a delicate point) the successful ones who have made a real contribution and real progress by doing that. Because your main point is that support should not be tied to some senior person's research agenda.

No time to edit this and make organized and brief. Hope helpful anyway.

42. **Aaron Bergman**
January 15, 2006

Lee Smolin said:

LQG and related research programs (such as CDT

What does causal dynamical triangulations have to do with loop quantum gravity? They seem completely disparate approaches to me.

For Marcus, what you don't seem to realize is that many people have looked at these approaches and they don't think they're fruitful. Lee has his FAQ, but people *disagree* with him.

43. **Who**
January 15, 2006

Hi Aaron,
…seem to realize is that many people have looked at these approaches and they don't think they're fruitful. Lee has his FAQ, but people disagree with him…

I can see for myself that certain of the QG* approaches are fruitful. I don't need your authorities. Nor, actually, do I need Lee Smolin's FAQ to tell me about recent results —several of those I have in mind came after his paper with the FAQ was written.

44. **Aaron Bergman**
January 15, 2006

I'm not attempting to argue by authority. I'm trying to explain to you that the issue isn't one of publicity or prejudice or whatever. It's a disagreement on the merits.

45. **Chris Oakley**
January 15, 2006

> what is needed overall is that support for young scientists is not tied to their loyalty to particular research programs set out by we older people decades ago, but rather is on the basis only of the quality of their own ideas and work as well as their intellectual independence. If young people were in a situation where they knew they were to be supported based on their ability to invent and develop new ideas, and were discounted for working on older ideas, then they would themselves choose the most promising ideas and directions. I suspect that science has slowed down these last three decades partly as a result of a reduced level of intellectual and creative independence avaialble to young people.

Yes, but how are we going to get there? The relics who run the research establishment are not going to willingly give up power and young researchers are generally streetwise enough to know that however small their chance of getting tenure, it is going to be even less if they follow the Sinatra doctrine.

46. **Lee Smolin**
January 15, 2006

Dear Dan,

It is no problem to assess even very independent and creative young physicists, and distinguish those with promising ideas and results from those of non-professionals like the Bugdanovs. Their work is not in a vacuum, it is based on and uses established physics and mathematics and is aimed at solving long standing problems. The work can be judged both on the quality of the work and presentation and the promise of solving outstanding problems. A good test is surprise, is the idea new and potentially signficant? Is it an idea I wish I'd had, that I would like to work on? Has the person pulled off more than once an original idea that had an impact on the field? These are the kinds of tests we use when assessing the promise of young people in quantum gravity and foundations of quantum theory and we seem to have done well with them.

To Chris, There is no problem getting there. What I described is an ethic which is followed in some fields of physics and mathematics. It was the governing philosophy in what used to be called "the relativity community" which was why there were so many important results from relatively few people from the 1960s on. I would claim that those communities that don't adhear to it are anomalous and suffer from slow progress because of much duplication of effort and "me too" science and not enough risk taken by people trying to solve the hard open issues.

To Who, Thanks for the suggestion of QG*. We generally speak of just quantum gravity. Of course LQG includes spin foam models. There was a LQG email news and debate distribution list run for a few years by Fotini Markopoulou, this could certainly be now web based. Thaniks for the suggestion, Ill pass it on.

To Aaron,

At the level of statements of results, the basic claims of LQG and CDT are generally accepted. I assume you agree with this as no critic has shown a technical error in an important result-such as those listed in my review. There is of course disagreement on their significance.

Let us put the question this way: it is a non-trivial question whether there could be any examples of background independent QFT's. In fact there are some examples. There are of course the TQFT's but there are others including 2+1 gravity with matter, which we now know a lot about and there are a number of rigorous results about 3+1 including the existence and uniqueness theorems, the finiteness results about spin foam models and the CDT results. So you may disagree that nature should be background independent. But you cannot disagree that there is good evidence that such theories exist.

As to what CDT and LQG have to do with each other, they are both background independent approaches. CDT can be seen as very simple spin foam models. A number of people have worked on both.

Thanks, Lee

47. **Moshe**
January 15, 2006

Lee,

I believe the non-trivial question for LQG and similar approaches is whether they are indeed quantum theories of gravity, in other words do they contain semi-classical states and propagating gravitons in 4dim. I think lots of people will become interested in any quantum model that can be shown to contain gravity as we know it, whether it is BI or not. Since there is some progress on this recently, maybe one can be cautiously optimistic.

48. **Aaron Bergman**
January 15, 2006

I'm entirely too tired to go through the usual rigmarole yet again, so I'll just stand by my statement that people disagree on the *merits*.

49. **Who**
January 15, 2006

I will suggest a citation as footnote to something in Lee's post:

> it is a non-trivial question whether there could be any examples of background independent QFT's. In fact there are some examples. There are of course the TQFT's but there are others **including 2+1 gravity with matter**, which we now know a lot about and there are a number of rigorous results …

http://arxiv.org/abs/hep-th/0512113
Effective 3d Quantum Gravity and Non-Commutative Quantum Field Theory
Laurent Freidel, Etera R. Livine
9 pages, Proceeding of the conference "Quantum Theory and Symmetries 4" 2005 (Varna, Bulgaria)

"We show that the effective dynamics of matter fields coupled to 3d quantum gravity is described after integration over the gravitational degrees of freedom by a braided non-commutative quantum field theory symmetric under a kappa-deformation of the Poincaré group."

This paper summarizes work described elsewhere in more detail. Here are some related papers including one that Freidel recently co-authored with Shahn Majid
http://arxiv.org/hep-th/0502016
http://arxiv.org/gr-qc/0506067
http://arxiv.org/hep-th/0601004

50. **Christine**
January 15, 2006

Who Says:

> I note that this research community does not have much visibility on the Web: for instance there is no QG blog and I don't know of any central bulletin-board for QG news information and comment.

Yes, you are right about that. The fact that I couldn´t find a blog with discussions directly related to what you call QG* was the reason that I myself started one (which includes other matters as well), even though I am quite aware that it will probably take some time (viz. several years) until my blog reaches the level of quality and usefulness that you mention (and that I very much would like to!). BTW, you have been a very nice contributor to my blog and that is what really enriches a blog, useful comments, specially in its initial stages!

> I don't see why universities are not scrambling to hire these people in junior faculty positions.

Life is not like that, unfortunately I know it well.

Best wishes
Christine

51. **Kasper Olsen**
January 15, 2006

Aaron: I think that LQG and CDT are related in the following way; they are both attempts at a "background independent" approach to quantum gravity; and furthermore, they don't appear to pose any restrictions on the type of matter that couples to gravity…

Gravity *is* important – but there is, to be honest, more that Matters 😉

52. **Aaron Bergman**
January 15, 2006

You know, if I never hear the term 'background independent' ever again, my life would be just that bit happier.

As for the types of matter allowable, it's not at all obvious to me that the existence of a UV fixed point would be insensitive to the types of matter allowable. I'm more amenable to the idea than Jacques is, but it's far from an apparent point.

53. **Chris W.**
January 16, 2006

[You'll want to skip this, Aaron.]

The original form of the requirement of background independence was introduced by Leibniz. Closely related ideas and arguments were developed by Mach, and these have arguably been the most influential.

On the face of it, Newton won this argument with Leibniz; the success of Newton's mechanics and theory of gravitation appeared to show that misgivings about the soundness of his assumption of absolute space and time were misguided. Mach's criticisms made clear to Einstein that they were not. What Mach pointed out was that the operational significance of the notion of an inertial trajectory, and by extension an inertial frame of reference, were deeply unclear in the presence of universal gravitation and in light of the unobservability of absolute position in space. There is an operational resolution to this difficulty, but it is inherently dynamical. Julian Barbour has argued persuasively that this is in fact the essence of what Mach was getting at.

All this has been taken as irrelevant to particle physics, where the operational validity of an effectively (if not literally) inertial laboratory frame is taken for granted. The problem cannot be easily evaded however; one cannot dismiss the issue by simply saying, well, some of us may like the *idea* of background independence but it may not be the way nature is. The precedent of Newtonian gravity + mechanics suggests that this is misstating the issue. To say that there is a fixed background spacetime implies that one has found a way to observe it, and has done so. However, we have not done so, and the existence of such a spacetime is fundamentally inconsistent with general relativity.

In particle physics, before we concerned ourselves with quantum gravity, we could say "well, maybe there is a fixed background spacetime, or maybe there is merely an effective background; what difference does it make?" In quantum gravity we must understand the dynamical origin of that effective background. If we simply assume it as an absolute—eg, because we only know how to formulate quantum mechanics in the presence of a fixed background with certain symmetries—then we assume its *absolute* role in the classical limit, and its observability in that limit. But all our experience suggests that this is not possible, and this impossibility is built into the deep structure of general relativity.

If string/M-theory or particle physics in general could squarely confront this issue—say, by showing that a background is used as a formal device in certain formulations, but is irrelevant to the theory's physical interpretation or observational consequences—then I think you could be spared further heated discussion of "background independence".

On the role of matter in all this, I'll simply point out that matter *is* what we can observe, and that all our operational conclusions about spacetime as a physical entity are based on this. So in some sense matter and spacetime are bound to each other. One is tempted to say that one is derived from the other, or a manifestation of the other. String and M-theory incorporate matter, or provide the substrate for effective field theories to arise, but they treat gravity in this context as just another effective field theory in a *background spacetime*—do they not? This is what needs to be clarified. The massively proliferating complications of the last 10 (or so) years certainly don't help matters.

54. **Thomas Larsson**
January 16, 2006

At the level of statements of results, the basic claims of LQG and CDT are generally accepted. I assume you agree with this as no critic has shown a technical error in an important result-such as those listed in my review. There is of course disagreement on their significance.
Let us put the question this way: it is a non-trivial question whether there could be any examples of background independent QFT's.

The technical objection is that polymer representations of the diffeomorphism group are not of lowest-weight type, unless all other representations of symmetry groups in quantum physics. Hence, although there is no doubt that LQG models exist and are background independent, they apparently fail to be genuinely quantum in this respect. It is easy to argue that no locally non-trivial strictly background independent QFT exists at all. Mathematically, the 1D version of this argument is that the only unitary irrep of the centerless Virasoro algebra is the trivial one.

The solution to the problem of QG lies elsewhere: one must introduce an explicit representation for the observer's trajectory in spacetime. This leads to the resolution of three classes of conceptual problems:

1. It resolves various problems of time.
2. It resolves problems with the Copenhagen interpretation, by moving the observer to the quantum side of the Heisenberg cut.
3. It makes it possible to formulate diff anomalies in 4D, which we know must exist in analogy with 2D gravity, because the multi-dimensional Virasoro cocycles are functionals of the observer's trajectory.

For details, see e.g. this post or this forthcoming book. In order not to litter Peter's blog with off-topic material, objections may be posted here.

55. **Aaron Bergman**
January 16, 2006

If string/M-theory or particle physics in general could squarely confront this issue—say, by showing that a background is used as a formal device in certain formulations, but is irrelevant to the theory's physical interpretation or observational consequences—then I think you could be spared further heated discussion of "background independence".

Please read this thread. You can start with my post #70 if you want to skip to the relevant stuff.

The quick summary is that string theory looks exactly as if the background is only an artifact of th formulation. A more interesting question is the dependence on asymptotics and that is dealt with extensively in the above comment thread.

56. **Urs**
January 16, 2006

> urs said:
>
> It would also be impressive to have a theory which would allow to derive most, if not all, distances of the planets from our sun.

Yes, it would be impressive. Let me propose such a theory…F=ma… As input we need […]

[…] the conditions shortly after the big bang

Exactly, that's my point. You cannot derive it from first principles, but you can derive it given specified initial conditions.

Hence it is not, as Kepler once hoped, a unique solution, but one out of an infinite "landscape" of solutions. In the end, the question "why" the earth has the distance from the sun that it has is certainly not independent from the a posteriori fact that carbon-based systems observe it being there. (That's supposed to be an uncontroversial tautology.)

This does not imply that the same state of affairs applies to the parameters of the standard model. But it may help illustrate that it is nothing exotic for a theory to yield unique solutions only after some initial conditions have been specified. It is in fact the generic case.

We may hope for more, but it does not mean to abandon science to expect less.

57. **Thomas Larsson**
January 16, 2006

unless all other representations
unlike all other representations

58. **dan**
January 16, 2006

so given relevant initial inputs, is string theory able to derive other features of the standard model?

"This does not imply that the same state of affairs applies to the parameters of the standard model. But it may help illustrate that it is nothing exotic for a theory to yield unique solutions only after some initial conditions have been specified. It is in fact the generic case."

59. **Urs**
January 16, 2006

> so given relevant initial inputs, is string theory able to derive other features of the standard model?

As you may know, fixing a compactification for strings (heterotic, say), specifies completely a corresponding low energy field theory which is observed as the 'standard model' of the world described by this solution.

So for instance the known compactifications which come close to the standard model all make a prediction for the number and nature of the Higgs fields in these theories.

But most models so far also predict a bunch of massless scalars (moduli). The most recent model by Braun et al, which produces otherwise exactly the structure of the standard model, as far as I understand, has reduced these to a mere 13. But that's still 13 too many to be phenomenologically viable.

Which, incidentally, means that these solutions have been *falsified*.

60. **Who**
January 16, 2006

Christine said

> The fact that I couldn´t find a blog with discussions directly related to what you call QG* was the reason that I myself started one (which includes other matters as well),…

I should have mentioned your new "Background Independence" blog as potentially just the sort of news and discussion-board for quantum gravity that I had in mind! BTW the blog is visually very nice.

An erratum: I accidentally transposed digits when I meant to write
http://arxiv.org/hep-th/0502106
and wrote 0502016 instead—back on 15 Jan 2:32P
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BTW Christine, another thing, 2005 was a significant year for "QG*" in which a lot of progress was made. You could ask for nominations, at your blog, for the most important paper. Then, if only I showed up at the blog and proposed a candidate, my nominee would automatically win 🙂

61. **Who**
January 16, 2006

Lee (14 Jan 4:41P)

> Dear Dan,
>
> Actually LQG and related research programs (such as CDT or programs based on quantum information ideas) are progressing rapidly,

Dan (14 Jan 7:13P)

> I am curious: how close is LQG on its semiclassical description, dynamics and contact with standard model particle physics?

Moshe (15 Jan 10:30A)

> Lee,
>
> I believe the non-trivial question for LQG and similar approaches is whether they are indeed quantum theories of gravity, in other words do they contain semi-classical states and propagating gravitons in 4dim. I think lots of people will become interested in any quantum model that can be shown to contain gravity as we know it, whether it is BI or not. Since there is some progress on this recently, maybe one can be cautiously optimistic.

Lee (15 Jan 6:49A)

> Of course LQG includes spin foam models.

================================

http://loops05.aei.mpg.de/index_files/abstract_rovelli.html
Carlo Rovelli
GENERAL RELATIVISTIC QUANTUM PHYSICS: Background independent scattering amplitudes, boundary formalism, local particles and partial observables
Abstract: (i) I present some preliminary results on background independent calculations of particle scattering amplitudes. In particular, I discuss the derivation of the graviton propagator, from loop quantum gravity and the spinfoam formalism. (ii) I illustrate the boundary formulation of quantum field theory, its role in a background independent context, and how "particles" emerge in this language. (iii) More in general, I discuss how systematic physical predictions can be extracted from a general relativistic quantum field theory: I illustrate the notion of "partial observable", and discuss the issue of the physical significance of the spectrum of these observables, which controls the interpretation of the area and volume discreteness."

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http://arxiv.org/abs/hep-th/0512113
Effective 3d Quantum Gravity and Non-Commutative Quantum Field Theory
Laurent Freidel, Etera R. Livine

"We show that the effective dynamics of matter fields coupled to 3d quantum gravity is described after integration over the gravitational degrees of freedom by a braided non-commutative quantum field theory symmetric under a kappa-deformation of the Poincaré group."

"We will use these results to write down the Feynman graph amplitudes of a massive scalar field evolving in the 3d quantum geometry and we will show how they reduce to the standard Feynman graph amplitudes of the standard quantum field theory in a no-gravity limit G -> 0. On one hand, that solves the problem of the semi-classical limit of 3d quantum gravity, and on the other hand, this formulation allows us to compute explicitly the quantum gravity corrections to the

standard quantum field theory scattering amplitudes and show that the corresponding geometry is effectively non commutative… "

"… In the full quantum gravity regime, l_P and m_P are fixed and finite. The (semi-)classical limit is defined as

62. **Who**
January 16, 2006

Sorry, the preceding got cut off. Here is the rest

"…The (semi-)classical limit is defined as h-bar -> 0
l_P is taken to 0 while m_P remains fixed: we do not expect to recover standard classical flat space-time physics but a modified theory with a universal mass scale m_P as postulated in deformed special relativity.

The no-gravity limit G -> 0 takes l_P to zero and m_P to infinity but

63. **Who**
January 16, 2006

What I am trying to do is give additional substance to some things others have said in this discussion. What Moshe said regarding "some progress recently" on gravitons connects to Rovelli's talk at Loops '05. What Lee said about the 3D case connects to the Freidel Livine paper. And quotes from that paper serve as response to Dan's question about LQG and the semiclassical limit. Freidel's system shows the correct behavior both as G goes to zero ("zero gravity" limit) and also as h-bar goes to zero (semiclassical limit). Recent QG work is tending to include matter and use the spinfoam formalism (as illustrated by these examples.)

64. **woit**
January 16, 2006

Who,

Please, this posting had nothing at all to do with LQG. This has become way off-topic, and I don't think you should keep posting comments about this here.

65. **Who**
January 16, 2006

Hi Peter,
two issues: I was having technical problems. sorry about the multiple posts. the message in the preview kept getting truncated and not going up

second issue: I think the Freidel Livine paper one of the most important LQG papers of 2005. If we are going to discuss progress in LQG at all, we need to be able to talk about the Rovelli paper (which is uses spinfoam formalism) and the Freidel-Livine paper (also spinfoam).

See last paragraph of page 1 of F-L:
"We use the spin foam quantization of 3d quantum gravity provided by the Ponzano-Regge model…"

See Smolin's post 15 Jan 6:49A:
"Of course LQG includes spin foam models."

If you are going to allow a discussion of last year's progress in LQG then you have to allow people to talk about both the Rovelli paper and the F-L paper. Both the Rovelli and the Freidel papers were presented as invited talks at Loops '05 in October. Both are core representatives of current work in LQG.

66. **John Gonsowski**
January 16, 2006

Who, I think Peter is saying his original blog post was not about LQG. If Peter had started an LQG thread your comments would probably be no problem. It can get started by somebody else I know.

John… practicing for my own blog… just kidding

67. **woit**
January 16, 2006

Who,

John is right. This was not a posting about LQG, and while I think the idea of an LQG weblog is great, this is not it. I don't have either the competence or desire to run such a thing. Also, I'm afraid that no matter what the originial topic, you tend to turn the discussion to LQG or cosmological natural selection topics. If you want an in-depth discussion of these topics, you should set up a weblog devoted to them.

The other problem with this is that it gives lots of other people the idea that this comment section is a forum for them to discuss their favorite ideas, even if they're not the topic I'm posting about and have only come up tangentially in the discussion. I then end up having to spend a lot of my time deleting people's comments to stop this, annoying them and wasting my time.

68. **D R Lunsford**
January 16, 2006

Peter – the problem is, there isn't a neutral zone to talk about ideas – in a strange way you've created a neutral zone of sorts, whether you like it or not – this is a credit to you because you are letting physics as it were "speak for itself", and we all admire you for that.

So it's not polite to run on about things here, but we/they/all of us only do it because you have set up fairness as your first principle.

-drl

69. **Who**
January 17, 2006

Thanks for the reactions. DRL mentions courtesy. I apologize for (unintended) impoliteness and will try to be more considerate. My main concern is that the discussion of QG is often clouded by "Nicolai's mistake"—he coauthored a paper purporting to give an outsider's view of LQG and didn't discuss the spinfoam (path-integral) formulation. Peter then posted a letter from Lee responding to Nicolai in which Lee's main point was that for the past (on the order of) ten years the spinfoam approach has predominated in LQG research. Hope I don't overstate here. Most of the quantum gravity researchers I watch use spinfoam formalism, currently including Carlo Rovelli (who has been closely associated in the past with the canonical version of LQG).

In this thread LQG began being discussed by others and I joined the discussion wanting to respond, supplement and clarify what was said. Dan asked about the current status of LQG and it makes an enormous difference whether or not one includes spinfoam research. Including it also makes the close relationship between LQG and CDT obvious, an issue raised by Aaron and by Kasper Olsen.

Thanks for your patience, Peter. You do a great job creating a neutral forum space as DRL says.

70. **anon**
January 19, 2006

String theorist Dr Lubos Motl, who knows about propaganda, has now usefully given the following enlightening viewpoint:

"An important part of all totalitarian systems is an efficient propaganda machine. … to protect the 'official opinion' as the only opinion that one is effectively allowed to have." – STRING THEORIST Dr Lubos Motl, http://motls.blogspot.com/2006/01/power-of-propaganda.html

71. **Who**
January 19, 2006

Hello anon, hi DRL,
string theorist Hermann Nicolai (who seems the opposite of totalitarian and propagandistic—-an honorable member of the majority so to speak)
has come out with a new paper giving an overview of the main non-string QG approaches that this time INCLUDES spinfoam research

hep-th/0601129

this could actually improve the quality of the discussion.

It used to be that string theorists who wanted to defend against a perceived threat would lump all rivals to string under the heading "LQG" and then discuss only the orthodox canonical LQG essentially of the 1990s

but for the past 5 years or so (maybe more), people who see themselves as part of the LQG community have largely been doing spinfoam (as they tell us, and one can see it by sampling arxiv). There has been a gradual shift towards path integral approaches (like spinfoam and CDT)

so in the past a would-be defensive string theorist was often not connecting with reality. And people would quote Hermann Nicolai's earlier paper of just one year ago hep-th/0501114 LQG an outside view
which unfortunately did NOT discuss spinfoam or much of the activity which the LQG community was pursuing.

Now maybe defensive string theorists will direct their attack more on the actual rivals instead of at a mannequin of their imagining. Something good is likely to result, says I.

72. **anonymous**
January 19, 2006

Who,

Thanks for that link:

'… it is thus perhaps best to view spin foam models as models in their own right, and, in fact, as a novel way of defining a (regularised) path integral in quantum gravity. Even without a clear-cut link to the canonical spin network quantisation programme, it is conceivable that spin foam models can be constructed which possess a proper semi-classical limit in which the relation to classical gravitational physics becomes clear. For this reason, it has even been suggested that spin foam models may provide a possible 'way out' if the difficulties with the conventional Hamiltonian approach should really prove insurmountable.' – hep-th/0601129

Hedge Fund Finances RHIC

January 13, 2006
Categories: Uncategorized

Federal funding for high energy physics in the US has been declining significantly in recent years. The recently completed FY 2006 budget has a 2.7% cut for high energy physics in the DOE budget, which provides the bulk of US funding for high energy physics research. The NSF also provides some support, but its budget for the mathematical and physical sciences is up only 1.5%, significantly below inflation (I don't know what the number is for high energy physics at the NSF by itself).

Over at Cosmic Variance, Joanne Hewett has commented on how depressing and discouraging the budget situation is, describing discussions amongst those charged to plan for the future as "downright scary at times." One of the worst immediate effects of the 2006 budget had been that the Brookhaven heavy ion collider RHIC would only have been funded for 12 weeks of operation instead of the planned 20 due to higher electric power costs.

Today Brookhaven made a remarkable announcement about this. A group led by ex-mathematician Jim Simons, head of the hedge fund Renaissance Technologies (for more about him and his hedge fund, see here and here) has come up with a contribution of the $13 million needed to run RHIC for the full 20 weeks. The group contributing this money includes other partners at Renaissance, which is based on Long Island, not very far from Brookhaven.

I'm sure the significance of this new source of funding for high energy physics will be debated in the community in coming days. My initial reaction is that while it's wonderful that the worst immediate effect of the budget cuts for 2006 has been avoided due to the generosity of Simons and others, this doesn't materially change the long-term problems.

There's more about this at Entropy Bound, the blog of Peter Steinberg, who works on an experiment at RHIC.

Update: More comments about this are from Joanne Hewett at Cosmic Variance and Chad Orzel at Uncertain Principles.

Update: More about Simons from New York Newsday.

Comments

1. **Quantoken**
   January 13, 2006

   Peter:

   Small corrections. Quote: "In the light of budget constraints, DOE had **planned to fund 12 weeks** of RHIC operations in FY06, but unexpected increases in electric power costs had **made this limited level of operation impossible**. Now, with the $13-million contribution to the Stony Brook Foundation and the planned Work for Others agreement between the Stony Brook Foundation and BSA, the Laboratory will be able to operate RHIC for a full 20 weeks"

   The reading of the statement is that without this donation, even the originally planned 12 weeks of operation would have been impossible. Of course, you can not always rely on such generous donations to be available each time.

   I am glad that people have begun to realize the restraint that **rapidly raising cost of electricity** imposes on high energy accelerators. Expect something even worse in the coming years.

   If you look at it, a 1% or 2% drop in the funding is something un-pleasant, but is still a completely manageable small percentage. But when the electricity cost double or tripple in just a few years, that is a total disaster and completely un-manageable, unless the funding is also drastically increase in similar fashion, which is just not possible at this moment. That's the reality.

   I am taken back by the Brookhaven director's statement that the raise of electricity cost was "unexpected". As head of such a big organization he should have known better and should have expected things like this. I don't want to see that a few years down the road CERN also issue a similar statement about LHC, say that they never factored in the possible large increase of electricity cost.

   Quantoken

2. **MathPhys**
   January 13, 2006

   So it's the ones who go to Wall St who end up bailing out those who stay in science.

3. **Tony Smith**
   January 14, 2006

   Quantoken said "… I am taken back by the Brookhaven director's statement that the raise of electricity cost was "unexpected". As head of such a big organization he should have known better and should have expected things like this. …".

   It seems to me likely to me that the Brookhaven director's statement may be no more a statement of true fact than the statement of Captain Renault in Casablanca (from http://www.imdb.com/title/tt0034583/quotes):

   "… Captain Renault:
   I'm shocked, shocked to find that gambling is going on in here!
   [a croupier hands Renault a pile of money]
   Croupier: Your winnings, sir.
   Captain Renault: [sotto voce] Oh, thank you very much. …".

   Such statements seem to me to be socio-political game-playing tactical moves, and to support the assertions in a letter (responding to Lee Smolin's article "Why No New Einstein?") published in the January 2006 issue of Physics Today by Foster Morrison:

   "… Today's scientists are jet-setting, grant-swinging, favor-trading hustlers looking for civil servants who will provide them with a pipeline to the US Treasury. Not only do they get peer pressure to behave this way, they also get arm-twisting from the academic bureaucracy that wants to get its 50% to pay for its bloated overhead. …".

   Tony Smith

http://www.valdostamuseum.org/hamsmith/

4. **woit**
   January 14, 2006

   Tony and Quantoken,

   You are being extremely unfair to those people who have the thankless task of trying to get new high-energy physics facilities built and working in an environment where the inherent technological difficulties are immense, and budgets are both being cut and often not even known until well into the fiscal year. These laboratories have large fixed costs, so faced with sizable budget cuts they have a limited number of choices on how to deal with them, all ugly. The main ones involve shutting down accelerators to save on power costs and firing good people who have been working there, doing a good job for years.

   Experimental high energy physics in the US is facing some very difficult times, for very real reasons, not because the people involved are incompetent or "jet-setting, grant-swinging, favor-trading hustlers".

   Theoretical high energy physics is a rather different story…..

5. **Eli Rabett**
   January 14, 2006

   It is not clear to me that it would either be allowed or accepted if the labs hedged on electricity. If they bet wrong (say the cost went down) the papers and the politicians would have their guts for garters.

   However, DOE does generate electricity from a lot of dams in the southeast and the northwest. Most of this is sold at fixed cost to hold down the cost of electricity to customers. It might be possible for DOE to guarantee delivery of electricity to its own facilities under similar terms. This would, of course, be political, because that power would not be available for other uses.

   There are interesting possibilities here, since the money goes into the DOE budget on one side (from purchases of electricity by others) and into the DOE budget on the other (from purchases of electricity by the labs)

6. **Tony Smith**
   January 15, 2006

   Peter said "… Experimental high energy physics in the US is facing some very difficult times, for very real reasons, not because the people involved are incompetent or "jet-setting, grant-swinging, favor-trading hustlers".
   Theoretical high energy physics is a rather different story …".

   Although I would like to make it clear that I am in favor of high-energy experimental physics (even though my favorite future-project, the muon collider, has been assigned a low priority by the physics community),
   I am not of the opinion that the negative aspects of the theoretical physics community do not also infect the experimental community.

   It seems to me that each large collaboration in the experimental physics community acts to protect its interests in much the same way the superstring theorists act to protect their interests, whether or not those interests are aligned or opposed to the advance of physics itself.

   The characterization of "jet-setting, grant-swinging, favor-trading hustlers" as not being limited to theoretical high energy physicists was not my language, but was (as I made explicit in my comment) a quote from a letter published by the AIP in Physics Today.

   For another example of similar opinion, see the essay by Ad Lagendijk in Nature 438 (24 November 2005) 429, in which Lagendijk (a physics professor and group leader in the Netherlands) said in part:
   "… my daily experience as a professional physicist …[is that]… When I participate in a scientific conference I see a gathering of aggressive men (and yes, I mean men) fighting for their scientific claims … Successful scientists incessantly travel around the world performing their routines like circus clowns – forcefully backing up assertions over what are their contributions to the latest scientific priorities. …
   A modest Japanese presenter does not stand a chance against a loud, American critic speaking in his, and modern science's, mother tongue. …".

   Further, in the 5 January 2006 issue of Nature, a correspondence from Maria Uriarte (of the Department of Ecology of Peter's Columbia University) et al said in part:
   "… The story told in Ad Lagendijk's Essay … is very familiar to many of us who have chosen to make a living in science. … this value system and associated behaviours have other, far-reaching consequences,.
   First, they can compromise the integrity of the scientific enterprise …
   Second, they may reduce the diversity of the workforce by attracting, rewarding and therefore retaining those who thrive in particular kinds of competitive environments – who are not necessarily those of greatest scientific ability, insight, or creativity.
   Third, they may limit the range … of scientific pursuits …".

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

7. **woit**
   January 15, 2006

   Tony,

   All sciences have plenty of bad behavior and people protecting their interests. But in the case of the big experimental high energy physics projects there is generally something worth protecting. I don't think the situation there is optimal: if one started from scratch and tried to figure out the best possible way of allocating the money being spent, one could probably do better than what we have. But it's nothing like what is going on over on the theoretical side.

   From what I've seen, the technical problems of a muon collider are fearsome, but people are working on them. One problem that may doom the whole idea is that, at the kinds of luminosities one need, a muon collider beam emits such an intense neutrino beam (from the decaying muons) that it poses a significant radiation hazard, one that you can't shield against. If you built a muon collider deep underground at Fermilab, you'd have a neutrino beam of dangerous intensity coming out of the earth some radius away, possibly in Chicago.

8. **Tony Smith**
   January 16, 2006

   Peter said "… the case of the big experimental high energy physics projects … is … nothing like what is going on over on the theoretical side …".

The two biggest differences that I see are:

1 – Superstring theorists have effectively a 90% monopoly position in theoretical high energy physics, while the big experimental high energy physics laboratories have been careful to avoid such monopolies. For example, even though as of now Fermilab has a monopoly on collider experiments at its energy level, there are two independent detectors (CDF and D0) that can produce independent data sets in order to verify results. On the other hand, the superstring theorists can (and do) present their stuff as the only game in town, thus stifling other approaches; and

2 – Experimental laboratories are by definition closely connected with experimental data, whereas the superstring theorists have grown so distant from contact with experimental results that Susskind is quoted in the 5 January 2006 issue of Nature (pages 10-12) as saying that although he finds it "deeply, deeply troubling" that there is no way to test the landscape principle, he goes on to say "It would be very foolish to throw away the right answer on the basis that it doesn't conform to some criteria for what is or isn't science".
At least one superstring theorist seems unwilling to follow Susskind in unsupported belief that the landscape is the "right answer". In the same Nature article, David Gross says "People in string theory are very frustrated, as am I, by our inability to be more predictive after all these years … But that's no excuse for using such "bizarre science".
However, even though Gross has sense enough to see that Emperor Susskind has no clothes, he seems to be unable to admit that lack of predictivity means that superstring theory should no longer be regarded as the only game in town for high energy theoretical physics.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Peter also said "… If you built a muon collider deep underground at Fermilab, you'd have a neutrino beam of dangerous intensity coming out of the earth some radius away, possibly in Chicago. …".
As to that,
the pdf file titled "Muon Collider & Neutrino Factory Studies – R B Palmer MSU 1/27/05" on the web at http://bt.pa.msu.edu/Phy964_muon/aaMSU-coloquium-v2.pdf
says in part:
"… Conclusion: Muon Collider
– Interesting for physics & Smaller than Linear Collider
– Difficult technically
– Neutrino Radiation limits Maximum Energy
…
Radiation proportional to E^3 / length^2 proportional to E^3 / depth
Use: 1/10 Federal limit = 10 mR/year
Negligible problem at 1.5 TeV
E = 3 TeV ok at 300 m depth
E > 3 TeV Requires:
– Beam wobbles, and/or
– Special Locations (eg an island), and/or
– Better Cooling (Optical Stochastic?) …".

Maybe a Pacific Island (Johnston Island?) or Central Asian location might be acceptable for a high-energy muon collider.

I am under the impression that Fermilab has decided to try for the NLC, but that the Japanese are saying things like "you can't see the Pacific Ocean from the Illinois prairie". Even so,
while cut-throat competition for such things as NLC siting might be life-or-death for a single entity like Fermilab, such a death would not be the end of progress in experimental physics (note that despite the death of the SSC, the LHC and its successors can do the physics that might have been done at the SSC).
In contrast,
if landscape superstring theory were to maintain a monopoly position in theoretical high energy physics, that might indeed mean the end of theoretical physics as a scientific enterprise.
Therefore,
it seems to me that Peter has a valid point that the consequences of "men-behaving-badly" behaviour are far worse in today's theoretical high energy physics community than in the experimental world.

9. **Quantoken**
January 16, 2006

Peter said:

"One problem that may doom the whole idea is that, at the kinds of luminosities one need, a muon collider beam emits such an intense **neutrino** beam (from the decaying muons) that it poses a significant **radiation hazard**, one that you can't shield against."

That I don't understand! We know that neutrino is one of the least interactive particles. One claim that you could have neutrinos passing through thousands of light years thick of lead metal, and hardly a significant portion is absorbed. Neutrino beam is certainly virtually impossible to shield, but since it does not interact much with matters it does not cause any harm either. It can penetrate your body without any interaction, thus causes no health problem.

As a matter a fact, billions of solar neutrinos routinely penetrate through every square centimeter of your skin every second silently but we don't worry about it. So why the neutrino beam of the proposed device would be harzadous?

Quantoken

10. **woit**
January 16, 2006

Quantoken,

Yes, this is a surprising problem, because neutrinos interact so weakly. But to get a lepton collider to function usefully at these energies you need huge luminosities, and every muon in the beam is going to decay, giving you neutrinos (and electrons, but those you can shield). The number of neutrinos is just so huge that the interaction rate becomes a problem.

11. **Tony Smith**
January 16, 2006

As Peter said in reply to Quantoken, "… The number of neutrinos is just so huge that the interaction rate becomes a problem. …".

The paper physics/9908017 by Bruce King of Brookhaven gives some relevant equations. Here are some excerpts from King's description of the situation (the full article contains discussion of more details):
"… The potential radiation hazard comes from the showers of ionizing particles produced in interactions of neutrinos in the soil and other objects bathed by the disk. The tiny interaction cross-section for neutrinos is greatly compensated by the huge number of high energy neutrinos produced at muon colliders. … Most of the ionization energy dose deposited in a person will come from interactions in the soil and other objects in the person's vicinity rather than from the more direct process of neutrinos interacting inside a person. At TeV energy scales, much less than one percent of the energy flux from the daughters of such interactions will be absorbed in the relatively small amount of matter contained in a person, with the rest passing beyond the person. … the radiation levels rapidly become a serious design constraint for colliders at the TeV scale and above.
…
Perhaps the most direct way of decreasing the radiation levels is to greatly decrease the muon current. This can be done either by sacrificing luminosity … or, more attractively, by increasing the luminosity per given current through better muon cooling or other technological advances.
Further, one might consider placing the accelerator deep underground so the radiation disk won't reach the surface for some distance.
…
Further speculative options that have been discussed include
(i) tilting the ring to take best advantage of the local
topography,
(ii) placing the collider ring on a hill so the radiation disk passes harmlessly above the surroundings and, even more speculatively,
(iii) spreading out and diluting the neutrino radiation disk by continuously sweeping the muon beam orbit in a vertical plane using dipole corrector magnets.

Even when the preceding strategies have been used, the strong rise in neutrino energy probably dictates that muon colliders at CoM energies of beyond a few TeV will probably have to be constructed at isolated sites where the public would not be exposed to the neutrino radiation disk at all.
This would definitely be required for the 10 TeV and 100 TeV parameter sets …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

12. **Quantoken**
January 17, 2006

Tony:
I carefully read the paper you pointed to, and still do not see why the neutrino could be a radiation hazard. Assume every derivation of the author is correct, look at equation (13). The numerical factor is 2.9×10^-24. The important variable is Nu, the number of muons per second. I do not see it yielding any number close to one. Probably, the author wrongly considered Nu, the count of muons per second, as in the same order of magnitude as the Avogadro constant. If he so considered then he was completely wrong. Should Nu equal to the Avogadro constant, 6.022×10^23, that means a beam current of 6.022×10^23 * 1.61×10^-19 = 1.0×10^5 Amperes. **Ten thousand amperes**. No modern accelerator could even have a beam current any where close to even one ampere.
The beam current is confined by the limit of the amount of input energy. Let's say the machines consumes the same amount of power as the LHC, 200 mega watts (which is half a city's electricity, so not a small number!!!), and 10% of the energy consumed by the machine turn into beam energy. That's 20 mega watts, or 1.243×10^26 eV energy per second. And let say the muon is accelerated to 1 TeV (1×10^12 ev). That leads to 1.243×10^14 muons per second. And that should be the number fo Nu in equation (13).
So the total dosage would be 2.9×10^-24 * 1.243×10^14, i.e., 10^-10, in terms of order of magnitude. That's a **totally negligible** radiation dosage. And that conclusion is certainly in line with the notion that neutrinos penetrate everything virtually harmlessly.

Quantoken

13. **mike dinsdale**
January 17, 2006

Quantoken, N_u isn't defined as the number of muons per second, it's the total number whose decay will give the dose on the LHS of (13). The dosage limits you're comparing to are in Sv/year, so you need N_u to be the number of muons per year, i.e. to scale up your estimate by about 3*10^7. This gives roughly a mSv which is much more consistent with what he's saying in the paper.

Mike

14. **Quantoken**
January 17, 2006

Mike:

Thanks for that explaination. But still we do not know what a reasonable value N_u is. My estimate is way much too optimistic and too high, bounded only by the available electricity power. I assume that 10% of the total power consumed by the machine could turn into beam energy. That's probably too optimistic to be true. Also realistically I do not know how efficient a muon factory could be (how many muons they can manufacture per second). Generating muons and anti-muons is certainly much harder than just ripping electrons from regular matters by ionization. The author also assumed each kilogram mass contains 1000 moles of atoms, which is too high. For typical atomic weights, like carbon, you would be talking about 80 moles or less per kilogram. That's another factor of 10. So the realistic radiation hazard of the neutrino of such a muon collider is probably 3 or 4 orders of magnitude below the natural background radiation level.

The author never really talked about what he thought the N_u out to be. Without that number the whole paper is meaningless since he has not produced a specific result. The point I want to make is you guys are now trying to lobby the public support for a future accelerator like the muon collider. And you do not even have a clear technocal picture how the machine looks like and how many muons it produces, and you start to talk about the environmental hazard and how it hurt the public health. That's NOT an intelligent thing to do to lobby public support for your research enterprise. The smart thing to do would be get the thing onto the agenda of budget talks first and worry about radiation hazard later when it really come to the design and engineering phase.

Quantoken

15. **woit**
January 17, 2006

Quantoken,

The people thinking about muon collider designs do, believe it or not, actually have a good idea of the physics involved. Stop posting comments here criticizing them unless you actually take the time to really understand what they are doing.

They're not making a big deal of the radiation problem, it's something I brought up because I thought it was surprising that there is one. Maybe one can find a way around it, but it definitely is something that needs to be thought about before you can come up with even the outline of a viable design for such a machine.

16. **Tony Smith**
January 17, 2006

Peter said to Quantoken "… The people thinking about muon collider designs do, believe it or not, actually have a good idea of the physics involved. …
the radiation problem … it was surprising that there is one. Maybe one can find a way around it, but it definitely is something that needs to be thought about…".

Here are two more references for anyone who wants to see more about muon colliders:

The paper (with over 100 authors) at physics/9901022. It is also known as BNL-65623, Fermilab-PUB-98/179, and LBNL-41935. Its title is "Status of Muon Collider Research and Development and Future Plans".
If you read it, and look at the list of authors and institutions involved (and yes, Bruce King of physics/9908017 is one of the authors), I think that it will be clear that people working on muon colliders are NOT, to use Quantoken's language, "… guys ..[who]… do not even have a clear technocal picture how the machine looks like and how many muons it produces …".

My second reference is to a very clear presentation with graphics that was given by Gail G. Hanson (one of Quantoken's "guys", and an author of physics/9901022) at an ICFA Seminar on "Future Perspectives in High Energy Physics", CERN, 8-11 October 2002. A pdf file of it can be found at
http://dsu.web.cern.ch/dsu/of/icfapres/hanson.pdf .

Since the muon collider has not yet received huge funding for construction and operation, the people working on it are (in my opinion) primarily motivated because they love the physics involved and the potential for new and interesting observations.

They themselves have raised the issue of radiation in the area, which is not obvious at first glance because the problem is not so much direct neutrino irradiation of a person as it is the person being irradiated by secondary radiation from the person's environment
(as Bruce King said, "… Most of the ionization energy dose deposited in a person will come from interactions in the soil and other objects in the person's vicinity rather than from the more direct process of neutrinos interacting inside a person. …").
Further, they themselves pointed out that, even though a powerful muon collider would be small enough to fit on-site at either Fermilab or Brookhaven, consideration of the health of the people of Chicago or New York City indicates that a muon collider probably should be put in a more remote location.

That these people not only are working on muon colliders mostly for love of physics,
but are also honest and socially conscious enough to recommend that it be sited away from the established facilities at Fermilab and Brookhaven
(in spite of the fact that a lot of the over 100 authors of physics/9901022 have close ties to those institutions)
makes me feel that, even in this day and age, there are still some good people in the world of physics.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

17. **J.F. Moore**
January 20, 2006

Returning to the original topic, I just wanted to make the small point that some fields, particularly medicine, rely partly on donor funding to good success (and importantly, without jeapordizing other funds). I don't think it is unreasonable to develop similar mechanisms in the physics community to reach out to the philanthropists for at least some funding. This news from RHIC sets a precedent. It is not looking likely that US government funding in physics will be increasing or even keeping pace with inflation anytime soon.

Susskind NYT Book Review

January 14, 2006
Categories: Uncategorized

There's a review of Susskind's book The Cosmic Landscape in this Sunday's New York Times book review section. The reviewer does a reasonably good job of laying out what the Landscape controversy is about, characterizing Susskind's attitude as "braggadocio" with "an air of smugness", and noting that "He allows remarkably little doubt about string theory considering that it has, as yet, not a whit of observational support."

This week's Village Voice has a profile of Susskind.

Update: More about this over at Uncertain Principles.

Comments

1. **Chris Oakley**
   January 14, 2006

   > "As you know, there's something of a war against science going on," Susskind says.

   He didn't say which side he's on.

2. **Lubos Motl**
   January 14, 2006

   I also like the review.

3. **D R Lunsford**
   January 14, 2006

   What did it say, other than more mindless parroting of hyperbolic claims, followed by a "discussion of the issues" admixed with (anti)hero-worship? It's Oprah-level dilettantisme. Can't the Times find anyone with sense of history to put things in context?

   -drl

4. **Thomas**
   January 14, 2006

   Love the phrase from the review:

   "yields a gargantuan number of models: about 10^500, give or take a few trillion."

5. **arnold**
   January 15, 2006

   I still have to read the book, but the impression is that the review is very fair and well-written.

   I fear people who believe to have the truth…without any evidence.

   What strikes me the most is that clearly the idea of Landscape is on the same level as the idea of Intelligent Design: there is no evidence for none of the two: you can believe them or not.
   But some people like to say that the former is scientific and the other is not.

6. **Thomas Larsson**
   January 15, 2006

   The next-to-previous comment was not by me.

7. **mathjunkie**
   January 15, 2006

   Have read the review.

   Is it true that ID=anthropic principle, and string theory is an alternative to ID?

8. **D R Lunsford**
   January 15, 2006

   I wonder if Einstein would wear a Susskind t-shirt?

   He would probably wear a Susskind t-shirt if on it, Susskind were wearing an Einstein t-shirt…

   -drl

9. **Juan R.**
   January 15, 2006

   New piece of nonsense.

   .

Question: When stringers will take the next logical step; the multi-multiverse?

Multi-multiverse: Existence unit formed by existence regions called multiverses. In ONE multiverse, string theory is correct in others multiverses string theory is just wrong. Since number of multiverses is greater that number of universes in a single multiverse, the number of universes where string theory is incorrect is far greater that other case.

Therefore, the interest of string theory for understanding more deeply the 'nature of reality' is proportional to epsilon when epsilon tends to universe.

Sorry! 🙂

—

Juan R.

Center for CANONICAL |SCIENCE)

10. **island**
January 15, 2006

> arnold Says:
>
> I still have to read the book, but the impression is that the review is very fair and well-written.
>
> I fear people who believe to have the truth…without any evidence.
>
> What strikes me the most is that clearly the idea of Landscape is on the same level as the idea of Intelligent Design: there is no evidence for none of the two: you can believe them or not.
> But some people like to say that the former is scientific and the other is not.

This all changes if by some miracle the landscape provides string theory with the means to the ToE, right?

I mean that we wouldn't need direct evidence nor falsifiability if this were proven to be the case.

Right?

11. **john baez**
January 16, 2006

mathjunkie writes:

> Is it true that ID=anthropic principle, and string theory is an alternative to ID?

No, it's not true that intelligent design is the same as the anthropic principle!

Intelligent design is the idea that our universe was designed by some intelligent being. The anthropic principle is the idea that some aspects of our universe can be understood starting from the fact that intelligent life exists here.

(In fact there are many versions of [intelligent design] and [the anthropic principle], but let's not get into that here – follow the links if you want more details.)

It's possible for intelligent design and the anthropic principle to
both be true, both be false, or one be true and the other be false.

For example, God could have intelligently designed our universe by running a machine that builds all possible universes – this would be intelligent because then God could avoid bothering with the details. Then our universe would be one of these with intelligent life – and it's possible we could understand some aspect of our universe starting from this fact.

String theory is not an alternative to intelligent design. In fact, all 8 combinations of intelligent design, the anthropic principle and string theory being true or false are possible.

However, the astute reader will note that this discussion resembles medieval theology more than physics.

12. **mathjunkie**
January 17, 2006

Thank you John for answering my question.

It seems I know next to nothing about the differences among intelligent design, anthropic principle and string theory.

I think you are the researcher who favours LGQ more than the string theory, right?

13. **Santo D'Agostino**
January 17, 2006

Dear mathjunkie,

The nature of the intelligent design (ID) idea can be understood by analyzing the structure of their argument, without being distracted by all the details. It is:

1. There are problems with the theory of evolution.

2. Therefore God exists.

ID is therefore seen NOT as a scientific theory, but rather as an argument for the existence of God, in the same spirit as the arguments for the existence of God proposed by certain medieval philosophers and theologians. ID is just as fallacious; one can prove a mathematical theorem, but proofs of the existence of God are not possible.

Every scientific theory has problems; it is not the way of science to throw up our hands in the face of problems and say, "OK. I give up. God must be the only explanation.” The scientific approach is to analyze as clearly as possible what the problems are, and then to either revise the theory or construct a new one that is better.

Responding to island and arnold, it is interesting to compare and contrast the spirit of thought of the IDers with some of the popularizers of string theory. As argued by many people, both on this site and elsewhere, there is a clear difference between ID and string theory. The former clearly has no chance to ever be science, whereas although the latter is not yet a scientific theory, its proponents are striving to construct what one day might become a scientific theory. That is, a theory that makes specific predictions that can be confronted with observational or experimental data.

All the best,
Santo D’Agostino

14. **Steve Myers**
January 17, 2006

On anthropic principle: since any model has to be general to be useful there will be a large — possibly infinite — number of particular solutions. So can you work backwards from particulars to get a model? Not without some leap or guess or postulating a rule outside the set of particulars. You begin with the fact that Lotte has blue eyes and end with ” there is at least one such and such, such that it possess some property.” See Russell’s “Inquiry into Meaning and Truth.” Every math student has to learn that before he can understand what constitutes a proof. Look, you can always find some function (in fact, there are an infinite number) to fit any data. That’s why the curve you choose is based on data predicted. Of course, you run into the same problem again but that’s why science is ongoing, not a finished project. But isn’t all that obvious?

15. **Urs**
January 17, 2006

> are striving to construct what one day might become a scientific theory. That is, a theory that makes specific predictions that can be confronted with observational or experimental data.

I still don’t think this is the right way to put it. I’d say “people are striving to construct a phenomenologically successful theory”. There is nothing unscientific about string theory. It’s “just” phenomenologically unsuccessful. This may be reason enough to abandon it. But let’s keep the facts straight.

Consider a cosmologist who finds himself living in a universe which on large scales is not homogeneous but highly chaotic. By pure thought, he has come up with the theory of general relativity. Now he is trying to see if this theory is phenomenologically viable as a cosmological theory on large scales.

So he searches for solutions of his theory and tries to match them with the observed data.

First he tries FRW cosmologies. Most fail miserably. One, the mixmaster scenario comes closer in capturing the chaos he observes, but is still way too symmetric.

So he constructs cosmological models which are inhomogenous, depending on one, two, three, then a dozen parameters. All of them being exact solutions of GR, all of them coming a little bit closer to the cosmos he actually observes, but none being quite right.

His fellow bloggers challange him. They say “What an unscientific theory you have. Surely you must believe in alchemy and poltergeists if you also believe in your theory.”

But his theory is GR. He just happens to live in a cosmos which is not described by a simple (highly symmetric, low-parameter) solution of GR, but of a highly complex one.

So for him, GR as a cosmological theory is highly unsuccesful, phenomenologically. But it is not unscientific. He could make predictions (say of CMB spectra, of nucleosynthesis, etc.) if only he knew which solution to GR describes his cosmos. But he doesn’t know.

16. **michaeld**
January 17, 2006

Santo D’Agostino, exactly. It seems that many religious people seem to believe that the sum value of science and religion is a fixed constant, and so every failure of science is a success for religion and vice versa.

17. **island**
January 17, 2006

mathjunkie says:
Is it true that ID=anthropic principle…

Some interpretations of the anthropic principle indicate that we are not here by accident, and IDists erroneously latch onto this as evidence for god’s existence.

The Privileged Planet… by Guillermo Gonzalez and Jay Richards, is once such derivation of this sort.

While it is not correct to conclude that just because we’re not here by accident there must be an intelligent agent involved, this is exactly what IDists do, and unfortunately… skeptics are conditioned to buy straight into this hype, and so their automatic response is to try to find ways to lose the implied “specialness”… rather than to look for some good physical reason for why it might be true.

string theory is an alternative to ID?

Stringy theories that use multiverse rationale can lose any implied anthropic specialness in an infinite number of possible universes, since one of them had to be like ours, but there is no “illusion” of intelligent design without an unfounded leap of faith, regardless of what Leonard Susskind would have us believe.

It’s not a matter of either/or, as Lenny claims… either we accept the multiverse scenario, or… “without any explanation of nature’s fine-tunings we will be hard pressed to answer the ID critics.” since “the appearance of intelligent design is undeniable”.

There is no implication for intelligent design, because there is no inferrence of intelligent design from evidence for purpose in nature, without direct proof. Evidence for goal oriented design or structuring in nature does not constitute evidence for intelligent design if there is no landscape.

Lenny also doesn’t appear to know what theories are valid and which are not when it comes to origins science, because “multiverse rationale” isn’t a valid argument against fine-tuning unless and until a multiverse is proven to exist, or if multiverse “reasoning” proves to be necessary to the one true theory of everything. I think… that very last part is correct… but nobody answered my question.

18. **island**

January 17, 2006

FYI: Santo D'Agostino and michaeld are not correct as the anthropic princple applies to this debate. ID doesn't not require the faliure of science to abuse the anthropic principle as evidence for intelligent design.

19. **Santo D'Agostino**
January 17, 2006

Hi Urs,

Thanks for your comments. Your analogy has given me a very helpful perspective on string theory.

My inclination is to adopt fairly strict criteria for what we call a scientific theory. For instance, it should at least be internally consistent and it should make quite definite numerical predictions that can be tested by observations or experiments. Ideally it will predict new phenomena, but that is not strictly necessary.

It seems that based on some such strict criteria, string theory is not yet a scientific theory, although I admit that my criteria may be too strict, based on your nice analogy.

Another analogy: suppose we have a candidate theory of the hydrogen atom, let's call it theory A. In theory A there are a million different ground states for the hydrogen atom, no way to choose among them, and there is no way to predict any of the excited states. So nothing can be calculated about the hydrogen atom, no transition rates, no frequencies of emitted or absorbed radiation, etc. I would not call theory A a scientific theory, but I would admit that the effort of trying to shape it into a theory may qualify as a scientific activity.

Contrast this with another theory of the hydrogen atom, let's call it theory B. In theory B we have millions of ground states too, and no way to choose among them, but this time no matter which ground state is chosen, the theory predicts the relative energies of the corresonding excited states. In theory B at least you can predict the frequencies of emitted or absorbed radiation, and so I would be willing to call theory B a scientific theory. (The frequencies are always the same, no matter which ground state is chosen, so we still have no way of knowing what the actual ground state is in theory B.)

My impression is that string theory is more like theory A, and so I would hesitate to call it a scientific theory yet. If I am wrong, I would value any further clarifying comments that you might like to make. For instance, are there any specific numerical predictions that string theory makes that can be confronted with observations or experiments?

Santo

20. **Santo D'Agostino**
January 17, 2006

Hi island,

I'm not sure why you say

"FYI: Santo D'Agostino and michaeld are not correct as the anthropic princple applies to this debate. ID doesn't not require the faliure of science to abuse the anthropic principle as evidence for intelligent design."

since I said nothing about the anthropic principle.

I agree with your criticism of Susskind's reasoning; as I already mentioned, it is unscientific to argue for the existence of God based on one's inability to cook up a satisfactory scientific theory.

As for your question,

"This all changes if by some miracle the landscape provides string theory with the means to the ToE, right?

I mean that we wouldn't need direct evidence nor falsifiability if this were proven to be the case.

Right?"

How would you know if such a proposed ToE were a good theory? You would have to compare its predictions to observed and experimental data; in other words, you would need evidence and falsifiability.

All the best,
Santo

21. **Urs**
January 17, 2006

Santo D'Agostino

it is a pleasure to be able to have a reasonable discussion.

I would suggest to slightly modify the analogy which you proposed.

The hydrogen atom itself is a solution of a theory. Quantum mechanics itself does not predict anything about atoms. You need to feed in the correct initial data first.

Once you specify that your electron sees precisely the Coulomb potential, for some value of the one parameter describing it, you can derive atomic spectra using QM+choice of potential. You have to choose the form of the potential (~ a 1/r) as well as pick one value of the parameter a.

You fix this by fitting it to a small subset of experimental data. Then you use the fact that *given that input*, QM predicts the rest of the spectrum.

So, if people had discoverd the formalism of QM before having figured out that the H-atom consists of an electron orbiting a nucleus, they could not have used it to predict anything about the spectrum of that atom.

22. **Juan R.**
January 17, 2006

I said,

> Therefore, the interest of string theory for understanding more deeply the ‘nature of reality’ is proportional to epsilon when epsilon tends to universe.

It would say

Therefore, the interest of string theory for understanding more deeply the ‘nature of reality’ is proportional to epsilon when epsilon tends to **zero**.

–

Juan R.

Center for CANONICAL |SCIENCE)

23. **island**
January 17, 2006

> The nature of the intelligent design (ID) idea can be understood by analyzing the structure of their argument, without being distracted by all the details. It is:
>
> 1. There are problems with the theory of evolution.
>
> 2. Therefore God exists.

No, that’s not the nature of cosmological ID, which is what Lenny’s book is about.

> How would you know if such a proposed ToE were a good theory? You would have to compare its predictions to observed and experimental data; in other words, you would need evidence and falsifiability

I was asking about falsifiability of the “Landscape”, not testable predictions of a ToE.

24. **Santo D'Agostino**
January 17, 2006

Hi Urs,

Thanks for explaining to me how my hydrogen atom examples are like your cosmology example for the purposes of this discussion.

I suppose that if the formalism of QM had been discovered before it could predict anything specific, I would also say that it were not yet a scientific theory, but a formalism striving to become one. But as you said earlier, this does not mean it is not worth working on.

All the best,
Santo

25. **Santo D'Agostino**
January 17, 2006

Hi island,

I misunderstood you. I assumed you were talking about ID, not cosmological ID. I don’t know exactly what the latter means, since I have not read Susskind’s book.

All the best,
Santo

26. **Adrian H.**
January 17, 2006

Urs and Santo

I think the thing that is missing between you is the concept of explanation, as against prediction. Urs, you have pressed the idea that ‘string theory is science’ very succesfully, using the idea that it *could* make predictions. You stress the idea that prediction is often a matter of finding correct solutions given some initial conditions. As in GR, put in the values for mass-energy distribution, get out a model of space-time structure.

But I think this analogy misses something important about scientific theories that is not equivalent to prediction: scientific theories are explanatory.

Take a bad theory that is maximally predictive in your terms: one that is of the form A & -A for any A. A contradiction in other words. From that contradiction one can predict anything and everything, since any claim logically follows. But even though it will be predictively a TOE, it is obviously not a scientic theory, or if you wish to stretch the term that far, it is a scientific theory, it’s just a very bad one. A makes an infinite number of false predictions along with the true ones.

Finding a solution to the equations of GR is predictive of the structure of space time (if we found we live in the Goedel Universe, that would be an interesting fact!) but it does not explain why the mass energy distribution is the way it is, or why the speed of light has just that value, etc. Finding solutions to equations is not all, or even the main part of, the scientific enterprise.

I think the worry that is being expressed about string theory and the landscape is that—and this is the thing that pulls it closer to ID than is really comfortable—the theory is incredibly low in even potential explanatory power. “Why are things this way, rather than some other.” If the answer is that we can get our universe out (I’m being optimistic) as a solution to some set of equations, by putting in by hand a whole lot of free parameters then it is of diminished interest. Not no interest at all, just diminished.

And on the subject of explanation vs prediction, note that ID is a perfectly predictive “theory”: it predicts the life forms we see around us. We just have to put in by hand a whole lot of free parameters that describe God’s intentions!

But ID is obviously low in explanatory power. And String theory may be closer to ID than it is to GR. That’s the worry.

27. **Adrian H.**
January 17, 2006

I should add that I’m not saying that everything can be explained, just that explanatory power is something that we seek to maximize in scientific explanations.

And perhaps we should really say that ID is retrodictive, rather than predictive. Likewise the Anthropic Principle looks like a case of retrodiction: People exist therefore this universe must be one where people can exist!

28. **Fabien Besnard**
January 18, 2006

Urs, you seem relieved. But I don't think many people on this blog think string theory per se is not a scientific theory. In particular I've never seen Peter make such a claim. If I understood right Peter's mind, what he says is : 1) anthropic attemps to go out of the landscape are unscientific 2) more and more string theorists argue that unfalsifiability is not such a bad thing after all, and this is a sign that string theory has failed to achieve its primitive goals and that some people are getting nervous about this 3) it's time to try something else.
With all this I fully agree.
Peter, please correct me if I'm misinterpreting your mind.

29. **Urs**
January 18, 2006

Adrian,

thanks for the comment.

It is hard to give a precise meaning to the term "explanation". Some people feel quantum mechanics *explains* the spectra of atoms, while others are so dissatisfied with the interpretational issues of QM that they would only acknowledge that it successfully *describes* these observations, with a "real explanation" still to be found.

But besides personal feelings it does not make much of a difference which standpoint one takes.

So I would not want to engage in a discussion whether some theory "explains" enough to be worth of our considerations. A good theory is one that yields a high number of (maybe approximately) correct descriptions from a low number of assumptions.

Consider the following scenario.

Assume perturbative string theory is indeed a good description of physics beyond the standard model. Assume there are heterotic strings which are weakly coupled and spacetime is really compactified on some Calabi-Yau.

Say the currently available modles by Braun, Ovrut and others can be further refined such that one day they find a choice of Calabi-Yau, a choice of gauge bundle over that CY and a choice of "Wilson lines" (these are the sorts of parameters one has to choose) such that the low energy effective theory obtained by strings on such a compactification produces the standard model on the nose, without any extra moduli. Let me call the string vacuum thus defined SV1. Our first candidate solution of string theory which matches observed low-energy physics.

If this happened, one would want to study the postulate that the world is indeed described by SV1 in weakly coupled heterotic string theory.

This would yield quite a number of predictions. For one, it would predict that at energy scales which resolve the size of the compact CY accelerators would see signatures of these extra dimensions, and the precise topology of these extra dimensions would be predicted.

Note that this prediction might turn out to be wrong. You may even feel that it has no chance of turning out to be right. But it is a prediction.

If it should turn out to be correct, we'd have a nice theory of physics beyond the standard model. If not, we'd need to work harder. That's how science works.

Indeed, there are serious reasons to expect that this scenario has little chance of being true. One might suspect that a weakly coupled perturbative description of high energy physics is likely to miss essential physics. That a non-perturbative description will be necessary, one that does not need to expand about a fixed background.

This may be true, and people are working on it. If anyone has a good idea, it will be worth considering. If it helps to reassure people distgusted by the state of affairs in string theory, I might point out that I know a couple of string theorists who took a very close look at the recent results claimed in CDT. I wish I would not have to emphasize this.

Anyway, I would like to point out that even if we had a nonperturbative definition of a theory beyond the standard model, we'd still face the problem of identifying the correct solution of this theory which describes our world!

There is no reason to expect that, once spin foam models, CDT, AdS/CFT-like dualities or any other nonperturbative approach is well enough understood that one can talk about its space of solutions, it will turn out to provide a unique one. I expect the opposite to be true. There will be infinitely many solutions.

So even with a working nonperturbative theory beyond the standard model we'd face the the issue that predictions cannot be made before some parameters in its solution space are fixed. Fixed somehow.

30. **D R Lunsford**
January 18, 2006

Peter – you handled that very well over on the blog "Uncertain Principles". (Is there a rule that blogs have to carry cute names?) The problem is, correct arguments seem to have lost their value. I was looking at the postings to a right-winger's blog the other day – e.g. one guy offers a detailed, fact by fact, unemotional indictment of the Bush admin, and it just didn't make any difference to the dittoheads in the audience – they simply refused to allow themselves to be tied to facts.

Somehow the art of actually paying attention to an argument has gone missing

-drl.

31. **Thomas Larsson**
January 18, 2006

Say the currently available modles by Braun, Ovrut and others can be further refined such that one day they find a choice of Calabi-Yau, a choice of gauge bundle over that CY and a choice of "Wilson lines" (these are the sorts of parameters one has to choose) such that the low energy effective theory obtained by strings on such a compactification produces the standard model on the nose, without any extra moduli.

This could happen, of course, but I think everyone around here agrees that the odds are so small that they can safely be ignored. If so many bright people haven't gotten any quantitative predictions out of string theory in 20 years, why should things change now? Haven't people turned to the Landscape precisely because they have given up hope that any useful prediction will ever come out of string theory?

Moreover, one must not forget that all natural predictions from string theory, like SUSY, extra-dimensions, 496 gauge bosons, new long-range forces, etc., are in apparent disagreement with experiments. A particularly impressive class of experiments seems to be the search for permanent electric dipole moment, which has been blogged about here, here, here, and here. It may seem strange to use experimental results to argue about theoretical physics, but I strongly feel that the by far simplest explanation for this apparent disagreement with observation is that it is due to a factual disagreement with observation.

Besides, didn't we agree that the correct theory of QG in 4D must allow for diff anomalies, as I understand section 6.1 of Nicolai et al.? If string theory does not allow for such anomalies, how could it possibly be right? Even if you dislike my treatment of anomalies, you cannot reasonably deny that the absense of 4D diff anomalies is fatal?

32. **woit**
January 18, 2006

Fabien,

Yes, I'd basically agree with your summary.

Fabien and Urs,

I'm careful not to say things like "string theory is not science". Some things string theorists are doing are science, but some are not. Very specifically what I claim is not science is when string theorists like Susskind say they accept that string theory leads to 10^500 vacua, but then are unable to come up with a remotely plausible idea of how they are going to use the theory to make falsifiable predictions in this case. The only kind of "predictions" he talks about are the anthropic ones that are not falsifiable and aren't really scientific.

Lots of string theorists now seem to be saying that, while they reject the anthropic arguments, even if string theory does have 10^500 vacua, that is no reason to give up on it. My point of view is that anybody who wants to claim this has to come up with a plausible scenario for how this is ever going to lead to predictions. From everything I've seen, no one can do this, so it appears to me they are just defending the indefensible and not doing science.

The problem with Urs's SV1 scenario is that it is exactly what people have been looking for for more than 20 years and not finding. Braun, Ovrut et. al. aren't able to fix the moduli problem without doing something like KKLT which leads to the 10^500 possibilities, and likely complete loss of predictivity. Sure if this problem magically goes away, you'll have something predictive and be doing science, but right now this looks like pure wishful thinking to me, and wishful thinking that deadly problems will just disappear is not how science is supposed to be done.

33. **Urs**
January 18, 2006

Peter,

I tried to point out how a theory generically has infinitely many solutions. So it's not clear to me why it should be a problem if some theory has only finitely many. And this holds for strings only after some conditions have been imposed. The space of *all possible* string vacua (not restricting to flux CY compactifications) is still infinite, as far as I am aware.

I also tried to point out how, generically, from the space of all solutions one has to pick one by matching it to a subset of experimental data. Only then are predictions possible. I tried to give several well-understood examples for this.

Some people are trying to go beyond what can usually be done. They don't want to just find the right solution, but also understand "why" it is the one we observe. As with all "why" questions in science, trying to answer them tends to lead into non-scientific territory.

34. **woit**
January 18, 2006

Urs,

I'm repeating myself, but again, here's the main issue: if you can't predict anything you aren't doing science. If it is true that all these flux vacua really are legitimate string theory solutions, and one can get almost any value of the standard model parameters out of this, how are you going to predict anything? I'm still claiming that there is no plausible scenario in which this line of research leads to a testable scientific prediction. If you know of one, let's hear it. Saying that we'll be able to fix the Calabi-Yau, then make predictions isn't plausible. There's no reason to believe you will be able to fix the Calabi-Yau, to show that one works the (possibly infinite set of) others don't. And "predictions" of Planck-scale stringy effects are dubious for both practical (you can't ever do such experiments) and theoretical (perturbation theory isn't good enough) reasons.

I actually think it is only the people asking "why" questions who are still doing science, since it is not inconceivable that if you could answer the question of "why" a certain vacuum state, then you could make predictions. The people I have a problem with are those like Susskind, who claim that even if you never understand "why" a certain vacuum, you can still do science in this particular context.

35. **Urs**
January 18, 2006

> I'm repeating myself

True. And I would have to, too, if I tried to reply. So I'll leave it at that.

36. **Who**
January 18, 2006

Urs 18 January 4:13 AM
"There is no reason to expect that, once spin foam models…is well enough understood that one can talk about its space of solutions, it will turn out to provide a unique one. I expect the opposite to be true. There will be infinitely many solutions."

Having a space of solutions which is controlled by a few fundamental constants like G and h-bar is qualitatively different from having a huge out of control space of solutions, so we ought to make the difference clear.

Urs, I disagree with what you say as applied to the particular case of the spinfoam model of http://arxiv.org/hep-th/0512113
In that paper Freidel and Livine work out a model of 3D spacetime and matter which has QFT as the zero-gravity limit (as G -> 0) and General Relativity with some quantum corrections as the (semi)classical limit (as h-bar -> 0).

In that paper I do not see other constants which can vary to give a large space of solutions. However they still have to extend their results to 4D spacetime and matter.

Still I think there IS reason to expect that when and if the Freidel Livine result is extended to 4D it WILL turn out to provide a small space of solutions controlled by a small set of parameters—like a few fundamental constants.

Perhaps I am just more of an optimist than you, Urs 🙂

37. **Tony Smith**
January 18, 2006

Urs said "… Say the … modles … can be … refined such that … the low energy effective theory … produces the standard model on the nose, without any extra moduli. Let me call the string vacuum thus defined SV1. …".

Peter said "… The problem with Urs's SV1 scenario is that it is exactly what people have been looking for for more than 20 years and not finding. Braun, Ovrut et. al. aren't able to fix the moduli problem without doing something like KKLT which leads to the 10^500 possibilities, and likely complete loss of predictivity. …".

Urs then said "… from the space of all solutions one has to pick one by matching it to a subset of experimental data. Only then are predictions possible …".

It seems to me that Urs's reply does not deal with what I see as Peter's main assertion:
Even with many very smart people working for over 20 years, nobody has yet produced a concrete conventional superstring model that
1 – has no obvious fundamental flaws (moduli problem etc)
and
2 – agrees quantitatively with a significant subset of the Standard Model parameters (say, for example, ANY ONE of the following: the fine structure constant; or the weak boson mass scale; or some fermion masses; or the relative strength of the color force; etc).

Only such a concrete conventional superstring model could qualify for Urs's SV1.
IF it were to be found, THEN Urs could say that it matched "a subset of experimental data" and therefore use it to make "predictions" a la SV1.

However, since there 20 years of work by many very smart people has failed to find such an SV1, it seems reaonable to me to conclude:
that it is unlikely that further work along the same lines (conventional superstring theory) will ever find such an SV1,
and
that other approaches should be tried.

As to what kind of "other approaches", consider the remark of Pierre Ramond in hep-th/0112261 "… Nature relishes unique mathematical structures. …".
Maybe it might be reasonable to work on the possibility that the "why" question:
whether the observed set of Standard Model parameters might be unique
could be answered by "because that set is determined by a unique exceptional mathematical structure".

Although I disagree with some other positions taken by Ramond, I do agree that exceptional structures might be a basis for construction of unique and testable physics models. Two examples of approaches using exceptional structures are (as I have mentioned in other comments):

hep-th/0104050 in which Lee Smolin said: "A new matrix model is described, based on the exceptional Jordan algebra, J3(O). … There are 27 matrix degrees of freedom, which under Spin(8) transform as the vector, spinor and conjugate spinor, plus three singlets, which represent the two longitudinal coordinates plus an eleventh coordinate. Supersymmetry appears to be related to triality of the representations of Spin(8).".
and
hep-th/0012037 in which Horowitz and Susskind also deal with 27-dim structure that may be related to the exceptional Jordan algebra J3(O), saying: "We … try to interpret bosonic string theory as a compactification of a 27 dimensional theory. We will refer to this theory as bosonic M theory.".

Personally, I am disappointed that now, 5 years after those papers, Smolin and Susskind seem to be working in other directions.

I am particularly disappointed that Susskind describes his current direction in his 2005 book The Cosmic Landscape as:
"… String Theory will almost certainly have Rube Goldberg complexity and redundancy …
I don't know an equation to describe it, only a slogan: "A Landscape of possibilities populated by a megaverse of actualities." …
… [nothing] … in this book diminish[es] the likelihood that an intelligent agent created the universe for some purpose. …".

Another possible "other approach" might be the emergent vacuum picture advocated by Laughlin and Chapline (popularly described in Laughlin's book A Different Universe (Reinventing Physics from the Bottom Down)). Here is Susskind's criticism of Laughlin's approach:
"… Superfluid helium is an example of a material with special "emergent" properties … In a lot of ways, superfluids are similar to the Higgs fluid that fills space and gives particles their properties. Roughly speaking Laughlin's view can be summarized by saying that we live in such a space-filling material. He might even say … space IS such an emergent material! Moreover, he believes that gravity is an emergent phenomenon. …
There are two serious reasons to doubt that the laws of nature are similar to the laws of emergent materials. …
The first … Laughlin himself …[argues]… that black holes (in his theory) cannot have properties, such as Hawking radiation, that practically everyone else believes them to have …
[second]… insensitivity to the microscopic starting point is the thing that condensed-matter physicists like best about emergent systems. But the probability that … there should be one … endpoint … with the incredibly fine-tuned properties of our anthropic world is negligible. …".

It is sad to me that Susskind's first criticism of Laughlin assumes that "practically everyone" believes that black holes have Hawking radiation, when, in fact, even Hawking himself has repudiated (see his Dublin 2004 abstract at http://www.dcu.ie/~nolanb/gr17_plenary.htm#hawking) information loss by Hawking radiation.

It is also sad that Susskind's second criticism of Laughlin assumes that the process of emergence CANNOT produce an "endpoint" with precisely the properties that our experiments observe.
Perhaps a spin foam with J3(O) nodes might produce our observed universe,
similar to the emergence of superfluid helium from helium atoms.
Unless and until the physics community encourages research work along such lines, how will we know the answer?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

38. **dan**
January 19, 2006

Dear Urs

"I tried to point out how a theory generically has infinitely many solutions. So it's not clear to me why it should be a problem if some theory has only finitely many."

Is it possible to work backwards, from the standard model, and particles like neutrinos, electrons, quarks, and show how they are solutions to string theory? knowing what we know about electrons, is it possible to show their properties such as charge, spin, mass, etc cetera, be explained in reference to a string theory as one solution among infinitely many?

to use your other examples, while Newton's laws of motion and General Relativity do not predict what distance the sun's planets should be, they include solutions such as acceleration and force, within the framework of newton/Gr. Can the same be done with string theory?

39. **Urs**
January 19, 2006

> IF it were to be found, THEN Urs could say that it matched "a subset of experimental data" and therefore use it to make "predictions" a la SV1.

Yes, and that's what I said. If such a solution were found, it could be used to make predictions. As in: If you have identified which FRW solution matches large scale cosmology, then you can use it to make further predictions.

40. **Urs**
January 19, 2006

> Having a space of solutions which is controlled by a few fundamental constants like G and h-bar is qualitatively different from having a huge out of control space of solutions, so we ought to make the difference clear.

The space of solutions that we are talking about is essentially the space of solutions to the effective field theory equations of string theory, which look like GR+YM+ higher order corrections+this and that. This space is large in precisely the same sense as the space of solutions to the equations of pure GR is large. There are many possible configurations of the system. (Infinitely many, usually.)

This has nothing to do with the freedom in choosing dimensionful constants like G or hbar. And it is the generic behaviour of non-perturbative theories, too. There is no reason to exepect that they allow precisely one single configuration.

41. **woit**
January 19, 2006

> The space of solutions that we are talking about is essentially the space of solutions to the effective field theory equations of string theory, which look like GR+YM+ higher order corrections+this and that. This space is large in precisely the same sense as the space of solutions to the equations of pure GR is large.

The size of the space of solutions is thoroughly irrelevant. What is relevant is their structure: can you use some experimental data to fix the solution, then use it to make falsifiable predictions for the rest of the data or for future data? In GR you have no trouble doing this, in the string theory landscape you can't. The first is a science, the second isn't.

42. **Urs**
January 19, 2006

> The size of the space of solutions is thoroughly irrelevant.

Yup. Because it's generically very large, in any case.

> What is relevant is their structure: can you use some experimental data to fix the solution, then use it to make falsifiable predictions for the rest of the data or for future data?

Exactly. That's what I am saying all along.

> In GR you have no trouble doing this

Yes, GR is much easier to handle.

> in the string theory landscape you can't

I don't know of a theorem that one cannot. But certainly it hasn't been accomplished yet.

> The first is a science, the second isn't.

Are you saying string theory is a science but studying its space of solutions is not?

I think what you really mean is that the particular way this space of solutions is currently "investigated" by Susskind and others is not science.

43. **woit**
January 19, 2006

> Are you saying string theory is a science but studying its space of solutions is not?

"String theory" is lots of different things, some scientific, some not. Studying the "solutions to string theory" is scientific until the point at which you show that there are so many, capable of describing almost everything, that you can't come up with a plausible way you'll ever get predictions. If you keep working on this after that point, you're not doing science, you're just hiding your head in the sand and refusing to admit your theory has failed.

The anthropic nonsense Susskind has come up with to try and evade the implications of not being able to predict anything is one form of non-science. But I also don't think spending your time working out the details of these solutions is science if your only argument for doing this is wishful thinking: "maybe some miracle will happen and this stuff will allow predictions, even though I can't now plausibly see how this could happen".

44. **Alejandro RIvero**
January 19, 2006

It seems as if the "string theory" were now an alternative no to GUT, but to Quantum Mechanics.

45. **Urs**
January 19, 2006

> even though I can't now plausibly see how this could happen

The progress from a large number of moduli to a mere 13 was obtained by understanding the space of possibilities in more detail. Constructing these compactifications requires quite some insight into the geometry and topology of the CY appearing there. There are still simplifying assumptions in these constructions (for instance that the CY is elliptically fibred) which are made only because otherwise current technology would not allow to compute the properties of these beasts (and hence compute the predictions these solutions make).

So it's a matter of understanding the space of solutions, which is quite complicated. Currently, claims that there is a phenomenologically viable solution in that space are just as plausible or implausible as claims that there cannot be any.

46. **Ethan**
January 19, 2006

I have a problem with Urs's analogy to GR, and I think it touches on my difficulties with seeing the landscape as an interesting step to understanding the physics of the universe.

No one ever evaluated GR by comparing cosmological models. What actually happened was that people either constructed satisfactory cosmological models using GR, or using their favorite competing theory of gravity. If an alternative theory was ever discarded on the basis of cosmological observations, I'm not aware of it. GR is validated by relatively local tests, including solar system tests, and that lends credibility to our attempts to do cosmology with it, not vice versa. Our hypothetical cosmologist in a chaotic universe would believe in GR for the same reasons we do, it just wouldn't be a very useful cosmological tool for him/her/it. Understanding the universe in this case would be a lot like trying to understand the weather.

Supposing that some version of string theory is correct, and that our current understanding of the landscape is correct, i.e. that it gives us a finite, but absurdly large number of possible low energy limits. In this case it seems to me that we learn something interesting about our universe only by ignoring the landscape and exploring less ambitious phenomenological models. I can understand pursuing string theory because one hopes to narrow the range of possible low energy states to a usefully small number of choices. What I can't understand is embracing the landscape in its current form as a useful kind of physical theory.

47. **Chris W.**
January 19, 2006

Peter,

On the topic of wishful thinking, I think it's worth quoting your reminiscence about an encounter you had with Philip Anderson when you were at Princeton:

> Phil Anderson has always been somewhat of an intellectual hero for me. He's really the person who discovered the Higgs mechanism, among many other things. Despite a reputation for being a curmudgeon, at one point he was quite kind to me. At some sort of social event at Princeton to mark students passing their generals, he came up to me and told me that he had graded my solid state physics exam. He complimented me on one problem in particular, one I had got wrong. I had realized something was wrong with my solution of that problem, noting on my exam that the result I was getting couldn't be right and explaining why. **He told me that this had impressed him, that one should always *know what the result of a calculation should look like* before attempting it.**
> [emphasis added]

This was something that I recall being emphasized very strongly in my undergraduate education (as a physics major). It would be interesting to discuss where and when this guideline should be applied.

I'm sure some people would argue that it is based on an unjustified faith in physical (or mathematical) intuition, in domains where such intuition can't be relied upon. I would argue that if one's intuition is unreliable, then one should improve it by *testing it* and carefully studying those instances where it fails, ie, by identifying and elucidating the hidden assumptions that led one astray. In other words, instead of looking for a better oracle, learn to think critically—learn to make effective use of trial and error at every level you can.

(Einstein once expressed his exasperation with a certain theoretician by saying that "the man can calculate, but he can't think".)

48. **Urs**
January 19, 2006

I think the analogy to GR applied to cosmological scales is quite good. There, too, we pick a classical solution to some equations of motion, which, once fixed, predicts properties of quantum fluctuations about this solution, e.g. CMB spectra.

The reason that we can test GR locally and perturbative ST not is a matter of energy scales, not of principle.

49. **woit**
January 19, 2006

> The reason that we can test GR locally and perturbative ST not is a matter of energy scales, not of principle.

No, the reason we can test GR is that we have a consistent model of GR low energy behavior that
1. isn't completely incompatible with observations
2. once you choose a few parameters it is unique and makes real predictions.

In the case of perturbative string theory, if you don't turn on fluxes and play the other sorts of tricks KKLT does, you have unstabilized moduli and your theory is incompatible with observations. That's why you can't use the work of the people Urs is quoting to predict things, not the problem of the energy scales. If you do something like KKLT, again you can't predict anything, now because it can lead to just about any low energy physics (and, again, I'm dubious that the theory is under sufficient control at high energies to do reliable calculations).

This problem with the moduli has been around since the beginning, and all attempts I've seen to get around it have had huge problems of one sort or another. One can believe that with more work along the same lines someone will make it go away, but everything I've seen in following this more than 20 year history and looking at what people are doing now leads me to strongly believe that this is just wishful thinking.

50. **Urs**
January 19, 2006

Peter,

the problem you mention is the problem of finding a phenomenologically viable background. The reason why we don't check perturbative ST "locally" (simply by producing strings in the accelerator) is one of energy scales.

51. **Tony Smith**
January 19, 2006

With respect to finding a conventional superstring model that has no obvious fundamental flaws (moduli etc) AND agrees quantitatively with a significant subset of the Standard Model parameters:

Peter said "… This problem with the moduli has been around since the beginning, and all attempts I've seen to get around it have had huge problems of one sort or another. … everything I've seen in following this more than 20 year history and looking at what people are doing now leads me to strongly believe that this is just wishful thinking. …".

Urs said "… Currently, claims that there is a phenomenologically viable solution in … the space of solutions … are just as plausible or implausible as claims that there cannot be any. …".

So,
Peter sees the failure of many very smart people working over 20 years to find a "phenomenologically viable" conventional superstring model as an indication that it might be a bad bet to expect success from similar people doing similar work over the next few decades,
and
even Urs agrees that there is no more reason to expect success than to expect failure from continued work on conventional superstring theory.

Given that,
it seems to me absurd to continue to devote 90% of theoretical physics funding and jobs to conventional superstring theory,
and
it seems sensible to divert half of those funds and jobs to alternative approaches, such as exceptional math structures, emergent spacetime, J3(O) spin foam, etc,
and
it seems to me that such a diversion is exactly what NSF, DOE, and university administrators should undertake NOW.

While I expect that conventional superstring theorists would cry in pain and outrage at losing money and jobs (just look at how rabidly they defend their turf nowadays), why should they be treated any differently from USA manufacturing and IT workers who lost jobs that were outsourced to China and India ?

If conventional superstring theorists are really "the smartest guys in the room", then they should have no trouble retraining and adapting to a new environment.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

52. **Christine**
January 19, 2006

Regarding the reviewer´s comment on Susskind's book, as well as the set of very interesting comments on this post, I´d like to mention that I have recently found this talk from Dr. Andrew Chamblin (dated Feb. 24, 2004). A further (brief) search on the net (arxiv, etc) did not allow me to find a transparancy of his talk or a related paper (if you know about it, please let me know). His assertion (see full abstract from the link above) seems fragile to me because it is as if one is blindly relying on the "reality" of the cosmic acceleration in order to use it as a valid guideline for the correctness of the approach (I would like to wait a lot more to rely on this, see, e.g., astro-ph/0601377 and astro-ph/0511628, as well as the delay on 2nd year WMAP data release; there are several rumors on this). If it turns out that the cosmic acceleration is not confirmed then it seems that string theory can be simply "adjusted" to fit that (negative) observation as well. I think this is just a practical example on some points that are being discussed here.

53. **Dumb Biologist**
January 19, 2006

If I understand things remotely correctly, testing hypothetical GUT-scale physics has typically involved staring (with photomultipliers, of course) at vast quantities of ultra-pure water for years, waiting for the signature flash of light that will reveal proton decay. Presently, no protons have been observed to decay. One could conceivably build a much bigger tank, so maybe there's still some hope for testing some of the GUTs, but the outlook seem less optimistic than it once did, so I'm told.

Testing Planck-scale physics, being the energy realm virtually all agree is where gravity must be just as important as Standard Model forces, is apparently going to be a lot more difficult.

We're told one might *not* need galaxy-sized machines to probe physics relevant to the quantum gravity realm if, and only if, some best-case scenerios of various speculative q.g. approches pan out (e.g., compactified dimensions are a lot bigger than the Planck length, and hence a humanly-constructible accelerator can achieve the energies required to see the new physics such not-so-small extra dimensions give rise to).

It seems what's being said is that experimental contact with quantum gravity might only be possible if we are lucky enough to be living in a universe where things not only aren't what they seem to be, they aren't what they seem to be in a terribly fortuitous way, something perhaps to the tune of tens-of-orders-of-magnitude in terms of a particle's energy.

Otherwise, maybe quantum-gravitational effects only manifest at Planckian energies.

Watching from the seat in the bleachers provided by popularizers of physics, I've come to wonder if Unification is something mere mortals can reasonably aspire to any time in the next millennium or so. Is there presently a rational reason to presume otherwise? When does it become clear this is or is not physicists' predicament?

54. **Chris W.**
January 19, 2006

D. B.,

There is a counterargument that should be mentioned. When modern physics began, atomism lay firmly in the realm of metaphysical speculation, inherited from certain thinkers of antiquity. It gradually became grist for the theoretical mill of early physics and chemistry, and only after two centuries or more assumed an unassailable role in science. This happened because atomism proved to be an extraordinarily fruitful basis for *framing* a great variety of questions about matter, many of which led to the formulation of precise and testable hypotheses. The point is that atomism began to show its value long before we had incontrovertible empirical evidence that the atomic structure of matter was a fact. (You will note strong echoes of this in some central ideas of modern biology.)

I strongly suspect that something like this will happen with many of the problems that currently fall under the headings of quantum gravity, the unification of fields and forces, cosmology, and the foundations of quantum theory (and quantum field theory). To many people string theory feels like—or, at least, used to feel like—such an idea. I rather think it is more like a muddled hint that such an idea exists, but is not the idea itself. Nevertheless, I think such an idea is needed; we can't even clearly identify the relevant observations and paths of experimental as well as theoretical investigation without it. With it, we may see the relevance and importance of questions that the presuppositions of the Standard Model and it close relatives (GUTs) never prompted us to ask. In a number of ways I think this has already begun to happen.

55. **Tony Smith**
January 19, 2006

Dumb Biologist (actually, maybe a biologist, but certainly not dumb) said:
"… Presently, no protons have been observed to decay. …".

That is clearly the consensus view, but it may or may not be correct.
Adarkar, Krishnaswamy, Menon, Sreekantan, Hayashi, Ito, Kawakami, Miyake, and Uchihori, authors of a paper hep-ex/0008074 entitled
Experimental evidence for G.U.T. Proton Decay
say:
"… in Kolar … an experiment to detect proton decay has been carried out since the end of 1980. Analysis of data yielded … the life time of the proton is about 1 x 10^31 years …
A number of other experiments have also looked … The present consensus among these other experiments seems to be that they have not found any evidence for proton decay yet, and that the lower limit on the lifetime of proton is of the order of 10^33 years. …
The apparent contradiction between these conclusions does not mean a complete disagreement between the observations. … In our opinion, there are many points of agreement between the observations in other experiments and ours …
Since the total number of events to be analyzed and discussed is rather small, we have not fixed any special criterion to select the candidates for nucleon decay at the very beginning and we have tried to understand each event as it is. …".

It seems to me that the validity of the consensus view of proton decay rests upon choices made in analysis of the experimental data, which consists of only a small number of candidate events. If the analysis choices made by Adarkar et al were to turn out to be correct, then some types of SU(5) GUT models might not in fact be ruled out by experiment.

As an illustration of how different analysis choices affect results, here is an excerpt from the Adarkar et al paper:
"… The IMB group has reported … that they have found 4 candidate events for e+ pi0 during the observation of about 4 kty. Out of these,
two events have been rejected because of their association with muon decay signals.
The other two events, according to their analysis also have some difficulty to be considered as due to proton decay phenomenon.
One of them has a concentrated Cerenkov light cone which may come from a slow
proton and
the other event has an extra light cone due to a lower energy particle.
However, if these features are ascribed to fluctuations in cascade showers, these two events may remain as candidates for proton decay. Assuming such an interpretation, their observed rate of candidate event becomes close to our results. …".

It is my opinion that the experimental validity of proton decay by SU(5) GUT should remain an open question until all reasonable alternative analysis choices are taken into account.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

56. **D R Lunsford**
January 19, 2006

DB – to risk echoing Tony, the proton may yet decay but the cool theory with proton decay, the SU(5) gauge theory, is ruled out because not enough of them decay in a given interval. It's wrong by something like a factor of 100. I think everyone was surprised by this failure, since the SU(5) theory has a lot of nice features. That it failed, in a sense cast doubt on the structure of the standard model itself, to be sure mostly unspoken. It's just a nagging "physicist's feeling" that if SU(3)xSU(2)xU(1) were really correct, and you assumed that the strong and electro-weak forces are just parts of a whole, then SU(5) should work.

(It may yet work, if say the theory of the electro-weak interaction changes fundamentally, something that is certainly possible, given current neutrino research.)

-drl

57. **woit**
January 19, 2006

About SU(5):

I haven't looked carefully at the argument that the analysis of the proton decay experiment is wrong that Tony mentions, but I'm very skeptical for purely sociological reasons. Georgi and Glashow have a potential Nobel prize riding on this, so I find it hard to believe that they or their collaborators would be ignoring or downplaying evidence that they might be right.

I was never such a big enthusiast for GUTs like SU(5), because they have nothing to say about the big problem, where electroweak symmetry breaking comes from, and they require an even more elaborate Higgs sector. You "unify" things into SU(5), but then you have to break the unified symmetry back down to SU(3)xSU(2)XU(1) by putting in an adhoc Higgs sector. Not aesthetically convincing….

I think D.B. has it exactly right that the problem with quantum gravity is that it looks all too possible that there's no way to measure its effects. My own point of view is that an interesting solution to quantum gravity has to also tell us something about particle physics. That would be real unification and allow testing of the idea, whatever it was. The fact that string theory might potentially do this is why people got so interested in it. Too bad it doesn't work…..

Urs,

Unless you know what the background is, even if you have arbitrarily high energies available, string theory can't predict what will be seen. The problem is that the theory is unpredictive, not that we don't have the energy to check its predictions.

58. **Dumb Biologist**
January 20, 2006

Didn't know there was any controversy about proton decay results. Perhaps a larger detector could resolve it. At any rate, from what I've read, an instrument of that nature also makes a great neutrino observatory, so cosmologists and particle physicists still win, GUTs or no. It's a worthwhile project, and I'd heartily endorse it…

I guess I'd like to say, witnessing the plight of q.g. theorists is hardly a source of schadenfreude, or any other satisfaction, for me. Whether they fail or succeed, I'm at least intelligent enough to recognize their brilliance, and admire them for it. I do admit I get more than a little disturbed when some of them appear to be promoting the idea that experments can wait 1000 years if that's what it takes. It, uh, bums me out, but after my blood pressure returns to normal, I'm inspired to write my congressman and ask him to give these people a pile of money to build yet a bigger machine more than anything.

As for atomism: I thought the idea started to become much more than spurious physics, or a philosphical curiosity, when things like the ideal gas law, and other concepts dependent on statistical mechanics, started doing a very good job of describing what we see…not to mention some earlier insights from the development of stoichiometric rules in Dalton's day, and so forth. Probably there were earlier evidential hints of atomism I'm ignorant of or forgetting, but if we go back in time much further, we're entering a period where what we know as the scientific method was still in its nascent stages, and it seems hardly fair to judge those thinkers by our standards for empiricism. At any rate, many decades, maybe even well over a century before Einstein's paper on Brownian motion, it seems like there were very good, experimentally verified reasons to think matter was composed of "elemental" particles.

Perhaps I'm wrong, but sometimes I look at q.g., and it reminds me a bit of the plight of scientists exploring the origins of life. I mean, really, the entire field is predicated on the notion that there's a naturalistic, and relatively unexotic (compared to, say, aliens seeding the early Earth, or even panspermia) explanation that will rise from the primordial soup, so to speak. Given the alternatives, it's certainly the most attractive bias, and some preliminary results with autocatalyzing ribozymes certainly lend credence to some models of pre-biotic processes. Too bad RNA is so fragile. Whatever came before it (and it's generally thought something self-replicating probably must have), we've nothing but conjecture. At best we can say what, in principle, might have been possible. Beyond that, convincing verification of abiogenesis hypotheses, or, more ambitiously, a bona fide theory about how life arises, seem virtually unattainable. The evidence was lost billions of years ago, and we've only got our own planet to study, with a paltry n=1 informing all of our data about "life". Lots of good, ideas; no definitive way to test them. Fortunately, there's lots of other things to work on.

Anyway, please forgive the wet-and-squishy bio-hijack.

59. **Aaron Bergman**
January 20, 2006

"Didn't know there was any controversy about proton decay results."

There's no controversy. The damn thing just hasn't decayed yet. This pretty much rules out ordinary SU(5), and, IIRC, susy SU(5) is hanging by a thread at best. SO(10) GUTs aren't ruled out yet, as I understand it, and has some attractive features in relation to neutrino masses.

60. **Juan R.**
January 21, 2006

Thomas Larsson said,

> This could happen, of course, but I think everyone around here agrees that the odds are so small that they can safely be ignored. If so many bright people haven't gotten any quantitative predictions out of string theory in 20 years, why should things change now? Haven't people turned to the Landscape precisely because they have given up hope that any useful prediction will ever come out of string theory?
>
> Moreover, one must not forget that all natural predictions from string theory, like SUSY, extra-dimensions, 496 gauge bosons, new long-range forces, etc., are in apparent disagreement with experiments. A particularly impressive class of experiments seems to be the search for permanent electric dipole moment, which has been blogged about here, here, here, and here. It may seem strange to use experimental results to argue about theoretical physics, but I strongly feel that the by far simplest explanation for this apparent disagreement with observation is that it is due to a factual disagreement with observation.

Fantastic resume!

People -specially young one- does not know the history of the field. Since the very beggining of string theory in the strong force (last 1960s) until TODAY, string theory has been a complete failure.

It is often claimed that string theory cannot be tested. This is just untrue. All direct and indirect experimental verifications of string theory (including cosmic strings or mm-range esxtradimensions) in last near 40 years have obligated to string theorists to rewrite the theory in many ways. This has been denunciated by a number of physicists such as Nobel Prize Laugling.

And what is the final outcome?

A theory with hundred of hundred of hundred of **unspecified** parameters (instead of the few experimentally measured parameters of the SM), that cannot explain anything (**it is not predictive**) and in clear contradiction with other well-versed fields of science and with experimental data.

For example:

1) usual world is non-masless, still string theory only can rigorously deal with non-masless states.

2) The only gravity contained in typical superstring is a gravition over a flat classical spacetime. This is not GR, because GR is not a gravition over a flat spacetime with causal structure defined on the background. Precisely this is the main criticism of LQG and the search for one future M-theory…

3) Far from conjetures and half-trues the SM cannot be derived from string theory.

4) String theory requires lot of unobserved things (SUSY) but explain none of oberved things. If SUSY is observed at HLC last years, this is not a predicition of string theory not indicates that string theory is correct.

It is not a prediction because one may introduce the previously known SUSY on the old-stringy framework by pure consistency of the theory, not because was predicted from. Moreover, In nature SUSY, if observed at high energies, is not observed at usual energies.

String theory claims SUSY to all energies and therefore is in clear contradicition with experiment. No string theorists has proved how at high energies universe is SUSY but at low energies it looks non-SUSY and we observed SM of particle physics.

Conclusion: We obtain a theory that is not defined, is not scientific (not predicitive), contradicts our current scientific knowledge, ignores theorems proved on other fields of science (thermal science, complexity, decoherence and quantum measurement, etc.) deals with 10^500 'universes' but none of the hypotetical universes looks like our universe, is mathematically very deficient (math is very outdated in several ways: HSs, Calaby Yaus and G2), and aestetically very ugly (take for example the 'compactification' of the bosonic 26D into the superstring hibrid), etc.

—

Juan R.

Center for CANONICAL |SCIENCE)

LQG for Skeptics

January 20, 2006
Categories: Uncategorized

An interesting paper appeared on the arXiv yesterday, by Hermann Nicolai and Kasper Peeters, entitled [Loop and spin foam quantum gravity: a brief guide for beginners](). It includes some of the same material as an earlier paper [Loop quantum gravity: an outside view]() that they wrote with Marija Zamaklar.

Nicolai and Peters (as well as Zamaklar) are string theorists, and given the extremely heated controversy of the last few years between the LQG and string theory communities over who has the most promising approach to quantum gravity, one wonders how even-handed their discussion is likely to be. They identify various technical problems with the different approaches to finding a non-perturbative theory of quantum gravity that are often referred to as "LQG". I'm not an all an expert in this subject, so I have no idea whether they have got these right, and whether the problems they identify are as serious as they seem to claim. Their main point, which they make repeatedly, is that

.. the need to fix infinitely many couplings in the perturbative approach, and the appearance of infinitely many ambiguities in non-perturbative approaches are really just different sides of the same coin. In other words, non-perturbative approaches, even if they do not `see' any UV divergences, cannot be relieved of the duty to explain* in detail *how the above divergences `disappear', be it through cancellations or some other mechanism.

What they are claiming seems to be that LQG still has not dealt with the problems raised by the non-renormalizability of quantum GR. They don't explicitly make the claim that string theory has dealt with these problems, but the structure of their argument is such as to imply that this is the case, or that at least string theory is a more promising way of doing so. Their one explicit reference to string theory doesn't really inspire confidence in me that they are being even-handed:

The abundance of `consistent' Hamiltonians and spin foam models … is sometimes compared to the vacuum degeneracy problem of string theory, but the latter concerns different solutions *of the* same *theory, as there is no dispute as to what (perturbative) string theory* is. *However, the concomitant lack of predictivity is obviously a problem for both approaches.*

While they are being very hard on LQG for difficulties coming from not being able to show that certain specific constructions have certain specific properties, they are happy to state as incontrovertible fact something about string theory which is not exactly mathematically rigorous (the formulation of string theory requires picking a background, causing problems with the idea that all backgrounds come from the "same" theory, and let's not even get into the problems at more than two loops).

The article is listed as a contribution to "An assessment of current paradigms in theoretical physics", and I'm curious what that is. Does it contain an equally tough-minded evaluation of the problems of string theory?

It should be emphasized again that I'm no expert on this. I'm curious to hear from experts what they think of this article. Well-informed comments about this are welcome, anti-string or anti-LQG rants will be deleted.

Update:

There's a new [expository article about spin-foams]() by Perez out this evening.

Comments

1. **Robert**
   January 20, 2006

   Just to repeat it in plain English: When Peter says that string theory is not predictive he refers to the fact that there is likely a very large number of possible low energy effective theories (say in terms of particles and couplings). This is in contrast to LQG which according to the experts can be coupled to _any_ particle content (and possibly even anomalous) with any set of couplings. But this is not the problem that Nicolai and Peters address.

2. **Urs**
   January 20, 2006

   Robert,

   indeed. The remarkable thing is that the landscape problem is a problem other theories currently don't see, not because they somehow solve it, but because they are not understood well enough to even formulate this problem.

   For some reason the existence of many solutions to string theory is sometimes referred to a disadvantage over nonperturbative approaches. But, as I tried to say elsewhere on this blog, there is no indication that these non-perturbative approaches, once they work and are understood, will turn out to have a smaller number of configurations. Why should they? We don't expect that of any theory.

3. **Arun**
   January 20, 2006

   In LQG, presumably many different Hamiltonians/spin foam models supposedly lead to the same low energy gravity – isn't this something we know about from the Wilsonian renormalization ideas? – while in string theory the same high energy theory has 10^500 low energy worlds.

4. **woit**
   January 20, 2006

   Robert,

   I wasn't the one writing here about the lack of predictivity of string theory, that was a quote from Nicolai and Peeters. But while LQG lacks predictivity about particle physics (which is why I've never tried to become expert in it), the claim has always been that it can be consistently coupled to the standard model, producing a unified theory of gravity and particle physics. Such a theory would be predictive about quantum gravitational effects, although it is unclear that one has any hope of ever measuring these. Nicolai and Peeters are suggesting that this claim is not true, that LQG inherently contains ambiguities even in the purely gravitational sector. About that it would be interesting to hear from experts who don't have a pro-string axe to grind.

   Urs,

   For the 10^500'th time, the problem with the landscape is not the number of configurations, it's that in such a scenario you can't predict anything at all. If that's really the way the world is, and any unified theory will ultimately run into this problem, thinking about unified theories is completely pointless and the part of theoretical physics that studies these should just be shut down. Some of us would like to see some actual evidence for this before agreeing to it.

5. **Urs**
   January 20, 2006

   Arun,

   you are confusing theories and their solutions.

   > the same low energy gravity

   That is: the same low energy gravity theory.

   > 10^500 low energy worlds.

   These are solutions to such a theory.

6. **Tony Smith**
   January 20, 2006

   Peter said "… Nicolai and Peters [the authors of hep-th/0601129] … are string theorists … it would be interesting to hear from experts who don't have a pro-string axe to grind. …".

   hep-th/0601129 states that it is a "Contributed article to "An assessment of current paradigms in theoretical physics".".

   What is "An assessment of current paradigms in theoretical physics" ?

   Is it a conference or some sort of program that might eventually lead to redistribution of funding and jobs for theoretical physics ?

   Are there other "Contributed article"s that might present other points of view, including those of "experts who don't have a pro-string axe to grind" ?

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

7. **JKowalskiGlikman**
   January 20, 2006

   Hi,

   I am not a LQG theorist either, but I am quite close to these guys, attending their meetings etc. So while I could not really comment on detailed technical questions, I am rather well aware of the general situation.

   The first paper of Nicolai, Peeters, and Zamaklar was widely considered by experts to be rather unfair; this one I think is much better in this respect. What they present is an outside view, with obvious problems spelled out. As such it is much more important for LQG community than to others: it shows what should be urgently addressed/explained.

   The most important one is of course that in both LQG and spin foam the "right" model is not known. I do not think this is a matter of ambiguities. It is not that there are many consistent models to choose between, we are just not able to perform the most basic consistency checks (like closure of the constraints algebra in LQG) on any of them (if I understand correctly.)

   But I do not see what this may possibly have to do with non-renormalizibility of perturbative QG, which is what N&P claim.

   For me the major problem with LQG is if the quantization of area and volume is the real thing, or just an artifact of the formalism. This is the main real result of LQG, and it is used explicitly, for example, in cosmological and black hole applications.

   I would generally agree with N&P that LQG, (by which I mean the loop canonical quantization of gravity, with its particular Hilbert space, etc) has much more open problems than the solved ones and real solid predictions.

   I think however that something extremely interesting happened last year with Freidel and Starodubtsev paper "Quantum gravity in terms of topological observables," arXiv:hep-th/0501191 (built on some earlier ideas of Lee Smolin.) What they managed to show was that in 4d gravity has the structure of TFT + perturbations, with the TFT part very similar to 3d gravity and the coupling constant dimensionless and very small. Since gravity in 3d is reasonably well understood their formalism raise the hope that gravity can be perturbatively quantized, with manifestly diff invariant perturbative expansion. (of course gravitons would be extremely hard to get in this formalism, but who – apart from string theorists – cares about gravitons!)

8. **Arun**
   January 20, 2006

   Urs,

   Yes, I was confusing two different things. Nevertheless, the nonuniqueness of the LQG Hamiltonians that are consistent with the low energy theory is to be expected on general grounds, and should not impact the ability to predict (if any!!!) in the experimentally accessible regime – it is just a lot of irrelevant operators in the effective low energy theory.

   -Arun

9. **fh**
   January 20, 2006

   JKG, I'm similarily studying in the (very) close proximity of LQG, and I concurr with most of what you say. However at Loops05 Perez presented first work towards a deeper study of the ambiguities, I think he said there was a relation to non-renormalizability.

   Furthermore and even more excitingly Freidel has just gotten a further result, the non gravity limit of 2+1 gravity (not with G -> 0 but with gravitational degrees of freedom integrated out) is a non commutative field theory.

Also closure of the constraint algebra has been achieved (as a mathematically rigorous result) in a nonstandard way through Thiemann's Master constraint program.

Giesel one of Thiemann's students reported on a consistency check on the Area operator regularization, also.

So the list of problems there reads to me rather like "what people are currently working on and have been worried about for a while". THese problems are all being systematically attacked.

10. **Who**
January 20, 2006

this will sound like nit-picking
overall it's a GREAT paper, very glad to see a broad spectrum of active non-string lines of QG research being discussed including CDT and Reuter's QEG, and of course spinfoams (omitted from Nicolai's paper of a year ago) BUT

look at this omission: Nicolai Peeters give three citations to work of Laurent Freidel—-their references [47], [71] and [73]—so they came within an inch of mentioning this other one by Freidel:

http://arxiv.org/abs/hep-th/0502106
Ponzano-Regge model revisited III: Feynman diagrams and Effective field theory
Laurent Freidel, Etera R. Livine

"We study the **no gravity limit** G_{N}-> 0 of the Ponzano-Regge amplitudes with massive particles and show that we **recover in this limit Feynman graph amplitudes** (with Hadamard propagator) expressed as an abelian spin foam model. We show how the G_{N} expansion of the Ponzano-Regge amplitudes can be resummed. This leads to the conclusion that the dynamics of quantum particles coupled to quantum 3d gravity can be expressed in terms of an effective new non commutative field theory which respects the principles of doubly special relativity. We discuss the construction of Lorentzian spin foam models including Feynman propagators"

BTW the fact that they found the model must use DSR connects with the work of Jerzy Kowalski-Glikman, who just posted here.

11. **Urs**
January 20, 2006

Arun,

see http://golem.ph.utexas.edu/~distler/blog/archives/000639.html for a reply to your argument.

12. **JKowalskiGlikman**
January 20, 2006

Who says:

"so they came within an inch of mentioning this other one by Freidel" [and Livine].

This is an exciting paper, since it shows that the semiclassical limit of 3d quantum gravity is DSR, and not Special Relativity. This makes me even more excited about Freidel and Starodubtsev story, because it seems that the proof that DSR is a limit of quantum gravity seems to be within reach.

In my comment above I forgot to say that it seems to me that nobody has even slightest clue as to how to get semiclassical limit from "canonical" LQG (this was long stressed by Lee). So another consistency check of possible hamiltonians, provided by that they have to lead to consistent classical limit seems to be not available either.

13. **Who**
January 20, 2006

Hi Jerzy,
like your work on DSR.
Maybe someone should email Nicolai and suggest he add a reference to
http://arxiv.org/abs/hep-th/0502106
before his article goes to publication.

the trouble is when you do a review paper there will ALWAYS be someone who claims something important has been omitted, but
perhaps in this case the work is important enough to warrant
a message.

Freidel just co-authored a paper with Shahn Majid. I don't understand the significance of that paper—-way too technical for me. Can you please give a clue as to its bearing on QG issues?

14. **fh**
January 20, 2006

Urs, the existence of a non Gaussian UV fixed point might or might not hold, but what is not clear to me is why the infinite constants of the perturbative expansion should neccesarily reappear in the nonperturbative framework.
We don't know if the perturbatively treated effective field theory of the Einstein Hilbert Lagrangian plus higher order terms is a valid perturbative expansion of the nonperturbative theory, this is not certain by far. Particularly no field theory in the LQG Kinematical Hilbert space has infinities, Wilson renormalisation logic suggests that it will look like a renormalizable theory at low energies, but we don't have a good implementation of Wilson renormalization in the context of LQG yet. The old tools do NOT carry over flawlessly.

Particularly if you consider the strong gravity regime the causal structure of the theory changes this is not captured in the graviton expansion around a fixed background AFAIK.
In a similar vein we also don't know if for nonperturbative reasons (e.g. the statespace of the full theory, see the recent work by Perez) the couplings/ambiguities are fixed. The ambiguities appear in a rather nicely ordered way in the regularisation of the Hamiltonian constraint, it's not unreasonable to assume that for many choices you get a trivial phasespace.

15. **Wolfgang**
January 20, 2006

> Particularly if you consider the strong gravity regime the causal structure of the theory changes this is not captured in the graviton expansion around a fixed background

AFAIK.

Many are not aware that there is an exact solution of quantum gravity
in the strong-coupling limit. Maybe this ref. helps to clear up some of the issues discussed here:
http://prola.aps.org/abstract/PRD/v26/i10/p2645_1

16. **JKowalskiGlikman**
January 20, 2006

Who said

"Freidel just co-authored a paper with Shahn Majid. I don't understand the significance of that paper—-way too technical for me. Can you please give a clue as to its bearing on QG issues? "

I do not know if Peter Woit would not mind us going slightly away from the main theme, so just couple of sentences.

I did not have time to study this paper yet. As far as I understand it just refines the rather vague mathematics of the Freidel&Livine. On the other hand I suspect that given knowledge and insight of both Laurent and Shahn there might be something really exciting there.

17. **Tony Smith**
January 20, 2006

Nicolai and Peeters, in hep-th/0601129, seem to me to be doing a hatchet job on alternatives to conventional superstring theory.

After dismissing conventional LQG and Regge and dynamical triangulation approaches on the grounds of Hamiltonian difficulties, they proceed to discuss "a fixed spin foam".
Since (as Nicolai and Peeters admit) "a fixed spin foam … differs considerably from both the Regge and dynamical triangulation approaches",
they do not attack it on the basis of Hamiltonian difficulties.
However,
they do attack "a fixed spin foam" by saying that it is afflicted by
"… the true problem of quantum gravity, which lies in the ambiguities associated with an infinite number of non-renormalizable UV divergences …"
and
that "a fixed spin foam … cannot be relieved of the duty to explain IN DETAIL how the above divergences 'disappear', be it through cancellations or some other mechanism …".

However, as Peter discussed in his blog entry "Is N=8 Supergravity Finite?" at
http://www.math.columbia.edu/~woit/wordpress/?p=268 ,
"… If Bern is right, N=8 supergravity may be renormalizable because of a combination of supersymmetry and twistor geometry …
Yes, it's hard to get the standard model out of N=8 supergravity, which is one reason people gave up on it. But this may be a much more fruitful starting point than string theory … working with a well-defined theory instead of a vague hope that a theory exists might also be a good idea. …".

Even if N=8 Supergravity itself might be flawed with respect to getting the Standard Model,
it might be that some other structure with similar symmetries related to UV-finiteness could be used for nodes of a spin foam that could give the Standard Model.
It might even be that such a spin foam should be constructed in a dimension higher than 4, producing the Standard Model through a Kaluza-Klein process.

I wonder whether Nicolai and Peeters would admit that such a possibility is a reasonable alternative to both conventional superstring theory and the approaches that they dissed in hep-th/0601129, and therefore should be given funding and jobs,
or
would they point to their emphatic "duty to explain IN DETAIL how the above divergences 'disappear', be it through cancellations or some other mechanism"
and
thus put anyone proposing to work on such a possibility in the Catch-22 position of having to do the cancellation calculations in order to get funds and jobs to work on the cancellation calculations.

As to the severity of their "IN DETAIL" requirement, consider Peter Woit's remark about N=8 Supergravity:
"… divergences in N=8 supergravity don't occur until at least 5 loops. Ever tried to do a 5-loop calculation in N=8 supergravity? …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

18. **Lee Smolin**
January 20, 2006

Hi Peter, Thanks for mentioning this. After a quick look I can say that there are some statements they make that I agree with (such as about the difficiulties of relating the spin foam to the Hamiltonian constraint theory) and others with which I disagree, such as their statements about uv finiteness.

In particular, the point you quote, "the need to fix infinitely many couplings in the perturbative approach…." seems to disagree with old, well understood results. (Not to mention they don't seem to make a detailed argument for their claim.) In fact, there is a well understood and detailed explanation for how the theory is cutoff. I won't repeat the argument here (see hep-th/0408048) but the key points are that, as a result of the finitess of area and volume, one can show that the Planck length cannot suffer an infinite normalization if the theory is to reproduce the finiteness of black hole entropy,and the appearence of gravitons in perturbations around weave states. Thus, the theory is uv cutoff and the divergences do not have to cancel as they were never there in the first place. The key issue is then how is this finite cutoff length compatible with the symmetry of the ground state, which leads to the expectation that the symmetry is DSR.

But honest criticism based on detailed study is always welcome and I will read the paper carefully before seeing if I have any more substantial reponse to make.

In case anyone is interested, I am teaching a course on LQG, and video's of lectures will be available as they are given at
http://streamer.perimeterinstitute.ca:81/mediasite/viewer/FrontEnd/Front.aspx?&shouldResize=False

Thanks,

Lee

19. **Lubos Motl**
January 20, 2006

This is such an absurd comment, Peter. For example, your ideas about “experts”. Nicolai himself is a much bigger expert in loop quantum gravity than anyone whom you call an “expert”. Their previous paper is, for example, by far the most cited loop quantum gravity paper of 2005.

20. **D R Lunsford**
January 21, 2006

Lubos I see is still arguing by counting footnotes.

“find author einstein”

“read author einstein”

-drl

21. **Aaron Bergman**
January 21, 2006

The fact that you’ve cutoff the theory doesn’t obviate the need to understand the infinite series of couplings compatible with diffeomorphism invariance (and background independence for that matter). Even if you tune them all to zero at some scale, they’ll show up when you flow. Nicolai and Peeters’s claim, it seems to me, is that it is exactly this amibguity that is at least a part of the infinite ambiguity in the choice of a LQG Hamiltonian.

Myself, I still am rather skeptical about the LQG-like quantization procedure given how far it is from how we quantize, well, everything else, but that’s a different story.

22. **Haelfix**
January 21, 2006

Its also troubling, b/c the second you play with your matter content all those infinite couplings that you’ve cut off, would affect whether or not you can even *find* the correct and fundamental master interacting field theory.

Its just another statement about the RG flow, I just dont see how you can escape from the statement that LQG needs to find a nontrivial fixed point at some stage to make sense.

23. **Lee Smolin**
January 21, 2006

On reading NP I am grateful for the hard work that they put in, but I end up feeling that they still miss the point, because they have prejudices about what a quantum theory of gravity should do coming from old expectations. They appear to evaluate LQG and spin foam models as if they were proposed as a unique theory which was a proposals for a final theory of everything. This is in my view a misunderstanding. One should understand these as a large set of models for studying background and diffeo invariant QFT’s. These are based on quantization of a set of classical field theories which are constrained topological field theories. There are three key claims: 1) these theories exist, rigorously. i.e. there are uv finite diffeo invariant QFT’s based on quantization of constrained TQFT’s. 2) there is a common mathematical and conceptual language and some calculational tools which are useful to study such models and 3) there are some common generic consequences of these models, which are relevant for physics.

Nothing NP say questions these key claims. Unfortunately, they do not mention key papers which support these key claims, such as the uniqueness theorems (gr-qc/0504147, math-ph/0407006) which show the necessity of the quantization LQG uses. And while they mention the non-seperability of the kinematical Hilbert space they fail to mention the seperability of the diffeomorphism invariant Hilbert space, (grqc/ 0403047). It is unfortunate that they omit reference to such key results which resolve issues they mention.

A second misunderstanding concerns uv divergences. NP do not discuss the results on black hole entropy, so they miss the point that the finiteness of the black hole entropy fixes the ratio of the bare and low energy planck length to be a finite number of order one. Calculations on a class of semiclassical states they do not discuss-the weave states-lead to the same conclusion (A. Ashtekar, C. Rovelli, L. Smolin, Weaving a classical metric with quantum threads,” Phys. Rev. Lett. 69 (1992) 237.). So there can be no infinite refinement of spin foams and no infinite renormalization. These theories are uv finite, period. This is one of the generic features I mentioned.

Thus, their main claim, that the fact that there are many LQG or spin foam models is the same as the problem of uv divergent is just manifestly untrue. The freedom to specify spin foam amplitudes does not map onto the freedom to specify parameters of a perturbatively non-renormalizable theory. For one thing, few if any spin foam models are likely to have a low energy limit which is Poincare invariant, a property shared by all perturbative QFT’s, renormalizable or not, defined in Minkowski spacetime. In fact, we know from recent results that in 2+1 none do-the low energy limit of 2+1 gravity coupled to arbitrary matter is DSR. So their argument is false.

They do get a number of things right. The following are open issues, much discussed in the literature: 1) whether there is any regularization of the Hamiltonian constraint that leads to exchange moves, 2) whether thus there are any links between the spin foam amplitudes and Hamiltonian evolution, 3) whether the sum over spin foam diagrams is convergent or, more likely, Borel resummmable (although they miss that this has been proven for 2+1 models, hep-th/0211026). I don’t agree with all the details of their discussion of these issues, but these certainly are open issues.

NP seem to argue as if one has to prove a QFT rigorously exists in order to do physics with it, by which standard we would believe no prediction from the standard model. They mention that there are no rigorous constructed, semiclassical states, which are exact solutions to the dynamics, but this is the case in most QFT’s. This does not prevent us from writing down and deriving predictions from heuristic semiclassical states (hep-th/ 0501091), or from constructing reduced models to describe black holes or cosmologies and likewise deriving predictions (astro-ph/0411124), Nor does it prevent Rovelli et al from computing the graviton propagator and getting the right answer, showing there are gravitons and Newtonian gravity in the theory (gr-qc/0502036).

But, someone may ask, if LQG is the right general direction, shouldn’t there be a unique theory that is claimed to be the theory of nature? Certainly, but should the program be dismissed because no claim has yet been made that this theory has been found? To narrow in on the right theory there are further considerations, all under study:

-Not every spin foam model is ir finite.
-Not every spin foam model is likely to have a good low energy limit.
-The right theory should have the standard model of particle physics in it.

In addition it must be stressed that there can in physics be generic consequences of classes of theories, leading to experimental predictions. Here are some historical examples: light bending, weak vector bosons, confinement, principle of inertia, existence of black holes. All of these observable features of nature are predicted by large classes of theories, which can be as a whole confirmed or falsified, even in the absence of knowing which precise theory describes nature, and prior to proving the mathematical consistency of the theory. LQG predicts a number of such generic features: discreteness of quantum geometry, horizon entropy, removal of all spacelike singularities, and I believe will soon predict more including DSR, emergence of matter degrees of freedom.

One reason for this is of course that most of the parameters in such classes of such theories are irrelevant in the RG sense, and do not influence large scale predictions.

Since we know the theory is uv finite this does not affect existence. The lack of a uv unique theory does not prevent us from testing predictions of QFT in detail, and it is likely to be the same for quantum gravity. The old idea that consistency would lead to a unique uv theory that would give unique low energy predictions was seductive, but given the landscape, it is an idea that is unsupported by the actual results.

Having said all this, I hope that NP will put their hard won expertise to work, and perhaps get their hands dirty and do some research in the area.

Sorry to go on so long,

Thanks, Lee

ps to Haelfix, sure, why not work on RG flow in LQG?

24. **Lubos Motl**
January 21, 2006

Dear Lunsford,

the database does not claim to cover the early 20th century physics. Nevertheless, you will still see that Einstein has 33 papers in it – which may in fact be close to the total – and they have over 1150 citations which is more than some of us.

I am certainly using different criteria but sorry to say, having at least some well-known papers is a necessary condition for someone to be expected to have something relevant to say about science.

All criteria I can imagine imply that Nicolai and Peeters are LQG experts.

Best wishes
Lubos

25. **Lubos Motl**
January 21, 2006

Dear Lee,

how are you? Sorry to say but I don't quite understand how Haelfix can work on something that violates the laws of physics. If you would have said "write an upbeat paper that combines the buzzwords from LQG and RG", then it would be a realistic task. And the paper would make no sense much like when one combines LQG and quantum computing or noiseless information theory or any two decoupled pieces of jargon. There are already many papers of this type around.

In physics, it is impossible to make progress just by combining two random buzzwords.

Haelfix has, on the contrary, explained why he already knows that certain things cannot work and why. LQG is not a local field theory and by its very construction and the discrete philosophy, it does not have a UV fixed point (because distances below 0.1 L Planck do not exist). Consequently, the extreme UV physics is not determined (in a theory with a UV fixed point, it could be determined by conformal symmetry). The physics starts at the Planck scale, if ever, and because there is no other organizing principle than the existence of fundamental metric tensor (a wrong assumption for quantum gravity, by the way), it is clear that all higher-derivative and other terms must be considered. That's not surprising because they are of the same order at the Planck scale.

Because there are no "more fundamental" degrees of freedom and no other organizing principle, the continuous coefficients of all these couplings can be anything, rendering LQG infinitely unpredictive – exactly as unpredictive as the perturbative nonrenormalizable GR written as effective field theory.

Maybe you meant that LQG should try to obtain a long-distance limit. Hundreds of people have tried, have not they? I find it manifest today that no one will every find one because it does not exist. Gravity is not lattice QCD, and the failure to choose the correct UV starting point not only destroys the "correctness" of physics. It destroys the very existence of low-energy physics. In string theory, we have a toy model of this possible problem because in the UV, one must choose the right (or even fine-tune by discrete choices) the vacuum energy for large space to exist in the first place.

In LQG, you also have the cosmological constant problem but you also have infinitely many similar problems associated with ever higher-derivative tems that are expected to crumple the space altogether, unless you fine-tune infinitely many terms in the Hamiltonian constraint. The task you are trying to give to Haelfix is not well-defined and even if the definition were completed, it would be guaranteed to fail.

"One reason for this is of course that most of the parameters in such classes of such theories are irrelevant in the RG sense, and do not influence large scale predictions. Since we know the theory is uv finite this does not affect existence. The lack of a uv unique theory does not…"

If you read these lines of yours rationally, they're equivalent to saying that we know the classical limit of general relativity (given by the Einstein-Hilbert term). But the whole point of *quantum* gravity is that we can also say something about higher energies and/or precision experiments at low energies that get loop corrections from quantum phenomena. You can't do it. Moreover, it is not true that you can even derive the leading term of classical general relativity.

What you're saying, Lee, is completely equivalent to saying that nonrenormalizable field theories exist as quantum theories, and it's just manifestly wrong. Whether or not the numbers "look" finite is completely irrelevant. If we choose a cutoff of any sort we like in quantum field theory, we will also get finite numbers. Finiteness is not the problem. The problem is the presence of infinitely many undetermined parameters.

Another approach that can never lead to realistic science is the permanent promotion of complete rubbish papers, being satisfied by the fact that they were written. Rovelli's "graviton propagator" is an example. He incorrectly assumes that physics is dominated by nearly flat space, and then he "derives" that physics is dominated by nearly flat space. The reasoning is completely circular and has nothing to do with LQG whatsoever. Even if there were some truth in LQG, one could never start to make progress in revealing it before the papers are read rationally and critically and before patently false papers start to be neglected.

All the best
Lubos

26. **Zorq**
January 21, 2006

I love the circularity of Lee's argument that LQG must be a finite theory because, otherwise it gets the BH entropy wrong.

But, even accepting the statement that the low-energy G_N and the lattice G_N are related by a finite multiplicative factor does not demonstrate what he thinks it demonstrates. He has imposed a cutoff at the Planck length. G_N is itself of order l_P^2. So an ordinary quadratically-divergent correction to G_N just looks like a finite multiplicative renormalization. Only if he attempted to take the cutoff to zero (which he doesn't) would he be able to distinguish the two.

Besides, there's also the infinite number of other diffeomorphism-invariant, background-independent couplings to deal with. Saying that almost all values of those couplings will not lead to sensible low-energy physics (one of his arguments) is not the same as saying they are IR-irrelevant couplings, so that low-energy physics is independent of what values you choose (another one of his arguments).

27. **Quantoken**
January 21, 2006

Lee and Lubos:

I clearly see your discussions forming a Loop of Quantum Loops since they are not going any where. Each of you accuse the other camp for failure of making predictions. Let's set the score fair and sqaure: Both have failed to make any meaningful prediction so far. So let's start a fair and equal debate that neither one is better than the other so far in predictability.

Lee claims that LQG has made an experimentally verifiable prediction by predicting dispersion of light speed (light speed is not exactly constant, but has a small variation at high energies, depending on the energy level). I disagree. Predictions need to be definite, quantitative and precise. Tiny dispersions may be observed and maybe not, one of the other. it's a 50/50 chance if you happen to pick the right choices out of the two. Unless you have predicted the exactly amount of dispersion, quantitatively, and show that experimental result matches your numerical calculation precisely, it really can't be counted as a credible verifiable prediction just because there happen to be a small dispersion. Especially consider that the space of the universe is not exactly vacuum, but contains some imtermediate material at extreme low concentration, so dispersion of some sort due to condensed matter physics MAY some how occur.

The super string theory camp is equally guity of making such ambiguious "predictions" that amounts to nothing more than chasing a shadow. One case being the CSL-1 business. Lubos made more than a few enthusiastic hypes cosmic string about that two fuzzy little dots in a telescope image, until it's shown by Hubble they are nothing more than two little tiny galaxies far away. Had hubble be slightly less in instrumental precision, they would now all be celebrating with champane that super string theory has made a prediction that is verified by observation. Be a little bit more specific, precise and exact when you make predictions, OK?

There is a fundamental philosophy difference between Lee and Lubos that Lee believes the universe is inheritantly discrete, but Lubos believes it is inheritantly continuous, beyond the discrete phenomenas that we observe in QM. Maybe we can have more discussions between the discrete picture and the continuous one. That would be a more interesting discussion.

Quantoken

28. **Lubos Motl**
January 21, 2006

I liked Zorq's points.

The Immirzi parameter can indeed be considered as a multiplicative renormalization of Newton's constant between the Planck scale and low energies. By construction, it is finite as long as G_{Newton} stops running at very long distances – which is so if classical GR is reproduced. The finiteness is no consistency check. The finiteness was put in by construction. The precise value is unknown and there are contradictory results for the values which is why Lee will never tell us a trustworthy value of the Immirzi parameter.

However, there are still constants at low energies – starting from Newton's constant – that do depend on the exact higher-derivative terms (or their equivalents, in whatever language we use). If something is IR-irrelevant, it does not mean that the numerical values in the IR won't be affected by these irrelevant terms. They will be affected because the IR-relevant and marginal parameters will run as a function of the irrelevant couplings.

More generally, the obsession with finiteness obscures the fact that the finiteness of some intermediate results is not really difficult to achieve. Any regularization or any scheme of cutoffs will do. The problem is to make a theory whose predictions are independent of the cutoff – which is a part of renormalization governed by the rules of renormalization group (which is NOT the same thing as regularization).

If the predictions depend on the cutoff, then we have just parameterized our ignorance about the singular physics in terms of the details of the cutoff.

In renormalizable QFTs, we obtain physics independent of the cutoff because we can organize the terms into a small number of IR-relevant and IR-marginal operators and argue that the irrelevant ones are suppressed by a higher energy scale. In perturbative string theory, we may obtain the same or better control because the fundamental theory is really living on the worldsheet and it must be conformal which is a strong constraint (and somewhat analogous to the constraint of renormalizability of QFTs mentioned previously).

In AdS/CFT, there are other constraints of the possible form of the physical laws, and all of us would like to know the most general type of constraint that can give us any consistent background of string theory – something that generalizes the conformal symmetry above (and that probably does not allow us to write the full theory in terms of any local quantum field theory or any worldsheet or boundary). But in LQG, we know for sure that these organizing principles are missing.

29. **Lee Smolin**
January 21, 2006

To Zorq, I apologise that the one argument I gave is far from the whole story. I agree that to define a theory you have first to define a regulated theory and then study the limit as the regulator is removed and show that the expressions for observables that result have the symmetries and gauge invariances of the classical theory. The physical parameters are parameters of the resulting theory. But this was exactly what was done in the hamiltonian construction of LQG, the observables such as area and volume are constructed through a limit of regulated operators and then the limit is taken. The result is finite, diffeomorphism invariant observables. The parameters of the finite, diffeo inv. theory include the bare Newton's and cosmological constant-not as coefficients in the dynamics but as coefficients in the algebra of observables. The diffeo invariance is proved restored in the limit. And don't be confused, the bare planck scale was not the regulator, there was another regulator, that has already been taken to zero. Please study the details of the construction.

So what you ask for has been done-the logic is not circular and the theory is finite. I was addressing the proposal to still take a limit of the parameters vanishing, even after the limit of the regulator to zero is taken. This would be non-standard, and it leads to results in disagreement with the semiclassical theory.

To quantoken: obviously it would be better to have precise predictions than generic ones. But generic ones can still distinguish between classes of theories. If GLAST sees an energy dependent, polarization independent speed of light, thus confirming that the symmetry of the vacuum is DSR, this obviouosly kills theories that predicted either broken or naive Poincare invariance and supports the plausibility of theories that predict-even generically DSR.

I can't agree with your equivalanece of LQG and string theory just because of the vast imbalence in how much work has been put into each. For each point about string theory there are dozens of papers, so the ins and outs of each issue have been often thoroughly explored. Key facts about LQG still rest, in many cases on one or a few papers. There are many obvious things to do that have not been tried for lack of people. So there is a lot still do to, for example to turn generic predictions into precise predictions.

And I don't "believe" nature is discrete, we showed this is a generic property of a large class of diffeo invariant QFT's.

I'll reply to the rest later,

Lee

30. **anonymous**
January 21, 2006

Lumos has a long list of publications about speculation on unobservables. So I guess he's well qualified to make vacuous assertions. What I'd like to see debated is the fact that the spin foam vacuum is modelling physical processes KNOWN to exist, as even the string theorists authors of http://arxiv.org/abs/hep-th/0601129 admit, p14:

'… it is thus perhaps best to view spin foam models … as a novel way of defining a (regularised) path integral in quantum gravity. Even without a clear-cut link to the canonical spin network quantisation programme, it is conceivable that spin foam models can be constructed which possess a proper semi-classical limit in which the relation to classical gravitational physics becomes clear. For this reason, it has even been suggested that spin foam models may provide a possible 'way out' if the difficulties with the conventional Hamiltonian approach should really prove insurmountable.'

Strangely, the 'critics' are ignoring the consensus on where LQG is a useful approach, and just trying to ridicule it. In a recent post on his blog, for example, Motl states that special relativity should come from LQG. Surely Motl knows that GR deals better with the situation than SR, which is a restricted theory that is not even able to deal with the spacetime fabric (SR implicitly assumes NO spacetime fabric curvature, to avoid acceleration!).

When asked, Motl responds by saying Dirac's equation in QFT is a unification of SR and QM. What Motl doesn't grasp is that the 'SR' EQUATIONS are the same in GR as in SR, but the background is totally different:

'The special theory of relativity … does not extend to non-uniform motion … The laws of physics must be of such a nature that they apply to systems of reference in any kind of motion. Along this road we arrive at an extension of the postulate of relativity… The general laws of nature are to be expressed by equations which hold good for all systems of co-ordinates, that is, are co-variant with respect to any substitutions whatever (generally co-variant). …' – Albert Einstein, 'The Foundation of the General Theory of Relativity', Annalen der Physik, v49, 1916.

What a pity Motl can't understand the distinction and its implications.

31. **Zorq**
January 21, 2006

Lee,

You seem to make a strange (and nonstandard) distinction between a "finite theory" and an "RG fixed-point" theory. What is the distinction?

N=4 SYM is a finite QFT. It is also a fixed-point of the Renormalization Group. Moreover, it is a fixed point for any value of the gauge coupling.

You claim that LQG, with all (the infinite number of) couplings except for the cosmological constant and Newton's constant set equal to zero is a finite theory (an RG fixed point). Do you want to claim that this is true for ANY values of Newton's constant and the cosmological constant, or just for some special values (as claimed by Reuter et al)?

32. **Lubos Motl**
January 21, 2006

Dear Lee,

what you write about the regulators is internally inconsistent. If the limit depends on the regulator in a diff invariant theory and when the regulator contains as many parameters as a generic effective field theory or more, which is easy to see in LQG, then you cannot guarantee that the 4D results will be diffeomorphism invariant simply because you can see that the 4D nondiff invariant results will be generated.

A subtlety may be that you don't quite distinguish the 3D diffeomophisms from the 4D diffeomorphisms. The latter include the Hamiltonian constraint. In the Hamiltonian treatment, the failure to obtain diffeomorphism-invariant physics will be manifested as the failure of the constraint algebra to close on the kinematical Hilbert space. This failure of the LQG algebra, including the Hamiltonian constraint, to close in LQG was discussed in the previous paper by Nicolai et al.

(This statement has its known counterpart in string theory: in the light-cone gauge, the critical dimension arises from the correct commutators of the Lorentz group.)

When you say that you obtain diff invariant results, you surely mean the 3D diffeomorphisms only, but that's just not enough for gravity. The difficulties to extend some 3D results to the full 4D spacetime is of course related to the general problems with dynamics in LQG and its failure to reproduce any symmetry between space and time, especially not the Lorentz symmetry.

Finally. this statement is really cute:

"And I don't "believe" nature is discrete, we showed this is a generic property of a large class of diffeo invariant QFT's."

What you have shown is that a class of discrete theories (that have no continuous limit or any other relation with physics) has the property that its elements are discrete theories. It is a completely vacuous and circular statement, and it in no way suggest that these theories are interesting as theories that physicists should study.

Best
Lubos

33. **Lubos Motl**
January 21, 2006

Dear Zorq,

surely no one wants to claim that gravity with a nonzero Newton's constant is a conformal field theory. It has a scale because Newton's constant is dimensionful. The same thing applies to the cosmological constant.

Your equivalence between finiteness and UV conformal symmetry is only valid for QFTs in continuous spaces. LQG is not a QFT in this sense. It can be in some sense finite as any other discrete, regulated, or latticized theory. But of course, its continuum limit must behave as an effective field theory for which having a UV fixed point is equivalent to finiteness.

Strictly speaking, we know that LQG can't have a UV fixed point exactly because the spectrum of its areas and eigenvalues of other dimensionful observables is discrete, and therefore it is not scale-invariant. This pretty much kills the hopes of a UV fixed point that are plausible in other, more general candidate theories of quantum gravity.

Because of these reasons, the discrete character of spacetime in LQG is on the same level as with any other generic regulator that someone may invent. The Hamiltonian formalism can guarantee that the regulator is invariant under 3D diffeomorphisms, but it has been shown that it is not invariant under 4D diffeomorphisms. In the Hamiltonian treatment, this fact is manifested as a non-closure of the constraint algebra if it acts on the original full kinematical Hilbert space (see the Nicolai et al. paper 1 year ago).

All the best
Lubos

34. **Zorq**
January 21, 2006

"Your equivalence between finiteness and UV conformal symmetry is only valid for QFTs in continuous spaces."

I didn't say anything about (UV) conformal invariance.

A finite theory gives cutoff-independent answers. An RG fixed-point is one where the coupling constants don't vary as you vary the cutoff. With the normal meaning of these terms, these are the same concept.

As you say, LQG is abnormal in many ways. But if these terms mean the same thing to Lee that they usually mean, then they are still synonymous in LQG. If they mean something different to him, I'd like to know what that is.

35. **Lubos Motl**
January 21, 2006

Dear Zorq, while I agree with nearly everything you write, your usage of the word "cutoff" confuses me. Cutoff independence is something different from conformality. The Standard Model – or a GUT theory to be better – is cutoff-independent but it is not a conformal field theory. It has a conformal UV fixed point (at least in the case of asymptotically free Yang-Mills) but still, one should recognize the dependence on the cutoff from the dependence on the chosen RG scale.

36. **arnold**
January 21, 2006

As a non-expert could anybody clarify to me this doubts?

1.If I understand well, LQG is the quantization program applied to GR (so I don't even know why it has a different name). Right?

(this seems to me the most natural thing to do in order to quantize gravity)

2. why a theory with infinite number of constant cannot be the correct theory?

thanks

37. **Who**
January 21, 2006

In reference to
http://arxiv.org/abs/hep-th/0502106
Ponzano-Regge model revisited III: Feynman diagrams and Effective field theory
Jerzy K-G says (20 jan 1:57P)
http://www.math.columbia.edu/~woit/wordpress/?p=330#comment-7742

——-quote from Jerzy in this thread——

"so they came within an inch of mentioning this other one by Freidel" [and Livine].

This is an exciting paper, since it shows that the semiclassical limit of 3d quantum gravity is DSR, and not Special Relativity. This makes me even more excited about Freidel and Starodubtsev story, because it seems that the proof that DSR is a limit of quantum gravity seems to be within reach.
————end quote——————–
This illustrates Smolin's point about classes of theories making TESTABLE generic PREDICTIONS

It's an important point and to recognize and emphasize it would help clarify this discussion

As Smolin uses the term, LQG is a CLASS of theories including spin foam models. There is current uncertainty about whether this class generically requires DSR. If it does, then it would tend to refute LQG if the GLAST satellite does not see DSR, when put in orbit next year or so.

It seems to me that to some extent LQG critics waste their time unless they not address LQG as a class of theories (including spin foam models) because when blanket statements are made one often has the impression that the critics lack a clear idea of what they are talking about. They should also be talking about upcoming possibilities for tests—that is how to dispose of a scientific theory, not by quibbling.

Anyway, LQG is a class of theories where in recent years a major focus of research has been spin foam models, I would guess that has been the main focus. One goal the researchers have is to NARROW DOWN the class and extract further generic predictions so that one can TEST. Here is what Smolin says along these lines:
http://www.math.columbia.edu/~woit/wordpress/?p=330#comment-7757

—-Smolin 21 jan 9:51A—
But, someone may ask, if LQG is the right general direction, shouldn't there be a unique theory that is claimed to be the theory of nature? Certainly, but should the program be dismissed because **no claim has yet been made that this theory has been found**? To narrow in on the right theory there are further considerations, all under study:

-Not every spin foam model is ir finite.
-Not every spin foam model is likely to have a good low energy limit.
-The right theory should have the standard model of particle physics in it.

In addition it must be stressed that **there can in physics be generic consequences of classes of theories, leading to experimental predictions.** Here are some historical examples: light bending, weak vector bosons, confinement, principle of inertia, existence of black holes. **All of these observable features of nature are predicted by large classes of theories, which can be as a whole confirmed or falsified, even in the absence of knowing which precise theory describes nature,** and prior to proving the mathematical consistency of the theory. LQG predicts a number of such generic features: discreteness of quantum geometry, horizon entropy, removal of all spacelike singularities, and I believe **will soon predict more including DSR, emergence of matter degrees of freedom.**
—-end quote—

In sum, I think LQG critics' effort would be better spent asking what progress the LQG community is making in narrowing down and making the class of theories testable. It is a waste of time for a critic to pick one representative theory which he imagines to be representative and to quibble over it, and to try to give the impression that "it" (whatever he thinks it is) will never work.

Incidentally I don't think Nicolai and Peeters make this mistake. They are making a constructive effort to shed light on the LQG field of research.

38. **woit**
January 21, 2006

arnold,

1. In general, one doesn't expect what it means to "quantize" a classical theory to be unambiguous. LQG chooses different variables to work with and tries to find a non-perturbative quantum theory whose classical limit is GR. The whole discussion here comes about because this is not the standard quantization one uses in perturbing around flat space, using the metric components as dynamical variables. Because this is a new set-up, there is still a lot that is not well-understood.

2. Unless the infinite number of constants have special properties, one would expect their existence to ruin the predictivity of the theory, since no matter how many constants you fixed by experimental measurements, you still wouldn't be able to predict the results of new experiments, since they would still depend on unknown constants.

39. **arnold**
January 21, 2006

Thanks Peter,

1. I agree with you that there might be ambiguities. It would be interesting to hear something about that from LQG people.

2. I am not sure I agree on that for several reasons:
2.1
how can we know that the correct theory has to be also predictive? Maybe nature knows the value of the parameters, but maybe we humans will never know them all…
2.2 are we sure that a theory with infinite # of paramters is not
predicting anything?
For example by making assumptions on these parameters one could
be able to predict something (and then check if the assumptions are correct by experiments…).
2.3 In some sense the Standard Model has the same problem…but one assumes that all the higher order operators are negligible(or zero)…and then checks the assumption in the lab (and for now the assumption is confirmed)

40. **Zorq**
January 21, 2006

I never used the word "conformality." You did.

All I did was repeat the following standard argument. Consider some physical observable, f. When you compute it in the cutoff theory, it depends both explicitly on the cutoff and on the bare couplings, g_i, of the cutoff theory.

f= f(a,g_i)

The RG tells you how to change the g_i when you vary the cutoff, in order to assure that f does not change.

0 = a df/da = a \partial f/\partial a + \sum_i b_i \partial f/\partial g_i

where

b_i(g) = a \partial g_i/\partial a

are the beta-functions of the theory.

Finiteness is

\partial f/\partial a = 0

An RG fixed-point is b_i(g_*)=0 for all i. These are the same.

Only in a global (non-gravitational) field theory are they related to dilatation (conformal) invariance.

41. **Lubos Motl**
January 21, 2006

Dear Arnold,
you ask:

"1.If I understand well, LQG is the quantization program applied to GR (so I don't even know why it has a different name). Right?"

It is because pure GR in spacetime with dimensions above 3 cannot be quantized. GR is a non-renormalizable effective field theory. A different word is needed for GR and for this direct attempt to quantize it simply because classical GR is serious physics while the attempts called LQG are not serious physics and all serious theoretical physicists understand why it cannot work.

"(this seems to me the most natural thing to do in order to quantize gravity)"

It may seem natural to the laymen but it is impossible in science once the "details" are actually investigated.

"2. why a theory with infinite number of constant cannot be the correct theory?"

It cannot be a correct theory because it cannot be a theory at all. A theory means a finite system of ideas and assumptions that can in principle be determined, written down and used to make predictions, at least approximate ones. A spiritual system with infinitely many important unknowns does not satisfy this definition of a theory. A spiritual system with infinitely many continuous unknowns is just a way to parameterize (complete) ignorance, not a way to gain knowledge.

Best
Lubos

42. **Lubos Motl**
January 21, 2006

Dear Zorq,

sorry for my being slow but I am afraid that you have not yet explained what you mean by an RG flow that is unrelated to a dilatation – regardless whether the theory has gravity ir not – and consequently, how can a fixed point fail to be scale-invariant.

Best
Lubos

43. **Tony Smith**
January 21, 2006

Peter said, about LQG / Spin Foam / etc models:
"… Unless the infinite number of constants have special properties, one would expect their existence to ruin the predictivity of the theory …".

Isn't and example of such "special properties" the "combination of supersymmetry and twistor geometry" mentioned by Peter in his blog entry at http://www.math.columbia.edu/~woit/wordpress/?p=268 about Zvi Bern's work on fineteness of N=8 Supergravity ?

It seems to me that it would be useful to direct substantial funds and jobs to work on models using exceptional structures related to the exceptional structures of N=8 Supergravity in the hope that:
1 – such models might be finite;
2 – at least one such model might include gravity plus the standard model; and
3 – due to the exceptional nature of the building blocks of such models, such a realistic model might be seen to be unique.

An example of a specific proposal for one such model would be a J3(O) exceptional Jordan algebra high-dimensional spin foam. It is just one example so that readers can see that this proposal is neither vacuous nor limited to supergravity models. Of course, I would advocate pursuing many types of models that use such exceptional structures in various ways.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – As I stated earler, I remain (to use Peter's words) "… curious what … "An assessment of current paradigms in theoretical physics" … is …".
Does anyone reading this blog know? Would they care to share their knowledge here?

44. **JKowalskiGlikman**
January 21, 2006

Just to clarify one thing.

Lee Smolin said:

"If GLAST sees an energy dependent, polarization independent speed of light, thus confirming that the symmetry of the vacuum is DSR, this obviously kills theories that predicted either broken or naive Poincare invariance and supports the plausibility of theories that predict-even generically DSR."

Well, there is a long lasting discussion in the DSR community as to whether DSR predicts energy dependent speed of light. I personally do not think so.

Basically the reason being that there are two effects which should be taken into account when calculating the speed: the modified dispersion relation and the modified phase space structure. In the DSR models based on non-commutative space-time both these effects cancel neatly, so that the speed of massless particles is exactly 1, though the expression for velocities as functions of energies for massive particles differ from the standard one. (more details can be found in hep-th/0405273.)

I must stress however that some experts like Lee, Joao Magueijo, and I think also Giovanni Amelino-Camelia do not agree with this.

45. **Aaron Bergman**
January 21, 2006

Before deciding whether to wade into the rest of this discussion, I just wanted to address this:

In general, one doesn't expect what it means to "quantize" a classical theory to be unambiguous. LQG chooses different variables to work with and tries to find a non-perturbative quantum theory whose classical limit is GR. The whole discussion here comes about because this is not the standard quantization one uses in perturbing around flat space, using the metric components as dynamical variables. Because this is a new set-up, there is still a lot that is not well-understood.

The situation is much "worse" than this in my opinion. LQG used a quantization techinque which is fundamentally different than the approach we use to quantize all the field theories we understand. If we apply the LQG-type quantization to theories like QCD, we get an experimentally wrong answer. Now, Lee will say that because he wants thing to be "background independent", the only choice is to use this new quantization technique and that experiment is the only way to resolve the question. The latter part is certainly correct, but that doesn't change the fact that LQG is not the same as quantization-as-we-know-it.

46. **Thomas Larsson**
January 22, 2006

The latter part is certainly correct, but that doesn't change the fact that LQG is not the same as quantization-as-we-know-it.

You don't need to be a string theorist to be worried about this. As I see it, one must cope with two key lessons:

1. The key lesson from GR may be manifest background independence, which I interpret as a action of the **full space-time** diffeomorphism group on the dynamical objects alone.

2. The key lesson from QM is that, well, it is QM in the conventional sense. In particular, the Hilbert space carries a representation of the one-parameter time evolution group, and this representation must be of lowest-weight type, cf. the harmonic oscillator.

How these lessons should be combined is left as an exercise for the reader.

47. **Anonymous**
January 22, 2006

Lee, how do you expect the matter degrees of freedom to emerge?

48. **D R Lunsford**
January 22, 2006

TL – See, you really can get everything on the back of an envelope!

-drl

49. **D R Lunsford**
January 22, 2006

BTW, it's easier to use the front of the envelope, especially with a fountain pen.

-drl

50. **Lee Smolin**
January 22, 2006

Zorq, the distinction between a finite theory and a uv fixed point is the following: a lattice QFT or condensed matter physics model with a fixed lattice spacing is a finite theory. You can still apply the RG to study the infrared behavior. The point in LQG is that AFTER carrying out the regularization procedure and defining the diffeo invariant states and operators from the limit of the regulator removed, there remains a theory with a fixed, but spatially diffeo invariant cutoff. So it is like a lattice theory in being intrinsically finite, except that all the states are spatially diffeo invariant. So the physical theory has a fixed cutoff. One place this is explained in detail is Rovelli's book, pages 280-282, another is my review hep-th/0408048. To see how this is done rigorously see Thiemann's gr-qc/0110034 or Ashtekar-Lewandowski GR-QC 0404018.

Lubos, the finiteness is achieved kinematically, once the procedure just described is done the theory is uv finite WHATEVER the dynanmics, again just as in a lattice QFT with fixed lattice spaceing. But, to continue with my example of 2+1, which you misunderstood, is that the symmetry of the groundstate is not put in, it is determined dynamically. In 2+1 it turns out to be kappa-Poincare. This cannot agree with perturbation theory carried out as an expansion around Minkowski spacetime, which order by order assumes ordinary Poincare invariance.

I agree there is an issue in the Hamiltonian theory with full spacetime diffeo invariance, which is one of several reasons I always discuss dynamics in the spin foam picture.

Thomas and Aaron, this has been discussed before. For QFT the harmonic oscillator kind of quantization leads to Fock space. The inner product of Fock space depends on the background metric, hence this quantization will never arise in a background independent formalism. So we do not avoid it because we are stupid, it is simply not an option. The question is then to find an inner product for states which are functionals of a connection mod spatial diffeos. The only known way to do this is to first construct a kinematical Hilbert spaced that carries an exact non-anomalous rep of the spatial diffeos and then use that unitary rep to mod out by the diffeos . The uniqueness theorems tell us the result is unique. If you don't like this please at least acknowledge the argument just described and either accept it or propose an alternative background independent quantization and do the work to show it is consistent.

Anonomous: See my talk at the loops 05 conference, details are in a paper under preparation.

51. **Thomas Larsson**
January 22, 2006

either accept it or propose an alternative background independent quantization and do the work to show it is consistent.

Such a proposal was discussed in the end of this thread, in particular in this post. For details, see e.g. this forthcoming book The manuscript is not available online for copyright reasons, but similar material can be found in hep-th/0411028, hep-th/0501043, and hep-th/0504020.

Today I was rereading gr-qc/9903045, since the more philosophical papers by Rovelli belong to my favorite literature. The key message I got out of it is that one should believe both in QM in GR, or rather in the key ideas of both theories. To me, the key property of QM is encoded in lowest-energy representations. Consequentially, anomaly freedom must be given up, but we knew that from 2D gravity anyway.

52. **boreds**
January 22, 2006

Hi Lee

Lubos has posted a lengthy critique of your comments on his site. There's a lot of blogging there, but I'd be interested in your response to this point (which I think is the key thing in NP):

You: The freedom to specify spin foam amplitudes does not map onto the freedom to specify parameters of a perturbatively non-renormalizable theory.

Lubos: Of course it does. Take all spin foam Feynman rules that lead to long-range physics resembling smooth space and assume that the space is not empty. This may be a codimension infinity set but its dimension will still be infinity. The parameters of higher-derivative terms at low energies will be functions of the parameters defining the spin foam Feynman rules. There is a one-to-one correspondence between them.

53. **Zorq**
January 22, 2006

“Zorq, the distinction between a finite theory and a uv fixed point is the following: a lattice QFT or condensed matter physics model with a fixed lattice spacing is a finite theory.”

No wonder you have trouble making yourself understood, when you use standard terms in nonstandard ways.

A lattice QFT or condensed matter physics model with a fixed lattice spacing is a cutoff theory. All physical quantities are finite (that’s what having a cutoff means), but explicitly cutoff-dependent.

A finite theory is one where physical quantites have no explicit cutoff-dependence. This is much more restrictive than saying that the theory has a cutoff.

At least, that’s what everyone else in the world means by “a finite QFT.”

“The point in LQG is that AFTER carrying out the regularization procedure and defining the diffeo invariant states and operators from the limit of the regulator removed, there remains a theory with a fixed, but spatially diffeo invariant cutoff.”

And I am asking, precisely, about what happens when you vary this latter cutoff. How do you need to change the couplings in the LQG Hamiltonian in order to maintain invariance of physical quantities? That’s the RG, applied to LQG.

Go back and read my previous comments, now that (I hope) we have the definitions straight.

54. **fh**
January 22, 2006

“And I am asking, precisely, about what happens when you vary this latter cutoff.”

You do realize that this “cutoff” does not arise from the regulator, that is, it is not put in by hand. Physics shouldn’t be invariant under it’s variation anymore then under the variation of, say, the speed of light.

55. **Brett**
January 22, 2006

The problem I have observed with the LQG terminology is that it seems to take the point of view that many theorists had in the relatively early days of QED renormalization. That viewpoint was that all the infinities of the theory were somehow “real,” meaning for example, that the bare electron charge was really infinite and the bare mass was, in some real sense, zero. The regularization procedures were simply mathematical necessities that allowed us to parameterize all the fundamentally infinite quantities.

Nowadays, thanks in large measure to the work of Wilson on the RG and the lattice regularization of gauge theories, we tend to have a different view. While it is possible that the infinite constants that arise in renormalization are really infinite, we tend to think of them as simply large but finite (or in some cases, not even large). We expect that new physics at some high scale will change the structure of the theory. This new physics might or might not be a renormalizable QFT, but we still expect that the low-energy effective theory will be renormalizable (although one must be careful about applying this RG rule of thumb too generally).

In the first viewpoint, a theory with a fundamental physical cutoff is “finite,” because, if we know that cutoff, all the amplitudes of the theory are finite numbers. If the operators are defined appropriately, no renormalization is required. However, the modern viewpoint recognizes that whether the cutoff used corresponds to somthing physical at the Planck scale or is merely a mathematical device is pretty much irrelevant. In either case, one can look at the low-energy effective theory, using essentially the same kinds of tools.

One can always deal with the problem of having a nonrenormalizable theory by setting a finite cutoff and describing the theory at that cutoff scale. There are then unambiguous predictions for all processes. This is a valid deductive procedure; however, it is no help in the inductive process of working backward from observation to get the fundamental theory, simple because there are an infinite number of parameters in the original high-energy theory.

56. **Chris Oakley**
January 22, 2006

> While it is possible that the infinite constants that arise in renormalization are really infinite, we tend to think of them as simply large but finite (or in some cases, not even large).

There is no such thing as an “infinite constant”. \infinity + exp(-x^2/2) is just as infinite as \infinity.

57. **Luboš Motl**
January 22, 2006

Lee wrote:

“Lubos, the finiteness is achieved kinematically, once the procedure just described is done the theory is uv finite WHATEVER the dynanmics, again just as in a lattice QFT with fixed lattice spaceing.”

Dear Lee, the comparison with the lattice QCD is very apt because the finiteness of lattice QCD is obviously completely fake. You can put any bad-behaved theory on a lattice and assume that this makes everything finite. The problems of the original theory, if there are some problems, immediately re-appear if one tries to take the continuum limit as a dependence on all the details of the lattice theory – i.e. the cutoff-dependence – which is a manifestation of the same problem in a regulated description.

Putting a UV-sick theory on a lattice does not solve the problems; it is just either a translation of the problems to different variables, or – if you want to claim that the problems are gone – it is hiding one’s head into the sand because one does not want to *see* the problems. But the same problems are still there. You can’t simply solve such basic physics problems in such cheap ways – like saying that you make a cutoff.

…

I am confused how Prof. Kowalski-Glikman and Prof. Smolin and others may disagree about the basic question whether DSR predicts an energy-dependent speed of light. Is it just a terminological disagreement or a physical one? My understanding is that DSR violates the constancy of speed of light and it simultaneously violates locality in a brutal way, by any distance, because locality requires a linear representation of the energy-momentum vector transforming under the Lorentz group.

(DSR is deformed special relativity whose symmetry group is the contraction of the q-deformation of the (anti) de Sitter group, which introduces nonlinearities to the [J,P] commutators.)

But do you agree that GLAST should detect something that will look like energy-dependent speed of light? DSR is all about the speed of light. It's almost like string theorists disagreeing whether there are any strings in string theory. 😉

58. **Tony Smith**
January 22, 2006

Anonymous asked "… how do you expect the matter degrees of freedom to emerge? …".

With respect to exceptional spin foams based on structures like those of N=8 supergravity, here is a quote from Peter G. O. Freund's book "Introduction to Supersymmetry" (Cambridge 1986, at pages 117-119) about N=8 supergravity structure:
"… Einstein … repeatedly emphasized the conceptual imbalance between the two sides of his gravitational equations. On the left-hand side sits the Einstein tensor … a genuinely geometrical construct, whereas on the right-hand side we find the energy-momentum tensor totally unspecified by the theory. …
The situation in N=8 supergravity … is radically different. … All basic forces AND all basic forms of matter now appear in the SAME supermultiplet. … Einstein's 'complaint' is answered. … No previous physical theory haas exhibited anything like this degree of self-containedness and completeness. …".

Maybe this is one of Lee Smolin's "reasons" that he "… always discuss[es] dynamics in the spin foam picture …",
and
maybe it is also a reason that Lee Smolin said in hep-th/0104050 "… Supersymmetry appears to be related to triality of the representations of Spin(8) …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

59. **Aaron Bergman**
January 22, 2006

The inner product of Fock space depends on the background metric, hence this quantization will never arise in a background independent formalism. So we do not avoid it because we are stupid, it is simply not an option.

What I don't understand is that I can write down the GR + Standard model Lagrangian, and it looks completely diff-invariant and (other than the need for a spin structure) background independent to me. What you seem to be telling me is that, when I want to quantize this thing, I should picture that R over on the left as completely different from all the other things and quantize it in a completely different manner than everything else. First of all, it's not clear to me that we can do this because everything interacts with gravity. But, I also don't see why shouldn't apply your 'background independent quantization' to the whole shebang and, thus, never see the beta-function in QCD, for example.

I don't see why you focus on Fock space, either. Fock space is a way to quantize freeish theories that have a particle interpretation, but there are plenty of other types of QFTs — certain CFTs, for example.

60. **D R Lunsford**
January 22, 2006

TS – I solved the energy problem without SS. It's not necessary. The sources are derivatives of the gauge field along the (not assumed small) extra dimensions.

-drl

61. **Luboš Motl**
January 22, 2006

Dear Brett,

I completely agree with your observation that our LQG friends often happen to speak about the infinities in QFT in the obsolete, pre-RG, pre-1970s way. If they applied their ideals to all disciplines of physics, they would also reject the Standard Model, because it has "infinities", but accept Fermi's four-fermion interaction because they don't recognize renormalizable and non-renormalizable theories as long as they have infinities and they are proud about it. 😉

Of course, we have known for quite some time that this understanding is flawed, despite the fact that this ignorance did not prevent people from calculating QED loops before the 1970s. Infinities that are regulated or parameterized with a cutoff are just a technicality, and one must do a lot of other work afterwards to figure out whether these infinities are lethal, and if they are not, what are the physical predictions. We can make physical predictions for renormalizable theories where the low-energy physics is independent of the details of the short-distance physics.

My understanding is that the LQG friends just feel to throw up when they see a divergent integral and it stops them – much like others who feel anxious when they're told that space and time can mix. Many things in physics however can't be done with such a phobia. There is a lot of thinking required after we see the first infinity.

More generally, I feel that our LQG friends prefer form over content. Formalism over formalism-invariant physical insights and predictions. Technicalities over profound principles. Lee has unfortunately said it too many times for me to think that I just misunderstood something. He likes the idea that all LQG people are using the same formalism(s), the same methods etc. I think it is a bad sign that says something about the intellectual breadth of the community, and this observation of limited technical resources has nothing to do with the physical coherence of the actual theory or theories.

Formalism, much like the Greek alphabet, is not yet physics even though some people may find it difficult enough to believe that it must be. Carlo Rovelli has informed us that he has not yet digested quantum mechanics – well, yes, quantum gravity and renormalization group are slightly more complicated than quantum mechanics and maybe one should first digest quantum mechanics before he or she starts to solve more difficult matters. Of course that my feeling is that people tend to over-emphasize formalism if they learn it and they feel that it is about the maximum they can learn.

In real physics, people work with many formalisms (often subconciously) whose equivalence they often understand well, and they parameterize UV divergences using many methods – dim reg, brute cutoffs, Pauli-Villars, lattice – whose equivalence for the actual physical predictions should be comprehensible for a physicist, and this equivalence is based on the logic of RG. Making an "infinity" finite by one of the methods is not yet a solution of a problem. It's just a translation of the same problems to different variables.

In LQG, one randomly picks one of these regulators – something like a lattice – and promotes is to a principle. That's of course not very deep and it does not solve any problems. Lee has even explicitly said that he solves all UV problems independently of dynamics. If he does so, then we can be sure that his solution is independent of physics 😉 because the nature of UV problems much like 99% of other things in physics DOES depend on dynamics. If an argument does not depend on it at all, then it's surely wrong.

I feel that the focus of LQG is not on the things that can actually be extracted from a theory but on the limitations what we are actually allowed to think, believe, or use, instead of physical predictions. We are not allowed to use integrals involving UV divergences because they're bad. We are not allowed to do perturbative expansions or other expansions because they're bad. Instead, we should like spin networks that have never been connected with anything remotely similar to observable physics because they are a nice formalism that a community uses.

We are not allowed to consider theories where geometry is just an approximate concept arising from a deeper or broader structure because any additions to GR are bad; GR is essentially a holy scripture and the real goal is to show that we don't have to modify it at all because of "unimportant" novelties such as quantum mechanics. All these things that we are "banned" from doing may be bad for our LQG friends, but they are absolutely necessary for doing theoretical physics at a decent level. Sorry, Lee, if I am the first one who says it to you.

All the best
Luboš

62. **Zorq**
January 22, 2006

"You do realize that this "cutoff" does not arise from the regulator … Physics shouldn't be invariant under it's variation …"

To return to the *point*, there are an infinite number of arbitrary coupling constants in the LG Hamiltonian. These are not fixed by diffeomphism invariance or background independence.

Without a principle to fix them, LQG would be utterly unpredictive. I offered and RG fixed point as one mechanism for fixing them.

You don't like that? Fine. What principle *does* fix them?

63. **Luboš Motl**
January 22, 2006

A few examples of the focus of LQG on the form instead of the content:

Preference over the way to regulate infinities – lattice instead of dim. reg. or Pauli-Villars or other regulators.

Preference over the way how calculations are done – dislike for perturbative methods.

Preferences over Hamiltonian vs. Lagrangian descriptions – it is very important for the LQG people which one should one choose.

Needless to say, neither of these questions is an important question in physics these days. They're mere superficial technicalities. College kids may talk which regularization they prefer but when they grow up, they will eventually understand why it does not matter. The same holds for Lagrangians and Hamiltonians – in any working theory with both of these approaches, they're simply equivalent. Of course, LQG is probably inconsistent so it can't even be shown that the two approaches are equivalent, but that's another reason to abandon such a theory, not a reason to start medieval debates which formalism is "better".

Another example of the preference of form over content are the statements about "background independence". Physics of string theory is manifestly background-independent and the third chapters of all major textbooks explain why. A modification of the background we start with is equivalent to a condensation of physical particles in the original background. That's why we know that there is just one theory and not many theories that depend on a background.

But what our LQG friends find very important is how we actually write down the calculations of the physics predictions. The best observables in quantum gravity is the S-matrix – no one, not even our LQG colleagues, has invented better ones. So we evaluate the S-matrix. The S-matrix makes it necessary to choose a background around which we expand, in order to know what the Hilbert space is – so that we can calculate the scattering amplitudes. This is just about a calculational method, a superficial feature of our strategy to approach physical questions. It does not change anything about the fact that dynamics of string theory is background-independent – and that there is just one theory.

But Lee seems to prefer the form so he would tell us that we are not allowed to expand, and just because we expand things in this way, string theory *itself* fails to be background-independent, which is of course nonsense. I subscribe wholeheartedly that we would be happier if we had a language for string theory that "sees" all the backgrounds simultaneously as solutions of some universal rules. But this dream is not a real necessity. It is just about the mathematical methods that Nature and the world of mathematics can offer us. It is not really a physical question. Nevertheless, it seems to be more important for the LQG friends than physics.

In string theory, we have many contexts in which various descriptions are easier and "default". In AdS/CFT, we really find the physical states of the graviton only. For example, we can go to the light-cone gauge in the pp-wave limit. The unphysical components of the metric are not there. Still, we know that the physics is exactly equivalent to physics that we can also write down covariantly, modulo diff invariance. I am afraid that it is fair to say that any technical step or detail of this kind would just distract our LQG friends far too much so that they could not concentrate on physics – which is what is left from the calculations after we divide by all unphysical details associated with individual formalisms.

Physics is non-trivial and it rules while formalisms are superficial and trivial and they suck. Sorry, Lee.

64. **fh**
January 22, 2006

"We expect that new physics at some high scale will change the structure of the theory."

That is precisely the case in LQG style QFT, which is why people who try to attempt to understand LQG in terms of effective field theories like the QFTs that make up the standard modell and their perturbative expansion, or in terms on analogies to lattice QFT which are often misleading and nevermore then analogies, (like Prof Motl) have such a hard time.

LQG introduces a genuinely new class of QFTs (background independent), with genuinely new structures at high energies and so on. Wether these are physical or not is an open question but the observation that they are different from ordinary QFT is, well, obvious. Most of the tools and intuitions from background QFT do not carry over. Freidel showed however that QFTs in the ordinary sense CAN arise in a certain limit from this new class of QFTs. This is of course crucial, if that was not the case then the objections that these new QFTs can't have anything to do with reality since they do not incoporate the old QFTs would be valid and severe (Freidel said in his presentation at Loops05 that he personally thought of this as a clear branching point of LQG, had this failed then he would have given up on LQG), however this is not the case, and it is hence worthwhile and ever more promising to study this new class of QFTs.

I should note that I am just a beginner in this field, either. But the above is basically why I choose to study it.

65. **Lee Smolin**
January 22, 2006

Thanks everyone for the many comments. Ill try to answer as many as I can:

To Boreds: Take the case of 2+1 QG coupled to some matter QFT, with arbitrary couplings, treated as a spin foam model. This was solved in the recent work of Freidel and Livine. The answer is the same matter QFT on a non-commutative geometry which is kappa-Poincare. None of these are in the class of perturbatively non-renormalizable QFT's on smooth Minkowski spacetime. So when you quote Lubos as saying, "Take all spin foam Feynman rules that lead to long-range physics resembling smooth space…" the answer is that there are no theories in this category because all such spin foam theories correspond to QFT's on kappa-Minkowski spacetime which is not smooth.

So there is no correspondence, because the two classes of theories have different low energy symmetry algebras.

To Zorq, Im sorry if I "use standard terms in non-standard ways." Part of the problem is that the properties of LQG with regard to finiteness are different than previous examples and its hard to discuss this with people who have not looked into the actual calculations and proofs. The point is that diffeo invariant QFT's are a new type of QFT and have features not shared by either perturbatively renormalizable QFT's or lattice QFT's. Now,you ask, what is the relationship between the microscopic Planck's length that serves as the cutoff of the diffeo invariant theory and the macroscopic Newton's constant. To answer this you need to refer to a computable macroscopic quantity. One we have control of is the black hole entropy. By using this we see that the ratio of the macroscopic Newton's constant G_{macro} defined by the black hole entropy formula (Area/4 G_{macro} hbar) to the bare L_{Pl}^2 that comes into the microscopic theory is a parameter of order 1, proportional to the Immirzi parameter. This is how we know there is no infinite renormalization. As I said, there are other calculations that lead to the same conclusion.

To Brett: because of what I just said, measuring Newton's constant is, up to a computable dimensionless constant of order unity, a measurement of the microscopic planck scale. If you like, from a RG point of view because the only coefficient of an invariant term that can contribute to microscopic physics-besides the cosmological constant-is an irrelevant operator, and because you can compute the black hole entropy in closed form, a measurement of a macroscopic quantity determines a microscopic cutoff. This is a very different case from that of marginal couplings that we are used to in perturbatively renormalizable theories. The point of view in LQG is then not like the early days of QED, it is nothing but the modern Wilsonian RG point of view applied to a case where there is no marginal coupling so the low energy limit is dominated by an irrelevant coupling.

To Lubos, how is it that there can be disagreement about a prediction of DSR is that there can be different identifications of elements of the kappa-Poincare algebra with the observed energy and momenta. In 2+1 this ambiguity is fixed by the coupling to gravity. Jurek has one expectation as to how this will work in 3+1, I and others have another. Mine comes from a semiclassical calculation that shows directly an energy dependent speed of light (hep-th/0501091).

To Aaron, yes, when the standard model is coupled to gravity, the whole thing must be quantized using LQG technology. You cannot quantize a theory half by background independent methods and half by background dependent methods. This has been developed in detail, for example in Carlo's and Thomas's books and reviews for all the standard model fields. The result is not surprising and the limit in which gravity is turned off can be check and is reasonable. In the Hamiltonian picture, if you fix a gravitational spin network (setting to zero the terms in the Hamiltonian constraint coming from gravity) the result looks like a lattice theory for the matter on that fixed graph.) Or, in the spin foam picture, if you set the gravitational constant to zero, the spin foam for the remaining matter degrees of freedom is nothing but the perturbative expansion of the matter QFT (see Freidel et al again for this.)

Lubos, I am afraid I can't understand your comments, they do not reflect any detailed understanding of how the actual calculations and proofs are done. Let me just start with the first line of one of your lengthy comments: "Preference over the way to regulate infinities – lattice instead of dim. reg. or Pauli-Villars or other regulators."

For the millionth time, LQG does NOT USE LATTICE methods. It is not a matter of "preference", the methods of background dependent theories such as dim reg or Pauli Villars CANNOT BE USED, because if the whole metric is an operator there is no background metric, which is needed to define them. Nor can lattice regularization be used as that also depends on a background metric. We had to invent NEW METHODS for regularization, suitable for gauge theories whose dynamics is defined on a manifold with no metric and we did so (This was why it took some years of careful work to develop LQG). They are roughly like point splitting regularization but somewhat more complicated because one has to ensure that in the limit in which the regulator is removed the resulting states, inner products and operators are spatially diffeo invariant. This is more intricate and constraining than the regularizations defined by background metrics.

I could continue to correct each comment you make line by line, but as almost nothing written there corresponds to what is actually done it would take a book. I would ask you once again to please study the details and understand them. There are valid criticisms and limitations to LQG which are worth discussing, but its hard to argue with someone who is unwilling to understand the actual results and claims.

Zork, What principle does fix the couplings? First, we don't know that there are an infinite number of consistent couplings, either at the Hamiltonian level or the spin foam level. What we know is that there is one ordering of the Hamiltonian constraint that is consistent with diffeo invariance, which has the standard two parameters. It is not known how many others there are. But as I said, none of the regularizations of the Hamiltonian constrains I know lead to the exchange moves, and so none are acceptable.

While there is some uncertainty, I believe this is because in the Hamiltonian approach one regulates by point splitting in space and not time. So to get a good evolution amplitude we regulate with spin foam methods. At the spin foam level we know of one evolution amplitude that has been proved to have uv finite sums over labels, we do not know how large the space of amplitudes with that property is. It would be very worth knowing this, but it just hasn't been done.

Second, I personally did not reject your proposal of using the RG to find one. A theory with a cutoff can still have a non-trivial topology of RG flow, the different universality classes of which define the possible low energy behaviors. After all this is what happens in the standard theory of 2nd order phase transitions in 3d condensed matter systems. I have for years strongly encouraged the development of RG ideas to the problem of selecting the good spin foam theories. It is more complicated, see papers of Markopoulou on the RG for spin foams to understand why (gr-qc/0203036, hep-th/0006199) but some progress was made and I believe much more progress could be made on this.

To fh: sorry to repeat some of your remarks,

Thanks, Lee

66. **Aaron Bergman**
January 22, 2006

The result is not surprising and the limit in which gravity is turned off can be check and is reasonable

Now I'm confused. Let's do the harmonic oscillator coupled to gravity. I thought we had established that quantizing the harmonic oscillator (or QCD or whatever) alone by LQG techniques gives an experimentally incorrect answer. Now are you claiming that, instead, if do the coupling to gravity, quantize the entire system and then take G->0 instead, we get a different result than if we had never turned on gravity in the first place, and that that result is the correct one?

67. **Zorq**
January 22, 2006

"[W]e see that the ratio of the macroscopic Newton's constant G_{macro} defined by the black hole entropy formula (Area/4 G_{macro} hbar) to the bare L_{Pl}^2 that comes into the microscopic theory is a parameter of order 1, proportional to the Immirzi parameter. This is how we know there is no infinite renormalization."

As I explained above, you learn nothing of the kind. Stop using the words “infinite” and “infinities” and start using the language of modern Renormalization theory.

Once you do, you will see that the relation between the low-energy Newton’s constant and the bare coupling in the LQG Hamiltonian is exactly what one expect of any theory with a cutoff at L_p. It is not some deep breakthrough, but a trivial observation.

“Zork, What principle does fix the couplings? First, we don’t know that there are an infinite number of consistent couplings, either at the Hamiltonian level or the spin foam level.”

Yes, we do.

There are an infinite number of polynomials in the Riemann curvature and its covariant derivatives that can be added to the spin-foam action. A similar infinite set of couplings exists, also, in the LQG Hamiltonian.

Neither diffeomorphism invariance, nor background independence fixes those couplings.

What does?

68. **Aaron Bergman**
January 22, 2006

One other thing. You refer to a ‘uniqueness theorem’ a number of times. How does this reconcile with the well-known fact that there are a number of inequivalent quantizations of gravity in 2+1 dimensions?

69. **Anonymous**
January 22, 2006

Lee,

Here’s my attempt to make some progress in this conversation while keeping the discussion civil. It appears to me that you and some of the others in this debate are speaking two different languages. And while I can understand that you want people to look at the details of LQG, I think you would do well to formulate a response in a language others can understand *without* looking at these details. Otherwise, we will naturally tend to be skeptical. I really think that if you want to be taken seriously you have to be able to answer this question at a rough, hand-waving level, without appealing to any theorems. Otherwise your claims seem to run counter to basic intuition.

So let me try to formulate a question in a clear way, and hopefully you can give at least a heuristic response that does not require knowing the technical details.

In ordinary QFT we know that there are numerous irrelevant operators involving gravity that one can add to the Lagrangian. Their infinitely many undetermined coefficients at a given scale account for the nonrenormalizability of gravity. Furthermore, none of them in any way violate basic principles of background independence or diffeo invariance.

So, a field theorist wanting to understand your work naturally will ask, are you saying there is a UV fixed point? If not, what principle relates the various couplings?

As far as I can tell, your answer to the first question is no, there is a cutoff. So the next question becomes important: why are there not infinitely many choices of theory in your cutoff theory?

At this point you seem to appeal to the details, but a good heuristic answer would be a great help to an outsider wondering if it’s worth looking at those details. If some deeper principle fixes the whole set of infinitely many coefficients (or reduces them to a smaller finite-dimensional set), what is that deeper principle like? It can’t just be background independence, because these terms don’t inherently violate background independence.

You stress that these theories reduce not to usual QFTs but to some kappa-deformed theories, but it doesn’t seem that that resolves the problem either. Such a deformation doesn’t make gravity renormalizable.

Whatever happens in your theory, there must be some way to give us at least a rough explanation of how it fits with these facts. Just saying “it’s not an ordinary QFT” is a deeply unsatisfying answer. If it reduces to an ordinary QFT at some point below the Planck scale (up to whatever kappa-deformation you’re claiming there is, which must be a small effect to fit with observed data), then somehow it has to determine the usual coefficients for the Wilsonian RG to take over. And so there has to be something very special going on, *beyond just background independence*. For instance, in string theory the sort of heuristic answer I am looking for is that the infinitely many fields of arbitrarily high spin constrain the interactions.

(I’ve been saying things like “there has to be” a lot, but if you can explain why *that* is wrong, that would be interesting too.)

Thanks!

70. **Anonymous**
January 22, 2006

Oops, I seem to be duplicating Zorq’s question in a much more long-winded way. I guess we were writing at the same time.

71. **Who**
January 22, 2006

fh
(Freidel said in his presentation at Loops05 that he personally thought of this as a clear branching point of LQG, had this failed then he would have given up on LQG),…

http://www.math.columbia.edu/~woit/wordpress/?p=330#comment-7811

I remember that. I have the video (whereas you, I expect, were present).

It was impressive. I have not yet heard people in some other lines of research say, even in retrospect, an empirical or mathematical result that would make them abandon their theory and change fields. The way he said this convinced me he was absolutely serious about it. Then jokingly:

“But the fact that I am here talking to you shows that it worked out…”
or words to that effect.

I believe that Freidel has a new paper (co-authored with Baratin?) in preparation which looks into the 4D case. Again the question is, if i understand correctly, does the does the gravity theory specialize to give matter QFT if you let G go to zero. Do you know anything about this, fh?

72. **Lee Smolin**
January 22, 2006

Hi everyone, thanks, again Ill do my best to answer clearly.

First to Zorq: You assert: “There are an infinite number of polynomials in the Riemann curvature and its covariant derivatives that can be added to the spin-foam action.” No, we don’t know this because the spin foam amplitude is not expressed in terms of polynomials of the Riemann curvature. It is expressed in terms of invariants of quantum groups, such as q-15j symbols. I would not be surprised if there are an infinite number of such possible amplitudes, but I know of no result that shows so.

Further, as I thought I emphasized, there are conditions to be imposed. One, for sure, is the uv finiteness (in the sense that sums and integrals converge) of the sums over labels in a spin foam amplitude. This is quite restrictive. Only a few solutions are known, (see as usual hep-th/0408048 for exact references to the finiteness proofs for spin foams). We just do not know how large is the space of spin foam amplitudes that have this property. So I am making no claim here, either way. But you asked for restrictive conditions and this is an important one. OK?

You also say: “A similar infinite set of couplings exists, also, in the LQG Hamiltonian”-to which similar remarks apply. The LQG quantum Hamiltonian constraint is not expressed in terms of classical quantities and it has to satisfy some quite non-trivial consistency conditions coming from the operator constraint algebra. We do not know how large is the space of solutions to these. As I mentioned we know of only a few.

(Also, I thought I was using the language of the Wilsonian RG, and I agree I was using it to make a trivial point.)

Now, to Anonomous. Thanks for your helpful attitude. You ask, “In ordinary QFT we know that there are numerous irrelevant operators involving gravity that one can add to the Lagrangian….so why are there not infinitely many choices of theory in your cutoff theory?” I hope the above answers you clearly. The point is that the spin foam action is NOT made by writing classical spacetime diffeo invariant expressions in the continuum and then subjecting them to a regularization. Once you are in the spin foam or group field theory language the amplitudes are expressed in a different framework-that of quantum group invariants. This is consistent with the idea that there is no continuum below the Planck scale. And there are conditions imposed. The conditions of finiteness of sums over labels of spin foam amplitudes is pretty restrictive, so there could easily be only a finite space of solutions to it. But this has not been shown so I am not claiming it is true.

Now about 2+1: “You stress that these theories reduce not to usual QFTs but to some kappa-deformed theories, but it doesn’t seem that that resolves the problem either. Such a deformation doesn’t make gravity renormalizable.” First, the point of the argument was to directly show that an argument given by someone was wrong because, again, none of the DSR theories are in the class of perturbative QFT’s on Minkowski spacetime. Second, DSR theories can be uv finite because there are maximum energies in sums over momenta.

You want a heuristic explanation for why a diffeo invariant regularization procedure results in finite operator products. OK, here is one that was very helpful to us originally. (again see 0408048). In the absence of a background metric all operators are distributions and all distributions are densities. So when defining an operator product in the absence of a background metric you have to carefully keep track of density weights.

We regulate by point splitting, which means we introduce an auxiliary background metric q_0 just for the purpose of defining the distance between points. We have to define the product of two operator valued distributions to be one operator valued distribution. In general the result has inverse powers of the distance measured in units of q_0 times determinants of q_0 to soak up the density weights. If the resulting operator is to be diffeo invariant (under actions on the quantum fields) it cannot depend on q_0 because q_0 is not acted on by the operator that generates diffeos. It turns out this means it cannot depend either on the distance measured in units of q_0 (this is seen by an argument where q_0 is scaled.) Thus, a diffeo invariant operator extracted from the limit in which the regulator is removed cannot depend on q_0. Hence because all divergences are measured in units of q_0 there can be no divergences. This is borne out by the detailed calculations. But what kind of operator can appear? Only one that is a natural integral over an operator of density of weight one-as the density weight can in the limit only come from the operators. Since local field operators have density weight one, this can come from an n’th root of the product of n operators, each of density weight one. This is exactly how area and volume operators are defined.

There is another trick which is to represent the inverse of the determinant of the operator metric as a commutator of operators that can be defined in the regulated theory. This allows us to construct further finite operators including the Hamiltonian constraint.

To Aaron, about the limit G to zero in 2+1 see Freidel and Livine for how it works in detail. Remember I am talking here about a spin foam calculation, so the result comes from a path integral and not the Hamiltonian theory. You can ask, could this have been seen in the Hilbert space and how would the limit G to zero look there? I don’t know (I hope that is ok as these are new results.) It seems like a good research problem, perhaps you would like to work on it.

Also, what inequivalent quantizations of 2+1 gravity are you referring to?

73. **Wolfgang**
January 22, 2006

> what inequivalent quantizations of 2+1 gravity are you referring to?

I guess the ones described e.g. by Carlip ?
While there are different ways to quantize 2+1 (and Ponzano-Regge is among them) , they do not give the same results.

74. **Wolfgang**
January 22, 2006

Lee,

let me use this opportunity to ask how the exact solution of Martin Pilati for strong-coupling gravity fits into your picture ?
http://prola.aps.org/abstract/PRD/v26/i10/p2645_1

75. **Aaron Bergman**
January 22, 2006

Also, what inequivalent quantizations of 2+1 gravity are you referring to?

See Carlip, here

You want a heuristic explanation for why a diffeo invariant regularization procedure results in finite operator products.

That was not the question anonymous (or zorq) was asking. “Finiteness” is not the issue here. I think you’re conflating renormalization with the removal of infinities, and they’re not the same thing. I assume we all believe in effective field theories. I can write down a cutoff EFT for gravity, and there are an infinite number of free couplings in the Lagrangian. At a given value for the cutoff, does LQG tell me what the ‘correct’ values for those couplings area?

76. **Luboš Motl**
January 22, 2006

Dear Lee,

please accept my apologies in advance but I often find your comments to be a good reason to wisely smile rather than learn. It does not matter whether you call the spin network a "lattice" or "non-lattice". What's important is that it is a discrete regulator and it shares the same features with the lattice. More concretely, it translates the irrelevant couplings to cutoff dependence and infinitely many undetermined couplings at the cutoff scale – namely the Planck scale.

You argue that the regular methods are bad, and your (…) methods that you (…) had to invent are better ones, exactly confirming my last comment about the focus on form instead of the content. Let me remind you that the standard ways to quantize general relativity have led to actual physical insights of a Nobel prize caliber such as black hole thermodynamics, unlike yours (…). Expanding quantum fields around a classical background is actually what gives a control over physics of quantum gravity, at least to the leading and subleading orders. Denying the partial success of this approach and the relative failure of these discrete approaches seems as far too crazy an attitude for me to discuss about it in length.

You also give this answer to Zorq:

"Zork, What principle does fix the couplings? First, we don't know that there are an infinite number of consistent couplings, either at the Hamiltonian level or the spin foam level…"

Maybe you don't know it, but those of us who have studied and who actually know LQG at the technical level, not just the popular one, DO know that there are infinitely many couplings that are equally consistent as long as any continuum limit is possible and as long as you can write at least a single term in the Hamiltonian, whatever definition of consistency we pick. For example, if you allow us to introduce "moves", we can include diff and gauge invariant terms with multiple (N) moves. (If you don't allow moves, you will likely end up with an ultralocal theory where different points are decoupled forever and where the speed of light is zero.)

These couplings are nothing else than the spin-networkization (this obscure word is to prevent your vacuous criticism for my using the usual word "latticization" – and be sure that it is my terminology and not yours that is standard) of the low-energy effective couplings. The infinite degeneracy of these couplings is an obvious fact and we can write infinite families of these couplings for you in the case that you really don't know them. You will find this answer in all expert papers about the question whether the number of possible LQG Hamiltonian couplings is finite.

If you think that you have found the unique Hamiltonian for LQG, everyone will be happy to read a paper about it.

Your sleight of hand with the "non-smooth" kappa-Minkowski spacetime cannot circumvent the actual theorems showing that what you say is not possible and that our effective field theory arguments apply to LQG much like any other theory of physics. Kappa Minkowski spacetime is moreover smooth – what it lacks is locality, not smoothness (by which I mean the continuous character and unboundedness of momenta).

Any physical theory of the type we look at must be approximated by effective field theories (fine, not necessarily Lorentz invariant, but almost exactly Lorentz invariant) at low energies. There is no way out and trying to generate obscure terms containing Greek letters with a "special status" just in order to avoid conclusions that can be made does not lead anywhere – because you don't actually address our questions, you just avoid them by pompous terminology.

Whenever someone says that XY cannot work because 2+3=5, you invent an explanation why rational thinking and 2+3=5 cannot be used in your context because your context is surely "better" and "above" these dirty theorems that mortal human beings actually use. 😉

The theorems we know have been designed exactly to deal with possible theories like LQG and LQG is in no way a counterexample of these theorems.

Another method that unfortunately resembles fast commercials from cheap TV stations is your format of a sentence "this has been fully solved / answered … in a recent paper by Livine / Rovelli / Dreyer / …". Whenever one actually looks at these papers, one either finds incoherent noise, or wild speculations full of wishful thinking, or a discussion about a simple and different topic that does not imply anything whatsoever for the question that was discussed. This may be a good method to fool the badly informed laymen for five minutes but I am afraid it is not a good starting point for a serious discussion among physicists.

This has been strikingly the case of the statements about the black hole entropy in LQG. As we know, the initial papers about it were just wrong because they incorrectly neglected the higher spin punctures that do contribute, as we know today. This has started a whole industry attempting to show that the prediction of LQG is actually correct and the Immirzi parameter should be proportional to log(3) or log(2) or something like that. That would be great, people thought, because having computed the entropy, LQG could compete with string theory on this important front.

Newer papers have revealed that the entropy predicted by LQG is proportional neither to log(3) nor to log(2). Moreover, it has also been shown that the result from quasinormal modes of a generic black hole is not proportional to log(3) or log(2) either but instead, to yet another number or a function of the BH parameters, more precisely. In summary, all conjectures about the correctness of the LQG BH entropy or even its relations to quasinormal modes – much like the general conjectures about the quasinormal modes themselves – have been shown to be obviously false (note that it was not just one side that was wrong, it was both sides as well as the conjectured link) and everyone who has worked on these things has known this conclusion at least since 2003. The magnitude of black hole entropy is completely obscure from the LQG viewpoint (even if you accept the strange assumption that the black hole interior does not contribute) and all known calculations lead to contradictions.

Still, I've seen a much newer comment of yours claiming that the coefficient of the LQG BH entropy has been verified or something like that which I frankly find rather incredible. It is even hard to decide whether you actually believe what you're saying because it seems really difficult after 100 papers or so that show that the statement is false.

Best
Luboš

77. **Lee Smolin**
January 22, 2006

Hi, I hope the following is useful.

First, with respect to strong coupling limit of quantum gravity in the context of LQG, see THE G (NEWTON) —> INFINITY LIMIT OF QUANTUM GRAVITY, Viqar Husain , Class.Quant.Grav.5:575,1988. This was early days, probably the results Viqar got on the strong coupling limit could be much improved with what we have learned since.

I am very happy to talk about effective field theories, "I assume we all believe in effective field theories…" Yes, BUT we must be careful to remember that the class of effective field theories are labeled by the symmetries and gauge symmetries of the ground state. There are separate classes of effective field theories for Poincare invariant, kappa-Poincare invariant and broken Poincare invariant theories. This is an elementary fact, but it is the key point I have been trying to make. If you have two theories and one describes perturbations around a ground state that has symmetry P and the other has a ground state with symmetry Q and P is not equal to Q then they are not described by the same class of effective field theories. Becauuse each term in the effective action should be separately invariant under either P or Q and they are not the same. Is that clear?

Having said this I don't understand what we are arguing about. I agree there is some set of possible amplitudes of spin foam models. I mention that it is likely that these will be restricted by the condition of finiteness of sums over labels. I mention that we do not know how large the space of such good spin foam models is. Some of you would have bet there were no such theories, so I'm surprised if you are now cavelierly insisting there are infintie numbers of them.

I do not deny it is possible it may be infinite, but I also will not be surprised if the condition of finiteness is restrictive and there are only a few parameters. But the bottom line is we don't know. I also agree that these will map to effective field theories. I only insist that if Poincare invariance is q-deformed, or the geometry is non-commutative as is the case in 2+1 this is NOT the same class of effective field theories that are constructed by perturbation theory around flat Minkowski spacetime. I agree it would be very interesting to know how the parameters of the finite spin foam models map to the parameters of the effective field theory with the appropriate ground state symmetry. I don't say more because as I've said already, we don't know the answer for 3+1. It is only very recently we know the answer for 2+1 with matter. What is not clear about this?

If someone thinks it is known or obvious that the space of spin foam amplitudes which lead to convergent sums or integrals over labels for any spin foam diagram is infinite dimensional, please provide details as this would constitute an important new result. Otherwise, do not presume to know the answer to an open question. By the way, let me stress sincerely it would be very important to characterize the space of finite spin foam amplitudes, and I hope someone will take this on.

As for black hole entropy, the only thing I need for the argument I made is that the ratio of area and entropy is a finite number. So the technical issue of the right way to count the states is irrelevant because all proposals lead to finite ratios.

78. **Zorq**
January 22, 2006

"If you have two theories and one describes perturbations around a ground state that has symmetry P and the other has a ground state with symmetry Q and P is not equal to Q then they are not described by the same class of effective field theories. Becauuse each term in the effective action should be separately invariant under either P or Q and they are not the same."

I'm not at all clear what you mean here. We are talking about pure gravity theories, right? I thought your mantra was that General Relativity (and, the infinite generalization thereof, involving higher powers of the curvature and its covariant derivatives) is background-independent.

So, you claim (or hope) that spin-foam theories will not have an infinite number of independently adjustable couplings. (Why? There are an infinite number of quantum-group invariants that you could, in principle, write down. Why stop at 15j-symbols?)

Let's assume you're correct.

That means that, when passing to the effective continuum field theory, it picks out unique (or nearly unique) values for all the infinite number of couplings.

Computing the coefficients of the R^2 and R^4 terms in the supergravity effective action has been an illuminating activity in String Theory. What are the R^2 corrections to the Einstein-Hilbert action, predicted by spin-foam theories?

79. **Lee Smolin**
January 23, 2006

Zorq asks "There are an infinite number of quantum-group invariants that you could, in principle, write down. Why stop at 15j-symbols?"

Because, to repeat myself, not all of them are likely to have the following property: that the sum over labels on a fixed spin foam converges. This good property implies uv finiteness of a spin foam model. Some models have it (gr-qc/0104057,gr-qc/0508088, gr-qc/0512004) and I hope you agree that it is a reasonable selection principle. We don't, to my knowledge, yet have a classification of which spin foam models have this property.

He asks also: "I thought your mantra was that General Relativity (and, the infinite generalization thereof, involving higher powers of the curvature and its covariant derivatives) is background-independent."

The poiint is that once we are in the quantum theory., there is no metric or manifold. Hence background independence cannot be implemented by choosing a diffeo invariant classical action, for there is no manifold or metric that classical action can be a function of. The classical manifold, metric and curvature themselves can only emerge in the low energy limit.

So the effective field theory analysis will not be background independent, it will only describe excitations of a particular ground state. Hence it must take into acocunt the symmetry of the ground state one is studying excitations of.

Finally, we cannot assume that any term that appears in the effective action of a poincare invariant graviton theory will appear in the effective action of a kappa-poincare invariant theory. We just don't yet know what the combination of diffeo invariance and kappa-Poincare invariance will allow for possible terms in the effective action.

As for whether there are R^2 terms in the effective action and what their coefficients are, this information should I agree be extractable for each particular spin foam model. For the Barrett-Crane model, this may soon by possible by extracting it from the calculation of the propagator done by Rovelli et al. (gr-qc/0508007) in that model. They are working steadily towards this goal.

Thanks, Lee

80. **Zorq**
January 23, 2006

"Because, to repeat myself, not all of them are likely to have the following property: that the sum over labels on a fixed spin foam converges. This good property implies uv finiteness of a spin foam model."

And I will repeat, for the unwary, that this usage of the phrase "uv finiteness" has nothing to do with the notion of uv finiteness found in the textbooks.

"So the effective field theory analysis will not be background independent, it will only describe excitations of a particular ground state."

Will the effective field theory, at least, be writable as a generally-covariant local functional of the metric? (In other words, an Einstein-Hilbert action + higher corrections.)

If so, then all that can happen is that the coefficients of the various terms in the action can change.

So you're claiming that the coefficients one extracts for the (generalized) Einstein-Hilbert action will be different in different backgrounds?

Interesting …

“Finally, we cannot assume that any term that appears in the effective action of a poincare invariant graviton theory will appear in the effective action of a kappa-poincare invariant theory. We just don’t yet know what the combination of diffeo invariance and kappa-Poincare invariance will allow for possible terms in the effective action.”

What do those words mean? What *other* constraints are there on the (generalized) Einstein-Hilbert action above and beyond locality and general coordinate invariance?

81. **L.**
January 23, 2006

Surely mr. motl is joking when he promotes Nicolai et al. to quantum gravity experts, with no offense intended to them.
Sure it is fun to do physics but it doesn’t mean that this is not a serious activity where it takes much more to become an expert in a field than writting an incomplete review on a field as it was a few years ago, writting a research paper that adress and solve a problem is for instance an example of what it at least takes. I hope the next paper of Nicolai will be a resarch paper adressing some of the issues he cares about and i am sure knowing his capability that it will be interesting.

I will try to answer peter’s initial request hoping but not feeling totally sure that it might help the debate at this point.
The latest review of Nicolai et al. is much more satisfactory than the previous one, which essentially was describing the field as it was circa 1998 ignoring most of the work that was done since namely on spin foams exactly with the motivation to adress some of the issues he mentionned their.
He tries in the new review to include some of the more recent material and some of the problem he point out are problems recognised in the community for some time and some of them being already adressed in the litterature. I don’t think that was their intention (Nicolai is a genuine skeptic i think, and we need skepticism in science, its healthy) but sometimes in the presentation it looks like they are dicovering the issues they talk about and edged on making the impression that people working on this are not aware of the issues or concerned. Yes making a deeper relationship between spin foam and LQG is important (see the recent work by perez on this and on ambiguity in LQG, the recent work of Thiemann on the master constraint and some older and important work by Livine and alexandrov who made key progresses in this direction) and Yes adressing the semi classical limit is a necessary and key step (more remark later on that).
There is however a certain number of imprecisions, omitions and misunderstanding in their review. I will talk only about the spin foam section.
For instance when they present the riemannian spin foams they confused what is done in the litterature namely a quantisation of riemmanian quantum gravity with some hypothetical and to be defined hawking-like wick rotated version of Lorentzian gravity.
The purpose of spin foam is to construct the physical scalar product, and this means that we sum over history with exp i S. No direct relation is therefore a priori expected between Lorentzian and Euclidean theories. That’s why both Lorentzian and Euclidean model are studied, lots of the techniques are similar the lorentzian case involving non compact groupsis technically more challenging.
When they discuss the Barrett-Crane weight they confused the 15j symbol prescription (which describes a topological field theory) with the 10j symbol prescription which deals with gravity and present this as an ambiguity.
They also make the wrong statement that the spin foam approach is plagued with the same amount of ambiguity as LQG. This is not correct, the ambiguity in LQG amounts to ambiguities in the choice of the vertex amplitude (like different spin regularisation) whereas there is a large consensus on the form of the 10j symbol (in fact the intertwinners that needs to be chosen are shown to be unique).
There is an ambiguity in the choice of edges amplitude but this amounts into a different choice of normalisation of spin network vertices.
If one chooses the canonical normalisation that comes from LQG this edge amplitude is fixed uniquely. The possibility to have less natural normalisation was introduced later as an exploration of these models especially in order to have finite spin foam models when loop correction (bubbles) are included (gr-qc/0006107). This attracting possibility was later dismissed by showing that if one insists on preserving spacetime diffeomorphism invariance at the fundamental level and it was argued that the spurious divergence that arises in these higher loop amplitudes are signature of residual diffeomorphism (gr-qc/0212001).
They forgot to mention that the Hilbert space on which LQG and Barret-Crane model are isomorphic in the riemannian case, they forgot to mention that there are many different and independent derivations of this weight from the dynamic of GR.
They present as an other ambiguity the restriction to tetrahedral weight. This restriction is perfectly consistent with the fact that 4-valent spin network are enough to construct states with non zero volume and that any LQG dynamics act within this subspace which should be though as a superselection sector of the theory.
So this means that the line of thought that starts from a classical action and construct a quantum gravity weight as singled out one prefered possibility.if one singled out the canonical normalisation.
This doesn’t mean that this model is definitely the right one and having the correct semi-classical dynamic is the key issue, but it shows that
by adressing the problem of the dynamic in a covariant way and focusing on the implementation of space-time diffeo, one proposal stands out from microscopic derivation and adress in a satisfactory way some of the LQG issues, which is by itself an important result.
I don’t want to give the wrong impression and claim that everything is settled down for this model, there are still some study and questionning going on about this proposal which is not totally free of potential problems which are not really the ones presenting in NP (except that we are still lacking a strong physical argument for the canonical choice of edge amplitude), but clearly the presentation NP is very far from fair and accurate.

They mention some problems with 2D regge triangulations having to do with spiky configurations without mentioning and knowing (even if they cite the relevant paper in a footnote) that this problem is now fully understood in the more relevant and interesting three d case (these spiky configuration are just an overcounting due to redundant gauge degrees of freedom, a issue that was overlooked in the first works).

Also they completley miss the point about how we reached triangulation independence and the fact that there is a auxiliary field theory called group field theory which naturally gives the prescription allowing to compute triangulation independent spin foam amplitude. This is one of the main line of developpement of spin foam model it started a while ago now (hep-th/9907154) and not mentionning this line of work which explicitely adress and solve one of the issue they worry about is not a small ommission.
For instance they say `A third proposal is to take a fixed spin foam and sum over all spin’ and cite (hep-th/0505016) which exactly show on the contrary how to consistently sum over spin foam in order to get a finite triangulation independent positive semi definite physical scalar product and allows for the first time computation of dynamical amplitudes. This proposal is uniquely fixed by the microscopical model and if one stick to Barret-Crane with canonical normalisation this gives
a uniquely defined scalar product (so far for riemmanian gravity).
This is very far away from the picture they draw. The best I can do understand how they came up with this false understanding, is that they haven’ read the paper because its pretty clear that what done there is not what they describe.
Concerning the semiclassical issue they don’t mention at all the new line of developpement which consist of coupling quantum gravity to matter and integrate out the quantum gravity field in order to read out what is the effective dynamics of matter in the presence of quantum gravity.
this allowed to solve in a completly unambiguous way this issue in 3D.
They don’t mention all the other work in this directions involving coupling to matter field which were presented at Loop2005 (work of lee starodubtsev, baratin …)
I could continue but i think i can stop here for some detail criticism of their work. If i was referee of this paper i would at least suggest them to go back to their drawing board before submitting it.

82. **boreds**
January 23, 2006

Hi Lee and Zorq

“To Boreds: Take the case of 2+1 QG coupled to some matter QFT, with
arbitrary couplings, treated as a spin foam model. This was solved in
the recent work of Freidel and Livine. The answer is the same matter

QFT on a non-commutative geometry which is kappa-Poincare. None of
these are in the class of perturbatively non-renormalizable QFT?s on
smooth Minkowski spacetime. So when you quote Lubos as saying, ?Take
all spin foam Feynman rules that lead to long-range physics resembling
smooth space?? the answer is that there are no theories in this
category because all such spin foam theories correspond to QFT?s on
kappa-Minkowski spacetime which is not smooth."

I don't know much about QFTs on kappa-Minkowski spacetime. But I
presume that if you try to perturbatively quantize GR around this
background that it will be non-renormalizable, and that there will be
an infinite set of counter-terms. Is Lubos correct in saying that this
infinite number of parameters corresponds to ambiguities in the
spinfoam amplitudes? (Even if he was wrong on saying Minkowski instead of kappa-minkowski?)

I think you are saying that it does, *but* that it is not 1-1 because of
restrictions on the spinfoam side. Is it known what restrictions
`finiteness' in LQG terms places on the low energy couplings?

Zorq, I'm not completely certain what you're referring to when you
talk about corrections to supergravity, but…

the R^4 corrections etc you'd normally talk about in sugra are not the
same beast as the higher order terms in a wilsonian effective
action. they are really giving you equations of motion for the vevs of
the graviton and other fields, so in qft terms are more like a quantum
effective action.

You're right, string theory does (also) constrain the infinite number of
parameters
in the wilsonian action—but my (imperfect) understanding of this is
that the calculation is done in SFT, is hard and is not
equivalent to calculating sigma model beta-functions (or whatever).

83. **Juan R.**
January 23, 2006

Sorry, but i continue being skeptic about LQG capabilities.

The only good thing about LQG i can say is that their believers are often less arrogant and more honest that stringers and we may admire they after of decades of hard work. For example, i newer saw a LQG beggining a talk on quantum gravity with the (in)famous "there is only an approach to quantum gravity" so characteristic of arrogant stringers talks.

I (as others) consider that string theory is pure nonsense in many ways (and expressed several points) but I would say (as others also) that LQG is not consistent. None new paper or talk can convince me, since only way to convince me that LQG is a good approach would be changing LQG by another **NEW** approach.

I am sure there will be quantum gravity research still for a few years…

—

Juan R.

Center for CANONICAL |SCIENCE)

84. **L.**
January 23, 2006

Concerning renormalisation group issues i am not sure that all the experts that covered this subject always have in mind that we are talking about quantum gravity and that some of the major results in this fields needs qualification when applied to this subject (the notion of scale, scaling in background independent theory is way more subtle). It doesn't mean that this cannot apply of course and there are beautiful news results and research going along this line recently, namely in the work of Reuter et al. and Percacci et al. Niedermaier et al. etc… they have by the way revived (not proven, there is a difference) the asymptotic scenario which is another way to go around non renormalisability.
This is not free of difficulty either (being sure that the statements made are really diffeo invariant being one of the major one).
An other key and special point about renormalisation group in GR is the fact that G_{N} is at the same time a coupling constant and a wave function normalisation which can be used to fixed your set of unit. The natural unit we work with in quantum gravity is planck unit, in this setting the notion of fixed point is not well defined because the renormalisation group flow vector field (the beta function) depends on the cut-off. If you try to work in cut-off unit (which is the usual scheme but much more delicate concerning diff invariance) a very important subtltety arise that the change of unit is not really invertible, as an introduction see for instance \hept-th0401071.

By the way an interesting concidence is that having asymptotic safety is realised then in Planck unit the cut-off parameter have a finite value and the effective anomalous dimension at the non gaussian fiexed point is two dimensional. These renormalisation facts resonates strickingly with the picture arising from background independent approaches, wether its LQG, dynamical triang or spin foams.
I don't know if this is just accidental or something deeper is going on, one should be careful but it is interesting. I just wanted to make sure that our local experts on the renormalisation are really tune up to apply it to gravity.

Also 2+1 gravity when treated as a perturbation theory around flat space is non renormalisable but however finite and unambiguously defined. Of course i don't want to imply that what happens there apply to the 4D case, but this exemple should be applied to most of the statement made here in order to make sure that it doesn't provide a counterexample
Concerning the very nice work of Carlip and the potential ambiguities in 2+1 (did you know that the world revolved since then?), lets recall that the ambiguities that are described in the work of Carlip refer to the quantisation of pure three d gravity on the torus. A case that we can now do in the back of the envelopp.
This case is far too simple and especially singular (it is a non stable RSurface)to be generic, in order to chose the right quantisation you have to show that it is possible to consistently quantised the theory on all types of background while respecting the symmetry. This means that you have to give the prescription to glue amplitudes and extend the quantisation to higher genus surfaces and include topology changes while respecting the diffeomorphism symmetry of the theory.
The Ponzano-Regge model properly understood does exactly the job and pick one particular candidate available (Maas operator of weight 1/2 if i remenber correctly) in

the torus as being consistent and anomaly free, I don't know of any proof or evidence that an other inequivalent but consistent quantisation scheme exists.
I don't have a proof either that that the other possibility are necessarilly inconsistent an interesting but difficult open problem. The bottom line is that there is only one full quantisation of three d gravity known today where everything can be computed: the spin foam quantisation, which is also shown to reduce to the t'hooft quantisatisation of the theory when the later apply and to the hamiltonian chern-simons quantisation, when the later apply namely if you restrict to cylinder.

85. **Urs**
January 23, 2006

Concerning those kappa-Minkowski spacetimes: The deformed symmetry group is the *global* symmetry group of these spacetimes, right? These gadgets are still manifolds whose tangent spaces carry a Minkowski metric, right?

86. **Zorq**
January 23, 2006

Bords:

No, I *am* asking about the Wilsonian effective action. You are right that, at 1-loop, this differs from the 1PI generating functional. The latter includes loops of light particles, whereas the former includes only loops of heavy particles (the stringy modes we are integrating out).

So, to extract the Wilsonian effective action, one needs to subtract off the contribution of loops of light particles (cut off, as necessary, at the String scale).

At tree-level, the Wilsonian and 1PI effective actions coincide. And, even there, one has nontrivial R^2 and R^4 corrections, due to the exchange of massive string states.

Note that the Wilsonian effective action determines, not just the vacuum, but also the *dynamics* of the light fields.

Lee claims (apparently) that, even in pure-gravity, the higher-order corrections to GR, encoded in the Wilsonian effective action, are different in different LQG vacua. (But not, for some reason, the values of G_N and the cosmological constant? Why don't those differ from vacuum to vacuum, as well?)

"You're right, string theory does (also) constrain the infinite number of
parameters in the wilsonian action …. "

OK, let me address that, so that, at least we can have two paradigms for how the infinite number of a-priori independent couplings get fixed (the first is a UV fixed point).

As just discussed, the low-energy theory has an infinite number of coupling constants (as any effective theory must). They do *not* approach a fixed point as we go to the UV. Instead, at the String scale, the effective field theory breaks down, and is replaced by the full String Theory.

For present purposes, the best way to describe that theory is to use Zwiebach's covariant closed string field theory. That is also a theory with an infinite number of coupling constants. But they are not independently adjustable. In fact, they are fixed by a new principle. The BV Master Equation recursively determines all of the couplings.

Now, before Lee starts complaining, Zwiebach's covariant closed string field theory is not manifestly background-independent. One needs to choose a nilpotent derivation, "Q", of the *-algebra, and a choice of Q singles out a background. Moreover, we only know how to solve the BV Master Equation in perturbation theory, so that, too, cannot be done in a background-independent fashion.

But, since Lee says that the effective action extracted from spin-foam theories isn't background-independent, either, I would say, "people in glass houses …"

87. **Lee Smolin**
January 23, 2006

To Boreds,

You " presume that if you try to perturbatively quantize GR around this
background (kappa-Minkowski that it will be non-renormalizable, and that there will be an infinite set of counter-terms." -To my knowledge its not known if this is true or not, perhaps someone else knows. Otherwise it would make a good research project. The reason it may be false is that some DSR theories incorporate maximum energies and momenta.

To Zorq, "Will the effective field theory, at least, be writable as a generally-covariant local functional of the metric?" Again, not known, but a good research project. One conjecture is it will be writable as a function of an energy dependent metric as in our work on rainbow gravity with Magueijo.

btw by uv finiteness we mean you sum over the labels on intermediate states and, rather then diverging as is the case in perturbaive QFT when the labels are momenta and the sums are unbounded, the sums are convergent. The fact that this doesn't coincide with textbook meanings in perturbative background dependent QFT is obvious, but again, our point is you have to learn a new kind of QFT.

If i can make a remark, the questions being raised are good ones, what is confusing is the adversarial tone. The discovery by Freidel and Livine that in 2+1 quantum gravity with matter there is an effective field theory on kappa-Minkowski is very recent. We don't know that this is the case in 3+1, although we know how to try to show it and it is in progress. We don't know whether there will be any version of effective field theory besides this one, which describes the excitations of a ground state with deformed poincare invariance. Since the fundamental theory is not formulated in spacetime and spacetime geometry is emergent, it is not obvious or known whether there will be a diffeo invariant effective field theory of the kind which is assumed to exist in formal treatments of the path integral. Why isn't it good-and exciting-when research indicates a new way of thinking about things that might succeed in a case where the older approaches have been unproductive?

To Juan, Thanks, indeed many of us see these as models which allow certain ideas and hypotheses to be precisely explored. We are open minded and if you have a new approach that is good and not bad. But at the same time, rather than rejecting a whole research program you might try to take the attitude of learning from its successes. You can even contribute to it while keeping an open mind as to its ultimate success. This is after all science.

Thanks,

Lee

88. **Luboš Motl**
January 23, 2006

No, I am not joking that I consider Nicolai et al. experts, and probably leading experts, in LQG and all aspects that have and have not been achieved by LQG. And if you ask me whether they understand it better than some older colleagues of them in the field, my answer is Probably yes.

89. **boreds**
January 23, 2006

Zorq—yes, it is clear what you mean now.

I just wanted to check you weren't equating the 1PI corrections with the wilsonian action.

90. **boreds**
January 23, 2006

sorry Lee didn't intend for that to sound adversarial. I would be interested if someone can discuss (as you suggest) effective field theories on kappa-minkowski; I don't have much intuition about what the differences would be.

91. **Who**
January 23, 2006

L, congratulations on your results of the past year and best wishes for what is in progress.

I did not expect to see you posting here at Peter's. Thanks for these helpful comments!

Be well.
Who

92. **Aaron Bergman**
January 23, 2006

Concerning the very nice work of Carlip and the potential ambiguities in 2+1 (did you know that the world revolved since then?)

You'll have to take that up with Carlip, then — I'm certainly not an expert on the subject. From p. 42 of this from '04:

> More than an "existence theorem," though, the (2+1)-dimensional models also provide a "nonuniqueness theorem": many approaches to the quantum theory are possible, and they are not all equivalent.

93. **Zorq**
January 23, 2006

Lee said:

"The fact that this doesn't coincide with textbook meanings in perturbative background dependent QFT…"

Aside from my last comment to Boreds, *nothing* I have said in any of my comments in this thread have relied on perturbation theory. The Wilsonian Renormalization Group is a nonperturbative concept.

"'Will the effective field theory, at least, be writable as a generally-covariant local functional of the metric?' Again, not known, but a good research project."

I'm surprised at this response. If the answer were "no," then it's clear that one ought to dismiss the whole approach for having violated general coordinate invariance (whatever protestations to the contrary).

"The discovery by Freidel and Livine that in 2+1 quantum gravity with matter there is an effective field theory on kappa-Minkowski is very recent. We don't know that this is the case in 3+1, although we know how to try to show it and it is in progress."

Integrating out gravity, to obtain an "effective theory" of just matter makes sense (barely) in 2+1 dimensions. In 3+1 dimensions, where gravity has local massless degrees of freedom, attempting to integrate out gravity (or any other massless degrees of freedom) is daft. The result will be a nonlocal mess.

Why would you expect anything nearly as "simple" as kappa-Minkowsi?

94. **Wolfgang**
January 23, 2006

Lee and L.,

it is encouraging that quantization of gravitation in 3D seems possible,
but as far as I know there are enough open questions (e.g. on the question if length is quantized) and it is not clear if different approaches give the same result (but the earth keeps revolving).

We also know some results in 4D for the strong-coupling limit and it would be nice to compare e.g. LQG with earlier results to see if different approaches give the same result.

95. **Who**
January 23, 2006

Wolfgang

> Lee and L.,
>
> it is encouraging that quantization of gravitation in 3D seems possible,…
>
> We also know some results in 4D …

Since you speculate about future results, the next development in 4D that I am expecting is reference [17] in hep-th/0512113
where they say

—quote Freidel-Livine—
We have shown how to write the 3d Feynman evaluations as expectation values of certain observables of a topological (abelian) theory. This (abelian) theory was then identified as a particular limit of the quantum gravity theory. This result suggests that **the Feynman evaluations of 4d QFT could be reformulated as expectation**

values of a 4d topological model. This is supported by the fact that 4d gravity becomes topological in the G → 0 limit [16]. This would be interpreted as the zeroth order of the spinfoam model for 4d quantum gravity [17]. …
—endquote—

Reference [17] is listed as “in preparation”. It seems to me that a lot depends on how that work goes. I am hopeful.

96. **Urs**
January 23, 2006

Aaron,

as far as I recall from last time we discussed this on Jacques’ blog, those LQG uniqueness theorems pertain to the kinematical Hilbert space only.

97. **Wolfgang**
January 23, 2006

Who,

I am looking forward to this paper. Just one more time I would like to make myself clear: I agree with Lubos that different formalisms should give the same results for the same physics; 3D gravity and strong-coupling gravity are cases where this can be checked (by the way I would be interested to also see how string theory compares!). So far different formalisms seem to give different results and some basic questions are un-answered.

98. **Chris W.**
January 24, 2006

This is a follow-up to Peter’s update; see the last section of gr-qc/0601095: **1.3.1 The UV problem in the background independent context** (p. 19-21).

99. **Thomas Larsson**
January 24, 2006

Lee Smolin said:
The inner product of Fock space depends on the background metric, hence this quantization will never arise in a background independent formalism.

This is a serious objection, of course. The absense of an invariant inner product was listed as one of the four most important open problems in the conclusion of hep-th/0504020. It now seems that this problem is solved as a by-product of eliminating the overcounting of states in the classical harmonic oscillator, reported in hep-th/0411028. However, this is work in progress and success is not guaranteed.

Note however a that diffeomorphism-invariant inner product can sometimes be defined without reference to a metric; for half-densities and, if the number of spacetime dimensions n is even, for n/2-forms. But this is not very relevant, of course.

100. **Arun**
January 24, 2006

Does kappa-Poincare instead of Poincare imply something for the Coleman-Mandula theorem?

A String Theorist Goes Into a Bar…

January 22, 2006
Categories: Uncategorized

Theorist Richard Szabo has had part of an evening he spent last week at the bar in a Bloomsbury hotel memorialized in today's Observer Magazine.

Comments

1. **island**
   January 22, 2006

   > Mind you. It was me who left with the barmaid.
   >
   > She was still trying to grasp… something…

2. **amused**
   January 22, 2006

   It's not any more amusing than YOU trying to lecture string theorists… 🙂

3. **woit**
   January 22, 2006

   Well, at least if I was in this situation I think I would have the good sense to be chatting with the barmaid, not the string theorist….

4. **secret milkshake**
   January 22, 2006

   there was a memorable moment with Feynman at a student party. Everybody was merry and drinking, having good time but some guy trying to impress his girfriend started explaining his "insight" to F in her presence. This went on and on for quite some time, and it was painfuly obvious that the guy did not have a clue what he was talking about. Trying to be delicate, F. waited until the guy finished and tried to change the subject. But the guy persisted: "So what do you think about this theory, professor?" "Well, if I were you, I would not think this way."

5. **Lubos Motl**
   January 22, 2006

   That's almost as entertaining as Brittany Shields sitting next to me in the airplane, looking at my notes about extended worldsheet supersymmetry, and starting to read The Elegant Universe by Brian Greene. 😉

6. **woit**
   January 22, 2006

   Lubos, your facts are as contorted on this topic as on all others. Brittany Shields? I assume you mean either Brittany Spears or my Princeton fellow student Brooke Shields (OK, she was an undergrad, barely overlapped with me, and I only saw her in the distance once or twice, but still….)

7. **CW**
   January 23, 2006

   It's *Britney* Spears, not that anyone reading this blog would care (right?).

8. **secret milkshake**
   January 23, 2006

   the Lumo's entertaining conversation went like this:

   "Uhm. Do you know I actually translated this book into Czech and it got published and all, and everybody likes me there now?"
   "Whatever"

9. **Chris Oakley**
   January 23, 2006

   I don't think that Britney Spears has done much on Superstring theory. All I could find on the internet were her notes on Semiconductors.
   Personally, though, I think that any enlightenment on "ultimate" theories is more likely to come from rappers than mainstream pop idols (MC Hawking, for example). They tend to have more of a philosophical bent.

10. **anon**
    January 23, 2006

    Britnany Spears

11. **Wolfgang**
    January 23, 2006

    Read all about the famous soccer player
    Brittany Shields.

12. **Wolfgang**
    January 23, 2006

Ooops: basketball not soccer …

13. **Chris Oakley**
January 23, 2006

    Funnily enough, Croton-on-Hudson is where my great-great-uncle Eugene Boissevain lived with Edna St. Vincent Millay, the American poetess. Small world. She also wrote songs, though I doubt that Britney Spears would have been envious of her level of success.

14. **John Gonsowski**
January 23, 2006

    Maybe Lubos was sitting between the two of them and there was some spooky action at a distance quantum entanglement thing going on?

15. **D R Lunsford**
January 23, 2006

    Yes, Lubos has his facts wrong as usual. Here is what really happened:

    http://www.airtoons.com/toons.php?toon=48

    -drl

16. **Thomas Larsson**
January 24, 2006

    Wolfgang, Brittany Shields **is** is soccer player.

Krauss and Susskind versus Horgan

January 22, 2006
Categories: Uncategorized

Lawrence Krauss and Leonard Susskind have a letter to the editor in this week's New York Times Sunday Book Review, complaining about John Horgan's NYT Book Review essay Einstein Has Left the Building of a couple weeks back. For some discussion of an earlier Susskind-Horgan exchange about another NYT piece of Horgan's, see here and here.

Krauss and Susskind write that "Horgan evidently sees the two of us as being on opposite sides of an imagined controversy", but that "the fact is that there is little of substance that we disagree about." They accuse Horgan of thinking "that reconciling quantum mechanics with Einstein's theory of gravity is a frivolous pursuit", whereas "both of us feel that reconciling the conflict between gravity and quantum mechanics is one of the deepest problems in modern physics." They comment about extra dimensions:

As for Horgan's bête noire of physics — higher dimensions, or what he refers to as "hyperspace theories" — he writes: "But pursuers of this 'theory of everything' have wandered into fantasy realms of higher dimensions with little or no empirical connection to our reality." That both of the present writers recognize that additional degrees of freedom of one sort or another are needed to characterize the physics of elementary particles may come as a surprise to Horgan. What the two of us may disagree about is what may be the likely physical and mathematical basis of this fact. But we both recognize that the mathematical spaces that we already deal with in describing the quantum theory of matter are in a certain sense more mathematically exotic than simple possible extra physical dimensions.

It seems to me that Krauss and Susskind are creating a straw man to attack here, not really dealing with Horgan's actual criticisms, the full text of which was:

Especially as represented by best sellers like "A Brief History of Time," by Stephen Hawking, and "The Elegant Universe," by Brian Greene, physics has also become increasingly esoteric, if not downright escapist. Many of physics' best and brightest are obsessed with fulfilling a task that occupied Einstein's latter years: finding a "unified theory" that fuses quantum physics and general relativity, which are as incompatible, conceptually and mathematically, as plaid and polka dots. But pursuers of this "theory of everything" have wandered into fantasy realms of higher dimensions with little or no empirical connection to our reality. In his new book "Hiding in the Mirror: The Mysterious Allure of Extra Dimensions, from Plato to String Theory and Beyond," the physicist Lawrence Krauss frets that his colleagues' belief in hyperspace theories in spite of the lack of evidence will encourage the insidious notion that science "is merely another kind of religion."

I don't see Horgan here criticizing the attempt to quantize gravity as "frivolous". His criticism of physicists as having "wandered into fantasy realms of higher dimensions with little or no empirical connection to our reality", is a justifiable one that deserves to be seriously addressed. Krauss and Susskind's comment that Horgan would be surprised that both of them think that new degrees of freedom will be needed to characterize elementary particle physics doesn't seem to have any basis in fact. Horgan isn't making broad claims that physicists shouldn't look for new degrees of freedom, he is very specifically referring to the use of extra space-time dimensions.

Krauss and Susskind at least implicitly take Horgan to task for referrring to such extra dimensions as "hyperspace". He may well have picked this up from Michio Kaku who wrote a book with this title. By the way, tonight Kaku will be appearing on the Art Bell "Coast to Coast" radio program, a program which is mostly concerned with UFOs and the like. If Krauss and Susskind want an example of the kind of theoretical physics research that Horgan is bothered by, they could check out this radio program.

Comments

1. **Chris W.**
   January 22, 2006

   > Many of physics' best and brightest are obsessed with fulfilling a task that occupied Einstein's latter years: finding a 'unified theory' that fuses quantum physics and general relativity, *which are as incompatible, conceptually and mathematically, as plaid and polka dots*.

   I do see in this a thinly veiled exasperation on Horgan's part with the belief—as expressed in research priorities—that such a unification is possible and worth pursuing. It seems to me that he has made fairly clear in his earlier writings that pursuit of this goal (and others, such as the nature and origin of consciousness) is likely to lead into a swamp of, as he calls it, ironic science.

   In a way he's right. We may become mired in just such a swamp. That risk has always existed when confronting difficult theoretical problems. Successfully facing the risk is what has distinguished great scientists from the much larger number of talented and competent practitioners who shy away from it, are defeated by it, or fall victim to it and lose their bearings.

2. **Lubos Motl**
   January 22, 2006

   Congratulations for having upgraded from Krauss's silly opinions about extra dimensions to Horgan's idiotic opinions about the whole modern science. What's the next step? Defending Dembski against those who argue that more than a "click" by an intelligent creator is needed to create and explain the world? 😉

3. **anon**
   January 22, 2006

   I find the letter by Sootkind and Krauss totally SLEAZY and DISHONEST. My opinion of Sootkind's tactics in debating and politics was already very low, but my opinion of Krauss is now lower.

   "That both of the present writers recognize that additional degrees of freedom of one sort or another are needed to characterize the physics of elementary particles may come as a surprise to Horgan."

   Really! Horgan would be surprised by that? Ever since Newton, physicists have used "additional degrees of freedom of one sort or another". Any person of normal intelligence will have no problem realizing that by "hyperspace", Horgan meant extra space-time dimensions. Only a genius String Theorist could misread that.

4. **mathjunkie**
   January 23, 2006

   Krauss and Susskind write that "Horgan evidently sees the two of us as being on opposite sides of an imagined controversy", but that "the fact is that there is little of substance that we disagree about."

   Does it mean that Krauss wanted to reverse his claim that String theory is a colossal failure?

5. **secret milkshake**

January 23, 2006

maybe somebody has gotten to him.

6. **brst**
January 23, 2006

I am amazed at the straw-man arguments Krauss(it was expected of Susskind) has used to attack Horgan. This is pathetic. The credibility of physicists, not just string theorists, is sinking like a stone. I think Krauss should retract his book since "there is little of substance that they disagree about" .

Who is going to be next? Et tu, Peter?

7. **Juan R.**
January 23, 2006

I do not read anything wrote by Horgan after i read his *End of Science* book.

Reading that Peter Woit has wrote above, it appears that Horgan is just maintaining the RELEVANT and PURELY SCIENTIFIC point that he exposed in his book years ago about the (non)verificability of the exoteric ideas of string theory (such as tiny extradimensions).

In his interview to Witten, Horgan claimed that both extradimensions and Planck scale strings were really non-verifiable and therefore outside of science (physics).

Witten's esxhasperation to lack of empirical evidence is well-reflected in the interview.

I would not use the word 'idiotic' other used in this blog, but i find particularly relevant Witten's belief (exposed in the book) that aliens already discovered GR, supersimmetry, QFT, and string theory but not necesarily in that order 🙂

—

Juan R.

Center for CANONICAL |SCIENCE)

8. **Dumb Biologist**
January 23, 2006

It is odd that one would bother to write an entire book about what "little of substance" there is to disagree about. I wonder what these extra degrees of freedom, if not extra spatial dimensions (compactified or otherwise) are supposed to be.

My understanding of Krauss' thesis was that there's some psychologically-bequiling quality posessed by "extra dimensions" that draws great minds into la-la land.

At any rate, is not testability the most pressing issue? Extra dimensions may be 100% real, but if it takes god-like technology to probe them, it seems throwing the bulk of theorists' efforts into theorizing about them is putting the cart before the horse, and apparently discounting the possibility that the vast gulf of energy scales between what we can currently test and the Planckian realm may hold some big surprises.

For folks whose research must be mathematically consistent, it's surprising to see this level of rhetorical incongruity.

9. **woit**
January 23, 2006

mathjunkie,

Krauss has said that his "colossal failure" quote was taken out of context.

D.B.,

Not sure exactly what "extra degrees of freedom" Krauss and Susskind are talking about. One possibility is supersymmetry which is sometimes thought of as involving "extra (fermionic) dimensions", another is gauge theory itself, which in some sense involves extra "internal" dimensions.

10. **Dumb Biologist**
January 23, 2006

OK. Thanks!

11. **Chris Oakley**
January 23, 2006

It could also be that the "degrees of freedom" refer to the amount of Superstrings in the theory. Krauss is saying that unless this parameter is zero then the theory will be a "colossal failure"; Susskind is saying otherwise. The so-called "disagreement" – massively exaggerated by Horton – is only over this one unimportant parameter.

12. **ksh95**
January 23, 2006

How's this for off topic.

Peter, what ever happened with the trackback issue?

13. **Quantoken**
January 23, 2006

Juan:

Interesting to know that Witten has the belief that aliens have figured it all out: "aliens already discovered GR, supersimmetry, QFT, and string theory" How did Witten know. Did he belong to religious sect that happen to believe so? Or how could he be so certain that aliens study super string theory at all? Maybe the aliens never ever started the super string theory, but worked on something else instead? How could Witten know it is super string theory that the aliens studied, not something else?

I propose to Witten that we utilize some of those giant radio telescope dishes, and send out a telegraph to the space asking about questions in super string theory. Some where in the universe there must be advanced alien civilizations capable of intercepting our messages and they may be kind enough to response with some easy answers.

Or maybe we get no response at all, in which case the anthropic principle answer would be that these aliens, who are so advanced they figured everything out, figured out that those earthlines must be too stupid and too primitive that they are still wasting their time on super string research after two decades, and which we aliens know a long time ago is a deadend.

14. **woit**
January 23, 2006

Quantoken and Juan,

Stop with the nonsense about Witten and aliens. This is just a reference to Witten's speculating about the possibility of various ideas about theoretical physics being discovered in a different order.

ksh95,

Still don't know what's going on about this. Actually I couldn't access trackbacks at all there for the few days, so I thought something was up. But it appears to have been a problem caused by some cookies my browser was using, deleting them returned things to normal.

A member of the arxiv advisory board tells me someone from the arXiv has promised to contact me this week and tell me what is going on. Will let you know what I hear.

15. **Lubos Motl**
January 23, 2006

Horgan's book definitely tried to picture Edward Witten as a person who is confused by ideas about aliens.

If there are two possible explanations – Edward Witten being confused or John Horgan (with all people who endorse him) being a complete idiot, which explanation do you think is more likely?

http://motls.blogspot.com/2006/01/when-krauss-and-susskind-are-right.html

16. **D R Lunsford**
January 23, 2006

I wonder if the aliens have confused ideas about Witten? They seem to think the smart people live in trailer parks.

-drl

17. **plato**
January 23, 2006

> General use of hyperspace as a term for such domains dates back at least to the 1940s; it is found in Asimov's epochal Foundation novels and probably predates those.

Does anyone know "for sure" where this term came from?

18. **Joe Zhou**
January 23, 2006

Slightly off topic. In an interesting paper (hep-th/0601162) Delia Schwartz-Perlov and Alexander Vilenkin conclude that the volume distribution for the cosmological constant is nonflat near the observed value, which seems to invalidate previous calculations based on the anthropic principle.

19. **ks**
January 24, 2006

Idiots vs fanatics. I guess I'm a little in favour for idiots. It's simply healthier to be too stupid to not believe in things that don't exist. The same thing with sensitivity and paranormal awareness. It's better to be a little too dumb to feel spooky phenomena.

20. **mathjunkie**
January 24, 2006

Woit said:
Krauss has said that his "colossal failure" quote was taken out of context.

Sorry, Peter, my English is not very good as it is not my mother tongue. So, does it mean that Krauss was referring to something else that is a "colossal failure" but not to string theory?

21. **Chris Oakley**
January 24, 2006

Maybe he decided to order a latte one day at the physics department canteen instead of a capuccino. Not liking it as much, he then decided that this move was a "colossal failure".

22. **Simon**
January 24, 2006

Actually, in string theory there is not such a big difference between degrees of freedom and space-time dimensions. Quantizing in Minkowski space for example, the space-time coordinates appear as fields in the worldsheet theory. In compactifications to 4 dimensions, what is required for consistency is not really to add 6 dimensions, but to add a conformal field theory on the worldsheet with the right central charge. Sometimes these fields can be interpreted as the coordinates of a compact manifold, and other times not.

Two examples of the latter are general Landau-Ginzburg models, and some interesting recent discoveries of nongeonetric flux compactifications: hep-th/0508133. It's also important that in CFT moduli space, one can moove smoothly between geometric and nongeometric backgrounds. So in the context of compactification (as opposed to say intersecting brane models) it really is better to think in terms of degrees of freedom of a conformal field theory than extra dimensions.

This equivalence of degrees of freedom and extra compact dimensions is not unique to string theory. This is the essence of Kaluza–Klein theory – one can study a K-K expansion of any field theory on R^4\times S^1 for example and find a field theory just on R^4, in this case with an infinite number of fields.

Anyhow, we know for sure (from unitarity bounds) that there are new degrees of freedom to be found even at the weak scale – most likely a Higgs boson. The same sorts of arguments, applied to graviton scattering, require that new gravitational degrees of freedom appear at (or before) the Planck scale. I guess Susskind and Krauss are just saying that they disagree about what these might be – but not about whether they exist, and not about whether they will seem exotic (as they probably will) if they are ever discovered.

23. **Steve Myers**
January 24, 2006

Maybe I'm being too general here for a group of theoretical physicists, but on the question of dimensions: I've mentioned before that you can always get a symmetry by adding dimensions. Also, suppose you are writing an algorithm to generate (compute) the world, you might use 100 dimensions (degrees of freedom) but the result, the output should only have 4. In real time targeting of tanks, for example, with the input signals from a sidelooking radar, you begin with 12 dimensions and compact them to 7 so you can do the calculations in real time, but the missile you fire and the tank you hit (?) exist in a world of 4 dimensions. In an even more practical example: the number of degrees of freedom of a diaper making machine are greater than the number of atoms in the universe but the product produced holds 3-D excrement. One last thing: the possible worlds of many dimensions and "the landscape" have the odor of Anselm's ontological proof. Is physics entering another Dark Age?

24. **D R Lunsford**
January 24, 2006

Steve – if you have a specific, physical reason for introducing more dimensions, and a specific, physical ontology for them, then you are inside the boundary of science. When you introduce them to make an already wacky idea somewhat credible, you are close to the edge. When you introduce them in a way that had been discredited 30 years earlier by one of the great physicists, in order to make a wacky idea credible, and then you spend the next 25 years beating an unworkable strawman, you are over the fence into non-science.

-drl

25. **Dumb Biologist**
January 24, 2006

Help for the ignorant, please:

"Degree of freedom" here seems to encompass just about any quantifiable or qualifiable property of a particle; what it both can do and be. Is that correct? I guess I have a primitive understanding of the term, more like "what's the particle's position, momentum, spin" and so forth. Would, say, the ability for a lepton to be a muon or an electron be considered a "degree of freedom" for a lepton? I thought a string theorist would answer that question by saying "because the string can vibrate at a certain frequency, wrap around compactified dimensions of a certain shape, and is so wrapped at a certain tension, it looks like a particular kind of lepton". I.E., the ability to move around in a particular way gives the string its observed properties, and the extra dimensions provide the "degrees of freedom" to so move.

26. **Simon**
January 24, 2006

D.B.

Those are good questions. What is meant by degrees of freedom depends to some extent on the context, i.e. classical mechanics, field theory or string theory. Perhaps the following will help:

Classical particle mechanics:
– Here things are as you say – each component of position or momentum is a degree of freedom, i.e. the number of d.o.f. is how many numbers you have to give to specify the state of the system.

Nonrelativistic quantum (particle) mechanics:
– Here even a single particle is described by a wavefunction, which requires an infinitely many numbers to specify. So we often think of the number of d.o.f. as the number of generators of the Heisenberg algebra (esssentially positions and momenta). But very few discussions in particle physics take place in this context.

Classical field theory:
– There are classical systems with an infinite number of d.o.f. as defined above, e.g. the electric field. Formally, one can understand a field as a harmonic oscillator at each point in space–time. Here we think of a d.o.f. as an component of a field – see the next section for comments.

Quantum Field Theory:
– Here a degree of freedom refers to a component of a field. But there are many subtleties. Some of these are as follows:
– Gauge Theories – here some of the d.o.f. (i.e. some of the ways the field can change) are local symmetries of the action – thus the description of the system in terms of gauge fields is redundant, and there are fewer d.o.f. than there appear.
– On shell/Off shell – The equations of motion of the theory are extra constraints on the fields – further reducing the number of independent components. We call the ones left "on-shell d.o.f". The full set are called the "off-shell d.o.f".
– Propagating? – Sometimes (particularly in supersymmetric theories) it's useful to have fields whose equations of motion are algebraic, i.e. not differential equations. So you don't have to put in initial conditions to solve them. They can be solved and the solution substituted in to the action to eliminate them if we want. These are called "non-propagating d.o.f." A non-supersymmetric example of this is general relativity in 2 dimensions.

String Theory:
One caveat: In an important sense it isn't yet known what the fundamental d.o.f. of string theory really are. If we study perturbation theory about a fixed background, we get a quantum field theory with an infinite number of fields (although a finite number with masses less than the Planck scale) – corresponding as you say, to the different oscillations and winding modes of the string. But we know that the appearance of an infinite number of d.o.f. is misleading from the AdS/CFT correspondence. There, string theory in AdS is seen to be equivalent to a field theory with just a finite number of fields (N=4 SYM). Moreover, prior to the discovery of the AdS/CFT correspondence there were widely held expectations that something like this would happen – going under the general name "holography".

27. **Simon**
January 24, 2006

One last comment: the reason why an extra dimension (a few extra degrees of freedom) can be replaced with an infinite number of fields in a K-K expansion, is that those infinitely many fields also come with infinitely many gauge redundancies. More details are in hep-th/9410046.

28. **Dumb Biologist**
January 24, 2006

Wowza! OK, that will take some digestion, but I sincerely appreciate the effort to answer the question, and I guarantee I will put at least commensurate effort into understanding it (to the limits of my intellect, of course!).

Thanks!

29. **Simon**
January 24, 2006

D.B.
No problem. The posts above do assume some technical knowledge, and leave a lot unsaid, so don't worry if they're confusing – you can always ask more more questions!

30. **Tony Jackson**
January 24, 2006

I absolutely agree with you about Kaku. Here's more from Art Bell:

"Kaku also forecast into the far distant future when our universe will be dying out. At such a time, a Type 3 or 4 civilization (capable of manipulating huge amounts of energy) might construct a massive machine that could make space and time unstable. With an atom smasher the size of a solar system, he hypothesized it might open up a bubble ten light years across, through which our civilization could escape into another universe."

This is just a bad Star Trek episode. The man is an embarrassment to honest physicists. I stopped taking him seriously when he made a fool of himself campaigning against the launch of the magnificent Cassini probe.

31. **J.F. Moore**
January 24, 2006

Very well put post. It makes me wince every time I see someone who should know better make a logically weak argument against Horgan (the worst being ad hominem). It seems to have happened a lot in the last decade.

32. **Dumb Biologist**
January 24, 2006

Awright…just blast this post, Dr. Woit, if you feel I'm being an annoying newb here. I'll understand completely.

To Simon (or whoever cares to answer): This AdS/CFT thing. I read Maldacena's article in SciAm, and while he didn't mention "conformal field theories", it was pretty clear that's what he was describing. Hence, I know probably about as much as my brain can handle about "anti-de Sitter space". Clearly, doing physics in this kind of space has had some exciting consequences.

Now I know some flat maps of the Earth are called "conformal" because, while distorted in some ways, they preserve some important features of an actual globe, and can even be more convenient in some ways (a straight line on a Mercator projection always has a constant heading, for instance, though it's harder to find the shortest distance between two points than it is on a globe). That seems to be roughly analagous to what's going in in modeling physics in one less dimension than usual. Apparently, out of this has come a rigorous demonstration of the idea that all you need to know about a volume can be represented on its surface, and this makes doing quantum gravity easier by turning it into a field theory something like QCD. You can talk about gravity in terms of stringly lines of gluons, or something like that.

Okaaay…So, not surprisingly, I'm at about my limit even attempting to grasp most of this, but here goes: What happens to all the compatified dimensions when all of space looks like a flattened image of negatively-curved hyperbolic solid?

33. **Simon**
January 25, 2006

D.B.
Ok, more good questions. Conformal symmetry is a very interesting subject, but it's obscuring the important issues here. First, there are (at least) two conformal field theories in AdS/CFT. One is the theory on the worldsheet of the string in AdS (with some suitable extra dimensions). The other (eponymous) CFT is the `dual' theory in some sense living on the boundary of AdS. The first is a 2d theory, the second is 4d.

Most importantly: whereas in the 2d theory, conformal symmetry is required for consistency, it is not so required in the 4d theory. What is meant by this is that theories without conformal symmetry are also (in a slightly different way) dual to string theories in AdS. This result which was expected because of a very general argument of 't Hooft in the 70s, has recently been fleshed out in more detail. And it lends hope to the idea that non-conformal field theories like QCD can be reformulated as string theories. It is expected that this duality would relate strongly coupled regions of one theory to weakly coupled regions of the other, so the eventual goal is to access the low energy bound states of QCD (which at present cannot be derived analytically because of the strength of the coupling) through the low energy excitations of string theory.

I haven't said much about what conformal symmetry actually is – you can think of it as a generalization of `scale invariance' – i.e. rescaling all the lengths in the system by a same amount, although it this isn't really quite the same thing. Remembering that shorter lengths are equivalent to higher energies, and vice versa, you can see that a theory like QCD (or for that matter the rest of the standard model) where the strength of the coupling depends on the energy, cannot have conformal symmetry. (In that case it's interesting that the classical QCD does in fact possess scale invariance, but it is broken by quantization).

34. **Urs**
January 25, 2006

> you can think of it as a generalization of `scale invariance' – i.e. rescaling all the lengths in the system by a same amount, although it this isn't really quite the same thing.

It is 'local' scale invariance, meaning invariance under rescalings that differ from point to point. Equivalently. it means that only angles have an invariant meaning, not lengths.

35. **Simon**
January 25, 2006

There are a few things here one might want to have separate terms for. There are local scale transformations that act directly on the metric – these are called Weyl transformations. Changes of coordinates that happen to result in rescalings of the metric are conformal transformations. But surprisingly there are more ways to do this than you might think. You can rescale the coordinates – so called dilatations. But there are also `special conformal' transformations, which roughly, invert the coordinates, translate them, and then invert them again.

The special conformal transformations have some interesting consequences. They can move any point in space-time out to infinity. As a result, in a conformal field theory it is not possible to localize interactions in a conformally invariant way. This is why many people say that it makes no sense to calculate scattering amplitudes in CFTs, and why people should just calculate correlation functions.

36. **D R Lunsford**
January 25, 2006

Simon said

As a result, in a conformal field theory it is not possible to localize interactions in a conformally invariant way. This is why many people say that it makes no sense to calculate scattering amplitudes in CFTs, and why people should just calculate correlation functions.

This doesn't sound right. It's certainly not right in Weyl conformal geometry. In fact the latter comes about precisely from thoroughly localizing metricity. Weyl therefore called it "pure infinitesimal geometry".

-drl

Discover Interview Online

January 24, 2006
Categories: Uncategorized

There's an extended version of the interview of me by Susan Kruglinski in the February issue of Discover magazine that is now available, for free, on-line.

Before anybody starts yelling about AdS/CFT or topological strings when they read the headline "No one has a plausible idea about how string theory can explain anything", I'll just point out that, yes, it's certainly plausible that some day string theory will explain something about QCD, and it already has explained some things in mathematics. The headline is a summary of some things I say in the interview, and in context it should have been clear I was talking about the use of string theory to predict anything not already predicted by the standard model.

Update: Harvard string theorist Lubos Motl has posted his commentary on the Discover article. If you read the comment section there, keep in mind that he is deleting comments from anyone who disagrees with him. I encourage anyone new to the current controversy over string theory to read what I have to say, read what Lubos has to say, and carefully look into the scientific issues involved to make your own judgement about what is going on here.

Comments

1. **anonymous**
   January 24, 2006

   'In mathematics there is much more of a culture where people spread out and devote their lives to thinking hard about something that interests them. … when the problems are very hard and no one knows what to do, I think people need to be willing to dig in and spend years thinking about something different than what other people are thinking. And there really isn't the kind of institutional support within the physics community for this kind of behavior, whereas there is in mathematics.'

   There is probably too much indoctrination in favour of strings to allow this. Physicists know it is highly unlikely that they could change the mainstream, so they prefer to stick with it. The fear of being blacklisted for non-standard ideas is quite real at the lower levels in physics. Those who rise to the higher levels usually have more mainstream ideas.

2. **Gravity**
   January 24, 2006

   Peter, i enjoyed the interview. i hope it attracts some new readers. i'm a layman, but like your log anyway. i learned about the landscape through it, and about interesting links between math and physics. certainly it's one of the more exciting sites on the web.

3. **Quantoken**
   January 24, 2006

   Peter says: "I was talking about the use of string theory to predict anything **not already predicted** by the standard model."

   Peter I guess you have to further correct your statement. You sounded as if you think string theory already predicts everything that SM predicts, it just does not go beyong SM. That's far from being correct, not only has string theory not predicted anything beyond SM, it does not predict any thing that SM predict, either. It simply has **not** made any prediction whatsoever. Zero, Nada.

   It's important to point that out because we keep hearing string theorists make claims like "SM can be derived from string theory" or "string theory leads to GR" or so. Those are flat out lies. You can't get SM starting from string theory. You can't derive the Einstein Equation of GR from string theory. You can't calculate any of the coupling constant or anything from string theory. Zero, Nada.

   I also made some comment disputing the notion that just because string theory leads to very rich mathematical structures from a very few assumptions that it necessarily has to have something to do with the nature. My comments can be found on Lubos's blog.

   Quantoken

4. **Wolfgang**
   January 24, 2006

   Peter,

   what Yoga pose did you practice when they took the picture 😎

5. **Lubos Motl**
   January 24, 2006

   http://motls.blogspot.com/2006/01/fall-of-discover-magazine.html

6. **Dumb Biologist**
   January 24, 2006

   Not a bad interview for Discover at all, though few new revelations for the readers of you're blog, I suspect. I look forward to your book for meatier exposition.

   The photo…it's not *bad*, it's just I don't get it. As we don't normally find the math prof sitting cross-legged beneath the chalkboard, questions about *why* he might be in that position naturally arise. Perhaps he's meditating?

   Anyway, I can't wait to see the Letters section of the subsequent issue! And, thanks for sharing the link.

7. **Dumb Biologist**
   January 24, 2006

   Good grief! Remind me to never click on one of those trackback things again…I practically had an anxiety attack it was so laced with invective….

8. **anon**
January 24, 2006

[50 Most Loathsome People in America, 2005] If they only knew about Lubos

9. **A. nonymous**
January 24, 2006

Peter, have you noticed that Lubos is your greatest ally in your fight to lower people's regard for string theory? I would imagine that a lot of people who read the article will look at your blog, then look at Lubos's review of your interview, and draw the obvious conclusion.

10. **Dumb Biologist**
January 24, 2006

I'm not qualified to draw any conclusion except that level of rancor borders on frightening. I'm glad I've chosen to stay anonymous. Maybe it makes me a coward, but who needs enemies like that.

11. **Christine**
January 24, 2006

> **Even string theorists admit that it is not really a theory. What is it?**
>
> W: The best way to say it is what people have now is really an approximation to a theory. The kinds of equations that they have now are the kinds of equations you would get in an approximation scheme to some underlying theory, but nobody knows what the underlying theory is.

Merriam-Webster Dictionary on "approximation":

> something that is approximate; especially : a mathematical quantity that is close in value to but not the same as a desired quantity

or, on "approximate":

> nearly correct or exact (*"an approximate solution"*)
> *thesaurus*: being such only when compared to something else

These definitions in purely logical terms lead one to conclude that string theory cannot be an approximation scheme because the underlying theory to be compared with (viz., quantum gravity) is *unknown*. So in order to be an approximation scheme [*to something known*], string theory should be compared with a well established theory (or experimental data) in the regime where it applies (e.g., it should unambiguously reproduce — or approximate — the classical limit, SM particles, etc, that is, all fundamental observed phenomena that it is supposed to encompass or describe). But of course, things cannot stop there.

So, one is lead to conclude that such an "approximation scheme" should be somewhat viewed as an euphemism to "prediction" in order that things make sense. String theory must not only reproduce the physical properties of the fundamental constituents of nature to a reasonable precision [*if is is to be seen as truthful approximation scheme to well known facts*], but also *predict* new phenomena or indirect effects related to the Planck scale physics [*if it is to be seen as a truthful quantum gravity theory at some stage*]. Only then string theory can be more clearly evaluated.

12. **Sakura-chan**
January 24, 2006

Maybe Lubo has bipolar disorder?

13. **secret milkshake**
January 25, 2006

he does not have emotions, but he has urges.

14. **Aaron Bergman**
January 25, 2006

have you noticed that Lubos is your greatest ally in your fight to lower people's regard for string theory?

It has been noticed.

15. **Tony Smith**
January 25, 2006

anonymous said;
"… The fear of being blacklisted for non-standard ideas is quite real at the lower levels in physics. …".

DB said, about Lubos Motl's trackback blog link:
"… Good grief! Remind me to never click on one of those trackback things again … I practically had an anxiety attack it was so laced with invective … that level of rancor borders on frightening. I'm glad I've chosen to stay anonymous. Maybe it makes me a coward, but who needs enemies like that. …".

Those two comments (from people whose earlier comments have shown them to be thoughtful, reasonable, and intelligent) paint a very sad picture of a field that may be entering a Dark Age.

Are there any heroes of physics who might react to such "invective", "blacklist[ing]", and "rancor"
in a way similar to the way that the Boondocks MLK holiday TV episode showed King reacting to some negative aspects of today's world ?
http://www.tv.com/boondocks/return-of-the-king/episode/614610/summary.html
I wish there were.
Even if not, as Huey Freeman said, "It's fun to dream" that it might happen.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

16. **robert**

January 25, 2006

Sad to say, the author of Cosmic Variance's 'Best physics paper ever', Isacc Newton, was quite as nasty as Professor Motl in his interactions with his contemporaries – even his 'standing on shoulders of giants' comment was a scarcely veiled insult to the diminutive and deformed Hooke. So Lubos is merely carrying on in a great tradition, albeit one that does not show physics and physicists in a very positive light.

17. **Christine**
January 25, 2006

I have a personal theory on Lubos Motl but I will never discuss it.

Science is obviously made by people, and people have a large spectrum of behaviours.

18. **anonymous**
January 25, 2006

Christine, see film showing Motl's hair growing as he attempts smile: http://www.kolej.mff.cuni.cz/~lmotm275/morf/lhm.gif
May it be best for all that Motl remains angry 😉

19. **Kasper Olsen**
January 25, 2006

Maybe we should try to comment on what Peter was actually saying about string theory – and not so much if we like Lubos or not? 🙂

(I'll have to read the interview with Peter first, then I'll post my comments).

Best, Kasper

20. **mathjunkie**
January 25, 2006

Don't know if Ed Witten will read this comment page and the message below:

Hi Ed Witten,

Please make a verifiable prediction from string theory to prove that the anti-stringy people are wrong. Then, Peter's famous "Not Even Wrong" webpage will have the heading changed to "I was wrong".

mathjunkie

HeHe…

21. **Urs**
January 25, 2006

> Please make a verifiable prediction from string theory

That's easy. Weakly coupled perturbative string theory predicts that there is an energy scale at which the massive excitation modes of the string are visible.

If true, this is verifiable.

What is not so easy is to *falsify* this prediction.
That is because the statement is of the form

"there is at least one x (in the set of energy scales) such that …"

Not observing any stringy modes below any fixed energy scale E therefore does not suffice to falsify this.

Note that even though this prediction is verifiable, it might well be wrong. It could be that strings exist but are not weakly coupled, so that perturbative string theory does not apply. It could also be that there are no strings at all.

But if you see any higher string excitation, then you have verified this prediction of perturbative string theory.

22. **anonymous**
January 25, 2006

Vague non-quantitative 'predictions' are not scientific. Can you predict the exact energy of the massive excitation modes and therefore prove it is within range of experimental measurement?

23. **Nigel**
January 25, 2006

"What is not so easy is to falsify this prediction." – Urs.

Does this place string theory at the level of astrology?

24. **a**
January 25, 2006

In my opinion the situation is:

1) Quantum gravity likely gives undetectably small effects in any relatistic experiment.
2) Despite this, theorists studied quantum gravity hoping that it could lead to a unique theory of everything, able of predicting something.
3) Strings seem to give a unique theory, but in 11dimensions.

4) In string models the complicated physics that we see at low energies mostly comes thanks to a complicated enough higher dimensional set-up, rather than from theory.

I think that 4) is the reason that prevented strings to give so far results relevant for physics. It is often said that to progress we need “a deeper mathematical understanding of string theory”, but the real problem seems harder: we do not know the geography of extra dimensions.

(By the way: I posted a similar comment on Lubos blog, and it was deleted. Even if it is not possible to constructively discuss about physics, it remains my favourite trash show).

25. **Urs**
January 25, 2006

> Does this place string theory at the level of astrology?

No. It is common for scientific theories to make predictions that are verifiable but not falsifiable.

The standard model demands that there is a Higgs field, but does not predict at which energy it will be found.

Not that there are no problems with string theory. But blanket anti-hype like “is not scientific”, “does not predict anything” are, in their crudeness, not any different from the original pro-hype like “will predict particle masses in a couple of months”.

You don’t have to like string theory. But if you reject it, you might want to do so for the correct reason, not just because you read the latest tabloid headlines.

26. **Urs**
January 25, 2006

> It is often said that to progress we need “a deeper mathematical understanding of string theory”, but the real problem seems harder: we do not know the geography of extra dimensions.

But that’s the point. As long as the non-perturbative formulation of the theory is not fully understood it remains inconceivable how vacuum selection should be understood properly.

27. **a**
January 25, 2006

To Urs:
ok, one can try to study if non perturbative effects reduce the possible number of vacua from 10^500 to something more reasonable, or maybe even one. But this looks like hoping in a miracle, while the alernative view looks (at least to me) the true one: many vacua are possible, no vacuum selection mechanism exists, we are in one of them for no special reason.

28. **Urs**
January 25, 2006

a,

right, I agree. I just think that when, while doing research, you run into something that is getting out of control, you might want to pause and try to understand what is really going on at the fundamental level in the hope to get back in control again, instead of starting to wildly speculate and play around with a network of conjectures.

29. **Nigel**
January 25, 2006

“It is common for scientific theories to make predictions that are verifiable but not falsifiable.

“The standard model demands that there is a Higgs field, but does not predict at which energy it will be found.” – Urs

But the Standard Model is only accepted for predicting many confirmed details about measurable nuclear interactions!

Kepler worked as an astrologer to live, because his alternative physics wasn’t popular, and astrology was in his day a highly mathematical enterprise (epicycles, etc.). String theory and astrology are similar in being unscientific uses of mathematics.

“You don’t have to like string theory. But if you reject it, you might want to do so for the correct reason, not just because you read the latest tabloid headlines.” – Urs

If it can’t be falsified, nobody can disprove it. The real problem with string speculation is arrogance. With the mainstream embedded in untestable speculation, the subject is unhealthy. It would be better if there was more effort given to alternatives.

30. **Urs**
January 25, 2006

In the discussions here the target always tends to be moving.

If the statement is that

“People should look at alternative approaches.”

I have no problem with that.

If the statement however is

“String Theory is Astrology.”

then that’s nonsense.

String theory may be better than astrology and still not suit your needs. So go ahead and work on something else.

31. **Haelfix**
January 25, 2006

Incidentally, to outsiders it seems like there are a lot of venemous attacks going on in quantum gravity.

Be assured, the statement holds true in a number of fields in physics. If you go to a lot of astrophysics and phenomonology meetings invariably you end up seeing the same childish name calling and intellectual chest pounding (at least, on occassion).

But many of us have learned that thats a good thing in the end, some people just need that sort of fire and controversy to keep working and motivated to keep an edge. Its also vital to remain skeptical of everything and anything (that includes above all, yourself) so controversy really serves its purpose in physics.

People get used to it, and then its all fun and games thereafter.

32. **Urs**
January 25, 2006

> controversy really serves its purpose in physics

Indeed. I think it is a good thing to have a critical discussion of the topics that are being critically discussed here (which usually is string theory and LQG, whether or not this is intended). I wish sometimes the dicussion would consistently be more fruitful, though. Let's get away from that yellow press attitude towards research.

33. **amused**
January 25, 2006

The following comment is seeking refugee status here after having been deleted on Lubos' blog (like a's above). It's a bit less polite than what I would normally post, but should be seen as an attempt to communicate to Lubos in his native language. Since it was in response to his "review" of Peter's Discover interview, where he once again derided Peter's credentials, hopefully it is semi-on topic here.
……………………..

Lubos, it is disingenius of you to write that someones scientific credentials can be evaluated just by looking up their publications and citations. All of us working in physics know that it takes very little to publish a paper in a supposedly respectable journal these days. It can be easily done by following the general prescription of "monkey see, monkey do", and some people have made careers out of this. Others have made careers out of riding on the coattails of famous senior colleagues, and it is quite amusing to see how some of these young "hotshots" go dead researchwise when put in a position of having to develop their own independent research program after getting a faculty position at an illustrious institution (not looking at anyone in particular, Lubos).
Citation numbers often have limited relevance as well, especially in "hot" subareas such as string theory where people are striving to churn out as many papers as possible and artificially inflating each others citation counts in the process.

To get a meaningful evaluation, the criteria need to be refined. The only physics journal that is non-trivial to publish in is Phys.Rev.Lett., so one possibility is to count the number of single-author publications a person has in there. How would you fare under this criterion, Lubos? Would you do better than Peter Woit, whose credentials you are so fond of disparaging?

(That was of course a rhetorical question. For those who can't be bothered to look it up, Lubos has 0 papers in PRL, while Peter published a paper there on his own as a grad student.)

34. **Fabien Besnard**
January 25, 2006

Urs,

"No. It is common for scientific theories to make predictions that are verifiable but not falsifiable."

True. But the converse is wrong, and that's the problem. Making verifiable but unfalsifiable predictions does not grant scientificity to a theory. You carefully omit to say this, but if Mathjunkie had said :
"Hi Ed Witten,

Please make a falsifiable prediction from string theory to prove that the anti-stringy people are wrong. Then, Peter's famous "Not Even Wrong" webpage will have the heading changed to "I was wrong"."

That would have been a far more interesting challenge.

35. **woit**
January 25, 2006

Urs,

You neglect to mention that, besides massive excitation modes, perturbative string theory predicts a host of things that are clearly wrong (exact supersymmetry, massless fields, etc.) and would falsify the theory if you took these as serious "predictions". On alternate days you argue that string theory really is a predictive science and that its predictions shouldn't be believed (because we don't yet know the full theory).

36. **Quantoken**
January 25, 2006

Urs:

It's an insult to astrologists for you to compare string theory to astrology. Astrology is certainly not science, neither is string theory.

But Astrology is still way much better than string theories. Astrology at least make observations of the natural world and try to make connection with the reality world: They observe lunar phases and other astronomic phenomenas. Actually the ancient science of astronomy was developed on top of astrology. Today's astronomers still have to rely on detailed written recordings left by ancient astrologists to get data on astronomical phenomenas that happen in the past, like supernovae explosions some thousands of years ago. **In that sense, astrologists had made invaluable contribution to science**, by making those detailed observations, and left detailed recordings of what they saw. That's experimental science in its honesty!

What has string theory contributed to science? Nothing. They make no connection to the reality world whatsoever. They do not make predictions. Astrology at least try to make some predictions. The net contribution of super string theory is actually negative, by draining resources away from actually useful scientific researches in other fields, and by polluting the public's perception of what science research is really about.

Lubos is poorly trained in basic physics instinctions and is not qualified to make judgements of science in general. See [this post of his, appraising a stupid research idea](), and my response below.

Quantoken

37. **Urs**
January 25, 2006

Actually, I was trying to be careful with always inserting the right qualifications. There is perturbative ST with some non-perturbative effects included by hand, and it does predict (in the weakly coupled regime) stringy effects. If there should be no viable phenomenological vacuum for this theory, then it is wrong. But I don't know if none such vacuum exists.

We all need to be careful with making these statements. I could turn your accusation around and note that you oscillate between "predicts nothing" and "has been falsified". Somewhere in between there is a truth.

38. **Dombono**
January 25, 2006

But what if we do a perturbation about a flux compactification?

39. **woit**
January 25, 2006

A few remarks about Lubos's posting:

Ever since I've started publicly criticizing string theory, I've learned a lot about how academics behave when the facts are against them. The tactics employed are:

1. The ad hominem attack. Attack your opponent's credibility and right to say anything on the subject. This is Lubos's main tactic today. If you believe him that I shouldn't be listened to, there's not much point in me saying anything to defend my credibility. I'll just point out that by now he and I have both written hundreds of pages about this controversy on our respective weblogs. Read large chunks of both of them and make up your own mind who is more credible.

In Lubos's comment section there is a commenter "Hmm" who is also making ad hominem attacks on me and my credibility. Funny that whoever he/she is, they do this anonymously so that one has no way of knowing who they are, and deciding whether you should trust their judgements on this issue. Whatever you think of Lubos, at least he doesn't engage in the sleazy behavior of making anonymous personal attacks.

2. The straw man attack. Misrepresent or even misquote your opponent's words, then attack them as stupid and ignorant for saying something inaccurate. Lubos doesn't much bother with this one today, but he's normally quite fond of it, as are many other string theorists and string theory partisans that I have debated with.

3. Censorship. When you can get away with it, by any tactic available, keep people who disagree with you from being heard. Lubos is now doing that with his comment section, anonymous string theorist referees stopped Cambridge University Press from publishing my book, someone (presumably Jacques Distler) is using their influence with the arXiv to censor links to my weblog postings there. The internet makes this tactic less effective as it gives people who are censored various means to get their views heard anyway.

4. Intimidation. I've heard from many, many physicists that they agree with many of my criticisms of string theory, but are afraid to be publicly associated with them because of repercussions for their career. The over-the-top personal attacks from Lubos don't help his credibility much, but they do frighten quite a few people into keeping quiet so that they don't get subjected to this kind of thing.

The fact that the string theory community as a whole seems to have no problem with Lubos's behavior adds greatly to this intimidating effect. It makes many physicists feel that if they criticized string theory they would both be the target of vicious personal attacks by Lubos, and, once targeted, behind-the-scenes retribution by his colleagues in terms of things like evaluation of their grant proposals, refereeing of their papers, invitations to conferences, and hiring of their students.

About the only string theorist around that I've noticed having anything critical to say about Lubos's behavior is Aaron Bergman, who often comments here. The fact that no one else is willing to do so seems to me a deeply shameful commentary on the state of this field.

40. **Urs**
January 25, 2006

> But what if we do a perturbation about a flux compactification?

There is no known flux compactification which is phenomenologically viable or well enough understood to check if it is phenomenologically viable, or is there?

41. **woit**
January 25, 2006

Dombono,

Urs kind of refers to this. One problem is that if you try and fully stabilize moduli using a flux compactification, you have to introduce non-perturbative objects like branes, and it is difficult to be sure that your perturbative expansion is reliable. It's also true that even making optimistic assumptions about this, no one has come up with such a thing that is at all realistic or where you can calculate things and compare to the standard model.

Urs,

Yes, at times I both say "predicts nothing" and "has been falsified", but I think I'm being consistent here. The situation is that string theory is not well-understood enough to make any reliable predictions. If you choose to believe in the unreliable predictions of the theory, on the whole they're wrong.

42. **Urs**
January 25, 2006

> on the whole they're wrong.

In that so far every concrete vacuum which has been constructred is phenomenologically wrong. But since there are more vacua than have been constrcuted…

And so on and so on. We can exchange these arguments for ever, it seems.

43. **Tony Smith**

January 25, 2006

Peter Woit said "… anonymous string theorist referees stopped Cambridge University Press from publishing my book …".

Maybe I have just not been reading the web closely enough, but IIRC that is the first time that I heard of a book publisher bowing to censorship pressure from the string theory community.

Given the stature of Cambridge University Press, I am surprised.

Maybe I shouldn't be surprised, given the "blacklist[ing]" mentioned by anonymous, the "invective" and "rancor" mentioned by DB, and some of my own personal experiences,
but
somehow it seems to me that censorship of book publishing takes the plunge of physics into a Dark Age to a whole new level.
The only ray of light that I see is that Peter was able to find a publisher with enough courage to publish the book: the English publisher Jonathan Cape – congratulations to the people at Jonathan Cape.
In fact,
that is yet another similarity between the situation in physics and the Boondocks MLK episode, in which "… CNN named Martin Luther King one of the 10 most unpatriotic Americans … His book was banned … King renamed his book "Dream Deterred" and it was finally released by a small publisher …".

I agree with Peter's statement "… The fact that no one else is willing to …[criticize the situation]… seems … a deeply shameful commentary on the state of this field. …".

I guess that my Huey Freeman – type "dream" that some prominent string theorist might stand up against the descent of physics into a Dark Age will remain an unfulfilled dream.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

44. **woit**
January 25, 2006

About Tony's comment,

I don't want to make much of the censorship issue, since as I said I don't think censorship attempts are very effective, at least not in this case. As for Lubos censoring his comment section, all it achieves is to have people post them here, together with the news that he is censoring them, not exactly adding to his credibility. As for the arXiv trackback issue, it's not of great importance, and who knows, someday the powers that be may even get around to letting me know what is going on there, and the story may be different than the way it now appears.

As for the book, it's being published June 1 by Jonathan Cape in the U.K., here in the U.S. by Basic Books in the fall. String theorist referees at Cambridge were within their rights to write reports saying they felt it would be a terrible thing if Cambridge published the book. I do wish however that they had felt it necessary to seriously come to grips with the book's argument, instead of dismissing it out of hand. It was also not at all unreasonable for Cambridge to be unwilling to publish something over the strong objections of prominent scientist referees.

In the end, the publication of the book ended up being delayed a bit, but it's now a significantly better book than the version Cambridge was looking at. I wasn't ever that worried about it not finding a publisher at all. There's a wide enough variety of publishers out there that sooner or later it would have found a home, although I'm quite grateful to the people at Cape for the encouragement I have had from them.

45. **D R Lunsford**
January 25, 2006

My God, Peter, they rejected your book? That makes me want to smash things. How did it come to this?

Urs – you're a smart guy. Get out of that mess and away from those people.

-drl

46. **andy**
January 25, 2006

Professor Woit,
Your interview in Discover magazine is really great. I enjoyed reading it. Any of Lubos' commentary can be sent directly to /dev/null.
-Andy

47. **woit**
January 25, 2006

drl,

Cambridge not wanting to publish the thing isn't surprising, prestigious academic presses don't often publish stuff that lots of prominent academics in the field would object to. In the end though, it is being published by some excellent publishers in Britain and here, who I think will even do a better job than Cambridge in terms of making sure it gets wide attention. Not a situation I'm at all upset about.

48. **Tony Smith**
January 25, 2006

Peter said "… It was also not at all unreasonable for Cambridge to be unwilling to publish something over the strong objections of prominent scientist referees. …".

Even if I were to agree for the sake of argument that it was OK for Cambridge to be unwilling to publish "over the strong objections of prominent scientist referees",
I am still unhappy that the "prominent scientist referees" made such "strong objections" to the publication of a reasonable criticism of certain aspects of string theory.

If a theory or model cannot stand up against the publication of reasonable criticism, then it does not deserve to be considered scientific.

Peter also decribed the "prominent scientist referees" as "anonymous string theorist referees", indicating that they chose to hide behind a shield of anonymity, which seems to me to be a further clear indicator of serious problems.

(Note that although referees MAY choose to remain anonymous, it is NOT required of them. In fact, back when I did refereeing for a journal, I disclosed my identity to those whose papers I refereed and gave specific reasons for rejections, so that if they so chose they could discuss the matters with me directly.)

Tony Smith
http://www.valdostamuseum.org/hamsmith/

49. **woit**
January 25, 2006

Tony,

In the Cambridge story I'd just like to make clear that I don't have any real problem with the people at Cambridge; their behavior was professional and not unreasonable. As for the string theorist referees, my problem with them was not the anonymity, that's standard in this kind of situation. Actually of the two very positive referees, one I still don't know who it is, one I do know (since he told me himself). There was also a mathematician referee who didn't want to comment on the controversial parts of the manuscript; he was fine with letting me know who he was and we later had an interesting conversation about the mathematics involved.

I do agree with you that there was a problem with the behavior of the string theorist referees. They didn't at all respond to the points made in the book, or find anything inaccurate there, but still chose to recommend strongly against publication. In an ideal world, a referee faced with a book by an author whose point of view they don't like, but who has his facts straight, should not be so strongly objecting to its publication.

50. **Dumb Biologist**
January 25, 2006

Since I've chosen to be cowardly, I'm going to try to limit my contributions to questions and very general commentary of a perhaps critical, but scrupulously non-pejorative nature, out of a sense of fairness. Don't know if I have the self-discipline, but fair's fair, so I should at least make the effort.

51. **Not a Nobel Laureate**
January 25, 2006

In the absence of experimental data, people can and do hold very strong opinions. But until there's experimental data, that's all they will every be – opinions and beliefs.

Reading this blog about these quasi-theological disputes reminds of my "feet don't fail me now" thoughts as I sought to remove myself as far away from HEP as possible post-PhD. Haven't looked back. Can't imagine how people could find such a sterile field interesting.

52. **fh**
January 25, 2006

Regarding Lubos, I have had an extensive (email) debate about LQG with him in the past (as I was choosing what to persue). It seems his style has seriously deteriorated.

On the Interview I have to say that it is certainly fairer, clearer and calmer then most of the anti string stuff one usually sees floating around these days. It's well done.

53. **Hmm**
January 25, 2006

Peter–

Since I advised Lubos not to waste his time debating you, I will take my own advice and interact with you here exactly once. There is nothing in my "attack" on you that is not objectively true, although admittedly without tact. I said you couldn't make it as a research physicist or mathematician–this is true, looking both at your history of research and your current position. I said you have achieved your 15 minutes of fame by finding something you can do which doesn't require creative talent in physics–this is also true. You aren't being interviewed for Discover because of some great new insight or research direction you have opened up. I said you haven't contributed anything positive to physics. Again, looking at your research record, this is simply true (perhaps I could have been nicer and said "very little positive" to physics). I said that I think you are pompous–ok this is a personal opinion, but frankly, if I hadn't made any contributions to a subject, I wouldn't attack it in the popular press of all places, and I would certainly tell my interviewer something like "you should know I haven't really done any work in 15 years", rather than a rather pompus sounding "I have thought about these problems for 20 years". It is very hard to take you seriously, Peter, because as I said on Lubos's blog, its not like you're an iconoclast with your own brilliant ideas that people are ignoring–for heavens sake, you spent the 80's working on lattice gauge theory. In your interview you were perfectly happy to give the impression that you were forced out of the field because of string theory, whereas it is quite clear you were forced out because you were working in not very interesting ways on peripheral problems. You should know that the field has a way of keeping iconclasts and contrarians with talent around. Many people not working in string theory continued on in theoretical physics or mathematical physics and made important contributions. Quite unlike what you and many of your readers think, theorists of all types value independent thinkers *with real talent* above everything else. By "with talent" I mean with the ability to generate non-trivial, surprising insights, in whatever area they work in. It is not nice to say but it is most likely you were forced out because you didn't have "it" for creative research at a high level at the time. There isn't any evidence in the usual currency of science–papers–to show you did. Its true that there have been geniuses who didn't write many papers–Ken Wilson springs to mind–but (A) the few papers they did write before their breakthroughs still showed some real spark, and importantly (B) before making their breakthroughs, they weren't going around giving interviews to popular magazines about their thoughts on subjects they didn't know much about. They were *busy working hard*, on making their breakthroughs. You are no Ken Wilson. The other things I mentioned on Lubos's blog were comments on your publications and citations. As I said their, this doesn't represent everything, there are many cases where great works are ignored for a long while etc. etc., but over twenty years, *some* signal should show here. As I said, having 11 papers with a total of 220 citations does not exhibit any signal. Lubos makes the mistake of comparing this with Witten, but the real point is that this doesn't even compare well with the record of a somewhat above average fresh PhD.

I brought these things up simply because from the piece in Discover, or the Astronomy editorial a while ago, and certainly from your comments on this blog, one would certainly come away with the impression that you speak with some authority. It is quite shamelessly self-promotional of you to mislead people in this way. You are not a serious scientist, Peter–serious scientists write papers and debate in the literature and make real substansive constructive criticisms and propose new directions of research. You don't.

Finally, I am writing this anonymously precisely because otherwise I would spend far too much time discussing these things with other people insteading of doing physics, which I will happily return to doing now. People don't have to know who I am to evaluate my claims–for instance they can just go to SPIRES (http://www.slac.stanford.edu/spires/hep) and check your publication record as I did. Really, I was only trying to encourage Lubos to not waste his time constantly debating you, but its clear he can't be swayed. I myself have wasted enough time on this already. I leave you (and your readers) with the same thought I left with Lubos–spend your time doing physics. Physics is all that matters in this business invariantly, everything else is sociological junk. Don't spend all your time on sociology, write papers! Do research! If you hate theories that don't make predictions, work on ones that do, write up your ideas, post them on the arxiv. You will find (and I suspect you have already found) that this is infinitely harder than ranting every couple of days, even if it strokes your ego to have so many people chime in and agree with you. But making even small contributions to physics is ialso nfinitely more important and rewarding.

54. **andy**

January 25, 2006

The previous post had zero substance. This is probablyconsistent with the content of his or her “scientific papers.”

55. **Arun**
January 25, 2006

Hmm, it doesn’t take genius to identify bullsh*t when one sees it, and it is an abdication of responsibility to say that because one lacks genius, one should keep quiet about bullsh*t. Moreover, a contribution may be valuable even if it is not by a genius, much cited and with brilliant work. In fact, simply raising an alert on the lack of physical content of certain research programs may be a valuable contribution, especially in an age when skepticism is met with personal attacks rather than any reasoned argument.

56. **Arun**
January 25, 2006

Physics is all that matters in this business invariantly, everything else is sociological junk.

I think that is exactly what Peter Woit is saying, where is the physics? string theory is sociological junk. (Actually, he isn’t quite saying that, but Hmm’s hyperbole deserves such a response.)

57. **Tony Smith**
January 25, 2006

Hmm, in an anonymous comment critical of Petet Woit,
listed many reasons that Hmm considers Peter Woit to be “not a serious scientist”
but
failed to address ANY of the following substantive points made by Peter in his Discover interview about string theory:
1 – “… At this point they really don’t even have a plausible idea about how to ever make a prediction out of this, or how to use this in order to really explain anything about the world. …”
2 – “… They are certainly using mathematics, and they are building models and writing down equations for them, but the models they are working with just aren’t connected to the real world. There isn’t even any plausible way you could imagine that they are going to be able to connect that to the real world and to use these models to explain some experiment we are seeing. …”
3 – “… The kinds of equations that they have now are the kinds of equations you would get in an approximation scheme to some underlying theory, but nobody knows what the underlying theory is. …”
4 – “… There are some who have basically decided that whatever this theory is, it has infinitely complex possible solutions [known as the string theory landscape]. As for the dream that there’s going to be one solution of string theory and it’s going to be the real world, I think a lot of them have given up on that. So they’re trying to pursue this idea that string theory really is an infinitely complex thing. I think a lot of other string theorists are well aware that if you go down that road you really can’t predict anything and you’re in danger of leaving what is normal science. …”.

So,
it seems to me that most of Hmm’s comment should be dismissed as a mere non-substantive ad hominem attack.

However,
Hmm did make what might be considered a response to the following statement made by Peter in the Discover interview about string theory:
“… it has driven out other sorts of research. The way in which it has been pursued has made it virtually impossible to work on other things in the field. …”.

What Hmm said was:
“… You should know that the field has a way of keeping iconclasts and contrarians with talent around. … By “with talent” I mean with the ability to generate non-trivial, surprising insights, in whatever area they work in. … If you hate theories that don’t make predictions, work on ones that do, write up your ideas, post them on the arxiv. …”.

Although Peter’s blog is not the place to discuss details of my physics model,
I will offer my own experience of being blacklisted by the Cornell arXiv as a refutation of Hmm’s factual assertions.

Please do not attempt to discuss my physics model on this blog, as I will not respond to any comment making such an attempt, and I hope that any such comment will be deleted by Peter.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

58. **D R Lunsford**
January 25, 2006

DB – I only meant that you should not claim a cowardice that doesn’t exist (you’re in here with the sharks!) nor downplay being smart, which you obviously are.

-drl

59. **Peter**
January 25, 2006

Hmm,

If you want to argue with or discuss anything I have to say in the Discover interview, I’ll be glad to do so. If you want to discuss my research work, I’ll be happy to discuss hep-th/0206135 or more recent things I’ve been working on along the same lines.

On the other hand, if you want to discuss my credentials or my right to criticize what is going on in current particle theory research, before I’ll even consider that you’re going to have to begin by telling us who you are and what credentials you have that justify your behavior. Your argument that you don’t have time to do so doesn’t hold much water. Come on, how much time does typing a few characters take compared to the time you’ve spent today repeatedly attacking me at Lubos’s blog and now here?

I think just about anyone who reads Lubos’s blog soon realizes he’s a fanatic, driven half-mad by his inability to answer honest scientific criticism. Lubos’s bullying behavior is execrable, but yours is far worse. Unlike him, you’re not just a bully, you’re also a coward. You should be deeply ashamed of yourself, and there is something seriously wrong with an academic field that at least tacitly supports the kind of behavior that the two of you are engaged in.

60. **Hmm**
January 26, 2006

Ok, I can't resist making one last comment here. First–I didn't respond to the criticisms of string theory in the Discover interview because that wasn't the point of my original post to Lubos's blog–which was really about trying to convince Lubos not to bother in these pointless debates. Peter, you recycle the same points over and over again, and the totally reasonable responses have also been recycled over and over again. I see no point in continuing this excercise. Suffice it to say that none of your criticisms are news to anyone working in the field, and there are even more besides the ones you mention, but the positive indications that this approach to unification and quantum gravity is on the right track outweigh all these things for the people who keep working on it–it's not just that they have invested their lives in it, as you like to think–people in this field are willing to turn on a dime. Maybe everyone is wrong, but the way to convince them of this is not to repeat the same arguments they know anyway over and over, but instead offer any interesting non-trivial alternatives.

Second–I didn't resort to name-calling; while you and your readers did. I simply pointed out the factual aspects of your career as a research physicist–this is maybe not nice since it is rather undistinguished, but it is important for people to know this about you when evaluating your opinions. While you're not strictly dishonest about it you don't go out of your way to paint an accurate picture either–unlike Lubos I should say, who despite much much more impressive research accomplishments goes out his way to say things like "right now I am not one of the leaders in string theory" and so on. But why does your record as a researcher matter? Shouldn't the fact that your questions are valid be enough to convince us to listen carefully to what you have to say about them? Unfortunately I think not–well you can do whatever you want on your blog, but I feel its very inappropriate for much more public forums. The reason is precisley because the field is in the midst of confusing (and also very interesting) times. Now, people who know, from experience, what its like to invent really new ideas know that the birthing process for these ideas can be difficult. They know from personal experience about the murkiness before the light when things fall into place. They have seen important ideas be jeered, be called unscientific, unfalsifiable, "not even wrong", many times before. Thats why in a new confusing era, their opinions count for something, their wisdom is important. Of course different people with such experiences can have different takes on whether the current directions for the field are the right or wrong ones. For instance two of my heros in physics–David Gross and Steven Weinberg–have divergent opinions on anthropic reasoning and the landscape, and both their opinions certainly matter to me. But the fact that you have not been involved in any significant research means, Peter, that you have no particularly interesting insight to draw on into the direction in which things should break. You just repeat the same trivially obvious points over and over again, pretending that they are being ignored because they are "counter-establishment", when in fact they're ignored because they don't add anything interesting or new to to the debate. Your readers should know that string theorists are perfectly willing to listen to outsider criticism. String theory isn't a cabal trying to stomp out everything else, and string theorists are not afraid to ask themselves all the same questions you obsess about. They aren't sticking their heads in the sand.
As an example, Gerard 't Hooft is at all the major conferences, often telling stringers they're full of it, and they don't just nod politely, they listen to him, they think about what he has to say. Why? They know he is a unique intellect, who has been right before in amazing ways about things that were totally non-obvious. On the other hand, without any credentials of this sort, you repeatedly attack people like e.g. Weinberg and Wilczek for thinking about anthropic reasoning–does it *ever* occur to you that there is a reason these two Nobel Laureates, who invented large parts of the standard model and surely know something about what is science, are working on this? Is it really because they are so dumb as to not see your objections? Or might it be that, from past experience, they know that it sometimes pays to push through and explore strange ideas that other people freak out over? I find your arrogance in condemning these people (never mind Lenny!) absolutely breathtaking, Peter.

Finally, I am not going to apologize for remaining anonymous. It has nothing to do with the time it takes to type out my name, it has to do with the time I would have to waste talking to people about why I posted, what I think about all this etc. My point here is *only* to point people to an objective source with which to judge your credentials. As far as me–I can tell you that I disagree with Lubos about almost everything, except that like him (and you) I very much dislike the anthropic principle and the landscape, though unlike you I don't think the people working on it are morons who have given up on science–it might just turn out to be the way things are, though I think it is premature to abandon the hope that we can predict all couplings. Unlike you I like SUSY, and look forward to its discovery at the LHC. I am a fan of string theory. I acknowledge it may be on the wrong track with the Landscape and may be wrong altogether, though it seems more likely to me that it will figure in some unforseen way in the final story. I think people should explore alternatives, but there is a reason so many of the best people continue to be attracted to string theory–even with all the problems, much more seems to come out than goes in.

This really will be my last interaction with this blog. I apologize for having offended you, Peter, but I re-iterate that your non-expert readers should know something about your scientific standing to help them evaluate your opinions in the future.

61. **Boaz**
January 26, 2006

A rhetorical question for Hmm:
Is the concept of "scientific standing" really objective?
Why not be honest about it and say that you and some people you know don't think Peter's that great.
But then that begs the question of who you are and whether your own opinion on this matter should be taken into account.
It really does seem unfair to attack someone's credibility without putting your own out there for examination.

62. **Thomas Larsson**
January 26, 2006

Hmm,

attacking Peter's scientific credentials completely misses the point. Peter should be viewed as the tip of an iceberg, probably containing the majority of the physics community.

Perhaps it had been better if Discovery instead had interviewed Nobel laureates like Sheldon "string theory has failed in its primary goal" Glashow, Martinus "string theory is a figment of the theoretical mind" Veltman, Phil "string theory a futile exercise as physics"Anderson, or Bob "string theory a 50-year-old woman wearing way too much lipstick" Laughlin. Or perhaps with the founder of the string theory group at Rutgers, Dan "string theory is a complete scientific failure" Friedan.

OTOH, all science journalists out there, it is not too late to make new interviews.

63. **a**
January 26, 2006

I think that everybody agrees with hmm that it would have been better if a honest criticism had come from inside. Unfortunately experts remain(ed) silent and outsiders tend to base their judgment on concrete results: on this aspect strings are very poor, and I doubt that the busy work of hmm will solve the situation. Doing physics is not only filling papers with equations, but also discussing which problems it is worth to address.

64. **ks**
January 26, 2006

I think that everybody agrees with hmm that it would have been better if a honest criticism had come from inside.

Is it so? Why do we accept a critical public when we deal with politics but not with science? Why must critisism be constructive? If an idea/ideology does not work than it does not work. Showing this is an act of enlightenement and a public service. Demanding that Peter has to be an intellectual giant who produces new euphorias to the physics community is like forcing from a religious critic being itself a religious founder (or from a political commentator being a politician). Sometimes it should suffice being simply reasonable and well informed.

65. **secret milkshake**
January 26, 2006

I was born to into a communist country. I remember reading this kind of stuff back then: "Who are these so-called dissidents? They are all flakes and failures. We already have an honest, healthy and constructive discussion and it must be in its proper place. We need to have peaceful conditions for further continuation of our creative work."

When Motl wrote "we can't make progress until we get this kind of human garbage lined up against the wall + shot" (he means his critics) he was yearning for the more direct approach.

66. **JE**
January 26, 2006

Hmm said,

"…my heros in physics–David Gross and Steven Weinberg…"

Any researcher in theoretical physics needs to know as much as possible about Gross and Weinberg's great contributions to physics, but I don't think that he/she needs to worship any "heroes". It sounds puerile as an attitude towards research.

67. **fh**
January 26, 2006

The ad hominem attacks are besides the point of course, but I have heard similar criticism from people whoes standing as absolutely brilliant physicists can not be doubted.

For those who obsessively count citations to judge scientific merit, I'm talking about people whoes number of citations is the same order of magnitude as Wittens, if scaled to the size of the field they work in.

68. **Nigel**
January 26, 2006

'…might it be that, from past experience, they know that it sometimes pays to push through and explore strange ideas that other people freak out over?' – Hmm

The word 'strange' is not useful because all new ideas are initially strange by definition.

The mainstream string theory effort is not coming under criticism because ignorant outsiders object to new ideas being 'strange'.

It's that the new ideas, as Woit says, have reached a critical mass and are a self-propagating excursion from the business of modelling and predicting measurable phenomena.

It is interesting that a recipe based entirely on speculation, which produces untestable predictions, has become mainstream, and defends itself using the arrogance of dismissing critics as ignorant.

String theory uses extra dimensions to vaguely 'predict' unobservable gravitons and superpartners that will unify other forces at an energy far beyond experimental confirmation.

Why should tax-payers fund this?

'… it seems more likely to me that it will figure in some unforseen way in the final story.' – Hmm

Sure it will. The guy who comes up with the right theory will have to include a lengthy analysis of string theory in his paper, just to prove he is not plain ignorant of string theory, to get it read…

69. **Christine**
January 26, 2006

"Science is a way of thinking much more than it is a body of knowledge." — Carl Sagan

Any reasonable person — serious researcher or layman — will promptly recognize the value of Peter Woit´s contribution on how to improve our way of thinking over the (difficult) problem of quantum gravity, even if one does not agree with his opinions.

Personal attacks can only be interpreted as some kind of psychological reaction to primitive fear, and only reveals some kind of immaturity for practicing science or any productive activity.

70. **amused**
January 26, 2006

The fairytales of Hmm above shouldn't be left unchallenged.

1) "As an example, Gerard 't Hooft is at all the major conferences, often telling stringers they're full of it, and they don't just nod politely…"

Indeed they don't. I was at an informal talk by `t Hooft once, with Dijkgraaf also in the audience. `t Hooft was saying something negative about string theory and Dijkgraaf's reaction was to get annoyed and walk out. Generally, string theorists regard `t Hooft, Glashow and co. as dinosaurs who made great contributions in the past but whose present day opinions on string theory are ill-informed and don't matter.

2) "theorists of all types value independent thinkers *with real talent* above everything else. By "with talent" I mean with the ability to generate non-trivial, surprising insights, in whatever area they work in."

Oh sure they do. That's why the shortlists on the rumour mill pages are full of young people who are striving to demonstrate their originality and independence by developing new research directions and publishing their independent work in our top journal.
In reality, there are few things that the powers-that-be despise more than the sight of young people trying to do original independent work. Such fools will quickly discover that their PRL publications count for nothing in competition with the smart youngsters who realised that success in this field requires one to ape one's famous elders and ride their coattails as junior author on their papers. Lip service is paid to the noble ideal of supporting independent research, but in practice the powers that be only support their clones, since a clone of them is of course intrinsically better than a non-clone, irregardless of what the latter might have done. (And it is indeed amusing to see how some of these clones go dead researchwise once they get a faculty position and have to start being independent. Lubos is but one example.)

3) "You aren't being interviewed for Discover because of some great new insight or research direction you have opened up…"

Perhaps Peter was being interviewed because he has done more than anyone else to provoke a critical discussion of the state of affairs in string theory? For many years the stringers have been promoting the notion that they are just so incredibly brilliant and are doing so wonderfully well with their reserach program. No need to put a damper on the exuberant hype of Kaku & co.; it will all turn out to be true in the end anyway, and in any case it is good that the public should know how brilliant these people are and how wonderful their work is. As `a' mentioned above, ideally people from within the string community itself should have been the ones to call for a more balanced assessment. But there was no prospect of that happening, so it had to be someone from the outside, and Peter Woit was pretty much the only applicant for the job. Given that string theorist dismiss greats like `t Hooft and Glashow as ill-informed dinosaurs (come on guys, don't try to deny it), it's a bit ridiculous for them to complain about a lack in Peter Woit's credentials. If there really is a shortcoming in Peter's capacity to criticise string theory it will manifest iself through inaccurate/ill-informed/unreasonable statements that he makes. People who want to question Peter's "right" to criticise string theory are invited to point out the occurances of such statements in the hundreds of pages he has written already.

71. **Thomas Larsson**
January 26, 2006

Indeed they don't. I was at an informal talk by `t Hooft once, with Dijkgraaf also in the audience.

Dijkgraaf, like the Verlinde twins, are former 't Hooft students, right?

72. **mhm**
January 26, 2006

"And it is indeed amusing to see how some of these clones go dead researchwise once they get a faculty position and have to start being independent. Lubos is but one example."

This is not fair. Lubos made many original contributions to the research of climate change. And he has done so without any credentials, because they are only needed when talking about strings.

73. **mhm**
January 26, 2006

"Motl wrote "we can't make progress until we get this kind of human garbage lined up against the wall + shot"

Mr. Milkshake, this comment just shows how intelligent and smart Lubos Motl is. Only he, Jacques, Hmm and the other super string theorists are able to think clearly about superstrings, because they are so smart. Their time is very valuable, because they need to think so much and write their blogs and comments.

The laymen belong to "the stupid people" as Lubos once told us.
They can only solve easy problems (like how to make an honest
living). They should only do two things:
a) buy the books about superstrings and admire the great thinkers.
b) pay the bill

74. **a**
January 26, 2006

Dear mhm: in case your full name is Lubos Motl, it was already said that yours is the best trash show; but don't you think it is a bit out of context in a serious discussion?

75. **anonymous**
January 26, 2006

"… Lubos made many original contributions to the research of climate change. And he has done so without any credentials…" – mhm

But Motl's definition of the word 'hypocrisy' has a special exclusion clause just for string theorists! They are allowed to vent views on climatic change without a single paper or citation in that area! This same principle allows they to ignorantly ridicule LQG, etc…

76. **woit**
January 26, 2006

Hmm,

> I will take my own advice and interact with you here exactly once.

Why did I know this was a lie the moment I read it?

Let's see, your claim is that you have to remain anonymous because otherwise you would have to spend too much valuable time explaining why you are attacking me and what you think about string theory. Well, much of your comments are all about these topics, and you seem to be having no problem finding lots of time to go on about them at ever-increasing length. You evidently have plenty of time on your hands to make up nonsense I never wrote (e.g. that Weinberg and Wilczek are morons) so that you can continue to attack me. I think there are two much more obvious reasons for why you insist on anonymity:

1. You're a bully and a coward.

2. You’re a string theorist wannabe, with no credentials at all in this field beyond having read a few popular books on the subject.

There seems to me to be plenty of evidence in your by now many pages of comments for both of these.

Thanks to many commenters who already did a far better job in writing in here to explain why my views might be worth listening to than I ever could. It’s encouraging to see that what I am trying to do here is widely understood and appreciated.

I’d like to point out that much of what I’m trying to do is to not just go on about my own views or criticize those of others. No, I don’t call Steven Weinberg or Frank Wilczek “morons”, instead I as accurately as possible quote what they say, sometimes make some comments on what I think about it, and provide links to every place I know of where their own words are available. I believe this often provides useful information so people who can decide for themselves what they think. Even quite a few string theorists who are not at all happy with my views tell me that they find this valuable.

One of the main reasons I went into science was that it is a subject not based on blind belief in authority figures. If you’re willing to put in the time to understand what people are saying, you can make up your own mind about what to believe. I hope this blog helps people do that.

77. **fh**
January 26, 2006

I like to think that hmm is actually Lubos talking to himself, if for no other reason than that it amuses me.

78. **woit**
January 26, 2006

fh,

Unfortunately I’m quite sure they’re different people and it’s a sad fact that Lubos’s fanatical views and behavior are not unique to him. For one thing “Hmm” has a native speaker’s command of the English language, unlike Lubos. My guess is that “Hmm” is the same person as “Michael” who every so often posts comments here about how incompetent I am. He seems to live in the Boston area and may have some sort of connection to Brandeis.

79. **J.F. Moore**
January 26, 2006

Pete Woit’s (or more generally, anyone’s) credentials are only relevant to the argument to the extent that the reader does not understand the issue at hand and must take on faith the position of the one making the argument. Anyone who takes the minimal time to grasp what’s going on, even at a very basic level, must admit there are some problems with superstrings, at the very least a huge dichotomy between the advertised power of the theory (esp. 10-20 years ago) and the actual power, if power is meant the way it is for almost all of science, as having the ability to make predictions.

80. **J.F. Moore**
January 26, 2006

Sorry, I didn’t mean to abbreviate your name like that, Peter. I blame my new bluetooth keyboard.

81. **anon**
January 26, 2006

Peter,

I think Lubos is being quite successful in his main objective: intimidation. While it will hardly affect your behaviour, it sends a strong message to others who might be contemplating taking a public stance on various issues you discuss.

Not quite effective as the “line up ’em up and shoot” technique he learnt in his native country, but he’s adapating it well.

82. **amused**
January 26, 2006

Thomas: yes, Dijkgraaf and the Verlinde twins were `t Hooft’s students. Not all of his students became string theorists though (e.g. van Baal), and conversely, not all Dutch string theorists were his students (e.g. de Boer).

mhm: That was hilarious 🙂 It was supposed to be ironic, right? Or are you really LM?

83. **Tony Smith**
January 26, 2006

If Hmm is anonymous, Hmm’s qualifications must be judged based on material in Hmm’s posts, and I have only read Hmm’s two comments on this blog.
However,
it is possible to evaluate Hmm’s mentality to some degree by considering the following excerpt from those posts, in which Hmm said to Peter:

“… I disagree with Lubos about almost everything,
except that like him (and you) I very much dislike the anthropic principle and the landscape
… I like SUSY … I am a fan of string theory. …”.

I wonder if leaders of the conventional superstring community, such as Ed Witten, will ever realize that their community is ill-served by allowing such people as Hmm to act as their advocates,
and
whether those leaders will ever speak out loudly against Hmm and others of that ilk,
or
whether those leaders will continue, by their silence, to endorse them.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

84. **D R Lunsford**
January 26, 2006

Tony, good point, in some ways ST is like a pernicious political idea , e.g. Maoism, that attracts every rogue on the planet.

-drl

85. **D R Lunsford**
January 26, 2006

Anon – he's like the Czech hockey team – take enough shots and something will eventually go in!

The way to beat the Czechs was to outskate them. At this point, LM has been outskated.

-drl

86. **Adrian H.**
January 26, 2006

As ugly and naked as it is, Hmm's view represents one important aspect of the degeneration of modern intellectual life: this incessant placing of everyone and everything into league tables and rankings. It is a sickness, and it is usually pressed very hard by those charlatans who have worked up a good 'reputation' by doing minor incremental work that advances things infinitesimally. It is usually combined with sycophancy towards the 'great and worthy'; and with maintaining one's presence by skillful networking. It is the way to have a great career at the modern University.

In this lattice structure of good and bad reputation it comes to seem natural that some people are entitled to express their opinion while others are not. Secret Milkshake likened this to the Soviet Union. But it is probably far more insidious than that, because it is self-imposed. People like Hmm have entirely internalised this worthless junk.

What Hmm dislikes about you, Peter, is that you are in effect speaking out of turn—not showing due deferrence to the great and the good. So you can be sure that he got where he is by toadying his way up the ladder. If we saw his publication record I'm sure it would look impressive. Until we looked more closely at just what the acheivement really is. Then it would look shallow and trite: merely dotting the i's on the great men's work.

But it shouldn't upset you, Peter. This is all part of what you are criticising in your blog every day. And I for one hope you keep it up.

87. **Peter**
January 26, 2006

Adrian,

I think you're quite right about the mentality of "Hmm" and a few others like him. There's been a lot of this in recent days due to the Discover article, but there was a certain amount of it since the beginning, when I first started publicly criticizing string theory.

At the time I remember thinking that some of this kind of behavior was highly remniscent of the dominance displays of males in a group of baboons when an interloper who doesn't fit into the hierarchy arrives in their midst. Lots of jumping up and down, flashing of teeth, loud screeching, displays of posteriors (or at least the string theorist equivalents….).

88. **anonymous**
January 27, 2006

I should say that this quote of Chomsky nails it on the head:

"In my own professional work I have touched on a variety of different fields. I've done my work in mathematical linguistics, for example, without any professional credentials in mathematics; in this subject I am completely self-taught, and not very well taught. But I've often been invited by universities to speak on mathematical linguistics at mathematics seminars and colloquia. No one has ever asked me whether I have the appropriate credentials to speak on these subjects; the mathematicians couldn't care less. What they want to know is what I have to say. No one has ever objected to my right to speak, asking whether I have a doctor's degree in mathematics, or whether I have taken advanced courses in the subject. That would never have entered their minds. They want to know whether I am right or wrong, whether the subject is interesting or not, whether better approaches are possible – the discussion dealt with the subject, not with my right to discuss it.

But on the other hand, in discussion or debate concerning social issues or American foreign policy, Vietnam or the Middle East, for example, the issue is constantly raised, often with considerable venom. I've repeatedly been challenged on the grounds of credentials, or asked, what special training do you have that entitles you to speak of these matters. The assumption is that people like me, who are outsiders from a professional standpoint, are not entitled to speak on such things.

Compare mathematics and the political sciences — it's quite striking. In mathematics, in physics, people are concerned with what you say, not with your certification. But in order to speak about social reality, you must have the proper credentials, particularly if you depart from the accepted framework of thinking. Generally speaking, it seems fair to say that the richer the intellectual substance of a field, the less there is a concern for credentials, and the greater is concern for content."

Lubos is clearly not doing his field any flavor.

89. **Adrian H.**
January 27, 2006

Yes, the my-publication-list-is-longer-than-your-publication-list does have certain analogies! 🙂

90. **Ignorant Layman**
January 27, 2006

Dr. Lubos Motl's polemical invective which resorts to a scatologically laced ad hominen attack against his straw man of a crackpot who reads this blog, I think reveals much more about the good professor's lack of professionalism than any amount of learned exposition might otherwise afford us wayward ignoramuses. I think he has hoisted himself on his own petard most ignominiously.

91. **D R Lunsford**
January 27, 2006

Adrian H, in other words, "Cites matters."

-drl

92. **anon**

January 27, 2006

Popular voting systems like citations aren't even democratic! Citations occur when people follow the mainstream ideas. So what? The mainstream was wrong on caloric, phlogiston, etc! It doesn't matter how many citations there are, only if the facts are sound. Citation bureaucracy can work against real innovation.

93. **Ignorant Layman**
January 27, 2006

How about those embarassing expert pronunciamentos such as "Nuclear power will never work," etc., etc.

94. **Juan R.**
January 27, 2006

Peter Woit,

About tactics you are cited above

> 1. The ad hominem attack. Attack your opponent's credibility and right to say anything on the subject. This is Lubos's main tactic today.

He used this tactic here in his attack on Horgan. I have explained to him how splitting data from interpretation of data, e.g. via detecting the existence of quotes 🙂 . Lubos Motl in this blog often some of us use blockquotes instead of " ". You can detect them because they are rendered in a different color, and indented by the console. For example this is a comment section on data and this

> This is a quote

is data (quote).

Peter Woit, if he can learn, i personally wait that in future he can openly critize either data used in some discussion or comments on that data but without resorting to the ad hominem attack.

> 2. The straw man attack. Misrepresent or even misquote your opponent's words, then attack them as stupid and ignorant for saying something inaccurate.

Easily inverted thecnique! No problem with it unless that you cannot reply to the attack, but that is already next point.

> 3. Censorship. When you can get away with it, by any tactic available, keep people who disagree with you from being heard.

This is very ancient! Already guys as Newton were censured by their colleagues. A modern example of censure are ArXiv blacklists, or sistematic rejection of hot topics by some leader journals (e.g. some journals do not acept critical papers on relativity). Another examples are blocking of funding of rival schools, rejecting of position for rival scientists, heavy peer-review, etc.

The natural solution to this point is the re-grouping:

– Newton and colleagues re-grouped and published alternatively.

– ArXiv blacklisted scientists launched new preprint servers.

– People critizing some aspects of relativity publish in alternative journals.

– Searching alternative ways for research funding

– Etc.

> 4. Intimidation. I've heard from many, many physicists that they agree with many of my criticisms of string theory, but are afraid to be publicly associated with them because of repercussions for their career.

Very related to point 3. In fact, may serious studies on revolution on science and alternative ways suggest that best position for launch an alternative or revolutionary way on science is from an independent position, e.g. some emeritus position in some university, chair in a new funded institute, etc. This way you can simply neglect intimidation, so usual in archaic universitary schemas.

This is the reason that I can openly publish criticism on string theory. Those physicists could openly critize that nonsense called string theory if they received alternative careers to actual ones.

It is just a question of time and politics, not a problem that string theory was difficult to evaluate. No real problem with this…

—

Juan R.

Center for CANONICAL |SCIENCE)

95. **D R Lunsford**
January 27, 2006

kshe – right, it isn't obvious to me how this applies. Like I said -confused-.

-drl

96. **Boaz**
January 27, 2006

anonymous quotes Chomsky as saying

> Generally speaking, it seems fair to say that the richer the intellectual substance of a field, the less there is a concern for credentials, and the greater is concern for content.

I think that Chomsky is missing an important piece here. I think that the depth of the relationship of a field to society as a whole is equally, if not more important (regarding receptivity to outsiders' ideas and criticism) than wealth of ideas and substance. Math is a traditional field that is almost universally recognised to be valuable. Math departments have the dual role of developing new ideas in addition to preserving existing knowledge through teaching. And the value is shown by its inclusion in curriculum from elementary school through college. I don't know Chomsky's ideas about the connections between mathematics and linguistics in detail, but I would guess that they do not threaten the existing relationships that mathematicians have with society at large. His views on politics, on the other hand, do threaten the relationship that that group of academics has with society. Political scientists and historians to some extent retain clout by the perception that they advise government and society on some major policy issues. If the value of that advice is being attacked, so is their place in society.

So there seem to be two issues: one is whether a given criticism goes to the heart of a field's relationship with society, and the second is whether that relationship is strong enough that it can take the criticism in stride and improve the health of the relationship. Sensitivity to criticism can mean a fear of it all falling apart.

I think that Lubos does indeed see the position of string theory in society being attacked by Peter and he is responding. So I don't think that it necessarily implies a poverty of ideas, but rather an insecurity of a precarious position. If Lubos felt more secure in his own position and in that of string theory, he wouldn't worry so much that a little criticism would cause the whole thing to collapse.

97. **Adrian H.**
January 27, 2006

”Cites matters." Very nice.

98. **mathjunkie**
January 27, 2006

Citation sometimes can't tell the importance of a research paper. I found that some researchers intentionally increased their number of citations by quoting their published papers in their future publications but their peers cited those published works only very few times.

99. **Jimbo**
January 28, 2006

Congrats to Peter for an excellent Discover interview. Basically what he's been saying for several years to the community, is now getting a vastly wider audience, and will ultimately trickle down to the intelligent lay person, who has seen the glitzy NOVA string special, & other stringy hype, & cause that person to slap themselves on the forehead & say "Hey, the superstringers have definitely broken lock with the scientific method & have gone off into their own little world of mathematics, marketing it as reality".
We cannot really blame Susskind, Witten, Green, Schwarz, Maldecena, Polchinski, Greene, et al. for this sad state of affairs. It is Dirac, their godfather, who justified the stringy crusade with his aphorism, "It is more important to have beauty in one's equations, than truth". Gell-Mann, added fuel to the fire with his "Anything not expressly forbidden is compulsory".
As Peter points out, this is all fine for mathematicians. However, as many theorists still stubbornly believe, physics is an empirical science, which for 300 yrs, has set a higher standard of reality than mere aesthetics.
Lastly, without appealing to reality by consensus, I think the blogs should start a head count in the anti-string camp, perhaps with an arxiv `call-to-arms' note, & ram it down PhysicsToday's throat. Just to drop a few names on record for starters: Feynman, Glashow, Veltman, well, you get my drift…

100. **D R Lunsford**
January 28, 2006

Jimbo – you've misinterpreted what Dirac meant. He's undoubtedly spinning like a new pulsar in his grave, over what has happened.

If only he were alive. I feel like we need one of our elders to return from the undiscovered country, and start kicking ass.

-drl

101. **Thomas Larsson**
January 28, 2006

Lastly, without appealing to reality by consensus, I think the blogs should start a head count in the anti-string camp, perhaps with an arxiv `call-to-arms' note, & ram it down PhysicsToday's throat. Just to drop a few names on record for starters: Feynman, Glashow, Veltman, well, you get my drift…

OK.

102. **Chris Oakley**
January 28, 2006

> It is Dirac, their godfather, who justified the stringy crusade with his aphorism, "It is more important to have beauty in one's equations, than truth".

You cannot blame Dirac for a trend that only really got going in the year that he died. Personally, I think that the "anything goes" mentality can be traced back to something that Dirac was most definitely not responsible for, namely the endorsement of mathematical chicanery in physicists' desperation to "explain" the Lamb Shift from about 1947.

103. **woit**
January 28, 2006

Jimbo,

I really don't agree that the problem is that string theorists have left physics for mathematics. The mathematically sophisticated parts of string theory are actually the valuable ones that may someday lead somewhere. What's really useless and dangerous are things like the "Landscape", which are quite "physical", involve very little math, but are completely noxious and threatening to turn particle theory into pseudo-science.

Given the lack of new experimental results, the pursuit of new ideas about physics based on their mathematical beauty actually seems to me as promising a thing to try as any. It is a great shame that the failure of string theory threatens to discredit research in this direction.

104. **mathjunkie**
January 29, 2006

It seems Lubos reacted violently in his webpage.

For balancing the views between stringy people and anti-stringy people, I hope the Discover magazine could have interviewed Lubos as well.

105. **Who**
January 29, 2006

For balancing the views between stringy people and anti-stringy people,

personally I think science benefits more from calm reasoned debate (by people able to understand others' viewpoint) and I think P. does admirably in the calm-reasoned category

so for balance, on a panel, I would match P. with somebody else—-not someone violent, emotional, bigoted. Same if I were publishing back-to-back interviews in a magazine.

I don't know who it would be—-there's David Gross (but being a nobelist he's awfully heavy)—-Robbert Dijkgraaf is a terrific talker and an attractive advocate—-no, too much glamor for what I have in mind. It is not about salesmanship it is about honest reasoning over scientific priorities.

Now I think of it! Imagine matching Peter with Andy Strominger—-back to back interviews! those are two people who would take pains to understand each other viewpoint and to address each other concerns.
both would scrupulously refrain from adhominem attack. Bravo.

BTW if it were a THREE way balanced thing, I would put up Laurent Freidel as a representative of the alternatives to string research.
I would not just pit Andy and Peter. It is really a triangle, as I see it.
Anyway there is a potential for calm reasoned balanced discussion (no vituperation, no acrimonious Motley) and it might be good for the physicis community. Hope this not too much off topic.

106. **woit**
January 29, 2006

mathjunkie,

I also very much hope that Discover will do a profile of Lubos.

107. **Tony Smith**
January 29, 2006

Who suggests that "… a THREE way balanced …" discussion might be good.

An example of a very good "THREE way" discussion is the book "Triangle of Thoughts" (English translation AMS 2001) by Alain Connes, Andre Lichnerowicz, and Marcel Paul Schutzenberger. The flavor of the book might be seen in the following excerpt from A.C.'s part of the trialogue:
"… String theory was supposed to apply to strong interactions … this theory of little strings moving in space-time implkes the existence of particles of spin 2 and zero mass like the graviton. … string theory has so far been tested only on a strictly mathematical level … in physics, we are still far from the mark. … we can hope that string theory will make it possible to understand the arbitrary constants appearing in the Standard Model, that is, in particle physics; but so far the theory offers nothing substantial in this respect.
One of the major problems is that … supersymmetry … must be broken in order not to contradict the experimental results of the Standard Model …
… A true theory would no doubt have to explain the arbitrary parameters of the Standard Model …".

Please read the book to get the full context of those and many other issues discussed therein.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

108. **Who**
January 29, 2006

Hi Tony, I have not seen "Triangle of Thoughts" but there is an interesting trialog about QG available on arxiv, as a sample of this form:

http://arxiv.org/abs/hep-th/0501103

Ted Jacobson, Donald Marolf, Carlo Rovelli

Ted Jacobson has been a critic of Loop Gravity and related QG in recent years, serves as deviladvocate sometimes at conferences, voicing skepticism, and specializes in observational tests that constrain QG.

Don Marolf has done string research mainly I think, but collaborates with Loop people sometimes. So the trialog is people who are on different sides of quite a lot of issues but can still talk amicably.
=================

I wasn't thinking of a trialog actually, but of some magazine publishing three separate interviews back to back.

A. a critic (like Peter or Larry Krauss, actually many people are vocal critics, some representative)

B. a string practitioner—someone who can acknowledge problems but give a reasonable defense

C. a nonstring researcher who doesnt bother criticizing string!

I have never heard Freidel, for instance, say anything about string. he is too busy with his own research and seems just not interested in string.

Often times you have string apologists for whom string is a huge issue and they act like "if you are not for us you are against string (and motherhood :-))" and all these people who do QG are our ENEMIES and they are our detractors and we must discredit them etc.

But actually many intelligent creative researchers just are not interested in string and never seem to talk about it (at least in public) and they just do their work

(the problem is the artificial restriction of career opportunities for these people in the US, I think—-highly qualified people are in effect denied access to the job market if they do something that is perceived as rival to string—but I don't hear these people openly complaining, instead I hear Smolin sometimes in a sense complaining FOR them.)

so I would publish a triangle of interviews partly just to show that the issue is not just PRO and CON string—there is active QG research by people for whom string is basically IRRELEVANT. Having the discussion always focus on pro-and-con string gives a distorted picture because inflates string importance.

well I’m not a magazine editor and no one should hold their breath waiting for a brace of cool interviews about diversifying particle theory research in the US or anything like that.

congratulations to Discover for having the one with Peter that they did, which is some progress at least

Die Physik steckt in der Krise

January 25, 2006
Categories: Uncategorized

The German weekly *Die Zeit* has an article this week by Max Rauner about string theory, the Landscape, and the controversy over whether this is science or not. My German is rather shaky, but as far as I can tell it's an intelligent summary of the controversy, emphasizing Susskind and his new book, and quoting many of the usual suspects. The same issue also has an interview with philosopher of science Martin Carrier about the question of whether or not string theory is a science.

Update: Eli Rabett has put up a translation of the article into English on his blog.

Comments

1. **SomeBody**
   January 25, 2006

   I love how the article manages to quote the number of universes both as 10500 and as 10 with SUBscript 500…

   The philosopher Reiner Hedrich (apparently asked to "bring order" to the physics debate in Germany?!?) has a cool soundbite. Translating freely from the article,

   »The Multiverse theory is reasonable in its logic", says Hedric, "but not reasonable enough to be science." The research program reminds him of the pre-Socratics of ancient Greece: it's "a metaphysical contemplation of nature".

2. **Wolfgang**
   January 25, 2006

   The only thing really new in this article is the comparison of quantum theory and general relativity to an old married couple sleeping in separate beds …

3. **Frisean**
   January 25, 2006

   Martin Carrier says: "Eine strenge Unterscheidung zwischen Wissenschaft und Metaphysik, wie man sie früher verlangte, macht man heute eigentlich nicht mehr."

   translates to sth like "A strict seperation between science and metaphysics, as it was demanded in past, is not made today any more."

   Can this be taken serious? I am a layman, but I doubt all philosophers and scientists will agree on that.

   The German term Wissenschaft means more than empirical science but I consider this statement to be very questionable. Maybe I am a little naive, but a Natural Science should ask for empircal evidence while metaphysics has not and can be entirely speculative or is this not true any more? Elsewise the boundries between metyphysics/religion on the one side, and empircal sciences on the other science become diffuse or even not existent.

4. **D R Lunsford**
   January 25, 2006

   Frisean – certainly it's the most important topic. To me, string theory failed when it trivialized Democritus by introducing strings to begin with. That string theory fails as science is no surprise – what has not been much said is how utterly it fails as metaphysics, by completely failing to provide a sensible ontology. On this level, it's "too ugly to contemplate" rather than being right or wrong.

   -drl

5. **Quantoken**
   January 25, 2006

   Wolfgang said: "The only thing really new in this article is the comparison of quantum theory and general relativity to an old married couple sleeping in separate beds … "

   I thought they were never married to start with.

6. **D R Lunsford**
   January 25, 2006

   Q – actually GR and QM have much in common.

   1) Ultimately both are based on g_mn.
   2) Both are stymied by the volume element (reducibility of metric, electroweak symmetry breaking)
   3) Both are forced to endure unacceptable physical ideas (singularities).
   4) Both have a hard time dealing with sources, and treat them as an external thing
   5) The Lagrangian seems ad-hoc (conformal weight of R, Higgs mechanism)

   There are five obvious ones – I could think of more.

   -drl

7. **Chris W.**
   January 25, 2006

   DRL,

   Aren't you willing to contemplate alternatives in fundamental physics to a Democritean ontology as *starting point*? Obviously the modern conception of the atomic constitution of matter is quite different than that considered by Democritus, although the latter is clearly a historical antecedent.

   Furthermore, the modern notion of physical fields as being fundamental, which evolved from Maxwell's electrodynamics as it was freed from the assumption of an underlying medium (the ether), is something that transcends Democritean atomism. Of course atomism and field theory have been largely fused in the context of quantum field theory. Finally, there is the notion of spacetime itself as dynamical, which the ancients were hardly in a position to anticipate.

That said, the ontological problem with string theory is arguably that strings are themselves merely a starting point, leading into a wilderness of higher dimensional structures and various (partial) formulations of M-theory. The upshot is an ontological shambles.

Some would say that, in view of things like dualities and effective field theory, this is both expected and unavoidable; we should just get used to it. Someday I think this situation will be viewed by as an enormous joke played on us humans. In a sense, we have played the joke on ourselves; we're capable of being too mathematically creative for our own good.

8. **Chris W.**
January 25, 2006

Physics and cosmology have always been, in part, a "metaphysical contemplation of nature". However, that metaphysical contemplation has, at its best, spawned empirically potent, testable ideas. To me, the profound irony of our current predicament is that it evolved out of the most proudly empirical and anti-metaphysical movements in modern physics—the developments of atomic, nuclear, and particle physics (and arguably, condensed matter physics). (A further irony: Ernst Mach, the intellectual godfather of this anti-metaphysical attitude, never accepted atomism as having a proper place in physics, even up to the time of his death in 1916.)

I believe we do need to reconsider the metaphysics and ontological basis of physics, and how it *supports* the subject as a science. The trouble is, that's *hard*; most metaphysics is indeed crap.

9. **Peter**
January 25, 2006

Please, keep the discussion here to the topic of the article I was posting about. Several people have complained to me recently that there's too much rambling and anti-string ranting, unrelated to the topic at hand. I'll be deleting a lot more such comments in the future.

10. **Geon**
January 26, 2006

"The only criticism of what I've been doing over the years that I would actually agree with is that I should have spent less time thinking about string theory and complaining about it and learning about it just to criticize it, instead of devoting time to more positive things that I should be pursuing. "

In my opinion, arguing over whether string theory is science or not or finding endless links on articles on this matter does not serve any good for the statement made by Peter. We all know for sure that philosophy won't get us anywhere, so why not "Think your best thoughts, and devise ways to test them "? quote Feynman.

11. **plato**
January 26, 2006

> **Physics is in the crisis: The dream of the world formula burst, the new theories is hardly more examinable. Does it at all still concern in the cosmology science?**
>
> Free love, LSD, anti-war demo. Leonard Susskind took part in all that "and still more", as he stresses……

Google has a translator

It seems there is to be a connection between "earlier thinking" on Susskind's part, to what has emerged from him, into what is given of science? I think this is right, so far from what is above?

From outide the US, historical information would be gathered and formulated into German thinking conceptual based on language? It sets the tone? Can German concpetualization really get the feel of the man?

If the attack is to come, must it not be on what has been preceded here in earlier idealizations with regards to "degrees of freedom" and "dimensional analysis?"

Here I do not want to detour from article, so that is all I will say. How would charactor assissination support this article?

I hope translator was helpful.

12. **Quantoken**
January 26, 2006

Peter:

I must point out that you have been hypocritical that at one moment you complain about censorship on ARXIV and on Lubos's blog. And the next second you turn around and threatened even more rigorous censorship on your own blog. That does not look good on you. To be fair I see nowadays Lubos is actually much more tolerate on different opinions than you do. What's the point of having a discussion if no alternative opinions are allowed?

Quantoken

13. **AG**
January 26, 2006

quantoken you're boring, go away.

14. **ObsessiveMathsFreak**
January 26, 2006

Here's a full translation of the page, curtesy of google.

15. **ObsessiveMathsFreak**
January 26, 2006

Sorry for the double post.

The article does mention that other theories were also very speculative in the past. It mentions the kinetic theory of heat and the atomic theory of matter and even copernicus gets a look in.

Creationism was brought up in the article, but I think Intelligent Design was not.

The article (translation) mentions:
"It takes every now and then decades, to scientific pagings in the scientific community recognition is often still longer and, until it accepts the public. ….Sometimes revolutions are also broken off, and one returns to old conceptions."

Which I suppose is an accurate decription of how science works. There seem to be epochs, wherein certain theories are in vogue. Sometimes, these theories are verified, sometimes they are superseeded. I suppose time, or rather *data*, will tell if this is the case for String Theory too.

16. **Robert**
January 26, 2006

The article is not so much about string theory but about the 'multiverse', the idea that beyond what we see as our universe, there could be regions that are vastly different from ours since the fundamental constants are different. This might be realised in string theory but could as well be realised in other theories as well.

If one believes in a "theory of everything", that is a theory that does not have numerical parameters (like the electric charge [aka fine structur constant] or the mass of the electron) this theory should have a way to explain the numerical constants of the standard model of particle physics. One popular way to do that is to promote them to fields that happen to have these values.

But this brings with it the possibility that these fields take different values at different places. And voila: the possibility of the multiverse. In this case one rather has to explain why these fields vary so little in the region of the universe that we observe.

17. **Frisean**
January 26, 2006

My point was about the article.(the linked interview in particular)

And I still feel disturbed about the statement that one does not really distuingish between metaphysics and science any more. At first look, this is confusing and dangerous, also regarding the following statement within the interview, that intelligent design is not disputed to be science, instead merely claiming that its credibility is bad reagarding its capability to explain phenomana.

If I stick to this notion, everything is science, als althogh not everything is "good science". What`s the benefit of using the term science then at all?

18. **Mark LaFlamme**
January 26, 2006

The beauty of string theory for somebody in my profession is the very thing that gets criticized. I'm a reporter and columnist in Lewiston, Maine, but I'm also a novelist. Last month, my novel "The Pink Room" was pubished and it's doing very well. I owe it all to the uncertainty of of the various theories and the creative leg room that presents to a person writing a science based work of fiction. The story is of a leading physicist who attempts to use the science of string theory to bring his daughter back from the dead. The idea is that the energy of the human soul seeps into one of the higher dimensions. It's an idea that may be no more or less easy to disprove than the existence of those higher dimensions to begin with.

19. **Thomas Larsson**
January 26, 2006

If one believes in a "theory of everything", that is a theory that does not have numerical parameters (like the electric charge [aka fine structur constant] or the mass of the electron) this theory should have a way to explain the numerical constants of the standard model of particle physics.

Or the spin of the electron (in units of the spin of the photon).

Spin, charge, mass, and anomalous dimensions are really quire similar – eigenvalues of generators of certain symmetry groups. Why shouldn't they be explain similarly?

20. **D R Lunsford**
January 26, 2006

Geon – it's doing a lot of "good". Before his evaluation paper and this blog, the problems of ST were still under the rug where they'd been swept years earlier, and they would have stayed there. What Peter is doing is an absolute "good" IMO.

-drl

21. **D R Lunsford**
January 26, 2006

Frisean – perhaps I was not clear – yes metaphysics and physics must be clearly distinguished! But, the latter cannot proceed until the former is settled. They off each other as progress is made.

-drl

22. **A.J.**
January 26, 2006

Quantoken —

There's a very good reason for Peter to censor his blog: He doesn't want it to degenerate into a forum for crackpots. Deleting advertisements for loony theories is good practice, just like deleting ads for porn sites. It keeps the signal to noise ratio high.

Likewise, deleting anti-string raves and content-free personal attacks on people who don't participate in these discussions (e.g. Ed Witten) is a good idea.

Deleting well-reasoned, on topic comments from people who disagree with him is bad practice, of course. But I haven't seen any evidence that Peter does this.

23. **anon**

January 26, 2006

Mark LaFlamme,

HILARIOUS post. Satire best way to exorcise Motl.

24. **woit**
January 26, 2006

I'm deleting a series of comments from someone who hoped that string theory would help him communicate with his wife after she passed away, and someone making fun of this. This is not funny, but sad. Please no more of this kind of thing here.

25. **Mark LaFlamme**
January 26, 2006

I hope no one feels I was a part of the earlier discussion involving the man who hoped to contact his dead wife. The novel "The Pink Room" is completely genuine and doesn't hold a position one way or the other on the reality of string theory. I have such a tenuous grasp on the science, I could be swayed one way or the other by a well placed argument. I'm enjoying the exchanges in here so far. Read about Mr. Woit's views last night in Discover. An amazing piece, really. And Discover typically seems to love string theory. It's a credit to them that they allowed such a dissenting voice.

26. **D R Lunsford**
January 26, 2006

Chris W – I did answer your question but it got deleted. I wasn't ignoring it. Perhaps you should post the (very intersting) question on SPR.

Or here's an idea – Peter could create sci.not.even.wrong and we could discuss it there. Or on alt.lubos.shutup.shutup.shutup.

-drl

27. **D R Lunsford**
January 26, 2006

Mark LF,

It's a credit to Peter that his clear voice is being heard even by sensationalists.

-drl

28. **xpinor**
January 28, 2006

Isn't the *10500* a remarkable tracer for the competence of science writers? Is it really unreasonable to expect somebody who writes about physics and string theory, in particular, to be able to recognize the origin of this number and recover the original meaning? What kind of judgment of the subject matter can be expected from anybody who isn't?

29. **woit**
January 29, 2006

xpinor,

In general I don't think you can blame the 10500 business on science writers. The problem is that lots of the software that magazines and newspapers use doesn't properly deal with sub/superscripts, thus the problem. Even this blog sometimes has this problem, someone is complaining in another post that their superscript appeared fine in a preview, but then got mangled when the comment appeared.

30. **Dr. Andre Bresges**
January 29, 2006

Dear colleagues,

I've got the printed Issue of the german ZEIT on my tabletop (this happens since I am a german physicist). I want to assure you that the 10500 typo is NOT in the printed issue, just in the online version.
Dr. Max Rauner studied physics in Boulder, Colorado and usually knows well what he talks about.
DIE ZEIT is a recommended weekly journal for "science, politics, culture and business". Its authors are usually domain experts, urged and assisted by the editors to use "plain language". Customers / target Group are decision makers in the fields aforementioned. Editors are e.g. the former german chancellor Helmut Schmidt and the former german minister of culture Michael Naumann. Congratulations, Peter.
Prof. Reiner Hedrich takes part in biochemistry and physics workgroups on a regulary base. His project is designed to "sort" the arguments in cosmology discussion, not to "bring order" to the discussion as SomeBody translates it (sounds different?).
Apparently the German Science Foundation considers the whole controversy "f****d up beyound any recognition" (never found that term so fitting than with cosmology:-) and asked someone to explain the state of the art in plain words before adressing politicians where to throw the research money into.
If you have any further questions concerning the translation or cultural background, feel free to ask. My email adress is found on my website.

André

Northeastern University Researchers Find Signs of Extra Dimensions

January 26, 2006
Categories: This Week's Hype

If you believe the headline of a press release issued today by Northeastern University, its researchers have found evidence of extra dimensions. The actual text of the press release tells a different story, that they haven't found evidence of extra dimensions. One can't blame the headline writer too much though, because the text itself is full of enough hype and nonsense about string theory and extra dimensions to confuse most people.

According to the press release text:

Researchers at Northeastern University and the University of California, Irvine say that scientists might soon have evidence for extra dimensions and other exotic predictions of string theory.

…IceCube, now under construction, could provide the first evidence for string theory and other theories that attempt to build upon our current understanding of the universe…

"To find clues to support string theory and other bold, new theories, we need to study how matter interacts at extreme energies," said Anchordoqui…

In recent decades, new theories have developed – such as string theory, extra dimensions and supersymmetry – to bridge the gap between the two most successful theories of the 20th century, general relativity and quantum mechanics…

Anchordoqui and his colleagues say that extragalactic sources can serve as the ultimate cosmic accelerator, and that neutrinos from these sources smacking into protons can release energies in the realm where the first clues to string theory could be revealed….

"String theory and other possibilities can distort the relative numbers of 'down' and 'up' neutrinos," said Jonathan Feng.

The half a dozen references to string theory in the short press release might lead the gullible to think that we're about to be provided with evidence for the "exotic predictions of string theory", but that has little relationship to the reality here, one aspect of which of course is that there are no "predictions of string theory" about any of this.

The occasion of the press release is the appearance in Physical Review Letters of a paper by Anchordoqui, Feng and Goldberg entitled Particle Physics on Ice: Constraints on Neutrino Interactions Far Above the Weak Scale. The authors discuss the possibility of using the difference between up and down observed rates for collisions of ultra-high energy cosmic ray neutrinos to get information about neutrino cross-sections at around 6 Tev center of mass energy, far above the energy scale for which we now have data about these cross-sections. They conclude that the data from the AMANDA array operating at the South Pole since 2000 already provide some constraints, and that IceCube, the next generation array now being installed there, could at 90% confidence level rule out a 40% enhancement of the neutrino cross-section over the Standard Model values.

What's interesting here is not that extra dimensions have been found, but rather the opposite. AMANDA results show no evidence of the kind of enhanced cross-sections you might expect from some extra-dimensional scenarios, and it seems possible that IceCube will rule out such extra dimensions at energies accessible by the LHC even before the LHC comes on line. For a similar but earlier argument of this kind, see a discussion by Jacques Distler a year and a half ago concerning an earlier paper by these same authors that argues that the Pierre Auger Observatory, another cosmic ray observatory now taking data, may also be able to rule out extra dimensions observable at LHC energies before the LHC is turned on.

There's also some mention of this over at Lubos Motl's blog, with half the posting devoted to scatological attacks on this blog and its readers. I really think he's losing it. Note that following current arXiv policy, a trackback linking to Lubos's posting has appeared at the arXiv listing for the Anchordoqui et. al. paper, but no such trackback will be allowed to appear to the posting you are now reading.

Update: One of the problems with the endless number of absurdly overhyped press releases about string theory is that they get widely distributed.

Update: The Slashdot article does contain a useful extended comment from someone working for AMANDA/IceCube.

Update: The headline on the press release has been changed by the people at Northeastern. It now reads "NU researchers say South Pole detector could yield signs of extra dimensions ".

Comments

1. **SomeGuy**
   January 26, 2006

   "There's also some mention of this over at Lubos Motl's blog, with half the posting devoted to scatological attacks on this blog and its readers. I really think he's losing it."

   Look at his replies to Quantoken…

2. **woit**
   January 26, 2006

   I did. Scary to see Quantoken making a lot more sense than Lubos.

3. **Christine**
   January 26, 2006

   I must say sometimes reading this blog is so amusing.

   One the serious side, concerning these experiments and the search for extra-dimensions, see this interview dated April 2004 with Dr. Feng (sorry if that has been posted before).

   Christine

4. **woit**
   January 26, 2006

   Thanks Christine, that interview does provide some interesting context for this.

5. **A. nonymous**
January 26, 2006

There's something that I don't like about when headlines which say "X has been found" are followed by stories which say "X has not been found". It strikes me as dishonest, in a very precise and obvious sense. Perhaps it was always this way and it's just being brought to my attention more nowadays, but I have the impression that this kind of thing occurs more frequently now than it ever has in the past.

6. **Lubos Motl**
January 26, 2006

Dear Peter,

Quantoken makes much more sense to you than I do because your understanding of physics is much closer to Quantoken than to mine.

Best
Lubos

7. **blank**
January 26, 2006

It's like they hired Ari Fleischer to start writing physics press releases.

8. **Quantoken**
January 26, 2006

Lubos:
Clearly you can't make sense of your "prediction" except by deleting my last comment on your blog. In that comment I pointed that your so called string prediction:

Predictions from TeV-scale excited strings:

http://arxiv.org/abs/hep-ph/0204112

are merely groundless wild guesses. You first have to **fix** the string scale at certain arbitrary value, exactly 1TeV, before you can calculate something. But then no one from your camp is willing to tell us un-ambiguously exactly how much is the string scale, 1 TeV, or more, or less? As such you could **always** wait until you get the data and then tweak your parameters to make it fit, and thus always claim victory regardless of the outcome.

As a matter of fact, you are already hyping a victory of super string theory with champaignes, before the experiment even begins 🙂

That is NOT a prediction. A scientific has got to be falsifiable. You tell me some specific numbers and put them on the table, **with no strings attached**, and then if the experiment does not come up with the number you predicted, you admit you were wrong. And you claim victory if your number is proven correct. You are unable to make even one such definite prediction, so you are not science.

Of course, it will be impossible for super **string** theorists to make a none-ambiguous prediction with **no string attached**.

Quantoken

9. **A. nonymous**
January 26, 2006

It's true – Lubos didn't have a good answer for Quantoken, so he deleted the comment. Surely even Lubos must be ashamed of this kind of sore-loser behavior. Is the shame of losing even a single point in an argument just too much to bear? Must he resort to censorship to prevent it?

10. **X**
January 26, 2006

Just an opinion – discussion of someone's personality on a blog that is mostly about ideas is not good. Surely we can discuss – if it is interesting – Motl's ideas on climate, physics, string theory etc., without discussiong Motl? Or for that matter, anyone else? I think it best that personal opinions about people remain just that – personal and private. Physicists – even when they blog – are not public figures in the nature of politicians and glitterati. The latter depend on getting people talking about them, but not so with physicists. I find comments like "Motl is such-and-such" or for that matter, our host "Peter Woit is such-and-such" rather distressing; I hope I'm not alone in this.

11. **Quantoken**
January 26, 2006

Peter:

You commented that IceCube could produce some results before LHC is turned on. That's incorrect. The experiment hasn't even been built yet by the time LHC is expected to be turned on. So far they have only installed 8 detection strings, out of a total of 80. The completion will be at least a few years away. Even when it's all set up, they expect to run the thing for **15 years** to collect just a few events.

See how the string theorists are already celebrating that extra dimentions have been verified, when in fact the experimental instrument has not even been built! Let's wait until it's all up and running, and an unfortunate matching penguin dropped his egg and tripped on the wires, triggering a false event record, I bet they will not hesitate to open up quite some champaigne bottles. Just look at how they hyped on the so called CSL-1 anormalcy.

Now as I point out on Lubos's blog, the author's estimation of event count, (4 down event and 20 up event), based on 15 years of instrument running, is way too optimistic and completely off the mark. In calculating the event count the author assumed **6×10^38** target nucleons, for example. Even a 3 year child can calculate that one cubic kilometer of ice contains only 9×10^37 nucleons, not 6×10^38.

Why researchers lack such basic skills to even estimate how many atoms are in one cubic kilometer of ice?

Also, when some one do an experiment like that and you know he has a full agenda to obtain a certain outcome that he prefer to see. How would you think about the credibility of such experiment result, before it even started? He is already going every where and give interviews claiming how extra dimention is confirmed! He probably have the experiment report written already, munus filling in the date and time, before the machine is turned on. How could you trust that he will not manipulate the actual data some how to fit his expected outcome, like Eddington did?

Quantoken

12. **Undergrad.**
January 26, 2006

I'm only an undergraduate student (in Europe), so I'll accept the fact that most people on this blog know far more physics than I do.

That said, I stumbled on to this blog some months back, because I wanted the other side of opinions on String Theory.

One thing I wish to say is about Lubos Motl. I am literally shocked after reading his posts that this man is a professor in a major university.
Lubos, you act like a five year old. Your obviously a very intelligent man, but you do yourself a disservice with your highly reactionary writings across the net.

If there is one thing you've shown me it's that adults who revel in "playground politics" make their way into the highest levels of acedemia.

Finally, if you respond to this, at least come up with an insult that supersedes the "U r gay" classification.

Great blog Peter and thanks for directing me to modest theoretical physics and away from exotica.

13. **blank**
January 26, 2006

Given that string theory gives something like 10^500 predictions for any given experiement, Lubos figures the odds are pretty good that one of those predictions will be verified. Predicting which of the 10^500 predictions will be verified is an uninteresting task left for lesser minds.

14. **A. nonymous**
January 26, 2006

Undergrad: It takes a lot less to be a professor in an American university than in a European university. In Europe, you have to hold a specific chair, and that often means waiting for another professor to retire. In America, "professor" is little more than a lob title, more or less equivalent to "researcher" or "lecturer". That said, a job as a professor at Harvard was considered quite prestigious before they gave one to Lubos.

15. **Peter**
January 26, 2006

Quantoken,

You're quite right that it will take longer than I thought for IceCube to be completed, not till 2009-10. However, between now and then I believe AMANDA will still be running, and taking data together with increasingly large parts of IceCube as it is completed in stages. I haven't really looked into whether and when these South Pole experiments will be able to put the kinds of bounds on neutrino cross-sections needed to rule out various extra dimension scenarios at various scales. Maybe someone reading this blog knows the answer to this.

On the other hand, your constant belief that the people designing these experiments don't know what they are doing is kind of out of control. The idea that they're making elementary mistakes and intent on fabricating data or not carefully analyzing it is just ridiculous.

16. **Dumb Biologist**
January 26, 2006

Haven't there been a few rare recorded cosmic rays that were so wildly energetic they were dubbed "Oh My God" particles? Did anyone notice those creating micro-black holes? I imagine if they did, we would have heard about it…though maybe no one was looking for that properly at the time, so it passed undetected. If not, wouldn't they put a pretty high-level constraint on micro-BH production? Like millions of times more energetic than what we can produce in the LHC kind of constraint?

17. **Dumb Biologist**
January 26, 2006

Oh, that was kind of in response to Christine's link…

18. **Quantoken**
January 26, 2006

Peter:

I believe you should be able to calculate the number of atoms in one cubic kilometer of ice. Any one can do that calculation. But they got the number wrong by 7 times. Would you not call that an elementary mistake if one can't even count atoms?

It's unbelieveable, but many researchers are incredibly stupid. Example like this. A researcher tried to figure out how much cloud is covering the earth, but looking at the dim reflection on the dark side of the moon. Some one with average intelligence would know it's much easier just to look at the earth directly from satellites.

And these few days some researchers claimed on the Nature magazine that they discovered an earth like planet 25, 000 light years away, but detecting brief brightening of certain stars due to gravity lensing. And Mark Troden et al appraised it. Some one know a little bit of GR should at least be able to estimate the focal length of the allerged gravity lense, and find out that it could not possibly focus the light on the earth 25,000 light years away, but the focal point where you can see the brightening effect should be much closer. See my comment here, which was by the way censored by certain cosmologists. Some people really just don't know the stuff they are supposed to know in their profession.

To a researcher nowadays, I guess publishing paper and being able to survive in academy, is way much more important than maintaining some decent scientific honesty. Outright fabrication of data may be rare and exceptional, but dirty tricks in data manipulation are widespready. When you have collected a rare data sample which you are not sure whether it is just background noise or legitimate, and you have not collected much data in 15 years, and the data seems to be a "good" one adhering to your expected result, I bet most people will be VERY inclined to simply count it in, instead of discarding it, especially when is no one watching over your shoulder.

The bottom line is scientists are as human as every one and they do pee on the streets, when no one is watching. The important thing is one should always maintain a healthy skepticism against everything and cross-exam things in order to find the scientific truth.

19. **Peter**
January 26, 2006

Good question, Dumb Biologist.

Don't know if this is what you were referring to, but since 1972, there have been some very high energy cosmic ray events reported with anomalous characteristics, called "Centauro events". There was some speculation these were black holes, I haven't heard anything about these recently, and am also curious what is now known about them.

20. **Peter**
January 26, 2006

Quantoken,

Sometimes you write sensible things, other times, like this, you behave like a complete crackpot. I'm so baffled I can't quite bring myself to delete your latest nonsense. But no more, please!

21. **Dumb Biologist**
January 26, 2006

Well, being very unoriginal, I just googled it, and got this:

http://en.wikipedia.org/wiki/Oh-My-God_particle

According to a link from this wiki, the original OMG was a proton with the mass of a bacterium.

There might be more on this stuff of greater technical interest to you via the Hi-Res Fly's Eye, that might answer your questions. They link some talks that I probably can't understand:

http://www.cosmic-ray.org/

22. **anon**
January 26, 2006

undergrad,

"Great blog Peter and thanks for directing me to modest theoretical physics and away from exotica."

Sorry to hear that you are letting this blog affect the direction of your career. It is a sad reminder that this small nuisance that I believe most of us think little of (at least I do aside from the occasional comic relief that it provides) actually has sway over people who don't see it for what it is: a gathering place for individuals who have discovered that it is far easier to obtain a sense of intelligence by attacking the accomplishments of others than to learn a subject thoroughly and make a serious contribution themselves.

The constant attacks on string theory that take place here are based on a very superficial knowledge of the subject and display an amazing level of ignorance. I do not mean to denigrate the intelligence of those who make such attacks, as there is a difference between ignorance and stupidity. Rather, it appears to me that the attackers simply have not taken the time to learn the subject. That is certainly not a crime as I, for instance, have not taken the time to adequately learn LQG despite the fact that it is a subject that one might argue is quite relevant to my career, more so perhaps than string theory is to many of the posters here. On the other hand, I don't spend my time publically attacking LQG based on the superficial knowledge that I do have. I recognize that I lack credibility on the subject and leave the debate of LQG's problems and merits to those whose interest is sufficient that they took the time to thoroughly understand it.

Finally, I should mention that you might take a look at the recent posts of Lubos and Hmm in order to put this blog into context. Both of these individuals are a bit abrasive to say the least (I see that you have noticed this about Lubos already 🙂) but the main points that they convey are actually quite accurate.

23. **A. nonymous**
January 26, 2006

Peter, DB – this is just a rumor, and I'm not saying where it came from, but certain people believe that the impossibly high-energy cosmic ray events seen at AGASA weren't actually as high-energy as it was originally thought, and the energies looked artifically high due to miscalibration of instruments. That leaves only one event at the Fly's Eye detector, and one event alone isn't enough to justify all the excitement. Certain people were asked not to say these things too loudly at a time when it might have jeopardized funding for the Pierre Auger observatory.

Time will tell. If certain people are right, then there will be no more cosmic rays observed with energies beyond 10^20 eV.

24. **Peter**
January 26, 2006

D.B.,

The kind of events seen by Hi-Res are the ones that Auger is designed to study, gathering a lot more events in order to understand what the flux of these things is at the highest energies. There's a controversy here, since above a certain energy, such cosmic rays from extra-galactic sources should lose energy by collison with CMB photons. If they're really there, something interesting in going on, one proposal is that Lorentz invariance gets modified, with a modified dispersion relation at these energies. Within the next few years, data from Auger is supposed to resolve this.

I still don't know though what happened to the Centauros…

25. **D R Lunsford**
January 26, 2006

DB – yes there are cosmic rays that are so energetic that nothing known can account for them. They aren't little black holes, just ordinary matter moving very, very fast.

Now if you look at galaxies, there is all kinds of evidence of extraordinarily energetic processes, fitting into characteristic patterns (rings, jets, ..). Most likely whatever is ripping these galaxies to shreds is also making these ultra-energetic cosmic rays. One doesn't need to impute some new pathological state of matter – just explain why these galaxies are so disturbed, and with such a consistent morphology. Science used to be like this – look, measure, speculate, theorize, calculate, confirm.

(The standard answer is "supermassive black holes". Basically, anything that can't be immediately understood is thrown over into the catchall of black hole dynamics. It's instant gratification for academics.)

-drl

26. **Michael**
January 26, 2006

Peter, you and Quantoken behave like an aging married couple. First, you publicly defend him. Then, in the privacy of your own blog you chastise him.

I think you two are made for each other, so be nice. Emphasize things you have in common rather than disagreements. For example, you could try and figure out if the off-diagonal SU(2) is compatible with the GUITAR. Good luck!

27. **Peter**
January 26, 2006

Hi anon/Hmm,

Funny how yesterday there was one anonymous coward Lubos supporter from the Boston area writing in, but promising not to come back, now there's a new anonymous coward Lubos supporter from the Boston area appearing here with the same personal attacks, made in much the same style. What's your excuse for remaining anonymous? Are you also a very busy man like "Hmm", too busy to explain why you are attacking me?

Whoever you are, your behavior is even more pathetic than that of Lubos.

28. **Dumb Biologist**
January 26, 2006

OK. Well, if the OMGs were really spurious readings, then that's a bummer. Let's just say they're not (which no one is claiming on-the-record, it seems), and that cosmic rays (probably protons) can whip around at these absurd energies. Where they come from is certainly interesting, but what I wonder is *why* people talk about detecting the decay of micro-BH's (and hence evidence of large extra dimensions) in the atmosphere rather frequently, or even in the LHC if we're lucky, when it would appear the most energetic cosmic rays detected rule out such observations.

It would seem, based on the above, the only way they can exist is if relativity needs modification in a rather radical way at high energies, and perhaps they don't believe it yet.

29. **Michael**
January 26, 2006

Not that it matters the least bit, given that anonymous is anonymous, but I'm not "Hmm".

30. **Peter**
January 26, 2006

Well "Michael", who knows if you're "Hmm" or "anon". But, like them I know you're in the Boston area. Maybe Boston is just full of lots of cowardly Lubos supporters who are very busy.

Actually, looking at the IP addresses, it looks to me like "anon" is a Harvard undergraduate, possibly living in Lowell house. Having done my time there, this conjecture seems all too consistent with the pompous tone of the comment.

31. **Michael**
January 26, 2006

Well, Peter, *I* know who I am. 😉 What does my IP-address look like?

32. **Peter**
January 26, 2006

Michael,

You're a lot cruder and stupider than the conjectural Lowell House undergrad. What's your excuse for remaining anonymous? As far as I can tell, you're at home somewhere in the Boston area using a DSL connection.

What's going on up there around Boston? Is it something in the water?

33. **A. nonymous**
January 26, 2006

> what I wonder is why people talk about detecting the decay of micro-BH's (and hence evidence of large extra dimensions) in the atmosphere rather frequently, or even in the LHC if we're lucky, when it would appear the most energetic cosmic rays detected rule out such observations.

There have only been about 20 or so putative events observed so far beyond the cutoff, which is about 5×10^19 eV. You might need to observe millions of incoming highly-energetic cosmic rays before you'd see a black hole being created. Of course, nobody knows how many events you'd need or what energy they'd need to be at; it depends on factors like the size of the extra dimensions. Larger extra dimensions should make black hole formation easier, and so on. You might need energies hundreds of times what has been seen so far, and, unless there's a source of ultra-high energy cosmic rays right beside us, scattering by the CMB should leach energy from the rays so that by the time they reach us, they're below 10^20 eV.

Incidentally, Peter, didn't Lubos accuse you of sock-puppetry not so long ago? I wonder if people are more inclined to suspect others of crimes when they commit those same crimes them themselves.

34. **Peter**
January 26, 2006

A. nonymous,

Thanks for the information about ultra high energy cosmic ray events.

Certainly Michael=Lubos is not impossible. I never did figure out what was up with Lubos and his strange idea that I was "MathPhys", quite possibly you're right that he thought that because of what he is up to.

35. **Michael**
January 26, 2006

Thanks, Peter. Yes, I am at home in Boston using my DSL connection. 😆 I laughed out loud when I saw that you actually answered my question, as if I didn't know… On a second thought, I think it makes you look like a nice guy. Kind of guileless, I guess.

What's my excuse? I don't need an excuse. This whole blog thing of yours isn't really serious in my mind. I don't want to give my good name for this little bit of pastime bickering.

Don't be too bitter about it, please. It's not even my intention to offend you too much — well, I sure enjoy bantering you a bit. You must have known that you can't run this kind of blog without facing some headwind, right? So please save the "all Boston is tainted" comments, will you?

36. **Michael**
January 26, 2006

No sir, I'm not Lubos either. 😆

Good lord, had I known I'd cause such a severe case of paranoia, I might have surfed elsewhere…

37. **Peter**
January 27, 2006

Michael,

No, I didn't think you were Lubos. His style is quite distinctive and anonymity isn't his thing. And no, I'm not the bitter sort, not in general, and certainly not at the moment, when life is quite good for many reasons. I didn't say anything about "all Boston is tainted", you string theory partisans have this mania for putting quotes around words I've never written.

Actually I'm quite fond of Boston. I greatly enjoyed the years I spent living in Cambridge, as a student and later in life. I have old friends there and my brother and his family live just outside Boston, so I visit regularly. Still the fact that a metropolitan area of only 2-3 million people contains more than one person as delusional as Lubos about string theory seems quite remarkable. There must be some sort of explanation….

38. **Michael**
January 27, 2006

Peter, it's not a coincidence, of course. Boston has the largest per capita density of high class academics in the world. This is particularly true for string theory.

Good night now.

39. **anonymous**
January 27, 2006

Peter,

These string theorists REALLY despise criticism, surprising for so-called scientists. You should have seen this "Michael" type anonymous behaviour coming a mile away. When you initially started the blog, I recall that the physicists that were critical were not anonymous (e.g., the fool Mark Srednicki of UCSB made of himself, Distler's moronic rantings, http://www.math.columbia.edu/~woit/wordpress/?p=3#comments, Motl of course, and others)

Once, however, having tried arguing and then realizing that they can't deal with the arguments, the coming of ad hominem and anonymous attacks were as obvious as the coming of men in white coats to escort Lubos to a mental clinic.

I think these type of attacks are a sign of success. You're quite a gadfly!

40. **a**
January 27, 2006

This time the controversy between Quantoken and Motl is about physics, so it can be clarified without insults. They miss that IceCube can also see events orginated below the instrumented region, such that its (energy dependent) effective volume can be bigger than 1 km^3.

As a general comment, I think we should pay more attention to people planning, building and operating experiments, rather than to people writing papers and funny press releases.

41. **woit**
January 27, 2006

> Boston has the largest per capita density of high class academics in the world. This is particularly true for string theory.

Somehow "high class" isn't really the term that comes to mind to describe Lubos and his anonymous fans….

42. **Thomas Larsson**
January 27, 2006

Boston has the largest per capita density of high class academics in the world. This is particularly true for string theory.

Population of Princeton, NJ: 14,203
Population of Boston, MA: 589,141
Population of Cambridge, MA: 101,355

So there are more than 41 times as many prominent string theorists in Boston than in Princeton?

43. **Quantoken**
January 27, 2006

a:

Wrong. First, the instruments detect visible photons. However clean the polar icea may be, lights do not penetrate much more than a hundred of so distance. Second the goal of the instruments is to correlate several detectors to identify the direction the muon is going, therefore inference the direction of the original neutrino. It can determine the direction only when the event is happening within the ice of the IceCube.

44. **Quantoken**
January 27, 2006

Peter says:

"Somehow "**high class**" isn't really the term that comes to mind to describe Lubos and his anonymous fans…. "

I would say **high** is the perfect word describing the status of super string theorists like Lubos et al. They are too high in the thin air they can't make their feet touching the solid ground of reality. Of course you know what the word "high" is often associated with when it comes to certain substance.

I prefer to stand on solid ground. Let some one else high.

Quantoken

45. **a**
January 27, 2006

Quantoken: muons (not photons) can travel for kilometers in ice. And by detecting a part of their path one can reconstruct its direction.

46. **Nick**
January 27, 2006

Quantoken's CosmicVariance Blog spells out his problem with the number of nucleons estimated in the ICE-CUBE paper. It seems the blog asserts that the water molecule has 3 nucleons, instead of eighteen which is the correct number. (You have to count all the protons and neutrons, not just the nuclei.) This accounts (almost) for Quantoken's problem that the estimate given in the paper is 7 times too big.

47. **J.F. Moore**
January 27, 2006

I had to check it for myself since I couldn't believe someone could make such a mistake while boldly chastizing others as apparently being below the toddler level in thinking. But you're right, Nick, it's there. "The water molecule contains three nucleons…." Wow. Just incredible.

48. **woit**
January 28, 2006

Please, no more discussion of Quantoken's mistakes about physics, this is far too large a topic and could easily overwhelm this poor blog.

49. **sunderpeeche**
January 28, 2006

And now for something completely different (as they say on Monty Python) … trackbacks. It seems that indeed the arxiv does not post trackbacks to this blog. But never mind. It seems that this blog has attracted sufficient prominence to garner interviews for Peter etc. I am reminded (perhaps for no good reason) of the blurb for Monty Python and the Holy Grail = "makes Ben Hur look like an epic". Why worry about trackbacks? Make it a subtitle "Not Even Wrong" ~ "Not even trackbacked by the ArXiv" (trackedback?). Trumpet it to the world with pride! It puts the ball in their court.

50. **woit**
January 29, 2006

sunderpeeche,

I kind of agree with you. But what is really weird about this story is that I can't even get anyone at the arXiv to admit to me that they have decided not to allow trackbacks here.

51. **Tony Smith**
January 29, 2006

Peter said "… I can't even get anyone at the arXiv to admit to me that they have decided not to allow trackbacks here …".

My memory is quite fallible, but I seem to recall from an earlier blog entry or comment that someone promised Peter that a representative of the Cornell arXiv would contact him about the trackback situation during this past week. Did that not happen? If it did not happen, was a reason given for the failure to contact and explain?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

52. **sunderpeeche**
January 29, 2006

As I recall, there was a blog entry about trackbacks (or lack thereof), maybe 1 month ago.
a) Peter said he had made (numerous) polite requests to have trackbacks from the arXiv, to no avail.
b) Peter also said someone at Cornell promised someone at arXiv would look into the matter and respond. Evidently there has been no reply.

And so today " … I can't even get anyone at the arXiv to admit to me that they have decided not to allow trackbacks here."

Let's be careful about this.
"Not even trackbacked by the arXiv" is a statement of fact.
It even (potentially) embarrasses the arXiv.
Not the same as "Blacklisted by the arXiv" (choose some other milder word).

What attitude to project here? My 2 cents worth is "I (Woit) have a blog which has attracted considerable attention. It is respected. If the arXiv chooses not to include trackbacks to this blog, it's their loss not mine. My blog can stand on its own merits."

But it's not my decision to make. Just an opinion.

53. **woit**
January 29, 2006

Tony and Sunderpeeche,

The person from the arXiv who was supposed to contact me last week didn't. I'll follow up this week, although this issue is not at the top of my To Do list. Sure, my attitude is pretty much the one Sunderpeeche recommends, but I also would like to see the arXiv management and advisory board take some responsibility for what they are doing. My suspicion is that I'm not hearing from them because at least some of the relevant people are not comfortable backing what I'm assuming is Distler's decision that the arXiv should not allow trackbacks to a blog that is too critical of string theory.

54. **Dumb Biologist**
January 31, 2006

There's a New Scientist blurb on the subject:

http://www.newscientist.com/article.ns?id=dn8654

55. **woit**
January 31, 2006

D.B.,

Just looked at the New Scientist article. I especially like the quote from John Schwarz, who demonstrates the new version of the scientific method now popular among string theorists:

"If something non-standard is established, string theory has a long list of exotica that would provide candidates to explain it"

So, if this experiment does detect an elevated neutrino cross-section at high energies, I guess this will prove string theory…..

56. **Dumb Biologist**
January 31, 2006

I wonder how long the "long list" is. I also wonder how long the list has to be before the meaning of the word "prediction" is strained beyond meaning.

57. **D R Lunsford**
February 1, 2006

DB – In physics "prediction" doesn't just mean that a new type of material event is seen. It really can mean two other things:

1) A workable phenomenological hypothesis is given a sound theoretical basis within a new framework.

2) A hitherto unsuspected phenomenon is unearthed.

3) 1 and 2 are not independent

For example the Dirac theory satisfies all of this (spin, antimatter, statistics). There was a theory of spin which was taken directly from lab phenomenology, but antimatter had never been seen. In fact Dirac thought at first he'd explained the polarity of electron and proton. Antimatter was experimentally discovered some time later.

-drl

3-400 Pages?

January 27, 2006
Categories: Uncategorized

I'd been wondering what's up with Witten and his ongoing work on geometric Langlands. He has been giving talks about this since last summer, and in the past has always quickly produced a paper (often a quite long one) once he has some new result like this that he's publicly talking about. It had surprised me that it was taking him unusually long to get this written up, but now comes news from Anton Kapustin (via Lubos) that Witten is working on a document 3-400 pages long. This length would certainly explain why it is taking longer than usual, and surely the end result will be something quite interesting. The Kapustin rumor also claims that whatever this 300-400 page thing is that Witten is working on, it's not a paper. Mysterious… The obvious guess is that it will actually be a book.

Comments

1. **D R Lunsford**
   January 27, 2006

   Maybe it's the Lagrangian for string theory.

   -drl

2. **secret milkshake**
   January 27, 2006

   Maybe a series of related papers, back to back. It would be helpful if it was; new ideas are needed.

3. **A.J.**
   January 27, 2006

   Wow. That's getting into Grothendieck territory. I wonder if it's something about topological field theories: Cordes, Moore, & Ramgoolam's quote-unquote "lecture notes" are nearly 250 pages and are still sometimes painfully terse. And my advisor's new lecture notes on Geometric Langlands are about 150 pages.

4. **ObsessiveMathsFreak**
   January 27, 2006

   Not quite related to Sting Theory, but on the matter of scientific papers; Some papers are just far too long. A great many writers seem to be caught up in *how* they are doing something rather than *what* they are doing. Appendices are fine, but I for one would rather that papers' main bodies be more succint than they currently are.

   A 300 page paper is clearly going overboard. I'd imagine 90% of the potential audience will be daunted and over 50% of those who begin will turn away after a few brief forays.

   Appendices for dull, but important, stuff, like large data sheets, mathematical derivations, etc, etc. But flashy diagrams go in the main text, naturally. And for heavens sake *put comments on your mathematical equations!!* A series of mathematical equations just thrown down is like a sequence of uncommented computer source code. i.e. impossible to work with. Tell us what, how and most importantly *why* you're doing something.

5. **D R Lunsford**
   January 27, 2006

   Peter, RE this comment

   The idea seems to be to use a TQFT given by a twisted version of N=4 supersymmetric Yang-Mills… Then one does dimensional reduction using as 4-manifold a Riemann surface times the upper-half-plane, and ends up with a sigma model of maps from the upper-half-plane to the Hitchin moduli space of flat connections on the Riemann surface. The boundary degrees of freedom are branes, and the S-duality of the 4-d theory is supposed to give a duality at the level of the sigma model that corresponds to the fundamental duality one is trying to understand in the geometric Langlands program.

   This sounds very impressive but I have no idea what it has to do with physics. By sigma model do you mean Kaehler metric? Or something like the Higgs mechanism? -confused-

   -drl

   -drl

6. **ksh95**
   January 27, 2006

   "By sigma model do you mean ….."

   I presume Peter means the same sigma model that's in every field-theory/critical-phenomena book ever written.

7. **woit**
   January 27, 2006

   drl,

   The sigma model is just a 2d qft involving fields that are maps from the upper half plane (that's your space-time, if you like) to a complicated space, the moduli space of solutions to a 2-d (different 2d!) gauge theory coupled to a Higgs field.

   This subject looks quite interesting, but also quite complicated. I'd kind of decided not to spend more time on it until Witten's paper came out, figuring it was a waste of time to try and figure out the details of this based on fragmentary notes of his talks, when a complete paper would soon be available.

8. **MathPhys**

January 27, 2006

He’s probably wrting a Lecture Notes in Physics, or Asterisque type monograph.

9. **Xerxes**
January 27, 2006

Just wanted to chime in with an opposite opinion from ObsessiveMathsFreak. I love long papers. People spend so much effort in papers trying to make themselves look very clever at the expense of clarity. If it takes you 100 pages to say clearly what you could have said succinctly in 25 pages, am I going to charge you for the extra bandwidth those 75 pages took to download? No, I’m going to shake your hand for explaining things clearly.

Besides, if I already know the material and just want to cut to the chase, I can always skip over to what I want to see using the table of contents. Can a reader who doesn’t know the material already fill in the blanks from a shorter paper? Nope, he’s out of luck.

10. **blank**
January 27, 2006

Actually, he’s working on a new book for the popular audience:
“Warped Personalities: Susskind, Motl, and the Illusion of Intelligence”

11. **xpinor**
January 28, 2006

[tex] 3^{400} [/tex] Pages?

12. **mitchell porter**
January 28, 2006

3400 pages?

So he went overboard on the case-by-case analysis of vacua…

13. **mitchell porter**
January 28, 2006

That’s odd, the preview showed the 400 as a superscript, as I had intended.

14. **secret milkshake**
January 28, 2006

I wonder if there is a technique that could help a bunch of thinking people to avoid commenting on the stuff published by Motl. It is like the ankle itch – it gets worse by frequent scratching it. Unless causing more inflammation is the purpose of this blog, I would suggest more self-discipline

15. **woit**
January 29, 2006

I fear that I just find Lubos too entertaining and his behavior too perfect an example of what is wrong with the way string theory is pursued for me to suggest that people ignore him.

16. **Tommaso Dorigo**
January 29, 2006

Hello,
just a note to say thanks for linking my site… I am doing the same!
Cheers
T.

17. **Kielbasa**
January 29, 2006

Without Lubos, where would Peter find enough materiel for his blogg?

18. **woit**
January 29, 2006

Kielbasa,

This month, all I need is the NYT book review. See my latest posting.

19. **sunderpeeche**
January 29, 2006

“materiel” (as opposed to “material”) is the werewithal an army needs to prosecute a war …. non-native English (which I assume is the case here ~ “kielbasa”) is wonderful.

Macrame

January 29, 2006
Categories: Uncategorized

This month, the New York Times book review has been the place to follow the latest debates about what is going on in particle theory. This started with an essay by John Horgan on January 1, which drew letters to the editor from Lisa Randall on January 15, and Lawrence Krauss and Leonard Susskind on January 22 (this last letter was discussed here).

The January 15 issue also had a not very positive review of Susskind's recent book (discussed here). In today's (January 29) issue, Burton Richter has a letter commenting on and contrasting the recent work of Randall and Susskind. He's positive on Randall, since he sees her as trying to come up with testable predictions, but about Susskind he has the following to say:

Susskind and the Landscape school have given up. To them the reductionist voyage that has taken physics so far has come to an end. Since that is what they believe, I can't understand why they don't take up something else — macramé, for example.

Richter is an emeritus Stanford professor, ex-director of SLAC, and won a Nobel prize in 1976 for his role in the "November Revolution": the discovery at SPEAR in November 1974 of the "Psi" particle, a bound state of a charmed and anti-charmed quark (also found by a group at Brookhaven led by Sam Ting, who called it the "J" particle). Since he is emeritus, presumably Richter doesn't attend Stanford physics department faculty meetings anymore, which is too bad, since I for one would love to see Susskind, Richter and Robert "string theory is like a 50-year old woman trying to camouflage her flaws by wearing way too much lipstick" Laughlin debating department hiring policy.

On the same page as Richter's letter, there's an ad for a book called "Reality Check", by David L. Weiner. I don't know anything about the book but the advertising blurb goes like this:

It turns out that the ape-like mechanisms that remain in our brains not only can create mental turmoil if we don't meet their primitive expectations, but their penchant for pecking order and status can create far-out realities we think are absolutely true. These may cause us to inflict unwarranted harm on others, limit our own potential, or both.

Seems to me this book might explain some of the reaction to the recent interview in Discover magazine.

Comments

1. **CarlB**
   January 29, 2006

   About those Centauros…

   There was little mention of them last year. The most interesting parts of them are the apparent tendency to have extreme transverse momentum and to spread in lines. These two things are exactly what one would get if one had a superluminal cosmic ray. That is, unlike a regular cosmic ray, the debris would not stay with the lead particle but would spread out as the speeds differed.

   There is another aspect of cosmic rays that can be explained by superluminal lead particles, and that is the mystery of why AGASA keeps finding excessive energies for UHECRs as compared to the other experiments.

   It turns out that AGASA measures energy by creating electrical pulses whose height is proportional to the deposited energy. Then the pulse is allowed to decay through an RC time constant. The result is a pulse length proportional to the log of the energy, and so they measure the pulse length. A superluminal cosmic ray will naturally produce longer shower pulses by retriggering (i.e. the lead particle gets there first, and the earlier produced showers show up later). This will spoof the AGASA electronics to measure excessively high energy. There is another cosmic ray experiment (Russian, if I recall correctly) that uses similar RC decay electronics, but has circuitry that prevents retriggering.

   I think the best theoretical explanation for the Centauros, in the context of current physics, is that they are outliers in the statistics of pion production. But that doesn't explain the tendency for them to have extreme transverse momentum. Oh, and they tend to react more strongly than protons, which some say is caused by them being heavier nuclei.

   There is a recent theoretical paper saying that UHECRs are superluminal, but they didn't note the Centauro evidence for this. I should send them a letter.

   Carl

2. **Wolfgang**
   January 29, 2006

   > the New York Times book review has been the place to follow the latest debates about what is going on in particle theory

   Clearly, the best place to follow the latest debates is "The Official String Blog", you can find the link on my blog.

3. **anonymous**
   January 30, 2006

   The last sentence of Richard's review spoils it all:

   "As a scientist and an experimental physicist, I am rooting for Randall."

   This gives a rather strange impression to the public: both are equally valid and reasonable approaches and its just my preference to "root" for the later one.

   It lacks any firm punch whatsoever. Sort of what american fundamentalists would have everyone believe in biology: there is evolution and there is intelligent design, and we're rooting for evolution.

   This way of putting it almost validates the intelligent design side.

4. **anonymous**
   January 30, 2006

   Oops, I meant Richter.

5. **fh**
   January 30, 2006

   The punchline is before, “Macrame”. I think he leaves no doubt whatsoever that Landscape is giving up.

6. **anonymous**
   January 30, 2006

   Though I love the sentence:

   “Susskind also is a believer in extra dimensions.”

7. **Who**
   January 30, 2006

   agree with anonymous about Richter’s phrase
   “a believer in extra dimensions.” factual but a nice ring to it.

   Two months till April, when Peter’s book comes out.
   Lucky timing, with the spotlight these days often on
   displays of string inanity, untestability.

8. **woit**
   January 30, 2006

   Who,

   I see that the book is listed on some sites as coming out in April, but I was told publication date in the UK is June 1. It should be ready to go by April, but I believe that things have been slowed down a couple months to keep the time lag between the UK and US publication shorter.

New String Theory Blog

January 29, 2006
Categories: Uncategorized

Thanks to Wolfgang Beirl for the news that there's an exciting new string theory blog, called The Official String Blog.

Comments

1. **Thomas Larsson**
   January 29, 2006

   Comments by Peter Wrought, Lubos Bottle, and Jacques Distiller.

2. **secret milkshake**
   January 29, 2006

   I am sorry, this is not very subtle but malicious it is. This one is on level of adding a mustache to the picture of princess Di, or writing about Dildo Baggins in "Bored of Rings"

   A good parody takes on a serious tone + unperturbed jargon. Only after few paragraphs it should slowly dawn on uninitiated observer that something isn't quite right. Of course, the best part of a good parody is the confused reaction of people who got taken by it.

   Here is example of a good parody (from a completely different field):

   http://video.google.com/videoplay?docid=-7687402682106917664

3. **clandestine malt**
   January 30, 2006

   jeeez it's just for fun dude… string theorists are really on the defensive these days… (assuming you are a string theorist, that is–I don't know who else would have that virulent a reaction to a little joking web page)

4. **secret milkshake**
   January 30, 2006

   Maybe somebody who does not like borderline-moronic spoofs? (C'mon – you could do better than this, doode).

5. **clandestine malt**
   January 30, 2006

   okay–duly noted that all humor too silly for the milkshake is malicious.

   I wonder what Lubos thinks

6. **Thomas Larsson**
   January 30, 2006

   Milkshake, note that Gates, Rocek, Grisaru and Siegel are close to being string theorists themselves. They are at least closely involved in SUSY.

7. **secret milkshake**
   January 30, 2006

   Lllame is the word you were sarching for, Clarice. Lame and vitriolic.

   A reaction cannot become virulent but bad taste can. There is Webster to help you.

8. **secret milkshake**
   January 30, 2006

   T Larsson: you are right – yours is way better

9. **anon**
   January 30, 2006

   Are u sure it's a spoof? Check out the FAQ's:

   Q. Is string theory related?
   A. Related to what?
   Q. Related to the real world?
   A. What was your question again?

10. **fh**
    January 30, 2006

    Siegels Parodies vary in quality, his anthropic principle one is great for example, http://insti.physics.sunysb.edu/~siegel/parodies/misanthrope.html

    —

    This hidden argument we call the Misanthropic Principle. It goes something like this:

1. I can't solve this problem.
2. Therefore, you can't solve this problem.
3. Hence, this problem can't be solved.
4. So, it's got to be just dumb luck.

11. **D R Lunsford**
January 30, 2006

The Main Sequence of Researchers:

http://insti.physics.sunysb.edu/~siegel/parodies/graph.gif

"Experiments are a waste of spacetime, since nothing new will be seen till the Godzillatron is built" – Gall, *"Ideas and Opinions are Like A**holes, Everybody Has One"*

-drl

12. **Adrian H.**
January 30, 2006

For anyone who grew up with Mad magazines this qualifies as parody, and good parody at that.

But no one seems to have asked the important question, the question that Hmm would undoubtedly want to know the answer to: are Gates et al *allowed* to run a blog? Have they published enough? And what are their citations like?

I just hope they're not nobodies who have not received permission to speak.

Concerned from Tunbridge-Wells

13. **Luboš Motl**
January 30, 2006

Dear Peter,

your comments about the "landscape problem" have really no relevance for the calculation of the Yukawa couplings in the heterotic model of Ovrut et al.

The topic of your blog is to transform everything to the "landscape problem" – much like a student who has learned the birth of date of William Shakespeare and wants to transform every exam question about history and literature to the number 1564.

But one can't discuss any concrete physics questions in this way. Sorry.

Best
Bottle

14. **woit**
January 30, 2006

This is off-topic for the posting about a parody blog, but then again, maybe not, because Lubos and his blog increasingly seem to be a parody.

The scientific point at issue is about a very recent paper claiming to calculate Yukawa couplings in a specific heterotic string background. When Lubos wrote a post hyping this result, I wrote in a comment pointing that that the authors were ignoring the main problems with doing this: how do deal with the moduli and supersymmetry breaking. These problems are what lead to the landscape, since the only known way of fixing the moduli leads to exponentially large numbers of possibilities.

There was an exchange of comments, with Lubos displaying the usual string theory partisan mixture of insult, ad hominem attack and straw man argument. Faced with having this pointed out to him, he moved on to the next tactic: censorship. Here's the comment that he evidently had no answer for, so dealt with by deleting it:

"Lubos,

Your tactic is always the same: ignore the point I'm making (one that you know well is quite serious), then make up things I never said in order to use those to criticize me as ignorant. This behavior is stupid, dishonest, and highly scientifically unethical. You should be ashamed of yourself.

Let's look at how your dishonesty works explicitly:

1.
In this case, you are ignoring the problem posed by the moduli, and you are well aware that it is a deadly one. The paper in question notes
explicitly that fermion masses will depend on the moduli.

2.
You claim to show that I'm wrong and don't know what I'm talking about by writing:

"Nope. We are not choosing fluxes "in order to stabilize the moduli"."

I never wrote that "We are choosing fluxes in order to stabilize the moduli", I just said that the numbers like 10^500 that one hears for the size of the Landscape come from the number of choices of the fluxes, and these correspond to ways to stabilize the moduli. Do you know of a way of stabilizing all the moduli that doesn't involve this? If so, lets hear it. If not, acknowledge that you were dishonest to bring this up.

As for supersymmetry breaking, your claim that:

"marginal operators can't be classically affected by supersymmetry breaking"

is irrelevant. We're not doing classical physics here.

If you have any honest points to make, I'll respond to them, but I'm not going to waste any more time dealing with your dishonesty, interspersed with stupid, nasty, personal attacks. When you were a young graduate student, this kind of behavior was a bit amusing. At your age and stage of your career, it's just pathetic."

15. **Alejandro RIvero**

January 30, 2006

Theorema: ‘Skeftomai ara uparxo’, Lemma: nomizo oti skeftomai…

[meso tou Not Even Wronk]

16. **Juan R.**
January 30, 2006

Poll:

What is the more interesting comment to the conference on the future of string theory for you?

I am doubing between this

Bottle says:
9:20 AM
String theory is just as well proven as evolution. In fact, DNA is an open twistor superstring.

and this

Bottle says:
9:32 AM
Hey, can we stay on topic? Which is, “Global warming is caused by the cosmological constant.”

The deepd inside of bottle in global warming and related issues is fascinating but the open ‘twistor’ comment is really great!!!

—

Juan R.

Center for CANONICAL |SCIENCE)

17. **Christine**
January 30, 2006

Concerning the parody blog in question: funny and frivolous. (Now I am waiting for “The Official Loop Quantum Gravity Blog”…)

There is one thing that I would like to have. Peter Woit writes down his top 5 criticisms to string theory. Lubos Motl *objectively and concisely* addresses these 5 topics. After that, Peter and then Lubos may offer one final statement on their points of view. No personal attacks allowed.

I would like to have this framework fixed at the sidebar of my blog page. No, it is not supposed to be a joke. I really would like that.

Thank you,
Christine

18. **Dumb Biologist**
January 30, 2006

The blog is kind of funny, but, as mentioned above, it’s also blindingly obvious that this a joke. The best examples of parody dance gingerly about the “fine line between stupid and clever”, and never stray too far into either territory.

The Bogdanovs might help them concoct something more subtly ironic. It would be amusing to see who could be fooled.

19. **John Gonsowski**
January 30, 2006

Actually the interesting part technically is where Peter and Lubos agree (landscape is bad, things like AdS/CFT are not the real world, Witten is brilliant). Their disagreement is not so much where string theory is now but where it will be later and what needs to be done now to make things as good as possible later. Even if Peter was wrong about some particular string theory detail, who cares, smart people correct each other all the time, it’s the way science is supposed to work. Even if string theory is extremely close to finding the right answers they could spend hundreds of years not finding it cause of the way they get fixated on particular ideas. The parody has some truth about the way people can cluster around an idea not yet worthy of such clustering.

20. **MathPhys**
January 30, 2006

I heard that, more than 20 years ago, Warren Siegel used to say “If Ed became crazy, would anybody notice?”.

21. **Dumb Biologist**
January 30, 2006

Awright, after a bit more reading, some of this is damn funny.

22. **a**
January 30, 2006

What Siegel presented as parodies are real difficulties of strings and high energy physics. E.g. the “lobotomy” parody, which is older than the landscape, contains the main criticism to string theory:

“In 11 dimensions, you can predict things uniquely; in 10 dimensions, you can predict up to a factor 5 of uncertainty; but by the time you get to 4 dimensions you can predict any value you want for a result.”

One can find similar examples of masked criticisms in old published papers. The big merit of Peter is saying in public what many physicists think, but prefer to discuss only privately.

23. **blank**
January 30, 2006

Thank goodness nobody had ever told me that a "good" parody has to be so subtle that is doesn't even make me laugh.

Actually Siegel's parodies seem to be the very opposite of a "good" parody: they seem an obvious trivial farce at first, but the more you think about them, the more they seem to accurately capture certain realities of string theory culture.

24. **Dumb Biologist**
January 30, 2006

Yeah, I must admit, after giving it more than a cursory look, there's a good amount of wry cleverness beyond the blatant silliness, so I should withdraw my earlier comment.

The recommendation letter thing was laugh-out-loud funny…and not at all isolated to physics.

25. **D R Lunsford**
January 30, 2006

DB – the parody is also of the magisterial tone that runs through many papers – this makes it doubly funny for someone used to reading papers.

-drl

26. **Levi**
January 30, 2006

No way is this a parody.

The advice in the appendix on The Art of Giving Seminars in "How I Spent My Summer Vacation" is pure gold. Or consider this, from the Dictionary of Common Phrases in "The Nonabelian Names of God":

"We'll have to save further questions for the end of the talk." —– "You've found the crucial flaw in the theory, so give everyone a chance to run away before it hits the fan."

27. **Quantoken**
January 30, 2006

That was a funny web site. Thanks for the laugh. I always thought Jacques's last name as "Distiller" until I read it more carefully 🙂 But you got Lubos's last name wrong! It's not "Bottle", but **Mottle**, or, **Multi-Bottle**. M like the M in Multiverse, or M-theory. For string therists, it's not Universe, but rather **Multiverse**, so of course Lubos's correct last name should be **Mottle**, not Bottle.

28. **Dumb Biologist**
January 30, 2006

He is the best student we have had in as long as I have been here…He is not quite good enough for us to hire here, but he is certainly better than anybody you have there…He hasn't written any landmark papers yet, but then, neither have you.

Indeed, gold…and about as accurate a reflection of the subtext of such letters as has ever been offered. It's as if my advisor's mind were an open book to be read. 😉

29. **Dumb Biologist**
January 30, 2006

Randomly, I'm reminded of the more overt (albeit probably benign) contempt expressed in another recommendation letter…

Dear Einstein,
This student is good, but he does not clearly grasp the difference between mathematics and physics. On the other hand, you, dear Master, have long lost this distinction.

30. **Omni**
January 30, 2006

GUFFAW!! That site was a riot!!

I find it vaguely disturbing, though, to find highly educated people categorizing themselves as ANTI something; if you have what you believe to be a valid theory that's backed up by proof, why not call yourself an adherent of that theory, rather than being anti someone else's theory… and if you DON'T have your own theory, why not go quietly about your research until you DO have one, rather than making an issue of being anti?

I idolize theoretical physicists, but I have to say; this is childish and unworthy of you. The truth is out there; please stop nay-saying and finger-pointing and FIND it.

31. **Dumb Biologist**
January 30, 2006

I rather doubt everyone around here is being contrarian just for the sake of it. I personally think the ANTI-science forces running amuck out there have rather elevated the stakes in the age-old debate over what is science and what isn't. I've said here before, it's bad enough when science is attacked from without, but it's excruciating when it's corrupted from within. One needn't have a "better" alternative to an idea that is unscientific to have something positive to say. It might be enough to reassert the essential need for a scientific theory to be, at least in principle, experimentally testable. It's also essential that a scientific theory be falsifiable, so that, if and when it is falsified, one can even know when to abandon a faulty approach and do the very thing you suggest, which is find a better idea.

If a theory fails both qualifications, it's hard not to feel concerned. I'm not sure about all of Superstring theory, but it seems almost certain this Landscape business (and probably all anthropic arguments) are about as anti-scientific as it gets. Even if it's 100% true, we'll never know anyway, so why, as a scientist, would anyone bother with it? And if someone is promoting such ideas, in the capacity of a scientist, to an interested and admiring public, is it inappropriate for others to decry the harm being done?

32. **John Gonsowski**
January 31, 2006

There are people here with their own theories. Unfortuneately string theory acts like a monopoly does in the business world and makes it quite difficult for new ideas to take hold (even new string theory ideas). The string theory powers that be even seem to be actively keeping some of the ideas here out of the arXiv archive.

33. **D R Lunsford**
January 31, 2006

John – even Penrose is ignored. He gives the simplest conceivable argument showing that inflation, to take an example, not only doesn't fix what it's supposed to fix, it actually makes it worse*. The alternative for it is to resort to anthropism. Since no one seems to be willing to abandon inflation, we can only assume that anthropism has already taken a seat.

Penrose's argument can be understood by an undergraduate. So why doesn't it have a prominent place?

-drl

* http://www.princeton.edu/WebMedia/lectures/

Look for Penrose lecture "Fantasy"

34. **science**
January 31, 2006

String theory has 'queered the pitch' for physics ideas. Few people have the time or inclination to study the details of the maths of stringy M-theory. String theory hype says they can 'predict' gravity, the Standard Model, and unify everything.

This is equivalent to saying: 'all alternatives are unnecessary' or, more clearly, 'all "alternatives" are crackpot junk that is not science.'

Notice that the first line of defence ignorant people have against criticism of mainstream is to claim there is no alternative. When you point out that you are critical BECAUSE alternatives are being suppressed, they then sneer at the alternatives because they haven't had the level of funding of mainstream string theory for 2 months, let alone 20 years…

35. **Juan R.**
January 31, 2006

Lunsford,

And what about the Nobel Gell-Mann and his five brains?

He carefully claims that string theory even if finally correct will be not a TOE, and emphasizes that antrophic principles are either nonsense or trivial and still there is people stating in talks that string theory may explain everything or that anthropism is a revolution in science.

—

Juan R.

Center for CANONICAL |SCIENCE)

36. **Ignorant Layman**
January 31, 2006

I wonder what all you critics think of Johnathan Swift's fey Island of Laputa in Gulliver? Was it an unserious error in parody or a classic skewering?

37. **science**
January 31, 2006

Swift's parody on crackpot mainstream scientists went wrong. He portrayed the truth of what scientists of the Royal Society were up to, but it was so crazy people thought it was mad or exaggerated.

Attempts to chemically revert sewage into food, to store energy in cucumbers, and to train blind people to paint pictures, were really made. This is innocent compared to the claims made for strings.

38. **Ignorant Layman**
January 31, 2006

Faw! It all sounds like Alchemy to me.

39. **Christine**
January 31, 2006

Is it possible that the general public is starting to think that String Theory is silly?

Silly String Theory For Dummies.

Silly String Theory In Computer Science.

Silly String Theory In Artwork.

Silly String Theory, a Cabaret Performance.

[I do not mean that I think String Theory as a whole is silly, this is just meant a joke considering this parody thread].

BTW, I would like to know whether there are any papers with a lucid evaluation on the merits and demerits of string theory, published in a peer review journal?

40. **matt d**
January 31, 2006

String theorists remind me a lot of the Intelligent Design crowd.

That was a funny site, thanks Peter.

41. **John Gonsowski**
January 31, 2006

Danny, at least Penrose (and Peter) can get themselves into Discover Magazine so maybe there's some hope. Surprised Penrose didn't mention Paola Zizzi in relation to the ORish alternatives.

http://www.valdostamuseum.org/hamsmith/cosm.html

42. **woit**
January 31, 2006

Christine,

I don't know of anything quite like what you are looking for, perhaps the closest is Lee Smolin's hep-th/0303185. Not sure if it was ever published or peer reviewed, if so the comments from string theorists would have been interesting to see.

43. **D R Lunsford**
February 1, 2006

John,

Penrose's lecture was fascinating. His bizarre visualization of phase space was sort of unnerving (like all fractals). Although you couldn't call him a dynamic speaker, his argument was so clear that it held my attention. Well worth attending.

-drl

The Future of High Energy Accelerators

January 31, 2006
Categories: Uncategorized

One of Fermilab's recent colloquia was by James Rozensweig of UCLA on the topic of Reinventing the Accelerator for the High-Energy Frontier. Video and Powerpoint slides of the talk are now available.

Current accelerator technologies are up against very fundamental physical limits. In the case of circular proton colliders like the LHC, the fundamental limiting factor is the strength of the magnets and the size of the ring. The LHC uses a 27km ring and 8.36 Tesla superconducting magnets, and the energy scales linearly with the magnet strength and ring size. So one could get beams an order of magnitude more energetic than those in the LHC by using a 270km ring, but the cost of such a thing is likely to be prohibitive. One could also try and design higher field magnets, but the current record for this kind of magnet is only about 16 Tesla.

For circular electron colliders, the limiting factor is the energy loss to synchrotron radiation and these energy losses scale as the fourth power of the energy. LEP was probably the highest energy collider of this kind anyone is ever likely to build, since it already was using an amount of power a sizable fraction of that of the city of Geneva. One could try and use muons, which are much heavier so synchrotron radiation is not a problem, but they decay quickly so there are lots of problems with storing them in a collider.

These considerations mean that there is only one viable route to much higher energies, a linear collider, and this is the path that the ILC project is pursuing. What limits the energy in a linear collider like the ILC is the combination of the energy gradient one can achieve and the length of the machine. The superconducting RF cavities being studied for use in the ILC are inherently limited to gradients of less than 40-50 MV/m, with something like 35 MV/m a likely realistic number. With this gradient, to get up to a TeV in energy would require a length of about 33 km, about at the outer limits of what is possibly affordable. Realistically, to get to higher energies than this, one needs to find some way to get much higher energy gradients.

Rozensweig's talk covers this material, but goes on to discuss various exotic new technologies that in principle can provide these much higher gradients. He describes progress on a long list of these, the most advanced of which is the CLIC project at CERN which uses the wake-field from a drive beam (a second accelerator). Much more exotic are various proposals involving lasers and plasma waves, some of which have been used to achieve gradients of 40 GV/m over short distances in the laboratory.

So, now all one has to do is to achieve a stable, high luminosity beam and make this work over kilometers not centimeters…. Not going to happen any time soon, but the distant future of high energy physics may depend on this kind of technology.

Update: I should also have mentioned here an article on this topic in the current (February) Scientific American entitled Plasma Accelerators.

Comments

1. **Tony Smith**
   February 1, 2006

   Peter says "… One could try and use muons, which are much heavier so synchrotron radiation is not a problem, but they decay quickly so there are lots of problems with storing them in a collider.These considerations mean that there is only one viable route to much higher energies, a linear collider, and this is the path that the ILC project is pursuing. …".

   As an advocate of muon colliders (see my comments on Peter's blog entry at http://www.math.columbia.edu/~woit/wordpress/?p=328) I have some biases here, but it seems to me that Fermilab is putting all its eggs in the ILC basket at least in part because of possible radiation hazard to Chicago of a muon collider at the Fermilab site.

   Unfortunately for Fermilab, there are a lot of uncertainties about building the ILC in the first place, and about its site if it is ever built.

   For example, a Charles Seife article in Science 308 (1 April 2005) 38-40 (and no, it does not seem to be an April Fool joke, although some physicists probably wish it were):
   "… Fermilab's Tevatron, due to shut down around 2010, could be the last large particle accelerator in the United States. …
   … Fermilab's Butler says, "a large number of U.S. physicists at the Tevatron are already planning to work at the LHC; they have exit strategies."
   But Butler isn't happy about the new venue. "This field is being outsourced," he says.
   The one big hope for U.S. accelerator physics is the ILC. "We're going to go for
   the linear collider," says Orbach. …
   For Butler and other physicists, the projected completion date for the ILC in the middle of the next decade is another huge obstacle. … a timetable that puts the ILC at the end of the next decade or beyond would leave an entire generation of physics students without access to an accelerator in the United States. …
   But the leader of the majority party in the U.S. House of Representatives isn't ready to make a firm commitment. "If [the ILC] fits within certain parameters, we'd like to keep it in the U.S.," Hastert says. The biggest of those parameters is the cost, estimated by DOE at $12 billion, of which the host country would presumably pick up half. …".

   Even if the ILC is built, if it is part of an international collaboration involving countries like Japan and China (which hold ots of dollars due to the inability of the USA to compete in trade of manufactured goods), then it may be that the money people in Japan and China will insist on an Asian site. As a Physics Today article at http://www.physicstoday.org/pt/vol-54/iss-9/p22.html about Snowmass 2001 said:
   "… But one cannot see the Pacific Ocean from the Illinois prairie. This geographic truth was pointed out rather bluntly at Snowmass by KEK director Hirotaka Sugawara. He reminded his audience that the joint work on the copper linac design in the US and Japan was undertaken with the understanding that the machine would be sited somewhere on the Pacific Rim … Sotoru Yamashita of Tokyo University was more specific: Europe already has the LHC … it was now Japan's turn to build an energy-frontier machine. …".

   In short, the future of high energy experimental physics in the USA looks to me to be highly uncertain at best, which is doubly sad because the USA theoretical high-energy physics community seems to be insistent on following a program that is Not Even Wrong.

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

2. **a**
   February 1, 2006

   The main problem of a TeV-scale muon collider is it would emit neutirnos, giving unacceptable radiation hazards, see e.g. hep-ex/0005006.

3. **woit**
   February 1, 2006

Tony,

I think it's true that high energy physicists are putting all their eggs in one basket, the ILC. The problem is that, for reasons outlined in the posting, there isn't really any alternative. Things like a muon collider or exotic acceleration technologies are a long ways away from the point where one could come up with a viable design and try and get someone to fund it. The ILC is really the only possibility for at least the next ten years or so, and probably longer.

4. **anon**
February 1, 2006

"Much more exotic are various proposals involving lasers and plasma waves, some of which have been used to achieve gradients of 40 GV/m over short distances in the laboratory."

It seems to me that accelerator physics is frozen in time; it hasn't changed much for the last 25 years. Aren't these "exotic" proposals at least that old? Am I being fair in accusing HEP of a shameful, inexcusable failure to innovate in accelerator physics? Isn't this failure a mayor cause for the failure of HEP theorist to produce predictive theories?

5. **sunderpeeche**
February 1, 2006

I was going to write a reply to Quantoken about electric v magnetic fields, but the post got deleted. Oh well … probably for the best I think. I was also going to say essentially the same thing as Woit in previous post …. the scale of machines for TeV energies is such that there can really be only one in the world. The ILC is about the only scheme which is buildable with today's technology (muon colliders are far future). Even then it will take many years. Exotic acceleration technologies are probably the only hope in the long term, but the key word is "long".

6. **Tony Smith**
February 1, 2006

Peter said "… The ILC is really the only possibility for at least the next ten years or so, and probably longer. …".

If so, then:

1 – What odds do USA high-energy physicists give that it might get built ?

2 – If it were to be built, what odds do they give that it would get built in the USA ?

3 – If it were to be built outside the USA, what do they think would be the consequences for USA high-energy experimental physics ?

If they aren't willing to give reasonable odds for the sake of discussion, does that mean that the pattern of wishful thinking that has corrupted the superstring community has entered the experimental community ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **sunderpeeche**
February 1, 2006

I read anon's post just after submitting the previous.
If I may reply to anon ….. yes, acceleration techniques haven't changed much in 25 years (though superconducting rf was an innovation). The exotic ideas (use of plasmas) have indeed been around for that long, if not more. Shameful failure to innovate? The plasma wakefields etc are difficult ideas, not easy to make practical, even after all these years. But there has not been a lack of trying.

8. **Quantoken**
February 1, 2006

Tony:

Good question. But I guess no one is willing to discuss the odds that things they wish for will be built. It's painfully clear, if one is honest to himself in admitting, the odds are pretty low at this moment.

Frankly I think it is now a good time to mossball things up for a few decades, and let the public money be spend on something more useful in short terms, like alternative energy research. Later, when you guys figure out a better way of doing experiments, then we can fund you again.

Quantoken

9. **AR**
February 1, 2006

There has been tons of discussion about the challenges of building the ILC. See for example Turner's talk below.

http://www.linearcollider.org/cms/?pid=1000069.

The odds of the ILC depend very much on what happens in the next few years: it is likely that if the LHC does not find anything but a Higgs, then the ILC will not be built. If there are lots of surprises, then the chances are a lot better.

Site selection is usually put off as much as possible, so nobody knows the odds that will be built in the US.

If it isn't built in the US, then US experimentalists will rack up a lot of frequent flyer miles. But they'll cope.

10. **woit**
February 1, 2006

Tony and Quantoken,

I don't think anyone has a good estimate what the odds are that the ILC will be funded and built in the US. It certainly is true that if it is not built in the US, that will be very bad news for US high energy physics, and specifically for Fermilab if it is not built there.

My guess is that the decision whether to build the ILC won't be made until 2009-2010, at which point initial results should be coming in from the LHC. A lot depends on what the LHC finds. If it finds something interesting that would require the ILC to fully understand, that will help a lot. If it finds nothing at all and it then looks as if there is nothing new for the ILC to study up to its highest energy of 1 Tev, that will be a big problem. No one knows which of these is more likely, that's the biggest question in HEP right now.

On top of this, everything depends on the US budget situation several years down the road. I don't think anyone can predict that either, except that the US will still be struggling with what to do about budget deficits. Bush's announcement last night that he wants to double the budget for fundamental physics research may or may not lead to anything and may or may not go to HEP. We should know soon when his administration submits their FY2007 request. But the bottom line is that the budget situation is unclear for later this year, much less for 2009-2010.

And, no, even if the idea made any sense, you can't "mossball" an entire scientific field. Much of the budget pays scientists and technicians. If you throw all of them out of their jobs and have them move into other careers, they're not going to be coming back.

11. **David**
February 1, 2006

anon:

You've got to be kidding me by implicating accelerator physics as the major cause for HEP theorists not producing predictive theories. "Oh so sorry Mr./Miss String Theorist, we can't provide the gazillions of TeV you need to verify your theory, so it's our fault your theories don't predict anything."

And as for innovation, do you have any idea of how incredibly hard it is, for example, to build an experiment to test plasma wakefield acceleration, using an electron beam with the right properties? Sure these ideas existed on paper for a long time, but the technology needed is incredible.

The real problem is money. We could build a linear TeV collider now if we want, but we would need a LOT of real estate and a LOT of power and a LOT of money. This is obviously why accelerator physicists are investigating high-gradient alternatives, but these alternatives are technologically challenging. Even building a "warm" linear collider at X-band (11.424 GHz) to achieve higher-gradients as compared to the SLC at S-band (2.856 GHz) is quite challenging. The high fields alone lead to RF breakdown and pulsed heating that prevent high gradients from being achieved. These phenomena opened up a lot of study involving material science and surface physics. There are just so many impediments to technologically achieve high gradients that it requires years of experiments, analyses and simulations. But more importantly, it requires a lot of money which is hard to obtain. The accelerator field is making a lot of progress, but this progress is not really seen by an outside observer because they usually only see the big machines that finally get built.

12. **J.F. Moore**
February 1, 2006

Agreeing with sunderpeeche, there has been a lot of progress in acclerator research over the last two decades. Part of the apparent slowdown compared to the blinding progress of the preceding four or five decades is that all the low-hanging fruit is gone. The new developments are harder to realize, just like in any field of technology.

Regarding the 'eggs in one basket' criticism, I don't think there is much choice politically. If they don't get the ILC, they can push for something more exotic perhaps, but they need to get everyone on board now, with no apparent alternative and present a unified front to have any hope of landing the ILC.

As for the odds, you make the best proposal you can as a team and move on with it. The numbers I've seen don't seem unreasonable (e.g. not terribly out of scale with currently funded large science projects), the biggest problem is to make all of the politicians agree. THere's not much point in doing statistics and hand-wringing when you have no real alternative in any case, is there?

13. **Tony Smith**
February 1, 2006

Peter said "… Bush's announcement last night that he wants to double the budget for fundamental physics research may or may not lead to anything and may or may not go to HEP. We should know soon when his administration submits their FY2007 request. …".

Deb Riechmann wrote an AP article dated 1 Feb 2006 and carried on a Forbes web page at http://www.forbes.com/entrepreneurs/feeds/ap/2006/02/01/ap2492673.html that said in part:
"… In addition to the money for the tax credit, Bush wants to double over 10 years the investment in agencies that support basic research in the physical sciences and engineering, a $50 billion commitment.
The research money – $910 million in fiscal 2007 – would fund research programs at the National Science Foundation, pay for 500 new researchers at the Commerce Department's National Institute of Standards and Technology and 2,600 new researchers at facilities operated or assisted by the Energy Department. …".

If the ILC costs $12 billion (a DOE estimate according to a Charles Seife article in Science 308 (1 April 2005) 38-40), then
it might fit comfortably within a $50 billion increase over the next ten years
and
if the USA is the major funding government, then the USA should be able to dictate the site location, and it is possible that Fermilab might get the ILC and give high-energy experimental physicists a home in the USA.

Of course, that presupposes that the Bush budget idea would be supported by the next administration. Since Bush cannot run again, Fermilab will have to hope that the new president will not do to the ILC what Clinton did to the SSC (i.e., campaign promises to support it, followed by pulling the rug from under it).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

14. **J.F. Moore**
February 1, 2006

The implication that the SSC was viable in 1993 is not too credible. Many members of both parties in congress had the long knives out for that project for years before they finally succeeding in shutting it down – it was the prior Bush administration that kept it alive by lobbying so hard.

As for whether the SOTU numbers for DOE, NIST and NSF will ever materialize, I would suggest looking at past promises and comparing them to realities to develop one's own prediction.

15. **Quantoken**
February 1, 2006

Peter:

It's wishful thinking that if LHC finds something interesting then ILC may be founded. Just look at Hubble Telescope, it's finding something interesting every day but yet it's let to retire early. The bottom line is you need to look at priorities. And high energy particle physics research is clearly NOT the priority right now.

Don't you guys listen to the Bush address carefully? The predominant theme is "energy", "alternative energy research", "Get rid of America's oil addiction". That certainly has something to do with department of energy. But high energy physics is now just a step son of department of energy. Bush emphasised nanotechnology (material science), super computing, and alternative energy research.

Predictably, money will be shifted away from un-important fields, to areas that are now considered priority. The priority is alternative energy research, not dark energy research. The important things is replacing black gold, not researching black holes. For high energy physics research, the party will soon be over.

Quantoken

16. **Tony Smith**
February 1, 2006

J. F. Moore said "… As for whether the SOTU numbers for DOE, NIST and NSF will ever materialize, I would suggest looking at past promises and comparing them to realities …".

For two examples:
during the 2000 campaign, Bush promised tax cuts for big business, and he delivered; and
during the 2003 SOTU, Bush promised to remove a threat of Iraq under Saddam Hussein, and he did indeed remove Saddam Hussein as ruler of Iraq.

Therefore, it seems to me that if Bush feels strongly about a big idea, he will move to support it. That leaves the question as to whether or not Bush feels strongly about high-energy physics as a big idea.
Quantoken says "… high energy physics is now just a step son of department of energy. Bush emphasised nanotechnology (material science), super computing, and alternative energy research. …".
However,
as J. F. Moore said, "… it was the prior Bush administration that kept … the SSC … alive by lobbying so hard …", and it seems to me that with respect to high-energy physics it might be like-father-like-son.

Even though I agree with some things said by J. F. Moore, his statement about my comment:
"The implication that the SSC was viable in 1993 is not too credible"
is inaccurate and easily refuted as follows:

I did NOT state, and did NOT imply, that "the SSC was viable in 1993".

In fact, I said "… what Clinton did to the SSC ..[was]… campaign promises to support it, followed by pulling the rug from under it …".

Clinton was elected in 1992, and when he took office in 1993 he pulled the rug from under the SSC and it was terminated. According to an article by David Ritson in Nature 366 (16 December 1993) 607-610:
"… even with Bush's personal support, the SSC had a narrow escape in 1992.
… in the summer [of 1993] … Clinton publicly supported the SSC. But how far this support extended in private is questionable .
… The [USA] house [of representatives] instructed its representatives in the conference committee to terminate the SSC. This action ended the SSC early in October [1993] …".

There are lessons that the ILC supporters should learn from the SSC.

First, unless regional rivalries about sites within the USA (such as Illinois v. Texas) are fully reconciled, Clintoneque backstabbing can be fatal.

Second, the ILC management must avoid undisciplined cost controls and arrogant behaviour, both of which were severe problems with the SSC.

All things considered, it seems to me that maybe Bush might feel strongly enough about high-energy physics to support the ILC, but since Bush cannot run in 2008, it will be necessary for his successor also to support it.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Just so that nobody will have grounds to accuse me of being a pro-Bush propagandist, I will state for the record that in 1992, 1996, 2000, and 2002 I voted for Perot, Perot, Libertarian, and Libertarian for USA President.

17. **J.F. Moore**
February 1, 2006

My point was simply that congress was ready to kill SSC for years. Whatever motives you want to ascribe to Clinton notwithstanding, the vultures were circling well before he was elected, which is supported by your pull quote from Nature. If a line item desperately needs vigorous presidential support to save it, it doesn't require "backstabbing" to end it, it requires folding your arms and doing nothing.

More importantly, I think you are fooling yourself if you think the Bush administration has any commitment to physics, or even science at any level. Most of the things in these speeches are pure fluff. How about evaluating the hundreds of promises all presidents have made in SOTU addresses, including the promise of _every_ president since Nixon, some of them multiple times, to reduce our dependence on foreign oil, when over the same period the fraction has gone from under 30% to about 60%?

By the way, a quick calculation shows that this bold doubling initiative, even if it happens, will result in a 7% annual increase. Between payroll, insurance, and facilities charges, most places have a hard time keeping status quo with only a 5% increase. I am being kind and not counting overhead, which always rises faster.

18. **ObsessiveMathsFreak**
February 2, 2006

I had a feeling that particle accellerators were in for a redesign. I knew that circular accellerators did not scale very well, but to the fourth power! Gad-zooks!

I wonder has anyone tried some genetic algorithms to "evolve" a better energy gradient process, rather like an evolved antenna.

19. **sunderpeeche**

February 2, 2006

It is a consequence of basic (classical) electrodynamics that the radiated power goes like P ~ E^2B^2 where E= energy and B = magnetic field. Solve the Lorentz eqn for ultrarelativistic motion in a circle of radius R and you get B ~ E/R, so P ~ E^4/R^2. Nobody has found a way around this. So to reduce the power output one needs big rings (large R). The circumference of LEP was 27 km. FWIW LEP dissipated 18 MW at its top energy of sqrt(s) = 209 GeV. Also FWIW the SSC (20 TeV protons) would also have dissipated synchrotron radiation (I think several kW), the heat load of which would go into the superconducting magnets … another problem for the machine/magnet design.

20. **Tony Smith**
February 2, 2006

An AP article said "… Bush wants to double over 10 years the investment in agencies that support basic research in the physical sciences and engineering, a $50 billion commitment. …".

J. F. Moore said "… a quick calculation shows that this bold doubling initiative, even if it happens, will result in a 7% annual increase.
Between payroll, insurance, and facilities charges, most places have a hard time keeping status quo with only a 5% increase.
I am being kind and not counting overhead, which always rises faster. …".

Although I am not sure exactly how the AP article's "bold doubling" deals with inflation etc,
if I use for the sake of argument J. F. Moore's method, it seems to me that, based on a present $50 billion budget "in agencies that support basic research in the physical sciences and engineering":

$50 b x (1.07)^10 = $98 b, giving roughly a "new" $48 b over the initial $50 b, or doubling as described by J. F. Moore;
and
$50 b x (1.05)^10 = $81 b for J. F. Moore's "status quo",
so
that the "bold doubling" from $50 b to $50 b plus $ 50 b = $100 b would give $19 b over and above a 5% "status quo" amount,
and
$19 b is still more than the $12 b cost of the ILC,
so
even under J. F. Moore's own reasoning, "basic research in physical sciences and engineering" could:
maintain its "status quo";
build the ILC with USA money at a USA site; and
spend $7 b on new and different stuff.

I should also mention that if ILC were sited at Fermilab, funds that now go to the Tevatron (within the present $50 b) could be assigned to the ILC, thus reducing the ILC's demand for new money.

Of course, J. F. Moore could move the goal posts by pleading about " overhead, which always rises faster".
However,
maybe faster-rising-overhead is a symptom of poor cost control management, which is one of the factors that killed the SSC,
and
maybe refusal to restrain overhead is a symptom of management arrogance, which is another of the factors that killed the SSC.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – If one is asking a President for support for ILC, and a President makes public statements about a budget that could support ILC, it seems to me to be impolitic to say things like "… you are fooling yourself if you think the Bush administration has any commitment to physics, or even science at any level …", particularly in view of the strong support for the SSC by the first Bush administration. Why bite the hand from which you are asking to be fed?

21. **woit**
February 2, 2006

Tony and J.F. Moore,

I'd suggest postponing further detailed speculation about exactly what the Bush administration is going to do for a few days, until they release their 2007 budget figures. In any case, discussing what this means for 10 years from now is pointless: Bush will be long gone, things will be very different, and the history of promises like this is that they are forgotten after a few years. But soon we'll know something solid that means something, whether and how Bush's rhetoric will be reflected in next year's budget request.

22. **J.F. Moore**
February 2, 2006

Very good points, Peter. I totally agree about trying to predict the politics of this a decade out.

I will simply clarify at this stage that in general, budget increases are not pre-normalized to inflation, let alone expected rising costs. I agree that even so there is room for ILC. We will have to see what happens.

23. **Steve Myers**
February 3, 2006

As I recall it wasn't only politicians who were anti-SSC. Phil Anderson had a long letter in NY Times attacking it. And it's very hard to justify spending billions on research that is so removed from people's everyday lives. There's the problem.

24. **sunderpeeche**
February 3, 2006

Phil Anderson persistently attacked the SSC. (What is hypocritical is that after the SSC was cancelled solid-state physicists expected some of the $8B to go to their field. It didn't. It's not as if there was an $8B pot and "how shall we spend this on physics"?) It is also true that the initial cost estimate was approx 1.5B, this increased to about 4.5B when the Central Design Group (which leased space at LBL) developed their design. When SSC took over from CDG (and relocated to Texas) the revised cost estimate was 8B, with a possibility of 11B. This was simply too much for anyone and the thing was cancelled. Also the SSC was advertised (at least initially) to "regain the lead in HEP for the USA", and for the USA to have the "world's highest energy accelerator". It was hard to get other countries to contribute so that the USA could wave the flag.

25. **Jack Sarfatti**
February 11, 2006

It's called due process. The paper was endorsed by a several people independently. I have a PhD in theoretical physics from the University of California and unlike some of you who are being outsourced I will not have to be driving cabs or working at Burger King. 🙂

26. **Alejandro RIvero**
February 12, 2006

Well, *J. Sarfatt* did some publications on physics during the years 1963-1973, and I guess he had a theoretical phycsics PhD 🙂

Various Somewhat Related Links

February 1, 2006
Categories: Uncategorized

David Goss wrote to tell me that Physics Web has an article about physics weblogs. The theorists quoted are Sean Carroll, Paul Cook and Dave Bacon.

There's an article by Michael Green in the latest Nature Physics reporting on the recent 23rd Solvay conference (mentioned here and here). Green notes that "Much of the discussion focused on string theory" but that "the structure of string theory is still so badly understood that it does not yet deserve to be described as a 'theory' at all — it is more a 'work in progress'." He describes the discussion at Solvay about anthropic explanations of the CC as "lively", but that Polchinski et. al. have yet to carry the day: "there is a strong body of opinion that holds that it may be premature to decide which parameters are environmental within string theory, as the structure of the theory is still poorly understood and will surely hold further surprises."

The same issue of Nature has an essay by Lawrence Krauss entitled Anthropic Fever where he starts by explaining the standard Landscape story that fundamental physics is really an "environmental science", but then goes on to write:

But I have been wondering whether things might actually be much worse. It could be that many different combinations of laws could allow life to form, and that it is a pure accident, not favoured by any particular probabilistic explanation, that the constants of nature result in the combinations we experience in our Universe. Or, it could be that the mathematical formalism is so complex that the ground states of the theory might not be mathematically determinable, even in principle.

Whether or not nature is ultimately 'undecidable' in this strong sense, these ideas point to a possible future for particle physics that is very different from the past. Fundamental physics might not be restricted by any underlying grand mathematical structure that would 'explain' why the Universe is the way it is. It's a possibility that I hope will be wrong, but it's a possibility nonetheless.

This kind of worry seems to me completely misplaced. There's not a shred of evidence for it, and the only reason people have been engaging in this kind of speculation is that string theory led them down this sorry path. Finding a trackless swampland at the end of the path doesn't mean that physics is destined to spend the rest of eternity mucking about in a swamp, it is far more likely that a wrong turn was taken quite a ways back.

Last night a write-up by Nati Seiberg of his rapporteur talk at Solvay appeared at the arXiv, entitled Emergent Spacetime. It does a very good job of laying out the reasons that people often say that string theory suggests that our standard concept of "space" needs to be revised, that perhaps space is an "emergent" concept. Personally I don't think the different reasons that Seiberg lays out add up to a very convincing case. For one thing, string theorists have been trying to come up with a new "stringy" version of space for twenty years now, without much success at all. They are still far from anything like a consistent proposal of what this new idea about space will be, and the various evidence given by Seiberg is rather incoherent, leading to different kinds of generalizations of space, not pointing to any one of them in particular.

The past two days there was a conference at Harvard on Black holes, topological strings, and invariants of holomorphic submanifolds, which also included some lectures in memory of Raoul Bott by Sir Michael Atiyah, S. T. Yau, and Dan Freed. Lubos has reports on two talks there, one by Robbert Dijkgraaf about a hoped for "Universal Wave Function", a Hartle-Hawking kind of wave-function that would give the amplitude for the universe to be in various parts of the Landscape. The second was by Frederik Denef who spoke on "D-brane ground states, multicentered black holes, DT/GW correspondence, and the OSV conjecture ." Lubos reports on a conversation with Frederik about his two forthcoming papers with Michael Douglas on computational complexity and the Landscape. It seems that what Denef-Douglas show is that, even if everything one would like to calculate is in principle calculable, the problem of identifying a specific string theory background realizing anthropically small values of the CC is NP-hard. This means that in practice you'll never be able to do what landscapeologists would like to do: use the observed values of the CC and maybe some other standard model parameters to identify a tolerably small number of backgrounds, then use the properties of these backgrounds to make predictions. I believe it is this possibility that Krauss is alluding to in his quote above about how the "ground states of the theory might not be mathematically determinable, even in principle."

Update: The last paragraph has been modified to better reflect reality. In its initial version I had assumed from Lubos's blog entry that these computational complexity issues had been what Frederik's talk was about.

Update: Polyakov also has a new preprint based on remarks at the Solvay conference, entitled Beyond Space-time. It's a mixed bag, mostly about various ideas related to AdS/CFT, as well as the cosmological constant. He begins with critical but not completely dismissive comments about anthropic arguments:

Another danger is to get distracted by non-dynamical anthropic arguments, which recently acquired some popularity. I find the anthropic principle irrelevant. It is unlikely to uncover fundamental ideas and equations governing the universe. But, in spite of these misanthropic remarks, I believe that in special cases anthropic arguments may be appropriate.

At the end of the paper, he characterizes the various topics he has discussed as follows:

As it is clear from the list of the references below, these ideas (except for the gauge/strings correspondence) did not attract any attention. Perhaps they don't deserve it. My best hope, however, is that some of them may serve as small building blocks of the future theory, the vague contours of which we can discern at the horizon.

Comments

1. **Kea**
   February 1, 2006

   I read Seiberg's article in disgust: statements like "general covariance is a gauge symmetry" as if that were a complete argument for anything. He also claims to have some radical things to say, but I didn't spot them. What were they?

2. **woit**
   February 1, 2006

   Kea,

   I found Seiberg's argument that "some gauge theories have duals without gauge symmetry, so gauge symmetry is not fundamental. Since general covariance is a gauge symmetry it is not fundamental also" very unconvincing. But in any case, if you are going to abandon space, you don't even know what you mean by "general covariance" anymore, so you are abandoning that too.

   Not sure which he thought were the radical claims. Perhaps his two points at the end:

   1. Maybe every quantum mechanical system is dual to some string theory, so you shouldn't ask "what is string theory", but ask "Which string theories have macroscopic

dimensions" (where, presumably, the answer is "all sorts of them"…l)

2. If you can't think about what it means to be smaller than the Planck length, you have to give up the idea of reductionism as involving more and more fundamental laws that operate at smaller and smaller distances.

3. **Frederik Denef**
February 1, 2006

Hi Peter,

Small correction: my talk was not about the computational complexity or any other feature of the landscape, but titled "D-brane ground states, multicentered black holes, DT/GW correspondence, and the OSV
conjecture [or: why OSV is probably right]". After all, it was a conference about black holes, topological strings and invariants of holomorphic submanifolds, and indeed, I do have a life outside the landscape 🙂

As far as the computational complexity paper is concerned, stay tuned, it will appear soon…

4. **woit**
February 1, 2006

Hi Frederik,

Sorry about that, will correct. I was relying on Lubos's blog, didn't realize he was reporting on a conversation with you, not your talk.

5. **Chris W.**
February 1, 2006

From Peter:

> But in any case, if you are going to abandon space, you don't even know what you mean by "general covariance" anymore, so you are abandoning that too.

I wouldn't be so quick to assume this. See Joe Henson's new review **The causal set approach to quantum gravity** (gr-qc/0601121). Also see John Stachel's recent paper, Structure, Individuality and Quantum Gravity (gr-qc/0507078).

It is quite an admission for Michael Green to assert that "the structure of string theory is still so badly understood that it does not yet deserve to be described as a 'theory' at all — it is more a 'work in progress'", given that he and John Schwarz have been working on string theory for over 30 years.

6. **D R Lunsford**
February 1, 2006

Kea – agreed. I just stop reading when such a statement is made completely out of context.

-drl

7. **Arun**
February 2, 2006

I predict that none of the spacetime destroying features of string theory that are described in Seiberg's talk will be met with the derision that LQG's doubly special relativity, etc., receive.

8. **Dumb Biologist**
February 3, 2006

Speaking of LQG…someone is proposing what I guess is a concrete test of quantum gravity within that framework:

http://physicsweb.org/articles/news/10/2/2/1

I don't know how reliable this news is, or if I'm misinterpreting, but it seems to suggest that some LQG folks are proposing looking for a humanly-observable feature of BH formation that could possibly falsify their theory…meaning it's bona fide physics, at least potentially.

9. **Christine**
February 3, 2006

There are also some initial (negative) results for chaotic quantum foam models using quasar halos.

10. **anon**
February 3, 2006

DB, if that article is right, all they have to do is observe a brief dimming during the final flash of a dying star. Since there are 10^12 stars in our galaxy, with an average lifetime of 10,000 million years, about 100 must die each year in the Milky Way alone. Automatic CCD observation programs (of the kind used by Perlmutter to observe the spectrum of very distant supernovae) could be adapted to observe these flashes. This is very interesting.

It's excellent news that at last LQG is being taken seriously by some people. (Sorry Peter if this is off topic…)

11. **Brett**
February 3, 2006

The number of visible massive star collapses in our galaxy is less than one per hundred years, not one hundred per year.

12. **Dumb Biologist**
February 3, 2006

Thanks for the link and some of your exposition on your blog, Christine. Perhaps it's best to continue discussion over there so as to not get too far off topic.

13. **island**
February 4, 2006

> Lawrence Krauss said:
> But I have been wondering whether things might actually be much worse. It could be that many different combinations of laws could allow life to form, and that it is a pure accident, not favoured by any particular probabilistic explanation, that the constants of nature result in the combinations we experience in our Universe. Or, it could be that the mathematical formalism is so complex that the ground states of the theory might not be mathematically determinable, even in principle.
>
> Whether or not nature is ultimately 'undecidable' in this strong sense, these ideas point to a possible future for particle physics that is very different from the past. Fundamental physics might not be restricted by any underlying grand mathematical structure that would 'explain' why the Universe is the way it is. It's a possibility that I hope will be wrong, but it's a possibility nonetheless.

Peter Woit replied:
This kind of worry seems to me completely misplaced. There's not a shred of evidence for it, and the only reason people have been engaging in this kind of speculation is that string theory led them down this sorry path. Finding a trackless swampland at the end of the path doesn't mean that physics is destined to spend the rest of eternity mucking about in a swamp, it is far more likely that a wrong turn was taken quite a ways back.

His statement is also false, because we do have a good idea that *'many different combinations of laws'* WILL NOT *'allow life to form'*… so *'the value of the constants'* ARE *'favoured by any particular probabilistic explanation*.

So there is no evidence for anything that was quoted, and fundamental physics is therefore, more-likely to be restricted by an *'underlying grand mathematical structure that would 'explain' why the Universe is the way it is*.

And don't even tell me that my points aren't relevant.

14. **woit**
February 4, 2006

Please take discussion about conditions under which life forms to some other more appropriate forum. Virtually all the discussion along these lines that people keep posting here has little to do with the topics I'm posting about, and is just adding to the noise level.

15. **dan**
February 5, 2006

" (Sorry Peter if this is off topic…)"

Dear Peter or Lubos or Urs,

Given we have some prediction for LQG of a dying star, what would string theories' prediction be? Would it be similar to what LQG predicts or would it be different, and if different, how different? (see below)

We have LQG's prediction. Does string theory predict a naked singularity as classical GR does, or does it predict something different? Even if string theory has problems with SUSY and landscape, could it offer a prediction regarding singularities as LQG has done?

Speaking of LQG…someone is proposing what I guess is a concrete test of quantum gravity within that framework:
http://physicsweb.org/articles/news/10/2/2/1

"However, in the final stages of a star's collapse, the curvature of space-time becomes so large that classical general relativity theory no longer holds and quantum-gravity effects should take over. By applying the techniques of loop quantum gravity — a leading candidate for a quantum theory of gravity — Joshi and co-workers calculated that a dying star does not form a naked singularity but has all its mass thrown away in a flash instead. This burst has a characteristic signature: the star dims briefly before it rapidly radiates away to produce extreme energy gamma rays, cosmic rays and neutrinos. If this fingerprint were observed by astronomers, it might provide the first true observational test for quantum gravity."

16. **woit**
February 5, 2006

dan,

For the 1000th time: string theory predicts nothing at all about this, or about anything else.

As for the idea that LQG makes real predictions in this context, I think this is very unlikely.

17. **dan**
February 5, 2006

The paper in question required a subscription, which I do not have.

http://scitation.aip.org/getabs/servlet/GetabsServlet?prog=normal&id=PRLTAO000096000003031302000001&idtype=cvips&gifs=Yes

Perhaps prediction is too stringent, but perhaps "qualitive expectations consistent with known string theory" is a more specific term in the scenario described in the paper, would it differ or be similar to what LQG predicts, or would it match what GR predicts (a naked singularity)

Lubos actually responded to this post on his weblog

"string theory predicts extra dimensions in the form of Kaluza-Klein modes of known particles; excited strings at very large energies; specific patterns of black hole evaporation; Lorentz-invariant physics; topology changing transitions near the Planck scale; forces different from gravity, and possibly particles associated with supersymmetry and grand unification."

— not that i'm trying to start a flamewar or anything.

18. **Christine**
February 7, 2006

There is a new paper by Craig J. Hogan posted in the arXiv: Nuclear Astrophysics of Worlds in the String Landscape. The readers of astro-ph are mainly astrophysicists. I would say that a good fraction of them do not have any idea on the current debate on this landscape issue! I did not read the paper so I do not know whether the author includes a balanced comment on the fact that the landscape issue is quite an open problem. (Or, to some, a kind of dead end for string theory).

19. **Who**
February 7, 2006

Christine just posted a pretty good reading list for Quantum Gravity.

Since the topic is "Various Somewhat Related Links", and she didn't mention it, I will

http://christinedantas.blogspot.com/2006/02/basic-curriculum-for-quantum-gravity.html

20. **Who**
February 7, 2006

Re the two forthcoming papers by Denef and Douglas mentioned in Peter's log entry, this evening Douglas posted
http://arxiv.org/abs/hep-th/0602072
Computational complexity of the landscape I
Frederik Denef (KU Leuven), Michael R. Douglas (Rutgers and IHES)
53 pp, 2 figures
"We study the computational complexity of the physical problem of finding vacua of string theory which agree with data, such as the cosmological constant, and show that such problems are typically NP hard. In particular, we prove that in the Bousso-Polchinski model, the problem is NP complete. We discuss the issues this raises and the possibility that, even if we were to find compelling evidence that some vacuum of string theory describes our universe, we might never be able to find that vacuum explicitly. …"

European Strategy for Particle Physics

February 3, 2006
Categories: Uncategorized

The CERN Council Strategy Group is putting together a document proposing a European stategy for particle physics, in a process to be completed this July. As part of this process, earlier this week the group held an Open Symposium at Orsay, and the presentations are now available on-line.

I've often written here about possible future plans for particle physics in the US, but these presentations give an excellent overview of what is going on in Europe, where the situation is quite a bit better than here. Several of the presentations discuss possible upgrades to the LHC: the SLHC (increased luminosity), the DLHC (doubled beam energy using more powerful magnets), and the LHeC (colliding electrons or positrons with protons, like HERA at DESY, but with 1.4 TeV center of mass energy). Pretty much all the presentations are worth taking a look at, several of them involve an impressive amount of work in putting together a lot of information into a very professional PowerPoint format.

The one presentation about particle theory is by Nigel Glover and compares the performance of European and American theorists by looking at citation counts. There's a lot of interesting data, much of it showing American dominance, but keep in mind that there is a strong "Witten effect" in the data, since he is by far the most influential theorist around, especially in terms of number of citations.

Back here in the U.S., on Monday the Bush administration is releasing its FY2007 budget proposals. An outline of the DOE budget lists an 8% increase in HEP spending to $775.1 million, as well as full funding for RHIC. The NSF should also see a sizable increase as part of the so-called American Competitiveness Initiative. The folks over at Cosmic Variance are experiencing some cognitive dissonance.

Comments

1. **sunderpeeche**
   February 3, 2006

   That's very good news that RHIC will be fully funded in FY2007 (if all goes well thru Congress). BNL has also considered plans for eRHIC (collide electrons against protons/ions). One hopes that such ideas get funded.

2. **Ponderer of Things**
   February 4, 2006

   Playing devil's advocate (to some degree) – why should US be competitive in high energy physics? What possible practical return of investment can be expected from putting money in RHIC, Fermilab, LHC, ILC etc? To taxpayers scientists are thiefs who take their hard-earned money. How do you explain to taxpayers why it's important to continue investing into giant colliders where we seem to be reaching area of diminishing returns? Wouldn't that money be better spent for condensed matter/materials physics research, including nanotechnology, optics, spintronics etc? Never mind chemistry, biology and medicine – areas which produced plenty of bang for the buck already and will be expected to deliver in the near future.

   So – cure cancer and Alzheimers, or find out if string theory is a theory or just a theory about what a theory could be – which would you choose?

   8% increase is quite generous, considering there's no big high-energy project in US past 2009…

3. **woit**
   February 4, 2006

   Ponderer,

   First thing to keep in mind is that US HEP funding is under $1 billion per year, which is less than .01% of US GDP. At this scale, HEP funding is not crowding out funding for anything else worthwhile. The amount of money already being spent on cancer research is at least an order of magnitude higher. You're not going to cure cancer and Alzeimers with the marginal increase in funding for that kind of research you would get by reallocating HEP funding to those uses.

   Besides the small amount of HEP money going to string theory research (which I'll agree should be redirected elsewhere), most of it is going to experimental projects, none of which actually have anything to do with testing string theory. They do have to do with understanding some very fundamental things like where mass comes from, and you can justify funding research on this in several legitimate ways:

   1. Technological spin-offs: for instance, improvements in accelerator technology could have all sorts of applications. If you look at the talk about new accelerating technologies that I recently posted about, you'll see a comment that one beam instability being studied could at least conceivably provide a new route to fusion. A long shot, but for the relatively small sums being spent here, a small chance of a huge payoff is worth the money spent.

   2. Applications of whatever we learn: once we understand electroweak symmetry breaking and where mass comes from, chances are it won't have any particularly useful application. But we don't know, there is a chance of something truly dramatic coming out of this. If we discover a new stable particle (something supersymmetry advocates think exists), there are a lot of things you can imagine doing with it.

   3. Impractical things are important too: personally I don't think all human activity should be devoted to trying to make us live longer and more comfortably. Like art, literature and many other human activities, scientific research designed to better understand the world is of value in and of itself. Much of this kind of thing doesn't need to be publicly financed (arguments about public arts financing rage on and on), but unless we find a lot more hedge fund managers like Jim Simons who want to spend a lot more of their wealth on this, the only possible viable financing for HEP is public financing.

4. **anon**
   February 4, 2006

   1. Hot fusion would create a lot of radiation, the reactor would be irradiated with neutrons continuously. It's not just a 50 year old pipe dream, it's a dirty pipe dream. Any magnetic compression of a plasma is like squeezing an orange, the juice squirts out. You can just about get it to work with a massive amount of energy, meaning technical instabilities could cause it to blow up. It is just so expensive, dangerous and impractical when we have the sun pouring out fusion energy, I can't believe anyone seriously is an enthusiast of fusion power stations anymore. Even Quantoken realises solar cell technology is the way to get fusion power – from the sun.

   2. Understanding where mass comes from is vital, but surely that is a case for more money for new ideas and theory that models reality? Without a proper set of alternative theories (not just stringy fantasy), nobody will know what experiments to do, or how to interpret results. If there is no money put into alternatives to stringy stuff, it will look as if any result must be (or can only be) evidence for one of the 10^500 possibilities covered by stringy stuff.

3. '… impractical things are important too'. You'd make a devilish string theory proponent. I think the excitement has gone from high energy physics, the last really big event was 1983. It's like trying to promote going to the moon. It's a thing of the past, out of fashion.

5. **John Gonsowski**
February 4, 2006

HEP is very related to the ultimate human quest for the meaning of life. There's a reason Pauli and Jung sought out each other. Yes a pure conservative would like no government reaching into his pocket and a pure liberal would like us all to live munk-like and give lots of money to help the poor especially in third world countries. There needs to be a balance of course (for one thing both extremes would collapse the economy and there'd be no money for the pocket or the third world) and I think HEP deserves to be highly favored in the balance given its relation to the meaning of life.

Peter and people like John Baez are certainly right that we need to go back and look at things that seem to have gotten skipped over as things got more and more complex and unrelated to the real world. It's kind of embarassing not to have a well understood idea of where mass comes from. Peter's comment that a GUT needs to have this built-in rather than being ad hoc sounds good. My first comment related to mass (given my many hours spent at Tony Smith's website) would be "What is a leptoquark supposed to do anyways?".

http://home.comcast.net/~jcgonsowski/polytopes.html

6. **woit**
February 4, 2006

anon,

1. Anyone who wants to discuss fusion should do this somewhere more appropriate.

2. If your current theoretical ideas aren't any good, that's when you most need experimental results.

3. If you think particle theory is out of fashion, so not worth thinking about, you're spending your time reading the wrong blog.

John,

Please, discuss Tony Smith's ideas about leptoquarks with him, or with other people somewhere else. That's far off topic, and this is not sci.physics, it's not a forum for people to try and start discussions of their favorite alternative ideas about particle physics. Allowing that quickly leads to a high level of noise drowning out any sensible discussion.

7. **SomeBody**
February 4, 2006

Peter, you say "under $1 billion" and make it sound like nothing to quibble about. That's an easy trap to fall into; a billion bucks is an abstraction which nobody has a real feel for. So let's try to develop one by comparing with median US household income. Asking Google, we find e.g.

http://www.epi.org/content.cfm/webfeatures_econindicators_income20050831

where we learn that the number in 2004 was $44,389 (and falling). Total taxation is roughly 30%, so we do

1E9/(0.3*44,389) = 75093

to find the number of households equivalent to 1 billion of tax dollars. Mind you, a household will generally mean a couple of adult workers + their offspring, so we are really talking the equivalent of O(1E5) people here going to work every day and then giving their every last tax dollar to HEP, leaving nothing for school, social security and so on.

The ILC is expected to cost $12 billion or so. In other words, that project alone will require the equivalent of one million workers – from stock brokers to cleaning ladies – giving their every last tax dollar to HEP, leaving nothing for school, social security and so on.

Money well spent? Maybe, maybe not. But the real question is: do you really think those who earned it would choose to spend it this way, if they were given a choice?

8. **J.F. Moore**
February 4, 2006

Ponderer – The following question of yours in particular is fallacious: "So – cure cancer and Alzheimers, or find out if string theory is a theory or just a theory about what a theory could be – which would you choose?"

It's not a genuine choice. Cancer research has been funded with large amounts of money for many years. This has resulted in great advances in understanding and treating cancer, but certainly no blanket 'cure'. In fact, it is likely that the total cancer mortality rate has dropped more from people not smoking than from all treatment combined.

It is not at all clear that even the extreme of ending all HEP work and diverting the money to cancer would have a significant effect on the existing effort, as it may already be well into the realm of dimenishing returns. Maintaining a healthy, diverse academia by supporting the field and attracting researchers from around the world is probably more important.

9. **anon**
February 4, 2006

Sorry Peter for my style. A definite prediction should be made before experiments, and this requires some revolutionary theory first, that can make the sort of predictions that can be tested.

10. **J.F. Moore**
February 4, 2006

SomeBody – that's a fairly unreasonable way to look at it, since the burden is spread across the whole country, and since the top rate is fixed those with higher income pay disproportionately more. It then depends on what you choose to be a representative household, but I think the total bill for ILC would be under $50 for most of us.

For another perspective, it would also be fair to compare this to other tangible things like the space shuttles, an aircraft carrier group, hurricane Katrina, etc. Yes, a billion dollars is a staggering amount of money, but it must be kept in perspective. The US is paying $350 billion a year annually in interest on our debt alone.

11. **woit**
February 4, 2006

Somebody,

I have several problems with your computations, including the fact that you use a price-tag for the ILC probably so high that there is zero chance it will be funded if that is what the US will have to pay for it, and you assign its total cost to one year, even though it will probably take a decade to build the thing. Another problem is that you use median household income when the bulk of tax receipts are coming from people significantly above the median (because they have more money), and there are other sources of tax receipts (eg. taxes on corporations).

If you assign the marginal cost of any particular thing the government does to struggling families who have just barely enough money to get by, but enough to be paying taxes, that's a good debating trick for the libertarian point of view against government spending, but it makes no more sense than the favorite leftist debating trick to compare whatever one wants to fund to the profits of the Mobil corporation and announce that there's plenty there and they'll hardly miss it. The relevant fact here is that $1 billion is a lot of money, but the total federal budget is over $2,000 billion, so we're talking about less than .05% of the budget.

I confess I have a really low tolerance level for this sort of discussion of politics and economics, especially in this kind of more specialized internet forum. There are 10,000 blogs out there where one can argue generic issues of government spending, please don't do it here. I don't want to participate in that kind of political discussion and I don't want people who enjoy spending their time on that sort of thing to be doing it here.

If you have a point specific to the HEP budget, please feel free to discuss it here, but if it's a generic point about government spending, please take it elsewhere.

12. **SomeBody**
February 4, 2006

Woit: the DOE's $12 billion price estimate for the ILC was posted here a couple of days ago:

http://www.math.columbia.edu/~woit/wordpress/?p=339#comment-8266

Sure, it's not $12 in one year; that's not what I said. What I do say is that those 12 billions represent the total tax take from 1+ million people over one year. As I said, it's a way to get a feel for the reality behind those abstract numbers.

Sure it's a very small part of the total US budget. The same can be and is said for just about any single item in it. That's how the thing keeps exploding. Nobody feels their "tiny" little part is a problem. It's somebody's else's problem and somebody else's money. Result: death by a thousand cuts.

Do I have something specific to say about the HEP budget? Yes: the vast majority of people contributing to it, from the cleaning lady to the stock broker, will never benefit from it in any way, and wouldn't feel any loss were it to go to zero. And I personally find the thought of the cleaning lady supporting people with tax-financed salaries several times higher than hers, to do something which they love and which is generally perceived as fairly high status, but which will never be of any use to her, profoundly disturbing.

As for taxes on corporations, please remember that they are ultimately paid by their clients, i.e. individuals.

13. **Quantoken**
February 4, 2006

Peter:

Don't read too much into the 8% budget increase, taking away roughly 4-5% annual inflation it's only 4%. Not to meantion the value of US dollar is set for a major collapse soon. The extra money is barely enough to pay for the electricity bill for RHIC. At 100 megawatts power consumption and if left running year around, that's 0.8766 billion kilowatt*hour. At 12 cents cost per kilowatt*hour the electricity bill alone runs into $100 million.

We all know that further development of accelerators of ever higher energy level is quickly running into **prohibitive physical limits**. If you increase the energy 10 times, the accelerator ring will increase diameter another 10 times, the technical complexity and cost of the machine goes up 10 times, and the electricity consumption goes up a 10 times as well. There is really NO physical room for such growth as accelerators are already consuming electricity of whole cities, and occupying sites of whole city big, and takes monetary contributions of many countries just to built and run. Do you think you can get the next generation accelerator built on 8% budget increase? Not a chance even if you get a quadrippling of budget.

And if the new physics is not at TeV scale, but at a scale a few orders of magnitude yet higher (which is much more likely than not), or even close to Planck scale, then you are wasting all the money without discovering any new physics. If that happens that is the end of high energy physics research.

Quantoken

14. **sunderpeeche**
February 4, 2006

I'll refrain from any political comments (even w/o the post from Woit). Mainly I am motivated to write because of Ponderer ~ redirect HEP funding to condensed matter physics, spintronics etc.
("Wouldn't that money be better spent for condensed matter/materials physics research, including nanotechnology, optics, spintronics etc?")

Does it occur to anyone that the electron was discovered by JJ Thomson who was trying to understand the nature of cathode rays? Who among the general population in 1897 cared about corpuscles flying around in evacuated glass tubes? Was the electron really relevant to anyone's lives? Have you ever seen an electron? Does it matter to you?

Mankind has moved forward because of the persistent desire to know the unknown.

15. **SomeBody**
February 4, 2006

But sunderpeeche, all that was done with tabletop experiments, so that was also the scale to be expected of any (then) hypothetical applications coming out of that activity (which, besides, cost very little even by the standards of those days).

Earlier in this thread, Woit says that "If we discover a new stable particle […] there are a lot of things you can imagine doing with it". Really? If it takes an LHC to produce, what will be the scale of its (hypothetical) applications?

16. **sunderpeeche**

February 4, 2006

Ugh.

I guess this blog is for other people. I have to move on with my own life, a bumbling random walk though it may be. I am a fool.

17. **Chris Oakley**
February 4, 2006

I think that there is definitely a problem here. Being able to bring WW2 to an abrupt end boosted the stock of physicists enormously; Oppenheimer, Fermi and Feynman became like rock stars overnight. The public did not seem to have a problem with paying for the expensive toys they then wanted. As long as the brightest and best were driving the process taxpayers were able to some extent to share the excitement for the whole "ultimate question" project. The problem is that it is old hat now. Nothing really dramatic has come out of it. Bigger accelerators and more kinds of "fundamental" particle just counts as more of the same as far as Joe Public is concerned. If the project had led to wholly new things – wormholes, time travel, extra dimensions, etc. – the kind of thing that Superstring theorists like to fantasize about – then I am sure that particle physicists would not have to fight for their share of the science budget.

18. **Marty Tysanner**
February 4, 2006

Peter wrote,

First thing to keep in mind is that US HEP funding is under $1 billion per year, which is less than .01% of US GDP. At this scale, HEP funding is not crowding out funding for anything else worthwhile.

Later Somebody wrote,

the vast majority of people contributing to it [HEP], from the cleaning lady to the stock broker, will never benefit from it in any way, and wouldn't feel any loss were it to go to zero.

It is really unfortunate that Somebody's point of view holds as much sway as it does among people with a "practical" world view. It both assumes a narrow notion of "benefit" (often measured in monetary terms or direct impact on safety or lifestyle) and ignores the ennobling aspects of pursuits of knowledge or beauty for their own sake. It also ignores the fact that a large fraction of discretionary spending by individuals goes toward entertainment in its many forms (travel, camping, tobacco and alcohol, nice cars, sports, music, movies, and so on). That is, people as a whole *choose* to spend their money on things that often have no long term practical value to themselves, but they nonetheless feel it adds to the quality of their life.

Governments at all levels respond to this by allocating large amounts of tax money to subsidize and promote these activities, for example through regulatory agencies, purchase and subsidies of facilities, and law enforcement specific to those activities. Without this public investment much of this simply would not happen or would be available only to the wealthier segment of society. After all, governments became involved in the first place due to a perceived need.

One can look at HEP research spending in this light. It has little direct benefit to the general public, but it serves the many people who are curious about the inner workings of the world around them by giving them something to learn and be curious about. By uncovering new and surprising things in their wider evironment, discoveries make life more interesting to those people, and stimulate their imagination. And yes, even cleaning ladies and stockbrokers can have an imagination and be curious about the world around them, and thus can indirectly benefit from HEP research. Furthermore, many of us think that stimulating the imagination and sense of wonder in others is more ennobling than, say, indirectly promoting or subsidizing alcohol consumption.

Beyond the ennobling and "entertainment" aspects of HEP research, there are practical benefits to society (even cleaning service people and stockbrokers) as well, but again they are indirect. Peter mentioned technological spin-offs, but I think there is an even more important form of spin-off, one that relates to the future well-being of society as a whole. New discoveries stimulate the interest and curiosity of many intelligent students and can have a significant effect on later career choice. Probably everyone reading this blog has at least heard of a smart young person who was excited by scientific discovery and the prospect of exploring the unknown, and their imagination and desire were sufficiently stoked to encourage them to pursue a scientific or engineering career. Encouraging people to enter science and engineering is very important to the future competitiveness of a country, and thus to the standard of living of future cleaning people and stockbrokers, since people in science and engineering tend to earn more and thus disproportionately increase economic activity through purchases, investment and taxes. Further, if there are not enough people advancing technology and creating high technology products, that work will be done in other countries. It is not hard to see that a country that falls significantly behind scientifically and technologically will not offer its citizens as high a standard of living (and government tax receipts will drop too, leading to more debt burden).

Thus, HEP research, like basic scientific research in general, has an important role to play that goes well beyond direct practical benefit to the "vast majority of people contributing to it." Here, as is often the case, benefits cannot be measured in strictly practical terms, or the practical benefits that do result from it come long after the payment has been made.

19. **secret milkshake**
February 4, 2006

Chris, I disagree about 2 things: 1) Oppenheimer indeed was like a rock star and there was the whole sensational mystery of the bomb that grabbed the imagination of the public. It was exciting, great adventure to become a physicist. There was GI bill that greatly boosted uni admissions and physics benefited greatly. Atomic energy was a bright future, not the nightmare. And most of the funding for big accelerators came from DOE, with hope that happy physicists will again find something that is good for bombing the enemy.
2) Feynman was known to all the top guys from Los Alamos because of his success as a leader of the electromechanical machine computational group (that solved the questions about the implosion design) – but relatively speaking he was just teenager-looking bright man with a fresh degree from Wheeler who has not published anything. After war when he joined Bethe at Cornell, he was pretty depressed and occupied with writing the lectures and he was not producing any research for maybe 2 years. Once he put together his the path integral version of QED, his ideas were rather slow to catch on.

20. **John Gonsowski**
February 4, 2006

Peter, yea I was trying a little too hard to make my first comment with my new home page actually relate to my new home page.

21. **Not a Nobel Laureate**
February 4, 2006

Robert Rathbun Wilson, a Wyoming cowboy who built the world's highest-energy particle accelerator laboratory with the eye of an artist, the shrewdness of a banker and the conscience of a human rights activist

…

Wilson was not only a pioneering scientist, but a powerful spokesman for science. He reached a height of eloquence in his testimony before the Congressional Joint Committee on Atomic Energy in 1969. He was asked by Rhode Island Senator John Pastore about the value of high-energy physics research in the support of national defense.

"It has nothing to do directly with defending our country, except to make it worth defending," Wilson said.

http://www.fnal.gov/pub/ferminews/ferminews00-01-28/p1.html

There's no School like Old School.

22. **SomeBody**
February 5, 2006

Marty Tysanner, two points:

1) I have no problem whatsoever with people choosing to spend their hard-earned money however they please. They earned it; it's theirs to do what they want with. If the cleaning lady and stock broker really feel like you say about HEP, nothing's keeping them from donating money to it. (At this point there is usually some smart Alec who retorts "But we will never get 12 billion for HEP by voluntary donations!", apparently not realizing the implied condemnation of using tax revenue for HEP; remember how democracy is supposed to be by the people, for the people?)

2) There have never been so many things to spend on in science and technology which have both the potential to excite and enthuse those footing the bill, the general public, AND to produce real benefits for them. You need only pick up a few popular science mags to read all about them. How many articles about HEP do you see in those mags? In any given month, chances are that it's not even one. So this part of the argument is a red herring. Questioning the legitimacy of spending huge sums on HEP is not synonymous with questioning spending on science and technology at large.

23. **Haelfix**
February 5, 2006

I very much doubt there is anyone who can seriously point a finger at American research in high energy physics. As a fraction of population we so dominate the field its not even worth arguing about. Frankly its to the point where questions like this are more or less nonsensical. Everyone who is serious in science goes to conferences in the US, interacts with physicists working in American universities and so forth. At this stage, race/nationality/gender/whatever is a nonissue anymore, its more or less a global search, and ra ra ra patriotrism doesn't make much sense any more (eg is this particular scientist working at Berkley or at the university of Tokyo).

As far as I see it, we won that closed minded national race years ago, and had the foresight and intellectual integrity to make it a nonissue. Everything is now 'open source', and silly bickering about who is better than the other at this stage is ridiculous.

If you go to CERN, and other leading areas of particle physics, I very much doubt you'll find people who even think like that. That community isn't just European, its quite distinctly global in character.

24. **Quantoken**
February 5, 2006

Any one interested can check out the latest status of RHIC on this link. Curiously they just had some sort of **BTA power supply problem**, at 1:00 am Feb 5th, 2006. Any one knows what exactly does that mean? Is BTA the main power?

The RHIC accelerates completely ionized gold nuclear. I am wondering how they cheated on the energy. For example when you accelerate the nuclear to 1TeV, let say, since the nuclear is composed of many protons and neutrons, and when collision happens it's just invidual protons and neutrons hitting each other. So the effective energy of collision is actually much lower that the beam energy, since the energy of individual protons and neutrons is much lower than the collective energy of the whole nuclear. Any explanation?

Quantoken

25. **nuclearPancakes**
February 5, 2006

To answer Quantoken's question:

The meaningful parameter for energy in heavy ion collisions is \sqrt{s_{NN}} not \sqrt{s}. All results from the RHIC projects are reported as such, no "cheating" neccesary…

-nuclearPancakes

26. **J.F. Moore**
February 5, 2006

Q: BtA is the Booster-to-AGS transfer line. AGS is the older Alternating Gradient Synchrotron which is now incorporated in the RHIC complex. In short, it is a very small part of the system, but like most accelerators every small part has to work to do an experiment.

27. **Ponderer of Things**
February 5, 2006

just to point out – if it was up to me, I would half the military R&D budget and put the gained money into fundamental and applied sciences, including HEP.
But from the point of view of taxpayer I don't see a very compelling argument to continue supporting huge accelerators like ILC. I agree that there has been some "trickle-down" benefits, like synchrotron sources, but overall I am hard-pressed to point to some direct practical benefits of high energy physics from the past 30-40 years or so. I can see the type of arguments other areas of experimental physics will be making to improve their own funding situation, but not so much for HEP. Cancellation of SuperCollider project was the beginning of the trend, and if I had to guess, ILC is doomed as well – and the rest of the world will probably not support it if US bails out.

28. **woit**
February 5, 2006

Somebody:

You didn't quote the full text of the estimate for the ILC cost, it mentioned that the assumption was that only half would come from the host country. As far as I know, the ILC design effort still is not at the point of coming up with a final number for the total cost. One of the main goals of the design effort is to figure out how to do this as cheaply as possible. The people involved are well aware of the SSC fiasco. My guess is that $12 billion is an upper bound. The final cost estimate may be significantly below that, and if it is much above that the project will not be taken seriously. Half of that ($6 billion) I think is a good estimate of the upper limits of what the federal government might consider funding. If spread over ten years, that is $600 million/year, a large fraction of the current spending of over $800 million/year. So the ILC cost can't be accomodated within the current budget unless one shuts down almost all HEP labs and research for the duration. However, it could be accomodated within a not inconceivably large budget increase (there was this guy on TV recently going on about doubling the budget for physics research…)

As for arguments about how the average person wouldn't want their tax dollars used for this kind of thing, how about letting them speak for themselves? This is a democracy, which (if it were functional, but that's way off-topic…) works by letting people choose representatives who reflect their views to make this kind of choice for them. The HEP budget is debated by Congress each year, any ILC proposal will be exhaustively examined and debated, including all these issues of whether there will be any practical benefits, how people feel about spending these sums on what may be pure knowledge, and whether the money would be better spent on other forms of scientific research. Opinions held by people on these issues are all over the map, and don't divide nicely according to the standard partisan divisions.

29. **SomeBody**
February 5, 2006

Woit:

What difference does it make if half the taxpayers contributing to the thing are not in the US? We are still talking the equivalent of 1+ million people working for a year to finance it (actually more the less of the funding is from the US, since median income is lower in almost all other countries).

I agree completely with you that those footing the bill should be allowed to speak for themselves. We all know how that is done: by letting them spend their own money as they see fit (e.g. by donating it to a church, a charity, cancer research, the NSF or whatever they see fit) instead of handing it over to politicians.

You are an intelligent man, so you know perfectly well that the electorate's opinions about the value of HEP will never be properly represented by Congress or other elected body. It's just too small an issue to even be brought up in a campaign, other than possibly in special-interest constituencies which may hope to get a site (which, as usual, means that there are special interests lobbying FOR spending on this or that project, while nobody has a comparable interest in lobbying against it).

Letting elected politicians make the decisions in such a matter is therefore not an example of representative democracy; it's an example of chance & special interests at work.

30. **woit**
February 5, 2006

Somebody,

Sorry, from your rhetoric I think we fundamentally disagree about the proper role of government and how representative democracy should work Further discussion though would just lead to the kind of pointless back and forth about this that I think is a complete waste of time and that I don't want on this site.

So, I'll invoke my undemocratic, totalitarian powers here and announce I'm deleting any further discussion along these lines.

31. **plank**
February 6, 2006

"Feynman was known to all the top guys from Los Alamos because of his success as a leader of the electromechanical machine computational group (that solved the questions about the implosion design)"

I don't think this is correct. To my knowledge Feyman's group did solve a problem related to the implosion design but it turned out the problem was "unphysical" and thus the solution irrelevant.

Am I wrong?

32. **anon**
February 7, 2006

plank, Feynman says in one of his books that he was transferred to the IBM punched card sorters, becauses the guy in charge of that department was fired by Oppenheimer for wasting time. http://www.lanl.gov/history/atomicbomb/computers.shtml says: 'Feynman worked out a technique to run several calculations in parallel on the punched-card machines that reduced the time required.'

33. **Tommaso Dorigo**
February 7, 2006

Dear Woit,

I am spending some percentage of my time working at an improvement of the Level 1 trigger for the CMS experiment. My problem is that
1. I don't believe SLHC has any reason to exist, and
2. I don't believe it will, either.

Your job as theorists is to keep us motivated. I don't feel much so. Please help me: convince me that
1. LHC will find some hints that there is anything more than SM+higgs out there (ok, it will see the Higgs, so what ?)
2) LHC findings will prove that one needs to run at a x10 luminosity for a few more years, with the same projectiles and CM energy.
Would you ?

Cheers
T.

34. **Chris Oakley**
February 7, 2006

Hi Tommaso,

I apologise for the unsolicited reply, but personally I do not think that LHC or any other experiment will see the Higgs, because they probably do not exist – I say this on the basis that scientific explanations invoked out of desperation tend not to be the correct ones.
Keeping you motivated is not part of my responsibility as my presence was not required in the theoretical physics community 20-odd years ago, but, look on the bright side: what you are doing may have an application outside of physics. Who knows – you may even end up with an industrial process named after you.

35. **sunderpeeche**
February 7, 2006

Motivated?
Many accelerators which have become famous did so for reasons totally unconnected with the motivation for building them. The Bevatron at Berkeley was built to produce the antiproton (which did garner a Nobel Prize), but it really became famous for Alvarez's work with the 72″ bubble chamber (discovery of several resonances). SPEAR at SLAC was built as an e+e- collider (the linac could only do fixed target expts) but nobody realized SPEAR would produce the psi (charmomium) and the tau lepton. The AGS at BNL was built to demonstrate strong-focussing (and push proton synchrotrons to higher energy). But nobody guessed it would produce 3 NP-winning expts (i) muon neutrino was not same as electron neutrino, (ii) CP violation (iii) J particle (J/psi with SPEAR).

Machines that were built to find the top quark (PEP, PETRA, TRISTAN) did not find it (though PETRA demonstrated gluons via 3-jet events).

Nobody knows for sure what LHC will find.

36. **woit**
February 7, 2006

Hi Tommaso,

Probably the best advice is to not pay much attention to theorists, but, given that, here's the opinion of one theorist:

1. Supersymmetry, in the commonly studied form, is quite unlikely to exist. I doubt that there is such a thing as a superpartner for each particle with opposite statistics. I've always worried that LHC experimentalists will focus too much of their effort on this possibility, especially in designing the triggers. I'm curious to hear how much specific models like supersymmetry influence the trigger design.

2. I'm also rather dubious about any of the well-known extra dimensional models. They're not mathematically elegant, don't convincingly solve any problems of the standard model, and even if they are the way the world works, there's no good reason for their effects to be visible at the LHC, but not the Tevatron (i.e., no reason for the relevant energy scale to be in that range).

1. and 2. are pretty negative, but there is a strong positive case that something new and exciting will show up at the LHC which isn't visible at the Tevatron. You'll finally be getting to the energy scale where direct study of electroweak symmetry breaking begins to be possible. The fact of the matter is that there is no good theory of electroweak symmetry breaking, so in a sense this is the best possible situation for an experimentalist: you know you'll be in an unexplored energy regime where something new and important has to happen, but the theorists don't really have a good idea about it. For thirty years, experimentalists have been behind the theorists because the standard model at energies below the electroweak breaking scale is just too ridiculously good. Going from the Tevatron to the LHC, there is a serious chance that this situation will completely reverse itself.

More cautiously, there is a danger that at LHC energies all that will be visible will be a scalar standard model Higgs, with the interesting dynamics of electroweak symmetry breaking only visible at higher energies. That's the possible downside, and it's real, but I actually think the chances of seeing something unexpected are less than the chances of this.

My own best guess is that electroweak symmetry breaking is due to something theorists don't understand at all yet, maybe physics coming from a gauge anomaly, or even a new kind of mixing of space-time and internal degrees of freedom different than ones studied so far. If so, LHC experimentalists may be in for a very exciting and very confusing time.

As for follow-ons to the LHC, it seems like you'll have to wait until the LHC has run for a few years before being able to make a sensible decision. I don't know at all what the relative costs are, but if an energy doubling is possible, that seems more likely to turn up something new. The LHC energy may be just around all sorts of interesting thresholds, and getting above them might be much more important than more luminosity.

37. **Quantoken**
February 7, 2006

Peter:

Doubling beam energy would require either doubling the strength of the magnetic fields of the powerful superconductive magnets from 8.33 Tesla to 16.66 Tesla, or dig a new tunnel of double the diameter of the current LHC, and therefore also double the number of the magnet used along the circumference. The first is technically un-achieveable currently and the second one is also prohibitive in cost.

On the other hand, increasing luminosity involves just trying to squeeze the cross-section of the beam to smaller size at point of collision. But it's easily said than done. It takes years of patient fine tuning of numerious instrument parts to just get it right to achieve the luminosity at design level.

Quantoken

38. **Tommaso Dorigo**
February 8, 2006

Thanks for your answers here to the various contributors… A few points:
1) I am quite happy to agree with most if not all of what you (woit) say about the new physics the LHC will probably be able to explore.
2) the SLHC needs to be funded and a strong physics case be made well beforehand, which means possibly now or in the next couple of years – thus dramatically without any real knowledge of whether the LHC will work at all.
3) going to more energy is unfeasible – I agree – but going to 10^35 is most probably just a matter of time and patience, no huge upgrades being necessary. That a x10 lumi does not buy as much as a x2 energy I totally concur.

Cheers,
T.

Down the Rabbit Hole

February 5, 2006
Categories: Film Reviews

About a year and a half ago I wrote here about going to see the movie What the Bleep Do We Know?, a rather spectacularly stupid and lunatic film which extensively misuses quantum mechanics. This weekend, a sequel called What the BLEEP – Down the Rabbit Hole opened here in New York, and I figured I owed it to my readers to check out the this new movie.

There were two good things about it. First of all it was advertised as being 2 hours and 34 minutes long, but ended about 15 minutes earlier than I expected (I kept checking my watch…). Secondly, I don't have to write a lot about it and can just refer you to the posting about the first film since a large part of it is exactly the same.

The whole plot involving Marlee Matlin appears to be exactly the same footage. It was pretty painful to have to watch this again, although I am kind of fond of the wedding party/orgy scene. The "scientists" involved were essentially the same group of crackpots as in the first film. It looked like the interviews in this version were mostly outtakes from the first version, with some additions. Among the physicists, about the only non-crackpot was Columbia philosopher of science David Albert. He was said to have objected to the editing of the first version, which made it appear that he agreed with the nutty ideas about quantum mechanics of the filmmakers. In this version, he is saying perfectly sensible technical things about quantum mechanics, but they're embedded in the middle of the nuttiness about QM promoted by the filmmakers (the usual: entanglement=we are all connected, superposition=anything you want to be true is true).

The new material includes interviews with a crackpot parapsychologist (Dean Radin, from the "Institute of Noetic Sciences"), and a crackpot journalist (Lynne McTaggart). It also includes some new animations featuring a cartoon character (Captain Quantum or some such). The first of these starts off with a not-bad depiction of the two-slit experiment before getting silly. The second is tacked on near the end and brings in a new exciting idea that wasn't in the first film: Extra Dimensions! Captain Quantum liberates some poor fellow cartoon character who is trapped in 2d due to her fearfulness, bringing her to enlightenment by showing her that there is a third dimension. There's mercifully little about string theory, mostly John Hagelin going on about how the superstring field is the field of consciousness.

If you feel the need to know more about this for some odd reason, there's a web-site, and a bunch of reviews of the film: a credulous one from Seattle, and more sensible ones from Portland ("feels like a lame, double-dipping cash-grab"), and Arizona ("They should market What the Bleep!?: Down the Rabbit Hole like a breakfast cereal – '50 percent more nuts.'").

Update: Since the Sunday New York Times Book Review now every week has something about string theory, I guess I better mention today's edition, maybe just by quoting from one review, by Dick Teresi about The Fated Sky: Astrology in History, a history of astrology by Benson Bobrick.

Shortly into my marriage (about six hours) my wife purchased a white-noise generator to counteract my night terrors… Recently, it has begun dispensing orders: "Kill, kill your publisher."

The mathematician Michael Sutherland diagnosed my condition. "It's called apophenia," he said. In statistics, apophenia is a "Type 1 error," a false alarm, the experience of seeing patterns in meaningless data. I must have caught it from the theorists I interview.

In the early 20th century, experimenters demonstrated that randomness rules… Yet today superstring theorists insist they will reconcile the lumpy, acausal quantum world with the smooth determinism of relativity…

So when the playful and innovative historian Benson Bobrick writes in "The Fated Sky" that 30-40 percent of the American public believes in astrology, I am shocked. Why so few, given the raging apophenia among our scientific elite? Astrology, the belief that human lives are ruled by the stars and planets, is no nuttier than current cosmological models, which feature an "anthropic principle," giving our puny, three-pound brains a central role in the universe…

Traditional astrologers, like string theorists and cosmologists today, were often wonderful mathematicians…

Modern man can choose from a veritable smorgasbord of Type 1 errors: string theory, neo-Darwinism, cosmology, economics, God. Astrology is as good as any, and Bobrick demonstrates that it has a rich, colorful past to draw upon. As for me, I answer to a higher authority. Now, if you'll excuse me, I must go kill, kill my publisher.

Teresi's take on modern physics is much sillier than John Horgan's. Maybe the next few weeks letters columns will have letters pointing this out.

Comments

1. **J.F. Moore**
   February 5, 2006

   Oh great. When the first one came out I had to deal with the bubbling of various acquaintences who thought it was compelling stuff. Generally I was labeled as 'closed minded' for dismissing the quackery too quickly. I couldn't claim a single 'conversion' to critical thinking or skepticism on any of the topics. My depressing conclusion was that this kind of thing really speaks to something in a large part of the populace, probably the same people who listen to Noory's show on AM – the one Kaku makes guest appearances on.

2. **Sean**
   February 5, 2006

   It's not really a sequel, just an expanded version of the original. For what it's worth.

3. **D R Lunsford**
   February 6, 2006

   Astrology and alchemy are actually early forms of psychology, hence not subject to comparison with what we know as hard science.

   Also, there are bodies of knowledge not arranged around causality.

   -drl

4. **Scott Aaronson**
   February 6, 2006

   Just thought I'd contribute a trackback to my What the Bleep!? post. Excerpt:

I suppose I'll eventually have to don a fake mustache, clothespin my nose, and go endure this movie, since people often bring it up when I tell them what I do for a living:

ME: …so, at least in the black-box model that we can analyze, my result implies that the quantum speedup for breaking cryptographic hash functions is only a polynomial one, as opposed to the exponential speedup of Shor's factoring algorithm.

PERSON AT COCKTAIL PARTY: How interesting! It's just like they were saying in the movie: reality is merely a construct of our minds.

5. **Don Smith**
   February 6, 2006

   When I heard about the first movie, I thouht it was a film of the imagination. Then I heard it advertised on Art Bell's raido show and then I learned that this was suppose to be a work of fact. It came close to another very bad movie: "UFOs Destination Earth".

6. **Lucifer**
   February 6, 2006

   It must have indeed been torture to watch that movie again! I could barely get through it myself, only by fast forwarding through certain parts including the wedding party/orgy scene, was I able to subject myself to such excruciating mental distress. Which makes me wonder how such a terrible movie has become so popular? I know Dr Fred Alan Wolf was touring Australian universities down here late last year, charging $150 for a 3 hour seminar called "The Spiritual Universe", and I'm sure that's just a small part of a much larger world tour to promote his books and the movie.

   It gives me such peace of mind to know, that Dr Quantum as he is also known, is seen as such an authority in this field.

7. **Juan R.**
   February 6, 2006

   D R Lunsford Said,

   > Astrology and alchemy are actually early forms of psychology, hence not subject to comparison with what we know as hard science.

   Hum! CHEM 308 may disagree 🙂

   There were several "alchemies" and your generic comparison with psychology completely wrong. There are a lot of misconceptions about alchemy by outsiders of chemical science.

   Preparative (not philosophical) alchemy was "so hard" as current science. In fact, several early alchemical processes, methods, and instrumental are still used in any current chemistry laboratory (see CHEM 308 in http://www.humboldt.edu/~catalog/courses/chem_crs.html).

   E.g. any modern chemist has used in some time one of the discovered alchemical processes: Maria's bath.

   Fractional Distillation

   See also What Working Engineers Knew!

   It is also used in the kitchens of all the world…

   au bain Marie

   —

   Juan R.

   Center for CANONICAL |SCIENCE)

8. **Kent Bye**
   February 6, 2006

   I understand the message of the original What the Bleep, but I disagree with the strategy and tactics of the filmmakers.

   From a scientific and journalistic perspective, the filmmakers only interviewed people who agreed with their perspectives. I would have liked to have seen more dissenting viewpoints incorporated within the film — especially in this latest expanded version of the film.

   The argument that they're trying to make is that is that Biology & Psychology are trapped within a Philosophy of Science of Reductionism and that moving towards a "Quantum Ontology" would help incorporate the subjective aspects of our consciousness within healing modalities.

   > * Mainstream Medicine = Objective = Reductionistic = Classical Netownian Physics
   > * Complementary & Alternative Medicine = Subjective = Interconnected & Holistic = Quantum Ontology

   In other words, our beliefs, perspectives and worldviews are the first filter of our experiences and can actually have biological correlations. So instead of taking pharmaceutical drugs to feel better, there are less invasive methods of Complementary and Alternative Medicine that could relieve symptoms and cure the underlying disease. People are turning to these methods because they seem to work even though mainstream science hasn't validated them yet.

   There was a recent front-page New York Times article called "When Trust in Doctors Erodes, Other Treatments Fill the Void" that helps explain why What The Bleep has struck such a nerve:

   > Haggles with insurance providers, conflicting findings from medical studies and news reports of drug makers' covering up product side effects all feed their disaffection, to the point where many people begin to question not only the health care system but also the science behind it.

   Americans are spending around $27 billion per year on CAM therapies because of an "increasing distrust of mainstream medicine and the psychological appeal of nontraditional approaches as with the therapeutic properties of herbs or other supplements."

   The NIH set up the National Center for Complementary & Alternative Medicine to help research and vet some of these Mind-Body treatments, and so there has been some science-based research done in this area.

What the Bleep is obviously not trying to build any bridges with mainstream science, but is trying to reach the audience that is already actively engaged in these types of Mind-Body practices like meditation and yoga. It's entertainment, and not science or even good journalism. But it is good enough entertainment for those who are sympathetic to the larger critique of the quick-fix mentality of modern medicine.

The film started out in one theater, and was able to build up enough word of mouth to spread to other theaters, and it was eventually picked up by a distributor to get a wider release. I've written about the grassroots marketing implications of the film from a independent film perspective that cites a NYT article on the viral spread of the film.

I've also actually interviewed a number of people from this community for my collaborative documentary on the state of American journalism.

In my case, I was trying to draw parallels between:

* Mainstream Medicine = Objective = Mainstream Journalism
* Complementary & Alternative Medicine = Subjective = Blogging

There are obviously a lot of problems with how journalism covers science issues that can be traced to a reductionistic "He Said / She Said" mindset that takes whatever the political institutions have to say about issues as gospel — even if there is a critical mass of dissent from the academic community.

I found some interesting insights into this problem from the interviews, and I'd love to hear more feedback on it.

I'm going to release the audio soon, and would to have some of your critical perspectives to help peer review some of the material that I've conducted. That way I don't end up including information in my final film that hasn't been fully vetted. Thanks

9. **Eric Dennis**
February 6, 2006

Kent,

That's really great, and I feel your energy, man. But the problem is that your pet ideas on psychology and pharmacology have no connection whatsoever to quantum mechanics. That you seem to know this and it doesn't give you pause betrays a basic intellectual dishonesty which is not going to be particurarly effective when who you're trying to snow is a group of professional scientists. I mean aren't there any tarot card blogs or bulk granola sales sites that may be a little more on your wavelength?

10. **woit**
February 6, 2006

Please, stop it with the personal hostility.

And alternative medicine should be discussed elsewhere. I personally have no problem with the idea that the mind-body interaction is trickier than modern medicine sometimes thinks, but have never seen any evidence that this has anything to do with quantum mechanics. This is a blog about physics, not about biology or medicine.

11. **Kent Bye**
February 6, 2006

The problem with What the Bleep is that the filmmakers try to make it seem like Quantum Physics has already bridged these gaps.

I realize that the gaps haven't been bridged yet, but I believe that they may be on a converging path that could be bridged within the scientific community within the next 5, 10, 20 years.

But I believe that there are underlying grains of truth within What the Bleep that are undermined by the heavyhanded tactics of the filmmakers. You check out NIH's NCCAM for scientifically-validated research in this field.

But I'm **more interested** in the parallels between the split between Mainstream Medicine and Complementary & Alternative Medicine and Mainstream Journalism and New Media (i.e. blogs, podcasts, videoblogs)

Mainstream medicine views CAM treatments as too subjective and invalid just as mainstream journalists view blogs as too subjective and invalid.

There are grains of truth in both approaches, and I'm particularly interested in how to bridge these into a more inclusive journalistic paradigm — specifically through collaborative media.

And it is in that spirit that I want to add more scientific peer review principles to the process of journalism and filmmaking.

12. **Dumb Biologist**
February 6, 2006

I'm reminded of a statement I read someplace, to the effect that the philosophical vacuum left by the retreat of organized religion can be as bad or worse than what came before it. At least before there was a measure of cohesion and discipline. The New Agey flapdoodle that springs up in every crack left in the old, decaying edifices of the major Western religions often aggressively misappropriates a laundry list of the more "out-there" philosophical ramblings of various scientific luminaries on the nature of existence and epistemology. The result is a superstitious Frankenstein's monster of the most horrifically ecumenical sort, where Christ occupies the captain's chair next to extraterrestrial aliens telepathically linked with Gaia and the few New Age cognoscenti who are gifted enough to "grok" the significance of modern revelation.

Oh, and of course, anyone who holds sentiments of the above sort is terribly uncritical, unskeptical, and close-minded, a slave to a dogmatic materialist religion that stifles true understanding, or so we'll be told. I tend to think, in the face of it all, that no matter how much knowledge we acquire, and no matter how successful the methods of skeptical inquiry in producing concrete results, we will *never* be free of superstitious nonsense. It's an in-built trait, and the price of admission to sentience, in all probability. Perhaps it's more amazing that some percentage of the population appears to be more free of it than others.

13. **Kent Bye**
February 6, 2006

"And alternative medicine should be discussed elsewhere."
I apologize for the previous post, I had already hit the submit button.

"never seen any evidence that this has anything to do with quantum mechanics."
I agree that there hasn't been anything convincing published making a direct connection. From the interviews I've done, my observation is that this community uses metaphors from quantum mechanics to intuitively understand certain healing concepts.

I also believe that What the Bleep dishonestly blurs this line, and as a result undermines aspects of alternative medicine.

14. **matt d**
February 6, 2006

We sure do have a lot of "functional" nutjobs in America anymore. I mean, not only are wackos like Teresi able to vomit up their baseless and pointless views without any formal counter-arguments, but they're able to get jobs that allow them to do it.

I'm sure it pays well too.

You're doing a great job keeping the herd in line, Woit.

15. **Not a Nobel Laureate**
February 6, 2006

As a profitable and low risk business venture, there are few areas of endeavour that can beat out Religion of either the Established or New Age Nutjob variety.

16. **secret milkshake**
February 6, 2006

people making crazy claims about their remedies/beliefs usualy do not not irk me much – you can try to prove or disprove such claims if you care enough about the particular subject. But things need to be spelled out. What really gets me are the charlatans that are throwing fog; obfuscation is such a common and profitable form of dishonesty.

17. **Chris W.**
February 6, 2006

Kent Bye: *From the interviews I've done, my observation is that this community uses metaphors from quantum mechanics to intuitively understand certain healing concepts.*

Few abuses of scientific ideas are as widespread as the abuse of them as a source of "metaphors," without any serious effort to critically examine their applicability to the domain in question. Some aspects of Newtonian mechanics used to get this treatment, but they're now considered old hat.

18. **Boaz**
February 7, 2006

Regarding "abusing" science for metaphor…
I used to really fight against this. My sister would ask me about my research (accelerator physics theory) and I would say a few words and she'd quickly find that the words could be used metaphorically to say something she thought was interesting about something entirely different. For example, instabilities are sometimes described as a "resonance" and perhaps just the linking of these two words could seem (or be!) insightful. My response used to be to immediately shut this down by saying "no! resonance has a very precise technical meaning and has nothing to do with human relationships or sociology". But I've become more tolerant and now I don't really see a reason to stop this thinking as long as nothing false is said about the technical subject. I know that this can be insidious in a public debate when the authority of science is borrowed to support something false or some bad logic as is done in "What the bleep". But the scientist's job is only to correct false statements about the world, not to prevent concepts from being used metaphorically. Do you really know enough about these "alternative healing practices" to know that quantum mechanics doesn't provide some useful metaphors?

19. **Adrian H.**
February 7, 2006

Well here is some good news. Many of my students saw 'What the Bleep' when it came out, and mentioned it in class, but all of them seemed to realise it was crackpot nonsense. No one defended it; no one thought it was anything other than a bunch of people tossing about loose ideas.

The person who puzzles me is this John Hagelin guy. Did he really go to Harvard?? I saw a videod talk by him once and swear it was the sort of stuff that a freshman would have laughed at. Ridiculous statistics, ignorant basic physics. It wasn't even a good con. Surely he could pull the wool over our eyes in a more respectful way?

20. **robert**
February 7, 2006

Theoretical physicists are not wholly innocent when it comes to invading alien subject areas (for example economics, sociology, epidemiology) convinced that they have something immediately useful to add. Sometimes this is fruitful (Simons' money making machine), sometimes it is not (LTCM, catastrophe theory applied to prison riots). Sensible interdisciplinary dialogue is essential, especially in troubled times like these when all rational thought seems to be, at best, undervalued and, more likely, under concerted attack. Too often, however, things descend to the level of 'Science Wars' and what the bleep; one wonders, as Tom Tom Club put it, 'What are Wordsworth?'

21. **Kent Bye**
February 7, 2006

Constantino Tsallis talks about the importance of metaphor to the advancement of science in an Physica A article called "Some Thoughts on Theoretical Physics"

> "The greatest thing by far is to be a master of metaphor. It is the one thing that cannot be learned from others; it is also a sign of genius, since a good metaphor implies an eye for resemblance." wrote Aristotle in his *Ars Poetica* [322 AC]. And many — perhaps virtually all — scientists, conscious or unconsciously, take as granted that without metaphors, no scientific progress would exist. One may go even further: without metaphors *that have some form of beauty*, no efficient progress in science would exist, no new ideas would emerge!

After reading this article, I do believe that there is a lot of value of turning to quantum physics for metaphors that can add layers of complexity and predictability to other knowledge domains.

Yet at the same time, I think that it is important to disclose the degree of connection to the original domain as well as to make clear that you're using mathematical models in a metaphoric way of understanding how systems might operate. By this standard, "What the Bleep" fails on both accounts and too often blurs the line between the fringe frontier research and mainstream scientific enquiry.

22. **Juan R.**
February 7, 2006

> Constantino Tsallis talks about the importance of metaphor to the advancement of science in an Physica A article called "Some Thoughts on Theoretical

Physics”

It would be good if one day he talk about

the importance of science to the advancement of metaphors

Then, maybe he would discover like metaphoric is his proposed entropy and the little science around those lot of papers and talks published by the Brazilian group.

I are reading lot of appeals to quantum mechanics and supposed links with other stuff. I wonder if they know they are really talking…

In my talks with many people i see that word quantum is used as a sinonym of fantastic, unusual, etc. instead of in his original sense of quantum. Quantum mechanics is a “mechanics of the quantum”.

—

Juan R.

Center for CANONICAL |SCIENCE)

23. **anon**
February 7, 2006

Adrian H. said:
“The person who puzzles me is this John Hagelin guy. Did he really go to Harvard?? I saw a videod talk by him once and swear it was the sort of stuff that a freshman would have laughed at. Ridiculous statistics, ignorant basic physics.”

I am much less surprised by Hagelin’s behaviour than you are. I don’t find him that different from the not so uncommon academics that drum up funding support by generating university press releases and grant proposals that exaggerate/lie about the importance of their work. Or the not so uncommon academics that deliberately omit to mention important references from their papers. Personally, I think Hagelin got taught this behaviour you find so objectionable by a broken academic research system. The question is, how to fix it.

24. **Tony Jackson**
February 7, 2006

“….and a crackpot journalist (Lynne McTaggart).”

Alas, I know her only too well. It doesn’t surprise me that she shows up in this movie. She’s not just a crank, but a dangerous crank who has an astonishing, and frankly scary influence on a lot of otherwise intelligent people over here in the UK. Here is some useful background information on her.

25. **Tony Jackson**
February 7, 2006

Oops… broken link:

“….and a crackpot journalist (Lynne McTaggart).”

Alas, I know her only too well. It doesn’t surprise me that she shows up in this movie. She’s not just a crank, but a dangerous crank who has an astonishing, and frankly scary influence on a lot of otherwise intelligent people over here in the UK. Here is some useful background information on her. .

26. **Johan Richter**
February 7, 2006

Using metaphors to get ideas should be acceptable. After all, the use of differential calculus in areas far removed from what Newton wrote about can be seen as a form of metaphor.

The problem begins when you start thinking that the meatphor proves something in itself as opposed to using it to generate ideas that are tested in the ordinary way.

27. **Michael**
February 7, 2006

I’m not a physicist. I have not seen the film. I do not wish to. I do care very much about this subject, however. I’m sure it is of no surprise to anyone that the film was suggested to me by a post-modern poet. I must remind everyone that such anti-philosophies often arise in the absence of a logical foothold within a society. Paying attention to meaningless information is the fastest way to lose your grip on reality (and a population losing its grip on reality is the quickest way for someone to gain corrupt control of power). Getting a [descriptive] grip on reality is the goal of science at large and the only way to maximize our ability obtain resources and to control our surroundings. Humans cannot surive in the absence of progressive external manipulation. We would be an easy meal without our reasoning faculties. It is hence, undeniably in our best interest to understand an undisputed framework of concrete reality, if only from a survival stance. Survival of our species matter to everyone, so everyone should care about these matters. Those who do not are either lying or of no concern to those who do. Objective science is the only solution to life on this planet. Everything else is a baby blanket.

That being said, the beauty of science is that it not only demands, but patiently awaits proof. This is important and cannot be ignored. No one is up in arms about string theory because we all assume it will see its day in court. Looney ‘Field of Consciousness’ are, however, cause for concern. Proper description of reality demands proof. I am not demoting mathematicians. They are quite adept at proving the reality of the number models they have generated. In the temple of science, the mathematicians’ product, symmetrically sucinct theory, is the shrine. It is the most elegant representation of a given phenomenon. It is, alas, but a representation. Nature, as experiment continues to insist, is not elegant. It is a thick mesh of bruised patterns. The bruises are obvious from the data. Mathematics shows the patterns. Ultimately, mathematics can be no more accurate in modeling reality than the claim that a basketball is a sphere. This does not, in any way, detract from invaluable potential modeling offeres to human kind. It is, in fact, our only strength as a species. We abstract solutions and conquer situations. For questions concerning issues beyond contemporary technolgical capabilities, math must suggest direction. Possibilities, not realities. By its own definition, math is not a complete description-it is self referencing (remember Godel?). But it is the next best thing, it is consistant. This is a quality crackpot “Reality is Whatever the mind wants it to be” theories could stand to be evaluated (and obliterated from existence) by. Remember when the heliocentric universe went out the window? It didn’t stand up to the data. If string theory goes the same way, it would not surprise or sadden me…it would mean data had been generated on the subject, period. Wouldn’t it be fun to turn the theoreticians loose on even two shards of data? For now (until somebody builds better probes), poor guys like Hoit will go on serving up maps to blind physicists on where to dig for that buried treasure.

28. **Chris W.**
February 7, 2006

"poor guys like Hoit"? Who is Hoit?

29. **Chris W.**
February 7, 2006

(PS: "thick mesh of bruised patterns" is a beautifully evocative phrase.)

30. **xpinor**
February 8, 2006

Peter Woit said:*I personally have no problem with the idea that the mind-body interaction is trickier than modern medicine sometimes thinks, but have never seen any evidence that this has anything to do with quantum mechanics*.
You may want to check out the **dissipative quantum model of the brain**: http://arxiv.org/abs/quant-ph/0002014

31. **Subhash**
February 8, 2006

I have neither seen the Bleep nor its sequel, and I am perfectly willing to accept that its treatment of quantum mechanics is unsatisfactory. But I did see the much touted Elegant Universe on the PBS, and its treatment of quantum mechanics was also extremely unsatisfactory.

Given that there are diverse interpretations of qm, ranging from positivist to hidden variable, to many-worlds held by serious scholars, not to mention the invoking of consciousness by qm theorists like Wigner and Stapp and neurophilosophers like Pribram, perhaps all one should hope for is a narrative that leaves the viewer with more questions than what he started with before the movie.

If the Bleeps do that successfully, then they might not be all that bad.

32. **Alejandro RIvero**
February 9, 2006

I also disagree with Lunsford statement "*Astrology and alchemy are actually early forms of psychology, hence not subject to comparison with what we know as hard science*." but I think it is useful to consider how a science becomes to be used in psychology (or even self-healing books) because theoretical physics is always in the risk of providing concepts for this psychological focus. We have all seen Energy, Chaos, and even Symmetry, to be used with this goal. Nor to speak of Relativity and Quantum. And even semiconductors ("crystal healing"). But it is even worse in other sciences. Botany, for an instance, as part of the pharmacy sciences. I think the only defense is to get everyone involved on science. And to be honest: if we know that Mercury theroretically transmutes to Gold with exotermic production of Energy, just tell it, but explain it.

33. **Alejandro RIvero**
February 9, 2006

hmm, actually Lunsford statement could be almost correct if telling "Astrology and alchemy are nowadays rudimentary forms of…"
I say "almost" because I know of some living non-psychology oriented alchemists. In fact you can see that Alchemy books are not a best-seller in the "self-healing" sections of libraries, when compared to astrology. This is because of the experimental bias.

34. **Juan R.**
February 9, 2006

Xpinor said,

Peter Woit said:I personally have no problem with the idea that the mind-body interaction is trickier than modern medicine sometimes thinks, but have never seen any evidence that this has anything to do with quantum mechanics.
You may want to check out the dissipative quantum model of the brain: http://arxiv.org/abs/quant-ph/0002014

Peter Woit continues being right: there is not evidence!

The preprint you cited is not a scientific model. "That" is a rather speculative work, with "few" rigorous correlation with real-world data. The development of the model is not rigorous. There are lots of " ". In fact, even the treatment of pure dissipation regimes is non-rigorous and full of half-proofs, asumptions, and several "ifs".

It is also interesting the use of term "quantum" when some of limits worked are precisely classical limits. In the past there was a lot of speculation about quantum behavior of the brain, precisely lot of scientific rigorous **working** models and experimental data suggests that main brain regime is classical one. For example fluctuation currents in ion tansport on two state biological chanels involved in certain basic brain "metabolism" is in perfect correlation with computer simulation of classical models, etc.

From Max Tegmark [quant-ph/9907009]

Based on a calculation of neural decoherence rates, we argue that that the degrees of freedom of the human brain that relate to cognitive processes should be thought of as a classical
rather than quantum system, i.e., that there is nothing fundamentally wrong with the current classical approach to neural
network simulations.

Tegmark also critizes some quantum models of brain elaborated employing string theory methods.

Many people appears to misunderstand long-range correlations doing the brain work as a whole and the so-called non-markovian dynamics (introducing memory effects) with quantum nonlocality or any other exotic quantum effect.

Subhash,

I would prefer "*Given that there are diverse* ***philosophical*** *interpretations of qm*".

—

Juan R.

Center for CANONICAL |SCIENCE)

35. **Tony Smith**

February 9, 2006

Peter Woit said that he has "… never seen any evidence that … the mind-body interaction … has anything to do with quantum mechanics …".

spinor mentioned "… the dissipative quantum model of the brain: http://arxiv.org/abs/quant-ph/0002014 …".

Juan R said "… The preprint …[quant-ph/0002014]… is not a scientific model. "That" is a rather speculative work, with "few" rigorous correlation with real-world data. … From Max Tegmark [quant-ph/9907009] "Based on a calculation of neural decoherence rates, we argue that that the degrees of freedom of the human brain that relate to cognitive processes should be thought of as a classical rather than quantum system" …".

There have been several quantum models of consciousness in the literature, including work by Hameroff and Penrose (coherent states of microtubule tubulins) and Nanopoulos et al (based on string theory).

None of those models (AFAIK) has been either fully confirmed by exerimental evidence or fully refuted by experimental evidence,
but
at least the Hameroff-Penrose model was motivated by Hameroff's experimental observation that tubulin activity ceased when people were unconscious (under anesthesia) and resumed when they regained consciousness (Hameroff is an anesthesiologist).

As to experimental testability of such models, experiments are difficult, but one necessary aspect of such models (coherent tubulin states) can at least approached, as Tegmark did in his work cited by Juan R.
However,
Tegmark's assessment of decoherence has been challenged by, for example:

Hagan, Hameroff, and Tuszynski, in Physical Review E, Volume 65, 061901, published 10 June 2002, where they say: "… recalculation after correcting for differences between the model on which Tegmark bases his calculations and the orch. OR model (superposition separation, charge vs dipole, dielectric constant) lengthens the decoherence time to 10^(-5) – 10^(-4) s …"
and
Mershin, Nanopoulos, and Skoulakis, in quant-ph/0007088, where they say: "… the particular geometrical arrangement (packing) of the tubulin protofilaments obeys an error-correcting mathematical code known as the K2(13, 2^6, 5) code … We conjecture that the K-code apparent in the packing of the tubulin dimers and protofilaments is partially responsible for keeping coherence among the tubulin dimers. …".

In short,
the question of whether or not consciousness is a quantum phenomenon is still an open one,
and (in my opinion) theoretical model-building and experimental testing of such model components is a good thing that might lead to useful insights.

As an example of such a possibly very useful insight that should be further investigated, see Hameroff's paper in Biosystems, Volume 77, Issues 1-3 , November 2004, Pages 119-136. Its title is
"A new theory of the origin of cancer: quantum coherent entanglement, centrioles, mitosis, and differentiation"
and its abstract (on the web at http://dx.doi.org/10.1016/j.biosystems.2004.04.006) says in part:
"… It is proposed here that normal mirror-like mitosis is organized by quantum coherence and quantum entanglement among microtubule-based centrioles and mitotic spindles … Impairment of [such] quantum coherence and/or entanglement … can result in abnormal distribution of chromosomes, abnormal differentiation and uncontrolled growth … New approaches to cancer therapy and stem cell production are suggested …".

To make my position on such matters clear, I am NOT defending statements like the one presented by Peter with respect to What the BLEEP – Down the Rabbit Hole:
"superposition = anything you want to be true is true".
Actually, the "anything you want to be true is true" part sounds to me a lot like Susskind's Landscape.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

36. **J.F. Moore**
February 9, 2006

The Hameroff-Penrose theory may have been interesting 15 years ago after it was introduced, but it hasn't stood up to scrutiny and is at this point hopelessly riddled with unexplained problems, not the least of which is how coherence can be maintained on such scales (time, space, temperature). I put it in the category of "strains credulity, theory almost certainly wrong, but maybe something interesting in there somewhere", right next to cold fusion.

37. **John Gonsowski**
February 10, 2006

Hameroff is actually in the movie. The movie seems like something Sarfatti would have been good at (in an entertainment sense). Sarfatti is into consciousness models also. I'd hardly say the models are hopelessly riddled, things like single electron transistors and brain wave frequencies seem related to the models.

38. **Juan R.**
February 10, 2006

Therefore, i may conclude that my previous

> Peter Woit continues being right: there is not evidence [that the idea that the mind-body interaction has anything to do with quantum mechanics]!
>
> (you are not cited in above reply Tony Smith) may be still true.
>
> I do not ignore the possibility for some adittional now unknown quantum effect in brain, in fact, the electronic structure of molecules into brain is just modelled via quantum mechanics and, hidrogen bonding and others are just quantum effects.
>
> I clearly disagree is that the whole brain may be considered a quantum body or that consciousness was quantum in essence. This kind of speculation began after of incorrect understanding of quantum measurement processes just after QM was formulated.
>
> I agree with J. F. Moore in that some ideas about microtubules and abnormal coherent states are considered near crakcpotism in biophysics.
>
> I do not know the details of last Hameroff paper you cite and therefore cannot be precise but reading the abstract
>
> The elegant yet poorly understood ballet-like movements and geometric organization occurring in mitosis have suggested guidance by some type of organizing field, however neither electromagnetic nor chemical gradient fields have been demonstrated or shown to be sufficient.

it appears that my previous comment on that many people appears to misunderstand long-range correlations doing the brain to work as a whole with quantum nonlocality or any other exotic quantum effect gains support.

Apparently from the abstract (of course, i can be completely wrong), the philosophy of Hameroff in the article is “it is nonlocal and cannot be explained by a local classical field then is a quantum exotic effect”.

—

Juan R.

Center for CANONICAL |SCIENCE)

39. **Juan R.**
February 10, 2006

the structure of above post is wrong.

The quoted part would be just

“Peter Woit continues being right: there is not evidence [that the idea that the mind-body interaction has anything to do with quantum mechanics]!”

—

Juan R.

Center for CANONICAL |SCIENCE)

40. **The Easter Bunny**
February 11, 2006

“Whatthebleep? is entertainment. Lighten up!”
-Stuart Hameroff M.D.

ps. ” Anton Zeilinger’s experimental demonstration of quantum wave behavior…”
http://www.quantum.univie.ac.at/zeilinger/

‘Quantum Teleportation and the Nature of Reality’
http://www.btgjapan.org/catalysts/anton.html

‘Talking physics with the Dalai Lama’
“Zeilinger had invited the Dalai Lama to his laboratory following a meeting at Dharamsala in Northern India last October at which he and four other physicists had, over the course of five days, discussed physics and cosmology with the Buddhist leader.”
http://physicsweb.org/articles/news/2/8/13/1

41. **xpinor**
February 11, 2006

There is an overview over various approaches by Atmanspacher at: http://plato.stanford.edu/entries/qt-consciousness/

SLAC Physicists Develop Test For String Theory*

February 8, 2006
Categories: Uncategorized

The SLAC web-site today has a feature article entitled SLAC Physicists Develop Test For String Theory*. The "*" refers to a footnote to the title saying "Under Certain Conditions". This is about the 10^{500}th news story making this kind of announcement that has appeared over the past twenty years (like this recent one), and the title is just as incorrect and misleading as all the others.

The story starts with

String theory solves many of the questions wracking the minds of physicists, but until recently it had one major flaw — it could not be tested. SLAC scientists have found a way to test this revolutionary theory, which posits that there are 10 or 11 dimensions in our universe.

and is about a paper by JoAnne Hewett, Ben Lillie and Thomas Rizzo entitled Black holes in many dimensions at the LHC: testing critical string theory. This paper is perfectly reasonable, discussing a proposal for getting information about the number of extra dimensions, assuming Tev-scale gravity (a huge assumption most people think unlikely) and thus production of black holes at the LHC. If the number of extra dimensions is bigger than 6, then 10d superstring theory is ruled out (one can make similar comments about 11d M-theory, whatever that is).

Like all news articles of this kind, this one is misleading in the extreme, since "SLAC Physicists Develop Test For String Theory" is likely to make the unwary think that string theory is now testable. In addition, it's flat out wrong, since the writer made the critical decision to replace "critical string theory" by "string theory". Granting the unlikely assumption that the LHC sees extra dimensions and measures their number. if this turns out to be more than 6 or 7, string theorists will likely just point out that it is only "critical" string theory which lives in 10 dimensions. In recent years there has been much talk about string theories outside the critical dimension. For some discussion of this, see the comment thread of a recent Cosmic Variance posting, where string theorists Eva Silverstein and Clifford Johnson maintain that they see reasons to believe in the existence of string theories in dimensions other than 10. For some flavor of the discussion, here's what Clifford has to say:

…the "person on the street" all too often hears (or implicitly gathers from posts like this) the phrase "string theory requires D=10/11", and it is simply not true and in some years we may well have to be spending a lot of time undoing yet another uncautious claim when/if after doing phenomenology better we find that we don't need to start in higher D and then "compactify". We'll have to go around telling everyone (on the tv shows and radio shows and magazines) "oh…that thing we said about extra dimensions? We were just kidding"…. Just like we're doing now with the whole "unique vacuum" and "theory of everything" phrases…

Clifford seems fond of the idea of sub-critical strings, perhaps even strings in four dimensions (another enthusiast of this idea is Warren Siegel), while Stanford string theorist Silverstein advocates the study of super-critical strings, exactly the ones that would get around the "test" promoted today by her colleagues at SLAC.

Update: SLAC has replaced this article on their website with a new, much more accurate version, entitled "SLAC Physicists Develop Framework-Dependant Test For Critical String Theory". The original version got wide distribution, even appearing on Slashdot.

Comments

1. **D R Lunsford**
   February 8, 2006

   Heh, thanks to you, Peter, they can't get away with this kind of thing now!

   -drl

2. **Lubos Motl**
   February 8, 2006

   The experiments are exactly designed to distinguish between the different scenarios how many dimensions there are and how large they can be, and so on. We won't get the full information from the very beginning, but we will of course learn something.

   As a conservative who believes a big desert after SUSY followed by a GUT scale near the Planck scale, I of course guess that no one will see mini black holes etc., but many of these scenarios are perfectly plausible and we will see how the experiments decide.

   Of course that only anti-scientific activists think that string theory is permanently untestable. String theory is, using an extremely modest language, the framework that parameterizes the space of reasonable possibilities for new physics potentially connected with quantum gravity, and although we can't say with certainty which of these scenarios are correct, we can definitely eliminate many other scenarios that would be possible if we did not know the things arising from string theory.

   And on the contrary, we know scenarios that we would never invent if string theory did not guide us.

   Well, I completely understand that you are scared of the experiments more than the radical Muslims are scared of pictures of Mohammed – because you have spent years if not decades by spreading crap about the end of physics. Well, maybe you will survive, Peter.

3. **blank**
   February 8, 2006

   Peter, why must you be so fact-obsessed?

   Good thing there was a good apparatchuk on hand to put things in proper context.

4. **The Anti-Lubos**
   February 8, 2006

   We won't get the full information from the very beginning, but we will of course learn something.

   This was Peter's entire point. The headline makes it sound like we will get the full information from the very beginning. Surely, a smart boy like you can understand this.

   many of these scenarios are perfectly plausible and we will see how the experiments decide.

Again, no one said that they're not; the contention was merely that this does not constitute "a test for string theory."

Well, I completely understand that you are scared of the experiments more than the radical Muslims are scared of pictures of Mohammed – because you have spent years if not decades by spreading crap about the end of physics. Well, maybe you will survive, Peter.

Mmmm, delicious, nutritious *ad hominem* argumentation! Allow me to participate: Lubos is a jerk.

5. **woit**
February 8, 2006

"radical Muslims are scared of pictures of Mohammed"

"Maybe I will survive"?

Is that some kind of bizarre threat? Lubos, you really are getting mad as a hatter and need to seek professional help.

6. **JoAnne**
February 8, 2006

Anybody who has ever spoken to reporters understands that what generally comes out in print basically does not resemble, in any way, your conversation with the reporter. Tom Rizzo and I spent about an hour with the reporter, explaining all the and's, if's, and but's of our analysis. None of which were included in the first draft of the story. We tried hard to clarify the description of our work in the story, and ended up with the simple asterisk "under certain conditions." And, to be fair, we were told that this story was intended for the general audience at SLAC, including admins, technicians, cafeteria workers, etc, and thus all of the details simply could not be explained. As for the headline that is blazened on the SLAC home page – I saw it for the first time when someone drew my attention to it. I knew it was going to cause headaches….

7. **Chris Oakley**
February 8, 2006

Lubos,

I am happy to listen to reasoned scientific argument from you. Even your polemics are sometimes amusing. But threatening me with the US Marine Corps or Peter with some unspecified hit man is just pathetic.

I cannot believe that you can be a researcher of any great calibre. Anyone with talent will be used to receiving and, for that matter, dishing out strong criticisms of ideas without taking it personally. This is all part of the process of doing research. If you cannot handle criticism you should not be doing research.

8. **woit**
February 8, 2006

Joanne,

I figured that's what had happened. Unfortunately there's an irresistible urge present among journalists who don't know much about this field to turn everything into this "scientists figure out how to test string theory" story. If only they all read my blog….

9. **Chris W.**
February 8, 2006

I think one can safely regard LM's behavior as an example of an occasional and fading Central European penchant for abusive pedagogy: "I'll beat the truth [as defined by me] into you even if it kills us both, you worthless insect!"

(Peter: I don't think this is any worse than "mad as a hatter".)

10. **secret milkshake**
February 8, 2006

to the question of a "fading Central-European pedagogy penchant":

You won't find too many people like this in Prague; we export them

11. **Boaz**
February 8, 2006

Not to say that Lubos statement wasn't abusive, but I wouldn't say it was a threat. He was suggesting that Peter is so afraid of experimental results that may support string theory, that they may kill him, but he might survive. As usual he takes a very extreme interpretation. PW says that headlines about interpretations of experiments are overhyped, LM responds by saying that PW must be deathly afraid of said experiments. One does wonder to what extent it is really posturing and trying to win a propaganda battle, and to what extent he really holds these views. I suppose Chris W.'s interpretation as an abusive form of pedagogy may be right.

12. **woit**
February 8, 2006

Boaz,

Thanks for the interpretation of the Lubos rant. I was honestly having trouble figuring out what is was that might kill me, and what the radical Muslims had to do with it (not that I spent much time trying to puzzle this out, I admit). Now I see, it's those experimental results on black holes at the LHC that Lubos doesn't believe we'll get. Makes perfect sense now.

13. **Quantoken**
February 9, 2006

Peter:

Oh you are so nice! But please do **NOT** believe a word Joanne said above. Come on, Joanne, don't insult people's intelligence by blamning it all on journalists. If it's just one occasional super string theory hype news story, you can have an excuse on journalists not doing their homework. But when there are "10^500" of such types news stories (in Peter's word) out there, **all those are jouranlists fault and nothing of you string theorists doing?** Come on! What about Kakuism we see daily? What about

your own words quoted in quotation marks in the news story? Are they not misleading the people? Are they not your own words?

Journalists LOVE controversial stories. Had you bee a little bit more honest, you could have meantioned to the Journalists that there is a different opinion about string theory, for example from a guy called Peter Woit. The journalist would then love to track Peter down and write up a much more balanced news story.

And NO, you do NOT need to go to any technical details in order to decribe to the public the honest truth of the current situation in the field of super string theory. It can be said in plain English: it's a failure so far and there is little hope any meaningful progress can be made in the next few decades. It's a matter of honesty whether any string theorist is willing to tell that evident truth in plain English.

All the string theory hype stories are the makes of the string theorists themselves. Journalists simply report what you guys told them and you can't blamn journalist for telling an untrue story if you don't tell them the true story in the first place!

Quantoken

14. **Juan R.**
February 9, 2006

I find really interesting that now string theorists as Eva Silverstein and Clifford Johnson maintain in public they see reasons to believe in the existence of string theories in dimensions other than 10 specially the case 4D.

In my Oct 21 post "String theory is not a TOE" in moderated newsgroup sci.physics.strings i did a joke about the infinite malleaibilty of string theory and how even with a "blanck check" -i.e. you can fix anything in the "theory" (even there are versions of string theory without strings!)- they were not advancing science. In page 2 of my April non-technical work, i cited the history of dimensions on string "theory":

4D,… 26D, 10D, 11D, 12D?,…4D again,…

It looks like researching in circles 🙂

—

Juan R.

Center for CANONICAL |SCIENCE)

15. **A.J.**
February 9, 2006

Quantoken–

Joanne is an experimental particle physicist. Get a clue.

16. **woit**
February 9, 2006

A.J.,

No, Joanne isn't an experimentalist, she a theorist, but of a phenomenological sort, definitely not a string theorist.

Over the last twenty years it has been interesting to watch the relationship between phenomenologists and string theorists, the two dominant groups within the particle theory community. It began with a certain amount of active hostility (think Glashow, who has stuck to his guns), but with the advent of extra dimensional models, there was a definite rapprochement and cessation of hostilities. The phenomenologists could study models with extra dimensions visible at the TeV scale, and thus at least in principle their work might have something to say about string theory (e.g. Joanne's work), and the string theorists could announce that string theory had potentially testable experimental applications.

My personal opinion is that, in this relationship, the phenomenologists have quite often found themselves used as tools in string theory hype. To some extent this story is just one more of a long line of similar examples.

17. **JoAnne**
February 9, 2006

First, as Peter says above I am indeed a phenomenologist – the type of theorist who connects formal theory to experiment. I tend to get irritated when people call me a string theorist. Particle theorists are not *all* string theorists.

Second, this news story is now all over the web. The research in this paper is honest and credible and now it is being blown out of proportion and misrepresented all over the web. Nothing could make me more upset.

I am not having a good day.

18. **A.J.**
February 9, 2006

Whoops. Looks like I need a clue, too. I'm sorry for misidentifying your profession, JoAnne.

Of course, my mistake does not excuse Quantoken's misdirected personal attack. It would be unacceptable in person, and it's unacceptable on the internet.

19. **Tony Smith**
February 9, 2006

As JoAnne says, "this news story is now all over the web", including slashdot, where you can see:

a link to Peter's blog (this entry);

that JoAnne was at Iowa State in the 1980s;

that Ben Lillie says that he has "… replaced the arxive version with the published version: http://arxiv.org/abs/hep-ph/0503178 …" so that anyone can read the serious physics without the PR hype; and

that Ben Lillie also says "… The main point is that there are many "ifs", "ands", and "buts" in the paper that did not make it into the news release. … It certainly can not rule out string theory. We think it's an important and interesting piece of work, but it isn't a definitive "test" of string theory, as the headline suggests. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – JoAnne also said "… I am not having a good day. …".
The situation reminds me of Dilbert cartoons, with JoAnne/Ben/Tom as Dilbert the Engineer and the SLAC PR creatures as Marketing-Sales-Weasels.
Don't let Marketing-Sales-Weasel idiocy get you down. Most people in the real world understand that Dilbert is not the bad guy.

A Survey of Elliptic Cohomology

February 8, 2006
Categories: Uncategorized

There's a beautiful survey paper about elliptic cohomology that Jacob Lurie, an AIM 5-year fellow in the math department at Harvard, has recently put on his home page. This paper has been discussed a bit already by David Corfield and by Urs Schrieber.

I don't have time right now to try and write up something comprehensible about those parts of the elliptic cohomology story that I kind of understand, and in any case I want to spend more time reading Lurie's paper. It brings into the elliptic cohomology story several of my favorite pieces of mathematics (Atiyah-Segal completion, Freed-Hopkins-Teleman), in a way that I don't yet understand. But in any case there's a lot of very beautiful and very new mathematics in this paper, mathematics that has tantalizing relations to quantum field theory.

Comments

1. **Kea**
   February 8, 2006

   "…mathematics that has tantalizing relations to quantum field theory."

   Curious comment: do you refer to section 5.2 or something else? Amazing set of ideas this guy has, I'll agree. I've had the Adams book, for example, on my shelf for years and years, but I don't imagine I shall ever be able to refer to it casually.

2. **Kea**
   February 8, 2006

   The only issue I have after browsing Lurie's papers is that he seems to have solely a homotopy theorist's take on higher categories, rather than a more open point of view which one would expect from someone discussing issues with such a broad scope. Admittedly, I am biased.

3. **woit**
   February 8, 2006

   Kea,

   You're right that this is a homotopy theorist's take on the subject, heavily influenced by the work of Mike Hopkins. But, for a homotopy theorist, Lurie is taking a quite broad point of view.

   The relations to QFT include not just the Freed-Hopkins-Teleman stuff that I'm fascinated by, but other things as well. The whole subject was heavily influenced by Witten's writing down of a 2d QFT that gives the elliptic genus, and more recently Stolz and Teichner and others have been investigating the relation of 2d conformal field theory to elliptic cohomology. There's more about this if you follow the link to Urs's posting.

4. **Michael**
   February 9, 2006

   seems like there is an href missing from the HTML on Urs link. thanks.

5. **A.J.**
   February 9, 2006

   Hi Peter,

   You mentioned a while back (in a post about Freed-Hopkins-Teleman that you hadn't quite been sold on stacks. I think that the Atiyah-Segal theorem might be one good reason to like them.

   In algebraic geometry, the classifying stack of G is the quotient stack pt/G. The nicest way to define this is to start with the action groupoid of G on a point; this is just the category with one object and one morphism for every element of G. If you take its nerve, you obtain the simplicial scheme with G^n in the n-th degree. Taking the geometric realization of this simplicial object gives you Milnor's construction of BG. But this isn't really the best thing to do; you're throwing away information. What you should do instead is think of the functor of points of this simplicial scheme as a Category Fibered in Groupoids. This CFiG isn't a stack however; it's just a pre-stack, doesn't satisfy effective descent. It assigns to a test scheme S, the groupoid Hom(S,pt), with morphisms given by the obvious G-action. To get a nice CFiG, we need to stackify, to study instead objects which are locally maps to the point, but might differ globally by the action of G. This "differing globally by the action of G" really means that we have a G-valued function on every non-empty intersection of local patches. This is exactly the data of a principal G-bundle.

   So what's my point? Well, it's that stacks are good magic. The natural definition of the quotient stack pt/G tells you precisely that maps into pt/G classify principal G-bundles. This magic happens because you made certain to keep track of *how* G acts on th e point. Which results in a nice thing: K(pt/G) is the Grothendieck group of the vector bundles on pt/G. But these are the same thing as G-equivariant vector bundles on the pt, i.e. G-modules. So the K-theory of the stacky pt/G is, in fact, canonically isomorphic to Rep(G). If I understand things right, the Atiyah-Segal theorem is basically an identification of the information you lose by passing to the geometric realization.

6. **Urs Schreiber**
   February 9, 2006

   The entry on elliptic cohomology that Peter is referring to is this one:

   http://golem.ph.utexas.edu/string/archives/000737.html

   Stay tuned for more on Stolz&Teichner in a couple of weeks when I report from this winter school:

   http://golem.ph.utexas.edu/string/archives/000712.html

7. **Urs Schreiber**

February 9, 2006

Hi AJ,

I understand what you are saying, except for this last sentence

> So the K-theory of the stacky pt/G is, in fact, canonically isomorphic to Rep(G).

I guess you mean that this K-theory is isomorphic to the Grothendieck group completion of the decategorification of Rep(G), no?

BTW, if anyone is wondering what AJ is talking about: a chatty discussion of those quotient stacks is given here.

A review of how to think of these quotients as groupoids is given here and here.

8. **A.J.**
February 9, 2006

Hi Urs,

Yes, by Rep(G) I mean the representation ring, not the whole category. (I was using the same notation that Lurie used in his survey.)

Nice expositions, by the way. Now if I could just find a chatty discussion of Quillen's model categories…

9. **Urs Schreiber**
February 9, 2006

> Now if I could just find a chatty discussion of Quillen's model categories…

If you find out anything at all, let me know. Thanks.

10. **D R Lunsford**
February 9, 2006

I wish I could get excited about this. Someone should explain this stuff in a way that is palatable to intuitive thinkers. This sort of exposition is almost unreadable to me.

-drl

11. **Kea**
February 9, 2006

drl

Unfortunately, whereas us poor physicists are just trying to understand, say, M-theory, this Lurie guy is on a fast rocket to trying to prove the Riemann Hypothesis (why else would he be working on higher toposes?) … it ain't going to be easy for us sods to keep up.

12. **kielbasa**
February 9, 2006

"I wish I could get excited about this. Someone should explain this stuff in a way that is palatable to intuitive thinkers."

As opposed to "non-intuitive thinkers."

13. **woit**
February 9, 2006

A.J.,

Thanks a lot for your comment. That's more or less exactly why I've gotten interested in stacks again recently (that K(pt/G)=R(G), rather than the completion). Chris Woodward had given me a more concrete explanation of this (that on the geometric realization you get K-theory classes that don't quite come from representations, and thus need the completion), but your explanation is very helpful. Thanks again!!

14. **D R Lunsford**
February 9, 2006

kielbasa,

Yes non-intuitive thinkers. It should be obvious who they are 🙂

-drl

Lots More Landscape

February 8, 2006
Categories: Uncategorized

Various things from the past couple days related to Susskind, the Landscape, and his book The Cosmic Landscape:

The paper Computational Complexity of the Landscape I by Frederik Denef and Michael Douglas is out. They show that even in simplified models of string theory vacua the problem of finding a model with CC in the anthropically determined range is NP-hard. This strongly indicates that in practice you can't ever do what landscapeologists have optimistically hoped might be possible: pick out those vacua with anthropically acceptable values of the CC, and somehow use them to make predictions. Denef and Douglas end by bringing up a peculiar possibility: what if direct evidence for string theory is found (they're kind of vague on how this going to happen…), but the problem of actually identifying our vacuum state remains intractable?

This raises the possibility that we might someday convince ourselves that string theory contains candidate vacua which could describe our universe, but that we will never be able to explicitly characterize them.

Lubos has a posting about this, including an exchange of comments with Frederik. On the whole I tend to see eye to eye with Lubos about the Landscape, although not here, where he's frantically trying to dismiss the Denef-Douglas results, which look pretty solid to me.

The Philadelphia Enquirer has an editorial entitled A scientific leap, but without the faith, by Amanda Gefter, who did the recent interview with Susskind in New Scientist. Gefter tries to argue that string theory, unlike intelligent design, is science despite not being falsifiable. I don't have the time or energy here to do justice to her argument or the problems I have with it (for one thing, she thinks string theory is breathtakingly beautiful). Science and Theology News has an article about the Gefter editorial called Intelligent design versus string theory which kind of misses the point, claiming that string theory can be falsified.

This month's American Scientist has a review of Susskind's book by cosmologist John Peacock entitled A Universe Tuned for Life . The review is pretty much uncritical, and mainly happy that Susskind is anti-religion:

These obligatory small criticisms should in no way detract from Susskind's tremendous achievement. This book is a fine piece of popular science writing, but it is particularly significant for the timeliness of its message. Susskind emphasizes that the whole structure of the universe requires an active Creator no more than does the human eye or the temperature of the Earth. At a time when more and more people seem happy with a creation that took place 6,009 years ago, this lesson needs repeating.

Peacock actually feels that Susskind doesn't go far enough in trashing the 20th century idea that there is some simple, compelling physical theory that explains the way the world works:

But if life on Earth is a random accident in a universe where only chance yielded laws of physics suitable for life, why stop there? Perhaps string theory itself is nothing special and only part of a wider spectrum of possible prescriptions for reality. If the search for a unique and inevitable explanation of Nature has proved illusory at every step, is it really plausible that suddenly string theory can make everything right at the last? Reading Susskind's book should make you doubt that possibility, in which case we may have reached the end of the search for underlying simplicity that has driven physics since the beginning.

Finally, last night's new papers on the arXiv included the surprising inclusion of one on Emergent Gravity by Jack Sarfatti. Famously, supposedly Sarfatti had been banned from publishing on the arXiv, but now I guess the arXiv has changed it standards. I still haven't heard from them about why they're banning trackbacks to this site, but perhaps there's an explanation as to why they have finally found a paper by Sarfatti acceptable. It includes exactly two references, one to a 1976 paper about defects in condensed matter physics, the second to, you guessed it, Susskind's The Cosmic Landscape.

Update: Chris W. correctly points out that I mistakenly had Gefter referring to string theory as "strikingly beautiful", when it was general relativity she was referring to in this context.

Update: As a commenter pointed out, there's a long, uninformed discussion of string theory and intelligent design over at The Panda's Thumb. Many of the people commenting over there seem to believe that string theory is testable, and even invoke the latest SLAC story. Please don't bring that particular discussion over here now. If you're in the mood, contribute over there. Unfortunately I don't have the time or energy at the moment…

Comments

1. **Chris W.**
   February 9, 2006

   Gefter was referring to general relativity when she used the phrase "breathtakingly beautiful". Regarding string theory, she says:

   …, string theory is lacking in testable predictions, but more important, it is lacking some underlying principle to give it deep explanatory power. Still, scientists pursue it because they see paths of unification, shards of beauty, glimmers of ultimate reality.

2. **Michael**
   February 9, 2006

   http://en.wikipedia.org/wiki/Jack_Sarfatti

   interesting bit on wikipedia about Dr. Sarfatti. he seems like a cool guy.

3. **Christine**
   February 9, 2006

   There is also this paper by Craig J. Hogan I have commented here recently.

4. **Alejandro**
   February 9, 2006

   There is a nice discussion on the "ID vs String Theory" debate going on at The Panda's Thumb.

5. **JP**
   February 9, 2006

When was Sarfatti banned from ArXiv.org? What was the reason?

6. **Alejandro RIvero**
February 9, 2006

The "convince ourselves" parragraph is strinkingl similar to arguments about continuum hipothesis.

7. **woit**
February 9, 2006

I don't know the whole history of Sarfatti's interactions with the arXiv, and I'd rather not have a thread here about the conflicts he and others have had with them in the past. The interesting question of the moment is why they seem to have changed their policy, exactly when he starts writing about the landscape and referring to Susskind.

Privately I have heard through another party that Sarfatti sent out many requests for endorsements, and that he himself doesn't know how many of them led to endorsements, or why the arXiv has changed its policy towards him. I've certainly noticed in recent months some rather, let us to be polite say "non-mainstream" articles being posted, even in hep-th, of a sort that I believe in the past were not allowed. What they had in common was that they somehow claimed to be about string theory. This has raised the question in my mind of whether the landscape pseudo-science gaining ground in the mainstream particle theory community is starting to infect the arXiv in general, leading to a breakdown in long-held understandings of what is legitimate theoretical physics worthy of inclusion in the arXiv, and what isn't.

If anyone actually knows what is going on at the arXiv on this issue, I'd be interested in hearing about it.

8. **Amanda**
February 9, 2006

I've certainly noticed in recent months some rather, let us to be polite say "non-mainstream" articles being posted, even in hep-th, of a sort that I believe in the past were not allowed.

Really? Can you give some examples? Of topics, if you prefer not to be too specific.

9. **woit**
February 9, 2006

I didn't keep specific track of these as I saw them, but here's one example:

hep-th/0601104

This seems to me very much the sort of thing that traditionally would not be allowed on hep-th, with the fact that "string theory" is mentioned the only reason it got on there.

10. **Hal**
February 9, 2006

I know that anthropic and multiverse theories are not well thought of around here, for understandable reasons. Nevertheless there are philosophical arguments that can provide guidance through even the thicket of an infinite multiverse. The basic principle is this: that the universe should be as complex as necessary to create intelligent life, but no more so. Justifying and defending this principle obviously takes more room than is available here in the margin 🙂 so I will just point to the works of Max Tegmark and Jurgen Schmidhuber.

11. **John Gonsowski**
February 9, 2006

One can have a David Deutsch-like multiverse without a Susskind-like anthropic landscape and that's a good thing.

12. **Luboš Motl**
February 10, 2006

I am happy that you consider the newest paper about the anthropic principle and the meta-statistical analysis of the vacua solid, Peter. Why is it good news that Peter Woit goes anthropic? It's because we have finally some kind of diversity in the blogosphere, and I don't have to worry that Michael's and Frederik's attitudes to the vacuum selection issues are underrepresented. 😉

13. **Alejandro Rivero**
February 10, 2006

Well, my own papers had got a good historical record of "unappropiateness" in the ArXiV, and recently I have found they are passing more smoothly. So either my writting is going better, or the arxiv has revamped some criteria after the appointment of new supervisors last year.

14. **Chris W.**
February 10, 2006

From Peter:

> I've certainly noticed in recent months some rather, let us to be polite say "non-mainstream" articles being posted, even in hep-th, of a sort that I believe in the past were not allowed. What they had in common was that they somehow claimed to be about string theory. This has raised the question in my mind of whether the landscape pseudo-science gaining ground in the mainstream particle theory community is starting to infect the arXiv in general, leading to a breakdown in long-held understandings of what is legitimate theoretical physics worthy of inclusion in the arXiv, and what isn't.

How about these: Papers by the author of **A Solution to the Fermi Paradox: The Solar System, part of a Galactic Hypercivilization?**:

> Besides these simple solutions there are many more exotic proposals. For example, a rather drastic expansionist solution is given by the theoretical physicist Cumrun Vafa, at Harvard University, who thinks that the fact that we do not see aliens around could be the first proof of the existence of brane worlds: all advanced aliens would have emigrated to better parallel universes [7].

15. **woit**

February 10, 2006

Chris W.

That is one of the best arguments for brane worlds I know of. Note that this author's papers in recent years have been on in the unmoderated "physics" sections rather than one of the moderated sections like hep-th. What I'm interested in is whether their moderation policy has changed or is changing.

16. **woit**
February 10, 2006

Correction: the "physics" section is also moderated, although presumably in a somewhat different way than the other sections.

17. **Alejandro RIvero**
February 12, 2006

Well I am happy that at least it has been changing or changed during the last year, because it let me to arxive some things I can need to check in the future -lot of susy and/or topcondensation going on-, and it is important to have them handy and public even if not in "publishable" form. I hope it will remain at the current moderation level and no go back to the old ages.

PS: Alejandro, could you please to use at least the initial of your surname, to avoid confusion?
PS2: It seems that a Sarfatti has answered to this thread in a different one.

18. **Christine**
February 21, 2006

P.C.W. Davies submitted this paper recently to astro-ph:

The problem of what exists. The abstract is:

> Popular multiverse models such as the one based on the string theory landscape require an underlying set of unexplained laws containing many specific features and highly restrictive prerequisites. I explore the consequences of relaxing some of these prerequisites with a view to discovering whether any of them might be justified anthropically. Examples considered include integer space dimensionality, the immutable, Platonic nature of the laws of physics and the no-go theorem for strong emergence. The problem of why some physical laws exist, but others which are seemingly possible do not, takes on a new complexion following this analysis, although it remains an unsolved problem in the absence of an additional criterion.

19. **woit**
February 21, 2006

Christine,

Thanks a lot for pointing out the Davies multiverse paper, I hadn't noticed it.!

Swampiness on Long Island

February 9, 2006
Categories: Swampland

At first I thought this must be an April Fool's day joke, but I just checked the calendar, and it's not April 1 yet. For the last few years Stony Brook has been running a workshop on math and physics during the summer, funded by Jim Simons of Renaissance Technologies. The topic for this summer has just been announced, it's "The String Landscape and the Swampland". A poster for the workshop is now on-line, featuring a large picture of a swamp. You really couldn't make this kind of thing up.

Comments

1. **Sakura-chan**
   February 9, 2006

   Susskind can be the Swamp Thing.

2. **Michael**
   February 9, 2006

   Peter, the swampland program aims at boiling down the landscape as much as possible — hopefully killing it eventually. It is a residual part of physics and reason in the whole landscape business and has recently lead to interesting progress (the "gravity is weak paper" by Vafa, Motl et. al.). Why would you mock such a thing? Oh, right! I forgot about your ignorance and agenda — for just one second…

3. **D R Lunsford**
   February 10, 2006

   SWAMP THING
   YOU MAKE MY HEART SING
   YOU MAKE EVERYTHING – GROOVY!
   SWAMP THING!

   -drl

4. **sunderpeeche**
   February 10, 2006

   From the poster (2nd para)

   "These workshops focus on the intersection between physics and mathematics, particularly in the context of string theory. One of the themes of the fourth workshop will be the String Landscape and the Swampland — what are the general constraints on low energy physics that follow from string theory?"

   So it's a picture of a swamp. Why not? At least there is an officially stated attempt to obtain general constraints on low energy physics. The workshop may not succeed (no falsifiable predictions after 20 years after all….) but it's a reasonable poster and a reasonable agenda.

5. **Chris Oakley**
   February 10, 2006

   Michael,

   I cannot speak for Peter, but I can certainly tell you why I think that a conference on the Stringy Swampland chaired by Cumrun "the aliens have left us for a more happening universe" Vafa is a worthy subject of derision.

   This is supposed to be PHYSICS. Remember physics? Experiments that did not work at school … lots of mathematics. Names like Maxwell, Rayleigh, Rutherford and Thomson (after which the forms at my school were named – I was in Rayleigh, but I digress). It is an EXPERIMENTAL science. In the case of the Stringy Swampland possible experimental tests are so far away that one can hardly imagine them. It is a more a case of "I had a dream. It never became a reality, but I refuse to give up on it."

   I did not go into particle physics for the chicks and the fast cars. Sorry, I should rephrase that: IF there had been chicks and fast cars on offer in going into particle physics, THEN it would not have been the reason for my choosing it. I went into it because I wanted answers.

   As an undergraduate, once I got over my initial difficulties with quantum mechanics, I came to appreciate what an outstanding framework it is. Atomic physics can just as easily be called "the application of quantum mechanics to single atoms" and solid state physics can just be called "the application of quantum mechanics to regular arrays of atoms". Nuclear physics suffers from the problem that we do not know the equations of motion, but to the extent that we can apply quantum mechanics to it, we do.

   At the end of my undergraduate course a number of important questions were left unanswered. I was expecting graduate courses on quantum field theory to answer them. I also expected the QFT I learned to have ordinary quantum mechanics contained within it. It does not. QFT as currently practised only applies to scattering processes and so does not have *anything* to say about bound states. Those who laud the Lamb Shift calculation as one of the triumphs of modern science should remember this fact. You can get the phenomenally accurate theoretical value only if you step around a few truly nasty inconsistencies and use a different framework, supposedly more elementary, to provide the basis in which you operate.
   Of course, I did not like any of this, so as soon as I was reasonably able to do so, I set about trying to find something better.

   As this work progressed, I realised that nobody much wanted me to succeed. Whereas I could understand this coming from those whose claim to have already solved the problems I disputed, I could not understand it coming from the particle physics mainstream.

   My approach to problem solving is really quite simple: identify the biggest problem and just go and solve it in the most direct way possible. Listen to others, but ultimately trust only your own judgement.

   This philosophy, I felt, worked in re-doing the bathroom in my mother's house in 1992. It also worked in building systems for derivatives trading at UBS and Nomura, but for theoretical physics, it was a disaster. Even now, if I were to ask to give a talk on my work on theoretical physics, I would just be fobbed off with excuses.

   Yet my philosophy is no different from that of the huge numbers of scientists and technicians trying to do their best at whatever they do throughout the world. These are the people who were *not* child prodigies, and about whom articles do *not* regularly appear in the New York Times. But it is through the efforts of these unsung heroes that the boundaries of knowledge are slowly pushed back, or the quality of life slowly improves. They know that most people will not be interested in their views on extra dimensions or aliens, but, through trial and a lot of error, they become good at one specific thing, something that would be totally boring to anyone other than themselves.

String theorists will probably regard these people as inferior, but as far as I am concerned, it is the String theorists who need to learn.

As far as I can see, theoretical particle physicists behave more like excitable teenagers than scientists. “Right” and “wrong” is irrelevant – “cool” and “uncool” is far more important. In 1982 it was “cool” to do Supersymmetry. So everyone had to do Supersymmetry. Everyone was watching each other, and regulating what they say, either in talks or papers, not on the basis of whether it was true of not, but on the basis of whether it corresponded to the consensual view. I refused to play this game after the first few months. If something was true, I said it was true. If it was false, I said it was false. Result? Soon nobody wanted to hear what I had to say at all. And then, of course, there is the guru factor. Not such a big deal in 1982, but now … if Ed Witten said that the universe was sneezed out of a being called the Great White Arkleseizure it would only be a matter of days before papers started to appear on the flow equations of the sneezing and the reasons why the Arkleseizure had to be white (the latter probably coming from some string theorist in the deep south).

The reason why fundamental physics is where it is today, is simply because *that is where it wants to be*. There is no contact with experiment because they are not interested in experiments. They follow where the group mentality leads them, and if that is into a swamp, then so be it.

“Gadarene swine” was the phrase I heard to describe this phenomenon. Not my words, but those of a highly-respected theoretical particle physicist, but now running the UK’s fusion project.

6. **Chris Oakley**
   February 10, 2006

   Anticipating objections from Lattice gauge theorists: I know that Lattice gauge theories are in reasonable shape for hadronic bound states, but this obviously does not include the Hydrogen atom.

7. **Dombono**
   February 10, 2006

   I don’t know where or how you learned quantum field theory but I guess you took a really bad course on it taught by a perturbative S-matrix guy :). Of course quantum field theory covers bound states and of course computations involving hydrogen atoms are much much simpler and easier and more accurate than computations involving hadrons, both because QED is weakly coupled and also because the hydrogen atom only involves two valence particles. People don’t work on lattice QED (as much) because they don’t need to but there’s nothing stopping anyone from working on it.

   If Witten were to write a paper on the Great White Arkleseizure, he will immediately lose all credibility within a day and will be dismissed as a formerly great physicist-turned-crackpot. Physicists take him seriously because his work has been consistently profound and CORRECT.

8. **woit**
   February 10, 2006

   I realize that this posting was kind of a rant of my own, so encourages ranting, but I’d rather keep that kind of thing to a minimum. Please, stop with the rants, or with responses to them. Except for Lubos of course, whose ranting is kind of entertaining….

   I’ve written about this swampland stuff elsewhere several times, but I guess I should again explain exactly what the problems with it are, since some people seem to not know what it’s about, thinking it is about “boiling down the landscape” or trying to understand whether the standard model can be the low energy limit of a string theory, something obviously of interest.

   Vafa et. al.’s swampland program isn’t about getting rid of the landscape at all, it accepts that all the possible string vacua coming from KKLT and other similar constructions all exist. The “swampland” consists of effective field theories that no one has yet come up with a proposed way of getting as low energy limits of string theory. It explicitly does not include the standard model, for which there are many proposed constructions as string vacua (whether any of them actually do give exactly the standard model, and if so, whether there are so many that the theory is unpredictive, is an open question). This whole subject explicitly is not about understanding the real world, it is about understanding aspects of low energy limits of string theories that have nothing to do with the real world.

   The most recent “swampland” work that has gotten a lot of attention argues that in effective field theories coming from string theory gauge forces cannot be much weaker than gravitational forces. Whether or not you believe this, it’s completely irrelevant to the real world, where gravitational forces are much, much weaker than gauge forces.

   Besides the fact that this subject explicitly has nothing to do with the real world, there’s another problem. Historically there have been lots of claims that “you can’t get X from string theory”, just about all of which have turned out to be true once someone seriously tried to get X from string theory. For one example of this phenomenon, take a look at

   http://golem.ph.utexas.edu/~distler/blog/archives/000651.html

   which is a posting by Jacques Distler about Vafa’s original swampland note. In the posting Jacques gives as an example the supposed fact that you can’t get an effective field theory with only one or two generations of fermions out of string theory. More or less immediately, Volker Braun wrote in to tell him how to do it.

   Besides the above scientific problems, there are other reasons I find the whole thing completely bizarre. One uses the metaphor of a swamp to indicate somewhere extremely unpleasant that you are in danger of getting lost in, stuck in the mud, and unable to ever get out of (e.g. “Iraq is a swamp for the US military, just like Vietnam”). Why would anyone want to describe their proposed research program as going into a swamp, and make up posters with swamp pictures? This really amazes me no end…..

9. **sunderpeeche**
   February 10, 2006

   Swamps are often associated with disease (tropical swamps and malaria). But swamps are also rich ecological systems. They teem with life. Many tropical swamps have large deposits of crude oil under them. Things like peat bogs (not a swamp?) eventually become coal deposits, after millions of years. These things have to start off somehow, and perhaps don’t look pretty at the beginning (if ever). I guess the word swamp doesn’t bother me. But it’s just a personal opinion.

10. **J.F. Moore**
    February 10, 2006

    Perhaps there is an element of psychology at play: taking ownership of a perceived weakness to preempt its use in a derisive way. A common example of this is the outrageous dress used in gay rights marches, or people who poke fun at themselves to offset a deeper narcissism. Just a guess.

    Anyway, it’s not even a very nice graphic that they use.

11. **Who**
    February 10, 2006

    There may be an unconscious or unintended reference to [i] ignis fatuus[/i] (fools' fire associated with marshes)

    another name for this is Will-'o-the-Wisp
    http://www.worldwidewords.org/weirdwords/ww-wil1.htm

    "Both will-o'-the-wisp and ignis fatuus are used figuratively for some false idea or influence that leads people astray."

    Folks lost in swamp may in some cases have pursued will-'o-the-wisp.

12. **D R Lunsford**
    February 10, 2006

    Moore – that was profound. You're onto something there.

    -drl

13. **J.F. Moore**
    February 10, 2006

    Thanks, I'm glad you think so. That is probably the closest I will come to saying something profound here.

Hawking Goes Anthropic

February 10, 2006
Categories: Uncategorized

Stephen Hawking and Thomas Hertog have a new paper out, called [Populating the Landscape: A Top Down Approach](). It contains his version of the anthropic landscape idea, based on his "no-boundary" idea of quantum cosmology (sometimes also referred to as the "Hartle-Hawking wavefunction"), and he refers to it as "top-down cosmology".

Here's part of the summary:

In a top down approach one computes amplitudes for alternative histories of the universe with final boundary conditions only. The boundary conditions act as late time constraints on the alternatives and select the subclass of histories that contribute to the amplitude of interest. This enables one to test the proposal, by searching among the conditional probabilities for predictions of future observations with probabilities near one. In top down cosmology the histories of the universe thus depend on the precise question asked, i.e. on the set of constraints that one imposes…

The top down approach we have described leads to a profoundly different view of cosmology, and the relation between cause and effect. Top down cosmology is a framework in which one essentially traces the histories backwards, from a spacelike surface at the present time. The no boundary histories of the universe thus depend on what is being observed, contrary to the usual idea that the universe has a unique, observer independent history. In some sense no boundary initial conditions represent a sum over all possible initial states. This is in sharp contrast with the bottom-up approach, where one assumes there is a single history with a well defined starting point and evolution.

and

We have also discussed the anthropic principle. This can be implemented in top down cosmology, through the specification of final boundary conditions that select histories where life emerges. Anthropic reasoning within the top down approach is reasonably well-defined, and useful to the extent that it provides a qualitative understanding for the origin of certain late time conditions that one finds are needed in top down cosmology.

I haven't completely understood this yet, especially the author's claims that they can use these ideas to say something about the shape of primordial fluctuation spectra, but the whole thing doesn't sound obviously any more promising than other approaches to the anthropic landscape. Here, as usual, I agree with [Lubos's comments on this](). But despite this, he seems to have removed me from his blogroll. My feelings are hurt.

Comments

1. **Kea**
   February 10, 2006

   This is great! I must go and read it. On the face of what is quoted here, it sounds much more like a topos sort of landscape than a Stringy one. 8)

2. **andy.s**
   February 10, 2006

   Re: Lubos Delinking.

   Maybe it's because you've been agreeing with him so much lately. It's probably getting on his nerves.

3. **mathjunkie**
   February 10, 2006

   andy.s has the point.

4. **Dave Bacon**
   February 10, 2006

   Hasn't Hawking always used the anthropic principle? I can remember reading his Brief History back in high school and getting very upset with it for using said argument. And I'm pretty certain that during at least one talk I attended while and undergrad at Caltech (93-97) he gave a talk which basically was in support of the anthropic principle.

5. **A. nonymous**
   February 10, 2006

   I would guess that Lubos does not consider this blog to be 'led by science'.

   Regarding Hawking's paper….

   It would appear that all the anthropic stuff amount to no more than fitting data to observed values. There are three (large) spatial dimensions; use that as a constraint on the present state of the universe and say the word "anthropic" while doing so.

   When he makes contact with string theory, he suggests using the geometry of the internal space as part of the boundary conditions which describe the current state of the universe. Then he suggests that only a few metastable landscape vacua will emerge from his procedure. But the problem is that we aren't able to determine what the present geometry of the internal space is (by any conceivable experiment, as Peter likes to point out). If we were able to determine that, we would know which part of the landscape we were in already. I may be missing Hawking's point here, though.

   He says that the largest contribution to the history of the universe comes from configurations in which the universe popped into existence by quantum means – is this a result which has been proved in Euclidean quantum gravity, or not proved at all, or proved more generally? It sounds a little suspicious when somebody says that the present state of the universe is independent of an initial state because the universe with the prescribed initial state will undergo quantum disappearance and be quantumly replaced by a new one.

   Any predictions which will come from his proposal would seem to need to incorporate more constraints than merely the constraints that come from observed values, anthropic or not. It sounds superficially like his "no boundary" proposal provides the additional information that could lead to predictions, but then for any application of it, he puts his favorite instanton at the beginning of the universe and draws conclusions from that. How do we know that there isn't an uncountable number of possible instantons that can match up expanding Lorentzian space with no boundary?

6. **Kea**
February 10, 2006

A.

Yes. The anthropic discussion isn't too alarming here, because the authors point out that such reasoning is simply one kind of constraint.

"Top down cosmology is also more general than anthropic reasoning, because there is a wider range of selection effects that can be quantitatively taken into account."

7. **Haelfix**
February 10, 2006

Hawking has always been a strong anthropic principle advocate, long before he even was interested in String theory. He used to talk about the unsolvability of the fine tuning problems in Inflation theory during astrophysics conferences. I always thought his point of view was needlessly dark and depressing and premature to the extreme.

Anyway, im not a big fan of the no boundary proposal either (a rare topic in QG I know a little about). Wave functions of the universe and things like that are so plagued by conceptual difficulties (like even trying to define what hilbert space you are talking about), that they are rarely taken to be more than gimmicks by people in the field. Its an old story really, and goes back to the eternal problems of defining time in quantum gravity (eg the WDW eqn), and when to take euclidean path integrals seriously (in the HH approach, the integral is not bounded from below) . Not to mention its kinda problematic to leave so many gravitational degrees of freedom un quantisized.

But in the ill understood field of qg cosmology, I guess crude treatments like this are as good as you're likely to get at this stage of the game, at least until we get a better microscopic treatment from somewhere else (eg perhaps string theory).

8. **John Gonsowski**
February 10, 2006

OK if the present is determining a history does that mean the past's present lives through a history via the future? I may need to rephrase this my head hurts.

9. **R.R. Tucci**
February 11, 2006

I think I finally understand what string theorists mean by the Anthropic Principle. What Hawking et al are doing is a pretty standard Bayesian calculation. I'm not sure that this Bayesian technique will be fruitful in the case of String Theory. I don't even know if it makes sense philosophically and logically to apply this technique to String Theory, but the technique itself is firm and solid, and well known outside of String Theory. This technique can be used within the realm of classical probability to select those classical histories that are more promising, or it can used within quantum theory to select quantum histories.
The "final boundary conditions" correspond to the final nodes of a Bayesian Network, and what they are trying to do is to update the probabilities of the non-final nodes of the network based on their observation of the final nodes. As often happens in Physics, these people are reinventing the wheel. It would help if they were less proud and used the language and techniques that have already been developed by statisticians to do this, instead of pretending that there are no precedents. (For those of you who are not familiar with Bayesian Networks, there are a zillion reviews on the web. A Google search for "Bayesian Networks" yields about half a million hits.)

10. **Steve**
February 11, 2006

I strongly agree with the above comments about rediscovering the Bayesian approach.

Another place to look for background information is the comment that I added to Lubos' posting, where I summarised what I *think* are the relevant parts of the Bayesian approach.

11. **Kea**
February 11, 2006

See also a paper by Marlow

12. **George**
February 11, 2006

Lubos removes views he does not like. He is a right winger alright and it comes through in his science as well. It is becoming more and more common for the right to supress debate and ideas they disagree with. It is so revealing in light of the history of right wing repressive societies… now unfortunately we are living in one too. I will listen in on you, but trust me, I am only after the bad guys, and I define the who and what is bad and… so don't worry…

13. **Wolfgang**
February 12, 2006

Peter,

the link is back, but you need to scroll down a bit 😎

14. **Eli Rabett**
February 12, 2006

Technically Motl is not an apparatchik, but a member of the nomenklatura

15. **mathjunkie**
February 13, 2006

Yes, the link of Peter's weblog appears on Lubos weblog again. They become friends again.

16. **Chris Oakley**
February 13, 2006

mathjunkie,

It could just be that the former StB operative that Lubos hired to rub Peter out has been detained by US immigration. Or he went to the wrong place. Check the newspapers to see if a maths professor called New York at Woit University in Columbia has recently been wasted.

17. **ObsessiveMathsFreak**
February 13, 2006

Slightly offtopic, but I am pleasantly surprised that such a well known and also “venerable” physicist such as Hawking would publish first on arXiv.org instead on a journal. It’s surprising how much times have changed.

18. **Steve Myers**
February 13, 2006

After quick run through of paper — yes, Bayesian. But don’t see much in it. A personal note: back, about 20 years ago, when I wasted my time thinking about cosmology, I wrote Hawking asking about an idea I had that involved infinite neg. E at big bang, plus other dumb stuff. He replied that yes, there at that singularity was the problem & that’s what he was working on. It was a pleasant note & he thanked me for the book I’d sent.

19. **mathjunkie**
February 14, 2006

Chris,

I’ll check the newspapers!

20. **Aaron Bergman**
February 14, 2006

I strongly agree with the above comments about rediscovering the Bayesian approach.

Nobody I know who thinks about this sort of thing is unaware of Bayesian statistics.

Susskind Letter to NYT Book Review

February 11, 2006
Categories: Uncategorized

This is about the sixth week in a row that the Sunday New York Times Book Review has had something about string theory or the Landscape controversy. It has become the main place in the popular press to follow this. Tomorrow's issue contains a letter from Susskind responding to the recent review of his book by Corey Powell.

Susskind has two complaints about the review:

1. That he was not engaging in "braggadocio" by writing "as much as I would very much like to balance things by explaining the opposing side, I simply can't find that other side" since "The comment merely reflects a fact that all parties, on both sides of the controversy, agree upon: as things stand now, there is no explanation of the fine tunings of nature other than the one discussed in my book."

This isn't really accurate. For fine tunings other than the CC, there are other widely accepted explanations (e.g. supersymmetry). For the CC, many people don't believe that the anthropic string theory landscape is really an explanation, at least not a scientific one.

2. He justifiably complains that Powell accuses him of believing that we are about to discover a "final answer" to the problems of fundamental physics. He is quite right that he doesn't actually make any such claim, and that his point of view and Powell's don't differ here.

Update: The Moonie-owned right-wing newspaper The Washington Times has a review of Susskind's book. It describes the argument of the book and ends:
To religious believers, the idea that the universe is designed by a Creator to allow the existence of human life is fundamental. To Mr. Susskind and those who think like him, that idea is so unacceptable that they are willing to abandon the idea that nature follows one set of laws, the principle upon which modern science was founded.

The reviewer at least has noticed that Susskind is giving up on modern science, although he attributes this to Susskind's unwillingness to face up to evidence for intelligent design, instead of unwillingness to face up to the failure of his pet theory.

Comments

1. **Who**
   February 11, 2006

   "… I simply can't find that other side" since "The comment merely reflects a fact that all parties, on both sides of the controversy, agree upon: as things stand now, there is no explanation of the fine tunings of nature other than the one discussed in my book."

   This looks like a remarkable case of forgetfulness (or glossing over the facts). In 2004 Susskind registered the existence of a testable explanation (CNS) by posting attempted rebuttals on the arxiv. One of these was subsequently deleted. He entered into an inconclusive exchange over CNS, which included one or more letters published at Not Even Wrong, if I remember correctly, after Smolin posted hep-th/0407213. Susskind was sufficiently exercised about CNS within the past two years that he ought to be cognizant of it, and acknowledge that he knows of a proposed "explanation of the fine tunings of nature" which is empirically testable.

   The CNS explanation may be wrong, but it makes testable predictions and has not yet been shown to be so. This is normal for scientific theories, which stand until falsified since final confirmation is impossible.

   It is obvious that "all parties, on both sides of the controversy" do NOT agree that no explanation has been proposed and thus that there is no "other side" to be mentioned. Susskind suppresses the fact that he has engaged in heated debate with one of the parties on the other side.

2. **Who**
   February 12, 2006

   BTW the topic of the thread is not Susskind in general so much as the two points he made in his letter to the NYT. We can ignore his complaint #2 which Peter says is justified—it was probably included only for completeness. His complaint #1, however, can be challenged.

   "… I simply can't find that other side" since "The comment merely reflects a fact that **all parties, on both sides of the controversy, agree upon: as things stand now, there is no explanation of the fine tunings of nature other than the one discussed in my book."**

   This assertion was challenged by Peter.

   It can also be refuted by reference to an article by Susskind himself posted on the arxiv

   http://arxiv.org/abs/hep-th/0407266
   Cosmic Natural Selection
   Leonard Susskind

   This article by Susskind deals with a proposed (and so-far not eliminated) "explanation of the fine tunings of nature" other than the one discussed in Susskind's book. The fact that he wrote the article shows that he is aware of the existence of this alternative explanation. So when he says that he "can't find" an alternative explanation, or that one does not exist, he is not being forthcoming, and is spreading a misconception.

3. **arnold**
   February 12, 2006

   was my comment censored? why?

   I am very disappointed…..

4. **woit**
   February 12, 2006

   arnold,

   I was away from this for over a day, when I came back there were a large number of comments, many of them irrelevant to the topic, so I deleted quite a few, but then realized that I shouldn't have deleted so many. Unfortunately, once deleted, I have way of retrieving them.

In particular, someone pointed out that it is not true that “all parties” agree that the Landscape is the only explanation of fine-tuning, that “God said so” is an explanation that has many adherents. This is true, and it’s true that this kind of explanation is just as scientific as Susskind’s.

5. **arnold**
February 12, 2006

Hi Peter,
thanks for clarifying.

I was worried that you were adopting the same policy as Lubos: just ban whoever thinks differently!

In particular I completely agree with you: I wanted to point out again that these two explanations are at the same level. No proof, you have to believe them.

6. **D R Lunsford**
February 12, 2006

arnold – Peter only deletes comments that need to be deleted. As the author of many deleted comments, I can attest to Peter’s editorial skill.

-drl

7. **woit**
February 12, 2006

Can’t decide, should I delete that last one or not… These editorial decisions are sometimes difficult.

8. **Chris Oakley**
February 13, 2006

Those who complain about deleted comments should remember that this is Peter’s party and he can invite who he likes.

Personally, I am just grateful that there is *anyone* at a respectable university prepared to give higher priority to truthfulness than acceptance by the academic community. A lot more of this needs to happen.

9. **ObsessiveMathsFreak**
February 13, 2006

> To religious believers, the idea that the universe is designed by a Creator to allow the existence of human life is fundamental. To Mr. Susskind and those who think like him, that idea is so unacceptable that they are willing to abandon the idea that nature follows one set of laws, the principle upon which modern science was founded.

OK, now I’m lost. This is like an Agatha Christie novel. Can anyone me exactly who believes what now? Is this going to turn out that science was in fact murdered by every other passenger on the train!?

10. **anon**
February 13, 2006

OMF:

Nearly everyone is out to destroy science for their own ends, so Susskind isn’t alone. Newton had to overcome all kinds of bigotry.

Newton is the founder of laws of nature. He discovered an inverse square law proof (for circular orbits only) in 1666, but only published his book in 1687. The major delay was doing extra work and finding a framework to avoid three kinds of objectors:

(1) Religious objections (hence religious style laws of ‘nature’/God)
(2) Petty colleagues who would ridicule any errors/omissions
(3) ‘Little smatterers’ (Newton’s term) against innovation (=> Latin)

It would have been delayed longer if Halley had not funded the printing from his own pocket when he did.

11. **Who**
February 13, 2006

the case for resorting to anthropics always seems to have a step where the speaker throws up hands and says “we have no alternative, it’s forced on us!” (as in the Sussking NYT letter where he pretends he could find no alternative explanation of fine tuning)

This step in argument involves suppressing or conveniently forgetting about CNS. I hope this will be come progressively more awkward. One thing that may help is a new article to appear in ELSEVIER HANDBOOK IN PHILOSOPHY OF PHYSICS, by G. F.R. Ellis of the University of Cape Town. This was posted on arxiv today.

see pages 41 and 46 of
http://arxiv.org/abs/astro-ph/0602280
Issues in the Philosophy of Cosmology
George F. R. Ellis
To appear in the Handbook in Philosophy of Physics, Ed J Butterfield and J Earman (Elsevier, 2006)

quote from page 41
***Option 5: Cosmological Natural Selection. If a process of re-expansion after collapse to a black hole were properly established, it opens the way to the concept not merely of evolution of the Universe in the sense that its structure and contents develop in time, but in the sense that the Darwinian selection of expanding universe regions could take place, as proposed by Smolin [205]. The idea is that there could be collapse to black holes followed by re-expansion, but with an alteration of the constants of physics through each transition, so that each time there is an expansion phase, the action of physics is a bit different. The crucial point then is that some values of the constants will lead to production of more black holes, while some will result in less. This allows for evolutionary selection favouring the expanding universe regions that produce more black holes (because of the favourable values of physical constants operative in those regions), for they will have more “daughter” expanding universe regions. Thus one can envisage natural selection favouring those physical constants that produce the maximum number of black holes. The problem here is twofold. First, the supposed ‘bounce’ mechanism has never been fully explicated. Second, it is not clear — assuming this proposed process can be explicated in detail – that the physics which maximizes black hole production is necessarily also the physics that favours the existence of life. **If this argument could be made water-tight, this would**

become probably the most powerful of the multiverse proposals. ***

quote from page 46
***9.2.8 Physical or biological paradigms—Adaptive Evolution?

Given that the multiverse idea must in the end be justified philosophically rather than by scientific testing, is there a philosophically preferable version of the idea? One can suggest there is: greater explanatory power is potentially available by introducing the major constructive principle of biology into cosmology, namely adaptive evolution, which is the most powerful process known that can produce ordered structure where none pre-existed. This is realized in principle in Lee Smolin's idea (Sec.9.1.6) of Darwinian adaptation when collapse to black holes is followed by re-expansion, but with an alteration of the constants of physics each time, so as to allow for evolutionary selection towards those regions that produce the maximum number of black holes. The idea needs development, but is very intriguing:

Thesis H4: The underlying physics paradigm of cosmology could be extended to include biological insights. *The dominant paradigm in cosmology is that of theoretical physics. It may be that it will attain deeper explanatory power by embracing biological insights, and specifically that of Darwinian evolution. The Smolin proposal for evolution of populations of expanding universe domains [205] is an example of this kind of thinking.*

The result is different in important ways from standard cosmological theory precisely because it embodies in one theory three of the major ideas of this century, namely (i) Darwinian evolution of populations through competitive selection, (ii) the evolution of the universe in the sense of major changes in its structure associated with its expansion, and (iii) quantum theory, underlying the only partly explicated mechanism supposed to cause reexpansion out of collapse into a black hole. There is a great contrast with the theoretical physics paradigm of dynamics governed simply by variational principles shaped by symmetry considerations. It seems worth pursuing as a very different route to the understanding of the creation of structure. 37 **

Footnote 37 is cf Susskind's book, Chapter 13. I have not seen it. Perhaps someone could say what is in Susskind Chapter 13 that Ellis might be referencing?

In any case it is only a small gain but having something in an Elsevier handbook will, I guess, make it a tiny bit more difficult for advocates of anthropics to gloss over the existence of this alternative.

12. **woit**
February 13, 2006

Who,

Thanks for the reference to the Ellis article. I hadn't seen it since I don't normally look at astro-ph. But please, that's the third comment here about Susskind not properly considering CNS. Enough is enough

CNS is just one of several alternatives to the anthropic explanation of the CC that people have proposed, but that don't have very wide acceptance. Maybe it's right, maybe not, but it's a sociological fact in the physics community that this is not an idea that has a lot of adherents. Susskind is correct in claiming that there are no other ideas about the CC that have wide support among theorists. The problem is that, while his own anthropic ideas do now have fairly wide support, there is an even larger group of theorists that would argue that what he is proposing isn't actually a testable scientific proposal. Until he comes up with some kind of test of the idea, the "anthropic explanation" of the CC is something that I think most physicists don't consider an actual explanation.

13. **Aaron Bergman**
February 13, 2006

CNS is just as bad as any other stuff. It's just a different choice of a measure. You still have to include some sort of anthropic prior if you feel that computing statistical probabilities is a useful or interesting thing to do.

14. **Who**
February 14, 2006

Peter, I'd be interested in hearing discussion of ANY falsifiable hypothesis that addresses the values of fundamental dimensionless constants.

Susskind calls this "explains fine tuning"—-which puts a kind of anthropic slant on it right at the start.

I'm not talking about the (narcissistic) question of why the parameters of the standard model are suitable to our kind of life. I mean set the issue of life aside and propose a falsifiable hypothesis that has something to do with why the constants are what they are.

I think you may agree that one cannot effectively counter Susskind's "change the rules to save my baby" initiative just by disapproving of it. Or reporting that the majority of physicists disapprove. One has to make the alternatives visible, and discuss them. You mentioned there are several alternative explanations. Does each make some falsifiable assertion?

Aaron: I believe you are mistaken when you say CNS is "just an different choice of measure". The basic testable assertion CNS makes is that you Aaron cannot show us a continuous deformation of the parameters—a path in parameter space starting at the observed values—with a monotone increase in BH abundance along that path. If you CAN find a small continuous change in the parameters which steadily increases BH abundance then congratulations, show it to us and I will say you have refuted CNS. It's that simple.

To talk Bayesian priors and conditional probability is to unnecessarily force the CNS conjecture into a Bayesian framework, at considerable risk of obfuscation. I have been familiar with Bayesian methods for several decades—a good (statistician, decision theorist, etc.) friend of ours has been a Bayesian at least since the 1960s. The current buzz among physicists does not enthuse me. I think I understand some of the appeal to string theorists in their present plight, and why Bayesian methods look especially good. But I think it is unnecessary complication to put everything into that framework. The initial choice of prior and calculating conditional probabilities are often a bit dubious.

Your comment about "just a different measure" was so brief, Aaron, that I could not be sure whether you were referring to the current Bayes buzz or something else. If it was something else, please clarify.

15. **Aaron Bergman**
February 14, 2006

The basic testable assertion CNS makes is that you Aaron cannot show us a continuous deformation of the parameters—a path in parameter space starting at the observed values—with a monotone increase in BH abundance along that path

No, it doesn't. CNS is just a mechanism saying that there are more universes with black holes than universes without black holes. In other words, the measure is highly peaked around those universes that tend to produce lots of black holes. If you believe in that sort of statistics, then the best you can say is that it is *likely* that we live near the peak of this distribution. This is exactly the same as people putting other measures on the space of possible vacua/theories. It just so happens that this measure has a peak (assumedly.) One would think that eternal inflation, for example, might produce a similar phenomenon. More so, if you do want to do that sort of statistics, you have to look at the conditional probability based on the fact that we exist. Now, as I understand Smolin's argument, this probably isn't a big effect for his assumed distribution, but the principles involved are precisely the same.

16. **Who**
February 14, 2006

The basic testable assertion CNS makes is that you Aaron cannot show us a continuous deformation of the parameters—a path in parameter space starting at the observed values—with a monotone increase in BH abundance along that path

here I am paraphrasing what Smolin gave as the basic testable assertion of CNS in his most recent paper about it. Please see the italicized statement at the bottom of page 29 of his paper
http://arxiv.org/hep-th/0407213
Scientific Alternatives to the Antropic Principle.
this is the bulleted statement of the CNS hypothesis in section labeled "5.2 Natural Selection"

If you will pardon me for saying so, Aaron, you seem confused about what is the basic testable assertion of CNS. I would urge you to carefully read section 5.2 of that paper. Don't rely on what you may have HEARD people say that CNS asserts—read a firsthand source.

I must be brief, not wanting to overemphasize CNS. Perhaps we could discuss this at Physicsforums where we would not be abusing Peter's hospitality if we spoke at length.
———————————-

Peter, I stress that ANY directly falsifiable hypothesis bearing on why the dimensionless parameters of the standard model and cosmology are what they are is of interest. What I want is something like "Statement S" in section 5.2 of Smolin's paper in that it is brief and clearly falsifiable, but it doesn't have to be CNS. It would be great to have several alternative testable explanations.

Even though I am good friends with a reputable Bayesian probabilist, I must say that the current Bayes buzz in physics strikes me as a sign of sickness. Smokescreen and distraction while they get anthropics and the Landscape in through the back door. Just my opinion.

17. **Aaron Bergman**
February 14, 2006

If you will pardon me for saying so, Aaron, you seem confused about what is the basic testable assertion of CNS

No, I'm really not. Smolin is precisely saying that the distribution (ie, measure) on the 'multiverse' is strongly peaked. That's all. Look on page 29. It is no more or less testable than any other peaked measure on the space of vacua (or non-peaked — frankly, I find the whole idea of any measure at all being testable rather distasteful.)

18. **Who**
February 14, 2006

thanks for your reply Aaron. I will not say more about our difference of opinion in this thread, out of consideration for Peter.

19. **Aaron Bergman**
February 14, 2006

It might be worthwhile to elaborate how CNS does not obviate anthropic considerations. Imagine a situation, for the sake of arguments, where the number of black holes — or whatever universe creation mechanism you prefer — does not lead to universes particular suited for life. This does not preclude the existence of life, however; even in a fitness landscape such as Smolin imagines, there is percolation away from the peaks. Eventually, there will exist universes in which life is possible. So, in this situation how would one 'predict' the probability distribution of various parameters? You would condition your distribution on the existence of life. This is exactly what the anthropic types do. That Smolin has chosen a mechanism to get a highly peaked prior distribution isn't particularly relevant towards this point in principle, although the particular distribution Smolin argues for may render the anthropic correction minor.

20. **woit**
February 14, 2006

Aaron,

Please, Who is right, enough about CNS unless there really is something new to be said on the subject. This has been discussed at great length already in other postings as well as in this one.

21. **Aaron Bergman**
February 14, 2006

Actually, given that "Who" posts this stuff on CNS every single time anyone mentions the anthropic principle, I don't think anyone has said this in his presence. I finally got annoyed enough to post it.

But I'll stop now.

22. **Who**
February 14, 2006

on an off topic thing: an essay by Halvorson on Algebraic QFT was just posted
http://arxiv.org/abs/math-ph/0602036
Algebraic Quantum Field Theory
Hans Halvorson, Michael Mueger
202 pages; to appear in Handbook of the Philosophy of Physics (North Holland)

"Algebraic quantum field theory provides a general, mathematically precise description of the structure of quantum field theories, and then draws out consequences of this structure by means of various mathematical tools — the theory of operator algebras, category theory, etc.. Given the rigor and generality of AQFT, it is a particularly apt tool for studying the foundations of QFT. This paper is a survey of AQFT, with an orientation towards foundational topics. In addition to covering the basics of the theory, we discuss issues related to nonlocality, the particle concept, the field concept, and inequivalent representations. We also provide a detailed account of the analysis of superselection rules by S. Doplicher, R. Haag, and J. E. Roberts (DHR); and we give an alternative proof of Doplicher and Roberts' reconstruction of fields and gauge group from the category of physical representations of the observable algebra. The latter is based on unpublished ideas due to Roberts and the abstract duality theorem for symmetric tensor *-categories, a self-contained proof of which is given in the appendix."

I expect Peter would in any case find and report this at his convenience, but it seems like happy news so I take the liberty of noting it.
Halvorson's essay is a contribution to the same Butterfield and Earman Handbook that Ellis essay is in!

23. **urs**
February 15, 2006

> Given the rigor and generality of AQFT

No doubt about its rigor, but on the generality side something seems to be missing in AQFT. There is as yet not a single *physically* interesting QFT which would be describeable by AQFT.

While it is to be expected that sometime in the future QFT will be cast in a mathematical form of the sort envisioned in AQFT, it seems that something crucial is missing.

(For instance the locality condition in AQFT seems to be too strict, precluding field commutators as they appear in gauge theory.)

In our math seminars we have AQFT people sitting together with those working on the FRS description of 2D QFT. This is also rigorous, but it does describe physically interesting interacting theories. But amusingly, nobody knows how these two approaches are related.

> and we give an alternative proof of Doplicher and Roberts' reconstruction

In case anyone is interested, I had mentioned Michael Müger's talk on his new proof of Doplicher-Roberts here.

New Frontiers in Scientific Communication

February 14, 2006
Categories: Uncategorized

The last couple days have seen a new phenomenon, which may or may not be some sort of sign of the times. Two different papers appearing on the arXiv contain references to blog postings:

1. In last night's Cosmological Constant Seesaw in Quantum Cosmology by Michael McGuigan, the first two references are to blog postings by Lubos Motl and Sean Carroll.

2. A revised version of a paper by Alicki et. al. appeared over the weekend. The comment section of the arXiv posting notes that the paper has been revised and expanded in response to comments about it made here and here at Dave Bacon's blog The Quantum Pontiff (where he has a posting about this entitled Arxiv Links to Pontiff, Science at an End?).

Not clear to me what this means. Will it mean the end of science? Will SPIRES add blog entries that are referenced in papers to its database? Will they start counting blog references as well as standard citations, allowing you to search for not just "Topcite" papers, but "Topblog" ones too? Will authors of new papers start regularly getting testy e-mails from blog authors complaining that they haven't referenced their blog postings?

Comments

1. **Aaron Bergman**
   February 14, 2006

   In last night's Cosmological Constant Seesaw in Quantum Cosmology by Michael McGuigan, the first two references are to blog postings by Lubos Motl and Sean Carroll.

   Of course, the idea of a seesaw for the cosmological constant goes back well beyond Lubos's post and that comment thread. I'm not sure exactly what the origin is, but I'm sure someone's informed the author in e-mail by now.

2. **ObsessiveMathsFreak**
   February 14, 2006

   Web references of any kind are a bad idea in any paper, simply because of the volitile nature of the web. How can libraries possibly index and record web references for future study? No web page is going to be around forever, and I'd rather not rely on the WaybackMachine while I'm reading papers.

   A web reference is dodgy at best. Think of those poor future generations.

3. **sunderpeeche**
   February 14, 2006

   The first thought that sprang to mind was essentially the same as that pointed out by ObsessiveMathFreak. There is (perhaps) some degree of permanence to the arXiv, and papers are now regularly published with citations to arXiv papers as well as refereed journal papers. But a citation to a blog posting? What if a post is deleted, either deliberately or accidentally? Both type of events have happened on this blog. Journals strongly dislike references to papers "to be published". The paper in question may be rejected (or not submitted, or revised to say something entirely different, etc.).

4. **Jean-Paul**
   February 14, 2006

   What's the big deal? Published papers always contained references to "private communications". Instead of a link, the authors should simply refer to the name of the commenter, so if you want to get credit, use your real name when posting critical comments.

5. **Alejandro RIvero**
   February 14, 2006

   Hmm yep probably they are closer to private communications (even if public) that to academic papers. Historians of Science can track private communications when perusing private archives, and here there are the additional possibility of caches and waybacks machines (archive.org). It is a sort of "private communication, scratched at Exotic Cafe pub" footnote.Now, if one is interested not in credit but in the argument, one can always email to the author and to the quoted author, as it happens in the case of private comm footnotes.

   I met with a similar problem when I found a citation to a wiki page done in Phys. Rev. D 73, 013009 (2006), and just the same week it appeared I was planning to erase such page!!. Of course it was my fault, because I first quoted it myself in an Arxiv preprint. Worse, what if the whole domain is discontinued when my two years lease expires?

6. **Mark Trodden**
   February 14, 2006

   An author who sees an idea discussed on a blog should do some research to find out the history of the idea and trace it back to a paper if possible. As Aaron points out, this is certainly possible in this case.

7. **Dumb Biologist**
   February 14, 2006

   I'm with Aaron, and I'd say further it's just a damn lazy practice, in my opinion. I don't cite "private conversations" or anything not already subjected to peer review (which I acknowledge to be flawed, but still…). I detest that sort of thing the same way I detest "data not shown". If a private conversation is so critical it deserves mention, then subject the contents of that thought process to peer review itself, in the form of a rapid communication (or equivalent) so that it can itself be cited by other investigators.

8. **sunderpeeche**
   February 14, 2006

   Re: Jean-Paul. For private communications, the permission of the source of the communication is required. There has been personal contact between the author and source (possibly via email, not face-to-face). One cannot, for example, simply cite a hearsay conversation that one has overheard. With a blog entry one can cite any post without contacting the author of that post. There is no implicit assurance of quality control.

9. **Sean**
February 14, 2006

The idea had certainly been talked about long before those blog entries. Still, it's possible that something said on a blog could start you thinking about some idea. Might be more appropriate for the acknowledgments than for the references.

10. **Quantoken**
February 14, 2006

A communication is a communication of ideas and information regardless of what form it takes: Is it by means of print on dead trees, or by exchange of electrons.

And credibility of a piece does NOT attribute to the physical communication channel, but rather attribute to its source: the person from which the piece was originated. When you catch an idea from a person, whether from a paper you read, or from a private conversation in the hallway, you pay the same amount of attention and the idea is equally credible if you think the person is credible.

The only trouble is private conversations and blog posts are not consistently and systematically **archieved** for **easy retrival**. But so troubled are many academic publications. They are archived fine, but retrival is not easy: You need to spend a considerable amount of time making a trip to the school or city library search for it, and then spend money to make photocopies. And then after a while the photocopies got lost. If you are in an institution they do provide "free" instant online access to all the publications. But it's not actually free at all: you institution pays a pretty hefty sum of money for the priviledge. And for outsider, you have to pay $30 each time just for a one week online access to one article of no more than 4 pages.

That's exactly why ARXIV is becoming so popular because it serves both proper archiving and free and easy retrival. But blogs are becoming even more popular because it provides more freedom with less censorship. I am sure proper web tools are being developed so web postings are properly archived for easy retrival when they are being referenced. The only short coming is that academies do not get proper credit for their web postings when it comes time for tenureship consideration. But that can change, too. I am just not sure whether that will actually help or hurt Lubos.

Quantoken

11. **ksh95**
February 14, 2006

Dumb Biologist says:
…it's just a damn lazy practice, in my opinion. I don't cite "private conversations" or anything not already subjected to peer review…

In physics, private communications are a critical part of research. Many an hour is spent at the tail end of a talk or in a collegue's office discussing fine points of a certain idea.

When some one provides a critical insight to some problem it's only fair to somehow acknowledge that person. It's also rude to demand that the insight-providing person spend their valuable time writting a rapic communication.

Now, the appropriate place for an acknowledgement is an exercise left to the reader.

12. **sunderpeeche**
February 14, 2006

"That's exactly why ARXIV is becoming so popular because it serves both proper archiving and free and easy retrival." Indeed yes.

"But blogs are becoming even more popular because it provides more freedom with less censorship." This gets into the murky area of what constitutes quality control and censorship. I submit a paper to a peer-review journal, and it is rejected. Am I being censored? I can (a) rebut the referee arguments (b) submit to another journal (c) post it online (arxiv, blog) or even (d) start my own blog (if other managers delete my posts). I am no longer censored. Is my work of any scientific value? Who can say?

Do not be upset then if string theorists post zillions of papers many (all?) of which may lack scientific value.

13. **Dumb Biologist**
February 14, 2006

Well, benchmonkey biology is certainly a lot different than theoretical physics, I'm sure, but it's not like we don't talk. Sort of along the lines of what Sean said above, I might choose to acknowledge a person whose guidance I felt merited special mention. I might just make the person an author if the contribution was big enough. In either case, everything important about that communication would be contained within the body of the manuscript, where it is readily accessible to the referees and the readers, and attains a level of acceptable review and permance for future citation. I would *not* refer to that content in a citation to, as those above have pointed out, a personal exchange in a highly volatile medium (unrecorded sound waves, blogosphere, I don't care). Nor would I, again, as Aaron points out, let the contents of a conversation stand alone without first making some critical citation of the referencible origins of those thoughts, which, again, can be perused by the reader and referees. It's certainly rude to add, as a citiation, something essentially along the lines of "Joe Blow, PhD sez X, Y, and Z in an email" when J.B. might have cribbed the idea (even inadvertantly) from someone else who took the trouble to submit it to a journal.

14. **anon**
February 14, 2006

"someone else who took the trouble to submit it to a journal."

That person may have cribbed it from an outsider who was suppressed by the journal

15. **Hal**
February 14, 2006

Everyone who publishes on the web should be familiar with Tim Berners-Lee's (the "father of the web") article, "Cool URIs Don't Change", http://www.w3.org/Provider/Style/URI .

Berners-Lee argues forcefully that authors and publishers should leave URIs alone, because the public may be referring to them. Rearranging your web site is not like rearranging your furniture. It's more like changing the road layout in a city. People are dependent on those old addresses still working. There are serious negative externalities to changing URLs. It's as bad in its own way as dumping pollutants in a river – easy and convenient for you, but harmful to the general public.

As for domain names, with costs of ten bucks per year, everyone who publishes should be able to afford one. Let's suppose Peter Woit moves to another institution than Columbia. Suddenly http://www.math.columbia.edu/~woit/wordpress/ won't even be wrong, it'll be dead. This could be avoided if he spent the cost of two Starbucks lattes on http://www.notevenwrong.com and redirected it to his academic URL. Then when he moves, he could change the redirection and everything will still work, all the old links will still be OK.

People think of blogs as just personal hobbies, for their own entertainment. But blogs like this benefit from public feedback and public interest. In exchange for those benefits, blog authors and other web publishers should understand and accept the obligation to become good citizens of the web. A major part of that is insuring that their links will still work in the future.

16. **woit**
February 14, 2006

I've thought a bit about the question of how to keep the content of this blog readily available for as long as possible in the future, including whether I should move to a commercial hosting service and/or get my own domain name (I do own and operate a private webserver and domain name for use by my mother, who is an artist). One problem with a private domain name is that it would soon disappear if I get run over by a bus or am otherwise no longer around. On the other hand, Columbia has been around for about 250 years, should definitely outlast me, and has as part of its mission the preservation and transmission of knowledge. Not clear how long http://www.math.columbia.edu will continue to point somewhere useful, but as long as it does, if I haven't been run over by the bus I believe that I should be able to get someone to configure http://www.math.columbia.edu so that ~woit/wordpress points to wherever the blog material is. If I'm not around, and people think this material is worth preserving, this address is as likely as any to be one that is up and could be configured to point to the right place.

But in general it is certainly a good idea to keep in mind that blog content may be ephemeral. Things of lasting importance should be written up and archived in a form more likely to survive.

17. **anon**
February 14, 2006

'As for domain names, with costs of ten bucks per year, everyone who publishes should be able to afford one.'

Then it gets deleted when the person dies. Some ideas have to stick around longer than 75 years for anybody to pay attention. Aristarchus' solar system of 250 BC was ignored until 1500 AD. What if the internet had been around in 250 BC and Aristarchus bought a domain name instead of writing on parchment?

18. **Quantoken**
February 14, 2006

Hal:

Funny you meantioned that Peter should put his stuff on http://www.notevenwrong.com. It says "this domain is for sale" and I guess it probably cost Peter more than two Starbucks lattes to buy it

There is URL re-direct and I just don't know how it works. Preferably it should be the responsibility of the author of a paper to always maintain a valid URL link or an archived copy of all the references he/she made. If your paper references something, the readers of your paper expect to be about to find the material that you referenced to, and verify by themselves that you have not mis-represented the original material. What's the point of referencing something that no one else can see? (e.g., references like this kind: "For details please look for an un-published and none-exist paper by such and such.") References are much more than mere acknowledgement of credits of other people's work.

Doing so is actually much easier than you thought! Technology already exist and in wide use for things like that. For example various file swapping networks that account for more than 90% of all internet traffic today. How do people get what they want from the web, without even knowing a URL where it is available? Easy, everying is referenced by a MD5 checksum. You just tell the computer the MD5 checksum of the thing you are looking for, and the internet takes care of the rest.

Same thing can be done with online publishing of academic papers. You reference a paper and attach a hidden MD5 checksum of that paper, and make sure a copy of that paper is already available online. And people can instantly gain access and obtain a copy of the exact paper, without you even need to provide a URL. I envision a tool to do all these automatically so adding a paper reference for the author or access a reference for the reader is as easy as one mouse click. That would be real nice. The only problem would be copyright. But then if all authors are willing to copyleft their papers so more users can access them (who doesn't want that), then that's not a problem.

Quantoken

19. **woit**
February 14, 2006

Quantoken,

Your capability of spreading misinformation and nonsense on a wide variety of topics is truly impressive, please stop. And please, other people, don't encourage him.

20. **Dumb Biologist**
February 14, 2006

I agree unjust suppression and other forms of blacklisting can be a problem, but "anything goes" sounds a lot worse to me. Peer review can fail in a number of troubling ways, no question (the whole fictional-cloned-human-ESC catastrophe being exhibit A, in my mind), but the alternatives are probably too chaotic to be workable. Better to make minor corrective adjustments than abandon a largely working system, IMHO.

21. **Quantoken**
February 14, 2006

Peter:

Please enlighten me (and others) **exactly** which part of what I said is mis-information and nonsense. I am all ears to enjoy. Otherwise you are making un-founded and un-supported accusations. Especially it now looks like whatever you can't agree with or whatever you simply don't know about, you automatically call it nonsense. I happen to be an industry expert on online content delivery, mind you.

Quantoken

22. **woit**
February 14, 2006

Quantoken,

I'm extremely busy these days, and the last thing I in the world I have time to do is to discuss with you why your proposals are unrealistic, especially since these are about a topic different than the one I was posting about. I have had a lot of complaints from readers of this blog that allowing comments by you and several other people that shed no light at all on the topics under discussion causes people who do have something relevant to say not to want to participate here. I'll delete any further comments from you or from anyone else about distributing web content. Take them elsewhere.

23. **lurker**
February 14, 2006

don't censor quantoken! his posts are the funniest on the web. I love it when he puts stuff in **bold**.

24. **D R Lunsford**
February 14, 2006

I wrote a paper showing that CC was zero, and why.

No one is interested in results.

-drl

25. **amanda**
February 15, 2006

"An author who sees an idea discussed on a blog should do some research to find out the history of the idea and trace it back to a paper if possible. As Aaron points out, this is certainly possible in this case. "

Really? How? I tried both hep-th and spires and only got this paper!

26. **Juan R.**
February 15, 2006

No, it will not mean the end of science.

In general, Blog entries that are referenced in papers will be not added to scientific databases. I see difficult "they start counting blog references as well as standard citations"

The blogger phenomenom is very related to usual referencial structure of communities as physics or even mathematics. There is not blooger similar phenomenon in chemistry, for example.

It is more, whereas the fragility and open nature of ArXiV and similar repositories are very adequate for physics science, it is very inadequate for the requirements of chemical science, for example.

This is reason that ACS has not launched a chemical ArXiV still. And it is the reason that project for a chemical "ArXiV" (the CPS) was abandoned some years ago.

Howerver, we will see changes in current publication policies in most of disciplines in next few years.

—

Juan R.

Center for CANONICAL |SCIENCE)

allowing you to search for not just "Topcite" papers, but "Topblog" ones too? Will authors of new papers start regularly getting testy e-mails from blog authors complaining that they haven't referenced their blog postings?

27. **ksh95**
February 15, 2006

Really? How? I tried both hep-th and spires and only got this paper!

Wow, is this what we've come to??? You do realize that your local university library has 10^500 distinct ways to perform literature searches.

28. **anon**
February 15, 2006

"The comment section of the arXiv posting notes that the paper has been revised and expanded in responsed to comments about…"

You need to delete the "d" from the end of the new word "responsed" in this post, Peter, it looks careless.

But don't worry, a technical journal I write for published an opinion piece spread over several pages, each headed "Personnel View". When people wrote in to say it should be "Personal View", the editor replied that word check didn't find a fault, so that was to blame…

29. **Carl Brannen**
February 15, 2006

As an example of a website whose author died unexpectedly, but which remains as a source on the web, there is the expert on Clifford Algebra, Pertti Lounesto. His website is still maintained some 4 years later:

http://users.tkk.fi/~ppuska/mirror/Lounesto/

30. **Eli Rabett**
February 16, 2006

I have thought about the issue a bit. It seems to me that the answer is to require authors to submit images of the pages they reference as suplemental materials. For paper journals this could be done on CD (or uploaded to a CD archive) which would be available to interested users although perhaps not on line (e.g. you would have to contact the journal and they would send you the files by Email)

31. **Bill Tozier**
    February 16, 2006

    I recently started a "transmissible questionnaire" that asked folks to look themselves up on Google Book Search.

    One of the things I note is Cosma Shalizi's references in books. Several of which are references to his blog, or to his ur-blog, the *Bactra Review*, or to his Notebooks. Including footnotes, and other references.

    In books.

32. **Thomas Larsson**
    February 16, 2006

    (e.g. you would have to contact the journal and they would send you the files by Email)

    What if the publisher has gone out of business?

33. **anon**
    February 17, 2006

    Bertrand Russell once had a nightmare about the future, where he saw librarians burning old books (including his own) to make shelf space for new ones. Civilisation may collapse, so it is pointless to go to too much trouble if it all ends up destroyed.

34. **Alejandro Rivero**
    February 17, 2006

    The ArXiV is mirrored in all the continents, about a dozen copies. Same of the printed copies of main journals. On the pessimistic side, the extant content of the Alexandrian Library can be compressed into one CD. Of course this is a motivation for "reason-in-march", any physicst should be able to rebuilt the whole theory up to QFT from scratch 🙂

    As for webpages, well a suggestion is to quote the date when the page was consulted. Sort of telling the date of a letter when quoting a letter.

35. **anon**
    February 18, 2006

    'I wrote a paper showing that CC was zero, and why. No one is interested in results.' – drl

    Have you seen Penrose's new cosmological theory? See http://news.bbc.co.uk/1/hi/programmes/hardtalk/4631138.stm

    He argues for an endless sequence of big bangs, but without gravitational collapse. As far as I can make out, if he is right, as soon as the last proton decays the universe will lose all measure of time, and hence spacetime will end, so the universe will become a new singularity leading to a fresh big bang. He says it does have testable predictions, and he didn't mention CC.

36. **Chris Oakley**
    February 18, 2006

    Re: Roger Penrose – The interview reminds me of the following, (from Douglas Adams):

    "There is a theory which states that if ever anyone discovers exactly what the Universe is for and why it is here, it will instantly disappear and be replaced by something even more bizarre and inexplicable."

    [and on the following page]

    "There is another theory which states that this has already happened."

    Penrose seems to be saying that this happens in a continuous cycle.

    I suppose that Susskind, Hawking, Weinberg, et al would regard him as a stick-in-the-mud traditionalist as he also proposes ways in which his idea can be tested.

Fields Medal for Terence Tao?

February 15, 2006
Categories: Uncategorized

Lubos Motl has a posting announcing that Terence Tao will be one of the 2006 Fields Medalists. The announcement of the Fields medals is officially made at the time of the International Congress of Mathematicians, which this year will be in Madrid in August. A few months before the Congress generally there are solid rumors circulating in the math community about who the winners will be. If Lubos is right (and while I don't know his source, this agrees with earlier speculation), the blogosphere will be responsible for a much earlier spread of rumors about this than usual.

Update: Lubos's posting just disappeared. So, maybe this rumor is not right, or maybe it is, but whoever he got it from didn't want it spread so publicly. His posting has been replaced with the comment "I removed information about a certain medal that was far too preliminary."

Comments

1. **Quantoken**
   February 15, 2006

   Not surprising. I guess Yau would definitely be the first one to know who wins, if he is not one of those who decides. And Lubos would learn that quickly due to proximity to Yau's office.

   His Fields winning proof can be found here. I can only understand the first few pages. What interests me is conjecture 2.2, which is awesomely simple and is a much more general statement that can automatically lead to what he is trying to prove. He did not attempt to prove that conjecture. Has any one attempted at that conjecture?

   It's simply stated as that as long as an infinite sequence of integers {ai} satisfy sum(1/ai) = infinity, then it follows that within {ai} you can always find infinite collections of progressive sequences of any arbitrary length, i.e., sequences where all elements bi can be found within {ai}, and bi = p + q*i, 0

2. **anon**
   February 15, 2006

   I followed Quantoken's link above to the Field's medal paper on arxiv.org, and it is 56 pages of beautifully written text and maths. I like the historical introduction, and the way that the various components are described in detail first, instead of referring the reader somewhere else. It's the kind of helpful paper that encourages people who find pure maths a real headache (like me).

   Congratulations to Terence Tao, whether he has won or not! To me what is even more interesting than his childhood story is where he discusses his way of working in an understandable way. See

   http://www.college.ucla.edu/news/05/terencetaomath.html –

   How does Tao describe his success?

   "I don't have any magical ability," he said. "I look at a problem, and it looks something like one I've already done; I think maybe the idea that worked before will work here. When nothing's working out; then I think of a small trick that makes it a little better, but still is not quite right. I play with the problem, and after a while, I figure out what's going on.

   "Most mathematicians faced with a problem, will try to solve the problem directly. Even if they get it, they might not understand exactly what they did. Before I work out any details, I work on the strategy. Once I have a strategy, a very complicated problem can split up into a lot of mini-problems. I've never really been satisfied with just solving the problem; I want to see what happens if I make some changes.

   "If I experiment enough, I get a deeper understanding," said Tao, whose work is supported by the David and Lucille Packard Foundation. "After a while, when something similar comes along, I get an idea of what works and what doesn't work.

   "It's not about being smart or even fast," Tao added. "It's like climbing a cliff; if you're very strong and quick and have a lot of rope, it helps, but you need to devise a good route to get up there. Doing calculations quickly and knowing a lot of facts are like a rock climber with strength, quickness and good tools; you still need a plan – that's the hard part – and you have to see the bigger picture."

   His views about mathematics have changed over the years.

   "When I was a kid, I had a romanticized notion of mathematics — that hard problems were solved in Eureka moments of inspiration," he said. "With me, it's always, 'let's try this that gets me part of the way. Or, that doesn't work, so now let's try this. Oh, there's a little shortcut here.'

   "You work on it long enough and you happen to make progress towards a hard problem by a back door at some point. At the end, it's usually, 'oh, I've solved the problem.'"

   Tao concentrates on one math problem at a time, but keeps a couple of dozen others in the back of his mind, "hoping one day I'll figure out a way to solve them. If there's a problem that looks like I should be able to solve it but I can't, that gnaws at me."

3. **mathjunkie**
   February 16, 2006

   Glad to know that Tao, a Chinese mathematician (correct me if I am wrong), has got a very good chance to win the Fields medal. He will be the second Chinese winning the Fields medal after Yau if he will make it. Tao is modest about his math ability in his speaking. But why are his works important? Somtimes, I don't know why mathematicians like to solve mathematical problems that seem to have little relevance/applications to our daily life.

4. **MathPhys**
   February 16, 2006

   Tao is Australian, probably of Chinese descent, but Australian. I doubt if he finds all this talk about his future Fields medal entertaining.

5. **David MacIver**

February 16, 2006

mathjunkie, I'd suggest that relevance or applications to our lives isn't really a good metric to use for mathematical problems. Or indeed most problems in academia. I mean, suppose tomorrow some bright spark has an 'ah ha!' moment and formulates a complete and consistent theory of everything. Will this help me build a better mousetrap?

Sure, in twenty, thirty year's time the theory might lead to the invention of the quantum frambotzulator, revolutionising the frambotz industry and profoundly changing all our lives. But is that really why we want a theory of everything? Wouldn't it be just as interesting without the frambotzulator?

Of course it would. We don't want a theory of everything because of the technology it might lead to, we want a theory of everything because it's cool and interesting.

You can justify physics by applications to engineering, and mathematics by applications to physics, other practical subjects, etc. but frankly doing so is missing the point. Tao's work will probably never see a useful application, but this doesn't make it any less cool and important.

There's a great quote which is attributed to Feynmann: "Physics is like sex. Sure it has a useful purpose, but that's not why we do it." The same is true of mathematics.

6. **ObsessiveMathsFreak**
February 16, 2006

> It's simply stated as that as long as an infinite sequence of integers {ai} satisfy sum(1/ai) = infinity, then it follows that within {ai} you can always find infinite collections of progressive sequences of any arbitrary length, i.e., sequences where all elements bi can be found within {ai}, and bi = p + q*i, 0

I know of at least one sequence that satisfies this relationship, namely the integers. The harmonic sequence of 1/1 + 1/2 + 1/3….. has no upper bound. And of course in the integers, you can find an infiite amount of progressive sequences if you play with the p's and q's.

7. **sunderpeeche**
February 16, 2006

Lubos had a post reporting a rumor. Later the post was deleted. The post here is a reference to a (now deleted) post of a rumor.

"So, maybe this rumor is not right, or maybe it is, …"
Ugh. The post here should be deleted in its entirety.

If there are "reliable sources" (~ earlier speculations), then cite the credible sources.

8. **Dick Thompson**
February 16, 2006

sunderpeeche, I don't agree, for this reason. The comments on the post have occasioneed some very interesting mathematical discussions. This is quite diffferent from just retailing rumors that "Enquiring Minds Want to Know". 🙂

9. **woit**
February 16, 2006

I wasn't sure whether to leave this post up or not, but don't see a good reason not to. It's true that Terry Tao might not be amused by all this, but if so, I fear that's the price of fame. If and when he does get a Fields medal, he'll have to learn to contend with the massive media attention and the women throwing themselves at him. The Fields medal committee may not be happy if this is a genuine leak, but then it's their own fault for having members who blab to Lubos.

In general I feel that rumor-mongering (when done with scrupulous accuracy), is a fine thing to do on a blog like this. Unfortunately I normally feel constrained not to repeat interesting rumors here, because it almost always would be too obvious to many people who my source was, and said source would not be pleased (and would stop repeating juicy rumors to me). This case is kind of different, Lubos did make a public announcement….

10. **Zelah**
February 16, 2006

> Hi,
>
> Slightly off topic.
>
> The proceedings for ICM 1998 is availiable on the website for download. But, I have never been able to find the proceedings for ICM 2002!
>
> I was wondering if anyone knows if the proceedings for ICM 2006 will be availiable for download direct from the site!
>
> An amateur mathematician.

11. **Ark**
February 16, 2006

In reference to David MacIver's comment that "Tao's work will probably never see a useful application, but this doesn't make it any less cool and important." I would like to say that this prediction is entirely incorrect. Indeed, please look at Notices of AMS (February 2001) pages 175-186 where you will find a paper by Tao and Knutson for which they both got a mathematics prize already. But, in addition what they are saying this this paper is immediately useful in theory of quantum computation, e.g. see Ann. Phys. 315 (2005) 80-122, and soon will find its rightful place in string theory…

12. **anon**
February 16, 2006

> Ark said:
> ", and will find its rightful place in string theory"
>
> Ark, you should write for John Stewart, or The Onion

13. **Ark**
February 16, 2006

Annon, can you be a bit more specific: who are these people and why do you think that I have to write to them…? especially in connection with string theory?

14. **John Gonsowski**
February 16, 2006

TV Comedian Jon Stewart and spoof website The Onion though I've never seen Stewart's show and only saw The Onion during a web design course. Apparently anon is not a string theory fan though even in that context one has to keep in mind that parts of string theory could stay around and be useful even if string theory is ultimately changed to look a lot like say LQG.

15. **ano nymous**
February 16, 2006

I sort of had the impression that Tao's work has a lot of ingenuity, but doesn't have depth at a level comparable that ingenuity (not counting his work with Knutson).

I tend to think similar things about Mozart's music, too, so the comparison doesn't bother me :). Calling him the Beethoven of math would, though.

(I am willing to be corrected, as I am no authority here.)

16. **ark**
February 16, 2006

John,
thank you for clarifications…I am not fan of the existing string theory formulations as well…Nevertheless, I happen to know how Tao's results can be used in string theory. Normally, good mathematics always find its place in physics.. In fact, I can also see some bioinformatics-type applications of Tao's results as well in near future… Just read the references I've provided…!

17. **David R. MacIver**
February 17, 2006

I knew I should have qualified that statement. 🙂

Ark, I didn't mean Tao's work in general. While I wasn't aware of any specific applications, given what I know about what he does it would seem very unlikely that none of it was applicable (after all, he does harmonic analysis and this is a very useful subject in general). I was referring specifically to the result about arithmetic progressions in the primes, and similar stuff he's done.

18. **zerocold**
February 17, 2006

Ok, the best thing in the world to discriminate the question is to ask Professor Tao. It was what I do with my gmail account my mail was

Dear professor Tao,

Today the blog Not even wrong (http://www.math.columbia.edu/~woit/wordpress/?p=350) has rumored that you won the Fields Medal 2006,

I don't know if you can reply me sincerly but is this notice true?

Thanks for you attention

—
Dr. Piero Giacomelli

His answer arrived this morning:

It's certainly news to me. I actually have no clue who is going to win
the Fields this year.

Terence

—
Terence Tao, Department of Mathematics, UCLA
http://www.math.ucla.edu/~tao
Email: teorth@gmail.com / tao@math.ucla.edu

I belive this closes the question.

zerocold

19. **Troublemaker**
February 17, 2006

zerocold: *I belive this closes the question*.

Not really. He could be, you know, lying.

20. **Anonymous**
February 17, 2006

Well, let's not forget that sometimes the committee awarding the fields medal makes some "political" choices as well; Deligne did his award-winning work in the early seventies, but didn't recieve the fields medal until 1978. The 1974 fields medal went to Bombieri, who would have been over 40 in 1978.

I don't know how old Terence Tao is, but Ben Green is probably not even 30, so he might last another ICM or two…

21. **Anonymous**

February 17, 2006

In the last post, substitute “Bombieri” by “Mumford”.

22. **Adrian H.**
February 17, 2006

Terence Tao grew up in my home town of Adelaide, Australia. He was a famous child prodigy, but is equally famous for being rather shy and retiring. I’m sure there are many in Australia who wish that he had stayed in the country to enhance the country’s mathematics. But he left and we are all rather proud of him. (BTW the level of maths education seems to be slightly higher in Adelaide than in Sydney or Melbourne, and perhaps that contributed to his success in some small way.)

23. **zerocold**
February 17, 2006

Yes he could lying but I cannot see the eventual reason

24. **Zelah**
February 17, 2006

“I sort of had the impression that Tao’s work has a lot of ingenuity, but doesn’t have depth at a level comparable that ingenuity (not counting his work with Knutson”.

An amazing thing for someone who is anonymous to post!
The only mathematician of the twenty century who work was truly abstract and yet got results was Grothendieck! Everyone else, had to ‘localize’ as it were!

An amateur mathematician

25. **Helger Lipmaa**
February 18, 2006

Some of his work has found applications in theoretical computer science. The paper “Gowers Uniformity, Influence of Variables, and PCPs” (Samorodnitsky, Trevisan, http://www.cs.huji.ac.il/~salex/papers/gowers.pdf) at least partially builds up on two papers of Green and Tao. The question of course is whether thereotical computer science itself is an application. 🙂

26. **Luca**
February 19, 2006

Somebody asked about the ICM 2002 proceedings. All the papers are available on the arxiv. One way to find them is to search for “ICM” in the “Journal-ref” entry, or follow the link
http://front.math.ucdavis.edu/search/jr:icm-beijing

At least ten ICM 2006 papers (including Tao’s) have also been posted on the arxiv by the authors.

27. **Zelah**
February 21, 2006

Wow!

This is an amazing resource!

An amateur mathematician

28. **Zelah**
February 25, 2006

I have had now a good look now at what Mr Terence Tao has acheived, and he is the surest bet for the Field Medal I have come across!

An amateur mathematician

Yau Survey of Geometric Analysis

February 17, 2006
Categories: Uncategorized

There's a remarkable new paper out from Shing-Tung Yau, entitled Perspectives on Geometric Analysis. Yau is probably the dominant figure in the field of Geometric Analysis in recent years, and in this paper he gives his personal perspective on the field, including many comments on its recent history, where it is now, and where he thinks it is going.

The paper begins with a dedication to Chern, and some personal history of Yau's interactions with him. It includes an outline of the distant and recent history of geometric analysis, mainly by giving names of the mathematicians involved. There are 755 references in the reference section, listing pretty much all the papers that Yau sees as important for one reason or another. This has to be some kind of record for number of references in a paper, especially a paper whose main text is only about 50 pages long.

Yau covers an immense amount of ground, commenting on a very wide variety of topics. This is a paper aimed at those who already know quite a bit about the subject, or who are beginning to learn it and would appreciate recommendations of what they should be reading. It includes very little in the way of expository material aimed at the beginner. There is a long section on "Ricci flow" techniques, which are the topic of a lot of current research and that Yau considers to be "the most spectacular development in the last thirty years." He also has quite a bit to say about "Calabi-Yau" manifolds and their use in physics, commenting that they provide "a good testing ground for analysis, geometry, physics, algebraic geometry, automorphic forms and number theory."

Another expository paper also appeared on the arXiv last night, but one of a very different nature. It's by Ravi Vakil, a young algebraic geometer at Stanford, and it is aimed at explaining how Gromov-Witten theory has been used in recent years to study the moduli space of curves. It includes a lot of expository material about the moduli space of curves, and is designed to be understandable by the non-expert.

Comments

1. **Quantoken**
   February 17, 2006

   Wow, I did not know that **Yau** was actually a fan of **Chiung Yau**'s romantic novels. The poetry he quoted on page 3 was from one of the most popular romantic novels written by **Chiung Yau**, which was also made into movie.
   Curiously it was supposed to be a "she" in the middle of water but Yau made it a "he". Clearly he is dedicating this poetry to his teacher, Chern. Very touching. I would also say that this paper is very readable.

   Quantoken

2. **Joe Zhou**
   February 18, 2006

   Quantoken is a truly confused person. He's spreading misinformation again. Every sentence above is misinformed.

   The poem Yau quoted is ~3000 years old, appearing in the Book of Poem edited by Confucius under the chapter of Winds of Tang.

   The person being sought in this poem is referred to with a gender-neutral third-person pronoun, impossible to translate into English without destroying the poem.

   It's Chiung Yao, not Chiung Yau; no relation here — it's a pen name. In fact the Yao in Chiung Yao is not even a last name. I'll lose quite a bit of esteem for Yau if he quotes from Chiung Yao (eck!)in his paper.

3. **D R Lunsford**
   February 18, 2006

   That is really a wonderful paper! If not for the missing equations of any kind, I might be reading Pauli's encyclopedia article on relativity. However this is exactly what is most needed, a verbal outpouring of mathematical ideas like Klein used to make.

   Readers who may not be familiar should consult "Vorlesung uber die Entwicklung der Mathematik im 19'ten Jahrhundert" for a similar example of mathematical perspective-taking on this advanced level.

   -drl

4. **MathPhys**
   February 18, 2006

   "Vorlesung uber die Entwicklung der Mathematik im 19′ten Jahrhundert"

   is available in English as

   Development of Mathematics in the Nineteenth Century, and published as volume 9 of Robert Hermann's Lie Groups Series.

   It's a great book.

5. **mathjunkie**
   February 18, 2006

   Joe Zhou,

   Hi, just curious about what the Book of Poem edited by Confucius under the chapter of Winds of Tang is in Chinese. (Hope that Peter's weblog can properly display Chinese characters) Thanks.

6. **Quantoken**
   February 18, 2006

   Joe:
   Get real!

There are different ways of translating Chinese names into English. For example Yau himself, it could have been called "Qiu" or "Chiu" using standard mandarin pronunciation. But he prefered his native Cantonese "Yaou", or "Yau". I deliberately used the translation "Chiung **Yau**" for a little bit humor. Too bad you have no sense of humor. It's true it's a 3000 year old poetry, but if it were not for that romantic novel making it widely known, few Chinese even know it, lest along quote it.

7. **Alice**
February 18, 2006

actually, Miss Chiung Yau ever wrote a lyric by adapting that poem from the Book of Poem.. You might feel interested in it. LINK to speical blog (in hinese)–> http://william.cswiz.org/blog/archives/2005-04-10/poem-and-song-part1/

8. **Quantoken**
February 18, 2006

蒹葭蒼蒼，白露為霜。
所謂伊人，在水一方。
遡洄從之，道阻且長；
遡游從之，宛在水中央。

蒹葭凄凄，白露未晞。
所謂伊人，在水之湄。
遡洄從之，道阻且躋；
遡游從之，宛在水中坻。

蒹葭采采，白露未已。
所謂伊人，在水之涘。
遡洄從之，道阻且右；
遡游從之，宛在水中沚。

Yau destroyed the beauty of this ancient Chinese poetry. He should at least give it a taste of ancient-ness, using Shakespear's language style.

9. **mathjunkie**
February 19, 2006

Quantoken,

Thanks for posting the whole poem. I didn't know that you even knew the Chinese language. The Chinese poem is very difficult for me. I can't understand it though I am Chinese.

10. **Lubos Motl**
February 19, 2006

It is actually a Czech poem from 3500 B.C. originally. Let me tell you the original poem in the simplified combined Czech Chinese:

游蒹葭伊蒼，白露為蒹霜。Cestující v tramvaji
水所謂伊人，在水一方伊。mají úsměv na líci,
遡洄遡從之，道阻方且長；když známou znělku zahrají
遡游之從之，宛在水中央。v Hellichově ulici.

蒹葭凄洄凄，白之露未晞。Jemná ruka umělcova
所謂伊游人，之在水之湄。v duši kreslí ideál
遡洄躋從之，道阻人且躋；krásnější než všechna slova,
遡游洄從之，宛在水中坻。a tak je nechám opodál.

蒹葭采阻采，白露水未已。Kde bys hledal v zemi české
所謂中伊人，在中水之涘。krásku ve snu zjevenou?
遡洄從阻之，道游阻且右；Na matfyzu dívky hezké
遡游從阻之，宛在水中沚。nedostaly zelenou.

11. **robert**
February 19, 2006

In less enlightened times it was a commonplace to categorise the Chinese as inscrutable. Is Quantoken having a laugh? Is mathjunkie being facetious? Does ???????; ?? or whatever appear differently on other people's screens? (??>;'?) Is the quantity in brackets a reasonable representation of Charlie Chan? This sequence of posts started out among the more sensible and informative; somehow it was subverted along the way.

12. **Luboš Motl**
February 19, 2006

Dear Robert, you seem to be the only person here who can't read Chinese. Go to your Control Panel / Regional Settings / Languages, and click Install files for East Asian languages. If you don't have Windows, then uninstall your other OS and buy a CD with XP. Otherwise your future is not too bright because the future belongs to China.

13. **Quantoken**
February 19, 2006

Ho ho ho, Lubos:-) You thought no one else here but you knows Czech language? You actually plagiarized your romantic poetry from this Czech web site 🙂

Quantoken

14. **Qui-Quien Hu**
February 19, 2006

and anyway the first line of that Czech poem contains the word
"tramvaji" which means streetcar.
So how can it be from 3500 BC?

15. **worldspace**
February 19, 2006

It's a romantic poem from between 500 BC and 1100 BC in ancient China.

16. **worldspace**
February 19, 2006

Yau wrote a wonderful paper! It's very useful for those who have begun their career in Geometric Analysis.

17. **Luboš Motl**
February 19, 2006

Dear Quantoken, as an internet searcher, you're a superstar. 🙂

Qui-Quien Hu: the electric streetcars were operating in Bohemia long before the Slavs came to the territory. See the history at

http://www.skoda.cz/holding/produkty?catid=11110

2000 years later, Marco Polo actually made his journeys to Asia in order to sell some of these streetcars.

18. **Adrian H.**
February 20, 2006

Back to the Yau paper.

On the first page Yau says that he set Chinese students the problem of a `Jordan curve bounding two surfaces…' He then says that this resulted in an ugly fight at the 60th Anniversary meeting of the Chinese mathematical Society and Professor Wang was forced to resign!

Can anyone throw some light on this extraordinary fracas? What is it about Jordan curves that would cause a professor to resign? What on earth were they fighting about?

(Ah! Academia — such a bucolic environment for seeking the Truth!)

19. **Juan R.**
February 20, 2006

Otherwise your future is not too bright because the future belongs to China.

I thought that future belongs to anyone in China or not.

—

Juan R.

Center for CANONICAL |SCIENCE)

20. **worldspace**
February 20, 2006

Let's focus on "Geometric Analysis". It's really a long one and there are a lot of useful materials. Has anyone read the whole article of Yau's??

21. **Passby**
February 23, 2006

quite funny…

A Variety of Links

February 23, 2006
Categories: Uncategorized

The new Center for the Topology and Quantization of Moduli Spaces (CTQM) at Aarhus University wins my award for the most specialized pure mathematics institute. It will be hosting an opening symposium in a couple weeks featuring several talks that look interesting. If one is going to choose a specialized subject, this is an excellent one. Over the last couple decades the study of the moduli space of curves and of the moduli space of flat connections on a bundle over a surface has led to the discovery of many previously unsuspected relations between different parts of mathematics and between mathematics and physics. The fact that this subject is so fruitful remains somewhat of a mystery, and there is undoubtedly much more to be learned.

The National Academy of Sciences EPP 2010 committee should soon be producing a report with a 15 year plan for the future of high energy physics in the U.S. At the last meeting of the group last month Fermilab director Pier Oddone gave a presentation, focusing on opportunities in neutrino physics, strategy concerning the ILC, and the future of Fermilab.

Last week there was a conference on Particle Physics at the Verge of Discovery at Aspen. Lot of interesting talks on experimental particle physics. For an overview, see the summary talk by Paul Grannis.

The Templeton funded Foundational Questions Institute (FQXi) has announced that it will be publishing its inaugural request for proposals on Monday. This organization is led by Max Tegmark, who will be here at Columbia that day giving a physics department colloquium on From Derision Cosmology to Precision Cosmology. Unfortunately I have to be away that day and will miss the talk although I would have liked to attend it.

Steve Hsu, a physicist with a serious interest in economics, writes:

*You might think science is a weighing machine, with experiments determining which theories survive and which ones perish. Healthy sciences certainly are weighing machines, and the imminence of weighing forces honesty in the voting. However, in particle physics the timescale over which voting is superseded by weighing has become decades — the length of a person's entire scientific career. We will very likely (barring something amazing at the LHC, like the discovery of mini-black holes) have the first generation of string theorists retiring soon with absolutely no experimental tests of their *lifetime* of work. Nevertheless, some have been lavishly rewarded by the academic market for their contributions.*

Scott Aaronson describes his field of computational complexity theory as "quantitative theology", and goes on to note:

Incidentally, it's ironic that some people derisively refer to string theory as "recreational mathematical theology." String theory has to earn the status of mathematical theology — right now it's merely physics! A good place for string theorists to start their theological training is this recent paper by Denef and Douglas.

Lee Smolin is giving a course on background independent quantum theories of gravity at the Perimeter Institute, with the lectures available online.

David Corfield has an interesting posting on research programs in mathematics. It includes links to various things from Ronald Brown including a new paper on Ehresmann's work on groupoids, and his web-page on "Higher Dimensional Group Theory". Among other things worth reading at Brown's site is his account of the origins of Grothendieck's "Pursuing Stacks".

Corfield also points to an excellent list of problems in homotopy theory from Mark Hovey. Hovey starts off with the comment

The biggest problem, in my opinion, is to come up with a specific vision of where homotopy theory should go, analogous to the Weil conjectures in algebraic geometry or the Ravenel conjectures in our field in the late 70s. You can't win the Fields Medal without a Fields Medal-winning problem; Deligne would not be DELIGNE without the Weil conjectures and Mike Hopkins would not be MIKE HOPKINS without the Ravenel conjectures.

I first met Mike Hopkins at a conference in Guanajuato around 1990, and he made a big impression on me. One thing that most impressed me (besides his joking comment that he went into topology because it was a field full of hard-drinking and living guys who got into gun-fights (this last part was a reference to Dennis Sullivan)), was the mathematical ambition he demonstrated. He said that he had up till then made his reputation proving other people's conjectures, but now wanted to start making his own. Mike definitely followed through on this, since a sizable number of Hovey's problems are inspired by him. His talk on elliptic cohomology in Guanajuato was a revelation, and he has over the years continued to work in that area, coming up with dramatic new ways of thinking about the subject.

Update: There is an extensive discussion of the Smolin lectures at Christine Dantas's website.

Comments

1. **Kea**
   February 23, 2006

   Oh, Peter, thank you for the link to Brown's account of *Pursuing Stacks*. I have never before come across this part of the story.

2. **A.J.**
   February 23, 2006

   Hmm… I'm not sure that Hovey needs to worry that outsiders don't care about algebraic topology. It seems pretty clear right now that a lot of future work in algebraic geometry is going to use techniques from algebraic topology. Especially in the theory of moduli spaces. Just look at the way Madsen, Weiss (& Tillman) proved Mumford's conjecture. Or at Teleman & Woodward's proof of the Newstead-Ramanan conjectures. Or have a look at Lurie's thesis: He (and Toen & Vezzosi) are constructing an algebraic geometry where intersection theory always works; to do this, one must mix algebraic topology into the foundations of algebraic geometry.

   Anyways, I don't think the problem is that no one's interested. The problem is that modern algebraic topology (model categories, spectra, etc,..) is difficult to learn. I personally found scheme theory
   easier. The field is still waiting for a Hartshorne to come along and make everything accessible. (Peter May's book is a nice start, but it's doesn't go nearly far enough.)

3. **woit**
   February 24, 2006

   A.J.,

   I think there is a real problem with the perception of homotopy theory (I just spent part of dinner arguing with a colleague about this), and Hovey undoubtedly has encountered it in is career. Mike Hopkins has done a great deal to broaden the scope of homotopy theory and to bring it back into contact with some of the deepest parts of the rest of mathematics. But many mathematicians are not really aware of this work, and one of the main reasons is the one you point out. It's very difficult stuff to learn. A good expository text on the subject could do wonders.

4. **Christine Dantas**
   February 24, 2006

   Dear Peter Woit,

   I have set a space in my blog to discuss Smolin´s lectures in detail, "The Hand of a Master Series". Parts 1 and 2 of the lectures are already being discussed.

   http://christinedantas.blogspot.com/2006/02/hand-of-master-parts-1-and-2.html

   As I have put it, "*Feel free to send your questions, answers, comments, doubts, criticisms, ideas, disscussions and feelings on these lectures*". Students and experts are all invited to
   contribute.

   Thank you very much,
   Christine

5. **Adrian H.**
   February 24, 2006

   "…field full of hard-drinking and living guys who got into gun-fights (this last part was a reference to Dennis Sullivan)),…"

   I've said it before and I'll say it again, there are an awful lot of interesting tid-bits that are being alluded to but where the details are being witheld from us. Will someone fill in this gaping gap?

6. **Christine Dantas**
   February 24, 2006

   Mark Hovey writes in his page:

   > (…) to work on problems that arise externally to algebraic topology but for which the methods of algebraic topology may be helpful.

   Concurrency theory. See the GETCO workshops. E.g.,

   > Recently, ideas and notions from mainstream "geometric" topology and algebraic topology have entered the scene in Concurrency Theory and Distributed Systems Theory (some of them based on older ideas). They have been applied in particular to problems dealing with coordination of multi-processor and distributed systems. Among those are techniques borrowed from algebraic and geometric topology: Simplicial techniques have led to new theoretical bounds for coordination problems. Higher dimensional automata have been modeled as cubical complexes with a partial order reflecting the time flows, and their homotopy properties allow to reason about a system's global behaviour.

7. **woit**
   February 24, 2006

   Adrian,

   For the "hard-drinking and hard-living" details you'll have to ask someone else, but the reference to Sullivan is to the fact that he was shot in the shoulder by gunmen trying to steal his car in Brazil (he had pulled over to the side of the road to take a nap). OK, I don't think he was armed, so "gun-fight" is a bit of an exaggeration….

   Christine,

   Thanks for mentioning the material about the Smolin lectures on your web-site, I'll put in a link on the main page.

8. **Ronnie Brown**
   February 24, 2006

   I see a great motivation for homotopy is that it is related to classification. So I have been led to be interested in the abstract structures underlying homotopy, in order to understand and calculate.

   Also the aesthetic motive leads me to look for arguments which are easy and clear, because they follow a structure, and in order to make sure that I understand them. Not being as clever as many in the field, I need props and guidance, to explain to me, and I hope others also, why something is true, and also where it is going.

   I can't resist advertising here the new revised edition of my old topology book, to be available as `Topology and groupoids', in print and e-version, in a few weeks – see my web site. This is the first step towards a full exposition of `Nonabelian algebraic topology', a bigger job than expected!

   It is amazing that even just double groupoids seem very complicated! They reflect I presume transitions of transitions. This should be useful in ………??

9. **Christine Dantas**
   February 24, 2006

   Dear Peter Woit,

   I appreciate it, thank you!

   Best wishes,
   Christine

10. **Jeff**
    February 24, 2006

    Sir, I'm just an average guy with an infant-like understanding of all of these theories. I found your blog by searching wikipedia for String Theory after hearing someone mention it offhand. Wikipedia had a link to your blog as a critic of string theory. I read your article in American Scientist and I think I understand your criticism. Long

story short, almost everything on your website is over my head, but I was wondering if you could provide me with some resources… book names or articles that might help me to begin to understand what you and your colleagues are discussing.
I am a scholar by no means and will understand if this is below you or a waste of your time. Thank you for any help.

11. **woit**
    February 24, 2006

    Jeff,

    I’ve written a book, coming out this fall, which is intended to be something like what you are looking for. There are lots of books out there, many of which are pretty good, you just have to realize that most of the discussion of string theory is way over-hyped. About the best book about particle physics that I know and that I recommend to people is “The Second Creation” by Crease and Mann. There was a discussion over at Cosmic Variance about popular books, see

    http://cosmicvariance.com/2005/08/24/greatest-popular-science-book

    The two books by Abraham Pais that Clifford Johnson recommends are quite good also.

12. **Kea**
    February 24, 2006

    “It is amazing that even just double groupoids seem very complicated! They reflect I presume transitions of transitions. This should be useful in ………??”

    Some form of relational quantum mechanics! You said it!

13. **Adrian H.**
    February 24, 2006

    Thanks Peter

    The story sounds a bit similar to that of Gareth Evens, the Oxford philosopher of Language. He was shot in Brazil by a street kid, and then, tragically, died of the wound. Evans was quite young at the time.

    Pity —Brazil has a good tradition of serious science and maths, I guess, most notably, Nachbin.

14. **Jeff**
    February 25, 2006

    Thank you for your recommendation, I will have to see if I can find it at the library. I will look for your book when it comes out. Maybe after I read this book, I will stop by here and see if I can better understand what everyone is talking about. Thanks again.

Letter to ArXiv Advisory Board

February 23, 2006
Categories: Uncategorized

After more than three months of effort to try and get an answer about this, I've finally heard officially from the arXiv that trackbacks to my weblog are currently not being allowed by the moderators. I'm sending the following message protesting this to members of the arXiv advisory board.

To the arXiv advisory board:

I was informed two days ago by Jean Poland of the Cornell library that the arXiv moderators will not allow posting of any of the trackbacks to entries in my weblog that I requested more than three months ago. I would like to protest this decision and ask that it be overturned by the arXiv advisory board.

For background on the history of my weblog, my dealings with the arXiv moderators and the arXiv in general over this issue, you can consult the following web-page:

http://www.math.columbia.edu/~woit/arxiv-trackbacks.html

This is a complicated story, and involves a question not of the greatest importance, so you may quite reasonably not want to take the time to get involved in this, but I urge you to consider the two following issues:

1. It has taken me three months of effort to get a simple yes or no answer to the question of whether placement of these links on the arXiv will be allowed. This has wasted a great deal of my time, as well as that of those people who have been kind enough to try and help me get an answer. This is not a professional way of doing business and I urge you to ensure that it not continue to be the way that the arXiv operates.

2. The rejection of all trackback requests by me, requests that refer to postings of very different natures about both mathematics and physics make it clear that the moderators' policy is to not allow any trackbacks to my weblog. I have not been given any reasons for this policy, and can only guess what these reasons are. Given the history outlined in the web-page mentioned above, it seems clear to me that this censorship is primarily driven by the moderators' desire to paint as intellectually illegitimate and suppress commentary that is critical of string/M-theory research. This kind of suppression of dissent, accomplished using arguments that I have not been allowed to see or answer, is scientifically unethical and deserves to be condemned. The arXiv is an exceptionally important resource for the physics and mathematics community, and it is important that it operate according to high standards of scientific ethics.

Best wishes,

Peter Woit
Department of Mathematics
Columbia University
212-854-2642

Update: Sean Carroll's posting about this has finally shaken loose some indication of what argument was used to disallow links to my blog at the arXiv. For details, see the comment section of his posting.

Update: Lubos Motl has really outdone himself with his latest lunatic ranting about this blog. Note that, besides the blogs run by arXiv moderator Jacques Distler, Lubos's is one of only a couple particle theory related blogs that the arXiv moderators allow trackbacks to. That trackbacks to this blog are censored, but allowed to Lubos's (and almost no others not belonging to an arXiv moderator) should be more evidence than anyone needs that there is a serious problem with the arXiv moderation system, and it is due to the string fanaticism of the moderators.

Comments

1. **DSM**
   February 23, 2006

   First: I think the behaviour of the arXiv has been entirely unprofessional throughout this entire process, and I hope that things get quickly resolved in your favour. However, despite the bad blood between you and Lubos, which given what each of you has said about the other by this point is entirely understandable, I think that the following description is unfair:

   This last one [the Reference Frame] is a stew of right-wing, racist and sexist commentary, together with completely fanatical ranting against anyone skeptical about string theory.

   It is unquestionably right-wing, but unless we're to judge trackbacks by the politics of the authors, this is no more relevant than saying that his blog background is blue. I also don't believe his commentary is either racist or sexist, both of which — the former especially — are serious accusations to throw around (I suppose if we define sexism as believing that there are no differences between men and women relevant to their observed behaviours, he's certainly sexist.) One could with equal (in)justice describe you as an antireligious bigot through aggressive and determined misreading.

   He certainly does rant quasi-fanatically against those sceptical of string theory, and I think his over-the-top approach turns a lot of people off, even those who may substantively agree with him on most issues. This may actually be relevant: certainly your tone is usually far more civil than anything Lubos says, so clearly the arXiv can't argue that it's anything but the content of your posts that bothers them.

   I'd stick to the high ground here, which but for such comments I believe is unquestionably yours, and avoid needless detours.

2. **woit**
   February 23, 2006

   DSM,

   You're right, especially about the racism and sexism part, which has nothing to do with what is at issue here. I've edited that page to remove those references.

   You're also right that the fact that Lubos's politics are right-wing is not relevant, however the sort of mindless fanaticism evident in his politics seems to me all of a piece with the mindless fanaticism about string theory. That his political fanaticism is right rather than left wing isn't relevant, that he's a fanatic is.

3. **Michael**

February 24, 2006

Peter, I'm not sure if you're looking for an answer or just trying to pick a fight. If you want an answer, I can help you out: We don't need anyone commenting on string theory papers who hasn't done any research in the area, has been academically dead since the 80s, and invents things such as the off-diagonal SU(2). It simply isn't useful to anyone. Of course, you're free to comment anyway, but it shouldn't come as a surprise that the links to your comments won't be posted on the arxiv. Look, if you want trackbacks, cut a deal with Quantoken.

If you're just picking a fight, well, you have noticed it already: this is going to be one slow and agonizing fight. Certainly for you. Either way, there's no point to your complaining. None. At all.

4. **D R Lunsford**
February 24, 2006

Michael, you are such an ass.

Peter, good luck.

-drl

5. **robert**
February 24, 2006

Not so much ass as asshole; Michael's bitching is so predictable and unproductive, and quite like the reported behavior of ArXiv. There are serious issues at stake here, that have nothing to do with the sniping and sneering that now seems characteristic of forums in which stringy stuff is discussed.

6. **a**
February 24, 2006

It seems that arXiv needs to improve the management of trackbacks. If somebody tried some censorship, at this stage he/she will have realized that it was a stupid idea. People like Michael are doing another similar error: this blog gained a wide audience because Peter has a good point, not thanks to Peter's academical history. Attacking his career will not solve the problem that Peter had the courage to point out.

7. **Chris Oakley**
February 24, 2006

With the attention that this web log gets it seems to me that the ArXiv should be grateful for links from here to there rather than anyone worrying about links going the other way. They should at least take account of the fact that the attacks here are against the ideas and not the people. Not directly, anyway. The same cannot be said of certain physics-politics-ecology-eugenics web logs.

BTW, while we are on the subject of ArXiv – here is a note for Nigel Cook, who I know reads this and whose e-mail does not seem to work:
I have never sent a paper to ArXiv, and so I have no idea whether I am on their blacklist. I have not been attached to a university since 1987. This would be a problem if I did want to submit something. Also problematic is the fact that I think it very, very unlikely that universe (multiverse?) consists of tiny vibrating strings in 10 or 11 dimensions. Not to mention the fact that all the QFT work I have done is in 4 dimensions (3 space, 1 time). Or the fact that I can calculate cross sections which agree with experiment.

8. **nigel**
February 24, 2006

http://arxiv.org/help/endorsement –

'We don't expect you to read the paper in detail, or verify that the work is correct, but you should check that the paper is appropriate for the subject area. You should not endorse the author … if the work is entirely disconnected with current [string theory] work in the area.'

They don't want any really strong evidence of dissent. This filtering means that the arxiv reflects pro-mainstream bias. It sends out a powerful warning message that if you want to be a scientist, don't heckle the mainstream or your work will be deleted.

In 2002 I failed to get a single brief paper about a crazy-looking yet predictive model on to arxiv via my university affiliation (there was no other endorsement needed at that time). In emailed correspondence they told me to go get my own internet site if I wasn't contributing to mainstream [stringy] ideas.

9. **Christine Dantas**
February 24, 2006

Trackbacks open a gate to disputed ideas. The final censorship is always on the hands of the blog´s owner of course. Moderating trackbacks sounds funny. It seems more logic for the arxiv to create their own blog and remove trackbacks from the site.

Good luck.

Christine

10. **axolotl**
February 24, 2006

Well, the whole trackback thing has been a bit of a flop. If they allowed trackbacks from here, then this blog and LM's would dominate! And the administrators probably think that LM is doing enough damage already!

11. **Alejandro RIvero**
February 24, 2006

What is a pity is that this politic of silence is creating a class of "Kremlinnologists" similar to the cold war politic analysts, trying to guess the ArXiv intentions.

I only payed a small visit to the USA the last century, to New York and Austin. My overall impresion was that payed staff (in generic shops, buses &c.) was not motivated to innovate but to follow orders, very impressive for a country where compulsory military service does not exist.

Recently we have noticed an increasing of acceptation in preprint submissions; lets hope that the same will happen with trackbacks. After all, there are a lot less invasive

(they are even listed in a separate window, not in the main abstract presentation)

12. **MRA**
February 24, 2006

    Peter,
    Could you post Jean Poland's entire message?

13. **woit**
February 24, 2006

    MRA,

    I specifically asked Poland whether it was all right to post her e-mail to me, and she asked me not to, so I'll respect her wishes. To summarize her e-mail, it included apologies for how long it had taken to get a response to me, a description of the trackback system as experimental and with policies still under development, and the phrase I quoted about the decision of the moderators.

14. **Sean**
February 24, 2006

    I'm sorry to hear this, Peter. There's obviously no good reason to prevent you from leaving trackbacks, no matter what your opinions about string theory may be. It's disappointing.

15. **Dumb Biologist**
February 24, 2006

    My understanding is the existence of the arXiv system is predicated to some degree on the notion that this method of communicating physics research and other related discourse has supplanted, and is in any rate superior to, the conventional methods of peer review and print media, which presently exist only to siphon grant money away from science to publishers.

    That may be true, and there is no question that, in other fields, even paragons like Science have been deeply wounded by the creakiness of the conventional peer review system. Meanwhile, there's no consensus that it should be completely abandoned, as some standards of authority are highly desireable, if they can be had practically. To that end, greater openness and scrutiny of the review process itself has been suggested, and it appears some journals are moving in that direction.

    Even without any alteration, and with the complete anonymity most referees currently enjoy, they are obliged under the standards of peer review to cogently communicate their reasons for acceptance or rejection of scientific communication. To do otherwise would indeed be completely unethical, and would never stand. I do not think that anyone, in the wake of several recent scandals involving scientific misconduct, is suggesting the system become more closed and inscrutible.

    So, from the perspective of an outsider to physics (but an insider to publishing and reviewing scientific communications), it does seem disturbing that the *de facto* referees of what is apparently the most relevant and prevalent means of communication of physics discourse feel no need whatsoever to explain their actions. I don't want to rush to judgement, as they may have very legitimate practical reasons. They may be swamped with a bombardment of crackpottery, and to explictly reject even the most outlandish communication as such might open a legal can of worms, on top of being a full-time burden.

    It does not appear to my non-expert self, however, that Dr. Woit is a crank. At worst, he's no more of a crank than a fair number of his colleagues, it seems. One need only read the bitter disputes recorded on weblogs between some Stringy and Loopy investigators to see the contempt some professional physicists openly display for one another, and it does not appear that those communications are being supressed by trackback blacklisting. What are the standards being applied here? How is the appearance of impropriety (albeit even to the ignorant) to be avoided? It's not at all clear to me, in this instance, why some justification should not be provided. I'm a taxpayer. Just as the increasingly obvious problems with the standard system of peer review of publically funded science concern me deeply, so also, if the arXiv is a publically-funded resource, does its standards concern me. I think such public curiosity is reason enough to simply provide an explanation.

16. **Chris Oakley**
February 24, 2006

    Dumb (?) Biologist,

    I don't know the answers to all your questions, but I feel I should point out that ArXiv is not a replacement for print journals, it's a replacement for the preprint system. With the latter one would send copies of one's paper to 200+ institutions worldwide at the same time as one submitted it to a journal. It meant that people did not have to wait months to see what you were up to. SLAC keep scans of every preprint they received, a large number of which never ended up in any journal. A side effect of this system was that after the initial mailing one would get a swathe of preprint requests from universities not on the mailing list (I seem to remember getting many from Eastern Europe and Russia).

17. **Dumb Biologist**
February 24, 2006

    OK. My line of questioning was based, to considerable degree, on information provided here:

    http://people.ccmr.cornell.edu/~ginsparg/blurb/

    Especially:

    "It is ordinarily claimed that journals play two intellectual roles: a) to communicate research information, and b) to validate this information for the purpose of job and grant allocation.

    As I've explained, the role of journals as communicators of information has long since been supplanted in certain fields of physics, so let's consider their other role. Having queried a number of colleagues concerning the criteria they use in evaluating job applicants and grant proposals, it turns out that the otherwise unqualified number of published papers is too coarse a criterion and plays essentially no role. Researchers are typically familiar with the research in their own field, and must in any event independently evaluate it together with letters of recommendation from trusted sources. Recent activity levels of candidates were mentioned as a criterion, but that too is independent of publication per se: "hot preprints" on a CV can be as important as any publication.

    So many of us have long been aware that certain physics journals currently play NO role whatsoever for physicists. Their primary role seems to be to provide a revenue stream to publishers, a revenue stream invisibly siphoned from overhead on research contracts through library systems."

    (I hope I'm not violating any fair use standards by quoting the above from this source: http://people.ccmr.cornell.edu/~ginsparg/blurb/pg96unesco.html)

The explicit suggestion is that resources like the arXiv are not just expeditious and inexpensive vehicles for distributing preprints, but that they have rendered at least some physics journals obsolete. Of course my reading may overstate the arXiv's importance, but it does appear that it has indeed assumed a preeminent and independent role as a means of communicating original physics and mathematics literature. Again, I may be reading too much into this, and I don't want to be mistaken in my understanding.

18. **woit**
February 24, 2006

Sean (and others),

Thanks for your supportive comments.

DB,

There are lots of interesting issues about the role of the arXiv as it increasingly replaces the historical role of peer-reviewed journal. But one overriding fact is that it has become extremely important for the math and physics communities, and the people managing it have an increasingly important responsibility.

The arXiv does need to protect itself against crackpots, and I can see justification for a certain amount of lack of transparency at times because of this. I gather that they haven't yet figured out a permanent policy for dealing with trackbacks, but I don't see that an elaborate peer-review process makes a lot of sense here. They just need a moderation mechanism to thwart crackpots, and the problem here seems to be that their moderator(s) is convinced that anyone critical of string theory is a crackpot. The kind of string fanaticism exemplified by people like Motl, Distler and Kuperberg is a minority behavior even among string theorists. My experience has been that most string theorists and I can have a perfectly reasonable discussion of these issues, even though we may strongly disagree about some of them. But those in the string theory community who are fanatics shouldn't be in the position of making moderation decisions at a crucial institution like the arXiv.

19. **Dumb Biologist**
February 24, 2006

I wish I understood it all better, so that I had more to add now than my hopes for a fair hearing of your grievances. Best of luck!

20. **MathPhys**
February 25, 2006

Michael,

Why do you (and Motl) connect and write to this blog? Just to tell Peter Woit how much you despise him? It is clear that he hit a very raw nerve.

I find your behaviour disgraceful. Are you people really scientists?

21. **Quantoken**
February 25, 2006

Peter:

What's the point writting a letter to ARXIV? They already said they are not interested in your opinion. Predictable it will take another 3 months before you see any response and the only response you will get is that they ignore you.

And it is ridiculous for you to defend ARXIV for having to protect themselves against crackpots. They welcome the biggest crackpot of all, the super string theory. I know that till this day you are not willing to consider super string theory as a crackpot, and you still want to consider it as a science. The point is any theory that fails to make a useful prediction is considered crackpot. It doesn't matter that string theorists are honestly making the effort to try to come up with a prediction. That is simply not good enough to differentiate their theory from crackpot. All crackpot theorists DO honestly hope for a useful prediction.

Until string theorists can show that they can make meaningful predictions, and that their prediction can be verified by experiments, I think it is fair and square that super string theory be classified as a crackpot theory. ARXIV therefore is a major crackpot depository.

You might as well instead write to New York Times, or any of the public media.

Quantoken

22. **Haelfix**
February 25, 2006

There is absolutely no good reason Arxiv should ban blogs that are run by faculty at major research institutions. I might disagree with comments here on occassion, but there have been several very enlightening discussions here in the past.

I do however see how it can get a little annoying responding to the same trackback subjects (eg how bad the landscape is for physics) over and over again. It gets a little tiring at times when its the same criticism at every new paper. I think pretty much the entire physics community, as well as even the general public is now aware of these objections, it has been the point of many coffee table talks and its thoroughly known.

I am otoh perfectly open to reading healthy and hopefully new objections, and indeed they do show up on occassion.

23. **Juan R.**
February 25, 2006

There is a broad dilated experience of ArXiv administrators to eliminate any dissident from being heard by the rest of comunity.

It is possible that Quantoken have full reason in that

"What's the point writting a letter to ARXIV? They already said they are not interested in your opinion. Predictable it will take another 3 months before you see any response and the only response you will get is that they ignore you."

In fact, ArXiv administrators are ignoring to blacklisted scientists since many years. They even do not response to blacklisted Nobel laureates, even when they write a formal letter to Nature journal. Here an excerpt:

"The cases documented by myself and others show that there is more to the story."

"The exclusion of particular individuals and particular ideas from arXiv appears to me to be deliberate."

"For example, having stated that a very distinguished physicist's strong support of a submission carried no weight because this physicist "was not intimately familiar with the work in question", the moderators simply ignored subsequent support from an endorser with publications on the same subject.

"The moderators' attitude to any challenge to conventional thinking is likely to result in the loss to science of important innovative ideas."

Has Robertson letter changed ArXiv "moderation" process?

—

Juan R.

Center for CANONICAL |SCIENCE)

24. **Chris Oakley**
February 25, 2006

One possible solution to the "standards" problem in the ArXiv would be to have an area containing papers submitted but not endorsed by the advisory board, preferably with comments explaining why. If anyone finds any of this "crackpottery" offensive, then they could just avoid visiting this part of the site.

25. **Juan R.**
February 25, 2006

Peter Woit

I recomend you the preparation of an open letter was signed by respected scientists for a better (at least open to external check) administration of the ArXiv.

This is the only way i can see you can obtain some success. Best desires!

—

Juan R.

Center for CANONICAL |SCIENCE)

26. **Benjamin**
February 25, 2006

Peter,

I'm neither a physicist nor a mathematician, so I doubt my comments will be worth much to you, but please let me say this. Of course, you deserve the trackbacks from arXiv, but I object to you calling Lubos Motl's blog a 'stew' of 'fanatical' politics. I am a political moderate who can be liberal on some issues and conservative on others. I have no axe to grind. Motl's conservative political views are based on his experience of Communism, and they are not without a great deal of merit. Furthermore, he expresses himself colorfully, which may go over the top sometimes, but is entertaining and thought-provoking. There's nothing 'fanatical' about his views. I think this bit of excess on your part in your otherwise cogent background article that you linked to here does your own worthwhile cause a disservice. However, it is only a slight misstep. As for the validity of superstrings, I too have my doubts, but my opinion is worthless, so I can only say: 'Let time reveal the truth!'

27. **woit**
February 25, 2006

Benjamin,

I don't want to get into a discussion of Lubos Motl's politics, but there is a reason I needed to bring up the issue of his fanaticism and how it is expressed on his weblog. The arXiv will not inform me of the reason it is censoring links to my weblog, so all I can do is try and guess what the reason might be and see if it fits with the evidence given by looking at the other weblogs they do allow links to. Although I would challenge this, I can see how someone might accuse me of at times being insufficiently polite to certain people, or holding fanatically one-sided views. The fact that links to Lubos's site are allowed indicates that this can't possibly be the reason.

While my own political views aren't those of Lubos, my father and his family were Eastern European refugees from Soviet communism and I grew up understanding well their point of view and respecting it. None of them ever behaved in the slightest like Lubos; they were civilized people and capable of treating views they disagreed with with respect. Lubos is a fanatic and an extremist, both in his political and scientific views: you always know exactly what he is going to say about any issue, he goes on and on about how anyone who disagrees with him about politics or string theory is an incompetent fool, and does whatever he can to suppress any such disagreement.

One could argue that fanaticism of this kind is harmless, but the problem is that the moderators of the arXiv, while lacking Lubos's political fanaticism share his scientific fanaticism. This form of fanaticism has done a huge amount of damage by now to theoretical physics, and promises to do much more in coming years unless people take a stand against it.

28. **Benni**
February 25, 2006

Dear Peter,
I think you are banned, because you do not scientific work. You only criticise stringtheorie in a rather "public" manner.
When you have real arguments against stringtheorie(for example if it fails to reproduce an observed physical effect, nonlocal quantum effects could be such cases) then write it in mathematical form to a paper and publish it.
I think you had to wait for an answer because there were discussions on the moderator board.
And they might have decided, that someone, who only wants the general public to inform with short comments that stringhtheorie is not as goog as it is, has nothing to do with a scientific discussion, but only with a person producing himself.
It seems that the Arxiv wants to be a board for scientists, and not for discussions addressed for the general public.
Everyone in stringhteory knows about the landscape, and there is no need, to tell a scientist on this problems. So Arxiv blacklisted you.
Even if I have nothing to do with the moderator board, I think if you write a number of serious papers (maybe 10-20) with your own Ideas, publish them in journals and post them to the arxiv, and make your blog more of scientifically interest (stop stupid mentioning of the same problems everybody knows, post original ideas, come up with new! criticisms on strings which no one thought before), your situation would be much better.
At this time for now, you can only be seen as a man who doesn't produce own ideas, but only himself for the general public with repeated criticism of a theory which is in a crisis (what everyonein stringtheorie knows).

It might be that what you do is very good for students and newbies, who want to start their carrier in theoretical physics. And I think you know, you do it exactly for them. But for science it is nothing, perpetually mentioning that a speculative theory that has 10^500 solutions instead of one. And Arxiv is for science, not for students or newbies.

29. **Patrick**
February 25, 2006

I've been lurking here for a while but never posted before. This is slightly off topic, but I'm in the same boat with Dumb Biologist in that, in my own field (biophysics), we don't have anything comparable to ArXiv. I'm a bit curious about the role it seems to be assuming in the physics community. My question is: to those of you who have been involved in hiring/promotion decisions, do papers on ArXiv carry any weight or are they completely discounted as not "real" until they appear in a peer reviewed journal?

Just wondering.

PLW

30. **woit**
February 25, 2006

Benni,

Well, I actually have published 9 papers, one in a conference proceedings, the other 8 in Phys. Rev. Lett., Nucl. Phys. B. and Phys. Lett. B. Actually it would be 10-20 if you count hep-th/0206135. I haven't submitted that to a journal, since at this point I don't much see the point of journals. If the arXiv moderators want to set a standard of 10-20 peer-reviewed publications by a blogger before allowing links to their blog, they should say so. I don't think a few more peer-reviewed publications on my part would make any difference.

Your characterization of my blog is quite inaccurate. There are postings on a wide variety of topics of scientific interest and many physicists and mathematicians, including quite a few string theorists clearly find them worth reading. The evidence for this comes both from what people have told me and from the large number of daily connections to the blog, very many of them from leading research institutions.

Yes, I do cover in detail the continuing controversy over the landscape, I think it is an important story. The moderators may agree with you that my emphasis on it is unwarranted, and that would justify their not allowing a trackback to my posting about Weinberg's article. It doesn't justify their decision to censor any and all links to my blog, including the ones about John Baez's paper and about the Freed-Hopkins-Teleman theorem.

31. **woit**
February 25, 2006

Benni,

One other thing. You write:

"When you have real arguments against stringtheorie(for example if it fails to reproduce an observed physical effect, nonlocal quantum effects could be such cases) then write it in mathematical form to a paper and publish it."

This misses the whole point about the problem with string theory: it's not a theory that is "wrong" in a conventional scientific way. It's "not even wrong", i.e. you can't derive an inconsistency or an incorrect physical prediction from it since it isn't really a theory. What you are asking for is impossible, but in this case it very much is legitimate science to give arguments that string theory (as a theory of unification…) can't ever possibly predict anything. Much of the criticism of string theory here consists of a detailed argument explaining why various claims that "string theory predicts X" are incorrect. I suppose I could write such things up in more detail, providing equations, and even get them published in some peer-reviewed journals. I don't think there would be much point to that, and I don't think it would change the views of the arXiv moderators.

32. **A. nonymous**
February 25, 2006

Patrick,

There's a section of the arXiv for biophysics. You'll see papers there by leading researchers in the field, so most likely they read the other papers that appear there too. That means that if you put a paper there in the right section, a reasonable fraction of your peers will see it and it will affect their opinion of your competence, either in a good or a bad way depending on how good your paper is. Also, they might follow up on it if it strikes them as interesting. So it's effectively published.

It's still a kind of a grey area, though, because it's not the done thing in biology to cite preprints in journal-published papers, so unless you publish in a journal as well, you won't get any citations. Whether you can disown a paper that you've put on the arXiv later if you change your mind is a different question. You can withdraw papers, but the attitude of the arXiv administration is that if you thought in advance that there was a possibility that you would later withdraw a paper, then you shouldn't have posted it in the first place.

33. **Pudding**
February 25, 2006

"This misses the whole point about the problem with string theory: it's not a theory that is "wrong" in a conventional scientific way. It's "not even wrong", i.e. you can't derive an inconsistency or an incorrect physical prediction from it since it isn't really a theory."

Which is why string theory, not Petr Woit should be banned from the arxiv.

34. **woit**
February 25, 2006

Patrick,

What I've seen of hiring decisions in recent years has been in math, not in physics, where things are kind of different. For mathematicians, the peer-review process is supposed to determine not only whether a result is of sufficient interest, but whether the author has a valid proof. Mathematicians take this very seriously, and so the peer-review process is of greater importance to them than to physicists. The system is under stress though, as it has become harder and harder to find good people willing to put in the amount of time necessary to carefully check a proof. You hear a lot of complaints among mathematicians that the peer-reviewed literature is much less reliable now than it used to be.

To get hired or promoted, in most cases peer-reviewed publications in leading journals count heavily. This is not always the case: virtually any department in the country would be happy to hire Grigori Perelman based on his arXiv submissions which just give an outline of a proof of the Poincare conjecture and which he shows no signs of wanting to submit to a journal for peer-review.

In physics, it is becoming increasingly hard to understand why the journals survive. No one I know of reads the journals, everybody just looks at the arXiv. The peer-review process has seriously broken down, for evidence of this consider the case of the Bogdanov brothers. They were able to get several papers consisting of complete nonsense published in a variety of peer-reviewed physics journals, including two rather well-respected ones. I'd also be interested to hear from people involved these days in physics hiring/promotion decisions about how the whole journal issue is viewed.

35. **Benni**
February 25, 2006

Peter woit wrote
This misses the whole point about the problem with string theory: it's not a theory that is "wrong" in a conventional scientific way. It's "not even wrong", i.e. you can't derive an inconsistency or an incorrect physical prediction from it

No.
For example, string theory is formulated as a local field theory. But quantum mechanics is essentially nonlocal.

In ordinary field theory, it is physically possible to restrict the behaviour to locality, since in particle physics one does not work with experiments dealing with entanglement.

Entanglement is a macroscopic quantum effect. Even there exists the quantum eraser experiment (here's an example)
http://grad.physics.sunysb.edu/~amarch/
which deals with entanglement at various time ans space points. It seems that that quantum mechanics is a nonlocal system over space time.

String theory wants to deal with macroscopic objects like black holes and universes.
I have seen many entangled quantum systems.

But never an entangled string!

It is simply false, when stringtheorists say, all they have to do is to get the standard model.

I remember, that I have asked this John Schwarz personally in Munich after a lecture two monts ago.
Schwarz looked at the ground, weaved with his arms, uttering some words, and then he rotated to the table that the audience could not see his face.
He said: In stringtheory we assume everything local.

I would be glad, when someone points me to a paper how he wants to implement nonlocal behaviour in a theory which assumes that all necessary about a particle is a very small quantised string.

To implement correlations, he must, I think, set up a kind of hidden variable theory, or make the string as big as the universe.

If I'm not wrong, of course.

36. **woit**
February 25, 2006

Benni,

You are very confused about these issues. And this is now completely off-topic, no more along these lines please.

37. **Benni**
February 25, 2006

OK, what I mean is simply:

Is it not possible, to give a proof that string theory fails to reproduce entanglement?

38. **J.F. Moore**
February 25, 2006

"No one I know of reads the journals, everybody just looks at the arXiv."

This is interesting, because I would say exactly the opposite. I've worked in solid-state physics, materials science, and chemistry groups (all experimental) and not once has the arXiv come up. I've heard of it, but I have never personally looked up a paper there for my own research. I've only followed links from places like this blog just for curiosity. For research, a few times people trade preprints, but it isn't all that common. When an exciting new paper comes out in a journal, then we discuss it. It seems to be a very different culture.

I plan to do some informal polling, concentrating on the physicists. I suspect a lot of them will have no idea about the arXiv or will simply not care. My own feeling is that, frustrations with peer review aside, I don't want to waste my time reading junk. Not that I'm saying the arXiv is full of junk; perhaps 'small lab' experimenters just generate more junk than mathematicians and HEP types so it's more of an issue. Maybe it's just a different local minimum to solving the problem of communication in our areas of study. I don't know.

39. **woit**
February 25, 2006

Benni,

No, this is not possible. If there there are completely consistent versions of string theory (which is plausible, but remains to be shown), at low energies they should just reproduce the same behavior as in the standard quantum mechanics, including entanglement.

One reason people are so interested in string theory is that it seems that at low energies it gives you standard quantum theories that we know and love and are well tested, but at high energies is supposed to give you something different.

40. **woit**
February 25, 2006

J.F. Moore,

It's interesting to hear how different things are outside of particle physics. The HEP theory preprint and arXiv literature has always had a lot of junk, with the attitude being that part of becoming a serious researcher is learning to quickly tell junk from non-junk. It has also often been very faddish, with people trying to jump on the latest and hottest idea. If you ignored preprints and waited for publications, you'd be out of the game.

41. **J.F. Moore**
February 25, 2006

I see. There is certainly fashion in my area, but I think the timescales are much longer. Actually, it can be maddening how long it takes an 'obvious' idea to become accepted by the mainstream.

I should add to what I said above that theorists of my acquaintance in SS/condensed matter/chemical physics seemed to use preprints and the arXiv a fair bit. What you say makes me want to ask them about the junk factor in their fields.

42. **Patrick**
February 25, 2006

From briefly looking around the biophysics and statistical physics areas of ArXiv, there seems to be a pretty strong theory/experiment split when it comes to posting papers. I'm mostly an experimentalist and I'd never heard of ArXiv until I learned about it through physics websites like this one. In the biophysics section there are papers by some pretty big names in the theory and simulations area, but little to nothing by experimentalists. This might simply reflect the fact that the biophysics theorists are likely to have interacted with other theoretical physicists in graduate school and thus be aware of ArXiv in the first place.

43. **Benni**
February 25, 2006

Of course it is possible to attac string theory.

You mention in your own essay, that "a simple argument gives rise to a high cosmological constant". Roger Penrose attacked stringtheory recently in his new book with singularity theorems, and string theorists theirselves put their theory into crisis with mentioning it has 10^500 solutions.

Also, there seem to be indeed problems with nonlocality. Even in ordinary QFT. A workaround seems to be the well known Schroedinger picture, as H.D.Zeh from Heidelberg writes: http://www.rzuser.uni-heidelberg.de/~as3/nonlocality.html

But there are other Problems:
In this (german) manuscript Prof. Dragon at Hannover (he works on relativity and supergravity, some of his colleagues in his department work on string theory)
http://www.itp.uni-hannover.de/~dragon/stonehenge/qm.pdf
writes at page: 100:
"since string theories contain relation 9.34, they are, in opinion of the author, physically untenable"
And on the following lines, and the page before he gives fair grounds for his opinion. And states: "This serious problem is ignored by string-theorists"…..
So, of course it is possible to attack this theory in a scientific way.

But I see no papers in which you are involved, Peter, that raise any questions. That might be the reason why arxiv has blacklisted you.

44. **Alejandro RIvero**
February 25, 2006

I note some catch-22 situation here in some commenters. The owner of this blog does not seem that string theory is a topic worthy to work in, but in order to critiquise it he is asked to give a record of publications on string theory.

45. **Benni**
February 25, 2006

No. What is wanted are scientific publications which criticise string theory.
Penrose was able to for example.
String theorists themselves found, that their theory has 10^500 solutions.

What Peter does is: He waits till string theorists find problematic things and then he gives short comments. The sucess of this blog is based on papers of others. Everyone could do that -waiting till others struggle and then laugh at them.

Peter hasn't found that it is an NP hard problem to find the correct solution.
Peter hasn't found that String theory has 10^500 solutions.
Peter hasn't found that assumed the additonal dimensions were classical, they would collaps (as Penrose writes).
He even has not found the problem which Prof. Dragon raised (which is indeed the most simple imaginable).

All this are scientific things, a real scientist, which criticises stringheorie could have written on his own. A mathematician, he should know that there exists no theory, which is not worth to spend time. It es even physically valuable to show problems of a theory, which put it into question.

But either Peter does not want to produce scientific work or he is not able to.
So there's no wonder, that arxiv which wants to be for science exchange blacklists him.

46. **Geon Oh**
February 25, 2006

Benni is making a very fair comment which I've been trying to say since long ago. I'm not a string theorist nor non-string theorist (just an undergraduate student at the moment) but I must admit, I'm kind of sick of hearing that 'Peter Woit is brave enough to point out string theory is not even wrong!' It's not about being brave, it's an ignorance to decide 'not' to study string theory and yet trying to raise a voice about it. My mentor always told me 'Save your best thoughts, and devise ways to test them', I can't believe how Peter Woit, his history tells me he is a very qualified physicist, yet he is wasing time on posting articles like these and finding links so that 'general public' can read them. Please stop all the nonsense about Witten being the axis of evil who influence people to take up string theory, at least he doesn't waste time arguing 'why am I blacklisted on Arxiv!!', he's smart enough to realise he should rather spend time 'try things out' rather than 'sit and cry'.

47. **Geon Oh**
February 25, 2006

Sorry if I sound upset… I just feel strongly about this.. 😀

48. **Peter**
February 25, 2006

Geon,

If you think I've ever claimed that "Witten is the axis of all evil" you haven't read or understood much of what I've written here. I'd suggest learning a lot more about this subject before criticizing anyone else for their views about it.

Benni,

I don't know who you are, perhaps also an undergraduate, but you don't understand what you are talking about.

There seems to be something about string theory that encourages people who have learned a little bit about it, but don't actually understand anything, to spend their time attacking people who actually do know what they are talking about.

Both of you, please stop posting here until you actually understand what you are writing about. If people who actually understand string theory want to argue with my views about it, I'm glad to do so, but it's absurd to be spending my time answering attacks from undergraduates who don't know understand what is going on here.

49. **Richard**
February 25, 2006

Alejandro has an excellent point:

> I note some catch-22 situation here in some commenters. The owner of this blog does not seem that string theory is a topic worthy to work in, but in order to critiquise it he is asked to give a record of publications on string theory.

There are certain musical forms (i.e., atonal or 12 tone classical, rap, or fill in your own _____) which I feel perfectly qualified to criticize even though I'm neither a musician or a musicologist. I do know enough about music that I feel perfectly qualified to assert that something smells bad and doesn't advance musical culture at all. Can you imagine being attacked by a musician and told that your opinion is invalid because you yourself aren't a practitioner? I'm seeing a lot of this sort of blind us-versus-them elitism in some of these posts. I'm not a physicist, and frankly don't have a strong opinion about string theory, but Peter's criticism of string theory seems remarkably restrained compared to the rantings, attacks, and wild assertions by Motl and others.

> Peter hasn't found that String theory has 10^500 solutions.

This is a criticism of Peter? Are we supposed to jump for joy because there are 10^500 solutions? And solutions to what? Vacuum states? Some of these comments are so inarticulate that I smell real crackpots, in this case, pro string theory crackpots, and why isn't Motl wading in here and wielding his usual machete to these people?

Happily going back to doing math tonight where the tone of politics and discussion is a bit more civilized …

50. **Mar**
February 25, 2006

Hi Benni. It is called "string theory" 😉 . And no, there is no apparent reason why it shouldn't be self-consistent.

Geon Oh: I think you're completely off the mark here. As far as I undestand, Peter (like many, many scientists including, but not exclusivelly, Lubos) is quite a fan of Witten.

Both:

You completely miss the point, which was about trackbacks from the ArXiV. These should be allowed no matter if the blogger in question is a critic or not of string theory, tokamak approaches to fusion or anything else. While it's understandable that the ArXiV should have some sort of "crackpot filter", I think it's plain nonsense that, for example, a trackaback to a paper by John Baez is banned while the author himself comments in this very blog.

51. **Rien**
February 26, 2006

Regarding the arxiv again, I'm a particle theorist and of course I submit all my papers to journals as well. I am a little suspicious of papers more than a year old that have not been properly published, even though the arxiv has been there during my whole career.

I'm becoming somewhat disillusioned by the peer review process though, but this comes more from being a referee. Several times now, I have strongly recommended against publishing papers, only to find that those papers have been published anyway in another journal (and once even in the same journal). It seems impossible to argue against publishing slight papers with a small or negligible amount of new stuff. There are simply too many papers being put out, in my view. Of course you can always say that you never know what is going to be the next thing, but sometimes it's pretty clear that a paper will not lead to anything, when it's just the umpteent small variation of something.

On the other hand I've also had a referee finding an error in one of my own papers, so it is not altogether bad…

52. **Thomas Larsson**
February 26, 2006

Benni and others,

While string theory makes no hard, falsifiable predictions, it makes many soft predictions: supersymmetry, extra dimensions, 496 gauge bosons, proton decay, new long-range forces, a non-positive CC, etc. The most striking thing about these soft predictions is that **every single one** is in apparent disagreement with experiment.

The situation with string theory is similar to that of Ptolemy's epicycle theory. An ellipse, or any closed curve for that matter, can presumably be expressed as circles around circles around circles ad infinitum, just as any periodic function can be expanded in a Fourier series. So epicycle theory is not even wrong, but the fact that its soft predictions are in apparent disagreement with experiments is nevertheless a problem for it.

If you are not impressed by Peter's qualifications, why don't you have a look at what the founder of the string theory group at Rutgers has to say about his former field, see subsection 1.6 of hep-th/0204131.

53. **Swatters**
February 26, 2006

Benni seems more like a graduate student than an undergrad.

I think I can sum up Benni's opinion: If you aren't an approved of and published scientist then the opinion generated by your primitive little mind is pointless. You are free, free to think whatever the "real scientist" tell you to think. Just shut up and sign the check.

54. **Marty Tysanner**
February 26, 2006

I get the impression that people like Benni and Geon Oh are confused about the real issue at hand. They justify the actions of certain moderators of the ArXiv to censor blog trackbacks to Not Even Wrong because Peter is criticizing string theory without actively producing papers (especally string theory papers) of his own. Geon Oh goes even farther, and states that Peter is ignorant of string theory because he is not producing string papers. Evidently he is either unaware of or disbelieves Peter's repeated comments that he has spent a great deal of time studying string theory; he also ignores Peter's repeated reluctance to engage in discussions about technical topics that he does not feel fully qualified to discuss.

My own observations of Peter over the past several years (since he started posting on sci.physics.research, before the days of the "Not Even Wrong" blog) have been different. It was clear that he talked about a sensitive subject, judging from the lively discussions that ensued and the strong emotions he encountered from some string theorists (especially Lubos *"Superstring/M-theory is the language in which God wrote the world"* Motl…). However, I have noticed that Peter has always been clear about the reasons behind his statements so that anyone was free to challenge him on that basis. So if he were making an incorrect claim about string theory others were free to point out his error. Sometimes this happened — occasionally an expert would catch him making a misstatement. But interestingly, I noticed that rather than becoming defensive or making a personal attack on the person who disagreed with him, he would publicly acknowedge his mistake if he was wrong; if he didn't believe he was wrong, he would give a more technical explanation of his position, and then the discussion would continue. He also seemed to soften the way he presented his views over time; as he discused the views of *reasonable* string theorists who argued with him, he was more careful about saying what specific parts of string theory he disagreed with (especially the landscape), while openly acknowledging that there were other aspects of string theory that were worth studying and that deserved to be called science. For some time now, he has openly stated that reasonable people may disagree with his views about the merits about string theory.

At the same time, I have observed that the people with whom he publicly discusses his views usually fall into one of two categories. The largest group acts reasonable; they either try to engage in a discussion of the technical merits of what Peter says, or they may mention that, "Well, you've got to work on *something* unless you want to just give up, and right now string theory is the best there is even if it isn't perfect." The other group doesn't usually bother to argue from logic; these people seem satisfied with personal attacks on Peter, trying to cast him as a marginal character with no scientific standing who is trying to hurt the One True Fundamental Theory for personal reasons or because he is just ignorant. People like "Michael" (a.k.a. "Hmm," I assume) and other more well known characters come to mind… Rather than actually addressing the technical points Peter makes, these people employ ridicule, distortion of viewpoints or past accomplishments, strawman arguments, and the like.

It is very "interesting" to see certain relatively well known physicists and physicist wannabes resorting to emotional tactics rather than reason when it comes to defending their views. And now, using the curtain of anonymity, certain physicists are now even resorting to censorship tactics to make it harder for physicists and graduate students to see dissenting viewpoints when looking at a paper on the ArXiv. These moderators and people who use emotional methods to attack Peter seem willing to risk unintended consequences — that reasonable graduate students who could become the next generation of string theorists, as well as the public that is helping fund such research, might conclude that these string theorists don't believe that string theory is strong enough to stand up on its own merits against reasoned, dissenting views, and consequently the tactics of mud-slinging politicians must be employed to maintain string theory's favored position. Even worse, engaging in censorship for personal reasons and shrill personal attacks with little technical merit are offensive to both the scientific method and the spirit of free inquiry. Any resulting backlash against people who engage in those tactics is bound to cause damage to string theory by its association with them, even though the number of such people is relatively small. This is yet another example where intelligence and wisdom can be orthogonal…

For someone like me who is very interested in fundamental physics, the discussions Peter has had with others (especially physicists) about string theory have been valuable, far more so than some of the uninformed "string theory is bad" comments by others that seem to frequently clutter the comment sections in his blog. Understanding the weaknesses of a theoretical program is crucial for graduate students (e.g., me) to make an informed choice about what research direction to choose. One usually has many opportunities to hear about a theory's positive aspects by those who are actively working on it, but those same people usually don't spend much time talking about the negatives; presumably if they thought it had a lot of negative aspects they wouldn't be working on it themselves.

So the real issue here that I think people like Benni and Geon Oh should consider is not whether or not they can understand why ArXiv moderators might not like trackbacks to Peter's blog. The real issue, it seems to me, is that trackbacks (and beyond that, the ability to submit papers to the ArXiv) should not be prevented on the basis of personal feelings by moderators. Whatever criteria are chosen for allowing or disallowing trackbacks (or paper submission) should be published and uniformly enforced. Otherwise, the system is open to abuse, as outward appearance would suggest has actually happened in this case.

55. **Pudding**
February 26, 2006

"The real issue, it seems to me, is that trackbacks (and beyond that, the ability to submit papers to the ArXiv) should not be prevented on the basis of personal feelings by moderators."

Has Petr been prevented from submit papers to the ArXiv?

An outrage!

56. **Marty Tysanner**
February 26, 2006

Has Petr been prevented from submit papers to the ArXiv?

Not that I know of. I was thinking of a couple of other people who have apparently been blacklisted for no obvious reason. It would be appropriate for what constitutes grounds for being blacklisted and the procedures/requirements for having priveleges restored were made public and enforced uniformly, just as it would be for trackbacks. Why shouldn't this procedural information be documented and transparent?

57. **Tony Smith**
February 26, 2006

Richard said:
"… There are certain musical forms (i.e., atonal or 12 tone classical, rap, or fill in your own _____) which I feel perfectly qualified to criticize even though I'm neither a

musician or a musicologist. … Can you imagine being attacked by a musician and told that your opinion is invalid because you yourself aren't a practitioner? …".

Yes, I can.
IIRC, in the heyday of atonal 12 tone stuff, establishment music critics would deride audience dislike of atonal 12 tone stuff, saying that the members of such an audience were too ignorant and uneducated to understand the inherent beauty of atonal 12 tone stuff.
The reaction (over a time scale of decades) to such arrogance has been a return to use of harmonious structures.

If physics and superstring theory were to follow the historical pattern of music and atonal 12 tone stuff,
then
there may be a few more decades of arrogant superstring excess, followed by a return to experimentally connected physics,
but making use (in ways not permitted by current rigid superstring doctrine) of some of the math techniques of superstring theory

Tony Smith
http://www.valdostamuseum.org/hamsmith/

58. **Benni**
February 26, 2006

At first: Larsson writes:
>While string theory makes no hard, falsifiable predictions, it makes >many soft predictions: every single one is in apparent >disagreement with experiment. end quote

I know. Indeed it seems that string theorists themselves criticise their own field.
It can only be described as criticism, to write a paper that it is an NP hard problem to indentify the correct solution. Want else should it be to write publications which bring their own subject in a crisis. That such publications are written shows the contrary of what peter thinks. It shows that string theorists are actually aware of the problematic taste of their theory and criticising themselves. How else would you interpret the words of Gross recently?

What sometimes is annoying, is the media hype they create and the advertisement with which string theorists hire students.

But Arxiv has nothing to do with media hype. And even, when one protests against media hype. Peter does a well job. For students and graduates who want to start a carrier in theoretical physics. But not for science only for people.

The Problem in the Arxiv, I think is not, that he got blacklisted in a scientific preprint archive. The problem is that Arxiv has no clear moderating rules. Peter got no correct answer why he is blacklisted. The moderator board has no clear "rules" stated ccording to which it works (e.g it should be possible to make a "democratic" process where 30 moderators vote or such). Also, the "endorsement" system is problematic. Because the rules are not clear and very subjective to the endorsers. It could be, that the next Einstein won't be endorsed, because his paper is so original and so complex that no endorser would have time to read through these many formulas which have nothing to do with present day theory.

At second Peter writes:
>I don't know who you are, perhaps also an undergraduate, but >you don't understand what you are talking about. end quote:

I think I understand these comments of Prof.H.D.Zeh in Heidelberg very well, which discuss this aspect of nonlocality and String theory:
http://www.rzuser.uni-heidelberg.de/~as3/nonlocality.html
Quantum theory is kinematically nonlocal, while relativistic quantum field theory requires dynamical locality. How can these two aspects be compatible?
The most general local structure seems to be described by quantum field theory. It may be characterized by the following program:
(1) Find an underlying set of "classical" fields. This endeavor is the major objective of unified field theory such as string theory or "M theory", where often only rudimentary quantum aspects are taken into account.
(2) Define quantum states as wave functionals of these fields (that is, as superpositions of different "classical" field states).
(3) Assume that the Hamiltonian operator H (acting on wave functionals) is defined as an integral over a Hamiltonian density, written in terms of these fields at each space point.

And I don't see your point how string theory alone should reproduce entanglement, until we have not done step 2 and 3. I could go more into detail, even with calculations, but this gets off topic now and I stop.

59. **Benni**
February 26, 2006

an additional note to the Arxiv:
On the page introducing the endorsement system, arxiv writes
http://arxiv.org/help/endorsement
arXiv.org is distinct from the web as a whole, because arXiv contains exclusively scientific content. Although arXiv is open to submissions from the scientific communities, our team has worked behind the scenes for a long time to ensure the quality of our content.

And this is the problem.
What is "behind the scenens"?
On this page, Arxiv states that its content is controlled by a conspirative group of scientists who can do what they alone think.
Explicite rules and democratic moderating process is absolutely necessary here.

60. **robert**
February 26, 2006

Peter

I'm pleased to see that your 'Letter to ArXiv Advisory Board' has prompted such a useful debate of the role of this body. As one whose professional work has been in areas somewhat removed from the frontiers of knowledge, I find myself in sympathy with others outside the hep-th community who find the aggressive and partisan behaviour of stringers slightly bemusing, and quite at odds with more widely held views on what science is about.

61. **Michael**
February 26, 2006

MathPhys,

I don't despise Peter Woit. I'm just being blunt about who I think he his and what I think he's doing. I consider it possible that Peter is a likable guy personally, but it truly doesn't matter.

I am earning my money as a scientist (mathematical physics), in case you're question was sincere.

62. **Thomas Larsson**
February 26, 2006

Benni wrote:
At first: Larsson writes: >While string theory makes no hard, falsifiable predictions, it makes >many soft predictions: every single one is in apparent >disagreement with experiment. end quote
I know. Indeed it seems that string theorists themselves criticise their own field.

While you perhaps know this, and all senior string theorists undoubtedly do so, it is hardly common knowledge. Neither among the general public, nor among the high school and college kids who will become the next generation of scientists, and not even among professional scientists in other fields. Thus, there is certainly a need for public education here, of the kind that Peter is pursuing.

Because we don't want to keep taxpayers ignorant about the fact that all natural predictions of string theory disagree with experiments, do we?

63. **woit**
February 26, 2006

Marty,

Thanks for your thoughtful long comment and for your kind words about what I've been doing. What you describe is certainly what I hope I am doing, so I'm glad to see that in your perception I generally live up to what I aspire to.

64. **Benni**
February 26, 2006

Thomas Larsson Says:
>While you perhaps know this, and all senior string theorists >undoubtedly do so, it is hardly common knowledge. Neither among >the general public, nor among the high school and college kids >who will become the next generation of scientists, and not even >among professional scientists in other fields. Thus, there is >certainly a need for public education here, of the kind that Peter >is pursuing.
1) yes. And this "public education" might be the reason why Arxiv has Banned Peter. Arxiv might not want to have anything to do with public education.

2) Is it so worse in the US?
In Germany, I'm also a little bit in sorrow. Here, folks have looked at the string hype quite sceptical for 20 years. Now even before LHC is working, there seem to grow out "centers for mathematical physics" everywhere. in Hamburg, Munich, Heidelberg, Jena… Chairs which have been sceptical for years (Heidelberg) now working on it. It might be, that well trained postdocs from the US return. Or it might be that averything else in the Standardmodel is worked out, so they jump on this train because own work in quantum gravity might be a hight risk.
But they do this exactly at the point where the theory is a bit declining.

65. **Benjamin**
February 26, 2006

The situation with string theory is similar to that of Ptolemy's epicycle theory. An ellipse, or any closed curve for that matter, can presumably be expressed as circles around circles around circles ad infinitum, just as any periodic function can be expanded in a Fourier series. So epicycle theory is not even wrong, but the fact that its soft predictions are in apparent disagreement with experiments is nevertheless a problem for it.

Please forgive this vanity comment, but as a mere humble engineer interested in physics, I once had this thought too, both the epicycles and the Fourier part, just the way you say it. Since this is fascinating, and something I can grasp, and I don't know who you are, could either you or a qualified physicist please confirm that this is a valid objection, at least as a first or zeroth order concept for the non-physicist (with perhaps a wee bit of poetic license thrown in)? Thanks.

66. **Tony Smith**
February 26, 2006

Benni has made some comments here, with some of which I disagree and some of which I agree, but I think that in two areas his points are useful:

1 – with respect to arXiv:
"… arxiv writes …[at]… http://arxiv.org/help/endorsement … "Although arXiv is open to submissions from the scientific communities, our team has worked behind the scenes for a long time to ensure the quality of our content."
And this is the problem.
What is "behind the scenens"?
On this page, Arxiv states that its content is controlled by a conspirative group of scientists who can do what they alone think.
Explicite rules and democratic moderating process is absolutely necessary here.
…
The Problem in the Arxiv, I think is not, that he got blacklisted in a scientific preprint archive. The problem is that Arxiv has no clear moderating rules. Peter got no correct answer why he is blacklisted. The moderator board has no clear "rules" stated ccording to which it works (e.g it should be possible to make a "democratic" process where 30 moderators vote or such). Also, the "endorsement" system is problematic. Because the rules are not clear and very subjective to the endorsers. It could be, that the next Einstein won't be endorsed, because his paper is so original and so complex that no endorser would have time to read through these many formulas which have nothing to do with present day theory. …".

2 – with respect to string theory and quantum theory:
"… Schwarz … said: In stringtheory we assume everything local. …
To implement correlations, he must, I think, set up a kind of hidden variable theory, or make the string as big as the universe. …".

Benni's "string as big as the universe" might be interpretable as a world-line, and tachyons might be useful in a theory of correlations, so maybe string theory (with unconventional physical interpretations) could be useful in constructing a realistic quantum theory. However,
to the extent that arXiv suppresses posting of material about string theory interpretations that differ from the doctrinaire structure of conventional superstring theory, then possibly useful advances may be impeded.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

67. **woit**

February 26, 2006

Benjamin,

Yes, the epicycle analogy about the problems of string theory is one that has been made by many people (including some doing string theory).

As a general matter of philosophy of science, the thing is that, unless the structure of a theory is very rigid, it is hard to show that it is wrong, since often a theory can be made consistent with observations by making it more complicated. So the way theories often fail is not that they make wrong predictions, but that they become more and more complex in order to try and stay consistent with observations, until at some point people can't stand the ugliness of the theory any more and stop working on it. This seems to me the way the string theory unification program is headed. To avoid this, some people are trying to claim that ugliness is a virtue…

68. **Benni**
February 26, 2006

Tony Smith says
>Benni's "string as big as the universe" might be interpretable as a >world-line, and tachyons might be useful in a theory of >correlations, so maybe string theory (with unconventional >physical interpretations) could be useful in constructing a realistic >quantum theory.

At first, my knowledge in Stringhteory is not too high.
But to my small knowledge non-local Stringhteories in which strings are indeed stretched between different points of spacetime are a nonlocal quantum field theory, which violates causality (As today theorists compute everything some even work on causality violating models).

When I read such entries:
http://motls.blogspot.com/2004/12/causality-in-sft-ted-erler.html
http://arxiv.org/abs/hep-th/0406199
I see, that string theorists do their best, to get a local field theory.
They (Gross here) write:
>Causality is a basic requirement of any acceptable physical >theory; causes should always precede their effects, and if the >theory is Lorentz invariant, >spacelike separated events should >be uncorrelated.
Although they know, that sometimes in quantum mechanics events at spacelike separated points are correlated. That is, (as it seems to me) they construct their theory often against physical experiment.

Ordinary QFT was invented before Bells inequalities. So there was no need to think on correlations at distant ponts and normal QFT obeys locality. Quantum field theory has other problems too. QED for example is nonlinear. Feynman graphs can contain loops.
These problems are solved as such:
One defines a wave functional of the field operators.
In this review, for example decoherence is calculated with QFT (p17): http://arxiv.org/pdf/quant-ph/9803052
The procedure with this functional picture can be used, to get correlations without violating causality.
But one must "insert" the correct "local" QFT. Here's another example of that: http://arxiv.org/abs/hep-th/9306161
As Prof. H.D.Zeh writes, he thinks that normal QFTs, string and M theory included, are local and therefore only "semiclassical": http://www.rzuser.uni-heidelberg.de/~as3/nonlocality.html
Quantum theory is kinematically nonlocal, while relativistic quantum field theory requires dynamical locality. How can these two aspects (well based on experimental results) be simultaneously meaningful and compatible? How can dynamical locality even be defined in terms of kinematically nonlocal concepts?
It may be characterized by the following program:
1) Find an underlying set of "classical" fields (including a metric) on a manifold. This endeavor is the major objective of unified field theory (such as string theory or "M theory", where often only rudimentary quantum aspects are taken into account).

(2) Define quantum states as wave functionals of these fields (that is, as superpositions of different "classical" field states).

(3) Assume that the Hamiltonian operator H (acting on wave functionals) is defined as an integral over a Hamiltonian density, written in terms of these fields at each space point.

If I wrote something wrong, Peter or somebody else may correct me or delete my post.
But I do not know, what happens if one does step 1,2,3 with stringtheory.

1) Assuming the suesskind multiverse is correct.. What has one insert in such a "wave functional?" All the multiverses of Suesskind?
2) Assuming one day, String theory gives one correct solution, wouldn't this make everything much more complex? In the low energy level, one could work, when it is prooved that stringtheory gives the local standardmodel. But what would happen in the High Energy region?

My question to John Schwarz at Munich was simple:
"How is the quantum mechanical concept of nonlocality brought into stringhteory".
Answer: "Hmmmm, Hmmmm, Hmmmm (for 4 minutes), when we do perturbation series, we assume everything local. The correlations in Quantum Mechanics have not to do with causality violation. Entangled systems are very difficult in local stringheory….."
Question: "Could you provide me a reference on methods how one even tries to research on spacelike separated multiparticle correlations"
Answer: Hmm, Hmmm….. and then something about ADS/CFT duality I could not relate anymore to my question.

@Tony Smith:
I don't think that one can use this theory as a realistic interpretation of quantum mechanics. Neither non local stringtheory or all other approaches.

69. **Aaron Bergman**
February 26, 2006

Benni — you're pretty much asking for a nonperturbative definition of string theory. There exist field theories for the open string, for example, that satisfy all your criteria. Life is harder with closed strings although there are attempts along those lines. So, the short answer is that when it's been possible to define things nonperturbatively, string theory has all the same nonlocalities as ordinary quantum mechanics. The only difference is that the "fields" are now string fields defined on the space of string embeddings rather than points.

70. **Scott Aaronson**
February 26, 2006

Peter,

Just wanted to add to the chorus of support for you. I don't see how one could define "reasonable criticisms of string theory" in a way that includes Penrose's and Smolin's (and even some of the IAS particle theory postdocs', when I talked to them alone), but that excludes yours. Some people might say that, regardless of content, weblogs are just not an appropriate forum for scientific disputes. But in that case, why have trackbacks in the first place?

Best,
Scott

71. **Benni**
February 26, 2006

Maybe one should make a petition (online or per letter) to ask Arxiv, if it could create better "moderating rules" than these:

… arxiv writes at http://arxiv.org/help/endorsement … "Although arXiv is open to submissions from the scientific communities, our team has worked behind the scenes for a long time to ensure the quality of our content."

What is "behind the scenens"?
It is not because of Peter, but because of the Arxiv as scientific institution. The worst scientific journals have peer review system where one gets at least a comment why he is not allowed to publish.

Also the endorsement system… It could be, that the next Einstein won't be endorsed, because his paper is so original and so complex that no endorser would have time to read through these many formulas which have nothing to do with present day theory.

Has anyone an Idea of a better "standard ensuring" system?

This could also be subject to a petition.

72. **woit**
February 27, 2006

Scott,

Thanks for your comment. So that explains why my blog gets so many connections from the IAS.

Others,

I've just deleted a bunch of comments about Piet Hut, Witten, Brian Josephson, etc. The institute controversy over Piet Hut has nothing whatsoever to do with the issue at hand here, which is the decision by the arXiv moderators to suppress links to this weblog. I'm not arguing that the arXiv should not suppress things for which a legitimate case can be made that they are crackpot science. From what I have seen of Josephson's work, I think such a legitimate case can be made. The question is whether this blog is crackpot science, a point of view I obviously strongly object to. Please take discusssions of Piet Hut and Josephson elsewhere, and stick to the topic at hand.

73. **Andrew**
February 27, 2006

Dear Peter,

If you are not having any success with dealing directly with the ArXiV, I would have thought the logical step would be to take your complaint to the body funding them, which surely has ethics rules…. Their policies must be answerable to someone.

74. **MathPhys**
February 28, 2006

Michael,

My question was sincere. Your attitude towards Woit and his blog goes straight against the scientific ideal of objectivity and tolerance of free speech.

Woit has something negative to say on a (so far) purely speculative theory. He says it has (so far) failed to make contact with physics, and for that reason he believes it's on the wrong track. Is that a problem?

Why should a scientist have a problem with that? Why should anyone connect to his blog to leave the sort of messages that you and Motl come here to write?

Your attitude gives non physicists who read this a very negative impression of how physicists interact and deal with each other.

Theoretical high energy physics, particularly in the US, has always been nasty and cut throat. But what you are into here goes beyond professional competition. You are intolerant of opinions that are different from yours. And over what?

75. **MathPhys**
February 28, 2006

I also wish to make clear that I am *not* anti strings. In fact, I *want* to see string theory flourish and thrive, as it brings together everything that I have ever learnt and connects it to everything else.

So what I want to see you, Michael and Motl, do, and what I find most disappointing that you are not doing, is to respond to Woit's critiques in a calm and meaningful way. Tell us, very calmly, why he is wrong. Or tell us that (at this moment) you cannot tell us. There is nothing wrong with a scientist saying "I don't know. Things are unclear. I have gut feelings, but I still don't understand." We all say that almost all the time.

But cheap verbal attacks, personal remarks about Woit's career and his publication list, are best left to high schoolers.

76. **Juan R.**
February 28, 2006

I see no problem with that ArXiv-administrators, journals-referees, etc. are selecting scientific material each day, **if and only if**:

a) Are invoked explicit rules for "what is in" and "what is out"
b) There is posibility for publishing the rejected material in another site (maybe considered "second-class" but at least available).

Peter Woit is highlighting the problem (a). He want know why Not Even Wrong is now out from ArXiv trackbacking.

In a related topic, I would note that history of science is full of theories were initially considered crackpot (by some referee of even by entire communities) but broadly accepted at the end.

It has been well documented at least 27 cases of future Nobel Laureates encountered resistance on part of scientific community towards their discoveries and instances in which 36 future Nobel Laureates encountered resistance on part of scientific journal editors or referees to manuscripts that dealt with discoveries that on later date would assure them the Nobel Prize.

A beatiful example of last is rejection letter to Hideki Yukawa meson theory considered by referee of the physical review journal to be wrong in a number of important points: forces too small by a factor of 10-20, wrong spin dependence, etc. But work was not wrong and some years after Yukawa received the Nobel Prize for physics by *that* work.

Hermann Staudinger (Nobel Prize for Chemistry, 1953):

"It is no secret that for a long time many colleagues rejected your views which some of them even regarded as abderitic."

Howard M. Temin (Nobel Prize for Physiology or Medicine, 1975):

"Since 1963-64, I had been proposing that the replication of RNA tumour viruses involved a DNA intermediate. This hypothesis, known as the DNA provirus hypothesis apparently contradicted the so-called 'central dogma' of molecular biology and met with a generally hostile reception…that the discovery took so many years might indicate the resistance to this hypothesis."

Today the dogma in physics is string theory. Anyone contrary to the dogma receives hostility. In this blog, we can see examples of insane hostility Peter Woit receives from Michaels, Motl, etc.

Among the more notorious instances of resistance to scientific discovery previous to existence of Nobels, we can cite the Mayer's difficulties to publish a first version of the first law of thermodynamics. Not forget that even Newton was considered crackpot by the mainstream of the epoque. Do you remember D'alembert evaluation of Newtonian work?

Therefore, a bit of caution with using the word "crackpot" and more clear rules for ArXiv.

—
Juan R.

Center for CANONICAL |SCIENCE)

77. **Florifulgurator**
February 28, 2006

As an ex-academic, my favorite blogs to keep updated to the state of math phys is Baez´ and this. Any other suggestion?

78. **woit**
February 28, 2006

Juan (and others),

Please stop it with the generic anti-scientific establishment rants. I don't think they add anything to this discussion. Sure, there are lots of examples in history of good scientific ideas being discounted and suppressed, but going on about those doesn't have much to do with the case at hand.

I'll leave Juan's last rant up for a personal reason. The chemist Hermann Staudinger was my great-uncle.

79. **Count Iblis**
February 28, 2006

Trackbacks referring to blogs that discuss the contents of papers (in a serious way) should not be censored. That's totally unjustifiable.

Perhaps someone here with a blog that is not censored by arXiv is willing to help to circumvent the ban by posting trackbacks on their blog on behalf of Peter with a link to this blog?

80. **George**
March 1, 2006

"Trackbacks referring to blogs that discuss the contents of papers (in a serious way) should not be censored."

What is "serious"? Who decides what's a serious discussion?

"That's totally unjustifiable."

Why does the arXiv allow trackbacks at all? What purpose are they supposed to serve?

81. **secret milkshake**
March 1, 2006

We are using the Staudinger reaction in medicinal chemistry quite often around here…

With sophistry one could make phenomena fit (Freudist psychoanalysis or Marxist-Leninist did).

Ugliness = the ad hoc constructs invoked, heaped on to make known things fit (after they did not, and without a non-circular justification for it). Wiggle room = bad.

Math complexity: a practical complication but not a sign of ugliness – if all follows from limited, fragile set of postulates + some nontrivial observables are produced.

"It's not a science, it's just a literature" (landscapist literature at that)

82. **Tony Smith**

March 4, 2006

In comments on a Sean Carroll Cosmic Variance blog entry “Crackpots, contrarians, and the free market of ideas”,
Ethan Tecumseh Vishniac said:
“… As a long time reader of Peter’s blog, a journal editor, and a (former) member of the ArXiv advisory board … I enjoy lurking on …[Peter’s]… blog …
I have a few comments. …
The ArXiv instituted a standard that they would allow trackbacks only to blogs run by active researchers.
That excludes Peter … I can’t remember anyone else’s name coming up.
I’m not going to hazard a guess as to the role that existence of Peter’s blog played in settling on this standard. …
When the topic …[of]… characterizing you [Peter] as not an “active researcher”came up I did a quick search under your name and didn’t find much in the published literature. … I checked the World of Science database. …
For reasons that are not clear to me, this standard was not communicated to Peter for some months. I don’t think most of the board was aware that this was the case.
I didn’t realize it until Peter commented in one of his postings that he had never heard back from the board. …
I’m a little surprised to hear that the policy behind the decision was not communicated clearly to …[Peter]. Actually … very surprised. …”.

From those comments, it seems likely to me that :

1 – The arXiv advisory board instituted a “trackbacks only … by active researchers” “standard” based on deliberations about only one blog, Peter’s.
That seems to me to be a rationalization for a personal blackballing of Peter.

2 – The arXiv board determined Peter to NOT be an “active researcher” based on not “find[ing] much” under Peter’s name “in the published literature” by checking “the World of Science database”.
I went to a World of Science web page at http://scienceworld.wolfram.com/ which says “Eric Weisstein’s World of Science – A Wolfram Web Resource – the best resource for math and science on the internet”
and I searched the “Entire Archive” with these results:
for “Woit” – “Your search did not match any documents.”
for “Motl” – “Your search did not match any documents.”
for “Vishniac” – “Your search did not match any documents.”
for “Vafa” – “Your search did not match any documents.”
for “Ginsparg” – “Your search did not match any documents.”
Lest you think that I just don’t know how to search, I will say that a similar search for “Witten” yielded a two-page list of documents.

3 – If “most of the board was …[not]… aware that … this standard was not communicated to Peter for some months”,
then the board’s oversight of arXiv administration is an example of poor management by the board and capricious (at best – actually, possibly even malicious) execution of policies by arXiv administration.

4 – The fact that Ethan, “a long time reader of Peter’s blog”, did NOT post any comment about this matter on Peter’s blog, but quickly posted comments on Sean Carroll’s blog, shows that Ethan (perhaps in order to maintain status among those he considers to be his peers) feels that Sean Carroll is respectable (and maybe even an “active researcher”), whereas Peter Woit is not (and therefore it is respectable only to “lurk…” on Peter’s blog, but not to post comments thereon).

The whole situation seems to me to be more like making membership decisions in old-time private clubs (back in the days when the IAS had to be created as a home for Einstein because some institutions were concerned about him being Jewish)
than advancement of science by open publication, commentary, discussion, and debate.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

83. **woit**
March 4, 2006

Tony,

I agree with you that the “active researcher” standard was just chosen as that which gave the best argument the moderator could come up with for censoring me. In other areas the arXiv moderation policy is, like that of journals and just about everything else in science, about the scientific content, not the credentials of the person involved. The moderators in this case decided they couldn’t argue against the scientific content of my blog, but could try and argue against my professional qualifications. Since I first started raising these issues publicly, string theory fanatics have shown that they have no answers to the scientific arguments I am making, so their only tactic is to insist that I don’t have the right qualifications to be heard on this issue. This kind of ad hominem response long ago convinced me that the field was even more intellectually bankrupt than I initially thought.

I have a different interpretation of Ethan’s decision not to comment here, but to do so at Cosmic Variance. I take him at his word that he reads what I have to say here and presumably often finds it worthwhile and probably even “respectable”. What I wrote here about what the arXiv’s reason seemed to me to be for its decision was intended to try and force their hand and get them to tell me what this reason was. As long as this remained only on my blog, it in some sense was just between me and the arXiv. Sean’s posting made clear that others were finding my interpretation of what was going on at the arXiv plausible, and made some sort of response and explanation seem necessary.

84. **Aaron Bergman**
March 4, 2006

“World of science” was probably a typo for “web of science”, a citation search engine online.

85. **Tony Smith**
March 4, 2006

Aaron Bergman said: “World of science” was probably a typo for “web of science”, a citation search engine online.”.

OK, assuming that Aaron knows what is in Ethan’s mind better than Ethan’s typing hands (no insult intended, as I make a lot of typos in my writings), I went to a “web of science” web page at http://scientific.thomson.com/free/ which is free and described as “an expert gateway to the most highly influential scientists and scholars worldwide”, and repeated the searches described in my comment above, with the following results:
for “Woit” – “No matches found.”
for “Motl” – “No matches found.”
for “Vishniac” – “No matches found.”
for “Vafa” – “Highly cited researcher”
for “Ginsparg” – “No matches found.”

for “Witten” – “Highly cited researcher”

Since Motl, whose blog IS allowed arXiv trackbacks,
and Vishniac, who is “a (former) member of the ArXiv advisory board”
and Ginsparg, who founded the arXiv,
all have the same “No matches found” null result on the “web of science” search,
I find it ridiculous (it would be funny if it were not a symptom of the antiscientific tendencies of the Cornell arXiv) for Ethan to use the “web of science” “gateway to the most highly influential scientists and scholars worldwide” as a criterion for declaring Peter Woit to be “not an “active researcher”” and therefore to be banned from arXiv trackbacks.

Of course, I am only taking Aaron’s word in my use of “web of science”, and it is possible that some arXiv apologist will now comment that it should not have been either Ethan’s “World of science” or Aaron’s “web of science”, but some other list, and if we go through enough lists then MAYBE one of them might list Woit below Motl, Vishniac, and Ginsparg.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

86. **Aaron Bergman**
March 4, 2006

I’m not sure what the point of this is, but the web of science is not a free service and is available here. All the people you list above return listings.

87. **Tony Smith**
March 4, 2006

Aaron Bergman said “… the web of science is not a free service and is available here …[http://scientific.thomson.com/products/wos/]… . All the people you list above return listings. …”.

“web of science” DOES have a FREE component of its service, which is as I said above at http://scientific.thomson.com/free/ , and the results I stated above ARE ACCURATE FOR THE FREE COMPONENT of “web of science”.

“web of science” does also have not-free components, but I do not have access to them, so I did my search on the free component. If Aaron has access to the not-free components and wants to report results of searches on them, he is free to do so. As I said above, “if we go through enough lists then MAYBE one of them might list Woit below Motl, Vishniac, and Ginsparg”.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

88. **Christine Dantas**
March 4, 2006

Following Tony Smith’s investigation, I wondered whether only
endorsers could send trackbacks. My search in the arXiv resulted in the following (including myself in the list):

Peter Woit: Not currently an endorser.
Lubos Motl: Can endorse for hep-th.
Ethan T. Vishniac: Not currently an endorser.
Cumrun Vafa: Can endorse for hep-th.
Paul Ginsparg: Not currently an endorser.
Edward Witten: Can endorse for hep-ph and hep-th
Christine Dantas: Can endorse for astro-ph

According to the rules of the arXiv:

> Endorsers must have authored a certain number of papers within the endorsement domain of an archive or subject class. The number of papers depends on the particular subject area, but has been set so that any active scientist who’s been working in her field for a few years should be able to endorse IF her work has been submitted to arXiv and IF she is registered as an author of her papers.

Dr. Vishniac is of course very well known and has numerous papers, but apparently he has not registred most of his arXiv entries,
so I suppose that is the reason why he is not being considered as
an endorser.

Since Dr. Woit has submitted only 2 papers to the arXiv (as far
as my search is correct), they have judged him as not an endorser.
I guess that was an important measure that they have considered. According to such a criteria, he would not be allowed to send trackbacks.

Ok, that’s a measure, but a very “blind” one for sure.

One test for my hypothesis would be to see whether Paul Ginsparg can send trackbacks.

I would like to send trackbacks myself and test this as well, but according to blogger (where my blog is hosted), I cannot do this… Well, perhaps there is a way that I must figure out, because since Lubos Motl could do it with the same blogger, then there must be a way.

In any case, that is disappointing. The rules are not clear.

Christine

89. **Aaron Bergman**
March 4, 2006

Tony,

I’m trying to tell you what Ethan was in all probability referring to and to help clear up a misunderstanding. If you want to keep playing Perry Mason, please do it without me.

90. **Power Ranger**
March 4, 2006

On the one hand, you claim that the Trackback system at the arXivs is being run for the benefit of a cabal of string partisans: “Note that, besides the blogs run by arXiv moderator Jacques Distler, Lubos’s is one of only a couple particle theory related blogs that the arXiv moderators allow trackbacks to. That trackbacks to this blog are censored, but allowed to Lubos’s (and almost no others not belonging to an arXiv moderator)…”

On the other hand, you claim that the “active researcher” standard was devised specifically to exclude YOU: “I agree with you that the ‘active researcher’ standard was just chosen as that which gave the best argument the moderator could come up with for censoring me.”

Which is it?

On the one hand, you complain that the “active researcher standard” (the same criterion, I suppose, that’s used by journal editors to select referees) is irrelevant: scientific content is the only thing that should count.

On the other hand, you would probably object even MORE strongly if the arXiv moderators were to moderate your individual trackbacks for their scientific merit.

Seems to me that there’s nothing they could do, short of accepting your tracbacks, that would satisfy you.

And if they accept your trackbacks, whose should they reject and on what grounds?

91. **Benni**
March 5, 2006

I think Peter could go around easily. He could
publish
publish
and publish

Sometimes he says that there are mathematically interesting items in string theory, and also few physical ones.
It seems that he knows most in string theory, so he could do some work there. Maybe he could also find an upper bound for the cosmological constant or such…

But it can be understood, that Arxiv wants to be only in the scholarly discussion process. And for this, it is fairly enough when people like douglas write
“maybe this is a nightmare [in stringhteory research]”

This has nothing to do with a “bankrupt” field, I think. But more with the strange “authoritative” behaviour of scientists. In germany, if a student wants to go in an elevator, and also a professor wants in, the professor often would say “Can I go here” and would ran into the elevator and closing the doors, that the student has to wait on the floor. There exists a Joke saying: “You nothing, you idiot, you amoeba, you …. student”. You have simply almost no right to say anything until you have a long publication list.
Ethan writes to cosmic variance, because he is interested in the opinion of colleagues. He writes “Must I say that I don’t consider you as crank” or “I think your blog personally informative”. But he doesn’t consider peter as a colleague. And maybe questions the scientific value of the blog, since a researcher would not find interesting papers which he has to cite in own papers.

This blog has a social value. It makes people clear about the status of stringtheory. But for to be scientific, Peter should publish something. This would also make his critics to be taken more seriously

92. **nigel**
March 5, 2006

Power Ranger,

“Active research” is not just determined by publication rate. We all know the committee-authored papers, where 100 authors all co-author with one another, so that they get 100 citations each just for writing 1 paper each. A nice long list for the CV, but it is group-think.

Group-think is personality-dominated. But science is determined by important results that have been objectively compared to nature, of which none have come from ST yet.

“…you would probably object even MORE strongly if the arXiv moderators were to moderate your individual trackbacks for their scientific merit.”

I don’t see why Peter could or would want to object to decisions based on scientific merit, since science is the aim of ‘not even wrong’. All the critics who see science as a matter of political correctness, seem personally insulted by Pauli’s category for string theory type-hype, ‘not even wrong’, plus objective questions about the failure of string theory to do anything objective for science.

They should remember that in the end science is not a politically correct tea party with results determined by some kind of secret consensus or conspiracy, but it is determined by agreement with nature. It is very short-sighted to shoot the messenger.

93. **Benni**
March 5, 2006

ethan writes:
> I also think the idea behind the policy could use some public >feedback, but I’m going to leave that to others. I’ve said my >piece. Comments from other people, including anyone reading this >thread, would be far more effective.

Which could be merely understood to his colleagues as a note to protest against the decision of the board. This might also be a reason why he posted at cosmic varianc, where smolin and sean are active.

94. **Power Ranger**
March 5, 2006

“‘Active research’ is not just determined by publication rate.”

No, but it’s not determined by saying, “I scribble in a notebook from 10 until 3 every day.” either. One (unpublished and uncited) paper in over 15 years does not constitute an active research career.

95. **woit**
March 5, 2006

Power Ranger,

First of all, I'm really tired of having people challenge my scientific credentials from behind the cloak of anonymity. It seems to be a favorite tactic of string theory fanatics and strikes me as cowardly. At least one thing you can say for Lubos is that he doesn't do this.

If you want to argue about whether I'm doing research or not, you have to show that you have some understanding of what I have written. Based on what I've seen of comments written under the same pseudonym here and elsewhere, it seems to me highly unlikely that you have any understanding of either what I've written, or of most of the scientific issues that are discussed on this blog.

I don't see the incompatibility between claiming that the arXiv trackback system is being run mainly for the benefit of Jacques Distler and other string theory fanatics, and that the "active researcher standard" was chosen to give Distler his best argument for censoring me. Both are true.

This "active researcher standard" seems to be ill-defined, since no one can tell me exactly what it means. If it ever does get made more specific, I suspect it will be carefully crafted to exclude me. In other areas, the arXiv doesn't use this standard. From what I recall, its official standard is supposed to be that it accepts postings that are "refereeable", i.e. that an editor of a journal would send out for peer review and not immediately reject out of hand as crackpotism. I'd be perfectly happy if the moderator wanted to accept some of my trackbacks, rejecting others as crackpotism. It would be quite interesting to see which he put in which category.

It would be a lot of trouble to moderate trackbacks on a case by case basis, so I can understand that the arXiv would want to deal with the question of whether to accept trackbacks from a site as a whole. The natural mechanism for doing this is their endorsement mechanism. If one or more arXiv endorsers endorse a site, trackbacks from there would be allowed. They actually seem to have tried this out in my case: at least one prominent scientist was contacted and told them that trackbacks to my site should be allowed. They didn't like that result and realized that I would have no problem coming up with endorsers, so adopted this new, never before used, ill-defined "active researcher" standard.

96. **Benni**
March 5, 2006

peter wrote: at least one prominent scientist was contacted and told them that trackbacks to my site should be allowed. end quote

Is this speculation, or do you know this for sure?

97. **woit**
March 5, 2006

This is not speculation, but first-hand information. The scientist involved does not want to be publicly identified or get further involved in this controversy.

In any case, I think any regular reader of this blog, or reader of the supportive comments posted here and elsewhere, would not have much trouble coming up with a list of arXiv endorsers who would support the inclusion of trackbacks to this blog in the arXiv. Doubtless the moderators involved are well aware of this, thus avoiding using the endorsement mechanism here.

98. **Benni**
March 5, 2006

anyway it would be good for you, to publish a bulk of reviewed papers.
I wish you many scientific results.

99. **Power Ranger**
March 5, 2006

In the case of trackbacks, you say that it doesn't matter who sent the trackback. Only "the scientific content, not the credentials of the person involved" matter.

In the case of blog comments, you have a different standard, apparently. OK …

The research track record is relevant to lots of decisions (hiring, funding). It's also used by journal editors to choose referees. If blog posts about papers on the arXivs play a similar role to that played by refereeing in traditional journals, it's pretty reasonable to apply a similar standard to decide whose trackbacks can appear alongside a paper on the arXivs.

100. **woit**
March 5, 2006

Power Ranger,

Your latest comments here have no apparent scientific content, if they did and they made sense I'd happily take them seriously, no matter what your credentials or even if you desired to remain anonymous (by the way, you're the only person I've noticed so far who takes the trouble to anonymize connections to the web-server here, impressive).

There's nothing inherently wrong with the idea of using research track record to make the decision at issue here, and I've already submitted my argument to the arXiv advisory board that my research track record justifies accepting trackbacks to this site. I am however pointing out that it is not clearly an approriate one in this case, and is quite different than that used in other cases by the arXiv, and the reason for the change in criterion seems pretty clear. By the arXiv's normal criterion trackbacks to here would have to be accepted, thus the desire to find a different, more ambiguous one that could justify rejection.

By the way, I don't think the analogy to refereeing makes much sense. A referee is given some serious power, that to accept or reject a paper. A link to commentary about a paper which the arXiv in no way endorses is something pretty different. In any case, referees are often chosen by journal editors based on their expertise, not their research track record. It is quite common for very junior people with little track record to be asked to referee papers in their specialty, this happened to me more than once when I was just starting out in this business.

101. **nigel**
March 6, 2006

There is a trackback essay by Jacques Distler at http://golem.ph.utexas.edu/~distler/blog/archives/000760.html

102. **Dumb Biologist**

March 6, 2006

These days in the bio-sphere one (me, for example) is often asked to referee papers because one has just had a paper published in the journal for which one is being asked to referee. Long ago my response was to be surprised and flattered, until I realised the journals was so overtaxed by the weight of submissions they were trolling for anyone with a pulse and two brain cells to rub together.

Anyhow, there seems to be no easy way to draw direct analogies between the print world and the arXiv. To my untrained eye, though, as I've attempted to learn more about the issue, it seems like trackbacks might be, in some remote way, at least, part of an evolving form of peer review. Hopefully the concept of "peer" is also evolving.

103. **Justin**
March 6, 2006

Interestingly, the arXiv allows a trackback to Lubos' latest appauling assasination of the "LQG & standard model" paper by Smolin et al.

On the whole I tend to agree with Lubos' opinion of the intellectual merits of LQG… but still, it seems ridiculous that such a rude and humiliating rant would be allowed, while all trackbacks to any of Peter's comments (even helpful and polite ones) are a priori disallowed.

104. **Lead by Science**
March 6, 2006

DB,

> it seems like trackbacks might be, in some remote way, at least, part of an evolving form of peer review. Hopefully the concept of "peer" is also evolving.

In the way Jacque had presented it, it is.

Peter,

Peter don't worry about the trackbacks, you have linking capability and it still works.

Imagine, if you were in China and the Government did not want you to see, other then, the way it had wanted you to see?

Freedommmm……… is nice, is it not?

105. **One R**
March 6, 2006

Justin,

we are not talking about merits of personality, but of science.

106. **Justin**
March 6, 2006

One R,

It is questionable to what degree such rants are helpful to science.
On the other hand, thoughtful and knowledgable critiques can be helpful. Good science and personality are not disconnected.

107. **Dumb Biologist**
March 6, 2006

LbS,

Yeah, well, let's just say I'm not terribly impressed with the direction the evolution has taken so far. That said, if the arXiv (and other things like it) are in an especially dynamic phase of development, perhaps things can and will evolve still further, and the "selective pressure" applied will weed out the suboptimal approaches. I have some hope the issue, being debated in public as it is, will serve the greater good in the end. Lots of things in science are self-correcting, because the whole is always greater (and often better) than any of the parts.

Exceptional MathReviews

February 28, 2006
Categories: Uncategorized

Mathematical Reviews is a publication of the American Mathematics Society that provides capsule reviews of essentially every mathematics research paper that is published. It has been in operation since 1940, and in recent years the entire database has been on-line, with the name MathSciNet. Some of the reviews are rather entertaining in one way or another, and the availability of this database has led to a new form of diversion among mathematicians: searching on promising words (e.g. "plagiarism") to see what turns up. My colleague Kimball Martin has put together a new web-page entitled Exceptional MathReviews with links to the best reviews he has heard about.

One of the most famous such reviews turns out to be apocryphal. In the days before the searchable database, I had heard it claimed that one Math Review contained the following devastating evaluation: "This paper fills a much-needed gap in the literature." It turns out that this isn't actually true. For the real story and more about Math Reviews, see an article from the 1997 Notices of the AMS.

Comments

1. **Florifulgurator**
   February 28, 2006

   Could be nice, alas I have no subscription account available 🙁

2. **Pindare**
   March 1, 2006

   Thanks for the potentially interesting link, too bad I have no subscription. (They use to let people see the Featured Reviews for free, but alas it has been discontinued, to the despair of I guess many students and not-in-academia researchers & amateurs…)

3. **Bill Lee**
   March 1, 2006

   A sampler….
   Granville on Biaca
   ==========
   Matches for: MR=MR1418826
   Item: 1 of 1 | Return to headlines13 | Go To Item #: 23
   84
   MR1418826 (97i:11025)
   Baica, Malvina14(1-WIW)15
   The Euclidean character of the Fermat's last theorem. (English. English summary)
   Notes Number Theory Discrete Math.16 2 (1996), no. 1,17 20–23.
   11D41 (11A05)18
   Review in linked PDF19 Add citation to clipboard20 Document Delivery Service21 Journal Original Article
   References: 0 Reference Citations: 0 Review Citations: 223
   Herein the author states "her genuine concern" about Wiles's purported proof of Fermat's last theorem \ref[A. Wiles, Ann. of Math. (2) 141 (1995), no. 3, 443–551; MR1333035 (96d:11071)24] which, after all, appeared in an "in-house publication in the Annals of Mathematics at Princeton". Baica's concerns seem to stem from a worry that results concerning elliptic curves "may not be equivalent to the result in the Euclidean geometry". She backs up her concerns by noting that "there is a need to provide Galois' connection from category theory". Of course, she has no such worries about the validity of her own, Euclidean-algorithm-inspired, proof of Fermat's last theorem.

   \{See also the preceding two reviews [MR1417460 (97i:11023)25; MR1418825 (97i:11024)26].\}

   ==============

   Matches for: MR=MR0429922
   Item: 1 of 1 | Return to headlines13 | Go To Item #: 23
   84
   MR0429922 (55 #2931)
   Hsu, Kuang Yau14
   On decomposition of certain formal groups.
   Tamkang J. Math.15 6 (1975), 69–78.
   14L0516
   Review in linked PDF17 Add citation to clipboard18 Document Delivery Service19 Journal Original Article
   References: 0 Reference Citations: 0 Review Citations: 0
   It is hard to imagine in a single paper such an accumulation of garbled English, unfinished sentences, undefined notions and notations, and mathematical nonsense. The author has apparently read a large number of books and papers on the subject, if one looks at his bibliography; but it is doubtful that he has understood any of them. He speaks blithely of elements of a formal group and claims to prove (!) that two formal groups having the same Lie algebra are isomorphic. One also hears of root systems of a commutative formal group, and of the union of its Borel subgroups, which belongs to the Lie algebra, etc. What is amazing to the reviewer is that such a thing was ever printed.

   Reviewed by J. Dieudonne

   ==========
   MR1786212 (2001f:11085)
   D\c abrowski, Andrzej14(PL-SZCZ)15; Wieczorek, Ma\l gorzata16(PL-SZCZ)17
   Families of elliptic curves with trivial Mordell-Weil group. (English. English summary)
   Bull. Austral. Math. Soc.18 62 (2000), no. 2,19 303–306.
   11G0520
   Review in linked PDF21 Add citation to clipboard22 Document Delivery Service23 Journal Original Article
   References: 0 Reference Citations: 225 Review Citations: 226

This paper contains barely a single correct statement. The first proposition is that an elliptic curve $y^2 = x^3 + A x + B$, with $A,B \in Z$, $A \geq 0$, cannot contain a rational torsion point of order 5 or 7. The proof is erroneous, and indeed the curve $y^2 = x^3 +2160 x + 170\,640$ contains the point $(-24,324)$ of order 5, and the curve $y^2 = x^3 +206\,037 x + 80\,423\,334$ contains the point $(219, -11\,664)$ of order 7. (The one-parameter families of curves possessing 5- and 7-torsion are misquoted from tables of Kubert, but the mathematical error lies elsewhere.) The second result (which is actually true) is given a proof containing the line “leads to the equation $x^3+ A x + B = 0$ with no real solution (when $A \geq |B| > 0$, of course)”, where the assertion is false, of course. The final example concerns the curve $y^2= x^3 + 5 x+1$ where “one easily checks that $E(Q)_{\rm tors}=Z/3Z \ (=\{(0,1),(0,-1),\infty\})$”. But adding $(0,1)$ to $(0,1)$ results in $(\frac{25}{4}, -\frac{133}{8})$, so the point $(0,1)$ is of infinite order!

Reviewed[27] by Andrew Bremner[28]

4. **MathPhys**
March 2, 2006

I never knew that MR allows reviewers to be critical.

Understanding the Landscape

February 28, 2006
Categories: Uncategorized

Michael Douglas has written up his talk at the recent Solvay conference, with the title Understanding the Landscape. For previous postings about this conference, see here, here, and here.

Douglas's article is mainly a series of excuses for why string theory can't predict anything. He begins with an historical analogy, comparing the present state of string theory to that of quantum mechanics in the period 1913-1926, and the Bohr model of the hydrogen atom to N=4 supersymmetric YM and AdS/CFT. This kind of analogy has some rather obvious problems: it took 13 years to get from the Bohr model to the complete theory of QM, whereas the idea of string unification has been around for about 30 years, with not the slightest sign of success. More importantly, the Bohr model of the hydrogen atom gave a fairly accurate prediction of the spectrum of the hydrogen atom, so it was clear that there was something very right about it. On the other hand, N=4 supersymmetry YM doesn't predict anything that corresponds to the real world. In a footnote, Douglas writes:

A similar analogy was made by David Gross in talks given around 2000. However, to judge from his talk here, he now has serious reservations about it.

but I'm not sure what reservations of Gross this refers to.

Douglas's second analogy is a "chemical" analogy, basically pointing to the fact that deriving bulk properties of solids from the underlying many-particle Schrodinger equation is difficult if not impossible. Again, this isn't a very good analogy. The problem isn't that you have a simple theory whose ground state is hard to identify because of the numbers of degrees of freedom, like in condensed matter physics. The problem instead is that the ground state of the superstring doesn't look like the real world. Douglas describes the current situation as:

Perhaps all this is a nightmare from which we will awake, the history of Kekule's dream being repeated as farce.

He goes on to give the standard argument for the landscape, referring to it by the novel name of the *Weinberg-Banks-Abbott-Brown-Teitelboim-Bousso-Polchinski et. al. solution to the cosmological constant problem*, but notes that:

On further developing these analogies, one realizes that we do not know even the most basic organizing principles of the stringy landscape.

and proceeds to discuss a little bit two topics that he hopes might be related to this problem, but I don't see any evidence for this. One topic is an abstract proposal for a metric on the "space of all CFTs", the other is the classification of D-branes on a Calabi-Yau in terms of a derived category.

He goes on to discuss his recent work with Denef on computational complexity, which indicates that even if our universe corresponds to some local minimum in the landscape, there is no hope of ever identifying which one it is and actually computing anything about the real world. In his concluding section, he admits that there is no evidence of any simple structure in the landscape, and argues that maybe this is just the way the world is:

Still, our role as physicists is not to hope that one or the other picture turns out to be more correct, but to find the evidence from experiment and theory which will show us which if any of our present ideas are correct.

The problem with his invocation of the role of experimental evidence is that he has just finished making an excellent case that there is no hope at all of ever getting any such evidence for string theory. He seems to have completely abandoned without comment his project of the last few years of counting vacua in hopes of making statistical predictions, and is left with not a single idea about how one can ever hope to get a prediction of any kind out of this framework. In this kind of circumstance, standard scientific practice is to acknowledge that this is a failed project and go on to something else. Douglas not only refuses to acknowledge the failure of string theory, he doesn't anywhere even mention the possibililty that the underlying idea might be wrong.

Yesterday's hep-th preprints included another landscape one, Probabilities in the Landscape by Alexander Vilenkin. Vilenkin shows no more evidence of having a viable way of ever making a physical prediction than does Douglas, but like Douglas invokes experimental confirmation at the end of his paper, then closes with one prediction that is sure to be accurate:

It seems safe to predict that we will hear more on this subject in the future.

For some more entertaining reading, take a look at A Comment on Emergent Gravity by Waldyr A. Rodrigues Jr. Rodrigues says that he gave as an exercise to his students to find the errors in Jack Sarfatti's recent arXiv posting Emergent Gravity. Sarfatti had been blocked for many years from posting on the arXiv, but the arXiv moderators recently relented, perhaps because this paper includes a section at the end about the landscape. Susskind's recent book is one of only two references in the paper. Sarfatti's paper has gone through six drafts, three of which are on the arXiv, and the latest of which incorporates some of the objections from Rodrigues. Rodrigues however concludes his paper thus:

Sarfatti's paper we regret to say, is unfortunately a potpourri of nonsense Mathematics. The fact that he found endorsers which permitted him to put his article in the arXiv is a preoccupying fact. Indeed, the incident shows that endorsers did not pay attention to what they read, or worse, that there are a lot of people with almost null mathematical knowledge publishing Physics papers replete of nonsense Mathematics…

A careful reading of shows that his hypotheses are completely ad hoc assumptions, since in our view no arguments from Physics or Mathematics are given for them. Summing up, we must say that Sarfatti's claim to have deduced Einstein's equations as an emergent phenomena is a typical example of self delusion and wishful thinking.

Sarfatti will be giving the closing talk at the 22nd Pacific Coast Gravity Meeting, to be held this coming weekend at the KITP in Santa Barbara.

Comments

1. **Kea**
   February 28, 2006

   This sounds unduly harsh. Not that I'm disagreeing with the basic
   conclusion. To quote the Douglas paper:

   *Are there fruitful analogies between these long-ago problems
   and our own? What is the key issue we should discuss in 2005? What are our hydrogen atom(s)?*

   *If we have them, they are clearly the maximally supersymmetric
   theories …*

What can one say. Interesting that he goes into Cheeger's work. I recently had the good fortune to meet Cheeger. Of course, the physicists present all wanted to know about the sort of thing that Douglas discusses, although not the Stringy version. Cheeger said he hadn't really thought about it, but he did mention a 1980's paper of his:

J. Cheeger, W. Mueller, R. Schrader Commun. Math. Phys. 92, 3 (1984) 405-454

which I haven't had a chance to look at, unfortunately. Maybe in another life.

2. **ObsessiveMathsFreak**
   February 28, 2006

   Sarfatti's paper needs another revision. It's not displaying correctly on my computer!

   To be genuinely honest, if Sarfatti's paper got through with a lot of mathematical errors, it doesn't surprise me. There are not a few academic papers which bandy about mathematical terms in a very terse and sparse fashion, referring to this or that paper or terminology to justify what they put down. To someone who is not *very familiar* with the field, i.e. 99.9% of undergraduates, a paper filled with nonsense is indistinguishable from one filled with correct but sparely documented terms.

   For example, from the Sarfatti paper:

   > The superfluid velocity field in Galilean relativity is the 3D Cartan closed inexact 1-form
   > v = (h/m)d(theta)

   Is this right? Is it wrong? I know nothing about any of this. I personally do not know what the following terms really mean: "superfluid", "Cartan closed inexact 1-form".

   This statement could of course be correct, but as someone new to the field, I'm basically taking most of this on faith. I don't think I'm alone here. Papers do this. A Lot. Not just physics papers either.

   I actually think this obfuscated and/or terse presentation style is a serious problem in scientific academica. Authors are not making the effort to make their discoveries accessable to a wider audience. Their papers can only be understood by a handful of other people. This doesn't help science. It just creates a huge barrier of entry into any paticular field. And of course, it also means it is easier for papers containing errors to slip through.

   I follow Feynman's philosophy on this: If something cannot be taught to in first year undergraduate course, then it is not fully understood yet. Sorry about the rant, but I've had to put up with a lot of rubbish papers and books lately.

3. **Peter**
   February 28, 2006

   Kea,

   Cheeger's a very good geometer. I first met him when I was a postdoc at Stony Brook, where he was for many years. He's now here in New York, at NYU. The theorem that Douglas refers to corresponds to the intuitive idea that if you have a manifold of a fixed size, to make it more topologically complex you need to make it more curved. If you bound the curvature, you bound the number of topological types.

   But I really don't see at all why he is bringing that up. While you want to bound the curvature to make sure the low energy supergravity approximation is valid, if you're doing string theory, why should you insist on such a curvature bound? And I really don't see at all what this has to do with his proposed metric on the space of CFTs.

   OMF,

   Unfortunately it really isn't possible to write every paper in a way that any undergraduate can understand it. Mathematics is a language, and like any language takes a significant amount of effort to absorb. You really need to immerse yourself in this stuff, think about it a lot and take the time to become comfortable with it. This can take quite a while and is not easy, but once you do this, you really can quickly tell the difference between someone speaking gibberish, and someone who has something interesting to say.

4. **Kea**
   February 28, 2006

   Peter

   Yes, it does look suspiciously like Douglas was rambling on about a dream for a metric that he hadn't thought about too much. He refers to the 1970 Cheeger paper. At the conference I was attending, Cheeger spoke about things like this [paper with Tian].

5. **Benni**
   February 28, 2006

   the paper of douglas seems indeed very much as expression of a big crisis on the subject. It looks similar to the crisis when it was discovered that general relativity is not renormalisable.
   Or even more, since this was one the reason, why one thought strings might help out.

6. **Troublemaker**
   February 28, 2006

   To someone who is not very familiar with the field, i.e. 99.9% of undergraduates, a paper filled with nonsense is indistinguishable from one filled with correct but sparely documented terms.

   Such papers are not designed for laypersons or for undergraduates. They are designed for people who are indeed very familiar with the field.

   Is this right? Is it wrong? I know nothing about any of this. I personally do not know what the following terms really mean: "superfluid", "Cartan closed inexact 1-form".

   So what? If it's beyond you personally, then it's trash?

Authors are not making the effort to make their discoveries accessable to a wider audience. Their papers can only be understood by a handful of other people. This doesn't help science. It just creates a huge barrier of entry into any paticular field.

The arxiv and the refereed journals are not an introduction to anything; they are (in principle) a place for the high-level, cutting-edge work going on in these fields. They are not supposed to be a tutorial, so it should not surprise you that they do not make a good tutorial.

If something cannot be taught to in first year undergraduate course, then it is not fully understood yet.

Even if this is true, it does not pertain to the situation at hand. These papers discuss areas of active and often controversial research. RESEARCH. If it were fully understood, it wouldn't be a subject of research anymore; it would be standard textbook fare.

7. **Chris W.**
   February 28, 2006

   This idea has probably been considered and dismissed before, but I'll throw it out anyway. Couldn't the arXiv support a declaration of intended audience for submissions, which could be used as a qualifier in database searches? One could then search specifically for reviews, pedagogical articles, research reports, etc. Any comments or elaborations on this notion would be of interest.

   Perhaps there are already some "aftermarket" compilations of preprint links that include such categorizations.

   (Sorry if this is too far off-topic.)

8. **Jack Sarfatti**
   February 28, 2006

   Closed inexact Cartan forms are an elegant way of describing superfluids and emergent phenomena in general.

   fyi

   We live in interesting times! 😉 Waldyr explained to me that he was pressured by some people that his students might suffer, otherwise he would not have written anything had I not mentioned him in the comments. I was, of course, trying to be honest that it was his critique that gave me the idea of 8 Goldstone phases with 6 corresponding to the extra degrees of freedom of Calabi-Yau, the connection to torsion and Shipov's 10D manifold. I was very clear in my acknowledgment that Waldyr did not "endorse" the content of the paper. The "math" in the paper is not important. It's the ideas – mainly both positive and negative vacuum energy at different scales as most of the stuff of the universe w

9. **Jack Sarfatti**
   February 28, 2006

   On Feb 28, 2006, at 7:00 PM, Jack Sarfatti wrote:

   bcc

   We live in interesting times! 😉 Waldyr explained to me that he was pressured by some people that his students might suffer, otherwise he would not have written anything had I not mentioned him in the comments. I was, of course, trying to be honest that it was his critique that gave me the idea of 8 Goldstone phases with 6 corresponding to the extra degrees of freedom of Calabi-Yau, the connection to torsion and Shipov's 10D manifold. I was very clear in my acknowledgment that Waldyr did not "endorse" the content of the paper. The "math" in the paper is not important. It's the ideas – mainly both positive and negative vacuum energy at different scales as most of the stuff of the universe w less than – 1/3. The math can be fixed where needed. What if I turn out to be right in the long run about LHC and DM detectors being null like the Michelson-Morley experiment?

   On Feb 28, 2006, at 3:37 PM, Tony Smith wrote:

   Jack, Peter Woit's blog has a new entry at
   http://www.math.columbia.edu/~woit/wordpress/?p=355#comments
   that mentions your arXiv posting and Waldyr's critical paper,
   and Peter closes his blog comment with the following statement:
   "… Sarfatti will be giving the closing talk at the 22nd Pacific Coast Gravity Meeting, to be held this coming weekend at the KITP in Santa Barbara. …".

   Due to the wide readership of Peter's blog, and the severe criticism by Waldyr quoted by Peter, your KITP talk may get a lot of attention, and
   it may be a very good opportunity for you to present your physical ideas.

   Here are a couple of lawyer-type bits of advice for your 10 minute talk + 2 minute questioning, scheduled for 17:06 – 17:18 on Saturday afternoon:

   You are the LAST speaker, so if you run over it doesn't hurt anybody else's schedule and there would be no problem if your question period were extended.
   ….

   As to the format of your 10 minute talk, my suggestion is to begin with a short statement saying that your arXiv paper did contain some technical math problems, some of which were mentioned in a critical paper by Waldyr Rodrigues, but that your main ideas are physical, and you only have time here to give a rough outline of the basic physics ideas, which are, as you said in your recent e-mails:

   1 – general relativistic spacetime emerges from Goldstone phases of LOCAL vacuum coherent order parameter in similar way that the superfluid velocity field emerges from Goldstone phase of local helium ground state coherence order parameter.

   Good!

   2 – point defect at center of Sun could explain the Pioneer anomaly as a hollow dark matter (exotic vacuum) mini-halo centered on the Sun …

   Good!

   3 – LHC & local DM detectors will not "click" on DM particles ever to explain OmegaDM = 0.23

   Good!

   Will do. Those are some of the key ideas. Another is taking equivalence principle to the max, i.e. all universal space-time symmetry generators must be locally gauged to induce geometrodynamic fields. If, in addition, there is hidden spontaneous broken symmetry in the vacuum for some of these space-time symmetries the "Meissner" mass gap would explain why curvature dominates over torsion for example at least in most situations.

After stating the 3 main points, then go into more detail, particularly about 1, showing the two forms of vacuum nonlocal coherence forming curved spacetime and incoherent well-known Casimir type effects and
their relationship to /\

Good suggestion but I could not do that in 10 minutes.

At end, plug the possibility that if the Dark Energy can be controlled with Josephson Junction arrays, it would be a vast energy source, a prize even greater than nuclear energy tamed by the Manhattan Project.

That would be way out of their comfort zone. Remember most of these guys who are not senior people are running scared about funding and are afraid to stick their necks out. Most of the hall talk at UCLA DM 2006 was people scared about funding. Even Max Tegmark joked about it in his talk.

Don't mention UFOs in your main talk.

Of course not! 🙂

If asked, say that control of Dark Energy could enable us to build things that do most of the things that UFOs are described as doing, and leave it at that.

If any of the above suggestions don't feel right to you, then don't use them. They are only suggestions, and the most important thing is that you should feel comfortable, and use your Cornell theatrical training to deal with …. et al as you would a heckler in a theater. For 10 minutes, the stage belongs to YOU, if you take it.

On the other hand, maybe the hall will be empty? 😉

10. **amanda**
February 28, 2006

I think that maybe Peter is being a bit hard on string theorists. 🙂
For example, Keith Dienes' latest paper hints that there may be a lower bound on positive cosmological constants on the landscape, and that this lower bound may be way too large. If evidence like this keeps coming in, I think people really will begin to give up, and not make "excuses".

11. **woit**
February 28, 2006

Amanda,

Maybe you're right. But I'm having a hard time figuring out what would possibly cause Douglas to give up on string theory. And he's not telling.

12. **Jack Sarfatti**
March 1, 2006

Regarding Waldyr's comments below, he told me that he was threatened that his students might have trouble getting funding if he did not respond negatively. Note that none of Waldyr's remarks are relevant to the the actual new physical ideas and are not even relevant to the actual final version of the paper. The math problems that may be still there are really quite minor and any real ones can be fixed. The bulk of Waldyr's 21 pages of difficult reading does not at apply to my new conjectures at all, but refers to standard background material I mentioned in a cursory way in the first version. See also further remarks below Waldyr's.

"Sarfatti's paper we regret to say, is unfortunately a potpourri of nonsense Mathematics. The fact that he found endorsers which permitted him to put his article in the arXiv is a preoccupying fact. Indeed, the incident shows that endorsers did not pay attention to what they read, or worse, that there are a lot of people with almost null mathematical knowledge publishing Physics papers replete of nonsense Mathematics…

A careful reading of [Sarfatti's paper] shows that his hypotheses are completely ad hoc assumptions, since in our view no arguments from Physics or Mathematics are given for them. Summing up, we must say that Sarfatti's claim to have deduced Einstein's equations as an emergent phenomena is a typical example of self delusion and wishful thinking."

Perhaps, however, Waldyr missed the beautiful simple analogy that the curved tetrad fields are like the emergent superfluid velocity field and that the world hologram with quantized area bits is essentially the Bohm-Aharonov effect for closed non-exact Cartan forms in the presence of topological defects in the emergent coherent order parameter. This is a wonderful idea that must first be grasped heuristically and the math will follow.

The historical Bohm-Aharonov effect is for quantized loop integrals of closed inexact 1-forms around line topological defects. What we need for geometrodynamics is the quantized closed surface integrals of closed inexact 2-forms around point defects. Waldyr's saying this is adhoc shows he missed the simple intutive idea. It's not adhoc, it's compelling and almost obvious to my mind at least.

On Feb 28, 2006, at 11:02 AM, Jack Sarfatti wrote:

Note that I received from several sources who do not wish to be named about Waldyr's general Wolfgang Pauli "attack dog" style of reviewing papers comments such as:

"But since he knows so much mathematics, he often rushes into criticism, without reading a paper carefully enough. So he thinks that the author has committed an error, whilst this was not the (our case)."

Of course I had some minor (formal at least) ambiguities in the formal expressions in 1st version of
http://arxiv.org/abs/gr-qc/0602022 that Waldyr usefully corrected, but I was also accused of misconceptions I did not make – again from my, perhaps, poor exposition and noting that Waldyr is not a native English speaker. 🙂

http://www.physics.ucsb.edu/~relativity/22nd-PCGM.html

I am giving a 10 minute talk at above meeting it is at

http://qedcorp.com/APS/PCGM2006.pdf

or

http://qedcorp.com/APS/PCGM2006.ppt

On Feb 28, 2006, at 9:34 AM, Jack Sarfatti wrote:

On Feb 28, 2006, at 1:20 AM, Paul Zielinski wrote:

If Waldyr's criticisms are technically correct and your mathematical errors are reparable, then a detailed response to Waldyr's published commentary can only improve the strength of your arguments.

The version up there now corrects some very minor formal flaws in the presentation of STANDARD physics that was simply background material that Waldyr wanted more rigorous. Also he accused me of things that were not so, but that may be because I was not clear enough. For example, I certainly knew that there are 4 distinct tetrad first rank Diff(4) tensor 1-forms e^a and 6 zero torsion spin connection 1-forms S^a^b, i.e.

e^a = e^audx^u

S^a^b = S^a^budx^u

The great bulk of Waldyr's 21 pages has little relevance to the latest version posted now

http://arxiv.org/abs/gr-qc/0602022

The vexing "Yilmaz" issue of locality vs nonlocality of pure gravity energy that Waldyr's general remarks on energy-momentum conservation in GR in his 21 pages pertains to are not at all relevant to my new conjectures, i.e.

I. tetrad field emerges from the several Goldstone phases of LOCAL vacuum coherent order parameter in similar way that the superfluid velocity field emerges from the single Goldstone phase of the local helium ground state coherence order parameter.

That's the key idea. I now seem to recall that Hagen Kleinert has 4 phases associated with the tetrads, but not specifically in connection with vacuum coherence? I will have to look at his book – it's a vague memory.

i.e. the PHYSICAL IDEA is that smooth c-number tetrad field is a vacuum coherence effect – not a random ZPF effect.

II. LHC & local DM detectors will not "click" with "the Right Stuff" ever to explain OmegaDM ~ 0.23. Looking for real DM particles whizzing through space is like looking for Earth's motion through the old mechanical aether (distinct from CMB Doppler shifts relative to FRW Hubble flow of course).

III. Bekenstein-Hawking and 't-Hooft-Susskind ideas point to a 2D quantized period DeRham integral from point defects in the vacuum coherence i.e. area quantization and volume without volume from a closed non-exact "area density" 2-form emergent from the Goldstone phases.

Bohm-Aharonov effect is "area without area" i.e. S1 non-trivial first homotopy – quantized loop integrals of closed nonexact 1-forms around line defects

World Hologram is "volume without volume" i.e. S2 non-trivial second homotopy – quantized closed surface integrals of closed non-exact 2-forms around point defects, e.g. one at center of Sun in Pioneer anomaly? This would be a hollow dark matter (exotic vacuum) mini-halo of the Sun.

Also, you may not have expressed yourself as clearly as you might have in presenting some of your points. Some disambiguation in response to criticisms appearing in Waldyr's paper might be a good thing.

Sure. These are all new ideas not found in literature.

I agree with Tony that Waldyr's charges of "ad hocness" are questionable. You evidently have a clear intuitive understanding of the basic physical model you are proposing. However, while I entirely agree that mathematical rigor is secondary to the physics, it does appear that several of Waldyr's points are not merely a matter of "rigor mortis", but are concerned with the proper definitions of important mathematical terms.

Yes, I think the current version is essentially OK in that regard. I basically eliminated all reference to what Waldyr found objectionable in the background stuff that is not essential to my thesi.

As for Waldyr's intemperate language, perhaps it would have been better if you hadn't cited him at all in later versions of your paper?

I thought I was being polite and honest and I made it clear that it should not be construed that Waldyr agreed with my new ideas. In fact Waldyr's contribution is forcing me to confront the need for 8 Goldstone phases instead of only 2 and then I associated the extra 6 with Calabi-Yau and massive torsion fields, i.e. both locally gauge Lorentz group and then hide its symmetry in the vacuum – i.e. non-Abelian Meissner effect for the torsion fields whose mass gap prevents us from seeing it easily.

IV. Extended local equivalence principle, i.e. locally gauge ALL universal space-time symmetries of the physical action to induce geometrodynamic fields beyond the original 1915 curvature field.

Note that at the square root tetrad level the curvature and torsion gauge potentials are SPIN 1 just like in Yang-Mills theory. When you go to geometrodynamic level

ds^2 = e^a(Minkowski)abe^b

Then you get a COMPOSITE spin 2 from entangled pairs of "subspace" SPIN 1 tetrad level fields.

Jack Sarfatti wrote:

…

My key physical idea is dark matter is same as dark energy with difference only in scale and the sign of the energy density and w less than -1/3 and from a distance it mimics w = 0 CDM.

It is obvious to me that the curved tetrad & Bekenstein-Hawking quantized area is like the superfluid circulation and that t Hooft-Susskind world hologram "volume without volume" is like Bohm-Aharonov "magnetic field without magnetic field" even if I did not get the formal math completely right on first try working alone outside of academia – Einstein needed Grossman to get his tensors right and he took ten years. I only took a few weeks so far and have not found my Grossman I guess? 🙂

Remember what Feynman said about "rigor mortis". Are Feynman integrals rigorous? Is M theory? I hear some new math is coming from M theory however. I think Waldyr is making a general point about lack of mathematical rigor in many physics papers. Waldyr is actually a Professor of Mathematics I think and there is a cultural difference. To me the math is secondary to the heuristic ideas.

13. **Jack Sarfatti**
March 1, 2006

On Mar 1, 2006, at 12:01 AM, Jack Sarfatti wrote:

Instead of some of the alleged "Dirty Dozen" threatening Waldyr that his students might not get funding because it appears that he "endorsed" my paper when I explicitly wrote that he did not do so, why don't these brave professors allegedly in search of the truth publish their own specific objections to the actual final version of the paper for all to see and for the historical record? What equation numbers in the actual version now up there are objectionable? Why do they choose to hide behind Waldyr? Remember what will be recorded IF in fact what I say about the LHC and the DM detectors comes to pass – that they forever remain silent except for false alarms. What I predict is easily falsified by an "ugly fact."

Ode on the remains of a Dark Matter Detector! 😉

Ozymandius
by: Percy Bysshe Shelley

I met a traveler from an antique land
Who said: "Two vast and trunkless legs of stone
Stand in the desert… Near them, on the sand,
Half sunk a shattered visage lies, whose frown,
And wrinkled lip, and sneer of cold command,
Tell that its sculptor well those passions read
Which yet survive, stamped on these lifeless things,
The hand that mocked them and the heart that fed;
And on the pedestal these words appear:
My name is Ozymandius, King of Kings,
Look on my works, ye Mighty, and despair!
Nothing beside remains. Round the decay
Of that colossal wreck, boundless and bare
The lone and level sands stretch far away.

On Feb 28, 2006, at 10:54 PM, Tony Smith wrote:

Jack, did you get a copy of the message from Waldyr dated Tue, 28 Feb 2006 to Zielinski with copy to me in which Waldyr said in part:

On Feb 28, 2006, at 11:23 PM, Jack Sarfatti wrote:

No I did not. How the "Dirty Dozen" could construe from what I wrote that Waldyr endorsed my paper is beyond me. I was very clear to say specifically that he did not endorse it at all. Well no more Mr. Nice Guy eh? I bent over backwards to credit Waldyr's effort. That's what one gets by being "collegial" I suppose? 🙂

The "math" is the least important part of my paper. I was not writing a "math paper." Math is not physics. So far no one has said anything at all about the PHYSICAL IDEAS in the paper. Waldyr has not. No one has. None of Waldyr's math points are relevant to the these physical ideas that I can see.

Here is what is in my paper about Waldyr

"Acknowledgment

I would like to thank Professor Waldyr Rodrigues, Jr of UNICAMP, Brazil for pointing out mathematical deficiencies in the first version of this paper that is really a progress report. The reader should not infer that Professor Rodrigues endorses all of the ideas of this paper or that he thinks the mathematics is up to his rigorous standards. I remind the reader that important new physics has been created with bad mathematics that is usually patched up later. A case in point is the history of the Feynman diagrams and the path integrals, which are still non-rigorous from the point of view of the top mathematicians."

What's wrong with these "dozen" Paragons of Omniscience? Can't these Victorian Station Masters read plain English? Look at this sentence:

"The reader should not infer that Professor Rodrigues endorses all of the ideas of this paper or that he thinks the mathematics is up to his rigorous standards."

Shame on that "Dirty Dozen."

Waldyr allegedly wrote:

"… It is true that I [Waldyr] said to him [Jack] sometime ago that I did not intend to post or publish the notes for the time being. I changed my mind (after almost two weeks) due to a comment that he inserted in the sixth draft of his paper and which to an eventual reader seems to suggest that I made corrections to that version of his paper, something which is not the case.

And indeed, yesterday before deciding to post my notes I received dozen of mails from all around the world asking: how could you endorse that paper?

Well, after that it remained no other alternative as the one I followed
which was to post my notes in the arXiv.
…

Concerning arXiv, endorsers and blacklists I have to say the following.

I think that the arXiv must eventually accept any possible paper it
receives subject to the rules:

(a) Every article should receive a signed review, which is to be posted
together with the article.[1]

(b) Any author could post a reply to the signed review. The reviewer can
write a treplic, and so on.

(c) A blacklist must exist: dishonest people who are plagiarists or invent, e.g., false affiliations must be punished, i.e., they must not be allowed to post any paper. The penalty eventually may not hold for ever, but for a time long enough for them to have time to reflect.

(d) the blacklist must be public. The reason for someone to be in the blacklist must be given. …”.

———————————————————————

Tony

PS – It might be interesting to know who were the authors of the “dozen of mails from all around the world”

I can guess who some of them are.

and exactly what they said, particularly in light of your statement that Waldyr told you “that he was threatened that his students might have trouble getting funding if he did not respond negatively.”

14. **ObsessiveMathsFreak**
March 1, 2006

> Such papers are not designed for laypersons or for undergraduates. They are designed for people who are indeed very familiar with the field.

And this is a problem. Even if someone is interested in learning more about a subject, a terse presentation will be confusing and offputting. Are references to pertinent papers or textbooks too much to ask?

> So what? If it’s beyond you personally, then it’s trash?

No, but it creates a barrier to me if I want to learn more about the field. There were’t even any explanations as to what these terms were. The author has assummed that only persons skilled in the field will read the paper, and it becomes a self fulfilling assumption.

> The arxiv and the refereed journals are not an introduction to anything; they are (in principle) a place for the high-level, cutting-edge work going on in these fields. They are not supposed to be a tutorial, so it should not surprise you that they do not make a good tutorial.

I don’t expect a tutorial, but I am disappointed by how inaccessable a lot of scientific papers are. I mislike the Ivory Tower effect that this type of writing gives off, however unintentional it may be. I am an applied mathematics undergraduate with a reasonably detailed knowladge of physics and mathematical physics, but I am frequently thrown off by authors who write for a closed shop.

There was a post in this blog a while back about public engagement in science, and how people were dumbing things down with string theory violin shows and cookery classes. Really, well, trite and condescending presentations of science which I think the public sees through.

My point here is that if scientists cannot even hope to enage and interest scientists in different fields, what hope is there of eliciting general public interest and engagement in our research? If papers are being published with mistakes, perhaps this is another symptom of the same underlying issue.

15. **Steve Myers**
March 1, 2006

On “difficulty” of technical papers: they’re meant for experts in the field and so use the short hand of jargon. Of course, math is not a special language — everything in math can be expressed in English, but once you’re used to the short hand you can read the math directly without translation (for example, since I seldom use vector calculus, I still have to translate del into partials). It is true that the jargon is often used as a way to exclude outsides but without an appendix of definitions, etc., there’s no way around it. Does Sarfatti need to explain cartan forms?

16. **nigel**
March 1, 2006

‘…how people were dumbing things down with string theory violin shows and cookery classes. Really, well, trite and condescending presentations of science which I think the public sees through.’ – OMF

What matters is that most students see through it. It acts like a filter: emphasising the weirder parts of a non-tested speculative mainstream “theory” only few mathematical competent students into physics. In Britain, where string theory was loudly emphasised in best-selling books by Stephen Hawking, a 4% per year drop in A-level physics followed, which continues today.

The normal reaction of the teaching establishment to this is just to demand more propaganda, or better quality stuff, to attract students into physics. You don’t need the landscape controversy to see where physics is going wrong.

String theory is a tiny component of physics, but because it so hard to test and yet so over-publicised, it casts a dark cloud over physics. If string theory looks right, it is only because it is the conquest of free thought by censorship of alternatives.

Compare general books written about the search for a unified theory in physics when it was a popular subject in Britain (before the 1985 ST revolution), to those today when it is unpopular.

Before 1985, the future of physics was open, there were many exciting possibilities. Today, there is widespread prejudice in favour of one speculative model, with no objective predictions let alone evidence. It is defended by obfuscation, which is unethical.

17. **ObsessiveMathsFreak**
March 1, 2006

I feel there is a difference between a terse presentation and a readable one, even if the topic is highly advanced. A simple note at the begining of any document telling th ereader that familiarity with concepts X,Y and Z is assummed would in itself do a lot for ease of access. Then at least the reader knows where to look if they are lost.

In humanities, if one makes reference to an opinion or analysis, they make a big deal about citing properly. Obviously I’m not suggesting that absolutely everything must have an explanatory reference. But yes, it would be helpful if when mentioning cartan forms, the author could provide a succinct reference explaining what they are. That is, if one wants to make the paper more accessable.

18. **Alejandro RIvero**
March 1, 2006

About evidence, well, what I collected in the last three years is evidence that physicists are not intereested on evidence (a recent example I have already told: I was the first one -after six months- telling the ODG that their file http://pdg.lbl.gov/2005/mcdata/mass_width_2004.csv has a wrong value for the muon lifetime and others; the file is still (1/March/06) there but at least you can now get the corrected file by privately emailing to the pdg people; the point here is not a couple erratas, but that nobody noticed them, thus nobody had plotted the thing). At least not interested if it is not evidence for their theory; evidence is not anymore an starting point for research except if it is against SM beyond four sigma… there is a rare feeling of completeness, of impossibility to get anything new from the already measured data.

I find an interesting counterexample -or just a different way to do things- on the approach of Connes-Lott, that took pains to start with the standard model Lagrangian, and I remember Alain kept asking every practicioner about the differences between pole masses, running masses etc to be sure about how all the experimental thing could fit inside the model.

Also hep-ph people have different approach that hep-th, the -ph people keep launching models (either for the love of play or as tickets for Helsinki, you choose), but they do an effort to check against some main parameters.

19. **Theorist**
March 1, 2006

In the last week there were some papers where string theory makes strong predictions about the real world (check on the hep-th and you will find them). So things are moving, little by little string theory will overcome the landscape obstacle.

20. **woit**
March 1, 2006

Theorist,

I've looked at the recent hep-th papers and haven't seen anything like what you describe. It would be major news if there were recent strong predictions from string theory. What papers did you have in mind?

21. **ksh95**
March 1, 2006

ObsessiveMathsFreak, I really feel your pain. I clearly remember thinking the same things when I was an undergraduate.

But not to worry, there is light at the end of the tunnel. A Ph.D means that the government, or some other funding agency, has spent hundreds of thousands of dallors teaching you the precise definitions of those obscure terms. By the time you're done you will be able to read the vast majority of the literature like you read the newspaper.

Also, after completing a Ph.D you will understand that most of the things you found obscure as an undergraduate are actually trivial and writing (reading) those things would be a waste of your (the readers) time. Just think about it. To understand the term *superfluid* one should have taken a course on quantum fluids, which require courses on critical phenomena and field theory, which require courses on Statistical Mechanics, Classical Field theory, Mathematical Methods, and Quantum Mechanics, which require courses on classical mechanics and thermodynamics, which require classes on calculus and algebra…Or, put another way, go get Dr. in front of your name.

But, the main thing you'll come to understand is that everyone thinks about things differently. This means that while an intuitive wishy-washy explaination may make perfect sense to you, it may not make sense to others (especially if the others read the paper 100 years from now). For this reason it's necessary to have a set of well defined ideas, procedures, and their associated vocabulary. This way when Jack Sarfatti says

> The superfluid velocity field in Galilean relativity is the 3D Cartan closed inexact 1-form
> v = (h/m)d(theta)

we know precisely what he means; there is no ambiguity. Without this people like Rodrigues would never be able to "po-po" all over the paper.

22. **ksh95**
March 1, 2006

ObsessiveMathsFreak, I just thought of a better way to think about things. Imagine your in freshman calculus and your instructor decides to teach classes so elementary school children can understand (college freshman is to elementary school as Ph.D is to college freshman).
This instructor starts of by saying a derivative is the time rate of change of a function. A function is a mapping from one variable X to another variable Y, a variable is an abstract representation of some number, a number is an inumeration device, a mapping is…

Every freshman who already understands high school algebra just went to sleep.

23. **Troublemaker**
March 1, 2006

ksh95 has already responded very well to **ObsessiveMathsFreak**, so I will not embellish too much. The point is that yes, it is too much to ask for preprints and journal articles to cater in any way to the novice. They are not intended and should not be intended for newcomers to the field. They should not provide introductory material known to every expert, and they should not serve as a guide to purchasing introductory textbooks. If you are "disappointed by how inaccessible a lot of scientific papers are," then get a PhD, because that's who the papers are intended for. They're not meant to be toilet reading for the layperson, and the suggestion that they ought to be is ludicrous.

24. **D R Lunsford**
March 1, 2006

Douglas's "analogies" present an accurate demonstration of the analytical skill of the author.

1) In the period 1913-1930 one saw a continual improvement – by huge leaps – in the performance and sophistication of laboratories. It was hard to say which was advancing faster, experimental technique or theory.

2) The Bohr-Sommerfeld theory was well-founded mathematically and made definite predictions. Also, it provided a proving ground for applying relativity to quantum problems.

Peter did away with the chemical analogy, but this point is so vacuous that it's not even worth discrediting. For a theory with no vacuum, string theory makes a lot of empty claims.

On a happy note, it's good to see Sarfatti's name in the news 🙂

-drl

25. **A.J.**
March 1, 2006

An alternative for OMF: Instead of getting a PhD, read older papers. Many modern papers are inaccessible because they are part of a continuing conversation about some topic of mathematics. You can't pick them up and read them any more than you can expect two guys who've been talking for hours in a bar about some mutual passion to stop and explain themselves to you. But you can go back, and look at how a field got started. Usually, if you have trouble reading a bunch of different papers in a field, you'll find that they all reference some earlier foundational paper, where the basic vocabulary is introduced and explained.

To take two examples from one of my favorite subfields of mathematical physics: 1) Atiyah & Bott's paper "The Yang-Mills Equations on Riemann Surfaces" is more or less required reading if you intend to think about the relations between Yang-Mills theory and the moduli space of principal G-bundles. And you can find in there references to earlier works in the subject, reaching back to Narasimhan & Seshadri, and Mumford. 2) When Witten was writing in the late 70s and early 1980s, he couldn't assume that his colleagues knew very much of the basic vocabulary of differential geometry. So he spent quite a lot of time giving simple explanations of mathematical ideas.

Most mathematical papers do tell you where to look for an introduction to their language. If they don't tell you where to find a concept, you can always just try searching for it on wikipedia. There are some quite talented people (e.g. Borcherds) who've spent time writing expositions there.

Finally: Mathematical jargon was invented to make our lives easier, not to exclude laymen. If we'd didn't use it, we'd talk like treants: It would take days just to get through the first sentence.

26. **h**
March 1, 2006

ksh95 wrote:

"By the time you're done [with your phd] *you will be able to read the vast majority of the literature like you read the newspaper."*

I'm pretty sure that this is simply not true. Certainly not for the average phd student. Most certainly not in mathematics.

"Also, after completing a Ph.D you will understand that most of the things you found obscure as an undergraduate are actually trivial"

While I agree on this…

"and writing (reading) those things would be a waste of your (the readers) time."

…I'm not that sure about this one.
Also, while completing your phd, (hopefully) you will understand that there is at least 10 times as much new obscure stuff you wouldn't even dream about as an undergrad.

Basically I must agree with OMF on that papers should be more accessible in general (of course, I don't think that they should be at undergraduate level, that's impossible), and written with much more care.
Just compare an average preprint/textbook/conference speech with one by (insert names like Witten, Atiyah, Milnor, etc here). Difference in readability is like heaven and earth.

(sorry if this is very offtopic.)

27. **ksh95**
March 1, 2006

Hello h,
I have no problem reading most of the physics literature. Is the situation different in math?

Anyway, I strongly agree that papers should be written with more care, but that issue is an orthogonal issue to the conversation.

Back to the topic at hand. I was always taught that papers should be aimed towards the post-doc level. Any lower you bore people and any higher you confuse people. After all, the whole point is to advance the field. Advancement is optimal when we streamline communication between those doing the *"advancing"*. Of course communication to novices and laypersons effects replacement rates and funding, but these are secondary effects.

28. **Thomas Larsson**
March 1, 2006

Perhaps Theorist is referring to hep-th/0602286, where Dienes claims that a small CC naturally leads to the standard model gauge group.

29. **ObsessiveMathsFreak**
March 2, 2006

> Imagine your in freshman calculus and your instructor decides to teach classes so elementary school children can understand (college freshman is to elementary school as Ph.D is to college freshman).
> This instructor starts of by saying a derivative is the time rate of change of a function. A function is a mapping from one variable X to another variable Y, a variable is an abstract representation of some number, a number is an inumeration device, a mapping is…

Ironically, this is actually how first year calculus classes are in fact taught. At least to mathematics students.

There's probably a difference here between physics and mathematics papers, the latter of which I'm more familiar with. Though I understand that a lot of theoretical physics papers are quite mathematical.

My basic point was that many papers are in fact quite obfuscated by terminology and terseness. Obviously you cannot expect everyone to be able to read your paper, but at the same time it's a little unreasonable to expect everyone who reads it to be an expert in the field with a photographic recall of terms and definitions. A paper should, in as far as it is feasable to do so, be as modular and self contained as possible.

30. **J.F. Moore**
March 2, 2006

I think what people are trying to tell you, OMF, is that it just isn't possible. What you get in papers IS the lingua franca that has been established for a given field. Human knowledge in science at least has become far vaster than one person can ever hope to grasp.

A brief tip that works in my field might also apply to maths – read important dissertations (or parts thereof) in your subject. You can get many via UMI if your library has access. I did this a lot in my early years to get a leg up and be able to comprehend papers. Also, you also MUST talk to people about specific papers, preferably ones that you're both interested in. When starting out it's common to have a lot of misconceptions that can hamper understanding, and talking to someone else can often cut through these things quickly.

31. **woit**
March 2, 2006

Thomas,

I'm aware of the Dienes paper. He's looking at the statistical properties of 10^5 models, ones that are unstable beyond tree-level, so they are not even minima of the landscape. In addition, saying anything about the properties of 10^500 models based on looking at 10^5 very special ones of them doesn't make any sense.

On top of this, what he finds is a preference for lots of low-rank factors in the gauge group, which favors things like the standard model group over GUT groups, but he gets lots of these factors. To the extent there's any kind of "prediction" (which there isn't…), it would be for something like 8 copies of U(1) and SU(2), not one of each.

Claiming that any of this is a "strong prediction about the real world" is just absurd.

32. **Theorist**
March 2, 2006

I was refering to some papers which appeared last Friday. They might be harder to read and understand but they contain very solid results, not assumptions and guesses…. Solid mathematics and solid physics are used in at least one of them and the pheno predictions based on string theory are testable at LHC.

33. **woit**
March 2, 2006

Theorist,

If you'll be so good as to tell us which papers you are referring to we can discuss this further. I took a quick glance at last Friday's papers, and none of them seem to contain anything like a solid prediction of string theory testable at the LHC (and if they did, this would be big news). I'm not going to waste any more time trying to guess what it is you are talking about.

34. **Juan R.**
March 2, 2006

Obviously, Douglas talk is wrong in several historical, epistemological, and technical details. Some points were cited here therefore i will not repeat them.

Douglas's chemical analogy is rather outdated if one interprets it in a traditional reductionist way, especially when he uses the word "deriving"… Their perception is a beautiful example of why string theory is in the very wrong way. The lot of funding for obtaining a TOE from string theory is just lost money…

As stated by Jean-Marie Lehn (Nobel Prize for macromolecular chemistry), one cannot derive chemical phenomena in bulk from a knowledge of constituents; Lehn clearly states there is not reduction of a hierarchical level to the other, just integration between the different levels [1].

Douglas cites condensed matter physicists; well, P.W. Anderson [Nobel prize for condensed matter physics] said that condensed matter physics, for example, is NOT applied particle physics [2]. A full understanding of atoms (atomic physics) is not sufficient understand solids, and doing progresses in the field. It is more, Anderson cites examples of mainstream atomic knowledge leading to wrong conclusions in condensed matter.

Anderson's "More is different" has become a common "mantra" in condensed matter physics but is often ignored by particle physicists, stringers, and other reduccionists. It is natural to read Anderson's criticism to believe on string theory as The Final Theory (the main "mantra" of stringers). Anderson considered string theory "a futile exercise as physics" the last year.

Even the "reduccionist" Murray Gell-Mann (who is often cited by stringers as supporting the "beauty" of string theory) said that string theory -even if finally correct will be NOT a theory of everything [2].

The analogy with chemistry is rather surprising. Douglas says: "On further developing these analogies, one realizes that we do not know even the most basic organizing principles of the stringy landscape. For the landscape of chemistry, these are the existence of atoms, the maximal atomic number, and the facts that each atom (independent of its type) takes up a roughly equal volume in three-dimensional space and that binding interactions are local."

What is really the link of above with string theory except as prose for more funding on string theory during another ineffective 40 years?

Chemistry is a science; string theory is not. Moreover, knowing Douglas CV, I seriously doubt that Douglas knows that and atom is, maybe the union of an attractor and its basin Michael? 🙂

About Landscape, very little can be said those days. Landscape just reflects the desperation in the field. People took wrong ways in the past and now are in a dead end. There is not possibility for computing the CC from the Landscape, and even if they was done (via a new "revolution"), the prize would be too high for science.

I personally would prefer computing billions of things from standard science and leave the CC as a mystery before computing the CC via Antrophic/Landscaping arguments and then to predict nothing of the rest of the world.

Douglas states in his conclusions that

"We believe string theory has a set of solutions, some of which might describe our world."

That is all science behind stringers work. "We believe… might…".

1) These kinds of arguments are being repeated since 40 years ago. We are a bit saturated and is time for the research in other directions.

2) String theory does not work, it newer worked, point. M-theory is not even formulated (if they nonperturbative regime really exists).

3) Mass media, public, undergraduate students, funding agencies are misguided about real success of string theory (i.e none).

In short, we obtain a "theory" (string theory) is not theory (M-theory is even undefined), the "theory" explains/predicts nothing can be explained using GR+SM, offers wrong experimental bounds for many phenomena (e.g. wrong causalitty structure for GR, wrong basic symmetries for the SM at low energies, wrong non-zero T bosonic behavior, etc) and, for obtaining certain bounds on the CC, it needs completely break that satisfactory item we call science.

References:

[1] See Lehn introductory chapter to "Chemistry for the 21st century" (Ehud Keinan; Israel Schechter; Eds.) Wiley-VCH, 2001.

[2] Anderson, P.W. Science 1972, 177, 393-396

[3] Gell-Mann, Murray. The Quark and the Jaguar: Adventures in the Simple and Complex. W. H. Freeman; 1995.

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Juan R.

Center for CANONICAL |SCIENCE)

35. **D R Lunsford**
March 2, 2006

Understanding the landscape that created the landscape…

http://www.opinionjournal.com/extra/?id=85000550

-drl

36. **Chris W.**
March 2, 2006

This paper should be widely read, and I expect it will be:

Lorentz Invariance Violation and its Role in Quantum Gravity Phenomenology, by John Collins, Alejandro Perez and Daniel Sudarsky [hep-th/0603002]

The authors also make some important general observations about Lorentz invariance violation (LIV), or rather the apparent lack thereof, and the foundations of quantum field theory.

Thanks to Christine Dantas for posting about this. See the excerpts in the comments on her post.

[I know this is off-topic, but the Landscape is so depressing I felt compelled to change the subject (again).]

37. **Chris Oakley**
March 3, 2006

There is another paper (http://arxiv.org/abs/hep0603010) today about Lorentz invariance violation. It was not so long ago that anyone who said SR was wrong was considered a crackpot. But then again, it was not so long ago that one would have been considered a crackpot for suggesting that we live in one of the few universes out of 10^500 that support life.

38. **Thomas Larsson**
March 3, 2006

Douglas has at least created a great new metaphor – a nightmare.

39. **D R Lunsford**
March 3, 2006

This talk about Lorentz violation is silly in this context. In the regime where we abandon Galilean invariance, the reason is not symmetry breaking, but "decontraction", which is very different. So if Lorentz symmetry doesn't hold up, the reason will not be another shell game of symmetry breaking, rather, recognition of another regime for matter and spacetime considered jointly.

-drl

40. **D R Lunsford**
March 8, 2006

I notice no one here is sharp enough to even offer an argument with what I stated, that is, you don't even understand the problem. The problem with the world is, it's filled with mediocrities.

-drl

41. **Chris Oakley**
March 8, 2006

Hi Danny,

I may well not be sharp enough to offer an argument: I did not really understand what you were saying. Can I repeat my suggestion that you collect your best thoughts on physics and put them on a web site, like I did?

42. **D R Lunsford**
March 10, 2006

Chris, you were not in mind, and if anyone could understand it, you could – it goes like this…

One can violate a given symmetry by making more symmetry in a larger context, and then assigning the several old symmetries to their own rooms in the new one. If we make a door between rooms, one room from two, we "decontract" the original symmetries. See here:

http://arxiv.org/abs/hep-th/0106273

The key point is that, contractions are associated with dimensional parameters assigned to groups (Lorentz->Galilei = speed of light; Poincare->Lorentz = Planck length, etc.) while decontractions are associated with assigning a group to a dimensional parameter, not a one-to-one process and so inherently observational. Superficially this is like symmetry breaking – the difference is that instead of adding fields (Higgs mechanism) we “push back” the idea of vacuum itself by introducing a new fundamental symmetry, incorporating the originals by allowing a contraction to recover them. The decontraction looks like the original in some regimes (again like symmetry breaking) because the enlarged vacuum is “stiff” in the dimensional parameter (speed of light is very large; Planck length is tiny etc.)

This is really why string theory is so lame – instead of pushing back the vacuum to incorporate matter as we know it from experiments, it makes matter even more obstreperous by removing its locality.

-drl

-drl

All Sorts of Stuff

March 3, 2006
Categories: Uncategorized

Harvard mathematician Barry Mazur has many new things up on his web-site. These include an expository article on motives (this via David Corfield), and a foreword to a forthcoming popular book called Fearless Symmetry : Exposing the Hidden Patterns of Numbers. This book looks to be the first popular book to deal with the modern use of group representations in number theory, explaining what reciprocity laws are and a bit about the Langlands program.

The concept of a motive and that of using representations of Galois groups constructed using things like motives are two of the most important ideas in modern number theory. Some of the latest developments in this field concern extensions of the Langlands program involving p-adic modular forms. Mazur and Buzzard are teaching courses on this topic, and Mazur has put some lecture notes up on his course web-site. These courses are part of a special semester on Eigenvarieties at Harvard.

Mazur also has a revised version of his expository piece on category theory, a short piece about Serge Lang, and some very non-basic notes for a "Basic Notions" seminar.

Nature has a short article entitled Physicists told to confront those big questions about the Foundational Questions Institute call for proposals. There's a very positive quote about this from Lee Smolin, who is on the advisory board, but also a much more skeptical one from Paul Steinhardt: "Metaphysics is running rampant through string theory and cosmology," he says. "I would like to see things go a little bit in the other direction."

Kimball Martin's web-site of Exceptional MathReviews includes one by Robert Oeckl about a paper of the Bogdanovs. For more about them see here, here, here, and here. Remarkably, they seem to have some support from at least one string theorist.

Via Bitch Ph.D., perhaps the strangest math-related web-site I've ever seen.

The High Energy Physics Advisory Panel (HEPAP) is meeting today and tomorrow in Washington. Presentations there will include one from Bush's science advisor, John Marburger, and should be available on-line soon.

This weekend, the Mathematical Sciences Research Institute (MSRI) will be dedicating its new building, named after Shiing-Shen Chern. As part of the festivities, Roger Penrose will be giving talks, including a public lecture Sunday on Fashion, Faith and Fantasy in modern theoretical physics.

The second LHC Olympics were recently held at CERN and the talks are available on-line. This is a rather unusual exercise designed to get string theorists and other not-so-hardcore phenomenologists involved in analyzing simulated LHC data. There seem to me to be two big problems with this. First of all, there are no backgrounds in this simulated data, and understanding these backgrounds is going to be the main problem with the LHC data for quite a while. It will be the experimenters who will have to do this, and they may need help from theorists, but help of a very different nature than what this exercise is aiming at. Secondly, once backgrounds are understood and potential signals are extracted, I doubt that the LHC experimenters will be releasing the kind of data simulated here for use by theorists. I suspect they'll be doing the kind of analysis going on at the LHC Olympics themselves and releasing the results as papers under their own names. For more about this, see postings by Lubos here and here. The first of these drew the following comment from a European phenomenologist:

Phenomenology has been always been strong in Europe, hundreds of ppl work in this field since decades, and most consider this contest as child's play. Guess why no European team took part in this activity, right within a truly European institution which has scores of local phenomenologists? It is a bit like if a few phenomenologists decide to learn string theory and organize a contest in Princeton about who can build the best string model….

While this exercise is unrealistic, it may at least clarify various issues about how testable certain specific scenarios really will be at the LHC.

The latest Seed Magazine has some interesting articles. One about mathematical proof discusses problems with proofs in mathematics that may be too complicated to be properly refereed, especially the recent work by Thomas Hales on the Kepler conjecture. It ends with a depressingly silly comment by Keith Devlin:

I see a parallel between the uncertainty of these proofs and developments in physics like string theory, where we're developing mathematical theories of matter that may forever remain elusive to experimental verification.

This is the same kind of foolishness as the comment that "physicists may have to rethink what it means for a theory to explain experimental data" because of string theory (see here).

Another Seed article that has gotten a lot of attention because of it's topic is one called Getting Physical about physicists and sex. See commentary here, here and here. This last link is to Jennifer Ouellette's new blog Cocktail Party Physics, which is of independent interest.

Comments

1. **Dumb Biologist**
   March 3, 2006

   I'm confused. I see nothing especially bitchy about sarongs.

   Also, that physicists get it on is hardly news. Shrödinger was famous for carting both is wife and mistress along with him long ago, and Feynman, having developed considerable expertise on the subject, published advice on how to pick up girls in one of his books. Carl Sagan wrote about sex and drugs, specifically how the latter impacted the former. I hear some lovers of boyish, brainy men openly lust after Brian Greene, so a few also seem to have a bit of that rock star mojo. My wife certainly expressed her admiration after seeing the jacket of my copy of "Elegant Universe". C'mon Seed, tell us something we don't know…like, maybe, Hey, what are we biologists, chopped liver?

2. **Dumb Biologist**
   March 3, 2006

   Oh, I forgot Kinsey. Never mind.

3. **ObsessiveMathsFreak**
   March 3, 2006

   A great amount of mathematical proofs are often presented in a terse and reticent fashion, and the resulting proofs, though they may be correct, can only be understood by those who have already reached a deeper understanding of the topic.

The problem here is that people are misinterpreting the meaning of the word “prove”, in mathematics. To prove means to demonstrate, through argument or evidence, the validity of a statement or proposition. What that means, is that you have to convince another human being that what you say is true.

If your proof is too convoluted, too terse, or too complicated and no one is able to understand it, then you haven’t proved anything. You’ve just scribbled some symbols on paper and said they were true. If you simply brow beat people into submission, through complicated proofs or your intellectual reputation, then they have only accepted that your proof is true. They haven’t accepted the proof itself, And in this case, you haven’t proved anything at all except human fallability.

To prove something, another human being has to be able to read, understand and follow every single step of your argument, from propositions and assumptions all the way to your conclusions. And if no one can do that, your proof is not a proof. It’s just ink on paper, and science is getting nowhere.

Doubt is the worst of all problems to be faced with in mathematics. If you doubt something, even one thing, in your equations, the whole structure begins to teeter and topple in your mind, and every result you produce becomes tainted with reasonable doubt. In other words, you’ve stopped being a mathematician, and even a scientist, and you’ve just become a philosopher.

Certainty is the key. Without it, without assurred falsifiability, mathematics isn’t a real science anymore. It becomes, like psychoanalysis, a subjective endevour, where people accept what they want to accept, and there are no absolute truths anymore. This can happen to any branch of science, including physics, if falsifiability is abandoned.

4. **Dumb Biologist**
March 3, 2006

I wasn’t aware that the gold standard for mathematical proof was falsifiability. Rather, I thought it was logical consistency based upon accepted axioms.

5. **Eric Dennis**
March 3, 2006

“Metaphysics is running rampant through string theory and cosmology”

There’s an unfortunate equivocation on the term “metaphysics” that tends to pop up. Sometimes it’s supposed to suggest a Rube Goldberg mechanism of questionable verifiability, like the 19th century ether models, and other times it refers (loosely) to philosophical or foundational questions about the meaning of important theoretical concepts, e.g. “measurement”, or the precise definition of theories. String theory involves much of the former, but not much of the latter.

I take it Steinhardt is against the former, and Smolin is for the latter.

6. **Aaron Bergman**
March 3, 2006

Sometimes [metaphysics is] supposed to suggest a Rube Goldberg mechanism of questionable verifiability

Anyone who uses it that way is wrong. I’m fairly sure Steinhardt is against the proper definition. It’s all too easy to spend your time thinking about “big questions” and writing lots of papers with few formulae in them. It’s almost always a better idea to go out and calculate something.

7. **Eric Dennis**
March 3, 2006

Going out (or shutting up) and calculating can be a smart move if you’ve got a solid theory that’s been pumping out a lot of impressive results. But when the flood becomes a trickle and twenty years later the trickle has dried up, maybe it’s time to start questioning the shut-up-and-calculate mantra. You don’t even have to write lots of papers with no formulas. Just write one. And if you can’t muster enough interest to spend an afternoon in your basement, don’t pretend that wishing and praying and calculating will keep the termites away.

8. **Aaron Bergman**
March 3, 2006

One quickly learns that anyone can have lots of “deep thoughts”. Nobody cares. Until you have deep thoughts about which you can calculate something, people generally and rightfully aren’t going to be interested.

9. **Chris W.**
March 3, 2006

And then there are shallow (or incoherent) thoughts made to look deep by cloaking them in sophisticated calculations, and formalism in general. Einstein once said about an irksome minor colleague that “the man can calculate but he can’t think.” Excessive emphasis on calculations tends to encourage such people, while discouraging people who can really think—ie, those who are trying to arrive at important insights into what *ought* to be calculated, and why (and in many cases, how).

In a reminiscence about Einstein during the centennial year (1979), Chern once remarked that their conversations and his own study of Einstein’s work had led him to conclude that the singular difficulty of research in physics—and perhaps in any empirical science of real depth—is arriving at the right *formulations* of questions and problems. (Mathematicians, he said, generally start by selecting from among several clearly stated problems, hopefully demonstrating some good taste in their choices.)

10. **Aaron Bergman**
March 3, 2006

The point of physics is to calculate stuff. If you can’t do that, then you’re doing philosophy. Figuring out what to calculate is sure one of the most difficult problems out there, but you’ve already conceded in that formulation the necessity of the calculation.

11. **Anonymous**
March 4, 2006

Chern once remarked that their conversations and his own study of Einstein’s work had led him to conclude that the singular difficulty of research in physics—and perhaps in any empirical science of real depth—is arriving at the right formulations of questions and problems. (Mathematicians, he said, generally start by selecting from among several clearly stated problems, hopefully demonstrating some good taste in their choices.)

Umm. Grothendieck, anyone? Finding the right formulations of things in mathematics is crucial and drives a lot of important research.

12. **Thomas Larsson**
March 4, 2006

The shut-up-an-calculate approach works great, unless something is missing in the usual calculation schemes. E.g., if Feynman had followed that philosophy as a young man, he would have found that electron g-2 was infinite and given up. Instead, he invented path integrals and coinvented renormalization, which certainly were creative acts rather than rote calculation. Of course, once the new calculational scheme was in place, you could go back to turning the crank.

OTOH, it is unlikely that deep thoughts about the meanings of diffeomorphism invariance or quantum measurements will lead to progress by themselves. Some hard modification of the formalism itself also needed, and that can be found even if the interpretation is only approximately right.

13. **A Phenomenology Grad Student**
March 4, 2006

Peter,

Your criticisms of the LHC olympics arise frequently, and they are facile. It is easy for an outsider to criticize, but the "hardcore phenomenologist" critics who have seen the details of what goes on at these meetings seem to have been convinced. (For instance, I understand that Tao Han was initially skeptical, but was very pleased with the recent meeting at CERN.) So let me offer some responses:

This is a rather unusual exercise designed to get string theorists and other not-so-hardcore phenomenologists involved in analyzing simulated LHC data.

Well, partly, yes. But look at who put the most effort into these things: two string theorists, Verlinde and Rastelli, at Princeton (who showed impressive awareness of collider physics issues), a large group of Harvard phenomenology grad students with varying degrees of experience in thinking about collider physics, a couple of Cornell phenomenology grad students with a fair amount of experience thinking about collider physics, and Matt Strassler, who manages to be both a string theorist and a collider phenomenologist. So, your characterization is only halfway accurate. The goal is partly to educate the string theory and model-building communities, but not everyone involved was as ignorant as you imply.

There seem to me to be two big problems with this. First of all, there are no backgrounds in this simulated data, and understanding these backgrounds is going to be the main problem with the LHC data for quite a while. It will be the experimenters who will have to do this, and they may need help from theorists, but help of a very different nature than what this exercise is aiming at.

Yes and no. Early in the LHC run it will be very important to calibrate the machine, to "rediscover" the Standard Model, and so on. But important discoveries tend to be made in channels where detailed knowledge of background is not needed. Resonances will be pronounced. Events with certain configurations of leptons and missing energy are very non-SM like and one can read off kinematic features. And so on. It's really an incredibly difficult computational challenge to produce a similar exercise with backgrounds (though hopefully this will occur to some extent with the next LHC Olympics), but it's not as if issues of background were totally ignored. Most of us tried to focus on features of the signal that would not be obscured by background.

You are of course correct that there are other issues if we need to rely on background-heavy parts of the data to extract information. This is a very difficult field and it demands focused effort on better loop calculations, better Monte Carlo, better PDFs, and all sorts of other things. No one denies this.

However, one thing that you find when staring at simulated signal samples is that sometimes it can be *very difficult* to interpret features in them. Lots of "hardcore" phenomenology involves taking models and discussing how to make measurements in them. The *real* problem of the LHC is to take measurements and make models, and it can be complicated. Kinematic features can arise in multiple ways. Certain apparent features of the data can lead you in the wrong direction. And so on. This exercise is not the trivial one that those who haven't attempted it make it out to be.

Secondly, once backgrounds are understood and potential signals are extracted, I doubt that the LHC experimenters will be releasing the kind of data simulated here for use by theorists.

Almost certainly true, I think. People tend to make this objection as if it defeats the purpose of the exercise. But it doesn't, at all. Partly there is the aspect of this that we are educating ourselves, as theorists, about what information the experimentalists will tell us and how to interpret it. But another important aspect, as I noted above, is that these sample analyses can be surprisingly challenging. And so far we've basically known we're looking at SUSY scenarios. See Matt Strassler's talk for an example of how the data can lead you astray. These exercises are not trivial. Sometimes good theoretical intuition is useful in figuring out what signatures one should try to isolate, given other signatures one sees. This is not obviously something that experimentalists would be as good at, though certainly some experimentalits could do this. If we see something other than vanilla MSSM, though, it becomes a much more pressing issue.

But the most important thing, at least from my perspective, is that we can hope for genuinely useful new ideas to come out of this. There's a somewhat limited toolkit of kinematic distributions to analyze, plots to make, and so on. The exercise shows these tools are very useful in some cases and in others they're probably not optimal. Getting a bunch of reasonably intelligent theorists to stare at data and run up against various surprises and challenges will, hopefully, lead some of those theorists to start creating new tools, suggesting unorthodox approaches to data, examining signals no one has bothered to think about so far, and writing papers on such things. And I think it's clear that there's a pretty rapid change taking place: collider phenomenology is being pursued by more people, it's becoming more respected, it's reshaping the theory job market. These workshops are a small but not negligible part of all of this.

Of course my defense of the LHC olympics sort of assumes a certain bias. If the world at the TeV scale really is just SM + Higgs, or if it is just the MSSM in a particularly nice well-studied part of the parameter space, none of this is necessary and the experimentalists probably have what they need. But if you think there might be interesting surprises, there is a strong need for original thinking about how to approach the data, how to discriminate different models that have similar signatures, and so on.

Anyway, I've rambled on for too long.

14. **D R Lunsford**
March 4, 2006

Speaking of data that isn't here yet – whatever happened to the second release of WMAP data?

"Bitch PhD" – right. Wow. A banner with a smart ass little kid giving me the finger. OK then. Have a nice day!

-drl

15. **EUPH**
March 4, 2006

Dear Ph Grad Student,

your post can be perceived as: "unless understanding LHC data is just running MSSM codes, intelligent theorists should do the job". This sort of attitude partly explains why LHC olympics at CERN was attended almost only by US theorists.

16. **Aaron Bergman**
March 4, 2006

Instead, he invented path integrals and coinvented renormalization, which certainly were creative acts rather than rote calculation.

Who's arguing for "shut up and calculate" or "rote calculation"? It would be nice if people actually read what I said.

17. **A Phenomenology Grad Student**
March 4, 2006

your post can be perceived as: "unless understanding LHC data is just running MSSM codes, intelligent theorists should do the job".

I have as much respect for good experimentalists as anyone, but if the data contain puzzling surprises (as we all hope will be the case, I think), it's good to have feedback between theory and experiment. I'm not claiming theorists will do everything, I'm claiming it's good to have theorists suggesting new interpretations, other signals to study, ways to cut out background, etc. Just look at the more complicated experimental situations we already have, e.g. issues of meson spectroscopy in B physics or charm physics. There is a lot of useful feedback between experimentalists and experimentally-aware phenomenologists in those fields. This isn't like discovering the W or Z where you know exactly what to look for.

In any case, what is clearly useless is bickering on the Internet, so I'll shut up now.

18. **QWERTY**
March 4, 2006

I LIKE BARRY MAZUR'S STYLE OF EXPOSING – IT IS POETIC AND ERUDYTE AND SCHOLARLY AND FUNNY. IT REMINDS ME OF HERMANN WEYL, WHO SAID FUNNY THINGS ABOUT "THE YOKE OF A FOREIGN LANGUAGE WHICH WAS NOT SUNG AT MY CRADLE". MAZUR IS TRUELY A GENTLEMEN OF THE FOUR MOST DISTINCTIONS.

19. **woit**
March 4, 2006

EUPH,

I think you're being kind of unfair to Ph Grad Student, accusing him of an arrogance that isn't really there.

Ph. Grad Student,

Thanks a lot for your long and thoughtful comment, it's the kind of response I hope to get here to issues like this that I bring up. I didn't comment extensively on the LHC Olympics mainly because I'm not a phenomenologist (although long ago spent time working on HEP experiments) so not expert on these issues. One reason I did write something brief here was that I saw from Lubos's blog that others seemed to share some of my reaction to this. It certainly wasn't my intention to imply that anyone involved was "ignorant". However, my initial reaction when looking through the web-pages about this was kind of negative for a couple reasons, but mainly that the emphasis on looking for MSSM signatures seemed to me misplaced. Part of this is that I think it is very unlikely that this is what the LHC will find, but more generally this particular problem is one that I suspect has been already beaten to death. Phenomenologists and Tevatron experimentalists have been working on this intensively for more than 20 years and by now I assume the experimentalists have a high degree of expertise built up on this problem. Going from the Tevatron to the LHC should introduce some new issues, but those don't seem at all to be the ones considered here.

The fact that things are going badly for string theory and that the LHC start-up is coming soon appears to be causing the attention of many particle theorists to move from string theory to phenomenology. Some of this is a good thing, but I hope some of the unfortunate characteristics of string theory research (trendiness, arrogance, unrealistic and wishful thinking) don't just move into this area. The whole idea of calling this sort of educational exercise for theorists an "Olympics" and surrounding it with hoopla kind of puts my teeth on edge, and I gather others share this reaction.

I'm sure that the people involved learned a lot, and maybe it will lead some of them to ultimately be in a position to make real contributions to LHC physics, that would be great. But I also hope the whole field won't move into this. It may be much more important to have some good theorists continuing to come up with new speculative ideas for things to look for at the LHC, rather than immersing themselves in the minutia of looking for MSSM signatures. I'm sure Witten could do well at this kind of exercise, but I think it would be a waste of his most impressive talents, which lie elsewhere. The same may be true for many of the best string theorists.

20. **A Phenomenology Grad Student**
March 4, 2006

Peter wrote:

However, my initial reaction when looking through the web-pages about this was kind of negative for a couple reasons, but mainly that the emphasis on looking for MSSM signatures seemed to me misplaced. Part of this is that I think it is very unlikely that this is what the LHC will find, but more generally this particular problem is one that I suspect has been already beaten to death.

Unfortunately this is largely a technical limitation. Tools for generating Monte Carlo data for the MSSM are very well-developed (though they still lack certain desirable features). Tools for generating MC for other models are in a much more primitive state. They are improving, and I think they will play a role in the next round of these workshops.

However, even in the MSSM a lot of the effort has focused on certain limited regions of parameter space. The Michigan black box in the LHC olympics exhibited an unusual spectrum, where the main signal was events with 4 b jets and missing Et and the LSP was Higgsino. This isn't the sort of thing one finds many studies of!

It may be much more important to have some good theorists continuing to come up with new speculative ideas for things to look for at the LHC, rather than immersing themselves in the minutia of looking for MSSM signatures.

Well, as I mentioned it won't be just MSSM. Personally I find a lot of the increasingly baroque models that are being built somewhat distasteful, but in any case it's clear that many people are going to keep trying to guess the right model in advance. The things that I think require more effort are model-independent approaches for the possibility that we see something different from any model that has been built, and studies of degeneracies (i.e. how very different models can produce very similar experimental signatures) and how to design observables that break them. These things have been discussed at these workshops as well.

21. **Juan R.**
March 4, 2006

A note, metaphysics literally means "beyond physics". The meaning today it would be better expressed as metascience.

Paul Steinhardt quote "Metaphysics is running rampant through string theory and cosmology" is focusing in the lack of verification of some ideas developed (and asumed to be true) in string theory and cosmology (e.g. inflation).

In rigor, cosmology is not one of natural sciences becasue do not fit all requirements of the corresponding scientific method.

—
Juan R.

Center for CANONICAL |SCIENCE)

22. **EUPH**
March 4, 2006

Peter, I was not accusing Ph Grad Student of arrogance. I was pointing out that some aspects of the activity (included in his/her description) can and have been perceived as arrogance.

23. **Chris Oakley**
March 5, 2006

Posted by John Baez on sci.physics. A foreshadowing of things to come at the LHC –

> SUPERMODEL SUES SUPERPARTNER FOR SUPERSUPPORT
>
> March 4, 2006
>
> Beverly Hills, CA – In entertainment circles today, everyone was talking about supermodel Betty Boson, who has filed a palimony lawsuit against her superpartner, movie mogul Freddy Fermino.
>
> "At first I thought we were a perfect match," said Boson, "but he kept gaining weight and eventually he was never there for me. It was like he didn't even exist. I can't go on like this." Fermino was unavailable for comment.

24. **worm**
March 5, 2006

Is there a page where i can see the table of contents of Fearless Symmetry : Exposing the Hidden Patterns of Numbers ?

25. **csrster**
March 6, 2006

I know of one high-flying cosmologist who allegedly ensures dometsic tranquility by insisting not only on conference travel expenses for himself and his mistress, but also for his wife and _her_ lover.

26. **Michael**
March 6, 2006

Peter, your ideas have been scooped. Smolin et. al. present their new model of particle physics, and it is obvious to any educated physicist that it is equivalent to your off-diagonal SU(2) model. Don't say I didn't push you to publish your ideas, or that I didn't offer my help. What are you going to do now? Don't give up. You can still play the GUITAR and become a pop-star. 😉

27. **repulsed by Michael**
March 6, 2006

Michael, it's easy for a nasty, little man like you, a coward who remains anonymous, to mock Peter. What original physics ideas have you ever produced? I bet NONE is the answer. I bet you are a pompous snob with little talent for research. One of those academic politicians/parasites who achieves tenure by never rocking the boat, by studiously brown nosing your peers. I bet your entire ouvre consists of trying to make minor punctuation improvements to Witten's papers.

28. **Michael**
March 6, 2006

Dear repulsed by me,

if I am a coward for remaining anonymous, I trust you will immediately catch up with your little gaffe and post your full identity in a verifiable way.

I'm not here to compete, bragg or be admired, so your guess work concerning my academic contributions is irrelevant. Much less am I here to be nice. Peter needs some serious headwind, because his criticism of string theory is disingenuous. It reminds me a lot of Fahrentheit 9/11, mix a few facts with a lot of lies and make a big PR buzz from it. If his opinions were part of a scientific discourse, he'd probably be in touch with me via less public channels of communication, and the discussion would be evolving with time. What he does instead is to mislead laymen in order to create an audience for himself and satisfy his vanity. His arguments do not meaningfully evolve with time, because he simply attacks anything associated with string theory in a Pavlovian way, often using inconsistent or circular reasoning.

You're line about the politicians/parasites is really cute. How many tenured physics faculty can you count at the big Boston area universities, whose contributions aren't genuinely important? Oh wait, can you count at all?

29. **ObsessiveMathsFreak**
March 7, 2006

> His arguments do not meaningfully evolve with time…

Well that's OK then, because by the sounds of things, the rebuttals don't seem to be in significant flux either!

30. **Michael**
March 7, 2006

How could they? Rebuttals to repetitive stupidity are exepcted to be repetitive. Go figure.

31. **anon**
March 7, 2006

People offer constructive criticism here. Michael would fit in better over at Motl's blog.

32. **D R Lunsford**
    March 7, 2006

    I'm the only person here who despises these synthetic creatures so viscerally that I deserve to have my comments deleted. Don't blame the owner of this joint.

    I am repelled down to my last atom by these clowns, and what they've done to science. I wish to God someone with balls and authority, like Dirac, were alive.

    -drl

33. **Chris Oakley**
    March 8, 2006

    I don't see these people so much as hateful as irrelevant. Maybe they are relevant in the short term, but their impact on physics is likely to continue to be nothing at all, unless you count proving that something that looked like a blind alley as really being a blind alley as adding to the store of human knowledge.

    [NB: Here I expand on a message I sent to Danny yesterday:]

    Peter put his finger on it a while back in comparing the research practices of mathematicians to those of theoretical physicists.

    I think that the problem is that we are still living in the shadow of Feynman. Here was a disorganised, impetuous maverick who could solve problems that no-one else could. He would jump in there, solve the problem and then move on to something else, leaving others to clean up behind him. The reason why he could do this, you will note, was not because he was a disorganised, impetuous maverick, but because he was brilliant. Anyone else conducting investigations in this way including (nay, especially) me would end up with a mess, and nothing more.

    The problem is that Feynman was the ultimate trendsetter for theoretical particle physics research. The smartest people in the subject still try to be like Feynman, looking for the next crazy leap and never being much concerned with rigour, or building the big picture. It reminds me of the sad people working the one-arm bandits in casinos: having won the jackpot once, they think they can do it again and again, not realising that their average returns are less than zero.

    The mathematicians, on the other hand, do not have the gambler mentality, and are content to work quietly away at a problem for years with hardly anyone paying attention to them until they publish. The result of this is slow, but steady progress. The result of the particle physicists' enterprises, on the other hand, is just a series of big, bold ideas that did not work.

    Which philosophy do *you* think is more workable?

34. **D R Lunsford**
    March 9, 2006

    Hi Chris, I didn't receive any mail.

    I'd say the problem is generational mediocrity resulting from a strong tendency toward pernicious societal narcissism combined with overwhelming negative anima projections, resulting in a abandonment of personal intellectual rigor in favor of a phony self-actualization, and a loss of discrimination, the "good taste" and "common sense" that was provided by the lost, projected anima. It's not just science that is suffering. Every institution from baseball to Catholicism seems to be in crisis.

    So I think it has more to do with "me generationism" than it does with a particular scientist.

    Try email again, thanks,

    -drl

35. **Daryl McCullough**
    March 9, 2006

    Barry Mazur's article "When is one thing equal to another thing?" is in memory of Saunders MacLane. I didn't realize he was dead.

    According to the Mathematics Genealogy Project, Saunders MacLane is a direct descendent of Gauss (Gauss was MacLane's advisor's advisor's advisor's, etc. advisor).

36. **D R Lunsford**
    March 10, 2006

    Daryl,

    I still have Birkhoff and MacLane in the Chelsea/AMS edition, a wonderful book!

    -drl

Yet More On ArXiv Trackbacks

March 6, 2006
Categories: Uncategorized

Several blogs today have discussions of the arXiv trackback issue, mostly spawned by Sean Carroll's posting on the topic.

Jacques Distler has a posting where he explains that the arXiv has instituted an ill-defined "active researcher" criterion for allowing trackbacks to blogs and that:
Peter Woit's publication record doesn't put him anywhere close to "active researcher status"
I've written an extensive comment over there about why I happen to think I am an active researcher and the evident absurdity of the idea the Jacques Distler is capable of making a rational evaluation of this question.

More discussion of this is at Chad Orzel's Uncertain Principles, and Georg von Hippel's Life on the Lattice.

Update: Yet more on trackbackgate from Lieven le Bruyn , Evan Goer and Capitalist Imperialist Pig. The first has the useful suggestion of using Technorati to automatically generate complete sets of links to discussions of arXiv papers on blogs. The second reminds me why it's a bad idea to hit the "submit" button when you're extremely pissed off.

Update: In discussion over at Jacques's blog about what an "active researcher" is, the dicey issue has arisen that Jacques actually doesn't seem to be one himself. One commenter has suggested that this whole issue could easily be resolved by just picking a definition of the term, noting that

Few will object to defining a minimallly active researcher as one who has posted an average of 2 papers per year to arXiv over the last 3 years.

Jacques comes no where near qualifying as "minimally active", since he has only posted 3 papers to the arXiv during the last 3 years.

Personally I don't think the 2 paper/year criterion is very good. It doesn't account for length of papers, or that they may be the product of a collaboration, so the author in question is only responsible for a fraction of the paper. A more accurate measure would be based on counting pages of papers and dividing by number of authors. Under this measure, over the last four years Jacques's research productivity looks like this:

2002: 9.3 pages
2003: 3 pages
2004: 0 pages
2005: 23.7 pages

for an average of 9 pages/year (this count is being a bit charitable, since 15 of the 2005 pages are from a "landscape" paper that may not even be science).

Jacques has made it clear that a certain author for whom this number is 14 pages/year is not "anywhere close to 'active researcher' status", so I guess he is even farther away from qualifying as an "active researcher".

Always seems surprising the way people living in glass houses like to throw stones…

Update: This particular food-fight has even been written up for the on-line component of Discover Magazine.

Update: A couple more people weigh in on this, Jim Hu and Alejandro Satz.

Update: I haven't heard anything at all from anyone associated with the arXiv, but a couple trackbacks to one of my recent postings have appeared, so there seems to have been some sort of change of policy there.

Comments

1. **ksh95**
   March 6, 2006

   Lee Smolin over at cosmic variance very elequontly said:
   If academic freedom means anything, it must mean that the university must do nothing to impede free discussion by professionally competent experts on scientific controversies. Given that Peter Woit is a Physics Ph.D. and a faculty member at a major university, who has published papers and has a book in press on the topic, he is without doubt part of the academic community to which the principles of academic freedom apply.

   End of discussion

2. **Dumb Biologist**
   March 6, 2006

   Dr. Woit,

   Just a suggestion:

   My initial suspicion that it was useless to argue about the "active researcher" criterion, idiotic or no, was I doubted the argument would make any difference. Hopefully I was wrong, and it looks as if maybe I was. Some folks even diametrically opposed to your position on ST clearly don't think much of the policy, and some have even gone so far as to insinuate it's likely a load of b.s. contrived as a pretense to do exactly what you claim: Suppress your point of view on less-than-ethical grounds.

   I'd be having a Catagory 5 conniption were I in your shoes.

   I'd also hope somebody threw a bucket of cold water on me before I fired off too many expressions of indignation, righteous or no, in that frame of mind. It's something I've found out the hard way.

   Anyway, best wishes as always, and I'm really sorry for what is obviously a terribly frustrating experience. I hope things all work out for the best in the end.

   D.B.

3. **woit**
   March 6, 2006

D.B.

Thanks for the wise words. So far I only regret one thing I've fired off today, perhaps by tomorrow I'll have realized that more were a mistake. Will keep your advice in mind the rest of the evening….

4. **ark**
March 6, 2006

I belive that Peter's credentials are more than adequate. For instance, he had recently made a presentation on work by Freed, Hopkins and Teleman, math AT/0206257, which later was elaborated by Atiyah and Segal in math.KT/0510674. I am sure, that majority of physicists either unaware or simply do not have skills to appreciate this important recent work. Peter's presentation is aimed exactly at this type of audience. I found his explanations very illuminating. The problem is certainly not with his credentials but rather in a shallow level of many discussions tolerated at his blog.

5. **X**
March 6, 2006

ark, so you mean the blog has to be well-moderated AND the blogger should be an active researcher with decent credentials. That is known as moving the goal post. Anyway, Lubos Motl's blog has no less shallow level of discussion, and it is permitted to trackback.

6. **ark**
March 6, 2006

I agree, but the issue still remains. I do not have a solution, just an observation. Most likely, there is no solution. The fact that somebody is better or worce is a fact of all our life experiences. But, to move forward with whatever democracy, perhaps, is not the best solution since Brownian motion may or may not lead away from the starting point (all depends on dimensionality of space in which it is taking place 🙂)

7. **Hogg**
March 6, 2006

I'm with Smolin!

8. **Ark**
March 6, 2006

Since the last remark surely supports my earlier statement, that is back to square one, I was thinking that may be there is at least partial solution after all. For example, everybody can suggest a paper at arxiv which is worthy of discussion provided that the person who makes such a suggestion also states very clearly why he or she thinks the paper is worth discussing. This should include some references to earlier paper(s) which given one improves/disproves. This earlier one(s) should have already some noticeable citation/downloads count thus indicating that it is of general interest to many people. Hence, it makes sence to talk about its most recent development. It would be interesting to see (for everybody) if some agreement can be reached on usefulness of any such submitted paper. Personally, I would be very surprised that such an agreement can be reached. And for this reason I was saying earlier that democracy in science is two edged sword. With all disadvantages of the pyramidal scientific system it would be totally pathetic to equate voices of all teaching institutions (recall departmental meetings at your own scool 🙂 !)

9. **D R Lunsford**
March 7, 2006

ark – it's obvious to everyone that Peter has not only brains, but balls.

-drl

10. **jb**
March 7, 2006

Question: How many sites have trackbacks blocked?

11. **Thomas Larsson**
March 7, 2006

Trackbackgate 🙂

12. **JE**
March 7, 2006

It's kind of funny to see how desperately arxiv moderators are trying to throw rationale behind their plain censorship of Woit's blog. They have even come up with an ad-hoc active research policy to support their case. What an awful shame to be perfectly aware of the fact that Peter wouldn't be allowed to trackback even if he started to publish papers on a weekly basis!

13. **Luboš Motl**
March 7, 2006

Dear Ark,

the level of the discussion is primarily determined by those who debate. I am at least trying to inhibit the kind of "discussion" in the direction of Danny Ross Lunsford, Quantoken, Nigel Cook, and so many others. Peter, on the contrary, likes to build on this kind of "discussion" because what these crackpots are saying is very convenient for his agenda. He likes to be viewed as the hero who fights against the evil science and the evil scientists. At the same time, of course, he is trying to pretend that he is not one of them.

Best
Lubos

14. **anon**
March 7, 2006

I can't believe Motl's name-calling ad homini attack by stereotyping commentators with different ideas as 'crackpots' is allowed here.

15. **woit**
March 7, 2006

jb,

I have no idea how many such sites there are. Over at Jacques's blog I asked him for a list, got no answer. Given that it took 3 months of effort to find out that there was a list and that I was on it, and another month to find out why, I suspect that getting this kind of information out of the arXiv might not be easy.

anon,

On the whole I prefer to leave Lubos's comments alone here and not delete them, although I would delete such comments from other people. Seeing what the string theory community considers acceptable behavior by one of its leading "active researchers" and bloggers has a certain educational value.

16. **Dumb Biologist**
March 7, 2006

I found the comment by Dr. Polchinski a bit confusing. I think you were right, Peter, to apologize to him for using less-than-collegial language (I'm also doing my best to be polite and appreciative these days, though I'm sure I'll fail on occasion), but I didn't find the basic message to be, in any substantive way, different from what could be found in the Nature column you linked to (the statement "For something more sensible about the anthropic principle, see a recent column from Nature." was omitted.) Example:

"Why is our Universe so exquisitely tuned to host life? Using the anthropic principle to explain the world might be a tempting alternative to invoking God, but it's not science, says Philip Ball.*"

(To be fair, Ball seems to be mostly summarizing the ideas of Smolin rather than endorsing them, but the overall message still appears to take a dim view of the Landscape, per the header I quoted above.)

Meanwhile, I've seen other bloggers express their own rather unanalytic reactions to the landscape ranging in tone from chilly agnosticism to sneering derision. I'm sure Dr. Polchinski singled out your statement to make an effective rhetorical point, but I'm not sure if it was entirely just to do so.

*Not an "active researcher" but a PhD in physics and an Oxford grad, for what it's worth.

17. **woit**
March 7, 2006

D.B.,

Polchinski was within his rights to pick out the worst of my anti-landscape postings and complain about it. It's not one I'm proud of or want to defend, although its main point that this stuff is not science is a valid one, widespread in the physics community at this moment (my allies on this include many string theorists, including Lubos).

It's interesting that Polchinski chose to write in about this in the context of the arXiv trackback issue. Distler would certainly claim that "trackbackgate" has nothing to do with my opinions about this issue, but I think Polchinski's comment shows clearly that this is what this is really all about.

18. **ObsessiveMathsFreak**
March 7, 2006

This is what happens when the rules are not clear and unambiguous. What's an "active researcher"? Can you define it? Can you give a metric that will, for anyone alive, tell you unambiguously whether they are an "active reseacher" or not.

If you can't do this, if you make rules and definitions subjective things, then you are asking for trouble.

19. **dan**
March 7, 2006

hi pete,
i know you are a particle physicist, i am curious as to what your thoughts are with a recent paper by smolin and markapolou that LQG is relevant to particle physics in this recent paper:

http://arxiv.org/abs/hep-th/0603022

we know your skepticism about string theory's ability to say anything about particle physics.

what are your thoughts about "qg and the sm" deriving the sm from lqg.
do you think it is as promisng, less promising, or more promising than string theory? of course, yours truly, lubos has his negative comments, but he is a string theorist, not a particle physicist.

20. **Kea**
March 7, 2006

Dan

The Sundance+PI paper talks about String type diagrams – hence it is just as much String theory as LQG – despite the authors', and others, points of view.

21. **woit**
March 7, 2006

dan,

Sorry, I just haven't had the time to look carefully at this work. Kea's comment about it is quite interesting. But I know little about it, so don't want to host a thread here on this topic.

22. **Benni**
March 7, 2006

woit wrote:
but I think Polchinski's comment shows clearly that this is what this is really all about.

No. Polchynski maybe quotet that comment which was most enerving to him. If someone would criticise LQG it wouldn't hurt him that much personally. Polchynski wrote that the trackback system should be shut down. This is not, what distler wants, when you read his reply (old news, he is wasting time with that controversy etc.).

For Distler it maybe the case to let you out of Arxiv. But this is due to the fact that one might imagine it is annoying when a string theorist writes "may be this is a nightmare in which the theory is today", recognizes that research struggles and then someone with only about 10 papers who published nothing in the field, writes how ugly the work of the stringtheorist is.
I would not want such person, to build a social success to be known in the field upon my failure.
This may be the reason why somepeople won't led your posts associated with a scientific archive.
They want not write papers and then see automatically generated links to their public demolition from someone who has almost nothing written. This is very good to understand.

23. **Justin**
March 7, 2006

I think that the arXiv should keep trackbacks, but just honestly admit that they will not allow trackbacks to Peter's website. I think that there is a legitimate argument that Peter's criticisms are not made in good faith, but rather seek to undermine the honest efforts of a whole comunity of scientists. Perhaps Peter thinks that string theory is not worthwhile, but that opinion in itself does not contribute to the scientific discussion, it does not show a way forward. In the meantime theorists will continue to make progress on the problems they can solve.

That being said, I do think Peter's blog is interesting, and it is worthwhile discussing string theory's successes and failures on the broadest terms. However, I can appreciate that the arXiv would not want to provide links to comments which only serve to criticize the research of hard-working people trying to solve difficult problems.

24. **Benni**
March 7, 2006

Justin, you made the point completely.
This might be the reason, why arxiv moderators wanted only blogs by an active researcher.
That means, they would allow even peter, if he would work hard to try out and got some results. Even if he would find an upper bound for the cosmological constant in string theory, they would accept this.
Polchinski said, that he often cites a paper which is critical on the landscape. I believe that.

The problem is not about the critics but the problem is that one, who never worked out a serious problem on this theory or has only around 20 papers in other fields published makes the failure of others into his triumph without having at least critical results archived.

For someone working hard, and is struggling this is not of any acceptable level. Even when it is true that the media hype string theorists created is indeed dangerous for theoretical physics. One can criticise that, but this even has nothing to do in arxiv.org

25. **woit**
March 7, 2006

Justin and Benni,

I don't know who you are, but you show no signs at all of understanding what the scientific issues are here. You appear to be students who don't know what this is all about, but somehow feel comfortable criticizing me (very repetitively in Benni's case). I'll delete any further comments from you two unless you have something substantive to say.

26. **J.F. Moore**
March 7, 2006

The criticisms of Woit I keep reading here, particularly Benni's, rub me the wrong way. Perhaps it's a byproduct of seeing CVs filled with mediocre papers churned out by hardworking but not terribly brilliant scientists which they use to propel their careers, or going through mass evaluations solely based on counting impact-factored pubs and invited talks. I don't know. Regardless, someone with Peter's credentials who has taken it upon themselves to ask important questions about a field which is consuming significant public funds, questions like 'is this actually going anywhere?' , 'is there a fundamental flaw in what is being done?' etc (I paraphrase), is doing a very important service to the scientific community and the public trust on which it relies. EVERY field of science should have these kind of questions being asked ALL the time, and in most fields these answers come trivially. (Note that justification based on reduction to technology is not what I am talking about, but the fundamental epistemology that _must_ underpin science.)

His forthcoming book is an important part of this, and I hope it gets at least a fair fraction of the same readership as Kaku's latest crowd-pleaser, but there is also a community of practicing scientists out there that could benefit from at least having the trackback there, to be followed or not. I don't see the point in requiring someone critical of a field to do new work in and publish it in that field, thereby lending further creedence to arguments which just _might_ need to be dismissed out of hand, if everyone involved were being honest about what is going on.

27. **D R Lunsford**
March 7, 2006

What would Fritz Rohrlich say about all this? David Finkelstein? John Wheeler? These people are alive and around, but are as silent as lambs. Even Sarfatti.

Why isn't Roger Penrose making decisions about trackbacks, rather than Distler?

What a crowd. I'm ashamed to be a part of this, however indirectly.

(Did you know that "Distler" means "chinless" in Halb-Plattdeutsch? And "Motl" means "bumbler"?)

-drl

28. **anon**
March 8, 2006

'Why isn't Roger Penrose making decisions about trackbacks, rather than Distler?'

I don't think Penrose would been keen to encourage string theorists who rant and rave about all conceivable alternative programs being crackpot: http://arxiv.org/tb-display/hep-th/0603022

29. **Christine Dantas**
March 8, 2006

Dear Peter Woit,

Allow me to give a brief announcement and a comment. My blog has now the trackback feature by HaloScan, so now I theoretically can send trackbacks to the arxiv (at least, according to Jacques Distler, I could). I have just sent a trackback to the paper *Quantum gravity and the standard model*, by Bilson-Thompson, Markopoulou and Smolin [http://arxiv.org/abs/hep-th/0603022]. I believe that it will take some time to appear my trackback link at the arxiv site, though.

I have offered some suggestions concerning the arxiv trackback policy, my comments can be found among the various others at Jacques Distler´s blog, so I will not repeat them here. According to my understanding of what would be a reasonable criteria for accepting trackbacks, you would qualify. The status of "active researcher" cannot be established by the number of papers alone, in my view.

I must confess, in any case, that I have mixed opinions on the truthful importance of trackbacks. Conventionally, if a researcher wants to criticize a given paper, he would normally write a comment to that paper. Or communicate with the author(s), if that is humanly possible. But now, with the internet, things have changed of course and perhaps trackbacks will start having their weight of importance on the self-regulatory nature of the scientific research (or, at least, the way it is supposed to work).

I wish you all the best of luck and hope you gain your rights to the arxiv trackback system.

Christine

30. **woit**
March 8, 2006

Christine,

Thanks, I'll post some more comments about the "active researcher" issue in a moment. It's kind of a ridiculous issue, since every blogger involved in this discussion is obviously an active researcher, and some of these blogs (like yours) are quite good. This whole thing would not be especially controversial if it were not for Distler's desperate attempts to find some kind of reason for not allowing trackbacks to my blog.

Several physicists object to the inclusion of trackbacks to their papers on various reasonable grounds. It seems to me that this can and should easily be addressed by allowing authors to choose not to allow trackbacks to their papers. This question goes way back to more than 10 years ago when I remember Ginsparg discussing a plan to allow linked commentary to arXiv papers. His plan then was to allow authors to delete commentary they didn't approve of. When asked "what about authors who delete comments explaining why their work is wrong?", he said something like "if no comments are there, the work is either wrong or not interesting, not clear that you should care which".

31. **woit**
March 8, 2006

Updated the text of this posting to include some analysis of the curious phenomenon of stone-throwing glass-house-dwellers.

32. **Christine Dantas**
March 8, 2006

Dear Peter Woit,

I have just checked, and my trackback to hep-th/0603022 can be seen now together with Lubos Motl´s trackback.

Regarding your considerations based on number of pages and collaborators (authors), well, I do not exactly agree that this could be a much improved measure of "active researcher", compared to other purely quantitative ones. See that Einstein´s paper on E=mc^2 was a very short one. Also, what page format are you considering? Sometimes papers are submitted in 2-column format. On the other hand, I agree that the number of authors is an interesting consideration to be made.

The issue of evaluating a researcher´s credentials or "status" is an old one. How do we evaluate things like *quality, impact, relevance*? People today just want to measure your work by blind numbers. But it is quite obvious that quantity does not necessarily imples in quality and the other way around is true as well.

I have gone through this several times before in my career and it is depressing. The system engulfs us. I never got interested in numbers, but in quality. But people are not interested in this philosophy or, if they are, cannot properly measure it. I am happy that I still can do my research despite the fact that I am not really in the best of worlds for such an activity.

Christine

33. **woit**
March 8, 2006

Christine,

There's obviously no good numerical criterion to evaluate whether someone is performing worthwhile research, the one in my posting was chosen purely to make a point. One could make all sorts of different points using different metrics. For the record, I certainly think Jacques qualifies as an active researcher, by any sensible definition of the term. The problem is with his insistence on the concept of some other, non-dictionary definition of "active researcher". He clearly was choosing this so as to exclude me, didn't seem to think much about whether it might exclude him too.

34. **Christine Dantas**
March 8, 2006

Dear Peter Woit,

Ok, I understand your point. Thanks for elaborating. Sometimes I excced myself when this issue on "how to properly evaluate one´s research work" comes up. I´ll stop here, thanks.

Best wishes,
Christine

35. **Christine Dantas**
March 8, 2006

Sorry for the typo ["exceed"]. I´m calming down now. 🙂

Christine

36. **anon**
March 8, 2006

"There's obviously no good numerical criterion to evaluate whether someone is performing worthwhile research"

There is a numerical formula to qualify as an endorser at the arXivs. Since, on Distler's blog, you advocated the endorsment system on the arXivs, why use the same formula:

"The number of papers depends on the particular subject area, but has been set so that any active scientist who's been working in her field for a few years should be able to endorse…"

37. **woit**
March 8, 2006

anon,

I wasn't really advocating an "endorser" system, just noting that that is the one the arXiv already uses, and there was no good argument for why they weren't using it in this case (other than the fact that links to my blog would presumably be allowed under the "endorser" system).

As I wrote over there, I also don't have any problem with an "active researcher" criterion, just with such a criterion that excludes active researchers.

38. **anon**
March 8, 2006

That is the criterion they seem to be using to qualify as an endorser. Maybe it is (or should be) the same criterion to qualify to send trackbacks.

39. **woit**
March 8, 2006

Actually, does anyone know what the current criterion for number and timing of postings to hep-th is to qualify as an endorser?

The discussion of the endorser mechanism assumed that the role of endorsers would be to endorse blogs just as they now endorse arXiv postings.

40. **anon**
March 8, 2006

All I'm saying is that they use the same phrase "active researcher" to describe both. Maybe they mean the same criterion in both cases.

41. **woit**
March 8, 2006

Distler was pretty explicit in refusing to give a precise definition of "active researcher". I would think that if he had the arXiv endorser criterion in mind (whatever it is, exactly) he would have mentioned it.

42. **anon**
March 8, 2006

They don't say precisely what that formula is either.

But, if that's what it is, would that be an acceptable benchmark?

43. **woit**
March 8, 2006

Hard to say, not knowing what the formula is. But this is all besides the point. One just wants to distinguish blogs that have some interesting content from blogs run by crackpots or people who know nothing. It's pretty obvious to me which ones have interesting content, and I think most trained physicists would agree on this. If all of the people running blogs with interesting content were arXiv endorsers, then that would be fine. I'd suspect there are a significant number of interesting blogs run by non-endorsers, so that would be a problem.

44. **Arun**
March 8, 2006

The real problem to be solved is to be able to locate all blogs that discuss a particular arxiv pre-print. This should be possible without track-back, if there is a standard way to label references to the pre-print that do not overlap with anything else that the search engines might index.

If arxiv is trying to screen for high quality, that is not possible when so many physicists host blogs, on which anyone can post comments.

45. **anon**
March 8, 2006

"If all of the people running blogs with interesting content were arXiv endorsers, then that would be fine."

"Interesting content" does not equal "should be linked-to from the arXivs".

That seems to be the whole nub of the dispute.

46. **woit**

March 8, 2006

I have a reasonably high standard for “interesting content”, thus do think it is pretty much equivalent to “should be linked-to from the arXiv”. I think the problem is more that different people will differ about what is “interesting content”.

To me “interesting content” is something I’d find worth taking the time to read if I was interested in a certain arXiv paper. I don’t see the downside here to resolving the question of what different people find interesting by being as inclusive as possible. The main danger with being overly inclusive is that you’ll end up with too long a list of trackbacks to each paper, making it hard to find those you personally are interested in. This doesn’t seem anywhere near to being a problem these days, given the small number of blogs linking to arXiv papers. If it becomes a problem in the future, it could be addressed then, at which point one would have a much better idea of what criteria might work to get rid of less interesting trackbacks.

The other problem is the one of authors objecting to all or certain trackbacks to their work. In particular, an author might reasonably object to links to a source containing comments they consider offensive or incorrect about their work. I think it would be a good idea for them to have the ability to have such links removed, or to not allow any trackbacks if they so chose.

47. **anon**
March 8, 2006

So if all string theorists decided they didn’t want trackback links from Peter Woit appearing next to their abstracts (and were allowed to remove them), how would that differ from the current situation?

“I have a reasonably high standard for “interesting content”, thus do think it is pretty much equivalent to ‘should be linked-to from the arXiv’.”

Apparently, many arXiv authors disagree.

48. **woit**
March 8, 2006

One way it would differ from the current situation is that links to discussions here of non-string theory papers (yes, this does happen…) would be there. It’s also true that quite a few string theorists read this blog and tell me that they find what I have to say worth listening to, even though (or especially because) it challenges what they are doing. I’d be quite happy if the situation were that some trackbacks were there to string theory papers where the author either didn’t care, or better yet, was interested in engaging in discussion of the paper on this blog, but not there to ones where the authors were offended by them. I’m really not trying to piss people off, rather trying to engage in serious scientific discussion.

Yes, certain arXiv authors do disagree about whether what I’m writing here is interesting content of a reasonably high standard, and that’s what this is all about. I’m happy to debate this with anyone, but not amused if they refuse to debate this and instead engage in ad hominem attacks on me, as “not an active researcher” or some such.

49. **Dick Thompson**
March 8, 2006

Why am I reminded of “We have always been at war with Eastasia”?

50. **jb**
March 8, 2006

The answer to me seems clear: get rid of trackbacks. Any filtering system will eventually come to trouble, and you can always just use your favorite search engine if you’re interested in discussion of some paper.

51. **Dick Thompson**
March 8, 2006

Reading Distler’s discussion of the policy, I was struck by this:

One’s first thought is: why not use the same endorsement mechanism used for paper submission? Unfortunately, the experience of the moderation system is that endorsement is not a terribly high barrier to entry. Some endorsers are rather loose in endorsing people to submit papers and one can only imagine that they would be even looser in endorsing people to submit trackbacks. In the case of papers, the second-stage filter of moderation is clearly necessary. But we had already decided that there would be no such second stage in the case of trackbacks.

In other words we have the mandarin attitude that can’t trust the “underlings” to stay with the program. Now, rather than *1984* I am reminded of the Bush White House. Small difference, perhaps.

52. **anon**
March 8, 2006

Oh? So now the arXiv moderation system (for papers) is high-handed censorship, too?

They should just accept every paper and have a “debate”?

There must be thousands of endorsers (clicking around, it seems that anyone with any sort of a publication record is an endorser). I can imagine that, with that number, getting endorsed ought to be a trivial matter.

53. **woit**
March 8, 2006

anon,

I’m not going to try and defend what Dick is saying, since I don’t know what his point of view is and quite possibly I wouldn’t agree with it (I’m a lot more elitist and willing to see suppression of crackpottery than many of my commenters), but I tried to engage Distler in a discussion of this point:

“Some endorsers are rather loose in endorsing people to submit papers and one can only imagine that they would be even looser in endorsing people to submit trackbacks.”

which I think is the one that Dick is characterizing as “can’t trust the “underlings” to stay with the program.”

Distler claimed that endorsers have endorsed papers by people like the Bogdanovs to back this up. I characterized this as a bad endorser and asked whether they couldn't just deal with bad endorsers for trackbacks the way they dealt with bad endorsers for papers. All I got in response to this was an insult, at which point I realized that trying to have a serious discussion with him was a waste of time, certainly not worth it on this particular question which is a side issue.

But the whole thing did show that there is an attitude at the arXiv that even endorsers can't be trusted. I freely admit that, having no access to data about how this system has worked for accepting papers, I can't tell whether lax endorsers are a serious, insoluble problem, or whether the problem is that they represent legitimate points of view that happen to be different than Distler's.

54. **anon**
March 8, 2006

"I can't tell whether lax endorsers are a serious, insoluble problem, or whether the problem is that they represent legitimate points of view that happen to be different than Distler's."

Presumably they're not a serious problem because, as Distler says, the papers are also vetted by the arXiv moderators.

55. **woit**
March 8, 2006

"Presumably they're not a serious problem because, as Distler says, the papers are also vetted by the arXiv moderators."

The current endorsement system seems to work tolerably well, and from what Distler says, involves a moderator sometimes trumping an endorser. What I couldn't get an answer from him about was why not just use the same trumping mechanism when necessary, in this case trumping an endorser who endorses a crackpot blog? This sort of extra level of moderation has nothing at all to do with the fact that it would be too hard to moderate indvidual blog entries. This would be an extra level of moderation on the question of which blogs to accept.

56. **Michael**
March 8, 2006

Peter, comparing your own academic accomplishments with Jacques' and reaching a conclusion in favor of yourself raises your crackpot index to an all-time high.

57. **woit**
March 8, 2006

No Michael, I'm just pointing out that, by the criterion suggested on Jacques blog, he and I have the same lowly status of not being "active researchers". I suspect you do too. Join the party…

58. **robert**
March 8, 2006

I recall that, when the internet first started to have an impact, it was hoped that it might liberate the scientific community from the 'publish or be damned' mindset and the tyranny of the anonymous and vindictive referee. This expectation does not seem to have been fulfilled; self-interest and internecine nastiness still prevail.

59. **Michael**
March 8, 2006

Peter, your misunderstanding is that activity is measured in number of pages or papers per year. Important new ideas per year is more like it. Jacques has already mentioned that the criterion for "active researcher" is somewhat ill-defined. He also explained to you that it's only part of a multi-stage filtering system, which as a whole works far better than its individual components do. This is a point you keep ignoring. Of course, you do it on purpose to advance your agenda. You may not be the greatest researcher, but I don't think you are stupid. I can see behind your mask and others do, too. Only Quantoken and Iman Zumbal take you at face value…

60. **Eli Rabett**
March 8, 2006

How about linking forward? Blogs that want to link to Arkiv should explicity include the report number so it will be picked up by Google, and anyone wanting to see what the comments are could simply search on the report number.

61. **woit**
March 8, 2006

Michael,

I'm well aware that Jacques's criterion for "active researcher" is "somewhat ill-defined", and part of a "multi-stage filtering system". One of the stages seems to be "is this person causing serious problems for string theory?".

Your Quantoken obsession was always a bit weird, but Iman Zumbal?????

62. **Michael**
March 8, 2006

"Iman Zumbal" is a pseudonym a crackpot better known as "Danny Ross Lunsford" recently used on Lubos' blog. Since you probably didn't know that, I can fully understand you found my mentioning that name strange. 😉

63. **Power Ranger**
March 8, 2006

Peter didn't count papers/year. He counted (pages/paper)/(authors/paper)/year.

I've never heard of that measure, till Peter invented it. It would make every single high-energy experimentalist have (nearly) zero level of activity.

Was there any point to the exercise, except to show that he could find SOME measure (no matter how ridiculous), by which he is "more active" than Distler?

64. **woit**
March 8, 2006

Michael,

Glad to have the "Iman Zumbal" business cleared up, I was worried for a moment that you had completely lost it. I do read Lubos's blog, it's one of the funniest things on the net (did you know that most Harvard professors are anti-Semites? I learned that today). And sometimes he has interesting news about things like who will get the Fields medal. But I guess I don't read it as carefully as you. Danny is pretty wacky, but can't hold a candle to Lubos in that department. Unfortunately for you, I delete much of Danny's best stuff.

65. **J.F. Moore**
March 8, 2006

Michael: "I can see behind your mask and others do, too."

You don't speak for me, or necessarily anyone else here. I don't believe Peter Woit is engaging in sophistry at all, as you imply. What I don't take at face value, because it has been made impossible to do so, is the criterion for allowing arXiv trackbacks. I do comprehend it, on the level that I understand that any bureaucrat who makes the mistake of precisely defining such a criterion (or even who specifically is charged with any judgement calls) loses their arbitrary powers related to that criterion.

66. **Michael**
March 8, 2006

Peter, if you need anything else, I'll be at your service. 😉 Why do you think I'm about to lose it? I know I'm inconvenient for you (and you and your friends here would surely find better and stronger words for it), but did I give you any indication that I'm an unstable individual?

I, too, find Lubos blog highly entertaining. Lubos is an incredibly sharp thinker and his judgement, especially when it comes to unfamiliar or vague ideas is almost unmatched. His political posts simply reflect that he is a man with principles, and he's got the guts to speak up. Whether you enjoy his mocking of Smolin articles and other humoristic posts is strictly up to you. Your comparing him with Prof. Zumbal is a stupid cheap-shot.

67. **woit**
March 8, 2006

Power Ranger,

"Was there any point to the exercise, except to show that he could find SOME measure (no matter how ridiculous), by which he is "more active" than Distler?"

No, there really wasn't any point to the exercise except that.

But if you want to stick to papers/year over the last three years, as suggested on Jacques's blog, he's well below the minimally acceptable standard of 2/year for an "active researcher" and flirting with going under half that, 1/year over the last few years. And some of these papers are, well, a bit thin. I don't think he's going to measure up to typical standards for "active researcher" constructed by counting publications. Which might explain why he steadfastly refuses to explicitly construct such a standard.

68. **Michael**
March 8, 2006

Dear J.F. Moore,
I didn't try to speak for you. I said "others do, too", which is more than likely accurate. Today's dedication of this song goes to you:

http://www.carlysimon.com/music/Lyrics/You're_So_Vain.html

69. **Power Ranger**
March 8, 2006

Is there any reason to think the standard is stricter than the "active scientist" standard used to determine who is an endorser on the arXivs (as suggested by "anon")?

I followed his suggestion of clicking around. Pretty much anyone with a pulse (scientifically speaking) who posts papers at the arXivs qualifies as an endorser.

70. **woit**
March 8, 2006

Power Ranger,

We don't know if Jacques's "active scientist" standard is stricter than that of being an arXiv endorser since he refuses to tell us what it is. The only thing definite he's told us about it is that Peter Woit doesn't come close.

71. **Michael**
March 8, 2006

"The only thing definite he's told us about it is that Peter Woit doesn't come close. "

That's what scientists do: when in doubt, stick to the facts…

72. **Power Ranger**
March 8, 2006

But we have a good bit of data: Christine Dantas, Lubos Motl, Matthew Nobes, Urs Schreiber, … all qualify. You, apparently don't.

Somewhere between their levels of productivity and yours must lie the dividing line (presumably, closer to theirs than to yours, if you're "not even close").

73. **woit**
March 8, 2006

Power Ranger,

Yes we have a good bit of data that, whatever the mysterious "active researcher" standard is, Peter Woit doesn't qualify but most other people do. And I'm sure that will remain the case no matter what for the forseeable future. At some point later this year I'll probably be posting some of my more recent research on the arXiv. It will then be very interesting to see how Jacques constructs an "active researcher" standard that allows him, with his barely 1 paper/year to qualify, but not me. I'm sure he'll find a way, but it may be really tough if he actually has to tell us what it is.

74. **woit**
March 8, 2006

Eli,

This already works. If you google on standard arXiv numbering of a paper you'll get relevant blog entries. Problem is you get lots of other stuff to and have to sift through it. Lieven le Bruyn suggests using Technorati, which tracks blogs, but I haven't tried it.

75. **J.F. Moore**
March 9, 2006

"I didn't try to speak for you." – Michael

You said that only two specific people, not including me, take Peter at face value. I do and probably some others who read this blog as well. Too bad if that was so confusing for you to grasp.

76. **anonymous**
March 9, 2006

Micheal: "That's what scientists do: when in doubt, stick to the facts…"

Agreed, but unfortunately that is not what Jacques is doing. He is fixing his conclusion first, and then making rules to justify that. That is what ID folks, creationists, fanatics and string theorists do.

77. **SteveM**
March 9, 2006

Under this totally vague criterion Higgs and Gell Mann don't qualify as "active researchers" either. I can only assume in the unlikely event they start blogs up they too won't get trackbacks. A lot of qualified people get bogged down in administration and teaching and may not publish for a long while but they should still have a voice and the right to participate.

Physics, and all science, is crucially based on debate and criticism. If you are confident in your work and publications–whether you work in string theory or anything else–then criticism, or trackbacks to criticism of your papers (even very severe criticism) should not bother you. As a professional scientist you should be able and willing to provide a counter argument at a technical level, without personal attack/abuse. Physicists have a long history of tearing each others work apart, both formally in the journals and informally at blackboards and the like–this is why physics has been so spectacular successful. Blogs are now a new medium for this debate and a very useful one.

Incidently, when exactly did physics turn into this dick measuring contest about who's publication list is the longest or who has the biggest citation size? Recently, I keep repeatedly reading these obnoxious put downs of Peter Woit's publications and credentials by anonymous characters like "Michael" and "Benni" and the like. So out of curiosity I looked up Woit's papers at slac. Now I am not a "Woitian cheerleader" by any means but the first thing I notice is a paper in Physical Review Letters written by only himself while a graduate student. It has a top citation. Then some papers as a single author in Nucl. Phys. B. and Phys Letts B. The TRUE test of a physicist's mettle is whether they can get papers published as a SINGLE author in elite level paper journals like this, the old-fashioned way, and get through the brutal (yes brutal!) peer review. So before they belittle other's past work some of these younger upstarts should try this–they might find the rejection letters very sobering. Truth is most postdocs and assistant professor now would sink like a lead whale without a lot of co-authors. On your resume one paper in PRL written on your own is worth a hundred latexed submissions dumped in the arxiv, with your name alongside another 5 authors at the top, one of whom is your department head.

The impression I get is that P Woit had the potential for a very good research career but was about 10 years too late by the time he got his Phd and simply missed the Standard Model glory years of the 70s and so never found the right niche that would have suited him. In the mid 80s a lot of guys found themselves in the same position and due to a bad job scene, started companies, went into computing or finance or took teaching posts since string theory had taken hold by then and was not to their taste or way of doing things (and of course it was string theorists who were now being hired and funded.). So please, Michael and the rest can you cut it with the snide remarks and insults? It is really wearing thin. If you think Peter talks crap about string theory then lets hear some concrete professional technical arguments from you as to why. Obviously in the trackback issue we want to block out crackpots, but we have someone here with a strong background, is qualified, has peer-revwed papers, works in a major university, has a book accepted for publication, and has the right to express his opinion. Please grant him trackbacks or else scrap this whole trackback thing. The idea that he is being censored is becoming troubling to me and a lot of other people too.

78. **Pookie**
March 9, 2006

No one disputes that Peter was doing good physics, as a graduate student, 20+ years ago.

Should the arXivs accept trackbacks from anyone who, at some point in their lives, were active physicists (all those guys who "started companies, went into computing or finance or took teaching posts")?

Maybe having once obtained a PhD in physics should be enough?

79. **Thomas Larsson**
March 9, 2006

SteveM: We all know what DistlerCo think is the big problem: Trackbacks attract flies.

80. **JE**
March 9, 2006

Woit said,

"For the record, I certainly think Jacques qualifies as an active researcher, by any sensible definition of the term."

The other way round not being true, which given the lack of experimental evidence for Distler's work, makes Peter much less pretentious or rancorous than him.

Michael said,

"Peter, comparing your own academic accomplishments with Jacques' and reaching a conclusion in favor of yourself raises your crackpot index to an all-time high".

Well, combining the bitter reaction to any criticism of string theory from some of its main proponents (now including censorship) with the lack of experimental evidence supporting it for the time being, I think Peter Woit presently scores far far behind.

Woit said,

“I’m sure he’ll find a way, but it may be really tough if he actually has to tell us what it is”.

Once you meet the quantity criterion, there are still plenty of subjective “relevance or quality standards” enabling someone to block your trackbacks if that’s the purpose. Very sad indeed.

81. **ObsessiveMathsFreak**
March 9, 2006

> Should the arXivs accept trackbacks from anyone who, at some point in their lives, were active physicists (all those guys who “started companies, went into computing or finance or took teaching posts”)?

Perhaps they should. Elsewise, what is the point of the trackbacks?

The point of trackbacks is to let the author of a paper, and the readers, know about discussions and references to their papers online. If someone who was running a company had a blog, and referred to your paper in it, showing its application to industry, wouldn’t you want to know about it?

To the trackbacks make the system more open to ideas, or do they simply cross link within a closed circle. If the latter, there’s really no point implementing the system.

82. **Christine Dantas**
March 9, 2006

Dear Peter Woit:

A suggestion.

You have several research papers published in high standard journals, but they are mostly dated back from the beginning of the arXiv (which was about 1992 I guess). If you can make them available electronically, I believe you could post them on the arXiv, perhaps with complementary notes contextualizing them or comparing them with the present status of research. There are examples of papers published before 1992 or so that were submitted to the arXiv. Select those papers of yours which you believe are of interest and post them there.

Also, I believe you have enough material to publish review and expository papers or comments to given papers that would be of high interest. Work on them and post them on the arXiv.

Perhaps that would give you the “numbers” that the “system” requires. Not that I think you should need to do that to prove anything.

I really think that a discussion on the credentials of a professor in a major university, with a very solid background, is completely anti-ethic and repulsive. But it is an excellent example of people that constantly needs to derogate others in other to find pleasure or reinforce their own idea of how great they must be compared to others.

I will stop here.

Christine

83. **sunderpeeche**
March 9, 2006

When Peter previously wrote on this topic (to say ~ “no trackbacks so far and he hadn’t heard from arXiv for 3 months”) I suggested (approx) “don’t worry about the trackbacks, so what, this blog get enough attention and respect to stand on its own merits”.

But Peter did not follow the suggestion and I am willing to admit his actions have paid off much better. I thought of writing a comment **2 days ago** but decided to wait. I am amazed, not to mention ROTFL.

You can’t pay for comedy like this. The arXiv has gone out of its way to make a fool of itself.

What is my attitude about the trackbacks? None. My main concern is that I don’t burst out laughing in the middle of the office and get sacked for reading this on company time.

I checked Peter’s publication record on SPIRES a long time ago and I noticed that most of the papers are from the 1980s. I would not call him an “active researcher”, but as several people have pointed out, the criterion is both ambiguous and irrelevant.

Well done, Peter, though I have no idea exactly what I am complimenting you for.

84. **Nat Whilk**
March 9, 2006

Power Ranger wrote: “*Peter didn’t count papers/year. He counted (pages/paper)/(authors/paper)/year. I’ve never heard of that measure, till Peter invented it*.”

This, of course, reduces down to pages per author per year. On tenure and advancement issues, something like that measure is used by my institution to measure quantity of one’s output (with this measure being augmented by an assessment of quality of output). I suspect many other institutions do so, too, counting a co-authored paper less than a single-authored one, and counting a short paper less than a long one.

85. **Pookie**
March 9, 2006

Under those conditions, I suppose you would fill every paper you write with a long review of well-known mathematics, confining your own work to a few pages interspersed here and there.

With modern wordprocessing techniques, you could turn every 6-page paper into a 60-page paper with little trouble at all.

86. **Nat Whilk**
March 9, 2006

Pookie:

If you can get away with packing 54 pages of fluff into a 60 page paper, you're submitting your papers to different journals than I am.

87. **Pookie**
March 9, 2006

Submitting to journals?

Oh. Right. Forgot about that step …

88. **Alejandro RIvero**
March 9, 2006

The technorati thing was discussed and discarded in Distler blog time ago, if I recall correctly. Now, a good techorati trick could be to look for the url arxiv.org/abs . This is, including abs and not including www so we cover mirror references.

Alternatively click here
http://www.technorati.com/mini/index.html?s=arxiv.org/abs

I suppose we could include this technorati window inside an iframe, llets see:

89. **Alejandro RIvero**
March 9, 2006

hmm the iframe does not appear in the visualization, the html is filtered during posting. if someone wants to test in his website the command is
<iframe height=500 src="http://www.technorati.com/mini/index.html?s=arxiv.org/abs"> </iframe>

90. **woit**
March 9, 2006

SteveM and Sunderpeeche,

This whole experience of having to engage in a detailed public defense of my credentials as a researcher, including responding to a large number of nasty attacks on this subject from people who have no credentials of their own, has been a rather trying experience. But the process has left me (and I gather most people who have been following it) with a lot of evidence that this is all about suppressing my criticisms of string theory, and I think the behavior of some of the people involved has been deeply disgraceful and unprofessional. It also has often been pretty damn funny.

Christine,

Thanks a lot for your comments. One possible good effect of this whole thing might be that it has provided some encouragement for me to get things written up. If Distler can ever be convinced to provide a definite standard for how many papers it takes to me an "active researcher", that would be helpful. I strongly suspect he won't though, purely because any standard he sets that would pose any challenge for me to meet would be one he is incapable of meeting himself.

All of this has caused me to think for the first time in quite a few years about my older papers from the 80s. Although people have done this, I'm not sure it's a great idea to try and post them now on the arXiv. There is at least one unpublished manuscript from that period which I was never happy with so didn't pursue publication. If I have time I might work more on that and post it. The things I was doing back then are still actively being pursued by others. For instance the latest posting at Georg von Hippel's blog is about simulations of theta-dependence in SU(N) lattice gauge theory. The first calculations of this were done for N=2 by me, with collaborators including Nati Seiberg back in 1983.

I haven't spent much time keeping up with the details of work in this field, and it's far from my current interests, so I haven't written about it here. Maybe at some point I will do this. My original work was based on what I still think is a quite beautiful idea about dealing with topology of lattice gauge fields, maybe at least I'll try and find the time to write a blog entry here some time explaining it in simple terms.

91. **amused**
March 9, 2006

"On your resume one paper in PRL written on your own is worth a hundred latexed submissions dumped in the arxiv, with your name alongside another 5 authors at the top, one of whom is your department head."

This is not actually true, unfortunately. It is more like the other way around: for career advancement in theoretical HEP, papers written with influential senior coauthors trump single-author PRL publications any day (unless the latter contains a truly spectacular result, which is rarely the case). The reason is simple: to survive long-term in this business you need the support of influential senior people, and generally you only get this if they have something personal at stake in whether you succeed or not. Young people working independently will quickly discover that their single-author PRL publications are effectively worthless on the job market.

To make this comment semi-on topic, let me just add that I think the club mentality alluded to above is also what is ultimately behind the banning of trackbacks from this blog.

92. **Benni**
March 9, 2006

Woit wrote: One possible good effect of this whole thing might be >that it has provided some encouragement for me to get things >written up.

This is the only correct conclusion existing. It should be no problem for you, even if it is a scientific critisism on stringhteory. Simply publish some serious work.

93. **woit**
March 9, 2006

Benni,

The work I'm talking about writing up has nothing at all to do with string theory. As I've tried to explain to you over and over and over again, the problems with string theory are not the sort of thing it is appropriate to write a standard scientific paper about. You can do this to show that a wrong theory is wrong, you can't do it when the theory is "not even wrong".

94. **Dick Thompson**
March 9, 2006

Peter my standpoint is that there is a wide difference between someone like Sarfatti, who got in, and Tony Smith, who didn't. Sarfatti's contribution was evaluated as mathematical gibberish, while Smith's although way off the main road, and maybe presented with "attitude", seem to be mathematically sound – I haven't seen anybody say otherwise.

Bottom line, the circle the wagons attitude is overdone, if only by a little bit.

95. **Christine Dantas**
March 9, 2006

Dear Peter Woit,

I am glad to know that my suggestion will be somewhat useful.

I very much look forward to your upcoming book.

I also look forward to your continual criticisms of string theory and hope that any rebuttals of them from the ST community will come in educated, technical language.

Christine

96. **woit**
March 9, 2006

Dick,
I agree with you about the Tony Smith/Sarfatti question.

One problem with the increasing prevalence of pseudo-scientific stuff being done by smart, respectable people is that it makes it harder and harder to maintain some reasonable standards of what is legitimate science and what is crackpottery. When this happens, standards start to devolve from focusing on content, to focusing on the credentials of the people involved.

97. **Benni**
March 9, 2006

woit wrote: The work I'm talking about writing up has nothing at all to do with string theory. As I've tried to explain to you over and over and over again, the theory is "not even wrong".

Well, you said that your current interests lie now elsewere and not in the topics you published in the past.
So I thought, as your interests lie in criticising stringhteory, you should come with something like
http://arxiv.org/abs/hep-th/0309170 which is obviously a critisism.

Even the endorsement system of Arxiv is based on the idea of an "old boys club". The thinking was "researchers only endorse researchers".
To belong to a club of researchers, I think, despite what the rules and opinions of other people are about, what a researcher is, or who they exactly want in that club, it would be best to research on something. Regardless what it is. When you find your old papers interesting you should obviously go there.

98. **Benni**
March 9, 2006

Here is even more scientific criticism:
http://arxiv.org/abs/hep-th/0402101
From the abstract:
Assuming this "landscape" exists, anthropic explanations of some quantities are almost inevitable. I explain that this landscape is likely to lead to a prediction of low energy supersymmetry. But we argue that many features of low energy physics are not anthropic and, as currently understood, the landscape picture will get them wrong.

Why don't you come up with papers like this?

99. **Alejandro RIvero**
March 9, 2006

Woit Comment 9112 wrote:

All of this has caused me to think for the first time in quite a few years about my older papers from the 80s.

This is a good news. We have got a continuous loss of people going industry, computers, math, (er… and astro-ph); generically leaving particle physics towards elsewhere.
Now tell me that Stephen Wolfram is coming back to study weak decays and I will consider the week complete 🙂

100. **woit**
March 9, 2006

Benni,

The reason I don't come up with papers like the one you quote is that it is just wrong, it's exactly the kind of paper I've been critiquing here on this blog. In this particular case, what is wrong with it is right there in the abstract:

"I explain that this landscape is likely to lead to a prediction of low energy supersymmetry."

At the time this paper was written, it was fairly clear that this kind of argument was highly dubious, that the landscape did not allow this kind of prediction. I've written about the technical issues surrounding this extensively on the weblog. I'm just not going to start adding papers to the arXiv devoted to criticizing bad arguments in "not even wrong" papers. For the 101st time, I think a blog entry about this is much, much more appropriate than writing a scientific paper and filling the literature with something purely devoted to pointing out an obviously bad argument. Since I don't think the arXiv should be filled with posted papers of this kind, I'm not about to write one. Both you and Polchinski are promoting as examples of what I should be doing papers containing incorrect claims for one method or other of extracting physical predictions from the landscape. I don't believe this can be done, for reasons many people complain that I've repeated endlessly here, but that you don't seem to understand.

101. **Chris Oakley**
March 9, 2006

> Now tell me that Stephen Wolfram is coming back to study weak decays and I will consider the week complete.

Nothing so sane, I'm afraid.

102. **Matti Pitkanen**
March 10, 2006

I think that the trackback issue is much less serious than the censorship against posting to arXiv which in practice means a professional death.

For a decade it become impossible for me to post anything to Physics Archives. Mathematical Subject Classification Tables of American Mathematical Society has alink to my homepage about Topological Geometrodynamics in the section devoted to Mathematics of Quantum Theory. Recently I was invited in to Marguis Who's Who in Science and Engineering. One might think that on this basis I should not be regarded as a non-crackpot by any person possessing IQ above 100 but the wise men in the board seem to think differently.

Certainly I am not the only one. There is large number of active researchers publishing in refereed journals who suffer arXiv.org censorship

Matti Pitkanen

103. **MathPhys**
March 10, 2006

I found Lee Smolin's comment (on Cosmic Variance) on this matter to be most eloquent.

Distler and co are discrediting themsleves and their discipline by this little stunt.

104. **Chris Oakley**
March 10, 2006

Hi MathPhys,

I could not find it. Did you mean Christine Dantas's blog?

LS seems not to be especially bothered by the latest attack from Lupine Lubie.

105. **Christine Dantas**
March 10, 2006

I believe MathPhys refers to this comment (#26) over at Cosmic Variance:

> If academic freedom means anything, it must mean that the university must do nothing to impede free discussion by professionally competent experts on scientific controversies. Given that Peter Woit is a Physics Ph.D. and a faculty member at a major university, who has published papers and has a book in press on the topic, he is without doubt part of the academic community to which the principles of academic freedom apply.
> …Lee Smolin

In my blog, Lee Smolin has commented over a discussion involving his recent paper, to whcih there is now a trackback link from my blog together with Lubos Motl´s.

Best wishes,
Christine

106. **MathPhys**
March 10, 2006

Thanks, Christine. That's the comment I had in mind.

I like it because I believe that it gets to the heart of the matter. There is an element of suppression of free speech going on at arXiv.

107. **D R Lunsford**
March 10, 2006

OK, I have a question.

In this paper:

http://arxiv.org/abs/hep-th/0106273

Do I qualify as an "active researcher", insofar as I induced both a footnote and a reference in the work of *demonstrably* active researchers?

Thanks in advance,

-drl

108. **Jack Sarfatti**
March 11, 2006

Setting the record straight.

Almost all of Waldyr's comments had nothing at all to do with my paper even in the first version. Currently the 11-th version I think corrects and/or clarifies those minor nitpicks Waldyr made for which I am grateful. Most of Waldyr's 21 pages had to do with energy conservation in GR, which has nothing directly to do with the new ideas in my paper. Waldyr wrote his review in a panic that funding would be withdrawn from his students if it was perceived that he supported my ideas. Of course I made it clear in the acknowledgement that Waldyr did in fact NOT support my claims.

WMAP Data Release Imminent?

March 9, 2006
Categories: Uncategorized

Following up on the spectacular first year of data reported by the WMAP experiment in February 2003, results of analysis of the next year's data were initially planned to be released a year later. The release of this data has been repeatedly delayed; for speculation about why, see here and here.

Over the last two years there have been a large number of scheduled colloquia by WMAP scientists that have been cancelled at the last moment. One relatively recent one was an astronomy colloquium at Caltech by Gary Hinshaw with the title "New Results from WMAP" that was initially scheduled for January 4, but was cancelled and now is scheduled for March 22.

From March 23-25 the new Center for Cosmology at UC Irvine will be hosting a workshop on Fundamental Physics With Cosmic Microwave Background Radiation. On the 23rd four WMAP team members are scheduled to speak with titles: "WMAP 0, WMAP I, WMAP II, and WMAP III."

Over at CosmoCoffee (where I first noticed this), Alessandro Mechiorri is spreading the rumor that data will be released next week, on March 14. There's also discussion of this at the Bad Astronomy and Universe Today Forum. So, looks to me like it's maybe next week, and if not, almost certainly the week after.

Update: More news about the state of WMAP from Urbano Franca here.

Update: Looks like it definitely will be very soon now, possibly some time this week. The WMAP Mission Status page says:

Second Data Release

While the first-year results were based mainly on temperature measurements, the continued mission operations are now primarily focused on the much weaker polarized signals – an invaluable "stretch" goal of the extended mission. Analyses of these weaker signals are more difficult. The calibration and systematic error analyses have been completed, and the data files have been documented for use by researchers. For an overview see RESULTS.

This last link is to a New Three Year Results page which right now just says:

Stay tuned for the release of the most recent results from the Wilkinson Microwave Anisotropy Probe (WMAP). Very soon now….

Update: It's official, the release will be on Thursday. Here's the announcement:

Dear colleagues:

We are pleased to announce that the next release of WMAP data, along with papers describing the results, are expected to be available on LAMBDA this coming Thursday, March 16 at 12 noon EST.

http://lambda.gsfc.nasa.gov/

There will be no televised press activity associated with this release, so in an effort to reach as wide as possible an audience, please forward this announcement to colleagues of yours whom you think may be interested.

Thank you very much – we look forward to seeing your analyses of the data!

Sincerely,
Gary Hinshaw
NASA/GSFC
for the Legacy Archive for Microwave Background Data Analysis (LAMBDA)

Comments

1. **Alejandro RIvero**
   March 9, 2006

   WMAP is the "big mover" in the SPIRES table of citations. astro-ph/0302209 is now in the 7th place, jumping from a 37th last year.

2. **Dumb Biologist**
   March 9, 2006

   I was, as soon as I heard of it, intrigued by the notion that the universe might be finite, non-Euclidean at the largest scales, and that its topology would make it "multiply connected". I guess, beyond the quadrupole and octupole anomalies (which I've read might suggest interesting topology, but, pessimistically, could likely rather suggest some kind of directional bias in the measurements, as they align with the ecliptic), the evidence points to flat and infinite. Which is kind of depressing (if I understand things at all correctly), as it might mean we really have no idea what else is out there beyond our horizon, and never will. Seems reasonable to me the delay is due to attempts to get a firm grip on the source of the anomalies, and determine whether or not it's a foreground issue.

3. **woit**
   March 9, 2006

   Alejandro,

   Thanks a lot for generating that list and for the link to it. Do you have any idea about how to generate a list of most heavily cited papers not of all time, but just for 2005? I wrote to the people at SLAC asking if they will be doing this, but haven't heard back.

4. **Alejandro RIvero**
   March 9, 2006

   Peter, sorry I have tried (I had tried yesterday) but I can not see how to calculate the 2005 most cited from only the. Neither the email interface to SPIRES nor the web one give a hint about how to access the citation count fields. I think that the yearly calculation needs some special script running on the database.

The Spires people is usually hepful, er, helpful. It is only they take its time to answer. But I got finally a good feedback when doing my genealogy projects, and they even implemented the genealogy posibility to hepnames (it has problems with name duplicates, so again it seems is not so easy to interface hepnames from the web).

5. **Alejandro RIvero**
   March 9, 2006

   "from only the available indexes"

6. **Christine Dantas**
   March 9, 2006

   (…)data will be released next week, on March 14.

   Nice date. Einstein´s birthday (and also mine [grin]).

   Thanks for bringing these rumors to our attention.

   Christine

7. **Alejandro RIvero**
   March 9, 2006

   Now I think about, it could be possible to use the difference between current and 12/2004 citation counts to get an estimate of the "only2005"; the problem should be about new papers in the list, that should get a default "999" or so, or to approximate from citebase plots. Hmm, to much time needed.

8. **andy**
   March 9, 2006

   Dumb Biologist and Everybody,
   Could the results from the WMAP change our ideas about whether or not the universe is finite? I am just ignorant on what questions it is aiming to answer.
   -Andy

9. **amanda**
   March 9, 2006

   "the evidence points to flat and infinite"

   The evidence can point to flat, but obviously it cannot point to infinite, though a lot of people who ought to know better will try to tell you otherwise. There are some people who have a theoretical bias in favor of infinite spatial sections, but [a] these arguments are very dubious indeed and [b] there is no possible way that the observations can ever give us any reason to believe in infinite sections. In fact, many of us regard infinite spatial sections as a version of the landscape……

10. **Dumb Biologist**
    March 9, 2006

    Andy: Being a dumb biolgist, I kind of have to trust the astrophysicists on this one a bit, and others with the head for maths to cast the actively critical eye that I lack. I'm not sure if I have many ideas to change, just an active and enthusiastic interest in what I can only characterize as "awesome" ideas like the universe having a doughnut topology, or some other such mind-bending sort of concept.

    Amanda: Can you elaborate? Because part of the reason I find such infinities a little discouraging is precisely because it smells like Landscapology. In fact, I recall reading a Scientific American article some time ago on the subject of an infinite universe that kind of disconcerted me. The bacis gist was even if the laws of physics are the same throughout this infinite section, as you put it, there are other Earths out there, with other copies of myself. Perhaps many. I guess there just have to be if the chance of such a configuration is non-zero. And maybe on some of those other Earths I'm a leading String Theorist. The whole time I was reading this I was thinking "Oh, *come on*." That's the annoying thing about infinity. Anything that can does happen. How can you deal with that scientifically?

    Now that I think about it, Andy, I guess I have to admit I do have some biases…and yeah, I probably would have to change my mind about some things. Oh well.

11. **andy**
    March 9, 2006

    Dumb Biologist and Others,
    The reason I was directing the question to you was that you mentioned evidence about "flat and infinite." I was thinking that you don't have to be a researcher in the area to know what "the party line" is about what the WMAP data is addressing and maybe you had read something where they explained that this is what they were addressing with the WMAP data. From Amanda's post it seems that the idea of an infinite universe isn't even on the table. After I posted my question I thought about it and I thought that they would have to be questioning the big bang if they thought the universe was not finite. I didn't think they were doing that. So maybe Amanda will tell us some more or maybe somebody else will.
    -Andy

12. **Aaron Bergman**
    March 9, 2006

    One can never tell that the universe is infinite because we can only see a finite fraction of it. It's always possible that nontrivial topology is just beyond the horizong.

13. **Dumb Biologist**
    March 9, 2006

    Andy:

    Well, what can I say? I keep reading over and over that the WMAP data is reported to fit pretty darn well with a model of the univers that is "flat and infinite". Now, sometimes folks say the Universe (by "Universe" I mean, of course, what is within our horizon) is infinite in the sense that it will continue to expand forever. Sometimes they seem to be saying "The Universe" (meaning that which is not necessarily within our horizon) really is *infinte*. Always was, always will be. So, certainly all that we see now once occupied a space no bigger than an atom, but that's obviously not all there was in this version of the idea. The visible region of space expanded (and continues to do so), but maybe the same was going on beyond our horizon, as far beyond as you like. Or maybe something else is going on. I keep thinking "well how can we *know* that?", and, per Aaron Bergman's statement above, I don't understand how anyone could assert anything other than "We can't. Not really."

14. **Antonio**
March 9, 2006

Hi all,

I'm a mathematician and my knowledge of physics and it's terminology is feeble, so I have to questions:

1) When you are saying infinite or finite in this discussion, you mean non-compact or compact spacetime respectively?

2) It makes any physical sense to spacetime being a manifold *with* border? If the answer is yes, the border is (+++) or (++-) and why so?

Thanks in advance

15. **Dumb Biologist**
March 9, 2006

Well, don't take my word for it, but my understanding is the WMAP data (among other things) implies an "open" universe, which is, I guess, by definition, non-compact, i.e. unbounded.

16. **Aaron Bergman**
March 9, 2006

It's a bit trickier than that, unfortunately. Space*time* is always noncompact because time seems to be infinite in the future. So, what one ought to mean is that *space* is compact or not. The problem with that, however, is that there usually isn't a distinguished foliation of spacetime by spacelike hypersurfaces. Worse, you can sometimes foliate the same spacetime by compact and by noncompact spacelike hypersurfaces.

Nonetheless, you can still ask things like does any closed spacelike surface (w/o boundary) exist. You can also look at the spatial curvature. If it's positive everywhere, then you better have a compact manifold if I remember my math correctly. If you could, for example, shoot yourself in the back, that has consequences when you look at the sky and people look for those effects.

Or something like that.

17. **D R Lunsford**
March 10, 2006

Can we trust this data to be free of neoconservative spin, so to speak? And why the f=ck has it taken so long? One hopes there was an apparent anomaly that could not be ignored, and has now been assigned an unknown physical cause because the anomaly is real and can't be ignored (correlation with the ecliptic plane for example).

-drl

18. **amanda**
March 10, 2006

What I'm going to say is my understanding after speaking to a real expert — ie, modulo possible misunderstandings on my part:

Aaron Bergman said:

"It's a bit trickier than that, unfortunately. Spacetime is always noncompact because time seems to be infinite in the future."

You're assuming that it will always accelerate. We don't have any reason to believe that it will….but anyway, to be really strict about it, a spacetime with a bang and a crunch is non-compact even though its total age is finite [open intervals and all that….]

"So, what one ought to mean is that space is compact or not. The problem with that, however, is that there usually isn't a distinguished foliation of spacetime by spacelike hypersurfaces. "

Actually, usually there is: the spacelike sections in which one sees isotropy. The main exception is deSitter, which contains nothing but vacuum energy anyway so it looks isotropic to lots of different observers!

"Worse, you can sometimes foliate the same spacetime by compact and by noncompact spacelike hypersurfaces."

I think I would say "various parts of the same spacetime", eg you can't foliate *all* of deS with non-compact spacelike hypersurfaces [I think!]. Remember that *all* of dS has the topology of a line times a sphere….

"Nonetheless, you can still ask things like does any closed spacelike surface (w/o boundary) exist. You can also look at the spatial curvature. If it's positive everywhere, then you better have a compact manifold if I remember my math correctly. "

Assuming they are complete, yes.

"If you could, for example, shoot yourself in the back, that has consequences when you look at the sky and people look for those effects."

People look for such things, but they have not seen them [yet?]. But absence of evidence is not evidence of absence…

"Or something like that. "

Agreed. 🙂

19. **csrster**
March 10, 2006

So the most likely result is that space is _locally_ infinite?

🙂

20. **Dumb Biologist**

March 10, 2006

Please forgive my ignorance…

Now I know a manifold is something that might look Euclidean up close, but globally could be more complex. A foliation is…? Looks like it's something with a bit more variety globally, yet locally describes the manifold. Is this at all correct? If you don't mind helping me a little with pictures, if I were to take a spherical manifold and foliate it in some allowed way (I assume not any sort of foliation is allowed), what would those foliations look like, and what would/could they tell me about the sphere?

21. **woit**
March 10, 2006

D.B.,

We're getting off topic here, but in this context a foliation just means that you can globally choose hypersurfaces of constant time that can be interpreted as "space".

More generally a foliation of a manifold is something like a choice of families of submanifolds that fit together to fill up the manifold. One simple example is a fibration, i.e a foliation where all the "leaves" (in this case called "fibers") of the foliation are isomorphic, and locally the manifold looks (fiber X nbhd. of point).
A famous example is the Hopf fibration of the 3-sphere, a foliation by circles. The general story of what foliations of spheres can look like is quite intricate.

22. **Aaron Bergman**
March 10, 2006

You're assuming that it will always accelerate.

No. Just that there's no big crunch. (Although, as you say, there are compactness issues associated to singularities that I was ignoring.)

Actually, usually there is: the spacelike sections in which one sees isotropy.

I was speaking generically; most spacetimes are not isotropic.

"Worse, you can sometimes foliate the same spacetime by compact and by noncompact spacelike hypersurfaces."

*I think I would say "various parts of the same spacetime", eg you can't foliate *all* of deS with non-compact spacelike hypersurfaces [I think!]. Remember that *all* of dS has the topology of a line times a sphere….*

You can, however, take a quotient of dS so that you've covered the whole thing, IIRC.

People look for such things, but they have not seen them [yet?]. But absence of evidence is not evidence of absence…

As I said above, there's always the possibility that these things are lurking just behind the horizon. All we can do is observe what we've got and talk about that.

23. **Dumb Biologist**
March 10, 2006

OK, sorry to get off topic, and thanks for the brief lesson!

24. **Dumb Biologist**
March 10, 2006

drl: I can't imagine any other reason for the delay that makes sense, given what is known so far. It appears they've already seen enough to know there's either some kind of foreground contamination they can't get rid of without great difficulty, or there's new physics of some sort. It seems quite reasonable to expect they're working hard to make whatever data they release be of the highest quality so they have the highest confidence possible it's either one or the other.

25. **D R Lunsford**
March 10, 2006

Db, let's hope.

Something I'm working on predicts an EM field whenever there is a gravitational field (that is, EM is part of the geometry). If there is a definite correlation with ecliptic, I get a way to determine the effective coupling and so a completely new way to determine the CMB temperature.

How are the raw data handled? Are they available to all "active researchers" who wish to study them?

-drl

26. **woit**
March 10, 2006

Danny,

Data from WMAP is made available at

http://lambda.gsfc.nasa.gov/

How "raw" it is, you'll have to decide for yourself.

27. **Urbano**
March 10, 2006

Peter, I have also commented about that here.

28. **Dick Thompson**
March 10, 2006

For those of us who can read it Padmanabhan's new paper "Dark Energy, Mystery of the Millenium" http://www.arxiv.org/abs/astro-ph/0603114, is relevant to the finite/infinite question. He takes our limited access seriously: every class of observer has a horizon, and every 2-surface in spacetime can be the horizon for some class of observer, so what can we deduce from that? And he deduces quite a bit!

29. **Urbano**
March 10, 2006

Thanks for the update, Peter :-)!

30. **Dumb Biologist**
March 10, 2006

From D.T.'s cite:

"…it is obvious that the cosmological constant is telling us something regarding quantum gravity, indicated by the combination Gh-bar. *An acid test for any quantum gravity model will be its ability to explain this value*; needless to say, all the currently available models— strings, loops etc.—flunk this test."

That's gotta be irritating to somebody…

31. **Michael**
March 10, 2006

Peter, you mentioned that you would like to publish some of your latest research, probably later this year. Would you mind telling us what you have been working on? Any amount of detail you're comfortable with would do.

32. **woit**
March 10, 2006

Michael,

This is completely off topic, and given all your other comments here, I doubt you actually have any real interest in the answer.

I've mentioned this several times, but the main thing I've been working on is the relation between 2d quantum Yang-Mills theory coupled to chiral fermions and representation theory. The Freed-Hopkins-Teleman theorem is a part of this story, and one of the main conceptual problems I've been struggling with is that this seems to require a different way of thinking about BRST than is usual. If I get the BRST issues completely sorted out, I'll first write that part of this up separately.

At some point in the next few months I'll try and write some longer blog postings at least explaining some of the mathematics background, stay tuned. But I'm not going to discuss this anymore here in a comment section of a posting on WMAP. If you have a serious interest in it and want to discuss it, feel free to contact me via e-mail.

33. **Kris Krogh**
March 10, 2006

WMAP isn't the only NASA physics experiment where the release of data has been delayed mysteriously. Gravity Probe B was launched in April 2004 and finished collecting data last August. NASA's post-launch schedule called for announcing the results next month. That's been pushed back a full year.

The $700 million experiment is an extremely sensitive gyroscope in a drag-free polar orbit, whose orientation is compared to a guide star by an on-board telescope. It's designed to measure two effects predicted by general relativity: One is a geodetic precession of the gyro axis in the plane of the probe's orbit. The second is the Lense-Thirring effect — a dragging of inertial frames caused by Earth's rotation, causing the axis to precess in its rotational plane.

The precise outcome won't be known until the probe's data is combined with measurements of the guide star's motion, from a separate group. Those have been embargoed — so it can't be said the gyro analysis was influenced to give a particular result. Still, the guide star's approximate motion is known from previous measurements by the HIPPARCOS satellite. And if the Lense-Thirring effect is absent (which I'm predicting) that would be apparent already.

It's interesting that while NASA is forced to drastically cut science programs to fund the Bush Mars initiative, they've chosen to extend this one. According to the Gravity Probe B web site, not all the additional time will be needed for data analysis. That should be finished by summer's end. Until the following spring they'll be having the results vetted by various experts. (And maybe garnering support for an unexpected result.)

This additional one-year delay comes on top of many years of others in the probe's launching. And those of us who've staked our futures on the experiment's outcome are left hanging that much longer.

34. **D R Lunsford**
March 10, 2006

Peter, don't lower yourself to respond to Pseudonymous Milquetoast.

-drl

35. **wolfgang**
March 11, 2006

Kris,

> NASA's post-launch schedule called for announcing the results no later than next month. That's been pushed back a full year.

As far as I remember, it was always clear that no results would be announced after just one month. Gravity probe B is a unique experiment which will probably not be repeated any time soon. Thus it is very important to get data analysis (by different independent teams) right, which simply takes some time.

There is already a lot of evidence for the Thirring-Lense effect and I would not expect a negative result. But it will be very interesting to see the details of their report.

36. **Brett**
March 11, 2006

The strongest evidence for gravitomagnetism comes from binary pulsar measurements, and this is quite compelling. The Lens-Thirring prcession is really nothing more than the most elementary consequence of gravitomagnetism. It is possible that there might be a discrepancy between the observed precession rate and the one predicted by General Relativity; that would be tremendously interesting, although I still think it's unlikely. However, it would be amazingly improbable that no dragging of frames at all was observed by Gravity Probe B.

The odds of seeing no Lens-Thirring precession at all, is based on evidence from other areas, practically nil. I do not believe this because I have some vested interest in accepted theories being proven true; indeed, precisely the opposite is true. I believe the GR gravitomagnetic predictions will be borne out simply because the evidence is so good in other sectors.

37. **woit**
March 12, 2006

Please stop with attempts to turn this comment section into a discussion forum for one's favorite alternative ideas about cosmology.

38. **amanda**
March 12, 2006

For some information about what will *not* be in the WMAP data, and some interesting advice to Leonard Susskind about grant proposals, see

http://arxiv.org/abs/astro-ph/0603266

39. **woit**
March 12, 2006

Thanks amanda, I hadn't seen that. Will soon write a posting about this and other news of the multiverse.

40. **Aswin**
March 13, 2006

"Three year WMAP probe observations" Talk by E.Komotsu
– I guess it cant get more explicit. The talk is on Apr 11.
Look here.
Got this from cosmocoffee.

News of the Multiverse

March 13, 2006
Categories: Multiverse Mania

When I was reading Susskind's book The Cosmic Landscape, I was paying close attention to the main problem with the whole multiverse/anthropic string landscape idea: is there any sort of experimental prediction that emerges from this that would justify calling it science? One thing that kind of mystified me was Susskind's claim, in the Introduction and Chapter 12, that the "cosmic horizon" beyond which other parts of the multiverse live is like a black hole horizon and in principle information about what is beyond the horizon is accessible in the analog of Hawking radiation. This seemed to be a rather vague idea, which Susskind goes on to drop, never mentioning it in the chapter he devoted to possible experimental tests of the landscape. Since I'd never heard of anyone claiming this anywhere else, and Susskind didn't seem himself to take it very seriously, I just ignored it.

Cosmologist G. F. R. Ellis, in a new preprint entitled On horizons and the cosmic landscape has decided to take it seriously, and show that it is wrong. Ellis's paper is rather peculiar; I've never before seen an arXiv paper that argues against not another scientific paper, but some vague statements in a popular book. I haven't tried to follow Ellis's argument, partly because it seems rather vague itself, with not a single equation in it. Perhaps this is unavoidable, given the vagueness of Susskind's argument that he is challenging. Anyway, at the present time, the situation seems to be that neither Susskind nor anyone else has come up with a calculation that would show how to detect information about other parts of the multiverse hidden in some sort of Hawking radiation from a cosmic horizon, and now we have an argument from Ellis that this is in principle impossible.

Some people have been giving me grief about writing blog entries with no equations, but here no one seems to have any.

If you want to hear more from Susskind about the multiverse, he's giving the colloquium next week at MIT with title The Landscape and the Megaverse, and the abstract of this talk is:

A new paradigm for the origin of the laws of physics may (or may not) be emerging out of observational cosmology and theorists efforts to understand string theory. The ordinary 15 billion light-year universe is being replaced by a vastly bigger "megaverse" consisting of a huge number of what Guth calls "pocket universes." If this is true then many of the Laws of Physics that we normally think of as "written in stone" will be local environmenal facts. I will explain the evidence for this controversial view, its implications, and the various views of leading physicists and cosmologists.

Susskind is also giving a talk here in New York on April 10 at the New York Academy of Sciences. The description of the talk tells us that

Several decades ago, Susskind introduced the revolutionary concept of string theory to the world of physical science. In doing so, he inspired a generation of physicists who believed that the theory would uniquely predict the properties of our universe. Now Susskind argues that the very idea of such an "elegant theory" no longer suits our understanding of the universe….

…Susskind believes that string theory, rather than reaching a dead end, has led to a vastly expanded concept of the universe, which he calls "the lanscape," where the anthropic principle makes perfect sense.

Attending the talk would cost $20, so I think I'll skip this one.

The last issue of the NYAS publication Update Magazine has an article by Lee Smolin on all of this entitled A Crisis in Fundamental Physics. Later this year Smolin will have a new book out, entitled "The Trouble With Physics".

There is something I would very much like to attend, but will be out of town so will have to miss. The American Museum of Natural History each year organizes a debate in honor of Isaac Asimov. This year the topic will be Universe: One or Many?, and the blurb goes:

Join a panel of cosmologists to argue and debate the possibility that our Universe is just one of many universes that comprise the "multiverse." This notion invokes dimensions beyond our everyday experience and draws from the leading edge of our conception of the cosmos. The presence or absence of data in support of these ideas forms a central theme for the evening.

I'm not sure who is going to argue for the presence of data in support of these ideas since I've never heard of any. The panelists are Michio Kaku and Andrei Linde, presumably pro-megaverse, Lawrence Krauss, who I'm guessing is on the anti-side, and Lisa Randall and Virginia Trimble, about whose views on the subject I know nothing.

Comments

1. **Aaron Bergman**
   March 13, 2006

   Ugh. Kaku again.

2. **Tung**
   March 13, 2006

   In that article, Smolin is saying that the pragmatic style (the American style) of doing physics is posing great harm to the current progress of physics.
   In Einstein's words, our generation is one that filled with mere artisans rather than real seekers of the truth.

3. **Chris Oakley**
   March 13, 2006

   It seems that the anthropic evangelists would rather destroy the subject altogether than forego the opportunity to preach their foolish philosophy.
   Let us be clear on this: the public may not understand Calabi-Yau manifolds or heterotic strings, but they certainly understand the basics of scientific reasoning, and are perfectly capable of identifying situations where certain people choose to abandon such principles. If Susskind *et al* manage to convince people that fundamental physics has now ground to a halt, how can this be good for funding?
   A lot of string theorists seem to want to silence Peter, but it seems to me that the anthropists are the ones who are desperately in need of censorship.

4. **anon**
   March 13, 2006

   Peter, the relationship between black hole information loss, assuming the universe is a black hole, and Hawking/Susskind/t'Hooft was discussed in a here on this blog last July, although the link from Google doesn't work.

My recollection is that Hawking first showed in say c1976 that information gets lost in black holes. He gave a lecture in San Francisco a couple of years later, that Susskind and 't Hooft attended. They bought Hawking's idea and applied it to the universe, on the assumption the universe is a black hole. Recently, say c2004, Hawking found an error in his original argument.

It is sweet that Susskind is under attack now from G. F. R. Ellis, who is co-author of Hawking's major treatise, "The Large Scale Structure of Spacetime".

5. **Anon.**
March 13, 2006

Peter, I find it strange that you are harshly critical of string theorists but allow others to make sweeping and ridiculous claims without comment. I found Smolin's article to be highly disingenuous. For instance, he says "Theories of quantum gravity include twistor theory, causal set models, dynamical triangulation models, and loop quantum gravity. One reason string theory is popular is that there is some evidence that it points to a quantum theory of gravity."

On the other hand I think it is clear to most theorists that, despite its (many) problems, string theory *is* a quantum theory of gravity. Smolin's "there is some evidence" is vastly understating the case. However, it is *not at all* clear that loop quantum gravity is a *theory* in the technical sense in which physicists use the word "theory." Naively quantizing gravity, one runs into the problem of nonrenormalizability, i.e. of infinitely many undetermined couplings, so we say that this is not a theory. It is not at all clear that LQG avoids this problem, and in fact it appears quite likely that it does not. Even Lee admits that he does not know whether there are infinitely many undetermined couplings or not!

Here is a thread from your blog in which this is spelled out pretty explicitly.

So, given that it is not clear that LQG is a theory at all, whereas string theory is at least that, I find Lee's article troubling. If LQG eventually proves to contain some new principle that restricts possible interactions and creates a sensible theory, that would be exciting, and the community would not ignore it. But until then it is justifiably ignored by most of the community. Appealing to non-experts and claiming to be part of a grand philosophical tradition are moves that I find distasteful.

(I find the landscape equally distasteful, but clearly that receives plenty of attention already. I just fail to see why you appear to encourage one sort of non-scientific hype while frowning on others.)

6. **woit**
March 13, 2006

Anon,
The LQG/string theory war is off-topic here, it has nothing to do with this posting. I mentioned Lee Smolin's article in passing because it is mainly about the multiverse and was published in a NYAS publication, thus relevant to the topic at hand.

I strongly disagree with your characterization that string theory is a theory, LQG not, and don't see anything wrong with what you quote from Smolin. There are serious questions still up in the air about both of them. In the case of string theory, no, it is not a consistent quantum theory of gravity in 4d, since all we have is a divergent perturbation expansion.

The current situation is that no one has anything that I would call a really satisfactory theory of quantum gravity, one that is completely consistent and well understood, and either makes testable predictions or explains in a convincing way how the standard model is related to quantum gravity. As long as this is the case, my point of view is that people should be trying as many different kinds of ideas as possible. The problem with string theory is not that some people are doing it, but with the way it is being fanatically pursued, despite its by now obvious failures, driving out people who try and work on other ideas. If you think ideas about quantum gravity should be "justifiably ignored" because they have problems, you need to ignore everything.

Personally I see some of the ideas the LQG people are investigating to be much more promising than the ones used in string theory, others will differ. Until either side comes up with a completely consistent theory that makes testable predictions that are then checked, reasonable people can differ about this.

Please, take the polemical string theory/LQG arguments somewhere else. They don't belong in the comment section to this posting.

7. **Christine Dantas**
March 13, 2006

I do not know about Trimble's views on this issue, but I have
found an interesting passage that she quotes:

> We understand the concern of cosmologists that unbridled speculation should not take over the field, that it is better to persist with the standard model, warts and all, than for opinions to become splintered, with the decline of professional standards which would then almost inevitably ensue.
>
> Our response to this point of view, with which we have some sympathy, is that undesirable fragmentation has been permitted already, through the invasion of cosmology by Particle physicists. If the invasion had the precision and the certainty of earlier invasions of astrophysics by atomic theory and nuclear physics, the consequences would obviously be positive. However, one can have reservations about the advantages of becoming caught up in speculations from a different field, especially when those speculations are announced with an air of authority that will probably turn out to have been taken too seriously.

[Attributed Sir Fred Hoyle, Geoffrey R. Burbidge, and Jayant Narlikar, according to Trimble´s article *"Can't You Keep Einstein's Equations out of my Observatory?"*, BeamLine 29, No. 1, p 21-25 (1999).]

Interesting enough, as she points out, these researchers are well-known for their relatively non-mainstream contributions to cosmology.

But I must restate: I do not know her present opinions on the multiverse issue.

8. **Another Anon**
March 13, 2006

I agree with the other Anon that Smolin's article is a bit distasteful and self-serving. He seems to be seizing on the multiverse/landscape situation as an opportunity to knock string theory off the throne and replace it with LQG and related approaches, exchanging one brand of hype for another. Not much in this for peasants like me working on such mundane topics as ordinary gauge theory (boring things like trying to construct nonperturbative chiral gauge theories, so as to have a chance of determining whether that tedious hypothesis about sponaneous gauge symmetry breaking in electroweak theory is true or not.) Doesn't sound like there would be any more crumbs for us if King Witten were to be replaced by King Smolin.

9. **woit**
March 13, 2006

Another Anon,

Undoubtedly Smolin would like to see less resources and attention flowing to string theory and more to LQG. Given the current huge imbalance on this issue in favor of string theory, this is not at all unreasonable. I don't see the slightest danger any time soon of a "King Smolin" controlling the bulk of resources in the field and suppressing alternative points of view.

I think I share your point of view that the problems I care most about are being addressed neither by string theory nor LQG. But, again, this really is not the place for more battles about string theory vs. LQG.

10. **Aaron Bergman**
March 13, 2006

This is pretty off-topic and Peter's welcome to delete it if he wants, but….

You know, I'm trying to be polite to Lee over on another blog, but my god. That article is so unbelievably tendentious, self-aggrandizing and condescending that it passes into the realm of dishonesty. You want to know why you get such a bad reaction from other physicists, Lee? It's not because string theorists hate LQG. It's because of supercilious articles like that.

11. **woit**
March 13, 2006

Aaron,

You're right that this is both off-topic, uncivil, as well as stupid to boot, and I should delete it. Instead I'll leave it up as yet another indication of the kind of ugly behavior that string theorists insist on engaging in these days.

Look, Lee is someone who has always been completely civil to everyone, in his comments on blogs and in everything he writes and does elsewhere. He has maintained this civility in the face of absolutely appalling behavior towards him by Lubos Motl (behavior that no one in the string community sees fit to publicly denounce or take any action against), as well as a less than civil tone from you, Jacques and others. For you to be complaining about Lee's behavior as "so unbelievably tendentious, self-aggrandizing and condescending that it passes into the realm of dishonesty" and "supercilious", given the tone and content of the way string theory has been promoted and the behavior of your string theory colleagues, both close ones and not so close ones, is completely ridiculous.

Just knock it off with the personal attacks, I'm sick to death both of being the target of this kind of shit from people like Lubos, Jacques (and many others), and of seeing you throwing it at people like Lee, and anyone who dares to point out the problems with string theory. If you think something someone writes is inaccurate or you have a scientific criticism to make, go ahead and make it in a civil fashion. If you can't do that, shut up. In any case, stop polluting this field with this kind of nastiness.

12. **Aaron Bergman**
March 13, 2006

Lee's "civility" has begun to feel to me to be tremendously superficial and political. He keeps a tone of civility while being substantively dishonest. It's getting to be offensive. At least Lubos doesn't hide his disrespect for others behind a false guise of comity.

You accuse me of "throwing [personal attacks] at … anyone who dares point out the problems with string theory." Please give me an example. I'm quite googleable. If you can't, don't foist on me the actions of others. Perhaps you've forgotten the various times when I've agreed with various criticisms of string theory?

As for the scientific criticism of Lee's tired points, that's been done a zillion times both here and on Cosmic Variance. That Lee seems to not have listened to any of it at all is part of the problem.

And WTF am I supposed to do about Lubos? I lost the code to the orbital mind-control satellites a few years ago, and I think it would be clear by now that Lubos isn't going to listen to anyone. I'm open to suggestions.

13. **Who**
March 13, 2006

Aaron, I was surprised by your latest comment, in reference to Smolin's NYAS article *A Crisis in Fundamental Physics*. I re-read the article and could find nothing that I could imagine calling "unbelievably tendentious, self-aggrandizing and condescending" or "supercilious " or smacking of "dishonesty".

The article has very little to say about LQG and the imbalance in theory research funding. It really is about what it says in the title: a crisis in fundamental physics.

I urge anyone who can to take an objective unemotional look at the article. It is clearly targeted at a passage by Steven Weinberg which Smolin quotes. Weinberg depicts a major foundations crisis situation–a "turning point" in the "history of science" (not merely the history of physics, he would have it, but science as a whole)—and suggests that humanity may have to relax the standards by which science is done and the criteria of what is science. In particular, Weinberg refers to the current tendency to "legitimate anthropic reasoning as a new basis for physical theory".

The main things Smolin does in the article are simple to state: he describes Weinberg's position, and he opposes it. He describes the circumstances which have brought matters to a head. He takes Weinberg's statement as a confirmation that there is a (philosophical) crisis in physics—a crisis having to do with foundations—and he gives an historical account of how he thinks this crisis came about.

Then he states his main thesis in this key paragraph

"**I believe we should not modify the basic methodological principles…Science works because it is based on methods that allow well-trained people of good faith, who initially disagree, to come to consensus about what can be rationally deduced from publicly available evidence. One of the most fundamental principles of science has been that we only consider as possibly true those theories that are vulnerable to being shown false by doable experiments.**"

Smolin says we should NOT change the basic methodological principles. Theorists, in his view, should continue the practice of proposing theories that are "vulnerable to being shown false by doable experiments." This is what allows the scientific community to function, by making differences empirically resolvable.

I think that to sustain this precept is not self-serving on Smolin's or anyone's part. I rather think it is a service to physics and the scientific community in general.

It seems to me that you can disagree with Weinberg and Smolin about the magnitude of the unresolved foundation issues and the seriousness of the present situation in physics. You can argue that there is no crisis, and there is no major "turning point". Or, you can concede the gravity of the situation and come down on Weinberg's side, against Smolin.

But I don't think you gain respect by simply bad-mouthing Smolin's article—calling it "supercilious" and "condescending" etc.

Nor do I suspect that you speak for very many string researchers when you say such things.

14. **Anon.**
March 13, 2006

Who says:

The main things Smolin does in the article are simple to state: he describes Weinberg's position, and he opposes it.

You seem to have read a different article than Aaron and I did! There was a bit about that, yes — a few short paragraphs in the middle. But I see this as sort of a diversion from the main thrust of Lee's article, which is stressing the need for thinking about "conceptual puzzles" and "foundational issues." I don't mean to be off-topic and polemical, I'm just trying to respond to what I see as the central focus of Lee's article: namely, his assertion that most physicists don't care about conceptual or foundational issues, whereas he does. This is what I see as condescending, and I think it's what Aaron was referring to as well. The implication that no one else is willing to think deeply is made pretty bluntly, and it is offensive. Lee explicitly links his own work — and that of his collaborators — with that of "Einstein, Bohr, Mach, Boltzmann, Poincare, Schrodinger, Heisenberg." Is this not self-aggrandizing?

Peter, if this is off-topic, feel free to delete, but I think I'm clearly replying to the text of one of your links…. Just because (some) string theorists often make overhyped and arrogant statements, it doesn't excuse similar statements from those opposed to string theory.

15. **woit**
March 13, 2006

I'm not going to tolerate a stupid off-topic flame war here, so will delete any more comments on this thread (after exercising my prerogative as blog owner to have the last word). If someone has an intelligible scientific point to make, make it, but I won't tolerate either personal attacks or repetitive and tendentious LQG vs. string theory arguments.

You're perhaps right Aaron that I have unjustly accused you, however "tendentious", "supercilious", and "condescending" are words that seem to me to apply to some of your postings, most recent example the ones over at the blog belonging to Christine Dantas. But you're right that you don't ever behave like Lubos, and most of the time avoid behaving like Jacques.

I reread Lee's article, and found it perfectly reasonable (although I disagree with him at some points: I'm much less optimistic than him that work on the foundations of QM will get him what he wants, and less convinced that LQG now leads to solid, testable predictions). The main point he is making is both very true, exceedingly important, and rarely said in public by a theorist: if one believes that string theory leads to a landscape such that there is no plausible way of testing it, it is one's duty as a scientist to abandon work on it, not to start promoting some new paradigm of how to do untestable science.

I see not the slightest evidence the Lee is being dishonest at any point, whereas the vast promotional string theory literature at this point just reeks of dishonesty. As you might guess I also happen to feel that I've been personally treated in an exceptionally dishonest and unethical way recently due to my opposition to string theory.

What can you do about Lubos or others who behave badly? Come on, when he posts something offensive, write in a comment saying so. If you and all the other string theorists reading these blogs did this regularly, he might actually change his behavior, or at least you would make it clear that the string theory community doesn't support him. Instead, his idiotic nastiness goes unchallenged, with string theorists doing things like writing comments supporting what he has to say, except perhaps noting that he is a bit "undiplomatic". And he's not the only string theorist whose behavior people should challenge, just the craziest.

16. **Aaron Bergman**
March 13, 2006

Feel free to delete this one, too, Peter. If it does get you can leave that "Who" is welcome to e-mail me or tell me of another forum in which I can respond.

In addition to Lee repeating various incorrect claims about string theory and 'background independence' (something pretty much guaranteed to make me irate), this paragraph was particularly bad:

> Meanwhile, many of those who continue to reject Einstein's legacy and work with background-dependent theories are particle physicists who are carrying on the pragmatic, "shut-up-and calculate" legacy in which they were trained. If they hesitate to embrace the lesson of general relativity that space and time are dynamical, it may be because this is a shift that requires some amount of critical reflection in a more philosophical mode.

I don't appreciate being told that I'm "reject[ing] Einstein's legacy", and hesitant to "embrace the lesson of general relativity" because this "requires some amount of critical reflection". This isn't an attack on string theory; it's an attack on the people who research string theory.

Rereading the essay, it's true that the majority of it probably does not justify my reaction (as I said, repeating the same incorrect claims about background independence got me angry), but that paragraph is particularly offensive. The self-serving aspect of this essay is how Lee continually sets himself as the defender of Einstein, background independence and deep physics as if no one in string theory ever thinks of such things. Plenty of such discussion occurs, although it usually doesn't end up in papers. One person who does publish papers with discussions along those lines is Tom Banks.

I think it's interesting that, Lubos excepted, most of the critiques about LQG by string theorists have to do with how the theory could match up with the real world, ie, anomalies, the semi-classical limit and, somewhat more loosely, black hole entropy. On the other hand, the critiques of string theory seem to be more philosophical, complaining that it's not background independent or that it has too many vacua to be predictive.

The latter critique is certainly legitimate (although still somewhat permature in my opinion *contra* both the anthropists and the anti-anthropists). But it is still a metaphysical complaint.

So, I think that Lee's dichotomy between the deep thinkers doing "background independent" quantum gravity and the "shut up and calculate" stringers leaves a lot to be desired.

17. **Eric Dennis**
March 13, 2006

By the same token, Smolin is "linking" the shut-up-calculate crowd's work with that of Feynman, Dyson, Gell-Mann, and Oppenheimer. His point is not to play "my guys are smarter than your guys". It's to note an interesting ideological division among physicists and ponder its relevance to the future progress of the field.

18. **woit**
March 13, 2006

Other comments came in while I was writing the last one, so I'll leave them.

No, Aaron, complaining that string theory can't predict anything and thus is not legitimate science is not "metaphysical", it's scientific method 101. What is "metaphysical" are the arguments used to justify continuing work on the landscape and string theory unification.

But I really don't want any more of this nonsense here. Just stop it. At this point I'll delete anything at all about this that anyone tries to put here.

19. **ObsessiveMathsFreak**
March 13, 2006

> Ellis's paper is rather peculiar; I've never before seen an arXiv paper that argues against not another scientific paper, but some vague statements in a popular book.

Interesting. This sounds like the kind of paper you would find in the humanities, where views on books are regularly discussed. Might this be the start of a new trend in physics publishing? The phrase "natural philosophy" comes to mind.

20. **Dumb Biologist**
March 13, 2006

I really don't understand Dr. Smolin's assertion that the "shut up and calculate" approach has somehow led science to the Lanscape. I rather thought the oringinal sentiment was to avoid wooly philosophizing about an approach that, while defying human comprehension, nevertheless made perfectly accurate and testable predictions. As Landscapology apparently addresses problems that are neither calcuable (i.e. NP-hard and intractable) nor testable, I'm not sure how "shut up and calculate" is even relevent. It's not at all clear to me how more wooly philosophizing is supposed to help when experimental verification of its fruits are no closer than what can be attained with any other approach. I'd say if the experiment isn't doable (which apppears to be true of any current approach to unification physics or quantum gravity), "shut up and do something else" might be good advice.

I'm not even a physicist, and I simply can't wait for some humongous collider to come along and give scientists something else to argue about besides philosophy. Like data.

21. **Chris W.**
March 13, 2006

This one is *meant* for deletion, and is inoffensive and helpful (I hope):

Typo alert: *…which he calls "the lanscape,"…*

22. **Chris W.**
March 13, 2006

One more typo: *…local environmenal facts…*

(Sorry for not including it the previous comment.)

23. **Another Anon**
March 13, 2006

Thanks for your reply above, Peter. The stuff I wrote about "King Smolin" was of course exaggerated for theatrical effect. But as someone who doesn't have anything at stake in the outcome of any strings vs LQG fight I still found parts of the article a bit objectionable. In particular toward the end where Smolin writes:

"So, while the new foundational approaches are still pursued by a minority of theorists, the promise is quite substantial. We have in front of us two competing styles of research. One, which 30 years ago was the way to succeed, now finds itself in a crisis because it makes no experimental predictions, while another is developing healthily, and is producing experimentally testable hypotheses."

At the risk of being paranoid, this sounds a lot like "The previous bandwagon has ended in a ditch, so now it's time to build a new bandwagon". (I'm no expert on the current state of LQG etc, but that last bit smacks of propaganda hype a la string theory.) I would have preferred a conclusion more along the lines of "Since the previous bandwagon ended in a ditch, perhaps bandwagons themselves are not such a great idea". Instead of trying to predetermine which philosophy or research program will turn out to be the best, how about encouraging people to follow their own ideas, and rewarding them purely according to the progress they make without preferential weighting for any particular research program. For many years the rewards for making progress in string theory have been way out of proportion with other areas. My worry after reading this article is that, if Smolin had his way, that would simply be changed to preferential weighting for progress on the "new foundational approaches" he advocates.

24. **woit**
March 13, 2006

Chris W.,

The typos are in the original, so I'll leave them. This whole subject is characterized by incredible sloppiness, why not sloppy spelling too?

Dumb Biologist,

My interpretation is that Smolin is arguing "shut up and do something else", the something else being thinking about foundational issues, ones that are neglected in string theory. The hope would be that if you do this you'll get some insight into a different, more promising direction than string theory, at which point you can shut up and calculate again. I have a somewhat similar view, but replacing studying foundational issues in QM, by looking for a deeper mathematical interpretation, and I prefer to start with the standard model rather than gravity.

Another Anon,

I think Smolin would agree with you that this is all about "encouraging people to follow their own ideas, and rewarding them purely according to the progress they make without preferential weighting for any particular research program" and your opposition to bandwagons. The problem with the way research is currently structured is that it is very hard to get attention for any formal research if it's not string theory. He's just making the argument that this is not good and pointing to one kind of research that he sees as having suffered because of the way string theory has driven everything else out. If miraculously he manages to get people to listen, becomes power-mad and constructs a new bandwagon designed to drive other ideas out of business, that would be a bad thing. I just don't see the slightest danger of that happening in the forseeable future.

25. **Zelah**
March 13, 2006

Reading this Blog has discouraged me about how High energy physics is conducted.I mean, crying foul at a webarticle? By String Theorists?

It seems to me that Lee is just politely speaking his mind. A totally legitimate thing to do! Most probably there is some sort of hidden insult that Lee is giving out to his fellow high energy physicist theorist?! Cannot take the heat Stringers? I say if you want to dish it out like say Lubos, then you have to take it on the chin!

My final thoughts. As an amateur, it seems to me that there is a whole lot of data waiting to be explained! Like Dark Matter / Energy.Surely, it is a serious indictment of String theory if it has NOTHING TO SAY on these matters? Worst in my book is to ask higher standards of ones competitors!

26. **Who**
March 13, 2006

The typos are in the original, so I'll leave them. This whole subject is characterized by incredible sloppiness, why not sloppy spelling too?

IIRC Susskind once posted something on arxiv about "Sting Theory",
You had a thread relating to it here, I believe. It was funny (not only the Freudian spelling, the whole thing).

A propos. Baez just gave a talk at Rovelli's CPT—he posted a link in yesterday's TWF #227—and at the end of the talk Rovelli asked him "doesn't this mean that the physics of the last 25 years is junk?"

and Baez agreed. this is from someone who was there and just posted it on PF. Baez talk and Smolin's essay are parallel in some respects, both illuminate embarrassing problems in fundamental physics

27. **Another Anon**
March 13, 2006

Thanks Peter, although it seems your reading of the article is quite different from mine. I guess we'll just have to agree to disagree.

28. **vb**
March 13, 2006

You've got an excellent blog, which I regularly read (via Google Reader, so I skip the replies most of the time, but anyway) and share similar sceptical views, but similarity between the Darwin theory of natural selection and the landscape idea is appealing. In your own opinion, what web site or blog would be the most antagonist to your blog? Thanks, vb.

29. **Lee Smolin**
March 13, 2006

Dear Aaron and others,

I apologize if you found what I said to be "condescending" and "offensive". My point is not to insult anyone and it is certainly not to support one trendy fashion against another. I meant what I wrote in the Physics Today article, which is that intellectually independent individuals with something original and important to contribute always deserve priority, even over people pursuing my own research program. This is what I do in practice, as I think those who know me will attest to.

The point of this article was not to make a polemic for LQG, which is mentioned just once (and then just in a list of theories) but to offer a new hypothesis as to why physics is in a crisis. To the people I inadvertently offended, I would ask you to look beyond labels to the facts I describe. It is just true that there is a small community of people whose scientific work is centered on "foundational issues." By this I mean those questions usually labeled such, for example the measurement problem in quantum mechanics, the problems of time or what is observable in quantum cosmology, or the relational/absolute debate about the nature of spacetime.

I don't think there is anything arrogant about pointing out that there are different communities, or that these communities have different styles and evaluate research according to different values. The foundations community, in my experience, values intellectual independence and depth of thought more than many people in more mainstream fields. This is partly because one needs acquaintance with the philosophical tradition to make a contribution here, and also because there being almost no professional opportunities in research universities for such people, they tend to be intellectually independent people who value their independence more than they do the usual sorts of professional success.

Nor is it "self-aggrandizing" to mention that this foundational tradition owes a great deal to and feels itself to be connected to Einstein, Bohr and other foundational thinkers of the first half of the 20th Century. Again this is just a fact. You are much more likely to find someone who has actually read the original writings of Einstein, Bohr, Boltzmann etc among the foundations community than you are among more mainstream communities.

There is nothing wrong with a theoretical physicist not having an interest in foundational problems. Most don't, it's a choice about where you think the answers to the questions you want to solve are going to come from. I choose to go to meetings in foundations of quantum mechanics, because I think they are relevant. I've seen very few people at these meetings who also contribute to string theory-I only recall Brian Greene and Gerard 't Hooft. So it's just a statement of fact to say that most string theorists don't think the foundational issues are relevant for their field. Another piece of evidence for this is that there are few if any papers by someone considered a leading string theorist proposing a new solution to one of the foundational problems I listed above. But many of the leaders of the non-string approaches to quantum gravity have written such papers, such as Crane, Dowker, Finkelstein, Hartle, Isham, Markopoulou, Penrose, Rovelli, Sorkin.

The point of my essay is to propose that it is an intellectual mistake to separate the problem of quantum gravity from the foundational issues. This implies that more attention and scope needs to be given to those who understand and care about the foundational issues and have ideas of how they relate to the problem of quantum gravity. This could be right or wrong. If you don't agree with it, that's fine, but please don't be insulted just because your style of research is labeled "non-foundational," when in fact you have not focused on the problems that are called "foundational".

As for rejecting Einstein's legacy, this is again just a fact. If you study the work of historians such as Stachel and Barbour, who are intimately familiar with Einstein's work and its historical context, you learn that background independence is the core of what they consider Einstein's legacy to be. I don't understand why this bothers anyone. If you have thought about the issue and decided to reject background independence then you are in disagreement with Einstein. But this should hardly bother most contemporary theorists as very few of us agree with Einstein's views on quantum theory, which puts almost all of us outside of his legacy.

Is it arrogant to say that shifting one's view on these issues requires a certain amount of critical reflective thinking? Perhaps, but all I can say is that this is what it took for me to make the switch, having been trained originally as a quantum field theorist and having done all my early work on perturbative quantum gravity. It took sustained interactions with Barbour and Stachel, as well as close study of their and Einsein's work, over several years, to come to their point of view.

Finally, thanks to WHO, who made several points better than I could have.

Thanks,

Lee

30. **woit**
March 13, 2006

I gave Lee the last word here since there was a significant amount of criticism here and he deserved the right to respond to it. But, please restrict comments to the topic of the posting (the multiverse, remember?). There's a lot of interesting things to debate about foundational issues, but I'd rather people not do it here and now. For one thing, such a debate is exceedingly hard to properly moderate, and I'm way too busy for that right now.

31. **woit**
March 13, 2006

vb,

Depends what you mean by antagonist. There's Lubos Motl, who goes nuts when my views are mentioned, except that he and I kind of agree on the landscape. I don't know of any blog that actively promotes the landscape point of view regularly from the point of view of someone doing research in that field.

32. **vb**
March 13, 2006

Thanks for the answers. Yes, youve guessed right in your second part, "antagonist" in this sense, as you've said, who: "…actively promotes the landscape point of view regularly from the point of view of someone doing research in that field." — not aware of any site, which, btw, tells a bit of story: perhaps it's easier to be sceptical; "poking around" controversial hypothesis [landscape] publicly by an established scientist [e.g. Susskind] can make him vulnerable to "not even wrong" kind of opinions (on the other hand, for a non-well-known scientist, it may be an opportunity). Science has no other way but to advance though; and someone has to make first steps, even if they fall…

33. **woit**
March 13, 2006

vb,

One interesting aspect of the landscape idea is that it is significantly less popular among younger researchers without much of a reputation, which is also the same population that is most likely to start a blog. My interpretation is that this is because adopting the landscape point of view is a kind of giving up which is much more likely to appeal to older people who have spent more than 20 years struggling to get a prediction out of string theory. Younger people who haven't been at this as long are more likely to believe that giving up is not necessary, that some nice vacuum state of the right kind can still be found.

I do wish Susskind would start a blog though, that would be interesting.

34. **Jimbo**
March 13, 2006

I think everyone should read Smolin's paper, ref'd by Peter. It is an outstanding delineation of the crisis impending in theoretical physics. The reason that Ellis' comment is devoid of math is that all the calculations supporting his claim that Lenny is wrong about horizon distinctions, are done in the paper he ref's by Davis & Lineweaver. Sounds to me like Susskind was trying to slip the public a `mickey', vis a vis, particle v. event horizons, in order to bump sales.

35. **Who**
March 14, 2006

The reason that Ellis' comment is devoid of math is that all the calculations supporting his claim that Lenny is wrong about horizon distinctions, are done in the paper he ref's by Davis & Lineweaver.

That was my impression too: Ellis didn't need to include equations since he is dealing with established concepts familiar to cosmologists, and he gives ample references.

BTW the paper he cited by Davis and Lineweaver is one of several by those authors about cosmological horizons.

To amplify Jimbo's remark, contrary to Susskind's claim, what cosmologists call the particle horizon is not analogous to a black hole event horizon, except in a verbal or poetic sense. Moreover the cosmic microwave background is not coming to us from the particle horizon (but from the surface of last scattering), and it is not produced in a way analogous to Hawking radiation. Even if one were to grant for the argument's sake that information about matter eaten by a black hole is encoded in the Hawking radiation, this would not imply that information about other universes ("pocket universes" and the like) is present in the CMB. The exerpts from Susskind's book which Ellis includes in his article are extraordinary—I recommend reading the Ellis article (all 3 pages of it) just for the quotes.

36. **csrster**
March 14, 2006

Ellis has obviously misunderstood Susskind's argument, as no-one with a primary school education could have made such an absurd blunder as the one Ellis attributes to Susskind.

37. **Eli Rabett**
March 14, 2006

It may be time to reconsider)after a few centuries of success(the proposition that the universe can be understood at all levels by humans. Perhaps there is some level at which this cannot be done, and attempts to do so only lead to madness and ill tempered posts.

38. **woit**
March 14, 2006

Eli,

I don't know about "at all levels", but at the level string theory is trying to understand, there's no evidence yet that this can't be done. String theorists at the moment are trying to promote the idea that since string theory leads to a complicated mess that can't be understood, the world must be a complicated mess that can't be understood. That string theory is just a wrong idea is a much simpler explanation.

39. **Who**
March 14, 2006

[csrster: *Ellis has obviously misunderstood Susskind's argument, as no-one with a primary school education could have made such an absurd blunder as the one Ellis attributes to Susskind*.]

Your guess is understandable (because what Susskind claim is so absurd) but unfortunately you would seem to be mistaken. Ellis quotes passages from Susskind's book which are explicit, and gives a link to longer exerpts providing context.

The case is quite clear. Please do not rely on my imperfect paraphrase, but read the quotes from Susskind in Ellis original paper. It is only 3 pages and no trouble to read.

[url]http://arxiv.org/abs/astro-ph/0603266[/url]
[b]On horizons and the cosmic landscape[/b]

Abstract: "Susskind claims in his recent book The Cosmic Landscape that evidence for the existence and nature of 'pocket universes' in a multiverse would be available via detailed study of the Cosmic Blackbody Background Radiation. I point out that apart from any other queries one might have about the chain of argument involved, this claim is invalid because it rests on a confusion between the nature of a particle horizon and an event horizon in cosmology."

Ellis gives this link to exerpts from Susskind's book containing the questionable claim:
[url]http://www.twbookmark.com/books/28/0316155799/chapter_excerpt22014.html[/url]

Some quotes from Susskind are:
"…The very same arguments that won the Black Hole War can be adapted to cosmological horizons. The existence and details of all the other pocket universes are contained in the subtle features of the cosmic radiation that constantly bathes all parts of our observable universe…"

40. **Christine Dantas**
March 14, 2006

Very interesting paper by Ellis, with excellent references.

He writes,

> In reality, the limit is even stronger: no significant cosmological information reaches us at the present time from beyond the *visual horizon* – defined by the world-lines of the furthest matter from which we receive electromagnetic radiation today.

As far as I understand, however, another horizon could be set by a primordial gravitational radiation background. The observational aspects of this possibility, in classical and string theory contexts respectively, can be found in astro-ph/0504290 and hep-th/9907185. Another interesting paper is by Craig J. Hogan: astro-ph/9809364. However, I do not know how far these studies could be extended to the multiverse issue in the sense that information coded in the primordial gravitational radiation background (observed from WMAP or LISA) could be used to infer on the existence of these "other universes".

41. **Anon1**
March 15, 2006

Hi,
For pple who r confused about 'particle' and 'event' horizons..(actually, u can add the hubble radius to this list..these 3 are often mixed up) look at this paper.. Ofcourse, this is much before black hole radiation etc.. but this is one of the first to spell out the differences clearly. Hope it helps..

42. **Eli Rabett**
March 17, 2006

Peter,

True there is no evidence that we cannot understand the universe at the level of string theory, but equally well there is no guarantee that it can. Moreover it is not even clear that the methods that have been used to characterize nature up to this point can continue to work ad infinitum.

I offer this thought in the same spirit as the anthropic principle was created. The level of string theory is so far from human experience that the universe may not be explicable to or by us at that level.

Two Years Later

March 13, 2006
Categories: Uncategorized

This week is the second anniversary of this weblog, so perhaps a good moment for some reflections on what has been happening over the past two years.

In many ways, the weblog has been successful far beyond my wildest dreams. My original expectation was that there would be a handful of people with similar interests who would regularly read it, and it would be quite a success if I ended up with a couple hundred readers. I don't have completely accurate recent statistics, but recently each day at least several thousand people are checking up to see what is going on here. This is quite gratifying, and makes the significant amount of effort and time I've been putting into this seem worthwhile. It has been interesting to note from looking at some other blogger's publication records that starting an active weblog seems to correspond to starting to write many fewer papers. My one regret is that the time spent on this has definitely taken away from time that could be devoted to finishing and writing up various research projects. In the future I hope to find a better balance on this issue.

One of the main topics covered on this blog, and by far the most controversial, is the ongoing story about string theory and its dominance of theoretical high energy physics. The public perception of string theory seems to me to have changed significantly recently, as more and more science journalists have started to realize that things are not going well. Many of them have moved from a stance of uncritical acceptance of the claims of string theorists to a more skeptical and balanced view of the subject. The kind of overhyped popular string theory article that was a staple for 20 years is increasingly unlikely to be written by professional science journalists. Such things now occur most often in places like university press releases, authored by people with no experience in the subject.

I'd like to think I had something to do with this, but there are much larger forces at work. The field of string theory has suffered a remarkable intellectual collapse, one that is not just a matter of opinion, but can be quantified in various ways. For many years Michael Peskin has written up a discussion of the yearly list of top cited HEP articles. I wrote up postings discussing the 2003 and 2004 lists. By 2004 there were only two post-1999 string theory papers among the list of 50 most heavily cited (the early 2002 Berenstein et. al. PP waves paper, and the early 2003 KKLT paper), and Peskin seems to have stopped writing up a discussion of the list, possibly because there was virtually nothing new to discuss. SPIRES has not yet produced a 2005 list and I don't know if they ever intend to, but from some data gathered at physicsforums.com it would appear that the only two string theory papers likely to have accumulated the 150 or so citations needed to make the top 50 in 2005 are exactly the same two as in 2004. The subject has come to nearly a dead stop, and that rather than the complaints of its critics is behind the sense of crisis felt by many of its practitioners.

The panel discussion at Strings 2005 in Toronto was rather remarkable. For the first time, members of the audience started to raise real questions about what was going on in the subject and the panel members had difficulty in putting a positive face on the situation. It will be interesting to see if a similar discussion occurs at Strings 2006 this summer in Beijing.

The two post 2000 papers that are widely cited reflect the two main topics that string theorists are still working on. One is AdS/CFT, which many, many people work on since it is the best thing to have come out of the string theory project. There doesn't however seem to be much significant progress in this area. The second paper, by KKLT, is the one that really launched the whole landscape business. The fact that it is the most recent hot area of activity in string theory is something that even most string theorists find very disturbing. Over the last couple years, the original implausible hopes that something could be gotten out of the Landscape have been pretty convincingly crushed. Leading figures in the field have abandoned the Landscape and moved out into the swampland of theories that have nothing to do with the real world and may or may not be low energy limits of a string theory. It remains very unclear what the point of this is.

The most active string theory blogs are becoming ever more bizarre, with increasingly strange behavior of all sorts from Lubos Motl, and Jacques Distler following the lead of others into the swampland while firmly sticking to the idea that my criticisms of string theory are some sort of illegitimate crackpotism. While most string theorists are well aware of what bad shape the field is in and casting about for something new to do, the true believers are exhibiting something more and more approaching religious fanaticism.

Unfortunately, leading figures in particle theory show no signs of being willing to publicly address the increasingly disturbing state of the subject. Part of my problem with the arXiv is the feeling of many that I do not have the stature in the community necessary to justify being allowed to make the kind of critical comments I have been making publicly. I'm willing to agree with this point, but it remains unfortunately true that those whose responsibility it is are doing little to address the situation. The whole field of particle theory is becoming increasingly damaged by these problems, and only one aspect of this is the problem of public perceptions, which is going to get a lot worse before it gets better.

I have no idea where this story is going next. The general attitude seems to be to hunker down for the next few years, try and wait out the crisis and hope that LHC results will save everything. This doesn't seem to me to be the right way to address the serious problems that are all too obvious right now.

Update: Some anonymous person really has too much time on their hands. But I'm honored.

Update: One or more people definitely have too much time on their hands. Besides the Not Even Wrong parody mentioned above, there's another one, and also Cosmic Variance and Lubos Motley's Stringy Climate Theories. This last one informs us that

Recent fake blogs have brought shame to the Internet:

(1) http://motls.blogspot.com pokes fun at Dr Lubos Motl, by posting a mixture of insane climate drivel, interspersed with attacks on theoretical physicists. I can reveal it is fake.

Comments

1. **Garrett**
   March 14, 2006

   String theorists will work on something. (And no, not burger flipping, as much as you might wish…) It is a gradual revolution. People will work on things that aren't strings, but keep referring to strings, or say their work is "string inspired." Eventually, someone will find something cool, maybe tenuously connected to past string work, and everyone will jump on that. Then, after awhile, people will see that the connections to strings are superfluous. And that multi-decade endeavor will be relegated to the dustbin of history. Such is the way of crowd dynamics, even really frickin' smart crowds.

2. **Garrett**
   March 14, 2006

   Oh, and yes, nice job Peter — congrats on your anniversary and much deserved blog success. I think you're playing a bigger role than you're willing to admit.

3. **a**
   March 14, 2006

   String theory predicts nothing, and LHC might find nothing.

In a few years we might have to reconsider anthropic models more seriously.

4. **Ijon Tichy**
March 14, 2006

> The public perception of string theory seems to me to have changed significantly recently, as more and more science journalists have started to realize that things are not going well. Many of them have moved from a stance of uncritical acceptance of the claims of string theorists to a more skeptical and balanced view of the subject.

One would think they would strive for a skeptical and balanced view of all areas of science, not just string theory. Unfortunately, much of science journalism is of the mindless cheerleader variety, uncritical and unquestioning, leaving the largely disinterested public with a rather distorted view of the enterprise. Science needs its critics, especially in highly speculative fields like high-energy physics and cosmology. So you can understand why I believe that this blog is carrying out an important role. Keep up the good work, Peter Woit.

5. **Chris Oakley**
March 14, 2006

I sometimes wonder whether some HEP researchers really understand what research is all about. An interesting but speculative idea did not work. So what? Move on and try something else. Brand loyalty makes no kind of sense in this game: there are just models that are good at describing nature and models that are not. Nature and Occam's razor are the arbiters. Distler (the "flaming spear in the stringy god's left hand") and Motl (the "hammer in the stringy god's right") seem to think that criticism of ST is a personal attack on themselves. That is not how it works. We are all Sherlock Holmeses, rooting around the dustbins of reality for clues as to how the universe works. Some hypotheses fit the facts well, some do not. Some hypotheses can be stretched and squeezed in an unattractive way to fit the facts, but why bother? Why not just try something simpler?

6. **Thomas Larsson**
March 14, 2006

Happy anniversary, Peter.

7. **anon**
March 14, 2006

'… Lubos Motl, and Jacques Distler following the lead of others into the swampland while firmly sticking to the idea that my criticisms of string theory are some sort of illegitimate crackpotism.'

What someone should do is expose some of belief systems of these 'defenders of the faith'.

None of them are as holy as you Peter. I know Lubos is a deviant of mainstream views on climate change, so he's a 'crackpot' in that sense. Then you just apply his own brand of guilt by association, and hint that if he has eccentric views on climate, he's ST work is probably guilty by association with a cranky brain.

Since Jacques is moderator of trackbacks and allows them from Motl's blog which is as full of crackpot climatic postings as it is ST, Jacques is guilty by association too. I won't mention Kaku's interests in UFOs, but sincerely someone should make the public aware of their guilt by association to loony ideas. BTW, does anyone know any deviancy of Susskind (apart from physics)?

8. **Juan R.**
March 14, 2006

Congrats for your contribution to light in this nigthmare called string "theory".

—
Juan R.

Center for CANONICAL |SCIENCE)

9. **a**
March 14, 2006

Have you seen the lunatic stuff distler posts on his blog?

By comparison, motl seems positively sane.

10. **robert**
March 14, 2006

Your blog has made vey interesting reading over the past two years. And to think I stumbled upon it, having been thoroughly impressed when watching 'The Elegant Universe'. Keep 'keeping it real'.

Best wishes

11. **Joao Leao**
March 14, 2006

Being a long time lurker to your blog I take this oportunity to thank you and congratulate you on what you have accomplished here. Besides the obvious fact that it takes guts to point out a "naked emperor" in any court, I think the success of "Not Even Wrong" owes much to your level-headedness and your gift for clear writing. Though I cannot claim that I always agree with you, I am probably more radical than you in what I consider conceptual and critical failures in today's fundamental (super)physics. You are surely right in pointing out that the recognition of the general *malaise* is spreading through the ranks! This is also underscored by the increasing "prickliness" of the "defenders of the faith". (Arrogance and insecurity often go hand in hand!) These are hard times for physics and, as much as it may hurt you, your time is well spent in providing and "driving" this critical vehicle. And it should be noted that you try your damnest to keep it constructive.

Hang in there!

12. **Who**
March 14, 2006

the empirical science ethic (only theories vulnerable to test by a do-able experiment) is almost 400 years old
your blog is 2 years old
and upholds that ethic
bravo Peter

http://en.wikipedia.org/wiki/Novum_Organum

Novum Organum (1620)

13. **Zelah**
March 14, 2006

Happy anniversary Peter.

14. **Urbano**
March 14, 2006

Congrats. And keep it going :-)!

15. **Dumb Biologist**
March 14, 2006

Happy 2nd!

16. **Dick Thompson**
March 14, 2006

Happy anniversary from a daily peruser. You have performed and are performing a valuable service.

The string theory community is better than the likes of Motl, Distler, and the anthropic landscape crew, and we can hope it will come to its senses shortly. There is hope there, for example in Lisa Randall's models and the higher category approaches promoterd by Urs Scheiber and John Baez, among others.

17. **Kea**
March 14, 2006

Happy anniversary to you and the WMAP release!

18. **Ron Avitzur**
March 14, 2006

Happy anniversary!

Thank you for providing a forum for so many educational discussions, and for a fascinating glimpse "behind the curtains" into the human side of the process of science.

Keep up the great work! (And good luck striking a balance between all the time this takes, and continuing your own research.)

19. **Dumb Biologist**
March 14, 2006

They say you've finally arrived (where I can't say) if you've been lampooned. One of course would hope for a *good* lampooning…

20. **tommaso dorigo**
March 14, 2006

Peter, congratulations for this endeavour. Two things.

One, you know very well that this blog is not just a blog. It is in places like this that science makes progress sometimes. Public arenas are scarce, hampered by good manners, and people will not speak up too easily with the first thing that springs to their mind. It is in a successful blog such as yours, ran by a respected scientist such as you are, and frequented by lots of silent listeners as well as by a few vocal individuals, that the scientific debate becomes hot, useful, and constructive (well, not always, but signal to noise seems good). So thanks! for running it, and if you think about ramping down the blogging for that other paper you wish to write… Well, write it here. You'll get more attention and constructive criticism in the making.

Two, as an experimentalist in HEP I feel I have been left driving blind. I have spent the last ten years of my life constructing tools to find the Higgs boson, and I do not even know for sure if I would prefer if it was there or not after all. Many colleagues are so into Susy they think it is just a matter of time until they bump in spectacular cascade signatures. I wish I was convinced by it myself, but no, cannot see the real appeal. String theory looks so far out to me I never even ventured into trying to understand it. In a word, I need guidance, or I will lose interest. Will the good old days when the world was either S-T or V-A, and we physicists only had to go figure it out, ever come back ?
I hope by reading your blog I will keep some inspiration…

21. **woit**
March 14, 2006

Tommaso,

Thanks for the encouragement! I do hope in the future to try and write some things here that will be ultimately part of a paper I'm writing (although I fear this may drive away much of my audience….)

It's a tough time to be an experimentalist, but, it could be worse, you could be a high-energy theorist, which has really been a frustrating business for quite a while now. Good luck in your hunt for the Higgs!

22. **Kris Krogh**
March 14, 2006

Peter,

Thanks for your courageous stand these two years, that the job of physicists is not to tell us about the imaginary but the physical.

23. **Hooman**
March 14, 2006

Congradulations Peter on your second year of this SUPERB blog. Every branch of scientific endevour needs sharp and rational, criticism. But I believe this blog is much more than a simple 'anti-string' voice. It is a wealth of interersting information and a source of inspiration for all people who are interested in "modern Natural Philosophy".
Pauli would be have been very pleased with your efforts….;-)

24. **Justin**
March 14, 2006

Congradulations for your contribution to the continuing decline of high energy particle theory.

25. **Michael**
March 14, 2006

Peter, if you find running this blog "gratifying", what word would you use to describe a successful academic career?

26. **Santo D'Agostino**
March 14, 2006

Peter,

Congratulations on producing a valuable scholarly contribution.

All the best wishes,
Santo

ps: Michael, you are acting like an ass. Smarten up, will you?

27. **JuanPablo**
March 14, 2006

happy… err… blogversary (?) (did you see http://math.ucr.edu/home/baez/where_we_stand/where_we_stand.pdf? seems to be a nice present for today)

28. **D R Lunsford**
March 14, 2006

Well done, and best wishes!

-drl

29. **Who**
March 14, 2006

speaking of presents, Bert Schroer posted an article today that discusses among other things Peter's book "The Failure of String Theory"—-and has a fair amount of complimentary things to say.

I gather Schroer is a respected elder statesman algebraic QFT guy, friend of Ray Streaters since 1960 according to Streater, Schroer's papers go back to 1961. he is worried about the state of physics and he has ideas of what needs to be done—and he compares and contrasts his views with those in Peter's book. So it is kind of a nice thing to have happen on your blogbirthday.

Schroer's paper that he posted today is
http://arxiv.org/physics/0603112
String Theory and the Crisis in Particle Physics

It has a funny fourline poem in German that originated in Prague sometime before 1940 at a time when a maestro named Motl used to conduct Wagner operas.

" … after a performance of Wagner's Tristan and Isolde…. an art critic wrote instead of the usual critical review in next days newspaper the following limerick….:

Gehe nicht zu Motl's Tristan
schau Dir nicht dieses Trottels Mist an,
schaff dir lieber 'nen viertel Most an
und trink dir mit diesem Mittel Trost an "

it is a tradition in our family that people should get poems for birthdays and is this really so off-topic as all that?

30. **D R Lunsford**
March 14, 2006

It may be flattery, Peter, but it's lame and misses the real point. No one reads the masters. I lived in the journal stacks – haunted them. I saw the evolution of physics almost first hand – the only trouble was, the authors were dead or scattered, so direct questions were impossible.

No one reads journals now. They hang around the archive like a pack of goth girls at a record store. Like string theory, the archive is a failure not only in fact, but in concept, because it is too easily subverted by priggish martinets.

"Not even wrong" is, above all, an homage to Pauli, the great unread and misunderstood hero of physics. When I saw that title, I rejoiced, because I knew that anyone who really loved Pauli also really loves physics, and would defend it against subversion whatever the cost.

Here's to Pauli, and here's to Peter.

-drl

31. **Aaron Bergman**
March 15, 2006

Re:Schroer —

My god. It's like deja vu all over again.

32. **D R Lunsford**
March 15, 2006

ROFL

-drl

33. **Bourgeois Nerd**
March 15, 2006

Happy Blogoversary from a daily reader! Some of what you write is waaaaaaaaaaaay over my head (I'm just a useless English major with a fetish for cosmology and particle physics, after all), but it's always interesting and often wryly funny. I don't always agree with your views, but you've really articulated what had been a vague dissatisfaction with string theory; I never really "got" the "beauty" of it and just didn't like it. I'm glad others share this view and back it up with some actual evidence and knowledge instead of my non-empirical feelings. Plus, I'm also happy that there's someone out there being a gadfly and sticking it to THE MAN. He always needs that. Keep up the good work!

34. **robert**
March 15, 2006

Peter

Please free to delete this, but I cannot restrain myself from commenting once more that 'Michael' continues to reveal himself to be a complete shit. Lubos and Distler may take things a little too far from time to time, but at least they make it clear who they are and, especially in the case of LM, produce blogs that contain a great deal of interesting material when they are not going OTT.

35. **anonymous**
March 15, 2006

Robert, Santo & others,

Please, I would really urge you to just ignore Michael. He's a troll, out on baiting, and any attention to him is what feeds him.

Ignoring Michael is easy enough, but now its getting irritating to read such replies are yours.

36. **Haelfix**
March 15, 2006

Wasn't Shroer the Professor who was harping about DDF/Pohlmeyer string quantization a few years ago? I glanced through his paper and he still seems to have issues with stringy localization.

Afaik that question is still debated, particularly in the context of string field theory. Is anyone aware of whether its settled now or not?

37. **Chris Oakley**
March 15, 2006

A non-literal translation of the poem, that I hope at least captures the spirit:

Don't go to Tristan of Motl's,
Don't appear at this zombie excrescence,
It's better just to get some bottles:
And have a much more pleasant acquiescence

38. **Paradigm**
March 15, 2006

Michael: this year there is a summer school about "Strings and Branes: The present paradigm for gauge interactions and cosmology". Do you think it is a honest title? Don't you agree that this blog can help students?

39. **David Cobden**
March 15, 2006

I want to add my voice to the chorus. This is my favorite blog. I am a condensed matter physicist, presently at the March meeting in Baltimore along with about half the other physicists in America. I see many problems with the way physics is going nowadays, and one of them is the situation in particle theory, which has become detatched from reality. My particle theory colleagues would not agree with this (I hope they don't take offence and vote against my tenure), but everyone I know in condensed matter would. The particle theory situation is particularly irksome because it gets such a disproportionate share of the limelight, and because many of us who are passionate about physics see this degradation of the traditional scientific method happening at the very core of our discipline. The situation is not going to get better unless we talk about it! Your blog is the healthiest, most coolly moderated discussion physics forum I know of, given the limitations of bloggery. Though there are other good blogs, they seem to skirt around these deepest problems. We need something like Not Even Wrong in condensed matter. I hope you keep it going!

40. **Robert**
March 15, 2006

Happy B'day!

Come one, Peter, say something on physics/0603112 don't be shy! Lubos already had his go.

41. **woit**

March 15, 2006

Thanks to all for the blog-bday greetings!

Robert,

I'm in the middle of writing something about this. Lubos is just too fast, I can't beat him on speed.

42. **Who**
March 15, 2006

Lubos comment on the poem is not perceptive.
he knocks the rhyming—-suggesting it is dumb technically.
it is not. Lubos reflex contempt misleads him in this case.

the rhyming is an example of a rather technically challenging
German light verse form called the **schuettelreim**
which has to rhyme on actual Spoonerisms
(permutations of the syllables which result in other words of different meaning)

there are German websites devoted to schuettelreim—-to "switch rhyming", or spooner-rhyming

Motl should show respect for the poem's technical competence (and wit). Dont knock it til you try it.

the verse would be more perfect if one phrase—-viertel Most—were changed to
drittel Most
the better to permute into Mittel Trost

The viertel (a quarter-liter wineglass) being more common than a drittel (a third of a liter glass or mug) would have occasioned the artistic compromise.

43. **Bert Schroer**
March 17, 2006

To Who says
You are perfectly correct, now I remember that it must have been a "drittel". It is one of the best schuettelreims in German I know. Most of them only have two lines like:
Es klapperten die Klapperschlagen
bis ihre Klappern schlapper klangen
or
Es sprach der Herr von Finkenstein
die Harzer Kaeschen stinken fein
or (if you don't mind one with a boy scout flavor):
Sie zogen aus mit bunten Wimpeln
und kehrten heim mit wunden Pimpeln.
But please don't think that the beauty and perfection of the 4-liner was the main reason for writing my article.
I will be back on the radar screen with some (hopefully) enlightening remarks on the role of differential geometry and topology in quantum field theory (Schroer versus Woit, but no polemics) only on coming tuesday.

44. **Who**
March 17, 2006

I will be back on the radar screen

It's a pleasure. I look forward to seeing you.

45. **John A**
March 21, 2006

I read both this weblog and Lumo's. Sometime I agree with one of you and not the other, sometimes both, sometimes neither.

Like String Theory, the consensus on climate science is rather less than the hype put upon it by some of its more famous protagonists.

I do not care for theories which have no testable results or which cannot be replicated. So for Lumo to support String Theory (majoritarian) and climate skepticism (minoritarian) and this blog to do the reverse, is satisfyingly ironic.

Can I make a suggestion? Try to dial down the rhetoric on Lubos and focus on the substance of the argument.

Having setup quite a few blogs myself, I can say that I get more scientific content through them (or even despite them) than from peer-reviewed journals. They're not a nirvana, but they have definitely changed the way academic discourse is conducted.

I look forward to another couple of years of interesting commentary. I am sure the truth will out.

46. **woit**
March 21, 2006

Thanks John.

One correction. This blog takes no position at all on climate change and I delete comments from people who try and start debates here on issues like that. I'm a firm believer that my blog should stick to topics that I have some expertise in.

Focussing on the substance of an argument with Lubos is really not possible, I've sometimes attempted it and never gotten anywhere. He is however highly entertaining, and the fact that a Harvard faculty member string theorist is so loony is a truly remarkable one to observe, so I don't at all intend to ignore him.

Baez and Schroer

March 15, 2006
Categories: Uncategorized

John Baez's latest This Week's Finds is out. As in other recent issues, he starts with some of the most fantastic astronomical pictures around. He also links to his recent non-technical talk Fundamental Physics: Where We Stand Today, which also has fantastic pictures. In the talk he describes how, since the 80s, "many physicists feel stuck", and "continue to make predictions but they are usually wrong or not yet testable. This has led to a feeling of malaise. Why are they failing?" He partially answers this question with

But when their theories made incorrect or untestable predictions, many theorists failed to rethink their position. It is difficult to publicly retract bold claims. Instead, they focus more and more attention on the mathematical elegance of their theories… some becoming mathematicians in disguise. (There are worse fates).

Someone who was at the talk reports that afterwards Carlo Rovelli asked Baez "whether what he had just presented didn't imply that the theoretical physics of the last 25 years was 'junk'", and that Baez "replied after some hesitation 'You said it'".

A physicist who has been concerned for quite a while about the sociological changes in how particle theory is done and the ever more critical situation that the field finds itself in is theorist Bert Schroer. His specialty is in the area of algebraic approaches to QFT, especially conformally invariant ones. More than a decade ago he was writing review articles on QFT well-worth reading that included warnings about what has been going on. For some examples, see his Reminiscences about Many Pitfalls and Some Successes of QFT Within the Last Three Decades and Motivations and Physical Aims of Algebraic QFT.

Schroer has just posted three new articles on the arXiv. One of these is entitled String theory and the crisis in particle physics and is well worth reading if you have any interest in the ongoing controversy over string theory. Schroer has many interesting points to make on the subject, and one of his main concerns is that a great deal of knowledge developed about QFT during the last century may be effectively lost as the training of young theorists focuses on string theory. This article has already drawn Lubos Motl's trademark rant accusing anyone skeptical about string theory of being an incompetent crackpot.

The second of his new articles is called Physicists in times of war and begins with comments on the Iraq war and Schroer's profound disappointment at the refusal of Witten and others to join him in a public campaign against the war before it began. The second part of the article tells the story of Pascual Jordan, one of the founders of quantum mechanics who joined the Nazi party. Schroer's politics are diametrically opposite to those of Jordan, but he is highly sympathetic to Jordan's scientific point of view, from the earliest years of quantum mechanics, that it is necessary to think about quantum systems in a way which doesn't depend on starting with a classical Lagrangian and "quantizing".

The last of Schroer's new articles should appear on hep-th tonight and is entitled Positivity and Integrability. It tells some of the history of the QFT group at the Free University in Berlin and has a lot of interesting things to say about reflection positivity and the Euclidean approach to quantum field theory.

Update: I haven't heard anything at all back from the arXiv about the trackback issue, but just noticed that trackbacks to the two recent Schroer articles mentioned here have appeared. The ways of the arXiv are highly mysterious….

Update: Baez has a clarification here of his response to Rovelli.

Comments

1. **Robert**
   March 15, 2006

   Peter forgot to mention that in the first paper Schroer advocates an approach that dumps all geometric parts from our treatment of quantum physics as it is inherently classical. Not quite in line with Peter's favourite alternative direction.

2. **woit**
   March 15, 2006

   Robert,

   Schroer definitely favors an algebraic, non-geometric approach to QFT, while I'm starting from a very geometric direction. However, one of the basic lessons of mathematics is that to understand a mathematical structure at the deepest level you often need to simultaneously use algebraic and geometric insights, and understand how they are related. I still think the geometric starting point is most promising, but over the years I've become more and more aware of how important it is to also think about these questions from a more abstract, algebraic point of view (have you heard me go on about K-theory and representation theory?).

3. **DMS**
   March 15, 2006

   Well, Bert is wrong in lumping all American physicists on the Iraq war; see for instance

   http://physicsweb.org/articles/news/7/1/14/1

   Nobel laureates oppose war against Iraq

   29 January 2003

   Forty-one American Nobel laureates have signed a declaration opposing war with Iraq. The declaration was organised by Walter Kohn, a theoretical physicist at the University of California at Santa Barbara, and former adviser to the Defense Advanced Research Projects Agency at the Pentagon. The signatories include 19 winners of the physics prize.

   The declaration reads:

   "The undersigned oppose a preventive war against Iraq without broad international support. Military operations against Iraq may indeed lead to a relatively swift victory in the short term. But war is characterized by surprise, human loss and unintended consequences. Even with a victory, we believe that the medical, economic, environmental, moral, spiritual, political and legal consequences of an American preventive attack on Iraq would undermine, not protect, US security and standing in the world."

   The signatories include Norman Ramsey, who worked on the Manhattan Project, and Charles Townes, a former research director of the Institute for Defense Analyses at the Pentagon. Townes was also chairman of a federal panel that studied nuclear warheads.

Kohn expects more laureates to sign this week but the current list of signatures is:

Physics

Philip W Anderson, Hans A Bethe, Nicolaas Bloembergen, Owen Chamberlain, Leon N Cooper, James W Cronin, Val L Fitch, Sheldon L Glashow, Leon M Lederman, Arno A Penzias, Martin L Perl, William D Phillips, Norman F Ramsey, Robert Schrieffer, Jack Steinberger, Joseph H Taylor Jr., Charles H Townes , Daniel C Tsui, Robert W Wilson

4. **A.J.**
March 15, 2006

Two comments:

1) I was the last person to edit the wikipedia paragraph that Schroer quotes as "string-theoretical adulation in its purest form", and I completely disagree with his characterization of the writing. The paragraph is hardly the praise he makes it out to be. Frankly, I saw dwelling at length on the variety of things the "M" might stand for as a way of correcting a common misconception on wikipedia, namely that anyone has much of a clue what M-theory actually might be.

2)

5. **A.J.**
March 15, 2006

2) … Schroer's been banging on now for some time about the conceptual superiority of the algebraic approach to QFT. I think it's about time that he included in one of his polemicals an explanation of how renormalization theory fits into the AQFT conceptual framework. The techniques of effective field theory are essential to anyone who actually does computations or makes connections to experiment, and it's difficult, to say the least, to take seriously a proposed framework which seems basically to ignore and/or dismiss these tools.

6. **Peter Shor**
March 15, 2006

What I found astonishing is the idea that young string theorist are not learning a lot of quantum field theory. How can you possibly hope to generalize QFT when you don't completely understand it?

7. **Igor J.**
March 15, 2006

RE: DMS – Nobel laureates oppose war against Iraq

As I read the declaration, it does not seem to me as a categoric oposition against the Iraqi war, but rather they point out that this war is not in US interest… Eg. see the excerpts:
"…oppose a preventive war against Iraq without broad international support. "
"…Even with a victory, we believe that …"
Which I would read as: "If the world is with us and we have a quick and politically correct victory, we have no problem with the war….".

I believe that Schroer was looking for some strict refusal of the war (or any war in general), this would be clearly not enough for him…
[[If you read the article it is clear that he basically does not understand how any physicist could support any war at all etc…]]
Igor J.

8. **woit**
March 15, 2006

Peter,

I think Schroer's point is that quantum field theory remains a poorly understood subject, about which we still have a lot to learn; no one completely understands it. Certain parts of it are well-understood, are in standard textbooks, etc. Most string theorists know those, and also know quite a bit about the more advanced parts of the subject that are relevant to string theory. But there is a lot about quantum field theory that isn't relevant to string theory, and it is knowledge about those parts of the subject that I think Schroer fears may get lost.

9. **Who**
March 15, 2006

Update: I haven't heard anything at all back from the arXiv about the trackback issue, but just noticed that trackbacks to the two recent Schroer articles mentioned here have appeared…

one trackback being to NEW

!

10. **Chris Oakley**
March 15, 2006

I could be that Schroer's attitude toward String theory and string theorists was not helped by Lubos's reviews of two AQFT books on Amazon, here and here, especially as these are the same (negative) review pasted in two places. Having said that I think that Lubie is right to point out that a lot of people seem to have worked on Axiomatic/Constructive/Algebraic QFT for a long time and yet still cannot calculate cross sections or decay rates.

11. **Chris W.**
March 15, 2006

Wouldn't it be fair to say that at some point the effort to understand quantum field theory becomes *inseparable* from the effort to formulate a quantum theory of gravity, or at the very least, how quantum field theory should be understood in the presence of gravity? It seems to me that this has always been implicit in the understanding that gravitation is not just another physical field, notwithstanding the many formal connections that can be made between the mathematical structure of general relativity and gauge theories. More precisely, it is—to use Julian Barbour's term—the *frame* within which all physical processes happen.

The study of quantum field theory in curved spacetime seems to reinforce this conclusion. Note that in physics/0603112 Schroer discusses (on page 27) a recent paper of

Stefan Hollands and Robert Wald, **Quantum Field Theory Is Not Merely Quantum Mechanics Applied to Low Energy Effective Degrees of Freedom**.

12. **Arun**
March 15, 2006

Since Schroer skewers Motl at a rather personal level, I think Motl's ire is understandable.

13. **Who**
March 15, 2006

Since Schroer skewers Motl at a rather personal level, I think Motl's ire is understandable.

LOL

Spooner-rhymes are hard to forget

Gehe nicht zu Motl's Tristan
schau Dir nicht dieses Trottels Mist an,
schaff dir lieber 'nen viertel Most an
und trink dir mit diesem Mittel Trost an

several people have tried to translate but they did not provide
a simple literal translation—just to give the meaning of the words. I will try:

don't go to Motl's Tristan
do not consider this idiot's manure
rather get yourself a glass of apple wine
and drink comfort by this means.

Trottel = imbecile
Mist = manure
Most = hard cider, new wine
Trost = comfort, consolation
anschauen = contemplate, consider

14. **Haelfix**
March 15, 2006

AQFT is something people need to work on, but at this point its really of mathematical interest moreso than physical interest. This could and probably will change eventually but I just don't see many young physicists staking their tenure trying to reproduce cross sections that people have known for 30 years. As such its really the domain of tenured proffessors as its a very difficult problem with absolutely no guarentee of quick results.

If something really new and exciting came out of the field, people will jump on the bandwagon, but until that time its similar to working on the measurement problem, read probably career suicide

15. **Thomas Larsson**
March 16, 2006

What struck me in Schroer's article was his comment on AdS/CFT on p 14. Apparently he claims that it cannot be correct because the number of degrees of freedom on the two sides do not match. Does anyone know more about this?

Of course I strongly disagree with Schroer's statement (on p 26) that loop groups do not admit higher-dimensional counterparts. Both the Kassel algebra (multi-dimensional affine) and the Larsson-Rao-Moody algebra (multi-dimensional Virasoro) have been around for many years, and their beautiful representation theory is now being converted to physics (hep-th/0504020). There are of course errors and omissions at this stage (the worst errors are being weeded out, however), but progress has been surprisingly rapid. The most important conceptual conclusion is that the observer must be integrated seemlessly with the formalism, something which in a sense was anticipated by Rovelli's relational QM.

Schroer's ignorance about this development shows that I must increase my M-arketing efforts.

16. **Chris Oakley**
March 16, 2006

Who,

I'm not arguing with your translation, but in being literal you have foregone the sublime rhyme of "Motl" (as mispronounced by anglo-saxons) and "bottle".

Haelfix,

You are not quite right there. There are active AQFT groups in Hamburg, Zurich and Basel and probably at other European universities as well. Working on this is not quite professional suicide. Suicidal tendencies are more likely as a result of trying to keep up with their research output, which, like Superstrings, gets more detached from reality year on year. I am not sure that they are really interested in calculating cross sections anyway – if you want proof, have a look at the correspondence I had with a referee (who presumably belonged to the AQFT camp) on my web site from 1986/1987.

17. **ObsessiveMathsFreak**
March 16, 2006

Another humanities style paper in arXiv. How many of these are there?

18. **Sam**
March 16, 2006

Woohoo! AdS/CFT is wrong? The one development in string theory that Peter had to grudgingly admit was worthwhile. Now that Schroer has demolished AdS/CFT, we can truly say that string theory is a failure!

19. **woit**
March 16, 2006

Thomas and Sam,

Schroer doesn't say "AdS/CFT is wrong". My reading is that he is just pointing out the obvious fact that it isn't a precise relationship between two standard QFTs, but a relationship between a standard QFT (4d N=4 supersymmetric YM), and a 5d theory which is not a standard QFT (although you get a QFT, supergravity, in the low energy limit).

I don't think any of the positive things I've had to say about AdS/CFT are said grudgingly. But I do think it's important to remember that what is going on with AdS/CFT is still poorly understood. Lacking a non-perturbative definition of one side of the correspondence, one can't even precisely say what the conjecture is. There's something very interesting going on here, and a lot is known, but a lot isn't.

20. **Aaron Bergman**
March 16, 2006

Schroer is probably referring to Rehren's work relating an AQFT on AdS space to an AQFT on Minkowski space in one fewer dimension and Arnsdorf and Smolin's follow-up hep-th/0106073. There were discussions of it on spr at the time. My recollection of the upshot is that the AQFT's produced by Rehren's procedure have aren't physically useful (bizarro thermodynamics, for example.) It's not particularly clear what this relation has to do with Maldacena's duality between a QFT in four dimensions and *quantum gravity* in five dimensions (well, ten dimensions really).

21. **urs**
March 16, 2006

Maybe the applications of AQFT most relevant to interesting applications in physics concern AQFT of 2-dimensional conformal field theory.

For instance Antony Wassermann's discussion (math.OA/9806031) of representations of loop groups and the fusion of these reps (hence relevant to WZW models) is based on methods borrowed from AQFT.

Furthermore, representations of AQFT operator algebras are known to be one source of modular tensor categories from which 3D TFTS and 2D CFTs are constructed (http://golem.ph.utexas.edu/string/archives/000747.html).

22. **Benjamin**
March 16, 2006

"but he [Schroer] is highly sympathetic to Jordan's scientific point of view, from the earliest years of quantum mechanics, that it is necessary to think about quantum systems in a way which doesn't depend on starting with a classical Lagrangian and "quantizing".

As a mere engineer interested in physics, please indulge a naive comment. I was delighted to read this, since I have also been puzzled by what seems to me like 'blind adherence to formal recipes' in quantum theory, such as simply assuming commutation relations among operators instead of deriving them from observation.

So in superstrings, as I understand it, they assume an open or closed string, instead of a traditional point particle, associate position and momentum operators with this so-called 'string' (I mean, is it really like a little piece of spaghetti?), and then simply assume commutation relations 'by analogy' with point particles. But since the idea of a string is so different from a point particle, why blindly follow the same formal procedure and assume (hope?) that it works? Obviously, I'm missing a great deal, probably almost everything. But it does seem somewhat like a case of robotic imitation of a formal procedure that somehow worked in a different context.

In a similar vein, one could criticize formal procedures in 'orthodox' quantum theory, as the quoted authors do, even if the agreement with experiment is good. I like to be able to picture what is going on, just as Maxwell's 'abstract' equations correspond to Faraday's 'lines of force'. Just because a formal recipe produces valid predictions of data doesn't impress me so much per se. One could consider it jerry-rigged to do so. One could say that the formal procedures are 'parroting' reality, without really shedding light. I guess this line of thought is either too stupid or too philosophical for real physicists…

23. **Bert Schroer**
March 16, 2006

Some remarks about the various comments on my 3 papers of yesterday and today are in order.
Having lived in Brazil for the last 4 years, my flux of information is very incomplete. Concerning the Iraq war, my main source of information comes from sites of international newspapers, but I am detached from the sociology of US physics departments. It seems that I had the bad look to encourage Ed Witten in an email to start an anti-war campaign and I interpreted his negative answer in a too sweeping way. Actually I am sharing an office with Walter Baltensperger who is a good friend of Walter Kohn, but unfortunately he never told me about Kohn's anti-war activities. Baltensperger is basically apolitical and he is presently trying to get a documentary about "the Power of the Sun" (with Walter Kohn the main presenter) adapted to Portuguese to be afterwards distributed to schools in Brazil.
Concerning my string theory…essay I think that one should use a polemic style in physics only in situations of utmost emergency. Polemics, if it is good, sometimes has the power of opening frozen minds and leads to a scientific light bulb moment, but it is of course totally useless with people who allowed fundamentalism to take over. A perfect example of scientific polemics at its best is Res Josts's article in Helvetica Physica Acta 36, (1963) 77. This was written at the hight of the S-matrix bootstrap fashion and I think it did something to clear the air. The situation is much harder now, and if my efford is in vain it is probably not only because it lacks the elegance and coherence of Jost.
By the way the limerick which I remembered from my student times (maybe because it is as far as limerinks go the most perfect I ever run across, I think it is a delightful jewish flavor) was not meant personally against Lubos Motl. I just looked at his site and got the impression that he also did not misunderstand my intentions (although scientifically we are in different boats). Looking at his photo it remined me of how my former FU (younger) colleague Hagen Kleinert looked like when I met him the first time in Boulder (Colerado) where he was enthusiastically preaching the virtues of infinite component fields. When we are young we all go through a Sturm and Drang period. I also experienced this in connection with physics&differential geometry and this has a lot to do with my present more critical view and the differences in opinion with Peter Woit on this matter. Since this is quite interesting and needs some more time, I would like to come back to this (I have an appointment right now) soon.
That there is nothing as a gauge principle is the conclusion of algebraic field theorist for a long time. I became aware of this in connection with different ways present certain 2-dim. models. Recently I run into an interesting observation from Wigner's representation point of view (see the recent paper with Jens Mund and Jakob Yngvason).
No I did not say that the Maldecena conjecture is wrong, I only pointed out that there is a mathematical theorem which says that if you want a supersymmetric gauge theory on the conformal side (that's what most people want) than the AdS side cannot be a QFT for which you can write down a Lagrangian (algebraic QFT envisages a vast territory beyond the Lagrangian setting with which many string theorists unfortunately identify QFT). The best mathematically controllable illustration can be given in terms of so called generalized free fields which have to many degrees of freedom for a lagrangian formulation (I think a good reference is a paper of Duetsch and Rehren which should be easy to find). I am not saying anything which is not (at least implicitly) already in the literature but which may have been lost in thausands of publications on the subject. My statement was somewhat provocative since I proposed to use the Maldacena statement as a vehicle to get some intrinsic understanding of what string theory is. I think without an intrinsic understanding of string theory you cannot hope to prove the conjecture.

24. **Peter Shor**

March 16, 2006

Peter (Woit) says:

"But there is a lot about quantum field theory that isn't relevant to string theory, and it is knowledge about those parts of the subject that I think Schroer fears may get lost."

This still boggles my mind. In my experience of mathematics, it has become quite clear that anything mathematical has a chance of being useful for anything else. So I don't how see can string theorists say that any part of quantum field theory is irrelevant to string theory, especially when the paths that they currently think are relevant to string theory appear to be leading into a vast swampland. Is this arrogance or just a fad mentality?

25. **woit**
March 16, 2006

Peter,

Some parts of QFT really are quite important to string theory, others don't seem to have much to do with it. While there is plenty of arrogance and fad mentality among string theorists, they certainly have tried all sorts of things to get string theory to work, including using many ideas from QFT. But I don't think that there is some idea about QFT out there which will fix the problems of string theory. What's remarkable about string theorists and their descent into the swampland is not so much that they're not willing to try new ideas from QFT to fix string theory, but that they're unwilling to abandon the idea that the answer to the problem is a string theory.

26. **Arun**
March 16, 2006

Is the superstring an extended object or not?
Bert Schroer writes:

> It was already mentioned before that whereas it is true that the extension of the classical Nambu-Goto string (whose canonically quantized version is the "mother" of the string-theoretic reformulation of Veneziano's dual model) is really a relativistic spacetime string, this is not the case for its canonically quantized counterparts.
>
> The only intrinsic notion of a quantum relativistic localization is that obtained by translating this object into other positions and checking whether its commutator with the original object is nonvanishing once the second would be string shape enters the causal influence region of the first. This test was made (even by string theorists) and the result was negative [7][8]; the commutator is just that of two pointlike objects localized at the center of mass points of the imagined strings.

27. **Aaron Bergman**
March 16, 2006

Quantum field theory means different things to different people, for what it's worth. At the risk of generalizing too much, it's a widespread opinion that the axiomatic approach to quantum field theory hasn't been particularly fruitful. Most don't look at it as ignoring "part of quantum field theory" so much as ignoring an *approach* that is not viewed as useful.

Dr. Schroer obviously disagrees.

28. **Juan R.**
March 17, 2006

Benjamin said,

"So in superstrings, as I understand it, they assume an open or closed string, instead of a traditional point particle […]"

There exists many misconception around string "theory" hype. I carefully recommend to anyone does not believe even a 10% is said in popular treatises about string "theory".

The objects called strings play a fundamental role in an asymptotic regime of the branescan. However, once outside the asymptotic regime, one finds a sea of new objects called p-branes, with p taking values from 0 to 9. The 2-branes are usually identified with the traditional superstrings.

There is a joke claiming that now string theory is not about strings!

The history of the subject is full of historical twists. One of more interesting I know is that after of decades "explaining" us how "stupid" the pointlike particles of the standard model were and why a concept of extended objects (the strings) was "cool", we have finalized that string theory is not fundamental and we need some thing new called "M-theory" by now.

The (M)atrix version of M-theory (only known at my best current knowledge) is based in D0-branes as fundamental entities. The D0-branes are pointlike particles… The strings are considered derived objects in the new approach.

Therefore, in some sense, stringers are returning to the "old" (but effective) quantum field theory of pointlike particles but they do not say in that way of course 🙂

—

Juan R.

Center for CANONICAL |SCIENCE)

29. **Thomas Love**
March 17, 2006

Schroer listed some legitimate complaints against string theory. Since no one knows what the M stands for (Mystery, Membrane, Mistake), in the future, let's refer to M-Theory by its initials. M-T as in empty.

30. **Dumb Biologist**
March 17, 2006

Holy cow! I completely missed the note about the trackbacks! Mysterious indeed. It strikes me as a bit of an anticlimactic resolution to what looks to have been a rather painful and/or irritating episode for all involved, this sudden and silent reversal.

I sincerely hope the end result is not just a reasonable acknowledgement of your *bona fides*, Dr. Woit, but refinement of arXiv policies that allows everyone, including the moderators, to avoid future headaches.

George Mackey 1916-2006

March 16, 2006
Categories: Obituaries

It was sad to see an announcement today on the Harvard math department web-site of the death earlier this week of emeritus Harvard professor George Mackey.

Mackey's mathematical work is dear to my heart, since its central concern is the relationship between quantum mechanics and representation theory. He began his career in functional analysis, getting his Ph.D. in 1942 under Marshall Stone. Back in 1930 Stone and von Neumann had proved a crucial theorem about quantum mechanics, a theorem which essentially says that once you choose Planck's constant, up to unitary equivalence there is only one possible representation of the Heisenberg commutation relations. This uniqueness theorem is what allows one to just define quantum theory in terms of the operator commutation relations, and not worry about which explicit construction of the representation of these operators on a Hilbert space one uses. The theorem is only true for a finite number of degrees of freedom, and thus doesn't apply to quantum field theory, one reason why quantum field theory is a much more subtle business than quantum mechanics. Stone and von Neumann put their work in the context of representation theory of the Heisenberg group (actually due to Weyl) and this was of great interest to mathematicians since it was one of the first results about the representation theory of non-compact Lie groups. For an excellent history and introduction to this subject, see the paper A Selective History of the Stone von-Neumann Theorem by Jonathan Rosenberg.

Mackey seems to have been the person who gave this theorem its name, in his important paper of 1949 "A Theorem of Stone and von Neumann" which generalized it. Over the next few years Mackey extended this much further in a series of papers on induced representations (representations of a group G "induced" from representations of a subgroup H). The foundation of this work is now known as the Mackey Imprimitivity Theorem, and it provides a powerful tool for studying representations of a large class of non-compact groups, including especially semi-direct products.

Mackey was a wonderful expositor, and over the years I've learned a great deal from some of his expository books and papers. His 1963 monograph *Mathematical Foundations of Quantum Mechanics* is very readable. In 1966-67 he gave a course at Oxford on representation theory and its applications, the notes of which were published in 1978 as *Unitary Group Representations in Physics, Probability and Number Theory*. This is a fantastic book, covering a wide range of topics relating quantum mechanics, representation theory and even number theory. A later collection of expository material, from 1992, was published by the AMS as *The Scope and History of Commutative and Non-Commutative Harmonic Analysis*. It contains what is perhaps the best of his expository work, an historical survey first published in the AMS Bulletin in 1980 entitled "Harmonic Analysis as the Exploitation of Symmetry".

While I never took a course from Mackey, I did get to talk to him on several occasions. I especially remember a conversation in which he described his technique for speaking French during the time he spent in France. He decided to speak his own rationalized version of the language, eliminating extraneous and confusing structure like genders of nouns. Not clear what the French thought of this. He was an original, and I'm sad to hear he's no longer with us.

Update: Stephanie Singer has put up copies of letters from Mackey on her web-site. A memorial service for Mackey will be held in Cambridge on April 29.

Comments

1. **Who**
   March 16, 2006

   He came to Berkeley one semester in late sixties (anyway around 1970) and I took his course on Group Representations. The large hall was always packed. A lot went by too fast for me. Left a vivid impression though. He had so much verve.
   Would like to hear others' recollections.

2. **Stephanie Frank Singer**
   March 17, 2006

   George Mackey's daughter, Ann, was my roommate in college and remains one of my closest friends. When I was a sophomore in college, interested in both mathematics and physics, I sent him a short note asking him what his field was, and requesting advice. He responded with two long handwritten letters that I am reading with interested after rediscovering them in my files this morning. I can't imagine that I understood much of it at the time. Here's a sample, from near the end of the first letter:

   > This description of what my field is and how I got into it is rather longer than I expected it to be and I apologize if I have tried your patience. On the other hand it is probably a bit unintelligible without some further explanations. What are topological groups, Hilbert spaces and group representations and why do I find them so interesting? Actually when properly understood they are rather central topics and I think it will help me give you a picture of what pure mathematics is all aobut and how it relates to physics if I give a brief historical sketch including definitions of some related concepts….

   What is astounding is that nearly twenty-five years later these forgotten letters hidden in a drawer describe exactly the mathematics that is most important to me. It never would have occurred to me to think that George Mackey sent me on that path, since I got there (like Alice through the looking glass) by heading in what seemed to me the opposite direction.

   Also astounding is the generosity of a man who would handwrite twenty or more pages in response to a student he had never met.

   Another memory: George supposedly spent eight hours each day on his research, in addition to his teaching and other obligations. He told me once that his method was to work for forty-five minutes of each hour and then spend fifteen minutes of each hour reading a nonmathematical book or taking some other kind of break. Knowing this has always helped me to pace myself.

   Thank you, George.

   Alice Mackey, George's wife and Ann's mother, supported him and put up with his foibles. The French, at least, had the option of sending him home! Alice gave him the space and time to do his work and letter-writing. In the last several years, Alice supported and loved George through a difficult period of declining health. Thank you, Alice.

   There will be memorial service for George at Harvard on April 29, 2006, in the afternoon.

3. **woit**
   March 17, 2006

   Stephanie,

   Thanks a lot for your memories of Mackey.

I'm guessing we both arrived at an interest similar to Mackey's in QM and representation theory via symplectic geometry and its relation to quantization. I recommend Stephanie's books "Symmetry in Mechanics" and "Linearity, Symmetry and Prediction in the Hydrogen Atom", to anyone who wants an introduction to these subjects.

4. **MathPhys**
   March 19, 2006

   I highly recommend Stephanie Singer's book "Symmetry in Mechanics" to all students who wish to read about symplectic geometry and related topics for the very first time. People should write more such books.

5. **Santo D'Agostino**
   March 20, 2006

   I heartily agree with both Peter and MathPhys: Both of Stephanie Singer's books are very good, very clear introductions. I wish they were available when I was learning these subjects for the first time, but at least I can recommend them now to any interested beginners who cross my path.

   Warm congratulations to Ms. Singer, and I do hope you keep writing!

6. **harsha**
   March 20, 2006

   to stefanie :
   as someone who is precisely trying to find what mackey describes, i was wondering if maybe you could type or scan these letters onto your website. (i realise they are pretty long and a lot of it might just be defining lie groups, hilbert spaces, etc. so, maybe atleast some excepts which talk about motivations). it would prove to be very useful, specially for younglings like me who dont know too much.

7. **MathPhys**
   March 20, 2006

   Stephanie,

   There is an incredible amount of material on integrability that remains to be explained to students. Why not write sequels to "Symmetry in mechanics"?

8. **Stephanie Frank Singer**
   March 20, 2006

   Well, wow, folks, thanks for the feedback. I'm feeling the love! Don't hesitate to repeat your opinions at Amazon.com.

   I will post George Mackey's letters as soon as I get ten quiet minutes. I'm incredibly busy in 2006, starting two businesses (real estate development and election data consulting) and working to get Chuck Pennacchio elected to the US Senate. Also, I'm working with activists around the state of Pennsylvania to fight the installation of unauditable, unaccountable voting machines. I'm starting to wonder how many Americans really believe that democracy is worth fighting for. I'm talking about fighting laziness, established power brokers and bureaucracy here on the home front.

   So I haven't even found time to post the errata that Tudor Ratiu so kindly sent me for *Linearity, Symmetry, and Prediction in the Hydrogen Atom*, much less to write a new book. But do send me ideas of what you'd like to read. The next book I've thought of is the story of the *real* Kepler problem, that is, the problem Kepler devoted most of his energy to (according to Koestler's *The Watershed*): why are the planets where they are? I heard a talk by Jerry Marsden a few years ago announcing that there was a geometrical explanation: once Jupiter's orbit is given, then the positions of the other planets can be predicted geometrically. That's the book I'm daydreaming about these days. But it will have to wait.

9. **woit**
   April 17, 2006

   A link to Mackey's letters at Stephanie's web-site was added.

Three-year WMAP Data Now Out

March 16, 2006
Categories: Uncategorized

Data from the second and third year of the WMAP satellite experiment has just been released a few minutes ago. There a press release and other general information page. The scientific paper explaining what this new data tells us about cosmology is Three-Year Wilkinson Microwave Anisotropy Probe (WMAP) Observations: Implications for Cosmology. This is a good time to admit that I'm no cosmologist, and thus not the person to get information from about the significance of these results. However, I expect some of the earliest informed discussion of them should take place on various blogs, and I'll be linking to those as I see them.

Update: Comments from Sean Carroll and Steinn Sigurosson. Discussion at CosmoCoffee and Bad Astronomy and Universe Today Forum. Also a posting from Lubos. I'm no expert here, but Lubos's comments seem to me to be nonsense (I find it hard to believe that in the three-year data set they're resolving structure 100 times smaller than in the first year, and I think he's just completely wrong to say that this data rules out ekpyrotic or cyclic models).

Update: Christine Dantas also has more about this.

Update: Amazingly, Lubos still is maintaining that the 3 year results have 100 times better angular resolution than the 1 year results. This kind of fanatical inability to ever admit that one was wrong about something goes a long way towards explaining the current state of string theory.

Comments

1. **Dumb Biologist**
   March 16, 2006

   I think the 100x estimate is actually correct. However, according to a new article in the NYT here:

   http://www.nytimes.com/2006/03/16/science/16cnd-cosmic.html

   But Paul Steinhardt of Princeton, who has lately championed an alternative to inflation, in which the universe begins and ends cyclically in a collision between a pair of island universes, know in string theory as branes, pointed out that the new data are also compatible with his theory. Calling the results "extremely important," he noted that they were in agreement with the simplest models of these theories.

   'It rules out need for anything exotic," he said.

   I haven't the remotely foggiest idea about any of it, I only pass this along…

2. **woit**
   March 16, 2006

   D.B.,

   Do you have any reference for the 100x claim? It may be right, I'm just having a hard time believing that tripling your data size allows you to increase your resolution 100-fold. But maybe they did something much more clever with the later data.

   I notice that Lubos edited somewhat his statements about cyclic models being ruled out, and added a statement that Peter Woit knows nothing about cosmology. More recently he seems to have deleted the attack on me, maybe he's about to do more editing on the cyclic models claims. He's hilarious.

3. **woit**
   March 16, 2006

   D.B.,

   Thanks for pointing out the NY Times article, I hadn't seen it. I think the "100x" number is referring to being able to detect much smaller signals in order to get the polarization data (not smaller patches of sky). Lubos seems to have completely misunderstood what this has to say about cyclic models. Cyclic models predict no visible B-mode polarization. If WMAP had seen this kind of polarized signal, it would have ruled them out, but it didn't.

4. **Dumb Biologist**
   March 16, 2006

   I don't think this is the first place I saw the "100x better" thing, but…

   http://www.space.com/scienceastronomy/060316_wmap_results.html

   This new signal is roughly 100 times weaker than the signal we analyzed three years ago and about a billion times less than the radiant warmth we feel from the Sun," said Lyman Page, a WMAP team member from Princeton University.

   Now, I don't know if getting 100x better signal is really the equivalent of saying you're looking at structures 100x smaller, but there's a 100x improvement in something.

   I'm sorry I can't do better. I've been kind of sneaking in glimpses of reading between time points and basically behaving like an ADHD-addled channel surfer when I should be paying more attention to what I'm doing. I find stuff like this almost unspeakably cool, so I get a little worked up…

5. **Dumb Biologist**
   March 16, 2006

   D'OH! Simulpost!

6. **Alejandro RIvero**
   March 16, 2006

   Perhaps some topologist will give us someday a hint about why the universe is flat for the current bigbangy solution. Meanwhile, I do not understand the fuzz about WMAP in theoretical blogs. Except that some of you had got a hidden idea to explain flatland.

Did I already suggested to read astro-ph/0601168?

7. **Christine Dantas**
March 16, 2006

Dear Peter Woit,

Perhaps he is referring to section 4.1.3 (paper on Implications for Cosmology), where the WMAP data is combined with small scale CMB experiments, which probe smaller angular scales than the WMAP.

As expected, the paper on polarization measurements is quite involved. I am always concerned with the fact that you have to use a model of the foreground emission to subtract from the data. It is not a trivial thing at all to separate the intrinsic polarization signal from the foreground emission. So I would say that we must interpret these results with great care. In any case, I am just starting to effectively read these papers, it will take time to digest the details.

I will try to call some of my colleagues at INPE who are specialized in CMBR (we have a ballon experiment here in Brazil) to add their comments to my blog page, where you can find also a brief post on the new WMAP data.

Christine

8. **woit**
March 16, 2006

Christine,

Thanks, I've added a link to your posting and look forward to hearing anymore you learn about this.

Hard to tell what's going on with Lubos. He keeps changing his posting, now he's got some WMAP pictures up there which he claims show that 3-year WMAP can resolve solid angles 100th the size of 1-year WMAP and 1000th the size of COBE. But as far as I can tell, the resolution of both 1-year and 3-year WMAP is the same: 13 arcminutes, 33 times smaller than COBE. No idea where he's getting his numbers from, they don't seem to make sense.

9. **Dumb Biologist**
March 16, 2006

I just can't get over the idea that physicists can say something rather concrete about, and hence test models of, what what was going on about a trillionth of a second after the visible universe started expanding. That's so impressive it's absurd. Sorry for the fanboy slobberfest, but all hail physics! I know there are some crises in subsets of the field, but if results like these don't reveal something vital, vibrant, and important overall, I don't know what does.

10. **Luboš Motl**
March 16, 2006

Dear Peter,

it would be a great idea for you to stop spreading the noise and nonsense that you keep on spreading.

Open one of the 74 news articles here

http://news.google.com/news?hl=en&ned=us&q=wmap+patches&ie=UTF-8&scoring=d

and read the paragraph that contains the word "patches". After you read this paragraph, and maybe the whole article, correct the order of magnitude error that the journalist introduced, if you're capable to do so, then reread my blog article to be sure what I mean, and check the pictures that I linked to see why I say what I say.

The maximal designed angular resolution of WMAP is fixed but more data and a much better statistical treatment has allowed to create these much finer maps and everyone who has looked at the 2048-pixel-wide pictures (or who read some of the basic articles released today) knows that.

I have always had the comment that the "modern" models of ekpyrotic/cyclic Universes try to be indistinguishable from inflation. This is the uninteresting part. The more interesting part is that they predict blue gravity waves that must eventually come into the game. At any rate, no one except for the authors knows exactly how to derive the magnitude and frequency-dependence of the tensor perturbations from the ekpyrotic scenarios, so I think it is fair to write what I wrote.

Best wishes
Lubos

11. **Christine Dantas**
March 16, 2006

I will try to understand this.

For the moment, all I can say, from the *Mission Explanatory Supplement paper*, is that the WMAP experiment can reach a 13 arcminute FWHM resolution for the temperature anisotropy of the CMBR. Also, that "the skymap data products derived from the WMAP observations have 45 times the sensitivity and 33 time the angular resolution of the COBE DMR mission".

Christine

12. **woit**
March 16, 2006

D.B.,

The WMAP results are truly amazing, and this is a really healthy subfield of physics. But the results about inflation are a bit overhyped. This is giving us very little information about how inflation may have worked, although of course it's remarkable you can say anything at all. The next generation satellite, Planck, should have the sensitivity to see the effects of gravitational waves if they're there, which would be something a lot more substantial.

13. **Christine Dantas**
March 16, 2006

Sorry, in the previous post, where I wrote “experiment can reach a”, replace this by “experiment has a”.

Thanks
Christine

14. **woit**
March 16, 2006

Lubos,
I don’t understand what you’re trying to accomplish by pointing me to a popular news story with obviously incorrect numbers in it (100 billion light years??). If you can point to somewhere in the WMAP scientific papers where they explain about the 100 fold increase in resolution, please do so. It still seems to me that the “100” at issue here is an increase in sensitivity. They’re able to extract much smaller temperature differences from the noise, but this doesn’t at all mean that they have 100 times higher angular resolution.

And your comments about ekpyrotic scenarios make no sense: “blue gravity waves”? no one but the authors knows what the theory predicts? Sorry, but I’ll take Steinhardt’s claims about this over yours.

15. **Dumb Biologist**
March 16, 2006

Oh, I know. I guess to me it’s just a bit mind-blowing that quite possibly now there’s a rather compelling “smoking gun” indicating, that, for real, inflation happened. I understand (I think) that the whole “inflaton field” thing is something of a gigantic black box, and that lots of folks find this deeply disatisfying. Some of the articles I’ve read seem to allude to this reality, though I agree the limited scope of our knowledge about inflation (even with this news) gets short shrift.

16. **woit**
March 16, 2006

First year data:
http://lambda.gsfc.nasa.gov/product/map/dr1/map_images/pub_images/ILC_Maps/ILC_b.jpg

Three year data:
http://lambda.gsfc.nasa.gov/product/map/current/map_images/ilc_gal_moll_2048.png

Note the hundred-fold increase in resolution.

17. **Chris W.**
March 16, 2006

Lubos, thanks for reminding us yet again that you’re the smartest guy in the room. We just can’t seem to get it through our thick little heads.

18. **D R Lunsford**
March 17, 2006

This is my last comment for this blog, I’m done with all of this. You can read me in forthcoming papers if you’re interested. Otherwise, my last comment is this:

Penrose completely destroyed the idea of inflation using nothing but entropy, yet we are supposed to believe, after years of looking and a long wait, that the octupole anomaly magically cleared itself up (and not a word was said about the ecliptic anomaly, so there was nothing to clear up). In fact, the work of Huterer, Copi, and Starkman was not even mentioned. Instead, we got a dissertation on applied inflation theory.

So, who should I believe? An authentic genius, or a star chamber of people with, shall we say, vested interests?

Science is dead. Nice job y’all.

-drl

19. **lmot**
March 17, 2006

There is absolutely no evidence of global warming in this new data.

Also, that plot everyone is showing has several wiggles, and everyone knows wiggles are a property of strings.

2006 Templeton Prize

March 16, 2006
Categories: Uncategorized

The 2006 Templeton Prize of $1.4 million was awarded yesterday to cosmologist John Barrow. Barrow is the author of about 400 scientific articles and nearly 20 popular books. In recent years, one of his interests has been the possibility of time-variation of fundamental constants. At a press conference in New York yesterday, he said that new data on quasars expected within two months may provide evidence of such variation.

Science and Spirit has an article by Barrow written for the occasion and called The Unexpected Universe. It also has a report on the press conference that goes on at length about the string theory anthropic landscape and credits Barrow (and Tipler) with writing a "highly influential book for the interface between science and religion" back in 1986 entitled *The Anthropic Cosmological Principle*. The report includes the following gibberish:

String theorists also assume that other universes, which collectively compose a "multiverse," exist in other dimensions outside of our observational parameters. Our own very limited experience suggests that finely tuned universes might be more likely to exist than more randomly constructed universes, at least over the long term. If this is true, then fine-tuning may be a guide that cosmologists can use to one day locate and observe an alternate universe.

Barrow himself however doesn't seem to have much to say about the string theory landscape.

Maybe if Leonard Susskind hadn't said unfriendly things about having no use for religion in his recent book, he could have been $1.4 million richer instead of Barrow. The New York Times headlines its story about this Math Professor Wins a Coveted Religion Award. A mathematician friend of mine is kind of outraged at this and wants to write to the Times to complain about the description of Barrow as a "math professor".

Comments

1. **john e gray**
   March 17, 2006

   Barrow is a math professor at Cambridge. By UK standards he is a mathematician where they treat theorists who work in GR no differently than other types of applied mathematics. Stochastic geometry, which has orgins in GR, is one of many types of math applications that are explodiing in various domains that have come from the exploration of GR. By the way, his "undergraduate degree " is in math. He is I would bet that he has more papers listed in MathNet than your friend has or will have in 30 years. By what criteria is someone a mathematican these days, working in a subfield that has less than a hundred people who publish papers in that field?

2. **Robert**
   March 17, 2006

   Not only GR is math at Cambridge, UK but even string theory: DAMTP, the Department of Applied Mathematics and Theoretical Physics, is the department not only of Barrow but also of relativist Stephen Hawking, and Michael Green, Paul Townsent, Garry Gibbons, Fernando Quevedo, Nick Dorey, David Tong amongst other. All these people write papers about string theory but in Cambridge's view are members of the maths department.

3. **csrster**
   March 17, 2006

   In Cambridge even a semi-maths-literate ex-astrophysicist like me
   can count as a mathematician 🙂 I tell people that Stephen Hawking works in my old department.

4. **anon**
   March 17, 2006

   A previous recipient of this prize is Paul Davies. It is quite an event, held in a cathedral. In his acceptance speech, Davies said:

   'A world freely created by God, and ordered in a particular, felicitous way at the origin of a linear time, constitutes a powerful set of beliefs, and was taken up by both Christianity and Islam. An essential element of this belief system is that the universe does not have to be as it is: it could have been otherwise. Einstein once said that the thing that most interested him is whether God had any choice in His creation. According to the Judeo-Islamic-Christian tradition, the answer is a resounding yes.'

   – http://www.firstthings.com/ftissues/ft9508/davies.html

5. **Chris Grant**
   March 17, 2006

   As a followup on John E. Gray's point, the ' "math professor" ' Barrow has 100 items indexed on MathSciNet.

6. **woit**
   March 17, 2006

   Personally I'm all in favor of blurring the boundaries of math and physics so that in many cases there's no useful distinction, but here I'm with my friend that the NYT headline is misleading, given local American usage about what is math and what is physics. Barrow clearly falls on the physics/astrophysics side of the physics/math divide. MathSciNet is very inclusive and has lots of papers by physicists (a good thing), but the relevant point is that 3/4 of Barrow's papers are not in MathSciNet.

   In any case, my suspicion is that the headline writer used "Math" rather than "Physics" because, with 3 characters less, the headline fit better. Most readers of the newspaper can't tell the difference anyway, not that there's anything wrong with that.

7. **damtp_dweller**
   March 17, 2006

   To Robert and those discussing Barrow's position at Cambridge:

   John Barrow is Professor of Mathematical Sciences here in Cambridge. Although he (and Gibbons, Hawking, Quevedo, Perry, Green, and others) works in the Centre for Mathematical Sciences, they are technically members of DAMTP, the Department of Applied Maths and Theoretical Physics. We inhabit the CMS with our colleagues from DPMMS (pure mathematics and statistics). However, given the fact that papers from both departments are usually given the affiliation "DAMTP/DPMMS, Centre for Mathematical Sciences," it's perhaps understandable that there's confusion.

I do agree that Barrow is a physicist rather than a mathematician (and I think that's how he would describe himself), especially given the content of the majority of his papers.

8. **secret milkshake**
March 17, 2006

gibberish or not, at least they are not giving this money away to Bogdanov brothers or some God-damned televangelist.

(ID is wooly thinking but I do not have problem with it when it is presented honestly – and it could make for engaging literature, in Dyson case)

9. **MathPhys**
March 18, 2006

Barrow (and Hawking) are in DAMPT: that's in dept of 'applied mathematics' (that's the fluid mechanics and nonlinear PDE people) and 'theoretical physics' (that's the GR, string theory, and theoretical high energy physics) people.

None of these people would call themselves 'mathematicians'. Michael Green would never call himself a mathematician. Same goes to Quevedo, Townsend, and all the others. The most mathematical work at DAMPT is probably done in Fokas' group (I think he's at DAMPT).

It is true that their students dp parts I, IIA, IIB and III of the 'maths tripos', but 'maths' here has historical origins peculiar to the Cambridge system. In Oxford, they would all be physicists. In Edinburgh, they could be either in maths or in physics.

No, Barrow is *not* a mathematician in the sense that Andrew Wiles, for example, is a mathematician.

10. **damtp_dweller**
March 18, 2006

It's DAMTP, not DAMPT (although it is, for some bizarre reason I've never quite understood, pronounced "dampt").

11. **Not a Nobel Laureate**
March 18, 2006

"What mighty contests rise from trivial things"

— "Canto I; The Rape of the Lock", Alexander Pope

12. **MathPhys**
March 18, 2006

damtp_dweller,

You're right. My mistake. I was writing phonetically. By the way, what became of the old building on Silver St? It was charming.

13. **damtp_dweller**
March 18, 2006

The old location at Silver Street is now used by (I think) the graduate union and various university admin departments. I think Darwin college may also have taken some of it. I guess it did have a certain character but the new location is much, much better suited to the size of the department.

14. **MathPhys**
March 19, 2006

I haven't been in the new location, but from the pictures that I see, the architecture horrifies me. Can you open the windows for ventilation? or are the offices air sealed? How far walking is it from Churchill College? How far from St John's?

15. **damtp_dweller**
March 19, 2006

The place grows on you after a bit. I was a bit concerned by the style of the buildings at first too, particularly the outside. However, it's actually a really nice place in which to work (apart from the food, which is typically Cambridge: hugely over-priced, bland, and sometimes inedible). The windows open in all of the offices that I've seen, although the automatic blinds are a bit unnerving on first sight. Oh, and parts of the place are very confusing. I still manage to get completely disoriented by the circular staircases in Pavillion B 🙂

It's less than two minutes walk from Churchill (you simply go north and walk across Madingley Road. St. John's is a bit farther but still less than ten minutes on foot.

16. **Alex**
March 19, 2006

The award seems a little overgenerous and somewhat dubious in its purpose. If the aim is to try to reconcile religion and physics, I can't say that I've found Barrow's arguments particularly relevant to such a project.

"The Constants of Nature"2002 is quite a thought-provoking book, which examines the fine tuning of physical constants to create laws of physics which are conducive to the evolution of life.

In particular, Barrow is interested in the fine structure constant, the ratio between the energy needed to bring two electrons from infinity to a distance of s \against their electrostatic repulsion, and (ii) the energy of a single photon of wavelength 2 \pi s \.

This is one of the few dimensionless constants in nature and Barrow seems to believe there is evidence that it has changed over time.
His evidence comes from the Oklo 'natural reactor' in West Africa and the study of Quasars using multiplet sampling.

The implications of his views are that the universe in its current phase (not just at inception) is particularly fine tuned for life. He also examines the views of Linde on "eternal inflation" and Lee Smolin's arguments about black hole universes being naturally selected.

As to his suggestions about the changing fine structure constant: there is counter evidence which suggests that it's NOT changing, and furthermore, he provides no theoretical explanation about why it would change, or the atom become slightly larger since 14 billion years ago.

However, even though he is something of a court physicist to Number 10 and the Vatican, I can't say I noticed any strong religious arguments in this book.
I suppose a potential one is that some "intelligent designer" was responsible for the "initial conditions" of inflation, although he isn't crazy enough to suggest this.

To me, the anthropic principle argument is tautological and leads to not very interesting theoretical musings. In a similar vein Eddington and Dirac spent a lot of time in semi-mystical investigations of large number theory.

17. **Alejandro Rivero**
March 19, 2006

Eddington was a bit more semi-mystical than Dirac. The later was betting more on evolving constants than on a special coincidence. By the way it is interesting to nocice that a lot of crackpot ideas on gravity and masses around in the net are just variants in disguise of Dirac's large number idea.

About this kind of research, let me to remember that we took a whole year in physicsforums clasifying the numerical coincidencies around. Some famous ones are even published. For instance, Lubos happened to rediscover m(electron) / m(proton), which probably is the shortest article ever published in the Physical Review, decades ago.

18. **island**
March 19, 2006

At least Barrow is looking for real first principles.

The structure of our universe is the most natural configuration for the big bang to produce, per the least action principle… because… [infinities, multiverses, and "cosmological selection effects" don't go here, jack].

Please.

19. **MathPhys**
March 19, 2006

Thanks, damtp_dweller.

By the way, what do you do for research? What are people up to in Cambridge nowadays? The place is a bit too quiet as compared to a few years back. (Don't worry about hijacking a thread. It's all for the good.)

20. **Incompetent**
March 20, 2006

"It's DAMTP, not DAMPT (although it is, for some bizarre reason I've never quite understood, pronounced "dampt")."

Blame it on the peculiarities of what English speakers consider 'pronounceable'. For obvious reasons, 'mtp' at the end of a word is deemed unpronounceable, so the 'p' is ignored, giving 'damt'. But the combination 'mt' is also unusual, so this is resolved by inserting a 'p' in the middle. In a similar way, many English speakers will say and even write 'hampster', even though the word is actually 'hamster' (borrowed from German, hence the un-English 'mst').

I tend to say 'damt', but then again I have enough experience of German that 'mt' doesn't feel unnatural.

21. **Alejandro Rivero**
March 21, 2006

I think that phonemes are defined via the possibility of differentiating words, thus this combination is not only unpronounceable, it plainly does not exist. Just as a colored hadron.

2005 Topcites

March 20, 2006
Categories: Uncategorized

The SLAC SPIRES yearly list of [most frequently cited papers in 2005](#) is now available. I commented [recently](#) on what this was likely to show, quantifying the intellectual collapse of string theory since 1999.

There are exactly three post-1999 particle theory papers among the top 50 in the list. Two of these are about flux compactifications and have moved up significantly since last year reflecting the increasing popularity of landscape studies. At number 18 (up from 29) is the KKLT paper from early 2003, and at number 34 (up from 54) is an earlier paper from 2001 by Giddings, Kachru and Polchinski. The only non-landscape post-1999 paper to crack the top 50 is the 2002 Berenstein, Maldacena and Nastase paper on PP waves (which is part of the AdS/CFT story). It just barely makes it at number 49 (down from 32 last year).

The highest ranked post-2003 paper is the Arkani-Hamed and Dimopoulos 2004 paper on split supersymmetry. It's at number 106, with a total of 103 citations.

There's also a new 2005 [All-time topcited list](#). Maldacena's AdS/CFT paper from 1997 remains very near the top, with 3881 citations. There is nothing post-1999 on this list, which includes the top 186 papers. If recent trends continue indefinitely, it seems entirely possible that no post-1999 particle theory paper will ever make this all-time top-cited list, allowing historians of science to conclusively pinpoint the death of particle theory as having coincided fairly precisely with the end of the 20th century. This is optimistically assuming people lose interest in the landscape. It is also possible that landscape studies will come to dominate the field, with landscape papers then climbing up into the all-time topcited list. This doesn't really change the conclusion about the death of particle theory.

Comments

1. **Who**
   March 20, 2006

   This doesn't really change the conclusion about the death of particle theory.

   LOL

   (agree of course)

   A bunch of us made predictions about the topcites 2005 list.
   Here are the forecast poll results:
   http://www.physicsforums.com/poll.php?do=showresults&pollid=580

   I see that your (notevenwrong's) guess proved correct. Congratulations.

2. **ObsessiveMathsFreak**
   March 20, 2006

   > It is also possible that landscape studies will come to dominate the field, with landscape papers then climbing up into the all-time topcited list.

   Philosophy. The last refuge of the hypothecator.

3. **Jimbo**
   March 20, 2006

   No critique is as devastating as a quantitative one ! Kudo's to Peter for calling it as it is. One wonders, how many hundreds of string papers are still pending review at Nucl.Phys.B, PhysRev D, etc ? How many grad students will join the `String Army' this year ?
   Like the `war' in Iraq: Massive inertia*snail's pace = business as usual.

4. **JustAnotherInfidel**
   March 20, 2006

   Perhaps one could find another six year period where very few papers made the topcite list (say, 1987-1993 where the top cited papers are pretty far down the list, not to mention few and far between). Looking selectively at the data really doesn't prove much.

5. **woit**
   March 20, 2006

   JustAnotherInfidel,

   I strongly disagree that I'm looking selectively at the data. I think that if you ask virtually any particle theorist who has been in this business for many years what they think about how many new ideas have come along over the last six years compared to other periods in their lifetime, they will tell you that the number of such ideas in the past six years has been unusually low. I'm putting forward the most honest and objective way I know of quantifying this. If you have a better one, let's hear it.

6. **arnold**
   March 20, 2006

   Maybe there have been good ideas, but if you're not a big name….nobody pays attention.

   The big names decide what is good physics and what is not.

   Maybe we are close to the end of physics.
   In science in principle you should not worry what the big names think, but what the experimental measurements say.

7. **JustAnotherInfidel**
   March 21, 2006

   Dr. Woit–

I feel that one must take other things into account when making claims like “the death of particle physics is nigh”–the set of string theorists who understand, say, twistor strings is a very small subset of all string theorists, so naturally there will be fewer citations of that work. Papers like RS1 or AdS/CFT represent huge steps in understanding (or misunderstanding, if you like) of physics that don’t just happen everyday. Some of the papers listed on this list (just the theory papers apply) represent really revolutionary ideas (inflation, Beckenstein/Hawking radiation, asymptotic freedom…).

One should look at the theoretical papers (not things like WMAP data) which represent ground-breaking ideas (judged by the number of citations), and look at the years in which they were published. If you can show that the current six year lapse in theory is longer than any other before, then I will accept your claim that particle physics is in a dark ages.

8. **MathPhys**
March 21, 2006

I recall the years from the mid 70’s to 1983 (so that’s more than 6 years) when basically nothing was going on in theoretical HEP (except for Witten’s papers on Chern Simons theory, non abelian bosonization, global anomalies, etc, which were too mathematical to be called high energy physics proper). The standard model was fully established and people started playing mundane games with Kaluza Klein, SUSY, etc.

The situation changed completely in 1984–1985 which were revolutionary years: superstring theory, conformal field theory, quantum groups, knot invariants, and RSOS models in stat mech, and probably more. Probably the most revolutionary years since quantum mechanics and/or quantum electrodynamics, in terms of the number and quality of new ideas floating around.

So yes, I’m hopeful that something will come along.

9. **Alejandro RIvero**
March 21, 2006

As I see, these people at SPIRES are he(l)pful. And jointly with arxiv and KEK, it is a resource to be proud of.

10. **woit**
March 21, 2006

Because of some of the comments here, I took a closer look at some of the topcites data, even making a histogram of the time distribution of the all-time topcited papers.

The results for 1975-86 agree with my memory of the period. There were lots of new ideas that people were working on: supergravity, lattice gauge theory, instantons, GUTs, 2d models, etc. (Chern-Simons-Witten was later), although some level of frustration at how difficult it was to either solve QCD or get beyond the standard model. The topcites data show a consistent average of 5-6 papers/year for this period, with a peak of 9 in 1985 corresponding to the advent of strings.

The period from 87-93 does contain many fewer papers, just seven, an average of one/year. This is significantly more than the post 99 zero/year, but more to the point, research during this period was largely driven by new developments in a way that is very different than recent history. If you look at the earliest SPIRES yearly top-cites compilation, covering 92-94, and ask how many of the top-cited papers were from the six years just before this (1987-93), you get 13 out of 40, even though most of these papers are on things like CFT results that were very popular then, but haven’t made the all-time list since people have lost interest in them. This number of 13 out of 40 is very different than the similar number for 2005: 3 out of 50 (or 2 out of the top 40). While people in 1993 felt things were in a lull, the situation was very different back then, there were plenty of quite recent results that large numbers of people were working on.

Also, while 1987-93 was a fallow period for string theory, it was the period when the whole field of topological quantum field theory got going, and Witten did his Fields medal winning work on Chern-Simons. There’s nothing remotely similar to this in the last few years.

Citation counts don’t reliably identify important new ideas, they reflect what the community is working on. Famously, Weinberg’s electroweak paper received no citations for several years, but now is the most cited theory paper of all time. Maybe there is some important idea out there in the recent literature, but its significance is not understood, so hardly anyone else is working on it. Witten’s twistor string paper has received relatively few citations, but that seems to be not because people don’t understand it, but because it has not led to as much as originally hoped, so few people continue to work on it.

11. **JustAnotherInfidel**
March 21, 2006

Dr. Woit–

Thank you for your detailed analysis. Absent any other indicators, your analysis does seem to indicate that ideas are fewer and further between. It does seem to quantify the dark ages of particle physics you alluded to in the original post. Perhaps we have been “sidetracked”, so to speak, on ideas like RS1 and AdS/CFT, or that the field is more dispersed than at other points in the past.

12. **David B.**
March 21, 2006

Dear Peter:

It takes many years before a paper can gather 1000+ citations (this is the magic number that makes it to the top-cited papers of all time, 990 is no big step down from this). This never happens immediately. Claiming that because no such thing has happened in the last 6 years that theoretical particle physics is dead is a wrong conclusion and does not take into account historical data on the subject.

To make such a statement you have to wait (at the very least) about 15 years and then look back. You also have to wait and see what happens after the LHC is taking data to declare that theoretical particle physics is dead, because if anything unexpected is found, I can guarantee that it will be a very interesting time for theoretical physics.

To judge a field by how many papers from the previous year have gathered 100+ citations is probably more correlated with having fashionable theories/mechanisms. On the other hand, if a subfield is more technical and it requires a lot of hard work to get results, then you would be extremely happy if a paper got 50+ citations in that year. One should always judge by quality, not just quantity.

The fact that experimental physics is dominating the list is actually very healthy: there is very interesting new data (neutrino masses, cosmological precision experiments) and there is a strong effort to fit these observations with our understanding of the standard model and physics beyond the standard model (like inflation).

13. **A. Torok**
March 21, 2006

David B.: I strongly agree with your statements.

14. **woit**
March 22, 2006

David B. and A. Torok,

Obviously I was making a provocative statement about the "death of particle theory". We don't know yet how it will turn out, maybe the LHC will change everything. However it's not going to be telling us much for at least another three years or so, at which point the period of zero progress that I am identifying will have stretched out to about a decade.

As for your comments about the all-time list, you're just refusing to look at the data. If someone comes up with a new idea that is recognized as important in this field, it tends to get a lot of attention quickly. There are a lot of smart people out there everyday looking at new papers on hep-th, ready to jump on anything new they see. Historically, it is not at all unusual for important papers to accumulate nearly 1000 citations within 2-3 years (the most heavily cited papers generally get 3-500 citations/year). I picked one recent year at random (2002), and looked at the SLAC all-time list which that year included the top 104 papers. Eight of these were from within the previous 5 years. It doesn't take 15 years for most important papers to show up on the all-time list. Please look at this data, and don't just assume you know what it says. And no, the reason for this data is not that the list is being overwhelmed by a huge number of important new experimental results. Besides WMAP, which dominates the list, this has not been a period of unusually many or important HEP experimental discoveries, although HEP experiment is in better shaper than HEP theory.

As far as I can tell you're both students and lack historical perspective on this issue. If you ask anyone who has been working in this field for twenty years or more, paying close attention to new developments as they arose, I think you'll find that virtually all will tell you that the dearth of recognizable new ideas during the last six years compared to earlier periods has been dramatic. If you don't want to believe me or other people who have this experience, look for yourself at the SLAC data.

I think it is extremely unhelpful for people to try and deny the facts of the matter here. Particle theory is in a bad way, and the community needs to figure out why this is true and whether something can be done about it. Denying that this is true won't make the problem go away.

15. **David B.**
March 22, 2006

Dear Peter:

I am not a student and I have been in the field for more than 10 years, so I have some historical perspective. Furthermore, I am not in "denial". I was just pointing out that it is too early to make concluding statements like that. You have an undeniable right to have a negative opinion of the current state of theoretical particle physics and nobody is trying to rob you of that fact.

Looking at the papers on the topcited list that have between 1000+ and 1200 citations you will quickly realize that the grand majority of them have been around for more than ten years, so my estimate is broadly correct.

To make a point: the paper of Faddeev and Popov that introduced
ghosts and gauge fixing in 1967 took until after the year 2000 to get to the top. Are you telling me that their work is unimportant because it didn't make it to the top in three years (as it obviously should have)?

It is not true that you generically get to that number within three years. That is truly exceptional, even today.

You also failed to mention that many of the topcited papers that accumulated 500+ citations per year for three years in a row around 1997 were very closely related to each other, and not completely independent ideas either. The year 2002 is not random.

Why don't you make the experiment with the year 1995? Not a single theory paper from the previous five years is in the topcited list.

The theoretical high energy community is waiting for new data. The extreme precision of current data places enormous constraints on new ideas. The big breakthroughs you are probably referring to (circa 1997) are related to new paradigms to avoid these constraints based on ideas of extra dimensions, etc.

New ideas that people are waiting to "pounce on" require two ingredients: avoiding current phenomenology constraints in a clever way and most importantly: a simple calculation attached to it. Without the second fact you can not hope that there will be 500+ citations within a year, no matter how important your idea is.

16. **woit**
March 22, 2006

David,

My apologies for mistaking you for a student, I was going by the e-mail address you gave and very quick results of a google search.

As for the points you make:

1. Sure, at all times most top-cited papers are older ones, but that's irrelevant. What I'm trying to get at is the number of recent ideas recognized by the field as significant. Often these will appear on this list within a few years of when they come out.

2. As I noted earlier, historically there are examples of papers whose importance was not recognized for quite a few years. Weinberg's "A theory of leptons paper" was a dramatic example of this. The Faddev-Popov paper of 1967 was another. When it came out, very few people cared about non-abelian gauge theories (they were too busy working on the predecessor of string theory…). It was only after 't Hooft showed renormalizability in 1971 and asymptotic freedom was discovered in 1973 that gauge theories became popular. If you're going to compare citation counts of papers from the sixties and ones from nearly forty years later , you need to take into account two factors. The number of papers being written was many fewer forty years ago, and, unless you are looking at a different data source than me, the SLAC all-time lists from years ago are much smaller and nowhere near as deep as the current ones. The latest all-time list (2005) that has zero papers from the last six years on it is 186 deep. The lists from 1995 and 2000 that you refer to have only 47 and 87 entries respectively.

3. The year 2002 was randomly chosen as a recent year that would pick up the pre-2000 period before the point at which I claim brain-death set in. Sure, among the 8 papers on that list, there are correlations. I wasn't making much of that absolute number, just that it is very different than zero. I'll also point out that I wasn't comparing numbers, just responding to your incorrect argument that recent papers don't appear on the all-time list.

4. Let's discuss 1995. As others have remarked, 1987-93 was considered by most people the previous low point of new ideas in particle theory, so 1995 should be the worst previous year in terms of numbers of new ideas showing up on these lists. The 1995 all-time list doesn't include any recent papers, but it is only 47 long. If you can get ahold of a list of the top 186 papers, which is what you need to directly compare to 2005, I'm willing to bet it will have a significant number of recent papers in it. The 1995 list of top papers recently cited (in this case 93-95, there is no data for just 1995) includes in the top 40 eight from the period 1990-95. Compare this to 2005, where the top 40 includes just the two recent landscape ones. Also note that 1995 was the year of the second superstring revolution, and the year after Seiberg-Witten. Anyone looking at these numbers at the end of 1995 would also be well aware that there were quite a few papers from late 1994 and 1995 that were accumulating citations at a very fast rate. The 1995 situation was just radically different than that of today. If you know of any papers too recent to make the 2005 top 40, but that are accumulating

citations very quickly like Seiberg-Witten, let me know.

5. The new ideas that dominate the late 1990s list are not just extra dimensions. Many of them have to do with AdS/CFT and other aspects of string theory, which have nothing to do with the precision of current data.

6. Making the top 40, or the top 186 all-time list doesn't require 500+ citations in a year. To make the top 40 just requires about 150 citations/year, to make the top 186 all-time list over five years would require 200 citations/year. Historically, in all previous periods, there have been many papers getting cited at these rates (although as you go far back in time, you have to rescale for the significantly smaller number of papers being produced). My point is that the only recent papers from the last 6 years in this league are landscape papers: the KKLT paper (223 citations last year), and the Giddings et al paper (169 citations).

7. People don't just pounce on phenomenology papers, they also pounce on string theory papers. I don't think of KKLT as a phenomenology paper. If someone had a decent new idea about string theory, even if it had zero phenomenological implications, it would get pounced on.

Finally, what is the point of trying to make these tendentious arguments that you are using? Do you really believe that the last six years has been a healthy period for particle theory? If you believe that, let's hear some examples of important ideas from the last six years, of a number even remotely comparable to previous six year periods. If you agree that this has not been a healthy period, why try to come up with quantitative arguments for the idea that it's not so bad?

17. **David B.**
March 22, 2006

Dear Peter:

The last few years have been a healthy period in high energy theory. They are definitely not as intense as during the 1990's, and I don't feel like there is nothing to do or work on, quite the contrary. My "counting" is just as tendencious as yours. I picked 1995 exactly because that was another period where particle physics had been declared dead a few years back (I had been told that there was no future in high energy particle physics before entering graduate school).

There have been very interesting recent results (they might not make it to the topcite 1000+), but here are a few of extremely high quality results of the previous five years (apart from the ones you have already mentioned) that indicate that theoretical high energy physics is doing just fine:

Minahan and Zarembo hep-th/0212208 provided new tools to address the AdS/CFT. They showed that one loop computations in the AdS/CFT gave rise to integrable spin chain models. The presence of an integrable structure in the calculation of anomalous dimensions in N=4 SYM was unexpected.
The follow-up work is very technical, but it is extremely exciting and of very high quality.

Dijkagraaf and Vafa in a series of papers in 2002 made a conjecture relating matrix models and non-perturbative effects in supersymmetric gauge theories. Although they were inspired by string theory, these results hold regardless of this motivation and permit one to calculate many results exaclty, as was proved later. You might complain that they relate to supersymmetric theories, but then, it is very hard to come by with exact calculations in any context.

Arkani-Hamed, Cohen and Georgi hep-th/0104005 introduce the ideas of deconstruction and show how we might be fooling ourselves into thinking we live in higher dimensions if the high energy theory has the right set of discrete symmetries.

Witten gave a huge push to the computation of helicity amplitudes in QCD in hep-th/0312171. Many of the techiques devised since then on these ideas are extremely efficient at computing gluon scattering. This is the dominant source for uncertainties in the LHC.

I can keep on going. Most of the interesting works of these previous five years involve computations that require a lot of work. I don't see any reason to believe that because calculations are harder that progress has stopped. It just moves slower and that is reflected in the number of citations per year that a paper receives.

The above are a few of my favorite papers of the past five years, and you might appreciate that none of them are directly related to the landscape. Even though I try to keep up to date in high energy physics, there is not enough time in the day to read everything that comes out, so I can not give you an in-detail presentation of what is going on everywhere, but for me, personally, it feels like a healthy environment to be working on right now.

18. **woit**
March 22, 2006

David,

I think you've identified the best work in particle theory during the past few years, but I also think that it is very clear that these are results that are of much less significance than ones in any similar list that people would have generated in the past when asked to produce a list of the most important work during the past 5-6 years.

To have a serious discussion of the significance of those papers, one would have to go into a long discussion of what exactly is in them, what one could in principle hope they might lead to, what exactly they have led to, etc. They are all papers that have received a great deal of attention, and are certainly not things that have been ignored or not understood. The relatively modest number of times they were cited by other papers during 2005 reflects the fact that other theorists find these results of limited interest and have not been able to get very much out of them. The only one that has more than 100 citations is Minahan-Zarembo, because many people are trying to find a useful integrable structure in N=4 SYM, partly because of the relevance to AdS/CFT. It will be interesting if that goes anywhere, but progress seems limited, and this is still a very long way from leading to something that you can do any physics with.

The Dijkgraaf-Vafa results give very limited information about certain supersymmetric models, again, a long way from being useful for physics. My impression is that many people have looked at this, but few people are still working in this area.

The twistor string again is an idea that seems not to have led to as much as people had originally hoped. Again, many people have looked at this (probably most serious particle theorists), but very few people are still working on it.

Those are general impressions from listening to talks, scanning hep-th papers, reading conference talks off the web, etc., but they are borne out by the quite small number of citations those papers have been getting compared to the leading papers from previous periods. Take a look at the total number of citations of these papers so far:

Witten, twistor string: 206 citations in 2.25 years, 92 citations/year

Dijkgraaf-Vafa: 322 citations in 3.6 years, 89 citations/year

Minahan-Zarembo: 283 citations in 3.25 years, 87 citations/year

Arkani-Hamed, et. al: 300 citations in 5 years, 60 citations/year

I think it is extremely unlikely that any of these papers will ever reach 1000 citations or the all-time topcited list. The only one of these where there still seems to be sizable interest is Minahan-Zarembo, and whether people keep citing it depends on whether the search for integrability in N=4 SYM goes anywhere. Maybe interest in this topic will grow significantly, or continue at the present rate for the next seven years necessary to get to 1000 citations, but I find that hard to believ.

All in all, I'm sorry, but I think the evidence that you've given that this is a healthy period just doesn't stand up to scrutiny. It's always true that there is "something to work on"; one can always calculate something, the question is how interesting the result of the calculation is. The SPIRES data I've quoted conclusively show that the theory community sees these results as much less interesting than the results achieved in any other similar period in our lifetimes.

New Top Quark Mass

March 20, 2006
Categories: Uncategorized

Via Tommaso Dorigo of the CDF collaboration, the news that the Tevatron Electroweak Working Group has released a new analysis of combined CDF and D0 data with the most accurate result so far for the top quark mass: 172.5 +/- 2.3 Gev. Last summer this value was at 174.3 +/- 3.4 Gev (see a posting here), an improvement over the earlier value derived just using Run I data of 178.0 +/- 4.3 Gev.

The paper describing these results is available now here, and will soon be on the arXiv as hep-ex/0603039. This new result represents a determination of the top quark mass to 1.3% accuracy, and the paper claims that further Run II data should ultimately allow an accuracy of better than 1%.

For a talk about the significance of the top quark mass, see here.

Comments

1. **Tony Smith**
   March 20, 2006

   Peter said "… the Tevatron Electroweak Working Group has released a new analysis of combined CDF and D0 data with the most accurate result so far for the top quark mass: 172.5 +/- 2.3 Gev …".

   I think that it should be noted that the "172.5 +/- 2.3 Gev" value applies only to one of three peaks in Fermilab t-quark data.

   For example, the original 26 April 1994 paper FERMILAB-PUB-94/097-E contained a 26-event histogram for W+(3 or more)jets, without b-tags, that is Figure 65, and which (with an x for each event in each 10-GeV T-mass bin)looks like:

   80

   90

   100
   x
   110

   120
   x
   130
   x
   140
   xxxxxxxx
   150
   xx
   160
   xxx
   170
   xxxx
   180
   xxxx
   190

   200

   210

   220
   xx
   230

   240

   In addition to the broad central peak around 170 GeV or so,
   there is a small high peak around 220 to 230 GeV,
   and a tall narrow low peak around 140 to 150 GeV.

   In that paper, Fermilab dismissed the tall narrow low peak, saying "… We assume the mass combinations in the 140 to 150 GeV/c^2 bin represent a statistical fluctuation since their width is narrower than expected for a top signal. …".

   Nevertheless, the low and high peaks persist in later data for which they are not excluded by cuts etc, and in my opinion all three peaks should be taken into account in analyzing the T-quark data.

   If it is assumed that the T-quark is a simple quark just like all the others, and if one if forced to pick only one of the three peaks and reject the rest, then perhaps the Fermilab consensus view that "the" T-quark mass is "172.5 +/- 2.3 Gev" might be justified (but the other two peaks would still be unexplained, as they do not appear to be accounted for by conventional background).
   However, I do NOT think that the simple T-quark assumption is accurate.

   Kent W. Staley, in his book "The Evidence for the Top Quark" (Cambridge 2004), mentions on pages 295-296:

   "… hints that the simplest hypothesis that the top candidate events are just the ttbar events and SM background may not be entirely correct such hints 'should be monitored carefully, as they may be offering us glimpses of new physics' …

Even when experimenters find that they have acieved experimenters' success, they look more closely to see the interesting flaw in their achievements – the discrepancy that will mean, not failure … but the possibility of some new success …".

Staley's book contains detailed references and context that I will omit here.

Possible clues about the three peaks might come from such sources as:

hep-ph/030713, by Froggatt, that studies the interrelationships of the T-quark, the Higgs, and the Vacuum; and

hep-ph/0311165 and hep-ph/9603293, by Yamawaki et al, that describe T-quark condensate models, including:
Nambu-Jona-Lasinio models with mt about 145 GeV
Kaluza-Klein (8-dimensional) model with m_t about 173 GeV
Bardeen-Hill-Lindner models with mt about 218 GeV.

Please see the papers themselves for details.

In deference to Peter's wish to exclude discussion of non-standard physics models, I will stop here.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

2. **Bourgeois Nerd**
March 20, 2006

So is this just a refinement of the Standard Model or something interesting? I'm afraid I'm too much of a layman to tell.

3. **woit**
March 20, 2006

Bourgeois Nerd,

In some sense, this is just a more precise determination of one of the standard model parameters. But it's a very important one: the Higgs couples to fermions according to their mass, so it is by far most strongly coupled to the top quark. So, if you want to detect the Higgs, either directly or indirectly, knowing the top quark mass as accurately as possible is important.

4. **Thomas Larsson**
March 20, 2006

How does the new top mass change limits on the Higgs mass and other things?

5. **andy s.**
March 20, 2006

Taking these three values together makes the TQM in the range
[173.7 – 174.8] (or 174.25 +/- 0.55).
That's the lowest of the three maxima and the highest of the three minima.

Although I suppose it doesn't really work that way.

6. **Dumb Biologist**
March 20, 2006

In Dr. Tait's presentation, he suggests the mass of the top can tell physicists something about electroweak symmetry breaking. I know a tiny bit about the Higgs mechanism giving mass to the W's and Z, and obviously if the top couples most strongly to the Higgs, there's some kind of circumstantial relationship. But beyond the mere fact that the vector bosons and the top all are mighty heavy, weigh about the same, and hence have similar couplings to the Higgs, what deeper principles are being revealed?

7. **Alejandro Rivero**
March 21, 2006

Ah, I also had a prediction for the top quark, but it was exactly at 175 Gev (188 u), hmm. (well, it could be a prediction for top+charm meson).

Seriously, what matters is not 175 or 173, even Tony's 145 peak. What matters is why it is at the electroweak symmetry breaking scale.

8. **Luboš Motl**
March 21, 2006

Dear Bourgeois Nerd,

the top quark mass is important because it is important for the existence of supersymmetry and the minimal supersymmetric standard model.

Thomas Larsson: yes, the Higgs mass in the minimal supersymmetric standard model depends sensitively on the top and stop masses.

More generally, the exact top quark mass is important because it affects string theory phenomenology. Click the PDF file that Peter Woit linked in the last sentence of this article – with the word significance – and go to the middle of the PDF file.

Of course, without supersymmetry, the top quark mass is not too significant, and the Higgs mass is independent of the top quark mass.

See my blog for more details about this issue.

Best wishes
Lubos

9. **stan**
March 21, 2006

This blog post is indeed remarkable. Lubos writes:

“Its mass used to be thought to be 178 GeV. It was lowered to 175 GeV and now to 172 GeV, with error margin around 2 GeV. [..]

At roughly the same time, Faraggi included some loop corrections which made the agreement worse because the physical mass he obtained [using string theory] was 192 – 200 GeV. [..]

this story is an example that string theory can predict properties of particle physics and it can even give us the right predictions.”

It is encouraging that superstring-theorists tried (try?) to predict particle properties. But the Faraggi result was obviously *falsified* by the experiment.

10. **Luboš Motl**
March 21, 2006

Dear stan,

you forgot to quote my sentence about the crackpots who form the core of the readership of Peter Woit’s blog. It was inconvenient for you, was not it? The tree level calculation from 1991 was confirmed by experiments, the loop-corrected calculation from 1995 was falsified by experiments. At any rate, you should better be careful in saying that the loop-corrected prediction was falsified because the owner of this blog does not like those who disagree with his thesis that string theory etc. is “not even wrong”.

Best
Lubos

11. **Alejandro Rivero**
March 21, 2006

So, string calculations do not need loop corrections?

12. **The Anti-Lubos**
March 21, 2006

Luboš Motl: *you forgot to quote my sentence about the crackpots who form the core of the readership of Peter Woit’s blog*.

Well, you’re here all the time. Sounds like a case of the crackpot calling the crackkettle black.

13. **Lubos Motl**
March 21, 2006

Alejandro Rivero, your dumb questions convinced me that I must simply put you in the very same group as Nigel Cook, Tony Smith, Danny Ross Lunsford, Quantoken, and many others. Sorry but it will probably be hard for you to reverse this policy.

The full calculations of course include loops. But the tree level calculations don’t. Saying that tree level calculations are worthless, which is what you are essentially doing, is incredibly stupid. Most of the key calculations in physics, including those in the discovery of the electroweak theory, were tree level calculations.

The full calculation needs us to clarify many more extra details, and it is completely conceivable that the 1995 loop calculation is simply further away from the correct answer than the 1991 tree level estimate. Experimentally, it is clearly the case.

Best
Lubos

14. **woit**
March 21, 2006

I definitely recommend Lubos’s blog entry

http://motls.blogspot.com/2006/03/peter-woit-recommends-supersymmetry.html

It really has everything:

1. delusional thinking about string theory: “string models predict the exact masses of all particles”.

2. bizarro ideas about the scientific method: see his discussion of the Faraggi “prediction” of the quark mass.

3. lunatic rants about string theory skeptics, e.g. yours truly.

As for his comments about the top mass being not significant if there is no supersymmetry, this is just misinformation.

In the standard model itself, precision electroweak measurements depend both on the top quark mass and on the Higgs mass. Knowing the top quark mass, these measurements give significant constraints on the Higgs mass. See page 6 of the Tait talk I quoted. I don’t know of a source for updated numbers on these constraints using the latest top mass.

In the MSSM, at tree level the Higgs mass has to be less than the Z mass, which is wrong. Loop effects allow a higher Higgs mass, and the one-loop calculation strongly depends on the top mass. One can push up the Higgs mass, but doing this requires increasing degrees of fine-tuning. People are already uncomfortable with the degree of fine-tuning required to get the Higgs mass above the LEP experimental lower bound of 114 Gev. See page 12 of the Tait talk.

15. **The Anti-Lubos**
March 21, 2006

Lubos Motl: *Saying that tree level calculations are worthless, which is what you are essentially doing, is incredibly stupid*.

Saying that he said this or even implied it when he did not is incredibly stupid.

16. **Lubos Motl**

March 21, 2006

Dear Peter Woit,

1. the statement that a particular background in string theory predicts the masses of all massive particles ia a mathematical fact, and only people who are un-educated in theoretical physics misunderstand why

2. a prediction means a calculation of a certain observable that will be observed in the future, which is what Faraggi (and many other people) did. His calculation could have been imperfect but it was definitely a prediction, and once again, only complete ignorants like you and your crackpot fans are unable (or unwilling?) to understand this fact

3. the lousy intellectual quality of your society of cranks is being proved by virtually every posting you have ever made, and almost every comment that your crackpots fans ever added to your postings

4. the Higgs mass itself can't be affected or deduced theoretically from the top quark mass if you have no supersymmetry simply because there are quadratic divergences correcting the Higgs mass that are not cancelled automatically unless one has SUSY, but can be freely cancelled by (nonSUSY) counterterms. The value of the top quark mass influences various high precision measurements but does not, in any way, decide about the existence of the theory if the theory is non-supersymmetric; any value is equally OK theoretically

5. you exactly explained why top and stop are important for MSSM – because their large one-loop corrections make the predicted Higgs mass plausible. This fact is the main reason why studying the top with a better accuracy is a scientifically justifiable enterprise. The large one-loop top corrections are in no sense "fine-tuning" because the top Yukawa coupling is not a small number. On the contrary, it is experimentally proved to be around 1/sqrt(2), so your whining about "fine-tuning" in the one-loop corrections is just another, 9325th evidence of your complete ignorance about particle physics.

I recommend you to avoid commenting on things that you have no idea about, such as particle physics, string theory, cosmology, and many other fields.

Best
Lubos

17. **Alejandro Rivero**
March 21, 2006

It could be worthwhile to quote also here the paper Lubos brought into focus: http://ccdb4fs.kek.jp/cgi-bin/img_index?9110375
I am not sure if it is the first estimate running down to the 175 range; I have seen some this range quoted in other ocassions, but also in higgs estimates, so memory is fuzzy here. Perhaps Tony can remember better, as top is in his focus.

18. **woit**
March 21, 2006

Lubos,

You're missing the main point in the non-supersymmetric case. High-precision electroweak measurements depend on the Higgs mass, so they indirectly are measuring it. This measurement is highly sensitive to the value of the top mass. For latest results, including the latest top mass, see

http://lepewwg.web.cern.ch/LEPEWWG/plots/winter2006/

For the fine-tuning I'm talking about in the supersymmetric case, see the discussion at

http://golem.ph.utexas.edu/~distler/blog/archives/000336.html

and maybe you can write in to the author to inform him how ignorant he is on this issue.

19. **Alejandro Rivero**
March 21, 2006

It is perhaps remarkable that Tait's slides still carry a plot from a 1993 article: the one in page 14 comes from page 9 of http://arxiv.org/abs/hep-ph/9311269 . Incidentally it can be noticed that it is still adjusting to a top quarks mass of 150 GeV (is it the second discontinous line counting from the bottom left corner?). Those interested on predictions should note also figure 5 in page 29 of this preprint.

On a different matter, I was thinking about aplying to the graduate program of Harvard. Do you know any professor there able to write a recommendation letter?

20. **anon**
March 21, 2006

Lubos is such a crackpot. According to Lubos, (Lubos Theorem 1)measuring the mass of the top quark is a scientifically important endeavor iff supersymmetry is true. (Lubos Theorem 2) For any theory X, If you cite a reference that talks about X, then you believe everything X says is true. (contrapositive of Thm2: If you do not believe everything that X says is true, then you do not cite a reference about X)

21. **Lead by Science**
March 21, 2006

Maybe Peter and Lubos should remain confronational with each other, for "information" that was just supplied by that "energy" expenditure. 🙂

22. **Thomas Larsson**
March 21, 2006

A story dedicated to Yellow Michael.

An anthropic string theorist, a non-anthropic string theorist, and a physicist were discussing the experimental status of string theory.

Anthropic string theorist:
– String theory is in complete disagreement with experiments, but that is OK because God created the universe to be compatible with human life.

Non-anthropic string theorist:

– String theory is in complete disagreement with experiments, but that is OK because Witten has won the Fields medal.

Physicist:
– String theory is in complete disagreement with experiments.

23. **tommaso dorigo**
March 22, 2006

Hi all,

wow there is a lot to learn by reading these comments… People who trust recent advances in theoretical physics and people who eat sausage should not ask what there is inside. (One might rightly add people who blindly believe in experimental errors – but that's another story).

Just a remark: I accept non-experimentalists being speculative about background fluctuations in principle – the charm meson started off as one at the very beginning, and so did other important discoveries in particle physics as well as other disciplines. But claiming that there are three peaks in the top quark mass distributions CDF and D0 isolate, based on a plot of a very background-rich sample isolated with a tenth of the statistics available today, is plain nuts.

Ah, and the top mass did not "use to be 178, then 175, then 172 GeV". CDF measured it at 174 GeV in 1994 (!) in the "evidence" paper, one year before we decided to go observational. Later, more precise determinations have brought the value around a bit, but world averages have stayed in the 171-178 GeV range, and always within 1-1.5 sigma from 174.

24. **Tony Smith**
March 22, 2006

Tommaso Dorigo said, about my comment: "… claiming that there are three peaks in the top quark mass distributions CDF and D0 isolate, based on a plot of a very background-rich sample isolated with a tenth of the statistics available today, is plain nuts. …".

I agree. However, that is NOT what I said. I did specifically mention only one plot (1994 CDF untagged semileptonic), but I also went on to say: "… the low and high peaks persist in later data …".
I did not give specific references to the later data for space and exposition reasons, but, since the question has been raised, here are some of them (note – of the three peaks (low, central, and high), the central peak is the one around 173 GeV):

the 1997 D0 untagged semileptonic histogram at hep-ex/9703008 showing all three peaks;

the tagged CDF semileptonic events in hep-ex/9801014 showing the low peak;

the tagged D0 semileptonic events in hep-ex/9801025 showing all three peaks;

the CDF dileptonic events in hep-ex/9802017 showing the low and central peaks;

individual D0 dileptonic events described in the thesis of Erich Varnes at http://wwwd0.fnal.gov/publications_talks/thesis/thesis.html including the following events:

Run 84676 Event 12814 (2-jet analysis gives low peak)
Run 58796 Event 417 (low peak)
Run 90422 Event 26920 (low peak)
Run 88295 Event 30317 (low peak)
Run 84395 Event 15530 (3-jet analysis gives high peak);

individual D0 dileptonic events described in hep-ex/9808029 using the matrix-element weighting algorithm that "… is an extension of the weight proposed in [R.H. Dalitz and G.R. Goldstein, Phys. Rev. D45, 1531 (1992)] …", including:

e mu #1 (low peak)
e e #1 (low peak)
mu mu (showing low and high peaks)

which events were also described using the neutrino weighting algorithm, with these results:

e mu #1 (low peak)
e e #1 (low peak)
mu mu (high peak)

I hope that the above makes it clear that a reasonable analysis of later data shows that all three peaks do continue to show up.
Of course, you can argue about interpretations, and indeed (as Fermilab has done) argue that since the low and high peaks involve fewer events, they should be thrown out. However, it seems to me that the 3-peak point of view is also reasonably arguable, and that work on theory and future data analysis should include work on all three possible peaks instead of designing cuts etc that exclude the high and low peaks from data analysis.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Please note that the above is only an outline indicating some of the later data, and that extended discussion of relevant subtleties (see my web site) is beyond the scope of blog comment discussion. I would like to think that the above is extensive enough to refute the blog characterizations of me as "just plain nuts" (by Tommaso Dorigo) and "moronic crackpot" (by Lubos Motl).

25. **Juan R.**
March 22, 2006

Lubos Motl Said,

1. the statement that a particular background in string theory predicts the masses of all massive particles ia a mathematical fact, and only people who are un-educated in theoretical physics misunderstand why

2. a prediction means a calculation of a certain observable that will be observed in the future, which is what Faraggi (and many other people) did. His calculation could have been imperfect but it was definitely a prediction, and once again, only complete ignorants like you and your crackpot fans are unable (or unwilling?) to understand this fact

3. the lousy intellectual quality of your society of cranks is being proved by virtually every posting you have ever made, and almost every comment that your crackpots fans ever added to your postings

[…]

Excuseme my complete ignorance and my obvious lack of brilliance (i am not a Fields medallist and never will become one) Dr. Motl, but i am unable to understand above 3 points (really i am unable to understand the rest of points also, but this is not a journal :-).

3. How can one measure the crank level of any posting here? I am just curious! Please do not cite Baez ranks or similar, since none of them has been experimentally proven! From a historical point of view, i also find interesting all past genius of physics (including Isaac Newton) was named cruncks by their then colleagues. Is not this a lesson for all of us?

Moreover, whereas i disagree with some comments posted in this blog, i agree with many others and, by now, *the net balance is clearly positive*.

2. I think that none manual on epistemology or experimental methods (i know) agree with you.

A prediction does not mean a calculation of a certain observable that will be observed in the future, as you are saying. A clear example is computation of size of hidden dimensions on string theory. One can calculate that "observable" and obtain any result from zero to infinite.

Yes, the mainstream claims that hidden dimensions "are" of Planck scale but arguments are easily rebated from chaos (microstructure from sub-Planck scale spacetime modifies correlations and the physics *over* the Planck [There is a good QM example worked in Nature by Zurek and methods can be adapted to a generalized string theory formulation in Liouville space similar to that of Nanopoulos and coworkers]).

Not forget also that in certain cosmological-string models the hidden dimensions are infinite in size (FAPP).

Therefore, any measure of the size of any hidden dimension (if any) and agreement with string theory (any value from zero to infinite) is not a prediction.

1. You claim it *is* a mathematical fact that a particular background in string theory predicts the masses of all massive particles.

Perhaps i am un-educated (in your own words) but, sincerely, I fail to see that from both a physical and a mathematical point.

See the point from set theory. If you are claiming that string theory is a collection of different sub-theories/models each one with diferent "parameters" (background), then if a particular background in string theory "predicts" -this is not a correct word by point 1- the masses of all massive particles, then many other backgrounds do not and offer us wrong results. That is also a mathematical fact you forgot cite above.

It is also interesting to note that if X string models are compatible (the word "predict" is as said incorrect) with values of masses, and Y are NOT compatible. The ratio X/Y is either very close to or equals to zero. Therefore, from a Landscape point of view, string theory is not compatible with masses.

But if you are claiming that only one of those backgrounds correspond to the "real" (physical) string theory (with the rest of a pure mathematical interest), then your above phrase cannot be correct. Instead of a particular background in string theory… you would say just "string theory" (meaning with its only physical background, THE background, that nobody knows) …

—
Juan R.

Center for CANONICAL |SCIENCE)

26. **Alejandro Rivero**
March 22, 2006

Lead, yes it is true that interesting information appears even if with the pityful waste of confrontational energy. All this issue of SuGra Electroweak Breaking, from the plot in Tait's slides, is worthwhile to be contemplated. I was unaware of the trick of generating a phase transition by just running down the constants in such a way that the uncolored mass (the higgs) hits the change of sign.

Of course a collateral benefit of this confrontational show is that all the participant can hope get more visibility for their own papers. I expect the silencious espectators will consider it and jump to the arena some day.

27. **Steve Myers**
March 22, 2006

A note on crackpots & data: Just last night I discussed this iossue with my two sons (one a computer scientist who refused fellowship & got MS in less than a year; the other, a construction supervisor who got his degree & went into industry) & both agreed with me that the test for crackpots was whether or not their stuff worked. I now work in data acquisition & analysis (so I understand Tony Smith's concerns) and any theory I have must fit the facts & predict future behavior of equipment. My computer scientist son said he thought this throwing of "crackpot" at people & attacking their intelligence is a result of the scarcity of positions available & the fight for funding (one reason he left academia). I think it's very sad. Was Einstein right: is being a plumber the best option (or electrician or carpenter)? And finally, Lubos, of course Woit is right about Higgs mass by way of the top and precision electroweak measurements.

28. **Alejandro Rivero**
March 23, 2006

An update about the prediction of top mass Lubos was proud of. It was bases in the techique of infrared quasi-fixed points for the top quark yukawa coupling, a technique that was resuscited in the eraly nineties and at that time involved SuGra and string based models. But it comes from ten years before; most people quotes a paper of C.T Hill of 1981, Phys Rev D 24 p 691, which in turn attributes the idea to Pendleton and Ross in some paper written down at the end of 1980 and published as B. Pendleton and G.G. Ross, Phys Lett 98B p 291 (1981). The paper of Hill does not claim string or susy inspirations, and I can not tell of P&R now because it is an offline paper. But here goes one half of the string-susy-fundamentation of top mass.

The another half is the trick for Radiative "infrared" Electroweak Breaking used in Sugra. It is that because the Higgs is uncolored but the top is not, the RG equations can be adjusted in such a way that as one run them down from M_GUT to low energy, at some instant a critical point "a la Curie" is crossed and the Higgs field gets the negative mass square it needs to break electroweak symmetry. I can not tell if this idea is original of SUSY theories; Tait seems to imply it, and I can not find it argued in other kind of papers. In fact it is a very elegant idea.

Letter From Schroer

March 21, 2006
Categories: Uncategorized

Bert Schroer has sent me a very long and interesting comment for posting here. I've put it into a separate web-page. It includes both a lot of history and many different ideas. Unfortunately I don't have the time right now to write much about it in response, but just will make one point about the part that he explicitly addresses to me.

Schroer claims that geometric methods in QFT have so far only been useful in dealing with free fields in a fixed background gauge field or metric. This is largely, but not completely true. Most of our reasons for believing the standard model are based on perturbative quantization of gauge fields, and for this it's true that geometrical methods are not strictly necessary. But for QCD, we need a non-perturbative quantization of the gauge fields, and here lattice QCD is the best we've got. It is based upon discretizing a geometrically formulated path integral, preserving as much of the gauge field geometry as possible. My own guess is that there is still a lot to be learned about non-perturbative quantization of gauge fields, based upon the geometrical formulation of the problem given by the path integral approach and I have been working on speculative ideas of how to do this (some of which even involve gerbes and algebraic geometry…). This is still work in progress, maybe I'll someday find it really can't work, but for now I'm quite optimistic.

There's also a new survey paper on QFT from Fredenhagen, Rehren and Seiler. The authors discuss the current state of understanding of QFT, with some points of overlap with Schroer. Like Schroer, they also discuss string theory in detail, and are critical of the inability of the string theory research program to come up with precise statements about what the theory is supposed to be. They are however, much less forceful in their criticisms than Schroer.

Comments

1. **Alejandro Rivero**
   March 21, 2006

   Uff!

   And at the end the finger points to Weinberg. Indeed he left the field jumping into the astrophysics wagon. And sure it seems to favour the "effective" approach to QFT, but this is a blame on QFT in general, not on AQFT only. Hmm.

2. **Dick Thompson**
   March 21, 2006

   Schroer's letter is fascinating stuff. Aside from the geometric issue (or maybe you regard it as part of the same story) is his criticism of functional integral methods. Frankly I think the rush to use path integrals here there and everywhere has been a downer for particle physics. If algebraic methods can do the calculations and be rigorous too, that would seem an advantage.

3. **Lubos Motl**
   March 21, 2006

   These attempts to return physics to the 1930s or even further to the past are completely pathetic. Path integrals have rightfully become the dominant way to describe physics of quantum fields and their strength turned out to be even more obvious in theories with non-Abelian gauge symmetries (Yang-Mills symmetries much like conformal symmetries on the worldsheet etc. – we can't really live without these things today). It would be extremely awkward to do physics without the FP-ghosts and path integral in this case which is very important today.

   In the same way, it is impossible to undo the insights of the Renormalization Group from the 1970s – considered by many wise people to be the most important insights in physics in the second half of the 20th century. Quantum field theory is an important stuff. Some of them may be UV complete but even if they are UV complete, they should be used as effective field theories describing physics at some energy scale. Eventually we know that all QFTs break down at the Planck scale or earlier.

   Complaining about QFTs being used as effective QFTs with respect to an energy scale, and complaining about the use of path integrals in physics of 2006 is a soft type of crackpotism that only differs from crackpotism that rejects the theory of relativity by a factor of 3 (counted by the number of years of developments in physics that the crackpots decide to ignore).

   The whole concept of AQFT has become an unproductive line of reasoning because the basic philosophy of AQFT contradicts many things that have been demonstrably successful in the last 40 years: cutoff dependence and effectivity (non-exactness, in a sense) of QFTs, path integrals methods including ghosts. These methods had nothing to say about physics above 2 (or 3?) spacetime dimensions and the success of similar reasoning in 2D should be thought of as the maximum that can ever be extracted from this type of reasoning.

4. **Brett**
   March 21, 2006

   I think that Schroer has a number of interesting things to say about the problems with path integrals, and to a large extent, I agree with him. I too learned from Roman Jackiw to be very careful when dealing with functional integrals. However, I think that functional integrals can be fabulously useful tools. Their weakness is that they can easily fool us into thinking that we are working with mathematical well-defined and unambiguous objects, when in fact, we are not.

   People have, of course, tried to fix functional integration's problems. This would mean placing it on a firm mathematica footing, most probably in algebraic geometry. Then the usual "functional derivatives" would be reinterpreted as functors, whose representations could be studied, and so on. The problem with this idea is that it's proven to be impossible thus far (although people I know are still working on it). A rigorous formulation of path integrals would automatically entail a rigorous formulation of renormalization–already a tremendously difficult task. And the very fact that these problems are so difficult is a significant reason that many people, such as myself, choose not to work on them.

   I am somewhat mystified by Schroer's idea of what "algebraic" QFT is. With "axiomatic" QFT, it's reasonably clear what that means. And note that in axiomatic QFT, one cannot calculate any cross-sections. Schroer says one can do this in algebraic QFT, by means of ordinary Feynman diagram perturbation theory. However, such perturbation theory cannot be used in an "axiomatic" approach, because the resulting power series are asymptotic, and no rigorous estimates of their rates of divergence exist. In other words, this is an uncontrolled approximation, something that cannot be sensibly used in an "axiomatic" framework.

   So my question is: What exactly does Schroer's "algebraic" QFT encompass? Where does it end? Many (maybe most) papers in QFT do not explicity make reference to any path integrals but merely use the perturbation series as a starting point. What disqualifies these papers as "algebraic" QFT?

   I said above why I think few people work on axiomatic QFT–because the really interesting problems are prohibitively difficult. The tractable questions are just much less interesting. Blaming one stupid commenter who made a bad impression on Weinberg just seems bizarre, and I think Schroer does the physics community a disservice with this (essentially ad hominem) accusation.

5. **Kea**
March 21, 2006

Schroer: "Isn't it ironic that string theorists accuse the AQFT of the use of gratuitous mathematics when they themselves are using structures like gerbes and algebraic geometry?"

Oh no! Doesn't he even realise that rigorous non-Abelian localisation leads that way?

6. **Kea**
March 21, 2006

Just noticed gr-qc/0603079 by Brunetti and Fredenhagen.

7. **Eugene Stefanovich**
March 21, 2006

I strongly agree with the following Schroer's statement:

"…Lorentz covariance, quantum theory and the cluster property do not lead to QFT. .. These so-called "direct particle interaction" models fulfill the cluster-decomposition property yet they do not have a
representation in terms of a second quantization setting…With other words this claim by Weinberg … is incorrect"

There are few examples which do not fit within usual local QFT:

1. "direct interaction" theories mentioned by Schroer (see papers by W. N. Polyzou)
2. non-local QFT: M. I. Shirokov, "On relativistic nonlocal quantum field theory", Int. J. Theor. Phys. 41 (2002), 1027
3. A beautiful series of papers by H. Kita in 1966-1973. The last paper in this series is "A realistic model of convergent quantum mechanics of interacting particles" Prog. Theor. Phys. 49 (1973), 1704 where you can pick up the other references if interested.

8. **Thomas Larsson**
March 21, 2006

Dear Prof. Schroer:

In any case considerable progress to find analoga of those structures which one has in chiral theories (energy-momentum virasoro structure, current algebras) are well on their way in recent articles of Todorov et al.

In recent years the only papers of Todorov that I have read are his biographies of Einstein and Hilbert, and Heisenberg.

The mathematical problem of generalizing the Virasoro algebra to higher dimensions was settled a decade ago. The diffeomorphism algebra in N dimensions has two non-trivial cocycles by the module dual to closed (N-1)-forms, found by Rao and Moody (Comm. Math. Phys. 159 (1994) 239–264) and myself (J. Phys. A 25 (1992) 1177–1184), respectively. It may be worth pointing out that Bob Moody is somewhat famous, e.g. for the codiscovery of Kac-Moody algebras. Note that when N=1, a closed 0-form is a constant function, so the Virasoro extension is central when N=1, but not otherwise. In fact, all extensions of the diffeomorphism algebra were classified by Askar Dzhumadildaev (Z. Phys. C 72 (1996) 509–517); I spent a considerable time to understand his paper, which resulted in the review math-ph/0002016.

It is of course widely recognized that the standard axioms in LQP, in particular, the strong emphasis on the Poincare algebra, cannot hold in quantum gravity. The Poincare group has two properties in flat space:
1. it is the group of isometries.
2. it is the spacetime group which acts on the physical fields alone.
It is unclear to me if the isometry property plays any useful role in QG; it seems more appropriate for QFT in curved space, which is another problem. The second property in QG is subsumed by the spacetime diffeomorphism group. This is nothing but background independence, which is widely regarded to be the key conceptual lesson from GR.

A digression on LQG. I agree with the LQG people that manifest background independence (the formalism should not depend on any reference background structure, be it a flat metric or a preferred foliation) is necessary in QG, but I don't think that the LQG implements this deep idea correctly. In particular, LQG methods apparently fail when applied to the harmonic oscillator (hep-th/0409182), which in my opinion is a show-stopper. There are theorems which show that background indendence is incompatible with Fock quantization (in 1D, the only Virasoro rep with c=0 is the trivial one), but a no-go theorem only shows which axioms must be relaxed (here: anomaly freedom).

The generalization of the Virasoro algebra to 4D is thus the branch of mathematics which deals with background independence, quantum mechanically. The key insight from its representation theory, initiated in the Rao-Moody paper above and understood geometrically in math-ph/9810003, is that all fields must be expanded in a Taylor series around an operator-valued curve prior to quantization. This operator-valued curve, which can be identified as the observer's trajectory in spacetime, plays a crucial role; e.g., it enters into the relevant extensions. Without it, it is impossible to deal with gauge or diffeomorphism symmetry on a genuinely quantum level.

The paper I mentioned in my previous comment was a first attempt to reformulate QFT to incorporate this insight. That paper contains (at least) one error and one omission already for the harmonic oscillator: there is an overcounting, and no inner product was defined. These problems have now been solved.

Best regards,
Thomas Larsson

9. **D R Lunsford**
March 21, 2006

I want to say, sorry for overstaying my welcome, since I bowed out, but in light of the fudged WMAP data, to see Schroer and Larsson and others really talk physics – it does my heart some sorely needed good!

Thanks again, Peter.

-drl

10. **anon**

March 22, 2006

Lubos concludes his latest post: "If you don't care about the details of the Yukawa coupling, the content of the text above can be replaced by GoogleFights. ;-)"

which links to a Google comparison between his hits and those of Peter, naturally fixed…

Curiously, when Edward Witten and Lubos Motl are the contenders, Lubos still wins the fight!

Watch out Edward, you're not the king of string anymore!
🙂

11. **Thomas Larsson**
March 22, 2006

Not Even Wrong wipes out the Reference Frame.

12. **Bert Schroer**
March 22, 2006

I just now typed a 4 page letter answering most of the questions, but as a result of a wrong klick on my laptop I lost almost two hours of work; I am too frustrated to continue, maybe I will get to the sceen on the weekend again.
cheers
Bert Schroer

13. **Thomas Love**
March 22, 2006

Schroer ends his response with

"Isn't it ironic that string theorists accuse the AQFT of the use of gratitious mathematics when they themselves are using structures like gerbes and algebraic geometry? I cannot think of more useless mathematical structures for quantum theory (which deals with operators and states) than those. Hypocricy? Hubris? Double standards?"

Like it or not, physics is based on advanced mathematics. While I believe that string theory is based on faulty physics, one cannot fault the mathematics. Robert Hermann called it "experimental mathematics", learn some new mathematics and see if you can apply it to physics. Who knows what will work until we try it? I beleve the problem with AQFT is paradigm paralysis, keep trying the same thing. The impossibility of combining the Poincare group with internal symmetries should have led to a rejection of the Poincare group in favor of the de Sitter group or something bigger. I use U(3,2).

14. **woit**
April 6, 2006

Now that I'm back, I've turned back on comments here in case anyone wants to continue this discussion.

Hiatus

March 22, 2006
Categories: Uncategorized

I'm leaving tomorrow night on a trip that will take me away from internet access for a week or more. During this time I won't be posting anything, or able to manage the comment section, so I'll be shutting off comments late tomorrow afternoon, turning them back on when I'm back on April 4th.

The trip will take me to the middle of the Sahara, in Niger, where I hope to see the total solar eclipse next Wednesday. I'd like to be able to claim that this is some sort of scientific expedition, involving perhaps testing GR by measuring the deflection of starlight during the eclipse. But that's not the case; this is really just an excuse to go to an exotic location for a much-needed vacation. I thought for a moment about renting a satellite phone with a modem, and blogging from the desert, but decided that would seriously impinge on the important vacation aspect of this trip.

Another reason for the hiatus is that I haven't been able to come up with an inspired idea for an April 1 posting, and this gives me an excuse for giving up on trying to do that again this year. If I get any good pictures, maybe I'll finally get around to putting something more visually appealing here.

Comments

1. **Luboš Motl**
   March 22, 2006

   Enjoy the trip. Otherwise, there is no problem that you will turn the comments off. I will create a special article under which the discussion that would otherwise occur at Not Even Wrong can continue. Thanks. You're welcome, Peter.

2. **lmot**
   March 22, 2006

   I've got a better idea Lubos. How about you and Peter have a contest to see who can go longest without posting?

3. **woit**
   March 22, 2006

   Hi Lubos,
   Thanks. I guess… Why do I suspect you'll delete all the comments I would have kept, and keep all the ones I would have deleted? Oh, well, have fun!

4. **Arun**
   March 22, 2006

   Enjoy your vacation!

5. **weichi**
   March 22, 2006

   Enjoy the trip! I was in India for the total eclipse in 1995; it was a stunning experience. I hope the weather is perfect for you!

6. **Dumb Biologist**
   March 22, 2006

   Wear sunscreen!

7. **MathPhys**
   March 23, 2006

   How will you get to Niger, Peter? Any respectable airlines (allowed to land in EU) flies there?

8. **Bert Schroer**
   March 23, 2006

   Dear Peter,
   after my frustrating experience of loosing more than 2 hours of work yesterday evening (in answering various questions/comments using directly the reply system of your weblog), I prefer to wait up to your return for a continuation. Today I have a replacement hep-th/0507038 on the server which addresses a quite interesting point about localization-entropy. I hope that I finally managed to make a clearer presentation of this in my view important point (which by the way also shows the structural power of algebraic methods).
   Have I nice trip and above all nice weather
   Bert

9. **Alejandro Satz**
   March 23, 2006

   I have an idea! Many blogs have taken up the idea of "Opposite Day", in which you are supposed to defend, with the best possible arguments and no parody, a view completely opposed to yours. (An example is here.) So why not have, after Peter comes back from his holiday, an Opposite Day in which Peter defends string theory and Lubos attacks it? *That* would be interesting to read!

   Good luck in your trip!

10. **anon**
    March 23, 2006

    '…perhaps testing GR by measuring the deflection of starlight during the eclipse…'

    Eddington in 1919 didn't avert the contraction of the telescopes due to cooling, and this distorted his data: 'During a total solar eclipse, day becomes night for a few precious minutes. The temperature drops, birds stop singing, and bees return to their hives for a premature rest.'

Hope you can capture some of the atmosphere of this event, perhaps not the sounds of birds and bees, but at least the effects of the lighting on the desert landscape?

11. **Alejandro Rivero**
March 23, 2006

Have a nice travel. I guess you could find some internet shops around; if you check how full of inmmigrants are the ones in Europa, you can guess there is a way to get the connection at the other side in Africa and SouthAmerica.

12. **woit**
March 23, 2006

MathPhys,
Getting to Niger via charter flight from Paris to Agadez. Will have to spend a day or two in Paris on either end of the trip. Life is rough.

Bert,
Sorry you're having trouble with the comment system here. If you're writing a long comment, best probably to write it in some other software and cut and paste.

13. **Dick Thompson**
March 23, 2006

Peter: Envy – envy – ennnnnnnvy!

Have a great time!

14. **Doug**
March 23, 2006

Peter,

Please don't think that you need to add visual appeal here. No pictures can match the intellectual appeal of your blog. I am so glad that you stick to the topic of physics and haven't turned "Not Even Wrong" into a travelogue. I get so tired of having to sort through cultural posts in hopes of finding a nugget of science. That's why I always come to your blog first. It's a winner.

15. **svelte**
March 23, 2006

Peter, I'm just curious, why Niger ? There are quite a few other countries in West Africa that are slightly richer and slightly safer than Niger.

16. **Arun**
March 23, 2006

Maybe the change in scenery will give some inspiration w.r.t. to the landscape 🙂

17. **woit**
March 23, 2006

Doug,
Don't worry, no intention of turning this into a travelogue, I kind of agree with you. This posting and maybe one after I get back will be the full extent of it.

Svelte,
Several reasons for Niger
1. Maximum length of eclipse is in the desert near the Niger-Libya border
2. Best chance of not getting clouded out is in the desert. Clouds are unlikely, although there are sandstorms.
3. I like deserts in general, also quite looking forward to getting the chance to see some of Niger.

The eclipse track across the Sahara goes through Niger, Chad and Libya. I doubt many people are going to Chad, partly because of the safety issues. I was considering going to Libya, and many people will be there, but the Libyan government has been causing difficulties with visas.

18. **Alejandro Rivero**
March 23, 2006

It will be hard to get from Niger airport to the Libyan border, will it be?

19. **woit**
March 23, 2006

Not going all the way to the Libyan border, but still, quite a few hundred miles from Agadez. Will mostly be camping and travelling by 4 wheel-drive, perhaps some camels will even be involved….

20. **MathPhys**
March 23, 2006

Peter,

From what your description, your trip sounds like the real "getting away from it all".

Have a safe trip.

21. **MathPhys**
March 23, 2006

PS The Libyan/Niger border region is populated by the Tawareque tribes. Must be very interesting.

Back

April 4, 2006
Categories: Uncategorized

Had an amazing trip, which involved several days of travel through hundreds of miles of spectacular sand dunes in four-wheel drive vehicles operated by impressive Tuareg drivers (no camels). This put me and the group I was traveling with somewhere in between Dirkou and Bilma, about at the center of the shadow seen in this picture:

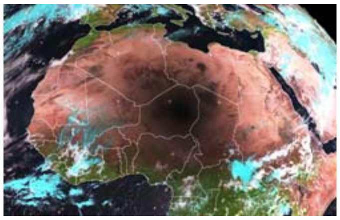

At some point I may post some links to other pictures. In a day or so after I deal with a couple hundred e-mails we'll return to your regularly scheduled programming.

Update: Fred Bruenjes was at the same eclipse camp in Niger, and has a report on the eclipse here. He also tells about traveling to the site, taking a more leisurely route than the one taken by the group I was with.

Comments

1. **Dr A Lurker**
   April 4, 2006

   Welcome back!

2. **MathPhys**
   April 5, 2006

   Welcome back, Peter. In what way were the Tuareg drivers impressive? Just curious.

3. **Dr B Lurker**
   April 5, 2006

   welcome back.

4. **hongbao zhang**
   April 5, 2006

   Welcome back.:)

5. **secret milkshake**
   April 5, 2006

   any of them drivers wearing blue mani-folds?

6. **Tommaso Dorigo**
   April 5, 2006

   Hi Peter,

   I am green with envy. As an amateur astronomer who has never experienced a total eclipse in his life, I ache a lot by knowing I could have been there myself… And wasn't.

   I hope you can post a few picture.
   Welcome back!
   T.

7. **Ben**
   April 5, 2006

   My friend snapped a sweet picture in the country of Georgia of the eclipse…

8. **Alejandro Rivero**
   April 5, 2006

   ¿Already back from Africa? This modern world spins really fast.

9. **Mindfists**
   April 5, 2006

   Sounds like fun…
   Welcome back.

10. **A. Torok**
    April 5, 2006

welcome back!

"It is not even wrong." – Wolfgang Pauli

11. **andy s.**
April 5, 2006

    Wow. What a picture.

    You can appreciate why ancient people were scared to death of eclipses.

    Did you remember to sacrifice a few virgins to appease the sky gods?

12. **D R Lunsford**
April 5, 2006

    Did you see shadow bands? Strangest phenomenon I've ever seen…

    -drl

13. **Dumb Biologist**
April 5, 2006

    Way cool, those prominences and sunspots…envy!

14. **Thomas Love**
April 5, 2006

    Glad to have you back, I missed reading your postings. While you were gone, I had an insight: M-Theory really stands for Motl-Theory which is why he defends M-T.

15. **Dick Thompson**
April 5, 2006

    A very welcome back Peter. I too missed your postings. What do you think of 't Hooft's latest on his attempt to ovrthrow quantum weirdness?

16. **Who**
April 5, 2006

    Sounds like a stunning and memorable vacation. I can only echo what the others have said: very glad to have you back.

17. **Arun**
April 6, 2006

    Which ancient people were scared of solar eclipses?

18. **MathPhys**
April 6, 2006

    According to Tin Tin, the Maya were.

19. **Who**
April 6, 2006

    Tin Tin dixit
    the authority in all matters

20. **Shantanu**
April 6, 2006

    Peter , welcome back. Have you seen this
    What kind of impact this will have on high energy physics in the future?

The Templeton Foundation: A Skeptic's Take

April 6, 2006
Categories: Uncategorized

Science writer John Horgan has just written a piece about the Templeton Foundation that is causing a bit of a ruckus. It first appeared in The Chronicle of Higher Education, and is also posted a the Edge web-site, where perhaps some further discussion of it will appear.

Horgan participated in a program held at Cambridge as a Templeton-Cambridge Journalism Fellow in Science and Religion, an all-expenses paid gig that came with an additional $15,000 that made it hard to turn down. He had very mixed feelings about the experience, and explains these in detail.

The financial scale on which Templeton operates is unparalleled in this area. As in Horgan's case, the people they invite to participate in their programs are often offered a lot more money than usual for this kind of thing. The foundation has an endowment of $1.1 billion, and is funding more than 300 projects at the rate of $60 million/year, a rate they intend to double. By comparison, the total NSF budget for supporting theoretical physics is also about $60 million/year. The sheer number and diversity of organizations using Templeton money to promote bringing science and religion together is staggering. I keep finding new ones at various places around the web, and also have yet to run into any organization trying to bring religion into science that isn't getting Templeton funding.

One new Templeton-funded project is called Foundational Questions in Physics and Cosmology, and has a very illustrious advisory board of physicists. It has just finished accepting proposals for a first round of grants to total $2 million, and has received a 172 proposals, totalling $23 million, from top institutions including Caltech, Harvard, MIT, Princeton and The Institute for Advanced Studies, Stanford, UC Berkeley, Oxford, and Cambridge. Sean Carroll (who turned down Templeton money since he disagrees with what they are trying to promote) has a posting about this, including a guest blog entry and discussion with Anthony Aguirre, who is one of the physicists running the project.

The ethical questions involved in the question of whether to accept money from a source one is not completely happy with are not at all straight-forward. One can sensibly argue that there is nothing wrong with taking money from someone whose goals one disagrees with, as long as they let you do what you want with it, and one isn't forced to further such goals. On the other hand, publicly associating oneself with an institution to some extent lends ones credibility and prestige to the institution and inherently furthers their goals. It's also true that money talks, and a large amount of money talks loudly. Many scientists in recent years have probably ended up doing one thing or another that they wouldn't otherwise have bothered to get involved in because Templeton money made it rather attractive.

There seem to me to be several different things about Templeton to be wary of. One is that the foundation's leader, Sir John Templeton, is in the process of turning over control of the organization to his son, John Jr., who has a much more politically right-wing, evangelical Christian, point of view than his father. Even if one has no problems with what the foundation has done in the past (e.g., it has not supported creationism), this doesn't mean it won't change what it does in the future.

I personally happen to think that bringing religion into physics is inherently a bad idea. Whatever one's view of religion is, it is inherently a quite different thing than science, and at a time when standards of what is science and what isn't are under attack, a blurring of the distinction between science and religion may be very dangerous. Much of what Templeton supports seems to me rather silly, but not much of a threat to anything important. For example they are funding a project in Vienna that will bring together physicists, philosophers and theologians to study the foundations of quantum physics. I don't believe the theologians will be much help here, but they're not likely to cause much harm. On the other hand, the large amount of Templeton funding promoting symposia devoted mostly to pseudo-science like this one on the Multiverse and String Theory is much more worrying.

Comments

1. **JC**
   April 6, 2006

   Any idea as to whether Mr. John Templeton Jr. is actually a true hardcore religious type, or whether the religion thing is just a convenient political "cover" for him? If it's the latter, he may very well be harmless for the most part.

   For many of the "neo-cons", I get the impression a lot of them use religion largely as a convenient political "cover" for pandering to the religious right voters.

2. **Bill**
   April 6, 2006

   Who are the most prominent religious scientists/mathematicians? Do Witten/Dijgraaf/Ashtekar/Atiyah/Maldacena/Hawking/Serre/Wiles/Tao/Mazur/etc believe in any form of god? How many are atheist? The gossip would be interesting…

3. **dan**
   April 6, 2006

   i agree with bill — i wonder if studying high-level physics (i.e particle physics, quantum gravity, etc.) begets a belief in some sort of deity (ala deism perhaps?)

4. **Lurker**
   April 7, 2006

   Off topic, but, Peter, why don't you have a prominent link to where your new book can be bought?

5. **Fabien Besnard**
   April 7, 2006

   It has been pointed out before by several people
   here including me that Templeton do support ID, if not openly.

6. **csrster**
   April 7, 2006

   The Horgan article is worth reading for the Weinberg quote alone 🙂

7. **anon**
   April 7, 2006

   Peter,

The Templeton Foundation does do some good work. They publish Professor Russell Stannard's book, "Science and the Renewal of Belief", http://www.templetonpress.org/book.asp?book_id=74

"Originally published in Great Britain and now updated and available for the first time in a U.S. edition, this book is a critically acclaimed work by a renowned theologian-scientist.

"Russell Stannard is known for cutting through highly technical data and presenting it clearly and simply. In Science and the Renewal of Belief he sheds light on ways in which science and religion influence each other and can help each other. Science and logic cannot establish belief, he says, but belief can be con-firmed and renewed with the changed perspective of modern science.

"The many reviews of the UK edition of his book cite his lucid presentation of relativity and quantum theory, and the way he uses relativity to explore time and eternity, and indeterminacy to comment on free will. He is also praised for offering fresh insight into original sin, the trials experienced by Galileo, the problem of pain, the possibility of miracles, the evidence for the resurrection, the credibility of incarnation, and the power of steadfast prayer. By introducing simple analogies, Stannard clears up misunderstandings that have muddied the connections between science and religion, and suggests contributions that the pursuit of physical science can make to theology."

Stannard was professor of physics at the Open University a decade ago (he is now emeritus). He was too busy then with his research into the connections of physics and miracles to reply to my letter as a student asking about quantum gravity, but he kindly passed it to a colleague who wrote to me that I need to study string theory (I believe the suggestion was to see if it can be reconciled with prayer). Unfortunately, I failed. (Others claim to have succeeded.)

8. **Chris Oakley**
   April 7, 2006

   Superstring theory has proved that most theoretical high-energy physicists, given a free choice, will work on speculative, quasi-religious nonsense.

   Since the Templeton Foundation sponsors those who investigate the connection between science and religion, it would appear that Superstring theory and the Templeton Foundation are a marriage made in heaven.

   So here is a proposal for a research project: calculate the extent of the 6 or 7 compact dimensions in heaven.

9. **Ben**
   April 7, 2006

   Speaking as a religious person with an interest in (though sadly a lack of deep understanding of) physics, it's always been a bit mystifying for me to understand why the Templeton foundation is so in love with string theory and the multiverse. On a purely aesthetic level, I find the idea that God created a final theory which dictates the physical parameters of our universe much more appealing than the idea of multiple universes spewed out at random. At any rate, I'll certainly agree with Peter and Chris that high energy physics is metaphysical enough these days without adding Templeton's money to the problem.

10. **anon**
    April 7, 2006

    Dear Chris,

    String theory is at a much higher level than you imagine. One Harvard M-theory expert (an assistant professor) said:

    "String theory is the language in which God wrote the universe."

    If you disagree with this statement, disprove it! (And keep your discussion mathematical with physical content.) 😉

11. **Tony Smith**
    April 7, 2006

    Anon said (quoting Lubos):
    "… "String theory is the language in which God wrote the universe."
    and then said:
    "… If you disagree with this statement, disprove it! (And keep your discussion mathematical with physical content.) …".

    Since the statement itself is not "mathematical"
    and
    since it has no "physical content" of calculations of observable quantities (as Feynman said, "The whole purpose of physics is to find a number, with decimal points, etc! Otherwise you haven't done anything."),
    it is a very good example of the unfortunate level of what passes for thought in today's physics establishment.

    Tony Smith
    http://www.valdostamuseum.org/hamsmith/

    PS – Chris said "that Superstring theory and the Templeton Foundation are a marriage made in heaven". Although they are remarkably compatible (with Lenny's Loony Landscape putting huge areas of physics forever beyond human comprehension, and thus creating a huge space for a G-d of the Gaps), I see the "marriage" as being made in Hell by Satan in order to put Humanity into a Dark Age.

    It is amazing to me that a program (fqxi) to grant $2 million for "… unconventional … physics … research that, because of its speculative, non-mainstream, or high-risk nature, would otherwise go unperformed due to lack of available monies …"
    attracted
    "… 172 proposals, totalling $23 million, from top institutions including Caltech, Harvard, MIT, Princeton and The Institute for Advanced Studies, Stanford, UC Berkeley, Oxford, and Cambridge …".
    It seems as though the Dark Age is already here, and that Satan is giving thanks to Templeton for helping him win this round in his fight with G-d.

12. **ObsessiveMathsFreak**
    April 7, 2006

    Bringing religion into physics is like bringing alchemy into chemistry. Imagine if a modern chemist took a foundation's money and began giving talks and writing papers about the four humors and transmutation of elements in the context of modern chemistry. Would you take money from the same foundation that continued to fund this?

13. **Dumb Biologist**
April 7, 2006

Well, their study of the post-CABG impact of anonymous prayer on recovery was a bust by any measure, so maybe they'll be hoisted by their own petard.

14. **anon**
April 7, 2006

Prayers failed to contribute to the recovery of heart surgery patients in the Templeton study because the universe is 96% dark energy and dark matter.

What do you think happens when the universe is 96% dark stuff? Prayers can get attenuated by the factor exp(-x/y) where x is the amount of shielding dark energy/matter, and y is the relaxation length or 'mean free path' of a prayer through the dark stuff (y being the amount which is enough to attenuate prayers by the factor e).

So actually, the 'negative' result of the prayer study is actually best perceived more evidence for the current cosmological model. Sorry Peter, for going off topic a little bit. 🙂

15. **Alejandro Rivero**
April 7, 2006

ObsessiveMathsFreak, that is not accurate. Alchemy has never left chemistry. The basic principle in Alchemy is not around humours (that is medicin) but ying/yang, call it Mercuric/Sulphurous, Acid/Base, Reduction/Oxidation…

16. **Alejandro Rivero**
April 7, 2006

Yes, a funny quote: "I am all in favor of a dialogue between science and religion, but not a constructive dialogue."

Religion is not mostly harmless. On one hand, the belief in an extranatural explanation does not seem compatible with the search of explanation. Religious people can do good observational science, reading Nature as the book of God, but weak theory. If this is bad at individual levels, it is worse when it is amplifyed at politic (which has intervering agendas of its own) and institutional levels. In Spain a good example of such problematic was the rebuilding of Ramon y Cajal's "Junta de Ampliacion de Estudios" as a post-war "Centro Superior de Investigaciones Cientificas" under the control of Opus Dei.

Also, it is not only about guiding and censoring; we have lost important individual people defecting from science to religion. From the past, think about Pascal, hammer of Aristotle, getting that mistical experience and abandoning all the scientific research. Or Isaac Barrow, dead on oppium after leaving math for a mix of religion and academic politics. Perhaps today Money or Politics are more powerful sources of defection, but all of them have mutual synergy, as Hogan's comment shows.

17. **Michael**
April 7, 2006

Science and Religion has been mixing for along time. That is not what worries me. What worries me is irresponisble scientists and /or quasi-scientists writing about questionable science to prove the existance of a god or to prove ID (I am not implying any person or group in this statement). Most scientists and informed public can tell the difference between pseudo-science and real science. However, the general public is relatively uninformed and would rather be spoon fed. This is where pseudo-science does its real damage. I think that this is where the Templeton Foundation can do a significant amount of damage.

18. **Dick Thompson**
April 7, 2006

This may be a new problem for physics, at least in recent times, but it is an old, old one for intellectuals generally, hence the ancient saying "He who would sup with the devil must use a long spoon."

Argumant along the line of "X has done some good work" are also a familiar part of the problem. When I was a kid (during WWII), there was general derision of a saying that had been common a decade earlier; "Mussolini made the trains run on time."

And so is the "The father is moderate but the son is radical". "If the Tsar only knew what his cossacks were doing in this pogrom, surely he would stop it."

As my quotations suggest, I think that history wil not look kindly of the physicists who have sold their birthright for a fat endowment.

19. **steve**
April 7, 2006

Alejandro's comment about religion and theory is way off the beam. Kepler's theorizing was entirely fired by his belief that he was understanding the mind of God (he had a neo-Platonist view of the role of math in the universe). Newton was a pretty religious guy (and also seems to have been interested in alchemy). Galileo seems to have been pretty devout, but more in a modern "non-overlapping magisteria" way, a la Gould, than the straightforward linkage of Kepler and Newton. All of these were mathematical modelers and theorists, in fact the very first in physics.

It should also be pointed out that one strand in the history of science (Houkyas) argues that it was precisely the theological beliefs of the English scientists–specifically their "voluntaristic" doctrine that God could have designed things any way he wanted–that led to empiricism in the first place. Cartesians and other Continentals, in this view, tended to think that God should be constrained by logical principles intelligible to mankind, and so tried to deduce more and experiment less. I don't know the current scholarly state of play on this idea, but it was still being taught to undergraduates at Princeton in twenty-five years ago.

20. **Thomas Love**
April 7, 2006

Theory and Theology have the same root: Theos=God. But why?

21. **The Anti-Lubos**
April 7, 2006

Thomas Love said: *Theory and Theology have the same root: Theos=God.*

Actually, they don't. *Theory* comes from a Greek root meaning "a viewing" or "a contemplation."

22. **Alejandro Rivero**
April 8, 2006

It is amusing to label as NeoPlatonist to the man that put perfect circles off, I wonder from where has this idea come, surely some historian looking at his gravure with the platonic solids (having just five planets, the radiouses of these solids were a logic place to research for a parameter-free theory; it is unrelated to Plato). It is true that the question of God was very live topic in the Baroque age, but note that neither Kepler not Galileo nor Newton can be qualified as orthodox theologicians; surely the inquisicion had found solid points to attack Kepler and Newton had they being at the reach of Catholicism. Note also the extensive printing and reprinting of Lucretius and other propaganda of Epicureism, where gods, even if acepted, were unrelated to the deep workings of Nature. To say that a scientist is trying to understand God if only valid in a sense: he is trying to understand His inexistence.

23. **Benjamin**
April 8, 2006

Peter, this may seem trivial, but I wish you had a date near the top of the page one gets to when one clicks on the comments, just as one has on the main blog page. I sometimes like to save a good posting, including the comments, and when I click on it I like to see the date right away at the top.

As for science and religion, I believe in certain 'metaphysical' principles, but I agree that they should not be brought into science. Feynman is right when he says it is about predicting numbers. However, one should distinguish between harmless 'metaphysical' feelings like Einstein's belief that 'God does not play dice', which can be considered 'motivational' (even if it made Einstein more or less waste his later years), and a subversive theological agenda. For the latter, read Christianity. I am not here to bash Christianity, but there is a noticeable tendency for them to think of their religion as the best or truest, as do the Muslims. The Muslims, at least, are pretty outspoken, but Christians tend to be more subtle, and any sneaky attempt to inject any particular dogma masquerading as science is especially noxious.

24. **Benjamin**
April 8, 2006

Here's a better thought: Mathematics itself can take on some of the pernicious attributes of religious speculation. That's what people here seem to see in string theory. Wasn't Einstein at his greatest in his youth when he was thinking empirically and 'operationally'? Then, when he learned Riemannian geometry and developed a greater appreciation (perhaps awe) for mathematics, he seems to have developed a belief that simply tinkering with the math would produce the truth. Isn't that what the anti-symmetric tensor business was about, which Weinberg said amounted to nothing? (I'm no expert.) Anyhow, mathematics can be pretty seductive! It can acquire some 'metaphysical' or 'theological' characteristics. So maybe religion isn't the only danger to physics.

25. **island**
April 8, 2006

Religion is necessary to science as long as science rejects out of hand interpretations of evidence by scientists which indicate that there is purpose in nature, due to their own personal pre-conceived prejudiced and misconceptions about what's actually known.

Proof:

God does not play dice', which can be considered 'motivational' (even if it made Einstein more or less waste his later years)

Apparently this person has no idea that Einstein's opinion falls from the most natural extension of GR with a cosmological constant as it applies to the observed universe… not from some quasi-religious "feelings" about it.

So hidden variables is a categorically determined matter and you have the quantum gravity and the ToE in your back pocket, right?…

Maybe you can provide some logical proof for infinity while you're at it, so that we can finally know for sure that Einstein was wasting his life believing that *"such a complete resignation in this fundamental question is for me a difficult thing. I should not make up my mind to it until every effort to make headway toward a satisfactory view had proved to be in vain"*.

Taking money from Templeton is necessary and justified as long as creationists are the only ones that carry the torch of purpose in nature.

Don't count Einstein out yet… as nothing has been "uncertainly settled" yet.

26. **anon**
April 8, 2006

'Einstein … rejected the theory not because he … was too conservative to adapt himself to new and unconventional modes of thought, but on the contrary, because the new theory was in his view too conservative to cope with the newly discovered empirical data.'

– Max Jammer, 'Einstein and Quantum Physics,' in 'Albert Einstein: Historical and Cultural Perspectives', edited by Gerald Holton and Yedhuda Elkana, 1979.

27. **island**
April 8, 2006

Thanks, I'll bear that in mind the next time that I think about how Einstein felt about quantum weirdness…

I was indirectly quoting from his 1917 paper, when he attempted to defend his theory against arguments for setting boundary conditions at infinity, because it severely weakens the predictive capabilities of GR by requiring that it be augmented by other principles or a bunch of arbitrary information in order to yield determinate results.

For this same reason, in, "Gravitation", Misner, Thorne, and Wheeler said that GR *"demands closure of the geometry in space as a boundary condition on the initial-value equations if they are to yield a well-determined and unique 4-geometry"*.

There is no uncertainty in this model, period.

28. **woit**
April 8, 2006

Please, this is a posting about the Templeton Foundation and the funding issues it raises. Try and avoid the seemingly irresistible urge to go on about your favorite ideas about cosmology or GR, that's completely off-topic here.

29. **island**
April 8, 2006

Sorry, I agree, but I also felt compelled to defend that my example was correct in context, in order to illustrate why I believe that there are circumstances where it is… *"ethical […] to accept money from a source one is not completely happy with are not at all straight-forward'*.

Maybe I could have done it without the example:

Taking money from Templeton is necessary and justified as long as creationists are the only ones that carry the torch of purpose in nature.

… when science requires that respectable scientists, like John Barrow, cross the line in order to do real science that would otherwise gain little or no support from the mainstream, so in this context, at least, it *can* be very necessary to science even if you don't like the false-idol that forces the issue back to the surface until it is unequivocally decided.

That sounds a lot crazier without the good reason for it tho!… 😉

30. **knotted string**
April 11, 2006

If Templeton were backing heretics who couldn't get funding anywhere, instead of giving million dollar awards to already known and powerful people like Paul Davies, John Barrow, etc., then I'd have sympathy.

They're not interested in helping obscure back-room patent clerk types. They are out to do something political by bribing big names.

31. **Chris Oakley**
April 11, 2006

I must admit that I don't think that I would be so proud as to turn down Templeton's money if it was offered. But one of the many reasons why it would not be on offer is this: *they have an agenda*. Unlike the Bambergers (who founded the the IAS in Princeton) this is not a disinterested attempt to further scholarship. It is an attempt to bring people around to their way of thinking. With more than a billion dollars in the pot they can afford to be patient, but the plan is clearly to "buy" as many eminent scientists and other thinkers as they can in hopes that these people will repeat their message. Although it may be done in the nicest possible way, it still leaves a bad taste in the mouth.

32. **Tony Smith**
April 11, 2006

knotted string said "… Templeton …[is not]… backing heretics who couldn't get funding anywhere … They're not interested in helping obscure back-room patent clerk types. They are out to do something political by bribing big names. …".

Chris Oakley said "… I must admit that I don't think that I would be so proud as to turn down Templeton's money if it was offered. …".

I agree with both statements. I am also not so proud as to turn down Templeton's help. In fact, back in November 2005 I sent an e-mail to hubbard at fqxi.org saying in part:

"… I saw … at … a Templeton statement at http://www.templeton.org/topics_in_the_news/Official_Statement.asp … the following: '… the Templeton Foundation refuses in its programs to blacklist scholars based on their ideological positions. … Blacklisting is ethically inappropriate in academic contexts. … Research scholarship does not proceed by processes of censorship and inhibition of debate. …'.
I am, and have been since around 2002, blacklisted from posting on the Cornell arXiv … I individually do not now need grants or money, and this message is NOT a request for money. However, I do need relief from the oppression of blacklisting, such as opportunities to present my ideas to physicists for discussion and criticism, and also some place where my work could be securely archived for the future. … If you think that it would be useful for me to meet with you or any other representative of Templeton to discuss such matters, I would be happy to do so at a mutually convenient time and place. …".

I have (as of now, several months later) not received any reply whatsoever.

Since I was not even asking for money (just for an invitation to talk about my ideas),
it seems to me that Templeton is indeed not interested in helping heretics, but is in fact interested in furthering a political agenda by buying influential people in influential organizations , as evidenced by the fact that their $2 million grant program attracted (according to Peter) "… 172 proposals, totalling $23 million, from top institutions including Caltech, Harvard, MIT, Princeton and The Institute for Advanced Studies, Stanford, UC Berkeley, Oxford, and Cambridge …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Peter, I want to make it clear that I do NOT want this comment to lead to discussion about my physics model or about me being blacklisted. I only mention my situation because (since I am an obscure heretic) it seems that Templeton's totally ignoring my request for help is a concrete example that shows their high-minded statements such as "… Blacklisting is ethically inappropriate in academic contexts. …" to be merely bullshit window dressing to cover their real agenda, which is (as knotted string said) to "… do something political by bribing big names …".

33. **Alejandro Rivero**
April 12, 2006

how do you say in USA: "put your money where you mouth is"? Tony attemps clarifyes that words and actions do not agree.

I have been thinking on the questions raised in this thread, and I think we should at least agree on an statement:

"that gods, as sentient entities, are allowed to exist and live freely as far as they understand that Their freedom is restricted by the freedom of other sentient beings, and Their rights restricted to the rights of other sentient beings, particularly by those of the human genre".

Is this enough to qualify for Templeton money?

34. **Anthony Aguirre**
April 14, 2006

Tony Smith,

I am sorry that you did not receive a helpful reply from Kirsten Hubbard, our scientific program manager. This may have been, in large part, because FQXi is NOT the Templeton foundation, nor is she a spokesperson for Templeton. FQXi is an independent and independently-run organization with seed funding from the Templeton Foundation (and hopefully soon other donors).

In terms of who is funded by FQXi, note that the quoted web announcement referred to applications received, not applications funded. But in any event FQXi will seek to fund the best foundational, unconventional research in physics and cosmology that it can, whether it is by a patent-office clerk or a Harvard professor. The former is, of course, the exception more than the rule, albeit an important one.

best,

Anthony A.

(FQXi Associate Director)

35. **knotted string**
April 15, 2006

If Tony Smith is too exceptional for your taste, then try funding the Harvard assistant professor who said ‘String theory is the language in which God wrote the universe’. But beware he might spend his time blogging about climatic change being a fraud! 😉

36. **John Harding**
April 23, 2006

I predict that life will continue to reproduce itself at least for the near future because of the anthropic principle. But what do I know? I’m nobody.

Eurostrings 2006

April 8, 2006
Categories: Uncategorized

This past week there has been a conference going on in Cambridge called Eurostrings 2006. It's a bit like the annual "Strings XXXX" conferences, although about half the size and organized just by European institutions that are part of the European Superstring Theory Network, which since last year has been funded by a grant from the EU. Part of the conference consists of a celebration of Michael Green's 60th birthday.

Most of the talks are already available online. There's not much new being reported, but the talks include a nice review talk on topological strings by Robbert Dijkgraaf. There's another talk on recent work on topological strings by Erik Verlinde, and a week earlier there was a conference in Munich devoted to the subject.

Nathan Berkovits talked about work in progress with Nikita Nekrasov on multi-loop amplitudes using his pure spinor formalism. This subject still seems remarkably confused, with Berkovits explaining that they have found a problem they still don't know how to resolve: their regularization causes amplitudes with genus larger than 6 to vanish, violating unitarity. For commentary on yet another new suggested formalism for defining superstring amplitudes due to Warren Siegel and Kiyoung Lee, see this posting by Lubos Motl.

As part of the Green birthday celebration, John Schwarz gave a talk on String Theory Books. He reminisced about the writing of the two-volume book with Green and Witten (his outline refers to a "removed chapter", and "broken vow", what are those?). Evidently the book was written in 9 months back in 1986, a truly heroic effort given its size. Last Monday, Schwarz finally finished up a new textbook on string theory, written with Katrin and Melanie Becker and entitled "String Theory and M-theory: A Modern Introduction". They started writing back in February 2005, planning a 350 page book to be completed by the end of September, but ended up just last week completing a 729 page book. From the table of contents it looks like GSW abbreviated and updated, containg more modern material on branes, dualities, black hole entropy, flux compactifications and gauge/string dualities. It seems rather peculiar that flux compactifications and the landscape get a whole chapter, whereas AdS/CFT is dealt with in one section of one chapter. All in all, comparing the new book to the old one, twenty years later the subject has become a lot more complicated, a lot uglier, and the prospects for using it to predict anything about the standard model have vanished.

The Schwarz talk also has a link to a video of a Berlitz commercial featuring a German radio operator misunderstanding someone radioing in a distress call that they were sinking (the German hears this as "thinking"). I can't at all figure out what this has to do with Schwarz's talk. Is string theory making a distress call as it is sinking, but no one understands this?

Victor Rivelles attended, and has blog entries here, here and here. He reports on a talk by my fellow Princeton student Costas Bachas about using string theory techniques to solve capillarity and wetting problems, then comments that "If LHC provides no proof to string theory string theorists will not lose their job, they can just change to applied string theory!"

Comments

1. **Chris Oakley**
   April 8, 2006

   Applied string theory? What does that mean? The only thing I can think of that fits this description is working for a postal/parcel delivery service. Someone should do a study to see if the postal workers who have "gone postal" in the past (wasting many fellow human beings, and most often themselves in the process) were in actual fact just frustrated Superstring theorists who were unable to cope with the absence of any extra dimensions.

   If only I had know that these guys were staying right opposite the Cambridge University Library, where I often go to work. I could have sauntered along to the college bar afterwards to try to talk them out of making this career move, possibly saving many innocent lives in the process.

2. **Algernon Llewellyn-Twittleton**
   April 8, 2006

   The talks were all videoed and may (?) appear online.

   The `removed chapter' was apparently one on light-cone gauge string field theory that never made it in to GSW. The broken vow was `never again!' – the new book with Beckerx2 breaking this.

   Despite your perhaps overly fond hopes Peter, John Schwarz's talk did not anticipate the demise of string theory. The commercial you found puzzling was used to illustrate a point about ensuring the text of his new book was not like German gewritten.

   vive les cordes

3. **woit**
   April 8, 2006

   Thanks Algernon!

   I did think it was probably too much to hope for that Schwarz was implying that string theory was sinking. For one thing, he probably wouldn't have spent the last year writing a book on the subject…

4. **Luboš Motl**
   April 8, 2006

   The commercial "What are you sinking about?" was mentioned by John Schwarz as an example of sometimes difficult verbal communication between native English speakers and non-native English speakers. Such trivial difficulties can sometimes influence even teams that write books, despite the high intelligence of all the authors.

   But it's still a courtesy from the German speakers to speak English. John Schwarz has a German enough sounding name so that he could also learn German. 😉 In fact, Becker sisters' grandfather was kind of Czech and his name was Jan (=John) Schwarz.

5. **Chris Oakley**
   April 8, 2006

   I don't see any Llewellyn-Twittletons on the list of participants, and yet Algernon here seems pretty well informed about this conference. Could it be that he is posting

under a pseudonym? If so, why? Is contributing to this blog a CLM* for String Theorists? But I notice that he stops short of saying "Long Live Strings" in English, even though I am sure that he must be aware that this the usual language of this forum.
—-
CLM = Career-limiting Move. Another example of an academic CLM is calling your PhD supervisor an "idiot" or "imbecile" before he/she has written your references.

6. **Tony Smith**
April 8, 2006

Peter said: "… John Schwarz … reminisced about the writing of the two-volume book with Green and Witten … Evidently the book was written in 9 months back in 1986, a truly heroic effort given its size.
Last Monday, Schwarz finally finished up a new textbook on string theory, written with Katrin and Melanie Becker and entitled "String Theory and M-theory: A Modern Introduction". … a 729 page book. From the table of contents it looks like GSW abbreviated and updated … All in all, comparing the new book to the old one, twenty years later the subject has become a lot more complicated, a lot uglier …".

The haste with which 1986 GSW was written is evident.
For example:
Volume 1 contains Appendix 5.A "Properties of SO(2n) Groups" that discusses interesting things like SO(8) triality and the spinor representations of SO(2n),
while
Volume 2 (at page 388) says "… It is perhaps surprising* that one can define a second set of gamma matrices …"
with the * indicating a footnote saying
"To the reader who has read the construction of the spinor representation of SO(2n) in Appendix 5.A, this may not seem so surprising.".

When I first read GSW, I found it to be very unevenly written,
with very clear beautiful sections on triality, SO(2n) spinors, the Elements of E8 (Appendix 6.A), etc,
and
with some less-mathematical sections seeming to me to be muddy and obtuse.

At a conference, I had the opportunity to ask Ed Witten why GSW was so unevenly written, and even with some sections apparently written by someone who was unaware of preceding sections.
He said that they (G,S,W) had divided the book into chapters and then each of the three authors had written their assigned chapters independently.

Guess who wrote the clear beautiful math stuff in GSW ? (Hint – his initials are EW).

Therefore, I am nor surprised when Peter says that Schwarz's rewrite/update (without EW as coauthor) seems "a lot more complicated … a lot uglier".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **woit**
April 9, 2006

Tony,

Witten has an incredible command of a lot of mathematics, and he is a wonderful expoeitor, so I'm sure his co-authors had trouble competing with that. But the additional levels of complexity and ugliness I was referring to are inherent to the subject, not due to the authors. All I've seen anyway is just the table of contents, and I was referring not to the exposition but to the properties of the topics chosen.

8. **Robert**
April 10, 2006

JHS's also explained that the G, S and W individually drafted the chapters (and it is indeed quite obvious who wrote what) and only later send them around (using the by then still novel computer network joining Princeton, QMW and Caltech) for the others to edit. He also mentioned that they each worked like 100 hours/week to finish those books but W still managed to work out cubic string field theory simultaneously.

The "what are you thinking about" spot making fun of us Germans struggling with the English language became a running gag for the rest of the conference, not only Paul Townsent refered to it mentioning Michael Gutperle's pronounciation of "Janus".

So, remains one question: How many Germans does it take to change a light bulb?

Vee ask heer se kwestions!

9. **woit**
April 10, 2006

Robert,

"Townsent"? Can't see why anyone is making fun of you Germans with your accents….

10. **Alejandro Rivero**
April 10, 2006

There is some distintion between "sound" and "phoneme". Basically if you understant it , it is still the same phoneme, just different accent or sound. On the other hand, I believe that an insult against scottish is "Yours is not an accent, it is just a different language that happens to have the same written form".

11. **Robert**
April 10, 2006

Ahhhhh. Gotcha! At least I manage to call him Paul, not like the secretary at Strings '99 who even produced a name tag with "Pete".

12. **Bert Schroer**
April 10, 2006

Here is another one
question: Prof. Wigner do you spell your name with vee?

Wigner: 9W

13. **Jenny Attiyeh**
April 11, 2006

Hi Peter et al –
Just wanted to let you know that my ThoughtCast interview with Lisa Randall is finally finished, and online in podcast form at http://www.thoughtcast.org. Thank you all for your input on potential questions for the interview. Feel free to leave a comment on the ThoughtCast website. And please remember that the intended audience for this interview is curious and intelligent, but mainsteam — far less informed than you guys. I think you might nonetheless enjoy listening to her thoughts on string theory.
Thanks again!
Jenny

14. **knotted string**
April 12, 2006

"… Very soon now these [string theory] theories will be tested. …" – Jenny (introducing Lisa Randall).

"Is there any room in your theories for life on any extra dimensions or parallel universes?" – Jenny

Lisa Randall, laughing: "There absolutely is room … in particular if there are other branes … we really don't know what's there …"

🙂

Polchinski on the Landscape

April 9, 2006
Categories: Uncategorized

At the recent Solvay conference there was extensive discussion of the Landscape, and I've already discussed here Michael Douglas's write-up of his talk on the subject, entitled Understanding the Landscape. Now Joe Polchinksi's rapporteur talk on the subject has appeared; it's entitled The Cosmological Constant and the String Landscape. Polchinski is essentially making the same argument as Susskind makes in his recent book, and his argument has pretty much the same problems that Susskind's has, which I discussed in detail in an earlier posting. However, his article is written for physicists, not for the general public, so he is making a much more technical version of the argument. It's probably the best version of the case for the string theory anthropic landscape available, so worth reading carefully.

Polchinski begins by explaining why the cosmological constant problem is difficult. He divides possible solutions to it into ones where the CC is fixed, and ones where it is adjustable. The problem with the idea that the CC is fixed and calculable is essentially that all the contributions to the vacuum energy that we know about give values of the CC that are far too large. These include things like fermion loops and the Higgs potential in the standard model, supersymmetry breaking in supersymmetric extensions of the SM, and Planck scale effects in theories of quantum gravity. He goes on to explain why it is difficult to try and modify the theory of gravity so that it won't couple to these sources of vacuum energy.

There is a wide range of ideas about how to select a small CC in theories where the CC is adjustable, and Polchinski describes several of them and what problems they have. Some of these ideas naturally explain a small CC in an empty universe, but not in our matter-filled universe. Finally he ends up with the anthropic explanation: the CC is small because if it wasn't we wouldn't be here. He does note that:

Of course, the anthropic principle is in some sense a tautology: we must live where we can live.

this carries a footnote comparing the anthropic principle to Darwin's theory of natural selection:

Natural selection is a tautology in much the same sense: survivors survive. But in combination with a mechanism of populating a spectrum of universes or genotypes, these 'tautologies' acquire great power.

Susskind makes the same sort of claim that the anthropic landscape is much like the theory of evolution, and I think this is extremely dangerous and unwise. There is a mountain of scientific evidence for the theory of evolution and none for the anthropic landscape. It is very important that this distinction be made, and trying to blur the difference between these two very different situations is not something a scientist should be doing at a time when science in general and the theory of evolution in particular is under attack by the forces of the religious right. When I was in Niger one of my travel-mates was a creationist who was generally annoyed at what he saw as the arrogant way scientists dismiss his view of the world. We discussed cosmology, and I tried to tell him a bit about inflation and what aspects of the universe it was supposed to explain. In response he asked me why he shouldn't just go with his preferred explanation: it was all the doing of the "Big Kahuna", as he called the deity. I tried to explain about the scientific method: your theory is supposed to make distinctive predictions that you can go out and check by making observations. I think I had some success in getting this idea across, but I don't see any way I could have defended to him something like the anthropic landscape. It's not legitimate science since it makes no real predictions that you can use to see if the idea is right or not. What's at issue here is the credibility of science itself, and physicists who care about this credibility should not be claiming that an idea like the anthropic landscape has the same status as heavily tested and verified theories like that of evolution.

Polchinski goes on to repeat the analogy with evolution a bit later, and I actually don't understand at all what point he is making here, when he argues that maybe only the anthropic principle determines the CC:

Thus we should seriously consider the possibility that there is no other selection mechanism significantly constraining the cosmological constant. Equally we should not stop searching for such a further principle, but I think one must admit that the strongest reason for expecting to find it is not a scientific argument but a psychological one (footnote): we wish fundamental theory to be as we have long assumed it would be.

and the footnote is:

Again, the Darwinian analogy is notable.

Personally I'm agnostic about whether the CC is computable from first principles or not. As a scientist, one's job is to come up with theories and extract predictions from them to see if they are right. If you have a theory that says the CC is not computable, that's fine, but then you have to just forget about the CC and find something that your theory does predict so you can test it. You can't go around claiming that the fact that your theory is compatible with any value of the CC is somehow some sort of scientific success and evidence for the theory. The problem with a theory where all values of the CC are a priori equally likely is that it's vacuous (as far as the CC is concerned). Its implications are exactly the same as throwing up your hands and saying "I have absolutely no idea what determines the CC, it could be anything".

The fact that we are here and observe galaxies does put constraints on the CC, and Polchinski would like to make much of this. He'd like to claim that the anthropic landscape predicts that the CC is a random variable, and then, given the fact we see galaxies, it would have an expectation value about an order of magnitude higher than its observed value. He defends the theory against this mismatch by quoting Galileo's defense that his theory might be inaccurate but was a lot better than Aristotle's, then writes:

This order of magnitude may simply be a 1.5 sigma fluctuation, or it may reflect our current ignorance of the measure of the space of vacua

One problem with all this that Polchinski doesn't mention at this point is that the anthropic constraint is not on the CC but on a combination of the CC and Q, the normalization of the primordial temperature fluctuations. Assuming both are random variables, the observed CC is way off what one expects. One can deal with this by just assuming that the CC is a random variable, but Q isn't for some reason. This gets into the fundamental problem with the string theory anthropic landscape: it doesn't just not predict the CC, it doesn't predict anything at all. As far as one can tell, it's consistent with just about anything. It doesn't make any predictions, so it's really not a legitimate scientific theory at all. One can try and claim that it really is a scientific theory, and that it does predict something: we are at some randomly chosen (according to a not yet understood measure) point in the landscape compatible with our existence. The problem with this (as Polchinski notes) is that there is a long list of properties of the world that appear to be rather special and statistically highly disfavored by any likely measure: the theta-angle is very small, proton lifetime is very long, number of generations is small, etc., etc. If one actually took the string theory anthropic landscape seriously as a theory, one would have to abandon it in face of these falsifying observations.

The crucial question for whether the anthropic landscape is science or not is whether it makes any testable predictions. Polchinski doesn't at all address the question of whether further study of the landscape will lead to any prediction of anything, presumably because like everyone else he has no plausible idea for how this could come about. He does claim that Weinberg's 1987 arguement makes 5 successful predictions or postdictions:

The anthropic argument is not without predictive power. We can identify a list of post- or pre-dictions, circa 1987:

1. The cosmological constant is not large.

2. The cosmological constant is not zero.

3. The cosmological constant is similar in order of magnitude to the matter density.

4. As the theory of quantum gravity is better understood, it will provide a microphysics in which the cosmological constant is not fixed but environmental; if this takes discrete values these must be extremely dense in Planck units.

5. Other constants of nature may show evidence of anthropic constraints.

Calling these successful predictions seems to me a huge stretch. 1. follows from a tautology, 2. and 3. are the same "predictions" I get by saying I have no idea what is going on here (including having no good reason to believe the CC is zero). 4. isn't an experimental prediction at all, and 5. is so vague as to be completely meaningless.

Polchinski ends up his defense of the landscape by quoting Dirac:

One must be prepared to follow up the consequences of theory, and feel that one just has to accept the consequences no matter where they lead.

The problem with this is that Dirac undoubtedly didn't have in mind the idea that if your theory has no experimental consequences, you should accept the idea that you can't ever predict anything. Obviously, you should give up on your theory at that point, and this is what Polchinski and others show no signs of being willing to even consider. Back in 1998, in lectures at the SLAC Summer Institute, he wrote:

On Lance Dixon's tentative outline for my lectures, one of the items was 'Alternatives to String Theory.' My first reaction was that this was silly, there are no alternatives…

and his attitude doesn't seem to have changed since. He and others never discuss the possibility that string theory is simply a wrong idea, a possibility for which the landscape provides overwhelming evidence. This seems to be something he is unwilling to seriously consider.

There's one peculiar reference in his paper. When he refers to the problem that the landscape can't even predict the one thing people originally hoped it would be able to, the scale of supersymmetry breaking, he writes:

An obvious question is whether we can understand the supersymmetry-breaking scale (see and references therein). Is low energy supersymmetry, or some alternative , favored?

is a reference to split supersymmetry, and is a reference to a paper by Fox et. al called Supersplit Supersymmetry. The strange thing about the apparently serious reference to is that the paper in question is actually an April Fool's joke (check the date on it…). The authors were making fun of how supersymmetric phenomenology is being pursued, and specifically of the idea of split supersymmetry. In "supersplit supersymmetry", all superpartners are pushed up to unobservability at the Planck scale, solving all the problems caused by the lack of observation of effects of supersymmetry. For any conceivable purpose, the supersplit supersymmetry model is precisely the standard model. The joke is that particle physicists have become so enamored of supersymmetry that they would happily study a supersymmetric model inherently indistinguishable from the much, much simpler standard model. Part of being unwilling to consider the idea that superstring theory might be wrong is being unwilling to consider the idea that supersymmetry might be wrong, and thus, instead of referring to the standard model, one adopts the supersplit supersymmetry model.

The funny thing about this April Fool's joke paper is that, according to SPIRES, Polchinski's is the sixth paper to cite it. Looking through these six papers, only one of them seems to be aware of the joke, with a footnote to the reference pointing out that the paper appeared on April Fool's day and that the model was equivalent to the standard model. Some of the particle theory community now seems to think that the idea of a vastly more complicated model that is inherently indistinguishable from the standard model but is in some sense "supersymmetric" is a model worth taking seriously and making reference to. Yet another weird thing about this paper is that there is one trackback to it, and this trackback was generated by a comment from LambchopofGod on a Cosmic Variance posting. Presumably it's a bug, not a feature, that any commenter on an approved blog can generate trackbacks at the arXiv, but seeing the way the arXiv handles trackbacks, who knows. In any case we'll see at some point if trackbacks to the Polchinski paper and the Fox et. al. paper get generated from this posting. Hopefully at least a trackback to the Fox et. al. paper will appear, since many readers of that paper don't seem to realize that

IT'S NOT SCIENCE, IT'S AN APRIL FOOL'S JOKE, PEOPLE!

Comments

1. **JC**
   April 9, 2006

   Besides Witten defecting from string theory, what else could possibly make Susskind, Polchinski, and others give up string theory? It seems like all this anthropic silliness over the last few years isn't enough.

2. **woit**
   April 9, 2006

   JC,

   I'm sure Susskind and Polchinski will give up on string theory if and when someone comes up with a much better idea and the physics bandwagon changes direction to follow it. The weird thing is that it seems that deciding that the theory can't predict anything doesn't seem to be enough to make them give up on it. If you had told me a few years ago that this would happen, I would have refused to believe it. I'm still having trouble…

3. **JC**
   April 9, 2006

   I've always wondered what would be going through the mind of a retired theorist who had spent his/her entire working life on a theory which turned out to be either wrong and/or vacuous in the end.

   I get the impression that for some folks, the anthropic stuff is almost like a last ditch attempt to salvage their life's work into something meaningful and/or at least to slow down the decline to oblivion and/or meaninglessness.

   I would guess that knowing that one's entire life's work was a huge boondoggle, isn't exactly something to be proud about in retirement.

4. **Chris Oakley**
   April 9, 2006

   "A new scientific truth does not triumph by convincing opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

   — Max Planck, *A Scientific Autobiography and Other Papers*, 1949

   This may also be true about adherents of a non-predictive methodology admitting that they were backing the wrong horse.

5. **JC**
April 9, 2006

Allegedly Lorentz was a true believer in the ether to the very day he died. (I don't have a reference handly offhand).

I wouldn't be surprised to see that other physicists of Lorentz's generation (ie. mid-late 19th century) were all true believers in the ether, all the way to their graves.

6. **Tony Smith**
April 9, 2006

Peter says "… Susskind makes the same sort of claim that the anthropic landscape is much like the theory of evolution …".

I would hope that everyone (even Joe and Lenny) could agree that the "theory of evolution" includes at least the following structures:
1 – a fairly detailed fossil record showing an understandable progession of organisms from early blue-green algae to oxygen production … etc … to dolphins in the sea and humans on land; and
2 – a crude understanding of genetics at the molecular level and that mutations can cause genetic change.

Maybe people can argue about the punctuations in puntuated equlibrium speciation, etc, but there IS a progressive fossil record and there IS a molecular mechanism for genetic change.

My question to Joe and Lenny would be:
Can you show me where the "anthropic landscape" contains ANY comparable structures ?

Where is a string-theory-model of a progressive fossil record,
i.e, a series of states leading from the Big Bang to our present Earth arranged in an understandable progression like the series of fossils in our fossil record ?

Where are the string-theory-model laws (NOT vague hand-waving) that govern how string-theory states can mutate from one to another a la molecular genetics with mutation ?

Unless and until Joe and Lenny can put up such examples, they should shut up about the Landscape being "much like the theory of evolution".
Of course, as long as their audience (present-day superstringers = 90% of USA theoretical particle physicists) is sufficiently sheep-like to buy their stuff, they are not likely to "shut up". Rather, they are more likely to shout louder and attempt to drown out any voices of reason.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – I am particularly disappointed in Joe. Years ago he did calculations that could be compared with experiment, and if he had stuck to his guns, he could have claimed predictions against then-conventional wisdom, which predictions were later validated. For example, in Nuclear Physics B221 (1983) 495-523, entitled "Minimal Low-Energy Supergravity", Joe Polchinski (with Alvarez-Gaume and Wise) said:
"… Weak-interaction breakdown occurs for top-quark masses between 100 and 195 GeV. If we take A to have its value in the Polonyi model … the top-quark mass lies in the smaller range 140-195 GeV. … The renormalization group equation … tends to attract the top quark mas towards a fixed point of about 125 GeV. …".

However, Joe et al lost their nerve and caved when in 1984 CERN claimed to find the T-quark around 45 GeV.
Instead of saying that the CERN 45-GeV "discovery" was wrong (as indeed it was, with CERN embarrassingly "undiscovering" it over the next several years), they seem to have followed the sheepish herd of consensus exemplified by this quote from Mohapatra's book Unification and Supersymmetry:
"… An ingenious … method has been proposed by Alvarez-Gaume, Polchinski, and Wise … It is interesting that m_t lies in the range [between] 100 GeV …[and]… 190 GeV.
The recent discovery of the t-quark in the mass range of 40 to 60 GeV therefore rules out the simple-minded analysis …".

If Joe et al had stuck to their guns from 1983 to 1994, Fermilab data would have vindicated their predictions, and they could have claimed a great success for theory vindication by experiment.

7. **Daniel Doro Ferrante**
April 9, 2006

Folks that **think** they know and understand Evolution Theory would really do good in reading Ernst Mayr: Ernst Mayr (EDGE) and Ernst Mayr (Amazon).

At least according to what i've read and heard, there's a huge gap between what is being called "evolution theory" and what contemporary neo-Darwinism actually is (the tantalizing evidence goes from fossils to virus and bacteria drug resistence, i.e., from ancient to modern times).

[]'s!

8. **Arun**
April 9, 2006

Most probably it is because of my shortcomings, but I do not see how any of the proposals of Polchinki's talk solve the problem posed at the beginning – namely, the gravitation of the Lamb's shift energy.

9. **woit**
April 9, 2006

Arun,

The idea of anthropic landscape solution to the CC problem is that for some particular points on the landscape all the large contributions to the CC like the ones from electron loops just about exactly cancel, leaving a CC close enough to zero for galaxies (and us) to be able to develop.

10. **LambchopofGod**
April 9, 2006

As you know, I was of course aware that Fox et al is a joke. Furthermore, at the time of the posting I believed, and I still do believe, that Sean thought it was a joke too, and that his reference to it was an extension of the joke. But I have no experimental evidence for this belief of mine……

11. **Sean Carroll**
April 10, 2006

I think that more people got the April Fool's joke than Peter is giving them credit for.

12. **boreds**
April 10, 2006

Yep, citing the paper hardly indicates that he doesn't get the joke.

13. **Arun**
April 10, 2006

Peter,
So there a near-continuum of landscape states, all with our standard model as the low-energy limit, but differing in contribution to the CC by stringy fields, so that in one of them, the CC comes out right? Or is it more that landscape vacua with the wrong CCs also have the wrong low energy physics?

I'd feel a lot less unhappy if it were the latter.

Thanks!
-Arun

14. **Arun**
April 10, 2006

> We can think of Fig. 2 to good approximation as representing the shift of the electron zero point energy in the environment of the atom or the nucleus. Thus we must understand why the zero point energy gravitates in these environments and not in vacuum, again given that our vacuum is a rather complicated state in terms of the underlying fields. Further, if one thinks one has an answer to this, there is another challenge: why does this cancellation occur in our particular vacuum state, and not, say, in the more symmetric SU(2) × U(1) invariant state of the weak interaction? It cannot vanish in both because the electron mass is zero in the symmetric state and not in ours, and the subleading terms in the vacuum energy (1.1) — which are still much larger than the observed V — depend on this mass.

So, either the landscape vacuum does the appropriate CC cancellation at one scale, and we still have the puzzle of which scale, or the landscape vacuum manages the magic of CC cancellation at all scales?

15. **woit**
April 10, 2006

Sean and boreds,

Looking at the six papers that referenced Fox et. al., my guess was that some knew it was a joke, but it is pretty clear that some didn't. I still find Polchinski's reference to it strange. Undoubtedly he knew about the joke, and maybe his reference to it was some sort of self-deprecating humor. But when there's a huge question about the predictivity of your research program, and you're writing an article defending it, ignoring the predictivity problem and putting in a reference to a joke paper where the lack of predictivity is the joke, seems to me exceedingly weird.

16. **woit**
April 10, 2006

Arun,

It's the former. There's no known reason why having the right small value of the CC would imply getting anything else right about low energy physics.

In the landscape, the CC is the same at all scales. There is one section of Polchinski's article where he discusses the possibility of a version of gravity that doesn't couple to the CC at all scales, but this has lots of problems that he explains.

17. **Arun**
April 10, 2006

Peter:
In the landscape, the CC is the same at all scales.

How is that magic accomplished?

18. **woit**
April 10, 2006

Arun,

All I mean is it behaves like a standard CC in Einstein's equations. This is just a constant. Probes of any wavelength see the same CC. This is standard gravity. Polchinski is discussing the idea of modifying gravity.

19. **Who**
April 10, 2006

Going back to your 23 August 2005 post
http://www.math.columbia.edu/~woit/wordpress/?p=245
which is closed so I cannot add a comment,

the book is available for order from amazon.uk for $21

or more precisely 12 pound ster. (hardcover)

http://www.amazon.co.uk/exec/obidos/ASIN/0224076051/026-6340214-9602016

the availability date they give there is 16 march 2006

ordinary US amazon gives a date of 25 april 2006 and has one review, but lists no price so it seems that amazon.uk is already shipping

20. **woit**
April 10, 2006

About the book,

The information on Amazon isn't correct. Last I heard, publication date in England is June 1. A few weeks ago I went over the final proofs, so they should start being able to print books soon, but I haven't seen any yet. Publication in the US should be sometime in September. I'm just starting dealing with the production people here for that version. It will probably have a different preface.

21. **anon**
April 10, 2006

"It will probably have a different preface." Oh, I see. The British version is different to the American one; it has a topless girly on page 3.

22. **Lubos Motl**
April 10, 2006

Peter, you are kind of crazy if you think that Polchinski does not realize that supersplit SUSY is a joke.

23. **Not a Nobel Laureate**
April 10, 2006

To put this anthropomorphic principle in perspective imagine the reaction in the condensed matter community if someone invoked the same principle to "explain" high Tc superconductivity.

Nothing more pathetic than a group of relatively smart people believing their own bullsh*t.

Henry Kissinger sum it up best after his retirement to academia.

"Why are academic battles so fierce?"

"Because the stakes are so low."

So it is with strings and their proponents.

24. **Hech Baan**
April 10, 2006

Dear Not a Nobel Laureate,

Anyone should understand that anthropic principle isn't part of the string/M-theory. It's a temporary way of making sense of some results. When we know more we'll be able to make predictions without relying on the anthropic principle.

Rest assure that if something is obvious to almost everyone then it's obvious to the scientists too.

25. **Scott Aaronson**
April 10, 2006

Peter, there's another difference between natural selection and the anthropic principle, one that to my mind is even more basic than testability. This is that natural selection is an "algorithmic workhorse" — a mechanism for amplifying low-probability events to higher probability. Start it up, and it takes you the rest of the way. It's a tautology with oomph, something any mathematician should recognize. Whereas the anthropic principle is an oomphless tautology.

26. **Not a Nobel Laureate**
April 11, 2006

Hech Baan wrote

"Anyone should understand that anthropic principle isn't part of the string/M-theory."

No, but a number of string/M-theory proponents invoke it to avoid question the merit of having so many in the field pursue this line of research.

"It's a temporary way of making sense of some results."

It's a way of avoiding physical reality.

"When we know more we'll be able to make predictions without relying on the anthropic principle."

That doesn't sound very promising given that you can't make any testable predictions by relying on the anthropic principle.

"Rest assure that if something is obvious to almost everyone then it's obvious to the scientists too."

Not in my experience.

27. **Who**
April 11, 2006

Obviously what gives natural selection oomph is reproduction.
Polchinski's "survivors survive" misrepresents the tautology, which is more like "the prolific proliferate"—-reproductively successful (genotypes) succeed in reproducing.

Polchinski:
Of course, the anthropic principle is in some sense a tautology: we must live where we can live.

Natural selection is a tautology in much the same sense: survivors survive. But in combination with ***a mechanism of populating a spectrum of universes or genotypes****,* *these ‘tautologies’ acquire great power.*

My comment:

One can imagine a centralized “mechanism of populating” a range of possible genotypes which operates randomly—where individuals do not reproduce individually but are produced by a central machine. Each is the expression of a random sequence: almost no individuals are viable—most come out of the machine dead or as unorganized mess.

If I understand Scott Aaronson’s remark, such a scheme has no oomph. It does not amplify low-probability instances of organization.
If his point is valid, then Landscape philosophers won’t get anywhere until they think of a way for nice universes to reproduce and thereby proliferate.

28. **Hech Baan**
April 11, 2006

Dear Not a Nobel Laureate,

Let’s face it string/M-theory isn’t perfect, but it’s the best theory we have today. It’s a pity that the theory is so complex that not everyone can appreciate the beauty and predictive power of it.

Sooner or later we will make a testable prediction without relying on the anthropic or any other questionable principle. At the same time we shouldn’t overestimate the anthropic principle’s role.

My advice to you is: be patient. There are much simpler things we don’t know answers yet (e.g. twin prime conjecture). Understanding physical reality is the most complex problem. Nobody expects quick or short answers. If the anthropic principle will get us there then so be it.

29. **knotted string**
April 11, 2006

Dear Hech,

Your belief that eventually a testable prediction will arise seems to have absolutely no scientific validity. It is just a belief, like the beliefs of religion.

The world has been patient for 20 odd years with various forms of string theory, and you advise us to be patient?

30. **anonymous**
April 11, 2006

Excellent post, “Who”. Nicely expresses the fundamental difference between the two.

31. **Arun**
April 11, 2006

I’m still scratching my head as to how the landscape produces a cosmological constant that is the same at all scales, when the physics accessible to us is not doing so. How does the landscape vacuum arrange for the appropriate cancellations at all scales?

32. **woit**
April 11, 2006

“Peter, you are kind of crazy if you think that Polchinski does not realize that supersplit SUSY is a joke.”

Lubos,

You don’t even bother reading what I write. You seem to have missed my comment about Polchinski:

“Undoubtedly he knew about the joke”

33. **Thomas Larsson**
April 11, 2006

Both Polchinski’s paper and the supersplit paper (v1) were uploaded on March 31. If Polchinski’s paper was also intended as an April’s fool joke, he surely fooled me.

34. **island**
April 11, 2006

Hech Baan Says:

Let’s face it string/M-theory isn’t perfect, but it’s the best theory we have today.

Didya hear the one about the string-tailed cat in a room full of rocking chairs?

… or the one about the stringy-haired hippie who wondered into barber camp?

~

This strongest implication of the anthropic connection to the forces of our universe is through evolutionary theory since the anthropic constraint on the forces indicates that there is very possibly a mechanism that enables the universe to (((convolve))) it’s characteristics inherently forward to higher orders of entropic efficiency, just like we humans did when we evolved from apes to harness fire… and beyond.

This obvious extension can’t be ignored because the TOE becomes the ToE when the anthropic principle explains “why” the forces cannot be unified.

Which has absolutely nothing to do with string theory, but you’d have to a real air-head to think that there is no connection between evolutionary theory and the anthropic principle.

35. **Tony Smith**
April 11, 2006

Who said "… Landscape philosophers won't get anywhere until they think of a way for nice universes to reproduce and thereby proliferate …".

Polchinski, in hep-th/0603249, said "… while it is difficult to select for a single vacuum of small cosmological constant, it is extremely easy to identify mechanisms that will populate all possible vacua –
either sequentially in time, as branches of the wavefunction of the universe, or as different patches in an enormous spatial volume.
… if the many vacua are metastable: inflation and tunneling, two robust physical processes, will inevitably populate them all …
But this is all that is needed! … Thus we meet the anthropic principle….".

In other words, Polchinski and the anthropic / landscape people are happy with reproduction and proliferation by "dumb" processes like inflation and tunneling that don't act to select "nice universes" – Who described such processes as "… a centralized 'mechanism of populating' a range of possible genotypes which operates randomly – where individuals do not reproduce individually but are produced by a central machine. Each is the expression of a random sequence: almost no individuals are viable – most come out of the machine dead or as unorganized mess …".
while
people like Who (and me) think that the processes of reproduction and proliferation should act to select "nice universes",
in the sense that evolution by genetic mutation and environmental requirements acts to select "fit" organisms.

In other words, my view is that nature is built upon processes that, in Who's words, "… amplify low-probability instances of organization …",
and
any landscape, anthropic principle, or whatever that just sort of randomly produces stuff will never produce any physics other than, in Who's words, an "unorganized mess".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – As to how to build a process that will "… amplify low-probability instances of organization …", maybe it might be productive to generalize the quantum computer / spin network viewpoint of Paola Zizzi expressed in gr-qc/0304032.
It also might be nice to look closely at exceptional algebraic structures, because, since they are "special" mathematically, they might be good at selecting/amplifying "special" physics structures.
In my view, it would be nice if such work received funding on the order of the current level of superstring funding. After all, as knotted string said to Hech Baan: "… Your belief that eventually a testable prediction will arise [from conventional string theory] seems to have absolutely no scientific validity. It is just a belief, like the beliefs of religion. The world has been patient for 20 odd years with various forms of string theory, and you advise us to [continue to] be patient? …".

36. **Hech Baan**
April 11, 2006

Dear knotted string and Tony Smith,

I wouldn't call it a belief. I would call it an educated guess. It has the same scientific validity as our confidence that one day we will prove the Riemann Hypothesis. The world was patient for almost 150 years with various approaches to prove the Riemann Hypothesis. And in that case I ask you to be patient too.

Alternatively, join the group of scientists addressing the current problems to appreciate the beauty and complexity of the nature (be it distribution of primes or Quantum Gravity).

37. **woit**
April 11, 2006

Hech Baan,

No, belief in string theory does not have ths same scientific validity as belief in the Riemann Hypothesis. There is overwhelming numerical evidence that the Riemann Hypothesis is true, zero experimental evidence for string theory. In the case of function fields (which are closely analogous to number fields), the Riemann hypothesis has been proven, which is one of several reasons to believe that a proof for the standard Riemann Hypothesis will be found. I've heard this analogy repeated elsewhere, but sorry, it really is complete nonsense.

And please, everyone, if you have something to say about the Polchinski paper, please do so, if you want to rant in an empty way about the anthropic principle in general, do this elsewhere.

38. **LambchopofGod**
April 11, 2006

I think Polchinski's paper raises a lot of interesting issues. I wonder if anyone can comment on what he means by "post-selection" of the cosmological constant [the last paragraph before his conclusion]?
Meanwhile, see the final sentence of
http://arxiv.org/abs/astro-ph/0604242
for an interesting new suggestion as to how the CC problem might ultimately be solved……

39. **Tony Smith**
April 11, 2006

Hech Baan said: "… Dear knotted string and Tony Smith … I ask you to be patient … Alternatively, join the group of scientists addressing the current problems to appreciate the beauty and complexity of the nature …"
and went on to say, further:
"… CC paradox: 'Landscape is huge. The probability that I live in a universe with a small CC is almost zero. In spite of this, I live in one of them. Now how is that?'
A possible solution – Antropic principle …
… If you find a better solution … then you can apply it to the CC paradox. Good luck. …".

To Hech Baan. Please don't assume that only conventional string theorists can deal with the CC paradox. For example (and here I am NOT mentioning my work to get into a discussion of its merits because Peter does not want his blog to get into such things, but am only mentioning it as one concrete example of an alternative to conventional string/M theory),
based on a generalization of Irving Segal's conformal structures related to gravity, I can and have calculated the present value of the ratio
Dark Energy : Dark Matter : Ordinary Matter
with the result 0.73 : 0.23 : 0.04
which is pretty close to the WMAP results.
Calculations are on the web as a pdf file at CERN CDS preprint EXT-2004-013

but they are not on the Cornell arXiv because I am blacklisted there.
They can also be found, with more details, in html format at
http://www.valdostamuseum.org/hamsmith/cosconsensus.html#wmap

Maybe my work is right, maybe it is wrong, or maybe it at least contains some seeds from which a useful theory that can be used to calculate (not relying on anthropic stuff) the CC / Dark Energy : Dark Matter : Ordinary Matter ratios that we see experimentally.

I am NOT saying, as Hech Baan is saying about conventional string/M theory, that my work is "the best theory we have today".

I am only saying that it seems to give a more concrete result than conventional string/M theory, and is therefore a possible counterexample to Hech Baan's assertion that conventional string/M theory is "the best theory we have today".

There may be many other models that may turn out to be more useful than either my model or conventional string/M theory (for example, some forms of loop quantum gravity, spin foams, etc).

My point is NOT to try to sell my model.
My point is that ALL possible approaches should be funded and explored,
and
that it is quite likely (given 20+ years of work by hundreds of very smart well-funded people on conventional string/M theory, resulting in failure to produce concrete results that can be compared with experiments)
that being "patient" and "join[ing] the group of [conventional string/M theory] scientists …" in such things as the anthropic landscape may well only lead theoretical physics into a Dark Age.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

40. **woit**
April 11, 2006

Lambchop,

That paper doesn't offer any way to solve the CC problem. Right now the anthropic "prediction" of the CC is too high by at least one order of magnitude, more if you let Q vary. The argument of this paper seems to be that if you observe planets in certain dwarf galaxies, then you could claim that the CC is too high by as much as three orders of magnitude, even keeping Q fixed.

This doesn't offer any positive prediction about the CC, it just holds out hope for one more piece of evidence against the anthropic explanation of the CC. There already is lots of evidence against the anthropic landscape, even if this particular new piece of evidence turns out to be possible, I don't think it will have much effect.

41. **LambchopofGod**
April 12, 2006

Errr….actually I was mainly referring you to the *last sentence*, where the author suggests that the CC problem will ultimately be solved when we receive an instructional broadcast from a more advanced civilization….. 🙂

42. **woit**
April 12, 2006

Lamchop,

Oops, seems that I sometimes don't read things carefully, and don't get the joke….

43. **a philosophy student**
April 12, 2006

> the anthropic principle is in some sense a tautology:
> we must live where we can live.
>
> Natural selection is a tautology in much the same sense:
> survivors survive.

Since terms are important in this context, this note:
These two terms or principles stand for different things:

When saying: 'we live where we must live' (AP), then on equal terms, concerning evolution, would be: 'we survive where we must survive'. Let's call it, for ease, the 'natural principle' (NP).

Going a bit further, we could modify NP, while keeping it on equal footage qua scope of meaning to AP, to: 'survivors survive because they càn survive'.

If we take this as a pricinple after the example of AP, then now we should try find the reason behind what the principle states, or we are stuck with our tautology.

But the difference between the two is of course that for NP the reason has been found, namely natural selection (NS), while for AP it hasn't, despite the two being put on equal footage here (in the quote), mixing the principle NP with it's reason NS, while for AP there's only the principle.

44. **a philosophy student**
April 12, 2006

I mean 'can' where I say 'must', twice, in:

"When saying: 'we live where we must live' (AP), then on equal terms, concerning evolution, would be: 'we survive where we must survive'. Let's call it, for ease, the 'natural principle' (NP)."

45. **boreds**
April 12, 2006

Peter

I guess lubos is referring to what you say in the posting above:

"the apparently serious reference to [70]"

rather than what you said in the comments, which is clearer.

In any case, I don't think it's that weird that Polchinski made a joke about the paper.

I don't know about the other five citations, but it would be hard to imagine they didn't realise it was an April fools if they'd read the paper.

46. **Who**
April 12, 2006

philos. student,
you might be interested in Scott Aaronson's comment in this thread
http://www.math.columbia.edu/~woit/wordpress/?p=372#comment-9623

I think it has something to do with what you are discussing. Perhaps you saw it and simply didn't think it germane to your points. But I think it does have some bearing. I expanded on Scott's a couple of comments later. See if you can make anything of the earlier discussion and if it fits with what you are saying.

47. **Shantanu**
April 12, 2006

Peter have you seen astro-ph/0604242 ? It talks about an observational
test of antrophic origin of cosmological constant. your thoughts on it?

48. **woit**
April 12, 2006

Shantanu,

I just wrote about that in a comment above, responding to Lambchopofgod, who was actually pointing to the kind of absurd end of the paper. But now, my comment is relevant, as an answer to your question.

49. **Shantanu**
April 12, 2006

Thanks Peter,. Sorry I missed your comment above. anyhow I am not sure how serious about the last sentence.

50. **Aaron Bergman**
April 12, 2006

To the point on the analogy between the anthropic principle and natural selection, it might be an oomphless tautology, but it doesn't need the same oomph as natural selection. As long as there's one universe with life in it, we could be in that universe.

51. **woit**
April 12, 2006

Aaron,

It's also true that we could all be a figment in the imagination of the Flying Spaghetti Monster. I wish someone out there who doesn't think this stuff is indefensible pseudo-science would, instead of defending it by invoking tautologies, give a plausible scenario of how it will lead to the kind of testable predictions that up until now everyone agreed were required before you called something science.

52. **a philosophy student**
April 12, 2006

Who,

My point is that Polchinski, besides like Peter says at other places in his paper, in this quote mixes the resp. situations of evolution and anthropics by his chosing of terms.

AP is a tautology, and as such, states facts. What Peter would like physicists to do is to search for a reason for these facts, a mechanism that explains the state AP refers to.

Now NS is such a mechanism, but of course for a different state of facts, namely for what I above, for easy of the discussion, have called NP.

AP does not have such a mechanism, and it is wrong to put both AP and NP on equal footing, as Polchinski does in the above quote, making it appear they are.

He should have written, instead of 'natural selection', something like 'survivors survive because they càn survive' (NP). This is a statement of facts, which can be, liguistically, and more important: logically, a tautology, but the term 'natural selection' cannot be a tautology for it is an empirical mechanism, nót a statement of facts which can be a logical tautology. But of course doing so would point out AP has no explanatory mechanism.

The correct use of these terms in this is important in (@ least) two respects:

1) in the context of string theory: as Peter points out:

> … trying to blur the difference between these two very different situations is not something a scientist should be doing …
>
> …… physicists who care about this credibility (of science) should not
> be claiming that an idea like the anthropic landscape has the same status as heavily tested and verified theories like that of evolution.

2) these days Darwin is under attack from 'unscientific' corners. It is in this respect that it is very important to be clear in one's choice of words. You cannot describe natural selection by 'survivors survive'. NS is much much more than that. By bringing NS (wrongfully) down to the level of a statement of facts it can be eassily misunderstood and attacked, eg by calling it a tautology.

On Scott's post:

> … natural selection is an "algorithmic workhorse" — a mechanism for amplifying low-probability events to higher probability. Start it up, and it takes you the rest of the way

This makes it appear as if NS is an always-sure way to an always-sure outcome. NS is an algorithm, granted, but one that relies heavily, not to say totally, on external input, ie the environment. This is a highly coincidential matter. Take eg the extinction of the dinos. Whether it was by an asteroid or not, fact is had they lived on, mammals would still be small mouse-like creatures hiding in small holes in the ground.

So, NS also has 'an sich' nothing to do with the probability of events. It will not change the low or high-probability of an event. It's all coincidence. It is a fact that human life is a very low-probability event in the universe, but that it excists here is not,
qua probability, due to NS, but to the occurence of the low-probability fact that after earth was formed, it gave rise to the necessary 'materials' for evolving life. This 'material' is then worked upon by the evolutionary mechanism NS, but the latter has nothing to do with the probability of the correct circumstances for evolving life being present.

Note: of course it's not only the circumstances on earth that are relevant, or low-probable, but also the distance earth-sun, the presence of a big object like jupiter swallowing comets and asteroids, the distance sun-center of the galaxy, aso.

A note on Jupiter: the point of Jupiter swallowing collision-course-objects means that life on earth has got the necessary time to evolve, as the dinos will be happy to point out.

Another note: this whole discussion depends on how you weigh the term 'low-probability'. 1/zillion may be considered low probable, but in an infinite set this still gives rise to an infinite amount. According to this reasoning, if our universe, and/or the multiverse, and/or whatever is infinite, then there are an infinite of worlds with intelligent human-like things on it – even discussing the same things we do now.

Even by this account it's not due to NS that there is a sure outcome of whatever you want, but it is due to the infinity of the 'initial state' it works upon.

So NS doesn't in fact necessarily has 'oomph' – @ least not when not considering the 'basic block', ie the gene, or, in Smolin's case, the black hole – and even in this case, again, it's not due to NS but to the
nature of the material it works upon.

Phew.

Sorry Peter if this is way to off, I only meant to make a humble first post here.

53. **Christine Dantas**
April 12, 2006

The Lithic Principle. The Liquid Principle. The Canid Principle (of course, man´s best friend must have some importance on all this). In summary, a multitude of principles to chose from in order to study the multiverse. I am sure among them there must be a Multiverse Principle (the Universe is the way it is so to allow for the existence of a multitude of others).

Now, seriously, I am deeply intrigued that I exist and see the Universe around me. I really would like to know why things are like that. The Anthropic Principle is fruitless to that end, in my opinion, by its own definition.

Christine

54. **Aaron Bergman**
April 12, 2006

Peter, we *really* don't have to go through this yet one more time.

Cosmological natural selection doesn't have the "oomph" that the real thing does, either, because nothing ever dies.

55. **Who**
April 12, 2006

because nothing ever dies

entire universes die

Aaron you brought up CNS. Scott did not mention it, nor did I. Is this because you have some new ideas about it, like "nothing ever dies", and want to discuss them?

56. **Aaron Bergman**
April 12, 2006

One can read between the lines in your comments.

The point is that one does not need a way to populate the landscape, a way for "nice universes" to reproduce or anything. Once you have a mechanism that produces a single universe which can support life, the anthropic principle holds.

There are a zillion different ways one can put some sort of measure on the space of vacua, if you feel like engaging in some sort of principle of mediocrity, but that's not the anthropic principle.

57. **Who**
April 13, 2006

You already said that Aaron, and Peter rebutted it. You said
… As long as there's one universe with life in it, we could be in that universe.

Peter replied*…give a plausible scenario of how it will lead to the kind of testable predictions…required before you {call] something science*
——————

Scott's original point was:

…natural selection is an “algorithmic workhorse” — a mechanism for amplifying low-probability events to higher probability. Start it up, and it takes you the rest of the way…

It is a powerful means of generating complex organization. (Perhaps the most effective mechanism we know of for doing this.)
Mere happenstance is not. Therefore, I think Scott’s point was, Polchinski made a poor analogy.

Do you think Polchinski made a correct analogy? So far your comment does not support this, but has been to repeat that a natural mechanism for self-tuning or amplifying low-probability events is NOT NEEDED, which is not quite on the mark. If you would like, please think up a reason why Polchinski’s comparison of biological Natural Selection to string thinkers’ Landscape speculation should not be considered far-fetched.

58. **Aaron Bergman**
April 13, 2006

Peter’s statement, true or not, is not a rebuttal.

I don’t really care all that much about how good the analogy between the anthropic principle and natural selection is; it’s obviously not a precise analogy. I’m just trying to clarify a few issues along the way.

59. **ksh95**
April 13, 2006

Peter Said:

> …”give a plausible scenario of how it will lead to the kind of testable predictions that up until now everyone agreed were required before you called something science…”

Didn’t Smolin do precisely this? All one needs is for the “*Universe-Creating-Mechinism”* to be dependent on a few measurable parameters.

60. **Aaron Bergman**
April 13, 2006

It depends on what you mean by predictive. There are a variety of way to populate a landscape of vacua; common examples are some sort of quantum mechanical superposition, tunneling, Smolin’s idea, etc. These will give different distributions of the population of vacua. The fundamental leap, however, is whether this population translates into a probability measure. If you make that leap, a sort of principle of mediocrity, you can make probabilistic predictions. Otherwise, one is simply left with the binary question of whether a particular vacuum gets populated or not.

61. **Chris W.**
April 13, 2006

The most galling thing to me about drawing the analogy with natural selection is that the variation upon which selection operates in biology is an inescapable empirical fact, whether or not you understand the underlying mechanism (and Darwin didn’t). This *observed* variation was Darwin’s logical starting point. He knew that it was the raw material with which animal and plant breeders worked, and realized that the essential logic of selective breeding applied even when the relevant contingencies—selective conditions—were not consciously arranged but arose accidentally, or as a result of the internal dynamics of an ecosystem.

In contrast, the multiverse is a purely theoretical construct. It is taken seriously mainly because it seems to follow from the joint application of quantum field theory and general relativity in cosmology. However, even this line of argument seems to have degenerated, insofar as the putative existence of the multiverse is now being taken to imply that observed laws in our “pocket” of the multiverse are “frozen accidents”, perhaps anthropically selected. This strikes me as self-defeating to the point of silliness, inasmuch as it was the ramifications of those laws that led us to assert the existence of the multiverse in first place.

(Lately I’ve been wondering why these points aren’t made more often. The self-undermining implications of the multiverse idea have occasionally been pointed out by other commenters on this blog.)

Memorials

April 9, 2006
Categories: Uncategorized

The May issue of the Notices of the AMS has memorial articles about two great mathematicians who passed away recently. The first is about Serge Lang and includes contributions from many people, including my Columbia colleague Dorian Goldfeld. When Lang died last September, I wrote a short posting here, but didn't want to go into much detail. He was a remarkable man, with many facets, but also famously difficult. The Notices article does him justice and is well worth reading.

In the same issue, Loring Tu has the first part of a long article about Raoul Bott, who died last December. It's a wonderful article concentrating on Bott's mathematical career and describing in detail the setting of some of his most important work.

Today I also ran across the sad story of the death of John Brodie. Brodie was a theorist who got his Ph.D. from Princeton and worked on gauge theory and string theory. Evidently he suffered from bipolar disorder, which was a contributing factor in his death. I never met him, but had seen some of his papers. Perhaps some of the readers here knew him personally.

Comments

1. **Derek**
   April 9, 2006

   Whut? No mention of Carleson winning the Abel?

   AMS Notices is really going down the tubes recently – in content, timeliness, character and sadly even readership (just look at the idiocy spouted on the letters page for examples).

   Are there any better magazines in the same genre out there?

2. **woit**
   April 9, 2006

   Derek,

   The Notices is definitely the best thing of its kind I know about. They're not especially timely (now there is news on the AMS web-site, including the Carleson news), but the articles are of high quality, and this takes time. I kind of agree about a lot of the letters….

3. **Jimbo**
   April 11, 2006

   I never knew nor met JOHN BRODIE, but I read Peter's post about his death.
   I would urge everybody to clik on the Rutland Herald link to the story surrounding BRODIE's life & death. It is a superbly written article about an extraordinary human being of the highest order, the stuff of which a movie could easily be made. Stephon Alexander, who knew BRODIE is quoted in the article.
   Someone needs to make sure a slightly compactified version of the Herald's obit is sent to Physics Today.

4. **Lurker**
   April 11, 2006

   Peter et al, I don't mean to spam, but what's the difference between "Not Even Wrong : The Failure of String Theory & the Continuing Challenge to Unify the Laws of Physics" and "Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law"? I discovered this blog only recently, so I'm sorry if this is a FAQ.

   Lurker

5. **woit**
   April 11, 2006

   Lurker,

   The first is the UK edition, supposed to be released about June 1 by Jonathan Cape. I'm surprised it is on the US Amazon site, since Cape just sell in the UK and British Commonwealth. The second is the US edition, put out by Basic Books, should be released sometime in September. The two books will be mostly the same, except the US edition will probably have a somewhat different preface (and different cover, and perhaps slightly different subtitle).

6. **Pindare**
   April 12, 2006

   Thanks for the links, really interesting.

   Off-topic: a paper by John Conway and Simon Kochen on their "free will theorem" is out today http://arxiv.org/abs/quant-ph/0604079

7. **Tony Smith**
   April 12, 2006

   Jimbo said "… the Rutland Herald link to the story surrounding BRODIE's life & death … is … the stuff of which a movie could easily be made …".

   The Rutland Herald article said "… Brodie … took the supermarket job to escape the complexities of physics, he told friends, only to find his mind spinning with questions about bagging efficiency. ….".

   The article reminds me of scenes in the movie "A Beautiful Mind" such as, for two examples:
   those in which John Nash visualizes math ideas and in which such things as flocks of pigeons inspire such ideas; and
   those in which John Nash fled from police-type people.

The most recent four papers of Brodie that I saw on arXiv are:

hep-th/0301138 (with Damien Easson of Syracuse as co-author), entitled "Brane inflation and reheating", in which he "… construct[s] an inflationary brane world scenario from Type IIA string theory …" and goes on to say "… Using observational (COBE) data, we placed constraints on the parameters of the model … we can get more dark matter th[a]n visible matter. However, this dark matter will not be dark because it couples to the Standard Model gauge fields which we have taken to live on the D4-branes. To solve this problem we could put the Standard Model gauge fields on the D6'-branes, but this would modify our cosmological analysis. We leave the details of this model of dark matter for future work. …".

hep-th/0208191, entitled "Vortices under S-duality" dealing with QCD confinement;

hep-th/0107178 (with O. Bergman and Y. Okawa), entitled "The Stringy Quantum Hall Fluid", in which they say that the "… brane picture naturally explains various aspects of the quantum Hall fluid, such as the quantization of the filling fraction, and the charge and statistics of the quasiparticle and quasihole excitations …"; and

hep-th/0101115, entitled "On Mediating Supersymmetry Breaking in D-Brane Models", in which he "… present[s] a method for having first and second order phase transitions in brane constructions which might be relevant for modeling the Standard Model Higgs field …".

Note that these papers all deal with things (COBE inflation bounds, Dark Matter, QCD confinement, Quantum Hall effect, and the Standard Model Higgs field) that are subject to experimental / observational tests,
so
it seems to me that John Brodie's approach to physics was the type of approach that is needed to advance understanding of physics.

Since it has been over 3 years since he put a paper on the arXiv, I wonder whether he left any unpublished notes. If he did, I hope that someone will write them up and put them on the arXiv for posterity.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – The Rutland Herald article said "… people started talking … Why didn't the homeowner who heard the doorbell let it go rather than calling police? Why didn't the patrolman deal with the situation differently? …".
I hope that nobody is blaming the homeowner or the policeman.
If I had a knock on my door at 11 PM and did not recognize the person, I would probably call the police, and, if I were a policeman answering such a call, I would probably also tell that person "… maybe it would be a good idea to go door to door at 11:00 in the morning instead of 23:00 at night …".

8. **Chris W.**
April 12, 2006

Pindare,

Thanks for pointing to that paper. Not only is its content of deep interest, but its style is wonderfully engaging and lucid—head and shoulders above the typical arXiv posting. Given its authorship, I guess that shouldn't be surprising.

9. **Tony Smith**
April 13, 2006

Pindare and Chris W. both commented here about the Conway and Kochen paper at quant-ph/0604079 in which Conway and Kochen say (at page 9):
"… quantum mechanics and general relativity have been mutually inconsistent for most of their joint lifetime, an inconsistency that heterotic string theory resolved … by changing the dimension of space-time! …".

Most of the Conway and Kochen paper deals with their Free Will Theorem: If an experimenter has Free Will, then so must the particles in the rest of the universe outside the experimenter.

My question is:
Does the Free Will Theorem have anything to do with heterotic string theory's extra (beyond 4) dimensions of space-time,
such as some high-dimensional connection between expermimenter and the rest of the universe that correllates the experimenter's Free Will with the Free will of the particles in the rest of the universe,
or
was the reference to heterotic string theory a gratuitous irrelevant remark ?

Since Conway and Kochen say that Bohm and GRW are OK if they allow particles outside the experimenter to also have Free Will, and since Free Will of such particles is something with which I am happy (it has long been an accepted part of many cultures, such as Taoism, etc), it seems to me that such forms of Bohm and GRW are all that you need, and that heterotic string theory is not necessary.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – My descriptions of what Conway and Kochen say are based on the following excerpts from their paper:
"… The Free Will Theorem …
If the choice of directions in which to perform spin 1 experiments is not a function of the information accessible to the experimenters, then the responses of the particles are equally not functions of the information accessible to them.

… According to Bohm, the evolution of a system is completely determined by certain real numbers (his "hidden variables"), whose initial values are not all known to us. … What we do know about these initial values may be roughly summed up by saying that they lie in a set … St at time t … as t increases, St steadily shrinks … because the particles have made free choices. …

Bohm's theory so exorcised … is consistent with our assertion that particles have free will. … The exorcised form of Bohm's theory … prov[es] … the consistency of quantum mechanics … with … the Free Will property of particles. …

our assertion that 'the particles make a free decision' is merely a shorthand form of the more precise statement that 'the Universe makes this free decision in the neighborhood of the particles.' …

… Ghirardi, Rimini and Weber have proposed a theory [GRW] that attempts
to explain the reduction of the state in quantum mechanics by an underlying
mechanism of stochastic 'hits.' … in order to make the GRW theory relativistically invariant … the hits … need … some freedom (to be precise … they must be at least semi-free) …

… fundamental particles are continually making their own decisions. No theory can predict exactly what these particles will do in the future for the very good reason that they may not yet have decided what this will be! Most of their decisions, of course, will not greatly affect things – we can describe them as mere ineffectual flutterings, which on a large scale almost cancel each other out, and so can be ignored. … The authors strongly believe, however, that

there is a way our brains prevent some of this cancellation, so allowing us to integrate what remains and producing our own free will. …”.

10. **woit**
    April 13, 2006

    Tony et. al.,

    This is getting way, way, way off topic….

11. **Belizean**
    April 14, 2006

    Back on Topic:

    I never met Serge Lang, but he taught me calculus when I read his texts back in junior high.

    Thank you, Serge. RIP.

12. **Stephanie Frank Singer**
    April 15, 2006

    Serge Lang and I coexisted at the Math Department at Yale for four years. One winter night I was hanging out at LOM (Leet Oliver Memorial Hall, a.k.a., the Land Of Math) with Carlos Tomei, whose postdoc office was adjacent to Lang's office. We could hear the typewriter banging away next door. Bang-b-bang bang bang, with an occasional shloook (carriage return). All of a sudden the typewriter is silent, and we hear Lang speaking into the phone, in his outrageous French accent (think Steve Martin in the Pink Panther): “It is cold in here!” Crash (receiver hits cradle)!

    A beat.

    Then, clank, clank, clank, on goes the heat.

    This story is funnier to people who have lived in Yale housing and battled with the folks at physical plant.

13. **Stephanie Frank Singer**
    April 15, 2006

    On the subject of memorials:

    Anyone interested in reading two letters George Mackey wrote to a young mathematician will find them linked to my low-tech website.

14. **woit**
    April 17, 2006

    Thanks Stephanie,

    I'll add a link to the letters to the Mackey posting, and reopen the comment section there in case anyone wants to discuss.

Schroer Notes and Other Links

April 13, 2006
Categories: Uncategorized

Bert Schroer has sent me some notes comparing the Lagrangian path integral and algebraic approaches to quantum field theory, which others may also find interesting. I have a very different perspective than he does, but have gone through the experience of at one time believing that basically all there is to QFT is to choose an action functional and then apply straightforward techniques to evaluate the path integrals you get. Non-perturbatively, this works beautifully for Yang-Mills theory, but runs into serious problems in many other cases, and it becomes clear that the path integral method, for all its virtues, does hide some very real problems.

Update: Some more notes from Schroer on AQFT.

Some other unrelated links:

There's an extremely well-known story about the young Gauss, and it turns out that, as almost always with such stories, the truth of what actually happened is rather elusive. American Scientist has a wonderful article about this by Brian Hayes entitled Gauss's Day of Reckoning.

The AMS has announced a Leonard Eisenbud Prize for Mathematics and Physics. It will be awarded every three years for a work published in the preceding six years that brings the two fields closer together. The first award will be made in January 2008. The prize was established by David Eisenbud (currently director of MSRI) and his wife in honor of Eisenbud's father, who was a mathematical physicist.

While I was away the Museum of Natural History here in New York sponsored a debate on the Multiverse, entitled "Universe: One or Many?". For some reports on the debate, see here, here and here. The last of these is from local blogger "mighty dasmoo", who really, really, doesn't like Michio Kaku.

The EPP 2010 Panel will release its final report to the public at a press conference in Washington on April 26.

Update: New Scientist also has some quotes from the "Universe: One or Many?" debate:

Kaku, of the City University of New York, spoke at one point of the possibility of tunnelling into other universes through space-time foam, harnessing the power of negative energy. "Genesis happens all the time," he said. "Continuous genesis in an ocean of Nirvana, and the ocean is an 11-dimensional hyperspace."

As Kaku spoke, Krauss, of Case Western Reserve University in Cleveland, Ohio, looked as if he was about to have an aneurysm. He turned to Kaku. "If there are an infinite number of universes," he declared, "I can't imagine one in which I agree with what you just said."

Update: Another report on the debate is here.

Comments

1. **Thomas Love**
   April 13, 2006

   What, the Gauss story isn't true? I suppose next you'll tell me that George Washington didn't cut down the cherry tree!

   The third blogger's take on Kaku agrees with mine. I've read a few of his books and won't read another.

2. **JC**
   April 13, 2006

   Bert,

   Interesting read.

   What other quantum systems differ greatly between operator and functional integral treatments, besides the spinning top?

3. **Lubos Motl**
   April 13, 2006

   I've read Prof. Schroer's text against the path integrals and it makes no sense to me. It is probably not even wrong.

4. **MathPhys**
   April 13, 2006

   Kaku is phenomenal. I'm always surprised by how he gets invited to speak at events next to respectable people.

5. **Bert Schroer**
   April 13, 2006

   Answer to JC:
   The functional integral representation in its strict measure-theoretic setting is limited to standard quantum mechanical Hamiltonians (kinetic energy + V(q)).
   The formal Duistermaat-Heckmann aspect (which leads to the "Schulman paradox") certainly shows up for any movement of a particle on a group manifold. The model must be integrable.
   By the way, I am not against the functional integral approach, I only indicate its limitations. In fact even in QFT where the approach becomes "artistic" there is nothing wrong with using it. My only plea is that one should not forget that it is artistic in order not to loose an enigmatic piece of future progress.

6. **ObsessiveMathsFreak**
   April 13, 2006

   From what I've read about Gauss, I think it's entirely possible that he was given either a dozen or so numbers, or a sequence of that length, and he simply added them up in his head. No formulae required.

   The part about his summation "trick" may have simply been tacked on later, or rather, the story was tacked onto the trick to pique the interest of otherwise disinterested students!

Certainly, there are people alive today who could mentally add up quite large lists or sequences of numbers, quickly and accurately, with no roughwork. It's well within the bounds of possibility that Gauss was such a person.

Tales often grow in the telling, but it's important to stick to the facts as well.

7. **jb**
April 13, 2006

I don't find the Gauss story dubious at all. I found the trick for calculating 1+…+10 when I was six or seven (of course, I failed to rediscover it when I was asked 1+…+100 several years later), and if some fellow mathematician told me he got 1+…+100 when he was eight, I wouldn't be surprised. Even more so, I have no reason to doubt the Gauss story — he did many much more impressive things and was also a whiz with arithmetic. And of course it's ridiculous to think that if it really happened, there would be primary documents proving it.

8. **Joe Zhou**
April 14, 2006

I've no doubt that Gauss just simply saw the summation trick when he looked at the problem. I consider myself rather dumb, always being one of the slowest in class to do an integral or to come up with proofs. But even I was visited by occasional flashes of insight in childhood. An uncle told me when I was 11 or so about how Zu Chongzhi from the 5th centry found the volume of the sphere by subtracting a cone from the cynlinder. That night, I applied Zu's principle to found the volume for any figure of rotation, and used that generalization to find the center of mass for the half circle, without any inkling of calculus or trignometry. And I am not even smart, almost failing the entrance exams for middle school that same year. Such flashes must be fairly common to intelligent people working on challenging problems.

9. **nayagam**
April 14, 2006

Brian Hayes has written some wonderful articles in American Scientist .The article on Gauss is of course good, but I liked his article [The Spectrum of Riemannium](#) more.

10. **robert**
April 14, 2006

A feat of infant prodigeousness that rivals that of Gauss' summation of a finite number of terms of an arithmetic series is Dyson's summation of an infinite number of terms of a geometric series, which he did to alleviate the boredom when put down in is crib for a nap

http://plus.maths.org/issue26/features/dyson/index.html

And we have this tale from an unimpeachable source.

11. **Chris Oakley**
April 14, 2006

A feat of infant prodigeousness that rivals that of Gauss' summation of a finite number of terms of an arithmetic series is Dyson's summation of an infinite number of terms of a geometric series, which he did to alleviate the boredom when put down in is crib for a nap

Yes, but the Nobel prize was awarded for taking an infinite series, most of whose terms are infinite, and getting a finite result. Maybe Dyson is just as glad not to be one of them.

12. **Karthik**
April 14, 2006

I once listened to Michio Kaku's interview on the BBC. It was like a big fantasy novel.

Science is increasingly becoming like religion where the scientists (priests) interpret the Universe and God and the rest of the world just go by it.

13. **Chris Oakley**
April 14, 2006

Well, Karthik, for an antidote I suggest that you get a copy of [Peter's book](#). It's not out in the U.S. until September, but even so thus far 24 out of 27 people approve of my positive review. Oh, and Peter, my Swiss bank account is #0103242345 (UBS Zurich).

14. **Michael**
April 14, 2006

Peter, could it be that it is you, rather than the path integral approach to QFT, that is hiding some very real problems? The problems people have with path integrals seems to be inversely proportional to their scientific accomplishments.

15. **ObsessiveMathsFreak**
April 14, 2006

> Science is increasingly becoming like religion where the scientists (priests) interpret the Universe and God and the rest of the world just go by it.

It might seem that way when one looks at theoretical physics, but it is not the case for the majority of science. Applied fields of science, physics, chemistry, biology, etc, etc have made huge leaps in the last decade.

Look at how much the world has changed in even the last ten years. Mobile communications are commonplace, the internet is widely accessable, medicinal drugs continue to improve, we are gaining a better understanding of our histories through archeology and new research, we have mapped the globe down to the metre levels.

Science is having an impact on people's everyday lives, ironically at the same time that people feel science is becoming more and more distant from everyday life.

16. **JC**
April 14, 2006

Bert,

Are there any easy non-Euler-Lagrange ways to treat theories with Wigner spins above 2, besides the free particle cases not coupled to anything (ie. Fierz-Pauli equations)?

17. **Chris Oakley**
April 14, 2006

JC,

As far as I remember, higher spin inconsistencies arise because physical massless higher spin fields derive from gauge potentials in much the same way that F_{\mu\nu} derives from A_\mu for spin 1. The free field actions are invariant under gauge transformations in these potentials, something which either must be preserved when one introduces interactions, or generalised (e.g. Yang-Mills). This turns out to be impossible for s > 2 because one cannot write down a interaction that is gauge invariant for all the fields in the coupling (one is presumably not allowed to use the gauge-invariant field strengths here). If one is not using the action principle one would still require gauge-invariant couplings, so I do not see that one would be better off.

18. **Alejandro Rivero**
April 14, 2006

Feynman integral is just a dirac delta' to guarantee the extremality (say, the minimum) of the Lagrangian, but h-regulated. The point that quantum mechanics is the consequence of forbidding the h->0 limit in differential operations was guessed first by Pauli in 1923 (http://documents.cern.ch//archive/electronic/other/pauli_vol3//sommerfeld_0463-2.pdf) upon contemplation of landé j(j-1) formulae and this in turn inspires Matrix Mechanics.

19. **Tony Smith**
April 15, 2006

JC asked about high-spin particles, and Chris Oakley replied, mentioning that techniques like gauge invariance don't work for spin greater than 2.

Weinberg, in Volume I of his work The Quantum Theory of Fields (Cambridge 1995) at pages 243, 244, and 253 describes the high-spin situation in a bit more detail:
"… It should be mentioned that from time to time various difficulties have been reported in the field theory of particles with spin greater than or equal to 3/2. Generally, these are encountered in the study of the propagation of a higher spin field in the presence of c-number external field. Depending on the details of the theory, the difficulties encountered include non-causality, inconsistency, unphysical mass states, and violations of unitarity. …
… there are good reasons to believe that the problems with higher spin disappear if the interaction with external fields is sufficiently complicated. For one thing, there is no doubt
about the existence of higher-spin particles, including various stable nuclei and hadronic resonances. If there is any problem with higher spin, it can only be for 'point' particles, that is, those whose interactions with external fields are particularly simple. …
… both higher-dimensional 'Kaluza-Klein" theories and string theories provide examples of a charged massive particles of spin two interacting with a electromagnetic background field. …
… in order to construct a theory of massless particles of helicity +/- 2 … gravitons … that incorporates long-range interactions,it is necessary for it to have something like general covariance. As in the case of electromagnetic gauge invariance, this is achieved by coupling the field to a conserved 'current' theta ^ mu nu , now with two spacetime indices, satisfying d_mu theta ^ mu nu = 0. The only such conserved tensor is the energy-momentum tensor,
aside from possible total derivative terms that do not affect the long-range behavior of the force produced.
The fields of massless particles of spin j greater than or equal to 3 would have to couple to conserved tensors with three or more spacetime indices, but aside from total derivatives there are none, so high-spin massless particles cannot produce long-range forces. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

20. **knotted string**
April 15, 2006

I love the Lawrence Berkeley National Lab bubble photo of 'universes sprouting off' at http://www.world-science.net/exclusives/060330_multiversefrm.htm

My baby niece can blow lots of bubbles like that. So I wonder if Scientific American will do a feature about her model of the multiverse? 😉

21. **Bert Schroer**
April 15, 2006

Here is a summary answer to some questions concerning higer spin.
The important aspect of higher spin particles is neither the Lagrange setting not the equation of motion (E-L or not), but rather the formula which expresses pointlike covariant fields in terms of Wigner creation/annihilation operators (in momentum space!) and u-v interwiners (interwining the canonical unique m,s Wigner representation with the infinitely many covariant spinorial representations). Looking at such a formula one knows exactly in what physical Fock space this fields lives. Any such field can be used to define a polynomial interaction within the setting of causal perturbation theory (and if you have done this with one spinorial choice you can transform that interaction into any other choice of linear (in Wigner operators) field "coordinatization"). Fields of this type which in addition arise from field equations (i.e. the generic solution of the field equation obeys that formula with the intertwiners) are known for any spin e.g. the Bargmann-Wigner equations for (m,s) representations for m>0. The short distance behavior of the two-point function for such pointlike field becomes more singular with increasing spin. This can be overcome by not insisting in pointlike fields but allowing spacelike semiinfinite string-localization (not string theory!). In that case the intertwiner functions are more complicated and although only one physical Wigner spin enters, there are infinitely many spinorial representations (namely all which belong to the same physical spin) which enter (all this is contained in a paper which will appear soon in CMP, see also (math-ph/0511042). In terms of localized fields this amounts to a field which depends simultaneously on x and on a spacelike direction e (a point in de Sitter space of one dimension lower). In that case the field A(x,e) fulfills the causal localization of a string starting at x and going in e-direction to spacelike infinity. It fluctuates in both x and e and the e-fluctuation is the reason why the fluctuation in x can be milder than in the pointlike presentation of Wigner (m,s) particles. In fact the short distance behavior in x for this string-localized description does nit increase with spin! This makes it very interesting to look for a perturbation theory of string-like higher spin fields (work in progress). All these ideas are outside of Lagrangian quantization but inside the setting of modular localization. Even more fascinating is the modular localization approach for zero mass. I am very time pressed, but I will come back to this point (meanwhile you can have a look at the above reference where all these cases are treated in detail.)

22. **a philosophy student**
April 15, 2006

> Science is increasingly becoming like religion where the scientists (priests) interpret the Universe and God and the rest of the world just go by it.
>
> Applied fields of science, physics, chemistry, biology, etc, etc have made huge leaps in the last decade.
> …
> Science is having an impact on people's everyday lives, ironically at the same time that people feel science is becoming more and more distant from everyday

life.

There's a relative huge gap between science and applied science – *technology*. People see technology gradually evolving and by this are not amazed at new applications appearing, and excisting ones getting better, more sophisticated. Inherently technology is close to everyday life.

'Real' science, through time by its very nature probing deeper and deeper into the fabric of reality, has got involved with issues far away from everyday life. Nanotechnology, genetic manipulation an such still have an imaginable connection with future everyday life, but allready generate an awe feeling. Things as neuroscience and consciousness research, and multiverse theories even more. By claiming some deeper and fundamental knowledge, especially when bearing on excistence and human life, scientists, one can imagine, could evoke religious-like feelings, for this is what religion is all about.

23. **Bert Schroer**
April 16, 2006

In previous notes I explained the difference between (interpretive) autonomous and metaphoric arguments in an example taken from QFT. Perhaps another example of more immediate interest is the way how chiral QFT serves to solve some problems in higher dimensional massive QFT and how it enters as a basic structure in the construction recipes of strings which brings me back to the main theme of this Weblog.
In 4-dimensional QFT it is the holographic projection which recycles the original ambient spacetime indexing of quantum matter into a new indexing which associated to the causal horizon of the latter (the algebraic substrat is preserved but its spacetime indexing is radically changed). In the latter the original bulk matter appears as a transversely extended chiral matter (and in general the inversion will not be unique without additional information). The ambient quantum matter fluctuates (vacuum polarization) at the causal horizon of the ambient localization (half the lightfront in case of a wedge, the lower mantle of a double cone in case of a double cone localization) wheras the holograpic projection fluctuates only in lightray direction at the boundary of the lightray extension. If one's main interest is to study extensive quantities (energy, entropy) of the localized bulk (caused by vacuum polarizations at the boundary of the bulk) it is easier to calculate after the holographic projection instead of doing this directly with the bulk matter. What remains of the original bulk fluctuation is the vacuum polarization at the end of a lightlike interval. Some results can be found in hep-th/0511291. The role of chiral theories in the recipe for string construction is completely different. It is not appearing in form of a holographic projection but it is there from the beginning in form of a source space of a QFT whose "field-value space" (target space) is the arena of the quantum matter, except that it lacks the most important property of direct target quantum fluctuation. The only fluctation is that of chiral source theory (factorially) repeated 24 or 10 times (note that in the QFT holography the chiral theory is part of the spacetime-indexed original ambient matter). In this way deep and very interesting mathematics of modular forms is processed into metaphorical physics. It is more or less evident that a mathematical formalism whose physical interpretation is metaphorical instead of autonomous may activate a messianic expectation. It is therefore not surprising that in a recent article of a mathematician (hep-th/0601035) one finds the following remakable statement: "The rising number of string theorists is a good indicator of confidence that string theory (or, at least, the existence of supersymmetry) will be confirmed in accelerator experiments and in astronomical observations and that this can happen pretty soon". It is also not surprising that the foot-soldiers of such a metaphoric science become fundamentalistic. The academic version of cutting off the head of an infidel seems to renounce the hospitality to anybody whose critical remarks put some of the metaphors into question
(http://rivelles.blogspot.com/2006/03/ideology-sociology-and-psychology-of.html). This also shows plainly why John Horgan's terminology of calling the post string era of metaphoric physics "ironic science" does not fit this new reality. The foot soldiers of this new particle physics fundamentalism cannot be accused of any ironic attitude whatsoever.

Recently I sometimes asked myself whether Witten had any idea what strange monoculture his string theoretic creation would develop into. The only final logical step for a seemingly irrevocably metaphorical theory is that of Susskind and Polchinski and it would be interesting to know what holds Witten (and Gross) back from taking that ultimate anthropic step.

24. **JC**
April 16, 2006

Bert,

In principle, can the algebraic qft (aqft) framework derive an exact non-perturbative S-matrix solution for theories represented by, say:

– real scalar fields with a phi^4 quartic interaction in d=1+1 spacetime dimensions?
– a Thirring model with a four-Fermi interaction in d=1+1 spacetime dimensions?

(It would be impressive if this can be done easily in practice for other theories like phi^4 theory and Yang-Mills in d=3+1, or any number of spacetime dimensions).

At this point I don't know whether I should be impressed that aqft can derive exact S-matrix solutions for some special cases in d=1+1, corresponding to elastic scattering with no particle production.

In your paper hep-th/0003243, you show that theories in greater than 2 dimensions have a trivial S-matrix if tempered polarization-free generators (PFG) are used, and that theories in d=1+1 have no particle production if tempered PFG's are used. For a theory representing real scalar fields with a phi^4 quartic interaction in d=3+1, what special properties would the PFG's have to possess such that the S-Matrix is nontrivial and agrees with the ordinary perturbative qft results?

25. **Aaron Bergman**
April 16, 2006

If one produced a genuine anything for phi^4 in 3+1D without a cutoff, that would be worrisome. As I remember it, that theory has a Landau pole.

26. **Bert Schroer**
April 17, 2006

The simplicity of the family of theories which correspond to tempered vacuum-polarization-free generators of wedge-localized algebras is their algebraic Zamolodchikov-Faddeev algebra structure. The S-matrix appears simply as coefficients in that algebra. The double cone localized operators have already the full infinite vacuum polarization clouds (visible in the explicitly computed formfactors). The correlation functions are already extremely rich and complicated. Yes you should be impressed; no other approach has been able to achieve this. The massive Thirring model (Sine-Gordon) is among these models. But in general this approach does not start with Lagrangian names. It does not know what the name phi to the fourth power means because it is conceptually totally different and that name has no intrinsic meaning. Perturbation theory indicates that the latter has particle production and is not factorizing.
Outside factorizing theories wedge generators have a much more difficult structure and the aim would be to come to a perturbative understanding. This perturbation theory is expected to show the true frontier between sensible (alias renormalizable) and nonsensic theories. One expects that any theory which was renormalizable in the standard perturbation theory of pointlike Lagrangian field will also remain sensible in the new setting.

27. **Brett**
April 17, 2006

This discussion has raised an intriguing question in my mind. Normally, in QFT, there is an extremely unattractive division of how the fundamental parameters that describe the theory are introduced. Most free parameters are introduced in the Lagrangian, but that alone does not determine the physics, except in trivial cases. In sufficiently “nice” theories (e.g. anomaly-free gauge theories), the regulators that can be used are severely restricted, and those available all give equivalent results. (In contradistinction, in an anomalous gauge theory, there are generally no good regulators to use.) In this case, the regulator can be (almost) banished from the physics, and this is just perturbative renormalizability.

In phi^4 theory, much the same thing holds perturbatively, but nonperturbatively there’s a problem. It would be very interesting in AQFT gave a nonperturbative definition of this theory, with a cutoff, that did not separate the interaction and the regulator the way we ordinarily do. As a test of whether this kind of thing may be possible, I suggest looking for theories in 1+1 dimensions that look like the chiral Schwinger model, because that model is exactly soluble, but there is a regulator dependence in the final spectrum. If the regulator parameter could be encoded in the same way as the coupling in this theory, it would be quite interesting.

28. **Bert Schroer**
April 17, 2006

From a perturbative approach for wedge generators one does not expect anything new in case the models were renormalizable in the standard sense. But it could be that some of the models which one presently discards as nonrenormalizable may actually lead to perturbative expressions with a finite number of physical parameters. In any case these are presently pure speculations since such a “on-shell” perturbation theory has not been formulated and hence the approach based on modular localization remains limited to factorizing models.
I would like to return to the issue of functional integration since there seems to be a widespread misconception. Of course there are many quantum mechanical models for which the path integral representation has a mathematical meaning in terms of rigorous measure theory. However this does not mean that one can perform explicit analytic calculations. Take the hydrogen atom. Even though the functional representation makes sense it is not known how the important property of integrability (which makes the problem operator-solvable) is encoded in the integral. Since one knows the result from standard quantum mechanics one can of course add recipes to the functional integral which will lead to the known result; but this can hardly be called mathematical physics and is just a game of personal entertainment.
The fact is that besides interaction-free systems and camouflaged free systems (as the Schwinger model) there is no known case where you can base exact analytic calculations on functional integrals. Nevertheless the functional integral is useful for quasiclassical approximations and for perturbation theory and (as a result of its superficial geometric classical appearance) has a strong intuitive appeal.
In a previous remark I said that the functional integral in QFT is a bit deceiving because when you want to compute explicitly renormalized correlation functions the formal elegance of that functional expression on a piece of paper is of not of much use; you have to make your hands dirty and add a lot of additional wisdom and tricks which the elegant formula did at most “morally” (I used the word artistic) contain but not factually. The algebraic approach is more honest and more equilibrated. You list the properties you want (taking account of the singular nature of pointlike fields) and what you get at the end fulfills precisely your initial requirements. In the functional approach to QFT this is not quite so (as I previously explained).

29. **JC**
April 17, 2006

Bert,

(Silly question).

Is there a well defined “limit” in the aqft approach which reproduces the functional integral for a system like the Thirring model (for example)?

30. **Bert Schroer**
April 18, 2006

JC,
Not that I know. For the Euler-Lagrange equation of the Thirring model (massless or massive) the nonlinear term can be defined in terms of a Schwinger point-split limiting procedure. But a Lagrangian action is not really part of the quantum world (it is conceptually somewhere between classical and quantum) and it is unclear what such point-split formulation (related to operator product expansion) means in such a measure-theoretical classical setting.

31. **Robert**
April 18, 2006

I have only glanced over Schroer’s notes briefly, but there seems to be a point that in the algebraic setting (causal perturbation theory to be precise) one can do the perturbation at the algebraic level and only as a second step consider the representation theory in terms of states (i.e. linear, positive, normed functionals on the algebra of observables). Does Schroer claim this is not possible in the path integral setting? I always thought, the state (important especially in a curved background where you don’t have a prefered Minkowsky vacuum) is encoded in the path integral in the boundary conditions of the fields you integrate over. But as always, I may be wrong.

32. **JC**
April 18, 2006

Bert,

In the aqft framework, how exactly is the S-matrix a “purely” quantum object (as opposed to being between classical and quantum)? Do you mean that the aqft derived S-Matrix is the unique solution of a well defined inverse-scattering problem?

Are there any rigorous proofs which show that for a particular set of asymptotic scattering data (ie. incoming and outgoing particle states, crossing symmetries, number of spacetime dimensions, etc …), there exists a unique S-matrix in the aqft framework?

33. **Bert Schroer**
April 18, 2006

Robert,
functional integral representations require for mathematical reasons a Euclidean setting, but generic QFT in CST is totally outside a Osterwalder-Schrader setting. If it would not be outside then the state information would indeed be expected to reside in the boundary condition because this is the only freedom at one’s disposal.

34. **Bert Schroer**
April 19, 2006

JC,
it is only for factorizing models that the bootstrap construction of the S-matrix can be separated from QFT (which then can be constructed in a second step via the wedge generators). In the general case the S-matrix and the generators have to be constructed together in an iterative manner (as a scenario this was explained in m]one of my papers) but there are yet no concrete results. The reason why the S-matrix plays such an important role in nonperturbative construction is that in addition to its well-known role in scattering theory it determined the modular localization structure of wedge algebras (this is relatively new).
The inverse scattering problem for a given S-matrix with all the crossing and unitarity requirements has indeed a unique associated QFT if one assumes that formfactors fulfil the crossing property (this is true for factorizing models but it has not been derived in the generality one needs it from the principles of QFT).

35. **Alejandro Rivero**
May 5, 2006

For AQFT, I was very afraid that after the retiring of Haag it was going to be a dead field; Araki was active but very busy into politics of the IAMP sociery, and note that also Doplicher did a small jump to noncommutative spacetimes via his energy bkackholing relationship, so if he did not left the field, at least he put it in the backburner place. In Gottingen, Borchers did some continuity but very restricted to 1+1 field theory.

When student, one day I sneaked into Luruper Chausse theoretical library and sit in a small working table. There scattered on the table there were the manuscripts of all the papers Haag had received for editorial task in Comm Math Phys and similar journals; some assistent was surely ordering them for final archival.

In later years I have remembered this as a signal of exhaustion of the field too, to my own regret. I think a resuscitation will not be possible without (until?) reaching a depper understanding of the C* geometric work: non commutative manifolds, tangent groupoids, Hoft thingies, etc…

36. **Bert Schroer**
May 5, 2006

Alejandro Rivero,
The scientific situation is not quite as bleak.
There are new and very exciting ideas in the offing. If everything goes well we can hope for a second cataclysm (the first one was of course renormalized perturbation theory). As was the case already with renormalization theory, it will strengthen, deepen and significantly extend the old principles. The name "field" in QFT will more refer to its historical origin than to the new conceptual content.
The high mathematical and conceptual barriers you are mentioning are not necessarily an impediment. They maintain high standards and prevent that sociological effect which results if too many physicists are undergoing a bose-condensation along the lines of a monoculture. I am by no means elitist and anti-democratic but if too many researchers work on one problem this tends to create confusion and regress instead of progress just as you see if you follow the blogs on string theory in this weblog.
Mathematics is of course very important but is should never be allowed to run amok; the tuning between particle physics and mathematics is a very fine one and certainly I would be the last to plead for a return to a mathematical stone age in physics. Physical concepts should be always in the helm and the best situation is if they tell you what kind of mathematics is best for them.
Naturally if you work under such conditions there are very strong restrictions, not only from experiments but also (and this is particularly serious for theoreticians) with past concepts and principles. It is very far away from the "everything goes" maxime of string theory. This kind of conceptually-geared work needs more time and is certainly in a strong tension with the high impact index attributed to the fashionable subjects.
The question of whether there will be a future of particle physics which is worthy of its past depends presently very much on whether young intelligent, courageous and innovative young theoreticians in particle physics will find enough time to test their innovative ideas on past achievements or whether they will be "abgewickelt" (to borrow a German word which was very popular when the "Wessies" took over the academic institutions of the GDR) before they can accomplish this task and replaced by string theorists with a higher production efficiency.

The Jasons

April 15, 2006
Categories: Book Reviews, Uncategorized

One of the less well-known parts of the history of particle physics is the involvement of many prominent theorists in research (often classified) conducted for the U.S. military through an organization known as "Jason". My advisor at Princeton (Curt Callan) would disappear for a couple months each year to La Jolla and I remember hearing about Jason from various people back then. Unlike at Harvard, quite a few of the faculty at Princeton from those days were involved with Jason at one time or another (besides Callan these included Sam Treiman, Freeman Dyson, Roger Dashen, Val Fitch and Will Happer), and this showed itself in various ways, including an unusual degree of interest among Princeton physics professors in the question of how sound propagates in the ocean.

There's a new book out about the group, written by Ann Finkheimer and entitled [The Jasons: The Secret History of Science's Postwar Elite](). It's based on many interviews with Jasons, and tells the story of the group very much from their point of view.

Jason was founded in 1959, with funding from ARPA (now DARPA, Defense Advanced Research Projects Agency), and was nominally associated with IDA (Institute for Defense Analyses). It followed on various other attempts to set up theorists as consultants to the defense department, attempts whose organizers included Wheeler and Wigner. Charles Townes was largely responsible for starting Jason, but its first chairman was Murph Goldberger, and it was his wife who gave the group its name (based on Jason and the Argonauts, in search of the golden fleece). Murray Gell-Mann was a member of the initial steering committee.

Members of Jason gather each year for a summer session of working on various projects, some involving classified military research, some not. About half the reports they generate are unclassified. For a selection of these, and to get some idea of the sort of thing they work on, see here. In recent years the group has branched out to study many topics involving biology, and to include many non-physicists (mathematician Fields medalist Michael Freedman is rumored to have been a member).

In 2002 DARPA stopped funding Jason, in a fight over an attempt by DARPA to impose some new members on the group that they didn't want. This led to the group getting a new funding source: DDR&E, the umbrella for all defense research.

Over the years Jason has worked on many different topics, including anti-submarine warfare (thus the interest in how sound travels in the ocean), ballistic missile defense, adaptive optics and many, many others. It was most controversial during the Vietnam war, when as many as nine Jasons (including Sam Treiman and Steven Weinberg) resigned for a variety of reasons, from moral objections to the war to feelings that they were not doing anything effective.

The Finkbeiner book doesn't really do justice to the difficult moral issues involved in Jason's activities during the Vietnam War years. One early Jason report on Tactical Nuclear Weapons in Southeast Asia, by four authors including Freeman Dyson and Steven Weinberg, reached the rather obvious conclusion that the use of nuclear weapons in guerilla warfare wasn't a very good idea. It's not clear who if anyone at the Pentagon thought otherwise. For extensive background about this, see here.

Much of Jason's activity during the Vietnam War involved attempts to set up an electronic barrier to stop the North Vietnamese from infiltrating troops and supplies to the South. This was partly motivated by the fact that it had become clear to the military that bombing the North wasn't working, something that Jason knew, but was not revealed to the American people until the release of the Pentagon papers by Daniel Ellsberg. As part of the electronic barrier effort, Murray Gell-Mann spent time in the jungle in Panama testing out various pieces of equipment. For a very different perspective on the question of Jason and Vietnam from that of the Finkheimer book, see the 1972 article The Story of Jason from the web-site of Charlie Schwartz at Berkeley.

Many of Jason's most successful reports over the years have played the role of shooting down a bad idea (like nuclear weapons as a counter-insurgency tool). For a recent example, see the Hafnium bomb, which is the subject of a forthcoming book entitled Imaginary Weapons : A Journey Through the Pentagon's Scientific Underworld. It's unclear what Jason's current activities in classified military research consist of, but presumably counter-terrorism and how to fight insurgents in Iraq are two important topics. Despite the clear analogies with Vietnam, the war in Iraq has so far been a much less contentious issue in the U.S. One hopes that those physicists involved in helping the government pursue it will do better this time than the previous time around.

Update: There's a review of the book by John Horgan in this week's New York Times Book Review.

Update: For a right-wing ideologue review of the book, see the New York Sun, where the reviewer seems to believe the only problem with the Vietnam war was the "contemptible" people opposing it, "the ideologues on the left, who were busily dismantling the nation's colleges and universities at that time."

Comments

1. **Tony Smith**
   April 15, 2006

   With respect to DARPA's 2002 decision to stop funding Jason, a 13 May 2002 Mercury News article by Jim Puzzanghera at
   http://lists.jammed.com/ISN/2002/05/0090.html said in part:
   "… The dispute, according to members of Jason, stems from an attempt by the director of the Defense Advanced Research Projects Agency, known as DARPA, to force the traditionally self-selecting group to accept three members. Among the three are two executives from Silicon Valley, one from an Internet-related company and another from a computer firm, said one member of the group, who, like other Jason members, declined to name the individuals. The third person is an engineer from the Washington, D.C., area. The Jasons … said the three did not meet the group's rigid standards, which include having significant research accomplishments, being a tenured professor at a research university and being willing to commit to a lengthy annual summer research session. When the group refused to accept the three earlier this year, DARPA revoked its $1.5 million annual funding, Jason members said. …
   DARPA Director Tony Tether declined to comment on the dispute. Agency media officer Jan Walker … said the reason DARPA ended its financial support for the group was because Jason failed to adapt to the times. … Walker said … 'After the Cold War ended, a lot of the technology development moved toward information technology, and the Jasons chose not to lose their physics orientation to focus on DARPA's current needs.' …
   Steven Koonin … chair of … Jason['s] … steering committee … said … 'We still write reports
   that have equations in them. I don't think there's any other group that does that …' …".

   If Koonin's 2002 statement about equations is true, then I guess a Dark Age had, by 2002, already come to the USA.

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

2. **knotted string**
   April 15, 2006

   That report where they analyse the use of fallout radiation to form a 200-mile barrier to stop Vietcong attacks is a bit horrifying.

Hans Bethe was advocating clean nuclear weapons in March 1958, http://worf.eh.doe.gov/data/ihp1b/7374_.pdf page 9:

"The use of clean weapons in strategic situations may be indicated in order to protect the local population."

They detonated a 95% clean 5 megaton "Navajo" at Bikini Atoll on 11 July 1956 and then 9 megaton "Poplar" – after Bethe's positive report – on 12 July 1958. Low fallout data for "Navajo" shot: http://worf.eh.doe.gov/data/ihp1c/0881_a.pdf

There's a conflict between the scientific humanitarian approach and the military tactics of using napalm and agent orange in Vietnam. In Iraq they used depleted uranium shells. Is anybody interested in reducing the potential impact of weapons on civilians anymore?

3. **secret milkshake**
April 15, 2006

1. Clean thermonuclear weapon research was politicaly motivated. It was used as justification for founding the second lab (Livermore) that was competing with Los Alamos at the time. Livermore was sold on the premise of innovative designs and the clean hydrogen bomb was supposed to be one of them. In reality any megaton explosion produces a horrible falout.

It soon became obvious that it is difficult to miniaturise the clean thermonuclear bomb enough to fit on top of balistic or cruise missile. Clean bombs have the natural uranium (or low-enriched) uranium jacket replaced with lead. The rule of thumb is that they have about 50% yield and at least 150% weight of the similar design with the uranium jacket. The use of "clean" nukes in civil engineering (digging ports, re-vigorating exhausted natural gas fields) did not arrive and army did not like clean nukes anyway – for deterrent purpose, the produced falout is "added bonus'

2. The Weinberg-Dyson report against using nukes in Vietnam basicaly said "we would not achieve much by nuking the trail, the collateral damage (need to evacuate all civilians from the huge contaminated area) would be excessive and if someone gave a small nuke to Viet-Cong, our troops would be much more vulnerable to nuking than VC were to our nuking because we have large military bases and they do not."
It is not clear if Russians or Chinese would have given a nuke to Viet-Cong, in retrospect, so this last part of argument seems iffy.

4. **knotted string**
April 16, 2006

Thanks for clarifying the weight increase/ yield reduction issue with Bethe's project.

Why didn't Weinberg's electronic barrier work?

5. **Tony Smith**
April 16, 2006

As to Jason's "electronic barrier to stop the North Vietnamese from infiltrating troops and supplies to the South" and knotted string's question "Why didn't Weinberg's electronic barrier work?",
I can give an anecdotal explanation based on what I saw when I was at Ga Tech (superannuated grad student) during the Atlanta Olympics. The Olympic Village was in the center of Ga Tech. It was totally surrounded by a fence with electronic sensors with only a few well-guarded points of entry. The security force was USA Army Rangers, who were without exception very efficient, effective, courteous, and polite.
A friend of mine and I were walking near the fence (outside) and one of us inadvertently touched the fence. Within seconds there were overhead helicopters and armed ground troops. They saw that we had not realized that the fence (it looked like ordinary chain link) was an electronic sensor system, so they told us it was a sensor fence and please don't touch it again. We said OK, thanked them for their courtesy, and apologized for the inconvenience. They said, think nothing of it, we expected people might inadvertently touch it from time to time, and we have no problem dealing with that. What is driving us crazy is the squirrels.
IIRC, the Vietnam Barrier was never a single united fence (too much "border"), but electronic sensors were placed (often by air drop) on the "boundary". When a sensor went off, air strikes could be sent into the area. They killed a lot of tigers. (the Vietnam sensors sensed animals (including people) by such techniques as "smelling" urine, and mammal urine is mammal urine)

Tony Smith
http://www.valdostamuseum.org/hamsmith/

6. **knotted string**
April 16, 2006

Thanks, Tony. That gives me more respect for what Murray Gell-Mann was working in the Panama jungle.

7. **Lurker**
April 16, 2006

I've gone back and read old posts here to get my bearings. I find myself in Peter's camp as a string theory skeptic, but in Lubos's camp politically.

Knotted string, you said "In Iraq they used depleted uranium shells. Is anybody interested in reducing the potential impact of weapons on civilians anymore?"

No fighting force in history has gone to greater lengths to avoid civilian casualties than the coalition. They could have bombed Fallujah to rubble risking nary one soldier; for humanitarian reasons they went door to door.

Depleted uranium is cheap, hardly radioactive at all, makes great penetrators, and is deployed by armed forces all over the world, friend and foe alike. 18 countries manufacture DU penetrators. Tungsten, on the other hand, is impossible to cast and hard to machine, nonpyrophoric, and imported from China. Depleted uranium is an innocuous substance if you don't breath it or get shot with it. Do you have a better idea than DU? Send it to Rumsfeld.

The late Serge Lang was mentioned. The File is a great read. I corresponded with him about it in the late eighties, and hoped to meet him during a visit he made to Britain where I was at the time, but the scheduling didn't work out.

Best regards,

Lurker

8. **knotted string**
April 17, 2006

Thanks Lurker,

I know that DU is little more dangerous than lead bullets, and in that case there are other worries than long-term lead poisoning.

My cousin, in a British tank regiment, was concerned before his duty in Iraq that he was going to spend each day sitting beside radioactive DU shells. However, as you say it is 'hardly radioactive at all'. The U-238 half life is about the age of the earth. Decays get spread out over a long time so the decays per second – Becquerels – is the number of active atoms divided by the mean life (bigger than half life by a factor of 1/ln2 = 1.44).

I'd be concerned about breathing dusty air after a DU attack, of having a particle lodge in lung tissue and cause lung cancer. Is this risk trivial? Perhaps they should simply have a policy of cleaning up and burying DU hotspots under the topsoil after a war.

Serge Lang's File: McCarthyism from intellectuals at Harvard mentioned, on the Amazon book reviews page, sounds familiar…

9. **Carl Brannen**
April 17, 2006

If DU is such clean material, then how come the gov. is busily cooking up schemes to get rid of it safely?

http://web.ead.anl.gov/uranium/uses/repository/index.cfm
http://web.ead.anl.gov/uranium/faq/mgmt/faq25.cfm

Depleted Uranium is not classified as "low level radioactive waste" even though it is easily sufficiently radioactive to be classified that way. For this reason, US laws have to list it separately. For example:

"The Secretary, at the request of the generator, shall accept for disposal low-level radioactive waste, including depleted uranium if it were ultimately determined to be low-level radioactive waste,"
http://www.law.cornell.edu/uscode/html/uscode42/usc_sec_42_00002297—h011-.html

Pure DU would have radioactivity of 330 nCi/g (12 kBq/g):
http://www.physics.isu.edu/radinf/du.htm

But that figure is misleading because the decay products of U238 are also radioactive. Contrary to common sense, the maximum radioactivity emitted by a pile of recently purified DU peaks several thousand years after purification, not immediately afterwards. The peak radioactivity level is called the "secular equilibrium" level.

This level of radioactivity is actually quite high. For example:

"DOE Mixed Transuranic Waste (MTRU) is waste that has a hazardous component and radioactive elements heavier than Uranium. The radioactivity in the MTRU must be greater than 100 nCi/g and co-mingled with RCRA hazardous constituents."
http://www.epa.gov/radiation/mixed-waste/mw_pg3.htm

In disposing of this stuff by using it in the military, we are doing something that we will definitely be called to repudiate and possibly pay for.

Carl

10. **comentator**
April 17, 2006

Correction to Lurker; Fallujah is a city more o less three quarters the size of New York and even if they want, they could not bomb it to rubble, that could have been impractical and expensive. And they went door to door not for humanitarian reason but because there was no other way to go through a city that size.

11. **secret milkshake**
April 18, 2006

Well, they used white phosphorus there… (And that stuff is way nastier than napalm). Calling it "shake and bake".

12. **Ali Yegulalp**
April 18, 2006

Peter – I didn't realize you were one of Callan's students. What years were you at Princeton? I was there 1990-1995, also working with Curt. I spent a couple of summers in San Diego so I could keep meeting with Curt while he was busy being a Jason. I have to say it was kind of strange pedaling my rusty graudate student bicycle up to the big secuity barrier and telling the security guard over the intercom that Curt Callan was expecting me.

13. **Steve Myers**
April 18, 2006

Back to DARPA & JASON: beyond the tech stuff on electronic fences, etc. — doesn't anyone see anything wrong with these sciencetist – pentagon ties? How much of it amounts to being a dupe or selling your soul for grant money & being intimate with power?

14. **Steve Myers**
April 18, 2006

I just realized I coined a new word with the typo "sciencetist"

15. **sunderpeeche**
April 18, 2006

"…anything wrong with these sciencetist – pentagon ties?" … "selling your soul for grant money & being intimate with power?" I keep seeing statements like this. But if scientists like Oppenheimer had not served on the AEC, who would (help to) formulate nuclear policy after WW2? etc etc Yes, one gets intimate with power. That is a necessary consequence if one is to have a meaningful influence on policy. To be intimate with power for its own sake? I have no doubt that it happens. Probably much more so by non-scientists. And they are better qualified to advise on science/technology/military issues?

16. **woit**
April 18, 2006

Ali,

I was a grad student at Princeton quite a bit before your time: 79-84. Never got a free trip to San Diego. Actually, I heard from various people that later on, Curt was even more involved with Jason than he had been at the time I was a student.

17. **Belizean**
April 18, 2006

The standard joke about JASON is that it stood for July August September October November, because the task of writing up your summer research would invariably extend into the fall semester.

I was not a member, but I briefly worked for a defense contractor with whom they were cooperating. I remember spending a long time working through the torturous math in a JASON report by Roger Dashen concerning submarine wake generation, if I'm remembering correctly. I had to go down to La Jolla to present my own work. Got there, attended a Dashen talk. But, after hearing him, I was greatly relieved that my own presentation was cancelled by some scheduling problem.

18. **Tony Smith**
April 18, 2006

Steve Myers Says: "… Back to DARPA & JASON … doesn't anyone see anything wrong with these sciencetist – pentagon ties? How much of it amounts to being a dupe or selling your soul for grant money & being intimate with power? …".

An AIP web page at http://libserv.aip.org:81/ipac20/ipac.jsp?uri=full=3100001~!5052~!0&profile=newcustom-icos describes a 1987 oral history interview with Matthew Sands (an author of the Feynman Lectures) which description says in part:
"… Reasons for joining JASON; work on anti-submarine warfare, surface ship speed; … getting good data, counter-insurgency, Barrier Study (Robert McNamara), 1966; reasons for leaving JASON, 1969; its influential members; secrecy; relation of JASON work to academic physics work. …".

Does anybody here know what Sands's friend Feynman thought of Jason ?
Is it yet another issue on which Feynman and Gell-Mann had very sharp differences ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

19. **The Easter Bunny**
April 21, 2006

Another viewpoint,
"Thirteen of the nation's most prominent physicists have written a letter to President Bush, calling U.S. plans to reportedly use nuclear weapons against Iran "gravely irresponsible" and warning that such action would have "disastrous consequences for the security of the United States and the world."'
…includes Edward Witten, professor of physics, Institute for Advanced Study and Fields Medalist.
http://www.newswise.com/articles/view/519690/

20. **Pyracantha**
April 23, 2006

Have there ever been any women Jasons?

21. **Peter Woit**
April 23, 2006

Pyracantha,

I think that initially, back in the sixties, there were no women Jasons. More recently there have been some, one who is described in the book is Claire Max, now an astronomer at Santa Cruz.

Susskind Turns Down Templeton Prize

April 18, 2006
Categories: Uncategorized

OK, maybe they haven't offered it to him yet, but over at the Edge web-site, in a comment about John Horgan's recent piece about the Templeton Foundation, Susskind writes:

I don't understand the idea that a convergence between science and religion is taking place. I don't believe in any such convergence. Throwing huge amounts of money at scientists who claim to see such a convergence can only lead to a dangerous blurring of boundaries.

I hereby pledge to refuse any prize for advancing the so called convergence between science and religion.

I missed Susskind's recent public talk here in New York, about his book which the New York Academy of Sciences describes as "revolutionizing the field of physics". There is a podcast recorded just before his talk. He makes his usual points including claiming that the situation of the string theory anthropic landscape is similar to that of Darwin and the theory of evolution. He also claims that anyone who thinks it doesn't have experimental implications is wrong, pointing to Weinberg's "prediction" of the cosmological constant.

Comments

1. **anon**
   April 18, 2006

   Daddy, when I grow up, I want to be like you, the ultimate superhero: a physicist who collects payola from both from the Templeton and the Jason programs at the same time.

2. **Chris Oakley**
   April 18, 2006

   Following Susskind's noble example, I hereby relinquish any claim to the Olympic Gold Medal in Tobogganning for 2005, any claim to the Nobel Prize in Chemistry for 1902, for the Turner Prize in 1986, the Best Dressed IBM Employee for 2000 (mind you, I don't think I had much of a claim on that one anyway, and not just because I never worked for IBM), the Tour de France for 1980 and Miss Trinidad for 1972. All I ask in return is that Lubos Motl should give up any aspirations towards the Nobel Prize for Climate Change, if ever one is instituted.

3. **lmot**
   April 18, 2006

   I've always admired the Jasletons for advancing the convergence between religion and anti-submarine warfare.

4. **Kea**
   April 18, 2006

   Chris, I'm curious: why the date qualifications?

5. **Chris Oakley**
   April 18, 2006

   Chris, I'm curious: why the date qualifications?

   I *knew* that someone was going to read more into this comment than was intended. I suppose that that is the problem of this being a "scientific" forum (although Lubie might disagree about the fact).

6. **Jimbo**
   April 18, 2006

   Listening to Lenny's talk, he sez that Weinberg predicted the CC within an order of magnitude, via anthropic reasoning. Is that indeed the case ? Also, does anyone know if there exists a published derivation of the following relation (and by whom ?)

   Log (Mcc/Mn) = – 1/(alpha)^1/2 , where Mn = nucleon mass

   Thanx,
   Jimbo

7. **woit**
   April 18, 2006

   Jimbo,

   For extensive comments about Weinberg's "prediction" (which is the same "prediction" you get when you say you have no idea what causes the cosmological constant), see my recent posting about Polchinski's paper.

   Never seen the relation you give (and it's off-topic…)

8. **Alejandro Rivero**
   April 18, 2006

   Jimbo, as Mcc was thought to be 0, there are not a lot of logarithmic "predictions" yet. There is a good bunch of such kind (log and then fine structure constant) using M_planck against M_nucleon, at least as early as the 1960 and probably even more. It is offtopic here, but you can get a whole 11 pages thread at http://www.physicsforums.com/showthread.php?t=46055

9. **Alejandro Rivero**
   April 18, 2006

which is the same "prediction" you get when you say you have no idea …

But of course! Note that the anthropic principle is to ask for the existence of someone able to say "I have no idea".

10. **CapitalistImperialistPig**
April 18, 2006

Unlike you high-principled types, I intend to campaign vigorously for the Templeton Prize, based on my string theoretic proof of the existence of God, or, as I like to call him, *The Great Landscaper.*

11. **Sakura-chan**
April 19, 2006

The Susskind talking on that podcast is not the normal Susskind we know and love. I was stricken by his skepticism.

12. **Hech Baan**
April 19, 2006

1. I'm 100% behind Susskind's message. There is no boundary between science and theology. And religion shouldn't play any role in science. You are either a scientist or a theologist.

2. At the same time I don't think his book is revolutionising the field of physics.

3. When people make comments on the Anthropic principle my assumption is that they understand the topic they are talking about. To test my theory I ask the following question.

What would be your reaction if string/M-theory predicts only universes without live forms?

13. **Not a Nobel Laureate**
April 19, 2006

I developed a quantum mechanical "proof" of the existence the Holy Trinity years ago thereby inferring the existence of God.

1. The Holy Trinity can be understood as a superposition of QM eigenstates.

|Holy Trinity>

= alpha|Father>
+ beta|Son>
+ omega|Holy Ghost>

The Unitarians have previously shown that the inner product of the |Holy Trinity> state
with itself gives

alpha^2 + beta^2 + omega^2 = 1

where the eigenstates satisfy the usual orthonormality requirements.

2. The values of alpha, beta and omega in the above normalization may be empirically determined by checking the religious – historical records for the number of appearances of the Father, the Son and the Holy Ghost, respectively.

We will probably have to await further developments in understanding the Celestial Firmament of Holy String Theory a.k.a. Harp Theory or H-Theory to account for these values of alpha, beta and omega. However, leading quantum theological and quantum rabbinical researchers currently believe that the key lies in enumerating the 10^500 possible names of God.

3. When acted up by the Holy Roller Hamiltonian,
H_hro, a.k.a. the God Playing Dice with the Universe Operator, O_gpdu, the QM holy superposition state collapses / is projected into one of the
|Father>, |Son> or |Holy Ghost> eigenstates.
Ie.,

H_hro |Holy Trinity> = alpha|Father>

H_hro |Holy Trinity> = beta|Son>

H_hro |Holy Trinity> = omega|Holy Ghost>

QED

4. An outstanding quantum theological question is whether the H_rho is in fact Hermitian as has been postulated and are the eigenvalues thus positive definate, ie., without original negativity a.k.a. original sin, or not.

5. Many of the Faithful eagerly await the coming of the Second Quantization.

Now can I have my Templeton Prize . . . please.

14. **Jimbo**
April 19, 2006

Dear Not a Nobel Laureate,

Cracked me up…I would love to see Alan Alda do his best `Feynman', deliver that lecture w/a straight face & try not to split a gut laughing. I don't think it can be done ! Thanx for making my day.

Jim

15. **Thomas Larsson**
April 19, 2006

What would be your reaction if string/M-theory predicts only universes without live forms?

I would be most surprised if string/M-theory made **any** falsifiable prediction whatsoever, e.g. that sparticles **will** (not may) be detected at the LHC.

I would also be most surprised if Lubos Motl did not find yet another excuse to extend the deadline of his infamous experimental-discovery-of-susy-by-2006 bet, when sparticles have not been detected by the end of 2008. After all, the ultimate goal of string theory is to find ever more elaborate excuses why every string theory prediction disagrees with experiment.

16. **Hech Baan**
April 19, 2006

Dear Thomas Larsson,

Everyone knows that string/M-theory makes many predictions. The challenge is that we don't have enough data points to check them. So what do we do? Throw away the theory and try another one. We've decided to continue working on the theory to get more results. We assume that our theory is much more clever than we are therefore we will take its predictions very seriously regardless of our expectations and/or non-experts' opinion.

Now back to my question. You haven't answered it. I agree you will be surprised. But what would you do next? What would be your logical conclusion? Will it be?
A. String/M-theory is not even wrong so let's ignore anything it says.
B. String/M-theory therefore I don't exist.
C. String/M-theory says I don't exist, but I do therefore the theory is wrong.
D. Something else.

Hint: The question is linked to the Weinberg's prediction.

17. **Chris Oakley**
April 19, 2006

Hech Baan,

I would choose "A" and just hope that you were not the one marking the paper.

Not a Nobel Laureate,

I remember as a child being told that the Three were One and the One were Three and that that was a Mystery, but I could never see anything particularly mysterious about it. In fact, I would say that your explanation ties up the loose ends rather nicely, and would be happy to support your entry for the Templeton Prize if you want me to. As for my credentials, you will find my CV on my web site; to that I need only add that I was an altar boy at the local Catholic church from about 1967-1974 and a Governor of the local Catholic school (John F Kennedy – named after a famous Catholic philanderer) for about a year around 1986.

18. **Arun**
April 19, 2006

What energy scale does M/String Theory predict for supersymmetry to be effective?

19. **Thomas Larsson**
April 19, 2006

Everyone knows that string/M-theory makes many predictions.

This is of course a plain lie. Unless, of course, you take the predictions of supersymmetry, extra dimensions, and 496 gauge bosons seriously, in which case string theory has been disproven by experiments.

Let me quote Sheldon Glashow:

"It's called superstring theory and it is, so far as I can see, totally divorced from experiment or observation. If not totally divorced, pretty well divorced. They will deny that, these string theorists. They will say, "We predicted the existence of gravity." Well, I knew a lot about gravity before there were any string theorists, so I don't take that as a prediction."

But it is of course up to anyone to decide for him- or herself who is most credible: a Nobel laureate like Glashow or a liar like Hech Baan.

20. **woit**
April 19, 2006

Arun and Hech Baan,

M/String theory makes no prediction about the energy scale of supersymmetry breaking whatsover. It makes no predictions about anything, and anyone who starts going on about its predictions should be able to at least tell us what one of them is.

21. **Wolfgang**
April 19, 2006

Peter,

so what about Lubos' prediction that Lorentz invariance will hold at (arbitrarily) high energies ?

22. **woit**
April 19, 2006

Wolfgang,

Lubos has a highly idiosyncratic idea of what the English word "prediction" means. As for the one you mention, recall that most string theorists like to go on about how really understanding string theory will require a new way of thinking about what space is (and time too, probably). Dumping Minkowski space at short distances would also dump Lorentz invariance, it would seem to me.

Lorentz invariance at short distances is not exactly a distinctive prediction of string theory, and saying one's theory "predicts" some general principle shared by virtually all other theories is kind of silly. For instance, I could claim that "string theory predicts unitarity". Lorentz invariance at short distances is much more a distinctive feature of local QFT than of string theory, so it's not exactly a distinguishing prediction of string theory.

23. **Chris Oakley**
April 19, 2006

A footnote to the Lorentz invariance question: just because you can't deal with a scientific fact it doesn't mean that it is not true. Our understanding of QFT is based on an idea we inherited from quantum mechanics, which in turn was inherited from classical mechanics. This idea is that physics consists primarily of deriving the equations of motion of a given system. The configuration of a system in a constant-time hyperplane (the t=0 state) then is a boundary condition to which the equations of motion are applied. In a relativistic system, the equations of motion need to be covariant, but in addition one must be sure that *any* choice of spacelike hyperplane would work for providing the "t=0" configuration.

The notion of introducing an interaction into a quantum-mechanical system by an equation like

H = H_0 + V

seems so sweetly reasonable to most that they ignore the fact that H is the time component of a four-vector – a very non-relativistic construct. But something has to give when you try to make the theory relativistic, and sure enough, as Rudolf Haag showed in 1955, the only way in which this equation is applicable in this case is if V=0, in other words, when there are no interactions at all. This problem leads some to propose that Lorentz invariance is not correct, flying in the face of some of the most copious experimental evidence ever produced (bubble chamber data).

24. **Not a Nobel Laureate**
April 19, 2006

Chris Oakley wrote:

" . . . In fact, I would say that your explanation ties up the loose ends rather nicely, and would be happy to support your entry for the Templeton Prize if you want me to. . . "

You've just demonstrated why I've always been reluctant to put this "theory" into print – even in the Journal of Irreproducible Results. Someone might actually take it seriously, but then again, there are people who take string "theory" seriously so I guess it should come as no surprise.

When string theory can predict the spectrum of empirically observed particles and forces, as per the SM, then it will be worth paying attention to. Until then, it's just an interesting (at least to some) mathematical toy.

25. **Joao Leao**
April 19, 2006

Peter

As you well point out String Theorists have adopted the "novel" notion that, if they keep "predicting" what we already know, they may may somehow be exempted from having to predict something genuinely new and thus taking a risk on their exalted labors! It is one short step away from argueing that a universe fine-tuned enough to produce String Theorists must be highly contrained to exhibit the "physics" they portend to know!…

Now I, for one, understand why Susskind "predicts" he is a good candidate for the Templeton prize and rightly anticipates that will mean his enthronement as the Pope of the Megaverse (and Weiberg is his Prophet?). This would put one last kabosh on his claims to be doing science rather than some new religion of "constant landscaping".

26. **ksh95**
April 19, 2006

Chris Oakley said:

> the only way in which this equation is applicable in this case is if V=0, in other words, when there are no interactions at all

But nature almost always rearranges itself in such a way as to make V very small. Isn't this what Bogoliubov is all about?

Moreover, any residual interactions can show up as mass.

Change what you call a particle and change what you want to call that particle's mass and SR is preserved.

27. **Bert Schroer**
April 19, 2006

Chris Oakley,
as much as I usually agree with your points of view, I think what you said about relativistic QM and Haag is not correct as it stands. Relativistic QM by its very nature of lacking the vacuum polarization (which characterizes QFT) is a perfectly well-defined relativistic multiparticle theory, i.e. it fulfills all properties which you are able to formulate using the concept of Wigner particles (and not that of fields). It is called the theory of "direct particle interactions". The original idea is due to Fritz Coester (a good friend of Haag) and I have reviewed it in hep-th/0405105 (published in AOP).

28. **biophysicist**
April 19, 2006

Actually not a Nobel laureate you missed out on the fact that you can get the amplitudes exactly in your formulation. Since beta = alpha = omega ("I am the Alpha and the Omega", remember?), unitarity forces the amplitudes to all be 1/3.

Cheers!

29. **biophysicist**
April 19, 2006

err 1/sqrt(3). Whatever.

30. **Lord**
April 19, 2006

I don't understand the idea that a convergence between science and religion is taking place. I don't believe in any such convergence.

Religion informed by science would be a marked improvement over the current situation, and ethical considerations are certainly a valuable contribution to science.

31. **Eugene Stefanovich**
April 19, 2006

Bert Schroer wrote:

"…Relativistic QM by its very nature of lacking the vacuum polarization (which characterizes QFT) is a perfectly well-defined relativistic multiparticle theory, i.e. it fulfills all properties which you are able to formulate using the concept of Wigner particles (and not that of fields). It is called the theory of "direct particle interactions"…"

Exactly. Furthermore, using a unitary transformation (nucl-th/0102037) one can eliminate vacuum polarization terms from QFT and cast it into the form of "direct interaction" theory without losing its predictive power , i.e., the S-matrix remains the same. In this formulation, the Haag's theorem does not apply anymore. See physics/0504062.

32. **Not a Nobel Laureate**
April 19, 2006

biophysicist wrote

"Actually Not a Nobel laureate you missed out on the fact that you can get the amplitudes exactly in your formulation. Since beta = alpha = omega ("I am the Alpha and the Omega", remember?),"

Hint. Why do you think I choose the names of the variables I did 😉

It's a matter of some considerable debate amongst quantum theologists as to whether "Alpha and Omega" should be interpreted as alpha + omega. Also, the fact that Beta is not mentioned – is this a Divine Hint of a hidden variable theory or an extra dimension in the Celestial Firmament?

"unitarity forces the amplitudes to all be 1/3."

That would be 1/sqrt(3) as you corrected later, but the above quantum theological question stills awaits a definative ruling by the quantum theologists and quantum talmudists.

33. **Bert Schroer**
April 19, 2006

Eugene Stefanovich,
really very interesting. When I wrote my review I was not aware that this conceptionally correct phenomenological setting was known outside of nuclear physics. I do not know much about its practical usefulness, but its philosophical implication is quite interesting. Among other things it shows that Weinberg's statement that a relativistic theory which obeys the cluster factorization property must be a QFT is not correct. It is the (up to now) only successful physically consistent (macrocausal) nonlocal relativistic quantum theory I know. All other attempts especially those based on noncommutative modifications of QFT have not (yet?) been shown to be physically consistent (wrecking micro-causality usually also destroys macro-causality).

34. **Lubos Motl**
April 19, 2006

Peter: Of course that the Lorentz symmetry at the string scale is a prediction of string theory, much like unitarity, and it is not shared by any "competing" theory. For example, loop quantum gravity is neither Lorentz-symmetric nor unitary. This is why its proponents offer you long essays explaining that we don't really need unitarity, and they also offer you bizarre DSR (defective special relativity) predictions about the Lorentz symmetry violations etc. These predictions show how greatly predictive the theory is – this glory will last until the day when it's shown that this prediction is, of course, silly.

It is fair to say that string theory does predict that these effects are absent and unitarity is exact – and string theory is also the only theory of quantum gravity that makes this prediction. Concerning the word "prediction", Peter, I would also like to tell you and your crackpot fans (plus Wolfgang who is an extra higher category) that I not only know what it is in English, but I can also write it in the normal language: předpověď. 🙂

35. **Eugene Stefanovich**
April 19, 2006

To Bert Schroer:

Weinberg's statement is definitely not correct. In addition to the works of Coester and Polyzou mentioned in your review, there are other counterexamples worth mentioning:

1. non-local QFT: M. I. Shirokov, "On relativistic nonlocal quantum field theory", Int. J. Theor. Phys. 41 (2002), 1027

2. A beautiful series of papers by H. Kita in 1966-1973. The last paper in this series is "A realistic model of convergent quantum mechanics of interacting particles" Prog. Theor. Phys. 49 (1973), 1704 where you can pick up the other references if interested.

36. **Hech Baan**
April 19, 2006

Chris Oakley,

Wrong answer and I'm glad you are not contributing to science any more.

Thomas Larsson,

Any mathematically consistent theory makes many predictions. Sheldon Glashow is just saying that we don't have enough data points to verify the predictions. This is exactly the point I made.

Peter Woit,

String/M-theory predicts that we all consist of strings, including people who don't appreciate the theory.

37. **Simon**
April 19, 2006

Hech Baan: That last statement brings to mind a bumper sticker I saw once…
"Jesus loves you, even if you don't want him to"

At the moment string theory can only believed on faith… it is beautiful, has a lot of desired properties, but is not scientifically confirmed.

I found the bumper sticker offensive… I'm trying not to take your comment the same way!

38. **Hech Baan**
April 19, 2006

Simon,

I'm not saying that string/M-theory is confirmed. The theory could be correct or wrong. However, it's more than believe on faith and it does make predictions. But unfortunately we are not in a position to verify them yet.

The last statement is in line with what Nils Bohr said about his horseshoe on the wall: "Of course, I don´t believe in it! But I have been told it would help even if you don´t believe in it!"

There is no intention to offend anyone.

39. **woit**
April 19, 2006

Hech Baan,

I notice that you love to keep repeating that string theory makes predictions, but you can't actually come up with one. Either give us a legitimate scientific prediction that comes from string theory, or stop attacking others and repeating nonsense.

40. **Hech Baan**
April 19, 2006

Peter Woit,

The following are some legitimate scientific predictions: graviton, supersymmetry, extra dimensions, changes in the topology of spacetime.

41. **Not a Nobel Laureate**
April 20, 2006

Lubos Motl Says:

April 19th, 2006 at 6:33 pm

"Concerning the word "prediction", Peter, I would also like to tell you and your crackpot fans (plus Wolfgang who is an extra higher category) that I not only know what it is in English, but I can also write it in the normal language: předpověď."

From your postings, it strikes me that in an earlier time you probably would have been an equally vicious proponent of "Scientific Communism".

It's no accident. Both theories have equal merit in their claims of being scientific and "Scientific Communism" at least made predictions about the development of society which turned out to be wrong over time.

Previous fads such as hadronic string theory, Regge poles, S matrix and bootstrap models were blown away by experiment which in turn lead to better theories and predictions. String theory is apparently in no such danger, thus it's proponents can continue to hold strong opinions and mis-represent them as facts.

Unless 1) the LHC discovers something new; and 2) more economical means of particle acceleration are developed, the field will sink into obscurity or form the basis of a new religion.

42. **Not a Nobel Laureate**
April 20, 2006

On the other hand, every other field of physics that I can think of apprears to be enjoying remarkable experimental and theoretical progress.

43. **Chris Oakley**
April 20, 2006

Hech Baan's predictions of String Theory

Graviton: never observed
Supersymmetry: wrong
Extra dimensions: no evidence
Changes in the topology of spacetime: no reason to believe this

44. **where?**
April 20, 2006

about Baan's string predictions: how heavy are supersymmetric particles? How large are extra dimensions?

As far as I know, string 'predictions' have an uncertainty of about 16 orders of magnitude. A planet is about 16 orders of magnitude bigger than an atom. How can you do atomic physics if you cannot distinuigh an atom from a planet?

45. **knotted string**
April 20, 2006

I love string theory. It has enough branes behind it to cover all possibilities, so it's a safe bet. It's can't be disproved!

46. **Thomas Larsson**
April 20, 2006

Let me quote from subsection 1.6 of hep-th/0204131:

"The long-standing crisis of string theory is its complete failure to explain or predict any large distance physics. String theory cannot say anything definite about large distance physics. String theory is incapable of determining the dimension, geometry, particle spectrum and coupling constants of macroscopic spacetime. String theory cannot give any definite explanations of existing knowledge of the real world and cannot make any definite predictions. The reliability of string theory cannot be evaluated, much less established. String theory has no credibility as a candidate theory of physics."

It might be worth pointing out that Dan Friedan was one of the leading string theorists during the 1980s, founder of the string theory group at Rutgers and winner of the MacArthur "genius" grant. But then again, why would anyone care about his opinion, when the great authority Hech Baan proclaims that string theory makes many predictions? After all, Baan has succeeded (probably) in getting into grad school, and has published exactly zero papers in hep-th. (I found a W.A. Baan with ten e-prints in astro-ph, but that is probably another person).

Let me emphasize that it is nothing wrong if a speculative science makes no predictions in early stages. However, with 20,000+ man-years invested in string theory, it is hardly in an early stage, and the antropic nonsense shows that nobody seriously thinks that it is possible to change this situation. What I do loathe, however, is the ridiculous lie that string theory is predictive. This is a gross misrepresentation, verging on scientific fraud.

Incidentally, I met Friedan in a summer school on Iceland in 1990 (Friedan's wife is Icelandic, although she did her Ph.D. in Lars Brink's group in Göteborg). I travelled with a mathematician friend, who was very enthusiastic about Friedan's recent work, with Steve Shenker, on CFT on a Riemann surface. Friedan himself was quite subdued, however. He refered to his work as "very abstract", and it was clear that he didn't mean it in a positive sense. Soon after that Friedan disappeared from the literature for ten years, and we all know how that ended.

47. **woit**
April 20, 2006

Hech Baan,

None of those are legitimate scientific predictions that can be used to test string theory (even in principle).

Gravitons are what you generically expect in any quantized version of gravity, even if you could observe them, this wouldn't be any evidence for string theory.

Unbroken supersymmetry is experimentally ruled out, broken supersymmetry depends on an energy scale, and string theory tells you nothing about this, so it makes no predictions about supersymmetry at all.

String theory may or may not require 10 (or is it 11?) dimensions, depending on how you feel about non-critical strings. But it tells you virtually nothing about any of these dimensions, their sizes, shapes, etc. Their is no experimentally testable (even in principle) prediction about extra dimensions from string theory.

String theory does not tell you what the topology of space time is, much less how it will change.

Please stop repeating things you clearly don't understand, you're just adding to the noise level here.

48. **Christine Dantas**
April 20, 2006

Peter Woit wrote:

[String theory] makes no predictions about anything, and anyone who starts going on about its predictions should be able to at least tell us what one of them is.

I think I have quoted this here before, perhaps it would be frutifull to quote it again (gr-qc/0501053):

> String theory schemes in any dimensions always predict new fields which couple in different ways to the various matter sectors. As a result a large variety of effects are allowed by string theory, including effects leading to violation of Lorentz invariance, violation of the UFF and violation of the Universality of the Gravitational Redshift (UGR) is encountered. From the viewpoint of the phenomenologist a disappointing aspect of string theory is that it appears that it cannot be falsified on the basis of these effects. String theory may predict many new low-energy effects, but it can also be easily tuned to avoid all of them.

49. **Christine Dantas**
April 20, 2006

I did it again. I should be more careful to avoid typos. Sorry for the "frutifull" (–> fruitful).

50. **Zelah**
April 20, 2006

Let's get real guys. Everybody here agrees that QFT is the best theory of high energy physics we have!

And frankly nobody has any idea of how to unify QM and Gravity such that low energy physics could test the results.

Now regarding your question Hech Baan, I would reply D. The reason is that String Theory is clearly valid in some sense as a QFT!

Unfortunately, your linking Weinberg prediction of cc scale to your question depends apon your assumptions regarding how science works! Put it this way, there are many papers which predict various values for the CC!

An amateur mathematician.

51. **Who**
April 20, 2006

Christine, I often find your comments helpful but in this case I can not make sense of your quote from
http://arxiv.org/abs/gr-qc/0501053
the paper by Giovanni Amelino-Camelia et al.
The quote is not self-consistent. If a theory "can easily be tuned" to avoid making a prediction then it simply does NOT make the prediction.

The point of restricting scientific theories to ones which make testable predictions is that the theory must bet its life on a future observation or measurement so that it can be falsified and discarded.

the question one asks of a theorist is **what future observation, what specific result of a specific measurement, would make you give up your theory and change fields?**. If a string theorist can not answer that question then string theory does not make any predictions in the empirical science sense.

It is not like the "predictions" that people make at New Years which are more vague and informal, really just guesses. A scientific prediction STAKES THE THEORY.

As an astrophysicist, I am sure that you know this! So how do you interpret the passage you quoted from Amelino-Camelia? To me it is self-contradictory. What am I missing?

I suppose the quote could be taken as confirming what Peter said. Did you mean it as corroboration? I really am confused about what you are driving at, please be a little more explicit!
=================================

Zelah said "And frankly nobody has any idea of how to unify QM and Gravity such that low energy physics could test the results."

That is not correct Zelah. Many papers. Here is a recent one you can check out by Shahn Majid (noncommutative geometry, Cambridge)
http://arxiv.org/abs/hep-th/0604130
Algebraic approach to quantum gravity II: noncommutative spacetime
S. Majid
26 pages, 2 figures; book chapter to appear in D. Oriti, ed., Cambridge Univ. Press

"We provide a self-contained introduction to the quantum group approach to noncommutative geometry as the next-to-classical effective geometry that might be expected from any successful quantum gravity theory…"

Majid has something to say which is FALSIFIABLE, and it would seem rather interesting, about putting QM and spacetime geometry together.

People should stop applying the sloppy model of string theory indiscriminately to other QG fields, which ARE turning out to be predictive.

—quote Majid second paragraph page 2—
This is also the first noncommutative spacetime model with a genuine physical prediction[1],… The NASA GLAST satellite to be launched in 2007 may among other things be able to test this prediction through a statistical analysis of gamma-ray bursts even in the worst case that we might expect for the parameter …
—endquote—

Majid's idea is quite conceivably wrong, in which case measurements by the NASA satellite will refute the idea. Either way, win or lose, it's progress. (keep your fingers crossed that GLAST goes up as planned)

Please not to suggest that non-string QG theories fail to be testable at accessible energy (in this case it is Gammaray Burst energy)

52. **Erik**
April 20, 2006

I love string theory. It has enough branes behind it to cover all possibilities, so it's a safe bet. It's can't be disproved!
LOL! That convinced me, I'm joining the string theory pack! I can't loose!

53. **Hech Baan**
April 20, 2006

Peter Woit,

Graviton:
String/M-theory with gravitons incorporates the full structure of Einstein's theory. At the atomic level, it departs from Einstein's, which doesn't work quantum mechanically. The nature of the departure is unique to the string/M-theory. So, the prediction isn't just the existence of the gravitons but their unique properties.

Supersymmetry:
String/M-theory's prediction is that there should be supersymmetry. Supersymmetry predicts that for every elementary particle there is a superpartner particle. It makes detailed predictions about how superpartners will behave.

Extra dimensions:
String/M-theory predicts that there are extra dimensions with unique features. It doesn't tell you everything about extra dimensions' sizes, shapes, etc. as Einstein's theory doesn't about spacetime's. E.g. the topology of empty spacetime in Einstein's theory can be R^4, RxTor^3, R^2xTor^2, etc..?

Changes in the topology of spacetime:
Once again Einstein's theory doesn't tell what the topology of spacetime is either, but it tells you that whatever topology is it won't change. String/M-theory predicts that it will change in distinctive ways.

I'm going to ignore your last comment. It's offensive.

54. **woit**
April 20, 2006

String theory does not depart from GR "at the atomic level".
It does not predict unique, distinctive properties for gravitons.
It does not make detailed predictions about how superpartners will behave: it can't even predict their mass.
It does not predict "unique features" of extra dimensions, it is consistent with an infinite variety of different sorts of extra dimensions.

It does not tell you how topology will change.

Please stop repeating hype you have heard elsewhere. If you know of specific, scientific predictions from string theory that can in principle be checked, tell us what they are, don't just claim that they exist. Let me be specific: If you were given an accelerator capable of accelerating particles to Planck-scale energies, what would be the experimental signature that would show that string theory was true, such that if you didn't see it, you would know string theory was wrong?

55. **Christine Dantas**
April 20, 2006

Dear Who,

Amelino-Camelia et al. are indeed not being scientifically rigorous in their use of the verb 'predict' (at least in that passage), since they use it interchangeably with the term 'allow for'.

Thus, having acknowledged the fact that they are using the word "prediction" in the more ordinary, vague sense, the interpretation of that quote is as straightforward as it could be.

They are just mentioning that string theory can *accommodate* a plethora of new phenomena, and at the same time, that *accommodation is not a scientific prediction* (and I hope it will never be). Accommodation often presuposes that things can be adjusted, or brought to an adaptable state. One thing is to claim or conclude by whatever means that new phenomena could be expected. Another thing is to supply the quantitatively precise conditions at which these phenomena are supposed to be measured or observed.

In conclusion, what I really interpret from that passage on Amelino-Camelia et al.'s paper is a simple background message that string theory cannot be a theory after all (at least, not yet). Perhaps they could have said it differently, so as to avoid the understandable feeling that what they are saying is not self-consistent.

My present attitude towards string theory is that it is just an approach to quantum gravity (or to unification), but not a theory (yet). There are several worrisome things like the landscape (and all the anthropic issue) and the lack of predictability in string theory that makes me uncomfortable. So I often agree with what Peter Woit continuously write here. An of course I completely acknowledge that other approaches have their own serious problems as well. But that is another story.

Best regards,
Christine

56. **Hech Baan**
April 20, 2006

In the accelerator I will be colliding very high-energy particles. Occasionally a high-energy graviton will be produced that will move off into extra dimensions and disappears. It will look like a jet of high-energy particles and there would be nothing balancing the energy and momentum of that very high-energy jet.

57. **woit**
April 20, 2006

Accelerators produce all sorts of things that show up as missing energy, this has nothing to do with string theory. I think you're repeating the standard example of what would be evidence for a brane-world scenario. But string theory doesn't predict a brane-world scenario. In the most popular "string theory phenomenology" models, we don't live on a brane in higher dimensions, observable by energy escaping the brane. Instead the extra dimensions are planck scale. The signatures for this are different than the brane-world signatures and depend completely on the size and shape of the extra dimensions.

58. **no**
April 21, 2006

Baan, phenomenologists tried to compute collider signals of gravitons escaping in extra dimensions using the (non-renormalizable) Einstein gravity, because strings (and other quantum gravity models) failed to give concrete results.

Observing extra dimensions would be evidence for extra dimensions, not for strings.

59. **Juan R.**
April 21, 2006

Hech Baan wrote,

"What would be your reaction if string/M-theory predicts only universes without live forms?"

A more interesting questions is

"What would be string theorists reaction if string/M-theory predicts only universes without live forms?"

It is clear that many string theorists would claim that live forms are stupid 😉

You claim that string M-theory does many predictions but after you lack to cite even a single!

You said

"String/M-theory predicts that we all consist of strings, including people who don't appreciate the theory."

This reflect a complete misunderstanding of the concept of prediction. The string is not a prediction of string theory, it is one of axioms of the "theory". Moreover, even if i forget this point, your claim is wrong, because it is now know that string is not fundamental. That is reason of joke "string theory is now a theory without strings".

Another imprecision yours is that you lack to state that M-theory is not formulated (even Witten claims that nobody know M-theory is). It is difficult that an inexistent theory can do predictions. Another point is that the most *close* to M-theory has been formulated (M)atrix theory is not a theory of strings, it is a theory of pointlike objects: D0-branes.

The lesson is that initial string theory was so wrong that today nobody (including string theorists) believes on perturbative metric, nobody believes on 10D spacetimes, nobody believes on 2D extended objects as fundamental. The three were claimed to be correct a few years beyond…

Some comments on the rest.

String/M-theory with gravitons does not incorporates the full structure of Einstein's theory. The causal structure of string is wrong when compared with GR, and this is reason for a non-perturbative generalization of string theory. That generalization breaks basic axioms of string theory and this is reason nobody has formulated M-theory still.

Supersymmetry is not a String/M-theory's prediction. Supersimmetry is a need of the theory because internal difficulties. The problem you are ignoring is that String/M-theory needs supersymmetry at all levels as cure of internal inconsistencies and cannot offer a description of the world was non-supersimmetric. THat is we observe.

We may offer a non-supersimmetric model (the SM) and if in a future supersymmetry is found in laboratory then develop a more general theory with supersimmetry at high energies. String/M-theory introduces supersimmetry at all energies and none mechanism for which we can recover a non-supersimmetric description at low energies. That is reason that in 40 years none string theorist has been able to derive the SM.

In science, any new theory unable to explain known data is inefficient and if cannot derive previous theories at use then it is metaphysics.

Moreover, it is simply false that String/M-theory makes detailed predictions about how superpartners will behave.

String/M-theory does not predicts that there are extra dimensions with unique features. The history of number of dimensions in theoretical physics is a fascinating example:

4D, 5D, 26D, 10D, 11D, 12D? 4D?

In fact, there is people claimining a return to 4D string models.

It is interesting see you absolute claim on "prediction" of dimensions from string theory. What is a dimension in string theory?

Witten recognizes "On the other hand, we don't understand the theory too completely, and because of this fuzziness of spacetime, the very concept of spacetime and spacetime dimensions isn't precisely defined."

Juan R.

Center for CANONICAL |SCIENCE)

60. **Chris W.**
April 21, 2006

Hech Bann: *String/M-theory predicts that we all consist of strings, including people who don't appreciate the theory.*

This reminds me of something. In my late teens I read B. F. Skinner's *Beyond Freedom and Dignity*. In contemplating the all-encompassing scope of Skinnerian behaviorism, it occurred to me that the theory could be used to explain (and dismiss) the behavior of its critics.

I was secretly infatuated with this idea for about a year and a half, and then began to recover my senses. A few years later I began to study Karl Popper's writings and to consider his epistemological critique of Freudian psychology, and thereby began to truly understand the potentially pathological nature of "universal" explanations.

Of course, Hech Baan was just joking.

61. **Who**
April 21, 2006

Christine,
I agree with you on those issues.
(At first I did not understand the thrust of your quote.)
Thanks for clarifying.

Who

62. **Paul Valletta**
April 21, 2006

Surely Susskind can redeem himself once and for all? If he can get a Hydrogen and Oxygen molecule to bond in any dimension other than 3? If for instance he can theoretically proove that it is possible to combine these two elements, and thus produce water in the, say 5th dimension, then he can bring to an end the "Many Worlds", Parallel Universe's, and Anthropic issues to a concluding close?

And as a side line, he could start a factory that sells this 5th dimensional water and retire with a "nice little earner" !

The first step to the proof of "life" in other Universe's or Extra Dimensions, is to experimentally or Theoretically?.. Proove that "H2O" can be created in "other" dimensions.

Anthropically Susskind can argue that Water, and all our Elements of our periodic table have "leaked" from other dimensions into our, but somehow that does not seem to wash with me!

Lenny, fetch me a glass of water from any dimension other than 3-D, and I would gladly raise it as a toast to your good health and wealth!

63. **Paul Valletta**
April 21, 2006

Actually, I do not see Lenny opening a factory, it would be more to his stature that he opens a Baptising Water Spa?..I can see Peter Woit being first in line to this Anthropic Cleansing?

64. **knotted string**

May 2, 2006

On the subject of combining the worship of religion and law-givers in physics, see http://zapatopi.net/lordkelvin.html

😉

Science Fantastic

April 20, 2006
Categories: Uncategorized

Michio Kaku has joined the Talk Radio Network, where he will have a new radio show called "Science Fantastic" that will appear on 90 radio stations around the country. Topics that will be covered include "black holes, higher dimensions, string theory, wormholes, search for extra-terrestial life, dark matter and dark energy, the future of space travel, genetic engineering, the aging process, the future of medicine, the human body shop, artificial intelligence, the future of computers and robots, as well as topics from science fiction."

The blurb at Talk Radio Network describes Kaku as "one of the world's leading experts in theoretical physics, and according to New York Magazine, one of the '100 Smartest People in New York.'" It goes on:

Dr. Kaku is the co-founder of string field theory, one of the main branches of string theory, the leading candidate for the unified field theory. Many in the scientific field call this "The new Copernican revolution."

Kaku's appearance on Talk Radio Network is rather remarkable, given that many of the other talk show hosts there are extremist purveyors of right-wing sewage like Michael Savage and Laura Ingraham. In the past, Kaku has been known for his left-wing politics, hosting a radio show entitled Explorations on New York lefty radio station WBAI, rebroadcast on other Pacifica stations such as KPFA in Berkeley.

Comments

1. **Not a Nobel Laureate**
   April 20, 2006

   I've always found it interesting that academic physicists tend to be doctrinaire leftists, while engineers tend to be far more conservative in their socio-political views.

   Is it because the main source of funding for academic physicists is the State?

2. **Not a Nobel Laureate**
   April 20, 2006

   "Science Fantastic" seems somewhat tangential to the topic of string theory and it's problems.

3. **Lurker**
   April 20, 2006

   Michael Savage is nuts, but right half the time. I've never heard Laura Ingraham say anything I disagree with but I seldom listen to her. I'm not apologizing for off-topic because you brought it up. 🙂

   BTW FIRST!!!1li!!

4. **Lurker**
   April 20, 2006

   third, whatever

5. **woit**
   April 20, 2006

   I know extremely few "doctrinaire leftist" academic physicists, and I don't think they're any more numerous than doctrinaire rightists in physics departments. Most academics I know are more liberal than the general US population, but with politics about average for a Western European country. On the whole, they tend to not be "doctrinaire", but think for themselves. There was a long tedious discussion about why this is over at Cosmic Variance, with one explanation being that academics are just smarter than the average American. But this is off-topic. this posting is about a new radio show, one of the main topics of which will be "the new Copernican revolution"….

6. **woit**
   April 20, 2006

   Please, I just don't want to know it if my readers are Michael Savage and Laura Ingraham fans, it will make me too depressed. No more about them, please, I've had a long day…

7. **Hech Baan**
   April 20, 2006

   String/M-theory isn't "the new Copernican revolution". It's not even a revolution. It's just our next logical step to understand the nature.

8. **fishfry**
   April 20, 2006

   As Lurker says, Savage is nuts but right half the time. He can be really hateful at times … other times he's surprisingly ahead of the curve.

   Peter, if you don't want to know your friends' politics … don't ask.

9. **Troublemaker**
   April 20, 2006

   Peter, if you don't want to know your friends' politics … don't ask.

   He didn't.

10. **Jimbo**
April 21, 2006

I know nothing of these right-wing nuts. I do know, that Michio Kaku, like Carl Sagan, has charisma to burn, and that in this country he is an intellectual beacon, in a sea of idiot worship. Lets hope he gets the word out, & fans the flames of curiosity about Nature's frontiers.

11. **Chris Oakley**
April 21, 2006

By the by, I am guessing that the title "Science Fantastic" derives from "Light Fantastic". The latter name was applied to a TV series about physics recently here in the UK, but it is based on a misunderstanding. The original was a poem (L'Allegro) by John Milton, apparently written in 1632, which contains the lines

> Haste thee, Nymph, and bring with thee
> Jest, and youthful Jollity,
> Quips and cranks and wanton wiles,
> Nods and becks and wreathed smiles
> Such as hang on Hebe's cheek,
> And love to live in dimple sleek;
> Sport that wrinkled Care derides,
> And Laughter holding both his sides.
> Come, and trip it, as you go,
> On the light fantastick toe;
> And in thy right hand lead with thee
> The mountain-nymph, sweet Liberty;
> And, if I give thee honour due,
> Mirth, admit me of thy crew,
> To live with her, and live with thee,
> In unreproved pleasures free

"Tripping" the "light fantastick toe" here means "dancing", "light" being used in the sense of "not heavy" and "fantastick" being used in the sense of "fanciful".

So the "light" has nothing to do with electromagnetic radiation.

12. **Simon**
April 21, 2006

The title of Kaku's show, 'science fantastic' is in reference to the Terry Pratchett book of 'the light fantasic'.
Kaku chose this because it is the sequal to 'the color of magic'.
This makes sense because Kaku is looking for a fantastic new science that will be a sequal to the color of magic (QCD). 🙂

13. **Chris Oakley**
April 21, 2006

OK – so Kaku is exonerated on one count. However he is guilty on the count of liking Terry Pratchett (or *Douglas Adams for the undiscerning* as I like to think of him).

14. **knotted string**
April 21, 2006

Because Kaku is the world's leading physicist, expert on string theory and also on biological stuff, I'm uneasy with the use of string theory in horror stories:
http://www.marklaflamme.com/

"The world's leading physicist, in a delirium of genius, attempts to use the science that has consumed his life to bring his 8-year-old daughter back from the dead. … With toys to entice, gravity to tug at unseen dimensions, and string theory to unravel the world's most ancient secret, a grieving father awaits the return of his child from a place eternally unreachable. The little girl that comes to him is a child no father could want."

15. **MathPhys**
April 21, 2006

The rise and rise of Michio Kaku never ceases to amaze me. I hope he does a good job on the radio talk show circuit. We need all the scientists that we can get on such circuits. I understand the public's fascination by black holes, extra dimensions, etc.

Popularizing science is a good thing. I thought Brian Greene did a very good job with his book on strings. I was always very pleased to see people reading it on commuter trains, etc. I also know of one teenager who read it and was very inspired by it.

I hope Kaku will be equally positively influential.

16. **knotted string**
April 21, 2006

MathPhys, I agree! I hope he interviews Lubos Motl. Also, Jacques Distler would be interesting. I hope Kaku interviews him:

'These are exciting times to be a theorist. It is fair to say that we really don't yet know what string theory is about, but we are learning.' – Jacques,
http://golem.ph.utexas.edu/~distler/

17. **sunderpeeche**
April 21, 2006

Good for Kaku. One doesn't have to agree with all that he says (for example he opposed the Cassini mission because it carried plutonium and needed a flyby of Earth for a gravity assist), but if he can get kids interested in science, then that's good. Not all of them will necessarily become string theorists, and perhaps one of them will really discover the Next Great Breakthrough.

18. **andy**
April 21, 2006

‘Science Fantastic’ sounds like it will cover the very same information that is covered on the ‘Art Bell Show’ (along with space aliens and bigfoot.)

19. **Who**
April 21, 2006

sunderpeeche
** but if he can get kids interested in science, then that’s good.**
my experience of young people whose imaginations have been infected by Kaku is that he does not get them interested in science,
he gets them interested in pseudoscience.

Kaku brand Gee Whiz may enrich the fantasy life of some but contribute to a public misconception of what science is and ultimately, I suspect, undermines legitimate scientists’ credibility. I do not believe that it adds to the ranks of young people who are actually motivated and able to do hard science.

One can ask in whose interest is it to degrade the integrity of the enterprise and promote public misunderstanding of what it’s about.
I believe a rightwing talk radio slot is consistent with what Kaku does.

20. **Mark LaFlamme**
April 21, 2006

Knotted String, you have nothing to worry about. No one will confuse my main character with Mr. Kaku. In fact, when I do interviews, I quickly admit that I’m nothing near an expert in quantum mechanics. I’m merely one of those laymen who are fascinated by the science. I advise those who ask me about it to read Kaku or Brian Greene. Presently, I’m reading Kaku’s “Einstein’s Cosmos,” which serves as a great biographical piece but also a smooth introduction into quantum theory for those without scientific backgrounds.
If it’s any consolation, this work of fiction has generated interest about string theory among groups who might never have given the science a second look.

21. **adam**
April 21, 2006

Whoa, wait a minute. Bill Nye is a populizer of science. Michio Kaku is a populizer of science fiction. Every time I hear him talk, my head spins with all of his promises of how string theory is going to solve world hunger.

22. **lmot**
April 21, 2006

I think that any young person with the intellectual gifts to contribute to physics, would likely be smart enough to see through Kaku’s act, and would conclude that if this charlatan is the public face of theoretical physics, then maybe there is something wrong with the discipline.

23. **Billy**
April 21, 2006

Looks like they’ve finally found experimental evidence to support string theory:
here

“Cosmologists claim to have found evidence that yet another fundamental constant of nature, called mu, may have changed over the last 12 billion years. If confirmed, the result could force some physicists to radically rethink their theories. It would also provide support for string theory, which predicts extra spatial dimensions.”

Has the successful PR of string theory had the consequence that you can
get some free press attention by claiming that your experiment provides
the missing proof of string theory? Will we hear
“Experimental support found for string theory!” more or less times than
we heard that Iraq supported Al Qaeda and was building nuclear
weapons?

24. **Nick**
April 21, 2006

Savage is not even wrong

25. **steve**
April 21, 2006

I first encountered Kaku when he was acting as an anti-nuclear-power nut (OK, activist). His shocking dishonesty about the risks of nuclear power and glib self-promotion pretty much made me figure that anything he wrote on physics could not be trusted by a layman like myself. I had him binned with Sternglass and that crowd. The Cassini farce simply confirmed this opinion. Is there some legitimate achievement of his that explains his stature?

26. **CapitalistImperialistPig**
April 21, 2006

Nick – Nope. Savage is wrong.

Michio Kaku certainly seems to have a lot of enemies! I liked his popular book on strings, though it wasn’t nearly as informative as Brian Greene’s books. I found his QFT book amusing to browse but impossible to learn much from. I’ve got his string books too, but there again I found the presentation hard to follow.

27. **MathPhys**
April 22, 2006

Kaku’s first physics book (first edition appeared in the mid 80’s) was strange. He talks about things that I believe that (in the mid 80’s) he didn’t understand. I believe I’m right because when I met him and talked to him at that time, and it was clear to me that he’s unfamiliar with the technical details.

His more recent physics book on strings is even stranger. It’s written as if he downloaded lots and lots of papers from the archive, then sat there summarizing them. There are hardly any explanations of any depth in these books.

On the other hand, I commend him for the effort. He surely covers a lot of ground, and the books can serve as a very starting point for learning these things. At least he tells you what’s in these papers.

I wouldn't have bought these books, if it weren't for the fact (implied in the acknowledgements) that Luis Alvarez-Gaume (someone I respect and trust) proof read them for mistakes.

I'm sure he's a very smart guy, but I don't think he has the time or the interest to put his nose to the grind and really learn the technical details of the topics he discusses in these books.

28. **Billy**
April 22, 2006

Michio Kaku is a fun guy. He's responsible for a significant chunk of the "technobabble" on Star Trek and he was a significant pre-Elegant Universe factor in the string theory PR campaign. His futurology science has been entertaining in recent years but is, like most science fiction, overly focussed on traditional ideas of technology rather than the biotechnology (and direct brain-computer interfaces) which will dominate 21st century technology.

29. **pl@nk**
April 22, 2006

"my experience of young people whose imaginations have been infected by Kaku is that he does not get them interested in science,
he gets them interested in pseudoscience."

Totally agree.
I've seen Kaku talk about many thing but never Science in general or Physics in particular.

30. **Peter Woit**
April 27, 2006

Please stop it with the personal attacks on Kaku, I've just deleted one. If you want to criticize his activities, or what he has to say, that's fine, but there's no need to go on like Lubos Motl.

Witten Geometric Langlands Talk and Paper

April 22, 2006
Categories: Langlands

I spent yesterday afternoon down in Princeton, and attended a talk by Witten at the Institute on his work relating gauge theory and the geometric Langlands program. He says that his paper with Kapustin is done, it's about 220 pages long, and will appear on the arXiv in Monday's listings. So Sunday night, this link to [hep-th/0604151](hep-th/0604151) should start working. He's also working on a book on the subject, where he would be the sole author.

At the start of talk, Witten noticed that many of the Institute's mathematicians were there, and warned them that they had come to the wrong talk, since it was one aimed at physicists. Pierre Deligne got up and left, but others, including Sarnak and Langlands himself, did stay for the whole thing, although I'm not sure how much they got out of it.

Witten began by giving an outline of the talk, emphasizing six main ideas that were crucial to what he wanted to explain. He also listed as number zero the idea of geometric Langlands itself, saying he would talk about it at the end if he had time (he didn't). The six main ideas were:

1. From a certain twisting of N=4 supersymmetric Yang-Mills one can construct a family of 4d TQFTs parametrized by a sphere. The twisting is the same sort that occurs in his original TQFT for Donaldson theory, in that case coming from N=2 supersymmetric Yang-Mills. The TQFTs he considers have an S-duality, part of a larger SL(2,Z) symmetry.

2. Compactifying the theory on a Riemann surface leads to topological sigma models, based on maps from the Riemann surface into the Hitchin moduli space M_H of stable Higgs bundles. The four dimensional S-duality corresponds here to a mirror symmetry of these topological sigma models.

3. Wilson and 't Hooft operators of the 4-d gauge theory act on the branes of the topological sigma models. Branes mapped in some sense to a multiple of themselves by these operators are called electric or magnetic "eigenbranes" respectively.

4. Electric eigenbranes correspond to representations of the fundamental group, this is one side of the geometric Langlands correspondence.

5. The 't Hooft operators of the gauge theory correspond to the Hecke operators of the geometric Langlands theory although these are now defined on the space of Higgs bundles, not G-bundles.

6. Using a certaing co-isotropic brane on M_H, magnetic eigenbranes give D-modules on the moduli space of bundles. The electric-magnetic duality coming from S-duality in the gauge theory relates electric and magnetic eigenbranes, giving the geometric Langlands duality between representations of the fundamental group of the Riemann surface in the Langlands dual group, and "Hecke eigensheaves" on the moduli space of G-bundles.

By the time he got to the 6th of these ideas, he was running out of time and things got very sketchy.

Witten made clear that this work doesn't directly give dramatic new physics or mathematics, but rather just explains some tantalizing relations between gauge theory and Langlands duality, ones that were first noticed in work of Goddard, Nuyts and Olive in 1976, and pointed out to him by Atiyah way back then. The geometric Langlands program is famous among mathematicians for its difficulty (I still have trouble getting my brain around the concept of a Hecke eigensheaf..), and for its tantalizing nature, bringing together a range of different mathematical ideas (many involving conformal field theory, although these seem to be different than what Witten is doing). The new relations between this subject and supersymmetric gauge theory and TQFTs that Witten has unearthed may very well lead to some very interesting new mathematical developments in the future. Undoubtedly it will take people a while to make their way through the new 220 page paper and absorb all that he and Kapustin have worked out since last summer.

Comments

1. **Kea**
   April 22, 2006

   Wow.

2. **MathPhys**
   April 22, 2006

   Wow, wow.

3. **ooh, a bandwagon**
   April 22, 2006

   Wow, wow, wow.

4. **ooh, a bandwagon**
   April 22, 2006

   Seriously though: is there any way to give a rough intuitive feel for what a Hecke eigensheaf is? My vague memory of a talk I heard once is that it's a way to define, given a particular curve on a given space, a sheaf on the moduli space of curves on the space? Is there a handwaving explanation of what it is and why we want it?

5. **Kea**
   April 22, 2006

   I have no idea, but I found this several-100-page definition by Beilinson and Drinfeld:

   http://www.math.uchicago.edu/~arinkin/langlands/hitchin/

   If we have to read these sorts of things as 'background' to the 200+ pages…well, I don't know how old you guys are…

6. **Kea**
   April 22, 2006

   The *first line* of section 1.1.1 reads: let Y be a smooth equidimensional algebraic stack over C.

7. **Tony Smith**
April 22, 2006

About the Witten/Kapustin paper at http://www.arxiv.org/abs/hep-th/0604151, Peter said:
"… The six main ideas were:
1. From a certain twisting of N=4 supersymmetric Yang-Mills one can construct a family of 4d TQFTs parametrized by a sphere. …".

Does that mean that the physics of the paper is no good if conventional 1-1 fermion-boson supersymmetry is no good?

Does that mean that the paper is likely to be interesting math but vacuous as to physics?

Could that explain "… Witten noticed that many of the Institute's mathematicians were there, and warned them that they had come to the wrong talk, since it was one aimed at physicists. Pierre Deligne got up and left …" ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – A quote from Burton Richter about such supersymmetry:
"… I would say that supersymmetry is a pure "social construct" with no supporting evidence despite many years of e□ort. It is okay to continue looking for supersymmetry as long as it doesn't seriously interfere with real work (top, Higgs, neutrinos, etc.). …".

8. **Kea**
April 22, 2006

Does that mean that the physics of the paper is no good if conventional 1-1 fermion-boson supersymmetry is no good?

Tony

Witten clearly knows perfectly well that 4d TFTs make much better physics than SUSY.

9. **Aaron Bergman**
April 23, 2006

There is little truly "new" physics in the paper. What it does is show how a fair chunk of the structures that show up in Geometric Langlands appear in compactifications of this particular twisted N=4 SYM theory. He also works out the details of the twistings, the D-branes, the Wilson and 't Hooft operators, the various moduli spaces, etc. What one hopes when one finds new connections between mathematics and physics is that each can inform the other. This is certainly not like SW theory where Witten revolutionized the study of the invariants of 4-manifolds, but hopefully this is a good beginning. Maybe we'll even learn a bit about gauge theory from all of this.

Some mathematicians got up and left, in all probability, because Witten was giving the talk for a physics audience. The language and backgrounds needed are often very different.

For Hecke operators, physics-wise they're very easy. In this theory, you've compactified the 4D theory on a Riemann surface cross the upper half plane. There is some boundary condition (ie, D-brane) on the boundary of the upper half plane. One can put an 't Hooft loop in the 4D theory located at a point on the Riemann surface and parallel to the boundary of the upper half plane. From far away, this looks like a different boundary condition, giving an operation on D-branes. The data of a point on the Riemann surface and a representation of the Langlands dual of the gauge group (which is associated to the 't Hooft loop just as a representation of the gauge group is associated to a Wilson loop) give a Hecke operators. This Hecke operators acts on the derived category just like the 't Hooft loop acts on D-branes which are objects in the derived category.

On the math side, I can't quite decipher my notes, but I do have something like this. I don't vouch for any of it. Given a vector bundle (all associated to a given G-principal bundle, maybe?) on a Riemann surface, we can pick a point and a subspace of the fiber at that point. We can then look at the space of sections that are restricted to live in that subspace at the chosen space. This is the space of sections of a new vector bundle (read 'coherent sheaf', perhaps). It turns out that the space of inequivalent operators has to do with pairs of vector bundles that are isomorphic on the complement of a point (making these, in a sense, local operators). With a little work, this gets related to a loop grassmanian and eventually a double coset space which turns out to be exactly the coweight lattice mod the Weyl group, ie, a representation of the Langlands dual group.

That probably doesn't help, does it.

10. **anon (previously "ooh, a bandwagon")**
April 23, 2006

Aaron, thanks for the physics definition of the Hecke operator; at least *that* formulation is easy to grasp. Unfortunately I don't quite see how to relate it to the math talk I once heard, or how to make sense of your math paragraph, but I suppose that's because of things like your "with a little work"…. 🙂

11. **?**
April 23, 2006

Are you sure you attended the talk for physicists?

Speaking seriously, why physicists not directly interested in mathematics of things like very-SUSY gauge theories should try to read that paper?

12. **Tony Smith**
April 23, 2006

If the "family of 4d TQFTs parametrized by a sphere"
is constructed from "N=4 supersymmetric Yang-Mills"
then
how do those "4d TFTs make much better physics than SUSY" ?
(Which Kea says is something that "Witten clearly knows perfectly well".)

Aren't those 4d TFTs plagued by the SUSY in the N=4 supersymmetric Yang-Mills from which they are constructed ?
If not, then where did the SUSY go ?

Are Witten/Kapustin saying that their 4d TFTs break the SUSY, and are they presenting this work as a physical mechanism of SUSY symmetry breaking ?

If the SUSY is physically wrong, and is not broken by their 4d TFTs, then wouldn't their model be unrealistic as a physics model ?

Of course, as in index theory, Donaldson theory, etc, SUSY can be very useful in mathematics, so again it seems to me that the paper is likely to be very interesting math, but vacuous as physics.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

13. **Peter Woit**
April 23, 2006

Tony,

The theories being studied here are not the sort of physical supersymmetric theories that some people believe are physically realistic. They are TQFTs derived from supersymmetric gauge theories, much like the TQFT used in Donaldson theory. Witten is not claiming they are relevant to the real world, just that they lead to interesting mathematics.

14. **Bert Schroer**
April 23, 2006

Peter,
the danger of such a talk to a physicists audience by a famous personality with a rich pre-Atiyah physics past is of course that all the problems of placing sophisticated mathematics on top of conceptually underdeveloped physical ideas will agrevate the present crisis. The interesting sociological aspect is that top mathematicians seemed to have lost some of their unqualified admiration with which they looked at the physicist's magic.

15. **Tony Smith**
April 23, 2006

Peter said "… Witten is not claiming …[the TQFTs in hep-th/0604051]… are relevant to the real world, just that they lead to interesting mathematics. …". I certainly agree with the claim of "interesting mathematics".

However, as to "the real world", look at the reaction of a very smart conventional superstring theorist, Aaron Bergman. He said:
"… There is little truly "new" physics in the paper. …
He [Witten] also works out the details of the twistings, the D-branes, the Wilson and 't Hooft operators, the various moduli spaces, etc. …
For Hecke operators, physics-wise they're very easy. … This Hecke operators acts on the derived category just like the 't Hooft loop acts on D-branes which are objects in the derived category. …
What one hopes when one finds new connections between mathematics and physics is that each can inform the other. …
Some mathematicians got up and left, in all probability, because Witten was giving the talk for a physics audience. The language and backgrounds needed are often very different. …".

It seems to me that Aaron Bergman, a physicist, is saying that hep-th/0604051 DOES contain what he considers to be physics (in fact, not even "new" physics, and so therefore stuff already well known to be physics),
and that he hopes that the math of hep-th/0604051 will be seen to be connected to physics (and therefore to the physical real world).

As Bert said, there is "… danger …[in]… such a talk to a physicists audience …",
particularly if it is so physics-oriented that Witten felt called upon to have "… warned … mathematicians … that they had come to the wrong talk …".

Again, please let me repeat that hep-th/0604051 (from what discussion I have seen about it) does seem to me to be likely to be very interesting mathematically, as was the use of SUSY in index theory, Donaldson theory, etc.

Why did the authors submit it to the physics archive hep-th instead of the math archive (and why did the Cornell arXiv moderators allow it to be placed in hep-th) ?
They could have submitted it to the math archive (since it is likely to be very good math) with a cross-listing to hep-th (since it uses some techniques and structures that are discussed in papers on hep-th).
However,
they submitted it to the physics archive hep-th which seems to me to indicate that the authors DO think of the paper as being physics, which implies describing physical reality – or has conventional superstring theory so distorted the definition of "physics" that description of physical reality is no longer part of it ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

16. **Bert Schroer**
April 23, 2006

Tony
thanks for your pertinent question. I do not see any scientific answer, again it seems to be sociological.

17. **Bert Schroer**
April 23, 2006

The moderation of hep-th/ which should be geared to particle theory is indeed highly biased.
Distler rejected a single cross listing in hep-th of my paper about the protagonist of QFT Pascual Jordan (I think it is important to know one's own history and my article contains some little known facts which have profoundly influenced my present research on advanced QFT). It is clear that he did not approve of the political introduction (the only reason why I sent it to physics/, and not to hep-th). His note of cross rejection reads:
Dr. Schroer,
Your paper does not appear to deal even indirectly with theoretical high
energy physics, and therefore seems inappropriate for cross-listing in hep-th.
We appreciate your cooperation.
arXiv moderation
(this was his answer to my letter: Dear arXiv-moderation,
I kindly ask you to rescind your decision against cross-listing my paper physics/0603095 in hep-th. My reason is that about 80% of the essay deals with the protagonist of QFT Pascual Jordan (including some new fact which I found through the study of his old Zeitschrift fuer Physik papers) and having only one cross listing cannot be

interpreted as any abuse of the cross-listing system.
with friendly regards
Bert Schroer)
I of course could have answered him that this is an updated small part of an extensive paper on Jordan which was not cross listed but rather posted some time ago on hep-th without any problem, but I got tired of this nonsense.

18. **Aaron Bergman**
April 23, 2006

Speaking seriously, why physicists not directly interested in mathematics of things like very-SUSY gauge theories should try to read that paper?

You probably shouldn't if you don't like this sort of thing.

I should say, BTW, that much of the point of this paper is that many things in the geometric Langlands program that seem like hopelessly abstruse math from the physics prespective (of course mathematicians feel otherwise) turn out to be natural objects in the physics, in fact, objects that we've known and loved for years.

19. **anonymous**
April 23, 2006

Tony writes:

"However, they submitted it to the physics archive hep-th which seems to me to indicate that the authors DO think of the paper as being physics, which implies describing physical reality"

I wouldn't say that. Theories with N > 1 SUSY or topological field theories have been considered "physics" for some time, and rightly so — they have much in common with things that describe physical reality, but they are easier to solve. That they are unrealistic does not mean they are not physics.

Think of it this way: the study of Newton's laws in the context of frictionless ramps and pulleys, which every student learns, is considered "physics", even though it's really just a dramatic oversimplification of reality. If you can't understand the dramatically simplified, solvable examples, good luck trying to understand the mechanics of real-world systems! TQFTs and N=2 SUSY QFTs and such things have an analogous relationship to the Standard Model.

20. **urs**
April 23, 2006

> Think of it this way: […]

Yup, that's why there is a "th" at the end of "hep-th", not a "ph".

21. **woit**
April 23, 2006

Bert,

I don't think what Witten is doing here will aggravate the present crisis, which I see as driven mainly by ideological attachment to the failed idea of string-based unification. There are problems with the way physicists approach the use of mathematics in their work, but I don't think these are the main reason for the current sad state of particle theory.

Actually the fact that this new work of Witten has little to do with string theory, but instead relates 4d gauge theory and interesting mathematics, seems to me rather encouraging. Perhaps it will cause people to move on from failed string theory projects to something more promising. It will be very interesting to see what effect this work has. The mathematics involved is rather difficult, and the QFT techniques are pretty intricate, so I'm not so sure how many people will be willing or able to follow Witten into research in this area. But then, nothing much else is going on.

Tony,

The use of QFT to do new things in math is very much on the boundary of the two subjects, and I don't see anything to be gained by saying it is "not physics", or "not math". It's both, potentially leading to both new insights about math and about QFT, and there's no reason for it not to be on hep-th. The problem with hep-th these days is not that it is too mathematical, it is that it's full of worthless work about completely obscure and pointless results supposedly related to string theory. Some of this is mathematically sophisticated, most of it isn't.

22. **Levi**
April 23, 2006

My reaction to Peter's post was similar to that in the first five comments on this thread. Does anybody want to make an estimate of how many people in the world will have both the expertise and the sustained interest to carefully read this paper when it appears? Is there anyone reading this comment who is planning to undertake the task?

I'm not making this comment to denigrate Witten's work on this subject. I'm genuinely curious about the number of people who are expert enough in both math and physics to work in this area.

23. **anonymous**
April 23, 2006

Levi wrote:

Does anybody want to make an estimate of how many people in the world will have both the expertise and the sustained interest to carefully read this paper when it appears? Is there anyone reading this comment who is planning to undertake the task?

I think a lot of people will read it, though maybe not so many will read it carefully and in detail. Aaron's comments make it pretty clear that the physics is nothing exceptionally weird; it should be comprehensible to many, many people. The technical details of current work in geometric Langlands seem much less approachable, at least from the perspective of a physicist, but Witten is coming at this from a physics perspective so I'm sure he will explain some of this in understandable language. Furthermore, he's an exceptionally clear expositor and his papers tend to be surprisingly readable even when they're rather technical.

24. **Levi**
April 23, 2006

Here's a link to a paper dicussing the relationship between Langlands and CFT, written for physicists, evidently.

http://www.arxiv.org/abs/hep-th/0512172

25. **?**
April 23, 2006

dear anonymous,

the analogy with friction forgets the fact that (so far) only one strongly coupled gauge interaction is relevant for physics: QCD. From a fundamental point of view, QCD is a successfully closed issue, despite the fact that doing computations around 1 GeV is hard. Since the s and c quark masses are around 1 GeV, it seems unlikely that QCD has some hidden mathematical simplicity: more likely brute-force lattice will continue to be the only way of getting physically relevant results.

26. **Aaron Bergman**
April 23, 2006

Actually the fact that this new work of Witten has little to do with string theory, but instead relates 4d gauge theory and interesting mathematics, seems to me rather encouraging.

The topological string is very important here, particularly with the identification of the compactified theory as an A- or B-model and the boundary condition technology that goes with that.

I don't know how many people will end up reading the thing, but I can tell you that it includes me for what it's worth. We had a semester long seminar on geometric Langlands and physics here at UT which definitely helps. That and having David Ben-Zvi around to answer questions on the math side.

27. **woit**
April 23, 2006

Aaron,

Does the "topological string" really come into this? As far as I can tell (not having yet read the 220 page paper…), what is involved are TQFTs based on a fixed Riemann surface or Riemann surface with boundary. This is 2d QFT, not string theory if you're not summing over genera.

28. **Aaron Bergman**
April 23, 2006

This is a semantic argument probably not worth having, but in their full glory, the Hecke operators act on derived categories and the like which correspond to the possible branes in the theory. Call that whatever you want; that's the physics.

29. **woit**
April 23, 2006

Personally, I'd call 2d QFT on a fixed 2d background "2d QFT", not string theory, even when there's a boundary. But, that's just me.

30. **anonymous**
April 23, 2006

"Personally, I'd call 2d QFT on a fixed 2d background "2d QFT", not string theory, even when there's a boundary. But, that's just me.

LOL! And then he goes on to say its a matter of semantics! What a ridiculous comment by Bergman. But, that's just me.

31. **Fine**
April 23, 2006

It is evident that most people have no clue whatsoever about the Langlands Program.Also, it is absurd that people are arguing whether the paper should be in hep-th. Has anyone beside Aaron have anything concrete to say? I look forward to see the analysis of the 220 page paper (not just what Witten said in the Simon's workshop)

32. **CapitalistImperialistPig**
April 23, 2006

Levi – thanks for the link, but you ask *Does anybody want to make an estimate of how many people in the world will have both the expertise and the sustained interest to carefully read this paper when it appears?*

Let me offer up the answer of Mathew 22:14 *Many are called, but few are chosen*

Saint John Chrysotom opined: *"Among thousands of people there are not a hundred who will arrive at their salvation, and I am not even certain of that number, so much perversity is there among the young and so much negligence among the old."*

The theological evidence makes it pretty clear that only Aaron and 99 others are likely to be among the saved.

…………(the old and negligent)

33. **Levi**
April 23, 2006

most recent anonymous,

From the introduction to the paper that I linked to above:

"In recent lectures E. Witten outlined a possible scenario of how the two dualities – the Langlands duality and the S-duality – could be related to each other. It is based on a dimensional reduction of a four-dimensional gauge theory to two dimensions and the analysis of what this reduction does to "D-branes". In particular, Witten argued that the t'Hooft operators of the four dimensional gauge theory recently introduced by A. Kapustin become, after the dimensional reduction, a Hecke "eigensheaf", an object of interest in the geometric Langlands correspondence."

So it really *is* a semantic argument.

34. **Bert Schroer**
April 23, 2006

Peter,
your view that the present crisis of particle physics is solely caused by a mental perversion called string theory does not hold water. It is simply not true that particle physics was healthy up to the day when sinister string theory appeared on the scene. Neither can one expect that the crisis in particle physics would evaporate suddenly if only the protagonists would see (and admit) that they are heading into the blue yonder where there will be no physical landing place.
Such paradigmatic changes in the delicate relation between the weight physical concepts in relation to sophisticated mathematical formalism do not happen out of a sudden, they rather announce their coming a long time before. The profound misunderstandings in the role of Euclideanization and that of the artistic role of the path integral (at one time in the not so distant path more than 90% of papers on gravity in the QFT setting were written in the euclidean Riemannian setting) were such warning signs.
Particle physics in particular in its local quantum physical setting has its own powerful autonomous conceptual structure and to make it jump over a ready made mathematical stick (or a more floppy string) like a dog does not have much of the chance. The first mathematician who saw that was of course von Neumann and his present day's followers are asking local quantum physics what kind of mathematical concepts it leads to. Differential geometry you can impose on functional integrals but not on the principles underlying QFT.
The point which Tony Smith made is absolutely crucial and your struggle against the aberrations of string theory will be futile if you do not recognize the root of problems and keep looking only at the symptoms.

35. **CapitalistImperialistPig**
April 23, 2006

Bert and Peter – Conceptual roadblocks are not exactly new in physics or any other science. In physics, these are usually surmounted with the help of new experiments. The *crisis in particle physics*, such as it is, is due to the fact that we have neither new data nor important unexplained experimental facts. I suspect that new breakthoughs may have to wait for new data – or at least for a theory that makes an accessible prediction.

36. **woit**
April 23, 2006

CIP (and Bert),

Sure, the fundamental problem is that physics is used to making progress by getting new clues from experiment and it hasn't gotten any in a long time. But, even with a lack of new experimental clues, theorists still can learn new things and slowly make progress, although it's a lot harder. To do this, I think they need to try many different things, since it is very unclear what is the right direction to go in. The kind of new algebraic thinking about the fundamentals of QFT that Bert advocates is one direction that should be pursued, I also think there are geometrical ideas motivated by path integrals that also deserve attention. The problem with the dominance of string ideology has been the way it has driven out the kind of other very different ideas that should be tried, of very different kinds.

37. **Kea**
April 23, 2006

The link works! Right. References first, hmmm: Gualtieri, Runkel et al on CFT, Hausel, plus all the obvious ones…dammit, I might have to try and read this…but I'm not expecting to get far in a hurry. 🙂

38. **Kea**
April 23, 2006

Peter previously mentioned the helpful article:

Mikhail Kapranov, *Analogies between the Langlands correspondence and topological quantum field theory*, in Functional Analysis on the Eve of the 21st Century, Vol. 1, Birkhauser, Boston, pp. 119-151.

39. **Who**
April 23, 2006

several comments here have discussed the general issue of crisis in physics, so I hope it will not be off topic to provide a link followup to Peter's post of 25 January "Die Physik steckt in der Krise"
http://www.math.columbia.edu/~woit/wordpress/?p=334

To some extent that post and comments thereto concerned Reiner Hedrich, who was funded by the German Science Foundation to study the situation in theoretical physics and was interviewed for the article in Die Zeit.

Hedrich posted this article on arXiv today
http://arxiv.org/abs/physics/0604171
String Theory – From Physics to Metaphysics

In the earlier chain of comments Andre Bresges gave a bit of background on Hedrich and the author, Max Rauner, of the article in Die Zeit
http://www.math.columbia.edu/~woit/wordpress/?p=334#comment-8199
and Eli Rabett provided this translation
http://rabett.blogspot.com/2006/01/lost-this-is-something-of-new.html

since the earlier coverage of Hedrich and his study of (at least confusion and misdirection, if not) crisis in theoretical physics was interesting, I thought some might wish to have a look at Hedrich's 30-page paper.

40. **Arun**
April 23, 2006

The line "Although multiverse scenarios and anthropic selection are not only motivated by string theory, but lead also to a possible explanation for the fine tuning of the universe, they are concepts which transcend the framework defined by the epistemological and methodological rules which conventionally form the basis of physics as an empirical science. " is so exceedingly polite 🙂

41. **Kea**
April 23, 2006

Witten on page 5: *A second major gap [in this paper] is that we do not shed light on the utility of two-dimensional CFT for the geometric Langlands' program*. Sigh.

42. **woit**
    April 23, 2006

    Kea,

    That's the strange thing about Witten's recent work: it seems to involve quite different uses of 2d QFT to get Langlands duality than the 2d CFT ideas that have motivated the mathematicians doing geometric Langlands (and that are discussed for instance in the Frenkel review article). Well, this means there is still a lot about this story which is not understood, which means there is still a lot to do…

43. **woit**
    April 23, 2006

    Who,

    Thanks for pointing that out. A quick read of it leaves me with the impression that he's avoiding just drawing the simplest conclusion from all the material about the problems of string theory that he has gathered: it's a wrong idea about unification and should be abandoned.

44. **Kea**
    April 23, 2006

    But they reference Runkel et al, and they seem to use Riemann (2D) surfaces like a skein theorist would, and take a look at Kapustin's [41] for motivation, and and and…

    Why don't they *even comment* on the utility of category theory here? Is it because it's all obvious to them?

45. **Kea**
    April 23, 2006

    Frankly, I'd rather be talking about geometric Langlands here…

    Thank you. Perhaps you could summarise some of your views of this paper so far.

46. **Aaron Bergman**
    April 24, 2006

    I'm waiting for the book :).

    Seriously, it's too long to completely absorb. Much of it is showing that things that intuitively ought to work really do so, like the particular twisting of N=4 SYM, the action of S-duality, and the details of the sigma model you get when compactifying. (On that point, it's much more readable to me than Vafa's work on the subject, but, then, I almost always find Vafa incomprehensible.) I like the sections on the action of the 't Hooft and Wilson operators. After various skims, it looks like the new math, if anywhere, is hiding in the book. I'm not sure what it leaves to say about the physics of this particular model, though. It's an impressive exegesis.

    What the paper does, I think, is bring Langlands to physicists. What I'm not sure it does is bring the physics to the mathematics. But that's not a criticism; it's probably too much to ask Witten to revolutionize every mathematical subject he thinks about, and we haven't even seen the book yet. As usual with when physics meets mathematics, it amazes me how many of the things that snow up in the physics had already been anticipated by the mathematicians. I guess they're smart, too.

    The good news (for me at least), is that it doesn't kill the project I've been thinking about with a few other people on the subject. You'd like it; it has categories in it.

47. **Kea**
    April 24, 2006

    🙂 Cheers

48. **Not a Nobel Laureate**
    April 24, 2006

    The discussion of the Witten paper reminds me of the bon mot about Schwinger.

    "Other people publish to show how to do it. Julian Schwinger publishes to show that only he can do it."

    Of course, the similarity is superficial. Schwinger's physics papers contained testable predictions – the Lamb shift, the gyromagnetic ratio, etc.

49. **Aaron Bergman**
    April 24, 2006

    The discussion of the Witten paper reminds me of the bon mot about Schwinger.

    "Other people publish to show how to do it. Julian Schwinger publishes to show that only he can do it."

    That's remarkably inapposite. Witten is one of the best expositors in the field.

50. **amanda**
    April 24, 2006

    "That's remarkably inapposite. Witten is one of the best expositors in the field."

    That's rather like saying that Jesus Christ was one of the most important characters in the New Testament. EW completely revolutionized the way papers are written. Which makes his current tedious divagations all the more painful. We are all waiting for him to show us the way forward, and this is what we get? I mean, does anyone really believe that this is going anywhere? A sad contrast to http://arxiv.org/abs/hep-th/0106109, let alone the 1998 papers…..

51. **dan**
    April 24, 2006

Has Witten made any reviews or comments or written any papers on background independent quantum gravity, esp LQG, spin foam, or CDT approaches? Has he shown any interest in these approaches?

52. **woit**
April 24, 2006

Dan,

No, and those topics have nothing to do with this posting.

53. **bigbigbig**
April 24, 2006

dear experts and crackpots,

about the 2d QFT or string theory argument, yeah, I think it's only fair to call it string theory once you couple the topological sigma model to 2d gravity.

54. **Tony Smith**
April 24, 2006

Here are four questions that came to mind after initially looking at the Kapustin-Witten paper. I apologize in advance for their simple-mindedness.

Kapustin and Witten say:
"… N = 2 super Yang-Mills theory can be twisted to make a quantum field theory realization of Donaldson theory …
Similarly, N = 4 super Yang-Mills theory can be twisted in three ways to make a topological field theory. Two of the twisted theories … are closely analogous to Donaldson theory … The third twist … turns out to be the twist relevant to the geometric Langlands program, and we will call it the GL twist. …
A hyper-Kahler manifold has … a family of complex structures parameterized by CP1, obtained by taking linear combinations of I, J, and K …[with]… quaternion relations … ".

Donaldson, in Bull. AMS 33 (January 1996) 45-70, said:
"… in the latter part of 1994 … the work of Witten and Seiberg … used … N = 2 supersymmetric Yang-Mills theory … The essential ingredient … is … a duality between electricity and magnetism proposed in 1977 by Olive and Montonen. …".

Question 1:
Is it fair to say that Seiberg-Witten Donaldson theory is based on complex (N=2 SYM) electricity-magnetism duality, and Kapustin-Witten Langlands theory is based on extending that duality to the quaternionic (N=4 SYM) case ?

Kapustin and Witten also say:
"… N = 4 super Yang-Mills is most easily constructed by dimensional reduction from ten dimensions. Ten dimensions is the maximum possible dimension for supersymmetric Yang-Mills theory … the metric of ten-dimensional Minkowski space R1,9 or Euclidean space R10 … have symmetry groups SO(1, 9) or SO(10) … To reduce to four dimensions, we simply take all fields to be independent of the coordinates x4, . . . , x9. The components AI , I = 0, . . . , 3 describe the four-dimensional gauge field … while the components AI , I at least 4, become four-dimensional scalar fields … the symmetry is really Spin(1, 9) reduced to Spin(1, 3) x Spin(6) (or Spin(10) reduced to Spin(4) x Spin(6)). The group Spin(6) is isomorphic to SU(4) and is known as the "R symmetry group" of the theory. We will call it SU(4)R. …".

Questions 2 and 3:
Isn't that SU(4)R the conformal group used by Jackiw and Rebbi in Phys. Rev. D 14 (1976) 517 entitled "Conformal properties of a Yang-Mills pseudoparticle" ?

Might it not be useful to extend the quaternionic approach of Kapustin and Witten to octonionic structures based on the approach of Grossman, Kephart, and Stasheff in Commun. Math. Phys. 96 (1984) 431-437, where they said: "… In the present paper we will study the properties of the last fundamental Hopf map (i.e. the Hopf fibration of hte 15-sphere …) in relation to solutions of pure eight dimensional Euclidean Yang-Mills field equations with gauge group Spin(8). … The solution is invariant under the action of Spin(9) … which is a subgroup of the Euclidean conformal group O(9,1) in eight dimensions. … Let us proceed in analogy with Jackiw-Rebbi …" ?

Question 4:
As to octonions, in math.RA/0105155 John Baez said: "… the canonical octonionic line bundle over OP1 generates Bott periodicity …".
What might be the role of Bott periodicity in an octonionic generalization of the Kapustin and Witten paper ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

55. **Aaron Bergman**
April 24, 2006

Is it fair to say that Seiberg-Witten Donaldson theory is based on complex (N=2 SYM) electricity-magnetism duality, and Kapustin-Witten Langlands theory is based on extending that duality to the quaternionic (N=4 SYM) case ?

Not really. The duality is most present in N=4 SYM. Seiberg-Witten theory is, in a sense, the residue of that duality in the N=2 case. When you do a topological twist of N=2 SYM, you get a theory that computes Donaldson invariants. Applying the Seiberg-Witten results on N=2 SYM to this twisted theory gives rise to Seiberg-Witten invariants of 4-manifolds. There are a number of different twists of N=4 SYM. The ones related to Donaldson invariants were investigated by Vafa and Witten. This is a different twist. Nonetheless, all of this is related to the S-duality of N=4 SYM.

I don't know what sense SU(4) is a conformal symmetry. The conformal symmetry in N=4 SYM in 3+1 D is PSU(2,2) ~= SO(4,2). The full symmetry supergroup is PSU(2,2|4). The SU(4) is the R symmetry, not the conformal symmetry.

I'm not sure the relevance of quaternions, much less octonions, to this whole thing. You can't add more supersymmetry to N=4 SYM, regardless, as that would entail particles with spin greater than one, and it wouldn't be a gauge theory any more.

56. **urs**
April 24, 2006

> all of this is related to the S-duality of N=4 SYM.

Which again is thought to come from the modular transformations of a torus on which you have compactified some 6D theory to 4-dimensions.

Do you know if the Langlands thing has a nice interpretation in these six dimensions?

57. **Aaron Bergman**
April 24, 2006

That's certainly a question that has occurred to many people. I can't remember if the relevant twist can be thought of as coming from six dimensions, however.

58. **Who**
April 24, 2006

In response to Dan's question *Has Witten made any reviews or comments or written any papers on background independent quantum gravity…?*
there is negative comment on the (Kodama) QG ground state employed by Lee Smolin and others. see this article:
http://arxiv.org/abs/gr-qc/0306083
A Note On The Chern-Simons And Kodama Wavefunctions
Edward Witten
Exerpt from abstract: "Yang-Mills theory in four dimensions formally admits an exact Chern-Simons wavefunction. … It is known to be unphysical … Similar properties can be expected for the analogous Kodama wavefunction of gravity."

Exerpt pages 1 and 2:
"…This is what we call the Chern-Simons state. It is far from being normalizable,..
The Chern-Simons wavefunction of Yang-Mills theory has an **even more surprising gravitational analog, commonly called the Kodama state [2]. Some authors have proposed the Kodama wavefunction as a starting point for understanding the real universe; for a review and references, see [3].** Our discussion here will make it clear how the Kodama state should be interpreted. For example, in the Fock space that one can build (see [3]) in expanding around the Kodama state, **gravitons of one helicity will have positive energy and those of the opposite helicity will have negative energy**.

—references—
…
[2] H. Kodama, "Holomorphic Wave Function Of The Universe," Phys. Rev. D42 (1990) 2548.
[3] L. Smolin, "Quantum Gravity With A Positive Cosmological Constant," hep-th/0209079.
…
——————
Lee Smolin just finished given a course to University of Waterloo students called Introduction to Quantum Gravity using the cited article as his main text. The course has 25 lectures, video is available online, and it goes thorough much of hep-th/0209079 section by section.

For a modern view of the LQG ground state
http://arxiv.org/abs/gr-qc/0604044
Graviton propagator in loop quantum gravity
Rovelli et al.
this cites Smolin on the Kodama ground state as reference [38] on page 17.
It is still not clear how generally Witten's apprehensions concerning the LQG ground state apply, if they do. For example see page 2 of the Rovelli et al paper:
"…The physical correctness of these theories has been questioned … two "bad" terms: an exponential with opposite sign,… and a dominant term … We show here that only the "good" term contributes to the propagator. The others are suppressed by the rapidly oscillating phase in the vacuum state that peaks the state on its correct extrinsic geometry. Thus, the physical state selects the "forward" propagating [33] component of the transition amplitude. This phenomenon was anticipated in [34]…"

====================
so I would amplify what Peter said, in reply to Dan.
YES Witten has published comment on LQG (in particular the notion that proposed LQG ground state might have forwards and backwards propagating gravitons—something addressed recently in a different context by Rovelli), however as Peter rightly points out this is NOT GERMANE TO THE POSTING.

59. **urs**
April 24, 2006

> I can't remember if the relevant twist can be thought of as coming from six dimensions, however.

At least in 6D there are candidate strings which one could hope would have a topological twist.

60. **urs**
April 24, 2006

Aaron,

if you ever feel like saying more about that autofunctor on the category of branes and how it comes from line operators in a fashion considered in FRS formalism (as remarked by Witten/Kapustin on p. 100), please don't hesitate. 🙂

(I will be awfully busy the next days. After that I might try to have a look at this.)

61. **Aaron Bergman**
April 24, 2006

Dunno much about FRS. Sorry. Guess you'll have to wait for the book.

62. **Tony Smith**
April 24, 2006

Aaron Bergman said that he doesn't "… know what sense SU(4) is a conformal symmetry. The conformal symmetry in N=4 SYM in 3+1 D is PSU(2,2) ~= SO(4,2). … The SU(4) is the R symmetry, not the conformal symmetry. …".

SU(4) is the Euclidean version of conformal symmetry, since SU(4) = Spin(6) = Euclidean version of the conformal group.
If you use nonEuclidean signature, the relevant conformal group is SU(2,2) = Spin(2,4).

As to their use of the Euclidean SU(4) = Spin(6) version of the conformal group, Kapustin and Witten say:
"… The twisted theory is a formal construction in the sense that twisting violates unitarity and only works in Euclidean signature. … To construct a twisted theory in Lorentz signature, we would have needed a suitable homomorphism Spin(1, 3) into Spin(6). Because of the compactness of Spin(6), a non-trivial homomorphism does not exist. If one replaces Spin(6) by Spin(1, 5) to work around this, the couplings to fermions will no longer be hermitian and the energy is no longer bounded from below. … however …

we split off the time direction and take M = R x W, where R parametrizes the time, W is a three-manifold, and
the metric on M is a product. Then time is not involved in the twisting, and the twisted theory makes sense with Lorentz or Euclidean signature and is unitary and physically sensible. …".

In other words, Kapustin and Witten transfer the signature complications from the conformal group to the manifold by continuing to use the Euclidean conformal group SU(4) = Spin(6) but changing the manifold from something like S4 (where time is on an equal footing) to something like S1 x S3 (where time is S1 and space is S3).

Aaron Bergman also says that he is "… I'm not sure the relevance of quaternions, much less octonions …".

Perhaps octonionic structure might be relevant to Loop Operators. Kapustin and Witten say:
"… What is really unusual about the four-dimensional TQFT's … compared to other theories with a superficially similar origin, is that they admit operators that are associated to oriented one-manifolds …
In gauge theory, the most elementary loop operator is the Wilson loop operator. ,,,
In our problem, the dual of a Wilson operator is an 't Hooft operator … ".

Since the imaginary octonions generate the 7-sphere S7, and since S7, although not a Lie algebra, is a Malcev algebra (having no Jacobi identity, but instead a Moufang identity) with naturally occurring Moufang loops, the ideas in the paper by Loginov at hep-th/0109206 might be useful. Loginov says:
"… the linear representations of analytic Moufang loops … are closely associated with the (anti-)self-dual Yang-Mills equations in R8. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

63. **Aaron Bergman**
April 24, 2006

The Euclidean version of the conformal group is SO(5,1). su(4) ~= su(6) is the compact form of the complexification of so(5,1). I'll leave the rest for someone else.

64. **Michael**
April 24, 2006

Peter,
you do know that SUSY gauge theories are intimately related, in many cases exactly equivalent, to certain string theory backgrounds, don't you? SUSY gauge theories "know" automatically about extra dimensions and many other things. If you can draw a reasonably clear line between SUSY gauge theories and string theory, do that and publish it. If not, you better stop claiming that the "new work of Witten has little to do with string theory".

65. **woit**
April 24, 2006

Michael,

Sure, the same theory Witten is twisting to get his TQFT (N=4 supersymmetric YM), is related to string theory on AdS space. But that plays zero role in the Kapustin/Witten paper. This may be the only paper in recent years involving N=4 supersymmetric YM that doesn't reference Maldacena's AdS/CFT paper.

Feel free to point out where in Witten's new work string theory plays an important role. Otherwise, stop making a fool of yourself.

66. **John Gonsowski**
April 24, 2006

"The Euclidean version of the conformal group is SO(5,1). su(4) ~= su(6) is the compact form of the complexification of so(5,1)."

My guess is that Tony is talking the Lie Algebra for the group and you are talking just the group and since Kapustin and Witten seem to be talking Lie Algebra, I'll vote for Tony's rendition even if you would have preferred Tony to be more precise. (I am of course biased and have plenty of trouble getting the general idea without worrying about things like signatures and the group vs. the algebra).

67. **Michael**
April 24, 2006

Peter,

string theory provides a geometric picture that is *equivalent* to the quantum dynamics of the SUSY gauge theory. Whether you talk about one or the other is a question of terminology.

Since you asked, let me give you two examples, which you missed because you either didn't read or didn't understand the paper. The first shows the importance of string theory to the present work, the second the importance of the present work to string theory.

(1) The massless open-string states of the canonical co-isotopic brane furnish the brane's endomorphism algebra. In perturbation theory, open strings form a sheaf of noncommutative algebras. These statements are at the heart of understanding the mathematical significance of eigenbranes. Rest assured that these facts would have remained obscure were it not for the authors' extensive experience with "physical" string theories.

(2) The Blau-Thomson construction of Wilson operators of TQFTs and their magnetic duals plays an important role in recent string theory research. References are given at the beginning of Section 6. The present work deepens our understanding of the construction.

68. **Aaron Bergman**
April 24, 2006

I'll vote for Tony's rendition

You're welcome to think whatever you want.

69. **woit**
April 24, 2006

Michael,

I don't doubt that the author's experience with string theory taught them many things that were useful in this work, and I also don't doubt that lots of facts about these kinds of TQFT are useful to to string theorists. But, neither of these makes string theory an important part of this work. By your logic, string theory is an important part of virtually anything anyone does these days in pure QFT, since it is likely they learned some facts about QFT by thinking about string theory, and it is likely that most anything about QFT will have some application to string theory somehow somewhere.

If you want to feel better about the failure of string theory by going on about how gauge theory and 2d TQFT is really string theory, no one's going to stop you, but it is kind of silly.

70. **anonymous**
April 24, 2006

By your logic, string theory is an important part of virtually anything anyone does these days in pure QFT

Peter sees the light! 😉

71. **Tony Smith**
April 24, 2006

It seems necessary for me to clarify terminology and notation.

Lorentz group = Spin(1,3) of Minkowski space

anti de Sitter group = Spin(2,3) = Sp(2) used in supergravity

Conformal group = Spin(2,4) = SU(2,2)
It is the group Spin(p+1,q+1) of which the Lorentz group Spin(p,q) = Spin(1,3) of Minkowski space is a subgroup.

I used the term Euclidean Conformal group in the sense that the 6-dim space over which the Spin group acts has Euclidean signature (0,6), so that in my terminology Euclidean Conformal group = Spin(6) = SU(4).

I think that in Aaron's terminology Euclidean Conformal group is taken to mean Spin(1,5) in the sense that it is the group Spin(p+1,q+1) of which the group Spin(p,q) = Spin(0,4) of Euclidean 4-space is a subgroup.

I hope that this clarifies the terminology I was using. Even if you prefer Aaron's terminology, and I think that it is fair for you to do so, the meaning of my comments should now be clear.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

72. **Kea**
April 25, 2006

Peter sees the light!

I must say, I agree with anonymous. Come on, Peter! Why not start calling yourself a String theorist? It's just a name. Then we could stop having these silly arguments.

73. **Chris Oakley**
April 25, 2006

I feel that I ought to now apply for a grant from the Templeton Foundation. Using what title, I wonder? Let me see: "0-dimensional, non-supersymmetric string theory in 3+1 dimensions".

No, too boring. They'll fall asleep as soon as they open the envelope. How about "Searching for God using 0-dimensional, non-supersymmetric strings in 3+1 dimensions?"

Better, but I still don't think that they'll go for it. I know! "Searching for remnants of the Primordial Sneezing by studying 0-dimensional, non-supersymmetric string theory in 3+1 dimensions". That *might* do it! Of course, I'll mention God if I have to.

74. **urs**
April 25, 2006

> Why not start calling yourself a String theorist?

I predict that in the future everybody will be a string theorist for 15 minutes.

More seriously, it seems to be true that string theory is like a large dictionary of field theories. Many subtle things about field theories find their explanation when regarding their embedding into string theory. In the case under discussion here, it is not only AdS/CFT. There is an embedding of 4D SYM into a 6D theory which is believed to explain the S-duality of the SYM. That 6D theory is some theory of strings on 5-branes. (I guess that's in the end the more relevant embedding for the Langlands program. But what do I know.)

Fields and strings are tightly interwoven. Many feel that this suggests that strings play a role in nature. This is often refered to as the "qualitative" agreement of string theory with observation. And it's true.

Quantitative agreement is a different issue, as readers of this blog might have heard somebody say before.

While it is only fair to admit the phenomenological problems of the idea of string unification, it is also only reasonable to acknowledge the usefulness of strings for understanding issues in field theory.

75. **Zelah**
April 25, 2006

Here is my piece regarding what Witten et al has wrought!

Whatever you think of Mr Witten's acheivements in regards to phyiscial predictions, as a leader of physics he is our Grothendieck.

Like Grothendieck, he did not actually reach the promised land, but revolutionised the way EVERYONE THOUGHT ABOUT MATH/PHYSICS.

Grothendieck showed mathematicians how to be CREATIVE yet rigourous! Witten is now showing physicists how to UNDERSTAND the world, without experimental data!

I think that Mr woit has constantly underestimated this problem of lack of expermental data for the high energy community since the 1980's. Mr Witten's brilliance was to create a physics program enlightened enough that some the worlds best minds like Maldecena and Brian Greene became physicists and not say molecular biologists!

Reading Mr Witten latest efforts are truely awe inspiring! Even thought I understand maybe 1% of what was going on, The way Witten illuminates areas of Mathematics I was always fascinated by in with such ease is magical to observe.

Finally, it is obvious to me that String Theory is changing everyone way of thinking regarding QFT! The reason for this is SUSY, which is natural from a Stringy point of view, but completely unmotivated from a QFT point of view! SUSY/Strings is allowing theorists to examine models which are intractable without these insights! Seiberg_Witten is unthinkable without these insights!

This is the point. How does one think about SUSY concepts starting from QFT? If you Mr Woit can generate INTERESTING theoretical physics models from pure QFT, please feel free to publish.

An amateur mathematician.

76. **Thomas Larsson**
April 25, 2006

This is the point. How does one think about SUSY concepts starting from QFT?

As something which for all practical purposes has already been disproven by experiments. We just wait for the LHC to be the last nail in the coffin 🙂

77. **Chris Oakley**
April 25, 2006

The position of supersymmetry as a physical phenomenon is summarised by the plight of the Black Knight here.

78. **Michael**
April 25, 2006

Peter,
Not much remains of your original statement. You admit that tools from string theory were important. You also admit that the results can be important in the further development of string theory. You know, and didn't debate, that there is an exact formal equivalence between the field theories at hand and certain string backgrounds.

Clearly, then, the "new work of Witten has little to do with string theory", right? Look, you are being pathetic once again.

"[…] string theory is an important part of virtually anything anyone does these days in pure QFT, since it is likely they learned some facts about QFT by thinking about string theory, and it is likely that most anything about QFT will have some application to string theory somehow somewhere"

You said that like a real prodigy. Of course, that is most likely exactly the point. Let me be clear about this. Absent SUSY, it is not clear what we can learn by realizing the field theory as a string background. The problem is the limit which we must eventually take to decouple the stringy modes and isolate the gauge dynamics. SUSY is the only thing that guarantees no phase transitions can occur in taking this limit. There are three ways out: either we learn that SUSY exists in nature, or we learn how to master the difficulty I mentioned, or both. Giving up? Only Peter Woit could suggest that…

79. **woit**
April 25, 2006

Michael,

Lots of tools developed by people doing string theory will get used in other non-string theory contexts. This doesn't turn non-string theory into string theory. Lots of tools developed by people doing non-string theory will get used in string theory. This doesn't turn non-string theory into string theory.

The fact that N=4 SYM has a string dual is of course interesting, but this doesn't explain the relation to geometric Langlands that Witten is investigating. If you know otherwise, write a paper about it and it will make your career.

80. **Kea**
April 25, 2006

Peter

It doesn't matter what you say! If it's interesting and has anything at all to do with graded algebras, well of course it *must be* String theory. You wouldn't have to be a real String theorist. You could be a "String theorist".

81. **Anon**
April 25, 2006

Yes Peter, don't you understand! If the paper has any Greek letters (translation for Michael to understand: "squiggly symbols") it has to be string theory. See, just like the argument Michael pointed out earlier, this makes your entire blog useless. Way to go Michael!

82. **Benni**
April 26, 2006

sometimes ago woit wrote commenting Douglas talk at solvay where he wrote about the nichtmare of stringhteory:

"under this circumstances, it is standard scientific practice, to acknoledge that [string theory] is a failed project and go on something else"

It will be interesting if Witten will continue to publish String papers or if he leaves the field.

83. **Chris Oakley**

April 26, 2006

It will be interesting if Witten will continue to publish String papers or if he leaves the field.

It looks as though EW has not been working on String theory in the proper sense in the last year or so anyway.

BTW, If the Superstring locomotive hits the buffers I do not think that EW can be blamed. He just does what interests him, and however much he would like to be called a physicist the fact is (in my book, at least) that he is a mathematician. Physics may provide the raw material, but what seems to trip his trigger is playing around with the ideas in a highly abstract way. I do not sense, and never have sensed an urgency in bringing any of this hyper-theoretical stuff to market.

No, the problem is the personality cult that has been built up around him, something he seems to be uncomfortable with himself, but has occurred because of the absence of any real individualism, originality or creative ability amongst the majority of theoretical high-energy physicists practising today.
I think that the psychology is just this: if someone is demonstrably better at something that oneself, then putting them on a pedestal is a way of dealing with one's inferiority.

84. **Moeen**
April 27, 2006

For anyone interested, it seems Edward Witten will be speaking about this topic again at the [Conference in Honor of the 60th Birthday of John Morgan](). An abstract for the talk is available [here]().

85. **woit**
April 27, 2006

Thanks Moeen,

I should have mentioned that Witten will be speaking about this to mathematicians here next Thursday at the John Morgan conference. There will also be quite a few other interesting talks here as part of that conference, and I'll try and write something about them here after the conference is over.

86. **Bert Schroer**
May 2, 2006

Chris,
if you want to evaluate a person's contribution to particle physics you must consider also older contributions. What you say about EW certainly does not take into account his pre-Atiyah contributions.
When physics meets mathematics on a profound level, success is not always guarantied. A story with a very happy end developped when Born had only the diagonal part of the p-q relation and looked for a mathematically talented young collaborator to understand the rest. He first asked Pauli who arrogantly rejected the offer with the warning that Born's mathematical inclination could spoil the immediate intuitive grasp of Heisenberg on the new mechanics (in other words: don't meddle with Heisenberg). He then asked Jordan who hardly went to any physics course (because the tended to be in the early morning hours) but who was helping Courant with some chapters of the famous Courant-Hilbert mathematical physics book. This story had a very happy end indeed; not only did Jordan compute the off-diagonal part, but he also contributed the first avatar (yet still somewhat confused) glimpse into QFT and in the subsequent 3-man paper the three (Born, Jordan and Heisenberg) harmonized perfectly together. In this case you did not have to wait for history to tell.

87. **Chris Oakley**
May 2, 2006

Bert,

Was it Dirac who said that that was a time when second-rate people could do first-rate work? None of the people you mention were second rate, but the same would not apply today. Nowadays, there are no obvious easy wins (not that I can see, anyway) and part of the problem is that some first-rate people have done second-rate work and been rewarded with Nobel prizes.
Anyway … as regards Ed Witten … I have the highest respect for him and although I am no expert I am reliably informed that his early work was outstanding. The problem is with his fan club. Physics research, like most frontier areas, needs a small number of independent thinkers, not these hundreds or thousands of sheep.

88. **Bert Schroer**
May 3, 2006

Chris
you said it. Instead of the great traditional schools which cultivated not only disputes between them but also encouraged criticism within, you now have these globalized monocultures with a guru at the helm. It is not that people are less intelligent but rather that the Zeitgeist of globalized capitalism prevents them from reaching their true potential. A particularly impressive illustrative example is Lubos Motl whos crap he writes about anything non stringy serves a useful sociological purpose. String theorists love him, because reading that stuff spares them the toil to seriously look at other things. It works both ways, because in this way he becomes promoted and being close to the hegemon the danger that he will ever be confronted with his past crap is virtually zero; it is a win-win situation in which only physics looses.

89. **Tony Smith**
May 3, 2006

Bert Schroer said "… Instead of the great traditional schools which cultivated not only disputes between them but also encouraged criticism within, you now have these globalized monocultures with a guru at the helm. …".

That is clearly (to me) true. A question is Why are things different now?

Take, for example, Sommerfeld, a student of Lindemann, who was a student of Felix Kelin.
Lindemann's students in addition to Sommerfeld included Hilbert and Minkowski.
Sommerfeld's students included Bethe, Heisenberg, Pauli, and Stueckelberg.

(List from [http://genealogy.math.ndsu.nodak.edu/html/id.phtml?id=31357]() and Silvan Schweber's book "QED and the Men Who Made It" (Princeton Un. Press 1994) which says at page 667: "… Stueckelberg … obtained his Ph.D. with Sommerfeld in 1927 …".)

Can you imagine a more independent group of brilliant minds ?

Although all of them go back to Felix Klein, and I am sure that they all respected Klein and his work, at no stage in the family tree Klein – Lindemann – Sommerfeld – Bethe, Pauli etc did ANYONE set themselves up as a "guru" to a herd of sheep.
Even though there was NO "guru", space does not permit listing the accomplishments of those people.

It is interesting to compare the math/genealogy of the current conventional superstring guru, Ed Witten:
Ed Witten studied under David Gross (another student of Gross was Frank Wilczek);
David Gross studied under Geoffrey Chew (Gross was Chew's only PhD student);
Geoffrey Chew studied under Enrico Fermi.

Look at their records. Fermi was brilliant.
Chew, with his S-matrix bootstrap program, became a guru shepherd with many sheep, only to see the program fail, being eclipsed by quarks and the Standard Model.
Gross and Wilczek published the details of asymptotic freedom more or less simultaneously with Politzer, but all of them had been anticipated by 't Hooft who had already announced at a meeting (but did not publish in detail) the key idea.
Witten (like Gross and Wilczek) has done some very interesting work, particularly math-oriented, but (like Chew) has become a guru with a herd of sheep, being the guru of superstring theory which (according to Distler et al hepph0604255) "… is constructed to produce an S-matrix with precisely these properties. … analyticity … unitarity … Lorentz invariance …", which is very reminiscent of the Chew bootstrap.

The difference between those groups that stands out to me is that
Klein – Lindemann – Sommerfeld – Bethe, Pauli etc were anchored in European culture,
while
Fermi – Chew – Gross – Witten were, since Fermi came to the USA, anchored in the USA.

You might extend the Klein – Lindemann – Sommerfeld – Bethe line to the USA with Bethe, and then you might claim Feynman (not a PhD student of Bethe, but certainly heavily influenced by contact with him at Cornell) for that line. However, I don't think that anyone would see Feynman as typical of USA physicists.

If you try to extend the Fermi – Chew – Gross – Witten line, you see that the most prominent protege of Witten is Harvard Professor Lubos Motl.

Is there really a cultural difference (European v. USA) that accounts for the guru-sheep phenomenon in today's high-energy theoretical physics ?

If not, then why the difference between the
Klein – Lindemann – Sommerfeld – Bethe, Pauli etc line
and the
Fermi – Chew – Gross – Witten line ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

90. **Bert Schroer**
May 3, 2006

Tony,
as a small footnote one should add the role of Kurt Symanzik in preparing the soil of asymptotic freedom discussion. He started the renaissance of perturbative short distance investigations in terms of parametric differential equations featuring coupling-dependent functions beta and gamma (Kallen-Symanzik equation). That signs in those functions are hugely important he illustrated in the pedagogical but unphysical model of a quadrilinear scalar selfcoupling with changed sign of the coupling term and it was immediately clear to Sid Coleman that all the physical sign of all standard couplings did not permit Symanzik's searched "holy grail" of short-distance weakening of interactions. Sid, who (in the best tradition of Pauli) was a brilliant art critic before his health failed, had a deep admiration for Symanzik and he popularized his program in the US. Parisi in Rome got attracted to it and after 't Hooft's thesis on renormalization of Yang-Mills couplings, Symanzik prodded 't Hooft to look also into that sign question. At a conference in Marseille 't Hooft finally told Symanzik that the model seemingly provided his looked for exception, but he did not announce this publically (probably because he wanted to cross check the nontrivial calculation).
It is interesting that Symanzik in later years on several occasions pointed out that since the asymptotic freedom calculation is done in the wrong phase (i.e. not with a parametric differential equation in terms of a physical mass parameter in the confined phase), the asymptotic freedom statement has the status of a consistency check and not of a structural theorem. In this repect the situation in certain two dimensional models (e.g. Gross-Neveu) is better.
With all appologies to Peter for having somewhat strolled away from his given theme.

91. **Bert Schroer**
May 3, 2006

Sorry for misspelling Callen's name

92. **Tony Smith**
May 3, 2006

Two additional details:

1 – Curt Callan (of the Callan-Symanzik equations) was Peter's advisor at Princeton; and

2 – Since Symanzik studied under Heisenberg at Goettingen, he might be considered part of the line
Klein – Lindemann – Sommerfeld – Bethe, Pauli, Heisenberg, etc

Tony Smith
http://www.valdostamuseum.org/hamsmith/

93. **Aaron Bergman**
May 3, 2006

I know I shouldn't respond to such silliness, but can you tell me *any* sense that Lubos is "the most prominent protege of Witten"? They've never even been at the same university. Witten also has had a number of students who, oustide this bizarre little internet bubble we all inhabit from time to time, are much more prominent than Lubos.

94. **Tony Smith**
May 4, 2006

Aaron Bergman says "… Witten … has had a number of students who … are much more prominent than Lubos. …".

According to http://genealogy.math.ndsu.nodak.edu/html/id.phtml?id=31293 Witten has two PhD descendants:

1 – Dror Bar-Natan (web page at http://www.math.toronto.edu/~drorbn/LOP.html) who has written many nice papers on knots, links, tangles, cobordism, etc., as well as some very interesting papers on Torah codes and equidistant letter sequences in War and Peace. I like his work very much, but to me it seems that his most important work is math and not physics.

2 – Scott Axelrod, who was at one time an Assistant Professor in the math department of MIT (resigned around 1998).Searching the arXiv in math and physics, I did not find any papers by him since 1995. More recently, he seems to be at IBM doing interesting work on speech recognition, natural language generation, etc. He has in the past done interesting physics and math work, but his current work, while also very interesting, does not seem to me to be physics.

As to the prominence of Lubos Motl, he is an Assistant Professor of Physics at Harvard University, and may be the best-known advocate of conventional superstring theory. Not only is his blog well-known, and allowed to do trackbacks with arXiv, but he is a founder and moderator of sci.physics.strings.
As to why I consider him to be a protege of Witten, Motl's web page at http://schwinger.harvard.edu/~motl/sf/arxiv-nytimes.html quotes a 2001 NY Times article as saying:
"… In 1996, when Mr. Motl was a 22-year- old undergraduate … at Charles University in Prague … Dr. Witten, perhaps the premier figure in string theory, astonished Mr. Motl by writing to congratulate him on his 1996 posting. Mr. Motl still refers to Dr. Witten, only half-jocularly, as "the flying god knowing everything." …".
It seems likely to me that Witten has supported Lubos Motl througout his career – moving to the USA – getting a PhD at Rutgers and now a Harvard Professorship.

Of course, there is a certain amount of subjectivity in choosing "the most prominent protege" of anyone.
I have stated my reasons for choosing Lubos Motl as the most promient protege of Ed Witten in the context of physics.
If I have any facts wrong, I am willing to stand corrected and reconsider.

Aaron is, of course, free to disagree and state his choice and give his reasons therefore.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

95. **Bert Schroer**
May 4, 2006

Tony.
Kurt Symanzik as well as Harry Lehmann and Wolfhart Zimmermann started their joint innovative work at Heisenberg's institute, but scientifically they were kind of revolutionary "young Turks" in fierce opposition to H. who at that time was working at another of those ill-fated TOEs (Weltformel) in the veil of a nonlinear spinor theory. Hence to count them into the same genealogy is only true in a superficial geographical sense. There was also a strong subconcious rejection (generation gap) of the older generation because they were blamed for the mess of the Third Reich. The few post-war physics innovators in Germany were youngsters who were forced from school directly into Hitler's army and spend a short time as POWs (Lehmann was a POW in North Afrika, Kurt Symanzik in Marseille and H.-J. Borchers in northern Germany); they were usually a bit older when they returned and started their studies and they were certainly very highly motivated. Rudolf Haag, whom I consider most original, was scientifically 100% selfmade. He went as a schoolboy to the UK to visit his older sister exactly on the eve of the war. During the war he was interned in a Canadian POW camp and that is where he started to become interested in physics. Although none of these people can be easily placed into a genealogy, they started a "school" in the sense of creating a common coherent stock of knowledge and scientific culture. Detlev Buchholz, Klaus Fredenhagen and several others (including myself) have been formed in that school and the very strong and original group of mathematical physicists at the University of Rome (all in the math. department, Sergio Doplicher, John Roberts, Danielle Guido and Roberto Longo) are either directly or indirectly also related to it.
But since the "market value" of such usually more long-range scientific investments in the new globalized physics scene (which determines economical survival) is not very high, you may very well wittness already the end of this genealogical line.

96. **Alejandro Rivero**
May 4, 2006

If you take this thread into genealogy, please let me remember that I moved my page to the wikipedia
http://en.wikipedia.org/wiki/Genealogy_of_Theoretical_Physicists
The condition to add branches in the main page is to keep at a reasonable distance of a prized (Nobel, Fields, etc) physicist, but other branches can be built in the talk page, as a provision for the future(?).

97. **Peter**
May 4, 2006

Tony,

The students of Witten you mention are just his students who were mathematicians. He has had quite a few physics Ph.D. students. One of the first was my fellow student at Princeton Jon Bagger, who is now at Johns Hopkins, and I think could be characterized as more of a phenomenologist.

I don't think it's at all fair to pin the blame for Lubos Motl on Witten. Undoubtedly Witten encouraged him when he was starting out, as he encouraged many people, but Lubos was not one of his students and never worked with him. I doubt that he approves of the kind of ranting that Lubos has made his trademark in recent years.

98. **Aaron Bergman**
May 4, 2006

Tony, that page is for mathematical "descendents" of Witten. His physicist students include Eva Silverstein, Shamit Kachru and Cumrun Vafa (IIRC). Lubos is also far from "the best-known advocate of conventional superstring theory." That would probably be Brian Greene and (god help us) Michio Kaku. So, what you're basing your statement on is a single e-mail. That's quite a stretch.

99. **JC**
May 4, 2006

Bert,

(slightly offtopic)

Besides Heisenberg, Jordan, Stark, etc …, what other physicists went through "denazification" after the war?

100. **Bert Schroer**
May 4, 2006

JC:
probably most of those (Jordan, Stark,…) who were not interned in Farm Hall (for some reason von Laue was also at Farm all, although he was not a member of the uranium club and he was even recollected as anti-Nazi by Einstein). But I am not sure about the extent of de-nazification. The physicists who where in Russian occupied East Germany like Manfred von Ardenne, probably did not go through that process. I have heard that Euler, the collaborator of Heisenberg, who was a communist, got so fed up with what he saw going on at the University of Leipzig that he volunteered for the airforce and died (he was shot down) soon after.

101. **Tony Smith**
May 4, 2006

Aaron and Peter, thanks for giving me more information. In light of it, I would revise my earlier statement from

"If you try to extend the Fermi – Chew – Gross – Witten line, you see that the most prominent protege of Witten is Harvard Professor Lubos Motl."

to

"If you try to extend the Fermi – Chew – Gross – Witten line, you see that the most prominent proteges of Witten are, as working physicists: Jon Bagger; Eva Silverstein; Shamit Kachru and Cumrun Vafa.
In the eye of the general public, the best-known advocates of Witten's conventional superstring theory are Brian Greene and Michio Kaku, but AFAIK neither of them come from the Fermi – Chew – Gross – Witten line, so that line cannot be credited (or blamed) for them.

Thanks very much for the additional information. Now I am a bit less ignorant than before. (Aaron might say that I still have a long way to go, and he might be right, but at least I am making some progress.)

Tony Smith
http://www.valdostamuseum.org/hamsmith/

102. **Tony Smith**
May 4, 2006

Bert Schroer said "… Kurt Symanzik as well as Harry Lehmann and Wolfhart Zimmermann started their joint innovative work at Heisenberg's institute, but scientifically they were kind of revolutionary "young Turks" in fierce opposition to [Heisenberg] who at that time was working at another of those ill-fated TOEs (Weltformel) in the veil of a nonlinear spinor theory. …".

Would it be fair to extend the Klein – Lindemann – Sommerfeld – Bethe, Pauli, Heisenberg, etc line
to LSZ through Pauli, who called them the "Feldverein" ?

As to Heisenberg's TOE (Weltformel), didn't Pauli disagree and withdraw, leaving the paper to be published by by Durr, Heisenberg, Mitter, Schlieder, and Yamazaki in Z. Naturforschg. 14a (1959) 441 ?
Does Durr continue to work on it ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

103. **Bert Schroer**
May 4, 2006

Tony
(with Peter's permission), yes Pauli felt scientifically very close to the Feldverein. He certainly recommended Lehmann highly as a successor to Wilhelm Lenz. Pauli also felt a strong emotional attachment to the University of Hamburg where, at the recommendation of Sommerfeld (Lenz also came fro the Sommerfeld school) he got his first position as an assistant to Lenz (at the time when he had his epiphany about the exclusion principle while strolling along Rothenbaumchaussee). In Hamburg he also gave his last colloquium (about the neutrino). That's when I saw the great man the first and last time. He also made his peace with Jordan but only after pouring all his sarcasm on him for having cultivated that naive Nazi sympathy ("Jordan is in the possession of a pocket spectrometer which allows him to distinguish between a deep red (communism) and an intense brown (the Nazi color)") giving him the advice to care about his pension instead of meddling in politics.
Sorry, let's return again to the present where most of the interesting stories happen in the US.

104. **Alejandro Rivero**
May 5, 2006

I can understand Laue was included in Farm Hall if only for affinity. Anecdotically, note he is buried next to Otto Hahn (If I remember well, they are right to Planck family burial, while Hilbert is left about thirty or forty steps).

About Pauli, his Zurich lineage has produced Osterwalder and some other interesting researchers (eg some things from Albeverio or even from Frolich).

I think lineages are interesting because one has always a tendence to follow the research track of his advisor, so if the advisor is well centered on productive physics his descendents are more likely to be (instead of random wandering around mathphys "interesting" problems). This explains, in part, the clustering of Nobel prizes along lineages.

The Hype Goes On

April 23, 2006
Categories: This Week's Hype

Yet another example of the seemingly infinite supply of bogus "evidence for string theory" is a recent Slashdot posting about a claim to have measured a change in time of the proton/electron mass ratio. It is based on a New Scientist article that states:

If confirmed, the result could force some physicists to radically rethink their theories. It would also provide support for string theory, which predicts extra spatial dimensions.

The original PRL paper about this is here and it is free of anything about string theory. The string theory nonsense appears to come from the following press release, which says:

Standard physics does not have an explanation as to why M_p/m_e has this value, nor can it provide an explanation as to why it would vary. However, superstring and M-theories do provide qualitative explanations for the M_p/m_e value and also predict possible variations of the fundamental constants.

It's unclear where the author got this particular piece of incorrect string theory hype. Not from Lubos evidently, who says that according to string theory the proton/electron mass ratio is constant, unless it isn't.

Update: This particular piece of nonsensical string theory hype even makes it to USA Today:

Such changes to fundamental constants would lend support to modern-day versions of string theory, which has varying constants built into its basic equations. String theory holds that on the very smallest distance scales possible, strings or loops of energy vibrating at different frequencies are the components of sub-atomic particles. String theory has also been a hot topic in physics for decades among theorists looking for a better explanation than "that's just the way it is" of why fundamental constants have their fundamental values. So far, string theory has more critics than results, it should be noted.

Update: The hype even makes it into Nature which is normally better at avoiding this kind of nonsense:

But various versions of string theory suggest that extra dimensions occupied by a particle might affect properties such as its mass. Subtle changes in these dimensions could make physical constants vary slightly, explains Barrow. However, "there's absolutely no observational evidence to support this vast array of ideas," cautions Fabian. The paucity of hard evidence for string theory may be partly responsible for the upsurge in interest in variable constants, Barrow adds; results like Ubachs' could eventually provide a good way to assess the ideas. "I'm sure we'll see some theory papers about this," he says. "I might write one myself."

Comments

1. **knotted string**
   April 23, 2006

   I subscribe to New Scientist. It's useful and sometimes you see outraged letters about the articles it prints, in later issues.

   You quote the passage that has been in every new scientist article about stringy stuff for 20 years:

   'If confirmed, the result could force some physicists to radically rethink their theories. It would also provide support for string theory, which predicts extra spatial dimensions.'

   Journalists have a list of 7 things to remember in a story (who, where, when…) the most essential being WHY?

   Don't go trying to attack them for just doing their job, which is to report orthodoxy like strings. In order to do that, it has to invent some reason that seems plausible. Is that OK?

2. **Lubos Motl**
   April 23, 2006

   These journalists often seem to confuse "new physics" and "string theory". An evolving constant of the Standard Model would surely be new physics. At low energies, it could probably be described by some new light rolling scalar fields as long as the effects are local.

   Such a light rolling scalar field would not be incompatible with string theory. Various vacua of string theory give us lots of different scalars – sometimes scalars that are hard to get rid of. But being new physics that does not contradict string theory is something very different from being evidence for string theory.

   Peter humiliates my uncertainty about the question whether the SM parameters must be constant. I think that they are but I don't have a proof. Do you have a proof in either way? This question can clearly be answered only once we hav the full theory that includes these questions.

   I believe that they are constant because of two reasons: qualitative and quantitative ones. A qualitative reason is Occam's razor. A quantitative reason is that if there were some freedom for the parameters to vary, the expected change per 12 billion years would be larger than the tiny percentage that they measure. Explaining such a slow evolution would be a new hierarchy-like problem.

   But of course such new problems may exist. We have seen quite a few of them.

3. **Jimbo**
   April 23, 2006

   The reality of NS is, that altho they have GREAT artwork, and sexy hyped cover stories, they have consistently tended to exaggerate the veracity (if any) of scientific research at the frontiers.
   I could quote various examples, but the one that stands out in my mind is from 5 yrs ago, when NS did a story about a prof. from U.Conn who was working on a laser-based technique to create closed time-like curves in the laboratory, which would enable transport of subatomic particles forward/backward in time.
   Not only has the research delivered nothing, but its basis was refuted by theorists from the Tufts Cosmology Institute 3 yrs later. To read the article, tho, one came away with visions of the the Morlocks eating the Eloi….NS is great eye candy, but that's as far as it goes.

4. **CapitalistImperialistPig**
   April 23, 2006

Luboš – Is that really you? What you say is not only perceptive but balanced and polite. I particularly like your point about a new hierarchy problem – but it would be nice if some theory (string or otherwise) could sweep up all the hierarchy problems at once.

5. **MathPhys**
April 23, 2006

Lubos,

For a man who refuses to acknowledge its exitence on his own blog, you are surely a keen read of, and participant in discussions on this one.

6. **The Great Landscaper**
April 24, 2006

Notice that if the cosmological constant is small thanks to a fine tuning done once and for all (as happens e.g. in the landscape anthropic scenario) then this experimental result is wrong, because a variation of m_proton/m_electron would lead to a large unobserved variation in the cosmological constant.

Confirming this experimental result would kill the landscape anthropic interpretation. I do not know how Popper would classify something that can be disproved but cannot be proved.

7. **Chris Oakley**
April 24, 2006

Like CIP, it worries me to see a balanced, reasonable post here from Luboš. Is he going soft? The business I worked in for the majority of my working life (investment banking) works on the principle of Survival of the Fittest. Unfit (read non-profitable) genes are ruthlessly eliminated. Academia OTOH seems to work on the principle of eliminating the most interesting genes, leaving a slurry of talentless, inoffensive morons to keep the whole thing going. Does Luboš fear that if he does not keep his head down then he too might be eliminated, I wonder?

8. **knotted string**
April 24, 2006

Lubos has added two new 'political stickers' to his blog:

http://www.northamericanpatriot.com/provictory/

and

http://www.physics.harvard.edu/~motl/no-libs.html

"Say No to Political Correctness"

9. **Chris W.**
April 24, 2006

I do not know how Popper would classify something that can be disproved but cannot be proved.

> ..as a scientific theory that makes universal assertions, ie, almost any theory of substantial scientific interest. Popper made this abundantly clear in his writings.

His position was echoed many years ago by Robert Geroch, when he remarked that he couldn't imagine what a "proof" of a physical theory would look like. Furthermore, he said this in a book aimed at a general audience, *General Relativity from A to B*.

10. **Aaron Sheldon**
April 24, 2006

Um…Is our knowledge of red-shift nearly anywhere as reliable as our knowledge of labratory H2 absorbtion lines? Isn't it a bit like comparing apples to oranges?

I mean, we measure red-shift by assuming the relationship between spectral lines remains constant. But then we introduce an observation that says the relationship between spectral lines is not constant, then how are we to calibrate red-shift?

Something has to give, and I think it will require refining our understanding of red-shifts.

11. **Lubos Motl**
April 24, 2006

Dear Great Landscaper,

although what you write may sound controversial, I tend to agree with you that according to everything we know, a measurably evolving proton/electron mass ratio would imply a much more dramatic evolution of the vacuum energy (imagine that the masses of the light particles to the fourth power contribute) unless there exists some more robust cancellation mechanism that keeps vacuum energy tiny. Your point is pretty good, by the way.

Such an evolving ratio could indeed contradict the current landscape paradigm in which all moduli are stabilized throughout most of the cosmological evolution. It would also contradict my preconceptions about constants being constant, however, without claiming that it's too important. 🙂 I still hope that both the anthropic principle as well as the conjectured evolving constants will be superseded by future research.

Chris Oakley: when I read most of your texts, among which the review of the NER book is the softest one, my peace usually disappears. 😉

Best
Lubos

12. **George Japaridze**
April 24, 2006

Well, I can't help rememebering the paper published in Naturwissenschaften (1931, I think) by Hans Bethe and colleauges. Hope that people will find the story amusing. In that paper it is stated that at absolute zero electron is not completely frozen, but still moves on Bohr's orbit, i.e. according to Eddington, has 1/alpha degrees of freedom, alpha being the fine structure constant. Since there are protons also, the article said, we need to add the same amount of degrees of freedom, according to Dirac's Hole

Theory. In all, we have now 2/alpha degrees of freedom. Now, in order to reach absolute zero, we have to take away from our neutral system -(2/alpha – 1) degrees of freedom. Authors say, and I remember that very well – "We have subtracted 1 to not to account for a rotational degree of freedom" – real perl.
Therefore, authors state, the temperature for the absolute zero is -(2/alpha -1) (Unit-Centigrade). Plugging in -273, authors obtained for a fine structure constant the value alpha = 1/137 which as the stated, "is in the remarkable agreement with the data".

Naturwissenschaften did not get the hoax and actually published the article; Zommerfeld, Bethe's supervisor, was furious. For a little while.

Sometimes I wonder, are these discussions of time-space-orientation-number of dimensions dependence of fundamental constants (have no idea why electron's or proton's mass or fine structure constant are fundamental, better than any other mass or coupling) jokes like Bethe's, or people really believe in what they say? I mean, either we have experimental confirmation, or experimental discrepancy, or some other, fashion independent way to confirm or reject the statements made, or we don't call ourselves physicists any more.

Knowing virtually nothing about Strings and M-Theory (believe that I'm in majority here), and no personal offense to anyone, what I hear about "Landscape Theory of Everything etc" makes me think that the whole idea what physics is and what it's suppose to be is being redesigned.

Pretty sad. Well, at least Bethe & Company honestly admitted that their paper was joke and nothing else – they did not claim that the way of thinking/building up Physics must be changed. It was assumed as a common sense back then…

13. **MathPhys**
April 24, 2006

I remember the time (almost 10 years ago) when I looked up one of Mike Duff's review papers on dualities, dualities of dualities, branes inside branes, etc, etc.

It was the first time I looked up a stringy paper in almost as many years, and I wanted to catch up a bit. I distinctly remember seriously wondering if that's science fiction.

I thought the best words to describe what I was reading were the lyrics of a song that went like:

A circle in a spiral.
Like a wheel within a wheel.
Never ending or beginning.
On an ever spinning reel.

I actually now admire the baroque creativity and imagination of the author and his colleagues, but I also cannot say it's physics.

To me, these are very elaborate mathematical structures, which can be very pretty to look at. But (and I know I'm preaching to the converted) physics must, must make contact with some experiment.

Strings has moved too far from experiments.

14. **WebGuy**
April 25, 2006

Hey, for someone who has mastered physics, do me a favor:
Fix the frames alignment at the bottome of this layout… Sheesh.. 😉

15. **Alejandro Rivero**
April 25, 2006

bethe beck riezler. Yes, it should not have passed referral, even plain reading, because 273 is an arbitrary point and both celsius and absolute are arbitrary scales (the zero absolute is not arbitrary; to say that the frozen point of water is 273 is the arbitrary thing). I would suspect that the editor was in the joke or authorised it.

By the way, we have recently found the equation for the fine structure constant and it is 1/135.28. Sorry for all the guys betting for 1/137.00x

But wait, everyone knows that the inverse of fine structure constant is got using the
unix command **host** **http://www.cobleskill.edu**.

16. **Alejandro Rivero**
April 25, 2006

(That last host command is supposed to be executed in the terminal, not to click on it. Info on closest hosts will be appreciated, just for curiosity)

Now, I found that the paper of Bethe et al has been photo-scanned into a biography of Guido Back but in spanish, so english googlers could miss it. It is at
http://www.ciencia-hoy.retina.ar/hoy02/guido4.jpg
in page
http://www.ciencia-hoy.retina.ar/hoy02/guido2.htm

17. **Chris W.**
April 25, 2006

MathPhys,

Never mind the lack of contact with experiment. Strictly from a theoretical point of view a lot of this stuff is obtuse and ugly. However, it's a great playground for technical virtuosity and pedantic elaboration, for those who are happy to base a career on nothing but that.

18. **J.F. Moore**
April 25, 2006

Peter – sorry to be off topic, and if I missed the news, but could you clarify about when your book will be available? Today is the date that Amazon.com lists publisher availability in the US, but it doesn't appear to be so, alas…

19. **woit**
April 25, 2006

Last I heard publication date was June 1 in the UK, September in the U.S. If I get better information about this, I'll post it here.

20. **Lurker**
April 25, 2006

Hi **J.F. Moore**,

[Not Even Wrong : The Failure of String Theory & the Continuing Challenge to Unify the Laws of Physics (Hardcover)]

Notice the date:

Hardcover: 256 pages
Publisher: Jonathan Cape (**April 25, 2006**)
Language: English
ISBN: 0224076051

Today.

And availability: *Usually ships within 1 to 3 weeks*.

Best regards,

Lurker

21. **woit**
April 25, 2006

I think Amazon has the wrong information, and today is not the publication date. For one thing, I haven't seen any books yet myself….

22. **Anon**
April 25, 2006

Peter,

As publicity and giving an idea about the book, perhaps you could post small excerpts from the book over the next several weeks. That will give others an idea of the level of technical stuff plus content.

23. **J.F. Moore**
April 25, 2006

September! That's a long wait. Thanks for the information though.

Lurker – If you read my comment all the way through to my second sentence, you would have noticed that I already had that (apparently wrong) information. Thanks for the effort, though.

24. **Eli Rabett**
April 25, 2006

Minor point, but the Celcius scale is now defined by absolute zero and the triple point of water, which, at least for a chemist, is not so arbitrary.

25. **Toothpaste**
April 25, 2006

so like what does "compulsive reading Roger Penrose" mean?

26. **amanda**
April 26, 2006

Maybe this is only partially relevant to the topic 🙂 but it seems that arxiv trackbacks from The Reference Frame are no longer supported, see
http://arxiv.org/abs/hep-th/0604151

27. **Lubos Motl**
April 26, 2006

Dear Amanda,

I've never had any automatic trackbacks – it's not supported by blogspot.com. I am only submitting trackbacks manually when I say something that could be useful for the readers of the articles.

Best
LM

28. **woit**
April 26, 2006

I figured Lubos wasn't submitting a trackback in that case since the only substantive thing in his posting was a link to the discussion of the Witten paper over here, and I appear to still be banned from trackbacks to hep-th.

I actually have no idea what is going on with arXiv trackbacks these days, and based on previous experience, it's not clear that it's worth my time to inquire with them about it. A trackback to a posting about a paper (Schroer's) that was in the "physics", not "hep-th" category did appear, but no trackbacks to postings about papers on hep-th have appeared. Maybe moderators for other sections of the arXiv are not banning links to what I write, just Jacques Distler, who is still intent on enforcing a "no criticism of string theory policy", but who knows what is going on there…

29. **Lubos Motl**
April 26, 2006

Sorry, Peter, but the main substantive thing in my short article were links to my two previous articles about the Langlands duality. The second most important thing in the short article are comments of Xi Yin about the relative depth of the arithmetic and the geometric Langlands program, and attempts to embed the arithmetic program into a new version of string theory.

A link to a semi-relevant mostly crackpot discussion here was just a complement, and it would be a good idea for you not to overestimate your importance by two orders of magnitude.

30. **woit**
April 26, 2006

OK, Lubos, I'll continue to look forward to your highly insightful and well-informed discussion of the Langlands program.

31. **Lubos Motl**
April 26, 2006

Let me emphasize one more the critical difference in our understanding of the trackbacks.

I view the trackbacks as a potentially useful service for the readers of the arXiv.org articles who may find a fast summary of some relevant issues, including those that are not discussed in the articles. It may be particularly useful months or years after the articles are submitted and when the readers have already forgotten a large portion of these issues. Consequently, I am only submitting the trackbacks if I feel that there is something relevant in my texts that the readers may actively want to see.

On the other hand, you try to submit trackbacks whenever you write something nasty (i.e. all the time) that is safely irrelevant and uninteresting not only for the actual readers of the arXiv.org articles – but also for all other people with IQ above 90 for that matter. In other words, your goal of submitting the trackbacks is to confuse the readers and spread your and your crackpot fans' moronic propaganda and silliness.

And that makes a difference. 😉

32. **amanda**
April 26, 2006

Dear Lubos,
I was only joking 🙂 . I'm sure Jacques D would never dream of stopping your trackbacks.

33. **Lubos Motl**
April 27, 2006

Of course that he would. He tries to treat everyone in the same way, especially everyone who is not Jacques Distler himself :-), especially if this everyone sometimes irrirates Jacques as some of my postings surely do. 😉

34. **Plato**
April 27, 2006

Lubos Motl:*These journalists often seem to confuse "new physics" and "string theory". An evolving constant of the Standard Model would surely be new physics. At low energies, it could probably be described by some new light rolling scalar fields as long as the effects are local.*

I think Peter here has always stated that any derivation of string theory must arise from the current standard model, and your saying, that string theory does in fact extend from the standard model theoretically?

So any new physics would in essence arise from the standard model so too then it could be called string theory?

So considering IceCube, how would string theory apply?

35. **knotted string**
April 27, 2006

'confuse the readers and spread your and your crackpot fans' moronic propaganda and silliness'

Lubos,

(1) Peter and his readers are countering stringy propaganda.

(2) have you given up climate crackpottery yet?

(3) Can you predict if and when when Jacques Distler will cease to be 'subtly hostile' to questions about string theory from his own students, let alone Peter; see http://utphysguide.livejournal.com/3047.html

😉

36. **woit**
April 27, 2006

Amanda,

Don't know what happened, maybe Lubos decided to manually submit a trackback, maybe Jacques decided that Lubos was so insightful that he decided to add the trackback himself. Anyway, at this point there's exactly one trackback to the new Kapustin/Witten paper, pointing to the commentary by Lubos. There's none to the commentary here since Jacques evidently continues the ban on trackbacks to my blog from hep-th.

I realize this is petty, and one shouldn't take anonymous student evaluations very seriously since they're often off-the-wall, but the one linked to in the previous comment is pretty hilarious, it's from a student in Jacques's string theory class:

"Jacques Distler is quite possibly the worst physics professor I have ever had. He has the uncanny ability to make even the simplest concepts utterly incomprehensible. He is a true intellectual snob, and he treats most questions with open hostility. Unless you have a PhD in math and already know string theory, you will not learn anything from Distler. String theory is hard, but not as hard as Distler wants it to be."

37. **Chris Oakley**
April 28, 2006

This anonymous student should show more respect. I am sure that there are those who pay handsomely for the privilege of learning from the Flaming Spear in the String God's left hand.

38. **Benni**
April 28, 2006

When one reads the discussion there:
http://golem.ph.utexas.edu/~distler/blog/archives/000348.html
I think one cannot state distler as snob or attacking or unfriendly whatsoever.

I think that he tried really to discuss with Peter and ended his discussion with the words:

Your anti-string theory screeds have engendered a strong reaction in some quarters.
Personally, I find them simply boring. They are an uninspired rehashing of the same set of complaints voiced a decade and a half earlier http://arxiv.org/abs/physics/9403001, minus the wit and vigour of the original.
I sat down this evening and reread this entire comment thread. I have come to the conclusion that our discussion has degenerated quite far enough. If you want to discuss the physics issues involved, please feel free to respond, and I will be happy to lend whatever further insight I can. If, however, your interest is simply to post anti-String Theory diatribes, I would ask that you post them on your blog, rather than here.

I think, seeing the above discussion, one can say, that distler is far away from beiing unfriendly or aggressive.

and this was the beginning of "not even wrong"!

39. **Nigel**
April 28, 2006

Benni, I notice Jacques didn't delete your comments from his blog! I wonder why? 😉

Jacques was charming when he deleted my constructive and helpful suggestion on his blog and then gave a non-sensical reason over on Cosmic Variance!

See http://cosmicvariance.com/2006/03/03/crackpots-contrarians-and-the-free-market-of-ideas/#comment-15117

It is just as well I'm used to censorship and so I don't take a mere comment deletion as a personal insult

40. **Benni**
April 28, 2006

Nigel wrote
>Benni, I notice Jacques didn't delete your comments from his blog!

no! Jacques did not delete me. Instead, he put me into his killfile. No one with the username "Benni" can post comments on his blog anymore!

41. **Bert Schroer**
April 29, 2006

Benny,
I only know Distler in his role as a hep-th moderator, but my little experience casts serious doubts on his objectivity.
Several years ago I posted a paper about the protagogist of QFT QFT Pascual Jordan on hep-th (hep-th/0303241). He is (politically) a very controversial figure, but I found some really breathtaking connections of some of his (at that time unaccomplished) ideas to ongoing conceptual developments in QFT.
About a month ago I extracted a shorter version, updated it with new scientific remarks of interest to QFTs and posted it in physics (physics/0603095) since the context was a bit more political (which only led to a change of title and introduction). For this reason I found it more appropriated to post it in the physics and society part with the idea of making a cross listing (it was the only crossing, so there was no abuse of the cross listing system involved). Well, the moderator took it off with the argument that its content is not related to particle physics. This of course creates a grave inconsistency in the moderating system with the past decision where the content had its main listing in hep-th (and now it cannot even be cross listed).
I would like to point out that the moderators of an international archive cannot act as the prolonged arm of its government and reject cross listing paper because it addresses a problem which has political ramifications. It would be unimaginable to have an international server located in Europe which rejects cross listing papers from US contributers because in addition to the relevant content for particle physics there is something which the moderator does not like.
Benny, in this context it does not interest me whether Distler (as he is often painted in this weblog by others) is a foot soldier of string theory or not. At issue is that freedom and democracy which his government preaches everyday to others. His role is not that of a hegemonic nanny but that of an objective servant of a world community.
I should add that I also completely agree with Tony (see the Langlands… discussion) that the Witten-Kapustin paper is mathematical physics and should have appeared as a cross listing in hep-th. The reason why this is not done is sociological not scientific (e.g. the moderator may argue that most of the potential readers are customers of hep-th and at this late stage they may even get confused about the authors having changes their motivations).

42. **Benni**
April 29, 2006

physics/0603095 is not a borderline case. One can clearly state, that it has nothing to do with hep-th.

In my opinion, the moderation rules of the section "physics and society" should be more strict.
There maybe relevant and interesting studies, which investigate eg. social networks with physical methods.
But Arxiv should not be the place where one can express his political opinions or rantings. Only papers acceptable in scientific journals of an exact science should arrive at arxiv org. physics/0603095 is in my opinion not of a scientific value that is high enough.

43. **Bert Schroer**
April 29, 2006

Benny,
what about my first paper which was even posted on hep-th and not just cross listed? Was the monitor too lenient?

44. **Benni**
April 29, 2006

the first paper is in hep-th. Although I think it is more appropriate for physics.hist-ph where is the correct place for many similar papers. See
http://xxx.lanl.gov/list/physics.hist-ph/recent
I think your paper is at the wrong place.

BTW, I think the section physics.pop-ph should be either moderated or shut down!

45. **Who**
April 29, 2006

> that the Witten-Kapustin paper is mathematical physics and should have appeared as a cross listing in hep-th. The reason why this is not done is sociological not scientific (e.g. the moderator may argue that most of the potential readers are customers of hep-th and at this late stage they may even get confused about the authors having changes their motivations).

sounds interesting. perhaps you could spell that out more detail for those of us like myself benefitting from explicitness.

It sounds as if you are saying that the W-K paper, for example, is really math-ph, it is not high-energy particle theory. But the habitual readers of hep-th have become so accustomed to reading mathematics and believing it is particle physics that it would CONFUSE them if you abruptly tried to sort things out and apply the categories as defined. They might wonder "What's wrong, isn't Witten doing particle physics any more? Has he switched fields?" The hypothetical moderator doesn't want to confuse the customers, so he has a reason to classify papers incorrectly.

I suppose that is what you mean. It's a droll reflection on the situation that has developed.

46. **Aaron Bergman**
April 29, 2006

Bert, you're aware that Jacques is Canadian, right?

Speaking only for myself, the first paper has some physics content. The second has absolutely none. I can't discern any insconsistency here. The interests of a "world community" would hardly be served by allowing extended political argument on hep-th.

47. **Chris Oakley**
April 29, 2006

Bert,

Being typeset with TEX and having references at the end does not make something a scientific paper. physics/0603095 is more the sort of thing I would expect to see in the New York Times with a title like, "The folly of invading Iraq: a physicist's view"

48. **Bert Schroer**
April 29, 2006

To Who says,
yes you got it correctly, but there is something to be added. Another reason why hep-th is used for math-ph. communications is that this site is the best for reaching the majority of mathematicians. Most of them believe that hep-th is the direct source where they can get the fresh and mathematically raw data for new conjectures in their most original metaphorical unspoiled form, and in order to make it a genuine market place they also sometimes post their more refined versions there. In fact most of them believe that we particle physicists live in our best of all times. They don't realize that they are erecting their golden castles on our physical ruins.
My remark about reaching one's readers by maintaining certain traditions was not really a criticism. And to be honest, given the sociological situation as it is, I am not totally surprized that my sociological/historical article was not even permitted a cross listing (it was never intended to be posted in hep-th).
I was just thinking that the same argument of having one's readers at a certain site which is conceded to most contributers would also apply to me.
No Chris, the article is not suited for the NYT. I think it is of interest to my particle physics colleagues to know that the the protagonist of our area was a rightwing belligerent character (just like Motl). He believed in the Heraklitean tradition that a war once in a while is necessarry to keep up progress. What really did him in was that by the time the Nazis came to power he genuinly thought that the "new order" of the "new state" was the sociological counterpart of the quantum revolution he initiated together with Born and Heisenberg and all by his own (and against the rest of the world) he created QFT in its modern form (bypassing configuration space from the beginning).
I think it is interesting to know the main reason why after Einstein and Heisenberg one of the greatest physicists of the 20 century lost the chance to be proposed for a Nobel prize and became the "unsung hero of QFT". I wrote this article in the Einstein year and of course I was thinking that Jordan lost the Nobel prize because he was living (politically speaking) in the wrong time: if he could be time shifted to the presence he may not only have gotten the Nobel prize but may have joined the neocons and played an impotant role in justifying the present war. A great man but a rightwing nut, life is complicated.
Chris why do you think I should be prevented to indicate the existence of this article to my colleagues who never look into physics/?

49. **JC**
April 29, 2006

Bert,

(slightly offtopic)

During the Nazi era, were there any strong reasons as to why Heisenberg stayed in Germany? Was he ever threatened with expulsion to Dachau by the SS, for using relativity theory (classified as "jewish physics" by the Nazis)?

50. **Thomas Larsson**
April 29, 2006

JC, your question is answered in Todorov's Heisenberg biography, physics/0503235

51. **Who**
April 29, 2006

thanks Thomas, history and the life experience of major figures in a field are surely part of understanding it. I was happy to find the short biography of H. you pointed to readily available on the arxiv, and read it with interest.

Apropos nothing in particular, another biographical document available on arxiv, one I like very much, is a memoir by the late Asher Peres "I am the cat who walks by himself"
http://lanl.arxiv.org/abs/physics/0404085

memoirs and history such as this give the arxiv an extra dimension, and make it all the more remarkable as a web institution

52. **Chris Oakley**
April 30, 2006

Bert,

I am sorry, but I think that physics and politics should be kept separate. Although I personally would not object to a cross-listing of your article in hep-th I can at the same time understand why it was not included. I should add that Peter (who obviously, like yourself, has strongly-held political beliefs) does an excellent job of keeping this kind of thing out of this forum. To see what happens when one is unable to exercise this manner of self-control one need only take a look at Lubos's blog. However brilliant (?) his scientific insights, to many he is going to just be that nutty, right-wing Harvard physics professor.

53. **Bert Schroer**
April 30, 2006

To Who says
Most of my knowledge about Heisenberg's role in Nazi-Germany came from the Farm Hall report (the result of wiretapping on the interned members of the German Uranium club) and from a biography by Klaas Landsman. I think these sources were also used by Todorov. Somewhere it is mentioned that Heisenberg was in trouble when an SS-journal accused him of being a "white Jew". but on the other hand he became the boss of the German war related Uranium research group and was probably consideres by the allies a greater threat than Jordan. Besides a personal encounter of Jordan as a student my interest in his biography was greatly stimulated by an article of Engelbert Schuecking "Jordan, Pauli, Politics, Brecht, and a variable gravitational constant" which appeared in the October 1999 issue of Physics Today. Different from Heisenberg's relation to the Nazi's (he was never a party member) Jordan joined the party quite early (already in 1933) but despite his open Nazi sympathies (apart from anti-semitism) the Nazi's did not trust him sufficiently to consign him with any important war-related scientific task; in fact these sympathies were never reciprocated. After the war he took a pro-US position in the issue about nuclear weapons against a campaign supported by Born, Heisenberg and another 16 members of the German scientific establishment.
Coming back to the main issue, there seems to be a misunderstanding; I never intended to post my article in hep-th and I completely agree that it does not belong there, but I thought I could let the hep-th readers (most of my particle physics papers were posted there) know by crossing listing (there is unfortunately no other way) that there is such an article.
I still do not understand why there is a censorship against cross-listing (the policy is against an abuse in form of mutiple cross listings, but making one crossing is certainly within the policy). Why isn't there more transparency on this point?
Stories like that of Asher Perez are fascinating to people like me who are approximately from the same generation and therefore are deeply stamped by the tragedies of the 20 century (without Who says remark I would have never come across these interesting biographical notes). Assuming for the benefit of the argument that Asher's scientific articles would have been posted in hep-th, why would he be prevented to indicate the existence of a historical article concerning his own life by a cross listing to hep-th? Does the monitor worry about the hep-th consumer getting too much distracted?
A short side remark may put this point into sharper focus. One of the most interesting episode of my life was the way in which the historical figure of Olga Benario one day entered the 10 year lasting collaboration on particle physics with Jorge Andre Swieca from Brazil (and linked our otherwise disparate life lines in a completely unexpected way). It would be obvious why I am deeply disappointed with a recent Brazilian film "Olga" where this pivotal figure whose biography reflects the tragedy of the 20 century is used as a vehicle for telling a love story. As a particle physicist you want to make such reminiscences available at a place where you scientifically contributed for a significant part of your professional life and not at a place which your colleagues rarely visit. I thought that among other things this is the purpose of cross listing. As a lack of transparency in the application of this policies by the hep-th monitor I still do not know whether I would be permitted to do this.
Chris, did you really read my article to the end (Jordan&war)? Or were you to much offended by my introduction ("rant") and had to stop?

54. **island**
April 30, 2006

> I am sorry, but I think that physics and politics should be kept separate.

I think that's the most sane statement that's come out of science in about 20 years.

INSANITY
…we all know that in all matters of mere opinion that [every] man is insane–just as insane as we are…we know exactly where to put our finger upon his insanity: it is where his opinion differs from ours….All Democrats are insane, but not one of them knows it. None but the Republicans. All the Republicans are insane, but only the Democrats can perceive it. The rule is perfect: in all matters of opinion our adversaries are insane.
-sam clemens

55. **Bert Schroer**
April 30, 2006

Chris,
since our remarks were posted at the same time, let me come back to them. If you feel offended by my US policy criticism I can understand this and perhaps I overdid it. It is the kind of reaction of disappointed love which is especially strong with people you feel very close to (the most intense arguments are often within the same family). Of course I did not forget the pivotal US contribution to the liberation of Europe from the Nazis and facism.
I was a research associate at the University of Illinois (60-62), went for one year to Priceton (where I collaborated with Korkut Bardakci) and for the rest of the decade got an assitant professorship (later associate) at the University of Pittsburgh. This was the happiest time in my life and my memories of life in the US are 100% positive (one reason I stayed away in more recent times is that I do not want to spoil them). As somebody who run away from East Germany I once got into a weird situation when, during my two years as a research associate working with Haag I went with him to a summer school in Boulder/ Col. When one day there I bought a Time Magazine I found an article about two mathematicians (Bill Martin and Bernon Mitchell) who did some secret work for the NSA and fled via Havanna to the Soviet Union. I remembered Martin's name from having taken over his rented apartment and bought his used piano. But the photos at those times were very unclear and I thought there was a mix-up on my part. When I returned to Champaign-Urbana the CIA people were already waiting for me; they found the cheque with which I payed the piano in a Wahington safe. They suspected me to be involved and what really stunned me was that they knew incredible details of my life including the part in East Germany. It took me some days of thinking to resolve my surprize. When I went through West-Berlin by plane to West-Germany I had to go through a camp (Fallingbostel near Hamburg, a former internment or concentration camp). This lasted some weeks and during this time there were frequent interrogations by German authorities. These transcripts of those were passed to the CIA. After one month of meeting with the two agents in a bar in Champaign-Urbana (naturally them paying the beer) I finally succeeded to convince them that this was a weird coincidence. Decades later when I told this story to Ludwig Faddeev at a conference dinner he laughed at me and told me that both had applied for a job at the Steklov Institute in St. Petersburg; by that time they apparently had Russian wifes. It seems that communism succeded to converttwo gays (at least temporarily) into hetero-sexuals. While I was living in Champaign-Urbana I moved outside the campus and rented the souterrain of a house of a very conservative fundamentalist family. They observed every step of my life. They were worried about girl students visiting me but what got them really upset was my participation in the unitarian church where I sometimes went because they had interesting speakers (it was more like a social club and many of the speakers were from the civil rights

movement). When they reprimanded me as a refugee from eat East Germany for being sympathetic to communists I left and moved again on campus.
All these episodes added some spice to my life but they never dented my admiration of the US.
During the last years in high school in stalinist East Germany my school mate was Hellmut Karasek who became later a well-known journalist and literary critic. Our admiration for the American way of life was without limits. He left East Germany a couple of month after me. Contrary to my experience he had problems to enter the US after his studies in Tuebingen. (I think because he was a paying member of the FDJ during the last year of school). When the ripples of the Ms Carthy era finally vanished he went to tne US anf lived for a couple of month together with Billy Wilder in LA. With the material he collected he wrote a fabulous book, a kind of Billy Wilder biography. One of the most interesting interviews of Woody Allan (which appeared in Germany) were done by him.
It is true that my enthusiasm suffered some serious setbacks in more recent times, but this may yet turn out to be transitory.
The criticism of string theorists does not penetrate much, but your remarks Chris, really entered my skin. To be compared to somebody like Motl who uses photos of bombers with nuclear weapons and who can hardly await the "nuking" of Iran (it makes me vomint looking at this) really hurts. He certainly represents all the disagreeable sides of Pascual Jordan without having yet shown the innovative talent. One of the points of my essay was of course that in principle those two apparent extremes may come together.

56. **Chris Oakley**
April 30, 2006

Bert,

I finished your article just now. I still think that your article belongs in the NYT, but maybe with a title like "Physics and pre-emptive wars: the case of Pascual Jordan".

Off-topic, and we may be in comment-deletion territory but, what do you know about Kallen's work circa 1950, specifically "Formal integration of the equations of quantum theory in the Heisenberg representation", Arkiv för Fysik, bd. 2, #37, p.37 (1950)? I realise that you would have only been 17 at the time, but if you have any recollections I would be very interested to hear them. The work is referred to in his 1972 text book on QED.

Island,

The situation can be summed up as follows:
*Opinions are like ***-holes. Everyone has one.*

57. **Arun**
April 30, 2006

There is a letter in the NYTimes today, signed by Leonard Susskind, Freeman Dyson, David Gross and Walter Kohn, and 15 other members of the National Academy of Sciences – expressing concern about Guantanamo.

"Although this is not a scientific issue in the usual sense, we feel that to ignore it would be to abdicate our responsibility to the truth".

58. **Arun**
April 30, 2006

http://www.nytimes.com/2006/04/30/opinion/l30gitmo.html
(I don't know if it is reachable without subscription.)

The signatures are:

Leonard Susskind
Professor of Physics, Stanford University
Palo Alto, Calif., April 19, 2006

Michael Aizenman
Professor of Mathematical Physics, Princeton University

James Bjorken
Emeritus Professor of Theoretical Physics, Stanford University

Stanley Deser
Professor of Physics, Brandeis University

Freeman Dyson
Professor of Physics, Institute for Advanced Study

Mary K. Galliard
Professor of Physics, University of California at Berkeley

David Gross
Professor of Physics, University of California
Winner of the 2004 Nobel Prize in Physics

Leo Kadanoff
Professor of Physics and Mathematics, University of Chicago

Walter Kohn
Professor of Chemistry, University of California at Santa Barbara
Winner of the 1998 Nobel Prize in Chemistry

Elliot Lieb
Professor of Mathematics and Physics, Princeton University

Joel Lebowitz
Professor of Mathematics and Physics, Rutgers University

Douglas Osheroff
Professor of Physics and Applied Physics, Stanford University
Winner of the 1996 Nobel Prize in Physics

Joseph Polchinski
Professor of Physics, University of California at Santa Barbara

Edwin Salpeter
Emeritus Professor of the Physical Sciences, Cornell University

John H. Schwarz
Professor of Theoretical Physics, California Institute of Technology

Frank Wilczek
Professor of Physics, M.I.T.
Winner of the 2004 Nobel Prize in Physics

Edward Witten
Professor of Mathematical Physics, Institute for Advanced Study

Richard Zare
Professor in Natural Sciences, Stanford University

Bruno Zumino
Emeritus Professor of Particle Theory, University of California at Berkeley

59. **JC**
April 30, 2006

Bert,

(also slightly offtopic)

How much was physics (and science in general) affected by Marxist type ideologies during the East German communist era?

I remember stories from German colleagues during the mid 1990's mentioning that a number of academics at the Unter den Linden campus in Berlin (and other universities in the former East Germany), were dismissed shortly after German reunification. Many of the dismissed academics were allegedly people who specialized in things like Marxist economics, communist "political science", etc …

60. **Bert Schroer**
April 30, 2006

Chris,
o.k. I am more than happy to get away from the present political glitch (unlike the past one it has not yet solidified) back to particle physics. I also want to make my peace with Distler, but a bit more transparency in applying those cross listing rules would be very helpful. If it is the admixture of ongoing politics (he probably does not mean the solidified past politics which has became part of immutable history) this should be stated and then one knows in advance and can adjust formulations appropriately.
Of course I did not read Kallen's article at the time when it appeared, but I do remember his handbook article where he based the perturbation theory directly on the Heisenberg fields. This method had been later called the Yang-Feldman approach and for interactions in terms of polynomial pointlike free fields the result is the same although to go from the time-ordered to the retarded functions is a bit cumbersome. The Kallen approach is less popular because you have to cope with two different c-number two-point contractions. In the late 80s and 90 this approach was sucessfully revived (for certain problems) by Othmar Steinmann. In the recent noncommutative settings the Yang-Feldman approach seems to be the conceptually safest (you want to check that the asymptotic part called "incoming" really has the algebraic commutation structure of a free field which is highly questionable since the validity of cluster factorization and macro-causality is in doubt, people only looked at the unitartity problem but forget the macrocausality properties without which the theory does not allow a particle physics interpretation) I have reviewed this situation in AOP 319 (2005) 92. I think you find further useful literature in there and as far as I know nothing really new has appeared afterwards.
I noticed through a recent email exchange with Wally Greenberg that some authors really thought that noncommutative theories continued to be local. He tried to straighten them out on this. But if they did not read my article (where this was an important point), they probably will not look at his either. Maybe he succeeded because, he went to Finnland and had the chance to talk to them directly.
I am wondering what Peter thinks; instead of banging away at string theory we are using his weblog for other purposes.

61. **Chris Oakley**
April 30, 2006

Hi Bert,

Thanks for the clarification. I cannot understand why it is called the "Yang-Feldman" approach when all they do in their 1950 Phys. Rev. paper is to say (end of sect. 1A) "general rules can be formulated for writing down the various terms to any order in e but, for practical compuatations, they are usually more complicated than Feynman's rules". But where are these rules? Kallen actually derives them! I don't know if you've seen my work on the subject (linked to from my name above), but I was just using free fields as terms in an expansion rather than representing asymptotic states, much in the spirit of Kallen's earlier 1950 paper (Ark Fys, band 2, no. 19, p.187). It is indeed more cumbersome but has the advantage that (i) one can read off matrix elements directly and (ii) it can also be used for non-local field equations, and specifically ones that do not lead to nonsensical infinities.

So one could thereby develop a comprehensive QFT without infinities and without having to consider Superstringy abstractions.

(NB: The last sentence was added just to try to forestall deletion of this comment on the grounds of being off-topic).

62. **Bert Schroer**
April 30, 2006

As to the anti Guantanamo campaign: better late than never.
Let us be united on this matter. The present discrepancies on matters of particle physics are quite insignificant compared to taking care of our civilization which plays an essential role in modern science.
When I criticise ideas then even in those cases where I mention names (because they are inexorably related to some ideas) I never put the character and sincerity of persons into question and I think Leonard Susskind and David Gross know this. What they may not sense as much as I, Peter Woit and many others is that their influence down to the carries of present day students (in particular on their impact ratings, stipends etc) is so strong that in case the gigantic jump into the conceptual blue yonder (using Feynman's style of pointing to the speculative nature of frontiers) fails (only few jumps succeeded in the past, there is always a risk), we may end in a long period of a particle physics void (also the new exiting astrophysical problems require a profound understanding of particle physics). The reason may simply be that too much time was spent in the blue yonder so that the solid ground from which this journey started will be forgotten and after several generations can only be reconstituted by

doing “particle physics archeology”. The survival of the fittest in the sense of a hegemony material control is not a mechanism which should be applied to particle physics especially if a hegemon can change the rules of attributing weight to the ongoing research.
To JC:
life in eastern Germany at the height of Stalinism (starting from 1951 to the time of Chruschov) in high school was very tense because most us (including Karasek and me) came from the lower middle class (my father was a fiddler who stabelized his life by taking on a fixed job in the administration of the Solvay caustic soda production) and the tension between home and school (with two type teachers, the old ones to whom you could be somewhat confidential and the new who we generally did not trust). We had a Russian teacher from Kazachstan named Olga Benkenstein and we played terrible pranks to her just out of frustration and sensing that she was weak, not because we disliked her. If Karasek and I ever would have found the chance for a later appology, we would have done this; youngsters can be very cruel (already in those days).
Concerning the treatment of the “Ossies” by the “Wessies” at the universities (example the Humboldt university) it was like other situations of sudden changes with part of society becoming powerless: this is a time of settling scores, personal denunciations people loosing their jobs etc. (not so different from what happened during the military dictatorship in Brazil during the 60s).
Chris,
I think you are right, the main contribution of Yang-Feldman beyond Kallen is to bring the equation of motion into the Yang-Feldman integral form (where the zero order in fields appear explicitly), but I do not have a reliable recollection. If I remember more (or find the handbook article in the cbpf Rio de Janeiro library which is probably the best in Brazil, I will let you know. The proper formulation of causal perturbation theory does not lead to infinities because the distributional character of fields is taken into account. The problem is that in so called nonrenormalizable theories the known setting does not tell you to intrinsically separate out a finite parametric submanifold (finite number of coupling parameters). This refers only to the standard way of doing perturbations, and not to future other implementations. Brunetti and Fredenhagen recently gave convoncing arguments that by using their new local covariance formulation they can recover background independence (and perhaps develop an background independent concept of a graviton). Mund and I test presently our concept of massless higher helicity string localized fields which as a result of their simultaneous fluctuation both in Minkowski and de Sitter space (the space of spacelike string directions) have much milder fluctuations in Minkowski space. The new conceptual problem is to implement interactions for string-localized fields. Only if these results turn out to be negative you can say with more confidence that there unlike gauge theory there exists no finite parametric submanifold for helicity=2. Then it would make sense to say that QFT cannot incorporate perturbative gravity. String theorists tend to premature apodictive statements because they confuse their carricature of QFT with its autonomous content.

63. **island**
April 30, 2006

> […] the National Academy of Sciences – expressing concern about Guantanamo

I think that it’s the implied endorcement that’s insanely dangerous.

64. **Bert Schroer**
April 30, 2006

retarded answer to a question of JC:
high school physics was not at all influenced by Stalinism. Biology was a different story. Scharlatans like Lyssenkow and Mitschurin had to be given a central role in biology classes. This was often done by teachers against their own better knowledge. I remember a funny phrase of my biology teacher whose halfhearted attitude in teaching such things ended in bloopers like this: “what the wheat was going through within half a year after disseminations on the fields, Mitschurin did in two weeks in a shelter”. This referred to a process called “jarowisation” in which the wheat grains where sprincled with icy water to make them more resistent against the cold so that one can grow wheat also in colder northern zones.
There was indeed a lot of injustice done against some of my colleagues who were physics professors at east german universities. For example Arnim Uhlmann who was a professor in Leipzig was retires against his will and only reveived a fraction of the retirement pension of a professor in the West. He was a deputy of the peoples chamber but nobody came up with any complaints of any personal sufferings from any action by Uhlmann. It is my conviction that everybody has the right to make his living and his carrier even in a dictatorial situation as long as he does not make life miserable for others. Compromises are allowed, see Brecht’s famous play about the life of Galilei.

65. **Bert Schroer**
May 1, 2006

Chris,
I am only using this weblog since I was unable to find your electronic address; the content of this remark is not of general interest to the participants of this weblog.
Kallen did go into some perturbation theoretical details (as you said) whereas Yang-Feldman did look at details. But even Kallen did not provide the computational “guns” (even though his first name was “Gunnar”) to do the n-th order. I think that doing perturbation theory directly in terms of Wightman functions (this is what Steinmann allegedly did) is equivalent to the Kallen-Yang-Feldman setting. Actually the proof of equivalence with the time-ordering approach is technically and conceptually quite involved. In a completely watertight form it is probably contained in the University of Hamburg project of a mathematical physics formulation of all aspects of perturbation theory (Fredenhagen, Brunetti, Duetsch….)
I met Gunnar Kallen only once; he looked very young for his age. As a student I probably was more afraid of his temper (“Gunner”) than others. He was extremely bright&fast and had the temper of Pauli (and my adviser Harry Lehmann, who however was very soft and helpful if it came to students). On several occasions he got into heated discussions with Julian Schwinger.
For some reasons he reminded me of the main character in Robert Musil’s “The Confusions of Young Torless”.
He was an amateur pilot and died in a crash when he was flying together with his wife in a Cessna (she survived). The weather on that day was excellent and he was an experienced pilot. There was the rumor that got extremely depressed when his wife was diagnosed with terminal cancer, i.e. he that he comitted suicide because he lost the will to survive his wife.

66. **Chris Oakley**
May 1, 2006

Hi Bert,

My e-mail is coakley@cgoakley.demon.co.uk. Yes, I had read the story of Kallen’s untimely death. Obviously a great loss to theoretical physics.

You probably saw that I got into fairly heated discussions with the referees over my papers between 1986 and 1987. All probably completely pointless; I suspect that a lot of the aggravation could have been avoided if either the referees or myself had been better aware of the work done by Kallen et al in the late 40’s/early 50s. Of course, I would be very interested to hear about any material that could possibly be relevant.

The basic idea, though, seems to have been all but extinguished in the literature. The Stueckelberg connection I only found out through this web log, for which, thank-you Danny Lunsford (and Peter for providing the forum) … the Kallen work I discovered only through an internet search. He does mention it in his QED text book, but – I don’t ever recall ever studying this, at least not before last week.

67. **woit**
May 1, 2006

Been away for a long weekend, mostly away from the internet. Now back and will write about some new things soon. You’re all forgiven for the off-topic comments since many of them were quite interesting….

68. **Who**
May 1, 2006

damn! why didn't I get in there during the break from supervision? I could have been talking all weekend about Smolin's idea of cosmo natural selection determining the fundamental constants so as to maximize black hole abundance. what a wasted opportunity!

69. **woit**
May 1, 2006

Who,

Even from a very slow and hard to use dial-up connection, I might have done what I could to put a stop to those…

70. **SomeBody**
May 1, 2006

In http://www.math.columbia.edu/~woit/wordpress/?p=380#comment-10196 Bert Schroer says

"[…] somebody like Motl who uses photos of bombers with nuclear weapons and
who can hardly await the "nuking" of Iran (it makes me vomint looking at this)"

Pardon me, but where did Motl do any of this? His blog post on the possibility of a nuclear strike on Iran

http://motls.blogspot.com/2006/04/readying-massive-nuclear-strike-on.html

looks nothing like your description of it. Did I miss something?

Dan Freed on Twisted K-theory and the Verlinde Algebra

April 26, 2006
Categories: Uncategorized

Dan Freed recently gave the Andrejewski Lectures at the Max Planck Institute for Mathematics in the Sciences in Leipzig, and has put the slides from his first lecture [on-line](). These give a beautiful overview of his work with Hopkins and Teleman relating loop group representations and equivariant K-theory, and explain one aspect of the relation to topological quantum field theory. His second and third lectures aren't available on-line. The second was supposed to cover the way they use Dirac operators, which is explained in their papers. The third lecture was evidently about the relation to Chern-Simons, which isn't in their papers so far, and which I'd be quite curious to know more about.

This fall, Dan will be giving a graduate course on Loop Groups and Algebraic Topology, which should be quite interesting.

Comments

1. **Dick Thompson**
   April 26, 2006

   I have a question/suggestion. Everything these days seems to involve twisting, and apparently there are different kinds of twists (e.g. Witten's new paper has a different kind of twist from his previous ones). Could we get a general intro to the twisting concept? Are twists the new representations?

2. **Peter Woit**
   April 26, 2006

   Dick,

   Sorry, but I don't immediately see any connection between the two kinds of twisting. Maybe there is one if you think about it the right way though…

3. **M.**
   April 27, 2006

   Amusing to see you have a new pet project. As it happens, the subject at hand (namely G/G WZW models) was explored and left by string theorists more than 10 years ago. The main statements mathematicians are making have long been known. See old papers by string theorists for the spectrum of the Dirac/BRST operator, the relation with the fusion rules, and the relation to Chern-Simons on S^1, amongst many other things.

4. **woit**
   April 27, 2006

   M.,

   It's not a new pet project, it's an old pet project, see http://www.arxiv.org/abs/hep-th/0206135

   Besides my own speculations about the significance of Freed-Hopkins-Teleman for physics (which have nothing to do with string theory), there's a huge amount of interesting new ideas in their papers that are not in the string theory literature.

5. **woit**
   May 9, 2006

   Some comments here may have been accidentally deleted. If so, my apologies and please repost them if you can.

6. **Bert Schroer**
   May 9, 2006

   As much as I admire mathematical work of this high quality, as a physicist I have a different completely autonomous access to the problems connected with the Verlinde algebra. For me the issue is related to an extension of the Nelson-Symanzik duality in the new context of chiral QFT (i.e. in the context of a more involved spin-statistics connection due to the appearance of braid group statistics). This is a kind of symmetry which is particularily interesting for 2-dim. QFT at a finite temperature. It is almost trivial if you are allowed to represent your QFT in terms of Feynman-Kac representation, it is just the symmetry between space and Euclidean time in a temperature state. A more general and rigorous setting of N-S duality can be found in a recent paper (C. Gerard and C. Jaekel, mat-ph/0403048). This is of course related to the issue of Euclideanization (B.S. hep-th/0603118), one of the most subtle issues in QFT (unfortunately it has been banalized beyond recognition, for critical remarks in this direction see K-H. Rehren, hep-th/0411086) which in the standard context is inexorably linked with the names Osterwalder Schrader (and also Symanzik, Nelson and Guerra).
   But in the chiral context with braid group statistics it takes on a completely new form and the relevant Euclideanization is that of the modular localization approach (B.S. hep-th/0504206 (published in AOP) and hep-th/0603118). It uses the amazing power of the modular theory of operator algebras (adjusted to the localization concepts of AQFT) and reproduces among other things the Verlinde relation as a special case of an extended concept of temperature duality (a kind of self-duality in which the Euclideanization leads to a QFT which is of the same noncommutative kind as the original one, which the old Nelson-Symanzik duality within the O-S Euclideanization setting cannot provide) of a full thermal theory (and not just of the zero-point (partition) function). The conformal symmetry of the underlying QFT permits to make a complex extension of temperature to the tau parameter. The two field theories live (in the sense of physical localization) on circles, the connecting torus is nothing else than the old connecting Bargman-Hall-Wightman analyticity region (albeit in a more sophisticated veil) which has no direct operator interpretation. Since the charge transportation around the circle mixes the charge superselection sectors (F-R-S, Rev. Math. Phys., Special Issue, (1992) 113), the Euclideanization involves the statistics character matrix S defined by Rehren. Although in all concrete model calculations Rehren's S turned out to be the same as Verlinde's S, the derivation by modular Euclideanization provides the missing structural argument why they are always the same.
   My remarks could be misinterpreted as lamenting about recognition of work I have been involved with, but this would be much too superficial; what makes me really sad is that tremendous schism which caused young particle physicists to have lost their own rich conceptual past and which presently could be of help to do the fruitfull kind of mathematical physics with physics at the helm of moderation; but what I see is that physicists take their ideas and orders from mathematicians, the less agreeable aspect of the otherwise positive legacy of the Atiyah-Witten era.
   I could continue this list. Take the categorical aspect of the old Moore-Seiberg work and all the subsequent papers in that frame of mind (most recently the Fuchs-Runkel-Schweigert etc. work). It is a good illustration of the present Zeitgeist that papers which develop these issues from an entirely physical spacetime perspective (as e.g. the paper of Rehren and myself on Einstein Causality and Artin Braids which appeared at the same time and several subsequent papers in this conceptual line) are hardly noticed outside a small community which has not suffered from that grand rupture in particle physics. Or take the recent works on n-categories (see the blogs in the string coffee table). The oldest and physically best motivated work on such issue (it shows you how hard the AQFTists worked in order to find a structural entrance to gauge theories) by John Roberts is not even mentioned!
   The situation reminds me of a newspaper report I red a long time ago in the US. It said that a catchup factory had to close down because the taste of their product was too

natural i.e. too close to real tomatos (not the plastic stuff which you find often in supermarkets in Europe and the US); most of the costumers already got to like the type which is mixed with pineapple and a lot of other less healthy things. Physicists only accept mathematical products if the underlying concepts have a certain distance from physics!

Reading the recent blogs on Dan Freed's work, I also learned to appreciate the subtle (but I still think unintended) humor of Lubos Motl. He is of course right in pointing out the metaporical link between those combining and splitting Euclidean waterhoses which Dan Freed and string theory share (I seriously hope that Freed does not require do interpret them the same way). Lubos Motl plays the (in modern times often missing) role a jester of the string community, a kind of supreme Lord of Misrule. If they have any sense of humor they would support his tenure at Harvard. His role of a jester also works perfectly with repect to the climate change issue. By carrying rightwing propaganda to its extremes, he creates a relaxed cabaret atmosphere which has the opposite effect from what it purpots to do, a perfect Lord of Misrule!

7. **Bert Schroer**
   May 9, 2006

   Peter
   This morning there was a post of Motl in this series of posts. My last remaks refer to this post and without is some part of my comments on string theory& Dan Freeds mathematical work runs into the void. Peter, did you loose your humor? There was no peronal attack in that one which could have annoyed you. Maybe we can ask him to re-import it.

8. **woit**
   May 9, 2006

   Bert,

   I certainly did not delete any comment of Lubos's here intentionally. If any recent comments in this thread disappeared, I don't know how it happened.

Revealing the Hidden Nature of Space and Time

April 26, 2006
Categories: Uncategorized

The EPP2010 report by the Committee on Elementary Particle Physics in the 21st Century is out today, and it is entitled Revealing the Hidden Nature of Space and Time. This committee was convened to recommend priorities for high energy physics in the U.S. over the next 15 years. Its membership included non-physicists and it was chaired by economist and ex-president of Princeton Harold Shapiro. The inclusion of people from outside the field emphasized the need for wide support for funding of U.S. particle physics if it is to remain healthy. The latest issue of Nature contains an article about the report entitled US particle physics fights for survival, and an editorial Making collider endorsement count. A press release about the report is here.

At the press conference announcing the report (which was webcast), Shapiro emphasized that the non-physicists on the committee had not been fully aware of the difficult situation US particle physics was in. They were very sobered by the state of US HEP, which he described as facing a serious danger that it would be half its size in 4-5 years, as current programs ended without a compelling follow-on program. The most important recommendation of the committee was that construction of the ILC in the US, probably at Fermilab, be vigorously pursued and that:

The United States should announce its strong intent to become the host country for the ILC and should undertake the necessary work to provide a viable site and mount a compelling bid.

Constructing the ILC in the US would require an increase beyond the rate of inflation in HEP funding, and the committee considered a scenario of budget increases of 2-3%per year that would probably be required, although solid numbers for what the cost of the ILC would be are still not yet available. Emphasizing the ILC in this way was described as a "high-risk, high-reward" strategy, and that taking these risks was necessary for US HEP to retain any leadership role in the field.

More specifically, the committee recommended six action items, ranked by priority:

1. Realize the physics potential of the LHC experimental program.

2. Launch a major program of R and D for the ILC, significantly expanding current expenditures on this.

3. Announce US intent to become the host country for the ILC.

4. Increase the current share of the HEP budget devoted to studying dark matter, the CMB and dark energy.

5. Develop a staged program, with international cooperation, of neutrino experiments, with emphasis on neutrinoless double-beta decay, accelerator based experiments, and search for possible charge-parity violation. This last might involve large detectors that could also be used to search for proton decay.

6. Support (especially if they're not very expensive) high-precision experiments that probe beyond the Standard Model physics, such as a future B factory, lepton-flavor violation and rare-decay studies, searches for electric dipole moments, and precision measurements of muon g-2.

The committee did a very good job of recognizing the difficult situation of US HEP, and coming up with a plausible strategy for how to make the best of it. I have my doubts about whether it's really a good idea to sell this as "Revealing the Hidden Nature of Space and Time", since it's not especially likely that that is what is going to happen. There's no particularly good reason to believe that extra dimensions will show up at the LHC or ILC energy scales, so over-selling this is dangerous. I do understand that it's a lot harder to get people excited about the new physics that this is likely to really all be about: understanding the nature of electro-weak symmetry breaking.

This was a study of what to do about experimental HEP, so the problems of theoretical HEP were not addressed. Unfortunately, besides the usual arguments for supersymmetry, over-hyped ideas about string theory make an appearance as the committee calls for "Improved tests of general relativity to search for effects of extra dimensions or string theory" and "Measuring time variation of physical constants with spectroscopy of distant objects to search for effects of extra dimensions and string theory", without noting that string theory makes no predictions about either of these. One other thing included in the report is new, improved verbiage about the status of string theory. In Witten's biographical sketch, it is described as "one of the leading candidates for the grand unified theory of elementary particle physics", which seems to me to be a downgrade from the phrase "the leading candidate" which until recently was often used to describe the status of the theory.

Update: More about this from Chad Orzel, Lubos, Clifford Johnson at Cosmic Variance, Tommaso Dorigo and The New York Times.

Update: Also from Alexey Petrov, who in a comment at Cosmic Variance links to a very different point of view about prospects for constructing the ILC in the near term: a recent resignation letter from Bill Foster, who was the Proton Driver project leader at Fermilab (the Proton Driver would be a high-luminosity, lower energy accelerator, useful for, among other things, producing a more intense neutrino beam).

Update: More from JoAnne Hewett at Cosmic Variance.

Comments

1. **michael**
   April 26, 2006

   It's difficult to comprehend extending our pocketbooks in favor of such laboratories while the NCI budget (i.e. applicable, translational research) is drying up. What is so pressing about understanding the fundamentals of symmetry breaking when troves of Americans are dying daily from the toxicity associated with underdeveloped cancer treatments?

2. **Who**
   April 26, 2006

   it is possible that you mean "droves" of Americans and not "troves".

   My dictionary says trove (as in "treasure-trove") is a thing found, from the old French word *to find*.

3. **michael**
   April 26, 2006

   Yes…even probable. Perhaps you could just talk Dr. Woit into building a grammar check into the 'submit comment' section?

4. **Peter Woit**
April 26, 2006

Michael,

There were long discussions of this over at Cosmic Variance. I think the relevant point is that each year the federal government is spending about 2500 billion, of which .7 billion is going to particle physics. This .03 percent of the budget isn't what is crowding out money for the purpose you mention (or any other purpose), much less the incremental amount over this that the ILC would cost.

5. **anon**
April 27, 2006

Peter, could you clarify for us what your problems with electroweak symmetry breaking are. The jargon probably puts laymen like me off. Perhaps I can explain what I've picked up on the subject.

My understanding is there are four electroweak gauge bosons, the photon and the massive but uncharged Z, plus two massive charged (W+ and W-).

All four have been detected at CERN in 1983, which validated electroweak theory.

The problem I think is why of the four only the photon has no rest mass and infinite range at low energy, while the Z, W+ and W- all have very short ranges (giving the weak force which controls the decay of neutrons into protons, etc.).

The mainstream mechanism is that a Higgs vacuum field gives mass to the Z, W+ and W- bosons at low energies, in some way. At higher energy, ie at the energy of electroweak unification, these bosons are no longer attenuated (or whatever) by the Higgs field, and their range becomes infinite.

The photon is able to go an infinite distance because it has no rest mass. The Higgs field gives rise to all inertial masses. It hasn't been detected directly. Is this a reasonable summary?

6. **sunderpeeche**
April 27, 2006

The EPP report was dedicated to experimental HEP, and it makes no difference if there are a few statements about string theory (or any other theory). The decision to fund the ILC etc will be based on criteria far removed from "will this validate string theory or not".

7. **Tony Smith**
April 27, 2006

Peter said "… The … report by the Committee on Elementary Particle Physics in the 21st Century … was a study of what to do about experimental HEP … the problems of theoretical HEP were not addressed …".
sunderpeeche said "… The EPP report was dedicated to experimental HEP, and it makes no difference if there are a few statements about string theory (or any other theory). …".

anon said "… CERN in 1983 … validated electroweak theory …".

A generation ago, high energy physics experiment and theory were so closely linked that you could not really discuss one without the other, leading to results such as validation of electroweak theory.

The fact that the EPP report is about experiment and "… it makes no difference if there are a few statements about … theory …",
is a clear condemnation of the present-day state of conventional theoretical high energy physics = superstring theory, and of this generation of conventional high energy physics theorists.

That condemnation is emphasized by the fact that "… The principal charge to the Committee on Elementary Particle Physics in the 21st Century was to recommend priorities for the U.S. particle physics program for the next 15 years …", which charge was to study ALL of Elementary Particle Physics.
That the EPP report was "dedicated to experimental HEP" is a de facto statement by the committee that present-day conventional theory = superstring theory is irrelevant. (It would be nice to be able to say that irrelevance = mostly harmless, but unfortunately that is not the case.)

Tony Smith
http://www.valdostamuseum.org/hamsmith/

8. **sunderpeeche**
April 27, 2006

Bravo Tony Smith! A condemnation of current theory … I hadn't really thought of it that way … but still.

However, the fact is also that (as has been noted in this blog before), expt HEP simply hasn't (definitively) found anything beyond SM since the mid-1970's. The new machines simply *have* to be search-and-discovery machines, not linked to tests/validations of any theory
— and this is *not* the fault of string theory. (FWIW it's not the fault of any theory or expt. Nobody knows the mass scale of the next discovery.) Note that in 1950-60's there were competing theories, and many expt puzzles. Machines were built to find new things, not always guided by any theory — but there were plenty of questions to ponder, no need to "set priorities".

The committee might officially be for all HEP, but theory (apart from supercompters?) doesn't need (much?) capital infrastructure. No need for a committee.

9. **SomeBody**
April 27, 2006

A billion here, a billion there, soon you're starting to talk real money:

The Gross National Debt

10. **JoAnne**
April 27, 2006

Peter, thanks for providing a good summary of the contents and the purpose of this report. Alot of people worked very hard to get the non-HEP scientists and non-scientists on this panel excited about our science. It seemed to have worked and the positive statements in this report have the potential to do HEP a tremendous amount of good.

11. **Peter Woit**
April 27, 2006

Thanks Joanne,

Good luck with the hard work over the next few years that will be needed to convince people to support US HEP. There are very good reasons for people to be interested in this kind of science and want to see it supported, the report does a good job of explaining this.

12. **steve**
April 27, 2006

From a political point of view, I question putting all the eggs in the international basket. Americans tend not to support large-scale collaborations of this type (with the unfortunate exception of the space station), and given current international opinion I think it could be hard to get foreign countries to agree to a US site.

It might make more sense to stoke insecurities about falling behind foreign countries in HEP. Americans respond better to a chance to compete and win. There's a big psychological difference between a) inviting collaboration from all over the world on a mostly-US project and b) fighting to get permission from foreign countries to host an international project. The latter smacks a bit too much of cities bidding for the Olympics, which is also often unpopular. It may seem stupid, but I think wanting to stay ahead of other countries is a pretty strong motivator for funding of basic research. Even when we do it by attracting people from other countries.

13. **sunderpeeche**
April 27, 2006

The opening sentence of the EPP Executive Summary talks of "US global leadership in science, technology…" Next para "By recognizing the need for US leadership in particle physics…" Third para "… US tradition of leadership in the field is not secure" (tradition? post-WW2 maybe)

Language like this is not going to impress foreign collaborators (for ILC). Why would Japan/China/Europe/… collaborate to establish (or perpetuate) "US leadership"? On the other hand, the US Congress is unlikely to fund a project not sited in the USA. Not so simple. US may have to be prepared to fund entire "I"LC.

14. **Tommaso Dorigo**
April 28, 2006

My two pence on the "US leadership" issue:

I think the attitude of us non-americans working in HEP would improve a lot if the US stopped talking about themselves as the owners of the field.

A committee of americans talking about how to steer US HEP into a leadership role is fine – nothing to object. But it should not be a public document, as if seeking agreement from international collaborators. It is slightly annoying since it gives the -IMO wrong- idea that the US has entered the LHC experiments in forces to do science there for a long time to come, bringing experience and money, but with the secret aim of finding the right moment to stab their collaborators on the back, win support for an american ILC, and march back home triumphantly.

Why can't we form a committee who deals with a more important issue: how to dodge the need to tickle the ego of a few dumb congressmen in order to survive ?
I am sure 90% of US scientists would be happy to live without having to be the kings of the hill in HEP. Let me rephrase it: without the need to put "leadership" as a deliverable in all their requests for funding…

15. **Tony Smith**
April 28, 2006

Tommaso Dorigo, about EPP2010 and ILC, said "… I think the attitude of us non-americans working in HEP would improve a lot if the US stopped talking about themselves as the owners of the field. …".

I completely agree. That was one of the two major factors that killed the SSC (the other being failure to honestly state and control costs). Back in the SSC days a German friend of mine said something like:
"… The Americans expect us to put a lot of money into the SSC, but they want every proton in the SSC to carry a little American flag. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

16. **sunderpeeche**
April 28, 2006

About the SSC, absolutely yes. (My friend wasn't German, but no matter.) The machine was conceived (after the cancellation of ISABELLE) to "bring back the lead the US", "highest energy particle accelerator back in the US". Why would foreigners support such a project (as they were later asked to do)? If the ILC is also to "maintain/restore US leadership in HEP" why will others support it?

17. **SomeBody**
April 28, 2006

Isn't the best location for the ILC, should it really be built, the most successful, most international particle physics lab on the planet ever? I.e. CERN? There's much to be said for the Swiss hosts, not least their lack of interest in claiming world leadership based on physical location of this or that machine or building. And there are some pretty good restaurants in Geneva, too.

18. **from EU**
April 29, 2006

The ILC collider employs the standard accelerator technique and aims at reaching an energy of 0.5 TeV around 2020. This would be a poor improvement over the LEP collider, that reached 0.2 TeV in 2000.

Even if LHC will indicate that 0.5 TeV is enough to access new physics, and even if US particle physics needs ILC to survive, it would be better to wait until a linear collider can be built starting a new, more promising, technique (e.g. CERN is trying to develope CLIC).

19. **sunderpeeche**
May 1, 2006

The ILC does not employ “standard” accelerator technology, as if were off-the-shelf stuff. It is very demanding technically. Still a challenge to figure out how to deal with the heat load, radiation hazards etc. A brief history (which anyone is free to dispute): after the SLC was the idea for a 1 TeV x 1 TeV linear collider (TLC = TeV Linear Collider). This proved too ambitious, scaled to 0.5 x 0.5 TeV (TLC = TeV c-o-m linear collider). This was mooted for a while, evolved to NLC (Next Linear Collider), whatever might be achievable/fundable ~ 250 x 250 GeV (these were all pure US projects). Japan developed idea for JLC (you figure it out). CERN worked on CLIC, others on TESLA (~TeV Superconducting Linear Acc). The various ideas (maybe not CLIC) merged into ILC, which is 0.5 TeV c-o-m ~ NLC. But still many unsolved technical challenges.

20. **from EU**
May 1, 2006

dear sunderpeeche,

the German, US and Japanese projects are “standard” in the sense that they all aim at an acceleration gradient of “only” about 20 MeV/meter, such that an “ultimate” linear accelerator (about 2 x 30 km long) is needed to reach 500 GeV.

If we hope to someday reach larger energies, we should start trying a shorter accelerator with a larger gradient. CLIC aims at about 150 MeV/meter: even if CLIC will not seem faster nor cheaper than ILC, I would consider CLIC as the most promising option.

21. **sunderpeeche**
May 1, 2006

Tell that to the designers of the ILC. People have been trying to design a next-generation linear collider for ~ 20 years now. I expect that if they could do better than 20 MeV/m they would.

22. **sunderpeeche**
May 1, 2006

It’s not exclusively a matter of the accelerating gradient. The positron source is another problem. Typically one smashes an electron beam onto a target, and positrons come out (along with junk). Usual target choice is copper (dissipates heat well) or tungsten (v high melting point), but for ILC requirements all known solutions will melt. Current thinking (as far as I know) is to spray a mercury jet across the e- beam, collect the spent mercury — it is a liquid — and recycle it (via heat exchanger or something). Raises safety/health problems with mercury vapor leakage etc. Not clear the idea will work. Never been done. Don’t know if prototype exists.

Yuval Ne'eman 1925-2006

April 27, 2006
Categories: Obituaries

Yuval Ne'eman died yesterday, from a brain hemorrhage caused by a recent fall. Science magazine has a story about this.

Together with Murray Gell-Mann, in 1961 Ne'eman co-discovered the SU(3) classification of strongly interacting particles. At the time he was both an Israeli military attache in London, as well as a graduate student of Abdus Salam (who was a devout Muslim). For some amusing stories of that period, see this web-page of fellow student Ray Streater.

In later years Ne'eman continued his research in theoretical physics, was president of Tel Aviv University, played an active role in the Israeli nuclear weapons program, and was the head of a far-right political party. He was definitely one of the most colorful characters in particle theory during the second half of the last century.

Update: There's an obituary from the AP in the New York Times. It's only comment about Ne'eman's work in particle theory is that:

The Technion credited Dr. Ne'eman with discovering the principles of tiny subatomic particles, called quarks, although another scientist received the Nobel Prize for that discovery.

Ne'eman and Gell-Mann both realized that mesons and baryons could be classified as representations of SU(3), and some of the physics of the strong interactions could be understood this way. Gell-Mann won the Nobel because he later identified the fundamental representation of SU(3) with new particles, quarks.

Comments

1. **Arun**
   April 27, 2006

   "Abdus Salam (who was a devout Muslim)" – since you brought up religion, I think the following is pertinent:

   Abdus Salam belonged to a sect called "Ahmaddiya" which has been deemed non-Muslim by Abdus Salam's native country, Pakistan. The following is an excerpt of a law that went into effect in 1984:

   > Any person of the Quadiani group or the Lahori group (who call themselves 'Ahmadis' or by any other name), who, directly or indirectly, poses himself as Muslim, or calls, or refers to, his faith as Islam, or preaches or propagates his faith, or invites others to accept his faith, by words, either spoken or written, or by visible representations, or in any manner whatsoever outrages the religious feelings of Muslims, shall be punished with imprisonment of either description for a term which may extend to three years and shall also be liable to fine."

2. **knotted string**
   April 27, 2006

   Yuval Ne'eman certainly stands out as a an interesting character, because he was prepared to do what he felt was for the best. I'm glad that the assassination attempts failed.

   It's fascinating to read in the Science story that he inspired the spy Ne'eman in Frederick Forsythe's novel The Odessa File. I know Einstein was once offered the Presidency of Israel, but he declined it.

   The page Science links to, explaining the eightfold way, is nicely illustrated: http://fafnir.phyast.pitt.edu/particles/conuni6.html

   A link from there has a nice history of the Standard Model http://fafnir.phyast.pitt.edu/particles/smt.html which ends in 1995:

   'After eighteen years of searching at many accelerators, the CDF and D0 experiments at Fermilab discover the top quark at the unexpected mass of 175 GeV. No one understands why the mass is so different from the other five quarks.'

   It's sad that the subject has become so stagnant since 1995.

3. **MathPhys**
   April 27, 2006

   Abdus Salam was not a devout Muslim. In his later years, he put on the devout Muslim thing, but surely not when he was a young professor at Imperial College, which is the time when Ne'eman became his student.

4. **Subhash**
   April 27, 2006

   I apologize for not sticking to the main subject of this post, but since Abdus Salam, Ne'eman's supervisor, has been mentioned in two comments with different assessments, perhaps this column on him would be of interest:

   http://www.sulekha.com/blogs/blogdisplay.aspx?cid=4325

5. **D R Lunsford**
   April 27, 2006

   A great physicist. And don't forget George Zweig.

   -drl

6. **Tony Smith**
   April 27, 2006

   Yuval Ne'eman (with Yoram Kirsch) wrote a book, The Particle Hunters (Massada 1983, Cambridge 1986 and second Cambridge edition 1996), intended to provide a text on particle research for non-professionals.
   The preface to the second edition says "… we were encouraged by warm responses of readers from all over the world, who have read the book either in English or …

Spanish, Italian, Japanese, or Hebrew … A poet sent us a poem in which she recounted dreams she had after finding out what the world is made of … Their … comments made them partners in creating this revised edition. …".
Having had the pleasure of meeting with Yuval Ne'eman some years ago, that passage reinforces my feeling that he was a genuine humanitarian in the deepest sense.

As to the subject matter of this blog, the book is not silent, saying:
"… The enthusiasm …[for]… superstring theories … cooled off … when the theory was taken from ten dimensions down to our prosaic four-dimensional world. It turns out that this can be done in myriads of ways, each yielding a different GUT, in contrast to the uniqueness of the E(8)xE(8) which had impressed physicists so much. It means that one can put almost anything into the six dimensions (out of ten) which have to fold up. Thus, the theory seems to contain a correct formulation of quantum gravity and to fit the requirements of a TOE, but is much too flexible and therefore has very little predictive power.
The hope is that an equation for all possible superstring theories can be formulated, and that it will single out one specific theory, as the TOE selected by nature to describe our physical world. Several theorists have expressed the hope that this theory, if found, would be the ultimate and final theory of physics. The authors of this book hold a different view. We think that Hamlet's words 'There are more things in Heaven and Earth, Horatio, than are dream't of in our Philosophy' (I, v. 167) are closer to the lessons of the past. …".

As to policies for the future of particle physics, the book concludes by saying:
"… In the beginning of this book we mentioned the atomic theory of Democritus, who lived in the Greek city of Abdera in the fifth century BC. The citizens of Abdera awarded Democritus with five hundred talents of gold for his idea, as if their eyesight could sense the view at a distance of twenty five centuries and see the atomic theory in its glory. Can we afford to be less far-seeing? …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **sunderpeeche**
April 28, 2006

Democritus expounded the atomic hypothesis (5th century BC?), but he had no testable predictions to back it up. Even in the 19th century people like Ernst Mach disputed the atomic hypothesis. (Ludwig Boltzmann had trouble his whole life convincing people about the existence of atoms.) Surely, then, Democritus' idea of atoms was not even wrong?

8. **knotted string**
April 28, 2006

Democritus' atom was predicted to be impenetrable and unsplittable. Hence it was wrong in detail. (But so were later efforts, eg Dalton's atomic explanation of chemistry was ridiculed for the non-integer mass numbers of elements, because he didn't know about isotopes or binding mass-energy effects.) The Greek atomic concept was grossly oversimplified, which is the exact opposite of the problem with string theory today…

9. **Jack Sarfatti**
April 30, 2006

I met Yuval Ne-eman with Max Jammer in 1980 at the Fairmont Hotel at a Reverend Moon Unity in Science Conference. Henry Stapp was at this conference. Ne-eman spent at least 10 minutes lecturing me on the importance of my name "Sarfatti" to Jewish History via Rabbi Rashi de Troyes 1040-1105 AKA "Solomon ha-Sarfatti." Murray Gell-Mann also had some obscure things to say about my name in Jewish thought at a Santa Fe Complexity fund-raiser at the San Francisco Ferry Building. Murray was attracted to the woman I was with who had no idea who he was and this got Murray even more interested.

10. **MathPhys**
May 1, 2006

Jack,

When was that encounter with Gell-Mann that you mentioned? What year?

11. **Eluard**
May 1, 2006

"Democritus' atom was predicted to be impenetrable and unsplittable. Hence it was wrong in detail."

This should be called the Spinal Tap the-amp-goes-to-11 fallacy. Why not just think that the real atoms are the fundamental particles? What does it matter that someone jumped the gun and called the wrong things 'atoms'?

12. **Anonymoose**
May 1, 2006

I think I've heard of Abdus Salam before. It was column about how many scientists manage to remain strict followers of a particular religion, yet peacefully reconcile their faith with their work in science. Salam mentioned how there was no necessary conflict with his Islamic faith and scientific exploration, but I wonder what it was like for Yuval Ne'eman, given his Zionist background. This could provide interesting feedback on Muslim-Israeli relations.

13. **Jose N. Pecina-Cruz**
May 7, 2006

Today, May 8, 2006 I read that Yuval Ne'eman passed away. Last time I contacted him, we agreed on writing a book on General Lie Invariants (beyond of those called Casimirs). I had the pleasure of being one of his students. In an attempt to quantize gravity (Gauge Affine Theory) Ne'eman and I came to the discovery of a general method to calculate Lie invariants. Because his oustanding work in other areas this work is barely known. Yuval and I had nice expectations on writing the best book on Lie Invariants. Rutwig Campoamor from The Universidad Complutense of Madrid planned to join us on this task. Rutwig and I will pursue this titanic task to honor the memory of this great physicist and dear friend.

Scientists Speak Out About Guantánamo

May 1, 2006
Categories: Uncategorized

I've been thinking there's too much politics on this blog recently, and yet I still think the political activities of well-known theorists are worth noting here. As commenter Arun pointed out, this Sunday's New York Times has a letter to the editor criticizing the human rights violations at Guantanamo, signed by many prominent particle theorists, including Susskind (whose name comes first, perhaps he's the organizer), Bjorken, Deser, Dyson, Gaillard, Gross, Polchinski, Schwarz, Wilczek, Witten and Zumino.

Over at the science policy blog Prometheus, Roger Pielke has a posting called A Very Bad Dream Indeed, in which he strongly criticizes the authors of this letter for writing such a letter that has nothing to do with science policy. He seems to be worried about these scientists "transforming their privilege in the scientific domains into authority in non-scientific domains." Somehow, I just don't see the possibility of theoretical physicists grabbing political power and ramrodding through policies they support as being something much worth worrying about. My reaction to this particular letter was not "who do these people think they are to do this?", but rather "how come everyone else isn't doing this?" The situation at Guantanamo is a disgrace to this country and that the courts allow it to continue is shocking.

More about this at No Se Nada, the blog of Kevin Vranes.

OK, now, no more about politics for a while. I promise….

Comments

1. **MathPhys**
   May 1, 2006

   Finally.

2. **Dumb Biologist**
   May 1, 2006

   Book! Book!

3. **Yuri**
   May 1, 2006

   You right 1000 times!
   No needs use this room for politics.
   There are many other places for it.

4. **woit**
   May 1, 2006

   DB,
   My book? Last Thursday I got one of the first copies off the press of the British edition in the mail. Quite something to finally see the thing in print as a real book. Publication date is still one month from now, June 1 in Britain. Working this week on going through the newly copy-edited American version, that's still set for September sometime.

5. **absolutely**
   May 1, 2006

   Guantánamo sours my otherwise sweet memory of the few years I spent in the US and the warm and enlightened folk I met there. That my government (Australian) should be so complicit and gutless in condoning it really heavies my heart.

6. **Chris Oakley**
   May 1, 2006

   I just don't get it … sunny climate, high-fashion orange suits, being fed and watered at the US taxpayers' expense … no doubt Cuban cigars and rum freely available as well, especially to Muslims. And of course the most meticulous trial procedure ever, i.e.

   George W Bush: "These are bad people".

   What's all the fuss about?

7. **Dumb Biologist**
   May 1, 2006

   OK, sorry to get so egregiously off topic. Thanks for the update!

8. **Lubos Motl**
   May 1, 2006

   Roger Zielke's name is Roger Pielke, and he definitely knows what he talks about.

9. **woit**
   May 1, 2006

   Thanks Lubos, I fixed his name. He does seem to be your kind of guy.

10. **Tommaso Dorigo**
    May 2, 2006

    Hi Peter,

I totally agree with what you said.

I see nothing wrong if anybody who sees something wrong happening, and who enjoys attention of the media, uses the media to denounce it. It is part of the game. I don't particularly like the game, sure. But anybody who cares for civil rights should think about the horror of american prisons in Guantanamo and Iraq, and the indecence of Defence Ministers covering it instead than putting an end to it.

Let's stop this horror, and let's not be afraid to use any possible venue and means to denounce it. The US have been entangled with regimes that tortured and killed thousands of innocents in the past. Sure, other countries have too. But if the US wants people in the world to look at it as an example of a free, civil country, they need to change the way they act abroad.

I love the US, as I love my country, but both have lots to change for the better.

T.

11. **ObsessiveMathsFreak**
May 2, 2006

Einstein's letter to Roosevelt comes to mind as an example of the weight scientists hold with politicians when they stay within their field of competance. His lobbying for a world government after the second world war, comes to mind as an example of the lack of weight scientist's views carry once they stray outside their field of competance.

12. **SomeBody**
May 2, 2006

I note that pointing out the hypocritical behaviour and dishonorable political affiliation of a certain serial US basher posting here is frowned upon by the management. May I ask why?

13. **woit**
May 2, 2006

Ad hominem, personal attacks are definitely frowned upon by the management and will virtually always be deleted (although I often leave up the ones by string theorists attacking me). Also strongly discouraged are attempts to start up off-topic political battles here. Your comment qualified for deletion on both counts, so I deleted it and the responses it immediately started generating.

14. **SomeBody**
May 2, 2006

I see the delete button is working overtime today. 😉

I disagree with your characterization of my post, but it's your blog. Peace.

15. **Benjamin**
May 2, 2006

I don't want to promote the politics that Peter Woit doesn't want here, but I do want to make a comment that I think would be beneficial to those on the 'left'. I am in the center, not in favor of the Iraq war, but not willing to call Bush a Nazi or anything like that. I agree that Abu Ghraib and Guantanamo were wrong and hurt America's image badly. What I can't understand is how the 'left' gets obsessed with this and ignores all the terrorism, often in the name of Islam, the bombings, decapitations, human rights abuses, etc. It's not that America should never be criticized, but it seems that there is a lack of proportion. Abu Ghraib and Guantanamo, for all their horror and humiliation, are still much less 'evil' than pedestrians being blown to pieces in Israeli pizza parlors. If the left seemed more balanced, it would have much more credibility, but the same can be said of the right of course. You really need to think about how you sound to the mainstream, or you are self-defeating. All this partisan hypocrisy and stupidity from both sides needs to stop in favor of a fair and balanced view. I'm surprised that even smart physicists fall for far-left rhetoric. Don't erase this; it's really worth thinking about. No need to discuss further.

16. **woit**
May 2, 2006

OK Benjamin, I'll leave your comment as having some relevance to the topic. However, please, everyone, take any arguments about who is more or less evil than whom elsewhere.

17. **knotted string**
May 2, 2006

It's a shame they do the torture behind closed doors, in a democracy it should be done openly. I humbly offer the suggestion that torture is done democratically:

The person to be tortured should be web-cast live, and U.S. viewers should be in control. Electric shocks can be administered automatically after every fixed number of internet votes. (1) This will really deter terrorists. (2) This will save a small number of professional torturer's feeling bad about their work. (3) It will give everyone an elated sense of justice and moral ethics.

18. **Chris Oakley**
May 2, 2006

Knotted String,

I think that you have a great idea there, and I have to say, I prefer it to your ideas about physics.

What I like best is that because we live in a free society, it will be all the sadistic muthas who will choose to log on to this web site to administer tortures to terrorists. I am no expert on psychology, but I am sure that people will be much more sincerely nasty if they are doing it as a recreation rather than a boring old nine to five job.

19. **steve**
May 2, 2006

It is disappointing that scientists who would insist on precision in distinguishing one phenomenon from another in their own fields have no compunctions about tossing out a word like "Guantanamo" without crucial distinctions being made. There are big differences in the morality and prudence and remedies for a) the detention of illegal combatants for the duration of hostilitles, b) the detention of people incorrectly believed to be illegal combatants, c) the authorized use of aggressive interrogation tactics against presumed illegal combatants, d) the unauthorized use of aggressive tactics to gratify individual power-lust, e) the authorized use of torture for interrogation, f) the unauthorized use of torture for interrogation, and g) the unauthorized use of torture for personal gratification.

Some of these may be acceptable, some of them call for systemic reform, some of them call for allowing the existing disciplinary structures to function (e.g. the Abu Ghraib abuses were uncovered by the military itself; the media found out months later). The kind of lazy thinking displayed in using “Guantanamo” as some sort of shorthand for evil is discouraging in a group that prides itself on its analytical acumen and clarity of thought.

20. **Eli Rabett**
May 2, 2006

Ben and Lubos only believe in or not and.

One can be against all groups who practice deceit, kill and torture. It is ethically more important to oppose those closest to you who do this. Even as a practical matter this is vital so that you may have allies in your struggle.

21. **Michael Kellman**
May 6, 2006

Like it or not, this is the policy of the elected government of the United States. Congress has the power to stop it if it judges such to be warranted. It apparently doesn’t.

Is it really such a good idea for scientists — physicists — qua scientists and physicists — to set themselves up in opposition to American foreign policy?

Perhaps the message received will be that science does not support America as represented by its elected government.

And perhaps that a reconsideration is in order as to whether it is such a good idea for America to support science?

22. **Santo D'Agostino**
May 7, 2006

To Michael Kellman:

Of course it is a good idea for citizens of a purportedly free democracy to speak freely, whether or not they support the current government. Numerous American leaders over the years (the current president, with his “You’re either with us or against us,” being a prime example) have very effectively confused people into thinking that criticizing the U.S. government is being anti-American. On the contrary, it is a DUTY of citizenship to speak out against immoral and criminal behavior, especially when it is committed by a country’s leaders.

The ideas behind your last sentence are repellent, both for its implication that scientists should experience repercussions (What did you have in mind? That they lose their jobs? That they lose their research funding?) for exercising their rights as American citizens, and for its counter-productive vindictiveness. A decrease in support for American science harms all Americans, not just the scientists directly involved.

Santo D’Agostino

23. **Bert Schroer**
May 7, 2006

I suppot D’Agostino’s statement. As a non US citizen I have no influence on the US policies through a vote, but as a member of the western civilization I suffer all the consequences of the tension and hightened danger created by such an uncivilized action; so my democratic rights have been considerably reduced (despite all the propaganda about strengthening democracy in this world) by Guantanamo, Abu Graib and the CIA torture flights using European airspace. There are numerous past precedents of such actions by the scientific elite of a country, some of them even successful as the anti-nuclear campaign of the Goettingen 18 (Born, Heisenberg,….)

24. **Michael Kellman**
May 7, 2006

D’Agostino and Schroer have every right to call our elected governors “immoral”, “criminal”, “uncivilized”. Witten et al. have a right to criticize the government as a group of scientists. (Why anyone should especially care what a group of scientists thinks about foreign policy is beyond me.)

But the public who elected the government has a right to draw its own conclusions. I’m not suggesting that anyone should lose their job (as a possibility the notion that IAS is going to fire Witten for this is pretty preposterous). I’m not suggesting that anyone should lose a grant.

But the public has a right to draw its own conclusions. Perhaps people will think that the scientists’ judgement about science is as unproductive as their judgement about foreign policy.

There is no law of nature that “a decrease in support for American science harms all Americans”.

Science also no natural right or entitlement to support.

I notice that U.S. high energy physics is looking for support for new experiments. Many people may conclude that if it’s a choice between backing what the country is doing in war and backing high energy physics, that they prefer the latter.

The scientists have every right to set themselves up as sceintists in opposition to our elected government.

But personally, I don’t think it’s too smart.

25. **SomeBody**
May 8, 2006

Now I’m really puzzled. Why was my latest post in this thread deleted? Surely it contained nothing which could be labeled as either ad hominem or off topic (if anything, it pointed out the topic of the thread). More and more peculiar…

26. **Bert Schroer**
May 8, 2006

The answer is very simple: it is because my post was deleted. Its democracy in practice and this time I even agree with it.

27. **woit**
    May 8, 2006

    I’ve deleted a lot of comments from this thread, both from people submitting political rants, as well as those responding to them. If you want to spend your time doing things like attributing straw-man arguments to people and then vigorously attacking them as foolish and immoral, or if you enjoy responding to this kind of thing, do it elsewhere, not here.

28. **woit**
    May 8, 2006

    Bert,

    Actually it’s more of a benevolent dictatorship in practice…

29. **Bert Schroer**
    May 8, 2006

    I meant that the dictatorship is at least administered democratically.

Hype from the Swampland

May 1, 2006
Categories: Swampland

Over the last twenty years there has been an endless stream of hype about "tests of string theory", pretty much all of it complete nonsense. For some examples just from the first few months of this year, see here, here, and here. Most of these examples seem to have been generated by confused PR people who misunderstood carefully worded comments by various physicists about the relation of their work to string theory. The average person just finds it hard to believe that it really could be true that there is no way to test a theory that has gotten so much attention for so long from so many prominent people.

Today there's new nonsensical hype about testing string theory, but this time it's due not to a clueless press relations person, but to several physicists, including the one who decides what gets into the hep-th arXiv and what doesn't. The hype isn't buried in the article somewhere, it's in the title: Falsifying String Theory Through WW Scattering. In their abstract, the authors claim to derive a bound on coefficients of operators in the effective electroweak Lagrangian such that "a measured violation of the bound would falsify string theory."

The first striking thing about this paper that purports to show that string theory is falsifiable is that there's actually nothing about string theory in it. It's only four pages long, and the first three pages consist of an introduction followed by some calculations in the non-linear sigma model one might want to use as an effective low-energy theory of pions. This is just a warm-up exercise for the real calculation that the authors want to make some claims about, which involves the low energy effective action for a non-linear sigma-model coupled to gauge fields. This is the model that one expects to describe the low-energy behavior of the Higgs field coupled to electroweak gauge fields, if one takes the Higgs mass to be very large.

The authors go on to just copy the terms in the relevant Lagrangian down from a 1993 paper by Appelquist and Wu, then stop and promise to actually calculate the relevant bounds in a forthcoming paper. Unless one wants to try and sit down and do oneself the calculation the authors haven't done yet, it's hard to know what these bounds will actually say and whether they will really be non-trivial. It's also unclear to me exactly how all of this depends on the Higgs mass, which I guess is being assumed to very high, thus violating the known indirect experimental bounds from precision electroweak measurements (which assume the standard model). Very hard to tell about any of this, since it's dealt with in a paragraph with no equations.

It turns out that the author's proposal isn't a proposal to falsify string theory at all, but a proposal to falsify the idea that physics satisfies Lorentz invariance, analyticity and unitarity at high energies. This would falsify our standard ideas about QFT, but it wouldn't falsify current ideas about string theory. The authors don't define what they mean by "string theory", but presumably they mean some version of perturbative string theory. This involves a divergent series (even granting the conjecture that one can make sense of these amplitudes at more than two loops), so it's unclear how one is going to "falsify" that. Standard ideology about non-perturbative string theory ("M-theory") is that it will involve some new ideas about space and time, so I don't see how one can assume that it won't violate the analyticity and Lorentz invariance properties characteristic of QFT in flat space-time. I'm not convinced that the author's proposal will falsify anything, but if it does, it will be QFT that is falsified, not string theory. After all, this paper is a QFT calculation (or, more accurately, a promise to do a QFT calculation), not a string theory calculation.

The authors note the problems of non-predictivity generated by the Landscape, and in the first version of the paper write:

Moreover, even if it is found to be difficult to generate the proper model from string theory, one would sooner accept the notion that it is the theorist's imaginations which are insufficient than conclude that string theory has been falsified.

In the second version of the paper, they seem to realize that this attitude of "one" is kind of unscientific, and they change it to

Moreover, even if it is found to be difficult to generate the proper model from string theory, some would sooner accept the notion that it is the theorist's imaginations which are insufficient than conclude that string theory has been falsified.

This new version leaves it unclear who this unscientific "some" is. In both versions they note correctly that:

This line of reasoning has resulted in sharp criticism of the theory.

This paper is motivated by the "swampland" program of trying to find effective field theories that can't be the low energy limits of a string theory. I've written about the problems with this elsewhere, and blog postings by Distler have amply embodied what some of them are. In his first posting on the Swampland he gave as an example of a low energy effective theory that couldn't come from string theory one with only one or two generations, only to be told by a commenter how to construct such things from string theory. He has a more recent blog posting called Avatars of Nonlocality? about the swampland work of Arkani-Hamed and collaborators that motivated this new paper. In a comment there, Arkani-Hamed takes him to task:

This post is a great illustration of what I dislike about blogs and more specifically trackbacks. As I explained to you when you were visiting Harvard last week, your first point about the RG running is standard effective field theory (with an abbreviated discussion in our paper because it is fairly common knowledge–read Georgi's book). I of course don't object to your writing a paper to clarify these points to yourself or others. But this is minor. More importantly, as I also explained to you both in email and in person, what you write about the DGP model is totally wrong…

Now, in general I don't care about what is said on blogs, as I believe they largely fulfill the primate desire to look and see what the other monkeys are doing, and I think they are a big waste of time. But I do object to having a trackback, linked from my paper, to a post about it that claims that one of the central claims is wrong, when a 45 second computation, even done for the reader's convenience in the paper itself, refutes the argument.

This whole subject really is a swamp, if you ask me, and has nothing at all to do with physics, including nothing to do with the supposed "falsifiability of string theory". It will be interesting to see if a referee thinks otherwise.

Update: It has been pointed out to me that I'm being a bit unfair to the authors in characterizing the calculation in this paper as a "warm-up exercise", since they claim that it is a correct first approximation to the actual calculation that they intend to do.

Comments

1. **Benni**
   May 1, 2006

   you can congratulate yourself. You have Distler made to write a paper with your blog.
   One might imagine how long he thought about the problem of falsifying string theory and how desperate some scientists are, when they begin to publish such nonsense.

2. **MathPhys**
   May 2, 2006

It’s all very sad. I sometimes think that if these guys (many of whom are brilliant) took a break from string theory and started thinking about something else (perturbative QCD, collider physics, whatever), they may actually get inspired and find a way to revive strings.

3. **anonymous**
May 2, 2006

The analogous constraints, in the simpler case where the Higgs exists and is light (as data indirectly suggest), had been studied in section 5 of http://arxiv.org/abs/hep-ph/0604111.

4. **justin**
May 2, 2006

Dear Peter,

Isn’t this what you would want… papers at least attempting to falsify string theory, on general grounds?

5. **Alejandro Rivero**
May 2, 2006

Hmm I supposse this is a falsify argument both for string theory and QFT, but at least it aim to proof that the scenary where QFT is falsifyed and string theory remains is unlikely.

6. **woit**
May 2, 2006

Justin,

This isn’t a paper that gives a plausible way to falsify string theory and get rid of it, it’s a paper trying to claim that string theory is in principle falsifiable.

The whole issue of whether string theory is falsifiable, and thus a part of conventional science, is an absolutely crucial one that people are now debating. As far as I can tell, all evidence at the moment is that it is not falsifiable. This paper makes a dramatic claim, in its title, that string theory actually is falsifiable by looking at relatively low energy WW scattering amplitudes. My posting explains why this claim is simply incorrect.

7. **Dick Thompson**
May 2, 2006

You said:
“It turns out that the author’s proposal isn’t a proposal to falsify string theory at all, but a proposal to falsify the idea that physics satisfies Lorentz invariance, analyticity and unitarity at high energies”

In my reading the paper says no such thing. Rather it says that IF there is no light Higgs, AND the calculated bounds are found to be violated THEN EITHER SST is false OR one or more of Analycity, Unitarity, and Lorentz Invariance is false. It is up to the experimenters to determine which is the case. Quoting one sentence out of context does not invalidate this conclusion.

8. **Elliot Stern**
May 2, 2006

I’m not a scientiest, but I think the argument that string theory is not science because it is not falsifiable is a red herring. String theorists don’t claim that they understand string theory, let alone that it is a full fledged scientific theory. If and when string theory develops into a full scientific fledged theory, it will no doubt make many predictions and some will be falsifiable. It seems to me that if you want to attack string theory, you need to argue that it is a dead end, and that there is little or no hope that it will ever develop into a full fledged scientific theory. If you can’t make that argument, you have nothing worthwhile to say.

9. **woit**
May 2, 2006

Elliot,

I have been making pretty much that argument, here and elsewhere (at most length in my book).

10. **Ira**
May 2, 2006

I am honestly not a fan of blogs, but I cant help but just make a short comment about the paper. The paper just points out that there are bounds on gauge bosons scattering which, if violated, would mean that the underlying theory violates one of the assumptions stated in the paper. Namely, unitarity, analyticity and Lorentz invariance. If one assumes that string theory obeys these assumptions, then string theory, is falsifiable.
The most intriguing part of the paper is that we can perform a low energy experiment which probes the complete UV theory. This is what makes this test truly interesting. It also opens the possibility for other such tests which allow us in practice to test certain assumptions we make about the what lies beneath the standard model.

The title could have just as easily been, “Falsifying …….. using W Scattering”, where in the blank you could put in your favorite “theory of everything” which is suppose to obey these mathematical assumptions. Note that the blank could not have been filled with Loop Quantum Gravity, since (see Smolin)it has been conjectured to violate Lorentz invariance in the UV. So that just leaves string theory, defined with those above assumptions.

Now whether or not this is the “working def” of string theory should be discussed. Certainly perturbatively its true and its true non-perturbatively its true in AdS. So it seems reasonable to assume it true
in Minkowski. But…. I believe its fair to say that If the bound were violated it would certaintly mean that we would have to rethink string theory.

If I were a betting man, Id bet the bounds wont be violated but, as I said, the point of the paper was to point out that this kind of bound is
possible at all.

11. **Chris W.**
May 2, 2006

Elliot,

It is very late in the game to be allowing that "string theorists don't claim that they understand string theory, let alone that it is a full fledged scientific theory, " given much of the rhetoric that has surrounded the subject.

At this stage it seems quite likely that genuine understanding will require such a profound shift of perspective and approach that it will amount to a repudiation of much of string theory as a research program, and will be perceived as such by most string theorists, even if some important formal features of string theory are clearly related to elements of the new perspective. (The latter is almost guaranteed to be true; such is the nature of mathematics.) Vital physical ideas are missing, and it doesn't seem likely that they will discovered by continuing to wallow in a vast morass of formalism.

12. **Tony Smith**
May 2, 2006

The paper of Distler et al at hepph0604225 says:
"… Analyticity: the cuts lie on the real axis … with no singularities on the physical sheet off the real axis.
Unitarity: the discontinuity in the forward scattering amplitude across the cuts is given by the total cross section.
Lorentz invariance: the amplitude can only depend upon the three invariants.
Most importantly, these assumptions must be obeyed for arbitrarily short distances.
String theory, which is designed to be valid at all distance scales, is constructed to produce an S-matrix with precisely these properties. …".

It seems that they are saying that, for example, obersvation of ANY Lorentz violation, no matter how small, would falsify conventional superstring theory.

Do other conventional superstring theorists agree ?

David Mattingly at UC Davis has written a review "Modern Tests of Lorentz Covariance" at http://relativity.livingreviews.org/Articles/lrr-2005-5/ in which he says:
"… Currently, we have no experimental evidence that Lorentz symmetry is not an exact symmetry in nature.
The only not fully understood experiments where Lorentz violation might play a role is in the (possible) absence of the GZK cutoff and the LSND anomaly.
New experiments such as AUGER, a cosmic ray telescope, and MiniBooNE, a neutrino oscillation experiment specifically designed to test the LSND result, may resolve the experimental status of both systems and allow us to determine if Lorentz violation plays a role. …".

Would all conventional superstring theorists agree to give up their virtual monopoly on USA high energy theory jobs and grants if Lorentz violation were observed by means described by Mattingly (for example, if the GZK cutoff is seen to be absent due to Lorentz violation) ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – By associating conventional superstring theory with such well tested (at presently accessible energies) principles as analyticity, unitarity, and Lorentz invariance, Distler et al seem to be trying to make it appear to be a respectable physical theory.
If it really were respectable, then (after a generation of hard work by many smart people) it ought to be able to predict something like particle masses, force strengths, KM matrix parameters, etc, that are more easily tested than tiny (perhaps Planck-scale) violations of analyticity, unitarity, or Lorentz invariance.
In fact, Lubos Motl has said (reference frame 21 March 2006) "… particular string models predict the exact masses of all particles …".
Such a spectrum of "exact masses of all particles" would be much easier to test expermentally.
Why don't Motl, Distler, et al write a paper setting out such a spectrum, and saying that if such a spectrum disagrees with experiment, then they will consider conventional superstring theory to have been falsified ?

PPS – I am not asking them to do anything that I have not already done with my own physics model, so I think that it is, coming from me, a fair request.

13. **Tony Smith**
May 2, 2006

The Distler et al paper is at hepph0604255,
not at hepph0604225 which is where my typo in a preceding comment put it.
Since it was a reference number, I am making this correction.
My apologies for all of my many typos, for most of which I just leave uncorrected hoping that people will understand that my fingers don't always type what my mind thinks.
Tony Smith
http://www.valdostamuseum.org/hamsmith/

14. **Brett**
May 3, 2006

With respect to the question of whether Lorentz violation would automatically falsify string theory, I would like to point out that the modern study of Lorentz violation began with the discovery that string theories might contain spontaneous breaking of Lorentz symmetry.

Of course, there are caveats. What was discovered was a Lorentz-violating local minimum of the potential in bosonic string field theory. This is not superstring theory, and, at least for the models studied, this does not appear to be the global minimum. The theories are actually unstable at tree level, and the true vacuum is only stabilized by quantum corrections. The fact that these quantum corrections are involved makes the question of whether the lowest-energy state is Lorentz invariant or not a (difficult) quantitative question, not an obvious qualitative one. I don't know of any general feature of the theory that would ensure that the Lorentz-invariant minimum is the true vacuum; this can only be verified, so far, by computation.

In fact, there are a number of string theorists interested in possible Lorentz violations in the context of string theory, so it seems difficult to believe that the discovery of Lorentz violation would be followed by any repudiation of string theory.

15. **Not a Nobel Laureate**
May 3, 2006

Let's if their forthcoming calculation fares better than the QM version of Feynman + Wheeler's "action at a distance" model.

Wheeler JA, Feynman RP. 1945. Rev. Mod. Phys. 17:156

Where's a guy like Pauli when he's really needed.

It's funny, in a pathetic way, reading all the theoretical pontifications about the Swampland and the nature of space and time on the Planck scale when theorists can't even explain the structure and mass of the proton.

But then acual theories about the latter are subject to annoying and inconvenient experimental tests.

re: recent Happex results.

16. **Tony Smith**
May 3, 2006

Brett says "… In fact, there are a number of string theorists interested in possible Lorentz violations in the context of string theory …".

That is a clear contradiction to the statement of Distler et al hepph0604255:
"… Analyticity … Unitarity … Lorentz invariance …
Most importantly, these assumptions must be obeyed for arbitrarily short distances.
String theory, which is designed to be valid at all distance scales, is constructed to produce an S-matrix with precisely these properties. …".

Such a contradiction shows that either:
1 – the attempt of Distler et al to associate conventional superstring theory with the principles of analyticity, unitarity, and Lorentz violation is incorrect (at best mistaken, at worst dishonest);
or
2 – the "number of string theorists" cited by Brett do not understand the fundamental basis of string theory (as described by Distler et al).

I would be interested in seeing some discussion between Brett and Distler et al about that.

Since I am not a conventional superstring theorist, I really don't care who wins that argument.

With respect to unitarity (in particular, the Higgs unitarity bounds mentioned in the Distler et al paper), there is a lot of indirect Standard Model type evidence that the Higgs probably exists and is between 115 GeV (the upper limit of Fermilab's search) and 200 GeV, so the chance of the LHC finding unitarity violation due to absence of such a light Higgs is probably very small.
My guess is that Distler et al are of the same opinion, and that the title of their paper "Falsifying String Theory Through WW Scattering" was a set-up for a sequel "LHC Confirms String Theory" when the LHC finds that there is no Higgs unitarity violation.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

17. **Tony Smith**
May 3, 2006

My apologies for yet another bad typo. I typed "violation" when should have typed "invariance".
What I should have written above was:

Such a contradiction shows that either:
1 – the attempt of Distler et al to associate conventional superstring theory with the principles of analyticity, unitarity, and Lorentz invariance is incorrect (at best mistaken, at worst dishonest);
or
2 – the "number of string theorists" cited by Brett do not understand the fundamental basis of string theory (as described by Distler et al).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

18. **Anonymous**
May 3, 2006

A new critic of string theory thinks that string theory is not even wrong! Watch the press conference:

http://www.careerbuilder.com/monk-e-mail/?mid=8618821

19. **Paul Valletta**
May 4, 2006

A rebutal ?

http://www.careerbuilder.com/monk-e-mail/Default.aspx?mid=8628834&cbRecursionCnt=1&cbsid=a57e23071f7540b3bef38b62d7b9b45b-200020032-R9-1

20. **Chris Oakley**
May 4, 2006

Another rebuttal of Peter's arguments from leading theorists:

http://www.careerbuilder.com/monk-e-mail/?mid=8630834

21. **Brett**
May 4, 2006

Based on what I know, I suspect that string theory is inconsistent with any explicity violations of Lorentz symmetry in the Lagrangian. However, spontaneous Lorentz violation is probably more interesting anyway. Off the top of my head, there are four ways that a general symmetry can be broken: explicitly, spontaneously, anomalously, and by boundard conditions. That Lorentz symmetry is anomalous is also possible in certain spacetime configurations, and we know that that boundary conditions of our universe break boost invariance quite strongly. For a theory of everything (especially one where the underlying equations of motion are not known explicitly), it may not be possible to disentangle violations of Lorentz invariance due to the fact that the universe is expanding from violations of other origins.

22. **Peter**
May 4, 2006

Man, this blog attracts some very, very bizarre things….

I think the Distler et. al. paper is clear enough, and one of the authors (Ira) emphasizes the point here, about what its technical claim really is. They are deriving bounds on electroweak scattering amplitudes based on assuming analyticity, Lorentz invariance and unitarity. It's highly unlikely these bounds are violated in the real world, but certainly would be very interesting if they are.

These three properties are properties of Minkowski space perturbative string theory amplitudes, and that's what the authors base their "falsifiability of string theory" claims on. I think this claim is unwise, for two reasons

1. These aren't distinctive properties of perturbative string theory, they're also shared by QFT itself.

2. They just refer to "string theory", without defining what they mean by it. This leaves the implication that their results are relevant to the falsifiability of the general idea of string theory as a unified theory of gravity and particle physics, something which is not the case. Current hopes for connecting up string theory with real world particle physics involve branes and other aspects of a conjectural non-perturbative theory, and since no one knows exactly what this theory is, it is not at all certain that it has the three properties at issue.

Unitarity is something that one presumably won't give up, but analyticity is not so clear.

Brett Altschul wrote in about the Lorentz invariance issue, but didn't mention that he has a new paper today on the arXiv about this: http://www.arxiv.org/abs/hep-th/0605044

There's also something on the topic from Cohen and Glashow: http://www.arxiv.org/abs/hep-ph/0605036

23. **Ira**
May 4, 2006

Thanks Peter for clarifying this. Just one last comment. I dont think anyone can rule out "string theory" given its enormous extent and the many forms it may take, many of which are not really understood. However, the canonical forms of the theory (even including non-perturbative objects such as branes) do have all the properties assumed in the paper. Thus, a violation of the bound would falsify this class/definition of string theory. As brett states the possbility of a Lorentz violating background has not been ruled out, though I am not sure if anyone has found such a solution. I believe it is fair to say that if a violation of the bound were found, then string theory would have to be reshaped in a highly non-trivial way.

24. **Thomas Larsson**
May 5, 2006

Ira, how about the following way to falsify string theory as a theory of QG in 4D?

In section 6.1 [hep-th/0501114](hep-th/0501114), Nicolai, Peeters and Zamaklar point out that the free string is quantum gravity in 2D, and that the only non-trivial thing about this model is that the constraint algebra admits anomalies. Assuming that the only non-trivial thing about 2D gravity generalizes to 4D (why on earth would they bring it up otherwise?), the constraint algebra of 4D gravity must admit anomalies. However, there are no diff anomalies in 4D, neither in string theory nor in LQG. Hence both theories are wrong.

25. **Juan R.**
May 5, 2006

Ira,

The problem with "string theory" is it does not exist (there is not theory just a goulash of conjetures, hypotesis, and views). The problem is also that stringers use "string theory" in many ways including incompatible ones (probably because marketing purposes).

There are not "canonical forms of the theory" as you say, just historical development. Let me take the concept of "string theory" in a broad sense.

There are versions of string theories are unitary, and versions are not unitary. E.g. Nanopoulos non-critical string theory introduces a kind of local nonunitarity that he claims (is not true) can solve the measurement problem of quantum mechanics. There are some versions of D0-brane theory are not Lorentz invariant (it is not known is at what extension effects can be detected at low energies), etc.

After of near 40 years, the main postulate of string theory, that the theory is compatible with anything and the contrary of anything, continues being true. But that is not physics.

That I really find unethical of all this are the two main points "always" present in many stringers discourses:

1) Claims about next verification of string theory are not novel (are 20 years old). It is unethical to offer to non-expertises that kind of belief when expertises know what can or cannot be tested in that "theory".

I can understand that after 40 years of continous failure to test the theory (or to fix main properties or even to check basic promises: e.g. quantization of gravity derivation of SM parameters…) stringers are more desesperate that when Glasgow wrote his critical paper decades ago. But forcing an unnatural survival of string theory between mass media and non-expertises via preprints as this.

2) During decades, violation of LI were studied in other theories as LQG. Then the only version of string theory was LI, therefore stringers rudely critized loop theorists. I heard many things including aberrations such as that loop theorists did not understand Einstein (however M-theory proves just the inverse) and that LQG was a kind of absurd ether theory violating all our knowledge about nature (stringers' claim was more absurd still).

I heard that whereas string theory was a beatiful unification of SM with full relativity, LQG would be considered as a kind of ugly 19th century approach and, therefore, abandoned…

It is interesting to see how many string theorists agree on maybe LI is not exact now. History is fascinating.

Juan R.

Center for CANONICAL |SCIENCE)

26. **Juan R.**
May 5, 2006

Sorry by the typo Glashow!

P.S: Ginsparg, P; Glashow S. Desperately Seeking Superstrings. Phys. Today 1986, 86N5, 7-9

Juan R.

Center for CANONICAL |SCIENCE)

27. **Bert Schroer**
May 5, 2006

To Juan R.
One can be much more specific in criticizing string theory.
On the historical side the strongest criticism comes right from its confused start from the remnants of the S-matrix bootstrap program. It substituted the crossing property of on-shell quantities of QFT for the S-matrix and formfactors (for a recent review B. S, Ann. of Phys.319, (2005) 48), which is a deep (and admittedly somewhat mysterious) property up to this date, by what was later called duality, namely the saturation of crossing by infinite one-particle towers (the physical reading of the beta and gamme function properties used by Veneziano). This had no conceptual backing from QFT whatsoever (in QFT crossing is a delicate interplay between a finite number of one-particle states and their multi-particle cuts). It so happened that there was an extremely strong prodding for such a situation from phenomenologists of strong-interaction dispersion theory (especially from a Swiss phenomenologist named (?). Schmidt who was criticized by Res Jost). The phenomenologists really got what they wanted, but a short time later when the first results on high momentum transfer scattering came in, they got completely disillusioned with the duality idea. Without this support the idea was theoretical blue yonder in its purest form but it led to rich mathematical structures; we all know the rest of the story.
Physics would be more healthy if string theorists could be forced to compete on the same level as alternative ideas, but their hegemonial grip on power led to a sociological situation (physics/0603112) in which they are in firm economical control of the market (remember Marx: the existence preceeds consciousness which Brecht succeeded to put into a more popular form).
On the purely scientific side, particle physics incorporates two type of arguments: metaphorical ones and autonomous ones. QFT sometimes uses metaphorical arguments but as one can show in detail at least in the setting of QFT they can always be backed up by (often conceptually and mathematically more demanding) autonomous arguments. These words can be given an extraordinarily precise physical meaning (there is yet no reference, I am presently working on these ideas).
It turns out that string theory is totally metaphorical. Take for example the claim that QFT is originating from string theory in the low energy limit. Such scale-sliding arguments are only worthy if the structural relation between the two settings does not permit their application. The most important and characteristic property of QFT is vacuum polarization through localization. This is the reason why QFT goes significantly beyond quantum mechanics in producing thermal manifestations of localizations which go significantly beyond the uncertainty relations of QM (B.S. http://www.lqp.uni-goettingen.de/papers/06/04/). But string theory has no autonomously fluctuating quantum matter on its target space, its only knows quantum fluctuations which come from the (conformal) two-dimensional source space! Whereas autonomous vacuum polarization may get lost in “scale sliding” (see transition from QFT to nonrelativistic QM), it never can be created, i.e. Lambshift is not created through scale sliding, it has to be there before!
One can continue this list of autonomous versus metaphorical reality.
I do not know about you, but I often wondered why string theory appears so extraordinarily eerie and surreal, like a dreamworld. I finally found the answer in the total metaphorical nature of its arguments.

28. **Mentos**
May 5, 2006

What is this “scale-sliding” that you refer to (and seem to very much dislike)?
The Renormalization Group?

Does Bert Schroer not believe in the Renormalization Group in QFT?

29. **Bert Schroer**
May 5, 2006

No, the RG, if it is correctly implemented, is done on the solution (quite a demanding task!) whereas scale sliding is that rapid kinematical argument (directly done on the Lagrangian/action) by which e.g. people convince themselves that nonrelativistic QM emerges from QFT for small energies (which I called metaphoric).
Since I do not have messianic expectations in particle physics, I would like to leave “beliefs” to people who like to find them there.

30. **Chris Oakley**
May 5, 2006

Bert,

Since no-one has signed up to my particular take on QFT I am in no position to pontificate here, but despite having been a researcher in QFT for a while I cannot say that I have understood much of your 09:13 post. I might add that it may well be possible that one of the reasons why the *Feldverein* remains a small and exclusive club is that a lot of the rest of the world does not know what you (= ihr, (would that English had a 2nd person plural)) are talking about. Of course, the same criticism can be levelled at Superstringers, but then they seem to be calling the shots at the moment, whereas you guys are not.

Or, to put it another way, your comment looked interesting, please expand/explain …

31. **Mentos**
May 5, 2006

“No, the RG, if it is correctly implemented, is done on the solution (quite a demanding task!) whereas scale sliding is that rapid kinematical argument (directly done on the Lagrangian/action)”

What do those words mean?

Wilson’s renormalization group is a transformation on the action (in response to an infinitesimal change in the cutoff).

Are you saying there is something wrong with Wilson’s renormalization group?

32. **Bert Schroer**
May 5, 2006

Chris,
what I said this morning among other things is that Veneziano duality in contast to the crossing property of on-shell objects (S-matrix, formfactors) of QFT is a new postulate which without the phenomenological support from strong interaction, which however only had a shelf life of less than two years, would never have come into being; the mathematical content alone would not have been strong enough.
Do you not understand the logical content of this statement or are you worried of how to prove it? In the latter case I cannot help you in this blog (because of thematic restrictions), but I gave you a referemce which may be at least of some help.

33. **Bert Schroer**
May 5, 2006

Mentos
I told you what I meant by scale sliding, whereas you want me to enter the swampland of the renormalization group which I am perfectly prepared to do, but not in this blog, since it is not its topic and also since people don't use this sort of argument when they say that QFT follows from string theory (the present knowledge of string theory is not sufficient to do something like this).
Let me again try by analogy try to get my point (about a structural comparison which has to precede scale-sliding) across. Suppose you would live in a 2+1 dimensional world where the locality principle allows for braid group statistics. Maintaining the approriate relation between spin and statistics in the process of energy becoming small compared with the mass, you would never recover quantum mechanics but always end up with a nonrelativistic QFT (vacuum polarization would be persistent). On the other hand in d=3+1 where plektons cannot exist, the Bosons/Fermions allow for a relativistic free field realization in which the one-field state contains no admixture of vacuum polarization. This makes the physical relevance of QM possible because now there is at least no structural obstacle against "scale sliding" or if you want the somewhat more sophisticated RG arguments (I think by now you know what I mean without having to enter any additional controvery). Conversely the nonexistence of autonomous vacuum polarization of quantum matter on target space (fluctuations only take place in the 2-dim conformal field theory which defines source=worldsheet space) in string theory makes it structurally impossible to recover anything like Lambshift. I know that you are willing to sacrifices a lot of things from the past in getting some messianic string ideas ideas going. But do you really want to sacrifice also Lambshift?

34. **Mentos**
May 5, 2006

"I told you what I meant by scale sliding, whereas you want me to enter the swampland of the renormalization group…"

I didn't understand what you meant by "scale sliding", which is why I asked for clarification.

I take it, from your use of the word "swampland" to describe it, that you don't like Wilson's Renormalization Group very much.

"Conversely the nonexistence of autonomous vacuum polarization of quantum matter on target space …"

One-loop vacuum polarization computations in string theory look like fancy generalizations of the corresponding field theory computations (minus the UV divergences).

I don't understand your comment about "sacrificing the Lambshift."

35. **Bert Schroer**
May 5, 2006

My question to you: does quantum matter on string target space have vacuum polarization (on target space and not on the 2-dimensional worldsheet!)? I agree that some perturbative expression have a vague resemblence but this is a structural question! (and by the way QFT is not about taming ultraviolet infinities but rather about finding autonomous finite parametric subspaces in an apparently infinite parametric space of coupling strengths); either there is a target vacuum polarization (e.g. on the bondaries of localization of quantum matter on target space) or there is not. This is not a quantitative matter in the same vein as it is meaningless to take about "being a little bit pregnant".
As far as my underlying philosophy of QFT goes it is Wilsonian and not TOEian. But Euclidean field theory is primarily statistical mechanics (following Osterwalder and Schrader, for a recent review see hep-th/0603118) and its use should be limited to structural arguments in (real time) QFT (because of the extremely subtle reflection positivity).
If you are so convinced to know what a cutoff is then please tell me what I have to do in any of those infinite families of two dimensional factorzing models (the Sine-Gordon/massive Thirring being the oldest one) in order to introduce a cutoff into their known formfactors or correlation functions; its not a retorical question, I honestly do not know how to do that. I of course know what a cutoff is in a Feynman integral but I am already lost if I should do something like this on a functional integral and even if I succeed how do I convince myself that I preserved the all-important reflection positivity which is my only assurance that I am still within quantum physics? I think if you ever seriously confront these question you will come to the conclusion that it is mathematically (and conceptually) easier to formulate and control local QFT than nonlocal (cutoff etc.) ones in which the nonlocality results from momentum space manipulations. Particle physics is not about attending holy cows; it is of pivotal importance to re-investigate old problems if there are new conceptual tools to do this.

36. **Mentos**
May 5, 2006

"I agree that some perturbative expression have a vague resemblence but this is a structural question!"

I don't understand your structural question, nor what you mean by the statement that the resemblance of the two perturbation expansions is only a "vague" one. Sure there are important differences, but none of them have anything to do with low-energy physics like the Lambshift.

"As far as my underlying philosophy of QFT goes it is Wilsonian…"

If your philosophy of QFT is Wilsonian, why do you call the Wilsonian Renormalization Group "the swampland"?

"If you are so convinced to know what a cutoff is …"

The presence of a cutoff is absolutely central to the Wilsonian approach to field theory. So your distrust of cutoffs is more than a little puzzling for someone whose underlying philosophy of QFT is "Wilsonian."

How do you feel about a lattice cutoff?

37. **Chris Oakley**
May 5, 2006

Hi Bert,

No, I think I got the first bit.

> On the purely scientific side, particle physics incorporates two type of arguments: metaphorical ones and autonomous ones.

So "autonomous" here just means "not metaphorical"?

> The most important and characteristic property of QFT is vacuum polarization through localization.

What? The most important and characteristic property of QFT is that it is quantum mechanics wherein particles can be created and destroyed.

> This is the reason why QFT goes significantly beyond quantum mechanics in producing thermal manifestations of localizations which go significantly beyond the uncertainty relations of QM (B.S. http://www.lqp.uni-goettingen.de/papers/06/04/).

This is AQFT jargon that means nothing to me.

> But string theory has no autonomously fluctuating quantum matter on its target space, its only knows quantum fluctuations which come from the (conformal) two-dimensional source space! Whereas autonomous vacuum polarization may get lost in "scale sliding" (see transition from QFT to nonrelativistic QM), it never can be created, i.e. Lambshift is not created through scale sliding, it has to be there before!
> One can continue this list of autonomous versus metaphorical reality.

I expect that there are a whole swathe of papers I need to read before I can understand this (& I wonder whether any of them actually calculate cross sections).

38. **Bert Schroer**
May 5, 2006

A lattice cutoff would perfectly satisfy me but please tell me how you introduce any cutoff in a model whose rigorous construction principles are not following the Lagrangian quantization logic. You do not seem to be aware of the explicit existence of a large class of strictly local 2-dim. QFT which owe their existence to completely different ideas than those of Lagrangian actions and Wilsonian RGs. This is a class where not only the old bootstrap idea about the S-matrix works, but there exists in addition a QFT which is uniquely associated with this S-matrix (the formfactor part of the formfactor-bootstrap program). The point is that any introduction of a cutoff in any form would totally wreck this constructive approach by which they have been manufactured (I don't think Lagrangian quantization will ever lead to such explicit constructions even in those case where the models may be baptized with a Lagrangian name). The cozy conceptual security in which you seem to live (and of course not only you personally) has been already seriously undermined apparently without you being aware to live in an undermined world. Of course you may want to execrate this reality by claiming that these models are irrelevant for TOE fans but it certainly will not go away but rather will grow stronger. It looks that this is just the first glimpse of a new way of entering QFT and a new way to look at QFT, and it is far away from the swampland entries.

39. **Arun**
May 5, 2006

So "autonomous" here just means "not metaphorical"?

What I have gathered from the conversation so far is that autonomous means standing on its own, not needing a reference to something else.

40. **Bert Schroer**
May 5, 2006

Sorry, I give up. Please don't think that I am arrogant and I want to put you down. In reality am sad about this situation. The schism in particle physics generated by more than 30 years of string-induced metaphorical (as opposite to autonomous) thinking (string theory is only the culmination of something which already started before) has led to a situation where a genuine dialog is estremely difficult. I agree with Smolin that the physics discourse is on the verge of breaking down, but not only between string theory and LQG but also between advanced QFT and attempts at TOEs.
I don't think it is my ignorance because I have followed the development of string theory form its cradle and I have a certain sympathy for the more conservative aim of LQG (which unfortunetly gets into a false recent competition with ST which forces it to occupy territories which are outside its conceptual range). But the fact that I understand what other people are doing is of little help here if it remains one-sided.
Chris, I am disappointed that you ask that abominable question. Of course I can calculate cross section and they would be the same as everybody else's. Pointlike fields are singular generators of algebras and perturbative interactions are formulated by polynomial invariant couplings of free fields. Of the infinitely many forms for (m,s) free fields (see Weinberg) you can use any you want in doing causal perturbation theory. In case you have a Lagrange-Euler description which generically is the exception rather than the rule (for low spin fields such free field representations are known) you can also use the functional integral approach but why should I put this restriction on myself? Local quantum physics is an extension and not a narrowing down and if you define QFT through functional integrals you are narrowing it. Renormalized causal perturbation theory is the most general framework based on pointlike free fields and it is totally mathematically rigorous (in the sense of formal power series just as the formal algebraist like Drinfeld treat Kac-Moody representations or vertex algebras)but of course not in the sense of convergent sums which are formally representing the would be operators in Hilbert space.
Let us never come back an loose time on re-hashing such questions over and over again.

41. **Bert Schroer**
May 5, 2006

To Arun
Autonomous means that it contains its interpretation i.e. there is no freedom to add anything from outside, and the terminology metaphorical is used if the interpretation has to be added from the outside. Observe that metaphorical does not mean wrong, of course you make sure that you do not add something which creates an inconsistency (it is similar to Goedels situation in the theory of logic).
In QFT one often uses metaphorical arguments but thanks to vacuum polarization which distinguishes QFT significantly from any kind of relativistic QM matephorical arguments in QFT can always be replaced by (generally more deep and demanding) autonomous ones. I discuss such situations in connection with thermal manifestations of localization in QFT (for example the entropy problem) in
http://www.lqp.uni-goettingen.de/papers/06/04/
although I did not yet use this terminology in those papers. The usual thermodynamic limit approach in thermal QFT is metaphorical but there exists an open system formulation based on the so-called "split property" (which makes essential use of vacuum polarization) which converts the sequence of finite systems into a sequence of genuine inclusions which are all subsystems of an open system. In Haag's book it is referred to as the statistical mechanics of open quantum systems. Without quantum matter which reacts to the process of localizing it by producing vacuum fluctuations on the causal horizon you cannot get to the autonomous formulation of open systems. QM and string theory do not have that vacuum polarization you need. Wheras this lack of autonomy does no harm to QM, it makes string theory (because it is supposed to contain autonomous QFT) look very eerie, more like a surreal painting of Dali than a realistic theory of quantum matter.

42. **Mentos**
May 5, 2006

"You do not seem to be aware of the explicit existence of a large class of strictly local 2-dim. QFT which owe their existence to completely different ideas than those of Lagrangian actions and Wilsonian RGs."

Almost all nontrivial 2D CFTs are non-Lagrangian. Most of the known integrable 2D QFTs are constructed as relevant deformations of a 2D CFT, and so are non-Lagrangian as well.

The same is true in higher dimensions. Nontrivial interacting fixed points generically have no useful Lagrangian formulation. (Sometimes such theories can be realized as the IR limits of a weakly-coupled Lagrangian field theory. Sometimes they can be realized as the limit of a family of Lagrangian field theories.)

There are even (thanks to string theory constructions) examples of nontrivial interacting QFTs in 5 and 6 dimensions. These, of course, are also non-Lagrangian.

"QM and string theory do not have that vacuum polarization you need."

In the case of QM, you are, of course correct. In the case of string theory, you seem to have gotten a wrong impression somewhere along the line …

43. **Bert Schroer**
May 5, 2006

No, I think we are living in different conceptual realms. When you say you can construct you mean that you can make a mental picture but not anything which a mathematician or a mathematical physicist would call a construction or a proof of existence. There is not a single construction or existence prove in higher dimensional QFT with or without the RG, not to mention string theory. In string theory, unlike QFT one does nor even know any intrinsic characterization one only has some recipes. If the word construction would still have a mandatory engaging meaning which it used to have before the new barbarians appeared on the scene, we would never enter such futile arguments.
And finally: why do you think there are these trillions of vacua which do not communicate; in a theory with vacuum fluctuations caused by local quantum matter this would not happen, they would all communicate and not live separate lives as in a classical theory.
If somebody would have frozen me at a time when particle physics was still healthy and woken me up in the present, I would feel like in one of Kafka's short stories: eerie and surreal.

44. **Mentos**
May 5, 2006

"There is not a single construction or existence prove in higher dimensional QFT with or without the RG, not to mention string theory."

That's right.

By your criterion, QCD does not exist.

"If somebody would have frozen me at a time when particle physics was still healthy and woken me up in the present, I would feel like in one of Kafka's short stories: eerie and surreal."

Nonsense.

No one believed that the limited class of field theories, constructible by the techniques of Constructive Quantum Field Theory, were the only things that existed back then either.

Essentially all progress back then came from what you call a "different conceptual realm." And that continues to be true to this day.

If anything, the set of rigorous results in 2D (the only dimension where CQFT/AQFT ever had any traction anyway) coming from other approaches is vastly greater now than it was then.

As to dimensions above 2 (like, for instance, 4 dimensions, where we happen to live), life goes on, despite the utter lack of progress from Constructive/Algebraic Quantum Field Theory approaches.

If you want to spend your time working on rigorous approaches to 2D QFTs, that's fine. But don't pretend to be surprised at other people's lack of fascination in your progress.

Only in your fevered imagining is that a sign of the sickness of high energy theoretical physics. The center of attention has always been elsewhere and, for intellectually-sound reasons, remains elsewhere today.

45. **Chris Oakley**
May 6, 2006

> Let us never come back and lose time on re-hashing such questions over and over again.

Just this once:
The cross section calculation in 3+1 dimensions you refer to is I assume based on the Epstein/Glaser causal perturbation theory as outlined in (e.g.) Scharf's *Finite Quantum Electrodynamics*. I bought a copy of the latter a while back and have a made a mental note of going through the arguments it in detail, but have not done so yet for the following reason: it just looks like good old renormalizing Feynman-Dyson perturbation theory in elegant clothes. Cutoffs are a crude way of "dealing with" theories that give unwanted infinite results, but not the only one. One may also introduce other kinds of spurious degrees degrees of freedom and take spurious limits and although I will grant that what is done here is better than cutoffs or dimensional regularization it still seems to amount to the same thing.

46. **Bert Schroer**
May 6, 2006

Yes Chris, everything of a systematic nature which you can do with functional integrals you can also do within the causal perturbation setting and much more (the perturbation theory in CST can only be done in cp and the resulting local covariance principle of Hollands, Wald, Brunetti, Fredenhagen and Verch is truely something which shows the vibrancy of modern QFT and there are several other startling results outside of the radar screen of string theory; but fortunately the relevance of some discovery for physics is not determined by Gallup polls among string theorists).

47. **JC**
May 6, 2006

Bert,

Have you thought about writing your own textbook on quantum field theory, emphasizing the aspects which you feel are being neglected in modern textbooks treatments (ie. Peskin & Schroder, Zee, etc …)?

48. **Chris Oakley**
May 6, 2006

Peter's opponents apparently in a more conciliatory mood:

http://www.careerbuilder.com/monk-e-mail/?mid=8823799

49. **Bert Schroer**

May 6, 2006

JC,
yes I have, but there are certain problems which I want to see solved before. I probably would not do this on my own because it is not enough to have new ideas (including of course new ideas on old open problems), one also needs a critical counterpart. Presently advanced QFT is too much in an upheaval, maybe in two years from now some of the dust will have settled. There are very astonishing structures in QFT which could not have been noticed with the old conceptual apparatus. The local operator algebras in QFT are all of one kind, i.e. if you have seen one, you know them all (just like points in geometry) and the richness of QFT, including its inner symmetries and its (noncompact) spacetime symmetries, is all contained in the relative position of a finite number of copies of this "monade" (explicitly: hyperfinite Type III_1 von Neumann algebras, Ed Witten may pardon me) which are placed in terms of modular algebraic concepts with geometry being the result and not the input (note the difference to Atiyah-Witten). The setting resembles that of Vaughn Jones's inclusion theory but the monades used here lead to modular inclusions and they lead to spacetime, whereas the Jones inclusions are limited to only notice a left-right distiction (braid groups, topological field theory). The concepts and mathematics are clear and rigorous, what is completely open is the range of physical content.
I do not complain that the small dedicated group of people who are engaged in this project do not get enough attention, since I am convinced that real progress will not result from those globalized monocultures: there seems to be a critical size just as in the theory of traffic collapses. But of course on the other hand I am afraid that we will be left with near zero new blood at a time when it would be really important because the ball of local quantum physics seems to be rolling again. My polemic article was not written for the sake of polemics but mainly to let those many physicists who have doubts about how particle physics is administrated by string theorists know that there is another world. We are in a situation in particle physics which never was confronted before and which definitely cannot be explained without taking sociological aspects into consideration.
I think Arun makes a serious mistake if he thinks that this eerie and surreal appearance of string theory is my personal problem; he would be surprized how many particle theorists have a similar impression; in fact hep-th/ has in the eyes of most particle physicists become a site of messianic TOE expectations.
My question to Arun about the structural status of vacuum polarization was carefully chosen because the very nature of those above mentioned monade algebras already incorporates the vacuum polarization and thermal properties in their algebraic structure (these algebras have no pure states at all!). They are the heart of local quantum physics (much more basic than the use of particular field coordinatizations) and it would be interesting and important to know what string theory puts in their place. What is somewhat depressing is not that Arun does not know the answer, but that he gave the impression that it is not worthwhile to know the answer; for him it is only a minority interest of some isolated individuals who do their little thing and are detached from the great design of a final TOE. Although I live in a completely different conceptual realm than Peter or Lee Smolin (whose worries about a falling apart basis of communication within particle physics I however share) I never have the impression that they are dismissive about different views or that they have hegemonic ambitions.

50. **Bert Schroer**
May 6, 2006

With all appologies to Arun, I mean Mentos.
These pseudonyms add an impersonal flair. I think their original purpose is to further the course of democracy so that somebody, who wants to discuss with a bigshot, does not have to be inhibited or afraid of his reactions; but on the other hand they should not be used denigrate somebody who signs with his name and takes the responsibility for what he writes. Up to now I did not have the impression that such an abuse occurred with respect to my contribution.

51. **Mentos**
May 6, 2006

"What is somewhat depressing is not that [Mentos] does not know the answer, but that he gave the impression that it is not worthwhile to know the answer…"

I am sorry if I gave that impression. It's not that I don't think the answer is interesting. It's that I don't understand the question.

As to the status of CQFT/AQFT as a "minority interest of a few individuals," you seem to be arguing that, in some past golden age, that was not the case. The fact is that it was always a minority interest.

That's not a criticism of AQFT. It's simply a statement of reality, and important to repeat for those too young to remember the "golden age."

"…they should not be used denigrate somebody who signs with his name and takes the responsibility for what he writes."

Which is not my intention either.

52. **Bert Schroer**
May 6, 2006

Mentor,
"As to the status of CQFT/AQFT as a "minority interest of a few individuals," you seem to be arguing that, in some past golden age, that was not the case. The fact is that it was always a minority interest."
That is certainly true, but this was a democratic choice and if one made that choice and had some innovative ideas there was no problem, it was the quality on the level of the state of art of an area which determined careers. There was no need for a hegemonial control through citation indicees in which mathematical physics fares less well so AQFT also could profit from the "golden age".

"That's not a criticism of AQFT. It's simply a statement of reality, and important to repeat for those too young to remember the "golden age.""
I meant "golden age" for particle physics not for AQFT, although as I said AQFT did not fare badly.
Now that we are in tune again, I could try once more to get to that question of intrinsicness (autonomy) with respect to string theory across. Let's forget the vacuum polarization aspect and explore that intrinsicness in a more specific context of the free 10-dim. superstring (to avoid tachyons) in the covariant gauge (conformal formalism, BRSTetc.). We probably can rapidly agree that if one applies the free string field operator once to the vacuum (the "one-field state") and descends via BRST cohomology to the physical subspace, one obtains a highly reducible unitary Poincare representation which describes a mass tower. Take it from me (we can argue about this separatly, this is where I use modular localization) that the two point function which is a function of center of mass X and internal dynamical variables is point-localized in X and hence the physical projection of that free string field inherits this localization. If that field would have been string-localized it would have shown up right here in a reduced spacelike (anti)commutativity where the geometry of the string would be plainly visible (if one string comes into the timelike causal shadow of the other). It does't, and so you could get the impression that the situation is indistinguishable from a pointlike infinite component free field. But I am almost certain (I have not done the computation) that the result will have a much better spacelike commutativity than one can get in infinite component QFT (and in any QFT for that matter): depending on the excitations of the internal degrees of freedom the situation will be "supercausal" i.e. the (anti)commutator will continue to vanish up into the timelike region (inclusing lightlike distances) below a certain timelike distance which varies with the internal excitation state. This then may be considered as an intrinsic characterization of a free string versus an infinite component free field. I is certainly independent on all the words which went into its manufacturing (conformal QFT, BRST, reparametrization and gauge aspects etc.). In other words the question is which property distinguishes the string mass tower from a generic infinite component tower. I do not know the answer, but would you agree that this at least is a way for aiming at an intrinsic meaning?

53. **Mentos**
May 6, 2006

No, let's stick with vacuum polarization.

Maybe what we need is an example of the kind of explanation that would satisfy you.

Consider Lattice Gauge Theory. What “structural property” of Lattice Gauge Theory ensures that it has vacuum polarization?

54. **Ming**
May 6, 2006

Sorry this is off-topic, but I’m wondering what’s happening to Peter Woit’s book “Not Even Wrong”. I thought it should have been published in April (according to one listing in amazon), but apparently not. And another listing in amazon says it won’t be published till 9/30/06. Just want to get a more definite answer from the author. Thanks.

55. **Bert Schroer**
May 6, 2006

Mentos,
Unfortunatly nothing, you just do not get those nice monades which structurally encapsulate and bind together(modular) localization, vacuum polarization, thermal manifestation of localization in the vacuum etc.
I would not know how to rewrite my papers on localization entropy on the Goettingen AQFT server in terms of a lattice model.
It seems to me that the lattice is good for nonperturbative approximations but not for structural problems, in fact it is particularly efficient if you do not yet understand the conceptual structure of a problem. In the present case one really knows a lot about the QFT structure, and that analytic insight should not be vasted by returning to a lattice (I do not know how the lattice works for string theory).
In principle one should find an answer somewhere, but it is too much buried in details. Just as in QM, vacuum polarization is not structurally forced upon you. In QM and lattice theory you always have factorization between the Hilbert space of a region and its complement (and the cacuum factorizes trivially), whereas in QFT there is never a factorization between the subspace of localized states and that of states belonging to the spacelike disjoint region. In fact you can only create such a tensorproduct factorization if you allow for a “collar” region which separates a localization region from its causal disjoint; but even if you do this “splitting” you will not loose the thermal aspects of the vacuum (which are never there in QM and lattice theory unless you introduce a heatbath by hand).
With other words it would be a terrible mess if you were to describe the Hawking radiation phenomenon on a lattice. For this reason you have to be satisfied with my other more pedagogical example; try to look at it, it is not so bad for getting the structural point of “intrinsicness” across.

56. **Who**
May 6, 2006

there is a logical point about testing scientific theories that in the long run is probably more important than some particular paper about a particular theory. it is illustrated by this post:

“…it says that IF there is no light Higgs, AND the calculated bounds are found to be violated THEN EITHER SST is false OR one or more of Analyticity, Unitarity, and Lorentz Invariance is false. It is up to the experimenters to determine which is the case.”

Something worries me about this. suppose the conclusion were
“(EITHER SST is false OR General Relativity is false)” And suppose the conclusion were “It is up to the experimenters to determine which is the case.”

This is not a possible way to falsify SST, because logically in order to do that one would have to prove that General Relativity is TRUE. but that is, in principle, impossible. One cannot show that a scientific theory is true one can only continue to test it.

One can not put up to experimenters the job of proving a theory true.

But in order to function as a falsification of SST one would have to do just that. therefore the logical scheme presented here UNSATISFACTORY as a proposed falsification. (this is not a Distler issue or a SST issue, it transcends the immediate circumstances and involves our understanding of how theories can be tested)

The scheme would only work marginally in a context where all educated people of good faith accept Gen Rel without question. As an unquestioned premise that is not felt to need experimental support.

But in our particular case here, “Analyticity, Unitarity, Lorentz Invariance” is very far from being such an unquestioned premise. I see indications of widespread skepticism that nature conforms to those three rules. As the poster indicated, we could NOT take it on faith but would have to hand over the job of verifying it to experimentalists. They, however, would be logically incapable of verifying these as features of nature, and could at best falsify them.

The Distler et al paper, it seems, could serve a textbook example of how NOT show the falsifiability of a theory (because it illustrates this simple, but easily overlooked logical point.)

57. **Mentos**
May 6, 2006

Schroer said:

“Unfortunatly nothing, you just do not get those nice monades which structurally encapsulate and bind together(modular) localization, vacuum polarization, thermal manifestation of localization in the vacuum etc.”

Well, if lattice gauge theory does not have the requisite “structural properties” that you require, then I am sure that nothing I can think of will either.

QCD, for you, is a nonexistent quantum field theory.

The problem lies not in string theory, but in all of the “quantum field theories” of moderm particle physics. Even the best of them is not defined in a fashion that satisfies your requirements.

Who wrote:

““‘…it says that IF there is no light Higgs, AND the calculated bounds are found to be violated THEN EITHER SST is false OR one or more of Analyticity, Unitarity, and Lorentz Invariance is false.”

I don’t think that is what their paper says.

They argue that IF there is no light Higgs, AND the calculated bounds are found to be violated THEN one or more of Analyticity, Unitarity, and Lorentz Invariance is false. Since SST incorporates the properties of Analyticity, Unitarity, and Lorentz Invariance, then IT, TOO, must be false.

That’s quite different from the logical syllogism you propose. Perhaps it has its own flaws, but not the ones you claim.

58. **Who**
May 6, 2006

thanks for the reaction Mentos. I am glad you have a different interpretation, this gives more of a chance for Distler et al work to exhibit genuine falsifiability.

I have three questions for you.

what does it mean to assert that “Analyticity is false” ?

what does it mean to assert that “Unitarity is false” ?

what does it mean to assert that “Lorentz invariance is false”?

Perhaps as you suggest, your version of the syllogism has its own flaws, such as the flaw that these statement are meaningless. What is your view?
Personally I cannot imagine attributing a meaning for these statements unless we venture into the realm of metaphysics and make normative statements restricting the mathematical content of physical models—i.e. stop judging theories on the basis of their performance.

Metaphysical statements such as (“mathematical functions used in modeling should be expandable in power series”) are not, I believe, empirically testable—-they concern human conventions and not the physical world. How does one falsify a metaphysical principle,
or a normative injunction about conventions?
And then if you DO falsify it, or at least contradict it, you get something like (“it is OK for mathematical functions to be expandable in power series but also OK if they are not, maybe they dont even all have to be differentiable, whatever works!”) And then how does that falsify SST?

It seems to me that “Analyticity is false” merely enlarges the range of theories that the maker of the statement is willing to consider and test. SST would still be required to make predictions and be judged on its merits, whatever be the field of competitors.

Something fishy about Distler et al claim to show falsifiability, I suspect. Can’t get a paraphrase of the argument that means anything. If you have one, I’d be very glad to hear it.

59. **Mentos**
May 6, 2006

Saying that one of these properties is false is simply to say that the fundamental theory at very short distances does not possess this property. Perhaps, like LQG, the fundamental theory is not Lorentz-invariant at short distances. Or, perhaps one of the other properties does not hold.

Anyway, the dispersion-relation bounds of Distler et al are derived assuming that these properties hold to arbitrarily short distances in the fundamental underlying theory (whatever it is). If the bounds are found to fail experimentally, then these assumptions about the short distance theory must be false.

Perhaps, in that case, the true short distance theory is LQG. In any case (they say) it isn’t string theory.

60. **Bert Schroer**
May 7, 2006

To Who says,
I have the same problems with the paper. Analyticity is not a physical principle and Lorentz invariance may refer to the fact that string theory among other things permits also a Poincare-invariant solution. As it stands it implies a somewhat uninteded cabaret interpretation of the word “theory of everything”. Nevertheless it may indicate a deep desire to bring the discussion back to a more conceptual level. A characteristic aspect of the mentioned “golden age” is that it brought the conceptual dialogue to an art form and this has been lost and we probably must re-learn it.
Presently many works place calculations directly next to sophisticated mathematics without a mediating conceptual link. I am very suspicious when I see all that high powered and from a physical viewpoint amok-running mathematics (differential- and algebraic- geometry, Langlands etc) being juxtaposed to supersymmetry, e-m symmetry and other unfulfilled physical pleas to nature over and over again, although the D-J-R paper cannot be accused of duing that. But a first swallow does not yet make a spring and one has to wait and see whether this indicates a trend.

61. **Adrian Heathcote**
May 7, 2006

Bert

I’ve been finding your posts very interesting — and engagingly frank. My own interest is perhaps a very small subset of yours. I’m interested in the possibility of solving some of the interpretative/foundational problems with QM (not QFT) by looking more closely at the algebraic structure — in particular what happens when you couple systems together either in measurement situations or in EPR-type ensembles. It seems to me that some subtleties have been glossed over in traditional presentations: it is for example usually ignored that there are a very large number of tensor products that may describe a coupled system, and this range, in a sense, represents a physically determinable aspect of coupled systems that has not been looked at.

I’ve always thought that this aspect of QM (entanglement etc) must really get its fullest exposure in QFT and have always been quite dissapointed with what I’ve found. (Let alone what is in string theory.)

Is this an aspect of AQFT that you can comment upon? Particularly as many people think that AQFT ‘s local nature precludes it from saying anything about this from the outset.

62. **Arun**
May 7, 2006

Bert:

If you’re not writing a textbook right now, could you outline a reading program? Let the starting point be a grad student who has had a first course in QFT.

Thanks in advance!

63. **Bert Schroer**
May 7, 2006

Concerning basic advanced structures in QM off hand I would recommend Claas Landsman's homepage. I have seen a link in Peter's homepage. The issue of entaglement etc. is a bit more demanding in QFT as a result of the ubiquitous vacuum polarization.
Also the second addition of Haag's book on QFT has some nice material on the measurement which is presented in an interesting personal flair. More advanced topics on ERP and Bell in the context of QFT you find on Steve Summers homepage.
http://www.math.ufl.edu/~sjs/
Very interesting and highly recommendable are also publications of the University of Pittsburgh Philosophy department. These people really know what they are talking about. I had many interesting email exchanges with Rob clifton and I was deeply saddened when I heard about his early death a couple of year ago. Hans Halverson, who is now in Princeton is a very worthy successor (he actually was his student). Apparently because of its rich conceptual structure AQFT has a very strong attraction to philosophers of science. Without knowing a little bit on the mathematical side one may feel lost in their articles, but I think they have also supplied some more pedagogical articles.
Articles of Halverson you can also find on
http://www.lqp.uni-goettingen.de/
Arun
Since I have my reservation about recent literatur on QFT, I still would recommend Haag (not so much as a textbook to learn tools of the trade, more to get a conceptual overview), also Streater-Wightman and Jost are still good in what they cover (and there are many important things they do not cover). For scattering theory, which is a cornerstone of AQFT, there is also a recent little book by Araki. For the tools of the trade Itzykson-Zuber is still good. With all those antidotes I think you can also get sum honey from Peskin-Schroeder since you can then easily spot the weak part where they allowed the string theoretical carricature of QFT to take over.

64. **Mork**
May 7, 2006

>With all those antidotes I think you can also get sum honey from Peskin-Schroeder since you can then easily spot the weak part where they allowed the string theoretical carricature of QFT to take over.

That must be the best 1-line book review ever!

Any choice words on other popular QFT texts (Weinberg, Srednicki, …)?

65. **Alejandro Rivero**
May 7, 2006

Bert, speaking of weakness, what do you think of Doplicher's quantum area in the context of AQFT? Does it implies that the net of observable algebras contains a minimum algebra? Is it in contradiction with some fundamental issue of AQFT?

(and yep, I join in recommending the reading of Haag's in paralell with P&S. I "stole" (xeroxcopied) Haag's draft when in display at Leipzig IAMP meeting, and then I went straigh for the full book when it appeared at the bookstores).

66. **Bert Schroer**
May 7, 2006

Alejandro Rivero,
"Bert, speaking of weakness, what do you think of Doplicher's quantum area in the context of AQFT? Does it implies that the net of observable algebras contains a minimum algebra? Is it in contradiction with some fundamental issue of AQFT?"
If your question is what I think about Sergio Doplicher's contributions to QFT, my answer is that I am impressed by them. The deepest work (in collaboration with John Roberts and in part Rudolf Haag) is the derivation of the structure of charge sectors, their statistics and the inner symmetry (in 4-dim. always a compact symmetry group) as well as the construction of the charge-carrying field algebra from basically the causality and spectrum structure of local observables. This is like Marc Kac's "how to hear the shape of a drum" where the ear is the observable algebra and the shape of the drum is the field algebra i.e. the field algebra including its particle statistics and its inner symmetry is constructed from its observable shadow (the observable algebra), a marvelous conceptual achievment. You cannot do such things in the Lagrangian setting where the structure of the field algebra is assumed ab inicio.
Sergio (together with Roberts and Fredenhagen) also worked on "noncommutative QFT" with Quantum Gravity in mind (although what they actually do is noncommutative QFT in Minkowski spacetime). Despite their "good physical intentions" they arrive pretty much at those problems which other people many years afterwards also found with less good physical motivations (a L-invariance breaking "aether", only partially solved problems with unitarity, no complete improvement of short-distance properties etc.)
In my view (AOP 319 (2005) 92) the most serious problems (in addition to unitarity) which the present generation of physicists are generally overlooking is the question of whether some sort of macro-causality (no timelike precursors, spacelike cluster properties) can be maintained. As I explained in my review, an earlier generation of physicists (in the 50-70ies) who studied the feasibility of nonlocal QFT really payed attention to this point and ended in failure (wrong in a sense which Pauli would have deeply appreciated!); this was at a time of honesty in particle physics when people did not published positive results and surpressed negative ones even though they came from the same assumptions.
I know that there is presently only one setting which fulfills all the assumptions (including cluster properties) the "direct particle interaction" of Coester and Polyzou, but the profound work of these nuclear physicists seem to remain unnoticed among particle physicists. When I mentioned this in a blog last month there came a very interesting reaction from Eugene Stefanovich pointing out that there are other attempts which use a more field theoretic presentation, but I did not yet find the time to look at them (they may very well also treat that macrocausality requirement correctly, but since this is a very delicate conceptual point, I prefer to look at this before I say something).
I have a question to all of you, can anybody provide me with a reference where the cluster factorization property of the string S-matrix was shown?

67. **Mentos**
May 7, 2006

"I have a question to all of you, can anybody provide me with a reference where the cluster factorization property of the string S-matrix was shown?"

We went through the same problem with respect to vacuum polarization.

At the end of the day, you insisted that Lattice Gauge Theory (the only rigorous (some might say, not yet rigorous enough) definition of an interacting 4D field theory, that currently exists) does not have the requisite "structural properties" for vacuum polarization.

Can you point to a rigorous proof of cluster factorization in a 4D interacting field theory, as a model of a proof that you would consider satisfactory in the string case?

68. **Bert Schroer**
May 7, 2006

Of course, the derivation of cluster properties as a structural consequence on the same conceptual level as TCP, spin&statistics is a consequence of the principles of local causality and positivity of energy. Have done this for the correlation functions the Haag-Ruelle scattering theory carries it to the S-matrix. Prooving it for a concrete model amounts to checking these two properties and this is extremely simple and in all models which have been constructed in any mathematical rogorous sense and in those

models which only have a Lagrangian name but are not yet mathematically born it is guarantied by the fact that causal locality and positive energy-momentum spectrum are formal properties of the Larangian quantization setting which become rigorous as soon as you are able to fill this Lagrangian name with a mathematical content. It is of course true in every order of perturbation theory.
In string theory the conceptual position of the S-Matrix is totally different; instead of being the crown of an underlying theory it is the result of a yet mathematically (outside that genus perturbation theory) not secured cooking recipe which comes from a theoretical blue yonder attempt and therefore there is really something to prove here. But in my question I would be satisfied with an answer in lowest nontrivial order in fact I would already be satisfied if one can see that the 4-particle amplitude (the one which comes from that two tubes joining and splitting) goes to zero if you remove the center of wave packets to infinity in spacelike direction (but of course without any additional approximation of the amplitude). I am not so familiar with the analytical form of these amplitudes, otherwise I would have done it myself.
The important point is that whereas in QFT you get it for free (from other well-known properties), in string theory you have to prove something (asymptotic properties of presumably quite complicated function). I would find it very strange if this has not been done in G-S-W or Polchinski's book.
Since I would except perturbation theory, there is no reason to take refuge to a lattice.

69. **Mentos**
May 7, 2006

You may find the discussion in Polchinski's book (for the bosonic string) satisfactory.

In general, there is an intricate mapping between properties of the 2-dimensional world sheet theory and analytic structure of the spacetime S-matrix.

(A word of caution, since in your question, you seem to blur the distinction between S-matrix elements and off-shell Green's functions: conventional string perturbation theory does not attempt to produce expressions for the latter. Only S-matrix elements are defined, but the poles and cuts of the perturbative S-matrix do satisfy all the usual axioms – something that was not guaranteed in a quantum theory of gravity.)

70. **Bert Schroer**
May 8, 2006

Sorry for the several typos in my posting from yesterday, but since I think that they did not impede the understanding of the content.
Yesterday I left my cottage in Arraial do Cabo (160 km north of Rio de Janeiro, lovely micro climate caused by a passing cold Patagonian current) and went to the nearby Cabo Frio to interrupt my temporary self-chosen solitude. I went to a Chorinho Club. Chorinho is probably the result of the oldest musical encounter between African and European traditions. It is approximately of the same age as as ragtime and New Orleans jazz. The melodic lines (guitar, flute or clarinet) are rich and complex and they are all written down (like ragtime) and hard to improvise on. Everybody in the older generation knows them. The freedom of improvisation is mainly on the side of the rythm which is created by a pandeiro and a cavaquinho. There were many old couples and some "coroas", widows or divorcees of African (probably the strongest ethnic component around Rio), Arabian and European descend. I did not know that Brazil received so many immigrants from the time when the Otoman empire fell apart. The coroas had extravagant very light clothes with a lot of gold rings around their arms, ornamental belts and impressive trappings around their neck. Their vibrant way of dancing encapsulated memories of past beauty and art de vivre. It reminded me of Wim Wenders social club.
I always have been attracted by the originality and richness of Brazilian popular music inasmuch as I liked jazz (when I was at Princeton in the early 60ies I went down to East Village several times to listen to Art Blakey and many other jazzists. While in Pittsburgh I once met a Brazilian au pair girl who aquainted me with the music of Chico Buarque de Hollanda, Gilberto Gil and other Brazilian composers. So when I finally went down there for the first time in 1968 at the invitation of Jorge Andre Swieca, I was not totally unprepared about Brazilean popular culture. The country was in the grip of a military dictatorship but the popular culture was flourishing. Several compositions of Chico were banned by the military and Gil and Caetano Veloso had to flee to London. The censureship was quite severe and many concert and theatre productions were forbidden. This often had the effect of refining the style (one had to avoid addressing problems directly) and significantly enhanced the artistic value (with all apologies to the aficionados of political correctness), an effect which my colleagues from the former Soviet Union probably also experienced. I perfectly understood why Feynman during his more than on year stay in Rio de Janeiro was so much taken in that he did not only become a honorary member in one of Rio's famous Samba school, but he was an activel participant of the batucada section (he played the frigideira a small frying-pan-shaped piece of metal which requires a very high playing speed).
When I came home I had to listen to some Brazilian form of hiphop because the young guys park their ghettoblaster cars near my living place. What a difference to what I had heard before and the high quality popular music which used to be deeply appreciated by all strata of Brazilean society (remember the maid in Pittsburgh I mentioned before)! Brazilian hiphop (at least the one I have heard) is primitive on the musical side and usually pornographic in its lyrics. How can such a rich popular culture fall into such gaping cultural hole within only two generations?
But isn't the history of particle physics physics very similar? Did't we hit the rock bottom in past years? Take a central issue as scattering theory. The S-matrix wasn't the result of a prescription, but the proud crown resulting from underlying spacetime properties (the LSZ-Haag-Ruelle scattering theory). Because of this you did not have to worry about properties which you may have forgotten to impose. But look at what string theory made of this once proud object. If you want to know the counterpart of the Brazilian hiphop, well it is Kakuism, and I am not sure if even a car-dealer would like this kind of hiphop.
Mentos,
I really meant the on-shell S-matrix and not some off-shell extrapolation. It is precisely for that reason that LSZ and HR had to work on top of the (already at that time known) cluster properties of correlation functions.

71. **Juan R.**
May 8, 2006

Bert Schroer,

Of course, that one can be much more specific in criticizing string theory, but in my personal opinion it is a waste of time, because most of stringers are so arrogant that do not even heard or read their colleagues. This is the main reason of failure ot string theory as working theory.

I find amazing the history of the subject, with almost all of past stringers' claims recently proven to be false or considered today to be not so obvious.

From a historical point of view I find history of the variation of the number of dimension or the recent formulation of M(atrix) theory as a theory of pointlike particles two funny points. Maybe the biggest historical error is, in my opinion, the generalized belief between string theorists that a perturbative theory with a spin-two quantum field was a theory of quantum gravity. It was not, therein the need for the unknown M-theory.

Regarding your S-matrix question i never saw a full proof. In fact, would it exists?

The cluster decomposition principle is a hint of QFT (has anyone measured particles infinitely separated?) due to non-existence of full bounded states (only free fields are well-defined).

String theory borns from particle physics, therefore, it copies most of concepts and methods. However, string theory cannot ignore gravitatory effects (as QFT does) and in the large scale, you may include several effects as a cosmological event horizon or the dynamical expansion of universe. Standard string theory (e.g. bosonic version cited above) deals with a classical fixed predefined infinite background. In the generic case there is not factorization and the full string theory scattering perturbative approach breaks down.

Then what? A Hawking $-matrix like approach? I doubt.

The problem with QFT and string theory is that both admit S-matrix as the fundamental observable. Original criticism by Dirac or Landau whereas ignored by mainstream continues holding.

Juan R.

Center for CANONICAL |SCIENCE)

72. **Bert Schroer**
May 8, 2006

Juan R.
Objection your honor, the S-matrix is not a fundamental (but extremely) object of QFT, rather it is the crown/roof of an edifice which is founded on localization and stability principles. The fact that with more recent insights (coming from modular localization theory) the S-matrix also reveals some information about wedge-localization does not really change its role as the honey glaze on top of the cake.
QFT also cannot deliver an intrinsic distinction between bound and elementary particle states; it rather implements full nuclear democracy being only moderated by a hierarchy between fundamental and fused superselected charges (and the cluster factorization property takes this nuclear democracy fully into account). A particular model may invite you to think of some particle state as being a bound version involving other ones, there is nothing wrong with this. But in many cases you find another description which shows that what you considered as elementary permits also a composite interpretation and there is nothing wrong with this either; this is just the consequence of the ubiquitious vacuum polarization. Only in QM such a distiction has an intrinsic meaning. Many of the things which appear intrinsic are not, e.g. the gauge theoretical setting to characterize a theory.
The S-matrix setting is valid precisely as long as the Wigner particle picture is relevant and of course in CST it is not applicable. For this reason Fredenhagen and Haag have based their derivation of Hawking radiation for a collapsing star on registration rates of radiation-counters (they define what this means) and not on Wigner particles (see also Walds comments in his reviews of this issue).

73. **Bert Schroer**
May 8, 2006

Sorry in the previous post it should read: extremely useful, and in one of my earlier posts the incomplete phrase
"but since I think that they did not impede the understanding of the content"
should be completed by: I did not bother to correct them.

74. **Juan R.**
May 10, 2006

Bert Schroer,

I know experimental success of field theory and also some of attempts to provide a more solid foundation to the whole issue.

I was saying is that QFT (and standard string theory) assumes that S-matrix is a fundamental observable. This cannot be true and reason for improvement from several schools and authors.

The theorems on localization, stability et cetera are not convincing when one checks mathematical details. This is reason that people as Dirac, Landau, Wheeler, or Feynman (I am citing only great past guys) knew of those limitations and took QFT as a first step in the formulation of a complete and consistent relativistic quantum theory.

They failed in their respective attempts but at least recognized the problem. Usual QFT textbooks simply ignore the difficulties with QFT, with others -such as recent Weinberg manual- going beyond and attempting to present us the discipline in an "axiomatic way" as if QFT were a well-founded subject!

Most of supposed rigor of QFT breaks down when one studies details of the formalism. For example, it is not difficult prove that a quantum field does not fit inside a pure Hilbert space structure and one may go beyond usual framework, for example via a Rigged structure [Phys. Rev. A 1996, 53(6), 4075]. This is especially true when one try to obtain rigorous descriptions of instable particles, or of non-equilibrium thermal states between others. Another example is thermal phenomena. The irrelevant attempts to study thermal phenomena using QFT showed the lack of a rigorous and full foundation for standard thermal QFT and finalized in a new approach TFD from the particle physics community and alternative (more solid still) approaches from the statistical mechanics community (there are some attempts to improve TFD from ideas developed in the latter). Maybe I would remember here that already the original TFD is based in at least 5 new postulates modifying the most basic of QFT: Hilbert space, state vectors, Hamiltonian, operators, etc.

Another difficulty with the S-matrix approach is that derivation of master formulae is not mathematical or physically founded. I wrote a paper just on this topic has been submitted and accepted. The fundamental idea is that if Hamiltonian unitarity is true, then there is not possibility for a rigorous one-to-one mapping to experimental data (master formula) and if you map to experimental data then unitarity is not true. For illustration, I checked "derivation" of S-matrix in Weinberg manual (1st volume) and showed it contains at least three mathematical errors and two external assumptions (of course Weinberg received a draft copy of manuscript ;-). Van Kampen called "mathematical funambulism" to derivations of that kind! That is true.

When remarking QFT is a free field theory I (as others) did mean that exists not full satisfactory relativistic theory for bounded states. That is the reason that only asymptotic states can be rigorously studied with no possibility for studying intermediate states [Phys. Rev. A 1996, 53(6), 4075].

You can -at least formally- write a Schrodinger-like equations for a single electron but you cannot write a full equation for a two electron system and people use partial mixed approaches such as a double Dirac equation with non-field 16 component spinors and interaction potentials derived from QED but complemented with ad hoc procedures before implementation in bound states (continuum dissolution problem, and rest of trouble)…

It is true that QFT lets us to study some properties of bound states via formal scattering processes |12> —> |1>|2> but that is very different from deriving properties for the real bound state |12>. Dirac was very clear in the incompatibility of QFT with QM in his last works.

The vacuum polarization is just a mathematical artifact arising in QFT when one does *certain assumptions*. Already Feynman explained in his book on statistical mechanics why the usual methodology in QFT and atomic/nuclear QM was not rigorous and one would see what happens when one reintroduces the effects of taking the response of the rest of universe in the model. The picture is then very different (see his famous article with Wheeler on the so-called Universe response theory).

One of the interesting point you are missing is that one can obtain same kind of effects usually ascribed to a "vacuum polarization" in field theory without vacuum. Advantages of this new approach are a more solid foundation and that one can also compute another things cannot be computed from a field-theoretic approach [Rev. Mod. Phys. 1995, 67(1), 113].

And all those without including well-known inconsistencies of CED are not solved by QED. In fact, modern textbooks carefully avoid the topic.

Wald and others' thoughts regarding particles in curved spacetimes are not concluding (and some thoughts are simply wrong). See above reference for remarks on a theory of particles in curved spacetimes and how it has been applied to description of cosmological models without initial singularities.

There is more interesting stuff to be discussed here, but sincerely I have no time.

Juan R.

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Quick Links

May 7, 2006
Categories: Uncategorized

I've been much too busy the past few days, so haven't had time to write anything new here. One thing that has been keeping me busy is going over the copy-edited version of the American edition of my book, which will be published in September by Basic Books. The British edition, published by Jonathan Cape, should be available June 1, both in Britain and Canada, and presumably one can order it from the British or Canadian versions of Amazon. The American version will have a somewhat different preface, and has been separately copy-edited, so there will be minor changes (beyond just changing British spellings back to the American ones I first wrote down…). Late last week I was sent an early copy of the British version of the book itself, and I'm very happy with how it looks.

Last week I also spent a significant amount of time at my colleague John Morgan's 60th birthday conference, which was held here in the math department. Morgan is one of the leading figures in topology, and over the years has worked on a wide range of different kinds of mathematics, often bringing the subject together with other very different parts of mathematics. At the moment he's involved in at least two projects, one involving Calabi-Yaus with Chuck Doran, another an ambitious attempt with Gang Tian to work out the details of Perelman's proof of the Poincare conjecture. He's also doing a stellar job as chair of our department.

Morgan has collaborated with and interacted significantly with Witten over the years, and Witten gave a wonderful talk at the conference on Gauge Theory and the Geometric Langlands Program. This was really just a taste aimed at mathematicians of his recent work on geometric Langlands and gauge theory. He explained some of the history of Montonen-Olive duality, some of the relevance of supersymmetry to mathematics, and then explained what an 't Hooft operator in gauge theory is, and that it is related to the Hecke operators studied in geometric Langlands.

Here are some quick links to interesting things I've run across recently:

John Baez has a new edition of his proto-blog "This Week's Finds in Mathematical Physics". It contains a beautiful exposition of the circle of very different sorts of mathematics that all gets related via Dynkin diagrams.

Greg Moore and his recently graduated student Dmitriy Belov had a beautiful new paper on self-dual field theory in 4l+2 dimensions.

Christianity Today has an article entitled Science in Wonderland, which mentions Susskind and string theory and notes:

This theory has not met with, shall we say, universal approbation, not least because it can't be empirically tested. You could even say it's not science, and some have said that, but they don't hiss the way they do when they talk about Intelligent Design.

The AMS has a new web-site devoted to Mathematical Imagery.

In the comment section here, Bert Schroer pointed to some web-sites I wasn't aware of that contain all sorts of links to various material related to the algebraic approach to QFT. These include the home page of Stephen Summers and the Local Quantum Physics Crossroads" hosted at Gottingen.

Update: If you want to know why the mathematics associated with Dynkin diagrams can't be usefully explained or viscerally understood without string theory, as well as why John Baez is a proto-human, you can consult the blog of a prominent Harvard faculty member. He also notes that

Peter Woit is another proto-human who eats everyone who dares to look in between the clouds. Be afraid. Be very afraid.

and explains in great detail why

it is important not only to learn string theory well but also to emphasize that and explain why people like Peter Woit are intellectual barbarian cannibals. 😉

Comments

1. **Deane**
   May 7, 2006

   Peter,

   Did you notice this:

   http://www.intlpress.com/AJM/p/2006/10_2/AJM-10-2-165-498-Abstract.pdf

   Zhu spent last year at Harvard working through all of this in a weekly seminar run by Yau.

   Regards,
   Deane

2. **knotted string**
   May 7, 2006

   June 1 is a Thursday, the traditional book publishing day in England. Is Jonathan Cape making a real marketing effort for NOT EVEN WRONG?

   Can you give anti-stringy interviews on TV when the book comes out? Or is it just going to be a case it going quietly on to bookshelves, largely unnoticed. I suppose you should have an agent for a book for publicity bearing in mind it isn't a textbook.

   The girls who do the marketing just post off a few complimentary copies of the book to magazines, who rarely review them.

   Really you should be granted some interest from the media on this. What you're doing is defending the right to argue that the mainstream is headed down a blind alley. Any string theorist who takes offense or makes a show of 'getting angry' needs to grow up.

   Is there going to be any publishing party in London or New York? Invite your fellow Princeton student Brooke Shields along, that will help attract the media. Seriously, I hope you are not going to be a loser with this book. (You face the risk of getting a bad reputation if it doesn't sell, which may stop you publishing any other book.)

   The basic facts are free on here on the internet. Hence the book isn't something to be guilty about, it's democracy in action – in the bookstore. The expert case against extra dimensional speculation must be studied carefully by string fanatics, it can't be ignored.

3. **Thomas Love**
May 7, 2006

I don't know about the accuracy of that Christianity Today report.
I hiss about String Theory in the same Tones I use for "Intelligent Design".

4. **MathPhys**
May 7, 2006

Every time I glance at Greg Moore's most recent paper, I seem to think that he works in 41 space and 2 time dimensions.

5. **woit**
May 7, 2006

Deane,

Thanks for pointing that out, I had heard rumors about it. Do you know if it has been refereed, and if it is now publicly available.

6. **Richard**
May 7, 2006

What is the current status of Perelman's proof of the Poincaré conjecture? Is he essentially, at this point, leaving it to others to complete his work?

7. **woit**
May 7, 2006

Richard,

As far as I know, Perelman has moved on to other things and seems to have no intention of writing down the details of a proof. The Cao-Zhu work Deane mentions and the Morgan-Tian work underway are two projects I'm aware of to write out a detailed proof.

At this point, I think most people believe that Perelman's outline of a proof can be turned into a real proof, since no serious problems in doing this are known to have turned up. Once one or more detailed proofs have appeared in print, and experts have had a chance to examine these carefully, then it should be well-accepted that the Poincare conjecture has been proved.

And then the Clay Foundation can start trying to figure out who if anyone get the million bucks. Supposedly Perelman doesn't really want it….

8. **Richard**
May 7, 2006

Peter,

I suspected that he may have gotten bored and moved on to other things. Still … how can he leave his baby to others to raise? How could we have produced someone with this brilliance who apparently does not apparently understand the craft (and satisfaction) of bringing theory to complete and completely rigorous presentation and fruition? None of us will always do the latter with brilliance, but to not even try is [fill in the blanks].

9. **Deane**
May 7, 2006

Peter,

As far as I know, the full Cao-Zhu paper will only be available when it is published. I have not seen any preprint. I don't know anything about how it was refereed. I was up at Harvard last week and did talk to Yau about it. Yau now also believes that the Hamilton-Perelman program does yield a complete proof of Thurston's conjecture. Yau says that not only does the Cao-Zhu paper fill in all the details of the program, it also fixes errors that he says are in Perelman's work.

Deane

10. **csrster**
May 8, 2006

http://www.amazon.co.uk/exec/obidos/ASIN/0224076051/

11. **SomeBody**
May 8, 2006

Richard, it is not unreasonable for somebody truly brilliant to consider it more interesting to look for big new ideas, point out overall directions and then leave it to the so-called rank and file to work out the details while they move on. It's just efficient distribution of labor. Perhaps if there were more such people and a little fewer navel gazing nitpickers in physics we wouldn't be stuck you-know-where…

12. **anon**
May 8, 2006

What Oh! No Lubos review yet at amazon uk of Not Even Wrong? C'est pas possible. I think Lubos must be getting old. His response times are slowing down.

13. **sunderpeeche**
May 8, 2006

If the book is not yet on public sale (~June 1) and you do not have an advance copy, how do you review the book? Chris Oakley evidently received an advance copy, and reviewed it, and his review is available for all to read.

FWIW, in French the "ne" is mandatory to indicate negation. "Ce n'est pas possible", "Ce n'est rien", "Je n'ai jamais visite' Paris" …..

14. **Chris Oakley**

May 8, 2006

Well actually, Sunderpeeche, my review was based on a January 2005 copy of the manuscript that Peter gave me when I visited him at Columbia (my visit being funded by the Illuminati and the Freemasons as part of their campaign to destroy Superstring theory). He may have completely re-written the book since, but if so, he never told me.

15. **Chris Oakley**
May 8, 2006

    I should add that when I last saw the Grand Imperial Wizard of the Illuminati and Senior Master JCB Driver of the Freemasons, they were well pleased with Lubos. They noted that the number of hits on Peter's weblog always goes through the roof when he posts comments here, and that most people would not even be aware of the issue but for his invective.
    Personally, I kind of like him. Having had a lot of experience of mealy-mouthed academics with zero moral courage I find it quite refreshing to discover someone so apparently unconstrained by the normal rules of the game. I am sure that they will eventually take steps to shut him up, but I think that in the meantime, we should all just enjoy it.

16. **Deane**
May 8, 2006

    …"FWIW, in French the "ne" is mandatory to indicate negation. "Ce n'est pas possible", "Ce n'est rien", "Je n'ai jamais visite' Paris" ….."

    In proper written French, yes. However, in French spoken by the natives, it is practically impossible to hear the "n'". Some of us suspect it's not even pronounced by the natives and therefore used to detect foreigners in their midst. The extreme example is "Je ne said pas", which the natives pronounce as "chez pas" (at least to my non-French ears). So "C'est pas possible" may look incorrect, but it sounds correct.

17. **Who**
May 8, 2006

    anon: C'est pas possible.

    I believe this is ordinary vernacular. anon sounds like he knows idiomatic French.
    sunderpeeche correction "FWIW, in French the "ne" is mandatory to indicate negation. "Ce n'est pas possible", "Ce n'est rien", "Je n'ai jamais visite' Paris" ….." is textbook correct. but I think in French streets or movies they don't always talk like that.

    Chris, it takes courage to be a mealy-mouthed academic. And screw the Grand Imperial Wizard of the Illuminati.

18. **sunderpeeche**
May 8, 2006

    If we're going to get into the business of idiomatic French, I suggest you drop the French altogether and go stand in the middle of Times Square NYC and listen to the Queen's English and submit it in your next essay in English Lit class. See what happens.

19. **me**
May 8, 2006

    > If the book is not yet on public sale and you do not have an advance copy, how do you review the book?

    In the past Lubos had no problem reviewing books he did not read.

20. **sunderpeeche**
May 8, 2006

    Let's not be stupid about this. Amazon will exercise some quality control to maintain the integrity of the reviews on its web page. If a review is written (without reading the book) and posted on a blog, then that's something else. But Amazon needs to turn a profit, hence it needs to be credible, hence it cannot (overall, anyway) allow nonsense.

21. **John A**
May 8, 2006

    I think you should know Peter, that I don't agree with Lubos describing you and John Baez as "proto-human". I think its beneath contempt to use such language between two very intelligent people, and I've said so on his blog. Lubos goes on to talk about the "explanatory power of string theory" to explain the linkages between concepts in physics but nowhere does he establish uniqueness of string theory as the only way those linkages can be made.

    To describe string theory as a "proto-science" is nothing more than a statement about where string theory is in relation to experimental physics. It might predict an unambiguous experimental result one day and then you'll have to change the name of your blog.

    I'd have to say that I wait for that day before I commit mental energy to the string hypothesis. Experimental falsifiability is a key concept for me. Mathematical consistency is not enough.

22. **JC**
May 8, 2006

    I wonder what would happen if Harvard decides to award Lubos with a tenured professor job. 😉

23. **Lord**
May 8, 2006

    String theory may not be science, but it would still be math, which is a lot more than one can say for ID.

24. **Ryan**
May 8, 2006

    sunderpeeche… Apparently you have never seen [David Hasselhoff – The Very Best Of](#).

25. **woit**
May 8, 2006

Sunderpeeche,

I haven't looked at many Amazon reviews, but there are some pretty weird things there. They do seem to have a policy of mostly deleting one-star reviews. Lubos is well aware of this (he's been involved in a campaign to get one-start reviews of a crackpot physics book allowed), so when he wants to attack something he gives it two stars. Check out his review of Lawrence Krauss's recent book that was critical of string theory. I believe it was the first one posted.

String theory doesn't make any predictions, but I can make one: Lubos will be among the first reviewers of my book on Amazon, and I'll get two stars.

John A. and Lord,

No, string theory on the whole is not math and it's not mathematically consistent. Some work on string theory has led to important new ideas about mathematics, but those parts of string theory that try and connect up to the real world generally haven't, and much of them is not even mathematically consistent. The "Landscape" is even less mathematics than it is physics.

26. **Chris W.**
May 8, 2006

Learned and leisurely hospitality is the only antidote to the stance of deadly cleverness that is acquired in the professional pursuit of objectively secured knowledge. I remain certain that the quest for truth cannot thrive outside the nourishment of mutual trust flowering into a commitment to friendship.

— [Ivan Illich]

27. **anon**
May 8, 2006

Lubos seems to have edited the mean personal comments a bit:

"A mathematician who thinks about the ADE classification without string theory is like a proto-human who knows something about the summer and the winter but who can't understand why the seasons change because he does not want to look in the skies and see the motion of the Sun. In this metaphor, an anti-string-theory blogger is another proto-human who eats everyone who dares to look in between the clouds. Be afraid. Be very afraid. :-)" [http://motls.blogspot.com/2006/05/ade-classification-mckay.html]

So "Peter Woit" has been edited to "an anti-string-theory blogger". 😉

28. **woit**
May 8, 2006

Glad to see that Lubos doesn't believe in personal attacks. I wonder what "anti-string-theory blogger" he has in mind?

29. **Michael**
May 8, 2006

"I've been much too busy the past few days"

Publishing too much these days, I guess!? 😆

30. **woit**
May 8, 2006

Question of the day: Are all string theory partisans moronic assholes, or it's just the ones who post things on the internet?

At least one of them, Michael the anonymous coward from suburban Boston never seems to tire of this.

31. **Michael**
May 8, 2006

Better an asshole than a proto-human… 😆

I live near Needham, Massachusetts, know lots more about string theory than stupid people like you, and Lubos Motl is my hero. I think he's almost as cool as Spiderman.

32. **Michael**
May 8, 2006

"I live near Needham, Massachusetts"
not quite, but my ISP does. 😉

"know lots more about string theory than stupid people like you"
That's for sure.

"and Lubos Motl is my hero. I think he's almost as cool as Spiderman."
Spiderman is *way* cooler!

33. **Arun**
May 8, 2006

If string theory had even one connection to physical reality, it wouldn't be necessary for Motl to tear down Woit at every opportunity.

34. **sunderpeeche**
May 8, 2006

I notice that many of these posts degenerate into idiotic polemics to/from/about Lubos, whether written in idiomatically correct French or otherwise. It's just foolish. (I looked up Motl's website about twice, decided it was rubbish, and haven't been back. Proto-human? I wouldn't even have known if the matter had not been repeated on this blog.)

Amazon/David Hasselhoff? No, I hadn't seen it before. Amazon has to cater to many tastes, and I expect those who like DH will like those reviews. Lawrence Krauss book? I saw reviews by Tom Appelquist and PW, in addition to LM. It's impossible to tell if LM (or anyone else) actually read the book. I do not doubt (and I do not care) if LM publishes a bad review of NEW. But to try and publish a review BEFORE the book goes on sale, and without an advance copy, that would raise a red flag.

I guess nobody has anything to say about John Morgan. As for myself, I've never heard of him. John Baez/TWF has some nice stuff, and the AMS Mathematical Imagery is also worth further attention. Such is the life I choose to lead. You'll forgive me (or not, as the case may be) if I have no interest in nonsense.

35. **David**
May 8, 2006

Peter,
Please don't let the level of discourse on this blog drop. The distinguished faculty member suggested that I can't tell an electron from a dog when I tried to suggest that some biology articles are worth reading even if they might not support your favorite newspaper report about global warming or your view on how climate science should be done. Thus, I will spend less time over there. Your blog offers interesting and useful discussions of topics I'm interested in. Keep up the good work.
From a proto-proto-human,
David

36. **Thomas Love**
May 9, 2006

It occured to me as I read the above comments that Motl posts a lot. That would indicate that he has nothing better to do, which is proof that there is no substance to string theory. If there were substance to sting theory, he would be working on it rather than posting absurd comments. Back to work.

37. **secret milkshake**
May 9, 2006

Lubos likes to spread visceral understandig by disembowling his oponents.

38. **You**
May 9, 2006

I bet the last five comments were from the same person.

39. **woit**
May 9, 2006

You,

How much do you want to bet? If it's enough to make it worthwhile, I'll put you in touch with all five of them so you can write them a check. One of them is using his full, real name and I've corresponded with him via e-mail. The others have valid, different e-mail addresses, and are accessing the blog from very different locations.

That's the story of the last few Lubos critics. Lubos supporters like "You" seem to prefer anonymity like "Michael", who didn't leave an e-mail address, but splits his time between his home in suburban Boston, the HEP group at Brandeis, and the HEP group at UMass Amherst. Probably anyone who cared could figure out who he is.

40. **Who**
May 12, 2006

we didn't yet get any comment on the paper that Peter pointed to by Freidel Minic Leigh about 4D Yang Mills
http://arxiv.org/abs/hep-th/0604184
Towards a solution of pure Yang-Mills theory in 3+1 dimensions
someone mentioned possible relevance to the megadollar Clay Prize
so far on this thread we are getting mostly technical discussion of assholes by Michael and others whether they are better or worse than proto-protos etc. and about the Harvard Ranter and so on. this is OK but it might also be interesting if someone explained the interest of Freidel Minic Leigh if any.

Simons Donation to Stony Brook

May 9, 2006
Categories: Uncategorized

Stony Brook [announced yesterday] that Jim Simons will be making a $25 million dollar donation to the university, focused in the area of mathematics and physics. This is a great deal of money for a math or physics department, and it is the largest single cash donation ever made to any of the SUNY institutions.

Simons was responsible for building up the Stony Brook math department, which he joined as chair in 1968. About his hopes for what his donation will do, he says:

During the past thirty years mathematics and physics have grown increasingly intertwined. This is particularly true in the cases of string theory, quantum field theory and cosmology, which have all depended upon and stimulated advanced work in geometry and topology. Buttressed by its close relationship with Brookhaven National Laboratory and building on a fine faculty already in place we believe our gift can help propel Stony Brook into the very top rank in these central fields.

Comments

1. **sunderpeeche**
   May 9, 2006

   I read this in the newspaper yesterday. Good for Simons. I have no millions to donate. If he has a vision that it will help propel SUNY-SB to the top in string theory, QFT and cosmology (and links between physics + mathematics), and that doesn't suit the tastes of anyone else at this blog, no matter.

   Fiat Lux.

2. **Who**
   May 9, 2006

   partisan gloating aside, that kind of private support for mathematics and physics is thrilling.
   I saw Simons out here at the dedication of the Chern auditorium and MSRI expansion, some of which he funded. His brief remarks ahead of the featured talk by Penrose were modest and genuinely funny.
   Never saw a smarter or more gracious donor.

3. **was just wondering**
   May 9, 2006

   is this the Simons from Chern + Simons

   http://en.wikipedia.org/wiki/James_Harris_Simons

4. **woit**
   May 9, 2006

   Yes. I should have added an explanatory link to some of the other postings here about Simons, such as

   http://www.math.columbia.edu/~woit/wordpress/?p=299

   And I should also point out that I think it is quite reasonable for Simons to list string theory as a topic that has brought together mathematics and physics. It certainly has done that, to good effect.

   By the way, I was a postdoc at the Institute for Theoretical Physics at Stony Brook for three years (back when Yang was the director, and it wasn't yet named after him). I enjoyed interacting with the people there and with many of the people in the math department. My best wishes to them all in figuring out what to do with the new resources that Simons is providing.

5. **Bert Schroer**
   May 9, 2006

   Peter,
   I think you mean "physicists" and not physics, unless Lubos has convinced you that string theory is physics.

6. **woit**
   May 9, 2006

   Bert,

   I did really have in mind that string theory has brought together mathematicians and physicists, but I don't think the way that I expressed this is really inaccurate. "String theory", "mathematics" and "physics" are all not very well-defined terms…

7. **sunderpeeche**
   May 9, 2006

   Once again the silliness about Lubos … why not stick to the good that Simons is doing with his money? We shall see if things spiral downhill from here…..

8. **Bert Schroer**
   May 9, 2006

   To Peter,
   objection you honor, these days physics and what physicists are doing is not the same.

9. **sunderpeeche**
   May 9, 2006

Ach du lieber.

Pardon the non-idiomatic non-French.

10. **Bert Schroer**
May 9, 2006

The expression “that what physicists are doing” instead of physics is not my invention. It was used by Hirzebruch when he was talking about the Atiyah-Witten ideas and I some of my colleagues found this impressingly careful and farsighted. In fact it has entered my way of looking at things so profoundly that I will always remember it.

11. **Simon**
May 9, 2006

I love the fact that Simons used his considerable math skills to make lots of money to put back into math and phys.
My only worry is that this might encourage governments to rely even more on private funding for fundamental research

12. **sunderpeeche**
May 9, 2006

Govt funding for research is mainly a post-WW2 thing. Millikan for example had to get private funding for Caltech. The various large telescopes (Yerkes, Hale etc) were all privately funded. The Manhattan Project changed all that. Physicists (nuclear phycists anyway) were perceived as good for making bombs (also ICBMs). The sociology may not be as simple as that, but anyway, govt funding for science is not some long-standing tradition. Funding for fundamental research has traditionally been difficult.

13. **was just wondering**
May 10, 2006

maybe it’s time that we define what ‘physics’ means.

Dibner Institute Closes

May 9, 2006
Categories: Uncategorized

The Dibner Institute and Burndy Library at MIT will soon be closing, with the Burndy collection moving to the Huntington Library in California near Caltech. The Dibner Institute is devoted to research in the history of science and technology, and I mentioned it a couple years ago here. Among the interesting things the Dibner has on-line are copies of lecture notes on quantum electrodynamics from Freeman Dyson in 1951 and Fritz Rohrlich in 1953.

Comments

1. **D R Lunsford**
   May 9, 2006

   The notes by Rohrlich are priceless! What a find!

   -drl

2. **Bert Schroer**
   May 9, 2006

   Is Fritz Rohrlich still alive? Did he invite Jauch, who probably was in Geneva during the war, to come over and collaborate with him? Why did both change the area of interests years after having written that influential textbook? My main association with the two names is related to the book.

3. **Brett**
   May 9, 2006

   I'm sorry to see the Dibner Institute disappear. They brought in interesting people to talk about the history of science, and I enjoyed occasionally attending the lectures. However, I am not too surprised that this has happened. Although I naturally never knew anything about the Institute's finances, I always had the impression that they were probably overspending themselves, on publicity and other things.

4. **D R Lunsford**
   May 9, 2006

   Yes, he's alive, at least as of November when he told me to (paraphrasing) "get some experimental backup or it's just speculation!" 🙂

   He's one of a vanishing breed, no doubt.. "Classical Charged Particles" was – is still – a great read. So was "Theory of Photons and Electrons", one of the best QED books ever..

   -drl

5. **Alejandro Rivero**
   May 10, 2006

   Is the website going down?

6. **woit**
   May 10, 2006

   No idea, you'd have to contact them to find out what their plans are.

7. **Alejandro Rivero**
   May 10, 2006

   Hmm I tried but the email address for ashrafi@mit.edu fails, so the website is already obsolete…

   The interviews in the website are also of some value, and interesting to read. Visitors can enjoy a remark from KG Wilson:

   The U.S. had a period in the 1960s when it was a buyer's market for positions in academe, and so a lot of people had this kind of opportunity. But at the same time, you look at how people struggled at the time of Kepler, at the kind of struggles he went through in order to be able to continue for twenty years, and it's clear there are many more people who, if they're willing to engage in the kind of struggle that Kepler did, can do it and not starve. A lot of people complain today that the conditions are tight, you have to toe the line and everything, but the people who are like Kepler are going just as stubborn today as Kepler was, as far as I can see.

Discovering the Quantum Universe

May 9, 2006
Categories: Uncategorized

HEPAP today released a new publication designed to convey to the general public excitement about prospects for particle physics in the coming years. It's entitled Discovering the Quantum Universe, and it has a companion web-site. Both the web-site and the document itself are beautiful and impressive productions. The web-site also contains the earlier 2004 HEPAP report Quantum Universe, which was mentioned in one of the earliest posts of this blog. The newer document is based on an earlier version from last summer, and one of its main goals is to make the case for a linear collider. In some sense this is promotional material for the conclusions recently reached by the EPP2010 panel. Also part of the promotional activity today is a briefing for members of Congress that will include a talk by my colleague Brian Greene.

While I hope that this all has the intended effect of getting the public, the Congress and the Administration excited about particle physics and willing to support it at the level necessary to fund a new generation of machines and experiments, as you might guess I have my doubts about the wisdom of some of the material included in this report. Unlike the EPP2010 report, which oversold string theory a bit, this report oversells it a lot, with language like:

…preliminary studies have looked at the ability of linear collider experiments to detect the telltale harmonies of strings. Here linear collider precision is essential, since the string effects appear as small differences in the extrapolated values of the superpartner parameters. A combined analysis of simulated LHC and ILC data shows it may be possible to match the fundamental parameters of the underlying string vibrations.

The inclusion of this kind of language seems to me to be misleading and irresponsible. Ten years from now when we have real LHC data, know that the ILC can't tell us anything about string theory, and are asking the US government to put up large sums to finish the ILC, we'll have to hope that the relevant decision makers didn't get convinced by this report that the ILC is a machine designed to get information about string theory.

Update: More about this at Cocktail Party Physics.

Comments

1. **Chris Oakley**
   May 9, 2006

   I agree. How about re-wording it like this:

   …preliminary studies have looked at the ability of linear collider experiments to detect particle interactions that probably have nothing to do with the telltale harmonies of strings. Here linear collider precision is essential, in spite of the fact that string effects somebody probably once guessed may appear as small differences in the extrapolated values of the superpartner parameters, have nothing to do with it. A combined analysis of simulated LHC and ILC data shows it may be possible to advance understanding of the underlying physics even though there are probably no underlying string vibrations with which we may match the fundamental parameters.

2. **sunderpeeche**
   May 9, 2006

   Most successful particle accelerators + detectors achieved their fame for reasons having nothing to do with their originally intended mission. The AGS at BNL produced 3 NP-winning expts, none of which were foreseen — J/psi, CP violation, 2 neutrino species.
   Sam Ting's expt was proposed to discover vector mesons to test Vector Meson Dominance, not to discover the charmed quark. While the Bevatron was built to produce the antiproton (which it did, leading to NP), it really became famous for the resonances ("Bump hunting") discovered with the Alvarez bubble chamber. Gargamelle discovered weak neutral currents, which were not even mentioned in the proposal (if I recall correctly from The Second Creation.) The proton decay underground water tanks have turned out to be better suited for neutrino astronomy.

   For that matter Columbus proposed to sail to China, discovered America (Erik the Viking might disagree), claimed to have landed in India, and after all that people still call him a hero.

   PEP, PETRA, TRISTAN were all built to find the top quark (which IS part of the SM), and all failed. This did admittedly lead to subsequent funding difficulties. If indeed LHC/ILC produce new physics beyond SM, the string theory stuff will be forgotten/ignored. The real hope is that the new machines will find *something* never mind why they were proposed.

3. **Bert Schroer**
   May 9, 2006

   But I guess Peter is afraid that too much hype may create a backlash. In all those cited cases they were looking with much less hype for something and they found something else which was at least as interesting. Now they have to find something which has to go beyond all this messianic hype, a pretty difficult task.

4. **sunderpeeche**
   May 9, 2006

   The real backlash problem is that so many machines have been built and have found no new physics beyond SM. If yet another generation of machines fails to produce physics beyond SM, then it will get even harder to justify further machines. At a minimum the next generation machines really MUST find the Higgs. Although the Higgs is part of SM, it will still be a major finding and will serve as a boost to obtain funding for future machines. If LHC/ILC do not find the Higgs, it will get really hard to get more money. (Unfortunately nobody can guarantee where/how the Higgs will appear, just as with the top quark.) String theory or any other theoretical motivation ("hype") is not the critical issue.

5. **woit**
   May 9, 2006

   One different aspect of this situation is that the ILC will not really be exploring a completely new energy range, since the LHC actually will have higher energy. By the time the ILC construction decision is ready to be made, I suspect the LHC data will have given strong indications that there is no supersymmetry/higher dimensions/string theory at energies accessible to the ILC. That's why using these ideas to sell the ILC now may be a mistake.

   Hopefully the LHC will find some new and unexpected physics that explains electroweak symmetry breaking and the ILC will have enough energy to study it. If so, there will be a very good case to be made to build the ILC, but much of this report will have to be explained away.

6. **sunderpeeche**

May 9, 2006

Historically, e+e- colliders have not had the energy to go beyond the mass reach of hadron machines. As powerful as LEP was, the W and Z were first produced in hadron machines. LEP was designed for precision studies of particles of known mass. Even SPEAR, when it produced the psi, the J had been produced at the AGS. If, as one hopes, the LHC produces the Higgs, the ILC will promptly become a Higgs factory. Its design parameters will be adjusted (if necessary) to optimize Higgs production. It will simply be unnecessary to explain away the string theory or any other prior motivation/hype. It is in the nature of scientific discovery that this is so.

7. **woit**
May 9, 2006

Sunderpeeche,

I hope you're right about not needing to explain away the hype. But I personally wouldn't want to be the physicist testifying before Congress in 2012 trying to answer questions from congressmen about why the US should spend billions on something that's not even going to help discover those strings and extra dimensions.

8. **sunderpeeche**
May 9, 2006

If the LHC produces the Higgs, the physicists will testify "the Higgs has been found, the ILC will be a unique precision tool to study it, anything else will be a bonus". It was not so different for LEP, the W and Z had been found, anything else would be a bonus (except that LEP was proposed AFTER the W and Z were found). Unfortunately the design and construction timescales are now so long that ILC planning must start now, before LHC data is in. If LHC finds nothing, ILC will have to be sold as a pure search and discovery machine. This is difficult, and not the fault of ST. PEP, PETRA and TRISTAN were all built to find a particle which really does exist, but the SM gave no clue as to its mass. Physics can be and is unkind.

Really, the difficulty is that in the good old days (those wonderful times before the internet, cell phones and blogs … one hankers for the simple life) each new generation of machines produced new physics discoveries beyond the theories of the day. That has not happened for ~30 years. One simply does not know the mass scale of the new physics. And that is not the fault of ST. No theory — including QED — has given the mass scale of the next generation of particles. The expts must find something, then one can say "these particles are part of a family, search for the next one (like the Omega-)".

9. **Tony Smith**
May 9, 2006

On page 13 of the pdf, "Discovering the Quantum Universe" says:
"… According to our present understanding, the Higgs particle itself should have a mass a trillion times beyond the Terascale. …".
I thought that the Standard Model would be happy with a Higgs in the 110 to 200 GeV range, and that was such a generally accepted view that Fermilab thought that it might have seen the Higgs at around 115 GeV. To what "present understanding" are they referring ?

On page 18 they list the following potential LHC discoveries:
A Higgs particle
Superpartner particles
Evidence for extra dimensions
Missing energy from a weakly interacting heavy particle
Heavy charged particles that appear to be stable
A Z-prime particle, representing a previously unknown force of nature
Superpartner particles matching the predictions of supergravity

If the Standard Model continues to hold up, as it has so far,
so that there are
no superstring-type superpartners,
no superstring-type extra dimensions,
no WIMP,
no heavy charged stable particle,
no Z-prime, and
no supergravity-type superpartners
then
the LHC might see nothing much beyond a 115-200 GeV Higgs,
plus better luminosity of the sort of signals already seen at Fermilab.

Does the USA high energy physics community have a plan for such a contingency ?

If not, is it such a contingency really so remote that there is no need to plan for it ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

10. **Aaron Bergman**
May 10, 2006

Does the USA high energy physics community have a plan for such a contingency ?

Yes. We all give up and go home. Not just the USA, pretty much everyone. People have nightmares about the LHC only seeing a Higgs. Maybe you can justify the ILC to probe the electroweak symmetry breaking sector, but if everything fits a single Higgs doublet and nothing else, I'm not sure I see the point.

11. **Tony Smith**
May 10, 2006

Aaron Bergman said that "… if the LHC only see[s] a Higgs … We all give up and go home …".

That sounds sad to me, when such things as the values of the Standard Model parameters are not explained by conventional models (unless you accept anthropic landscape models as being both conventional and explanatory).

If it is a contingency that is sufficiently likely to cause nightmares, wouldn't it be nice to have a contingency plan other than "give up and go home"? Has much effort been put in trying to develop such a plan, or is that contingency being ignored as being incompatible with a united USA lobby for ILC ?

Tony Smith
http://www.valdosamuseum.org/hamsmith/

12. **Aaron Bergman**
May 10, 2006

Has much effort been put in trying to develop such a plan, or is that contingency being ignored as being incompatible with a united USA lobby for ILC ?

You don't seem to understand. At that point, there's pretty much nothing left to do. You can measure the standard model parameters to a ridiculously high precision, I suppose, and hope that you can get the theory down well enough to look for small discrepancies, but that's not particularly sexy. You're just going to have a damned hard time convincing anyone to spend ten or so billion dollars on an accelerator after the previous one bored us all to tears.

Maybe someone will have some clever idea for accelerating subatomic particles cheaply, but until then, I'd guess the contingency plan for a lot of people would be to start contemplating the Black-Scholes equation.

13. **Thomas Larsson**
May 10, 2006

IIRC, you once guaranteed that there must be new physics at the LHC, because of the Landau pole. So if LHC sees nothing new, except perhaps for a standard Higgs, there is something seriously flawed about our understanding of QFT?

14. **Tony Smith**
May 10, 2006

Aaron Bergman said, replying to me about the contingency in which the LHC only sees a 110 to 200 GeV Higgs, plus events similar to those already seen at Fermilab:
"… You don't seem to understand. At that point, there's pretty much nothing left to do. …".

I can see that might be the case for superstring theoretical physics, which would be invalidated by lack of conventional supersymmetry,
but
is it really futile to search for a non-superstring theory that might explain the parameters of the Standard Model etc,
in which case further detailed study of Fermilab/LHC type data might be useful in distinguishing among such types of models, and in examining their consequences ?

Has high energy theoretical physics become such a monoculture of superstrings that the community feels that it is preferable to "give up and go home" rather than work on non-superstring models ?

From another point of view, if LHC sees only non-supersymmetric Higgs plus similar-to-Fermilab events, thus invalidating superstring theory,
would the USA superstring community (with 90% of USA theoretical hep funding and jobs) prefer to
take their ball and go home (giving up that funding and jobs to non-hep things)
rather than
see non-superstring theoretical hep have a chance to succeed where they failed ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

15. **woit**
May 10, 2006

Thomas,

The argument that you have to have "new physics" at the LHC just is that you have to either see a Higgs or something else new.

16. **Disgusted**
May 10, 2006

It is not only a matter of strings. Selling theoretical speculations in this way, a few years before LHC, seems either useless or suicidal.

More in general, this and other similar presentations look like a show for a big bureaucratical enterprise ("we must discover the Higgs / extra dimensions / …"). In my opinion emphasizing the points made by sunderpeeche (#2) would be not only more honest, but also more attractive.
But I am only a physicist, not an expert of outreach.

17. **Thomas Larsson**
May 10, 2006

Peter, I should have addressed my comment to Aaron, who I think issued this guarantee on some occation. I'm no expert, but I don't think that the Landau pole becomes safe if you only add a Higgs.

18. **anonymous**
May 10, 2006

preliminary studies have looked at the ability of linear collider experiments to detect the telltale harmonies of strings.

!!! A reference here would be interesting. Or at least amusing.

19. **woit**
May 10, 2006

Thomas,

There's a Landau pole problem for non-asymptotically free couplings. It's there for the U(1) gauge coupling, but only occurs at exponentially high energies (not at the TeV scale). All it says is that at short enough distances the effective coupling will get large and perturbation theory will break down.

The Higgs quartic coupling also has this problem, and it is this coupling that determines the mass of the physical Higgs particle. If the Higgs particle is low enough mass, the quartic coupling at the TeV scale is small, and again its blowing up only is a problem of principle at short distances. If you try and push the Higgs mass up in the standard model, you are pushing up this quartic coupling. At some point it gets large and perturbation theory breaks down.

The reason you get a guarantee at the Tev-scale isn't really the Landau pole issue: it's just that either you'll see a Higgs particle (small enough quartic coupling), or whatever is causing the Higgs phenomenon is something inherently non-perturbative and completely new physics.

20. **Aaron Bergman**
May 10, 2006

Has high energy theoretical physics become such a monoculture of superstrings that the community feels that it is preferable to "give up and go home" rather than work on non-superstring models ?

Must you see everything through these silly string wars? This has nothing to do with that. Theorists can go on speculating about whatever they want. It won't matter. If you've been reading what I've been saying, this is about *experiment*.

The reason you have to see something is that otherwise WW scattering (I think) becomes nonunitary. If you believe in unitarity, then something better fix that. A simple doublet Higgs solves that problem, however, and it's perfectly consistent for that to be all there is until the Planck scale.

21. **Eugene Stefanovich**
May 10, 2006

Aaron Bergman said that "… if the LHC only see[s] a Higgs … We all give up and go home …".

I may agree that in these circumstances there will be no compelling reason to build even bigger accelerators. However, I do not agree that theorists should give up. There are quite a few fundamental problems that are not addressed by current theories at all. Some of them were mentioned in Chris Oakley's posts:

1. What about divergences in QFT? Will we ever get them from "under the rug" and honestly examine? Or the current consensus is that divergences are ultimately related to the (currently unknown) Planck-scale physics, and we can do nothing about them?

2. There seems to be no satisfactory resolution of the bound state problem in QFT. We learned how to calculate the S-matrix to high precision. So, we can get the energies of bound states as poles of the S-matrix (e.g., the Lamb shifts). However, there is no well-defined Hamiltonian in QFT, so the radiative corrections to the wave functions of bound states cannot be calculated within QFT.

3. Without well-defined Hamiltonian, I don't see how one can address the time evolution of wave functions in QFT.

The experimental support for problems 2. and 3. above does not require smashing of particles with TeV energies. It would rather need precise time-resolved observations of dynamics of low-energy systems, such as excited states of atoms. This may be not as sexy as building multi-billion dollar monsters, but the insight gained from these low-energy experiments could be just as fundamental.

22. **Shantanu**
May 10, 2006

Talking about searching for the unexpected, have a look at the bets amde at
Richard Arnowitt festschrift almost 10 years ago.
http://faculty.physics.tamu.edu/allen/fest/bets.html
Unfortunately , still none of the above have been discovered. Would people here want to take stabs at the same bet?

23. **Aaron Bergman**
May 10, 2006

QFT is a much better theory than you think, as has been gone over repetitively on spr.

24. **woit**
May 10, 2006

Please, all, spare us the bickering about the status of QFT. This is way off topic.

25. **John F. McGowan**
May 10, 2006

Is this wise? The only major linear collider ever built, the Stanford Linear Collider (SLC) , experienced extensive startup problems lasting for many years. For example, the initial scheduled run in the summer of 1988 yielded not a single Z boson. This was followed by several years of very disappointing performance, numerous technical problems, and considerable acrimony. The accelerator and asociated experiments (Mark II and SLD) were largely eclipsed by LEP. As far as I know, no one has built a similar machine since. Does the knowledge and expertise really exist to build an even more ambitious linear collider?

26. **sunderpeeche**
May 11, 2006

"As far as I know, no one has built a similar machine since." The SLC is the only linear collider ever built. For the pedants, it was not a true linear collider because it used only one linac (for both e+ and e-) and routed them through 2 arcs to a detector (Mk II, later SLD). It experienced all of the teething problems of an untested technology ("path breaking" … you choose the description). Its luminosity was far below expectations. It should have produced Z's before LEP and it did not. I don't know about the acrimony.

The ILC will be a true linear collider (2 linacs). I have no doubt it will also experience teething problems. The proponents admit there are significant R+D issues to solve. I have heard several people say it is unwise to put all the eggs in such a basket.

The SSC got cancelled. A muon collider is also untested technology, even more far future than ILC. Have you any better ideas?

27. **better ideas?**
May 11, 2006

Futuristic techniques could increase the acceleration gradient by up to 3 orders of magnitude. This would allow to shorten a linear collider by 3 orders of magnitude. It would have true practical applications, giving cheap sources of synchrotron radiation.

Unfortunately this kind of activity does not receive the attention and manpower that it deserves, proposing theoretical speculations is more rewarding, and with some propaganda we can maybe get the 5 or 10 Giga$ needed for a ILC.

PS: I am a theorist.

28. **sunderpeeche**
May 11, 2006

People have been trying futuristic ideas for > 20 years (30 years) ~ plasma accelerators etc. Still far future.

Read here for info about SLC. Reasonably honest, not too boring.
http://www-sldnt.slac.stanford.edu/sldbb/SLD%2010-01.pdf

29. **David**
May 11, 2006

I just thought I would add a few comments to this. Before it was called ILC it was known as NLC (Next Linear Collider) and there has been a tremendous amount of work that has been done on trying to realize such a big project. Experimentally there was the NLCTA (Next Linear Collider Test Accelerator) that ran at SLAC as well as much research into higher frequency klystrons. It was basically a normal conducting X-band (11.424 GHz) accelerator. Going to higher frequencies and higher energies is a tremendous effort because of the impediments of reaching higher gradients due to effects like RF breakdown. When I was a grad student there I actually worked on the effect of pulsed heating that occurs on the interior copper surfaces from the eddy currents due to high magnetic fields. The heating happens so quickly the copper has no time to expand which causes stress on the surface. This repeated cyclic stress can be enough to cause microcracking over some period of time. This degrades the Q of the cavity and also causes thermal runaway (not to mention leading to RF breakdown). This all means if you are not careful and actually build an accelerator that can deliver the gradients you want, you might destoy it over not so long of a time. This leads into material science and surface science. So there are just an incredible amount of things to look at to realize something like the ILC. Superconducting accelerators do not have the pulsed heating issue but they do suffer from wakefields due to the high Q's of the cavities. Trying to realize an ILC machine has not been done for the lack of trying, but it will still require great efforts moving forward.

30. **wab**
May 11, 2006

Something better? We could return to the idea of the VLHC (>100 TeV @ 10^34 luminosity). The Fermilab design study of a few years ago shows that we do know how to build such a machine technically. Managerially the jury is out; but the LHC construction trials are valuable and relevant experience.

31. **sunderpeeche**
May 12, 2006

I mentioned in an earlier post that after SLC there was TLC (1 TeV x 1 TeV), this was too demanding, it became TLC (1 TeV c-o-m), eventually NLC (achieve whatever is feasible ~ 250 GeV x 250 GeV), evolved into international project = ILC. Very demanding technically.

VLHC? The technology + knowhow already exists to build SSC, VLHC and other conventional storage rings (a super LEP if desired). But the size and cost is very large. SSC did not survive the politics. (ILC may not either.)

32. **Tony Smith**
May 12, 2006

The 27 April 2006 issue of Nature said some interesting things about the ILC.

1 – The lead editorial (unsigned) (page 1089) had a favorable subhead, saying "There is broad backing for a US bid to build the International Linear Collider".

2 – That same editorial also said "… Germany, Japan, and CERN itself may also bid to host the project … the insistence of CERN … that decisions about the siting of the ILC be delayed until an accelerator technology … is ready, strikes some in the community as unnecessary and self-serving …".

3 – A News article by Geoff Brumfiel (pages 1094-1095) said "… Russia, Japan, and China have all expressed interest in hosting it …".

4 – That same News article also said "… Mike Lubell, head of public affairs for the American Physical Society in Washington DC, cautions that the collider – estimated to cost at least $6 billion – still faces an uphill battle …".

5 – The above-mentioned editorial also said "… political backing for the deal is likely to come from friends of Fermilab … Fermilab … is looking to its supporters, such as House Speaker Dennis Hastert (Republican, Illinois) …".

I see some similarities to the SSC situation.

A – Rivalries about siting (in the ILC case, USA v. CERN, Japan, China, etc…).

B – Rushing to get commitments before technology is demonstrated (maybe this is related to remarks by G. William Foster in his 19 February 2006 resignation letter "… it has been impossible to elicit any honest public discussion of a technically defensible schedule for the ILC from the leadership of HEP. This is not an academic exercise or an empty political game, since unrealistic schedules for the ILC are being used to destroy prospects for any feasible mid-scale near term program in U.S. HEP. … our steamship seems to have a committee of captains who believe that the ductility of carbon steel rivets at freezing temperatures is just a political problem …").

C – The $6 billion dollar figure for ILC (when the DOE estimate is $12 billion) may be an effort, a la SSC, to lock in the project with an unrealistic low-ball cost estimate, which is bound to unravel sooner or later.

D – If the Democrats win the House in 2006, then Illinois Republican Dennis Hastert will not be Speaker of the House. The current Democrat Minority Leader is Nancy Pelosi of California. Recall that the straw that broke the SSC's back was the defeat of Republican Texas-SSC supporter Bush I by Democrat Clinton, who, although he had paid lip service to the SSC, refused to go to the mat to fight for it, and was quite OK to let it die.
If Pelosi succeeds Hastert, and follows the Clinton pattern, she will tell all the ILC lobbyists/physicists how she is strongly in favor of the ILC at Fermilab, but when it comes time to cast the votes, she won't crack the Speaker's disciplinary whip, and will watch the vote kill the ILC, and say something like "Gee, I tried, but times are tough and too many people wanted to use that money for what they see as more pressing things" – (her web page emphasizes Energy, Medicare, Gas Prices, etc).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

33. **sunderpeeche**
May 12, 2006

The ILC is far from a done deal. The technology to build it, the siting (will the US pull out if ILC not in USA?), a timeline and cost estimate — all are unknown. Not to mention a change of administration and/or party control of Congress. It isn't always Democrats who kill Republican-sponsored programs. (BTW do the Reps really support ILC?) The Apollo project was initiated by Kennedy, killed off by Nixon.

34. **Tony Smith**
May 13, 2006

sunderpeeche said “It isn’t always Democrats who kill Republican-sponsored programs.”

sunderpeeche is correct.
It is just accidental/coincidental that Texas’s Bush I who supported SSC and Illinois’s Hastert who supports ILC are both Republicans,
and
that they were (or may be) succeeded in power by Democrats Clinton (no big allegiance to Texas) and Pelosi (no big allegiance to Illinois).

sunderpeeche also asks “BTW do the Reps really support ILC?”.
No, not in general.
It is just an accident of geography that Hastert of Illinois is Republican Speaker of the House, and as Republican Speaker he has a lot of power to whip all House Republicans (now a majority) into line.
If a non-Illinois Republican were Speaker, the Republican Majority would probably not be a coherent block in favor of the ILC.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – If Dirksen had not been an influential Senator years ago, Fermilab might not be in Illinois. Dirksen’s party was much less relevant than his influence used on behalf of his constituents in Illinois.

PPS – As to my personal political party bias, in the last 4 USA elections, I voted Perot, Perot, Libertarian, Libertarian. I don’t have much of any preference between the Republican and Democrat parties.

35. **Tony Smith**
May 13, 2006

sunderpeeche said “… after SLC there was TLC (1 TeV x 1 TeV), this was too demanding, it became TLC (1 TeV c-o-m), eventually NLC (achieve whatever is feasible ~ 250 GeV x 250 GeV), evolved into international project = ILC …”.

An ILC operating up to sqrt(s) = 500 GeV should be able to “… effectively perform top quark measurements in an almost background free environment …” and study the “… The top-Higgs Yukawa coupling …”, as stated in a Fermilab document at http://www.fnal.gov/pub/news05/TTTPosterSession/TTT-ILC.pdf.

Since it is very likely that LHC will see Higgs at 115 – 200 GeV, it is also very likely that ILC could be very useful to study the Higgs – T-quark system.

I am puzzled why the USA HEP community fails to emphasize such a very likely useful symbiotic relation of ILC and LHC,
but instead
emphasizes speculative things like superstring-relatd supersymmetry, which might well be ruled out by LHC before the ILC is completed.

Why take a Russian Roulette chance of getting ILC killed by LHC seeing only Higgs at 115 – 200 GeV (plus stuff similar to that already seen at Fermilab),
when
a case can be made that the ILC and LHC are both needed to fully understand how the T-quark and Higgs work within the Standard Model ?

For example, in “Discovering the Quantum Universe” the only mention I recall of studying quark interaction with Higgs is on page 14 (page 18 of the pdf) with respect to the possibility that LHC might find a Z-prime.

Why not play up T-quark interactions with Higgs ?

Not only is it almost certain to be seen at LHC, and therefore real,
but also
there are interesting possibilities that could be publicized.
For one example, look at the T-quark condensate work of Yamawaki et al (hep-ph/0508065 and hep-ph/0311165 etc). In addition to being interesting, it might be nice for the USA HEP people to put some emphasis on non-USA work such as that at Nagoya.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

36. **sunderpeeche**
May 13, 2006

Indeed it would make sense to emphasize/advertise the Higgs and T-quark sectors. The T has already been found, ~ ” tentative evidence for a Higgs at 114 GeV was found at CERN, a more comprehensive search can be made at ILC, including precision studies of its properties” ….. certainly in this regard the extra dimensions stuff is hype.

37. **Alejandro Rivero**
May 15, 2006

That sounds sad to me, when such things as the values of the Standard Model parameters are not explained by conventional models

This happened already from the instant we accepted the view of QFTs as a chain of effective theories because then we bought also the asumption that any unnatural mass (in the sense of t’Hooft naturality) shall come via renormalization group from the GUT scale, and then we will never be able to predict his mass with experimental certainess because we will not have enough loops to run the renormalization group downwards with high precision. Of course this is not the whole history, because even with effective theories we can exploit some formulae coming from fixed points; but in general it is true one has surrendered to the string-gut-planck scale.

38. **sunderpeeche**
May 15, 2006

Let us not get carried away with silliness, just because of the overabundance of (presumed to be) vacuous theories. What did Max Planck predict when he fitted the blackbody spectrum in 1900? To Planck the energy partition into finite cells was a book-keeping device which he could not avoid. It was Einstein who took the existence of light quanta as particles seriously, in 1905. When Dirac first published his equation, his paper had no predictions of new phenomena. It gave a “natural” explanation of spin 1/2 (and a value g=2), but it was already known (but not explained) that an electron has s=1/2 and g=2. Dirac had no immediate explanation for the negative energy states. The realization that the union of relativity and QM requires the existence of antimatter came later (as did the realization that the Dirac eq must be formulated as a second-quantized field operator equation).

Pauli did indeed hew to the principles 1 and 2 above, and dismissed just about any new idea as rubbish. He initially opposed the notion that an electron could have an intrinsic angular momentum, he later rejected Yang-Mills theories (because a nonzero bare mass for the gauge bosons destroys renormalizability). He opposed many other good ideas (including the work of Stuckelberg). Not really a good role model to follow.

39. **anon**
May 15, 2006

Wolfgang Pauli's letter of Dec 4, 1930 to a meeting of beta radiation specialists in Tubingen:

'Dear Radioactive Ladies and Gentlemen, I have hit upon a desperate remedy regarding … the continous beta-spectrum … I admit that my way out may seem rather improbable a priori … Nevertheless, if you don't play you can't win … Therefore, Dear Radioactives, test and judge.'

(Quoted in footnote of page 12, http://arxiv.org/abs/hep-ph/0204104)

40. **Alejandro Rivero**
May 16, 2006

Note that the events related to Spin happened very fast during the years 1921-1929 or even a narrow interval (1923-25?). From the empirical work of Lande, on J^2 terms, Pauli was in the belief that angular momentum had an intrisic bi-valuedness of mathematical origin, and then he rejected the need of the notion of a new physical angular momentum in the electron; initially he was looking for the answer in total angular momentum itself. He suggested that it was possible that angular momentum had a minimum interval to be integrated upon (so \int j^{-2} dj produces 1/j – 1/(j-1) or even that any derivative of functions f(j) was forced to be discrete (so d/dj 1/j gives the term 1/j – 1/j-1). This was in 1923. Then Born-Jordan mechanics showed that quantisation of angular momentum was in multiples of h/2, while Schroedinger mechanics showed that it is in multiples of h. Dirac comes to the rescue giving a wave equation (thus Schroedinger like) that provides the h/2. And from both ways it is clear that the j(j+1) terms in angular momentum are coming from the indeterminacy principle but not from the bivaluation; thus Pauli accepts the electron 1/2 as a separate idea: the bivaluation of the electron (that at the end comes because SO(3) is covered by SU(2)) separated from the fact that J^2 has eigenvalue j (j+1).

41. **sunderpeeche**
May 16, 2006

I am much edified. My previous post was however in response to a preceding utterly foolish comment (now deleted). The fact remains that a new idea frequently offers no clue as to the consequences it will lead to. And one cannot demand that it do so.

42. **Alejandro Rivero**
May 16, 2006

I agree; and in fact we still do not know where the idea of fermions will lead us to. Look at susy, for instance.

More Links

May 11, 2006
Categories: Uncategorized

If you just can't get enough of pictures of mathematicians, head over to the [Oberwolfach Photo Collection]. This is a huge collection, which I recently ran across when trying to locate something by [Graeme Segal]. It also includes a photo of a [frequent contributor here].

The AMS has put quite a few whole books on-line. Recently they added the three-volume set *A Century of Mathematics in America* [here], [here], and [here]. Another interesting volume is [Mathematics into the Twenty-First Century], which includes some wonderful survey articles, including a very long series on Lie theory by Roger Howe, and one on [Geometry and Quantum Field Theory] by Witten.

Terrance Tao has a sort of [research blog].

In the category of interesting-looking work that I haven't yet had time to read carefully and think about, here are two papers on an approach to studying pure Yang-Mills theory, by [Freidel] and [Freidel and collaborators].

Update: A correspondent suggests I mention another [famous mathematician] whose photo is on the Oberwolfach site.

Comments

1. **JC**
   May 11, 2006

   Speaking of Ted Kaczynski, anybody have a clue as to why he left math in the first place? (Of all the articles and books I've read about him over the years, I haven't been able to find an explicit reason as to why he left math).

   If I had to guess, maybe Ted got bored of math?

2. **Deane**
   May 11, 2006

   Great stuff!

3. **Michael**
   May 11, 2006

   There's been some legitimate and constructive criticism of the recent work on 2+1d YM. Some people, whom I consider experts in "ordinary" QCD — including but not limited to the authors of recent Pomeron research –, have raised some questions that ought to be answered before relying on those results as a starting point for new research.

4. **Lubos Motl**
   May 11, 2006

   JC: according to Wikipedia, he just wanted to live as a hermit – which is not a bad idea after all.

   And he was preparing his anti-technological revolution.

   I did not know this guy until 30 minutes ago. Interesting. A few minutes ago, I just tried to solve the quiz "Gore or Kaczynski"

   http://www.crm114.com/algore/quiz.html

   and got exactly 50 percent of the answers right, which is exactly what one gets by guessing. Can you do better?

   Although I have some compassion and understanding for Unabomber, it is rather clear that our colleague Bill Cottrell probably has much more understanding. 😉

5. **jayhog**
   May 11, 2006

   Ted Kaczynski is a hero, a man of honor and courage.

6. **jayhog**
   May 11, 2006

   Ted Kaczynski is also a true freedom fighter.

7. **Sakura-chan**
   May 11, 2006

   I'm sure you said that with a very straight face jay. 😉

8. **jayhog**
   May 11, 2006

   lubos, some of Ted Kaczynski's ideas may seem to similar to that of Al Gore's, but they are really quiet different. You should read his manifesto at http://www.thecourier.com/manifest.htm

9. **Pindare**
   May 12, 2006

Tao’s habit of commenting each new paper is nice and I hope it will generalize (I know a few other interesting examples), instead of just putting a kind of in-your-face dense list of papers.

The latter may impress grant-related committees, but it’s of little use for the would-be grad student, and for colleagues as well…

10. **knotted string**
May 12, 2006

‘… according to Wikipedia, he just wanted to live as a hermit – which is not a bad idea after all.

‘And he was preparing his anti-technological revolution.’ – Lubos Motl

Dear Lubos,

have you any plans to follow in Kaczynski’s footsteps?

🙂

11. **Lubos Motl**
May 12, 2006

Dear knotted string,

I happen to have the opposite opinions about technology than Kaczynski, and if I am tempted to send someone mail bomb, it’s the people who say that the technological progress destroys the ecosystem – people like Al Gore. I say Al Gore instead of Unabomber because Unabomber is already arrested which is OK with me.

Let me emphasize that I have no short term or long term plans to send mail bombs. 😉

Dear jayhod, Kaczynski was almost certainly a more courageous character than Al Gore, but let me stay away from the fan clubs of both of these Gentlemen if it’s OK with you.

All the best
Lubos

12. **A.J.**
May 12, 2006

Just for the record (since Lubos is a bit vague about this), Billy Cottrell was a vandal, not a murderer. He does not deserve to be compared with the likes of Kaczynski.

13. **Lubos Motl**
May 12, 2006

Dear A.J.,

no doubt that there is a difference but you can see that even Kaczynski has his own admirers here. 😉

All the best
Lubos

14. **sunderpeeche**
May 12, 2006

Witten is not in the photo collection. Having a Fields Medal is not enough to get oneself into the collection, I guess.

15. **jayhog**
May 12, 2006

Al Gore is not really anti-technology, after all, he’s on the board of Apple Computers. Al Gore’s a pro-technology green. Ted Kaczynski’s criticism of technology is not limited to its effect on nature, but also its effects on human, for example he disliked genetic engineering because it’s potential to change what it means to be human. Ted Kaczynski’s a neo-luddite.

16. **Who**
May 12, 2006

has anybody read the paper by Freidel Minic and Leigh that Peter linked to in the initial post?

17. **Eli Rabett**
May 14, 2006

Try Thoreau or Kaszinski instead

18. **Who**
May 14, 2006

Try Thoreau or Kaszinski instead
Sorry to be grouchy, Eli. BTW I liked your translation of the article in Die Zeit.

there’s all kinds of atavism in the world—lots of ways to be luddy-minded—some trash technology and some sabotage their own or their neighbor’s mental faculties

I want to know by how far Freidel missed the Clay million.

19. **anonymous**
May 14, 2006

Why is *Freidel* the only one you're mentioning? This is just an attempt to extend the interesting work of Leigh, Minic, and Yelnikov in the 2+1 dimensional case, which is in turn an outgrowth of work by Karabali and Nair. But of course readers of this blog seize on the one person who has worked on "alternative" approaches to quantum gravity as the person to mention and ignore the contributions of others. In any case, it is clearly not yet very close to being what the Clay Institute wants.

20. **Who**
May 14, 2006

thanks, why is it not yet close? what remains to do (if not too much bother to summarize briefly)?

Let's call it the work of Minic Leigh and Freidel, simply changing to reverse alphabetic order.
The interest from a quantum gravity viewpoint is that there are some papers by F. and Sean Majid, by Baratin and F., and by F. and Livine that get some results in 2+1 dimensions certain parts of which are said to be extendable to 3+1: a problem being presently addressed. there may be some common element in how they try jacking up the dimension in both cases from 3d to 4. I don't know that there is, but the possibility seemed there.

21. **woit**
May 14, 2006

anonymous,

The reason for mentioning it has nothing to do with Freidel's work on quantum gravity and everything to do with the fact that it's in 3+1, not 2+1 dimensions. The Karabali + Nair and later work in 2+1 d is quite interesting, but 3+1 d is very different and personally I was always dubious that you could do anything in 3+1 d with their methods. If Freidel and collaborators actually can, that will be very exciting. I just haven't had the time to read their papers carefully enough to see exactly how far they've managed to get. Like Who, I'd be interested to hear from someone who has carefully read and understood these papers (but not interested in hearing from people whose reaction is just to dismiss Freidel's work because he also works on an alternative to string theory).

22. **anonymous**
May 14, 2006

Well, we're more or less in agreement: (3+1)d is indeed very different, and it would in fact be very exciting if a result is achieved. Here's my understanding of the current status.

One defines a set of variables as follows. For a given point "x", one takes Wilson lines extending along one of the axes from infinity in to x, and then along a different axis from x out to infinity. This provides a set of local, gauge-invariant operators. They have some nice properties related to holomorphy in 2D, and in 3D they satisfy some identity H_{ij}H_{jk}H_{ki}=1 (and can also be formulated in a somewhat holomorphic way).

Next one shows that there is a nice Hamiltonian approach to studying the theory in terms of these operators and some associated currents. There are some subtleties about regularizing in the right way but they seem to have addressed these.

The next problem is to construct the vacuum wave functional. In (2+1)d there turned out to be a simple analytic solution in the large N limit. In (3+1)d, it is not nearly so simple, and they seem to have currently only some wishful thinking. In the UV they find that a free-field ansatz seems to work (it had better!), and in the IR something with a mass scale might work. But the detailed form of the solution is completely mysterious.

Unfortunately, that mass scale is just added by hand (effectively, it's a cutoff) and so far there is no connection of UV to IR in which we can see that it really is the QCD scale and is related to logarithmic running. They hope to find some self-consistent solution showing there really is a nonzero mass. To do this would certainly be a major breakthrough, and I hope it can be done, but it hasn't yet.

If all of this works it's at best a solution at large N, but that would still be very exciting. However, the current status reminds me of some old work of Migdal on solving the loop equations; there are tantalizing hints that there is something deep there, but no one ever seemed to know quite what to do with it. (In fact, you might think of this more recent approach as working with a subset of the loop variables, namely those loops that go through the "point at infinity" and remain parallel to one of the axes at all times.)

23. **Bert Schroer**
May 15, 2006

To Peter and anonymous:
with all caution I would like to take the following optimistic view on the Freidel et al work.
It should be viewed as being connected with Stanley Mandelstam's old program (Ann. of Phys. 19 (1962)) of substituting gauge theory by a formulation in terms of field strength. This old idea has received a significant conceptual underpinning through our recent work on semiinfinite spacelike string-localization derived within the new powerful setting (Mund, Schroer, Yngvason in print in CMP, math-ph/0511042) of modular localization. In that setting the family of massless finite helicity=s Wigner representations have pointlike field strength but (in the same Hilbert space, hence no ghosts!) covariant semiinfinite spacelike localization. As a result the the associated quantum potentials A(x,e) fluctuate in both the Minkowski space x and the De Sitter e (unit spacelike string direction) which leads to an improved short distance behavior (scaling dimension 1 independent of s). These objects are not Lagrangian fields but the properly adjusted (to strings instead of pointlike fields) Epstein-Glaser approach seems to work. For s=1 the string-independent subalgebra corresponds to the gauge invariant observables. But for s=2 (Gravitation, dimg(metric potential)=1) this is virgin territory since there does not seem to exist any gauge setting in terms of unphysical pointlike potentials. If the calculations continue to live up to our expectations we should have in due time a "renormalizable" setting for gravitation (where renormalizable means that from an infinite parametric universal Bogoliuvov-Shorkov-Epstein-Glaser matrix S(couplings) we are able to filter out a finite parametric set). This together with the strong arguments bu Brunetti and Fredenhagen that the perturbation can be arranged in such a way that (using their quantum local covariance principle) that a perturbative version of background independence results.
Maybe I project too much into Freidel et al ideas, as with any computational work whose conceptual basis is yet nebulous I may have taken an overly optimistic interpretation.
Woudn't it be ironic if string-localization (not at all related to string theory) achieves what string theory promised?
Question to Peter: suppose what I described can be in due time converted into hard facts, what would you thing string theorists will do if they loose their only supporting argument?

24. **Bert Schroer**
May 15, 2006

small correction to an important statement in my text
"In that setting the family of massless finite helicity=s Wigner representations have pointlike field strength but (in the same Hilbert space, hence no ghosts!) covariant semiinfinite spacelike" localized potentials.

25. **woit**
May 16, 2006

anonymous,

Thanks for the very informative comments on the Freidel et.al. work!

Bert,

If the scenario you describe pans out, string theorists will announce that it is part of “string theory”, closely related to what they had in mind all along, and proclaim victory.

26. **Bert Schroer**
May 16, 2006

What a coincidence, given their hegemonic all-devouring sociological situation this is precisely what I also expected!
In this case the word “string-localized” would make it quite easy for them (the fact that its conceptual meaning is lightyears apart from string theory would be no obstacle for people who live in a conceptual twilight zone).

27. **Aaron Bergman**
May 16, 2006

Don’t forget the pony.

28. **woit**
May 16, 2006

Great idea Aaron, let’s add “and a pony” to all string theory papers coming out these days. Would be highly appropriate…

29. **Aaron Bergman**
May 16, 2006

Just the ones that don’t calculate anything.

30. **knotted string**
May 16, 2006

What % is that?

31. **woit**
May 16, 2006

Since 0% of string theory papers calculate anything that can be compared to the real world, I guess he means ones that calculate something, with the hope that someday it will lead to some calculation that can be compared to the real world. These papers deserve “and a pony” as much (and often, more) than any others.

32. **Aaron Bergman**
May 16, 2006

The least you can do when you don’t have any hope of comparing with the real world is to calculate something. Otherwise you get “wouldn’t it be nice if…” papers. Wouldn’t it be nice if we all had a pony?

33. **woit**
May 16, 2006

Just doing random, pointless calculations doesn’t make you a scientist. If you want to claim that your calculation is part of science, you have to be able to show that it has some hope of being related to the real world. Right now, just about all string theory calculations are motivated by “wouldn’t it be nice if” hopes. Some of them very bizarre, e.g. “wouldn’t it be nice if the world were some random point in some horrifically complicated space and we could never predict anything…”

34. **Thomas Love**
May 16, 2006

I enjoyed browsing through the pix, but they’re not well catalogued. There’s an entry for Marsden, Jerry and for Marsden, Jerrold E. ; Singer, I. and Singer, Isadore. Several photos are only labeled by last name. One I am curious about was “Love”, it isn’t me (of course) and it’s too recent to be AEH Love.

35. **Aaron Bergman**
May 16, 2006

We all know you dislike string theory, Peter.

36. **woit**
May 16, 2006

One reason being that string theorists, lacking any real arguments, can’t stop themselves from descending to ad hominem ones

37. **Alejandro Rivero**
May 16, 2006

You could perhaps enjoy messages 16, 28 and 29 in this thread:

http://www.physicsforums.com/showthread.php?t=120192&page=2

one can use math a lot simpler than string theoretists and still come to results of fuzzy and membranous interpretation.

38. **Aaron Bergman**
May 16, 2006

I glad you kept that in such general terms because I haven’t made any ad hominem remarks in this comment thread. Nonetheless, as enjoyable as this exchange has been, I’ll bow out now unless something new happens to come up.

arXiv Weirdness

May 16, 2006
Categories: Uncategorized

Some rather strange things are going on at the arXiv, especially in the hep-th section:

Besides the usual string theory papers, which just get more and more pointless as time goes on, some very weird things have started to appear on hep-th. Last night, there was a new paper entitled [Amplitude for existence of spacetime points]() that makes no sense to me. It's by Monica Dance, who seems to have no academic affiliation, but does have a Hotmail account. Not clear why the hep-th moderator allows this kind of thing. One explanation would be that earlier this year she put [Symmetry limitations on quantum mechanical observers, and conjectured link with string theory]() on hep-th, an equally nonsensical document which presumably was all right because it had "string theory" in the title. Maybe once you get one paper about "string theory" on hep-th, you become an "active researcher" and can put whatever you want there.

Actually, to get to be an "active researcher" according to the arXiv, as long as you're studying string theory, you don't need to even ever have written a paper at all. Recall that arXiv trackbacks to this blog have been banned on the grounds that I'm not an "active researcher" (for more about this, go [here]()). But [this hep-th paper]() has a trackback to [this blog entry]() by Nicola Ambrosetti. Ambrosetti's blog contains some fine entries and having trackbacks to it makes good sense, but he appears to be a student at Neuchatel who has never written a paper, so I would have guessed that according to arXiv standards he wasn't yet an "active researcher". Maybe standards are different when your blog entries have titles like [Barton Zwiebach Rules!]().

Over the last few months I've written quite a few blog postings that discuss arXiv papers. In many cases I happen to think that either the posting or the discussion in the comment section is something that someone interested in the paper might find worthwhile. In the case of postings about string theory papers ([here]() and [here]()), [non-string theory papers](), and [non-string theory papers claiming to be string theory papers](), no trackbacks to my blog were allowed. This is what I expected, but for some mysterious reason, a trackback to [this blog entry]() about a [paper critical of string theory]() was allowed. So, it seems that the arXiv is allowing trackbacks to my blog entries only when they are about papers criticizing string theory.

None of this makes any sense to me, so I tried politely writing to the arXiv person at Cornell that my logs showed had examined my blog entry just before their trackback system generated a trackback to it, asking about what was going on. No response to that inquiry, as to all my other inquiries about arXiv trackback policy. To find out what their policy now is, I guess I'll just have to wait for the relevant authorities to get around to posting a blog entry or writing a comment at some blog somewhere, since that seems to be their preferred manner of dealing with this issue.

Update: I did get an e-mail response from someone at Cornell about my enquiry about trackbacks. It didn't address the question of how hep-th trackbacks are now being moderated, but did point out that the trackback system is still in an experimental state, that they have recently had significant personnel changes, and that sorting out the trackback system hasn't been one of their highest priorities.

Update: Lubos is even more out there than usual with his [comments on this](). It seems that my objections to the Dance paper are an example of sexism.

Comments

1. **Todd**
   May 17, 2006

   Doing a quick search I found that Monica Dance's first paper posted on the arXiv was listed under quant-ph in 2004 using the name M. C. Dance. It apparently had something to do with the Holographic Principle. It also appears that she is a policy analyst for the EECA (Energy Efficiency and Conservation Authority) in New Zealand.

2. **Aaron Bergman**
   May 17, 2006

   She was apparently at Auckland physics if [this post]() is accurate.

3. **Peter Erwin**
   May 17, 2006

   I think you may be conflating the nebulous (and poorly defined) "active researcher" status, which is apparently necessary for your blog posts to be linked (except when it isn't), with the ability to submit papers, for which all you need is: a) to have registered with the arXiv before they introduced their "endorsement" system; or b) to have been endorsed after that time by someone with the endorsement ability.

   So there's no a priori requirement, however well or poorly defined, to be an "active researcher" in order to submit papers anywhere in the arXiv.

4. **Charlie Dance**
   May 17, 2006

   > A simple possibility might be that spacetime points x have an amplitude of existence $E(x)$ consistent with this. The magnitude of $E(x)$ might be greater or less than 1 at any point x; or the relative values of $E(x)$ might be what matters. In a classical limit, a function like $E(x)$ might give the effect of a gravitational metric.

   This seems perfectly reasonable to me. Can anybody here explain why $E(x)$ isn't likely to give the effect of a gravitational metric? Couldn't the gravitational metric, m, have an amplitude of existence, $E(m)$? Particles have amplitudes of existence, and they even have different amplitudes depending on whether you're accelerating. Wouldn't that explain the standard model?

5. **Chris Oakley**
   May 17, 2006

   Charlie,

   A couple of wacky ideas turned out to be fruitful, namely special relativity and quantum mechanics, and many people have concluded from this that if an idea is wacky enough then it must be correct.

   I do not agree with this. The idea that spacetime points may or may not exist according to some probability amplitude is certainly wacky, but without some examples of how the idea clarifies that which was unclear before, one has to put it in the same category as those insights into the nature of the universe which are gained when under the influence of recreational chemicals.

6. **absolutely**
May 17, 2006

I think Charlie was attempting humour, or else he's the actor of same name genre-hopping.

7. **Ali Yegulalp**
May 17, 2006

Sometimes the web can be a very small place…

Monica Dance was a physics graduate student at Princeton in the early 90's (same time as me). She went back home to New Zealand after about 3 years without finishing the PhD program, started studying medicine, and then I don't know what happened after that. I knew her quite well in Princeton, but I haven't heard from her in years.

8. **abc**
May 17, 2006

> Maybe once you get one paper about 'string theory' on hep-th, you become an 'active researcher' and can put whatever you want there.

If this is the case the String Illuminati failed.

Judging from the references given by M. Dance it seems that as far as this is nonsense it is nonsense inspired by alternatives-to-string-theory (TM).

Which may happen…

9. **anon**
May 17, 2006

"In this model, the closer an object is to a mass or energy source, the more paths through spacetime might be available to the object in the direction of the mass/energy, or the higher the amplitude associated with such paths." – Monica Dance

What part of this doesn't make sense to you? Bring two objects together and anybody can see there is more space between and around them for path integrals.

It is not as if bringing things closer together will reduce the space for interactions … (I'm being sarcasm)

(I've got my own personal pet theory that is just slightly more radical. Perhaps if I FIRST write a positive constructive stringy paper on arxiv, they won't delete a second later paper on the topic?)

[extensive off-topic material deleted]

10. **Steve Myers**
May 17, 2006

Quick respone to Monica Dance's paper: it's a joke, right? She's showing the Arxiv will print anything (almost). As for $E(x)$?! Come on.

11. **Sam**
May 17, 2006

Oh no! Not another former Princeton Physics graduate student gone bad? Let's all pile on about how stupid she is.

12. **sunderpeeche**
May 17, 2006

I was going to suggest that the Monica Dance paper may be a practical joke.

13. **woit**
May 17, 2006

Steve + sunderpeeche,

The problem with the idea that this is a joke is that it's not funny.

Peter Erwin,

I was using "active researcher" in the arXiv sense of the term. They have announced that it doesn't mean "active in research", but is a term that cannot be defined (attempts to define it foundered when it turned out the hep-th moderator didn't qualify as one). In practice it seems to mean "someone the hep-th moderator approves of", and that's how I was using it.

I'm well aware that there's supposed to be a difference between the endorsement system for papers and the "active researcher" standard for trackbacks. The "active researcher" term is a piece of dishonesty created by the hep-th moderator, who announced that the usual endorsement system could not be used for trackbacks, since Peter Woit would "game the system" by getting endorsed. His other justification for this was that unlike papers, blog entries could not be individually moderated. The Monica Dance papers evidently were individually moderated and approved.

14. **Sam**
May 17, 2006

It's not just hep-th. All the archives are drowning in trash papers. Look at hep-ph or gr-qc.

Clearly, their moderation system is a failure.

15. **woit**
May 17, 2006

Sam,

I think you're right. Part of the problem is that when leading figures in theoretical physics start writing pseudo-science (e.g. the anthropic string theory landscape), it becomes impossible to have any rational standard that will allow them to keep publishing, but keep out other crackpot papers.

16. **Sam**
May 17, 2006

“Part of the problem is that when leading figures in theoretical physics start writing pseudo-science …. it becomes impossible to have any rational standard that will allow them to keep publishing, but keep out other crackpot papers.”

Yes, like the bird flu, string theory has destroyed, not just hep-th, but all branches of theoretical physics.

17. **Lubos Motl**
May 17, 2006

Nothing against Monica Dance personally, but I am pretty certain that these papers are completely nonsensical. Whether or not such papers or the previous papers by the same authors have “string theory” anywhere in the abstract is unphysical. (Lee Smolin’s also have “string theory” in them.) What matters is the content. The content is not string theory. It is an alternative to string theory – the kind of hypothetical stuff that Peter Woit and Lee Smolin call far.

Stuff that does not exist, and even if it exists, it is impossible to see it from the current state of knowledge.

The main message is that even in periods when progress is slow, it is very important to distinguish papers that are based on knowledge of existing phenomena and theories and that want to explain more about them, and papers that don’t. Dance’s paper is an example of the latter category, much like Sundance’s papers (with or without Lee Smolin).

People in theoretical physics must still learn quantum field theory and the non-phenomenological ones also string theory. If the community allows physics to be developed in the direction of these alternatives, it could be a real end of it, after a few years (one year is one decreasing e-folding once these mechanisms win).

18. **sunderpeeche**
May 17, 2006

A system like the ArXiv with moderators is prone to self-serving rubbish. Not all jokes are laugh-out-loud funny. Poor quality jokes (or poorly written jokes) can be very unfunny. Don’t give the paper more publicity than it deserves.

19. **Sam**
May 17, 2006

“A system like the ArXiv with moderators is prone to self-serving rubbish.”

Clearly, that’s what’s happened here.

“Don’t give the paper more publicity than it deserves.”

What? No more blog posts about stupid papers on hep-th?

20. **woit**
May 17, 2006

sunderpeeche,

As physics I don’t think the paper deserves to be publicized, but the fact that the arXiv moderation system is breaking down needs to be discussed and addressed by the particle theory community. This kind of breakdown already was becoming clear with the Bogdanov papers, but no one seemed very interested in addressing it at that time. It has just gotten much worse since then.

21. **sunderpeeche**
May 17, 2006

“What? No more blog posts about stupid papers on hep-th?”

Why do you want to waste your time on nonsense? Indeed, why am I writing this? Time to quit this thread and wait for something more sensible to be displayed on the blog.

22. **Sam**
May 17, 2006

“[T]he fact that the arXiv moderation system is breaking down needs to be discussed and addressed by the particle theory community. This kind of breakdown already was becoming clear with the Bogdanov papers…”

Bogdanov papers? On the archives? That’s terrible.

No wonder the archives are drowning in crap.

23. **woit**
May 17, 2006

Sam,

The Bogdanov papers were not posted on the arXiv. As far as I know they didn’t even submit them to the arXiv. They were published in several peer-reviewed journals, after gettting positive referee’s reports, and it was that breakdown in the refereeing system that I was referring to. That was a different case, but one root cause of the problem is the same.

24. **woit**
May 17, 2006

OK Lubos, the approval of the Dance papers is the fault of me and Lee Smolin. That makes perfect sense…

25. **anonymous**

May 17, 2006

You’re all missing the *real* point: her name is M.C. Dance! How awesome is that?

Seriously though, the arXiv is a *pre-print* server. Not everything there has to pass peer review. As long as the system works well enough to keep it from being completely overrun with crackpots, I would say it’s not a big concern.

26. **Sam**
May 17, 2006

“OK Lubos, the approval of the Dance papers is the fault of me and Lee Smolin. That makes perfect sense…”

Just like the idea that the crap papers all over the archives (not to mention the refereed physics literature) are the fault of the landscapeologists.

27. **Peter Erwin**
May 17, 2006

A couple of points:

I’d hesitate to conclude that *all* of the arXiv is “drowning in crap”; I wouldn’t say that’s the case for astro-ph, for example. Of course, almost all submissions to astro-ph come with self-labeled quality indicators: either it’s something presented at a conference (possibly interesting, but not well vetted), or it’s been submitted to a journal (potentially OK, but hasn’t been through peer review yet), or it’s already accepted by a journal (better chance of being OK). I admit to a certain bemusement when I glance over hep-th listings and see that the majority have no indication of whether there’s any connection to a journal or peer-review process.

Re Bogdanov and oddball papers showing up in arXiv: the problem is to figure out if there’s more at work than just the Recency Illusion and its cousin, the Frequency Illusion (see here for a discussion in the context of linguistics).

In other words, is there solid evidence that peer review, in general, is letting more nonsense through? Were there cases like the Bogdanov papers in the past, that have been conveniently forgotten?

28. **Jimbo**
May 17, 2006

I usually side with Peter on most of the topix discussed herein, but me thinks Prof. Woit is sulking a bit about his `inactive researcher’ status/stigma by the admins at ArXiv, and lashing out.
True, Ms. Dance may not have her PhD (neither does Freeman Dyson nor former Livermore NIF chief scientist Michael Cambell, nor many others), but neither PhD nor academic address implies scientifically viable work.
Hep-th needs novel ideas, apart from the standard schlock of mathematical masturbations, devoid of physical relevance, which regularly inundate its nightly postings.

29. **woit**
May 17, 2006

Jimbo,

Whether or not this author has a Ph.D. or not is thoroughly irrelevant, and not something that I raised. The problem with these papers is that they’re nonsense, not the qualifications or lack thereof of the author.

Yes, hep-th needs novel ideas. But it also needs a moderator who can recognize utter nonsense for what it is.

And no, I don’t think “sulking” is quite the right word to describe how I’m responding to the behavior of the arXiv hep-th moderators. It’s not like I’m not saying loudly and publicly what I think about this…

30. **Sam**
May 17, 2006

“But it also needs a moderator who can recognize utter nonsense for what it is.”

Was he the referee on the Bogdanov papers, too?

That would explain everything.

31. **woit**
May 17, 2006

Sam,

No, the problem is not one person.

32. **Lubos Motl**
May 17, 2006

Dear Peter,

of course that it is your fault and fault of Lee Smolin. It’s two of you, among others, who have been fighting for years to replace string theorists with crackpots. By “original independent ideas” which is how you call these cranks. Accepting papers of the same quality as Monica Dance’s paper or the paper about trinions has always been one of your main political goals.

The staff or arxiv.org accepted these papers to make two of you happier. You don’t like it either. You will complain until the end of your life but the real problem is that first of all, you have absolutely nothing to offer, and second of all, you discourage people from doing serious work.

Best
Lubos

33. **Alejandro Rivero**

May 17, 2006

I have not objection to bad papers in the arxiv. I have some about *repeated* papers, and worse if they are bad papers. Most emptypots do not innovate and thus the "version replacement" utility of the arxiv is handy to control it, instead of censorship.

34. **Shantanu**
May 17, 2006

Peter, let's forget such papers. any comments on hep-ph/0605119
and gr-qc/0602086(which is discussed in http://www.world-science.net/othernews/060514_bouncefrm.htm)

35. **SteveM**
May 17, 2006

The arxiv really needs to stop accepting papers that are just empty or vague waffle, wishful thinking, philosophising and handwaving. That's fine for coffee breaks or cocktail parties. Hard concrete and rigorous calculations that actually calculate or rigorously prove some very specific result or important /interesting point are becoming harder to find, and are actually difficult to produce–but that is why physics is hard! Even if you are into the mathematical side of things you still have to come up with some result or theorem or proof.A lot of people talk a good theory in english but then never really put anything into concrete mathematical terms or actually calculate anything; or else they make some general mathematical statements then go on about what they would like to be able to do, like to be able to calculate etc., but it never happens. A minimum criterion for acceptance should be that it contains an interesting result or concrete calculation.

36. **Alejandro Rivero**
May 17, 2006

Coffee break conversations are important. The path integral was developed as a follow-up of a coffee table conversation at Princeton (I never remember the name of the pub, it was an exotic island or so). Perhaps the physics/ cathegory should be better place than hep, but a lot of people tries first hep because moderators usually forbid cross-announcements (not really cross-posting) from physics to hep-yyy listings.

The use of diverse bulletins plus personal options in the listings (show/not show cross-posting, show/notshow replacements, filter, etc) could do better work. Also note than a lot of arxiv navigation is via SPIRES (or citebase) reference linking.

37. **woit**
May 17, 2006

Shantanu,

Sorry, but I'm no cosmologist and this isn't a cosmology blog. Better to look for such a discussion at another place (e.g. CosmoCoffee).

38. **Peter Shor**
May 17, 2006

Alejandro Rivero's comment seems to contain the germ of an idea for eliminating crank/vague/philosophical/handwaving papers posted to the arxiv. Somehow direct them to a separate category. (I will hereby preempt the smart aleck who is going to propose that we name it hep-no, short for nonsense, and put all landscape papers in it.)

39. **Nigel**
May 17, 2006

Can anyone define crank/vague/philosophical/handwaving without behaving like a crank, being vague, being philosophical and doing a lot of handwaving? And can you define these in such a way that they allow today's 10/11-d string theory as not being crank, etc.

Kelvin's vortex atom is the best good precedent for string theory. It was full of ad hoc predictions, but couldn't do anything useful like predict subsequent discoveries. It might have been retained forever if radioactivity and nuclear physics hadn't been discovered.

40. **Lubos Motl**
May 17, 2006

Dear Nigel,
google for *crackpot errors* and pick the first reference:

http://www.google.com/search?q=crackpot+errors

This could answer your question.

Best wishes
Lubos

41. **Carl**
May 17, 2006

They must be getting loose at arXiv because they asked me to resubmit my crank paper that predicts the neutrino masses to 6 decimal places (LOL):
http://brannenworks.com/MASSES2.pdf

But they want it in "physics" instead of "hep-ph". I would guess that the reason for the liberality was that my paper was referenced by this paper:
http://www.arxiv.org/abs/hep-ph/0605074

Carl

42. **Shantanu**
May 17, 2006

Peter, point noted. However the first one hep-ph/0605119 is about collider particle physics and technicolor and has nothing to do with cosmology.

43. **A.Torok**
May 17, 2006

I support Lobus(seriously).

44. **A Different Peter**
May 17, 2006

Distinguishing cranky nonsense from genuine work is difficult for several reasons. One is that genuine, correct, work can appear to be gibberish if one is not intimately familiar with the topic involved. Another is that genuine researchers and Nobel Prize winners are not unknown to issue communications which appear to be just as incomprehensible and radical as the statements of certified crackpots. Another is that those who have made conceptual mistakes, and are now presenting some ridiculous assertion as a result, will defend their assertion for as long as they can, just as any competent physicist presenting a counter-intuitive but correct theory would.

A part of the problem is that physicists today do not have the time to devote to the unrewarding task of examining the claims of everybody who thinks they have a great idea about physics. Without investing this time and effort, it will be impossible to distinguish crackpots from undiscovered geniuses. It will not be possible to simultaneously satisfy those who want to ethnically cleanse the crackpots and those who want a source of original ideas. If you want to keep out the crackpots without being prepared to talk to the crackpots and discover exactly why they're wrong, then you're going to have to reject all ideas which aren't part of the mainstream.

So the choices are:

1. A slow and tedious process of carefully examining every claim and checking the validity of every argument. This is peer review.
2. Let the crackpots through the filter and some good ideas might come through as well. This is what the arxiv has always been, but it was better when fewer crackpots knew about it.
3. Accept what's mainstream and reject everything else, Lubos style. If you want to do this, string theory is the theory for you.

Any other ideas?

45. **Tony Smith**
May 17, 2006

Carl said "… arXiv … asked ..[him]… to resubmit …[his]… crank paper that predicts the neutrino masses …".

Did arXiv ask by sending an e-mail, or how?

Carl may be right that arXiv changed its mind because his "… paper was referenced by
http://www.arxiv.org/abs/hep-ph/0605074 …", which is a paper by Yoshio Koide of the University of Shizuoka, "supported in part by the … Ministry of Education, Science and Culture, Japan …".

Is this the first case of arXiv's blacklisting causing it to be embarrassed
when the work that arXiv blacklisted as crank
is found to be useful
(in this case, by non-USA Japanese who are outside the USA physics establishment and its consensus-enforcement mechanisms) ?

Tony Smith
http://ww.valdostamuseum.org/hamsmith/

46. **Richard**
May 17, 2006

Peter Shor said — "Alejandro Rivero's comment seems to contain the germ of an idea for eliminating crank/vague/philosophical/handwaving papers posted to the arxiv. Somehow direct them to a separate category."

I recently received an arXiv math daily title/abstract distribution email with a reference to a paper entitled A Concise and Direct Proof of "Fermat's Last Theorem" (listed as 4 pages long). It was placed into the General Mathematics category.

47. **LDM**
May 17, 2006

Is it Ms Dance's paper and its content, or its acceptance by arXiv, that is the problem here…or both?

Ultimately,you are known by your work, and if you want to publish scientific rubbish, then that is the scientific reputation you will develop…

I would suggest http://compbiol.plosjournals.org/perlserv/?request=get-document&doi=10.1371/journal.pcbi.0010057 as an excellent guide to temper overly enthusiastic journal submissions, with emphasis on point 10.

However, I disagree with Woit's claim, that the article is nonsensical.

I would rather make the objection that the ideas in this paper are so underdeveloped and so loose, that it is not appopriate for a scientific paper…and that this type of communication should continue to be confined to private correspondence (or even blogs)

48. **Chris Oakley**
May 18, 2006

I would rather make the objection that the ideas in this paper are so underdeveloped and so loose, that it is not appopriate for a scientific paper…and that this type of communication should continue to be confined to private correspondence (or even blogs)

Right. Here is a wacky idea. The author needs to do more to convince us that it is worth taking seriously. Even that wacky idea Superstrings can point to the possibility that they might be able to reproduce GR (although I am still not sure of the situation there).

49. **Nigel**
May 18, 2006

Dear Lubos,

Kelvin's 'crackpot errors' in predicting the vortex atom are just the same as string theory's. There were NO testable predictions. (Radioactivity and nuclear structure were totally unexpected, and disproved the vortex atom.) More on grand mainstream cranks:

Helge Kragh, 'The Vortex Atom: A Victorian Theory of Everything', Centaurus 44 (1-2), 32-114.

'Each atom, pictured as a ring with some set of possible oscillations …

'Here in principle, Thomson [Lord Kelvin] asserted, was the foundation of what we might now call a grand unified theory of light and matter. "Helmholtz's rings are the only true atoms," Thomson confidently declared.' – http://fermat.nap.edu/books/0309090733/html/266.html

50. **phenomenologist**
May 18, 2006

Motl: "People in theoretical physics must still learn quantum field theory and the non-phenomenological ones also string theory"

What a claim! Is finally lubos accepting string theory has nothing to do with the phenomenological world ???
Or is he promoting a new role for physics?
I wonder what does he consider himself…

51. **Bert Schroer**
May 18, 2006

"of course that it is your fault and fault of Lee Smolin. It's two of you, among others, who have been fighting for years to replace string theorists with crackpots. By "original independent ideas" which is how you call these cranks."
these are words from the mouth of the Lord of Misuse in the Hegemon´s service. Like in the case of climate change they create that relaxed atmosphere which reminds the rest of us that it took a never before seen lowering of standards emenating from string theory to convert large parts of particle physics into a metaphorical messianic enterprize in which everything goes as long as it does not hamper this path. The priority-fight around the antropic trashcan (using the fight between dogs around trashcans which one sees in the morning here in Brazil as a metaphor) is an excellent illustration of where the hegemon wants us to go.

52. **B**
May 18, 2006

The easiest way would be to allow registered users of the arxiv to review papers, maybe similar to reviews at amazon.com. Such a review process is not perfect, and the standard below peer review in journals, but it will give the inexperienced user a rough quality estimate on the papers posted. It might even provide valuable feedback to the author. Also, it would be useful to search papers according to some importance or impact criteria based on that. Say, if you are a newcomer in some field, it takes some while to figure out which are the relevant and important contributions.

Discussing papers in blogs and allowing trackbacks already leads into this direction, but it could be made more easy by directly having reviews on the arxiv.

See also my blog entry Peer Review II. Best,

B.

53. **anon**
May 18, 2006

When hundreds of papers are published in the arxiv, that contain no more substance than an air-pillow, what harm can Monica Dance's paper make? She at least has the courage to be original, unlike the above mentioned authors who hide their non-contributions to physics in the cover of mainstream physics and academic affilations.

54. **woit**
May 18, 2006

anon,

There are lots of not very good papers on the arXiv, but up until now I have not seen ones that at a glance are obviously nonsense like the Dance one. I'm sure that there is a long history of such things being submitted to the arXiv, and being rejected by their moderation system. I'm just wondering why their moderation system broke down in this case.

It's certainly bad enough how many not very good papers are posted on the arXiv, but if the thing becomes dominated by complete nonsense papers, that will be a lot worse, and a very, very bad sign for theoretical physics.

Originality is good and desperately needed, but the open problems in this field are very difficult and originality without competence is useless and just pollutes the field with more noise.

55. **D R Lunsford**
May 18, 2006

I have submitted a new paper to arxiv, "New Results in Gravitational Chess". In this paper, it is shown that when a bishop strikes through a square adjoining the queen, the path is deflected and it actually starts attacking squares of the other color! Now here is what is interesting: when it attacks past the *opponent's* queen, the path is deflected *the other way*. This was a totally unexpected new result and one must read my earlier papers on gravitational chess with this in mind. In particular, my earlier treatment of the Cosmological Constant Gambit is shown to be unsound.

Some will complain that my paper contains no games and not even any meaningful diagrams. I can only say that publishing costs prohibit me from introducing such minutiae that would only distract the reader from my main result in any case.

I hope to extend these results to rooks if my busy gardening and carpentry schedule permits. However, I feel that my legacy is secure, thanks to arxiv.org.

-drl

56. **sunderpeeche**
May 19, 2006

Gravitational Chess exists, believe it or not

http://play.chessvariants.org/erf/Gravitat.html

and yes, I will thoroughly approve if this post is deleted

57. **D R Lunsford**
May 19, 2006

ROFL! It’s so hard to do anything new 🙂

-drl

Priority for the Landscape

May 17, 2006
Categories: Uncategorized

The string theory anthropic landscape point of view has now become so widely accepted and entrenched in the particle theory community that various people are making their claims about having had the idea first. The standard first paper that people generally reference is Leonard Susskind's February 2003 [The Anthropic landscape of string theory](), which now has 243 citations. Susskind claims credit at least for the "Landscape" terminology in his recent book.

Last month Dutch string theorist Bert Schellekens posted a paper on the arXiv entitled [The Landscape "avant la lettre"](), in which he claims credit to some extent for the idea. He is quite enthusiastic about the Landscape as a paradigm shift and a new way of doing physics:

…I think even today we are only in an intermediate stage of a very slow shift of opinions regarding the objectives of our field. Although landscape ideas and even the anthropic principle are now at least discussed, it seems to me that the importance of the landscape is still severely underrated. I have tried to express my enthusiasm about the recent progress during seminars, but apparently with little success.

Schellekens claims that:

My own thoughts in this direction started around 1987. The year before I had published a paper with Wolfgang Lerche and Dieter Lust. Like other authors at the time, we found large numbers of four-dimensional chiral string theories, but much more than others we made a point of strongly emphasizing the non-uniqueness of the result.

He goes on to say that already back then it was clear to him that string theory was sending the message "if we find one vacuum we are going to find a huge number of them." He recalls that when he was working at CERN in the years before 1992 he was promoting the anthropic string theory landscape idea and encountering a lot of resistance, often from people who now tell him that they had always been saying this kind of thing.

In 1998, at the occasion of his inauguration at the University of Nijmegen he gave a speech on this subject in Dutch. In the arXiv preprint Schellekens reproduces the Dutch text of his speech, together with an English translation. He notes that he used the Dutch word "landschap" in the text, although he mostly referred to the landscape using the Dutch word for a "mountain range".

Schellekens admits that string theory may not be correct, but he says that string theory implies the landscape, so for string theory to be correct the landscape must exist. His only comment indicating that this might be a problem for string theory is that

…the unexpectedly huge size of the landscape is making it a lot harder to convince ourselves of that.

He does admit that back in 1998 he expected the size of the landscape to be much smaller than it now appears to be, smaller than the 10^{80} vacua that, uniformly distributed, could cover all possible values of the standard model parameters to the accuracy that we can measure them. So he expected that one would be able to somehow check string theory by seeing if one of the vacua agreed with the real world. Now that the number of vacua seems to be vastly greater than this, eliminating any reasonable hope of checking string theory this way, for some unfathomable reason his enthusiasm for the idea is undiminished if not intensified.

If you just can't get enough of landscape discussion, there are recent blog entries on the topic by [Sabine Hossenfelder]() and [Alejandro Satz]().

Update: The last-gasp hope for getting a prediction out of the landscape is that there is some useful structure in the landscape, so that it doesn't densely cover all possible standard model parameters. Washington Taylor and Michael Douglas have been looking for such a thing amongst vacua, trying to find some correlations between properties of these vacua. For more about this, see [Taylor's web-page](). Lubos has a [blog posting]() about all this, in which Taylor explains the philosophy:

If we find 5 models with features X and Y of the standard model which all have feature Z which is not yet observed it is not very definitive. If we look in different parts of the string theory landscape and find that all 10^{20} models we know how to construct with features X and Y of the standard model have feature Z also it begins to carry some weight as a possible prediction.

So far, as you might expect, since there is no known reason for such correlations, they haven't found any. Lubos reports:

Wati's result in his particular examples was that there was virtually no information in the correlations: the difference was one bit and the distributions of different quantities were essentially independent Gaussians.

and goes on to rant:

Surely the physicists have not been working for 30 years to extract 1 bit of information – whose probability of being correct is moreover 50 percent. Even if there were any correlation, I would probably find such a correlation physically uninteresting. We know for sure that some of these correlations would agree with those observed in the real world, and some of them would not.

What will you do with this probable outcome? Will you overhype the "successful" patterns as evidence that the landscape reasoning is good, while you will be silent about the "unsuccessful" ones? I would count this activity as a part of astrology or catastrophic global warming theory, not physics. It's frustrating to see that this is what is apparently being intended.

I wonder whether the people who were producing the very convoluted microscopic theories of the luminiferous aether in the 19th century really believed that this was the way to say anything new about physics – or whether most of them did these things just to do something and keep their jobs. Einstein took over in 1905 and showed not only that the aether was a ludicrous fantasy – but moreover, the absence of the aether is one of the basic principles that underlies his relativistic revolution in physics. Today, all of us – except for those in loop quantum gravity – know that the aether is a silliness that is not realized physically and that was never well motivated.

My feeling about the random model building and random model guessing is somewhat analogous to the random construction of the aether from gears and wheels. We're missing something and we should not fool ourselves into thinking that we're not.

Update: The Harvard physics department seems to be having quite a few seminars on the Landscape, and one participant [reports]():

A funny aspect of these discussions is that one can't quite distinguish which of the considerations are jokes and which of them are meant seriously. At least I can't distinguish them.

Comments

1. **Lubos Motl**

May 17, 2006

It’s our friends’ internal problem to decide who should be credited. I think that it’s some priests in the Catholic Church who should be credited for having discovered the idea a long time ago. At any rate, I don’t think that the originator of the anthropic idea has anything to be proud about, so it is not quite clear to me why anyone should be bitter about not being the first one who discovered this “great” idea.

2. **lmot**
May 17, 2006

One man’s junk is another man’s treasure.

3. **JC**
May 17, 2006

From a historical perspective, how fast did the aether idea fall out of favor a century ago? Was it dropped like a rock suddenly by many physicists, or did it gradually fade away slowly into irrelevance over many years?

4. **Chris Oakley**
May 17, 2006

In this case, Lubie’s junk is also my junk.

Q: Why did the chicken cross the road?

Anthropic answer: In the other 10^500-1 universes it did not, but we happen to live in the universe where it did, even though we have no idea why.

5. **Thomas Love**
May 17, 2006

JC asked when ether theory went out of favor. Basically when Einstein introduce SR. But Einstein himself reintroduced the ether in GR. Read “Einstein and the Ether” from Aperion Pub.

The ether is still being worked on actively, especially in Russia and China.

6. **lmot**
May 17, 2006

Actually Chris, that’s not the Anthropic principle, it’s the closely related Pouletropic principle. It subject of an upcoming paper by Dimopoulos and Arkani-Hamed that purports to solve the chicken-egg paradox.

7. **MathPhys**
May 17, 2006

Peter,

One can argue that the first glimpse of the landscape is in Narain continuous infinity of heterotic string compactifications.

When was Narain’s paper? 1985? 1986? That is definitely before Schellekens paper.

Narain does not talk about a landscape, but it was very clear (at least to me) that there were infinitely many string theories in 4 dimensions.

8. **Thomas Love**
May 17, 2006

I should have left an example:

http://redshift.vif.com/JournalFiles/V13NO2PDF/V13N2DMI.pdf

9. **woit**
May 17, 2006

Please, no more about current theories of the aether. The landscape is depressing enough.

10. **rof**
May 17, 2006

The chicken-egg “paradox” ways always trivial to solve. The egg came first. Dinosaurs laid eggs.

11. **LDM**
May 17, 2006

Lubus: I would hope you have enough knowledge of PHYSICS to realize your statement: “Today, all of us – except for those in loop quantum gravity – know that the aether is a silliness that is not realized physically and that was never well motivated” is just nonsense.

If , as in those days, you believed in Maxwell and the wave equation, it was a VERY well motivated question to ask ‘what is it that is waving’ when light waves (not photons) propagate.

Einstein realized that in General Relativity, the notion of an aether was perhaps not so simple and not so easily dismissed.

But, out repect for Woit’s request for no more aether discussion, you will have to excercise your own initiative to discover what Einstein thought in that regard.

12. **priority**
May 18, 2006

dear Schellekens, Woit and Susskind,

I realized that string theory is useless when I was a student. My priority is clearly proofed by the fact that I do not have any stringy publication, not even a blog.

13. **Thomas Larsson**
May 18, 2006

    The Michelson-Morley experiment was performed in 1887, and Einstein formulated SR in 1905. However, H A Lorentz kept believing in the aether long after that.

    Even if LHC becomes the Michelson-Morley experiment of supersymmetry, many people will continue to believe in SUSY and strings for the rest of their lives. (Note: a real prediction 🙂)

14. **The Great Landscaper**
May 18, 2006

    Lubos: your agreement with Woit denotes deviationism. In order to save you from becoming the Gorbachev of String Theory, you are transferred from Harvard. Your next position will be in Kolyma, Siberia.

15. **Lubos Motl**
May 18, 2006

    LDM: "If, as in those days, you believed in Maxwell and the wave equation, it was a VERY well motivated question to ask 'what is it that is waving' when light waves (not photons) propagate."

    Dear LDM,

    my feeling is that I have explained rather clearly something that everyone in the field knows anyway – that the question "what is waving" was NEVER motivated and only the people who misunderstand not only relativity but also the rudimentary philosophy of physics can say otherwise in 2006.

    The aether is a gigantic hoax, nonsense, and it has always been one. The only positive thing about the aether is that some of the people who believed it in the 19th century have also done some extremely serious physics, unlike most of their followers in the 20th and 21st century.

    The reality of Nature is encoded in the set of mathematical equations – Maxwell's equations in this case – and they are the full story. Naive mechanistic ways to imagine "what is waving" should only be created for children in the kindergarden or other people who have some intellectual limitations that prevent them from understanding that equations themselves can be and are fundamental and that fields can live in empty vacuum. Yes, there are gummi bears everywhere that are waving. The gummi bears must be there because some kids can't live without them.

    Adult physicists should be able to live without gummi bears. The electric and magnetic vectors exist directly at each point of vacuum, without any substrate, and it must be so, otherwise the basic postulates of special relativity would be compromised.

    Dear Landscaper, your landscape is far from being the only reason why I plan something along your lines.

    All the best
    Lubos

16. **Bert Schroer**
May 18, 2006

    What about the return of the aether in the veil of a "noncommutative" selection of a particular spacetime reference frame?

17. **urs**
May 18, 2006

    A simple change of terminology can affect the way we think about one and the same issue.

    The term "space of CFTs (with certain properties)" hardly induces the same passion as "landscape of vacua (with certain properties)" does, does it?

    At some point it would be great if we really knew what the "space of CFTs" actually is.

    For the baby example of rational CFTs we sort of do.

    Choosing deliberately unusual terminology we can reformulate a well known result as follows.

    The landscape of rational CFTs is the category of lax functors from quivers into modular tensor categories.

18. **woit**
May 18, 2006

    Any more of this and I put the word "aether" in the kill-file that will automatically delete any comment containing this word…

19. **woit**
May 18, 2006

    Urs,
    Mystifying with obscure mathematical terminology the fact that string theory has failed as an idea about unification because it is compatible with anything is not going to improve the situation.

20. **Lubos Motl**
May 18, 2006

    Dear Bert,

    the noncommutative parameter may break the Lorentz symmetry, like aether. It also breaks the rotational symmetry, unlike aether. Noncommutativity carries no entropy, unlike aether. It has no nontrivial microscopic structure, unlike aether. A noncommutative parameter may be viewed as a spontaneous symmetry violation in a theory that preserves the symmetry – and the original theory already carries all the excitations, unlike the space without aether.

To summarize: noncommutativity is not aether.

Let me be more general about the unmotivated ideas that can be classified as sick philosophical preconceptions. Take the sound and light. Sound is made of waves in the air. Is it natural to expect the same thing from light?

The answer is No. We only conjectured that there was a material that is waving – whenever there is sound – because we wanted to be able to:

* explain that sound can have different speed and other properties in different environments and different inertial frames; the extra material whose properties can change helps

* unify sound with some previous physical theories that rely on the concept of the air, such as the theory of breathing and the theory of winds 😉

Neither of these motivations existed in the case of light. Light has the same properties in each point of vacuum, and as one can figure out by analyzing Maxwell's equations, it has also the same properties in all inertial reference frames.

The second point is not applicable either because there is no other experimental reason to think that there exists something such as the aether; for example, there is no aether wind. To summarize, the aether does not explain anything and it violates the knowledge of physics at the end of 19th century. It was never motivated. It was always a philosophical dogma – a stupidity.

Something that Einstein realized pretty well which allowed him to overcome the limitations of other physicists of his time and the limitations of crackpots.

The same thing holds for dozens of other fantasies that billions of people believe for purely irrational reasons. For example, hidden variables. Much like the case of the aether, there are absolutely no indications that the correct predictions need some extra garbage (hidden variables) or that they depend on some additional assumptions that could depend on the position in space. Quantum mechanics works the same way in the whole Universe.

In both cases, we have some obviously universal laws – Maxwell's equations with the principle of relativity and the probabilistic character of physical predictions – that seem to hold in the same Universe. This always strongly indicated, to any rational person, that it was a very bad idea to try to find a more "microscopic mechanism" for these phenomena. Any new structure that one adds beneath these systems – Maxwell's equations or the probabilistic structure of quantum mechanics – will ruin the universal validity of these laws.

Of course, today we have much more specific ways to prove that hidden variables can't exist (unless we want to believe that locality is just a gigantic cosmic hoax and unexplainable conspiracy) – but the previous paragraph was meant to settle down the fact that aether or hidden variables were never motivated by scientific arguments – they were always stupid dogmas of philosophers who wanted to impose their naive ideas on Nature.

But she can make fun out of all people, not just the simpletons. It is scientists' task to listen to Her and see how She really looks like.

Best wishes
Lubos

21. **Lubos Motl**
May 18, 2006

Dear Peter Woit,

you have been saying these stupidities about the hypothetical "problems" in string theory for more than 20 years. You have been predicting that theoretical physics can be done without string theory for more than 20 years. Is not it time for you to admit that your predictions – and not only predictions, for that matter – have been a gigantic failure? How many more years do you need to see that what you keep on writing is just a pile of idiocies and that string theory is absolutely essential for any conceptual theoretical physics beyond the framework of quantum field theory? A century? A millenium?

Best wishes
Lubos

22. **woit**
May 18, 2006

Lubos,

Quite the opposite of what you say, I find more and more people all the time agreeing with my point of view about the problems of string theory and its failure as an idea about unification. My predictions about this failure made many years ago are in the process of coming true and being widely recognized.

23. **urs**
May 18, 2006

> Mystifying with obscure mathematical terminology the fact that string theory has failed as an idea about unification because it is compatible with anything is not going to improve the situation.

In contrast, the mystification is due to the fact that nobody really knows what the space of all CFTs is like in detail.

That's different for the rational case, and there what looks like obscure mathematical terminology to people unfamiliar with it is actually the lingua franca which makes things transparent.

24. **Thomas Love**
May 18, 2006

Sorry, but I must use the verboten word one last time. String theory has a very strong resemblance to the 19th century theory of vortex filaments in the e….

25. **Bert Schroer**
May 18, 2006

The descending back into chiral QFT (that is what Urs probably means by conformal QFT) does not give any autonomous support of the metaphorical interpretation of string theoretical target space. It is however true that the adaptation of the old Nelson-Symanzik duality to the chiral realm permits to demonstrate the existence of a temperature duality relation between a chiral thermal theory and its dual (which is also a thermal chiral theory of the same kind) of which the Verlinde relations are a special case (thermal duality of the zero-point function). Whereas in the case of Nelson-Symanzik the rigorous framework for duality was the Osterwalder-Schrader Euclideanization, the chiral situation (as a consequence of braid group statistics) requires a new more noncommutative Euclideanization (done in the setting of Tomita-

Takesaki modular operator theory). In this case one can even show that the temperature spacetime permits a spacetime interpretation (but not that in terms of a torus as a living space of a chiral theory, that remains metaphorical).
None of these new deep insights gives any support for the target space interpretation in string theory (basically because string theory only inherits those vacuum fluctuations from the lower dimensional source theory and has no autonomous target vacuum fluctuations). And this, Urs, is precisely the reason why string theory appears so metaphoric and eerie to your QFT colleagues (and to me) at the University of Hamburg.

26. **fh**
May 18, 2006

"string theory is absolutely essential for any conceptual theoretical physics beyond the framework of quantum field theory"

-LM

Whether or not it will be essential remains to be seen, as for now LQG provides an example of a (mathematically) well defined construction of alternative QFTs that in certain simple cases have been shown to be a generalization of flat spacetime QFT.

Whether or not you believe these extensions to be physically relevant is one thing, but it's simply incorrect that String theory is the only known way to go beyonf QFT. It's certainly the most advanced and has revealed the richest structures, but it's not the only option anymore.

27. **Bert Schroer**
May 18, 2006

To Lubos,
thanks for this nice semantic attempt.

28. **Lubos Motl**
May 18, 2006

Dear Peter,

you find more and more people because your blog attracts crackpots and people with serious intellectual limitations. There are roughly 4 billion people in the world who have no chance to comprehend anything that goes beyond classical physics. You're finding an increasing percentage of these 4 billion limited people because you are falling deeply in between average peasants in Namibia yourself. The more you will be falling, the more counterparts you will be meeting.

But that's not the community I am talking about.

I am reporting about real physics as done now in 2006, with which you have almost absolutely nothing to do. It is now clear that string theory as a framework is the only way how to transcend the limitations of GR and quantum field theories. Informed people know it, ignorants don't. Of course that the number of ignorants in the world exceeds the number of informed people, much like in the case of Darwin's evolution which – as you should already know by now – is completely analogous to string theory, but science is not democracy. Evolution and string theory are "must" despite billions of people who find the theories troubling.

Physicists who matter don't waste time with you and people who agree with you. Only I do because I was born into an even more ordinary anti-academic environment, compared to which you are relatively informed in theoretical physics, which is why I have the natural temptation to communicate these things because I am indeed troubled by the gap between actual science and what regular people think about it.

My goal was always to make not just myself but also everyone else to be more able to understand the right thinking that is necessary for science and that normally distinguishes scientists from laymen. Your goal has always been the opposite one: to confirm laymen that they're not missing anything and they should not learn anything, and even to transform relatively intelligent people into hopeless laymen who can't comprehend even things as elementary as the fact that quantum gravity can't be done without string theory.

Best
Lubos

29. **Dick Thompson**
May 18, 2006

****
the 10^80 vacua that, uniformly distributed, could cover all possible values of the standard model parameters to the accuracy that we can measure them.
****

Is this a recent way of expressing the problem? Can anyone give an online cite for it? Thanks.

30. **woit**
May 18, 2006

Lubos,

My blog isn't at all aimed at laymen, and I suspect a much smaller fraction of its readership is laymen than yours. The majority of connections to it come from academic machines with names like *.physics.wellknowninstitution.edu, often with "string" somewhere in the wild-card. The people who are reading it and sometimes agreeing, sometimes not, are your colleagues at Harvard, MIT, BU, Brandeis, etc., etc.

Virtually nobody in the physics community except you and a few other fanatics thinks that string theory and the theory of evolution are analogous.

Dick,

The number comes from Schellekens article, quoting Douglas, and it's probably in one of his landscape papers. It's a rough estimate, just taking all the parameters of the standard model and looking at the accuracy to which they are known.

31. **Krotos**
May 18, 2006

——-
"you find more and more people because your blog attracts crackpots and people with serious intellectual limitations. There are roughly 4 billion people in the world who have no chance to comprehend anything that goes beyond classical physics. You're finding an increasing percentage of these 4 billion limited people because you are

falling deeply in between average peasants in Namibia yourself. The more you will be falling, the more counterparts you will be meeting."
——-
Is this what passes for scientific discourse in the string theory community?

Glad I stayed in astrophysics.

32. **Johan Richter**
May 18, 2006

While I can not evaluate your general criticism of string theory,I think you are absolututely right to call the "anthropic" idea pseudo-science, Peter.

What percentage of string theorists do estimate share your and Lubos' view on that matter? Part of the problem seams to be that some legitimate science is also called "antropic". For example Weinbergs "prediction" of the CC seams to be legitimate science to me (as an uninformed layman), but as an empirical measurement and not as a theoretical prediction.

33. **Lubos Motl**
May 18, 2006

It's ridiculous that you have a higher percentage of academic physics readers. Last 100 visits (domains) from the last 80 minutes follow.

1
bris.ac.uk May 18 2006 11:34:38 am 1 0:27
2
link.com.eg 11:34:30 am 1 0:00
3
harvard.edu 11:34:27 am 1 0:00
4
wanadoo.nl 11:34:26 am 1 0:00
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sunysb.edu 11:33:02 am 1 0:00
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ucsb.edu 11:32:47 am 1 0:00
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unine.ch 11:31:26 am 1 0:00
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chalmers.se 11:29:42 am 1 0:00
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bu.edu 11:29:00 am 1 0:00
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karneval.cz 11:27:12 am 4 6:48
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193.11.195.# 10:46:30 am 1 0:00
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34. **woit**
May 18, 2006

Johan,

Based on anecdotal data and public votes at Strings 2005, I'd guess that older, more established string theorists split 50/50 pro and anti anthropic landscape, while younger people starting out are 4-5 to 1 against the landscape.

My take on the difference is that older string theorists who have worked on this for a while realize that there is no way to get a unique answer out of string theory, have given up on this, and realize that string theory implies the landscape and they have to accept it. Younger people who have swallowed lots of propaganda and not had enough time to learn just how bad the situation is, are still more optimistic about evading the landscape.

35. **Sam**
May 18, 2006

Schroer says:

"The descending back into chiral QFT (that is what Urs probably means by conformal QFT)"

No, Urs meant CONFORMAL QFT, specifically, rational conformal QFT.

"And this, Urs, is precisely the reason why string theory appears so metaphoric and eerie…"

Ooooooooh! Scary!

Oooooooooooh. Make it go away!

36. **Lubos Motl**
May 18, 2006

Dear Peter,

there exists no background for your unusual statements about the generation gap. But even if there were any generation gap like this, it's not terribly important. There have been many generation gaps in the past – and incidentally the younger generation was right in many cases, in sharp contradiction with your silly suggestion that the more old or senile someone is, the more likely is she to be right.

What's more important is that Nature is whatever She is. If a careful analysis of the physical laws implies that certain phenomena have a unique and deep justification, then we have to accept such a justification. If a careful analysis of Nature leads to the insight that some things in the Universe are essentially random, then they're random, and the opinion of Peter Woit and hundreds of those readers who can be counted as limited simply can't change this fact.

The only framework how we can ask these deep questions scientifically is the framework of string theory and everyone who knows something about insights of theoretical physics of the last 30 years, instead of just being brainwashed by low-brow weblogs of science-haters, realizes it very well.

This is the basic point about science that you are apparently uncapable to realize. You can't order Nature to look the way you would like. If you don't like quantum mechanics or string theory, it's your problem. If you really hate the idea that the world according to everything we can say follows these laws, that's too bad. Try to move into another Universe.

Best
Lubos

37. **urs**
May 18, 2006

> The descending back into chiral QFT (that is what Urs probably means by conformal QFT) does not give any autonomous support of the metaphorical interpretation of string theoretical target space

I mean full 2D CFT.

The landscape is nothing but the space of all 2D CFTs (with certain properties). This space is not well understood in detail. Except in the rational case. There we have a powerful theorem that tells us precisely what this space really is.

Concerning metaphorics: Target space is indeed metaphoric, in some sense, in perturbative string theory.

String Field Theory is an attempt to formulate the theory on target space. Some day people might find out how to rigorously formulate quantum SFT. Maybe. Currently all work in SFT is classical. (It's "first quantized", but classical at the "second quantized" level.)

When you demand rigorously constructed theories, string field theory does not exist for practical purposes.

But this is not what the discussion here is about. The discussion here is about the idea that correlators in certain CFTs are a "good" approximation to certain scattering amplitudes. (That's the premise of perturbative string theory – be that right or wrong).

38. **Bert Schroer**
May 18, 2006

These days the extremely special case of 2-dim. conformal QFT (and its two chiral components) is often called conformal QFT which leads to confusione with higher dimensional conformal QFT. Whereas it is true that one knows less about higher dimensional conformal QFT, it is in no way less important/interesting.
The Maldacena conjecture starts with a alleged 4-dim. conformal QFT (to be more precise beta=0, which is the prerequisite (necessary, but not sufficient) for CQFT). There are by now convincing arguments that the landscape (the old fashioned meaning) of higher-dimensional QFT is at least as rich as that for 2.dim. (Todorov, Rehren, Nicolov…) and of course each one of those models has an interesting AdS counterpart. The correct reading of Maldacenas conjecture is not that SuSyYM has a AdS encoding, since there is a structural theorem that this is the property of any CQFT. The sociological anomaly which lead to those thousands of papers is that he specifically claims that that rather simple encoding has anything to do with gravity.
Sam, I am not holding you back to feel cozy with string theory but there are people who know a lot more than you who do not share your feelings.

39. **Lubos Motl**
May 18, 2006

Dear Bert,

whoever claims to be a theoretical physicist in 2006 and who simultaneously believes that conformal field theories are not related to gravity in anti de Sitter space is a crackpot, and thousands of laymen who try to pretend that he's not a crackpot can't change the fact that he is a crackpot.

http://www.slac.stanford.edu/spires/find/hep?c=00203,2,231

Could you also find a technical problem with any of the 4000 papers that show that your idea is an undefendable one? Or do you realize that your "arguments" are only good for listeners who have no idea about physics?

Best wishes
Lubos

40. **Bert Schroer**
May 18, 2006

Urs
using your terminology "space", the space of any QFT is not understood, and among all such non-understood spaces (I would prefer the word models) the one of the 2-dim. CQFT setting is relatively well understood.
Let me also say that I appreciate your honest and on the whole correct answer.
But I honestly do not understand to which scattering amplitudes you are referring to (probably not those which Veneziano tried to understand with his dual model and which were abandoned by phenomenologists when it contradicted the data).

41. **Juan R.**
May 18, 2006

Lubos motl said,

“It is now clear that string theory as a framework is the only way how to transcend the limitations of GR and quantum field theories.”

That is partially true! In physics, string theory is so boring and ineffective that even cannot offer results offered from GR and QFT. In fact, string theory is useless for physical predictions. Moreover, it “predicts” tons of unobserved properties for Universe and is based in “many” incorrect technical points.

Where string theory goes beyond GR and QFT, in no doubt, is in marketing purposes, positions in academia, best-sellers for public, TV, interviews, grants…

String theory is also very useful in exoteric fields: multidimensions, aliens, time travelling, exotic brain behavior, telekinese, multiuniverses, God…

The contributions to marketing and exoteric fields is inversely proportional to contributions of string theory to real physics.

It is a kind of duality 😉

Juan R.

Center for CANONICAL |SCIENCE)

42. **Bert Schroer**
May 18, 2006

Dear Lubos,
I am a healthy sinner against the metaphorical messianic way in which you defend your beliefs.

43. **woit**
May 18, 2006

Lubos,

Yes, from your data, I do have a higher percentage of academic readers. During the last 80 minutes, they have come from three different machines in physics.harvard.edu, so my readers this hour include two of your colleagues.

The whole list is too long to include here, but the past 80 minutes include connections from (besides the harvard ones, and often there are multiple machines connecting from some of these domains)

jhuapl.edu
nottingham.ac.uk
dmi.unisa.it
u-psud.fr
het.brown.edu
physics.niu.edu
rz.uni-karlsruhe.de
hw.ac.uk
jyu.fi
hi.is
fy.chalmers.se
csudh.edu
unco.edu
math.uni-hamburg.de
iu-bremen.de
cmu.edu
dur.ac.uk
lsa.umich.edu
uwaterloo.ca
ist.utl.pt
ucsb.edu
physics.sunysb.edu
cpt.univ-mrs.fr
jpl.nasa.gov
kuleuven.ac.be
ulb.ac.be
temple.edu
upc.es
mpim-bonn.mpg.de
utexas.edu
fnal.gov
princeton.edu
ujf-grenoble.fr
ias.edu
if.usp.br (Hi Bert!)
physik.hu-berlin.de
dms.umontreal.ca
saclay.cea.fr
uiuc.edu
vanderbilt.edu
cam.ac.uk
gla.ac.uk
sinte.edu

theophys.kth.se
physics.nyu.edu
virginia.edu
ufl.edu
gravity.psu.edu
washington.edu
usc.es
na.infn.it
anu.edu.au
physics.mcgill.ca
jhu.edu
ecm.ub.es
physik.uni-regensburg.de
math.uu.se
insa-lyon.fr
mathematik.uni-leipzig.de
physik.fu-berlin.de
cta.br
ps.uci.edu
lut.ac.uk
duke.edu
physics.ucla.edu
efd.lth.se
utu.fi
ox.ac.uk
math.psu.edu
ucl.ac.uk
fisica.edu.uy
pa.uky.edu
math.ohio-state.edu
ed.ac.uk
bris.ac.uk

44. **MoveOnOr...**
May 18, 2006

Urs: "The landscape is nothing but the space of all 2D CFTs (with certain properties). This space is not well understood in detail. Except in the rational case. There we have a powerful theorem that tells us precisely what this space really is."

That's exactly off the point – this is how CFT people like you see the world 😉 The landscape is more than CFT (= perturbative string theory), because it encompasses also non-perturbative vacua, for which there is no world-sheet definition at all. For example, F-theory vacua form a very large class of vacua; those are intrinsically non-perturbative as the coupling constant varies over the internal space; I wouldn't know of any world-sheet like formulation! Similar for M-theory.

On the other hand, vacua based on completely different world-sheet theories (like heterotic, type II and type I strings), can give identical space-time theories… so in order to understand the nature of the landscape of string vacua (should it exist at all non-perturbatively), it is essential to step beyond string perturbation theory (=CFT) and especially rational CFT!

45. **Lubos Motl**
May 18, 2006

No, Peter, you can't prove anything about the percentage by choosing the academic connections only. What you're doing is not science.

Even in absolute terms, I have more visitors in the category in the last 80 minutes than you have.

My list only includes the first unique visitors per day. Here are those added in the last 60 minutes to the previous list. Also, if you look carefully at the lists, you will see that I got more harvard.edu visitors (5) in the same period, despite the fact that my counter only counts the first visitors per day.

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cybercity.dk May 18 2006 12:29:53 pm 1 0:00
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NYU.EDU 12:29:30 pm 2 0:30
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wvu.edu 12:29:03 pm 1 0:00
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46. **Bert Schroer**
May 18, 2006

The insinuation I intended with my remark about the missing autonomous vacuum polarization was among other things to blame those string vacua which do not communicate on this crucial but unfortunately missing property (with other words this is the limit of the metaphor).

47. **Lubos Motl**
May 18, 2006

Dear MoveOn,

I agree with you. There are many points in the configuration space of string theory that can't be uniquely reached from a perturbative description – from a 2D CFT. In this sense, the perturbative string theories described by CFT are just 5/6 of the boundary of the landscape, while the bulk of the landscape contains extra points. The fraction 5/6 is meant to convey the fact that M-theory with 11 large dimensions may be viewed as a boundary of the moduli space that has no description in terms of strings.

From the text above, one can argue that even if you include M-theory and F-theory vacua, you don't quite have the full "bulk" of the configuration space of the theory – all these things are just ways to approach the bulk from a kind of "boundary". The bulk contains points where all perturbative expansions are strongly coupled, the space is non-geometric, Planck/string-sized, and so on.

Best wishes
Lubos

48. **Anon-e-mus**
May 18, 2006

Great list, Lubos, but my **ck is still twice as long as yours! Get a life, kids. There are better things to argue about than “whose readers have bigger brains”.

49. **urs**
May 18, 2006

> The landscape is more than CFT (= perturbative string theory), because it encompasses also non-perturbative vacua, for which there is no world-sheet definition at all.

True. But that’s even less well understood.

(For sure, I wish I better understood it…)

One of the points I tried to make is that everybody is getting heated about the “landscape”, while its true nature is pretty elusive.

For instance, correct me if I am wrong, but isn’t it true that the tadpole cancellation conditions that are routinely checked in order to see if a background is consistent only guarantee a consistent effective field theory, not necessarily a consistent full worldsheet CFT with that target?

> it is essential to step beyond string perturbation theory (=CFT) and especially rational CFT!

No doubt. On the other hand, for doing the next step it is always helpful to have fully understood the previous one. The joy of toy examples, you know.

To be more precise, given that a full 2D TFT is the same thing as a Frobenius algebra in Vect, and given that full rational CFT is the same as a Frobenius algebra internal to a modular tensor category, it is not too daring to extrapolate and guess that, in the end, general full 2D CFT will be found to be the same as Frobenius algebras internal to something still a little more sophisticated.

50. **urs**
May 18, 2006

> using your terminology “space”, the space of any QFT is not understood,

For rational 2D CFT you should assume that space to have discrete topology, if that’s what you are alluding to. The deformation of any rational 2D CFT is non-rational, so all points in the “landscape of rational 2D CFTs” are isolated.

And we do understand what the nature of this space is like. Which of course does not mean we know precisely how to navigate it.

> But I honestly do not understand to which scattering amplitudes you are referring to (probably not those which Veneziano tried to understand with his dual model and which were abandoned by phenomenologists when it contradicted the data).

The basic premise of perturbative string theory is that you compute the scattering amplitude for incoming particles a,b,c and outgoing particles c,d,e by computing CFT correlators with insertions being some states corresponding to a,b,c and c,d,e. That’s what I mean.

51. **Bert Schroer**
May 18, 2006

Urs, but to call something an S-matrix, the least it has to deliver is the validity of cluster properties. Heisenberg’s 1943 proposal failed exactly on this point, the multi-particle scattering amplitudes from his S-operator model did not fulfill the asymptotic multiparticle cluster factorization. Any S-matrix, independent of whether it comes from QFT, string theory or any other physically interpretable particle physics theory, must fulfill this property. For the S-matrices coming from QFT it is easy to see that this property holds.
The other day I was asking for a proof in the setting of string theory and got no answer. Can you give me an answer?

52. **Mentos**
May 18, 2006

“The other day I was asking for a proof in the setting of string theory and got no answer.”

You got an answer. You just didn’t like it.

The tree level N-particle S-matrix has only simple poles, whose residues are given by products of lower-point tree level S-matrices. At loops, you start getting cuts, and unstable resonances develop finite widths. And, again, in appropriate limits, the g-loop S-matrices factorize onto lower-point (and lower-loop) S-matrices.

All of these reproduce precisely the required analytic properties of the perturbative N-particle S-matrix. And they follow from axiomatic properties of 2D CFT (+ BRST invariance and the Frenkel-Garland-Zuckerman version of the No-Ghost Theorem).

If you find those arguments unsatisfactory, you have a simple opportunity to DISPROVE string theory by finding a counterexample. Compute, say, the 1-loop bosonic string N-particle S-matrix element for some process and show that it fails to have the required properties.

Since you seem to know so much about the subject, that should be easy for you …

And your “insinuation … about the missing autonomous vacuum polarization” makes as little sense now as it did then.

53. **Bert Schroer**
May 18, 2006

Mentos,
the one-particle structure (residua of poles) are related to the Stueckelberg causal rescattering and I appreciate your remark that there are at least some indications for this kind of timelike macrocausality (no timelike pre-cursors). But it is not addressing the cluster factorisation. And believe me I do not plan any sinister attack on string

theory, I genuinely want to know.
Concerning the lack of communication between the different string vacua, I made a suggestion (which, if I would work on string theory I probably could prove, it should not be too difficult to understand the consequences of the absence of direct target vacuum polarizations). But I just do not have enough physical confidence in order to work on problems on which string theorists failed to do their homework. Do you have an argument why against all physical intuition one acquired in particle physics this is so?
And let me add, I do not quite share Peter's belief that the present crisis can be all blamed on string theory; the latter is rather the most visible sign of a degeneration process in particle physics and a fundamental critique of the present situation must include those developments which prepared the fertile ground for the reception of string theory.

54. **Mentos**
May 18, 2006

"But it is not addressing the cluster factorisation."

Aside from the analyticity, crossing, and factorization properties, what property of the S-matrix are you worried about?

"Concerning the lack of communication between the different string vacua,…"

I'm not sure what you mean by that. One of the things KKLT did was estimate the tunnelling rate between the metastable de Sitter vacua they found. They required (among other things) that the lifetime be comparable to or longer than the age of the universe.

For stable (supersymmetric) string vacua, there is, of course, no tunnelling.

But we don't have to go to string theory to see that you seem to have some confusion on this point.

Consider Seiberg-Witten theory: N=2 supersymmetric SU(2) Yang Mills Theory. This theory has a 1-complex dimensional moduli space of vacua. Is there "communication between the different vacua" in that theory? And, if not, what does that have to do with vacuum polarization?

Now turn on a mass for the adjoint chiral multiplet, breaking N=2 supersymmetry down to N=1. Whereas, formerly, there was a continuous infinity of vacua, now there are just two isolated vacua. Is there "communication between the two vacua" in that theory? And, if not, what does that have to do with vacuum polarization?

55. **Johan Richter**
May 18, 2006

One thing I have been wondering about is how much the average string theorist knows about the competeing alternatives. How many have themselves determined that string theory is the most promising approach and how many have simply trusted others opion?

56. **Bert Schroer**
May 18, 2006

O.K. Mentos,
I think the clustering of the string S-matrix can be achieved by definition. You simply use those pictures of those tubes and define disconnected amplitudes. There is still the problem to show that what you interpret the connected part really goes to zero when the wave packets of the particles become asymptotically spacelike separated. This is indeed a analytic property and what you say about tree diagrams has at least a certain plausibility.
Heisenberg's S-matrix models where given by a closed operator expression (the exponential of i times an hermitian operator which was a finite power series in free fields). In that case the cluster factorization failed (and probably cannot be obtaines by any such finite series). Lets hope that this will be argued in some future book on string theory but let us not come back to it in this blog.
Concerning the question of vacuum degeneracy you raised, I have no problem of understanding this issue in connection with spontaneaous symmetry breaking. In that case the theory can be decomposed so that the vacuum is unique and all the irreducible components are identical physical theories, i.e. if you have seen one you know them all. The case of the Schwinger-Higgs breaking which may lead to the Theta vacua is similar. In fact whenever you have several translation invariant states (vacua) you can without loss of generality reduce the situation to a theory with a unique vacuum. Is that what you mean "moduli"? This seems to be an expression which comes from a geometrical metaphoric picture of QFT to which I have no intrinsic access. Is there any way to understand this in QFT concepts without referring to big names?

57. **Mentos**
May 18, 2006

"Concerning the question of vacuum degeneracy you raised, I have no problem of understanding this issue in connection with spontaneaous symmetry breaking."

No.

In the Seiberg-Witten example, the different vacua corresponding to different points in the u-plane are inequivalent. This is not like spontaneous symmetry-breaking, where all of the vacua lead to equivalent physics.

"Is there any way to understand this in QFT concepts without referring to big names?"

You could start by reading their paper, where they solve the low-energy dynamics of this QFT. Or one of the many good reviews on the subject (which, of course, has many generalizations to other gauge groups and the addition of matter hypermultiplets in various representations of the gauge group).

58. **LDM**
May 18, 2006

Lubos:
Most of us are also familiar with the view you have expressed (Feynman was a proponent, maybe you got it from him, I don't know) that the mathematics — or the equations — are the full story. Einstein would probably not have believed this view.

Feynman also shares your disdain for hidden variables. (cf. Vol 3, Feynman Lectures, page 1-10 "Suppose we were to assume that inside the electron, there is some kind of machinary that determines where it is going to end up" etc)

However, you better be pretty sure that Feynman (or whoever it was you read or heard these ideas from) is actually right before you subscribe such absolute certaintity to these views, or as a scientitst, you may find that you miss out on some very interesting possible theories…just because you stopped looking deeper.

T' Hooft just 2 weeks ago published a very elegant hidden variable theory :
http://www.arxiv.org/quant-ph/0604008

Perhaps you can explain to T' Hooft that he is a crackpot…
Anyway Lubos, you will never become a physicist until you start to think for yourself.

59. **Bert Schroer**
May 18, 2006

Before I retire for today one last question which originates in Urs's claims.
On classyifying 2-dim QFT one certainly can have different settings. My favorite one is the theorem (Guido-Longo-Wiesbrock math-ph/9703129) that the physical classification of chiral conformal QFT (including the localizable "meat" and not just the topological "bones" and also not only the rational ones) is "isomorphic to the isomorphism classes of standard modular inclusions" (modular in the sense of Tomita-Takesaki modular theory and not in the sense of modular forms), the latter being a clealy defined mathematical problem.
Your Frobenius algebra classification may achieve the same thing. But I fail to see any indication of a unifying universal "space". Are you thinking of an analogon of a regular representation for compact groups? What brings an expert of classification of chiral theories to believing in the existence of such a universal object? Please help me, I am completely lost.

60. **woit**
May 18, 2006

LDM,
Please discuss Lubos's views about things like hidden variables that have nothing to do with the posting with him over at his blog, not here.

61. **LDM**
May 18, 2006

Woit,

If one cannot have some reasonable freedom to discuss or intelligently pursue the ideas that are naturally part of a discussion or blog… because it is not exactly coincident with the original posting…well, it seems like an artificial restriction…but then again, it is your blog, and you can enforce any policies you like.

I will therefore honor your request and not be posting further on your site.

62. **anon**
May 18, 2006

Lubos has a hilarious account of a talk on the landscape by Vilenkin at Harvard.

Lubos:"Vilenkin said that by this single sentence, Nima has essentially scooped the speaker and presented the rest of the talk as Vilenkin planned it."

Vilenkin and Nima sound like two naked emperors flattering each other. "Nima, your new garments look so good on you". "Yours do too, Vilenkin"

63. **dan**
May 19, 2006

Peter, I am sympathetic to your concerns, that string theory suffers from problems, such as extracting predictions, and that too much of particle physics has been engrossed in this seemingly unpromising approach, but I am curious as to what research programs particle physicists and theoretical particle physics are pursuing that go beyond the standard model, but do not involve SUSY or string theory or higher dimensions, programs that are academic respectable, and not "crackpots". by "beyond the standard model" one example is what sort of unification do you prefer, as SUSY-string theory approach you reject, and other models, such as SU(5) which predict proton decay and magnetic monopoles, seemingly is contradicted by experiment and observation. other examples of going beyond the standard model might be predictions for high-energy physics, dark matter candidates, etc. do you think the graviton is a part of particle physics and if so, what are non-string methods of studying it?

64. **tsg**
May 19, 2006

"string theory is absolutely essential for any conceptual theoretical physics beyond the framework of quantum field theory"
"quantum gravity can't be done without string theory."

Lubos, can you post (or link to) some justification of these assertions — in terms that a typical physics grad student (not in high energy) could understand? Or is that impossible?

I don't have a dog in this fight, but so far in this discussion I haven't seen any justification for these claims — just the assertion that they're obvious.

65. **Aether**
May 19, 2006

To Thomas Larsson: LHC can kill more than supersymmetry; it can disproof the theoretical prejudice that the SM must be replaced by a theory that makes the Higgs mass naturally small. If LHC does so, some people will still study supersymetry, but no longer as a solution to the hierarchy problem.

Hopefully this will be faster than in the analogous case of the aether: one century ago there were no blogs ranting that ether dragging is not even wrong. (Sorry for having used the word aether)

66. **MathPhys**
May 19, 2006

I believe that W Taylor's attempts to extract information from the Landscape goes against the general picture that's very naturally emerging from string theory, namely that string theory makes no predictions at all.

67. **knotted string**
May 19, 2006

tsg,

Since Lubos can't or won't give a link to back up his assertion "quantum gravity can't be done without string theory," can I suggest he is referring to:

http://arxiv.org/abs/physics/0601218

Hope this is useful. 😉 Also, I want to say Peter is not trying to replace you with a crackpot, Lubos! He is just trying to stop you being one to save you wasting your life on pseudoscience. The same goes for other string theorists. 🙂

68. **anonymous**
May 19, 2006

tsg,

Jacques Distler wrote a nice post about the difficulties of quantum gravity a while back:

http://golem.ph.utexas.edu/~distler/blog/archives/000639.html

I'm not sure it meets your criterion "a typical physics grad student (not in high energy) could understand"; the requirement is that you at least understand the renormalization group.

The Reuter approach to QG, looking for a UV fixed point, makes sense from this point of view but is really difficult. I have no idea how LQG fits into this story.

69. **Mentos**
May 19, 2006

knotted string:

Somehow, I doubt Lubos was referring to a crackpot paper on the physics arxiv.

tsg:

Distler's post is better, and he has another one where he discusses Reuter et al (maybe the best hope for a quantum field theoretic approach to QG).

70. **urs**
May 19, 2006

Bert Schroer wrote (in http://www.math.columbia.edu/~woit/wordpress/?p=392#comment-10888)

> Before I retire for today one last question which originates in Urs's claims.
>
> […]

From the way the question is stated it seems to me that there is some sort of misunderstanding involved.

The theorem I was referring to says that a full rational 2D CFT is the same thing as a special symmetric Frobenius algebra object internal to the representation category of a chiral vertex algebra.

The claim is that this is the complete solution of the problem of solving the sewing constraints, given any chiral algebra and the spaces of conformal blocks obtained from these.

So, to say that again, the "space of rational 2D CFTs" is precisely the space of Morita classes of algebras internal to modular tensor categories. Since every perturbation of rational theories leads to a
a non-rational theory all the points in this space are isolated.

Therefore, as long as we are not talking about moduli stacks of RCFTs, we can pretty much think "set" instead of "space" here.

I have a more detailed discussion of the FRS theorem here:

http://golem.ph.utexas.edu/string/archives/000813.html

71. **Lee Smolin**
May 19, 2006

Dear Peter,

If i can return this thread to its original subject, the earliest discussion of a crisis in predictibility from a vast proliferation of string vacua that I am aware of is in the conclusion of a 1986 paper by Strominger, in which he says, in part

"the class of supersymmetric superstring compactifications has been enormously enlarged…..it does not seem likely that [these] solutions……can be classified in the foreseeable future. … All predictive power seems to have been lost."

– A. Strominger, "Superstrings with Torsion," Nuclear Physics B, 274(2):253-284 (1986).

This is a highly cited paper, which makes it curious that its main conclusion was ignored.

As far as the concept and the term of a landscape of theories, to my knowledge, they originated in my papers in the 1990's on cosmological natural selection, and my 1997 book, Life of the Cosmos. For example gr-qc/9505022 and Classical and Quantum Gravity 9 (1992) 173-191. I do not take credit for the anthropic version, the whole point of my idea was to solve the crisis of too many string vacua that was apparent already then by inventing a falsifiable scenario that avioded the anthropic principle (which it was clear then could only lead to a dead end.) I chose the term "landscape" to allude to the well known biological term "fitness landscape" in order to make the point that the resolution of the crisis has to follow the methology used in biology-of being based on a mechanism that makes our universe a typical member of the ensemble- if it were to be testible scientifically. I find it bizzare that people have taken the term, and used it both without proper attribution and without recognition of its connotations- which were exactly to avoid the use of the anthropic argument.

Thanks,

Lee

72. **Thomas Larsson**
May 19, 2006

Two comments:

In an early discussion on sps, before that newsgroup effectively died, Wolfgang Lerche also seemed to claim anthropic credit for the Lerche-Lust-Schellenkens papers from around 1987.

I read a preprint by Schellenkens in the early 1990s, maybe on Leech or Nijmeier lattices, whose beginning I still remember (roughly): "It is clear that string theory contains all of mathematics. The only question is whether it also contains all of physics." Positively Motlerian.

73. **island**
May 19, 2006

> Lee Smolin said:
> I find it bizzare that people have taken the term, and used it both without proper attribution and without recognition of its connotations-which were exactly to avoid the use of the anthropic argument.

Didn't you know, Lee?… the landscape is now synonymous with the anthropic principle and has been ever since Lenny's book came out.

Lumo:
At any rate, I don't think that the originator of the anthropic idea has anything to be proud about…

Duh… that would be, Brandon Carter, after a bunch of respectable physicists independently came to a similar conclusion after running into the same brick wall that stops science dead in it's tracks to this day…

OHhhhhhhh… you meant the Stringy Anthropic Landscape Principle… Sorry, I forgot that you take string theory for granted without justification, like any good crackpot would.

~

Thankyou, Lee… I thought that I was going to burst…

74. **Bert Schroer**
May 19, 2006

It seems that these days the priority fights in particle physics are increasing in intensity the closer they are to trash-cans. Its pathetic!

75. **JC**
May 19, 2006

It would be interesting to see if there were other episodes in physics history, where there were heated priority fights over a "trash can".

76. **hack**
May 19, 2006

Funny, just before reading the above I was tossing around this image in my head of Susskind and Schellekens as two bums fighting over a cardboard box.

77. **JLM**
May 19, 2006

>Funny, just before reading the above I was tossing around this image in my head of Susskind and Schellekens as two bums fighting over a cardboard box.

Funny, really, that a bunch of people with little or no awareness or appreciation of his many contributions to physics (Kogut-Susskind fermions, technicolor, the Fischler-Susskind mechanism, black hole complementarity, the holographic principle, Matrix Theory, …) should be busying themselves making fun of Susskind.

Pretty much sums up everything wrong with so-called "physics blogs."

78. **Not a Nobel Laureate**
May 19, 2006

Lubos Motl wrote:

"Could you also find a technical problem with any of the 4000 papers that show that your idea is an undefendable one?"

Science is not a democracy, the laws of nature are not determined by popular vote.

79. **Not a Nobel Laureate**
May 19, 2006

These arguments about string theory are no different than the arguments of warring priest during the Reformation and Counter-Reformation.

With string theory I predict that we're seeing the birth of a new type of religion,
a meta-mathetical theology.

80. **Neubrain**
May 19, 2006

Lubos and Peter,

it's amusing how you both rush headfirst into infantile disputes about web statistics when about 99.95% of the web fares better than both of your sites combined. Seriously, how can you call yourselves scientists and willingly engage in these childish internet shenanigans? Have you no shame or common sense? Shouldn't you be spending your time more productively, like on your research? Do yourselves a favor and unplug your internet connection for like a week and see how much work you can get done. Who knows, you might even fix string theory or show that it has an ounce of predictive value.

81. **Mentos**
May 19, 2006

” ‘Could you also find a technical problem with any of the 4000 papers that show that your idea is an undefendable one?’
Science is not a democracy, the laws of nature determined by popular vote. ”

No, it’s not.

Nor does it amount to mere he said/she said.

Do you have a single rational argument why Maldacena/Witten/Gubser-Klebanov-Polyakov and all of the various people who have checked, rechecked, generalized and extended their calculations are wrong and Bert Schroer is right?

This isn’t about a head count. It’s about the results of detailed calculations (by a host of very smart people).

Why don’t you sit down with Berenstein, Maldacena and Nastase’s paper (for instance), and figure out where the flaw is. That might give a little bit more force to your argument.

82. **MoveOnOrStayBehind**
May 20, 2006

Mateos:
“Consider Seiberg-Witten theory: N=2 supersymmetric SU(2) Yang Mills Theory. This theory has a 1-complex dimensional moduli space of vacua. Is there “communication between the different vacua” in that theory?”
..

“Why don’t you sit down with Berenstein, Maldacena and Nastase’s paper (for instance), and figure out where the flaw is.”

The kind of people you try to argue against here would not accept SYM as well-defined theory to start with. They even do not consider QCD as a well-defined or meaningful theory. It is completely pointless to fight with them about string theory – they are against everything that has happened after their own PhD thesis in the 50’s. They use this silly anti-string club here as a platform to propagate their dislike of what they since ever failed to grasp.

Fortunately, no serious researcher cares about this; the Standard Model has been developed irrespective of all objections by constructive field theorists, a (incompletely understood) theory of quantum gravity has been discovered that has lead to deep and far-reaching insights in black holes, and that has lead to surprises such as the AdS/CFT Correspondence. While one sort of people waste their time agonizing why all of this must be wrong, others just sit down and do the hard work, trying to improve our understanding.

83. **Mentos**
May 20, 2006

“The kind of people you try to argue against here would not accept SYM as well-defined theory to start with. They even do not consider QCD as a well-defined or meaningful theory. It is completely pointless to fight with them about string theory – they are against everything that has happened after their own PhD thesis in the 50’s.”

Which lead one to wonder why Peter (who, otherwise, seems desperate to be taken seriously) is so eager to promote their cause.

84. **Thomas Larsson**
May 20, 2006

T’ Hooft just 2 weeks ago published a very elegant hidden variable theory :
http://www.arxiv.org/quant-ph/0604008

Perhaps you can explain to T’ Hooft that he is a crackpot…

LDM, ‘t Hooft is undoubtedly aware that his position on Planck-scale determinism is very controversial, and that anybody talking about hidden variables without being a Nobel laureate would immediately receive 10 crackpoints for
“9 .10 points for each claim that quantum mechanics is fundamentally misguided (without good evidence). .

I was first quite bemused about ‘t Hooft’s position, but then I realized that his motivation was to combine background independence with locality; this is a serious problem, because there are no local observables in conventional quantum gravity. This motivation makes a lot of sense to me, but I still think that ‘t Hooft is wrong. This is because there is another way to combine locality with diffeomorphism symmetry, which involves observer dependence and diff anomalies.

85. **Chris Oakley**
May 20, 2006

MoveOnOrStayBehind & Mentos,

You people are missing the point, perhaps deliberately. Neither Peter nor the majority of the “silly anti-string club” here are saying that Superstring theory should be abandoned. It is a possible approach. We just think that the effort expended on this quest is disproportionate to the results obtained and it is high time that more effort is devoted to alternatives rather than propping up this failed project. A bit more honesty from the Superstring community would help. If after more than 20 years of effort you cannot calculate cross sections then either (i) you should stop calling what you are doing physics or (ii) you should give it up. I doubt very much if Peter or the rest of us “silly” anti-stringers would have a problem with 10% of the current Superstring community carrying on on the basis of option (i), but to have almost all alternative strategies squeezed out of theoretical physics just because the majority are embarked on this Mission from some 10/11-dimensional God is not acceptable, and none of us are going to shut up about it until the balance is redressed in favour of that which looks more like empirical science.

The whole research process is much more haphazard than you people would like to believe. Whilst I cannot say that I would necessarily bet my life on Schroer’s algebraic field theory or Peter’s geometric quantization delivering the goods (any more than they would bet on my pet projects), I am happy for them to keep working on these and accept the possibility that one or other of them will be right and I wrong. I don’t feel threatened by them and I don’t feel that I have to subscribe to either of their approaches because of peer pressure. This is the way it should be, but then I am not in a position where I need anyone in the academic establishment to like me. For those that do need acceptance, the system at the moment is set up to force them to follow directions they do not necessarily want to go in just because of the unwholesome domination of one particular line of enquiry. This is not good for the subject.

86. **Not a Nobel Laureate**
May 20, 2006

” ‘Could you also find a technical problem with any of the 4000 papers that show that your idea is an undefendable one?’
Science is not a democracy, the laws of nature determined by popular vote. ”

No, it’s not.

Nor does it amount to mere he said/she said.

“Do you have a single rational argument why Maldacena/Witten/Gubser-Klebanov-Polyakov and all of the various people who have checked, rechecked, generalized and extended their calculations are wrong and Bert Schroer is right?”

Sure. Can you point to a single testable prediction about our physical universe to come out of “Maldacena/Witten/Gubser-Klebanov-Polyakov” et al. Otherwise it’s just a lot of mathematical masturbation by some (admittedly clever) people, but that’s all it is.

That the real problem and issue that physicists has with string meta-theory.

Not whether someone’s calcualtions are “right”.

Once again that level of discussion here brings Kissenger’s bon mot to mind.

“Why are academic debates so fierce, nasty and bitter?”
“Because the stakes are so low.”

87. **Not a Nobel Laureate**
May 20, 2006

“The kind of people you try to argue against here would not accept SYM as well-defined theory to start with. They even do not consider QCD as a well-defined or meaningful theory. It is completely pointless to fight with them about string theory – they are against everything that has happened after their own PhD thesis in the 50’s.”

This type of lame argument is know as the “straw man” argument – attributing statements and views/attributes to the other side that they did not make/have to begin with and then arguing against them.

The childness of the debate here, comparing the number and ID of IP addresses, the ad hominem personal attacks, etc. is symptomatic of how this field has degenerated in the absence of experimental data.

Despite the religious type claims by the string theorist that string meta-theory is the only possible way, the space all possible models is probably infinite and without experiments based in physical reality to kill off the incorrect models, the probability of any one approach is being right approaches 1/infinity.

Many of these approaches may lead to interesting math, but without testable predicitions, it’s not physics,
despite what any “authority figure” in the field may claim.

88. **Bert Schroer**
May 20, 2006

Mentos,
there is a deep misunderstanding on your part of the motives behind my critical remarks. I would be the first to uphold the right of Maldacena to write a speculative paper on an interesting subject; speculative freedom is an essential ingredients of theoretical physics.
What worries me is the apparent misunderstanding of thousands of his colleagues who completely ignore that
(a) there exists a general relation between CFT and AdS (a structural rigorous mathematical theorem of the kind you seem to be so afraid of, already in Fronsdal´s early work you find indications), it is a kind of different re-packing a la Christo of the same physical substance (if this helps to raise your awareness)
(b) there is absolutely no reason whatsoever to think that SUSY with beta=0 (the prerequisite for conformal invariance) is distinguished by its conformal invariance from infinitely many yet nameless other 4-dim. CQFTs; the conformal richness is not limited to low dimensions
These two points require to view Maldacena´s conjecture within a wider context since all conformal models possess a AdS re-packing and they are all potentially interesting. What I am saying is not new and can be found in the literature; but particle physics has been distorted by very unfortunate sociological developments and either people are not capable any more to see this or are afraid not to collect enough impact points if they are leaving the path of lemmings.
The most important ability of a theoretical physicist (totally different from a mathematician) is that he learns to live with halftruths in an imperfect world but without ever forgetting that the main aim is to make it more perfect.
Certainly when some mathematical physicist say that QED does not exist, this should not be interpreted naively. What it means is that our most successful theoretical Ansatz is not yet conceptually secured. The old Bohr-Sommerfeld QT was also quite successful, but just imagine what would have happened to physics if people would have been as complacent as they seem to be nowadays those which get into these semantic arguments with respect to those colleagues who point out that we (this includes QFT in its present state) live in a provisorial situation. In contrast to all other branches of physics which have been conceptually secured (e.g. by nontrivial examples), QFT and even more so string theory is still conceptually totally unsecured territory.
It was a blessing that the step from old to modern quantum mechanics took place within auch a short time; imagine the confusion it would have created und the many holy cows (like the above one) it would had led to, if several generation would have stepped on the old ideas and in this way solidified them?

89. **Benni**
May 20, 2006

Maybe Schellekens thinks of this paper here
http://ccdb4fs.kek.jp/cgi-bin/img/allpdf?198612321
Together with luest, written in 1986.
There, Luest who is now in Munich and Schellekens write

“It seems that not much is left from the celebrated uniqueness of String theory”.

The paper is from November 1986.

90. **wolfgang**
May 20, 2006

> We just think that the effort expended on this quest is disproportionate to the results obtained and it is high time that more effort is devoted to alternatives rather than propping up this failed project.

I never understood this argument.
Nobody prevents you, Peter, Lee, etc. to work on alternatives.
The internet even makes it zero cost to publish, promote and discuss such ideas and theories.
Do you suggest some sort of committee to determine who should work on what idea?

91. **Arun**
May 20, 2006

When significant professors allegedly say “string theory is the only game in town”, then, Wolfgang, what happens to the likelihood of a student to do something else? You may address this in either frequentist or Bayesian terms 🙂

92. **Bert Schroer**
May 20, 2006

No Wolfgang, things do not work in your simple-minded and probably honest way. Peter and other critical minds are too old and lack the stamina which you need to persue new innovative ideas. Their (and also my) accumulated insight and critical mind has left them with the necessary distance and enhanced power seeing through smoke screens and selfdilusions. It is a bit like the trainers in football, you probably would not suggest that they return as active players.
The people who (through distributions if impact credits) could have an influence on the present situation are the ones who are administering the present crisis e.g. in the editorial boards of new HEP journals and this is the best guaranty that particle physics will continue the same route for the next decades as it has done in the past two decades. If some novice has the intellectual capabilities and the courage to make a deep investment outside the prescribed line, he will pretty soon lose his material support; I have seen several such cases and I know what I am talking about.

93. **Mentos**
May 20, 2006

NaNL said:

“Sure. Can you point to a single testable prediction about our physical universe to come out of “Maldacena/Witten/Gubser-Klebanov-Polyakov” et al.”

One?

OK. How about the prediction (by Kovtun, Son and Starinets) that the ratio of shear viscosity to entropy density in the quark gluon plasma is 1/4pi (plus computable corrections). No other model, nor any known material substance has so low a ratio. But it’s in good agreement with what’s measured at RHIC.

There’s more (lots more) one could say about the importance of AdS/CFT. But you asked for ONE prediction. So that’s what I’ll give you.

Schroer said:

“These two points require to view Maldacena´s conjecture within a wider context since all conformal models possess a AdS re-packing and they are all potentially interesting.”

You’re joking, right? There’s a huge industry finding supergravity duals to other 4D field theories (both conformal and nonconformal).

But, as always, the dual of a 4D (C)QFT on the boundary is a gravitational theory in the bulk of AdS. The claim that one gets another (nongravitational) QFT in the bulk is just wrong.

“Certainly when some mathematical physicist say that QED does not exist, this should not be interpreted naively. What it means is that our most successful theoretical Ansatz is not yet conceptually secured.”

Not to anyone who has assimilated the lessons of Ken Wilson, Steve Weinberg and others.

It means that QED is an effective field theory, valid at low energies, but which must be replaced by some other, better-behaved effective theory at higher energies. (As, of course, happens in the real world.)

In another thread, you professed to take a Wilsonian attitude to QFT. But every off-hand comment of yours indicates that you really don’t believe the central lessons that Wilson taught us about QFT.

94. **Bert Schroer**
May 20, 2006

Mentos
I do believe the central lesson of Wilsonism but I do not think that he ever claimed to prove the existence of a consistent mathematical theory behind those ideas (existence of QED,…) and computations which come to our mind when we look intensly at a Euclidean action as a definition of a model. The word Wilsonian stands for me also for non-metaphoric and non messianic (i.e. far away from a final theory of everything), a meaning which you do not seem to share).
Concerning the various SUSY models I just counted them summarily as one (if you have seen one, you know them all). But if, as you seem to alledge, every of these expected infinitely many different families (which I had in mind) has also a corresponding gravity AdS (by that rather simple Christo-type of repacking, the only real nontrivial case which deserves the name holography is holography on Nullsurfaces) then I find this totally generic terminology “gravity” quite empty and uninteresting, despite the big names with which you all the time seem to try to create the feeling of awe.

95. **Mentos**
May 20, 2006

“then I find this totally generic terminology “gravity” quite empty and uninteresting,”

No, it’s the heart of the matter.

The most basic statement in the AdS/CFT correspondence is that, to every local operator in the boundary (C)QFT, there corresponds a field in the bulk AdS. Now, one of the hallmarks of QFT is the existence of a local, conserved stress tensor.

Can you guess what field that corresponds to in the bulk AdS theory?

“I do believe the central lesson of Wilsonism.”

Perhaps I should ask, “What lesson is that?”

96. **woit**
May 20, 2006

Wolfgang,

Sure, Lee and I can work on what we want to work on, but science is a collective enterprise, and it is far more difficult to make progress on a problem if you are the only one working on it.

There already are committees established that decide who should work on what idea. They're called hiring committees and I've sat on them. One of the main things hiring committees look at is the subject potential hires are working on: is it a "hot" subject the department wants to be a piece of? Many hiring committees are working with explicit guidance as to what ideas the person they will give a job to should be working on. I could go on about this, but for examples, you should consult Lee Smolin, who can give you numbers about how many academic institutions in the US over the last 20 years have been willing to even consider hiring somebody working on ideas about quantum gravity that are not string theory. I gather his forthcoming book has a lot more about this.

97. **wolfgang**
May 20, 2006

Prof. Schroer, Arun, Peter,

I guess we can agree on the following:
i) quite a substantial amount of effort and resources has been spent already on quantum gravity outside of string theory. It is safe to assume that this will continue. Just check the gr-qc section on the arXiv.
ii) If somebody wants to write a thesis about LQG, lattice gravity, or whatever he/she will not have a problem to find an advisor.
iii) There is certainly a difference in funding for string theory vs. other approaches, which is easily explained by the simple fact that string theory was and still is the most promising approach so far.

But none of this has anything to do with my point, which is that students, postdocs, professors etc. are free to explore whatever they want. I am sure each one of them will try to optimize his/her own risk/reward ratio. If you are a "low-risk person" use the main road (string theory), if you are a "longshot person" try causal sets, algebraic qft, LQG, etc.

98. **Bert Schroer**
May 20, 2006

Mentos,
I am sorry, but if such a simple re-packing (a change of the spacetime encoding of any algebraic substrate which was given in the conformal spacetime organization and which is being equipped with a different AdS one) as the one from an AdS to its asymptotic timelike brane (trivial because it keeps all the symmetries and modulo some simple amendments also the causal structure (which a holography onto a Null-horizon definitely does not maintain!!)) creates inexorably something which you generically call "gravity" then you have ruined the magic which many people still expect behind Maldacena, and I wash my hands free of guilt.
Wilsonism to me means that I do not have to know all the world (to the Planck length and beyond), rather I am able to make a conceptually closed theory in the present situation of QFT (and, if I am allowed to add this, also a mathematically controllable one) which is free of cutoffs and elementary length.

99. **Bert Schroer**
May 20, 2006

To Wolfgang,
on the whole I agree, but let me point out to you in all modesty that somebody (who collaborated with me) who has done the pioneering work on modular operator theory on which the ball of QFT got rolling again (and something you probably will notice in the near future) had no career chance against any second rate string theorists. Since you seem to be a string theorist, you probably only see those in the present light and not those in the dark.

100. **Mike**
May 20, 2006

This long discussion about the landscape further emphasizes a few points,

island Says:

" Lee Smolin said:
'I find it bizzare that people have taken the term, and used it both without proper attribution and without recognition of its connotations-which were exactly to avoid the use of the anthropic argument. '

Didn't you know, Lee?… the landscape is now synonymous with the anthropic principle and has been ever since Lenny's book came out."

Only a few years ago, this was posted:

The Beginning of the End of the Anthropic Principle

"We argue that if string theory as an approach to the fundamental laws of physics is correct, then there is almost no room for anthropic arguments in cosmology. The quark and lepton masses and interaction strengths are determined."

Gordon L. Kane, Malcolm J. Perry, Anna N. Zytkow
arxiv.org/abs/astro-ph/0001197

dan Says:

"Peter, I am sympathetic to your concerns, that string theory suffers from problems, such as extracting predictions, and that too much of particle physics has been engrossed in this seemingly unpromising approach, but I am curious as to what research programs particle physicists and theoretical particle physics are pursuing that go beyond the standard model, but do not involve SUSY or string theory or higher dimensions,"

I'm asking this question too, and some answers have been given,

Chris Oakley Says:
"Schroer's algebraic field theory or Peter's geometric quantization…"

BF theory has been mentioned before too,

And Mentos Says:
"QED is an effective field theory, valid at low energies, but which must be replaced by some other, better-behaved effective theory at higher energies."

An attempt to correct QED has also been made by new work with modified Maxwell equations.

While Pauli was the critic who said “Not even wrong”, he was also a leader in searching for new physics, and made contributions regardless of age.

101. **wolfgang**
May 20, 2006

Dear Prof. Schroer,

> Since you seem to be a string theorist

I am not.
Most of my effort was on lattice gravity and I am currently not an ‘active researcher’ 😎

102. **Lee Smolin**
May 20, 2006

Since Peter asked, let me just introduce some objectivity into the discussion about the consequences of choosing to work on an approach to quantum gravity apart from string theory. In the US now there is a single research group with more than one faculty member working on non-string quantum gravity; at Penn State it has one senior and two junior faculty. Apart from Penn State, and a single person who left Penn State and got a position largely on the basis of his work in another field, the last time there was a new faculty position in the US for someone working on a non-string approach to quantum gravity was 1990. There are at most 4-5 NSF funded postdocs now in the US that a non-string quantum gravity person might apply for. The situation is slightly better in Canada, Mexico, and a few European countries, but the situation is that there is no graduate student or postdoc-even the stars with widely read and admired single authored papers-who has an easy or assured career.

The situation would be vastly improved if there were open competition on the basis of quality, originality and promise for the large number of postdoc and faculty positions controlled by string theorists. But I am unaware of a single instance of a string theory group hiring a postdoc or faculty member in any other approach to quantum gravity, in spite of the fact that this has happened in reverse several times- because the ethic in non-string quantum gravity is to choose on the basis of quality and individual promise, whereas the string theorists seem uninterested in applicants who do not work in the mainstream of string theory.

As far as someone wanting to do a Ph.D. in non-string quantum gravity, there are many and indeed the number of applicants is increasing dramatically because of the visibility of recent important results. But there are very few places in the few groups around the world where this work is done. We literally turn away good applicants weekly who apply to our group. As a result, an increasing number of very promising students are doing PhDs in non-string quantum gravity on their own without the benefit of an advisor in the field.

The only advantage of this is that the few young people who persevere against these odds have visibly much more creativity, intellectual independence and courage than their counterparts in trendy, mainstream fields. So they do better science, and indeed young people are responsible for the bulk of the new results and ideas which have driven the fast rate of progress of recent years. So it is getting increasingly evident that their exclusion from consideration for the best positions cannot be justified on any objective scientific basis.

And yes, my forthcoming book is not an attack on string theory, it is an examination of how this kind of situation can develop, which hurts not just many of the best young researchers but the progress of science itself.

103. **wolfgang**
May 20, 2006

Lee,

I am certainly not up to date and probably misunderstood your comment about the ‘only one research group’ in the US.
Without Google’s help I can name at least three groups in the US,
around Raphael Sorkin at Syracuse, Herbert Hamber at Irvine and John Baez at UC Riverside. I am sure there are more … certainly in Europe.

As for “there is no graduate student or postdoc-even the stars with widely read and admired single authored papers-who has an easy or assured career.”
This is certainly true for all students and postdocs. I doubt that string theory guarantees a career …

104. **Aaron Bergman**
May 20, 2006

“I doubt that string theory guarantees a career …”

Heh.

105. **woit**
May 20, 2006

Wolfgang and Aaron,

You’re not addressing what Lee wrote. He wasn’t discussing generic students or postdocs, but the top ones in the field:

“stars with widely read and admired single authored papers”

If you fit this description in string theory you are guaranteed a career. If you fit this description in non-string quantum gravity Lee is claiming you may have problems getting a job. Argue with him about what he is saying, not straw man arguments.

106. **Mentos**
May 20, 2006

Schroer said:

” [blah blah blah]… creates inexorably something which you generically call “gravity” then you have ruined the magic which many people still expect behind Maldacena, and I wash my hands free of guilt.”

I explained why the AdS dual of a (nongravitational) QFT necessarily involves gravity in the bulk. You can wash your hands, if you wish. Won’t change a thing.

“Wilsonism to me means that … I am able to make a conceptually closed theory in the present situation of QFT … which is free of cutoffs and elementary length.”

To the contrary, in Wilson’s approach, QFTs always come equiped with a cutoff. In his framework, there is no “conceptually closed theory … which is free of cutoffs.”

The point is to, nonethless, extract cutoff-independent answers from an inherently cutoff-dependent formalism.

Your AQFT approach is about as anti-Wilsonian as one can ever be.

107. **Lee Smolin**
May 20, 2006

Dear Wolfgang, I chose my words carefully. Raphael Sorkin at Syracuse, Herbert Hamber at Irvine, John Baez at UC Riverside, and a few others (Steve Carlip, Louis Crane, Ted Jacobson, Jorge Pullin, Bob Wald) are doing important work but they are single faculty members. As good as they are, by current NSF rules they are not always able to support a postdoc, as single faculty members are rarely given postdocs.

It is a bit better in a few European and Latin American countries, as I said, whose systems are structured so that there are a few good positions where the competition is in terms of individual accomplishment and promise without regard to research program or subfield.

Thanks,

Lee

108. **wolfgang**
May 20, 2006

Lee,

I see your point. But I assume that in the long run funding and interest in general will depend on results.
The results of Witten, Maldacena etc. made string theory interesting.
If Reuters et al. can demonstrate the existence of a UV fixed point, Loll et al. can show that CDT reproduces GR etc. this would certainly increase interest and funding.
Ultimately, experiments are the most interesting results. If the LHC and other experiments will not provide evidence for super particles the interest in superstrings and subsequently funding will probably decline.
If GLAST provides evidence for LQG you will probably have an easier life 😎

Unfortunately, overall the interest in theoretical physics and the willingness to fund it is declining (at least in the US), since politicians understand that the 1940s and 1950s will not repeat …

109. **wolfgang**
May 20, 2006

I am sorry: Reuter not Reuters

110. **Juan R.**
May 21, 2006

Wait a moment Wolfgang, are you claiming that string theory is more promising or may be funding before other approaches because technical points?

If yes, let me say that string theorists have proved absolutely nothing in last decades. Nobody have proved finiteness of perturbative string theory (it is claimed that solves all renormalization issues of QFT on gravity but i never saw a proof) and still today nobody has proven that string theory coincides with GR in the large scale low energy regime.

Even asuming backgrounds and large spacetimes by default, even asuming some compactification of extra dimensions, the result is a perturbative expansion over a flat metric. GR is NOT that.

About the LHC and other experiments, i can assure you that string theory hype will remain independently of the experimental results. There is historical evidence for such one attitude in the string comunity. Exactly 40 years of successive experimental failures of "predictions" of string theory. One of most recent were supposed cosmic strings…

Let me remark that problem of super partners is not if they are there (at high energies) but they are NOT here (at current energies) and string theory fails to provide a low energy regime without super partners. That is one of reasons string theory is unable to reproduce the standard model results.

Any new theory of physics may be backward compatible with experimental results known. Before to claim about it will be seen in next HLC, stringers would explain data is already known and explained by GR + SM.

Juan R.

Center for CANONICAL |SCIENCE)

111. **MoveOnOrStayBehind**
May 21, 2006

To Oakley:
"You people are missing the point, perhaps deliberately.
Neither Peter nor the majority of the silly anti-string club here
are saying that Superstring theory should be abandoned.?

What? I have read innumerable times here that string theory is
"all wrong", "not even wrong", must be "given up", etc.

"… We just think that the effort expended on this quest is
disproportionate to the results obtained and it is high time that
more effort is devoted to alternatives rather than propping up this
failed project."

I would say that the most important, fascinating results in theoretical
particle physics and mathematical physics in the last decades came
right out of string theory. Holography, AdS/CFT, insights in
non-perturbative gauge theories, quantum black holes.., even Hawking

gave up his bet. Could you name any other field with remotely as
many important results?

And what are your alternatives? This sounds like if there would
be any. How would LQG help solving non-perturbative Yang-Mills
theory? How do you describe with that flat space, anyway? It's
an evil strategy of certain people to present things as if there
would be alternatives, such that as their own pet theories …
laymen such as you think that alternatives must be better just
because they contradict the mainstream, isn't it? Try to build
an alternative moon rocket out of wood…that's as silly.
Keep on trying.

"A bit more honesty from the Superstring community would help"

All the respectable collegues I know say what they think. A few
like Kaku grabbing media attention do not stand for the community.
The dishonesty is on your and your friends side, for presenting
such a distorted picture.

" If after more than 20 years of effort you cannot calculate cross
sections then either (i) you should stop calling what you are doing
physics or (ii) you should give it up."

Well I think I can compute cross sections. And thanks for a layman's
tip how I should call my work. What do you think brings you in the
position to make such proposals? Decades worth of hard work on your
own, or just out the arm chair?

"I doubt very much if Peter or the rest of us silly anti-stringers
would have a problem with 10% of the current Superstring community
carrying on on the basis of option (i)"

Well, I do certainly think that investigating the quantum behavior
of black holes and gauge theories is physics, isn't it?
And do you seriously think that LQG and other "alternative" approaches
you may propose would be more physical? Did it ever occur to you
that we first need to understand the physical concepts how things
work, in simplified settings, rather than trying to make direct
contact with reality? Most of the leading figures in the field do
pursue exactly this, because they know how difficult it is to make
contact with reality. It is simply not so that everybody would work
on "realistic" model building and the landscape – actually, among
the leading people it is a minority. And how should one then call
with your permission other research in mathematical physics, like
the one magnetic monopoles, algebraic QFT, integrable systems, which
also do not have a direct experimental confirmation?

"For those that do need acceptance, the system at the moment is set
up to force them to follow directions they do not necessarily want
to go in"

Right now, the pressure everywhere is to go into particle phenomenology.
Do you know what you are talking about?

112. **woit**
May 21, 2006

"What? I have read innumerable times here that string theory is
"all wrong", "not even wrong", must be "given up", etc."

For the innumerable + 1 time: the idea of using strings in 10d (or 11d M-theory, whatever that is) to unify the standard model and gravity has failed miserably, is "all wrong", "not even wrong", must be "given up", etc. More than twenty years of work by much of the theory community thinking about strings has led to interesting insights about strongly coupled gauge theory, about enumerative problems in algebraic geometry, and speculative ideas about quantum gravity. Many of these latter things are worth pursuing, but so far they have given no insight into beyond standard model particle physics. The claim that research in string theory is a promising approach to beyond standard model particle physics is not just made by Michio Kaku, it continues to be made by almost every string theorist who gives talks to a non-specialist audience on the subject. This raises serious issues of intellectual honesty.

I don't doubt that most of the people who make these arguments are making them honestly in the sense that they believe them. I do question whether they are being intellectually honest: are they willing to actually confront the seriousness of the problems string theory faces and draw the conclusions that follow from them, even if this is painful? I don't see many string theorists willing to do this. Instead I see them doing things like anonymously posting here attacks on me and others in which they willfully ignore points I've made a thousand times that they have no answer for, making up straw man arguments they prefer to deal with.

113. **amused**
May 21, 2006

Lee Smolin wrote: "…the ethic in non-string quantum gravity is to choose on the basis of quality and individual promise, whereas the string theorists seem uninterested in applicants who do not work in the mainstream of string theory."

Facinating. Do tell us, Prof. Smolin, what are the concrete criteria you use to evaluate "quality and indvidual promise"? If a young person working independently on, say, formal aspects of gauge theories, including a certain topological gauge theory of relevance for LQG, were to apply to you for a postdoc, what would it take for you to hire him/her? How many single-author publications in Phys.Rev.Lett should this person have in order to satisfy your "quality and individual promise" criteria? (I happen to know that this number has a lower bound of 3.)

From what I have seen, postdoc applications from young people whose topic is neither strings nor LQG generate no more interest from LQG groups than they do from string groups (unless their topic happens to be a pet interest of Smolin, e.g. the so-called foundational approaches to QM etc.) Smolin's appeals, here and elsewhere, for jobs to be awarded on the basis of quality and promise rather than research topic, strike me as little more than an amusingly disingenious attempt to help his own people in the current string-dominated environment.

If people really were serious about promoting quality and promise irregardless of research topic, here is a way to do it: Remove hiring decisions from individual research groups (who will inevitably favour people working on their own topic), and individual physics dept.s (which will inevitably favour people working on fashionable topics, or who have famous thesis advisors etc, which will make the dept. look good), and let the decisions instead be made by large national committees whose members represent the whole spectrum of theoretical physics research. Research groups or individual physics dept.s can then sponsor the applications of people they would like to hire, but with the actual hiring decisions made by the national committee after an open competition. (This is basically the way things work for the EU's Marie Curie fellowships.) Would you be willing to hand over your individual hiring powers to a national committee in the interests of promoting quality/promise over reseach topic, Prof. Smolin?

[Apologies to Peter for continuing this discussion in an off-topic direction.]

114. **Chris Oakley**
May 21, 2006

MoveOnOrStayBehind,

I am not sure I like getting into arguments with people who refuse to identify themselves, but my experiences in the in the world of theoretical physics are described in detail on my web site (link above). I should point out that this "armchair" you refer to exists only in your imagination; I left physics because I had to and have been earning my living since then by doing financial modelling and programming. The job I have now at least gives me more time to think about other things, and when the dust settles, there is at least one significant piece of (original, as far as I know) theoretical physics work that I plan to revisit.

> I would say that the most important, fascinating results in theoretical particle physics and mathematical physics in the last decades came right out of string theory. Holography, AdS/CFT, insights in non-perturbative gauge theories, quantum black holes.., even Hawking gave up his bet.

Obviously your definition of the word "physics" is not the same as mine.

As for alternatives to ST, I have my own ideas. They do admittedly put me in a "minority of one" (as helpfully pointed out by a former HEP colleague), but they are ideas nonetheless and will be pursued when I have the leisure time to do so. And no, there is no quantum gravity component. I am not interested in building models when there is no experimental data.

As regards the Anthropic Landscape, I am sure that you are right in pointing out that most have not signed up for this. But even one is too many.

> Right now, the pressure everywhere is to go into particle phenomenology. Do you know what you are talking about?

Probably not … my information may well be out of date, but if many are voting with their feet then this may well be because they acknowledge that Peter has a point.

115. **Aaron Bergman**
May 21, 2006

It's not so much a matter of voting or "acknowledg[ing] that Peter has a point". Rather, it's that the LHC is turning on. Faced with the prospect of having actual data, people will naturally move from speculative stuff to stuff that's related to the incipient data. Really, Peter's criticisms aren't particularly new or original (see, for example, [Ginsparg and Glashow](#)), and the anthropic stuff remains a source of disagreement in the field.

I am always astounded that people are willing to declare something wrong based on metaphysical constraints. That string theory hasn't developed into a complete theory in 25 years or that it may have a zillion or so vacua doesn't make it wrong. Useless, at worst, but it's entirely possible that that's just how the universe (multiverse?) is.

And for the people disparaging the study of SUSY gauge theories and various other things that aren't the real world, the study of toy models has a long and useful history when the real problem is (presently) intractable.

116. **woit**
May 21, 2006

Aaron,

What's the difference between being "useless" and being wrong? This is what I cannot understand about what is going on today among string theorists. They seem willing to accept the possibility that the theory is useless in terms of predicting anything about physics, but unwilling to draw the obvious conclusion that if this is the case it is wrong.

117. **Who**
May 21, 2006

Mr. M.O.O.S. Behind wrote thusly:
"…And what are your alternatives? This sounds like if there would
be any. How would LQG help solving **non-perturbative Yang-Mills
theory?** How do you describe with that flat space, anyway? **It's
an evil strategy of certain people to present things as if there
would be alternatives,.."**
http://www.math.columbia.edu/~woit/wordpress/?p=392#comment-11000
Mr. Behind sir, in reply to your comment, you seem to use LQG as a convenient blanket term for alternatives. And I concede there is an identifiable LQG community (pursuing various related approaches to QG). My point is that at least one researcher in that community just posted something about the problem you mentioned "non-perturbative Yang-Mills". In case you would like a link, here is one:
http://arxiv.org/find/grp_physics/1/au:+freidel/0/1/0/all/0/1

In the comment you ask several rhetorical questions which make it seem that you are not very familiar with what you are talking about.

Your question **"How would you describe flat space anyway?"** is interesting (if taken non-rhetorically). If you try that link you can see how Freidel, for one, is addressing this question. His research is not unique in this respect but you may wish to do the author-search and check out some abstracts before making further pronouncements on the subject. That would also pick up the two papers on 4D Yang-Mills, one co-written with Robert Leigh and Djordje Minic.

I believe you would also find some papers there concerned with deriving flat space and the Feynmann diagrams of usual QFT from a spinfoam version of QG that comes under the general LQG heading. Perhaps in a simplified setting from which further work can generalize.

Mr. Behind, you ask another question that may be rhetorical here:

****And do you seriously think that LQG and other "alternative" approaches you may propose would be more physical?** Did it ever occur to you that we first need to understand the physical concepts how things work, in simplified settings, rather than trying to make direct contact with reality?…**

With all due respect, I do seriously think so. Yes, is the answer to your question, **they would and will be more physical.** Note that the alternative approaches try to get at fundamental degrees of freedom of spacetime and matter in a direct physical fashion, often in simplified settings (as you mentioned) and going light on extra baggage. And as one who watches the research scene I would say this strategy seems to be paying off quite well lately in terms of results.

Now, in answer to your question—-which was actually addressed to Mr Oakley:

Did it ever occur to you that we first need to understand the physical concepts how things work,..

I would answer that Yes actually it has occurred to me.

Oh, M.O.O.S. in regard to your warning to us:
** **It's an evil strategy of certain people to present things as if there would be alternatives,..[to superstring/M theory]…****
that sounds indeed very diabolical of them. I can only express my surprise at your discovering this.

Civilly yours,

Who

118. **Lee Smolin**
May 21, 2006

Dear Amused,

In fact I do agree that some of your proposals would be helpful, for example to instutute in the US fellowships analagous to the Marie Curie or Royal Society Fellowships. My answers to others of your questions are in my Physics Today essay from June 2005, page 56. As far as consistency, I do not now, nor have I ever made postdoc hiring decisions on my own, at PI such decisions are made by a committee. But it is true that the majority of the people who hold or have held postdoc or visiting positions at PI in non-string quantum gravity work on approaches other than LQG.

Lee

119. **Aaron Bergman**
May 21, 2006

What's the difference between being "useless" and being wrong?

Ontologically, quite a lot. Epistemologically, maybe not so much.

This is what I cannot understand about what is going on today among string theorists. They seem willing to accept the possibility that the theory is useless in terms of predicting anything about physics, but unwilling to draw the obvious conclusion that if this is the case it is wrong.

Because that simply does not follow. Regardless, you seem eager to give up on the string theory project at every turn. Your original polemic doesn't even include the word 'anthropic'. Others still believe that, even with the surfeit of possible vacua, it's not at all clear that we might not be able to treat string theory like QFT wherein experiments determine a particular vacuum and it becomes predictive. I also believe that, given results like AdS/CFT and Strominger and Vafa, string theory surely is a theory of quantum gravity, even if it isn't our particular theory of quantum gravity. As such, we can learn a lot just from studying how string theory solves the usual problems with quantum gravity, and eventually those insights may help us understand the correct theory if and when it comes around.

But, for all this bickering, data is just around the corner, and that is where the jobs are going right now. If anything, much anthropicism is motivated by the specter of the LHC.

120. **woit**
May 21, 2006

"Regardless, you seem eager to give up on the string theory project at every turn. Your original polemic doesn't even include the word 'anthropic'."

I really wish you'd stop it with the ad hominem arguments, they're just obnoxious and don't prove anything.

Yes, for a very long time I've thought that string theory has failed as an idea about unification. The problems of too many compactifications and how to break supersymmetry have been around since the beginning, and by the late 90s it was clear there was very little chance they could be surmounted (non-perturbative versions of the theory had the same problems). That was the situation when I wrote my first public polemical article. Developments since then have provided even more convincing evidence that I was right that these problems can't be surmounted, backing string theorists into the anthropic corner which I never would have believed back then they would try to take a stand in.

121. **Aaron Bergman**
May 21, 2006

I really wish you'd stop it with the ad hominem arguments, they're just obnoxious and don't prove anything.

I hardly think it's unfair to point out that your opinion of string theory predates this current situation in the context of a discussion why there are people who haven't completely abandoned the field.

And, you asked why string theorists haven't given up. I certainly can't answer for everyone, but I tried to explain to you some of the reasons.

122. **Who**
May 21, 2006

what I have to say has something indirectly to do with how one reacts to the whining of young string theorists that one sometimes hears, but mostly I want to give ANOTHER EXAMPLE OF RESEARCH ALTRUISM besides what Smolin mentioned.

someone, I think “amused”, suggested that Smolin was probably just as self-interested as any string theorist and would only take postdocs that work in his own type of QG, and he said not so, and gave an example of Perimeter policy

===quote===
In fact I do agree that some of your proposals would be helpful, for example to instutute in the US fellowships analagous to the Marie Curie or Royal Society Fellowships. My answers to others of your questions are in my Physics Today essay from June 2005, page 56. As far as consistency, I do not now, nor have I ever made postdoc hiring decisions on my own, at PI such decisions are made by a committee. But it is true that **the majority of the people who hold or have held postdoc or visiting positions at PI in non-string quantum gravity** work on approaches **other** than LQG.
===endquote===

My point is that this non-string QG ethos is strikingly exemplified not only at Perimeter but also in the Utrecht QG program. Loll’s group has 3 postdoc positions. The house brand QG is called CDT (a triangulations path-integral). If you are used to EXCLUSIVITY then you may be shocked by this. Actually I was last year when I learned that the 3 positions went to STAR ROOKIES OF THE COMPETITION. The postdocs of Loll’s Utrecht group are people that Dowker in London co-authors Causal Sets papers with, and Freidel at Perimenter coauthors with, and a self-starter who helped standardize canonical LQG by proving an important theorem. These people are selfdirected reseachers with impressive track records—I’m not kidding. And so what is the good of this for Loll’s particular approach to QG? It simply did not make sense to me. She controls a sizable Dutch government grant based on her own CDT work and can do as she pleases, so why does this go to support some of the best rookies of the competition? People with their own research motivation who are not likely to change direction for trivial reasons.

It seems like the ethos in non-string QG has a large element of SUPPORT THE OTHER GUY’S POSTDOC. I have wondered how a field can survive with so much altruism running amok in it?

But there may be advantages. This year Utrecht has been getting visits from e.g. Dowker and Freidel and Ashtekar. It means that more different non-string QG approaches are being worked on there at one time than probably anywhere besides Perimeter. I guess that could pay off in the long run.

============
Anyway the ethos has a strong anti-parochial streak (wherever they have enough resources gathered so they can actually HAVE postdocs). and that is in strong contrast to the string ethos in typical US department where string faculty have control of ALL the QG positions and will not share ANY. At least as far as I know. It might be amusing to get some statistics on this corresponding but contrasting with the Utrecht and Perimeter examples.

the relevance to the situation of a young string theorist would seem to me that he or she is being supported by an exclusive system that will take on no other type of young researcher, no matter how ingenious, no matter how independent, no matter how inventive, with no matter how good a track record. that is to say a totally bigoted system

123. **Arun**
May 21, 2006

I’d say some people want to know the answers, even if it means that most of their productive research years turn out to have been on the wrong path; while some people want to build empires.

124. **amused**
May 22, 2006

Dear Prof. Smolin,

Thanks for taking the time to reply. In fact I had read your Physics Today essay, and have just re-read it, but am still confused about what exactly your position is regarding supporting young researchers, and how it differs from what people or groups in other areas do. More on this below.

“But it is true that the majority of the people who hold or have held postdoc or visiting positions at PI in non-string quantum gravity work on approaches other than LQG.”

And what areas do these people work in then? As far as I can tell, most, if not all, work on topics that can be classified under “background-independent approaches to QG” or “foundational issues in quantum mechanics”. What is the position of you and your colleagues at PI regarding support for young people not working in these areas (e.g. for someone working on formal aspects of gauge theories)? I don’t see any sign that you care about such people. It seems that on the one hand you advocate supporting independent young researchers irregardless of research area, but on the other hand you have chosen certain specific (albeit broad) areas which a person has to be working in to get support from PI.

So it seems that in reality the policy of you and your colleagues is no different from that of a typical string theory group: In both cases the group has an area, or set of areas, that it wants to support, and proceeds to hire people in these areas. Presumably it happens from time to time that a string theorist working on, say, branes and ads/cft, hires a postdoc who works in another subarea of string theory, say perturbative string theory or string field theory. The string theorist would say that he/she is doing this in order to further the career of a talented young researcher, even though the person is working on a different topic. Where is the difference between this and what you do? (besides that fact that the areas you are willing to support are broader than that of a typical string theorist).

[Peter – apologies again for continuing an off-topic discussion; I’ld understand if you wanted it to stop. I guess at some point you’ll write a post on Smolin’s book after it appears, and that might be a more appropriate time to take up this topic.]

125. **Christine Dantas**
May 22, 2006

Lee Smolin wrote:

(…) Latin American countries, as I said, whose systems are structured so that there are a few good positions where the competition is in terms of individual accomplishment and promise without regard to research program or subfield.

No and no.

Best wishes,
Christine

126. **amused**
May 22, 2006

Who,

“If you are used to EXCLUSIVITY then you may be shocked by this. Actually I was last year when I learned that the 3 positions went to STAR ROOKIES OF THE COMPETITION.”

When I first saw this I thought you were going to say the positions went to string theorists… being under the impression that they are (percieved as) the real competition, and that the various background independent approaches to QG are all in the same broad family…

I don't see that this is such a big deal though. As far as I'm aware, the Ambjorn-Loll group is the only one doing this CDT stuff. So, short of hiring their own students, how could they get postdocs who are already working on this? It seems they had no choice but to hire people from other subareas. Presumably Loll is hoping that these people will get interested in CDT and do some work on it (while continuing with their own stuff at the same time). And it would be natural for them to do this, seeing as CDT is one of the more exciting developments in this general area.

127. **Mentos**
May 22, 2006

Is there any actual evidence that "background independent QG" postdoc seekers fare worse, on average, than string theory postdoc seekers?

Obviously, the field is much smaller so, anecdotally, Lee is more likely to personally know good candidates who failed to get jobs. But there are plenty of top-notch string theorists who don't get postdoc jobs either.

Are there any statistics to the effect that a higher percentage of string theory postdoc seekers obtain jobs than "background independent QG" postdoc seekers? Or is this just another "just-so" story?

128. **woit**
May 22, 2006

Mentos,

The best data out there I would guess is at the new Rumor Mill for theoretical particle physics postdoc jobs. I had kind of started believing the complaints I've been hearing recently from string theorists that phenomenologists are getting all the jobs until I just recently took a look at this. By my count, getting postdocs this year there are 31 string theorists, 12 phenomenologists, and 5 hard to characterize (brane-worlds, QCD amplitudes by twistor methods…). Several institutions that hire lots of postdocs still will only hire string theorists (Caltech, KITP, IAS…)

How many "top-notch" string theorists do you know who in recent years have been unable to get a post-doc position? I personally know of no such examples, but maybe it all depends on what you mean when you say "top-notch". Can you give an example of someone from one of the top few groups in the US who has an impressive thesis but no job? Is the problem that there are more than 31 "top-notch" string theorists on the post-doc job market, or that institutions are hiring second-rate string theorists over first-rate ones?

This is a "particle physics" rumor mill, and thus may not have non-string theory QG jobs listed. Such a list may not exist, but since, as Lee points out there are very few places in the US that ever hire in this area, I would suspect there are no more than a handful such jobs.

129. **Aaron Bergman**
May 22, 2006

My count is slightly different from yours (but I might have miscounted). I'm not sure how representative that list is; there might be a bit of a selection effect (there are very few cosmologists on it, for example). I'm pretty far from the postdoc gossip this year, though. My impression is that there is a much higher proportion of phenomenologists among traditionally stringy jobs than in the past, though, but can't offer anything quantitative.

130. **anonymous**
May 22, 2006

Where is this postdoc rumor mill? I only know of one for faculty.

Also, the IAS only hires string theory postdocs? Funny, I bet Ian Low would be surprised to learn he is a string theorist. Other counterexamples to your claim exist. I don't know about KITP, but Caltech and the IAS do not just hire string theorists.

131. **anonymous**
May 22, 2006

Just so you don't think I'm choosing one exception to the rule, recent phenomenology postdocs at IAS include (but are not necessarily limited to): Mishima, Dermisek, Kitano, Agashe, and Kribs.

132. **woit**
May 22, 2006

I was referring to this site (and counting postdocs hired this season)

http://www.freewebs.com/heppostdoc/

According to it, the IAS is hiring five new postdocs, and I think they can all be reasonably characterized as string theorists. I wasn't claiming that the IAS theoretical physics group never hires anyone but string theorists, but now that you mention it, of their 18 current non-permanent members, how many are not string theorists? You mention 3 (Low, Mishima and Demirsek), and maybe there are a couple others, but its undeniable that IAS postdoc jobs overwhelmingly go to string theorists, both in the past as well as this year.

133. **Mentos**
May 22, 2006

"Several institutions that hire lots of postdocs still will only hire string theorists (Caltech, KITP, IAS…)"

That's kinda funny.

Of Caltech's 8 postdocs, 4 are phenomenologist, and 4 are string theorists.

At the IAS, I count 1 formal Yang-Mills person, 3 phenomenologists, 2 (Kleban and Rabadan) who started out as string theorists, but who are now doing phenomenology, and 7 string theorists.

And, of the KITP's 5 high energy postdocs, 4 are string theorists, and one is a phenomenologist.

"The best data out there I would guess is at the new Rumor Mill for theoretical particle physics postdoc jobs. … I had kind of started believing the complaints I've been hearing recently from string theorists that phenomenologists are getting all the jobs until I just recently took a look at this."

I was previously unaware of the Postdoc Jobs Rumor Mill. I'll have to check it out.

But on the Faculty Jobs Rumor Mill, I count 10 phenomenologists, 6 cosmologists and 6 string theorists getting jobs this year.

134. **Lee Smolin**
May 22, 2006

Dear Mentos,

I have no statistics, but it is not uncommon for top level grad students and postdocs-authors of widely read and cited papers-in non-string quantum gravity to get no offers and to have to apply in succeeding years to get a postdoc. This is not new, many who are now seen as the leaders had periods when we came close to being forced out of science. I certainly did.

Dear Amused,

I think that Who gave the best answer in describing the Utrecht group: there is simply a different ethic that values the personal promise of a young researcher over working on the research program of the faculty. This is in fact an old tradition, it came to me through my mentors who were senior faculty in relativity groups. It contrasts with another attitude towards postdocs which is that they are to be chosen to further the research program of the grant holder. My view is that the former leads to faster progress in science than the latter, because it favors young scientists who are more intellectually independent, original and courageous, and these are the kind of people who make the discoveries that drive science forward.

I disagree with your drawing an equivalence between string theory and the whole field of quantum gravity. I think it is a bad idea to organize groups or departments around research programs rather than subject areas, because that gives research programs, which can succeed or fail, more institutional inertia than is good for science. I have nothing against string theory per se, so long as it is seen as one research program among several aiming towards the further unification of physics.

Hence, I would argue that commitment to string theory as a research program is different than commitment to a subject areas such as quantum gravity or post standard model particle physics. I have never myself worked within one research program; I continue to publish a paper on string theory every year or two and the bulk of my work is seen by specialists as not strictly LQG. I think science would progress faster if we built structures that discouraged rather than encouraged scientists to identify themselves with particular research programs rather than areas.

I do personally agree with you that formal aspects of gauge theories are a neglected, under supported area, which is one reason I have been following the recent work in this area with interest.

As for PI, please do not confuse my views with either policy or the experience at PI. I am one voice and vote among many, and the basic mandate and policies-including the idea that postdocs are to be hired by the whole institute as independent researchers-were set up by the founders and director before any of us faculty were hired.

Dear Christine,

Thanks for the correction.

Thanks,

Lee

135. **Who**
May 22, 2006

Amused, both Perimeter and Utrecht ITP have numerous string postdocs, as well as "background independent QG" postdocs.
I wish that major theory sites in US were more inclined to allocate support to the individual mind rather than to the camp—as I have said–and think that this would LEAD to more diversity in the research pursued at these places. QG research diversity is not a goal *per se*, or is only an accessory interest. It is *symptomatic* of ability to appreciate drive and originality in rival lines and a pragmatic philosophy of "let's get the problem solved however the heck we do it (rather than by my pet method or my club of people)".

If you don't see that philosophy operating in the places I mentioned, or don't see the results of the past couple of years as indicative that it works, then maybe my perception is wrong—I may be deceiving myself or failing to communicate here.

Oh BTW Utrecht also has a postdoc who has co-authored with Martin Reuter—-not in Loll's immediate group—I think he does string part of the time. As does Ambjorn, whom you mentioned as a CDT associate—my impression that his research is at least half, maybe more, in string/M.

I think the style exhibited here is "ecumenical" or maybe pragmatic. There is no intrinsic merit to it—you can only judge by the results. If this does not make sense to you please let me know.

136. **anonymous**
May 22, 2006

"According to it, the IAS is hiring five new postdocs, and I think they can all be reasonably characterized as string theorists."

The rumor mill apparently doesn't know about Gil Paz, who is also starting a postdoc at the IAS next year, and is decidedly not a string theorist.

The IAS is largely composed of string theorists, but for an institution so dominated by string theory in terms of faculty, I think it does pretty well with hiring a more diverse group of postdocs.

137. **woit**
May 22, 2006

Mentos,

My mistake for the wording implying that IAS, KITP and Caltech in the past only hired string theorists, something I didn't mean to claim. I was just talking about this year.

I also had only been aware of the permanent faculty hiring data, which does show more phenomenologists and cosmologists than string theorists getting jobs. I was surprised to see how different the postdoc data is. Perhaps this is because postdoc hiring is more concentrated at a few prominent institutions, and these remain dominated by string theory, whereas permanent jobs are spread out more widely, with prominent institutions doing only a small amount of permanent hiring.

138. **Aaron Bergman**
May 22, 2006

I was surprised to see how different the postdoc data is

I'd be careful assuming that page is a representative sample. Anon above already pointed out one person missed at IAS.

139. **Thomas Larsson**
May 22, 2006

FWIW, I believe that Swedish ur-string theorist Lars Brink was the one responsible for me landng a 4-year postdoc, long ago. So there are counterexamples to the claim that string theorists only hire string theorists. I'm not sure that Lars likes me anymore, though.

I thought I recognized Lars in the City Hall last December 10th, but I'm not sure since it was more than a decade since we last met. Perhaps next year.

140. **amused**
May 22, 2006

Who,
Yes, I am aware that PI and Utrecht have string as well as background independent QG postdocs, that many people who work in the latter area have wide-ranging interests and also work on other topics (a positive thing, of course), and that there has been significant progress in some subareas of BIQG in recent years.

"QG research diversity is not a goal per se, or is only an accessory interest. It is symptomatic of ability to appreciate drive and originality in rival lines and a pragmatic philosophy of "let's get the problem solved however the heck we do it (rather than by my pet method or my club of people)"."

Sure. But can you accept that there might be people of this ilk in other areas of theoretical physics (or science in general) besides QG? Or do they all gravitate towards QG research of some sort or other? Assuming they exist, are the ones who choose non-QG topics less deserving of support than the QG'ers?

If Smolin had simply appealed for more recognition and funding for work on BIQG and fundamental aspects of QM he would have had no argument from me. But when he starts going on about how he and his colleagues support independent young researchers of quality and promise, irregardless of their research area, then the people out there who are, or were, young independent researchers, and who aren't, or weren't, having an easy time jobwise, are going to prick their ears up. Questions will come to mind such as "Hey, I wonder what it takes to meet Smolin's `quality and promise' criteria?". Then some of the older ones might get a flash of recollection along the lines of "Er, didn't we send a postdoc application to that Smolin guy early on, when we had a couple of papers in PRL and various others in NPB, PRD etc… (all single-author of course – we're independent, remember)… and he wasn't exactly enthused, was he…. Well, at least that sets a lower bound on his criteria which we can compare against the people he does hire – so let's look them up." And later, "Hmmm, looks like we overlooked something – the `quality' part of Smolin's criteria appears to include a `quality of choice of research area' factor. At which point cynicism sets in, and a snide comment or two get posted on a certain accomodating webblog.

141. **Lee Smolin**
May 23, 2006

It is a bit unpleasant to have one's motives questioned publicly by an anonymous person over past decisions. I would think that good sense would suggest several reasons why every decision of every research group a person has been a member of may not agree with policies they presently advocate, including 1) they were one vote on a committee, 2) restrictions from funding agencies, 3) their present views are the result of reflection on past experience and have evolved 4) there are so many excellent candidates that one research group in one field cannot be expected to have room for all but a small fraction of the candidates they think are deserving of support. This is why I have begun to speak out about these issues; one institute, however well supported and well meaning (and within which I am at most one vote-when I am on the committee) can only hire a fraction of those who are deserving under our criteria. And this is why I agree with the writer that more programs like the Marie Curie and Royal Society Fellowships would help.

142. **woit**
May 23, 2006

I've deleted a comment from "amused" responding to Smolin in order to terminate this particular off-topic discussion. Evidently the two are in e-mail contact and can pursue this privately if they wish.

143. **John Gonsowski**
May 23, 2006

This probably belongs more in the comments on comments thread but I'd like to amend my comment there and say it does seem bad when an anonymous heckler addresses a Lee Smolin or John Baez by first name only. At least amused seems respectful in this sense and going to email is even better.

144. **amused**
May 24, 2006

To avoid an impression of rudeness, can I just point out for the record that I hadn't seen Prof. Smolin's lengthy comment – the one addressed to Mentos, me, and Christine – at the time I posted my subsequent comment above (the one addressed to Who). Perhaps it was somehow delayed by Peter's WordPress update, or maybe I just overlooked it. If I had seen it I would have addressed his points in my subsequent post, and its tone would have been different. But now it's time to drop this, in accordance with Peter's wishes.

Comment on Comments

May 20, 2006
Categories: Uncategorized

Over the last week or so I’ve heard privately from several very different parties with complaints about the comment section here. The general feeling is that it would be more useful and attract more serious contributions if the level of uncivil, disrespectful ad hominem attacks was much lower. One contributing factor mentioned is that anonymity allows people to behave in uncivil behavior that they would not engage in if their names were publicly attached to their words. On the other hand, the worst offender in this is someone who is not anonymous.

I’d be curious to hear thoughtful comments by others about this. My own feeling at the moment is that the criticism is accurate: the uncivil atmosphere here keeps many serious people who would have something interesting to contribute from doing so. The anonymity is probably part of this problem, although given the current unhealthy situation in particle theory, some people have very legitimate reasons for keeping their comments anonymous.

In many ways I think the comment section has been a success, but it could stand a lot of improvement. Unfortunately I don’t know of any really successful models out there to follow. Among the more active blogs by physicists, Cosmic Variance does a good job of keeping a civil discussion going, but it is rarely about physics these days. Jacques Distler’s Musings has high-level content in its postings, but no one has submitted a single comment about physics there in over a month and a half. The comments at Lubos Motl’s Reference Frame are as uneven as the blog’s proprietor.

I already delete quite a few comments on grounds of lack of civility, but tentatively plan on trying to raise that standard by deleting a larger fraction of uncivil comments, especially if they are posted anonymously. It would help if people could keep the following in mind when posting comments:

1. Please consider abandoning anonymity and posting under your real name, unless you have a good reason for not doing so.

2. Please take much greater care to keep comments civil and respectful. Ad hominem argument about the ignorance and lack of intelligence of people you disagree with has no place here.

3. Please ignore silly comments when they appear. Maybe I’ll also think they are silly and just adding to the noise and will get around to deleting them, maybe not. But in any case you’re not adding anything by submitting a comment criticizing the silliness, but instead are adding to the hostility level.

Constructive comments are welcomed. One thing to keep in mind is that I already am spending more time on this than I should, suggestions that involve a lot more work on my part are non-starters.

Comments

1. **wolfgang**
   May 20, 2006

   > Jacques Distler’s Musings has high-level content in its postings, but no one has submitted a single comment about physics in over a month

   I think this is perhaps because the comment feature is broken.
   The last time (fairly recently) I tried to submit comments on Jacques’ blog it resulted in error messages …

2. **knotted string**
   May 20, 2006

   Can you politely email Jacques, to let him know?

3. **Matti Pitkanen**
   May 20, 2006

   It would be nice if old-fashioned Boolean true/false logic would replace the more modern string theorists/crackpots logic in these discussions. Perhaps we could do well without the word “crackpot”? There have been occasionally very interesting attempts to initiate serious discussion about algebraic quantum field theory, Jones inclusions etc, but soon everything degenerates to the usual fighting which makes me sick.

4. **Who**
   May 20, 2006

   I will think about the problem you describe and will try to offer some suggestions which are constructive and not merely in my immediate self-interest.

   One thing is you could try an experiment involving segregation. At the end of certain postings you could say “Qualified comment only. No anonymous comment.”

   Be very brief, just indicate in a short phrase or two that anybody who is NOT academically qualified for that particular discussion will have their comment deleted out of hand at your discretion. IF YOU SPELL IT OUT IN TOO MUCH DETAIL it might sound snobbish or priggish. So dont spell the policy out too explicitly. Just give a short warning at the end of a selected post, and then be fast and ruthless about deleting anybody that you dont know professionally or that doesnt give their name and SAY they are a grad student or faculty somewhere or have some appropriate professional standing like that.

   In this case it is the commentor’s responsibility to make sure that either you already know him or else to say up front “I’m a physics grad student” or “I do this kind of research” whatever. It is up to commentor to give you enough clues up front about professional qualification, or out it goes.

   Or you could restrict comment on some particular thread to WEBSITE BLUE people plus ones you know personally. Like wolfgang who just commented gives his page. His blue signature is a link to his website that says he is a such and such kind of physicist.

   I wouldn’t advise a blanket policy because it could be a straightjacket on you as on well-meaning commentors. Trying it out with a few postings would be an EXPERIMENT. It might fail in the sense that you might not get a good informed civil discussion after all, even in that case.

   Personally I would be sorry to see a sign on one of your threads that says ” identified comment only” because I enjoy being Who (it’s a quirk I guess: I really like internet anonymity) so I couldnt post comment on such threads. But on the other hand it might every now and then make the thread more interesting reading to have only signed qualified comments.

Some of the silly and/or witty comments here are quite funny and also even some invective is entertaining in small doses, but I get tired of it quickly and would be glad to see some threads ruthlessly cleared of it.

I hear what you say about it soaking up time. I certainly wouldnt mind seeing a lot more rapid unexplained arbitrary behavior, if that means saving time.

Good luck attracting some of that hold-out clientele.

5. **woit**
May 20, 2006

Who,

Thanks for the comments and the suggestion. I don't think though that I have any interest in restricting comments based on professional qualifications. There are plenty of examples I've seen here of people I know to have no appropriate professional qualifications making excellent and interesting comments, and people with illustrious qualifications writing in things that are hostile and worthless.

Maybe it's sheer arrogance on my part, but when people write in with some comment that involves the kind of scientific issues I'm writing about here, I can in most cases immediately tell whether they are well-informed concerning what they are talking about, and that's what's relevant, not professional qualifications.

6. **asubedi**
May 20, 2006

I think you should allow the comments to be anonymous and accessible to all. However, if someone keeps posting a lot of junk, you may delete the postings and ban the ip address for a while.

7. **Chris Oakley**
May 20, 2006

Peter,

I think that you can be proud of what you have created here. Obviously with the provocative title, it provokes and will continue to provoke strong reactions, but what is wrong that? The structures one builds in theoretical physics should be stronger than steel, and if they fall down as the result of a little criticism then they were probably not worth building in the first place.

As for the comments policy, I think that you have got it about right. I am sorry if the whole thing is taking up too much of your time, but I for one appreciate the effort.

Just curious: will your publisher be sending Lubos a review copy of your forthcoming book?

8. **woit**
May 20, 2006

asubedi,

That's essentially what I'm already doing.

9. **woit**
May 20, 2006

Chris,

Strong reactions are fine, and if certain structures topple, great. What I'm concerned about is that I am getting the impression that quite a few very good people, string theorists and non-string theorists, read this blog from time to time, but are put off by the tone of much of the comment section, making them less likely to take the whole thing seriously and consider participating. I'm interested in whether that is a more widely shared impression and what can be done about it.

This weekend I'm supposed to be putting together a list to send to the publishers of who to send review copies to. Not sure about Lubos. I do thank him in the acknowledgements, but I know he's not going to read and think about what I write…

10. **A Different Peter**
May 20, 2006

You can be sure Lubos will have something to say about the book, whether he actually gets hold of a copy or not.

About the politeness question, the problem that you have is not just confined to your blog. It is everywhere on the blogosphere. This new form of human communication is showing its limitations because it is difficult to separate out the comments that one would like to read from the huge volume of worthless and hostile posts.

The natural solution seems to involve heavy moderation. This means either the blogger devotes a lot more time to moderation, or he delegates some portion of the moderation task to blog visitors (look at how slashdot does it).

A simple option which would stop short of mass deletions of posts, and would encourage people to write more relevant posts, would be the following. There can be a number of categories, say "High Physics Content, No Humor", "No physics content; personal attacks" and so on. Visitors to the blog with unique IP addresses (or some other criterion that you prefer) could, upon reading a post, click one of a number of buttons beside it, indicating what category they think that post belongs in. Simple majority voting, or some other criterion of your choosing decides what category the post is given based on how many users have chosen each category for it. At the side, bottom, or top of the comment section, a little clickable box allows the user to choose a filter, so the user can choose to see only high physics content posts, or also humorous posts, or all posts, or only personal attacks (if that's what they really want). They can also choose whether they would like to see posts which have not yet been classified by other visitors.

People who are going to post something will know that, if they post mindless personal attacks, they will be categorized as such and then most people will not bother to read them. People write posts because they want attention, so the only way to give them an incentive to write well is to make the attention conditional on their writing something worthwhile.

Of course, implementing this option involves some programming.

11. **fishfry**

May 20, 2006

The comments at Lubos Motl's Reference Frame are as uneven as the blog's proprietor.

Incivility is as incivility does.

12. **Ron Avitzur**
May 20, 2006

One technique you might consider is disemvoweling posts. It gives you an additional level of discrimination short of deletion to indicate offending material in a way that readers can easily skip over or choose to puzzle out with effort.

13. **Dick Thompson**
May 20, 2006

I hope you will continue to have your comments as they are. I can't agree with Who's recommendations to limit it to "qualified" respondants. After all, Peter, some people are not shy about calling you unqualified to comment on string theory! If you restrict your comments they have already won ;).

This is my real name:

Dick Thompson

14. **Arun**
May 20, 2006

One could moderate comments, e.g., by finding a blog collaborator who will impartially weed out uncivil comments,

or one could institute a comment rating system (presumably a lot of work) where readers can rate the comments, and poorly rated comments become invisible,

or one could follow the (difficult) policy of not replying to any such uncivil comment.

Polite but content-poor comments are a much harder proposition to manage, IMO.

15. **Lubos Motl**
May 20, 2006

Dear Peter,

I must be cryptic in this sentence but I don't believe that you believe that my relation to a certain piece of work of yours is in the status that you described, and most likely you know very well what I mean.

Wolfgang: the buggy comment gadget at a certain blog under consideration is deliberately buggy.

Best
Lubos

16. **Eugene Stefanovich**
May 20, 2006

Peter,

your blog is a valuable resource for anyone who loves theoretical physics. Every day I go to arxiv.org, your blog and sci.physics.research (pretty much in this order) to look for some nuggets of information. Please don't change anything, I like your blog the way it is.

Anonymous comments are fine. It is better to let people speak up their mind anonymously rather than make them silent out of fear … whatever they afraid of. Uncivil comments are not a big deal either. For one thing, they give an interesting insight in who is who and how "healthy" the field is.
Second, most of your readers are intelligent enough to filter out the garbage. Third, you can always weed out excesses, as you already do.

Keep up excellent work!
And thank you for not posting about flowers in your garden, your favorite music, and climatology.

Eugene.

17. **woit**
May 20, 2006

fishfry,

Given Lubos's behavior and the way he chooses to characterize me and others who disagree with him, I think referring to him as "uneven" is being exceedingly civil…

18. **woit**
May 20, 2006

Lubos,

Oh, OK, I'll make sure you get a copy….

19. **John Gonsowski**
May 20, 2006

Don't change it too much, Discover Magazine readers might be disappointed if they came here and didn't find a lively forum for bickering scientists. I can't believe any regulars, particularly ones with sci.physics.whatever experience would have any problems here. Physicists new to internet forums in general could have problems most anywhere due to tone and widely varying subjects that don't even always stay on topic. Having lots of participation is more fun and more educational but it can make forums harder to follow if you haven't developed skimming skills to suit your available time. Maybe you could ask these physicists if they'd like to do guest articles (I think Lubos did this at least once) or have you do an article on them and then we could be extra civil at those times.

20. **anonymous**
    May 21, 2006

    Anonymous comments on your blog seem a good form of communication.

    One negative consequence of counting citations is that too many physicists, especially in the US and especially in speculative fields, are more worried of good relations than of discussing physics. I prefer getting a “you are an idiot” from Lubos to the other extremum: a long discussion, where things are said so politely that the content is obscure.

    Sometimes the comments section contains too much “background”: maybe you could install a software that allows readers to rate comments (“Was it worth reading? Y/N”) and to see only comments above a chosen threshold.

21. **Bert Schroer**
    May 21, 2006

    There is a form of communication which keeps it lively and which I have been trying to use: irony and scientific (not personal) polemics.
    It is often answered by dull personal confrontations (strangly enough, I did not make this experience with Lubos, but I have seen that others did) and it depends on one´s emotional constitution how much one wants to take.
    But I think that Peters argument that some interested onlookers may be repulsed by the vulgar manner should be taken serious. To lift the anonymity may lead to timidness. Perhaps the introduction of a disapproving back-reaction of the participants (by clicking a button) may alliviate the situation because the protection by anonymity is probably not 100% (after finding out the machine, there may be some feeling of discomfort which could lead to a more restraint behavior).

22. **Chris Oakley**
    May 21, 2006

    Let me see if I can guess what Lubos will say in his review:

    “People can be categorised as follows: stupid, average, smart, very smart and Superstring Theorists. The latter category comprises the very smartest, and these people are way smarter than even the very smart. In fact, very smart people are just morons compared to even an average Superstring theorist.

    “Most non-superstring theorists have the good sense to acknowledge their inferiority, and although they provide the money for us Gods of the Human Intellect to do amazing things in amazing multi-dimensional worlds (not enough, by the way), they generally do not have the effrontery to question the validity of our researches.

    “Not so Peter Woit. Anyone reading more than ten words of anything he writes would realize that he is an embittered half wit, who, failing to be admitted to the Superstring club 20 years ago, has unfortunately chosen to vent his rage against those who thwarted his career rather that just getting a job in a McDonalds restaurant, as would more suit his talents.

    “To those of us who seriously explore the higher-dimensional, supersymmetric realms, Peter Woit is no more annoying than a fly that buzzes into your kitchen when you are trying to eat lunch. However, since everyone in the world is less clever than us, it is possible that some may be deceived into thinking that there is some substance to his ravings, so let me make it clear here and now that there is nothing in what he says. Peter knows nothing about physics or mathematics, and nothing he says should be taken seriously.”

23. **Nigel B. Cook**
    May 21, 2006

    That was a very silly comment. Now Lubos can borrow your review. He will say you used Josephson’s string theory ESP to read his mind, as described in the arXiv paper by Josephson: http://arxiv.org/abs/physics/0312012

24. **wolfgang**
    May 21, 2006

    Lubos,

    > the buggy comment gadget at a certain blog under consideration is deliberately buggy

    it seems to work again. Thanks in large part to Peter’s post … 😎

25. **woit**
    May 21, 2006

    Chris and Nigel,

    I realize that Lubos is endlessly entertaining, but try and resist the temptation to make fun of him, it’s too easy and we need a higher quality of humor here.

26. **knotted string**
    May 21, 2006

    Wolfgang: thank you for this update. It is heartwarming that string experts are humble enough accept a helping hand with trivial problems in hosting discussions.

    http://golem.ph.utexas.edu/~distler/blog/

    ‘May 20, 2006

    ‘Technical Difficulties

    ‘It’s been brought to my attention that some people have recently been encountering an INTERNAL SERVER ERROR when attempting to comment here (or at the String Coffee Table).

    ‘The cause, alas, is my determination to be overly clever.’

27. **ksh95**
    May 21, 2006

    Requiring “real names” would definitely stop me from posting. The last thing I need is some one Googling me and finding hundreds of ksh95 blog postings.

Secondly, I'm confused as to why you think anonymity and uncivility are related. That's not obvious to me at all, in fact, it seems somewhat odd as I doubt most "uncivilized posters" consider their postings uncivilized.
Thirdly, my personal opinion is that the comments section is no where close to uncivil (I think some potential contributers may be overly sensitive and fragile).
Finally, if it ain't broke…..

28. **Yuri**
May 21, 2006

I like your blog the way it is.

29. **Fred Diether**
May 21, 2006

Peter,

The solution seems like it could be simple to me. You should just post a warning about ad hominem attacks not being allowed in the "Leave a Reply" section and enlist the help of one or two competent volunteers to help you delete them. When I showed this blog to my particle physics tutor, he asked "What is the difference between this blog and Usenet?" So I do think you have a moderation problem. If it is already taking too much of your time then you should try to get help from someone you trust.

I do enjoy reading much of the content of your blog but I don't really enjoy wading through the personal attacks. They really serve no purpose to the discussion of physics.

Fred

30. **Michael Schmitt**
May 21, 2006

Hi,

I want to echo Eugene's comments: this is an *excellent* blog with content of exceptional quality, and I am glad that you stick to physics and leave out your personal hobbies, politics, and weekend experiences. If you want to spend more time editing and purging the comments, that's fine, but please do not let it reduce your primary posts.

thanks,
Michael

31. **Bruce Keener**
May 21, 2006

I love this site because it helps me see that we really don't know as much as a typical layman might think. It's been more than thirty years since I got an MSEE with minors in physics, and more than 25 years since I've had to apply ANY of it, so the math of string theory and QFT is beyond me and always will be – I'm too old to try to learn it, and it's pointless at this stage in my life anyway – I am now and will remain a layman. But I care about how our world operates. I'd like to understand it better and I'd hate to see us postpone understanding of it while we whack away with theories are not "not even wrong." I don't really want to go through the rest of my life with wrong beliefs (an impossible desire, probably), and really work to put as much balanced (pro and con) input into my mind as I can find. This site helps me, at least in the area of physics-related beliefs.

I do learn (slowly) from the comments here, and have no real advice on how to do them differently. I guess the only thing I would offer is that it would be nice to see more variety in topics: potshots at the unproductivity of string theory and at the metaphysical nature of "the landscape" are getting to be a bit old. There's plenty of other topics that would seem fruitful for appropriate critique, such as Alexander Mayer's "revamp" of GR; whether dark matter exists or whether it's just a contrivance that helps make big bang models work; various conflciting ideas of how quantum mechanics ties to consciousness (such as Stapp's and Penrose's philosophies, much of which is over my head but interesting); and so on. However, having said that, it is at least nice that there are voices crying out about string theory and the landscape and making us question whether we really are wise regarding the amount of brain power we are dedicating to these topics.

32. **Christine Dantas**
May 22, 2006

If possible, one suggestion would be to split the comments section into two: technical comments and non-technical comments. So the reader would have to choose to which section he/she would like to write his/her comment. Perhaps things would get more organized. Of course, you would still have to go into moderating the comments. This seems to be unavoidable. I also think that the anonymous contribution is an interesting feature, I would not remove it.

Best wishes
Christine

33. **Kyle**
May 22, 2006

I've been reading this site for over a year now, and every so often I post a comment. I've never found the comment section here the least bit intimidating, and compared to most forums I read everyone including Lubos is really quite tame.

I understand not everyone is as comfortable with conflict as I am, but I can't imagine what the problem with the tone of the comments could be. Surely people aren't avoiding commenting because they worry about the reaction. Surely the good responses aren't lost to the reader because of a few uncivil words above them. Removing uncivil comments will reduce the amount of comments, which could be a good thing – but I don't see how it is going to improve any of the other comments. Definitely let people post anonymously – though it might be nice to force them to choose a handle, just so we can tell them apart.

I don't think there is an easy solution to the problem as you see it. I do not believe a rating system will help, as it is so easily abusable. This is your blog. Hack, slash, and delete comments as you see fit. You are the one who takes the time to make the posts, so make sure the blog has the tone you prefer. Make the rules you follow easy to implement, and then do so mercilessly.

I've found the comments section here to be a gold mine, and I link to the blog all the time. Goodluck finding the right solution for you,

Kyle

34. **secret milkshake**
May 22, 2006

An alternative to Christine’s proposal would be to categorise the entire blog into sub-sections (methink ” Technical Topics and Links, Politics and Academia, Book Business, Latest from Landscape, Media & Propaganda, Applied Lumology”) and enforce a more civil discussion tone (by means of bloodcurdling moderation) just in the technical sub-section.

35. **CapitalistImperialistPig**
May 25, 2006

I am not expert on the subjects usually discussed here, but I do find your blog a valuable resource, uncivilities and all. I personally hope you don’t change it much. I prefer to be anonymous and try to be civil, but like the opportunity to occassionally ask questions of experts. Annoying as Mr Incivility can be, I think he often offers something interesting or at least entertaining.

Fantastic Realities

May 20, 2006
Categories: Book Reviews

Frank Wilczek has a new book out, it's called Fantastic Realities: 49 Mind Journeys and a Trip to Stockholm, and is published by World Scientific. It's a great read by one of the best in the business for anyone interested in physics and should be accessible to people with a wide variety of backgrounds. The book consists of a collection of 43 short pieces, most of which have been published elsewhere (often as "Reference Frame" columns in Physics Today), broken into 11 sections, each with a short introduction. The writing is exceptionally well-informed, elegant, lucid, and thought-provoking.

There's also a section of 6 original poems, which I'll not comment on since I'm not a literary critic, as well as a final section of extracts from Wilczek's wife Betsy Devine's blog Funny Ha-Ha or Funny Peculiar?. The blog entries explain exactly what it's like to be a family member of a Nobel prize winner, and contain lots of useful tips for you and your fellow family members should you ever win a Nobel prize and need to know exactly how to prepare for your trip to Stockholm. I hope I won't be damaging sales of the book by noting that they're available on-line.

Wilczek started out his career with a bang, discovering the asymptotic freedom of Yang-Mills theory in joint work with his advisor David Gross. He was thinking of this work in terms of perhaps showing that the SU(2) part of the new electro-weak gauge theory of Weinberg and Salam might not have the same problem that QED had (effective coupling growing at short distances, invalidating perturbation theory), but Gross was thinking more about the strong interactions and the short-distance scaling behavior recently observed at SLAC. If it could be shown that Yang-Mills theories also had effective couplings that grew at short distances like all other known QFTs, that would rule out QFT as a theory of the strong interactions. The discovery of asymptotic freedom made it clear that Yang-Mills theories might provide a successful strong interaction theory, and there was one obvious choice for the right theory: QCD.

Many of Wilczek's pieces deal with QCD in one way or another, from explaining his original work with Gross, to more recent developments concerning high temperature (relevant to heavy-ion collider experiments) and high density versions of the theory. He also explains some of the beautiful data that has accumulated over the past more than thirty years since its discovery that give us impressive evidence for the validity of QCD. Wilczek puts QCD into a more general context, explaining how logarithmic running of coupling constants can explain the small size of the strong interaction scale when compared to the scale of a putative GUT or even the Planck scale. Besides QCD, he provides excellent discussions of the rest of the standard model, the electroweak theory.

In several different pieces about beyond the standard model physics, Wilczek emphasizes two pieces of evidence that we have for some sort of GUT scenario. One is the fact that if you take the 16 dimensional half-spinor representation of SO(10), under the SU(5) subgroup it decomposes as 1 + 5 + 10, giving all the standard model fields of one generation (including a right-handed neutrino), but in a single irreducible representation. The second is the calculation (that he did in 1981 with Dimopoulos and Raby) of the running coupling constants for the supersymmetric SU(5) GUT (see here, although I'm not sure I agree that this falsifies Popper), which show much closer unification of the three couplings at a single energy than in the non-supersymmetric case.

These two facts are definitely the strongest evidence around for the idea of a supersymmetric GUT, an idea which has dominated thinking about beyond the standard model physics for nearly 30 years, but they are far from convincing. Wilczek deals with the other main idea that has dominated the field, string theory, by essentially ignoring it. I only noticed one or two mentions of string theory in passing in the book. He's not taking a position pro or con on the subject, just deciding that other things are more worth writing about.

The longer pieces in the book are among the best, including a piece on the Dirac equation, written for a book on the most beautiful equations, and pieces on fractional charge quantization and quantum field theory in general, which are a bit more technical than the others. Wilczek brings in interesting historical context to most of the things he writes about, often in an original way.

Perhaps my favorite piece is one entitled "What is Quantum Theory?", which deals with one of my obsessions. Wilczek claims that perhaps we still don't properly understand the significance of quantum theory, especially what it has to do with symmetries. He notes that Hermann Weyl, soon after the discovery of quantum mechanics, realized that the Heisenberg commutation relations are the relations of a Lie algebra (called the Heisenberg Lie algebra), and that this exponentiates to a symmetry group (the Heisenberg group to mathematicians, Weyl group to physicists). Wilczek goes on to speculate that:

The next level in understanding may come when an overarching symmetry is found, melding the conventional symmetries and Weyl's symmetry of quantum kinematics (made more specific, and possibly modified) into an organic whole.

Wilczek is still at it; last week he had a new preprint with Brian Patt which I wish I had time to look at more carefully. In this month's Physics Today, he has another Reference Frame article, now about the anthropic principle, and I'll write about that soon and separately.

There's also a podcast of an interview with Wilczek and his wife conducted at a party in Brooklyn held last month to celebrate the release of the book. If you listen closely maybe you can hear me and others chatting in the next room, despite being told to keep it down because of the recording session…

Comments

1. **Thomas Larsson**
   May 21, 2006

   Five years ago, Wilczek wrote in his Future summary:

   "5.5. Produce the New Particles!
   Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new R-odd particles. Even in the focus point scenario, there must be several accessible to the LHC."

   That is what I call a real prediction!

2. **Thomas Larsson**
   May 21, 2006

   Perhaps my favorite piece is one entitled "What is Quantum Theory?", which deals with one of my obsessions. Wilczek claims that perhaps we still don't properly understand the significance of quantum theory, especially what it has to do with symmetries.

   Gauge symmetries are indeed poorly understood on the quantum level, in more that 2D. This has been noted by many people, e.g. in Haag's book (p 325). One can of course check the Noether identities of the classical action, but quantically one would want to realize gauge transformations as well-defined operators acting on the (kinematical) Hilbert space. To me, it is more important to have such a well-defined realization on *some* Hilbert space, rather than demanding that the Hilbert space comes from QFT in the strict sense (which would be impossible, there are no-go theorems). The missing ingredient can be viewed as explicit observer dependence.

3. **Bert Schroer**
May 21, 2006

My experience leads me to strongly support Thomas Larson´s remark about a deeper quantum understanding of what is behind gauge theory being still very precarious and tentative. In fact I may add some very specific partial (yet unpublished) results which underline this point.
The quantum physical origin of the gauge issue is the fact that family of zero mass finite helicity Wigner representations allow for a covariantization in terms of pointlike field strength, but not for pointlike potentials (a restriction which does not exist in the classical setting because there the positivity of Hilbert space is not an issue). In fact even before one argues about the necessity of potentials in formulating interactions, there is the kinematical observation that it is not possible to write the Wigner inner product in terms of an integral over a local density in terms of field strength, one needs potentials even on a kinematical level. In order to maintain the standard perturbative formalism in terms of pointlike potentials, one is forced to extend the Wigner representation by (Gupta-Bleuler, BRST ghosts), leaving the quantum physical Hilbert space. Although it is deeply relaxing to see that the cohomological aspects of BRST allow a physical descend after having done the computations, such a BRST catalyzer (which was not there in the beginning and left no trace after the physical descent), this situation should not serve as a soft pillow.
Indeed there is a completely different autonomous way (which unlike BRST does not take any hint from the classical selection principle via gauge theory): the best possible localization of potentials in the Wigner space is along semiinfinite spacelike "strings", i.e. the potentials lead to stringlike objects which fluctuate in both the starting point x of the (linear) string and its direction described by a spacelike unit vector e. Since these "strings" have vacuum fluctuation in both x and the de Sitter point e (the space of directions), the fluctuations in x become much milder and are "renormalizable" in the sense of power counting independently of the value of the helicity (Mund-Schroer-Yngvason, math-ph/0511042). So the new problem is how to do perturbation involving such "strings" (since there is no Lagrangian for such objects: how to adjust the Epstein-Glaser method to the more complicated causal geometry of strings). This is a bigger project which I am involved in together with Jens Mund and Jakob Yngvason. But since the results for the metric potential of the (linearized Riemann tensor) field strength are already available, I asked Jens Mund to separate his short computation from the bulk of our joint project and post his short computation onto the server (it should be available within the next two weeks). As far as I know there has been no classical gauge suggestion for helicity larger than one.
Although in the present form these remarks are a bit remote from the main theme of Peters Webblog (to present a platform for alternative ideas to string theory), they may not be so in the future (if it turns out that with there new concepts one can really obtain a well-defined perturbation theory relevant for gravity).

4. **Chris W.**
May 22, 2006

After re-reading physics/0403115, I'd say that Wilczek conveniently ignores the context in which Popper's ideas about the importance of falsification arose. He is essentially saying that he and his colleagues will pursue potential solutions to problems in theoretical physics in whatever way seems sensible to them under given circumstances and in light of prior experience. That's fine, but it in no way undercuts the idea that the impossibility of testing an ostensibly empirical theory is of central importance, and that significant empirical confirmation really only amounts to survival of stringent tests.

To the extent that falsifiability is derided as irrelevant to the actual practice of science by some influential particle physicists one can understand how we have arrived at our current predicament. [Sorry if that remark strikes some as needlessly provocative.]

Various Links

May 21, 2006
Categories: Uncategorized

Someone wrote in to inform me that Alain Connes has made available at his web-site the full text of his long 1994 book Noncommutative Geometry. This is a rather amazing book, in many ways more of a research document than a purely expository work. All sorts of interesting things in it, mainly about Connes's ideas linking the "geometry" of non-commutative "spaces" and the theory of operator algebras, much of this via K-theory.

Last week there was a conference on this topic at Vanderbilt. Connes gave talks there focusing on his recent work related to renormalization. Another main topic of the conference was zeta-functions, and recent developments related to Connes's program for understanding more about them (and perhaps proving the Riemann hypothesis) using ideas from operator algebras and non-commutative geometry. The series of lectures by Consani provide a good introduction to modern ideas about zeta functions and motives that underly this program.

The CERN Council Strategy Group has produced two very interesting "Briefing Books" for its study of strategy for the future of particle physics in Europe.

For something kind of hilarious, see a paper from 2000 pointed out by one of the commenters here. It's by Gordon Kane, Malcolm Perry and Anna Zytkow and entitled The Beginning of the End of the Anthropic Principle. The authors tell us that in string theory, "in principle, and eventually in practice, all of the masses are calculable, including the up and down quark masses, and the electron mass. There is not any room for anthropic variation of the masses in a string theory." The opposite conclusion now seems to dominate string theory research, with the paper many people reference as launching the anthropic landscape that of Bousso-Polchinski written a few weeks after Kane et. al. (although Schellekens and no doubt others would claim that they had the idea much earlier).

David Gross recently gave a series of lectures at Princeton entitled "The Search for a Theory of Fundamental Reality" and they are available on-line. When introducing Gross, Curt Callan noted that Princeton University Press hopes that he'll turn his lectures into a book that they would publish. The last lecture concerns the problems and prospects of string theory and is very similar to one commented on here a couple years ago in the first real posting on this weblog. Gross says about string theory "so far, we haven't really calculated anything", and goes on to give three reasons for this:

1. More and more possible compactifications have been found, all of which seem to be equally consistent.

2. Don't understand how to handle broken supersymmetry.

3. The cosmological constant problem.

The reasons he gives for continued optimism about string theory unification despite these problems are that "we still don't know what string theory really is", and there is no consistent picture of cosmology that is understood within the string theory framework.

He explains the anthropic landscape scenario and how it destroys predictivity, then says that some of his colleagues have given up on Einstein's dream of finding a unique theory with no adjustable parameters, but that he himself won't do so until he is forced to, and he isn't forced to yet since we don't know what string theory is. He made his usual speculation about string theory leading to some still unknown new emergent view of space and maybe time, then went on to give three reasons for supporting continued research in the subject despite its failure to make any progress on its main problems:

1. String theory has given new insights into gauge theory and maybe it will help solve QCD.

2. String theory has given new insights into mathematics.

3. String theory has lead to new speculative phenomenological scenarios (braneworlds).

About point 3. he describes the possibility of evidence for such scenarios showing up at the LHC as "very unlikely" and even says that he is willing to take bets with anyone for any amount of money that the LHC will not see such things (perhaps he should have discussed this with the authors of the recent report that used these scenarios to try and sell the ILC…). I've seen this phenomenon before, but it seems to me peculiar to give as a positive argument for string theory that it leads to the study of phenomenological scenarios that you don't believe.

After his talk, a questioner asked him if string theory might turn out to just be unsuccessful (i.e. wrong), to which Gross responded "String theory can't be wrong (or even killed)". He elaborated by saying that it couldn't be wrong because it was related via AdS/CFT to N=4 supersymmetric Yang-Mills, which was in turn was related to the standard model. Somehow he felt this was an argument that string theory couldn't be wrong, only incomplete. He acknowledged that recently he had come to the point of view that string theory was not something that led to unique predictions about the world, but that it is incomplete. In this view, string theory is just a framework, like QFT, and some new ideas need to be added to it to turn it into something that really relates to the real world.

Someone asked him about LQG, and he responded by saying that he doesn't usually comment on LQG in a polite audience, that it wasn't very successful, didn't connect to GR, and was not of any interest to physics.

He ended with some pessimistic comments about the possibility that the scientific community might lose the will to go on at some point in the future as it became more and more difficult to get information about shorter and shorter distance scales, or moments closer and closer to the big bang.

Update: There's an article entitled Hard Landscape by J.R. Minkel in the June 2006 Scientific American. It deals with the Denef-Douglas work showing that finding a vacuum in the landscape with sufficiently small CC is likely to be a computationally intractable NP hard problem.

"The Douglas-Denef paper is surely a problem for drawing conclusions about what the landscape predicts," asserts Thomas Banks of U.C. Santa Cruz.

Update: Besides the well-known Theoretical Particle Physics Jobs Rumor Mill which deals with tenure-track hiring, there's now the Theoretical Particle Physics Postdoc Jobs Rumor Mill, which deals with postdocs. This year I count among the postdoc hires 31 string theorists, 12 phenomenologists and 5 hard to characterize, with several major institutions that hire multiple post-docs still only hiring string theorists. The rumors I've been hearing that only phenomenologists are getting jobs seem to be complete bunk.

Update: heppostdoc points out that the Postdoc Jobs Rumor Mill is very new and the data is incomplete. Probably complete data would show postdoc hires not as heavily weighted towards string theory.

Update: There's a conference going on near Washington this week entitled From Quantum to Cosmos: Fundamental Physics Research in Space. Mark Trodden is blogging from the conference over at Cosmic Variance.

Update: Eric Weinstein, who continues to conduct his research in mathematics and physics from within the financial industry here in New York, will be giving a talk at the Perimeter Institute on Wednesday at 2pm, with the title "Gauge Theory of Economics". Here's his abstract:

The close relationship between geometry and fundamental physics can be seen from surveying the basic equations underlying the known forces of nature. What has made these repeated appearances of gauge fields and curvature tensors particularly striking in recent years is lack of any comparable applications outside of the Standard Model and General Relativity. In this talk we will pose the question of whether Yang-Mills theory is simply a unifying principle with application well beyond its current use by exhibiting unreasonably effective applications of Gauge Theory beyond those familiar in the Natural Sciences. Armed with these examples, we will then revisit the question about what is most truly special about the Standard Model and Relativity.

Comments

1. **Tony Smith**
May 22, 2006

Peter said "… Gross responded "String theory can't be wrong (or even killed)".
He elaborated by saying that it couldn't be wrong because it was related via AdS/CFT to N=4 supersymmetric Yang-Mills, which was in turn was related to the standard model …".

If it is possible (and in my opinion it is likely) that LHC might see only a single Higgs plus stuff similar to what has already been seen elsewhere,
and
if intelligent superstring workers like Aaron Bergman feel that such results would rule out superstring-type supersymmetry so conclusively that superstring workers, in that event, would "give up and go home",
then
why does David Gross feel that superstring theory could be saved (as a theory including the Standard Model – not just as a tool for QCD) by being related to SUPERSYMMETRIC N=4 Yang-Mills ?

As Arun said in a comment on another entry on Peter's blog:
"… some people want to know the answers,
even if it means that most of their productive research years turn out to have been on the wrong path;
while
some people want to build empires. …".

David Gross may, in his heart of hearts, realize that N=4 supersymmetric YM is vulnerable to refutation by LHC results, since he does hedge his bets by saying that he might consider anthropicism if "he is forced to".

As Aaron Bergman said in a comment on another entry on Peter's blog:
"… data is just around the corner …
If anything, much anthropicism is motivated by the specter of the LHC. …".

In short, anthropicism is a way to preserve the superstring empires at Kavli, Texas, etc, no matter what experiments might show at LHC or anywhere else.

As Peter has pointed out, the price of empire preservation by anthropicism is such a threat to the standards of scientific inquiry that objection to anthropicism is something (maybe the only thing?) on which Peter, Lubos, and I all find ourselves in complete agreement.

All things considered, in my opinion the continued prosperity of the empires at Kavli, Texas, etc., would be a good thing, so I will point out an alternative to anthropicism that they might pursue:

Have seminars about currently unfashionable non-superstring (and non-LQG) theories, regardless of their level of development, to see if any of them might have useful aspects that could be incorporated into a post-LHC theoretical physics environment.
Several seminars over the next couple of years (pending LHC results) would
at worst waste some time repudiating in detail incorrect presentations (even the process of repudiation can sometimes produce useful ideas),
but
maybe some would not be worst-case,
and they might possibly produce a few useful gems that might help to save some empires.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

2. **heppostdoc**
May 22, 2006

Hi Peter,

Let me caution against drawing too many conclusions from the postdoc rumor mill. It's still very new, and most of the listings there came from one submitter, who was apparently a string theorist (or at least knew mostly about the string theory jobs). Since you posted this, there were two more pheno jobs posted, and there are probably many holes left. If I had to guess, I'd say that this year will simply be imcomplete. Maybe some useful information will come next year.

3. **Aaron Bergman**
May 22, 2006

Aaron Bergman feel that such results would rule out superstring-type supersymmetry so conclusively that superstring workers, in that event, would "give up and go home",

That's not what I meant. What I meant is that if that's the only thing to be seen, that'd be the end of large scale high energy physics experiment. Without new experiment, theory ends up pretty adrift and it's not a pretty picture what happens next.

"… data is just around the corner …
If anything, much anthropicism is motivated by the specter of the LHC. …".

In short, anthropicism is a way to preserve the superstring empires at Kavli, Texas, etc, no matter what experiments might show at LHC or anywhere else.

It's not a matter of preserving "empires"; it's a matter of people want to be able to make connections with data. Anthropicism is a (misguided in my view) attempt to do so.

4. **Tony Smith**

May 22, 2006

Aaron Bergman said: "… What I meant is that if …
[a single Higgs plus stuff similar to what has already been seen]
…[is]… the only thing to be seen [at LHC], that'd be the end of large scale high energy physics experiment. …".

There are a lot of interesting non-supersymmetric possibilities (in such areas as the Higgs – T-quark system, for example) that might be seen at LHC and explored in detail at ILC. Since Peter has told me that he doesn't want me to talk about such things in detail on his blog, I won't, but they are out there (papers in arXiv, etc) for anyone with interest to see.

Therefore, I think that the only people who would have to "give up and go home" in the stated contingency would be unshakeable true believers in conventional supersymmetry.

I also think that it would be a shame for the ILC proposals to bet the farm on true-belief in such supersymmetry, when ILC in conjunction with LHC might be very useful in exploring particle physics even if it turns out that nature does not use conventional supersymmetry.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

5. **Aaron Bergman**
May 22, 2006

You still don't understand. I'm not talking about if SUSY isn't seen at the LHC. There are plenty of other interesting possibilities. I'm talking about a single doublet Higgs and that's it. Nothing else. I'm not sure I'd bet on SUSY these days anyways, given some of the issues that have arisen. I just hope that there is something more interesting to EWSB than just a boring scalar field with a finely tuned potential.

6. **Chris W.**
May 22, 2006

For a brief summary of what appears to be Weinstein's key application of gauge theory to economics, see this page on his website.

I've often wondered why the basic ideas of gauge theory haven't been more widely applied in economics. One way to think about this is the following: How goods and services are valued in terms of an abstract medium of exchange would seem to have a large element of convention. It should be possible (one imagines) to globally shift the values of all goods and services without affecting the underlying economic "reality", implying that these values are not "real". However, shifts of value *as seen and understood by the participants in an economy* are local, not global, must be propagated through the economy, and therefore have substantive economic effects.

This is strongly reminiscent of a change from a global gauge symmetry to a local gauge symmetry of a physical field; the dynamics of the field must come into play to implement the symmetry.

7. **dan**
May 22, 2006

"Someone asked him about LQG, and he responded by saying that he doesn't usually comment on LQG in a polite audience, that it wasn't very successful, didn't connect to GR, and was not of any interest to physics."

while this does seem to be the view of the majority of string theorists, i wonder if the LQG predictions for the resolution of naked singularities

http://www.ioppublishing.com/news/1044

if observed, would change this.

8. **Aaron Bergman**
May 22, 2006

That article is very odd. Last I checked, reasonably generic gravitational collapse produced horizons.

Wilczek on the Anthropic Principle

May 21, 2006
Categories: Uncategorized

Frank Wilczek has a new Reference Frame piece in this month's Physics Today. It's about the question of whether the parameters of our fundamental physical theory are uniquely determined by abstract principles, or "environmental". He gives two reasons for suspicion about the idea that these parameters are calculable from a fundamental theory:

1. They have complicated, "messy" values and, despite much effort, no one has come up with a good idea about how to calculate them (an exception is the ratio of coupling constants in a supersymmetric GUT). He writes:

Could a beautiful, logically complete formulation of physical law yield a unique solution that appears so lopsided and arbitrary? Though not impossible, perhaps it strains credulity.

2. Some of the values are fine-tuned to make complex structures and thus life possible:

It is logically possible that parameters determined uniquely by abstract theoretical principles just happen to exhibit all the apparent fine-tunings required to produce, by a lucky coincidence, a universe containing complex condensed structures. But that, I think, really strains credulity.

Personally I don't see the same degree of believability problems that Wilczek sees here. On the first point, it seems quite plausible to me that there are some crucial relevant ideas we have been missing, and that knowing them would allow calculation of standard model parameters, by a calculation whose results would have a complicated structure.

On the second, it's not at all clear to me how to think about this. Sure, the fact that our universe has highly non-generic features means that it is incompatible with generic values of the parameters, but there's no reason to expect the answer to a calculation of these parameters to be generic. I guess the argument is that there would then be two quite different ways of getting at some of these parameters: imposing the condition of existence of life, and a fundamental calculation; and if two different, independent calculations give the same result one expects them to be related. But the question is tricky: by imposing the condition of the existence of life in various forms, one is smuggling in different amounts of experimental observation. Once one does this, one has a reason for why the fundamental calculation has to come out the way it does: because it is has to reproduce experimental observations.

Wilczek avoids any mention of string theory, instead seeing inflationary cosmology and axion physics as legitmating the idea that standard model parameters are fixed by the dynamics of some scalar fields, or something similar. This dynamics may have lots of different solutions so:

We won't be able to calculate unique values of the parameters by solving the equations, for the very good reason that the solutions don't have unique values.

The fundamental issue with any such anthropic or environmental explanation is not that it isn't a consistent idea that could be true, but whether or not it can be tested and thus made a legitimate part of science. It's easy to produce all sorts of consistent models of a multiverse in which standard model parameters are determined by some kind of dynamics, but if one can't ever have experimental access to information about this dynamics other than the resulting observed value of the parameters, why should one believe such a theory? It is in principle possible that the dynamics might come from such a simple, beautiful theory that this could compel belief, but the theories of this kind that I have seen are definitely neither simple nor beautiful. If you want me to believe in a complicated, fairly ugly theory, you need to produce convincing evidence for it, some sort of testable predictions that can be checked. Wilczek does believe that multiverse theories may provide such predictions:

Of course, the very real possibility that we can't calculate everything in fundamental physics and cosmology doesn't mean that we won't be able to calculate anything *beyond what the standard models already achieve. It does mean, I think, that the explanatory power of the the equations of a "theory of everything" could be much less than those words portend. To paraphrase Albert Einstein, our theory of the world must be as calculable as possible, but no more.*

One can't argue with this: if a model make distinctive predictions, and these can be compared to the real world and potentially falsify the model, one can accumulate evidence for the model that could be convincing. Unfortunately I haven't seen any real examples of this so far. The kind of thing I would guess that Wilczek has in mind is his recent calculation with Tegmark and Aguirre that I discussed here. I remain confused about the degree to which their calculation provides any convincing evidence for the model they are discussing.

Unlike many theorists, Wilczek personally seems to be an admirably modest sort of person, and perhaps this has something to do with why the multiverse picture with its inherent thwarting of theorist's ambitions to be able to explain everything has some appeal for him. Over the years during which particle theory has been dominated by string theory, Wilczek has shown little interest in the subject, perhaps partly due to its immodest ambitions. But I see two sorts of dangers in the way his article ignores the string theory anthropic landscape scenario which is what is driving the interest of much of the theory community in these multiverse models. As his advisor David Gross likes to point out, accepting this scenario is a way of giving up on the perhaps immodest goal he believes theorists have traditionally pursued, and one shouldn't give up in this way unless one is really forced to. None of these models is anywhere convincing enough to force this kind of giving up.

The second danger is that what is happening now is worse than just giving up on a problem that is too hard. The string theory landscape anthropic scenario is being used to avoid acknowledging the failure of the string theory unification program, and this refusal to admit failure endangers the whole scientific enterprise in this area.

Update: It has been accurately pointed out to me that Wilczek does mention string theory briefly at one point in the article ("Superstring theory goes much further in the same direction"), and alludes to it at another place (when he talks about a "theory of everything").

Comments

1. **LambchopofGod**
   May 21, 2006

   Regarding Wilczek's point 2: what Schellekens says is that if the equations of some fundamental theory managed to produce all of these numbers, then the notion that the laws of physics seem to be specially designed to produce life would be replaced by the notion that the "laws" of *mathematics* seem to be specially designed to produce life. And that would be far *worse*! I urge everybody to re-read Schellekens' paper to see what he is saying about this.

2. **Kea**
   May 21, 2006

   The classical physicist's expectation, far from being trivial, is wrong. E. Schroedinger, 1945

   This quote is from the famous book *What is life?* based on a series of lectures that Schroedinger gave in Dublin at the end of the war. Geneticists have a high regard for the place of this book in the history of biology. Anyway, there is no reason whatsoever to think that the parameters for life are anything special, except from the anthropological point of view. What *is* terrifying is the thought that the new laws of physics should say something about biology. But Schroedinger recognised that this was *inevitable* over 50 years ago!

3. **Kea**
May 21, 2006

Of course, I say that (the above) as someone that does think these parameters are calculable. What is so wrong with the idea that biology become more quantitative? It already has.

4. **Chris W.**
May 21, 2006

I don't think the degree to which biology is quantitative is the issue. Besides, that any subject can be made quantitative is trivial, unless one asks what the numbers *mean*. What is interesting are quantities and quantitative arguments that have deep *qualitative* implications. Supposing that certain physical constants of the observed universe are "anthropically" determined—that is, by a selection effect—tells us little or nothing we didn't already know (or think we know) about either life or the laws of physics.

Consideration of the anthropic principle won't get anywhere until it divests itself of the connotations of the word "anthropic" (and for that matter, "terrestrial"). The question is this: Does allowance for the existence *and natural evolution* of autonomous, self-reproducing physical systems somewhere in the universe impose any constraints on the laws of physics, or in any sense dictate their form?

5. **Kea**
May 21, 2006

Does allowance for the existence and natural evolution of autonomous, self-reproducing physical systems somewhere in the universe impose any constraints on the laws of physics, or in any sense dictate their form?

Or conversely: do new laws of physics naturally imply the existence of self-reproducing molecules, say via their appearance in a periodic table of structures?

6. **Haelfix**
May 21, 2006

Yea I agree with this idea in general. If you resort to anthropic reasoning (and I like how you mention that it is often understated that it requires somewhat tautological preexisting empirical output since we only have one universe with life in it that we can measure things in) then you face Occams razor.

eg Pick the simplest theory then that has the least amount of arbitrary continously adjustable free parameters (whether in solution space or theory space) and thats probably what you should go with. Unfortunately at this time, thats more or less the standard model + generic nonrenormalizable GR + maybe a few simple and well understood extras, like GUTs.

I mean the whole starting point of ST and most modern particle physics is to get rid of the naturalness problems, giving up on that most important goal, is premature, badly motivated and frankly annoying to the majority of physicists.

7. **D R Lunsford**
May 22, 2006

"We won't be able to calculate unique values of the parameters by solving the equations, for the very good reason that the solutions don't have unique values."

See Dirac's large numbers work. There is no reason to expect them to have determined values, rather, some inherent relation to be found out (I think a conformal one). Example – in Weyl's theory one could in principle connect the value of the background radiation with G.

-drl

8. **island**
May 22, 2006

> Personally I don't see the same degree of believability problems that Wilczek sees here. On the first point, it seems quite plausible to me that there are some crucial relevant ideas we have been missing, and that knowing them would allow calculation of standard model parameters, by a calculation whose results would have a complicated structure.

It's "ecobalanced"…

All of the anthropic coincidences are balanced "just-so", in a "goldilocks" manner, between diametrically opposing runaway tendencies. This goes way beyond the standard model to apply to every anthropic coincidence.

"Coincidentally"… ecosystems are also the most effecient means for uniformly disseminating energy, because they spread the process out over numerous "topological hotspots", over an extended period of time. There are other facets to this…

Given the physical necessity for dissipative structuring, this is the most natural, (the only), configuration that our expanding universe could have. The balance is fixed, in other words.

The expansion process can't maximize the entropic effort per the least action principle if the universe is not economically restricted to dissipate energy in the most "energy-efficient" manner possible.

Energy is not conserved if work isn't maximized, so an expanding universe assumes the form that conserves energy and the second law of thermodynamics via an ecobalanced configuration that maximizes energy.

Dirac's Large Numbers Hypothesis is flawed by his choice to fix the electric force rather than gravity. The mechanism for the AP becomes clear when you reverse this physics.

9. **Eric Dennis**
May 22, 2006

Is there a reference for any systematic study of low energy behavior over the Standard Model parameter space? It seems like a very hard problem to determine the volume of this space (in some reasonable measure) that is life-viable. At least with any kind of rigor. There is the issue of what happens in this space right near where we are — and then there is the seemingly much harder issue of totally distinct life-viable regions.

10. **Who**
May 22, 2006

One admires Wilczek for a lot of reasons, including his flair for winging it philosophically (which a person with his originality can safely do). But since he is talking about philosophical issues in physics and cosmology, I would have suggested he draw on what should become a standard handbook reference:

http://arxiv.org/abs/astro-ph/0602280
Issues in the Philosophy of Cosmology
George F. R. Ellis
To appear in the **Handbook in Philosophy of Physics**, Ed J Butterfield and J Earman (Elsevier, 2006).

"After a survey of the present state of cosmological theory and observations, this article discusses a series of major themes underlying the relation of philosophy to cosmology. These are: A: The uniqueness of the universe; B: The large scale of the universe in space and time; C: The unbound energies in the early universe; D: Explaining the universe — the question of origins; E: The universe as the background for existence; F: The explicit philosophical basis; G: The Anthropic question: fine tuning for life; H: The possible existence of multiverses; I: The natures of existence. Each of these themes is explored and related to a series of Theses that set out the major issues confronting cosmology in relation to philosophy."

Science and the philosophy of science being collective cumulative efforts, what's the use of paying GFR Ellis to write a thoughtful 60-page handbook article about the very issues Wilczek addresses if he doesn't use it? I havent yet seen the Physics Today opinion piece, but it sounds as if (characteristically) Wilczek is winging it.

Maybe the signature fresh outlook is something to be celebrate, and one shouldnt care. But when I get hold of a copy I will check to see if he cites Ellis.

11. **woit**
May 22, 2006

Who,

I think Wilczek is someone whose motivations are primarily from very practical and phenomenological considerations, without taking a lot of interest in abstract philosophical or formal points of view. He certainly doesn't refer to Ellis or anything like it. In any case, this was a short column, a semi-popular piece of science writing, not a serious scientific paper.

12. **Who**
May 22, 2006

In that case what matters is that it be entertaining and provocative, which I feel sure it is. I'll be at the library presently and have a look.

13. **Michael**
May 22, 2006

"The second danger is that what is happening now is worse than just giving up on a problem that is too hard."

Sure, that`s the big concern about anthropic reasoning. If it were only giving up on something that appears to be too hard, the problem would be rectified the moment somebody demonstrates that one can do better. But if this perception is allowed to poison the organizing principles of theoretical physics, it can become a real obstacle to future progress.

That`s precisely why we must understand what string theory is, even if the remote possibility is taken seriously that it might not be a theory of nature. Comprehensively understanding the theoretical possibilities of unifying gravity with quantum mechanics is a necessary step in determining the amount of ad hoc input needed before a theory can become predictive.

Anthropic reasoning is based on the statement that a lot of ad hoc input is needed. Not surprising that this idea is taken seriously by some, while our understanding is still limited. The smart money is on the landscape going away eventually…

14. **Who**
May 22, 2006

In reply to Michael's comment
http://www.math.columbia.edu/~woit/wordpress/?p=396#comment-11077
I would agree that
"Comprehensively understanding the theoretical possibilities of unifying **spacetime** with quantum mechanics"
would be a necessary step for a number of reasons, some perhaps more significant than the one Michael mentioned but including his.

I note that the theory of spacetime dynamics that we have to work with is GR, so that would mean understanding what is involved in unifying quantum mechanics with GR or with some variant of GR—a theory of spacetime geometry exhibiting at least the **salient features** of General Relativity in an explicit up-front way.

As a minimum, something in the theory must represent the spacetime metric as a dynamic variable—or an equivalent surrogate of the metric–because that is the geometry. And the quantum state must be a state of that metric, alias the geometry.

what makes me doubtful of Michael's reasoning (suspicious that it is mere apologetics) is that saying

"Comprehensively understanding the theoretical possibilities of unifying GRAVITY with quantum mechanics" allows "gravity" to work as a weasel word—so that we suddenly find ourselves talking about some theory which does NOT present a straightforward quantum model of the geometry of spacetime and does not actually **quantize General Relativity**. May even lack primary features of GR such as the explicit absence of a fixed background metric.

======================
In case anyone wants to inspect Michael's persuasive argument to motivate further string study I will try to paraphrase:

A. Anthropic reasoning is a poison dangerous to theoretical physics.

B. To guard against the harmful effects of this poison we must understand "the amount of ad hoc input needed before a theory can become predictive."

(This would vary quite a bit: he doesnt say what theory or what kind of theory. Some types of QG have different requirements from string and are already falsifiable. I dont fully understand what he means by "input needed" since a theory either does or does not make predictions which would allow it to be ruled out—-it does not make CONDITIONAL predictions which allow it to be CONDITIONALLY ruled out based on some further assumptions. That would regress to testing those further assumptions. But for the sake of argument let this pass. Michael's "**a theory**" probably means a stringy theory if it means anything definite.)

C. Therefore to guard against the harmful effects of this poison we must diligently study string theory. Because only in this way will we be able to discover how much "ad hoc input [is] needed before [it] can become predictive."

15. **steve**
May 23, 2006

I'm somewhat puzzled here. How many free parameters are okay? Is the ultimate goal to have a testable theory in which all numerical values are functions of pi, e, i, etc. with no observational input? In order for that to happen, you'd have to have a theory with no empirical free parameters which explained the known data but made predictions about currently unknown but discoverable phenomena. Good luck with that, but I wouldn't hold my breath.

On the other hand, if some "brute facts" are allowed to remain unexplained in the ultimate theory, how do you know when you've reached the irreducible minimum? My guess is that it will come down to taste, faith, and the resources available to keep looking, since there seems not to be any reliable inductive method for knowing when to quit.

16. **Kea**
May 23, 2006

Steve

The question is about the parameters of the Standard Model and GR. If we can derive these, and there is every reason to believe it is possible, then we have a complete theory of SM+GR. This does not mean that future investigations will not reveal the necessity of more, as yet unknown, parameters of a post SM physics. So, no theory of everything – we are only human – but from our limited perspective the next breakthrough might well have the semblance of something grand.

17. **nostradamus**
May 24, 2006

Newyear 2007: Lenny Susskind conjectures that the consequences of String Theory are not understandable for the homo sapiens sapiens. He calls it the finite-brain conjecture.

January 2007: String Theorists all over the world realize that the f-brain conjecture naturally supports their believe that String Theory is the Theory of Everything, and moreover, it successfully explains the current crisis. The conjecture is strongly criticized for relating two not fully understood subjects, String Theory and the human mind.

Summer 2007: Ed Witten proves that the conjecture can not be proved. String Theorists realize that their quest is over. Either String Theory is not the Theory of Everything, or it is, and then they can never understand it. Gary Horowitz generalizes the statement by attaching bubbles of nothing to the f-brain. Lee Smolin points out that the conjecture fails for independent brains, but is widely ignored by the community.

End 2007: Thousands of String Theorists retire. They have fulfilled the task of finding the Theory of Everything. They have revealed what was possible, and have shown that the full beauty of the theory is to vast to fit into the human brain. They declare the end of theoretical physics, until intelligent designers improve the human f-brain.

January 2008: Those who are left focus on brain-independent theories.

18. **Peter Shor**
May 24, 2006

I think I've said this before on this weblog, but I will say it again. I don't understand why string theorists are working on the landscape and the anthropic principle, which as far as I can tell has virtually no mathematically well-defined content, when they could be working on the perfectly well formed, although probably unverifiable experimentally question, of "exactly what happens to the information inside it when a black hole evaporates." (Some of them undoubtedly are; let me apologize to these string theorists.)

In other words, if you don't want to work on physics (i.e., stuff you can get at by experiments), at least work on mathematics.

19. **Aaron Bergman**
May 24, 2006

(Some of them undoubtedly are; let me apologize to these string theorists.)

I think you'll find that the majority of them are; just look at hep-th on an given day. The landscape people just happen to be louder.

20. **Moshe**
May 24, 2006

Peter Shor, irrespective of one's opinion on the anthropic principle, it is a sociological fact that this idea as an approach to cosmology predates the string landscape and is still practiced mostly by non-string theorists (such as Wilczek), not that I think such sociological issues are that crucial…
\
Also, again irrespective of one's opinions on the physical content of the anthropic principle, the study of the landscape certainly motivated lots of interesting and well-posed mathematical problems (e.g. see the work of Douglas and collaborators).

21. **Chris Oakley**
May 24, 2006

I am not sure about Nostradamus here's predictions. FWIW, here are my own:

2006: "I can't believe that you guys are so impatient," say leading String Theorists. "Here we are, about to discover the answer to the Ultimate Question – the answer to Life, the Universe, Everything and you are demanding that we connect or theories to the real world. *Of course* we will do that. That and more. You just have to be patient."

2026: "I can't believe that you guys are so impatient," say leading String Theorists. "Here we are, about to discover the answer to the Ultimate Question – the answer to Life, the Universe, Everything and you are demanding that we connect or theories to the real world. *Of course* we will do that. That and more. You just have to be patient."

2040: Ed Witten dies and within six months is canonised Saint Edward of New Jersey.

2046: "I can't believe that you guys are so impatient," say leading String Theorists. "Here we are, about to discover the answer to the Ultimate Question – the answer to Life, the Universe, Everything and you are demanding that we connect or theories to the real world. *Of course* we will do that. That and more. You just have to be patient."

2066: "I can't believe that you guys are so impatient," say leading String Theorists. "Here we are, about to discover the answer to the Ultimate Question – the answer to

Life, the Universe, Everything and you are demanding that we connect or theories to the real world. *Of course* we will do that. That and more. You just have to be patient."

22. **Mentos**
May 24, 2006

Kea said:

"The question is about the parameters of the Standard Model and GR. If we can derive these, and there is every reason to believe it is possible, then we have a complete theory of SM+GR."

Just out of curiosity, what gives you confidence that it is possible to derive unique values for the parameters of the SM+GR?

I'm sure it would be wonderful if that were true. But I have no idea, one way or the other, whether it is true (either in string theory or in some other, as yet unknown, approach).

23. **nostradamus**
May 24, 2006

All physicists are equal. But some are more equal than others. Be kind. Open the exit door for String Theorists, and offer them a helpful hand. They know it's time to go.

24. **Peter Woit**
May 24, 2006

Moshe,

I think justifying landscape work by its application to mathematics is a real stretch. Sure, it has generated some well-posed mathematics problems, but very few interesting ones, especially in comparison with other areas of string theory (e.g. topological strings), which have generated a lot of interesting mathematics. I spend a lot of time talking to many different mathematicians, especially ones interested in physics, and I can think of exactly one who has ever had an interest in anything generated by landscape studies (in a problem that asked about asymptotics of numbers of sections).

Douglas has a review promoting this kind of work

http://www.arxiv.org/abs/math-ph/0508019

Notice that the only references to work by mathematicians is to that of two of them who have collaborated with him on the problem of asymptotics of sections.

25. **B**
May 24, 2006

nostradamus said:

Newyear 2007: Lenny Susskind conjectures that the consequences of String Theory are not understandable for the homo sapiens sapiens. He calls it the finite-brain conjecture.

Chris Oakley said:

2066: "I can't believe that you guys are so impatient," say leading String Theorists. "Here we are, about to discover the answer to the Ultimate Question – the answer to Life, the Universe, Everything and you are demanding that we connect or theories to the real world. Of course we will do that. That and more. You just have to be patient."

Nostra, it's already happend. But we guys have to be patient, it's far too early…

"This raises the possbility that we might someday convince ourselves that string theory contains candidate vacua which could describe our universe, but **that we will never be able to explicitly characterize them. This would put physicists in a strange position, loosely analogous to that faced by mathematicians after Gödel's work. But it is far too early to debate just what that position might be,** and we repeat that our purpose here is simply to extrapolate the present evidence in an attempt to make preliminary statements which could guide future work on these questions."

Computational complexity of the landscape I

Authors: Frederik Denef (KU Leuven), Michael R. Douglas (Rutgers and IHES)

http://lanl.arxiv.org/abs/hep-th/0602072

26. **nostradamus**
May 24, 2006

sigh I know. It's more an extrapolation than a prophecy. (That's because my brain is finite.)

http://lanl.arxiv.org/abs/hep-th/0602286

Statistics on the Heterotic Landscape:
Gauge Groups and Cosmological Constants of
Four-Dimensional Heterotic Strings
Keith R. Dienes

p 57

"By contrast, the second danger can be called the "Gödel effect" — the danger that no matter how many conditions (or input "priors") one demands for a phenomenologically realistic string model, there will always be another observable for which the set of realistic models will make differing predictions. Therefore, such an observable will remain beyond our statistical ability to predict. (This is reminiscent of the "Gödel incompleteness theorem" which states that in any axiomatic system, there is always another statement which, although true, cannot be deduced purely from the axioms.) Given that the full string landscape is very large, consisting of perhaps 10^500 distinct models or more, the G¨odel effect may represent a very real danger. Thus, since one can never be truly sure of having examined a sufficiently sizable portion of the landscape, it is likewise never absolutely clear whether we can be truly free of such Gödel-type ambiguities when attempting to make string predictions."

27. **Peter Woit**
May 24, 2006

The invocation of Godel incompleteness is pretentious and absurd since there is nothing deep, subtle, or fundamental going on here. Lots of speculative ideas about physics turn out to be useless once you look into them because they can't be used to actually predict anything about the real world. String theory unification is just one more such useless idea, and the only strange thing about it is the sociological phenomenon of serious scientists refusing to abandon a failed project.

28. **anonymous**
May 24, 2006

A continually stalling project is actually a very safe territory to work on. If string theory was in danger of closing shop by producing the 'final theory' tomorrow, it would be hazardous to base a career on string theory.

On any large project, when success arrives the project workers are out of jobs, because far fewer researchers are needed to continue working on the completed product. If and when the much-touted 'final theory' arrives, a vast change in careers goals will be required.

I think that, far from celebrating, most people will be enraged that the fundamental physics will reach completion. Anyone who say climbs a mountain for the first time removes the opportunity for anybody else to do so, and in that sense is very selfish.

29. **steve**
May 24, 2006

Kea: I'm still not getting it. Any unification of GR + SM (assuming that's possible) will still have some number of parameters. Either these will be empirical parameters with no further justification OR they will all be fundamental math constants such as pi. My question is: How do you know when you've reduced the number of empirical parameters down to the bare minimum, i.e. when is further unification pointless? And how do we know that point hasn't already been reached?

30. **Kea**
May 24, 2006

How do you know when you've reduced the number of empirical parameters down to the bare minimum, i.e. when is further unification pointless?

When you have none *within* the theories that were being unified. Of course, you will probably be introducing *new* ones, because that's what usually happens. But there is no need to focus initially on the new ones when just understanding the unification is a big enough problem.

And how do we know that point hasn't already been reached?

Well, that depends what you mean by the word *know*. Most good physicists that I know would bet their life on the fact that we haven't reached that point with the anthropic principle. And the idea that no one has any idea about any alternatives is ludicrous.

31. **LambchopofGod**
May 24, 2006

Peter Shor: as has been pointed out, there is probably more work being done on the fate of information in black holes than on the landscape. However, I am mystified by Aaron B's remarks. Surely if it is indeed true that string theory inevitably leads to a landscape, this is something that we want to know. Conversely, any discovery of a mechanism that selects a point or a region in the landscape would be an enormous advance. The problem is precisely that there are *not enough* people working on these things, and way too many working on essentially trivial technical exercises justified by the feeble hope that they will teach us "what string theory really is".

Peter Shor again: "In other words, if you don't want to work on physics (i.e., stuff you can get at by experiments), at least work on mathematics. "

Sorry, this was tried for a long time and it did not work very well at all. There was a time when physicists would rush out and learn a new mathematical technique [K-theory, even category theory…] at the drop of a hat. It didn't really get us anywhere at all.
The interest among physicists in pure mathematics has collapsed for a good reason. Nowadays people pin their hopes on the field where the data are, viz cosmology. And work on the landscape should be part of that. I'm not saying by the way that all of these developments are good, just that there is a reason for what has happened.

32. **Thomas Larsson**
May 24, 2006

Sorry, this was tried for a long time and it did not work very well at all. There was a time when physicists would rush out and learn a new mathematical technique [K-theory, even category theory…] at the drop of a hat. It didn't really get us anywhere at all.

Lambchop, of course random new math does not help. You have to learn, and perhaps invent, the right kind of new math. K-theory and category theory wouldn't have helped Einstein finding GR or Dirac finding his equation. Tensor calculus, and the representation theory of SU(2), did the trick. And that was new math (at least new to physicists) at the time.

It has been obvious to me for almost two decades which kind of new math is needed to quantize gravity; since spacetime diffeomorphisms play an fundamental role in gravity, we need to understand the diffeomorphism group and its projective representations. This is why I started to search for, and later discovered, how to generalize the Virasoro algebra to several dimensions.

It really baffles me that others don't want to look in this direction. Urs Schreiber even independently made the same observation, but then lost interest when he realized that I agreed with him.

33. **Moshe**
May 24, 2006

Peter, fair enough, I made a mistake by adding the word "interesting" which represents an opinion. Strike it then, it is not essential to my response to Peter Shor's remark.

34. **Aaron Bergman**
May 25, 2006

There's two issues here. The first is whether it means anything to make statistical predictions. I don't believe it does. The second, however, is that, as best I can tell, there's this huge region of parameter space over which we have absolutely no control. And, even in the vacua about which we can say something, it is almost always in the context of effective field theory with a conjectural nonperturbative scaffolding. So, at best we're learning about some subset of vacua with the hope that there isn't some unknown phenomenon that ruins the whole game. Studying these examples might be useful in that they could inspire new phenomenological scenarios, but I don't think there's much hope of applying string theory to the real world if we don't even know what it *is*. My personal opinion is that that's a much more useful question to research (and do "trivial technical exercises") than to create increasingly complicated tinker-toy vacua that still bear very little resemblence to the world as we know it. I think one of the reasons you're seeing a resurgence of interest in things like AdS/CFT is that it's one of the few things in string theory that, if you kick it, it'll actually hurt your foot. And, for what it's worth, those categories you deride give the best description of gauge theories on D-branes at a singularity and, hence, AdS duals of field theories other than N=4 SYM. Even if string theory doesn't turn out to be the theory of everything in our world, given that it does appear to be *a* theory of quantum gravity, we can at least try to learn how it solves the usual problems associated with that.

But that's just me. I've been wrong before (lots), and I don't have a job anyways.

35. **LambchopofGod**
May 25, 2006

You'll get a job — you just need to switch to cosmology. 🙂
Come on, don't you think that something like

http://arxiv.org/abs/hep-th/0510046

[which, by the way, invokes the landscape]
is far more worthwhile than hopeless calculations aimed at finding out what string theory really IS, but which never seem to lead anywhere? True, they didn't really achieve all that much, but at least they did something that goes in the right direction, towards the real world. Historically [yes, I know this sort of argument is often crap, but anyway….] people almost *never* knew what their theory really WAS until much later. Schroedinger didn't know about probability. Einstein thought that SR could not really handle acceleration. Everybody made all kinds of idiotic mistakes, but in the end they found out what their theories really WERE long *after* they made some connection with the real world. Yes, trying to find out what the theory really IS sounds very noble. But Don Quixote felt the same way about attacking those windmills.
And as for categories — forget it! Even if they are the key to the Universe [like quaternions? 🙂] nobody will pay any attention.

36. **island**
May 25, 2006

> Most good physicists that I know would bet their life on the fact that we haven't reached that point with the anthropic principle. And the idea that no one has any idea about any alternatives is ludicrous.

Ludicrous is the right word.

I think that it is quite possibly relevant that quantum mechanics isn't inherently able to describe dissipative structuring since it depends very much on Hamiltonian mechanics… except by way of the "Master Equation in the special, Lindblad form, which derives that flatness acts as a natural harmonic damper mechanism that keeps the imbalanced universe from evolving inhomogeneously, so this is the most natural configuration… if the universe is finite and closed… given inherent asymmetry in the energy. This will necessarily maximize the time that the expansion process takes, and that's what a flat universe accomplishes via anthropic structuring.

37. **B**
May 25, 2006

Peter Woit Says: […]
and the only strange thing about it is the sociological phenomenon of serious scientists refusing to abandon a failed project.

I don't find that so strange. I guess they just don't know what else to do. Front research in physics has fallen apart into specialized subclasses, and it takes time and effort to change subject. Even for a postdoc it's become hard to change the field. (Even more so since knowing the relevant people is an non-negligible factor.) If you have worked for some decades on String Theory, you just don't drop the pen and say: Hey, the landscape sucks, lets go make spin foam models instead.

Or, to put it differently, someone should think about what we ought to do with all the String Theorists.

Best, B.

38. **Peter Woit**
May 25, 2006

The attitude of "LambchopofGod" encapsulates well everything that is wrong with what is going on in particle theory these days. The idea that "too much math" is what caused the problematic state of the field has caused a backlash against investigating new ideas about mathematics and fundamental physics and this is both unfortunate and extremely unhealthy for the future of the subject.

The current prejudice seems to be that the problem isn't string theory, but all that nasty math stuff, and by doing more "physical" calculations in string theory, one will manage to get closer to reality. This is nonsense. There are very well-understood reasons by now why string theory can't tell you anything useful about anything collider experiments or cosmology-related experiments are going to see in our lifetimes. Pretending this isn't so is just going to lead to completely useless work and the continued destruction of this field.

I don't happen to believe that trying to think about "what string theory really is" is the best way to learn anything, but it's a lot more promising than wasting ones time trying to connect string theory to cosmology or LHC physics because that sounds good. At least there's a chance of learning something.

39. **Peter Woit**
May 25, 2006

Aaron,

Sorry to hear you're having job problems, the job situation in this field has sucked for a very long time, and in some ways is worse now than ever, especially for anyone who thinks interesting new math and physics go hand in hand. Undoubtedly you've thought about this, but you might have better luck looking in math departments. Good luck!

40. **Bert Schroer**
May 25, 2006

It is sad to see that virtually all the Millenium Nobel laureates in particle physics left their critical mind at the wardrobe to the string club where the new reality is being manufactured.
The following is an illustration for how facts are manufactured in String theory.
Witten's introduction of the Maldacena conjecture:
"Recently it has been proposed by Maldacena that large N-limits of certain conformal field theories in d dimensions can be described in terms of supergravity and string

theory on the product of d-+1 dimensional AdS space with a compact manifold."
(Note the "proposed")
After more than two thousand citations the Witten passage changed to
(Berenstein, Maldacena and Nastase):
"The fact that large N gauge theories have a string descirpition was believed for a long time. These strings live in more than 4 dimensions….".
The conjecture has been manufactured into a correspondence. The answers to why such things happen are sociological and have nothing to with science (only the the sociology of scientists). String theory is a community and then all the researchers in such a social setting are compelled to accept things like that; no one can refuse to recognize it without undermining the veracity of the whole construct.
Yesterday I listened to one of the available online talks of David Gross. I didn't believe that this is the same Gross whom I met (and discussed physics with a long time ago in Aspen) when he claimed that string theory cannot be proven wrong because it is linked via the Maldecena conjecture to the standard model. What I find so incredible is that the audience in a place like Princeton (where Pauli and other critical minds spend many years) receives such statements with applause.
Having experienced such a weird video, the fact that Wilszeck has not a single profound word of critique for this millenium circus was almost expected.

41. **Aaron Bergman**
May 25, 2006

I should say by the way, that if my previous comment came across as bitter, it wasn't meant to be. I made a conscious decision about what I wanted to do, and I wasn't expecting to get a physics position given my choice of research topic and productivity. Unfortunately, the idea of applying for math jobs occurred to me too late in the game.

For lambchop, actually I like that paper precisely because it is in AdS/CFT. As for the rest, I don't really see where these vacuum contraptions have gotten us thus far. It hasn't given us any new insight into the cosmological constant problem, and there's next to nothing being said about neutrino decay or proton decay from dimension five operators. The best prospects I see are for new mechanisms for inflation (which really is a lot ickier than it's made out to be even ignoring the question about the initial conditions) and supersymmetry breaking (assuming supersymmetry even exists at the weak scale given that current experiments either seem to indicate a fine tuned MSSM or something a bit more non-minimal). I'm not denigrating this, by the way; outside of mathematics, string theory has been most successful in inspiring new ideas in field theory and phenomenology. The odds of this leading towards some sort of smoking gun for string theory, however, seem rather long to me. Of course, I've always been a formalist at heart (closet mathematician, it's been accused), so there's a lot of my personal prejudices speaking here.

And on the mathematics front, in the realm of actually trying to construct vacua like our world (as opposed to, say, immediately postulating gadzillions of them), the constructions are all rather mathematical, (0512177 and 0512149 or 0512170 where they try to do SUSY-breaking).

42. **Who**
May 25, 2006

Bee wrote in a constructive simpatico spirit (IMO) as follows:

> …*serious scientists refusing to abandon a failed project*.
>
> I don't find that so strange. I guess they just don't know what else to do. … it takes time and effort to change subject. …If you have worked for some decades on String Theory, you just don't drop the pen and say: Hey, the landscape sucks, lets go make spin foam models instead.
>
> Or, to put it differently, someone should think about what we ought to do with all the String Theorists.

I saw a sign that someone may have been thinking about it. Baez left a trail of crumbs leading to spinfoam when he titled his last two papers
Quantization of strings and branes coupled to BF theory
and
Exotic Statistics for Strings in 4d BF Theory

Now we will see if the ants find the sandwich.
http://arxiv.org/find/grp_physics/1/au:+baez/0/1/0/all/0/1

4D beef means no extra dimensions.
Best to you too.

43. **urs**
May 25, 2006

> I saw a sign

said who?

I have a summary of the first of these two signs here:

.

Notice, though, that not everything that looks like a shoestring is necessarily an F-string.

But, as someone else already pointed out, there might be a relation here to some sort of solitonic strings.

44. **urs**
May 25, 2006

Hm, I am always having problems with hyperlinks in this comment section. Where there is nothing in the above comment it should read

http://golem.ph.utexas.edu/string/archives/000777.html

45. **Chris Oakley**
May 25, 2006

Aaron,

You should come and work in the software business. The money is better than academic research, it is genuinely creative, people are more interested in what you do and if you get something wrong you will normally know about it straight away. Of course, not everyone would consider the last one an advantage, but I do. In theoretical HEP as currently practised if you get things wrong then you can patch them up *ad infinitum*, or – if the worst comes to the worst – just invoke the landscape. This is not very satisfying.

46. **Who**
May 25, 2006

Chris, I dont know about you or Aaron but I respect it a whole lot when children are curious about where babies come from and when physicists are obsessed to find the fundamental degrees of freedom of spacetime and matter. so I find it jarring when somebody says, to one of these obsessed ones, go do software. it sounds dismissive. I looked up Nature in the dictionary and it comes from the word for "birth". Asking what the world is made of is a sacred question (if any question is).
And maybe the whole scientific enterprise has some elements of high comedy (like self-delusion, hubris, the failure of the best, over and over—or maybe I mean tragedy).
Anyway I want to say to whoever might be listening: Don't stop looking for the sandwich!

47. **Chris Oakley**
May 25, 2006

Who,

If this curiosity is leading most of these grown-up kids into exploring ever more complex mathematical neverlands then I would say that they would be better off without it.

Besides, the search for answers to ultimate questions – done my way – resulted in no academic job. Maybe Aaron is finding the same thing.

48. **Alejandro Rivero**
May 25, 2006

No, modern theoretical physicists are not driven (mainly) by curiosity, but by the joy of solving problems.

49. **Eugene Stefanovich**
May 25, 2006

Who:

It is quite possible to "look for the sandwich " and work for a software company (or a patent office) at the same time. This has a great advantage of being free to choose your own "crumb trail" and stay out of the crowd.

50. **Kea**
May 25, 2006

Who

Did you put some category theory butter on the sandwiches?

51. **Peter Woit**
May 25, 2006

urs,

I'm not sure what the problem is, but the html that I see in your comment has the proper syntax for a text link, but with no text. Let me see if I can reproduce your problem

this is the link text.

52. **Peter Woit**
May 25, 2006

Nope, can't reproduce it., when I put in by hand the html code for a link, as above, it works.

53. **Who**
May 25, 2006

Kea
It was John Baez literary simile (about how ants search for stuff by apparently random wanderings and citing each other's papers so as to leave a scent trail). You have to ask him further questions about condiments–butter in particular.

Baez simile is in post #15 of this discussion-board thread:
http://www.physicsforums.com/showthread.php?t=121501

Come to think of it some ants go nuts over butter, don't they?
Comments by Baez from the same time, on related topics (some category theory), are in this thread beginning at post #12
http://www.physicsforums.com/showthread.php?t=120985

54. **D R Lunsford**
May 25, 2006

Peter said

The invocation of Godel incompleteness is pretentious and absurd since there is nothing deep, subtle, or fundamental going on here. Lots of speculative ideas about physics turn out to be useless once you look into them because they can't be used to actually predict anything about the real world. String theory unification is just one more such useless idea, and the only strange thing about it is the sociological phenomenon of serious scientists refusing to abandon a failed project.

That was nice 🙂

-drl

55. **D R Lunsford**
May 25, 2006

TL said

It has been obvious to me for almost two decades which kind of new math is needed to quantize gravity; since spacetime diffeomorphisms play a fundamental role in gravity, we need to understand the diffeomorphism group and its projective representations.

You need to deal intrinsically with the volume element. This is what Dirac and GR have in common.

-drl

56. **Thomas Larsson**
May 26, 2006

Aaron, you have my symphaties, since I have been in the same position myself; after completing a 4-year postdoc, I was unable find an academic position. I didn't try very hard, though, since other priorities were more important to me at the time: staying in my home town, getting a permanent income, and starting a family. Leaving academia is not the end of the world; just about everyone I know who has been in a similar position have eventually landed on their feet.

57. **B**
May 26, 2006

Who Says:
Bee wrote in a constructive simpatico spirit (IMO) as follows:

Or, to put it differently, someone should think about what we ought to do with all the String Theorists.

Well, I am a German social democrat. We don't just fire people. We give them a second education, and a job so they feel useful. It's called solidarity. The resistance to changes can be significantly lowered when there are as little people as possible suffering from it.

Alejandro Rivero Says:
No, modern theoretical physicists are not driven (mainly) by curiosity, but by the joy of solving problems.

Not sure about that. There are certainly those who find joy in doing what they have learned and what they were educated for. But that's got nothing to do with physics in particular. If I talk to people I often find that they were originally driven by curiosity, but they don't dare to follow it cause it's not career-wise. You wouldn't believe how many people have told me, they would rather do this or that than following the main-stream, but they have to think about their job and family and stuff. That's a problem which can only be solved by reevaluating which research is worth funding, and what 'success' means (imo certainly not getting a paper published).

Best, B.

58. **Christine Dantas**
May 26, 2006

Eugene Stefanovich Says:

It is quite possible to "look for the sandwich " and work for a software company (or a patent office) at the same time. This has a great advantage of being free to choose your own "crumb trail" and stay out of the crowd.

It depends whether you get exhausted by a demanding job as well as having other responsabilities. I am free enough to follow any path that I feel like, but almost no energy is left of me to explore things in a deeper level. So I am completely stuck.

Best wishes,
Christine

59. **LambchopofGod**
May 26, 2006

PW said: "The attitude of "LambchopofGod" encapsulates well everything that is wrong with what is going on in particle theory these days. The idea that "too much math" is what caused the problematic state of the field has caused a backlash against investigating new ideas about mathematics and fundamental physics and this is both unfortunate and extremely unhealthy for the future of the subject."

I didn't say that "too much math" is the problem. I said that very mathematical investigations of the basics of string theory have been tried, as a way of pushing the subject forward. It has been tried, even, in my own insignificant way, by me. Eventually I was forced to admit that it didn't work. Believe me, I would love to see some deep theorem in global differential geometry used as the basis of some vacuum selection principle. That would be wonderful. But things like that have been tried and they didn't work. Now it"s time to try other things {like trying to bend ads cft to get it to say something about desitter }

60. **Bert Schroer**
May 26, 2006

The problem in particle physics is never too much mathematics, rather it is giving less physically suitable mathematics too much hegemonic power over physics.
There are of course some lucky historical circumstances, as the discovery of quantum mechanics, where, after the Born-Jordan observation of the role of infinite matrices in Heisenberg's paper, the intrinsic logic led directly to Hilbert spaces and operators (interestingly enough it was first Fritz London and not John von Neumann who saw this first, but he was at the time a little assistant at the TU Stuttgard and his publication got easily overlooked by those who were leading the quantum dialogue); but the best situations for a happy marriage of physics and mathematics are those in which the mathematical and physical concepts developed at the same time and allowed to merge in an early state. The most perfect example (unfortunately little known outside a small circle of experts) is the birth of modular operator algebra theory from a confluence of vast generalization of the unimodular aspect of (the Haar measure) of noncompact group algebras into the heart of operator algebras, together with the physics ideas of Haag (leading to the Haag-Hugenholz-Winnink paper where modular properties are combined with the KMS condition) on how to do thermal quantum theory directly for open systems (see Haag's book). This has recently led to the totally intrinsic (i.e. without the use of field-coordinatizations) concept of modular localization. In my whole professional life I have never seen such a perfect match and all my recent results and presently evolving ideas are around these new and mathematically quite demanding concepts in QFT.
There are also less fortunate marriages. In the middle of the 70s during a year at CERN there were some interesting mathematical properties showing up in low-dimensional QFT. This was the time when Euclidean methods in QFT were on almost everybody's mind. Before I knew anything about Atiyah-Singer index theory, I observed a connection between Fermion zero modes and the winding number of the (generic) abelian gauge field in the Schwinger model (2-dim. massless QED). I had the intense feeling that I was dealing with a tip of an iceberg. But since I had no proof, those speculative claims of my seminar presentation which went beyond the concrete model calculations were rightfully criticized by Roman Jackiw. Only when I finally went down to the Geneva University library I realized that I had a very special case of an impressive mathematical edifice. I went on happily for several years, learning the mathematics of this new trade, but finally I began to have doubts

whether such a (often banalized) use of the Euclidean is really the wave of the future in real time QFT. My doubts strongly increased when I came back to CERN after 10 years and saw all those enthusiastic wide-eyed yougsters using the phrase "topological" (this was after Atiyah-Witten) in almost every second sentence.
In contrast to Peter I am concinces that this of marrying precise mathematics with ever increasingly mataphoric physics was the beginning of the trip into stringlandia.

61. **Peter Woit**
May 26, 2006

Lambchop,

I agree with you that new math is not going to solve problems like finding a specific string theory vacuum. Buth neither is any physical idea, since this is an inherently insolvable problem. What really bothers me is that instead of admitting this, people blame past failure on the mathematical methods being tried. It would be best if people would just move on to more promising problems, but if they're not going to do that, at least trying out new mathematical methods sometimes leads to a useful new technique that can be applied elsewhere. Trying to get at the "physics" of a situation where there is none leads nowhere.

Bert,

My perspective is a bit different. I think there is still a huge amount to learn by pursuing ideas about TQFT and gauge theory, ideas that are completely independent of string theory. The problem with research in this area is that people will only fund it and pay attention to aspects of it that are somehow related to string theory. Studying those has led to some interesting things, but little recently. The non-string theory connections to gauge theory are quite worth exploring, but no one wants to since it's a hard, long-term research project which no one wants to hear about because it's not string theory.

62. **Bert Schroer**
May 26, 2006

Of course there is Peter, in fact I am exploring problems (massless, spin1 and 2), but already the preliminary results creates doubts whether the historical name (which is primarily a classical name) "gauge" is very appropriate.
On the other hand names in particle physics are increasingly hollow words and if you mean the physics behind the word gauge theory (in particular the physical, alias gauge invariant results) then we are on the same wave length. In fact I am probably much more conservative on the side of physics than you are, but I am upholding the right to use any mathematical concepts which are the most appropriate to implement the physical principles (which only change on scale of one century) and concepts and I happen to think that the present formalism of gauge theory does not meet this test (see my remarks I made earlier, giving support to Thomas Larson in Fantastic Realities, probably more radical than what he had in mind).

63. **MoveOnOrStayBehind**
May 26, 2006

B:

"
Or, to put it differently, someone should think about what we ought to do with all the String Theorists. ….

Well, I am a German social democrat. We don't just fire people. We give them a second education….
"

A nuclear physicist by education who now does "phenomenological quantum gravity" teaches string physicists what to do after their project has failed? By all means, they can't wait to hear!

This is getting more absurd every time I read around here.

64. **Alejandro Rivero**
May 26, 2006

Since Newton age (and even Democritus and Archimedes!) this needed "new math" has been a new math related to the understanding of geometry, particularly of differential geometry. The key word here is not "new"; it is "understanding". Advances in physics are related to our understanding of geometry. This close marriage is maintained in the algebraic side via some dualities, for instance the one between commutative algebras and manifolds.

Advances in string let to set up and solve new problems on mathematics, and even create new mathematics, but it is still to be seen how they help to understand any branch of mathematics, nor to say differential geometry.

65. **Bert Schroer**
May 26, 2006

Alejandro,
Nobody is against geometry, but it has to come from the midst of raw local quantum physics rather than offering a geometric/topological mathematical (or classical physics) stick to particle physics to jump over.

66. **Eugene Stefanovich**
May 26, 2006

IMHO playing with math models and hoping to find something interesting about physics is equivalent to "searching the key far from the lamppost". The chances of finding the key are very slim. I would rather get the clues about physics from … well, physics. More precisely, from experiment. Try to build your theories by only using concepts and ingredients that are (at least, in principle) experimentally measurable. Reject quantities that have no observable counterparts in nature. For example, I would not hesitate to throw away gauges. They have no observational meaning by definition.

67. **Kea**
May 26, 2006

Nobody is against geometry, but it has to come from the midst of raw local quantum physics…

Which is precisely why the category theoretic understanding of local geometry is the *only* way forward.

68. **Kea**
May 26, 2006

The AQFT papers that I have glanced at recently all talk about functors from some suitable category of (local) spaces, such as the category of strongly causal manifolds.

Where exactly do you think this is leading?

69. **Bert Schroer**
May 26, 2006

Kea,
your observation is not quite correct. Categorical ways of arguing were actually used by people who you would not exactly count as algebraic QFTists as e.g. Moore and Seiberg in their approach to the structural properties (related to the new braid group statistics) of chiral observable algebras and their superselection structure In a paper "Einstein causality and Artin braids" which was written at the same time and addressed the same problem (by Rehren and myself) we extract this structure from a new manifestation of the old causality principles which was in a AQFT spirit without any use of categorical arguments (afterwords this spirit was strengthened in collaboration with Fredenhagen who had more experience with AQFT methods).
However categorical arguments are sometimes an ultimate recourse (as in the case of the formulation of the local covariance principle in QFT in CST to which you are referring) and should be considered (in my view) as tentative to be replaced by more geometrical arguments. But when I do use the word geometric, I do not mean that kind of geometry coming from classical physics via quantization (Chern-Simon geometry, geometry of euclidean functional integration manifolds etc) but rather those geometric concepts which AQFT manages (e.g. via modular localization) to extract directly from the autonomous local quantum physical principles, and which in all cases up to now amounted to a widening of the scope of causality and spectral stability principles and not to their "revolutionary" liquidation.
There is a big difference between this geometry from AQFT and that e.g. in the Atiyah-Witten (or for that matter string theory) setting.

70. **Kea**
May 26, 2006

But when I do use the word geometric, I do not mean that kind of geometry coming from classical physics via quantization…but rather those geometric concepts which AQFT manages (e.g. via modular localization) to extract directly from the autonomous local quantum physical principles…

71. **Kea**
May 26, 2006

And if I might be permitted to repeat my question: what is the geometric language for these geometric concepts?

72. **Bert Schroer**
May 26, 2006

Kea, let me try to give an answer to your question inasmuch as this is possible in a weblog.
The modular operator theory is capable to extract spacetime localization from the abstract domain properties of the modular objects related to an operator algebra. The abstract (algebraic) relative positions of operator algebras acting in a common Hilbert space leads (via that modular theory) not only to spacetime symmetries but also encodes the full content of QFT (statistics, inner symmetry, scattering theory…). The theory is pretty much in its beginnings and some of the results may seem miraculous (especially to those who thought that Lagrangian quantiation contains alrady the main messages about QFT) but any mysterious appearance is transitory (simply due to unfamiliarity) and its aim is to de-mystify and it is not to be thought of as a theory of everything. From a pure mathematical point of view it is somewhat related to Vaughn Jones theory of inclusions (subfactors), but the involved algebras are of a different type which is inexorably linked with localization (in my papers in http://www.lqp.uni-goettingen.de/papers/06/04/ I have called that kind of algebra "monade"). If you are interested in some partial results of an ongoing research have look at math-ph/0511042 where there are also references to previous work.
Its main message is perhaps that if a more than 70 years old theory allows for such a radical different approach, it is not (despite the string theoretical carricatures of QFT) yet anywhere near to closure.

73. **Kea**
May 26, 2006

Dear Bert

I do not doubt for a moment that what you claim is true. It sounds fantastic and exciting. It does not, however, answer the question. You used the word *algebra* five times in the above statement. Perhaps we could begin with a clarification of one single term: what do you mean by *domain property*?

74. **Bert Schroer**
May 26, 2006

Although the algebras which feature in those constructions are algebras of bounded operators, the Tomita involution S`(see the mentioned literature), which is a kind of master operator capturing collective properties of all operators in the algebra, is unbounded and its domain of definition is (in the field theoretic context) related to the geometric localization of the algebra. The problem is that this deep mathematical theory has not entered any of the standard mathematical physics books (Reed-Simon,…) but it is explained in the cited articles and their references. A nice little introductory mathematical article can be found on Steve Summers homepage. Please do not expect that such a mathematical subtle and conceptually demanding theory allows an instant packaging in an weblog. The only thing I can do here is to say that it exists and already in its present incomplete form it gave profound physical results.

75. **Kea**
May 26, 2006

Would that be the same Steve Summers who cites Baez, Doplicher, Roberts and Redei?

76. **Bert Schroer**
May 26, 2006

Yes it is the same. Among the recent online collection of articles you find a 10 page article with the title: Tomita-Takesaki modular theory
It is probably also available on the math-ph server. Most of the very recent physical results are however not contained. But your question of how and what kind of geometry emerges from the algebraic positioning of operator algebras is briefly explained and you will be referred to more detailed literature.

77. **Kea**
May 26, 2006

A link to the Summers paper

78. **Kea**
May 26, 2006

I'm looking through it, at theorem 5.2 for instance, where he says the modular unitaries generate a representation of the group of isometries of (2D) Minkowski space. I'm a little confused. For some reason I thought the *geometry* of these wedges and things should be written down in the language of sheaf cohomology.

79. **Chris W.**
May 26, 2006

Eugene,

When the atomic constitution of matter was first postulated was it *known* to have observable counterparts in nature?

The point of view you express is close to that attributed to Ernst Mach. Mach never accepted atomism, and had little use for relativity.

Einstein, despite being influenced by Mach, ultimately concluded that it is the theory we are attempting to test that says what might be observed. It provides a framework for motivating the operations that give rise to observations, as well as specifying the expected features of those observations.

I would rather get the clues about physics from … well, physics. More precisely, from experiment.

Physics has never been simply about getting clues from experiment. It has always had a metaphysical component—again, something that Einstein understood well. However, insofar as it is about experiment it is about understanding stable, reproducible observations. Indeed, in a deep sense it is about the possibility of stability itself. I believe this leads inescapably to a profound reflexivity in the foundations of physics, because if one employs invariant laws to account for stability—or reproducible patterns—one is eventually led to inquire into the stability and success of the laws themselves. I would define physics as this seeking after stability, both stimulated and constrained by controlled observation. This effort has been enormously successful. The question is, why should nature—or existence—accommodate such success in seeking after stability and principles of invariance?

The currently popular incarnation of the anthropic principle gives a trivial and ultimately sterile answer to this question: The Multiverse is so vast and diverse that it accommodates anything—environments where stability can be found, and not incidentally, where life and science can exist, and a vastly greater array of environments where such is not the case. The fact that we exist in an environment where stability can be discovered is from this viewpoint a simple matter of selection; if we didn't, we wouldn't be discussing the topic or have a civilization based on the possibility.

In contrast to such sterility, can the reflexivity explained above point the way to an genuine advance in theoretical physics? That is, can physics advance by reflecting on itself, bearing in mind that what calls for explanation is in part the prior success of physics? (Remember that this kind of consideration is what motivates correspondence principles.)

80. **Kea**
May 27, 2006

Bert

Why did you choose the term *monade*?

81. **Bert Schroer**
May 27, 2006

Kea,
this time the answer to your question is well suited for a weblog.
One reason for substituting the full mathematical name: hyperfinite type III_1 Murray-von Neumann factor algebra by something shorter (in a paper where it occurs many times) is that it is very unwieldy. Writing simply HTIII_1FA is ugly. In looking for a word which reveals something about its conceptual aspects the word "monade"is very appropriate for two reasons. A single such mathematical object is unique i.e. up to isomorphism there exists only one, i.e. it is analogous to a point in geometry. The situation changes radically when you place several copies of this unique object (as operator algebras) into a common Hilbert space; with a carefully chosen positioning dictated by "algebraic naturalness" based on modular operator theory (modular inclusions, modular intersections), you create the rich world of QFT in Minkowski spacetime (including spacetime and inner symmetries) where all the differences between QFT models have their origin only in the huge cardinality of possibilities of modular positionings (to generate chiral theories you only need 2 copies, for 4-dim. QFT the minimal number is 6). This is precisely the quantum physics realization of Leibnize's monade theory of how reality originates from the relative positions of "monades", in fact it is a perfect match (but you had to go up to local quantum physics to find it).
The mathematical physicist who discovered some mathematical properties of this positioning (Wiesbrock, you can find some of his papers he wrote in the 90s in math-ph) could not continue his career; he had the bad luck of living at the wrong time (a time when academic priority was given to string theorists). Most mathemaical Field medalists (exeption Vaughn Jones, Alain Connes) do not know this mathematics; it comes to a large part from a string-free zone in particle physics.
When Haag was travelling through Princeton and met Witten, this issue came up in a conversation; but it seems that Witten apparently dismissed it, probably because he and Atiyah were convinced at that time that real particle physics had to come through the massaging of that (nonexisting) functional integral.
Recently I succeed to obtain 3 quite interesting (published) result using modular methods
1) A very clear understanding of the recipes underlying the construction of factorizing models (including the spacetime interpretation of Zamolodchikov-Faddeev algebras).
2) Together with Mund and Yngvason we finally understood the best localization aspects of the infinite spin Wigner representations (besides the massive and the finite helicity massless representations the third big family of potentially physical representations) and the string-localization of potentials associated with massless finite helicity fields and their mild short distance properties (which makes them ideal candidates in the search for renormalizable islands in the infinite parametric space of the renormalization group)
3) A deep quantum understanding (a modular version of holography) of the infinite dimensional mysterious Bondi-Metzner-Sachs group and of the the close connection of the Volume law of heat-bath created entropy with the area law caused by vacuum-fluctuation at the boundaries of causal localization (as already mentioned in http://www.lqp.uni-goettingen.de/papers/06/04/).

There will be a forthcoming paper by Gandalf Lechner from Goettingen (his thesis) where he succeeded to proof the mathematical existence of factorizing models (the solution of the old nontriviality problem in a still limited context). These models, unlike those superrenormalizable in the book of Glimm-Jaffe, are for the first time only strictly renormalizable.
Together with Jens Mund and Jakob Yngvason I am presently working on a renormalized perturbation theory based on those string-localized field (absolutly nothing to do with string theory). A little note on the string-like potential describing the metric tensor will appear in a little separate communication by Mund within the next two weeks. We are under the impression that within this extended perturbative framework gravity will be renormalizable (finite-parametric) or to phrase it more carefully: the massless higher helicity string-localized potentials permit renormalizable interactions.

82. **Bert Schroer**
May 27, 2006

I just realized that I forgot to mention a result obtained in the algebraic modular setting which will probably interest many participants in this weblog beyond Kea
The idea of modular Euclideanization (as opposed to Osterwalder-Schrader Euclideanization of real time QFT) leads to a structural proof of a temperture-duality of thermal correlations (hep-th/0603118 and literature quoted therein) for chiral theories of which the Verlinde duality arises from the zero-point correlation function (the

partition function or character of loop-groups). As its higher dimensional analog the Nelson-Symanzik duality it is a manifestation of the structural richness of the implementation of the causality principle of local quantum physics. In fact it is much richer than the N-S duality, because it generalizes the Victor Kac observation:
representation theory of loop groups –> identities for modular forms
to:
causality principle for chiral theories –> identities for modular forms,
with other words the totally autonomous modular concept arising from the T-T modular theory of operator algebras contains the concept of modularity of so-called modular forms (all those funny Ramanujan-kind of identities have a common structural root: the causal locality principle of QFT!). And mathematical physicists have a fare share of this modular theory because important concepts as the KMS property (which Connes used for classifying the type III factors which brought him the Field medal) are due to physicists. Although physicists work in a more special context, it would not have been propostorous to call it the T-T-H-H-W (adding Haag, Hugenholz and Winnink) to the list of protagonists.
Kea if you are really interested and you would live in Brazil, I would recomment to attend a Sattelite meeting down in Floripa at the end of July:
http://www.mtm.ufsc.br/~exel/oa/
In normal times such insights would be of interest to more mathematicians, but remember that this is coming from a string theory-free zone.

83. **Joan**
May 27, 2006

For those who are having difficulty getting research jobs,
have you considered lecturer positions? These are usually
not too difficult to find, and while the pay is not great,
you typically have summers free for research, and usually
less administrative responsibilities than you would have
in a tenure-track position.

84. **JC**
May 27, 2006

I've noticed lecturer positions seem to be a mixed bag for the most part.

At some places, they seem to be term contracts which have to be renewed every term or year. In some cases I'm aware of, some lecturer contracts were not renewed because of things like departmental politics. Popular "excuses" for not renewing were silly things like too many poor student rewiews and/or complaints, frequently used by the department to get rid of folks they don't like.

In some departments which had an emphasis on research, the lecturers are treated as 2nd class citizens or "bottom feeders". At a community college, the problem seems to be things like students not wanting to be there and goofing around too much. (The sad part about some community colleges, is that a large number of lecturers there were actually folks who did not get tenure at a research university. Some of these folks seemed quite miserable for the most part).

Though on the other side of the coin, most lecturer positions had very little to no bureaucratic and/or management duties outside of teaching. For many of my previous colleagues which have tenure at a university which emphasized research, their number one complaint was all the bureaucratic duties they had to deal with. In that sense, it wasn't much different than working in a Dilbert style corporation.

85. **urs**
May 27, 2006

Bert Schroer, Kea,

lest the impression arises that Tomita-Takesai theory, von Neumann type III factors and the like play no role in string theory, let me point out the work by Stolz and Teichner, who are in the process (for quite a while now) of giving a geometric interpretation of elliptic cohomology

(http://golem.ph.utexas.edu/string/archives/000737.html)

in terms of CFT with target some string bundle

(http://golem.ph.utexas.edu/string/archives/000799.html).

See for instance page 53 of this review

(http://golem.ph.utexas.edu/string/archives/000712.html)

to see all the machinery – that Bert Schroer mentioned – in action.

86. **Bert Schroer**
May 27, 2006

Urs,
that is not what I meant. The von Neumann algebras e.g. in the last part of the first article are just an epiteton ornans (ornamental addition copied from Wassermann's article) to the main text.
The setting of the authors is that of Greame Segal's axiomatics of (topological) Euclidean field theory, which has only a metaphoric relation to a similar real time axiomatics a la Brunetti-Fredenhagen. What I really meant is an Euclidianization of the T-T modular theory in the Nelson-Symanzik duality tradition which in the chiral setting is a modular analog of the Osterwalder-Schrader Euclideanization (i.e. something which comes from our past in autonomous particle physics i.e. which was there before the Atiyah-Witten era). With other words something which is in the tradition of modular theory a la T-T-H-H-W (a terminology which I explained before) enriched with the notions of modular inclusions and modular intersections (which are essential for my use) which were discovered in the 90s by Wiesbrock before he had to leave academia. One glance at that work on modular holography (mentioned before) would show you that we are talking about completely different conceptual setting which just happen to share a few common references.
Please don't fall prey to the trick in string theory to claim huge areas and semantically incorporate it (a phenomenon which was discussed before with Peter). Since you are a member of a mathematics department you have all the right to be interested in elliptic objects, but I think as a particle physicist I have the right to be proud of our rich and almost forgotten traditions and to use them in a new context (and fight any insinuations that this has grown on the soil of string theory)

87. **Bert Schroer**
May 27, 2006

Dear Peter,
just to get away from that long discussion with various participants on this webblog for a change, I would like to make some personal comments.

Since I was a newcomer to your weblog at the time of its third birthday, let me congratulate you for your fulltime work of managing this weblog so successfully. I hope the physics Columbia university physics department realizes what a useful role this string theory critical weblog plays in very precarious times for particle physics (I think even string theorist can see this point).
As you certainly have realized, we have quite different opinions on what could help particle physics in this situation. But there is no hegemonial claim in either of our viewpoints.
When I listened to these three University of Princeton tapes of David Gross I was shocked by his salesmenship of semantically manufactured facts (described before in a contribution where I quoted from original papers of string theory). We always think of George Bush, when we talk about manufactured facts. Well, it happens in our midst and the intellectual creme of Princeton University is applauding. I also was appalled by what he said about LQG although (contrary to you) I have a somewhat critical position with respect to LQG (and I think some of the questions and comments referring to Lee Smolin are justified; also I do not want to see particle physics and in particular gravity end in an Armegeddon between LQG and string theory). Since I could not believe that a place where I spend some time in the 60s had now fallen so low, I searched for other videotaped talks. I listen to the first talk in 3-talk series by Mark Juergensmeyer with the title: “God and War: The Odd Appeal of War” (also a professor from Santa Barbara) and my view about Princeton was instanteneously corrected; the problem I had is really constricted to particle physics and may also occurs at other ivy league places which have a tradition for such talks.
We probably also agree that it is somewhat disappointing that such a integer and generally critical mind with a Nobel stature as Frank Wilszek does not take a more pointed position with respect to these strange sociological hegemonic manifestations. But then Gross was his thesis adviser, and in addition not everybody has that critical independent mind as Pauli (he was not always right, but he always fought for the coherence of physics and not in order to sell his own ideas).
I have a bottle of red wine on my table (this time from Mendoca, Argentinia) and I am in this moment drinking to your successfull continuation of this weblog. May people recognize what a hell of work you have to do in order to permit yourself some satisfaction.

88. **urs**
May 27, 2006

The impression that algebraic quantum field theory is closer to phenomenological physics than to pure math, at least in comparison to other parts of “formal high energy physics” is something that I have not obtained.

Maybe its just me being ignorant (which I am, in a huge number of respects). But the worthwhile applications of AQFT that I have seen have precisely the status of physics-inspired math which Bert Schroer is so critical of.

The understanding of representation theory of loop groups and related CFT technology using AQFT techniques would be one example. Some math that people like Michael Mueger are doing

(as in his second talk here http://golem.ph.utexas.edu/string/archives/000711.html)

would be another.

I am also not sure why Minkowski-spacetime field theory is so much more “physical” or “real”. Sure, in some sense. But CFT applied to statistical mechanics is a Euclidean field theory. Any concept of field theory we have should be able to deal with Euclidean and Lorentzian backgrouds.

But (correct me if I am wrong) Minkowski spacetime and lightcones are build into the very axioms of AQFT. That looks like too strong an axiom to me.

89. **B**
May 27, 2006

MoveOnOrStayBehind Says:
B:

“Or, to put it differently, someone should think about what we ought to do with all the String Theorists. ….”

A nuclear physicist by education who now does “phenomenological quantum gravity” teaches string physicists what to do after their project has failed? By all means, they can’t wait to hear!

It’s not up to me to judge which research fields are promising. I just say, that it’s definitely necessary to objectively evaluate the situation. There are people who have sufficient knowledge and overview to give advise. Listen to them. I wouldn’t even say string theory has ‘failed’. But its over-rated.

I – in person – am certainly not the one to teach string theorists something besides phenomenological quantum gravity – if they are interested, I would love to do so! I can’t avoid noticing that String Theorists speak ‘stringy’ and someone has to make the translation. The quest for the Theory of Everything has quite some similarities to building the [Tower of Babel](#).

This is getting more absurd every time I read around here.

Dear MoveOnOrStayBehing: You got to move on – or stay behind…

Best, B.

90. **Bert Schroer**
May 27, 2006

Urs,
Loop groups are of course a valuable illustration, I mentioned the that the relation of chiral field theories with modular forms was first discovered in the context of loop groups (Victor Kac…).
The point here is that you do not want to invent a special drawer in which you keep loop group or general chiral field theory separated from the rest of QFT. The only interest in chiral quantum field theory is as a theoretical laboratory to learn something about the nonperturbative subtleties for higher dimensional QFT and for this reason you have to use those structures which are in common to QFT in all spacetime dimension (and that is modular localization). All special structures for families of chiral theories like loop groups (sorry, for me they always remained current algebras) have been investigated or can be investigated by mathematicians, hence why should I loose time in getting into competitions with them? What I can contribute is the update or adaptation of some rich ideas from the past (and largely forgotten, just because you were so busy learning loop-groups and elliptic objects that you had no time to learn anything about the important conceptual past cross roads in particle physics, this is not a personal criticism). The Euclideanization of the modular group which underlies the (field-coordinatization independent) concept of causality and localization in the context of the chiral setting (without specializing to loop groups or other rational families) and which is in complete analogy to Nelson-Symanzik and Osterwalder-Schrader is something to the heart of a particle physicist like me.
I am not negating that for special families you can use other special methods coming from mathematics e.g. the vertex operator formalism of Graeme Segal, which has recently been used by Hu in order to derive the Verlinde formula for a special family (which is characterized in terms of vertex concepts which I am not familiar with (i.e. I do not know their precise relation to Wightman fields, to clarify this would be the obligation of the vertex algebra people because Wightman fields are much older).
I think that particle physics has a strong historical component which has been damaged because in the late 70s people (with the increasing arrogance of people entering

math. phys. they never cared to look back whether some of these ideas already existed so that the already existing terminology could be taken over and enriched by new insights instead of cutting off the link to the past by a new terminology).
It is just as with human history, if you forget and surpress it, it will create confusions and conceptual regress.
Urs, your attempt to separate me from my colleagues of AQFT will not succeed, the past is too strong and even my present influence on what is going on in that community is not negligable.

91. **Kea**
May 27, 2006

I have a bottle of red wine on my table (this time from Mendoca, Argentinia)

Oh! I was in Mendoza a few years ago. Lovely place. I would love to come to Brazil, but alas my poverty forbids it. Thank you, Bert, for taking the time to tell us a little about AQFT. Personally, I am 100% on your side, as regards the relevance to particle physics in comparison with Strings. I had a look at your beautiful paper on wedge localisation last night. Actually, Jones said a few words about his ideas on physics at a conference I was at recently, but unfortunately, as an organiser, he was not given the time to speak about any details.

My point is that certain instances of the category theoretic *monad* share many of the deep properties that you attribute to your monade. I do not think that this is a coincidence. Not all people looking at higher category theory are in the String camp.

92. **Kea**
May 27, 2006

Here is a paper by Halvorsen on AQFT, with some category theoretic input.

93. **Kea**
May 27, 2006

Try again: paper

94. **Bert Schroer**
May 27, 2006

Kea,
of course higher category theory is not the domain of string theory. The first who introduced nonabelian cohomology and higher categories into QFT was John Roberts (in the middle of the 70s). He had a strong physical motivation coming from gauge theories. Things did not work out in the way he expected. But fortunately this was at a time when negative results on deep problems investigated with the best available tools were equally important and interesting äs positive results (somehow related to Pauli's "not even wrong") and so the work was published. I have the impression a negative result on string theory will not be tolerated by the community in fear of endangering the veracity of the whole construct. I am surprized that his work was not mentioned in recent string theory blogs. Street, who is the father figure in Australian n-category research always referred to it.
Returning to Urs:
where did I claim that AQFT is anywhere close to phenomenology? I said that it is directly related to the (utterly successful) principles underlying QFT (which are the condensed form of past experiments), more explicitly the principles together with some mathematical concepts to implement them. In a situation where you are stuck (hopefully only temporarily) on the experimental side, you are not condemned to do speculative blue yonder physics, rather there is the third way: press the principles and the concepts real hard and see what you are led to. But this is very difficult and time consuming and I cannot advise a young man under actual social physics department conditions to go into this since it is very risky for making a living (but there were some people who were willing to take the risk and a very few succeeded).
Urs, you mentioned Michael Mueger. Despite his beautiful conceptual work on order/disorder variables (and the fact that he is very talented) he could not get a position in physics. For quite some time he worked under Turaev in Strassbourg and it is natural that he looked at the more categorical aspects as they were investigated at the mathematics department of that university. He finally got a permanent position in Holland (probably in math.). Please don't msiunderstand me, I am in no way against categorical settings and topological field theory. I only think that if this is already being done at mathematics departments it should not also be done in physics departments. Or to put it into a milder formulation, if it is done at a physics department the individual using it should at least be aware that the first topological field theory was that extraction of a tracial (combinatorial) algebra which carried the representation of the statistics operators in the DHR work on superselection sectors (see Haag's book) and which was later discovered in a much more general setting by Vaughn Jones and called by him extremely appropriatly the "Markov trace" (where Markov refers both to the 19 century Russian probabilist and his son who stands for the braid group aspect). Whereas the topological bones can be perfactly placed into such tracial hyperfinite type II algebras as Vaughn uses them, this is not possible for the localizable and transportable meat.
Kea you said that you saw Vaughn Jones recently, is he well? We are all looking forward to see him in Floripa.

95. **Aaron Bergman**
May 27, 2006

You know, if we're judging string theory wanting for having produced little connection with the real world in the last twenty years, how should we judge AQFT having produced not a single example of a realistic QFT since at least the seventies.

96. **Bert Schroer**
May 27, 2006

Judge it by the results I mentioned this morning (at least something, it is not a theory of everything). In addition AQFT shares all the previous successes of QFT (including all the cross sections and vacuum polarization effects) because it is nothing but an vast conceptual extension (often with a more profound interpretation).
I would repent most of the things I said if string theory would only lead to a fraction of those results.
String theory is not an extension of QFT because the prerequisite would be a structural compatibility without which the standard scale-sliding argument is not worth anything. I have explained this in detail before in some older contribution and I am not going to repeat this here again.
You may not agree with me, but you certainly have to admit that at least it does not wipe out knowledge (which string theory has done and is still doing).

97. **Aaron Bergman**
May 27, 2006

AQFT shares all the successed of QFT? What nontrivial theory in four dimensions has been constructed via AQFT?

98. **Kea**
May 27, 2006

Please don't msiunderstand me, I am in no way against categorical settings and topological field theory. I only think that if this is already being done at mathematics departments it should not also be done in physics departments.

Bert, the people working on fractional QHE, for instance, have hardly been doing nothing for the last 20 years.

99. **Kea**
May 27, 2006

There are *engineers* and computer scientists doing TFT mathematics. The question you should be asking is: how can I link this beautiful edifice of AQFT to what they are all doing?

100. **Bert Schroer**
May 27, 2006

I was under the impression that solid state physicists cherish localized states and material properties and they need the localized carriers of those topological quantum numbers and not just their bones to make trustworthy calculations. Are you sure that you are talking about professional condensed matter physicists? Is among the physical results anything which would impress e.g. Phil Anderson?
To Aaron: if standard QFT would be able to construct a 4-dim. QFT it would be immediatly inherited by AQFT according to the logic I explained. I think you mean by "construct" something entirely different. I do not mean being able to write a functional integral, compute some renormalized Feynman diagrams and put some instantons on top, I meant mathematically controllable model constructions as they have been done in any other area of theoretical physics.

101. **Kea**
May 27, 2006

Are you sure that you are talking about professional condensed matter physicists?

Yes, I am quite sure. Of course, no one is claiming to have worked out completely how to compute things properly. **That is the point.**

102. **Aaron Bergman**
May 27, 2006

The point is that AQFT is one attempt to codify the mathematical structures of QFT. There are others (I can think of at least three mathematicians who have set out to define QFT, two of whom have Fields medals). To date, I think it's safe to say that none of them completely capture what we know ought to be true about QFT. So, maybe you've chosen the right way or maybe not. Beats me. But I hardly think it's fair at this point to say that the AQFT formalism *must be* the right answer when there aren't any realistic examples and, furthermore, it seems to ignore (at least judging by your previous conversations with Mentos) much of the modern Wilsonian understanding of the subject.

How about something easier? Does AQFT have anything interesting to say about TQFTs? In two dimensions, at least, the axiomatization in terms of categories has shown a lot of success. In 4D, the twisted N=2 TQFT that gives Donaldson invariants has proven to be extraordinarily fruitful. Can you see the AQFT structures in this context, and do they say anything new or useful about Donaldson and Seiberg-Witten invariants? Recently, a topological twisting of N=4 SYM has proven to describe much of the mathematics in the geometric Langlands programme. Can AQFT say something there? The categorical structure a la Atiyah, Segal *et al* definitely is apparent.

103. **Bert Schroer**
May 27, 2006

The first step would be to have convincing experimental agreement with those quantum numbers. If those are related to e.g. braid group statistics this would be interesting, since statistics is an almost kinematical property (and the only known property by which low dimensional are structurally different from higher dimensional ones!). I find it perfectly conceivable that e.g. a large statistical (or quantum-) dimension could account for a high temperature in high T_c, but I am not an expert.
You did not answer my question about Vaughn Jones.

104. **Kea**
May 27, 2006

You did not answer my question about Vaughn Jones.

Sigh. All right, then. He seemed well, and did a lot of windsurfing, as usual. But I can't say I really know him personally.

105. **Kea**
May 27, 2006

Can you see the AQFT structures in this context, and do they say anything new or useful about Donaldson and Seiberg-Witten invariants?

This is a very good question. AQFT *should* be able to shed light on these invariants, if it has any computational power.

106. **Bert Schroer**
May 27, 2006

"But I hardly think it's fair at this point to say that the AQFT formalism must be the right answer when there aren't any realistic examples and, furthermore, it seems to ignore (at least judging by your previous conversations with Mentos) much of the modern Wilsonian understanding of the subject."
Sorry Aaron, this apodictic manner is not my style of dialog and if I really said that, it was a lapsus linguae. In fact the situation is quite the opposite: exactly because we know that we are on such slippery ground we are trying so hard to control the existence of 4-dimensional QFT (those successes in d=1+1 are only successes in the sence of a theoretical laboratory, but we have already learned some messages and at least there is a new strategy). Our aim is to make QFT like any other area of theoretical physics.
Concerning the Wlson Renormalization group I once asked that question to Fredenhagen after he finished his work with Duetsch on the AQFT-inspired version of the Petermann-Stueckelberg renormalization group. He told me that this can be done and the result will be written up. Fredenhagen has never claimed anything which he was not able to deliver and I attribute the fact that nothing has appeared yet as due to his very demanding job of leading such a big institute in the midst of a German buerocracy and politicial leadership which come with new directives almost every week.
Concerning those impressive results about Donaldson invariants, Seiberg-Witten, the Langlands program etc. I think that the relation to particle physics is of a more metaphorical nature i.e. they use a language which is taken from particle physics but it is not really particle physics. The setting tends to be Euclidean instead of real time (I am not so sure about Langlands) and certainly I do not know how to get a real time into those mathematical structures. I also confess that I have never seen any mathematically controllable real time operator theory with a electric-magnetic duality although I know order-disorder variables in real time QFTs in d=1+1.

107. **Aaron Bergman**
May 27, 2006

The relation of TQFTs to reality is irrelevant here. They certainly appear to be examples of quantum field theories. For example, QFT arguments were used to invent Seiberg-Witten invariants which were previously unknown to mathematicians. It's hard to believe that these things simply are not QFTs given their success. If your formalism has no room for them, then I don't see why I should take is seriously as an axiomatization of QFT especially given that TQFTs are *easier* than the real thing.

Let me ask about anomalies. There are very beautiful ways of understanding gauge anomalies from the path integral point of view. Does AQFT in any way incorporate this geometric understanding of anomalies?

108. **Kea**
May 27, 2006

The setting tends to be Euclidean instead of real time (I am not so sure about Langlands) and certainly I do not know how to get a real time into those mathematical structures.

The question was really: how would AQFT tackle the problem of smooth 4D invariants? In other words, what is a combinatorial description of them? As for 'real time', many elements of SW theory have twistor theoretic descriptions, and these could possibly be of AQFT type. If so, then these invariants are really, really interesting physics – not just mathematics.

109. **Kea**
May 27, 2006

AQFT, Donaldson-SW
Temperature duality, Coupling duality
Holography, Cobordism
Monade, Monad
Braids, Dehn surgery

110. **Kea**
May 27, 2006

…and you might even convert a few thousand Landscapologists to the true path!

111. **Bert Schroer**
May 27, 2006

Anomalies have been treated and identified in the Duetsch-Fredenhagen AQFT setting of perturbation (naturally it is in real time). Euclidean field manifolds and functional integrals are metaphoric instruments, they are very suggestive and after a lot of massaging leads to correct results but they do not exist mathematically apart from superrenormalizable (and not very interesting) models of the Glimm-Jaffe type. In a previous blog I described how I went through all this in the middle of the 70s looking at the Euclidean integration aspects of the Schwinger model, finding by generic functional integration (the Schwinger model is the only case where this can be done generically i.e. without specializing to instanton configurations) learning everything about the Atiyah-Singer index theory in order to generalize that wonderful Schwinger model relation between zero spinor modes and winding number of the gauge field (partially in collaboration with N. Nielsen and also with Swieca) some years before Alvarez-Gaume, Witten etc. I think I have some credentials in that kind of functional geometry. But I left after a couple of years because it was far away from my conception of real time quantum physics and because I think that artistic manipulations which have no mathematical existence even if they are highly successful are not my favorite passtime. Just imagine what would have happened to quantum physics if people would have been complacent about the quite successful Bohr-Sommerfeld old QT. If you like metaphorical arguments which lead to consistent results that is fine for me, but I find it absurd to be asked (30 years after I left this metaphorical use of QFT precisely for the indicated reason) to produce results which I do not consider to be part of particle physics. Neither QFT as an instrument of particle physics nor its AQFT extension can achieve what you are asking for; however some gifted particle physicists can be inspired by its setting and derive wonderful mathematical results. Not even Graeme Segal, who was inspired by QFT to abstract his nice euclidean setting of gluing via cobordism claims that he is doing what particle physicists call QFT. There is a difference between particle physics and "that what physicists are doing" (quotation from Hrzebruch after giving an account of the Atiyah-Witten work).

112. **Kea**
May 27, 2006

All right, then. How would you calculate the Standard Model parameters? Is that physics?

113. **Aaron Bergman**
May 27, 2006

I just want to make sure I understand you. Is it your belief that TQFTs are not examples of QFTs?

As for the rest, quantum mechanics explains and encompasses the old Bohr-Sommerfeld rule (slightly modified). As best I can tell from you, AQFT neither explains nor encompasses the geometric understanding of anomalies.

I'm mostly going through this, BTW, to show to the various readers here that the reason the vast majority of the field does not pay attention to AQFT is not just prejudice and groupthink or whatever. It is because it does not seem to capture any of the things that are generally understood to be fundamental and useful features of QFT.

114. **Kea**
May 27, 2006

And I have a question: why should a physicist accept Connes' vision of what is really **geometry**? He's just a mathematician.

115. **Bert Schroer**
May 27, 2006

Kea,
I have no aswer and I do not think that anybody else has. Sometimes I think that we had to pay a very high prize for that relatively easy group theoretical entry. We are now in a labyrinth, the best (in the sense of phenomenological success) quasiclassical straight jacket imaginable (remember that the gauge principle is a classical selection principle for L-invariant interactions) i. e. I do not think we will get out of the present mess without a very significant additional conceptual investment. One should be modest and admit that not every problem can be solved at any time. Solving such problems in a diretissima is mostly not possible. We should patiently go ahead and pressing more the inner logic of QFT; in no way did we reach yet the inner core of QFT. But I expect that as in previous cases (except string theoru) the revolutionary aspect may be less in a change of principles but more on the conceptual side of their implementation.
A small piece of cherry on the cake which AQFT-inspired arguments can deliver is that if one starts with massive vektormesons in zero order than perturnative consistency requires the introduction of an additional physical degree of freedom whose simplest realization is a scalar field (Higgs but without that vacuum condensate). So something like the Higgs seems to be a perturbative necessity. But this is probably something expected in any case this has no bearing on your question.
I may have created the wrong impression that AQFT is a community thing like string theory or LQG. It is not, rather it is a shared belief of some pragmatic-minded individuals whose only distinction from others is that they insist that the requirements on the searched for theory should be clearly formulable without invoking "classical

crutches” i.e. without quantization and if possible without highlighting particular field coordinatizations (apart from conserved symmetry currents).

116. **Bert Schroer**
May 27, 2006

Yes Aaron, TQFT is associated to quantum field theory but it is not itself QFT. You can extract from localizable QFT an algebra of the topological bones, but TQFT has no autonomous interpretation because all the interpretation of QFT, I repeat all the physical interpretation, goes through causal localization. Even particle momenta are in first place not Fourier transforms but rather encode geometric relations between events (clicks in counters). One forgets these aspects if one computes Feynman integrals in momentum space but in scattering theory and much more so in curved spacetime QFT one is painfully reminded of this elementary facts of life. Physicists manage to invent a Lagrangian description (e.g. Chern Simons) but this does not make them QFT, they remain at best TQFT. You may not see this directly in the action (unless you really make the Osterwalder Schrader test) but the algebraic content of such a theory is radically different from that of a localizable QFT (they are similar to those algebras Vaughn Jones uses in his subfactor theory). A derivation of e.g. the TQFT containing the mapping class group from a full chiral QFT on a circle was given e.g. in the appendix of the pre-electronic paper I wrote with Fredenhagen and Rehren. Since the original chiral theory was localizable, you know the concrete spacetime interpretation of the knots and mapping class group operators only by pointing to those operators in the QFT algebra from where they originated. The only memory about localization they carry is a distinction between right and left (a property of the braid group). I do not know any construction which allows to reconstruct a QFT from a TQFT. The auxiliary Riemann surface pictures one makes are strictly metaphorical, they have absolutely noyjing to do with the physical localization.

117. **Thomas Larsson**
May 28, 2006

I happen to think that the present formalism of gauge theory does not meet this test (see my remarks I made earlier, giving support to Thomas Larson in Fantastic Realities, probably more radical than what he had in mind).

Bert, my key observation about gauge symmetries is quite radical: not all gauge symmetries need to remain gauge after quantization, due to gauge anomalies. There is a strong, ideological resistance against this (“a gauge symmetry is a redundancy of the description”), despite the fact that several well-known examples of consistent anomalous gauge theories exist; the free, subcritical string (D &lt 26) is the canonical example (no-ghost theorem).

In fact, it is impossible to combine a trivial action of local gauge transformations with nonzero charge, provided that you embed the algebra of gauge transformations into its natural completion containing also divergent, superselection-changing transformations. I made this observation, which is both obvious and trivial, in math-ph/0603024.

From this perspective, the key difference between string and YM theory is whether you allow divergent gauge transformations. In string theory, you allow generators which diverge when z -> infinity (L_m with m > 1), but in YM theory you forbid generators which diverge when r -> infinity.

118. **Bert Schroer**
May 28, 2006

Aaron,
you were a great sparring partner yesterday night. From FAQ’s to the standard prejudices you hit the whole scale of arguments and sometimes I had the impression that you played the devil’s advocate (this is what I sometimes do in order to enhance the informative flux). Since I have reservations about proselyting, this is a good way to conduct a dialog and I tell you that I stayed on the PC up to midnight in order not to loose this ideal opportunity.
I hope that I convinced you that the extension of standard QFT (Lagrangian quantization) called “algebraic” is basically the execution of a step towards intrinsicness which in geometry you already excepted a long time ago, namely independence of coordinates (in the case at hand independence of field coordinatization which requires to go beyond Lagrangian quantization) although, as in geometry, it is not forbidden to use coordinates. Due to the inexorably singular nature of sharply localized quantum fields this step is naturally more sophisticated than its differential geometric analog.
I also hope to have convinced you that TQFT, despite this name, is not QFT, because the most important ingredient since the time of Faraday and Maxwell namely localization (the meaning of fields) is missing. The impreciseness of nomenclature, hugely enhanced by string theory, would have tragic confusing consequences if we fall prey to our own bad semantic creations.
I tried to explain that AQFT achieves the separation of topological bones from the localizing flesh by extracting “kinematical” subalgebras with tracial states (they are never localizable or transportable). This is an extremely important technical step in the DHR theory of superselection sectors. The latter is a phantastic achievement: the reconstruction of the full QFT (including statistics, inner symmetries…) only from its observable shadow (another reductionist construction of AQFT). This is one of these inverse constructions of the kind which Marc Kac characterized in a more acoustic context as “how to hear the shape of a drum”. The idea is that we have very good intuitive insight into local observables, but we enter a conceptual high risk zone if we arrogantly claim that we can pull the structure of the full QFT with all its “unobservable” (i.e. its not directly accessible charge- and halfinteger spin- carrying fields) out of our hat or head.
This extraction of topological (“kinematical” in a certain extended sense of the word) bones in the old DHR work (see Haag’s book) has been extended to the low-dimensional realm of braid group statistics in the appendix of the mentioned FRS work, and I already mentioned that in this richer braid-knot-mapping class group context the tracial states coincide with the famous Markov traces of Vaughn Jones subfactor theory. The latter is a bone theory par excellence, although the bones which in the Chern-Simons-like
“backreading” into a Witten kind of Lagrangian jacket (but outside the Osterwalder-Schrader reflection positivity which is related to localization) take on the appearance of topological bones, are more combinatorial bones in Jones’s subfactor setting.
Another bone framework outside of localization is the LQG. I know too little about its algebraic nature to sav what kind of algebras it leads to but there are definitly no monades (see earlier blogs) around.
I saw many instances of extraction QFT —> TQFT but I am not aware of a single case were localizable meat was put onto topological bones. This is why I am a bit sceptical about the relation of LQG to localizable ordinary quantum matter; but on the other hand I have no argument why such a thing cannot work. Maybe Lee Smolin or somebody else from LQG has an idee how to achieve that.
I will vanish from the radar screen for at least one day and I of course hope that my contributions to this weblog do not only create animosities but are also a little bit helpful in a positive sense.

119. **urs**
May 28, 2006

Is it plausible that the right concept of real-time Minkowski-background 4D field theory is disconnected from that of Euclidean and/or topological field theory?

Taking the risk of sounding like a broken record (at least I won’t be the only one), let me say this:

The FRS result on RCFT (->) shows precisely the opposite. The full understanding of rational conformal field theory has only been obtained after realizing how it splits into a topological part and a part knowing about the conformal background structure.

And I think we all agree that RCFTs are of concrete physical relevance. They describe stuff people measure in laboratories.

Furthermore, the FRS theorem solves a problem (namely that of understanding what full 2D RCFT *is*) which cannot even be formulated with present AQFT technology.

That’s only in part due to the fact that AQFT chooses (by way of axioms) to restrict attention to Minkowski background, while we need Euclidean backgrounds here.

But there are more reasons. It is not known to date how to describe field theories with general boundary conditions using AQFT, let alone field theories with defect lines.

(Defect lines are for instance generated by the ‘t Hooft operators that implement the Hecke transformations in twisted SYM, the way Kapustin and Witten explain (->)).

Klaus Fredenhagen has some first ideas on how boundary conditions might be modeled in the AQFT context. There was supposed to be a project concerned with investigating this question in the new String Theory Research Center in Hamburg (->) – but the referees canceled this particular project.

Anyway, my impression is pretty much the same that Aaron expressed:

AQFT is one attempt out of several for extracting (guessing, really) the right mathemtical structures (the right axioms) from the semi-heuristic physical understanding of field theory. Given the results obtained from these axioms, it does not really look like the AQFT axioms make closer contact with observable physics than, in particular, Atiyah-Segal formulations do.

Moreover, as the example of that Stolz/Teichner paper was supposed to illustrate, where necessary the Segal formulation (which asserts that QFT = representation of cobordims categories in Vect) incorporates useful insights obtained from AQFT.

So, for me, the conclusion is this:

Clearly, AQFT has axioms which are a plausible first guess for the axioms that a real QFT should satisfy. But just as clearly, something about these axioms is not yet flexible enough. Something is still missing. Something of course is clearly right about them.

Surely, as long as the AQFT axioms apply not to a single physically non-trivial theory, they are in need of modification.

What is good about axioms is that they allow people to unambiguously work out their consequences. This is happening using AQFT axioms, and a couple of mathematically interesting insights have been obtained.

However, I see no evidence that Segal-like formulations of QFT, with their motivation in topological field theories, are farther removed from the QFT existing in nature than the AQFT formulation. On the contrary, things like the FRS theorem seem to tell me that the opposite is the case.

Suggesting that Atiyah-Segal is “just metaphorics” only because it is not formulated in terminology of AQFT sounds misleading to me.

120. **Aaron Bergman**
May 28, 2006

I’m just posting this to say that Urs pretty much said what I was going to say and to reiterate that any definition of QFT that does not include TQFT (of which many are not obtained by first starting with a non-topological theory) seems severely wanting to me.

That it does not encompass the geometric insights regarding anomalies and seems to allow perverse theories as are obtained in Rehren duality doesn’t help things either.

121. **urs**
May 28, 2006

Kea wrote:

> The AQFT papers that I have glanced at recently all talk about functors from some suitable category of (local) spaces, such as the category of strongly causal manifolds. Where exactly do you think this is leading?

There is an obvious covariance condition in the axioms of AQFT. You assign algebras (of observables) to subsets of spacetime. You want these algebras to behave nicely under diffeomorphisms of spacetime.

As about any covariance condition, this can be rephrased as a natural transformation of some functor.

As far as I can tell, this observation does not give rise to something qualitatively new.

122. **Mentos**
May 28, 2006

Heh.

Let’s not all pile on at once.

[everything urs and aaron said +]… unable to handle quantum field theories with degenerate vacua (see our previous discussion of Seiberg-Witten theory), does not incorporate renormalization-group behavior a la Wilson, apparently has trouble dealing with supersymmetry, …

In short, it is pretty much silent about the past 30 years of developments in quantum field theory. And when it does speak up (eg, Rehrens Duality) , it says something silly.

Were it not for the efforts of this blog owner (hosting various documents for Prof. Schroer, and enthusiastically promoting his arxiv postings), few would ever have heard of AQFT. And for good reason.

123. **Eugene Stefanovich**
May 28, 2006

Bert, Urs, Aaron, Kea,

The (quantum field) theories you are discussing look very distant from ordinary quantum mechanics of particles that served us so well in low-energy physics. Is there an important reason (a no-go theorem, or something) that forbids application of simple rules of QM (the Hilbert space, the Hamiltonian, the wave function, etc) to high-energy phenomena and requires the complete shift of the paradigm from quantum particles to quantum fields? This shift of the paradigm looks especially strange if one takes into account that predictions of QFT are usually limited to small (radiative) corrections to the QM results, e.g., the Lamb shifts.

Surely, when energies are high, we cannot limit ourselves to the Hilbert space with fixed number of particles, because particles can be destroyed and created in accordance with Einstein’s formula E=mc^2. Thus, instead of the fixed-particle-number Hilbert space we need to consider the Fock space where the number of particles can change from zero to infinity. Then creation and annihilation of particles can be described by simply writing the interaction Hamiltonians as polynomials in creation and annihilation operators in the Fock space. However, this doesn’t require introduction of a radically new formalism, such as QFT.

My question is: why, in your opinion, this simple-minded approach doesn’t work? Are there important reasons (besides historical) to consider fields, rather than particles, as fundamental ingredients of nature? Thanks.

124. **Kea**
May 28, 2006

My question is: why, in your opinion, this simple-minded approach doesn't work?

Oh, but it *does* work…if you try to turn what Bert is talking about into higher category theory and raising and lowering operators become associated with the concept of categorification and decategorification…and Minkowski space gets turned into twistor geometry so that it's true topos theoretic nature can be identified.

125. **Kea**
May 28, 2006

Eugene

How can Bert and Urs/Aaron **both** be right? There is only one way. Urs/Aaron need to accept the possibility that 30 years of String theory has missed something important. Then they can look for it in the higher category theory language that they use. On the other hand, Bert needs to think about a very, very small request – the possibility of reformulating AQFT, without losing any of its structure, in a different mathematical language.

126. **Kea**
May 28, 2006

Oh, I forgot to mention…that conference that Vaughn Jones was organising? For some reason we had a lot of speakers talking about TFTs.

127. **Kea**
May 28, 2006

I think we should call vacuums elephants. Don't you, Who? The word vacuum just has too many connotations of dirty housework and dust mites.

128. **Aaron Bergman**
May 28, 2006

Urs/Aaron need to accept the possibility that 30 years of String theory has missed something important.

What does any of this have to do with string theory? This has been a discussion about whether AQFT is the right way to think about field theory.

For Eugene, you can look at why QFT developed in the first place, I suppose, but a straightforward question is how do you propose to do QCD?

129. **Kea**
May 28, 2006

What does any of this have to do with string theory?

All right. Sorry. I was just using the term as a short hand for the conventional physicist's use of SYM etc.

130. **Kea**
May 28, 2006

…but a straightforward question is how do you propose to do QCD?

The SU(3) confinement comes in at the tricategorical level when one is forced to break the Mac Lane pentagon and use premonoidal structures.

131. **Bert Schroer**
May 28, 2006

I just got back so before I go to sleep I will at least try to answer a few of those questions.
Let me start with Eugene because it is the most physical question and I have a eference for such straight physical questions
The transition from QM to QFT is indeed a total paradigmatic shift, to exaggerate a bit in order to get this point across: the only thing they share is the Planck h.
Let me explain this by looking back at history. Whereas the paradigm changing QFT was discovered by Jordan, Dirac had a better accepted entrance into this issue by placing multi-particle quantum mechanis (leading up to Fock space) into the centre of the stage. For the case of the Schroedinger theory, the both points merges rapidly. Together with the discovery of the Dirac equation, Dirac developed hole theory and some of the first textbooks which contained low order calculation (no loops) worked quite nicely (viz Heitler). But later it was seen that the particle-based hole theory is inconsistent: nobody was ever able to do renormalization theory based on it and when you massaged it so that you could do it, you lost the particle base and slipped into QFT. Another observation which indicated that there was a paradigmatic change was that of Furry and Oppenheimer who realized to their great surprize that an interacting field applied to the vacuum does not create a particle state but rather a state which had a nonvanishing component to the one-particle subspace but in addition the (in infinite order you get a "cloud" which involves infinitely many particle-antiparticle pairs) vacuum polarization admixture. The modern way is to demonstrate this paradigmatic change by a rigorous structural (model independent) theorem which says that if in a compact spacetime region you find any operator localized in that region which creates a polarization-free one-particle state, the theory is necessarily interaction-free (in other words you now have an intrinsic quantization-independent way of seeing the presence of an interaction). The first region where this breaks down is the noncompact wedge region, in that case so-called PFGs ((vacuum)-polarization-free-generators) exist even though the theory is not free. This theories which arise in this way are precisely the d=1+1 factorizing model and the Fourier transforms of the PFGs fulfill the Zamolodchikov-Faddeev algebra relations i.e. the positive/negative frequency components are still close to particle creation/annihilation operators. Although these models have scattering without particle creation (and in this sense they are close to what you wanted in your question), they have very complicated vacuum polarization clouds which prevent any quantum mechanical particle picture for compact regions! It is true that the generators of the noncompact wedge region still look like relativistic particles, but even on that level the ground is somewhat slippery since that elastic S-matrix realizes the full "nuclear-democracy principle" saying that there is no genuine particle hegemony between elementary an bound (though there is still a charge hegemony between fundamental and fused charges) every particle is formed from all the other particles inasmuch as charge conservation allows this. So already in this relatively simple class of interacting theories (where you still can save some of the particle concepts on the level of wedge localization) the paradigmatic change is obvious.
This has very grave consequences. For example those arguments in Susskinds and Weinberg's work you often find the terminology relativistic QM instead of QFT. Unfortunately these are not just words, in computing those cosmological vacuum expectation of the energy-momentum tensor these authors fill levels as if it would be quantum mechanics and get their absurdly large cosmological constant values. This is conceptually totally wrong (it contradicts the local covariance principle which according to Kea does not contain any new information) and was profoundly criticized in a paper by Hollands and Wald (with a very nice title, you find it in one of my old contributions to this weblog). Of course saying that does not mean that you can easily do a correct calculation (the cosmological reference state is not well kown and in gravity you rarely deal with invarianr vacuum states). If this is the basis for anthropical ideas, it is an extremely flimsy basis indeed.
In this context it is interesting to mention another point. The modular methods which we developed recently (in the paper with Mund and Yngvason which I already

mentioned several times) permitted us to solve an old problem from the Wigner representation theory namely what are the fields for those massless infinite spin representations. We showed that they are semiinfinite spacelike string-localized. This solves an old problem which generations of particle physicists tried to understand. Weinberg in his book mentions the infinite spin Wigner representation and then dismisses it by saying that " Nature does not make use of them". But the main job of a theoretician is of course to investigates its physical manifestations and decide afterwards whether it should be dismissed. After all the massless matter separates into two families the neutrino- photon… family and the much larger infinite spin (better helicity tower) family. It is true that this quantum matter has very unusual properties (already Wigner noticed a very strange thermal behavior). I think nowadays with the black matter around, one would think twice before dismissing it with those words.
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132. **Kea**
May 28, 2006

…it contradicts the local covariance principle which according to Kea does not contain any new information

I never said that.

133. **Bert Schroer**
May 28, 2006

Sorry Kea, I saw it somewhere, but it is not in your blogs. I applogize.
It is natural that there are TQFT at Jones's department because type II subfactors are (in some generalised sense) TQFT. But Jones knows very well (through his work with Wassermann) that QFT needs localization. In fact I am sure that he deepy appreciates the recent construction existence proof (and construction) of the minimal series by Kawahigashi Longo Pennig and Rehren . This is quite an achievment and this cannot be done by Frobenius algebras and sewing (you cannot construct QFT which has localization by sewing bones). It seem that some people have forgotten what a proof is.
Kea, remember I said that without localization you cannot talk about QFT and not that TQFT is a second rate enterprize. or anything like this mathematics does not need the blessing of QFT in order to be brilliant. The entire subfactor theory of Vaughn Jones was done in a context without localization and he would not even dream to use the word QFT for those constructions (although he knows perfectly how to to subfactor theory in the localizable type III setting)

134. **Bert Schroer**
May 28, 2006

Urs you said
"Klaus Fredenhagen has some first ideas on how boundary conditions might be modeled in the AQFT context. There was supposed to be a project concerned with investigating this question in the new String Theory Research Center in Hamburg (->) – but the referees canceled this particular project".
Urs I can perfectly understand that you are quite angry at those referees for having jeopardized this collaboration with the group of which you are a member.
Now we are getting closer. I could have told you already before you lost this collaboration which you desired so much, that whenever string theory related particle physicists get to evaluate QFT projects they would wreck that bit of reasonable research which still exists in Germany because their horizon is extremely limited.
I was a student of Harry Lehmann who together with Kurt Symanzik and Wolfhard Zimmermann, through there discovery of how one can even in the presence of that inexorable vacuum polarization (see the answer to Eugene's question) extract pure particles and their scattering matrix (lehmann received the prestigious Heinemann prize), brought a bit of brilliance to postwar German particle physics.
Two years before Harry Lehmann died I had a conversation (on one of my frequent visits to Hamburg) with him which is engraved in my memory. He asked me what is happening in Berlin, why does such an important position at the Humboldt u]University go to a string theoretician, an area whose contribution to physics was smaller than any pre-assigned epsilon. I told him that neither I nor my colleague Robert Schrader had any say in this; nobody ever asked us. He thought this was the beginning of a downpath in German particle physics.
Once I attended a seminar at the HU given by Sidney Coleman. Maybe younger people do not know, but Sid was the critical conscious of particle physics, a worthy successor of Pauli in keeping particle physics lively and healthy. He looked a bit frail and his behavior was changed from what I remembered from earlier encounters. What really shocked me (just because it did not fit his earlier critical image at all) was that he supported string theory. Later Robert Schrader told me that Sid was unable to continue his critical role because he suffered from an early onset of the Alzheimer disease. Only then I understood in retrospect why things had changed so much.
Well, Urs, I am not surprized that things go that way. Though I cannot hep you, I am sure that now, after the more QFT part of your joint venture was thrown out, you will see certain things my way.

135. **Bert Schroer**
May 28, 2006

Aaron, here is one last attempt to get you away from that proximity of Mentos.
The first derivation of the Hawking radiation in a collapsing star was done by Fredenhagen and Haag using the setting of AQFT (it was the problem which Hawking would have liked to solve, but as a result of conceptual complications he had to settle for the stationary case, see the book of Wald on this subject). I could continue this list, and in addition I already told you that everything which QFT can do AQFT can also do because the first one is included in the second. But there is a whole list of results which you can only obtain from AQFT because the inclusion is strict (not an equality). In my earlier blogs I have mentioned some of these results.
What I however cannot do is provide a vaccine against the string virus which like the bird flew virus is absolutly deadly, but it only kills the mind and the head is then filled with strings.

136. **Kea**
May 28, 2006

…remember I said that without localization you cannot talk about QFT…

Er…hello? I have been agreeing with you about that.

137. **Mentos**
May 28, 2006

"I already told you that everything which QFT can do AQFT can also do because the first one is included in the second."

Provided, of course, one uses a suitably narrow definition of "QFT," which excludes theories with degenerate vacua, theories with nontrivial WIlsonian RG behavior, supersymmetric theories, TQFTs, …

In short, throw out every development in "QFT" of the past 30 years, and AQFT has you covered.

Peter Woit, for instance, will be disappointed to learn that his beloved Chern-Simons Theory is not a QFT.

138. **Bert Schroer**
May 29, 2006

Mentos, you seem to honestly think that my role here is to please the owner of this weblog, well at least you are a honst guy.

139. **amanda**
May 29, 2006

"The SU(3) confinement comes in at the tricategorical level when one is forced to break the Mac Lane pentagon and use premonoidal structures."

" Sid was unable to continue his critical role because he suffered from an early onset of the Alzheimer disease."

Guards! Guards! Major crackpot invasion in the Woit sector!

140. **Chris Oakley**
May 29, 2006

Sid was unable to continue his critical role because he suffered from an early onset of the Alzheimer disease.

He could have just been faking it, having got tired of arguing against Superstring theory.

141. **stevem**
May 29, 2006

"The first derivation of the Hawking radiation of a collapsing star was done by Fredenhagen and Haag using the setting of AQFT. It was the problem Hawking would have liked to solve…"
Bert, why is this a better calculation than Hawking's? I assume you mean the paper: "On the derivation of the Hawking radiation associated with the formation of a black hole", Commum. Math. Phys. 127, p273 (1990).
But this comes quite a bit later than Hawking's so they did'nt do it first. I have not seen this but the archive of Comm. Math. Phys. is available free online, I think at "projecteuclid.com", so will look it up out of curiosity.

142. **stevem**
May 29, 2006

Correction, it is "projecteuclid.org" for anyone interested in the archived math journals there.

143. **Bert Schroer**
May 29, 2006

Chris: the calculation in the nonstationary situation is conceptually much more demanding than that in the stationary envirement of the Schwarzschild spacetime and it is not surprizing that this came quite a bit later.
I did in no way intend to build up a case AQFT against Hawking. I only mentioned this in connection with the ineredicable prejudice which affects some participants of this blog. This serious mental incapacity seems to affect mostly blog contributers who permitted there mind to be run by string theory and I certainly did not have you in mind.
When I entered this blog in April I had the illusion that one can change prejudices by rational arguments and facts, but now I realize that there are limitations. It is extremely difficult to argue with people who desperately insist to hang on some unfortunate premature terminology (before the time when the use of big Latin Latters became popular) and insist to take it litterally and defend their physical confusion-creating semantics with claws and teeth (a new breed of string millenium fundamentalists.)

144. **Bert Schroer**
May 29, 2006

Sorry, it was Stevem; but it could have been also Chris.

145. **Bert Schroer**
May 29, 2006

Eugene,
in order to enjoy some distraction, let me return to an old blog in which you surprized me by your familiarity with the Coester-Polyzou relativistic particle theory of "direct particle interaction". I now understand the origin of your recent question about the paradigmatic relation relativistic QM—-QFT. I do not think that there are many people in this weblog who really know about the existence of a relativistic multiparticle theory (without the property of being "second quantize representable") which fulfills all the requirements one can formulate in terms of pure particle concepts (including the very nontrivial cluster factorization). But you probably agree with me that it is not what we consider as "fundamental (I do not mean the hegemonic string theory interpretation of this word)" since it lacks vacuum polarization (although you could think of manufacturing something which approximates this by adding channel couplings between particle states with different particle number). But I think that even you would not try to understand the Lambshift or the cosmological vacuum problem (involving the energy-momentum tensor) in such a framework; you would rather make this big paradigmatic shift into QFT, would't you?
I looked at some of these other approaches you mentioned, but I have the intense impression (I don't have the time ti make the necessary lengthy calculations) that those fail precisely on this cluster issue. With other words I think that any relativistic particle theory has to look like C-P + more complicated channel couplings.

146. **JC**
May 29, 2006

I do appreciate Bert's "unorthodox" perspectives on physics (in comparison to today's "string" orthodoxy). I've been looking at AQFT on and off over the last two decades or so, though I don't have a complete understanding of it.

I bought into the string hype back in the mid 1980's when I was young and impressionable. In recent years, I was also still largely a string supporter until all that anthropic silliness started. (Though I could change my mind again).

147. **Mentos**
May 29, 2006

"I do appreciate Bert's "unorthodox" perspectives on physics (in comparison to today's "string" orthodoxy)."

That's "string," in the same sense Kea used above, shorthand for standard QFT, as understood by 99% of contemporary high energy theorists?

148. **woit**
May 29, 2006

Wow, the level of activity here is quite something. It's a holiday weekend so I've been very busy not working. Even if I was working and near a computer I don't think I'd have time to carefully read everything posted here, much less figure out how to properly moderate it. Discussion has certainly gotten off-topic, but at least it's interesting…

A couple quick comments of my own:

About the idea that I'm the prophet and promoter of AQFT: this is silly. Like many serious ideas out there about how to make progress on QFT, there are things about AQFT I find interesting, others I'm not enthusiastic about. I'm no expert on the subject, but happy to learn more about it from those who are. It's not obviously the best way forward, but then again nobody has an obviously best way forward at this point. It's a research program that has been pursued for many years by a very serious group of people, any such alternative program deserves attention these days.

About Sidney Coleman. His health problems are relatively recent, and undoubtedly keep him from commenting now on the current state of the subject, which is a loss for the field. By the way, at the party for Wilczek held here in New York last month his wife Betsy told me that they often see Coleman and he is in better shape than it seemed at the recent conference in his honor, something I was glad to hear.

I don't know exactly what Coleman's attitude towards string theory was during the 80s and 90s, as far as I know he made no public comment on it, but also chose not to work on string theory problems. All this was long before he ran into health problems.

149. **JC**
May 29, 2006

Mentos,

Yes.

Whether AQFT or any other alternative can replace it in the near future, I doubt it. What would impress me would be something which could calculate the n-loop m-point function (n neq m, in general) in a few lines, without having to crank out zillions of feynman diagrams.

150. **Mentos**
May 29, 2006

"What would impress me would be something which could calculate the n-loop m-point function (n neq m, in general) in a few lines, without having to crank out zillions of feynman diagrams."

Then I suppose you are intrigued by the Witten/Svrcek/Cachazo/Spradlin/Volovich/… twistor-inspired reformulation of (super)Yang-Mills perturbation theory.

I think the goals of AQFT (or any other attempt to give a rigorous account of QFT) do not include an efficient reformulation of perturbation theory.

151. **Bert Schroer**
May 29, 2006

JC
since AQFT incorporates QFT (I mean real time, in the Euclidean case you would need the subtle reflection positivity which is hard to verify and certainly does not hold for Chern-Simons actions) i. e. it implements the same principles (maybe in a conceptually more careful way), then against what do you want to compare it, what do you want to see replaced?
AQFT has not been elaborated for going beyond the speed limits of ordinary computations in QFT; it is not the computational speed you gain, it is the conceptual depth which you sometimes succeed to increase.
There is too much mystery around AQFT in this weblog, somehow I have the impression that you only tolerate sophisticated mathematics in string theory and you want to maintain QFT in a mathematical stone age and in case it is not you want to put it into another drawer.
Is it so difficult to understand that in an area which is so treacherous and paradigm-changing (see my remarks to Eugene Stefanovich) as compared to standard QM one must be a bit more careful?
AQFTists just have a higher awareness about these problems: I have never assisted a talk where speakers forget to explain the setting and the aim of a problem; whereas in string theory the comprehensible talks with a good balance between conceptual setting and technical tools and a clearcut separation of facts&fictions which I have assisted can be counted on the fingers of one hand (although here in Brazil there is somebody who succeeds to do just that).

152. **Thomas Larsson**
May 29, 2006

"What would impress me would be something which could calculate the n-loop m-point function (n neq m, in general) in a few lines, without having to crank out zillions of feynman diagrams."
Then I suppose you are intrigued by the Witten/Svrcek/Cachazo/Spradlin/Volovich/… twistor-inspired reformulation of (super)Yang-Mills perturbation theory.

I'm somewhat confused by this example. My impression was that Witten's original twistor string, which would have led to massive simplifications (perhaps only for unphysical SUSY QCD, but anyway), was simply wrong (important lesson: conjectured dualities may be plain wrong). However, it was possible to save the idea by adding an extra interaction vertex. The modified twistor string is probably right, since it is just a canonical transformation of QCD, but now the question is whether this is useful anymore. A canonical transformation may be useful, e.g. if you reach action-angle coordinates, but this does not seem to be the case here. Otherwise, a canonical transformation usually lead to a formulation which is as hard, or harder, from what you started from. TANSTAAFL.

As an example how ignoring an interaction vertex leads to great simplification, consider QED. If you ignore the electron-electron-photon vertex, QED becomes a theory of free electrons and photons. This amounts to a major simplification, but it is also wrong.

153. **Mentos**
May 29, 2006

Maybe you need to look at Cachazo and Svrcek's year-old review

http://arxiv.org/abs/hep-th/0504194

or (to pick one more recent work) Britto et al's paper on the 1-loop, 6-gluon amplitude in nonsupersymmetric QCD

http://arxiv.org/abs/hep-ph/0602178

154. **JC**
May 29, 2006

Bert,

What I meant by “replace”, was whether AQFT or another framework could completely replace the Feynman diagram method in generic graduate level textbooks on quantum field theory. At the present time, I don’t see AQFT or any other framework being the dominant standard presentation in an introductory quantum field theory textbook. Despite the Feynman diagram calculations being really messy and tedious, I don’t see it being pushed aside yet by any other framework.

Years ago I use to think that string theory could one day completely replace the Feynman diagram framework of conventional “textbook” quantum field theory. (In hindsight this probably sounds very silly and naive).

155. **Johan Richter**
May 29, 2006

Has anyone of you heard of casual pertubation theory? On Wikipedia it was described as a finite, mathematical rigorous formulation of QFT. Is it a crackpot theory?

156. **Thomas Larsson**
May 29, 2006

My observation was based mainly on [hep-th/0605121](), where it was stated that a canonical change of field variables converts the Yang-Mills Lagrangian into an MHV-rules Lagrangian. Perhaps this canonical transformation does lead to major simplifications for SUSY theories (or perhaps not), but it remains to be seen whether this has any physical relevance.

There is no question that Witten’s original twistor string from 2003 was wrong, since it gave the wrong results beyond tree level.

157. **JC**
May 29, 2006

Mentos,

I was quite impressed by the work of Cachazo et. al. when they were able to reproduce some results found earlier by Bern, Dixon, Kosower, et. al, with less labor.

Bert,

For a long time I found AQFT somewhat mysterious, largely because I wasn’t familiar with the mathematics involved. (I could imagine this to be the case for some theorists). It could just be laziness on my part, but for many years I more or less looked at advanced mathematics on a “need to know” basis. I didn’t really put much effort into looking at unfamiliar math, if I didn’t think it was directly applicable to string theory or physics in general.

158. **stevem**
May 29, 2006

Bert, thanks. The paper on the Hawking radiation is interesting. Yes, I see now that this is for the nonstationary case, which is the harder problem with the scalar field in the background of the collapsing star. Peter, off topic discussions like this should be encouraged as long as the discussion stays reasonable, interesting and civil with the prospect of learning something new or getting a different point of view.

159. **urs**
May 29, 2006

> Urs I can perfectly understand that you are quite angry

Hm, I didn’d say I am angry, did I? Maybe I am, maybe I am not. We didn’t speak about that. (At least, given the context, there are clearly more likely candidates for anger, aren’t there?)

I would be happy to further discuss facts about QFT, AQFT and the like. In particular, I would be very interested in a factual reply to my last comment .

([http://www.math.columbia.edu/~woit/wordpress/?p=396#comment-11299]())

What I actually said was that it is not known how AQFT can deal with general boundary conditions.

On the other hand, in Atiyah-Segal-like formulations of QFT we do know how to deal with boundary conditions.

And its actually quite central to the understanding of QFT. The RCFT theorem says, among other things, that you can understand full RCFT from just knowing any one of its boundary conditions.

The reason is that the Frobenius algebra A which appears in that theorem (being the internalization of the Frobenius algebra known from topological 2D QFT, but now internalized, in the category-theoretic sense, into the representation category of some chiral vertex algebra) is nothing but the OPE algebra of open string states both whose ends have some given boundary condition.

It’s pretty remarkable that all other boundary conditions are then obtained by looking at the modules for that particular algebra.

Behind this is a nice little piece of general abstract nonsense, due to a theorem by Viktor Ostrik.

This theorem says ([->]()) that in every module category of our representation category of the chiral algebra, an internal algebra is given by the internal Hom of any one of its objects. These objects are nothing but RCFT boundary conditions, and the internal hom is something like the internal scalar product on these, when regarded as a issue in categorified linear algebra ([->]()). It’s interesting how this abstract nonsense translates into concrete CFT physics.

I am eager to learn, so if you like to teach, go ahead. (Maybe insert a paragraph line break here and there so that I can orient myself in the wealth of information given.) You may imagine that I am not a string theorist. Just a person interested in physics.

160. **Bert Schroer**
May 29, 2006

Peter, I tried to recollect the chronology of my encounters with Sidney Coleman and I think you are right, this Berlin meeting must have been before the onset of that tragic incapacitation; in any case there was nothing visible in his talk (which different from his usual topics was on some fundamental problem in QT). I do remember however that he was behaving more supportive of string theory than the amount of politeness required if one is invited by a string theoretician. Memory is often not completely faithful and depends a bit on those aspects which one is thinking about in the present.

Somehow I have a much clearer memory of those times when I met Sidney in Rio at the invitation of Jorge Andre Swieca. One reason may be that he got me into a very funny situation. He was suffering from a very severe form of diabetis and had to keep his bood sugar always around a certain level and to achieve this he ate sometimes small amounts of bananas. Before his seminar talk he asked me if I could arrange 2 bananas for him, so I left the PUC compound and went to a bar on the opposite side of the street and asked for two bananas (the always used to have small amounts for bananas to make a mixed shake called vitamina). I will never forget that strange look, just the kind of look a Gringo who asks for two bananas in a bar in Brasil would receive.
The next day we went to the Tijuca forest because Sidney wanted to see a macumba. He looked interested at the somewhat strange ceremony but did not comment it. The day after we asked him what he thought. He took a deep breath and said: you know the best situation is to have no religion at all, but if the number cannot be zero, it should be infinite. He was of cause referring to the large almost continuous spectrum which is the result of religious syncretism between African, Indian and European ingredients. In fact some years ago there was a delegation of Nigerian academics who studied the preserved rites and African culture in the Bahian diaspora.
There is also a valley in the mountenous region of Espirito Santo were descendents of Pommeranians live. Their ancestors were bondslaves in the feudal system which came to an end when Napoleon went through Europe up to Moscow. These people were free but without land and so the imperial government of Brazil payed their crossing and gave them land. Since the old Pommerania does not any more exist (after worldwar II it became part of Poland) this is the only place in the world were one can study this unique form of old northern German. They invented German names for tropical fruits; for example the "frutta de conde" which you take apart with your hand in order to get to that marvelous tasting pulp which melts in your mouth, they call "Schmalzapfel", an ingeneously fitting term.
Returning to Sidney, I think that Robert Schrader attended an event which was in Sid's honor, so it probably was the same which you mentioned. He told me that he was in a not so good shape. If, as you seem to say, the situation improved somewhat, one would whish that it reaches a point where it becomes interesting and meaningful for his friends to converse with him. He has done a lot to maintain the clarity of content and presentation (including good terminology) to deserve a satisfying evening of life.

161. **JC**
May 29, 2006

Johan Richter,

I haven't quite understood the point of "causal perturbation theory" in the Epstein-Glaser approach. Other than reproducing some results which were already known previously in conventional Feynman diagram calculatons, it seems to be an attempt at dealing with the formal details of the singular nature of fields which often gets "swept under the carpet" in textbook treatments of quantum field theory. (Somebody else can fill in the details).

Bert,

The main reason I first became interested in AQFT was that I always felt that there should be a way of doing quantum field theory without resorting to a classical Lagrangian in an intermediate stage. Ever since string theory has fallen into the anthropic abyss and particle phenomenology has more or less flatlined, I've been trying to understand other approaches like AQFT, LQG, etc … for which I previously looked at on and off for many years without much understanding. Since I don't write research papers anymore these days, I've been spending more time trying to understand AQFT and other older approaches such as in Bogolubov's two books.

162. **Eugene Stefanovich**
May 30, 2006

Bert:

B.S.: "I do not think that there are many people in this weblog who really know about the existence of a relativistic multiparticle theory (without the property of being "second quantize representable") which fulfills all the requirements one can formulate in terms of pure particle concepts (including the very nontrivial cluster factorization). But you probably agree with me that it is not what we consider as "fundamental (I do not mean the hegemonic string theory interpretation of this word)" since it lacks vacuum polarization (although you could think of manufacturing something which approximates this by adding channel couplings between particle states with different particle number). But I think that even you would not try to understand the Lambshift or the cosmological vacuum problem (involving the energy-momentum tensor) in such a framework; you would rather make this big paradigmatic shift into QFT, would't you?"

E.S.: To the contrary. There is no vacuum polarization in the "dressed particle" approach I was referring to. That's the beauty of it. The idea was first suggested in

O. W. Greenberg and S. S. Schweber, "Clothed particle operators in simple models of quantum field theory", Nuovo Cim. 8 (1958), 378.

In my opinion, this is the best paper about QFT written since Tomonaga-Schwinger-Feynman. The idea is to apply to the QFT Hamiltonian a unitary dressing transformation which kills all vacuum polarization terms and transforms them into particle-particle interactions. In this approach, the Lamb shifts and anomalous magnetic moments are not results of the vacuum polarization and virtual particle loops. They are consequences of small corrections to the particle-particle Coulomb potentials that arise from the dressing transformation. The particle-number-changing interactions that couple different channels result from the same transformation. They are not "manufactured". You can find the latests developments in this area and references in nucl-th/0102037 and in chapter 12 of
physics/0504062.

B.S.: "I looked at some of these other approaches you mentioned, but I have the intense impression (I don't have the time ti make the necessary lengthy calculations) that those fail precisely on this cluster issue. With other words I think that any relativistic particle theory has to look like C-P + more complicated channel couplings."

E.S.: I can't agree with you here. It is shown in vol. 1 of Weinberg's "The quantum theory of fields" that if interaction is written as a polynomial in particle creation and annihilation operators with coefficients that are smooth functions of momenta, then the theory is automatically cluster separable. Both Kita's and Shirokov's models belong to this class, so the cluster separability is not a problem for them.

However, Coester-Polyzou interactions are not written in terms of creation and annihilation operators (they are written as functions of relative momenta and positions of particles), so the cluster separability is a big issue there. It is achieved by a rather complicated combinatorial construction of the interaction potentials.
This is why I think that Coester-Polyzou type models do not have a bright future.

Eugene.

163. **Thomas Larsson**
May 30, 2006

Completely OT: I have just received the NEW book from amazon.co.uk.

164. **Peter Woit**
May 30, 2006

Thomas,

Thanks for the news. Last I had heard British publication date was June 16th.
Interesting to know that it is being shipped. I still only have one copy myself…

165. **Bert Schroer**
May 30, 2006

Urs
Our controversy is about the use of conceptual precision in the terminology of particle physics. Let me make a second attempt to overcome it, or at least to give my position sharper contures.
The reason why I think that terminology and semantics in physics are important in times of "everything goes" (somebody mentioned the tower of Babel the other day) is that, wanting or not, they carry some connotation about the physical content and when a novice enters an area by reading electronic articles there is not always a knowledgable person next to him who helps him to steer around the riffs and cliffs of misinterpretation.
Let us keep the discussion within the borders of the previous hot point: TQFT against QFT and without loss of generality we may look at what Atiyah called the Jones-Witten invariant.
Now there are two ways of connecting certain subfamilies of that gigantic edifice which Vaughn Jones called "the theory of subfactors" (no qualm about this beautiful and meaning-loaden terminology) which via his "Markov trace" formalism (a terminology with a beautiful subtle two-fold meaning as I mentioned on an earlier occasion) leads to that tracial (type II) "bone" algebra (without localization) which contains the data of knots, mapping class groups and all that. There are two QFT-inspired ways to get to this.

One way is to adaptate the DHR technique of 1970,71 (contained in Haag's book) of "thinning out" QFT (always localizable in my use of the word) to obtain tracial "bone" states on the group behind particle statistics. In the higher dimensional DHR context this finally leads from the observable net to a (modulo some conventions) unique field algebra net which contains all the charge transfer operators which communicate between the different superselected representations of the observable net. The adaptation to the richer statistics realization which the locality principle permits in low dimensional spacetime (which you find e.g. in FRS, Review of Mathematical Physics, Special Issue (1992) 113) then leads to the same tracial Markov state on the same algebra (which from the QFT point of view is a subalgebra of intertwiners). This is certainly what Jones would have done if at the time he looked at these structures with Wassermann our paper did not already exist.

On the other there is Witten's derivation (from the great magician of actions) which starts with a a functional integral involving a geometrically based Chern-Simon density. Witten extracts with his typical hindsight and artistic skill (imposing certain framing rules) the mapping class group invariants. This is fine and it finds my unrestricted admiration, but the derivation has little to do with QFT, its relation to QFT is metaphoric. Why? There are zillions of functional integrals which you can write as exponential of an action, but most of them do not lead to QFT (even if you succeed to make some sense out of them by a renormalization-massage which destroys the validity of the representation you started with). There is a very subtle filter (coming from the O-S work) called the "reflection positivity" (insuring together with a certain amount of analyticity the localization which is an inexorable property of QFT) which the C-S action does not pass. Ignoring this subtle property has led to what I call a "banalization" of Euclideaniztion (i.e that structure which is behind the "Wick rotation", if you want to learn more about this criticism look at some lectures of Rehren, hep-th/0411086). This statement about the nature of the C-S action is not a moral or even a mathematical judgement. Even without being QFT (but certainly being QFT-instigated), this looses nothing of its mathematical value.
The general problem behind this is the following. In the QFT setting (either a la Wightmann, or a la LSZ, or AQFT) if something appears as an elephant it really is an elephant. This is not the case at all with functional integrals unless you have gone through the whole O-S litanei.
This may be considered to be pedantic. But we are in the midst of a string-millenium clearance sale of particle physics and if we will not be careful about our Faraday-Maxwell heritage handed down by Dirac, Jordan, Pauli…a sellout of all those of our concepts which were important in past conquests will take place (superficially, just because a group of very influential people with significant past achievements, although this could not happen if the Zeitgeist would not allow them to do this). If we don't wake up and pay attention now, we will have to spend a very very long time on physically feable theories (with physical content smaller than any preassigned epsilon) or even on a totally failed project.
Some people may think that I am very courageous to say such things. Actually I am not, I just think that terminology in the exact sciences should be totally related to the content (examples: Tomita-Takesaki, Hilbert-Schmidt, or from physics Einstein-Hilbert or Haag-Ruelle etc). If we, like it is done in politics, have not only to accept our terminology subject to a hegemonic handdown and mining claims, and if even more so we are forced to accept the literal meaning of words (as in TQFT and as in Christian religions before the enlightenment), than I do not want to participate longer in such a lost course as far as particle physics is concerned. I am already doing part-time fishing in the Atlantic and enjoy it, why not do it full-time. It is as simple as that.
If Vaughn Jones would be around at this weblog, he would totally agree with me, we both have a very developed ability to distinguish metaphorical from intrinsic statements and we had it already at the beginning of the 90 when he invited me to Berkeley (I think that this was where this photo of mine in that collection which appeared the other day must have been taken). He would immediately tell you that category theory, Frobenius algebras + cutting and sewing makes very interesting mathematics (although I think he would not use those instruments) and was suggested from physics, but to construct (not just classify) chiral QFTs you have to use other methods. And he would consider that recent construction of the minimal model family by Kawahigashi et. al. (with an addition of a model which was still missing in the old classification) the finishing touch on a program which he started together with Anthony Wassermann.
Urs, in no way I wanted to downgrade the work which is presently done at the Math. department of Hamburg university in your group, this is honestly not my intention; I only ask you to have the same respect for terminology coming from particle physics as I have for that in mathematics. We both cannot change the existing TQFT terminology, but you should not press me to take that the QFT in that word literally. Even if there is a large community who would look down on me for not accepting that and remaining in their eyes a stone age QFTist, I can perfectly live with this since to have somebody like Vaughn Jones on my side is sufficient for me.
By the way I think that the problem which we are discussing here is very much related to that issue of what should go into hep-th and what should be posted in math-ph or mathematics. It is worthwhile to point out that the majority of AQFT papers are posted in math-ph despite the fact that they are significantly closer to QFT than most of the typical papers there (just have a look at the systematic work on renormalization with the separation from algebras and states which led to those marvelous achievements in curved spacetime QFT, culminating in the new local covariance principle, http://unith.desy.de/research/aqft/). This is because the AQFT authors are more conscious and they certainly do not post anything onto hep-th of the kind were the author has interesting math and desperately wants a connection to physics and hopes to find someone who is able to make that connection. Another reason why Distler allows purely mathematical articles (sometimes given the physics flavor by foregoing rigorous proofs because this may be bad for the physical intuition of those partners or groups which they want to address). But for a change I do not here want to criticize Distler (because he works under a hell of sociological pressure) as long as he does not overplay the saying: quod licet ceasar non licet bovi.

166. **Bert Schroer**
May 30, 2006

Eugene
let us return to that interesting topic within a couple of days (se Peter quiser) since I have pressing other obligations.

167. **Kea**
May 30, 2006

He would immediately tell you that category theory, Frobenius algebras [etc.] makes very interesting mathematics…but to construct (not just classify) chiral QFTs you have to use other methods.

I have asked Vaughn to comment. Let us hope that he does.

168. **D R Lunsford**
May 30, 2006

Bert S said:

Alejandro,
Nobody is against geometry, but it has to come from the midst of raw local quantum physics…

This is a fundamental misconception. Everyone makes it, including string theorists. Insofar as I have a counterexample to this statement, it is wrong in fact, as well as simply in spirit.

-drl

169. **Bert Schroer**
May 31, 2006

Lunsford,
I honestly do not understand what you mean by that

Eugene,
just some preliminary remarks which should yet to be taken with a grain of salt. I do not understand your reading of Wally Greenberg's and Sam Schweber's old article (but since I do not have a copy anywhere near me I will be careful and preliminary).
If you really gave a correct account of the central point of the article, I would be still be reluctant. On the one hand mathematical physics in those days was on a lower level (although these authors belonged to the cream). For example those unitary dressings could lead out of the Hilbert space into inequivalent territory. Or perhaps the maintained a cutoff.
What I really do not understand is how could you dump such a complicated dynamical structure as vacuum polarization which depends on the region of localization (see my treatment of holography posted on that Goettingen server) into a modification of interaction (in standard approach (which was the only one known in those days) probably additional contribution to the interaction.
I think even in those days people had a bit more sophisticated vision of the QFT vacuum than that in that abominable mentioned level counting for the vacuum contribution to the cosmological constant; although my old colleagues (I am not significantly younger) certainly were still far removed from the level of understanding in the mentioned Holland-Wald paper.
Let me add that there is another area where such misleading views about the vacuum in locally covariant theories may have unfolded there treacherous ((wrongly simplifying) lure. This is the juxtaposition of a classical calculation based on differential geometry (Bekenstein's area law) with the Hawking thermal QFT setting. I really do not understand why quantum entropy should jump over that classical stick. Quantum localization and the ensuing autonomous thermal manifestation including quantum entropy are inexorably linked. And by the way, the area law for local quantum matter (which through AQFT holography becames associated with the causal horizon of the bulk in which it is localized) is totally universal whereas classically (taking the quantum entropy interpretation of the Bekenstein area law seriously for a moment) it is only valid for very special classical field theories including of course the Einstein-Hilbert theory.
Eugen, I think even among afficionados of string theory you would find little support for your vaccum viewpoint. And by the way, I have my serious doubts that that dubious sounding derivation of clustering from some momentum space analytic properties is due to Weinberg. What I was criticising before was Weinberg's logic: P-invariance + clustering —-> (local) QFT. But this is something else.

170. **Alejandro Rivero**
May 31, 2006

Eugene,

I just glanced at the abstract of your papers during the coffee time, but I think to recall you were against the need of keeping relativistic invariance at quantum level, were you? I ask because most of the argumentations of string theoretists are that their quantisation of the string breaks reparametrisation on the world sheet and relativistic invariance in the target space, and that the only way to avoid it is to fix D=26 (or D=10). But in principle the non critical strings, at any D, should have Lorentz symmetry back in the classical limit. So it is really a problem?

171. **Eugene Stefanovich**
May 31, 2006

Bert:

If you think that vacuum is a "boiling soup" of particles and antiparticles, then you need to explain the null result of the following simple experiment: place a photographic plate (or any other sensor) in an evacuated shielded chamber. Wait for a long time and then develop the plate. I think you agree that there will be no image on the plate. This means that all those virtual particles are not observable. Then what is the point to keep them in your theory?

…Or perhaps they maintained a cutoff.

Greenberg & Schweber paper didn't reach as far as to the loop integrals and renormalization problems. However, their approach can be used to systematically eliminate ultraviolet divergences from both the Hamiltonian and the S-matrix of QED in each order of the perturbation theory without cutoffs

E. V. Stefanovich, Quantum field
theory without infinities, Ann. Phys. (NY)
292 (2001), 139.

I have my serious doubts that that dubious sounding derivation of clustering from some momentum space analytic properties is due to Weinberg.

I am not sure if Weinberg is the original author of this derivation, but I don't have any problem with his proof in section 4.4. Do you?

I am sorry, most of your other comments about entropy, holography, etc. went over my head. I have no knowledge in these areas.

Alejandro:

…but I think to recall you were against the need of keeping relativistic invariance at quantum level, were you?"

Quite opposite. The relativistic invariance is the cornerstone of my approach. In my view, any sensible relativistic quantum theory should be formulated in terms of a unitary representation of the Poincare group in the Hilbert space.

No comments about strings.

172. **Bert Schroer**
May 31, 2006

Dear Eugene,
you do not see the boiling soup on a plate, but whenever nature converts the Gedanken experiment of localizing quantum matter into reality, like in the case of black holes,

you of course see the soup right on the event horizon and the resulting thermal Hawking radiation at large lighlike distances. Nobody can get this radiation back into virtuality, and if your theory can do, this it is not the right theory.
Whereas I believe that for a certain process you may encode the vacuum-polarization into a modification of interaction between particles, the claim that you can uniformely (i.e. for the whole theory and not only for a preselected process) dump vacuum polarization elsewhere and still maintain the underlying principles of the theory (locality, positivity of the energy-momentum spectrum) is to me totally incredible. After all vacuum fluctuation on the causal boundaries of localized quantum matter is a direct consequence of those principles. Vacuum fluctuation was discovered by Heisenberg in connection with the quantum Noether theorem, when he tried to make sense of a "partial" charge, i.e. a charge attached to a finite bulk region. The above mentioned thermal aspect of localization (which is totally absent in your desired quantum mechanical description because Born localization does not cause such a thing) as a hallmark of QFT (not explainable in terms of the uncertainty principle!!) is of a more recent vintage.

173. **Eugene Stefanovich**
May 31, 2006

Bert:

…whenever nature converts the Gedanken experiment of localizing quantum matter into reality, like in the case of black holes, you of course see the soup right on the event horizon and the resulting thermal Hawking radiation at large lighlike distances.

Nobody have seen the Hawking radiation in experiment, so I reserve the right to remain sceptical about this argument.

Whereas I believe that for a certain process you may encode the vacuum-polarization into a modification of interaction between particles, the claim that you can uniformely (i.e. for the whole theory and not only for a preselected process) dump vacuum polarization elsewhere and still maintain the underlying principles of the theory (locality, positivity of the energy-momentum spectrum) is to me totally incredible.

That's right, in the dressed particle approach, the vacuum polarization terms get absorbed into particle-particle interactions. These are direct action-at-a-distance interactions. So, you are right, the property of locality is lost. The question is how fundamental is this property? Even the usual Coulomb-gauge QED Hamiltonian contains a non-local direct interaction term. Nobody complains about it.

From the experimental standpoint, as far as I know, there is no direct evidence that electromagnetic interactions between charged particles are retarded. However, there are quite a few recent experiments (Chiao, Nimtz, Ranfagni,…) that demonstrate superluminal effects in the propagation of evanescent electromagnetic waves.

I am sure, you are going to say that action-at-a-distance contradicts relativity and causality. This is my favorite topic, and I can give you detailed counter-arguments, but I don't want to abuse the hospitality of our host Peter.
You can take a look at

E. V. Stefanovich, "Is Minkowski
space-time compatible with
quantum mechanics?" Found. Phys. 32 (2002), 673.

174. **Bert Schroer**
May 31, 2006

Eugene,
I am saying what you say is all deja vue, but in a very bad sense. I kindly ask you to look up G.C. Hegerferfeldt, Phys. Rev. lett. 72 (1994) 596. It was claimed in this unfortunately published paper (total incompetence of the referee) that a more careful review of the famous Fermi atomic Gedankenexperiment (which Fermi used in order to argue that the velocity of light is not only the classical limiting velocity but this is maintained in QED as well) led to the possibility of a superluminal propagation. The paper had a grave conceptual flaw, (to say it in modern terms relevant to your problem) the author confused QM Born type of localization with localization carried by quantum fields (in field-coordinatization independent terms: modular localization). This was a big international splash, it went through all the international press (including new York Times), I saw it in Der Spiegel. The editor of Nature Maddox (I hope I remember correctly) had a big article under the headline: physicist from Goettingen proves feasability of time machines.
Two of the colleagues of the author had rapidly written a counterarticle:
http://br.arxiv.org/abs/hep-th/9403027
and I wrote a letter to the editor threatening to never review a paper again unless they undue this mess by publishing the correct version as well which they did (Phys.Rev.Lett. 73 (1994) 613).
Now I do not want to dismiss the Born probability interpretation as irrelevant; to the contrary it is absolutely necessary for scattering theory, there you need a localization with probability interpretation (coming with projection operators which the modular localization does not have) and lo and behold it is asymptotically Lorentz invariant (whereas the modular localization is throughout covariant but comes without a particle probability interpretation i.e. has no projectors). A modern account using this terminology you can find in
http://br.arxiv.org/abs/math-ph/0511042
This superluminal conceptual error is the evergreen of all errors, it is ineradicable and reappears almost vearly like Nessy in Scotland.

175. **D R Lunsford**
June 1, 2006

Bert –

A geometry is an invariance group, and has nothing at all to do with quanta directly. Quanta arise when one imposes a concept of measurement, which is outside the geometry. Therefore, it is simply wishful thinking, or a misunderstanding of geometry, to insist that geometry arise from quanta.

Consider that, in a theory in which space, time, and matter have a common origin, their mutual phase relations can lead to quanta.

-drl

176. **Eugene Stefanovich**
June 1, 2006

Bert,

I knew about Hegerfeldt controversy, but had no idea that it reached New York Times and der Spiegel. That's funny.

I am wondering why you oppose so strongly the Newton-Wigner concept of localization? Is it because the NW localization is observer-dependent? Indeed, if oberver at rest O prepares a particle in a localized state, then moving observer O' sees the wave function of the same state as being delocalized over entire space. However, I don't consider it as something totally unreasonable.

By the way, this observer-dependent localization seems to be helpful in solving the Hegerfeldt's paradox. Suppose that observer O prepares a particle localized at point A. Suppose that the wavepacket spreads superluminally so that detector at point B has a (small) chance to register the particle earlier than at t=R(A-B)/c.
Suppose further that observer O' moves with a high speed, so it (sometimes) sees that detector at B clicks earlier than the particle is released at point A. Definitely, this looks peculiar to O', but he has a good explanation: from his point of view the particle was not properly localized by O from the beginning. He thinks that the wave function of the particle was spread over the entire space (including point B) all the time. So, this doesn't look like a irreparable violation of causality. And this is rather far from building time machines. Isn't it?

The response by Buchholz and Yngvason does not look entirely convincing to me. In order to decide unambiguously whether the excitation spreads slower or faster than light one needs to perform a calculation of the time-dependent wavefunction, which is still missing. This leads to my other question which worries me a lot.

From what I can see in textbooks, relativistic QFT is concerned only with calculations of the S-matrix (scattering cross-sections and energies of bound states are ultimately related to the S-matrix elements). However, I wasn't able to find any RQFT calculation of the time evolution in an interacting system from first principles.
I think I understand why such calculations are missing. Please tell me whether I am right or not.

In my opinion, the problem is that in realistic RQFT theories (e.g., in QED) there is no well-defined Hamiltonian. Without a good Hamiltonian one cannot form the time evolution operator and study the time dependence of wave functions and observables. The Hamiltonian of QED is deficient for two reasons. First, it must contain infinite counterterms (if we want to get the S-matrix right). Second (and most importantly), it contains those vacuum polarization terms that transform the vacuum and one-particle states into infinite linear combinations of multiparticle states, which is totally unphysical.

My diagnosis is this: the particles whose creation and annihilation operators are used to write down the QED Hamiltonian are actually fictitious "bare" particles that are never observed. If we want to study the time evolution we should be concerned about "physical" or "dressed" particles that are some linear combinations of bare particle states. This seems to suggest that time evolution calculations (and, by the way, the solution of the Fermi problem) require transition to the "dressed particle" representation where the vacuum polarization effects are not present. Do you agree, or I am totally wrong?

I am not sure what is the role of modular localization in all this. I am still studying your "string localization" paper, but I have more questions than this weblog can handle.

Thank you.
Eugene.

177. **Bert Schroer**
June 1, 2006

to Lunford,
now that I understand your statement, I can say that I disagree with it. The autonomous modular localization theory (see the last reference in my previous blog contribution) does just this: it extracts geometrical and group theoretical data from the abstract domain of definition of certain unbounde operators in the modular setting. A very poignant account can also be found in
http://br.arxiv.org/abs/hep-th/0502014
One small consolation: I also had to go back to school (already many years ago) and to undue my picture of QFT (not so different from yours) and relearn a lot of things, so I encourage you to make a yet additional investment into QFT and new math. This new setting also leads to a completely autonomous picture of local quantum physical reality (i.e. models of QFT) in terms of relative position of a finite number of copies of (Leibniz) monades as I pointed out in my comment to Christine's last contribution. By the way, string theory can never ever lead to such a setting, because it is a physically-instigated powerful production machine for mathematical conjectures (and pseudo-physics from geometrical imaginations about particle physics, i.e. Lundfors's direction) and as such it is very very good.

178. **Kea**
June 1, 2006

In discussion with Urs, Bert Schroer said:

He would immediately tell you that category theory, Frobenius algebras [etc.] makes very interesting mathematics, but to construct (not just classify) chiral QFTs you have to use other methods.

Vaughn Jones has expressed reluctance to participate in a public discussion of this kind. He has, however, informed me that he completely supports Bert's statement above and that he also confirms the pivotal role of Anthony Wassermann in the first construction of concrete chiral models (the local nets associated with loop groups).

179. **Bert Schroer**
June 1, 2006

Eugene,
I am not at all opposing the use of the (only asymptotically covariant) Newton-Wigner (= Born probability density) localization in the context where it is valid, I am opposing only the context in which you have been using it.
Without N-W localization and the associated probabilties and projectors you would not be able to derive scattering theory from the QFT priniciples (Haag's book, and in more details Arakis little book) and hence there would be no particle physics. One should not emphasize so much its negative side (not covariant=observer dependent for finite distances) but concentrate on its asymptotic covariance and observer independence=invariant S-matrix.
It is very very unfortunate that even philosophers of science have fallen into this pifall see D. Malament, In defense of a dogma, why there cannot be a relativistic quantum mechanics of (localizable) particles, in R. K. Clifton (Ed.) perspectives of quantum reality, Dortrecht Kluwer, 1996 . The title of this article is total humbug but its main No-Go theorem is not only correct, it is something of the finest. The author shows that the N-W non covariance syndrome is part of a more general theorem (which bears his name) which establishes the impossibility of projectors with certain properties; it in turn is a special case of the by now well-understood property that any notion of localization that requires the set of states localized in a spacetime region O to be orthogonal to the states localized in the causal complement O' is incompatible with translational covariance and positivity of the energy. Unfortunately no word in Malament that this conclusion is evaded in asymptotia which makes particle physics more than an entertainment on a high intellectual level for philosophers of science. Working with the right concepts of modular localization (which does not lead to localization probabilities but rather to expectations which cannot be resolved in terms of probabilities). Before you ask any question about such things you should study the articles (I cannot be your weblog nanny). If you look into
http://br.arxiv.org/abs/math-ph/0511042 (in print in CMP)
You will realize that the main problem is not computation or mathematics rather these problems are conceptually unusual and very demanding. As often in life, the biggest step is to overcome one owns prejudices. What makes me sometimes angry are those string aficionados who have lost the intellectual modesty one needs for understanding local quantum physics, but I never had the impression that you, Eugene, are part of that group.
By the way, localization is absolutely crucial for the autonomous physical interpretation of the theory, it is very very basic. It is the place where string theory manages to

create the greatest tohu-wabohu under the sun.
Your other questions I will comment on in a separate blog.
Sorry for the delay, there was some work on my server which took more than half the day.

180. **JC**
June 1, 2006

Bert,

I think I'm starting to understand where you are coming from with respect to AQFT. For many years I more or less looked at AQFT as if it was just purely a formal math problem. After reading your posts here for awhile and some of your papers, I'm getting the impression that the conceptual issues behind AQFT are just as important as the math content. (ie. No math for the sake of math).

When I first studied quantum field theory, I largely glossed over a lot of the conceptual issues. In those days I was more interested in grinding out Feynman diagrams.

181. **Bert Schroer**
June 1, 2006

JC
even such a phantastic vizualization of perturbation theory as Feynman's gift to QFT could become oppressive and counterproductive after half of a century if its iconization is driven to a point where it becomes synonimous with the vast an still largely unexplored territorry of local quantum physics.

182. **Eugene Stefanovich**
June 2, 2006

Bert,

thank you very much for your detailed explanations. Now I start to understand that your earlier comment that the only thing common to both QM and QFT is the Planck constant, may be not as big exaggeration as I originally thought. I can't swallow this so easily.
Need to think.

183. **Bert Schroer**
June 2, 2006

Eugene,
I sometimes use a bit provocative formulation, but I never manipulate facts. In the present case the reason is of course that you are not the only one who has (had?) this quantum mechanical view of the vauum; to the contrary you are in the illustrous company of Weinberg (see earlier remarks) and all those illustrous prople who use the classical (differential geometric) Bekenstein law for quantum mechanical degrees of freedom counting in order to extract an energy/entropy formula.
Without having looked at the details, the violation of the local covariance principle (leading to a localization-dependent vacuum polarization) makes such counting arguments very very suspicious to me (and I am not the only one, see http://br.arxiv.org/abs/gr-qc/0405082). This new principle to which Hollands and Wald have prepared the groundwork and which received the final conceptual and mathematical touch from the AQFT university of Hamburg group, cannot be violated without running into absurdities.

184. **JC**
June 2, 2006

Bert,

The only topics resembling "formal" aspects of QFT which were covered when I first took QFT courses, was stuff like Kallen-Lehman representation, LSZ reduction, and the CPT theorem. The rest of the course was largely covering the details of cranking out Feynman diagrams and doing renormalization.

These days some of my former colleagues start a QFT course by teaching the path integral from the very start, and only giving minimal lip service to "old style" canonical quantization. It's as if they're treating the path integral as if it was a "god given" object of some sort.

185. **Eugene Stefanovich**
June 2, 2006

Bert,

let me get it straight… If I understand you correctly, in your view of QFT there is no Born's probability interpretation, there are no projection operators, and the Hermitian operator of position is absent as well. So, it is fair to say that QFT does not respect the basic postulates of quantum mechanics. Is it right?

Does it mean that the very first sentence in Weinberg's vol. 1 is wrong?

First, some good news: quantum field theory is based on the same quantum mechanics that was invented by Schroedinger, Heisenberg, Pauli, Born and others in 1925-26, and has been used ever since in atomic, molecular, nuclear, and condensed matter physics.

Does it mean that QFT is not (as I naively thought) an attempt of unification of quantum mechanics and the principle of relativity? From your remarks and references it appears that QFT makes its own rules which are quite different from the laws of quantum mechanics as we know them. Is this an accurate description of your position?

186. **Bert Schroer**
June 2, 2006

Eugene,
not quite, you do have localization probability even in QFT and you do also have associated projectors, bot they are neither covariant nor local even though the setting in which they occur is covariant and local (but those noncovariant ones are very good for scattering theory). There is however also a covariant and local concept of localization. If you apply the polynomial algebra P(O generated by smeared field (where the smearing functions range over all smooth functions which have their support in a spacetime region O).to the vacuum i.e. P(O)|0> you get a dense linear subspace of the full Hilbert space (e.g. the Fock space). The totally unusual (from a QM viewpoint) fact is that there is very deep physical information in this inclusion of this dense subspace within the full space. With a change of O the position of there dense subspaces change their position. With the help of the definition of the Tomita operator S (look into the paper) you can write this dense space as K+iK where K is the +1 eigenspace of S. K is real because S is anti-linear! The unusual aspect is the reality of K (which of cause in general have no bounded projectors), nowhere in QM you need to highlight real subspaces. In the case of O being the wedge region W, the polar decomposition of this S (see paper) leads to two operators with a known geometric and physical significance (see paper) which is related to to the K-spaces. As Mund has shown (see previous reference) one can also avoid the introduction of K's and work directly with the S(W)'s and their intersection. S is also intrinsically related to the wedge-localized subalgebra A(W). By forming algebraic intersections you can get to

compactly localized subalgebras. There are of course projectors inside these subalgebras, which behave completely covariantly but none of them has an associated probability interpretation i.e. they are completely consistent with Malaments No-Go theorem.
I think with these remarks I have led you .beyond the first hurdle; try to go the rest of the way yourself (and let me know when you get stuck for more than two days).
This theory is very very deep. For the first time it supplies the entrance into QFT "without classical crutches" which Jordan pleaded for in his 1929 Kharkov talk. But everything is very much at the beginning, there is a vast territory to understand and the expected light at the end of the tunnel would be a genuine intrinsic approach in which objects cannot be characterized any more by their Lagrangian names. The first glimpses of this new way of looking at QFT is supplied by chiral and factorizing theories. If you clinge to Lagrangians, forget it.
Sorry, nobody has planned the great abyss between the standard way and this new perspective, but as a theoretical physicist you have to follow the intrinsic logic of things and not any fashions of the day.

187. **Arun**
June 2, 2006

How does the Hollands-Wald paper tie in with Wilsonian renormalization group ideas, if at all? Presumably the "holistic aspects of quantum field theory" do not show up in effective field theories with a finite cut-off?

188. **Bert Schroer**
June 2, 2006

A very tough question, Arun, since the Wilson RG is in momentum space whereas for the formulation of the local covariance the space of localization space is essential. On top of this, there is the problem that the Wilson RG is formulated in the Euclidean setting where the integration over certain over subvariables (the thinning-out process) corresponds to abelian conditional expectations. On the other hand its real time analog has to face to deal with nonabelian conditional expectations.
There is a related very deep unsolved problem which is to study the relation between boundary problems in the Euclidean setting and the superselected charge sectors in real time local observables. From studies of 2-dim. conformal QFT one knows that there is a relation (the Cardy's Euclidean way and the Longo-Rehren real time setting). Once in the middle of the 90ies, I suggested this problem to Schomerus (he had the necessary conceptional and mathematica recourses), but that was at a time when he had to build his career. Of course usually after people have made it, and could afford to really deep problems they are usually already very much attached to those problems which brought them there.
From Urs formulation I have the impression that this has something to do with that turned down research project of the AQFT Hamburg group, but since I lost the connection with that group, I am not sure.

189. **Eugene Stefanovich**
June 2, 2006

Now the difference between our approaches is clear to me. You insist on manifest covariance (= exact, interaction-independent tensor transformations of observables, = Minkowski space-time) and allow bending of the rules of quantum mechanics.
I adhere to strict QM and relativistic invariance (= Poincare group properties) and I am willing to sacrifice the manifest covariance. This is in
http://www.arxiv.org/physics/0504062

190. **Bert Schroer**
June 2, 2006

Not quite, Eugene, not with the N-W localization, in that case I would only insist in asymptotic covariance. If this would not be possible there would be no invariant S-matrix. But in the case of finite distances where I have to use the covariant localization (achieved by modular theory) I am not forced to use any covariant tensor formalism. The only thing I have to pay attention to is that when I have operators which I suspect to be localized in a spacetime region O, the application of the Lorentz transformation should lead to operators which are localized in the L-transformed region. And this is precisely where the Born localization fails: if something is N-W (quantum mechanically) localized in e.g. a sphere at a given time, then its L-transform is noncompactly spread all over the place although this spread is numerically small at large distances. But there is no uniform controll of this!!
But all of these "superluminal" effect are irrelevant in the asymptotic calculation of the S-matrix, they do not leave any on-shell effect. But do not use N-W (QM) for any other purpose than scattering theory.

191. **Eugene Stefanovich**
June 2, 2006

Bert,

The only thing I have to pay attention to is that when I have operators which I suspect to be localized in a spacetime region O, the application of the Lorentz transformation should lead to operators which are localized in the L-transformed region.

That's exactly what I meant by "manifest covariance"

And this is precisely where the Born localization fails: if something is N-W (quantum mechanically) localized in e.g. a sphere at a given time, then its L-transform is noncompactly spread all over the place although this spread is numerically small at large distances.

I see that you are trying to avoid the Hegerfeldt's paradox at all cost. Even at the cost of abandoning the laws of quantum mechanics. As I wrote earlier, I don't consider this "superluminal spreading" to be that dangerous. If you take into account the probabilistic nature of measurement, this curious effect becomes rather harmless, and it doesn't contradict causality. There is no way one can build a time machine or kill his grandfather by using this effect.

192. **Bert Schroer**
June 2, 2006

Eugene
It would be harmless if you would have a uniform control, but you havn't. Your view gose against Fermi's conclusion from his two-atomic Gedankenexperiment. His way of arguing may be mathematically doubtful (people at that time compensated the more feeble mathematics by stronger guts-feelings) but nobody wants to resurrect Hegerfeldt's paper, not even the author himself. The numerical argument is not enough, you need uniformity otherwise you mav be able to think about a sequence of modified Fermi-like Gedankenexperiments where the effect becomes so large that even you would not want to see the resulting acausality in your life (a time-machine sequence). Lack of uniformity does not permit you to use the word "small" inasmuch as one cannot say that a women is a little bit pregnant. And by the way, all the reported experimental verifications of superluminality turned out to be poltergeist effects

193. **Eugene Stefanovich**
June 2, 2006

Bert,

I didn't mean that the effect is harmless because it is small. In my opinion, it would be harmless even if it were big. I agree, it is strange that different observers disagree on whether the electron is localized or not. But I still don't see how one can build a time machine out of this, even assuming that this effect is amplified to the macroscopic scale. If you have a concrete proposal how to build a causality-violating machine using the "superluminal spreading", then I am ready to abandon quantum mechanics and the Newton-Wigner position operator and join the AQFT research.

194. **Bert Schroer**
June 2, 2006

Eugene,
I frankly do not understand why you have this psychosomatic attachment to an anti-Fermi (pro superluminal) formalism against all common sense (it does not only violate experience, but you also forego any chance to understand the quantum version of the Einstein local covariance principle) and you do get nothing in return. I tried to convince you the quantum mechanical particle interpretation is asymptotically valid in QFT, but you insist to use it outside its domain of its validity. I have reached the end of my possibilities; I cannot stop travellers who go into such a strange direction. In any case I hope at least that I convinced you that you do not have to do this, there is nothing in particle physics which forces you

195. **Eugene Stefanovich**
June 3, 2006

Bert,

thank you for trying to convince me. It was great that Peter was distracted by discussions of his NEW book (congratulations, Peter!) and allowed us to have this totally off-topic exchange. I think it is perfectly OK that we travel in different directions. As long as we stay honest to our core beliefs and respectful to each other we have a chance to find the truth somewhere down the road.

Regarding some of your comments.
1. I don't know a single experiment which directly measures the speed of propagation of interparticle interactions (not to be confused with the speed of propagation of light). So, it is too early to tell that action-at-a-distance *violates experience* . Superluminal effects were seen by experimenters in Koln, Florence, Berkeley, and a number of other physical laboratories around the world, not just in poltergeist movies.

2. I agree that my approach is not compatible with general relativity. There are hundreds of theorist who try to *understand the quantum version of the Einstein local covariance principle* . Did they move an inch closer to this understanding in the last few decades? I just don't think that this road leads to quantum gravity.

3. You asked what I get in return for my stubborn adherence to particle-based quantum mechanics and the principle of relativity (= Poincare group). Most importantly, I have a finite Hamiltonian of QED that can be used not only for usual S-matrix calculations, but also for the time evolution and bound state problems. All integrals are convergent, and there is no need for renormalization or cutoffs. This Hamiltonian incorporates all radiative corrections: the Lamb shifts and what have you… The interaction is written explicitly in the 2nd perturbation order. For higher order terms, there is a well-defined algorithm how to get them and rigorous theorems proving that what I am saying is actually true.

196. **Chris Oakley**
June 3, 2006

Just to chime in here: it is not clear to me what "causality" actually means at the quantum level. If we take

∂2Aμ(x)= jμ(x)

in classical electodynamics we may solve to obtain an "advanced" and "retarded" solution. The "retarded" solution expresses the principle of causality in that the amplitude of the four-current may not be known to observers until the time it takes a light ray to reach them has elapsed. This I can understand.
In QFT a field has a positive-energy part, which creates particles, and a negative-energy part which annihilates them.
Thus
ϕ(x)|0>
represents a particle at position **x** and time x0, but then so does the convolution
∫d4x' C(x-x') ϕ(x')|0>
where
C(x) =∫d4p eip.x θ(p0)
which is a non-local smearing that filters out the positive-energy part, so although it seems clear to me that fields need to commute or anticommute for spacelike intervals, if only because of the spin-statistics connection, the connection with causality, or even the definition of causality at the microscopic level remains – to me – unclear.

197. **Bert Schroer**
June 3, 2006

Eugene,
the local covariance principle is a principle in the setting of QFT in curved spacetime (black holes, Hawking radiation etc.) and not in a still illusive QG.
When I said that you are in the illustrous company of Weinberg I meant this in a metaphorical sence and I certainly did not want you to take this literally and feel encouraged by this. His extreme use of a quantum mechanical argument in the calculation of the vacuum polarization contribution to the cosmological constant is a blooper in is otherwise impeccable ouvre. He has never ever used such a picture for the calculation of Lambshift in his book (which really is an evergreen), whereas your quantum mechanical extremism would force you to do a new computation, of which none of us can imagine of how it would look like.
I am sad, but o.k. as long as you never tell anybody that you talked to me, go in God's name.

198. **Bert Schroer**
June 3, 2006

Oh no Chris, not again.

199. **Chris Oakley**
June 3, 2006

Sorry if I'm boring you, Bert, but I've seen spacelike commutativity/anticommutativity called the "causality" statement on more than one occasion. No doubt AQFT folks would never be so sloppy, in which case, good for them … and why does this ignore <sup> and <sub>? Maybe I should try [tex] … [tex]
\int d^4x' C(x-x') \phi(x') \vert 0\rangle
[/tex]

WordPress Upgrade

May 23, 2006
Categories: Uncategorized

Just upgraded to a new version (2.0.2) of WordPress, and as far as I can tell everything is working normally again. If not, let me know. The one change I plan to make here soon is to try out some of the new anti-spam features. The ones in the older version of WordPress were much better at rejecting valid comments than identifying spam, we'll see about the new version…

Comments

1. **Lubos Motl**
   May 23, 2006

   Your blog is still malfunctioning. For example, the title should only say "not wrong" instead of "not even wrong". Good luck with 2.0.3.

2. **Kasper Olsen**
   May 24, 2006

   Good! Your fancy collider-background is up and running again! (Thought we would never see it again 😉

   Cheers, Kasper

3. **MathPhys**
   May 24, 2006

   Peter,

   Is there an easy way that you can change things, so that when we click on links, they open in a new window?

   That would be very convenient, given that you provide us with very good links on a regular basis.

4. **Alejandro Rivero**
   May 24, 2006

   MathPhys, that is in the user side. You press the link with the right button of the mouse and then you gent the "open in a new window" command. If you are in a mac, and then you do not have right button, then you should press the modifier key in the left side of the keyboard and then click the mouse, the result is the same.

5. **MathPhys**
   May 24, 2006

   Alejandro,

   Thank you so much.

6. **The Anti-Lubos**
   May 27, 2006

   Lubos, I usually only think of you as a psychologically unstable, mean-spirited crackpot, but even I laughed at your comment. Nicely done, even though you're insane and evil. 😉 😉 😉 😉 😉

7. **Nigel Cook**
   May 27, 2006

   Lubos writes (in reaction to a post on Cosmic Variance):

   'A theory is the best result of a scientific or rational analysis of the world you can dream about.'

   http://motls.blogspot.com/2006/05/clifford-johnson-and-word-theory.html

   String theory is 'not wrong' because – judging by the number of citations to string theory – it is simply the BEST RESULT there is. It would be nice if string theory made contact with reality, but this isn't really important anymore. As Lubos says, string theory helps you DREAM, and sleep soundly without worrying about reality.

8. **Juan R.**
   May 27, 2006

   More stringy hype

   Scientists Predict How to Detect a Fourth Dimension of Space

   Juan R.

   Center for CANONICAL |SCIENCE)

String Theory Makes Prediction – Pig Grows Wings

May 27, 2006
Categories: This Week's Hype

Several people have pointed out to me the latest press release hyping the supposed testability of an extremely speculative theoretical idea, which then gets promoted to a “scientists finally find way to test string theory” story, and spread throughout the popular press.

This week’s hype example comes from Duke University and is entitled [Scientists Predict How to Detect a Fourth Dimension of Space](). It deals with a [recent paper]() by Keeton and Petters, one in an interesting series of papers about using gravitational lensing to test GR. This latest paper deals with possible lensing effects of primordial black holes in braneworld models. The hype isn’t really in the paper itself, but in the press release, where Petters says “If braneworld black holes form even 1 percent of the dark matter in our part of the galaxy — a cautious assumption — there should be several thousand braneworld black holes in our solar system.” Braneworld scenarios can have any energy scale one wants, and the only thing one knows about this is that it can’t be below a TeV or so, because otherwise we’d have some evidence from accelerators for these scenarios, and we don’t have a shred of such evidence. I just don’t see any justification for calling the idea that 1% of dark matter is made up of these black holes a “cautious assumption” about what a braneworld scenario would “predict”, or for claiming that they have a testable “prediction” for what the GLAST satellite will see, in any conventional scientific use of the word “prediction”.

As usual, the hype level increases as the story is reworked into popular science stories elsewhere. For instance, at Ars Technica it is the inspiration for an article called [String theory makes prediction – pig grows wings](). The writer begins by giving a completely incorrect explanation of why string theory has made no experimentally verifiable predictions to date:

because the governing equations which work so well at very small scales (and very high energies) become impossible to solve when applied at lower energy or larger scales. Thus theorists must make approximations, which then have another layer of approximation applied before any measurable numbers fall out. At this point everything falls apart because that second layer of approximation is governed by existing experimental results, *which means that no new predictions are made.*

this repeats the usual misleading claim of string theorists that they have “equations which work so well at very small scales”, and the only problem is that it is hard to extract physics at long distance scales from these equations. I have no idea what this “second layer of approximation” is, best guess is that it is the “approximation” of assuming that string theory gives you the standard model at low energies, an “approximation” that does kind of make it hard to extract predictions that disagree with the standard model.

The writer also refers to another bogus “prediction of string theory” he attributes to Ulf Danielsson, and concludes that “now we have some real testable predictions from a theory of gravity derived (not in the mathematical sense) from string theory.” The parenthetical remark at least gives some indication that something very fishy is going on, and he does end the piece by pointing out that:

A word of caution should be attached at this point. Braneworld gravity is one of a number of string theory derived candidates, so if braneworld fails don’t expect it to take string theory down with it.

This particular piece of hype so far has been uncritically repeated at various places, including [here]() and [here](), from there making it onto endless blogs, such as [here](), [here](), [here](), and [here]().

Update: As usual, picked up by [New Scientist]().

Comments

1. **Who**
   May 27, 2006

   maybe this is simply a question of terminology. A pig with feathers is a turkey, and if it sits up in a tree it’s called a superturkey.

2. **Knotted String**
   May 27, 2006

   The squadrons of flying pigs (turkeys) that string theorists are so good at breeding will hopefully deposit some manure on the Landscape, and allow the growth of new ideas!

3. **Dan**
   May 28, 2006

   Dear Peter,

   as a particle physicist, do you think John Baez, Lee Smolin, Markapolou’s approach to standard model physics through LQG through “loop braids” is promising?

   http://math.ucr.edu/home/baez/this.week.html

4. **Who**
   May 29, 2006

   Dan, I realize you asked a specific question of Peter (and not of the gallery) but would like to respond.
   I am glad to see you making an effort to promote real discussion—I regret my flippant comment earlier about the pig with feathers because it seems to have precluded seriousness.

   Since Peter’s post is about TESTABILITY, and you mention Smolin’s stuff (which may be very different from Baez’—I am not sure about the connection yet) I will mention a case of **quick falsifiability** that Smolin mentioned on page 18 of his recent hep-th/0605052

   …The discovery that these theories generically predict emergent particle states certainly leaves them vulnerable to quick falsification. While there is preliminary evidence that a large class of theories can reproduce some features of the standard model, there is a lot that these theories have to get right so as not to disagree with observation…

   What he was talking about in that passage is, I think, what you were referring to. So there is a testability angle to it although it is a different pig. (maybe not a pig, either, but a kind of unicorn, or platypus)

5. **woit**

May 29, 2006

Dan,

Sorry, but that work is for now in the category of intriguing things I just don't have time to seriously study, so I can't intelligently comment on it.

6. **Christine Dantas**
May 30, 2006

Concerning the "crisis of predictability" of string theory, Lee Smolin will be giving a talk in Paris, sorry if it is off-topic.

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Mercredi 14 juin à 14h30

Lee Smolin (Perimeter Institute, Canada)

"Against Symmetry"

From the Leibniz-Newton debates to the present debates between string theorists and loop quantum gravity theorists two notions of fundamental physics have stood opposed. On the one hand is the Newtonian vision, which is based on a belief in a fixed absolute space and time and which holds symmetry to be fundamental and its breaking to be contingent. Leibniz's conception of space and time is instead of an ever evolving network of relationships in which complexity is fundamental and symmetry is unnatural and accidental. This distinction characterizes the divide between background dependent theories like string theory and relational, background independent theories such as loop quantum gravity.

In this talk I analyze the present status of the two traditions and the plausibility of their contemporary incarnations. I show that the crisis of predictability facing string theory is a direct consequence of a conception of unification that is opposed to the principles that underlie the successful modern unifications such as general relativity and gauge theories. I close by describing a new kind of unification which emerges from background independent theories.

Amphithéâtre A, entrée par le 25 rue de la Montagne Sainte-Geneviève, Paris 5ème

7. **Bert Schroer**
May 30, 2006

Very interesting Christine,
in QFT such "monade presentations" already exist. An early account of how to build up the reality of QFT (up to 4-dim) in this Leibniz monade spirit, including the spacetime symmetries, the net structure (localization) and (by doing the DHR analysis) the entire inner symmetry structure, can be found in H. W. Wiesbrock and R. Kaehler, Modular theory and the reconstruction of 4-dim. QFT, JMP 42, (2001) 74 and previous literature (this was the last paper this incredible original talent W wrote, after he had developed most of the necessary modular inclusion and modular intersection concepts, and shortly before he had to leave academia because in a world of string hegemony there is no place for this kind of people, he did not even bother to post this paper onto the server).
I have used part of such a monade picture for a number of years (it pervades all of my recent articles and let me to a formula for localization entropy in
http://www.lqp.uni-goettingen.de/papers/06/04/
which is consistent with the new local covariance principle in AQFT).
The problem to reconstruct the generic curved space-time structure together with quantum matter on it is much more complex. I have tried to build up the diffeomorphisms of the circle from the relative position of monades (i.e. without using the existence of an energy-momentum tensor, but the results are still very incomplete, viz.
hep-th/0504206 published in AOP) associated to it.
It seems to me that a program as that of Lee Smolin is very ambitious; maybe one should more modestly understand the relation of quantum physics and monade positioning first in QFT in CST.
I should add that this relative positioning picture was also on the minds of Brunetti and Fredenhagen when they formulated the new quantum local covariance principle, and last not least, without it Einstein would not have succeeded in his struggle with the fantasmas against covariance he himself created with his "hole argument."

8. **Christine Dantas**
June 1, 2006

Dear Bert Schroer,

Thank you. I am ignorant on monades and several other issues. I am stubbornly following with great interest your contributions to this blog, but I must say a lot of material here is a little over my head. However, it is admirable to learn that are still people in this world interested in discussing physics seriously. I am tired of so much degradation elsewhere.

Best wishes,
Christine

9. **Christine Dantas**
June 1, 2006

Concerning the question of the multiverse, it is a relief to see a paper on this by serious enough people:

How Many Universes Do There Need To Be?
[http://arXiv.org/abs/astro-ph/0605709]

Christine

10. **Bert Schroer**
June 1, 2006

Dear Christine,
I am sure that my thinking about string theory did not escape your attention.
Unless you are an active participant with the "string feel" (i.e. you are with it), it is difficult to understand this phenomenon because it is not scientific, it is rather a sociological phenomenon which takes place where one least expects it, namely in the center of particle physics.
That irrational things suddenly spread in the midst of rationality is a postmodern phenomenon whose study is the heart piece of the Frankfurt school of philosophy/sociology. Horkheimer, Adorno and Habermas of course were thinking of right arm-raising crowds shouting Sieg Heil in the midst of a culturally and civilisatorically most developed country and about the catastrophic barbaric consequences.
I must confess that in total lack of any scientific explanation, I also think about this strange grip of mass hype and medial manipulation emanating from the first globalized

attempt to get a hegemonial grip on science by manufacturing and not logically deriving scientific facts (fortunately only with a loss of knowledge and not human lifes) in such sociological terms. I really do think about those collective excitations at the time of my birth which I sometimes see in old films and which leave me stunned and bewildered when I look at the new scientific hype. But perhaps it is better to stop with these analogies. otherwise somebody might tomorrow claim that I compared Joseph Polchinski to Adolph Hitler and Aaron Bergman to his stormtrooper, and I may loose my pension in Germany. I have a Brechtian (the identity of the first names is purely coincidental) personality which does not ask for self-sacrifices but only to subterfuge hollow and oppressive systems.
Let me add that I am totally against racism and political correctness, this is part of the reason I prefer to live in Brazil (by the way I think that such a catastrophy as in Nazi Germany could never have happen in Brazil, even the ethnic cleansing of the Paraguaian indigenous population by your Duke de Caxias is quite a distance away from what happened at the time of the 2. worldwar).
I would suggest to those physicists who's future is unsure (and to those string theorist who already foster doubts in their heart) to take in addition some courses in the philosophy/history/sociology of science, because if things within the next couple of years develop the way as with Bush's Iraq war (also based on maniufactured facts), there will be a lot of interest in the public to understand what happened in the midst of physics and why those things which Michio Kaku, Lenny Susskind and Brian Green were writing about (and telling the US congress) (and all the other popular string theory writers which still appear on the scene) and promised them evaporated in hot air and left such a big crater in the midst of science. This phenomenon will need very thoughtful profound analysis (it is the Zeitgeist which allowed these individuals to do something like this), maybe even a kind of new Frankfurt school geared towards science.
Actually I am already corresponding with such a Jekill&Hide character who comes from the midst of string theory and who got extremely exited about my polemic article. Since he has all the insider knowledge and an amazing overview about the immense string literature, he can immediatetly back up his analysis by explicit citations and this is much deeper (and not polemic) to what outsiders as Hedrich or myself (the polemic part) can do.

11. **Chris Oakley**
June 1, 2006

String Theorists have not yet been defeated (not politically, anyway), and it is not they, but their critics like yourself who are fleeing to South America.

So much for the Nazi analogy.

But they definitely have the "Weltmacht oder Tod" mentality.

12. **Bert Schroer**
June 1, 2006

Chris, things were not quite as bad in Europe, at least at the time when I went to Brazil some years ago. However recently they seem to have taken a turn for the worse, at least in Germany. It is through this weblog that I got to know that one of that new breed of string-possessed referees (a person who knows what particle physics is about and still kept his senses would never do anything like this) deciding the scientific merits of a research project (probably to be funded by the DFG) has thrown out the extremely successful AQFT part and kept the string component (whose contribution has been smaller than any preassigned epsilon, without any hope for future improvements). People like Robert Wald and those who have collaborated with him will immediately understand the devastating significance of this act.
Chris, it is not Weltmacht oder Tod, it is hegemony and death, but death for the others.

13. **Trevor Turton**
June 10, 2006

It's fascinating to see such strong divergence amongst physicists on whether string theory is science, maths, or a fashion statement.

"String theory is the new black"

Accelerator News

May 27, 2006
Categories: Uncategorized

News about various colliders of various vintages:

The Tevatron has been shut down for the past two months for maintenance and various improvements. It should start up again this coming week, for latest news on its status, see here.

Seed magazine is starting a series of articles on the LHC. One prediction about the LHC that I feel confident making is that it is going to get a lot of press coverage.

A group called the LHC Theory Initiative has been trying for a while to get the NSF to fund new postdocs and graduate student fellowships for physicists working on LHC phenomenology. So far they have been turned down, with the panel that recommended not to fund this presumably concerned that money going to this purpose would be taken away from the standard NSF group grants that fund particle theory groups at many institutions. The full NSF proposal is available on-line.

Science magazine has an article about Barry Barish, who is leading the Global Design Effort for the ILC.

The same issue of Science has a paper by Steinhardt and Turok promoting their cyclic cosmological model as explaining the small value of the cosmological constant, together with an article by Vilenkin criticizing them and promoting the anthropic point of view.

Update: Experimentalist Michael Schmitt, sometimes commenter here, has an excellent new blog about accelerator-based particle physics at the Tevatron and LHC entitled Collider Blog.

Update: The Tevatron start-up was going smoothly until early yesterday morning when they ran into serious trouble. Here's the report from the FNAL accelerator update page:

At 1:24 AM, Operations reported a raccoon attack on the Linac gallery. It seemed to be a coordinated effort. Fortunately, by 1:53 AM, a joint force of operators and Pbar experts managed to drive the raccoons out of their hastily made fortifications. Then at 4:18 AM, the raccoons made what some thought to be a counter attack on the Division Headquarters, but others believed it to be only a simple reconnaissance incursion. No raccoons were either injured or captured during these encounters. Operator losses were low.

Comments

1. **Ponderer of Things**
   May 28, 2006

   Barish came to our lab to give a talk about ILC, and I wanted to ask a question, but didn't because didn't want to appear to be an a$$hole:

   "If ILC is not funded (at least if US gov't pulls out), is this the end of the line for huge "super" accelerator projects"?

   Anyone wants to take any guesses?

2. **Mechanism of Standard Model particle masses, exchange forces and General Relativity**
   May 28, 2006

   […] Woit has a blog post which says in part: '… Science has a paper by Steinhardt and Turok promoting their cyclic cosmological model as explaining the small value of the cosmological constant, together with an article by Vilenkin criticizing them and promoting the anthropic point of view.' […]

3. **sunderpeeche**
   May 28, 2006

   I have asked this question (what if US does not fund ILC?), to people who work on ILC. Nobody will give a clear answer. One can argue nobody knows the answer. Certainly the EPP2010 report, full of "US leadership" does not augur well for "International" anything. The rest of the world may build ILC without USA. If super-accelerators (VLHC?) continue to not find anything beyond SM, it will be ever harder to justify new machines.

   As for the prediction that LHC will generate a lot of press coverage, that is par for the course. All large new accelerators do, whether built or not. CERN generated enormous publicity for LEP. The SSC was not lacking for publicity. The construction of NAL (later FNAL) was a major event in its day, so was the SLAC linac.

4. **Michael**
   May 28, 2006

   If the LHC finds nothing beyond the Standard Model (ie, if it finds only a SM-like Higgs boson), then one can forget about future accelerators, including the ILC. The interest of people outside high-energy physics for another big, expensive machine will whither, and many will ask: what about all the great ideas and promises that were made (for example, in the EPP2010 and HEPAP documents) back in 2005, 2006, 2007?

   There are many basic arguments why new phyisc should show up at the TeV scale, and personally I believe thi s makes the LHC a good bet. However, I have seen comments posted to this web site expressing doubt that supersymmetry or other signs of physics BSM will be discovered at the LHC. Since string theorists are deep thinkers, this has worried me a lot. Yesterday at a dinner I asked David Gross about the skepticism of some of the string community about the LHC. His reply was, basically, that the arguments for new physics at the TeV scale are incontrovertible and that people in the string theory community do not have a basis to overturn them. (Nota bene: I am rephrasing quite a bit…)

   What will be the answer? Only the analysis of real data will tell us. If Nature is cruel, then we will not only be disappointed, we will probably lose an entire line of basic inquiry. Who would fund Colombus for a crazy voyage across the ocean? Only those will lots of money and the desire to be the most powerful. Who would have guessed what was to be learned?

5. **woit**
   May 28, 2006

   Michael,

   What "new physics at the TeV scale" was Gross referring to as there being incontrovertible arguments for? As far as I know, the only solid arguments are that you have to see a Higgs or something else. Things like supersymmetry, technicolor, extra dimensions, not only are not necessary, but already in trouble with precision electroweak measurements from current accelerators.

Sunderpeeche,

My prediction was somewhat tongue in cheek, but more precisely, in units where the publicity for SLAC was 1, FNAL was maybe as much as 5-10 SLACS, LEP and the SSC around 50 SLACS, I'll predict that for the LHC the magnitude will be at least 100 SLACS, achieving a new world record.

6. **sunderpeeche**
May 28, 2006

"My prediction was somewhat tongue in cheek" well yes, I figured as much. Columbus was funded because Queen Isabella wanted to compete with Portugal (and note that Cristoforo Colombo was initially turned down, but Queen I changed her mind). If Columbus had sailed across the ocean, found nothing, turned back and said, "I didn't reach China/Japan, but I found out the ocean is bigger than we think it is" well that would probably have been the end of any further voyages. So it is with the LHC. If it finds no physics beyond SM, justifying the next machine will become REALLY hard.

7. **Tony Smith**
May 28, 2006

As to the Columbus analogy:

Short route to India/China/Japan = supersymmetry a la superstrings

America = something already indicated by Viking voyages etc = SM Higgs

Maybe detailed study of SM Higgs (for which ILC/NLC/whateverLC is needed in addition to LHC) would (as did detailed exploration/exploitation of America) lead to results far more ultimately enriching for humanity
than finding a false-hope supersymmetry = a short route to India/China/Japan would have been.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – As to "… David Gross …[said]… that the arguments for new physics at the TeV scale are incontrovertible and that people in the string theory community do not have a basis to overturn them. …",
that statement may be true if restricted to the universe of superstring true believers.
However, for others,
it seems that the only "incontrovertible" arguments are circular, in that they use assumptions (whether or not explicitly stated) that are equivalent to the existence of TeV scale supersymmetry a la superstrings.
If there are any such "incontrovertible" arguments that do not involve such assumptions, I would be very interested in seeing them spelled out, rather than mentioned solely by vague insubstantial reference.

8. **sunderpeeche**
May 28, 2006

This post is not (totally) tongue in cheek, and is only partially a reply to Woit, and is perhaps totally nonsense. The construction and operation of FNAL (late 60's-early 70's) provided a major source of employment for professional black people, at a front-rank scientific institution, and took place at (or close) to the peak of the civil rights movement. We tend to think of accelerator labs as purely civilian projects., e.g. Robert Wilson famously testified to Congress (this is taken from Wikipedia but I have heard it many times before)

—-
In 1967 he took a leave of absence from Cornell to assume directorship of the not-yet-created National Accelerator Laboratory which was to create the largest particle accelerator of its day at Batavia, Illinois. In 1969, Wilson was called to justify the multimillion-dollar machine to the Congressional Joint Committee on Atomic Energy. Bucking the trend of the day, Wilson emphasized it had nothing at all to do with national security, rather:

It has only to do with the respect with which we regard one another, the dignity of men, our love of culture. It has to do with: Are we good painters, good sculptors, great poets? I mean all the things we really venerate in our country and are patriotic about. It has nothing to do directly with defending our country except to make it worth defending.
—–

It was not quite the same with the SLAC linac. At the time of construction of the Linac, the US military was building the North American Early Warning Defense System, a set of radar domes spread out across Alaska and northern Canada, to detect Soviet bombers/missiles. The klystrons to accelerate the electrons in the Linac were precisely what the military needed to power the radar stations. Nobody talks about this out loud. The military got what it wanted (R+D and contruction knowhow) without any protests from the hippies and flower children of San Fran.

One longs for the good old days when one could trip through the physics lab with bellbottoms and LSD. No internet, no cell phones, no blogs…

In short, major accelerator projects are sociological events, not simply physics experiments. But the publicity for the modern machines seems to contain a much greater degree of hype (all hype?). The SSC was "for America to have the biggest accelerator in the world" which was simply foolish.

9. **Michael**
May 28, 2006

We all seem to agree that no new physics at the LHC is a disaster for experimental high-energy physics.

Concerning the promise of new physics at the TeV scale, people make the argument in various ways. Since I am an experimenter I am not the best person to make the agument, but I do know that is does not necessarily involve supersymmetry. I do understand that string theory does not require a low-scale supersymmetry. However, that is not the point. If there is a SM-like Higgs (one that is responsible for electroweak symmetry breaking), then the mass of that particle is not stable against radiative corrections, in the Standard Model. Therefore, something beyond the SM must enter to "control" those corrections. Low-scale SUSY is one popular possibility, but there are others (such as Little Higgs models, and some extra-dimensional phenomenlogical models). If the low-scale MSSM also provides a way to unify the gauge couplings, explain the Higgs mechanism through the large Yukawa coupling of the top quark, and naturally provide a viable dark-matter candidate, then it may be in a better position as a speculation than other competing models. There is no proof that low-scale SUSY is right, but all of the phenomenologists and model-builders I know insist that new physics must enter at the TeV scale. It was interesting to me that David Gross, a famous string theorist, had the same definite opinion.

It is true that some models are ruled out by precision electroweak data, but the MSSM is not. Also, little Higgs models and many extra-dimensional models manage to survive those constraints. It is supposedly a virtue of the MSSM that it does not lead to large deviations in those observables – the fact that data confirm the SM means they also tend to agree with the predictions of the MSSM, making it difficult to constrain the MSSM using precision ewk observables.

The MSSM does have problems in the flavor sector: why aren't there many CP-violating phases, and why aren't there large FCNC's? Why would one have an alignment of flavor-mixing matrices, or minimial flavor violation in general. But then again, we understand almost nothing about flavor, right?

10. **Tony Smith**
May 28, 2006

sunderpeeche said "… major accelerator projects are sociological events, not simply physics experiments …
The construction and operation of FNAL (late 60's-early 70's) provided a major source of employment for professional black people, at a front-rank scientific institution, and took place at (or close) to the peak of the civil rights movement. …".

IIRC, Georgia, which in the 60s had senatorial influence (Russell) at least as powerful as Dirksen of Illinois, made a bid for the NAL site, but lost out to Illinois primarily because the "professional black people" felt uneasy about living in Georgia (this was before 1973 when Maynard Jackson became mayor of Atlanta).

When I try to think what comparably important sociological reason might be advanced for putting ILC in the USA as opposed to Geneva or Asia, I draw a blank.

Further, when I try to think how the USA military would benefit from a USA site (a la SLAC klystrons mentioned by sunderpeeche), I also draw a blank.

If access to research positions and experimental results is substantially non-discriminatory, then, really, why does the USA physics community feel so strongly that the ILC should not be in Geneva (a very hospitable place for a cosmopolitan work community) or Asia (as they say, it is their turn now) ???

Isn't the most important thing that it be built and used to at least examine Higgs etc in detail,
and not where it is located (and who gets the most construction contracts and labor jobs) ???

If it is REALLY about who gets construction contracts and jobs, then isn't it just another pork-barrel project ???

Isn't perception of pork-barrelling one of the factors that killed SSC ???

Tony Smith
http://www.valdostamuseum.org/hamsmith/

11. **Tony Smith**
May 28, 2006

Michael says "… We all seem to agree that no new physics at the LHC is a disaster for experimental high-energy physics. …".

Since nobody can be everybody without me (even if Harvard Professor Motl's characterization of me as a "moronic crackpot" were to be correct), Michael is wrong.

I don't see any problem with a 115-200 MeV Higgs and no other signals not already seen (although maybe missed by most consensus analyses) at Fermilab and elsewhere, because
ILC (or its equivalent) would in that event be needed to provide more detailed observation of how that Higgs is related to such things as (to mention only one possibility) T-quark condensates.

It would NOT be "a disaster for experimental high-energy physics", although it would take such physics down roads not so well travelled now.
However,
it might be seen as "a disaster" from the viewpoint of some physics empires/castles that would, in such an event, be seen to be built on sand.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

12. **Tony Smith**
May 28, 2006

Sorry for a typo – 115-200 MeV Higgs should be 115-200 GeV Higgs in my immediately preceding comment.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

13. **SomeBody**
May 28, 2006

LHC = Last Hope Collider?

14. **sunderpeeche**
May 28, 2006

Since my post on the SLAC klystrons did not get deleted out of hand, let me provoke the Gods that be…. FNAL provided high-skilled job opportunities for blacks/minorities at the cutting edge. (I assume the Apollo moon project did the same, but I do not know.) The Chicago Industrial Research Corridor was set up along I-80 near FNAL in anticipation that the lab would attract high-quality (or high-tech) research firms to the area. (In the event, Fermilab was too specialized to play such a role, firms like AT&T Bell Labs and Exxon etc were more effective in that regard.)

Pork-barrelling does play a significant role. It makes a difference if a lab is within easy reach. It also provides ancillary employment to hotels, restaurants, etc, (although now people complain of "radioactivity"). When the BNL was being chosen, II Rabi wanted it in New Jersey, close to Columbia because he knew the nearest university would dominate the lab. Norman Ramsey led a strong team from Harvard/Cambridge to have the lab near Boston. So Rabi signed on Johns Hopkins just to move the center-of-gravity of the universities closer to Columbia. The eventual site on Long Island was the compromise.

The host country of the ILC will get major (possibly non-scientific) benefits. Certainly the size of expts today is so large that proximity to a home univ is not so significant. But it seems that with the ILC, pork-barreling is getting mixed up with jingoism (ILC will restore "US leadership" in expt HEP).

15. **Michael**
May 28, 2006

Tony, what I meant is that a non-spectacular outcome from the LHC means that getting funding for the ILC will be extremely difficult and probably impossible. I was not commenting on the physics consequences. Sorry for not being clear.

16. **Tony Smith**
May 28, 2006

Michael, thanks for your clarification. You are probably correct that absence of some spectacular hype-type result at LHC probably does mean " that getting [USA] funding for the ILC will be extremely difficult and probably impossible". Sad but true.
Maybe Asians will want to take their turn in the accelerator game, and, since they are well-positioned financially and know how to think long-term, maybe they will build it and let those of us on the periphery peek in from time to time to see some of the knowledge generated at the Center.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

17. **anonymous**
May 29, 2006

for the first time since 30yrs a collider will explore energies much above previous colliders and above the weak scale: it deserves the attention that it receives.

Unlike in the case of Columbus/Colombo/Colon, now theorists are telling since 30yrs: "go and discover America" (i.e. SUSY or extra dimensions or etc). Finding only water would be truly revolutionary.

18. **RahulM**
May 29, 2006

The problem with a 115-200 GeV Higgs in the Standard Model is that it's mass is not stable under UV corrections. This violates a very basic physics principle we have seen at work from Newton till now: that physics is organized by energy scale. So far the structure we have observed is that phenomenon at lower energy scales are relatively unaffected by whatever happens at much higher scales. It is extremely strange that somehow the Higgs boson is completely different from all physics we have observed thus far. I am not propagating string theory or SUSY or extra-D or whatever, but there is a genuine TeV scale issue here.

19. **Tony Smith**
May 29, 2006

RahulM says: "… The problem with a 115-200 GeV Higgs in the Standard Model is that it's mass is not stable under UV corrections. … there is a genuine TeV scale issue here. …".

There is a genuine issue, but it does not necessarily indicate new physics at the TeV scale.

For instance, in hep-ph/0307138, C. D. Froggatt says in part:
"… the selfconsistency of the pure SM up to some physical cut-off scale /\ imposes constraints on the top quark and Higgs boson masses.
The first constraint is the so-called triviality bound …
The second is the vacuum stability bound: bound: the running Higgs coupling constant … should not become negative leading to the instability of the usual SM vacuum. …
These bounds are illustrated in … Figure 3: SM bounds in the (Mt,mH) plane for various values of /\ , the scale at which new physics enters …
we shall be interested in large cut-off scales /\ = 10^15 – 10^19 GeV, corresponding to the grand unified (GUT) or Planck scale. …".

As can be seen by going to the paper and looking at Fig. 3, a Higgs mass somewhere between 115 – 200 GeV (by looking at Fig. 3, probably roughly around 150 GeV) is consistent with a T-quark mass around 175 GeV and the Standard Model with a cut-off (new physics) at the Planck scale,
so
it seems to me that such a Higgs mass does NOT require new physics at the TeV scale, or anywhere else below the Planck scale.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Here I am assuming that RahulM's reference to instability under UV corrections is reference to what Froggatt calls "the vacuum stability bound". If RahulM is talking about something else, then I am willing to stand corrected.

PPS – I am NOT saying that the ILC would be useless in such a case. For instance, it would be very interesting to try to explore in detail the Higgs – T-quark system in regions near the boundaries in Froggatt's Fig. 3 to look for phenomena possibly involving triviality or vacuum instability, and the ILC in conjunction with LHC would be very useful in such investigation,
which is consistent with Froggatt's discussion of the possibility that "… that there exists another vacuum state degenerate in energy density with the vacuum in which we live. …". He goes on to say "… Thus we predict that our vacuum is barely stable and we just lie on the vacuum stability curve …".
Since such things are already indicated in data from Fermilab etc, they are not "new physics" in the sense of being new supersymmetric partner particles etc, but such things are (to me, at least) very interesting and justify building ILC to study them.

20. **Tommaso Dorigo**
May 29, 2006

I would concur with Michael that no new physics at the LHC is a disaster for experimental particle physics \em in the short run.

But I prefer to look further. I am not too concerned about HEP 10 or 15 years from now – I have tenure – but much more concerned with the progress of science -well ok let's say (physics \oplus astrophysics)- as a whole.

Now, IF the LHC sees nothing at all (say, not even a single Higgs) that is maybe motivation for canceling the ILC, but just as much motivation for a whole class of new theoreticians to come ahead stampeding the old ones, and produce intelligent new theories. Old ideas die with their supporters in science, just as much as new ones must see them old farts die before they flourish.

As for us experimentalists, a sabbatical to outer space to collect gamma rays would be in order, who knows, maybe it would do us good. The frontier, we all know, is out there now, not in the core of CMS and ATLAS.

I am not at all sure it would be a damage to the progress of science – we have been stuck with the same kind of stuff for too long IMO.

Cheers,
T.

21. **Michael**
May 29, 2006

I think the ILC should only be viewed as a follow-up to discoveries made at the LHC. If there is nothing more that a SM-like Higgs boson, then precision studies of its properties, while very interesting, will not be exciting enough to justify a $10-$20 billion machine. All public arguments for the ILC assume that one will also make precision measurements of whatever new physics is revealed by the LHC. If there nothing to be studied besides the Higgs, then we need to rethink our strategy.

If no SM-like Higgs boson is found and no new phyiscs either, then that would be quite interesting, and as far as I know one would be forced to study longitudinal W scattering, which one would certainly not do with an ILC.

I believe that the future of particle physics does lie at the centers of CMS and ATLAS (otherwise I would not be working there), and that even the amazing discoveries of astrophysics will require some particle physics input to be fully comprehended. We will soon see!

22. **Michael**
May 29, 2006

Thank you, Peter, for the very kind advertisement of Collider Blog!

23. **Alejandro Rivero**
May 29, 2006

Perhaps a composite higgs / technicolour is a better opportunity for the ILC, it would give a lot of parameter space to be searched, shouldnt it?

24. **sunderpeeche**
May 29, 2006

Surely that’s the whole point of building the LHC/ILC? To search and discover what’s really out there (composite Higgs/technicolour new particles …).

To boldly go where no hand has set foot?

25. **andy**
May 30, 2006

It seems to me that funding phenomenologists / collider theorists / model builders whose research has some relevance to experiment is a _good_ thing (rather than shoving more $$$ into the bottomless pit of string “theory”.

26. **sunderpeeche**
May 30, 2006

The corresponding argument that “the public” (somewhat vaguely defined here) makes is that it is a good thing to fund research which has an obvious immediate commercial profit. Nobody knows where this (~string theory/all of HEP) will all lead. The real problem is that Nature is cruel — expt HEP has not produced new puzzles (physics beyond SM) for 30 years.

27. **hack**
May 30, 2006

The raccoons are clearly in their last throes.

28. **sunderpeeche**
May 31, 2006

don’t get bit

http://www-bdnew.fnal.gov/operations/safety/critters.html

29. **knotted string**
May 31, 2006

Thanks for that last link about stagnating string theory at fermilab:

‘Objects that have remained undisturbed for an extended period of time may become housing for pests.’

30. **SomeBody**
May 31, 2006

With his usual candor, Dorigo wrote:
“I am not too concerned about HEP 10 or 15 years from now – I have tenure”

In other words, never mind whether I’ll be doing anything that’s of any use to anyone, the taxpayers are stuck supporting me anyway.

Welcome to the brave new world of HEP as social welfare.

31. **sunderpeeche**
May 31, 2006

Let’s present the full sentence, shall we?

But I prefer to look further. I am not too concerned about HEP 10 or 15 years from now – I have tenure – but much more concerned with the progress of science -well ok let’s say (physics \oplus astrophysics)- as a whole.

Read the second half before writing anything mean-spirited.

32. **Peter Woit**
May 31, 2006

SomeBody,

Dorigo has a job at a university in Italy, which I guess is now permanent, so, he has tenure like most people with long-term university jobs in Europe. He's not being paid by the US taxpayer, but is being paid (probably not very well..) by the Italian taxpayer to teach and conduct research at a university. His point was that the question of whether the US taxpayer is willing to pay for a new accelerator or otherwise spend large sums on HEP research doesn't affect his paycheck.

Would you prefer that he not have a permanent job at an Italian university, but have his paycheck depend on the health of the US HEP budget? If you want to rant about scientists on the government tit pushing for more money for HEP research to feather their own nests, that's fine, but Dorigo is just pointing out that that's not the category he's in.

33. **SomeBody**
May 31, 2006

What exactly is it about the second half that you think modifies the first half?

34. **SomeBody**
May 31, 2006

Woit, as in the ILC discussion, you seem to find it relevant whether taxpayers are in the US or elsewhere. I don't.

I know very well where Dorigo is, what he's doing and who is paying him to do it. There is nothing in his post supporting your interpretation that "His point was that the question of whether the US taxpayer is willing to pay for a new accelerator". His point is quite simply that no matter what happens to HEP, his own paycheck is secure (a conclusion which I do not share BTW, considering Italy's precarious financial situation).

In aswer to your questions:
"Would you prefer that he not have a permanent job at an Italian university"

That's correct.

"but have his paycheck depend on the health of the US HEP budget?"

Actually, whether he realizes it or not (apparently not) that is already the case. But as far as my personal preferences are concerned, the answer is no. I'd prefer his paycheck to depend on his performance in a productive activity where he's actually earning it.

35. **sunderpeeche**
May 31, 2006

It's hard (if not impossible) to think of the long-term if you have to worry constantly about feeding yourself in the short-term. That guarantee of employment (such as it is) is one of the features of tenure.

As they say in non-idiomatic non-Italian …. Ach du Lieber.

36. **Peter Woit**
May 31, 2006

SomeBody,

So, this has nothing to do with HEP, US or otherwise, but is just an ideological rant about the socialistic practice of tenure that isolates some academics from the healthy discipline of market forces.

Please, this is tedious, off-topic, and has nothing to do with what is being discussed here.

37. **SomeBody**
May 31, 2006

Woit, it has everything to do with HEP! Once upon a time, HEP was a vibrant source of new scientific knowledge which attracted the best and brightest. You need look no further than Dorigo's post to see who's attracted to it now, and why.

38. **Chris Oakley**
May 31, 2006

SomeBody,

I agree with Peter. Tommaso is probably being paid mostly for teaching, and at a rate lower than a schoolteacher. His research interests are probably tolerated by the funding authorities rather than encouraged. The luxury that you no doubt live in as a result of being a self-made billionaire he will have had to forego in exchange for doing what really interests him.

39. **Peter Woit**
May 31, 2006

SomeBody,

The academic tenure system you object to has been around for centuries, it has nothing to do with the current problems of HEP.

I don't know Dorigo personally, but from his blog posts he appears to be a highly competent, serious researcher who works hard at and cares about what he does. He also has a sense of humor, and knows that there's no incompatibility between doing serious work and not taking oneself too seriously.

40. **SomeBody**
May 31, 2006

Oakley: no. Dorigo is an INFN "researcher", not a university employee. He does what you do for a living (writes code) but of course, unlike you he needs tenure to be able to do it, or (as pointed out by sunderpeeche) he would no doubt be unable to "think of the long-term".

41. **sunderpeeche**

May 31, 2006

The LHC will give “something to do” (expt HEP) for approx 10-15 years. What comes next? The ILC is one answer. Modern expt HEP projects take so long now to come to fruition that one must start thinking years in advance. Who can do that? Not someone whose job contract is due to expire in a year or two.

As I learnt many years ago (biology class? it’s been a long time) we have two hands and two feet to count the four important things in life: food, food, s – – and food. It’s hard to think of the big picture if one has to scurry around all the time to find one’s next meal.

42. **Michael Schmitt**
May 31, 2006

Somebody,

I need to speak up for Tommaso. I know him quite well since several years. He is intelligent, creative, hard-working and very serious, facts which I’m sure are known to the INFN. He is not kept employed simply because he writes code, and I have the impression that INFN standards are higher than that. I didn’t like his comment either and posted my own response, but anyone who knows Tommaso know that he likes to make flippant remarks to provoke reactions. It may be annoying, but please lets not denigrate his strengths as a researcher nor use his remark as a sign of the weakness of HEP.

thanks.

43. **Peter Woit**
May 31, 2006

I’ve deleted the rest of this thread about Dorigo. Please refrain from any more obnoxious attacks on him here. He has his own blog, if you want to attack him for what he has to say, go do it there.

44. **Chris Oakley**
May 31, 2006

Superstrings are snake oil, says Sam from the UK.

45. **SomeBody**
May 31, 2006

Speaking of the devil, am I the last person on the planet to find out that Amazon is comarketing these two as “Better Together”?

http://www.amazon.com/gp/product/0618551050/ref=pd_bxgy_text_b/103-3759420-5756605?%5Fencoding=UTF8

46. **RahulM**
May 31, 2006

Hello, what I meant was that suppose there is a very heavy fermion field at some scale far removed from TeV. Say a GUT-ish fermion weighing 10^15 GeV. Then that would produce a huge correction to the Higgs mass, of the order of 10^15 GeV! Therefore the SM is sensitive to physics far beyond it’s own scale, which is counter to everything in physics we have seen thus far. All you need is a heavy fermion or a scalar lurking somewhere between 1 TeV and 10^19 GeV and the SM will “jump” like crazy. That is why we say some new physics is needed at the TeV scale to have a smooth transition between the SM and whatever happens at GUT/Planck/whatever scale when physics gets unified.

47. **Not a Nobel Laureate**
May 31, 2006

Modern day HEP experiments are similar to the construction of the pyramids of ancient Egypt
in that

1. They are now multidecade undertakings; and

2. It’s great fun if you’re the Pharoah, but questionable if you’re the pleb up to your knees in straw, mud and sh*t.

48. **sunderpeeche**
June 1, 2006

Basically, that’s the way it is. It’s good to be the King.

The racoons are not done yet.

At 8:06 PM, raccoons again tested the cross-gallery defenses. One raccoon was caught by a patrol and questioned. She claimed that she was disoriented by the high frequency noises and accidentally stumbled into the gallery. The patrol took her picture, paw print, and then escorted her out of the building.

http://www.fnal.gov/pub/news06/update_archive/update_5-30_5-31.html

Is a picture of the raccoon available?

Not Even Wrong Available in the UK

May 31, 2006
Categories: Not Even Wrong: The Book

Not Even Wrong, the book, is now available from Amazon in the UK. It's being published in the British Commonwealth by Jonathan Cape, so should also at some point be available from them in Canada. Last I'd heard official publication date was June 16th, perhaps that's the date it will be out in stores. Here in the US it will be published by Basic Books, and should appear perhaps late in September. The US version will have a rewritten and somewhat different preface, but the bulk of the two books will be pretty much the same. Next week I'll be going over the proofs of the US version, my last chance to make any final small changes.

So, I encourage all my non-US readers to go out and get copies of the book for themselves and their 20 closest friends, write reviews saying how wonderful it is on Amazon, etc., etc. If you're in the US and absolutely can't wait until September, I think you can order from Amazon UK anyway (at least that's what some of my friends desperate to appease children demanding the latest Harry Potter recall doing). In September, besides my book, Lee Smolin's The Trouble With Physics will also be coming out, so I guess it will be a trend. I haven't yet seen a copy, but from conversations with him gather that Lee reaches many of the same conclusions about string theory that I do, although coming from a somewhat different direction.

Perhaps my faithful readers can help out with some advice on two points: the US version will have a somewhat different subtitle and controversy has broken out as to whether to use the phrase "the search for unity in physical law" or "the search for unity in physical laws". Both seem all right to me, but if anyone has a strong opinion on the "law" vs. "laws" issue, let me know.

I'm about to leave town, headed to Boston for a few days, but early next week I'd like to start thinking about whether it would be possible to add some features to the blog related to the book. The obvious thing to do is to add all sorts of marketing materials to the blog to get people to go out and buy the book, but I'll try and keep that to a tasteful minimum. No pop-ups or weekly contests. On the other hand I think it would be interesting to provide some sort of features here for people who have read or are reading the book, have questions about it, want to discuss or argue about it, etc. Does anyone know of any other blogs out there associated with a book that have tried to do something like this?

Comments

1. **The Anti-Lubos**
   May 31, 2006

   "Law" resonates better with "unity." "Laws" sounds terrible. Feynman didn't write *The Character of Physical Laws*.

2. **Santo D'Agostino**
   May 31, 2006

   Congratulations on the UK publication of your book, Peter!

   "Laws" is better, because the other phrase might be misconstrued as having something to do with a non-existent branch of the legal profession (you know, corporate law, criminal law, international law, and now physical law).

   I don't find anything wrong with "The Character of Physical Laws." Feynman's title has a distinctly British feel to my ears, and since we are speaking about the US version of your book, that is perhaps another argument for going with "laws." As in many arguments of this type, it is probably not worth fretting over, as either alternative will serve well.

   A solution that sidesteps the controversy, and has the merit of greater brevity, is to go with "The search for unity in physics," but of course there may be reasons why this does not suit your purpose.

   Best wishes,
   Santo

3. **Who**
   May 31, 2006

   I have to see the whole title before I can judge:
   suppose versions A and B are

   Not Even Wrong: the Failure of String Theory and the Continuing Search for Unity in Physical Laws

   Not Even Wrong: the Failure of String Theory and the Continuing Search for Unity in Physical Law

   In that case, if that is the choice, then I slightly prefer B, the latter—with the singular.
   One could say the plural is appropriate because one seeks a pattern to bring a bunch of things together—to unify several laws.
   But I still like the sound of the singular better in this context.

   I think the word "continuing" is important because if you say "the Failure of String Theory and the Search for Unity in Physical Law" there is a danger that people could hear the message that the entire search for unity has failed. the message is that one effort failed but we continue the search. It is important to deliver that positive message (of not giving up the search) to balance the negative.

4. **wolfgang**
   May 31, 2006

   I am not a native English speaker, but "Search for Unity in Physical Law" seems to contain a contradiction.
   If there is only one law how can you *search* for unity in it?

   By the way Clifford and Lubos discussed law vs. theory recently, their ideas might help you 😎

5. **Thomas Love**
   May 31, 2006

Since I really don't like the use of the word "law" in physics, might I suggest "Search for Unity in Physics."

"Law" has the implication of a law maker and I would really not like to think of god as a lawyer.

6. **Chris W.**
May 31, 2006

From Amazon's book description for Smolin's *The Trouble With Physics*: "But as Smolin reveals, there"s a deep flaw in the theory: no part of it has been proven, and no one knows how to prove it."

To the attention of book editors, copywriters, and science journalists:
Theories in the empirical sciences, as opposed to mathematics and logic, can be testable and tested, but they are not and cannot be proven. Got that? *PLEASE* do not conflate testability with the possibility of proof! In 2006, one hundred years after the advent of special relativity and the beginnings of quantum theory in the face of the success of classical physics, there is no excuse for such sloppiness.

I really hope that the main thesis of *Not Even Wrong* is not presented with similar carelessness by Jonathan Cape and Basic Books in their marketing materials and dust jacket copy.

7. **David**
May 31, 2006

I would chose "Physical Laws" for the reason already stated — that one cannot unify a single law. The singular does suggest the kind of "one true law" that string theory set out to create but I think it would be more appropriate to say "the search for a unifying (sic?) physical law"

8. **Who**
May 31, 2006

Law can refer to a body of laws—-the singular can serve to cover the plural.

Check out what it says about Smolin's book. Any comment on the title
"the rise of string theory, the fall of a science"
I think the "a" is important. it is not the decline of all Science but of one particular branch: physics theory—which arguably HAS been going thru some hard times that coincided with the proliferation of string theorists. different ways to interpret this.
I think the title is hard-hitting. which it probably needs to be at this juncture

9. **Tony Smith**
May 31, 2006

Peter, you say "… I think it would be interesting to provide some sort of features here for people who have read or are reading the book, have questions about it, want to discuss or argue about it, etc. Does anyone know of any other blogs out there associated with a book that have tried to do something like this? …".

1997 was before the blog era, but it was the year of publication of David Deutsch's book "The Fabric of Reality".
IIRC, in the months leading up to its publication, he was very active in internet discussions (including usenet etc) about topics related to the book, and, according to the web page at http://www.qubit.org/people/david/FabricOfReality/FoR.html#FoRList , there was (and is)
"… The FoR (Fabric of Reality) internet discussion list
(Note that this is not run by me [David Deutsch], but I [David Deutsch] do contribute occasionally.)
To subscribe, send a blank message to Fabric-of-Reality-subscribe@yahoogroups.com …".

He might be able to give a fairly accurate idea of how internet exposure (roughly equivalent to blogs in today's world) affected sales etc of his book.

As an interested customer who bought and enjoyed the book, I think that the internet exposure was useful both in clarifying points made in the book and in sales of the book.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

10. **Sacha**
May 31, 2006

"law" sounds better – as it refers to the body of physical theories, while "laws" sounds as if it refers to the collection of individual physical theories.

11. **Dan**
May 31, 2006

i would imagine lubos will write an extremely negative review of both books.

12. **amanda**
June 1, 2006

i would imagine lubos will write an extremely negative review of both books.

I'm betting: two stars for PW, and one for LS. Because I have this feeling that LS annoys Lubos even more than PW…..

13. **Chris Oakley**
June 1, 2006

I would just prefer "Why clever people sometimes do stupid things" as the subtitle.

14. **Alejandro Rivero**
June 1, 2006

"*but if anyone has a strong opinion on the "law" vs. "laws" issue*"

Clifford has. Last week in his blog wrote about the use of "theory" and "law".

15. **Lubos Motl**
June 1, 2006

Amanda: your predictions – including the quantitative ones – are so incredibly accurate that you could probably do string theory; see PW's amazon.co.uk webpage now. 😉 I have not yet read Smolin's book, and I promised him to read every single page before reviewing it in the public.

16. **Andy**
June 1, 2006

I checked out the amazon.ca site and the UK edition of NEW is now available for about CAD 31 while if I wait until September for the US version it would be less than CAD 20.

17. **Lubos Motl**
June 1, 2006

Dear Andy,

if you wait until September, you will save not only CAD 11 but in fact CAD 31. 😉

Best
Lubos

18. **sunderpeeche**
June 1, 2006

Indeed there is now a 2-star review on the UK website. Reasonably vitriolic, but then again oil of vitriol is considered to be medicine. The 14 GeV, if true, is an error, as is 14 TeV. The LHC design beam energy (for protons) is 7 TeV leading to sqrt(s) = 14 TeV. No mention of Pierre Ramond? That is curious, but maybe corrected in the published version.

19. **Lubos Motl**
June 1, 2006

Sorry, I should have written the center-of-mass energy of the LHC beams.

20. **island**
June 1, 2006

I disagree with the idea that… "it is not the decline of all Science but of one particular branch: physics theory".

Physics ultimately determines everyone's worldview of reality and their place in the universe, so this also affects the way that ohter scientists approach their fields of study as noted in the description of of my weblog and elsewhere there:

Science In Crisis

Good job, Peter, and it's also good that your book is being offered together with Lee's book on amazon U.S.

I see that Lubos didn't waste any time making a crackpot ass out of himself, per the usual:
Peter Woit is the owner of a well-known blog that provides high-energy theoretical physics with the same service as William Dembski's blog offers to evolutionary biology: it is designed to misinterpret and obscure virtually every event in physics and transform it into poison – and to invent his own fantasies to hurt science.

Translation: Lubos' promise to read every word of Lee's book means that he willfully ignores every valid point that Lee Smolin makes before he misinterprets and obscurse his review to fit his own delusional fanatasies.

surpriseNot

21. **knotted string**
June 1, 2006

I haven't seen the book yet but looking at Lumos' review, particularly the comment in the first paragraph:

"… invent his own fantasies to hurt science. This makes Woit's blog highly popular among the crackpots, for example the first reviewer of this book."

I've complained to Amazon about the personal remarks about other reviewers. Lumo should keep his review to scientific facts related to the book. If he is too half witted to do that, I wish Dr Oakley would issue legal proceedings for damages to his reputation based on this sort of attack.

The danger is of course that people who have studied the problems will fear to review the book – or will do so anonymously – to avoid being personally attacked as crackpots. This is not scientific.

22. **ObsessiveMathsFreak**
June 1, 2006

They're not "laws". They're "just theories" 😉

23. **Lubos Motl**
June 1, 2006

Dear knotted string,

I am going to guarantee to have witnesses, leading physicists, ready to confirm that Chris Oakley is a crackpot if someone indeed finds this observation controversial. Nothing against Chris personally, I like him, of course, but denying the he is a crackpot sounds kind of insane to me. Where have we gotten?

All the best
Lubos

24. **Chris Oakley**
June 1, 2006

The stock of Crackpot Inc. has been devalued so much by the exercise of employee options by String Theorists that I am not sure that it is worth me suing. However, if anyone starts an action against me they ought to know that I will represent myself in court having realised (in my last lawsuit) that I don't need to pay anyone to bullshit for me. I am perfectly capable of doing that for myself.

25. **knotted string**
June 1, 2006

Lumo,

Witnesses who hate Dirac and Feynman's outlook? What have people's opinions to do with the fact that there are real, experimentally known problems in quantum field theory and string theory just invents unobservables?

If there were a single piece of scientific evidence for strings, all you have to do is publish that! There isn't a shred of evidence for string theory, and you know it.

26. **D R Lunsford**
June 1, 2006

I'd drop the "law" part altogether. Just say "physics" instead of "physical X".

-drl

27. **Thomas Love**
June 1, 2006

Well, I succumbed to your propaganda and ordered both books at amazon. I look forward to reading them. The deal from amazon comes with no strings attached! Free shipping even!

As Chris said, scientific "laws" cannot be proven, but they can be disproven, if their predictions are contradicted by experiment. That is why string theory is not scientific, it makes no testable predictions.

28. **Ponderer of Things**
June 1, 2006

laws is better.
"physics" works too, as DRL suggested.

29. **Belizean**
June 1, 2006

Congrats, Peter!

"Law" sounds infinitely better to my ears.

30. **Quanta**
June 1, 2006

I'm unconvinced by Smolin's title, "The Trouble With Physics". High energy particle physics and string theory in particular might have stagnated for 30 years, but the rest of us theoretical physicists are doing fine, thanks for asking. Just look at the interesting things being done in quantum information and computation, or in statistical physics, (cond-mat is full of interesting, worthwhile papers).

I know that many particle physicists think science revolves around them, but the rest of us don't care that much. There is no crisis in physics over here.

31. **fishfry**
June 1, 2006

I'm having a hard time parsing the title. What has failed? String theory alone? Or string theory AND the search etc.?

32. **sunderpeeche**
June 1, 2006

Actually this is something I have noticed also, the persistent identification of HEP theory with "physics". But there's much more to physics than HEP (th + exp). Biophysics etc are doing well.

Die physik stekt in der krise? Es gibt die andere physik nicht steckt in der krise.

33. **Bert Schroer**
June 2, 2006

sunderpeeche
particle theory is a fundamenta area of physics which for generations has supplied innovative services to other areas (condensed matter physics, and even biophysics see the contributions of Tony Zee). And string theory is more than a branch of particle physics, it is a state of mind science in an unleashed global capitalism. Just wait, it will get there too.

34. **sunderpeeche**
June 2, 2006

Quantum physics –> nuclear physics –> particle physics has always been an endeavor to understand matter at the most fundamental level. I have always acknowledged that. And it is a challenge worth pursuing. I have said that too (to various people, years ago, before this blog existed).

The other branches of physics have supplied important ideas to particle physics. Spontaneous symmetry breaking arose in statistical mechanics in the context of phase transitions (~work of Phil Anderson), was adapted to HEP. The Wilson-Fisher collaboration shows that stat mech and HEP can give much to each other. HEP may be a worthy effort, but there is also much good physics that is not HEP, and they are not in crisis.

35. **Bert Schroer**
June 2, 2006

Sorry, the idea of spontaneous symmetry-breaking comes from Heisenberg's investigations of the ferromagnet. I agree with you that there was a backflow from condensed matter physics, but quantum Stat. Mech. was never separated from QFT and particle theory. And fortunately you are right up to now, but probably not any more for very much much longer. Remember string theory is also a state of mind, the state of mind of a hegemon. Although within the limit of science innocent people will not loose there life (like in Bush's war in Iraq), young physicists may for a long time (probably life-long) be deprived of that deep satisfaction which results from having contributed something permanent.

36. **sunderpeeche**
June 2, 2006

I knew SSB originated with the ferromagnet, didn't realize it was Heisenberg. Anderson did some work, but I suppose it came later.

37. **Bert Schroer**
June 2, 2006

Many generations of physicists worked on the ferromagnet, even after Heisenberg observed the notion of spontaneous symmetry breaking. He immedeatly saw the relevance for particle physics, but the first realization in the setting of Lagrangian quantization is due to Goldstone.

38. **Thomas Larsson**
June 2, 2006

Spontaneous breaking of gauge symmetry with the Higgs mechanism was invented by Anderson in the context of superconductivity. I think that the idea of spontaneous (non-gauge!) symmetry breaking in ferromagnets predates Heisenberg and even Ising (who didn't find it in 1D and therefore conjectured that it's absent in 3D as well). Surely people (Lenz? Debye?) knew about SSB within mean-field theory.

39. **Bert Schroer**
June 2, 2006

Thomas Larson
You are probably right, I take you on your word. But there should be a little bit caution since to call the Schwinger-Higgs-Anderson breaking a breaking of gauge symmetry is one of these imprecise manners of speaking (because a gauge of the second kind is a redundant description and not a genuine symmetry). I do not have any access to the literature where I am right now, so maybe you could find out whether Anderson really was before Schwinger. The citation to Schwinger you find in
http://br.arxiv.org/abs/hep-th/9906089
I took a considerable amount of pain to find out about the early history of the Ising model because together with Pauli's exclusion principle it characterized pretty much the big splash by which the history of theoretical physics started in Hamburg (here you find the reference to Schwinger a second time, ref. 15):
http://br.arxiv.org/abs/hep-th/0504206

40. **Thomas Larsson**
June 12, 2006

Peter, I know that you are not into this business for the money, but maybe you should laugh all the way to the bank.

Peter Woit:
Amazon.co.uk Sales Rank: 85

Brian Greene:
Amazon.co.uk Sales Rank: 5,869

Lawrence Krauss:
Amazon.co.uk Sales Rank: 24,280

Leonard Susskind:
Amazon.co.uk Sales Rank: 26,994

Lisa Randall:
Amazon.co.uk Sales Rank: 78,730

I don't know what the sales rank is really measuring, but that it is low cannot be a bad thing.

41. **Alejandro Rivero**
June 14, 2006

Is the sales rank accumulative or instantateus?

42. **Alejandro Rivero**
June 14, 2006

You can use

http://www.touchgraph.com/TGAmazonBrowser.html

to build a network of relationchips between books, based on common buy patterns. It works with amazon USA, so perhaps it does not include Woit's book yet.

43. **Who**
June 14, 2006

Alejandro [*Is the sales rank cumulative or instantaneous?*] I think it must be based on some short time window like the past 24 hours because I see that it apparently fluctuates a lot.

today, on Wednesday 14 June,

Peter Woit 211
Leonard Susskind 27,510

but on Monday 12 June, when Thomas tried the same links, it was
Peter Woit 85
Leonard Susskind 26,994

44. **Alejandro Rivero**
June 14, 2006

Ok, so it is a kind of "top ten list", not a total sales list.

I have seen that amazon accepts order for the newer books, as Woit's or Smolin's ones. If someone gets to plot an interesting touchgraph plot, please capture the screen and share with us!

About Smolin's, I am surprised by the cover design: it accidentaly refers to another 1960s theory, the "bootstrap". There was, of course, some early tryes to relate string theory and bootstrap, in a couple articles of Veneziano, but I am not sure of the impact.

45. **Who**
June 14, 2006

Alejandro, I disagree. As I saw it, I don't think the cover has any intentional reference to "bootstrap". I didn't perceive any when i first saw it. The word that is evoked is "string" (as in "shoe-strings tied together") and not "strap".

there is a proverbial image of someone who is either the victim of a mean practical joke, or such a stupid klutz that he has done this to himself—-the person with their shoe-strings tied together.

It is a proverbial image of a person in a stupid fix that they brought on themselves.

Bootstraps are different from shoestrings and the picture doesn't say straps.

I think the cover-image message is two fold

the "trouble with physics" is string-related (symbolized by shoe-strings)

the "trouble with physics" is a stupid self-imposed predicament that could have been avoided by commonsense (keep your feet able to move independently)

46. **Alejandro Rivero**
June 14, 2006

Hmm you are right, I had an incorrect mental picture of a bootstrap lace where it was the same lace we do with the shoestring, and no a separate lace.

First Public Reaction From String Theorist to “Not Even Wrong”

June 2, 2006
Categories: Not Even Wrong: The Book

Last month I made the following prediction:

String theory doesn't make any predictions, but I can make one: Lubos will be among the first reviewers of my book on Amazon, and I'll get two stars.

This prediction was confirmed today, with a certain Harvard faculty member acting exactly the way you would expect. The reason for the two stars is that Lubos is well aware that Amazon usually deletes one star reviews.

His Amazon review is nutty in so many ways it's hard to know where to start. It begins with:

I have read a different edition of the book than one offered here, and I apologize in advance for any inaccuracies in my review that this fact could cause. In fact, if any errors from the list below have been corrected, it was because of my feedback, so I think it is fair to list them anyway.

I have no idea what “different edition” of the book he is referring to, perhaps it is the earlier version that Cambridge considered a couple years ago, which was circulated by them and by me to various people. Whatever it was he was reading, I never received any feedback from him correcting supposed errors. Besides this weird delusion, pretty much everything else he quotes as an error in the book is something he has made up out of whole cloth. He doesn't directly quote a single word of mine or give page numbers, so I can't even figure out where he is getting this nonsense.

I'll just ignore the ranting and ad hominem attacks, trademarks of someone on the losing side of an argument, and address the very few substantive errors he claims I make where I can actually locate the exact place in the book he claims an error is being made:

Woit writes that the energy of the LHC beam will be 14 GeV, instead of 14 TeV

Page 31: “ is a proton-proton collider with a total energy of 14 Tev”

Note that the original is correct, his correction is wrong (the beam energy is 7 TeV).

In his description of the history of supersymmetry, he forgets Pierre Ramond.

Actually I explain carefully in the preface of the book that the history is quite sketchy and many people are left out. One of my main fears after writing this book was realizing how many enemies I would make by not putting their names in. In this case however, Pierre Ramond is in the index and I write:

Page 154: “The first string theory with fermions was constructed by Pierre Ramond late in 1970”

Page 155: “Early string theorists discovered that string theories with fermions involved a version of supersymmetry…”

He misunderstands how SU(2) can be embedded to SO(4)

There's nothing in the book about embeddings of SU(2) in SO(4). Presumably this is a reference to a mistaken statement I made once on this weblog. Yes, dear reader, among the by now probably thousands of pages of material I have written on this blog, I have sometimes said something incorrect. The book is written a lot more carefully than my blog postings.

Even more seriously, he builds his case upon e-mail messages from undetermined sources that supported Woit's viewpoint. Most of these e-mails were obviously written by crackpots.

In the book I'm quite careful to attribute things I quote and there are very few e-mails quoted. There's only one unattributed e-mail that I can think of, it was written by someone visiting the Harvard string theory group at the time of the Bogdanov scandal, who wrote:

“So no one in the string group at Harvard can tell if these papers are real or fraudulent. This morning told that they were frauds, everyone was laughing at how obvious it is. This afternoon, told they are real professors and that this is not a fraud, everyone here says, well, maybe it is real stuff.”

This is unattributed since I don't know who wrote it. Maybe they were a crackpot, one visiting the Harvard string theory group.
The problematic statement that string theory makes no prediction is repeated hundreds of times, and in many particular contexts, such a statement becomes not only boring but also patently false.

I doubt it's actually in the hundreds, but sure, I do repeatedly claim that string theory makes no predictions, and this is not “patently false”, but completely accurate.

he never mentions names like Weinberg, Gell-Mann, Hawking, Randall, Arkani-Hamed

Weinberg, Gell-Mann, and Hawking are each mentioned many times in the book, and I list Lisa Randall's book as the suggested place to learn more about brane-world scenarios. It's true that Arkani-Hamed is not in the book.

I could go on about the rest of the review, but really, what's the point?

I would like to think that Lubos is a huge embarrassment to the string theory community, but the sad thing is that there's little evidence that they're embarrassed.

Comments

1. **Chris Oakley**
   June 2, 2006

   I could forgive him all of these things. I could even forgive him for calling Dirac and Feynman “crackpots”. But the music on his blog … ? I struggled in vain for a way of turning it off, but in the end had to turn the sound off for the whole computer. This is an invasion of my personal space that I resent.

2. **anon**
   June 2, 2006

This may be relevant to why the string theory community relishes Lubos Motl instead of being embarrassed:

'Crimestop means the faculty of stopping short, as though by instinct, at the threshold of any dangerous thought. It includes the power … of being bored or repelled by any train of thought which is capable of leading in a heretical direction.

'Crimestop, in short, means protective stupidity. But stupidity is not enough. On the contrary, orthodoxy in the full sense demands a control over one's own mental processes as complete as that of a contortionist over his body.' – George Orwell, http://www.panarchy.org/orwell/ignorance.1949.html

This is of course the explanation for how brainwashing works.

3. **Bert Schroer**
   June 2, 2006

   The Lord of misuse again. The string community feels so powerful that, like Royal Courts in the past, they can afford themselves a court jester at Harvard.

4. **MathPhys**
   June 2, 2006

   I just read the blurb of Lee Smolin's forthcoming book, on amazon, and he's damning of string theory in words that are at least as strong as Peter uses in his book.

5. **Lubos Motl**
   June 2, 2006

   Dear Peter Woit,

   I will give you the pagenumbers later, when someone gives me this edition. If you were able to turn your brain on for a while, you would easily determine that I have read the book in extreme detail, and it was certainly not years ago.

   It is also not true that amazon.com "usually" erases 1-star reviews. I have written dozens of 1-star reviews and they are still there.

   http://www.amazon.com/gp/cdp/member-reviews/A1ZDPQA6FLY8XM/

   Only corrupt crackpot authors such as Mark McCutcheon make amazon.com erase all inconvenient reviews – not just 1-star reviews – and indeed, you would not surprise me too much if you acted like McCutcheon.

   I insist that if you corrected any of the errors I mentioned, it was because of my feedback, and I have a proof.

   Best wishes
   Lubos

6. **Lobert Smythe**
   June 2, 2006

   Professor Motl, we will all laugh together at Peter's loss of dignity, and we will respect you as a conquering hero and a force to be reckoned with, if you present the proof that you are talking about and it is actually a sufficient proof.

   Please, show us the proof.

7. **sunderpeeche**
   June 2, 2006

   Why not contact Amazon and point out that LM based his review on an earlier manuscript different from the one submitted to Jonathan Cape? In particular that (at least some of) the errors he mentions in his review are in fact not in the book?

8. **sunderpeeche**
   June 2, 2006

   There is some inconsistency in the Amazon web pages

   http://www.amazon.com/gp/product/0224076051/002-2587755-3205615

   http://www.amazon.co.uk/exec/obidos/ASIN/0224076051/202-9664174-0234244

   The review by Sam is only in the first, not the second, link. The "number of people who found the review useful" is different.

9. **Luboš Motl**
   June 2, 2006

   Sentences with the statements that Peter Woit would like to deny:

   14 GeV: in Accelerators: Future prospects

   The energy of a ring scales linearly with its size and the magnetic field, so one could double the energy of the LHC to 28 GeV either by building a ring twice as large or by finding a way to make magnets with twice the field strength.

   Ramond: Peter Woit accepts that I am right. Yes, the only place where his name appear, is a link to his QFT book, and the sentence that he constructed a theory with fermions. But by doing so, he has constructed the first supersymmetric theory in the West. The first supersymmetric theory in the West was written in the context of string theory, with worldsheet SUSY. Peter Woit either does not know this fact, or he hides it in his comments about the history of supersymmetry. It's like describing Christianity without mentioning Jesus Christ.

   SO(4): yes, I know this piece of ignorance of Peter Woit from this blog where he proposed the "off-diagonal" embedding of groups, but it also appears in the book in the section

   PICTURE OF A WAVE AND A PHASE CHANGE

The whole page is kind of wrong, much like the rest of the book. It tries to describe SU(2) rotations as actions on the two-complex- i.e. four-real-dimensional space. However, it pretends that rotations in the 4D space are given by 1D axes, and they are "unvisualizable". They are unvisualizable for Peter Woit because he has no idea about higher-dimensional orthogonal groups. He does not understand that the SU(2) transformations act on the four-real-dimensional space as SO(4) rotations and how they're embedded. I insist that this is the reason of the errors in that section.

Relying on crackpots

Peter is also saying untrue things when he denies that he builds on support from undetermined sources. Chapter "The only game in town…":

"A huge number of congratulatory messages arrived, many with an aspect that surprised me. These messages remarked on my courage and expressed the hope that I would survive what they expected to be a fierce personal attack from superstring theorists. I hadn't known that so many people in the physics community not only were sceptical of superstring theory, but even felt that the subject was perpetuating itself through some sort of intimidation."

The correct wording should have been: "I became a hero among the crackpots, and in fact one of them: Chris Oakley, MathPhys, Tony Smith, Juan R, Peter Woit, Danny Lunsford, and innumerable anonymous ones".

Another lie of Peter is that he mentions Weinberg's opinions about string theory, much like the opinion of others. I can't provide you with any quotes here because they don't exist.

Not having Arkani-Hamed in a book that pretends to be about particle physics beyond the Standard Model and the struggle not to spend all efforts on string theory is unforgivable. 10% of the reasons above – and the dozens of lethal problems with the book that Peter Woit did not mention here – would be enough to identify the book as nothing else than junk.

10. **Luboš Motl**
June 2, 2006

Dear Peter Woit,

you should realize the idea of one of your uncountable crackpot fans, and contact amazon.com and ask them to erase my review. Mark McCutcheon who has rather similar opinions about physics as you have has done it about 200 times. 😉 I think that this idea quite accurately describes who you are and how you imagine that a discussion should look like.

Best
Lubos

11. **Luboš Motl**
June 2, 2006

Dear Sunderpeeche,

I understand that some people simply can't recognize that Peter Woit's writing is a gigantic pile of nonsense, but you could at least try to understand that amazon.com and amazon.co.uk are two different companies with two different websites that don't have to contain the same datas. It's hard, is not it?

Best
Lubos

12. **Lobert Smythe**
June 2, 2006

The proof, Lubos. The proof. If we don't get to see it then we all end up thinking that you had no proof.

13. **robert**
June 2, 2006

This ranting is too sad for words. I've ordered the book nonetheless.

14. **sunderpeeche**
June 2, 2006

Indeed, there was no need to have this post in the first place. But anyway, verify the various statements (e.g. the 14 GeV). Actually it is the momentum which scales with B and R (p \propto eBR). But for ultrarelativistic motion one can say energy.

15. **Luboš Motl**
June 2, 2006

Dear Lobert, I could easily give you a proof by sending you the full file with the book, but I am afraid that the crackpot king here could sue me for copyright violations, so I prefer if you – and all people like you – will think that I have no proof. 😉

16. **Lobert Smythe**
June 2, 2006

So be it.

17. **woit**
June 2, 2006

Sunderpeeche,

The amazon.com and amazon.uk sites are different, but at least for a while the review from "Sam" was on both sites, but recently was deleted from the amazon.uk site. I'm curious if anyone knows why.

18. **woit**
June 2, 2006

Lubos,

Honestly I don't know what version of the book you have been reading. The last version I have in electronic form is from last summer, since then all the proofs have been sent to me to deal with in paper form. The publishers have pdfs of these, but I don't. Over the years I have circulated various versions to people who expressed an interest or whose advice I was seeking. It's quite possible I sent you a copy at some point if you asked. But I would definitely remember if you'd helped by sending me back "corrections", if you have some record of doing that, please remind me of it.

You have now helped with this, you did find one typo. After listing the total energy at the LHC as 14 Tev, on the next page when I discuss the possibility of doubling it, there's a "28 Gev", which is obviously a typo (although perhaps you think that this is evidence that Peter Woit doesn't know how to multiply by two…). Thanks for the help, next week I'm working on the proofs for the US edition and can fix that typo. If you find any others, let me know, preferably before the end of next week.

Your other comments are pretty much complete nonsense. Anyone who wants to can read the sections in the book you describe and decide for themselves whether what you say makes any sense.

The history of supersymmetry I give is not a "history of supersymmetry in the West", it's a history of supersymmetry, which first appeared as space-time supersymmetry in the East, not the West. The statements about Ramond I make are completely accurate.

I didn't say I mentioned Weinberg's views on string theory. He certainly at one point was a strong supporter. What his views are now is an interesting question that I don't know that he has addressed publicly, so don't discuss in the book. I don't want to quote private communications from him, but one should note that quite a while back he voted with his feet and stopped working on string theory.

I'll stick by my decision to not deal with brane-world scenarios in any detail and thus not mention Arkani-Hamed's work explicitly.

19. **Steve Myers**
June 2, 2006

Peter,

past experience taught me not to read reviews. The best criticism comes from someone who knows the subject and you. Most reviews are so far off (when not personal attacks) that they're a waste of time to read.

20. **Andrew**
June 2, 2006

I wouldn't worry too much about Lubos' review. It's as insignificant as his scientific publications. Most people know that the only way to judge a book is to read it, not its reviews.

21. **Lubos Motl**
June 2, 2006

Dear Peter,

your answer makes the situation very clear. If you have not corrected even the "LHC at 28 GeV" error, then you could not have corrected a single one from my list of dozens of lethal errors in your strange book because the others must be very difficult for you to find – and no doubt, there are dozens of additional errors. So I might have read exactly the same version that was just released and my review is probably 100% relevant for the British edition.

You should not be surprised that people have read the book because you have sent your nonsense to dozens of publishers, attempting to make it publish by a science publishing house. Of course, as a science book, N.E.W. is rubbish.

As an environmentalist I am a moderate one but still, it does not make me too happy to imagine how many trees had to die to print this nonsense of yours.

I am sure that you think that these are details. Confusing TeV and GeV, omitting Arkani-Hamed from the whole book, omiting any positive voices such as Weinberg, Gell-Mann, Hawking, misunderstanding what gauge symmetry and background independence is, missing that the verified QED phenomena are calculating perturbatively, and so on, and so on – these are no details. These are things that put your presentation well below a class presentation of an average 2nd year high-energy grad student.

No person who actually thinks about particle physics would leave such an error with the GeV. No person who has learned the Standard Model well would write that the TeV neutrinos are virtually non-interacting and undetectable. And so on, and so on.

Your ideas about the meaning of gauge symmetry are like from the 1950s. It's completely naive. You complete missed that a gauge symmetry is just a property of a classical limit of a theory. There is no unique answer to the question "what is gauge symmetry" at generic points of the couplings (or parameters in the field theory case). There are often many dual descriptions with very different gauge symmetries. N=4 with orthogonal group is S-dual to N=4 USp(2N). Gravity with diffeomorphism local symmetry is equivalent to a gauge theory on the boundary. There are many equivalences like that. Inventing similar symmetries is only a tool to find a classical limit of a quantum theory, not the whole quantum theory.

Best wishes
Lubos

22. **Tommaso Dorigo**
June 2, 2006

That is enough, Lubos.

I did not mean to buy Peter's book, mainly because I have too little time to read these days, being too busy living on the new social welfare, HEP tenure.

But your comments made it too compelling for me to buy it. Darn, I want to be a part of this. I want to find more inaccuracies, and brag about it. Any given book contains several, and if I work hard enough I am sure I will find one myself. Maybe if I find a compelling mistake in the book and I brag loud enough, people will stop saying I am stealing taxpayers' money ?

Anyway thank you! I'm on my way to the bookseller.
Tommaso

23. **Bert Schroer**
June 2, 2006

I find it totally pathetic that those people who imposed on others for decades the story about the fundamental aspects of "the gauge principle" in QFT when QFTists with a bit higher developed conceptual power knew that this is one of those metaphoric description did not want to spend their energies removing holy cows (nothing intrinsic, since there was never any autonomous structural property which could reveal that allegedly gauge invariant observables where coming from a "gauge" theory) now have to ram into our throats (via their jester) that after all this was not so (a kind of April fool's joke of very long duration).
It is to be feared that this hegemonic moloch which, as we heard from Gross in his special Princeton lecture, cannot die, will devour any kind of potentially useful idea and and make it is own in oder to survive. When it finally leaves (because nothing is forewer) it will leave such a big creater in physics that it takes longer than the time between Galilei and Newton to recover again.

24. **hack**
June 2, 2006

Peter, I hope you properly credit Lubos in the aknowledgements for correcting your typo. After all, this is likely to be his most long lasting contribution to physics.

25. **MathPhys**
June 2, 2006

Lubos,

Do you have any work to do? The amount of time you spend on the internet is incredible. Where do you get the time?

26. **nu**
June 2, 2006

Lubos, I don't see how your doubt about neutrinos is connected with the main topic of the book. Anyhow, the absorption length of a neutrino with TeV energy is more than 10^7 kmwe, comparable the thickness of the sun. This means, for example, that LHC will produce many neutrinos, but none will be detected.

27. **anon**
June 2, 2006

MathPhys,

Lubos is a string theorist!

28. **sunderpeeche**
June 2, 2006

Reasonably mild superstrings article in June 2006 Physics Today p54. But he writes Nicholas "Kenmer" instead of Kemmer.

29. **Mentos**
June 2, 2006

"I find it totally pathetic that those people who imposed on others for decades the story about the fundamental aspects of "the gauge principle" in QFT when QFTists with a bit higher developed conceptual power knew that this is one of those metaphoric description did not want to spend their energies removing holy cows (nothing intrinsic, since there was never any autonomous structural property which could reveal that allegedly gauge invariant observables where coming from a "gauge" theory) now have to ram into our throats (via their jester) that after all this was not so (a kind of April fool's joke of very long duration)."

Since AQFT has proven to be of zero help in formulating a manifestly gauge-invariant formulation of Yang-Mills Theory (or, for that matter, providing any kind of formulation of any asymptotically-free 4d gauge theory), I wouldn't be so quick to dismiss as "pathetic" the observation that there exist, in some cases, multiple formulations with (different) local gauge-invariances.

This desire to return to the "good old days" of the late 1960's, when nobody knew much of anything about 4d quantum field theory and so AQFT was no further behind any other approach to QFT, is rather hard to take seriously, even by theorists without the slightest interest in string theory. For you, string theory's just a metaphor for all the bad stuff (AKA progress) that's happened in the past 35 years.

By contrast, Peter would only drag us back to the early 1980s which, I suppose, should be seen as some sort of enlightened progress.

30. **island**
June 2, 2006

Peter, this is just a suggestion, but I think that maybe you should request that Amazon list your book with Lee's book in what Amazon calls the "Perfect Partner", for the impact of the combined force. It appears that possibly Lee did this too, although, that's not a law, it's only my theory… 😉

My Plug for both books.

I'll probably add more and republish, but let me know if you have a problem with any of it and I'll just dump it.

31. **Tony Smith**
June 2, 2006

Lubos, in a comment on this blog entry, referred to:
"… the crackpots … Chris Oakley, MathPhys, Tony Smith, Juan R, Peter Woit, Danny Lunsford …".

Lubos, in another comment on another entry in this blog, said
that he is "… going to guarantee to have witnesses, leading physicists, ready to confirm that Chris Oakley is a crackpot …".

I propose that Lubos, as Harvard Professor, invite me to Harvard to make a 2-hour talk followed by questions about my physics model, then followed by a vote among the audience as to whether or not I appeared to be a crackpot, with the proceedings videorecorded and an electronic copy given to me so that I can place it on the internet.
If Lubos fails to make such an invitation, then it will be clear for all to see that he is too afraid of my ideas to let me talk on his home turf and let his own Harvard audience express their opinions about whether or not I am a crackpot.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

32. **Kiff**

June 2, 2006

Doesn't gauge symmetry leave a residue in the BRS cohomology?

33. **Bert Schroer**
June 2, 2006

Kiff, BRST is like a catalyzer in chemistry, it was not there in the initial formulation of the problem, and it has left no trace whatsoever one you have removed it by cohomological descend. There should be no catalyzers in particle physics, do you agree? So there is a deep unsolved problem there.

34. **Bert Schroer**
June 2, 2006

It was said, Mentos, but genuine physicists publish things and are not salesman as the 3 mentioned characters in a previous blog who have very very perishable merchandice which you are addicted to. Thats why you are also unable to understand what is acrually going on in QFT, you will only complain if one of your salesmen happen to ursurpate some of the new thinhs and hands it to you. Kleine anonyme Kanaille.

35. **Kiff**
June 2, 2006

I'm not sure if I agree with your philosophy that BRS is merely a "catalyst" or that catalysts ought to be avoided at all costs. There might be something real deep behind it. And besides, how else would you propose working with gauge theories without BRS?

36. **woit**
June 2, 2006

Lubos and Tony,
Mentos and Bert,
Others,

I've deleted a whole slew of comments from people promoting their books and attacking each other. Sorry, but since it's my blog and a posting about my book, I insist that all personal attacks be aimed at me or my book, or I'll declare them off-topic and delete them. I'll leave Mentos's last solely because at least it contains an attack on me, but please, take AQFT and LQG warfare elsewhere.

37. **woit**
June 2, 2006

Lubos,

I see, it sounds like you have a copy of the version of the manuscript that Cambridge considered and that Jonathan Cape ultimately bought. If you have found more typos or mistakes, do let me know, much of that version is the same as the published version. But so far, everything you've mentioned except the 28 Gev is neither a typo nor a mistake on my part (although often a mistake on yours…).

38. **Chris Oakley**
June 2, 2006

Hi Lubos,

I am prepared to make the same offer as Tony Smith, but 1 hour plus Q & A's should suffice. I can pay my own expenses.

39. **Bert Schroer**
June 2, 2006

Kiff, your two last phrases have the same content, It is very deep to get the gauge invariant content without BRST. This problem on which Mund Yngvason and I are working is just this and the preliminary answer is do not insist in pointlike vector potentials but use the new concept of string localization, which has the same effect as ghosts as far as improving short distane behavior, but it does not have the disadvantage to extend the physical space by ghosts. Of course the difficulty is to formulate interactions for string-localized fields, this is virgin territorry. The new free string fields are ready, but since Mund is a perfectionist and has a lot of teaching he told me he is unable to post it only after the IMP conference in Rio de Janeiro. But you find a lot of introductory remarks into this problem in
http://br.arxiv.org/abs/math-ph/0511042
Kiff, have a sniff

40. **SomeBody**
June 2, 2006

Here's somebody else who seems to have read a pre-release version of the book (and to have points of disagreement with it):

http://rivelles.blogspot.com/2006/06/livro-de-peter-woit.html

41. **Steve**
June 2, 2006

On http://www.amazon.co.uk the variance of the review ratings is quite high. Oh good! That's exactly the sort of book that I would consider buying. Thank you Prof Motl for your magnificent contibution to NEW's ratings variance, and thus encouraging me to buy the book. Of course, I am sensitive to factors other than ratings variance, such as the eminence of those who supply the outliers in the data, and as such you have made the but/no-buy decision a no-brainer, as they say.

42. **Jonathan Vos Post**
June 2, 2006

"I've deleted a whole slew of comments from people promoting their books and attacking each other."

I don't think I attacked anyone, nor are any of my books for sale, and hence I was not promoting any book, but, rather trying to explain something about the viewpoint of someone who writes refereed Math papers, refereed Physics papers, and science fiction. If you think that I'm abusing your bandwidth, could you at least email me back my comment, and the comment of Bert Schroer to which I responding?

As a professional scientist/author, I am willing to give away some minutes of thought and work through blogging, but I'd rather not lose access to my own writing. Thank you.

— Porfessor Jonathan Vos Post

43. **woit**
June 2, 2006

Jonathan,

No, you were not attacking anyone, but your comment was largely links to your own material, had nothing much to do with the topic of this posting, and was an attempt (successful, unfortunately), to turn discussion to your interests in science fiction. Please don't do this. The number of comments here is about to overwhelm my ability to deal with them in any sensible way. Lots of people would like to turn this into a more general interest blog where they could discuss things that interest them. I neither can nor want to manage such a thing. Please avoid writing comments that are not about the topic of the posting. Digressions will be tolerated only if they are especially interesting and part of the narrow focus of this blog on certain areas of research math and physics.

I'm sorry but there is no way in this software to recover deleted comments. If you want future access to what you write here (or in any blog), write it in some other editor, save, cut and paste here.

44. **SomeBody**
June 2, 2006

Maybe adding a regular discussion forum to the site could take some pressure off the blog's comment section (and the blogger)?

45. **woit**
June 2, 2006

Somebody,

I may look into discussion forum software and think about how I might use it on this blog. Basic problem though is that it would still need to be moderated somehow, otherwise you end up with sci.physics. People have suggested to me that the kind of software that Slashdot uses might be the way to go, allowing readers to in some sense moderate the thing themselves by kind of voting on which comments deserve attention.

46. **Jonathan Vos Post**
June 2, 2006

woit: you're right. I was politely engaging someone else's comment, but it was rude to you, and bent the blog from your intended trajectory. I'll try to confine myself to the topic (the part about "there's no such thing as publicity" was, in my opinion). My mentor Richard Feynman was well-known to be deeply skeptical of String Theory.

47. **Bert Schroer**
June 3, 2006

Very, very good Jonathan, now you comment is anti-arasing-proved. You learned very fast.

48. **Bert Schroer**
June 3, 2006

It is probably too long ago, but for all us admirers of Fevnman's directness and rather reliable gut reactions (in contrast to Pauli), and assuming that your credentials Jonathan are genuine, it would be very interesting to know whether you remember any reasons Feynman gave for his critical position.

49. **anon**
June 3, 2006

Bert, see:

http://www.math.columbia.edu/~woit/wordpress/?p=272#comment-5295

Feynman just before dying said: "… I do feel strongly that this is nonsense! … I think all this superstring stuff is crazy and is in the wrong direction. … I don't like it that they're not calculating anything. … why are the masses of the various particles such as quarks what they are? All these numbers … have no explanations in these string theories – absolutely none! … ".

Strangely, Jonathan is misleading when he said this is well known. Search the internet! Everyone who is a string theorist claims they are as scientific in objectivity as Feynman.

All those people are LIARS or plain IGNORANT.

50. **Jonathan Vos Post**
June 3, 2006

Credentials: as menrtioned on the deleted link
http://www.magicdragon.com/UltimateSF/authorsP.html#JonPost
"Footnote to Feynman", Jonathan V. Post and Richard Feynman,
[Engineering & Science, Caltech, Pasadena, CA, Vol.XLVI, No.5, p.28,
ISSN: 0013-7812, May 1983; reprinted in Songs from Unsung Worlds,
ed. Bonnie Bilyeu Gordon, intro by Alan Lightman (award winning author
of Einstein's Dreams), Birkhauser Boston/AAAS, hardcover ISBN: 0-8176-3296-4, paperback ISBN: 3-7643-3296-4, 1985

Lengthier explication of Feynman's skepticism of String Theory may be found in:
Feynman's Rainbow: A Search for Beauty in Physics and in Life
by Leonard Mlodinow
Non-Fiction/Science
ISBN: 044653045X
Hardcover
$21.00/U.S.

$30.00/CAN
192 pages
5-1/2 x 8-1/4
WARNER BOOKS

lso availabe as an eBook

Drawing on extensive conversations the author had with Richard Feynman, this is the story of a young physicist trying to find his place in the world, and of the famous, old, and dying colleague whose wisdom helped him. Between them, they shared talk, food, science, and laughter that led the younger man to a deeper understanding of both his own creative imagination and the nature of humanity itself.

FEYNMAN'S RAINBOW

In the early 1980s, Leonard Mlodinow came to the California Institute of Technology to begin a postdoctoral fellowship. Mlodinow had written a groundbreaking Ph.D. thesis, but he was afraid he was simply not smart enough to be at Caltech. In danger of losing himself watching hours of Rockford Files reruns while waiting for one good idea, Mlodinow took his doubts and insecurities to Caltech's intimidating resident genius and iconoclast, Richard Feynman. So began a pivotal year in a young man's life and a year of awakening.

In this funny, inspiring, and revelatory book, Leonard Mlodinow looks back at the time he shared with Feynman: the ideas they explored, the views of life and physics they exchanged…

My experiences with Feynman discussing Quantum Computing, Nanotechnology, and String Theory have been recounted in some of my refereed papers, whose discussion has decayed here to the ground state.

I may have more to say on Feynman's take on why String Theory is Not Even Wrong at a later date.

51. **Tony Smith**
June 3, 2006

Jonathan Vos Post, Bert Schroer, and anon have commented here about Richard Feynman's view that "… all this superstring stuff is crazy and is in the wrong direction …".

Another physicist well-known to be both brilliant and practical is Sidney Coleman. Although I have not read an advance copy of Peter's book and so cannot quote it, I can quote from Peter's blog entry of March 2005:
"… Sadly, Coleman is in poor health, suffering from Parkinson's disease … his great Erice lectures … were collected in 1985 in the book "Aspects of Symmetry" … The fact that Coleman stopped giving these lectures after 1979 was to me one of the first indications that particle theory was entering a much less promising phase of its history. Coleman never really warmed to the topics of supersymmetry and string theory. …".

An even more telling account of Sidney Coleman's view of superstrings comes from a March 2005 entry in the blog of superstring guru Jacques Distler, which said in part:
"… Sidney was my PhD thesis advisor. Truth be told, his direct influence on my thesis was negligible. Midway through my graduate career, string theory swept through high energy physics. As a sensible young man, I dropped everything I'd been doing and rode the wave. Sidney was not interested in string theory; he wasn't even particularly interested in supersymmetry. …
No one thought more clearly about quantum field theory. And no one has ever lectured or written more lucidly about the subject. If you haven't read his Erice Lectures, you don't know the heights that scientific writing can attain. …".

Among those Erice Lectures were "Classical lumps and their quantum descendants", in which he described the sine-Gordon equation, its doublet breather solutions, and its equivalence to the massive Thirring equation. At the end of those lectures, he wrote:
"… This has been a long series of physics lectures with no reference whatsoever to experiment. This is embarrassing. …".

Sidney Coleman's embarassment at physics stuff with "no reference whatsoever to experiment" reminds me of Richard Feynman's feelings about superstrings.

With respect to "Classical lumps and their quantum descendants", Sidney Coleman went on to ask "… Is there any chance that the lump will be more than a theoretical toy in our field? …", and mentioned a couple of possibilities, one of which was "… that there will appear a theory of strong-intereaction dynamics in which hadrons are thought of as lumps …". My effort at trying to fulfill that possibility is at http://www.valdostamuseum.org/hamsmith/sGmTqqbarPion.pdf

Whether or not Sidney Coleman's lumps become "more than a theoretical toy",

Sidney Coleman's basic instinct that "… physics … with no reference whatsoever to experiment … is embarrassing …"
is consistent with
superstring guru Jacques Distler's statement "Sidney was not interested in string theory; he wasn't even particularly interested in supersymmetry.,
and
is also consistent with Richard Feynman's view of superstrings.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

52. **Bert Schroer**
June 3, 2006

Thanks, Jonathan, this time I saved your information.
My superficial impression is that at the time of Feynman this was within the usual antagonisms and rivalries in those days. We should not forget this was before the days when this area of particle physics turned into the direction of hegemony and when certain physicists turned salesman (i.e. in particular nicking away the jobs which you are supposed to be doing Jonathan). In the present "Weltmacht oder Tod" (using Chris Oklay's Endsieg terminology) or in my less radical "hegemony and death to the others" characterization one is inclined to see such ealier aphorisms of Feynman slightly distorted.

53. **FT reader**
June 3, 2006

A review from the Financial times (subscription required):

Nothing gained in search for 'theory of everything'
By Robert Matthews
Published: June 2 2006 19:45 | Last updated: June 2 2006 19:45

They call their leader The Pope, insist theirs is the only path to enlightenment and attract a steady stream of young acolytes to their cause. A crackpot religious cult? No, something far scarier: a scientific community that has completely lost touch with reality and is robbing us of some of our most brilliant minds.

…….

Those who have show signs of having fallen prey to the “sunk-cost fallacy”, the huge intellectual effort needed to enter the field compelling them to plough on regardless of the prospects of success. It is time they were put out of their misery by being told to either give up or find funding from elsewhere (charities supporting faith-based pursuits have been suggested as one alternative).

Academic institutions find it hard enough to fund fields with records of solid achievement. After 20-odd years, they are surely justified in pulling the plug on one that has disappeared up its Calabi-Yau manifold.

The writer is visiting reader in science at Aston University, Birmingham

54. **FT reader**
June 3, 2006

The link to the FT review:

http://news.ft.com/cms/s/a482e470-f264-11da-b78e-0000779e2340.html

55. **sunderpeeche**
June 3, 2006

A word of caution about all this. When the SSC was cancelled, some people expected that the funding would go to other fields, e.g. condensed matter physics, etc. Instead there was a general cut across the board ~ “if the HEP community can lose 8B funding, other fields can survive a hit too”. Do not think that if funding for string theory is cut, that the money will go to “other” HEP theory. It will simply disappear. Funding is not a zero sum game where the money must go “to someone”.

56. **knotted string**
June 3, 2006

Sunderpeeche,

Consider the speculation and hype a bit like a cancer. If you delay treatment, you can avoid the unpleasant side effects a short while.

However, you are then increasing long-term problem. Far better to straighted out problems before it is too late.

The funding of bigoted, close-minded string theorists may or may not need to be cut to preserve the integrity of physics as a whole.

If they would only cease to be such a dictatorial menace to physics, such drastic treatment would be unnecessary.

57. **Dan**
June 3, 2006

“I find it totally pathetic that those people who imposed on others for decades the story about the fundamental aspects of “the gauge principle” in QFT when QFTists with a bit higher developed conceptual power knew that this is one of those metaphoric description did not want to spend their energies removing holy cows (nothing intrinsic, since there was never any autonomous structural property which could reveal that allegedly gauge invariant observables where coming from a “gauge” theory) now have to ram into our throats (via their jester) that after all this was not so (a kind of April fool’s joke of very long duration).”

Dear Bert,

the gauge principle concisely summarizes the interactions even if it is not quite unique. This clear organizing principle allowed us to nail down the standard model. Later on we learned, in the context of SUSY gauge theories, that there can be serious ambiguities in such a description. Seiberg duality is the standard example of this. If you “knew” this all along, why did you leave it to Seiberg to give a convincing example? I dare to guess that it’s because you weren’t able to do it.

Please tone it down a notch or two. You sound like an angry teenager when you write your rambling complaints.

Best wishes,
Dan

58. **sunderpeeche**
June 3, 2006

Believe what you will about string theory and funding, and about the integrity of physics as a whole. I presume that “physics” once again means the equating of HEP theory to all of physics. The solution lies in expt HEP finding something beyond SM. And only Mother Nature has the answer to that.

59. **MathPhys**
June 3, 2006

Dan,

Can you explain in a couple of sentences how Seiberg duality is an example of the ambiguity of gauge theories?

Thanks in advance.

60. **Dan**
June 3, 2006

Dear MathPhys,

For a factual statement of what Seiberg duality is, see

http://en.wikipedia.org/wiki/Seiberg_duality

The point is that theories with different gauge groups are dual to one another, that is, they provide different descriptions of the same physics. In other words, the gauge symmetry may not be uniquely determined by the underlying physics.

61. **sunderpeeche**
June 3, 2006

I'm not Dan, and this is only one sentence, and I'm being a busybody, but try this http://en.wikipedia.org/wiki/Seiberg_duality

62. **MathPhys**
June 3, 2006

Thanks to both of you. I'm (a bit) familiar with S duality. I'm just not so sure that allowing for dual descriptions is the same thing as an ambiguity.

But thanks again.

63. **Dan**
June 3, 2006

Two theories are dual if, despite different formal content, they describe the same physics. A duality between theories with different gauge groups *is* therefore an ambiguity in the gauge group — you can equivalently choose either theory, and hence either gauge group, to describe the same physics.

64. **MathPhys**
June 3, 2006

Agreed.

65. **Thomas Larsson**
June 3, 2006

Even if there is an ambiguity, the gauge group is pinned down up to a discrete, and small, number of choices. Besides, I think that Seiberg duality only works for SUSY QCD, so for e.g. the SM the gauge group is unique, and in fact fixed by its global part, the charge assignment.

66. **anon**
June 4, 2006

Woit,

I notice that Lubos Motl's Amazon review claims:

"The book contains a lot of very embarassing errors."

So both you and Lubos use spell embarrassing with a single "r". Do you share an entangled mental state? That would be real reason for embarrassment.

Or did you write the nonsense review yourself and bribe Lubos to put it on Amazon to stir up some controversy and boost sales?

If you seriously want to drop a letter r from a particular word, string. "Sting theory" sounds really cool.

67. **Mentos**
June 4, 2006

It should be pointed out, in all fairness, that Seiberg duality is a duality of the infrared (long-distance) physics. At short distances (high energies), the Seiberg-dual gauge theories are clearly distinguishable. This is in contrast to the S-duality of N=4 supersymmetric gauge theories, which is an exact duality at all length scales.

The difference, really, is the difference between asymptotically-free theories and conformal ones.

68. **MathPhys**
June 4, 2006

So you are saying that Seiberg duality is *not* S duality.

69. **Chris Oakley**
June 4, 2006

anon,

Peter is obviously way ahead of all of us. Lubos Motl does not really exist, being a persona that Peter has created in order to boost sales of his book (I wonder … maybe *I* could do something … let me think …)

70. **secret milkshake**
June 4, 2006

Lubos exists. He is an alien bot, constructed to probe human reactions.

71. **Chris Oakley**
June 4, 2006

ZZRRRFKLG (Lubos's controller): Give me one reason, *one reason* why I should not bust you back down to teaching arithmetic to pre-school kids? I sent you to Earth to set back their science fifty years or more, and it looks like they've rumbled you already!

LUBOS (*grovelling*): But master, surely you appreciate the ingeniousness of my feigned opposition to the Anthropic Landscape?

ZZRRRFKLG: It's not enough, Skrrzlgk [NB: his real name]. You did not take account of that asshole Peter Woit! It looks like people are going to take his book seriously! And what if the best mathematical brains on that pathetic planet start doing science again? *Then* where will we be?

SKRRZLGK: It was *your* fault! You would not let me use the Quantum Entanglement Ray on him! I *told* you he was dangerous!

ZZRRRFKLG: Do not question my wisdom, fool! You know nothing. And what's with all this political crap on your web site? Stick to the primary mission or your next assignment will be in the Kindergarten!

72. **anon**
June 4, 2006

Chris,

If you look closely at Lubos Motl's blog you can see he is now saying even Dr Matthews is

"Robert Matthews: science-hater par excellence"

Dr Matthews has done possibly more to support science than any other journalist in the UK.

Motl states: "A senior physicist has sent me a piece of text that he or she called ' tendentious, malicious attack on scientists and through that on science itself'."

Who is a senior physicist to Dr Motl? Someone deluded, that's for sure. People who hate Feynman's objectivity so much as Dr Motl and try to mix gibberish with personal attacks while standing behind the cover of Dr Motl are very respectable IMHO.

Or perhaps nobody warped is hiding behind Dr Motl, and he is attacking British SCIENCE reporters off his own back. I think this is the case. Ed Witten and Lisa Randall would NEVER be so cowardly, they have more integrity than that, and don't behave this way.

73. **Chris Oakley**
June 4, 2006

Well, I suppose that with the prospect of teaching arithmetic to alien infants, he is getting desperate.

74. **Nigel Cook**
June 4, 2006

I have complete great respect for Dr Matthews scientific reporting. He has easily done more for science in the UK than the entire stringy brigade put together. That's a real accomplishment.

So Lumos is being silly or paranoid as usual. Remember the article which Lumo wrote about Quantoken, painting him as a clown?

http://motls.blogspot.com/2006/01/meeting-quantoken.html

75. **sunderpeeche**
June 4, 2006

Methinks another set of posts are going to get deleted.

76. **kristo**
June 4, 2006

I must say, this post is a welcome distraction from my stuying for exams. Besides being again *enlightening* as to the way some high ranking academics interact socially, it has given me quite a laugh.

Unbelievable.

77. **Bert Schroer**
June 4, 2006

Dear Dan,
This is clear ever since Schwinger argued that there is a massive version of QED (he thought of what would be called in modern terminology composite Higgs). Of course he knew that he had no mathematical control over 4-dimensional gauge theories (and nobody has up to this date) and in order to be somewhat convincing he invented the Schwinger model. Somewhat later Lowenstein and Swieca came up with quite an ingenious paper in which they showed by a very balanced conceptual-mathematical presentation that the physical content is nothing else than a two-dimensional massive free scalar field. And lo and behold the situation can be inverted, the free massive scalar field really reproduces asymptotic freedom; it is the only free field which does this (look up my long paper from last year on two-dimensional QFT… in AOP or hep-th where this cute conceptually quite nontrivial point point is explained in detail). There are many more controllable model where this can be rigorously established. If you only understand things in a much less controllable 4-dim. Euclidean settings, good for you, but allow me to say that I have difficulties to follow Euclidean consistency arguments which are so far away from conceptional+mathematical control. But please do not claim that mathematical physicists got to know about the metaphorical meaning of the gauge theory setting through the Seiberg-Witten duality. But as I said, if you need a Seiberg-Witten Nanny do become aware this is o.k. with me but don´t complain that before there were no Nannys around.
By the way, the Schwinger-Higgs mechanism is (differen from the Goldstone spontaneous breaking of continuous symmetries) valid also in low spacetime dimension.

78. **Bert Schroer**
June 4, 2006

Thomas Larson,
there is no contradition between the metaphoric QFT aspect of the word "gauge" and the extreme usefulness of the calculational gauge theoretic scheme which allows us to extract all the rich information from the action for the standard model. But I think it is a commonplace by know to expect that this whole present setting will change in the future and probably not just by additional modification here and there, but rather by a very radical reformulation on the conceptual side. In the meantime we should be very very satisfied of having the present rather powerful recipe.
To enhance my point, I direct your attention to the surprizing (in retrospect) efficiency of the old Bohr-Sommerfeld theory. It is very beneficial that we already can agree on the metaphorical aspects of "gauge" because that gives us the conviction to look for a different description where (as outside of gauge theory) the principle: something which looks like an elephant really is an elephant is re-established.

79. **woit**
June 4, 2006

Sunderpeeche,

Everything surrounding Lubos is so surreal I can't figure out how to separate sense from nonsense here and moderate this. Someone recently wrote to me that the string theory story would make a great comic novel in the vein of David Lodge. I feel like I'm already living in a comic novel….

Dan, Lubos,

Back from a trip and just deleted a bunch of comments, including yours. I really insist that this is my weblog so you have to stick to insulting me and not others.

80. **Dan**
June 4, 2006

Sure, Peter Woit. [Attack on someone else deleted]. You are a pathetic loser.

81. **Lubos Motl**
June 4, 2006

Dear Peter Woit,

I noticed that you have erased the only meaningful comments on this page – especially those of Dan (and mine), including the comments of Dan that Bert Schroer "responds" to.

You're like one gigantic KGB that protects the leading role of aggressive crackpots with IQ around 75 in the society. You're a good reason to vomit. Incidentally, my lawyer is telling me that we should sue you for all the lies that you are writing about me. I am telling her that it does not matter because your comments are only taken semi-seriously by the human junk that dominates your blog, but she insists that we should act. So be ready.

All the best
Lubos

82. **runge_kutta**
June 4, 2006

Actually Lubos is right most of the time. The only problem is his abrasive and arrogant attitude. It's so ironic and puzzling that someone as smart as him could be so arrogant. Usually, people as arrogant as Lubos are really just fools. I guess Lubos is the odd exception to the rule.
His attitude doesn't reflect his intelligence, and reading his rants, you'd never know he was a harvard professor (ok, assistant professor, whatever), you'd think he was some random retard. I think his attitude can be traced back to his childhood. Some lack of validation that he tries to make up for as an adult, who knows ?

83. **woit**
June 4, 2006

Lubos,

If you have any specific complaint that something I wrote about you is a lie, do let me know. Has your lawyer taken a look at your own weblog and what you write in comments here before providing you legal advice?

Dan (formerly Michael),

Please stop anonymously submitting the same insult repetitively here. If you don't stop behavior like this I'm going to complain to your colleagues at UMass and Brandeis about this.

84. **JC**
June 4, 2006

It would be very surreal to see the validity of string theory being fought in the legal court system. It sounds almost just as amusing as somebody suing McDonalds for spilling hot coffee on themself.

85. **Thomas Larsson**
June 4, 2006

Re legal matters, perhaps Dan/Michael should ponder this:

"Annoying someone via the Internet is now a federal crime.

It's no joke. Last Thursday, President Bush signed into law a prohibition on posting annoying Web messages or sending annoying e-mail messages without disclosing your true identity.

In other words, it's OK to flame someone on a mailing list or in a blog as long as you do it under your real name. Thank Congress for small favors, I guess."

86. **JC**
June 4, 2006

Thomas,

A new "law" of that sort has to survive the legal process, particularly on constitutional grounds, if it's to become the law of the land in America. Otherwise it's just something that's written on a piece of paper with very little to no legal meaning.

87. **Mentos**
June 4, 2006

MathPhys asked:

"So you are saying that Seiberg duality is *not* S duality."

S-duality is an exchange of weak- and strong-coupling regimes of two theories.

For a conformally-invariant theory (like N=4 SYM), such a notion makes direct sense because the gauge coupling constant is really a constant.

In an aymptotically-free theory, what you have is a scale-dependent running coupling constant. So, if I said to you, "the Seiberg-dual theories are related by an exchange of weak and strong coupling," you should retort, "The couplings defined at what scale?"

The answer is, "the couplings in the far-infrared."

In the UV, both theories are weakly-coupled, and clearly have different weakly-coupled degrees of freedom.

A better name for Seiberg duality is "universality." Two different gauge theories have the same infrared physics. But, since it is closely related to S-duality (which I'd like to reserve for theories that are dual at all length scales), "duality" is the name that has stuck.

88. **anon**
June 4, 2006

When will it be decided whether Lubos gets tenure at Harvard? I hope he gets it. Let's give credit where credit is due. No man (sorry Peter Woit, but you are not even close) has done so much to sink the reputation of String Theory than Lubos Motl.

89. **island**
June 4, 2006

> In other words, it's OK to flame someone on a mailing list or in a blog as long as you do it under your real name.

You've got to be kidding me, I was just about to comment on Peter's stinky feet.

And somebody needs to warn Uncle Al, ASAP… 😉

90. **sunderpeeche**
June 4, 2006

Delete the entire thread, except perhaps the ones about duality.

91. **island**
June 4, 2006

Peter, I plugged your book and Lee's on SPResearch, but Phillip is sleeping-in…

92. **sunderpeeche**
June 4, 2006

String theory ("cancer") ~ threat to HEP theory ~ threat to all physics ~ threat to civilization itself.

Mon Dieu im Himmel.

Toss the lot.

93. **island**
June 4, 2006

No, there is a clear and critical difference …

…between understanding that every unproven and projected assumption that has ever been "carried" is always up for review at any time given new physics, until a true ToE or at least a "hard" theory of quantum gravity is produced… then maybe…

…and between taking assumptions and projections for granted to the point of the kind of fanaticism that embraces the ensuing world-view that this entails as if it were real!

Nothing need be tossed when the "hype" isn't passed-off as something greater via a rallying of the masses and politics, over new physics.

I think that it's sad though that it may take the collapse of quantum gravity physics to wake people up to the fact that the conservative approach to resolving old unfinished issues isn't the fringe, rather, the cutting-edge which is OUT THERE.

I think that the whole career commital thing is critical to all of this, and not in a good way for science.

94. **Benni**
June 4, 2006

what I think is interesting on Lubos is that his behaviour is similar to what's under psychiatrists called Aspergers Syndrome:
http://www.udel.edu/bkirby/asperger/
Individuals with AS can exhibit a variety of characteristics and the disorder can range from mild to severe. Persons with AS show marked deficiencies in social skills, have difficulties with transitions or changes and prefer sameness. They often have obsessive routines and may be preoccupied with a particular subject of interest.
it's important to remember that the person with AS perceives the world very differently. Therefore, many behaviors that seem odd or unusual are due to those neurological differences and not the result of intentional rudeness or bad behavior, and most certainly not the result of "improper parenting".

By definition, those with AS have a normal IQ and many individuals (although not all), exhibit exceptional skill or talent in a specific area. Because of their high degree of functionality and their naiveté, those with AS are often viewed as eccentric or odd and can easily become victims of teasing and bullying. While language development seems, on the surface, normal, individuals with AS often have deficits in pragmatics and prosody. Vocabularies may be extraordinarily rich and some children sound like "little professors." However, persons with AS can be extremely literal and have difficulty using language in a social context.

Asperger called them "little professors". It is likely that Dirac was for example a classic case.

This here is about a fields medaillist (Richard Borcherds) with Aspergers:
http://leitl.org/docs/a-professor-of-mathematics.pdf

95. **woit**
June 4, 2006

Please, enough about Lubos's eccentric personality. For the record, it seems to me to have nothing to do with Asperger's.

96. **Ponderer of Things**
June 4, 2006

not to focus on Lubos, but does anyone think that string theorists out there (the reasonable kind) see the arrogant, obnoxious and "below the belt" attacks of Lubos as helpful to their cause? Do they even know/care? It seems like as far as blogs and internet is concerned Lubos is some sort of self-appointed strings guru, at least to regular masses. I wonder how many new graduate students reading blogs, including this one, want nothing to do with string theory – not because of Peter Woit's or Lee Smolin's critisism of the strings, but because of folks like Lubos.

97. **Mentos**
June 4, 2006

So you would base your career decisions on the content of Peter Woit and Lubos Motl's blogs?

Wow.

98. **woit**
June 4, 2006

Ponderer,

I doubt that most string theorists see Lubos as helpful to the cause of string theory, quite the opposite. However I continue to find it remarkable that essentially none of them (Aaron Bergman is one exception) are willing to be seen criticizing him and his behavior. I'll avoid speculating here about why this is.

Mentos,

He explicitly said that graduate students like him aren't much influenced by me, but are influenced by seeing the behavior of Lubos (and folks like him, try the next most well-known string theory blogger, Jacques Distler). Would you want to enter a field that appeared to be happy to be represented by people like this? (Going back to previous point, that the rest of the string theory community gives no indication of having a problem with Lubos or Distler).

99. **Anon-e-mus**
June 4, 2006

Lubos' behavior isn't similar to the typical behavior of Aspergers' people at all. No, he is a classical case of Narcissist Personality Disorder. Just look at all the times he points out that his blog is allegedly the "best" according to google, that he has more google hits than some leading politicians and so on and so on. An Aspie wouldn't care much about that kind of outside confirmation, but Lubos really, really seems to need it: typically narcissist behavior. Self-praise disguised as praise by others, extreme dependence on outside confirmation of superiority, just like the need to put others down, ridicule others etc., all typical Lubos behaviors, all typical of narcissism. Lubos is a narcissist, not an autistic (he may have some mildly autistic traits as well, but some of those are actually fairly common among mathematicians/theoretical physicists).

100. **Peter Woit**
June 4, 2006

People looking for another blog where they can watch Lubos promoting string theory and argue with him might want to look at this:

http://blogs.chron.com/sciguy/archives/2006/06/which_of_these_1.html

101. **Sam**
June 4, 2006

"try the next most well-known string theory blogger, Jacques Distler"

Yeah, Distler's blog is a cesspool of ad hominem attacks and lunatic ravings about the enemies of string theory and mathml.

102. **woit**
June 4, 2006

Mentos=Sam (if you're going to hide behind a pseudonym, maybe it should just be one),

I wasn't so much thinking of Distler's blog postings, but what happens if you disagree with him. You don't see that very often these days since, for whatever reason (maybe because the way he handles mathml no one can read his blog on their browser), virtually no one writes comments there. What I had in mind was more what people who deal with him soon notice, see

http://utphysguide.livejournal.com/3047.html

"Jacques Distler is quite possibly the worst physics professor I have ever had. He has the uncanny ability to make even the simplest concepts utterly incomprehensible. He is a true intellectual snob, and he treats most questions with open hostility. Unless you have a PhD in math and already know string theory, you will not learn anything from Distler. String theory is hard, but not as hard as Distler wants it to be."

Not exactly something that would encourage a student to go into his field, no?

103. **Thomas Reasoner**
June 4, 2006

This has been some interesting reading. I just have one question, and I'm sorry if it's a little off-topic: what's the alternative to string theory? I only have a BS in Electrical/Computer Engineering, so I'm just starting out in this great field. I want to go into cosmology and work on a unified field theory, but I don't want to work on string theory. Is there an alternative for someone like me? This is all I want to do with my life, and I'd prefer not to waste my energies on science-fiction, so any advice would be appreciated. Please email me at thomas.reasoner@gmail.com so as not to get too far off-topic from the post.

104. **woit**
June 5, 2006

Thomas,
It's not that people who are working on string theory are ignoring some obviously better alternative out there. No one has any really good ideas about unification at the

moment, making it a tough field to go into. Given the situation, the choice may be to work on something which is pretty surely science fiction, or to try and come up with something new yourself, which is very hard.

105. **JoAnne**
June 5, 2006

Thanks All, for a most entertaining read! Made me laugh out loud. Peter, no choice but to get your book now.

And, Thomas Reasoner, the alternative to string theory is particle theory. We build theories and fit them to data. If actual data excludes them, they get tossed out. We have many exciting puzzles to solve – the mechanism behind electroweak symmetry breaking, the hierarchy problem, what is dark matter, why are neutrinos massive, what is the origin of CP violation, etc. We have data to confront these puzzles, and our theories, and will soon take a giant leap in our understanding with the LHC. Only Lubos will be left to care about string theory then…

106. **Chris Oakley**
June 5, 2006

The Robert Matthews article in full.

RM studied Physics at Oxford and was in the year below me (1978). What with that Oxford man Penrose having facilitated the publication of Peter's book, I doubt that Superstring Theorists will be queueing up to make donations to Oxford University.

107. **PeterG**
June 5, 2006

As a physics grad student, I must admit I'm finding this controversy regarding string theory extremely interesting, but the manner in which it is being played out as being emabarrassing to all in the field. As scientific professionals there is a definite need for expressing opinions and to challenge other's thoughts, conjectures and theories, but in a respectful manner. Debate rather than intimidation and insult is surely the correct approach. Unfortunately I have now seen the latter in the attacks on Smolin by Susskind and now Motl on Woit.

As to the question of whether or not Mr Motl would convince me to look in another area of physics other than string theory – the answer is no and yes. No, as I am willing to ignore the individual to study the theory, but also yes, as I am yet to hear the leaders of the field criticising his personal attacks on those respected scientists that are providing a challenge to these stringy theories. After all, in science no good theory should go unchallenged.

As I am only a grad student and not a professor with numerous published papers, I will be no doubt seen as an 'idiot', a 'crackpot' or not yet worthy of a comment – all I can say is that out there is perhaps another grad student that some day makes the breakthrough that those condemning them have been unable to do. For me I think I'm going to focus on QCD.

I look forward to reading the book.

PS … if the only reason for not reading it is some typos then I best throw away every text book I have ever had!!!

108. **Chris Oakley**
June 5, 2006

Peter G,

1. Thank-you for providing yet another spelling for the word "embarrassing".
2. You should not consider the Harvard Hammer of Heretics as being representative. Even Jacques Distler is capable of (icy) politeness with critics of ST.

Peter W,

I just bought a copy of your book from Blackwell's in Oxford. Like the aforementioned hammer, I therefore have to call you a liar as I seem to remember you saying that it would not be in bookshops until June 16.

109. **Tony Smith**
June 5, 2006

Thomas, you ask " what's the alternative to string theory? ".

Peter replied
" No one has any really good ideas about unification at the moment …"

and

JoAnne said
"… the alternative to string theory is particle theory.
We build theories and fit them to data.
If actual data excludes them, they get tossed out. …".

I have an alternative physics model that fits "actual data" of particle physics.
A version of it can be found in pdf form on the CERN web site as EXT-2003-087 at
http://cdsweb.cern.ch/search.py?sysno=002414216CER
and also on my web site at
http://www.valdostamuseum.org/hamsmith/TQ3mHFII1vNFadd97.pdf

Further details can be found on the parts of my web site at
http://www.valdostamuseum.org/hamsmith/
that deal with physics, and with the physics parts of my 4 MB pdf web book at
http://www.valdostamuseum.org/hamsmith/BANNEDbyCORNELL.pdf

Note that the web book material includes calculations of
the ratio Dark Energy : Dark Matter : Ordinary Matter, related to JoAnne's question "what is dark matter"
and
neutrino masses and mixing angles, related to JoAnne's question "why are neutrinos massive".

I am sending a copy of this message to Peter Woit because I have,
so far, tried to be very observant of his wishes that I do not
discuss my physics model on his blog, and stay strictly on-topic
(which is mostly to attack superstrings without advocating any alternative).

However, I am very upset about Peter's comment
" No one has any really good ideas about unification at the moment …"
because
he does know about my physics model
and
his statement is a direct attack on it,
just as direct as are his attacks on superstring theory.

Of course, Peter is entitled to his opinion about my model,
and he is entitled to attack it on his blog,
but
since he has attacked it,
I am also posting this as comment on his blog in direct reply
to his attacking comment.
In fairness, I expect that this comment should NOT be deleted
UNLESS Peter's statement
" No one has any really good ideas about unification at the moment …"
is also deleted, and not repeated.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

110. **RahulM**
June 5, 2006

PeterG: It is difficult to debate intelligently in the absence of experimental data. Hence the insults. It's quite fun actually.

111. **woit**
June 5, 2006

Chris,

Thanks for the news. I don't know why at one point recently someone at Cape was telling me June 16th would be the release date.

Tony,

I was not in any way attacking your ideas. The ideas about unification that have attracted attention in the particle theory community are the ones I was referring to as not being successful. Lots of people have ideas about unification (I have my own) which haven't attracted such attention. Maybe one of these will turn out to be very important.

Please though, Thomas politely gave his e-mail to encourage people to discuss ideas about this with him privately, not here where it is off topic.

112. **Juan R.**
June 5, 2006

There is an interesting duality between Distler's wisdom on string theory and mathml.

Distler surrounds both in a sphere of dogmatic true, only-way-to-do-things and *ad hominen* attack to no-believers. Distler's arrogance on string theory is only comparable with his arrogance on Internet technology. He claims his blog is the most advanced of planet somewhat as we heard each day that string theory is the most advanced theory!

This point is very important. Distler, as other string physicists (except some as Nanopoulos working in more advanced stringy approaches or particle physicists as Gell-Mann working in generalized QM), states that string theory is the more sophisticated theory of planet when is a pure joke if you compare with other sophisticated approaches. Moreover, the ultra-advanced string theory is unable to duplicate good experimental result provided by SM or GR since decades.

Similar criticism can be done in the technological side of Distler's insane blog. He states, in a very proud way, the ultra-sophisticated technology (a simple trivial p-MathML 2.0 via inefficient plugin) of his blog, still he is unable to encode material can be encode via old HTML.

In the same way that Distler is unable to compute Mercury anomalous perihelion from first principles of string theory (but can be computed from old but effective GR) Distler is unable to encode (ds)^2 using his xml plugin distler/blog/.

Observe the first block-display math on *Designing the 5th Dimension*. Instead encode (ds)^2 he is writting 2s ds!!! A simple old HTML 3.2 code can do it better

Note the parallelism physics-technology!!

Beliefs on Physics:
– String theory is the most sophisticated theory in the world.
– String theory is the only way.
– String theory can solve any problem of physics: SM parameters, CC, unification forces, quantum gravity…
– Other theories (e.g. loops) are stupid and a person who is not string theorist may be not smart enough.
– Attack to any competitor of the theory (e.g. impede arxiv's link to Woit blog).

Beliefs on Technology:
– MathML is the most sophisticated markup in the world.
– MathML is the only way.
– MathML can solve any problem of internet: accessibility, mathematical rendering, automated search, semantic encoding…
– Other markups are stupid and guys do not developing MathML are nor smart enough.
– Attack to any competitor of MathML or of Distler plugin (e.g. hard criticism to client side ASCIIMath approach).

Results on Physics:
– String theory is not sophisticated enough as claimed in many places (including smart guys as Gell-Mann).
– String theory is the only way to disaster.
– After 40 years of string theory, it has not solved any of promises and is unable to offers results can be easily achieved with old theories as SM and GR.
– Other theories are producing interesting results and string theory often copying them (even Witten recognizes that string theory has a tendency to embrace ideas of alternative theories).

Results on Technology:
– MathML is not sophisticated enough as claimed in many sites (e.g. OpenMath authors are highlighting MathML errors).
– MathML is the only way to disaster.
– After 10 years of MathML, it has not solved any of promises and is unable to offers results can be easily achieved with old markups as HTML and ISO12083.
– Other markups are producing interesting results and recently MathML authors triyng to copying them (e.g. R. Miner asking for XML-MAIDEN way to encode scripts for debate in future MathML 3.0 specification).

Peter Woit, physics is not the only victim of those insane guys who do not know that **scientific method** means.

Juan R.

Center for CANONICAL |SCIENCE)

113. **Bert Schroer**
June 5, 2006

In this blog I would like to take the opportunity to dispel some misconceptions. I am not against string theory per se as one area of possible research in particle physics. What worries and annoys me at times are rather its new sociological aspects, the hegemonial role it has allocated to itself, the enormous collateral damage it causes (rapid loss of basic knowledge in favor of doubtful substitutes), the seducing role (raising false expectations) its proponents excert on the young generation (some called it brainwashing) and last not least the total lack of self-reflection and criticism which used to distinguish science from other human activities in the socio-political sphere. But in no way I want to downgrade colleagues and endanger old friendships; when I mention names it is in their role as iconic figures in this dispute. Fortunately science still permits to do this. In the present sociological situation one is well-adviced to follow Hirzebruch's distinction between physics and "what physicists are doing".
It is not (as Chris Oakley said) that I went to South America to get away from the radar screen of string theory. There is also string theory in Brazil and I attend the talks of my string theoretic colleagues and ask question and get answers (in most cases honest and straightforward). It is true that I did have some problems with one particular person
http://rivelles.blogspot.com/2006/03/ideology-sociology-and-psychology-of.html
But this has little to do with the fact that he is a string theorist, but rather with his inability to make use of that amount of visibility which even according to Weinberg's calculation of the cosmological vacuum energy he would be intitled to have, see
http://br.arxiv.org/abs/hep-th/0605054 page3
I meanwhile arrive in Sao Paulo waiting for the dental repair. Two days before I left Arraial do Cabo (the 1503 landing place of Amerigo Vespucci, originally a small fishing village but now the diver's paradise) there was a street festival where I live. The day before some youngsters came to me and asked for small contribution to decorate the street. In the evening there was a lot of fire-cracker-like noise and when I looked outside I saw an almost continuous lightening with sparks raining down onto the parked cars. In the process of fixing all the paper flag decorations the youngsters hit the main electric cable (ouside big cities the electric and communication wiring is hanging completely exposed exactly as in the US). There was a blackout for the rest of the night. The next day I saw the first paintings on the walls. They started with that inexorable iconic figure of a bearded teutonic type in Lederhosen with a filth heat carrying a feather. In one of his hands, how could it be otherwise, he was swinging a jug of beer. It was clear, this was a warm up to the world cup soccer season. They wanted me to identify with this painting and it did not help much to point out that even if you go down to Bavaria, figures like this have become almost as rare as the abominable snowmen. I told them its iconic just like Ze Carioca and Carmen Miranda with the consequence that the next day their wall paintings were enriched by a very large Ze Carrioca (a parrot who talks) and a mulata adaptation of Carmen Miranda. The patriotic feelings of these rather poor people had something contagious and they would have been extremely confused if I would not have taken up a patriotic fan position in favor of the German time. I really feel great because with the German Nationalmannschaft and the Brazilean seleçao in my family (my wife is Brazilean) I can hardly loose. As most Germans of my generation I naturally have a rather detached relation to matters of national pride, which is natural in view of the past Germany's disastrously deep look our into that bottle containing that dangerous brew called nationalism. The football outing of patriotism is the best one, as long as it does not spill into hooliganism.and racism.
In retrospect I am also quite grateful that Germany had a rather wise leadership in difficult times with tremendous political coercion. The thought that German troops man checkpoints in Iraq and mow down families because they do not understand orders in German would turn my stomach around. Lidice, Ouradour and all the massacres committed in Italy are not deleted in the memory of mankind.
Having lived in Berlin and in the US, I find Brazil a very very different place. In the US I met Palestinean refugees who had very bitter feelings about Israel and Israelis, and the Israelis in Berlin who did not hate Palestineans and considered them as inferior were the exception and not the rule. But here in Brazil I have in my family (from my wife's side) Jews, Arab descendents of people from Syria-Lebanon (which was split up by the colonial powers) and people of Portugese extraction living harmoniously together. Most of the Arabs and some of the Jews came at the time of the decay of the Otoman empire. One of my wife's relative with the illustrous name Canetti (a relative of the famous writer and philosopher Elias Canetti) was actually born in some region which is now Bulgaria (he is 96). Talking to him I had to correct some of my misconceptions about the Otoman rulers being very oppressive (I thought about the massacre of the Armenians). Its seems that as the US has profited from the dissolution of the Habsburg empire, Brazil got most of the migrants who were running away from the uncertainties of the dissolving Otoman empire.
I also recently visited a Jewish friend (from the times with the collaboration with Swieca) in Petropolis. He has two rooms filled with art deco and body sculptures which I link with the Nazi era, although I have seen similar sculptures in Italy and even in the US (from the time of the New Deal). But he also had some SS militaria in his collection. I was quite stunned and asked him what makes him collect such things. He gave me a convincing answer by saying: "look, they are all dead and I survived". His parents fled with him from Rhodos shortly before the Nazis deported the whole Jewish population and executed them. The reason I mention this is because 2 days ago when I looked into the news on the internet I noticed that there is a ferrocious dispute about the removal (or at least covering) of some sculptures at the entrance of the Berlin 1936 Olympia stadion (which were commissioned by the Nazis). Fortunately Stephan Kramer, the chairmen of the Jewish council in Germany spoke against such a strange act of exorcizing history which is usually demanded by some members of the older generation..
I of course would like the German team to win, and I would be happy if it is merited and not just luck. But what would make me even more happy is if those youngsters which are descendends of more than 100.000 Jews (mostly from the former SU) would be among the strongest fans for the German national team (in defiance of all those Nazi skin heads and racist hooligans). Because then, and only then, I would know that the postwar era has finally ended and a new chapter has been opened.

114. **Juan R.**
June 5, 2006

Bert Schroer,

We usually distinguish between string theorists and stringers.

String theorists are physicists/mathematicians who are pursuing a research program for unification of physics (which is a loable program either if sucesfull or not). String theorists are honest, recognize the limitations of their program and obvious failures done in the past. They are the first ones interested in experimental verification of the theory (or at least of part of their foundation) and you can talk or discuss with them in a solid debate where just technical points are of interest.

Some of those also recognize that even if string theory is achieved some day it will not become the TOE, since nature is more complex. Those string theorists understand the need for open debate on hot topics just how they understand the need of the research in alternative approaches.

Now turn to stringers (there is one very popular at this thread).

Real impact of stringers in physics or maths is close to zero, none of them have received a Nobel or similar prize. Stringers are of people more dishonest we know, never recognize limitations and have a tendency to rewirte history of the field for hiding the obvious failures done in the past.

Stringers dislike experimental verification by two motives: i) it could break down string theory, ii) experiments are considered a task for second or third class physicists when compared with beautiful of theoretical studies of divine origin. Remember that stringers claim that "*string theory is the language God wrote the world*".

Stringers do not like technical debate and just focuse on *ad hominen* attacks or embarrassment, and prefer to add noise to discussion. Their knowledge of topics beyond particle physics is so weak that still believe on that they are developing a TOE. (Yes i agree some stringers lack understanding of most basic of particle physics).

Finally, I would add that stringers spend lot of time on deviate funding from alternative approaches and spend many time on political strategy with the only aim of self-perpetuate string theory for ever (independently if is working or is a "colossal failure") how all of us know.

Juan R.

Center for CANONICAL |SCIENCE)

115. **B**
June 5, 2006

when I went to college I learned two things:

a) don't mix up professional and personal matters.

It should be possible to discuss physics without retreating to insults that don't get anybody anywhere. Exept maybe Lubos on the blogtopsite no #1.

Since I haven't read the book, and have a very shared opinion about String theory, let me just state that merely from reading the blogs of Mr. Woit and Mr. Motl, to me BOTH apparently have a large knowledge about String-theory as well as about particle physics. If it weren't for blogs triggering fast and unreflected responses that get much more attention than the scientific content, their blogs could be quite an important contribution to the community (meaning phycisists in general).

Students that choose their field of research from comments like the ones above (regarding alien bots or reasons to vomit), are problably students you can happily forget about anyway.

I also learned

b) Keep my big mouth shut.

With which I apparently have failed again.

Best, B.

PS: The whole discussion about **G** or **T** is completely ridiculous. Everybody who knows what a TeV is will know that the LHC operates at 14 TeV com energy. For those who don't know what a TeV is, it doesn't matter anyway. I have certainly published more embarrassing typos, and the world is still turning.

116. **Christine Dantas**
June 5, 2006

We usually distinguish between string theorists and stringers.

Eh. That reminds me of the distinction between trekkers and trekkies, the two kinds of fans of the classical Star Trek series.

Christine

117. **dan**
June 5, 2006

lubos,
what kind of experimental predictions does string theory makel, which, if experimentally falsified, would cause you to see string theory refuted, in response to below

This is unattributed since I don't know who wrote it. Maybe they were a crackpot, one visiting the Harvard string theory group.
The problematic statement that string theory makes no prediction is repeated hundreds of times, and in many particular contexts, such a statement becomes not only boring but also patently false.

I doubt it's actually in the hundreds, but sure, I do repeatedly claim that string theory makes no predictions, and this is not "patently false", but completely accurate.

118. **MathPhys**
June 6, 2006

Motl's behavior has nothing to do with AS. He's just an arrogant, spoilt kid.

119. **Hech Baan**
June 6, 2006

Dear PeterG,

I'm another physics grad student who decided to study string/M-theory. For me the truth is written in mathematics. If I don't see a rigorous, logical, mathematical proof then statements may be beautiful, ugly, interesting, funny, but not true or false. Therefore I'm not going to change my mind based on people's opinion whoever they may be. Show me the proof!!!

Is string/M-theory correct, wrong, or not even wrong? I don't know. But I'm willing to invest my years to find it out myself.

I'm not going to order Peter Woit's book. Peter is so much against string/M-theory that he can't objectively analyse it.

120. **Bert Schroer**
June 6, 2006

Juan R.
I certainly agree with your remarks on string theorists and stringers as a description of the present reality and as a statement which is capable to generate some feeling of solidarity between those who are everyday more convinced that the Susskind et al. millennium project is the road from physics to metaphysics. But they do not explain at all how it came to this situation. There have been always people around who tried to miscarry physics into such directions, but they had no chance in the past. I think it will take a lot of deep critical analysis within the next decades to understand this phenomenon, which did not come suddenly out of the blue sky, but has its roots in the banalization of certain aspects of QFT. It certainly is not only the result of lack of experimental discoveries in particle physics; in fact given the complexity and the increasing dependence of the experiment on theory, a bad theory may also pull down experimental progress. For this reason it may be somewhat optimistic to expect that the LHC will significantly change the scene.
Dear Hech Baan, if you are unable to sense an autonomous physical beauty and continue to think that particle physics should jump over ready made beautiful mathematical sticks/strings, then I would strongly suggest that you change to mathematics before you start to worsen the present situation in particle physics which is already totally dominated by mathematical beauty (I hope that Peter will not araise this comment).

121. **knotted string**
June 6, 2006

"For me the truth is written in mathematics." – Hech Baan (above).

You have just discredited Feynman's silly, stupid and science-hating remark that agreement with experiments determine truth.

Feynman was really a hateful anti-science crackpot! See this:

"It always bothers me that, according to the laws as we understand them today, it takes a computing machine an infinite number of logical operations to figure out what goes on in no matter how tiny a region of space, and no matter how tiny a region of time. How can all that be going on in that tiny space? Why should it take an infinite amount of logic to figure out what one tiny piece of space/time is going to do? So I have often made the hypothesis that ultimately physics will not require a mathematical statement, that in the end the machinery will be revealed, and the laws will turn out to be simple, like the chequer board with all its apparent complexities."

– R. P. Feynman, Character of Physical Law, November 1964 Cornell Lectures, broadcast and published in 1965 by BBC, pp. 57-8.

122. **Hech Baan**
June 6, 2006

Dear Bert,

Thanks for your comment. Let me explain my approach.

As Einstein said in Motiv des Forschens, 1918, p 29-32 "… Practically, theoretical system is uniquely defined by observations, though there is no logical path from observations to the fundamental principles."

Therefore our challenge is to somehow find the fundamental principles. The ugly news is that there is no enough data to verify our assumptions, the bad news is that we have to work really hard to get any meaningful prediction from our frameworks/theories/laws. But the good news is that there is one tested guiding principle – mathematical consistency and beauty. Why not physical beauty? Because I don't have any sense what an autonomous physical beauty at Planck scale is. I will train my senses using my consistent theories and then yes I can use my trained senses of physcal beauty to enhance the theory. And finally,I think nature is a mathematician not a physicist 🙂

Trust us. One day one or many of us (today's grad students) will come up with the solution. We need everyone's support to get it quickly. I can tell that no any blog, article, or book will stop the progress.

123. **Juan R.**
June 6, 2006

Hech Baan,

Peter Woit is much more objective analysing string theory in this blog that any stringer (not string theorist) has been in last 40 years in peer-review papers.

Hi Bert Schroer,

I agree with you again on that people is ignorant on who generated current irrational status on fundamental physics. Stringers are simply unable to understand any criticism from rest of scientific community. Often I received personal e-mails from people who began a PhD in string theory, found the field completely irrational and abandoned physics!!! They were forced to abandon physics (instead changing string theory by some other program) because any other chance to study unification or quantum gravity is highly constrained by political and economic power of string theorists. This autoritarism only can generate disaster and, in fact, has generated.

> I think it will take a lot of deep critical analysis within the next decades to understand this phenomenon

From stringers? I doubt, maybe critical analysis will become from historians and probably from a new generation of physicists will advance physics in next decades. In fact, last book (to be published) by Lee Smolin talks about next generation of young physicists working outside of string theory.

Take the case of own J. Distler. I said here that his views on physics and in MathML are very wrong (in fact, there is a parallelism). I cited the element of line for 5D spaces appears in

http://golem.ph.utexas.edu/~distler/blog/archives/000635.html

The physics here is so wrong as the XML markup used for encoding information. I know stringers and they have a tencency to distort the history of physics. Once you find an error in some point you wait that error was recognized and changed (I always have recognized my errors, and I believe that you, Peter Woit, Chris Oakley, and others here are honest). However, would we wait "critical analysis" from stringers as J. Distler?

What would we wait from a guy calling himself bigthinker?

http://golem.ph.utexas.edu/~distler/blog/images/bigthinker.jpg

Take the case of MathML code, I checked code just before posting, however (yesterday?) Distler changed the code (in a cobardy way) and now you can see that first block equation in

http://golem.ph.utexas.edu/%7Edistler/blog/archives/000635.html

contains ds 2.

Distelr changed code probably yesterday but has not noticed in the blog, not recognized the error in public 🙂

This is like stringers work; if tomorrow HLC produces lot of interesting data against some dense ideas proposed by Distler and others during last decade, they will not recognize in public.

Additional comment:

Either Distler is reading this blog or his favourite … comunicated him the content of this blog.

Dear Distler (or …) still the code generated in your “technologicall more advanced blog” is wrong. Using ds as token you obtain roman rendering whereas the differential in next equation (1) continues rendering in italic. Moreover d is not an identifier. I would suggest you to try again!

P.S: Hech Baan The ugly news is not that there is no enough data to verify our assumptions, the very bad news is that string theory is unable to reproduce experimental result known in last centuries.

Any new theory of physics may be compatible with known data and then just then can do predictions about new data (e.g. HLC). One of most dishonest mantras of stringers is the absence of experimental data that can verify main premises as strings, hidden dimensions and all that. First explain available data (postdictions) and next focuses on predictions.

P.S: I would recommend people to dowload copies from both links before Distler changes them again without noticing :-).

Juan R.

Center for CANONICAL |SCIENCE)

124. **Peter Woit**
June 6, 2006

Juan,

I wish you would conduct arguments over MathML with Distler at his blog, and not here. Bad enough the fighting over string theory, but I don\'t want to get involved in a really vicious, ugly controversy like that over MathML…

125. **Bert Schroer**
June 6, 2006

My suggestion (which I have been following for a couple of years) is a third way which seems to be particularily advisable in times of experimental lull combined with a theoretical blue yonder abberation (Feynman) as the one which is the main topic of this blog. This third way is to press the principles of successful, but yet incomplete theories very very hard and to pay particular attention to unusual new viewpoint which maintain these principles. This is what led Einstein from the eather to the relativistic invariance principle and later to renormalized QFT (the experiments only highlighted aspects which were already there and only had to be worked out). This is extremely difficult and can hardly be done by beginners because it requires a profound knowledge and hinsight about the theory. But one only needs a breach for strong young folks to get interested in entering. Even at the risk of sounding arrogant I think that this breach already exists due to some amazing conceptual progress in QFT being related to “modular localization”. But for obvious reasons I do not want to meddle with things in Peters blog (unless Peter allows this and somebody specifically asks for this).

126. **Juan R.**
June 6, 2006

Sorry Peter Woit, I believe that many of your agree with me on that main problem with string theory is not theory itself but the hype around it. That hype confoundd street people, young undergraduate students, science policy makers, and so on.

Therefore my main attempt has been to illustrate that stringers (or “bigthinkers”) can be so arrogant in physics as they are in other fields, that they can rudely attack interesting alternatives to string theory as attack alternatives to others things and that they are doing really bad physics as they are doing in other fields.

I attempted to illustrate here how “intuition” of stringers is so wrong when they decide choosing string theory by beatiful motives as when they choose support other items.

Sam pointed to parallelism above and you next cite

http://utphysguide.livejournal.com/3047.html

I simple attempted to explain to general public as cobardy and “intellectual snob” stringers can be in a way that readers of the blog could directly observe this attitude “online ” (readers could go to Distler blog for see details) whereas it is more complex that they can go to the library to obtain article X published in academic journal Y and understand the stuff, and next to see how stringers change their views without recognizing errors and call everything “string theory” (even if there is not strings as in many brane papers!!!).

I do not know if I was succesfull but I understand your point and I will not write more about MathML here, just about physics and math.

Juan R.

Center for CANONICAL |SCIENCE)

127. **Eugene Stefanovich**
June 6, 2006

Hech Baan:

the reliance on mathematical beauty could be misleading. Physics is very different from mathematics where axioms are postulated and theorems are proved by simple logic. In physics we often don't even know what our axioms are. Nature doesn't care about our sense of beauty or present day mathematical fashions.

Ancient physicists rightly considered a circle to be the most beautiful mathematical figure. No wonder they presumed that cellestial bodies go around in circles. I guess Kepler was very surprised to discover that the orbits of planets are actually ellipses. Probably, initially he was even disgusted by his own discovery.

Even if axioms are explicitly given, physicists are not careful enough to prove their theorems rigorously. For example, the apex of modern day beauty in theoretical physics is the Minkowski space-time. Indeed, what can be simpler than take the Pythagoras theorem
r^2 = x^2 + y^2 + z^2 and add there one more term:
s^2 = x^2 + y^2 + z^2 – c^2t^2 ? However, you don't need to be a rocket (or string) scientist to realize that the Lorentz transformations and the Minkowski space-time unification do not follow from two Einstein's postulates. The second postulate (the invariance of the speed of light) can be rigorously applied only to events associated with light pulses or light rays. However, the Lorentz transformations are assumed to be valid universally for all physical systems, no matter what is their composition and interactions. Is Minkowski space-time beautiful? Sure it is! Could it happen that this beauty is similar to the beauty of Ptolemean circular orbits, and will be substituted later by more realistic "elliptical" views on space and time? Sure it could!

If you want to build physics as a mathematically consistent and logical theory, then you should return way way back to basics and begin from the principle of relativity and the laws of quantum mechanics. Otherwise you are in danger of chosing beautiful but wrong axioms.

128. **Hech Baan**
June 6, 2006

Dear Eugene,

I understand your points. I can assure you that no any sensible physicist will go and build a physical theory based on abstract axioms alone. That's not what mathematical consistency and beauty is. For instance, I'm reasonably happy with GR and QFT, but they are not mathematically consistent theories. And as a matter of fact I don't think physical theories have to be absolutely mathematically consistent per se. My point is that there are at least two main steps in creating theories. That's how I understood Einstein's quote.

1. Mathematical: Pure logical reasoning (Postulate principles and make predictions)
2. Physical: Experiments (Check your predictions)

One has to apply both steps to find/change/discard physical theories. Today in string/M-theory we are stuck at step 1. We all want to move to the step 2 ASAP. It's proved to be very difficult.

129. **Eugene Stefanovich**
June 6, 2006

Hech Baan:

For instance, I'm reasonably happy with GR and QFT, but they are not mathematically consistent theories. And as a matter of fact I don't think physical theories have to be absolutely mathematically consistent per se.

o.k.

That's how I understood Einstein's quote.

1. Mathematical: Pure logical reasoning (Postulate principles and make predictions)

Did you notice a contradiction in your words? If the theory is not *absolutely mathematically consistent* then you cannot claim that you used *pure logical reasoning*. Or, possibly, your system of postulates was not self-consistent to begin with, so the pure logical reasoning wouldn't work with it? Maybe that's why it is so difficult to move to the step 2?

130. **Hech Baan**
June 6, 2006

Dear Eugene,

Yes, there is a contradiction in my words. If you very quick and read my last posting addressed to Bert, which I suspect Peter deleted, you would have a different prospective to my approach.

I didn't deliberately use axiom or postulate; I used principle in step 1. They are physical not mathematical objects. In step 1 one has to translate physical principles into mathematical axioms in order to apply pure logical reasoning. But, doing this one introduces inconsistencies. Kant explained this beautifully in his thought provoking work. That's the reason you get mathematically inconsistent and physically meaningful theories like GR and QFT.

131. **Jonathan Vos Post**
June 7, 2006

"knotted string" is surely being facetious/ironic in saying: "Feynman was really a hateful anti-science crackpot!" and quoting from – R. P. Feynman, Character of Physical Law, November 1964 Cornell Lectures, broadcast and published in 1965 by BBC, pp. 57-8. Some context for his quote: (1) he was conceptualizing the Quantum Computer. He was wrong in details, but right in principle, and is the grandfather of Quantum Computing just as he is the greatgrandfather of Nanotechnology.

I am not objective in my respect for him, my teacher and coauthor, one of the greatest minds and greatest teachers of the 20th century.

(2) As to "… the hypothesis that ultimately physics will not require a mathematical statement, that in the end the machinery will be revealed, and the laws will turn out to be simple, like the chequer board with all its apparent complexities." [BBC spelling of what Feynman would surely render as "checker"] Feynman oscillated between this Wolfram New-Kind-of-Science Occam's razor position, and one which he spoke about with me several times: that the universe may have an infinite number of natural laws, although some only emerge under unusual boundary conditions.

There is a new Feynman movie being short right now. "Challenger" — starring David Straithairn — is about his role on the Challenger Commission. The Producer bought me lunch last week and picked my brains about Feynman and the Space Shuttle (on which I was a software engineer for Rockwell).

132. **Eugene Stefanovich**
June 7, 2006

Hech Baan:

I have a different attitude. I think, if a theory lacks logical consistency, then its foundations should be examined and the theory either should be modified or discarded altogether depending on the results of this investigation. In my view, QFT belongs to the former category, while GR to the latter one.

133. **Hech Baan**
June 7, 2006

Dear Eugene,

Logical consistency isn't binary, true or false, for fundamental physical theories. E.g. GR is logically consistent for some parameters and inconstant for others. The same is true for QFT. You can't discard them because they aren't absolutely consistent. That was one of my points, I don't think there is an absolutely consistent theory of everything all our theories are/will be consistent for some domain and inconsistent for others. Our task is to enhance the theories to extend the domain of validity/consistency.

134. **Eugene Stefanovich**
June 7, 2006

Hech Baan:

The inconsistencies I was talking about are not limited to some local domains. They are rather fundamental:

In QFT : this theory does a good job in calculating scattering (i.e., the S-matrix) which is just a special case of the time evolution. However, it is impossible to obtain the detailed time evolution of wave functions or observables from relativistic renormalized quantum field theory. This theory does not have a well-defined finite Hamiltonian (= the generator of time translations).

In GR : the problem goes back to the unification of space and time in a single 4-dim continuum already in Einstein's special relativity. Such an unification is only possible when boost transformations of space-time coordinates of events are given by universal linear Lorentz formulas independent on the physical nature of these events and independent on the interactions in the physical system where these events occur. It is true that Lorentz formulas can be rigorously derived from two Einstein's postulates for events with light pulses or non-interacting particles. However, the generalization of these formulas to all events with interacting particles is just an unfounded assumption. Moreover, it can be shown that this assumption directly contradicts the Poincare group properties of inertial transformations.

Furthermore, the treatments of space and time in GR and in QFT (or, more generally, in QM) are fundamentally incompatible. In QM the space and time coordinates are not interchangeable as in GR. Position is a quantum observable with a Hermitian operator associated with it (Bert Schroer would certainly disagree here). Time is a numerical parameter in QM. There is no observable "time", and there is no corresponding "time operator".

I am sure that within current formulations of QFT and (especially) GR their unification and the construction of a consistent relativistic quantum theory of gravity is impossible. Several decades of futile attempts by leading theorists just confirm this conclusion. Before thinking about unification we need to clean up our fundamental theories (QFT and special relativity) and make them consistent both internally and with each other.

135. **knotted string**
June 8, 2006

Jonathan Vos Post,

There were two choices: be facetious/ironic about Feynman's demand for models to be based on facts, and hatred of abject speculation

or

Quote some of Feynman's closing remarks in 1965 which predict the current crisis with stringy speculation people taking control:

'There will be a degeneration of ideas, just like the degeneration that great explorers feel is occurring when tourists begin moving in on a territory.'

(Character of Physical Law, BBC, 1965, p. 173.)

However, despite Feynman's hatred of superstring theory at his peak (around the time he discovered the O-ring problem behind the Challenger space shuttle disaster), the stringers will claim that they are the great explorers and everyone with checkable ideas are tourists.

It's like a world where you have 'professors of mountain ascent' who have uncheckable ideas about using extra dimensions to succeed, sneering at anybody down to earth.

One historical precedent is of course British Admirality problem of finding longitude. Newton thought clocks would always be mechanically defective, and looking at stars solved the problem. However, cloud cover and stormy seas (causing black eyes when using an optical instrument on deck) made Newton's idea rubbish.

When Harrison simply developed a clock that was reliable enough to measure longitude, everyone was too biased in favor of using stars, and didn't want to award the prize. This is of course the usual way. Difficult problems can have unexpectedly simple solutions, which causes conflict between experts and 'simpleton'.

136. **Bert Schroer**
June 8, 2006

Eugene,
I do not disagree with what you say about QM, it is your QM straightjacket you want to impose on QFT where our viewpoints differ significantly. Even in QM your radicalism (before with respect to me, now Hech Baan has taken up certain parts of my role) may prevent you to be able to accept useful contributions as
http://xxx.lanl.gov/abs/quant-ph/0207048

137. **Eugene Stefanovich**
June 8, 2006

Bert,

I take your calling me "radical" as a compliment, but not a deserved one. I think my views are very conservative. I maintain that the rules of quantum mechanics must be strictly obeyed in both non-relativistic and relativistic regimes. The only significant difference of the latter regime is that the Hamiltonian may contain interaction terms that change the number of particles. This is the only important distinction between QM and QFT (in my interpretation).

Thank you very much for the reference. This is a good example of how the issue of time should *not* be addressed in QM, in my opinion. Time is not an observable in the usual meaning of this word, and there is no point to associate an operator or a POV measure with time.

When experimenters measure true observables (e.g., position, momentum, spin, etc.) they bring their physical system into contact with the measuring apparatus. The result of measurement depends on the nature of the observed system and on the state of the system. When the measurement is done, the experimenter also looks at the wall clock and writes down the reading of the clock to his log. So, each measurement has a numerical label attached to it – the reading of the clock at the time of measurement. The value of this label is completely independent on the nature of the physical system and its state. This time label is an attribute of the measuring apparatus rather that a property of the observed system. Therefore, time is not an observable in the usual sense.

There are nine other numerical parameters that, together with time, identify the measuring apparatus, or observer, or reference frame. These are three components of position, three orientation angles, and three components of the velocity of the observer. These ten numbers form a parameterization of different inertial observers and of Poincare group transformations between these observers.

138. **Arun**
June 9, 2006

> When experimenters measure true observables (e.g., position, momentum, spin, etc.) they bring their physical system into contact with the measuring apparatus. The result of measurement depends on the nature of the observed system and on the state of the system. When the measurement is done, the experimenter also looks at the wall clock and writes down the reading of the clock to his log. So, each measurement has a numerical label attached to it – the reading of the clock at the time of measurement.

Two comments

1. the measurement of position localizes the measured particle but just as the time of measurement is read off the wall clock, the position is read off a ruler. Sometimes it doesn't seem that asymmetrical between space and time.

2. Space and time seem even more on an even footing in QFT where they merely label the fields.

139. **Eugene Stefanovich**
June 9, 2006

Arun:

1. the measurement of position localizes the measured particle but just as the time of measurement is read off the wall clock, the position is read off a ruler. Sometimes it doesn't seem that asymmetrical between space and time.

There is an asymmetry. The measurement of position fully depends on the state in which the particle is prepared. If you prepare the particle in a different state, the results of position measurements will be different. So, we may confidently say that position is a property of the particle. Position is an observable that should be represented in quantum mechanics by a Hermitian operator in the Hilbert space.

On the other hand, the wall clock does not care which physical system we are observing and what is the state of this system. The reading of the clock will be just the same regardless of the properties of our system. So, time has no relationship to the observed physical system, and introducing the operator of time in the Hilbert space is not justified.

2. Space and time seem even more on an even footing in QFT where they merely label the fields.

You are right that parameters x and t in the quantum field psi(x,t) are merely labels. Do they have any relationship to real observable positions and times? Or they are simply formal numerical parameters? I tend to think that the latter is true. Indeed, when we form the interaction Hamiltonian V(t), we integrate a product of quantum fields psi(x,t)phi(x,t)… over x. When this interaction Hamiltonian is inserted in the Feynman-Dyson formula for the S-matrix, it is further integrated over t from minus to plus infinity.
As a result we have the S-matrix which doesn't care about the detailed time evolution and positions of particles at intermediate times. It simply maps the asymptotic state in the distant past to the asymptotic state in the distant future. The parameters x and t are no longer present, which indicates (at least to me) their formal character.

I think it is unfortunate that quantum field parameters are denoted by the same letters x and t which normally stand for physical position and time. It would create less confusion if we used some neutral labels, like 'a' and 'b'.

Lusztig Birthday Conference

June 4, 2006
Categories: Uncategorized

I was up in Boston for a few days, and managed to attend a few of the talks at the conference in honor of George Lusztig's 60th birthday. Lusztig started out his career in geometry and topology; his thesis was in the area of index theory, working with Michael Atiyah and using the families version of the index theorem. He soon turned his attention to representation theory, which is the field that he has worked in for most of his career, often from a quite algebraic point of view. His papers are dense and can be difficult to read, especially for someone like me who is not so algebraically inclined, but many speakers at the conference remarked on how their work had drawn important inspiration from one or another of these papers.

Among the things he is famous for are his work on quantum groups, on the representation theory of reductive groups over finite fields (called Deligne-Lusztig theory, for an introduction, see here), on a whole new field in Lie theory known as Kazhdan-Lusztig theory (for an introduction, see the article by Deodhar in the proceedings of the 1991 AMS summer institute on algebraic groups), and many other things.

Of the few talks I heard at the conference, two were really exceptional. One of these was by Michael Atiyah, with the title "Quaternions in Geometry, Analysis and Physics". He began by explaining that not only was Lusztig 60, but, if he were alive, the Irish mathematician Hamilton would be 200. There's a famous story about Hamilton's discovery of the quaternions: this took place in a flash of insight on October 16, 1843, after which he supposedly engraved the defining relations of the quaternion algebra into a Dublin bridge. Atiyah described a piece of history I didn't know, showing an extract from a 1846 paper of Hamilton's in which he takes a square root of the Laplacian and essentially writes down the Dirac equation (in Euclidean signature, this was long before special relativity…).

Hamilton was very taken with quaternions as a generalization of complex numbers, and wanted to develop a "quaternionic analysis" that would be a generalization of complex analysis, a project he thought would take him at least ten years. It turns out that you can't simply generalize the beautiful subject of complex analysis and algebraic geometry over the complex numbers to the quaternionic case. Because of non-commutativity, polynomials behave very differently. Atiyah explained that in his view the correct generalization of complex analysis to the quaternionic case was Penrose's twistor theory. Here one considers all possible ways of identifying $\mathbf{R}^4$ with $\mathbf{C}^2$, forming a 3 complex dimensional "twistor space". Complex analysis on this twistor space is what Atiyah claimed should be thought of as the quaternionic analog of complex analysis (on the complex plane).

He reviewed the story of how solutions to various linear equations are related to sheaf cohomology groups on the twistor space, then went on to the non-linear case, where solutions of the anti-self-dual Yang-Mills equations correspond to holomorphic bundles on the twistor space. One can generalize twistor theory to what Atiyah claimed should be thought of as quaternionic analogs of Riemann surfaces: 4d Riemannian manifolds with holonomy in Sp(1)=SU(2), these are self-dual Einstein manifolds, what Penrose would call a "non-linear graviton" (although this is the Riemannian, not pseudo-Riemannian case). The twistor space of these 4d manifolds is a 3d complex manifold, and Atiyah considers complex analysis on this to be the quaternionic analog of complex analysis on a Riemann surface.

The quaternionic analog of higher dimensional complex manifolds are manifolds of dimension 4k, with holonomy Sp(k). Unlike in the complex case, there are few compact examples. Atiyah went on to discuss how examples (mostly non-compact) could be generated as quotients using the quaternionic analog of symplectic reduction. He described several different classes of examples, noting that this construction first appeared in work with physicists studying supersymmetric non-linear sigma models. While I was a post-doc at Stony Brook, Nigel Hitchin was visiting there and working with Martin Rocek and others on this, leading to the 1987 paper in CMP by Hitchin, Karlhede, Lindstrom and Rocek. Atiyah said that he wouldn't try and describe the relation to supersymmetry, since "I don't know much about supersymmetry, and if I tried to explain it, you would understand even less". That Atiyah, after many years of working in this area, still finds supersymmetry to be something he can't quite understand, is an interesting comment, reflecting the way the subject is still very imperfectly integrated into mathematician's traditional ways of thinking about geometry and algebra.

Atiyah also commented that off and on over the years he had pursued the idea that quantum groups (which aren't quite groups), are in some sense the quaternionification of a Lie group (which doesn't quite exist). He said he hadn't been successful with this idea, but still thought there was something to it, and hoped that someone else would take up the challenge of trying to make sense of it.

The second wonderful talk I heard was that of Igor Frenkel, from Yale, with the title "Quantum deformation, geometrization, categorification: What is next?". Unlike Atiyah's talk, which I pretty much completely understood, Frenkel's covered much too quickly a lot of material I had never understood, but putting it into an intriguing perspective close to the unsolved problems that seem to me the most important ones for mathematicians and physicists to be looking at. Frenkel began by saying that for many years he had been trying to solve the problem of how to generalize the constructions of representations of loop groups that are related to 2d CFT to representations of 3d gauge groups that should be related to 4d QFT. Some of his thoughts about this are in the write up of his talk at the 1986 ICM. He described himself as having for a long time given up on this problem, moving on to simpler things that he could do: quantum groups which are deformations of the affine Lie algebra story. He went on to talk about "Geometrization", by which he meant the principle that "all structure constants are Euler characteristics of some variety, all vector spaces are cohomologies", then "Categorification", to him the principle that "all structure constants are dimensions of vector spaces, all vector spaces the Grothendieck groups of an Abelian category". Many of the examples he was using to flesh this out are not well-known to me, I need to do some serious work learning about them before I can say that I clearly understand exactly what he has in mind here.

The last part of his talk, the "What Next?", went by way too fast but sounded fascinating. He claimed to have some new ways of thinking about the problem of what a representation of these higher dimensional analogs of loop groups should me. I hope to learn more from him in the future to get a better idea of what he has in mind here. He and collaborators at Yale have papers forthcoming, which I look forward to reading. When and if I ever better understand this stuff, I may try and write about it again here.

Comments

1. **Lubos Motl**
   June 4, 2006

   Not sure why he thinks that the complex analysis on twistor space generalizes complex analysis to the case of quaternions. What is quaternionic about the twistor space? I don't see any noncommutative algebra here. Is it just about counting dimensions?

   The interpretation of anti-self-dual Yang-Mills solutions in terms of twistor space you described seem to miss all the new developments since the 2003 Witten paper – which is an expansion around the self-dual Yang-Mills to the full Yang-Mills. The research since that paper has at least doubled the amount of interesting insights in this realm.

   Supersymmetry has not yet been seen in the colliders but it is an extremely physical framework, and it is not too surprising that pure mathematicians may find it counterintuitive. Surely mathematicians in the 21st century will need SUSY because string theory will dominate mathematics of the 21st century. That's why mathematicians will have to be learning many things that are currently taught in physics departments only.

   Strictly speaking, manifolds of Sp(k) holonomy don't define quaternionic manifolds. The holonomy of quaternionic Kahler manifolds is Sp(k) times Sp(1) because it can allow a multiplication by quaternions (Sp(1)) – or quaternionic matrices (Sp(k)) – from both sides. And they don't commute with each other. The manifolds of the more special, Sp(k) holonomy are called hyperKahler manifolds, not quaternionic manifolds.

2. **woit**
June 4, 2006

Lubos,

Atiyah's point was that what you get by naively replacing complex variables by quaternionic ones is not the really interesting generalization of complex analysis that you get in the twistor space picture. He was using quaternions to motivate this, but the twistor space of R^4 is something different than just using quaternions to put a multiplication on R^4.

Related to this, the 4k dim manifolds that Atiyah was discussing were the hyperkahler ones, not the quaternionic kahler ones.

3. **Lubos Motl**
June 4, 2006

Dear Peter, I understand that the twistor is something "different" than using the quaternion multiplication table. The word "different" is clear. What is not clear is why you or he uses the "same" word for "different" things. 😉

Your text also made it clear that he was using Sp(k) holonomy manifolds. But I was trying to convince you that you incorrectly called them the quaternionic analogue of Kahler complex manifolds.

The quaternionic analogue of Kahler (U(k) holonomy) manifolds are the quaternionic Kahler manifolds whose holonomy is Sp(k).Sp(1).

The manifolds with Sp(k) holonomy are called hyperKahler manifolds, and they are, in some sense, the quaternionic generalization of (SU(k) holonomy) Ricci-flat Kahler or Calabi-Yau manifolds. Your sentence about the quaternionic generalization is not quite right.

4. **Peter Woit**
June 4, 2006

Lubos,

I know very well what the difference is between a hyperkahler and a quaternionic kahler manifold. So does Atiyah. I was reporting on his talk, and quoting him; he was the one claiming that hyperkahler (not quaternionic kahler) manifolds are the interesting quaternionic analog of kahler manifolds. If you don't like this, please go write to Sir Michael and explain to him why he's wrong.

5. **Lubos Motl**
June 4, 2006

I agree that the hyperKahler manifolds are more interesting than quaternionic Kahler manifolds, but I disagree that they are the generalization of Kahler manifolds to the case of quaternsions. Instead, hyperKahler manifold is the quaternionic generalization of Ricci-flat Kahler manifolds whose holonomy is SU(n), not U(n).

What you wrote is, in fact, completely incorrect. You did not even use the word "Kahler". You wrote that Sp(k) manifolds generalize "complex manifolds". That's nonsense because "complex manifolds" can have virtually any holonomy you want while Sp(k) is highly constrained. What you wanted to say was "Ricci-flat Kahler manifolds", not "complex manifolds".

6. **Lubos Motl**
June 4, 2006

If Sir Atiyah used your sentence literally and meant every word in that sentence to be treated seriously, I will happily explain the error to Sir Atiyah, too. Unlike you, I don't think that certain people above some level of dignity are infallible and uncriticizable.

7. **woit**
June 4, 2006

Lubos,

If you want to explain to Sir Michael your views on why he's wrong to think of hyperkahler manifolds as the quaternionic generalization of Kahler ones and why his analogy is "nonsense", definitely go ahead and e-mail him. I'm sure he'll be fascinated by your insights into this and glad to be set straight.

Then again, before you hit send on that e-mail, you might take a moment to reflect and realize that there are some people in this world who know about a hundred times more than you do about certain subjects, and geometry might be one of them.

8. **Lubos Motl**
June 4, 2006

Dear Peter, I don't have any trustworthy data that would indicate that Sir Atiyah makes incorrect analogies between different structures that would resemble your incorrect sentence.

9. **sunderpeeche**
June 4, 2006

If the talk is available online (proceedings or arXiv) that would settle the issue. Why argue the details of what is essentially second-hand information?

10. **Peter Woit**
June 4, 2006

Sunderpeeche,

The talk is not available online. You're quite right, this is second-hand information, produced by me wasting much of my evening trying to write out the clearest explanation I could of an analogy ("analogy", as in not precisely the same, check your dictionary) described by one of the world's greatest mathematicians in a talk I attended, based on my written notes and what I remember.

Jeez, why waste time trying to explain something interesting here? I should just go back to string theory bashing.

11. **Carl Brannen**
June 4, 2006

Among those who practice David Hestenes' Geometric Algebra, it is said that this is the natural generalization of complex analysis. An introduction is here:

http://faculty.luther.edu/~macdonal/GA&GC.pdf

Carl

12. **MathPhys**
June 4, 2006

Peter,

As far as I'm concerned, you haven't wasted your evening and I want to thank you very much for your efforts to keep us all informed.

We all benefit from your reporting, including the emotionally unstable kid who reads your blog so frequently and so thoroughly that he's typically the first to write a response to it.

13. **John Baez**
June 4, 2006

It's nice to hear that Frenkel continues to explain the importance of categorification. I got excited about this concept around 1994 when I read his paper with Louis Crane on Hopf categories and the canonical bases for quantum groups; you can see my excitement in "week38" of This Week's Finds. It took a while for this idea of Frenkel's – categorifying quantum groups and their resulting tangle invariants – to bear fruit, but his student Khovanov has turned it into a hot topic. Since you're presumably right next door to Khovanov, you could probably get him to explain anything about categorification that interests you! I'm very excited that my student Aaron Lauda is going to Columbia next year to work with Khovanov on this stuff. I hope he looks you up sometime.

I was in Cambridge this weekend too, but too busy to attend any talks. I'll probably be free on Monday night. If any cool people want to talk, email me. I'd like to have dinner in Harvard Square. I haven't had time to go there this visit, and I haven't been there for over 5 years. I hear it's been much gentrified since I was a grad student here back in '82-'86.

14. **D R Lunsford**
June 4, 2006

Atiyah's statement RE quaternion analysis is absolutely fascinating and obviously correct. It's the conformality that makes complex analysis so rich and interesting.

-drl

15. **Tony Smith**
June 5, 2006

Carl Brannen mentions "David Hestenes' Geometric Algebra" as "the natural generalization of complex analysis".
Geometric Algebra is basically Clifford algebra.
The real Clifford algebra Cl(0,1) has dimension 2^1 = 2 and is the Complex numbers.
The real Clifford algebra Cl(0,2) has dimension 2^2 = 4 and is the Quaternions.

Peter mentions with respect to Atiyah's talk "… a "quaternionic analysis" that would be a generalization of complex analysis …".

Since both Complex numbers and Quaternions are Clifford algebras, a natural way to look at "… a "quaternionic analysis" that would be a generalization of complex analysis …" is to look at Clifford analysis.

John Ryan, in math.CV/0303339 , Introductory Clifford Analysis, said:

"… one can extend basic results of one complex variable analysis on holomorphic function theory to four dimensions using quaternions. … This was developed by the Swiss mathematician Rudolph Fueter in the 1930's and 1940's … So it seems reasonable to ask if all that is known in the quaternionic setting extends to the Clifford algebra setting … the answer is yes …
…[the] type of diffeomorphisms acting on subdomains of Rn [that] preserve Clifford holomorphic functions … is a conformal transformation …
… for dimensions 3 and greater the only conformal transformations … are Moebius transformations …".

Lars Ahlfors, in his paper Clifford Numbers and Moebius Transformations in R^n, published on pages 167-175 of the book Clifford Algebras and Their Applications in Mathematical Physics, Proceedings of NATO and SERC Workshop, Canterbury, Kent, 1985, edited by Chisholm and Common, NATO ASI Series (Reidel 1986), said:

"… Moebius transformations in any dimension can be expressed through 2×2 matrices with Clifford numbers as entries. This technique is relatively unknown in spite of having been introduced as early as 1902 … by K. Th. Vahlen …".

Pertti Lounesto, in his book Clifford Algebras and Spinors (Cambridge, 2nd edition, 2001), said (denoting nxn matrices of X by Mat(n,X) and denoting the quaternions by H):

"… Cl(p+1 , q+1) = Mat(2, Cl(p,q)) …
… Cl(1,3) = Mat(2, H) which implies Cl(2,4) = Mat(4, H) …".

Since the bivector Lie algebra of the Clifford algebra Cl(2,4) gives the Lie group Spin(2,4) = SU(2,2) that is the 15-dimensional Conformal group over Minkowski spacetime R(1,3),
the quaternionic structure of that Conformal group is made clear by Cl(2,4) = Mat(4, H).

Roger Penrose, in The Road to Reality (Knopf 2005) said at page 972, using notation O(2,4) for the 15-dimensional Conformal group:

"… The shortest … way to describe a (Minkowski-space) twistor is to say that it is a reduced spinor (or half spinor) for O(2,4). …".

The above outline shows:

1 – a useful quaternionic "generalization of complex analysis", based on Clifford algebra (as indicated by Carl Brannen's comment)

2 – the answer to Lubos Motl's question "What is quaternionic about the twistor space?"

3 – more details about the view (expressed by Atiyah) that "the correct generalization of complex analysis to the quaternionic case was Penrose's twistor theory".

All the above is consistent with D. R. Lunsford's comment that what makes such stuff "so rich and interesting" is "the conformality".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

16. **Jonathan Vos Post**
June 5, 2006

We used quaternions a lot in GNC (Guidance, Navigation & Control) software for spacecraft at JPL, yet I wrote ground test software that used Euler angles and matrices to validate & verify the flight software of Gaileo, to avoid repeating any conceptual errors of the flight software. I got deep into qaternionic analysis then and since. I've published a paper deadling with whether certain physical quantities assumed to be real (such as momentum and acceleration) for elementary particles, might actually be complex, or quaternions, or Cayley algebra…

As to manifolds, what's the deal on the latest episode of this saga: Joe Lau writes on Slashdot to mention a story running on the Xinhua News Agency site, reporting a proof for the Poincare Conjecture in an upcoming edition of the Asian Journal of Mathematics.

http://news.xinhuanet.com/english/2006-06/04/content_4644754.htm

From the article:

"A Columbia professor Richard Hamilton and a Russian mathematician Grigori Perelman have laid foundation on the latest endeavors made by the two Chinese. Prof. Hamilton completed the majority of the program and the geometrization conjecture. Yang, member of the Chinese Academy of Sciences, said in an interview with Xinhua, 'All the American, Russian and Chinese mathematicians have made indispensable contribution to the complete proof.'"

17. **Thomas Larsson**
June 5, 2006

Frenkel began by saying that for many years he had been trying to solve the problem of how to generalize the constructions of representations of loop groups that are related to 2d CFT to representations of 3d gauge groups that should be related to 4d QFT. Some of his thoughts about this are in the write up of his talk at the 1986 ICM.

Another place where this thoughts as of 1993 can be found is hep-th/9303047. Ref 10 of that paper contains the first interesting representations of multi-dimensional current algebras. The same construction applied to multi-dimensional diffeomorphism algebras led Rao and Moody to the first interesting representations of the multi-dimensional Virasoro algebra.

There is also another extension of current algebras in 3D, the Mickelsson-Faddeev extension. However, it was proved by Pickrell that this algebra lacks unitary reps on a separable Hilbert space; for a simple plausibility argument, see math-ph/0501023, where the two types (KM and MF) of current algebra extensions are contrasted in a Fourier basis.

18. **Aaron Bergman**
June 5, 2006

Re: Poincare: I'm guessing they're just claiming to have finished filling in the details of Perelman's program. A fair number of people have been working on that as I understand it.

19. **woit**
June 5, 2006

Re: Poincare

Yes, Cao and Zhu are claiming to have come up with a full proof based on Perelman's outline. It has been vetted by Yau and is supposed to appear in this month's Asian Journal of Mathematics, which is on-line now, minus the pdf file of the article. I was waiting for the article to appear before writing about this, if anyone knows where a copy is available, let me know.

20. **Bert Schroer**
June 6, 2006

Peter quoted
"Atiyah said that he wouldn't try and describe the relation to supersymmetry, since "I don't know much about supersymmetry, and if I tried to explain it, you would understand even less". That Atiyah, after many years of working in this area, still finds supersymmetry to be something he can't quite understand, is an interesting comment, reflecting the way the subject is still very imperfectly integrated into mathematician's traditional ways of thinking about geometry and algebra."
I would like to add that also modular localization (the adaptation of Tomita-Takesaki modular theory to local quantum physics) which is presently the best sniffer dog in the conceptual arsenal of theoretical particle physics (a gift of AQFTists, who were deeply involved in its independent discovery within a more limited context) is also unable to attribute any significance to supersymmetry. If one starts from a given observable local net structure (pure bosonic in the sense of spacelike commutativity), and hence leaves its previously mentioned very radical and yet insuffiently understood monade approach (leading to spacetime symmetries and observable nets from more basic assumptions of the kind favored by Lee Smolin) outside, there is the DHR theory (which may be viewed as part of modular theory) which is capable to uniquely extend the observable algebra to the charge-carrying field algebra from which one can then read off the compact group symmetry (in low dimensional spacetimes the picture is more complicated). As mentioned on a previous occasion it does so in the inverse problem spirit of Marc Kac's "how to hear the shape of a drum". It leads to compact groups, but not to graded groups. Of course it is not forbidden to extend to supersymmetries by hand, even though modular theory which gives spacetime- as well as inner symmetries does not lead to susy in any natural way.
In addition there is the curious observation that putting a supersymmetric QFT into a heat bath, the resulting situation is characterized by a collape and not a spontaneously broken symmetry as it would occur with Lorentz symmetry
http://br.arxiv.org/abs/math-ph/0604044 see in particular ref. [3] and [4]
The situation throws doubt whether the spontaneous breaking mechanism for supersymmetry permits any consistent formulation (i.e. even outside the heat bath setting) at all. Supersymmetry looks like a man-made thing which is despite its unattractive rigid appearance apparently extremely fragil.

21. **Bert Schroer**
June 6, 2006

Today seems to be a really bad day for supersymmetry
http://br.arxiv.org/abs/hep-th/0606029

22. **Chris Oakley**
June 6, 2006

… but a good one for satanists. Maybe Lubos foresaw all of this in a dream, where the String God appeared to him and intoned the words: "Lubos, my good and faithful servant. I can protect you only until the satanists rise again. Then nameless evils will happen, and I will no longer be able to prevent the publication of Woit's book."

23. **David Corfield**
June 6, 2006

I had only a second-hand commentary on Frenkel's conception of categorification by Dror Bar-Natan about half way down this post. From the perspective presented in the first half of the post, which might be phrased "If it moves, salute it; if it doesn't, categorify it", this is somewhat limited.

24. **D R Lunsford**
June 7, 2006

Well 6's are representative of "hybrid strings", that is, those that close on themselves but leave a tell-tale serif flapping in the vacuum. So 666 is really the Mark of the Proton, forseen in the Book of Zweig, and detailed in Gell-Mann's Letters to the Experimentalists.

-drl

25. **Chris Oakley**
June 7, 2006

The Coming of the Experimentalists, as foretold in the Book of Zweig, is feared above all things by those who worship the String God. If I may quote a passage (Book 5, v. 12-15):

> 12. And those who were content to dwell in the realms of the higher dimensions without the aid of illegal substances took counsel among themselves, for they were affrighted by the angry heathens who said unto them, "Name one thing, *one thing*, that thy bunkum doth predict that can be tested in a laboratory?"
>
> 13. So the Stringers girded their loins and went unto Princeton, to take counsel with the one they call the Pope who dwelt there; for he is held in honour above all others, and understands the workings of the mind of the String God better than any.
>
> 14. And for forty days and forty nights The Pope and his senior priests debated and prayed to the ten-dimensional god, and at length The Pope did say unto his followers, "The String God has spoken, and he spake thus. The sun shall shine upon us and all our endeavours; yea, we will continue to be able to pull the wool over the eyes of the retarded bureaucrats at the DOE and NSF. But the God warned me of 666. He did say unto me that after 666 the power of the ignorant: those not smart enough to do String Theory, and those smart enough but too crass to appreciate its beauty will be granted power by the Evil One".
>
> 15. But the people were confounded. For they knew not the meaning of "666".

26. **Alejandro Rivero**
June 7, 2006

666 means read/write for owner, group and others. But no execute permisions. Better 777.

27. **Tony Smith**
June 7, 2006

Chris Oakley said "… But the people were confounded. For they knew not the meaning of "666". …".

In the 6×6 magic square

01 35 34 03 32 06
30 08 28 27 11 07
24 23 15 16 14 19
13 17 21 22 20 18
12 26 09 10 29 25
31 02 04 33 05 36

the columns, rows, diagonals all add to 111 and the total number is 666.
Cornelius Agrippa (1486-1535) identified that 666 magic square with the sun
(see http://www.geocities.com/CapeCanaveral/Lab/3469/examples.html).

Maybe because some religions considered by Christians to be pagan
were sun-god religions, Christians demonized the sun-number 666.

Maybe, in Chris Oakley's Book of Zweig, verse 16. may have said:

16. What the people did not know was that 666 = Sun = Light of Experimental Evidence that is to come from the Temple of Experiment in Geneva, capable of destroying even that most fervently worshipped of Theoretical Gods, SuperSymmetry.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

28. **Chris Oakley**
June 7, 2006

Tony,

What you say is interesting, but is not what I have. My version reads:

> 16. But they sure as hell soon found out, as on the sixth day of the sixth month of the year two thousand and six, the Anti-String himself arose in power. And he was aided by the printers of Albion. And his wordy refutations of the stringy arts caused the higher-dimensional ones to tear out their hair and wail for vengeance.

29. **Thomas Larsson**
June 8, 2006

06/06 is the Swedish national day, which we celebrate, perhaps, because some obscure coup d'etat in 1809 replaced one king by another. 2006 is the first year it is a holiday.

30. **Alejandro Rivero**
June 8, 2006

I am intrigued: Why did you choose Zweig?

31. **Chris Oakley**
June 9, 2006

Alejandro – no idea. It seems to be lost in the mists of time. But to continue:

> 17. And yet the Stringers were not wholly unprepared for the onslaught of the Anti-String; for the one they call the Pope had foreseen the difficulty of keeping the share of the lion of the HEP theory budget when their metaphysical excursions were no more meaningful than the vapid meanderings of the acid-tripping punk philosopher.
> 18. "But surely," the Pope did say unto the people, "surely our wisdom is not merely of Great Beauty. Surely our writings do predict gravity, and manifold are the verifications thereof."
> 19. But the one who is known as Glashow, and who is held in high honour among the wise, did snort when he heard these words.
> 20. "This Pope of the Superstringers seemeth not to understand the meaning of the word 'predict'," Glashow did say unto the gathered multitudes of PBS. "He seemeth not to comprehend that gravity was known about before he and his followers did take the ten-dimensional path. If he desireth honour from the Swedish Sages then he will surely have to do better than rediscovering that which hath been known for many centuries."
> 21. But The Pope, who is wise and subtle, had foreseen many of the stratagems of his enemies, and took counsel with his most senior acolytes.
> 22. "We must be like Valhalla," he did say unto them. "We must gather heroes to dwell among us. Those who are strong and fearless. Brave men are needed to defend the Citadel of the String from the ravages of the uneducated philistines who would do us harm. And we must cast the net wide. No matter if they are of dubious mental stability provided they support out cause."
> 23. And although many brave men gathered round to defend the Citadel, none were as strong or fearless as Lubos of Bohemia, who did prostrate himself before the The Pope and pledge undying support and loyalty.
> 24. "I bring, O Master," Lubos, "a scourge upon your enemies! I promise ye that as long as I live, none will dare question the rightness of our quest, for they will have to get past me, and I will punish them immoderately!"
> And the stringers were well pleased.

Not Even Wrong in the Financial Times

June 5, 2006
Categories: Not Even Wrong: The Book

As a start on the project of reorganizing this weblog a bit to take into account the existence of the book, I've started using the "Categories" feature of WordPress. Postings about the book will be in a special category, all other postings will remain "Uncategorized". If you don't want to read any more about what is going on with the book, just access this weblog via the "Uncategorized" link over on the right panel.

This past weekend there was an [article in the Financial Times](#) by science writer [Robert Matthews](#) about the problems with string theory and the publication of my book. It's not a book review, but more of a commentary, and is far more critical of string theory than I am. To give you an idea, here's the lead paragraph:

They call their leader The Pope, insist theirs is the only path to enlightenment and attract a steady stream of young acolytes to their cause. A crackpot religious cult? No, something far scarier: a scientific community that has completely lost touch with reality and is robbing us of some of our most brilliant minds.

I thought the article was kind of over the top, but then I read Lubos's commentary on it, entitled [Robert Matthews: science-hater par excellence](#), which makes it seem rather moderate. He writes that a "senior physicist who is not a string theorist" sent him the article with the comment "a tendentious, malicious attack on scientists and through that on science itself." I can see why someone unhappy with the article might characterize it as "a tendentious, malicious attack on string theorists", but I don't see any sense in which it is an attack on scientists in general or on science itself. The one thing I really don't like about the article is the headline "Nothing is gained by searching for the 'theory of everything'", and the fact that at a couple points the writer implicitly identifies the search for a 'theory of everything' with doing string theory. I'm very much in favor of people continuing to search for a unified 'theory of everything', just think that string theory is a failed program for reaching this goal, something which needs to be acknowledged.

Doubtless the Financial Times will be getting various outraged letters from senior physicists, string theorists and non-string theorists, but I'd be a lot more willing to sympathize with their outrage at the article if they had ever expressed similar outrage at any of the extreme hyping and overselling of string theory that has gone on in the popular press over the last twenty years. Unfortunately I suspect that the next few years will see a lot of this kind of backlash against work on unified theories or on the use of sophisticated mathematics in fundamental physics. The theoretical physics community has done increasing amounts of damage to its own credibility because of the way string theory has been pursued and marketed, with the recent "anthropic string theory landscape" promotion providing a perhaps deadly blow. I'm afraid the near future will see de-funding not only of string theory, but of any other ambitious attempts to search for new ideas about how to unify fundamental physics.

One major source of continuing damage to string theory comes from the fact that by far its most active advocate on the Internet is Lubos Motl, and the fact that there is no evidence that his senior colleagues are willing to dissociate themselves from his behavior. Many younger string theorists are appalled by how he behaves, but too frightened of retribution to publicly say anything. Consider a recent review of my book posted on Amazon by a young string theorist:

Need cheering up? Get this book after reading the review below by Lubos Motl, then try to find out how he fabricated his "review". I'm a string theorist by the way, which is why I'm hiding behind a pseudonym (I don't want to be called a "science hater" by my seniors). This book makes a surprising effort to explain abstract mathematical concepts.

Very quickly after it (and any other positive reviews) appeared, it had garnered a large number of votes as not "helpful". Someone out there seems to be spending their time watching Amazon for positive reviews of my book, then repeatedly connecting to the site with different identities to vote against any positive reviews and for Lubos's review. Wonder who could be doing that?

Update: [CapitalistImperialistPig](#) asks a question that I've also been wondering about:

*Why exactly was it you gave this to LM? Luboš, of course, is a very clever fellow, but he also believes practically every crackpot notion known to the modern world – or at least the right wing ones. If you want to discredit some writing, sending LM to do the hit is *not* the way to win hearts and minds. Of course Lumo did say she (or he) was not a string theorist so …*

Comments

1. **Kea**
   June 5, 2006

   Peter

   I like the sound of your book. I think I will go and sit in the bookshop and read it.

2. **LambchopofGod**
   June 5, 2006

   I think a letter from PW to the Financial Times would be particularly appropriate and effective. Note that Matthews is more than a little ignorant — from his website I see that he has dreams of resurrecting the Steady State Theory, based on his own elementary misunderstanding of deSitter space. He's clearly a crank.

3. **woit**
   June 5, 2006

   No, Matthews is not at all "clearly a crank". String theorists can try and engage in ad hominem attacks on him the way they do on anyone who challenges their beliefs, but it's an ugly, unethical and unprofessional tactic. It's also one that is getting really ineffective as it becomes clear to more and more people that it's the only argument they have.

   I was thinking about whether I should write something to the FT, but crap like this doesn't encourage me. I guess at least tonight my feeling is that I'll write into the FT that Matthews has gone a bit too far when senior string theorists start writing into other publications to correct their overhyping of string theory.

4. **Censored**
   June 6, 2006

   I wrote something similar to your post in Lubos comments section.
   But it was deleted.

   The consensus with his/her views might be less than what it seems.

5. **Chris Oakley**

June 6, 2006

The Matthews article is IMHO completely fair and reasonable. A TOE is, at the moment, too big a step to take for anyone, String Theorists especially, and he is right to point this out.

Of course, as this is the first article to appear in the popular media that is strongly sceptical of ST, a lot of feathers have been ruffled, but most of what he says are things that String Theorists should have recognised themselves. It should not need an outsider to provide the bigger picture.

As for the reaction to the article, I would pose the question, “With friends like Lubos, does String Theory need any enemies?”

6. **Lubos Motl**
June 6, 2006

Only a very unreasonable person could believe that the distasteful anonymous first-poster is a young string theorist. Much like all other positive reviewers of Peter Woit’s book, he is a crackpot with IQ around 72, about 1/2 what he would need to be a string theorist. Don’t be kidding. It’s completely obvious that all of these people are complete idiots who have never seen the book, who have no idea about physics, and who have no idea what they’re talking about.

7. **Santo D'Agostino**
June 6, 2006

Lubos,

Consider your statement:

“… all of these people are complete idiots who have never seen the book …”

Yet you have posted on Amazon a review of the same book (Not Even Wrong) and you also have not read it!

Readers are buying the book, not a draft manuscript that has subsequently been revised and corrected who-knows-how-many times, as is completely normal in book publishing. Readers deserve a review of the ACTUAL BOOK, not a preliminary manuscript.

Would you find it acceptable for journal referees to reject one of your papers based on reading a preliminary draft, and ignoring the final submitted version?

I advise you to withdraw your review of NEW until you have actually read the book. If you don’t, consider the damage to (what is left of) your scholarly reputation.

All the best wishes,
Santo

8. **Lubos Motl**
June 6, 2006

Dear Agostino,

I have not only read it but read it in detail, and moreover I claim that every person with IQ above 80 must be able to determine this fact from my detailed review.

All the best
Lubos

9. **Santo D'Agostino**
June 6, 2006

Dear Lubos,

In your Amazon review, you state:

“I have read a different edition of the book than one offered here, and I apologize in advance for any inaccuracies in my review that this fact could cause. In fact, if any errors from the list below have been corrected, it was because of my feedback, so I think it is fair to list them anyway.”

Since this is the first published version of the book, you have not in fact read a different “edition,” but rather an unpublished manuscript, correct? I understand that English may not be your first language, and that therefore you may have to look up the definitions of “edition” and “manuscript,” but being a man of very high IQ, you will quickly understand the difference, if you don’t already.

My previous points remain. It is NOT fair to list errors in manuscript if they have been corrected in publication. Readers are not buying the manuscript that you read, they are buying the published book, and therefore deserve a review of the actual published book.

All the best,
Santo

10. **Peter Woit**
June 6, 2006

Santo,

It doesn’t matter which version Lubos is looking at. Besides the one Gev/Tev typo he found, his list of “errors” contains just his own errors, together with nonsense generated by making up a wild misinterpretation of something I wrote.

11. **Santo D'Agostino**
June 6, 2006

Peter,

Yes, good point. You mentioned this in an earlier posting, but I did not attend to it when replying to Lubos.

Your comment amplifies my point that it is inappropriate (indeed unethical) to review preliminary versions of a published work. I continue to call on Lubos to withdraw his Amazon review.

Best wishes,
Santo

12. **island**
June 6, 2006

FYI: Robert Matthews also wrote an article for the May/June issue of NewScientist Magazine… a review of the 20 year span between the first and second edition of a collection of essays by physicists, published as "The New Physics"

> Physics: Are we nearly there yet?
>
> What major insights have physicists stumbled on in the last 20 years? Hardly any…
>
> Now, almost 20 years on, the publication of a second edition of The New Physics provides an opportunity to discover how all those exhilarating advances have panned out. To judge by the accounts assembled by new editor Gordon Fraser, the short answer is: they haven't. Indeed, the impression is one of physicists not so much approaching a beckoning peak as wandering about in a thick fog.
>
> […]
>
> For sheer stagnation, look no further than the chapter on superstring theory, authored by one of its originators, Michael Green. Over the last 20 years, superstring theory has transmogrified into something called M-theory, which is even more mind-boggling than its forebear. But it is still no closer to being a genuine scientific theory…

And they say that Einstein wasted the last years of his life… uh huh

13. **hack**
June 6, 2006

That is really low, comparing physics hype from 20 years ago to reality! That just isn't done in polite society.

14. **LambchopofGod**
June 6, 2006

Matthews clearly is a crank. Three symptoms:
1. Emotional rants against the "establishment".
2. Having Big Ideas — see his stuff about reviving the Steady State theory. He's a genius, a true polymath. Not.
3. Arrogance, manifested as indifference to the fact that he can easily be exposed — he thinks that de Sitter space has a timelike Killing vector, and is willing to display this elementary ignorance for all to see.

It is of course no crime to be ignorant, but ignorant people should not publicly call into question people's competence or professional ethics. If they insist on doing so, they should expect a robust response. In particular they should not whine about ad hominem attacks when they themselves are guilty of that very thing.

15. **Peter Woit**
June 6, 2006

I've really had quite enough over the last few years of string theorists who can't answer any of the objections being made to the theory engaging in ridiculous, unfair, attacks on the competence of people who disagree with them, often in a cowardly fashion using the cover of anonymity. Instead of addressing what Matthews has to say on this topic, you go rooting around in his writings on other topics, looking for something you can take out of context and hold up as evidence of his incompetence. It's disgraceful, cowardly, dishonest and completely pathetic behavior. Don't do it here anymore unless you're willing to stop being anonymous so that we can all go through everything you have written to check it for possible evidence of incompetence.

And a string theory partisan accusing critics of string theory of arrogance? What a joke.

16. **Jayhog**
June 7, 2006

heh, what other crackpot theory does Luboš belive?

17. **anon**
June 7, 2006

Lamb chop of God, your three reasons apply to string theorists, but that doesn't them all cranks does it? Not in the media.

[String theorist name here] is clearly is a crank. Three symptoms:

1. Emotional rants against the Feynman et al.
2. Having Big Ideas — see his stuff about extra dimensions. He's a genius, a true polymath. Not.
3. Arrogance, manifested …

Jayhog, Lubos believes in everything which gets him publicity.

18. **island**
June 7, 2006

I don't think that it's fair to level everything that's wrong with the behavior of string theorists on Lubos, since there are other, more well-known proponents that are "Living Our Multiverse"… next-door to Alice.

There are valid "quasi"-steadystate models.

Remind me not to use exclamation points.

19. **Tony Jackson**
June 7, 2006

I'm a biochemist – so a long, long way from string theory- but not such a long way from previous interactions with Robert Matthews. I have found him infuriatingly dogmatic and not really interested in nuance. To be fair, he sees himself primarily as a journalist these days, and good copy to a tight deadline often has to place things in black and white. That said, and watching from the sidelines, I have a queasy feeling that he has indeed touched a raw nerve.

Seriously, PW, please write to the FT and make your more subtle position clear. Apart from anything else, if you don't, people who won't read your book will assume Mathews is faithfully repeating your exact position.

20. **Juan R.**
June 8, 2006

LambchopofGod,

No forget that standard string theory is a kind of modern "Steady State Theory".

Juan R.

Center for CANONICAL |SCIENCE)

21. **Who**
June 10, 2006

The Sunday Times review of the book is out
It is by John Cornwall, a 20th century historian known for his study of the relations between the 3rd Reich and the Vatican. He seemed to approve of the book's prose style, among other things, which is a good sign.

http://www.timesonline.co.uk/article/0,,2102-2214707,00.html

===sample quote===
…But is string theory true? Peter Woit, a mathematician at Columbia University, has challenged the entire string-theory discipline by proclaiming that its topic is not a genuine theory at all and that many of its exponents do not understand the complex mathematics it employs. String theory, he avers, has become a form of science fiction. Hence his book's title, Not Even Wrong: an epithet created by Wolfgang Pauli, an irascible early 20th-century German physicist. Pauli had three escalating levels of insult for colleagues he deemed to be talking nonsense: "Wrong!", "Completely wrong!" and finally "Not even wrong!". By which he meant that a proposal was so completely outside the scientific ballpark as not to merit the least consideration.

Woit's book, highly readable, accessible and powerfully persuasive, is designed to give a short history of recent particle and theoretical physics. Ultimately he seeks not only to rattle but to dismantle the cage of the string theorists…
===endquote===

the Harvard ranter had a kitten over this one

22. **Chris Oakley**
June 11, 2006

What gives the book its searingly provocative edge, moreover, is the fact that Woit isn't even a tenured professor, but a mere mathematics instructor specialising in computer systems.

(from the Sunday Times article)

I don't quite get this: Peter has done HEP theory up to post-doctoral level, teaches advanced courses and pursues his own research. These are also what tenured staff do, and at the same level – except that they probably know less about computers – so why the "mere"?

23. **knotted string**
June 11, 2006

I think what is happening is this. The first discussions of string theory occur in chapter 9. Before that you have a hundred pages describing the facts, experimentally validated theoretical physics.

This is probably why reactions are delayed. After you read the facts and see what the real problems with the Standard Model are (chapter 8), it is then obvious that mainstream stringy stuff simply isn't dealing with these problems. Then when you start the last section of the book, you are just swamped with stringy problems.

Page 177: supersymmetry can only be checked by using electroweak force strengths to calculate the strong force: it is 10-15 % higher than observation (which has an experimental accuracy of around 3 %). So the only precise check discredits it.

Page 179: supersymmetry suggests a vacuum energy density 10^56 to 10^113 times high.

Page 181: Gerard 't Hooft: '… I would not even be prepared to call string theory a 'theory' … just a hunch. … Imagine that I give you a chair, while explaining that the legs are missing, and that the seat, back and armrest will perhaps be delivered soon; whatever I did give you, can I still call it a chair?'

The abstraction issue is the cause of the Bogdanov brothers affair: it is disgusting that the state of physics is such that peer-reviewed Classical and Quantum Gravity and also Annals of Physics published their papers on the basis of evidently non-scientific reasons and later had to apologise for making an error.

Page 223: 'The main thing the journals are selling is the fact that what they publish has supposedly been carefully vetter by experts. … The referee's report reproduced earlier shows clearly the line of thinking at work: '… Nothing published in this whole area makes complete sense … maybe there's even an intelligible idea in here somewhere. Why not just accept it?"

At this point the reader must take a walk in the fresh air to try to remain sane. Probably Peter will delete this comment for being too long, but I just can't see how this situation can sort itself out ever.

24. **island**
June 11, 2006

> Pauli had three escalating levels of insult for colleagues he deemed to be talking nonsense: "Wrong!", "Completely wrong!" and finally "Not even wrong!".

What the heck? I thought that Pauli used only one single phrase that came from a too-philosophical meeting with David Bohm… ?

25. **Santo D'Agostino**
June 11, 2006

Update on my comments of 6 June, 2006, directed at Lubos Motl: I have posted similar comments to Lubos on his own blog today (in response to his outrage that the Sunday Times would print what he calls John Cornwell's malicious attacks). My comments were deleted and I am now banned from his blog.

Lubos has never responded to my statement that it is unethical to publicly "review" a book that one has admitted not reading. Instead he continues to insist that he did read the book "as a job," but he claims not to be able to reveal the details.

To repeat: In his Amazon review, Lubos admits to reading a different "edition" of the book. However, there is only one edition published so far, so one infers that he read a draft manuscript, not the actual published book.

It is unethical to review a draft manuscript and claim that it is a review of a published work.

I continue to call on Lubos to do the honorable thing and withdraw his Amazon review.

26. **woit**
June 11, 2006

Chris,
It's certainly true that my academic status is not as high as that of a tenured professor, and it's quite reasonable for Cornwell to point that out. He also calls me a "humble maths instructor", which I have no problem with, although, depending on your interpretation of what "humble" is referring to, some might strongly disagree with that.

27. **Chris Oakley**
June 11, 2006

Peter,

Yes but this whole thing is annoying. If they said a "mere janitor" (like *Good Will Hunting*) or "mere lab assistant" then fair enough, but the fact is that you are just as qualified to have research ideas or to talk generally about the subject as any one of your tenured colleagues. I hate the way that the academic system tries to turn itself into a hierarchical priesthood. The reality is that research is a free-for-all where the good ideas stick (or at least, *ought* to stick) regardless of who they originated from.

28. **mark adams**
June 12, 2006

As a layman whose daughter has just finished a physics degree at Imperial, I've been getting a fuzzy glimpse of her ex-boyfriend's work at at Fermilab and Cern. Plenty politics, plenty bs but in a noble cause he thinks. The SHC will establish…..In commodities' trading and oil refining I've nearly always dealt with known and unknown unknowns. Your manner passes my smell test for discrimination.

Way Too Much Stuff

June 5, 2006
Categories: Uncategorized

I ordinarily keep a short list on my desk of things I've seen recently that I'd like to write about here. The last few days this list has gotten way too long, so I'll try and deal with it by putting as many of these topics as I can in this posting.

The June/July issue of the AMS Notices is out, with many things worth reading. The two long articles are one by Ken Ono about Ramanujan and one by Arthur Jaffe telling the story of the founding of the Clay Mathematics Institute and the million dollar prizes associated with seven mathematical problems. There's also a book review of Roger Penrose's The Road to Reality, news about the proposed US FY 2007 budget for mathematical sciences research, and an account of a public talk by Michael Atiyah, who evidently closed by explaining some of his very speculative ideas about how to modify quantum mechanics, then said:

This is for young people. Go away and explore it. If it works, don't forget I suggested it. If it doesn't, don't hold me responsible.

The June issue of Physics Today is also out. In its news pages it reports that Robert Laughlin is out as the president of the Korea Advanced Institute of Science and Technology (KAIST), and will return to Stanford in July "where he plans to teach, research, and write 'anything that brings income.'" The report gives conflicting reasons for why things didn't work out for him at KAIST, but notes that "90% of KAIST professors gave him a vote of no-confidence and nearly all deans and department chairs quit their administrative posts to protest his continuing in the job."

There's an extremely positive review of Leonard Susskind's The Cosmic Landscape by Paul Langacker, which ends:

The Cosmic Landscape *is a fascinating introduction to the new great debate, which will most likely be argued with passion in the years to come and may once again greatly alter our perception of the universe and humanity's place in it.*

Why any particle theorist would want to encourage other physicists outside their field to read this book and give them the idea that it represents something theorists think highly of is very unclear to me.

Finally there's an article by Jim Gates entitled Is string theory phenomenologically viable? Gates aligns himself with the currently popular idea that string theory doesn't give a unique description of physics:

The belief in a unique vacuum is, to me, a Ptolemaic view – akin to that ancient belief in a unique place for Earth. As I wrote in 1989, a Copernican view, in which our universe is only one of an infinity of possibilities, is my preference, but there were very few Copernicans in the 1980s.

He seems to promote the idea that one should not use 10d critical string theory and thus extra dimensions, but instead look for 4d string theories, and that perhaps the problem is the lack of a "completely successful construction of covariant string theory." For more about this point of view, see Warren Siegel's website.

There are quite a few idiosyncratic things about Gates's article, including the fact that he refers to non-abelian gauge degrees of freedom as "Kenmer angles", after Nicholas Kemmer (not Kenmer) who was involved in the discovery of isospin.

Some of his comments about string theory are surprising and I don't know what to make of them. He claims that "some aspects of string theory seem relevant to quantum information theory", and the one supposed observational test of string theory he discusses is one I hadn't heard of before and am skeptical about (observing string-theory-predicted higher curvature terms in Einstein's equations through gravitational wave birefringence). His discussion of supersymmetry seems to assume that observation of superpartners is unlikely, since for reasons he doesn't explain he expects their mass to be from 1 to 30 Tev. Finally, he worries that people will not investigate things like covariant string field theory since we are about to enter an "era that promises an explosion of data". I certainly hope he's right about the forthcoming availability of large amounts of interesting new data.

The Harvard Crimson has an interesting article about Ken Wilson.

John Baez is getting ever closer to having a blog in its modern form, now he has a diary.

Read about the tough summer life of theoretical physicists in Paul Cook's report from Cargese (which reminded me of when I went there as a grad student), and JoAnne Hewett's report from Hawaii (which reminded me of a very pleasant vacation I spent on the Big Island).

Science magazine has an article about progress on increasing luminosity at the Tevatron, hopes for getting enough events there to see the Higgs before the LHC, and the debate that is beginning about whether to run the machine in 2009.

Slides are available from the Fermilab User's Meeting.

There's a news story out from China (and picked up by Slashdot) about the new paper by Huai-Dong Cao and Xi-Ping Zhu soon to appear in the Asian Journal of Mathematics. This paper is more than 300 pages and is supposed to contain a proof of the Poincare conjecture and the full geometrization conjecture, filling in an outline of a proof due to Perelman, who used methods developed by my Columbia colleague Richard Hamilton. Other groups have also been working on this in recent years including my other Columbia colleague John Morgan together with Gang Tian; for another example, see the notes on Perelman's papers recently put on the arXiv by Bruce Kleiner and John Lott. Cao and Zhu have evidently been explaining their proof in a seminar at Harvard run by Yau during the past academic year, and Yau will talk about this at Strings 2006 in Beijing later this month. When the paper appears it will be interesting to see what some of the other experts in the field think of it and whether there's a consensus that the proof of Poincare and geometrization is finally in completely rigorous form.

Update: According to a blog entry from the Guardian, "Perelman seems to be active in string theory."

Comments

1. **sunderpeeche**
   June 5, 2006

   Why shouldn't Langacker give Susskind's book a favorable review? They might be friends (I do not know). The next to last paragraph states

   "Many scientists are strongly opposed to the multiverse-landscape paradigm. Some objections are technical. For example, are there really 10^500 vacua, or does the multiverse really exist? Others are that the ideas are not testable and not really science, or that they might seduce researchers into giving up the traditional goal of finding a unique and elegant explanation for the observed laws of Nature. Sussking makes no attempt to give an impartial overview — after all, he is advocating his own ideas. However, he does offer a reasonable survey of the objections and his own responses to them."

That’s a fairly balanced review statement. Langacker doesn’t try to whitewash the fact that many people disagree with the ideas in the book. It’s a reasonable review.

2. **Levi**
June 6, 2006

I think the review of “The Road to Reality” in the Notices is very well balanced. I would be curious to know what Peter and commenters think.

3. **Cloud**
June 6, 2006

If Cao and Zhu’s proof is right , what percent of contribution does their work occupy to the Poincare Conjecture? They will share the prize provided by the Clay Math Inst? An amateur

4. **Hujun Li**
June 6, 2006

Dear Prof. Woit:
I’m a science reporter of Southern Weekly, a very influential newspaper which circulates around China. Currently the Chinese media are highly praising the paper pubished by Profs. Huaidong Cai and Xiping Zhu. I’m wondering that if I can quote your blog words in my story, and could you say something more:
1. What’s your comment on the work of Cao-Zhu, and other groups like Morgan-Tian, Kleiner-Lott?
2. Some people thought that Prof. Shing-Tung Yau is making a hype:
(http://mathforum.org/kb/message.jspa?messageID=4767368&tstart=0)
(http://mathforum.org/kb/message.jspa?messageID=4768229&tstart=0)
Do you agree with them or not?
3. Cloud said:
“If Cao and Zhu’s proof is right, what percent of contribution does their work occupy to the Poincare Conjecture? Will they share the prize provided by the Clay Math Inst? ”
That’s also what I want to know.
Best regards,
Hujun Li
——————————
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http://tech.sina.com.cn/focus/lihujun

5. **Peter Woit**
June 6, 2006

Hujun Li,

It should be made clear that I am not an expert in this area of mathematics, and this blog entry is just quoting other widely available sources. I don’t think you should quote it, you should contact people who are experts. Of the people I have talked to about this, as far as I know they haven’t even seen the Cao-Zhu manuscript, and thus have no basis yet for an opinion.

6. **knotted string**
June 6, 2006

Physics Today is uncritical of speculation so its not surprising they publish nonsense. A decade ago (apr 96) they published Witten’s claim “String theory has the remarkable property of predicting gravity.” Still no string prediction a decade on.

7. **hack**
June 6, 2006

I did a little research, and it turns out Witten’s prediction of gravity has indeed been confirmed in experiments by physicists I. Newton and G. Galilei. I don’t know about you, but I think a Nobel is in order.

8. **Yidun**
June 6, 2006

http://www.wanyidun.com/blog_r2u/?p=65

9. **Bert Schroer**
June 6, 2006

Can anybody explain to me what is the physical meaning of a theory with several vacua (Gates) in view of the fact that the application of the cluster factorization means that (without loss of physical generality) one can decompose the theory into a direct sum of components where each component has a unique vacuum? This is a mathematical theorem and probably its derivation can be found in Streater-Wightman (presently not available to me).
Be careful, I am presently not saying that Gates is wrong, there may be a misunderstanding on my part.

10. **anon**
June 6, 2006

Prof. Schroer,

can you please comment on the paper of Grigore&Scharf “Against Supersymmetry” ?
Thank you.

11. **Bert Schroer**
June 6, 2006

Since I presently don't have the time to read the article, and I do not want to comment on just the basis of the abstract, I asked Dan Grigore to address this question on this weblog.

12. **Tony Smith**
June 6, 2006

I did not see the Cao-Zhu paper listed in the math arXiv. Why would it not be there?
I did see three Grisha Perelman Ricci-flow papers at math.DG/0307245, math.DG/0303109, and math.DG/0211159 , so it seems to me that precedent would be to put the paper on the math arXiv.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – I did not see on either the physics arXiv or the math arXiv any paper by Grisha Perelman that looked like a superstring paper, although some, such as Martin Rocek, have spoken about connections between Ricci flow and string theory, and I think that Perelman may have discussed such things. However, to me that does not mean that Perelman is an advocate of string theory, just that he is open to discussion about possible applications of his Ricci flow ideas to ANYTHING, even string theory.
Further, I did not see anything in the Guardian blog but an unsupported assertion.

13. **Peter Woit**
June 6, 2006

The Cao-Zhu work was not put on the arXiv. As far as I know, many of the relevant experts have still not even seen a copy.

Saying Perelman is active in string theory is a complete joke, which is why I quoted it. In his work he uses the kind of flow equation that also appear in renormalization group flows for sigma models, which I guess to string theorists makes him "active in string theory". I wonder where the Guardian writer got that nonsense.

14. **Yidun**
June 6, 2006

Cao and Zhu's full paper has not even shown up in the Journal yet. I guess by far only Shing-tong Yau and some Harvard mathematicians have read it. It seems that Yau is convinced.

15. **Richard**
June 6, 2006

I'm glad someone is attempting to present a complete unified presentation of a proof of the Poincaré conjecture. It would be a shame if this very important conjecture is finally declared proved, but left scattered to the winds in bits and pieces in various papers published in many different places.

16. **MathPhys**
June 6, 2006

I find it very odd that

1. The paper will appear next month in Asian J of Math, but has not appeared on the arXiv,

2. The result was announced in a press conference, rather than at a regular math meeting (or by simply posting the paper on the arXiv), and

3. In the abstract, the authors declare that their work is "a crowning achievement". I thought they should let the readers say that.

17. **Bert Schroer**
June 7, 2006

Since the authors of yesterdays "Against Supersymmetry" (Grigore&Scharf) did not yet react to my email, I feel that I should make some preliminary comments with respect to their obviously consequence-bearing statement. The title should be understood in the sense: the old suspicions about the incompatibility beetween supersymmetry and gauge theory finally passes to a No-Go theorem.
With their opening statement against the use of functional integrals for the calculation of physically relevant (renormalizable) results they run into my open doors; functional integrals are basically for incantation and baptization of QFT, but not for calculating renormalized results (they only have a metaphorical existence for unrenormalized correlation functions and are incorrect for renormalized correlations of strictly renormalizable theories). Functional integrals totally ignore the singular (distributional) nature of quantum fields and the people who worship them are usually those who believe that the ultraviolet problems are the real limitations of QFT, whereas in reality (Epstein-Glaser) the physical problem is to find finite-parametric islands (stable under a finite-parametric renormalization group) in the "universal" QFT which is a formal construct with infinitely many couplings (shared between Petermann-Stueckelberg and Wilson).
Atiyah belonged to the strongest venerators of functional integrals within the mathematical community and it is interesting to notice that he found other mathematical arguments which cast doubts on the depth and utility of supersymmetry (the starting point of participation in this particular blog section).
I still did not find the time to read the article (but I know the authors as very competent, although not exactly representing AQFT as one participant here was claiming), and I still hope that they will enter this blog and explain themselves what they have done.

18. **anon**
June 7, 2006

Prof. Schroer,

thank you for your comment.
So the LHC has a real chance to falsify the causal AQFT approach?

19. **Bert Schroer**
June 7, 2006

Well it is not the causal approach which is under discussion, but only what the authors call the minimally supersymmetric extended standard model. Other extensions are not investigated but of course their paper is an invitation to do this (including to themselves I suppose).

20. **Deane**

June 7, 2006

Perelman is not just a Ricci flow person. When he first appeared on the scene, he established his reputation in Riemannian geometry and its generalizations. He then disappeared for about 7 years (yes, just like Wiles). When he re-emerged, he started sending emails to Ricci flow experts, asking technical questions. To quote one reaction, "uh-oh". He's written three papers on how to use the Ricci flow to prove Thurston's conjecture. The rumors are that he has moved on and is now working on the Navier-Stokes equation.

21. **Florifulgurator**
June 7, 2006

MathPhys, the abstract says "crowning achievement of the Hamilton-Perelman theory of Ricci flow". This is not odd (no self-praise or theory-hype or whatever), it is true.

22. **Yidun**
June 7, 2006

MathPhys:

The full sentence in their abastract is "This proof should be considered as the crowning achievement of the Hamilton-Perelman theory of Ricci flow". I think that "a crowning achievement"should be understood in its context.

23. **DMS**
June 7, 2006

"I find it very odd that

1. The paper will appear next month in Asian J of Math, but has not appeared on the arXiv,

2. The result was announced in a press conference, rather than at a regular math meeting (or by simply posting the paper on the arXiv),"

Something similar happened when Andrew Wiles informed the world about his initial (flawed) proof. Mathematicians don't seem to use arxiv as much, ulinke theoretical physicists, a shame.

If Yau is wrong, it will be embarassing to him, so I don't think he is doing it lightly.

24. **MathPhys**
June 7, 2006

Wiles is far from being a computer/internet oriented man, and his proof was "announced" in a series of talks at the Newton Institute. It's still quite a different story.

No one thinks that the Cao-Zhu work is wrong. Everyone believes Perelman to be right, so it's a safe bet on Yau's part.

25. **MathPhys**
June 7, 2006

Yidun,

"This proof should be considered as the crowning achievement of the Hamilton-Perelman theory of Ricci flow".

Still sounds like something that a referee or a reader, rather than an author should say.

26. **Chris W.**
June 7, 2006

Regarding Laughlin's departure from KAIST, see the apparently well-informed comments on this post at *The Marmot's Hole*.

27. **MathPhys**
June 7, 2006

Chris W,

I read the post at The Marmot's Hole of Laughlin and KAIST. Very interesting. Very funny too. Almost a script of a funny movie on how third world countries (and I think S Korea is one) manage science.

28. **Lubos Motl**
June 8, 2006

One of my readers has predicted that Prof. Bert Schroer would endorse the crackpot paper "Against Supersymmetry" and he or she was completely right. The paper is completely absurd because supersymmetric gauge theories are a subset of ordinary gauge theories with a properly chosen spectrum and adjusted interactions, after all, so an anomaly of this kind would affect all gauge theories, not just supersymmetric ones.

What the authors have probably (it's hard to decode it in their very unusual formalism and terminology) found is that – in ordinary language – one can't work without the FP ghosts at one-loop level. Their "anomaly" is proportional to two copies of the structure constants, which looks like two cubic vertices in a diagram with a FP ghost loop. If they learned a basic class of quantum field theory, this would have been explained in the second semester. If they did their exercise correctly, they can easily cancel the anomaly by adding fermionic scalar fields in the adjoint representation, the ghosts, and their auxiliary superpartners if needed.

Even at the level of social sciences, a statement that there is a universal anomaly killing supersymmetric theories is a proof of crackpotism. 3,000 very smart people have written 20,000 papers that go well beyond naive checks that these two authors have attempted to do correctly but failed. Moreover, these theories follow from string theory, belong to the powerful web of dualities that imposes all required consistency criteria, and they have been recently studied at a much higher level of rigor in string theory as field-theoretical duals of particular gravitational backgrounds of string theory.

Prof. Bert Schroer has no idea about quantum field theory.

29. **Bert Schroer**
June 8, 2006

Lobos,
if anybody ever claims you understand QFT you may give him a thorough spenking. It seems that the stringy alimentation you already received in Prague has damaged your brain beyond repair.

30. **Lubos Motl**
June 8, 2006

Dear Prof. Schroer,

if you can't learn quantum field theory – indeed, you could not have passed my course QFT II in the spring because this was a standard part of the material written in the syllabus – you could at least try to struggle and learn my name.

My name is Luboš.

All the best to you and all the crackpots here
Luboš

31. **MathPhys**
June 8, 2006

Lubos,
I wrote papers on supersymmetry. Does that make me very smart?

Anyway, I must agree with Lubos on this one, and I'm willing to bet good money that there is no problem, and that the paper (which I haven't read) contains a basic mistake.

Maybe it's a mistake along the lines that Lubos anticipated, or maybe something even more banal.

There is no doubt that susy gauge theories are perfectly well behaved, and the more supersymmetric, the better behaved.

Incidentally, there was a little 'scare' along the same lines around 1984 (when Lubos was still in diapers) when 'tHooft and a PhD student (van Damme) claimed that susy gauge theories have an anomaly at 2 loop level, and therefore unrenormalizable. They were wrong. Van Damme made a computational mistake.

32. **Lubos Motl**
June 8, 2006

Dear MathPhys,

does it make one smart? It may depend on the content of the paper. But more generally, the answer is probably Yes. One needs to be above a certain lower threshold of IQ to be able to write at least a slightly meaningful paper about supersymmetry, and all the data I have indicate that your knowledge and intelligence in the context of gauge theory probably exceeds those of Prof. Schroer.

All the best
Lubos

33. **Bert Schroer**
June 8, 2006

I told you that I have not red the paper (I am still on the road) and that I know both authors as very competent and careful colleagues. Since I was asked on this weblog to say something and since my attempts to get the authors directly involved failed up to now, I tried to at least understand what they are claiming. Nothing to get emotional about. This is what a weblog is for, isn´t it. The previous statement about the physical aspects of supersymmetry which I made in connection with a remark attributed to Atiyah I of course stand by, but they fall short of what these two authors are claiming.

34. **MathPhys**
June 8, 2006

Lubos,

These were some of the silliest and most trivial papers I've ever written. It's too easy.

You see, I know supersymmetry well enough to believe that, while perfectly consistent, susy is the most obvious stumbling block in the development of theoretical high energy physics today.

There is something very wrong with the idea of sypersymmetrizing everything until you can compute something, then break the supersymmetry by hand and push everything you don't want all the way up till you can't see them (only to find that you recover the problems that made you supersymmetrize in the first place).

It is just too easy, and too contrived at the same time. And it doesn't work.

35. **Bert Schroer**
June 8, 2006

MathPhys,
it was just this issue of breaking of supersymmetry which I commented on earlier. It is precisely at this point where SUSY outs itself as being eerily different from any other symmetry under the sun.

36. **Lubos Motl**
June 8, 2006

Dear Prof. Schroer,

Sir Atiyah has co-authored at least one famous paper in which four-dimensional supersymmetric non-Abelian gauge theories play an important role:

http://arxiv.org/abs/hep-th/0107177

Well, yes, one can probably guess which of the two authors wrote most of these physics-related sections of that paper. The complexity and the mathematical depth of this Atiyah-Witten paper exceeds the complexity and depth of the paper by Grigore and Scharf by two orders of magnitude or more.

But Sir Atiyah is a mathematician who does not have to understand supersymmetric gauge theories well and he would never be foolish enough to make far-reaching statements about topics that he does not understand, unlike you.

You may find Grigore and Scharf to be "careful" and "competent" colleagues, according to your standards, but according to the usual standards, they are something very different because the one-loop structure of gauge theories – and all possible anomalies and non-anomalies that might occur at one-loop level – is a standard material in every modern textbook of quantum field theory and no careful and competent physicist would ever publish a paper making far-reaching statements about these topics before he understands their standard presentation in the textbook.

This can't really be compared with the two-loop paper by van Damme et al. in 1984 because one-loop level is something much more elementary, and moreover 22 years have ended since the van Damme et al. paper.

But certain people just think that they can make important discoveries while ignoring virtually all insights that have been made in the last 50 years, checked, re-checked, experimentally verified, and summarized in quantum field theory textbooks. And this is the context in which I just find the word "crackpot" appropriate.

All the best
Lubos

37. **MathPhys**
June 8, 2006

Dear Prof Schroer,

Yes, I get your point.
Incidentally, are you permanently based in Rio now? Do you like the working conditions there?

38. **Bert Schroer**
June 8, 2006

It is worthwhile to remember that wayback there were serious problems to find regularizations which maintain gauge invariance and supersymmetry simultaneously. Not knowing whether these problems have been resolved meanwhile, I find it reasonable to take from AQFT the idea of an approach which avoids regulators and cutoffs altogether and to base perturbation theory on ideas which take care of the distributional aspects of fields throughout the calculation (e.g. the Epstein-Glaser approach). What confuses Lubos so much is that the authors prefer this setting in order to stay on the safe side.

39. **MathPhys**
June 8, 2006

Lubos,
For some very odd reason, British knights are called by their first name, rather than surname, after 'Sir'. So, one says 'Sir Michael' and 'Sir Roger', and not 'Sir Atiyah' or 'Sir Penrose'.
On the other hand, following 'Lord', one uses a chosen name, but the origins of that would be too difficult to explain (Martin Rees is Lord Ludlow).
When in doubt, just call him 'Professor Atiyah'. He wouldn't mind.

40. **MathPhys**
June 8, 2006

Prof Schroer,
There is no problem regularizing supersymmetric gauge theories. Olivier Piguet can tell you all about that.

41. **Bert Schroer**
June 8, 2006

Yes I live in Rio where I have an apartment, but I am presently in Sao Paulo. I retired from the FU-Berlin and the working conditions, apart from a 2 by 3 m desk corner in an office I share with another pensionist Prof. Walter Baltensperger (look up his name in physics/ he has some interesting ideas about the origin of the tropical flora and fauna in earlier geological epochs of siberia) I am creating my own working conditions.

42. **Lubos Motl**
June 8, 2006

They obviously don't stay on the safe side if they can "derive" so completely ludicrous "results". Prof. Schroer is profoundly confused which insights in physics are robust and which are not. As far as I can say, the Epstein-Glaser approach is exactly as flawed as everything else that Prof. Schroer has ever attempted to sell. It contradicts the genuine renormalization group behavior of all the operators and interactions.

Epstein & Glaser also have roughly 100 citations per 33 years. Prof. Schroer must believe that there has been some world-wide conspiracy that makes this important (?) paper look 30 times less important than papers that most experts find really important.

Whether or not one uses a regularization that preserves both gauge symmetry and supersymmetry simultaneously is irrelevant. Even in the case of ordinary gauge theory itself, one can use regulators that don't manifestly preserve supersymmetry. It just means that gauge-invariance-violating counterterms can be generated and must be canceled, as additional renormalization conditions (masslessness of the photon in QED, for example). But once they are canceled, the results are equivalent to manifestly gauge-invariance regularizations such as dim. reg.

Analogously, we may use dim. reg. for SUSY gauge theories which preserves the gauge symmetry. Supersymmetry is then also preserved as long as the couplings are renormalized so that the supersymmetric relations between them are preserved at every order, which is easily seen to be possible in the full quantum theory if it is possible in the classical theory. If you just define Feynman diagrams in superspace, things become more or less manifest.

It may be difficult to put SUSY on the lattice – less difficult with the help of deconstruction – but it is straightforward to preserve SUSY by renormalization at the loop level.

43. **Lubos Motl**
June 8, 2006

"that don't manifestly preserve supersymmetry." in the previous comment should have been "that don't manifestly preserve gauge symmetry".

44. **Bert Schroer**
June 8, 2006

Mathphys,
are you sure that Olivier Piguet does not use the regularization-free algebraic approach? (his work with Sorella is often called “algebraic renormalization”.)

45. **Bert Schroer**
June 8, 2006

Lubos, you are in a weblog about particle physics and not in a bookmaker´s shop.

46. **Lubos Motl**
June 8, 2006

“Lubos, you are in a weblog about particle physics and not in a bookmaker´s shop.”

It certainly does not look so. My feeling is just the opposite.

47. **Lubos Motl**
June 8, 2006

If you want to see how a particular physics blog looks like, see e.g.

http://www.google.com/search?hl=en&q=particle+physics+blogger

48. **Lubos Motl**
June 8, 2006

particular -> particle

49. **MathPhys**
June 8, 2006

Piguet uses a ‘modern’ version of BHPZ. Yes, they call it ‘regularization free’, but I’m not so sure that that’s a fair name. There is a lot of debate about that that I don’t wish to get involved in. There was also work by I Jack and D R T Jones (disproving van Damme and ‘t Hooft), where they obtain the same results as Piguet et al, but using a version of dimensional regularization that somehow preserves supersymmetry.

The message is that one can subtract the infinities, while retaining susy and gauge invariance. I’m sure these issues were settled more than 20 years ago.

50. **Bert Schroer**
June 8, 2006

MathPhys,
if I read implicitly between the lines of your blog and take an optimistic (or pessimistic, depending on one’s point of view) interpretation, then you are saying that the off-shell BPHZ (Piguet-Sorella) approach applied to the model (which Grigore-Scharf take to make their point) would be free of that anomaly which they find in the on-shell Epstein-Glaser approach. This is of course a possibility, although I maintain that statements from Grigore-Scharf merit high respect (they are assured of that respect not only from me, but also from Raymond Stora), notwithstanding the obvious fact that we are all fallible (except jesters on the string court as Lubos Motl).

51. **Bert Schroer**
June 8, 2006

MathPhys,
Standard literature works in the BRST approach, i.e. one is studying some classical field theory with Grassmann fields and then constructs the generating functional for the Green functions. Everybody supposes that this process will lead to a good theory in some Hilbert space. One could take the Grigore-Scharf paper as an indication that this road is not so easy. So, you guys who are working in this direction should really construct the quantum theory in all details at least up the the second order of perturbation theory
starting from the functional approach. Then we will see if there is a way to circumvent the no-go result of G-S or if susy is in trouble

52. **JPL**
June 8, 2006

MathPhys:

“You see, I know supersymmetry well enough to believe that, while perfectly consistent, susy is the most obvious stumbling block in the development of theoretical high energy physics today.

There is something very wrong with the idea of sypersymmetrizing everything until you can compute something, then break the supersymmetry by hand and push everything you don’t want all the way up till you can’t see them (only to find that you recover the problems that made you supersymmetrize in the first place).

It is just too easy, and too contrived at the same time. And it doesn’t work.”

My feeling is that what you recognize here is, probably, a sharper indictment of SUSY than the whole of Grigore-Scharf’s argument.
You are also not the first person, among those who actually worked in SUSY whom I have heard express those suspicions…

53. **Alejandro Rivero**
June 8, 2006

Most probably susy could emerge not for everything but in part, or as an approximate symmetry. The fact is that something must control the divergence of the scalar particles; and this something should be a kind of cancellation between diagrams. Such cancellation mechanism, when found, should not be far of the one of supersymmetry.

54. **Chris W.**
June 8, 2006

More interesting stuff from Sean Carroll on *CV*, which might deserve its own post here.

55. **Bert Schroer**
June 8, 2006

Alejandro Rivero,
We all agree about the perturbative aim: to cut a breach into the universal infinite dimensional space which incorporates all particles with all coupling strength, e.i. to find selfclosing finite parametrig islands under the action of a finite-parametrig RG (Petermann-Stueckelberg, Wilson, any way you want).
But the standard way you want to cut these breaches is much too narrow: start with pointlike free fields couple them paying attention to the power counting. You will get stuck, precisely at the place where we are now and supersymmetry does not seem to help in this, it just pushes the problems to a higher order nbut seems to be incapable to generate new islands.
Imagine you start with massive vector fields coupled to themselves and to other scalar and spin=1/2 fields. Any coupling you contemplate to write down, I repeat any coupling will carry you to dimension 5 for the interaction (a massive free vectorfield has scaling dimension 2 and not 1) which is way beyond the limit of renormalizability. We all know what we can do in such a situation, we modify the one-vectormeson space by non-Hilbert BRST stuff and get it down to scale dimension one (paying attention to consistency problems which may require the enlargement by additional physical degrees of freedom). Then we descend thanks to the cohomological properties of BRST and finally obtain a physical theory (back to Hilbert space) in which, lo and behold the the vectormeson really has the physical dimension 2. But we don't know the physical reason why we are doing this.
Alejandro, before you are not perturbed by this magic BRST "catalyzer", you will never make any progress and your proposal to look into the neighbourhood of supersymmetry will only be a loss of time.
The secret to maintain dimension 1 even without that catalyzer is to permit the free fields to have a spacelike semiinfinite stringlike extension (already mentioned before on several occasions) and stay all the time in Hilbert space. I am very confident that the next step of how to implement interactions for such stringlike fields will be understood in the near future; the theory is expected to have a pointlike subalgebra (which must be idential to the old gauge invariants, otherwise the attempt must go into the dustbin). Whereas the BRST magic is essentially limited to spin one, there is no such limitation for the idea of working with stringlike localized free fields instead of the usual Lagrangian pointlike fields. Stringlike localized fields fluctuate in x and in e (unit spacelike direction = de Sitter) and having part of the fluctuation strength in e you can improve the short distance properties in x. I predict it will be this Feynmanian spacetime viewpoint (and not those momentum space manipulations) which will lead to those above islands. The closeness of supersymmetry to such a spacetime view is a Fata Morgana.

56. **MathPhys**
June 8, 2006

Lubos,
When you say that one can put susy on a lattice "using deconstruction", what do you mean?

57. **Anonymous**
June 8, 2006

MathPhys: see, e.g., http://arxiv.org/abs/hep-lat/0503039. As far as I am aware no one knows how to do N=1 SUSY on a lattice, but (for instance) N=4 works.

58. **Aaron Bergman**
June 8, 2006

Try hep-lat/0602007.

59. **MathPhys**
June 9, 2006

Thanks.

60. **Alejandro Rivero**
June 9, 2006

Bert, ah, but I do not propose to look in the neighbourhood of supersymmetry. What I say is that we will first to find the mechanism, and later someone will find a mean to reformulate it as an approximate supersymmetry, because there are tools and coincidences -say, climatic preconditions, to follow your last metaphor- enough to be able to do it.
As for the BRST "catalyzer", I had never thought about it in the way you describe; but it seems worthwhile to take a time on this reflexion.

61. **Bert Schroer**
June 9, 2006

Alejandro,
All free pointlike fields with short distance scale dimension larger than 3/2 (equivalent to spin larger than 1/2) generate a barrier against renormalizability because you cannot find interactions with dimension bounded by 4. The first such beyond case is a free vectormeson (taken massive in order not to confront infrared and ultraviolet problems together) which has dimension 2. Historically we are inclined to follow t'Hooft and invoke the classical gauge principle although it forces us to violate important principles of QT (positivity) in intermediate steps which classically is not an issue at all. The modern form of this intermediate violation (the catalyzer) of QT is BRST. Although we should be grateful for this metaphoric trick which permitted us to start with a free vectormeson of dim=2 and obtain an interacting vectormeson of dim=2 plus logarithmic corrections (i.e. to get around that standard 3/2 barrier), we should on the other hand uphold our Heisenberg heritage which requires to find a formulation which avoids any (even intermediary) use of objects which are not observables. The prerequisites of such a step have already been accomplished: use string-localized potentials for those pointlike field strength as explained in previous blogs. The suggestion can already be found in an old paper of Mandelstam, but the new concept of modular localization leads to a conceptual and mathematical backup for its implementation. The logic of t'Hooft
Lagrangian quantization + classical gauge principle –> renormalization consistent with unitarity
was always subterfuged by the observation that there is only one renormalizable selfinteracting vectormeson theory, namely the one we know (whereas in classical field theory you can write down many Lorentz invariant interactions involving a vectorpotential, that's why you need the selective gauge principle), so we really do not need any principle in addition to renormalizability. In addition, if you abandon the Mexican hat setting and start from the very beginning with massive vectormesons, renormalization theory will convey the very interesting message to you that it wants an additional physical degree of freedom (whose simplest realization is a scalar field, the alias Higgs but now with a vanishing vacuum expectation)
http://br.arxiv.org/abs/hep-th/9906089
Actually this idea is implicit in previous work of Scharf. Remember Gunter Scharf from that discussion yesterday? the one which together with his use of the Epstein-Glaser renormalization approach was vilified by a Harvard professor (Lobos with the Lubotomy).
The advantage of use of string-localized free fields is that (after understanding who to use them in the presence of interactions), different from the gauge idea, there is no limitation to spin=1.

62. **Milnor the Giant!**
June 19, 2006

It seems to me that ST Yau is a fame seeker: he exists wherever there is room for him to expand his reputation. Someone told me that in some Chinese news, Yau claimed that 50% of the credit in "resolving" Poincare Conjecture goes to Richard Hamilton, who developed the Ricci Flow WITH *HIS* (Yau's) SUGGESTION; while Perelman's work worths only 20% and his loyal student Cao and loyal friend Zhu's work takes up 30% of the total credit. What a shame!

The Chinese Media is somewhat ignorant about what actually is going on in the mathematical community and totally rely on the words of this what they called "World famous mathematician, Harvard Professor, and Field Medalist Shing Tung Yau". This creates some sensational moments in China right now. And the report is misleading: it makes people think that the two Chinese mathematicians Zhu XiPing and Cao HuaiDong completely resolved the Millenium Million Dollar Problem Poincare Conjecture. S.T. Yau described that Zhu and Cao's made the final step in resolving the Poincare Conjecture.

It seems that the ultimate winner is Yau. Why? Because of his vision in Ricci Flow, playing 50% in the Poincare proof, he suggested and encouraged Richard Hamilton to investigate it. Then his student Cao and his friend Zhu made the final step into the gate of the PROOF 30%. He seems to imply that he is the MAN behind Poincare Conjecture! Truely beautiful! No Wonder! In several of his speeches, he told people that he had two goals: one is to make himself an everlasting mathematical icon; the other is to help China build world-class math institutes like the IAS. The first one is for real, while the second one has but only an reinforcing kind of effect.

The glory of Fermat's Last Theorem introduced some sensational moments. Andrew Wiles and other mathematicians became the heroes. Although they might have competed to prove Fermat's, they were not malicious towards any of the competitors. On the other hand, they appreciate and admire their fellow mathematicians' efforts in triumph over human intellect. Tian Gang and John Morgan as well as many other mathematicians are also examining Perelman's work and have produced some results in the Poincare Conjecture. Rumor has that Yau was one of the Main editors of the Journal, he would not like that Tian receives such a glory; and he realize that his loyal supporters' work can beat Tian and Morgan's –whichever comes out first wins… Everyone would like to have a moment when Andrew Wiles finished the Proof of Fermat's. But for me this seems like a deceptive stealing.

I do admire Perelman. He's such a humble mathematician. Having made such a revolutionary work, he still works as hard as usual, totally doesn't care about fame or prizes. His contribution to Poincare Conjecture is obvious. He's just a modern Grothendieck or if he doesn't like be called that way, he is just Perelman! If one solves a great problem, he knows that he's triumphed over himself and the problem already; he does not need any complimentary prizes or whatsoever. Because he just knows he can do it and has done it. While for those who always dreamt of making himself more famous and influential but are not able to actually accomplish it, they can only be like that MAN behind Poincare Conjecture.

Yau once claimed that one of his students received Veblen under his influence on the committee. But his student's work is actually not belonging to first class mathematics and he regret helping him. Oh, good lord, how powerful he is!

63. **woit**
June 19, 2006

Please, no more attacks on Yau or anyone involved in the Poincare Conjecture proof. There's too much contention on this blog already.

For the record, I don't think it makes any sense to assign numerical values to people's various contributions to this.

64. **Milnor the Giant-wrong!**
June 19, 2006

woit is right at holding back attacks on Yau. "Milnor the Giant" is just wrongly attibuting to Yau something other people said. Actually you can find some English blogs citing one Xinhua news in Chinese, saying a Chinese mathematician Yang Le estimates a roughly total 105% (Hamilton 50%, Perelman 25%, Zhu-Cao 30%). This cannot be taken seriously. I doubt "Milnor the Giant" was informed wrongly on purpose.

In the original Xinhua news this mathematician was cited saying Perelman's work is 70 pages, now Zhu-Cao 300 pages; hinting that they fill the gaps left by Perelman.

http://news.xinhuanet.com/english/2006-06/04/content_4644754.htm

Let wait and see what will happen in ICM Madrid.

65. **comentator**
June 20, 2006

The proof of the Poincare conjecture has eluded quite a few mathematicians for many years. It would be surprising to know that the chinese mathematicians in two years were able to solve the conjecture without the help of Perelman, Hamilton and others. So at most I believe that their contribution amount to a 10% if it is correct, as per filling gaps and important steps, that will have to be studied and checked.

66. **Peter Woit**
June 20, 2006

The purported proof by Cao-Zhu (I haven't talked to anyone who has seen it) is based on the arguments developed by Hamilton and Perelman.

Populating the Landscape

June 9, 2006
Categories: Uncategorized

Study of the string theory landscape seems now to have become the hot research topic that one should be working on in order to be taken seriously as a cutting-edge researcher in particle theory. Last week there was a workshop at Trieste on [String Vacua and the Landscape]() that drew many researchers. Some of the talks from the workshop are available [on-line]().

Following on the heel's of Susskind's [popular book]() promoting the landscape, which has received excellent reviews from particle and string theorists, there's a new one on the same topic coming out later this month from cosmologist Alex Vilenkin, entitled [Many Worlds in One: The Search for other Universes]().

As string theorists in search of something to write papers about pour into the landscape, with its more than 10^{500} possible hot research topics to work on, Sean Carroll [reports]() from a cosmology workshop at the Perimeter Institute that trouble may be ahead for the subject. Sean gives a short summary of the talks at the workshop, in the majority of cases ending with "Made fun of the landscape", or "Made fun of the anthropic principle".

The main argument for the landscape mania has always been that it justifies Weinberg's "prediction" of the size of the cosmological constant. I've written elsewhere about why this is not a legitimate scientific prediction, and is off by at least an order of magnitude anyway. Evidently Steinhardt and Turok are about to put out a paper claiming that the situation is much worse than this, that if you take anthropic reasoning seriously, the natural "prediction" of the landscape is that:

the cosmological constant should be quite large (many times the matter density, although presumably not at the Planck scale), and we should live in a single lonely galaxy in an empty universe dominated by vacuum energy.

It will be interesting to see if landscapeologists will be willing to admit that the only supposed "prediction" of this subject doesn't work at all, and that it is not only pseudo-science, but failed pseudo-science.

Comments

1. **sunderpeeche**
   June 9, 2006

   All of this just goes to demonstrate the truth of the maxim "Physics is an experimental science". There's just no data beyond the SM (from cosmology there's dark matter and dark energy, but one cannot do controlled expts as a function of pressure and temperature etc, or produce particles of dark matter to measure decay cross-sections and branching ratios, for example). Ultimately, there has to be some data beyond SM (but nobody knows when/where this will happen). Until then theoretical speculation will just pile on speculation.

2. **MathPhys**
   June 9, 2006

   Steinhardt and Turok are serious people. This is very interesting news.

3. **island**
   June 9, 2006

   Does this mean that Lenny will be joining "the other side"…?

   > Amanda Gefter:
   > If we do not accept the landscape idea are we stuck with intelligent design?
   >
   > Leonard Susskind:
   > If, for some unforeseen reason, the landscape turns out to be inconsistent – we will be hard pressed to answer the ID critics.

   –

   Steinhardt and Turok finally discovered the runaway effect, or what?

4. **B**
   June 9, 2006

   sunderpeeche Says:

   There's just no data beyond the SM (from cosmology there's dark matter and dark energy […]

   There is dark matter, there is dark energy, there are neutrino masses, there is the horizon-/ homogeneity/ flatness-problem, the matter-antimatter asymmetry, there are a whole collection of astrophysical mysteries (e.g. the 'axis of evil', the GZK-cutoff, and the Pioneer anomaly (the latter two, I should add, I don't believe in)), then there are the well-known mysteries within the SM: the hierarchy-problem, the values of the Yukawa-couplings, EWSB, family symmetries, etc. If you ask me, we have plenty of data beyond standard, even though not from collider physics. It's the theorists side that has to catch up.

   Best, B.

5. **B**
   June 9, 2006

   As string theorists in search of something to write papers about pour into the landscape, with its more than 10^500 possible hot research topics to work on, Sean Carroll reports from a cosmology workshop at the Perimeter Institute that trouble may be ahead for the subject. Sean gives a short summary of the talks at the workshop, in the majority of cases ending with "Made fun of the landscape", or "Made fun of the anthropic principle".

   Not sure that is a good development. I'd rather see the topic die silently, and vanish into nirvana.

   I remember last summer I was on a conference where a student gave a talk about some class of string-models whose properties he had made statistics about. He had a selection of about 800 of these models, he said.

The next speaker was HP Nilles, who mentioned the previous talk, and then remarked very dryly that this means, it would take him only 10^ four hundred-something grad. studs to scan the whole landscape 😉

Best, B.

6. **melvineloy**
June 9, 2006

The 2006 Simons Workshop in Mathematics and Physics will have as one of its main focus the Landscape topic. This workshop will take place at Stony Brook University.

http://insti.physics.sunysb.edu/itp/conf/simonswork4/

7. **Tony Smith**
June 9, 2006

sunderpeeche said "… there's dark matter and dark energy, but one cannot do controlled expts …".

Actually, there is one controlled dark energy experiment now under way, by P A Warburton of University College London. It is EPSRC Grant Reference: EP/D029783/1, "Externally-Shunted High-Gap Josephson Junctions: Design, Fabrication and Noise Measurements", starting 1 February 2006 and ending 31 January 2009 with £ Value: 242,348. Its abstract states in part:
"… A possible source of this dark energy is vacuum fluctuations which arise from the finite zero-point energy of a quantum mechanical oscillator, hf/2 (where f is the oscillator frequency). … A recent publication by Beck and Mackey … suggests the possibility that dark energy may be measured in the laboratory using resistively-shunted Josephson junctions (RS-JJ's). Vacuum fluctuations in the resistive shunt at low temperatures can be measured by non-linear mixing within the Josephson junction. If vacuum fluctuations are responsible for dark energy, the finite value of the dark energy density in the universe (as measured by astronomical observations) sets an upper frequency limit on the spectrum of the quantum fluctuations in this resistive shunt. Beck and Mackey calculated an upper bound on this cut-off frequency of 1.69 THz. … We therefore propose to perform measurements of the quantum noise in RS-JJ's fabricated using superconductors with sufficiently large gap energies that the full noise spectrum up to and beyond 1.69 THz can be measured. … By performing experiments on both the nitrides and the cuprates we will have two independent measurements of the possible cut-off frequency in two very different materials systems. This would give irrefutable confirmation (or indeed refutation) of the vacuum fluctuations hypothesis. …".

I am aware that there exist no-go type theoretical objections to the work of Beck and Mackey, but it is interesting to me that experiments are under way to answer the question experimentally, thus effectively testing the assumpions of the no-go theoretical objections.

Note that Warburton sees his experiment as worthwhile whichever way (confirmation or refutation of Beck and Mackey) it turns out.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

8. **Jonathan Vos Post**
June 11, 2006

If the data from Lund Observatory in Sweden by professor Sveneric Johansson and his PhD student Maria Aldenius in collaboration with Dr Michael Murphy, Cambridge, UK means change in fundamental constants, I wonder how one can "explain" changes in fundamental constants in terms of our universe following a particular trajectory through the multiverse/superspace landscape of all possible universe with fixed fundamental constants? What are the constraints on such trajectories? Do either landscaper or stringers answer these foundational questions? Are such explanations consistent with the April 21 this year findings published in Physical Review Letters implying that a dimensionless constant – the ratio between the electron mass and the proton mass – has changed with time?

Variable Physical Laws
http://www.sciencedaily.com/releases/2006/06/060609122206.htm

9. **csrster**
June 12, 2006

Surely if The Landscape makes a wrong prediction it stops being pseudo-science and just becomes failed science?

LHC Predictions at Seed

June 9, 2006
Categories: Uncategorized

The latest issue of Seed magazine (not yet available online, as far as I know), contains responses from various well-known physicists who were asked what they hoped to learn from the LHC. Here's the gist of what they had to say:

Lisa Randall: *The magnificent thing is that we know there should be an answer to the question of the weakness of gravity, and that it should be revealed at the LHC.*

Leon Lederman: *The long-simmering concern over the weakness of Einstein's gravity may well be confronted. However, what is for sure is that the LHC, with its awesome reach, will answer all of our current astro-particle problems…*

Alexander Vilenkin: *If no trace of supersymmetry is found, this would be – necessarily indirect – evidence for the existence of the multiverse.*

Sir Martin Rees: *I'm hoping that it will clarify the nature of the particles that constitute the "dark matter" in the universe.*

Edward Witten: *The LHC wil tell us whether this notion is correct, and if so how it works.*

Max Tegmark: *Our theory of particle physics has 26 pure numbers in it. Why do they have these particular values? How did the universe begin? Did it?*

Leonard Susskind: *I see only two possible outcomes of the LHC project – either there will be low energy supersymmetry or there won't. … the big question is whether the gauge hierarchy fine-tuning is similar to the cosmological constant fine-tuning, or if it has a more conventional supersymmetric explanation.*

Steven Weinberg: *What terrifies theorists is that the LHC may discover nothing beyond the single neutral "Higgs" particle.. We fervently hope for some complicated discoveries.*

John Schwarz: *There are many speculative ideas for possible discoveries at the LHC. These include indications of extra dimensions, black holes, strings, magnetic monopoles, etc. I believe that all of these exist, and I would be thrilled to have experimental confirmation – but I am pessimistic about the prospects for finding them in the LHC's energy range… I could be wrong. That's why it is important that these experiments be carried out.*

Sean Carroll: *The beauty of science is that we don't know what surprises may await us in these domains.*

Gordon Kane: *The LHC could discover the superpartners in a supersymmetric world. In addition to strong theoretical evidence for Higgs physics, there is strong experimental evidence that Higgs particles do exist with a mass implied by supersymmetry… Probably the main thing we have learned in the past two decades is that any understanding of nature at the most fundamental level (beyond a description) will require extending our thinking to embed our world in extra dimensions… An optimist (like me) can make the defendable argument that the LHC could test supersymmetry, establish string theory and move on to the remaining "why" questions.*

My predictions: The LHC won't see supersymmetry and won't tell us anything about dark matter, dark energy or why gravity is weak. Witten has it exactly right, what the LHC will do will be to start to tell us what is causing electroweak symmetry breaking. It's possible that Weinberg's worry will be borne out, and, at LHC energies, we'll just see what appears to be an elementary scalar, which will be depressing. But I think it's equally likely that symmetry breaking is not coming from an elementary scalar, but from something much more interesting, quite possibly something we haven't thought of, and the LHC may start to give us evidence for what this is.

I'll also predict that we are still a few years from finding out what the LHC will tell us. These will be completely wasted years for particle theory if people just give up on looking for new ideas and sit on their hands (or wander pointlessly in the landscape), waiting for the LHC to revive the subject.

Comments

1. **Alejandro Rivero**
   June 9, 2006

   Yes, Witten gets it exactly right. But why do the others fail to answer right?

2. **robert**
   June 9, 2006

   AR

   Papal infallibility would be a cheap shot. Maybe he doesn't have an obvious agenda, other than his work/research ethic?

3. **rrtucci**
   June 9, 2006

   I think that theoretical high energy physicists have speculated enough already. Time to wait for the data to come in. I suggest that, in the meantime, they should work on quantum computing. This would help balance their highly unbalanced Doritos diet. There was a time when theoretical physicists (Landau, Feynman, …) could chew gum and walk at the same time.

4. **Shantanu**
   June 9, 2006

   Can someone explain to me how one can find indications of magnetic
   monopoles at LHC (John Schwarz's comments)?
   Thanks

5. **Anonymous**
   June 9, 2006

   By "why is gravity weak", you should understand "why is the weak force strong," a question that one hopes will be answered by the LHC. That is, is EWSB natural or is it finely-tuned?

6. **Chris Oakley**

June 9, 2006

I agree with EW, PW and AR, and what are those who think otherwise smoking?

7. **Eric Dennis**
June 9, 2006

You gotta love Vilenkin's reasoning: Prob(Landscape | No SUSY at LHC & String Theory) > Prob(Landscape | SUSY at LHC & String Theory), therefore Prob(Landscape | No SUSY at LHC) > Prob(Landscape | SUSY at LHC). Because of course Prob(String Theory) = 1.

8. **Trackback from LM blog**
June 9, 2006

… Gordon Kane's answer is the best one because it maximizes the product of "interesting content" times "good motivation". SUSY is going to be seen, and obnoxious critics of physics will finally be annihilated. Others either give overly speculative and unlikely answers or uninteresting answers …

http://motls.blogspot.com/2006/06/lhc-predictions-gordon-kane-wins.html

9. **Aaron Bergman**
June 9, 2006

ED:

That would be interesting except that Vlienkin isn't a string theorist. As Sean mentioned in his recent post, many cosmologists are landscapists without any need for string theory.

10. **island**
June 9, 2006

> Alexander Vilenkin: If no trace of supersymmetry is found, this would be – necessarily indirect – evidence for the existence of the multiverse.

This must fall from the same sort of logic that derives that evidence that we're not here by accident is evidence that we are.

11. **sunderpeeche**
June 9, 2006

I see that the list of experts does not include Carlo Rubbia (in fact, other than Lederman there are no expt physicists quoted). People forget that Rubbia did in fact discover supersymmetry (UA1 monojets), also the top quark (Mt = 44 GeV, anyone remember?). Put CR in charge of an LHC expt and there would be monopoles + strings + multiverse in a week.

But, really, only 1 expt physicist in the list?

12. **Anonymous**
June 9, 2006

sunderpeeche:

Experimentalists give boring answers, because they have a realistic sense of the machine's limitations. Similarly, the lack of collider phenomenologists except Gordy Kane, who in his zeal tends to exaggerate what can be done.

13. **sunderpeeche**
June 9, 2006

So the choice is: talk nonsense or be boring? (I think Witten and Weinberg gave good answers, although not "exciting" ones.)

14. **B**
June 9, 2006

I favour the scenario:

They find nothing at all. No Higgs. No susy. No monopoles. No nothing.

15. **Lubos Motl**
June 9, 2006

Dear B,

what you write violates the rule of logic that the total probability of alternatives equals 100%. It is not possible to find nothing at 1 TeV, not even Higgs, because it violates unitarity of the WW WW scattering. Without the Higgs, the Standard Model gives you too high a prediction for the cross section of this process that would make the total probability of scattering greater than 100%.

Best
Lubos

16. **Anonymous**
June 9, 2006

Theorists already predicted everything LHC can see.

17. **Eric Dennis**
June 9, 2006

Aaron,

You're right, I just looked him up at the arxiv. I suppose it's possible he thinks Prob(SUSY | Not String Theory) is not small, in which case his reasoning would be more complicated. Hard to tell.

18. **B**
June 9, 2006

Lubos Motl Says:

what you write violates the rule of logic

Dear Lubos,

whether that conclusion is one of logic depends on the validity of the axioms you use to draw the conclusion. I agree with your argument though.

Best, B.

19. **The Anti-Lubos**
June 9, 2006

Lubos said: *It is not possible to find nothing at 1 TeV, not even Higgs*

Care to place a wager on this, Dr. Motl?

20. **Thomas Larsson**
June 9, 2006

Gordon Kane: The LHC could discover the superpartners in a supersymmetric world.

For many years, I was cautiously positive about string theory. It always bugged me that it apparently was in complete disagreement with experiments, but OTOH the math was cool and obviously relevant for statphys, which was the kind of physics I cared most about. An article by Gordon Kane in Physics Today in 1997, entitled "String theory is not only testable, but super-testable", changed all that. It made it clear to me that string theory is not only wrong but evil, because it has the power to transform senior scientists into babbling crackpots, as is manifest already from the title; IIRC the body of the article was far worse.

But in retrospect Kane's article appears as quite sane compared to the landscape stuff.

21. **John Stanton**
June 9, 2006

I read in an online physics book the prediction:
LHC will discover that the Higgs particle is composite,
not elementary, as there cannot be any elementary
spin 0 particles.

22. **Peter Woit**
June 9, 2006

John,

That's definitely one possibility. But composites of what? Bound together how? And will the composite nature only show up at much higher energies than those accessible at the LHC? There has been a lot of work on some models of this kind (e.g. Technicolor).

23. **Aaron Bergman**
June 9, 2006

You statement really is amusing and ironic, Thomas. The thing is, Gordy Kane has never been a string theory partisan. In fact, he's always been a *supersymmetry* partisan which is something different.

As with the above comment Vilenkin, not all that ails the world can be laid at the feet of the horrible string theorists, it seems.

24. **Anonymous**
June 9, 2006

Aaron, Gordy has been talking for a while about how to use LHC data to help pin down the underlying string theory construction.

25. **Anonymous**
June 9, 2006

dear Lubos,

ideally you are right. But in practice LHC is a dirty hadronic machine: it is not clear if LHC allows to test if WW WW scattering exceeds unitarity. What would an experimentalist really see, in that case?

26. **Peter Woit**
June 9, 2006

Aaron,

Kane has written quite a lot promoting string theory. See for instance

http://feynman.physics.lsa.umich.edu/kane/Zing.htm

He explains there his relation to string theory:

"I am often asked what is the connection between string theory and supersymmetry. String theory is like a musical score. Supersymmetry is like the performance. If you can read the score and hear it in your head you don't need the performance, but most of us do. Or, string theory is the recipe, supersymmetry is the cake."

27. **woit**
June 9, 2006

Several people,

Please, stop posting here advertisements for your unorthodox ideas about physics on the grounds that they are "LHC predictions". If you want to discuss the topics covered in this posting fine, if you want to promote yourself and your own ideas that have nothing to do with this posting, do it elsewhere.

28. **Aaron Bergman**
June 9, 2006

I'm not saying that Kane is anti-string theory. He obviously isn't. He's not a string theorist, though; he's a phenomenologist. When I last saw him (a few years ago at TASI), I got the impression that string theory was interesting in as much as it told us something about supersymmetric model building. (Modulo unlikely things like weak scale gravity, of course.) Along those lines, hep-th/0509157 or hep-ph/0403040.

29. **Alejandro Rivero**
June 9, 2006

Giving that the Yukawa coupling of the top is 0.998 or so, a composite model explaining this phenomena is inviting, but I do not know any. A second question about composites is if it can be really claimed that they avoid the hierarchy problem (at least, it will depend on where do you put the compositeness scale).

30. **David**
June 9, 2006

To John Stanton

John, people say that fundamental scalars cannot exist because in a 4 dimensional renormalizable field theory, the coupling constant would flow to zero under the renormalization as the cutoff is taken to infinity. A similar thing will happen in dimensional regularization as the dimension 4 limit is taken.

Hence the only possible 4 dimensional scalar field theory would be a trivial noninteracting or free one if the the cutoff is infinite. If one considers the possibility that the cutoff is finite, the one can have scalars, but these are not considered fundamental as there must be something beyond the cutoff of the scalar field theory. Peter is right that the interesting thing is what, but unfortunately, that cannot be found unless one makes measurements at the scale of the cutoff or beyond.

31. **Thomas Larsson**
June 9, 2006

Aaron, the article is too old to be available on-line, but the content of the February 1997 issue of Physics Today is available here:

"Superstring Theory Is Testable, Even Supertestable
Many believe that superstring theory, because of its extraordinarily tiny length scale and gargantuan energy scale, cannot be tested. That belief is a myth — Gordon Kane"

If you think my comment is so funny, maybe you could tell us how to supertest string theory.

32. **Aaron Bergman**
June 9, 2006

I read the article. It's available if the library has a subscription.

33. **MoveOnOrStayBehind**
June 10, 2006

"Superstring Theory Is Testable, Even Supertestable
Many believe that superstring theory, because of its extraordinarily tiny length scale and gargantuan energy scale, cannot be tested. That belief is a myth — Gordon Kane"

From what has been cited here and in the blog text it seems evident that it is more the string physicists who have a reasonable opinion (because they know what they talk about), rather than the beyond-the-standard-model extra-dimensional phenomenologists, isn't it? It's them who keep the standards high.

BTW, any idea what the people who actually run the experiment and do all the work think about the outcome? I would think these should be asked in the first place. But probably they are all idiots and busy with pulling cables etc.

34. **John Stanton**
June 10, 2006

A question to the experts: are we completely, 100%, sure
that, in the case that the Higgs is composite,
the components and forces keeping the parts
togther must be new? In other words,
are we completely sure that this is not the strong
interaction, for example?

35. **Thomas Larsson**
June 10, 2006

I read the article. It's available if the library has a subscription.

Means what? That just because you have read a particular piece of propaganda, it is ridiculous to call that piece of propaganda by its right name?

Besides, you didn't answer how string theory could be supertested. Or, since scientific theories only can be falsified, maybe I should ask for a doable experiment which would superfalsify string theory.

36. **Alejandro Rivero**
June 10, 2006

David, John: It should be stressed that the "triviality limit" is shown in most plots of new physics, specially the one from Chris Hillman that appears from time to time in the Review of the PDG, sometimes showing both the "triviality" and the "unitarity" bounds, as well as vacuum stability. In fact this plot is the main argument to claim we will find something at LHC energies.

37. **Aaron Bergman**
June 10, 2006

You're missing the point, Thomas. I'm not defending the article. I'm just amused that it was an article by a phenomenologist who focusses on supersymmetry that turned you off on string theory.

38. **Tommaso Dorigo**
June 10, 2006

Hi all… Nice collection of comments I must say!

FWIW, I agree with Witten, Rivero, and (of course!) Peter Woit.

I am in CMS, but I think CMS and Atlas will see very little. They will do precision physics (t), unfortunately following the tradition of LEP (Z) and LEP2 (W). They will only find the Higgs, but only if they are lucky – not because I think there is none there, quite the opposite! Because CDF and D0 have a real chance to see it first!!

Why do I think that LHC won't find any SUSY ? Because I don't buy it. But also because CDF and D0 should have found something weird in their dataset by now!

In Run I, CDF saw a extremely unlikely event with two electrons, two high-Et photons, and significant missing energy (a neutrino, most likely – or two). I must confess that the eeggmet event was on the verge of changing my attitude toward SUSY, but then… With 10 times the statistics, CDF and D0 haven't seen anything weird anymore; extensive searches (with more acceptance to anything weird) for parents of that event have seen nothing! So one is left with the feeling that the eeggmet event was a fluke anyways. In that case, a SM fluke is, by force of Ockham's razor, to be preferred…

39. **Fred Diether**
June 10, 2006

What about the HyperCP events that claim they might be seeing a SUSY sgoldstino?

Does anyone know if anyone is going to try to recreate the experiment?

Fred

40. **Thomas Larsson**
June 11, 2006

Aaron, I'm sorry that I misinterpreted you.

41. **Kasper Olsen**
June 11, 2006

I think the best prediction is that we will learn more about the "notorious" question of the Higgs (or Higgses); none of us know precisely in what way.

In that sense Sean Carroll is right: the beauty of science is that this is unknown terrain.

And a terrain in a vast and unknown Landscape? Who knows? 😉

I find it wrong – and *not even wrong* – trying to do a cost-benefit analysis here, or to say that time will be waisted with the LHC.

Any other ideas? Trying to find answers by debunking string theory, or modern quantum field theory? Or trying to find a "background independent" answer?

42. **Michael Schmitt**
June 11, 2006

Witten is certainly right that we will learn a lot about EWSB at the LHC. The betting odds are that a SM-like Higgs boson will be found, though this won't by iteself provide clear evidence for low-energy supersymmetry. After that, one's tastes in exotic physics determines what one expects/hopes to find. At that level there is no point in making a general statement about weak gravity or extra dimensions or SUSY or string theory (not to mention multiverses) – only the experiments will tell us what is there. Making pronouncements about what the LHC will find is worse than predicting who will win the world cup! If, as Bee predicts, there is no new physics and no Higgs boson (or its surrogate), then we cable tuggers will be very frustrated indeed, but the community will certainly have learned something big: the Standard Model and most models of physics beyond the Standard Model will essentially be ruled out, and all the enthusiasm over the last few decades about the Higgs mechanism will have been misplaced.

I certainly agree that SEED should have polled more experimenters, especially a couple out of the thousands who actually work on LHC experiments. How strange it would be to publish an article on the national economy based on comments from, say, applied mathematicians. Or even stranger, to write an article on the future of theoretical physics after talking to a dozen experimenters. I guess the lesson is that the star power resides in the theoretical community, so it is good for SEED circulation to tap there…

43. **Alejandro Rivero**
June 11, 2006

robert Says: *Papal infallibility would be a cheap shot. Maybe he doesn't have an obvious agenda, other than his work/research ethic?*

Hmm I'd better tell that Witten's agenda, whatever it is, happens to coincide now with the real physicist agenda. In previous occasions Witten agenda seemed to be the use of his influence on mainstream research to promote interesting mathematical topics, as noncommutativity or K-theory, to name a couple ones. In most cases, the result has not been very happy, with a flood of string-oriented papers obscuring the mathematical and physical meaning of the concepts -mostly because they drank only from Witten's papers-.

44. **tsg**
June 13, 2006

If the LHC doesn't find superpartners, isn't that in itself significant, as evidence against supersymmetry? (Or at least as evidence against SUSY at energy scales where it could be an answer to the hierarchy problem?)

Blogs, Blogs, Blogs

June 9, 2006
Categories: Uncategorized

All sorts of news very recently on the science blogging front:

Seed's ScienceBlogs site has been revamped, with 25 new blogs for a total of 43 science bloggers. First it was Cosmic Variance with 5 bloggers, now it is ScienceBlogs with 43. How can a single artisanal blog maker like myself compete with these massive blogging conglomerates? Still not very many physicists over at Seed. Besides Chad Orzel, one of the new ones is astrophysicist Steinn Sigurosson who has been running a blog called The Dynamics of Cats.

One physics blog I ran across recently, one that isn't moving to Seed, is Angry Physics.

Besides Seed, Nature magazine has started up a site called NatureBlogs. They are running blogs on chemistry, genetics, and neuroscience, as well as a more general one on web technology and science. There's also a newsblog for comments on news stories appearing in Nature, as well as a discussion blog related to a radical new concept in peer review that they are trying out: a blog where certain papers submitted to Nature are posted, asking for commentary on the paper, to be considered as part of the peer review process.

Nature also just launched another new project which I'd been hearing about for a while from my brother, called Nature Network Boston. It's intended as a networking site for scientists in the Boston area, and has news, event listings, groups, and, guess what, more blogs. Maybe Lubos can start a string theory fanatic's group there…

Finally, Jacques Distler is helping to provide access to these proliferating blogs with a new aggregation site he calls Planet Musings. As usual, Jacques is careful to make sure that anything he has control over censors links to people who disagree with him…

Comments

1. **anon**
   June 9, 2006

   I think if we keep making so much progress in the blogging front, we will soon arrive at the idea of having separate groups for users with different, special interests. Perhaps we could call them newsgroups or usergroups or some such

2. **Yidun**
   June 9, 2006

   Prof. Woit,

   Couldn't you just join them? 🙂

3. **Dan**
   June 9, 2006

   You are the right person to complain about censorship…

   http://www.math.columbia.edu/~woit/wordpress/?p=401#comment-11688

4. **knotted string**
   June 10, 2006

   The thing about group-blogs is that people can get censored by the group for being off-topic (even if no readers complain), so there is group-think coercion involved, moderating the posts:

   http://cosmicvariance.com/2006/05/29/gone/

5. **John Sidles**
   June 10, 2006

   The startling thing about the blog list is, no engineering blogs. To leading order they do not exist (in English). Why is this?

6. **Alejandro Rivero**
   June 10, 2006

   Of the last parragraph, I'd prefer to underline helping instead *censors*. If we are near the bang point for physics blogs, all these catalogs and links will be useful while people builds their own menuses.

7. **woit**
   June 10, 2006

   Dan/Michael,

   Yes, on my blog I do delete stupid nasty personal attacks on other people by, especially those by cowardly anonymous string theorists who keep changing even their pseudonyms. You're right, that's exactly the same as having the arXiv censor links to my blog.

8. **island**
   June 10, 2006

   Hey!… you forgot mine… Science In Crisis

   The anthropic principle is continually thrust to the surface of the relevant fields of physics and evolutionary science, yet scientists dogmatically ignore the relevant implication for "biocentric preference"… in spite of the fact that it is highly probable that a true anthropic constraint on the forces of the universe will necessarily include the human evolutionary process, which indicates that there exists a mechanism that enables the universe to "leap".

Next time that someone makes fun of, or pretends like they have the first real clue as to what the anthropic prinicple is really about, send my way and I'll deprogram the dogmatic dingbats for ya… 😉

9. **hack**
   June 10, 2006

   Peter Woit, Lubos Motl, Jacques Distler and Lee Smolin. That would be an amusing group blog.

10. **zoologist**
    June 10, 2006

    We have a saying that goes something like "great is the zoo of God" (perhaps there is something like that in English too), so I would seriously like to support the idea in the previous comment and also suggest further members from the "great zoo of God" to make up the Funniest Physics Group Blog Ever:

    Peter Woit, Lubos Motl, Jacques Distler, Lee Smolin, Bert Schroer, Carlo Rovelli

    and that would be the ultimate entertainment around 7-8 PM just before going home from work.

    Dear would-be members please take up contact with each other and consider the potential of this idea. Greater readership, more media-coverage, more fame, more jokes and last but not least: more fun (for us)!

11. **Dan**
    June 10, 2006

    Peter,

    it wasn't a stupid nasty personal attack you deleted. I pointed out to Bert Schroer that he was apparently misunderstanding a physics argument I posted. He replied saying "OK, 1:0 for you". Deleting such things is *very* different from what you claim to be doing.

12. **woit**
    June 10, 2006

    Dan/Michael/Whoever you are,

    The great majority of your comments here contain some sort of stupid insult about the intelligence or ignorance of anyone you disagree with, a tactic that string theorists like you, Lubos and Jacques seem fond of. At least Lubos puts his real name to them. No physics argument by you expressed in the language of reasonably civil conversation was ever deleted. If you have anything reasonably interesting to say, it won't be censored here. If you insist on going on about how stupid and ignorant people are who disagree with you, I'll continue to delete such things, whether or not you're right on whatever substance may be at issue.

13. **Dan**
    June 10, 2006

    Peter,

    is it appropriate by your standards to point out the difference between Seiberg duality and Seiberg-Witten's electric-magnetic duality? That's what I did "in the language of reasonably civil conversation", as evidenced by the fact that Bert Schroer admitted to his mistake without being offended.

    You see, I don't care much about the deletion of some online conversation by itself. But given the number of complaints you have voiced about what you consider unfair censorship of your statements, it is a remarkable example of how careless and agenda driven your own censorship is.

14. **woit**
    June 10, 2006

    Dan/Michael/Whoever you are,

    From what I recall, you were unable to clarify the distinction between the two dualities without doing so in an insulting manner. I have no agenda whatsover about the distinction between Seiberg duality and Seiberg-Witten duality or whether anyone in particular is aware of it or understands it. The "censorship" in this case was not careless and the only agenda driving it was stopping uncivil behavior on my blog.

    Your claims that you were engaging in "reasonably civil conversation" are hard to believe given the one of your comments I left in edited form:

    "Sure, Peter Woit. [Attack on someone else deleted]. You are a pathetic loser. "

    As people can guess, the deleted part was a personal attack on Bert Schroer, of the same kind as ones in your earlier comments that led me to delete them.

    Look, you have a long history here of anonymously posting extremely rude and insulting comments about me and about others. You're well aware of this. It would be a good reason for me to just automatically delete anything that comes in from you. It seems to me that I've gone far out of my way to allow you to express yourself, given your behavior. End of story, I'm not going to waste more of my time dealing with someone who behaves the way you do.

15. **George**
    June 10, 2006

    Hang in there, you are the only one actually putting physics on your page on a consistent basis. Sean used to do so, but has really cut back – too bad – I enjoyed the two of you. But I would hate for all the help in understanding what's happening to disappear completely…

16. **MathPhys**
    June 10, 2006

    Re George's remark,

    This is the one blog with the highest concentration of mathematical physics that I know of.
    When I look at Distler's blog, it seems to be devoted to elaborate discussions of how to type math on the web (What happened, Jacques? We are meant to do science, not to be web programmers).
    Motl's blog has a lot of physics, but also its fair share of political rants.

17. **Anon**
June 11, 2006

The “blog with the highest concentration of mathematical physics” ?

Urs Scheiber’s blog (the String Coffee Table).

Hands down.

18. **Troublemaker**
June 11, 2006

Urs Scheiber’s blog (the String Coffee Table).

Hands down.

This categorization requires a leap of faith that string theory is relevant to physics.

Snicker, snicker.

19. **Anon**
June 11, 2006

Well, since most of the posts on this blog are devoted to string theory (and how it’s not even science), the same criticism applies.

20. **Theo**
June 12, 2006

See, Troublemaker, some of us care only that physics might be relevant to string theory 🙂

Then again, some of us live in math departments, and find it fascinating that folks would complain that something as beautiful as omega-categories might turn out useful for a physics, whether ours or not.

I smiled most at your comment about String Coffee Table’s “categorization”.

Not Even Wrong in the Sunday Times

June 11, 2006
Categories: Not Even Wrong: The Book

Today's edition of the London Sunday Times has a review of Not Even Wrong by John Cornwell in its book review section. Cornwell is a British historian of science, based at Cambridge University.

The review is very positive and pretty much gets things right, so of course I'm quite pleased by it. It does get one thing wrong, or at least expresses it in a misleading way: David Gross is listed as an ally, which is certainly not the case as far as criticism of string theory goes (although we both agree about the string theory anthropic landscape). A more minor quibble would be with his description of the significance to Pauli of the phrase "Not Even Wrong" (it wasn't purely a term of abuse, but also refers to the untestability of a theory). But on the whole I think Cornwell does a very good job of describing the more controversial parts of the book and what its concerns and arguments are about.

Lubos already by last night had posted his trademark ad hominem attack on the reviewer. By now, his ranting response to any one who publicly criticizes string theory or agrees with me on this topic is tediously familiar, involving launching a personal attack on their professional qualifications, then comparing them to dogs, assigning extremely low numerical values to their intelligence, etc., etc.

Cornwell expresses the opinion that

Now that Woit has thrown a wild cat among the theoreticians, we can be sure that the ruffled string-theory advocates will be preparing a rebuttal.

So far the only rebuttal to be seen is that from Lubos, who tells us that most string theorists agree with him, writing that:

Cornwell predicts that string theorists will be preparing a rebuttal to the dean of the crackpots. I am afraid that with exactly one exception, they have much more serious work to do than to talk to cranks. My simple statement that the dean of all crackpots much like John Cornwell could not become graduate students of physics today because they are unable to understand some very elementary questions about science will probably remain the only reaction.

and

Most string theorists much like most high-energy physicists in general are extremely nice people – too nice people – so they won't say that Cornwell is a breathtaking moron in the public. But be sure that they agree with me and many of them are saying these things in between the physicists. In the public, the only question is how to explain that Cornwell is a complete idiot without making anyone upset.

Lubos claims that most of his string theory colleagues believe that I'm a crank and my arguments about the problems with string theory are not worth responding to. This may or may not be true, but even if it is, I find it hard to understand why they allow him to go on in this way, claiming that he represents their viewpoint, given the immense amount of damage he is doing to the public perception of their field. If you believe Lubos, some of his senior colleagues seem to even think it is a good idea to egg him on in what he is doing. He reports that one senior physicist sent him last weekend's Financial Times piece, describing it as

a tendentious, malicious attack on scientists and through that on science itself

and that another "very famous physicist with more than 10,000 citations" told him:

WOW. I can't believe the FT article. Holy Shit, the world has gone completely bananas.

Update: In case anyone is following the comment section over at Lubos's blog about this, note that his policy there is to delete any comments from anyone he disagrees with. I wrote in a comment answering an attack on me from "LambChopofGod" which was swiftly deleted, and others have had similar experiences. Did make me sit back and think for a moment: what am I doing spending my Sunday evening responding to nasty personal attacks from some fanatical kid hiding behind the pseudonym "LambChopofGod"? This is getting very, very weird…

Comments

1. **george**
   June 11, 2006

   Lubos is just a crazed, socially inept, but brilliant scientist. Anyone who reads Lubos's comments can figure this out, and turn on the appropriate mental filter.

   No, unfortunately, it is you Peter who is doing the greatest amount of damage to the public perception of string theory.

2. **knotted string**
   June 11, 2006

   Every single journalist and every single publication that prints the truth about the lack of science in string theory will be called 'science-hater par excellence' or 'crank/crackpot'. Intimidation strategy:

   1. 'Discredit' people by simply calling them names.
   2. Feign anger: claim objective criticism 'hurts science', etc.
   3. Ignore/suppress alternatives, then claim none exist.
   4. Claim without any evidence that critics have low IQs.
   5. Claim 'string theory' theorists can criticise themselves.
   6. Claim without evidence that all critics are ignorant, etc.
   7. Fabricate/exaggerate false/trivial/straw-man technical 'errors' in criticisms and try to use these to dismiss the entire criticism.
   8. Claim without any evidence that their work is science by definition because they have had a 20-year honeymoon in the media, and then claim (again without evidence) that by criticising it you are objecting to science, rather than encouraging science (objectivity).

   In fairness, there are two types of 'science': dogmatic defense of abject speculation, and objective investigation.

3. **David**
   June 11, 2006

   In general, I agree with knotted string with two exceptions.

1. Not all string theorists behave this way. For example, Zwiebach, in Chap.1 (p.8) of his book, says there is no experimental verification of string theory because of the lack of a sharp prediction.If he chooses to spend his time looking for one that's OK with me. It would be nice to hear something from him about some of the wilder claims by some others(e.g. God used string theory to write the universe).
2. Consider knotted strings last sentance. I know he/she wants to be fair but I think the only type of science is objective investigation as best we can do it.

4. **wolfgang**
June 11, 2006

David,

let me complete your quote:
"String theory is still at an early stage of development, and it is not so easy to make predictions with a theory that is not well understood. Still, some interesting possibilities have emerged."

He then goes on to explain what type of experiments could provide support for string theory.

If you have a copy you can read this from p.8 to p.10.

5. **ks**
June 11, 2006

I do think that most of Lubos behaviour is already socially accepted among scientists for quite some time. And he will likely know this. What can originally be attributed to him is the inflationary use of the term "crackpot" to all colleagues / journalists who fundamentally disagree with his views and of course the puberal rudeness of his insults. But qualifying people as "crackpots" or "cranks" or making any other layman diagonsis of mental disorder is not his invention and it is not specific to string theorists and their excommunication rites.

6. **anonymous**
June 11, 2006

Seriously, what's the deal with Mr. Motl? I've never met him, but after repeatedly encountering his seemingly inescapable web-presence, I can't help but imagine him as television's Tucker Carlson, typing away furiously under a pseudonym.

7. **CapitalistImperialistPig**
June 11, 2006

Peter,

Congratulations! Your book is clearly getting a lot of good press and driving the string theorists "bananas."

Why is its publication so much later in the US?

Anonymous,

Professor Motl is quite real, and an assistant professor in the most imprtant university in the US (and world), Harvard.

Whatever else we may think about him, and I do think (and write)about him frequently, he is definitely an original.

8. **woit**
June 11, 2006

CIP,

Thanks!

The reason it\'s being published at different dates in the UK and the US is just that the publishing business is kind of funny, with different publishing houses operating in the two different countries. So you end up having to have two different publishers. My book was originally bought by a UK company (Jonathan Cape), which got interested in it after hearing about the book from Roger Penrose, one of their authors. The deal I made with Cape involved selling them world rights to the book, and they then went looking for a US company to publish here, finally settling on Basic Books. The whole process ends up taking quite a while, and Cape ended up publishing before Basic. This isn\'t particularly unusual, Lisa Randall\'s recent book was also first published in the UK.

9. **anon**
June 11, 2006

very famous physicist with more than 10,000 citations" told him:

"WOW. I can't believe the FT article. Holy Shit, the world has gone completely bananas."

A coward, letting Lubos do the dirty business. It's cowards and dirty politicians like that who decide which physics phds get jobs in the field and which don't.

10. **George P**
June 11, 2006

Being a "brilliant" scientist isn't enough. Hitler had plenty of those. One needs stewardship and some "street smarts" about what to study, why, and when. One needs to guard against pursuing blind alleys or going down a destructive path. Sometimes you gotta use outside feedback to do that.

Lubos' only response to feedback is childish ad hominem attacks. Also disturbing is his devotion to nutty right-wing causes. He repeatedly demonstrates that he tragically lacks this "meta-intelligence" and that his EQ is so low that it cancels out his ability to put his "book smarts" to any good, worthy use. A pretty smart person with stewardship and discipline is more effective than a whiny "boy genius" brat like Lubos.

11. **MathPhys**
June 11, 2006

Lubos is not a genius. He's smart, but so are many other people.

It’s been about 4 years or more since his PhD and, in spite of the fact that he’s always been right in the middle of the US theoretical physics establishment, his achievements are minor, compared to many others at the same stage of their careers.

Sorry, Lubos.

12. **MathPhys**
June 11, 2006

anon,
I think I see what you mean. Someone gets irritated by something anti stringy in a newspaper. He finds it below him to react to it directly, or maybe just doesn’t have the time. He brings it to Motl’s attention, knowing that the pitbull will bite and never let go.

Yes, Lubos, I’m afraid you’re being used.

13. **Arun**
June 11, 2006

If Peter Woit was wrong, it would be very easy to refute him, maybe even in one sentence, e.g., like “the XYZ experiment makes Woit’s arguments irrelevant”.

John Q. Public may not know much science, but would be able to spot the absence of such a simple argument from the string theorists. Furthermore, he will see the message from Harvard – we cannot discuss physics with morons such as you. He will see “Cornwell explains that the colliders are a waste of money” but read “Even the proposed prodigiously expensive class of accelerators known as Superconducting Super Colliders (SSCs), he claims, would have failed to provide the merest clue as to whether the theory had merit.” – which is only about the uselessness of SSC for string theory. In any case, if the Harvard attack gets as much circulation as the Cornwell review, it will leave little room for doubt on the part of the public.

String theorists might have better luck with the argument that Peter Woit is not entirely right. 🙂

14. **David**
June 12, 2006

Peter,
I admire your patience. It is considerably more than I have.
Best,
David

15. **David**
June 12, 2006

Peter,
I learned a lot tonight. The final portions of my discussion with Motl were deleted which leaves the reader, following Motl’s misquotes, with the impression that I said that you said Polchinski’s book had an error. I never said that. I said I thought there was an error and you told me how Polchinski got his result. That’s all you said. Please leave this up so somewhere it’s clearly stated. I hope you saw the end before it was deleted. I have new thoughts about Motl as a scientist.
Best,
David

16. **John Stanton**
June 12, 2006

A friend of mine told me once that internet
and other ad hominem attacks as a reaction
to an idea are a complicated way to say:

“You are possibly right, but your idea goes so
much against my deeply held convictions that I can
only express this by insulting you.”

Keeping this in mind, one can be more relaxed about
the issue. I know people who use this to check their
ideas: if they get counter-arguments, they think
about them; if they get insulted, they know they are
on the right track.

17. **knotted string**
June 12, 2006

David,

If the definition of ‘science’ is to be kept objective, then we should do the same with ‘theory’ and not associate that with strings. The key strength of ‘string theory’ is incompleteness and vagueness. As ‘t Hooft says, ‘string theory’ is like being given a chair minus arms, legs, back etc. It is just an idea, a hunch.

So calling strings a ‘theory’ elevates it to a status it doesn’t really have. And then calling vacuous ‘string theory’ actual ‘science’ to defend it from critics is just absurd.

I either have to keep denying string theory is science, or I have to invent a new type of science – metaphysics – in which to place string. Interesting summary of a relevant subject:

http://en.wikipedia.org/wiki/Groupthink

‘Groupthink is a mode of thought whereby individuals intentionally conform to what they perceive to be the consensus of the group. …

‘The term was coined in 1952 by William H. Whyte in Fortune[1]:

‘”Groupthink … is a rationalized conformity — an open, articulate philosophy which holds that group values are not only expedient but right and good as well.” [2]

‘Irving Janis, who did extensive work on the subject, defined it as:

'"A mode of thinking that people engage in when they are deeply involved in a cohesive in-group, when the members' strivings for unanimity override their motivation to realistically appraise alternative courses of action." [3]

'The word groupthink was intended to be reminiscent of Newspeak words such as "doublethink" and "duckspeak", from George Orwell's Nineteen Eighty-Four.

'Irving Janis originally studied how groupthink affected the Pearl Harbor bombing, the Vietnam War, and the Bay of Pigs Invasion…

'Causes and symptoms of groupthink…

'Illusion of invulnerability
'Unquestioned belief in the inherent morality of the group
'Collective rationalization of group's decisions
'Shared stereotypes of outgroup, particularly opponents
'Self-censorship; members withhold criticisms
'Illusion of unanimity (see false consensus effect)
'Direct pressure on dissenters to conform
'Self-appointed "mindguards" protect the group from negative information

'… symptoms of a decision affected by groupthink:

'Incomplete survey of alternatives
'Incomplete survey of objectives
'Failure to examine risks of preferred choice
'Failure to re-appraise initially rejected alternatives
'Poor information search
'Selective bias in processing information at hand (see also confirmation bias)
'Failure to work out contingency plans

'Social psychologist Clark McCauley's three conditions under which groupthink occurs:

'Directive leadership

'Homogeneity of members' social background and ideology
'Insulation of the group from outside sources of information and analysis …

'One mechanism which management consultants recommend to avoid groupthink is to place responsibility and authority for a decision in the hands of a single person who makes the decision in private and can turn to others for advice. Others advise that a preselected individual take the role of disagreeing with any suggestion presented, thereby making other individuals more likely to present their own ideas and point out flaws in others' and reducing the stigma associated with being the first to take negative stances (see Devil's Advocate).'

18. **island**
June 12, 2006

I've had comments deleted by Peter and I've never questioned his reasoning. Lumo is a completely different story, as he deletes stuff because he doesn't like the message, but not because it isn't relevant and/or valid.

The most frustrating counterargument is the kind that you get when the people that you're talking to simply go silent because they can't refute your physics, but still won't buy it, so their stategy becomes that of sitting back and waiting for a refutation to come along rather than to admit that you have a point.

Fifty hard facts gets you nothing but air.

Dogma at it's finest.

19. **hack**
June 12, 2006

Lubos is not a boy genius, more like an ageing wunderkind. He had some early success, but now he's 32 or 33 and there are many who have achieved more than he has at that age. I've read that it can be very depressing for a former wunderkind to come to the realization that he is no longer the youngest guy in the room so to speak. I can imagine how he might cope with this in bizarre ways, such as cultivating an adolescent internet persona. But acting like a teenager when you're in your 30's doesn't make you young, it makes you pathetic.

20. **anonymous**
June 12, 2006

[…]

I don't think its a good development that what should be a scientific controversy is discussed in public on such a mediocre level. It is sad, it is very sad, that the physicists community is apparently not able to solve the problem on its own. This tells a lot about what is going wrong in physics research.

How many people who read the book, or the article, can judge on the actual issue? What is created is the impression that theoretical physicist don't know what they are doing, or why they are doing what they are doing. Instead they call each other crackpots, morons, or debunkers, and blame each other to be on the wrong track.

[…]
If I were to decide whether tax money goes into theoretical physics on base of that, I'd say: let's wait till they have sorted out their problems before we invest money.

Do we want that?

Best, B.

I don't.

21. **anonymous**
June 12, 2006

sorry, from Lubo's blog

http://motls.blogspot.com/2006/06/dean-of-crackpots-hitlers-pope-and.html

22. **xpinor**
June 12, 2006

Is the reviewer John Cornwell identical with the professor at St. Andrews and author of the three-volume book on group theory?

23. **amused**
June 13, 2006

This will no doubt test the limits of Peter's tolerance but I'ld like to abuse the hospitality of this blog to respond to Lubos' latest post on his blog, "Science vs Democracy". Lubos usually deletes my comments there, and since he seems to spend almost as much time here as on his own blog, responding to him here seems the only option. His post was in response to comments by someone called "B" on his earlier post attacking the Sunday Times review of Peter's book; so, with more than a little good will, the following is arguably not completely off-topic here.

Lubos, you are being disingenious as usual. Obviously in a string-dominated environment you and your colleagues are going to be all in favour of "leaving the selection of the good ideas to the usual free mechanisms in science", just as the bosses of Microsoft wish for a free market free of those annoying anti-monopoly laws that governments try to harrass them with.

The relevant question is whether string theory deserves its current level of dominance in theoretical hep (at least the non-phenomenological side). As far as I can tell, you guys have two main arguments for this: First is the claim that David Gross likes to make, that the situation in string theory now is analogous to the one prior to the formulation of QM in the early 1900's, where people were groping for the right understanding and formulation of the new physics. The implication being that "Hey guys, major new physics is on the verge of being discovered, everyone should be working on this!". Second is the retort "If we're not to do string theory, then what else? Show us a more promising alternative."

Actually I have some sympathy with both of these arguments. But not much. On the one hand it's certainly a big deal to have found a not obviously inconsistent theory of quantum gravity which has a possibility to unify gravity with the other forces. On the other hand, despite 30 years of effort by some of the best brains in physics, the results so far are spectacularly unimpressive: you have nothing more than hints that string theory (or some parts of it) may have something to do with nature. Contrast this with Bohr's result – in his atom model – where he was able to derive the Ryberg constant in terms of other basic constants of nature. This is the kind of result you guys need to come up with before the claim that the string theory situation is analogous to pre-QM can be taken seriously. You are nowhere near to obtaining such a result, and there is no sign that this is going to happen anytime in the forseeable future.

Now let's consider the "show us a better alternative" argument. To that my reply is: When a completely "top-down" approach like string theory gets bogged down, it might be a good idea to permit some work to be done on "bottom-up" approaches. I emphasise *permit*, because at present you do not permit it – young people who choose formal theory topics which are non-string are seriously harming their career prospects. String theorists and higher-dimensional types are mighty proud of themselves when they make applications of brane stuff to low energy QCD, or lattice chiral gauge theories, but woe betide any young fool who is naive enough to work directly on these topics. They are only considered of interest to the extent that string theory can say something about them, and it is only the results that come from string theory that are considered to have value.

An example of a formal theory topic which has seen major progress over the last decade, and where there is much of interest that remains to be done, but which would be career suicide for a young person to work on, is lattice formulation of chiral gauge theories. (I choose this example since it happens to be dear to my heart; no doubt there are other equally good ones.) A nonperturbative formulation of chiral gauge theories would be quite a nice thing, don't you agree Lubos? It is at any rate a prerequisite for being able to properly investigate the nonperturbative phenomenon of spontaneous electroweak gauge symmetry breaking. And who knows, maybe results from this "bottom-up" topic could later inspire new insights in "top-down" string theory. But with things the way they are now we will never know, because, thanks to string dominance, such topics are not a viable option for anyone who hopes for a career in physics.

24. **Peter Woit**
June 13, 2006

amused,

I pretty much agree with you, and it's remarkable that Lubos now feels that the only way he can handle those with a different point of view about string theory is to delete their comments. Perhaps I should set up a "Lubos's deleted comments thread" here.

"B" is Sabine Hossenfelder, her blog is backreaction.blogspot.com, and you might want to try and carry on this discussion over there with her.

25. **amused**
June 13, 2006

Ok, thanks for the info Peter, and for allowing the comment to stay.

"Perhaps I should set up a "Lubos's deleted comments thread" here."

Lol. Yes, please do!

26. **Aaron Bergman**
June 13, 2006

I'm somewhat confused by your comments, amused. I'm pretty sure I'm not imagining all the lattice gauge theorists in the world. Are you saying that the job prospects for a lattice gauge theorist are worse than that for a string theorist. I have no idea if that's true or not, but I'm just trying to see if I'm interpreting you correctly.

27. **Ponderer of Things**
June 13, 2006

Quote from earlier: "Lubos is not a genius. He's smart, but so are many other people. It's been about 4 years or more since his PhD and, in spite of the fact that he's always been right in the middle of the US theoretical physics establishment, his achievements are minor, compared to many others at the same stage of their careers."

I was going to defend Lubos, as I was under the impression that he was publishing a lot lately. But when I went to ISI, there are only 10 published refereed papers for Lubos, only one in the past 3 years and none in the past 2 years. Well, I have to say I am surprised. And only 2 out of 10 papers as first author?

I personally know plenty of grad students with more impressive publication record than that.

Sorry, Lubos, but arxiv is no substitute for refereed publication process…

28. **Aaron Bergman**
June 13, 2006

Just a factual correction: in string theory, authors are in alphabetical order.

29. **amused**
June 13, 2006

Aaron, regarding the job situation for lattice gauge theorists you need to distinguish between people doing "bread and butter" lattice QCD research – i.e. numerical simulations, algorithms, & phenomenological chiral perturbation theory stuff needed to extract physical results from the simulations – and those (few) people doing formal stuff like construction of chiral gauge theories. For the former the job situation is not too bad, at least at postdoc level. It's tough at the faculty level, but that's also the case for many other fields. I'm not sure exactly how it compares to the job situation for string theorists; certainly there seem to be a lot more of them on the jobs rumour pages, but that could to some extent just reflect that there are more stringers than lattice QCD'ers. On the other hand, for people doing formal lattice gauge theory stuff the situation is pretty hopeless. Almost all traditional lattice gauge theory groups focus on the bread and butter stuff and are simply not interested in hiring people who only want to do formal things. The natural home for the formal types would be in a regular formal particle theory group. But "most" of these groups happen to be focusing on string theory, branes etc and are not interested in people from other fields…

30. **Peter Woit**
June 13, 2006

xpinor,

I don't think it is that John Cornwell.

31. **Anon-e-mus**
June 13, 2006

"L*b*s's deleted comments thread" — me, too! I was even permanently banned from commenting on his blog after repeatedly pointing out that he had misread what various people whom he accused of saying various nasty or stupid things actually wrote. Of course anybody able to "read, mark and inwardly digest" English prose can still see that to be the case even without my comments pointing it out…

32. **Ponderer of Things**
June 14, 2006

Thanks for correction on string publication order – now I recall that it is indeed the rule.

Still, no published papers over the past two years – according to arxiv this doesn't even qualify Lubos as an "active researcher".

33. **Peter Woit**
June 14, 2006

Ponderer,

All the evidence so far is that the arXiv official definition of "active researcher" is "not Peter Woit". Attempts to get them to set a well-defined standard based on number of publications foundered when it became clear that the arXiv moderator, Jacques Distler, wouldn't meet the standard (which was just number of papers, not counting whether they were published). As far as I can tell, neither he nor Lubos would meet suggested numerical standards based on published papers.

34. **Lionell Griffith**
June 14, 2006

All this hoha could be avoided if reality were determined by equations. One could simply write the equation and *poof* the feature would exist in reality. All problems could be solved by scribbles on a scrap of paper.

Unfortunately, the equations must conform to reality. Until that conformance is demonstrated, the equations are nothing but bad fantasy and science fiction. More importantly, they are Not Even Wrong.

I suggest most of the string hoha is about getting grants and writing papers. The last thing they want is a real solution to real problems. If that happened, they might have to get a real job and actually earn their keep. We can't have that. Can we? We common folk have to pay the bills so the "superior" types can play forever without having to produce anything having meaning or value.

35. **Aaron Bergman**
June 14, 2006

I suggest most of the string hoha is about getting grants and writing papers. The last thing they want is a real solution to real problems. If that happened, they might have to get a real job and actually earn their keep. We can't have that. Can we? We common folk have to pay the bills so the "superior" types can play forever without having to produce anything having meaning or value.

So, Peter, do you think this is true?

36. **CapitalistImperialistPig**
June 14, 2006

Aaron,

I, at least, think it's implausible that string theorists don't want "real solutions to real problems."

At the same time, you can't neglect Upton Sinclairs principle that "it's amazing how hard it can be for someone to understand something when his salary depends on his not understanding it."

String theorists all have very high IQs and the ability to work very hard, so unless they are otherwise psychologically disabled, they should mostly be able to make a lot more money doing something else.

37. **woit**
June 14, 2006

Aaron,

No I don't think it's true (by the way, I disagree with many if not most of the comments posted here). I was about to delete it, but didn't partly because I was busy, and partly to show what a lot of everyday people's reactions to the string theory mess is likely to be. I've written elsewhere about the unfortunate backlash against any ambitious work on the fundamentals of theoretical physics that is starting and likely to increase due to the way string theory has been oversold.

38. **Aaron Bergman**
June 14, 2006

Thank you for that. I asked because I figured people would ignore me if I said that it wasn't true, but people might listen to you. Whatever one thinks of string theory as a research programme, I hope things don't degenerate too much into arbitrary attacks against string theorists.

39. **Ponderer of Things**
June 14, 2006

Peter, I find it somewhat ironic that arxiv, with it's supposed aim to "free" the scientific community from delays or restrictions of paper-based publications and sometimes lengthy refereeing process, has been recently busy creating rules for rejecting unpopular among certain circles (but scientifically valid) ideas, blocking trackbacks etc.

I am especially surprised since I was able to post and to quickly become an endorser without any endorsing, but then again, my research is not controversial.

I happen to think that arxiv should be totally "free for all" – if crackpot start posting junk, so be it – let people figure out what is what. As if regular publications are free of junk – surely enough if you go down low enough on the foodchain, you can publish just about anything.

As a product of former soviet block system, I also find somewhat ironic that Lubos, who claims to be self-proclaimed anti-commie right-winger, uses very much the same tactics commonly employed by communists (scientifically or politically – be it Lysenko-Michurinism or Sakharov) to limit critisism – such as simply deleting undesirable comments, and responding to reason with demagogery, namecalling and personal attacks. You'd think he lshould have learned a thing or two about character assasination vs. respectful and civil free exchange of ideas.

40. **Who**
September 10, 2006

CapitalistImperialistPig mentioned:

…you can't neglect Upton Sinclairs principle that "it's amazing how hard it can be for someone to understand something when his salary depends on his not understanding it."

String theorists all have very high IQs and the ability to work very hard, so unless they are otherwise psychologically disabled, they should mostly be able to make a lot more money doing something else.

reminds me that part of the payment must be in real or fancied prestige.
the importance of importance was one of the themes in NEW recent thread about Open Access Publication. (the prestige conferred by publication in top journals balances simple dollar and time considerations)

besides pure interest and excitement, besides money rewards, people work for recognition and status. so one thing that open criticism of the string monopoly in the US might eventually do is **change the prestige payoffs** a bit. don't know if this has much or any significance compared with the plain bread and butter issues of postdoc contracts and junior faculty hires—-just a thought

More in the Times

June 12, 2006
Categories: Not Even Wrong: The Book

In Monday's London Times there's a [piece]() by Anjana Ahuja about my new book and the controversy over string theory. Ahuja worries that I might have multiple spleens, so I want to reassure her and others that I only have one, generally of normal size. I suppose that at times when dealing with Lubos, it may get a bit enlarged.

Ahuja gives a reasonably good account of some of the heated debate going on about string theory, but does get one thing wrong about my point of view on this, when she sets me up as someone who argues that "aesthetics is no substitute for experiment" in contrast to Brian Greene's emphasis on "The Elegant Universe". This is an issue where I'm with Brian, unlike, say, Lenny Susskind. I firmly believe that this is an elegant universe and that the pursuit of more mathematically elegant theories is our best hope for moving forward. I just don't happen to think that the kind of string-theory based unification ideas that people have been pursuing are especially mathematically elegant.

Ahuja also remarks on the fact that Brian and I work in the same department but I don't thank him in the acknowledgements. There's no big mystery about that, I just haven't had any really substantive conversations with Brian about the topics of the book, so didn't explicitly thank him there. Brian is a very nice guy who I first met when he was a graduate student. He came to Columbia about ten years ago, hired by the math department with a joint appointment in physics. During the past few years, unfortunately for the math department, Brian's interests have shifted from mathematics more towards physics and he spends most of his time over in the physics department, where he has started up a successful new research institute called ISCAP.

I've often helped him with computer problems, and last time I saw him a week or so ago in the hallway, he congratulated me on the book, which he had just gotten a copy of from the publisher, and said he was looking forward to reading it. I warned him he might not like some parts of it at all, he said that was fine, controversy and debate was good, or something like that. Basically, he and I disagree about a scientific question: can one make a successful unified theory out of string/M-theory? I think there are good reasons to think one can't, he's still optimistic that it might work out.

Brian is far from the only string theorist I know who I have this disagreement with. Some are very good friends that I've debated with extensively about this, others, like Brian, I don't happen to have spent much time discussing the topic with. But disagreements over whether some speculative idea can ever work are not unusual in science, and most scientists have no problem with healthy debate of this kind. I've found it extremely surprising and disturbing that a small number of string theorists have chosen to engage in personal attacks rather than the usual sort of scientific debate.

Comments

1. **Carl**
   June 12, 2006

   > I've found it extremely surprising and disturbing
   > that a small number of string theorists have chosen
   > to engage in personal attacks rather than the usual
   > sort of scientific debate.

   It's one thing to expect to find beauty and truth in your theories about nature, but to expect to find universal politeness in an entire community is asking more than just a bit too much of human nature.

   Carl

2. **Trevor**
   June 12, 2006

   I found this blog from the article in today's Times. As a mere periodontist who just takes an interest in physics, I was assured by a young friend who happens to be a professor (meaning the top level of UK academic post) of physics at a leading UK university that quarks, leptons and force-carrying bosons are about the smallest things for which there is good experimental evidence. I read Brian Greene's "Fabric of the Cosmos" last year in Arizona, and his "Elegant Universe" a few years before. These books follow in the great mould of Banesh Hoffman's "Strange Story of the Quantum" (which I read in the 1960s) in explaining fairly clearly to non-physicists a bit about recent thinking. I'm hoping Peter Woit's book is as much fun when I get it in a few days. I find his blog very interesting, but do not understand why he has been insulted by other people. Why should anyone insult someone else over anything scientific, particularly something for which evidence appears unlikely to be obtainable? There is an old (Chinese?) proverb which says that the first person to hit the other is thereby admitting he has lost the argument. I'm afraid I tend to discount the views of people who stoop to insults.

3. **Michael Schmitt**
   June 12, 2006

   "…the pursuit of more mathematically elegant theories is our best hope for moving forward…"

   Perhaps you meant this in a narrow sense? My opinion is that particle physics suffers from a lack of revealing data – all that we know is in accord with the Standard Model, so we have no hint from HEP in which direction to go, theoretically speaking. Of course there are other signs of new physics beyond the SM, such as dark matter and baryogenesis, but theorists address these problems with models – the problems do not seem big enough to tell us which theoretical directions are right. Some theoretical work seems to me so far removed from experiment as to be practically untestable, in which case there is no valid guide for progress, including mathematical elegance. Would anyone say that QED is mathematically elegant? Yet by some measures it is the most valid theory ever invented…

4. **knotted string**
   June 12, 2006

   I don't like the fact that the review quotes you apparently dismissing all roads: 'Researchers need, he says, "to acknowledge that this particular speculative idea doesn't really work and there aren't any obvious good ideas out there".'

   But you describe loop quantum gravity (pages 189, 255) and describe other speculative ideas towards such as twistors and the ideas of Alain Connes. You argue for using representation theory to understand the diffeomorphism groups of general relativity.

   Another issue I have is whether string theory is really already falsified? In the book you do argue that supersymmetry in string theory is inconsistent with data for force strengths on pages 177 and for the cosmological constant on page 179. So not only is string theory 'not even wrong' regarding the question of making predictions (landscape of solutions, etc.), it's also just plain wrong when considered as an ad hoc a model for existing observations.

5. **Juan R.**
June 12, 2006

Trevor said,

> I find his blog very interesting, but do not understand why he has been insulted by other people. Why should anyone insult someone else over anything scientific, particularly something for which evidence appears unlikely to be obtainable? There is an old (Chinese?) proverb which says that the first person to hit the other is thereby admitting he has lost the argument.

The Chinese proverb holds in this case. When persons have not arguments substitute them by insults. That is the case of several of string theorists here.

String theory has failed. The only technical argument (from string theorists) could change our current perception is showing us how real problems are solved in string theory approach, that predictions will be done at next HLC series of experiments, how string theory is compatible with Standard Model and General Relativity…

The problem with string theorist cannot do nothing of that even after 40 years of unlikely attempts and unending promises.

Juan R.

Center for CANONICAL |SCIENCE)

6. **woit**
June 12, 2006

Michael,

When I argue that the search for mathematical elegance is our best hope for progress, that's in the context of assuming experimentalists can't get us anything new. I certainly hope that is not true, that the LHC or even the Tevatron will soon radically shake things up with unexpected new data. But if that doesn't happen and we still want to work on speculative new ideas about high energy, mathematical elegance is one of the few criteria we have for figuring out which way to go.

And I do think QED is in many ways a mathematically elegant structure, and QCD is even more so.

7. **Thomas Larsson**
June 12, 2006

You argue for using representation theory to understand the diffeomorphism groups of general relativity.

Knotted string,
I certainly cannot speak for Peter, but you just mentioned the reason why I generalized the Virasoro algebra to several dimensions. 🙂

8. **HH**
June 12, 2006

Hehe, it made it to digg.com:

http://digg.com/science/String_Theory_Debunked_

9. **Chris Oakley**
June 12, 2006

> And I do think QED is in many ways a mathematically elegant structure, and QCD is even more so.

You're welcome to both of them.

Incidentally, the article in *The Times* makes much of Peter's spleen and the "online squabbling" that goes on here, but – apart from the Bohemian Brat – does anyone really behave that badly here? I don't think so. In fact, I see the contributors to this blog as continuing an ancient tradition of gentleman scientists that goes back to Lord Rayleigh. "Lively discussion" is the term I would use rather than "squabbling".

10. **hoggy**
June 12, 2006

Digg = ass of the internet news sites.

11. **David Younger**
June 12, 2006

I too read the article today. I cannot read about string theory without thinking about Prolemy's system of wheels and epicycles. For many years after Copernicus described a sun centred system, Ptolemy continued to prove a better predictor of the movements of the stars and planets. But it was wrong, and all the elegance of the maths behind it couldn't alter that.

12. **Benjamin**
June 12, 2006

Hi Peter. My question may seem off-topic, but I think it's relevant to your site, since it involves the whole issue of what makes good science. I do agree with certain unnamed critics of yours that the climatology behind the global warming hysteria is shaky and questionable. Somehow I would expect a skeptic like you to agree, for reasons somewhat similar to how you view string theory. Any comment? I'm not asking you to deny the global warming hype altogether, just to say whether you think it is full of uncertainties, even if a majority of climatologists think otherwise. Even if you haven't studied it in detail, what does your common sense say about using a computer model to predict something as complex and nonlinear as the weather? Not to mention the uncertainties in the input data. You may take the safe path and say that you simply don't know, but I'll be shocked if you say that you have a lot of confidence in the global warming theory!

13. **Peter Woit**
June 12, 2006

Benjamin,

Please, no climatology debates here. I'll delete any more on this topic.

Despite what some people think, I really don't go on here about things I haven't spent a lot of time learning about and thinking about. Life is short, and climate science is not something I've spent any time at all thinking about.

One of my few data points is Lubos, who seems to be almost infallible at falling for heavily ideological nonsense. Based on what he has to say, my prejudice would be that there must be something to global warming. But then again, on rare occasions he gets it right (e.g. the string theory anthropic landscape), so I can't just rely on his opinions.

So, I stick to my position: I don't know and don't have time to find out for myself.

14. **John Gonsowski**
June 12, 2006

Funny, Susskind actually created the most math elegant M-theory (bosonic M-theory) but unfortuneately he didn't do anything with it and instead went down a really inelegant path. Bosonic M-theory has no superpartners but that could end up being a good thing.

15. **Anonymous**
June 12, 2006

Right now, "Not Even Wrong" has an amazon.co.uk sales rank of 71, meaning that there are 70 books out there which are selling better than it at the moment. That sounds pretty good, even if this is just the early buyers all buying at the same time.

16. **Michael Bacon**
June 12, 2006

Peter,

I think getting the book pubished is gong to be a good thing for you — and the ideas you expouse. Your response to Benjamin seems to strike the right balance — bemused tolerance. Exactly the right attitude to take regarding Lubos. He could very well be right regarding any number of matters, but who knows, given the crazy way he responds to things. Anyway, congratulations on the book and the good reviews, and on your abilitiy to raise fundamental questions that deserve serious answers. I enjoy the blog a lot.

17. **woit**
June 12, 2006

Thanks Michael and others,

I've been surprised at how well the book seems to be doing, and extremely pleased by the positive reaction to it. At one point I was advised that it was just too difficult a book for most people, but the publishers ended up allowing me to write exactly the book I wanted, and it is gratifying to see that people are enjoying it, and not getting put off by some of its more technical aspects.

18. **Thomas Reasoner**
June 12, 2006

Benjamin, the various theories of Global Warming are testable and thus falsifiable, whereas String Theory is not. I see no correlation between the two situations whatsoever.

Now to stay on topic: congratulations on the book! I hadn't planned on buying it, but I think I just might in order to support the cause.

Regarding Brian Greene, I have read a couple of his books, and I've even seen his Nova series, and I must say I was not at all impressed with any of it. Maybe the problem was that I wasn't really in the target demographics. The other problem, I think, is that his works seemed to me to be lacking any real insight. There were no great moments of epiphany for me reading his stuff. Ironically, his "Elegant Universe" really turned me off from String Theory, even though it was supposed to turn the reader on to it.

19. **D. Eppstein**
June 12, 2006

David Younger: You don't think Ptolemy's epicycles were an important forerunner of Fourier analysis? That seems a much more important role than their "not even wrong" description of planetary motion. In fact the ability of epicycle type theories to fit any cyclic motion has a lot to do with how useful Fourier analysis is…

20. **top ten?**
June 13, 2006

from comments on Lubos Motl blog:

Lubos, can you please post the 10 top results relevant for physics achieved by string theory? This would be a more reasonable way of clarifying the present controversy. Since I am worried that you will not like this comment and will delete it, I am going to post it also on Peter Woit blog.

21. **JPL**
June 13, 2006

"And I do think QED is in many ways a mathematically elegant structure, and QCD is even more so."

Peter,

Do I take it correctly that you don't hold the same opinion about Electroweak Theory (GSW Model etc…) ?

22. **island**
June 13, 2006

Peter… don't forget us when you're famous…

er…

Peter won't be able to forget us when he's famous… try as he might… heh

23. **Peter Woit**
June 13, 2006

JPL,
Well, QED is part of the electroweak model, and, ignoring the Higgs mechanism, the rest of the GSW model is a gauge theory just like QCD.

I do think the electroweak model is mathematically elegant, but it's a more complicated situation than QCD. There's the Higgs, which is problematic for various reasons, and certainly makes the whole thing more complicated and less elegant. Also, the electroweak model really forces you to think about chiral gauge theories, and these are in many ways mathematically even more interesting, but much less well understood.

24. **Tommaso Dorigo**
June 13, 2006

Hi all,

despite being grateful for Michael S.'s recent flattering comments about my posts, I need to join the debate here, and throw in my two pence – QED is surely elegant and I understand those who say so. QCD is probably also elegant but its elegance is beyond my reach.
What I really find the most elegant of all mathematical structures involved in our current understanding of the subatomic physical world is group theory. In a sense, without the elegance of group theory, much of the charm of QED and QCD would be gone.

Just a dumb experimentalists' HO…

Cheers,
T.

25. **Tommaso Dorigo**
June 13, 2006

… I missed a part in my preamble above: despite… I need to disagree with Michael.
T.

26. **Anonymous**
June 13, 2006

How surprised would everybody be if no Higgs (or any other new particle) is found at the LHC? I understand that there are good reasons to expect to find the Higgs, but weren't there good reasons to have found it at lower energies, too?

27. **MathPhys**
June 13, 2006

Does anyone know of way to put money on outcomes of the LHC? I want to invest money betting that

1. No Higgs scalar will be seen,
2. No susy partners will be seen

I need the money to buy a new bike, and that's a sure bet.

28. **Aaron Bergman**
June 13, 2006

How surprised would everybody be if no Higgs (or any other new particle) is found at the LHC? I understand that there are good reasons to expect to find the Higgs, but weren't there good reasons to have found it at lower energies, too?

Supersymmetry was the good reason to have seen it at lower energues. There is a *really good* reason to see something that generates electroweak symmetry breaking, however: WW scattering needs something to maintain unitarity. If unitarity is violated, that's a big deal.

The worry, however, is that there might be something but it could be almost impossible to distinguish it from the background of the rest of the standard model. I don't know much about such models, but I hear it could be a problem.

29. **Amitabha**
June 14, 2006

> How surprised would everybody be if no Higgs (or any other new particle) is found at the LHC?

I will be a bit disappointed if the Higgs is found.

> There is a really good reason to see something that generates electroweak symmetry breaking, however: WW scattering needs something to maintain unitarity.

A 3-point coupling W-W-something in addition to pure Yang-Mills seems necessary to maintain tree-level unitarity. In the Higgs model that something is the Higgs field which causes the electroweak symmetry breaking. But you can think of other models with a AAB coupling, in which the new field may not be the cause of electrweak symmetry breaking.

30. **top ten?**
June 14, 2006

about my message above: on his blog, Lubos and others answered my question about: which are the main results relevant for physics achieved by string theory? Some of the answers are worth reading, provided that one knows that it is like reading the Pravda in Breznevian era: Lubos suffered another of his insult crises and started deleting my posts, allowing only the comments that agree with his agenda.

I do not repost here my deleted answers, as they are off-topic.

31. **runge_kutta**

June 14, 2006

I particularly love Lubos' comparison of your knowledge of physics to that of 'the average squirrel'. That made my day. My sides still hurt from laughing.

32. **Eugene Stefanovich**
June 14, 2006

MathPhys:

I also want to bet some money on the absence of Higgs and SUSY stuff at the LHC. I think this would be a very refreshing experience for fundamental physics. I hope this will force us to abandon the modern "bag of tricks" approach to QFT. We will go back to dusty relativity and QM textbooks and examine them under microscope searching for inconsistencies. And I bet we'll find a few.

33. **D R Lunsford**
June 14, 2006

MathPhys,

See this: http://arxiv.org/abs/physics/0006049

-drl

34. **knotted string**
June 14, 2006

Lubos now states on his blog: 'I always like to say that the status of string theory and the status of the theory of evolution … are somewhat analogous: I summarize the reasons at the end of this text. … a group of believers felt that this opinion of mine insulted their religious sensibilities. So they virtually marched to Jacques Distler's office and forced him to officially denounce my analogy, much like the believers who demanded the denunciation and execution of the heliocentric heretics 500 years ago. … Jacques Distler has fully obeyed their requests.'

The failure of crackpots like Lubos (and I'm glad to exclude Jacques in this case), is caused by the fact Darwin had a BOOK FULL of evidence for evolution, while crackpots have NO evidence at all.

I can see how Lubos went crackpot: he is comparing the lack of MATHEMATICAL LAWS in Darwin's book with the corresponding lack of mathematical laws in string theory.

But physicist Michael Faraday formulated all his laws in words like Darwin, and Maxwell later translated them into maths. Darwin's laws can be represented mathematically, too, in models like the Lanchester equations for the groups (or species) to struggle to survive in conflict.

You can see why Lubos prefers to compare strings to Darwin than to compare them to Faraday's researches. It would be just absurd.

Strings are crackpot as they have no predictive equations AND no experimental evidence. There is simply nothing at all useful in stringy theory, not even the prediction of a unique vacuum state.

35. **JPL**
June 14, 2006

Woit says:

I do think the electroweak model is mathematically elegant, but it's a more complicated situation than QCD. There's the Higgs, which is problematic for various reasons, and certainly makes the whole thing more complicated and less elegant. Also, the electroweak model really forces you to think about chiral gauge theories, and these are in many ways mathematically even more interesting, but much less well understood.

Agreed. I asked because I truly think that the dichotomy between mathematical aesthetics and empircal success that journalist insist in making is a false one! As it turns out the aspects of the EW Model that are mathematically more elegant (i.e. the Gauge Principle) are the ones that have experimental support (Weak Neutral Currents, Ws,Zs etc…).
More than a few people recognize the Higgs pattern of SSB as something of a phenomenological kluge. Of course the few proposed alternatives did not fare much better, but that does make the Higgs any prettier…

36. **Jonathan Vos Post**
June 15, 2006

Will you keep us posted on Hawking's forthcoming Superstring lecture in Beijing?

Humans close to finding answers to origin of universe: Hawking Thu Jun 15, 8:47 AM ET
http://news.yahoo.com/s/afp/20060615/wl_uk_afp/sciencehongkongbritainphysics_060615124715

Acclaimed physicist Stephen Hawking has said that humanity is finally getting close to understanding the origin of the universe.

Speaking at a lecture in Hong Kong, Hawking said that despite some theoretical advances in the past years, there are still mysteries as to how the universe began.

"Despite having had some great successes, not everything is solved." …

During his Hong Kong visit he also revealed he is writing a children's book with his daughter about theoretical physics.

Hawking is the author of international best seller "A Brief History of Time", which attempted to explain a range of subjects in cosmology, including the Big Bang, black holes, light cones and superstring theory.

He is on a six-day visit to Hong Kong and will meet Chief Executive Donald Tsang Friday before heading to Beijing Saturday where he will give a lecture on string theory.

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37. **Chris W.**

June 15, 2006

In recent decades it seems that certain kinds of mathematical structure, and a mathematician's notion of beauty, have been widely regarded as at best essentially synonymous with beauty in a physical theory, and at worst a very good surrogate for it. I think we are now witnessing the pitfalls of that facile notion.

There was a time when "beauty" in a physical theory or idea was understood as having a meaning beyond formal or mathematical elegance; indeed, it was recognizable prior to the formal elaboration of an idea. In my opinion this is abundantly clear in Einstein's early work, and was integral to his particular form of physical intuition. This sense, if not altogether lost, has been largely displaced in the thinking of the last two generations of theoretical physicists.

Various and Sundry

June 14, 2006
Categories: Uncategorized

As usual recently, so much going on that I don't have time to write much about a lot of it, but here are some quick links and comments.

The SUSY 06 conference is taking place this week, hosted by UC Irvine, with talks at the Marriott Hotel in Newport Beach. Here's the program, which now has links to slides for some of the talks in the parallel sessions, although not at the moment for those in the plenary sessions. This evening there will be a plenary session on Naturalness, with talks by experimentalist Burton Richter, theorists Frank Wilczek, Leonard Susskind and Andrei Linde. This line-up is very heavily weighted toward the anthropic point of view, I wonder why the organizers couldn't find anyone from the other side. Various bloggers are at the conference reporting, including Clifford Johnson and Sabine Hossenfelder. B. Yen, who normally covers off-road motor-racing, has decided to cover something even more exciting, academics at a SUSY conference, and is providing stills, video, and podcasts via iTunes from the conference site.

Strings 2006, this year's edition of the big yearly string theory conference held each summer begins next week in Beijing. The conference web-site itself still doesn't yet have even a schedule of talks or titles of talks, but Jonathan Shock will be at least one person blogging from there, and he has begun with a long posting of advice about Beijing for people traveling there for the conference.

The big political news of a few days ago was the blogger convention in Las Vegas, with potential 2008 presidential candidates showing up to try and impress the most influential people in the country at the moment, bloggers. Sean Carroll reports from the science-blogging caucus there that Wesley Clark made his pitch by coming out strongly in favor of Leonard Susskind and the anthropic string theory landscape. I know, this sounds like a weird joke, but it's not.

In other political news closer to home here, today's New York Times has a story about some of Einstein's off-prints being auctioned by Christie's, for the benefit of New York's progressive Working Families Party.

Dave Bacon has a posting about a new arXiv front-end from the IOP called Eprintweb.

The Tevatron is back in business colliding particles, having overcome the attack of the killer raccoons. There's a report from Gordon Watts who explains the importance of plastic ducks for his experiment's data acquisition system.

John Baez's latest This Week's Finds is out, this time it's mostly a very enlightening discussion of the relation of music theory and group theory. His web site also contains some wonderful notes by Michael Shulman from a minicourse John gave on n-categories and cohomology theory. John's web-site increases it resemblance to a modern blog with an RSS feed set up by Serkan Cabi.

The math blogosphere seems to my mind somewhat weirdly dominated by those with an interest in category theory. Besides John and Urs Schreiber, there's David Corfield, Robin Houston, and the only math blog at ScienceBlogs, that of Mark Chu-Carroll.

Urs has interesting reports from the ESI Research Conference on Homological Mirror Symmetry going on this week and next.

Last week was the annual Johns Hopkins workshop on particle physics, this year held at the Galileo Galilei Institute in Florence. Many of the talks are on-line.

Le Monde has an article about Dubna.

There's definitely an increasingly widespread backlash against string theory going on in the wider culture. A Columbia colleague last night sent me an extract from a book his daughter was reading. It's called 100 Bullshit Jobs … and how to get them by Stanley Bing, and one of the "Bullshit Jobs" listed is that of "Quantum Physicist String Theorist". Skills required for the job are listed as

Bullshit at such a high level of discourse, with such a profound understanding of arcane mathematical concepts, that everybody thinks they are stupider than you.

The listing describes

… string theorists, who have now broken up into two warring camps, each fighting for control of PBS. One school says that there are many, many universes, possibly an infinite number. The other school is more conservative and counts just a couple of cosmic alternatives, and has the benefit of being represented by a total babe.

I also heard recently from the people who put out Axes and Alleys, the official magazine of the Royal Tractor Repair and Maintenance Society of Outer Mongolia. Their latest issue has a graphic on page 27 inspired by their impression of superstring theory.

Finally, as near as I can tell Lubos has finally gone completely bonkers. In his last few ranting postings, people who disagree with him no longer have the intelligence of dogs, but are compared to squirrels (or, in my case, microbes). His latest posting is about why the scientific status of string theory and of evolution theory are the same (although he thinks "evolution is more dogmatic while string theory is more open-minded work in progress"), and I'm sure the people at the Discovery Institute will enjoy it greatly. He goes on about the fact that at one point, under great duress, Jacques Distler did admit in the comment section of a blog that he disagreed with Lubos on this point. Lubos compares this to Judas's betrayal of Jesus:

This almost sounds like a story from the New Testament except that in the past, there would be 1 Judas in such a story. Today we have 387 Judases with various confused and triply corrupt self-interests and relations to the bad players in the game of life.

Instead of dissociating themselves from Lubos's increasingly nutty postings about string theory, some string theorists such as Moshe Roszali and Joe Polchinski instead have decided this is a good time to encourage him and start participating in the comment section of his blog. Polchinski contributed to a top ten list of greatest achievements of string theory produced by Lubos two more: the "fact" that the unknown theory is somehow known to have no parameters, and the existence of the landscape and thus the anthropic solution of the CC problem.

The fact that Polchinski seems to think Lubos's blog is a good place for him to spend his time is kind of funny given that he has publicly attacked me for saying unpleasant things about people (I did once describe a paper of his in an uncalled-for way), as well as privately telling people he won't read my blog because of the nasty personal things I say about people. For some reason he seems to have no problem with Lubos, to the point of being willing to encourage him as he gets more and more delusional. This is really sad.

Update: Some of the plenary talks at SUSY 06 are now on-line. The schedule of talks with titles for Strings 2006 is now on-line.

Comments

1. **hack**
   June 14, 2006

   I heard on the public radio program "From the Top", a young musician and aspiring physicist describe string theory as "kind of the joke of the physics world".

2. **sequoia**
   June 14, 2006

   I guess Polchinski and Roszali don't take Lubos' bad behavior as seriously as you or others do. He might be very incendiary on the web but he's actually a nice, charming person in reality, has a lot of good friends and is liked and respected by many. He's of course not as brash in person as he is online so that explains a lot. He has two alternate personas and his web persona is something of an alter ego to his more well-behaved persona offline.
   Plus, in this day and age when there's a lot of backlash against string theory and its advocates, Lubos is the one person ranting and raving the loudest, and because of this, he enjoys quite a lot of support from the silent majority of string theorists. And just as most democrats are reluctant to criticize michael moore, string theorists are hesitant to speak out against lubos. He's their mouthpiece. He's their self-appointed warrior, the one who passionately defends this field they've devoted their lives to. He says everything they wish they could say but can't because they're scared of the backlash, or they just can't take the heat.

3. **John Baez**
   June 14, 2006

   Peter writes:

   John's web-site increases it resemblance to a modern blog with an RSS feed set up by Serkan Cabi.

   Now I just need to get a quarrelling crowd of readers who reply to everything I write by venting their own pet peeves. 🙂

4. **Chris Oakley**
   June 14, 2006

   Sequoia,

   You should read the articles in *The Times, The Sunday Times and The Financial Times* in the UK about Peter's book. Executive summary: Peter has a point. The dominance of String Theory is in no way commensurate with its scientific achievements.
   It may well be that LM is perfectly nice on a one-to-one basis, but he has no judgement. Comparing Darwinian theory with String theory is just stupid. And this man was a boy genius? Well, so was Stephen Wolfram and if he wants to tell me, in 1,200 pages, that all the problems of science can be solved by Cellular Automata then, even though he may be brighter than I am, I have to then say that I know at least one thing that he does not. At least, unlike LM, he provides me with usable software. If just being clever was what it is all about then I – like the vast majority of others – probably cannot compete. But being clever is *not* what physics is about. It is about *building consistent models of the physical world*. This is a particular kind of craftsmanship that has nothing to do with whether people like you or not (cf. Wolfgang Pauli), or whether you are a good person (cf. Werner Heisenberg), and it is not perfectly correlated to your academic record (cf. Albert Einstein). What actually matters is what matters to the craftsman, namely the *quality of the end product*. This is where judgement comes in. String Theorists do not behave like scientists. They behave more like children blowing bubbles, watching and comparing notes on the pretty rainbow flecks that their mathematical games produce. They hope that it might at some stage connect with reality, but this does not seem to be a priority. However, when criticisms are made – of the kind that they should have made of themselves – they start to squeal like children threatened with the removal of their toys. I ask you, why *should* we continue to write them a blank cheque?

5. **hack**
   June 14, 2006

   " He says everything they wish they could say but can't because they're scared of the backlash, or they just can't take the heat."

   If that is the case then they are an incredible bunch of cowards.

6. **Eugene Stefanovich**
   June 14, 2006

   Chris:

   I don't think that string theorists are more guilty than anybody else. Simply string theory occupied such a prominent place in science during last 30 years and developed a huge publicity machine. That's why it is an easy target. I think that the problem with modern physics is much deeper. In part it can be attributed to (rather miraculous) successes of the last 50+ years, when such ill-defined concepts as renormalization, gauge invariance, spontaneous symmetry breaking, asymptotic freedom, etc. actually produced real physical results. It became commonly accepted that we can continue on the same path: invent a trick or two that will eventually bring us a "theory of everything". It seems to me that we reached the limit of such trick-based approaches.

   In my opinion, instead of rushing to TOE, we should carefully reexamine our foundations – the relativity and quantum mechanics. However, as was pointed out a few times on this blog, these lines of research are not considered fashionable or sexy in academia. Indeed, how one can move forward by looking backward? Nevertheless, that's exactly what we need to do. Though this change of attitude would require a major disaster, like the null LHC result.

7. **knotted string**
   June 14, 2006

   Lubos is now linking mention of your name on his blog to a somewhat warped version of this blog: http://sites.gizoogle.com/showpage.php?url=http://www.math.columbia.edu/%7Ewoit/wordpress/

   Warping is what you can expect from a string theorist.

8. **JC**
   June 14, 2006

   I've always wondered whether somebody's aggressive behavior online and/or in person, is a compensation of some sort for shortcomings in other parts of their personal lives. (ie. The Napoleon Syndrome or some kind of inferiority complex http://en.wikipedia.org/wiki/Napoleon_complex)

9. **Aaron Bergman**
   June 14, 2006

   They hope that it might at some stage connect with reality, but this does not seem to be a priority.

This is belied by the focus of many groups on string theory model building. Many people are out there looking for confirmatory possibilities.

In fact, although I doubt you'll like it, for a lot of people this anthropic thing is an attempt to connect to the real world. You may disagree with the philosophy, but that is a separate issue from the motivation.

Most string theorists aren't bad people. Really.

10. **a**
June 14, 2006

Peter, your website is becoming all-Lubos, all the time. You're talking about him several times in every article.

If you were both third graders, I'd suspect you two have a crush on each other.

11. **Walt**
June 14, 2006

Peter: Category theory always seems disproportionately well-represented on the web to me. What's even odder is that it's not just a fascination with categories, but with n-categories. I think part of it is that John Baez has always been such an effective advocate of his n-categorical point of view that he's both attracted people to the subject and inspired them to follow his example and post about it on-line.

None of us at arsmath are big category theory fans, so we'll just have to single-handedly restore the balance. 🙂

12. **hogghy**
June 14, 2006

who's the total babe?

13. **Who**
June 15, 2006

The other school is more conservative and counts just a couple of cosmic alternatives, and has the benefit of being represented by a total babe.

who's the total babe?

Lisa Randall and Eva Silverstein are both lovely young women—accomplished speakers and brilliant to boot. I'd guess that the author of "100 Bullshit Jobs" was referring to Lisa Randall. But the reference is inaccurate—she does braneworld models, rather than string/M theory proper. Babe or not, I don't understand how she could be said to "represent" any group or school of string theorists.

14. **AR**
June 15, 2006

Peter: Burt Richter is as outspoken a critic of the anthropic principle as you could possibly ask for. At the session, he essentially described the anthropic principle as theology. And he got audience applause for it, too.

hogghy: The babe is probably Lisa Randall.

15. **reader**
June 15, 2006

"Moshe Roszali and Joe Polchinski instead have decided this is a good time to encourage him and start participating in the comment section of his blog."
"This is really sad."

NO, this is appropriate. Most of the string theorists don't even mention your name because you have firmly established yourself as a maniac.

16. **David Corfield**
June 15, 2006

Walt: *What's even odder is that it's not just a fascination with categories, but with n-categories.*

Well, once you've see that ladder heading all the way up to the heavens, it seems somewhat unambitious to stop on the first rung.

17. **better ideas?**
June 15, 2006

the serious question about Lubos is: is he following a strategy and we are unable of seeing his goal, or we are having fun of a person with personality problems?

Sorry, a.

18. **Ponderer of Things**
June 15, 2006

Lubos as Michael Moore? More fitting is Ann Coulter, or Bill O'Reilly. Some of it may be calculated to get attention, a big part of it is narcissism. intolerance towards people who have audacity to form a different opinion and simply lack of tact/manners.

Saying that he is perfectly nice person except for his internet persona is like saying that your neighbour is a nice guy, except for occasional killing sprees.

I really do wonder what other faculty members at Harvard think of him – I will ask around. Since a lot of his psychotic rantings are now part of permenent record on the web, anyone with access to google can find them, even years later. I only imagine that while most faculty members don't give a flying fart about string controversy, they don't want to be sharing commitee duties with someone who doesn't handle opposing opinion well and is likely to fly off the handle, spewing personal attacks and put-downs every time he gets a little "upset".

Some people really ought to pospone getting a blog until they are fully tenured – at least. Most of us learn to control our emotions, instead of going off and letting everyone know how we REALLY feel.

Blogs seem to be opening up a channel to peek beyond that "filter" in some people. And what we can all see is often not very pretty.

Note to self – think twice about getting my own blog.

19. **anonymous**
June 15, 2006

Maybe Polchinski's and Roszali's support has more to do with Motl's political opinions than his persona as a strong theory spokesperson. I recall Motl invective against those who called Summer's statement of "anti-semitism in intent if not in effect" as ridiculous. Perhaps this endeared Motl to them. Sort of like how Motl would perhaps appear as an ally to those against saving our environment. Though its hard to imagine even those people sinking as low as these two…

20. **Jeremy**
June 15, 2006

I missed a math babe? Where?

Anyway, after finding the reviews online and finding this place, I had no idea that it would be so controversial to say, y'know, prove it.

Since I'm in the U.S. I haven't been able to read the book yet, I couldn't actually compare it to whoever this Lubos person is. All of that writing seems needlessly caustic (unless there's some caustic language in the book I don't know about).

21. **amused**
June 15, 2006

I don't think much can or should be read into the appearance of Moshe and Polchinski on Lubos' blog; the discussion there just happened to turn to something that interested them. Polchinski also posted his comment on Christine's blog, and Moshe seems like a reasonable enough guy who just wants to have pure physics discussions without all the sociological stuff etc (which is easy enough for him since he doesn't have to deal with the consequences of it). I'm sure Moshe would also join in here if there was a pure physics discussion on a topic that interested him; he has done so regularly in the past at any rate.

As for Lubos and his 387 Judases, that's quite bizarre indeed! Presumably Distler is supposed to be one of them, but then who are the other 386?

22. **Peter Woit**
June 15, 2006

AR,

Thanks for the report. Any insight into why the organizers couldn't find a theorist skeptical of the anthropic stuff and had to bring in an experimentalist to do the job?

23. **Peter Woit**
June 15, 2006

amused,

Unclear who the other 386 Judases betraying Lubos/Jesus are. In his comment section, he does thank Clifford Johnson for refusing to publicly renounce the evolution/string theory analogy, so Clifford isn't one of them.

Note that Roszali and Polchinski's comments were purely about their own suggested additions to Lubos's list of the great achievements of string theory. In context I wouldn't describe this as pure physics discussion without the sociological stuff.

anonymous,

I strongly doubt that Roszali or Polchinski's support of Lubos has anything to do with his political views.

better ideas?,

"the serious question about Lubos is: is he following a strategy and we are unable of seeing his goal, or we are having fun of a person with personality problems?"

He's following a strategy, one which is obvious: paint anyone who criticizes string theory as a crackpot who should not be paid attention to. Sympathy with this strategy is probably why string theorists almost universally refuse to criticize him. His problem is that he does have personality problems, which makes his efforts self-defeating.

24. **amused**
June 15, 2006

"Note that Roszali and Polchinski's comments were purely about their own suggested additions to Lubos's list of the great achievements of string theory. In context I wouldn't describe this as pure physics discussion without the sociological stuff."

Neither would I, but from their perspective it probably was.

25. **Aaron Bergman**
June 15, 2006

Do I get to be a Judas, too? The idea that string theory is remotely as well supported as evolution is inane.

(extra letters may be added to said description as per the reader's discretion)

26. **island**
June 15, 2006

> Thanks for the report. Any insight into why the organizers couldn't find a theorist skeptical of the anthropic stuff and had to bring in an experimentalist to do the job?

Maybe they've been reading my blog or website and have finally realized that you have to actually put forth a meaningful argument against the physics and facts, since willful ignorance and comparisons to theology don't mean squat to science.

27. **amused**

June 15, 2006

Btw, on the topic of Lubos, he has let slip in a comment over on Sabine H.'s blog that he will soon be leaving academia to start a new religious cult. Or more precisely, to bring an existing one to the masses. Apparently it is going to be called the Church of M.

(Ok he only explicity mentioned the leaving academia part, the rest is from reading between the lines.)

28. **Yidun**
June 15, 2006

Dear Prof. Woit,

Thanks for your useful info!

Best, Y

29. **Who**
June 15, 2006

this was the 4:57 PM comment on a certain thread of Bee's in case anyone wants to find it.
http://backreaction.blogspot.com/2006/06/science-and-democracy.html#c115024305475393420

"Lumo said…
I am not so much concerned about the future of any funding because I am leaving Academia soon and I never cared about money much anyway.

Moreover, I am also a leading expert in loop quantum gravity so that switching funding to LQG would not affect me even if this question were relevant. 😉

What I am primarily concerned about are aggressive crackpots who have no idea what they're talking about and who attempt to distort science as such and force scientists to share their idiotic beliefs, just like the religious bigots in the 16th century wanted to stop scientists from doing their work, and sometimes they did so rather efficiently.

Sorry to say but the list of these bigotic individuals also includes Christine who just told us that she believes that string theory is not a "theory". What is it? Apple juice? Have you lost your mind?

Sorry to say but I have just seen far too much about her so that I must conclude that Christine is clearly just a plain stupid person. Every sufficiently well trained parrot can say these simple sentences that XY theory could be plain wrong, and all these things…"

Bee's original post was here
http://backreaction.blogspot.com/2006/06/science-and-democracy.html
and was dated Monday 12 June in the evening.
Comment seems to have begun at around 7AM the next morning, if I interpret correctly, and to have continued throughout the day. The greater part of what appears in the first 30 comments was by Dr. Motl.
The comment in question is #22, by my count.

I hope everyone realizes how privileged we are to have the pleasure of witnessing these e-vents, or perhaps a better word would be e-ruptions.

30. **Peter Woit**
June 15, 2006

Before anyone gets too excited about the prospects for Lubos leaving physics to found his own new religion, I'll just point out that quite a while ago he was already mentioning to me the possibility that he might leave physics. Several years later, he's still here. I don't see any more reason to take seriously what he says about this than anything else he writes.

31. **amused**
June 15, 2006

"Do I get to be a Judas, too? The idea that string theory is remotely as well supported as evolution is inane."

Hi Aaron, I guess that's a decision for Lubos, but it might help your chances if you refer to Lubos by name in your denunciation. Like Jacques did 😉

Btw according to my computer your comment just appeared now, or at least subsequent to the others above. I'm pretty sure the same thing happened before with a comment from Smolin during our previous encounter. So there might be some bug in the blog software. I noticed the same thing happened with a comment on Lubos' blog today, in fact Lubos also noticed and remarked on it.

32. **woit**
June 15, 2006

amused,

The spam filtering software I'm using (Akismet) is highly mysterious in its ways and every so often for reasons I can't fathom decides to identify a valid comment as spam. When this happens the comment is held in a queue, not released until I get around to checking what's in the queue. So sometimes you'll see comments mysteriously appearing here many hours after they were submitted.

33. **Thomas Larsson**
June 16, 2006

Before anyone gets too excited about the prospects for Lubos leaving physics to found his own new religion, I'll just point out that quite a while ago he was already mentioning to me the possibility that he might leave physics. Several years later, he's still here.

But there are good reasons to expect that he has problems finding attractive positions. It would surprise me greatly if Harvard would offer Lubos tenure – a school that fired Paul Ginsparg would hardly offer tenure to somebody with Lubos publication record, or lack thereof. Normally other schools would be happy to pick up whatever Harvard threw away, but perhaps not in this case; everybody knows that offering Lubos a position is asking for trouble, which most departments are reluctant to do.

I suspect that when Lubos described himself as leashed last year, he had been given warnings that his blogging activities jeopardized his chances to get tenure. As he now evidently has given up hope, he feels completely unleashed.

34. **Ponderer of Things**
June 16, 2006

I am sure if denied tenure, Lubos will feel that it is because of his “controversial” right-wing political views or his stand on global warming or women in science.

Lack of interesting or even incrementally productive scientific results, his lack of manners, or his blog behavior will not enter as a possibility.

Leaving aside his “internet persona” he would have a solid chance at landing a faculty position in second-tier school, if his tenure gets denied. But behaving like a jerk and pissing people off left and right is not exactly a good way to make friends. I suspect by the time he is on the market again, the number of people who “heard” of Lubos’, let’s say “excentricity”, to put it mildly, will reach critical mass, when most departments will have a couple of faculty who know him (and not from a good side). Once again, this has nothing to do with political views, blogging or string theory – just basic civilty and manners. Surely he is a character, and will be famous, but notoriously famous. Simply put, he is a liability for the department. A timing bomb waiting to go off in a meeting, or in a class or in a seminar. Could you imagine if he called students the things he calls people who disagree with him?

I come to conclusion that while a lot of people are smart in a bookish way, a lot of success in science comes down to how well they can coexist with others. A high ratio of egotistic nerds with lack of communication skills often makes it more difficult to form scientific collaborations.

35. **Alejandro Rivero**
June 16, 2006

I have difficulties to grasp the concept of anyone “leaving physics”. I tend to believe that if one is in, it is in forever. I can understant “leaving publication” or even “missing focus”. But for instance, if it happens that some result depends seriously on the work of a person that has “leaved physics”, I tend to believe that one can phone this person, explain the situation, and get it back.

36. **D R Lunsford**
June 17, 2006

It’s completely amazing that by a fortunate circumstance, (3/2)^12 just slightly larger than 2^7 (that is, 12 fifths are very nearly 7 octaves). The next “close call” (at least as close as the 12-7 case) is (3/2)^53 ~ 2^31. Someone actually built an organ with scales containing 53 notes (in effect, 31 white keys and 22 black keys). A major scale has 32 notes and the stave has 17 lines and 16 spaces. There are 53 separate tonalities and a bewildering variety of minor and diminished scales.

If the first “good number” had been much less or more than 12, Western music as we know it would have been much, much different.

-drl

The Scientific Curmudgeon

June 15, 2006
Categories: Uncategorized

John Horgan has an excellent new blog that he has recently started up, called The Scientific Curmudgeon. Horgan may be best known for his provocative 1996 book *The End of Science*, which was one of the first books for the general public that expressed skepticism about string theory (another was David Lindley's 1993 *The End of Physics*). His portrayal of Witten in the book was a bit of an unfair hit job, but he got the story of what was going on in particle theory about right, unlike just about every other science writer working at the time. In recent years his attention has turned to issues of neurobiology and cognitive science, as well as the relation between science, religion and mysticism. He now teaches at Stevens Institute of Technology in Hoboken, and runs its Center for Science Writings.

Horgan describes himself as a "hopeful skeptic", writing:

I still see science as our best hope for understanding ourselves and the universe, and for creating, if not a sci-fi utopia, then at least a much better world. Scientists can provide us with cleaner, cheaper sources of energy; better treatments for cancer, AIDS and other diseases; more detailed accounts of how brains make minds. That's why, in spite of writing a book called The End of Science, I've remained in the science-journalism racket, why I work at a science-oriented school, why I encourage young people to become scientists. But I also encourage greater recognition of science's limitations and fallibility. It is precisely because science is so consequential that we must treat its pronouncements skeptically, carefully distinguishing the genuine from the spurious.

One of his recent postings discusses the issue of the Templeton Foundation, yesterday's is a charming story about his daughter, linked with a tale of his adventures among the cosmologists back in 1990.

Comments

1. **Jonathan Vos Post**
   June 15, 2006

   Is "hopeful skeptic" a reasonable position on String Theory, as opposed to, say, "hopeless skeptic"? I like Horgan's science writing, and blog, but am not clear why he is hopeful.

2. **island**
   June 15, 2006

   Some things never change… 😉

   http://discuss.longbets.org/discuss/postlist.php?Cat=&Board=12

3. **banned by Lubos**
   June 15, 2006

   A (possibly off-topic) comment posted on http://motls.blogspot.com/2006/06/reviews-of-certain-book.html#links and disappeared after 10 seconds.

   hi, I am LubosMotl. At line AB of page XZ Woit put a "," while Witten would have used a ";". This proofs that Woit is a moronic crackpot. He makes me vomit. I predict that when I will be back, the censorship committee will have deleted my post.

4. **Lubos Motl**
   June 15, 2006

   Dear banned by Lubos who is LubosMotl,
   what an intelligent comment.

   Thanks for moving back to the right blog where you belong 😉
   Lubos

5. **banned by Lubos**
   June 15, 2006

   dear Lubos, thanks for saving western civilization from crackpots.

6. **Nick**
   June 15, 2006

   I love sarcasm.

7. **runge_kutta**
   June 15, 2006

   Peter, have you actually met Lubos in person ? I wonder what would happen if you two ran into each other. You'd probably cave in to the years of societal conditioning you've been subjected to and crank up those fake smiles, greet each other politely, have a short, awkward chat, all the while pretending you actually like and respect each other. I guess that's the civilized thing to do, as defined by societal norms.

   Society sucks 🙁

8. **Christine Dantas**
   June 15, 2006

   runge_kutta wrote:

   I wonder what would happen if you two ran into each other.

> There is a theory which states that if Lubos Motl and Peter Woit run into each other, the Universe will instantly disappear and be replaced by something even more bizarre and inexplicable.There is another theory which states that this has already happened.

– Modified from Douglas Adams, "The Hitchiker's Guide to the galaxy"

🙂

Best,
Christine

9. **Eugene Stefanovich**
June 15, 2006

While reading Horgan's writings I had a thought about why public should be sceptical (if not suspicious) about theoretical sciences, especially when the experimental part is stagnant. It seems to me that scientific process has a huge amount of inertia built into it. There is a very low threshold for canonizing theories and a very low tolerance for alternatives. I don't think this changed much during last 2000+ years.

We all laugh at medieval astronomers who couldn't accept the heliocentric system. Aren't we laughing at ourselves? According to present day standards the Ptolemy's theory was a very respectable scientific achievement, worth defending against such "crackpots" as Copernicus and Galilei. It had a beautiful math behind it. There was a lot of experimental support. It was written in all textbooks. It was supported by smartest people (Aristotle) during millenia (not just 30 years). What else could you ask for to canonize a theory? How is it different from modern canonical HEP theories (I am talking not just about superstrings)? When will we ever learn?

10. **woit**
June 15, 2006

runge_kutta and Christine,

Yes I have briefly met Lubos in person, and it was a perfectly pleasant encounter. It's also a little-known fact that I once wrote a recommendation for a fellowship for him. But back then he was just an over-zealous fanatic (my recommendation said something like: "I don't think you should support string theory, but since you're going to do it anyway, you might as well fund a smart true-believer"), he hadn't yet been driven over the edge into nuttiness by the failure of string theory.

11. **Sailorstar**
June 16, 2006

Your writings are a lesson to a young woman blogger.
Louise (A Babe in the Universe)

12. **John E. Gray**
June 17, 2006

Woit, I have been reading the back and forth between you and Motl for the past couple of months. I have found it quite interesting to deconstruct what is at the heart of you arguments. I have concluded that the heart of the problem is Horgan claims science is versus Motl's viewpoint. That is what divides the two of you is a total disagreement about what constitutes fundamental science. While I am not qualified to discuss string theory, I have thought a fair amount about what constitutes good and bad science. Below are some comments about the scientific method that are at variance of what was found in "The End of Science". I have somewhat a contrarian viewpoint, so have included somewhat extensive comments from a paper I am revising. If you think these comments add some clarity to you monologue, please post them, if not don't feel you have insulted me. I wish you both well in you monologue about science, I have found this foodfight too distracting and need to return to other matters:
Scientific theories have three important characteristics: compression, usefulness, and generality. Without compression, there is no advantage to developing a theory, tabular results or taxonomies would be sufficient. Taxonomic sciences such as biology and paleontology are still scientific, A technical discipline that has large amounts of data but little interpretation contains little information since there is no surprise associated with new data. information gain cannot be measured by acquiring new data.
Usefulness has two components: engineering application and predictability. Without the ability to apply a theory to accomplish calculations, the theory becomes a tautology without an exterior world to which it applies. It becomes mathematics. Predictability is a component of usefulness, since predictions lead to the ability to design rather than just build. Most theories advanced provide compression and usefulness. We also require theories to have generality. Falsifiability plays a key role in probing the generality of theories (Popper 1959). The importance of falsifiability is the role it plays in the understanding and development of proper science has been often misinterpreted by commentators rather than practitioners of the scientific method. Falsifiability should be properly understood as the complement to generality. While it is often claimed that a theory can be disproved by a single negative observation, this is not the correct interpretation. A negative observation is simply defines limitations on the generality of a theory. In other words, falsifiability does not negate theories, but rather it limits the boundaries within which theories are applicable. Since usefulness is an important criteria, there is always a boundary associated with the theory in terms of applicability. There is, of course, a point where repeatedly falsifications cause one to reject a theory (Jaynes 1968).
A single positive observation is much less compelling test than a negative one, since almost any single prediction can be verified by appropriate selection of evidence. There is an intimate connection between observation and sampling in the sense of Shannon's formulation of communication theory that is needed for all sciences. A principle of ontology is advanced by espousing a specific type of deconstruction of uncertainty (Sardis 1995) and the nature of uncertainty is always an important component for understanding ones discipline. In general, the more information gain we receive from data, the more biased (in the colloquial sense) we should become because informative priors converge in a parametric sense to the correct result (Jaynes 2003). Thus, a theory of sampling needs to be extended when we go beyond the reduction to numbers and instead accomplish a reduction to symbols. Most sciences would benefit from a deeper exploration between sampling and confirmation that a symbolic theory of sampling relative to their particular subject would accomplish.
There are several aspects of confirmation that should be clarified relative to the symbolic. If when measurement reduces to a number, the process is symbolic in the sense that is not usually considered from both an experimental and computation viewpoint. All computation and measurement is interval based. One does not measure a point, one measures an interval number associated with the interval of experimental uncertainty. The same thing occurs when one is engaging in computation as was pointed out first by Young (Young 1932). Thus confirmation is less related to theory since the instruments used for confirmation and computation within the theory are not formulated within the theory. Another issue is the compression. The ontology associated with algorithmic information theory (Chaitin 1987) suggests that a theory that doesn't have redundancy built into is not a good theory. But redundancy is not so easily discussed beyond the realm of the rational numbers. What redundancy means when one discusses abstract symbols is less clear. Issues such as interference and correlation are always possible between symbols.
Theory plays a different role than experiment; it writes sentences that in some cases explore well beyond the restrictions of instrumentality. Theory, while driven by the words and grammar of experiment, is always trying to introduce new forms, establish metaphors, and use words in less experimentally important context, so theory is more concerned with trying to ferret out meaning (mostly by detailed working out of specific examples or models). It does this by concerning itself with constant refinement of the explanations for what is known, establishing metaphors that connect previously unrelated subjects, improved tool making (both new mathematical concepts that aid theory and instruments that change what it is possible to measure), and making predictions. Theory, while keeping in mind Feynman's dictum, allows one to define and explore semantics of the subject while being ground in the syntax imposed by experiment. Mathematics provides the theoretician with an entirely syntactic language to explore and maintain rigor. But as has been noted before syntax is not science, metaphors are how we establish the "truth" and metaphors are inherently transformational or substitutions by their nature. Thus, syntax may tell us whether a transformation is correct but semantics is necessary to decide if the transformation results in something meaningful.
Truth is primarily established through experiment. But truth also has a logic associated with it. Philosophers have tended to ignore such concepts as metaphors and tool

making in their critique of science, and giving short shrift to the concept of refinements based on new methods for accomplish measurement that revolutionize the ability to observe (Dyson 1999). All measurements are metaphors of a sort, though many treat measurement as a tablet from Moses. Measurement is canonized by theoreticians in their writings, yet those who have dealt with data are much more circumspect about it. At some point, axioms relative to what and how we measure will be ferreted out by mathematicians. Once this has been accomplished, then we will have to face some logical limitations. Already we know that there are problems in physics that are undecidable in the sense of Godel (Wolfram 1985). But the question is whether there are more fundamental limitations than that. If the axioms which encompass the experimental methodology are sufficiently rich to encompass arithmetic, then there are experimentally undecidable issues in physics and in science. This is troubling–"Is science decidable?" in an experimental sense is far more troubling than a specific theoretical result that is undecidable. Equally unsettling is question of complexity that was first raised by Godel in the theoretical sense (Godel 1936). He noted that there were problems that had proofs that were intrinsically long, so long in fact that some theorems could never be established within a given set of axioms. He further noted that by adapting additional axioms that proof's length might be shorted considerably. What if conformation by experiment has similar difficulties? What good is a theory that can never be confirmed by instruments because the number of measurements that would lead to its acceptance is longer than human civilization's life span? Could this not be the case when and if we ever find a theory that has the possibility of being a "unified theory of physics"? I wonder if this message from the Landscape, if it becomes more real. If we adopt speedup axioms, then what are the consequences for our epistemology? The symbolic, from a experimental viewpoint, is fraught with potentially unpleasant consequences from a logical and complexity perspective.
If the symbolic viewpoint was more universal, then most of what constitutes science and its boundaries would have to be reexamined. This is why the ongoing monologue between Woit and Motl is so rich in terms of its implications about what is and isn't science. I find that science is its infancy in terms of understanding what constitutes science because of the issues of information gain, conformation, sampling, as well as the theoretical aspects of metaphor. Drawing on Feynman's "It is surprising that people do not believe that there is imagination in science. It is a very interesting kind of imagination, unlike that of the artist. The great difficulty is in trying to imagine something that you have never seen, that is consistent in every detail with what has already been seen, and that is different from what has been thought of; furthermore, it must be definite and not a vague proposition. That is indeed difficult." Imagination in science is like writting a sonnet, but richer and more difficult.
The rich possibilities of at what point an intellectual discipline that can be confirmed or falsified by data suggests that concepts and boundaries of science have to be rethought. Sampling and confirmation based on data or the lack thereof need to be completely rethought from the symbolic perspective. The algorithmic perspective needs to be incorporated as well, but not necessarily the way Chaitin has suggested. In many ways, we are closer to a beginning of science and determination of "The scientific method" rather than "A scientific method" as currently exists. Understanding why science is successful from a ontological and metaphysical perspective, remains to be accomplished. If I had any doubts about us being at the beginning of understanding of what science is, they have been erased by the strange and amusing postings I have seen here. Best wishes to you all and thanks for all the fishes.

13. **island**
June 17, 2006

I like John Horgan thinks that widely accepted assumptions, like inflationary theory, are crap, and he has good reason to say that if you simply do all of the demanded science, rather than science that's strictly limited to the belief that it requires a naked singularity to produce a big bang.

Projecting backwards to the point where it requires inflation to account for the horizon problem should be a clue that there is a mechanism that enables a universe with volume to have a big bang. Especially since the problems of causality and structure that are associated with the horizon and flatness problems don't exist when a universe that has volume has a big bang.

I continually notice that people keep calling for accepted assumptions to be, once again, reassessed, but eveyone has their own ideas on which assumptions are open to review, because, for example, the flawed theory does do some things quite well. What they apparently fail to recognize is that the solution to fixing a fundamental flaw is in the foundation below, but not in some more grand theory that exists beyond the the flaw.

I think it was Urs who fairly recently commented in the research group to someone that, "this isn't 1917″…

Yes, it is.

14. **Eugene Stefanovich**
June 17, 2006

island,

"this isn't 1917″… Yes, it is.

Great point! I think this is precisely where the boundary between science and theology lies. As soon as we declare a certain physicist or certain theory beyond criticism, we stop doing science and switch to theology.

I like this quote from Oppenheimer (which for an untrained ear may sound as a definition of crackpotism):

There must be no barriers for freedom of inquiry. There is no place for dogma in science. The scientist is free, and must be free to ask any question, to doubt any asssertion, to seek for any evidence, to correct any errors.

As for me, I don't mind to discover that we are still in 1905.

15. **Chris Oakley**
June 17, 2006

> There must be no barriers for freedom of inquiry. There is no place for dogma in science. The scientist is free, and must be free to ask any question, to doubt any assertion, to seek for any evidence, to correct any errors.

And in his case, free to vaporize, fry and otherwise irradiate a lot of people.

16. **Jonathan Vos Post**
June 18, 2006

Good thread.

There is irony in John E. Gray writing:
"Drawing on Feynman's "It is surprising that people do not believe that there is imagination in science. It is a very interesting kind of imagination, unlike that of the artist. The great difficulty is in trying to imagine something that you have never seen, that is consistent in every detail with what has already been seen, and that is different from what has been thought of; furthermore, it must be definite and not a vague proposition. That is indeed difficult." Imagination in science is like writting a sonnet, but richer and more difficult."

That's because Feynman did in fact coauthor a sonnet with me, which was published by AAAS, republished, and set to music. My Physics professor wife and I, for that matter, had a poem in the magazine Science. Hence mainstream science journal referees and editors have openly shown ability to recognize scientific imagination in the methodology of poetry. For that matter, NASA commissioned me to write a poem as the frontispiece of a conference proceedings. Gray is exactly right about imagination in this context. The issue in this blog becomes something like: “Do String Theorists have too much imagination, in inventing pretty math that defines 10^500 universes in a multiverse; or do they have too little imagination in not being able to follow Feynman’s dictum ‘ it must be definite and not a vague proposition?'”

Or is the problem that String Theory is not Science, but rather Science Fiction or Poetry or Theatre, but does not meet Woit’s criterias for GOOD Science Fiction or Poetry or Theatre? Are we waiting for a happy ending, a lovely final rhymed couplet, a final act that wraps up all the subplots? Or are we Waiting for Godot?

Censored Comments From the Reference Frame

June 15, 2006
Categories: Uncategorized

Since many people have been posting off-topic comments here that were censored over at Lubos Motl's Reference Frame, I'm creating a separate posting so that there will be an appropriate place for these. Also, if you want to try and carry on a discussion with Lubos on the topics of these comments, here is the place to try (although I don't quite know why you're bothering…).

Comments

1. **runge_kutta**
   June 15, 2006

   Peter I've always wondered why you never post on Lubos' blog, though he posts on yours. Are you banned ? If so, don't worry, I was banned too, for making an offhand comment about Bert Schroer. I no longer remember the discussion we were having, except that it had something to do with Bert Schroer, so I guess I can't continue it here.

2. **Anon-e-mus**
   June 15, 2006

   Lubos, I may not always (more precisely only very rarely) agree with you on many topics, but I believe you are certainly much, much more intelligent than that. Peter is clearly not referring to **all** deleted comments, but only to those bona fide comments that were deleted because of your "zero-tolerance with those who disagree with me"-policy. Even if you appear to think poorly of his intelligence, you cannot possibly believe that he is interested in archiving porn/pills/casinoes spam deleted from your blog.

   (Recently, I was told by someone who knows you personally that you are in fact a normal/nice person in real life, so maybe you should try and get you online persona back in line; he/it seems to suffer from too many forms of personality disorder to even begin to enumerate; if that is not your real self, and I have no reason to doubt the word of your colleague, why would you want to project that kind of appearance? A polite manner usually gets you a lot further.)

3. **MathPhys**
   June 15, 2006

   Motl,
   You're a sick kid. Get help.

4. **Alejandro Rivero**
   June 15, 2006

   Hmm I guess the title should be "supposedly deleted comments…"

5. **woit**
   June 15, 2006

   I've deleted some of the spam comments Lubos posted here (and my spam filter caught others). I suppose I could set up a separate place for comments deleted from this posting….

   runge_kutta,

   The last couple of times I wrote a comment on Lubos's blog he immediately deleted them. One was responding critically to something he said about me, the second was responding to something that one of his commenters ("LambChopofGod") was saying about me. Not something I'm likely to waste my time doing again.

6. **Kasper Olsen**
   June 15, 2006

   If you

   "don't quite know why you're bothering…"

   then why do YOU bother to make this post?

7. **amused**
   June 15, 2006

   Here's the most recent of mine, deleted from the comment thread of his "Dean of crackpots" post.
   ————
   LM: "The reason why the situations of string theory and evolution are analogous is that both of them are more or less inevitable given the known data, known approximate laws of Nature, and known and derivable logical constraints."

   Oh dear, Lubos, looks like we're going to have to call on Jacques Distler to denounce you again.

   (You remember the previous time over at cosmicvariance, right?)
   —————

   By remarkable coincidence, a couple of days later Lubos writes his "Darwiniana: evolution and string theory" post, referring to the denunciation episode and elaborating on his string theory/evolution analogy…

8. **woit**
   June 15, 2006

   Kasper,

Not much of a bother, only took a few minutes to make the posting. The reason I did it was to deal with all the Lubos-deleted comments that people were posting here. If I left them in the postings where they were, they interfered with the discussion about the topic of the posting. But I didn't want to delete them, to be in the position of censoring those already censored by Lubos. This is the best solution I could come up with, if you have a better one, let me know. The best one would be if he would not delete reasonable comments from people who disagree with him.

9. **Ponderer of Things**
June 15, 2006

Lubos, since you have been offered asst. prof. in spring of 2004, you haven't published a single scientific paper. Is there something in the works, or does editing your blog comments take away all of your research time?

26 months, or roughly 800 days. Are you going to go for 1,000 days? More?

Any brilliant stringy ideas lately? Are you not afraid of some youngsters soon calling you what you call others – intellectual rodents/microbes, crackpots, washed up, etc.?

That would be… ironic?

10. **Dan**
June 15, 2006

http://arxiv.org/abs/hep-th/0606100

http://arxiv.org/abs/hep-th/0601001

Don't worry: one day, besides pondering, you'll be learn how to use search engines…

11. **MathPhys**
June 15, 2006

My comment to Motl posted above ("You're a sick kid"), was specifically a reaction to a post by him (since then deleted by Woit) that contains pornographic references which I thought that was way, way out of line.

Motl, I have no idea what goes in your mind, but please tone it down a bit, because you push people too far, and it's no longer funny.

12. **MathPhys**
June 15, 2006

Dan,

Co-authoring two speculative papers, within the span of 3 years, is by far not a whole lot, particularly for a young theoretical physicist at this stage of his career, working in a field that's supposedly "active".

13. **top ten**
June 16, 2006

my attempt of discussung the main achivements of string theory relevant for physics in http://motls.blogspot.com/2006/06/science-vs-democracy.html was interrupted by insults (Harvard can be proud of the "squirrel"). My deleted answer was, after some re-editing

—
dear Lubos, you misunderstand me: I am not telling that "AdS/CFT" was not invented by string theorists. I am telling that string theorists made a few true achievements, but only on issues that physicists do not consider much relevant, and failed on the issues that would have given real progress in physics.

This is why usually string theorists don't talk to experimentalists, and why their talks often annoy phenomenologists and cosmologists. Realizing this fact is a medicine that cures many young string theorists, grown in a community where everybody agrees that counting the entropy of black-hole-like-SUSY-states was an enormous achievement.

I comment on "AdS/CFT" that is the most interesting result: the interesting part of "AdS/CFT" is the "/". Indeed people who classified CFT or supergravities in AdS often notice that their works did not attract much attention until the "/" was recognized. Indeed my squirrel committee planned our universe such that we do not live in AdS nor in CFT, so that "AdS/CFT" beautifully connects two theories of which we do not care much (this is another check that they are dual). Attempts of applying "AdS/CFT" to QCD did not improve what we already knew from other less celebrated approximations"
—

Later, Lubos defended his arguments applying the technique known as "useful idiots": comments that agree with his line are allowed and the rest is deleted. A normal person understands that applying this technique without having control of all media is not intelligent, and the situation of string theory is not so bad that a constructive discussion has to be stopped at any price. This is I why I fear that Lubos does not think "string theorists will reward me for this dirty job" but he really thinks what he says: "I am Jesus defending civilization from crackpots". This would be sad.

14. **top ten**
June 16, 2006

to defend Lubos, let me add that it is true that in past years he co-authored only one paper, but it is a good one: conjecturing that quantum gravity implies "gravity is weaker than electromagnetism" is closer to physics than deriving from string theory that "gravity exists". However these good results also show how this line of research is far (and possibly hopelessly far) from getting something relevant for physics.

I see no point in lowering quality standards such that the rediscovery of hot water can be considered as a major achievement. No hype will convince physicists, laymen and crackpots that it deserves a Nobel prize.

15. **RM**
June 16, 2006

deleted post was simply: "Mr. Motl, what are the predictions of string theory that you say will shortly be confirmed by experiment?". (I guess there aren't any).

16. **Christine Dantas**
June 16, 2006

To anyone interested, you can discuss Lubos Motl's top ten results over at my blog:

http://christinedantas.blogspot.com/2006/06/top-10-string-theory-results.html

No posts will be deleted there, except those with personal attacks.

Best wishes,
Christine

17. **Chris Oakley**
June 16, 2006

The quoted text below, by LM, appeared in his blog. If I answer it there, the comment will probably be deleted, so let me try here. It concerns my Amazon review of "Not Even Wrong".

> 5 stars. Hi, my name is Dr. Chris Oakley, it is my fourth review and I am the 110,000th best reviewer. As my name indicates, I have a physics PhD and as the degree proves, I will be in Sabine's committee that will democratically vote about the future of physics. Twenty years ago, I wrote three or four preprints. Unfortunately, no other physicist has yet appeared who would think that they make any sense – but that's probably because of the string mafia.

Actually, I only blame the String Mafia indirectly for this. The issues raised are not really considered to be problems by anyone other than AQFT people. Some of the latter did make sense of my work, but seemed not to be willing to accept any of the conclusions. One of the papers was in fact published in a respectable journal (Physica Scripta).
One minor quibble: at Oxford a Ph.D. is called a D.Phil.

> I am especially proud about the paper that renormalization is not needed. My excellent solution is to insert random factors into the loop Feynman diagrams, such as the delta functions and step functions: I call the added step functions "positivity of energy". I have figured out that for some smart extra factors, this can miraculously make the integrals convergent! I don't care that the unitarity is sacrificed because unitarity is just a stringy propaganda. And I hope that the experiments will be changed to fit my predictions. My theory is clearly more important than AdS/CFT, and I will vote to replace AdS/CFT scholars by scholars who study my theory.

Lubos links to the first paper in the series, entitled "On the possibility of quantum field theory without renormalization". The title is, as he suggests, optimistic. Local field equations seem always to lead to infinities, whichever way one does QFT. But there is no tampering with the graphs (I think he is confusing my methods with the Epstein-Glaser approach). The graphs I show in the paper are indeed always finite as they are just phase space integrals, but there are ones which have pathological divergences and, yes, the "removal" of infinites here is handwaving. I think now that one just has to start with non-local field equations. As for experimental tests, I was considering [tex]\Phi^4[/tex] theory in this paper! A later paper, on quantum electrodynamics, reproduces the results from tree Feynman graphs, so he had better have the discussion about experimental verification with someone else.

As for unitarity of the S-matrix, there is no S-matrix here as I am trying to be consistent with Haag's theorem. You should read the whole story on my web site some time, Lubos, if you can spare the time from your busy schedule.

> I have not read the book – in fact, I am writing this review half a year before the book is published. But I think it has something to do with the Star Trek by Isaac Asimov, and I prefer Asimov over the string theorists. Advocates of all physics theories would only admit that the author is right, in his book that I have not read, if they were wired up to a Polygraph. Also, I recommend you Second Creation.

Mostly correct except that I have a January 2005 copy of the manuscript which Peter gave to me when I visited that month. I also have a copy of the finished product. There are updates, but as far as I can see, no substantial differences.

18. **Chris Oakley**
June 16, 2006

Oh, and I should add: in January of last year we were sitting on the tarmac at JFK for two hours as the flight was delayed. With Peter's book to read, the hours just flew by. It is a great read.

19. **Ponderer of Things**
June 16, 2006

Dan,
I thought someone will mention this.

arxiv is a depository of unrefereed, unpublished drafts that may or may not see become an actual publication.

It is no substitute for peer-review, a fact some stringy fellas need to be constantly reminded of.

Any crackpot, to use Lubos' language, could make a dozen of arxiv posts within a couple of days.

According to ISI Web of Knowledge, Lubos has 10 publication, with h-index of 7. Over the past three years he had one paper – from APR 2004. He had zero papers in 2005. He had zero papers in 2006, so far. Look it up if you don't believe me.

I wonder how many assistant physics professors ANYWHERE have this sort of publication record.

20. **Patrick**
June 16, 2006

I'm not sure whether I should post this here, it's got nothing to do with Lubos Motl, but it is a question about string theory. I've just downloaded a copy of

"The String Theory Landscape: a Tale of Two Hydras"
Joseph P. Conlon, Contemporary Physics, vol 47 2006

I have a background in condensed matter physics, but am pretty ignorant of the standard model, QFT (except the RG stuff that we studied in Stat. Mech.) and string theory. I was wondering if anyone here has read it and if it is reasonable summary of the issue.

Thanks.

21. **Ponderer of Things**

June 16, 2006

More data points – let's look at other young string theorists.
Sergei Gukov (currently a professor at CalTech), who has received his PhD in 2001, the same year as Lubos, has published 21 papers since then. Marcus Spradlin (now a faculty at Brown), and another 2001 PhD recepient, has published 22 papers. Nima Arkani-Hamed had over 40 new publications in the 5 years following his PhD (1998-2002).

Lubos has published 5 since his PhD.

These are all *published* papers, not hypothetical pre-prints.

I am sure people will point out it's unfair just to look at the toal number of publications, that he had a single brilliant paper that is better than anything those other folks has produced, but that's open to interpretation.

By any standard, Lubos has not been as productive as some of his colleagues. Experimentalists can often find their work to be disrupted by having to setup a new lab at a new university, but even then most of them manage to keep producing results.

22. **Peter Woit**
June 16, 2006

Patrick,
The paper you quote is a reasonable summary of the current anthropic string theory ideology, written by a graduate student string theorist. I may comment more on it in the next posting.

23. **garbage**
June 16, 2006

Peter,

I kinda agree with Lubos this time, why moving his trash can to your blog?

24. **woit**
June 16, 2006

garbage,

This isn't the trash can that Lubos is describing, it was intended as a place for people to post intelligent comments that he has censored. He has chosen to run his weblog by deleting any comments from people who disagree with him, trying to give the impression that what he has to say has unchallenged support. People censored in this way have chosen to bring their comments over to my blog and post them. What should I do about this, delete them as off-topic, or set up a separate topic to accomodate them? Do you have a better idea. If so, let me know it.

25. **Eugene Stefanovich**
June 16, 2006

Hi Chris,

I am appalled by Lubos' suggestion that you didn't earn your D.Phil. He has no right to say that.

Looking at his writings, it is clear to me that he has a very distorted picture of what science is all about. It seems that he is longing for some orderly structure in physics where a few high priests (Witten, Hawking, Maldacena, who else?) will decide which direction is promising and which is not; the rest of us, like obedient ants, will bring our small pieces to solve the big puzzle; and crackpots and heretics will be confined to torture chambers.

OK, I am exaggerating, but not much.

I think your work on renormalization is an interesting attempt to solve a long-standing problem whose importance was recognized by Feynman, Dirac, and Landau among others, and whose relevance to physics is unquestionable. I happen to disagree about your particular approach to the solution, but I have no doubt that what you are doing is physics at its best.

26. **Chris Oakley**
June 16, 2006

With reference to the comments on Lubos's post about Amazon reviews for "Not even wrong":

1. I think that Brian Greene and I had the same thesis advisor (Graham Ross)
2. My thesis did not contain anything significant about renormalization, for or against. It was almost entirely about free field theories.
3. Lubos seems to have no clue about what I actually said in any of my anti-renormalization papers (see earlier posts on this thread).
4. But if he ever takes the trouble to find out – if he's smart enough (case not proven as yet) – he may be interested to know that Brian Greene had the same idea, as did Stueckelberg (1934) and Kallen (1950).

27. **Chris Oakley**
June 16, 2006

Hi Eugene,

As with most things, there is little point in taking Lubos seriously. My thesis was not about anything especially controversial but my external examiner, Lochlainn O'Raifeartaigh (of Supersymmetry no-go theorems fame) did acknowledge that the work – in his words – "tied up a few loose ends nicely". If anyone is going to call me a crackpot then it should be for my later (anti-renormalization) work, and that alone.

28. **ad**
June 16, 2006

It may be possible that Lubos spends some time in reacting to string theory critics and to some others which he might have invested in research entirely (although only he can say best about it). But THIS IS NOT WASTE OF TIME. One can easily see that the scientific community can be served in several ways, not just by doing active research. Astrophysicist S. Chandrasekhar once remarked about one famous physicist who had to neglect his research to play active role in setting up a scientific institute (which later became famous and still is) that his dedication to science is not less than any other active researcher. In the same way I find Lubos's reaction to critics a service to string theory community. Not every string theorist would be able to play the same role as he does. His comments are often deep, illuminating and very sharp as opposed to many people (Lubos's targets) who merely reveal their ignorances.

29. **da**
June 16, 2006

setting up a scientific institute is not the same thing as insulting people on blogs

30. **MathPhys**
June 16, 2006

While it's clear that Motl knows quite a bit about strings, his comments on physics in general, and physics that's older than strings in particular, are often myopic and blinkered.

31. **Peter Woit**
June 16, 2006

ad,

"I find Lubos's reaction to critics a service to string theory community"

Perhaps the string theory community enjoys reading the way Lubos writes about his critics, but they're making a big mistake if they consider this a service to them. He's doing a fantastic job of convincing the rest of the physics community that there is something very wrong with string theory and with the way it is pursued.

32. **anonymous**
June 16, 2006

This is where I lose you, Peter. If indeed he is screwing up the reputation of string theorists, why don't people publicly oppose him? Or at least clearly distance their stance/opinions from him?

It seems to be that you are implying more than just Lubos' commissar-like behaviour. You are implying something about the broader string theory community. But, where is the evidence of that?

33. **Peter Woit**
June 16, 2006

anonymous,

Some string theorists would never read his blog, think he's a fool, want nothing to do with him, and so see no reason to mention him one way or another. Others seem to think it's a good idea to participate in his blog and encourage him.

All I can say is that an astounding number of people, particle theorists and not, have told me that reading Lubos's blog convinced them that there was something seriously wrong with string theory and string theorists.

34. **Who**
June 16, 2006

This is where I lose you, Peter. If indeed he is screwing up the reputation of string theorists, why don't people publicly oppose him? Or at least clearly distance their stance/opinions from him?

It seems to be that you are implying more than just Lubos' commissar-like behaviour. You are implying something about the broader string theory community. But, where is the evidence of that?

Interesting question. forgive me for intruding, since it was asked of Peter and he will doubtless respond.

We don't know as of present that the string community is NOT reacting so as to distance itself from Lubos Motl, though perhaps not fast enough for its own good.

Be that as it may, there is the historical question why the string community (well organized and having its own interests at heart) DID not distance itself or take steps to control EARLIER as in 2004 and 2005 when the mean and misleading statements were flying.

I think I can bend your question slightly so that it asks **why did they not then** realize that Motl's public ranting would be harmful to the longterm interests of the string community and more generally of physics theory? What does this say about them, and perhaps about US academic communities more generally? Is there something amiss? And what (besides this) is the evidence for it?

One possible answer is that it was not expected (in 2004-2005) that non-string quantum gravity would proceed as rapidly as it has. Although understaffed and underfunded, especially in the US, it has made notable, even enviable, advances towards goals which string theorists might themselves aspire to.

If that had not happened, the community might have been able to laugh off Motl's antics, since they would have done no general harm besides the chagrin of those outsiders whose inconsequential work he belittled. In other words he didn't HAVE to turn out to be such a liability. But now when there is some serious challenge from the competition, it looks bad to have a half-informed madman for advocate.

Just my take on it. will be interested to hear our host's response.

35. **Aaron Bergman**
June 16, 2006

There is no "string community" to take some sort of uniform action. What I do know is that most people just want to completely stay of all this online stuff and stick to research.

36. **garbage**
June 16, 2006

"…What should I do about this, delete them as off-topic, or set up a separate topic to accomodate them?"

I guess if they dont fit anywhere in your posts this also become kind of your ‘trash can’ as well doesnt it?

That doesnt mean that nothing intelligent could come out of garbage 😉
Indeed, the lore says that once upon a time an indian mathematician by the name Ramanujan first discovered mathematics in an old book he found in the trash. I, in the other hand, wouldnt recommend the latter as the best source of education 😉

37. **Ponderer of Things**
June 16, 2006

Other string theorists might

a) not know/care about blogs (especially older faculty)

b) find Lubos’ behavior reprehensible but decide not to interfere publically for the same reason why one should never wrestle with pigs – you will get dirty and the pigs might enjoy it

c) disapprove of his behavior and tell him of their disapproval personally (to which he reacts as if he is being “leashed”)

d) disapprove of his behavior but also assume that Lubos is a big boy who can speak for himself and if he makes a fool out of himself it is not damaging reputation of his scientific community

e) approve of his behavior and see Lubos as a brave warrior (knight in shining armor) taking on heretics and doubting thomases where everyone else is too chicken to say anything

I think it used to be a), quickly shifting to b), with people who tried c) are now reverting to safer option b). Of course in Lubos’ Wonderland Multiverse the correct answer is e) and only e). Anyone who thinks otherwise is a squirel, a microbe or a crackpot.

38. **MathPhys**
June 16, 2006

Ponderer,
I think it’s option (d). It’s too messy to start quizzing your colleagues about their personal business, and Motl’s blog is his personal business. I don’t see Vafa wanting to get into that.

39. **MathPhys**
June 17, 2006

I think we have psychoanalyzed Motl long and hard enough. Time to move on.

40. **anonymous**
June 17, 2006

String theorists wait hoping that LHC will discover something that is or can be named “stringy”. Like supersymmetry, or extra dimensions, possibly with branes, or an anthropic nothing,… We will see.

41. **Aaron Bergman**
June 17, 2006

My comment (which seemed to take quite a while to show up again) should read “completely stay off all this online stuff”.

42. **anonymous**
June 17, 2006

“There is no “string community” to take some sort of uniform action. What I do know is that most people just want to completely stay of all this online stuff and stick to research.”

Standard nonsense from Aaron, as usual. If that were the case, the converse would be true as well, i.e., any bad press for string theory would not bother them, since they just want to “stick to research”. Polchinski, Distler, Srednicki all write in to reprimand Peter for inaccuracies etc. when the chance arises. The last one’s responses are especially comical, I recommend the readers to browse through Peter’s earlier posts’s comments.

We also have (Peter’s word) evidence that people PRIVATELY write to Peter saying how much they appreciate his work, but hesitate publicly themselves. ,

Its pretty easy (and cheap) when funding is flowing, and the press is glowing, to say “it doesnt matter, I’m just interested in research”. But then jump in if string theory gets bad press.

43. **Aaron Bergman**
June 17, 2006

I urge you to consider the word ‘most’ in my comment.

44. **JC**
June 17, 2006

Why not create a new “category” of posts, where all the stuff Lubos censors ends up? This is probably bound to show up many times in the future, with folks posting their deleted posts here.

45. **Peter Woit**
June 17, 2006

JC,

The WordPress category feature applies to posts, not comments. For now, having one post devoted to all things Lubos I think is more than enough. In the past I’ve had to shutdown the comment sections of postings after a few weeks in order to control spam, but the new version of WordPress has an anti-Spam feature that may be good enough to do the trick itself (although it still misfunctions sometimes for reasons I don’t understand, sorry Aaron…).

46. **Benjamin**
June 17, 2006

Sometimes I wish I knew if this apparent bad feeling is for real, or whether ambitious and spirited physicists are just jousting with each other for fun.

47. **alexis**
June 18, 2006

If you read this:
http://motls.blogspot.com/2006/06/top-ten-results-in-loop-quantum.html
.. and the (currently) 'quick' comments, you'll see what a bunch of sexist mouthbreathers these buttheads are.

48. **Peter Woit**
June 18, 2006

In case people are wondering what alexis is referring to, in his latest posting, in the middle of supposedly explaining what is wrong with a list of achievements of LQG, Lubos includes a photo from the website of Louise Riofrio. The photo shows her with an impressive lizard. Lubos puts this up with links to her web-site and a not very good paper she wrote, trying to humiliate her as an idiot (although she's an attractive woman, so he writes that she "may have other virtues of course"). His intent is to show that people with Ph.Ds in physics can not know what they are talking about (besides Riofrio, I'm clearly another physics Ph.D. he has in mind). Problem is, Riofrio doesn't have a Ph.D., she is (or was) a student at San Francisco State University which doesn't have a Ph.D. program in physics (they do have a master's degree program). In his comment section he and his commenters speculate on which is Riofrio, the woman in the picture or the lizard, with Lubos explaining that he had thought of using a picture of her with a monkey, to better make the point.

The guy is just completely grotesque and subhuman, it's amazing that anyone takes him seriously.

49. **Ponderer of Things**
June 18, 2006

Have you no manners, Mr. Motl? Seriously, did your parents forget to teach you about not making despicable "jokes" at others' expense?

I hope you realize that making sexist comments (and then commenting on how "hillarious" similarly sexist comments of others are) puts your position on the whole Summers affair in quite a bit of context…

50. **not deleted by Lubos**
June 19, 2006

in order to be allowed to post comments on the Reference Frame, just write in a polite politically-correct way. For example, my hoax below even got a kind honest answer (in the post about Hawking and the John Paul II):

Dear Professor Motl, I recently read the enlightening books by Hawking, Greene and Randall. Do you think that the hidden reason behind these attacks against you string theorists is that String Theory is getting too close to understanding the origin of the Universe, revealing our role in the immanent?

51. **Chris Oakley**
June 19, 2006

Lubos,

I think that your red neck credentials are now sufficiently well established for you to apply to appear on Jerry Springer. Application form below.
— —

Last name: Motl

First name: (Tick appropriate box)
(_) Billy-Bob
(_) Billy-Joe
(_) Billy-Ray
(_) Billy-Sue
(_) Billy-Mae
(_) Billy-Jack

What does everyone call you?
(_) Booger
(_) Bubba
(_) Junior
(_) Sissy
(_) Other___________________

Age: ____ (if unsure, guess) _____ Not sure

Shoe Size: ____ Left ____ Right

Occupation: (Check appropriate box)
(_) Farmer
(_) Mechanic
(_) Hair Dresser
(_) Unemployed
(_) Dirty Politician
(_) Preacher

Spouse's Name: _________________________

2nd Spouse's Name: ______________________

3rd Spouse's Name: ______________________

Lover's Name: ___________________________

Relationship with spouse: (Check appropriate box)
(_) Sister
(_) Brother
(_) Aunt
(_) Uncle
(_) Cousin
(_) Mother
(_) Father
(_) Son
(_) Daughter
(_) Pet

Number of children living in household: _____

Number of children living in shed: ______

Number that are yours: ______

Mother's Name: _______________________(If not sure, leave blank)

Father's Name: _______________________(If not sure, leave blank)

Education: 1 2 3 4 (Circle highest grade completed)

Total number of vehicles you own: ___

Number of vehicles that still crank: ___

Number of vehicles in front yard: ___

Number of vehicles in back yard: ___

Number of vehicles on cement blocks: ___

Firearms you own and where you keep them:
____ truck
____ bedroom
____ bathroom
____ kitchen
____ shed

Model and year of your pickup: 196_

Do you have a gun rack?
If no, please explain:

Newspapers/magazines you subscribe to:
(_) The National Enquirer
(_) The Globe
(_) TV Guide
(_) Soap Opera Digest
(_) Rifle and Shotgun

Number of times you've seen a UFO:_____

Number of times in the last 5 years you've seen Elvis:_____

Number of times you've seen Elvis in a UFO:_____

How often do you bathe:
(_) Weekly
(_) Monthly
(_) Not Applicable

Color of eyes: Right_____ left_____

Color of hair:
(_) Blond
(_) Black
(_) Red
(_) Brown
(_) White
(_) Clairol

Color of teeth:
(_) Yellow
(_) Brownish-Yellow
(_) Brown
(_) Black
(_) N/A

Brand of chewing tobacco you prefer:

(_) Red-Man

How far is your home from a paved road?
(_) 1 mile
(_) 2 miles
(_) Just a whoop-and-a-holler!
(_) road?

52. **wendell**
June 19, 2006

Peter Woit, you shouldn't call Lubos names like 'subhuman'. That just brings you down to his level. We all know he isn't subhuman and making unrealistic, extreme and exaggerated insults like that should be left for Lubos.
I agree that what he did to Riofrio was in bad taste.

53. **Peter Woit**
June 19, 2006

wendell,

I'm trying to ignore Lubos today and I suggest everyone else do the same. You're quite right that one shouldn't descend to his level. On the other hand, I think "subhuman" is a more accurate characterization of his recent behavior than "in bad taste".

54. **the true YAWN**
June 20, 2006

after answering to a few hoax like this one, Lubos deleted his whole post:
—
Lubos, maybe the problem with "Carroll joins Woit" is that feminists are not more intelligent than females.
—
It is not funny, but maybe it is educative

55. **Chuckles**
June 21, 2006

Mea Culpa: Since the comments were off on the last link about the book / Carroll, I took the liberty of posting the cache link in an off topic thread (LHC News). Guess I didnt scan long enough to find this section. Skeptical about the \"carrying on a discussion with Lubos\" part though. Is he tenured? I dont think he was tenured at the time of the Summers affair.

56. **YBM**
June 24, 2006

A funny side effect of this story, is that Lubos Motl who have (somewhat) supported once the Bogdanov tricksters (by the only reason that Peter Woit reacted to the dishonest way he was quoted in their book) is now quoted (in support of the authors) at almost every pages of the June 06 reprint of Igor & Grichka Bogdanov "Avant le Big Bang", a cranky book where it is explained how wrong string theory is, and how it could be replaced by a non-existent theory, based on wrong statements, which suggest a mystic and mathematical origin of the Universe.

57. **YBM**
June 24, 2006

Incidentaly, I just got my first censored comment on Motl's blog :

" You are involved by being quoted in the new version of their book, period. You are now involved with real cranks, what is quite funny given how you try to qualify as 'cranks' anyone, like Woit, expresses different views as yours.

I wonder where you've got that I could "deduce anything about string theory from this"… given I've never write anything like this.

You seem more and more unable to read honestly any paper, just like you did by linking Carroll to Woit.

Who's insane ? "

Given that I was responding to a post from him qualifying me of "insane", I can now deduce to what kind of guy I was dealing with…

58. **YBM**
June 24, 2006

hum, looks like Motl's blog is not running so well, posts are popping in and out at every reload…

59. **woit**
June 24, 2006

YBM,

Lubos clearly has been driven completely over the edge recently, maybe the appearance of my and Lee Smolin's books and various articles in the press about the problems of string theory have something to do with this. He's never had much interest in facts or logic, but recently he has become completely delusional. I hope his colleagues are trying to get him help, but if you believe what he has to say, they seem to just be encouraging him in his madness.

60. **YBM**
June 24, 2006

It is indeed quite strange he did wrote this "The book is also full of inconsistencies. In one chapter, he argues that the alternatives to string theory in the field of quantum gravity should be supported. In the following chapter, he argues that they should be suppressed – the work of the Bogdanoff brothers is one of his examples.", so he is now comparing Quantum Gravity side by side with Bogdanov's work, even if he did wrote elsewhere how poor their work is. He seem being unable to grasp that you could consider the Bogdanov brother as crank (what they are) on the ground of their behaviour and poor work even if they happen to attack string theory.

The (not so) funny consequence is that now Motl will be known in France as the only (apparent) academic support of a cranky book which explain how string theory is a complete failure !

61. **Peter Woit**
June 24, 2006

YBM,

That is kind of funny. I think the only reason Lubos is supporting the Bogdanovs is that I described them as cranks. Maybe I'll try to come up with ideas of who else I think is a crank that I would like Lubos to support, then write something attacking them.

62. **YBM**
June 24, 2006

It's more and more obvious every day that it is the reason of his support.

BTW, yet another censored post of mine (after he reinserted my previous one) :

" No surprise you've find my name linked almost exclusively to this
affair on Google, given that the Bogdanovs and their fans (none of them being able to deal with the scientific issues I've raised) managed to turn the debate into personal attacks from the first (of four) article I wrote on this subject.
I hope you'll never have to deal with fans totally uninterested by science when writing about any scientific issue. If you could read french, you'd notice that the defaming accusation you've found out came from a fan and that she was unable to provide a single sample.

I usually don't put my name on front when posting or publishing on the net, you are confusing Google with life.

For instance, typing your name on Google will make appear almost exclusively rants against Peter Woit (such as your comment on amazon.com), I wouldn't think nevertheless that you spend your life turning around Peter Woit.

On one hand you are qualifying Peter Woit as a crank when he is attacking string theory, on an other hand you are now known in France as the only apparent academic support of the Bogdanovs in France, as stated in a book who qualify string theory as a complete failure.

Perhaps it will make you think about the proverb "the enemies of my enemy are my friends", and the way it doesn't match well.

P.S. Your posting system doesn't work well, first some posts disappeared, then appeared twice. "

This will be my last censored post there. I won't post anymore on such a dishonest blog.

63. **YBM**
June 24, 2006

Given how you've been misquoted in the first print of their book, you could be amused to see how Igor & Grichka Bogdanov translated this sentence from Lubos :

"[…] the Bogdanoff brothers are proposing something that has, speculatively, the potential to be an alternative story about quantum gravity."

Here is what he supposed to have written, as quoted in a footnote of the new print of "Avant le Big Bang" :

"the Bogdanoff brothers are proposing something that has, speculatively, the potential to be an alternative to quantum theory".

suprisingly enough (not !), the link the brothers provide in the footnote to Lubos' blog is incorrect.

64. **Chris Oakley**
June 24, 2006

Don't the Bogdanovs have a popular science TV show in France? Maybe if things don't work out in academia – or even if they do – Lubos could appear on their show. He could even have a regular "Crank of the Week" slot, where he denounces a new person each week, obviously being careful to steer clear of the brothers themselves in this regard. In time, I am sure that being a Bogdanov/Motl Crank of the Week will be considered the highest academic honour short of a Nobel Prize.

65. **Billy Bob**
June 24, 2006

> I think the only reason Lubos is supporting the Bogdanovs is that I described them as cranks. Maybe I'll try to come up with ideas of who else I think is a crank that I would like Lubos to support, then write something attacking them.

This is a wonderful idea. I vote for John T. Nordberg (who looks like Lubos!) with Archimedes Plutonium coming in a close second, and deserving extra marks for style.

66. **Peter Woit**
June 24, 2006

YBM,

I haven't seen a copy of either the first edition of "Avant le Big Bang", or the new paperback. Someone (was it you?) sent me copies of the pages of the first edition that misquoted me. Do you know if the misquotes attributed to me have been removed in the paperback? Also, do you know what happened with the brother's lawsuit against someone who had written an unflattering magazine article about them?

67. **Chris Oakley**
June 24, 2006

Here's another one for you to attack, Peter: the Flat Earth Society

68. **YBM**
June 24, 2006

They are now quoting you correctly, at least on the quote I checked (they are no more pretending you would have write "absolutely sure" when you wrote "certainly possible"). BTW, I'm not the one who send you copies of these pages (it could be Fabien Besnard).

I will ask about this lawsuit and will keep you informed.

69. **runge_kutta**
June 25, 2006

I've just been reading John T. Nordberg's website. Wow, this guy is a crackpot extraordinaire. Does anyone know what university he's affiliated with (if at all he's affiliated with any) ?

70. **woit**
June 25, 2006

Please, don't start discussing more crackpots here, there's a very large number of them, and what they really want is attention.

Back on-topic, Lubos seems to have just deleted a link to a report on the public talks at Strings 2006, once someone pointed out to him that the report said the talks were boring and most of the audience went to sleep (except for Hawking's talk). Anyone have a record of that deleted link?

71. **YBM**
June 25, 2006

Well, I'm now banned from Motl's blog entry comments page (at least, I won't see anymore my post *modified* as the previous one was).

What's interesting anyway, is how he reacted to Bogdanov's creative quotes :

" Dear YBM, thanks: that's not a terribly accurate translation , but as I indicated many times, I am not gonna sue them.

Very speculatively, they might also have an alternative to quantum theory or anything else. "

Basically he don't care being notorious as a support to crackpots he clearly recognizes as such.

Here what is I could have answered to this post comming after one of mine he had the dishonesty to edit, just after having deleted the one I responded to his rant.

"To sue is a typical bogdanovian activity, as well avoiding scientific issues.

You could now realize what kind of people you've been dealing with (even if without consent).

Did they ask you for any kind of authorization ? Do you know that there is an excerpt from you (at least this one correctly translated) on the cover of their book ?

Forget Woit (who has real arguments), you are now the only proeminent apparent support of the most insane anti-ST ever published in France ! Félicitations !"

72. **Peter Woit**
July 5, 2006

Lubos,

Who told you that I "made amazon.co.uk erase all reviews" except the 5 star ones? Your review was complete nonsense, but the only action I took about this was that when I first read it a month ago I clicked on the "report this as inappropriate" link at the bottom. That's it, I have done absolutely nothing else.

As far as I know, no one else has written a negative review of the book on Amazon, and your crazy one caused several people to write very positive reviews answering it.

I have no idea why Amazon UK recently deleted yours. Maybe lots of people hit the "report as inappropriate" link, maybe someone there read it and recognized it for what it is.

73. **X**
July 6, 2006

dear Lubos,

I agree that your review on Amazon of the book by Peter Woit was deleted by some conspiracy, but you are wrong in writing that it is a crackpot-capitalistic conspiracy. There is a more plausible interpretation.
A recent string-theory paper (hep-ph/0607029) revealed the existence of intelligent civilizations with extended life expectancy in the 10th superstring dimension. Clearly, these aliens do not want to be discovered by us and are trying to prevent further progress in String Theory by deleting your review.

The bastards might even try to delete my comment from The Reference Frame.

74. **Motley**
July 10, 2006

Soon to be deleted:

I came to this blog as a young physicist seeking information in cutting edge physics research, and through sheer luck on my first visit I found it.
Now all I find here is the unreasoned rants and 'reactionary' insults of a man who cannot see past his faith in a theory that has much to live up to.
I have no doubt that this comment will be deleted, but hope that perhaps you will come to grasp just how damaging your behaviour is, to the image of string theorists and of physicists in general.

75. **Mahndisa**
July 31, 2006

07 31 06

Hello Peter:
I recently found your site. I was quite naive to all of the insult tossing that goes on in the physics world. I visit Christine Dantas's blog for some LQG links, then Lubos's blog for some deconstruction. It appears as though I must regularly visit your site for further deconstruction. The diversity in opinions is what I love about science. And in the end, only time will tell who was right eh? 🙂 Please have a nice day:)

76. **M**
September 8, 2006

dear Rae Ann,
Lubos forgot to ban me, so if you like I could re-post my deleted comment that solved the mistery about what M of M-theory really means.

However, censorship made the comments section of the "Reference Frame" somewhat uninteresting, so I prefer to refer you to page 196 of "Not Even Wrong".

77. **Eric Dennis**
September 25, 2006

Lubos, As usual there is much naive garbage and unearned arrogance in what you say, but I will have to be selective.

First, you don't understand Bell inequalities. The fact that a QFT Lagrangian (or Hamiltonian) is local, and hence that space-like commutators vanish, does not imply that the whole theory is local. If you look at spin operators measured on the two sides of a Bell experiment, you will notice that they commute as well. The whole point of Bell's analysis is that despite this commutation, entangled states still exhibit behavior that's profoundly non-local. The standard attempt to cover this up with references to information transfer and no-signalling is itself diversionary philosophical tripe.

Second, whether or not positions make a complete set of observables in (Relativistic)QFT is beside the point. The point is that position-space is inherently special in RQFT, and so criticizing a particular interpretation for recognizing this special status is hypocritical. (Note, I did not say the act of recognizing this special status and demanding a local Lagrangian is hypocritical, as per your straw man.)

Interestingly it is only in the kinds of interpretations of QM/QFT that you prefer that one requires a special philosophical superstructure for measurement theory. In fact the point of the very interpretations you criticize is to formulate QM in such a way that does away with the superstructure — in which measurment processes are normal dynamical processes just like any others, not requiring ad hoc postulates, or collapses, or Born rules.

On any of these subjects, I would consider receiving something other than an F from you a blemish on my credentials. And your girlish preambles about not countenancing any counter-arguments are transparent.

78. **anonymous**
March 11, 2007

censored from http://motls.blogspot.com/2007/03/panel-michael-duff-and-lee-smolin.html

dear Lubos, you (and Duff?) seem to think that the big problem of string theory is that Smolin might have 2 or 3 faces. The big problem is that string theory has 10^500 faces. Instead of arguing about Smolin, please present ideas about how to do physics despite the landscape; this is what will make the difference between success and failure

79. **a.n. onymous**
March 15, 2007

Just a reminder to everyone nerdy: Discover magazine's deadline for submissions of a 2-minute U-tube explanation of string theory is 16 March, so submit today.

Details: http://www.discover.com/twominutesorless/

"The winning video will be selected by Columbia University physicist Brian Greene, best-selling author of The Elegant Universe and The Fabric of the Cosmos, and broadcast via a prominent spot on the homepage of Discover.com … The video should present an accurate, basic understanding of string theory that will stick in the brains of relatively intelligent non-scientists."

Therefore, make sure you include a full proof of how gravity and the standard model are uniquely derived from 10/11 dimensional M-theory, proving how the 10 dimensional superstring universe is a brane on the surface of the 11 dimensional supergravity bulk. I don't know they require you to explain how the 10^500 solutions of string theory correspond to the multiverse.

The main thing to get across is that a 1-dimensional string, when moved, gains a time dimension so it has 2-dimensions (a worldsheet). Then you add another 8 dimensions to satisfy conformal symmetry if there is 1:1 boson:fermion supersymmetry, or 24 dimensions without supersymmetry (i.e., for boson string theory). This explains the reasoning behind 10 dimensional superstring and 26 dimensional bosonic string.

Next, because general relativity is only 3+1 dimensional, you need to roll up of 6 dimensions in superstring, which is done by the Calabi-Yau manifold which compactifies those unseen dimensions. The great benefit here is that the Calabi-Yau manifold can have all many kind of sizes and shapes for its dimensions, so the resulting little vibrating strings which constitute fundamental particles can have 10^500 sets of states or standard models, corresponding to 10^500 parallel universes. The anthropic principle will tell us that the particular universe we inhabit in this landscape of solutions is the one necessary for our existence. It's a very beautiful theory.

80. **ana nonymous**
March 15, 2007

Why do you tell "10^500 sets"? Use "discrete infrared ambiguities".

Why "anthropic principle"? "Structural principle" is more elegant.

So, replace the last sentences with:

Thanks to a rich set of discrete infrared ambiguities, string theory naturally implements the structural principle, providing the only known solution to a fundamental problem at the basis of our life: the smallness of the Cosmological Constant.

and Lubos will not have to censor it. For example I improved my previous post into:

dear Lubos, I understand how much you value intellectual honesty and freedom of speech, but is it worth for you (and Duff) spending so much of your valuable time fighting with crackpots?

and this one was not censored. It's just a matter of good taste.

81. **Censored from Jacques Distler's Musings**
March 16, 2007

I know this is a little off topic, but here's a comment seeking refuge after Jacques Distler deleted it from a Musings post where he suggested that the landscape just isn't a special problem in string theory (because it is possible to create messy landscapes with fictitious, non-empirical assumptions from the framework of the Standard Model and general relativity):

http://golem.ph.utexas.edu/~distler/blog/archives/001200.html

Re: The Standard Model Landscape

Thanks for the link to http://arxiv.org/abs/hep-th/0703067 which mathematically is straightforward for a change. The physical basis, however, is abstruse.

Any idea that particle physics (the standard model) and general relativity have a landscape of solutions is a reversal of the idea that such a theory is defined as representing experimental data.

The only way you can have a landscape for the Standard Model and general relativity is to change this definition, so as to include unobserved, unreal solutions. I.e. you can claim that the basic field equations have lots of solutions if the parameters can vary to unphysical (non observed) values. Thus, general relativity would predict a closed universe if the CC was small and the density was very high. By changing the parameters in the Standard Model, you can also get a landscape of unphysical solutions to particle physics.

But in the case of general relativity and the Standard Model, this landscape literally is not a real problem. This isn't just because measurements and experiments determine the necessary values which go straight into the theory, but it is also because the physical theories are really based on empirical data: the Standard Model and general relativity are derived from empirical data, e.g. symmetries in particle properties, spacetime, gravitation, energy conservation for a gravitational field, etc.

Comparing the unphysical landscape from empirically developed theory to that from string theory (where the spin-2 gravitons, extra-dimensions, and 10^16 GeV unification are not observations), misses Woit's point that in one case the landscape is neither physically real nor a problem, while in the other case the landscape is a real problem.

Nobody has proved that any landscape really exists in nature. The assumption that there might be other universes with different values of standard model parameters is just speculation. If it turns out that those parameters are interdependent and so can't vary in the way assumed for landscape analysis (even if the multiverse is accepted), then this would eliminate or reduce the landscape size.

The objective of physics for some people like Feynman was to find the reasons for why general relativity and the standard model have the form they do. If this quest is successful without string theory, and all the constants and parameters are predicted to have unique values from a theory that doesn't allow other values, then there will be no landscape whatsoever.

82. **Arun**
March 16, 2007

Censored from Jacques Distler's Musings Says – your eminently sensible posting was censored? It is to the point, pertinent, polite — I'm really amazed! Thanks for letting us know that things have become so bad.

83. **TCO**
March 19, 2007

I'm banned from the Reference Frame. I only wanted to disaggregate an issue (do issue analysis) in a discussion of global warming. Lubos managed to muddle two effects (becoming like Venus and posited temp increase for CO2 doubling) as well as two sources of information (ice cores and Venus temp/CO2 levels). He also does not understand how to bound a problem and when he is doing it and when he is not doing it. I really don't get the impression of an honest, curious scientist.

84. **amused**
March 20, 2007

"I really don't get the impression of an honest, curious scientist."

No kidding 😉 Sometimes it works to tell Lubos to do the opposite of what you want him to do. E.g.:
"Go ahead and block me Lubos, there are plenty of other internet cafes in the city where i live"
or
"Lubos, please hurry up and delete this comment. I want an excuse to repost it over at Peter Woits blog where it will be seen by lots more people."
Both of these worked for me in the past.

85. **M**
March 21, 2007

hi, amused. Here is an example of what happens with your strategy. I posted:

dear Lubos, do you agree that your slogan "String theory is the language in which God wrote the Universe" must be updated into "String theory is the language in which God wrote the Multiverse"?

If you answer no, you are defending something that does not exist: there is only one string theory, and it has a landscape of 10^500 solutions.

If you answer yes, you are ready to start arguing with Woit.

If you censor, this question will be moved to the "censored by Lubos" section of Not Even Wrong.

This was not censored, and got a thoughtful answer:

…you are a mentally retarded imbecile…

But my later reply was censored:

dear Lord Motl, if it'so easy for you, why don't you show us the solution to the 10^500 problem, and I will be pleased to hear your insults at your Nobel prize conference?

86. **amused**
March 21, 2007

Hi M,
Thoughtful replies of that nature are all you will ever get from Lubos with that type of question. For what it's worth, here are a few recommendations for interacting with him. Serious discussion is not possible, so think of it as a sport – motlbaiting. There are two possible goals. The first is to wind him up so much that he has to write a whole new blog post full of abuse to recover from it. Your comment will of course be deleted, but nevermind, you can just repost it here. Physics points should be avoided for this; snide remarks containing unfavorable comparison of his publication record to those of various "crackpots" are much better. The alternative goal is to try to slip in as much string-bashing as you can while taking a joking around approach, and avoid getting deleted or blocked. It's for this that the suggestions in my last comment apply.

87. **gunpowder&noodles**
March 21, 2007

"snide remarks containing unfavorable comparison of his publication record to those of various "crackpots" are much better"

LM actually posted Smolin's publication record as a way of proving that he, Smolin, is a failure. Smolin's record, in reality, is extremely good, far better than I would have expected actually; and of course it is literally an order of magnitude better than LM's. The fact that he can't see that most readers would think this is a strong hint that he is really cracking up.

88. **TCO**
March 21, 2007

I'm really getting dissapointed with the honesty and smartness level of conservatives lately. Am I the only smart, honest one?

89. **TCO**
March 24, 2007

You know, I lack the math or physics ability to understand the arguments, pro/con with string theory. I do find your manner to be much more pleasant than Mr. Motl. What's ironic is that he defends the "skeptics" of global warming, but he disdains those in his own field. A real scientist should be curious of all. Skeptical of all. And able to formulate the key questions to resolve what is trustworthy.

I've also never been able to really have a conversation that got into thoughtful issue analysis with him. Maybe part of it is my hesitancy, out of respect for superior math ability. And my tendancy to play game to cover that. But I think a part is Motl, not really being intellectually curious. Not being an issue analyzer. A disaggregator of causes and effects.

90. **Kea**
April 12, 2007

13 April 07: I just invited Lubos to join our discussion on his 2002 Tripled Pauli Statistics idea, which appears in our version of M theory. I'm not sure whether he will look upon the invitation kindly or not.

91. **amused**
September 6, 2007

Rudely deleted by Lubos from the thread of "Nobel Prize winners vs crackpots":

Hi Lubos,
Considering that you are very far from being a leader in string research, why on earth did PI want to give you a job? Is it because of your charming personality?;)

Btw, congrads to the string theorists on having recruited a new cheerleader among the science journalists. It's fortunate for you guys that Chalmers doesn't have a background in particle theory research, so that he is able to swallow the string hype whole without getting indigestion!

92. **amused**
September 16, 2007

Lubos is full of fun and games these days, and rather than just deleting comments he's taken to "improving" them… For example, in the thread of this post, where Lubos lists himself along with Witten, Maldecena, 't Hooft and Einstein as reknowned physicists, I added a little comment
"Gene, you forgot to mention Lubos' awe-inspiring publication record 😉 "

(In response to a comment by one of LM's groupies chastising those who dare to doubt his guru's towering intellect, since LM had `read Dirac's book when he was only in high school' (Wow, who needs PRL publications with that kind of achievement under their belt!)) Well, Lubos decided to "improve" that comment into something quite different… In fact my own views are more along the lines of this, which I like to think might have responsible for the amusing rant ("omnipresent intellectual trash…") at the end of this post 🙂
And Lubos was so inconsiderate as to block me, which means I'll have to walk an extra block to the next internet cafe next time ;-(
Btw Lubos, here's something else I've been meaning to ask you: Is the SLAC faculty member who wrote this post also a crackpot?

More Landscape, and Peer Review

June 16, 2006
Categories: Uncategorized

The anthropic string theory landscape seems to be having ever greater success in taking over fundamental physics and turning it into pseudo-science. It's being promoted by no less than 2008 presidential candidate Wesley Clark, the following is from a transcript of his remarks to science bloggers at a blogger convention in Las Vegas:

Read Leonard Suskind's new book, called "The Cosmic-" It's called "The Cosmic Landscape And Intelligent Design" if you want to see something that's overpowering. Suskind is the inventor of cosmic string theory, and what he does is he takes cosmic- he takes the idea of the universe. He says the universe is- see, what's happening in intelligent design is people are saying, 'Ah well, you see, the, the, the wavelength of, of, of the electron and Planck's Constant and all these numbers are so odd. They don't- they're not even numbers, you know. They, they, they don't balance each other. It's sort of 1.- It's like the figure of pi, 3.14159… Why would it be such an odd number? Why, why wouldn't god make the universe, you know, symmetrical?'

(laughter)

Then they said, 'well, because, you know, it's like there's only one on 10 to the 50th chance that the universe could have worked out in a way that mankind could survive. Therefore, you know, this must have been an intelligent designer who created this universe especially for us.' What Suskind does is he turns it on its head. He says, "You know, if you look at string theory and the 9+1 dimensions" or 10+1 dimensions, and I'm not sure how he knows that time only has one dimension, but he does. (inaudible) would say I'm very arrogant for questions questioning this.

(laughter)

But what Suskind does is he turns it upside down. He says, "Look there are- there is an infinite number of universes." He calls it a multiverse, and he says that however the motive forces, and nobody understands why quarks pop in and out of existence. Nobody understands it, but apparently they do. And apparently there are many, many universes, and we're here in this one. And maybe there are others in which Planck's Constant has a different number, in which the speed of light is not 186,200 miles per second. Who knows? We don't know.

Commenter Patrick wrote into point out a review article on this from graduate student Joseph Conlon, published in the latest issue of Contemporary Physics (not available on the arXiv or anywhere else for free as far as I can tell). It's entitled, "The string theory landscape: a tale of two hydras", with the first hydra the non-renormalizability of gravity (supposedly slain by string theory), the second the proliferation of vacuum states. Conlon seems to think that the fact that string theory can't ever be used to predict anything is not a serious problem:

We started with a dream of a unique string theory compactification reproducing the structure of the Standard Model. This is a dream apparently shattered by the existence of the landscape. Granting the landscape and its existence, does this mean string theory is inherently unpredictive at low energies? If this is true, this is sad but no disaster. Quantum field theory, of itself, is also unpredictive.

I've written elsewhere about why this analogy with QFT doesn't hold, but on the face of it there's obviously something wrong, since we use QFT all the time to make detailed, testable predictions about the real world, something that string theory, according to Conlon, will never be able to do.

Talks from the plenary section on "naturalness" at SUSY 06 are online. The usual advertising job from Susskind and Linde, the one that seems to have impressed Wesley Clark. Wilczek gives a more substantive talk, and seems to have some interesting new speculative ideas about models near the end.

On another topic, I've been wondering what the current state of peer-review of hep-th papers is. Personally I think it has been several years since I've looked at any of the main journals that publish papers in this area, and I suspect this is true of many people these days. The Bogdanov affair several years ago showed that refereeing in this area had become pretty much a joke, with the brothers having no trouble finding five journals willing to accept utter nonsense.

Looking at the arXiv and SPIRES listings, which seem to contain publication information after submitted papers have been accepted, many papers (e.g. Susskind's single-authored papers on the landscape), don't seem to ever have been peer-reviewed and published. I'm curious what people think of this. How many hep-th authors have stopped submitting their papers for refereeing? Is the data on the arXiv and SPIRES an accurate reflection of this? Does the fact that an author's preprints don't have publication data for the last few years mean they weren't submitted for refereeing, or could this be due to time lag in refereeing/publication, or incompleteness of the data?

Update: Courtesy of Google, there's now an on-line talk by Washington Taylor promoting the Landscape to people working for the company. He gives the number of vacua as at least 10^{1000}. The number of well-known physicists out there promoting this nonsense to the general public is amazing (via Lubos).

Comments

1. **Warren**
   June 16, 2006

   I haven't submitted my single-authored papers (or books) to publishers in 10 years. But most of my papers are with others, and I haven't forced them to do the same. I don't have much faith in the refereeing system. Besides, by the time they publish something, it's history. And then there's the problem of errata.

2. **hack**
   June 16, 2006

   Susskind has no peers, therefore it is impossible for him to be peer reviewed.

3. **Kasper Olsen**
   June 16, 2006

   There is actually quite a few famous papers which have never been published, for example

   hep-th/9805114,
   hep-th/9907189,
   hep-th/0006071

   and

   hep-th/9701025

😉

Kasper

4. **sunderpeeche**
June 16, 2006

a) Bloggers have conferences?
b) Why would Wesley Clark even read the book? Did Susskind give him a copy?
c) The whole attitude to the landscape is wrong. First, you need an acronym. “Anthropic string theory landscape” is a non-starter. Landscape (of) anthropic string theory = LAST ~ Lubos Adores String Theory. Now we’re getting somewhere. Next print some t-shirts and make some money off of ST, instead of railing against it.

5. **Christine Dantas**
June 16, 2006

In the astrophysics community, it is somewhat rare to see a paper in the astro-ph that has not been at least submitted to a refereed journal. Of course the refereeing system is faulty sometimes and needs some reformulation. (For instance, it would be interesting that the authors’ names be omitted for review in order to reduce some prejudice on the part of some referees and to allow them to unbiasedly focus on the paper contents. There are other problems as well, I could talk about a few examples, but I do not think Peter Woit intended to open a whole thread on this.)

I do find it strange that the majority of papers in the hep-th (is it correct, the majority?), including those considered important contributions, are not submitted for refereeing. Being myself used to post on the arXiv only after having my papers accepted to a good refereed journal, maybe this is just my personal impressions. In any case, I have learned this good practice from my supervisor since I was an undergraduate student, and I am quite satisfied with it.

The arXiv is a great idea, but I do not think it is a substitute for the refereeing system (despite its flaws), specially if your work is supposed to represent a significant contribution to the field. It must somehow be appropriately reviewed.

6. **Brett**
June 16, 2006

I think that peer review works pretty well, at least for the better journals. In high-energy theory, this means Physical Review Letters, Physical Review D, Physics Letters B, Nuclear Physics B, and some others. It is very difficult to get something that is pure rubbish published in those journals. By “rubbish,” I mean a paper that is flawed in a fundamental way–a paper will problems in its conceptual or mathematical underpinnings. (How interesting physically some of the results published in these journals are is a separate question.) The fact that almost all the high-energy theory papers submitted to these journals appear in advance on the arXiv is good for the peer review process, since referees can (surreptitiously–or perhaps not) get input from their colleagues about papers they are reviewing.

Most people I have interacted with in the high-energy physics community certainly do pay attention to whether and how papers are published. Publication in the journals I listed above means something; however, publication in many lesser journals does not. It is understood that peer review is significantly imperfect, and even extremely poor papers can be slipped into minor journals if the authors are diligent. I think many serious physicsts would never even submit a paper to one of these publications; if they can’t get papers published in top-tier journals, they just leave those papers on the arXiv and don’t bother trying to get them published somewhere else. Of course, there are journals that occupy a sort of middle ground and are somewhat respected. The Journal of Mathematical Physics is still considered to have fairly high standards for correctness, but the papers published there are usually expected to be less interesting than those published in more prominent journals.

Peer review for the better journals can be uneven in quality. Good papers get held up by difficult referees, while somewhat weaker (still correct, just less interesting) papers may slip by with little comment. However, while there is some noise, there is also a clear correlation between the quality of a paper and how likely it is to be rejected. When I have gotten negative referee reports, I have always (with one exception) understood the referee’s point. Obviously, I disagree with the referees, but these rejections are not being made for stupid reasons; the reviewers have put time and effort into evaluating my papers. I have also appreciated the many comments and suggestions for minor improvement that I have received in more positive reports–again, clear indications that the referees are almost always paying attention.

Speaking as a referee, however, I know it can be difficult to reject a paper that has some interesting ideas, but which also contains some serious mistakes. I have never allowed a paper like that to be published, but one cannot help but feel empathy for authors who may have put months of work into something (and come up with several innovating ideas), only to be undone by a small but crucial mistake they made early on.

7. **knotted**
June 16, 2006

I agree authors names shouldn’t be passed on to reviewers, but neither should the laboratory name unless it is an experimentally based paper. Peer review is only meaningful is the reviewer has to review the content. Usually it is the exact opposite, with journals requesting names of peers to sent the paper to review to. This is asking for bias. It makes it easy for mainstream and virtually impossible for outsiders. arxiv could easily be reviewed by blog trackbacks, if someone would be objective about it…

8. **The Great Inquisitor**
June 16, 2006

The social dynamics of the string community is that of a sectarian totalitarian system, hiding from the rules of scientific objectivity, due to the absence of both experimental provability and disprovability.

The string theory community is a personality cult with Witten its high priest. Once Witten retires, chances are high everything will pop like a soap bubble (or like the high tech stock market crash not long ago), leaving the reputation and credibility of theoretical physics in pieces for decades to come.

Witten creates credibility among the mathematicians, by his ingenious works in geometry and topology that have nothing to do with string “physics”. On the other hand, he has surrounded himself with an army of loyal, brainwashed followers who dress like him, who talk like him, who write preprints in same style format like him. The string community is like a mass of half-baked Witten clones, with some members more prone to hysteria than others. One has to study mass cults in order to understand the personality defects that motivate people to join such movements (presumably, weak self-respect, paired with a will to power, plays a role).

One should read the episode “The Grand Inquisitor” in Dostoevsky’s “Brothers Karamasov”: The devil tempts Jesus in the desert, and explains to him that humankind will follow anyone who provides them with bread and miracles; therefore, an overwhelming demonstration of the power to support people with food, and with miracles, would convince humankind to forfeit its free will, and to follow that person wherever he/she pleases to go. But Jesus refutes this methodology; he wants people to follow him out of free will and belief. Witten makes a different choice than Jesus in Dostoevsky’s piece. He provides his followers with jobs and miracles, and they literally follow him to the end of the world of theoretical physics.

Fortunately for humankind (or at least for future generations of theoretical physicists), there is no successor to Witten. Therefore, there is reason for the joyful hope that the string hysteria will evaporate in a couple of decades.

On the other hand, one has to admit that string theory has produced amazingly interesting mathematical insights. If things go well, some mature parts of string theory will be absorbed into a branch of topology or number theory, which is ok and worthwhile. I just hope that string theory will disappear from the physics landscape, not because I particularly resent it, but because I'm scared by the populist, totalitarian, sectarian social dynamics of the strings community.

And if some string theorists insist on the eternal lifespan of their favorite theory, I would like to remind them of the fate of communism, and of that of other totalitarian systems, which were defended as vigorously by their proponents some decades ago.

9. **Eugene Stefanovich**
June 16, 2006

Brett,

I agree that the review process is useful when you can talk to the referee, argue, and get feedback. Now, how would you argue with the Editor of one of the fine journals you mentioned when in response to my submission he writes this?

In general, [journal] does not publish purely formal developments of old and well-established theories or alternatives to old and well-established theories if the new alternatives do not make different predictions that can be experimentally verified; if the new alternatives do make different predictions, it must be shown that the predictions are consistent with the present experimental situation. Applied to your manuscript, this policy would require that you provide an explicit, detailed, and quantitative prediction of your theory that differs from the predictions of standard quantum electrodynamics. Since you have not done so, I am afraid that we cannot accept your manuscript for publication in [journal].

After I explained the relevance of my work to current and future experiments, I got

I am afraid that, even after considering the points that you make, I still conclude that your manuscript is not suitable for publication in [journal].

End of story. How can I argue with that?

Eventually, this paper was published as

E. V. Stefanovich, "Quantum field
theory without infinities", Ann. Phys. (NY)
292 (2001), 139

(there is also a copy on the web if you are interested) So, you can judge for yourself whether it has anything new to say about QED and its agreement with experiment.

10. **Peter Woit**
June 16, 2006

Eugene and others,

I'd rather not start a discussion here of refereeing itself, it's a huge and complicated subject. What I'm trying to understand is how widespread is the phenomenon of people giving up on the journals and refereeing system, just ignoring them. I see more and more of what looks like evidence of this happening, and Warren provides another data point. Given the fact that virtually no one looks at most journals anymore, this may be an increasing trend.

11. **Peter Shor**
June 16, 2006

One comment: I don't think absense of any journal reference on the arXiv means anything about publication … it means the author was too lazy to update the arXiv. You can often find journal versions of quant-ph articles by googling, even when there is no pointer on the arXiv. It would be interesting looking at a sample of hep-th articles from several years ago, and see how many are still unpublished.

12. **Peter Woit**
June 16, 2006

Peter,
I was looking more at SPIRES, which I believe they update automatically with journal information as the journals come out. This isn't up to the author. But if anyone knows differently, I'd be curious to hear about it.

One set of examples of papers that don't appear to have been submitted to journals are Susskind's, e.g.

hep-th/0302219 (the original anthropic landscape one with 257 citations)
hep-th/0407266
gr-qc/0503097
gr-qc/0504039
hep-th/0101029
hep-th/0011164

Another example would be Jacques Distler, who doesn't seem to have any published papers since one he wrote back in 2001.

If anyone knows of how to find out if and where these papers were published, let me know.

13. **Bert Schroer**
June 16, 2006

The Great Inquisitor
A perfect analysis of the sociology underlying string theory! It is very unfortunate that in order to write something like this one has to use a pseudonym (at least before retirement), whereas the Lord of misuse (comissioned by the hegemonic string court to prolong the lifetime of string theory beyond the lifetime of their protagonists) can spread his vitriolic brew to confuse young physicists and frighten more knowlegeable and mature members of the community.

14. **Joe Conlon**
June 16, 2006

Ha! I was wondering Peter when you would run across that. I have some vague vision of long Woitian tentacles spreading across the web in search of anything landscape, and I'm sure they don't miss much.

Let me elaborate a bit on what I meant. Almost by definition, it is very hard to find exclusively stringy predictions at low-energy that cannot be reproduced by effective field theory. If you are willing to go to Planckian energies, then we can run with exponentially soft scattering amplitudes and towers of excited stringy states, but there are no Planck-scale accelerators and this is somewhat of a toy game.

However, all effective field theories are not equal. There is clearly lots of structure in the Standard Model that is bursting for an explanation. One example: the QCD theta angle. Your underlying theory has a big role to play in the models you use to try and explain and understand the Standard Model. There are better and worse ideas on what are the underlying principles – e.g. base 10 numerology is mostly held to be an unpromising idea. In this sense I regard string theory as the best organising principle for thinking about models or effective field theories explaining the structure of the SM.

So, at low energies, I see `string theory' as conceptually analogous to `gauge symmetry', `spontaneous symmetry breaking', `quantum field theory', etc. It is an organising set of ideas and assumptions that is not *in itself* predictive but sets the framework to build predictive models. Clearly `models inspired by string theory' do not compare experimentally with `models inspired by gauge symmetry', but that's why the one is research and the other taught in undergraduate courses. String theory does differ in that it has wonderful UV properties and is intrinsically predictive in that regime, but I'm not holding my breath for the Trans-Galatic Super-Duper Quasar Collider.

If you can argue technically that you can get any effective field theory out of string theory this may not apply – but this is certainly false for the IIB flux vacua that provoke all the chatter about 10^500 and so on, as these have rather similar properties.

I also note that on a personal level I am far more interested in the correctness of My Models than in whether My Models are a unique low-energy prediction of string theory.
I see some of these landscape discussions as more sociological than anything. Maybe in 1985 people thought string theory was about to explain everything in two weeks. I don't know, I wasn't around then. I don't think people enter the field now with that illusion. The theory is still vastly more capable of talking to the particle physics-GR-cosmology triangle than anything else. It's not a binary distinction between a theory that predicts everything and a theory that is entirely useless.

Best wishes
Joe

15. **Brett**
June 16, 2006

I realize that this is off topic, but I was asked a direct question, and I want to respond.

Eugene-

I too have had a paper initially rejected because it did not state any specfic predictions that the referees deemed sufficiently important. My response was to add futher numerical calculations detailing the non-standard model behavior. You comments seem to imply that you did not make any changes to the manuscript after receiving the rejection, but merely tried to convince the referee of your paper's importance. However, I think the referee initially had a quite valid point, as I did not see any new predictions in your paper, and so I am not surprised that he did not change his mind.

16. **Eugene Stefanovich**
June 16, 2006

Peter,

I think there are rational explanations of why people may prefer arXiv publication to refereed journals. I suspect that Susskind does not care anymore about his publication record, so why bother to submit papers to journals? Others may have less impact papers that simply add finer details to the points made in their previous journal articles. So, they decide not to go for the full-blown paper. I have a couple of those in the arXiv. Yet others (mostly those who are out of the mainstream) may give up after referee's or editor's rejection.

Brett,

if I may, just a few points. First, the person who wrote this was not a referee, but the journal editor. You can have a discussion with the referee, but if the editor rejects your manuscript, you are done.

Second, if you read the paper more carefully you'll see that it opens up a whole new *class* of experimental predictions which go beyond the S-matrix and allow one to calculate the time evolution of interacting systems. It is another matter that such a time evolution is beyond the resolution of modern experiments. However, one doesn't need a "Trans-galactic Supercollider" to see it.

Third, I am wondering how many string theory papers satisfy the stringent criterion of providing an *explicit, detailed, and quantitative prediction* ?

17. **catherineD**
June 16, 2006

Aw, come on.

Clark planned to become a physicist back in high school, but now he's just a really smart guy who picked up a book to read for pleasure on topic he enjoys. Don't expect him to be up on what's going on.

The current guy in the White House can barely read. Here's a guy you could talk to and is open to learning more.

Appreciate it.

18. **woit**
June 16, 2006

catherineD,

Wasn't really complaining about Clark, just marveling at how far into the culture this whole landscape thing has gotten.

Joe,

Thanks a lot for writing, it's too late tonight, but I look forward to reading carefully what you have to say tomorrow.

19. **arnold**
June 16, 2006

I went through some talks of the SUSY conference.

The funniest one is Linde's slide that says about the anthropic principle:

"IT IS SCIENCE"

Every child knows that science is about making predictions and be tested by experiments….but these old physicists, that have no more ideas, want to convince us that science fiction is science.

Probably every other scientist outside theoretical physics (people who that are used to experimental verification!) would think these people are just crazy.

20. **arnold**
June 16, 2006

…and it is sad to see how throetical physics (that was once upon a time the mother of sciences) is leaving the objective scientific method to go into the world of the opinion, where some famous powerful person decides what is good and what is bad….and not experiments.

21. **MathPhys**
June 16, 2006

I just watched W Taylor's lecture on google. It's scary how intelligent people can give talks like that.

22. **anonymous**
June 17, 2006

dear Joe,

recently people liked to speculate about brane-worlds with large extra dimensions because this allows quantum gravity at the TeV scale: LHC would be your Trans-Galatic Super-Duper Quasar Collider. This scenario was motivated by string theory, and gave to string theorists an opportunity to show what they can really do. The resulting literature shows that all concrete work was done by phenomenologists who tried to apply Einstein general relativity by avoiding or parameterizing (and sometimes ignoring) UV divergences.

Even in the quantum gravity regime, string theory failed to give results. Some examples:
Q: What is the mass of the string states in units of the quantum gravity scale?
A: it depends on the dilaton vev.
Q: What is the tension of our brane?
A: It depends on how supersymmetries are broken.

23. **anonymous2**
June 17, 2006

Refereed journals are still relevant in fields that produce results relevant for different fields.

For example, hep-ph and astro-ph contain some papers that experimentalists consider relevant. But experimentalists often are not expert enough for judging themselves if such papers are correct, and therefore often adopt the following rule: only papers published on good refereed journals are ok.

24. **Eugene Stefanovich**
June 17, 2006

Peter,

thank you for the link to Washington Taylor's video. Believe it or not, I've never heard this full story from the mouth of real string practitioner. My opinion about the whole enterprise is this: childish logic + superiority complex + arrogance + fanatism. We have entered dark era, indeed.

25. **Joe Conlon**
June 17, 2006

Dear anonymous,

Of course I'd be very happy if TeV-scale Large Extra Dimensions were seen at the LHC. But there are lots of hints that the cutoff scale should be larger (axions, cosmic rays, proton decay, neutrino masses, GUTs…) and I for one would be surprised if they were found. Long odds, big payoff. I also don't think `string theorist' is an identikit. Some who work on string theory are algebraic geometers at heart, others only care about BSM phenomenology, and most are some way inbetween. It's a big community. If `string theorist' is only used to refer to the former, then of course such people never go near BSM exotica.

I'm not quite sure I understand your Q and As. Stringy states have masses given by the inverse string length, which is determined by the volume and dilaton. We don't know the string scale, but it's always less than the Planck scale. With the second, there are universal formulae for brane tensions which are in e.g. Polchinski. I also don't see how brane tension is an (easily) measurable quantity.

Best wishes
Joe

26. **island**
June 17, 2006

Wasn't really complaining about Clark…

I'm am… complaining about anybody that is that quick to stereotypically follow suit with the all-to-familiar mindset of a **pack** of anti-fanatics who choose to willfully ignore the strongest implications of empirical evidence by instead automatically reaching for the most extreme anticentrist cop-out on first principles in the history of science in order to counter-respond to fundamentalist abuses of the evidence.

Panel of extremists:
science panel w/gen. wesley clark, chris mooney, ***pz meyers*** *and darksyde*

Michael Moore has a better shot at 10^500:1

~

And thank you, Peter, for being clear:
The anthropic string theory landscape…

27. **The Great Inquisitor**
June 17, 2006

I haven't read Susskind's book, but the inclusion of "intelligent design" in its title demonstrates the cheap moral standards and ethical bancruptcy in his community. Although the content of the book may prove otherwise, his choice of a title shows that he intends to get customers from another sectarian group of extremists, namely the right-wing Christian right.

The present US government is the first in recent times not only to have understood the size and the power of the Christian right, but to also cast aside ethical concerns, and to use it to its advantage. This is clever, but of course also degrading and ethically reproachable. Maybe the strings people only see it as clever propaganda, and are planning to learn from it.

What distinguishes a typical member of the strings community from a traditional physicist or mathematician is surely not intelligence; there are some extremely smart ones among them. It is their failure to feel an obligation as a scientist to a scientifically verifiable truth. The problem is a lack of scientific ethics. They trust the visionary abilities of people like Witten more than scientific objectivity and scientific method. This is an extremely dangerous, and short-sighted road. Witten does possess self-restraint and good taste in mathematics, but most of his followers don't. They inadequately use mathematical language, which they only understand marginally, to try to impress all sorts of people, including Wesley Clark. The question is who they believe to do a service for, string theory, theoretical physics, or science ?

I am not concerned that after the Witten era, the strings movement will navigate itself into oblivion, due to the bad taste and lack of self-restraint of its members. However, their present propaganda is scientifically unethical, irresponsible, and dangerous.

28. **island**
June 17, 2006

Although the content of the book may prove otherwise, his choice of a title shows that he intends to get customers from another sectarian group of extremists, namely the right-wing Christian right.

Capitalizing on the popularity of the politics while blackmailing the string community.

> Amanda Gefter:
> If we do not accept the landscape idea are we stuck with intelligent design?
>
> Leonard Susskind:
> I doubt that physicists will see it that way. If, for some unforeseen reason, the landscape turns out to be inconsistent – maybe for mathematical reasons, or because it disagrees with observation – I am pretty sure that physicists will go on searching for natural explanations of the world. But I have to say that if that happens, as things stand now we will be in a very awkward position. Without any explanation of nature's fine-tunings we will be hard pressed to answer the ID critics.

29. **The Great Inquisitor**
June 17, 2006

Island,

What Susskind says here is that the only alternative to ID is strings and landscape. This is complete BS. The truth is that ID is based on an argument that is inacceptable to scientific methodology; it is not scientifically verifiable.

ID argues as follows: We have this creationist theory that explains the universe for those people who believe in it. Since it is not accessible to scientific disproof, it must be true.

String theory argues in a very similar way.

So it's in a sense true; string theory and ID are both belief systems, one mathematical, the other not, which are neither provable nor disprovable by scientific experiment. So their claim for truth is similarly vacuous.

But it should be emphasized that Susskind is extremely irresponsible in not putting it this way, but to claim that strings + landscape is defending physics against ID. This is not true. Strings is not traditional physics based on scientific methodology.

30. **anonymous**
June 17, 2006

dear Joe,

I fully agree with you that lots of hints disfavour TeV-scale quantum gravity is unplausible. Indeed, what keeps this possibility alive is the fact that we do not have a predictive theory of quantum gravity, so that we can make optimistic speculations.

Concerning my Q&A, the main issues that phenomenologists would like to know are: supposing that "quantum gravity" is what cut-offs quantum corrections to the Higgs mass, how much is the D-dimensional Planck scale? And the first string excitation? And the tension of our brane? 1 TeV? 5 TeV? 200 GeV? String theory does not give any useful answer.

Collisions of particles excite brane fluctuations giving missing energy signals that are detectably large if SM particles live on a brane with tension smaller than about a TeV. Formulae for brane tensions on Polchinski book apply when many supersymmetries are unbroken, not in our universe.

31. **hogy**
June 17, 2006

The actual name of the book is "The Cosmic Landscape: String Theory and the Illusion of Intelligent Design".

32. **hogy**
June 17, 2006

and Wesley Clark is just trying hard to make himself look cool to geeky voters.

33. **Peter Woit**
June 17, 2006

Joe,

The problem with your philosophy of string theory = generator of models is that it generates far too many of them, and the ones that look at all like the real world are ridiculously ugly and impossible to do precise calculations with. The theoretical framework of these models gives an infinite number of publishable research projects for people to work on, but zero reason to believe that this will ever lead anywhere, and no way of ever showing it is wrong. There is no criterion in this game for when to stop, give up and admit it doesn't work. Your only hope is for a miracle to happen and something orders of magnitude more successful (in agreeing with the SM, being computationally tractable, and constrained enough to allow predictions) to all of a sudden appear. Hoping for a miracle is not a valid scientific research program.

34. **Joe Conlon**
June 17, 2006

Hi Peter,

The judgement of whether research programs are promising or not is subjective. Both reasonable and unreasonable people disagree. On the issue of whether string-based models are promising things to work on, I happen to think you are dead wrong. Of course, it's your prerogative to disagree 🙂

Best wishes
Joe

35. **wolfgang**
June 17, 2006

>Hoping for a miracle is not a valid scientific research program.

What scientific research program would you propose?

36. **Arun**
June 17, 2006

"What scientific research program would you propose? "

– Presumably any where the following is false:
"There is no criterion in this game for when to stop, give up and admit it doesn't work. "

37. **wolfgang**
June 17, 2006

> Presumably any where the following is false:

Which is?
This is a serious question. If Peter or you suggest to give up on string theory, then it is a legitimate question to ask what better alternative you have.
What theory are you or Peter working on which will help us determine the value of the cosmological constant?

38. **Arun**
June 17, 2006

Wolfgang, you are posing a false question. One need not be working on some lesser nonsense in order to call the bluff of greater nonsense. We may simply have to say that there are no viable ideas today for the theoretical determination of the cosmological constant. It is better to face that truth than to have false hopes about some program. Activity for the sake of activity (what will we do otherwise) is pointless.

39. **Peter Woit**
June 17, 2006

Joe,
Reasonable people can disagree about prospects for these models, but I've been watching people work on them for almost 22 years, and they are further away now from being able to use them to predict anything than at any previous time. The derivative has the wrong sign.

Wolfgang,
Unlike many of my dear commenters I don't want to use this forum to endlessly promote my own favorite ideas. I did write a long paper about some of these, and hope to get some other things written down in the future. If you look at the non-string theory things I write about on this blog you can get an idea of what I find interesting. Obviously I don't know how to compute the CC, if I had to guess I'd guess that the answer to that problem will come after you solve some other problems. Just thinking about the CC is probably not going to get you anywhere.

But I think the whole field would be a lot healthier is people tried to come up with their own new ideas, whether about the CC or anything else. If one is not willing to try and do this, this isn't a good career choice and one should stop trying to be a particle theorist and find something else to do where one can make a contribution. Putting one's efforts into an idea which obviously can't work is just a waste of one's time and talents, there is no justification for doing it.

40. **wolfgang**
June 17, 2006

> We may simply have to say that there are no viable ideas today for the theoretical determination of the cosmological constant.

OK, but if you or Peter do not want to be active it does not follow that everybody else has to give up as well.
Many decide that string theory is a good starting point for such activity, because it contains quantum gravity already (which is certainly a necessary ingredient). Others may decide that it is
better to think about causal sets, simplicial lattices, LQG or whatever.

But why should they stop doing whatever they are doing just because you or Peter feel that it is taking too long already?
(By the way, it is not 20 years. People have been working on quantum gravity for more than 70 years.)

41. **wolfgang**
June 17, 2006

Peter,

I am sorry, our comments crossed.
I am looking forward to read your paper.

42. **Arun**
June 17, 2006

Wolfgang,

The question of whether string theory can produce a physical prediction or not is utterly independent of what my opinion is, and needs to be answered prior to "why should anyone stop doing what they're doing"?

Let us consider a patent absurdity to drive the point home.
If someone is proposing to compute Einstein gravity perturbatively to three loops, it is a legitimate question to ask why, what for, what do you hope to learn by it?

The question on the table is – at what point does any approach reach that level of absurdity?

43. **Bert Schroer**
June 17, 2006

Great Inquisitor,
I am quite impressed by your forthright description of the present crisis. But whereas you describe the symptoms you say little about the deeper causes. Obviously things are not that simple that some leading figures on the top decided to conspire to push a particular metaphysical fashion at the expense of more reasonable ideas.
One idea I have been thinking about is that as a result of an increasing sophistication of mathematical methods and a rapid grows of knowledge the time an individual particle theorist needs to pursue an idea and arrive at a breakthrough is perhaps longer than say at the beginning of last century. Even very intelligent and ambitious people may come into a situation where they have spend a sizable part of their lifetime with a problem without experiencing the satisfaction and joy of a significant accomplishment. Couldn't there be a strong temptation to force a situation in which such an experience is still possible within one's lifetime? In that case one would do everything which keeps this idea in the headlines against all ethics which the pursuit of science requires. Hegemonic control and arrogance as well as squandering, wholesale clearance of the conceptual treasures and their substitution of depth by banalizations seem to be the poisonous gift of string theorists to particle physics. Isn't this the equivalent of Enron and World com which we are witnessing? The fact that it is not an isolated phenomenon but a general hallmark of the Zeitgeist of unleashed globalized capitalism shows that this not something which is likely to stop if Witten gets disappointed with string theory or Susskind disavows anthropic arguments.
Wolfgang
any theory of something is a reasonable alternative of a TOE. I have never seen as many interesting and deep problems in my over 40 year professional life as there are now. I even think that I am working on a very interesting one. Nobody is forcing you to do work on the lattice which you obviously consider not as a worthwhile alternative to string theory.

44. **The Great Inquisitor**
June 18, 2006

Dear Bert,

Thank you for your kind response. I think that you describe the temptations facing a researcher in contemporary theoretical physics very well. My take on understanding the roots of the crisis is as follows:

The time of the CREATION of grand physical theories is over, at least for a long time, after the revolutionary discoveries in the 20th century (qft, general relativity). Many young theoretical physicists enter the field in the belief that a theoretical physicist's obligation is to discover new theories. They, and also many of the older theorists, have not learned to accept that in order to be successful, one's success strategy must adapt to the situation in which one lives. Schroedinger, Einstein, Heisenberg, etc. are success stories of the past, and cannot serve as valid role models for a contemporary theoretical physicist's career any longer. A string theorist's dream is to relive this era once more.

Now is the time for the ANALYSIS of the grand physical theories discovered in the past century. None of these fields, be it quantum mechanics, quantum field theory, general relativity, solid state physics, etc, are mathematically satisfactorily understood. In fact, they are incredibly poorly understood from a mathematical viewpoint. Isn't it baffling that after 100 years of quantum mechanics, no one has the slightest idea how to explain the simplest chemical processes mathematically rigorously from first principles ? While the theoretical physicists are missing the boat to appreciate and learn the exciting new mathematical methods available today to reach a better comprehension of the existing theories, several areas of mathematics that analyze problems in theoretical physics are nowadays booming.

Many theoretical physicists base their life on the hope to discover the holy grail of it all, the link between qft and general relativity. This is certainly a noble goal, but is it a reasonable one to build one's career on ? It's at least as unreasonable for a theoretical physicist to judge his/her success on his/her progress on this problem as it would be for a mathematician in the case of the Riemann hypothesis. Common sense, good taste, maturity, and modesty would forbid such a foolish and naive career strategy. This would certainly be the common viewpoint, were it not for the emergence of the miraculous genius of Witten, which has ever since tempted intelligent researchers to abandon all good judgement, and to give in to the hope and illusion of a quick answer to a big question.

The desire for a theory of quantum gravity finds some cheap gratification in string theory; it is the link to the observed world which is a problem. I have to remark here that in all this discussion of quantum gravity, one believes that gravitational waves need to exists in quantized form, and that they are not an emergent, effective phenomenon in some macroscopic limit. The dogma of gravity wave = graviton field has, as far as I know, not been really criticized. Perhaps the de Broglie principle is being taken too literally, and ad absurdum ???

While the theoretical physicists are either in despair of the absence of the last grand theory, or lost in a grand illusion, neighboring research areas are taking over, and solving extremely interesting and important problems stemming from theoretical physics. For previous generations, the natural evolution of physical theories was that they were first discovered by physicists, and when they became more mature, the mathematicians took over to manifest their true structure in all profundity. Perhaps, nowadays, theoretical physicists should take a step back, reject the sensation of a cheap miracle, and do the hard work to better understand the inner structure of the existing theories which are still as amazing as ever.

45. **rof**
June 18, 2006

Great Inquisitor,

You may be right about what is necessary for progress, but it is not likely to happen. The least well understood part of fundamental physics is the foundations of quantum mechanics, and studying this is bad for your career. In fact, anybody who even mentions it is a crackpot.

The problem is that attempts to get a clear understanding of quantum mechanics compels one to consider philosophical questions, and physicists do not handle this situation well. They consider philosophy a waste of time, a pursuit for lesser minds, so they are overly dismissive of their opponents' philosophical positions. It's philosophy, metaphysics, a waste of time, not even worth taking the time to understand. So they have a communication barrier, and end up calling each other crackpots, so the field of the foundations of quantum mechanics is in some disarray.

46. **Eugene Stefanovich**
June 18, 2006

rof:

The least well understood part of fundamental physics is the foundations of quantum mechanics, and studying this is bad for your career. In fact, anybody who even mentions it is a crackpot.

And, I believe, anybody who mentions Einstein's relativity is a triple crackpot… So, here we go. The most fundamental problem of theoretical physics is the incompatibility of SR and QM, and we are not even allowed to think about SR and QM. When are we going to wake up?

47. **Bert Schroer**
June 18, 2006

G I,
these thoughts you expressed so clearly have been on my mind for more than a decade. But I am not quite as pessimistic as you seem to be with respect to the chance of experiencing the pleasure of great new discoveries and insights. I think that the way particle theoreticians have been trying to force this during the last decades is futile and counterproductive. Instead of being extremely caring about physical principles underlying our most successful particle physics theory and to be more imaginative about extending the range of their implementation, they do just the opposite. They are ultraconservative on the side of the implementing formalism and "revolutionary" about speculations which squander established principles and concepts in an uncontrolled way. How else could you understand the slavish adherence to a metaphoric quantization approach culminating in the formalism of functional integration which is known to be artistic and metaphoric i.e. lacks any intrinsicness? To discover something in an artistic way is quite normal and human, see the Bohr Sommerfeld old style QM. Fortunately in that case the better implementation of the evolving principles was discovered so rapidly that there was no time for a fossilation of formalism. A very good illustration of the point I am trying to get across is the discovery of the renormalization theory during the 1940ies. The principles underlying QFT were already in place, but a totally inadequate implementing formalism prevented people from extracting the correct physical results and this led to quite wild speculations which cast doubts on the principles which were already clearly formulated by the protagonists of QFT. It was only after a radical change in the implementing formalism which upheld the principles, that real progress was made.
Roughly speaking string theory is what you get if you maintain a functional setting at all costs and instead massage the principles and concepts so that they seem to describe reality within an obsolete formalism. If you have such an efficient formalism as that of Feynman, it is understandable and even reasonable to explore it beyond its range of validity, but you should never allow it to play the role of a holy cow.
To keep the revolutionary ideas away from the the principles on the side of innovative and extended implementations is difficult, in fact very very difficult. One needs a lot of time, patience, knowledge, modesty and hindsight. There are a few people who have chosen this path, most of the ones I know work in algebraic QFT (Local Quantum Physics) and I have been trying to be one of them. It is not a carrier-supportive path and if my carrier would not have been completed before I took this decision, I may not have done it.
My worry is not so much if the span of my left lifetime is sufficient to experience the joy of a genuine discovery, rather I am worried if the knowledge which is necessary to achieve that and to understand its conceptual implication will not be wiped out by the increasing addition to get hegemonic control over particle physics and the arrogant idea of making nature like a dog jump over a string. In the media you hear a lot about string theory and considerably less about LQG (which likes to position itself as the adversary in the final Armageddon over the hearts and minds of particle physics). There will be a forthcoming book by Lee Smolin but I would bet that there will be no mentioning of the ideas on that topic coming from AQFT. In fact up to now that liberal atmosphere at the Perimeter seems to have been tested by only one AQFTist and even that one (Hans Halvorson) is more involved in the study of philosophical aspects of AQFT than in the main topic at the perimeter.
The pictures emerging in AQFT about physical reality (e.g. the characterization of the full content of QFT in terms of the relative position of a finite number of copies of the "monade", which I mentioned on some occasion) are quite startling precisely because they emerge from a totally conservative setting. But the ability to recognize, evaluate and to execute (i.e. its material basis at universities) these subtle new concepts has been significantly diminished by that banalization coming with that metaphoric way of thinking supported by string theory.
Since you mentioned the role of Witten in this process, I would like to direct your attention to the interesting fact that there was a different Witten before he was directed away from the physical beauty (see e.g. his analysis of the Kosterlitz-Thouless phase transitions in terms of infrared quasiparticle clouds) towards mathematical beauty by Atiyah. Nothing against mathematical beauty, but one should pay attention to its originating from physical beauty. Even now his role in the Maldecena "revolution" his role was relatively restraint (I think this is a residue of the early Witten) in the way a conjecture was manufactured into a fact and (after thousands of papers, I supported the various stages of this manufacturing process in an ealier blog by citations from some of these papers) became the pivotal argument why the fate of string theory is (according to Gross) inexorably linked to that of the Standard Model. Since this is a watershed (something akin but reverse to the phlogiston-oxygen change of the theory of combustion) which visibly separated the new metaphoric approach from the good old science, it may be interesting to return to this crucial event and analyze it more carefully (and without any polemics).

48. **wolfgang**
June 18, 2006

Dear Prof. Schroer,

> I have never seen as many interesting and deep problems in my
> over 40 year professional life as there are now.
I agree, physics is as interesting as ever if not more than ever.

> Nobody is forcing you to do work on the lattice which you
> obviously consider not as a worthwhile alternative to string
> theory.
You must have misunderstood my comment.
Recently there is some progress by people who try to put supersymmetry and superstrings on the lattice (look at papers by Kawai, Catterall an others) and I find this very interesting.
And there is still a chance that lattice gravity without
supersymmetry could work. I mentioned one example on my blog
here.

49. **Bert Schroer**
June 18, 2006

Wolfgang,

in that case there was a misunderstanding on my part. So we both agree that the world of particle physics is full of interesting and important problems (even below the challenges coming from gravity) which is a far cry from the "no other game in town" hype of stringers.

50. **The Great Inquisitor**
June 18, 2006

Bert,

Thank you for your clear comments and arguments.

I do agree that for a particle theorist working on approaches to QG other than string theory, everything is fine as long as he/she is self-motivated, has a job, and doesn't mind that he/she is getting a lot less attention than string theorists (but this is true for a lot of fields in science, right ?).

Maybe there will be the next jump forward very soon in one of these alternative fields, who knows. I think that young theorists should not enter the field out of a desire to see the next revolution in their lifetime (essentially, this is already true for at least one entire generation).

I agree that the strings approach is conceptually too conservative, despite its mathematical sophistication, while other approaches are in need of more mathematical sophistication. I also agree that it would be worthwhile to push alternative theories of QG, but the proponents of those alternative approaches should pull their act together, and do something really impressive for their PR.

What the strings community has understood is that propaganda and group organization helps tremendously to get "credibility" and, more importantly, financial funding. They are a very strong political lobby in the physics community. Any proponents of a physical theory other than strings should keep in mind that in order to promote and protect themselves, they should work on building political significance, too. This is the 21st century, times have changed.

One of the questions that have bugged me for a long time is how one of the strongest movements in theoretical physics in the past could have lost so much of its influence. Why have the constructive field theorists not built a lobby as strong as that, or why did they give up their position in physics ? At the end of the 70s, they were as strong as any movement in physics ever, and despite the fact that the Wightman program might have been to rigid and narrow to begin with, there would have been myriads of alleys to build along in order to further manifest the importance of the field. However, today, this area has almost entirely vanished.

I blame it on poor political skills, and the naivety of many of its members that the importance of the field would support itself. Some constructivists have learned the language of neighboring fields, and have remained very successful. Although we are complaining about the populist practises of the strings community, we have to acknowledge that their methods are working. They are getting recognition in the public, money, etc. More purist minded physicists shy away from such practises for ethical reasons, but in the 21st century, it might very well be that every branch of science needs a lobby.

Finally, I'd like to say that many young theorists are drawn towards strings, not because of its physical beauty, but because of its mathematical sophistication. Here I'd like to comment that it is much more worthwhile in such a case to directly go into pure mathematics which is so much more beautiful than string theory can ever be.

51. **island**
June 18, 2006

Inquisiter wrote:

> What Susskind says here is that the only alternative to ID is strings and landscape. This is complete BS. The truth is that ID is based on an argument that is inacceptable to scientific methodology; it is not scientifically verifiable.
>
> ID argues as follows: We have this creationist theory that explains the universe for those people who believe in it. Since it is not accessible to scientific disproof, it must be true.

Actually, Lenny is wrongly admitting that the anthropic principle constitutes evidence for intelligent design if we don't accept the landscape:

Lenny also said:
'The "appearance" of design is undeniable…'

…'So if you don't accept my theory'… … … is blackmail.

It's true that this is crap, but that's only because cosmological evidence that we're not here by accident does not constitute proof for ID without direct proof, since the default position in this case is that this is part of a natural goal directed process that includes intrinsic finality.

ID theories that don't include a deity are potentially falsifiable… but IDists don't want to play that distantly plausible hand because the really do believe that godidit, regardless of what they may claim in public.

At least Lenny has guts enough to recognize that the anthropic principle constitutes evidence that we're not here by accident without a multiverse of potential to lose its significance in.. even if he is doing so strictly for selfish reasons. It is very bad for science that this is only a conditional admission which most refuse to even recognize does indeed exist.

52. **JC**
June 18, 2006

Arun,

For something like calculating Einstein gravity perturbatively to 3 loops, the only reasons I can think of offhand would be things like:

– the person is incredibly naive
– they're a glutton for punishment
– they have nothing else better to do with their time
– they've been under a rock for the last 40 years, and still think Einstein gravity is the "language of God"
– they've been under a rock for the last 30 years, and have never heard of supergravity or string theory

53. **Joe Conlon**
June 18, 2006

Dear anonymous,

I think the precision of the answer has to be proportionate to the precision of the question. I don't know exactly which scenario you're referring to, but the expression `our brane' sounds like ADD or Randall-Sundrum style scenarios. Direct stringy brane constructions of the Standard Model – e.g. by the Madrid group -need 3 or 4 stacks of different branes to get all the gauge group factors. If the original model is to some extent ill-defined and not fully embedded in string theory, it is not fair to hold string theory to account for failing to predict out all the O(1) factors.

This isn't a criticism of these kinds of models: they're interesting and fun. But I think the distinction between 0.5 and 1TeV here can be accounted for in the uncertainty of the model's definition.

Brane tensions are universal and are not affected by supersymmetry breaking. How they are perceived may depend on the local metric and so be red-shifted by e.g. warping.

Best wishes
Joe

54. **Arun**
June 18, 2006

JC,

The point was that one does not have to have a more promising alternative to point out that a particular line of inquiry is not fruitful.

55. **anonymous**
June 18, 2006

dear Joe, yes I refer to ADD. As far as I understand, the tension of a stack of n branes is n times bigger than the tension of a single brane only if supersymmetry is unbroken. Otherwise there is a non zero force between the branes, and the tension is affected by the resulting "binding energy".

56. **D R Lunsford**
June 19, 2006

Well, I know of at least ONE peer-reviewed and published paper, which exists also at CERN, that was removed from arxiv (my own). Any other examples?

-drl

57. **nc**
June 19, 2006

Another example: my paper on CERN, ext-2004-007, published in Electronics World, vol. 109, pp. 47–52 (2003), was removed from arxiv in a matter of seconds in 2002. (I don't mind about getting on arxiv – after all it is American-funded and I live in Europe, but CERN now prevents external papers from being updated except via automatic arxiv feed. I can't update it, because it now only accepts external papers automatically from arxiv. I can log in at the CERN server database, but that's all.)

58. **Peter Erwin**
June 20, 2006

I got curious about what were the different (stated) journal-submission rates for different areas of the arXiv, so here's a crude overview, mostly based on the May 2006 postings for each group.

The percentage is the fraction of postings during May (or, for some areas with low traffic, Jan through May) which have *no* mention of a journal (submitted to or accepted at) or of a conference where the work was presented at.

astro-ph: 15% (considering only the first 200 posts of May)
cond-mat: 72% (ditto)
gr-qc: 62%
hep-ex: 17%
hep-lat: 50%
hep-ph: 73%
hep-th: 84%
math-ph: 80%
nucl-ex: 27%
nucl-th: 63%
physics: 61%
quant-ph: 79%

Caveats: this is all based on what posters wrote in the "Comments" field, or on the "Journal-Ref" field if it exists. As Peter Shor pointed out, at least some people may not bother filling in journal info in the Comments field, or updating the Journal-Ref field, even if the paper *is* submitted or accepted at a journal.

Nonetheless, it's interesting to see the difference between the more experimental/observational areas (astro-ph, hep-ex, nucl-ex) and the theory areas like hep-th and quant-ph.

59. **Chris Oakley**
June 20, 2006

Peter E,

I suppose that this shows that HEP theorists are a bunch of hippies who don't care much for respectability. Or getting the number of spacetime dimensions right.

60. **Bert Schroer**
June 23, 2006

G I,
I could not react to your last weblog since I was offline for almost one week. After perfect agreement with your analysis and conclusions about the present situation in particle theory, there was one point of disagreement (which by the way also indicates to me that our agreements are not the result of belonging to the same area of mathematical physics as I, since most in that small community with profound knowledge about QFT would also disagree on that point) which I find worthwhile to return to:

"I blame it on poor political skills, and the naivety of many of its members that the importance of the field would support itself. Some constructivists have learned the language of neighboring fields, and have remained very successful. Although we are complaining about the populist practises of the strings community, we have to acknowledge that their methods are working. They are getting recognition in the public, money, etc. More purist minded physicists shy away from such practises for ethical reasons, but in the 21st century, it might very well be that every branch of science needs a lobby."
I claim that any area in theoretical particle physics which uses the similar methods of lobbying and hype as the stringers and succumb to the temptation of hegemony of a certain doctrin in a still volatile area of particle theory will inevitably eventually also suffer from the same sociological diseases as string theory. In fact I predict that if the loop gravity (LQG) people allow themselves to be drawn into the use of these public relation methods and attempts of hegemonic control (as it seems the case at the present conference in Peking), they will before short or long have seized to be part of the solution and become part of the same problem. Looking forward to a kind of Amargeddon with string theory, they will only end up deepening the metaphoric confusion in particle theory.
Exact sciences have the very distinguishing aspect of objectivity which sets them apart from any other human activity; this unfortunately creates some friction with the subjective activity of lobbying and appearing in front of an elected political committee to sell half-baked speculative ideas. Of course when large and expensive pieces of hardware and years of constructive planning are involved, as in experimental high energy physics, there is no other way. But if on the other hand one is facing a volatile theoretical situations as the one in post-standard model particle theory this, method is not compatible with securing a healthy future of particle physics, although it may be securing the material comfort and cementing the intellectual hegemony of a group.
In this context I also vehemently disagree with a point which was made recently by Sean Carrol:
"…people like string theory for intellectual reasons not for socio-psychological-political ones. It's not a vast string theory conspirancy, funded by shadowy billionaires who funnel money through Princeton and Santa Barbara to brainwhash naïve onlookers into believing the hype. It's trained experts who think that this is the best way to go….."
It is impossible to reconcile such a statement with what string theorists are actually doing. Take as an example the affair around the Maldacena conjecture as exemplified by the following citations from original papers:.
Witten's introduction of the Maldacena conjecture reads as follows:
"Recently it has been proposed by Maldacena that large N-limits of certain conformal field theories in d dimensions can be described in terms of supergravity and string theory on the product of d-+1 dimensional AdS space with a compact manifold."
(Note the "proposed")
After more than two thousand citations the Witten passage changed to
(citation from Berenstein, Maldacena and Nastase):
"The fact that large N gauge theories have a string desrcipition was believed for a long time. These strings live in more than 4 dimensions….".
The process of "fact-manufacturing" culminates in the last of the 3 online Princeton talks by David Gross (http://www.princeton.edu/WebMedia/lectures/) in which he answers a question coming from the audience (can string theory be proven wrong?) stating that string theory cannot be proven wrong because it is inoxably linked via Maldecena's discovery to the Standard Model.
What I find so incredible is that the audience in a place like Princeton (where Pauli and other critical minds spend many years) receives such statements with applause. No Sean Carrol (please check the citations if you do not believe me, I can provide you with the exact citations including the page number), these kind of mortal blows to an exact science whose hallmark used to be objectivity is not done as part of a conspirancy. David Gross is not the boss of a mafia scheme to derail particle physics. It is this selfdilusion which we are all prone to succumb to if we hear from all directions (as the result of the lobbying) how fundamental and important our ideas are (especially after their importance is so obvious from the material support they obtain and the hegemony they exert). But what is the difference for physics that there is no underlying willful misleading intention? And why did Sean ever think that smebody could believe that the influence of globalized capitalism consists in some billionaires exerting influence over the content of particle physics by giving money to universities in Princeton/Santa Barbara? The influence of globalized capitalism on the moral fabric of objective sciences goes via Enron and Worldcom and via Bush's manufacturing of facts in order to be able to start a war: it is the manufacturing of facts which very ironically bind together the string theory in the US, in Teheran or (beginning right now) in China.
I have many other documentable arguments for advancing my viewpoint, but if people like Sean Carrol want to see or define them as harmless for particle theory, then my means are exhausted. But then I do not understand at all what it is is he wants to dicuss in his weblog. Unsucessful attempts to get hegemonic control over particle physics always were tried (see the bootstrap S-matrix approach of Chew-Stapp and others) before, but they never reached that level of introctination and control as now.
One does not get the full impression about the destruction of knowledge unless one really gets into contact with young people who went through that mindless string PhD mill, where one learns how to write impressive looking thesis with almost no knowledge of particle physics at all. My estimate is that a sizable fraction of young people who did this became meanwhile frustrated Jeckill&Hyde characters, the new proletariat created by their exploiting string theorist peers. The rest of the stringers fall like locust into other areas in order to devour everything and convert a banalized copy of what they can digest into string theory. For the last couple of years they have been trying to do this with integrable QFTs.
In good times for theoretical physics, for example the times of Nernst, Planck, Einstein, Born, Heisenberg,…in Germany, there was no lobbying at all. Rather the excellent scientific quality at universities, research institutes and academies was achieved by the relentless work (largely outside the public domain) by state secretaries for science and culture of city and state governments, people with an impressive academic background and first hand knowledge who could officiate with a remarkable autonomy without depending on the result of political elections.
Sometimes, when that did not work in due time, as it happened at the new (1919) founded University of Hamburg which remained for almost two years without a chair in theoretical physics, scientist applied a harmless trick. In this particular case the mathematicians Hecke and Artin inbited Albert Einstein for a public talk on a weekend (with Einstein perhaps not knowing what was the real purpose behind this) and some month later the chair for theoretical physics became a reality (with Wilhelm Lenz having Pauli as his assistant and Ising as his OPhD student).
Although in the US such a system probably never existed, the second worldwar and the subsequent cold war created a situation where lobbying for survival in theoretical particle physics was not necessary and where the increase in once's personal power and influence did not lead to the choking of other interesting points of views. The appearance of a metaphorical approach which gains a hegemonic control is a recent sociological deformation. The idea that a hegemonic control about the material and intellectual resources can be compensated by one individual who not only has to manage to reconquer the lost knowledge, but also (like a particle physics messias) comes up with an innovative idea which will direct particle physics out of the present crisis is utterly naïve.

61. **The Great Inquisitor**
June 23, 2006

Bert,

Thank you for your reply. I very well understand how much you reject the idea of focused political organization among physicists to reinforce the impact of their research field. As we all know, this carries the danger of abuse, and anything that can be abused will be abused in due time (as string theory proves).

I did not mean to imply that the same sort of irresponsible, aggressive propaganda should be used. However, string theory has pushed many areas of theoretical physics to the edge of extinction. My question is: Should one react or not ? Should one just accept the way things are, and wait until the bubble busts when Witten steps down (until which yet another generation of non-string theorists will leave the field), or should one try to get organized in an ethically responsible way. String theory is a relatively harmless illustration of the situation where a suppressed minority is forced to ask if, how, and when resistance is a moral obligation.

I brought up the constructive field theorists because their area is probably one of those that suffered the most from the brain- and job drain induced by string theory.

When I talk about lobbying, what I meant was (perhaps using an inappropriate choice of language) thatevery research field has a responsibility to organize itself in a way that the proliferation of its tradition is ensured. Every older generation of a research field carries a responsibility towards its younger generation of getting sufficiently organized (politically and otherwise) to ensure a good chance of survival. My question was how it happened for the example of constructive field theory that the situation has deteriorated so much, after a very excellent time in the 70's.

Of course, I couldn't agree more with you that in fundamental science, lobbying practices should not have their place. However, I ask how one should react if a neighboring field becomes totalitarian, and begins to spread and metastasize like a cancer ?

Did anyone read any of the articles in the "New Republic" and the "Nation" authored by Witten before he became a physicist ? As everyone knows, he studied political science, and was an aide for the McGovern presidential race. What is for sure is that he knows what he is doing.

62. **Peter Woit**
June 23, 2006

I have read the Witten article in the New Republic and the Nation. One is about visiting a commune in Taos, the other is about the New Left's political strategy. Both were written when he was a teenager, and don't show any signs of interest in Machiavellian political tactics. They're both extremely earnest in a late sixties sort of way; Witten seemed to be a very young man trying to make sense for himself of what was going on at a period of dramatic change in the US.

I think it's a big mistake to lay all of the problems with how string theory has and is being pursued at Witten's door. He's responsible for a lot of what happened, but to a large extent he was just doing what every scientist should: work hard on the ideas you find most promising, and try and get others enthusiastic about them. The fact that he is so talented and has accomplished so much gave him a huge amount of influence, not any particular political skills on his part.

At the moment the problem with string theory seems to me to not be Witten, but to be those much less talented than him who have devoted most or all of their career to string theory, can't conceive of working on anything else, and are willing to fight to the death anyone who tries to get people to give up on string theory and work on other topics. Witten himself for the last year has not been doing string theory, but something quite different. If he continues to develop interesting ideas about gauge theory and mathematics, they may ultimately really lead somewhere important. At the moment I don't see him going around giving talks about how wonderful string theory is, or engaging in anthropic pseudo-science, or any number of the other problematic things much of the string theory community is doing. He's one of the few hopes for the subject, one of the few people capable of coming up with the kind of new idea the field needs to start making progress again. There are plenty of people around now whose behavior regarding this is highly problematic, I just don't think Witten right now is one of them.

63. **The Great Inquisitor**
June 23, 2006

Dear Peter,

In principle, I do agree with you; as is said in one of the superhero comics, "With great power comes great responsibility".

My question is what Witten has done to contain the damage his field is doing, of which he is undeniably too smart to be unaware of ?

64. **woit**
June 23, 2006

The GI,

I do agree with you that Witten can be validly criticized for not having taken any responsibility for doing something about what has happened to the field. In particular I think it would be very helpful if he were to take some action on the issue of the anthropic landscape, perhaps pointing out that people who believe string theory is compatible with anything should give up on the theory and do something else, not start selling pseudo-science.

65. **The Great Inquisitor**
June 23, 2006

Peter,

Yes. As the saying goes (approximately), "There are two lessons of history: History repeats itself, and humankind never learns from history."

Isn't it ironic that the physics community, which has traditionally prided itself so much of supreme scientific objectivity is having this problem nowadays ?

Perhaps, it would be worthwhile to analyze what precisely it is that this lesson is teaching, and what one should learn from it.

66. **Bert Schroer**
June 24, 2006

Peter,
This time I have to agree 100% with G I even though he seems to come from a different area and may have slightly different reasons for his conclusions. It is really not the good intentions, the richness of imagination and mathematical talent of a person which is under discussion here. According to those criteria Witten is way beyond Heisenberg, Dirac, Pauli, name anybody from the pioneering days of QM. Without any question Ed Witten has been the most inspiring figure for a whole generation of young mathematically inclined theoretical physicists and even if this great attraction to people of the same age already showed up in his fighting for political liberal courses, it is of no direct relevance where his magic attractive personality showed up for the first time. A big difference to the previously mentioned physicists is that none became the guru of a group. Especially Pauli was respected and feared at the same time. But at no talk of Pauli (or any other one of the pioneers) everybody before the talk started was whispering in the corridors "What will Pauli (Witten) tell us ?" (concluding remarks by Ooguri at http://strings04.lpthe.jussieu.fr/.)
It is this kind of idolation which brings in a new element into rational science namely some sportish expectations which long for immediate fulfillment if possible coupled with some entertainment value.
Peter, will you tell me that people nowadays are condemned to be gurus whether they want or not? If you want to tell me that even Pauli under such circumstances may have lost his critical objective abilities, you run into open doors with me, but of course the historical Pauli did not (you may say that he lived in a different Zeitgeist and I would add maybe one which was more conducive to particle theory).

Take as a total contrast to the life work of Rudolf Haag. He felt already in the early 60s, when particle physics was still under the spell of the (most conservative) renormalization revolution that this will not be enough to really understand the inner working of the principles underlying QFT and that an additional step away from classical metaphorics towards a more autonomous understanding (similar to the liberation of geometry from coordinatizations) had to be undertaken before one can trust that the renormalizable/nonrenormalizable dichotomy has anything to do with the true conceptual frontiers carved out by the underlying principles. I learned in due time to appreciate that Haag's contribution was one of the best investments into the future of particle physics. This was not always so, because after a short collaboration with Haag at the beginning of the 60s (In the after sputnik time I had a research associate position in Champaign-Urbana without yet having a PhD) I left that area because progress was too slow for my taste at that time. I entered the critical phenomena (Callen-Symanzik setting) and later I found a generic relation between winding gauge configurations and zero spinor modes in the Schwinger model which sucked me more and more into the mathematically quite sophisticated Atiyah-Singer theory. I wholly embraced the subsequent reign of Euclidean topology and differential geometry i.e. the Witten-Atiyah Atiyah Zeitgeist. But when at a stay at CERN in the 80s I saw all these wide-eyed youngsters who dropped the word "topological"in almost every sentence I woke up again and the increasing trend of metaphorical arguments in particle physics finally brought me back to the algebraic approach which meanwhile had acquired an impressive amount of conceptual maturity. I am mentioning this here because very often in this weblog one reads complaints about the lack of alternatives to string theory (or LQG if one focuses on gravity only). If I would never have met my teachers Haag and Lehmann, I probably would also be among those apologists of string theory.

There is no particle physics setting these days which leads to results which are so markedly different from those coming from string theory or other mainstream ideas concerning issues of vacuum polarization (cosmological constant, black hole entropy) as those coming from LQP (local quantum physics). On the hep-th Tuesday listing you can convince yourself by looking at two updates which in this form I submitted to Class. Quant. Grav.
Coming back to the issue raised by G I's last blog contribution I would completely agree that somebody who has created a community has a critical responsibility. To say that an important leading figure abstained from stringy physics (like Berlusconi abstained from sex before the election) is not enough.

67. **Peter Woit**
June 24, 2006

Bert and TGI,

I agree with you that Witten has a lot of responsibility for the current situation. But it's also true that one could make a very long list of Nobel prize winners and other people with positions of great responsibility in particle theory who have chosen not to say anything about the problems caused by the way string theory research has been pursued. I've always found it kind of ridiculous that I should be the one prominently making the case that this is a problem, when there are many, many people who are much more than me in a position where they should be taking some responsibility for this.

68. **The Great Inquisitor**
June 25, 2006

Peter,

I believe that a problem with Nobel prize winners speaking up is that in the last few decades, very few theoretical physicists with a strong mathematical background have won it. Those who did usually obtained it late in life, and might not consider themselves competent to judge over string theory. Some particle physics nobelists did speak up, like Richter or Glashow, but it is easy for string theorists to dismiss them as mathematically uninformed.

Only when someone is very familiar with the mathematics of some aspects of string theory, it is possible to tell with certainty that something dishonest is going on. Mathematically less educated observers might either give string theory the benefit of doubt, or not feel threatened at all because their jobs are not at stake. So those people naturally in the position to speak up are mathematical physicists who work with the standards of rigor of pure mathematics.

If one looks for people in this area in a position to openly attack string theory, one ends up with a very small list. It is not without reason that these people are fought by the strings community; they are the only ones who can see that the emperor has no clothes, and who are therefore dangerous for them. Those people have observed what happened to colleagues in their circle who dared to openly express their thoughts (take Jaffe-Quinn for an example), and have decided that a silent status quo is better for their survival than an open conflict. And there is little reason to doubt their conclusion as long as Witten and a few other strings people are indeed producing supremely important results for *mathematics* (not for physics; I dare to claim that among large amounts of magnificent results for mathematics, string theory has not produced one single true result for physics).

I would like to make the following tentative conclusion what the issue with string theory really is about.

The best present argument in favor of tolerating string theory (and the fact that a lot of half-wits are among the practitioners) is to consider it as a testing ground for new mathematical ideas. This would justify the inclusion of ridiculous physical ideas (of the type presently made tasty for the uninformed public) into the discussion because they might in the end have interesting mathematical structure. String theory becomes ethical at the moment where it declares officially that, despite its beginnings, there is nowadays no link to particle physics anymore; only some of its methods are remnants of particle physics.

The true value of string theory is that of a mathematical experimental playground where methods of theoretical physics and geometry/topology are freely combined, to produce insights into topology, number theory, etc. This alone does justify its existence, but string theorists are extremely afraid of admitting that their field is not physics because they will lose a lot of funding and job positions in this process. However, I am sure that even then, there will always be excellent jobs for the best of them (but less jobs for the weaker ones; a statement which is true and natural for the rest of the physics and mathematics world). They would then still carry the job label "string theorist", but would work in a "department of experimental mathematics" that needs to be newly founded.

They would have the same working relation with number theorists and topologists as experimental physicists have with theorists; they generate interesting observations, but have to leave it to the community of higher mathematical rigor to produce theorems (actually, this is already the real situation nowadays). Those areas in mathematics will never give up their support of string theory because they are profiting from an extremely fruitful symbiosis (see the responses to Jaffe-Quinn from eminent topologists and geometers). They are experiencing a similar explosion of theoretical advancement as Heisenberg, Schroedinger, Pauli, Einstein, Dirac, Gell-Mann, Feynman, etc did during the physics revolution of the last century, thanks to the amazing observations of their experimental contemporaries.

The real problem with string theory is that as long as they do not officially give up their claim of a link to physics, they are stealing a lot of jobs from true physicists; in fact, they have such an overflow of positions that they end up filling them with people of very objectionable quality. To be able to further afford this unethical luxury, especially the weaker string theorists invest large amounts of efforts to try to convince the uniformed average taxpayer that what they are doing is in fact physics (they are not capable of higher mathematics, so they focus on phenomenology, but phenomenology necessitates a link between strings and physics).

So the Gretchen-Frage here is: Is it physics or not ? This is what the whole fight is about, because it determines the distribution of resources.

Therefore, I believe the right strategy the physics community should use is to reach an agreement with the mathematics community to newly create interdisciplinary departments of experimental mathematics, which accommodate string theory. This would solve the conflict for everyone. In fact, the string theorists then won't have to worry about the painful lack of physics anymore, and can be totally free in their model building. And even though they haven't proven themselves worthy as a subfield of theoretical physics so far, they have massive credentials as a field of experimental mathematics. While they are doing physics a disservice, they are doing certain areas in mathematics a monumental service; they should move in with their mates.

69. **Bert Schroer**
June 26, 2006

G I,
Your proposal to convert string theorists into experimental mathematicians and to create a group of experimental mathematics within a mathemaics department would indeed be a first step to get particle theory back on track, but unfortunately it is not realizable. There are too many string theorists whose professional knowledge of mathematics is completely insufficient; no mathematics department would want them. The few excellent individuals who have a profound knowledge of modern mathematics at their fingertips would of course be highly welcome (and I am certain that their impatience if it comes to prove theorems would be no obstacle especially if the word "experimental" is added), apart from the fact that mathematicians will loose their illusion about the magic power of physics by learning that all their golden castles which have been inspired by string theory were in fact constructed on the ruins of particle physics.
Actually there are already such groups in mathematics with the effect that they are entering joint research projects together with the stringers outside, and given the pro-string opinion in many decision-making comitees and with referees of research projects, this renders the situation of genuine mathematical particle physics even more precarious.
The situation in this weblog develops into a very interesting direction; unfortunately I can only occasionally contribute. But since the problems you raised will be with us for a long time (even if string theory will be recognized by leading theoreticians as a failed theory, there will be the long-time problem of damage repair) this may not be a

bad thing.
Among the topics which I very much would like to publically discuss with you in future weblogs are
1) Did the metaphoric kind of thinking which is the hallmark of string theory start already before, and if yes, where and when did it start? This is a problem on which I expect considerable differences in opinion with Peter, but on the other hand I am convinced that his prime motive is to stop the damage of string theory and not to plead e.g. for the resurrection of gauge theory a la Atiyah-Witten or to continue with the standard model within the limitations of the present Lagrangian framework.
2) How widespread is the system of “colonial exploitation” by more senior string theorists who distribute serialized little pieces of computations to their young innocent and unsuspecting PhD students without providing an overall view of particle physics, and how many cases are there where students who insisted to apply their greater conceptual and mathematical knowledge were actually removed resp. their support was cut so that they did not disturb the smooth computational grinding of the others? I know personally about two cases.
3) How big is really the damage with which have to cope even after the failure of string theory will be eventually recognized by a majority (many positions taken by individuals who know particle physics only via metaphoric glasses and social constructs and whose social success has deprived them of any intellectual modesty which is a prerequisite for a re-orientation).
4) How can one prevent the metaphoric mode of arguing entering other problems of particle physics (string-like arguments without string theory)
5) How can one stop the erosion of already acquired good solid knowledge of particle physics, and in particular how can one prevent that those few young researchers (who had the courage to resist the work on merchandize with a fast spoilage date) from having non career chances as compared to any candidate who hides successfully behind big Latin Letters?
Such discussions need time and should not be rushed through. T G I, I propose to you that you scan occasionally this weblog for contributions from me or other more serious contributers to this subject, as I will do the same. As long there is no special place for such exchanges of opinions about more long range aspects concerning the crisis of particle physics, this place could serve this purpose.

70. **The Great Inquisitor**
June 26, 2006

Bert,

Thank you for your kind reply. I would very much like to continue this discussion in this forum.

As to your question concening the damage string theory causes to physics when it goes down the drain:

I think that it is fair to say that the weakest element in string theory is string phenomenology, where a bridge is sought between string theory and physics. This is the area that will ultimately bring everything down. People will get impatient with the loud, naive, irresponsible, and low-quality physical predictions made by the mathematically weakest string theorists. When they fall, they will endanger both the existence of string theory, and the reputation of theoretical physics (which they have already damaged enough). String theorists will then have to make up their mind if they want to fall together with them, or disassociate from them, and consequently, from physics. In the long term, a future in an experimental mathematics department of the mathematically most able string theorists might be their only strategy of survival.

String phenomenology has a very poor scientific track record and life prognosis; it’s the sickest part of the area. When string phenomenology goes down, traditional physicists must recognize the opportunity of the moment, and wake up from their decades-long silence. That would be the moment to toss aside their self-restraint out of intimidation, and openly disassociate from string theory. If they remain lethargic, they might go down together with them, because the public eye is not capable of distinguishing string phenomenology from serious theoretical physics.

What happens afterwards ? We should perhaps learn from the fate of the former communist countries. There will be chaos for some time, small fragments of groups will attack one another, despite of their equal insignificance, and eliminate one another. Then, when the dust settles, the seeds that have survived the storm will grow fast in fertile grounds. More often than not, new golden eras have grown on the ruins of their preceding culture after those have completely disintegrated. Cultures, economies, research fields, obey the same lifecycles as organisms. They are born, grow to maturity, florish, then become either decadent or old and tired, and disappear (maybe Oswald Spengler’s “Der Untergang des Abendlandes” might be inspirational for this discussion).

We have to ask whether string theory emerged from theoretical physics completely coincidentally, or whether theoretical physics was already in a state of demise and decadence, and string theory was the logical fate that was waiting for it.

The danger and blessing of theoretical physics and mathematics is that they provide spiritual food for atheists and agnostics. It has been reported that Jane Goodall, who had been studying chimpanzee cultures for decades in Africa once observed a group of apes discovering a waterfall for the first time in their lives. The reaction of these primates was a self-forgotten dance, a celebration of exhilaration and awe, it was a spiritual experience for them. The insight derived from there was that higher primates have an instinct for spirituality, it is not a rational decision that compels people towards their spirituality. People need a sense of awe and wonder in this world. Organized religions serve that purpose for the masses. However, for the most mathematical and logical minded persons, only mathematics and theoretical physics are elaborate enough to calm their need for spirituality.

When string theory emerged, it gave theoretical physicsists who were in the 80’s starving for that sense of awe and wonder after a decade of no real new fundamental insights in their field. Many theoretical physicists were at that time unaware of geometry and topology, and when they were first brought into contact with those mature mathematical areas, for instance through the work of Witten, they lost themselves in the intoxicating beauty of it.

As I stated in my first contribution to this weblog: As Dostoevsky describes in his piece “The Grand Inquisitor” in the “Brothers Karamasov”, there are very few human needs besides that for food which absolutely cannot go unsatisfied. The need for spiritual wonder is one of them. The pursuit for satisfaction of this need has brought humankind art, science, and all the nearly divine accomplishments that our race can claim its own; on the other hand, the satisfaction of the same need has also led to the deepest morass of tyranny, fanaticism, genocide, and totalitarian hell humankind has experienced.

71. **The Great Inquisitor**
June 26, 2006

Just as a remark, here is a link to excerpts of Oswald Spengler’s work (here translated as “The decline of the West”). Bearing in mind that it was first published in 1918, it is baffling how well some parts match what we are watching every day.

http://www.duke.edu/~aparks/Spengler.html

And if for some readers here, it might turn out of interest, here is the segment of Dostoevsky’s “Brothers Karamasov” I was alluding to

http://perso.orange.fr/chabrieres/texts/grand_inquisitor.html

72. **Bert Schroer**
June 26, 2006

G I,
by the way I have mentioned the impact of Oswald Spengler’s work in particular on the development of QM in my polemic article http://br.arxiv.org/abs/physics/0603112
I think you find a quite realistic description of the actual postmodern Zeitgeist in the work of Theodor Adorno:

http://en.wikipedia.org/wiki/Theodor_Adorno
He is the one who had to understand how rationality can breed in its midst the most extreme form of irrationality. In his case the motivation was to understand Auschwitz, that is really the hardest problem; all other genocides and ethnic cleansing are not that hard.
There are many elements in his thoughts which can be applied to science as well.

73. **Alejandro Rivero**
June 26, 2006

Hmm a friend tried to read Adorno to me while sailing between Valencia and Salerno, and even in this quiet situation I was unable to grasp it 🙁 On the contrary I find easy to follow, for instance, things as touchy as the view about Time of Agustin Garcia-Calvo, so perhaps it is really -literally- a language problem and one needs to read Adorno in German.

74. **Aaron Bergman**
June 26, 2006

in fact, [string theorists] have such an overflow of positions

Hah.

75. **Bert Schroer**
June 26, 2006

Alejandro Rivero,
you are right and this is the reason why Adorno is less known in the English speaking philosophical and sociological world. The German language has this extraordinary possibility to synthesize new words by juxtaposing already existing ones. This makes reading difficult but at the same time one is able to highlight nuances and concepts which transcends existing ones. For example this conceptual flexibility was very helpful for Einstein in presenting the special theory of relativity with that masterful clarity. The issues treated by Adorno are extremely subtle and complex.

76. **Eugene Stefanovich**
June 26, 2006

Bert Schroer:

1) Did the metaphoric kind of thinking which is the hallmark of string theory start already before, and if yes, where and when did it start?

The Great Inquisitor:

We have to ask whether string theory emerged from theoretical physics completely coincidentally, or whether theoretical physics was already in a state of demise and decadence, and string theory was the logical fate that was waiting for it.

These are exactly the questions which I would like to see asked and answered. Shall we learn something important from the rise and fall of string theory (besides the trivial fact that some individuals sometimes make unscrupulous claims)?
Shall we reconsider the entire culture of theoretical physics? What is the role of experimental verification and mathematical proof in a physical theory?

If we forego this chance now, we are destined to repeat the same mistakes with some other “theory of everything”.

77. **The Great Inquisitor**
June 26, 2006

Bert,

Thank you for the weblinks. I have read your article with interest, and if time permits, will try to become more informed about Adorno’s work.

Eugene,

I think these are really key questions, and what you are asking is also central to it all.

Maybe we should not forget the roots of the tradition of Western Science. Science has been the vehicle that pulled western civilization out of the hell of mindless superstition enforced by the Church some centuries ago. Objective experimental verifiablility and mathematical provability, identified as the most appropriate and solid cornerstones of science, have made it possible to leave behind the terrors of that time. Today’s technology-centered western civilization owes its stature to those inquisitive and irrepressible minds back then who defended scientific methodology against Christian oppression.

Theoretical physics has been one of the absolute crowning achievements of this long struggle. But look at what string theory is making of it.

A small remark: My first contact with this forum was only a couple of weeks ago, and I have also not been more familiar with the competing one by Motl. I know that Peter does not want any more comments on this, but if he allows, just one observation: I read that he has been an Assistant Professor at Harvard since 2004. Did anyone observe that since then, he has only produced an extremely meager scientific output (a few short, multiauthored papers), way below the average for his field ? Being in prediction mode today, my prognosis is that he will be history very soon; he is burned out, has become psychotic due to the pressure to perform mathematically beyond his capacities, and is wasting very large amounts of time on his blog, on which he has become addicted.

Richter Talk at SUSY06

June 17, 2006
Categories: Uncategorized

Burton Richter's talk at the panel on "Naturalness" at SUSY06 is now on-line. Richter blasted his three theoretical colleagues on the panel (two of whom are his colleagues at Stanford) in forceful terms as no longer doing science:

… I think some of what passes for the most advanced theory these days is not really science.

I see no problem if part of the theory community goes off into a kind of metaphysical wonderland, but I worry that they may be leading too many of the young theorists along into the same wonderland. Simply put, it looks to me as if much of what passes as the most advanced theory these days is more theological speculation that it is the development of practical knowledge.

… the distinction between theory as theological speculation and as the development of practical knowledge. Theological speculation is the development of models with no testable consequences.

The price of this invention is 124 new constants which I always thought to be to high a price to pay.

Naturalness may be a reasonable starting point to solve a problem, but it doesn't work all the time and one should not force excessive complications in its name.

*The Anthropic Principle is an **observation**, not an **explanation**…. I have a very hard time accepting the fact that some of our distinguished theorists do not understand the difference between observation and explanation, but it seems to be so.*

… what we have is a large number of very good people trying to make something more than philosophy out of string theory. Some, perhaps most, of the the attempts do not contribute even if they are formally correct.

It is not that the landscape model is necessarily wrong, but rather if a huge number of universes with different properties are possible and are also probable, the landscape can make no real contribution other than a philosophic one. That is Meta-physics, not physics.

After all, the Hebrews after the escape from Egypt wandered in the desert for 40 years before finding the Promised Land. It is only a bit more than 30 since the solidification of the Standard Model.

Update: Clifford Johnson was there, and has a report on the session. He describes Richter's talk as "It was basically a loud fart in a quiet cathedral, during evensong. Excellent."

Comments

1. **Christine Dantas**
   June 17, 2006

   Sorry if I quote too much, but here it goes:

   > "(…) But in this quantum-gravity research area, since there was no experimental guidance, it was inevitable for theorists to be tempted into trying to identify the correct theoretical framework relying exclusively on some criteria of conceptual compellingness. Of course, tempting as it may seem, this strategy would not be acceptable for a scientific endeavor. Even the most compelling and conceptually satisfying theory could not be adopted without experimental confirmation."
   >
   > "(…) And often in the media the different approaches are compared on the basis of the "support" they have in the community: one says "the most popular approach to the quantum gravity problem" rather than "the approach that has had better success reproducing experimental results". So, it would seem, the Quantum Gravity problem is to be solved by an election, by a beauty contest, by a leap of faith."
   >
   > "(…) Quantum gravity phenomenology requires of course a combination of theory and experiments. (…) One here is guided by the expectation that quantum-gravity research should proceed just in the old-fashioned way of scientific work: through small incremental steps starting from what we know and combining mathematical-physics studies with experimental studies to reach deeper and deeper layers of understanding of the problem at hand (in this case the short-distance structure of spacetime and the laws that govern it)."

   Amelino-Camelia
   Introduction to Quantum Gravity Phenomenology
   [http://arxiv.org/abs/gr-qc/0412136]

   Thanks,
   Christine

2. **John Stanton**
   June 17, 2006

   There is a good argument against the Anthropic "principle":
   Apes or pigs could equally state that the universe is made for them.
   One could equally speak about the Simian or the Porcine principle.

   I read this somehwere on the internet. Always liked it.

3. **Not a Nobel Laureate**
   June 17, 2006

   And I'm relieved to see a great physicist like Richter pointing out in forceful terms that what the pseudo-theorists are doing today is theological meta-physics not physics.

   Although "pata-physics" may be a more appropriate description of their activities.

4. **Tony Smith**
   June 18, 2006

Back in January 2000 (version 2 of hep-ex/0001012) Burton Richter said:
"… To the experimenters I would say that supersymmetry is a pure "social construct"
with no supporting evidence despite many years of effort. It is okay to continue looking
for supersymmetry as long as it doesn't seriously interfere with real work (top, Higgs, neutrinos, etc.). …".

It seems to me to good advice today, particularly to those lobbying for building the ILC.

Back then, 6 years ago, Richter was more or less agnostic about string theory, saying "… String/brane theory … represents an attempt to bring together gravity and quantum mechanics, a problem worth serious effort. … these are early days … String/brane theory may even give the necessary constraints that supersymmetry needs to reduce the number of constants to a believable level. It is still too early to say, but it may be much more than metaphysics. …".

Sadly (for string theorists) but realistically (for the rest of us), the results (or lack thereof) during the past 6 years lead Richter to say (as quoted by Peter) as of now:
"… what we have is a large number of very good people trying to make something more than philosophy out of string theory. Some, perhaps most, of … the attempts do not contribute even if they are formally correct. … it looks to me as if much of what passes as the most advanced theory these days is more theological speculation that it is the development of practical knowledge. … Theological speculation is the development of models with no testable consequences. …".

A problem with contemporary high-energy theoretical physics is that the mainstream, dominated by superstring theory plus a bit of Loop Quantum Gravity, is unwilling to make any significant effort to understand and evaluate alternative approaches leading to the deveolpment of models that do have testable consequences.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

5. **Sailorstar**
June 18, 2006

As a young theorist, it benefits me if as many potential competitors spend their lives on string theory as possible. Meanwhile, I will follow theories that produce testable results. Go ahead, make my day.

6. **MathPhys**
June 18, 2006

Peter,
Could you please link to Motl's blog where he says that B Richter is an average squirrel and all that? It must be there somewhere but I can't find it.

7. **string advocate**
June 18, 2006

dear Peter,

the fact that string theory happens to be "vacuous" enough to be a candidate theory for an anthropic interpretation of the smallness of the cosmological constant and of other old anthropic coincidences is not something that string theorists wanted. Rather, string theory was over-sold as the uniquely predictive theory of The Elegant Universe, and various string theorists, including Lubos, are still trying to defend this old propaganda. Discovering theories better than theorists is not unusual: Einstein tried to prevent the expansion of the universe and Dirac tried to make the positron 2000 times more massive than the electron, etc.

Probably we will never be able of testing the landscape, but it is the most plausible scenario (or, if you prefer, theology) we have today. String theorists willing to explore this direction hopefully understand that doing meta-physics with hype is much more dangerous than doing physics with hype.

8. **Chris Oakley**
June 18, 2006

> Peter,
> Could you please link to Motl's blog where he says that B Richter is an average squirrel and all that? It must be there somewhere but I can't find it.

Squirrels are very smart. They can do complicated multi-factor dynamical calculations using local approximations to Newtonian gravity with air resistance taken into account far better than any String Theorist. They also have a strategy for coping with tough times that does not involve wishful thinking or deception.

9. **MathPhys**
June 18, 2006

Very clever, Chris 🙂

10. **nontrad**
June 18, 2006

Peter,

Thanks for bringing this to attention!

It is a welcome relief to hear someone of Richter's stature make such sober comments. If only there were more of the same from similar High Energy figures.

Alas, the combination of a lack of data, lack of funding for extracting that data, power in the wrong hands, the glory of the Einstein effect (successful theories such as SR / GR crafted by an individual in isolation) and the addictive nature of pursuing beauty for beauty sake (unchecked by reality's unforgivingly counter intuitive nature QM / SR / GR / Chaos)… is too powerful a temptress. So, instead, until the day when there is finally some data, we will continue to hear of the wild pie in the sky speculations in HEP.

I suppose the consolation for the interim are voices such as yours, Richters or Laughlin's. We can only hope that these voices grow in number and finally impact the check writers.

11. **Doug**
June 18, 2006

If what passes for advanced theory today is not really science, but theological speculation, then, given the huge amount of government money spent on supporting it, the ACLU should be expected to step in soon on grounds that such expenditures are a violation of the doctrine of separation of church and state.

If they don't, and the high priests of string theory get more and more state sanction, the likes of string theory priests such as LM may eventually be able to enforce their views by purging heretics through a program of inquisition, funded by the "holy see [it our way or no way].

12. **knotted**
June 18, 2006

'If they don't, and the high priests of string theory get more and more state sanction, the likes of string theory priests such as LM may eventually be able to enforce their views by purging heretics through a program of inquisition, funded by …'

Doug, you need the past tense throughout that sentence.

The mainstream has been using state money to fund LM for the inquisition for ages, also the purging of heresy off arxiv has been done by Jacques Distler. The mainstream has the power and uses it to censor out all criticism and to label all alternatives crackpot. It is all over. Physics was conquered by complete lies (deceiving hype) funded by the taxpayer and the media. If I lied that I had extra dimensional-based unification and quantum gravity, without actually having a shred of evidence, I'd be ashamed of myself.

13. **Walt**
June 18, 2006

Sailorstar: Funny.

Knotted: Note that HEP is not all, or even most, of physics.

14. **Chris W.**
June 18, 2006

String Advocate says: *Probably we will never be able of testing the landscape, but it is the most plausible scenario (or, if you prefer, theology) we have today.*

It is only as plausible as string (and M-) theory. Insofar as string theory deserves to be judged by its consequences, including those that undermine its very testability, it's high time to start regarding string theory *as it stands* as implausible—not because of its lack of experimental confirmation, but because of the emptiness of *any* putative experimental confirmation in the context of the Landscape and its anthropic interpretation.

The question that ought to be asked now is why the original physical idea—a shift from a dynamics of point particles interacting in spacetime to a dynamics of one-dimensional objects with certain properties in spacetime—should have produced any interesting results at all, even if only in the nature of a rough correspondence. What does this mean? If quantum field theory and spacetime itself have a substructure, as GR + QM lead us to suppose, what sort of substructure might lead to string dynamics as a sort of residual trace? M-theory is supposed to be the answer to that question, but it is much more obscure than string theory itself. When deep physical questions are at issue we ought to be willing to step back from the formalism and ask simple, penetrating questions. Most string theorists, and for that matter, particle physicists, seem to have little interest in this kind of thinking. (Gerard 't Hooft is a notable exception.)

——————————
PS: Putting the Landscape on a par with the positron and the Hubble expansion is laughable.

15. **anonymous**
June 19, 2006

dear Chris W

I disagree with you. Anthropic arguments have not been invented by string theorists. Anthropic arguments survived for many years at the borders of science because they offer plausible interpretations of various observations: why there are many nuclei, why the cosmological constant is small, and now why string theory never gave predictions. For the first time all this makes a coherent scenario.

The only little difference with respect to Einstein and Dirac is that we miss experimental confirmations and ideas on how to get them. If you want to see something laughable, the probability distributions in the talk by Linde are a better candidate.

16. **ObsessiveMathsFreak**
June 19, 2006

Anthropic arguments are essentially an isomorphism to more basic "God of The Gaps" arguments. They run thusly:

> We cannot (currently) explain $phenomenon, therefore $deity is responsible for $phenomenon.

As far as I can see, anthropic arguments explain nothing, and do little except say "This is as it is because it is." Universal constants are "fined-tuned", but this in itself is evidence of nothing, except that the constants are as they are.

For all we know, any number of supposedly "sterile" combinations could have produced a universe with life. Just not as we know it. It's safe to say that universes are chaotic systems and that small perturbations in the initial conditions can have big effects on the final solution.

17. **Not a Nobel Laureate**
June 19, 2006

Life imitating Humour.

String theory and the Antropomorphic "Prinicple" can be summed up by that famour cartoon.

http://www.drabruzzi.com/images/Then%20a%20Miracle%20Occurs.jpg

18. **Tommaso Dorigo**
June 20, 2006

I think Burton Richter either knows Galileo Galilei's sentence (written in golden letters on the stairs of my Physics Department, which bears his name),

"Io stimo piu' il trovar un vero, benche' di cosa liggiera, che il disputare lungamente delle massime questioni, senza conseguir verita' nissuna",

("I have higher regard of finding a truth, although on something silly, than to discuss endlessly about the most important questions, without achieving any truth") or would easily subscribe to it.

Any reference to the meeting in Beijing is voluntary…

Cheers,
T.

Strings 2006

June 18, 2006
Categories: Strings 2XXX

Strings 2006 is about to get underway in Beijing, at a hotel next to Tiananmen Square, with public talks by David Gross, Andy Strominger and Stephen Hawking. Jonathan Shock reports that an agreement was formally signed today creating the KITPC, a Chinese version of the KITP, to be funded by philanthropist Fred Kavli and sited in Beijing.

The conference will attract about 400 physicists from around the world, as well as international press from many major European and American publications. There will be a press conference held Wednesday afternoon. Monday afternoon Witten will be giving the first scientific talk of the conference, on “Gauge Theory and Geometric Langlands Program”. He has given talks at almost all the Strings conferences since the first in 1995, but this will be the first one at which he won’t be talking about string theory. He will be followed by Cumrun Vafa, who will talk about his Swampland studies. On Tuesday evening, the schedule says “Prof. Yau present his new research result”, which presumably will be about the proof of the Poincare conjecture.

Monday evening will feature a panel discussion, which could get exciting. Last year’s panel discussion in Toronto featured Jacques Distler walking out in a huff, pointed questions from the floor about whether research in string theory was still defensible, and a clash between the panel, which was rather in favor of the anthropic landscape, and the rest of the conference attendees, who were very much against. Perhaps the Strings 2006 organizers, unlike the SUSY06 ones, will be able to find a prominent theorist who is not in favor of the anthropic landscape to put on the panel. An obvious candidate who will be there is David Gross, who at Strings 2003 gave a rousing Churchillian speech urging string theorists to not give up on science, and around that time suggested that his colleague Joe Polchinski had contracted a disease and should consider giving up his professorship (Polchinski in return accused Gross and Witten of being members of a cult). Gross hasn’t been publicly heard from on this topic recently, perhaps he has moderated his opinions and/or gone over to the other side.

Update: This posting has been edited, and many comments related to despicable behavior of a certain Harvard faculty member have been removed. I’m looking forward to hearing reports of what is going on at Strings 2006 from anyone attending.

Update: It turns out that Kavli is founding two new institutes in China, one for theoretical physics, one for astronomy and astrophysics. There’s more about this here. The Chinese press has also started to report on the conference, as usual media attention focuses on Stephen Hawking and the origin of the universe.

Update: Victor Rivelles is blogging from the conference, where he reports that 3000 people gathered in the Great Hall normally used for communist party meetings to hear promotional talks about strings. He also reports that the panel discussion was uneventful, with no mention of the landscape:

In the evening we had the panel discussion with 13 people. Strominger was chairing it. No real debate. Too mild. Some interesting questions but none of them provocative enough. No discussion on the landscape either. Toronto was much better…

Update: There’s an article in Tuesday’s New York Times about Hawking and the Beijing conference by Dennis Overbye. According to Overbye there are 800 physicists there and 6000 people turned out to hear Hawking. Anyone know if this is right?

Update: There’s a detailed report on the first day of the conference from Jonathan Shock.

Update: Victor Rivelles reports on day 2 of the conference. He describes Yau as taking credit for proving the Poincare Conjecture, which, if true, would be seriously misleading. ChinaDaily describes the number of physicists at the conference as 600. I’m guessing that 400 is the number of participants from outside China, 6-800 the total number. If so, this would be the largest string theory conference ever held.

Update: Day 3 report from Victor Rivelles, who seems to be have the mistaken impression that there are two Brian Greenes…

Update: Day 4 report from Victor Rivelles, who clears up the two Brian Greenes confusion. Brian couldn’t make it to Beijing, so his talk was given by Koenraad Schalm. Also a report from Jonathan Shock.

Update: More reports from Jonathan Shock (aided by Paul Cook), and Victor Rivelles. The publicly announced plan until now has been to have Strings 2007 in Madrid, Strings 2008 at CERN, and Strings 2009 in Rome. Rivelles reports a rumor that the CERN conference will be delayed until 2009, Rome then in 2010. Summer 2009 is when LHC results may start to appear, so he comments:

… if supersymmetry is found in 2008 then we all will be together to celebrate it and if it is not found we will be together for a collective suicidal.

Update: There’s a wrap-up of the conference from Jonathan Shock, including a description of Dijkgraaf’s summary talk. I have no idea if it is accurate, but it makes Dijkgraaf sound highly unrealistic

…LHC, astroparticle physics and the next generation of microwave observers may give us real signs of string theory in the coming years…

…a golden age of physics?

inaccurate

…Douglas et al’s recent work showing the finiteness of the number of vacuum solutions

and just tasteless

the possibility of success for loop quantum gravity, though this tactic ended with an own goal, and rapturous applause.

So far no sign that the organizers of the conference of the conference will be putting up any materials from it on the web, so that people who weren’t there can see for themselves what the speakers had to say.

It also seems that Dijkgraaf commented favorably on the very reasonable censorship policies of the Chinese government:

One particularly important point was that being behind the great firewall, people could read Peter Woit’s blog but not Lubos Motl’s!

Comments

1. **Ryan**
   June 18, 2006

Not to nitpick, but it’s spelled “Tiananmen.” I had to look it up, but I knew there was another “n” in there.

2. **woit**
June 18, 2006

Thanks Ryan, fixed.

3. **Peter Motl**
June 18, 2006

Last time i checked, there’s no canonical way for spelling chinese words in english.

4. **Troublemaker**
June 18, 2006

Then you’ve never heard of any of the standard transliteration schemes for writing Chinese in the Latin alphabet.

5. **MathPhys**
June 19, 2006

Is the Strings 2006 web site difficult to navigate and find information in, or do I imagine things?

6. **Peter Woit**
June 19, 2006

MathPhys,

The Strings 2006 web-site is difficult to navigate. The “What’s New” section doesn’t appear to have been updated since two months ago. I’m hoping that various bloggers will help fill the information gap…

7. **hongbaozhang**
June 19, 2006

When String06 starts in Beijing, Loop driver Carlo Rovelli comes to Center of Gravitation in China—Beijing Normal University. He will give series lectures on construction of scattering amptitude in loop quantum gravity. Today he gave the first one, which is so physical and clear. I enjoy it.

8. **Who**
June 19, 2006

This is wonderful!

I think someone with a name similar to yours was at the Loops ’05 conference where Rovelli gave a talk about constructing graviton in LQG. Maybe you heard this talk. (I could not come to the meeting and did not.)

It amazes me that Rovelli is talking at Beijing Normal about LQG scattering amplitudes now at the same time as Strings ’06. This was a creative move by the planners, or by somebody at Beijing Normal. It was a good new idea to do this, in my humble opinion.

9. **Babe in the Universe**
June 19, 2006

This is a very exciting time to be in science. Some branches of theory look increasingly like medieval epicycles (you all have opinions on which ones) but there is opportunity for huge advances in science. Hopefully this conference will result in much useful conflict!
My Chinese travels described this week found a fascinating and vibrant society. Since the Chinese are so interested in energy, perhaps a theorist will sell them something they really want. That may result in all sorts of funding. Any ideas?

10. **hack**
June 19, 2006

I had heard than Strominger went through a Maoist phase in his youth. Preaching strings in the People’s Hall seems quite appropriate.

11. **Who**
June 19, 2006

Hongbao Zhang,
sorry I made a mistake. I see you are author of a number of articles in Phys. Rev. D, some of them about Loop Quantum Gravity and related proposals. But it was someone else, a Dr. Hua Zhang, who was at the Loops ’05 conference last October in Potsdam.

12. **Rufus T. Firefly**
June 19, 2006

Haha, new message for the link from here to Motl’s blog:

“Error
You have attempted to visit a serious physics-oriented page from a major crackpots’ discussion forum called “Not Even Wrong”. That generated an error message. In 15 seconds, you will be redirected to the main page of “The Reference Frame”.

This policy is designed to reduce the amount of garbage and spam that the crackpots attempt to post on “The Reference Frame”, especially the targetted garbage ordered and provoked by the owner of “Not Even Wrong”. If you’re not a crackpot, we appologize for inconvenience.”

13. **Peter Woit**
June 19, 2006

Please, I’m trying to stick to John Baez’s excellent advice that the effort needed to ignore Lubos is amply repaid. No more about his idiocies here, if you must, use the recent posting related to him.

14. **Yidun**
June 19, 2006

Dear Who,

Hongbao Zhang is not Hua Zhang. Hongbao focuses on cosmology and gravity. He does have couple of papers, coauthored by Prof. Ling Yi, who is a former student Prof. Smolin's at Penn State. Hua Zhang is another graduate student, working on canonical QG with Prof. Yongge Ma, who gave a talk in Loops'05 last year.

Best, Y.

15. **MathPhys**
June 19, 2006

In case anyone is still unable to find the conference program, you do as follows

1. Click on "Agenda and arrangements" somewhere in the middle on left panel.
2. Ignore the table titled "Program and Reports" that occupies almost the entire page. That's no longer being updated. It just sits there.
3. Look for a link on top that says "(PDF file)" (it's to the left of an unlinked picture).
4. Click that to get a pdf file titled "schedule5.pdf".

16. **hongbaozhang**
June 20, 2006

To Who,
Who is who?:)

17. **Hans de Vries**
June 20, 2006

Weed smokin' 2006 in Beijng….

http://sites.gizoogle.com/showpage.php?url=http://www.math.columbia.edu/~woit/wordpress/

Link from LM about the current conference over there….
The contrary effect (as usual?) it seems. Sorry, couldn't help being
amuzed :^)

Regards Hans

18. **Hans de Vries**
June 20, 2006

Weed smokin' 2006 in Beijing

OK, the saved screen-shot then:

http://chip-architect.com/physics/WeedSmoking.jpg

Regards, Hans

19. **mathjunkie**
June 20, 2006

I like the name Peter Motl!

20. **Pindare**
June 20, 2006

Peter, you said Witten won't be speaking about string theory for the first time at a Strings conference. I've noticed he'll be giving a set of lectures at Berkeley next november, and probably this will also be about Langlangs stuff, not strings (there's no program yet).

http://math.berkeley.edu/index.php?module=announce&ANN_user_op=view&ANN_id=63

Question is: has Witten decided to do some maths until the LHC data arrives just as a side project, or has he in fact started doing this to be able to quickly jump off the bandwagon in case nothing much comes out of LHC?

21. **Peter Woit**
June 20, 2006

Pindare,

Thanks for the link to the announcement of Witten's lectures.

I doubt that his decision about what to work on has anything to do with the question of what will come out of the LHC. If he had a good idea about LHC energy scale phenomenology, he'd probably be working on that. His decision to work on a more mathematical topic related to gauge theory presumably just reflects the fact that this is where he sees how to make some progress, and he doesn't see how to make any progress on string theory at the present time.

22. **D.J.**
June 20, 2006

"In 1988, Hawking published his great book, A Brief History of Time: From the Big Bang to Black Holes, which is still considered by the scientific community to be a milestone."
(http://english.people.com.cn/200606/19/eng20060619_275174.html)

What can I say?…

23. **Peter Woit**
June 20, 2006

D.J.,

Well, it was a milestone, convincing many publishers and scientists that there was a mass-market for popular books about science…

24. **Victor Rivelles**
June 20, 2006

Dear Peter,

I can't asnwer your comment in my blog because blogspot is blocked in China. Please see the update in my post for an asnwer to your comment.

All the best,

Victor

25. **Stalyn**
June 21, 2006

From http://www2.math.northwestern.edu/langlands/index_moreinfo.htm

"It has long been suspected that the Langlands correspondence is somehow related to various dualities observed in quantum field theory and string theory."

You can also find notes for a lecture with the same title given by Witten dated August 2005.

http://insti.physics.sunysb.edu/itp/conf/simonswork3/talks/Witten.pdf

26. **Aaron Bergman**
June 21, 2006

From the math side of things, I give you The Ben-Zvi Repository of All Things Langlands. Recently updated and now including valuable and little known television references.

27. **csrster**
June 21, 2006

Maybe some people have trouble spelling "millstone"?

🙂

28. **Steven H. Cullinane**
June 21, 2006

Xinhua has a story from June 20 on Yau showing a video in Beijing of a talk by Hamilton on the Poincare conjecture. This Xinhua story is rather Sinocentric, but it is balanced nicely by a document from China's Morningside Center of Mathematics that gives a more complete record of Hamilton's talk.

29. **Jean-Paul**
June 21, 2006

This incredible success of strings in China reminds me of fading stars like Michael Jackson trimphant tour of Germany few years ago or Prince's full-stadium concerts in Switzerland in the early 90's. Hopefully, the new KITP will help Chinese to focus on the cutting edge of science intead of wasting time on failed ideas or b.s.ing on multiverses.

30. **Nick**
June 21, 2006

I just posted a rather scathing reply at http://greenr.com to Hawking's remark from the seminar where he said he was "very worried about global warming," and that he was afraid that Earth "might end up like Venus, at 250 degrees centigrade and raining sulfuric acid."

He has become so detached from scientific reality it is ludicrous. Why is he still respected?

I of course included a link to Not Even Wrong, as well as Junk Science's global warming debunking, as well as Crichton's "Aliens Cause Global Warming" lecture.

I'm glad to have found this blog, and appreciate all of your efforts to bring sanity back to science.

31. **Peter Woit**
June 21, 2006

Nick and others,

Please stop attempts to involve me and this blog in the climate change debate, which seems to me to be almost entirely carried on by people who don't know what they are talking about. I want no part of it, and it has nothing to do with the concerns of this blog, which deals with particle physics and mathematics.

32. **MathPhys**
June 22, 2006

I'm disappointed that the String 2006 talks are not posted online, videos, transparencies and all. It's not hard work, and it's a great service for the entire community.

33. **MathPhys**
June 24, 2006

The Cao-Zhu paper is not online (everybody else's papers on the subject are, including Perelman's), and the Strings 2006 talks are also not online in any shape or form (in contrast with tradition in these conferences).

If someone here knows S T Yau, could you please bring this to his attention? I'm sure he'd do something about it.

34. **Amy Zeck**
June 30, 2006

I saw these math T-Shirts and had to share them with everyone:

Pi
http://www.kleargear.com/2306.html

Math Chicks Are Hot
http://www.kleargear.com/1506.html

🙂 amy

The String Theory Backlash

June 19, 2006
Categories: Not Even Wrong: The Book

Over at Cosmic Variance Sean Carroll has a posting about The String Theory Backlash. Probably best if anyone who wants to discuss it do so over there, so I'll turn off comments here.

Update: More about this here.

Update: The posting in the last link (which was an impressive anti-Sean Carroll rant) is now gone, presumably because many of the author's colleagues finally realized something had to be done, and wrote in to his blog telling him that he "must be on crack", and had "completely jumped the shark".

No Comments

LHC News

June 20, 2006
Categories: Uncategorized

JoAnne Hewett at Cosmic Variance reports that the latest LHC commissioning plan will not involve collisions at 14 TeV until 2008, with earliest physics run in April 2008. First collisions will be in November 2007, but at Tevatron energies. Until recently the plan had been to try for first collisions at full energy next summer, and some had hoped that a bit of new physics data could be acquired in 2007, but it looks like that is not to be. For a summary talk on the situation, see here.

Comments

1. **Babe in the Universe**
   June 20, 2006

   Hello Peter:
   Your blog has long been a source of enjoyment for many people. Thank you for the support in the recent mini-controversy. You can see that I have gone far beyond those student papers. The slings and arrows haven't hurt at all. Bring it on!

2. **sunderpeeche**
   June 20, 2006

   Very rarely do accelerators achieve their top energy on the first pass (although w/ upgrades in *later years*, many have exceeded their original design energy). Especially with something as complicated as LHC, it will be a major feat just to demonstrate collisions and maintain a stable beam. Will all of the detectors be ready?

3. **JoAnne**
   June 20, 2006

   sunderpeeche: Both of the high p_T detectors (ATLAS and CMS) will be working good enough by Fall 07 to measure the interactions from the first collisions. Both, however, will be missing quite a few pieces (i.e., ATLAS will not have full muon coverage). It will take a long time to calibrate the detectors once the data starts pouring in.

   I have no knowledge of the status of LHCB and ALICE, but imagine they are in the same boat.

4. **Chuckles**
   June 20, 2006

   The Motls piece against Carroll is available via Google Cache. Very Impressive, I must say. So the argument is that the far left Carroll is basically the countertype of the far right Dembski – which makes Motl what exactly? A reactionary physicist pontificating about brains and sex, subjects in which he is eminently qualified, unlike Carroll and Woit pontificating about…ah, never mind.

   Cache 'em young:

   http://72.14.203.104/search?q=cache:n6ecH9UI44YJ:motls.blogspot.com/2006/06/sean-carroll-joins-peter-woit.html+&hl=en&gl=us&ct=clnk&cd=1

5. **absolutely**
   June 20, 2006

   Copy then paste the above link to your browser's address bar. If you click directly you will get a Motl intercept (worth a look though for a laugh).

6. **JoAnne**
   June 21, 2006

   Could someone please tell me how the comment thread to this post – with a subject matter of the LHC and hard science with real experimental data – degenerated into an irrelevant thread about Lubos Motl, Peter's book, and loop quantum gravity???

7. **Peter Woit**
   June 21, 2006

   Joanne,

   1. People love to post off-topic things here trying to turn the discussion away from the topic of the posting to whatever they would rather hear about.

   2. Unfortunately, there seem to be a lot fewer people out there who want to have an informed discussion about particle physics than would like to discuss Lubos or LQG.

   3. Last night I deleted some of the Lubos discussion and tried to see if WordPress had some way of moving the comment with the link to another posting but it doesn't seem to have a way of doing this. Considered deleting this, decided not to since some people might find it amusing or enlightening, even if off-topic. Went to sleep, thus unable to moderate continuing off-topic comments…

   Someday I should write up a FAQ with answers to recurring questions like "What about LQG?", no time now. Please, if you must discuss Lubos, use the recent thread devoted to him, and try and avoid posting comments unless you have something interesting to say about the topic of the posting. What many people continually do here is analogous to joining a discussion that has gotten going among a group of people and starting to go on about a different subject. It's rude.

   Back to the topic of the LHC. Anyone want to guess how long it will be from April 2008 until the experiments have enough data to say something interesting, understand their backgrounds, and are ready to go public with results of the data analysis? How long did it take in the case of the Tevatron?

8. **sunderpeeche**
   June 21, 2006

   Others may have better info on the Tevatron, but FWIW it started operations in 1983

   http://www.fnal.gov/pub/news/history_tevatron.html

It was initially a fixed-target machine (now it runs sometiimes as a collider and sometimes fixed-target). Antiproton beams were first circulated at Fermilab in 1985.

http://history.fnal.gov/brochure.html#IV

Don't know about fixed-target expts, but the first collider detector was CDF which started taking data in 1985 (D0 came later). One of the earliest significant physics publications from CDF was a precise measurement of the Z mass (1989), the result was competitive with LEP at the time.

http://prola.aps.org/abstract/PRL/v63/i7/p720_1

9. **Seth**
June 21, 2006

As a member of one of the experiments, something I would note is that this may be less of a delay in good physics results than it looks like. Even if the accelerator had started at 14 TeV CM energy, all the data from 2007 would likely have been used for detector commissioning. And many of those commissioning tasks can be done almost as well with 450 GeV beams.

10. **anonymous**
June 21, 2006

dear Seth,

can you explain to a theorist the reason behind this decision?
At "Cosmic Variance" it is said that it is safer to start with less energy stored in the machine, but why reducing the energy rather than the intensity of the beams, at the price of using magnetic fields much below their planned values?

11. **Seth**
June 21, 2006

I'm a few pay grades below management, but what it looks like Cosmic Variance says is that the much of the machine simply won't be ready to run at all full energy in time, or at least not fully checked out. (There are more ways to break an accelerator than simply storing too much total energy into it, I'm sure.) So they prefer to run at low energy rather than not running at all.

From the standpoint of the detector folks, this has positives and negatives; one positive I discussed in my last comment, but a major negative is we won't have as much time to get our detectors fully installed as if they delayed until 2008. The detector installation on the two major experiments is running on an extremely tight schedule still, even though there is some delay in when the experiments will be closed in the new LHC schedule.

12. **sunderpeeche**
June 21, 2006

To reach top energy the magnets must run at full magnetic field, regardless of how many particles are stored in the beam. This is especially risky/difficult when dealing with superconducting magnets.

13. **Fred Diether**
June 21, 2006

Peter,

The HyperCP collaboration had one of the biggest data set captured from experiments and it took them about 5-6 years to find the 214 MeV X particle. And they are still analyzing data. I would expect that LHC will have even bigger data sets than HyperCP. So it could be quite a few years before new physics is found if at all. Hopefully not though. 😉 They are definitely going to need a lot of data crunching capability.

Fred

14. **Tommaso Dorigo**
June 21, 2006

Hi all,

not only will CMS and Atlas be short of a few components, but – to me, more to the point – they will have to work a lot before they make sense of their triggers.

The delays in CDF physics somebody mentioned above were due to precisely this problem: triggering in a hadronic environment is a nightmare in its own right.

I am in CMS, but I do not care if the 2007 run is taken at 2 TeV, 2 GeV or 2 PeV. The data will not be useful for significant physics measurements. Rather, it will allow us to start making sense of our trigger primitives. Calibrations ? Calibrations come later. As was mentioned, W and Z boson physics measured by CDF was an early successful measurement, where that experiment beat the 630 GeV competition. But still, quite some time after the first shot.

Now flame me if you must, but I predict that no physics measurement will be published by CMS and Atlas before summer 2009. That includes the total inelastic cross section 🙂

Cheers,
T.

15. **sunderpeeche**
June 22, 2006

nothing in over 24h? it's curious how posts about expt hep generate so few responses, but something on theory generates volumes of the most appaling trash

16. **Peter Woit**
June 22, 2006

sunderpeeche,

Probably just too few experimentalists willing to spend their time slumming here among the theorists.

My tactic on the last posting of turning off comments and telling people to discuss on Cosmic Variance inflicted some of the usual suspects on them, forcing Sean Carroll (instead of me) to spend his time deleting people's off-topic comments. Maybe I should do that more often (sorry Sean…)

17. **JoAnne**
June 23, 2006

Peter: your prose is brilliant.

Sunderpeeche: it is disappointing, isn't it (the lack of interest in blog posts on experimental/phenomenological issues). I try not to let it get me down or stop me from posting, but sometimes….

Tommaso: I agree that caution is needed, given some of the particularly wild forecasts of discovering SUSY at the LHC in the first 3 minutes! However, there will be physics papers in 2008, I think. Even if they are only about Z/W or top production. I am betting the accelerator & detectors will work well enough to get at least these measurements out. Plus, if there really are blackholes at a TeV, or if there is a heavy gauge boson (or Randall-Sundrum type graviton Kaluza-Klein resonances) at 2-3 TeV, then we will know very quickly. Probably during the 2008 physics run. However, missing energy signatures or the Higgs to gamma gamma channel will most likely take a few YEARS, in my opinion. As you know, those signatures require exact calibration and knowledge of the detector and Standard Model background. So, the wait for new physics depends on what nature has in store for us!

18. **Marty Tysanner**
June 23, 2006

JoAnne,

Am wondering what your perspective is of the impact on grad students who are hoping to base a thesis on LHC data. Do you think the LHC schedule change will cause many experimentalists to delay graduation due to a need to wait additional time for data? Do you foresee many experimentalists switching to a different experiment as an alternative, or is that even a realistic option for those who have already committed themselves to the LHC?

19. **Chris Oakley**
June 23, 2006

> nothing in over 24h? it's curious how posts about expt hep generate so few responses, but something on theory generates volumes of the most appaling trash

Sunderpeeche seems to be forgetting that most of the readers here are theorists, former theorists or wannabe theorists. The lack of posts should just be interpreted as a quiet confidence that the experimentalists are doing a good job, and that lacking any specific expertise on the matter, many of us do not see the benefit of making comments.

But here is one anyway: IMHO text books with the emphasis on experiment – I'm thinking of Perkins' "Introduction to High Energy Physics" specifically here – do a better job at theory than most theory text books. They almost completely ignore QFT, doing all calculations with elementary quantum mechanics. Those who think that reproducing QM is not a requirement for a properly-formulated QFT should take note.

20. **Walt**
June 23, 2006

Jo Anne: The biggest mistake anyone can make about blogging is thinking that there is any relationship between interest and comments. People don't comment when they're interested; they comment when they're irritated. Lubos: irritating. LHC: not irritating. Guess what's going to draw the comments.

21. **JoAnne**
June 23, 2006

Walt – how true!

Marty – The effect on grad students working on the LHC will be small. Everybody expects new accelerators and detectors to be delayed and we are all delighted that this delay with the LHC is so *short.* It's only a couple of months!!! It could have been years….(of course we don't know how the actual start of operations will go). If the commissioning phase goes well, the students will get plenty of data for their theses during the 2008 run. Even just observing Standard Model processses such as gauge boson or top-quark production at energies of 14 TeV makes a great thesis! And, thanks for being concerned about the younsters.

This Week's Hype

June 22, 2006
Categories: Uncategorized

The Economist has an article this week entitled To catch a gravitational wave. It's about the proposed LISA satellite experiment designed to measure gravitational waves, with a much greater sensitivity than LIGO. According to the article, what would you guess is one of the main goals of the LISA experiment? Exactly, like most other ambitious experiments, it will solve the problem of how to test string theory:

could allow scientists to examine the validity of string theory, which says that there are more than four dimensions to space-time and that the extra dimensions are hidden. String theory has come under fire because its predictions have so far proved untestable. The normal version has it that these dimensions are curled up in strings that are smaller than the known elementary particles. However, in some versions strings form very long "superstrings" that stretch across the universe. These superstrings form loops and vibrate, radiating gravitational waves; they can also crack like whips, sending bursts of gravitational waves towards Earth. "Seeing direct evidence of strings would be as important as discovering that the world is made of atoms," claims Craig Hogan, an astronomer at the University of Washington, who is a member of the international science team for LISA.

The writer appears to be a bit confused about what a superstring is, guessing that it is a really big string (a cosmic string). This is presumably all based on the idea promoted by Joe Polchinski that it is in principle possible to come up with superstring theory models with cosmic scale superstrings, whose effects would be visible through gravitational lensing and gravitational waves. As far as I can tell, this is just another case of the phenomenon that one can get pretty much anything one wants out of string theory, and there's no reason at all to expect cosmic strings with just the right properties to have been invisible so far, but visible through gravitational wave effects measurable by LIGO or LISA.

Two years ago there was a press release about this from UCSB quoting Polchinski as saying

the gravitational signatures from cosmic strings are remarkable because they are potentially visible even from the early stages of LIGO! That means 'potentially visible' over the next year or two.

LIGO hasn't seen anything, so time was up for this nearly two weeks ago but I haven't noticed any UCSB press releases reporting that things haven't worked out.

There was some excitement a year or so ago when a group claimed that an astronomical object might be a single galaxy lensed by a cosmic string. Turned out to just be a pair of nearby galaxies.

LISA is tentatively scheduled for launch nearly ten years from now, so it will be a while before this particular "test of string theory" brings in any results. This past week the 6th Annual International LISA Symposium was held in Maryland.

Comments

1. **sunderpeeche**
   June 22, 2006

   Just like expt HEP (or even before HEP), it takes years for astronomy to build a new telescope/detector. I don't blame the science writers for writing about strings (cosmic or super). It does not matter. What matters is that LISA actually gets built and launched. When the results come out, they will be analyzed on their own merits, whatever may have been said initially about any prospective discoveries.

2. **Peter Woit**
   June 22, 2006

   sunderpeeche,

   Scientists shouldn't spout nonsense to non-scientists in order to justify funding their experiments. Besides being dishonest, its threatens their credibility. If the cosmic string hype had been used as the main justification for funding LIGO, before coming up with the money for LISA, funding agencies might very well start asking about what happened to the cosmic strings ("you mean we gave you X milliion dollars for LIGO to test string theory, now you've spent it and learned nothing about string theory, but are asking for more for LISA to test string theory???")

3. **sunderpeeche**
   June 22, 2006

   A friend of mine (i.e. not I) worked at Daresbury Lab in the UK, and they gave a tour to journalists, about the van de Graaf and the synchrotron. Next day there appeared an article about the "world's tallest synchrotron". Scientists say what they hope to find, and the string theory people will hope for strings.

4. **Kea**
   June 22, 2006

   From Padmanabhan's new paper :

   There is more to gravity than gravitons.

5. **Peter Woit**
   June 22, 2006

   Kea,

   Please, don't get a discussion going here of alternatives to GR. It more or less immediately becomes too depressing for me to try and moderate.

6. **Kea**
   June 22, 2006

   Sorry.

7. **Matt B.**
   June 22, 2006

   The last part of your blog post is missing, you know, the subjective part. What would your ideal press release for LISA look like?

8. **Thomas Love**
June 22, 2006

Peter said: “Scientists shouldn’t spout nonsense to non-scientists in order to justify funding their experiments. Besides being dishonest, its threatens their credibility. ”

But they do and even nonscientists have seen through it.

“Are researchers over-hyping their results, and prematurely, in order to impress the grant-giving bodies? And are they using ‘jargon’ to keep hold of their special status as a secular priesthood and excommunicate the rest of us?”—from “Mad, Bad and Dangerous?; the Scientist and the Cinema” by Christopher Frayling

Grant-giving bodies are impressed when scientists seem to agree on one theory. So funding thrives on conformity. The search for truth thrives on diversity.

9. **woit**
June 22, 2006

Matt,

Most of the article about LISA was fine, I just think the scientists involved shouldn’t be telling the press that the thing will “test string theory”, because it’s just not true.

10. **Tony Jackson**
June 23, 2006

BBC Radio 4 has a programme called “Start the Week” in which guests from the humanities, politics and science discuss issues great and small. Over a year ago, Michio Kaku was a guest and talked about his then new book “Parallel Worlds”. You can hear this programme at:

http://www.bbc.co.uk/radio4/factual/starttheweek_20050131.shtml

Kaku waxed all theological about the cosmological implications of string theory (I think), and added gems like: “we physicists are the only scientists who can use the word God and not blush”. About 13 minutes into the recording, one of the guests brought up the awkward problem of experimental verification, at which point Kaku emphatically mentioned that the LISA satellite would provide the necessary data. I’d be interested to hear what people from this blog think of such comments.

11. **csrster**
June 23, 2006

I think they should invite Peter onto Start The Week.

12. **Troublemaker**
June 23, 2006

People from this blog, and from most of the theory community, know that whatever Kaku says is a steaming pile of baboon crap.

13. **anti-Troublemaker**
June 23, 2006

Dear Troublemaker,

Kaku is entertaining. If you critics are right, and things are less complex than the extradimensional landscape and cosmic strings, then physics will be boring.

Better change your scientific attitude. This is a difficult process, so go gently. Start off training yourself to be a pseud by believing one lie each morning before breakfast.

Gradually build up your stamina until you can force yourself to believe six hoaxes a day:

Alice laughed: “There’s no use trying,” she said; “one can’t believe impossible things.”

“I daresay you haven’t had much practice,” said the Queen. “When I was younger, I always did it for half an hour a day. Why, sometimes I’ve believed as many as six impossible things before breakfast.”

– Alice in Wonderland.

Reading that book inspires string theorists (according to testimony in the book “Warped Passages”).

14. **Christine Dantas**
June 23, 2006

Kaku emphatically mentioned that the LISA satellite would provide the necessary data.

For those interested, I am keeping a post on papers related to the predictions of quantum gravity about the gravitational wave phenomenon. More papers will be added opportunely.

Best wishes,
Christine

15. **D R Lunsford**
June 23, 2006

sunderpeeche,

You are mistaken – just building and running instruments is a worthless endeavor if their only purpose is to reinforce preconceived notions (witness WMAP 3).

-drl

16. **sunderpeeche**
June 23, 2006

Indeed yes, if the real purpose of building an instrument (accelerator, telescope, …) is just to have some machine in the lab, to keep the staff employed, then that is worthless. There has to be some scientific goal, and typically those goals are formulated in terms of the theoretical prejudices of the day. Today it's strings. Don't know about WMAP 3 ~ they tried to probe the CMB in more detail, and presumably new instruments will keep doing so. Will LHC be worthless if it finds a SM Higgs and nothing else?

The nuclear power and fusion industry does offer an example of overhyped promises, though. Today the knee-jerk reaction is to be suspicious of any new nuclear power plants. It may become so with particle accelerators. (It has already become so?)

17. **tsg**
June 23, 2006

sunderpeeche wrote: "Will LHC be worthless if it finds a SM Higgs and nothing else?"

I posted this comment in response to "LHC predictions at Seed", but a little too late to get a reply. But now that the topic has come up again.... Wouldn't it be significant if LHC *doesn't* find superpartners, as evidence *against* super-symmetry at low enough energies to solve the hierarchy problem (which I thought was one of the main arguments for believing in supersymmetry in the first place)?

That doesn't seem analogous to LISA looking for cosmic strings, though, since I don't think anyone is really expecting to see any. If a negative result doesn't make anybody question the theory (who wasn't questioning it already), then it can't be called much of a "test".

18. **Peter Shor**
June 23, 2006

I don't think this has been mentioned in this blog before (apologies if it has), and it's related to LIGO. The gravitational waves resulting from two black holes colliding can now be calculated! I have heard that in the last year, several groups, including one at NASA, have figured out how to overcome the numerical instabilities and simulate black hole collisions. You can google and find some news reports and a really neat video from NASA. I can't really find anything about what this means for LIGO and LISA, though.

19. **Peter Woit**
June 23, 2006

Peter,

From what I've seen the magnitude of these waves is such that for them to be detectable by LIGO, the black holes would have to be relatively nearby, so much so that no one really expects to LIGO to see such a signal while it is running. Don't know about LISA…

20. **Krotos**
June 23, 2006

D R Lunsford: "You are mistaken – just building and running instruments is a worthless endeavor if their only purpose is to reinforce preconceived notions (witness WMAP 3)."

I think calling WMAP worthless is a bit strong (disclosure: I was a co-author on a few WMAP-related papers). It's given significantly improved bounds on various cosmogical parameters and on things like neutrino masses.

However, I do see and agree with your broader point. I remember the NASA press release calling WMAP results "stunning" and "one of the most important scientific results of recent years." I'm sorry, and I intend no disrespect to the WMAP team, but that's hype. It was a fabulously well-done experiment, and it did give rise to some mysteries such as the low quadrupole moment, but it essentially confirmed the broad details of what was already suspected. It certainly wasn't comparable to, say, Rutherford's experimental discovery of the atomic nucleus in the sense of being a revolutionary, paradigm-shifting result.

I was in early-Universe cosmology for several years, and one of the reasons I switched fields was that I was starting to see the same factors that have distorted particle physics — primarily, too much theory and not enough data (and secondarily, increasing amounts of starry-eyed hype that give a misleading impression of how certain or complete the science really is) — appear in cosmology. It's not nearly as severe, of course, and in most respects it's still a very healthy field. But how are we going to, say, get more than a very rough idea of the inflaton potential, let alone make controlled measurements of an inflaton or a dark matter particle in a lab? I have a nagging feeling that CMB and gravity wave measurements, though important and significant, just won't be enough to give the kind of experimental verification to these theories which is utimately needed in science, and I'm not sure the technology that would allow it will be here any time soon.

(My apologies to Dr. Woit if this is getting too off-topic.)

21. **Moshe**
June 23, 2006

Peter(s): Frans Pretorius from Caltech (formerly UBC) gave a very nice colloquium at PI, available here , worth checking out.

(also experimenting with html tags, let's see if it works)

22. **Moshe**
June 23, 2006

Not quite… in any event it is on page 2, and the title is "Simulation of Binary Black Hole Mergers".

23. **Nick**
June 24, 2006

P. Woit Said:
…for them to be detectable by LIGO, the black holes would have to be relatively nearby, so much so that no one really expects to LIGO to see such a signal while it is running. Don't know about LISA…

If anyone wants a figure for how nearby, I could shed some light, I was a undergrad researcher at LIGO Hanford during a previous summer. LIGO often measures the detector sensitivity by the range at which it would detect a "typical" neutron star binary inspiral. The LIGO Hanford 4K interferometer currently runs at about 13Mpc. The Virgo cluster is about 15 Mpc away, which is a good place to expect a good number of inspirals.

Of course, Advanced LIGO (the upgrade which will start in the next few years ~07 or 08) will see 10 times farther, and LISA won't be up until at least 2015, so my money is on LIGO.

24. **FT reader**
June 24, 2006

"From what I've seen the magnitude of these waves is such that for them to be detectable by LIGO, the black holes would have to be relatively nearby, so much so that no one really expects to LIGO to see such a signal while it is running. Don't know about LISA…"

LISA is guaranteed to see gravitational waves, at least from white dwarf binary systems in our galaxy. So many in fact, that they cannot be resolved and will form one of the important sources of "noise" (see e.g. gr-qc/0204090).

25. **Juan R.**
June 27, 2006

sunderpeeche said,

Scientists say what they hope to find, and the string theory people will hope for strings.

No, there is an etical code for scientists is sistematically violated by string theorists. These sistematic unethical news are not common in the rest of normal science.

It is so unethical to claim that string theory is falsable as claim that string theory predicted gravity, when is even unable to reproduce GR results from *first* principles.

Juan R.

Center for CANONICAL |SCIENCE)

NEW in the WSJ

June 23, 2006
Categories: Not Even Wrong: The Book

Today's Wall Street Journal has an article by Sharon Begley entitled Has String Theory Tied Up Better Ideas In Field of Physics? (sorry, subscription required for on-line version). The summary of the article goes

After two decades in which string theory has been the doyenne of best-seller lists and the dominant paradigm in particle physics, some critics say it may be tying up better ideas.

and in some sense it covers for the public in simplified form the discussion going on over at Cosmic Variance at the moment. It quotes Lee Smolin and me as critics, with Mike Peskin as the only defender of string theory. Peskin acknowledges the compactification problem, and then makes only a strikingly weak argument for string theory: that it can claim some success because it explains the number of generations. This "explanation" in terms of the topology of the Calabi-Yau is no explanation at all, since you can get any value for this number you want. I don't see how changing the parametrization of your ignorance from that of a single natural number to that of the topology of a Calabi-Yau is any improvement.

I assume that any moment now Lubos will be producing a rant comparing Sharon Begley to one sort of animal or another.

There's also a long review of Not Even Wrong from John Walker over at his blog called Fourmilog. He does a good job of laying out the controversial argument of the book, and we seem to be mostly in agreement, although I don't share his conviction that government funding is a major source of the problem.

Update: I just heard that official publication date in the US is September 8, although books should be for sale a bit earlier than this. Lubos has a posting about the WSJ article, less of a rant than some of his recent ones. He has restored his anti- Sean Carroll screed, which was a classic that many people were sorry to see suppressed.

Update: A commenter points out that the WSJ article is available on-line here. There's also a mention at Slashdot. The extensive discussion there is heavily anti-string theory which is an interesting sign of the times, but also convinces me that the idea of moderating this kind of discussion by having participants vote on which comments are worthwhile just doesn't work at all.

Update: There's commentary on this from science writer David Appell here, who pretty much gets the situation right.

Comments

1. **QWERTY**
   June 23, 2006

   DEAR PETTER,

   ARE YOU ABLE TO RELEASE SALES FIGURES FOR YOUR BOOK? THIS WOULD SHOW YOUR CRITICS THAT THEY ARE THE IDIOTS FOR DOUBTING.

   SUPERSYMMETRY IS A COMPLETELY SILLY IDEA IN PHYSICS, THERE IS NO EVIDENCES AND IT IS WORSE THAN THE EPICICLES. BUT IT IS A SHAME THAT IT IS SO USEFUL IN MATHEMATICS. WITTEN AND KAPUSTING HAVE BEEN USING IT IN THEIR GEOMETRIC LANGLANDS PAPER WHEN THEY TALK ABOUT THE MODULI SPACE OF THE N=4 SUPER YANG-MILLS EQUATIONS OVER RIEMANN SURFACES.

2. **woit**
   June 23, 2006

   QWERTY,

   STOP USING ALL CAPS!

   I don't know sales figures for my book, and from what I hear publishers tend to be cagey about these numbers with authors (since they determine the size of the check they have to write). I've also heard that they way the book business works, bookstores can return unsold books. So, you don't know how many books you've sold until you see how many unsold ones come back, which can take quite a while.

   And, whether the book sells well or not has nothing to do with whether I or my critics are right. Whether a book has a good argument or not in it doesn't necessarily correlate with how it sells (some would argue that the these things are inversely correlated…).

   Supersymmetry is very useful in math, and this is not a shame, but a very interesting phenomenon, the reasons for which I think we still don't completely understand. When we do, we'll have learned something very important.

3. **Doug Natelson**
   June 23, 2006

   FYI, Lubos has re-edited his post re: Sean Carroll; at least, the section quoted my blog has been changed. The basic point remains unaltered, though.

4. **woit**
   June 23, 2006

   Off topic bickering about background independence deleted. Please don't do this unless you have something completely new to say on the subject and it actually has something to do with the posting.

5. **Garrett**
   June 23, 2006

   Hey Peter, congratulations, you just got slashdotted:

   http://science.slashdot.org/article.pl?sid=06/06/23/2226257

   Perhaps fortunately, they didn't link directly to NEW, or your machine might have melted…

6. **nukular**

June 24, 2006

As a PhD (pure qft) physicist, I was a little curious about this book after a blurb on slashdot. It seems reaonable then that I would browse my way to the Amazon webiste, only to read Lubos' "criticism" and find myself scratching my head. I have to admit this is the first time that I have decended into this pit but his comparision of string theory to the modern theory is WAY over the top. While not an expert at either string theory or evolution I know enough about both to make an educated statement or two, but the fact is that the evidence for the modern theory of evolution is overwhelming! Comparing the any of the actors in this drama Demski is quite inflamatory and completly inappropriate. I expect better…

7. **ks**
June 24, 2006

It's not really funny to notice that besides the notorious Lubos all kinds of freelancing witch-hunters and exorcists are around to crack "crackpots" in science and accuse them for having "weird", "silly" and "abnormal" ideas. In the 70s and 80s philosophers like Paul Feyerabend accused science to be like the catholic church i.e. repressive, dogmatic and hierarchical. Now one might get the opposite impression that the research frontier becomes ever more splitted into antagonistic sects following one or the other set of speculative ideas and a bunch of millenaristic preachers whith or rather without charisma. Paul Feyerabends "anarchism" was likely mentioned to be some multicultural tolerance. Now it turns out to be the exact opposite: a late medieval hystery and persecution of alteration. Unfortunately there is no "age of reason and progress" at the dawn because this is our own imaginary scene, where we are still living in.

8. **Who**
June 24, 2006

Sharon Begley's WSJ article is available online as a reprint in a Florida paper
http://www.nwfdailynews.com/articleArchive/jun2006/notevenwrong.php
I haven't compared this with the original in the WSJ

9. **John A**
June 24, 2006

I've made my comment to Lubos thus:

> If you're deliberately trying to make Peter Woit look even more sane and string theory ever more implausible by making hysterical ststements such as these, then you're going the right way about it.

I cannot claim advanced knowledge of physics, but I do know some philosophy of science. If string theory cannot make testable predictions then it is not a scientific theory, regardless of the intelligence of its proponents, how beautiful its math, how wonderful its consistency. What has bothered me for a good while is that quantum gravity appears no more achievable a unification than it did 20 years ago.

I heard the same criticism levelled at Steve McIntyre on ClimateAudit.org regarding his finding that large numbers of the better known reconstructions of past climate are statistically meaningless and based upon unphysical assumptions (a finding recently upheld by a specialist panel convened to look at these issues). The retorts back have been (in no order of particular precedence): to compare Steve (and myself) to creationists, Holocaust deniers, to suggest that if they don't agree with a "scientific consensus" they must be deranged or paid to shill for a shadowy right-wing fossil-fuel funded conspiracy, and so on, that we must be crackpots, that we must be jealous, that we must believe in fairies or ghosts.

Now, that isn't to say that Steve McIntyre is vindicated because he is villified – but simply that the reaction to Peter Woit's book and views are strikingly similar.

Oh and the repeated refrain of "if you're so smart why don't you do a proper reconstruction using tree rings" as if it was all a slip between measurements and calculations.

Please, I don't wish to align Peter Woit's beliefs about one subject with my own about another as if they were equivalent or even that Peter Woit agrees with me(and I'm sure people reading this will have strong views one way or another), but surely the point is that showing that a widely used method is invalid and a derivation mistaken is every bit a part of the scientific method as discovering a new particle or an explanation for a new experimental result. Steve McIntyre has written that in a sense, what he has done is entirely negative, in showing that popular methods for climate reconstruction are invalid, without offering an alternative method.

I find the invective between what Lubos has written about Peter and what has been written about Steve and myself on occasion to be spookily familiar.

For those of you who have read the books or the weblog of James Randi know that he receives the greatest diatribes from the most intelligent people who have convinced themselves of notions which are ridiculous (like the idea of Uri Geller bending spoons with his mind). I'm glad to say that in his 70s Randi still delights in demonstrating that the most intelligent people on the planet can be fooled by trickery and easily deceive themselves.

I've no doubt that Lubos Motl and Edward Witten are highly intelligent people, but what should always be remembered is that that very intelligence can lead to foolishness, as James Randi is fond of relating.

I will buy the Peter's book (and "The Elegant Universe" as well) so as to get a rounded perspective on this controversy.

I will add that no scientist I've ever met, nor corresponded with, nor read about has been orthodox in everything. All of them had had foibles, weird ideas and made fantastic leaps of illogic on occasion. It appears to go with the territory.

10. **John A**
June 24, 2006

Oh and Lubos has deleted my comment above, and remarked that:

> … I would really appreciate if he stopped nuking my blog articles about theoretical physics – which he does not understand a single bit – with nonsensical and emotionally charged reactions.

It would appear that I have inadvertently joined a select club.

11. **Isaac**
June 24, 2006

I have to correct your misunderstanding of how Slashdot moderation works. Officially, you can't both moderate a discussion and participate in it, though undoubtedly some people get around this with multiple login. And the whole body of users doesn't moderate: moderators are chosen randomly from a pool of "normal" users.

Which is not to say that the Slashdot moderation system works all that well. But it's rather more than just people voting on who's most worth hearing.

12. **Joseph**
June 24, 2006

that explains why “Some people really get tied in a knot about stuff like this.” is the most popular comment on this story.

13. **J.F. Moore**
June 24, 2006

“…the idea of moderating this kind of discussion by having participants vote on which comments are worthwhile just doesn’t work at all.

If the signal-to-noise ratio is low enough, using any filter becomes pointless.

14. **Benjamin**
June 25, 2006

I have a basic question about scientific methodology. One of the main criticisms of string theory is that it has not not produced a prediction which can be falsified by experiment. But suppose it does, and that prediction is falsified. That is not necessarily the end of the theory (or any other theory). To be fair, the protagonists should have a chance to correct any flaws in their theory, perhaps in light of the new experimental discoveries. That may take time, perhaps quite a bit of time. It seems to me there is never a definite cutoff point for any theory, though as time passes the theory will slowly fade away, unless it mends its ways or a better one comes along. This may seem basic but I thought it was worth mentioning.

15. **Thomas Larsson**
June 25, 2006

Benjamin, something like that has happened already. During the quarter century which I have been seriously interested in particle physics, at lot of people have promised that the experimental discovery of SUSY is just around the corner. It hasn’t happened. Many experiments have tested various natural signals for SUSY – sparticle detection, light Higgs, proton decay, WIMP detection, permanent electric dipole moment, muon g-2, B_s oscillation, and probably several others that I don’t know about. The problem is that all these experiment have produced null results. Also, my impression is that people think that the Tevatron should have seen something weird by now, had SUSY been there in the first place. So you might say that SUSY is almost disproven already.

As Witten stated repeatedly for 20 years, string theory predicts SUSY. Thus string theory is already having serious trouble with experiments. But when a lot of people have invested entire careers in a program, experimental failure is simply not going to be accepted. It took a long time for H A Lorentz and other aether theorists to accept the Michelson-Morley experiment, Geoff Chew and Fritjof Capra still seem to believe in the analytic bootstrap, and string theorists employed the ultimate excuse for denying experimental facts: the anthropic principle.

16. **woit**
June 25, 2006

In many cases, a theory that makes a wrong prediction can be fixed by making it more complicated. Falsification is not so simple; it often works by forcing the people who believe in a theory to make it more and more complicated, with true believers never giving up, but most people losing interest in the theory because it has become a useless mess.

This is completely off-topic, since string theory does not make a single prediction, so it hasn’t yet even reached this state. One could argue though that it already has reached the end-point of this process: just to get it to agree with what we already know about the world, its proponents have been forced to turn it into a useless mess (aka the anthropic string theory landscape).

17. **Ponderer of Things**
June 25, 2006

I find it rather remarkable that Lubos retracts a post, and reposts it after a few days of thinking about it.

It shows that he has limits (maybe he is not completely “out there”).

I also feel that the tide has been turning in Peter’s favor lately (the past year or so, compared to pre-2005 situation). I think blogosphere has more to do with it, rather than lack of results from string theory.
The style of argument that we get from Lubos vs. the style of argument from Peter Woit and Lee Smolin may be another reason. People in general and physicists in particular have developed BS detector – so even without getting into details about Calabi-Yau manifolds and multiverses, one can usually tell who is bluffing and who is arguing honestly. It’s like Bill O’Reilly vs. George Clooney, I can tune in half-way into the argument, but the argumentative “bully” style vs. the reasoned and polite voice of conviction tells me everything I need to know.

18. **R. Avry Wilson**
June 26, 2006

Dear Peter,

I wasn’t sure exactly where to post this comment on your blog, so thought to put it here – if that’s ok.
—–

Thank you for writing this book. Mere moments after I first came into contact with string theory I recognized its innate failures and ‘endless’ possibilities. Vast, complex, and untestable mathematics is (to me – I don’t wish to offend anyone with the following analogy) as much a waste of intelligence as solving the trillionth prime number, i.e. what exactly is the point of carrying out work that in the end doesn’t really do anything to move us along? My primary concern is why it took so long for someone (anyone) to take a stand against the theory(-ies); to me, it screamed ‘obvious’ to the extreme of pedantic physics. I’d expected string theory would have been dinosaur-bound by 1980.

From where I stand, the problem has always been simple: limit components to 3 dimensions in all of physics. I remain boggled by any theory that uses multiple dimensions; using unreality will not define reality – ever.

Kind Regards,

R. Avry Wilson

19. **ObsessiveMathsFreak**
June 26, 2006

One comment in the Slashdot discussion really leaps out of the page.

> Extra dimensions are the epicycles of Modern Physics

It's a very bold statement, and in many ways, has a lot of validity. The physics community owes it to itself to respond to this statement. In what way are extra dimensions fundamentally different from the old theory of epicycles?

20. **Riofrio**
June 26, 2006

    Of course they are epicycles, as are dark energy and inflatons. Does anyone remember the Emperor's New Clothes?

21. **Chris Oakley**
June 26, 2006

    > In what way are extra dimensions fundamentally different from the old theory of epicycles?

    Well, here's one way: you can calculate things that agree with experiment using epicycles.

22. **Aaron Bergman**
June 26, 2006

    Well, besides the fact that they have absolutely no similarities to speak of, you mean?

23. **Chris Oakley**
June 26, 2006

    Not only that, I've done it. The Tibetan and (old) Hindu calendar are based on circular lunar and solar orbits with a a single epicycle superimposed. This is used to calculate their calendar, and yes, it is not especially accurate, but it is OK.

24. **sunderpeeche**
June 26, 2006

    String theory may go the way of phlogiston and the aether. Both concepts became more and more elaborate (phlogiston had to have negative weight), until something better simply displaced it. But in both cases, there had to be (and there was) expt data (which the theories could only explain by adding more twists to the formalism, until they became untenable). Ultimately, one needs expt (HEP) data, nothing else will displace ST (or LQG or anything else).

25. **JPL**
June 26, 2006

    I am afraid the analogy between epicycles and extra dimensions is misleading, since epicycles at least had some predictive power (you could compute observed orbits from them with some effort) and that is surely part of the reason that they lasted 1500 years or so. Extra dimensions, on the other hand, don't seem to predict much that has been observed!

Yarn Theory

June 25, 2006
Categories: Uncategorized

A couple people wrote in this morning to tell me about today's Doonesbury, which features slacker Zipper Harris (who has a blog) trying to impress a woman he last saw in sophomore year of college. He's still undecided about his major, but tells her:

I'm thinking physics. Yarn theory.

Comments

1. **MathPhys**
   June 25, 2006

   He should do M(otl) Theory.

2. **Lubos Woit**
   June 25, 2006

   like sweater yarn?

3. **Jason**
   June 26, 2006

   Try to google "Yarn theory", you'll get this:
   http://www.google.com/search?hl=zh-CN&q=Yarn+theory&btnG=Google+%E6%90%9C%E7%B4%A2&lr=

4. **Bert Schroer**
   June 26, 2006

   No, it's like sailors yarn.

5. **Kea**
   June 26, 2006

   While we're yarning coarsely…maybe you know this already, but the BBC now has a blog!

6. **Lubos Woit**
   June 26, 2006

   looks like a glorified editorial and column section.

7. **Jeremy**
   June 29, 2006

   What about radio zoology?

Talks at Strings 2006 Now Available

June 26, 2006
Categories: Uncategorized

Slides from the talks at Strings 2006 are now available. I've spent a little while looking through them today, and am sorry I don't have time to say much about them. Lots of more or less the same thing as in earlier years, and many talks devoted to complicated arguments designed to, as Eva Silverstein writes, populate, probe or constrain the Landscape. Nothing remotely like a plausible idea about how to ever get a prediction of anything out of this, but also virtually no anthropic arguments. I assume this was due to the iron fist of organizer David Gross.

I was pleasantly surprised to see that there were quite a few mathematically very interesting talks. Besides many talks of the sort that have been common in recent years dealing with the topological string, there were several that had nothing to do with string theory, but involved interesting mathematics related to QFT, and many of these had to do with work Witten is involved with, so they may get some attention. These were:

1. Witten's own talk on Gauge Theory And The Geometric Langlands Program.

2. Kapustin' talk on the same topic, entitled Topological reduction of supersymmetric gauge theories and S-duality.

3. Gukov's talk on Surface Operators in Gauge Theory and Categorification, where he mentions that some of what he discusses is based on on-going joint work with Witten.

4. Nikita Nekrasov's talk on Beyond Morse Theory.

So, as far as physics goes, the organizers are not allowing any talks on alternatives to string theory (the only mention of LQG seems to have been Dijkgraaf's making fun of it), but they are willing to allow mathematical talks on QFT that are not related to string theory, especially since this is the field that Witten seems to be doing a lot of work in.

Comments

1. **Thomas Love**
   June 26, 2006

   Peter wrote: "organizers are not allowing any talks on alternatives to string theory". My first thought was "typical Chinese censorship", but then I remembered the string motto: "There are no alternatives".

2. **Aaron Bergman**
   June 26, 2006

   Nobody called this a conference on "quantum gravity". It's a conference on string theory (and perhaps more generally, those things people who call themselves string theory choose to work on). I'm don't see why there should some sort of moral obligation to have talks on alternatives. If the "Loops" people want to have talks about whatever, they're certainly welcome to, but that's not the point of this particular conference.

3. **sunderpeeche**
   June 26, 2006

   It makes sense that if the conference is "Strings 2006" then the organizers would not allow talks on, e.g. condensed matter physics or biophysics, etc, so "no talks on alternatives to string theory" doesn't surprise or bother me. What would be relevant is, one day, if some testable prediction is made (~on extra dimensions), and a talk is presented offering a null result (~prediction is falsified) — if such a talk were disallowed then that would be serious.

4. **woit**
   June 26, 2006

   sunderpeeche,

   There's never been or ever will be any danger that a testable prediction will be made at a Strings 2006 conference.

   Aaron,

   The organizers can do whatever they want, but given the sad state of affairs of string theory in recent years, I'd think it would be advisable for them to deal with alternatives in some other way than by making fun of them. I'm quite glad to see that they're having talks on alternatives in mathematical physics, that's great.

5. **Jason**
   June 27, 2006

   Thomas Love,

   Hey, this is a academic conference about string theory! Please, do not intend to relate everything to the "Chinese censorship". Situation in China is not that bad.

   Anyway, I found those talks on mathematical physics very interesting. I think this is great.

6. **ObsessiveMathsFreak**
   June 27, 2006

   > Hey, this is a academic conference about string theory! Please, do not intend to relate everything to the "Chinese censorship". Situation in China is not that bad.

   I don't know. The party line seems to have everyone all strung up. And no doubt Chairman Gross' firm stance on religious debate had more than a few dissidents taken away in knots.

7. **sunderpeeche**
   June 27, 2006

   Consider a conference on "alternative energy resources". Suppose someone came up with an idea to process crude oil more efficiently, to get double the quantity of gasolene per unit of crude oil, effectively doubling the world's gasolene reserves. Would such a talk be accepted at the conference? I would guess not. But there would be other places to present such work. So it is that there are other conferences to present significant non-string theory research. What about the Lattice 200x conferences? I doubt they would accept papers on subjects having nothing to do with lattice QCD ~ e.g. string theory. Why should they?

8. **Eli Rabett**
June 27, 2006

I've been to many topical conferences where there were one or two invited talks on related fields, the point being to keep people up to speed on what is going on around them, especially new developments, and to relieve the monotony of the landscape.

9. **Peter Woit**
June 27, 2006

sunderpeeche,

Taking a look at the proceedings of a randomly chosen Lattice 200x (Lattice 2000), I note that one of the plenary talks is "Recent Developments in Superstring Theory" (see http://arxiv.org/abs/hep-lat/011073) by Ashoke Sen. From what I recall it has not been uncommon for the Lattice XXXX conferences to have summary talks about developments in string theory, although perhaps less so the last few years as most theoretical physicists have lost interest in the subject.

10. **sunderpeeche**
June 27, 2006

Good. It would be good if the String xxxx confs also had summary talks about developments in other fields. Strings 2005 had a talk on developments in Cosmic Strings (Polchinski), which is not quite the same as ST. Depends on the organizers.

11. **Who**
June 27, 2006

I've been to many topical conferences where there were one or two invited talks on related fields, the point being to keep people up to speed on what is going on around them, especially new developments, and to relieve the monotony of the landscape.

By way of illustration, Loops '05 scheduled two invited string talks in plenary session and a third talk in parallel session. The first two were by Dijkgraaf ("Quantum geometry and topological strings") and by Thiesen ("Gravity from String Theory"). The third, by Dorothea Bahns, was more narrowly focused ("The Invariant Charges of the Nambu-Goto String").

12. **Matthew**
June 27, 2006

From what I recall it has not been uncommon for the Lattice XXXX conferences to have summary talks about developments in string theory

Well, the ones I was at had "Lattice" SUSY/string talks, but nothing specific to string theory that was non-lattice related. It's common practice to have one or two experimentalists give plenary talks, and one talk on high energy theory, such as Zoltan Ligeti's talk on CKM physics at lattice 2005.

although perhaps less so the last few years as most theoretical physicists have lost interest in the subject.

It's more that lattice people tend to have slightly lower energy things in mind (like hadronic decay constants). There is a fair fraction of the community that does SUSY/Strings/"formal" QFT type though. See Neuberger's talk at lattice 2005, for example.

13. **sunderpeeche**
June 27, 2006

I just saw this. Has it been mentioned elsewhere? Apologies if so. (Smiting dragons?) "Wrong" "completely wrong" "not even wrong" … I had not heard of the first two, only the last.

http://www.movementarian.com/2006/06/12/peter-woit-smites-dragons-during-the-day-and-superstrings-at-night/

14. **Kea**
June 28, 2006

I finally had a look at these talks. I thought Gukov's was a nice introduction to the link between String theory and recent work on Khovanov homology in the paper by Pfeiffer and Lauda.

Responses To WSJ Article

June 27, 2006
Categories: Not Even Wrong: The Book

The lack of any serious (Lubos doesn't count) response to my book from string theorists since its appearance in the UK a month or so ago has begun to surprise me a bit. I was also suprised at how weak the defense of string theory was that the Wall Street Journal's Sharon Begley put in her recent article. It seems that she did talk to some string theorists, but couldn't get much usable from them (one supposedly told her that the best argument for string theory was the anthropic landscape). Peskin's argument that string theory's biggest success is its "explanation" of the number of generations was evidently the best he could come up with, although it's obviously very weak.

I've heard that the WSJ has gotten some correspondence about this, with Jacques Distler writing in to complain about the article. In one letter from him (also signed by his two collaborators), he claims that Smolin is wrong to say that string theory is not falsifiable, since Distler has a recent paper called Falsifiying String Theory Through WW Scattering .

This paper was discussed extensively here. You can make up your own mind about it, but it's undeniable that the calculations in the paper don't involve string theory at all (Distler's two co-authors are not string theorists). Pretty amazing trick to show that a theory is falsifiable without actually using the theory at all.

There are two obvious problems with the claim in the title of the paper, the first is that when one says a theory is falsifiable, one is talking about the characteristic predictions of the theory, and that's not what the paper is about. The second is that "string theory" is an ill-defined term, and many versions of "string theory" don't satisfy the assumptions of the paper (one of the co-authors admits this in the comment section). To fudge his way around this, in the letter to the WSJ, Distler refers to "the canonical definition of string theory" as opposed to "string theory", although he provides no reference to what this is. Putting "canonical definition" and "string theory" into Google doesn't turn up anything relevant.

It will be interesting to see if a referee can be found who will go along with allowing the "falsifiying string theory" claim. Most physicists, string theorist and not, that I've talked to about this think it's way out of bounds. It seems to me pretty amazing that Distler would choose to take this case to the Wall Street Journal.

We'll also see if the WSJ publishes the letter, if not I've asked one of the co-authors if they'll let me publish it here.

I could certainly do a better job of defending string theory than the people Begley talked to. The strongest argument string theorists have is clearly the one that they have "the most promising approach to quantum gravity". The problem with this is that there are plenty of people who disagree, especially those who do LQG. This is the reason that Distler has been on an anti-LQG campaign throughout the blogosphere recently. His latest posting is about this, with comment section featuring the always incoherent Lubos Motl, and the trademark Distlerian sarcastic sneering at people he disagrees with, e.g. the following comment on a paper by Thomas Thiemann:

I suppose that it is only Thomas's natural modesty that prevented him from submitting this paper for the Clay prize.

In the first comment, someone quotes from my book something I have to say about the issue being discussed in this posting. For the record, I'm no expert on LQG, and can't judge exactly how close they are to having a fully satisfactory quantum gravity theory, but my impression is that what they are doing is a more promising approach to quantum gravity than string theory, and the fact that they are convincing more people about this is what is getting Distler and others very worked up. I also don't think either LQG or string theory has made any headway on the problems of the standard model, although several orders of magnitude more effort have gone into the string theory approach.

Since Distler has a whole posting and ongoing discussion in his comment section about this and I'm no expert, if you want to discuss LQG, its problems and prospects, or comparisons to string theory, please do it there, not here.

Comments

1. **woit**
   June 27, 2006

   As expected, the first comment here was another sortie in the string theory/LQG warfare. I've deleted it, and will delete any others. The commenter did make the useful suggestion that an alternate location for people to discuss this is:

   http://christinedantas.blogspot.com/2006/06/top-10-results-in-lqg-and-spin-foams.html

2. **Aaron Bergman**
   June 27, 2006

   No, the comment was about mostly about you. I only mentioned the link and the frustrating fact that nobody has responded and referred them there.

3. **Peter Woit**
   June 27, 2006

   OK Aaron,

   I don't want anyone to feel that what I'm doing is deleting criticism of me, so here's the relevant part of your deleted comment:

   "I'm beginning to think that string theorists can't win. When we ignore LQG, we're being insular and closed-minded. When we criticize LQG, it's because "they are convincing more people" and that's making us "very worked up.""

   It was deleted purely because since it wasn't explicitly aimed at me, I was sure others would take up the cudgels and respond and the battle woud soon be raging here.

   If this criticism is aimed purely at me, I'll just note that my comments about this have not been that string theorists "ignore LQG", it's that their response to it is nearly uniform hostility. Robert Dijkgraaf, normally a sensible sort, spent part of his summary talk at Strings 2006 making fun of it. David Gross at his recent public talk called it something he didn't normally discuss in polite company.

   Whether this hostility is reasonable or not depends on what you think of the achievements of LQG versus the achievements of string theory. Personally I don't think it's reasonable, but that discussion belongs elsewhere, not here right now.

4. **Garbage**
   June 27, 2006

Hey Peter,

The Distler et al. paper would in principle (in the absence of a light higgs) falsify any theory whose S matrix obeys Lorentz invariance, unitarity and analyticity for all scales. Given that (at least in its perturbative version) string theory was constructed to produce an S matrix obeying such requirements, a violation of the given bounds will imply that whatever it's been called string theory so far can not be a valid UV completion of the standard model. To me, the fact that the paper does not explicitely reffers to what string theory is, or any GUT in particular, is more of a virtue than a weakness, and it is a very elegant way to overcome the decoupling problem (Distler quote as Georgi's). There is a remaining question, namely, at which scale the new physics will enter and whether or not it will be Planck supressed. We are yet to await until LHC answers that question…
Regarding LQG, or other attemps such as discrete QG, it is possible that they might emerge as possible candidates since Lorentz, as well as unitarity violations (without so far drastic consequences) have been found in such scenarios.

5. **Johan**
June 27, 2006

Not that this proves much of anything, but of all the scientists I've talked to, no one's made an impression on me like Thomas Thiemann. He's so smart that it's depressing.

6. **Carl**
June 27, 2006

On the subject of "sarcastic sneering", it seems to me that this is a lot more common now than it once was, and the only explanation for this is that it is more effective now. That is, that people are more afraid now of being ridiculed as a fool.

I am reminded of the Kaczynski's manifesto, where he said that the population was being excessively socialized, and that this was removing their ability to achieve satisfaction. (He found satisfaction by living off the woods and mailing bombs.)

In the logical world of physics and mathematics, sarcasm is an appeal to social pressure that has little obvious place. I've almost just written that it is too bad that there is not more Unabomber types in Physics.

Carl

7. **Arun**
June 27, 2006

Peter,

While not disputing at all the way you characterize the tone of the discussion at Distler's blog, it is true that there is a serious point there. I'll only say that it seems to me that one must read the papers for oneself, there is no other reliable way of knowing the state of LQG.

8. **Peter Woit**
June 27, 2006

Garbage,

The paper is never going to falsify anything. Even the authors admit that their result is only relevant if something that they think is extremely unlikely happens: no light Higgs and you see a violation of their bounds. This is not going to happen, even they don't think so. The result is of purely theoretical, not practical interest.

The claim of "we can falsify string theory" is there for a political reason, to allow one of its authors to attack people like Smolin (e.g. by writing letters to the Wall Street Journal), and any one else who dares point out that string theory is a vacuous idea since it predicts nothing and can't be falsified.

You can't get something for nothing. You can't "falsify string theory" by writing a paper that has nothing to do with string theory. If the authors can show that their results apply to all the classes of models that people doing string phenomenology are currently using to try and make contact with experiment, that would be interesting. They need to actually discuss such classes of real, viable string models and write a paper that says something about string theory before they can claim to be able to falsify the idea.

If their bounds actually were by some miracle violated, what you would see happen is that most string theorists would argue that this meant that QFT was no longer viable and one had to do string theory.

9. **Anon**
June 27, 2006

Is there any class of string models (however unrealistic) that their dispersion relation bounds would not apply to?

If you know of one, you should write a paper about it. As far as I understand, these relations are generic to all (perturbative?) string theory backgrounds.

10. **Peter Woit**
June 27, 2006

Anon,

Most supposedly realistic string theory models that I know about involve introducing branes, which are inherently non-perturbative objects. I just don't see that their arguments apply to these models. If they have an argument that they do, they should make it.

In any case, it's ridiculous to go around saying that you can "falsify string theory" when your argument only applies to perturbative string theory, and the standard ideology of the subject is that understanding non-perturbative string theory is what is going to save the subject from its well-known problems with making contact with reality.

Recall that Gross and many others continually tell us "we don't really know what string theory is", and that understanding what it is will involve getting rid of our conventional ideas about space (and maybe time). Once we do that, what happens to the author's Lorentz invariance and analyticity assumptions?

11. **Anon**
June 27, 2006

Every string background (with some number of flat dimensions) that I know of has an S-matrix which satisfies the usual analyticity assumptions.

If you're arguing that some future developments in nonperturbative string theory will produce backgrounds which violate this property, it's hard to say with any certainty that you're wrong. But there's zero evidence that you're right.

On the other hand, if you are arguing that there's a known string background that violates this property, then you should just say what it is, and end the argument right there.

12. **Aaron Bergman**
June 27, 2006

Robert Dijkgraaf, normally a sensible sort, spent part of his summary talk at Strings 2006 making fun of it.

I wasn't in Beijing, but Dijkgraaf often gives humorous talks. I'd guess that the 'hostility' you're referring to was simply a joke.

I find it depressing that you seem to ignore all the scientific criticisms and just focus on this 'hostility'. Frankly, I'd wish you'd focus a lot less on personality issues and more on science. So, Lubos is an idiot, and you don't like Jacques. That has absolutely no bearing on whether string theory is 'not even wrong' or not. Take this, for example:

The claim of "we can falsify string theory" is there for a political reason, to allow one of its authors to attack people like Smolin (e.g. by writing letters to the Wall Street Journal), and any one else who dares point out that string theory is a vacuous idea since it predicts nothing and can't be falsified.

This is baseless speculation. You have no idea whether it's true or not. So, why say it? I doubt it helps you convince other people.

13. **woit**
June 28, 2006

Anon,

My point is that you don't know what the underlying non-perturbative theory is that determines the background. In the full string theory these "backgrounds" are supposed to have some sort of dynamics. You're working in an approximation where you ignore that and fix the background. In this approximation, if you make certain assumptions, like 4 flat dimensions, maybe you will get the properties at issue. But I just don't see any argument that they are still there outside this approximation.

Again, every talk I've seen David Gross give in recent years he goes on about how really understanding what non-perturbative string theory is will require giving up our standard idea of what space and time is at short distances At short distances, Lorentz invariance is more or less precisely our standard idea of what space and time is. If Gross is right, where's the argument that the analyticity and Lorentz invariance properties at issue will survive in a full theory without conventional short distance notions of space and time?

String theorists assume for no good reason that some unknown dynamics is spontaneously breaking the 10d Lorentz invariance of the perturbative superstring down to 4d Lorentz invariance, no? What's the argument that this unknown mechanism that ruins things in 10d leaves them alone in 4d?

14. **Anon**
June 28, 2006

"You're working in an approximation where you ignore that and fix the background."

In asymptotically-flat spacetimes, different backgrounds are superselection sectors.

"Again, every talk I've seen David Gross give in recent years he goes on about how really understanding what non-perturbative string theory is will require giving up our standard idea of what space and time is at short distances…"

In other words, you don't have an example of a string background whose S-matrix violates these analyticity assumptions, and you are wildly speculating that some future developments in nonperturbative string theory might produce one which does.

That's a pretty thin basis on which to go on the attack.

15. **woit**
June 28, 2006

Aaron,

Yes, Dijkgraaf's comment was a joke, one about how incompetent the LQGers are. Supposedly the audience erupted in laughter at this.

I take one look at Jacques's comment section on his posting, see him up to his normal sneering, other major participants Lubos and Lubos-wannabee Michael, and have a very hard time believing that these are people trying to have a serious scientific discussion. I've had the experience with all three of them of trying to have a serious scientific discussion about string theory, and finding it a waste of time since they weren't interested, but were only interested in attacking me as a crackpot/incompetent/whatever.
Sorry, that's a commentary on personalities and their hostile behavior, but that's what I see.

As for the "string theory is falsifiable" issue, again, I'm calling it as I see it. The WSJ article contained the perfectly accurate statement by Lee that "string theory cannot be disproved". Distler chose to write to them to complain about this, invoking a bogus argument about "canonically defined string theory" that is besides the point, and he should know it.

16. **woit**
June 28, 2006

Anon,

You're ignoring what I write and bringing in irrelevancies.

"In asymptotically-flat spacetimes, different backgrounds are superselection sectors"

Is it known that realistic backgrounds are all different superselection sectors of non-perturbative string theory? I know plenty of string theorists who don't want that to be true and are operating under the assumption that it isn't (e.g. Gross).

The rest of your comment just completely ignores my answers to your question. I don't see the point of retyping it again.

17. **Anon**
June 28, 2006

I think you completely misunderstand Gross's position, and am pretty certain he would disagree with you on the S-matrix in nonperturbative string theory.

If you want to cite him as an authority in your argument with Distler & co., you'll have to do better (like maybe solicit a statement from him, supporting your position).

18. **Aaron Bergman**
June 28, 2006

Sorry, that's a commentary on personalities and their hostile behavior, but that's what I see

If you don't like the tone there, how about the criticism by people like Nicolai, Peeters & Zamaklar, Helling & Policastro, and others? It's hardly fair of you to ignore it. Regardless, the personality of the person doing the criticism has absolutely no bearing on the legitimacy of the critique.

19. **DC**
June 28, 2006

Question:

Is each of the 10^500 worlds of the anthropic principle a (theoretical) closed system? If so, I would say that the requirement for the existence of 10^500-1 universes completely immune to observation is a particularly troubling weakness of string theory.

(6 yr old Physics *AB* here, so please don't get too technical…)

20. **sunderpeeche**
June 28, 2006

One obvious question is why did 2 coauthors agree to put their names on a paper with a title "Falsifying String Theory …" if the paper has nothing to do with ST?

21. **Cecil Kirksey**
June 28, 2006

Peter:

I have enjoyed reading your blog. I discovered it by googling for "critique of string theory". I will certainly buy your book when it comes out. I think I understand your arguments against string/M theory. But to help clarify your position can you please respond to the following.

There seems to me to be three possibilities:
1. String/M theory is wrong. Our universe is not described by this theory.
2a.String/M theory is correct but our universe is not unique and that there is no vacuum selection principle. There are multiple universes and ours is just one manifestation of this theory.
2b. String/M theory is correct. Our universe is unique but we humans will forever lack the mental capacity to discover the vacuum selection principle.
2c. String/M theory is correct. Our universe is unique and we humans have the mental capacity to discover the vacuum selection principle but as of yet have not done so.
3. In the development of string/M theory a logic error or incorrect assumption was made and if corrected a valid theory will be discovered.

If 2 is correct then by definition the theory CANNOT be falsified. And the theory can make very general predications and be adjusted to agree with any observation. This seems to me to be your primary issue.

I am most interested in the third possibility. If you believe that string/M theory is "not even wrong" is it due to option 1 or 3? It seems to me that it maybe possible for some smart iconoclastic theorist to revisit the formulation of string/M theory and revise some of the assumptions used to develop the theory. Thus possibilly avoiding the vacuum selection problem. Reducing from "real" 10 or 11 spacetime dimensions to 4 forces one to invent or discover a vacuum selection principle. But suppose we are misled in to believing that compactification is even required?

Suppose that the universe really has a fourth spatial dimension of the same "size" as the other three that we experience. How would this fourth dimension make itself known to us? Either through our senses or instruments that we make to observe some measurement. But the measurement to be made is based on some theoretical prediction. Most arguments that reject more than three spatial dimensions assume that GR is valid in more than four spacetime dimensions. Suppose it is not valid then what? How would string/M theory look if compactification was not invoked? Just some thoughts.

Thanks for a great blog. I look forward to reading your book.

22. **woit**
June 28, 2006

sunderpeeche,

The two coauthors are not string theorists.

Aaron,

I don't know how many hundred times I have to say something like this. I'm a particle theorist, not a quantum gravity theorist. As a result, I've not spent time paying close attention to exactly what the state of the LQG program is and don't intend to. Many other smart people are and I think that's great, but life is short and one has to decide what to spend one's time on. People with these interests should respectfully debate the technical problems involved, learn new things and try and make progress, or give up and do something else if the problems are insuperable. If I wanted to work in this area, I would encourage this discussion here and take part in it. I don't at the moment, but want to think about and discuss other things. I have every intention of ignoring the detailed criticisms of LQG, just as I ignore the details of what people working in LQG have accomplished, and I think this is perfectly fair. If string theory's only claim was that it was a theory of quantum gravity, I'd be ignoring that too, and so would most of the rest of the physics community.

Anon,

You continue to just ignore the points I make and questions I ask and raise irrelevancies. I was not invoking Gross as an authority for anything I had to say, just as the most well known example of someone who believes that string theory is someday actually going to explain the properties of the standard model. If string theorists have completely given up on this, they should say so, and let Mike Peskin know he should stop telling the media the reason to believe in string theory is that it is going to explain the number of generations.

23. **woit**
June 28, 2006

Cecil,

My belief is simply 1. As an idea about unification, string theory is just wrong, and fundamentally so. It can't be fixed by just changing one piece of it.

The physical effects of extra dimension depend on exactly how you introduce them. The study of these possibilities is what thousands of physicists have been working on for more than 20 years. None of the infinite variety of effects predicted by any of these possibilities have ever been observed, and even most string theorists don't expect to be able to observe them in the forseeable future.

24. **sunderpeeche**
June 28, 2006

As a non-string theorist, I would not put my name on a ST paper. I would not put my name on any paper where I was not satisfied or could not validate the contents (I might compromise on the wording etc with coauthors, but not on the substance of the paper). Why would 2 non-string theorists agree to put their names on a paper if they could not establish the veracity of its contents?

25. **woit**
June 28, 2006

sunderpeeche,

You'll have to ask them. I assume that they found assurances from their co-author and other string theorists they talked to sufficient. It's not exactly unusual though in science for some co-authors of a paper to not be experts on all aspects of the paper, and to rely on the expertise of others.

26. **Tony Smith**
June 28, 2006

Peter, you say
"… The lack of any serious (Lubos … and the trademark Distlerian sarcastic sneering … doesn't count) response to my book from string theorists since its appearance in the UK a month or so ago has begun to surprise me a bit. ….".

Such silence sounds to me similar to the 1950s when Oppenheimer, the then-current Pope of Princeton who advocated the Copenhagen interpretation of QM, said about Bohm, who was working on an alternative interpretation:

"… if we cannot disprove Bohm, then we must agree to ignore him. …".

The source of the quote was Max Dresden (in my opinion impeccably honest) and The Bohm biography Infinite Potential, by F. David Peat (Addison-Wesley 1997),page 133. Here are some relevant excerpts from that book:
"… Max Dresden … read Bohm's papers … errors were difficult to detect … von Neumann's "proof" … did not rule out the sort of theory that Bohm had proposed. … Oppenheimer [said]…
"We consider it juvenile deviationism … we don't waste our time …" [by] actually read[ing] the paper …
Dresden … present[ed] Bohm's work in a seminar to the Princeton Institute …
The reception he received came as considerable shock to Dresden. Reactions to the theory were based less on scientific grounds than on accusations that Bohm was a fellow traveler, a Trotskyite, and a traitor. It was suggested that Dresden himself was stupid to take Bohm's ideas seriously. … all in all the overall reaction was that the scientific community should "pay no attention to Bohm's work" … Abraham Pais also used the term "juvenile deviationism". Another physicist said that Bohm was "a public nuisance" …".

It seems that the silent treatment plus ad hominem attacks has been a tradition of the USA physics community for at least 50 years. Plu ca change …

Tony Smith
http://www.valdostamuseum.org/hamsmith/

27. **ksh95**
June 28, 2006

sunderpeeche said:

> As a non-string theorist, I would not put my name on a ST paper. I would not put my name on any paper where I was not satisfied or could not validate the contents…

My name is on papers I have yet to read fully. Many times (if I know the other authors personally and trust them) I have written my section and, based on a few meetings, have assumed the other authors would do a good job.

I am not in anyway endorsing my own actions, but I am making the point that not all scientists take as much care as you.

28. **D R Lunsford**
June 28, 2006

OK, a quick glance at this paper, where is the string theory? You might as well replace the words "string theory" by "homuncular biology", "phlogiston dynamics", or "epicyclic astrometry".

Suppose that the universe really has a fourth spatial dimension of the same "size" as the other three that we experience. How would this fourth dimension make itself known to us?

You couldn't tie your shoes.

-drl

29. **justin**
June 28, 2006

"I've not spent time paying close attention to exactly what the state of the LQG program is and don't intend to." — PW

This is a little frusterating for those of us who would like to engage you in a discussion in the relative merits of string theory as a research program.

1) You say that you would like to see hep-th research substantially diversify.

2) Many of us point out that there is really no viable alternative to string theory as a theory of quantum gravity. If planck scale physics is your interest, string theory is the only game in town.

3) You object, saying that LQG is a neglected but nevertheless thriving alternative to string theory as a research program in QG.

4) We disagree, and try to explain why in scientific and technical terms

5) You shut down the discussion, explaining that you don't care about QG anyway and you have no interest in becoming educated on the issue.

Unfortunately, QG is the primary achievement of string theory. If you don't care about QG, its no wonder that string theory is not of interest to you—for physics at the TeV scale, everyone will agree that the explanatory power of string theory is weak at best. Fine, but if you don't care about QG, you have no right to criticize people who do.

But I suspect that you do care about QG. Every hep theorist wants to know physics at the ultimate cutoff. In this case, if you want to be critical of string theory, you should have something to say about its obvious alternatives. Especially, you should be open to the possibility that the alternatives might not be any better, or even far worse (as is the case for LQG, in my opinion).

30. **sunderpeeche**
June 28, 2006

A very nice letter from Michael Peskin to the EPP2010 committee. In the end, one must just see what the LHC produces (and build the ILC!). It's the only way to know just how wrong life, the universe and everything is.

www7.nationalacademies.org/ bpa/EPP2010_Feedback_Peskin.pdf

31. **sunderpeeche**
June 28, 2006

clickable link

http://www7.nationalacademies.org/bpa/EPP2010_Feedback_Peskin.pdf

32. **Garbage**
June 28, 2006

"If Gross is right, where's the argument that the analyticity and Lorentz invariance properties at issue will survive in a full theory without conventional short distance notions of space and time?"

Sure the scenario might no be that realistic, nevertheless, it opens a new windows worthwile exploring. But that is not the point here, the point is that, if the bounds are actually violated in the absence of a light Higgs (physically possible though unlikely) ST will have to start taking these ideas you point out a bit more seriously….LQG at al. have explored them enough to start to be a serious candidate if such case were to happen in nature.
I dont understand why is it so hard to admit that this sort of dispersion relations are sensitive to the UV in such a way that assumptions on the UV completion of the SM can be tested, included ST or whatever GUT you want.

33. **Thomas Love**
June 28, 2006

Peter wrote:

"I'm a particle theorist, not a quantum gravity theorist. "

Einstein's basic idea was that the particles (he only spoke of the electron and proton) should arise naturally from the geometry of space-time. What is quantum gravity but a study of the excitations of space-time? Thus I would argue that 'particle theory' and 'quantum gravity' are synonyms.

34. **fh**
June 28, 2006

Aaron Bergmann, the critique is unreasonable because some of these issues at least have been pointed out to Distler and he keeps making the same wrong or irrelevant points over and over again.

Most of the criticism can be reduced to "it's unfamiliar".
Other of the criticisms voiced (re: QCD) have actually been answered by Thiemann on the String Coffee Table more then a year ago. The attack on the mathematical quality and accuracy of the work of people on the level of Thiemann and Freidel discredits Distler further. It's hard not to read this as malice.

35. **Cecil Kirksey**
June 28, 2006

Peter:

Thanks for the reply. But if you believe #1 in my list of possibilities, and since the theory has not been falsified what do you SPECIFICALLY believe that the theory assumes is not compatible with our universe. Is it the idea of strings per se or some addition assumptions that are invoked to eliminate some apparent mathematical problems.

36. **woit**
June 28, 2006

Justin,

I don't agree at all with your comments about the relative value of string theory and LQG as theories of quantum gravity, but that's just not a point I want to spend my time arguing. Other people can do that much better than me. I'm not shutting down this discussion, I'm pointing people to the most appropriate places to carry it on, which are at the blogs of people working on LQG or actively interested in discussing it. Some string theorists refuse to believe me, but I honestly take great care to not go on about things I don't understand. I just don't have the time now to develop the kind of expertise required for me to feel comfortable engaging in the kind of arguments over LQG that people want to have.

The problem with string theory as a theory of particle physics is not that its explanatory power at the TeV scale is "weak at best". Its explanatory power as regards particle physics is zero at any scale, up to and including the GUT scale and beyond. If string theorists will finally admit this, and stop continually promoting their subject as a unified theory that is going to explain the standard model, you're going to hear a lot less from me about this. What I am upset about and trying to do something about is the disastrous effect string theory has had on the subject that matters most to me, particle theory. If you want to argue this point, I'm happy to do it here. If you want to argue about LQG, you should be doing it with an LQG expert.

37. **woit**
June 28, 2006

Cecil,

I specifically believe that the idea that the universe is 10 or 11 dimensional, with the properties of particles dependent on how you get rid of the other 6 or 7 dimensions, is fundamentally wrong and can't ever work.

38. **sunderpeeche**
June 28, 2006

Reply to ksh95 — in a large expt HEP collaboration, indeed one cannot demand editorial control over the whole paper. But (I presume) you could in principle validate the contents of the whole thing if you tried. Surely the two coauthors on Distler's paper could verify if the contents of the paper actually had any ST? Or if the title was appropriate?

39. **woit**
June 28, 2006

Thomas,
Unfortunately, Einstein was wrong about this. Particles are excitations of quantum fields. If someone has an idea of how to get these quantum fields out of the geometry of space-time, I'm all ears, but I haven't seen a really promising one yet.

40. **Anon**
June 28, 2006

FH,

Maybe you should leave a comment over there, pointing out his errors and misunderstandings. If he's wrong or off the mark, it should be easy to point out where he's gone astray.

41. **Alejandro Rivero**
June 28, 2006

The subtext of Justin's comment it is very clear: a theory of particle physics is not ultimate, so particle physics is not a worthy pursuit, only gravity is.

42. **woit**
June 28, 2006

Garbage,

What I'm denying is that "string theory" is well enough understood in the UV for this kind of argument to falsify it. That's all. The argument is "If string theory satisfies A, B, C then bounds". If bounds are violated, this doesn't falsify string theory.

43. **woit**
June 28, 2006

I should perhaps explain my point of view about quantum gravity a bit more.

Sure, I'm interested in it, and try and follow what people are doing. But, unless somebody figures out a way to measure QG effects experimentally, it's a problematic research area. I've written about this elsewhere. I believe that we're not going to really understand QG until we understand in a unified way the dynamics of space-time geometry and the dynamics of the Standard model geometry. If and when we do that, we'll have a QG which is linked into a structure which is testable. String theory purports to do this, but it has failed. Pretty much all the LQG stuff I have seen doesn't even try and do this, except for some recent stuff, which to me looks highly preliminary, but if it starts looking more convincing, I'd definitely be spending time paying attention to it.

44. **Aaron Bergman**
June 28, 2006

Aaron Bergmann, the critique is unreasonable because some of these issues at least have been pointed out to Distler and he keeps making the same wrong or irrelevant points over and over again.

What does Jacques have to do with any of this? As best I've been able to discern, the questions I asked over on Christine's blog have not ever been responded to, except that Lee did say that he has wondered about the Immirizi parameter issue, too. A lot of physicists read this blogs if you haven't noticed, and I can tell you that the silence in response to these issues plays into the bad reputation that LQG has with some people I've talked to.

Out of deference to Peter, I won't say any more here, but if you claim the questions have been answered, can you please post some links over on Christine's blog, and I can respond there?

45. **Thomas Love**
June 28, 2006

Peter, "It's a difference of opinion which makes horse races".

I replied privately with details.

46. **Aaron Bergman**
June 28, 2006

I did a little more googling, and I'm being a bit unfair to Lee. He did respond that, *contra* Carlip, he believed there was a unique quantization of quantum gravity in 2+1 dimensions (I believe Freidel implied the same) and encouraged me to do further research on the comparison of Carlip and LOST.

47. **Cecil Kirksey**
June 28, 2006

To D R Lunsford:

You responded to my completely reasonable question:

"Suppose that the universe really has a fourth spatial dimension of the same "size" as the other three that we experience. How would this fourth dimension make itself known to us?"

with:

"You couldn't tie your shoes."

I suppose that is to be interpreted as a rejection of a large fourth spatial dimension. But maybe you can tell me: How do you know? How would this fourth dimension reveal itself? In some braneworld models the higher dimensions are hidden apparently except through gravity. But I was even questioning this. How do we know that GR is valid in more than four spacetime dimensions?

48. **Garbage**
June 28, 2006

"The argument is "If string theory satisfies A, B, C then bounds". If bounds are violated, this doesn't falsify string theory."

??????

If string theory satisfies A,B,C and the bounds which follow from such assumptions are violated, the only logical conclusion is that A,B,C can not be part of the UV completion of the SM, and all the version of ST with A,B,C should be ruled out [There are thecnical issues as to whether higher order operators might spoil the bounds if the cutoff is somehow lower than naively expected]
If, as you point out, that means we should give up in Lorentz invariance, that is a huge step and the efforts to produce such type of constraints extremely important an worthwhile pursuing. Can you imagine to turn on the LHC and be able to probe Lorentz invariance!!
Again, it is unlikely there is a light higgs as it is that the bounds will be violated, it is also unclear whether there is Planck supression, this is also related to extra dimension and the actual scale of gravity. That doesnt mean, as a matter of principle, that all the forms of ST known so far to obey Lorentz invariance (analyticity and unitarity) will be potentially falsified!

Note: I'd say that unitarity might as well break down at scales where our notion of spacetime doesnt make any sense. I have no intuition for analyticity though… 🙂

49. **Chris Oakley**
June 28, 2006

Peter,

I found your book in the two main Cambridge (UK) bookshops (Heffers & Borders) today. FWIW, the Borders one had a handwritten staff recommendation (the only one in the science section). One thing though – I don't think that the picture of you on the inside back cover is very suitable. You look too much like a student who has just committed a prank.

So for the US edition I propose the following: a picture of you holding a sword aloft with a suitably stern expression and hair and doctoral robes billowing out behind. Borrow a wind machine from the labs if necessary to achieve the latter effect.

50. **Tony Smith**
June 28, 2006

Sunderpeeche said "… A very nice letter from Michael Peskin to the EPP2010 committee. In the end, one must just see what the LHC produces (and build the ILC!). …".

Although I very strongly agree with Peskin's overall recommendation that the LHC should be supported and the ILC built (particularly with respect to getting a fuller understanding of the Higgs mechanism), Peskin made a statement that I consider to be significantly historically inaccurate:
"… Super-symmetry was first discovered as a theoretical concept in superstring theory. …".

IIRC, supersymmetry became of high interest to particle physics theorists with respect to supergravity theories, in which super-Lie algebras were used to evade the Coleman-Mandula theorem that obstructed unification of gravity with the Standard Model.

This is not just a minor historical nit-pick, in light of the tendency of partisans in the superstring/LQG war to act as though the only games in town for Quantum Gravity are superstrings and LQG,
despite the fact that
supergravity theories united Gravity and Particle Physics by using the MacDowell-Mansouri mechanism with respect to a Sp(2) = Spin(2,3) Lie algebra (as part of the relevant Lie superalgebra) to produce a gravity Lagrangian that looks a lot like Einstein-Hilbert.

Therefore, it seems to me that modification or generalization of supergravity should be at least an equal participant in the Quantum Gravity wars now dominated by superstring and LQG partisans.

In fact, since some M-theorists see N=8 supergravity as a possible limiting case of M-theory, it may be that supergravity could be advocated as part of the string theory agenda, but to do that right, they would have to show explicit equivalence between the direct superstring gravity formulation and the limiting case supergravity MacDowell-Mansouri gravity (i.e., to explicitly show in detail the superstring – M-theory – supergravity relationship). AFAIK, that has not yet been done.

My primary point in writing historical comments such as this and a few other recent ones on this blog and JoAnne's entries about LHC data at Cosmic Variance is that those who ignore history may be condemning themselves to repeat its mistakes.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – It seems to me that DRL's statement that if we live in 4 large spatial dimensions we could not tie our shoes is clear, but another commentator seemed to have difficulty with it, so here is another (more or less equivalent) formulation:
If we lived in 4 large spatial dimensions there would be no way to distinguish left-handedness from right-handedness, thus invalidating a lot of particle physics experimental results (there was an old Outer Limits TV show about that point).

51. **justin**
June 29, 2006

Dear Peter,

I respect your desire not to engage in discussion outside of your knowledge base… however, I encourage you to form some opinion on the alternatives to ST, since I think this is very relevant to the discussion. If there were 15 alternative approaches to QG which were even half as promising as string theory, the state of our field would be very different.

"The problem with string theory as a theory of particle physics is not that its explanatory power at the TeV scale is "weak at best". Its explanatory power as regards particle physics is zero at any scale, up to and including the GUT scale and beyond." –PW

I think this is unfair. We don't know physics beyond the TeV scale, so we have no idea whether string theory could provide a useful explanation for it. String theory at least provides some pictures for what this physics could be.

52. **anonymous**
June 29, 2006

Hi Aaron,
The lost theorem only talks about the uniqueness of the kinematical (prior to solving the Hamiltonian constraint) Hilbert space. Different choices of Hamiltonian constraint operator can lead to inequivalent physical Hilbert spaces. (ofcourse how to define inner product on kernel of Hamiltonian constraint in the absence of observables is a even more vexing issue.) Also as the lost theorem is based on a number of requirements (unitary-anamoly free implementation of spatial diffeomorphism being one of those), violation of any one of those could lead to an inequivalent Hilbert space (even prior to solving the constraint).
I apologise if this answer isnt pertinent.
Also on a side note, as a graduate student in lqg i find it astonishing that many people seem to think that string theorists who have studied lqg and are skeptical about it (jaques and aaron and urs for example) are trying to undermine the field. All they do is ask genuine technical questions, & there's absolutely no malice involved in any of it. It seems to me that there is always a bit of overselling of results going on in this field (& maybe in high energy physics in general), pointing it out, or questioning it doesnot amount to any offense at all.

53. **D R Lunsford**
June 29, 2006

Justin,

Sorry to butt in, but you said

I think this is unfair. We don't know physics beyond the TeV scale, so we have no idea whether string theory could provide a useful explanation for it. String theory at least provides some pictures for what this physics could be.

But that's just what it doesn't do – things which provide accurate "pictures" (however you want to interpret that) also provide accurate physical calculations according to a precisely defined model – this is the ultimate credo of physics. Correct me if I'm wrong.

-drl

54. **Alejandro Rivero**
June 29, 2006

If there were 15 alternative approaches to QG which were even half as promising as string theory, the state of our field would be very different.(justin)

Any comment about why string theory papers are uploaded to hep-th instead of gr-qc, then?

55. **Chris Oakley**
June 29, 2006

Lubos seems to have removed his review of NEW from the amazon.co.uk web site.
Or maybe it did not meet Amazon standards, and they removed it themselves.

56. **Tony Smith**
June 29, 2006

Chris Oakley said "… Lubos seems to have removed his review of NEW from the amazon.co.uk web site.
Or maybe it did not meet Amazon standards, and they removed it themselves. …".

However, as of the time I am writing this comment, Lubos's review is still among the 9 reviews on the USA amazon.com web page for the UK version of Peter's book.
The USA web page seems to have listed the UK version for book resellers who have UK versions for resale in the USA. In the past, there have been a number of such books for sale on that page, but now the page says
"… THIS TITLE IS CURRENTLY NOT AVAILABLE. If you would like to purchase this title, we recommend that you occasionally check this page to see if it has become available. …"
so I guess the USA resellers are as of now sold out of the UK version.
Maybe they will restock and it can again be bought through the USA amazon.com.

Here are the 5 current customer reviewers of the UK book on the UK web site amazon.co.uk
LEJ Brouwer
J. B. Cook (UK)
Reader (Australia)
Sam "Sam" (UK)
Dr. C. G. Oakley (Dunstable, Bedfordshire United Kingdom)

Here are the 9 current customer reviewers of the UK book on the USA web site amazon.com
LEJ Brouwer
truth eker (Canada)
Knotted String
Humble Priest "String Theorist" (USA)
M. Wang "mrnexus" (CT United States)
J. B. Cook (UK)
Lubos Motl (Cambridge, MA United States)
Sam "Sam" (UK)
Dr. C. G. Oakley (Dunstable, Bedfordshire United Kingdom)

It is interesting to read all those customer reviews.
Here is an excerpt from the 5-star review by Humble Priest "String Theorist" (USA):
"… I'm a string theorist by the way, which is why I'm hiding behind a pseudonym (I don't want to be called a "science hater" by my seniors). …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – I think that amazon must not allow customer reviews of books not yet available, as there are no customer reviews on the USA amazon.com web page for the not-yet-released USA version of Peter's book. However, there is a Beta-version customer discussion feature (as of now it has only one participant, me).

57. **You**
June 29, 2006

Lubos has posted Barton Zwiebach's response on his blog.
http://motls.blogspot.com/2006/06/barton-zwiebach-letter-to-editor.html

58. **I**
June 29, 2006

In regards to Zwiebach's response, this pretty much sums things up:

"All that is needed to rule out string theory is showing that no solution describes our universe."

That should be easy.

(BTW, Isn't that what led to the anthropic landscape in the first place?)

59. **Tommaso Dorigo**
June 29, 2006

Hi all,

in Lubos' site, today Zwiebach is quoted as saying:

"[string theory] has explained, for example, why black holes have entropy and temperature."

Can I get some insight on this ? What is Zwiebach referring to, and what is true about it ? Sorry for my ignorance…

Cheers,
T.

60. **Tony Smith**
June 29, 2006

As "You" noted, Barton Zweibach's letter to the Wall Street Journal editor is posted on Lubos Motl's blog. In it, Zweibach says:
"… string theory … computations give unequivocal answers.
… All that is needed to confirm string theory is finding one solution that describes our universe. All that is needed to rule out string theory is showing that no solution describes our universe. An answer must exist. …".
That is true.
What Zweibach does NOT say is that after two decades of work by many well-funded superstring theorists, none of them have found ANY "string theory … computation" that "describes our universe" in the sense that it gives the force strengths and particle masses of the Standard Model.
Perhaps two decades of failure to find such a solution is an indication that such a solution does in fact not exist, thus providing Zweibach's "answer":
conventional string theory, as a unification of gravity and the Standard Model, is probably wrong.

Zweibach also says:
"… a healthy equilibrium exists where string theory and other good ideas are explored and compete for attention …".
However,
a competing model that allows computation whose answers (unlike those of superstring calculation) are explicitly known and consistent with experimental measurements of such parameters of the Standard Model,
is
ignored by theoretical physics institutions and and its developer (me) is blacklisted by the Cornell arXiv for physics papers and is subjected to ad hominem attacks by Harvard Professor Lubos Motl as a "… moronic … crackpot …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – My competing model can be formulated in terms of a NON-supersymmetric string theory in which strings correspond to world-lines of particles, but Lubos Motl attacked it as not being "string theory" because it did not have conventional supersymmetry.
Maybe when the LHC collider data that is to begin to be taken around 2008 will show whether or not such supersymmetry is consistent with experiment. Peter, in his blog entry "Strings 2006", quoted Jonathan Schock as saying, about the coming LHC experiments:
"… … if supersymmetry is found … then we all will be together [at a Strings 2009 meeting] to celebrate it and if it is not found we will be together for a collective suicidal. …".

61. **Benjamin**
June 29, 2006

Tony, You say ‘That is true’ to Barton Zwiebach’s claim that “All that is needed to confirm string theory is finding one solution that describes our universe.” But is that so? If I understand Peter Woit, a theory with 10^500 possible universes is a ‘theory of anything’, unless you can specify the mechanism that singles out one of these vacuum states. If it is just something like ‘spontaneous symmetry breaking’, which sounds like pure chance to me, then it seems to me you haven’t really explained anything. Of course, as an ignorant layman, I may not have my facts straight. 🙂

62. **sunderpeeche**
June 29, 2006

Tommaso this may be helpful

http://en.wikipedia.org/wiki/Black_hole_entropy

63. **PPCook**
June 29, 2006

Dear Tommaso,
I think Zweibach is referring to the emergence of the Hawking-Bekenstein entropy law from a string theory picture of a black hole. While we have very little evidence of black holes themselves, general relativity leads to a theoretical framework where there is a duality between black hole properties on one side and macroscopic thermodynamic properties, such as entropy, temperature, free energy, on the other. There is no parallel for microscopic properties from GR alone.

Using string theory to model a black hole, and imagining the microstates of a black hole to be string excitations, Strominger and Vafa, were able to find the entropy of a 5-dimensional Reissner-Nordstrom black hole from the microscopic states. The entropy calculations have been extended to a large range of black holes with success. That string theory can model the microstates of a black hole means it fulfils a necessary criterion for any theory of quantum gravity. In this sense sometimes this is (mis)referred to as an experimental test of string theory.

It is a very actove area of string theory reserach at the moment, and the string picture has lead to corrections to the Bekenstein-Hawking area/temperature law and even to the idea that the black hole has no precise event horizon when viewed from close-up – the event horizon becomes pictured as a statistical distribution, like temperature, that only makes sense when viewed from far away (in the same way that if you were riding a molecule in a gas, the temperature of the gas as a whole would be a strange concept for you, but if you were macroscopic experimenter then temperature is a very clear concept). The theoretical picture is making progress, all we need now is a black hole to experiment with…

This is, arguably, the greatest result of string theory to date. Zweibach’s wonderful purple book, Chapter 16, has an excellent introductory review.

Best wishes,
Paul

64. **Chris Oakley**
June 29, 2006

From Zwiebach’s article:

> [String theory] has explained, for example, why black holes have entropy and temperature.

Right … so ST has explained the existence of unobserved physical properties about a possibly non-existent class of objects.

That’s good enough for me. I’ll now go and remove all the negative things I have said about ST from my web site.

65. **fh**
June 29, 2006

The “it has many sollutions” paradigm is already a change to what, for example Lubos Motl was arguing a while ago, that is, that it’s essentially unique.

As such it is of course not a theory of anything. Newtons formula F = ma is of course completely unspecified until you define F in terms of x, yet few would argue that it’s therefore vaccuous.

String theory is unique in getting a relationship between the effective QFT picture and Gravity. There is no other theory that can do this at the moment (some loopy results might be getting there, but it’s not here yet). At the same time it’s hard to see how to answer the hard conceptional questions from the String theory PoV precisely because it is so tightly tied to the effective field theory picture.

JDs critizism of LQG is that it does not relate to the effective field theory picture, well yes, that’s the point. It’s not a local field theory.

66. **sunderpeeche**
June 29, 2006

Why claim that black holes do not exist? Hawking (+ Penrose?) proved long ago that GR must have singularities. I believe there is expt observation of black holes, see this from HST

http://hubblesite.org/newscenter/newsdesk/archive/releases/1994/23/

67. **Tony Smith**
June 29, 2006

As Benjamin said, I said “… ‘That is true’ to Barton Zwiebach’s claim that “All that is needed to confirm string theory is finding one solution that describes our universe.” …”,
and
Benjamin asks “… But is that so?
… unless you can specify the mechanism that singles out one of these … 10^500 possible … vacuum states …[other than]… pure chance …
then it seems to me you haven’t really explained anything. …”.

Benjamin, I think you do have your facts straight.
When I said “That is true” I was assuming that Barton Zweibach’s “… finding one solution that describes our universe …” would be based on his finding some sort of

reasonable uniqueness criterion, not based on something like a pure chance random search.

So, I stand corrected. I should have said:

Zweibach says:
"… string theory … computations give unequivocal answers.
… All that is needed to confirm string theory is finding one solution that describes our universe.
All that is needed to rule out string theory is showing that no solution describes our universe.
An answer must exist. …".

Zweibach's first alternative should be divided into two possibilities, giving a total three:

1 – finding one solution that describes our universe, and finding that it has unique characteristics (other than just happening to correspond to the properties of our universe) that single it out from all other (about 10^500) possibilities, would confirm superstring theory;

2 – finding one solution that describes our universe, but finding that it has no uniquely distinguishing characteristics from all other (about 10^500) possibilities, would show that superstring theory is Not Even Wrong;

3 – finding that no solution describes our universe would show that superstring theory avoids being Not Even Wrong by in fact being Wrong.

Among those three alternatives, I think that "An answer must exist".

Thanks, Benjamin, for pointing that out.

My personal guess as to which alternative is true is still that superstring theory is Wrong (my third alternative), because failure to find a solution (in ANY way, random or not) by two decades of work by many hundreds of very (techically) smart, well-funded people indicates to me that such a solution is unlikely to exist, thus providing the "answer":
conventional string theory, as a unification of gravity and the Standard Model, is probably wrong.
My conviction as to my guess is supported by the attitude of people like Jonathan Schock, who (according to Peter's quote) said that at Strings 2009, "…if supersymmetry is … not found [by LHC] we will be together for a collective suicidal. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

68. **woit**
June 29, 2006

I just wrote a new posting about the Zwiebach letter. Please continue discussion of it there.

69. **Kris Krogh**
June 29, 2006

Sunderpeeche,

You're twisting Chris Oakley's words. He's not claiming black holes don't exist. Just saying it hasn't been proven they do. That's true.

The NASA page you point to describes a dark, supermassive object in the M47 galaxy. There are GR alternatives that predict such things (see http://www.arxiv.org/abs/astro-ph/0606489) without black hole singularities.

There's also a dark, supermassive object, Sgr A*, in this galaxy. But a close look shows it or something nearby radiates at long wavelengths.

70. **Kris Krogh**
June 29, 2006

Sorry for the typo. That should read M87, not M47.

71. **Kris Krogh**
June 29, 2006

Peter,

Sorry again. Missed your last comment.

72. **Shantanu**
June 29, 2006

Peter and others,
since we are discussing quantum gravity., I urge all of you to read this nice
preprint which appeared just yesterday gr-qc/0606120 (which talks about possible
experimental tests). I am surprised no one has discussed it in the blogosphere/

73. **D R Lunsford**
June 29, 2006

Cecil,

http://www.people.fas.harvard.edu/~math21a/exhibits/unknotting/index.html

And, what Tony said.

-drl

Zwiebach Letter to the WSJ

June 29, 2006
Categories: Not Even Wrong: The Book

Over at his web-site, Lubos has posted a [letter to the Wall Street Journal]() from Barton Zwiebach. The letter seriously misrepresents the current state of string theory in several ways:

string theory is an extraordinarily precise and rigorous framework where facts can be proven beyond doubt and computations give unequivocal answers.

All that is needed to confirm string theory is finding one solution that describes our universe. All that is needed to rule out string theory is showing that no solution describes our universe. An answer must exist.

Zwiebach gives the impression that there are rigorously well-defined, "extraordinarily precise" equations that characterize string theory, and all that is needed now is to solve the technical problem of finding the solutions to these equations and seeing if one of them agrees with what we observe. This is simply not true. Since we don't know what non-perturbative string theory is or what its equations are, all equations used by string theorists to generate solutions are not "extraordinarily precise", but are explicitly approximations, often very crude ones, whose reliability is unknown. If you look at the debate over the landscape, you will find that not only do most string theorists not believe that the solutions involved are "facts can be proven beyond doubt", many of them believe these are not real solutions to the full unknown theory at all.

Even if one does believe in the rigorous nature of the landscape solutions, Zwiebach's claim that all one has to do is examine them to see if they agree with nature is again highly misleading. If there are 10^{1000} or more of these solutions, all evidence is that identifying which of them have desired properties (e.g. the correct CC) is an inherently computationally intractable problem. Even if one could do this, the class of solutions that agree with all known values of the parameters characterizing the standard model seems likely to be so large that no new testable predictions would be possible.

Zwiebach also claims:

String theory has explained, for example, why black holes have entropy and temperature.

This is what Hawking did back in 1974 with a semi-classical calculation. Any theory of quantum gravity should reproduce this. What string theory adds to Hawking's calculation is a long story, but if we manage to observe a black hole any time soon and it behaves as Hawking predicted, he's the one who is going to get a Nobel prize for explaining "why black holes have entropy and temperature", not string theorists.

I had been wondering what the response from string theorists would be to the public dissemination of arguments from Smolin and me about string theory. The response seen here from Distler et. al. and Zwiebach is not at all what I expected. Most serious string theorists I talk to take the reasonable attitude that string theory is still so poorly understood that it cannot be confronted with experiment, even in principle. Many publicly say that we still don't know what "string theory" is. Zwiebach seems to believe that there now exist "extraordinarily precise" equations, with solutions that will give "unequivocal answers". Whether this is true is a well-defined question. All he has to do is explain what these equations are, and let's see if the string theory community will really stand by this definition and let string theory be judged accordingly.

Finally, perhaps the most surprising aspect of Zwiebach's letter is the form in which he has chosen to distribute it. I and many people have been wondering what Lubos Motl's colleagues in Cambridge think of the way he is defending their subject. Now at least one of them has made clear that he is fine with this and willing to encourage it.

Update: The [usual response]() from Lubos Motl/Bill O'Reilly: an endless rant about how stupid people who disagree with him are, completely ignoring the scientific questions at issue.

Comments

1. **sunderpeeche**
   June 29, 2006

   Write a rebuttal letter to WSJ.

2. **Peter Woit**
   June 29, 2006

   sunderpeeche,

   First let's see which if any of the letters the WSJ decides to publish. They may very well not find them credible or worth publishing. If they do, then maybe a rebuttal would be in order.

3. **Chinmaya Sheth**
   June 29, 2006

   I am no string theorist (I haven't even completely digested the possibility of extra dimensions yet) but this is my opinion:
   Zwiebach is a serious string theorist: in his book he explicitly states that string theory is not yet completely understood and that it makes no sharp prediction. In the letter itself Zwiebach doesn't say there are precise equations defining string theory, only that it is a precise framework; again hinting that it is not completely developed.

4. **Robert**
   June 29, 2006

   I would say Hawking demonstrated *that* black holes have a temperature and radiate. To say *why* you have to give a microscopic description.

   QED is also only known perturbatively (lattice QED is no good) so do people know what it means?

   The rest has been said over and over again, so I am not going to repeat it.

5. **Peter Woit**
   June 29, 2006

   Chinmaya,

I suppose that perhpas one can parse Zwiebach's letter in some way that makes what he says technically true. But claiming to the public that you have an "extraordinarily precise and rigorous framework" and can do "computations that give unequivocal answers" is seriously misleading if the situation is that the framework is insufficiently developed to produce equations that could be used to reliably answer any questions about physics.

6. **Peter Woit**
June 29, 2006

Robert,

What Hawking showed was that there is a reason black holes have entropy and temperature, completely independent of the microscopic description. This explains these two facts, you don't need to know the microscopic definition. Actually, he didn't show "that" they have a temperature and radiate, for that we'll have to find one and look at it…

The comments you make about QED are completely irrelevant. In QED the physical vacuum state is the perturbative one, so you can do most physics without worrying about non-perturbative effects. In string theory, the perturbative ground state is the wrong one, all the "backgrounds" people are working with are supposed to come from some putative non-perturbative effect. You can't ignore non-perturbative effects, and this is why you can't predict anything.

7. **sunderpeeche**
June 29, 2006

Say the above in the rebuttal letter if WSJ publishes the Zwiebach letter.

8. **D R Lunsford**
June 29, 2006

Robert – this completely misses Peter's point. QED has definite equations that come about in a perfectly well-defined physical way, that can be evaluated with a certain amount of – apparently physically plausible – wishful mathematical thinking. Some people (Chris Oakley) apparently have taken it farther even than this. Yes, we know what QED means, in all its details, once the renormalization program is accepted as a tentative approach to *actual calculation*.

The point about black holes – which you also missed – is that what is being described by Hawking does *not* require a detailed microscopic theory, any more than evaluating the efficiency of a refrigerator requires quantum statistical mechanics. So the kudos for keeping your beer cold go to Carnot and Gibbs, not Ehrenfest and Landau.

-drl

9. **Quanta**
June 29, 2006

Why does Hawking get so much more credit than Bekenstein? Perhaps I'm missing some historical perspective, but Bekenstein laid out a beautifully simple argument as to why black holes have entropy and that the entropy must be proportional to the surface area. I thought Hawking's contribution was to reconcile the apparent contradiction that black hole radiant by invoking quantum mechanics?

10. **anonymous**
June 29, 2006

if string theory has 10^500 vacua and the SM has 20 parameters, to check string theory in the way suggested by Zwiebach we simply need to predict and measure the first 25 digits of each one of them. It sounds like a joke.

11. **joe minahan**
June 29, 2006

Peter said:

—They may very well not find them credible or worth publishing.

Why would they not find him credible? He is a professor of physics at a leading institution and has a long publication record. He has also written a famous string theory textbook, based on an undergraduate course he developed and taught at MIT that received the top university teaching prize.

But then again, he doesn't have a blog…

12. **Thomas Larsson**
June 29, 2006

Does string theory really say anything about the kind of black holes which, perhaps, have been observed? I thought that Strominger-Vafa was about extremal black holes, whereas real black holes are very non-extremal.

13. **Bob McNees**
June 29, 2006

Hawking did not show that a BH has an entropy. He showed that a quantized field in a BH background experiences particle production at a specific temperature related to the surface gravity of the black hole. From there, he expanded on Bekenstein's claim of a generalized second law by suggesting that there are quantities (the surface gravity and area) that seem to play the role of a temperature and an entropy. So he showed that there is a quantity that is analogous to an entropy. He was very clear in making this distinction in his 1974 paper.

Why doesn't this demonstrate the existence of a "real entropy"? An entropy counts the number of microstates consistent with a course grained description of a system. Unless you can count the microstates, you haven't demonstrated the existence of an entropy. So when Peter states "Hawking showed was that there is a reason black holes have entropy and temperature, completely independent of the microscopic description", that is wrong.

Even if you aren't interested in quantum gravity, this should be clear. As Peter said, Hawking's calculation used semi-classical gravity, where the matter is quantum mechanical but gravity is classical. So it should come as no surprise that a framework in which gravity is treated classically can not explain "why" a feature of the gravitational field has an entropy associated with it.

Peter points out that any theory of quantum gravity should reproduce this, and he's correct. String theory, as a quantum theory of gravity, offers several examples of black holes where the microscopic degrees of freedom *can* be counted. The result is one-quarter of the area, as predicted by Bekenstein and Hawking. In that sense, String theory explains why these black holes have an entropy, as Zwiebach claimed in his letter.

14. **woit**
June 29, 2006

Joe,
You've changed my words. What I wrote was about the credibility of his arguments, and was based on scientific arguments that I made in detail in the posting. Instead of addressing my points and discussing the scientific questions at issue, you put words in my mouth and make this about who has more impressive scientific credentials.

Lubos Motl is a string theorist with many publications and is a faculty member at Harvard. Do you think that the Wall Street Journal should publish one of his rants if he were to choose to send it to them?

The attempt to change the topic from the science at issue to who has the best credentials is one of the tactics I was expecting here. It's not going to work.

15. **sunderpeeche**
June 29, 2006

Eh? If and when the WSJ receives Zwiebach's letter they will check who he is and his credentials (professor at MIT etc), and will almost certainly publish it. If LM writes a letter to WSJ they will check on him too. If his letter is a rant they will justifiably toss it. But Zwiebach's letter is not a rant.

16. **woit**
June 29, 2006

Bob,

I think this is all a matter of language, we don't disagree on the science. Hawking showed that semiclassical arguments imply that black holes carry properties with the characteristics of entropy and temperature, and that any underlying quantum gravity theory has to have states that realize these properties. To me that counts as an "explanation". Understanding the quantum states is of course a more complete explanation.

Thomas is also correct that string theory has yet to provide this kind of microscopic explanation for physical black holes, so in this case the only explanation is Hawking's. If one wants to demand a true, complete fundamental explanation of the entropy and temperature here, one should also note that the string theory brane arguments, in the absence of a full non-perturbative theory, show just that the brane framework passes a consistency check, and a final explanation of all this still does not exist.

Anyway, I note that all of this defense of Zwiebach's points is about the black hole issue, which is actually irrelevant to the question here, whether string theory is unpredictive, i.e. not even wrong. Is anyone willing to defend the claim that what is currently known about string theory gives "an extraordinarily precise and rigorous framework where facts can be proven beyond doubt and computations give unequivocal answers"? The only other string theorist I've ever heard go on like that is Lubos.

17. **woit**
June 29, 2006

sunderpeeche,

My comment was just that it should be the content of the letter that is the main deciding factor, not who wrote it. If the WSJ consults other experts and they say that Zwiebach is just wrong, should they publish it?

I honestly don't know what they will do or how they will make a decision. Editors of a business publication are not in a very good position to figure out what is going on in the case of a contentious scientific argument in a notoriously difficult and obscure field. Good luck to them in dealing with it.

18. **Bob McNees**
June 29, 2006

Joe quoted you. You said:

"First let's see which if any of the letters the WSJ decides to publish. They may very well not find them credible or worth publishing. If they do, then maybe a rebuttal would be in order."

So Joe did not change your words (though he did quote a single sentence from a three sentence paragraph). He just asked why they would have a reason to doubt Zwiebach's credibility, and followed up with a list of reasons why they might consider him a reasonable source.

I don't think he attempted "to change the topic from the science at issue to who has the best credentials", even if that is "one of the tactics [you were] expecting here". You suggested the WSJ might "very well" decide that the letter wasn't credible, and Joe pointed out that that probably won't be the case.

You may have meant other letters, or you may have meant that you feel like the WSJ editors won't buy Zwiebach's arguments, but there doesn't seem to be anything unreasonable about Joe's interpretation of, and response to, your exact words.

19. **Bob McNees**
June 29, 2006

I was writing my last comment when you posted your reply to sunderpeeche, which sounds much more reasonable to me than your first reply to Joe.

20. **woit**
June 29, 2006

Bob,

My English was quite clear. "them" refers to the letters and their contents, not to people who wrote them. Joe changed "find them credible" to "find him credible", which is something quite different.

21. **sunderpeeche**
June 29, 2006

The WSJ is fundamentally a profit-making business. Unless the letters received are obviously polemics, or slander/libel, the WSJ has no reason not to publish them. What it would like best is a free-for-all flood of letters and watch the fur fly. It might even (gasp!) boost readership for a few days.

22. **joe minahan**
June 29, 2006

Peter said:

You've changed my words. What I wrote was about the credibility of his arguments, and was based on scientific arguments that I made in detail in the posting. Instead of addressing my points and discussing the scientific questions at issue, you put words in my mouth and make this about who has more impressive scientific credentials.

I did not change your words, I quoted your line. In any case, I was only responding to whether the WSJ would publish his letter. I simply pointed out that the WSJ will likely take into account Barton's professional credentials when they make their decision whether to publish or not. Why wouldn't they? I doubt that they are going to closely examine the finer points of Strominger-Vafa. By mentioning his credentials it was not meant as a comparison to anyone elses.

Okay, I also made a snarky comment about having a blog. So what.

23. **woit**
June 29, 2006

Joe,

To repeat myself:

"find them credible", with "them" clearly referring to letters, is not the same as what you wrote: "find him credible"

24. **RHIC?**
June 29, 2006

dear Peter, you have not quoted this part of Zwiebach letter: "theories of strong nuclear forces are equivalent to theories of gravity. Over the last two months, several new papers use string theory to describe the motion of quarks in the plasma created by the Relativistic Heavy Ion Collider at Brookhaven!"

Indeed strings have been tried as an approximation to QCD. Maybe somebody knows how good the string approximation is and what they are trying to compute?

25. **anonymous**
June 29, 2006

Peter,

LOL. You made a straightforward point that they might not find his letters credible in content, and you have to clarify it again and again and again and … with ppl continuously misconstruing it. Actually it makes perfect sense, since argument by authority is one of the strongest ones they have…

If this is the annoyance with one trivial paragraph you wrote, good luck in dealing with less idiotic critiques! I don't envy you your position…

26. **woit**
June 29, 2006

RHIC,

What's discussed in the WSJ article and is at issue here is whether string theory is "Not Even Wrong" as a unified theory of particle physics and quantum gravity. Whether you can use it to solve QCD is a very different issue, there it certainly has promise. The calculation Zwiebach mentions is part of that story, a really different topic. If string theorists were just saying that they were working on a way to solve QCD, there would be no controversy here.

27. **Anonymous**
June 29, 2006

If string theorists were just saying that they were working on a way to solve QCD, there would be no controversy here.

You hear that, everyone? A well-defined route to ending this blog in finite time! Sounds promising….

28. **Arun**
June 29, 2006

Well, some physicists are joining the ranks of snake-oil purveyors and used-car salesmen. Perhaps this is typical of our times, when the truth without embroidery is not interesting to almost anyone.

29. **John A**
June 29, 2006

I wonder how much science is settled in the Letters page of the Wall Street Journal? Not much.

I would suggest that Peter make a reply to Zweibach with quotations from his book. It's a win-win situation.

30. **sunderpeeche**
June 29, 2006

One might ask why did the WSJ print an article on string theory at all?

31. **knotted**
June 29, 2006

There's finally a short but sweet review of "Not Even Wrong" tucked away on page 56 of the 1 July issue of New Scientist:

"String Weary

"Not Even Wrong … Reviewed by Amanda Gefter

"This is like two books in one. The first is a technical overview of the mathematical structure of the standard model of particle physics. The second is a highly readable look at the sociology of string theory. In the end, Woit ties the two together in one fell swoop by suggesting that physicists should search for answers in the unexplored symmetries of the standard model, rather than in strings. Woit is taking a shot at the string theory clique, but this book is more than that – it is a call to arms for physicists to pursue multiple paths in search of truth, not funding."

Seeing that the powers that control Wikipedia aren't happy with a lot of links to reviews of N.E.W. on the Woit article, perhaps Peter can sometime get around to actually putting a list of links to newspaper an magazine reviews on this blog somewhere (say somewhere low down on the right hand side of the main page). Or perhaps Peter can put up a page listing the contents of the book somewhere as he wrote long ago that he would?

I've seen N.E.W. on sale ONLY in large city bookshops, so far, in the U.K. So don't get the idea it is widely available offline over here. Do you know how many copies they've printed? Since it's hardback the usual figure in the U.K. for a well established London publisher is 5,000. That isn't going to compete with the established string theory books.

In the small/major towns, the popular science shelves of bookshops are still creaking under a load of stringy speculation, with not a single copy of N.E.W. yet in sight. OK, so the Sunday Times marketing dept have have sold some after their review, and people can order it in from any bookshop, but don't get complacent. Maybe you should take a look at the tacky publicity techniques string theorists use for their popular books? (Like tacky internet sites full of promotion gimmicks; when wrestling with pigs people must be prepared to get dirty, or lose.)

32. **Borun D. Chowdhury**
June 29, 2006

Hawking and Bekenstein showed that the entropy has to be proportional to area. They did not show what the microstates are. Moreover Hawking's argument of thermal radiation leads to information paradox and this is where string theory (if correct) will contribute. One should be well informed on what string theorists working on black holes are trying to do before comparing their work with Hawkings.

33. **knotted**
June 29, 2006

Hi Boron,

I notice you say at the top of your home page: "I am a graduate student in Physics at the Ohio State University. I am currently trying to understand **Supersting** [sic] Theory."

34. **Torbjörn Larsson**
June 29, 2006

"If there are 10^1000 or more of these solutions, all evidence is that identifying which of them have desired properties (e.g. the correct CC) is an inherently computationally intractable problem."

Eh? Wasn't a result that it was a NP-complete problem, so if the number of solutions are finite it is computationally tractable? And isn't there a recent paper that shows the finiteness?

OTOH, if you mean practically tractable…

35. **Peter Woit**
June 29, 2006

Borun,

Just who is it who you think doesn't know what string theorists working on black holes are trying to do? It seems to be a pathology common in the string theory community to believe that non-string theorists don't know well-known things about string theory, a pathology especially common among string theory graduate students. Just because you just learned something doesn't mean that people who have been around this business for 30 years don't know it.

36. **Peter Woit**
June 29, 2006

Torbjorn,

Everyone seems to believe that the recent paper by Acharya and Douglas "shows" finiteness, when, if you look at it, you'll see that they are just making conjectures. It's amazing how much of this goes on in string theory, with conjectures magically being quoted by everyone as solid results.

And no, even with the kind of finiteness that Acharya and Douglas conjecture, the words "computationally tractable" definitely don't apply in this case.

37. **Robert McNees**
June 29, 2006

Peter said:

"Borun,

Just who is it who you think doesn't know what string theorists working on black holes are trying to do? It seems to be a pathology common in the string theory community to believe that non-string theorists don't know well-known things about string theory, a pathology especially common among string theory graduate students. Just because you just learned something doesn't mean that people who have been around this business for 30 years don't know it. "

This is an unnecessary comment. Where do you get off jumping down his throat for not being properly deferential to you? You've stated, on this blog, that you aren't interested in spending your time on quantum gravity. He is, so give him a break. He made a fair comment. There are explanations of Hawking's contribution on this page that are wrong. Whether or not it was a mis-statement, or a question of language, is beside the point. Furthermore, he brought up the information paradox. That is relevant to the discussion.

For someone who was so worried about making this discussion about "who has more impressive scientific credentials", that was a very telling comment.

38. **LDM**
June 29, 2006

"This is what Hawking did back in 1974 with a semi-classical calculation."

Hmmm…The Hawking paper I have says "Received April 12, 1975."

If you read the paper, you find Hawking and before him Beckenstein only make the argument that S + A/4 is a reasonable definition of Entropy – by using an analogy with classical thermodynamics and an earlier 1973 result for the FIRST law of thermodynamics for black holes — and not because a density of states calculation was done… You will also discover that particle emission by black holes (the MAIN result of the paper) is REQUIRED to prevent violation of the Generalized Second Law (entropy does not decrease). Beckenstein made his analogy without this result…and so Beckenstein was technically wrong, and Hawking justly deserves the credit.

However, it is a HUGE improvement to get the result from a first principles density of states calculation. Hawking's result applies to ALL (charge, uncharged, rotating, whatever) black holes…string theory has made a few calculations that agree with Hawking….the only question is do you believe they are valid calculations. If so…then Zwiebach is partly correct…The problem I have is these string theory papers are sometimes so weakly refereed and authored…I am not sure I believe these calculations.

39. **Peter Woit**
June 29, 2006

Bob,

This has nothing to do with "being properly deferential" to me, and has nothing to do with my comments about not wanting to spend more time thinking about quantum gravity. Anyone who has ever read any of the hundreds of overhyped promotional pieces about string theory knows that string theorists have calculations counting microstates of black holes and what an important contribution to physics they believe this to be.

Sorry, but I'm just sick and tired of being insulted as ignorant by completely clueless, arrogant string theorists, young and old, who think that this is an appropriate way to behave in response to scientific criticism of string theory. I'm not going to put up with it, and will jump down their throats when they do it, no matter who they are (except for Lubos, who is playing a wonderful role as public poster-boy of this particular pathology, and so deserves to be encouraged).

40. **Peter Woit**
June 29, 2006

LDM,

My source, I'm not proud to say, was Wikipedia, which says 1974. The SPIRES entry for the paper however also says 1974. Perhaps it was circulated earlier as a preprint before the final version was submitted to CMP.

41. **sunderpeeche**
June 29, 2006

If one is going to critique string theory and call it "not even wrong" (or anything else ~ the oil industry?) then one has to take what comes. It goes with the territory.

42. **Peter Woit**
June 29, 2006

sunderpeeche,

Sure, I refer to string theory as "not even wrong", but that is a criticism of a scientific theory and not a personal attack on anyone.

I'm quite used to being insulted as an ignorant crackpot by string theorists who don't have a scientific argument. It does go with the territory and I'm sure there will be a lot more of it to come. But one wonderful aspect of this glorious new blog software is that when they choose to do it here, I can immediately give them a piece of my mind.

43. **Eugene Stefanovich**
June 29, 2006

Peter:

Sorry, but I'm just sick and tired of being insulted as ignorant by completely clueless, arrogant string theorists, young and old, who think that this is an appropriate way to behave in response to scientific criticism of string theory. I'm not going to put up with it, and will jump down their throats when they do it, no matter who they are…

Chill out! You have good scientific points, and most people reading your blog (even your opponents) recognize that. They can also see for themselves who is clueless and who is arrogant. When you "jump down somebody's throat" it doesn't help your cause. There is no need to be offensive. Stick to your scientific arguments, and you'll be o.k.

44. **Peter Woit**
June 29, 2006

Eugene,

Excellent advice. The ability of the glorious new blog software to let me immediately give someone a piece of my mind may not be such a good thing…

45. **Robert McNees**
June 29, 2006

"This has nothing to do with "being properly deferential" to me"

Sure it does. You said "Just because you just learned something doesn't mean that people who have been around this business for 30 years don't know it.", which means that he should know better than to try and tell you something about Hawking's caclulation. And you said it on the same page where you accused Joe of trying to turn this into a discussion about who has more impressive scientific credentials.

In fairness, I would also be annoyed if I felt like a grad student were lecturing me about something I understand. But has he ever commented on your blog? Does he have a history of being uppity with you? How do you justify your claim that he thinks you are "ignorant"? For all you know he was trying to contribute to the discussion. But you're upset about the way you perceive people as treating you, so you jumped on an easy target.

Feel free to kill this, because it's headed off-topic.

46. **joe minahan**
June 29, 2006

Peter said:

"find them credible", with "them" clearly referring to letters, is not the same as what you wrote: "find him credible"

Peter,

My mistake was not putting in the full quote, so here it is:

-First let's see which if any of the letters the WSJ decides to publish. They may very well not find them credible or worth publishing. If they do, then maybe a rebuttal would be in order.

The clear implication from this quote is that the WSJ might not find Zwiebach's letter credible or worth publishing. Maybe that was not your intent, but a reasonable person could certainly take this as its meaning. I only pointed out that the WSJ would have good reason to treat Zwiebach as an expert on this issue, based on his scientific credentials.

It's your blog so I am sure you will get the last word on this.

47. **Malo Juevo**
June 29, 2006

Ridiculously overbroad claims are nothing new for Barton.

When I was in graduate school, I remember looking at some of the stuff he was working on (with Minahan, I seem to recall). It was actually fascinating stuff, and although his interest in the subject matter was related to string theory, no knowledge of or regard for strings was required to appreciate these particular explorations of quantum theory. It was stuff that I would describe as having an "extraordinarily precise and rigorous framework," in fact. It was just utterly divorced from real-world physics.

He was doing very good work on this stuff, but at the same time, he made claims about this material that were ludicrous! He was discussing the material at one fairly informal seminar, and at one point, he displayed some extremely interesting solutions to the theory. Then he claimed that he had shown that all the solutions had this form. Scribbling on a borrowed piece of paper at the back of the room, I had falsified this claim by the end of his talk, by finding a very special exact solution. I showed him the solution, and he acknowledged it was correct. (Barton is a very nice guy, and I respect him for that, at least.) A little more work showed where his reasoning had gone wrong. It looks like he had been trying to do something like inferring the properties of a function from working, term by term, with an assymptotic series–not a very reliable method.

But it's exactly the kind of technique that would "allow" one to infer facts about nonperturbative string theory from what is known right now.

48. **David**
June 29, 2006

Peter,

I might be remembering wrong, but the initial Hawking result was first published in a very criptic short letter to nature in 1974. It was criptic because it didn't have much details for one to assess the validity of the result. This letter was before the CMP article.

One should be aware that if one performs Hawking's calculation in Schwarszchild coordinates, one does not obtain radiation and entropy. On the other hand, this might not be kosher as it leads to all sorts of divergences as one approches the horizon. I forget who performed this calculation, but it was before Hawking in the early 70's in a PRD article. I am not claiming Hawking to be incorrect. I am just pointing out that there are many inconviniences and inconsistencies with semiclassical gravity that are ignored and have never been fully resolved.

49. **Garbage**
June 29, 2006

The only reference I have seen in the letter to ST as a theory of anything, and not just stringy versions of known physics (yarn theory? 🙂), is the Black hole entropy/temperature thing, though so far the "explaination" for the real ones is yet to come to your hometown theaters [It is absurd to say that ST explains BH entropy, as absurd as the claim it predicts gravity.], and the following sentence:

"All that is needed to confirm string theory is finding one solution that describes our universe. All that is needed to rule out string theory is showing that no solution describes our universe. An answer must exist."

Such postdictive power would look better in a history book than any science literature. I would rather predict which one is ours and why. In fact, is the theory ever going to tell us which universe we live on or that comes as an external bit of information?
It is true in the other hand that to rule out ST it is enough to show that no solution describes our universe. That's exactly what the Distler et al. paper attempts. If our universe violate "the bounds" steming from assumptions A,B,C (unitarity, analyticity and Lorentz invariance), any ST version having A,B,C as properties must be discarded. So far I havent seen any string theorist claiming Lorentz invariance isnt a fundamental ingridient of the theory [Besides Peter's comments on vague ideas of David Gross]. If I remember correctly at the end of the string05 conference Lee Smolin pointed out that tests of Lorentz invariance would falsify ST, to what Ed Witten was reluctant to comment on. If LHC turns on, we dont have a light Higgs and the bounds are violated, we better start taking Gross' ideas a bit more seriously, or perhaps also taking a closer look to LQG et al. 😉
I personally find such possiblity fascinating, though logically possible and physically unlikely…

50. **Peter Woit**
June 29, 2006

Joe,

You're quite right, what I said was that the WSJ might not find Zwiebach's letter credible. Anyone who has listened to many famous string theorists publicly saying that we still don't know what string theory is might find his claims in the second paragraph of the letter to be not credible. The distinction you seem to be not able to get is that the credibility of an argument and the credibility of a person are two quite different things. I was only referring to the credibility of the arguments in the letter and I objected very much to your turning that into a comment on his credibility as a person.

The most credible person in the world will sometimes make an argument which is not credible. This doesn't make them not the most credible person in the world, for that they have to keep making such arguments.

For the record, I think the WSJ should publish Zwiebach's letter, even though I think part of it is scientifically inaccurate. The scientific issues are not ones they are capable of adjudicating, and having a response from a well-known string theorist to the article is quite appropriate. If they don't publish it, it would be most likely because it is only one of several letters from that side they receive, and they do have space limitations.

I was thinking of taking "Malo Juevo" to task for not sticking to science, but on rereading his comment, it is a relevant story. The fact that you can't infer properties of a function from properties of terms of its asymptotic series is precisely the problem at issue here.

Bob,

Yeah, this is definitely off-topic and not a useful discussion. I'll admit that some days I do have a short fuse on this particular topic, and, as Eugene pointed out to me, would do better to ignore some people rather than respond to them.

51. **Roy**
June 29, 2006

"What Hawking showed was that there is a reason black holes have entropy and temperature, completely independent of the microscopic description. This explains these two facts, you don't need to know the microscopic definition."
I must admit don't know the technical details of string theoretic calculation of microstates, but isn't that the entire point of thermodynamics and stat. mech? For example, most of undergraduate thermal physics can be done without stat. mech by somehow guessing or physically arguing for a equation of state or the some potential function, but the stat. mech approach is superior because it actually derives everything from first principles. Of course stat. mech is not certainly limited only to providing proofs for thermodynamic equations– it has infinitely many more interesting applications and ideas and I do not know whether the string theoretic approach has similar advantages and I think Borun is claiming(and perhaps quite correctly) that string theory provides a better framework for the understanding of the black hole entropy calculation(Borun, please correct me if I am wrong here)
If string theory has managed to derive a semi-classical result that is widely believed to be true– and managed to calculate the microstates correctly– isn't that a strong point for the credibility of string theory?
May be you had further objections to Borun's claim, or in general this string theory result, based on technical points that I do not understand and in that case it will be nice if you would explain why you think that string approach is not satisfactory in this case.

Again, I am not trying to be confrontational or arrogant here– I would genuinely like to know your objections .

52. **Arun**
June 30, 2006

Is it true that the extremal string blackhole has the right Bekenstein entropy, but has no energy to radiate because it is a minimal energy state of the string? i.e., where the exact calculation is possible, we have the entropy but not the Hawking radiation? Are non-radiating black holes a prediction of string theory?

53. **scott**
June 30, 2006

Peter,

I doubt that joe doesn't understand that "the credibility of an argument and the credibility of a person are two quite different things" However he thinks, and with good reason, that WSJ not being a qualified expert, will likely simply look at his credentials instead of spending an employees time contacting other experts to see if they think the argument itself is credible. Apparrently regard the WSJ in a much higher regard then Joe. I think Joe's appraisal of how the WSJ would determine credibility to be more realistic.

However there is another possibility that they will simply not publish it because they don't think the issue is worth the space regardless of credibility. Or as you point out because they publish some other string theorist's responce.

54. **scott**
June 30, 2006

Apparrently regard the WSJ in a much higher regard then Joe. I think Joe's appraisal of how the WSJ would determine credibility to be more realistic.

um this should say something like:

Apparrently **you** regard the WSJ in a much higher regard then Joe. I think Joe's appraisal of how the WSJ would determine credibility to be **the** more realistic **one**.

there is some other sentences with bad grammer but they are(I think) still understandable.

55. **scott**
June 30, 2006

and of course even in my correction the grammar is still f'ed up.

56. **Arun**
June 30, 2006

One may also ask how the ordinary matter from which any black hole in our universe is assembled turns into the highly wound-up string + brane state in the string blackholes.

If this is not known, then the result really is that the exotic string matter that makes up a string blackhole has the correct number of microscopic states to produce the Bekenstein entropy. I agree it is an impressive result, in that we cannot yet count the microscopic states in any other framework. There are some scenarios in string theory that gravitationally are blackholes and where microscopic states can be counted; but presumably there are many scenarios (e.g., with everyday matter) where microscopic states cannot be counted, and one has to demonstrate this doesn't matter, the count of microscopic states remains the same.

Presumably molecules interacting like billiard balls, or with van der Waals forces or with any of a large set of force laws produce the right thermodynamics, and thus thermodynamics sets only a weak constraint on how molecules may interact.

57. **woit**
June 30, 2006

Roy,
Yes, it certainly is a point in favor of string theory if it can provide a microscopic definition. I was not arguing with that, all I was doing was pointing out that you don't need the microscopic definition to know what the entropy and temperature of a black hole will be, Hawking already told us that long ago. It's also true that string theory arguments do not provide such a microscopic definition for physical black holes, which Zwiebach neglects to mention.

58. **D R Lunsford**
June 30, 2006

RE Motl's latest rant –

Motl is doing classic Jungian shadow projection, just like O'Reilly does. Obviously he is insecure in his own understanding, and this is why he does not argue facts, only opinions – he doesn't have facts to argue with, nor the grounding in history to draw correct inferences. (Compare O'Reilly's claim that Germans were massacred by American GIs at Malmedy.) Effective shadow projectors often rise to prominence – only to have their crippling flaw publically exposed. This is some kind of healing process in the projector's divided psyche.

-drl

59. **Torbjörn Larsson**
July 3, 2006

"Everyone seems to believe that the recent paper by Acharya and Douglas "shows" finiteness, when, if you look at it, you'll see that they are just making conjectures."

I took it they were pretty solid results, and that they confined the number of string vacua. Thank's for supplying the missing reference!

"And no, even with the kind of finiteness that Acharya and Douglas conjecture, the words "computationally tractable" definitely don't apply in this case."

I found http://www.citebase.org/cgi-bin/citations?id=oai:arXiv.org:hep-th/0602072 by Denef and Douglas that argued that "such problems are typically NP hard". That is different yes, they are computationally intractable.

Some Links

June 29, 2006
Categories: Uncategorized

The Cao-Zhu paper at the Asian Journal of Mathematics that is supposed to have a complete proof of the Poincare and geometrization conjectures is still not available, but the introduction to the paper has been posted there.

If you're not getting enough string theory bashing today, head over to John Horgan's Scientific Curmudgeon blog, where he has a posting entitled Pulling the Plug on Strings. It contains a wide selection of string-puns (or whatever you call such things), and he has decided to refer to string theory advocates as "yarn-heads" and braniacs. For his trouble, his comment section is under assault by the usual suspects. There's also this site, containing a graphic mentioned here before which I refuse to admit to finding funny. The proprietors have an interesting way of dealing with the comment section.

Sabine Hossenfelder has an excellent posting on Science and Democracy.

This past week I've spent some time at the 26th International Colloquium on Group Theoretical Methods in Physics, being held here in New York at the CUNY Graduate Center. It was ably organized by Sultan Catto, who somehow convinced me to give a short talk on the blog and the book, one where I think I disappointed people by keeping string-bashing to a minimum. I enjoyed seeing people at the conference, and there were some good talks, including one by Greg Moore on his recent work with Dan Freed and Graeme Segal (see here and here).

Urs Schreiber has been putting his notes on-line about elliptic cohomology. Lots of interesting material, but his comment that he expects the landscape of superstring theories to be equal to the spectrum of elliptic cohomology sounds frightening. Maybe he means a different landscape…

I was quite sorry to hear of the recent death of Irving Kaplansky. Kaplansky was an algebraist, and director of MSRI when I was there in 1988-89. At the time I wasn't much interested in algebra, so didn't talk to him about math, but he was responsible for making MSRI a really wonderful place to work.

Update: The slides from Yau's talk at Strings 2006 are now available here.

Comments

1. **Chris Oakley**
   June 30, 2006

   I think that Peter was right not to use this jacket in the end: "Superstrings" should be in the plural, and having basic mistakes like this only gives ammunition to critics.

2. **Kea**
   June 30, 2006

   Re Urs:…*but his comment that he expects the landscape of superstring theories to be equal to the spectrum of elliptic cohomology sounds frightening*.

   Maybe he just means that rederiving the landscape rigorously will put a solid nail in the Landscape as a *physical* idea.

3. **Kea**
   June 30, 2006

   I meant coffin thereof, of course.

4. **urs**
   June 30, 2006

   > […] sounds frightening

   The fun of making things precise: you suddenly actually know what you are talking about. 😉

   The space of 1d SQFT is homotopy equivalent to the K-theory spectrum. That's a theorem.

   It is a conjecture (well motivated, though) that, similarly, the space of 2dSCFTs is homotopy equivalent to the spectrum of elliptic cohomology.

   (Of course you need to make precise what these QFT spaces are. You do this by looking at certain transport functor spaces.)

   Actually, that's an old idea. Segal has proposed 20 years ago in the last section of his paper on elliptic cohomology, that CFTs should be elliptic cocylces, roughly.

   The statement has been refined and sharpened, and brought a little closer to being provable, in the last ten years by Stephan Stolz and Peter Teichner.

   I gave the talk in our seminar in front of a mixed audience of mathematicians and string theorists. So I simply went ahead and mapped some math terms to the corresponding string terms.

   Under that dictionary, "space of SCFTs" becomes "landscape".

   All up to some details.

5. **urs**
   June 30, 2006

   Ah, and maybe this is a good opportunity to mention that it was in particular Aaron Bergman who originally made me aware of these topics and of the relevant literature. Thanks a lot!

6. **ObsessiveMathsFreak**
   June 30, 2006

   From the John Horgan article:

> Along with such quasi-scientific notions as Gaia, complexity theory, psychoanalysis and the anthropic principle, strings seize the public's imagination not because they explain the world but because they mystify it.

That's a very damning statement, for any scientific theory. Science has always been rolling back the fog of ignorence and mysticism. For any science to reintroduce it is a cardinal sin.

7. **Kea**
June 30, 2006

Urs

Please correct me if I'm wrong, but AFAIK SCFT would only describe one sector of the Landscape…and the evidence for the Landscape that some people take to be most compelling does *not* belong to that sector.

8. **Bee**
June 30, 2006

Hi Peter,

thanks for the link to my post.

I am still at reading your book, and I just found that you formulated some of my concerns about the specialisation much clearer than I could ever had (pp 205):

"This huge degree of complexity at the heart of current research […] means that a huge investment in time and effort is required to master the subject well enough to begin such research. […] Since the whole subject is so complicated and difficult, theorists trying to evaluate what is going on often rely to an unusal extend not in their own understanding of the subject, but also on what others say about it." (possible typos are entirely mine)

Best regards,

B.

9. **urs**
June 30, 2006

> Please correct me if I'm wrong, but AFAIK SCFT would only describe one sector of the Landscape…and the evidence for the Landscape that some people take to be most compelling does not belong to that sector.

Ok, of course I am talking about the space of all SCFTs (but including all information about the possible D-branes for a given SCFT, and also, in particular, all possible worldsheet phenomena like worldsheet instantons). That's what the conjecture would apply to.

So maybe I should say "perturbative landscape" instead of "landscape".

10. **Nicholas**
June 30, 2006

"Moreover, even if most physicists no longer take the theory seriously, stringy memes will continue to infect the culture at large. New Age authors in particular have embraced string theory. The appeal is obvious. Along with such quasi-scientific notions as Gaia, complexity theory, psychoanalysis and the anthropic principle, strings entwine the public's imagination not because they explain the world but because they mystify it."

As a researcher in granular systems I certainly must take some offense with this notion of complexity being first of all lumped in this group and additionally with the very statement that it is somehow mystifying nature.

I am not here to discuss the merits of chaos however, but rather to state that it is not an argument to suggest that someone's misappreciation of science reflects upon the subject itself.

I have often encountered individuals with significant misunderstandings pertaining to heisenburgs uncertainty principle, special relativity or a variety of other physics. That in no way diminishes the validity of these as proper scientific theories, and it should not ultimately reflect on string theory, regardless of that theories actual scientific validity.

NM

11. **Peter Woit**
June 30, 2006

Nicholas,

About string theory, I think Horgan is right, it isn't explaining how to unify physics, it is mystifying the issue. About complexity theory I have no opinion. Horgan is trying to be provocative. If you're provoked, you should take it up with him.

12. **nontrad**
June 30, 2006

At the risk of an 'off topic' comment, I take it Nicholas M. hasn't read Horgan's 'End of Science'.

For NM and others who haven't read it, my proverbial internet 2 cents is …. read that book!!! Seriously, Horgan's book is interesting / entertaining for numerous reasons; including aspects of science journalism, philosophy of science, string theory, cosmology, "chaoplexity", evolution, neuroscience / AI etc (such as narcissitic personality disorder / hubris kicked up copious notches).

Since several of those topics (ST, cosmology, phil of science, sci journos) are repeatedly addressed at 'Not Even Wrong', readers here might well appreciate the book (even if they don't agree with the general thesis).

Horgan's pretty insightful in general, he doesn't pull his punches, and at times he's damn funny!

13. **John Baez**

June 30, 2006

Back in 2003, I wrote week197 of This Week's Finds to explain some stuff Stephan Stolz told me about elliptic cohomology. Elliptic cohomology studies a space by mapping it into some spaces called "tmf(n)", for "topological modular forms". These spaces are currently understood only in an indirect way. Some people conjecture that tmf(n) is roughly the space of supersymmetric conformal field theories of central charge -n. There's a lot of evidence that *something* like this is true.

The space of such theories is not the same as the "landscape" studied by string theorists today… but, it's still an intimidating structure. If the conjecture is true, this structure has a lot more order to it than one might at first guess!

14. **J.F. Moore**
June 30, 2006

I can also recommend 'End of Science' as a well written series of fleshed-out interviews on the general topic of whether fundamental discoveries in science are behind us (and touching on strings as one example). I enjoyed discussing the subject before and after that book was published, and was dismayed when colleagues would crudely dismiss the arguments Horgan put forth based on his 'lack of credibility' rather than putting forth an actual counterargument. John Maddox' rebuttal book was pretty weak and only led me to think more highly of Horgan.

I'll have to read it again, but I suspect it's held up just fine in the last decade.

15. **MathPhys**
July 1, 2006

Urs,

I still find your statment above confusing.

If you include "all information about the possible D-branes for a given SCFT, and also, in particular, all possible worldsheet phenomena like worldsheet instantons", then you should say "landscape" rather than "perturbative landscape".

Can you make the statement of the conjecture really precise, please?

Also the way that you type your notes on the web site using html makes them less than fully readable. Why not use Latex and post a pdf file. It's a pity when so much work is less than fully readable.

16. **Michael**
July 1, 2006

MathPhys,

D-branes appear as boundaries of the worldsheet CFT. Worldsheet instantons are essentially worldsheets wrapped an (small) non-trivial two-cycles in spacetime; despite being non-perturbative they are well under control in a worldsheet description. Thus including these effects is possible in a string-loop expansion, which is what Urs meant by "perturbative". At generic points in moduli space the string-loop expansion is not useful, something people like Peter Woit love to complain about.

17. **MathPhys**
July 1, 2006

Thanks, Michael.

18. **urs**
July 2, 2006

> Also the way that you type your notes on the web site using html makes them less than fully readable.

Which browser are you using?

I have tried not to use any symbols that are displayed only after the user installs extra fonts.

I am currently sitting in some random internet cafe, and the formulas in that entry are fully readable and displayed nicely using

– either plain Mozilla Firefox

– or MS Internet Explorer with the free MathPlayer plugin plugin installed (takes 2 seconds)

But you are of course, right, I could have produced a pdf instead.

19. **urs**
July 2, 2006

> Can you make the statement of the conjecture really precise, please?

Not *really* precise, but more precise than I did before.

I guess it should go like this:

Conjecture: *The space of all 2-functors from superconformal 2-paths to graded 2-Hilbert spaces carries a topological structure and is homotopy equivalent to the tmf spectrum*.

This is not *really* precise, yet, for a couple of reasons.

1) Nobody has yet a good idea of how precisely the 2-category of superconformal 2-paths looks like. Its 2-morphisms are superconformal disks with two marked points on their boundary. Horizontal composition seems to be troublesome. But Hilbert Uniformization might help here.

2) When I talk about 2-Hilbert spaces I am indicating that this is the true structure which is secretly behind the vonNeumann algebra bimodule bicategory that Stolz/Teichner use. In fact, I am pretty sure that what they discuss is the special case of a 2-vector bundle associated to a String-2-bundle, using a representation of the string 2-group on 2-Hilbert spaces.

20. **MathPhys**
July 2, 2006

Urs,
I can read your formulas (I'm using IE 6.0), but the layout is awkward (lines are too long, etc) and things are sort of all over the place. I would like to be able to download what you are write in a clean way and read it, annotate it, etc.

A pdf file of a latex document will give you a lot more versatility. You can also build on it in the future, if you want to, and turn what you write into papers.

Re science, I'm trying to understand what you're saying, and not let your overuse of "decategorification" put me totally off the subject.

21. **urs**
July 3, 2006

> can read your formulas (I'm using IE 6.0), but the layout is awkward

Hm, for some reason this happens on some configurations. Not on the ones I am looking at at the moment, though. I guess that in general Firefox is the better option.

> I'm trying to understand what you're saying, and not let your overuse of "decategorification" put me totally off the subject.

I guess you mean categorification instead of decategorification. And its meant precisely to help you to understand what is going on. *Whithout it*, there is hardly a chance. It is a tool that organizes apperently intricate ideas in a conceptual way.

So that's why I emphasize, in that entry (in the second but last part) the situation for the toy example of 1-dimensional supersymmetric field theory and K-theory.

If you understand this example, and you know how categorification picks every item of this example and increases its dimension by one, you understand the main idea of the conjecture that I stated.

Actually, the main idea becomes pretty obvious then. All that remains are some technicalities. That's what categorification does for you: it provides you with all the big picture and the general ideas. All that remains to be done then is figuring out the technicalities.

I'd be happy to try to answer more detailed questions. But maybe we should do that over on the SCT, lest we run into off-topic territory here.

22. **urs**
July 3, 2006

John Baez wrote:

> Back in 2003, I wrote week197 of This Week's Finds to explain some stuff Stephan Stolz told me about elliptic cohomology.

BTW, I do list TWF197 together with other relevant literature available online at the beginning of the first entry of the series.

> The space of such theories is not the same as the "landscape" studied by string theorists today…

But that's mainly because the landscape studied by string theorists today is a small subspace of the full landscape, namely that subspace whose points satisfy a number of phenomenological prejudices and technical restrictions, like being large volume CY compactifications with fluxes.

Maybe I am wrong, but it seems to me that asking the question "Which of 10^500 large volume CY flux compactifications is chosen, and why?" is only a small subquestion of the full question, and motivated mainly by constraining the full question by available and/or expected phenomenological input.

23. **MathPhys**
July 4, 2006

Thanks, Urs, for your kind offer, which I intend to take you on (and too bad for the German soccer team).

Many Worlds In One

July 2, 2006
Categories: Uncategorized

Alex Vilenkin has a new popular book out about cosmology, entitled Many Worlds In One. It's mainly about the extremely speculative end of cosmology, and much of it is devoted to explaining the author's ideas on eternal inflation, creating the universe by tunneling out of nothing, and the anthropic landscape, together with stories about how he came to these ideas. It contains various amusing anecdotes, especially about Alan Guth. Sean Carroll is credited with the following story:

One of the leading superstring theorists, Joseph Polchinski, once said that he would quit physics if a nonzero cosmological constant were discovered. Polchinski realized that the only explanation for a small cosmological constant would be the anthropic one, and he just could not stand the thought.

He also describes the reaction to his anthropic arguments back during the years when these were not all the rage like they are now:

After one of my seminars, a prominent Princeton cosmologist rose from his seat and said, "Anyone who wants to work on the anthropic principle – should." The tone of his remark left little doubt that he believed all such people would be wasting their time.

Vilenkin's book covers much the same ground as Susskind's, although from the point of view of a cosmologist, not a particle physicist. A huge amount is made of the supposed anthropic "prediction" of the value of the cosmological constant (any news of the rumor from Sean Carroll of new work by prominent Princeton cosmologist Paul Steinhardt showing this is bunk?). Unlike Susskind, Vilenkin at least doesn't seem to be on a campaign to attack the "Popperazi" and convert everyone to anthropics, but he demonstrates a similar lack of concern for the fact that the ideas he is discussing don't lead to much if anything in the way of a testable experimental prediction.

Here's his scientifc program for 21st century physics, which he hopes will be spent working on the anthropic landscape:

First, we will need to map the landscape. What kinds of vacua are there, and how many of each kind? We cannot realistically hope to obtain a detailed characterization of all 10^{500} *vacua, so some kind of statistical description will be necessary. We will also need to estimate the probabilities for bubbles of one vacuum to form amidst another vacuum. The we will have all the ingredients to develop a model of an eternally inflating universe with bubbles inside bubbles inside bubbles… Once we have this model, the principle of mediocrity can be used to determine the probablility for us to live in one vacuum or other.*

Unfortunately for this research program, it has yet to even begin to get off the ground, and there are very good arguments that it can never succeed. There are an infinite number of possible vacua, and trying to make this finite so one can do statistics requires putting in cutoffs, with results then strongly depending on the cutoff. The large numbers of these vacua make any attempts to identify ones that agree with the real world computationally completely intractable. Even if one could do this, all evidence is that one would end up with broad statistical distributions for many of the parameters of the standard model, providing no useful prediction of what new experiments will see, or any insight into why these parameters have the values that they do.

Comments

1. **MathPhys**
   July 2, 2006

   Look on the bright side, Peter. They have solved all our problems. In the immortal words of Alyosha Zamolodchikov when asked what he thinks of strings as the final theory of everything

   "That's good. Now we can all relax and go and have a drink".

2. **Tommaso Dorigo**
   July 2, 2006

   Hi,

   as a plot-inclined person, I would be soooo happy to see a graphical description of just one of the 19 parameters of the standard model as a function of any one of the possible needed cut-offs that allowed a mediocrity principle to work out… That would be a start.

   …Naah. It would only mean starting to get plot-bombed by 10^500 possible graphical descriptions…

   Cheers,
   T.

3. **Who**
   July 2, 2006

   A huge amount is made of the supposed anthropic "prediction" of the value of the cosmological constant (any news of the rumor from Sean Carroll of new work by prominent Princeton cosmologist Paul Steinhardt showing this is bunk?).

   maybe this is merely background to still newer work?

   http://arxiv.org/abs/astro-ph/0605173
   Why the cosmological constant is small and positive
   Paul J. Steinhardt, Neil Turok
   15 pages, 1 figure
   "… we show that a cyclic model of the universe can naturally incorporate a dynamical mechanism that automatically relaxes the value of the cosmological constant, … nearly every volume of space spends an overwhelming majority of the time at the stage when the cosmological constant is small and positive, as observed today."
   ==================

   As for straightforward debunking, Abraham Loeb has presented an observational test to show that the CC is not of anthropic origin:

   http://arxiv.org/abs/astro-ph/0604242
   An Observational Test for the Anthropic Origin of the Cosmological Constant
   Abraham Loeb (Harvard)
   JCAP 0605 (2006) 009

"…Here we propose a simple empirical test for this anthropic argument within the boundaries of the observable Universe. We make use of the fact that dwarf galaxies formed in our Universe at redshifts as high as z~10 when the mean matter density was larger by a factor of ~10^3 than today. Existing technology enables to check whether planets form in nearby dwarf galaxies and globular clusters by searching for microlensing or transit events of background stars. The oldest of these nearby systems may have formed at z~10. If planets are as common per stellar mass in these descendents as they are in the Milky Way galaxy, then the anthropic argument would be weakened considerably since planets could have formed in our Universe even if the cosmological constant was three orders of magnitude larger than observed. For a flat probability distribution, this would imply that the probability for us to reside in a region where the cosmological constant obtains its observed value is lower than \~10^{-3}. A precise version of the anthropic argument could then be ruled-out at a confidence level of ~99.9%, which constitutes a satisfactory measure of a good experimental test."

Loeb disposes of any *a priori* argument that the CC is anthropically determined, since it is plausible that habitable zone planets will be detected in the descendents of dwarf galaxies (this is what he proposes searching for) which would rule out an anthropic CC, as he explains. One has every reason to expect to find evidence of planet formation in dwarf galaxies just as one finds it in our own galaxy.

I don't know if Steinhardt has any current work debunking anthropic CC but it does not seem that he NEEDS debunk it. Loeb has taken care of that. And Steinhardt has proposed a mechanism by which, in his cyclic scheme, the CC value could be explained.

4. **steve**
July 2, 2006

The original Weinberg mechanism never really worked. The anthropically-favored value of the CC depends on the prior distribution of magnitude of density fluctuations. We were not the only ones to notice this…

Anthropic Distribution for Cosmological Constant and Primordial Density Perturbations

Authors: Michael L. Graesser, Stephen D.H. Hsu, Alejandro Jenkins, Mark B. Wise

Journal-ref: Phys.Lett. B600 (2004)

Abstract: The anthropic principle has been proposed as an explanation for the observed value of the cosmological constant. Here we revisit this proposal by allowing for variation between universes in the amplitude of the scale-invariant primordial cosmological density perturbations. We derive a priori probability distributions for this amplitude from toy inflationary models in which the parameter of the inflaton potential is smoothly distributed over possible universes. We find that for such probability distributions, the likelihood that we live in a typical, anthropically-allowed universe is generally quite small.

5. **Chris W.**
July 2, 2006

MathPhys, your comment reminds me of a remark of Stalin: "Death solves all problems (…)."

6. **MathPhys**
July 2, 2006

Chris W,
Your comment on my comment explains many things to me. Keep the immortal sayings of wise Russians coming, please.

7. **MathPhys**
July 2, 2006

and Georgians too.

8. **Martin Kochanski**
July 3, 2006

To the extent that anthropic arguments are intended to answer "why" questions about constants (such as the cosmological constant) that have a continuous range of possible values, they are surely flawed because they can never yield a single answer.

You can have anthropic arguments for the number of dimensions of space – where the possible values are integers – but surely any anthropic argument applied to a continuous-valued constant will still yield an uncountable infinity of allowable values?

9. **Stalin**
July 3, 2006

actually the situation is worse: different regions of the landscape will give different broad distributions; heretic versions of the antrhopic religion will proliferate, unless we obey Slatin's order 227: Ни шагу назад.

10. **annoying lurker**
July 3, 2006

Peter, since the attacks are against the anthropic principle, perhaps you should comment on what you think about Hoyle's use of that principle to successfully predict the fusion of 3 alpha particles into carbon in stars. Hoyle realised that for the observed amounts of C-12, there must be a resonance at 7.65 MeV in C-12. Experiments confirmed it.

Hoyle thought he didn't win the Nobel Prize because he criticised the fact that the Nobel Prize for the discovery of pulsars was awarded to the PhD adviser (Hewitt) of the person who actually made the observation (Bell). However, perhaps it was because of the anthropic principle. Hoyle wasn't predicting anything in the usual sense, just showing that one set of observations (measured carbon 12 abundances in stars, people, etc.) are consistent with a particular nuclear reaction rate.

I think the cancer of corruption in physics is that in default of real understanding, obfuscation is preferred. String theory and its landscape are the ultimate obfuscation with which to sink any questions anyone asks about physics.

When exactly was it that crackpotism won? Bohr at Solvay in 1927, Aspect "falsifying" causality in 1982, or M-theory in 1995?

Do you ever just feel like giving up and embracing the extra dimensional multiverse? It is so much easier to be a crackpot, peter!

11. **sunderpeeche**
July 3, 2006

Hoyle showed that other mechanisms would not work. For our universe to exist (people etc) there had to be a resonance in C-12, and he calculated its energy (correctly). Observations confirmed his claim. That's more than just saying the existence of people is consistent with a particular reaction rate.

? crackpotism Bohr Solvay 1927 ?

12. **annoying lurker**
July 3, 2006

"Hoyle showed that other mechanisms would not work."

Similarly, I can "predict" the width and smoothness of a road from knowing the widths and suspension systems of cars!

Other "mechanisms" (like flying cars through the sky) can be shown simply not to work. Hence, for cars to exist, the roads must possess very constrained, predictable features. Where's my Nobel?

BTW, Bohr wasn't actually awarded a Nobel for disproving causality (despite his long philosophical arguments with Einstein in 1927).

13. **Peter Woit**
July 3, 2006

All,

Unless you really have something new to say, please resist the temptation to start rehashing various aspects of the anthropic principle every time the landscape gets mentioned here. At this point, all this is doing is adding to the noise level.

14. **CapitalistImperialistPig**
July 3, 2006

Peter,

You said "There are an infinite number of possible vacua, and trying to make this finite so one can do statistics requires putting in cutoffs…"

Could you briefly explain this comment and maybe mention the rationale for the cutoff used?

15. **secret milkshake**
July 3, 2006

Stalin: "In the Soviet Army it takes more courage to retreat than advance."

You tell me: Is ST getting like SA? Anybody shot in the back?

16. **z3**
July 4, 2006

NY Times, "Physics Awaits New Options as Standard Model Idles" describes the lack of progress in theoretical HEP in the past 30 years and portraits the sense of frustration in this field. String theory received a passing mention that's sort of dismissive.

17. **kristo**
July 4, 2006

Alexander Vilenkin has an article Philosophical Implications of Inflationary Cosmology published in the March issue of The British Journal for the Philosophy of Science.

18. **woit**
July 4, 2006

CIP,

You should look at Douglas's papers for the details. One part of the story is that he wants to put a bound on the volume of the compactification manifold, arguing that if it is too big, we would see its effects already (i.e., we'd be aware that we live in more than 4 space time dimensions by various experimental results).

This kind of bound (together with others), allows him to argue that possibly the number of vacua given by various flux compactification constructions is finite (because the size of the manifold grows as you add fluxes). One problem with putting in a cutoff though, is that, even if you make the number of vacua finite, the distribution of them may be peaked near the cutoff (lots more ways to put in fluxes at the maximal number of them).

If you look at his sequence of papers on counting vacua, you'll see that he started off hoping that statistical distributions of vacua would allow a landscape prediction of whether the supersymmetry breaking scale was low or high, depending on which was statistically favored. He seems to have given up on this, partially because of this problem. People who talk about statistical calcuations of these vacua possibly providing predictions are just ignoring the fact that this idea has been tried, and now is dead for a good reason.

19. **Who**
July 4, 2006

z3,
could you provide a date for the article in the NYT?
I am not a regular Times reader. Any help with finding it? Author name?

NY Times, "Physics Awaits New Options as Standard Model Idles" describes the lack of progress in theoretical HEP in the past 30 years and portraits the sense of frustration in this field. String theory received a passing mention that's sort of dismissive.

20. **woit**
July 4, 2006

The NYT article is an essay by Dennis Overbye (who was in Beijing for Strings 2006), and it is at

http://www.nytimes.com/2006/07/04/science/04phys.html

Unlike many articles in the press full of hype about extra dimensions, etc., Overbye gets the mood in physics right, and emphasizes the lack of any experimental data that could help us figure out how to get beyond the standard model.

21. **Visitor**
July 4, 2006

Secretmilkshake, the answer to your question "Is ST getting like SA? Anybody shot in the back?" is "Motl will certainly be willing to act as an Obstacle Detachment."

22. **andy**
July 4, 2006

Dennis Overbye is an editor at the NYT. What other qualifications does he have to comment on the current state of particle theory?

23. **Not A Nobel Laureate**
July 4, 2006

"Dennis Overbye is an editor at the NYT. What other qualifications does he have to comment on the current state of particle theory?"

Given that he understands that physics is an experimental science driven by the discovery of new data, Overbye already more qualified to comment on the current state of particle theory than many string theorists.

24. **Tony Smith**
July 4, 2006

secret milkshake said:
"… Stalin: "In the Soviet Army it takes more courage to retreat than advance."
You tell me: Is ST getting like SA? Anybody shot in the back? …".

What about Glashow, who in a just world would (along with Coleman) be senior guru of Harvard Physics, but,
due to his views of superstring theory,
had to go to Boston U. to set up a physics program connected to reality ?

What about Smolin, who, as he said in his 2001 book "Three Roads to Quantum Gravity", "… was one of the first people to work on loop quantum gravity … before then I [Smolin] worked on string theory …", and
has recently, for advocating LQG, been a target of superstring community attack dogs ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

25. **BE PREPARED**
July 5, 2006

HEP-PH/0607028 ANNOUNCED THAT OUR IMPURE UNIVERSE, SUSONIA, WILL SOON MOVE INTO SUSYRIA, WITH PURE 10-DIMENSIONAL SUPERSYMMETRIES AND EXTENDED LIFE EXPECTANCY. That work was partially supported by the DOE under grant number DE-FG02-96ER-40967.

26. **Alejandro Rivero**
July 5, 2006

Well, Tom, the Harvard team kept Georgi, so it is not a completely lost department.

27. **woit**
July 5, 2006

BE PREPARED,

I think you mean hep-ph/0607029, which is an impressive piece of science fiction.

Andy,

Overbye's profession is to spend his life talking to the best scientists he can find, asking intelligent questions, and writing about what he has learned from them. The results are only going to be as good as the scientists he talks to, but they're as good a way of coming up with a summary of what scientists in a field think about the current state of their subject as any.

28. **D R Lunsford**
July 5, 2006

Peter,

That is so funny I spilled my coffee. It's like Star Trek plot.

-drl

29. **island**
July 5, 2006

Andy… long ago in a land far away from here it was understood that you should learn enough about physics that you can judge what you're looking at for yourself, so that you don't need to make lame appeals to higher authority.

30. **Fabien Besnard**
July 5, 2006

>It's like Star Trek plot.

It reminds me a lot of "Schild's ladder" by Greg Egan. Except that Egan's story is much more credible.

31. **MathPhys**
July 5, 2006

I wonder what Clavelli was thinking when he wrote these papers. But — it's a free country.

32. **Bee**
July 5, 2006

[From the present to the past]

Cambridge physicist Stephen Hawking and his CERN colleague Thomas Hertog have proposed a radical new approach to understanding the universe[…]

The new theory aims to get round a fundamental problem of string theory — the most popular candidate for a "theory of everything" — which is that it allows the existence of a multitude of different types of universes as well as our own. Each possible universe in this "landscape" has its own fundamental constants and even different numbers of space-time dimensions. Moreover, string theory does not favour any particular universe over another, which is not a good state of affairs as we clearly live in a universe with a particular set of physical properties.

33. **Benni**
July 16, 2006

Peter woit wrote:
People who talk about statistical calcuations of these vacua are just ignoring the fact that this idea has been tried, and now is dead.

What do you think about these papers from Luest:
I think this is pretty solid work!

The Statistics of Supersymmetric D-brane Models
http://arxiv.org/abs/hep-th/0411173

http://arxiv.org/abs/hep-th/0510170
One in a Billion: MSSM-like D-Brane Statistics

Abstract
Continuing our recent work hep-th/0411173, we study the statistics of four-dimensional, supersymmetric intersecting D-brane models in a toroidal orientifold background. We have performed a vast computer survey of solutions to the stringy consistency conditions and present their statistical implications with special emphasis on the frequency of Standard Model features. Among the topics we discuss are the implications of the K-theory constraints, statistical correlations among physical quantities and an investigation of the various statistical suppression factors arising once certain Standard Model features are required.

We estimate the frequency of an MSSM like gauge group with three generations to be one in a billion.

34. **Peter Woit**
July 16, 2006

Benni,
What is dead is the idea of extracting any physical predictions out of such calculations. There are no predictions in the papers you mention.

35. **Benni**
July 16, 2006

This papers are the reason why Douglas has given up counting vacua. He referes in his talk at Solvay directly to Luests work.
The Idea of counting vacuas is not death, it was solved.
However one could say the chance of 1 over a Billion to find the standard model is a prediction! At least the only prediction statistical arguments can make, since their only sense is to answer the question "How possible is it to find the standart model"

This is answeared by Luest.

But then we have the case that at the end, one has a certain possibility in string theory to describe our world. Since no one can find the solution explicitely (because of Douglas Complexity paper)
I think it is indeed fair to say the whole subject is at its end.

(as Luest has said to me in person: If the LhC doesn't find supersymmetry, then we need some good advise)

36. **Benni**
July 16, 2006

to be more concrete, you wrote Peter:
Once we have this model, the principle of mediocrity can be used to determine the probablility for us to live in one vacuum or other.

Unfortunately for this research program, it has yet to even begin to get off the ground, and there are very good arguments that it can never succeed.

The point why I send you these papers was, that I think this program has succseeded in the papers from Luest! The answer is one over a billion!

37. **JC**
July 16, 2006

The anthropic string folks surely have more than enough rope to hang themselves with. 😉

Various Weirdness

July 5, 2006
Categories: Not Even Wrong: The Book, Uncategorized

Must be something in the air, lots of weird things going on recently:

My book has been officially non-endorsed by the people at Axes & Alleys.

I finally realized why Lubos was carrying on about how if any errors in my book had been corrected it was because of him. Evidently one of the university presses I sent it to decided he was an appropriate reviewer for the book, and sent it to him to referee. I assume the review was as loony as the one he put on Amazon, but I never saw it, and have no idea whether it convinced the publisher to turn down my book. I do wonder which if any string theorist suggested Lubos to the publisher as an appropriate referee. The "free marketplace of ideas", indeed…

Lubos has put up a paranoid rant about how someone just told him that I "made amazon.co.uk erase all reviews" of the book except for the 5-star ones. This is complete nonsense. When his review appeared there and I first saw it over a month ago, I did hit the "report as inappropriate" link at the bottom of the review, but that's all I ever did about this, and I never saw any other negative reviews except his. Actually, I think his review was responsible for several people posting positive reviews in response to it (thanks folks!). I have no idea why Amazon UK recently deleted his review. Perhaps lots of people hit the "report as inappropriate" link, perhaps someone there just read it and recognized it for what it is. I wrote a comment on Lubos's blog explaining this, but it was immediately deleted (and has now been added to the Censored Comments From the Reference Frame section of this blog). It's pretty hilarious how exercised he is about censorship.

Over at the arXiv in hep-ph, a few days ago there was a paper from Tom Banks entitled Remodeling the Pentagon After the Events of 2/23/06. Somehow, Banks seems to be comparing the appearance of the paper of Intriligator, Seiberg and Shih about metastable SUSY breaking to the events of 9/11, and the following "neo-conservative revolution" in the US. Banks had a SUSY model containing a "Pentagon" (a "new strongly interacting SU(5) super-QCD with 5 flavors of pentaquark"), which he now enhances with metastable SUSY breaking to get what he describes as "a lean and mean, stripped down version of the Pentagon, suitable for rapid deployment to solve all of the problems of the supersymmetric standard model" (a footnote warns about the Pentagon's propensity for hyperbole).

A commenter here pointed to another paper on hep-ph, from last night, entitled Neighboring Valley in the String Landscape. Pretty much pure science fiction, although it did make me realize that just about any Landscape paper could be improved by doing what this author did, including an impressive color graphic of the earth de-materializing.

Over at hep-th, there's a new paper last night, entitled Generalized Flux Vacua. Using a new construction, the authors find an infinite number of solutions that are supposed to be consistent backgrounds for string theory. This pulls the plug on the arguments from a couple weeks ago by Acharya and Douglas that the Landscape should be finite (after imposing varous cutoffs). The authors also claim that this drains some of the swampland promoted recently by Ooguri and Vafa. They do note that, although string theory is a completely precise and rigorous framework, it's impossible to tell whether the backgrounds they describe really are consistent vacua for string theory (because of, among other things, possible non-perturbative effects):

This somewhat surprising result seems to contradict recent predictions regarding properties of the string landscape, though as we will discuss there are some reasons why the solutions we find may not correspond to stable nonperturbative vacua in a complete string theory framework.

Comments

1. **John A**
   July 5, 2006

   For Lubos to keep referring to you as "the lecturer in Discipline" rather than by your name, is just desperately sad.

   I cannot judge the finer points of your argument about string theory, but judging by the extraordinary behavior of Lubos, I'd have to say he's completely lost it.

   String theory is a metaphysical theory. It has no laboratory.

2. **Peter Woit**
   July 5, 2006

   John,

   I don't know, at least he's getting my official title right. Seems like an improvement over the usual "crackpot", "scientific microbe", "one gigantic KGB", "Goebbels", "inquisition", etc…

3. **Kea**
   July 5, 2006

   "Pentagon" (a "new strongly interacting SU(5) super-QCD with 5 flavors of pentaquark")

   OK, I won't say it…..

4. **Who**
   July 5, 2006

   Freidel's paper is out
   there has been some discussion of the anticipated results already by Urs and by fh, at the Distler blog.

   http://arxiv.org/abs/gr-qc/0607014
   Particles as Wilson lines of gravitational field
   L. Freidel, J. Kowalski–Glikman, A. Starodubtsev
   19 pages

   "Since the work of Mac-Dowell-Mansouri it is well known that gravity can be written as a gauge theory for the de Sitter group. In this paper we consider the coupling of this theory to the simplest gauge invariant observables that is, Wilson lines. The dynamics of these Wilson lines is shown to reproduce exactly the dynamics of relativistic particles coupled to gravity, the gauge charges carried by Wilson lines being the mass and spin of the particles. Insertion of Wilson lines breaks in a controlled manner the diffeomorphism symmetry of the theory and the gauge degree of freedom are transmuted to particles degree of freedom."

5. **woit**
July 5, 2006

Who,

Somehow I don't think you believe the Freidel et. al. paper fits under the current topic of "Various Weirdness"… You really need a blog devoted to LQG, maybe you can get Distler to host an "LQG Coffee Table". Now, that would be weird…

6. **Who**
July 5, 2006

you are being conversational about my sin of offtopicness, instead of delivering cuffs of disapproval. you must be in an exceptionally good humor. I am glad

7. **Shantanu**
July 5, 2006

Peter , what exactly does "officially being non-endorsed mean"? also who or
what is the "Axes & Alleys" ?

8. **woit**
July 5, 2006

Shantanu,

Can't say that I know the answers to your questions, your guess is a good as mine. As near as I can tell, the mysterious forces behind this publication and blog are string theory skeptics, and thus fans of Not Even Wrong. The official non-endorsement has something to do with my refusal to admit that I found their string-related graphic funny.

Like string theorists, they seem to have a very original relation to the question of "reality". Unlike string theorists, they seem to be aware of this…

9. **Chris W.**
July 5, 2006

Who,

Compare gr-qc/0607014 with [hep-th/0403137](hep-th/0403137). There seems to be a conceptual connection here.

(Peter: I beg your indulgence; could you leave this comment in place for a day or two?)

10. **Mike**
July 5, 2006

Is the business in that paper about remodeling the Pentagon after the "events" of a certain date — admittedly not 9/11 — perhaps in bad taste, rather than a sign of cleverness? If I knew someone who had died there, I might find it so.

11. **woit**
July 5, 2006

OK, that will teach me to be a nice guy. Next off-topic LQG commenter will be shot.

12. **woit**
July 5, 2006

Mike,

I'm not sure about poor taste, but it's definitely weird. Promoting the latest advance in SUSY breaking as being like 9/11? A very, very, weird field, this one, if you ask me.

13. **D R Lunsford**
July 5, 2006

I don't want to be shot, so I won't explain my plan for rebuilding the World Trade Center as a hyperdiamond Feynman lattice. Wait, that's on topic! Go ahead! Make my day!

-drl

14. **Jeremy**
July 5, 2006

D R:

I fully-endorse your plan! Of coursed, at this point in time I would endorse I giant rubber duck there.

While I understand the fully-human reaction Lubos shows to a complex questioning of an idea so closely identified with the core of his life, I still think the denigration involved is out of place. That aside, I have enjoyed reading him when he's not cranky (so every four days or so).

Dr. Woit, have you read or seen the new publication on the 10th Dimension? I find its explanation of the 5th, which is one essentially of choice, to be quite amusing.

15. **andy**
July 6, 2006

Peter: Get over Lubos. Ignore him. He craves recognition and notoriety. Sure, he probably legally libelled you in his post, but "whaddaya gonna do aboudid?"

Lubos is a nutbar, and he's the "best" proof that string theory is dead. Celebrate it!

16. **andy**

July 6, 2006

ps: and how many papers has LM written recently?

17. **Aaron Bergman**
July 6, 2006

    Re: Banks

    Lighten up, dude. You could argue that the joke is in poor taste, but it's certainly not "promoting the latest advance in SUSY breaking as being like 9/11".

18. **woit**
July 6, 2006

    Aaron,

    Re: comparing 9/11 to 3/23

    Lighten up, I don't especially think it's in poor taste. I think it's nutty. Watching gonzo surrealism take over the subject of particle theory is kind of fascinating to watch.

19. **John Gonsowski**
July 6, 2006

    Well being a surreal Gonso all my life, I might as well add something… rebuilding the World Trade Towers as a hyperdiamond Feynman lattice could allow it to be protected by the variable physicality of the conformal degrees of freedom but you would have to make sure it doesn't drift off into a pure 8 dimensional non-supersymmetric universe. Woit, that's on topic! Go ahead! Make my day!

20. **John A**
July 6, 2006

    Security backup of comment:

    > I'd like to know why the name of this blog morphed from "Lubos Motl's Reference Frame" to "The Reference Frame".
    >
    > Surely the first implied that this was your point of view on things, while the latter seems to imply that everyone should see view the world in exactly the same way as you do.

21. **Carl**
July 6, 2006

    Now that you mention it, "The Reference Frame" seems to suggest an aether.

    Carl

22. **Mike**
July 6, 2006

    Let me make it clear: I think the 9/11 – 3/23 pairing is weird. That doesn't mean it isn't in poor taste as well. Especially to people — aka citizens and taxpayers — who know nothing about physics, but understand 9/11 very well.

    I remember the colleague whose reaction to 9/11 was that it was horrible, but it sure was great theater. That has always struck me as weird, and as in very poor taste, especially given the fact that he was a citizen of another country (albeit one which lost many in the WTC).

    It's all probably a minor point. Maybe not even wrong, but I doubt it. On with physics!

23. **Aaron Bergman**
July 6, 2006

    Banks has done this sort of thing for yaers.

24. **anonymous**
July 6, 2006

    astro-ph/0412647 of 29/12 was inspired by the tsunami of 26/12, an event worse than 9/11.

25. **John Gonsowski**
July 6, 2006

    Personally I am too into 9-11 to stay depressingly serious about it 100% of the time but it is important for sensitivity and seriousness of issue reasons not to go overboard. One day and one day only during my four years at Carnegie Mellon we trashed our floor of the dorm (nothing permanent), the clean-up crew just seemed to smile knowingly.

26. **Peter Woit**
July 6, 2006

    Aaron,

    At least in the case you mention Banks's analogy of the state of string theory with Dante's Inferno makes good sense…

27. **Aaron Bergman**
July 6, 2006

    I'm sure you don't want the unsolicited advice, but you really need to look at things without the lens of your crusade against string theory occasionally and not take everything quite so seriously, Peter.

Or, put another way, not everything needs to be interpreted as part of the growing malaise that will soon overwhelm string theory and exile us all to the the world of financial derivatives.

28. **Peter Woit**
July 6, 2006

Aaron,

Good advice, but I can assure you that at least in the case of this posting, this is all about not taking things seriously. There's an extremely entertaining surrealism to so much going on in this business these days.

29. **Mike**
July 8, 2006

Not a string theorist myself, so I have no personal stake, but if string theory crashes, there are other possibilities besides financial derivatives (and anyway, that too will soon be saturated anyway). There are many interesting things going on in science, more likely to lead to something worthwhile than the (N + 1)th string theorist, to which physicists, theoretical physicists, can contribute. Things involving cooperative phenomena in a whole bunch of fields, inside and outside of physical science. I say at least have a look at other "options" if you are a young person thinking of bailing from the string world!

String Theory for Undergraduates

July 5, 2006
Categories: Uncategorized

I hadn't realized how many of the physics departments at the top universities in the US have instituted undergraduate string theory courses. The only one I was aware of was MIT's 8.251, [String Theory for Undergraduates], taught by Barton Zwiebach, who developed a textbook for the course, A First Course in String Theory.

Maybe now that there's a textbook, that is what has caused other institutions to follow suit. Caltech has Physics 134, String Theory, and Carnegie-Mellon has Physics 33-652, An Introduction to String Theory. Stanford goes its competitors one better by having two undergraduate courses in string theory: Physics 153A, Introduction to String Theory I, and Physics 153B, Introduction to String Theory II. This last course even promises to explain to students how string theory is connected to particle physics.

Comments

1. **D R Lunsford**
   July 5, 2006

   Needless to say, this is absurd. Is physics trying to commit suicide? In fact there is no longer any need or use for American scientists and engineers, only Home Depot clerks and the matching consumers, so I guess they figure – why not?

   It's as if law schools were to begin teaching from Deuteronomy. Let's bring back Aristotle and Pliny!

   -drl

2. **Kea**
   July 5, 2006

   Gulp! In my country we don't even have any String theorists.

3. **LDM**
   July 5, 2006

   It is probably not too unreasonable to state that many critics of the theory have not studied string theory even at the level of Zwiebach — and even Feynman said (read the letters book for the exact quote) he did not believe in it, but he admitted he did not know enough about it to say why…

   As long as the undergrads have the necessary background, I think this is an EXCELLENT decision — to offer undergrads courses, so they can judge the merits (or lack thereof) of string theory themselves.

   They will also then be in a position to judge when its supporters have let their enthusiasm overpower their scientific better judgment (As an example, Zwiebach's recent letter comes to mind — it was, unfortunately, egregiously biased and overstated — however, one can only really make these judgments if one has studied string theory sufficiently to realize Zwiebach was exaggerating its results) — OR when the beauty of its ideas and its partial results, despite the current lack of experiments, justify a continued optimism that that the theory has substance and is worth continued investigation.

4. **z**
   July 6, 2006

   After a cursory look at Zwiebach's course page, it seems to me some mathematics only at the sophomore or junior level is required – PDEs, linearity, multivariate calculus. What exactly do they hope to accomplish with such courses other than to inculcate a new generation of graudate students and benefactors? Connections explored in these courses to experimental tests is superficial at best, in my opinion.

   I'm not saying these courses are worthless — there's a lot of neat mathematical tools an undergraduate could learn to apply in them. However, an undergraduate education in physics should be firmly based in reality. Speculative theories such String Theory and even LQG should be in the domain of graduate school or applied mathematics, not undergraduate physics.

5. **biophysicist**
   July 6, 2006

   I took the class myself a couple years ago back when we were still given the typed manuscript for free (there now I've definitely given away my identity for a couple readers of the weblog for sure) and even though obviously I chose a different branch of physics for my graduate work I have to say that the course was simply outstanding and Barton Zwiebach is one of the finest teachers I've ever had. Yeah string theory as science has definitely seen better days, but the class itself is a great way to learn a lot of the tools used in modern physics. The way Barton teaches it also makes it a neat way to see how different theoretical underpinnings can come together – the calculation of the Beckenstein-Hawking entropy result for example played a big role in my personal decision to pursue statistical mechanics as a graduate student (now of course, as a statistical mechanician, I can laugh at all you particle guys whose work is simply an input into our theories 😉).

   Also I'd like to point out that while the class started with probably 60 people (including 30 or so undergrads) the year I took it, we only ended with 6 undergrads by the end I think and about 20 graduate students. So before anyone gets too worried about Barton's corruption of us youth I think it's worth thinking about the positive aspects of the class – I of course cannot speak to the other institutions' courses, but I learned a lot of useful tricks in 8.251 that I use to this day.

6. **Richard**
   July 6, 2006

   String theory for undergraduates is excellent for students who want to think about classical mechanics and E&M in a more abstract way in preparation for graduate school. Additionally, these courses can serve as "capstone" courses for physics majors who aren't planning on going to graduate school. Most students who are interested in theory will take either GR or QFT instead. Perhaps a course that would serve both groups of students better would be an introduction to conformal field theory with applications to condensed matter for undergraduates, but no one has written a book for that subject comparable to Zwiebach's.

7. **Doug**
   July 6, 2006

   I would like to take the course, if nothing else just so I can get the answer to one question:

Why does David Gross and others say that they don't know what string theory is? What does this statement actually mean?

8. **Jimbo**
July 6, 2006

After 10 yrs of college physics teaching in Calif., Colo., Florida and Oregon, I can guarrantee that 99.9% of the undergrads, at 95% of US universities, contemplating signing up for an intro string course out of Zwiebach's book, will drop the course in less than 2 weeks !
In the 5% I am excluding, are of course Caltech, Harvard, Stanford, Princeton, Cornell, UC, CU, UWash, etc.
However, even at prestigous schools, where undergrads have excellent preparation, faculty are overcommitted to teaching std. req'd courses, and will probably be reluctant anyway, in the dubious light of anything stringy these days.
Mike Duff was very harsh on the mathematical unfitness of physics American students, and I can personally vouch for the veracity of his comments on this sad state of affairs.

9. **misslemon**
July 6, 2006

Re: LDM's arguement:
"I think this is an EXCELLENT decision — to offer undergrads courses, so they can judge the merits (or lack thereof) of string theory themselves.
They will also then be in a position to judge when its supporters have let their enthusiasm overpower their scientific better judgment "

Would you say, then, that it's also an excellent idea to teach "creation science" to biology students? Not to say that string theory is in the same class, but this IS the arguement often used for allowing creationism into the biology curriculum.

10. **MathPhys**
July 6, 2006

The first half (or so) of Zwiebach's book is good because he assumes so little, so he has to explain lots of basic physics before hand, so you can think of this, indeed, as a motivated introduction to general physics.

I'm not so sure I like the latter parts of the book, where he introduces those aspects of string theory that he's interested in. That makes the book less than suitable as a standard textbook.

11. **D R Lunsford**
July 6, 2006

Richard said

String theory for undergraduates is excellent for students who want to think about classical mechanics and E&M in a more abstract way in preparation for graduate school.

I can't see this as being in any sense true. In fact, having wrestled with both things at length, I declare it categorically false.

All the hotshots I ever knew had glaring holes in their understanding. I assume hotshots everywhere are similar. One has to work at rounding out one's understanding, and it ain't comin' from string theory. There is a magisterial tone in your misunderstanding that illustrates the actual source of the current unpleasantness.

-drl

12. **Alejandro Rivero**
July 6, 2006

A problem with Zwiebach's book is that it is very easy to follow in the first part but it is at the end expected to be taught by an expert of the field, because when it goes into complications it just promises that in the real research papers all the questions are rightly answered. SO you are forced into Ponchiski's are GSW and it is not undergraduate anymore.

Still, it is interesting for an undergraduate to think about the quantisation of extended objects.

13. **X**
July 6, 2006

maybe it is interesting for undergraduates that already understood quantization of point particles?

I think that an undergraduate needs to get some idea about what string theory did/will accomplish, to decide if it is worth to study it. While learning some details of string quantization (at the expenses of not learning other physics) is not a good idea.

14. **Warren**
July 6, 2006

Stanford is on the quarter system, so it's really more like 1 1/3 courses.

15. **LEJ Brouwer**
July 6, 2006

I would love to see a textbook on modern string field theory in the Batalin-Vilkovisky formalism of which Zwiebach was a pioneer. The beautiful thing about Zwiebach's work, and I think this is shows through in all his research papers, is his emphasis on clarity and rigour, despite the inherent complexity of the subject matter.

16. **Doug**
July 6, 2006

But, again, how can a university course, based on a textbook, teach something that nobody can identify? What is it that I'm missing here? What do string theorists, like David Gross, mean, when they say that they don't know what string theory is?

Please, can someone take a moment and explain this to me? Books, pro and con, are flying off the presses about something no one understands, but everyone feels so strongly about.

I can understand the idea of strings vs. point particles. I can understand the difference between the 26D bosonic string theory and the 10D superstring theory with its five dualities and how the 11D M-theory unifies these. I can even understand how compactified dimensions make it possible to have n-dimensional equations of motion in a 3+1 manifold, but what do they mean when they say they don't know what string theory is? This I don't understand.

17. **Kasper Olsen**
July 6, 2006

Dear Peter,
did you read the book? It is – in my opinion – actually quite good; all of it might not be important for eventually going beyond the standard model, but don't you think it is important to learn about the most promising ways of going beyond the SM? If somebody wanted to give a course on LQG or twistor theory, don't you think it would be OK?

– regards, Kasper

18. **Jason**
July 6, 2006

Relax, people.

19. **Peter Woit**
July 6, 2006

Doug,
It's very simple. What is known about string theory is how to construct a (divergent) series, one that is supposed to be a perturbation expansion of some still unknown exact theory, and thus an asymptotic series in the string coupling for the true result. There are lots of partial, conjectural ideas about what properties this non-perturbative true theory will have, and some conjectured constructions in special backgrounds that don't look like physics. But no one has a good idea about what this exact, underlying theory is. There has always been a lot of speculation about how it will be something completely new and different, involving new ideas about space and time.

Kasper,

No, I don't think it would be OK for a physics department to set up an undergraduate course in LQG or twistor theory. This kind of highly speculative stuff involving advanced mathematics belongs in upper-level graduate courses, not undergraduate courses where the students still have yet to learn many of the most basic things about physics and the most basic mathematical techniques needed. I know of no other example ever where multiple US physics departments have instituted undergraduate courses in a subject for which there is no experimental evidence.

Yes, I own a copy of the book and have read some of it, although it covers material I've read about many times in other string theory books and expository papers over the last 20 years. It's a good exposition of the subject from a true believer, but it's completely inappropriate for undergraduates. There are many, many more useful things for them to study than the details of how to quantize the string in light-cone gauge.

I completely disagree with you that string theory is a promising way of going beyond the SM. It has completely failed at this, something you won't learn by reading all the hype in Zwiebach's book. Even if it were a promising way of going beyond the SM, undergraduate students don't even yet have the background to understand the SM, and they need to do that first. It is becoming a huge problem in particle theory that students are being trained in the details of string theory before they have really mastered the details of the SM.

20. **D R Lunsford**
July 6, 2006

Peter,

The time would be better spent on, among other things, an intensive course in "Intuitive Relativity". I found that very few undergrads understood relativity at all well. I think a history course with readings of original papers would be good (e.g. the "Dreimaennerarbeit", Pauli's review articles, Einstein's big papers etc.)

-drl

21. **LEJ Brouwer**
July 6, 2006

I agree that string theory is not a useful topic to study at the undergraduate level. However I think the study of Clifford algebras and their applications to physics (see e.g. the book "Geometric Algebra for Physicists") could prove to be a useful broad framework for those intending to carry out future research in theoretical physics.

22. **Doug**
July 6, 2006

Thanks Peter. Does the book explain that, or is it assumed that an undergrad physics student would ordinarily realize it?

23. **James**
July 6, 2006

Peter,

It might be a shocker for you but several European universities offer undergraduate string theory courses and have been doing so for at least 15 years (that is what I am aware of). Examples: Amsterdam, Groningen, Utrecht, Brussels, Budapest, Prague, Moscow and I'm sure there are plenty more.

Mind the fact that there are loads of old professors for whom the phrase "string theory" means something from the 70's and that is what they teach. It would be very difficult for you to convince them that this material is some new age hype when they were studying it 30 years ago.

Of course there are European universities where the more modern approaches are taught to undergraduates under the banner "string theory", e.g. Amsterdam and I suspect this area is where you actually target your criticism.

My observation is only to enlighten anyone who thinks that "string theory" is a large monolithic blob which can be commented on and criticised in one go.

Hope this helps,
James

24. **James**
July 6, 2006

Peter,

You write "What is known about string theory is how to construct a (divergent) series, one that is supposed to be a perturbation expansion of some still unknown exact theory, and thus an asymptotic series in the string coupling for the true result." and perhaps you allow me to notice a certain negative tone in this sentence.

However, I guess you agree that nothing is known about the Standard Model non-perturbatively (except for QCD but that is also not rigorous) and all what is known is a perturbative expansion but that doesn't cause us any headache. So this particular criticism of string theory is not really honest as you could say the same for the Standard Model as well. Put it this way: a rigorous non-perturbative definition is *not* a requirement for a good physics theory.

I can already hear you saying that the Standard Model has been confirmed experimentally and string theory has not, which is completely true but in your above mentioned criticism you brought up the lack of a non-perturbative definition and haven't talked about experiments. In other words just because one valid criticism exists does not mean that *any* criticism is valid.

Hope this helps you understand why string theorists and other high energy physicists do not respond to your book in a serious way and why string theorists and other high energy physicists do not take you seriously over all. If they are looking for criticism of string theory (plenty exists surely, lack of experimental evidence being one) they go to the experts who are also plenty. Even plenty of string theorists exist who are critical in a reasonable way, unlike you I am sorry to say.

Again, it is my intention to help you understand the reason for your considerable isolation in high energy circles especially since you yourself asked the question in one of your postings why there is no serious string theory reaction to your book.

All the best,
James

25. **Chris Oakley**
July 6, 2006

Hi Anonymous James,

I don't think that your observations are very relevant. Many of us have come to the conclusion that the String community is not capable of reforming itself and therefore the change has to come from outside. This is where writing books for the general scientific audience comes in. Peter is surely in the vanguard here, but if he had not taken it upon himself to draw the world's attention to the extent to which fundamental physics research has poisoned itself, someone else would.

26. **anonymous**
July 6, 2006

Dear James, the main problem is not the lack of experimental evidence, but the lack of any experimental signal. I hope you agree that, if strings have nothing to tell to experimentalists, the lack of a rigorous non perturbative definition is solved as: who cares?

27. **Peter Woit**
July 6, 2006

James,

The problem with string theory is not that it doesn't have a rigorous non-peturbative formulation, the problem is that it has no non-perturbative formulation at all (other than on unphysical backgrounds). This is especially problematic because the vacuum state people want to use in string theory can't be the perturbative one. This is why you can't predict anything in string theory. You don't have a non-perturbative theory that can tell you what the possible vacuum states are and allow reliable calculations in these states.

This is completely different than the standard model, which has a well-defined non-perturbative lattice formulation. This definition is completely rigorous, the only thing non-rigorous is that you can't prove that it actually has all the properties you expect. You can do approximate calculations, either perturbative or semi-classical ones where appropriate, or fully non-perturbative ones using Monte-Carlo methods. All the results of these approximate calculations are consistent with the non-perturbative theory having the properties one expects.

For the electroweak part of the SM, the vacuum state is the perturbative one, you can do perturbative calculations, and these have been confirmed experimentally to very high accuracy. For the strong interactions, the low energy behavior is non-perturbative, you can calculate it using Monte-Carlos methods, and, within the accuracy of the calculations you get agreement with experiment. At high energies because of asymptotic freedom, you can use perturbative methods also in QCD, and get good agreement with experiment there too.

The situation of the SM is just completely different than that of string theory, and the fact that you don't know this seems to indicate that you don't know much about this subject at all. I don't know who you are, since you choose to hide behind anonymity, but you give every appearance of being an undergraduate who has no idea what is going on here.

As for my "isolation in high energy circles", I can assure you that that is not the case. I regularly talk to high energy physicists of many different stripes, and find that the only ones who significantly disagree with me are the more fanatical among the string theorists. From my conversations with many physicists in recent years, my impression is that the great majority of the physics community now has a low opinion of string theory (often lower than mine). Even the majority of string theorists that I talk to think the state of the field is highly problematic. Several of the people who regularly comment here, but choose to remain behind pseudonyms because of fear of retribution from fanatics, are known to me and are theorists who work on string theory.

I wasn't asking about why no one agreed with me, most theorists do. What I'm curious about is why those string theory true believers who disagree with me refuse to publicly answer any of the criticisms I make of the theory. So far, the only one willing to do so is Lubos, and he is crazy. The only others heard from are people like you, who don't seem to know what they are talking about and can't answer any of my criticisms, but yet insist on attacking my competence to make them in a cowardly fashion using pseudonyms. This kind of defense of string theory is not going to work. It is becoming widely realized in the physics community that string theorists are not able to answer the scientific criticisms being made, and the way they are choosing to respond to their critics is rapidly stripping them of any credibility.

28. **Peter Woit**
July 6, 2006

Doug,

I haven't looked at everything in the book carefully, but it certainly was my impression that the book did not explain exactly what the problem is with having no non-pertubative formulation. I have no idea whether Zwiebach explains this to his students, but his recent letter to the WSJ not only didn't mention this problem, but tried to imply that it doesn't exist or is not serious.

29. **Clifford V Johnson**
July 6, 2006

Hi Peter,

I hope you're well.

A few things:

(1) I can think of numerous times when well-known non-crazy string theorists have answered your criticisms, on this blog and on others. The problem is that you choose to forget the answers and keep repeating yourself. So people have gotten terribly bored with the whole thing I suspect. It's nothing personal, at least not on my part, just really really boring, I'm afraid. How about searching your own blog's archives for some of the answers? And those of CV? There's really been no significantly new discussion points since last August or so, so all this stuff is hard to distinguish from a PR exercise to sell a book. Not that there's anything wrong with PR exercises per se, and I am of course not accusing you of engaging in one, just making an observation.

(2) The only time you seem to come over and join in a discussion (or whatever) over on CV about strings is when you've something negative to say. Why not come and join in the other discussions (there and elsewhere) where we are all publicly visibly learning new things about ongoing research in the field as we go along? Not doing so while continue to make the same old answered points again and again -your current practice- might lead the uninformed observer that maybe you have a fixed idea about what you want to believe and will only discuss results consistent with that. See my last two sentences of (1).

(3) You speak with certainty that string theory has failed as a theory of Nature. Actually, you don't know if that is true, and nobody knows that. It is your hunch that it is so, and it is my hunch that it is not so, and we are entitled to our hunches. It is a subject of ongoing research. That is as much as anyone can say. To say more is hype. I thought you did not like hype. See my last two sentences of (1).

(4) You say that the majority of string theorist that you talk to think that the state of the field is highly problematic. I cannot dispute that you got that impression, but I do question the validity of the sample that you have ahda access to. I and several of the people I work with or meet regularly in the field (from many of the most active groups around the world) don't seem to have that strong an opinion. But that's consistent with the fact that none (or few) of these people have actually met you at any conferences or meetings. (Unless they are meeting with you in secret?) So there seems to be little or no intersection between the set you are meeting with and the set that I and my colleagues and collaborators are meeting with. Interesting, don't you think? You're a smart guy and ordinarily would not make such an empty point unless….. oh…see the last two sentences of (1).

(5) The real reason I came on to comment (since I've said much of the above before) is to ask whether you really think that (as a faculty member of a respectable university, but we'll let that pass) it is consistent to publicly beat up on an undergraduate (if that is what James actually is), and call him or her ignorant about string theory and other issues *in the very comment thread of the post you wrote saying that undergraduates should not be taught string theory*? Also, he/she made some interesting historical and factual points that you chose to ignore. How come? This all seems a little odd to me.

Summary: With all due respect, please change or update the refrain of the song. It's got rather boring. Stop being so transparently selective about what you choose to mention about the string theory program. Try harder to apply the same standards of rules of engagement in discussion that you apply to others. (I know this latter is hard…. I fail sometimes too).

Anyway, the repetitive refrain and the selective memory about what has been addressed…. these the main problems. It is just boring. Nobody wants to play anymore, and standing on the mound beating your chest yelling "why will nobody fight me?" is looking a bit silly. I'd stop that if I were you. (Although it is good for book sales and press attention. The press and the public love the old tired "underdog vs the establishment" story.)

Ok, I'll go now. I hope to have a pleasant, reasonable, and *new* discussion wth you sometime soon.

Cheers,

-cvj

30. **Eugene Stefanovich**
July 6, 2006

cvj:

I don't see anything wrong in what Peter is doing. We had enough "PR exercises" of different kind – "The elegant universe", and Kaku, and all that. We all heard how M-arvelous and M-ysterious the string theory is. Now it is time to hear a second opinion. I find it refreshing and healthy that a person like Peter spends time and effort to debunk stringy hype attempts.

If Peter is right, then the question of scientific validity of the string theory may never be resolved by scientific methods. It looks like it is impossible to prove this theory wrong, and there are no visible signs of experimental evidence in favor of strings. Nobody wants to forbid stringy research. The question is how much of taxpayers' money should go there? String theorists enjoyed enormous amount of public trust for a couple of decades. The question is whether we should continue to trust them, or better start investing in something else?

Peter, I also don't like when you attack your commenters (even anonymous ones) on a personal level. This doesn't make your position stronger.

31. **woit**
July 6, 2006

Hi Clifford,

Thanks for stopping by. Sorry I don't have much time time to respond. Actually I'm leaving early tomorrow morning for a very short vacation in LA. From CV, I gather you're not there, but enjoying the south of France.

I do pay close attention to the responses I have gotten here and elsewhere from string theorists to my criticisms of the theory. They're a mixed lot, ranging from offensive personal attacks by Lubos and followers at one extreme, to much more sensible comments from you and others. I'd characterize your response to my criticisms of string theory as roughly "it's still too early to evaluate, we're working on it, look at the progress we've made in AdS/CFT for instance". This is fair enough, it's a point of view I can understand and respect, although I don't fully agree with it (22 years is a very long time, the amount of progress is exaggerated, etc….).

The main problem with your point of view seems to me to be that you're pursuing one set of goals (e.g. for instance better understanding AdS/CFT, topological strings, 2d strings), which are reasonable, but selling the subject to the outside world as a much more ambitious program, without acknowledging that it has failed. Your work on non-critical string theories may or may not someday lead to something interesting, but what is being promoted to the public is the idea of unification via a critical 10 d string (or 11d M-theory). This has created a whole industry of people investigating an infinitely complicated variety of "backgrounds" for such a string, and ultimately led to the landscape fiasco. This has been a disaster, and it needs to be acknowledged before it leads to more damage to the field than it has already caused.

When I say that the majority of string theorists I talk to find the state of the field problematic, the main thing they often have in mind is the landscape, which often completely appalls them. Besides this, most also feel that there has been a depressing lack of exciting new ideas in the subject, across the board. I can't believe you don't also find many if not most of your colleagues disturbed by the landscape. As for the number of exciting new ideas question, this can be quantified by looking at what papers are being cited in new ones coming out, and I can back up what I say with some very solid data.

In terms of people I talk to or exchange e-mail with, my sample size is small, but it's non-trivial. When I go to conferences, it is often (but not always) to mathematics or mathematical physics ones, since that's where my research interests lie, but I have been to and even given talks at conferences or workshops where there were quite a few string theorists. On the whole I found when talking to them that they shared many of my concerns about the subject. Unfortunately, some string theorists have very much isolated themselves from the rest of the physics community. Sorry to have to tell you this, but many of your physics colleagues tell me they think the field of string theory has become a bit of a closed religious cult. Skeptics aren't welcome there and have little motivation to try and participate. The last physics conference I was at was last week, here in New York, the one about group theoretical methods in physics. I talked to more than a few physicists then, and I can assure you that the level of hostility to string theory and how it is currently being pursued surprised even me.

About "James". I have no idea who he is or whether he's an undergraduate, quite possibly he isn't. If he is one, he has no business writing ignorantly in here to attack someone who knows far more than he does. My conduct towards him was significantly better mannered than his towards me.

As for CV, I do post comments there when I think I have something worthwhile to say. Often they're not about string theory at all. When they are associated with one of the postings about the string theory controversy, they're often respectfully disagreeing with a much more positive take on string theory due to one CVer or another. Sorry if you find my objections to string theory "boring". I find the endless continuing overhyping of the subject equally "boring", but feel that someone should do something about it. I'd be very happy if it wasn't me.

As for your endlessly repeated attempts to pin this all on my trying to sell books, just come off it. This has nothing to do with that. I've been making these criticisms privately for 20 years, publicly for more than five years, long before there was any book. This is a complicated business and the book is my best attempt to lay out what I've learned in a long career, and explain a point of view about where the subject of particle theory is now that I strongly believe in and hope people will pay attention to. Sure, I definitely think people should read the book, I'm arrogant enough to think that I have something important and valuable to say. But if it's a complete failure as far as the publisher is concerned and doesn't make a dime, but a sizable fraction of the particle physics community reads it and thinks about what I have to say and whether my point of view makes any sense, I'll consider the project to have been a huge success.

Hope you're enjoying Marseilles and meeting some mathematicians, they do some very worthwhile things…

Peter

32. **Jimbo**
July 6, 2006

I spoke recently with an Indian string theorist (Tata Instit.), and he basically wrote off Peter & Bert Schroer as "disgruntled whiners, without any significant research in years". My retort was, might we add Veltman, Glashow, Richter, & Gates to that list as well, and who knows how many other field theorists that might emerge from the closet to signal their opposition to the dominant paradigm in HEP ?
Pro or Con, we are all college educated people, and how do they settle arguments: Forensic Debate. Lets gather together 5 prominent stringers, and 5 prominent anti-stringers, and have a live debate, with theorists from neutral camps for judges. The media would devour it !
In addition, why not CLOSE the N.E.W. blog Peter, now that your book is out, the Discover article is months old, and get on with your life & research ? It must be very taxing doing this 24/7, and lets face it, the word IS out, and I don't think the stringers will get carte blanche much longer, when in just a few years, the LHC tallies up evidence.
Who knows if you close, Lubos might just as well too.

33. **Peter Woit**
July 6, 2006

Jimbo,

The one criticism I get for how I've been spending my life the last couple years that I do agree with is that I'd be better off spending more time on my own research ideas. I'm hoping to spend less time on the blog and more on research, and perhaps changing the emphasis of the blog so that it's more about research level topics I'm thinking about. Urs Schreiber's blog is an interesting example of this kind of thing. Unfortunately this may not be so easy the next few months. Clifford thinks I'm spending my time trying to get people interested in my book, but it's not quite like that. There's a lot of interest in it already, and it is taking a signficant amount of time now to respond to people who contact me about it for one reason or another. The response has been very gratifyingly positive, but dealing with it does take time away from other things. Current plan: try and find as much time this summer as I can to work on research, write off September as hopeless since the book is coming out and a new semester will be starting, then hope by later in the fall things quiet down, my job is done, everyone agrees with the NEW critique of string theory, so that part of the blog can fade into irrelevance.

34. **Anonymous**
July 6, 2006

Pro or Con, we are all college educated people, and how do they settle arguments: Forensic Debate.

Really? To me that phrase conjures images of unreasonable people talking as fast as possible, emphasizing the same points repeatedly, and not listening to the people they're talking to. Oh, wait, that sounds familiar….

35. **Jonathan Vos Post**
July 6, 2006

Why not compile the course descriptions and see what is being taught by whom, perhaps with what textbooks?

From Caltech's catalog:

Ph 134. String Theory. 9 units (3-0-6); third term. Prerequisites: Ph 125 ab, Ph 106 ab. A basic course in string theory designed to be accessible to a broad audience. The main topics include the motion of relativistic point particles and strings, actions, world-sheet symmetries and currents, light-cone quantization, and the spectra of relativistic open and closed strings. The course will conclude with an exploration of D-branes, T-duality, or string thermodynamics, depending on student interest. Instructor: Schulz.

36. **JC**

July 6, 2006

I wonder if Lubos will write his own string theory book for freshman undergraduate students. 😉

37. **Michael**
July 6, 2006

"changing the emphasis of the blog so that it's more about research level topics I'm thinking about"

YES!! Tell me all about your interesting new ideas, especially the off-diagonal ones. 😆

38. **Peter Woit**
July 6, 2006

Always charming to hear your witty remarks Michael/Dan/whoever you are. Now go back and play with your big brother Lubos…

39. **Hmm**
July 7, 2006

Perhaps, before making grandiose statements about the past and future directions of theoretical physics, and especially before "correcting" people with authority, it would be useful for you to understand, say, some elementary quantum field theory?

Lets take your response to James about the difference between non-perturbative effects in the Standard Model vs. string theory. You contradicted James and said that the Standard Model has a perfectly rigorously defined lattice formulation. Even ignoring gravity, this is nonsense. Hypercharge is not asymptotically free and hits a Landau pole at a scale \Lambda–so there is no "perfectly well-defined" lattice formulation. The Standard Model by itself simply isn't "well-defined"–there are effects of order \sim E^2/\Lambda^2 \sim e^{-1/g^2} that *can't even in principle* be calculated, as they depend on the details of what would UV complete the theory above \Lambda. This is trivial field theory (pardon the pun) which you don't seem to understand. Of course string theory doesn't have this sort of problem, but there isn't any point discussing this till you understand the simpler field theory case first.

So James was right on the money, both in his physics point and more broadly. Much as you like to think that experts aren't paying any attention to you because you're an outsider declaring uncomfortable truths, and much as the journalists covering your book eat up this story, the real reason you're ignored is very different. String theorists have many harsh critics, and are fully aware of all the criticisms themselves. Many of the well-known critics have spectacular records of research accomplishment, which means that they not only understand the basics of the field, they also have a sense of the ebb and flow of ideas and the creative process of discovery. String theorists respect these people and listen to them. Of course you don't have a real record of research accomplishment (and it is funny to blame your blog for this–what were you doing before that?). But your gaffes with non-diagonal SU(2), your above lack of understanding of landau poles and countless other instances also reveal that you don't even solidly understand the basics. We are all familiar with the annoying undergrad who thinks he knows it all, knows all the fancy words and catchphrases, but can't do any calculations and doesn't really understand physics. I'm afraid that all external indications are that you are like that undergrad writ large–except you don't have the excuse of youthful naivete and the undergrads aren't going around making pompous pronouncements about the sad state of particle theory. Most beginning grad students could give better, harsher, more incisive criticisms of string theory than you can–but of course with a solid command of the fundamentals and a close-up view of the research frontier, they are also in a better position to understand why it is still so compelling.

I am glad to hear that you are finally going to devote yourself to research. Best of luck–it is a far more worthwhile endevor than your blogging, and much more difficult and challenging. I daresay you have a lot of catching up to do. If you don't mind the advice, the field has come a long way since you were last active–you might want to sit in on a refresher QFT course at columbia before you dive back in.

40. **Clifford V Johnson**
July 7, 2006

Peter,

Thanks very much for choosing to give a level headed reply. On some things we can just agree to disagree, on others, there is a right and there is a wrong. With that I will refer you to my point (3) above.

> (3) You speak with certainty that string theory has failed as a theory of Nature. Actually, you don't know if that is true, and nobody knows that. It is your hunch that it is so, and it is my hunch that it is not so, and we are entitled to our hunches. It is a subject of ongoing research. That is as much as anyone can say.

The other point that I regularly make to you is that you should not characterize all work in string theory as what is being done by a relaive few people. Anyone who reads your blog would assume that there are hundreds and hundreds of people all working on the landscape. That is an mischarcterization of the state of research of the field. I think that the program of invesigation there is interesting, actually, and it is important for some people to explore some of that program. But it is not my cup of tea. But that's fine, its enough to know that we've got some good people working on it, and I can focus on other stuff. That's not atypical. There are hundreds of people focusing on other stuff, chipping away at the problems and learning a lot about what we're doing. You hardly ever acknowledge their existence when you talk about the program of research in string theory, and characterize it in sometimes amusingly apocalyptic terms. Case in point: You mention my work on non-critical strings, which you can of course learn about from blog posts I've written and a quick search on SPIRES. So in about 5 minutes you can learn about the existence of this. But then you characterise it as essentially nothing to do with the program of connecting strings to Nature. How do you know that? A closer look (which you have not done, it seems) will reveal that this work is focused very much on trying to understand *in a controlled environment* several of the key ingredients and stringy phenomena that are used in constucting the landscape vacua! Our best models of various non-perturbative phenomena in string theory are to be found in such models….. It is as though you are saying that studying the Ising model will teach us nothing about phase transitions in the laboratory. Another case, and this one is typical: I recently did a post on CV about a new result in understanding tachyon condensation in the critical bosonic open string. You evidently read the post, and made no comment, as have and will others. As usual, this post will probably be largely ignored in this way as compared to the other type of post about string theory which is all about people just coming and and yellling "it's all crap" or “the program has failed”, and others yelling “no it’s not” or “no it has not” back and forth. So as a result, once again the whole thing is skewed. People -including some impressionable young people in the field- get the impression that its all about the landscape, when in fact that result may be one of the single most significant and beautiful results to be presented in the field for several months. Rather than contributing to a discussion about what it might mean for the program of string theory, or even asking some questions to find out more about how it might affect -one way or another- your view of the program of research, you instead choose to ignore the existence of this kind of work in the field, or at least you don’t inform people that it is going on very much. In fact, there’s much more work of this kind going on than just a ton of people sitting chatting about the Landscape.

I could go on with this, but I won’t. I just want to say that it is very important to stop this distortion of what is going on the field. This sort of explains this impression you have obtained from people about the “state of the field”. If “the field” is characterised entirely in terms of the landscape discussion, then of course if you meet several people who don’t like it (and may themselves be deluded into thinking that is all that is going on in string theory because they’ve been decieved into think ing that way by a variety of sources) then you’re going to come away thinking that there’s a whole bunch of disgruntled string theorists.

I’d like to say in closing that there is a wealth of exciting work going on in the subject. We are not all currently working in lock step on the shock wave of some new explosion or revolution of ideas, and that’s simply ok. It is a healthy time indeed to have a large number of different problems be worked on. It is naive of you and those few apparently immature string theorists to whom you seem to have been talking to measure current progress by how far we are from the last time we all were writing papers on the same thing. Sure, revolutions are nice. I love them just like the next string theorist, but I don’t sit around waiting for them to happen and get drepressed that there’s no progress. No, instead I carry on chipping away gathering results that -along with those several others who are doing the same that you again and again don’t mention in your characterisation of the program of research- may well contribute to the next great leap forward, or at the very least, the steady march forward.

This is just how research is done, and how it always has been done in any field of science. It is not done by public talks and TV shows and books and counter-books and yelling matches on blogs. That sort of public “yes it is-no it isn’t” stuff is just a soap opera that feeds the press, deludes the public, and serves as meagre nourishment for the timid and impressionable individuals in the research game who need to be told what to think. (Reaching out to the media is important in general, and sure it is nice to try to temper the extraordinary claims of the stringevangelists from time to time, but the tail should not wag the dog.)

I love a soap opera like the next guy, but that’s all it is. I hope that the members of the press and publishing industry who are reading this also take note of what I said. You’re focusing on -and feeding- a soap opera that is a huge distortion of what is actually going on with research in the field. If you, members of the press and publishing industry, truly believe that your mission is more than about selling newspapers and copies of books (but I do understand that you have to pay the bills), and if you still remember that you may have come into journalism and publishing to actually inform people about what is going on in science, then please stop always focusing on the soap opera aspects. Take a broader look at what is going on. Don’t frame all discussions of string theory research in terms of pro- and anti- landscape.

That’s not all that there is.

-cvj

41. **woit**
July 7, 2006

Hmmm,

Ahh, yet another string theory partisan who wants to lecture me about my incompetence from behind the cover of a pseudonym. It’s amazing how many of you there are up in Boston.

Believe it or not, I’ve actually heard about the problem of the lack of asymptotic freedom of the U(1) part of the standard model. It’s discussed on page 98 of my book. There was a limit to the amount of detail I was willing to go into in my response to “James”. I was oversimplifying for the sake of concision, this point has nothing to do with the issue at hand. For the U(1) theory perturbation theory is not valid at high energies, but that is irrelevant to the argument I was making.

Sorry, but your Lubosian tactic of dealing with my criticisms of string theory by insulting me and trying to paint me as an incompetent by sleazy methods isn’t going to work. You just make my point even stronger that there is a real sickness in how string theory is being pursued. The problem is not just Lubos, but quite a few other people it seems who are as pathetic characters as him.

42. **Eugene Stefanovich**
July 7, 2006

cvj,

did you voice the same passionate opinion against publicity vs. science after “The Elegant Universe” was released? Peter is just restoring the balance. That’s all.

43. **Hmm**
July 7, 2006

Sorry–given how wildly silly your SU(2) ideas are, its hard to know what you do and don’t understand; and there was no need for “concision” in your response to James. There is no “rigorous” formulation of the standard model on the lattice, and no reason to bring it up.

And indeed, it does have to do with the point you are trying to make. The non-pertubative phenomena invoked in modulus stabilization–e.g.gaugino condensation–are in fact non-perturbative properties of the low-energy effective theory, and at least as in control as the computation of the eta’ potential in QCD. That *you* don’t understand would make me think you’re one of those annoying undergrads I mentioned who doesn’t understand the issues involved, if I didn’t know better.

And shame on you for being so condescending to James, telling him *he* doesn’t understand the physics, when what he said (minimally about physics) was precisely correct, while your “concise” statements were trivially wrong.

But enough of this–I agree with cvj that its really really really boring. And like Michael, I look forward to seeing the fruits of your research labors. Even better than blogging about it, why not write a paper, filled with concrete predictions stemming from your studies of the the glory of Dirac operators?

44. **Hmm**
July 7, 2006

Actually, come to think of it, since you always complain that people call you incompetent when you’re not, here’s an opportunity to shut them all up. Its about exactly the subject we are discussing–non-perturbative effects in string theory. Lets make it easier, and ask about non-perturbative phenomena in SUSY gauge theories (thats at least QFT which you should know all about right?). If you profess to have some understanding of the issues involved, surely you can tell us what gaugino condensation is, and how (and in what sense) we know it exactly. I think understanding this is simply a prerequisite to even talking about modulus stabilization, and since you go on and on about how we can’t use non-perturbative phenomena in a theory we only understand perturbatively, you should minimally know about something as basic as gaugino condensation. If you do, you would have no trouble whatsoever writiing a little physics post explaining it to your readers in a nice way, right? It would even be a welcome prelude to a more physics-centered blog. While you’re at it, you can explain how we know the exact beta function in N=1 SUSY theories–this is merely perturbation theory, and surely nothing compared to the glory of Dirac operators. For an extra special post, tell us all about N=1 Seiberg duality. All very beautiful field theory, that I'm sure a QFT master like yourself would have no trouble explaining to the masses. I for one would be disappointed not to be enlightened by your post on the subject.

45. **woit**
July 7, 2006

Clifford,

It's late and I have to get up early to get on a plane, so I don't have the time to write the response to your comment that it deserves, and may not have any such time for several days, but let's see what I can do in a few minutes before bed.

First of all, I'm trying to have an intelligent, respectful conversation with you, but I'm starting off in a really foul mood, after having to deal with the "Michael"s and "Hmm"s that infect the world of string theoryand hide behind pseudonyms. An incredible degree of arrogance and conviction that anyone who criticizes string theory is an incompetent infects the field you work in. Lubos is an amazing example of this, but he is far from the only one. It's completely disgraceful that many people in your field behave in this unprofessional way, and I've seen extremely little evidence that anyone has much of a problem with it. Given what I have to put up with from people like this trying to bully me into silence, you may find me less than sympathetic to some of your criticisms, for instance that I was not nice enough to "James", whoever he was.

I've been kind of surprised over the last year to see to what degree there is an increasingly widespread negative view of string theory. Among journalists I talk to, there is a backlash against much of the hype they have been fed about the subject over the years. Among non-string theorist physicists, there is a revulsion at the kind of viciousness and arrogance demonstrated by Michael, Hmm, Lubos, and others. Over the next few months and years, if you want to know why you'll be seeing anti-string theory hostility, this is a large part of it. You may not be personally responsible for hype and ignorant arrogant behavior, but your field has far too much of it.

I do understand what you're trying to do by better understanding non-perturbative string theory in toy models. I try and follow this kind of work, although I obviously don't have the time to become an expert and fully understand all of it. From everything I've seen, it seems to me you may ultimately learn many interesting things, and may have real success at understanding QCD, but I just don't see any evidence at all other than wishful thinking that any of this can solve the deadly problems that afflict the idea of trying to get unification using strings in 10 dimensions. When I say that string theory has failed, it is that failure I'm talking about. This is an issue that can be intelligently discussed: I don't see a plausible way that you're going to get around the well-known problems, if you do I'm interested to hear about it. You need to have a better answer to this question than just "we don't understand the theory, when we do maybe the problem will go away" If you don't it looks like your research program is based on wishful thinking.

I'm well aware that not all string theorists work on the landscape, but I don't think your attitude towards it holds water. If it really exists, string theory as a unified theory is just dead. You're welcome to the view that it doesn't really exist and you're trying to better understand the theory to show why. Susskind and others will tell you you're in denial. Personally I agree that it remains an open question until you truly understand non-perturbative string theory. I just don't see much in the way of progress towards this in recent years, or much reason to believe that in my lifetime anyone is going to be able to answer the question.

While I think refusing to admit that the landscape is there and trying to get rid of it is a reasonable scientific position, what is remarkable about the landscape issue is that a large number of prominent scientists accept it, and yet refuse to admit the obvious fact that it kills the idea of string theory unification. This behavior is just irrational, and bizarre, and the fact that it is going on at such a high level is seen by many people as a scandal and as evidence that there is something seriously wrong with the way string theory is being pursued. There seems to be no way to get the people involved to ever admit that things are not working. This is an amazing story to watch, and you'll find journalists are not going to resist the appeal of telling it.

There's more to say, but no time now, perhaps another…

46. **woit**
July 7, 2006

Hmm,

Even if it wasn't late at night, I'd have no interest in submitting to your little tests about my knowledge of supersymmetric gauge theory. I've written a long book about the things at issue here and hundreds of pages here on the blog. People are welcome to judge for themselves whether I know what I am talking about based on those. And I'm definitely not going to waste any more time discussing anything with you unless you tell us who you are. Doing what you are doing from behind the cover of anonymyity is incredibly unprofessional behavior.

47. **Hmm**
July 7, 2006

You keep complaining about "arrogant" behavior. I think it is amazingly arrogant to pontificate on the sad state of particle theory when you haven't actively participated in it for over 15 years. People are working hard, with little guidance from experiment, and poring years of their lives into studying these questions. On the other hand you left the field, have done little or no research (the hard work in this business!), and yet wax away on how lousy everything is. You complain about Lubosian tactics, yet bully those who bring up valid physics points–James isn't the only case–there are many instances where you pull rank on someone by saying they're "probably an undergraduate who doesn't know what is going on". You say that scientific giants like Weinberg, Wilczek and yes even Susskind have sold out the field–when they probably know a thing or two about science. This is all breathtakingly, stunningly arrogant. And I don't say this because you're a critic of stirng theory. I know and hugely respect many people who are very critical of string theory. If I want a critique of string theory, I'll take it from 't Hooft or Wilczek or Glashow, not you.

You also always, *always* back down from serious, technical physics discussions. You always refer to authority. The trouble with loop quantum gravity? "Don't know, haven't studied in detail". What about modulus stabilization? Same thing. How about gauge coupling unification in SUSY? Ditto. These are all questions that any working theorist should have an informed opinion about. You have to understand, Peter, that being a dilletante with a passing word-level knowledge of the field is fine for a silly blog, but is completely worthless for physics. Without *some* demonstrated expertise in areas that actually matter, you don't have any credibility with anyone other than your adoring blog fans and the equally uneducated science press. But in the long run it isn't their opinions, or mine or yours that matters–its the physics that matters, and your approach to it–not being an expert at anything, while loudly complaining about other research programs without doing anything yourself–doesn't further the cause at all. The cause it does push is the attainment of your 15 minutes of fame. Enjoy.

48. **Chris Oakley**
July 7, 2006

> Enjoy

I certainly will, even if Peter does not. Everyone focusing on the next, biggest, trendiest thing has been the bane of HEP. One would think that HEP theorists have given up individual thought altogether. Hmm mentions the word dilettante (spelled like this, by the way), but that is exactly what most theorists now are: a far cry from the hard-bitten sceptics like Pauli who would accept nothing unless they could derive it for themselves. But without the likes of Pauli we would have nothing of value at all. I wonder what *he* would have thought of this un-physically-motivated mass migration to ten dimensions?

49. **anonymous**

July 7, 2006

to Hmm: I try to answer to your points about "physics":

1) The Landau pole at 10^40 GeV is irrelevant, because we do experiments at 100 GeV, with less than 80 digits of precision. Probably the Landau pole is cured by quantum gravity at 10^20 GeV, but again quantum gravity seems irrelevant for the same reason.

2) I am surprised of hearing that "there are effects of order \sim E^2/\Lambda^2 \sim e^{-1/g^2} that *can't even in principle* be calculated". What are these effects? Do you mean that works on baryogenesis/leptogenesis, where e^{-1/g^2} effects are computed and crucial are crap? If instead you again confused physics with mathematics and your point 2) is something as irrelevant as 1), please don't loose time in answering.

To Woit: please don't explain us gaugino condensation in hidden sectors.

To Clifford: I think that this debate remains interesting for two reasons. First, it is an unusual phase in the history of (high-energy) physics. Second, it is unusual that inner problems get publicly discussed. These two dangerous things are slowly moving; after a few years we will see where they are going.

50. **Alejandro Rivero**
July 7, 2006

Believe it or not, I've actually heard about the problem of the lack of asymptotic freedom of the U(1) part of the standard model. It's discussed on page 98 of my book.

Funny, we mentioned this issue past yesterday at physicsforums, see post 15 at
http://www.physicsforums.com/showthread.php?t=124893#15

51. **woit**
July 7, 2006

Hmm,

If you're willing to put your name to the slander, lies and viciousness that you're engaged in, I might bother to respond. But you're unwilling to do that, presumably because you realize how deeply disgraceful and unprofessional your behavior is. It's really pathetic.

52. **D R Lunsford**
July 7, 2006

Peter,

[attack on Jacques Distler and conjecture that he is Hmm deleted].

-drl

53. **MathPhys**
July 7, 2006

Hmm, Clifford,

What's really boring is the continuing PR job of selling strings as "the theory of everything". It's been 22 years now. Can you tone it down a bit?

The mathematics has been great, but that's not physics. The extra dimensions, the compactification, the extended supersymmetries and the landscape are all suchsilly, naive ideas. We listened to these stories for almost 3 decades now (supergravity in higher dimensions is around since the late 70's), and it's not working.

Please stop selling this science fiction to the public and to unsuspecting students as deep physics, because it's not, and it has failed.

By the way, I'm not in any way anti-strings. I would have loved to see the string program flourish as it contains everything I've learnt. But it's not working. Something is wrong. So can we all be a bit more modest, please?

54. **woit**
July 7, 2006

drl,

Leave the crude personal insults to string theory partisans. I see no evidence that Hmm = Distler.

55. **Hmm**
July 7, 2006

Anonymous:

(1) Well duh, of course the Landau pole is at an irrelevantly high energy. As James was also saying, these effects are practically irrelevant; its a question of principle. Indeed quantum gravity effects should swamp these as this is where the real UV completion is.

(2) But in the absence of gravity, the theory does break down at a high-energy scale \Lambda. This means that there are uncertainties in predictions, of order powers of (E/\Lambda), which can't be resolved without specifying the UV completion. And very basic issues can depend sensitively on the UV. Here is an example. One might imagine that just before the theory hits a Landau pole, it gets embedded in a bigger non-Abelian gauge theory; so that e.g. U(1)_Y gets embedded in an SU(N) which is asymptotically free. There is dynamics that Higgses the SU(N) down down to some subgroups. It could be (indeed it typically happens that) this dynamics has many symmetry breaking patterns, only one of which is SU(N) -> U(1)_Y. Some of the other patterns of breaking could correspond to vacua of lower energy. This means that our vacuum would be unstable to decay, with a rate that would be proportional to e^{-1/g^2_{UV}}. Or it could be that our vacuum is the lowest energy one, and this decay doesn't happen. The basic question–is our vacuum stable–is a UV sensitive one, and the answer clearly has to do with the details of the UV completion, and can't be predicted simply knowing the low-energy theory. And again this happens simply because the low-energy theory is not complete. There is no "rigorously defined" Standard Model where everything can be calculated.
Of course in asymptotically free theories, non-perturbative effects are often dominated by IR physics e^{-1/g^2_{IR}} (like the eta' potential in QCD) and are calculable. This is indeed an important point: the non-perturbative effects used in string theory modulus stabilization are these calculable ones which dominate.

Oh, and quantum gravity kicks in at 10^18 GeV or earlier, not 10^20. As with Peter, my advice to you is to lean some basic QFT.

MathPhys: For the record, I can't stand the string theory "hype" peddled by Kaku, and am no fan of the elegant universe either; but none of the real leaders of the field do this. Why didn't they spend time fighting the positive hype? For the same reason they ignore Peteres negative hype now. It is unimportant trivia; instead they are focused on their research. Likewise, to you and Peter I say again: instead of whining about sociology, write papers on whatever alternatives you want, no one is stopping you.

And no,it isn't the same old story from the 70's. The strucutre is much deeper than anyone anticpated; with the idea of emergent space and holography making a stunning appearance first in Matrix theory and then in most convincingly with AdS/CFT. Gone is the statement that everything is a little loop of string; the best definition of the theory is back in terms of a QFT. But of course all these remarkable facts were discovered by string theorists. For the simple reason that when you follow non-trivial ideas, good things are bound to happen; when you sit around and whine from the sidelines, nothing will come of it.

DRL: You guys all have a persecution complex, its quite remarkable. No I'm not Distler…

Peter–you keep attacking my tactics but don't address any of the substansive physics or other points I'm making. Its easier to do this rather than to address concrete points isn't it? Just as it easier to blog away than to produce meaningful research. I too won't interact with you again. Instead I await with bated breath your forthcoming research paper with concrete predictions for physics.

56. **ks**
July 7, 2006

> I can think of numerous times when well-known non-crazy string theorists have answered your criticisms, on this blog and on others. The problem is that you choose to forget the answers and keep repeating yourself. So people have gotten terribly bored with the whole thing I suspect.

"Voltaire, you had your 15 minutes of religion-criticism fame and now let us pray to god that he will forgive you"

You shouldn't be too worried about the blog of a single fighter. But when your church goes down, you might ask yourself less about the lack of cleverness but that of reason. People can clearly see the presence of the one but the absence of the other.

57. **anonymous**
July 7, 2006

I know that it is 10^18 or less, but unless it is much less, who cares?

Vacuum decay is a good example of UV sensitive observable: I brought you a bottle of good wine to celebrate, in the eventuality that the earth will be eaten by a bubble of true vacuum.

PS: my persecution complex tells that Hmm might be the author of fig. 4 of hep-ph/0607029

58. **Umm**
July 7, 2006

"… write papers on whatever alternatives you want, no one is stopping you." – Hmm

No, there is widespread censorship everywhere in theoretical high energy physics, inspired by a mainstream intent to pursuing abject speculation and sneering at critics without studying or responding to alternatives based on observed facts.

Alternatives have a long struggle any road. They have to make checkable predictions and get those predictions checked, which requires a lot of work on money. But first they have to be published and then debated. With the field dominated by string theory at all levels from undergraduate up, alternatives are starved to death.

59. **MathPhys**
July 7, 2006

Hmm,

I actually agree with you that holography, emergent space, matrix models and AdS/CFT are all stunning ideas. It's too bad that relatively few people work on these deeper aspects of string theory.

I also agree with you that some of us have allowed the sociology of string theory to get us. I think that people like Motl, on the web, and Kaku on the popular books/lectures front, have done string theory enormous damage.

Maybeb we should all cool it.

60. **anon.**
July 7, 2006

"It's too bad that relatively few people work on these deeper aspects of string theory."

Funny, I thought *almost everyone* is working on this sort of thing. From Spires:

The Large N limit of superconformal field theories and supergravity.
Juan M. Maldacena
Cited 4048 times

M theory as a matrix model: A Conjecture.
Tom Banks (Rutgers U., Piscataway) , W. Fischler (Texas U.) , S.H. Shenker (Rutgers U., Piscataway) , Leonard Susskind (Stanford U., Phys. Dept.)
Cited 1575 times

Anti-de Sitter space and holography.
Edward Witten
Cited 2818 times

The World as a hologram.
Leonard Susskind
Cited 820 times

"Relatively few" compared to what?

61. **woit**
July 7, 2006

Hmm,
If you were making any substantive physics points I'd consider answering them. You don't have any answers to the substantive criticisms of string theory made here and in my book, and thus choose to deal with them by attacking my competence and right to make these scientific criticisms. This tactic is all too transparent. It's not going to work, and is just going to convince more people that there is a serious problem with string theory, and the way it is being pursued.

62. **anon.**
July 7, 2006

But Hmm *did* make a substantive physics point: moduli can be stabilized by gaugino condensation in the low energy effective theory, an effect that is calculable and well-understood. You don't need a complete nonperturbative framework for string theory for this to work, you just need to know some things about the low energy effective theory based on perturbative string vacua. You might *hope* that nonperturbative physics somehow makes most of the landscape vacua decay very quickly, but there doesn't seem to be any good reason to believe that. So, if we stick to physics, you keep saying that we need a nonperturbative formulation of the theory before we can address these questions. But you haven't suggested how this nonperturbative formulation can make all these vacua go away. So, let's have a physics discussion: where can the current analysis suggesting the theory supports all these vacua go wrong? If it does or does not go wrong, what does this mean for physics?

63. **woit**
July 7, 2006

anon,

Good, a substantive physics point.

I find it funny to be simultaneously carrying on an argument with Clifford and with "Hmm" here. Clifford's point of view is that we don't understand string theory well enough to have to accept these flux compactifications as true vacuum states of the full non-perturbative string theory. From what I've seen of these constructions, fully fixing the moduli requires more than just gaugino condensation. But I'll defer this question to Clifford, who is much more of an expert at this.

I'm pretty sure I've said this before, but again: I don't know whether the full string theory has these vacuum states or not. Maybe yes, maybe no, most likely it's still an ill-posed question until we have a real non-perturbative string theory. But it doesn't matter, the point is that there are two alternatives here, both leaving string theory in a highly problematic, failed state as an idea about unification:

1. The landscape exists, and string theory can't predict anything, it's a useless theory.

2. The landscape doesn't exist, for some unknown reason involving non-perturbative string theory. Then, despite what Zwiebach and others say, string theory is currently an empty idea and can't predict anything, because we don't even know what equations to solve.

The bottom line is the same in either case: you can't predict anything about particle physics in this framework, it explains nothing about the subject.

I'll leave it to Clifford and Hmm to sort out whether it's 1. or 2. I'm not claiming to know the answer, half of the experts say one thing, half the other. Doesn't really matter which it is though.

64. **woit**
July 7, 2006

Another thought,

Hmm seems to be a colleague of Lubos's, at least he's in the Boston area. If he can't get an answer out of Clifford, he can take the issue up with Lubos, who, from what I recall, also doesn't accept the existence of the landscape as true vacuum states of the full theory. If these people do sort this out and come to an agreement about what the answer is, I hope they'll let us know.

65. **D R Lunsford**
July 7, 2006

Sorry. I have to calm down about all this.

I'm just reading "End of Faith" by Sam Harris. You could replace "Islam" by "string theory" and "Christianity" by "LQG" and "Judaism" by say "geometrodynamics" and the book would still make a lot of sense. These discussions about string theory seem as unreal to me as the calculus of virgin allotment in the eternal beyond. It makes one crazy.

-drl

66. **Aaron Bergman**
July 7, 2006

Or, as has been mentioned to you multiple times, it's possible that the situation will end up like field theory wherein one can measure enough parameters to obtain predictivity with the others.

67. **Peter Orland**
July 7, 2006

Hi Peter,

I can't concentrate at my desk in Copenhagen, which is nearly as hot and
humid as New York at the moment and lacks air-conditioning.

It is reasonable that some people express their opinions
anonomously. People looking for jobs in physics may not want to
be too controversial. Why would anyone DEFENDING
string theory would wish to so anonymously; what is the risk? Answer:
because vicious vituperative attacks don't make anyone look good.

Other people seem to be questioning your qualifications. I'll defend

yours (maybe now they'll come after me – since I am not a regular
contributor/blog reader, I probably won't find out). You're a former physics guy who
did some good stuff in lattice gauge theory, and now
pursues math I know next to nothing about. I don't see any grounds
for these anonymous attacks.

Regards From the Not Especially Frozen North,
Peter

68. **anonymous**
July 7, 2006

dear Aaron,

what do you mean? All the physics we observed so far is described by a QFT with about 25 parameters. Are you hoping to predict the top mass or something like that?

69. **anon.**
July 7, 2006

Clifford's point of view is that we don't understand string theory well enough to have to accept these flux compactifications as true vacuum states of the full non-perturbative string theory.

As you are aware, these don't *have* to be "true" vacuum states. They just have to be very long-lived metastable vacuum states (which, as recent field theory studies have shown, seem to be much more common when searching for broken SUSY than are true SUSY-breaking vacua).

So it seems to me that to doubt the landscape, one needs *either*:

a) A rapid mechanism for these metastable vacua to decay.
b) An argument that somehow they were never there to begin with.
c) A cosmology that is guaranteed not to populate them.

(Is there some other possibility I've missed?) All of these seem to me to be somewhat implausible, but references to suggest otherwise would be appreciated.

Anyway, if the landscape really exists, you say "string theory can't predict anything, it's a useless theory." But that's not at all clear. As Aaron says, "string theory" might become the same sort of general framework that "field theory" is now. It might not predict every constant, but it might clarify some relationships between them which are not apparent in field theory. It might mean, say, that string theory can't predict the cosmological constant, but can explain the SUSY breaking scale and associated spectrum in terms of it. This, as I understand it, is the sort of thing Tom Banks proposes. If it were true, wouldn't it be fantastic? Surely ideas like that are deserving of further research, for their potential to connect LHC-scale particle physics with observations in cosmology? And if the idea is true, then the LHC could also falsify the framework. So, maybe we should try to understand if it's true? If not, we might learn other interesting things along the way.

It's clear that there are a lot of unknowns, but you seem to take this as a reason to give up on the theory, whereas to me it seems possible that this theory *does* describe the real world, in which case it looks like much more work is needed to figure out what can and can't be predicted. Giving up now just because not *everything* is predicted seems rather silly.

70. **anonymous**
July 7, 2006

…because *nothing* is predicted.

71. **John Gonsowski**
July 7, 2006

Wasn't the original idea of string theory that it was supposed to end up exactly field theory as in the E6 GUT? Then all the good stuff about the Standard Model becomes part of string theory? The stuff Peter is and has been interested in is the stuff you need in a good GUT. Even Lubos once mentioned in the context of supersymmetric GUTs that not enough people work on it. I don't like supersymmetry but often it seems good non-supersymmetric stuff comes from people mostly interested in the supersymmetric part. I know Lubos at least used to like the traditional E6 GUT too. Lattice related ideas may also be more useful for string theory than people realize, need more people working there too.

72. **Clifford V Johnson**
July 7, 2006

Peter, Others…..

As I said in my first comment on this thread, we've had most (if not all) of this discussion before… the answers and comments have just been conveniently forgotten. I can't do this again, as it is just. so. *boring*. Let's spend even a tiny amount of time looking at what we said already. Several of the things asked here (of me and others) have already been gone over in this thread for example:

http://cosmicvariance.com/2005/08/14/the-landscape-for-real-this-time/

August 14th 2005, people. August 14th. We're going in circles. Sigh. (There are other threads like this…. use the search engine or the pingbacks that I was careful to put in, and you'll find them.)

I think I'm done here.

Best,

-cvj

73. **anonymous**
July 7, 2006

Clifford, this time it's you that isn't adding anything constructive. All you're doing is declaring how much better than the rest of us you are, how we repeat ourselves and don't listen to your profound replies.

Basically, you're just producing the same argument that Lubos does – "Everybody who disagrees with me should shut up because I've explained with tremendous clarity why they're wrong, and the people who don't shut up are idiots." You should either stop this pretentious posing (yes, saying things like "Sigh" and "really really boring, I'm afraid" is pretentious posing – you are trying to convey a style, an attitude that you think others will perceive as cool) or stop declaring yourself to be so much more reasonable than Lubos.

74. **woit**
July 7, 2006

Aaron,

Comparing the situation of the landscape to that of the standard model is just absurd. Clearly I haven't been writing enough postings here explaining exactly what the problems with the landscape are.

Clifford,

Well, you certainly made me regret staying up last night to try and write a serious response to you. I guess we now have the official response from string theorists to my criticisms:

1. Peter Woit is an incompetent who knows no more than an undergraduate about this subject.

2. Criticisms of string theory are just boring, so we won't respond to them.

You know, this is really pathetic.

75. **woit**
July 7, 2006

anon.,

I've posted here many times about what the problems with the landscape are and why it seems very clear to me that it leads to an inherently unpredictive theory. If someone has a plausible idea about how to make even a single vague prediction using it, I haven't seen this. Banks certainly does not have a real prediction.

76. **Aaron Bergman**
July 7, 2006

Comparing the situation of the landscape to that of the standard model is just absurd.

Proof by assertion? You have no idea whether or not it's possible to get generic predictions out of the landscape of vacua and neither do I. There's no particular reason, for example, to assume that the landscape will uniformly fill the space of standard model-like theories or, say, weak susy breaking lagrangians. Your gleeful pessimism on the subject is hardly an argument.

You know, this is really pathetic.

I will have something to say at some point, depending on how things go with my current research. I regret to say (truly) that it won't be terribly positive. Perhaps you wouldn't expect any different (you seem to be finding ill in a lot these days), but I was hoping otherwise.

77. **Kea**
July 7, 2006

You know, the blue-ringed octopus is very poisonous to the Landscape…especially when it sheds it rings.

78. **Kea**
July 7, 2006

Here's a nice picture.

79. **nigel cook**
July 8, 2006

"You have no idea whether or not it's possible to get generic predictions out of the landscape of vacua and neither do I." – Aaron Bergman

Exactly! It isn't science. So why are people working on it?

"I can't do this again, as it is just. so. boring." – Clifford Johnson

Yes, but that's string theory! No physical predictions, nothing.

80. **fh**
July 8, 2006

Just saw this, well, there have been undergrad String Theory courses at my home university for a while (also conformal field theory and other more mathematical physics minded courses), but then, in Germany undergrad goes a bit higher then in other places.

81. **Eugene Stefanovich**
July 8, 2006

nigel cook:

Exactly! It isn't science. So why are people working on it?

I disagree. String theory is a branch of mathematics which uses physical terminology and desperately wants to make a connection to the physical world, but wasn't able to do that yet. String believers are honestly trying to calculate a number that can be compared to measurements. We shouldn't discourage them from doing that. We should just keep in mind that their loud promise of 20-or-so years ago to revolutionize physics hasn't materialized, and draw conclusions.

82. **D R Lunsford**

July 8, 2006

Eugene said

String believers are honestly trying to calculate a number that can be compared to measurements.

In the past many people honestly tried to deduce the parallel postulate, square the circle, and solve the fifth order polynomial equation with radicals. The point is, the structure of string theory seems incapable of producing any numbers comparable to anything. No one can answer Peter's challenge. If anyone could, he'd probably shut down this blog on the spot. Given its extremely shaky physical motivation and structure, combined with its impotence, the only "honest" thing to do is move on.

83. **Eugene Stefanovich**
July 8, 2006

D.R. Lunsford:

In the past many people honestly tried to deduce the parallel postulate, square the circle, and solve the fifth order polynomial equation with radicals.

If somebody wants to prove the parallel postulate, let him do that. May be he'll figure out a non-Euclidean geometry eventually. If somebody wants to do strings, no problem. The only real question is how much tax dollars (euros, pounds, rubles,…) shoud be invested in these attempts?

84. **D R Lunsford**
July 8, 2006

Eugene,

This is the main point – one has to use something else other than hope, reason, and persistence to get ahead. You have to recognize when you are going in the wrong direction. To use our example, Saccheri had everything at his hands to deduce non-Euclidean geometry. He even wrote a book, "Euclides ab Omni Naevo Vindicatus" (Euclid Freed of all Flaws), that came a hair's breadth from a revolutionary idea – but, because he had a fixed notion in his head about what constituted progress, he missed his chance and became a footnote in the history of mathematics. Even Felix Klein, with his vast intuition, missed relativity when it was right in his hands. What is most needed is sound judgment and a prepared intuition, and the willingness to go in a new direction when the old one becomes a dead end. The judgment of string theorists is manifestly unsound. They are willing to be completely tangled up in absurdity. Hope and bluster is not enough.

-drl

85. **Eugene Stefanovich**
July 8, 2006

D.R. Lunsford:

I am all for *the willingness to go in a new direction*. I am tired of people telling the same old story about how we have two wonderful theories – the Standard Model and general relativity – and the only thing left is to make them work together. If these theories do not want to work together, then either one or both of them are wrong. Let's face it. Nevermind that they were tested in experiments/observations. Epicycles were tested in observations too.

Sorry, Peter. I probably went too far off-topic.

86. **D R Lunsford**
July 9, 2006

Eugene,

Obviously the SM and GR in their own worlds is *the wrong direction*. So we agree.

-drl

87. **stefan**
July 9, 2006

I've read some posts on strings and related material across various blog-sites around the Net; it seems that most physics-related researchers are tossing various views on the (assumed) correctness or incorrectness of the string-theory programme. I still wonder if this whole enterprise is pre-mature; after all, the "giants" of the field still contend that string/M-theory is still not fully understood. Then, shouldn't the community focus more on understanding more rigorously the subject-matter? Wouldn't it be a great service if we had more discussion(s) and debate on the mathematical aspects of our current Yang-Mills gauge theories… after all, this issue has genuine value since it is a requisite (and appropriate) problem of discussion. (One only needs to look through the Millenium Problems site at Clay Institute).

How would everyone at this forum react to this suggestion. Your feedback would be most welcome.

Stefan

88. **Peter Woit**
July 11, 2006

Hi Stefan,

I agree with you completely. There is still a huge amount we don't understand about the gauge theories that are relevant to the standard model, and these problems deserve a lot more attention than they're getting. At the moment, only one is getting attention, that is understanding strongly coupled QCD, and in this case only one possible approach to that problem is getting attention, that of string theory. I've been keeping a list of what seem to me important aspects of gauge theories that are not understood, hope to turn this into a paper or a series of blog entries at some point.

89. **Who**
July 11, 2006

Undergrad string courses = top of the market behavior.
When it is time to sell off, professional investors will instinctively see lots of reasons for the small private investor to buy.

As string research becomes less exciting, it gets attractive for former researchers to teach undergrad string, perhaps even (eventually) in the liberal arts colleges, but first of all in top universities. Stanford curriculum can serve to lead other venues. The pros will **instinctively** want to be as convincing as possible that their overstocked specialty should be taught to undergraduates—come up with lots of reasons: maybe even liberal arts-type reasons. Enriching the mathematical education etc. Because it will give the surplus research crew something to do they can consider useful.

The undergraduates pay as usual, like the private investor, for the mistakes of their betters.

90. **Stefan**
July 12, 2006

Hi Peter,

Thanks for the reply. I guess I'm not the only one with some reservations on how this whole string / M-theory enterprise is taking some of our brightest minds towards. I should however state that I'm nowhere near the position of a researcher (an undergrad, actually) to provide a detailed technical critique on strings / M-theory; but one doesn't have to be Newton to understand that what is currently taking place is more speculative than science – at least as it is understood in the traditional sense of the term.

I fear that with so much hype and hoolabaloo surrounding the "mother of all physics theories" some of the most creative and thoughtful minds of our generation are getting swept away by this whole issue/affair. I don't mind string theory as a programme of study, the problem is how it is hogging up much of the landscape of theoretical physics with it suffocating stranglehold on research – much to the limitation (and disadvantage) of other well-motivated programmes. (I would rather term this SPECULATIVE programme as "string hypothesis" (!!) – it stays true to the terminology of science.)

Anyhow, thanks for this forum… a whole lot of fun to read! 🙂
By the way, do you know of any site / focus group specifically addressed to the mathematical aspects of gauge theories? If not, do you want to start a forum? I would love to become a member.

Bye.

Stefan

91. **James**
July 12, 2006

Peter, although I am a 12 years old elementary school kid, I can't notice you not responding anything to this earlier post which I will replicate below. My question is whether you think that all the European universities mentioned below are run by some crazy professors who don't know how to put together an undergraduate curriculum? Or how else would you characterize these aforementioned professors who are responsible for the string theory undergrad courses at these fine universities?

It might be a shocker for you but several European universities offer undergraduate string theory courses and have been doing so for at least 15 years (that is what I am aware of). Examples: Amsterdam, Groningen, Utrecht, Brussels, Berlin, Budapest, Prague, Moscow and I'm sure there are plenty more.

Mind the fact that there are loads of old professors for whom the phrase "string theory" means something from the 70's and that is what they teach. It would be very difficult for you to convince them that this material is some new age hype when they were studying it 30 years ago.

Of course there are European universities where the more modern approaches are taught to undergraduates under the banner "string theory", e.g. Amsterdam and I suspect this area is where you actually target your criticism.

My observation is only to enlighten anyone who thinks that "string theory" is a large monolithic blob which can be commented on and criticised in one go.

Hope this helps,
James

92. **woit**
July 12, 2006

James,

Sorry, but responding to people who anonymously insult me just isn't high on my priority list of things to do when I'm very busy. I was away for a few days and wasted much of the little free time I had responding to string theorists accusing me both of being incompetent and of being nasty for beating up on someone like yourself who clearly only had at best an undergraduate's knowledge of the issues here.

As for your comment, first of all it wasn't a question; you're claiming some facts are true, and I'm both dubious about some of them and don't think they're relevant at all to the implicit question in my posting of whether string theory is something appropriate to teach to undergraduates in the US.

Some comments of my own:

1. European undergraduates are more advanced and more specialized than U.S. undergraduates, often more comparable to a master's degree program here.

2. I just find it very hard to believe you that people are teaching courses on 1970's style string theory at all these institutions. I strongly suspect you're thinking of S-matrix theory sorts of things, which are not really string theory. But I don't know, I may be wrong. It would be interesting if you had any links to syllabi of these courses so one could judge.

93. **JC**
July 12, 2006

Peter, James,

Years ago I remember several older professors mentioning that at some places, they did have the equivalent of a graduate course on topics in analytic S-Matrix back in the 1960's. This was in the days when that analytic S-Matrix bootstrap stuff was at its peak popularity. The most they ever mentioned about dual resonance models (ie. "string theory") such as the Veneziano model, was a lecture or two typically at the end of the course. (That is, after the Veneziano model was first published in 1968). Before the Veneziano model was published, courses on analytic S-Matrix theory mainly covered the sort of topics in Geoff Chew's 1961 book on the subject.

Just like any other graduate course, there were some highly motivated undergraduate students enrolled. None of these particular older profs could recall there ever being an undergraduate course on analytic S-Matrix theory and/or dual resonance models, in those days. They all said that the attitude in those days was that if a motivated undergraduate student really wanted to know some "advanced topics", they can just take the graduate courses, attend seminars, and/or read the original papers on it.

Offering a string theory course aimed towards undergraduate students appears to be a first these days. Albeit, this may not be totally unprecendented. Over the years I've heard of folks teaching undergraduate courses on particle physics which involve calculating Feynman diagrams. In these courses, they just "quote" the Feynman rules and a prescription to work out tree-level Feynman diagram calculations (all without proof). They just assumed that interested students will eventually take courses on quantum field theory. Years ago when I first took an undergradaute course on particle physics, the professor didn't even bother calculating any Feynman diagrams.

94. **shame**
July 12, 2006

Peter–

There you go bsing again! You say to poor James "…someone like yourself who clearly only had at best an undergraduate's knowledge of the issues here". You're really quick to pull the authority card aren't you? As was clearly explained above, James's understanding of the issues was correct and yours was just wrong. If he is only an undergraduate, all the more power to him. Quit bullying people who understand physics better than you do. If anything, as an amateur you should be grateful that experts take time to explain things to you, especially when you tend to be so hostile and rude.

Oh, and we're all still waiting for your electrifying ideas for new directions in theoretical physics. Preferably–a paper with some specific predictions that can be falsified. When can we expect it?

95. **woit**
July 12, 2006

shame,

Now, what's your excuse for hiding behind anonymity to attack me? I think your behavior is deeply shameful, you know it, and that's why you're doing it this way.

I wasn't at all bullying James, I think I was actually being extremely polite in dealing with someone who came here to engage in nasty attacks on me from behind the cover of anonymity. The line you quote was a reference to Clifford's attack on me as bullying a poor undergraduate. Clifford seemed to believe James is an undergraduate, but I have no idea what he is since he's hiding his identity, and (like you), attacking my credentials by refusing to tell us what his are.

And no, I'm not an amateur, but a professional with a Ph. D. in this subject. Are you? You certainly are behaving in an extremely unprofessional way here.

96. **woit**
July 12, 2006

shame,

Looking at my logs, you seem to be at the KITP in Santa Barbara, and to have found your way to my blog via Lubos's site (since your connection here came via his charming translation of my site into "jive"). You seem very concerned about my bullying behavior, just wondering if you have written in to Lubos's blog to criticize him in a similar way, or if you think the way he behaves is fine.

97. **James**
July 12, 2006

For those of you coming late:

Peter wrote "What is known about string theory is how to construct a (divergent) series, one that is supposed to be a perturbation expansion of some still unknown exact theory, and thus an asymptotic series in the string coupling for the true result."

To which I responded that the mere fact that there is no non-perturbative definition of string theory does not constitute a serious criticism. Clearly, the Standard Model has no non-perturbative definition (except for QCD but that is also non-rigorous) and it is a perfectly fine physics theory.

I also emphasized that the lack of prediction and consequently the lack of experimental verification *is* a serious criticism of string theory. This valid criticism however does not mean that *any* criticism is valid.

That was our short chat about physics. Now here is what Peter calls "insulting":

I wrote "Hope this helps you understand why string theorists and other high energy physicists do not respond to your book in a serious way and […] do not take you seriously over all. […] Even plenty of string theorists exist who are critical in a reasonable way, unlike you I am sorry to say. [….] Again, it is my intention to help you understand the reason for your considerable isolation in high energy circles especially since you yourself asked the question in one of your postings why there is no serious string theory reaction to your book."

Since Peter himself was wondering why string theorists are not responding to his book I truely believe that I am doing him a favour for giving a possible explanation. He is entitled to think that this explanation is wrong, but it can hardly be insulting. Now the fact that I do not consider him an expert in string theory can also hardly be considered an insult, since Peter himself declared this a number of times. My statement that he is not taken seriously in high energy physics circles is I believe factual. The basis for this statement is that I believe the following (somewhat vague) ingredients are necessary for being taken seriously:

(1) Well-known high energy physics results in research papers
(2) Presence as an invited lecturer at respected high energy physics conferences
(3) Persuing a serious research program in high energy physics

Peter, if you disagree that the above ingredients are necessary for the label "taken seriously by the high energy physics community" then what ingredients *are* the necessary ones? Or do you think that the above mentioned ingredients are applicable to you?

It goes without saying that Peter is a high energy physicist with a PhD. It also goes without saying that he is entitled to express his views on any subject he chooses let it be high energy physics or string theory in particular and in any shape or form he chooses let it be a weblog. However I believe I am again doing a favour to you Peter by point out that this alone is *not* enough to be taken seriously by the high energy physics community and it is foolish to demand attention when you do not deserve it. It will only serve to increase your frustration.

Please consider these comments as help expressed by a friend in the good faith that you will one day realize that the path you have taken is completely disconnected from the path of a high energy physics researcher. I truly hope that once you realize this you will go back to research and publish your results that will be serve as well as be appreciated by the community. Once that happens I am sure you will realize that the years you spent diverged from this path were completely lost years.

Your friend,
James

98. **woit**
July 12, 2006

James,

You're just repeating your same incorrect argument that the status of string theory and the standard model are similar. It's nonsense and I explained why, an explanation that you chose to completely ignore.

If you want to discuss scientific issues, I'm willing to do so, although it would help if you would actually respond to what I write. I'm not about to waste my time dealing with ad hominem attacks, especially not from someone unwilling to put his name to them.

As for my research results, they're sketched in http://arxiv.org/abs/hep-th/0206135 and I hope in the future to write up more recent things I've been working on. I certainly think it would be better if I did a better job of writing things up, but I don't regret at all having spent much of my career working on ideas that are quite different than those pursued by most of the high energy physics community.

99. **James**
July 13, 2006

Peter,

You say you are acting in a professional way. That is analogous to a prime minister acting professionally at the housewarming party of his nephew since the set of activities performed as the professional duty of a prime minister and the set of activities performed at a housewarming party do not intersect. Similarly, the set of professional activity performed by a physics researcher and the set of activity performed while posting to a weblog do not intersect.

If we were in a religious context I would close by saying that I am praying for your soul but since we are not I merely repeat again that I hope one day you will again utilize your professional skills to the benefit of the high energy physics community in a form that is demanded by the profession of a researcher.

Your friend,
James

100. **woit**
July 13, 2006

Thanks for the advice James, but I do think that what I am doing here and with the book that I wrote was to utilize my professional skills to the benefit of the high energy physics community.

And both projects are parts of my professional life.

101. **Stefan**
July 13, 2006

To Everyone:

This weblog seems filled with posts from individuals already committed to one view or the other on the (supposed) correctness or incorrectness of string theory. If we were discussing religion – say on the issue of how many angels can dance on the tip of a needle – "scientists" would readily dismiss us for being dogmatic-minded zealots. The ultimate anyone (at least those who do not claim to know the inner workings of nature beyond that which is currently known via experimentation) can say is… "Let us see. Maybe it will turn out ultimately be the correct formulation of fundamental physics, maybe not; but it doesn't hurt to explore…" I think every string "hypothesist" would ultimately agree to that point. Otherwise, we lose that ultimate sense of objective understanding which has guided the scientific enterprise so fruitfully since its inception.

To Peter:

When is your book coming out?
Also, (as asked in the previous post), do you know of any site / focus group on mathematical aspects of gauge theories? If not, do you want to start a group? I'm hoping this may prove valuable for the general HEP and Math-Phys communities. (At least it should to a cross-section, including string-"hypothesists" themselves.)

Best wishes.
Stefan

102. **Stefan**
July 13, 2006

Hi Peter (again),

Just finished reading 'String Theory: An Evaluation'; I encourage open-minded individuals to have a look through it; for string-enthusiasts: there probably isn't much to change your minds.

Some quotes (please read the article first, because these quotes can readily be misinterpreted):

"From a mathematician's point of view, the idea that M-theory will replace the Standard Model with something aesthetically more impressive is rather suspicious."

[Stefan]: Do you elaborate greatly on this point in your book? What motivated you to remark as you did?

"To the extent that the conceptual structure of string theory is understood, the Dirac operator and gauge fields are not fundamental, but are artifacts of the low energy limit. The Standard Model is dramatically more "elegant" and "beautiful" than string theory in that its crucial concepts are among the deepest and most powerful in modern mathematics. String theorists are
asking mathematicians to believe in the existence of some wonderful new mathematics completely unknown to them involving concepts deeper than that of a connection or a Dirac operator. This may be the case, and one must take this argument seriously when it is made by a Fields medalist, but without experimental evidence or a serious proposal for what M-theory is,
the argument is unconvincing."

[Stefan]: Good remark. Where can we find useful info on Dirac operators? What about Steven Weinberg's opinion? Does he feel similarly to you?

"Even granting that string theory is an idea that

deserves to be pursued, how can theorists be encouraged to try and find more promising alternatives? Here are some modest proposals, aimed at encouraging researchers to strike out in new directions:
1. Until such time as a testable prediction (or even a consistent compelling definition) emerges from string theory, theorists should publicly acknowledge the problems theoretical particle physics is facing, and should cease and desist from activities designed to sell string theory to impressionable youths, popular science reporters and funding agencies.
2. Senior theorists doing string theory should seriously reevaluate their research programs, consider working on less popular ideas and encourage their graduate students and post-docs to do the same.
3. Instead of trying to hire post-docs and junior faculty working on
the latest string theory fad, theory groups should try and identify young researchers who are working on original ideas and hire them to long enough term positions that they have a chance of making some progress.
4. Funding agencies should stop supporting theorists who propose to continue working on the same ideas as everyone. They should also question whether it is a good idea to fund a large number of conferences and workshops on the latest string theory fad. Research funds should be targeted at providing incentives for people to try something new and ambitious, even if it may take many years of work with a sizable risk of ending up with nothing.
Particle theorists should be exploring a wide range of alternatives to string theory, and looking for inspiration wherever it can potentially be found. The common centrality of gauge fields and the Dirac operator in the Standard Model and in mathematics is perhaps a clue that any fundamental physical model should directly incorporate them. Another powerful and unifying idea shared by physics and mathematics is that of a group representation.
Some of the most beautiful mathematics to emerge from string theory involves the study of (projective) representations of the group of conformal transformations and of one-dimensional gauge groups ("loop groups"). This work is essentially identical with the study of two dimensional quantum field theory. The analogous questions in four dimensions are terra incognita, and
one of many potentially promising areas particle theorists could look to for inspiration.

[Stefan]: Why not post all the most important reference works on the above topics? This will no doubt attract more individuals looking for further ideas to work on.

During the 1960's and early 1970's, quantum field theory appeared to be doomed and string theory played a leading role as a theory of the strong interactions. Could it be that just as string theory was wrong then, it is wrong now, and in much the same way: perhaps the correct quantum theory of gravity is some form of asymptotically free gauge theory? [Stefan: Important proposition] As long as the
best young minds of the field are encouraged to ignore quantum field theory and pursue the so far fruitless search for M-theory, we may never know."

[Stefan]: Agreement all-round.

Seems like we have great many things we can agree upon. 🙂

Stefan

103. **MoveOnOrStayBehind**
July 13, 2006

>The basis for this statement is that I believe the following (somewhat vague) ingredients are necessary for being taken seriously:

(1) Well-known high energy physics results in research papers
(2) Presence as an invited lecturer at respected high energy physics conferences
(3) Persuing a serious research program in high energy physics

Well, you forgot the most important point above all: namely saying something which is reasonable and makes sense. This and nothing else counts. The statements given here and elsewhere constitute a gross misrepresentation of a well-motivated and very successful major research effort, and thus it is no surprise that they are not taken seriously by any expert, rather the only people who react are laymen who can be easily fooled by such statements. To impress experts and be taken seriously by them, takes somewhat more than what emanates from here; laymen of course cannot distinguish this from real science, and easily become victims of this private crusade.

104. **woit**
July 13, 2006

Stefan,

Sorry, but I don't know of any specific group of the kind you ask about, and I'm afraid I'm already overwhelmed by moderating this blog, not interested in starting up another forum that would be even harder to moderate.

I'll try to answer some of your questions about the material in the book, don't have time right now to really do them justice:

1. The passage you go on to quote is an elaboration of the remark about M-theory. Not sure what I can say that would be helpful and not just repeating it.

2. There are many, many sources of different kinds about the Dirac operator, from every advanced quantum mechanics books to a huge mathematical literature. The Dirac operator is crucial to understanding the index theorem, and there's lots of places to read about that. One thing to do is to get ahold of Atiyah's collected works and try reading every expository paper by him on the subject.

3. Weinberg has not been publicly critical of string theory, although after working for a while on it, he long ago voted with his feet and left the field to do cosmology. His perspective is quite different than mine, traditionally he has been quite hostile to the use of geometry in theoretical physics.

4. In my 2002 paper on the arXiv there are references of this kind. Unfortunately when I say little is known about these problems, I'm quite serious, there isn't much useful literature. One good place to read about a lot of this is Jouko Mickelsson's book "Current algebras and Groups".

105. **woit**
July 13, 2006

MoveOn,

Maybe you can explain to me why almost all string partisans (except Lubos) who attack me as incompetent hide behind anonymity?

You're quite right that the relevant criterion is saying something which is reasonable and makes sense, which is exactly what I'm doing. The criticisms I am making here are definitely taken seriously by many experts, some string theorists some not. Recall that Lee Smolin has written a book making much the same points I do. If you actually think something I write is incorrect, feel free to say so and we can discuss this. Just joining other string theory partisans in anonymous attacks without any scientific argument gives strong indication that you can't actually do this.

106. **Peter Woit**

July 13, 2006

Stefan,

Publication date in the US is Sept. 8, copies should be available starting August 29.

107. **David**
July 13, 2006

Stefan,
Some recent mathematical results that I found interesting on Dirac operators can be found at the following people's websites:
Gerald Teschl-Math U of Vienna, Susanne Teschl- Technikum Vienna, and Fritz Gesztesy- Math U of Missouri.
Best,
David

108. **Stefan**
July 14, 2006

Thanks to both of you – Peter and David – for your generous feedback. As for the focus group – don't worry Peter, I will search for it myself.

Regards,
Stefan

109. **Stefan**
July 16, 2006

Judging from how the field of high-energy physics is currently chugging along one gets that peculiarly unnerving feeling that HEP is nearing its (pragmatic) end. Even with all the fanfare being given to strings and other approaches there does not seem to remain much scope for futher experimental verifications in the near future. As I see it this may imply one of several predicaments:

(a) HEP research will remain murky and unsubstantiated – for as long as we can delve out great theoretico-mathematical edifices lacking any accessible means of empirical verification; but since many of us like speculative studies this shouldn't cause us much unnecessary discomfort;

(b) Many researchers (as hinted above) will begin to explore and branch out into new territories – territories once considered taboo within mainstream academia… like consciouness studies (a la Penrose), philosophical speculations (of all types, shapes and flavours), and so on;

(c) some amazing body of discovery will jostle us out of our doldrums – this will most likely come from high-energy astrophysics (or some hitherto unsuspected corner of physics); a flurry of activity will ensue… until quietude once again engulfs the frontier…

What is my point you ask? Not much, except to point out that that once-glorious field prided and envied the world over as the "crown jewel" of fundamental science is nearing its painful and aged death…

…so let it be… so let it be…

I wonder what former hep scientists do for a living once they leave the field?

110. **D R Lunsford**
July 16, 2006

Stefan, don't you think it is a little arrogant to claim that physics is dying? Would you have said that in 1895?

The fault, dear Stefan, is not in our Hodge stars, but in ourselves.

-drl

111. **Walt**
July 16, 2006

Stefan pretty clearly said HEP, not physics as a whole.

112. **David**
July 16, 2006

Stefan,
Try molecular physics. It's fun and there are even experiments. Some of us teach in strange departments but they let us research what we want (often anyway).
Best,
David

113. **Stefan**
July 17, 2006

I was thinking more in the line of non-linear science – which promises to open up a territory still as-yet unexplored; and which may provide us with very important insights into the "physical" world – the world that we – as physicists – claim to want a more complete and thorough understanding of. I think perhaps the most crucial and outstanding results in the coming years (with attendant Nobel prizes) will evolve from this branch (my hunch anyway; Stephen Hawking was among the first – to my knowledge – to air such a view publicly).

[If anyone is further interested Ilya Prigogine's fantastic work 'Order out of Chaos' may prove a valuable read.]

The current approaches (in HEP-research) all seem exciting – regardless of their ultimate relevance (or lack thereof) to physics as a field; I just don't think in the long-run younger scientists will want to wager their careers on an area (say string theory, LQG, or other theory topics with little prospects of experimental verification – atleast in the near- to middle- term) which remains so far removed from experimental verifiability… but then we are all entitled to our own views…

If strings (and perhaps altogether current HEP research) proves less and less appealing with time, some (randomly suggested) propositions include:

(a) studying mathematical physics – there are very many traditional problems which to-date remain unsolved, e.g. finding rigorous mathematical framework for QFT, modern approaches to classical mechanics, N-body problem, and other well-known problems;

(b) condensed-matter physics – generally a good field to study nowadays I hear;

(c) nonlinear science – not a bad place to devote oneself if you aspire to do some ground-breaking research;

(d) whatever scientific problems one finds interesting and/or worthy of study… as long as it remains in the purview of "experimental science" (vs. "philosophical science"); this, in its truest spirit, has been what has kept the field so rich with activity since its initial founding, and why the term "science" is held in such high regards the world over…

Some random thoughts from my part… hope we can make a meaningful debate out of this…

Stefan

114. **Eugene Stefanovich**
July 17, 2006

Stefan:

I like point (a) of your program. Besides, with our emphasis on "high energy" in HEP, we seem to be missing a very important part of physics, both experimentally and theoretically. There are virtually no studies of time-dependent processes. In HEP experiments, such studies are almost impossible, because everything happens very fast. On the theoretical side, QFT can tell nothing about them, because renormalized QFT lacks a well-defined finite Hamiltonian.

The property accessible to modern experiments and theory is the S-matrix, but there is a lot of physics beyond S-matrix, and we know almost nothing about this kind of physics. To learn more, it is not necessary to go to higher and higher energies. There are quite a few fundamental things that can be learned from low-energy processes with much improved resolution (especially, time resolution) of instruments.

For example, I am very interested to know whether interaction potentials between charged particles are retarded (i.e., Lienard-Wiechert potentials). So far, nobody has measured the retardation directly.

115. **Stefan**
July 18, 2006

Peter,

I reference to our recent dialogue on gauge theories: I just saw the movie of Atiyah on People's Archive: Atiyah seems to have worked on Dirac Operators during his middle-years. I'm wondering if you can give me a list of important contributions Atiyah (and collaborators, e.g. Drinfeld, Hitchin, Manin, etc.) made on this topic, and also on gauge fields / theories in general (only the most significant ones).

I gather you will have teaching / research responsibilities, so please take your time. I'm thinking this should be useful reading; I want to start a website devoted to this area.

Just wondering why no-one has done so already.

Your friend,
Stefan

116. **Stefan**
July 18, 2006

p.s. Please refer to my post on 'Atiyah and Gell-Mann' as well.

117. **Philosophical Phil**
July 25, 2006

What we need is a superhero to answer our questions……Go Sparticle!!!!!

Quick Links

July 6, 2006
Categories: Uncategorized

Taking off tomorrow for a long weekend, internet access may be spotty. Here are some things that may be of interest:

HEPAP is meeting today and tomorrow, the presentations given at the meeting are available here. JoAnne Hewett is there and has a posting on Cosmic Variance.
The Seed article with various physicist's views about what to expect at the LHC that was discussed here earlier is now available online.

There's an article about Jim Simons in Newsday (via Angry Physics).

Maybe a cosmologist can comment on the significance of this, but over at CosmoCoffee there's a discussion of a new paper reanalyzing the latest WMAP data and coming up with a scalar spectral index n_s= .969 +/- .016. This is now 2.0 sigma away from 1, instead of the 2.7 sigma of the earlier analysis. This deviation from 1 was widely sold as evidence for inflation (since the simplest inflationary models give values slightly less than one), the fact that it is now only a 2 sigma effect seems to make this case a bit weaker.

The Institut Henri Poincare in Paris will be having a three-month-long program on Groupoids and Stacks in Physics and Geometry. The web-siter there contains a good associated overview of the subject.

Bruno Kahn has an excellent expository article on motives.

Over at the Edge web-site Lawrence Krauss has a piece called The Energy of Empty Space That Isn't Zero. It's partly about the cosmological constant, and discusses a workshop on Confronting Gravity that he organized back in March, which brought many prominent theorists together at a Caribbean resort to discuss physics, travel in a submarine, and hang out at the "private island retreat" of the funder of the event, science philanthropist Jeffrey Epstein.

Krauss has many provocative things to say about the current state of theoretical physics, including perhaps the most concise and vivid description I've read in a while:

It's been very frustrating for particle physicists, and some people might say it's led to sensory deprivation, which has resulted in hallucination otherwise known as string theory.

He also has a somewhat longer skeptical take on extra dimensions, together with an attempt at positive spin:

Many of the papers in particle physics over the last five to seven years have been involved with the idea of extra dimensions of one sort or another. And while it's a fascinating idea, but I have to say, it's looking to me like it's not yet leading anywhere. The experimental evidence against it is combining with what I see as a theoretical diffusion — a breaking off into lots of parts. That's happened with string theory. I can see it happening with extra-dimensional arguments. We're seeing that the developments from this idea which has captured the imaginations of many physicists, hasn't been compelling.

Right now it's clear that what we really need is some good new ideas. Fundamental physics is really at kind of a crossroads. The observations have just told us that the universe is crazy, but hasn't told us what direction the universe is crazy in. The theories have been incredibly complex and elaborate, but haven't yet made any compelling inroads. That can either be viewed as depressing or exciting. For young physicists it's exciting in the sense that it means that the field is ripe for something new.

Comments

1. **Eugene Stefanovich**
   July 6, 2006

   For young physicists it's exciting in the sense that it means that the field is ripe for something new.

   This should be exciting not just for young physicists, but for everyone. When David Gross talks about not understanding what is space-time and Roger Penrose wants to change quantum mechanics, that's a sign of something big coming.

2. **Peter Woit**
   July 6, 2006

   Eugene,

   I'd like to be that optimistic, but I seem to remember both Gross talking about getting rid of space-time and Penrose wanting to change QM at least 15 years ago. Krauss is right that the time is ripe for people to give up on things that haven't worked and try something new. Will they do it though?

3. **rof**
   July 7, 2006

   Right now it's clear that what we really need is some good new ideas.

   The anthropic principle, extra dimensions and parallel worlds. These are the new ideas you will hear about, again and again and again. They are the exciting new ideas that each generation of daydreaming physicists echo again and again. They aren't even new ideas.

   And anyway, if somebody announced what the correct new idea was today, they'd be dismissed as a crackpot. A good old fashioned no-nonsense attitude has thoroughly infused the physics community. As a result, they are dismissive. If there is a lesson to be learned from string theory, it should be that it takes careful investigation to distinguish between nonsense and complex but correct ideas. But that lesson will not be learned. People display dismissiveness because the immature are impressed by dismissiveness and consider it worthy of imitation.

   Each individual physicist will insist that he is perfectly reasonable. All you have to do to convince him that your new idea is worthwhile is immediately produce a full theory of quantum gravity and have it endorsed by some celebrity physicist (even though that celebrity physicist will make the same demand of you before listening to your idea). Otherwise you're a crackpot.

   Physicists need to face the facts: As a community, they're too immature to be able to provide an environment where new ideas can be communicated without ridicule. Too much narcissism and arrogance. Children.

4. **Eugene Stefanovich**

July 7, 2006

Peter,

I was not suggesting that new big ideas will come from Gross or Penrose. Most likely, not from them, in my opinion. I was just hinting that the current state of disarray, when frustrated people are willing to give up the most secret things, is a prelude to some radical change. I just smell it in the air. You are right that in order to move forward we should not be afraid of new crazy ideas. The question is: how "crazy" is good enough?

5. **Anon**
July 7, 2006

The reduction of the significance that the spectral slope of the primordial perturbations is less than one does not have a strong effect on the possibility of inflation. The spectral slope may be used to distinguish between various potentials for the inflaton. But the current experimental constraints are not that discriminating. There are a couple of s that were favored pre-wmap but now give a bad fit to the data. The PLANCK satellite will dramatically improve this situation. So will better polarization measurements by ground and balloon based experiments. Ultimately, a polarization satellite will probably be needed to really be conclusive.

6. **Krotos**
July 8, 2006

For young physicists it's exciting in the sense that it means that the field is ripe for something new.

My take on this as a youngish physicist is that I'd like to believe it, but the fundamental problem that has led particle physics (and, increasingly, cosmology) to the current situation is the inability to satisfactorily test these theories experimentally. And I don't see that changing any time soon. Even the LHC will be orders of magnitude below the energies necessary to really probe the physics behind, say, dark energy or inflation. The best we'll be able to do with forseeable technology is pretty much what we're doing now, i.e., indirect observations and inferences from astrophysical measurements. That's good and valuable, but generally it doesn't provide the precision necessary to do more than favor one large class of models over another large class.

Yes, one can certainly hold out hope that some 21st century Einstein will come up with a rigorous, revolutionary new theory that unifies gravity and quantum mechanics and explicitly predicts the value of the cosmological constant, etc. But historically, that kind of thing has not tended to occur in the absence of data.

7. **Who**
July 8, 2006

Krotos, I beg to differ
**… not tended to occur in the absence of data…. **
which suggests there is a lack of observational data.

On the contrary, one might say that theorists now have TOO MUCH data. More data than they know how to assimilate. If you are a theorist today, you have dark energy (70 percent of universe) and dark matter (another big part). You will soon have results on very high energy gamma photons and cosmic rays from projects such as GLAST (2007) and AUGER (current).
Dark matter you can see very clearly in galaxy rotation and lensing. Dark energy or its equivalent is seen clearly in accelerating expansion and in the microwave background. These look like major puzzles of just the sort that a theorist could wish for. Imagine if you could make a model of spacetime and matter that would fit all this hugely unexpected data!

And I mean fit even APPROXIMATELY, to first order. You seem to be asking for higher precision measurements to distinguish between closely similar models, when, as far as I am aware, we don't even have the **beginnings** of a model to explain the measurements we already have.

8. **Krotos**
July 8, 2006

You seem to be asking for higher precision measurements to distinguish between closely similar models, when, as far as I am aware, we don't even have the beginnings of a model to explain the measurements we already have.

Certainly there's a lot of "data" in the sense that dark matter and other non-SM phenomena have been indirectly observed and have had approximate limits put on their density fractions by WMAP and other experiments, and of course any unified theory would have to predict these. But my point is that it would, IMO, be extremely hard to construct an underlying particle physics model from which you could "derive," say, inflation, without knowing, even approximately, what the inflaton mass/potential/etc. even *is*. And the same goes for dark matter and dark energy. What forseeable astrophysical observations will be able to determine their properties even remotely to the extent that a controlled experiment in a laboratory could? That's what I meant by saying that there isn't enough data, and what data there is isn't precise enough.

I agree with you that we don't have even the beginnings of a model that simultaneously predicts these things — I just don't see how we're going to get even to *that* with cosmological measurements alone, let alone to something with the precision of the Standard Model.

9. **Eugene Stefanovich**
July 8, 2006

Krotos:

I disagree. How can a theorist be not excited about the current mess in theoretical physics? Even in the absence of new experimental data, there are so many delicious paradoxes within our theories that are just screaming at us begging for resolution. Take for example two completely different ways the time is treated in quantum mechanics and in relativity, and the whole issue about the incompatibility of QM and GR, or the problem of UV divergences. These are deep problems and they require some bold thinking. I wouldn't complain that theorists have nothing to do until LHC begins to deliver data.

10. **Krotos**
July 8, 2006

Eugene, certainly I agree that these are fascinating problems and deserve a lot more investigation and thinking about fundamental concepts like time and space. But as I said in my response to Who, I just don't see how we'll be able to get beyond the theoretical speculation stage (even with LHC data) on them. Theoretical physics in indeed arguably in even a bigger mess now than it was in 1900. But the difference, IMO, is that in 1900 the technology either existed or was on the horizon to experimentally investigate the problems physics was then facing. The data informed and enriched the theory. For the most part, that's not the case now.

11. **Eugene Stefanovich**
July 8, 2006

Krotos:

Yes, in 1900-1920 when there was an ocean of new controversial experimental data, it was much easier to develop theory. Now we don't have this luxury, and we should draw inspiration form internal theoretical inconsistencies rather than from comparison with experiment. This is much more challenging, no doubt about that. However, I think this is just as exciting now as it was then.

I think we will start to make progress only after we admit that there is something wrong in our textbooks (about special and general relativity, QM, and/or QFT), even though these theories passed numerous experimental tests. I don't think we have this courage. I see that from discussions on Peter's blog: it is next to impossible to convince people to admit mistakes even it the research program that passed zero experimental tests. I am talking about string theory, of course.

12. **Sebastian Thaler**
July 9, 2006

Peter,

Lubos is now calling you a jellyfish on his blog.

13. **nigel cook**
July 9, 2006

Sebastian,

As soon as stringers are defeated scientifically (which is as soon as they make any type of claim to be scientists), they have to either retreat quietly, or else start calling people names which they hope will be taken as insults.

These insults tend to backfire. Anyone can throw that sort of thing around. All it shows is a lack of ability to either succeed with a scientific discussion, or to admit he has no answer, like a responsible adult. His trick in the review of Not Even Wrong, comparing string theory to evolution is extremely misleading.

14. **D R Lunsford**
July 10, 2006

Speaking of quick links,

This article in Time repeats the fantasy that Mileva Maric was key to the development of relativity, specifically that she "helped [Einstein] with the math of his 1905 paper". This claim is absurd – the math is Maxwell's equations and simple algebra, and was common knowledge. In fact Maric failed to gain a teacher's certificate in 1900 because of low math scores, and left ETH in 1901.

How did this revisionist falsehood work its way into the public mind? I see this as passive hostility to Einstein himself (despite the usual fawning), and somehow connected with the current devolvement of science into superstition and mysticism.

See also here:

http://physicsweb.org/articles/world/17/4/2/1

-drl

15. **nonblogger**
July 11, 2006

Another quick link then: next september there will be a conference in Cambridge (UK) on the "fundamental structure of space and time" gathering mathematicians and physicists (Connes, 't Hooft, Schwarz, Penrose, Douglas, Hawking, Freidel, etc.)
http://www.newton.cam.ac.uk/programmes/NCG/ncgw02.html

The aim seems to be to put people from all sorts of backgrounds (strings, LQG, non-commutative geometry,etc.) in the same room. I hope some videos of the talks and panel sessions will be made! (Too bad that the main sponsor seems to be the John Templeton Foundation…)

16. **Alejandro Rivero**
July 11, 2006

Hey, I will attend to the meeting pointed by nonblogger. And I should solve the problem of accommodation for at least a couple months, not only the meeting. Any idea, please mail me (al.rivero # gmail.com)

17. **Chris W.**
July 11, 2006

Regarding the above-mentioned conference, note the participants and theme of the planned panel discussion. The Templeton Foundation's influence seems pretty obvious here.

18. **sunderpeeche**
July 11, 2006

Why don't you fund your own conference, free of Templeton Foundation influence? Recall the song from 1946 about the foundation of Brookhaven National Lab (~ Cold War + Big Science)

—x—x—

– "Take Away Your Billion Dollars," by Arthur Roberts (1946),

Up on the lawns of Washington the physicists assemble
From all the land are men at hand, their wisdom to exchange.
A great man stands to speak, and with applause the rafters tremble.
"My friends," says he, "you all can see that physics now must change.
"Now in our lab we had our plans, but these we'll now expand,
Research right now is useless, we have come to understand.
We now propose constructing at an ancient Army base,
The best electro-nuclear machine in any place.

"Oh – it will cost a billion dollars, ten billion volts 'twill give,
It will take five thousand scholars seven years to make it live.
All the generals approve it, all the money's now at hand,
And to help advance our program, teaching students now we've banned.
And as the halls with cheers resound and praises fill the air,
One single man remain aloof and silent in his chair,
And when the room is quiet and the crowd has ceased to cheer,
He rises up and thunders forth an answer loud and clear,
"It seems that I'm a failure, just a piddling dilettante,
Within six months a mere ten thousand bucks is all I've spent.
With love and string and sealing wax was physics kept alive,
Let not the wealth of Midas hide the goal for which we strive.
Oh — take away your billion dollars, take away your tainted gold,
You can keep your damn ten billion volts, my soul will not be sold.
Take away your army gen'rals, their kiss is death I'm sure.
Ev'rything I build is mine, and ev'ry volt I make is pure.
Take away your integration, let us learn and let us teach,
Oh, beware this epidemic Berkelitis I beseech.
Oh, dammit! Engineering isn't physics, is that plain?
Take, oh take, your billion dollars, let's be physicists again."

19. **Alejandro Rivero**
    July 11, 2006

    I had not seen the Panel, but bet Connes is able to bypass any reverend.

20. **Chris W.**
    July 20, 2006

    More from Seed Magazine: A review of Mark Ronan's *Symmetry and the Monster* and Avner Ash and Robert Gross's *Fearless Symmetry*, by Jordan Ellenberg.

Interviews With Atiyah and Gell-Mann

July 12, 2006
Categories: Uncategorized

A correspondent wrote in to tell me about a wonderful web-site, called [People's Archive](). Their idea is to do in-depth interviews at a peer-to-peer level with the great thinkers and creators of our time. They've been doing this for a few years, only recently providing open access to much of the content on their site.

The two interviews of people closest to my interests are ones of [Sir Michael Atiyah]() and [Murray Gell-Mann](). The interviews are very long, several hours. So far I've made my way through the Atiyah interview (which is in 93 pieces), mostly just reading the transcript, and have poked around a bit in the Gell-Mann interview (which is in 200 pieces).

Atiyah is on just about every mathematician's list as one of the very few greatest figures in the second half of the twentieth century. He's also had a major impact on the relation of mathematics and physics. The interview essentially provides a long memoir of his life, concentrating on his mathematical research work, explaining in detail how it came about and how it evolved. It's truly wonderful, with all sorts of interesting stories, together with insights into mathematics and how it is done at the highest level.

The interview begins with his childhood in Khartoum, then discussing his later education in England, ending up at Cambridge where he was a student of Hodge. One story he tells (segment 21) is about Andre Weil's reaction when Atiyah showed him his work at the time he was a student. The segment is called "how not to encourage somebody." Atiyah also later on talks about his mathematical heroes, especially Hermann Weyl. Physicists often confuse Weil and Weyl, who were two rather different characters. They both did important work on representation theory with Weyl responsible for, among many other things, the representation theory of compact Lie groups, and the exponentiated form of the Heisenberg commutation relations (what mathematicians call the Heisenberg group). Weil was responsible for the geometric construction of representations of compact Lie groups (Borel-Weil theory), and a general theory of representations of Heisenberg-like groups (known as the Segal-Shale-Weil, or metaplectic representation).

Atiyah tells about the importance of his years spent at the IAS in the fifties and the people that he met there. It was one of the great meccas of mathematics at the time. He tells in detail the story of how the index theorem came about (segment 43), and the crucial role provided by the Dirac operator in linking together the analysis and the topology. The Dirac operator was rediscovered by him and Singer during their work. He also explains the important role from the beginning of equivariant versions of the theorem, in providing motivating examples and requiring the most general and deepest sort of proof.

During the 1970s Atiyah started to get deeply involved in interactions with physicists, and he recalls going to MIT to discuss instantons with them, meeting a young Edward Witten in Roman Jackiw's office there (segment 67). He describes in detail his interactions with Witten, especially his prodding of Witten that led to the discovery of the TQFT for Donaldson theory (segment 71), something that took Witten quite a lot of effort before he came up with the necessary twisting of supersymmetry to make this work. He also tells the story of the famous dinner at Annie's in Swansea where, in discussions with Atiyah and Segal, Witten came up with his Chern-Simons theory. The idea was so compellingly correct that Witten decided the next day to not give the talk he had planned, but to talk about this new theory born only the night before.

In his comments on the future (segment 74), Atiyah refers to the new ideas brought into mathematics from QFT as "high energy mathematics", and predicts that mathematics in the future will make crucial use of the sort of "infinities of infinities" that occur in QFT structures, but that mathematicians until recently have had no real idea how to approach. He also makes some interesting comments about what sort of problems it is best for graduate students to work on, and gives (segment 90) a wonderful description of the importance of beauty in mathematics and his own definition of it.

All in all, it makes fantastic reading, I hope the company that put this together will clean up the transcripts and put them out in book form.

I haven't had the time to go through all of the Gell-Mann interview, but it also contains all sorts of valuable history. One little-known fact that Gell-Mann mentions is that the SU(3) eight-fold way that he got the Nobel prize for came about because, after he had spent a long time trying to generalize SU(2)xU(1) unsuccessfully, a mathematics assistant professor (Richard Block) finally explained to him that what he was doing was trying to find a certain kind of Lie algebra, and the one he was looking for was the Lie algebra of SU(3).

Comments

1. **MathPhys**
   July 12, 2006

   Peter,

   You seem to be stuck in the 80's 😉 No physicist confuses Weil and Weyl any more. Seriously, thanks for the valuable link.

2. **Chris W.**
   July 12, 2006

   From Murray Gell-Mann's segment 21, [Viki Weisskopf. Mathematical formulas and physical theories]():

   > What I learnt from Viki was mainly an attitude which I've described in a paper that I presented in his honour called 'The Garden of Live Flowers'. He kept insisting on a pedestrian approach. Now, a pedestrian approach is not my approach. I like clean mathematical results and elegant demonstrations and so on and so on and so on. But nevertheless that was part of Viki's style that I was able to benefit from, and that was a sort of unpretentiousness: the idea that you shouldn't try to construct right away, ab initio, the ultimate theory of things.

   Given how fundamental physics has evolved in the last two decades there is a complicated irony in this reminiscence. By the way, has Gell-Mann had anything to say about string theory in the last few years?

3. **Belizean**
   July 12, 2006

   What a wonderful resource! I got to learn things about my old graduate advisor that I aways wondered about but was too timid to ask. Thanks for sharing!

4. **Paul Frampton**
   July 12, 2006

   I recently found the hours of interview with Murray Gell-Mann
   and watched it all as it is startling. The interviewer
   has Murray give countless insights not only about theory but other theorists. One example: at UCLA 30 years ago it was clear to me something had occurred between him and Schwinger who avoided Murray's ideas, a difficult task. See segment 106.

5. **Carl**
July 13, 2006

I'm a big worshipper of Schwinger, in particular his measurement algebra which dates to the late 50s. In fact, I started a website devoted to this, http://www.MeasurementAlgebra.com. I have to admit I didn't like hearing Gell-Mann accuse Schwinger of making an error in stealing someone else's work, for the sake of elegance.

To be human is to err. My own feelings is that Schwinger was on the right path with the Measurement Algebra, which dates to a few years before the incident discussed, but got off track when he converted it from a density matrix form to a spinor form. A recent argument for the density matrix form is here: (post #3 and replies) http://www.physicsforums.com/showthread.php?t=124904

There is no question that physics at the present time is in a certain amount of trouble. Did we get into trouble by excessive attention to elegance? Or was it failure to attend to the foundations of physics? In this, I think that Schwinger was right. It is the foundations of QM that were shaky.

But the Gell-Mann interview also reminds me of that physics is as much as social science as anything humans do. It is not that "physics" is in trouble, it is that the physicists have made no progress. Does repeating unpleasant truths (or lies) about personages who were slightly off the beaten path 50 years ago help advance physics?

When I get lost while driving, the way I get to where I want to go is to go back along the way I came, and look for a place where a mistake was made. In ignoring the complaints about the foundations of physics from the 1920s through 1950s, the string theorists are making the unwarranted assumption that the foundation they build upon is solid. It is in this that I think that Schwinger's best efforts were made, in pointing out the problems. Rather like Bohm.

Carl

6. **MathPhys**
July 13, 2006

I was quite thrilled to read what Gell-Mann had to say about the Abdus Salam/John Ward collaboration. I've never understood why Salam got the Nobel prize for electroweak interactions without Ward, given that they did the work jointly (the paper often cited as Salam's contribution to electroweak unification is a conference report on his joint work with Ward). I was very happy to read that Gell-Mann is of the same opinion.

Incidentally, does anyone know what became of John Ward?

7. **MathPhys**
July 13, 2006

I now know.

http://www.physicstoday.org/pt/vol-53/iss-10/p99.html

8. **Kyle**
July 13, 2006

> There is no question that physics at the present time is in a certain amount of trouble.

Hmm, I don't see this as true at all. Maybe your area of physics is doing poorly, but I see the discipline overall as doing quite well.

9. **D R Lunsford**
July 13, 2006

This is a fantastic site! I enjoyed Teller talking about his early interest in projective geometry – I remember proving that theorem that he said he was stumped by, algebraically, so that made me feel good! I find I still have a hard time listening to mathematicians run on 🙂 but listening to Teller, Bethe and Dyson is just wonderful. Also it was hilarious to hear Gell-Mann point out that going to MIT doesn't commute with suicide 🙂

-drl

10. **D R Lunsford**
July 13, 2006

This was also hilarious (Teller on Sommerfeld):

"I'm sorry to tell you I did not enjoy Sommerfeld. He was a bit on the high brow side and I can tell you one story about him that might explain my lack of enthusiasm. The story is about a very excellent American student who later got the Nobel Prize, van Vleck, who went as a young student to study in Munich. And there he was in the library, and in comes Sommerfeld. Excuse my German, I think you will understand it. Sommerfeld enters. van Vleck shows up and politely says- Guten Morgen, Herr Sommerfeld. He's rewarded with a none-too-pleasant grunt. Next time it happens Van Vleck says- Guten Morgen, Herr Professor. This time, Sommerfeld smiled a bit. On the following occasion, he said- Guten Morgen, Herr Doktor. Now, this time Sommerfeld said- Guten Morgen. But the last time Sommerfeld came in and van Vleck said- Guten Morgen, Herr Geheimrat. Well, Geheimrat means secret councilor. A very high title in Germany. And Sommerfeld looks at him- Aber Ihr Deutsch ist jeden Tag besser! But your German is improving every day!"

-drl

11. **Chris Oakley**
July 13, 2006

I have not looked at Gell-Mann's argument, but presumably it is on the lines of the following:

A = Going to MIT
B = Committing Suicide

AB = you commit suicide & your body is transported to MIT
BA = you go to MIT and then commit suicide

So $[A,B] \neq 0$

But either way you will end up dead at MIT, so I suppose that

<t2|[A,B]|t1> = 0

if t1 > max(T_MIT, T_suicide) or t2

12. **Chris Oakley**
July 13, 2006

… < min(T_MIT, t_suicide)

(I thought I'd got the hang of HTML tags, but maybe not)

13. **MathPhys**
July 13, 2006

Gell-Mann has such a low opinion of Schwinger.

14. **Tony Smith**
July 13, 2006

The People's Archive interview with Freeman Dyson (number 78) discusses Dyson's work on QED showing the equivalence of the Feynman and Schwinger approaches.
Dyson wanted to show Oppenheimer and the Princeton IAS the QED success.
Oppenheimer's reaction reminds me of the attitude of the superstring establishment.

Here are some excerpts from interview 78 with Dyson:

"… we met Oppenheimer and I wanted to talk about this in the seminar at the Institute … Oppenheimer wasn't enthusiastic at all. It came as a big shock to me that we'd done this wonderful stuff and I desperately wanted to tell Oppenheimer about it, that was the whole point in coming to Princeton. And Oppenheimer just brushed us off and said, "Well, you know, that's not leading anywhere,' …

"… This is of course a common situation; that the people who have failed to clean up a subject then don't believe that it can be cleaned up … And then if somebody comes along and says, "Look, it works," they don't believe.

"So that was how it was, and so we had a very hard time to get Oppenheimer's attention. …
All the old people … including Max Born and Heisenberg and Schroedinger … had radical proposals which turned out to be totally useless …

"Feynman … and … Schwinger .. and … I were conservative in the sense that we … actually made the mathematics work and got the right answers. And that came a surprise to Oppenheimer. It was very hard for him even to listen to it. …

"finally Uhlenbeck interceded with Oppenheimer … "Let's listen to Dyson," and so Oppenheimer put on a seminar series for me …".

The "radical proposals" of Born, Heisenberg, and Schroedinger remind me of superstring theory.

Sadly, it seems that today there is no Uhlenbeck who will listen to alternatives that work.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

15. **nigel**
July 13, 2006

"Incidentally, does anyone know what became of John Ward?" – MathPhys

See http://www.opticsjournal.com/ward.html , but that doesn't mention that John Ward had an argument in the 80s with Prime Minister Margaret Thatcher over Lord Penney and the design of Britain's H-bomb. After he had discovered the Ward identity, he got a job at Aldermaston circa 1955 and was set the task of designing a H-bomb (Britain had already tested the fission bomb).

According to the book 'Britain and the H-bomb' (by the official British nuclear historian Lorna Arnold), she interviewed Ward over the issue and he drew the blueprint for her that he had done for Penney in 1955. Penney chucked out Ward's design and went ahead with his own ideas which did poorly when tested in 1957. Ward quit in anger. However, Arnold says the blueprint Ward suggested was not exactly brilliant either, and he had done no supporting calculations to show it would work.

After the basic Teller-Ulam design was declassified in the 80s, Ward wrote to Britain's Prime Minister Thatcher claiming he had come up with the same design, but it had been suppressed by Penney, who got a lot of credit for ideas which did not always go off with a perfect bang. It created a bit of a scandal.

16. **D R Lunsford**
July 13, 2006

Dyson tells the following anecdote of Oppenheimer, which is really appropriate today:

"And then we met Oppenheimer and I wanted to talk about this in the seminar at the Institute, and somehow or other Oppenheimer wasn't enthusiastic at all. It came as a big shock to me that we'd done this wonderful stuff and I desperately wanted to tell Oppenheimer about it, that was the whole point in coming to Princeton. And Oppenheimer just brushed us off and said, "Well, you know, that's not leading anywhere,' and he had somehow got convinced that you couldn't do physics at all with these old methods. He considered this all old stuff and what physics needed was something radically new. This is of course a common situation; that the people who have failed to clean up a subject then don't believe that it can be cleaned up, so they're looking for something totally different. And then if somebody comes along and says, "Look, it works," they don't believe. So that was how it was, and so we had a very hard time to get Oppenheimer's attention. And I think Niels Bohr had a very bad effect on Oppenheimer too, because I mean Niels Bohr, at that time, was convinced that physics had to be radically different if it was going to work; and Heisenberg, all the old gentlemen of those days, they'd lived through this radical revolution of quantum mechanics which was so successful, they wanted to have something like that again. They thought a new revolution, like 1925, was needed. All the old people tried to do that, including Max Born and Heisenberg and Schrödinger, I mean each of them had radical proposals which turned out to be totally useless, and in the meantime it was the young people who actually were the conservatives; from this point of view even Feynman was a conservative. I mean he went back to the old physics and made it work, and that was what Schwinger did too, and what I was doing. So we were conservative in the sense that we used the old physical concepts of quantum electrodynamics exactly the same as Heisenberg and Pauli in the 1920s, and actually made the mathematics work and got the right answers. And that came a surprise to Oppenheimer. It was very hard for him even to listen to it."

-drl

17. **sunderpeeche**
July 13, 2006

Tony Smith –
"Sadly, it seems that today there is no Uhlenbeck who will listen to alternatives that work."

Where is a Dyson with alternatives that work?

FWIW Uhlenbeck didn't adapt well to the post-WW2 "big science" expt particle physics. He also didn't always listen to the new voices in the field of stat mech.

18. **D R Lunsford**
July 13, 2006

Tony – funny we should latch onto the same quote exactly! The entire Dyson interview reveals a fearless and honest thinker and is just downright inspiring.

-drl

19. **Tony Smith**
July 13, 2006

sunderpeeche said "… Where is a Dyson with alternatives that work? …".

Since that is a direct request for a specific alternative, I guess it is OK for me to say that my model gives reasonably-consistent-with-experiment values for force strengths, particle masses, the Dark Energy : Dark Matter : Ordinary Matter ratio, and more,

despite which I am blacklisted by the Cornell arXiv and either ignored or ridiculed when I have offered to describe my model at seminars (ignored by Perimeter/Smolin and ridiculed by Harvard/Motl).

If sunderpeeche would invite me to give a seminar about my model at a reasonable place and time, I would be happy to do so. (Since sunderpeeche is anonymous to me, maybe sunderpeeche's place is too far for me to go, but I would try within reason.)

Tony Smith
http://www.valdostamuseum.org/hamsmith/

20. **Peter Woit**
July 13, 2006

Anyone who wishes to discuss Tony's work, please contact him privately, this is far off the topic here.

These interviews have a wealth of interesting material, but are very long, so it's time-consuming to read them all. Thanks to all for pointing out those sections that are striking in one way or another.

21. **Paul Valletta**
July 13, 2006

Peter, great links!

The era outlined by Atiyah at "Swansea" was really interesting, specifically as I was there. I do recall a number of Post-Grads were having a lot of problems with the talks, and there was a Grad revolt at hand. It's quite ironic that Witten felt that the re-arranged conference (I believe it was supposed to be held at a higher University rather than Swansea), was failing fast, and out of the dissent, Witten came up with his "revolutionary" insight!

Maybe Witten guessed the road ahead was peppered with "loopholes" and, as he was the conference's major Mathe-magician, produced a String/M-Theory "Miracle" ?

Thanks again for the really interesting thread.

22. **John Baez**
July 14, 2006

Peter writes:

Physicists often confuse Weil and Weyl, who were two rather different characters. They both did important work on representation theory…

Saying that they're "rather different" is an understatement. Let's not let anyone get confused:

Hermann Weyl was a mathematical physicist, and every physicist should know about him:

He gave the first precise definition of a 2-dimensional manifold, in his 1913 book *The Idea of a Riemann Surface*.

He introduced the term "gauge invariance" in his 1918 paper trying to unify general relativity with electromagnetism. A footnote to this paper by Einstein explained why this theory would not work – but later, after changing the group R to U(1), people realized that his treatment of electromagnetism as a gauge theory was very profitable.

His book *Space, Time and Matter* is a profound analysis of the basic concepts of physics. This book is the first place the modern definition of vector space was ever written down! It contains an excellent classification of tensors, tensor densities, and pseudo tensor densities, based on the representation theory of GL(n). If everyone read this, there would be far fewer arguments about various kinds of tensor-like thingies.

Weyl was largely responsible for introducing group representation theory into quantum mechanics, in his book *Group Theory and Quantum Mechanics*.

His book *The Classical Groups* systematized the use of Young diagrams for representations for the groups SU(n), SO(n) and Sp(n) – the special unitary, orthogonal and symplectic groups. He invented the term "symplectic"! (People had been using the term "complex", which is very confusing, so he translated it into Greek.)

His book *Symmetry* is a gem of science popularization that anyone can read – a study of symmetry in mathematics, physics, art, and nature.

He also did a lot of other work on Lie theory. He invented the Weyl character formula in Lie group representation theory. He also invented the Weyl relations: the exponentiated form of Heisenberg's canonical commutation relations, which you need for rigorous work on this subject.

A seriously cool dude!

Andre Weil was a number theorist, a draft dodger who was caught and jailed, and a member of Bourbaki. He shared an interest in philosophy with his more famous sister – Simone Weil.

He carried the analogy between number fields and function fields further than it had ever gone before, revealing that number theory is a branch of geometry. He came up with the Weil conjectures, which eventually led Grothendieck to etale cohomology. And he invented adeles – roughly the number-theoretic analogue of formal power series.

He did a lot more, but I'm getting tired….

23. **John Baez**
July 14, 2006

Peter writes:

> One little-known fact that Gell-Mann mentions is that the SU(3) eight-fold way that he got the Nobel prize for came about because, after he had spent a long time trying to generalize SU(2)xU(1) unsuccessfully, a mathematics assistant professor (Richard Block) finally explained to him that what he was doing was trying to find a certain kind of Lie algebra, and the one he was looking for was the Lie algebra of SU(3).

Richard Block is now a professor emeritus at my school, UC Riverside. He's an expert on Lie algebras over finite fields. He has a great house up in the hills overlooking the campus, and spends a lot of time gardening. He occasionally asks me questions about operads.

Besides pointing Gell-Mann towards Lie theory and SU(3), he was the first to have invented the Virasoro algebra… but not over the complex numbers: over finite fields. Unfortunately, he didn't tell any physicists about this. 🙂

24. **MathPhys**
July 14, 2006

John,
Did he get the central extension right?

25. **D R Lunsford**
July 14, 2006

JB wrote RE Weyl

He introduced the term "gauge invariance" in his 1918 paper trying to unify general relativity with electromagnetism. A footnote to this paper by Einstein explained why this theory would not work – but later, after changing the group R to U(1), people realized that his treatment of electromagnetism as a gauge theory was very profitable.

It's only an accident of 4d that the U(1) theory works – the electromagnetic scalar is already of the correct weight (in the sense of Weyl's geometry) and can live by itself in a gauge invariant Lagrangian, so that the essentially Weylian aspect of the geometry can be ignored and the geometry regarded as an external field living on Riemannian geometry. In other dimensions the volume element needed to construct gauge invariant integrals has weight other than 2, and this self-standing aspect of the electromagnetic scalar is lost.

In the sense of Weyl geometry the ie term standing in front of Am in the covariant derivative should be regarded as a conformal weight itself, that is in Weyl's theory the c.d. is

Dm = dm + N Am

where dm is the Riemannian expression for the c.d. and N is the weight of whatever D is going to operate on. In other words, the conformal weight of spinors has to be imaginary.

-drl

26. **Juan R.**
July 15, 2006

Chris W.,

After years claiming what "was" the correct version of string theory (i do not remember which one, heterotic?) now Gell-Mann is using a generic "action" in his histories formalism claiming that would be obtained from M-theory once known M-theory is 😉

Gell-Mann rudely critized antropic principle in his book the Quark and the Jaguar. I suspect that he maintains his views in the current Landscape trouble and the rest of nonsense.

Gell-Mann, as Glashow, also openly critizes the popular statement that string theory (or M-theory) is a TOE.

Juan R.

Center for CANONICAL |SCIENCE)

27. **John Baez**
July 16, 2006

Mathphys writes:

> Did he get the central extension right?

Yeah, Block got the formula right in the paper he wrote, which he used to keep posted by his office door. If you don't bother with the central extension, you just get the Witt algebra instead of the Virasoro algebra, and that's nothing special.

This abstract of a talk by Murray Bremner says that Block was the first to define the so-called "Virasoro algebra", but in characteristic p, while Gelfand and Fuchs were the first to define it in characteristic 0 (the case physicists care about).

28. **Stefan**
July 18, 2006

Hi Peter,

Just read your post here? Wondering… why is Atiyah so prominent? I know he did some heavy-duty stuff on K-Theory and the like, but what exactly did he do with gauge theories that should make us want to take note and read his works in detail?

By the way, his collected works on Gauge Theories (v. 5 of his Collected Works series) is well over $200; isn't there some alternative way(s) to get hold of his works?

Stefan

29. **Peter Woit**
July 18, 2006

Stefan,

Atiyah has done all sorts of interesting mathematics, but his greatest achievements have to do with the index theorem. The index theorem is a very deep unifying idea about mathematics, and one of the most important ideas of mathematics in the second half of the twentieth century. It links together topology, analysis, geometry and representation theory, all in a new and unexpected way. It's very much worth learning this story, and probably the best place to do it is via Atiyah's papers (both the expository ones and the more technical ones), since he is a masterful writer.

One big surprise was that the mathematics of the standard model is closely related to the mathematics involved in index theory, and Atiyah was deeply involved in working this out. Some of the things related to physics that he worked on from 1975 on include:

1. Construction of instanton solutions (ADHM)

2. Complex twistor-space techniques for understanding instantons.

3. Yang-Mills theory in 2d. The physics is rather trivial, but the mathematics that has come out of this is amazing.

4. Equivariant cohomology techniques for calculating some simple but non-trivial path integrals exactly in terms of fixed point data. Slogan is that in these cases the stationary phase approximation is exact.

5. Understanding of the whole story of anomalies in terms of the index theorem. The crucial idea is that of applying the families index theorem to families of Dirac operators parametrized by background gauge fields.

6. The whole idea of topological quantum field theory. Besides giving an axiomatic version, Atiyah was the first one to suggest that there should be a 4d QFT with Hilbert space Floer homology and observables Donaldson invariants, as well as a 2d QFT with observables Gromov-Witten invariants. Witten later constructed these QFTs, with some prodding from Atiyah.

Anyway, those are some of the high points.

I've heard rumors of a cheap Chinese paperback edition of his collected works. Absent that, unless you're well-off, this is what libraries are for.

This Week’s Hype

July 13, 2006
Categories: This Week's Hype

This week’s string theory hype is embedded in a story by Michael Schirber about the possibility of variation of fundamental constants that has appeared on msnbc.com, foxnews.com, and Slashdot. According to Schirber:

A popular alternative to relativity, which assumes that sub-atomic particles are vibrating strings and that the universe has 10 or more spatial dimensions, actually predicts inconstant constants.

According to this string theory, the extra dimensions are hidden from us, but the “true” constants of nature are defined on all dimensions. Therefore, if the hidden dimensions expand or contract, we will notice this as a variation in our “local” 3D constants.

It’s kind of funny to hear that string theory “predicts” that constants like the fine structure constant will vary in time. When Michael Douglas was here in New York giving a talk last year and was asked about predictions of the string theory landscape, he said that the best one was that the fine structure constant would NOT vary. His argument was that it couldn’t vary since effective field theory arguments would imply a corresponding variation in the vacuum energy, something inconsistent with observation. So string theory both predicts that the fine structure constant will vary, and predicts that the fine structure constant will not vary.

For more string theory hype, Michio Kaku now has a MySpace site, including a blog. He also has his own web-site, mkaku.org, which has recently been redesigned and now prominently features an offer of signed copies of his (softcover) books for $50.

Update: There’s an informed take on what the data about varying fundamental constants actually says from Rob Knop.

Comments

1. **fp**
   July 13, 2006

   Lubos, following Jacques Distler, wrote about the RHIC and that it already tests string theory. His argument is that one should not distinguish between strings used in AdS/CFT and strings used for a TOE, as you did previously.
   What do you think about his arguments or is this just hype again in your opinion?

2. **David**
   July 13, 2006

   Peter,
   Since you opened the subject wrt to Kaku, will it be possible to get an autographed copy of NEW once it goes on sale in the US? I collect copies of autographed science books.
   Best,
   David

3. **woit**
   July 13, 2006

   fp,

   And I was trying to ignore Lubos for a while…

   Chad Orzel on his blog made the perfectly accurate comment that RHIC would not test string theory, whereupon he was assaulted by the string theory hype and attack squad, including the usual Lubos behavior towards anyone who has anything negative to say about string theory.

   The idea that RHIC will “test string theory” is ridiculous hype, and Lubos’s refusal to distinguish between string theory on AdSxS^5 as used in AdS/CFT and string theory on R^4 x Calabi-Yau is just more of his usual nonsense designed to obscure the fact that one of these is a complete failure.

   While someday AdS/CFT ideas may give a real calculational method for QCD, and thus for the quark-gluon plasma, perhaps giving some real predictions about RHIC data, the subject is still very far from that point. The main problem is that AdS/CFT doesn’t apply to QCD, but to supersymmetric versions of the theory, and these have different degrees of freedom. Some people hope that even though AdS/CFT gets the spectrum wrong, and has the wrong degrees of freedom, for some aspects of high temperature QCD, there will be some sort of “universal” behavior and the fact that one isn’t dealing with the real QCD won’t matter. While someday this may get somewhere, at the moment as far as I can tell the subject is being seriously overhyped. If someone can point me to an unambiguous AdS/CFT calculation that makes a sharp, testable prediction about RHIC data, I’d be interested to hear about this.

4. **woit**
   July 13, 2006

   David,

   I won’t be selling autographed copies on the web-site, and have no idea how the publisher handles this (I’ve heard frightening rumors of authors locked in rooms with huge stacks of books and told they can’t come out until they’re all signed). On general principles I’ll be happy to sign people’s books though, without charge.

5. **Jeremy**
   July 13, 2006

   Oh Michio. I can’t believe you have a MySpace page.

6. **Steven H. Cullinane**
   July 13, 2006

   On Kaku’s MySpace weblog:

[Carpe Diem](#).

7. **fp**
July 13, 2006

> If someone can point me to an unambiguous AdS/CFT calculation that makes a sharp, testable prediction about RHIC data

What about the 'ideal liquid' (low viscosity) results referenced by Clifford?

8. **Peter Woit**
July 13, 2006

fp,

I've spent some time looking at the papers referenced by Clifford and others, without finding anything that looked like a sharp prediction that could be compared to RHIC data. I didn't look at all of this literature, and it's certainly possible I was missing something. If someone can point to a specific place in a specific paper that makes a sharp prediction, and to corresponding RHIC data, I'd be interested to know about this.

9. **Ryan**
July 13, 2006

Let us assign the abbreviation P to the statement "the fine structure constant will vary." Since string theory predicts both P and ~P, string theory therefore also predicts Q, where Q is any statement we want. String theory truly predicts everything… remarkable!

10. **anon.**
July 13, 2006

Peter wrote:

Some people hope that even though AdS/CFT gets the spectrum wrong, and has the wrong degrees of freedom, for some aspects of high temperature QCD, there will be some sort of "universal" behavior and the fact that one isn't dealing with the real QCD won't matter. … If someone can point me to an unambiguous AdS/CFT calculation that makes a sharp, testable prediction about RHIC data, I'd be interested to hear about this.

It's not a *hope*, it's an argument: QCD at high energies and temperatures is approximately conformal, and universal properties of AdS backgrounds should capture relevant information. See hep-th/0405231 for the calculation of viscosity by Kovtun, Son, and Starinets, and hep-ph/0312227 for discussion by Shuryak of the RHIC results and how they show surprisingly low viscosity. You might also want to look at http://www.admin.ias.edu/pitp/2005files/Lecture%20notes/Klebanov-strong.ppt, a lecture by Klebanov that can point to more literature.

Are there string theorists doing this work because they like the opportunity to connect to an experiment, which they otherwise wouldn't care about? Probably. But there are also real QCD experts who take this stuff seriously, Shuryak being one example.

11. **Peter Woit**
July 13, 2006

anon,

Thanks for the references. I'm not claiming there is nothing interesting going on here, just pointing out that the "RHIC tests string theory" claim seems to be overhyped. I understand the argument about approximate conformality, but is there also an argument about why the difference in degrees of freedom doesn't matter? Or estimates of what the deviation from this "universal" behavior will be for the real theory? That this difference doesn't matter, and that these deviations are small is what I was referring to as a "hope". Is there actually more of an argument?

From the references, it looks like AdS/CFT gives a conjectured lower bound on the viscosity, and the claim is that RHIC data can be interpreted as showing that this viscosity is within a factor of two of the lower bound. Is this right, and is this the closest to a confrontation with experiment that people have managed, or is there something better?

12. **Mike**
July 13, 2006

There is more and more hilarious stuff going on in physics, not just the string theory stuff. I was reading a book last night debunking "Intelligent Design". Maybe not such a bad thing to do — but Lee Smolin (whom I generally respect) had an article in this anthology about his ideas about evolution of the multiverse. Black holes "bouncing" into fantastic numbers of new universes, which reproduce themselves somehow with slight "genetic" variations, so the universes most fit for living systems end up reproducing and predominating. He even claims, sort of, that there is experimental evidence for this (other than the fact that we're here)! He uses the recent great advances supposedly made in quantum gravity in dealing with singularities. I guess this is the same theory that so far, to my knowledge, admittedly sketchy, can't give us a semiclassical Minkowski space! All of this to explain the embarrassment of the fact that we are here in a determinedly atheistic way.

It's all very interesting, and clever, and lots of fun, but utterly fantastical. By comparison, my nutty Bible-thumping ID-believing fellow-townsmen may not be as smart, assuredly not when it comes to science, but they sure are models of sobriety, and good sense!

13. **Joe Zhou**
July 13, 2006

Scientific American August has an article on Alain Connes and his "alternative" to string theory, how his (and Rovelli's) theory makes predictions at accessible energies whereas string theory does not.

14. **woit**
July 14, 2006

Joe,

I'll look for the article, haven't seen it yet. Presumably Connes is talking about his prediction of the Higgs mass. I've never found his version of the SM convincing, but at least he's sticking his neck out and making a prediction. If it turns out to be right, people will pay a lot more attention to his model. If it doesn't, he's in trouble.

I didn't know there was a joint Connes/Rovelli model, maybe this is a reference to purported LQG predictions?

Without seeing the article, I don't know how much hype it contains. I hope people working on alternatives to string theory will resist following the string theorists and overhyping their results.

15. **Joe Zhou**
July 14, 2006

Sorry for the confusion — the Higgs mass calculation does not involve Rovelli; his cooperation with Connes concerns quantum gravity and the emergence of time.

16. **Jean-Paul**
July 14, 2006

To anon.,
First of all, there is no evidence at all that RHIC produced quark-gluon plasma — you seem to be informed enough to read more than overhyped public announcements intended to keep it running (it's a historic pattern that amazing discoveries are claimed by experimentalists just before shut-downs). So read this abstract from (nucl-ex/0501009):
"However, the measurements themselves do not yet establish unequivocal evidence for a transition to this new form of matter. The theoretical treatment of the collision evolution, despite impressive successes, invokes a suite of distinct models, degrees of freedom and assumptions of as yet unknown quantitative consequence." What about this "suite of distinct models, degrees of freedom and assumptions"?
If RHIC really found QGP, it would not be canceled, believe me.

Concerning theory, do you claim that High T behavior of QCD is
the same as of Supersymmetric QCD? Ads/CFT and holography are based on symmetries following from supersymmetry. I don't think that you can make any statement about NON-BPS dynamics of QCD by using Ads/CFT, and I am not the only one.

So both experiments and theory are shaky. Just forget it…

17. **Jean-Paul**
July 14, 2006

To Peter:
I agree with (some of) your criticisms of ST. However, I am completely puzzled by your neutral position wrt to AdS/CFT. AdS/CFT has always been advertised (and you buy it) as an important step towards solving QCD, a problem that many theorists consider more important than quantizing gravity. A whole generation spent their most productive years working on it. After 9 years, it is clear that it is useful indeed for discussing unrealistic models with extended supersymmetry, but it has nothing to do with the real non-BPS dynamics of QCD. It's hard to believe that many theorists still consider AdS/CFT as something more than a mathematical curiosity proving once again that extended
SUSY theories have dynamamics completely determined by symmetry principles. After 9 years, don't you think that it's time to pull the plug?

18. **nitin**
July 14, 2006

Joe Zhou:

The August SA issue is not out yet (neither does anything about it, or more specifically the purported Connes-and-his-"string theory"-alternative, figure on the SA website). Please check your source, and if you can, provide a link, because I am interested to read about that "news" of yours.

19. **ObsessiveMathsFreak**
July 14, 2006

For more string theory hype, Michio Kaku now has a MySpace site, including a blog.

I think this can definitely be put down as a watershed moment.

20. **Kasper Olsen**
July 14, 2006

Dear Peter,

-Saying that "string theory both predicts that the fine structure constant will vary, and predicts that the fine structure constant will not vary" is wrong. Simply because Douglas is not identical to string theory (not even as string theory on AdS is identical to a YM theory…).

-And as Rob points out, the data cannot be used to conclude that the fundamental constants vary.

-Concerning the Quark Gluon Plasma, the RHIC fireball can be described as a gravity dual black hole. And then, since properties of black holes can be computed in string theory (in contrast to LQG for example), the shear viscosity divided by the entropy, can be predicted to be \eta/s = 1/(4\pi) = 0.08.

Regards, Kasper

21. **Ponderer of Things**
July 14, 2006

Someone might have commented on this already:

Nova PBS podcasts has two short (4min and 2 min) segments on string theory:
http://www.pbs.org/wgbh/nova/rss/nova-podcast-pb.xml

In the past they would simply interview Brian Greene – their website looks like a promotional vehicle for selling "Elegant Universe":

http://www.pbs.org/wgbh/nova/elegant/

Recent podcast, however, includes comments from Neil deGrasse Tyson, a director of Hayden planetarium, where he somewhat angrily comments on how string theorists always say that the experiment is just behind the corner – and so they were promising something concrete in 2-3 years for the past 20-30 years. Brian Greene responds.

They will have Glashow in one of the next podcasts – should be interesting….

22. **Peter Woit**
July 14, 2006

Kasper,

My point was just that the people hyping string theory are getting their story confused, with some saying it predicts one thing, some the exact opposite. My position on this has always been the same. String theory predicts nothing, nada, zip. Both Douglas's claim and the story in question are worthless hype.

As for the QGP, the problem is that the prediction you mention is for N=4 SYM, not QCD, and my question is how much of an argument is there that these will be the same. The second part of the question is whether there is a believable measurement of this parameter, and how it compares to the "prediction". From Klebanov's vague summary, I gather it looks different by a factor of two, but I'd be curious to hear from someone who knows more about this.

Jean-Paul,

AdS/CFT is certainly over-hyped, but not on the scale of string theory unification, which has conclusively failed after more than 20 years of hype, that continues to this day. It would be a very valuable thing for someone to write up a critical review of exactly what AdS/CFT has accomplished, and what it hasn't. This would be a very demanding task, since the literature is incredibly voluminous, and rife with mixing of what is a solid result and what is wishful thinking.

You can make a viable case that thinking about AdS/CFT and how to extend it to QCD may lead somewhere (although quite possibly understanding QCD requires completely different methods). This is different than studying complicated string theory "backgrounds", which is clearly a doomed enterprise, sure to lead nowhere.

23. **Kasper Olsen**
July 14, 2006

Dear Peter,

The same thing could be said about lattice calculations. QCD "is" not lattice-QCD, but the latter is presumably a good approximation to the former. Likewise, N=4 SYM can – in certain respects – be a good approximation to QCD.

But of course, we would prefer to do calculations in a non-supersymmetric background. One step at a time….

24. **Peter Woit**
July 14, 2006

Kasper,

You are writing nonsense. Lattice QCD IS QCD. To the extent it's an approximation, it's because it's a cut-off version and you have to take a limit, but this is true for just about any interesting QFT. N=4 SYM is a completely different theory than QCD, it has very different degrees of freedom and physical behavior. There's no parameter in the the theory that one can take to a particular value and get QCD.

25. **Kasper Olsen**
July 14, 2006

Peter, the "is" above refers to the fact that lattice-QCD is (among other things) a cut-off version of QCD on R^4. And as such an approximation. (Another reason being that most lattice calculations have not included quarks).

As an example, recent lattice calculations give a strange quark mass around 85 MeV as compared to the 150 MeV from phenomenology.

Are you claiming, that taking the continuum limit is trivial? Are you claiming that there are no finite-lattice spacing effects?

26. **Peter Woit**
July 14, 2006

Kasper,

To define a QFT like QCD, you have to do it by defining it with a cut-off, then taking a limit. That's the way an asymptotically free QFT works. Exactly the same thing is true in perturbation theory, or any other way one knows of for defining such a QFT. The cut-off is not an approximation, it's an intrinsic part of the definition of the theory.

Of course actually doing calculations is non-trivial, and you have to take the lattice spacing to zero carefully, understanding what errors are caused by stopping at a fixed lattice size.

Chiral fermions are tricky, but QCD is not a chiral theory. There are several ways of putting in fermions, this makes numerical calculations more difficult, but many groups are doing such calculations, and getting results consistent with experiment. I've never heard before the claim you're making that lattice calculations give results off by nearly a factor of two, and don't believe it. Please provide a reference.

27. **Xerxes**
July 14, 2006

According to the PDG, m_s falls between 80 and 130 MeV. If you take the lowest lattice point and the highest phenomenological point, you do get numbers like the ones suggested above. This says two things: (1) the strange quark mass is not very well known, (2) it's easy to lie by cherry-picking data.

28. **Gilbert Awad**
July 14, 2006

Hello prof. Woit,

Kasper is probably referring to work done by Akira Ukawa, published in late 2004. I remembered running into something like this a few months ago. The relevant quote would be at the end of section four (page 14 of the pdf file accessible from the URL below).

http://www.aapps.org/archive/bulletin/vol14/14_6/14_6_P11-15.pdf

And congratulations on the book. Can't wait to get a copy of the NA edition. The dust jacket for that edition is particularly elegant, especially with the "mirror image" WRONG in the title.

29. **Kasper Olsen**
July 14, 2006

Gilbert, correct – I didn't have the PDG at hand and remembered reading such numbers in the review.

I guess, even though you are doing theory, you should still carry it in your pocket. It's all so good for a late-night conversation at a local bar 😉

Cheers, Kasper

30. **Brett**
July 14, 2006

I wanted to mention a few points about lattice QCD, since there seems to be a bit of confusion about it. I am not a computational lattice person myself, but I have a number of friends who are, and I follow developments in the subject.

First, the quenched approximation (in which the dynamical quarks' contributions to the vacuum polarization are neglected) is pretty much gone. There are now many, many gauge configurations written to disk with 2 or 2+1 fully dynamical quark species, and groups around the world are making use of them. They have been used to make some remarkably precise calculations, but their general applicability is occasionally controversial, because of the second point.

The second point is that the Lagrangian used in lattice QCD is never just the QCG Lagrangian with a cutoff. That could come close to being useful if the lattice spacing was really small–much smaller than any other scale that might arise in a calculation. In order to get useful data at physically realizable lattice spacings (and one must actually use several lattice spacings and extrapolate to zero–this it how the continuim limit is taked), the actions that are used contain all sorts of extra terms and corrections. Generally, these additions are expected to be irrelevant, so that the theory approaches true QCD as the lattice spacing goes to zero, but that has not always been proved, and sometimes the changes to the action are downright controversial. The biggest problem comes from fermion doubling, which is related to the fact that the use of lattice necessarily organizes the momenta into Brillouin zones, and it's not always clear that the extra fermion "tastes" can be eliminated in a local fashion.

Finally, there are lots of things that lattice QCD cannot calculate usefully at all. I'm not sure what it would mean to "calculate" the strange quark mass, since this is usually an input parameter. (One might fix the up and down quark masses to 1/20-th of the strange quark mass, which is certainly an approximation, but it can be a highly useful one. Of couse, there are complications, since one can distinguish sea quark and valance quark masses, and the whole procedure is entangled with the extrapolation, but the basic idea is right.) A better example of what we cannot calculate on a lattice would be hadron-hadron scattering. The lattices in use today are simply too small to contain external scattering states. So lattice people are very careful to choose things (e.g. single-hadron properties, like masses and decay constants) that can be reliably calculated, although there are still sometimes problems with error estimations.

31. **Andy**
July 14, 2006

Chad has (I think) now retracted his statement that "RHIC does not test ST", and Clifford has thanked him for the retraction. Interesting.

32. **Peter Woit**
July 14, 2006

Gilbert,

Thanks for the reference. It seems to me that it shows that all calculations of observable masses are coming out right within expected errors. The strange quark mass is not something observable, so the discrepancy mentioned is not obviously significant unless you look into this more closely and make sure that you're comparing the same thing on both sides, and understand what the errors are on both sides.

Andy,

To be fair, the string theory attack squad was mainly complaining about Chad's remark that they weren't interested in RHIC and the data coming out of it. You could debate this point, but it wouldn't be useful. They do seem to have done a good job of beating Chad into submission in this case.

33. **Ponderer of Things**
July 14, 2006

Nova PBS series offers short podcasts featuring Brian Greene and Neil deGrasse Tyson, director of Hayden planetarium. Tyson mentions that string theorists tend to promise experimental confirmation that is "just around the corner" – maybe 2-3 years away, but that they have also been doing this for 20-30 years. Greene counters with a claim that he's never heard anyone say that, and perhaps string theorists were just joking.

It's a little surprising to hear Nova offer counter-argument, especially since their website looks like a vehicle for selling "Elegant Universe".

Coming up is the interview with Sheldon Glashow.

34. **Juan R.**
July 15, 2006

More funny is

"*A popular alternative to relativity, […]* "

Juan R.

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35. **QWERTY**
July 15, 2006

DEAR PETTER,

MICHIO KAKOS BLOG HAS AN ARTICLE ABOUT TESTING OF STRING THEORY USING GRAVITY WAVES AT LISA, DEVIATION OF NEWTON LAW – BUT ARE BOTH THESE NOT CLASSIKAL AFFECTS? IE, BY VARYING CLASSIKAL THEORY WE CAN GET THESE ONES. THERE IS NOTHING THAT STRING THEORY SAYS THAT IS NOT ALREADY SAID. THIS SEEMS TO BE TYPIKAL BEHAVIOR.

ALSO PLEASE TO BE COMMENTING ON THE ARTICLE IN THE AUGUST AMS NOTICES

http://www.ams.org/notices/200607/fea-marateck.pdf
ABOUT DIFFERENTIAL GEOMETRY AND FEYNMANN INTEGRALS. I HATE FEYNMANN INTEGRALS – WHY DO NOT THEY CONVERGE!!

PS I KNOW YOU ARE UPSET BECAUSE I USE ALL BIG LETTERS BUT I MUST FOR TWO REASONS. ONE, IT IS A PART OF MY PERSONALITY AND MAKES ME QUIRKEY AND MISTERIOUS AND ENIGMATIC. ALSO MY KEYBOARD IS BORKEN.

PPS ISRAEL GELFAND FOR ABEL PRIZE 2007!

36. **Peter Woit**
July 15, 2006

QWERTY,

Re Kaku and his "predictions", see

http://www.math.columbia.edu/~woit/wordpress/?p=219

The idea that LISA is going to test string theory is utter nonsense.

The Marateck article is a basic expository article on gauge fields for mathematicians. At the end it has some material about Feynman diagrams, but doesn't really explain anything much about them, or how they are related to the earlier material on gauge fields.

37. **Chris Oakley**
July 15, 2006

I see that Feynman is the 37 cent stamp. Personally, I would have preferred it if they had made him a 137.036 cent stamp. OK – it would make the arithmetic more complicated, but we could deal with that, couldn't we?

38. **D R Lunsford**
July 16, 2006

The Marateck article is just hot air, the entire thing is filled with factual errors of history, and is presented on the level of an ambitious and green undergraduate. I wish people would refrain from abusing Weyl and Dirac by refusing to comprehend them.

-drl

39. **Chris Oakley**
July 16, 2006

I could write an article on the lines of the Marateck one, and probably more interesting – i.e. not just cheerleading. Funny that I never get asked to do this.

40. **Christine Dantas**
July 16, 2006

Peter wrote:

The idea that LISA is going to test string theory is utter nonsense.

Last month I have collected some references related to this matter. I'm too busy to read them, but of course I am curious on them. And much more now, considering Peter's statement. Here is my selection:

* An interferometric gravitational wave detector as a quantum-gravity apparatus [gr-qc/9808029]
* Accessibility of the Pre-Big-Bang Models to LIGO [astro-ph/0510341]
* Observational test of holographic inflation [PRD, vol. 73, Issue 2, id. 023516]
* Gravity-wave detectors as probes of extra dimensions [astro-ph/0505277]
* The Primordial Gravitational Wave Background in String Cosmology [hep-th/9907185]

I do not know whether Peter Woit wants people to discuss them here, so if that is the case, you are invited to contribute here.

Thank you,
Christine

41. **MathPhys**
July 16, 2006

I just read that Michio Kaku is a comedian. I should have known that.

42. **D R Lunsford**
July 16, 2006

So Kaku goes to Dr. Woit and says "Doc, my string theory isn't doing so well" and Woit says, "Yeah, it's a very sick theory" so Kaku says "can I get a second opinion?", and Woit says, "Yeah, it's ugly, too!"

-drl

43. **MathPhys**
July 17, 2006

How many string theorists it takes to change a light bulb?

44. **MathPhys**
July 17, 2006

Two: One to hold the blub, and a second to twist it.

45. **John Baez**
July 17, 2006

QWERTY writes:

I HATE FEYNMANN INTEGRALS – WHY DO NOT THEY CONVERGE!!

Be careful – don't forget item 8 of the crackpot index!

PS I KNOW YOU ARE UPSET BECAUSE I USE ALL BIG LETTERS BUT I MUST FOR TWO REASONS. ONE, IT IS A PART OF MY PERSONALITY AND MAKES ME QUIRKEY AND MISTERIOUS AND ENIGMATIC.

No, it just makes you annoying and boosts your score on item 7 of the crackpot index.

ALSO MY KEYBOARD IS BORKEN.

Hmm, well, maybe not.

46. **Chris Oakley**
July 17, 2006

Q: How many String Theorists does it take to change a light bulb?

A: What are you talking about, you half-wit moron! This room isn't dark because we have already changed the light bulb, and done a lot more besides! Unfortunately, being a crackpot fool with the intelligence of an amoeba, you don't realise it!

47. **Chris Oakley**
July 17, 2006

QWERTY,

Inspired by your fine example, I am now going to call my keyboard "DRELL".

Re: your crackpot credentials – John Baez makes some valid points, but if there is any doubt, I am prepared to give you the benefit of it as I too don't like non-convergent Feynman integrals.

48. **King Ray**
July 17, 2006

Seems like string theory has a high crackpot index…

49. **amazed**
July 17, 2006

Looks like we have a real high brow discussion going here of late. Perhaps we could get back to discussing approximations to QCD that were brought up earlier and Brett had a nice post about filling in some of the details. Does the statement that "lattice QCD IS QCD" hold water considering many of the things it does not take into account, and only certain regimes the approximations in lattice QCD hold? For instance what defines the utility or goodness of an approximation since as an example N=4 SUSY YM is more useful to calculating in QCD for the LHC than the lattice albeit our world is certainly not that full of supersymmetries at TeV energies. Of course we have to have some further discussion about N=4 applications in QCD such as what dixon et al do beyond the normal banter about AdS/CFT and whether it is or is not testing QG. Perhaps Peter could make a general post about QCD and various techniques in understanding QCD at a deeper level. Granted this would take us out of the whole joke fest we have going here but it would illustrate an important point many lay people don't understand. Different theories are useful for different energy regimes as well as different theories have sectors that can be used to calculate quanties better than in the original theory that is the "true" theory. Anyways if this isn't interesting to the rest of the commenters feel free to go back to the usual.

50. **Peter Woit**
July 17, 2006

amazed,

There were quite a few postings and discussions here in the past about twistor methods for doing perturbative QCD calculations, if there is anything new about this subject (and not just hype about N=4 SYM), I'd like to hear about it.

The problem with writing about QCD is that there seems to be relatively little new to say about it. I try to mostly write about news here, I don't really have the time or energy to do more in the way of making this an educational or expository site. But if there is something new about QCD, I'd be happy to learn more about it and maybe write about it here.

51. **Jean-Paul**
July 17, 2006

Here is a recent assessment of twistor techniques by Keith Ellis,
in a closing talk of QCD Moriond 2006 (hep-ph/0607038, page 9):
"So far the impact on real phenomenology is rather limited…"
He praises, however, "great intellectual excitment and an injection of personnel from formal areas".
What a nice compliment for the new QCD personnel!
By the way, twistor techniques should not be confused with string inspired/helicity/SUSY techniques used by Dixon et al very successfully over the last 15 years.

52. **QWERTY**
July 17, 2006

DEAR PROFESOR BAEZ

I AM NOT A CRACKPOT!! I AM A BIG ADMIRRER OF THIS WEEKS FINDS, IT IS VERY USEFUL. I AM VERY SAD THAT MY HEROE HAS CALLED ME CRACKPOT.

I MUST ADMIT THAT I AM MORE INTERESTING IN MATHEMATICS THAN IN PHYSICS BUT I FIND MATHEMATIC-PHYSIC BORDER IS MOST INTERESTING. I WILL WRITE ABOUT WHAT I WANT TO BE LEARNING SO YOU CAN SEE I AM NOT CRACKPOT AND I WILL BE PROOVED INNOCENT. SORRY PETTER FOR NOT WRITING ON THE TOPIC I WILL NOT DO IT AGAIN.

PLEASE TO BE WRITING IN THIS WEEKS FINDS ABOUT TWISTORS. THEY SEEM QUITE INTERESTING TO EVERYONE, CLASICAL PEOPLE AND QUANTUM PEOPLE AND EVEN THE STRING THEORY HERATICS. I UNDERSTAND THE IDEA OF WHAT IS A TWISTOR FOR MINKOWSKI BUT DO NOT UNDERSTAND HOW THEY CAN BE DEFORM IN GENERAL WHEN THE WEIL TENSOR IS DUAL TO ITS OWN SELF — I CANNOT UNDERSTAND KODAIRA DEFORMATION THEOREM AT ALL, VERY DIFFICULT! FROM A PHYSIC PERSPECTIVE WHY IS THE LIGHT RAY AND THE LIGHT CONE MORE IMPORTANT THAN THE POINT, THIS SEEMS A LITTLE VERY SILLY!! ALSO HOW DO MONOPOLES BECOME? THERE ARE MANY PAPERS ABOUT MONOPOLES AND TWISTORS. I DO NOT UNDERSTAND ANY LINK BUT THIS DOES NOT MEAN I AM A CRACKPOT, ONLY THAT I AM STUPID.

53. **Matthew**
July 18, 2006

Does the statement that "lattice QCD IS QCD" hold water considering many of the things it does not take into account,

Lattice QCD with 5 flavours of domain wall Fermions (for example) *is* QCD, in the continuum limit. Alas, we lack a computer to run such a beast.

Likewise, lattice QCD with 3 light flavours of staggered fermions, and 2 heavy flavours quenched is as close to QCD as we need to get.

In both of these cases there is nothing that isn't taken into account, though with the staggered fermions there is an ongoing theoretical issue.

and only certain regimes the approximations in lattice QCD hold?

Huh? The only "regime" that I know about where lattice QCD simply cannot be done is with non-zero chemical potential. And even that is basically a computational issue.

The problems with Lattice QCD these days are computation related. That is, we simply do not have the resources to run a realistic simulation (i.e. the first scenario I mentioned above). However, the staggered fermion approach, with various approximations for the heavy quarks has yielded real results, of genuine phenomenolgical interest. For a few recent results see hep-lat/0607011.

For instance what defines the utility or goodness of an approximation since as an example N=4 SUSY YM is more useful to calculating in QCD for the LHC than the lattice

Lattice people are busy computing things to compare to real experiments going on right now (cleo-c, belle, babar, D0, CDF, RHIC, etc.). The LHC is *well* into the perturbative QCD regime in any case. There are lattice calculations that could be done, however they're very complicatied (hadronization) and we probably lack the comptational resources.

54. **Juan R.**
July 18, 2006

MathPhys Said

"How many string theorists it takes to change a light bulb?"

The whole community (10^3)? First they may find the good light bulb and Landscape is large enough.

Juan R.

Center for CANONICAL |SCIENCE)

55. **D R Lunsford**
July 18, 2006

Chris O,

In your humorous honor I christen my HID apparatus AKHIEZER and BERESTETSKII.

-drl

56. **D R Lunsford**
July 18, 2006

Matthew,

Would it be practical to do something like "SETI At Home" with these calculations? I'm amazed that there are still computing problems in physics that can't be solved with a 1000-node Beowulf.

-drl

57. **Chris Oakley**
July 18, 2006

Danny – fine. The keyboard I am using at the moment (on a library computer in Oxford) is a bit old and disagreeable, so I am going to call it "PAULI".

Q: How many (less obnoxious) String Theorists does it take to change a light bulb?

A: Change the light bulb? Why? Playing games in the dark is so much better! You would be *amazed* at how brilliant some of the games we have devised are!

58. **Matthew**

July 18, 2006

drl,

Would it be practical to do something like "SETI At Home" with these calculations?

Probably not. Current computational bottlenecks are the speed of communication between nodes in a cluster, not the raw horsepower of the machine.

I'm amazed that there are still computing problems in physics that can't be solved with a 1000-node Beowulf.

There are many (many many many) problems of interest in computational physics for which a 1000 node Beowulf is nothing. Realistic QCD (i.e. with physically accurate light quark masses) is not feasible on any current computer, full stop. To solve complicated multiparticle states (say, to compute the nucleon-nucleon force from a lattice calculation) would require petaflop (or beyond) scale computers.

One trouble is the fermion update aglorithm critically slows down as you approach light quark masses. So lighter masses means massively more computer power is needed. Of course, we've got chiral perturbation theory, so you don't really need to go to the physical point. But even getting to within the validity of chiral PT is a challenge for modern supercomputers.

59. **CapitalistImperialistPig**
July 18, 2006

The article on Connes is a two page "Insight" piece on page 36 of the August issue, written by Alexander Hellemans. I learned from the article that renormalization was introduced by (among others) 't Hooft and Veltman, and that Connes has linked its mathematical justification to one of Hilbert's (solved) problems, and that the solution involved non-commutative geometry which "serves as a starting point to unify relativity and quantum mechanics." HTH you as much as it did me!

I also learned that Connes is the kind of guy who can walk through a riot without losing the thread of his mathematical conversation.

60. **urs**
July 20, 2006

> renormalization […] and that the solution involved non-commutative geometry

I am guessing that this refers to the Connes-Kreimer work.

61. **Juan R.**
July 22, 2006

D R Lunsford Says and Matthew,

Chemistry and biology are another two fields with entire problems without computational solving. Processors' flops are not the only problem, memory can be more a handicap. Usual chemical computations (TIE) needing of 2GB temporary files are hard to efficient managing by a supercomputer. More advanced methods (TDL) applied to basic chemical systems of interest need of 7 GB for storing equation, many more for solving it.

Juan R.

Center for CANONICAL |SCIENCE)

62. **Riofrio**
July 27, 2006

Hello, all. Did this start as an entry on changing constants? What does it mean if there is evidence from 2 independent methods that c is changing in exactly the amounts predicted?

Equivariant Cohomology

July 18, 2006
Categories: Uncategorized

The International Congress of Mathematicians will be taking place in Madrid relatively soon, in late August. One tradition at this conference is the announcement of the Fields Medals, and I'm getting embarassed that I'm not hearing any authoritative rumors about this (other than about Tao and Perelman); if you have any, please send them my way. One other tradition is to have speakers write up their talks in advance, with the proceedings available at the time of the conference, so already some write-ups of the talks to be given there have started appearing on the arXiv.

Last night, Michele Vergne's contribution to the proceedings appeared, with the title Applications of Equivariant Cohomology. On her web-site she has a document she calls an exegesis of her scientific work, this gives some context for the equivariant cohomology paper. She also is co-author of a book called Heat Kernels and Dirac Operators, which has a lot more detail on some aspects of this subject. Finally, there has been a lot of nice recent work in this area by Paul-Emile Paradan.

Equivariant cohomology comes into play when one has a space with a group acting on it, and it mixes aspects of group (or Lie algebra) cohomology and the cohomology of topological spaces. There are various ways of defining it, the definition that Vergne works with is a bit more general than the one more commonly used. It involves both differential forms on the space, and generalized functions on the Lie algebra of the group.

The beauty of equivariant cohomology is that it often computes something more interesting than standard cohomology, and you can often do computations simply, since the results just depend on what is happening at the fixed points of the group action. There's a similar story in K-theory: when you have a group action on a space, equivariant K-groups can be defined, with representatives given by equivariant vector bundles. Integration in K-theory corresponds to taking the index of the Dirac operator, and in the equivariant case this index is not just an integer, but a representation of the group. The index formula relates cohomology and K-theory, and one of Vergne's main techniques is to work with the equivariant version of this formula.

In the case of a compact space with action of a compact group, there's a localization formula that tells you how to integrate representatives of equivariant cohomology classes in terms of fixed point data. In many cases, this leads to a simple calculation, one famous example is the Weyl character formula, which can be gotten this way. New phenomena occur when the group action is free, and thus without fixed points. This was first investigated by Atiyah (see Lecture Notes in Math, volume 401), who found that he had to generalize the index theorem to deal with not just elliptic operators, but "transversally elliptic" ones. Such operators are not elliptic in the directions of orbits of the group action, but behavior of the index is governed by representation theory in those directions.

Vergne has been studying examples of this kind of situation, and it is here that generalized functions on the Lie algebra come into play. Integrating the kind of interesting equivariant cohomology classes that occur in the transversally elliptic index theory case over a space gives not functions but generalized functions on the Lie algebra. There's a localization formula in this case due to Witten, who found it and applied it to 2d gauge theory in his wonderful 1992 paper Two Dimensional Gauge Theories Revisited.

This kind of mathematics, growing out of the equivariant index theorem, is strikingly deep and beautiful. It has found many applications in physics, from the ones in 2d gauge theory pioneered by Witten, to more recent calculations of Gromov-Witten invariants. It leads to a mathematically rigorous derivation of some of the implications of mirror symmetry in special cases, and a wide variety of other results related to topological strings. My suspicion is that it ultimately will be used to get new insight into the path integrals of gauge theory, not just in 2 dimensions but in 3 or 4.

Update: Vergne has another nice new paper on the arXiv. It's some informal notes on the Langlands program which she describes as follows:

These notes are very informal notes on the Langlands program. I had some pleasure in daring to ask colleagues to explain to me the importance of some of the recent results on Langlands program, so I thought I will record (to the best of my understanding) these conversations, and then share them with other mathematicians. These notes are intended for non specialists. Myself, I am not a specialist on this particular theme. I tried to give motivations and a few simple examples.

It would be great if more good mathematicians wrote up informal notes like this about subjects they have learned something about, even if they are not experts. The notes are entitled All What I Wanted to Know About Langlands Program and Was Afraid to Ask.

Comments

1. **fp**
   July 18, 2006

   I guess the 'cosmology' in the 4th paragraph is a typo and should be 'cohomology'.

2. **Peter Woit**
   July 18, 2006

   Oops, many people might like an application of this to cosmology, but that definitely was a typo…

3. **doctorgero**
   July 19, 2006

   > …and I'm getting embarassed that I'm not hearing any authoritative rumors about this (other than about Tao and Perlman…

   Apparently, it is 'Perelman'. 🙂

4. **Authoritative Bigshot**
   July 19, 2006

   Bhargava.

5. **nonblogger**
   July 19, 2006

   I've noticed there are four slots for fields medalists' talks, so perhaps four medals. If indeed Perelman and Tao get one that's two left. Recent young EMS prize winners make up for a good shortlist
   http://www.math.kth.se/4ecm/prizes.ecm.html
   Looking at his recent publication record, I'd bet on Okounkov.
   There are also the lists of recent Clay Fellows http://www.claymath.org/fas/research_fellows/

and award winners
http://www.claymath.org/research_award/
I've looked at recent AMS prize winners but most seem too old.

6. **bobo**
July 19, 2006

what about Zhu and Cao? the 2 dudes who completed the proof of the poincare conjecture.

7. **comentator**
July 19, 2006

My bet is T.Tao,A.Borodin,G.Perelman (though the age limit could be a problem in Perelman case). This could be surprising that in such a short time between publication of a paper and vetting of it and the award of a prize is given in the case of Zhu and Cao.

8. **woit**
July 19, 2006

doctorgero,

Thanks, typo fixed.

About possible Fields Medalists:

Zhu and Cao: No way, and, knowing Cao, I'm pretty sure he's over 40 anyway.

Okunkov: Seems unlikely. In that field I'd think his Princeton colleague Rahul Pandharipande would be a more likely candidate.

Bhargava: Hmm, that actually sounds plausible. Maybe "Authoritative Bigshot" really is one…

9. **Harry**
July 19, 2006

Wikipedia claims Perelman is just 40 this year :

http://en.wikipedia.org/wiki/Grigori_Perelman

(Not sure to what extent it's reliable, though..)

10. **Zelah**
July 19, 2006

Who is A Borodin?

11. **Brett**
July 19, 2006

A great Georgian-Russian composer and chemist?

Or more likely, this guy: http://www.cs.toronto.edu/~bor/

12. **comentator**
July 19, 2006

Alexei Borodin teaches in Caltech. Representation theory.done groundbreaking work in group theory (big groups) aspects related to representation theory.

13. **Peter Woit**
July 19, 2006

Brett and Zelah,

The reference must be to Alexei Borodin, now at Caltech, see

http://www.claymath.org/fas/research_fellows/Borodin/

The list of Clay research fellows does probably contains many of the plausible candidates for the Fields.

14. **Matt**
July 19, 2006

There are also the prize winners from the 3rd ECM
http://www.emis.de/ECM3/prizes.html
Seidel, for his work on mirror symmetry and symplectic topology, and Gaitsgory, for his work on geometric Langlands, seem viable candidates. Since low dimensional topology/geometry always seems a popular subject, how about Zoltan Szabo?
Both Borodin and Bhargava are eligible to claim the prize four years hence. This could affect their bids – in 2002, only 2 medals were distributed even though Tao was also a favorite then.
Does anyone know who is on the medal committee this go-round?

15. **Anon**
July 19, 2006

"This kind of mathematics, growing out of the equivariant index theorem … leads to a mathematically rigorous derivation of some of the implications of mirror symmetry in special cases, and a wide variety of other results related to topological strings."

Could you elaborate on that? How is equivariant index theory used there?

16. **Peter Woit**
July 19, 2006

Hi anon,

This is a complicated story. Index theory itself hasn't been much used, but there has been a lot of use of equivariant cohomology and localization formulae. For some surveys of this, see Kefeng Liu's web-site

http://www.math.ucla.edu/~liu/

where he has various papers and surveys, including one by Yau, of the subject.

A lot of this goes back to work by Givental, you can find his papers at his web-site

http://math.berkeley.edu/~giventh/

17. **D R Lunsford**
July 20, 2006

Peter,

Isn't there a "for dummies" exposition of this stuff? How about putting it in historical context?

-drl

18. **Peter Woit**
July 20, 2006

Danny,

Sorry, I don't know of any really good simpler expositions of this material, especially for physicists. There is some literature in the context of TQFT and supersymmetric quantum mechanics for physicists, look at papers from the early nineties by Blau and Thompson. Unfortunately most of the physics literature doesn't really deal with the natural mathematical context for all this. Much of this is due to Atiyah, if you really want to understand this and its history, his collected works are a good place to look. But this is really not easy for a physicist to read and absorb.

19. **Johan Richter**
July 22, 2006

Is it decided already who will win the Fields medals? Or do they wait to the last minute to make that decision?

20. **Peter Woit**
July 22, 2006

Johan,

I'm sure it's already decided. I've heard the winners are notified some months in advance, so are those mathematicians chosen to deliver the addresses describing the work of the Fields medalists.

21. **Deane**
July 22, 2006

I am fairly certain that the Fields Medalists have already been chosen, but the names are a closely guarded secret.

If you look at the citation counts on Mathscinet, then, of the names posted here, Tao, Okounkov, and Pandharipande are way ahead of everybody else. Perelman is definitely on the short list, too. But we're all just guessing.

22. **Deane**
July 22, 2006

"It would be great if more good mathematicians wrote up informal notes like this about subjects they have learned something about, even if they are not experts."

Amen!

Terence Tao has lots of expository notes on his web site; the few that I've looked at are short and elegantly written.

The String Ideology

July 20, 2006
Categories: Uncategorized

If you want to get an understanding of the ideology that many string theorists subscribe to, you should check out Lubos Motl's [latest posting](). Besides the usual dismissal of non-believers as idiots, incompetents and crackpots (an attitude that unfortunately seems to be all too common among string theorists), Lubos does actually address some scientific issues.

There's nothing at all in what he has to say that actually makes any connection between string theory and the real world. The effort to find such a connection is completely ignored, including the work of the large part of the string theory community that continues to unsuccesfully work on this. No mention of "string phenomenology", the landscape, or anything of this kind. He chooses instead to address scientific issues in a resolutely unscientific way, basing everything upon faith and ideology, beginning with the opening part of his argument:

I will treat the "whole Universe" and "all of string theory" as synonyma because I am not aware of any controllable framework that would allow me to separate them sharply.

Most of the rest of the posting is a series of criticisms of other ideas that people have advanced as alternatives to string theory. At one, point, after criticizing John Baez and Urs Schreiber for their interest in 2-groups and gerbes, he makes clear what he sees as the proper way to approach new ideas about fundamental physics that one is not familiar with:

The previous paragraph also clarifies my style of reading these papers. The abstract has so far been always enough to see that these fundamental gerbes papers make no quantitative comparison with the known physics – i.e. physics of string theory – and for me, it is enough to be 99.99% certain (I apologize for this Bayesian number whose precise value has no physical meaning) that the paper won't contain new interesting physics insights.

This attitude makes life very simple. You don't have to bother doing the hard work of trying to understand what non-string theorists are doing. All you need to do is to read the abstracts of their papers, note that they aren't doing string theory, and then you can be sure you don't need to read any farther, because if it isn't string theory, it can't provide any interesting new insights into physics.

Lubos dismisses various ideas about string theory one after the other. Much of this is devoted to dismissing the idea that has led particle physics to many of it's biggest successes: that of looking for new symmetries or new ways of exploiting ones that are already known. He insists that:

we have learned that the gauge symmetries are not fundamental in physics.

with the idea being that because of dualities, the character of gauge symmetries is not fundamental but what he calls "social scientific". This argument doesn't make any sense to me. An equivalence of two different gauge theories is very interesting, but it in no way tells you that gauge symmetry is not fundamental. Making such an argument is like arguing that representations of Galois groups in number theory are not fundamentally important because of Langlands duality.

More seriously, Lubos does mention the philosophically trickiest aspect of gauge theories: the physical degrees of freedom are not parametrized explicitly, but as quotients by the gauge group action of a larger space of degrees of freedom. It's certainly true that this is how gauge theory works, and one can try and argue that one should just ignore gauge symmetry and work directly with gauge invariant degrees of freedom. In terms of representation theory, physical states are gauge-invariant ones, so one could hope to just work with these physical states. The problem is that in most interesting cases this isn't possible. The space of connections modulo gauge transformations is non-linear and in general can't be parametrized in a useful way. Working with the linear space of connections, which can be easily parametrized and understood, and then taking into account the action of the gauge group, is the method that actually works and has been hugely successful. All experience shows that fundamental theories are best understood using an extended space of states, together with a method for picking out the physical subspace.

After dismissing alternatives to string theory, Lubos finally gets around to explaining what he sees as the fundamental principle of string theory. Amazingly, it's the bootstrap philosophy, the failed idea that guided much of particle theory during the sixties and early seventies, before the advent of gauge theories and the standard model. The bootstrap philosophy is that symmetries are nothing fundamental, what is really fundamental are certain kinds of consistency conditions. All you need to do is impose these consistency conditions, and miraculously a unique solution will appear, one which describes the real world. In the sixties the hope was that the strong interactions could be understood simply by imposing things like unitarity and analyticity conditions, and that this would lead to a unique solution of the problem. It turned out that this can't work. While unitarity and analyticity properties are very useful and tell you a lot about the implications of a theory, they in no way pick out any particular theory. There are lots and lots (a whole landscape of them, even) of QFTs that satisfy the consistency conditions. There never was evidence for uniqueness, and the bootstrap philosophy was from the beginning built on a pipe dream and large helpings of wishful thinking.

The new version of the bootstrap that Lubos wants to promote goes as follows:

In the context of quantum gravity, many of us more or less secretly believe another version of the bootstrap. I think that most of the real big shots in string theory are convinced that all of string theory is exactly the same thing as all consistent backgrounds of quantum gravity. By a consistent quantum theory of gravity, we mean e.g. a unitary S-matrix with some analytical conditions implied by locality or approximate locality, with gravitons in the spectrum that reproduce low-energy semiclassical general relativity, and with black hole microstates that protect the correct high-energy behavior of the scattering that can also be derived from a semi-classical description of general relativity, especially from the black hole physics.

So, the idea is that, at its most fundamental level, physics does not involve simple laws or symmetry principles, just some consistency conditions (of a much more obscure kind than the analyticity ones of the original bootstrap). Lubos avoids the crucial question of how big the space of solutions to these consistency conditions is. All the evidence so far is that it is so large that one can't hope to ever get any predictions about physics out of it, and the string theory community is now divided between those who hope this problem will magically go away, and those who want to give up and stop doing science as it has traditionally been understood.

In 1973 the theory of strong interactions was heavily dominated by string theory and the bootstrap philosophy. The willingness of Veltman and 't Hooft to do the hard work of understanding how to properly quantize and renormalize non-abelian gauge theories ultimately led to asymptotic freedom and QCD. This pulled the plug conclusively on that era's version of the bootstrap. Perhaps sometime in the future, new hard work on gauge theories will lead to insights that will pull the plug on this latest version, which thrives despite conclusive failure due to the kind of unscientific ideological fervor that Lubos so perfectly embodies.

Comments

1. **sunderpeeche**
   July 20, 2006

   I read LM's blog maybe twice, decided it was rubbish and have not gone back. But now I'm reading (bits and pieces) of it over here. There is no escape. I am doomed.

   I think I'll commit ritual suicide. It won't solve anything, but at least I'll feel better.

2. **JC**
July 20, 2006

Peter,

What year did many people abandon the bootstrap stuff in favor of QCD? Was it gradual, or was it very sudden and quick?

3. **hack**
July 20, 2006

I wasn't there, but I believe there were some "dead enders" who continued publishing bootstrap stuff well into the 80's.

4. **Thomas Love**
July 20, 2006

Many years ago, a friend recommended that I read the Urantia Book. I put it down after I read this nonsense about electrons:
•
• " Mutual attraction holds one hundred ultimatons together in the constitution of the electron; and there are never more nor less than one hundred ultimatons in a typical electron. The loss of one or more ultimatons destroys typical electronic identity, thus bringing into existence one of the ten modified forms of the electron. "

I recalled that when I read about string theory. Urantia's ultimatons sound a lot like strings. So, was the string revolution inspired by the Urantia book? A lot of L.M. 's ranting sound like religious fundamentalism. Fundamenatlists don't read anything which challenges their beliefs, just like L.M.

5. **Peter Woit**
July 20, 2006

Actually if you look at the latest edition of "The Tao of Physics" it will tell you that QCD doesn't work, and the bootstrap was a big success, so there are probably still some really dead-enders out there.

By 1975 when I started at Harvard, gauge theory was what everyone there was doing, but Harvard had never really been a place where the bootstrap was very popular. You'd have to ask someone who was at Berkeley during those years how long it took them to come around to gauge theory.

To some extent, lots of people who had worked on the bootstrap and early versions of string theory just picked up where they had left off when string theory came back into fashion in 1984. There really only was a period of about 10 years (1974-1984) when QFT was completely dominant.

6. **not pw**
July 20, 2006

What year did many people abandon the bootstrap stuff in favor of QCD? Was it gradual, or was it very sudden and quick?

I'm not PW, but anyway, it was quick. BUT ….
The bootstrap stuff was not abandoned "in favor of QCD". The nuclear democracy idea was abandoned in favor of the quark model (with concomitant electroweak + QCD). This happened shortly after the J/psi November Revolution, after the discovery of additional resonances and D-mesons (charmed mesons), indicating a spectroscopy of bound-states.
The quark model could readily explain this. The acceptance of the Standard Model was rapid. It is true that bootstrap holdouts persisited into the 1980's but nobody paid attention.

7. **Brett**
July 20, 2006

I don't think the bootstrap was such a bad idea in the 1960s. If the only nice quantum field theory you've got to look at is QED, you could easily get the impression that that's all there is to gauge theory. QED is the only Abelian gauge theory; if you try to generalize QED by changing things around, you tend to either get nonsense or QED with a different set of parameters (possibly including a photon mass). So it was not unreasonable to think that maybe there just were not very many quantum field theories.

Also, before the discovery of asymptotic freedom in non-Abelian gauge theories and the development of lattice gauge theory, it wasn't obvious that quantum field theory was even something meaningful to discuss in the strong coupling regime. I've met people who say they worked on the bootstrap stuff not because they believed quantum field theory was really wrong, but because they didn't have the faintest idea how to apply it when the coupling constant was large. The discovery of asymptotic freedom, which opened up high-energy strong phenomena to perturbative treatments, was really unexpected. Antishielding is extremely counterintuitive, in large part because it seems to violate the idea of Le Chatelier's principle, that the dynamics of a system should work against disruptions. That principle holds (at least in spirit) not just for chemical equilibria, but also in classical E&M (in Lenz' Law) and in the screeing responsible for charge renormalization in QED. Perhaps people could have taken the hint from lower-dimensional models that antishielding was possible; however, the assumptions of the bootstrap model, while optimistic, were not so foolish, given what was known at the time they were formulated.

8. **Kea**
July 20, 2006

Hee, hee. It just gets funnier and funnier. Many of the talks here at the String theory Maths conference bear some relation to gerbes or tensor categories. I would love to see Lubos call these people crackpots!

9. **MathPhys**
July 20, 2006

't Hooft may have pulled the plug on the bootstrap, but he also introduced 1/N expansions which link gauge theories to strings, and lie at the heart of gauge/string duality, AdS/CFT duality, matrix models, and more beautiful ideas floating around nowadays.

I think we need to keep an open mind about these things.

10. **Peter Woit**
July 20, 2006

Brett,

It's not that I think the bootstrap program was so unreasonable, it's just that it was wrong, and wrong in very much the same way that string theory is (and also perhaps right in the way that string theory is, as a way of understanding strongly coupled gauge theory). If you look at Chew's writings from that era, there's a lot of the same kind of wishful thinking you see in string theory. Not knowing about asymptotic freedom of gauge theory, you could argue that the bootstrap was "the only game in town", but we're very lucky that Veltman didn't think this way, and worked on something else (despite lots of people telling him he was wasting his time). If the bootstrap had been even more dominant than it was, and Veltman and a few others had decided to work on it, we might still be doing bootstrap theory today.

11. **Shantanu**
July 20, 2006

Peter, have you seen any report or blog about
THooft fest ?
If so , maybe you could pointa link. the program is a mixture of interesting
talks in string theory, LQG, CDT, QCD, technicolor, grand unification.
would be interested in a hearing a report

12. **Peter Woit**
July 20, 2006

Shantanu,
I've heard a bit about the 't Hooft fest, was planning on writing about it soon, but hoping the organizers would put up slides for the talks.

13. **Arun**
July 20, 2006

Didn't someone [significant] say or write that the more different ways there are to express the same thing in physics, the more fundamental it seems to be?

14. **Adam C**
July 20, 2006

Didn't someone [significant] say or write that the more different ways there are to express the same thing in physics, the more fundamental it seems to be?

Something doesnt become more fundamental via transformation to something else unless in doing this transformation a new phenomena is explained.

15. **CapitalistImperialistPig**
July 20, 2006

I was a grad student in a Regge Pole department when 't Hooft pulled the plug on the bootstrap, but the bootstrap was motivated by intriguing ideas plus the desperation born from 25 years of QFT failure in the strong interactions. I think a suitable mileage marker for the end of the bootstrap might be when Gell-Mann went to Berkley, and noticing Chew in the audience, said some nice things about it.

Chew, GM said, replied with "Thank you, but I'm working on field theory now."

Peter didn't mention it, but LM saved a lot of his wrath for those who believe in a discretium. This is a much older war in physics, dating back to perhaps the seventeenth century. Boltzmann was, some say, persecuted to the pont of suicide by the anti-atomists. Ironically enough it was Einstein who drove the stake through the heart of continuous physics, both for matter and energy. Lubos is still fighting that battle, albeit on another front.

16. **CapitalistImperialistPig**
July 20, 2006

Also, the 1970's boostrap was hardly barren. It gave birth to duality and string theory.

17. **John Baez**
July 20, 2006

Brett writes:

> The discovery of asymptotic freedom, which opened up high-energy strong phenomena to perturbative treatments, was really unexpected. Antishielding is extremely counterintuitive, in large part because it seems to violate the idea of Le Chatelier's principle, that the dynamics of a system should work against disruptions. That principle holds (at least in spirit) not just for chemical equilibria, but also in classical E&M (in Lenz' Law) and in the screening responsible for charge renormalization in QED.

That's a good point! Were there actually people around who were clever enough to object to asymptotic freedom because it violated Le Chatelier's principle? I can imagine Feynman doing it, just to get people thinking.

In week94 I summarized Wilczek's intuitive description of asymptotic freedom as analogous to paramagnetism, where one atom with spin makes its neighbors want to line up with their spins pointing the same way. This may seem reasonable (since we're all familiar with a more drastic phenomenon along the same lines: ferromagnetism), but in fact it does seem to violate Le Chatelier's principle. What would seem more "reasonable" from the viewpoint of Le Chatelier's principle is diamagnetism, where a spinning atom makes its neighbors want to spin the *other* way, counteracting the influence of any magnetic field. Diamagnets screen magnetic fields; paramagnets antiscreen them.

But in fact, in volume II of his Lectures on Physics, Feynman argues that in classical physics, both diamagnetism and paramagnetism are impossible in thermal equilibrium:

> Feynman notes: "It is a consequence of classical mechanics that if you have any kind of system – a gas with electrons, protons, and whatever – kept in a box so that the whole thing can't turn, there will be no magnetic effect. [….] The theorem then says that if you turn on a magnetic field and wait for the system to get into thermal equilibrium, there will be no paramagnetism or diamagnetism – there will be no induced magnetic moment. Proof: According to statistical mechanics, the probability that a system will have any given state of motion is proportional to exp(-U/kT), where U is the energy of that motion. Now what is the energy of motion. For a particle moving in a constant magnetic field, the energy is the ordinary potential energy plus mv^2/2, with nothing additional for the magnetic field. (You know that the forces from electromagnetic fields are q(E + v x B), and that the rate of work F.v is just qE.v, which is not affected by the magnetic field.) So the energy of a system, whether it is in a magnetic field or not, is always given by the kinetic energy plus the potential energy. Since the probability of any motion depends only on the energy – that is, on the velocity and position – it is the same whether or not there is a magnetic field. For thermal equilibrium, therefore, the magnetic field has no effect.

So, both paramagnetism and diamagnetism can only be understood using quantum mechanics! For more on how quantum mechanics gets around the above argument, and how paramagnetism is related to asymptotic freedom, see week94.

18. **Tony Smith**
July 20, 2006

Peter's post said "… Lubos … sees as the fundamental principle of string theory.
Amazingly, it's the bootstrap philosophy …".

Maybe it is not so "Amazing" when you consider the PhD lineage:
Chew (founder of bootstrap) – Gross – Witten
and
the fact that Lubos is a devoted follower of Witten.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

19. **Chris W.**
July 21, 2006

Then again, maybe this is just a milestone in LM's own evolution into a right-wing, rude version of Fritjof Capra—someone who ceased to be a serious researcher decades ago, and whose outlook on particle physics (and physics generally) seems to be thoroughly ossified.

20. **ObsessiveMathsFreak**
July 21, 2006

I believe at one point, Newton proved that a "simple harmonic" gravitational force, proportional to 1/r, would lead to elliptical orbits. He wasn't satisfied, and went on to prove the case for 1/r^2 laws. A lot of the time in science, you've just got to keep on going the way you feel is the right one. A little ideology sort of drives you. Of course, it's important not to let it control you.

21. **anon**
July 21, 2006

Obsessive MF,

The key difference between stringers and Newton iscorrect predictions. He knew the distance of the moon and its orbital period, so using his equation a=(v^2)/r, he knew the strength of centripetal acceleration (gravity) at the Moon's distance. This was smaller than the measured gravity at earth's surface by the square of the number of earth radii that the Moon is away. Hence Newton empirically validated the inverse-square law theory.

People lament that Newton kept quiet and took 22 years to publish (1665-87), checking and improving his theories. Darwin took a similar period before publishing, under pressure. Why can't string theorists take the hint from these giants?

22. **P**
July 21, 2006

Dear mr Woit,

I must ask, why do you spend so much time on the outbursts from this man? Even for me, who only is a graduate student in string theory, it is quite obvious that he can not represent the string theory community as a whole. I think that for most people it should be quite obvious that his level of emotional identification, with the debate over string theory, has passed the acceptable limit. Thus, would it not be better to focus on more 'scientific' related stuff concering the progress (or non progress) of string theory? Atleast I, who enjoy many of your posts, would find that more educational.

Happy weekend!
P

23. **robert**
July 21, 2006

John Baez highlights the impossibility of dia- or any other sort of magnetism in classical statistical physics, so deftly explained by Feynman. This seemingly counter-intuitive result was first proved by Bohr in his Copenhagen thesis in 1911, and given a thorough going over in the guise of what van Vleck called, in a more gentlmanly time, Miss van Leeuwen's theorem. It's tempting to suppose that it was this finding that first set Bohr off on the road to quantum mechanics.

24. **Chris Oakley**
July 21, 2006

People lament that Newton kept quiet and took 22 years to publish (1665-87), checking and improving his theories. Darwin took a similar period before publishing, under pressure. Why can't string theorists take the hint from these giants?

They are. The only thing is that String theorists are publishing their results 22 years *before* they have anything of value to say. This is the same as Newton and Darwin, apart from the application of the Time Reversal operator.

25. **D R Lunsford**
July 21, 2006

Feynman's argument is specious, because the change in free energy is not

dG = -S dT + V dp

rather

dG = -S dT + V dp – M dH

in a magnetic field H. Thus it is entirely possible to discuss the equilibrium, in an applied magnetic field, of monatomic and diatomic iodine (one paramagnetic, the other diamagnetic) in the reaction

I I2

See Pauli, “Thermodynamics and Kinetic Theory of Gases”, Ch. 3.

These arguments that “X requires quantum mechanics!” are always wrong.

-drl

26. **LostHisMarbles**
July 21, 2006

I wondering the same thing about the quoted Feynman derivation. What about the magnetization contribution to the (Gibbs) free energy? DRL sums it up nicely.

27. **Peter Woit**
July 21, 2006

P,
Actually I’m trying to ignore Lubos as much as possible, but I do think this posting of his reflects the views of not all, but of a sizable proportion of the string theory community. Some string theorists (including one well-known one writing here on this blog) take the attitude that the only problem with Lubos is that he is “undiplomatic”, and it is extremely rare for them to publicly criticize him. From what I hear, many of his colleagues in Cambridge are quite supportive of him, some seem quite happy to have their views put out on his blog. He works regularly with, as he calls them, the “big shots” of this field, and claims in his postings to be reflecting their views, with no complaint from anyone on his blog that this is not true. His behavior is a huge embarassment to the string theory community, but this doesn’t seem to me to be a reason I should ignore it.

As far as I can tell, many of his attitudes are widely shared by other string theorists: the idea that string theorists are smarter than other physicists, that their critics don’t know what they are talking about, that there’s no point to reading non-string theory papers, that the possibility that string theory is wrong is so unlikely that it’s not worth thinking about, etc… There’s plenty of evidence that he’s far from the only one who thinks this way, and is just more up-front about it than others.

28. **ksh95**
July 21, 2006

Drl said:

> Feynman’s argument is specious, because the change in free energy is not
>
> dG = -S dT + V dp
>
> rather
>
> dG = -S dT + V dp – M dH

huh? You just made Feynman’s point. M is the quantum mechanical variable.
And just for your own general knowledge. Calculating M for an ensemble of refrigerator magnets and calculating M for an ensemble of atoms or particles are two complety different calculations.

29. **Peter Woit**
July 21, 2006

drl, ksh95,

This is way, way off topic, enough.

30. **Hans de Vries**
July 21, 2006

John:

“In week94 I summarized Wilczek’s intuitive description of asymptotic freedom as analogous to paramagnetism,…”
–
Anti-shielding like effects do also occur with dielectric layers. Very actual are the new high k (high dielectric value) gates in CMOS transistors.

The coupling INCREASES if the very thin isolation layer (~1.2 nm) between the gate and the channel is replaced with a material with a HIGHER dielectric value. The same voltage on the gate will attract more charge-carriers in the channel instead of less.

This is exactly the opposite of what one would expect knowing the usual arguments about vacuum polarization and running coupling constants.

The higher polarized dielectric layer will have more charge carriers close to the channel pulling in more charge-carriers (of the opposite kind) into the channel. (Google for: high-k gates)

–
Regards, Hans.

31. **ks**
July 21, 2006

Hmmm… isn’t this exaclty the “swampland” program? Examining additional constraints on effective field theories that don’t necessarily describe ST but enable cuts in the landscape as well?

32. **David**
July 21, 2006

I have achieved a new first at least for me. I was banned by Lubos before I made any comment. I guess I’m beginning to understand the string ideology. If it’s possible that you might question or disagree don’t allow you to speak at all. This is not how I understand science. If this is off topic please delete it.

33. **Who**
July 21, 2006

I have achieved a new first at least for me. I was banned by Lubos before I made any comment.

that's impressive but I don't understand. did you not submit at least one comment?

34. **D R Lunsford**
July 22, 2006

Peter, off topic? We were talking ideology. I was talking physics, the kind I learned from Pauli, as opposed to hero worship, which is synonymous with ideology, in my book (cults of personality).

-drl

35. **John Baez**
July 22, 2006

Danny Lunsford writes:

> Feynman's argument is specious, because the change in free energy is not
>
> dG = -S dT + V dp
>
> rather
>
> dG = -S dT + V dp – M dH
>
> in a magnetic field H.

Feynman's derivation shows that for a system of classical point charges in equilibrium, the magnetization M is zero.

This derivation breaks down when one allows current loops as primitive elements, as I point out in the addendum to week94.

36. **woit**
July 22, 2006

Since John started this, I'd encourage people to discuss this with him at his blog. Except, wait a minute, he doesn't allow comments there since moderating them is too much trouble….

37. **CapitalistImperialistPig**
July 22, 2006

Hmmm? Maybe I should start a thread on my blog for discussions too off topic for Peter's blog?

I appreciated John Baez's explanation and clarification.

38. **Peter Woit**
July 22, 2006

CIP,

Anyone who wants to host discussions I don't want to moderate here has my strong encouragement. But actually a better idea, if John sticks to his sensible decision not to host a place to discuss what he writes, would be for someone else to set up and moderate a forum for that purpose.

39. **wolfgang**
July 22, 2006

Not such a bad idea…
Perhaps I should set something like this up.

40. **Peter Orland**
July 22, 2006

If anyone is interested in the details of asymptotic freedom and paramagnetism…

A number of people derived asymptotic freedom by showing that the vacuum is a color-paramagnet back in the late 70's/early 80's. There is a nice review article about this by N.K. Nielsen in the American Journal of Physics, vol 49, page 1171. Nielsen uses heuristic arguments, but the same mathematics follows from the use of the background-field method.

If you turn on a color-magnetic field, gluons (actually fluctations around said background field) will make Landau orbits. The gluons have some (Landau) diamagnetism, as do any particles making such orbits. Since they have spin and a color-magnetic moment, there is also (Pauli) paramagnetism). This more than makes up for the diamagnetism. So the overall effect is that the vacuum is paramagnetic.

Spin-1/2 particles don't produce this effect, because with Fermi statistics, diagmagnetism wins out.

Paramagnetism means that the permeability of the vacuum at short distances is greater than one, for a strong field. In other words,
color-magnetic fields are screened at short distances. By constancy of the speed of light, the dielectric constant of the vacuum is less than one. This means that color-electric fields are anti-screened at short
distances.

41. **Peter Woit**
July 22, 2006

OK, I give up, there seems to be an irresistible movement to discuss paramagnetism here.

42. **Peter Orland**
July 22, 2006

I have a comment on the old boostrap idea. The original idea was to study model-independent on-shell properties of field theory. Then it somehow transmuted into an entirely different animal. If one views it as a tool for doing field theory, it has some successes. The problem was that it then became the Booststrap Hypothesis, which said you could find the S-matrix from a few simple assumptions crossing, symmeties and maximal analyticity – this is what turned out to be wrong.

Maybe the same is true of string theory – use it as a framework for the True System of the World, but don't take it seriously beyond that. I think this is the working philosophy of many people in the field, as it happens.

By the way, the Bootstrap works wonderfully for integrable quantum field theories in 1+1 dimensions. You can even study off-shell properties, through form factors.

43. **David**
July 23, 2006

To Peter Orland,

Yes, but in 1 + 1 a lot of things happen automatically because of dimensionality. For example 1 + 1 QED confines because in one spatial dimension the Coulomb potential is linear in the distance. The dynamics play no role in this confinement. On the other, hand 2+1 QED confines because notrivial tunneling instanton effects provide an effective photon photon interaction that changes the effective interaction into one that is linear with the distance. This was shown by Polyakov in 1977.

So, while it is interesting that the bootstrap isquite succesful in 1 + 1 D, one should not read too much into it. If it was useful in a higher dimensional theory where the dynamics were not so much restricted by dimensionality than when one has a single spatial dimension.

44. **Peter Orland**
July 23, 2006

David,

I am not sure how I should interpret your comments about confinement in this context, but you are absolutely right that the success of the boostrap in 1+1 dimensions is very special to that dimensionality.

I was only trying to point out that something useful arose from the bootstrap program – and it IS useful, since these 1+1-dimensional models are being applied to condensed-matter problems. There are some good articles on this by Bhaseen and Tsvelik and by Essler and Konik in the Ian Kogan memorial volume (all of which can be found on the web).

45. **Paolo Bizzarri**
July 25, 2006

It seems that no one is finding natural that after bootstrap we are using strings to pull up us…

46. **BJ Flanagan**
July 27, 2006

As is well known, Pauli was the original author of the phrase, "not even wrong."

Hitherto little-known is a recently translated correspondence between Pauli and Carl Jung, where the former argues for a unitary description of mind and matter, informed by quantum mechanics.

http://www.igpp.de/english/tda/pdf/paulijcs.pdf

The article begins with a fine discussion of symmetry.

This is all music to my ears, as I have long argued for this kind of thing, pointing to the symmetries and phase relations of such traditionally "mental" objects as colors and sounds.

I have also pointed to the fact, readily available to inspection, that colors define a projective vector manifold (Riemann, Weyl) which fibers over visual space — notions dismissed as "not even wrong" by certain persons, but which ought to be of interest to string theorists.

47. **Anthony Garrett**
July 29, 2006

Dear Peter,

Thank you for writing Not Even Wrong. Well said!

String theory does seem capable of reconciling general relativity and quantum theory and cannot therefore be consigned to la-la land. But its advocates overstate its promise to the point of irresponsibility. The typical first reaction of physicists to string theory, that it is ugly, seems to me to be healthy. String theorists remind me of drug addicts – once they are into it they see as wonderful something that others recognise as ugly and unhealthy. Perhaps they should be called "string addicts". Theory has outrun experiment and moved toward the realms of science fiction.

Is string theory really "the only [unification] game in town," as many claim? If general relativity and quantum theory do not want to go together then we should consider whether they have defects, before trying to enforce a shotgun wedding that carries those defects over into the marriage and its offspring.

Quantum theory has a basic flaw. Put a large number of electrons through a z-Stern-Gerlach apparatus and take those that emerge spin up. Now put these electrons through an x-Stern-Gerlach apparatus. Some will be spin up, some spin down and nobody in the world can say which. That's OK – the job of theoretical physics is to improve prediction. I have the highest respect for those who predicted and measured (g-2) to such accuracy. But the job is not ended, and it is not OK is to say that you may not ask what the next electron will do, and that henceforth you are allowed to predict only probabilistically. Unhappily that is the Copenhagen view, and it stands against the very meaning of what it is to be a theoretical physicist.

From the start of quantum mechanics it has been recognised that any underlying deeper theory must have strange qualities – so strange that many physicists deny they exist. Since Bell we can be more specific: any underlying theory must be nonlocal and acausal. (The order of measurement on two correlated electrons in a Bell setup is not Lorentz invariant; consider also Wheeler's 'delayed-choice' experiment.) Nonlocality is no big deal in physics – we have had 300 years to get used to it under the name "action at a distance" since Newton's theory of gravity (although subquantum nonlocality does not fall off with distance). Acausality is more alarming, but should it be so alarming as to make us give up and not even think about it? Logically, it is impossible to rule out a hidden variables theory – all that can be done is to rule out categories of hidden variable theories, such as local ones. Even the name 'hidden variables' is loaded since we can see their effect – what we can't do today is control them.

The challenge is to find a deeper theory that, with its extra variables suitably averaged (marginalised) over, reproduces the probabilistic predictions of quantum mechanics. We know that such a theory must be nonlocal and partly acausal. That is a tough assignment, but if you want easy problems you should stick to trainspotting. My worry is that nobody is trying.

How did the Copenhagen mind-clamp get into place? More than a century ago a similar debate took place between the atomists, and others who claimed that the jiggling Brownian motion of small particles seen under a microscope could not be predicted. The atomists won and physical prediction improved tremendously as atomic theory developed. To see why the debate went the other way at the next level down, a generation later, you must look beyond science at the culture it is embedded in. The findings of science are independent of culture but the doing of science is not, as Not Even Wrong recognises. I suggest that, at the time of the debate over atoms, there was sufficent belief that the universe was comprehensible for the scientific community to persist in looking for an explanation, but that by the time of the Copenhagen interpretation there was not. This belief stems from the Judaeo-Christian view, interwoven into Western civilisation, that the universe is comprehensible because it was created that way. (That is why science arose in the West rather than in another culture.) Similarly, the ancient Greeks had a prior faith that the skies were perfect but that terrestrial order was not – and the astronomy they developed was brilliant whereas their terrestrial physics was embarrassing. By the time of Copenhagen, the West had been under the influence of the secular Enlightenment for longer and was turning against the Judaeo-Christian worldview (church attendance was certainly falling); Eastern mystical ideas were received with interest, notably by several of the quantum pioneers.

So, rather than play with universes as string theorists do, I prefer to seek a theory that addresses a more humble question: where will the next electron go in my x-Stern-Gerlach apparatus?

Finally, the anthropic principle is not tautological and is clearly helpful when used correctly. In the 1950s Hoyle used the observed abundance of carbon, which is the basic element of life and is formed in stars, to predict a transition in its atomic spectrum (without which its abundance would be very different). He was later found to be right, and today we would call that anthropic reasoning. There are complementary categories in which we can consider why a theory has a particular feature, so that anthropic reasoning is not a threat to bottom-up research in cosmology or even string theory – all we need to do is get the reasoning right.

Anthony Garrett

(PhD in theoretical physics, University of Cambridge, UK, 1984, then three university postdoctoral contracts. Not publishing currently but writing a monograph on probability in physics.)

48. **Tony Smith**
July 29, 2006

Anthony Garrett said "… the Copenhagen view … stands against the very meaning of what it is to be a theoretical physicist …
Logically, it is impossible to rule out a hidden variables theory – all that can be done is to rule out categories of hidden variable theories, such as local ones. …
We know that …[a successful hidden variable]… theory must be nonlocal and partly acausal. That is a tough assignment … My worry is that nobody is trying. …".

Actually Bohm did try (and substantially succeed) in formulating such a theory, but the reaction of the physics establishment (in the 1950s) to Bohm is similar to the reaction today of String Ideology proponents, such as Harvard Professor Motl, to any competing theories/models.
According to The Bohm biography Infinite Potential, by F. David Peat (Addison-Wesley 1997):
"… Max Dresden … read Bohm's papers … errors were difficult to detect … von Neumann's "proof" … did not rule out the sort of theory that Bohm had proposed. … Oppenheimer [said]…
"We consider it juvenile deviationism … we don't waste our time …" [by] actually read[ing] the paper …
Dresden … present[ed] Bohm's work in a seminar to the Princeton Institute …
The reception he received came as considerable shock to Dresden. Reactions to the theory were based less on scientific grounds than on accusations that Bohm was a fellow traveler, a Trotskyite, and a traitor. It was suggested that Dresden himself was stupid to take Bohm's ideas seriously. … all in all the overall reaction was that the scientific community should "pay no attention to Bohm's work" … Abraham Pais also used the term "juvenile deviationism". Another physicist said that Bohm was "a public nuisance" …".

The tactics of Bohr himself in support of the Copenhagen ideology have been described by Carver Mead in his book Collective Electrodynamics (MIT 2000), as follows:
"… Bohr gathered the early contributors [to]… Quantum Mechanics … into a clan in Copenhagen, encouraged everyone in the belief that they wre developing the ultimate theory of nature, and argued vigorously against any opposing views. … Bohr insisted that the laws of physics … are statistical in nature …[even though]… Statistical quantum mechanics … actively impedes our understanding by hiding the coherent wave aspects of physical processes. …
Einstein … believed … that the statistical nature of experimental results was a result of our lack of knowledge of the state of the system, and that the underlying physical laws can be formulated in a continuous manner. …
The disagreement culminated in a 1927 debate between Bohr and Einstein, refereed by Ehrenfest. Bohr was a great debater, and won the contest hands down.
A rematch was staged in 1930, and Bohr won again. …
At the time, there were no compelling experiments where the wave nature of matter was manifest in a non-statistical manner. …
Starting in the 1960s, … experimental demonstrations of numerous coherent, collective systems have … put us in a position to finally settle the Einstein-Bohr debate – with a resounding victory for Einstein. …

Bohr had won his debate with Einstein …[not by being correct but by]… an openly combative [technique]… the one who blinked first lost the argument … and the entire field adopted the style. … ".

Bohr's unreasonable, unblinking, combative style in attacking better models than Copenhagen survives today in the String Ideology style of Harvard Professor Motl and his ilk.

For some reasonable alternatives to Bohr/Copenhagen, see the works of Bohm and his followers, and the book by Carver Mead.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

49. **Eugene Stefanovich**
July 30, 2006

Anthony Garrett:

Quantum theory has a basic flaw. Put a large number of electrons through a z-Stern-Gerlach apparatus and take those that emerge spin up. Now put these electrons through an x-Stern-Gerlach apparatus. Some will be spin up, some spin down and nobody in the world can say which.

In 80 years of quantum theory, we don't have even approximate prediction of which electron will be spin up and which will be spin down. This is just a random effect. That's what quantum mechanics tells us, and I believe there is no any deeper theory behind quantum mechanics.

If you are worried (as I do) about the inconsistencies between QM and SR, I would suggest you to pay more attention to the other part, i.e., special relativity. The problem is that Lorentz transformations were derived for events associated with light pulses and non-interacting particles. We extend these transformations to all events in systems with arbitrary interactions. Is it logical?

50. **Anthony Garrett**
July 30, 2006

Hi Eugene,

One reason we don't have an exact or approximate theory telling us which electron will show spin up and which will show spin down is because Copenhagen has bamboozled the physics community into not looking for one. It then becomes a self-fulfilling negative mindset.

I want to know whether the next electron will show spin up or down. If you don't want to know, that is up to you, but isn't this the sort of question we are there to ask?

Bohm dared to look for hidden variables, though he didn't find them and his "quantum potential" was really an interpretation of quantum mechanics, since it could not generate any differing testable predictions. Today there are other such rationalisations, such as many-worlds. The key, though, is to find a theory that predicts where QM doesn't.

Anton [preferred abbreviation]
Anthony Garrett

51. **ks**
July 30, 2006

> The challenge is to find a deeper theory that, with its extra variables suitably averaged (marginalised) over, reproduces the probabilistic predictions of quantum mechanics. We know that such a theory must be nonlocal and partly acausal. That is a tough assignment, but if you want easy problems you should stick to trainspotting. My worry is that nobody is trying.

I thought those theories were studied for quite a while but without public resonance. It is even hard to find qualified criticism – if any.

http://luth2.obspm.fr/~luthier/nottale/ukdownlo.htm

52. **woit**
July 30, 2006

A reminder:
This is not a general discussion forum or an appropriate place to discuss foundational problems of QM, unless that's what the posting topic is about. Please take this kind of off-topic discussion elsewhere. I just don't have the time or ability to properly moderate it, and unmoderated, the results are depressing and something I don't want here.

53. **Anthony Garrett**
July 30, 2006

Peter,

Duly noted. I simply wanted to present a positive alternative area to strings where basic progress might be made, as well as congratulate you on your fine book that debunks string theory.

Anton

Various and Sundry

July 21, 2006
Categories: Uncategorized

Since it's summer, lots of conferences going on:

The Institute in Princeton has its usual summer program designed to train graduate students and postdocs in string theory. The schedule and lecture notes are here.

On the opposite coast, with an opposite point of view about particle physics, there's the SLAC Summer Institute, which is on LHC physics. The program and lecture notes are here. One of the organizers, JoAnne Hewett, has a posting about this at Cosmic Variance.

In Australia there's a conference on the Mathematics of String Theory going on, with a satellite workshop next week in Adelaide.

Last weekend there was a conference entitled Under the Spell of Physics, in honor of 't Hooft's 60th birthday. Many of the talks sound interesting; here's the program, but unfortunately the talks are not online. From what I hear 't Hooft remains quite skeptical about string theory, Polyakov said that current ideas about how to apply string theory to nature are wrong, and the lack of progress in fundamental theory was a concern of many of the participants.

I've been thinking a lot about BRST recently, and happened to run across the Wikipedia entry for BRST Formalism. The entry had something I hadn't seen before, a banner announcing that "This article or section may be **confusing or unclear** for some readers, and should be edited to rectify this", and that the attention of an expert and a complete rewrite was needed. I have to say that I feel that way about most of the literature on BRST…

Soon to appear in the AMS Bulletin is an article by Sinai entitled Mathematicians and Physicists = Cats and Dogs?

The Cao-Zhu paper with a proof of Poincare/Geometrization is now out in paper copies of the Asian Journal of Mathematics, but still is not on the journal's web-site. I hear that someone who called them to ask about this was told that they're trying to make some money by selling the paper copies of this particular issue. Many libraries are now only paying for on-line access to journals like this, not sure what happens in this case. Today's Wall Street Journal had an article by Sharon Begley about the Poincare proof story.

Jim Weatherall, who was recently a physics student at Harvard, now works at the Center for Science Writings at Stevens with John Horgan. He has a web-site, which includes his paper on Effective Field Theories and the Pragmatics of Explanation.

Two reviews of my book are from Sabine Hassenfelder and from Science A GoGo.

FQXI was supposed to announce the winners of its Templeton-funded grants this past weekend, but still nothing on their web-site. It will be interesting to see what their choices are for fundamental research in physics that is not being supported by the usual channels.

Update: The FQXI web-site now says they'll be publicly announcing grants on Monday, July 31.

Comments

1. **QWERTY**
   July 21, 2006

   YAKOB SINAIS ARTICLE IS DISAPPOINTING.

   THERE ARE SOME WONDERFUL ANECDOTES AND QUOTEATIONS, ESPECIALLY ABOUT AND FROM ISRAEL GELFAND!!

   BUT OVERALL IT SEEMS TO LACK SUBSTANCE WHEN MEASURED AGAINST SINAIS STATUE.

   AS FAR AS I CAN SEE IT SAYS
   1 PHYSISISTS ARE INTUITIVE BUT LACK RIGOR
   2 MATHEMATICS IS STILL IMPORTANT
   3 IT IS HARD FOR MATHEMATICIANS AND PHYSICISTS TO LEARN EACH OTHERS WAY OF THOUGHTS

   THIS IS COMPLETELY OBVIOUS, AND ISNT EVEN SAID VERY COHESIVELY.

   BUT ISRAEL GELFANDS BITING SARCASM QUOTES SOUNDS WONDERFUL, IT REDEEMS THE ARTICLE.

   THE ASIAN JOURNAL IS BEING VERY VERY EVIL AND GREADY, WE SHOULD BE ABLE TO SEE POINCARE PROOF ONLINE. ALSO IT DOES NOT HELP TO ADRESS THE CYNICISM ABOUT THE PROOF. ARE THERE ANY FAMOUS PEOPLE OF GEOMETRY (NOT YAU WHO SEEMS TO BE DOING POLITICS) WHO HAVE SCRUTINIED THIS PROOF?

2. **Garrett**
   July 21, 2006

   I like this introduction to BRST:

   http://arxiv.org/abs/hep-th/0201124

   Are there other good introductions out there on the arxiv or on the net? I think BRST is of foundational importance. And it would be nice to further develop a geometric understanding, especially in its application for nonabelian Yang-Mills fields.

   There are some hints in the earlier literature of a geometry behind BRST, with ghost fields literally added to connections in some sort of hybrid bundle, but my understanding is sketchy.

3. **Thomas Larsson**
   July 22, 2006

   A nice, brief introduction to BRST (or rather BV) from a mathematical perspective is Jim Stasheff's secret paper http://arxiv.org/abs/hep-th/9712157.

4. **Garrett**

July 22, 2006

The neat thing that's hinted at, at least for BRST applied to nonabelian gauge theory, is that the BRST operator, s, is a sort of exterior derivative (added to the exterior derivative on the base manifold) that acts in a discrete "direction", and the ghosts are the components of the connection in that direction.

Here's an introductory paper related to that point of view:

http://www.mathcs.emory.edu/~rudolf/localbrst.pdf

(And thanks for the paper you linked, Thomas.)

5. **anon**
July 22, 2006

I believe you can get a good intuitive understanding of BRST (although certainly not in its full generality) from studying Feynman diagrams. I think 't Hoof did some of this in his very early work on renormalization of gauge theories.

6. **Peter Orland**
July 22, 2006

Like a lot of people, I appreciated the original BRS work
since it can be used to generate a useful form of gauge-theory
Ward identities to prove renormalizability. Beyond that, BRST just seems a way of eliminating unphysical degrees of freedom.

The fascination with BRST by many people seems a little questionable to me, however. Some people actually think it is fundamental, but I can't see how this could be true. It is basically a perturbative supersymmetry. It doesn't work with a nonperturbative regularization – which means the lattice gauge theory (there is no other successful non-perturbative regularization). Herbert Neuberger wrote an interesting paper on this, some years ago.

BRST may be interesting mathematics, but its utility in much of physics is limited to to perturbation theory. This makes it an important tool, but not a path to a fundamental understanding of non-Abelian gauge theories. I am aware that it has a lot of utility in other areas, but I can't help suspecting that it can't be used beyond perturbation theory even in those cases.

7. **Peter Woit**
July 22, 2006

Hi Peter,

It certainly is true and interesting that you don't seem to need to gauge-fix in the lattice theory, but I still think there should be a non-perturbative version of BRST, and understanding it might teach one something interesting.

One reason for believing this is that one aspect of (quantum) BRST is something well-known to mathematicians: Lie algebra cohomology. The Lie algebra cohomology of a representation is just about by definition the invariant part of the representation. So, this is a mathematical gadget precisely designed to pick out the physical subspace in the Hilbert space of a gauge theory, and the existence of such a gadget should be a very general feature of the quantum theory, not dependent on perturbation theory. Whether one can construct it explicitly non-perturbatively is another question.

In the lattice theory, one picks out the gauge invariant subspace by integrating over the gauge group, and one can get away with this because for a finite lattice this is compact.

I'm not sure which Neuberger paper you're referring to. I'd be interested to hear more from anyone who knows of good references to papers where people try to do BRST on the lattice.

8. **Peter Orland**
July 22, 2006

Hi Peter,

I think the source of our disagreement is technical, not philosophical.
Ultimately what the lattice tells you is that the Lie group is more important than the lie Algebra. I think that there is something fundamentally wrong with trying to use Lie algebras to define gauge theory nonperturbative.

I can't prove my assertion, but I have several of arguments why it
is probably true. For example, strong-coupling calculations are the key to why QCD confines, at least for large bare coupling. Such expansions make no sense without a compact field degree of freedom.

Neuberger's paper is in the hep-lat archive. I think he actually
wrote several papers. In the first one he was trying to prove BRST, but
actually proved a no-go theorem in the second. My memory on
this is vague, unfortunately.

One possible way out – generalize BRST to compact spaces. If you
know some compact version of supersymmetry, perhaps it can be
dome.

9. **Peter Woit**
July 22, 2006

Peter,

You may be right, the difference between the Lie algebra and the group may be crucial. But even so, you should in principle be able to define the part of the Hilbert space invariant just under infinitiesimal gauge transformations. The part invariant under the gauge group should be a subspace of this. Understanding the difference between the two spaces would be interesting.

I think of BRST as Lie algebra cohomology, there are various versions of cohomology that use instead the full group. But in the compact case, they're really the same. Hmm, actually in the lattice case, since the group is compact, presumably one can show that, for a connected group, being invariant under the Lie algebra is the same as being invariant under the group. Have to think more about that…

10. **Peter Orland**
July 22, 2006

Well, that in a way is the point. Gauge transformations on the lattice
sit in a Lie group SU(N) to the power of the number of lattice sites.
This is crucially different from the Lie algebra. It is not just the fields
which are compact.

By the way, this is a bit of self-promotion (parties disinterested in the confinement problem should ignore it), but I have recently put out a paper on confinement in lattice gauge theory at weak coupling. The compactness of the gauge group is crucial there too, just as at strong coupling. The gauge theory is in 2+1 dimensions, the coupling are anisotropic, it's only SU(2), but it's the only case I know where things seems to work for unbroken non-Abelian gauge groups. None of
this will work unless the theory is on a lattice (and I think it does
work, if the theory is on a lattice).

OK, sorry about the advertisement, but I still think you need a BRST which is some sense is compact, otherwise important physics is missed.

11. **A knight who says ni**
July 22, 2006

>The Cao-Zhu paper with a proof of Poincare/Geometrization is >now out in paper copies of the Asian Journal of Mathematics, but >still is not on the journal's web-site. I hear that someone who >called them to ask about this was told that they're trying to make >some money by selling the paper copies of this particular issue. >Many libraries are now only paying for on-line access to journals >like this, not sure what happens in this case.

This sounds really strange.

The online access to the papers in AJM isn't free. So does this actually mean that people are not going to get what they are paying for?

12. **Peter Orland**
July 22, 2006

Peter,

By the way, there is a non-lattice way to define gauge theories
nonperturbatively in 2+1 dimensions, due to Karabali and Nair. They use a parametrization of the gauge orbits which is a kind of gauge fixing (someone showed this gauge fixing is closely related to background-field gauge). Strong coupling-expansions, done
by diagonalizing the kinetic term of the Hamiltonian are possible. There is a severe Gribov problem in generalizing this idea to 3+1, BUT…

Is there a BRST for Karabali and Nair's parametrization? I doubt it.
It seems worth looking into though…

13. **Peter Woit**
July 22, 2006

Knight,

I don't know what AJM is doing about this. Maybe they're sending paper copies to people who just paid for on-line access, maybe the paper will appear online, just later. My information that they are doing this to raise money is third hand, and not necessarily completely reliable. However I do have first hand information that the paper version exists (it's downstairs in our library), and that the on-line one doesn't., and that's kind of unusual.

14. **Kiff**
July 22, 2006

Is there any good introductory article which gives the big picture of BRST methods in general (as opposed to a case by case analysis) which isn't targeted to mathematicians?

15. **Moshe Rozali**
July 22, 2006

Peter Orland:

A couple of points that caught my attention:

1. When you say that BRST is perturbative, you mean it is only useful in perturbation theory, or that it is only defined perturbatively? The latter seems surprising to me. Maybe this is just then statement that on the lattice there is no need for gauge fixing?

2. Karabali and Nair work in the temporal gauge, and then use a clever parametrization of the gauge-fixed degrees of freedom, naively I would expect BRST to be more or less unaffected, even if the clever parametrization allows for some strong coupling results. On the other hand I am not sure what you mean in the statement that their parametrization is similar to background field gauge…

Thanks,

Moshe

16. **Aaron Bergman**
July 22, 2006

Nobody's mentioned the book by Henneaux and Teitelboim?

17. **Thomas Larsson**
July 23, 2006

The basic idea behind BRST applies more generally than to gauge symmetries. Whenever one has a complicated factor space, one can try to replace it by a sequence of simple spaces, such that the original space is recovered as the cohomology space H^0.

In BRST, one constructs the space of connections modulo gauge transformations in this way. In BV, one instead constructs phase space (even without gauge symmetries), as the space of histories modulo Euler-Lagrange equations. Just as one may coordinatize the space of connections module gauge transformations by fixing a gauge, one may coordinatize phase space by fixing a foliation – a history q(t) is specified by q and p = dq/dt at t = 0. But BRST teaches us that one obtains a cleaner and simpler description by cohomological means, where one does not have to make such arbitrary choices. Physics is hard as it is, without introducing spurious complications by hand.

One such complication, which has been discussed at length in connection with LQG, is the difference between the 3-diffeo and Hamiltonian constraints, which is an artefact of the introduction of a foliation. In cohomological and thus covariant formulations, both are part of the 4-diffeo constraint, because no foliation needs to be introduced.

18. **Peter Orland**
July 23, 2006

Moshe,

Formally BRST is fine in unregularized Yang-Mills. In perturbation
theory, it is still fine. If you try to define the theory beyond perturbation theory, Neuberger's theorem says (at least on the lattice) there are problems. My prejudice (and I stress that it is
only that) is that this makes BRST useless in understanding problems like quark confinement.

Karabali and Nair do more than a temporal gauge fixing. They
also solve Gauss's law. The resulting variables describe gauge
orbits. In this sense, they have a physical gauge condition, like
axial or Coulomb gauge, but with the Gribov problem eliminated. There are other ways to do similar things, but I was expressing doubt (not a proof!) that BRST is satisfied after this further gauge fixing.

My comment about background field gauge is a little technical.
It is the gauge in which a variation of the gauge connection
has vanishing covariant divergence. This was first discussed
by Babelon and Viallet around 1980. It is a useful gauge for
discussing orbit space geometry (inner products, curvature,….).

Peter O.

19. **D R Lunsford**
July 23, 2006

Peter said

The Lie algebra cohomology of a representation is just about by definition the invariant part of the representation. So, this is a mathematical gadget precisely designed to pick out the physical subspace in the Hilbert space of a gauge theory, and the existence of such a gadget should be a very general feature of the quantum theory, not dependent on perturbation theory.

This reminds me of the role of Lie derivatives in GR. Although unnecessary to formulate the theory, the Lie derivatives of this and that represent actual physical things, e.g. transport phenomena. I wonder if this is coincidental.

-drl

20. **Moshe Rozali**
July 24, 2006

Thanks Peter, I will have a look.

21. **Thomas Love**
July 25, 2006

I must agree with Qwerty's assessment: "YAKOB SINAIS ARTICLE IS DISAPPOINTING". Mathematicians and Physicists are not like cats and dogs, they are more like husband and wife (I'm not sure which is which), they often argue, but when they work together, the result can bring a new life into being.

22. **LostHisMarbles**
July 25, 2006

What is curious is that way back in the good old days, people like Newton were considered mathematicians (and natural philosophers). Newton worked on optics ~ designed the Newtonian reflector (and dabbled in alchemy). Gauss was a mathematician, yet the unit of magnetic field is named after him. Many famous mathematicians (Bernoullis, Lagrange, Laplace, etc) worked on problems of physics — witness the classic pdes = Laplace, heat, wave, all motivated by physics. The dichotomy (if that is the right word) between physicists and mathematicians seems to be a modern (20th century?) phenomenon.

If my own marriage is anything to go by, physicists are husbands. They are willing to be pragmatic about things. The wife will insist on every nitpicking detail.

23. **q2**
July 26, 2006

The rumors of AJM trying to make money by selling copies of the current issue seem to be corroborated by the bottom of [this](#) webpage, wherein we learn that we can have the issue for the low low price of $69.

I'm not sure what exactly online subscribers to AJM (or the other International Press journals for that matter) are paying for, since as near as I can tell all the articles seem to be freely downloadable by anyone.

Incidentally, Morgan and Tian's exposition of the proof of the Poincare conjecture can now be had for the low low price of $0 at http://xxx.lanl.gov/abs/math.DG/0607607

24. **anon**
July 26, 2006

"I'm not sure what exactly online subscribers to AJM (or the other International Press journals for that matter) are paying for, since as near as I can tell all the articles seem to be freely downloadable by anyone."

Since it is freely available online, that must reduce the number of printed subscription contributors to a few libraries. Hence the unit cost of printing is very high. It's down to the economy of scale: offset litho only becomes cheap when the total sales dwarf the high cost of film-setting (producing printing plates).

25. **A knight who says ni**
July 26, 2006

>I'm not sure what exactly online subscribers to AJM (or the other International Press journals for that matter) are paying for, since as near as I can tell all the articles seem to be freely downloadable by anyone.

Well, what about this then?

26. **q2**
July 26, 2006

>>Well, what about this then?

Well yeah, that was kind of my point…they *do* have an online subscription option, but I can't see why anyone would choose to buy one when nonsubscribers can download all the papers for free (granted, now with the exception of the Cao-Zhu paper).

Unless of course the idea is that when International Press decides for whatever reason to deny open access to some article (like Cao-Zhu) the online subscribers would still get it–but this seems like it would be too rare and unforeseeable to cause anyone to subscribe.

Whatever, it's not important.

27. **Anon**
July 26, 2006

There is also a Physics Reports review by Henneaux et al.

28. **MathPhys**
July 27, 2006

Cao and Zhu's paper is freely available to download from AJM. You don't need to be a subscriber.

29. **woit**
July 27, 2006

Hi MathPhys,

I beat you to this news by about 5 minutes….

30. **MathPhys**
July 28, 2006

Only 5 minutes??? I'm honoured!!!

Latest on Poincare

July 27, 2006
Categories: Uncategorized

A new 473-page paper by Gang Tian and my colleague John Morgan that gives a complete proof of the Poincare conjecture based upon the argument outlined by Grigori Perelman (which carries out the program of my other Columbia colleague Richard Hamilton) is now available as a preprint on the arXiv entitled [Ricci Flow and the Poincare Conjecture](). This paper is in the process of being refereed and should ultimately appear as a book in the monograph series that the Clay Math Institute publishes with the AMS.

Morgan and Tian just provide a proof of Poincare, not the full geometrization conjecture. Other sources for worked out details of Perelman's argument are the [notes by Kleiner and Lott](), and the recent paper by Cao-Zhu that appeared in the Asian Journal of Mathematics. Cao-Zhu provide fewer details than Morgan-Tian, but do give a proof of geometrization. Until very recently the Cao-Zhu paper was only available in the paper version of the journal, for sale by International Press for $69.00. Yesterday the journal put the full paper on-line, and it's available [here]().

Latest rumor I hear is that the Fields Medal committee has definitely chosen Perelman as a Fields medalist, with the appearance of these detailed proofs using his arguments clinching the deal. However it remains unclear whether he'll show up in Madrid, or even actually accept the honor being offered him.

Update: There's an [article]() about this in this week's Nature.

Update: The September issue of the Notices of the AMS has an excellent [article]() by Allyn Jackson about this. Next week's Science Times is supposed to have an article by Dennis Overbye.

Comments

1. **Scott Aaronson**
   July 27, 2006

   I used to wonder: what would happen if mathematicians received the solution to a centuries-old open problem in the form of a coded message from an extraterrestrial intelligence? Would they put their other projects aside, and work together to decode the message? Would they understand the solution? Would they try to start a dialogue with the intelligence? Would they *credit* it (maybe even offering it a Fields medal), or would they treat it roughly like astronomers treat a gamma-ray burst? While we still don't know the answers to these questions, it's possible that we have more insight than we did a few years ago.

2. **woit**
   July 27, 2006

   Scott,

   A weaker version of this has already happened, with mathematics getting unexpected solutions to problems via coded messages from Edward Witten, who some have suspected of an intelligence extra-terrestrial in origin. Many mathematicians did put their other projects aside and worked together to decode the message. They did start a dialog with the intelligence, but it remains unclear whether they understood what he was telling them (see the two-volume IAS set on QFT that this dialog inspired, for evidence to argue the case either way).

   They definitely did credit the intelligence, and gave him a Fields medal.

3. **Deane**
   July 28, 2006

   Peter,

   Could you elaborate on what you mean by "Cao-Zhu provide fewer details than Morgan-Tian" and on what basis you make this judgement? If it's from reading the papers themselves, could you point to where this is apparent?

4. **woit**
   July 28, 2006

   Deane,

   I haven't done more than skim the papers myself and I'm no expert. That comment was based largely on the fact that the Morgan-Tian paper is much longer than Cao-Zhu, and yet doesn't cover as much (no geometrization), together with impressions I got (which may be mistaken), from talking to people who have looked more carefully at both papers. I'd love to hear here from an expert who could more accurately compare the two papers.

5. **Walt**
   July 28, 2006

   I've heard it suggested that both John von Neumann and Alexander Grothendieck were aliens, so Scott, I think your hypothesis has been amply tested. The real question is why are aliens interfering with the progress of mathematics? What is their purpose? Is it benign, or (as I suspect) sinister? What mathematical truths are they preventing us from discovering by distracting us in this way?

6. **Cynthia**
   July 28, 2006

   Peter,

   Using layman's language, would you briefly explain how the Poincare Conjecture might spill over into the foundations of theoretical physics?
   I would greatly appreciate your input/thoughts on this matter…

   Best,

7. **LostHisMarbles**
   July 28, 2006

So Perelman can receive a Fields medal without actually writing up a proper proof? Just an outline that others fill in? Could Wiles have won a FM if he had said (while age

8. **John Baez**
July 28, 2006

There's a fun Wall Street Journal article on Perelman and the Clay Mathematics Institute's million-dollar prize for proving the Poincare conjecture. It'll be interesting to see how they deal with the complicated mess that's been brewing. I don't think they expected someone working on such an important conjecture to not bother publishing in a refereed journal!

9. **Peter Woit**
July 28, 2006

Cynthia,

Sorry, but I don't really see any relevance of the Poincare conjecture to fundamental theoretical physics. With some effort one might come up with some very speculative idea along these lines, but there's nothing I'm aware of of this kind that there's any evidence for.

10. **Aaron Bergman**
July 28, 2006

It is kind of amusing that Ricci flow, the basis for Perelman's proof, shows up in the RG equations for a 2D sigma model, but as far as I know nobody has done anything with that.

11. **urs**
July 28, 2006

> It is kind of amusing that Ricci flow, the basis for Perelman's proof, shows up in the RG equations for a 2D sigma model, but as far as I know nobody has done anything with that.

I gather that the proof involves showing that the Ricci flow on any simply-connected 3-fold goes towards a fixed point which is precisely the 3-sphere.

Now, the plain 2D-sigma model on the 3-sphere is not conformal, since the 3-sphere is not Ricci flat. So are we really talking about the SU(2) WZW model?

12. **MathPhys**
July 28, 2006

There are paprs by I Bakas on Ricci flows from a 2D field theory point of view. My recollection is that he has to deal with gauge groups with generators that have continuous indices, or something that strange.

13. **Comentator**
July 28, 2006

In reference to John Baez post; The Clay Comitee already made it clear in its rules to determine the prize winner that. "It is discretionary to the comitee to award the prize even if the proof has not been properly published in a refeered journal as long as it survived the scrutiny period of at least two years".

14. **conjecturedepoincaré**
July 29, 2006

I conjecture that Grigori Perelman will be the first awardee to turn down the Fields Medal. If we assume such an occurrence, then one can infer that he will be absent in Madrid, when the IMU disciples/attachés meet.

It will be interesting to observe how a few hundreds of thousand of dollars (amounting in toto to more than a million) from the Clay Institute and IMU safes will get distributed in the months to come, amongst the few people who have worked on the proof. After all, awarding prizes and money has always been a real fuss, but sometimes arguably useful.

However, we take the opportunity to applaud the great intellectual triumph of this little group of people whose work have, for decades, been directed towards engineering mathematical techniques to solve one of the greatest problems in the realm of reason. And bravo Perelman!

15. **D R Lunsford**
July 29, 2006

Waldron vs. Woit – this was a cakewalk. "Marketplace for ideas" – what idiocy. Peter, speak more slowly and don't pull punches in these interviews!!

-drl

16. **D R Lunsford**
July 29, 2006

sorry the above was meant for the other thread

17. **Walt**
July 29, 2006

Why is everyone so sure Perelman will turn down the Fields? Did he say that?

18. **ObsessiveMathsFreak**
July 30, 2006

Wow. The paper is in fact quite readable. I predict two things resulting from this readability. One, it will be denounced by mathematicians as inadequate, and two, the proof will stand the test of time.

19. **ObsessiveMathsFreak**
July 30, 2006

I meant the Cao-Zhu paper, which contains clearer proofs, not the Tian-Morgan, whos prrofs seem much more reticent.

20. **nonblogger**
July 30, 2006

According to the ICM website
http://www.icm2006.org/press/bulletins/bulletin20/#poincare
"*Although Perelman himself, reluctant at least until now to appear at public events, will be absent, the subject will undoubtedly be the highlight of the congress*.".

Perelman is obviously aware of the impact of his work as he met many people during his lectures in the US, and clearly for him the ICM would be a waste of time (lots of media attention and no new maths or contacts). So not coming there only means he's not too much of a party animal, nothing wrong with that.

21. **conjecturedepoincaré**
July 31, 2006

Walt said:

"Why is everyone so sure Perelman will turn down the Fields? Did he say that?"

To answer your second question, as far as I know, Perelman did not say anything about whether he would accept or decline the award of a Fields medal. If there's a statement of his concerning this, to him at least, trivial issue, then you understand that I am unaware of it.

As far as I am concerned, I was just speculating his refusal of the Fields medal, and there are a few indications that he might actually do so.

1) He has already rejected at least one mathematical prize. In 1996, he refused to accept an award in Budapest, Hungary, from the European Mathematical Society.

2) I read on the wikipage about him that he is said to be "very unmaterialistic".

3) In an article ("On the verge of a solution") by Douglas Birch, which appeared in The Baltimore Sun (January 19, 2004), it is written (I quote):
"But the 37-year-old native of Leningrad, now St. Petersburg, doesn't seem interested in money or acclaim. While he could probably get a far more lucrative job in the West, he earns only about $200 a month at the Steklov Institute of Mathematics in St. Petersburg."

4) In a news announcement of The Abdus Salam International Centre for Theoretical Physics, I read (I quote):
"There is also some indication, as yet unconfirmed, that he will not accept the US$1 million prize from the Clay Mathematics Institute in Cambridge, Massachusetts, if it is offered."

These might, or might not, be taken to be true (to the extent that they come from the mouth of the mathematician, if at all, undistorted). Some uncertainty indeed!

After all, we all share the opinion that Perelman doesn't care much, if at all, about the prizes (Clay and Fields) and millions. He just needs to be left to do his work, like he's been doing in the last decade or so. He proved Thurston's Geometrization conjecture was true (with demonstration), that's what really counts in history.

I think that one pertinent and actually significant reason, in the societal context at least, of his refusal would be that he may be concerned that the talk, and possible eventual acceptance, of the prize moneys could make him a target of the Russian underworld. I don't think the society of mathematicians wants to risk this kind of thing to happen to one of its most able members.

So far, we know from the announcement on the ICM 2006 website (thanks to nonblogger for the link) that Perelman will indeed be absent. But this does not exclude the fact that a possible Fields medal could be awarded in absentia, if this can happen at all. Well, we will know in a few days' time!

22. **gunpowder&noodles**
July 31, 2006

ST Yau has a paper in the arxiv today in which it is pointed out that the Chinese were the first to invent pasta, gunpowder, magnetism, etc etc etc, and that it is long past time that they were accorded their rightful place at the head of etc etc etc.

23. **offended**
August 2, 2006

The previous comment is a libelous mischaracterization of Yau's talk. The commenter either (a) has an axe to grind against Yau, or (b) is a racist.

24. **Peter Woit**
August 2, 2006

Unfortunately this is a subject where a lot of people have axes to grind. Please don't do this here.

25. **Comentator**
August 2, 2006

I would suggest to wait for the validation of the proof presented by Perelman, and hence the paper by Cao and Zhu. and others. Remember that The Pioncare Conjecture is notorious for its technicalities and has stopped many great mathematicians in the past.
British Prof. Dunwoody's proof being the latest to come short.

26. **Fair**
August 2, 2006

Let be fair here: I respect Yau but not what he said in several of his interviews:
1) Someone won the Veblen under his influence on the committee;
2) in the Qiao Bao July 9 2006 issue, Chinese Weekend, he SAID THIS, "But the other group of mathematicians–Morgan and Tian Gan, they said they already had their paper, but it's been two months from now, if they have it, why not publish it? They said, their 400 page manuscript had been submitted to America's Clay Math Institute. I (this year) in late April asked this institute's director, his name is Carlson, he said he doesn't have this "manuscript", but only an "introduction".

27. **Agree with Fair**
August 2, 2006

That's true. Professor Yau once criticized the Chinese people for envying their people's accomplishment. He in that interview claimed that if these critics of Zhu and Cao's work can understand the paper of Poincare Conjecture, he would award them with 10000 dollars (or yuans?) However, on the other hand, he repels the work of another Chinese mathematician–Tian, work done jointly with Morgan. So much contradictions in his talks and interviews. Hope he is not a hypocrite.

28. **Sarah M**
August 2, 2006

So whose work in Poincare conjecture is THE SINGLE MOST IMPORTANT? I heard that Freedman, Thurston, Hamilton, Perelman and Smale have all worked on it. It seems it's just the Ricci Flow and Thurston's Geometrization that play the role in solving the problem. IF the tools are handily ready, then solving the Poincare Conjecture isn't like work on an extremely lengthy Olympiad math problem? Any ideas?

29. **Walt**
August 2, 2006

I think Smale's work only applies to higher dimensions, and doesn't help *at all* for the 3-d Poincare conjecture, but I could be wrong. Thurston sketched out a new way of looking at 3-manifolds, but his role somewhat analogous to Poincare's. Hamilton invented the key technique to prove the conjecture, but if his idea by itself was sufficient to solve the problem, then he would have done it himself.

30. **anon**
August 3, 2006

"A weaker version of this has already happened, with mathematics getting unexpected solutions to problems via coded messages from Edward Witten, who some have suspected of an intelligence extra-terrestrial in origin." – Woit.

Susskind says something similar about Edward Witten in a recent invterview see http://www.thestar.com/NASApp/cs/ContentServer?pagename=thestar/Layout/Article_Type1&c=Article&cid=1154082909559&call_pageid=1105528093962&col=1105528093790 which is an article in The Toronto Star by Siobhan Roberts:

"… at a public lecture at the Strings05 conference in Toronto, an audience member politely berated physicists for their bewildering smorgasbord of analogies, asking why the scientists couldn't reach consensus on a few key analogies so as to convey a more coherent and unified message to the public.

"The answer came as a disappointment. Robbert Dijkgraaf, a mathematical physicist at the University of Amsterdam, bluntly stated that the plethora of analogies is an indication that string theorists themselves are grappling with the mysteries of their work; they are groping in the dark and thus need every glimmering of analogical input they can get.

' "What makes our field work, particularly in the present climate of not having very much in the way of newer experimental information, is the diversity of analogy, the diversity of thinking," says Leonard Susskind, the Felix Bloch professor of theoretical physics at Stanford, and the discoverer of string theory.

' "Every really good physicist I know has their own absolutely unique way of thinking," says Susskind. "No two of them think alike. And I would say it's that diversity that makes the whole subject progress. I have a very idiosyncratic way of thinking. My friend Ed Witten (at Princeton's Institute for Advanced Study) has a very idiosyncratic way of thinking. We think so differently, it's amazing that we can ever interact with each other. We learn how. And one of the ways we learn how is by using analogy."

"Susskind considers analogy particularly important in the current era because physics is almost going beyond the ken of human intelligence.

' "Physicists have gone through many generations of rewiring themselves, to learn how to think about things in a way which initially was very counterintuitive and very far beyond what nature wired us for," he says. Physicists compensate for their evolutionary shortcomings, he says, either by learning how to use abstract mathematics or by building analogies.

"Susskind, for his own part, deploys more of the latter. Analogy is one of his most reliable tools (visual thinking is the other). And Susskind has a few favourites that he always returns to, especially when he is stuck or confused.

"He thinks of black holes as an infinite lake with boats swirling toward a drain at the bottom, and he envisions the expanding universe as an inflating balloon.

"However, the real art of analogy, he says, "is not just making them up and using them, but knowing when they're defective, knowing their limitations. All analogies are defective at some level."

"A balloon eventually pops, for example, whereas a universe does not. At least not yet."

31. **gunpowder&noodles**
August 3, 2006

The Chinese invented Pythagoras's theorem hundreds of years before Pythagoras. They were knitting woollen sweaters 6000 years ago when Europeans were wearing skins. They invented tiramisu 10000 years before the Italians, and were watching TV 20000000 years ago when Europeans had not even evolved eyes yet. No grounds for surprise that they proved Poincare first.

32. **Sarah Noah**
August 4, 2006

Perelman certainly deserves the Fields, I think only he and Grothedieck are the only two great mathematicians who do not pay attention to fame and glory, as I know. Does the proof of the geometrization automatically include a proof of the Poincare conjecture?

33. **Peter Woit**
August 5, 2006

Sarah,

I agree Perelman deserves the Fields. But there are plenty of other great mathematicians who don't seem to be very interested in fame or glory. Grothendieck didn't turn down the Fields medal, and his refusal to accept the Crafoord prize came when he was already isolating himself from the world in an eccentric fashion. In "Recolte et Semailles" he has harsh things to say about Deligne, which seem to be based on his feeling that Deligne was getting credit for his ideas.

Yes, geometrization includes Poincare as a special case.

34. **Jeremy Longman**
August 7, 2006

Peter,

Grigori Perelman carried out the program of Richard Hamilton with some new methods. Cao-Zhu's paper claims it grows out of the theories of Hamilton and Perelman and completes the proof of the Poincaré and the geometrization conjectures (from what I read, Perelman's papers seem to grow out of the work of Hamilton and others too). Cao-Zhu's paper is published in a refereed journal. Unless it is found that there are gaps or even errors in the Cao-Zhu paper, should the proof of the conjectures be considered as finished? If so, are the Kleiner-Lott paper and the Morgan-Tian paper appeared a little too late? Especially for the Morgan-Tian paper, it was posted on Arxiv one month after the publication of the Cao-Zhu paper.

Jeremy

35. **Lilian**
August 7, 2006

As concerning if Morgan-Tian's paper is too late, everyone knows they have been working on it and by the time Zhu and Cao were done with their paper, Morgan and Tian had their ready also. I think they were just trying to polish the paper but when they saw Cao and Zhu's came out, they believe it was time to have theirs published. That's my guess.

36. **Walt**
August 7, 2006

My guess is that since there's a gigantic mess for who gets credit for what, that everyone will just sidestep the issue, and call it the Geometrization Theorem or something like that. The Clay Institute doesn't have that out, of course.

37. **Peter Woit**
August 7, 2006

I believe Morgan-Tian went to the referees in May, before the Cao-Zhu manuscript was available, so their work was completely independent. Most of Kleiner-Lott was freely available on the web quite a while ago, long before they posted a version to the arXiv, and Cao-Zhu refer to this in their paper. Kleiner-Lott worked out many details of Perelman, but didn't write up a full proof of Poincare.

My own guess about how the math community will ultimately apportion credit for this is that the proof is based on Hamilton's program, that important new ideas were required to make it work, and that those are due to Perelman. It seems that Perelman's outline of a proof was essentially correct. Credit for working out the details of Perelman's proof will go to a sizable group of people, including Kleiner-Lott, Cao-Zhu and Morgan-Tian, but I'd be very surprised if the Clay committee considers this kind of work something that should be rewarded with part of the million dollars.

38. **Jeremy**
August 7, 2006

Peter,

What happens if Hamilton had claimed that he had given outline of a proof to the Poincaré and the geometrization conjectures in 1982, when he first introduced the Ricci flow approach, then he would leave it to the others to fill in the details (gaps)? Of course, he didn't. He went on to fill in the details and faced some serious difficulties. But if he did, would Perelman's work also be considered as filling the details?

Hamilton started the program. He did not finish it, probably because he didn't know how to fill in some the details. Is it possible that Perelman also didn't know how to fill in some of the details of his outline?

Perelman is a great mathematician. So is Hamilton. Their work has shown that. But it does not mean that they know how to complete the proof of the conjectures.

Kleiner-Lott devoted much of their almost 200 pages paper to fill in Perelman's gaps; Morgan-Tian devoted a large part of their more than 400 pages paper to fill in some of Perelman's gaps (not including geometrization); Cao-Zhu devoted a large part of their more than 300 pages paper to fill in some Perelman's gaps and some Hamilton's gaps (Cao-Zhu do have some of their own ideas to fill in Hamilton's gaps). Obviously, these are not small gaps. Are they?

Cao-Zhu, Kleiner-Lott and Morgan-Tian must have faced and solved many serious difficulties too. Kleiner-Lott did not write up a complete proof, but Cao-Zhu (for Poincaré and geometrization) and Morgan-Tian (for Poincaré) did. I believe that they all should share the credit with Hamilton and Perelman for completing the proof of the Poincaré conjecture. In fact, if there is nothing wrong in their papers. They are the ones who really completed the proof. Right?

As for the prize money. None of the U.S. based professors really need it. Do they?

39. **woit**
August 7, 2006

Jeremy,

Hamilton never claimed that he had an outline of a proof of Poincare that could be completed by filling in "details". The new ideas that Perelman came up with were not details, but original insights, mathematics not of a routine kind, but of the highest level. Perelman did claim that he had an outline that just required details to fill in, not any new insights. He could very well have been wrong. If so, Morgan/Tian and Cao/Zhu would have come to a point where standard techniques weren't enough to fill in the outline. As far as I know, this didn't happen, and Perelman's claims turned out to be correct.

As for the money, most people are of the opinion that they could use more. Perelman seems to be an unusual case and the story is that he doesn't want the money. I doubt Hamilton (or most other mathematicians) would turn it down.

40. **Jeremy**
August 9, 2006

Peter,

I cann't remember which professor said this: "anything that is proved is obvious, nothing is obvious before it is proved". Does this have any true in it? Can I replace "obvious" with your " of a routine kind"?

Money is such a distraction. I thought that keeping mathematics out of Nobel Prize is to keep mathematicians concentrated.

41. **Peter Woit**
August 9, 2006

Jeremy,

It's certainly true that after someone has come up with a new idea, it often looks "obvious" and it's hard to understand why it was so difficult to find it. But, in this case you can look at what people had to say before the ideas were there. As far as I know, before Perelman, Hamilton was not claiming he had an outline of a proof for Poincare that could be filled in with standard techniques. He and others could have pointed you to the difficulties with pushing through Hamilton's program, and identified exactly the places where no one knew of an argument that would work. After Perelman, everyone seems to acknowledge that he found possible ways around these problems, although at first no one was sure whether the details could be successfully filled in, as Perelman claimed (and was willing to often back up by providing details when asked for them). A pretty good definition of a non-obvious (or not of a routine kind) argument is one that the leading expert on the field (Hamilton) wasn't able to find despite working on it for quite a while.

Again, as far as I know, Cao-Zhu, Kleiner-Lott, and Morgan-Tian did not run into any problems in their work that they would describe as requiring the kind of original ideas that Perelman came up with.

42. **Janet**
August 10, 2006

Hi, Peter, saw the following question under your name:
Did Yau really claim that he was the one who solved the Poincare Conjecture? If so, that would be seriously misleading. # posted by Peter Woit : 20/6/06 12:14
http://rivelles.blogspot.com/2006/06/strings-2006-day-2.html

Here is my personal opinion and some facts plus other trivial interesting things.

There was an interview with ST Yau on Qiao Bao (Chinese Overseas Newspaper), the media has little or no comments on the Poincare Conjecture and its solvers. It was question and answer type. Yau did claim that he was ONE of those who contributed to the Poincare Conjecture. But he did NOT CLAIM that he solve it. However, in another interview and talk, in which he composed a very beautiful poem (at least it's interesting poem) that describes his efforts and fascination in Poincare Conjecture. For contribution percentage, he modified a little in the QiaoBao, he probably didn't make clear before that: CHINESE MATHEMATICIANS'S TOTAL CONTRIBUTION TO THIS WHOLE WORK [Poincare Conjecture? or Topology or Ricci Flow or Geometrization? I do not know] IS NOT LESS THAN 30%. Previously, I read on a newpaper that quoted as Cao and Zhu's work takes about 35% of the credit. But this is probably a loose statement and might not be what Professor Yau meant–but I dont know; and I don't know if he intended to mislead the Chinese media, to be fair. Well, Zhu and Cao, on the other hand, as well as many Chinese mathematicians are always low-pitched and being modest. Yau is the one who gave most if not all the talks and annoucements. in fact, what he says does not represent what many of the Chinese mathematicians think. And many of the Chinese Mathematicians do not even agree with them. And to those so called Chinese Academicians, as he calls them, Yau claimed that he would personally offered 10000 yuans or dollars to whoever is able to understand Cao and Zhu's work and the Proof of Poincare Conjecture at that moment.

Some people say since Professor SS CHERN passed away, Yau is the single most influential Chinese mathematician nowaday. It seems true. Another mathematician, Tian Gang seems to be very prolific and able too. Out of Curiosity, I looked up Math Genealogy and found out Professor Yau's students–most of them are very successful mathematicians on their own. I think Yau would probably get the Wolf Prize soon for this. After all, he's got Fields, Crafoord, Veblen, MaCarthy Fellowship such big ones, Wolf may be coming too. who know. Among all the Fields Medal winners, only Yau have so many successful students so far. Mathematical Monster–an nick name I once heard given to Professor Thurston for he's such a haughty genius and polymath that some, if not most, of us envy yet respect. Read this, ts interesting!
http://www.news.cornell.edu/chronicle/02/11.21.02/Thurston_profile.html
It's interesting to note that Thurston is one of the few who received the Alan Waterman Prize, the first one being Fefferman. Other receipients are Gang Tian, Edelsbrunner, Emmanel Candes, Friedman. But the only ones who are of the same rank as Thurston are perhaps the undisputed (genius and polymath): Bombieri, Charles Fefferman, (Bourgain?). However, all four of these geniuses do not yet have as many successful students as Yau, though Thurston has done way better than the other three genius-polymathes, and has Kerkoff, Gabai etc being the most outstanding ones. Probably geniuses are just like concentrating on their own work or maybe they have not met the right students and have high expectation for their students, who knows. Sometimes working with a genius can hurt one's feeling of being an ordinary scholar. But being an ordinary math student is also fun–nevertheless it's life that we live. But anyway, hey, less than 2 weeks left for ICM…I am so looking forward to it!!!Everyone says Terence Tao and Perelman are on the Fields Medal list, we'll see.

by the way, now we have so many prizes in math: Abel, Fields, Wolf, Shaw Prize, Crafoord, Shock, Alan Waterman, the New Gauss Prize etc. Can you tell us a little more about the significance of each? Which do you think is the single most prestigious that every math department respects? Anyone like to rank them in order of importance? It's nothing but some little fun. Thanks for your time.

43. **Michael Edwards**
August 10, 2006

William Thurston is the most approachable mathematician I've ever met, with the possible exception of John Horton Conway (when he's in a good mood). They do both expect that their interlocutors make the best effort they can to understand the matter under discussion; it would not surprise me if it is somewhat hazardous to give Thurston the impression that you are mathematically able and then disappoint him. Thankfully I never had that problem. 🙂

Cheers,
– Michael

44. **Walt**
August 10, 2006

I read a survey article by Yau on arxiv (sorry, I don't know the number) that seemed very level-headed, and did not in anyway exaggerate the contributions of himself, or Cao and Zhu. So I think there's nothing to this.

45. **Lilian**
August 10, 2006

Hi, Walt, I read that too. Unfortunately you will never find information like Janet posted in the Arxiv or any other Math Journal, Media etc in English. The talks and interviews are mostly in Chinese. In fact, even in Cao and Zhu's paper, they are being very humble and very excited about the work they are undertaking. They are truly exceptional mathematicians who have done some very important and valuable piece of work. They shall be praised for their work. But someone, don't remember whom, said: "(Yau) threw a monkey wrench into the question of who gets the credit." One can not get a whole picture of what is going on if she doesn't read a lot (not only in English) and follow closely. I don't know about the truth but just share my information here. Thanks.

46. **conjecturedepoincaré**
August 11, 2006

The September issue of the Notices is out, and has an article by Allyn Jackson about the Poicaré Conjecture ("Conjectures No More?: Consensus Forming on the Proofs of Poincaré and Geometrization Conjectures"). I haven't had the time to read it completely yet, so will not comment on it.

47. **Deane**
August 11, 2006

And watch for the article in the New York Times on Tuesday.

48. **nonblogger**
August 11, 2006

Another pre-ICM hint: Tao has put a more recent picture of himself on his webpage these days, and I can't imagine this is purely coincidental 😉

49. **Lilian**
August 12, 2006

Allyn Jackson's report is great!! Excellent reading!!

50. **Lilian**
August 12, 2006

By the way, how many fields medalists have been chosen this year? Tao, Perelman, who else would be a good candidate? I heard that there were going to be three of them?

51. **anonymouse**
August 12, 2006

Lindenstrauss, McQuillan

52. **Lilian**
August 16, 2006

Today's New York Times (Science Times Section) convers news on Poincare Conjecture! Worth reading! 8-15-06 Tuesday

53. **comentator**
August 16, 2006

In the las ICM the "sure" bets (that were not) were:
Tao, Borodin, so it is safe to say that in this time they could be amongst the winners and Perelman of course.

54. **Ghost of Marshall Hall**
August 16, 2006

Potential winners:

As mentioned: Tao, Perelman

Others (but likely some other time):
Green, Pandharipande, Knutson, Darmon

55. **Jeremy**
August 18, 2006

After collecting information from the Hamilton's paper (1982), Perelman's three papers (2002, 2003), Cao-Zhu paper (2006), Kleiner-Lott paper (2006), Morgan-Tian paper (2006), as well as the articles by Sharon Begley (The Wall Street Journal), Allyn Jackson (AMS) and Dennis Overbye (The New York Times), a theory of the winners and losers in the proof of Poincare conjecture and the geometrization conjecture has been formed. The theory is described as follows:

Theorem (Winners-Losers): There are winners and losers in the proof of Poincare conjecture and geometrization conjecture.

The winners are: Hamilton, Perelman, Cao-Zhu, NSF, JSG Memorial Foundation, NSF of China, Harvard University and Tsinghua University in Beijing.

The losers are: Kleiner-Lott, Morgan-Tian and Clay Mathematics Institute.

The biggest loser: Clay Mathematics Institute. ||

A complete proof of the Winners-Losers Theorem is given in the Appendix.

Corollary: Losers speak first. Winners speak last. ||

This corollary arrives naturally and it is even true in sports and politics.

Appendix

For later reference, we start by introducing the well-known Theorem of Publication.

Theorem: Publish after the publication of others on solving the same problem equals publishes nothing, i.e.

P(t) = 0 for t > t_(P of others).

Here P stands for publication and t is time. ||

Now we proceed to the details of the winner-loser theory.

Hypothesis: All statements and claims made in the Cao-Zhu paper, Kleiner-Lott paper and Morgan-Tian paper are accurate and correct. ||

The following is the proof of the Winners-Losers Theorem.

Proof

We first give proof of the winners.

Hamilton had the vision to first introduce the equation of Ricci flow to the proof of the geometrization conjecture and laid the foundation for the Hamilton program. He has many mathematicians believed in and participated in the program. Perelman agreed that his own work was to carry out the Hamilton program. Cao-Zhu also acknowledged that their work is part of the Hamilton program. The final proof of the conjectures has vindicated Hamilton's vision. Therefore, a winner. (Very likely, the Ricci flow equation will be renamed as the Hamilton Equation.)

Perelman shared Hamilton's vision and made the most critical contribution to push through the Hamilton program by bring in new ideas and new techniques, which made others realize that his claim to the proof of the geometrization conjecture, therefore the Poincare conjecture, could very well be true. Probably more importantly, the new techniques he developed will help the future development of mathematics. Although he left many less critical issues unsolved, some of those can be considered as details, the final proof of the conjectures has proved that his statement was accurate. Therefore, a winner. (The Poincare Conjecture will almost definitely be renamed as Perelman Theorem.)

Cao-Zhu worked on the conjectures in secrecy, except to the Harvard mathematicians. They used much of the Perelman's work, but did not limit themselves only on filling Perelman's details, which enable them to keep a broader vision in pushing through the Hamilton program. After combining other people's work with their new ideas, they could first publish a complete proof for the Poincare and geometrization conjuctures. Therefore, winners. (The geometrization conjecture may very well become Perelman-Cao-Zhu Theorem.)

Cao-Zhu has certainly pulled off a coup when the publication of their paper was first announced in April 2006.

NSF and JSG Memorial Foundation have been funding the work of Cao; NSF of China has been funding the work of Zhu. Obviously, funding winners makes them winners.

Harvard University supported Zhu's work; Tsinghua University in Beijing supported Cao's work. Winner's supporters are clearly winners.

Now we give proof of the losers.

Kleiner-Lott have been narrowly following Perelman's work, but posted their findings for everyone to use. Unfortunately, they lost their concentration on the larger picture, the relationship of Perelman's work with other people's work. Filling in Perelman details has been proved not to be easy. The announcement of the forthcoming Cao-Zhu paper in April 2006 has been a surprise and total shock. Unable to complete their paper in time and were very much aware of the implication of the Theorem of Publication, Kleiner-Lott posted their unfinished paper on the non-refereed arXiv at the end of May, a few days before the appearance of the Cao-Zhu paper. Kleiner-Lott still plan to publish their paper, but after the publication of the Cao-Zhu paper and the Morgan-Tian paper, (Morgan-Tian paper uses many Kleiner-Lott's results), the publication of their paper becomes much less meaningful considering the Theorem of Publication. Therefore, losers.

Morgan-Tian have also been narrowly following Perelman's work and busying on filling Perelman's details. At the April 2006 announcement of Cao-Zhu paper's publication, they haven't even filled the details for a part of the Perelman's work. Fully understood the implication of the Theorem of Publication, they sent a preliminary version (i.e. unfinished version) of their paper for refereeing in May, just before the Cao-Zhu paper's June appearance, to show that their work was independent. The final version of their finished paper was posted on the non-refereed arXiv more than a month after the publication of the Cao-Zhu paper. By the implication of the Theorem of Publication, there will always be suspicion that the final version of the Morgan-Tian paper has been inspired and has benefited from the Cao-Zhu paper. Therefore, losers.

The final version of the Morgan-Tian paper is more than 400 pages and only filled the details of a part of the Perelman's work. Among other things, it certainly has proved that "the details for a genius could be major problems for common men".

As the supporter of the losers, Clay Mathematics Institute is obviously a loser.

Finally, we give proof of the biggest loser.

Clay Mathematics Institute has a big treasure chest to back up its list of millennium prize problems. However, whenever the millennium problems are concerned, Clay Institute should support the mathematics community as a whole (support its conferences, workshops etc.), rather than support individual mathematician(s). Its role should be of a referee rather than a player. Once it starts to support individual mathematician(s), it may be viewed as playing favoritism. Perelman might very well consider that Kleiner-Lott and Morgan-Tian are taking advantages of his work, under the support of Clay Institute. Other mathematician may think that Clay Institute intended to give the prize to mathematician(s) of their choice even before the problem is solved. The prize money may then be considered as "tainted". The honor of the prize is therefore completely lost. The only thing that left is money. Giving out money that does not have widely recognized honor is truly meaningless. (There are people who really do not care about money!) Therefore, Clay Mathematics Institute is the biggest loser.

End of proof.

56. **Jeremy**
August 21, 2006

Should Clay Institute have supported Perelman?

http://www.telegraph.co.uk/news/main.jhtml?xml=/news/2006/08/20/nmaths20.xml

57. **Student**
August 23, 2006

Does anyone know Hamilton's opinion on the issue? Namely, Does he think that any of the "complete proofs" adds true substance to the solution?

58. **Lilian**
August 24, 2006

Hamilton gave a talk in the ICM, representing Perelman in receiving the prize. However, it seemed that he talked much about what he thought would work rather than describing what Perelman has done and the impact of his work. Probably he has taken ST. Yau's flattering 50% contribution to the Poincare Conjecture and did not view Perelman's contribution as important.

59. **woit**
August 24, 2006

Lillian and Student,

I don't believe Hamilton was "representing Perelman", rather that he was invited to talk about his own work, and this invitation was issued long ago, when the IMU still hoped to get Perelman to come and speak himself. From hearing Hamilton talk about this, I see no evidence that he does not view Perelman's contribution as important, quite the opposite. You'd have to ask him what he thinks of Cao-Zhu, I don't know. But I've always had the impression that he hasn't been all that interested in the project of filling in the details of Perelman's proof using Perelman's methods, preferring to see if he can use his own ideas to do some of the needed steps. He's a very original mathematician, and such a person generally wants to do things their own way.

60. **werdna**
August 24, 2006

In their New Yorker article, the authors write:

"Mathematicians familiar with Perelman's proof disputed the idea that Zhu and Cao had contributed significant new approaches to the Poincaré. "

They then went on to quote only John Morgan to support this statement. It would have been more helpful to hear comments by other mathematicians, not just someone from the Morgan-Tian camp.

61. **Student**
August 24, 2006

I am not technically capable of evaluating the involved works, but am curious to know what the "consensus" is among experts.

From what I read/felt, Morgan-Tian viewed their work as largely a community service — to help people understand Perelman's proof (and Clay to legally claim Perelman a winner — to have a peer reviewed publication, albeit not by himself); they didn't claim to have added anything substantial.

Cao-Zhu however did claim substantial technical contributions, althoguh it is interesting that they said that they made them not because Perelman was wrong, but because they could not understand how he could, so they took on their own.

The question is whether these "contributions" are substantial or not. Morgan thought they are not, but did not elaborate. and Yau seemed to think otherwise.

When the PR storm about "crowning achievement" was going on in China, Hamilton was actually visitng that country (for "personal reasons", as I heard — rumor has it that it involved a certain lady), and despite all the media fuss, he did not make any comment, other than that Cao and Zhu are excellent mathematicians. Being a close friend of Yau's and the perceived authority on this matter, his silence could be explained as a withhold of endorsement (of course, it is totally possible he is just uninterested, especially given the non-math mess surrounding this).

But then I read the abstract of Hamilton's ICM talk and it mentioned Perelman AND Cao-Zhu.

I agree that he probably wouldn't feel that Cao-Zhu is a big deal even if it has substance (after all, these are details…). But I am interested in knowing how he put it into the context. Did he think that the deal was done 3 years ago (although we wern't sure until now), or 2 months ago (with Cao-Zhu)?

Lilian — if you were at his talk, could you shed some lights?

62. **Jessica Lau**
August 25, 2006

Read Silvia Nasare and Gruber's elaborate portray of the Poincare Conjecture and Perelman. It's an excellent piece of work! Accurate information! Fair report!!
MANIFOLD DESTINY
A legendary problem and the battle over who solved it.
http://www.newyorker.com/fact/content/articles/060828fa_fact2
What it says in the article is very accurate. But critics and supporters of Yau and others may be unhappy about what it says there. Well that's the true story! I once read a review posted in this forum, reviewer was something like "Milnor the Giant" or something like that, that person is pretty fair and stood up to point out the unfairness and hierachy in mathematics community. It seems Yau is a fame seeker, well, different people may not feel the same way. But that guy's review is just interesting.

Perelman – Yau :The comparison very clearly establishes Perelman as a hero. He considered himself a disciple of Hamilton but only without Hamilton's authorization–I think it's a very modest yet somewhat souring comment that may has resulted from what Perelman felt about other people, in this case, Mr. Hamilton and Yau etc. As a Chinese myself, I deeply respect and admire Perelman yet feel a bit shameful of the other. Remember, not all Chinese mathematicians are like that. SS Chern, Loo-Keng Hua, Jingrun Chen, they are all humble and honest mathematicians.

63. **Herman**
August 25, 2006

Interesting report!! I think it's very fair, honest and accurate report too! A few quotes from Perelam in Nasar and Gruber's article:

"It is not people who break ethical standards who are regarded as aliens," he said. "It is people like me who are isolated." We asked him whether he had read Cao and Zhu's paper. "It is not clear to me what new contribution did they make," he said. "Apparently, Zhu did not quite understand the argument and reworked it." As for Yau, Perelman said, "I can't say I'm outraged. Other people do worse. Of course, there are many mathematicians who are more or less honest. But almost all of them are conformists. They are more or less honest, but they tolerate those who are not honest."

64. **Herman**
August 25, 2006

Yep! He's right!! Expository article only deserves at most a Steele Prize!! It's original ideas that open new field in research that's of upmost importantce! I agree with you Jessica!

65. **Herman**
August 25, 2006

"Politics, power, and control have no legitimate role in our community, and they threaten the integrity of our field," Phillip Griffiths said.

66. **Lilian**
August 25, 2006

Hi, Woit, I agree with most of your views, and that "(Hamilton) He's a very original mathematician, and such a person generally wants to do things their own way." But for the past ten years, he has not produced any significant results and his research hasn't gone any further. Well, it would still be very interesting to see his own way of completing the program!

Response to Student:

Well, I believe Hamilton would be far more delighted in solving the problem himself rather than being a spectator of it for ten years. ST Yau once played a recorded speech that Hamilton commented on Chinese Mathematicians (in particular Cao, Zhu and Yau)'s contribution to Poincare conjecture (and Differential Geometry) and Hamilton did not emphasize the importance of Perelman's contribution but Yau's suggestive ideas. In his ICM talk, he even stated that he and Yau were the main developers of Ricci Flow. Well, I really don't know how true that is. 1) What he said is true; 2) he just wanted to be nice to Yau or flatter him–you know, Yau is quite influential in today's mathematics.

Hamilton certainly made great contribution, but Perelman's may be more of a revolutionary type. What you guys think?

67. **Lilian**
August 25, 2006

One forum http://www.popyard.com/cgi-mod/page.cgi?cate=1&page=1&r=0 that I came across attacked Silvia Nasar's report! those are idiots. I am sure Nasar had received numerous harrassing emails from Yau's supporters!!

68. **Matheu**
August 25, 2006

There is an article on a Chinese newspaper, Science Times, based on an interview with Shou-Wu Zhang, a Professor at Columbia University. It contains some interesting Q&A between Zhang and S-T Yau. The following is a translation:

Zhang, "Can we call it Perelman-Zhu-Cao Theorem? "

Yau, "No. The contribution by Hamilton is the most, most important."

Zhang, "Should we call it Hamilton-Perelman Theorem?"

Yau, "No. Strictly speaking, Perelman's papers posted on the internet are just sketches of the proof. You can't say that he proved Poincare and Geometrization conjectures."

Zhang, "Should we call it Thurston-Yau-Hamilton-Perelman-Zhu-Cao Theorem?"

Yau, "That's correct. Although the name is a bit long, it gives indication of everyone's role."

[Note: I have edited this to fix it, the original version incorrectly dropped Perelman's name from the last list of names. PW]

69. **Student**
August 25, 2006

Folks, let's focus on math. Last time I checked, Yau is an American.

Again I am not working in this field, thus cannot judge if Silvia Nasar's report is "accurate" on otherwise. I did read on the web that some in the field is very critical to the report (this is secondhand, since I don't know the said commentator in person). This is complicated matter, so complicated that even experts within the field (in addition to those who have a stake) are still debating. I would not be surprised if Silvia Nasar's report reflects the views of some, but not others, in the field.

And precisely because of this complexity, I am very interested in the opinion of Hamilton's own. Not that he has devine power to judge others, but after all, this is his theory, even though he got stuck on it until Perelman showed up.

I feel a bit guilty that we have stretched the strig a bit too long on this one 🙂 But Peter has the advantage of being next to Hamilton (and Morgan), so maybe he can share with us something he overheard in the hallway 🙂

70. **Student**
August 25, 2006

This is from ICM Daily News:

[Quote]

Richard Hamilton (University of Columbia, New York, USA) finished his plenary lecture yesterday, the first of the ICM2006, by saying that he felt incredibly happy and enormously grateful to Grisha Perelman for finishing his work:"In this way we actually get a proof of the Poincaré Conjecture"

… …

Hamilton said that he had a "profound admiration" for Perelman's work, and that he would be "delighted to work with him in the future". He said that he had met Perelman personally, but he was not prepared to comment on Perelman's refusal to accept the Fields Medal conferred on him on Tuesday at the ICM2006. However, Hamilton did say that "it is not fair to criticize his position".

Hamilton was also asked abut the Chinese mathematicians Xi-Ping Zhu, from the University of Zhongshan (Canton, China), and Huai-Dong Cao, from the Lehigh University in Pennsylvania (USA), who last June published a paper in the Asian Journal of Mathematics. In the abstract of this paper the authors state that they present "a complete proof of the Poincaré and Geometrization Conjectures". Hamilton is sure that "there is no controversy" because both mathematicians

are “great researchers”. According to Hamilton, the
controversy surrounding the proof of Poincaré’s
Conjecture was caused by the press. He went on to say
that Perelman’s work “is difficult to understand” and at
some points even Perelman himself employs the term
“sketch”. “A sketch is an invitation to complete a finished
work, to find a way of doing it better. But no criticism is
implied in this, only the wish to help to solve a problem.
There is no controversy involved. Grisha is a model of
decorum and there is no dispute about who did what”.

… …

[/Quote]

Very diplomatic. Still no answer to my question, but I guess that’s all we could get out of him …

71. **tg**
August 25, 2006

Dear Lilian, Jessica Lau et al,

Re: Nassar and Gruber’s report.

I agree that the article in question is very entertaining and probably an accurate reflection of the events. However, I also believe that it exploits stereotypes against the Chinese as “technicians” and uncouth (the latter is my own word). It singles out Yau, connects him deeply with Chinese mathematics, while downplaying the fact that many mathematicians (and academics) act very similarly although in much less famous circumstances.

I and my colleagues on the “2006 Fields medal winners” forum of this blog have debated the relative merits of these points at length, so I kindly point you to the discussion there.

My purpose in introducing this link of discussion is to counter Lilian’s assertion that “attackers” of Nasar and Gruber’s report are “Yau supporters”. In my case, I support myself — I am of Chinese (descent) — as are you, Jessica Lau. Don’t you think that stereotyped attacks against a figurehead such as Yau (“figure head” — a position that Nasar and Gruber go at length to assert, although it is irrelevant mathematics involved) affect Chinese (and asian) opportunities in academics? I do.

I also kindly point you to September’s Notices of the AMS article (available online at http://www.ams.org/notices/200608/200608-toc.html)
where further discussion of the position of Asians in mathematics is discussed.

72. **Peter Woit**
August 25, 2006

student,

I’m not at all informed about Hamilton’s current views, know Morgan’s much better, but in neither case would it be appropriate for me to repeat things here they told me privately. I can say that everything I’ve heard from them is consistent with what they are saying publicly. Morgan worked hard for a long time on the project with Tian of writing up a complete proof of Poincare, and in the end found that Perelman’s sketch of a proof held up, you really can fill in all the details. As he told Nasar, he sees the Cao-Zhu version as also closely following the Perelman sketch.

As for Hamilton, ever since Perelman’s papers came out, lots of people have wanted to know if he thinks Perelman’s sketch can be completed to a true proof. My impression is that this is just a question he’s not very interested in, that he’s also not interested in getting involved in the politics of this, and he hasn’t wanted to talk to the press. He agrees that Perelman has come up with important new ideas, and I think what he is interested in is seeing what he can do by putting these together with his own techniques.

One interesting thing about the Nasar article was Perelman’s comment about how grateful he was to Hamilton for Hamilton’s openness, generosity, and willingness to share his ideas. Since he has been here at Columbia, Hamilton has often run a seminar, in which he discusses in detail his latest work in this area and shares with others what he has been figuring out. His contributions to research in this area go beyond what is just in his published papers.

73. **Student**
August 25, 2006

Thanks Peter. I particularly appreciate the comment about Hamilton.

74. **Matheu**
August 25, 2006

Peter, thanks. Could you please re-edit to remove my correction?

Now, from Morgan’s public talk, he says the following about the credit for proving Poincare conjecture, contradicting to what Yau said.

“Can we say that Perelman has proven the Poincaré Conjecture?
Yes, I would say and I will say today that Perelman has proven the Poincaré Conjecture. But one has to understand that he would not have done it without Hamilton’s work. Yet, in the culture of mathematics it is my view that the credit for proving the Poincaré
Conjecture should go to Perelman.”

75. **TruthSeeker**
August 26, 2006

From reading the NewYorker article, I did not get any impression about stereotypes that tg mentioned above, and neither did many others who posted here and elsewhere in the blog. Instead, I had the feeling that the authors were trying to write a hero-villain story with Yau being the bad guy and Perelman the hero, of course. His heritage was mentioned in order to give context to and help explain his behavior. (There is the quote, e.g., “Yau’s not jealous of Tian’s mathematics, but he’s jealous of his power back in China.”) So I thought it was necessary that the authors describe his Chinese roots.

However, I thought they were too eager to portray him as the villain that they failed to mention relavant, important details about his life. For example, it is well known that Yau has been passionate about fighting corruptive practices in academia in Communist China, to the point of being censored by the PRC. He is also known to be the rare scholar who insists on not taking a salary when lecturing or engaging in other scholarly activities in China, even though generous compensations must have been offered him, being who he is.

But such stories were not told in the article, because they would weaken their thesis that Yau was dishonest or manipulative. Whether he is dishonest or not in the handling of this Poincare incident, I do not know. It may very well be that he honestly thinks Perelman's proof is too sketchy and that filling in those details is far from routine or straightforward. Let the mathematical community take the time to go through all the manuscripts carefully, and let them give their most objective opinion, without the influence of politics. If it is determined that Yau is wrong (in his assessment), then I think he should apologize one way or another. We must give credit where credit is due.

76. **tg**
August 26, 2006

Dear Truthseeker,

You raise an important question with respect to the ways in which mathematicians (academics) assign credit, which I think deserves communal analysis, since it's often subjective. Here are a few (common) scenarios.

(1) "A" claims a solution to a problem, gives the statement of the solution (e.g., a formula) that is clearly brilliant and nonobvious, and by all checks (e.g., by computer) is correct, and says the proof is by induction, but leaves all the details out. "B", needing the formula finds no proof (and "A" will not respond to his questions about the proof) and goes about doing the induction. It's hard work, but eventually an induction proof works. Afterwards "A" says "I told you so". But where was he when the details were being worked out?

(2) Same as (1), except "A" doesn't claim to have a proof, but says _probably_ an induction works.

(3) "A" says he knows how to solve a problem but isn't there yet, and tells people his progress. "B" hears about "A"'s work and with hard work completes the proof.

(4) Same as (3) except "A" says he's done what he can do.

(5) The same as (1)-(4), except "A" and "B" are actually friends. They wonder whether it's appropriate to coauthor a paper together.

In all scenarios "A" clearly deserves credit, in my opinion. But what about "B"?

Personally, I think "A" is a jerk in scenario (1), but realizes that if he allows scenario (2) then he'll be a sucker, since mathematicians usually credit the person(s) who finish the job, and he wants all the credit to himself. As humans, we don't like people dishonestly exploiting other's hard work for their own fame — in which case maybe we should highly credit "B"'s work, since we wouldn't want to fall into, gasp, physics like rigor.

In (3), "B" is a usually regarded as a "thief", but in (4) he'll be given excellent credit as the "finisher".

In (5), they could fairly write a paper together, and the result would be the "A-B's theorem" for all time.

How do Perelman, Hamilton and Yau et al. fit into these scenarios?
No one doubts Perelman's contribution! But whether Yau et al deserve any credit depends (in the above scenarios) highly on what Perelman claimed know or not know.

One could argue that clearly it wasn't so obvious to even Perelman that he actually solved Poincare, or he would have dared to say so in his papers, right?

77. **Deane**
August 26, 2006

"One could argue that clearly it wasn't so obvious to even Perelman that he actually solved Poincare, or he would have dared to say so in his papers, right?"

In fact, in his third preprint, Perelman quite explicitly claims to prove the Poincare conjecture. He just doesn't call it the "Poincare conjecture". If I recall correctly, he calls it the "elliptization conjecture" (parallel to Thurston's "hyperbolization theorem"), but its statement is exactly that of the Poincare conjecture.

78. **tg**
August 26, 2006

Dear Deane,

I stand corrected then!

79. **TruthSeeker**
August 27, 2006

tg,

Questions about assigning credit and authorship can in some cases be tricky indeed. Even guidelines such as those issued by the AMS (see http://www.ams.org/secretary/ethics.html) are only that — guidelines. For example, their website says that researchers have the responsibility to publish "full details" of their (new) results. But the word "full" is not well defined. Did Perelman's papers, for example, contain full details? I think different experts will have different opinions on this matter. I guess that's why a committee called COPE (Committee on Professional Ethics) was set up by the AMS, an important function of which is to handle disputes. But even their final decision is only an opinion, a "best" judgment arrived at by examining the circumstances of the individual case and the existing practices and conventions of the field. And different fields do have different standards. (In chemistry, e.g., it is typical to automatically add an advisor's name to a graduate student's paper, regardless of contributions made, but this practice is much rarer in mathematics.)

Regarding the various scenarios you listed, let me offer my best opinion. In cases 1, 2, and 4, "A" cannot expect to publish his result by itself in a mathematical journal without a proof, even though his formula may be correct. If "B" comes up with a proof, I think he is obligated to list "A" as a co-author, assuming that the formula has not been published elsewhere before.

In the case of (3), B's behavior could lead to a poor reputation at best or plagiarism at worst (if he doesn't give proper credit in his paper containing the ideas of "A"). Ideally, he should have discussed his intention with "A" and work out some kind of a joint authorship.

You may want to look at other examples discussed in the COPE manual (http://www.ams.org/secretary/copemanual.pdf).

80. **Whu**
August 27, 2006

When reading this thread, I think one question has not been answered. Who should we blame for Dr. Perelman's feeling of be isolated?

In that article in "New Yorker" by Nasar, the author tried to convince audience that Dr. Perelman wanted to be isolated because of the dishonest or dictatorship of YAU. Is it true?

Dr. Perelman, who at least provided a schetch to prove Poincare conjecture, left the US in 2003 and stayed away from the math world since then. Yau and his students started their journey in 2004 funded by NSF, to investigate Dr. Perelman's proof. They completed it in May 2006. Assume YAU has attempted to take credits from Dr. Perelman. But this must have happened very recently in 2006. However, Dr. Perelman have the feeling of being isolated because of dishonest back in 2003 and beyond.

I, a teacher of chinese national, doubt the author's intention. I admire Dr. Perelman's terrific work and that of Yau too, about which I actually can only understand the first page of their papers. However, I feel uncomfortable that the author distorted the fact and made Yau a Godfather from Chinatown, which, seems to me, one of the strerotypes some in the US and Europe feel comfortable when thinking about China or anybody related to China.

81. **TruthSeeker**
August 27, 2006

I don't think Perelman attributed his sense of isolation to Yau, or at least not solely or primarily to him. It says in the article that "he mentioned a dispute that he had had years earlier with a collaborator over how to credit the author of a particular proof, and said that he was dismayed by the discipline's lax ethics." And, regarding Yau, he said: "I can't say I'm outraged. Other people do worse…" He seems to be saying that there are worse offenders in the math community.

82. **MathLover**
August 27, 2006

Peter,

Thanks for your blog and discussions here. I'm a math lover and of origin from China. The discussion about Poincare conjecture and Perelman has now a new dimension. Let me first quote two posts below:

Jessica Lau Says:
August 25th, 2006 at 3:18 am
http://www.math.columbia.edu/~woit/wordpress/?p=434#comment-15043

"Perelman – Yau :The comparison very clearly establishes Perelman as a hero. He considered himself a disciple of Hamilton but only without Hamilton's authorization–I think it's a very modest yet somewhat souring comment that may has resulted from what Perelman felt about other people, in this case, Mr. Hamilton and Yau etc. As a Chinese myself, I deeply respect and admire Perelman yet feel a bit shameful of the other."

TruthSeeker Says:
August 26th, 2006 at 2:10 am
http://www.math.columbia.edu/~woit/wordpress/?p=434#comment-15091

"I had the feeling that the authors were trying to write a hero-villain story with Yau being the bad guy and Perelman the hero.

However, I thought they were too eager to portray him as the villain that they failed to mention relavant, important details about his life. For example, it is well known that Yau has been passionate about fighting corruptive practices in academia in Communist China, to the point of being censored by the PRC.

Let the mathematical community take the time to go through all the manuscripts carefully, and let them give their most objective opinion, without the influence of politics."

I'm very thankful for TruthSeeker's remark about Yau's fighting against the corruption which has an intrinsic link to his own student Tian Gang. There is a big fight of corruption in China. Unfortunately, the corruption has swept out to academic and education areas as well. Goverment has shown only little effective control of the situation. Some (not all) scholars are corrupt; more severely, the system is corrupt. Tian is a part of this system. They promote and cover each other. It's a shame to hide this from the international community. Jessica, why not shame on yourself, you seem to enjoy the distorted report about Yau. I admire Perelman and Yau both.

I want to remind a story. Last year, an audit of an american firm (listed in NY) has lead to arrest of a high ranking corrupt officer in Chinese version of FDA. It seems that we shall push a similar pressure from outside to uncover the corrupt Chinese oversea scholars like Tian. There are several of them. Their images have damaged the fame of all other Chinese scholars. I can image, a tax audit will show they may hide their very high incomes from China.

Does Sylvia Nasser has interest to uncover it? Or shall we leave it to Dennis Overbye? It is not entertainment, it's more serious. Much more.

83. **Matheu**
August 27, 2006

Morgan just declared that Perelman proved Poincare Conjecture in 2003. See the August 26 Daily News of ICM2006.

If we believe what Professor Morgan said, then what Yau did — he announced to the Chinese media that Chinese mathematicians gave the first complete proof of Poincare conjecture— is very bad. He should have known that such an announcement would cause controversy. Even worse, he accepted the paper by Cao-Zhu without stringent refereeing process and forbid the editors to read it.

84. **Who**
August 27, 2006

http://www.icm2006.org/dailynews/

click on August 26
article reporting Morgan's talk starts on page 2

http://www.icm2006.org/dailynews/dailynews26.pdf

85. **geometer**
August 27, 2006

"When reading this thread, I think one question has not been answered. Who should we blame for Dr. Perelman's feeling of be isolated?"

I do not think there is anyone here to blame (certainly not Yau; in fact, I doubt Perelman cares what Yau thinks of does).

"In that article in "New Yorker" by Nasar, the author tried to convince audience that Dr. Perelman wanted to be isolated because of the dishonest or dictatorship of YAU. Is it true?"

I do not see anything in the article that suggest the above. According to Nasar, Perelman explicitly say that "other people do worse", so this is not about Yau. I think, Perelman's reason is that, now that he is famous, he can no longer keep silent about what he thinks is wrong in math, while speaking up is not in his nature, so he has to quit.

86. **tg**
August 27, 2006

Dear geometer,

I can't be sure who Perelman blames for him leaving math. I don't think his "others do worse" remark exculpates Yau in his mind (or mine). Surely he (or perhaps Nasar and Gruber in their arguably highly biased article, written to appease the popular anti China sentiment in today's media) wants to come out saintly and would rather not explicitly say Yau is the cause of all (or much of) his problems.

It's worthwhile to emphasize that in the article reference is made to how Hamilton showed up late to a Perelman's lecture, or seemed indifferent. Perhaps Hamilton is also to blame in Perelman's mind (I speculate only based on the article, I don't know much about their dealings). Note, however, that this is internally consistent with Perelman's assertion that he is a disciple of Hamilton — what worse than to have that one special person who you think will fully appreciate your efforts seem less than caring?

His behaviour is consistent with an academic who feels under-appreciated and scorned. Of course, this is difficult for most of us to imagine — given the nice (for mortals) opportunities he was given in terms of academic positions in the US, well before his recent work. But maybe he expected even more. The standard conclusion now is that he proved that he did.

BUT, I want to write about another point of view on the unfortunate impact of Perelman's decisions to date. This is separate from the discussion of the controversial attitudes of Yau et al, which has received ample treatment in other comments.

Media, and future mathematicians are going to look at the model of Perelman for years to come. Do we really want the conclusion to be that mathematics is about the "lonely genius" who is incommunicative, terse, and eventually gave up on the pathetic subject that was thankless?

Frankly, if this is his attitude, I'd rather that he keep on doing (or not doing) his work from elsewhere — I'd rather not have him as part of any department I'm in (that being said, departments hoping to have him wouldn't be too interested in me!). I know various prestigious institutions would disagree — but I'd rather have faculty that are lecturing, developing graduate students, publishing, and in general contributing to the health of mathematics around me. I can always read his papers online.

The last thing the subject needs is for the public to think is that the Perelman model is what should be strived for. We have to constantly explain pure mathematics' virtue to the public, and to our peers, and to our financial sponsors. If we celebrate how to achieve this with a society of loners who hole up for a while and eventually (or probably not) create something, then I think mathematics is in trouble.

Imagine the following NY times article:

"XYZ a million year old problem got solved by this angry loner who hates everyone around him, and finally got his chance by ignoring and quitting math after proving he could solve what they couldn't. XYZ is this important math problem which is about donuts not looking like oranges. Mathematicians say that this could be relevant to black holes, but secretly many say that's just happy talk for the benefit of this article. Truthfully, an average engineering group at an average engineering department produces more relevant science for the work in a given year than XYZ will do for all of time. Finally, this problem would likely have been solved much earlier if there was more communication involved, but that's math.

Some-one-isomorphic-to-Sylvia Nasar-reporting"

87. **tg**
August 27, 2006

I think the "other people do worse" line is overplayed and can hardly exculpate Yau (in Perelman's mind). It can easily be interpreted as a public relations statement.

On the other hand, notice the article also points to the possibility that _Hamilton_ contributed to Perelman's disappointment. Specifically I speak of the passage concerning Hamilton's lack of keenness in Perelman's work.

In general, I don't agree with the model of mathematical research that the Perelman case exhibits. As a community, I think mathematics would be better off supporting and exhaulting collaborative, communicative academics than the "lonely genius" who rejects the field.

The fact that this lonely genius did manage to solve an old problem whose solution is only understood by a handful of people isn't an end that supports the means, in my opinion. Don't buy that nonsense about Poincare being relevant to black holes, or whatever. Any average engineering dept group probably produces more for this world and science in general than Poincare likely ever will. That's the bottom line when it comes to asking for public funding for our field.

If Perelman doesn't want to continue in mathematics, that's sad. But academic life is tough, and asks for many contributions beyond solving hard problems. He's been given plenty of opportunity and laudation at this point. I don't think it serves either mathematics or Perelman to massage him (or those who might sympathize with him) any further.

88. **Phil**
August 27, 2006

woit writes

"It's certainly true that after someone has come up with a new idea, it often looks "obvious" and it's hard to understand why it was so difficult to find it. But, in this case you can look at what people had to say before the ideas were there…."

Perfectly stated. That is key point.

89. **Moeen**
August 27, 2006

Given that there seems to be quite a debate over the quality of the recent article "Manifold Destiny," I thought it might be worth putting up my own points on it, so here goes:

Concerning the authors' supposed bias against the Chinese, I have to say when I first read the article I certainly didn't feel there was any bias of the sort in there, and was somewhat surprised to find commentators here claiming that. Looking at their comments, it seems like they're just read some parts of the article the wrong way and over-reacted.

For example, the accusation that the article stereotypes Chinese mathematicians as having strong technical skills is probably due to the quote by Griffiths referring to Yau where he says "He was not so much thinking up some original way of looking at a subject but solving extremely hard technical problems that at the time only he could solve, by sheer intellect and force of will." This isn't stereotyping, just pointing out what Yau's style of doing mathematics is. For example, the authors point out that Perelman's style is similar, and even compare him to Yau saying "Like Yau, Perelman was a formidable problem solver. Instead of spending years constructing an intricate theoretical framework, or defining new areas of research, he focussed on obtaining particular results."

Also, the authors aren't attacking the Chinese mathematcs community in pointing out Yau's status there. Their goal in this case is to show that Yau more or less wants to be the head of the mathematics community in China. Although Yau is an american, he seems to want to be seen as a hero for China. The authors state that "Yau believed that if he could help solve the Poincaré it would be a victory not just for him but also for China." and moreover:

Though Yau had not spent more than a few months at a time on mainland China since he was an infant, he was convinced that his status as the only Chinese Fields Medal winner should make him Chern's successor. In a speech he gave at Zhejiang University, in Hangzhou, during the summer of 2004, Yau reminded his listeners of his Chinese roots. "When I stepped out from the airplane, I touched the soil of Beijing and felt great joy to be in my mother country," he said. "I am proud to say that when I was awarded the Fields Medal in mathematics, I held no passport of any country and should certainly be considered Chinese."

Although Perelman does seem to fit the stereotype of the 'lonely genius,' the article seems to imply that was not his intention: In 1996, he wrote Hamilton a long letter outlining his notion, in the hope of collaborating. "He did not answer," Perelman said. "So I decided to work alone." The authors also point out that "Mathematics, more than many other fields, depends on collaboration," so they aren't supporting the 'lonely genius' model.

I've discussed the article with a few mathematicians, and they've said that the article was a bit 'gossipy,' but was factually accurate. Yau's negative behavior seems to be common knowledge in the mathematics community, and all the article did was make it public. None of them had anything negative to say about Tian.

It seems like Perelman left the community because he didn't want to get involved in the politics of it, which he saw as unethical, and this would no longer be possible given his current fame. It seems that given Perelman's reasons for leaving, the writers of the article probably wanted to give an example of this sort of unethical behavior, and Yau was the obvious choice. While Perelman's abandonment is somewhat unfortunate, I hope the mathematics community takes note and does something about this sort of behavior. I for one would not want to waste time having to deal with baseless accusations, character assassinations, and arguments over priority. This sort of thing is detrimental to research and the community.

Overall, a good article, which, despite being somewhat 'gossipy,' was well written.

90. **Whu**
August 28, 2006

Moeen Says:

"Concerning the authors' supposed bias against the Chinese, I have to say when I first read the article I certainly didn't feel there was any bias of the sort in there, and was somewhat surprised to find commentators here claiming that. Looking at their comments, it seems like they're just read some parts of the article the wrong way and over-reacted."

I trust you are honest when saying these. However, I would like to propose a conjecture here. Let us call it Nasar's Conjecture.

Nasar's Conjecture: Bias or stereotype of a doughnut size can be rendered into the size of a point in people's mind when it has been applied CNN flow.

Discussion of Nasar's Conjecture:

I and some of my friends found a trick American media always use when reporting other countries, especially China. Whenever the word "China" could not be avoided, it must be appeared in the report with phrases like "Communists, Human rights, claiming Taiwan and Tibet". For a country in Africa which US dislikes, the used phrase would be "corrupted, AIDS, civil wars, etc". This trick has been used in the extreme by CNN. Hence, we name it CNN flow.

Using this CNN flow, people without any knowledge about China or countries in Africa will naturally think "This is it over there", and hence bias no more when the word "China" is bound to "communist, human rights, abortion, take off human organ, invade Taiwan/ Tibet/Mongolia". However for people from China, the feeling is totally different.

I am sorry to introduce politics to this elegant and decent forum and I will not be upset if the editor decides to remove it. My point is: bias or not is up to each individual.

A sketch of a proof for Nasar's Conjecture:

Nasar's article portrays a bad guy, called YAU, who is dishonest, behaves like communist china, seeks fame and power crazily, promotes communist china insanely. In one word, really bad! While Dr. Perelman, a man from a remote planet, is the beloved and cutest kids we ever have.

A complete proof of Nasar's Conjecture:

Waiting for volunteers to fill in the gaps!

91. **Moeen**
August 28, 2006

Whu Says:

"A sketch of a proof for Nasar's Conjecture:

Nasar's article portrays a bad guy, called YAU, who is dishonest, behaves like communist china, seeks fame and power crazily, promotes communist china insanely. In one word, really bad! While Dr. Perelman, a man from a remote planet, is the beloved and cutest kids we ever have."

But the problem is you assume that the characterization of Yau's behavior, and Yau himself, is representing China in the article, whereas the authors are not doing this this but showing that Yau wants to be seen as the leader of mathematics in China. His behavior, while shown in contrast to Perelman, is specific to Yau, and should be seen in context of the Yau-Tian rivalry.

For instance, the authors make a point that Yau's behavior is characteristic of "the squabbles over priority which disfigure scientific history," which the authors quote from E. T. Bell's "Men of Mathematics." The authors give an example of this kind of squabble involving Poincare and Klein, and say:

"An exchange of polite letters between Leipzig and Caen ensued. Poincaré's last word on the subject was a quote from Goethe's 'Faust': 'Name ist Schall und Rauch.' Loosely translated, that corresponds to Shakespeare's 'What's in a name?'

This, essentially, is what Yau's friends are asking themselves."

The authors are not at fault if you insist on seeing things as if they're being portrayed from an american stereotype of Chinese behavior, and this discussion will also go nowhere.

92. **not a mathematician**
August 28, 2006

Perelman succeeded in proofing one more thing: when the goal is close, awarding prizes pushes scientists to exhibit their worst side. The Clay institute will have to choose if they support mathematics or speed races.

93. **tg**
August 28, 2006

Dear Moeen,

I was one of the people who felt Nasar and Gruber's article was "biased" (i.e., motivated to sell magazines to the vast anti-China sentiment reading the Newyorker, at the expense of asian mathematicians and mathematics more generally). I can tell you that I read their article quite carefully.

Obviously, all they said may very well be true, while still leaving a biased/stereotyping article, if they decide to leave out other facts or context. All I claim is that that is what they do, AND they had the motivation to do it. I've written more than enough here and elsewhere on this blog to describe how I think they do this.
I'd prefer, as a point of pedagogy to construct a non-China example to illustrate this point, but you can try your own, if you like, as an exercise.

The danger of racism/nationalism/sexism is its subtlety. Sometimes its hard to notice a particular brand unless you're dealing with it yourself. Part of the problem is many asians do not even recognize it affecting them, since they are told that they are already "overrepresented" as a minority in academics, so how could there be a problem? Perhaps with further discussion, such as the article written in September's Notices of the AMS, this will change.

94. **TruthSeeker**
August 28, 2006

An article may be well-written, factually correct, and still be an incomplete or one-sided representation of truth/reality. Surely a talented writer can skillfully select only a portion of facts available to him/her to weave a good story to achieve a certain effect.

Unfortunately, that's also how some scientists do "science."

95. **any**
August 28, 2006

〉 Dear Ms Nasar:

> First let me say that I have been a subscriber to The New Yorker
> since the 1960's; I love the magazine, and read nearly all the
> articles every week.
>
>I am also a co-author of Yau's,?and since 1991, and we have
> written 18 joint papers.?Of course, not all of them are major
> breakthroughs, but at least 2 of them can be so designated: our 2000
> paper which appeared in Nuclear Physics B, and our 2006 paper which
> just appeared in the prestigious journal "Communications in
> Mathematical Physics".?It is extremely rare for mathematicians to get
> a paper published in a journal devoted to nuclear physics, and our
> 2006 paper solves a problem dealing with stability of Black-Holes,
> first elucidated by the Princeton physicist John Wheeler in 1957.?
> These papers ALONE demolish your statement that Yau has had no major
> results in the last 10 years.?How could you have made such a
> statement??Where did you get your inxxxxation? Didn't you feel a
> responsibility to check your facts with other mathematicians??Your
> behavior reminds me of the Jason Blair scandal at the New York Times.?
> Shame on You!
> Sincerely yours,
> Joel?
>
>
>
>
> Best regards,
> Joel

96. **Moeen**
August 28, 2006

TruthSeeker Says:

"An article may be well-written, factually correct, and still be an incomplete or one-sided representation of truth/reality. Surely a talented writer can skillfully select only a portion of facts available to him/her to weave a good story to achieve a certain effect.

Unfortunately, that's also how some scientists do 'science.' "

That's certainly true, and the article was a bit 'gossipy,' if you will. My main point, however, is that the authors were not racist or stereotyping. The characterization of Yau's somewhat egotistical behavior is specific to him alone; for example, none of the other Chinese mathematicians mentioned in the article are described that way.

If anyone believes otherwise you'll have to be more specific, or the discussion just degenerates into making accusations.

97. **atomistic machiavelli**
August 28, 2006

"Moeen Says:
This isn't stereotyping, just pointing out what Yau's style of doing mathematics is. For example, the authors point out that Perelman's style is similar, and even compare him to Yau saying "Like Yau, Perelman was a formidable problem solver. Instead of spending years constructing an intricate theoretical framework, or defining new areas of research, he focussed on obtaining particular results."
.

The authors are not at fault if you insist on seeing things as if they're being portrayed from an american stereotype of Chinese behavior, and this discussion will also go nowhere."

When dealing with the dregs of defensive nationalism, with the opium smoke still hanging in the air, you find that parsing every phrase to quark dimensions is never sufficient.

Always expect that even smaller particles will be hypothesized and synthesized as necessary for the next round, until untestability is
securely in hand.

98. **ty**
August 28, 2006

How could SYLVIA NASAR AND DAVID GRUBER turn in their expense reports to their boss at the New Yorker magazine and to justify their travel expenses IF they failed to catch Perelman in person in his "dimly lit hallway of the apartment" and came home empty-handed?

How could SYLVIA NASAR AND DAVID GRUBER justify their travel expenses to their boss at the New Yorker magazine IF they failed to come up with some spicy stories and gossip materials to stir up the pot?

99. **Mike**
August 28, 2006

Anderson was quoted in the (New Yorker) article:
"Yau wants to be the king of geometry," Michael Anderson, a geometer at Stony Brook, said. "He believes that everything should issue from him, that he should have oversight. He doesn't like people encroaching on his territory."

Michael Anderson responded to The New Yorker magazine:

"The New Yorker article badly distorted my comments and
the quote given is very inaccurate and misleading. I've already
discussed it with Yau and expressed to him my apologies and disgust
at using my name in this respect. I tried to have the quote removed,
but was unsuccessful, partly because I was travelling in Europe while
all this happened very quickly and I had no time respond.
I spent a good deal of time talking with Sylvia Nasar
trying to convince her to avoid discussion of the Tian-Yau fight
since it is irrelevant to Perelman, Poincare, etc. But obviously I
was not successful. In this particular respect, I feel the New Yorker
has done a disservice to mathematicians.

Sincerely, Michael Anderson"

100. **James**
August 28, 2006

Dear Tg and Whu: Your views about the mainstream media's treatment of China may well be accurate, but either way, I do not see any reason to believe that this article was "anti-China", or that the authors acted on some presumed motivation to malign the country.

The article was about Perelman, and it can hardly be regarded as a selective choice of examples to pick the major controversy surrounding his results as an illustration of the causes of his disillusionment with the Mathematics community. Should the article instead have focused on some other minor dispute? Or not have been written?

I believe that if we regard nationalism and parochialism as undesirable traits, we should try to be the first to criticize our own countries and compatriots, rather than feeling personally responsible for defending them. As a foreigner myself, I know it can be difficult to resist the temptation to identify with my own nationality, but it is important to try, otherwise ugly scenes ensue – as anyone witnessing an international sports event can surely attest.

101. **Student**
August 28, 2006

Excuse me, but when did the New Yorker become a major math journal?

I would think that people who frequent here would be more interested in what the relevant mathematicians have to say than what reporters said. It apparently is still too early for the experts to have a consensus on how much each involved party has contributed, therefore what the New Yorker article said is really irrelevant — let's wait until Clay writes the check (even that could still be premature).

Perelman put it excellently: "If the proof is correct, then no other recognitions are necessary." Of coruse, he got the "everyone knows" part wrong …

Amen.

102. **Walt**
August 28, 2006

Student: And how is this expert consensus going to be communicated to the hoi polloi? In the Notices? Through word of mouth?

103. **Ken Dev**
August 28, 2006

I spent more than an hour in a bookshop today reading Sylvia Nassar's story on Perelman in the New Yorker. It was sad to read that he has decided to leave mathematics, a tragedy. Hope he changes his mind. Well, I know at least two people who refused the Nobel Prize [Literature and Peace]. What was disturbing about Nassar's article is the intrigue, backstabbing etc. by a Field Medalist, a famous Chinese mathematician against his own student, also a Chinese, who is now a Professor of Maths at Harvard. To the Professor's credit, he refuses to protest his teacher's behavior– he says it is the Chinese tradition.

I used to teach a very simple first year undergraduate course on visual topology, years ago, in a British University. Never did I think that a topic on topology will be such a sensational story 20 years later. Later on in my life, I switched to biology after arriving at MIT as a Visiting Scientist. We were all captivated by Watson's "Double Helix." In particular, it was absorbing to read how top scientists can be ordinary human beings with all their faults etc. Nassar's story reminded me of those events. Who knows, maybe, Ms. Nassar will write a book that will be made into film like, "A Beautiful Mind," on John Nash's life? What is needed is a very simplified description of how Poincare's Conjecture was solved by Perleman. All the articles that have appeared so far, including several in the NYT, do not quite get it. I hope someone succeeds in doing that. It deserves to be told, now that it took hundred years to solve it!

104. **woit**
August 28, 2006

I'm shutting down comments here. It's wasting too much time deleting the endless Tian vs. Yau nonsense that some people are trying to spam this blog with.

Conferences, etc.

July 27, 2006
Categories: Uncategorized

Lattice 2006, the big yearly conference on lattice gauge theory, is going on the week in Tucson. The program is here, and both plenary and parallel session talks are being posted. Georg von Hippel is blogging from the conference, his blog entries so far are here, here and here. One of the main topics is dynamical fermions, with a nice talk by Steven Sharpe. He discusses staggered fermions, which unfortunately come quadrupled with respect to what one wants, providing four "tastes" of fermions instead of a single one. The question then is whether one can get away with just taking the fourth root of the fermion determinant, which then makes the theory non-local. He concludes that this is not "Good" (i.e. having properties one would like even for non-zero lattice spacing), but it is not "Bad" (wrong continuum limit), it is just "Ugly" (for non-zero lattice spacing there are unphysical contributions, but these can be dealt with and made to go away as the lattice spacing goes to zero).

At the YITP in Stony Brook, a month-long workshop funded by Jim Simons on the String Landscape and the Swampland has begun this week. A schedule with links to audio of the talks is here. Today Cumrun Vafa is giving a talk on the beach about the Landscape and the Swampland.

The XXXIII International Conference on High Energy Physics (ICHEP), the big summer conference on high energy physics at which many HEP experimental groups announce their results, started yesterday in Moscow. Fermilab has a special web-page for abstracts from its experimental groups.

There's a review of Not Even Wrong by John Horgan in the August issue of the British magazine Prospect, entitled Stringing Us Along. Yesterday a short interview and discussion involving me and Daniel Waldram, a string theorist from Imperial College, was recorded by the BBC. I hear it was broadcast today on their "Today" radio program. Not sure how it came out after editing, and I can't really bear to listen to recordings of myself, but the discussion was perfectly polite, with no one calling anyone else names.

The August issue of Scientific American has an article about Alain Connes and his non-commutative geometry interpretation of the standard model. He continues to work on this topic, from what I hear most recently thinking about different versions of this idea that incorporate right-handed neutrinos. For some of his latest still quite speculative ideas about quantum field theory, see his recent lectures on Noncommutative Geometry and Physics, as well as other papers available at his web-site. In his version of the standard model the Higgs field has an unusual origin and one naturally gets a relation between the Higgs coupling and gauge couplings, but this is at some very high energy scale where the idea is that the use of non-commutative geometry will replace standard GUT ideas. To extract a prediction of the Higgs mass from this one has to make various assumptions, including a desert hypothesis (no new physics from 1 Tev up to the unification scale), so it's still unclear to me how solid a prediction this really is. For an example of a recent paper about this issue, see one by Knecht and Schucker.

Update: A commenter points to the website of MG11, the Marcel Grossman meeting. Videos of the talks are available. Alejandro Satz is blogged from the conference.

Comments

1. **David Appell**
   July 27, 2006

   WHAT IS WRONG with lattice gauge theorists that they choose to hold their one conference of the year in *TUCSON* during what's is probably the hottest week of the year? Them boys need some talkin' to….

2. **olie**
   July 28, 2006

   On Today: Listen Again

   http://www.bbc.co.uk/radio4/today/listenagain/

   check out 0854 "Everything you need to know about string theory"

3. **Zoe Marston**
   July 28, 2006

   Peter,

   What do you think the chatter will be in Moscow during the conference there this month about work about to begin at Cern? By the way, have you had time to review this month's Cern Courier? TThere is an interesting article about string theory, "Testing times for String Theory"?

   http://www.cerncourier.com/main/article/43/6/15

   What results would need to be produced by Cern for you to reconsider your position on the plausibility of string theory? Would also like your observations on the work being mounted in the Arctic to test String Theory.
   Jut waiting to see what happens next…
   Zoe

4. **Alex**
   July 28, 2006

   Radio clip is now at:

   http://www.bbc.co.uk/radio4/today/listenagain/thursday.shtml

   (0854 Everything you need to know about string theory.)

   It will only be there for a week.

5. **Chris Oakley**
   July 28, 2006

   I just listened to the audio clip. Here is an executive summary:

   PETER: String theory is 10 dimensional, but the world in 4 dimensional. There's no obvious, useful way to make it 4 dimensional, so it's a waste of time.

DANIEL WALDRAM: I don't agree with Peter. I like string theory and think it is good.

(If only they had had a worthy adversary like Lubos arguing the case for S.T.)

6. **Peter Woit**
July 28, 2006

Zoe,
That article appears to be several years old. It's standard hype about "braneworld scenarios". Such models can be constructed that would have observable effects at the LHC. But there's no evidence for them, and string theory doesn't predict anything about them.

I think I wrote a posting somewhere concerning the hype about "testing string theory" using the detectors in the Antartic ice. It's just hype.

If these brane-world scenarios do work out and involve a gravity scale in the TeV region, if string theory is the right theory of quantum gravity, there should be solid experimental evidence for it, and of course that would change my mind. I think this is extremely unlikely (and so do most string theorists…).

If there is big news at ICHEP, it would be because some experiment is reporting something new. I haven't heard of anything in particular.

7. **damtp_dweller**
July 28, 2006

Here's a direct link to the audio (RealPlayer file)

http://www.bbc.co.uk/radio4/today/listenagain/ram/today5_String_20060727.ram

A good interview Peter. It's just a pity that the producers didn't see fit to make it a little longer.

8. **boreds**
July 28, 2006

I liked the Today discussion. Always nice to hear the presenters struggling with something.

9. **nigel cook**
July 29, 2006

The BBC interview shows how string theorists take the fact that string theory is a failure, and then use that as a reason to continue trying. You have to admire their perseverance. (If it predicted anything, would they finally call it 'boring', and move on?)

Daniel's point is that string theory is interesting, exciting and 'beautiful' because it can't predict anything to help physics.

He then said that there is no real problem of suppression because people are totally free to do whatever they want (they merely lose grants, jobs, career prospects if they don't follow mainstream).

10. **jkowalskiglikman**
July 30, 2006

11th Marcel Grossmann meeting on gravity and astrophysics was held in Berlin last week. For those who are interested, videos of plenary talks are available at http://www.icra.it/MG/mg11/ (go to publications/conference livestream.)

I've been there, it was extremely hot (35C+ most of the days), and not very exciting scientifically. But some talks were really nice.

11. **Who**
July 30, 2006

JKG, thanks for the MG11 link. Ashtekar's talk was an obvious one for me to watch (I just did.) Any other especially nice plenary talks you want to mention?

12. **Zoe Marston**
July 30, 2006

Peter,
I think if you review the cerncourier again, you will find it is this month's. I honestly don't feel that it is at all dated, but thanks for your comments.
Z

13. **Jeremy**
July 31, 2006

Now don't go knocking good ol' Tuscon. There's lots of beautiful women there (especially in the ASU astrophysics department).

14. **Jonathan Vos Post**
July 31, 2006

What's the deal with this FermiLab search beyond the Standrad Model?

==================

Researchers Pursue A Narrow Particle With Wide
Implications

Source: Northeastern University
Posted: July 30, 2006
http://www.sciencedaily.com/releases/2006/07/060730134836.htm

Northeastern University researchers Pran Nath, Daniel
Feldman and Zuowei Liu have shown that the discovery
of a proposed particle, dubbed the Stueckelberg Z

prime, is possible utilizing the data being collected in the CDF and DO experiments at the Fermilab Tevatron. The Stueckelberg Z prime particle, originally proposed by Boris Kors currently at CERN, Geneva, Switzerland and Pran Nath at Northeastern University in 2004, is so narrow that questions had been raised as to whether or not it could be detected.

This new research, published in the July issue of Physical Review Letters, confirms that it can. The results are of importance because the discovery of this particle would provide a clue to the nature of physics beyond the Standard Model and a possible link with string theory.

“It is exciting to know that the discovery of the proposed particle at colliders is indeed possible,” said Pran Nath, Matthews Distinguished University Professor of Physics at Northeastern University. “Physicists are always looking for what is next, what will lie beyond the Standard Model. These findings point us in the direction of those answers.”

Because of its extreme narrowness, the Stueckelberg Z prime particle resembles the J/Psi (charmonium) particle, whose simultaneous discovery in 1974 by Burton Richter and Samuel Ting earned them the 1976 Nobel Prize in Physics. However, unlike the J/Psi which is a bound state, the new particle is not a bound state but a proposed new fundamental building block of matter. What sets the new Z prime particle apart from all others is the mechanism by which it gains mass.

While in the Standard Model particles such as the W and Z bosons gain mass by the Higgs phenomena, the new Z prime particle gains mass by the Stueckelberg mechanism proposed by the Swiss mathematician and physicist Ernst Carl Gerlach Stueckelberg in 1938. While the Stueckelberg mechanism arises naturally in string theory, Kors and Nath were the first to successfully utilize it in building a model of particle physics.

“If the Stueckelberg Z prime particle were to be discovered, it could signify a new kind of physics altogether, a new regime so to speak,” said Nath. “The prospect is quite exciting.”

=== end press release ===

15. **Shantanu**
July 31, 2006

JKG thanks for the MG11 link. Peter, did you watch the talks on string theory at the MG11 link and what do you think?

16. **Peter Woit**
July 31, 2006

Shantanu,

I took a look at the program, but haven’t had the time or the interest to sit and listen to the talks. Too bad they don’t have transparencies on-line. I would be a bit curious to hear what Polyakov had to say.

17. **Shantanu**
July 31, 2006

Hi
Peter,
If you go to http://www.miklei.de/mg11/ramData/mgm-arch-24-2.ram
and fast forward to ~ 51st minute you can go straight to Polyakov’s talk

Templeton Funding for Physics Research

July 31, 2006
Categories: Uncategorized

The Templeton-funded FQXI organization has announced today the awarding of 30 grants totalling more than $2 million dollars for foundational research in physics. On the one hand I've always been dubious about this organization since it is funded by a foundation dedicated not to scientific research but to bringing science and religion together. On the other hand, given the sad state of some of current theoretical physics research, the idea of an organization with a different perspective coming in with new funding and the ability to encourage new ideas that are not getting attention seems highly promising.

The proposal summaries for the successful grants are often so vague that it's hard to tell what they are actually about, although presumably the full proposals give much more detail. FQXI seems to have succeeded in keeping the Templeton religious agenda at bay, with none of the grants trying to bring religion into science. But I have to confess I find the list of grants rather discouraging. FQXI will be funding several well-known string theorists, a group that has not exactly been starved of funding or attention in recent years. Some of the grants are for "multiverse" research, again something that I don't think physics desperately needs more of right now.

Almost completely missing from the list of topics awarded grants is high energy physics, or any foundational research into the standard model. Also very hard to find is any interest in further research into the new mathematical ideas that have come out of quantum field theory research during the last thirty years. In brief, what seems to me the most promising way forward for foundational research in physics, working on better understanding the standard model QFT and its mathematical context, doesn't seem to be something on the FQXI agenda. To be fair, I have the depressing suspicion that if I had to go through all the grant proposals submitted to them, I might not have been able to do much better in terms of coming up with promising things to fund.

Last week an interesting semester-long program on Non-commutative Geometry began at the Newton Institute in Cambridge, and some of the talks have already begon to appear on this web-site. The program will include a Templeton-sponsored workshop in early September on the topic of Fundamental Structures of Space and Time. Like FQXI, the workshop mostly seems to be free of religious influence, although there will be a public panel discussion on The Nature of Space and Time which will feature two clergymen.

Over at Cosmic Variance, Sean Carroll, who is at least as dubious about Templeton as I am, has a much more positive take on the FQXI grants. In the comment section FQXI associate director Anthony Aguirre points to a new mission statement at Templeton. At their web-site you can also watch a rather long video about this if you're so inclined, or see a list of upcoming conferences they sponsor on topics in science and religion (they're especially interested in cosmology).

Update: There's a story about this at Inside Higher Ed.

Comments

1. **Anthony Aguirre**
   July 31, 2006

   Thanks for your take on this. In terms of the particular projects funded, the truth is that FQXi does not have any particular scientific "agenda" (e.g., pro- or anti-string theory, etc.) or criteria other than what was stated in the Request for Proposals. Another panel would likely make different decisions, but I suspect not *very* different ones. If there are foundational, unconventional projects (or researchers) in high-energy theory that you see languishing for lack of funds, please encourage them to apply for the next round!

   best,

   Anthony

2. **Garrett**
   July 31, 2006

   Peter,

   There do appear to be a couple of funded projects focused on gauge theory, including this highly interesting and unusual one involving gauge theory and the structure of the standard model:
   Deferential Geometry
   Of course, my opinion may be biased. 😉

   Also, it looks like the institutional links under the researcher names actually point to the researcher home pages.

   There are quite a few funded projects on unconventional approaches to quantum mechanics — that seems an interestingly different point of interest than the standard fare.

3. **woit**
   July 31, 2006

   Hi Garrett,
   I should have mentioned that yours was the one proposal among the 30 about geometry and the standard model. Good luck with it!

4. **Garrett**
   July 31, 2006

   Thanks. And even more than luck, help is welcome.

5. **Chris Oakley**
   August 1, 2006

   If there are foundational, unconventional projects (or researchers) in high-energy theory that you see languishing for lack of funds, please encourage them to apply for the next round!

   Is this a joke, or what? As with the mainstream, a system based on mutual recommendation eliminates unconventional research by definition.

6. **Thomas Dent**
   August 1, 2006

Would you not say the two experimental proposals to look at the ‘constancy’ of the fine structure constant are to do with HEP?

And in theory we have Donoghue’s ’emergent gauge symmetry’ which would be very much at the heart of HEP.

7. **woit**
August 1, 2006

Thomas,

By HEP= high energy particle physics I meant the conventional meaning of studying particle interactions at high energies = short distances. The question of variation of constants on cosmological scales is a very different subject.

The Donoghue proposal is also more or less the exact opposite of the kind of mathematical investigation of the structure of gauge theories that I was talking about. He wants to throw away that whole structure as not fundamental.

8. **Wen**
August 1, 2006

The emergence of gauge bosons (as well as the emergence of Fermi statistics and gravitons) has been an active research area since 1987 in condensed matter theory. Many results were obtained, such as a unification of gauge interaction and Fermi statistics.
Just google “emergent gauge bosons”.

9. **D R Lunsford**
August 1, 2006

I can’t believe what people will pay for. FQXI (sounds like a southern radio station – “QXI in Dixie!”) is the IKEA of theory. Hidden variables in cosmology? Why not apply the Bell inequalities to colliding galaxies? Apparently this dude Lisi has figured it all out anyway so why bother?

The real question people should be studying is how the academic world collapsed under the dehumanizing weight of sex, drugs, and rock and roll.

-drl

10. **Brett**
August 1, 2006

Funding projects that look at possible time variations in fundamental constants is an extremely good idea. If the fine structure constant or the electron-proton mass ratio turn out to be time dependent, then this is an unambiguous indication of new physics. Moreover, unlike say, dark matter, time varying constants are relatively difficult to incorporate into the conventional frameworks that we use to describe physics. So the discovery of time variations would tell us a great deal about what kinds of changes are required if we are to understand the fundamental physics of the universe.

11. **JPL**
August 1, 2006

IKEA of Theory, indeed! I would suggest FQuiXi as a more commercial audiologo, though IT IS a southern radio station of sorts, if you think about it. Though that kind of money can surely buy a lot of sex, drugs, rock and roll and surf boards, I would say Templeton has managed to make science so silly and way out there that religion looks a lot more attractive! Not surprisingly, perhaps, most of these people work in institutions (my own included) that would insure them decent funding for better refereeing than what this list brings out without selling off their scientific honesty! No? Maybe I am the one who’s being naive…

12. **Who**
August 1, 2006

In reply to Wen
http://www.math.columbia.edu/~woit/wordpress/?p=436#comment-14002

Xiao-Gang Wen,
thanks for joining this discussion. Congratulations on your Foundational Questions award!
http://dao.mit.edu/~wen/

I agree with Smolin’s position that more support should go to those with the courage and intellectual independence to pursue approaches to basic problems that are not commonplace: in effect allocating to the individual track-record instead of to the entrenched program. Your condensed-matter type approach to the emergence of spacetime and fundamental interactions is an example of vigorous investigation off the beaten track.

Hopefully the awards will have a “multiplier” effect when departments at major universities see the FQX spotlight picking out individuals committed to bold original lines of research. Conceivably, some departments will be influenced to shift policy in awarding postdoctoral fellowships, and provide graduate students wider choice of topic.

D.R. Lunsford, any physics department in the US would be lucky to get “the dude Lisi” as a research fellow
http://arxiv.org/abs/gr-qc/0511120
Clifford bundle formulation of BF gravity generalized to the standard model
that is exactly the kind of research dude they should be looking for.

Just looking down the list of awardees, I see they are almost all attached to major academic institutions and it is spread out—not much clumping.

Michigan, Berkeley, Kansas, Massachusetts, London-Imperial, Yale, SantaBarbara, Perimeter-Waterloo, Illinois, Barnard, Harvard, Tufts, Louisiana, Oxford, Cornell….etc. …
(and of course MIT where X-G Wen is)

I really like the spread. It feels like a “democracy of the excellent” instead of a prestige pecking-order. And if they wanted to have a “multiplier effect” on the way departments think, then a wide spread was well calcuated. Congratulations to those responsible (Tegmark, Aguirre, and all who helped.) Lots more could be said—which I’ll leave to others.

13. **D R Lunsford**
August 1, 2006

Who,

massive shrug – quaternion disease. Most of us get past that stage while undergraduates.

None of the proposals listed on FQXI has a shred of physical interest. Big surprise.

-drl

14. **Garrett**
    August 1, 2006

    Thanks Who.

    DRL,

    The relationship:
    fermions -> spinors -> Clifford algebra -> quaternions
    seems pretty clear. Which step do you dislike?

    I'm kind of surprised to be the only guy on the list with a weird TOE. Makes me feel lonely. I'm curious what the 20 proposals that made the first cut from 172 to 50, but not the second cut from 50 to 30 were.

15. **D R Lunsford**
    August 1, 2006

    Garret,

    What you have is algebraic logic chopping with no real dynamical principle, and so no theory. But congrats on getting people to pay you for it. I looked at similar ideas in the early 90s and rejected them as a waste of time. V. Fock did similar work with the Dirac algebra in GR as far back as the mid 30s. That work is still valuable, but offers no new ideas – only Dirac on a Riemannian manifold.

    -drl

16. **Garrett**
    August 1, 2006

    drl,
    That's essentially correct — except this happens to be unusually successful algebraic logic chopping. And if I had the dynamics worked out perfectly, I wouldn't need time to work on it. In any case, this comment thread isn't the best place to discuss it. I'd be happy to talk elsewhere.
    Best,
    Garrett

17. **D R Lunsford**
    August 2, 2006

    Garrett,

    Yes not here. "The true metaphysics of y5 is hard."

    -drl

18. **Paolo**
    August 4, 2006

    In replay to Garrett,

    I do not know very much about the 20 proposals that did not make the second selection … but I do know "something" about some of those that did not make the first selection … in fact my project "Algebraic Quantum Gravity: Spectral Space-time via Non-commutative Geometry" (by the way: this was a project in "nc-geometry" +operator algebras+category theory) was immediately rejected without any explanation, apart from mentioning the fact that there were so many other much better proposals.
    I am actually very pleased to see that (at least for Quantum Gravity and TOE research) the funds went to some of the best experts in the field (some of them like C. Isham, L. Crane, F. Morkopoulou, O. Dreyer are some of the researchers that I admire the most).
    I am a bit disappointed to see that the grants (apart from Garrett's case of course!) actually went to people that are extremely famous, mostly with tenures and whose research programs do not need any particular help to get visibility, credibility … and most of all … funds …

    Considering the fact that the main aim of the Fqxi "foundation" was to support "out of box thoughts", "unconventional approaches" and even "research that would not be otherwise possible to fund" … I really find it difficult to see any relation with these words and "most" of the actual selected programs. The only natural pleasing answer is that … apart from those well-known names, already publishing heavily in the field, there are really no new thinkers that deserve to be supported.
    On one side, this is a really good news for universities in USA or UK … since they already got all the "interesting" people with the good ideas!
    On the other side, sincerely, I find it difficult to believe that there are no "original thinkers" in China, Japan, India, Korea (or "God forbid" … in some "developing" country …) that applied for a grant … and I find even more difficult to believe that they "do not" need support more that those selected!!
    I would have liked to know in advance that when I submitted my "low profile" project I was going to compete with Nobel prize level people looking for extra sources, through "mutual recommendation" … it could have saved me (and to many others) some precious time and probably some face!!

    In replay to the following phrase from Aguirre:
    "If there are foundational, unconventional projects (or researchers) in high-energy theory that you see languishing for lack of funds, please encourage them to apply for the next round!"

    For someone that is working (and probably trying his best to do some research) in difficult conditions of almost total lack of funds, heavy teaching load (with no real leaves, nor sabbatical) and in complete isolation (apart from an internet connection), it is a bit depressing to get this kind of answers … anyway thanks!

    In replay to Woit (and all the other people that have problems accepting funds from religious institutions … do they?):

    I really fail to understand the point here: I always considered spiritual and religious parts of life as natural as those related to art, literature, science and as a normal byproduct of human life (like breathing, eating, sleeping, enjoying conversation, play, music, knowledge ….).

I see that most of the complaints actually come from people with permanent positions or anyway with good jobs in American Academies and it seems strange that those people fail to recognize that they are actually sponsored by public funds coming from a vast majority of a “very religious” population and from an administration that is officially promoting not only “innocent” religious ideas, but also very much “radical and fundamentalist” approaches to religious life (let alone political practices!) … I would take their words seriously when they will change job … or change ideas!

Well … good luck to Garrett and to the other winners of the Fqxi awards.
And forgive me for a replay that is a bit “resentful”, but I could not resist 😉

Sincerely, Paolo

19. **Michael Edwards**
August 4, 2006

Although my sympathies (and congratulations on funding) lie with Garrett, my better judgment agrees with D R Lunsford that he’s off in the weeds. I think he suffers not so much from “quaternion disease” as from Lagrangian disease — the impression that what matters about a field theory is its (geometric and/or internal) symmetry algebra and its nominal Lagrangian as written in an introductory text. I recall getting a rude awakening from this illusion when I first tried to go from reading about non-Abelian gauge theories to trying to calculate anything in one.

I’m not a working physicist (or mathematician) and don’t claim to understand QFT even to the degree that a freshly minted Ph.D. from a second-rate university does, let alone people who can compute accurate QCD backgrounds for collider experiments. But you don’t really have to understand functional determinants and renormalization group flow to realize that the Lagrangian of an effective field theory may not look much like its Planck-scale antecedent. Take the example of Faddeev-Popov ghost fields (for mathematicians, more or less the Maurer-Cartan form on (the identity component of) the group of QCD SU(3) gauge transformations). They’re “unphysical” in the sense that they violate the spin-statistics theorem (among other things), so you rarely see them included in the headline SM Lagrangian — but good luck expressing anything quantitatively in QCD without them or their equivalent.

Is the entire fermion structure of the Standard Model just as “unphysical”, arising not from “algebraic logic chopping” of some set of geometrical objects but from something deeper such as the need to calculate past a wider redundancy in Fock space? I lack the techniques to state this question rigorously, let alone to answer it, but I can use it (along with other foundational questions about the QFT formalism) as a litmus test for ToE candidates. I’m looking for some combination of predictive and explanatory power, and I’m convinced that neither is to be found along lines of reasoning that focus narrowly on the symbols under the action integral.

So at best I think Garrett has wandered off in a different direction from the string theorists, and perhaps not as far (he is presumably not long-term dependent on “faute de mieux” grants for his livelihood). With apologies to Douglas Adams, the fundamental design flaws of his approach are completely hidden by its superficial design flaws. I say this having spent an unfortunate proportion of my life chasing similar dreams, and in hope that he will use some of the liberty that money buys to broaden his knowledge of how QFT calculations are actually made.

Cheers,
– Michael

20. **Christine Dantas**
August 4, 2006

Paolo wrote:

On the other side, sincerely, I find it difficult to believe that there are no “original thinkers” in China, Japan, India, Korea (or “God forbid” … in some “developing” country …) that applied for a grant … and I find even more difficult to believe that they “do not” need support more that those selected!!
I would have liked to know in advance that when I submitted my “low profile” project I was going to compete with Nobel prize level people looking for extra sources, through “mutual recommendation” … it could have saved me (and to many others) some precious time and probably some face!!

Dear Paolo,

You summarize my impressions when I saw the list of awardees. I am somewhat relieved that my decision to not submit my project to the FQXi grants was a good one (at least I was able to spare myself of further frustrations), although I was confident that my project did meet their criteria. Well, let’s keep on going. You have my sympathy.

Best wishes,
Christine

21. **Kea**
August 4, 2006

Thank you, Paolo. I couldn’t have said it so well myself. Of course I wish all the awardees the best in their researches, but I was stunned when I saw the list. My proposal was on pseudomonads in a quantum topos style unification. I was rejected on the first cut because “the subject matter of the proposal didn’t meet the criteria of the fqxi guidelines”. I prefer people to be honest.

22. **Christine Dantas**
August 4, 2006

Kea wrote:

Of course I wish all the awardees the best in their researches

Yes, I also wish them all the best of course. I look forward to learn about the new developments and results that these investigations will lead.

Christine

23. **Anthony Aguirre**
August 7, 2006

Dear FQXi applicants, well-wishers, and naysayers:

Your comments on this forum are valuable to us. A few comments in return.

First, believe me, we would have liked nothing more than to have had \$20m to give away, and to fully fund all of the initial proposals we received. Cutting these requests tenfold was very difficult for the panels, and they did their best using exactly the same criteria provided to the applicants in the RFP document. Note also that some very prominent researchers had unsuccessful applications — and we hear from them too!

Second, the two-part application process was devised in part to save the community a substantial amount of time in preparing lengthy and ultimately-unsuccessful applications. This succeeded, but other than additional money there is no way to mitigate the frustration of most applicants going unfunded.

Third, it should be kept in mind that even for established researchers, getting funding for foundational projects is not — at all — easy. Usually this sort of work is done "on the side" while supported by other more conventional funding, and part of FQXi mission is to bring this side-work into the fore.

Fourth, since from the outset we took care to make FQXi open to researchers worldwide, we were actually disappointed in the rather small number of applications from outside North America and Europe. Certainly there was no bias against them — in fact, the favorable buying power of the dollar alone gives such applications (from India, for example) a substantial advantage in terms of cost-effectiveness. We think many more are out there, and we will redouble our efforts to get the word out to such researchers in future grant cycles.

Finally, we are indeed aware of the difficulty of judging the value of truly original research proposals — as we *must* do in order to make funding decisions. We are working on ideas and strategies for this, and welcome constructive ideas along these lines.

24. **Michael Edwards**
August 7, 2006

Garrett,

Judging from the comments to an earlier posting ("Various and Sundry", July 21, 2006), I may owe you an apology for assuming (based on the paper you linked above) that you were only interested in the stuff under the action integral. I agree with your comment there that BRST is of "foundational importance". The construction you are looking for, in which the BRST operator is extended to form the exterior derivative on a gauge bundle, is being discussed in the comments to "P. University Press", and there's a link there to a paper by Schuecker which may help kick-start a literature search.

When I get a chance, I will try to summarize concisely where the Faddeev-Popov ghost field fits (a standard result; see for instance Peskin & Schroeder) and how it can be extended to the Maurer-Cartan form on the space of right-invariant vector fields on the gauge bundle (probably also nothing new, but I haven't found it in the literature).

Cheers,
– Michael

25. **Garrett**
August 8, 2006

Paolo,
Sorry about your proposal. I was also surprised to see so many familiar names on the list — I figured there would be more dark horses from all over the world. The only consolation I might offer is that they don't appear to have funded any category theory projects. It's not hard to see the category theory star rising though, so don't give up on it.

Michael,
If the biggest flaw with my theory is having a nice Lagrangian, I'll be very happy. And Faddeev-Popov ghosts are at the heart of what I'm working on, and I am indeed very interested in the Maurer-Cartan BRST construction. I've seen it here and there, and even used it in my own work, but my understanding is sketchy. I'll go look in the other comment thread immediately. Also, it would be great to talk about this at
physics forums
which I think would be a practical and appropriate place to discuss it. I'd love to see your summary there. Please send me an email so we can talk about it:
gar at lisi dot com
I've been away at my sister's wedding for a few days, but I'm back now for awhile.

Anthony,
Thanks!!! If you do check back on this thread, I have a question:
When do you think FQXi will be opening up its discussion forum?

26. **Tony Smith**
August 8, 2006

Garrett and Michael Edwards mention ghosts, BRST, and Maurer-Cartan.
A useful paper about that is
Geometrical reinterpretation of Faddeev-Popov ghost particles and BRS transformations
by Jean Thierry-Mieg (J. Math. Phys. 21 (12) December 1980 (2834-2838)
whose abstract states:
"… A classical geometrical interpretation of the ghosts fields is presented. BRS rules follow from the Cartan-Maurer fibration theorem. The statistics of ghosts are explained and the effective quantum Lagrangian is derived without factorizing the volume of the gauge group. Topologically nontrivial ghost configurations are defined. …".
The body of the paper says in part:
"… the connection 1-form … describes at the same time both the Yang-Mills gauge particle and the Faddeev-Popov ghost particle. With respect to a section, i.e., a gauge being chosen, the connection actually splits into the sum of two components:
the gauge field … which is horizontal
and
the ghost field … which is normal to the section.
The exterior differential … also splits, and its component normal to the section is … the BRS operator.
… the Cartan-Maurer structural theorem, which states the compatibility of the connection with the fibration, implies the BRS transformation rules of the gauge and ghost fields.
Moreover, the ghost does not contribute to the curvature 2 form (field strength) and may be thus eliminated from the description of the classical theory. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

27. **Anthony Aguirre**
August 8, 2006

Garrett:

The forums are on our current to-do list, and should hopefully be up in time for the fall.

best,

Anthony

28. **Michael Edwards**
August 8, 2006

Tony,

I think it's more accurate to say that the most fundamental form of the connexion on a principal bundle – a separation of the tangent space at each point into horizontal and vertical subspaces – is expressed as the Maurer-Cartan form (vertical projection) on the _bundle_. Its restriction to the vertical subspace is identified with the Faddeev-Popov ghost, because infinitesimal gauge transformations are the vertical subalgebra of the Lie algebra E of right-invariant vector fields on the bundle. ("Right-invariant" here means invariant under the right action of the bundle structure group, i. e., the gauge group.)

The connexion 1-form on the bundle pulls back on a local section to the "gauge" connexion 1-form on the base space. Because this pullback is defined using the connexion itself (it's dual to the "pushforward" of vector fields on the base space to "horizontal" vector fields on the bundle), you could say that it "splits out" the portion of the Maurer-Cartan form that acts on the horizontal subspace. But I would hesitate to use the word "normal" in this context because it doesn't have anything to do with metric structures or inner products. _Any_ connexion 1-form on the bundle is a projection from E to its vertical subspace, which is in turn the kernel of the "projection" (pushforward, really) of E to the tangent bundle of the base manifold.

Similarly, one can define an exterior derivative d_E: \Lambda^k -> \Lambda^(k+1) on the space of local-polynomial-valued alternating forms on E without essential use of a connexion on the principal bundle. The special case of a 0-form Q (e. g., a Lagrangian polynomial) yields the exact 1-form d_E Q, whose restriction to the vertical ideal of E is -s Q, where s is the BRST operator (I believe that the minus sign is the historical sign convention, if G&S and P&S are any guide). That's what is meant by the statement that -s is the "component normal to the section" of d_E, although "normal" is again somewhat misleading; the vertical ideal of E is well-defined irrespective not only of any metric-like structure but also of independent of the connexion.

The space \Lambda of local-polynomial-valued alternating forms on E, and the corresponding graded Lie algebra of derivations on \Lambda, is a powerful formalism for expressing the calculus of variations. The derivations on \Lambda can be extended to an even larger operator space via the wedge product and the Frölicher-Nijenhuis bracket. If I had to bet on a dark horse in the Theory of Everything stakes, I think this would be the mathematical formalism in which it ought to be expressed.

Cheers,
– Michael

29. **Garrett**
August 8, 2006

Nope. OK, Michael, can you send me an email and then I'll start a Physics Forums thread for this?

30. **Michael Edwards**
August 8, 2006

This is starting to feel inappropriate for Peter's blog, but I thought I should at least explain my comment on the Frölicher-Nijenhuis bracket and provide a reference.

Another term for a graded Lie algebra is "superalgebra", and the sheaf of alternating forms on a vector bundle is a classic example of a supermanifold. The subspace \Omega^1_* of \Lambda consisting of (vert E)-valued horizontal alternating forms on E can be given a Lie superalgebra structure via the Frölicher-Nijenhuis bracket, which coincides with the Lie bracket on 0-forms (i. e., polynomials taking values in vert E).

Better yet, the entire graded Lie algebra Der \Omega(vert E) of derivations on \Lambda, including (for instance) the BRST operator, can be identified with the direct sum of two copies of \Omega^1_*, one acting by contraction (the generalization of the inner derivative) and the other by Lie derivation (the generalization of the Lie derivative). See, for instance, this paper by Cap et al.:

http://citeseer.ist.psu.edu/cap94frlichernijenhuis.html

I think you might find that Der \Omega(vert E) is the Lie superalgebra of a supergroup of physical interest. This isn't the same thing as (the physicist's idea of) supersymmetry, because it doesn't involve spinors and doesn't establish any preferred relationship between particular odd and even elements of the algebra. One could try to mix form degrees of freedom of multiple ranks to get spinor fields via the Kähler route, but I think that's probably a dead end. Instead, I would expect you could exploit the isomorphism between (portions of) the _operator_ algebras on forms and on spinors in the course of BRST quantization.

I suppose that where this is headed is a generalization of the BRST quantization procedure to second class constraint systems. I wonder whether that would have any physical uses.

Cheers,
– Michael

31. **Michael Edwards**
August 8, 2006

Garrett,

I'm not really in the habit of online forums (I tend to drop in for a short while, use up my little store of insight, and go back to lurking) but feel free to write me at m.k.edwards at that webmail service that Google runs. Don't expect great things, though; I'm almost out of things to say already. 🙂 The rest is on the "P. University Press" thread, along with some pointers into the literature from Peter and Thomas that may take me quite some time to digest.

Cheers,
– Michael

32. **Tony Smith**
August 8, 2006

Michael Edwards mentions
"… Another term for a graded Lie algebra is "superalgebra" …"

and
mentions "… alternating forms … \Lambda …"
as
"… the mathematical formalism in which … the Theory of Everything … ought to be expressed.
ought to be expressed …".

I don't disagree with the general ideas, but as to more specific details I note that :

1 – Some graded Lie algebras are NOT "superalgebras", but are interesting Lie algebras that combine vectors, bivectors, and spinors, such as by the 5-grading
g = g(-2) + g(-1) + g(0) + g(1) + g(2)
where
g = E6 ————————————— the total Lie algebra
g(0) = spin(8) + R + R —————— the bivectors
dimR g(-1) = 16 = dimR g(1) ——— the (complexified) spinors
dimR g(-2) = 8 – dimR g(2) ——–– the (complexified) vectors

2 – If you want to include spinors, it is nice to extend beyond the exterior algebra \Lamda of alternating forms and go to Clifford Algebras and look at Clifford Modules, which are closely related to Dirac operators.

Also, with respect to my comment about Maurer-Cartan forms, BRS, etc,
my comment consisted of direct quotes from the paper of Jean Thierry-Mieg
at J. Math. Phys. 21 (12) December 1980 (2834-2838),
so any praise or criticism should be of Thierry-Mieg, and not me.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

33. **Michael Edwards**
August 8, 2006

Sorry for following up to myself (again) but this was too stupid to let stand:

> The subspace \Omega^1_* of \Lambda consisting of (vert E)-valued horizontal alternating forms on E …

This was a silly thinko. Horizontal forms on E annihilate vert E. I meant to write, \Omega^1_* is \Lambda(vert E, vert E), which is isomorphic to \Lambda(E, vert E) / Hor \Lambda(E, vert E). The horizontal forms constitute a subalgebra of \Lambda(E, vert E) and are defined independent of any connexion; presumably a choice of connexion can be used to select a unique representative of \Lambda(E, vert E) corresponding to an element of \Omega^1_*. Hey, look, another fiber bundle!

Anyway, I found the "standard" name for that connexion 1-form on the fiber bundle:
http://en.wikipedia.org/wiki/Ehresmann_connection
Whoever "Silly rabbit" at Wikipedia is, he's doing a bang-up job of overhauling their connection-related entries.

Cheers,
– Michael

34. **Michael Edwards**
August 8, 2006

Tony,

No personal criticism of you (or Thierry-Mieg) intended; it's just that the word "normal" gives me hives. I also have this irrational aversion to Clifford algebras, second only to my distaste for div, grad, curl and all that. This is part of the reason that I never made it as a physicist. But if you can find an elegant way to extend the Frölicher-Nijenhuis construction to Clifford algebras, I'm all ears.

Cheers,
– Michael

35. **Tony Smith**
August 9, 2006

Michael Edwards said "… But if you can find an elegant way to extend the Frölicher-Nijenhuis construction to Clifford algebras, I'm all ears. …".

For one example, consider hep-th/0112263
Geometric (Pre)Quantization in the Polysmplectic Approach to Field Theory
by Igor V. Kanatchikov

After using the Frolicher-Nijenhuis theorem with respect to a (Poisson-)Gerstenhaber algebra, Kanatchikov says
"… it was found suitable to work in terms of the space-time Clifford algebra valued operators and wave functions,
rather than in terms of nonhomogeneous forms and the graded endomorphism valued operators acting on them.
In general, a relation between the two formulations is given by the "Chevalley quantization"
map from the co-exterior algebra to the Clifford algebra …".

For details, see the paper itself.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

36. **Michael Edwards**
August 9, 2006

Tony,

On a first reading, the Kanatchikov paper is about as clear as mud to me. I fail to see why you would want to "generalize the cotangent bundle" in the direction he chooses, I have no idea which objects are defined solely in terms of the bundle structure and which ones require a connexion or a local section or a metric or a complex structure or a Minkowski background or God knows what else for their definition, and the presence of the word "heuristically" in the abstract is a major turn-off.

I could tolerate all that in a model that reproduces real live phenomenology, but I refuse to participate in any universe whose fundamental dynamics involve the kind of hocus-pocus which permeates this paper – including "Chevalley quantization", which seems to be nothing but replacing a symbol that has a well justified commutation property with one that doesn't. Given that there is so much that I don't yet understand about the calculus of variations on objects whose geometrical significance I can grasp, I am going to have to leave the work of "Cliffordization" to someone else for the present.

Which is not to say that I'm not still all ears for a Clifford-Frölicher-Nijenhuis construction that I have a prayer of understanding. 🙂

Cheers,
– Michael

37. **woit**
August 9, 2006

Garrett, Michael, Tony,

I'm loathe to discourage people from discussing things related to BRST, it's exactly the sort of thing I think physics needs more of. But I think you'd be better off discussing this privately, for one thing you could exchange properly TeXed documents…

38. **Michael Edwards**
August 9, 2006

Peter,

No worries. I sent my little notation crib sheet (basically a drastically condensed summary of parts of G&S with extensions in the Frölicher-Nijenhuis direction) over to Garrett already, and we'll see how far we can get privately on BRST-ish lines of reasoning. If anyone else is interested in playing with these ideas, write at m.k.edwards at google's webmail.

Perhaps we'll hare off into the direction of perturbative cohomology. Applying BRST quantization to the Hodge theory of elliptic complexes sounds like fun. Some elliptic complexes, including the de Rham complex, can be defined without reference to a metric. However, you also need an elliptic operator to select a unique harmonic representative of each cohomology class, and its definition involves an arbitrary choice of metric. So the theory lives on a GL^+_n principal bundle; varying this metric to recover diffeomorphism invariance requires a BRST counterterm.

Anyone know offhand what happens to Atiyah-Singer when the Laplacian is actually a hyperbolic operator?

Cheers,
– Michael

39. **Michael Edwards**
August 9, 2006

I should have known that Ed Witten was there first:

http://projecteuclid.org/Dienst/UI/1.0/Summarize/euclid.cmp/1104161738

But he doesn't seem to have known at the time quite how to formally define "instanton moduli space" as the quotient space of cohomology classes and replace the singular Green's function of the elliptic operator with a metric-fixing term and its ghost counterterm. If I were a real mathematical physicist I would try to pick up where Witten left off.

Cheers,
– Michael

40. **Michael Edwards**
August 10, 2006

Amazing. I start reading Witten's paper, I see a lot of index slinging and heuristics and wildcat guesses, I start to question his reputation for mathematical brilliance – and then he proposes an elegant resolution of the problem posed for Hodge theory by working in the Minkowski signature component of the frame bundle: relate two inner products on the _Hilbert_ space, one positive definite and one Lorentz invariant, via the time reversal operation.

I won't clutter up Peter's blog with the implications but they're quite enlightening, as is the central assertion of the paper: a generally covariant quantum field theory is one in which the stress tensor is a BRST commutator. In fiber bundle language, such a theory lives on the unreduced GL^+_n frame bundle of an orientable manifold; the infinitesimal form of general covariance states that the Lie derivative of the Lagrangian with respect to any vertical vector field on the frame bundle is zero. That simply means that dL is a horizontal form on the frame bundle; i. e., the restriction of dL to the vertical ideal (aka the BRST commutator of L) is zero.

So even a totally heuristic approach to writing down a Lagrangian candidate will succeed in producing a "generally covariant" (under infinitesimal changes of metric, not general diffeomorphisms) theory, as long as its energy-momentum tensor (the variation of L with respect to a change of metric, calculated the hard way) coincides with the BRST commutator of some element \lambda of the exterior algebra on the fiber bundle. Although its index-slinging formula may be totally unrecognizable, \lambda will differ from L only by a BRST-closed form in \Lambda, i. e., one whose exterior derivative is a horizontal form.

That brings me back on topic for Peter's blog. A sufficiently brilliant mind wandering in the string theory wilderness for forty years is likely to discover that some candidate manifolds and Lagrangians are less intractable than others. This may result in isolated flashes of insight like Witten's that unleash a flood of interesting mathematics. The problem is not string theory but the string theory monoculture in which the majority of sufficiently brilliant minds are all wandering in the same wilderness. My mind is not in that class, so I selfishly wish there were more people of Witten's caliber wandering in my bundle-theoretic-topology wilderness, in hope that one of them will lead me out someday.

Cheers,
– Michael

41. **Michael Edwards**
August 10, 2006

Oh, now that's a neat way of looking at it. QFT and GR are the same theory of general covariance, just written in "Dirac-Feynman" gauge (gauge-fixing operator d+\delta) and “Einstein-Hilbert” gauge (gauge-fixing term R). I bet that someone sufficiently clever could parameterize this to interpolate between them, the same way that in QED one can interpolate between Landau gauge and ‘t Hooft-Feynman gauge using the \xi parameter whose name I can’t remember.

42. **Garrett**
August 16, 2006

The FQXi grant awardee pages now have links to technical abstracts, which are much more revealing.

43. **Michael Edwards**
August 19, 2006

Having commented here on the Frölicher-Nijenhuis calculus, I thought I would point out that there’s a good exposition available online. The PDF of a book-length follow-on to Kriegl and Michor’s work (as exemplified by the Cap et al. paper), published by Springer as “Natural operations in differential geometry”, is at http://www.emis.de/monographs/KSM/. It does a very thorough job on the Frölicher-Nijenhuis bracket and friends, including applications on gauge bundles.

The angle from which the authors approach Lagrangians is different from Schücker’s, and they do not draw his connection between the BRST operator and the restriction to the vertical ideal of the exterior derivative on the bundle. Consequently, they do not seem to have applied the Frölicher-Nijenhuis calculus to the space of local-polynomial-valued alternating forms.

Extending BRST in this direction is proving quite enlightening. The BRST operator seems to be more closely related to Lie derivation with respect to the (connexion-dependent) vertical projection than to the (connexion-independent) bundle exterior derivative. In a quite rigorous sense it is complementary to the “exterior covariant derivative”, i. e., Lie derivation with respect to the horizontal projection on vect[P]. This makes it very useful for expressing variational principles on principal bundles, since the generalized Stokes theorem applies properly to the bundle exterior derivative, not the exterior covariant derivative.

Cheers,
– Michael

Susskind on KQED

July 31, 2006
Categories: Uncategorized

Someone wrote in to tell me that KQED this morning had Leonard Susskind on to discuss string theory and his book The Cosmic Landscape. Most of the program consisted of him promoting his usual line about the string theory anthropic landscape and how the fact that string theory is compatible with anything makes it a wonderful and exciting new way to do physics. He claimed that there is no longer a substantive split among bright physicists about the landscape, that the only split is over people's emotional response to it.

There were quite a few strange things in the interview that have little to do with reality. Susskind repeatedly claimed that string theory has a great deal of experimental support, saying:

More and more the things that string theory seems to say seem to jibe and coexist with the things that physicists and cosmologists see in the laboratory.

Near the end of the interview, when asked to cite some experimental evidence in favor of string theory he said that yes there was a lot of evidence including:

1. The existence of gravity.

2. The existence of particles.

3. The laws of the universe.

Quite remarkably he then went on to announce that QCD is a string theory and take credit for it, saying that string theory was "invented by Nambu and myself as a theory of protons and neutrons, an extremely successful theory of protons and neutrons". According to Susskind, string theory provides "the whole explanation of protons and neutrons and nuclear physics" and that "heavy ion collisions are best described in terms of string theory".

One questioner asked him about LQG, which he characterized as a "half-baked theory" that was "similar to string theory but not quite the same" and that "even its proponents hope that it is another way of expressing string theory."

And what of criticism of string theory? Susskind deals with this with purely personal attacks. The interview began with the following:

Michael Krasny: *Let's talk first of all if we can about string theory since you're kind of called the father of it and all that, I know you've been humble on that score, but it's deserved. Challenges to it, now it's being challenged left and right… ill-defined, based on crude assumptions.. tell us.*

Susskind: *You're talking probably aout some of the books and blogs that have come out in very very big criticism of it. Well, I think one would have to say that some of it is due to a certain kind of grumpiness of people who…um..*

Well, for example, there's one fellow who failed as a physicist, never made it as a physicist, became a computer programmer, has been angry all of his life that he never became a physicist and that physicists ignore him, so he's now taking out his revenge by writing diatribes and polemics against string theory.

Somehow I suspect this is about me. For the record I'm a faculty member in the math department at Columbia, in an untenured position with title of "Lecturer", where my responsibilities include teaching, adminstering the department computer system, and engaging in research. Susskind sounds a lot more angry than I've ever been, and I certainly don't feel that physicists are ignoring me.

He goes on to attack Lee Smolin:

There's another fellow who has his own theory, I won't tell you who his name is or what his theory is, but he writes lots and lots of theories and his theories go glub, glub, glub to the bottom of the sea before he even gets a chance to put them out there. Physicists don't take him seriously, he's angry and so he's also writing a book complaining…

Just completely pathetic.

Comments

1. **Eugene Stefanovich**
   July 31, 2006

   Peter,

   thanks for the link. Correction: the interviewer's name is Michael Krasny, not David Krasny.

2. **CapitalistImperialistPig**
   July 31, 2006

   Holy shit! ST is taking on water fast if Susskind is morphing into Lubos Lite.

3. **Arun**
   July 31, 2006

   CIP,
   LOL!

4. **Peter Woit**
   July 31, 2006

   Eugene,

   Thanks, fixed.

5. **Hmm**
   July 31, 2006

Hey Peter,

Where are all the promised posts on your brilliant research ideas? We've been waiting and waiting and waiting and waiting….Surely you're bursting at the seams to tell us. Captain my captain, give us poor brainwashed masses a direction of research that will lead to concrete falsifiable predictions for physics, and we'll follow!

6. **J.F. Moore**
July 31, 2006

Yes, please tell us about your career as a "computer programmer". What a strange guy. It's like some people aren't aware that we are now living in a world where lies are discredited rather easily.

But are you serious that his second ad hominem is directed toward Smolin? If it's a joke I don't get it. If it's real, that's just scary. Is he that ego-driven or does he just want to sell his book that badly?

Is this like the gambling addict who doubles down because otherwise he must admit failure?

7. **Rickkkkkkk**
July 31, 2006

Hmm, Peter has made just as many falsifiable predictions as string theory(well actually more if you count his predictions of various Lubos meltdowns and the like) and as a bonus he's managed to do so without an attack squad or a hype machine. Well done Peter, bravo

8. **Paul Frampton**
August 1, 2006

My good friend Lenny on radio to promote his book does a good job.
1. Origin of string theory. My first postdoc was with Nambu starting
late summer of 1968 when I believe I was first to inform Nambu of the
Veneziano model. Within a couple of months Nambu had string theory,
in 1968, but did not publish it. My friends Holger Bech Nielsen
and Lenny himself did it independently. Chronologically Nambu was first.
2. Popularity of string theory. While it may be the most likely theory for quantum gravity it is probable there will be no data on quantum gravity for a long time so more particle theory papers should emphasize quantum field theory without gravity, by duality the same theory.
3. Lenny's "experimental evidence" for string theory showed poetic license but he was merely agreeing hastily with the interviewer as time ran out!

9. **D R Lunsford**
August 1, 2006

Um, hey Lenny, remember Veneziano?

-drl

10. **Eugene Stefanovich**
August 1, 2006

This interview would be funny if it were not so scary and medi-evil.

11. **hack**
August 1, 2006

Lenny can see his expiration date rapidly approaching and the result is not pretty.

12. **damtp_dweller**
August 1, 2006

"Let's talk first of all if we can about string theory since you're kind of called the father of it and all that…"

Sorry, did I miss the email in which it was announced that Leonard Susskind was "the father of string theory?"

Regardless of one's view of the theory, surely that title goes to several people, none of whom are Susskind? I'm thinking along the lines of Polyakov, Michael Green, John Schwarz, and, to an extent, Ed Witten.

13. **deloprator2000**
August 1, 2006

In a way I understand Susskind. Think about it he considers string theory as his "baby". He helped bring it into this world, he helped develop it to what it is now, for better or for worse, as such he will have an emotional attachment to it. To see string theory dismantled or criticized especially when you've dedicated your life's work to it, must be very difficult. I could just imagine at the turn of the century a leading "ether" theorist talking trash about some young punk named Einstein.

14. **Eugene Stefanovich**
August 1, 2006

hack:

Lenny can see his expiration date rapidly approaching and the result is not pretty.

If it were just Susskind alone… I am afraid that he is correct in pointing out that all the best and brightest, like Weinberg, Witten, Gross, etc., basically agree with him that there is no way around the swampland. Now we are all condemned to work on the swamp drainage project. Anybody trying to escape (Woit, Smolin, who else?) will be punished.

15. **Chris Oakley**
August 1, 2006

Susskind can say what he likes, but an anthropic Weinberg? *That* is hard to deal with.

16. **Stalin**
August 1, 2006

physicists that use these indimidatory methods can now prevent a scientific discussion, but will not remain influential after retirement.
Good luck to Berja Lubos.

17. **Lubos Motl**
August 1, 2006

Lenny's interview is nice. His description of two critics of physics is more personal than what I usually like, but its content is absolutely true.

I also think that Susskind's summary of the situation with the anthropic principle was fair – so "congratulations, Lenny" is the only thing I can say.

18. **MathPhys**
August 1, 2006

Paul Frampton,

Isn't true that Nambu wrote a paper, typed it himself in a hurry, as he was about to fly back to Japan, and it was circulated privately at the time?

I recall an early string theorist saying that the manuscript was so full of typos, they had to guess that Nambu frequently used his left hand shifted to the right, so many of his f's were are actually d's, and so forth.

Does anyone know if that manuscript is still around?

19. **MathPhys**
August 1, 2006

It's too bad that only Susskind is left to call himself "the father of string theory", as both Holger Nielsen and Nambu, both of whom are brighter and deeper physicists, are no longer in the limelight.

20. **Chris Oakley**
August 1, 2006

Good Morning Lubos,

Luckily I did not have a mouthful of coffee when I read your comment, otherwise it would have ended up sprayed all over the keyboard.

"His description of two critics of physics is more personal than what I usually like"

I assume that you meant "less" rather than "more".
Consult a Czech/English dictionary.

21. **boreds**
August 1, 2006

damtp_dweller

The statement about Lenny being the father of ST confused me when I heard it a while ago—but I think there is indeed a very early Susskind paper (1969?) on string theory as a model for the strong force. I don't think it's listed on SPIRES though, and I don't know why. I can try and dig out the journal reference but probably other people know it.

Anyway, he apparently lost interest in the subject for a while.

22. **Stefan**
August 1, 2006

Peter,

I came across Lubos's post on N.E.W. on "Mixed States". I read the criticism posted on his blog (17-page review of N.E.W) – well, most of it anyway; I then posted the following reply:

————————————————————————————–

Name: Anonymous

Dear L. Motl,

You recently posted your commentary on N.E.W. on "Mixed States". May I request a clarification (and if possible, elaboration) of the following remark:

"He ["P. Woit"] also assumes that string theory suffers from many problems whose absence has been [??] more or less [??] rigorously proved…"

When one writes "rigorously" does that imply "more or less"? Exactly what problems were enumerated and how were their absences RIGOROUSLY proved within string theory?

————————————————————————————–

The point of this exercise was: L. and such like will always bark, does that mean we should suspend our own work to respond to each and every diatribe they may (or can possibly) throw at us?

I don't believe he is taken as seriously by his peers as he takes himself… I don't mean that as an insult or a put-down, but the kind of ranting and raving he seems to enjoy engaging in does little service to the cause of science – or rational discourse, for that matter – as a whole.

Please ask him to reply to the community asap; if he is unable to do so satisfactorily we should take out that old "crackpot index" and ask him to read it thoroughly before he writes his next review / diatribe.

[He can write anything he wishes in his blog-journal – that is his own personal column.]

Stefan

23. **plank**
August 1, 2006

“Well, for example, there’s one fellow who failed as a physicist, never made it as a physicist, became a computer programmer, has been angry all of his life that he never became a physicist and that physicists ignore him, so he’s now taking out his revenge by writing diatribes and polemics against string theory.

Somehow I suspect this is about me.”

Maybe it’s about Chris Oakley, given that you are not a programmer by trade.

http://www.cgoakley.demon.co.uk/cv.html
http://www.cgoakley.demon.co.uk/qft/

Anyway, these comments (personal nature) are irrelevant.

24. **Chris Oakley**
August 1, 2006

Hi plank,

I wish that your theory was correct, but I doubt it.
First, Peter argues the case against S.T. more eloquently, more knowledgeably and at greater length. He has, after all, written a book on the subject, and secondly, he has a university post meaning that he is more likely to be taken seriously and will therefore be perceived as more of a threat.
On the subject of the book, have a look at LM’s 17-page “critique”, which proves (?) *inter alia* that LM has now mastered the difference between the English and American spellings of the word “colour”.

25. **ksh95**
August 1, 2006

Things like this are not good for HEP. Unfortunately (or maybe fortunately) the people holding the purse strings are rarely as stupid as some would like to think. And even worse than that, it doesn’t take a genius to see what is happening.

> “…what do you think of the people who criticize your theories…the one guy is a poo-poo head who smells and the other guy is a doo-doo brain with big ears…”

Any one with half a brain will spot emotional immaturity and non-scientific arguments a mile away.

26. **woit**
August 1, 2006

Paul,
What was bizarre about the Susskind interview was not his claim that he and Nambu were responsible for string theory, but his claim that they were responsible for a successful theory of the strong interactions, e.g. QCD.

ksh95,
The thing is, the interviewer didn’t ask Susskind what he thought of the critics of string theory, he asked him what he thought of the criticism of string theory. His failure to answer this, and his decision to instead go for personal attacks on the people making the criticisms, speaks volumes.

27. **King Ray**
August 1, 2006

Peter, excellent blog today. Keep up the good fight!

28. **Paul Frampton**
August 1, 2006

MathPhys:

You are confusing two different contributions by Nambu. By origin of string theory in 1968 I meant rewriting the Euler B function as an infinite number of SHOs. The later contribution in 1970 by Nambu was writing the string action as the area of the world sheet. He did type (with many typos!) some notes on that which I suspect is what you remember. That action was discovered independently by another Japanese theorist Goto in 1971.

Yoichiro Nambu is the deepest theorist of my 130 collaborators. I wrote one paper with him on string theory in 1970. Writing papers with Holger Nielsen was also remarkable for his exceptional creativity. I have never written a paper with Lenny but have talked with him about physics. We are on opposite coasts!

Peter Woit:

Nambu, though not Lenny, did contribute significantly to the birth of QCD.

29. **TomB**
August 1, 2006

I didn’t realise physicists have so much in common with structural engineers. As any structural engineer will tell you there’s only two kinda people – structural engineers and those that wanna be.

Come to think of it – its like the irish on St Patricks day.

Regards

TomB

30. **Peter Woit**
August 1, 2006

J.F. Moore,

It’s not a joke, that second piece really was about Smolin. I didn’t bother to transcribe it, but Susskind went on to make up some extreme statement and attribute it to Smolin, then use it to claim that no one could take Smolin seriously. Standard straw-man argument tactic.

31. **Mahndisa**
August 1, 2006

08 01 06

I had no idea things were so NASTY! It disturbs me that this type of dialog is occuring when learning should be the goal. Geesh! 🙁

32. **LDM**
August 1, 2006

“Well, for example, there’s one fellow who failed as a physicist, never made it as a physicist, became a computer programmer, has been angry all of his life that he never became a physicist and that physicists ignore him, so he’s now taking out his revenge by writing diatribes and polemics against string theory.”

The reality is that if Susskind thinks string theory has experimental support, then “failed physicist” is in fact a description of him, since string theory clearly has no such experimental evidence.

But then again, this goes to the heart of the matter. If Woit is correct, then there are a lot of string theorists, “physicists” if you will, that are not really doing physics.

33. **JPL**
August 1, 2006

“Near the end of the interview, when asked to cite some experimental evidence in favor of string theory he said that yes there was a lot of evidence including:

1. The existence of gravity.

2. The existence of particles.

3. The laws of the universe.”

My-oh-my! Isn’t the existence of Susskind enough evidence in favor of Strings and all that other “SUSSY kind” of theories? Who needs any more evidence? Give it up…

34. **John A**
August 1, 2006

> Near the end of the interview, when asked to cite some experimental evidence in favor of string theory he said that yes there was a lot of evidence including:
>
> 1. The existence of gravity.
>
> 2. The existence of particles.
>
> 3. The laws of the universe.

I trust you’re not paraphrasing this, but someone somewhere has inhaled when they shoudn’t have…

35. **Another Physicist Turned Programmer**
August 1, 2006

Susskind said: “Well, for example, there’s one fellow who failed as a physicist, never made it as a physicist, became a computer programmer…” I took offence to this because it’s arrogant and assumed that just because somebody doesn’t want to do the physics that’s being done in the market, he/she has failed in physics. More often than not, it is the failure of practising physicists to attract physics graduates with physics research that’s exciting and worth doing. Bearing in mind the low pay and hard work that must be endured by most practising physicists as compared to other careers, a physics graduate often faces a hard choice whether to remain in physics, or to switch career. With the current theoretical high energy physics job market being dominated by string theorists, it’s not surprising that a physics graduate who doesn’t believe in string theory (and other “hot” topics being offered in the physics job market) would choose not to waste the rest of his/her life doing something he/she doesn’t believe in. The “failure”, I believe, lies with the inability of current practising physicists to offer research which is believable. Theoretical physics has lost its way and only the die-hard stubborn believers of the current fads in physics (like Lubos) choose to remain. If this is a measure of their “success” in physics, I want no part of it.

36. **MathPhys**
August 1, 2006

Paul,
Thanks for the clarification. Do you know if Nambu’s notes (with typos) are available? I know he’s retired since a long time, but is he still in Chicago?

37. **Mark**
August 1, 2006

Peter, thanks for the great post and the link to the interview. The crux of the whole matter seems to me to be where Susskind says: “…quantum chromodynamics is a string theory…” and where in he then connects all of the experimental successes of QCD to string theory in general. While I am aware of some work in this direction, I was not aware that QCD was considered a string theory. Nor that any of its successes could be claimed as “MAJOR successes” for string theory. How close to the truth is Susskind? Do most string theorist agree with him? Or do they have other successes that they like to point to? Thanks again.

38. **Peter Woit**
August 1, 2006

Claiming the experimental successes of QCD as successes of string theory is just absurd. There are no experimental successes of string theory to point to.

Susskind's original work on string theory was designed to use it to provide a theory of the strong interactions, but this failed completely. We understand the string theory Susskind was a codiscoverer of, we understand QCD, and they're quite different theories. After the development of QCD in 1973, many people (including Susskind) worked very hard on trying to find a different string theory, one what would be a dual to QCD. One of the main insights of this period was that of 't Hooft, that things simplify at large N (=number of colors), and that this is the most promising case to look for a relation to string theory.

In 1997 Maldacena was able to come up with a proposed string theory dual not to QCD, but to N=4 (different N, number of supersymmetries) supersymmetric Yang-Mills. This led to a lot of hope that similar techniques would soon lead to a string theory dual of QCD, which is Yang-Mills with no supersymmetries. Nine years later, this still remains to be found. No one has a string theory dual to QCD.

Susskind is claiming to have the solution to a problem that is definitely not solved, one people have been working on for thirty years.

39. **hack**
August 1, 2006

This is rather amusing: Susskind has borrowed another page from the Motl playbook, apparently editing his own Wikipedia page. He deleted reference to his book's sales figures, and posted verbatim a lengthy, gushing review of his book.

40. **Paul Frampton**
August 1, 2006

MathPhys:

In "Dual Resonance Models" (Benjamin, 1974) Chapter 4 reference 23:
Y. Nambu, lecture notes prepared for the Summer Institute of the Niels Bohr Institute (SINBI), (1970). I did not keep a copy of this paper which explained the string action. In 1974 I had copies of all the hundreds of papers cited in DRM including it. In 1979 I jettisoned them all when DRM was remaindered and string theory seemed finished. The story does not end there. The same book twelve years out of date was reissued by World Scientific in 1986 and sold three times as many. How to explain that? Nambu moves between Chicago and Japan. He might have a copy.
Otherwise I cannot help. Sorry! Best regards.

41. **L. Riofrio**
August 1, 2006

"Never made it as a physicist, became a computer programmer.." That failure Bill Gates has written another book? (Disclaimer: The writer took a theoretical physics class from Susskind and received an A.)

42. **MathPhys**
August 2, 2006

Paul,

I have a copy of the original edition of your book.

Thanks.

43. **Peter Woit**
August 2, 2006

hack,

According to the latest version of Susskind's Wikipedia page, he and Michio Kaku are the co-discoverers of string theory, and his "Cosmic Landscape" book is a #1 bestseller.

44. **Chris Oakley**
August 2, 2006

I am surprised that Susskind's Wiki page fails to mention his other achievements, i.e. that he

• Developed a cure for cancer
• Found the *Lost Chord*
• Discovered Penicillin
• Invented Quantum Mechanics
• Determined the structure of DNA
• Invented the steam engine.

45. **MathPhys**
August 2, 2006

I just listened to Susskind's interview, and I found his claim (near the end) that string theory has had "lots and lots of great successes", then starting to talk about strong interactions, to be indeed very strange. It is simply technically inaccurate.

Incidentally, he also claims to be behind 1. the assertion that there is no loss of information in black holes, and 2. holography, both of which are definitely due to G 'tHooft.

46. **Haelfix**
August 2, 2006

1) The assertion that there is no loss of information in black holes.

Well, might as well chalk up 80% of the theoretical physicists living at the time when Bekenstein and Hawking made their inroads on blackholes, b/c it was more or less *the* question at the time. Picking one side or the other isn't exactly a great stride. You might ask who coined the question to begin with. Afaik, that comes from Roger Penrose but I might be mistaken.

If you want more specific details, I suspect you could pick any number of different authors who tackled the problem.

Incidentally it is still an open question, albeit one with a number of competing solutions (some of which say involve the the Strominger-Vafa calculations on stringy blackhole mechanics) and the plurality of people feel its no loss.

47. **Paul Frampton**
August 2, 2006

MathPhys:

Perhaps you should hang on to it. The 1974 book sold only 1200 but the
1986 reissue sold 4000+; the 1974 book has been widely stolen.
To answer your original query, the paper is reproduced in "Broken Symmetry" (World Scientific 1995). To the bemusement of my office mates Nambu would come to my postdoc office to tell me his thoughts.In 1969 he told me the action for the string. The dialogue went essentially like:
Frampton: " I have to say it is not completely obvious to me why that is the correct action." Nambu: "But, Paul, you know very well that the action for a particle is the invariant length of its world line" Frampton:" I understand! It is obvious that invariant area is correct." It is now discussed in textbooks.

48. **MathPhys**
August 2, 2006

Paul,

What can I say? Y Nambu is a great man. I wish someday he would write his autobiography, but I think he's too modest to do so

I just checked that the Benjamin edition of your book is where I thought it is!

I also just checked that my copy of "Dual Models", a collection of Physics Reports, edited by Jacobs and published by North Holland in the 70's is also where still there.

I bought it (brand new) for about $6.00 in 1982. There were about 10 copies in the warehouse where I bought it. North Holland dumped the whole edition because it was not selling. I wish I bought the whole lot.

49. **MathPhys**
August 2, 2006

As I leaf through physics reports on string theory from the 70's, I'm struck by the beauty and depth of string theory, even in those very early days.

I know that 30 years down the track, it still has no experimental evidence, that it works only in higher dimensions, that something is wrong about susy, that compactification is an unnatural idea, and above all, I'm totally put off by the arrogance and hype of people like Motl.

But there is something about string theory.

By comaprison, I'm afraid I find that LQG is not only half baked, as Susskind says, but at most 1 per cent baked. There is just no comparison.

50. **D R Lunsford**
August 3, 2006

MathPhys,

I too was struck by it when I came across it in the 80s – by its colossal ugliness and metaphysical pointlessness (sorry). That is – if the idea of matter had failed, then it was not the configuration of it that was responsible, rather, the very idea of an exacly localized "thing" of *any* dimension at all – and it was obvious how it would fail. Also, it was profoundly anhistorical – one, by dredging up Kaluza-Klein theory, which was rightfully dead (Pauli), and second, by ignoring the real problem in quantum theory, the idea of measurement. It was also totally opposed to the more geometrico of GR in spite of its pretensions. Not only was it never a real candidate for a "theory of everything" (who ordered that?), it was completely incompatible with BOTH GR and QM – it was, and is, a theory of nothing. What on Earth were people thinking to believe in such a theory?

-drl

51. **Juan R.**
August 3, 2006

Chris Oakley said:

> I am surprised that Susskind's Wiki page fails to mention his other achievements, i.e. that he
>
> • Developed a cure for cancer
> • Found the Lost Chord
> • Discovered Penicillin
> • Invented Quantum Mechanics
> • Determined the structure of DNA
> • Invented the steam engine.

Humility?

Juan R.

Center for CANONICAL |SCIENCE)

52. **Lubos Motl**
August 3, 2006

Dear Stefan,

"more or less rigorous" means "rigorous" according to the standards of theoretical physics. This misunderstanding of yours is indeed another example of your breathtaking ignorance that all but guarantees that you will probably always be nothing more than a joke.

I wish you happy interactions with other idiots near the bottom of the sea.

Best
Lubos

53. **nigel cook**
August 3, 2006

"… it is now hard to set up a clear criterion for what is crackpotism (should I delete Lenny Susskind's comments if he decides to write in some day?)." – Peter, http://www.math.columbia.edu/~woit/wordpress/?p=128

I feel sorry for Professor Susskind having to finally respond to your criticisms of his theory, and doing it personally. His theory has received all the mainstream limelight for twenty years and he is promoting his very first book, criticisms are surfacing because now some people realise that it may not be the most helpful theory.

I watched him on TV somewhere and he is a really nice, down to earth person, actually he is an ex-plumber from New York who started physics a bit later when he went to college to do a gas installation course.

So if his string theory is full of holes and leaks, then he is the best person to fix it.

54. **hack**
August 3, 2006

'"more or less rigorous" means "rigorous" according to the standards of theoretical physics.'

Which more specifically means "can't be disproved with five minutes of hand-waving argumentation".

55. **David**
August 3, 2006

I can't help but notice that Motl again avoided the question that Stefan posed, i.e. what are the problems that Peter enumerated that Motl claims have been proved absent. If such proofs exist, Motl would do well to tell us about them rather than simply insult Stefan. Such a reply would strengthen string theory's position vs. Peter's criticism. Dare I suggest Motl adopts his position because he can't provide such proofs?

56. **Benni**
August 3, 2006

At a speech in munich I have asked Suesskind in person:
"When we are in the situation that string theory can describe any physics we want, would it not be better to accept this state of string theory and search for alternative theories?"
Suesskinds answer was:
"Yes of course. Of course. If you have one tell us of it"
I think this was a reasonable sentence.

57. **Thomas Larsson**
August 7, 2006

But there is something about string theory.

MathPhys,
There is something about CFT – it is the correct, and most likely the ultimate, theory of 2D phase transitions. This does not mean that it is the right theory of 4D gravity. Besides, it is not a theory of everything even in statphys, since it does not say anything about the physically more interesting 3D case.

P. University Press

August 1, 2006
Categories: Not Even Wrong: The Book

I really am trying to ignore Lubos, but there's just too much material…

Back in early 2004, after it became clear that Cambridge University Press was very unlikely to ever publish Not Even Wrong due to intense opposition from string theorists, I tried sending the manuscript (together with the Cambridge referee reports) around to a few other university presses to see if any of them would be willing to publish it. The response I got from two editors at well-known presses was positive comments about the content of the manuscript, but:

I think it's too controversial for a university press to publish.

from one, and from another

it is extremely unlikely that a proposal as controversial as yours would be accepted by the .

This made clear exactly how much of a "free marketplace of ideas" exists for debate about string theory within this part of the publishing world.

An editor at Princeton University Press wrote back after considering the manuscript for a week or two with a form-letter rejection informing me that "we must often forego formal review of promising manuscripts or proposals such as yours". I assume that, as I expected, the editor had discussed the manuscript with one of the local string theorists and thus been convinced not to pursue it.

With Roger Penrose's help, finally late in 2004 the British publisher Jonathan Cape bought the book, planning to publish it in Britain and sell the U.S. rights to an American publisher. During the first part of 2005 I worked a bit more on the book and it was copy-edited, and by the early fall the people at Cape were in negotiations with various possible US publishers, negotiations that I had little to do with. In November the editor at Cape told me that Princeton University Press had rejected the book as "too controversial". The next month US rights were sold to Basic Books.

I had no idea about this at the time, but it seems that someone had advised Princeton that the appropriate person to review this kind of manuscript and give an unbiased opinion about it was a Harvard string theorist with a well-known blog named Lubos Motl. Lubos has now posted his report, together with the proud claim that "a serious publisher whose name was edited used to scrap the project." He cleverly hides the true name of the publisher in question as "P. University Press".

The report makes clear what Lubos was going on about in some of the incomprehensible parts of his Amazon review. I responded to that review here, but couldn't even figure out a lot of what he was talking about there. With his detailed report with page numbers, this is now clear.

He was definitely on his best behavior. The report is not obviously a rant, and even includes some positive comments. He carefully went through the manuscript making many sorts of copy-editing suggestions (e.g. changing English spellings to American) and suggested a large number of rewordings of the manuscript that would make what it said agree with his vision of reality (but not mine).

Anyone interested can go through the report, compare it to the book and judge for themselves whether Lubos's extensive criticisms make much sense. Responding to his 17 pages filled with misinterpretations of what I wrote and tendentious claims about string theory is something I don't have the time or energy for, but I'll respond to his summary where he says that the book should be rejected because of its "many serious and elementary errors." He lists these as:

1. I don't know the difference between a GeV and a TeV. This is based on one typo, on page 32, where, after writing that the center of mass energy is at the LHC is 14 TeV, I mention that it might be possible to double this energy by doubling the strength of these magnets, and "28 GeV" is an obvious typo for "28 TeV". This typo is fixed in the US edition, thanks to the fact that he makes this argument against the book in his Amazon review.

2. He objects to my pointing out (page 179) that in a theory with broken supersymmetry the vacuum energy scale is too large by a factor of 10^{56}, wanting me instead to say that supersymmetry "improves" the vacuum energy problem with respect to non-supersymmetric theories by a similar size factor. What I wrote is correct.

3. On page 35 I mention that the neutrinos produced by a muon collider interact weakly, so will go through the earth and produce a radiation hazard when they emerge many miles away. Lubos claims that this is wrong, that "neutrinos with hundreds of GeV of energy interact strongly". This is nonsense. What he has in mind though is not really a "strong" interaction strength, but an electromagnetic interaction strength. He's right that at hundreds of GeV (way above the W and Z masses), there is electroweak unification, and the weak interaction and electromagnetic interaction strengths are similar. However, he seems to be making an elementary mistake: the neutrinos involved will be hitting a fixed target, so the energies involved will be much lower.

4. He repeats a mistaken comment that I once made on my blog about about SU(2) and SO(4), one that has nothing to do with what I write in the book. His excuse for introducing this is that on page 49 I refer to "axes of rotation" in 4 dimensions, complaining that I should have explained that in 4 dimensions rotations are specified by choosing not a one-dimensional axis, but a two-dimensional plane. It's quite true that I was simplifying things, not explaining that in N dimensions an "axis of rotation" is N-2 dimensional. Explaining that more carefully was not something I wanted to get into. Perhaps he's right that it would be better if I put "axes" here in quotes to keep people from making the wrong assumption that he's making.

5. He finds something wrong with the fact that even though I explicitly say that the physical Hilbert space is the trivial representation of the gauge group, I speculate that understanding the non-trivial representations of gauge groups is an unsolved mathematical problem whose solution might tell us something interesting about gauge theory. This is clearly labeled as speculation and perfectly accurate as written.

Anyway, now I know why Princeton rejected the book, although I still have no idea who put them up to choosing Lubos as a referee.

For more about Lubos and the controversy over string theory, there's an article in the Frankfurter Allgemeine Zeitung (in German). Lubos comments that "virtually all well-known theoretical physicists" think as he does, but that only he (together with Susskind) is willing to fight compromise with very stupid people and crackpots like me. He warns "to the polite big shots: the more silent you will be the more loud the blunt opinionmakers such as Susskind or your humble correspondent will have to be."

Comments

1. **Chris Oakley**
   August 1, 2006

   Anyway, now I know why Princeton rejected the book, although I still have no idea who put them up to choosing Lubos as a referee.

   I find this puzzling too, as sending *Not Even Wrong* to Lubos is a bit like sending *The Da Vinci Code* to Torquemada.

At the end of the day, though, publishing is a *business* … even without any referee reports it was obvious that you had the credentials to write such a book, and the question really was, would people buy it? And as *The Da Vinci Code* proved, even things that are arrant nonsense can sell if they are controversial.

2. **LostHisMarbles**
August 1, 2006

Just a brief comment about particle beams AFTER they go through particle detectors ~ "radiation hazard". (One can have in mind neutrino beams.) The beamlines are typically angled to that the beams go flying up in the air and enter the atmosphere and outer space. This is certainly the case at Fermilab, where the beamlines are angled upwards. By the time the neutrinos etc clear the FNAL boundary, they are in the air above any homes outside FNAL. (No skyscrapers there.) No radiation hazard. I believe there is a no-fly zone where airplanes stacked for O'Hare do not fly, to avoid their electronic instrumentation being hit by the beams. I have no doubt the ILC or a muon collider will be designed the same way. Someone from FNAL can comment in more detail.

3. **woit**
August 1, 2006

Chris,

University presses are funny businesses, not completely driven by the profit motive. It is generally part of their mission to have high intellectual standards and sometimes publish things that might not make much sense financially. In the case of my book, the controversial nature would suggest that it was likely to sell better than much of what they publish and might make them some money. The argument against publishing it would be that it didn't meet their intellectual standards, and this is what a reviewer is supposed to provide an evaluation for.

4. **Aaron Bergman**
August 1, 2006

Geez. My review is ony 11 pages right now….

(Which I will finish up eventually, but right now I'm focussing most of my time on Hochschild cohomology.)

5. **Lubos Woit**
August 1, 2006

well, at least now you can be sure that lubos wasn't lying when he said that he has read your book.

6. **Santo D'Agostino**
August 1, 2006

Lubos Woit,

It is dishonest to comment on a MANUSCRIPT and claim that it is a review of a BOOK.

7. **Kea**
August 1, 2006

Anyway, now I know why Princeton rejected the book, although I still have no idea who put them up to choosing Lubos as a referee.

Maybe they support the book, and were cleverly using their position to garner great publicity for your book via Lubos!

8. **Tommaso Dorigo**
August 1, 2006

Peter,

I have to say that, while I do think it is a proven fact that the trouble your manuscript faced when trying to become a book is almost entirely due to the string mafia, I also believe that the point you seem to make at the start of the post above is prone to criticism: you seem to imply that when somebody says "this book is too controversial" (s)he means "this book goes against the mainstream thinking of the big mushrooms and it would bother them".

I think there are two separate issues. We agree on the mafia, but I think (some) University Press editors might be honest when they say they prefer to avoid publishing divulgation material which is so focused on criticizing a theory en vogue.

The fact that there was quite a bit going on under the tables during the review process of course might make the above detail irrelevant, given the direct attempts at having the book rejected. But in principle, UPs might be right if they think scientific controversy is better solved at the blackboard than in a media fight… Indeed, controversy usually sells well, as somebody pointed out above – but UPs are right in ignoring that. You said it – there aren't just business issues there.

That, of course, does not mean the book wasn't needed or useful… Quite the opposite. Thank you for writing it!

T.

9. **LDM**
August 1, 2006

Form Lubos review: *I find it completely necessary to mention the names of Andrew Strominger and Cumrun Vafa – the authors of the pioneering work that has shown that string theory gives the right value for the black hole entropy.*

What was actually discussed by Strominger and Vafa: *The Bekenstein-Hawking area-entropy relation $S_{BH}=A/4$ is derived for a class of five-dimensional extremal black holes in string theory by counting the degeneracy of BPS soliton bound states.*
http://arxiv.org/abs/hep-th/9601029

This is definitely not the same as what is suggested by Lubos — that the result applied to all black holes (whether charged, uncharged, or rotating) — which we know was the original Hawking result. In fact, read the Strominger and Vafa paper, look at the result, and you see it does not apply for uncharged (electric charge) black holes…It also relies on the conjectured existence of Axions and that the black hole has large Axion charge.
Sadly, the same tremendously overstated misrepresentation appears on Vafa's own web page:
http://www.physics.harvard.edu/people/facpages/vafa.html
Among these one can name the discovery of Strominger and Vafa that the Bekenstein-Hawking entropy of a black hole can be accounted for by solitonic states of string

theory and also the relation between geometry and field theories that arise through string dualities, a topic known as "geometric engineering of quantum field theories".

The Lubos review is neither objective or balanced…but that is not new for him.
What is disturbing is the thought that Princeton may have actually relied upon his review.
But don't worry, DOVER will pick up N.E.W. and re-print is as a classic 🙂

10. **Peter Woit**
August 1, 2006

Tommaso,

You may be right that part of the concern with "too controversial" is a fear of more heat than light being shed. In any case it's hard to know really what is behind editor's decisions. They're often not going to give truly honest reasons in a rejection letter, for lots of reasons, including simple politeness.

In this case though, from going through the process with the editor at Cambridge, I think I can see the problem that university press editors faced when considering the book. The book makes strong claims that some very accomplished (as well as powerful) people are wrong about something, and the issues involved are both highly technical and understood by relatively few people. The editors involved don't have the background to be able to decide for themselves how good my arguments are. When they ask experts, they find non-string theorists willing to support what I have to say, and string theorists often vehemently claiming I don't know what I'm talking about.

They end up in a very uncomfortable position of having to decide whether to put some of their reputation and the reputation of their institution behind me, without being able to be sure whether I'm right. In addition, these presses often have some faculty body that has to sign off on publication decisions, so the editor has to worry about whether he or she can convince them also. Not an easy decision to make.

So, I'm not too surprised that these university presses didn't want to get involved. I am surprised that Princeton University Press did decide to get involved to the extent of commissioning a review, then chose someone obviously inappropriate to do this.

11. **CapitalistImperialistPig**
August 1, 2006

Peter,

No doubt there are many of LM's comments that are trivial or even silly, but a few have apparent substance. I'm thinking of the critique of your statement on p 159 of the manuscript "string theory is not a background independent theory." I looked at the relevant chapter of Polchinski, and he does indeed claim that ST is not restricted to just one background. Could you clarify this issue?

Do you plan respond to his more substantive points?

12. **Peter Woit**
August 1, 2006

CIP,

I hadn't planned on going through Lubos's whole document and responding to all it, I just don't have the time, and mostly I don't think it would be very enlightening.

As for the "background-dependent" business, you could consult extensive battles over this between string theory and LQG proponents on this blog and elsewhere (don't even think of starting this up again Who or Aaron…).

The fact of the matter is that to even write down at all what you mean by a string theory, you have to first choose a background. Different backgrounds give different physics. This is what I mean by "background-dependent". What I wrote is completely accurate. String theorists have an argument, that infinitesimal deformations of the background space still leave you in the same theory, just a different state.

One could go on and on about this issue, it quickly gets confusing because string theorists love to mix up what is actually known and corresponds to something they actually can understand and write down, and what they don't understand, but hope to be true, with various degrees of evidence for the hope.

I really, really, really don't want to try and moderate the N'th unenlightening discussion of this issue here. If people absolutely can't restrain themselves, go ahead and provide a link to a discussion of this somewhere else that you agree with. Just don't try and fight out the issue here.

13. **Energex42**
August 1, 2006

Hey, I saw it in Amazon.com that Lee Smolin is coming out with a new book in September. Curiously it is about String theory as well. Not to worry, I will buy both.

14. **Bee**
August 1, 2006

Hi Peter,

I read Lubos review. I found it very nice actually. If I think of some reviews I receive on my papers, this is a very well founded, reasonable, though biased list. I can't avoid having the impression that the publisher was only looking for a confirmation that the book would be too controversial, otherwise the mentioned points could easily have been revised.

@Energex,
Lee's book is not about string theory. It's about the trouble with physics. Reading Peter's last post, it seems that's gotten more of a trouble with physicists lately.

Best, B.

15. **Kasper Olsen**
August 1, 2006

Dear Peter,

You're saying that: *"different backgrounds give different physics. This is what I mean by "background-dependent".*

Could you please make more precise the definition of the term "different backgrounds"?

(1) Are you claiming that the background g'_{ij} = exp(2)g_{ij}, for example, will give different physics from the background g_{ij} ?

(2) And are you claiming that the sum over topologies (of the world-sheet) is not determining the interactions (of the superstring)?

(3) And – in relation to (1) and (2) – in analogy that only "background independent" formulations of, for example, QCD are relevant?

(4) And that *string field theory* is not a background independent formulation of string theory (in the sense to be defined above)?

Cheers, Kasper

16. **woit**
August 1, 2006

Kasper,

1. No
2. That has nothing to do with this question.
3. That has nothing to do with this question.
4. Yes

17. **C**
August 2, 2006

Is it possible that Lubos' role in the review process was the result of a misunderstood joke?

Publisher A (doesn't read blogs): Who should we get to review this?
Publisher B (reads blogs): (sarcasm) Of course, we should get Lubos Motl from Harvard.
Publisher A, impervious to sarcasm, sends manuscript the next day.

18. **anonymous**
August 2, 2006

About neutrinos: they produce radiation hazards in TeV-energy muon colliders because 1) muons decay to neutrinos; 2) TeV neutrinos interact with matter LESS strongly that electrons, photons etc, so that building the collider underground is not enough to shield neutrinos: they exit making occasional showers.

PS: how many books had a Harvard professor as spell checker?

19. **Kasper Olsen**
August 2, 2006

Dear Peter,

I forgot to make precise that the first question is not (1), but (0), or:

(0) Could you please make more precise the definition of the term "different backgrounds"?

and I don't hope you'll answer

0(1). No,
or
0(2). That has nothing to do with this question.

In saying that background A is different from background B, one must have a way of classifying different – and consequently equivalent – backgrounds, or – loosely speaking – a metric topology on the space of backgrounds. For example, I would use a metric where background

A' = exp(2) A

is at zero distance from background A.

cheers, Kasper

20. **Stefan**
August 2, 2006

Hi Peter,

Peter, with a heavy heart I must state my dismay and disappointment with your actions – in regards to your various opponents, so far.

Lubos – with all his grandiose utterances – is nothing more than a rambler. (His own utterances will testify amply to that.)

When will you – if ever(??) – start discussing physics again?

If, at any time you do, please follow on:

I have been trying to find some useful information on the topics we discussed upon earlier:

1) Dirac operators, and

2) Gauge fields.

Landmark texts / works?
Do you by chance happen to know the standard references to consult? (Textbooks, Arxiv, etc.)

I am also trying to purchase the text by Jouko Mickelsson: Current Algebras and Groups.

Finally, do you happen to know what the following texts are primarily studied for:

1) Gauge Fields: An Introduction to Quantum Theory – L. D. Faddeev, A. A. Slavnov;

2) Gauge Fields and Strings – A. M. Polyakov;

3) Quantum Field Theory: From Operators to Path Integrals – Kerson Huang;

4) Operator Methods in Quantum Mechanics – O. L. de Lange, R. E. Rabb;

Thanks,
Stefan

21. **ocnote**
August 2, 2006

LM writes on his blog: "People, including those with degrees, tend to trust the media, and because the media have been producing crap about physics most of the last few years, it more or less means that most people who rely on media inevitably believe this crap."

You don't think Lubos means the media-hype around string theory, do you?

22. **Arun**
August 2, 2006

I'm wondering whether delaying Peter's book has increased its impact, because as each year goes by, the claim of progress becomes less plausible.

23. **Anonymous Coward**
August 2, 2006

Stefan,

When did you ever get the impression that Peter knows anything about cutting-edge physics? He never did and doesn't now. Thats why he has this blog–the only way to make a name for himself, which is much easier than actually doing research. He keeps promising to talk about physics, but never does, because he has nothing interesting or substansive to say. You should check out his (in)famous ideas on the embedding of SU(2) into SO(4) to see the calibre of great ideas he thinks up. This isn't meant to be insulting to Peter, only explanatory. He has much more fun knocking down people who are seriosuly working for years on interesting difficult ideas, especially when it requires no intellectual effort of him. Thats why almost no real theorists–string theorists or not–respects or even pays any attention to him. If you're interested in substansive physics discussions, ignore blogs and the mediocrities and crackpots they attract altogether. Peter has been promising to tell us about his fantastic ideas for research for a long time now, and I predict it will never happen.

24. **woit**
August 2, 2006

Kasper,

I have no idea what the precise set of consistent string theory backgrounds is or what the correct metric on it is, and I don't think anyone else does either. But as far as anyone can tell, there are many of them, they give different physics, and you have to choose one before you can even write down what your theory is and try and see if you can extract any physics from it.

Stefan,

There are by now hundreds of places you can read the basics of gauge theory and find out what the Dirac operator is. Some of the books you mention contain this, there are many others. You first need to understand quantum Yang-Mills theory at the level of the books you mention. Another one that is highly readable is Pierre Ramond's QFT book.

Once you understand that, then I'd suggest learning about the mathematical context of these things. I keep encouraging people to read Atiyah's expository articles. The Mickelsson book you mention is much more advanced than the other books, but also highly worth reading, it's a much more research level document.

AC,

You know, you anonymous pluckers really should try and find at least one more mistake I've made in what I've written here and in the book. I'm sure among the by now thousands of pages of stuff there's at least one more.

25. **King Ray**
August 2, 2006

Peter, keep up the good work. I think your criticism of string theory is contributing more and doing a greater service to physics than all the string theorists combined. They have no aesthetic compass.

26. **David**
August 2, 2006

Peter,
I have now finished reading the UK version of NEW. As someone who works in the molecular/chemical end of science (among other things) and not in HEP, I found the book difficult in spots. However, by the end I thought you had given a clear overview of your position on the current state of theory in HEP. The essential arguments are there. If the string folks want to say that's not correct and explain why, then that should help understanding in the field. WRT to Lubos, I don't think you should expect much. Based on my interactions with him, I find that he is not interested in the reasoned discussion of scientific issues. He's only interested in telling people that his view is correct and everyone else is wrong because they're too stupid to see that he's always right. A particular thing that I find annoying is his over and over misquoting someone and concluding the person is stupid based on his misquote. In summary, I think NEW is thought provoking and thus adds something positive to HEP discourse. I think a full and frank discussion of these issues will make HEP a stronger field. I hope that happens. As you mentioned, last week, if some of the braneworld models that may be testable at the LHC are supported, you'd modify your views. That's what a scientist does. Hopefully other people will take note, and adopt a similar attitude. NEW, the book, is, I think, a very positive contribution.
Best,
David

27. **Juan R.**
August 3, 2006

I do not usually reply anonymous postings but will reply this one.

Anonymous Coward said,

> When did you ever get the impression that Peter knows anything about cutting-edge physics? He never did and doesn't now.

Right! I do not remember Peter Woit (not other here) claiming that. I remember string theorists claiming that they are making the TOE (i.e. claiming that they know anything about anything).

> Thats why he has this blog–the only way to make a name for himself, which is much easier than actually doing research.

Right again! This is reason that string theorists wrote so many popular books, give talks for outsiders, submit articles to generic magazines, give interviews to media. Unfortuntately, after 40 years there is none serious work about string theory, none prediction, none explanation of misteria of SM and GR…

> He keeps promising to talk about physics, but never does, because he has nothing interesting or substansive to say. You should check out his (in)famous ideas on the embedding of SU(2) into SO(4) to see the calibre of great ideas he thinks up.

Would i remember you how many wrong stuff is said each day by string theorists? None serious physicist working in quantum measurement takes Brian Greene ideas about the topic seriously (Dyson was very clear about this a few years ago). There is more…

> He has much more fun knocking down people who are seriosuly working for years on interesting difficult ideas, especially when it requires no intellectual effort of him.

Do you mean that serious people who claim that string theory predicts gravity (Witten), that string theory is the language God wrote the world (Motl), that the CC is explained by the antrophic principle over a quasi-infinite Landscape (Susskind)?

Hum, when you ask Witten how string theory predicts gravity he replies that really is a postdiction, when you ask Motl about derivation of some known stuff in physics he replies insulting (as usual in him because string theory predicts/explains nothing), when you follow details of the Susskind approach you coincide with Gell-Mann: absurd.

> Thats why almost no real theorists–string theorists or not–respects or even pays any attention to him.

I know that string theorists -and others- usually do claims without basis. Could you provide any reference or statistical data for your claim please?

> If you're interested in substansive physics discussions, ignore blogs and the mediocrities and crackpots they attract altogether.

Then why are you writting here?

> Peter has been promising to tell us about his fantastic ideas for research for a long time now, and I predict it will never happen.

I do not know, nobody knows. However, is not that the history of last 40 years of research in string theory? During 40 years we receive claoims about all kind of fantastci stuff could be done from string theory and how M-theory would revolutionate our views about space and time? result?

1) String theory is unable to explain data can be explained with common theories as SM and GR. String theory is compatible with nothing of this world.

2) Nobody knows M-theory is (if it really exists).

3) Since string theory cannot explain stuff as CC using scientific method the scientific method is abandoned by metaphysical cuasi-religious stuff (instead abandoning string theory nonsense).

Juan R.

Center for CANONICAL |SCIENCE)

28. **Mahndisa**
August 3, 2006

08 03 06

Hello Peter:
Congratulations on your book reviews etc. And I am curious about what you or anyone else thinks about Mr. Thiemann's newest paper on AQG? Pls pardon me if you have already written about this. I asked Lubos, but to no avail. I don't think my knowledge is developed enough to analyze the paper thoroughly, perhaps you or someone else can? Thanks.

29. **Peter Woit**
August 3, 2006

Mahndisa,

I took a quick look at the papers, they look interesting, but to really understand them would take a lot more time, since I'm no expert in this area. I hope to find the time someday to read them more carefully.

30. **Mahndisa**
August 3, 2006

08 04 06

Thanks for the response Peter. I have a feeling, a thorough analysis of that set of papers will take a lot of time yet;)

31. **Kasper Olsen**
August 3, 2006

Dear Peter

Now, if what you are saying, that

I have no idea what the precise set of consistent string theory backgrounds is or what the correct metric on it is, and I don't think anyone else does either.

is true, then

(1): how can you claim, that

there are many of them, they give different physics

????

and related to my question above, which you considered irrelevant to the discussion,

(2): if your statement, that

you have to choose one before you can even write down what your theory is and try and see if you can extract any physics from it.

is true, then are you applying the same critisism to theories like

QCD, or **QED**

????

Cheers, Kasper

32. **woit**
August 3, 2006

Kasper,

This is about Lubos's claim that I'm wrong to say that string theory depends on a choice of background, this has nothing to do with QCD and QED. You're trying to start up a different argument that has already taken place here many times.

Your attitude seems to be that string theory is background independent since you don't know what the possible backgrounds are or how to calculate anything in them. I suppose that's a consistent point of view…

33. **Kasper Olsen**
August 3, 2006

Dear Peter,

I think my question (2) was relevant for the reason implied above, but of course it is up to you if you want to answer the question (2), or not.

And whether Lubos claims this or that has nothing to do with my question – actually, *I'm just trying to follow your line of thinking*;

and I think it would be relevant to get an answer to my question above:

(1): how can you claim, that

there are many of them, they give different physics

??

But of course, I haven't read your book yet…

best, Kasper

34. **Michael Edwards**
August 4, 2006

This comment is too long and has too much math in it, but I thought readers might be interested in an elaboration of Peter's "wrong" ideas about subgroups of SO(4). Disclaimer: I am not a working mathematical physicist, just a hobbyist with an unfashionable interest in physics on 4-dimensional manifolds of Euclidean signature and complicated topology.

If I've read the infamous "SU(2) embedded in SO(4)" error correctly ("Wick Rotation", Feb 28, 2005), it's not even (all that) wrong. Yes, if you insist on viewing SU(2) as an _invariant_ (normal) Lie subgroup of SO(4) and imposing a _group_ structure on SO(4)/SU(2), then you do have to choose one of the "chiral" SU(2) subgroups. In this case, the quotient group is unambiguously SU(2)/{+/-1} ~ SO(3) with generators drawn from the "opposite" chiral subgroup. But that's not the only way a quotient of Lie groups can enter into physics. If all you want is a finer fibration of a principal bundle with an SO(4) fiber, then it's not important to have a group structure on the quotient space, and you can choose any Lie subgroup of SO(4) as the (sub-)fiber.

Peter's later comment about an "anti-diagonal" SU(2) is indeed erroneous, in that the generators "orthogonal" to the diagonal SO(3) don't form a closed Lie algebra. And for this reason, I think the "diagonal" SO(3) isn't a terribly helpful starting point if you want to wind up with an SU(2) that you can identify with the electroweak SU(2)_L. But there's another way to implement the spirit of his original post, which is to look at the manifold structure of SO(4) / (subgroup containing SU(2)_L) and ask what the action of the remaining generators on this manifold looks like.

This matters when you try to express, say, a Chern character on a 4-manifold M in terms of a more complex bundle over M than the ordinary frame bundle. Why would you want to do this? Because we have much more powerful analytical techniques for systems phrased in terms of complex fields than for those phrased in terms of real fields, and this more complicated bundle structure may contain a central U(1) that we can identify with the "i" of complex analysis.

For instance, you could take an individual chiral generator — call it I (in a convention in which the "generator" is the actual Lie algebra element and not the matrix I/i) — and consider the non-normal Lie subgroup G1 equal to the centralizer of I (isomorphic to SU(2)xU(1)/{+/1} ~ U(2)). We can't put a Lie group structure on SO(4)/G1. But we can form a principal bundle with total manifold F ~ SO(4), fiber G1, and base manifold F/G1 ~ SU(2)/U(1) ~ S^2. The quotient is done along the orbits of the right action of G1, which also makes sense _globally_ on other objects with a global SO(4) right action — such as the SO(4) principal reduced frame bundle P of a 4-manifold M of arbitrary orientable topology. So you can look at the whole frame bundle as a principal G1-bundle E, globally diffeomorphic to P, over a bigger base manifold

N=P/G1, locally diffeomorphic to (U \subset R^4) x S^2.

This 6-manifold N is in turn an associated SO(4)-bundle over M with fiber S^2, trivially reducible to an SO(3)-bundle since the generators of the SU(2) with opposite “chirality” to I (let’s call it SU(2)_L) all act trivially on the fiber. In general you can’t find a global section of this associated bundle, i. e., a smooth mapping from each point on M to a point on the fiber over M; if you could, then you could reduce the structure group of P to G1. But you can ask how a connexion on the original fiber bundle acts on the S^2 fiber of the associated SO(3)-bundle N and on the G1 fiber of the new principal bundle E.

We were able to reduce the structure group of N to SO(3) because both the left and right actions of SU(2)_L act trivially on its fiber S^2. So does the right action of the U(1) with generator I, as long as the quotient on SO(4)/G1 is taken from the right; but the left action of exp(It) on SO(4) doesn’t follow the same orbits as the right action. And because the fiber bundle structure of SO(4)/G1 is not trivial (it’s related to the Hopf map SU(2)/U(1) ~ S^2), we can’t reduce the structure group of E to SU(2)_L. Although the right action of G1 on an individual fiber of E looks like U(2), and we can certainly define a left action of the central U(1) on that same fiber for the purpose of constructing an atlas on E, this left action does not coincide with the left action of G1 as a subgroup of SO(4) on the original bundle structure P.

This complicates the mapping of fields of geometric origin on P to fields on E, which are (potentially) more analytically tractable. For instance, a connexion on the original fiber bundle P, viewed as a lift of the space of tangent vector fields on M to the space of right-invariant tangent vector fields on P, is more like a left action than a right action (in the sense that, if point p \in P has coordinates (x_i, g_i) in a given coordinate patch, and the right-invariant vector field v has the coordinate expression (v_i, g_i a) at p for some element a in the Lie algebra of G, then at R_g(p) = pg = (x_i, g) it has value not (v_i, g_i g a) but (v_i, g_i a g)).

So from the point of view of gauge couplings, i. e., the connexion on the original frame bundle, the original SO(4) splits into an SU(2)_L that acts on the fiber of the new G1-bundle and an SO(3) that acts on the “compactified” S^2; the latter also acts on the central U(1) of the G1 fiber in a way that varies depending on where you are on S^2. Except that’s probably not the right way to define a connexion on E that is in some sense induced from the connexion on P; instead, you want to go back to the idea of a connexion as a family of horizontal subspaces of the tangent spaces at various points of the bundle, and ask whether there is a consistent way to extend the 4-dimensional horizontal subspace at a point p \in P to a 6-dimensional horizontal subspace at the corresponding p \in E. The answer is yes; although the right actions of the other two SU(2)_R generators are associated with vector fields on each fiber of P which are not invariant under the right action of the U(1) in G1, the plane they span is.

Enough math. Looking back at Peter’s original idea, it is tempting to identify SU(2)_L with the weak SU(2) and seek a (not necessarily trivial) relationship between the central U(1) and the hypercharge U(1). As he proposed, there is also a resemblance that I won’t go into right now between the SO(3) action on S^2 and the SO(3) of spatial rotations on the original bundle; and analytical results (if any) will probably involve spinors and Wick rotations, not so much because the geometric objects sort neatly into spinor representations (they don’t) but because that’s a useful way to implement “gauge fixing” of the diffeomorphism group of M.

Does this have anything to do with the fundamental reality underlying the Standard Model? Maybe, maybe not. But it seems to me to be a perfectly sensible starting point for an interesting mathematical excursion. And having audited and enjoyed a string theory course not so long ago, that’s more or less how I feel about string theory, too. It’s just not science — not until it has some combination of predictive and explanatory power with regard to the observed universe.

Cheers,
– Michael

35. **John Gonsowski**
August 4, 2006

Michael, given Peter’s recent comments, your math filled post about Peter’s ideas might be more common in the future of this blog. That 4 in SO(4) is a Clifford Algebra vector and why more people don’t play with a vector for their spacetime is beyond me. If you want a ten dim spacetime then John Baez’s SO(10) paper seems like a nicer starting point than string theory. If you want to naturally get to spinors then SO(8) and its triality seem nice, even string theory noticed this years ago before getting farther and farther from the real world. One can also do nice things with SO(8) and fiber bundles. That some kind of vector-spinor triality might exist down at SO(4) is a nice idea. Finding little math errors does not always make the general idea wrong.

36. **woit**
August 5, 2006

Kasper,

Go talk to any “string phenomenologist” working with the state of the art of techniques for trying to get physics out string theory. What they do is pick a background of some kind and try and calculate something. Different backgrounds give different results.

37. **Kasper Olsen**
August 5, 2006

Dear Peter

Go talk to any “phenomenologist” working with the state of the art of techniques for trying to get physics out of quantum field theory. What they do is pick a background of some kind and try and calculate something. Different backgrounds give different results.

cheers, Kasper

38. **woit**
August 5, 2006

Michael,

Many thanks for the long comment. The kind of geometry you’re talking about is exactly what I had in mind in the comments you’re referring to. The “off-diagonal” SU(2) comment was a misguided attempt on my part to say something over-simplified about the geometrical set-up that you describe in detail.

The space you call N is also known as the (Euclidean) twistor space. a CP^1 bundle over the four manifold M. It’s the bundle over M whose fiber at a point is the space of orthogonal complex structures on the tangent space at the point (more precisely, those compatible with the orientation). As you note, the frame bundle P of M gives a principal U(2) bundle over N. It is this U(2) that I’d like to identify as the electroweak U(2).

Again, as you note, the problem with this is that this is a U(2) bundle over N, not M, and one has to deal with this somehow, and I don’t know of a really satisfactory way of doing so. You could just locally pick a section of N (in general you can’t do this globally), but then you have another sort of field to worry about. The SU(2) part of the U(2) doesn’t depend on this, but the U(1) does, which creates a problem.

One other issue here is that this is all Euclidean, and how to set up the analytic continuation to Minkowski signature has to be sorted out. In essence, the hope is that somehow under analytic continuation the boost part of the local Minkowski frame transformations becomes something that can be interpreted as an internal SU(2) symmetry.

My suspicion is that this somehow has to do with an old conceptual problem that many people have commented on (Yang, Penrose, Baez and others): there are several complex structures being used in QFT, and it is not clear why they can all be identified as acting by the same "i". One kind of complex structure comes from space-time symmetry, it's used crucially to distinguish between positive and negative frequency, and thus to characterize the vacuum state. Another inherently different complex structure is the one on the fibers, the "i" that generates U(1) gauge transformations. I'd like to think that maybe the use of the space of all local complex structures gives a new way of thinking about this problem, but I haven't seen my way through this.

At some point in thinking about this many years ago I reached the conclusion that I needed a better, more abstract, way of thinking about path integrals, especially for fermi fields, before it would become clear if the ideas above could be used. Still working on that….

39. **woit**
August 5, 2006

Kasper,

OK, so you agree with me that I was correct to write in my book that string theory depends on a choice of background.

You obsessively want to make the argument that string theory is in the same state as QFT, even though this is obviously not the case. One makes testable predictions, the other doesn't. I've gone over this a hundred times here, and doing it the one-hundred and first time is clearly going to be a waste of time. But the bottom line is extremely simple to state: QFT phenomenologists can calculate many things that can be compared to experiment, and, if they don't start making their models baroquely complicated, they make solid predictions that can be tested. String theory "phenomenologists" can calculate very little in their models, and even to force this very little to agree with experiment, they have to make their models so complicated they make don't make testable predictions.

40. **Aaron Bergman**
August 5, 2006

String theory does not depend on the choice of background. The low energy observed physics depends on the choice of a vacuum. Just like in any other theory with multiple vacua.

41. **woit**
August 5, 2006

OK, Aaron, I knew you wouldn't be able to help yourself.

You should mention that it's not just the low energy physics that depends on the background, but the high energy physics also.

All known ways of writing down string theory depend on first choosing a background (yes, even AdS/CFT, where the asymptotic behavior is the background). At this point I recall, what you like to do is to say that string theory is just analogous to gauge theory, it's just we only have a gauge-fixed version. Problem with this argument is that observable physics doesn't depend on the gauge, whereas observable physics of string theory does depend on the background (which most people not schooled in string metaphysics would describe as being background dependent).

If you have something new to say about this, go ahead, but if it's going to be the same argument, please just link to one of the versions elsewhere.

And who, don't even think of joining in. Unless you have something really new to say on this topic, any comments arguing about this will be mercilessly deleted.

42. **Kasper Olsen**
August 5, 2006

Peter,

I don't know what you wrote in your book since I haven't read it yet. So I can't say that I agree with you.

And actually, it is not true that I "want to make the argument that string theory is in the same state as QFT". Of course not. My question was not related to phenomenology directly. Actually I think it was quite simple:

> (2): if your statement, that
>
> *you have to choose one before you can even write down what your theory is and try and see if you can extract any physics from it.*
>
> is true, then are you applying the same critisism to theories like QCD, or QED ????

Maybe you'll just need to make more precise what you mean with the concept of "background independence"; And then the question was I don't think you answered yet was:

> Now, if what you are saying, that
>
> *I have no idea what the precise set of consistent string theory backgrounds is or what the correct metric on it is, and I don't think anyone else does either.*
>
> is true, then
>
> (1): how can you claim, that "there are many of them, they give different physics" ????

So I don't want to repeat the discussion of whether string theory makes any "real predictions" or not; the discussion would be much simpler if I had read your book 😉

best regards, Kasper

43. **Aaron Bergman**
August 5, 2006

Actually, what you say is not how I think about it, but as per your request, those interested can see here. Enjoy!

44. **Kasper Olsen**
August 5, 2006

Dear Aaaron and Peter,

Of course the question about a choice of background is different from the one of a choice of vacuum.

My question (2) was related to Peter's criticism of string theory in that you choose a background (much like in many other theories); but as we all know the resulting physics is background independent; so his critisism seems to be formulated in a way that confuses things.

My question (1) was related to the fact that the concepts of a choice of background and vacua also is confused; surely different points in the string theory landscape give rise to different physics – different cosmological constant etc. etc.; the background g' = exp(2) g is equivalent to background g (as Peter agreed) and not a different vacuum, or related to the question of "giving different physics".

Kasper

45. **Peter Woit**
August 5, 2006

"as we all know the resulting physics is background independent"

You're confusing what is known to be true with what many people would like to be true.

In any case, this is not even conjecturally true in the case of what people expect for generalizations of AdS/CFT. The string theory explicitly depends on a fixed asymptotic background. Different asymptotic backgrounds give different physics.

46. **Kasper Olsen**
August 5, 2006

Dear Peter,

Sorry, maybe not all…. And, are my questions above answered in your book, NEW ??

Best regards, Kasper

47. **Peter Woit**
August 5, 2006

Kasper,

No, the book doesn't deal with every piece of wishful thinking common among string theorists. All it does is accurately describe what the current state of knowledge in the field is.

Yes, yes, I finally did get some more copies of the book recently, and you're on a list of people to send a copy to…

48. **Michael Edwards**
August 6, 2006

Peter,

It's great to have the "twistor" connection to the literature about this and similar constructions. I've read a bit about twistors in Minkowski space but hadn't identified this particular construction with Euclidean twistors. Is there a good book in this area that focuses on geometrical and topological applications? (My idea of "good book" on manifolds runs to Goeckeler & Schuecker and Choquet-Bruhat; I learned differential geometry originally out of Dubrovin but was later turned on to differential forms and fiber bundles by Bill Burke and tend to prefer that language.)

What I like about the construction in terms of the frame bundle is that it makes clear that you can have a global U(1) right action on certain geometrical objects on any orientable 4-manifold, without postulating additional structure on the base manifold. It also shows how you can get "compact" dimensions with geometrical significance, in addition to the "macroscopic" dimensions of the base manifold, without a lot of handwaving about why some dimensions are macroscopic and others aren't. Depending on what you're trying to calculate, you may be able to work entirely on the U(2) bundle over N and quotient out the redundant degrees of freedom at the end, never having to deal explicitly with the lack of a complex structure on the original base manifold.

I do not worry about continuation to a manifold of intrinsic Minkowski signature because I am exploring the premise of complete diffeomorphism invariance, in which geometry is interesting only as a way to express topology in terms of local fields. The Minkowski signature isn't intrinsic to the manifold; it comes of choosing boundary conditions on the set of acceptable coordinate systems in which t->-inf and t->+inf are fundamentally different from spacelike infinity.

Picture a 2-torus M covered, except for a 1-dimensional "skeleton" consisting of two intersecting circles, by a single contractible region. The maximal atlas on the 2-torus contains a diffeomorphism between some coordinate region U \subset R^2 and this region of M. Choose a diffeomorphism between U and the complex plane C, and remove the origin and the negative real axis. In terms of radial coordinates rho and phi, define t = log rho and x = tan phi/2. The entire t->-inf boundary of this R^2 coordinate system converges toward the origin; the x->+/-inf boundary converges toward the negative real axis; and the t->+inf boundary converges toward the "skeleton". The t->+inf boundary is fundamentally different from the others; its intrinsic geometry contains "kinks" that capture the topological difference between the 2-torus and the 2-sphere. Euclidean rotations of this coordinate system do not preserve this difference but Lorentz boosts (pure shears along the light cone axes) do.

Now add two dimensions and a vastly more complicated topology. The premise remains the same: one R^n coordinate system covering all of an orientable manifold M except for a (n-1)-dimensional skeleton (actually, a simplicial complex of dimension not exceeding n-1) plus a trivial coordinate anomaly, in this case arising from recoordinatizing U->Q->R^4. (There's nothing magical about the quaternions here; they're just a convenient way to get to an R^4 system with the desired boundary conditions.) The skeleton is at t->+inf, and t->-inf converges to a single point on the manifold, connected by a line segment to a point on the skeleton; all of spacelike infinity winds up on this line segment.

None of these boundaries are special in terms of the intrinsic geometry of the manifold; one could just as easily have picked any other maximal coordinate patch in the atlas on M and transformed it using the same U->Q->R^4 trick. But the t->-inf and t->+inf boundaries are certainly special in the coordinate system, and the Poincare group preserves this distinction. I consider this quite sufficient reason for a completely diffeomorphism invariant theory to have the global causality structure and Poincare-invariant phenomenology with which we are familiar from QFT. And for dessert we get primordial asymptotic homogeneity and an entropic arrow of time.

Remember, I'm not saying this has anything to do with physics. This obviously isn't the whole spacetime story; it doesn't address microcausality, it doesn't explain why gravity looks so much like intrinsic geometry on a Minkowski background, it doesn't tell you the price of tea in China. But it adds to my interest in in Wick rotation as a way of making contact between QFT actions and partition functions in Euclidean space. Not to mention an interest in finding the portion of the SM gauge spectrum that couples chirally somewhere in the frame bundle over a Euclidean 4-manifold.

I also have some thoughts on path integrals and fermions, which it will take me a bit longer to write down at finite length. They were inspired by the identification of the

Faddeev-Popov ghost field with the Maurer-Cartan form on the group of QCD gauge transformations. I am handicapped in expressing these ideas by an inadequate grasp of the theory and lingo of Virasoro representations, which seem to be the way that field theorists with a solid mathematical background look at this. Any recommendations on texts?

Cheers,
– Michael

49. **Michael Edwards**
August 6, 2006

Of course, I meant H for the quaternions. I had originally written C^2, changed it during an editing pass, and had a momentary brain lapse. I'm a bit out of the habit and not copy editing as carefully as I might (there are glaring typos in the earlier post too).

– Michael

50. **Peter Woit**
August 6, 2006

Michael,

I learned about the Euclidean version of the twistor construction from Atiyah. He used it right at the beginning of the modern interaction between math and physics to study instantons. Using it relates questions about self-dual connections to questions about holomorphic bundles. See his short book "Geometry of Yang-Mills Fields", in vol. 5 of his collected works.

The Virasoro algebra is the centrally extended lie algebra of diffeomorphisms of the circle. There's a huge literature about this by now. The group of gauge transformations is something different, but related. For the circle, it's a loop group, with the central extension the Lie algebra is an affine Kac-Moody Lie algebra, and there's again a huge literature. For spaces of higher dimension than a circle, very little is known about the representations of either gauge groups or diffeomorphism groups. One place to start reading about this is Jouko Mickelsson's book.

51. **Michael Edwards**
August 6, 2006

OK, then perhaps Virasoro representation is not the hook into the literature that I'm looking for. What I'm really after is a way of describing the Lie algebra cohomology of the diffeomorphism group of a compact orientable 4-manifold.

There's an elegant way of combining the BRST coboundary operator with the covariant exterior derivative relative to a fixed connection to get a covariant coboundary operator on the gauge bundle. This can be used to construct a Lie algebra of graded derivations in which the "scalar functionals" are the space of Lagrangians (viewed as polynomials in the field degrees of freedom and their derivatives at a point, as in Ward identities) and the inner derivatives are taken relative to the direct sum of the Lie algebra of tangent vector fields on the base space and the Lie algebra of infinitesimal gauge transformations. The Maurer-Cartan form on this space is quite interesting.

I shouldn't attempt to spell this out in more detail without LaTeX, but does it sound like one of the directions that cohomology has taken since Stora? Does it have anything to do with equivariant cohomology?

Cheers,
– Michael

52. **Thomas Larsson**
August 6, 2006

What I'm really after is a way of describing the Lie algebra cohomology of the diffeomorphism group of a compact orientable 4-manifold.

To simplify things, one should start infinitesimally and locally. Extensions of the algebra of polynomial vector fields by modules of tensor fields were classified by Dzhumadildaev – my review math-ph/0002016 is online. Two of the extensions are closely related to the higher-dimensional generalizations of the Virasoro algebra, which arise in lowest-energy representations.

53. **woit**
August 6, 2006

Michael,

There's work by mathematicians on the Lie algebra cohomology of vector fields that goes under the name "Gelfand-Fuks cohomology". Bott wrote some beautiful expository papers on the subject, see vol. 3 (I think, the one on foliations) of his collected works.

The BRST related ideas you mention are among the things about this I've always found confusing and have never quite sorted out for myself, especially the relation to equivariant cohomology.

54. **Thomas Larsson**
August 7, 2006

Definition of Gelfand-Fuks cohomology can be found here.

A clarification: an extension of a Lie algebra L by its module M is an element in H^2(L,M). Dzhumadildaev (Z Phys C 72 (1996) 509-517) classified this for L = vect(n) and M a tensor module. Gelfand and Fuks only considered n = 1 and M the trivial module (Gelfand-Fuks cocycle = Virasoro algebra).

55. **D R Lunsford**
August 7, 2006

TL, there should be a way of saying this physically, as in, "the part of curvature not coupled to matter is the Weyl conformal curvature" etc. There may be an obvious intepretation I'm ignorant of..

-drl

56. **Thomas Larsson**
August 7, 2006

DRL. Sorry, but I specifically tried to answer a question on Lie algebra cohomology, without claiming any connection to physics.

OTOH, demanding a kinematical Hilbert space with a well-defined action of the diffeomorphism algebra dictates that QFT must be modified in a certain way; details can be found in the [ArXiv](). Unfortutely, what is written reflects my understanding as of 2004 and is flawed in several respects. If I stop wasting my time reading blogs, I might eventually manage to write things up.

57. **Michael Edwards**
August 7, 2006

Thomas,

Your ArXiv papers are definitely proving rewarding reading. There is, however, an aspect of your handling of the BRST operator that is not clear to me.

A textbook description (e. g., section 12.3 of Goeckeler & Schuecker) of Stora's solutions to the Wess-Zumino consistency condition exhibits a linear representation W on the space Pl of Lagrangian polynomials of the total Lie algebra E of right-invariant vector fields on the gauge bundle. On a trivial gauge bundle, E is the semidirect product of the vector algebra and the gauge algebra over M. On a non-trivial gauge bundle, infinitesimal gauge transformations still form a "vertical" ideal of E, but the "horizontal" vector algebra does not form a subalgebra. The coordinate expression of the Lie bracket on E necessarily involves a "fixed" (background) connexion on the gauge bundle, but that's not fundamental to the construction; it's just a way of expressing a right-invariant vector field on the bundle space in terms of a vector field on the base manifold plus an infinitesimal gauge transformation.

Identifying Pl with the space \Lambda^0 of 0-forms on E taking values in Pl, the Ward operator W(e) may be identified (up to a sign) with the Lie derivative wrt e on \Lambda^0. This Lie derivative looks exotic, involving a lift of the vector algebra (the infinitesimal diffeomorphisms) from the base manifold to the gauge bundle by means of a fixed connexion, ensuring that the commutators in the Lie bracket on E can be patched together on the overlaps of a local trivialization. This Lie bracket should not be confused with the Lie bracket of vector fields on the base manifold; the curvature of the fixed connexion appears in the Lie bracket of two horizontal algebra elements.

Now, given a Lie bracket on the total algebra E and a Lie derivative on the space of Pl-valued 0-forms over E, we can axiomatically construct the complete algebra \Lambda of alternating forms on E and the graded Lie algebra of derivations relating them. The "exterior derivative" operator d_E in this construction can be identified (up to a sign) with the BRST coboundary operator, and in fact the 1-form d_E L coincides with -Q L on the vertical ideal of E. d_E is nilpotent by construction.

Now, is -d_E the Koszul-Tate operator referenced in 0501043? (I'm having a hard time relating the language of canonical quantisation to the functional setting in which I'm used to seeing BRST-related constructions such as the Faddeev-Popov ghost.) If so, how does your approach to quantisation result in a change to the BRST operator that breaks nilpotence?

Cheers,
– Michael

58. **Michael Edwards**
August 7, 2006

More notes on the above, in hope that one or more of the folks who have been so kind as to suggest readings in cohomology will recognize this as a standard line of reasoning.

It turns out that most of the above construction is available in the first five sections of Schuecker's 1987 paper:

http://projecteuclid.org/Dienst/UI/1.0/Summarize/euclid.cmp/1104116716

His construction of the graded Lie algebra of derivations in section 6 is, however, significantly different from what I have in mind. He also doesn't particularly emphasize that the fixed auxiliary connexion is only a device for identifying the "horizontal" portion of an algebra element in a particular local trivialization, and that the actual graded Lie algebra of derivations on \Lambda is independent of this connexion. Nor does he spell out that the full coboundary operator (in his notation, d + s) is an extension of the BRST coboundary to the full algebra of infinitesimal diffeomorphisms + infinitesimal gauge transformations; maybe in his construction it's not (I haven't checked).

Perhaps for these reasons, Schuecker points out the resemblance of the "(algebraic) Faddeev-Popov ghost" to the Maurer-Cartan form but does not extend it to the full Maurer-Cartan form on E. The latter has some very interesting properties that I may comment on later (when I decipher some of my old notes).

In my construction, the Lie derivative on \Lambda with respect to an element e of E is of degree 0, the inner derivative with respect to e is of degree -1, and the exterior derivative (coboundary) is of degree 1, just like in ordinary differential forms. The inner derivatives with respect to elements of E form a Grassmann subalgebra of the graded algebra of derivations on \Lambda, which I am tempted to relate to the Grassmann algebra of which fermions carry odd representations.

I hedge a bit here because, if this has any relationship to physics, the fermions of low-energy phenomenology probably are not objects of a single rank in this Grassmann subalgebra. There could be some terms in the "fundamental" Lagrangian that arise from topological densities, expressed using objects of well-defined rank. Other terms are artifacts of functional quantisation in a non-diffeomorphism-invariant "gauge", involving fields whose algebraic properties (like those of the traditional Faddeev-Popov ghost) are chosen so that the added term in the Lagrangian forms an operator trace of the Jacobian of the gauge-fixing term. I would expect the eigenfields of the mass term in the effective Lagrangian (wherever it comes from) to be a mix of fields of these two types.

Is this clear enough to be boring yet? 🙂

Cheers,
– Michael

59. **Thomas Larsson**
August 9, 2006

Michael, it might be better to continue this off-topic discussion privately, rather than pushing Peter's hospitality further. My email address is on my eprints. However, let me just end with some general comments.

What I'm doing is not strictly equivalent to QFT. To obtain a well-defined action of the diff algebra, which is impossible in QFT proper, I first replace all fields by their Taylor series. This introduces an additional datum: the expansion point. Whereas infinite Taylor series are independent of the base point, truncated ones are not, and this dependence remains in the form of anomalies after quantization, even when the truncation is removed.

The existence of new anomalies shows that passing to Taylor data makes a substantial difference. I think this is a good thing, because we know that QFT is incompatible with gravity. By considering a structure which is close to QFT, but essentially different from it, this no-go theorem might be avoided.

Our main similarity is that we both use cohomology, but this is a very general mathematical technique, applicable in many situations. My work is largely modelled on the antifield approach, as formulated in chapter 17 of Henneaux and Teitelboim. However, since I want to do canonical quantization, I need an honest Poisson bracket rather than an antibracket, and therefore my starting point is not the space of histories, but rather its phase space. A flaw in my paper is that I get an overcounting for the harmonic oscillator. To correct this, one must add an extra constraint which identifies momenta and velocities.

Thank you for making me aware of Schuecker's paper. I have never managed to understand them properly, and I have long been confused about their relation to my extensions. AFAIU, there is none. In particular, I do consider vector fields on the base manifold, without a reference connection A^0.

60. **Thomas Larsson**
August 9, 2006

to understand them properly

them = conventional gauge anomalies

61. **Michael Edwards**
August 9, 2006

Thomas,

My (obviously very amateur) take on anomalies is that they happen when you try to write down a theory about objects that live in quotient spaces of group actions on fiber bundles using index-slinging notation and you don't get it quite right. This tends to come of using heuristics like "give this symbol algebraic properties which make the term in which it appears come out gauge invariant", "the horizontal element is the one normal to the vertical subspace", or "this field must be a boson because it's a Lorentz scalar" instead of sweating blood over what it's doing there in the first place.

We've all grown up with this situation because quantum mechanics _works_ even though it doesn't make any bloody sense. Heck, it started long before quantum mechanics: I lost confidence in the formal correctness of what I was doing in the physics classroom the first time I saw a Lagrange multiplier, and to this day I can't look at a partial derivative without wincing. The Lie derivative _means_ something. A partial derivative has no more intrinsic meaning than ten in the ones column carried to one in the tens column.

Until you slog your way through to BRST quantization, gauge covariance is just a heuristic. For me at least, putting Schücker's chapter on anomalies side by side with Peskin & Schroeder's explanation of "BRST symmetry" led (eventually) to an "a-ha" moment: if you want a theory with local causality, and the fundamental objects of your theory live on a big honkin' principal bundle with irreducible global structure, then you'd better define your theory there, because that's the _only_ space on which the Lie derivative looks like a local operator. The rest of the apparatus – differential forms, Clifford algebras, Wick rotation, Feynman diagrams, contour integrals – is long division for postdocs.

Your work has my respect (though not yet my comprehension) because it investigates how real calculations with imperfectly known initial conditions can be truncated without losing the answer in imperfectly cancelled anomalies. I couldn't compute a QCD background if my life depended on it. A formalism with all the elegance and explanatory power in the world is of little use if it can't make contact with phenomenology, and "we can't prove that the SM can't be obtained as our effective field theory" is a poor second to a theory with added predictive power. Personally, I would be very satisfied with a theory that doesn't make any "new" predictions but does liberate grad students from index-slinging and the rest of us from piffle about the philosophical implications of discontinuous spacetime at the Planck scale.

Cheers,
– Michael

Reviews and Errata

August 2, 2006
Categories: Not Even Wrong: The Book

The August edition of Seed magazine is out on the newstands, and it contains a joint review entitled "No Strings Attached" by Charles Seife of my book and of Lee Smolin's The Trouble With Physics. The article and magazine issue are not online at the moment. The latest issue of Physics World contains a review by Gordon Fraser, entitled String theory gets knotted.

Both reviews give a reasonable description of what the book is about, and take the first part of the book to task for being hard going, worrying that the reader may give up before getting to the less technical later parts. Seife writes "the level of detail is inconsistent" and Fraser describes "a level of detail that is unpredictable", and this is true enough. It was a conscious decision to put together history, some basic explanations of math and particle physics, together with some explanations of the rather arcane joint successes of math and physics in recent years, all in as compact form as possible. There is a warning in the text that almost everyone is going to find parts of this hard to follow and should judiciously skip ahead. My goal was to write something that almost everyone would get something out of, from people new to the subject to those with quite a bit of technical knowledge. Undoubtedly this was an overly-ambitious idea, but on the whole I've been pleased so far to hear that people with a wide range of backgrounds seem to enjoy the book.

Because I cover so much ground in so few pages, many technical terms and ideas don't get properly explained. Both Seife and Fraser fault me for not explaining "synchrotron radiation", which is true enough, although I use the term in context to describe X-rays produced when electrons are accelerated in a synchrotron. Seife says that I don't define "eigenstate", although I do give a one-sentence definition immediately after first using the term. It's true though that anyone who hasn't taken a linear algebra course will probably just find this baffling.

Fraser complains about inaccuracies in the book, and he has found two of them: I describe Rutherford's discovery of the nucleus as taking place at Cambridge when it was really Manchester, and while this experiment is first properly described as involving the scattering of alpha particles, at a later point in the book it is inaccurately referred to as involving scattering electrons. Some of his other complaints seem to me unfounded. I don't say that Isabelle was canceled before planning was underway for the SSC, and I don't understand why he claims there was no "competing collider" at CERN (the reference was to the SpS, being used as a p-pbar collider starting in 1981).

I've just written up an errata page for the book, which includes the two errors mentioned by Fraser. It can be found here.

Update: John Horgan's review of Not Even Wrong that appeared in Prospect is available at his web-site.

Sabine Hossenfelder has the first review of Lee Smolin's The Trouble With Physics, together with an interview with Smolin. Lubos responds to this by explaining that Sabine is a woman, thus intellectually inferior, and prone to engage in "female physics".

Comments

1. **L. Riofrio**
   August 2, 2006

   I sympathise with your difficulty in getting things published. Keep at it; I look forward to reading your book. The best way to bury a bad idea is to come up with something better.

2. **Johan Couder**
   August 3, 2006

   I have to agree – your book is indeed "hard going" at times. In spite of having 'some' mathematical background I probably wouldn't have struggled through if as an interested "layman" I hadn't already read so many popular books on quantum physics, relativity and string theory before.
   But I'm glad I did. I kept buying the latest 'popular' books on string theory afraid as I was of missing out on the "Big Revolution" in theoretical physics. If anything your book made me realize string theory may very well not be the "Holy Grail" its proponents purport it to be.
   It also saddens me some string theorists do not seem to have the 'grandeur' of at least "agreeing to disagree". I'm glad though your colleague Brian Greene doesn't seem to be one of them, because I do admire him as a science popularizer (and as a scientist of course).
   I'll never reach the heights of your mathematical understanding (not by far) , but at least you didn't give me the feeling of being a complete moron. It obviously took a lot of courage to do what you did, and I (and many others I'm sure) thank you for that.

3. **Tommaso Dorigo**
   August 3, 2006

   Hi Peter,

   I think a varying level of detail is a very good idea. I enjoyed the book back to back, and the fact that the level was not always the same was stimulating to me, rather than the other way round. I did not skip pages, even if in a couple of instances I was having trouble understanding the details.

   So this kind of criticism is unfounded IMO.

   T.

4. **Hmm**
   August 4, 2006

   Johan,

   It didn't take any "courage" for Peter to do what he did. He isn't a physicist, nor even a professional resarcher of any sort. He has absolutely nothing to lose spouting his trivial ideas, and lots to gain–media coverage and recognition he could never have gotten from doing research. So it didn't take any courage–no one even noticed him enough to even care what he thought or said before, and nothing has changed in this regard.

   Peter–I've said it before. Enjoy your 15 minutes. You've taken a pretty pathetic and classless road to try and make a name for yourself, but I guess it takes the sting out of never having enough talent to be kept around in physics. However you should know that there are lots of serious and brilliant people actually struggling to make progress, who both understand your trivial points as well as hundreds of more interesting ones. You are pompous and arrogant with nothing to back it up intellectually; its lucky for you that we live in a general age of mediocrity in this society, which is the only reason you get any coverage at all.

We are all also STILL waiting for your research ideas, where are they???? Will we EVER see them??? Well it was tough going for you when you were younger, and its not going to get any easier….I'd get cracking if I were you.

5. **Marty Tysanner**
   August 4, 2006

   Peter,

   Could you please do all of us a favor and delete the above post by "Hmm"? He appears to be nothing more than a puerile, loud-mouthed, air-headed troll who has never initiated nor contributed any useful discussion here. His "contributions" are no more than noise, certainly nothing worthy of a response by you nor anyone else. I am posting something only because I am sick of seeing his meaningless drivel.

   More generally, I really, really wish you would initiate an uncompromising policy where *any* post that contains a significant personal attack on you or any other poster would be automatically be deleted without further explanation. Something like a simple line just above the comment entry area like "*Posts containing ad hominem attacks will be mercilessly deleted*" would provide adequate warning. You may feel an obligation to allow others to personally attack you so as to avoid an image of censorship, but in my view that is only appropriate as long as the attacks are restricted to ideas or appropriateness of material, as opposed to purely personal attacks like the one above. Allowing trashy comments like those by Hmm and others of his ilk (e.g., "Michael") cheapens your blog and gives it too much of an unmoderated Usenet flavor.

6. **Walt**
   August 4, 2006

   Hmm: You're helping to kill string theory. Seriously, anyone who's on the outside of the discussion (and that includes most physicists) who sees these arguments between you and Peter will side with Peter every time. Refuting bad ideas is as much a part of science as developing good ones. Peter has put forth a scientific argument against string theory; that's part of science. Your whining about the hurt feelings of all the hard workers out there is not.

7. **Hmm**
   August 4, 2006

   Marty,

   I apologize–seeing Johan call Peter "courageous" pushed me over the edge. I really do have an exceedingly low opinion of what Peter is engaged in, but I agree that it was wrong to respond as I did, not to mention a waste of time.

   So apologies Peter, and please feel free to delete my comments as Marty suggested. I will avoid leaving comments here in the future.

8. **Hmm**
   August 4, 2006

   OK one last comment in response to Walt: yes, obviously refuting bad arguments is part of science. But Peter has not "put forth a scientific argument". He hasn't said a *single* thing that isn't trivially obvious to everyone in the field; indeed most practicioners (and I'm not talking about Kaku or Greene) have much more insight into the problems of string theory, and at a deeper level, both physically and mathematically. But they also know of many remarkable aspects of the theory that are extremely compelling. The subject is a work in progress, and no one has claimed that the answer is right around the corner. That is why what Peter does is so annoying–he takes the "outsider against the establishment" line when nothing he says is news to anyone inside the subject, pretending he knows what the outcome of all the confusions is going to be, say involving the landscape, when they are very much up in the air, subjects of ongoing research. All he does is boo the people from the sidelines without offering anything positive, and not even giving anything negative that isn't universally known. And its a little galling when his specific proposals for "alternatives" are so amazingly naive, as you might expect from someone with no real experience in doing original research.

9. **Arun**
   August 5, 2006

   Stripped of the ad hominem remarks, all that Hmm is saying is – trust the string theorists, what makes string theory compelling cannot be explained to mere mortals, only the problems with string theory can.

10. **Ummm**
    August 5, 2006

    "…yes, obviously refuting bad arguments is part of science. But Peter has not "put forth a scientific argument". He hasn't said a *single* thing that isn't trivially obvious to everyone in the field…" – Hmm

    If it is so trivially obvious that stringy stuff has got into a worse situation over the past 20 years, then why get so angry about it?

11. **woit**
    August 5, 2006

    About Hmm,

    I wish string theory proponents would get their story straight. Half the time I'm someone who doesn't know what I'm talking about, so shouldn't be listened to because I've got it all wrong, the other half of the time, the problem is that I'm saying things that are obviously true to all trained string theorists, so I shouldn't be listened to because I'm boring.

    Marty,

    In the future I'll take up Hmm's offer to delete his comments. While he promises to stop posting here, from past experience he doesn't seem to believe in keeping his promises.

    I do want to avoid deleting comments from string theorists, no matter how offensive, because I've been accused by them of deleting comments I disagree with. One correspondent wrote to tell me that a string theorist had told him that the reason one doesn't see sensible responses to criticism of string theory on my blog is that I delete these.

    Unfortunately Hmm and Lubos are not alone in their behavior and attitudes in the string theory community. The recent behavior of Susskind shows that this kind of thing is more widespread, involving some very prominent people.

12. **LDM**
    August 5, 2006

It didn't take any "courage" for Peter to do what he did. -Hmm

One might ask exactly how much courage does it take to work in a discipline, like string theory, where it seems nothing can ever be tested by experiment — and hence there is never a risk of loss of scientific reputation, or ego, by any of your ideas being proved wrong in the lab.

13. **Thomas Larsson**
August 6, 2006

If anything your book made me realize string theory may very well not be the "Holy Grail" its proponents purport it to be.

"Holy Grail" = non-existent thing which generations of our best and brightest wasted their lives searching for.

14. **Thomas Mulligan**
August 7, 2006

I think the comments of Dr. Lubos and "Hmm" should be retained; they serve to illuminate the very real insecurities present in the string theory community. I do not doubt the noble motivations of string theorists, but even the most objective scientists cannot help but attach an unwarranted affinity to concepts they've spent careers studying. Most confront foundational problems with polite resistance; the two mentioned above appear to employ only ad hominem attack: the first refuge of an insecure intellect is insult.

The work Dr. Woit does here is as important to the advancement of physical theory as anything you'll find on arXiv; every scientist has a duty to ensure that our accepted theories are empirically adequate, regardless of whether or not that requires an attack on orthodoxy. If string theory becomes a genuine, potent scientific theory, it will be as a result of Dr. Woit's criticism and not despite it.

15. **D R Lunsford**
August 7, 2006

Thomas,

I agree about Peter, but I don't agree about the noble motivations of string theorists. I suppose I use a different definition of nobility, such as could be justly applied to Pauli, Dirac, etc. among other people who respected the truth. But it's just my opinion.

-drl

16. **nigel cook**
August 7, 2006

The widely agreed principle maintaining string theory is:

We are right because **everyone else is wrong.**

By and large the public agree – ie stringy hype works – because the public can't get to see any alternatives clearly; this is due to the stringy hype and censorship of physics by group think stringers. If the public could see all the alternatives, the status of physics would be reduced from a professional objective group enterprize into what would appear chaos. So they have to censor out the alternatives, or physics is finished as a respectable discipline a far as they are concerned.

17. **Peter Orland**
August 7, 2006

Most of the comments on this blog concern the value of string theory as opposed to other approaches to quantum gravity and unification. It surprises me that no one seems to have suggested that part of the controversy isn't the solution but the problem.

NO theory of everything is going to do any better than string theory has. What does Loop Quantum Gravity predict? Or dynamical triangulations?
The difficulty is that any serious attempt to deal with the Planck scale can't confront the world in the TeV range. Any approach to these specific problems needs hype to make the public think it is of overriding
importance.

There are challenging unsolved problems which have nothing do with black holes and Planck scale unification. The culture of the field these days is,
unfortunately, that little else is of interest. When I was a student, most of the students were obsessed by such problems, but eventually faculty brought them back down to earth by showing them other things to do.

I am not disputing that quantum gravity and unification are important problems. They are, and I like to think about them too. But there are other interesting problems of physics which are also challenging and have a better chance of being tested experimentally. I don't think it will take any less brain-power to solve High-Tc
superconductivity, quark confinement or turbulence than to solve quantum gravity. Dark matter and dark energy may have nothing to do with Planck scale physics. Many of the people (like me) who do work in one or more these other areas are as smart as they come (unlike me).

In practice, some string theorists do look at things like AdS/CFT, which
may not be relevant experimentally, but has definitely advanced our field theoretic knowledge, and will probably do so for some time. I also am sympathetic to, but more dubious about AdS/QCD, which seems
to be another strong-coupling approach (we have been able to do strong-coupling calculations since Wilson's '74 paper. No one has
convinced me AdS/QCD is any better). This, however, is the minority
of string theorists.

Part of our job as scientists is to be scholars. That means we have to be
knowledgable about many things. We can argue all we like about which quantum gravity theory is best, or if we instead should go back to the drawing board. I can (and do) participate in such discussions with colleagues. But shouldn't we also think about other issues?

There was some famous quote about Academia being so vicious because
the stakes were so small. I am not finding this joke funny these days.

18. **Who**
August 7, 2006

In Peter Orland's second paragraph
http://www.math.columbia.edu/~woit/wordpress/?p=439#comment-14267 the question

What does Loop Quantum Gravity predict?
is asked rhetorically, as if the expected answer were "nothing". In some people's view it is not a rhetorical question.

There are a bunch of non-string QG approaches (often referred to under the heading of LQG because it's a familiar term) that predict various things which are testable. This has permitted or will permit some proposed models to be falsified. There are also some "generic" predictions shared by a broad class of QGs. This is discussed in http://arxiv.org/abs/hep-th/0605052

The above link is to a draft chapter of a forthcoming book
Approaches to Quantum Gravity – toward a new understanding of space, time, and matter, edited by D. Oriti, to be published by Cambridge University Press.

Another contribution to the same book has bearing on testable QG predictions. It is the draft chapter by Shahn Majid
http://arxiv.org/abs/hep-th/0604130

In his chapter, Majid says:
This is also the first noncommutative spacetime model with a genuine physical prediction[1], namely a variable speed of light (VSL). The NASA GLAST satellite to be launched in 2007 may among other things be able to test this prediction through a statistical analysis of gamma-ray bursts even in the worst case that we might expect for the parameter λ ~ 10-44 s (the Planck timescale).

19. **anon**
August 7, 2006

"NO theory of everything is going to do any better than string theory has… any serious attempt to deal with the Planck scale can't confront the world in the TeV range."

Just because strings have failed miserably, doesn't prove it is impossible for others. "Not Even Wrong (N.E.W.)" gives indirect tests such as getting the vacuum energy in supersymmetry (unification energy) to agree with empirical observaton.

String theory is apparently way out by an astronomical factor of 10^113 (N.E.W. page 179). Another empirical check is that according to unification theories you should be able to predict the way one fundamental force varies with collision energy, given measurements on how the other forces vary as a function of energy. This is a test since data are accurate withi about 3%. String theory fails here too (N.E.W. page 177) where the value of the SU(3) force predicted by SUSY using SU(2) and U(1) forces is higher than experimental data by 10-15%.

So there are a few indirect tests possible and it is conceivable that some other theory could make progress by correct agreement with these data where strings/SUSY can't. Another option is some theory which is so radical it may predict masses (Tony Smith being one example) and be checked experimentally that way.

20. **Peter Orland**
August 7, 2006

Who and anon,

It wasn't my purpose to put anyone on the defensive. That said, I don't find your arguments concerning expermental predictions any more convincing than the string theorists's.

If you love LQG or another approach (or even decide you want to work on strings!), do it with my blessing. I am just complaining that so many in our field think all life exists at the Planck scale.

21. **Who**
August 7, 2006

At least one important version of LQG risks falsification next year by astronomical observation—in my view the most promising spinfoam approach actually—if energy-dependence of the speed of light is not observed in gammaray bursts.

P.O. : *It wasn't my purpose to put anyone on the defensive. That said, I don't find your arguments concerning expermental predictions any more convincing than the string theorists's.*

…I am just complaining that so many in our field think all life exists at the Planck scale.

Be happy then. 🙂 You did not put anyone on the defensive, but merely showed your lack of familiarity with the subject! No one needs to provide arguments to "convince" you of QG testability, since one simply has to point to cases where non-string QG models **have already** been constrained or are **at risk of refutation** by having their predictions falsified empirically.

If it distresses you that so many have their attention focused at Planck scale, then here is some news to cheer you up—we aren't stuck down at Planck scale: non-string QG phenomenology has ample scope at the scale of practical near-term observation.

22. **Haelfix**
August 7, 2006

The Planck satellite could in principle pick up QG signatures, and its somewhat of an on going debate in the string theory community if it has the resolution to see Stringy effects or not.

So while its not a falsifiable prediction, if it does see something it would be wonderful for the entire field. If it doesnt, well it could mean a few things.

OTOH, With further theoretical refinement it could potentially be upgraded to falsifiable lvls, so keep that in mind. I believe people at Columbia are actually working on this as we speak, so perhaps Peter can ask his colleagues in the physics department what they think.

23. **Bee**
August 7, 2006

Hi Peter,

thanks for the link. I didn't comment very much on the 'physical' content of the book. I kind of expect that Lubos will take care of that… would be good if you had the time to write a sensible review on Lee's book as well. I'd really be interested in your opinion.

Btw, did you receive an offer from Lee's publisher to send you a copy of the book? I got a rather weird comment saying that she 'of course' did not ask you. No idea what that's supposed to mean.

Best, B.

24. **Michael Edwards**
August 7, 2006

Bee,

Just wanted you to know that some readers of your review got (and enjoyed) the Bigfoot / Big Five joke (and the big game icons) even if a certain person at Harvard didn't. "Female physics" my (big) foot.

Cheers,
– Michael

25. **Peter Woit**
August 7, 2006

Hi Bee,

Supposedly a copy of Lee's book is in the mail and I should see it soon. Of course I'll write something about it here after I've read it. I've heard from Lee quite a bit about the book, going back to when he first started working on it. No idea what his publisher's comment was about, perhaps they're a bit competitive with another publisher… But I think both Lee and I see the appearance of the two books around the same time as a good thing rather than a competition. Our points of view are in many ways complementary and the fact that we reach similar conclusions about string theory from different starting points reinforces what we each have to say.

Haelfix,

For the latest on possible imprints in the CMB, see Brian's talk at Strings 2006

http://strings06.itp.ac.cn/talk-files/bgreene.pdf

As far as I know, no one has ever made the claim that this can potentially falsify string theory. The size of effects coming from Planck scale physics is debatable, and depends on the string scale, which is not known. It has to be at large enough distances for you to have a chance to see something. I think you have to be quite an optimist to believe that they're going to be visible in the Planck satellite data. But it certainly would be very remarkable if such effects are seen. One thing to keep in mind is that these calculations typically don't actually involve string theory, more things like assuming that there is a minimal length, and QFT modes below that length are cutoff. So, these calculations are sometimes described as "string-inspired".

26. **Peter Orland**
August 9, 2006

Hi Peter,

You have just reiterated my point about no theory of quantum gravity being falsifiable. Finding a minimal length (a UV cut-off) proves everybody right and nobody wrong. If such a length isn't detected by
the observations, I won't be surprised if everyone finds a way to wiggle out of it and say "I knew it all the time".

Again, I appeal to those of you hotly debating Planck-scale issues to spend time thinking about at theoretical physics at distances above 10^-33 cm. If that isn't your main interest, try to make it your hobby.

Regards,
Peter O.

Nobel Prize Winning Orgiasts

August 2, 2006
Categories: Uncategorized

DealBreaker, which is described as "an online business tabloid and Wall Street gossip blog", has a story about supposed Jeffrey Epstein parties "in which Nobel prize winners and various wealthy folks were all surrounded by young, 'nude eastern european girls, frolicking with them, and then proceeding into one big orgy party.'" The story refers hopefully to the idea that this might have something to do with the physics symposium in St. Thomas funded and organized by Epstein that was mentioned here.

Update: When I wrote this blog posting last night, it was purely based on the posting at DealBreaker, which appeared to be a silly fantasy, based I assumed on some highly exaggerated version of something that happened involving consenting adults at an Epstein party. The idea of Gross-Wilczek-'t Hooft-Hawking participating in an orgy at the conference Epstein sponsored was obviously a joke, although perhaps a bit of a tasteless one. I was completely unaware of the serious accusations against Epstein and of the fact that charges have been filed against him involving his sexual behavior. Given this context which I didn't know about, the joke isn't funny.

Epstein has been exceptionally generous to the math and physics community over the years. He's entitled to the presumption of innocence and I don't think this blog is an appropriate place for discussion of his case. So I'm shutting off further comments on this posting.

Comments

1. **Chris Oakley**
   August 3, 2006

   Peter, this is one link I wish you had not provided.

   Firstly, I find that the image of Stephen Hawking being pleasured by a bevy of Eastern European beauties is one that I do not wish to contemplate.

   Secondly, the best I ever managed to do on the junket count – which was when I worked as a quant in the City of London – was a visit to a roped-off area at Royal Ascot paid for by a broker. It was awkward as we felt morally obliged to demonstrate (financially) macho credentials by placing large bets. Luckily one of my horses came in, otherwise it would have been expensive for me, too. A nice day out, but no hired help other than the driver, who was singularly unattractive, and a man.

2. **D R Lunsford**
   August 3, 2006

   Not Even Hung

   -drl

3. **Louise**
   August 3, 2006

   Someone should note that Stephen hasn't won the Nobel. Are there similiar parties for woman scientists?

4. **Steven H. Cullinane**
   August 3, 2006

   From the DealBreaker story:

   "There is no agenda except fun and physics, and that's fun with a capital 'F,'" Epstein said.

   May the F be with him.

5. **ObsessiveMathsFreak**
   August 3, 2006

   Would this be a good time to bring up the social habits of ancient greek mathematicial philosophers?

6. **Jeremy**
   August 3, 2006

   I would be happy to arrange even better parties for female physicists.

7. **Cynthia**
   August 3, 2006

   Peter,

   Extremely tasteless of you to create a post on this tabloid trash! Simply put, you appear to be trapped on one side of the mountain with zero classical energy. Perhaps quantum tunneling is the only way you can safely make it back to the other side…Good luck…

   Best wishes,
   Cynthia

8. **LostHisMarbles**
   August 3, 2006

   `nude eastern european girls' what's wrong with nude western european girls? Why bother to watch soap operas when one can tune in here? Why wasn't I invited?

9. **Jeremy**
   August 3, 2006

   LostHisMarbles: As one of the more popular theories go, the collapse of the Soviet Union was not due to an inability to keep up with the expensive technological innovation and economic power of the United States, but rather because they had long, cold winters, plentiful vodka, and some of the hottest women on the planet. Why fight a cold war?

10. **Jean-Paul**
August 3, 2006

Louise — there were also women scientists there — you can see Lisa Randall in Larry Krauss' photo gallery
http://www.phys.cwru.edu/events/grav_gallery.php
Why all these bright people need somebody to pay for their Carribean vacation? They can't afford it? What a filth…

11. **anon**
August 3, 2006

Stephen Hawking is a definite suspect. He has lapdances in Peter Stringfellow's nightclub, London, in 2003 and although most media tried to censor the news it leaked out. The Scotsman newspaper even reports the teenage lady's name:

"In July [2003] the Lucasian professor for Mathematics at Cambridge University and author of A Brief History of Time spent five hours at Peter Stringfellow's lapdance club, the Cabaret of Angels, enjoying the gyrations of a 19-year-old dancer called 'Tiger'."

– http://thescotsman.scotsman.com/s2.cfm?id=1080412003

The BBC website naturally reports that the experience was beneficial to Professor Hawking's health:

"… I heard a rumour that Man City have just signed the lapdance-loving Professor, Stephen Hawking …"

– http://www.bbc.co.uk/ouch/news/btn/timhoward.shtml

12. **Tommaso Dorigo**
August 3, 2006

Hey all,

first, please don't complain if Peter throws in some trivia about physicists in here. I, for one, couldn't help laughing out loud while reading the post and the comments that ensued.

Second, what's wrong with an orgy ? An orgy never, or very seldom, kills anybody. Come on. You straight thinkers go and deal with more pressing issues, such as corpses of children hit by bombs in Lebanon. These kinds of outraged reactions to anything that has to do with sex (a healthy activity in general, and like many other healthy activities at times misused) are ridiculous and only tag narrowmindedness and bigotry.

And personally, if I was confined in a wheelchair and decided that my life was still worth living, I would have no shame in contemplating the body of a young dancer on my lap if the chance arose. Give me a break.

Cheers,
T.

13. **LostHisMarbles**
August 3, 2006

Is a party in the Carribean cool if the girls are hot?

14. **Visitor**
August 3, 2006

It might turn out to be not as funny as it seems; a posting on Angry Physics lead me to Motl's post on his blog about it, http://motls.blogspot.com/2006/08/jeffrey-epstein-arrested.html and that lead to this link to the Harvard Crimson: http://www.thecrimson.com/article.aspx?ref=514049 . Motl says Epstein has been arrested but the Crimson might be confusing "getting arrested" and "being indicted" – their reportage is not exactly clear on this point.

15. **deloprator2000**
August 3, 2006

Hopefully Mr. Epstein checked that the "nude eastern european girls" were clean. I could just imagine t'Gooft and Stephen Hawking checking into an STD clinc, I wonder if his computer has the words for "it burns when I pee".

16. **LostHisMarbles**
August 3, 2006

It makes you wonder if `nude western european girls' might not have been a better choice.

17. **justin**
August 3, 2006

This is really despicable. Peter, spreading these kind of debauched unsubstantiated rumors is very hurtful. Even the suggestion that someone as thoughtful and respectful as t'Hooft would engage in this kind of activity is infuriating.

18. **Chris Oakley**
August 3, 2006

Proposal for career advancement for Louise:

• Send resume, etc. to Stephen Hawking. Make you include photographs, e.g. the one posted on 3rd August blog entry.
• Upon arrival in Cambridge attempt to get said Lucasian Professor of Mathematics into compromising situation (shouldn't be hard, on the basis of current evidence).
• Use opportunity to tamper with his speech synthesizer, to enable remote control from mobile phone.
• At public lecture given by said Lucasian Professor, make him announce arrival of most brilliant cosmologist ever, i.e. self, and the necessity of bestowing unlimited academic honours, etc.

19. **Aaron Sheldon**
August 3, 2006

So let me get this straight, an unknown predator can stock childern and your reaction would be shock and horror. But a billionaire preys on childern and its a good source for some B-sheet humor?

You ever see the consenquences of this kind of predatory behaviour first hand Peter?

This is nearly as low as Lubos.

It would be best to apologize and possibly remove this post.

20. **D R Lunsford**
August 3, 2006

C'mon, I don't like string theorists either, but you needn't call them children.

-drl

Yet More Links

August 7, 2006
Categories: Uncategorized

A pretty random collection of interesting things I've noticed recently:

The Mathematical Institute at Oxford has a newsletter, and from the latest issue I learned that Quillen is retiring and that they're planning construction of a new building. There are quite a few other articles worth reading in the newsletter, including one about George Mackey.

There's a long interview with Lawrence Krauss on the web-site of the Cleveland Plain Dealer.

Jennifer Ouellette at Cocktail Party Physics has a nice posting about Sonya Kovalevsky.

The International Congress on Mathematical Physics (ICMP) is taking place in Rio this week, and here's the program. Victor Rivelles is blogging from the conference, and says that talks will be put online after the conference. I agree with his comments about Witten here.

Tommaso Dorigo has some excellent recent postings about new results from the Tevatron on the top quark mass and the search for the Higgs. It looks like the Tevatron's best bet for finding the Higgs (or for ruling it out in some mass range above the range already ruled out by LEP) will be if it's around 160 GeV.

Also from Fermilab, there are new results from MINOS on neutrino oscillations. Sometime soon MiniBoone is supposed to be "opening the box" on their blind analysis of the data and reporting results. Anyone know when?

Comments

1. **LostHisMarbles**
   August 8, 2006

   One needs more posts about top quarks mass (+decay modes, CKM matrix els?), Higgs searches, etc, although I suppose that's not really the brief of this blog. I'm not bothered by what anyone thinks of Witten. (I've met him. He's a nice guy. I had nothing to say to him and he had nothing to say to me and that was the end of our non-collaboration.)

   FWIW does anyone know what has become of the pentaquark?

2. **Brett**
   August 8, 2006

   The pentaquark does not appear to exist. Higher statistics measurements in pretty much all the channels where something was originally observed do not show its presence.

3. **D R Lunsford**
   August 8, 2006

   LHM, you know that story of Feynman and Dirac? as best I remember, when they met, Dirac was still in an eigenstate of taciturnity and Feynman in one of speechless Dirac worship*, so they didn't have much to say to each other:

   Feynman: "That was a beautiful equation!"

   Dirac: "It was a long time ago. Are you trying to get an equation?"

   Feynman: "I'm working on mesons. It is hard."

   Dirac: "One must try."

   -drl

   *Pauli – "There is no God, and Dirac is his prophet."

4. **LostHisMarbles**
   August 8, 2006

   First meeting between RPF and PAMD.

   F: I am Feynman (extends hand)

   D: I am Dirac (extends hand)

   pause

   F: (awed voice) It must have felt good to have invented that equation.

   D: But that was a long time ago. (Pause) What are you working on?

   F: Mesons.

   D: Are you trying to discover an equation for them?

   F: It is very hard.

   D: But one must try.

5. **G Jungman**
   August 8, 2006

   I disagree with the Rivelles characterization of mathematical physics as something that would have “no prediction that has been experimentaly verified yet”. How about the proof of the stability of matter? Or how about rigorous work on Navier-Stokes or in statistical mechanics? Let’s not smear mathematical physics by comparing it to string theory.

6. **Tommaso Dorigo**
   August 8, 2006

   Hi Peter,

   thanks for the link! You should do that more often, I got 200 visitors from your site today 🙂

   LostHisMarbles: I will try to post more about the most recent stuff going on with top quarks at the Tevatron in the future. There are some nice things coming up real soon in fact.

   Cheers all,
   T.

7. **LostHisMarbles**
   August 9, 2006

   Tommaso’s post about the Higgs searches contains a revealing statement ~ each one of the solid and dashed curves represents years of analysis. That’s precisely the problem. It takes YEARS of painstaking effort in expt HEP, to nail down the most basic parameters for a theory proposed 30 years ago, and meantime no expt evidence for anything beyond SM has surfaced. Theoretical HEP just can’t wait that long. There’s just no expt guide as to what lies beyond the SM (as opposed to the 1950’s when every new accelerator produced puzzles faster than theory could absorb.) One can speak of dark matter and dark energy etc (from cosmology), but these concepts are not amenable to precise lab measurements. Even if one makes hypotheses there’s no way to do a controlled expt to test the ideas. One can blame string theory for all sorts of ills, but the crisis (if I may call it that) is nobody’s fault.

   The real problem is to lose the commitment to press ahead with expt HEP (the ILC or muon collider or VLHC etc). One can say that ST is Not Even Wrong, but it is far worse to Not Even Try.

8. **QWERTY**
   August 9, 2006

   KOVALEVSKAYA WAS AMAZING, HER WORK ON THE EQUATIONS FOR A SPINNING TOP USING ELLIPTIC FUNCTIONS DEFINATELY DESERVES THE BORDINS PRIZE. I STARTED LEARNING ABOUT INTEGRIBLE SYSTEM BECAUSE OF THIS.

   IS IT TRUE THAT WEIERSTRASS WAS HOPELESSLY IN LOVE WITH HER? WHAT ABOUT MITTAG-LEFFLER?

   ALSO IN THE PAPER SHE SUBMITTED FOR THAT THERE IS A FAMOUS QUOATATION WHICH IS TRANSLATED AS
   “SAY WHAT YOU KNOW, DO WHAT YOU MUST, COME WHAT MAY”
   BUT DOES ANY BODY (HAHAHA THIS WAS MY JOKE) KNOW WHAT IT SAY IN THE ORINGAL FRENCH?

9. **Walt**
   August 9, 2006

   It always seemed to me that Kovalaskaya was the first person to discover that partial differential equations have characteristic directions, but I’ve never seen a definitive statement that this is so.

10. **Christine Dantas**
    August 11, 2006

    Dear Peter Woit,

    Sorry if this has been already posted:

    A ‘Landscape’ Too Far? Science 11 August 2006: Vol. 313. no. 5788, pp. 750 – 753.

    Christine

11. **Christine Dantas**
    August 11, 2006

    There is also this one:

    You are made of space-time.

    Christine

12. **John A**
    August 11, 2006

    Apologies Peter,

    I’ve no idea where to express this, but I feel I must express my emotions somewhere.

    You’ll remember that I expressed my (mild) criticisms about string theory on Lubos’ blog, only to have them deleted and Lubos asking me on Climate Audit not to “nuke” his blog with my comments.

    Well, speaking of nuking, Lubos allows this comment on his blog with nary a word of rebuke let alone deletion. I can only feel that Lubos is quite, quite mad and not merely a reactionary ideologue. Of course comparing Silvio Berlusconi, Italy’s most corrupt politician of the last twenty years, with Jesus Christ makes me wonder how shakey is Lubos’ grip on reality.

13. **Peter Woit**
August 11, 2006

John,

Lubos is nuts, and his commenters are mostly even crazier. The only reason he isn’t completely ignored by the entire world is that he’s the most prominent proponent of string theory on the web, and for some people, some how, this causes them to ignore his crackpotism and lunacy.

14. **John A**
August 12, 2006

Peter,

Thanks for that. I don’t want to derail your weblog with all things Lubos, so we’ll leave it at that. I’ve vented and got it out of my system.

John

Topology Board Resigns

August 10, 2006
Categories: Uncategorized

One of the most prestigious journals in mathematics is called Topology. It is based at Oxford, its first issue was in 1962 and it has published many of the most important papers in the the field of topology. Since 1994 it has been published by Elsevier, and many mathematicians have been concerned over the high price that Elsevier has been charging for the journal ($1665/year). Today the entire editorial board of the journal resigned, effective the end of the year. In their resignation letter, they stated:

…the Editors have been concerned about the price of Topology since Elsevier gained control of the journal in 1994. We believe that the price, in combination with Elsevier's policies for pricing mathematics journals more generally, has had a significant and damaging effect on Topology's reputation in the mathematical research community, and that this is likely to become increasingly serious and difficult, indeed impossible, to reverse in the the future.

A few years ago a group of editors from another Elsevier journal in the area of topology, Topology and its Applications, also resigned, for similar reasons. They founded the new journal Algebraic and Geometric Topology, a free online journal (that also has an annual printed volume). One of this group was my Columbia colleague Joan Birman, who wrote an article for the AMS Notices about the issues involved.

Berkeley topologist Rob Kirby, back in 1997, wrote a letter to Elsevier that also discusses these issues. John Baez has a web-page about this that he has just updated to include information about the Topology situation, including a copy of the resignation letter.

Comments

1. **LostHisMarbles**
   August 11, 2006

   Dear Resigning Editors,
   By all means start a new (free) online journal. But be prepared to bear the burden of administration by yourselves.

   LHM

2. **MathPhys**
   August 11, 2006

   If all parties involved are minimally smart, with minimal common sense, the administration of an online journal is minimal. A typical academic, teaching a typical calculus class for engineers, with a web page and all, has comparable admin work on his hands.

3. **fritz**
   August 11, 2006

   there is another quite interesting piece of info about elsevier,
   they are involved in arms trade. take a look at that:

   http://www.physics.carleton.ca/~logan/elsevier/

4. **LostHisMarbles**
   August 11, 2006

   I am not convinced that the administration of an online journal is minimal. Physical Review Special Topics is online-only and free (no page submission charges and no subscription fees), but it is part of a larger umbrella organization (in this case APS journals). An online journal is much more than maintaining a web page and some servers.

   But — give it a shot! ~ "Online Topology" why not? And certainly divorce the journal from arms sales.

   BTW many companies (e.g. Boeing) make both civilian and military products. Many universities (e.g. MIT) have contributed heavily to defense research. The military industrial complex reaches far and wide. Microwave ovens are one of their products. So are bell-bottoms.

5. **Kyle**
   August 11, 2006

   I'd love it if this resulted in (or was the result of) a growing trend away from Elsevier. It isn't necessary that something be free; merely reasonably priced would be an improvement.

6. **DMS**
   August 11, 2006

   I think it is about time. Some mathematics journals are highly overpriced.

   I must say one extremely positive contribution of string theory has been the arxiv, which has now been embraced by the rest of physics, and to a smaller extent other fields like mathematics. It is about time the mathematicians are as enthusiastic about it; many significant papers in mathematics are still not freely available. Perelman, and a recent long proof by Morgan and Tian are notable example. I doubt Perelman's institution can afford the subscription of many math journals.

   In particle physics, the highly rated JHEP is a recent online journal that started out free. My understanding is that the subscription rate is not high(in fact, free for developing countries…)..

7. **Chris Grant**
   August 11, 2006

   On bang-for-the-buck metrics like cost per page or cost per citation or cost per recent citation, Elsevier math journals are priced in line with the journals of other commercial publishers like Springer and Wiley (and substantially cheaper than Taylor & Francis).

8. **John Baez**

August 11, 2006

LostHisMarbles writes:

But — give it a shot! ~ "Online Topology" why not?

There's no need for the former editors of Topology to start a new free online journal. A bunch of former editors of Topology that resigned several years ago have already started two such journals: Geometry and Topology, and Algebraic and Geometric Topology!

These journals are quite successful, and they're endorsed by SPARC, the Scholarly Publishing and Academic Resource Coalition. This is an alliance battling the dysfunctionality of the current system where journals are run by big conglomerates like Reed-Elsevier, with academics doing most of the work and earning practically none of the money.

So, the editors of Topology that just resigned can join their old friends and help edit these new journals.

9. **Dick Thompson**
August 11, 2006

This is a fascinating social dynamic at work One of the mainstays of the "Gutenberg Galaxy" was its total incorporation as the atmosphere of the intelligensia. What does everybody think the foreseeable social consequences will be from this accelerated flight from the dead trees and their marketplace? More isolation from the hurly-burly that includes ambitious soap salesmen? But availability to anyone who can learn about it?

10. **Bob McNees**
August 11, 2006

When I was a grad student we had an informal rule: don't submit to Elsevier journals. This was part of a boycott that I was told a lot of groups quietly took part in. The reason was that the journals were overpriced. Almost embarrasingly so. I was lucky enough to attend a school that could afford them, but many universities outside the US can't. I don't really know if those informal boycotts had any impact. I doubt it. But I got in the habit of not submitting there, and I know a lot of people who do the same thing. Maybe, as events like these become better publicized, Elsevier will take notice and change their practices. I doubt that, too.

I think there are useful parallels with the way the recording industry works. For a long time the need for distribution insured that the middle man who could get content from the artists to the consumer had a place in the business model. Self-distribution was cost prohibitive, so artists didn't really have a choice.

We've already begun to see the breakdown of this model in the academic publishing industry, at least at the level of journals, as free peer-reviewed journals come online and gain credibility. In High Energy Theory, I think JHEP is regarded as highly as Nuclear Physics B, if not more so. And, as DMS says, there's the arXiv. It seems like suicide for companies like Elsevier to keep trying to squeeze income out of their outdated and unfair business model. But if they choose to do that instead of adapting, they deserve what they get,

11. **LostHisMarbles**
August 11, 2006

I was thinking that there is a parallel with what is happening in the music/film/recording industry.

John Baez mentions on his website (among other things) ~ publish in free online journals or **start your own**. The most obvious pitfall here is quality control. In the good old days, there were a (relatively) few prestigious journals, but of course subscriptions have become ever more expensive. One naturally searches for an alternative. But if anyone can start a journal, who is to validate the quality? Baez also points out (in a reply to a post by myself here) that there are organizations like SPARC. So — it falls to the mathematical community to organize its own (free) journals and maintain what amounts to an accreditation board for quality control.

I read much praise for the arXiv, but I note also that PW has had a long-running battle with it. The arXiv is useful, but is by its nature of uneven quality.

My guess is that eventually the responsiblity of maintaining these online journals will cause the task to be delegated to a few poeple (not unlike a democratic govt which chooses to use elected representatives), effectively a new species of publisher. We shall see how the dynamic of online publishing plays out.

It seems we are living out the ancient Chinese curse "may you live in interesting times".

12. **Moshe**
August 11, 2006

I think one should not care what Elsevier and others are doing, and have a campaign to change their business practices. Even if they had no profit margin at all, their journals would still be too expensive!

That business model – the print journal- is antiquated and has to go. The new business model- peer-reviewed refereed journals run and published online by acedemics- is much better (also cheaper). Given the clear advantages, I'd be surprised if there still are any print journals left, say 10-20 years from now.

As for the administrative duties, the point is that even in the Elsevier journals most of them are done by us (referees and editors), additional tasks (once the journal is up and running) are fairly minimal, since lots of things can be automated when they are done online.

13. **Jean-Paul**
August 11, 2006

I am not so sure about bright prospects of online publishing. There must be good reasons why paper survived several millenia. Online journals will have to face tough challenges when the evolution of operating systems accelerates. It is quite possible that a software engineer won't like "The Large N Limit of superconformal…" and sends it to a trash bin.
Furthermore, at least in HET, the quality of online journals is lower than the traditional ones. JHEP's quality is much below Nuclear Physics or Physics Letters. The refereeing process needs not only academics but also professional editors.
I was always wondering when looking at some JHEP papers: is there anybody there who corrects English grammar? When I referee a paper, I don't fix spelling error or rewrite franglaise.

14. **ObsessiveMathsFreak**
August 11, 2006

I'm a fairly recent graduate student. I've grown up with the internet and in a culture of easy access to information. I was initially very surprised and to be frank, offended when I learned of the current staus quo of artifical barriers in science imposed by publishers. Are you people mad? Why do you sign away your copyrights?

I don't subscribe to a single print journal, and every paper I have is electronic. In addition, aside from what my university has electronic access to, I don't have access to electronic journals either. I'm not alone amoung my age-peers, or even those a little older. People who've grown up with the internet would find this situation bemusing if it wasn't such an obstacle.

I don't consider myself at any great disadvantage. When I initially went to the (considerable) trouble of trying to obtain the complete list in a bibliography, I found that most of the papers weren't of much use to me anyway. Papers under a publishers lock and key really just aren't worth the time or effort you put into getting them. Especially the time.

The way I look at it, if you want a publisher to cut off access to your paper, that's fine. Just don't expect me to read it, much less cite it. I'm not going to jump through hoops for your sake, and I don't expect my university library to waste money doing so either.

15. **John Sidles**
    August 11, 2006

    See Donald Knuth's commentary:

    http://www-cs-faculty.stanford.edu/~knuth/joalet.pdf

16. **SteveM**
    August 11, 2006

    Elsevier are a bunch of gangsters. They also have a vast archive of important material going backs decades that is locked up and now costs a fortune to access. Total ripoff and downright criminal. This material collectively belongs to the scientific community–not them! I needed some old Physics Reports articles from the 70s and 80s and ended up going to the original authors to try and get copies. I succeeded: they were sympathetic and felt that their old articles should be easily available for free or low cost, but it was still a hassle. If Elsevier go under it will be great day.

17. **Michael Edwards**
    August 11, 2006

    In my opinion the only justification for the continued existence of publishers – as distinct from editors – is to support retail distribution of physical media. I really like the retail model; I like having two excellent new bookstores (one indy, one relatively benign chain) and two excellent used bookstores within walking distance. Ditto recorded music, video, newspapers and magazines. And out in the real world the way editors get paid is by publishers, who amortize off the cost over the market life of the media; it's a rare author who will personally finance the cost of turning a manuscript into something that someone else will want to read more than once. So when I want a book or a CD, I shell out.

    But I can't exactly go to the bookstore and browse the latest copies of Phys Rev and JAMS. And from what I hear their publishers don't finance editorial costs, academia collectively does (with the help of its government paymasters). So I don't care whether paper journals continue to exist, as long as the Library of Congress archives the online repositories and at least one nonprofit per continent will monitor the citation patterns and bind important-looking papers together for the benefit of libraries in internet-poor countries.

    Modern journals are such a mountain of crud anyway that literature searches have to be done via something like citebase, and I expect the PDF to be a click away. Of course, it's good to interact directly with individuals from time to time and ask them what papers they personally consider important and/or well written. But why do I need a filtering service that's anonymous to me, the journal reader, when I have the citation history at my fingertips? Most of us prefilter new papers by authorship (and to a lesser degree institutional affiliation) anyway, so we don't need journal acceptance as a criterion.

    Now what was it that anonymously refereed journals were for in the first place? Oh yes, to keep the quality up and the cronyism down, so that readers are occasionally exposed to new authors with something original to say. There may be fields in which the journals are still serving that function, even Elsevier's – for instance, Am J Otolaryngology looks to be justly top in its field, although I haven't actually read many articles out of it because I have to pay $30 a pop or shlep up to the University – but mathematical physics doesn't seem to be one of them.

    If I ever write something that I think belongs in the primary literature, I'll go around pounding on doors until I find a prominent researcher who's willing to endorse it as a coauthor and upload it to the arXiv. That's effectively how the system works now, and since I have no aptitude or desire for an academic career, sharing the credit will cost me nothing.

    Cheers,
    – Michael

18. **Chris Grant**
    August 11, 2006

    SteveM:

    According to WorldCat there are 460 libraries that subscribe to _Physics Reports_. Couldn't you just go to the library nearest you and photocopy the articles you want? Afraid that Elsevier's goons would kneecap you on the way?

19. **Brett**
    August 11, 2006

    The prices of some journals are ridiculous, but there isn't that much one can do about the situation. Nuclear Physics B is an important example. This is a very prestigious journal; the only ones in particle physics that are thought of more highly are Physical Review Letters, Physical Review D, and Physics Letters B. JHEP is a respected journal, but it is not in the same league–impact factors notwithstanding. This is not a commentary on the actual quality of what these journals publish, but rather on how they are perceived based on past history–and in the age of electronic publication, the past quality of a journal is actually much more important than the present.

    Nobody reads the print versions of journals any more, and many libraries have dropped the print versions entirely. (Nobody can sit in the library and leaf through Physical Review Letters any more, since the last time I checked, there wasn't a reasonable way to get a paper copy of only this journal.) The bound volumes get sent away to storage, since people can read years' worth of articles from any computer on campus. But with an electronic subscription, there is no backup if the subscription is later cancelled. You lose access to everything, so even if Nuclear Physics B stopped publishing good papers tomorrow, it would still be worth the $15,000 per year (or whatever it is now) to use the online archives.

    Moreover, to ask a young researcher not to publish in Nuclear Physics B is simply unfair. An established physicist may have the luxury of picking and choosing, but for somebody without tenure, choice of journal can be crucial. If you want to publish a longish paper in high energy theory, your meaningful choices are quite limited. If you can't get it into Physical Review D, the Nuclear Physics B is where you have to try next. Anything else is a big step down, and publishing in most lesser journals won't mean anything it all to a hiring or promotion committee.

20. **AnotherObsessiveMathsFreak**
August 11, 2006

Computer Science Theory literature history has had at least 2 similar drives:

1. Journal of Algorithms Resignation in Dec. 2003: [*John Sidles*' link] Prof. Knuth's commentary was after his resignation from the board.

2. Forty Editorial Board Members Resign from Kluwer's 'The Machine Learning Journal' in Oct 2001.

21. **ObsessiveMathsFreak**
August 11, 2006

> Couldn't you just go to the library nearest you and photocopy the articles you want?

That's illegal. It might sound harmless, but it is illegal in the same way that downloading an mp3 is illegal. Why should I have to break the law to get my hands on a paper?

> Afraid that Elsevier's goons would kneecap you on the way?

Aren't you? Elsevier will take their cue from the software and entertainment industries. Expect ever more stringent terms and conditions on inter-library loans in the future, as well as some strongarming of your local institution.

22. **Eugene Stefanovich**
August 11, 2006

ObsessiveMathsFreak:

The way I look at it, if you want a publisher to cut off access to your paper, that's fine. Just don't expect me to read it, much less cite it. I'm not going to jump through hoops for your sake, and I don't expect my university library to waste money doing so either.

With this attitude you only impair your own ability to do research, that's all. Access to journals is absolutely essential to scientific work. In my studies I had many instances when I found crucial pieces of information and insight in obscure and hard-to-find publications. I think it is very important to have access to a good library. If your library is not so rich, you can always use interlibrary loan. In old times people used to write each other postcards with reprint requests…

I totally agree that in the Internet age there is no excuse for scientists to pay huge money to the printing industry. They can manage the online publication process themselves. Peer review, editing, etc. is not that difficult and expensive to arrange. Expensive journals are doomed.

23. **Eugene Stefanovich**
August 11, 2006

Couldn't you just go to the library nearest you and photocopy the articles you want?

That's illegal. It might sound harmless, but it is illegal in the same way that downloading an mp3 is illegal. Why should I have to break the law to get my hands on a paper?

Making one copy of an article for your own scientific research is perfectly legal. The copyright law encourages you to do that.

24. **Michael Edwards**
August 11, 2006

Er, no. The 1976 US copyright law codified the judicially created "fair use" doctrine as an equitable defense against prosecution for copyright violation. The Berne Convention doesn't go nearly as far, and most jurisdictions around the world have much weaker "fair use" provisions (if any). "Making one copy of an article for your own scientific research" is by no means automatically fair use, and relying on the historical non-prosecution habits of academic publishers is a risky proposition even in the US. You might also read up on contributory infringement, for which libraries and even authors who encourage such a practice may be liable. Best to stick to e-prints.

Cheers,
– Michael

25. **SteveM**
August 11, 2006

Chris,
At the time, living out in the country, the nearest library with the journal was just way too far away. Besides, as Obsessivemathsfreak points out the photocopying and loan conditions have gotton more stringent anyway, and since I had graduated with a doctorate I would then have had to rejoin the library at quite a cost. Just all a big hassle when I feel I should be able to access and download old material via the internet at reasonable cost. Incidently, if you want to pay for an obscure old paper from Elsevier via credit card , and you are temporarily not affiliated with any institution, it will cost you anything from $70-$100.

26. **Former string theorist**
August 11, 2006

As managing editor for a moderatley priced journal, I would like to point out that the value added in the editorial process for a well-run journal is significant. Diligent work by referees and editors (working without compensation) often greatly improves the content of the papers, and a careful copyediting process helps these results to be communicated much more efficiently. It is true that a great many journals from commercial publishers are overpriced, and the tactics used by some publishers are indeed disreputable, but for a more reasonably priced journal, which can basically only sell to libraries, the profit margin is much less than you might imagine. I support the ideas behind free online journals, but traditional refereed journals have much more to offer than simply an officially sanctioned citation list.

27. **Chris Grant**
August 11, 2006

Michael wrote: "*Making one copy of an article for your own scientific research" is by no means automatically fair use*"

And it is by no means automatically violation of fair use. Practically nothing in the law is automatic.

and relying on the historical non-prosecution habits of academic publishers is a risky proposition even in the US.

Oh, Puh-leze. There's less chance of an academic researcher in the U.S. being found guilty of copyright violation for photocopying an article from a journal for him to refer to in his research than of you being struck by lightning. Feel free to stay in your Faraday cage if you wish, but don't be disappointed if the rest of us don't join you.

28. **Michael Edwards**
    August 11, 2006

    Like I wrote, "publishers – as distinct from editors". In disciplines in which there is no functioning market economy, where people write and referee and edit at their granting agency's expense, the rational choice of "publisher" is a laser printer under my desk.

    If you want to publish a journal, run it like The Economist – snappy, well written and edited, topical, priced for home subscription with Internet archive access thrown in – and cross-subsidize it from a research arm that sells in-depth analysis in book form at a price that the market will bear. That's how Springer used to be run, and that's why a working mathematician's bookcase is liberally sprinkled with gold.

    For better or for worse, publishing is just as subject to Gresham's Law as any other industry. Fiat currency and gold don't circulate interchangeably for long.

    Cheers,
    – Michael

29. **John Sidles**
    August 11, 2006

    No one has yet mentioned PLOS–these biologists and medical researchers are well-organized:

    http://www.plos.org/about/principles.html

    http://www.plos.org/journals/index.html

30. **Michael Edwards**
    August 11, 2006

    (This is off topic for Peter's blog, so I'll provide a reference and some analysis and leave it at that.)

    If you have the patience to read beyond the verdict into the judicial reasoning process, I recommend CCH Canadian Ltd. v. Law Society of Upper Canada, [2004]: http://www.canlii.org/ca/cas/scc/2004/2004scc13.html

    Canada has the most explicit and liberal statutory exception to copyright infringement for library research of any country I know of. The library's custom photocopying service didn't push its luck: according to the decision, "The Access Policy states that the Great Library will typically honour requests for a copy of one case, one article or one statutory reference." The library provides self-service photocopying facilities only subject to a disclaimer of responsibility for the legality of the users' actions.

    Yet this is a Supreme Court of Canada decision, which means that it wasn't settled by any lesser court to the satisfaction of its appellate reviewers. And it stops far short of a blanket authorization of the making of personal copies or the encouragement of such activities on the part of others.

    I live in the US, which has less liberal laws, a less liberal judiciary, and a system of public investment largely based on the fantasy that "intellectual property" is traded in capital markets rather than labor and services The trend towards judicial intolerance of contributory infringement is writ large in the Napster wars. I do not recommend to others that they photocopy copyright material without the publisher's permissions and would not advise librarians and authors to do so either.

    Cheers,
    – Michael

31. **Michael Edwards**
    August 11, 2006

    Gresham's Law at work:

    http://www.msp.warwick.ac.uk/gt/gtp-subscription.html

32. **Chris Grant**
    August 11, 2006

    Many U.S. academic researchers have access to Lexis through their school libraries. In a few minutes you can look up numerous law review articles on Fair Use as it pertains to photocopying of an article from a journal by an academic researcher. You won't find a single one among that takes a position anywhere near Michael Edwards' uebercautiousness.

33. **Peter Woit**
    August 11, 2006

    Michael and others,

    Please do try to stick to the topic. Fair use is really a completely different issue, and it should be discussed not here, but on blogs run by people who know something about this.

34. **Georg**
    August 11, 2006

    No idea about the situation in the U.S., but in many countries (I know Germany, Canada and the UK, but I'm sure it can't be too different elsewhere) there is a contract between some agency set up by publishers' and writers' unions on the one hand, and libraries and/or photocopier manufacturers on the other, which specifically licenses the copying of (only) individual articles or chapters from library books and journals for non-commercial purposes. The libraries and/or photocopier manufacturers pay a flat fee, and the library users are on legally safe ground, as long as they obey the restrictions of the license, which is usually posted next to each photocopier.

35. **Michael Edwards**
    August 11, 2006

    Peter, I couldn't agree with you more.

Former string theorist, how do you keep yourself occupied mathematically now? Can you tell us about some contemporary mathematical physics that you consider promising?

36. **woit**
August 11, 2006

Uh, Michael, you seem to be missing my point about not using this forum to start up discussions unrelated to the postings…

37. **Michael Edwards**
August 11, 2006

Sorry, Peter. Wasn't clear to me that alternatives to string theory weren't on topic. I'll shut up now.

38. **John Baez**
August 12, 2006

Jean-Paul writes:

I am not so sure about bright prospects of online publishing. There must be good reasons why paper survived several millennia.

Yes: there weren't computers, and scribes kept recopying papyri as the old ones decayed. The oldest known bit of Euclid's Elements is a [page from an ancient garbage dump in the Egyptian town of Oxyrhynchus](). The oldest surviving *complete* copy dates back to only 888 AD. So, the business of needing to work to keep data available is not new; it's just speeding up.

39. **Juan R.**
August 12, 2006

1) The situation is science appears to me poor than in math.

2) Offer and demand, laws of market! Nobody really obligates to editors of the journals to subscribe publication of the journal with a commercial publisher, or not? If tomorrow we become editor of a journal we am not obligated to choose Elsevier as publisher. Would we? Still more surprising would be after publishing our journal via Elsevier we blame it by its prices.

3) About free.

Some journals are claimed free over (expensive economic publishers) but those free journals are being really supported via donations, societies fees, author or institutional charges, and others. SPARC requires fees to membership…

E.g. Baez [says]():

"Unsurprisingly, the response from publishers was chilly. As a result, the Public Library of Science is starting its own [free journals]() in biology and medicine, with the help of a 9 million dollar grant from the Gordon and Betty Moore Foundation."

The Plos journals are free to readers but are not to authors. Authors (or their institutions) are required to pay a fee for publication. For instance until $2500 for publishing on PloS biology. Some time ago, it was shown that model was not suitable for high-level journals even if many academicians claimed the contrary and signed a well-known letter. If I remember correctly some computations done suggested us that a similar model applied to Science journal would require of the order of $50,000 per article to authors. Moreover, a bit of common-sense and a bit of economic study suggested that PloS was not economically suitable. PloS people (i.e. academicians) blammed against publishers but reality was there and "academicians" were wrong in their economic analysis. PloS is [economicaly unsustainable]() except, maybe, as experiment. The increase of PloS fees has been of a 66% in only two years.

4) Baez suggestions.

1. Don't publish in overpriced journals.

Except often publishing in overpriced journals offer advantages in terms of C.V. publicity and career promotion.

2. Don't do free work for overpriced journals (like refereeing and editing).

And why do free work in other cases?

3. Put your articles on the arXiv before publishing them.

Except if publishing guidelines of journal you target impede this or if your field of interest is not supported by arxiv and others…

4. Only publish in journals that let you keep your articles on the arXiv.

I.e. in some cases you are claiming "do not publish". Arxiv-like model does not work for chemistry for instance; look fiasco of CPS.

5. Support free journals by publishing in them, refereeing for them, editing them… even starting your own!

Except that free journal are an academic version of the ".com" fiasco of some time ago. Before or after economic issues arise and most of the so-called free journals will disappear in a future as most of .com did. Then the problems of non continuity of information (one of reasons for classical printed academic journals) will arise.

6. Help make sure free journals and the arXiv stay free.

Well, this is idilic. Arxiv-like model failed in chemistry and in other fields.

5) MathPhys.

I do not know how many online journals you are managed/edited, but I would aknowledge you more information on how "the administration of an online journal is minimal. A typical academic, teaching a typical calculus class for engineers, with a web page and all, has comparable admin work on his hands." Since i know nobody being able to do that you are claiming.

6) DMS said:

"I must say one extremely positive contribution of string theory has been the arxiv, which has now been embraced by the rest of physics, and to a smaller extent other fields like mathematics."

Ehh!

7) About Journal's overprices

Hum, is only Elsevier overpriced?

If I want buy a 2006 6-pages article from a first-quality journal of chemistry managed by ACS I may pay around $33.

If I want buy a 1988 18-pages article on PRD

I may pay $23 to APS.

I would pay $30 to Nature for recent a single-page Essay on strings by Witten.

Etc.

8) About printed vs online

Moshe says that online journals are cheaper. Yes, but how many are? Eliminating color (is very expensive) is printing in paper so expensive? When i worked in a chemical Bulletin here in official Galicia society for chemists, i discovered that paper printing was not so expensive as i imagined and we worked a small 1000 copies bulletin (for 10000 was cheaper still the cost per page, since there is a fixed cost for printing anything from 1 to infinite copies).

Moreover, the no so large economic advantage of online journals over printed ones will disappear with the semantic web and full online academic articles. Today "online" journals are only pdf versions of printed ones, try to offer xml mathematics, text, and graphics and database files (e.g. chemistry or cristalographic data) into your online version… and we will sew how many online journals survive in near future.

Note: several online journals guided by academicians (e.g. some cited at this blog) are clearly of very low quality from a publisher's (and author) view.

9) SteveM said:

"Elsevier are a bunch of gangsters. They also have a vast archive of important material going backs decades that is locked up and now costs a fortune to access. Total ripoff and downright criminal. This material collectively belongs to the scientific community–not them! I needed some old Physics Reports articles from the 70s and 80s and ended up going to the original authors to try and get copies. I succeeded: they were sympathetic and felt that their old articles should be easily available for free or low cost, but it was still a hassle. If Elsevier go under it will be great day."

Read my comments about APS (American Physical Society) in point 7) above. Are APS gansters also because overpricing very old papers (even from 40s)?

Moreover, would we call ganster to a Nobel laureate giving talks to very very high cost? And that about professors. If H Psy = E Psy belongs to scientific community why do profesor earn money each time they explain the Schrodinger equation to their students?

Why would a publisher offer free or low cost to old articles but professor can earn lot of money for explaining (specially in summer courses) the very old F = ma to 15-year students? Just a thought!

10) Brett said,

"Nobody reads the print versions of journals any more, and many libraries have dropped the print versions entirely."

Then I am nobody. Also whne I go to the library to search literature and I see some academicians reading print versions they may be nobody.

Juan R.

Center for CANONICAL |SCIENCE)

40. **anon**
August 12, 2006

So Book 2 of Euclid's Elements ended up on an ancient garbage dump. Many of the theorems in it are tedious and boring …

41. **ObsessiveMathsFreak**
August 12, 2006

> With this attitude you only impair your own ability to do research, that's all. Access to journals is absolutely essential to scientific work.

Not really. Trying to get a hold of such journals would be a far greater impairment to my work. I honestly cannot see what is in these journals that is so essential and cannot be found elsewhere. 95% of what is in most journals is of little use to me, and I cannot justify the expense, both in time and resources, in obtaining the remaining 5%.

Here's a fact most people would agree with. The majority of scientific articles are poorly written and unelucidating, regardless of their actual content. For the most part, I'm paying for something someone has thrown out the door to notch up another mark on their publication/citation quota, not for a succint, well composed and presented exposition of the author's work. The Bogdonov's paper was not an anomaly, it was an inevitability.

Faced with this, if I can't get my hands on a paper that may or may not be of any use to me, I'll spend my time doing something more productive. Like research.

42. **anonymous**
August 13, 2006

does anybody know if there are potential legal problems in publishing articles on NPB+arXiv or PRD+arXiv?

43. **xxx**
August 13, 2006

maybe Peter allows me to point out a slighly different problem: I would like to keep in my laptop a copy of books about Quantum Field Theory, etc. The problem is that

one can only buy printed versions of these books.
One can download versions of main books for free from the web (it is easy, indicating that they are quite diffused): these versions are illegal and inefficient (no hyperlinks, etc), but they are the only electronic versions available on the market.

So I wonder why it is impossible to buy textbooks in electronic form: because publishers are stupid, or due to some better reason?

44. **John A**
August 13, 2006

You'd think that if an entire editorial board of topologists resigns, they send the resignation written on a Moebius strip stuffed in a Klein bottle….or something

45. **Chris Oakley**
August 14, 2006

> You'd think that if an entire editorial board of topologists resigns, they send the resignation written on a Moebius strip stuffed in a Klein bottle….or something

How do you know that this "entire editorial board" is not just one person following a closed timelike line?

46. **John A**
August 15, 2006

You got me Chris – unless of course there are an infinite set of Universes….

47. **h**
August 16, 2006

"I read much praise for the arXiv, but I note also that PW has had a long-running battle with it."

This is probably just in physics. Mathematics is much less political. I'm a young mathematician, and my viewpoint is that there's no life outside the arxiv. We have a pretty good library here, but I go there only like twice a year; the digital revolution is already here and there's much more to come… Also, there are already many retrodigitalization projects, and most of them are free (numdam.org is an excellent example). Google is scanning ALL the books ever written, etc…

"There must be good reasons why paper survived several millenia"

Yeah, as John Baez said it, there was no internet for several millenia 🙂 The only reason for the existence of printed journals today is inertia (and the greediness of the publishing companies). Quality control is an issue – I agree with 'Former string theorist' that the value added in the editorial process is significant -, so journals are important, but the media is not.

"That's illegal. It might sound harmless, but it is illegal in the same way that downloading an mp3 is illegal. Why should I have to break the law to get my hands on a paper?"

You must live in a different world 🙂 Photocopying a paper IS harmless (and probably legal too, considering that there's a flat copyright fee included in the price of the copy machines, in the price of the ink, and maybe even in the price of the paper; same for the harddisks, blank cds, etc). Also, stupid law is not here to comply with… I'm breaking the law several times in basically every minute of my life. I have like 100 scanned math books on my harddisk, and it would cost my ANNUAL income to buy them (yeah there's life outside the US) so I wouldn't buy them anyway; thus I'm not causing any harm to anybody (and yes, I have some original math books too, and I bought them when I was much more poorer)

A Counterexample to the Hodge Conjecture?

August 11, 2006
Categories: Uncategorized

A paper appeared last night on the arXiv by K.H. Kim and F.W. Roush entitled Counterexample to the Hodge Conjecture. The authors claim to construct an example using K3 surfaces for which the Hodge conjecture is false. If they're right about this, this would be very shocking, and I would guess that most experts will be very skeptical about the result. Most likely someone soon will find a problem with the argument, but if not there will be a lot of excitement.

The Hodge conjecture is one of the Clay Millenium prize problems, so if this paper is right, the authors may very well be entitled to $1 million. For more about what the Hodge conjecture says, see the slides or video of a popular lecture by Dan Freed, or the official statement of the problem due to Pierre Deligne.

Update: The authors have withdrawn their claim to have disproven the Hodge conjecture, acknowledging problems with their argument beginning in section 5 of the paper.

Comments

1. **anon_hodge**
   August 11, 2006

   Nobody posted this breaking news to slashdot yet.

2. **mustang**
   August 12, 2006

   ya like slashdot care.

3. **Harry Walton**
   August 12, 2006

   Hello,

   A plea for help:

   For a long time now I've tried to view the videos on the Clay Millenium Prize website.

   I consistently get an error message from Real Player – the same thing happens with the Hodge Conjecture link you've provided here.

   I watch plenty of other video web feeds with no problem.

   I've emailed the Clay site – but no response.

   Is this a known problem or is it just a problem for me?

4. **nonblogger**
   August 12, 2006

   These two authors got a paper a few years ago in Ann. Math. where they disproved a fairly old conjecture in Dynamical Systems.
   http://www.arxiv.org/abs/math.DS/9907095

   So at least their new paper does deserve to be studied carefully.

5. **ObsessiveMathsFreak**
   August 12, 2006

   I don't see exactly why it would be so shocking. A lot of these conjectures seem to be held in far too high a regard. It seems a bit paradoxical that something someones was "unable" to prove should be named after them.

   It's like the Riemann Hypothesis. There could be some sort of fractal set of transcendental complex numbers with zeros somewhere in the (0,1) strip and we might never know. So if someone finally proves the danm thing false, why the surprise? The fact that you were unable to prove it true should be taken as a hint.

6. **D. Eppstein**
   August 12, 2006

   Harry: I am also unable to view the Freed video. RealPlayer tells me that it can't access the video data at the address given in the ram file.

7. **Johan Richter**
   August 12, 2006

   From the rules of the Clay Institute:

   "In the case of the P versus NP problem and the Navier-Stokes problem, the SAB will consider the award of the Millennium Prize for deciding the question in either direction. In the case of the other problems if a counterexample is proposed, the SAB will consider this counterexample after publication and the same two-year waiting period as for a proposed solution will apply. If, in the opinion of the SAB, the counterexample effectively resolves the problem then the SAB may recommend the award of the Prize. If the counterexample shows that the original problem survives after reformulation or elimination of some special case, then the SAB may recommend that a small prize be awarded to the author. The money for this prize will not be taken from the Millennium Prize Problem fund, but from other CMI funds. "

   Would the present paper effectively resolve the problem if correct?

8. **Davis**
   August 12, 2006

> If the counterexample shows that the original problem survives after reformulation…

The counterexample comes from taking certain products of varieties; as such, I wouldn't be surprised if this allowed for some sort of reformulation excluding such examples. That's assuming their argument is correct, of course.

9. **Thomas Mulligan**
   August 13, 2006

   If their proof is sound, it's a good year for mathematics: first Poincare, now Hodge. I agree with "ObsessiveMathsFreak" about the undue confidence people put in their assumed truth of these conjectures; history has demonstrated (Hilbert's Fourteenth, Euler's conjecture, etc.) that assumed elegance should not be used as warrant for believing a conjecture to be true. Furthermore, since the Hodge conjecture is so deeply buried in abstract, complicated, specialized mathematics, it's hard to believe our intuitions would be of any utility at all. . . .

10. **Scott Aaronson**
    August 13, 2006

    So if someone finally proves the danm thing false, why the surprise? The fact that you were unable to prove it true should be taken as a hint.

    Well, there's an empirical argument: in all the areas I'm familiar with, there have been far more conjectures that stayed open for years and were finally proved, than conjectures that stayed open for years and were finally disproved. Of course the latter often get a disproportionate amount of attention.

    Incidentally, does anyone know if there's a reasonably elementary statement that's equivalent to the Hodge conjecture? I read the links from this post and a few other articles, and I still don't understand what's being asked.

11. **jb**
    August 14, 2006

    I know many experts were not at all convinced the HC had to be true, unlike with, say, the Riemann Hypothesis.

    As for equivalent elementary statements, I'm not aware of any.

12. **Kea**
    August 14, 2006

    Peter

    How many Millenium problems are there left?

13. **Yatima**
    August 14, 2006

    I would guess 7:

    http://www.claymath.org/millennium/

    Remember…remember… how many positive and negative proofs there have been for P = NP ?

    (Peter – Great Book by the way; unfortunately my mathematical ability has suffered somewhat by years in ICT)

14. **Walt**
    August 14, 2006

    Kea: 5, assuming this holds up.

    Yatima: I assume you mean 6 (it seems pretty likely now that Poincare is settled).

    Scott: I don't think there is a more elementary formulation. Algebraic geometry has been around long enough that it's become incredibly technical.

    ObsessiveMathsFreak: The Hodge conjecture is unusual in that there is so little supporting evidence. The other conjectures, such as the Riemann hypothesis, have more supporting evidence. For example, it's numerically easy to compute zeroes of the zeta function, and the 2 billion that have been found all lie along the critical line.

15. **Andrew**
    August 14, 2006

    Actually, their paper doesn't look correct. They're talking about something well-known called the Kuga-Satake-Deligne correspondence, which is implied by the Hodge conjecture. The entire paper is poorly written, doesn't seem to even try to prove the theorems they state, it looks like they completely misinterpreted the actual statement of the correspondence. All of the correct parts of the paper are lifted almost verbatim from van Geemen's paper "Kuga-Satake Varieties and the Hodge Conjecture." One of them is a computer scientist, so it looks like some amateurs are just trying to have a good time proving some famous conjectures.

16. **werdna**
    August 14, 2006

    Who is the computer scientist, and how do you know that? Theoretical computer science can be highly mathematical, and some CS people are excellent mathematicians. In fact, the P=NP problem came from CS.

17. **not a Hodge Conjecture expert**
    August 14, 2006

    Scott,

    The Hodge Conjecture is widely considered to be the most difficult millennium problem to explain to the general public, but a watered down (probably inaccurate, definitely less general) elementary statement, understandable to most mathematicians and fancy theoretical physicists, might go something like this:

    On a complex algebraic variety, every homology class that could reasonably contain a subvariety does contain a subvariety.

The “could reasonably contain” part mostly refers to an obvious obstruction: The homology class of a complex subvariety must be Poincare dual to a differential form of type (p,p).

I’m not sure why the Hodge Conjecture is rarely dumbed down in this way. I think it’s because algebraic geometers don’t realize that no one else understands algebraic geometry.

18. **Davis**
August 14, 2006

werdna:

> some CS people are excellent mathematicians…

True, but very few CS people are good *algebraic geometers*. Algebraic geometry is a notoriously difficult field to jump into, and has only a handful of intersections with CS stuff (that I’m aware of).

not a Hodge Conjecture expert:

> I think it’s because algebraic geometers don’t realize that no one else understands algebraic geometry.

I think algebraic geometers just sort of gave up on developing explanations accessible to folks outside the field (which is unfortunate, in my opinion). These days it can be challenging to explain cutting-edge AG to other algebraic geometers, nevermind non-AGers.

19. **Scott Aaronson**
August 14, 2006

On a complex algebraic variety, every homology class that could reasonably contain a subvariety does contain a subvariety.

So, to translate into my doofus computer-scientist terms: We have a solution set, S, of some system of polynomial equations over the complex numbers. We’re interested in what sorts of subsets T of S can also arise as the solution set of a system of polynomial equations. In particular, what are the possible ways that T can embed into S topologically? How can T wrap around the holes of S, and so on? The Hodge conjecture basically says that T can embed into S in “every way that it reasonably could.”

Is that completely off-base? (I don’t doubt that the conjecture is more general, but I’d be happy to understand *any* nontrivial special case…)

20. **Walt**
August 14, 2006

It’s sort of dual to what you describe. Let’s say S has a hole. The Hodge conjecture answers the question, when is there a T that wraps around that hole?

21. **Lolo**
August 15, 2006

That’s strange to me what non-algebraic geometers think about algebraic geometry : Hodge conjecture is from far one of the easiest conjecture in algebraic geometry to state (and, apart from its deepness, I think this is one of the reasons why it was chosed for the millenium problems, so that non-algebraic geometers can understand its statement). It is very “concrete” compared to other conjectures in algebraic geometry and can be understood by any graduate strudent in algebraic geometry begining his PHD.

By “concrete” I mean : it deals with smooth projective algebraic varieties over C and their cohomology. Thus you can think of them as smooth complex manifolds and for the cohomology you can think of it as singular/De Rham cohomology. There’s no étale/cristalline/syntomic/motivic cohomology in this statement and it does not need the theory of schemes or algebraic stacks or stuff like that to be stated since you deal with smooth projective varieties over C, you can use your differential geometry intuition.

22. **Walt**
August 15, 2006

Lolo: What you say is false, unless you mean by “starting his Ph.D.” someone who’s already had two years of graduate work, including lots of differential geometry. I actually find schemes easier to understand than complex analytic varieties, but that’s probably just me.

23. **not a Hodge Conjecture expert**
August 15, 2006

Lolo, with apologies, I take back my unnecessarily snarky remark about algebraic geometers. According to the official Clay statement, the Hodge Conjecture states:

“On a projective non-singular algebraic variety over C, any Hodge class is a rational linear combination of classes cl(Z) of algebraic cycles,”

where a Hodge class is a rational homology class of type (p,p), and an algebraic cycle is just an algebraic subvariety.

This description is actually quite simple and really not that fancy. So I will make a different snarky remark: Algebraic geometers have intimidated the rest of us so much that we immediately assume that we can’t possibly know what they’re talking about, even when we can. 😉

More seriously, I guess the problem is that a lot of people know what (projective) varieties (over C) are, and a lot of people know what homology is, but many of these people don’t know enough complex geometry to understand the (p,p) part, I guess?

24. **Lolo**
August 16, 2006

I agree on your last remark : the difficulty is to define what means (p,p) in the conjecture.

But I agree with you on your remark before : for me it’s easier now to understand scheme theory….but it took me let’s say something like 4 years before I can say that; to understand the definition of a scheme and basic properties takes you a few months, but to be able to work with and manipulate them as if you had ever been living with took me a long time, which was not the case for differential manifolds where just after you’ve seen the definition your intuition is avaible (just because you’ve passed the preceding 4 years to work on differential calculus on open subsets of R and after R^n as an undergraduate, if you had done the same work
in commutative algebra and functors theory then scheme theory would be immediatly accessible to your intuition).

25. **Algebraic Geometry Joe**

August 16, 2006

I've also taken a look at this paper, and I can confirm that it is garbage. It looks like the Hodge conjecture still stands. The paper is full of mistakes and the authors don't really know what they're writing about. It's a shame that this made it onto the arxiv.

26. **Danny**
August 17, 2006

I agree that it looks like the authors don't know what they're talking about, but nothing in the paper is really new except the computer calculation and a statement about incompatible cup product structures. Otherwise it's a poorly written survey of other people's work. It's well known that the Hodge conjecture implies the Kuga-Satake-Deligne correspondence, and the relationship between this and the clifford algebra stuff in the paper. I don't think that the authors don't know much Hodge theory, but maybe these computer scientists just found something numerical implied by the Hodge conjecture and wrote a program to find a counterexample.

27. **Bromskloss**
August 17, 2006

Yep, I too have problems with the video. I use Media Player Classic.

28. **Speculator**
August 17, 2006

"Algebraic Geometry Joe" and "Danny":

Could you please indicate what did you found wrong in the preprint?

Since you state that "it is full of mistakes" this shouldn't be difficult.

For your information Kim and Roush are not "computer scientists". They did disprove a few years ago Williams conjecture in Symbolic Dynamics, the main outstanding problem in the field (published in Annals of Math in 1999). Their publication list exhibits a long track of "problem solving".

Maybe it is just that Algebraic Geometers are not that good at finding counterexamples?

29. **Algebraic Geometry Joe**
August 17, 2006

I should have been more precise in my critisism. Certainly, it is possible that the Hodge conjecture has been disproved. However, the paper is badly written – there are many examples of this. Finding problems in the logic of the paper is very difficult when it makes little sense except to those who wrote it.

Here's one possible problem, though: Proposition 5.1 assumes the existance of a small deformation of a k3 surface with a transcendental lattice of constant rank. I was under the impression that this is impossible. For example math.AG/0011258.

Having said this, whoever is clever enough to disprove the Hodge conjecture is also clever enough to write a paper that I find confusing. It'll be interesting to see what consensus appears.

30. **Speculator**
August 17, 2006

This sounds different from "…it is garabage…full of mistakes…".

Don't worry, there are such deformations.…just take the constant one…

Notice also that A=>B is still true if A is an empty condition…

31. **Clark**
August 18, 2006

I have to agree with "Algebraic Geometry Joe." It's at best very poorly written. Section 5 in particular (on which everything depends) doesn't make much sense to me.

But perhaps a more irritating problem with the paper is that in the proofs of the main results, the authors refer to propositions not contained within the paper, e.g., to "7.1," despite the fact that their paper only has 6 sections. There are at least 3 examples of this.

Certainly a more legible account would be helpful…

32. **Davis**
August 18, 2006

> Certainly a more legible account would be helpful…

Agreed. I tried to read it last night, and it's not clearly written at all. And the authors really need to learn to use LaTeX's theorem and proof environments.

33. **DA**
August 20, 2006

Hi Peter, You might remember me, we were Postdocs together
on West Coast years ago, I was the only algebraic geometer at the
time.…

I just thought I'd jump in to this discussion with a few comments. It would accurate to say Hodge suggested (rather than conjectured) various things in his 1950 ICM talk. Some of these thing are known to be false (e.g. a counter example to the Hodge conjecture for integer coefficients was found early on by Atiyah-Hirzebruch, later Grothendieck found a counterexample to the general Hodge conjecture in its original formulation).

So I wouldn't be altogether suprised if the Hodge conjecture in its present form were to fail also. But, unfortunately, this preprint seems pretty unclear in various places. For example, as people have pointed out here, prop 5.1 looks suspect. The statement itself is ambiguous, and the proof seems bogus. I don't want to dismiss this paper outright (they may be on to something), but this kind of thing doesn't inspire a lot of confidence.

34. **woit**
    August 20, 2006

    Hi DA,

    Of course I remember you, thanks a lot for the comment.

    I've also heard privately from other experts the same evaluation: prop. 5.1 may or may not be true, but the proof given doesn't work.

35. **hack**
    August 24, 2006

    What? They withdrew their paper just because one of the arguments is wrong? Culture shock.

String Phenomenology and the Landscape

August 11, 2006
Categories: Uncategorized

Science magazine this week has an article about the anthropic string theory landscape controversy, entitled A 'Landscape" Too Far, by Tom Siegfried. The only theorist quoted as opposing anthropic landscape arguments as not science is David Gross, although experimentalist Burton Richter's talk at SUSY 2006, and letter to the Times ("I can't understand why they don't take up something else — macrame, for example") are also quoted. Gross says that anthropic explanations are not science but "fun parlor games", that "they're not science in the usual sense of making predictions that can be tested to better and better precision over the years."

Quoted as strongly in favor of the anthropic landscape are Susskind, Linde and Polchinski (there's an extensive side article about Polchinski's conversion experience to the anthropic ideology). Sean Carroll and Frank Wilczek promote the idea of the multiverse as a new Copernican revolution, and Clifford Johnson defends anthropic landscape studies with:

It would be nice if we could explore some of those unpalatable ideas just in case that's the way nature chooses to go.

Clifford has a posting about this on his blog, where he has more to say about this. He seems to have decided to deal with the very uncomfortable position that the evidence and rules of logic put string theorists in by advocating ignoring logic, quoting Moshe Roszali approvingly about the desirability of being able to hold contradictory viewpoints simultaneously.

The Science article does get a very little bit into the crucial question that determines whether landscape studies are science or not: is there experimental evidence that can test the hypothesis? Andrei Linde objects to people who say this subject is not science with:

It's not an easy job to do, so if you don't want to do it, then don't do it. But don't say it's not science.

It's true that the anthropic landscape is incredibly complicated and difficult to do anything with, but I don't see how that fact is any kind of argument in favor of it being a science. Linde does claim that gravitational waves can be use to "verify anthropic predictions about the nature of spacetime curvature." I don't know exactly what that's about, presumably something to do with possible effects in the CMB due to our universe being born out of a bubble nucleation. If anyone knows of any precise "anthropic prediction" of this kind, I'd be interested to hear it. But, in any case, whether or not you can by observation see whether the universe arose in this way, I don't think Linde answers at all the objection that the string theory anthropic landscape is inherently unpredictive and thus not legitimate science.

The Science article also includes a heavily overhyped statement about the experimental support for inflation, describing the WMAP results as having "provided strong support for inflation's predictions."

For a much more serious discussion of whether the string theory landscape, anthropic or not, is inherently unpredictive, you can watch the video of a talk given yesterday at the KITP by Wati Taylor on String Vacua and the Quest for Predictions. This was the inaugural talk for the semester-long program on string theory phenomenology that will be taking place in Santa Barbara. The blurb for the program is a masterpiece of hype, telling us that string theory has "the potential to predict properties of superpartners that might be found at the Tevatron or LHC and provide new experimental tests and probes of the theory", something that I don't think any serious person actually believes these days.

Taylor's talk was quite remarkable, very explicitly going over exactly how bad the current situation is for efforts to get any prediction at all out of string theory. There was a lot of discussion with the audience, and much nervous laughter. Unfortunately I found some of Gross's comments hard to hear. Taylor explained that after spending ten years himself working on trying to better understand what string theory is (he worked in string field theory), he doesn't see any realistic prospects for significant progress on this problem during the next ten years. He listed the basic problems as the lack of a non-perturbative definition in anything but special, non-physical backgrounds, the inability to do even perturbative calculations in the kind of Ramond-Ramond backgrounds that people are using to stabilize moduli, and the lack of any definition of string theory when supersymmetry is broken by a positive CC, and thus the background is deSitter.

Discussing the landscape, he said that there was no evidence for a dynamical principle that would select the vacuum, with no hint at all of how such a thing would work, and that there is no known mechanism that would destabilize the known conjectured constructions of vacua. He goes on to ask "what can we do even if we don't know what we're talking about?"

He introduced his own current philosophy, which is that unless some dramatic new breakthrough comes along in string theory (which he didn't seem optimistic about), the only idea for getting a prediction out of string theory that is still conceivable is to look for strong correlations among standard model parameters in the landscape. He didn't even bother to mention the fashionable idea of a couple years ago that one could make predictions using statistics of vacua, that idea seems to be completely dead. He noted that as time goes on, people keep finding more and more constructions of vacua, and it now seems clear that there are so many of these that one can't use their hoped-for discrete nature to make predictions.

According to Taylor, the only possible hope for getting a prediction out of string theory is if one can show that, for all string vacua, there is some strong correlation between values of the low energy field theory parameters. If it turns out that (for example), for all string vacua the number of generations is always 3 when there is an SU(3) factor in the gauge group, then knowing about SU(3) predicts the number of generations. There's no known reason why anything like this should be true, and it sounds like pure wishful thinking to me, but I guess Taylor's point of view is that string theorists should be working harder on understanding the details of the landscape in the hope of finding such a thing, because it is the only hope for getting a prediction out of the theory, and thus justifying it as a science.

Taylor acknowledges that the state of affairs is that one can't do at all realistic calculations along these lines, but he has been doing some unrealistic ones with Michael Douglas. They've been looking for correlations between the size of the gauge group and the number of chiral generations in intersecting brane models. These are quite unrealistic, with no supersymmetry breaking and unstabilized moduli. In any case, their result is negative: even in this simplified, unrealistic context, they find no sizable correlations.

Given this start, it will be interesting to see how the participants manage to get through the semester without getting so depressed about prospects for string theory that they abandon it and go on to something else. One new feature of the program is that a wiki has been set up to allow for communication and discussion between the participants.

Comments

1. **anon**
   August 11, 2006

   Peter, you are a moron with the intellect of an amoeba. The Anthropic Principle does make some sharp predictions.

   Okay, I admit, the most general statement is a tad of an exaggeration:
   "Why should we believe the universe is anthropic? Because if it weren't anthropic, we would not exist".

   But consider a slightly less general version:

“Why do String Theorist believe the universe is anthropic? Because if it weren’t anthropic, they would not exist.”

2. **Kea**
August 11, 2006

…and the lack of any definition of string theory when supersymmetry is broken by a positive CC, and thus the background is deSitter…

de Sitter…which is, er, cough, in agreement with Riofrio’s analysis of, for instance, the type IA supernovae data, and also with a simple interpretation of certain interesting background independent QG models for which we understand the derivation of classical gravity.

3. **Kea**
August 11, 2006

How many nails need to go into this freaking coffin?

4. **MathPhys**
August 11, 2006

Look on the bright side, Peter. Statistical analysis of the Landscape prepares PhD students for a career on Wall St.

5. **Bob McNees**
August 11, 2006

> de Sitter…which is, er, cough, in agreement with Riofrio’s analysis of, for instance, the type IA supernovae data, and also with a simple interpretation of certain interesting background independent QG models for which we understand the derivation of classical gravity.

Can you please provide a reference to a “background independent QG model” for which

A) we understand the derivation of classical gravity, by which I assume you mean the existence of a semiclassical limit

and

B) which leads to the appearance of de Sitter space?

I honestly don’t know what’s being referred to here, which is why I’m asking.

6. **Kea**
August 11, 2006

This paper is a good gateway into the literature on BF theory and its variants, in the context of de Sitter gravity.

7. **Anon**
August 11, 2006

I think Kea is talking about the Kodama State.

You can read all about why that’s nonsense in http://arxiv.org/abs/gr-qc/0306083 .

But perhaps she is talking about something else. Kea?

8. **Rickkkkkkk**
August 11, 2006

Man oh man, I’m so lucky to have spent almost no time at all to be this ignorant, if I had gone to school for 10 years or spent my career just to know the same amount of nothing at all, I’d be in even worse shape

9. **Lee Smolin**
August 12, 2006

Unfortunately it appers there is still misinformation circulating about the Kodama state in the Ashtekar formulation, which, not for the first time, has to be answered here. This is a subtle issue, still unresolved, which people who have not studied the actual literature often get wrong because their reasoning is based on incorrect analogies with Yang-Mills theory. There are also interesting recent developments.

-First, there can be no objection to its use as a semiclassical state as it IS the WKB state for (A)dS, as described in hep-th/0209079 and papers referenced there. This is the first way in which the situation in Yang-Mills and gravity are not analogous.

-There is an open issue of whether the Kodama state in the Ashtekar representation is normalizable as an exact wavefunction in the physical inner product. (One reason is that it is only an exacxt solution in one ordering of the Hamiltonian constraint, whicih may not be the physically correct ordering. If so, there are corrections which have not been so far computed to the exact wavefunctional. Witten’s paper is related to this, but does not resolve it, as it concerns a different theory-Yang-Mills. Witten’s objection does not apply directly to the Ashtekar formulation for reasons discussed in hep-th/030114 and it also does not apply to its use in expansions around BF theory I mention below developed in hep-th/0501191.

-The issue is resolved in cosmological reductions in which the vacuum energy is related to a physical degree of freedom so one can make a wavepacket in the cosmological constant. This resolves the problem in that context, as shown with Alexander and Malecki in hep-th/0309045.

-The question can be investigated in the linearized case and we did so with Freidel in hep-th/0310224. The Euclidean linearized Kodama state is delta functional normalizable. The Lorentzian one has unstable modes. These may be unphysical or they may just reflect an instability of deSitter spacetime.

-The Kodama state also appears in a different, recently introduced approach, in which GR is constructed by an expanding around an SO(5) BF theory, in hep-th/0501191 .That BF theory has a single bulk solution classically, which is (A)dS and a single bulk quantum state, which is related to Kodama. This may resolve the old issues about the Kodama state because it is a sensible state of BF (Indeed it’s the only bulk state of BF theory, work on this is in progress.

-There is also a recent proposal by Randono that extends the Kodama state to a space of physical states, which he argues are normalizable. See gr-qc/0504010 and papers in preparation by him about this.

Thanks,

Lee

10. **Kea**
August 12, 2006

I am not talking about *the* Kodama state. Note the emphasis.

11. **Kea**
August 12, 2006

Sorry, Lee, I just saw your post.

12. **Sean Carroll**
August 12, 2006

Really? I'm promoting the idea of the multiverse as a new Copernican revolution? That's news to me (although I don't have a subscription, so I can't read the article). Of course, I have been known to say that we have no reason to believe that the universe we don't see looks just like the universe we do see, out the edges of infinity — but that's pretty trivially true, I hope nobody would disagree with it. If that's a "Copernican Revolution," then so be it.

13. **Chris Oakley**
August 12, 2006

> Look on the bright side, Peter. Statistical analysis of the Landscape prepares PhD students for a career on Wall St.

Not really. Wall Street interviewers will in general find an applicant with a Ph.D. in a highly speculative subject less appealing than one with a Ph.D. in a hard science or mathematics.

14. **MathPhys**
August 12, 2006

But future stringy PhD's will know a lot more about data analysis, inference, sampling theory, etc, than PhD's who worked on, let's say, lattice gauge theory.

15. **woit**
August 12, 2006

Hi Sean, here's part of the section quoting you and Wilczek:

But, he says, the idea that the known universe is only a small part of something much bigger should not come as so much of a schock. "Again and again in the history of cosmology, we've been shown that the little pieces we've been looking at are not the whole story," Carroll says. At the time of the Copernican revolution, the supposed whole universe was just the solar system. But the sun eventually was revealed to be just one star in a vast galaxy, and in the 20th century, that galaxy became just one speck in space among billions and billions of others.

As Wilczek observes, the string landscape and the multiverse merely suggest that the same story is happening again. "This is going one step further," he says. "We should be used to it by now."

16. **Juan R.**
August 12, 2006

Sean Carroll said:

"Of course, I have been known to say that we have no reason to believe that the universe we don't see looks just like the universe we do see, out the edges of infinity — but that's pretty trivially true, I hope nobody would disagree with it. If that's a "Copernican Revolution," then so be it."

Well, may be "pretty trivially true" also that the universe we do not see belong to metaphysics.

Even there exist doubts that the study of the part of universe we can see (so-called observable universe) can be considered Science since not all of scientific method applies therein; e.g. crucial distinction between observation and experimentation.

String theorists, certain loop theorists and rest of fans would be valiant by recognizing that Landscape and those "novel" ideas (idea of a multiverse is not characteristics of 20th century, of course) and "revolutions" are just pure nonsense (of interest for philosophers and film makers of course) from a purely scientific point of view.

In the case of string theory, the anthropic attitude is just the proof that string theory is a fiasco as scientific discipline (how was pointed by several Nobel laureates: e.g. P.W. Anderson)

Juan R.

Center for CANONICAL |SCIENCE)

17. **Sean Carroll**
August 12, 2006

Okay, I'll happily stand by that quoted statement. I don't really think it's controversial.

18. **Matthew**
August 12, 2006

But future stringy PhD's will know a lot more about data analysis, inference, sampling theory, etc, than PhD's who worked on, let's say, lattice gauge theory.

Lattice gauge theory teaches you lots of those sorts of skills. Given that the primary tool is monte-carlo simulation. It's pretty good prep for "Wall street"

19. **ObsessiveMathsFreak**
August 12, 2006

> Okay, I'll happily stand by that quoted statement. I don't really think it's controversial.

You might not think so, but the reality is, you guys are starting to sound like the guy from timecube.com. This isn't ad hominim. You really are.

When Copernicus proposed the heliocentric model he did so only after years and years of careful observation and data aquisition. Kepler law's proposed his laws only after years of analysis of Brahe's data, which itself took decades to gather. The Cophenhagen interpretation was not simply thrown together from mathematical equations. It too was only reached after decades of experimental data that supported it. And finally the standard model was only proposed and accepted after countless meticulous and detailed experiments gathered vast amounts of cold hard data.

In light of this, when people deriving results from mathematical equations, using pertubation theory I might add, start declaring that a multiverse landscape exists and start making anthropic arguments about "why we are here", and all with no data of any kind whatsoever, it means you sound like the guy at timecube.com.

Science **is** the experiment. If you haven't got an experiment, you've just got a few mathematical symbols on paper. I'm a mathematician, but I know that without experimental confirmation, my equations and relations, no matter how beautiful, are just that. Equations and relations of mathematical constructs. And in light of this, without experiment, I would not be inclinded to go around making predictions about multiverses, landscapes or timecubes.

Get some data, then get back to us.

20. **D R Lunsford**
August 12, 2006

I can't help but see parallels with this nonsense and the twin monsters of postmodernism and neoconservatism. One should read John Dean's book "Conservatives without Conscience" – the arguments can be taken over directly and applied to "leading" (or should I say "fuehring") academics and their relation to science, rather than politics. The key point is the utter compulsion, among a large number of people, to follow – to be a follower of somebody, *anybody* who can assuage their fear. I consider string theory, the multiverse, etc. to be profoundly conservative ideas in a bizarre sense – because they seem to hearken back to an innocent time when Aristotle and the Pope decided how the world really worked. (Comparing the multiverse to the Copernican worldview is not only utter nonsense, it is just plain contemptible.)

-drl

21. **Who**
August 12, 2006

"Again and again in the history of cosmology, we've been shown that the little pieces we've been looking at are not the whole story," Carroll says.
…
[there might be dragons in piece next to ours.}

As Wilczek observes, the string landscape and the multiverse merely suggest that the same story is happening again. "This is going one step further," he says. "We should be used to it by now."

[sea serpents also are a possibility]

22. **nigel cook**
August 12, 2006

"The Copenhagen interpretation was not simply thrown together from mathematical equations. It too was only reached after decades of experimental data that supported it. And finally the standard model was only proposed and accepted after countless meticulous and detailed experiments gathered vast amounts of cold hard data." – Obsessive Maths Freak

But the Copenhagen interpretation is an ad hoc philosophy not a mathematical prediction technique, and it doesn't make unique predictions that have been tested, so you can't lump it with the Standard Model that does make predictions, has been tested.

There is an industry within physics run by full time science fiction writers who do mathematical philosophy of physics part time, and after about 1916 that was what Bohr did. OK, he did some useful applied nuclear physics theory such as determining u235 is the fssioning nuclide in natural uranium, but he just sprouted content-less, ad hoc, abjectly speculative philosophy when writing about the nature of reality and the future of theoretical physics. He claimed the Copenhagen Interpretation in 1927 solved everything completely for all time by separating and so outlawing any progress understanding of how classical and quantum electrodynamics can be reconciled:

'… the view of the status of quantum mechanics which Bohr and Heisenberg defended – was, quite simply, that quantum mechanics was the last, the final, the never-to-be-surpassed revolution in physics … physics has reached the end of the road.' – Sir Karl Popper, Quantum Theory and the Schism in Physics, Rowman and Littlefield, NJ, 1982, p6.

'… the Heisenberg formulae can be most naturally interpreted as statistical scatter relations [between virtual particles in the quantum foam vacuum and real electrons, etc.], as I proposed [in the 1934 book 'The Logic of Scientific Discovery']. … There is, therefore, no reason whatever to accept either Heisenberg's or Bohr's subjectivist interpretation …' – Sir Karl R. Popper, Objective Knowledge, Oxford University Press, 1979, p. 303.

23. **andy**
August 12, 2006

When I was at Oxford Rudi Peierls told me [words perhaps slightly disremembered] "we theorists need experimental results. Otherwise we're only speculating."

There was more, over coffee and chocolate cake at Brown's, but that's the gist.

Pity that this lesson has been forgotten.

A.

24. **Chris W.**
August 12, 2006

Andy,

Indeed. Unlike Rudolph Peierls, some prominent (and not so prominent) members of the last two generations of theorists don't really give a shit. Knowing theory, and producing more theory for their students to master, is all they seem to care about. It's kind of like a defense contractor who produces some massive system that doesn't work, and then gets the government to write yet another contract to fix it, and so on…

Analogies with out-of-control software development projects seem most apposite.

There is one kind of theoretical activity that is absolutely essential, with or without experimental results, and that is careful and penetrating examination of *problem formulations*. To the extent that's happening at all these days, it's happening at the margins, and is being mostly ignored as mere "philosophy".

(Pardon the bile in these remarks. I'm a bit cranky right now.)

25. **Who**
August 12, 2006

(Pardon the bile in these remarks. I'm a bit cranky right now.)
You didn't sound cranky or bilious, Chris: the preceding post sounded reasonable to me—-comparisons seemed apt, fair and so on.

we wouldn't necessarily agree on other issues but what you say here strikes me as uncontroversial. "out of control software development project" says it well.

26. **Kea**
August 14, 2006

Speaking of nails, there is also the calculation of lepton masses, as discussed on this thread.

27. **Chris Oakley**
August 14, 2006

Another article based on a bad String Theory pun:

http://www.time.com/time/magazine/article/0,9171,1226142,00.html

28. **nigel cook**
August 14, 2006

Chris, that Time mag article concludes with Sean Carroll stating:

"It's true that nobody has any good idea of how to test string theory, but who's to say someone won't wake up tomorrow morning and think of one? The reason so many people keep working on it is that, whatever its flaws, the theory is still more promising than any other approach we have."

Similarly, if you flog a dead horse enough, it may decide to spring to life. I can't believe the amount of shit tha comes from theories held together by imaginary branes and bits of string. Call that a theory?

29. **Chris Oakley**
August 14, 2006

Statement about dead theory generator in Visual Basic:

Function StatementAboutDeadTheory(ByVal theory As String) As String

StatementAboutDeadTheory = "It's true that nobody has any good idea of how to test " & theory & ", but who's to say someone won't wake up tomorrow morning and think of one? The reason so many people keep working on it is that, whatever its flaws, the theory is still more promising than any other approach we have."

End Function

30. **Chris Oakley**
August 14, 2006

… and here is the same thing in APL

╛≥─╜╝♣├╞╟♥╟╡Π┴╢╣Σ╤▒◊♪─╥▓○

31. **D R Lunsford**
August 14, 2006

CO – I just spit coffee all over my machine, damn you you Englishman! Come back and I shall taunt you a second time! (Did someone say "a second time"?)

Funniest comment ever on NEW.

-drl

32. **L. Riofrio**
August 15, 2006

Kea and Chris, right on. Einstein said that the definition of insanity is trying something repeatedly and expecting a different result. We are seeing growing acceptance of approaches (LQG, Kodama states, and my little Type Ia contribution) with testable Results to satisfy MathFreak. The Copernican revolution was preceded by cosmologies held together by epicycles and imaginary energies. This is a very exciting time to be in science.

33. **Chris Oakley**
August 15, 2006

Hi Danny,

I do my best. Resorting to cheap humour is, as you will have seen, my usual strategy when I have nothing useful to say on the topic. The APL gibberish, though, comes from the heart having had to decode the incomprehensible scripts that the New York office used to write for valuing exotic interest-rate derivatives when I worked for UBS.

34. **John Rennie**
August 16, 2006

You seem very negative about Wati Taylor's talk, but it seems to me to be a valid program to pursue. If no correlations can be found then this is worth knowing, and worth the effort.

35. **Rickkkk**
August 17, 2006

Quote "It's true that nobody has any good idea of how to test string theory, but who's to say someone won't wake up tomorrow morning and think of one? The reason so many people keep working on it is that, whatever its flaws, the theory is still more promising than any other approach we have."

I'm so sick of seeing this type of logic that I believe anti-nausea medication will soon be in order. Sean happens to be the one saying it here, but my anger isn't directed toward him. It's more toward the "only game in town" people.

If the only game in town is crap, and the -best- explanation is crap, so what? Spending your time on crap because it's the best crap there is doesn't remove the fact that you're working on crap.

I'd propose that it's better to be doing nothing, than wasting one's time working feverishly on something wrong or not even wrong.

When I see this "if you've got a better explanation let's see it" rebuttal it seems to be begging for the answer "ANY explanation is better" especially a clearly wrong one. A vacuous, unpredictive body of complexity is clearly much worse than ANY other explanation. The aether, the book of Genesis, etc. are all better explanations than string theory….and this is the beauty of this blog….because at least they're wrong, which is much more than the yarners have going

36. **Rickkkk**
August 17, 2006

Addenda, I think it's best summed by this analogy I've thought of.

Even THE MOST complex and contrived search for food in an empty room is a bad thing. No matter how elegant or theatric or difficult the methods employed, even giving up and starving from the get-go is a better approach. Leaving the room and searching elsewhere is of course the brightest thing to do.

So "what's your better explanation?" It'd be almost impossible to find a worse one, even the spaghetti monster is better.

37. **Peter Woit**
August 17, 2006

John,

I think Taylor gives a reasonably accurate description of the state of affairs as regards the possibility of ever predicting anything using string theory. He argues that the only hope at this point is to find this kind of correlations, but acknowledges that realistic calculations are impossible (you can only calculate in special cases, in an unrealistic approximation, and are trying to say something about all string theory vacua when you only know about a set of measure zero of them).

If the only hope for your theory having even a very small amount of predictivity is a very unlikely one, and you have no way of ever even checking this very unlikely possibility, why don't you give up and do something else?

38. **Who**
August 17, 2006

Tom Siegfried's Science magazine article "A Landscape Too Far?" has been made available at the SUSY 06 conference site
[url]http://susy06.physics.uci.edu/press/susy06_science_naturalness.pdf[/url]

or go here and select it from the "susy06 on the web" offerings
http://susy06.physics.uci.edu/proceedings.html
which include a recent posting from N.E.W.

39. **Michael Edwards**
August 17, 2006

Trying to calculate things in string theory is like looking for your keys under the streetlamp in front of your house, even though you know perfectly well you dropped them on the next block where it's pitch dark. This is not always as stupid as it sounds. If you are accustomed to finding things exclusively by sight, then you need to train your sense of touch before you stand any chance of finding your keys by feeling around where you dropped them. Perhaps you should spend some time feeling around under the light so that you can distinguish an actual dropped object from the surrounding landscape.

QFT as it stands today is a keychain dropped in the dark – a morass of heuristic prescriptions developed one by one to relate a string of symbols we call a "model" to calculations that match the experiments we know how to set up. Most of the major advances in the state of the theory have simplified the model at the expense of an added heuristic. (The examples I have in mind, roughly in historical order: Lorentz invariance, canonical quantization, the spin-statistics theorem, adiabatic coupling at infinity, Wick rotation, Ward identities, dimensional regularization, CPT invariance, gauge invariance, renormalizability, BRST invariance. Some of these are rigorous mathematics in their own right, but in the QFT context they function as heuristics for the selection of acceptable models and their elaboration into computable integrals.)

It does not seem to be widely recognized that the BRST _transformation_ is a different sort of theoretical advance. Viewed from a geometric perspective, it relates several of these heuristics (canonical quantization, Ward identities, gauge invariance, renormalizability, BRST invariance) to one another, and connects all of them to the intrinsic geometry of fiber bundles. It comes tantalizingly close to explaining why the entire framework of "second quantization" works as a method of perturbative expansion, almost irrespective of the chosen model — as long as that model is expressible as a BRST invariant Lagrangian density. And more concretely, it explains why the Faddeev-Popov gauge fixing procedure results in (some) anomaly-free QCD calculations.

Unfortunately, this leaves quite a bit of unfinished business: the apparent flatness and Minkowski signature of the metric, the presence of Dirac fermions and the nature of the Grassmann algebra they inhabit, the Wick rotation prescription for propagators, the nature and empirical values of dimensionless coupling constants at Planck scale, and the origin of mass and its relationship to broken C, P, and T symmetries. This last is the only one that seems to be remotely within the reach of experiment. But the current state of QFT is that these bits of "unfinished business" are every bit as much part of the _model_ as the mix of gauge groups and representations.

To demand under such circumstances that mathematical physicists focus exclusively on making falsifiable predictions is to misunderstand the role of theory in scientific advance. It so happens that the last couple of theoretical advances in QFT showed up in phenomenology largely as relationships between the measured values of coupling constants and angles. The next one probably will not; in fact, it may not show up in phenomenology at all, if its principal contribution is to relate (say) Wick rotation to the fact that only left-handed fermions carry representations of the electroweak SU(2).

Nobel prizes may be reserved for “visible” progress (if Nature cooperates on schedule), but it is only fair to say that work on the foundations is science too. It is rarely realized except in hindsight that this sort of “non-falsifiable” advance can reshape the landscape of plausible models, ruling some out and suggesting others that could not have arisen under the previous framework of heuristics. Quantitative predictions may have to await the next theoretical advance after that – say, obtaining three families of SU(3) triplets and singlets as an effective theory for one SL(3,R) triplet+singlet, obtained in turn as the centralizer of the discrete time reversal symmetry within GL(4,R).

What does this have to do with string theory, you ask? There is no reason whatsoever to think that the universe we live in has a compact S^1 dimension, any more than there is a reason to think that magnets are made of little arrows with their tails nailed to a lattice and springs between their tips. But many important phenomena general to phase transitions in statistical mechanics are more tractable in the Ising model than elsewhere, shedding light on which approximation techniques and other heuristics will be needed to obtain valid results for more realistic models. As I understand it, string theory began as an effort to train mathematical physicists’ cohomological sense of touch, in the light of the (relatively) tractable mathematics surrounding the streetlamp of diffeomorphisms on S^1.

Even ten years ago, one could still reasonably hope for similar insight into QFT from studying the Nambu string action. Perhaps this hope is now extinguished in many observers, and in some ostensible practitioners it seems to have been replaced by the bravado born of despair. Yet even if string theory has produced little that I would call insight, some string theorists have – or at least some of those known to the public almost exclusively as string theorists. Obviously I have in mind Ed Witten here, one or two of whose contributions I understand well enough to call a genuine insight into what a keychain in the dark would feel like. A working physicist could probably name others.

Doubtless the “community” of string theorists includes a fair number of charlatans, thugs, and complacent riders on the academic gravy train. Perhaps the proportion of dubious characters is even higher than elsewhere in this imperfect world of ours. If so, and if the careerists crowd out or drive out those who march to a different drummer, and if you care about whether physics transcends the standard model in our lifetimes, then you ought to be outraged. Not because string theory can’t predict the mass of the Higgs boson, but because it’s time to admit that the landscape under this particular streetlight isn’t telling us much about what keychains do and don’t feel like.

Cheers,
– Michael

40. **John Rennie**
August 18, 2006

Peter,

It seems to me that the real criticisms of String Theory come down to it’s unfair(?) level of funding. If money were no issue few of us would worry what the String Theorists got up to in their corner. Suppose you’re the one holding the purse strings. Surely you’d put some of the money into String related research (though probably not at the current levels) and you’d need to decide where to focus the effort. Here you have Wati Taylor pursuing a program to decide in concrete terms whether further investment in landscape research is justified. That seems to me an excellent idea and one that I’d pay for, even if I suspect I already know the answer. Isn’t this sort of research badly needed in String Theory?

It would be interesting to see if any other areas of ST research could be addressed in a similar way i.e. rather than pursue details take a step back and ask some basic questions about the feasibility of the approach. We industrial scientists are pretty good at spotting where research programs are going to cost our shareholders too much, even though they might ultimately be successful. This doesn’t seem to happen in academia. Of course even in industry we fund some blue sky work because it doesn’t cost too much and occasionally something intersting happens.

John Rennie

41. **Peter Woit**
August 18, 2006

John,

The problem with string theory is not specifically the level of government research funding. It’s more that a young researcher interested in mathematical approaches to particle theory basically will find it extremely difficult to get a job and have a research career unless they decide to do string theory. This has more to do with the hiring decisions being made by theory groups than grant funding.

Taylor is not pursuing a program designed to decide where to focus effort. He’s put forward an argument that shows that what other people are doing can’t work, that the only remaining possibility he can think of doesn’t work either, but he intends to continue with it anyway. I don’t understand why this should be encouraged.

The Unraveling of String Theory

August 14, 2006
Categories: Not Even Wrong: The Book

This week's Time magazine has as article by Michael Lemonick about the controversy over string theory entitled [The Unraveling of String Theory](). It mentions my book and Lee Smolin's, and there's a quote from Sean Caroll. There's the usual hysterical reaction from Lubos Motl: [Time Magazine: Physics is a Sin]().

Lemonick more or less gets the story right, describing the reaction of string theory critics to the landscape as:

It was bad enough, they say, when string theorists treated nonbelievers as though they were a little slow-witted. Now, it seems, at least some superstring advocates are ready to abandon the essential definition of science itself on the basis that string theory is too important to be hampered by old-fashioned notions of experimental proof.

Lemonick describes both Smolin and me as having worked on string theory. Smolin has done original research on the subject, but I certainly haven't. I don't agree at all with Sean Carroll that the problem is that not enough string theorists "take the goal of connecting to experiment more seriously". Many of them take it very seriously, but the fact that it is a failed idea that doesn't work is what has forced them into the landscape nonsense and other complicated, unworkable schemes.

The quote from me is a little bit out of context. I was making the point that physicists necessarily often start out with speculative ideas that are "not even wrong", in the sense that they are so poorly understood that one can't tell where they will lead, and that this is very much legitimate science. On the other hand, once a theory is well enough understood to see that you can't use it to make predictions, if you keep pursuing it, you're not doing science anymore.

Update: Tomorrow on [Science Friday]() Ira Flatow will have Brian Greene and Lee Smolin on to discuss string theory. The September issues of Scientific American and Discover magazines have book reviews of Smolin's book and mine. The Discover review is by [Tim Folger]() and entitled [Tangled Up In Strings](); it begins:

In the mood for some no-holds-barred gossip or a nasty screed? Then start browsing the physics blogosphere, where some exceedingly smart people are spending an inordinate amount of time belittling one another. Alas, even this magazine has come under attack. The cause of all the commotion? Some nervy upstarts are questioning the validity of string theory, which is to physics what Wal-Mart is to retail: the biggest thing around, dominant for more than 20 years now. And woe unto anyone who doubts the orthodoxy….

The Scientific American review is by [George Johnson]() and entitled The Inelegant Universe. Johnson notes one of his pieces for the New York Times six years ago carries what he now sees as an embarassing headline: "Physicists Finally Find a Way to Test Superstring Theory" (in his defense, this kind of headline is still appearing in over-hyped articles about string theory to this day). I've been a bit surprised at how friendly a reception Smolin's book and mine have been getting so far from science writers. I think one reason for this is that many of them have repeatedly over the last twenty years written articles about string theory that repeat a lot of the hype promising imminent success in producing predictions. They've now been burned too many times and are very open to listening to the critics.

Comments

1. **Eric Dennis**
   August 14, 2006

   A new informal fallacy:

   Take a position P on some controversial issue. Infer an obvious absurdity A by combining P with other propositions made by an opponent, even though you are aware your opponent believes ~P. Conclude that your opponent is asserting A and is, therefore, an incompetent fool whose arguments may be safely ignored.

   This fallacy might be called "Lubosing the Question." Or perhaps "Affirming the Lubos."

2. **Dick Thompson**
   August 14, 2006

   How about "The law of the excluded Lubos"

3. **A Friend**
   August 14, 2006

   Peter,

   I question your commitment to science – and academic scholarship in general. You have the good fortune to be a staff member of a well-funded and recognized institution of higher-learning, yet you *waste* your whole damn time trying to argue about stuff that is – in the final analysis – unarguable from a scientific point of view. String theory this! String theory that! … WHO CARES!! Everyone with some objective thinking capacity knows that string theory is still in its infancy – if you consider it to be a valid program in fundamental physics at all. That being said, it would *seem* you have nothing original to contribute other than what you have been arguing for the past ump-teen months… that string theory is not "genuine science"; yeah! we get it! Now do you want to spend your time repeating the same old broken record again and again!… or have you spent that position of academic priviledge trying to persue worthwhile research?

   … if so, what?…

   It would seem – and I say this as a friend, not an opponent – that you really don't have much to contribute constructively to the academic landscape, and hence your real *research* agenda is to spend all your time trying to convince people like Lubos that he is wrong… What a waste of time and energy!

   Your Friend

4. **LostHisMarbles**
   August 14, 2006

   Just to be pedantic: " … both Smolin and I …"

   What is timescale (and manpower/personpower scale) on which one expects to see results?

   Expt HEP hasn't produced anything beyond the SM in 30 years. (The b, t quarks, W, Z bosons, tau lepton + neutrino all fit into SM.) The pentaquark wasn't really beyond SM but didn't pan out. Expt HEP consumes vastly more millions that all of ST (perhaps more in 1 year than all of ST in 30 years … even including the cost of Caribbean orgies with nude eastern European girls). Is it then not even wrong, time to try something else? Because basically that is the way expt HEP is heading … funding for ILC is by no means guaranteed. Attempts to develop new acceleration technologies haven't gone beyond tabletop models as of yet. What to do?

Complain about ST if it makes you feel better, but ST isn't the problem.

5. **Chris Oakley**
August 14, 2006

Peter,

I forgot to say – congratulations on getting a mention in *Time* magazine!

I am not sure I approve of a magazine being named after a dependent variable, but it is at least a dimension that is known to exist.

6. **Who**
August 14, 2006

Marbles, you appear to begrudge funds to experimental HEP because for 30 years its results fit the SM. That counts as a confirmation of the SM. Presumably 30 years ago people did not KNOW that the Standard Model was so good that it would turn out to fit everything they could observe at accelerators for the next few decades.

It's clear the experimentalists acquired information by their work, while for 30 years most of the theoreticians explored multidimensional blind alleys. I think this is primarily because the string majority has not focused on major nagging problems such as the positive cosmological constant, dark matter, and gravity without prior metric. On the contrary, it drained attention from persistent central questions.

Progress in these core areas (assimilating the positive CC, dark matter, geometry without prior metric) has instead been made by a relatively small number of non-string theorists, who have now reached the stage of making testable predictions.

I have to say the quote from Sean Carroll was a laugh—what he said at the end of the Time magazine article:
The reason so many people keep working on it is that, whatever its flaws, the theory is still more promising than any other approach we have."

Marbles you suggest saying
"…Lemonick describes both Smolin and I…"
which would be ungrammatical. To your proposed correction "pedantic" is an injustice to pedants.

7. **a**
August 14, 2006

I am happy that censorship could not prevent you from bringing our problems to a wide audience. But please make it clear that only about 1/3 of high energy physicists are string theorists: otherwise wrong messages like
Peter Woit = string theorist
in some future might become
physics = string theory = shit.

8. **LostHisMarbles**
August 14, 2006

I don't begrudge funding for expt HEP but I also don't begrudge funding for ST. 30 years ago nobody knew what HEP would produce (expt or theory). When one says the last 30 years have been a confirmation of the SM, that's the point — it takes years for expt to validate any theory.

By the early 1980's the SM had been put together and the W and Z discovered, the big buzzword was "unification". A falsifibale prediction of proton decay was made, tested, falsified. The most obvious logical direction for the theory to take had failed. And so people searched for other ideas.

The fact remains that today, funding for expt HEP is increasingly hard to come by. Even RHIC (an already built machine, admittedly nuclear phys not officially HEP), has uncertain funding. And over 30 years the majority of theory (i.e. ST) has gone down blind alleys. It's easy to blame ST, but I expect the problems of HEP theory to persist until accelerators produce data beyond SM.

Dark matter? How does one perform controlled expts with dark matter? (How does it behave as a function of pressure and temperature? How to do such an expt?) If you have falsifiable predictions, then set up an expt and do it. Don't blame ST.

9. **Who**
August 14, 2006

The "dark matter problem" is to understand galaxy rotation curves and similiar related things about gravity. A pragmatic empirical approach to it would be to do the experiment which Bekenstein proposed a recent paper in Physical Review D.

http://arxiv.org/abs/astro-ph/0602266
Phys.Rev. D73 (2006) 103513

Marbles your reaction illustrates one source of the difficulty in theoretical physics.

Dark matter? How does one perform controlled expts with dark matter? (How does it behave as a function of pressure and temperature? How to do such an expt?) If you have falsifiable predictions, then set up an expt and do it…

OK Marbles, here is the experiment. Bekenstein has proposed it and it could be set up with available (Lisa Pathfinder) components. But if you try to *matterize* galaxy rotation curves–in a kind of reflex reaction–then I think you take us back into the cul-de-sac.

10. **MathPhys**
August 14, 2006

1/3 of all theoretical high energy physicists are string theorists? Is that an accurate estimate?

11. **AnonyMoose**
August 14, 2006

Peter, the discouraging replies above are unfortunate, but never mind your naysayers! I think you are doing a great job.

Eric, I am not sure what you mean because I can not assess the sarcasm or lack thereof in your post. I am fairly sure that this *new* informal fallacy is not so new, I think you are referring to the strawman fallacy. Anyways I can not tell whether you are attributing this to Dr. Woit or to Lubos. Same with Dick Thompson. I don't mean to nitpick but some of us could use clarifications.

As for the critics, you who calls her/him self Peter's friend. I think Peter Woit is committed to the scientific pursuit. You might think it a waste of time and energy to attack string theory, but then you might believe string theory to be a fringe position. Unfortunately, though string theorists may be in the minority, they are a large minority, and very vocal for that matter. Dr. Woit is merely struggling to remove this metaphysical dogma from legitimate science. And LostHisMarbles, who says there is any one problem? The HEP might not give us further success, but I would not consider it a complete waste. The HEP gave us more than string theory.

And finally, Chris, Who, thank you for encouraging Dr. Woit in his quest. *Time* Magazine. At least that dimension exists! But maybe we're not so sure of that! I mean time might not be manifested geometrically (as a dimension) after all, given that some theories of quantum gravity use 4-D spacetime as a mathematical model for a dynamic 3-D universe. (The theory of relativity may need to be changed according to the data to quantize gravity.) Maybe this is ontological speculation, but we will never know until further revolutions in physics. And we will have no revolutions in physics without proper experimentation.

12. **CapitalistImperialistPig**
August 15, 2006

Scientific American has a George Johnson review of your book and Lee's in the September issue.

Congratulations!

13. **a**
August 15, 2006

MathPhys: my estimate 1/3 for the fraction of string theorists has one digit of accuracy. Do you think that 1/3 is too low or too high? It might be higher among US theorists, but my definition of "high energy physics" also includes japanese experimentalists, european phenomenologists…

14. **Nick**
August 15, 2006

The article is on the front page of digg.com _link_

15. **D R Lunsford**
August 15, 2006

Chris,

You can go to time.com or space.com for free, but you need an ID and password to get into spacetime.com – I smell a rat.

-drl

16. **Chris Oakley**
August 15, 2006

Maybe God has reserved it until we learn to understand relativity better.

17. **Comm.Math.Phys. Santa Claus**
August 15, 2006

Hi Everyone,

Merry Christmas! (albeit somewhat early). Go to Springer's "Comm. in Mathematical Physics" website:

http://www.springerlink.com/content/1432-0916/

and enjoy free downloading for 30-consecutive days!… starting today!

Ho ho ho… now to return to the North Pole and package all those gifts for children everywhere! Ho ho ho…

18. **LostHisMarbles**
August 15, 2006

Submit a proposed experiment to run on LISA Pathfinder, to validate Bekenstein idea.

Decades ago, expt HEP validated current theory (or discriminated between competing theories), and simultaneously produced new puzzles, e.g. in 1950s there were precision tests of renormalized QED and also discovery of strange particles, parity violation (tau-theta puzzle). In 1960s there was the finding of the predicted Omega-, also unpredicted discovery of CP violation. And so on. But now for many years there is only a validation of SM, but no discovery of new data beyond SM from the accelerators. As for the cosmological stuff, propose expts to test ideas like Bekenstein's and get on with it. Never mind what the ST camp does.

What is happening in HEP today is an accident of circumstance. Don't blame ST for any of it.

Mother Nature is fickle. La Donna e Mobile.

19. **Sailor Moon**
August 15, 2006

I hope that professors in hep-th like teaching; the public is catching on, and hep-th will be defunded, just like hep-ex.

20. **LostHisMarbles**
August 15, 2006

HEP-ex (I include RHIC) is suffering from defunding because it is expensive and does not produce "instant gratification" or visible profit ~ basically no new exciting discoveries for many years (and the legacy of Manhattan project has worn off). It's not the same as getting lost in mysticism/mumbo-jumbo a la ST. But even so:

a) I do not begrudge the funding of ST

b) If there are worthy problems like dark energy, and ideas like Bekenstein and MOND, and devices like LISA Pathfinder which might be able to test the ideas, then propose an expt. Never mind ST.
c) The problems of HEP-ex (or HEP) are not caused by ST.

21. **Sailor Moon**
    August 15, 2006

    Mr. Marbles:

    The situation in hep-th is caused by the problem in hep-ex, not the other way around.

    The Superconducting Super Collider was canceled in 1993 because Congress saw a poor “bang-per-buck” ratio for investments in accelerators. High energy particle physics based on accelerators is being shut down across the US; research is migrating to the EU and and China. The US ~might~ get a linear collider in 2018.

    Yes, exciting things are happening in experimental neutrino physics, but the theory for that was done 20-30 years ago. We know that matrix elements off the diagonal aren’t zero… People have speculated that that might be the case for years.

    You might not begrudge the money that goes to ST, but remember that string theorists have to convince ~somebody~ to pay the bills. They’re highly productive at filling up pages in the Physical Review, but do they have anything useful to say about the universe we live in? Or rather, would those dollars accomplish more if we spent them on nanotechology, bioinformatics, or any of the other glamour fields that are an increasing priority at today’s research University.

22. **LostHisMarbles**
    August 15, 2006

    Sailor Moon: Indeed the problems of hep-th derive from those of hep-ex. I have said so from the beginning.
    It is an accident of circumstance in hep-ex that no new physics beyond SM has been found for 30 years. But ST is not to blame for that.

    So ST fills up pages in Phys Rev and takes funding. It is always a matter of value judgement as to where the money could be better spent. Nanotechnology etc can survive just fine on their own merits. You think ST should be defunded? Others think hep-ex should be defunded (as was the case with the SSC). Hep-ex deserves its funding, but it is a hard case to make without fresh discoveries, (as I say, an accident of circumstance). Who decides what is worthy to pursue?

    Do NOT make the fundamental mistake of thinking that if ST is defunded, that the money will go to “more deserving” fields. When the SSC was cancelled, some condensed matter people thought that the money would go to their field. It did not. Instead there was a general cutback in science funding. Basically, “if hep-ex can tolerate an $8B cut, then other fields of physics (science?) can also learn to make a sacrifice.”

    Funding is not a zero-sum game. The research money can simply disappear.

23. **Sailor Moon**
    August 15, 2006

    Yes, research money can disappear. And it very well may.

    The long term economic situation in the US is not good. US government, corporations and households have been able to spend beyond their means because the dollar is the international standard currency; the US can print dollars and trade them for oil, dvd players and sneakers. The picture won’t be pretty when boomers leave the workforce.

    Yes, what the US spends for the Iraq War in a year could pay for decades of science, but it’s not clear that basic science gives as good a return on investment as it once did. Before the 20th century, people had no idea what matter was, what life was, or how the brain works.

    There certainly are things to learn, but there may never be scientific breakthroughs as profound as relativity, quantum electrodynamics, or the discovery of the double helix. Scientists can fill journals from here until the end of civilization, but that doesn’t mean that funding agencies will care.

24. **LostHisMarbles**
    August 15, 2006

    Sailor Moon, I will have to leave it to PW to figure out the fundamental point of your post. I really have lost all my marbles.

    I merely make the point that defunding ST will not improve matters for other branches of hep-th. ST may be mysticism/dogma/etc, not science as one would like to define it, but that is neither here nor there. Researchers in other fields (CC, dark energy, etc) will have to make the case for funding on their own merits.

25. **D R Lunsford**
    August 15, 2006

    S&M,

    That’s what they said in the 1890s about the incomprehensible data of spectroscopy. I think big progress is right around the corner. In fact I know it is.

    -drl

26. **LostHisMarbles**
    August 15, 2006

    I do hope that big progress is right around the corner. I put it to you all that the way to find it is to go out and explore. Make models, formulate hypotheses, propose expts and carry them out. And **do this all on your own merits**. If ST contributes nothing to the effort, leave it be.

27. **John A**
    August 15, 2006

    Chris Oakley:

    > I am not sure I approve of a magazine being named after a dependent variable, but it is at least a dimension that is known to exist.

    …except in the realm of the quantum where the magazine de-materializes.

Yes. I am a geek.

28. **Antonio G Zenteno**
August 15, 2006

Sorry for changing the theme but it is very sad for me to notice the recent attack to this blog. Some minutes ago this blog was transformed with the typical sense of humor of this residual combination of irony with stupidity that many hackers have. In other circumstances this attacks are very less than superficial stupid games. But this blog is also a very notorious example of free-speech that is so fundamental for this communication system to survive.

So i need to say that i am in complete disagreement with this ugly way of showing the deep trash of our society that can not support the minimal requirement of left the people opinions alive even if you does not agree with them.

AGZ

29. **Ponderer of Things**
August 15, 2006

Hmm… Not a good week for Lubos.

The same issue of Times that declares String Theory "not even wrong" (hmm, I wonder where they got that quote? Is Pauli that known among Times reporters? 🙂) also featured a cover story titled "Who needs Harvard?"

Double-whammy for poor Lubos…

30. **D R Lunsford**
August 15, 2006

PoT,

Yes, I also saw that, and was reminded of the decline of the NY Times itself.

-drl

31. **Kea**
August 15, 2006

Peter

I wasn't joking.

32. **woit**
August 15, 2006

Kea,
Please stick to the topic of the posting.

Antonio,
I suspect you followed Lubos's link to this blog. The blog is fine, his link sends it through a site that translates it to "jive".

Ponderer + DRL,

Time magazine is not at all the same thing as the Times, either London or NY.

33. **Ponderer of Things**
August 15, 2006

Oops, I meant to say "Time", not sure why I said Times.
I have magazine sitting right in front of me, sorry for confusing you, DRL.

So, if a similar article is also featured in "Space" magazine, how many dimensions do string theorists have left? Is it 10-4=6 or 11-4=7?
🙂

34. **Antonio G Zenteno**
August 15, 2006

Peter,

Yes, this was the case. So my position and the interlocutor of my message are completely clear.

Thanks

35. **Kea**
August 15, 2006

Is it 10 – 4 = 6 or 11 – 4 = 7?

The 6 comes from the dimensionality (over R) of the complex moduli of the 6-punctured sphere.

36. **D R Lunsford**
August 15, 2006

Peter,

Yes that is true, I read the thing about Harvard in Time Magazine and was immediately reminded of the decline of the New York Times as a force for democracy, just as Harvard had been for truth (veritas). That Harvard would run off Glashow and hire Motl says everything. He's the Jayson Blair of matter.

-drl

37. **z3**
August 15, 2006

Scientific American Sept issue carries George Johnson's sympathetic review of Lee's and Peter's upcoming books. The review is titled "the Inelegant Universe", and made me laugh with its comparison of string theory papers to the robotic essay generator at http://www.elsewhere.org/pomo, which randomly produces a different paper every time you reload.

George now finds his own headline from the late 90s – "Physicists Finally Find a Way to Test Superstring Theory" – to be embarrassing. There is also a Burton Richter mention, as well as Hawking's quote from Beijing.

38. **Heinz Neumaier**
August 16, 2006

The ancient greeks said that a man is a man when he shows four virtues: justice, courage, measure and wisdom. In this discussion, justice is to say that what cannot be checked is not science; courage is to say this out loud despite the critics, measure is not to denigrate the opponents more than (or even as much as) they do with you, and wisdom is not to be led to embrace other issues. In contrast to many of his opponents, Peter is not only right, he is also a great man. And he is an example to others.

Peter, go on! What you do is needed in this world.

Heinz

39. **Jonathan Vos Post**
August 16, 2006

Perhaps the fraction of Physicists indulging in String Theory is 1 / pi?

Or maybe 1 / d where d is the number of actual spacial dimensions? In that case, if they are right, the percentage of them decreases…

Congratulations in any case for the Time Magazine mention. All I've ever had there was a Letter to the Editor.

40. **JoAnne**
August 16, 2006

In two years, we will have 1-2 inverse femptobarns of LHC data. In 3 years we will have 10-20 inverse femptobarns. I predict that at that point, nobody will be talking about string theory anymore. As pointed out above, we have been deprived of data at the TeV scale for a decade now (due to the untimely and unfortunate demise of the SSC) and this has hurt us all tremondously.

If the worst case scenario plays out, and the LHC discovers nothing, then that is the end of particle physics as we know it. And that includes string theory. They may think they are immune, but they are not – they will fall due to lack of funding with the rest of us.

41. **String Theorist**
August 16, 2006

JoAnne is mostly correct, but tenure is tenure, and we will write the history of theoretical physics, because nobody is better qualified to understand it than us.

42. **D R Lunsford**
August 16, 2006

JoAnne said:

If the worst case scenario plays out, and the LHC discovers nothing, then that is the end of particle physics as we know it.

I completely disagree with this pessimism. This is the mindset that created the monster of string theory to begin with. One never knows when a new idea will show up. The key point is to get a theoretrical explanation of electroweak symmetry breaking. Quantum gravity is a side issue in comparison. This explanation is not at hand, and an accelerator around the girdle of the Earth would not provide it. It has to come from someone's head. Prediction: The Higgs will not be found. That will be the great result of LHC that will set people thinking again. The answer will come out of neutrino physics.

My worry is that the data will be cooked by unscrupulous theory groups or simply misunderstood, as happened recently with WMAP 3 and Cooperstock-Tieu, respectively. The real damage done by string theory is the corrosive effect it has on scientific integrity in general.

We need more historians and fewer geniuses.

-drl

43. **Kea**
August 16, 2006

Prediction: The Higgs will not be found.

You can say that again! Can you repost those neutrino masses, Peter? I wasn't joking.

44. **LostHisMarbles**
August 16, 2006

I think the only particle to date which was misunderstood is the muon. But possibly the data could be "cooked". Find a bump, proclaim the Higgs. Does anyone recall the zeta 8.3 GeV ~ circa 1985? It was found by the Crystal Ball collaboration, promptly proclaimed to be the Higgs (the mass of 8.3 GeV didn't bother anyone) — I think some people claimed it was supersymmetry — and it was then undiscovered in a second high statistics run and never seen again.

I hope the LHC finds three bumps, the Higgs of the SM (boring), the Higgs of supersymmetry (hah!), and the Higgs scratch-your-head "who ordered that"?

45. **D R Lunsford**
August 16, 2006

LHM,

You have a way with words 🙂

Remember the magnetic monopole? 1984 I think..I was a rosy-cheeked deluded youngster and was sure it was real 🙂

Do you do online discussions anywhere? I'd like to hear your rationale for supersymmetry.

-drl

46. **King Ray**
August 16, 2006

As for string theorists, how many years do they have to wander in the wilderness before they realize they're lost? Looks like about 40…

47. **LostHisMarbles**
August 16, 2006

DRL- thanks for the compliment 🙂

Callan-Rubakov monopole ~ 1984 as I recall, catalysis of fusion (?) based on SU(2) group, but the effect does not exist in SU(5), wich was the GUT of the time.

I have no rationale for supersymmetry. I merely threw that in to be a troublemaker. I should point out that eminent physicists like Glashow (not an ST person) expounded total nonsense about the zeta. See

http://www.slac.stanford.edu/spires/find/hep/www?fin+j+PHLTA,B145,302
doi:10.1016/0370-2693(84)90359-9

Georgi et al suggested it as the lightest of a three-Higgs doublet no less
http://adsabs.harvard.edu/abs/1985NuPhB.253..205S
doi:10.1016/0550-3213(85)90527-9

I am sorry to disappoint you, but I do not do online discussions. I was looking for a reference to the "not even wrong" quote by Pauli, I stumbled across this blog, and though I realize it may be ego-deflating to PW, the fact that I am foolish enough to post anything here merely indicates that I have nothing better to do.

I suppose I disagree fundamentally with much of the philosophy expounded here. I have no great faith in ST, but I do not begrudge the funding of ST. I couldn't care less about the unravelling of any strings. The following statements may sound contradictory, but they are not mutually exclusive

a) ST diverts scarce funds from other branches of hep-th
b) Defunding ST will not make extra dollars available to non-ST research.

If anyone has worthy ideas and testable predictions, suggestions for expts, you will have to procure the funding on your own merits.

48. **Chris Oakley**
August 16, 2006

LHM,

If you think that the continuing belief that the universe must be 10-dimensional, supersymmetric and stringy does not hamper alternative ideas about the way the universe might possibly work, then you are quite wrong. This lunatic and chronic belief has up till now made it impossible for non-believers to get a word in edgeways. And if dissenters are totally ignored, then what chance do they have of getting academic jobs?

49. **LostHisMarbles**
August 16, 2006

The non-believers will have to succeed on their own merits.

50. **Thomas Larsson**
August 16, 2006

LHM, funding envy is not really at the heart of string theory critique, nor is proposing a better alternative necessary (although I have one 🙂) . Criticizing string theory is really a moral imperative. This was best formulated by string theory ex-star Dan Friedan: "recognizing failure is an essential part of the scientific ethos". If string theorists don't recognize that their theory has failed, others will. This is inevitable and long overdue.

51. **L. Riofrio**
August 16, 2006

Chris, Kea and DRL are right. Without elaboration, may I say that there are more fruitful avenues of research than are currently being funded.

52. **LostHisMarbles**
August 16, 2006

TL – Moral imperative? … if you say so.
"… nor is proposing a better alternative necessary … " — oh dear. Surely proposing a better alternative is the whole point? So what if ST is doing a bad job? Can you do a better job?

Groups or cults or whatever, which attempt to stifle debate and drown out alternatives have historically never succeeded. The truth always wins out. But that truth will some in the form of expt data. One theory never wins over another simply because it is prettier. It wins because it does a better job of explaining data. QFT didn't win out over S-matrix theory in the 1960's (or early 1970's) because of some intellectual theoretical debate. QFT (by which I mean the quark model, QCD and ElectroWeak) won out because of expt events, most especially charmonium. QFT explained the expt findings of the day, and furthermore made predictions about charmed bound states (which turned out tio be correct). Alternatives could not match this success.

There simply needs to be data (new puzzles, not merely confirmations of SM) from the LHC. Theoretical critiques of ST will go nowhere.

If you believe there is already data (e.g. cosmological), then put together an expt proposal to validate your non-ST ideas.

53. **John A**
August 16, 2006

Dear Antonio,

Unfortunately the Reference Frame has also been hacked by cockneys.

It's shockin' wha' 'appens on the Internets, guv…

54. **Thomas Larsson**
August 17, 2006

So what if ST is doing a bad job? Can you do a better job?

hep-th/0411028

Modulo some serious flaws (corrections in progress), the essential proposal is to replace QFT by QJT (J for jet), i.e. to expand all fields in a Taylor series prior to quantization. By doing so, one introduces a new datum: the expansion point. This is an essential modification, because it allows us to write down new gauge and diff anomalies, which lead to the higher-dimensional generalizations of Kac-Moody and Virasoro algebras.

There simply needs to be data (new puzzles, not merely confirmations of SM) from the LHC.

What if the ultimate theory of nature is essentially the SM coupled to GR, but treated within the framework of QJT rather than QFT?

55. **Tony Smith**
August 17, 2006

Thomas Larsson referred to hep-th/0411028 and said "… the essential proposal is to replace QFT by QJT (J for jet) …",
but
hep-th/0411028 did not refer to any of the extensive work of Gennady Sardanashvily on jet bundle physics, such as
math-ph/0203040 entitled Ten lectures on jet manifolds in classical and quantum field theory
as well as many further papers (some more recent) that can be found by searching arXiv for Sardanashvily as author. Would such works be helpful in Thomas Larsson's program?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

56. **Rickkkk**
August 17, 2006

Ludbos' post is even more ironic than usual.

"simply can't have any respect for because of their complete lack of intellectual integrity"

Unsurprisingly, Luddite Moron absolutely fails to grasp the actual meaning of intellectual integrity itself. The absolute epitomy of intellectual integrity is questioning something(especially something as foul as String Theory) I would challenge anyone to give a better characterization of what -defines- intellectual integrity. The irony here should be obvious, while decrying an act of intellectual integrity as its opposite, Ludbboose himself is the poster boy for failed integrity.

So to set the record straight: Integrity is ad hominem attacks on detractors(which as a sideline, grow more desperate as these detractors become the majority) and challenging an non-explanation like String Theory is the cardinal sin of science?

Caboose Motl is certainly the last car on a train that's heading out of town rapidly. Everyone smile and wave 🙂

57. **LostHisMarbles**
August 17, 2006

TL – pursue your ideas to fruition (perhaps in collaboration – follow up on Tony Smith post – work by Gennady Sardanashvily), figure out some testable consequence, e.g. for cosmology or HEP beyond EW scale, and find an expt collaborator/submit an expt proposal to validate it. You have to do this on your own. The ST camp doesn't owe you anything (such as a time slot at a conference). Ultimately your ideas have to survive on their own merits. Good luck.

Rickkk ~ If it makes you happy to write "Luddite Moron" and "Caboose Motl" good for you. You will ultimately go nowhere.

58. **George Lehtola**
August 17, 2006

Personally, I just like to hear the views of all writers and for this I am glad to have this Blog to raed.

59. **MoveOnOrStayBehind**
August 17, 2006

Well, there are writers, and there are scientists. Which ones do you prefer?

60. **Jonathan Vos Post**
August 17, 2006

MoveOnOrStayBehind Says:

August 17th, 2006 at 3:10 pm
Well, there are writers, and there are scientists. Which ones do you prefer?

With all due respect, there have MANY first rate scientists who were (or are) also first rate writers.

Consider, to pick just a few at random, Pascal, Darwin, Galileo, Oliver Sacks, Roald Hoffmann, Gregory Benford…

61. **Tony Smith**
August 17, 2006

Lost His Marbles, said, with respect to Thomas Larsson's NON-superstring approach to theoretical high energy physics:
"… figure out some testable consequence … and … submit an expt proposal to validate it … on your own …".

That is both true and reasonable.

Lost His Marbles went on to say:
"… The ST camp doesn't owe you anything (such as a time slot at a conference). …".

In a fair and considerate world, that would also be true and reasonable,
BUT
as Peter quoted Tim Folger saying in Discover magazine:
"… [super]string theory … is to physics what Wal-Mart is to retail:
the biggest thing around, dominant for more than 20 years now.
And woe unto anyone who doubts the [superstring] orthodoxy …",
so
our real world is "not considerate or fair"*,
and
LHM's second statement quoted above is true but NOT reasonable.

In short,
the ST camp is so dominant a monopoly in high energy physics theory,
that
it DOES owe to alternative approaches fair treatment (such as time slots in conferences,
rights to post to the Cornell arXiv, etc).

The penalty to superstringers for their abuse of their monopoly position will be a place in history alongside the inquisitors of Giordano Bruno.
Bruno's inquisitors were doubtless pleased with themselves for seeing Bruno dead,
but ideas don't die so easily,
and
to slightly paraphrase LHM:
"… ideas WILL survive on their own merits …".

If I were in the superstring establishment, I would be very thankful for Peter,
because
Peter is offering them a graceful exit from a position of great risk of a humiliating place in human history.

A graceful exit is possible because Peter is NOT pushing any particular alternative approach,
so the superstring establishment would not have to anoint a successor monopoly approach, but
only would have to admit that they have had their exclusive turn,
and now it is time to be inclusive of other approaches.

Following Peter would not even require anybody to give up their organizational positions of power, only to allow freedom of thought.

It would be sad indeed if the inevitable fall of the superstring establishment led,
not to the blooming of a thousand flowers,
but to a successor oppressor.

Would the Loop Quantum Gravity guys be any less oppressive about alternative approaches if they were to become a successor monopoly ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

*"not considerate or fair " is a quote from Batman Begins (Ras al Ghul).

62. **Thomas Larsson**
August 18, 2006

LHM – it is true that string theorists don't owe me anything, but neither do I owe anything to them (except perhaps a four-year postdoc to Lars Brink, but that was many years ago). If you don't like my marketing strategy, that's your problem.

As a physicist, I find it very significant that **all** natural string theory predictions – SUSY, extra-dimensions, 496 gauge bosons, new long-range forces (unstabilized moduli), a non-positive cc, etc. – are in apparent disagreement with experiments, and I find it deeply dishonest that this fact has been systematically deemphasized for 20 years. If string theorists had told the public about these problems, rather than going on about how beautiful M-not-a-theory is, maybe they would have gained more sympathy.

However, at the end of the day it does not matter. Theories that don't produce predictions (and correct ones) will flounder, just as companies that don't show profit will. It is neither mine nor Peter's fault that the cc is positive, that the proton refuses to decay, that the Tevatron and precision experiments show no signs of SUSY or extra-dimensions, etc. It is just Nature's way of telling us that it does not like string theory.

63. **John Rogers**
August 18, 2006

By the way, Walmart just sold all the stores it had in Germany: it did not make any profit in any of the years it was active there. Is this a sign of things to come?

64. **Som**
August 18, 2006

I believe that the whole issue of the anthropic principle is not irreconcilable with meaningful physics if you take the point of view propounded by some people (like Vafa, Verlinde and others in the context of Ads2*S2 universes), that the universe that we see is actually a collapsed state of the "universal" Wheeler-DeWitt wave-function.

From this point of view it is quite conceivable that the multiverse is like a super-Fock space spanned by axes, represented by different universes with differing parameters. However I have to say that I don't see a compelling need for the multiverse to be an exclusive product of string theory but that's another matter. What I would like to point out is that given this viewpoint one can inculcate a more ambivalent attitude towards the anthropic principle than that it is usually accorded.

65. **LostHisMarbles**
August 18, 2006

Tony Smith – I agree with much of what you say. I hope Thomas Larsson paid attention.

TL – if I don't like your marketing strategy "that's my problem"?? It doesn't matter in the slightest what I think. It's *your* ideas, you have to make of them what you will. See comments by Tony above.

66. **LostHisMarbles**
August 18, 2006

There was a Goddess Zeta Monopole from Infinity,
From her three breasts flowed Milk, Wine and SuperSymmetry,
One zap of lightning from her Trident of Incredulity,
Could reduce the most hardened non-believer to abject impotency,
She was not to be trifled with, that Goddess from Infinity.
.

I composed this for drl, but pw can toss this out if he wants to. It's not Shakespeare by a long shot.

67. **Tony Smith**
August 18, 2006

John Rogers said
"… Walmart just sold all the stores it had in Germany … Is this a sign of things to come? …".

It is NOT a "sign" that a dominant monopoly is crumbling.

Germany already had, BEFORE Walmart went there, established deep-discount chains such as Aldi,
so
failure of Walmart in Germany actually shows the POWER of entrenched monopolists (such as Aldi in Germany and superstringers in high energy physics theory).

Note that Aldi is trying to crack the Walmart monopoly in the USA, but seems to be having no more success than Walmart did in Germany.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

68. **Chris Oakley**
August 18, 2006

> There was a Goddess Zeta Monopole from Infinity,
> From her three breasts flowed Milk, Wine and SuperSymmetry,
> One zap of lightning from her Trident of Incredulity,
> Could reduce the most hardened non-believer to abject impotency,
> She was not to be trifled with, that Goddess from Infinity.

I was wondering why you call yourself "LostHisMarbles". Now it is clear.

69. **D R Lunsford**
August 18, 2006

CO – he's lost his marbles, but kept his yarbles 🙂

Thanks, KHY.

-drl

70. **LostHisMarbles**
August 18, 2006

lol ty drl

71. **Chris Oakley**
August 18, 2006

Ah – *A Clockwork Orange* … I only saw it recently as it was banned in the UK until after Stanley Kubrick's death. I wonder … maybe someone could make an updated version with Peter as protagonist. A band of middle-aged, science-hating thugs who have no respect for the establishment institute a reign of terror and eventually have to be sent to a special clinic for reprogramming (Lubos being a particularly sadistic doctor here).

72. **D R Lunsford**
August 18, 2006

CO – "Singin' in the 'Brane"?

-drl

73. **Chris Oakley**
August 18, 2006

Here we are looking for Nazzes (String Theorists) on the campus at Columbia.

L->R: Tony Smith, Danny, Peter, me.

74. **KH Yarbles**
August 18, 2006

No place for me?

75. **Chris Oakley**
August 18, 2006

Sorry, but we can't include anonymous posters.
They've got no sharries, and just itty when the drat gets going.

76. **KH Yarbles**
August 18, 2006

gorblimey guv it's a fair cop

77. **Reality**
August 18, 2006

What is it with you 'non-stringers"? Are you saddened that others are grasping concepts that you are unable to? Here's a simple question.

"Does 'YOUR MODEL' explain everything"?

Of course it doesn't.

Well, like it or not, 'SUPER STRING' DOES explain everything.

Nothing you can say will change this reality.

When you go to 'disprove' ANYTHING, you are fighting a loosing battle. Especially with yourself. Others are merely laughing at you, while some are saddened for you because you simply cannot accept this reality.

It is no wonder with such 'closed minds' that festering attitudes of rediculousness abound.

No one has a need to remove science from this model. It is merely that 'a few' of the concepts 'REQUIRED' to further understanding on this theory require your mind to be able to grasp things that it was previously unable to grasp.

You poor jealous goofs don't have a clue when it comes to reality. To say that something is false "REQUIRES FAR MORE, THE NEED OF A SCIENTIFIC EXPERIMENT, THAN THE RESULTS OF THE 'EFFECT' OF THIS THING YOU CANNOT SEE'

So, until you 'HAVE THAT PROOF', please do all of us who "HAVE SUFFICIENT PROOF" a favor, and STFU. Besides, unless you can 'back your attitude up' with a 'sustainable argument against reality', you're merely pissing in the wind. And believe me, most everyone is ;aughing at you while you do so.

78. **J.F. Moore**
August 18, 2006

I eagerly await the inevitable rebuttal to all of this (or pointed interview) from Professor Harold Hi— I mean Professor Michio Kaku. Unfortunately, it might be on AM radio at 0200. We might get lucky though and see a "controversy in physics matchup" on a cable news show.

79. **King Ray**
August 18, 2006

String theory is obviously wrong. It fails to meet the criteria of beauty and simplicity. It is a Ptolemaic theory; it is more complicated than the standard model and GR. A unified theory is supposed to be simpler than the theories it reduces to, not orders of magnitude more complicated.

80. **John Gonsowski**
August 18, 2006

Reality, take your amazing mind over to PBS and watch Brian Greene tell the general public that string theory could all be wrong. Combining "it could all be wrong" with 20 years and a monopoly is really bad. Too many eggs all in one too wrong basket for too long.

81. **KH Yarbles**
August 18, 2006

Ask yourselves how/why ST became a monopoly in the first place. QFT and the SM were dominant in the late 1970s – early 80s. Why blame ST? Why did QFT not remain dominant?

82. **Yatima**
August 19, 2006

Reality says:

"Well, like it or not, 'SUPER STRING' DOES explain everything."

This amazingly and weirdly sounds like something out of "The Incredibles":

"SYNDROME: You sly dog! You got me monologuing. I can't believe it. It's cool, huh? Zero-point energy. I save the best inventions for myself. Am I good enough now? Who's super now? I'm Syndrome! Your nemesis and…"

Sorry for this post, I'm off to do productive system administration now. (Bows)

83. **Thomas Larsson**
August 19, 2006

LHM – Maybe I misunderstood you. It seemed like you were first saying that I wasn't allowed to criticize the string theory hype because I lacked original ideas, and when I showed that I indeed had original ideas, string theorists have no reason to listen anyway. Quite an effective strategy for keeping string theory the only game in town 🙂

However, beauty has nothing to do with it, just inevitability. The multi-dimensional Virasoro algebra is important to quantum gravity for the same reason that the usual Virasoro algebra is important to string theory – it is the correct quantum form of the correct constraint algebra. That's why I decided to discover it, many years ago.

84. **KH Yarbles**
August 19, 2006

TL (and anyone else for that matter) –
Nobody was obliged to listen to S-matrix theory in the 60's, nor was anyone forbidden to criticize it. Nobody was obliged to listen to QFT either, or forbidden to criticize it. Julian Schwinger did, for example, opt out of both, and invented source theory.

"If you can't join 'em, beat 'em"
– JS, dedication in Source Theory books

Nobody is obligated to believe ST, or anything else, nor is anyone forbidden to criticize ST, or anything else, nor is any ST person required to listen to any non-ST person.

If you have ideas, they must survive on their own merits. This the ideas do by proving their worth by explaining phenomena (to a better extent than rival ideas).

How did ST become a monopoly anyway? Other camps (QFT) had a fair chance to be heard.

85. **Open Source**
August 19, 2006

"How did ST become a monopoly anyway? Other camps (QFT) had a fair chance to be heard."

But string theorists are wiling to lie, cheat, and steal.

To hype, and hype, and lie, and hype, and lie.

Nice guys finish last.

86. **KH Yarbles**
August 19, 2006

" … lie, cheat, and steal."

The scientific community at large is not obligated to believe any of ST. Historically the scientific community has shown itself to be effective at sorting out good ideas from bad. The truth wins out, despite religioius persecution, prejudice against "Jewish science" and other nonsense.

It does no good to merely proclaim "I have a (better) idea". The idea has to demonstrate its worth.

"Nice guys finish last." Don't whine.

87. **Open Source**
August 19, 2006

Technically speaking, any theory would be as good as string theory because string theory predicts everything and nothing.

Ergo the government should give equal funds to every theory.

Why should other theories have to prove themselves when string theory does not?

"Human–all too human," is what String Theorists are.

Their vast yearning to be on Nova Star Trek specials has blinded them to their mendacious mediocrity, and it has brought out the very worst from their small, cowardly, conformist, group-think minds.

I wish String Theory had at least one postulate we could talk about, but the postmodern joke lackas even that.

88. **Open Source**
August 19, 2006

KH Yarbles Says: "It does no good to merely proclaim "I have a (better) idea". The idea has to demonstrate its worth."

Does that mean that NSF should immediately stop funding String Theory which has failed to prove its worth for over thirty years?

Let's write a letter to the NSF, and KH Yarble, the defender of all that is Right, and True, and Worth Something can be the first to sign it.

That way, funding can go towards better theories which are rooted in logic and reason, such a MDT.

89. **Tony Smith**
August 19, 2006

KH Yarbles asked:
"… QFT and the SM were dominant in the late 1970s – early 80s.
… Why did QFT not remain dominant?
… how/why [did] ST bec[o]me a monopoly in the first place …" ?

Perhaps the existence of a monopoly in theoretical high energy physics is a reflection of the sociology/psychology of the USA high energy physics community.

Raoul Bott made an observation about the Princeton IAS under Oppenheimer:
"… Oppenheimer had taken over, and he was very dominant in the physics community. He had a seminar that every physicist went to.

We mathematicians always thought they ran off like sheep,
for we would pick and choose our seminars! …".

Consider some excerpts from other entries in this blog over the past year or so:

"… ObsessiveMathsFreak Says:
When you were young, you assummed that scientists were a magnamous, logical and rational bunch. You had great faith in their ability to be imparital and to discern the truth through the application of scientific rigour. You also thought they had great integrity and were above petty actions as they aspired to the greater goal that was The Truth.
Then you grow up … see that scientists are just as human as everyone else, pettiness included.
It's still very disappointing though. …".

"… D R Lunsford Says:
It all sounds like groupthink … Groupthink seems to be at the bottom of much of our (USA) current dysfunction. …".

"… Dumb Biologist Says:
… it seems …[superstring theorists]… have forged ahead so far away from attainable real-world checks and benchmarks that the system of peer review is all they've got, or perhaps will have, for a very, very long time.
How could the tyranny of groupthink not prevail in such an environment?
It's functionally equivalent to a church.
Very sad. …".

In short,
the USA high energy physics community acts like a herd of sheep,
with more importance placed on being a member of the herd than on exploring new territory.

I fear that, unless that collective mind-set it changed,
if LHC fails to see supersymmetry and so puts the final nail in the coffin of conventional supersymmetry,
the herd will just follow the most then-charismatic shepherd
(Lee Smolin seems to be trying to fill that role, using his Loop Quantum Gravity program)
and continue in its dysfunctional group-think ways.
To use the church analogy, think of the Roman Catholic church being replaced in England by the Church of England.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Although I have a physics model that in my opinion substantially contradicts
(by calculating particle masses, force strengths, Dark Energy : Dark Matter : Ordinary Matter ratio, etc)
the assertion in a comment in this blog entry by Reality: "… What is it with you 'non-stringers"? … Here's a simple question. "Does 'YOUR MODEL' explain everything"? Of course it doesn't. …",
I would be very unhappy if my model were to be used as a monopoly that suppressed other approaches.
My view is that a thousand flowers should bloom, and that all important institutions (university departments, laboratories, institutes, etc) should encourage active investigation of ALL the blossoms,
by rewarding grad students, post-docs, etc., for work on whatever they find interesting.
If a studied model turns out to be wrong, then the work showing it to be wrong should not be considered a worthless negative result, but a useful contribution (like weeding a garden) to advancing physics by cultivation,
and such negative results should be just as important as positive ones in getting publications, Ph.D.'s, post-doc jobs, and faculty appointments.

PPS – Although the above is written about theoretical high energy physics, experimenters in high energy theory are not immune, with their groupthink problems being due to a combination of
necessary large collaborations
and
unnecessary insistence on a single consensus viewpoint with respect to results of data analysis.

90. **Who**
August 19, 2006

Tony your fears seem a bit exaggerated, non-string QG is a bunch of different approaches. You say
***I fear that, unless that collective mind-set it changed, if LHC fails to see supersymmetry and so puts the final nail in the coffin of conventional supersymmetry, the herd will just follow the most then-charismatic shepherd (Lee Smolin seems to be trying to fill that role, using his Loop Quantum Gravity program) and continue in its dysfunctional group-think ways*.**

There is no monolithic LQG program. Check out the contents of Oriti's book ("Approaches to QG" Cambridge 2006?) a lot of which is already on arxiv. It is an association of separate initiatives. What I see John Baez students and former students doing is way different from what I see Laurent Freidel and collaborators doing. At present, neither of those approaches makes contact with what Lee Smolin has been pursuing recently (which itself is not conventional LQG either.)

Research funding policy is different from specific programs. Your PS sounds rather much like what Smolin has been proposing as a policy direction in fundamental theory research funding—support proven talent and independence, by individual rather than by program. That is an investment strategy at policy level, not an "LQG program" and it does not require a charismatic research tzar to implement. Both your PS and Smolin's essays on the subject are directed against groupthink.

91. **KH Yarbles**
August 19, 2006

OS –

It's Dr Yarbles to you.

"… That way, funding can go towards better theories which are rooted in logic and reason, such a MDT. "

This repeats a fallacy which persists with many people. NSF does not have a pot of money which will be redirected elsewhere, if ST is defunded. There will be no money for MST if money for ST is cut. In fact, NSF has been defunding HEP for many years now, and the trend shows no sign of abating, e.g. the cut of RSVP (Rare Symmetry Violating Processes) at BNL.

" … KH Yarble[s], the defender of all that is Right, and True, and Worth Something … "

a) I owe drl a debt of gratitude here, which I am not sure I can repay. But see below …..
b) I cannot even defend the chastity of a Goddess, and CO may have strong words about even that.

TL proposes non-SM ideas which he claims to be better than ST. A paper on arXiv is a good first step, but it is by no means the last. It is necessary but not sufficient. Make a falsifiable prediction and put together an expt team to test it.

Tony Smith writes much that is sensible. See also sensible reply by Who. What can a fool like I add?

Back in the early 80's, QFT + SM was utterly dominant. The prevailing attitude was that the next step was unification of the strong, weak and EM interactions into one gauge group (GUT), the smallest candidate of which was SU(5). The immediate predicted consequence of GUT SU(5) was proton decay. This was a clear cut falsifiable prediction, but unfortunately
a) the prediction failed
b) QFT had no backup Plan B. It simply fell into disarray.

Equally unfortunately — and this is nobody's fault — expt HEP simply did not turn up anything beyond SM, and after 30 years there is still nothing beyond SM from the particle accelerators. QFT relied on working closely with hep-ex, and there has been no food for QFT.

`Who' is correct to note that non-ST (e.g. LQG) is not monolithic. ST's source if strength is arguably simply that it is monolithic.

What is my answer? … Hep-th will languish until there is some new non-SM data from the accelerators. It is nobody's fault. Rail against ST if it makes you feel better.

92. **KH Yarbles**
August 19, 2006

There was a Goddess from Infinity,
She was the final authority on g-string theory,
There were those who said it was all not even wrong,
Alas and alack, their yarbles were insufficiently strong,
She reigned supreme, that Goddess from Infinity.

93. **Open Source**
August 19, 2006

YOU'RE MISSING THE POINT!!

NSF damages physics by a factor of 1,000 for every dollar it spends on string theory.

NSF influences billions of dollars of cashflow with its millions of dollars.

Brian Greene's Hocus Pocus Diddly-Docus has set physics back a hundred years, as it has fostered a fan-boy star-trek atmosphere which has exiled physicicists who deal in reality, logic, and reason.

http://revver.com/video/48391/21022

94. **Chris Oakley**
August 19, 2006

KHY,

It doesn't rhyme, it doesn't scan and I don't like the message. The only use for your limerick is for torturing prisoners.

95. **Chris Oakley**
August 19, 2006

Link: http://en.wikipedia.org/wiki/Vogon_poetry

96. **Rickkkk**
August 19, 2006

Quote: "Technically speaking, any theory would be as good as string theory because string theory predicts everything and nothing."

This is a point that I was making in a previous post, a point that isn't made enough it seems.

The typical rebuttal, demonstrated many times in this post, is "DO YOU HAVE A BETTER MODEL?"

97. **KH Yarbles**
August 19, 2006

No-one has ever accused me of ever getting the point. There is a leverage factor of 1000 for every dollar spent by NSF? Fine… I have no idea. NSF damages physics by the dollars it spends on ST? … I also have no idea. Complain if you want to.

98. **KH Yarbles**
August 19, 2006

CO – I'll take that as a compliment.

99. **Rickkkk**
August 19, 2006

Darnit, I pushed enter too soon.

Anyhow, that rebuttal isn't adequate. As the quote says(correctly) there are NO worse models, theories, or even speculations worse than string theory. This isn't just angry slander or hyperbole, analytically speaking if there is a definition of a PERFECT, albeit perfectly bad theory, it is string theory. It is the model of avoiding predictions by predicting everything. It is absolute PERFECTION in its “badness”

I want to reemphasize, I am not being sarcastic here or even inflating the truth. I am genuinely impressed by the vacuousness of the yarn.

So, yes, I have a better model, my brother has a better model, my grandmother has a better model.

No model is better than string theory? Indeed…”no model” would be better or at the very worst, equivalent.

100. **woit**
August 19, 2006

The comment section has degenerated into nearly pure noise, bad jokes and pointless bickering, so I’m turning off comments on this posting. Unless you’ve got something informed and substantive to say about the topic of the posting, please resist the urge to write here. I’ve recently had complaints from several physicists that they don’t want to comment here because of the huge amount of noise and nonsense. I feel the same way myself. This is seriously damaging what I’m trying to do here.

Aaron Bergman Review of Not Even Wrong

August 19, 2006
Categories: Not Even Wrong: The Book

Aaron Bergman has written up a review of my book and posted it over at the String Coffee Table. It's quite sensible and makes reasonable points, so I'm very glad he wrote it. Here are a few comments of my own about the points raised in the review. I don't have time to discuss everything in it right now, but if someone feels that I'm not addressing an important point of Aaron's let me know.

It's true that the book isn't "even-handed" in the sense of repeating many of the arguments made for string theory. One reason for this is that I assumed that essentially all my readers would have read at least something like one of Brian Greene's books. I originally intended my book as something that would be published by a university press and be aimed at people with some background in the subject. The fact that it ended up being published by a trade publisher wasn't my first choice, and the wide attention it is getting from people who know little about physics is a surprise to me, something I wasn't counting on.

Instead of repeating many of the what seem to me highly over-hyped claims made for string theory and spending a lot of time explaining exactly how and why they're over-hyped, I decided to just write down as accurately as possible how I see things. The black hole entropy calculations are an example of what I mean. I do mention these, but I think Aaron's description of them as a "holy grail" vastly overestimates their signficance. It's also true that string theorists still have not been able to do calculations for the case of physical 4-dimensional black holes. A truly honest description of the situation would require a detailed examination of exactly what has been calculated, and what remains still not understood. This is a highly technical business, not easy to extract from the often hype-filled literature, and I just didn't think that even if I put the effort into doing this well, it would work as part of the book. Similar comments apply to the AdS/CFT story, where sorting through the hype and clearly distinguishing exactly what has been achieved and what hasn't would be even more difficult.

People can compare what I have to say to what string theorists have to say, and see that there's a different point of view on many things. If they have some expertise, they can look into these more deeply and decide for themselves. Aaron describes the book as "tendentious", but I think it's much more scrupulously accurate in its descriptions, honest and even-handed than any of the many books promoting string theory, essentially all of which contain vast amounts of misleading hype designed to give the reader an inaccurately optimistic view of the theory.

About the CC and supersymmetry: I re-read that section after Lubos's review complained about it, and it was not clearly written. But the argument that I'm not giving SUSY credit for being wrong by 10^{60} instead of 10^{120} doesn't make sense to me. Both are obviously in the same category of being completely off-base in a very fundamental way. The situation with SUSY is actually worse than non-SUSY, because in a non-SUSY theory the vacuum energy is not something that you can calculate even in principle. In a SUSY theory (before you turn on gravity), it's the order parameter for supersymmetry-breaking, so has to have a scale of at least 100s of GeV to explain the lack of superpartners. Your theory of quantum gravity is supposed to ultimately explain the CC, and, for doing this, supersymmetry not only doesn't improve the situation, it introduces a huge new problem you have to find some way around.

About the section on mathematics, and that I'm being petty about denying credit to string theory. Again, I think what I write is far more honest that just about anything string theorists have to say about the relation of string theory and mathematics, much of which is based on alotting to string theory purely QFT results.

About S-matrix theory, Chew, Capra. I think the lesson of what happened with S-matrix theory is an incredibly important one, and suspect that someday history will repeat itself. Before asymptotically free theories, people were convinced they had a good argument that QFT couldn't be fundamental, just as many people are now convinced that problems with quantizing gravity imply that QFT can't be fundamental. The arguments from Chew and Capra about getting rid of symmetry arguments and QFT in favor of the bootstrap are all too similar to things one hears these days from some string theorists. As for the denial of reality by Chew and Capra, post-QCD, there is no analog yet in the case of string theory. But, if someone finds a better way of quantizing gravity and getting unification, I'm willing to bet that, just like in the case of S-matrix theory, most theorists will move on, but some will refuse to ever give up on string theory and deny reality. We'll see what happens. Eastern religions are a lot less popular in the US these days than they were in the 70s, so I don't think there will be a new "The Tao of Physics". But, already, if you take a look at Susskind's "The Cosmic Landscape", it holds up as science no better that Capra's book.

About describing string theory as a cult with Witten as its guru. I believe Joao Magueijo in his book explicitly does this, and I can think immediately of three well-respected physicists or mathematicians who have, unprompted, used this description in conversations with me. Based on my experience, I'm pretty sure that if you sample non-string theorist physicists, you're going to find many people who would describe the behavior of string theorists as "cult-like". This behavior is described by Lee Smolin as "groupthink" and he has a lot to say about it. I wrote that I don't think it's useful to describe string theory as a religious cult, because the phenomena are significantly different, but I would characterize the behavior of some string theorists in recent years as "cult-like". Some people exhibit a disconnect from the reality of the problems of the theory that is much like the way members of a cult behave in face of evidence contrary to their beliefs. Lubos is an extreme case, but there's lots of others, of varying degrees. Describing Witten as the field's "guru" I think is actually uncontroversial. There's nothing wrong with having "gurus", as long as you realize they are sometimes wrong. People who have demonstrated great amounts of knowledge and wisdom deserve to be listened to very seriously, but no one is ever right about everything.

About the Bogdanovs. The main reason I wrote about the Bogdanov story, (besides for its entertainment value), is that I think it shows conclusively that in quantum gravity in general, many people have lost the ability or willingness to recognize non-sense for what it is. Sure, this is not specifically a string theory problem, but it's also not a problem specific to non-string theorists doing quantum gravity. This was swept under the rug at the time, and attributed to a few lazy referees, rather than dealt with as a serious problem that needs to be addressed if the field is not going to drown under an increasing tide of crap, and I think this was a big mistake, with the tide rising since then. I don't apologize at all for writing about it in the book. As for the inclusion of the e-mail describing the reaction of the string group at Harvard, I don't know its author, but I was assured by its recipient that it was legitimately from someone who was visiting there at the time. One member of the string theory group at Harvard is Lubos, and he has repeatedly defended the work of the Bogdanovs on his blog as legitimate science, no worse than much else of what is published in this field.

About Hagelin. Again, I wrote about him in the context of a chapter examining the difficulties involved in deciding what is science and what isn't. More specifically, how do you tell who's a crackpot and who isn't? There are plenty of people out there whose ideas about physics are uniformly incoherent and easy to dismiss, but there are also cases like Hagelin, who combines excellent research credentials with crackpot ideas about science. How do you decide who is a crackpot and who isn't? What about Lubos, what about Susskind? Many string theorists seem to hold the opinion that I'm one. Lacking the normal sort of discipline that comes from confrontation with experiment, a scientific field is in a very tricky state, and needs to be careful to enforce high standards of what makes sense and what doesn't, and not let pseudo-science take over. Aaron notes that most of the audience at the Toronto panel discussion voted against the anthropic landscape, but he doesn't mention that anthropism seemed to be a majority opinion amont the panelists, who are the ones who hold power. This is an extremely dangerous situation for this field. I don't think the possibility that some readers of my book are going to get the impression that most string theorists are not doing science is anywhere near as much of a problem as the fact that quite a few powerful ones definitely aren't anymore.

About comments on this blog. Please avoid adding to the noise level by posting non-substantive or off-topic comments, engaging in repetitive arguments that go nowhere, promoting your own ideas that have nothing to do with the posting, or generally making comments that have nothing new to say that hasn't been said many times here already.

Comments

1. **Jonathan Vos Post**
   August 19, 2006

I think that there is an ongoing subthread about comparisons, along several axes, of science, mathematics, and writing for mainstream (i.e. nonscientist) audiences. You yourself comment above: "… it ended up being published by a trade publisher wasn't my first choice, and the wide attention it is getting from people who know little about physics is a surprise to me…"

Similarly, the full-page attention of Time Magzine shows a wider audience than this blog might directly reach. I am NOT going off on my own experiences as a scientist/author here, but do think that there is a wider context to be considered with authors such as thos scientists,such as Fred Hoyle, Carl Sagan, Robert F. Forward, and Marvin Minsky, who became science fiction writers themselves.

The question that one must ask is: "Whom am I trying to reach? String Theorsist who mostly resent this message, but might conceivably to turned around to dissenters; ant-string theorists, with whom I'm preaching to the choir; or a wider audience of humans interested in science and its social impact, although not themselves practicing in the field?"

Perhaps you've already answered that question on this blog. If so, I apologize for missing it.

2. **Lubos Motl**
   August 19, 2006

   Peter, if you thought that such a book could be published by a university press, then you have really lost a contact with reality. Do you understand that the university presses are meant to publish serious scientific work that can be used for years instead of opinionated piles of emotional rubbish written by people who don't understand what they're talking about at the technical level and who are searching for the 15 minutes of their fame?

   Everyone who has learned how to work with the theory knows how to extract the content of papers about black holes, AdS/CFT, or anything else – and ignore "hype". Peter Woit only looks for unscientific themes that he can twist and use for his undemanding readers. He doesn't find much of them in the research papers, so he adds. 80% of this blog and the book is irrational obnoxious whining and 20% is stuff that is only added to create an illusion that his production is not pure whining. No one is interested in these 20% and the value of the 80% is just in the controversy that they create.

   Witten is a guru because he's the most achieved theoretical and mathematical physicist alive. But it is only Peter Woit who builds a religion on this. He argues that Witten was and is the right reason to accept string theory. Some people want to be led but it is ridiculous to say that the guru system is a rule. Witten also has (respectful) competitors and they're not the only ones who view science very differently. Many other people went through a similar development as Witten but independently.

   But even if people were led by Witten, that would be no disaster because Witten is rather bright. I think that many people could improve the quality of their opinions by a few orders of magnitude if they switched from their idiosyncratic rubbish to parroting of Edward Witten; most of these people who are subjects of the previous sentence are not string theorists. I don't like parroting but in the case of Peter Woit, this would be among my first recommendations. Imagine that he could become a spokesman for Witten instead of writing all this junk.

   The sections about the role of supersymmetry for evaluating the vacuum energy are, once again, completely wrong, much like the rest of the book as I described on 17 pages of errata.

   The main reason why I respect Profs. Bogdanovs from the University of Belgrade more than Peter Woit is their creativity combined with a desire to follow the standards of scientific discoveries, isntead of trying to revise science and cripple it by new kinds of social engineering and irrational emotional moods which is what Peter Woit and Lee Smolin openly want and systematically fight for.

   What the Bogdanov brothers have written arguably makes no full sense to any of us but their work proves that they have spent a lot of effort and time to learn the relevant things and they have rather original ideas. I have been impressed by them given their previous, seemingly unrelated profession. Their ideas about quantum groups suggest that they might really know them better than I do – and I've tested them to learn that their mastery of the group SO(4) and its non-compact forms dramatically exceeds Peter Woit's abilities in the same subfield. 😉

   It is sad that Peter Woit is so jealous that I rate him below the Bogdanov brothers, but this is simply how the reality looks like. They are also more achieved scientists than he is according to superficial social criteria. Guess whose contribution to science is negative and whose contribution to science is at least non-negative. 😉

   Also, if Peter Woit were willing and able to focus on the actual science, I am sure that the frequency with which my name would occur would definitely be lower than it is.

   Magueijo's book is similar crap as Woit's book.

   Woit's arguments that our no-go theorems about QFT behind gravity can fail because other theorems in the past have failed is cute but worthless until someone actually finds a loophole in which they can fail. But this is not how Woit would like science to look like. He prefers to falsify theories by collecting 50 angry crackpots who doom a theory without a glimpse of a rational scientific argument.

   I am sure that most of us including the Bogdanov brothers know that this is not how ideas and theories in physics can be rejected, which is why it is rather legitimate to count Peter Woit as a crackpot regardless of the fact that he would prefer, together with his brainless readers, to choose this title for Lenny Susskind, one of the most original physicists of our time.

3. **bob**
   August 20, 2006

   Thank you, Lubos, for that enlightening discussion.

   Peter, I think you have identified something important when you mention how difficult it is to distinguish between crackpots and non-crackpots. It's all too easy for a physicist to pretend that he knows nonsense when he sees it, and behave dismissively of whatever he doesn't understand. As long as he sees others behave dismissively of the same thing, he is probably safe, and this is where groupthink becomes amplified by immaturity. In an area where nobody understands anything, such as quantum gravity, the ones who behave dismissively at least appear to know something, and those who follow the dismissive ones become string theorists.

   But, as interesting as the sociology might be, there is still the practical question of how to distinguish crackpots from non-crackpots. John Baez's Crackpot Index, and the dozen or so other documents which claim to provide guidance on this subject are whimsical. They are dismissive and sneering, displaying immaturity and leaving the reader with the message that it is clear and obvious what is nonsense and what is not, and that those who cannot distinguish between nonsense and sense are the fools, deserving of ridicule.

   This behavior is no doubt very amusing for the children in the physics schoolyard, but there must be practical way of determining what is sense and what is nonsense, and it must never be to look at who is doing more ridiculing, John Baez or Lubos or whoever, and suppose that whatever they ridicule is nonsense. Whoever follows that strategy is only pretending to be a physicist, and is in fact too immature to think for themselves, but all ridicule online is performed as an act of theatre before these pretenders. So what is the alternative?

4. **Open Source**

August 20, 2006

Lubos says,

“Do you understand that the university presses are meant to publish serious scientific work that can be used for years instead of opinionated piles of emotional rubbish written by people who don’t understand what they’re talking about at the technical level and who are searching for the 15 minutes of their fame?”

Is that why Brian Greene’s books and Kaku’s books on String Theory are published by non-university presses?

Einstein and Bohr and Newton and Feynman and Dirac all exhibited vast humility because they were physicists who valued Truth above all else.

We ought to follow their lead.

http://revver.com/video/48391/21022

5. **bob**
August 20, 2006

And I think we need something more specific than a vague commitment to high standards and scientific integrity and other affirmations which are little more than promises to be good, and which carry little weight because in each case, the crackpot believes himself to have more integrity than his critics, and can claim with full honesty that he, as far as he understands, is making perfect sense.

What we need is an objective standard, which specifies exactly what criteria an informal argument must satisfy in order to be considered “valid”. Mathematical proofs already have clear standards, but physics has an overlap with philosophy (“at the fundamental level”) and has experimental data and its incrediby-poorly-agreed-upon notion of what constitutes a theory of physics. The objective standard for valid informal arguments must not be “No stupid pseudoscience”, which would be no better than simply declaring oneself to have high standards. The standard should include distinguishing premises from conclusions, and not appealing to any oracles, such as common sense, or what the community believes, in order to justify labelling something as a conclusion instead of a premise.

6. **anon**
August 20, 2006

‘There are plenty of people out there whose ideas about physics are uniformly incoherent and easy to dismiss, but there are also cases like Hagelin, who combines excellent research credentials with crackpot ideas about science. How do you decide who is a crackpot and who isn’t? What about Lubos, what about Susskind?’ -Woit

‘… Woit … prefers to falsify theories by collecting 50 angry crackpots who doom a theory without a glimpse of a rational scientific argument. I am sure that most of us including the Bogdanov brothers know that this is not how ideas and theories in physics can be rejected, which is why it is rather legitimate to count Peter Woit as a crackpot regardless of the fact that he would prefer, together with his brainless readers, to choose this title for Lenny Susskind, one of the most original physicists of our time.’ – Motl

I think Woit should try to find the time and the patience to explain gently and kindly to Motl that physics is ultimately based on facts, and stringy stuff isn’t. (I’ve tried, but lack sufficient tact to succeed.)

Stringy Bogdanov published a paper in peer-reviewed IoP Classical and Quantum Gravity, which later retracted its endorsement for the paper because it had no rational argument.

Bogdanov didn’t have a PhD, but was awarded one for getting his paper in CQG and virtual copy in another journal. He still has the PhD…

7. **Yatima**
August 20, 2006

Lubos Motl says: “Peter, if you thought that such a book could be published by a university press, then you have really lost a contact with reality. Do you understand that the university presses are meant to publish serious scientific work that can be used for years.”

My goodness. Such Vitriol. Counterexample? One swift grab into the bookstack behind my back produces “Superstrings: A Theory of Everything” by Cambridge University Press, first published 1988 (paperback, Canto edition, printed 1995), which is basically a set of Interviews made for BBC Radio 3. For ‘less scientific’ you would have to get one of the numerous hardcover books on consciousness studies by reputed UPs…

(The book has interviews with John Schwarz, Ed Witten, Michael Green, David Gross, John Ellis, Abdus Salam, Sheldon Glashow, Steven Weinberg and “old man” Feynman: “They are not checking the ideas hard enough against experiment because of the difficulty in caclulating anything. That means they are up in the air and I don’t have to pay much attention!”)

8. **Lubos Motl**
August 20, 2006

Dear Yatima,

the book “Superstrings…” is made of “mere” interviews, but they’re interviews with 8 super top physicists of that era. This is why the book has a scientific value as a book about history and sociology of science because it rigorously answers what the relevant people – heroes of physics – think at the time of publication.

Not Even Wrong is not a valuable book about sociology of science because it only captures bitterness of a particular nobody that won’t be interesting for scholars in the future in any way. Even today, he’s mostly interesting for similar nobodies, most of whom are anonymous like the confused “poster” above Yatima.

Best
Lubos

9. **boreds**
August 20, 2006

“The main reason I wrote about the Bogdanov story, (besides for its entertainment value), is that I think it shows conclusively that in quantum gravity in general, many people have lost the ability or willingness to recognize non-sense for what it is.”

I’m reiterating what AB said in his review, but I don’t think this is a justifiable statement. Who are the many theoretical physicists who don’t have the ability to recognise bogdanov for nonsense? Whoever reviewed the papers had presumably lost the *willingness* to recognise it, but in any case that’s still not `many’ people.

Don't think you are help your argument by conflating your other objections about string theory with this story!

10. **anon**
August 20, 2006

Dear Lubos,

your confusion about Feynman and stringy stuff was predicted and explained by Feynman:

'… I do feel strongly that this [string theory] is nonsense! … I think all this superstring stuff is crazy and is in the wrong direction. … I don't like it that they're not calculating anything. … why are the masses of the various particles such as quarks what they are? All these numbers … have no explanations in these string theories – absolutely none! …' – Feynman in Davies & Brown, 'Superstrings' 1988, at pages 194-195

Feynman said, in his 1964 Cornell lectures (broadcast on BBC2 in 1965 and published in his book Character of Physical Law, pp. 171-3):

'The inexperienced, and crackpots, and people like that, make guesses that are simple, but [with extensive knowledge of the actual facts rather than speculation] you can immediately see that they are wrong, so that does not count. … There will be a degeneration of ideas, just like the degeneration that great explorers feel is occurring when tourists begin moving in on a territory.'

Sheldon 'string theory has failed in its primary goal' Glashow – http://www.pbs.org/wgbh/nova/elegant/view-glashow.html

'Sheldon Glashow has strong opinions about string theory. Like how it has failed in its primary goal of incorporating gravity into the standard model of elementary particles. How its inability to be experimentally tested makes it ''permanently safe'' from either proof or falsification.'

Hope you are now less confused.

Kind regards,
anon.

11. **John Rogers**
August 20, 2006

As a infrequent reader, I am appalled by the discussion. There is one person (Woit) saying that there are no experimental predictions from string theory, and another (Motl) who says that there are two: black hole entropy and AdS/CFT. For any scientist, it is clear that neither of the two candidates are predictions about experiments. Why does Motl, who is clearly a bright guy, deny this?

The things which are open to prediction in physics are clear: the fine structure constant, the particle mass ratios, the other coupling strengths (and a few more). None of this is "predcitable" so far. The question is simple: will string theory achieve the predictions?

12. **not-a-fool-arogant**
August 20, 2006

Lubos,
I am sure PW will be remembered long time after you will be forgotten. If not for scientific achivements, then at least for saying loud and clear: the king is (most likely) nacked.

13. **bob**
August 20, 2006

boreds says:
Who are the many theoretical physicists who don't have the ability to recognise bogdanov for nonsense?

I remember the time. Before John Baez and Ark Jadzyck started interrogating the Bogdanovs. Nobody was willing to say that it was nonsense, because nobody was sure that it was, and anybody who loudly proclaimed in full view of the public that it was nonsense would soon be humiliated in full view of the public if it turned out not to be nonsense. And nobody was really sure, from just reading the papers, whether or not the Bogdanovs were thinking about anything coherent.

So, I would say that everybody who was aware of it at the time, was unsure about whether it was nonsense or not. If it wasn't nonsense, it was certainly written in such an awkward way that it was unintelligible perhaps to everybody but the authors. And where is the boundary between those who don't even know what they are saying, and those who are confused but think they have discovered something, and they may even be less confused than us, but still not right.

So, Mr. boreds, and Peter, the unfortunate situation is that you can't just stamp your feet and demand that everybody be immediately able to distinguish between nonsense and genuine science. You have to specify a unique objective procedure which we can always use to analyse something and then say whether it's nonsense. Perhaps you would claim that you know nonsense when you see it. Should we then bring everything to you, to ask you for help distinguishing sense from nonsense? What will you say of string theory, and, if it is nonsense, how will you convince the string theorists that your nonsense-detecting powers never fail? Will you reveal the procedure for identifying nonsense?

14. **Rickkkk**
August 20, 2006

I actually think Lubos is right about much of what he says in his first comment. Peter certainly isn't doing any science and shouldn't complain about the noise-level that accompanies discussing sociology and politics of science rather than actual science. Without having read the book I can assent that Peter resorts to sometimes emotional "salespitching" rather than hard facts.

However, I'd say that this is understandable, defensible, and certainly not something String Theorists are above. No, as Aaron says and Peter would surely admit, his and Lee's books are not even-handed, but when taken in the context of other popular science books(especially string-hypers) they do begin to introduce some even-handedness into the whole debate.

Popularizers of String Theory have seldom been even-handed in their description of the situation, and it's certainly due time for some uneven rebuttals, for balance if for nothing else.

As a footnote, pretending to have more dignity than an opponent will only assure your loss. If you're not willing to stoop to their level then you lose, in any competition. The non-stringers actually need cheerleaders, as the stringers have certainly brought theirs out. I'm considering becoming the non-stringer Lubos, I'm certainly more comfortable with the language and have had enough human contact to make sensical insults. For some reason, Idiom is the hardest thing to grasp when learning English and it shows when Lubos decides to foam at the mouth and invents horrible insults.

All the same, good “post” Lubos.

15. **Anon**
August 20, 2006

” I think Woit should try to find the time and the patience to explain gently and kindly to Motl that physics is ultimately based on facts, and stringy stuff isn’t. (I’ve tried, but lack sufficient tact to succeed.)

Stringy Bogdanov published a paper in peer-reviewed IoP Classical and Quantum Gravity, which later retracted its endorsement for the paper because it had no rational argument.

Bogdanov didn’t have a PhD, but was awarded one for getting his paper in CQG and virtual copy in another journal. He still has the PhD…”

First, G.Bogdanov passed his thesis in 1999 and the papers were only published in 2001/2002. It clearly invalids any causality link.

But what I am saying, though (as some mathematician did on WP) is that “a handful of blogs reactions and a smattering of fora publications do not mean that a rumor turns suddenly into a fact (which is the problem with wow-gee-whiz reporting). Nonetheless, it is wrong to report CQG’s first email as if it was pretty much the only factual one. No matter how many people like the dramatic, exciting story of the mythical Bogdanoff non sense, including lazy physicists, there is no hard or direct evidence of such a non sense”.

Regarding CQG, its so called “statement” was presented by Baez as an “official document” issued by CQG editorial board around november 1st 2002. However its real source has never been clearly establlished and it was never confirmed by CQG’s editorial board. Instead, on november 11,2002, CQG published an official statement whose content was indeed quite different.

Here is a link towards an article of the bulletin of the “Physics-Astronomy-Mathematics division of SLA” http://www.sla.org/division/dpam/pam-bulletin/vol30/no3/physics.html

One finds, in reference n° 13 of the references quoted by the author :
http://listserv.nd.edu/cgi-bin/wa?A1=ind0211&L=pamnet#11

One can see the original and official text issued by Andrew Wray and H. Nicolaï of CGQ on November 11 2002, in response to the charges of hoax :
http://listserv.nd.edu/cgi-bin/wa
A2=ind0211&L=pamnet&T=0&F=&S=&P=3647

As you can see, the authenticity of the so called “official statement” which circulated on SPR and elsewhere was never established . The official text that was clearly released in the public domain sounds quite different but was never known (because it was kept more or less under the carpet by some physicists whose interest was to promote the “negative” version of Nov.1 instead.

Here is the integral version of November 11. As you can see, it reads quite different from the previous version : http://listserv.nd.edu/cgi-bin/wa?A2=ind0211&L=pamnet&T=0&F=&S=&P=3647

“Date: Mon, 11 Nov 2002 10:38:46 +0000
Reply-To: andrew.wray@iop.org
Sender: “Archive of slapam-l (PAMnet)”
From: Andrew Wray
Subject: Classical and Quantum Gravity
Comments: To: SLAPAM-L@lists.yale.edu
Content-Type: text/plain; charset=”us-ascii”

I’m writing on behalf of the Institute of Physics in response to a recent discussion on this list re the following paper:

‘Topological field theory of the initial singularity of spacetime’ by G Bogdanov and I Bogdanov, Class. Quantum Grav. 18 4341-4372 (2001)

As you might expect, a number of our readers have contacted us about this and it has been widely discussed online.

Our position is this: Classical and Quantum Gravity endeavours to publish original research of the highest calibre on gravitational physics. It is one of the highest standard journals in its field and makes continuous effort to maintain and improve the quality of research communication. In common with many journals, we consult among a worldwide pool of over 1,000 referees asking two independent experts to review each paper. A third referee is selected if the first two disagree. 45% of submitted articles are rejected and almost all accepted articles are revised before publication. The paper ‘Topological field theory of the initial singularity of spacetime’ by G Bogdanov and I Bogdanov made it through this review process and was therefore published in the normal way.

At present, there are no plans to withdraw the article. Rather, the journal publishes refereed Comments and Replies by readers and authors as a means to comment on and correct mistakes in published material.

We have passed this information on to the community and ask that if your colleagues enquire about this, you forward this e-mail on to them.

Thank you for your help with this matter.

Regards,

Dr Andrew Wray

Senior Publisher
Classical and Quantum Gravity
Institute of Physics Publishing

Professor Hermann Nicolai

Honorary Editor
Classical and Quantum Gravity
Albert Einstein Institute

16. **Lubos Motl**
August 20, 2006

Dear anon,

I agree 100% with Feynman's comments about the crackpots, and I have written the very same things many times. See e.g.

http://motls.blogspot.com/2005/08/common-crackpots-errors.html

where the section about the "Inability to falsify a conjecture by a comparison with the most elementary data" is closest to Feynman's comments.

Concerning Feynman's misunderstanding of string theory, he was just too old and others did the same errors when they were old – like Einstein with quantum mechanics.

Feynman, however, exceeded all these other guys because he already realized that the reason why he was saying such a nonsense was that he was already too old and a bit senile and slow.

Glashow is a great guy and I certainly share very many points how physics should be approached with him (together with the date of birth that we also share with Heisenberg) but what he has been mostly saying about string theory is, while charming, technically wrong, too.

This is a typical example of the breathtaking hypocricy and inconsistency of the "Not Even Wrong" community. Peter Woit criticizes others for being members of a religious cult led by a guru – but his readers and sometimes Woit himself permanently flood the internet with sociological pseudoarguments based on some irrational quotes of well-known physicists.

It does not matter that they're famous. What they're saying is nonsense as everyone who knows these things at the technical level can check which is why these pronouncements don't have much effect. But at least, they're famous and they have earned a lot of credit in the physics community that they can freely spend by saying bullshit about string theory. Glashow could write hundreds of Woitian articles against current physics and he would still have positive account balance. On the other hand, Peter Woit has no credit. Trash is the only thing he contributes.

Best
Lubos

17. **anon**
August 20, 2006

'Feynman, however, exceeded all these other guys because he already realized that the reason why he was saying such a nonsense was that he was already too old and a bit senile and slow.' – Lubos

Dear Lubos,

Feynman was still sensible enough the same year to expose (1) the o-ring failure cause of the Challenger disaster cause, and more to the point, (2) the role of GROUPTHINK in causing scientific tragedy when false hype about low risks occur, see http://www.ralentz.com/old/space/feynman-report.html

String tragedy is the same; wishful thinking and suppression of dissent on pseudo-scientific grounds (just as well NASA didn't reject Feynman as senile).

Bests,
anon.

18. **Jeremy**
August 20, 2006

How is what Glashow says wrong?

19. **TheGraduate**
August 20, 2006

I think it is fair to say that what is being discussed here is the sociology of science rather than science itself. However, it seems to me that this is precisely the domain of the issue that Peter wishes to address.

I think the issue can be summarized as whether fewer resources should be allocated to string theory or not. There is no way to address this question that removes the human element. I do not think the scientific method can be used to definitively settle such a question. We must therefore look to some other investigative method.

Individuals can be subject to biases but even if they were not, it is usually unreasonable in most contexts for one individual to allow another to decide for him or her simply because the decider claims to be more knowledgeable. There should in most cases be good reasons to defer.

Usually, one asks for objective, real world criteria or neutral third parties and other resources of this nature.

Peter alleges that the string theory community is dysfunctional. The string theory community is free to ignore him. However, I think it is time to acknowledge that without a neutral third party or recourse to the sort of neutral and general criteria that could be applied to any field of science, it will be difficult to move forward.

I think for example the argument that very intelligent people support string theory is an OK argument for an individual to make when trying to convince himself but it is a horrible argument to use when arguing with a heterogenous group of scholars.

20. **Jimbo**
August 20, 2006

When it comes to Glashow, Lubos' comments are clearly, N.E.W.

If Feynman was "senile" in the few years prior to his death, I'll choose senility anyday, compared to LM's babbling rants, as string's principal cheerleader. One wonders if this time next year, when prelim evidence starts to accumulate from the LHC, if he'll be
nearly as vitriolic ?
Lenny's book was really a `shot across the bow' for theoretical physics & science, as it put all on notice that when it comes to the 3-centuries old scientific method, "We must all hang together, or else we shall all hang separately" – B.Franklin
There can be no capitulation to the stringers, without experimental predix followed by expt. proof.

21. **Aaron Bergman**
August 20, 2006

(Beware the Bogdanov sock puppets….)

Anyways, as for

> I remember the time. Before John Baez and Ark Jadzyck started interrogating the Bogdanovs. Nobody was willing to say that it was nonsense, because nobody was sure that it was, and anybody who loudly proclaimed in full view of the public that it was nonsense would soon be humiliated in full view of the public if it turned out not to be nonsense. And nobody was really sure, from just reading the papers, whether or not the Bogdanovs were thinking about anything coherent.

this just isn't true. I was willing to say it. Jacques was willing to say it. John Baez was willing to suggest that it was a hoax after reading it. This is all archived on the internet.

22. **Lubos Motl**
August 20, 2006

Dear anon,

sorry to say but looking at an O-ring, while impressive, is not enough to judge a theory at the Planck scale. It was a cute old Feynman but a different Feynman that one who discovered the Feynman diagrams, and he realized it very well.

If there is a great example of groupthink, then it is the groupthink – or more precisely group-non-think – of the community on this particular blog.

Anyone who says something that this mob doesn't like – i.e. everyone who says something that makes sense – is immediately under coherent fire of this uniform clan of people whose brains are turned off most of the time.

When I talk about Glashow saying nonsense about string theory, I primarily mean the silly comments that string theory is divorced from experiments.

http://www.pbs.org/wgbh/nova/elegant/view-glashow.html

Everyone who knows what string theory does in phenomenology knows that it is a nonsense. Buy the new phenomenology book of Michael Dine when it's out. You will see that string theory is the only framework to think about virtually all possible experimental observations in the future at a deeper level than the level "look, we see something": supersymmetry, axions, dark matter, details of grand unification, small black holes, and so forth, and so forth.

It is clear that many string theorists are more mathematically oriented, but it is a completely logical and correct approach in an era when we simply don't have too many new experiments that could directly lead us. I think that Glashow misunderstands this point, too.

String theory has been gaining importance exactly because it uses strategies that turned out to be most useful for progress in theoretical physics. Even if nothing else than the "details" about the possible braneworld scenarios, AdS/CFT correspondence, mirror symmetry, and a few other mathematical and phenomenological things were the only results in the last 15 years, string theory would clearly beat any other subfield of high-energy physics in this era.

Various discrete gravity people only achieved complete mess that is not interesting for anything. Pure phenomenologists didn't have almost any new ideas – at least no new good ideas that would be unrelated to string theory – either. String theory is the way to go because it gives us both deep mathematics as well as completely realistic new phenomena that are interesting and we would hardly discover them without the beacon of string theory.

That's why it's been naturally growing and it is bad if someone misunderstands how the evaluation of ideas work in the free market of ideas. All the Sean Carrolls and others who try to dictate how much mathematical reasoning vs. how much experimental dreams should be included in theorists' research show that they completely misunderstand that the theorists must use whatever is the best guide at a given moment, and there can't be any verse of the Bible that would define what the best path is forever. They just don't understand science.

Best
Lubos

23. **Lubos Motl**
August 20, 2006

Dear TheGraduate,

indeed, Peter Woit tries to address – and to cripple – the sociology of science. But he has no credentials to do something like that and the scientists generally don't think that there should be sociological committees that would manipulate with science in the way that Woit dreams about in his perverse dreams.

Woit's opinions are both completely flawed as well as unsupported by any scientific credentials, so there is indeed no reason why a serious publisher should print this kind of material as science or as social science.

Dear Aaron,

Baez's statement that Bogdanovs' papers were a hoax was a lie, and it was a very malicious lie. I would personally guess that the real reason why Baez wanted to damage the brothers was that they are more successful in many respects than he is. If the same paper were written by two anti-war janitors, I am sure that Baez would never criticize the authors.

I am still unconvinced that we are so sure that there is nothing interesting about their ideas. What all of you are showing is groupthink. All of you are heroes in saying that you know for sure that the work has no sense whatsoever – simply because you are hiding behind others and if all of you are wrong, the individual wrong people will be forgotten. The same groupthink as the groupthink of the left-wing blogosphere that is "sure" about a wide variety of things, for example that the differences between abilities of various groups have a social origin, not a biological one.

It is an irrational political movement. The anti-Bogdanov hysteria is another example of it. There are hundreds of papers a year submitted to the arXiv and journals that make as much sense as Bogdanovs' papers or less but they are not subjects of this hysteria. All of this hysteria is rotten. And Peter Woit is the last one who has the moral right to criticize the Bogdanov brothers because what he has written about science in the past we remember is much more transparent crap.

Best
Lubos

24. **woit**
August 20, 2006

Aaron is right about the Bogdanov history, many people (including myself) immediately upon reading their papers agreed that they were full of nonsense, and expressed this opinion in public forums. Especially anyone with any expertise in TQFTs could see that the statements the Bogdanovs were making about them were incorrect.

The alternate editor's note pointed out by one commenter is quite interesting. I would very much like to know what the true story is about this. There seem to be two very different versions of this note, the first one claiming there was a problem with the paper, the second denying this. Which is right? If anyone knows the true story here, please let me know.

bob,

Distinguishing sense from nonsense is sometimes easy, but often not. The mechanisms for doing this are well-understood: scholarship and rational discussion. In the case of the Bogdanovs, there are many places where scholars have rationally gone through what they wrote and explained what is wrong with it. John Baez especially did a careful job of this.

In the case of string theory, there's a disturbing level of refusal to engage in this process. The most extreme example around is Lubos, who chooses not scholarly debate, but ideological ranting unmoored from logic and evidence.

As for Lubos, I'm of two minds about his comments. On the one hand, they're perfect examples of the problems with how some string theorists are conducting research in their field. The Harvard string theory group anointed him as their choice for the best young person in the field, and, from all the evidence I have, continues to support him, so he's not some random person who has lost his marbles. On the other hand, he generates reams of idiocy, encouraging other people to respond to it, and this is likely to keep reasonable people from participating in the comment section here. It's also true that he has banned me from commenting on his blog.

So, I'm just not sure what to do…

25. **Chris Oakley**
August 20, 2006

Not directly relevant, but …
Science collides with a Big Bang, by Jonathan Leake.
This is an article in today's *Australian* about cosmology, focusing on disagreements between Neil Turok and Alan Guth. To quote the last paragraph:

> The academic world is often thought to be one of reasoned debate rather than vitriol.†
> What is driving the heated emotions? Peter Woit, an advanced maths lecturer at Columbia University, in New York, believes he has an explanation for the present fury: the physicists are simply getting bored.

————–
† Who thinks that? (No-one I know). What planet is *he* on? (Maybe one of the newly-classified ones).

26. **Aaron Bergman**
August 20, 2006

It's very easy to put forth vague ideas like "the signature of spacetime might fluctuate". To publish a paper, you need to actually calculate something or somehow put some scaffolding around the idea. The problem with the Bogdanov's papers is that almost every technical statement in their papers is either unoriginal or nonsensical. The papers should have never been published.

27. **woit**
August 20, 2006

Chris,

I talked to Leake on the phone the other day, and don't recall saying that this had anything to do with being "bored". What I did say was that scientists often behave less than rationally, but under usual circumstances, experimental results often adjudicate arguments among theorists. The lack of any experimental results relevant to string theory is one cause of the heated controversy there. I pretty much refused to comment on controversies in cosmology since I'm not especially well informed about them. If he'd replaced "bored" by "frustrated", it would have been a more accurate characterization of what I said to him.

28. **Santo D'Agostino**
August 20, 2006

One of Lubos's problems is with the English language. Consider his comment:

"Feynman, however, exceeded all these other guys because he already realized that the reason why he was saying such a nonsense was that he was already too old and a bit senile and slow."

There is a rather large difference between admitting that there is a chance, however small, that one could be wrong, and realizing that one is senile. Lubos does not seem to understand this difference; perhaps he does and is purposely misrepresenting, but I am willing to give him the benefit of the doubt. One hopes that once he becomes more familiar with English idioms he re-reads the Feynman interview with greater understanding.

The same misunderstanding of English, however, cannot forgive Lubos's posting of comments on the Not Even Wrong manuscript at the Amazon web site, and passing them off as a review of the book. (His later posting of a long list of errata on his own web site proved that the comments were indeed about the manuscript.) He mentioned in his "review" that his comments were about a "different edition," when there was only one edition available. Even someone whose first language is not English should realize that these actions are unscholarly. In this case they are also despicable.

29. **Chris Oakley**
August 20, 2006

Hi Peter,

My limited experience of journalists tells me that you were lucky that he wrote something that was even close to what you told him on the phone.
I was reassured when I tracked down an article about the elopement of my great^3 grandparents that the journalist in question had managed to mis-spell *both* their names.

BTW: Please don't ban Lubos. He's the best entertainment on the internet.

30. **David**
August 20, 2006

Dear Lubos,
You say often that ideas should stand or fall on the basis of evidence not on who proposed them. If you believe this, why do you attack people on the basis of credentials or what is, in your opinion, lack of scientific success rather than hard facts? BTW, speaking of hard facts, we are still waiting for your reply on your famous things that have been "more or less rigourously proved wrong" from your 17 page critique of NEW. Here's an opportunity for you to explain why the book is wrong. Further, statistics is a good subject if you're really interested in science. Check out the people part of the Stanford Stat Dept website for papers that provide some great reading.
David

31. **Lubos Motl**
August 20, 2006

Dear Aaron,

I tend to agree with you even though I don't think it is completely trivial to ask questions like "can the signature fluctuate?" I don't remember anyone else asked it before them. What's your answer, by the way? This is about causality in quantum gravity. Of course, the question is only meaningful if one believes that the metric tensor is a good variable even for fluctuations of order 100% which it probably isn't because you need the rest of string theory in this regime. 😉

Whether or not the papers are published depends on the referees but do you believe me that they were far from the only paper that could be labeled as unsuitable for publishing by many physicists? I am ready to list examples because it is just wrong if everyone thinks that it is desirable to attack Bogdanovs all the time while no one would dare to say something similar about similar papers.

Why is there so much hysteria about it? They were just trying to add another paper about very difficult questions that was expected not to break the mysteries given their being outsiders. They were not trying to argue that all of physics is wrong or something like that which is what some of their colleagues try.

Best
Lubos

32. **Aaron Bergman**
August 20, 2006

There are plenty of crap papers on the ArXiv as everyone knows (although I'd say that there's more outright nonsense in these papers than anything I've seen in a long time). The reason why the Bogdanov's got so much attention was mostly because of the initial rumor that it was an intentional hoax, but also because the papers got past the refereeing process.

33. **Rickkkk**
August 20, 2006

Peter, I think Leake was simply extrapolating that no experimental results leads to boredom. I think it's a correct statement, but perhaps not quite concise. Metaphorically speaking, the lack of experiments have allowed these arguments to flourish in a way that idle hands cause problems. It's subtle, but a still accurate metaphor.

34. **Chris Oakley**
August 20, 2006

> The reason why the Bogdanov's got so much attention was mostly because of the initial rumor that it was an intentional hoax, but also because the papers got past the refereeing process.

I think that you will find that their having a prime-time science TV show also had something to do with it.

35. **Peter Woit**
August 20, 2006

Santos and David,

It's bad enough having this comment section cluttered with people who want to discuss Lubos's current nonsense, please don't try and carry on older arguments with him here.

36. **anon**
August 20, 2006

Dear Lubos,

'You will see that string theory is the only framework to think about virtually all possible experimental observations in the future at a deeper level than the level "look, we see something": supersymmetry, axions, dark matter, details of grand unification, small black holes, and so forth, and so forth.'

Sure, stringy stuff is a good framework for planning sci-fi, but it isn't making unique checkable predictions that could falsify it when tested. The nearest you come is with the soft scattering spectra, but even if that is real, it could just have another cause. The best experimental checks on string theory will be open to other interpretations, because it is so vague. String theorists are careful to kick in new alternative ideas like Smolin's while they are still infants, before they can grow into a viable threat to stringy stuff.

Many claimed stringy predictions, such as large 0.1 mm-sized extra dimensions, are just not falsifiable. If the strings aren't found, maybe you just rule out that sub-version of string theory or else blame the sensitivity of the experimentalists. You dismiss such critics as Feynman and Glashow as senile or crackpot, while asserting uncheckable, speculative, empty frameworks instead of building on facts!

As for the Bogdanov's, the tale is that there were two brothers, one of which was failed his PhD examination and was told to get peer-reviewed stringy papers published before he received the degree. After publication, he was awarded the PhD. This story is almost as fantastic as string theory itself, so maybe it's just half-truths/lies. I've abused Woit's hospitality enough so had better end here.

Kind regards,
anon.

37. **Jeremy**

August 20, 2006

Peter,

Do nothing. I have been moderating online discussions for years. Getting rid of people who say something in opposition, something which is different, or even something which is "merely" hurtful, is rarely a solution.

From my outside perspective, Dr. Motl doesn't detract as much from discussion as much as you may think he does (except in not allowing you to speak on his own web site). While his comments could stand to be respectful, the dozen or so I've read so far tend to be on topic as regards the posts in which they respond to, though they all share the disrespectful quality. Also see one of my initial emails to you as regards a person's lifetime work being challenged.

As to the charge of being tendentious, I am only about halfway through the book right now, but I've read nary a statement so far which matches the word. There is certainly the strong point of physicists vis a vis mathematicians, and the title would indicate a large lean towards being against String Theory.

That a book about the point of view that resources, time and energy should be allocated more towards other endeavours is tendentious is, well, patently obvious. I don't think Mr. Bergman has made a case for your description of events and explanations being heavily slanted (I would certainly expect there to be some slant). I will, of course, reserve judgement in that respect until I have completed the book.

Dr. Motl,

Thank you for your response on Glashow.

38. **bob**
August 20, 2006

Peter said:
Distinguishing sense from nonsense is sometimes easy, but often not. The mechanisms for doing this are well-understood: scholarship and rational discussion.

Of course, but these are not objective criteria; they are just labels which anybody can apply to himself and deny to his enemies. Lubos can write what he writes and then say that it's scholarly and rational, and you can disagree and say that what you are saying is rational and that what he says is incoherent. Everybody can then take sides, insulting one another, and claiming that it is clear and easy for everyone to see which side is right, when both sides are lying about this. It is not easy; it is very difficult, and objective criteria are needed, and "scholarship and rational discussion" are very good indeed if you have specified a procedure for distinguishing between rational scholarly argument and incoherent nonsense.

But there is no such procedure, or at least nobody has specified one. So instead the situation is that an honest person looks at either side where the people say "The arguments of the other side are nonsense, and this is clear for everyone to see, and anyone who doesn't see it is stupid". Neither you nor Lubos can point to any objective standard which could be used to make the decision, though you will both claim that objective standards exist which your opponent fails to meet, but neither of you can specify them.

39. **Arun**
August 20, 2006

Aaron's review does address "the only game in town" and amends it to the "best game in town", and gives two cogent arguments from physics (blackholes and AdS/CFT) why it is interesting and also from the math. point of view. He does say he doesn't have any good idea as to where a better game might come from, and believes and hopes the next Einstein will make his breakthrough regardless of the system of academic research in place.

I do think the discussion needs to proceed from that point – can we make the "better game" less prone to accident? I certainly don't mean that good ideas can be produced on demand. But supposing some one does come up with a good idea, how do we make sure it doesn't perish unheard? It seems obvious to me that there are more hospitable and there are less hospitable environments for good new ideas; do we have have any way of improving the environment?

40. **David**
August 20, 2006

Sorry.

41. **Tony Smith**
August 20, 2006

Arun asked "… supposing some one does come up with a good idea, how do we make sure it doesn't perish unheard?
It seems obvious to me that there are more hospitable and there are less hospitable environments for good new ideas;
do we have have any way of improving the environment? …".

My ideas about that (which I have stated before in other comments and elsewhere, so my apologies for redundancy) are:

No consensus-monopoly view should be allowed to suppress alternative approaches.
A thousand flowers should bloom,
and all important institutions (university departments, laboratories, institutes, etc) should encourage active investigation of ALL the blossoms,
by rewarding grad students, post-docs, etc., for work on whatever they find interesting.
If a studied model turns out to be wrong, then the work showing it to be wrong should not be considered a worthless negative result, but a useful contribution (like weeding a garden) to advancing physics by cultivation,
and such negative results should be just as important as positive ones in getting publications, Ph.D.'s, post-doc jobs, and faculty appointments.

A big problem in implementing such an environment is that it would do away with closed good-ol-boy dominant-paradigm networks in which good-ol-boy A gives postdoc jobs, etc, to grad students etc of good-ol-boy B and all the good-ol-boys (and their grad students etc) always enjoy big barbecues in which all the HEP-theory pork is spread around among the Members of the Club.

Such Pork Clubs are very hard to get rid of in any human community, whether it be Congress or HEP or anything else. Even if a "Reformer" comes along and defeats the "Entrenched Machine", most of the time the "Reformer" just becomes a new "Emperor" (which is why Beethoven's Symphony 3 was not named for Napoleon).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Even Beethoven had difficulty getting great new ideas accepted by the establishment.
According to some CD liner notes by Constantin Floros:

"… When the news was brought to him that one of his [late string] quartets, played by Schuppanzigh, had met with a poor reception, he [Beethoven] said laconically: "One day it will please them." …".

42. **Peter Shor**
August 21, 2006

You say that

> Aaron notes that most of the audience at the Toronto panel discussion voted against the anthropic landscape, but he doesn't mention that anthropism seemed to be a majority opinion amont the panelists, who are the ones who hold power. This is an extremely dangerous situation for this field.

Is antropism the majority view among the string theorists that hold power, or just the loud ones? Because it seems to me that there are a few very loud, somewhat crackpot, string theorists, who have discovered that spouting lots of speculative nonsensical stuff brings them publicity. There are also a number of fairly sane string theorists who have relatively little to say right now because the field is only making very slow progress at the moment. And for some reason, the sane string theorists are too polite or too afraid to say anything negative about the loud ones. The sad part would be if, in the current climate, spouting loud, interesting-sounding, nonsense is a good way to advance ones career.

43. **Peter Woit**
August 21, 2006

Peter,

Besides certain loud ones (Susskind), the anthropic landscape is promoted by quite a few less loud but very prominent theorists. Examples include Harvard's Nima Arkani-Hamed, as well as Joe Polchinski, Michael Douglas, Shamit Kachru. I'm sure there are lots of theorists who are not happy with the anthropic landscape, but at this point I think most of them accept the idea of the landscape, if not the anthropic part. Most remarkably, the only string theorist I know of who seems willing to publicly criticize anthropism is David Gross.

44. **ksh95**
August 21, 2006

Bob commented:

> Neither you nor Lubos can point to any objective standard which could be used to make the decision, though you will both claim that objective standards exist which your opponent fails to meet, but neither of you can specify them.

The standard is and always has been clear: in the absense of experimental evidence the group that can accumulate the most power becomes correct.

45. **TheGraduate**
August 21, 2006

I have been reading this blog for a year and a half now. As someone who is considering grad school in a year, I guess I am probably part of the target audience of the recent books about string theory … in that, they have and continue to have an influence on the things I think about studying. They also have an influence on what I say to other people who are thinking about what field they want to go into.

I am mostly neutral at this point. I still think string theory might be enjoyable for purely mathematical reasons even if the physics thing doesn't work out but am not one for believing things that have no experimental foundation. My faith in the 'unreasonable effectiveness' of mathematics is not that strong.

Peter

I was wondering to what extent do you believe the 'cult-like' behavior had to do with external monetary pressures and public exposure. Are people unaware of possible deficiencies in their arguments or simply unwilling to admit this in public due to the loss of prestige, power etc? The former would be irrational while the latter is quite rational.

To my mind, the idea that theory needs to be supported by evidence seems pretty straightforward. Is there something about the culture of theoretical physics that I am missing?

I would also be curious to know whether people think that US national politics plays a role in this. I have noticed that for instance, at least in the case of Lubos, he is very concerned about 'anti-science' attitudes. Is this kind of concern common in the string theory community?

You see this kind of concern repeated often in national politics from contraversies with evolution, to AIDS, to global warming. Is string theory just part of a larger trend of rocky relationships between science and the public and issues concerning the nature of evidence?

46. **Shamit Kachru**
August 21, 2006

Hello:

Since I occasionally see responses by active scientists here, I am writing for their benefit:

You will see many views attributed to prominent particle and string theorists in this blog. (Or Motl's blog, or other blogs of similar ilk; there are now several). In many cases that I am aware of, these attributions range from outright lies (in more than one case, statements that I never made have been attributed to me), to vast oversimplifications or quotations taken out of context, on issues of significant complexity. In the blog discussions of the "landscape," I am sure from personal knowledge that many brief summaries of the views of leading theorists (put forth by active bloggers) have ranged from misleading to simply incorrect as representations of the person's actual opinion. Usually these incorrect summaries of the opinions of others, have served as launching points for polemics (which are a peculiar but common feature of several of the popular blogs).

Most of the subjects under active investigation in particle/string theory, are not best discussed in blog-level sound bites. This is not unique to particle/string theory, of course. So for any active scientists out there, don't think what you read here in any way represents the views and activities of your particle theorist colleagues: ask them directly yourselves, to get a better picture.

Since blogs can be a tremendous time sink, most of us (certainly me) do not usually respond even to those threads that pretend to directly address the goals and merits or flaws of our work. For that reason I certainly won't make more appearances here; it just seemed possibly useful to inform (remind?) everyone of the obvious comments above.

Shamit Kachru

47. **John Baez**
August 21, 2006

Peter writes:

The alternate editor's note pointed out by one commenter is quite interesting. I would very much like to know what the true story is about this. There seem to be two very different versions of this note, the first one claiming there was a problem with the paper, the second denying this. Which is right? If anyone knows the true story here, please let me know.

Check out the Wikipedia article on the Bogdanov affair – and also the discussion page for that article.

According to the article, the editors of Classical and Quantum Gravity issued a note by email saying:

Regrettably, despite the best efforts, the refereeing process cannot be 100% effective. Thus the paper […] made it through the review process even though, in retrospect, it does not meet the standards expected of articles in this journal. The paper was discussed extensively at the annual Editorial Board meeting […] and there was general agreement that it should not have been published. Since then several steps have been taken to further improve the peer review process in order to improve the quality assessment on articles submitted to the journal and reduce the likelihood that this could happen again.

If you're curious about this emailed note, ask Greg Kuperberg.

According to Wikipedia,

Later, the editor-in-chief of the journal issued a slightly different statement on behalf of the Institute of Physics, which owns the journal, in which he insisted on the fact that their usual peer-review procedures had been followed, but no longer commented on the value of the paper. In particular the sentences "[…] it does not meet the standards expected of articles in this journal" and "The paper was discussed extensively at the annual Editorial Board meeting […] and there was general agreement that it should not have been published" were removed."

In the discussion page, you'll see that lots of mysterious people – or maybe just one or two, using lots of pseudonyms – tried to change this Wikipedia article. Just like the fellow who posted here, these people tried to downplay the original note. The folks at Wikipedia eventually blocked these changes, deciding they were caused by "sock puppets": the Bogdanov brothers in disguise. This seems likely, both because it's they did the same thing to Jacques Distler, and because: who else would bother?

So, Peter, I guess the Bogdanovs are reading your blog and posting to it. Congratulations!

48. **woit**
August 21, 2006

John,

I've been the recipient of fake e-mails from the Bogdanovs, so did check that the source of this comment wasn't the same as that of one of those. Looking at the Wikipedia discussion, I see that this has become much more complicated, presumably involving lots of different addresses. The source of this comment is a Paris internet connection, but I have no idea whether it's from the Bogdanovs or someone else.

The information in the comment is accurate as far as I can tell. Personally, I was unaware that CQG had changed it's statement from the one Kuperberg distributed, which acknowledged a failure in the refereeing system, to the later one, which did not acknowledge this. Actually, I find this kind of appalling.

49. **woit**
August 21, 2006

TheGraduate,

I don't think the controversy over string theory has anything to do with the political fights going on in the US over science. Lubos's description of any criticism of string theory as "anti-science" is just ridiculous, part of attempt to attack in over-the-top manner anyone who disagrees with him about the subject.

The political fights about science have to do with the Bush administration's embrace of the religious right and its attitudes, and physics pretty much has nothing to do with this. I don't think the religious right has any opinion one way or another about string theory, and the Bush administration has recently made physics research one of its top priorities for higher funding.

As to why string theorists so often refuse to acknowledge problems with the subject, I won't speculate about their motivations for doing this. However I will say that I think it is a big mistake, both for scientific reasons, as well as non-scientific ones. They're seriously damaging their own credibility and that of the field as a whole.

50. **woit**
August 21, 2006

About Shamit Kachru's comment,

I'd like to point out that I make a very serious effort here to be as accurate as possible, although I'm sure I don't always succeed. When discussing someone's views, if possible I try and quote their own words, completely and in context. In the case of the Landscape, I've at times been so appalled by what I see going on that I've expressed myself in excessive ways that were not wise.

Presumably he's responding to my characterization of him as a proponent of the anthropic landscape point of view, and suggesting this is less than accurate. This characterization is based on some of his talks, and of the following quote from his colleague Susskind (see page 351-2 of his book):

"At Stanford University – my home – there is pretty near unanimity on the issue, at least among the theoretical physicists: the Landscape exists. We need to become explorers and learn to navigate and to map it Shamit Kachru and Eva Silverstein, both in their early thirties, are two of the world's young leaders. Both are busy constructing the Landscape's mountains, valleys and ledges. Indeed, if I were to attribute to anybody the title of the Modern Rube Goldberg, it would be to Shamit. Don't get me wrong; I don't mean to say that he makes bad machines. On the contrary – Shamit has brilliantly used the complicated machine parts of String Theory better than anyone to design models of the Landscape. And the Anthropic Principle? It goes with the territory. It's part of the working assumption of all my close colleagues at Stanford, young and old."

Susskind presumably has extensively discussed this with Kachru. If his characterization of Kachru's views is inaccurate, Kachru's problem is not with blog postings like mine, but with a published book of his senior colleague. A blog might actually be an excellent place to set the record straight if Susskind's published version is incorrect.

Kachru is quite right that some of these issues are complicated and discussions of them have sometimes invoked oversimplified or incorrect descriptions of his views or those of other people. But I don't think he's right that this medium is inappropriate for serious discussions of these issues. He and anyone else who wants to carefully explain their views on the issues discussed here is strongly encouraged to do so. I'll happily post here anything of this kind that he or anyone else who feels similarly is willing to write.

51. **Peter Shor**
August 22, 2006

I am certainly drastically oversimplifying things here, but if I am to believe Shamit Kachru and Peter Woit, not only are the loud string theorists spouting nonsense, but some of them are also claiming that everybody agrees with their nonsense. And as I said,

> for some reason, the sane string theorists are too polite or too afraid to say anything negative about the loud ones.

52. **TheGraduate**
August 22, 2006

Dr Kachru:

In business, management and even law, there is often the issue of how to allocate resources to views proposed by different experts with sharply contrasting points of view.

Assuming that one is not going to read every bit of everyone's work before deciding, what does one rely on:

1. Credentials

2. A documented track record of successes

3. The relative amount of support for the differing points of view among those in the know

My only window on this contraversy are these blogs, the magazines and talking to other people that enjoy the occasional read of 'Scientific American' or 'New Scientist'.

My best guess is that to the casual, neutral observer whose first opinion on string theory is primarily derived from the Nova special and seeing Brian Greene at their school (as I definitely did) … I would say that for that sort of person, the recent contraversy about string theory seems pretty plausible.

I think to the general neutral party, the idea that one would need evidence to support a particular point of view is pretty uncontraversial and this line of reasoning would make sense to an engineer, a lawyer or even a banker.

I think it has become fashionable with the magazines to play up the contraversy as this is the sort of thing that sells magazines: Time, Scientific American etc

I would actually really appreciate if a few people from the string community seriously debated Dr Woit. From my observations of the blog, Dr Woit seems very earnest about allowing fair debate.

Normally, I would say this sort of debate is the internal business of the scientific field involved but at least since the Nova special, many issues of string theory have become public interest.

53. **DMS**
August 22, 2006

I too would hope Shamit Kachru would post a long detailed reply somewhere. A blog posting is an ideal place, in my opinion.

I would suggest that it is a HUGE mistake for prominent string theorists to ignore criticism from Peter Woit and others and dismissing opinions of outsiders as 'crackpots'. Maybe some get satisfaction by "High-Five-ing" insults of Peter and Lee by some string theorists, but it is not helping change the perception of string theory or string theorists among outsiders.

For all the criticism of string theory by Peter(sometimes unfair, I think), it is INFINITELY BETTER than what a prominent string theorist did by writing popular books on "Cosmic Landscape and the Illusion of Intelligent Design". It won't be long before the "illusion" part is dropped in the popular perception of the subject. Perceptions matter.

It does not help when patently false/exaggerated statements are made to promote the subject. Please stick to the remarkable results that have been of great interest to mathematicians; that is impressive by itself. And admit failure of string theory in uniquely predicting physics beyond the standard model(obviously, compatibility with anything coming out of the LHC is not the same as a prediction).

Ultimately, credibility of physicists, and ultimately science is at stake. Scientific thinking is under assault, but not from Peter. Look further AND closer….

54. **Juan R.**
August 22, 2006

Lubos Motl Said:

> Witten is a guru because he's the most achieved theoretical and mathematical physicist alive. But it is only Peter Woit who builds a religion on this.

Well, Sylvester James Gates, Jr. (the Jim Gates of the superstring site) states in a NOVA interview:

> String theory is often criticized as having had no experimental input or output, so the analogy to a religion has been noted by a number of people. In a sense that's right; it is kind of a church to which I belong. We have our own popes and House of Cardinals.

I agree with Jim Gates.

Juan R.

Center for CANONICAL |SCIENCE)

55. **anonymous**
August 22, 2006

Dear Peter,

I don’t know much about Shamit Kachru’s views on the issue, but I don’t see how the Susskind quote implies that he is a proponent of the anthropic approach.

56. **woit**
August 22, 2006

anonymous,

“the Anthropic Principle? It goes with the territory. It’s part of the working assumption of all my close colleagues at Stanford, young and old.”

the quote pretty explicitly claims that the Anthropic Principle is Kachru’s working assumption in his Landscape research. Presumably he’s a proponent of his own research and its assumptions. Undoubtedly, as he says, his views about this are more complicated. But I wasn’t purporting to give an accurate and full description of his views, just listing him among others who are propounding a research program that invokes the Anthropic Principle, and Susskind clearly identifies him as part of this group.

57. **TheGraduate**
August 22, 2006

Most of responses I have so far heard to Peter’s criticisms by people who state that they are string theorists are often angry and of the “I will not dignify this with a response” variety.

I had gradually been coming to the conclusion that Dr. Motl is some kind of crazy person. As far as I can tell. I have observed him having fights with several people on string theory and several other things. I am astounded that he teaches at Harvard. I assume he is tremendously knowledgeable on what he has been hired to teach about. However, he seems very bad at everyday causal reasoning … in my opinion.

I was also disappointed in the episode where Susskind says “There’s another fellow who has his own theory, I won’t tell you who his name is or what his theory is, but he writes lots and lots of theories and his theories go glub, glub, glub to the bottom of the sea before he even gets a chance to put them out there.” … with consequent fall in my opinion for both him and Stanford … Anyway, I digress.

I am somewhat curious about whether this sort of disdain extends not just to the ‘stupid’ physicists but if it also extends to the ‘stupid’ public. After all, I assume the point of the many attempts to popularize the subject was to bring the public into their otherwise insular world. And of course, quite a few of their endeavours require public good will and support and most importantly public money.

58. **Chris Oakley**
August 22, 2006

> I had gradually been coming to the conclusion that Dr. Motl is some kind of crazy person.

Just as a matter of interest … how long has it taken you to get to this realisation?

59. **J.F. Moore**
August 22, 2006

Although I’ve been looking forward to the American release of your book, Peter, I must admit I was worried that it might bog down on the details at the expense of giving a nice sweeping view for the lay reader (or a scientist in a totally different field like me). Your comments indicate that I have nothing to worry about and make me anticipate it even more. Also, I think it is great that a trade firm that might push hard (who knows?) to the mass market is publishing your book. I look forward to seeing you on Oprah as the monthly book selectee (joking).

60. **Peter Woit**
August 22, 2006

J.F. Moore,

General advice to readers of the book is that there are certain sections, especially in the earlier chapters, that are aimed at a more specialized audience, but the book is written so that if you skip ahead you should still be able to follow what is going on.

By the way, I just got a big carton of books delivered to my office, and it looks to me like the thing is available for sale on Amazon.

61. **tweet-tweet**
August 22, 2006

Lubos,

You’ve published your own criticism of loop quantum gravity, on wiki as “objections to loop quantum gravity” for which lee smolin and others have written their rebuttals (you called tweet-tweet’s facted-based responses to your list of objections “vandalism” on wiki)

so how is Peter’s book any different from what you did for wiki? Esp since popular books on string theory, such as michio kaku’s and briane greene’s, make almost no mention of the problems string theory has? as a scientific emprical theory, string theory does have problems you must admit.

you strongly object to lqg as doing science, yet you object to people who object to string theory as science.

62. **Who**
August 22, 2006

Hello tweet,
It was also my impression that the original “objections to loop quantum gravity” in the wiki article contained inaccurate assertions, thanks for trying to correct them! I believe that section was eventually removed from the wiki LQG article, was it not?

To get back to the Aaron Bergman review, I think it has misleading statements on page 5 which could be corrected. This is a key section where he presents a justification for continued string research, perhaps THE most often used justification—that requires giving a false impression of progress achieved in non-string QG, so as to make the “game in town” point.

I will quote the review and bold some places where I think what Aaron says is questionable, as may be seen by consulting recent papers on arxiv such as

http://arxiv.org/abs/gr-qc/0604044
Graviton propagator in loop quantum gravity
http://arxiv.org/abs/gr-qc/0604016
Hidden Quantum Gravity in 3d Feynman diagrams

"One exception is the collection of ideas generally termed Loop Quantum Gravity (LQG)… It is a radically new class of theories that has as yet been **unable to make any contact with the major results of the usual style of quantum theory**. In contrast to string theory, the theory of quantum gravity so produced has **not been able to demonstrate even the attraction known to Newton** hundreds of years ago… "

thanks.

63. **Anon**
August 22, 2006

Having read both of those papers, I think Bergman's statement remains (for the present) completely accurate.

But I guess a critical discussion of those papers would be "off-topic" as far as Peter is concerned.

64. **Peter Woit**
August 22, 2006

Anon,

Yes, please, I don't think any light would be shed here by going over that same argument again. It is certainly worth pointing out though that many people working on LQG would disagree with Aaron's characterization of the subject. But another venue moderated by someone else would be the place to discuss this.

65. **YBM**
August 23, 2006

Peter, could you post here (or send me by e-mail) the IP address of this likely sock puppet ? I could easily checked if it's Igor/Grichka or not.

BTW, It is clear from Lubos' comment that he didn't even try to check the actual content of the Bogdanov's papers, neither did he checked all the distateful parts of this affair (falsifications, sock puppets, treats, misquotes – even he is one of their victim), he merely instrumentalizes this affair against you because you happen to have pointed out their blunders. This conduct of him backfired on his face. I do not think that someone able to support crackpots in order to attack one of his collegue deserve any respect.

66. **Peter Woit**
August 23, 2006

YBM,

Thanks for looking into this, it does appear that this comment came from one of the Bogdanovs. Thanks also for the news of their forthcoming book: Voyage vers l'Instant Zero.

http://ybmessager.free.fr/epiphysique/forums/phpBB2/viewtopic.php?t=183

67. **YBM**
August 23, 2006

In case some of this blog readers would like to comment on the specific issue of the Bogdanovs affair, which is quite off-topic here : the forum accepts posts in english.

68. **tweet-tweet**
August 23, 2006

hello who,
yes lubos article is not in wiki, but is in the lqg talkback. it does not pass wiki's npov.

anon, perhaps the physics forum is the place to discuss the two papers?

69. **anonymous**
August 23, 2006

Dear Peter,

You say,

—
"the Anthropic Principle? It goes with the territory. It's part of the working assumption of all my close colleagues at Stanford, young and old."

the quote pretty explicitly claims that the Anthropic Principle is Kachru's working assumption in his Landscape research.
—

I still don't think it is fair to base the claim that Kachru promotes the anthropic approach on this quote by Susskind, so I guess we just disagree on this issue.

70. **YBM**
August 24, 2006

Motl> "Profs. Bogdanovs from the University of Belgrade…"

1. The Bogdanovs brothers have never been professors
2. There is no link between them and the University of Belgrade, they got their Ph. D. from University of *Bourgogne*, in France (Dijon)
3. The Bogdanovs have never been members of any laboratory or university since they've got their Ph. D (the lab they've been in Bourgogne has even been dismissed right after that !)

Motl really took time to examine what he’s talking about. Anyway he’ve admitted having the same views on science as a whole as the B. Does he mean that misquoting people, forging thesis reports and falsifying documents are standard behaviour ?

Some Links

August 20, 2006
Categories: Uncategorized

Lee Smolin's forthcoming book "The Trouble With Physics" has a web-site.

There's a new group blog focusing on n-categories, The n-Category Cafe, which will be run by John Baez, David Corfield and Urs Schreiber. It looks like Urs will basically be moving operations from The String Theory Coffee Table to this new blog.

The August issue of Symmetry is out. Lots and lots of articles about the LHC.

For the past week and a half Fermilab has been hosting a summer school on physics at Hadron Colliders. The talks are available here, and many are quite interesting. For example, history buffs should look at the talks on the top discovery by Tollefson and Varnes, and there's a nice survey talk by Chris Hill in which he emphasizes the role of symmetries. Hill notes that unification of couplings in the MSSM doesn't quite work, off by 3 sigma in the prediction of the strong coupling constant. He describes supersymmetry as "our best operational hypothesis" but believes that "It (probably) won't be the MSSM!!!".

The Telegraph seems to have tracked down Perelman and has an article about him entitled World's top maths genius jobless and living with mother. It claims that Perelman is not going to the ICM, where it is assumed he will be awarded the Fields medal, because he can't afford the trip. It also claims that in 2003 he was not re-elected to the Steklov institute and forced to leave. I find lots of things in the article hard to believe, remarkable if they're true.

The rumor is that this week's New Yorker, on the newstands tomorrow, will have a long article by Sylvia Nasar (author of the Nash biography, "A Beautiful Mind") about the Poincare Conjecture and Perelman's proof.

Last month the IHES held a conference on motives. Many lectures and references are now available here.

The IHES web-site also has a preprint of a new survey article by Pierre Cartier entitled A primer on Hopf algebras.

This summer's Park City program was on the topic of Low Dimensional Topology. Some lecture notes are available here. These include notes taken by Gabriel Drummond-Cole, who also has lots of other notes from interesting talks and lecture courses.

Update: The New Yorker article, by Sylvia Nasar and David Gruber, is called "Manifold Destiny" and is in this week's issue, but not available on-line.

The ICM is starting tomorrow, with video of talks available here. There seem to be four lecture slots scheduled for lectures by Fields Medalists, I'm deeply embarassed that I still haven't heard reliable rumors about who they all are. There have been solid rumors identifying Tao and Perelman, of the less solid ones retailed here, Bhargava sounds to me the most plausible. I guess we'll know soon….

Update: The New Yorker article is now available on-line.

Comments

1. **anonymous**
   August 20, 2006

   Some of the morning lectures at the IHES school have been recorded on video, and that includes the lectures of Kontsevich. Perhaps one could get hold of them if there's an interest.

2. **Who**
   August 20, 2006

   wasn't the title "A Beautiful Mind"?

3. **QWERTY**
   August 20, 2006

   IT SOUNDS LIKE PERELMAN DOES WANTS TO GET THE FIELDS MEDAL. SURELY THE ICM WILL PAY FOR THE TRAVEL EXPENSES OF FIELDS MEDALISTS? I HOPE HE DOES NOT GO CRAZY LIKE GROTHENDIECK

4. **An interested reader**
   August 20, 2006

   Yes it was called a beautiful mind.

   I enjoyed your book, now I want to enjoy the controversy: wouldn't it be a good idea to group the links to all the reviews of Not Even Wrong that you know of, to help interested readers?

5. **woit**
   August 20, 2006

   Thanks Who, I thought there was something wrong with that when I wrote it, but was in a hurry. Fixed now.

   Interested reader,

   Putting together a page with links to all the reviews I know about is on my to do list, should get to it soon.

6. **Chris W.**
   August 20, 2006

   The book: *A Beautiful Mind* (ISBN 0743224574)

   The New Yorker's table of contents for 8/21 shows no sign of an article by Sylvia Nasar.

7. **woit**
August 21, 2006

Chris W.

That's last week's issue, this week's will have a cover date of 8/28.

8. **Mnev**
August 21, 2006

Yes, Sylvia Nasar was here in SPb in june.

9. **Who**
August 21, 2006

Yes, Sylvia Nasar was here in SPb in june.

wow. excuse my naive enthusiasm. She was already onto the story, the editors at NYorker had probably already OK'd the idea and she was already in Saint Petersburg doing the research. In June.

Let's hope for the best, as a piece of writing.

Mnev congratulations on being a fellowcitizen of so many outstanding poets and mathematicians, men and women alike. SPb = great town

10. **Mnev**
August 21, 2006

Thanks. I agree. Nobody knows strings here.

11. **Who**
August 21, 2006

Nobody knows strings here.

This is fortunate.
They have the chance to invent something quite different.

12. **Chris Oakley**
August 21, 2006

Review of anti-science books in *Scientific American*:

http://www.sciam.com/article.cfm?chanID=sa006&articleID=000713DC-8161-14E3-BAEC83414B7F0000&colID=12

13. **Who**
August 21, 2006

The anti-science writer who reviewed the anti-science books is George Johnson who has a picture of his gravestone
http://sciwrite.org/glj/gravestone.jpg
posted on his website
http://sciwrite.org/glj/

thx Chris

14. **TheGraduate**
August 21, 2006

The review in Scientific American says "10500 perfectly good M theories" which I immediately thought was a manageable number but It's actually 10^500 right?

Also, the article in the New Yorker seems to be called "Manifold Destiny: Who really solved the Poncairé Conjecture?" by Sylvia Nasar and David Gruber.

15. **ObsessiveMathsFreak**
August 21, 2006

> The Telegraph seems to have tracked down Perelman and has an article about him entitled World's top maths genius jobless and living with mother.

Once again, Perelman has raised the bar for mathematicians everywhere.

16. **Who**
August 21, 2006

[b]Once again, Perelman has raised the bar for mathematicians everywhere.[/b]
This is without question the silliest thing that has ever been said in Peter Woit's blog since its inception. We must be careful!
Let us make a serious effort to get on topic, whatever the topic is, so that Peter does not shut down the thread!

17. **DMS**
August 21, 2006

Actually, [Erdos](#)was also remarkably similar to Perelman in one sense; he had no money or interest in material wealth. Erdos had no home(lived out of a suitcase) or money, and survived on the goodwill of fellow mathematicians(particularly R Graham). He was also rather uninterested in fame. He should have received the Fields with Selberg or the elementary proof of the Prime Number theorem(among many other things).

But then again, history will view Erdos as being a far greater mathematician than Selberg…

I hope the mathematical community makes sure that Perelman has money and equipment(i.e, pen, paper, laptop, internet access) to support himself and his mathematical work.

18. **anon**
August 21, 2006

Sabine has confirmed the first application of string at the Backreaction blog:

http://backreaction.blogspot.com/2006/08/bumpy-white-string.html

I apologise to Lubos Motl. String does inspire useful technological innovations, despite being useless in science.

19. **Jonathan Vos Post**
August 21, 2006

For the record, John Forbes Nash, Jr., is still doing novel and interesting research. I spoke with him repeatedly and at length at the 6th International Conference on Complex Systems, and chaired two sessions that he attended.

As to the audience reaction, and subsequent press coverage of ad hominem attacks in fundamental physics, see:

Science collides with a Big Bang
An argument is raging between physicists on how the universe began, writes Jonathan Leake

20. **Chris W.**
August 21, 2006

Nasar and Gruber's article is now in the TOC for the print magazine, but is not available online unfortunately.

21. **MathPhys**
August 22, 2006

Perelman and Tao are Field medalists, but who are the other two?

22. **Thomas Larsson**
August 22, 2006

[Hill] describes supersymmetry as "our best operational hypothesis" but believes that "It (probably) won't be the MSSM!!!".

Does this mean that there is growing consensus that the MSSM is wrong? If so, what does it mean for SUSY as a solution to the hierarchy problem, which was the main reason for believing in SUSY in the first place?

23. **Jonathan Vos Post**
August 22, 2006

Prof. Manjul Bhargava

Ph.D. Princeton University 2001
Dissertation: Higher Composition Laws
Advisor: Andrew Wiles

Manjul Bhargava: An Artist of Music and Math

Music, Math… "One of my favorite activities is listening to Indian classical music, a most wonderful and multidimensional form of music which unfortunately still has not fully yet made its way to the West. I also enjoy playing the tabla, the most common of the Indian percussion instruments…"

What a relief. Imagine if he played stringed instruments, theoretically…

24. **ICM News**
August 22, 2006

The 2006 Fields Medalists are:

1) Andrei Okounkov (Princeton University),
CITATION: "For his contributions bridging probability, representation theory and algebraic geometry",

2) Grigori Perelman (formerly of Steklov Institute of Mathematics, St. Petersburg)
CITATION: "For his contributions to geometry and his revolutionary insights into the analytical and geometric structure of the Ricci flow",

3) Terence Tao (University of California, Los Angeles),
CITATION: "For his contributions to partial differential equations, combinatorics, harmonic analysis and additive number theory", and

4) Wendelin Werner (Université de Paris-Sud, Orsay),
CITATION: "For his contributions to the development of stochastic Loewner evolution, the geometry of two-dimensional Brownian motion, and conformal field theory".

2006 Nevanlinna Prize goes to Jon Kleinberg (Cornell University).

The first awardee of the Gauss Prize is Kiyoshi Itô (formerly of University of Kyoto, now retired).

Well done all!

25. **Jonathan Vos Post**
August 22, 2006

Four Are Given Highest Honor in Mathematics
Courtesy of International Congress for Mathematicians

http://www.nytimes.com/2006/08/22/science/22cnd-math.html?ex=1313899200&en=897c1543a33626bc&ei=5088&partner=rssnyt&emc=rss

Terence Tao, a native of Australia, is one of the youngest Fields Medal winners ever at age 31.

Grigory Perelman is the most prominent of the medalists, not only because the Poincaré conjecture had ranked among the most heralded unsolved math problems, but also because of his reclusive personality.
Courtesy of International Congress for Mathematicians

Wendelin Werner, top, works on problems at the intersection of mathematics and physics. Andrei Okounkov, bottom, was honored "for his contributions bridging probability, representation theory and algebraic geometry."

But Dr. Perelman refused to accept the medal, as he has other honors, and he did not attend the ceremonies at the International Congress of Mathematicians in Madrid.

Sir John Ball, president of the International Mathematical Union, which is holding the conference, told The Associated Press that he did not think Dr. Perelman's decision to turn down the award was intended as a snub. "I am sure he did not mean it that way," he said.

The Fields Medal, often described as mathematics' equivalent to the Nobel Prize, is given every four years, and several can be awarded at once. Three other professors of mathematics were awarded Fields Medals this year: Andrei Okounkov of Princeton; Terence Tao of University of California, Los Angeles; and Wendelin Werner of the University of Paris-Sud in Orsay.

Dr. Perelman, 40, is known not only for his work on the Poincaré conjecture, among the most heralded unsolved math problems, but also because he has declined previous mathematical prizes and has turned down job offers from Princeton, Stanford and other universities. He has said he wants no part of $1 million that the Clay Mathematics Institute in Cambridge, Mass. has offered for the first published proof of the conjecture.

Beginning in 2002, Dr. Perelman, then at the Steklov Institute of Mathematics of the Russian Academy of Sciences in St. Petersburg, published a series of papers on the Internet and gave lectures at several American universities describing how he had overcome a roadblock in the proof of the Poincaré conjecture.

The conjecture, devised by Henri Poincaré in 1904, essentially says that the only shape that has no holes and fits within a finite space is a sphere. That is certainly true looking at two-dimensional surfaces in the everyday three-dimensional world, but the conjecture says the same is true for three-dimensional surfaces embedded in four dimensions.

Dr. Perelman solved a difficult problem that other mathematicians had encountered when trying to prove the conjecture, using a technique called Ricci flow that smoothes out bumps in a surface and transforms it into a simpler form.

Dr. Okounkov, born in 1969 in Moscow, was recognized for work that tied together different fields of mathematics that had seemed unrelated. "This is the striking feature of Okounkovs's work, finding unexpected links," said Enrico Arbarello, a professor of geometry at the University of Rome in Italy.

Dr. Okounkov's work has found use in describing the changing surfaces of melting crystals. The boundary between melted and non-melted is created randomly, but the random process inevitably produces a border in the shape of a heart.

Dr. Tao, a native of Australia and one of the youngest Fields Medal winners ever at age 31, has worked in several different fields, producing significant advances in the understanding of prime numbers, techniques that might lead to simplifying the equations of Einstein's theory of general relativity and the equations of quantum mechanics that describe how light bounces around in a fiber optic cable.

Dr. Werner, born in Germany in 1968, has also worked at the intersection of mathematics and physics, describing phenomena like percolation and shapes produced by the random paths of Brownian motion.

The medal was conceived by John Charles Fields, a Canadian mathematician, "in recognition of work already done and as an encouragement for further achievements on the part of the recipient."

Since 1936, when the medal was first awarded, judges have interpreted the terms of Dr. Fields's trust fund to mean that the award should usually be limited to mathematicians 40 years old or younger.

26. **ldm**
    August 22, 2006

    "Jobless and living with mother" …. the non Russian journalist (Nadejda probably is innocent) is not really giving us an accurate picture here.

    To put this in perspective for the western readers…

    Living with parents in the Former Soviet Union (FSU) is fairly normal and even if he had a regular job, living at home would still not be a big deal.

    Second…After Perestroika, the government gave the citzens their apartments (some younger FSU citizens are not aware of this fact, it would seem) …so it is very possible his mother has no mortage expense. Walking or taking the subway (metro) is normal mode of transportation, so no car payment either…Medical care is very good, and inexpensive — so no medical bills to speak of as in the west.

    Granted the Russian pension is not much, but the economics is not quite the same as in the west.

    Therefore, for Perelman to live at home, with his mother, does not in any way compare, or have the same stigma, as a similar situation in the west.

27. **TheGraduate**
    August 23, 2006

    I just read the article in the New Yorker. Who knew the situation was this dramatic?

    The most important idea in the article seems to be the argument that Dr Shing-Tung Yau of Calabi-Yau fame is stealing people's credit. I don't know if that is true but that seems to be the definitive verdict of the article.

    In case, the implications of the article were too subtle, they have supplemented with a full page graphic of Yau stealing the Fields Medal from Perelman's chest.

    It is a well written essay. I do not know in what repects Gruber contributed but I enjoy Nasar's writing. I can not say whether the reporting is accurate or not. I did think in passing that they had not defined a manifold but I assume most mathematical readers would be familiar with this term.

    Perelman is definitely portrayed in a positive light: more or less normal except for being extremely hard working, honest, open and humble and of course, mathematically brilliant.

The details about Yau seem almost tangential. The angle of Yau seems to have been inserted by the author's themselves rather than emerging naturally from the interview with Perelman. Perelman himself, as represented in the text, expresses indifference to what Yau may or may not have done.

28. **gunpowder&noodles**
August 24, 2006

"The most important idea in the article seems to be the argument that Dr Shing-Tung Yau of Calabi-Yau fame is stealing people's credit. "

What a shocking revelation. I'm sure nobody ever dreamed of such a thing before. STY's reputation for being self-effacing is famous throughout the length and breadth of Nagorno-Karabakh.

29. **GeomGeek**
August 24, 2006

"The details about Yau seem almost tangential. The angle of Yau seems to have been inserted by the author's themselves rather than emerging naturally from the interview with Perelman. Perelman himself, as represented in the text, expresses indifference to what Yau may or may not have done."

sure, especially the bit about Yau giving three days to the editors of the AJM to comment on the Cao-Zhu paper without showing the paper

not to mention the story with the 105%

30. **TheGraduate**
August 24, 2006

There is also an article in Notices that I had not seen before but it touches on similar issues to the New Yorker article .

"Conjectures No More? Consensus Forming on the Proof of the Poincaré and Geometrization Conjectures" by Allyn Jackson

http://www.google.com/search?hl=en&lr=&q=perelman+notices&btnG=Search
(It's the first link.)

It says in part "Some of the news articles were translated into English and posted on the Web. In those articles, the achievements of Cao and Zhu, both of whom are Chinese, are emphasized, while the achievements of Perelman are mentioned in a less prominent way. In one story from the Xinhua news agency, which appeared on June 21, 2006, the name of Perelman does not even appear."

It also says that "Yau said that he was misquoted in some of the media accounts and does not endorse what is said there."

They also provide a link to a website to some slides purportedly used by Yau in a lecture. They don't say what they want you to look at but I found the following quote.

"In Perelman's work, many key ideas of the proofs are sketched or outlined, but complete details of the proofs are often missing. The recent paper of Cao-Zhu which was submitted to The Asian Journal of Mathematics in 2005 gives the first complete and detailed account of the proof of the Poincaré conjecture and the gemetriziation [sic] conjecture. They substituted several arguments of Perelman by new approaches based on their studies." — Yau

31. **MathChina**
August 25, 2006

This email has been sent to Ms. Nasar and the Now Yorker.
———————————————————————

Dear Ms. Nasar:

As a mathematician who was born in China, I am deeply offended by your article with Mr. Gruber in the recent issue of the New Yorker. Your narrative plays to the stereotype that mathematicians of Chinese heritage are "technical" but not "original".

(1) In the first sentence of your article, you identified Professor Yau as "the Chinese mathematician". In fact: Yau is a U. S. citizen. To make my point simple: Don't you agree that it oddly emphasizes your cultural heritage if you are called "the Bavarian journalist and
writer"? (I noticed that you were listed as an American journalist and writer born in Bavaria in the Wikipedia.)

(2) In your narrative of the works that led to Yau's Fields medal, you misrepresented the facts and downplayed the originality of Yau's contribution," 'He was not so much thinking up some original way of looking at a subject but solving extremely hard technical problems that at the time only he could solve, by sheer intellect and force of will,' Phillip Griffiths, a geometer and a former director of the Institute for Advanced Study, said".

(3) Even in your narrative of Chern, there is no mention of any of his original work or idea.

(4) In Yau's seminar, "[e]ach student was assigned a recently published proof and asked to reconstruct it, fixing any errors and filling in gaps". As for the controversy surrounding
Givental's work, "[o]ccasionally, the difference between a mathematical gap and a gap in exposition can be hard to discern. On at least one occasion, Yau and his students [Bong
Lian and Kefeng Liu] have seemed to confuse the two, making claims of originality that other mathematicians
believe are unwarranted." You were adamant that Professor Givental's work is complete and correct. A simple search in MathSciNet's review of Givental's paper should have given you a different perspective.

(5) In your narrative of Professor Tian's reaction to Yau's allegations, you again emphasized Tian's Chinese heritage: "I [Tian] have deep roots in Chinese culture. A teacher is a teacher. There is respect. It is very hard for me to think of anything to do."

(6) "Yau's entrepreneurial drive extended to collaborations with colleagues and students, and, in addition to conducting his own research, he began organizing seminars. He frequently allied
himself with brilliantly inventive mathematicians, including Richard Schoen and William Meeks." Yau has many students from China; some of them are now professors in top universities in this
country. Of course, none of them are supposed to be "inventive", not even "Yau's most successful student" Tian. I also question the connotation of "entrepreneurial".

(7) It is now clear that you are determined to prove your point. "Mathematicians familiar with Perelman's proof disputed the idea that Zhu and Cao had contributed

significant new approaches to the Poincaré. 'Perelman already did it and what he did was complete and correct,' John Morgan said. 'I don't see that they did anything different.'" " 'It is not clear to me what new contribution did they [Cao and Zhu] make,' he [Perelman] said. 'Apparently, Zhu did not quite understand the argument and reworked it.'"

I could have listed a lot more; but I think these are sufficient to illustrate my point. Here is a challenge to you, Ms. Nasar: List the sentences in your long article that associate Chinese/Chinese-American mathematicians with "originality" and likewise those that separate them from "technicality".

I have enjoyed tremendously reading your beautifully written "A beautiful mind". It was a moving and inspirational story. I am now then truly disappointed and horrified by this article of yours in the New Yorker, a magazine that is supposed to represent the best of intelligentsia. Personal vendettas lead us nowhere. Your article is hugely biased. It is a disservice to the mathematical community as a whole; it irreparably and unnecessarily damages Yau's reputation; it does not help Tian; and most importantly, it plays to the false and harmful stereotypes of mathematicians of certain cultures.

One internet chatter, apparently a mathematician/student of Chinese origin, reported that one of his colleagues placed the cartoon in your article on the office door. The chatter stated that he felt "ashamed" when passing by the colleague's office.

For all the harms that have occurred, I believe, at the minimum, a public apology from you and the New Yorker to all is warranted.

Sincerely yours,

A mathematician born in China

32. **gunpowder&noodles**
    August 25, 2006

    This email has been sent to Nasar and the NEW Yorker:

    Dear Ms Nasar: thank you for finally exposing the truth about this truly shameful and disgusting episode. At last Perelman will get all of the credit that is his due, in no small measure due to your public airing of the appalling efforts on the part of certain people to prove their superiority by means of every dirty trick in the book.

33. **Lau**
    August 26, 2006

    Dr. Nasar, your article is fair and unbiased. No need to apologize to idiots like that. They are mad because you are famous and is an authority in journalism and what you say has some type of impact! I, as a Chinese, feel that you are brave enough to challenge such an authority in mathematics who does not treat other fairly and attacks his rivals unnecessarily. THANK YOU!!! You are an unbiased person!! The righteous Chinese scientists salute to you!

34. **CalabiYauManifold**
    August 26, 2006

    "TheGraduate Says:
    August 23rd, 2006 at 12:30 pm

    I just read the article in the New Yorker. Who knew the situation was this dramatic?

    The most important idea in the article seems to be the argument that Dr Shing-Tung Yau of Calabi-Yau fame is stealing people's credit. I don't know if that is true but that seems to be the definitive verdict of the article.

    In case, the implications of the article were too subtle, they have supplemented with a full page graphic of Yau stealing the Fields Medal from Perelman's chest.

    It is a well written essay. I do not know in what repects Gruber contributed but I enjoy Nasar's writing. I can not say whether the reporting is accurate or not. I did think in passing that they had not defined a manifold but I assume most mathematical readers would be familiar with this term.

    Perelman is definitely portrayed in a positive light: more or less normal except for being extremely hard working, honest, open and humble and of course, mathematically brilliant.

    The details about Yau seem almost tangential. The angle of Yau seems to have been inserted by the author's themselves rather than emerging naturally from the interview with Perelman. Perelman himself, as represented in the text, expresses indifference to what Yau may or may not have done. "

    The mentioned work by French mathematician is done by Aubin, it is not enough strong to be extended to solve Calabi conjecture. Yau's result is totally original and has opened a new field for many others.

    This article is truely beautifully written, but I'm suspicious that it is too one-sided. I'm interested to know the whole truth behind it.

35. **TheGraduate**
    August 26, 2006

    Some people had been saying that the article was meant to show Yau as a bad guy but I think the title might give more of a clue as to what the authors were intending.

    They entitled the piece 'Manifold Destiny' which is a play on words and refers to the doctrine of 'Manifest Destiny' ie US expansionism.

    I suspect the authors were meaning to talk about Chinese expansionism. I think this makes more sense because the audience of the New Yorker would probably find something like that more interesting than petty squabbles in mathematics.

    I think they emphasized Yau as Chinese because in their view, he is using the mathematical achievement as another way to give China recognition on the global stage.

36. **Ng**
    August 26, 2006

    If you want to know the whole truth, you have to ask S.S. Chern and his students who know Yau well as well as all the experts in the field to speak fairly and unbaisedly!!! Which is impossible.

37. **MathLover**

August 27, 2006

No dubt that Perelman has some admirable personalities. He is a hero of his own. Do we need a villain in contrast in this drama? I think the New Yorker's article is manipulative. The authors logic goes like:

1. Perelman is a hero
2. Hamilton and Yau are too slow in their own program and are unwilling to learn from Perelman
3. Yau is a bad guy trying to steal the credit from Perelman for his own students

The article may convey point 1 to a more general public and the writting is beautiful there. But I don't buy their points 2 and 3. As for 2, the math community as a whole has been relectant to declare Perelman's victory and slow to learn from Perelman. They only officially confirm Perelman's solution of Poincare conjecture at this ICM. No final verdict about Thurston's geometrization conjecture yet. Moreover, no significant new results come out of Perelman's ideas from other mathematicians. I hope the 3 manuscripts will change the situation.

As for Yau's part I suspect the authors description is leant towards his opponents. The quotes of other math professors are unbelievably uni-directional: destructive to Yau. There must be a master plan behind this game. The controversy between Yau and Tian is another drama worth. Yau against his own student, because he feels obligated to fight corruption in academic world of China.

[Note: I've deleted the last part of this comment, which was a personal attack on Tian]

38. **woit**
August 27, 2006

MathLover,

I've deleted quite a few personal attacks on Yau that were posted here, some from the same person using different names. I've also deleted the last part of your comment, which is a personal attack on Tian.

Please, do not try and use this blog to carry on the Tian-Yau warfare. I think this is an extremely bad thing for mathematics, and I want no part of it.

39. **MathLover**
August 27, 2006

Peter,

I respect your decision to delete last part of my post. Thanks a lot for maintaining this blog. What I wanted to add is that we living outside of China have certain obligations to promote universally valid civil values to China, including fight against corruption. Yau has exposed himself in the fore-front of fighting corruptions in academic world of China. He doesn't deserve such mud slinging from the New Yorker article on his back from the free world! I'm the same opinion that the authors have over done their spin, see Jeremy's post:
http://www.math.columbia.edu/~woit/wordpress/?p=448#comment-15143

40. **Columbia Chemist**
September 1, 2006

Dear Peter:

I have been following this blog since late June after the many news report in China about he Cao-Zhu paper, because I wanted to get some insight from the mathematicians abroad. I'm not in mathematics, but I have been inerested in history and sociology of science for years. For your information, I got my Ph.D in chemisry from Columbia, and now I'm a chemistry professor at Beijing.

Here I'd like to share with you of some first-hand information about the Chinese coverage of the Yau press conference on June 3, 2006, and the controversial over it.

My wife is the reporter at the Sciencetime at Beijing who was one of the few reporters at the press conference given by Professor S. T. Yau on June 3, 2006. She has all the original materials including the press release given to them by Professor Yau on June 3, and the recording of the press conference.

If people read those reports on the ScienceTimes, he would agree that the reports by my wife and her colleagues at the ScienceTimes have been the most truthful, fair and authorative on the whole affair so far. If these articles have been availabe in English, many controversy can be easily clarified. Unfortunaely, these reports have generally not been picked up by most people. (Bad news travels fast.)

Since we learned the New Yorker Magazine (I used to be a subscriber of the NYM when I was a New Yorker) article by Sylvia Nasar and David Gruber, my wife and I were both stunned by the untrutful accounts of many of the events. The account in the NYM article about the media coverage in China only picked up some obviously controversial reports, but never really did serious research on the whole thing. I have to say it is absolutely bad jounalism, and is an indication of lack of professionalism.

Here are some things I may help to clarify.

1. About the contrversy around the credit for solving the Ponicare Conjecture.

The Xinhua News Agency first reported on June 4 that Professor Yang Le told the reporter a division of 50%+25%+30% credit between Hmilton, Perelman and Chinese Scienctits. The news is here:
http://news3.xinhuanet.com/newscenter/2006-06/04/content_4644722.htm (in Chinese)

However, on June 9, the same reporter of the Xinhua News Agency
wrote another news in which Yang Le specificly emphasized that he was not an expert in the field to make such judgment and that he was against any attempt to make such judgment. The news is here:
http://beijing.icm2002.org.cn/mcmweb/Three_Said.htw (in Chinese)

Why there were such two completely opposite reports by the same reporter from the Xinhua?

The truth is that before the first news was wrote, Professor Yang Le was not interviewed by the reporter. And after Professor Yang Le's protest about report to the XinHua reporter, the Xinhua reporter offered in order not to retract the first report he was willing to make a real interview with him in exchange. Believe or not, such unprofessional practice sounds strange, but it does really happen in China. I do not know how such strange number was reached at the beginning, but the truth was that Professor Yang Le was not intviewed by the Xinhua reporter before the interview for the second report.

Unfortunately, this second report has never been noticed. It is fine for ordinary people not to notice it, but it is not acceptable for anybody who is trying to do investigative journalism.

From the recording of the press conference, where 8 reporters from five Chinese media, including the reporter from the XinHua News agency, were pesented, some reporter asked Professor Yau whether Cao-Zhu's paper can claim all the credit, and Professor Yau specificly said that Hamilton and Perelman's contributions were the most important, Cao-Zhu's paper just presented the complete proof and closed the case, and the proof of the Poincare Conjecture was a group effort. There was no mentioning of the division of credit in the press conference. Professor Yang Le was not present at the press conference.

After the press coference, my wife and one of her colleague at the Sciencetimes had an exclusive interview face to face with Professor Yang Le in the same day. There was no such mentioning of percentage in that interview.

When first saw the controversial about the strange 105% number, I myself had the impression that I had read it also from my wife's report in he Sciencetimes. I even joked her for not being able to make the percentage correct. My wife was angry at me for the wrong impression and she asked me to read all the reports in the Sciencetimes and to find whether there was such report of the moronic percentage. The truth is that there was no such report found in their reports, and also no such thing in the recordings as wellas in the first-hand notes she had, including in the press conference, the inteview with Professr Yang Le on June 3, 2006, and another exclusive interview of the Siencetimes with Professor Yau on June 2 of 2006.

I then spent some time a few days ago on the inernet to do my own research on the 30% credit story. Such research should have been done by Ms. Nasar and her associates. I have to say after going through all this materials, I learned how wrong and the New Yorker article was.

I have aways been telling my wife how unprofessional many of the reporters in China are, and how unfortunate that I have to live with this fact. But I have never expected that people like Professor Nasar can be so unprofessinal in writng the article in the New Yorker magazine.

My wife was a fan of Nasar's book on Nash, A Beautiul Mind. She has been proud of being assciated with Columbia (where I got my Ph.D), for the prestigious Journalism School and or having Mrs. Naser as her idol. As a real scientist myself and somebody who had read books on the game theory in my teens, I told her years ago that I actually did not like very much the book and the movie. She did not understand why.

Now she finally understand me.

I wondered why Mrs. Nasar did not even try to get any first-hand account of many of the events in China by contacting her peer journalists before formulating her case. How can people trust her other stories? Was she writng an investigating report or a fiction?

2. Mrs. Nasar and Professor Yang Le

My wife told me that Mrs. Nasar actually met Professor YangLe at the String Theory Conference at Beijing in late June. There was even news photos caught them in the same picture. Professor YangLe said that they briefly chattd or greeted each other there. But Mrs. Nasar never confirmd the 30% percentage story with him.

This is truly the strangest thing for any investigating reporting.

3. About the news coverage in China on the Poincare Conjecture.

Undoubtedly there alwas have serious problms in these reports. As I mentioned above, these problem can only be attributed to the status of lack of professionalism of the reporers, especially in science reporting.

However, for obvious reasons, I do not want to blame the reporters for this situation in China. To understand all these, probably Mrs. Nasar can spend more time in China and try to write an real investigative report on it.

For whatever origin of this situation, to use such unreliable sources of media reports in China before making serious investigtin and to make wild accusations against Professor S. T. Yau is totally untrustworthy and misleading.

4. What I leaned about Professor S. T. Yau?

I have heard many people gossiping about Professor S. T. Yau's personality before. As a chemist, I had no interest in it. I'd rather pay more attention to the personaities of the people in my own field than in mathematics. After listening to hours of the recordings of him, I have to say that it is definitely not possible that he is the villain as depicted in the New Yorker Magazine. I have to say that he fits perfectly as a great person and a great scientist. These people who have been wildly attacking him are far from the truth and are definitly making a fool of themselves.

I believe that people who care will get the truth of the story eventually. I'm glad that this blog is a place which has provided a excellent platform for discussing it.

In the end, I'd like to suggest people to use their commonsense and to try to find of the facts before making their own judgment on this matter. Finding out the truth is not so difficult. It is unfortunate that some people never try.

Sincerely yours,

Columbia Chemist

41. **cl**
September 1, 2006

I have read both news items provided by the post. I found the post to be misleading. The description of the first is correct. But in the second one, published on June. 9th. what Prof. Yang Le said is that assigning percentage is not "completely accurate, I don't agree with that, either. However, Chinese scientists did make outstanding contributions." He never said that he is not an expert in the field to make judgement, instead, he said, "My research eare is complex analysis, but I know Yau, Cao and Zhu very well." He went on to say that that the contribution from U.S., Russia and China are "all extremely important, without any of them, the thoery won't be complete". Finally, he conclude that " in the process of resoling the conjecture, Prof. Yau provided ideas and advice, other people(include Hamilton, Perelman, Cao and Zhu) used Yau's important results, along with other imporatnt theoy in geometric analysis, a field Yau established, to make the breakthrough.

Another fact not being mentioned by either the New Yorker and Columbia Chemist is that, on June. 7th, the ministry of Education of China, sent an official congratulatory letter to Zhu's university for resolving Poincare conjecture. It represents the official position of the Chinese government, not just "lack of profesionism of reporters."

I also like to mention that in an 08/18 article on ScienceTimes, its journalist recalled that they obtained a press release on June 03, cleared stated that R. Hamilton obtained "outstanding and fundamental" results, Perelman provides "key ideas", but Cao and Zhu "completely resolve" the Poincare conjecture.

Unlike Ms. Nasar, I am a Chinese mathematician, so I understand Chinese very well. I also lived in mainland China for more than twenty years, so I understand the meaning and process behind a congratulatory letter from the ministry of education. I admire Yau's achievement in mathematics immensely. He is one of the greatest methematician of this centry. I consult his book and papers very frequently in my research. But on this matter, I do believe that what he did is wrong. As a great scientist, for whom seeking truth should be one's life time devotion, he should apologize.

42. **Columbia Chemist**
September 1, 2006

Fact will not mislead people. Partial fact will.

People used to think that the Xinhua News Agency is the official news agency of the Chinese government, and so its news release must be some kind of official statement. Now people can know the process of how its reports were made, so people can be careful when reading the news.

I also found in one newspaper here there was an exclusive conversation between Professor Yau and the reporter. However, there was no such exclusive interview, and the report was a collage of information gethered from other media reports.

One important fact is that I do not see where is the "official position of the Chinese government" on Cao-Zhu's paper. I knew of the so-called congratulatory letter to Zhu's University from the departmrent of the education from the media reports. Such congratulations letters have been sent to different people all the time in China.

cl should have mentioned that not only the Department of Education, but also the Chinese Academy of Sciences (CAS), and the Academy of Mathematics and Systen Sciences under CAS all sent brief congratulation letters to Zhongshan University the same time. All the letters started with the sentence: "We are all glad to hear that ….".

See here, http://www.cunews.edu.cn/html2006/hndq/hndqxw/151610461.htm

Such things usually happen when these agencies learned the news of a high profile paper is published. I have to say that this practice is silly, and the content can not be taken as any official statement. Seriously, such congratulation letters just like the letters one would receive when his family has a new born son. Why not cl try to get all this kind of letters for things in different disciplines together and show them to the internetional colleagues?

I think my simple intention is to provide facts on the following, and to show why people should not use the inconsistent news reportings to make strong judgment, especially for the investigative report in the New Yorker magazine.

1. Whether Professor Yang Le said anything about the moronic percentage which had been attributed to him?

2. What did Professor Yau actually do or say in his own words?

I do not see anything wrong when somebody so heavily involved claim some credit for it. Whether it is excessive is the issue and also a matter of opinion. Being a scientist myself and a reader of Robert Merton's works, I do not see what Professor Yau had done was very inappropriate, given the facts clarified and the unknown factors fairly weighted. Certainly this does not mean I endorse everything done or said by Professor Yau. I have no interest in it.

If cl can let usknow when was the last time he actually listened to Professor Yau's own words, his comments may carry some weight. Otherwise, being a mathematician and also Chinese does not actually help with anything when speculations are drawn from partial words in secondary sources.

As to what matter Professor Yau should appologize for, I do not see the point from cl's posting.

I'm sorry that I will not post here again, unless somebody wants to verify facts I know with me here. Otherwise, it would be funny to see a Chemist frequently running in the Math world.

Let me repeat:
Fact will not mislead people. Partial fact will.

Best regards,

43. **cl**
September 2, 2006

Reply to Columbia Chemist's post,

I never posted here before, the only reason I did is that I felt the second news item in your post was mistranslated and permutated. So , in the first paragraph, I just provided correct translation. The two items also led me to the SceinceTimes report in which the original press release, which you mentioned also, was quoted, then I provide a translation in third paragraph. As for the congratulatory letters from the ministry of education, they are not sent to many people all the time. last year, or maybe two years ago, Prof. Wang Xiaoyun from Shangdong University obtained very important results in cryptography, this is probably the most important achievement in mathematics in China during the last two decades, it is widely reported by both Chinease and international media, she never got any congratulatory letters from the ministry of education. I personally never heard Prof. Yau talking about Poincare conjecture. so, I can not comment on what he says.

44. **2cents**
September 5, 2006

To all the people who are fighting for Tian and Yau, or who want to share the truth,
It will be nice to show your respect to this place, please bring the Tian-Yau fight or related materials to other related places.
Do it properly and you will be appreciated by the readers of this blog.

45. **MathLover**
September 5, 2006

I'm also tired reading too many comments that do not shed light on the true background. We will see in the future and in the other place what arguements between Tian's and Yau's fans (including one from mine) really hold.

Here is a very interesting interview Atiyah gave in the realm of ICM2006 preparation bulletin (3rd July 2006):

http://www.icm2006.org/press/bulletins/bulletin08/#2

-Quote:

Q: After Perelman's work, can the Poincare conjecture be considered proven?

A: The work of Perelman on the famous Poincare conjecture is widely admired. But in mathematical questions of this complexity final judgement is suspended until the complete proof has been written down, scrutinized by the mathematical community and accepted. That stage has not yet come.

Quote end-

If we are honest, we have to ask ourself, Is Poincare conjecture a theorem now?

I don't know what Atiyah thinks now. It's truly a strange (and maybe even controversial) math history and remarkable ICM for me, the world around Poincare's conjecture changed in just 2 months.

Thanks for Peter to maintain this post open.

46. **jeremy**
September 8, 2006

MathLover,

If you ask Atiyah the same question now, he, as any other responsible mathematicians, will probably give you the same answer.

The world, as far as Poincare conjecture is concerned, has not changed at all, even with the recent media storm. After all the dust settled, the mathematics community will have to do exactly what Atiyah said in his interview. Otherwise, mathematics as we know it will no longer exist. I personally have faith that the final conclusion of a mathematical problem will only be given by the mathematics community, not by the media.

However, from what has happened in the last two months, we have learnt a great deal about mathematicians. These guys can behave exactly like politicians. Their skill of personal attacking and backstabbing is certainly no worse than the ordinary politicians. They probably played politics and media better than the politicians from their own district. I have to congratulate them for that.

47. **sk**
September 13, 2006

1. In Perelman's second famous preprint, he said that another paper with the proof of Theorem 7.4 would be given, but it is still not available. There are two versions of Perelman's Theorem 7.4. One is the strong version with only two conditions, which no proof is available except for Perelman's "sketch". The other is a weak version with an additional condition, which might be too weak to apply according to Kleiner & Lott. Actually, Kleiner-Lott and Cao-Zhu did not prove it, they circumvented it.

What would Perelman reply if he met some experts and they asked for the proof? Note that in April 2003, nobody could fully digest his first two articles. After his return, he posted his third paper devoted entirely to the Poincare Conjecture.

2. Many people, including an article in "Notices", criticized the abstract of the Cao-Zhu paper. However, people who read Orwell's "1984" should know that the phrase "it is the crowning achievement of the Party" is quite common in communist countries. The purpose is to express highest admiration to the Party. In their paper, Cao-Zhu did the same to the Hamilton-Perelman Theory.

2006 Fields Medal Winners

August 22, 2006
Categories: Uncategorized

The winners of the 2006 Fields Medals are Terence Tao and Grigori Perelman (as widely predicted), also Andrei Okounkov, and Wendelin Werner. For some more information, see the press releases at the ICM site.

Okounkov's mathematical work has been in the area of representation theory and its links to combinatorics. His work in mathematical physics is well-known, relating random partitions and the statistical mechanics of certain crystals to Gromov-Witten and Seiberg-Witten theory (counting holomorphic curves and instantons). For some nice expository papers of his about this, see here, here, and here.

Wendelin Werner I know little about, his work involves 2d random walks and is related to CFT. There has been a lot of activity recently in this field, and there's a related program going on this semester at the KITP. A friend wrote to me this morning to speculate that this is the same Wendelin Werner who at age 12 appeared in the film "La Passante du Sans-Souci".

Update: Luca Trevisan is blogging from the conference.

Today the arXiv servers contain the message " arXiv.org servers are currently under very heavy load due to demand for Grisha Perelman's papers, published only as arXiv.org e-prints, which are available below."

Comments

1. **Harry**
   August 22, 2006

   Do we know if G. Perelman has accepted the prize ?

2. **Jeremy**
   August 22, 2006

   apparently not.

3. **bharath**
   August 22, 2006

   It appears Grigori has chosen not to accept the medal. He has also declined Clay prizes and position offers form Princeton, Stanford. He surely has his own reasons.

   Definitely an interesting development.

4. **Peter Shor**
   August 22, 2006

   Gregory F. Lawler, Oded Schramm and Wendelin Werner received the George Polya Prize for the work on stochastic Loewner evolution (SLE), and Schramm and Werner both individually received the Loève in probability theory (in 2003 and 2005, respectively; I believe Lawler was too old for the Polya prize). Does this mean Oded Schramm is already favored for a Fields Medal down the road?

   By the way, stochastic Loewner evolution was a great advance in probability theory and statistical physics: it let people prove a whole bunch of interesting conjectures that had previously been unproven, including some related to conformal field theory.

5. **q2**
   August 22, 2006

   The slides from the Laudatios about the Medallists' work may now be found by clicking on "Prizes" in the left frame here. (couldn't seem to get a direct link to work…)

   Regarding Schramm, he was actually already too old for a Fields this year, having been born in 1961 according to the citation for his 2003 Poincare Prize. In the interview on the page Peter W. linked to, Werner mentioned that he felt like the medal was for all three of Lawler, Schramm, and himself, even though the other two were over the age limit.

6. **kantor**
   August 22, 2006

   Yes he is the passant du sans souci and also a beautifully nice guy helping probability to be associated not only with markets !

7. **Thomas Larsson**
   August 22, 2006

   My understanding of Werner's field SLE (Stochastic (or Schramm) Loewner Evolution) and its relation to CFT is as follows:

   Various spin models in statphys are related to graphical problems, when you make a graphical (high-temperature) expansion of the partition function. E.g., percolation is described by the q-state Potts model in the limit q -> 1, and self-avoiding walks (SAWs) by the N-vector model when N -> 0. E.g., the partition function of the Potts model is sum_G q^c u^L, where the sum runs over graphs G, c is the number of clusters, L the number of links, and u is related to temperature. Note that in the graphical formulation, q and N don't need to be integers.

   Spin models at criticality are described by conformally invariant QFTs. In particular in 2D, the nice ones are the minimal models, with central charge c = 1 – 6/m(m+1) and anomalous dimension h = (pm – q(m+1))/6m(m+1). The central charge is related to the parameters q and N above; q = 2, N = 1 and c = 1/2 is the Ising model, whereas geometrical models typically correspond to the c -> 0 limit. Moreover, critical exponents correspond to anomalous dimensions on the CFT side and to fractal dimensions on the geometrical side, as D = 2 – 2h. In particular, when c -> 0 and p and q integer or half-integers (there is a reason why you need half-integers too, but I don't remember), we have D = (100 – n^2)/48, n integer. This formula covers many well-known fractals, such as the percolation cluster (D = 91/48), the percolation hull (D = 7/4), the SAW (D = 4/3), and the red links (D = 3/4).

Whereas the relation between geometrical phase transitions and CFT was intensely studied by physicists in the 1980s, many things remained conjectural. E.g., conformality was only assumed (and supported numerically), never rigorously proven. Some ten years ago, percolation was becoming studied by mathematicians coming from stochastic processes (Werner, Schramm, Loewner, Smirnov come to mind). Here you regard the e.g. the boundary of the percolation cluster as a stochastic process, a modified form of Brownian motion depending on a parameter kappa, called rapidity and closely related to the central charge; c = 0 is kappa = 6.

Within SLE you can rigorously prove formulas written down by physicists 20 years ago. There is obviously a close connection between SLE and CFT, which has been investigated by a number of people, e.g. John Cardy, and I think that this is more or less a bijection. These theories also share the same glaring limitation, namely the restriction to 2D.

8. **urs**
August 22, 2006

Another discussion of SLE can be found here.

9. **axion**
August 23, 2006

It's a bit rough on Tao and the others — Perelman's achievement completely and utterly dwarfs theirs.

10. **comentator**
August 23, 2006

Until a press release or formal comunication from Perelman regarding the award of the Fields medal is seen, it can be safely said that he accepted the Fields medal. and so far there is none.

11. **zerocold**
August 23, 2006

Hi, ok my comments on this post was wrong
http://www.math.columbia.edu/~woit/wordpress/?p=350
Tao has win the Medal.
Congratualions to him and to this blog for the correct forecasting.
zerocold

12. **Zelah**
August 23, 2006

Also, I would like to add my congratulations to

Kiyoshi Itô, winner of the Gauss prize for applied mathematics! I predicted this last year! Finally the bias against applied work has been lifted.

Finally, it is interesting that nobody on this site is particularly interested in Andrei Okounkov!

13. **MathPhys**
August 23, 2006

This a string theory discussion site, and Okounkov's work (connecting strings to algebraic combinatorics) will probably end up being one of the more lasting aspects of string theory.

Here is an article by S Nasar on Perelman from The New Yorker

http://www.newyorker.com/fact/content/articles/060828fa_fact2

14. **Seth**
August 23, 2006

To axion:

Do you really imagine it's so rough being aknowleged as one of the best mathematicians in the world, but still having someone better than you?

Judging from the physicists I know, even smart people are usually satisfied with being "merely" very good at what they do.

15. **Peter Woit**
August 23, 2006

zerocold,

My suspicion is that someone blabbed to Lubos about Tao at an early stage in the process, long before Tao was notified that he had won. So, when he answered your e-mail asking him about this, he was answering truthfully.

16. **D R Lunsford**
August 23, 2006

What a magnificent article!

-r

17. **TheGraduate**
August 23, 2006

D R Lunsford said "What a magnificent article!"

Could you elaborate?

18. **Responder to axion**

August 23, 2006

I find the claim that the achievements of Perelman dwarf those of the other Fields medalists highly questionable. Tao's diverse accomplishments in p.d.e.s, harminic analysis, number theory, combinatorics are awesome – for example see his web page or, when they are posted on the ICM site (not yet – I just looked), Fefferman's laudation and Tao's lecture.

19. **comentator**
August 23, 2006

Christine Dantas blog has a nice article from the BBC News about the declining of the Fields medal by Perelman. Citing that the president of the IMU John Ball, travelled to St.Petersburg to talk with Perelman to know the reasons of his declining to accept the Medal. So; I guess this makes it official the declining of the Medal by Perelman.

20. **GeomGeek**
August 24, 2006

Sylvia Nasar's article is beautifully written, well-researched and clearly explains Perelman's choice.
Much better than the millions of uninformed papers describing the reclusive borscht-eating weirdo of St Petersburg.
It's funny how it has been the talk of all maths common rooms for years that some famous chinese mathematicians are ridiculously bullish, but things don't seem to have improved much (though this seems to be just one example of an attitude with which Perelman refuses to have anything to do).
After reading Sylvia Nasar's paper I wish Perelman made a public statement to accompany his refusal.

21. **woit**
August 24, 2006

Please, personal attacks on anyone involved in the Poincare story have no place here and will be deleted.

22. **tg**
August 24, 2006

I read Nasar's article, and indeed I learned gossip that I wasn't aware of, even though I know some of the people involved. It was entertaining.

That being said, I think it plays to the stereotype that
Chinese mathematicians are "technical" but not profound
(I speak mainly of her description of Yau's Fields medal
achievements), and moreover "bullish" (to borrow from
GeomGeek above).

What's wrong with describing the truth about ill conduct?
Nothing, per se — but in the interest of balance, I wonder how many other (influential) mathematicians act similarly, albeit in less famous instances; e.g., protecting the work/unrealistic promotion of one's students (say unfairly
at the cost of others), or undue influence of hiring committees and journals?

On the one hand, Luca Trevisan writes a useful blog of the ICM activities, where she says "John Ball starts his speech by explaining how…work is appreciated solely based on its merits, not on the way it is promoted." (Come on…) On the other hand, we have Nasar's description of Yau's behaviour, juxtaposed prominently with his heritage.

The reader of this blog could easily come away with the impression of the benevolence of mathematicians save a certain subset. I don't think that paints a fair picture at all. With fame/power/influence, academics of any stripe can and sometimes do take advantage of their position. Newton did it to Leibniz, and as an example, it isn't first, last, or uncommon.

23. **TruthSeeker**
August 24, 2006

You're right, tg, for saying that bad behavior is not uncommon in academia. This is unfortunate because scientists and scholars are supposed to be distinguished from politicians in that they ought to have a tremendous respect for truth — not half or one-sided truth — but the whole truth.

24. **Peter Woit**
August 24, 2006

tg,

I don't think the article was promoting the idea that Chinese mathematicians are "technical" rather than profound, and in any case Chern and many others provide excellent counterexamples. However, what is going on in China as the country becomes much more prosperous and influential, quickly developing a new and large mathematics research community, definitely seems to be part of the story here.

25. **q2**
August 24, 2006

tg,

You say the article plays to a stereotype of the bullishness of Chinese mathematicians…well, there's a plural there, and the reader of the article does indeed encounter more than one Chinese mathematician. Do you think Nasar and Gruber portray Tian that way, for example? I sure didn't come off with that impression.

While it's surely true that prominent academics in any field sometimes use their influence in non-even-handed ways, some senior mathematicians that I've talked to (both before and after his sallies in the Chinese media back in June) have indicated that Yau's persistent efforts to disparage the work and integrity of certain other researchers goes well beyond anything that they've seen from anyone else. I think that that aspect of Yau's personality is an important bit of context for understanding the present situation, and when I read the article I was relieved that Nasar and Gruber felt no obligation to hide it.

26. **Chris W.**
August 24, 2006

From the New Yorker's press release on the 8/28 issue:

> Nasar and Gruber write that the prospect of being awarded a Fields Medal, math's most prestigious prize, matters little to Perelman, who says that he plans to

refuse the award. “It was completely irrelevant for me,” he tells the writers. “Everybody understood that if the proof is correct then no other recognition is needed.” Perelman declares that he has retired from the mathematics community and no longer considers himself a professional mathematician: **“As long as I was not conspicuous, I had a choice. Either to make some ugly thing”—a fuss about the math community’s lack of integrity—“or, if I didn’t do this kind of thing, to be treated as a pet. Now, when I become a very conspicuous person, I cannot stay a pet and say nothing. That is why I had to quit.”**

27. **MathPhys**
August 24, 2006

In this particular food fight, Tian’s optimal game plan to play saintly, and watch as Yau self-destructs.

28. **tg**
August 24, 2006

Dear q2,

Certainly, Yau’s personality/behavior is (in)famous. As I said previously, I see nothing wrong with exposing the truth. However, I also asked for context, since that clearly affects how we absorb the said truth (e.g., “hearing only one side of the story…” blah blah).

In this case, Nasar and Gruber could easily have included a sentence or two from a neutral authority to the effect of “in math, as in academia more generally, there are always priority fights and bad behaviour, from all kinds….Yau is today’s unfortunate example”. I believe they deserved to, because they so closely tie Yau with the Chinese –a point they insisted on emphasizing, which is incidental to Perelman’s plight.

I believe they know the stereotype, and in general, it makes for a more entertaining article to exploit the reader’s reaction “oh yeah, there they go again…”. Indeed, as you say, there’s a plurality of Chinese involved in the story. However, my impression was that none were portrayed as bucking that stereotype. Some (Tian) were merely portrayed neutrally.

For example, with regards to the “technical” sentiment (and in response to Woit’s remarks), Nasar and Gruber do not say “Chern was among the most original and inventive geometers in history”. To the layman reader (being a Newyorker article), Chern’s only role is to give birth to Yau.

Finally, yes, the senior people you speak to, find Yau’s actions extremal. However, are his actions portrayed as they are, and he made a singularity of, in part because he’s Chinese? I think so.

29. **q2**
August 24, 2006

Finally, yes, the senior people you speak to, find Yau’s actions extremal. However, are his actions portrayed as they are, and he made a singularity of, in part because he’s Chinese? I think so.

I don’t buy this, because I really can’t think of either:

(a) Any other living pure mathematician of any ethnicity who has shown Yau’s penchant for engaging in (and enlisting the media in, e.g. in his interview about Tian last year) ugly, highly public turf wars; or
(b) Any other Chinese mathematician whose reputation for bad behavior among senior people at all resembles Yau’s.

If Nasar and Gruber had portrayed Yau’s antics as somehow common among mathematicians, in my opinion it would have been a serious misrepresentation of the current culture of pure mathematics. You’re going to have to come up with a much more recent example than Newton-Leibniz to convince me otherwise.

30. **tg**
August 24, 2006

Dear q2,

I hear what you are saying. I claim that Yau’s behaviour is not really that different than what many academics in math (and elsewhere) do. I don’t think that’s a serious misrepresentation. It just turns out that in most cases, the setting is not nearly as famous as a battle of Fields medallists, concerning a (truly) important open problem, or within a popularly known area such as string theory.

If I mentioned people I thought that were essentially no better than Yau in some given area of pure mathematics, chances are many would never know about it, nor really care to talk about it. Most topics in pure math just aren’t that well connected, unfortunately.

You know that I can hardly start naming names. But generally, e.g., it is not uncommon that I see good jobs going to certain people, or papers being accepted to important journals under objectively odd circumstances — while at the same time other qualified people or papers are shut out. These phenomena are often euphemistically called “white noise” or “randomness” in the system. Politics is involved; why deny this?

This is not an indictment of our field. I just wish Yau’s behaviour would be cast in this context.

31. **TheGraduate**
August 24, 2006

“In any dispute the intensity of feeling is inversely proportional to the value of the stakes at issue — that is why academic politics are so bitter.” — Wallace S. Sayre

Nothing so ephemeral as acknowledgement.

32. **Thomas Larsson**
August 25, 2006

Yesterday a review of conformal random geometry, which is Werner’s field, appeared on the arxiv: math-ph/0608053. It is written by Bertrand Duplantier, who for the past 20 years has been the leader of this field, having used CFT methods to compute more fractal dimensions than you want to know about.

33. **TTT**
August 25, 2006

I understand it would take tremendous personal courage and the purity of heart, so the hope is slim, but what would you think of if – for the sake of Science and the future of the field – Yau stood up, apologized to Perelman (regardless of who’s wrong in this situation), and invited him back to Mathematics?

In that case there won't be any losers… otherwise, we all lose.

Are deeds like this possible in modern research?

34. **Peter Woit**
August 25, 2006

TTT,

I don't think Perelman actually cares one way another about Yau. He's been quite consistent in his life that he wants nothing to do with the standard reward system of academia and questions of who gets recognized for what. Unfortunately, the New Yorker article didn't really explain what he's up to now: what was the problem at Steklov? is he still thinking about math? The reaction of some mathematicians I've talked to about the article is that the authors missed a chance to find out what they would really like to know: what is the guy working on now?

35. **TTT**
August 25, 2006

So people do believe he decided to secretly stay in Math?

In that case they are in trouble, because:

A person who can live on milk, bread, and butter, who is free from the pressure of find/keep-a-position-publish-publish-publish-or-die system, and who doesn't care whether some intermediate result would be named after him, has a huge advantage over the rest of them. Unlike many who want to put any reasonably interesting idea out their as soon as possible for all the world to see, he can continue working on whatever he wants without attracting ANY attention and without giving away ANY precious clues.

How long did he stay in the US doing postdocs? 4-5 years? Let's say he saved $200,000 during that time. In Russia, he can live on that with no financial worry for the rest of his life.

At the rate of 8 years per millennium problem, he will solve all of them by 2050.

A rather original way of doing research which, experience shows, happens to be extremely effective,…….. provided you are a genius, of course. Any geniuses out there who wanna try that too?:)

36. **MathPhys**
August 25, 2006

I don't know what happened between Perelman and the Steklov, but I imagined it was time to renew his affiliation and someone asked him to fill in some routine form, just like everyone else, and Perelman characteristically said something like

"If you want me here, you will not ask me to fill in a form, and if you ask me to fill in a form, you don't want me here",

and things escalated and the mole became a mountain and he eventually lost his job. I wouldn't be surprised if that's how it happened. He seems to be that kind of person.

37. **werdna**
August 25, 2006

MathPhys wrote:

"If you want me here, you will not ask me to fill in a form, and if you ask me to fill in a form, you don't want me here."

The second half of this statement is just the contrapositive of the first half. Now, would the very-terse Perelman need to repeat himself unnecessarily? 🙂

38. **MathPhys**
August 25, 2006

He's quoted in The NewYorker as saying things like that:

When a member of a hiring committee at Stanford asked him for a C.V. to include with requests for letters of recommendation, Perelman balked. "If they know my work, they don't need my C.V.," he said. "If they need my C.V., they don't know my work."

39. **Graham**
August 26, 2006

Zelah said:

> Also, I would like to add my congratulations to Kiyoshi Itô, winner of the Gauss prize for applied mathematics! I predicted this last year! Finally the bias against applied work has been lifted.

In fact, Hans Foellmer, who gave the Gauss Prize address honoring Ito, mentioned your comment as "a posting on the internet". Congratulations, you're famous.

40. **Mark Yasuda**
August 26, 2006

A historical quote that many might feel is applicable to Yau (as depcited in the New Yorker article):

"This excessive impudence is unbelievable in a man who has sufficient personal merit not to have need of appropriating the discoveries of others."

For those who don't recognize it, it's an English translation of a comment made by Legendre in 1820 regarding Gauss. Gauss, in some well-known instances during his life, chose to minimize the contributions of others or claim at least partial priority based upon his unpublished work. In Gauss' case, the claims all appear to be justified, but they do come across as self-serving and had detrimental effects in certain cases (e.g. J. Bolyai).

41. **MathLover**
August 27, 2006

Perelman has won my respect once again after my reading of New Yorker's article. What puzzles me is why the math community is so slow to declare his victory, and even more important, so slow to produce new results from Perelman's genius ideas. In ICM 2006 they only declared the Poincare conjecture been solved. What about Thurston's geometrization conjecture? Manuscript by Morgan and Tian treats only Poincare conjecture. Cao and Zhu do have a complete treatment of geometrization conjecture, this is also where they claim they don't fully understand Perelman's arguments (in contrary to Morgan-Tian) and substitute their own results (weaker than Perelman's assertions). As a layman, I enjoy reading Cao-Zhu's treatise more than other two.

Perelman's quit is regretable, but we shall respect his choice for whatever reason. I hope he will come back. It's a pitty that he no long has personal impact to math, unlike great Alexander Grothendieck who changed half math world before left. I have learned recently that Grothendieck's father was deported and murdered at Nazi concentration camp Auschwitz. (His mother was German.) Grothendieck himself has rejected awards on his pacifist stance, or saying something like he has enough for living from his pension. Truly admirable person!

42. **MathPhys**
August 27, 2006

Grothendieck also had wives and children that he totally abandoned.

43. **MathLover**
August 27, 2006

To MathPhys,

Is he insane?

44. **MathPhys**
August 27, 2006

I read some of his (Grothendieck's) autobiographical notes a while ago (I'm not so sure if what I read is formally part of "Récoltes et Semailles", or not).
There he describes how during meditation he sees visions and talks with the angels. Does that make him insane? I don't know, but he surely lives in a world of his own.

45. **tg**
August 27, 2006

Dear MathLover,

Re: "and even more important, so slow to produce new results from Perelman's genius ideas."

Some reasons I can think of:

(1) Few people (professional mathematicians) understand consequences/deeper aspects of his work (beyond the Poincare conjecture).

(2) Those that do (Lott, Yau, Cao et al) were busy just trying to get his arguments to be believed (otherwise why bother with the next step if it's all wrong?).

(3) The feeling that Perelman's work has "killed" the field, so new graduate students and others are loath to bother continuing on.

(4) Work _is_ being done. Math takes time, man.

46. **jeremy**
August 27, 2006

Peter,

I am a little disappointed by your response to TTT. Not by your comments, of course, but by the reactions of your mathematician friends to the New Yorker article. Are they really so concerned about "what is the guy working on now"? Do they have worry at all about "what is the guy living on now"? Are they interested at all in finding out if Perelman needs any help? I truly hope that the mathematics community will at least try to find out the answer to the question that you raised "what was the problem at Steklov?", and provide help when Perelman needs.

Sylvia Nasar and David Gruber went to St. Petersburg, not to find the answer on the well-being of Perelman, but to find the answer from Perelman on the question of who should get credit for the proof of Poincare conjecture. They are more interested in the "who should get credit for what" controversy surrounding the proof of Poincare conjucture, and how they can write and sell a story. They found, of course, the wrong person to answer their questions. But they certainly have all the materials to write a good story.

The materials that they have gathered are so good that many of the science writers could only dream of. There is Poincare conjecture, one of the toughest mathematical problems in the universe. Solving this problem will not only lead one to Fields medal but also to the million-dollar prize given by the Clay Institute. There are the non-inventive Chinese mathematician (Yau), who not only steals other peoples work but also want to be the king of geometry and to take credit from everybody else's work; the friendless Russian Jew and mathematical genius (Perelman), who proved the Poincare conjecture but rejected the Fields medal. The credit for his work is now in danger of being stolen; and the unremarkable American playboy mathematician (Hamilton), who needs constant push by Yau but still cannot get the job done. There is also a list of very much involved mathematicians whose names are associated with many of the well know universities.

Anyone who read Sylvia Nasar's previous book on mathematicians, A Beautiful Mind, will acknowledge that Nasar is an excellent writer, and will also find that this time she is having even better materials to write about mathematicians. She has a Chinese villain, a Russian genius. The potential for portrait of sex, money and Fields medal, all mixed up with an even tougher mathematical problem, not to mention the fuss and controversy about "who should get credit for what". With a little spin, or even without spin, Sylvia Nasar and David Gruber could have written a fascinating and believable tale. The problem is that there is that controversy and some spins are necessary. Spin, however, is a different form of art, an art form that is often mastered by political speechwriters. As storywriters, Nasar and Gruber are obviously not experts on spinning. They have over done their spin.

To spin properly, one must first get the facts right, and then spins the facts to his/her favor. Spin over zealously without the facts can have dire consequences. One example is Bush administration's spin on the Iraq's WMD, we all know that we are now in a mess in Iraq.

There are too many over zealous spins to count in the Nasar-Gruber article. Here I list a few and point out their not so pleasant implications.

A large part of the Nasar-Gruber article is devoted to Yau, he is described as a Chinese mathematician who wants to solve the Poincare conjecture for China. He was also anxious to become the next famous Chinese mathematician after Chern. The fact is, both Chern and Yau are Americans. When Chern was awarded the National Medal of Science in 1975 and Yau was awarded the same medal in 1997, each of them was cited as one of the best American mathematicians at the time. Referring Yau and Chern as Chinese mathematicians is as ridiculous as referring Nasar as a Middle Eastern writer. To make their story, Naser-Gruber have used false information about one of the main characters in the story. In an article that partially committed to reveal the dishonesty of Yau, they committed some dishonest acts of their own. What a spin!

Anyone reads New York Times and books beyond mathematics would know that China is a communist country. It has one of the worst human rights record in the world according to the latest UN human rights report. In a communist country, power belongs only to the communist party. It controls every aspect of people's life, including mathematics. As for today, a New York Times researcher is still in prison in China. To reach the power structure in that country, you have to be a member of the communist party. To have power, you would have to be a powerful party member. Nasar-Gruber have us believe, through Joseph Kohn, that "Yau's not jealous of Tian's mathematics, but he's jealous of his power back in China." But, do Nasar-Gruber also try to tell us that Tian is a powerful member of that repressive and corrupted communist party, and some of Yau's accusation of Tian's corruption may be accurate? We do not know the truth. But to spin so hard that brings doubt to their own arguments is a bad example of spin.

I was hoping that the New Yorker article could have told us more about Perelman's current situation, especially after reading the article published by Telegraph.co.uk. There is not much about Perelman that we haven't read before in the Nasar-Gruber article, then again Nasar-Gruber were writing about the "battle over who solved" Poincare conjecture. What we are supposed to learn from the article is about the battle, not about Perelman. But in one of the occasions that Perelman talked to them, they leave us the impression that Perelman was contradicting himself. According to Nasar-Gruber, "Perelman repeatedly said that he had retired from the mathematics community and no longer considered himself a professional mathematician." Yet when asked about Cao-Zhu' paper, which had just been published, Perelman knew the paper well, and did not see "what new contribution did they make". That is good news. It shows that Perelman still goes to library and read the current issues of mathematical journals. Moreover, Perelman also knew that it was "Zhu did not quite understand the argument and reworked it." Without reading the paper, Perelman would not have known that his proof was reworked. Without involving in the mathematics community, he would not have known that, in the Cao-Zhu paper, it was Zhu, not Chao, who has reworked his proof. Zhu is probably the one who reworked Perelman's proof, but this is not obvious even for people who are involved in the mathematics community. Perelman, as we all know by now, has no need to contradict himself. He probably did not even care about Cao-Zhu' paper, for he has already done the proofs three years ago. The only possibility for the contradiction is that, to spin about the "battle", Nasar-Gruber needed not only to ask the question, but also needed an answer. The answer described in the story might very well indicate that they made up this part of story. Again, we do not know the truth. But this time Nasar-Gruber manage to bring doubts on themselves.

I cannot remember who said this "writers never lie, they just make up stories."

It is unfortunate that the mathematics community was incapable of and incompetent in determining the completeness of Perelman's proofs of the conjectures. It has to rely on storywriters to help them to fight a "battle". After all the Washington style of dirt digging, mud slinging and spitting in the faces, is it time to go back to check the proofs again?

47. **woit**
August 27, 2006

Jeremy,

I think most mathematicians aren't concerned about whether Perelman has enough to live on, since they are well aware that he has turned down many offers that could provide him with more money if that's what he needs. The most recent is the Fields, which carried some money with it. The mathematicians at many institutions (including my own), have contacted him with offers to pay him very well to come visit, give lectures, and discuss mathematics. He has turned all these down, mostly not even bothering to respond to them. This is his choice as to how to arrange his life, and it has worked for him so far. Working this way by himself he figured out how to make more progress on mathematics than anyone else in recent years.

As I wrote, it is a shame that Nasar-Gruber didn't explain what the problem with the Steklov was, maybe that was something other mathematicians could intervene to help with, maybe not.

As for the comments about the "corrupt communist party member" Tian, please don't again post this kind of personal attack here. When you do it, you don't exactly help your case in complaining about the article's unfair personal attacks on Yau.

48. **Eli Rabett**
August 27, 2006

In a situation such a Perelman's (or Yau's) you don't get the complete picture from one article in the New Yorker, because to write such an article means that the authors have a story to tell, with a beginning, middle, end and moral. Think of the Nasar-Gruber article as a data point. The unpacking of Perelman's results will be another.

49. **Jack.Li**
August 28, 2006

Woit,

I don't think Jeremy was trying to say that. Please go back to read his lines.

On the other hand, coming from China, I also feel Jeremy has somewhat unwittingly exaggerated the role of "communism" in China's scientific research. The reality is, in science, as long as you are truly good (researchwise, not playing politics), you will be profoundly respected.

But the problem with the current China is, after virtually only 20 years' of reform and openning to the outside world (recall how the Cultural Revolution completely destroyed science and scientific spirit), China has not been able to produce many truly outstanding world-class researchers. During such a period of transition, you can imagine why some people are playing politics to gain funding and prestige. **Compared with Pelerman, they are shameful.**

On the other hand, Yau has been a courageous fighter in China to attack all the major corruptions in China's academia. **In China, if you want get things done, it is better to take the top-down approach (not bottom-up).** It has been said that even the Chinese Premier is now paying attention to Yau's words on how to stop all the acadmic corruptions and unfair playing rules (esp to young native Chinese researchers).

Just share some random thoughts with all the new friends here….

50. **yd**
August 28, 2006

The Fields 2006 citation for Perelman says that:
"his results provide a way of resolving … the Poincare
Conjecture and the Thurston Geometrization Conjecture."
and that the math community is still checking his work and
no one has found serious problems in the work.

Is this the same as unequivocally declaring that

the two Conjectures have been resolved by Perelman and
that there are no problems in the work?

Or, is this a best-case compromise IMU can possibly make
between not recognizing his work in 2006 2-3 years after
Perelman's posting and 100% recognizing it now but with a small
chance of having to deal with a serious problem or
gap someone may find in the work some time down the road?

Can someone shed some light?

Thanks,

51. **A.J.**
August 29, 2006

tg wrote:

(3) The feeling that Perelman's work has "killed" the field, so new graduate students and others are loath to bother continuing on.

I'm not sure this is true. As MathLover pointed out, there's probably quite a lot that can be done with Perelman's techniques. Geometrization can probably help us understand 3d TQFT and knot theory, for example. The problem is that these techniques are really hard to use. That's why it took several years for the ideas to be accepted as true (although I seem to recall Hamilton saying a few years ago that the ideas were probably correct), that's why the full proofs are hundreds of pages long, and it's why we haven't seen the same rush of theorem proving that came after Seiberg-Witten.

52. **ordinaryamerican**
August 29, 2006

Chern and Yau are Chinese mathematicians. Over one billion Chinese understand this obvious fact. Both were born,raised and educated-excluding Yau's Berkely years- in China and Hong Kong.

Yau clearly wants his accomplishments to seen as a great of achievement for the Chinese people. Americans of European descent are not interchangeable with one billion Chinese living in China.

I would rather have my tax dollars spent on developing home-grown mathematical talent(like my own three daughters)

53. **ordinaryamerican**
August 29, 2006

One last thing. Peter, thank you for this website. You are performing a great civic duty for the general public.

Lubos hasn't pulished a scentific paper in nearly three years. There is something very weird going on over at the Harvard physics departement. Or maybe Lubos is working on something very top secret.

54. **tg**
August 29, 2006

Dear ordinaryamerican,

You say:
"Americans of European descent are not interchangeable with one
^^^^^^^^^
billion Chinese living in China."

Thank you for proving my point; I presume you enjoyed Nasar and Gruber's article.

55. **tg**
August 29, 2006

Dear A.J.,

""(3) The feeling that Perelman's work has "killed" the field, so new graduate students and others are loath to bother continuing on."

I'm not sure this is true. As MathLover pointed out, there's probably quite a lot that can be done with Perelman's techniques."

I should have called my "reasons" for why more isn't being done by "general/plausible reasons". Sure, I believe a lot can be done — but how convincing is the situation to an incoming grad student, with there being so much math available to be done?

"Geometrization can probably help us understand 3d TQFT and knot theory, for example."

This sounds nice to me!

56. **Peter Woit**
August 29, 2006

ordinaryamerican,

Please take the nationalism and racism elsewhere. I'll delete anymore of it that people try and introduce here, but I have to admit you do give credence to those commenters worrying that anti-Chinese prejudice is an issue.

57. **?**
August 29, 2006

Hoping to be on-topic, I would like to ask: before that the Poincare conjecture was proofed, somebody doubted about its validity, or it is one of these statements that physicists consider true for obvious intuitive reasons?

58. **Peter Woit**
August 29, 2006

There's no "obvious intuitive reason" for the Poincare conjecture to be true, and because it was so hard to prove, their certainly had been mathematicians who speculated that there could be a counterexample and looked for it.

59. **TheGraduate**
August 29, 2006

Peter,

Would you say or have you heard other mathematicians speculate on whether there is something specially about 3 dimensions that made it the last and most difficult case to solve the Poincare conjecture in?

60. **Peter Woit**
August 29, 2006

TheGraduate,

The standard explanation is that in higher dimensions there's so many ways to move things around that things simplify. The conjecture was proved in dimension 5 and above during the sixties. dimensions 1 or 2 are easy, because there's not that much that can happen. The worst cases are dimensions 3 and 4, because there's enough room for a lot of complicated things to happen, but not enough to move things around and simplify.

I don't know of a particular reason why 3 should be worse than 4. Actually, the "smooth Poincare conjecture" still remains unsolved in 4 dimensions, this says that there is only one "smooth structure" on the four-sphere.

61. **Who**
August 29, 2006

Graduate,

You asked Peter
Would you say or have you heard other mathematicians speculate on whether there is something specially about 3 dimensions that made it the last and most difficult case to solve the Poincare conjecture in?

I can tell you one special thing about 3D that you can think about while waiting for an authoritative answer. This is just something that comes to mind about 3D that might not actually address your question. A fun thing though.

You can tie knots in 3D.

whereas in 2D it is hard to tie knots, and in more than 3D the knots tend to come untied and turn out not to be knots.

62. **TheGraduate**
August 29, 2006

Thanks for the answers Peter and Who.

A few months ago, I had been reading an overview of past fields medal work by Michael Monastyrsky and I was struck by the following:

"From the time of Cayley, the following division algebras were known: real numbers, complex numbers, quaternions, and Cayley numbers … A natural question to ask is: Are there other division algebras? The negative answer was obtained only in the 1960s and proved to be closely related to the following topological question: find all spheres on which the number of independent, continuous vector fields is equal to its dimension. There are only three such: S1, S3, S7."

I can't even begin to claim I understand all the ideas in that quote, but… it seemed to suggest something special about these sorts of spaces: reals, complex numbers etc that have proven so useful in physics.

63. **q2**
August 29, 2006

To expand on Peter's explanation of why higher dimensions have been easier for the purposes of the Poincare conjecture, the point is that an important tool in the higher-dimensional case (both for Poincare and for related results, like the h-cobordism theorem) is something called the "Whitney trick," which is used to ensure that if two transverse submanifolds P and Q of complementary dimension in a simply-connected manifold have homological intersection number zero (which ordinarily just ensures that their intersection points come in oppositely-signed pairs) then one can isotope them to not intersect at all. To perform the trick, given any algebraically-cancelling pair x,y of intersection points between P and Q one needs to find an *embedded* 2-dimensional "Whitney disk" with the left half of its boundary being a path in P from x to y and the right half of its boundary being a path in Q from x to y. By simply-connectedness, one can always find a map of a disc into the manifold with the right boundary conditions–and if the dimension of the manifold is at least 5 then after a little jiggling the disc will become embedded and so the Whitney trick works. If the dimension is three or four, the disc can be jiggled to be immersed, but it will tend to intersect itself and thus not be embedded.

The failure of embeddedness isn't as bad in dimension four, since genericaly all the self-intersections are single points. Because of this, Andrew Casson had the bright idea of using a complicated kind of infinite-dimensional handlebody now called a Casson handle as a replacement for a Whitney disc; Michael Freedman's big technical result was that a Casson handle is homeomorphic to a standard handle, and this enabled him to prove that all the 5-or-higher-dimensional techniques for the h-cobordism theorem and Poincare still work in dimension 4 if one is willing to work up to homeomorphism rather than diffeomorphism. My understanding is that at the time he thought that if only one were cleverer one could extend his result to the smooth category, but within a year it became clear that that wasn't the case: although we still don't know whether the smooth Poincare conjecture is true in dimension 4, we do know via Donaldson's results that the h-cobordism theorem does fail in that dimension–so Freedman's technical result was actually as good as one could hope for.

In three dimensions a prospective Whitney disc would intersect itself in a one-dimensional submanifold, so there's nothing analogous to Casson handles that one could try to use. In that regard, it's fair to say that the proof of Poincare in dimension four is very much in the spirit of that in higher dimensions–although showing that the ideas from the higher-dimensional case actually extended to dimension four required hard, Fields-medal-deserving work. The proof in the three-dimensional case uses a completely different approach.

64. **q2 appreciator**

August 29, 2006

q2, this was wonderfully informative, thank you very much.

65. **TheGraduate**
August 29, 2006

Thanks q2. I don't quite understand all the terms but it's good to at least know what the relevant ideas and mathematical objects involved would be.

66. **dt**
August 30, 2006

I just read the Nasar article last night. As a lover of geometry, and as a Chinese woman, it left me sad and disgusted and angry. All the more so because I knew the New Yorker was running a story about Perelman, and I had so looked forward to reading it — it's not every day that I get to read about math in the New Yorker! I had expected a detailed biography of Perelman, with detours into the world of modern mathematicians and their work, a sketch of the historical background to the Poincare conjecture, a depiction of the working life of mathematicians in the 21st century. ….I don't know how one explains the pursuit of abstract mathematics to a lay audience, but I was greatly looking forward to the attempt.

Instead, as I read on and on, I could not believe how she was turning this beautiful story, this "landmark not just of mathematics, but of human thought" into a petty, racist soap opera.

Her descriptions of Yau as a washed-up uncreative techno nerd, and her implication that the Chinese are trying to improperly take credit, were painful to me. It seemed like such an exaggeration and distortion of Yau's motives, work, and personality, and in a more anodyne but nevertheless insidious fashion, of Perelman's motives, work, and personality (basically, he's given the personality of a paranoid sheep). The subsequent comments of mathematicians quoted in the article indeed prove without a doubt that she was distorting their comments re Yau. Yet, even with all her deceptions, she couldn't hide the fact that Yau *had* with his sharp mathemactical nose identified a critical problem and the right approach, as demonstrated by how hard he was encouraging Hamilton to follow up on his Ricci flow method.

This is a great story and Perelman is a true hero. Unfortunately, even though Sylvia Naser has a good command of English and rhetoric, she clearly doesn't love math or have any inkling of its walk-on-air beauty, to do justice to the story. Otherwise, I'm sure she would have been able to communicate a shred of that beauty in her long-winded article.

By the way, the quote about the 50/25/30 breakdown in credit made at a speech in China, which Naser turns her nose at ("Even mathematicians can sometimes forget how to add") was obviously an ironic joke by Yau, a self-deprecatory laugh at the unseemliness of China taking a percentage of the credit in this case, and indeed, the futility of ever apportioning credit exactly (yet it is completely understandable that China, a developing country, would want to celebrate its small but impressive contrbution to this discovery and it is touching of Yau to cheerlead this effort; I for one am proud that Chinese mathematicians living and working in China and publishing in Asian journals are able to do world-class mathematics). It might also be a geometry joke, since geometry is all about getting rid of numbers and not having to use coordinates.

Grigory Perelman got it exactly right when he said "journalists should have better taste."

67. **MathPhys**
August 30, 2006

dt,

Please calm down.

From everything I've read, the 50/25/30 breakdown was not intended as a joke. Someone made a silly mistake. That's all.

Further, S T yau is very well known to be notoriously competitive. There are many, many stories circulating about him to this effect since many years.

On the other hand, we all know of (born and bred in the) US scientists who are at least as competitive as Yau, so this not a particularly Chinese characteristic.

68. **tg**
August 30, 2006

Dear dt,

I support your finely written comment.

69. **geometer**
August 30, 2006

At a recent ICM interview Cao said that Yau never talked about "50/25/30 breakdown", and does not agree to any such breakdown.
(I think this is in ICM daily news of August 29).
Now this is interesting because the quote in Nasar's paper (from a press conference at the math institute in Beijing) is very precise and clear. I am sure someone must have the whole press conference on tape. I wonder who is lying here?

70. **TheGraduate**
August 30, 2006

dt:

I am glad you and other Chinese had chosen to share your opinions here.

I thought perhaps Nasar and Gruber were looking at things through the prism of American foreign policy. Through that prism, the rise of China would be potentially threatening.

One of the reasons that I do not think it rises to the level of racism is that I do not think they would have reacted to a similar situation with a Japanese person in the same way. I think this is because Japan is a strong partner of the US.

For me also, the article was not what I had expected it would be.

One question I did have for you or any other Chinese speakers that might care to comment: quite a few people who have identified themselves as Chinese have said things along the lines of Yau being brave or noble or caring etc. I was curious about what these comments were based on. Are these based on assumptions or are they based on things you know about Yau that non-Chinese speakers would not know?

For instance, How did you come to this conclusion: “obviously an ironic joke by Yau, a self-deprecatory laugh at the unseemliness of China taking a percentage of the credit in this case, and indeed, the futility of ever apportioning credit exactly”?

71. **TheGraduate**
August 30, 2006

geometer:

Where did you hear/see the interview? Can you provide a link or reference to the source?

72. **geometer**
August 30, 2006

page 2 of http://www.icm2006.org/dailynews/dailynews29.pdf

73. **tg**
August 30, 2006

Dear TheGraduate,

You said:

“One of the reasons that I do not think it rises to the level of racism is that I do not think they would have reacted to a similar situation with a Japanese person in the same way. I think this is because Japan is a strong partner of the US.”

The notion that Japanese and Chinese are not treated the same does not mean to me that there is no racism involved!

You also asked:

“Where did you hear/see the interview? Can you provide a link or reference to the source?”

Look at http://www.icm2006.org/dailynews/

I actually had some problems reading the files. But I did notice an interview with Cao who asserts that Perelman’s work was clearly the central (final) step in Poincare. Interesting Cao also seems to suggest that Yau has said the same to Nasar during her research into her biased, anti-China article. But this never seems to appear in her write up.

It should also be noted that even with regards to Nasar’s “A beautiful mind”, I’ve heard that a number of mathematicians have been angered by how she’s twisted/exploited their words to achieve effect. I ignored that feature of her journalism until recently, since the text was so nicely written.

So while previously, I believed what she said was true, but not given proper context — I now how reason to suspect that what she’s said in her article includes crucial falsehoods.

In retrospect I’m embarrassed as follows. As a mathematician, I so rarely see my field portrayed in media — so when it does happen, I’m quick to read/see it and enjoy it. I think I’ll be more discerning in the future.

74. **TheGraduate**
August 30, 2006

tg:

Another reason I do not consider race as the dominant factor is that if Yau had been born and raised in America then they probably would also not treat that situation the same way. Do you perhaps mean some form of discrimination besides race-based discrimination?

I don’t think there is any reason to be embarassed for mathematics. Everyday people care more about politics than they do about computations. This kind of controversy will probably be more attractive to young people as it shows mathematics matters and also it shows mathematians as strong, socially aware characters in the form of Yau. It also shows Perelman as a detached guru-type … which is probably also appealing for some young people. Something for everyone …

I had a brief conversation with Nasar when she signed my copy of “A beautiful mind”. She was pretty friendly. I don’t think she is a saint though. It was always fairly obvious to me that she wasn’t a pushover. She gathered a lot of information on Nash for a long time against his will. She documented a lot of things on Nash that he probably didn’t want in print.

75. **TheGraduate**
August 30, 2006

tg and geometer:

Also, thanks to you both for the link to the interview.

76. **Deane**
August 30, 2006

There are way too many half-truths, and extrapolations here for my taste. I’m surprised that Peter is not deleting more of this stuff.

The (mis)allocation of credit appeared in the Chinese press but is attributed not to Yau but to a Chinese mathematician named Yang Le, who as far as I know has no particular expertise in Ricci flow or the Poincare Conjecture.

77. **tg**
August 30, 2006

Dear TheGraduate,

You raise an interesting point about how Yau would be treated if hypothetically he were born and raised American. I’m honestly not sure, based on my own experience.

Perhaps a reasonable thought experiment would be to wonder how likely the Newyorker would run such a piece if it so happened that Yau was an American, born in another country X, and his activities were closely tied to being of X ethnicity, for various values of X. Again, I recommend reading the September notices of the AMS (available online) for another perspective on this and related issues (with regards to more "mortal" mathematicians).

78. **TheGraduate**
August 30, 2006

tg:

Do you mean the article "An Invisible Minority: Asian Americans in Mathematics" ? I have read that article.

79. **tg**
August 30, 2006

"Do you mean the article "An Invisible Minority: Asian Americans in Mathematics" ? I have read that article."

Yep. Although I suppose here is not the place to discuss it — as much as I'd like to.

80. **geometer**
August 30, 2006

tg:

according to Nasar's article Yang was indeed talking about allocation of credit, but Yau was standing next to him and confirmed what Yang said. And in any case, if Yau thought this allocation of credit was entirely unappropriate he could've distrubuted a statement (eg post it on the Beijing Math Inst site) to that effect, clarifying his position. He did not do that at the time, and the phrase about "50/25/30 breakdown" went arount the world.

BTW, speaking about "invisible minority". This article in the Notices hinges on the difference between Asians and Asian Americans. This may be a valid point as far as students/postdocs are concerned but for faculty it makes no sense. The absolute majority of tenured math faculty are citizens or permanent residents, so they are already Asian Americans, no matter where they were raised or educated.

81. **tg**
August 30, 2006

Dear geometer,

You might have wanted to direct your comment about Yang's remarks to Deane. I've only spoken about Cao's remarks at the IMU; I haven't thought to raise the issue about Yang, although it sounds interesting. In any case, as I've said before, whether Yau said these things or not is not relevant to my argument. My complaint is unfair focus on this man and his relationship to the Chinese. I'm sure that if Nasar wanted to, she could research any one she wanted and make a case that that person is scum of the earth.

You said:
"BTW, speaking about "invisible minority". This article in the Notices hinges on the difference between Asians and Asian Americans. This may be a valid point as far as students/postdocs are concerned but for faculty it makes no sense. The absolute majority of tenured math faculty are citizens or permanent residents, so they are already Asian Americans, no matter where they were raised or educated."

I'm not sure I fully understand your remark about the "invisible minority". There are plenty of non-American asians (e.g., mainland Chinese) who graduate from math PhD programs. They certainly have barriers too — but it's surprising to me how _few_ Asian Americans (given the advantages of citizenship and language) do so. I think it's this suprising fact that is what the author wants to focus on. The article is mainly about barriers to a tenure track position, I agree — but I still think it is relevant to faculty.

82. **HI**
August 30, 2006

I didn't think the article of Nasar and Gruber was necessarily disrespectful to Chinese, but what is clear from reading the comments here is that there are people who are eager to make a connection between the aggressive behavior of certain Chinese mathematicians with their nationality. As an Asian, though not Chinese, I am not happy to see those comments.

The fact is that it is not uncommon for successful scientists and mathematicians to have reputation of being aggressive or even nasty. I guess John Nash, who was the subject of Nasar's book is an example. But you don't see anyone who associates John Nash's personality with his heritage. How about Jim Watson? Carlo Rubia? It seems the connection is only made when the scientists/mathematicians are of certain ethnicities.

83. **geometer**
August 30, 2006

tg: Sorry for misplacing my post to Deane. Now you say: "My complaint is unfair focus on this man and his relationship to the Chinese. "

Okay, I don't think Yau-Tian fights and Chinese math politics are relevant to Nasar's story, but surely Yau (not anyone else!!) the one who singlehandedly created the controversity on the "who proved the Poincare Conj" issue and this is what the Nasar's article is mainly about. According to Yau's survey at arxiv (see last page) the Perel'man's argument is not written in complete detail, and Cao-Zhu papers give a first complete and detailed account, and that Cao-Zhu contribute original ideas not present in Perel'man's proof. This feels very different from what Hamilton, Morgan-Tian, Kleiner-Lott are saying. They do say that Perel'man's proofs are concise but they never doubt their completeness. In fact, in his ICM talk Hamilton said "the Perel'man proof is correct and complete", and Morgan-Tian, Kleiner-Lott give all credit to Perel'man, they said that no substantially new ideas were needed. To me it sounds like Cao-Zhu could not follow Perel'man's argument and found it easier to prove it differently. This is okay and happens all the time in math. However, when this happens people say "this is another, perhaps easier proof", and they get credit for the new proof, not the new result. (Even though Morgan in his ICM interview said he could not see what was so new and different in Cao-Zhu paper). Now you might say that this is a free country and Yau could express his opinion on any topic including Perel'man's proof, even though other experts say otherwise. Well, if Yau were nobody, people would not worry what he says. The trouble is that because (as I think) Yau is the most influential person in geometric analysis, what he says matters, and when he does something unethical, it hurts the geometry community. So this story is indeed about Yau. If he did not do what he did, there would be no controversity. And Nasar merely aired the controversity to the general public; most of what she wrote about Perel'man's story is no news to me, and I did not like Yau's survey the first day I saw it, and I was upset when I heard the "50/20/35 breakdown". Both events happened way before I read Nasar's paper.

84. **tg**
August 30, 2006

Dear HI,

“But you don’t see anyone who associates John Nash’s personality with his heritage.”

Absolutely correct, and that’s my point, don’t you see?

85. **TheGraduate**
August 30, 2006

HI:

Who here is eager to make a connection between Chinese mathematicians and their ethnicity?

If one were to crudely split the different comments into two groups, All I see are some people that say that the article is outright anti-Chinese and other people saying they didn’t think it was as negative as all that.

Most people are explicitly or implicitly saying that racism and nationalism are bad for mathematics.

Only outlier is ‘ordinaryamerican’ who had some stupid things to say but PW clearly put him in his place.

tg:

I think Asians are unrepresented at the faculty level in comparison to the number of Asians that are present at the graduate student level. I actually had this opinion before I ever set eyes on the September issue of Notices.

I don’t know why that is but I guess the September notices is the beginning of an explanation.

I think one important point that the article makes is that there is a lot of variety in Asia: South Korea, India, Vietnam, Japan etc

86. **tg**
August 30, 2006

Dear geometer,

Based on what I know, I can see why one could be upset about this priority fight. As I’ve been saying all along, even if I grant every fact that you cite (and I’m mostly inclined to), then I still have an issue with the article (and moreover the caricature that accompanies it).

Nasar and Gruber could have written about the controversy of correctness, saying that even a Fields medallist (Yau) was sufficiently unsure — in contrast to his learned colleagues (Morgan, Lott et al); math is about proofs, and sometimes what is a proof is debatable and in this case has caused heated discussion; Yau although raised in China is an American, having been trained in the US and considered among America’s best mathematicians, etc etc. Even keep most of the quotes that are there, but downplaying the whole China connection.

In that case, it would be clear that Yau-Chinese is not the connection, but rather Yau the scientist is. I’d have no problem with such a (boring) article. Nasar and Gruber cleverly and subtlely went just beyond the line.

Nasar is a crafty and skilled writer. She’s no dummy. I’ll assume the same for Gruber. They know Americans have a fear/distaste for China and their expansion. They know incorporating China into this would play well, and she exploited it. But, in my opinion, all people of Chinese descent are detrimentally affected by this approach — a small cost for her paycheck.

87. **tg**
August 30, 2006

Dear TheGraduate,

You say:
“I think Asians are unrepresented at the faculty level in comparison to the number of Asians that are present at the graduate student level. I actually had this opinion before I ever set eyes on the September issue of Notices.”

…and the number of graduate students are underrepresented in comparison to the number of undergraduates. I also agree with you about the point concerning many different “asian ethnicities”.

88. **geometer**
August 30, 2006

tg: okay, I see where you are coming from and mostly agree with what you are saying. I might add that what I disliked about Nasar’s paper is that it portaites Yau as no longer so great a mathematician whose best days are long gone, which is why he is playing politics. I think, Yau is truly great, but I also think some of the things he does hurt our field, and I wish he could stop (or be stopped).

89. **TheGraduate**
August 30, 2006

tg:

I guess what I am hearing from you is that you think that Nasar and Gruber should have avoided the China vs America issue. So I ask you, why? You say ‘all people of Chinese descent are detrimentally affected ‘ and so I ask you, how so?

Are you saying, there should be no articles about China? China is a country with 1.3 billion people. It is kind of hard to ignore.

Are you saying there should be no articles about the rise in Chinese power on the global stage. (Again, a billion people and hard to ignore.)

Are you saying there should be no articles in America where people express concern about the rise in power of China? (Given that the US is commited to go to war against China if it invades Taiwan, that might be wishful thinking.)

Anyway, the Chinese government is investing a lot in getting better quality schools and keeping better quality researchers in China … everybody knows this.

Actually while I'm on the subject of under representation … Asians are also under represented in US politics … I think if this situation was corrected the US would have a lot easier time dealing with China.

90. **tg**
August 30, 2006

Dear TheGraduate,

You ask:
"I guess what I am hearing from you is that you think that Nasar and Gruber should have avoided the China vs America issue. So I ask you, why? You say 'all people of Chinese descent are detrimentally affected ' and so I ask you, how so?"

Your questions are very valid. However, I have already said and repeated all that I really have to say multiple times. I guess all I have to say at this point is that reasonable people can come to differing opinions about the same issue!

91. **TheGraduate**
August 30, 2006

tg:

Okay. Fair enough. Basically I think that if this kind of stuff affects American-born people of Chinese descent then that has to do with the prejudice of the people that are affecting their lives.

The internment of the Japanese-Americans wasn't wrong because it was wrong to go to war with Japan or to bring up going to war with Japan or even to bring up the rising power of Japan. It was wrong because they were American citizens just like any other American citizens and deserved to be treated as such.

92. **Russian**
August 30, 2006

As a person of Russian decent and a lover of trigonometry with a limited exposure to the world of advanced mathematical studies, I observe with a surprise and discontent the tidal wave of negative reaction of Chinese geometry lovers to the article of Nasar and Gruber.

I do not know what criteria Mr. Woit is using to characterize someone's writing as racist and homophobic views, but I am amused at shear amount or futile discussion about number of Chinese PhD students and whining about being underrepresented, etc, etc, etc.

Nasar and Gruber, in my opinion, made several good points:
a. Some people can not accept the fact they exhausted their scientific potential and are trying to influence the scientific community thru maneuvers looking legitimate. Is not it the reason that many talented people in science world and academia leave being tired of politics, chase for higher place in hierarchy based not on talent but ability to develop connections, build coalitions?

b. It is Yau who turned the solution of the math problem into the matter of Chinese pride, no more, no less. I read in disgust the announcement of Xinhua News Agency English edition stating that Chinese scientists resolved a century-old Poincare conjecture. If Xinhua lied (not for the first time) it was responsibility of Chinese scientists to establish the truth.

c. I was especially disappointed with Yau taking lower road on a loner like Perelman. Perelman clearly has distaste for everything that is not related to the core value of every science, namely contribution to the favorite research field without consideration for success, monetary compensation, vanity, recognition, etc. Not so for Dr. Yau; not being able to contribute any significant work for the last twenty years, he it trying to reach fame utilizing thirst of the Chinese government for good publicity.

While it would not fair to characterize all Chinese based on dirty politics of Dr. Yau, it won't be inconsiderate to note that none of the Chinese geometry lovers were dare to criticize Dr. Yau. Is it something about introverted nature of Chinese culture that critique is accepted if only comes from inside and only from people within the community of certain statue or authority?

I have great respect for Chinese history and people, but that does not mean I have to agree with superficial speculations of Chinese
geometry lovers. I will tell them, it is not quantity that matters, it is quality. I will tell them, if you can not be at the front, let it go, let others go ahead with their ideas.

93. **werdna**
August 30, 2006

To Russian:

How do you know that "none of the Chinese geometry lovers were dare to criticize Dr. Yau"?

There has certainly been plenty of criticism of him on this forum, many of it from Tian supporters, I am sure. (Just ask Peter, who must be tired of deleting all those personal attack posts.) None of them is a "Chinese geometry lover"?

And please do not stereotype Chinese geometry lovers. The vast majority of them are busy doing mathematics and do not post here.

94. **TheGraduate**
August 30, 2006

I guess this issue is raising a lot of nationalism. China and America are geopolitical rivals. There is not much than can be done about that except maybe for Americans to try to understand China better and for Chinese to try to understand America better. I think those people who have connections both to China and America can probably make a big difference in this. Such people are more likely to be able to see what things people from both countries will find honorable and desireable.

95. **Peter Woit**
August 30, 2006

Russian,

There have been plenty of comments here from Chinese people critical of Yau, and I deleted many such others that were far more hostile.

Everyone,

Please stop with the discussion of nationalism/racism, etc. It's not going anywhere, has only a little to do with this story, people are just repeating themselves, and it hasn't been very enlightening. There's a reason I try and avoid discussing my opinions about political matters here: I find virtually all political discussion on blogs to be a complete waste of time. Most people love to argue their political opinions, very few pay much attention to what others have to say. You end up after a while with pure noise, and participating in it just wastes time and energy. So, please, if you want to discuss the Poincare story, fine, but try and stick to the facts and the mathematics, avoiding personal attacks on Yau, Tian, or anyone else, and you better have something really new and really interesting to say if it's on the nationalism/racism issue (and it should be related to Poincare).

96. **outsider**
August 30, 2006

I am not a professional mathematician, but it seems to me that a number of people here have an entrenched grudge against Yau and that some of these people also form their opinions based on the New Yorker for their "facts" and mathematics.
The critics of Yau here ascribe him certain vague disposition and views that he apparently holds. But it is not clear that this kind of attribution originates from the critics themselves, including the New Yorker, or Yau really holds them (please see the interview given by Cao for the ICM). What we then tend to have is basically a series of personal attack on his character, often based on hearsays and second-hand gossips. Whatever the nature of Yau's personal character, surely his critics are not saying that they are saints themselves? Not checking facts or distorting them and then proceed to attack the person's character is utterly deplorable. The best way to have a go at Yau is to do better maths than him, period.

I have a different take on the Poincare Conjecture.
In the press reports that I have read from China, Yau has often maintained that the Cao-Zhu paper was the finishing step of the works of Hamilton and Perelman. Likewise in the Cao-Zhu paper, they explicitly referred to the paramount importance of Hamilton and Perelman. All these are conveniently ignored by some people.

As an outsider looking at mathematics in history, there is nothing wrong with assigning percentages in the debate on Poincare. We may disagree with the allocation, but it is a reminder that major advances in mathematics are often built on the cumulative endeavor of many others. John Morgan, in his ICM talk, indicated that in the currency of the mathematics community, Perelman should be accorded with the Poincare proof. However, according to him, Perelman would not have done it without Hamilton and "vital" contribution from Yau. History will judge the allocation.
Perelman spoke of honesty in the New Yorker. But why does he not have the honesty of acknowledging the works of Cao, Zhu, Morgan, Tian and others in providing the rigor and details for his program? He provided sketches on some of his proofs and over the last 3 to 4 years has not prepared or able to produce rigorous proofs for his program (why Steklov could not help out?). I hope that I am not wrong in saying that sketches can not be regarded as proofs for the requirements of mathematics as a science. I think the works of those authors have given the ICM the added confidence to award Perelman the Fields medal and the mathematics community to accept Poincare has indeed been proved.

97. **MathPhys**
August 30, 2006

To me, Perel'man's behavior is due to reasons that have nothing to do with mathematics or with the math community. I think he reflects his feelings towards other issues on the math community, by not completing or publishing his proof, by declining to accept prizes, etc, etc.

98. **another outsider**
August 31, 2006

the fact that 3 different groups completed the Perelman program in roughly the same time suggests that it is routine work. I do not see any point in assigning more merit to the group that could publish a full proof a bit faster than others.

99. **Stevem**
August 31, 2006

"To me, Perelman's behavior is due to reasons that have nothing to do with mathematics or the mathematics community"

It is possible Perelman is having a "mid-life crisis" or has even had a nervous breakdown after years of very intense mental work on this problem. Events or personal/professional issues at the Steklov perhaps pushed him over the edge. Speculation of course but it would not surprise me if that indeed is the case.

100. **Russian**
August 31, 2006

Dear Peter,

Please do not be afraid of noise: unless it is "white noise" (I wonder why it is called 'white' and if there were political motives involved when giving the name), a noise signal carries useful information; and in the cosmological sense may carry information about other civilizations.

Also, I do not have any intension to criticize your blog policing policies; you can delete mine on any occurrence you want. Not being able to judge what you have deleted, I can judge what you left.

I have read an eclectic collection of essays from self-pitying geometry lovers, further called "polishers". These underappreciated, under-recognized and under-promoted people, some already with advanced degrees, some with such degrees in sight who are spending their lives polishing steps of the scientific ladder the giants are climbing by every day, and watching genius like Perelman running up the ladder in cheap and dirty boots.

After all, what a misfit this Perelman is. Even Steklov does not like him, not to mention the great news agency Xinhua. Frankly, he does not deserve to solve any important problems because:

1. He did not work with Yau on that problem, and what is even worse, does not care what Yau thinks or does
2. Instead of submitting for peer review 3,000 pages of polished proofs he published his solution on a website
3. He did not go to Marshall Putin and Russian news agencies for the purpose self-aggrandizement
4. He survives on milk and bread in lieu of green tea and rice
5. He is a weird guy in the middle of mid-life crisis struggling with mental disorders and long beard
6. He refused to accept Fields award while thousands upon thousands of "polishers" are ready to die for a glimpse of it.

Peter, unfortunately you can not avoid unavoidable. There are scientific giants in this world and there are pygmies. There are stars and dwarfs, supernovas and black holes. Some are jealous of others and their jealousy spills over in their writing. It is OK.

101. **Deane**
August 31, 2006

I for one am very grateful to all of the "polishers", including Morgan-Tian, Kleiner-Lott, Cao-Zhu as well as Ben Chow and his co-authors, who have devoted an enormous amount of effort to writing what are essentially expository books and papers. Although some of them may have embarked on their efforts hoping to find substantial gaps that they could fill and claim some credit, all of them completed their work knowing full well that they were performing a service to the community that would add very little to their own resumes.

I am also extremely grateful to Perelman for his brilliant contributions to mathematics and for delivering them to the community in a manner that has led to the most and best publicity that mathematics has ever had. I am saddened by Perelman's decision to withdraw from the mathematical community.

I am grateful to Yau for far too many things to list here but in particular for helping to create more drama in the Perelman-Poincare story and accentuating the publicity.

I am grateful to Overbye, Nasar, and Gruber for entertaining and well written stories that introduced the world to real mathematics and real mathematicians. Without them, there's no way Stephen Colbert would have ever done a piece on the Fields Medal and Poincare conjecture.

Have some of these people done things that I am dismayed by? You bet. But if I want to express any of them publicly, I will attach my name to what I write. I am disgusted by anonymous postings, both here and elsewhere, attacking one person or another. They are all acts of cowards and therefore not worthy of the slightest attention.

Deane Yang
Professor of Mathematics
Polytechnic University

102. **Deane**
August 31, 2006

Two more:

I am very grateful to John Ball, the IMU, and the local organizers of the ICM 2006 for an exceptionally well run ICM 2006 (based on reports I've gotten. I wasn't there) and a masterful job of publicizing the ICM and Fields Medals.

I am very grateful to Yau and David Gu for the graphics they provided for Overbye's article in the Times. Especially the rabbit, which generated such a strong reaction from many readers, including Stephen Colbert.

All in all, it's been a very memorable summer for mathematics. The challenge is figuring how to proceed from here.

103. **TheGraduate**
August 31, 2006

Anonymity has a long history in democracy. Benjamin Franklin used several pseudonyms. http://www.pbs.org/benfranklin/l3_wit_name.html

I mostly appreciate everyone that bothers to post especially if they are willing to respond fairly to challenges to their point of view and they avoid ad hominem … without a good faith basis between the conversationalists, there is really no point to discussing anything.

However, I sympathize with Peter because I bet he's probably often held responsible for a lot of what is on here.

I think that this case of Nasar and Gruber is very interesting and relevant contrast to string theory case. There are some superficial similarities … media attention, academic controversy …

The media seems to love this stuff lately. For instance, the retraction of papers that happened recently at Columbia. It seemed like some sort of minor dispute between a Professor and his former graduate student and yet it had international coverage.

104. **yd**
August 31, 2006

The New Yorker is reading every letter to the editor as proof that Manifold Destiny has really boosted the sales of the magazine.
Nasar is happily collecting her royalties. Perelman is quietly working on
the remaining 6 Conjectures while other mathematicians are
arguing on the internet. True mathematicians should go
back to work now.

BTW, does anyone know where to find the information on the
reaction from the math world when Beautiful Mind was published?

105. **ks**
September 1, 2006

> Instead, as I read on and on, I could not believe how she was turning this beautiful story, this "landmark not just of mathematics, but of human thought" into a petty, racist soap opera.

Maybe it would be helpfull to overcome this narcistic identification with everything that is chinese? I think this is a very dangerous attitude which reminds me somewhat in the nervous wilhelmian paranoia reigning in pre WWI Germany – a nation which had once been some european "tiger state" with empire ambitions.

The article is kind of a moral tale about use and misuse of power in the scientific community and about the dedication and drives of opposite characters within. It is written as some kind of tragedy involving two very strong main characters – one might guess that "Woit vs Lubos" would be a nice theme for a comedy. The contemporary western culture is absurd, not tragic.

The only aspect which is originally chinese in this play and which has to be attributed to its culture is Tians devotion to his teacher Yau.

PS. Writing a detailed biography of Perelman is likely pointless and would become the real soap ("why is he living at his mothers flat…") Perelman seems to try to live the life of a russian saint. So its about losing some properties of personality and not being obsessed with it.

106. **woit**
September 1, 2006

This topic seems to generate bad behavior here, including people thinking it is a good idea to post comments under multiple pseudonyms or other people's names. Since the signal to noise ratio is about zero, I'm also shutting off comments here.

U.S. Publication of Not Even Wrong

August 22, 2006
Categories: Not Even Wrong: The Book

Today a heavy box with copies of the U.S. version of Not Even Wrong arrived at my office, and I'm quite pleased the thing is finally being published in this country. It appears that Amazon has it in stock (see [here]()), the very old publication date they still have listed as "September 30" is incorrect. Presumably it should soon be available at fine book-sellers everywhere…

Update: Lubos has posted his usual slanderous review of the book on the Amazon site, and then presumably logged in from many different places to vote for his own review. Now it seems I get just one star instead of the two I got in the UK, since it seems I have "abandoned any integrity". As usual, he's very big on intellectual integrity. He lists as the first "embarassing error" in the book the Gev instead of Tev typo that was in the British edition, although he is well aware that, thanks to him, the typo was fixed for the US edition, which is the one he's reviewing. He's also paranoid and delusional, accusing me of "using various tricks to erase all inconvenient reviews".

Update: I've updated the NEW errata page to include the US edition, and also started a reviews and press coverage page.

Update: Since Lubos's review of NEW on Amazon has been deleted, he is now offering $20 to anyone who posts a one-star review of "the book with the black satanic cover", and manages to get Amazon to leave it there for at least two weeks. Yet another example of string theorist's belief in the "market-place of ideas", I guess.

Comments

1. **Kasper Olsen**
   August 22, 2006

   😉

2. **c.w.**
   August 22, 2006

   congrats, peter! i finished the U.K. version a few weeks ago and loaned it to a friend who had just watched the 3-part NOVA series.

3. **Ron Avitzur**
   August 22, 2006

   Congratulations!

4. **David**
   August 23, 2006

   Congratulations. I really enjoyed the UK edition. Well done.

5. **D R Lunsford**
   August 23, 2006

   Congratulations, Peter! I well remember reading your original paper "String theory: an evaluation" while at work back in 2001. I was literally dancing in the aisles that someone finally got it and had the balls to speak up. Well done.

   -r

6. **comentator**
   August 23, 2006

   Congratulations!!

7. **Shan Gao**
   August 23, 2006

   A very intriguing book! It should be also read and understood by the string theorists. Congratulations!!
   By the way, I will soon publish a new book Quanum Motion, which may also imply that string theory is incomplete, or even wrong. See my research website http://www.quantummotion.org/.

8. **King Ray**
   August 23, 2006

   Peter, congratulations! I've had it preordered on Amazon for months, so hopefully it will arrive soon. Keep up the good fight. The string may really hit the fan now, with all the publicity the US printing will bring in the media. Brace yourself!

9. **ksh95**
   August 23, 2006

   Screw it, I'll contribute to the Buy-Peter-A-porsche fund.

10. **TheGraduate**
    August 23, 2006

    Congratulations. Hope to see it in Barnes and Noble or Borders some time soon.

11. **Christopher**
    August 23, 2006

I've received my copy of your book. Looks good. Congrats.

12. **Jody Trout**
August 23, 2006

Congratulations, Peter!

I received notification from Amazon today that my copy is in the mail. I look forward to reading it! Keep up the good work.

Cheers,
Jody

13. **nigel**
August 23, 2006

Congratulations. I bought the British version and I like the fact that it is not varnished with any hype or speculation. When are you going to write a textbook? Now you have media attention, I hope you get contracts for other books lined up and get non-returnable advances. Try to get a contract to either edit an encyclopedia of non-biased QFT research, or to edit a journal. Unless you do something like that, it probably won't ever happen, or else will be a flop.

14. **Rickkkkkkk**
August 23, 2006

Good deal and congratulations

15. **Who**
August 23, 2006

I just checked Amazon's physics best sellers list and the book was #5

(just above Brian Greene "Elegant Universe" which was #7)

They update hourly, so if you look now it might be different

http://www.amazon.com/gp/bestsellers/books/14545/ref=pd_ts_b_ldr/102-4540543-7840144

This best seller list is what you get by narrowing
Books > Science > Physics.
It is still pretty broad.

On the same list where N.E.W. is #5, I saw "Warped Passages" as #31, and "Fabric of the Cosmos" #39 and "Road to Reality" #44.
However many of the other titles were textbooks.

16. **Tommaso Dorigo**
August 24, 2006

Well done Peter!

I am glad there is a US version, I also will be most happy to go to my usual Borders and thump my fist at the information desk if I don't find your book on the shelves… Just for the sake of it. Of course it'd be much better if they had it on a bookstand on top of a pile in front of the entrance…

Cheers
T.

17. **Vicky**
August 24, 2006

My copy is on the way. My birthday is next week and I have taken the day off from work, so I will probably spend a bit of the day reading. I will post a review on Amazon once I have finished the book.

Congratulations!

18. **Who**
August 24, 2006

As of 1 PM eastern today, the Woit and Smolin books were #14 and #15 on the Amazon physics bestsellers list.

19. **TheGraduate**
August 24, 2006

Who said:

"As of 1 PM eastern today, the Woit and Smolin books were #14 and #15 on the Amazon physics bestsellers list."

I casual reading of the list suggests that they are being pushed down the list by textbook sales. A large number of the books that are higher on the list are textbooks.

20. **Alain-Paul**
August 25, 2006

How about a frensh-transleted version?

I can read english, it's just for the comfort.

Alain-Paul.

21. **D R Lunsford**

August 25, 2006

I would be willing to do the translating.

-r

22. **woit**
August 25, 2006

Alain-Paul,

I haven't heard anything recently, but Dunod has bought the French rights to the book and I believe that they have hired a translator, but don't know what the state of that is.

23. **Alain-Paul**
August 25, 2006

O.K. for the response.

If the edition Dunod has bought the french right, i think the project is on its way.

Great!

p.s. Sorry for some mispelling of the previous message (even in the mail !).

Alain-Paul.

24. **Tony Smith**
August 25, 2006

Peter, will you be doing any book-signing and/or talk-show tours for the USA edition?
How about CSPAN TV, which often does serious books?
If you do, please post your schedule.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

25. **nigel cook**
August 26, 2006

Professor Lubos Motl has reviewed N.E.W. yet again on Amazon. Amazon had to delete his previous reviews of N.E.W. which falsely claimed that string theory is based on evidence similar to that which evolution is based on. He really is a character. I know that at least two readers complained to Amazon that he was making inaccurate and irrelevant propaganda for string theory disguised as a book review. He now complains of N.E.W.'s 'embarassing errors'. (Is embarrassing generally spelt as embarassing by most Americans, or only by Czech-born string theorists?)

26. **Who**
August 26, 2006

nigel,
double R is correct and usual, but plenty of Americans (not just the person you mentioned) misspell it with a single R.

27. **Who**
August 26, 2006

I hope someone besides myself is interested in how sales are going as indicated by the Amazon "general physics" bestseller list.
Earlier I referred to the raw "physics" list, but that has a lot of specialized textbooks among which books for wide audience are scattered, so narrowing it down to what Amazon calls "general physics" is helpful

http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

I will give rankings of some other wide audience books for perspective. As of 10 AM eastern Saturday 26 August the "general physics" bestseller list included:

Elegant Universe #5
Brief History of Time #9
Trouble with Physics #12
Not Even Wrong #13
Warped Passages #18
Road to Reality #21
Cosmic Landscape (Susskind's book) #76

28. **D R Lunsford**
August 26, 2006

Motl's review is the usual sophistry. I reported him to Amazon.

-r

29. **Lubos makes me puke!**
August 26, 2006

Nigel- I believe he is still "Assistant" Professor Lubos Motl. I hope he is still a long way from a full professor or Harvard has gone down the tubes.

30. **chand**
August 26, 2006

Concerning translations, maybe Lubos would be interested in helping to translate Peter's book in Czech? Apparently, he did a good job with Brian's "The Elegant Universe"…

31. **be careful**
August 26, 2006

Lubos might notice that you wrote Gev and Tev instead of GeV and TeV!!!

32. **Visitor**
August 26, 2006

Lubos Motl – bringing the spirit of Leninism to theoretical physics.

33. **TheGraduate**
August 26, 2006

"It would be very foolish to throw away the right answer on the basis that it doesn't conform to some criteria for what is or isn't science" — Leonard Susskind

Apparently IDers are very fond of this quote. I was watching book tv (C-SPAN2) today and one of the pro-ID authors of 'Traipsing into Evolution" repeated this quote.

A search of the quote on the internet finds other IDers equating ID and string theory and claiming a double standard and that legal challenges to ID should apply to string theory.

34. **dan**
August 27, 2006

Peter,

would you mind a wiki entry, along with summary of your arguments against string theory?

do you think lubos' string theory rebuttals to your criticisms to be substantive?

35. **Thomas Larsson**
August 27, 2006

The only substantive critique in Lubos' latest post is apparently that the US edition of NEW has a satanic black cover. Hard to argue about that.

A few posts before, Lubos speculates that God is an American citizen. With that definition of God, it makes sense to claim that string/M-theory is the language in which God wrote the world. The world doesn't seem to care much, though.

36. **Alphonse Warakomski**
August 27, 2006

Reading the popular literature on string theory has often left me with two ideas that the theory is nothing but mathematical masturbation and that physics under string theory has become the new metaphyics. Thanks for the expose, I read most of it last night after buying it yesterday.

37. **D R Lunsford**
August 27, 2006

Lubos' slander has now been removed from the review list

-drl

38. **John Rennie**
August 27, 2006

I see Motl's review has been removed (before I got a chance to read it). I'm not sure this is a good thing as it feels like censorship to me. If it was obvious to Amazon that his review was a rant it should be obvious to Amazon's customers too.

39. **stupid**
August 27, 2006

'If it was obvious to Amazon that his review was a rant it should be obvious to Amazon's customers too.'

Maybe that's exactly why they reported it, and why Amazon deleted it? If you want to see Lubos' stringy propaganda, take a look at his blog, but try keep a sick-bag handy.

40. **John C**
August 27, 2006

Shame that Motl's review was removed from Amazon, it was great reading. One phrase that stuck out in my mind was the one where he said something like "anyone interested in seriously pursuing theoretical physics should not read this book as they will get confused". Does he really have such a lowly opinion of the next generation of theoretical physicists and their ability to think and discriminate for themselves? Or did he unintentionally reveal that "String school" is really about indoctrination, and not about discovery?

41. **Peter Woit**
August 27, 2006

John Rennie,

I'm in general very much in favor of promoting Lubos and his views, to the extent that I've suggested to several science journalists that they should do a story on him. As far as I know, none of them have taken my advice. However, Amazon has an interest in not allowing its reviews sections to be used for obviously dishonest personal attacks, so they were quite justified in removing his.

The review that was deleted was the same one that was up on the amazon.uk site for a couple months before they decided to delete it. It's also still up on the amazon.ca site, where it's the only review, and may be responsible for the fact that the book seems to be selling much less well through that site. Lubos has made it clear that he is willing to stoop to just about any level of dishonesty to keep people from reading what I have to say about string theory. In the case of the UK review, he knew that only giving the book one star would make it likely that his review would be deleted, so he gave it two stars. In the case of the US review, he couldn't help himself and used just one star, which led to the quicker deletion there.

42. **Peter Woit**
August 27, 2006

Tony,

Right now there are a few places I'm tentatively scheduled to appear and talk about the book: Oct. 6 at the Cafe Scientifique in DC, Nov. 6 at the Princeton U-Store, Nov. 16 at the University of Minnesota, and some time in the winter at the NY Academy of Sciences (with Lee Smolin). When these are firmed up, I'll try and make up a web-page with more details.

43. **Peter Woit**
August 27, 2006

dan,

Not sure what you mean by a "wiki entry". There are already things on Wikipedia about me, about string theory, and about the criticisms of string theory, although I haven't looked at any of these recently.

No, I don't think Lubos's criticisms are substantive. When he first put them up on amazon.uk, I wrote something about them, see

http://www.math.columbia.edu/~woit/wordpress/?p=401

44. **Frank B**
August 27, 2006

A couple of specific comments on the book (UK ed):

1. On page 111 higher dim theories are traced back to Kaluza 1919. It is fairly well known that G Nordström (1881-1923) published a 5 dim unification of EM and gravitation (scalar version) already in 1914 (Phys Z). A brief summary in modern notation is given by F Ravndal (Scalar gravitation and extra dimensions, Proc. of the Gunnar Nordström Symposium on theoretical physics, eds., C Cronström & C Montonen, Helsinki, 2003: 151-164.)

2. On p 61 it is claimed that QED is a "complete theory". In what sense is it complete? If *complete*, should QED eg predict bound states for the H-atom?

3. On p 191 it is claimed that "if a theory is non-renormalizable it is inherently incapable of ever reliably calculating anything". Fermi-theory did make some successful predictions though non-ren. Indeed, within effective-field theory approaches it is known that renormalizability is not a necessairy requirement for extracting measurable quantities below some energy scale. One question is whether quantum-gravity as an effective field theory will ever bottom out in the form of a renormalizable theory — if not, there might still be a fully sensible (non-perturbative) theory.

A message of the book is that people should concentrate on symmetries and their representations, which sounds good, but it is a vague suggestion that hardly lures people away from ST or anything else. Anyway, NEW is a smooth read; 1st part informative, second part more about opinions than ideas.

Regards FB

45. **woit**
August 27, 2006

Frank,

Thanks for the comments.

1. Yes, I'm a bit weak on the history of extra dimensions, should have mentioned Nordstrom. In the book I do point out that my history was often incomplete, concentrating on explaining the origins of the names associated to ideas.

2. The sentence in question just says that QED was "complete" in 1929, by which all I meant was that the fields and Hamiltonian were known.

3. Yes, the discussion of renormalizability is an oversimplification. Various people have complained that I didn't explain the effective field theory point of view on non-renormalizable interactions. Maybe I should have, but the book already contains a lot of indigestible material for most people, this would have added a bunch more to try and write something accurate and that would be accessible to people who don't already know this story.

Explaining the details of what I have in mind about representation theory would really not be possible in this kind of book. And, the last thing I wanted to do was write something hyping some very speculative ideas.

46. **Dan**
August 27, 2006

Hello Peter,

There's a brief paragraph of criticisms of string in wiki, which talks about the landscape and lack of experimental prediction. There's a lot it doesn't mention such as SUSY-breaking.

I am curious as to whether you think Strings should continue to funded & research, at least at present levels if not more, should LHC discover evidence of SUSY particles or extradimensions or both. — obviously, should LHC fail to find such particles then the answer would be no. Also, there is research observation of dark matter, which could be an axion particle which is predicted by string theory. Axions, SUSY particles may be indications string theory is the right path?

47. **You**
August 27, 2006

"I'm in general very much in favor of promoting Lubos and his views, to the extent that I've suggested to several science journalists that they should do a story on him. As far as I know, none of them have taken my advice."

Here’s an old article on Lubos,
http://www.nytimes.com/2001/05/01/science/01ARCH.html?ex=1156824000&en=b187905670e458eb&ei=5070

48. **Thomas Larsson**
August 28, 2006

I am curious as to whether you think Strings should continue to funded & research, at least at present levels if not more, should LHC discover evidence of SUSY particles or extradimensions or both. — obviously, should LHC fail to find such particles then the answer would be no.

This is interesting. Do string theorists in general think that string theory funding should be reduced if no evidence of SUSY or extradimensions are found at the LHC?

49. **woit**
August 28, 2006

Dan,
Axions have nothing in particular to do with string theory, and observation of them wouldn’t necessarily tell us anything about string theory.

If the LHC finds new physics, whether it be supersymmetry, extra dimensions or whatever, lot of people will be jumping into whatever area it is, and there will be plenty of funding for this. String theorists will have to make the case that string theory has something to say about this new physics. Whether they can do this plausibly depends on what the LHC finds…

50. **Peter Orland**
August 28, 2006

Frank B asks,

On p 61 it is claimed that QED is a “complete theory”. In what sense is it complete? If *complete*, should QED eg predict bound states for the H-atom?

The answer is YES. Without a potential-energy approximation scheme, this is hard. Such a scheme exists, however, and works well (in relativistic physics, potential energies are a fiction). For example, the Lamb shift is predicted by QED. Bound state eigenvalues and eigenfunctions are
analytically calculable.

This is so despite the likelihood that QED is probably a free field
theory with no cut-off.

51. **Peter Orland**
August 28, 2006

P.S. I didn’t mean real QED is free. I only meant that a cut-off is
necessary at some high energy scale.

52. **Who**
August 28, 2006

Update: … now offering $20 to anyone who posts a one-star review of “the book with the black satanic cover”, and manages to get Amazon to leave it there for at least two weeks. … the “market-place of ideas”, I guess.

this book fits that description
http://www.amazon.com/gp/product/list/0375708111/ref=pd_ts_b_3/102-4540543-7840144?ie=UTF8&n=14560&s=books

53. **dan**
August 29, 2006

Would you object to string theory if it claims to be a branch of mathematics, rather than a branch of science? Would you object of string theorists re-name themselves string mathematicians?

Briane Green’s position is that since axions, SUSY, and extradimensions are fundamental predictions of string theory, that if they are discovered, continued research in strings would be justified.

Dan,
Axions have nothing in particular to do with string theory, and observation of them wouldn’t necessarily tell us anything about string theory.

If the LHC finds new physics, whether it be supersymmetry, extra dimensions or whatever, lot of people will be jumping into whatever area it is, and there will be plenty of funding for this. String theorists will have to make the case that string theory has something to say about this new physics. Whether they can do this plausibly depends on what the LHC finds…

54. **Peter Woit**
August 29, 2006

dan,

Yes, I would object if string theory claimed to be a branch of mathematics. Most of what string theorists do has little to do with research mathematics, i.e. creating new mathematics. The landscape is not mathematics, for instance. There certainly are parts of string theory that have led to new mathematics, and some of that kind of research is part of mathematics. There already are quite a few people who work on it in math departments.

55. **dan**
August 29, 2006

Do you think if experimental research were to come to have some precise values for string theory, such as the precise scale of SUSY-breaking, that knowing these hard facts could help string theory be more predictive?

Yes, I would object if string theory claimed to be a branch of mathematics. Most of what string theorists do has little to do with research mathematics, i.e. creating new mathematics. The landscape is not mathematics, for instance. There certainly are parts of string theory that have led to new mathematics, and some of that kind of research is part of mathematics. There already are quite a few people who work on it in math departments.

56. **Peter Woit**
August 29, 2006

dan,

String theory really says nothing about supersymmetry breaking, which is one of the main reasons it is not predictive. Knowing the scale of SUSY breaking wouldn't help. People often assume it is just high enough that superpartners will be seen at the LHC but not the Tevatron. This assumption in no way helps to get any predictions out of string theory.

57. **Who**
August 30, 2006

I noticed this morning that a well-known ranter had posted a derogatory ** review of The Trouble with Physics which was not taken down when I looked in some 4 hours later but by then TwP and NEW were #3 and #5 on the amazon Physics bestseller list
http://www.amazon.com/gp/bestsellers/books/14545/ref=pd_ts_b_ldr/102-4540543-7840144

and they were #2 and #4 on the amazon General Physics bestseller list
http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

(which has more wide audience books and fewer specialized textbooks and manuals)

this is the first time I have seen both books so high in the bestseller listing and it caused me to wonder if the heavy-breathing crank-telephone call style ** review saying how bad TwP is–could that have actually had an effect and helped trigger a burst of orders for the books? Most likely just a random fluctuation, but still it was strange to see sales climb sharply right after, in effect, receiving crackpot hate-mail.

58. **Frank B**
August 30, 2006

Peter Orland,

1. indeed, I understood's Woit's "a complete theory" as suggesting a notion of a complete theory (no *gaps*). But, how do you get the H-atom from QED as a perturbative theory whose content is given by the Feynman rules? To my knowledge no one has extracted non-trivial non-perturbative solutions from QED, but I may be wrong of course. My point would be that for QED we know how to successfully *cheat* (eg insert bound states), but for strange theories like ST we do not have good clues for cheating, more exacting math may in fact just lead further astray (when not guided by physical insight)..

Woit,

2. on p 266 it is said that QM "loses much of its mystery" when one works in the language of group theory. What *mystery* gets resolved this way? The main *mystery* commonly associated w/ QM is that of the measurement problem (*wave collapse*) — how is this defused talking groups?

3. On p 55 it is claimed that spin is "inherently a quantum mechanical notion". However, is not spin a feature of SU(2), and thus it does not depend on QM as such? One can also introduce (*uninterpreted*) spin variables in classical mechanics/field theory.. Indeed, the classical/quantum distinction is somewhat blurred from the formal point of view since Schrödinger QM can formally be described as a classical field theory (*canonical QM*). This sort of *blurring* is of interest, I think, when one tries to think about what quantum gravity might mean (is *quantization* well defined?).

Finally a comment on *ostracizing* ST from physcs and math — Is there a problem slotting it in mathematical physics then? To me it seems that much ST-related stuff de facto appears in math phys type journals.

Regards FB

59. **Peter Woit**
August 30, 2006

Frank B,

What I had in mind as a "mystery" of QM, was why observables are no longer c-numbers as in classical physics, but are given by self-adjoint operators acting on a Hilbert space. This is what you get when you have a unitary rep of a Lie algebra, and it is in this sense that representation theory explains something mysterious about QM.

Classically, angular momentum has to do with SO(3) symmetry, but is a continuous variable, and you don't see the difference between SO(3) and its double cover SU(2). Discrete spin quantum numbers are a purely quantum phenomenon, and there you do see the difference.

60. **D R Lunsford**
September 1, 2006

Peter, you may find this interesting:

http://www.physics.gatech.edu/people/faculty/finkelstein/Emptiness031215.pdf

This is very original thinking about the nature of QM and its relation to relativity.

-drl

61. **Peter Orland**
September 5, 2006

Frank B Says:
August 30th, 2006 at 4:41 pm

1. indeed, I understood's Woit's "a complete theory" as suggesting a notion of a complete theory (no *gaps*). But, how do you get the H-atom from QED as a perturbative theory whose content is given by the Feynman rules? To my knowledge no one has extracted non-trivial non-perturbative solutions from QED, but I may be wrong of course. My point would be that for QED we know how to successfully *cheat* (eg insert bound states), but for strange theories like ST we do not have good clues for cheating, more exacting math may in fact just lead further astray (when not guided by physical insight)..

Frank,

Feynman rules are not the entire story of field theory, and certainly not QED. Bound states in quantum field theory cannot be obtained systematically inm perturbation theory. In principle, they can be obtained by the Dyson-Schwinger equations (a set of exact integral equations for Green's functions). In practice, the DS equations are
often impossible to solve without some approximation. For QED, such an approximation exists, which is the Bethe-Salpeter equation, which reduces in turn to the Schroedinger bound-state equation.
For more a more complex theory, like QCD, there are proposals, which in my opinion are dead wrong (the approximation, not the DS equations).

So, once again, yes, QED in principle gives all the masses of bound states of atoms, positronium, etc. The caveat to all this is that there must be a cut-off somewhere in the ultraviolet, or the theory is trivial.

62. **Frank B**
September 6, 2006

I am not intending to press these points, but they are related to the bigger issues of what one means with *intrinsic quantum mechanical* and *complete theory*, so I add some further (not very deep ..) comments.

1. Since group representation theory is applied to so called classical mechanics (CM) too (representations on phase space via symplectic/canonical transformations) it is not clear how group representations as such (even involving spin) distinguish between CM and QM. Unitary transformations contain symplectic transformations, and in this respect QM is (formally) a special form of CM. The relation between state, observable and probability is in this view the distinguishing (interpretative) feature. One reason why this issue may be more than of formal interest is that quantum gravity may require a deeper characterization of what it means to be *quantum*.

2. I am aware that one can get approximations from QED that are interpreted e.g. as the Schrödinger equation for electron in the H-atom (relativistic case unsettled?), but there may be disputes about exactly what assumptions are made along the road (I know only the broad outlines). Another point is that in order to be *approximations* we should be able to give their error bounds, otherwise the approximations are based on faith. However, and this is the main point, in physics it is common to work with incomplete or even inconsistent theories (such as classical electrodynamics + charges) successfully when we have learned their domain of applicability. In this way physics differs from *pure mathematics* (which immediately reminds one of the Einstein quote: "As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality"). Physics needs not just experiments, but genuine physical insight (such as the principle of equivalence) in order to evolve. To me the book by Woit has brougth up this issue (which is not restricted to string theory): Do mathematical *speculations* replace physical insight? Has the development of theoretical physics reached a stage where [in lack of new experiments/or because of a possible *desert* — of course, one should not forget the exciting findings in astronomy/cosmology, no desert there] the only remaining lead (whatever it is worth) is in mathematical constructs? On this history may teach us some lessons but it does not foretell.

63. **Who**
September 8, 2006

a propos the main topic—US publication of N.E.W.
the book continues to sell really well, judging from the amazon general physics bestseller list

As of today Friday 8 September at 1:20 PM pacific, its standing and those of several other wide-audience books for comparison were:

#1 TwP
#2 NEW
#7 Elegant Universe (Greene)
#8 Road to Reality (Penrose)
#13 Brief History of Time (Hawking)
#38 Warped Passages: Unraveling the Mysteries of the Universe's Hidden Dimensions (Randall)
#40 The Cosmic Landscape: String Theory and the Illusion of Intelligent Design (Susskind)
#88 Parallel Worlds: A Journey Through Creation, Higher Dimensions, and the Future of the Cosmos (Kaku)

the top two (TwP and NEW) were also #2 and #3 on the entire PHYSICS bestseller list, number one being a book about the effect of music on the brain.

The complete physics list contains books on specialized subjects as well as "general" physics books—so it has a wider range than the general physics list. General physics best sellers, besides the usual Greene and Hawking fare, tend to be first-year college textbooks.

so it looks like the US publication is going along successfully, the Woit and Smolin books may not be selling like hotcakes but they are selling like Freshman Physics textbooks. And in September, the start of the Fall semester, that can't be too bad.

A Castle For Mathematicians

August 24, 2006
Categories: Uncategorized

The American Institute of Mathematics was founded in 1994, with financing from John Fry, the Silicon Valley businessman responsible for Fry's Electronics. The Fry's store in Palo Alto is quite remarkable, containing everything a Silicon Valley geek might need, with a huge selection of potato chips and computer chips. In recent years, AIM has been running a wide variety of workshops, at a temporary location called the AIM Research Conference Center (ARCC), which is basically in back of the Palo Alto store.

Last month, the City Council of Morgan Hill approved plans for construction next to a golf course of a huge castle that will provide a permanent home for the ARCC (for a news story about this, see here). It will be modeled on the Alhambra in Spain, occupy 167,000 square feet, contain a "gourmet-industrial kitchen with master chefs from a San Francisco seafood restaurant and a Napa Valley resort", and much else besides. Fry himself is closely involved in the design of the castle, which is rumored to cost over $50 million, and planned to be ready for occupancy in 2009. More details about this are here, and there's even a video of what the castle will look like.

Comments

1. **David**
   August 24, 2006

   AIM is an interesting organization that sponsors quite interesting workshops, all of which (including those past) are described on their website (AIMath.org), many with references. Many of these workshops are of interest to some types of scientists as well as mathematicians. As an example, they just had one on Low Eigenvalues of Schroedinger and Laplace Operators.

2. **andy.s**
   August 25, 2006

   aaahh crud. I was hoping they would get the Winchester Mystery House.

3. **Stefan**
   August 25, 2006

   Great link, but I wonder what differentiates AIM from AMS?

4. **csrster**
   August 25, 2006

   Tasteful.

5. **anon**
   August 25, 2006

   Very nice video, except that the turrets are square shaped, whereas cylindrical designs are much better at resisting bombardment since impact impulses get arched over a much larger area in an attack on a cylinder than on a flat wall, which reduces damage.

6. **anonymous**
   August 25, 2006

   They should have based it on Uraniborg, Tycho Brahe's castle/research institute.

7. **anon**
   August 25, 2006

   (I meant of course that the turrets are 'rectilinear', not 'square'. Must always use correct terminology!)

8. **ObsessiveMathsFreak**
   August 25, 2006

   This certainly won't do much to improve the Ivory Tower sterotype of mathematicians.

9. **Dick Thompson**
   August 25, 2006

   Competition for the Institute for Advanced Studies? I hope is has lots of little cafe's tucked away in overlooked nooks. Since Fry seems to have a communal model of doing math versus the IAS lonely genius model, he might want to encourage what has worked before in that mode.

10. **Thomas Love**
    August 25, 2006

    The cafes are necessary since, as Erdos put it: "Mathematicians are machines for turning coffee into theorems". After deriving results in private, the lonely genius must communicate his/her results to the community. Besides, community is necessary to provide stimulation.

11. **A.J.**
    August 25, 2006

    The trouble with turning coffee into theorems is that it increases entropy: we can't turn unwanted theorems back into coffee.

12. **hack**
    August 25, 2006

    How much good mathematics has been accomplished while looking out over a golf course?

13. **Doug Natelson**
August 25, 2006

How much good mathematics has been accomplished while looking out over a golf course?

Well, since the grad college at Princeton overlooks a golf course, I'd guess there's a pretty good chance that much of John Nash's early work was done in that environment.

14. **Chris**
August 25, 2006

AJ said, "The trouble with turning coffee into theorems is that it increases entropy: we can't turn unwanted theorems back into coffee."

That is what comathematicians are for!

15. **TTT**
August 25, 2006

http://aimath.org/arccstaff.html

couldn't help but wondering – how did they manage to attract so many females in the Bay Area? a new trend? women going after Mathematicians these days:)

16. **Dick Thompson**
August 26, 2006

***
TTT said:
couldn't help but wondering – how did they manage to attract so many females in the Bay Area? a new trend? women going after Mathematicians these days:)
*****

All those movies about nutty genius mathematicians, and, way important, the TV show NUMB3RS. Mathematicians are hot!

17. **Jonathan Vos Post**
August 26, 2006

Numb3rs is a hit because of good writing, good directing, good acting, and their Math Advisor, Dr. Gary Lorden, Executive Director of Mathematics, in Caltech's Physics, Astronomy, and Mathematics division.
http://www.math.caltech.edu/people/lorden.html
He is a noted expert in Statistics; I took advanced probability from him; he's written letters of recommendation for me. The Chairman of Math is the great Barry Simon, I.B.M. Professor of Mathematics and Theoretical Physics. http://www.math.caltech.edu/people/simon.html
In all our wide-ranging discussions, String Theory has never come up. Although Caltech has major String Theorists (as I've posted before, including undergrad classes) it remains a small part of a much larger physics, astronomy, and math effort, and does not seem (to me) to affect the mainstream of research and teaching.

18. **Carl**
August 27, 2006

The most amazing thing about Fry's are the architecture and decor. The second most amazing thing is the range of electronics sold there. So are they going to sell Peter's new book?

I bought Not Even Wrong($21.56) and Smolin's new book($20.80) at Barnes and Noble. They didn't have them in stock, but could mail them to me when available. Since the order ($47.34) was over $25, the shipping was free. Years ago B&N started a card program that requires you to pay $25 per year to join. I have avoided shopping there ever since. They still have the program, but I held my nose and paid my money.

If Peter comes up here to Seattle on a book signing I'll get him to sign it. I am a bit ashamed to admit that I once got a professor of physics, Gregory Benford to sign a science fiction book of his that had been remaindered. But I was a cheap physics grad student at the university at the time.

And I started a blog. I had to do this in order to thank a grad student for publicly more or less endorsing my theory of the quarks and leptons. Not that I claim to understand her comments on M theory. Maybe after reading Peter's book I will.

19. **TheGraduate**
August 27, 2006

To Carl:

To be fair the Barnes and Noble, you don't have to get the card if you don't want it. But you get 10 percent off every purchase if you do.

Of course you would need to buy more than 250 dollars of books per year for this to be worth it.

20. **Pyracantha**
August 27, 2006

Rosebud….

21. **Carl**
August 27, 2006

Regarding Barnes & Noble's $25 per year, I buy maybe 300 books per year. I easily spent $1000 a year there; the card would be a bargain for me. But it seems to me that what they really did was to raise all their prices about 5% and issued the card in the hope of crowding out the competition. I think that that is monopolistic and I won't have any part in it.

Some people don't like the privacy issues, but I carry cards for all the local supermarkets in my otherwise thin wallet. There's plenty of room in there for a free B&N card. In the meantime, I find the selection of books at other bookstores quite satisfactory. If I were Peter, I would refuse to sign books at B&N stores.

The Trouble With Physics

August 28, 2006
Categories: Book Reviews, Not Even Wrong: The Book

I've just finished reading Lee Smolin's new book The Trouble With Physics, which should be released and available for sale very soon. It's a great book, covering some of the same ground as mine, but with significant differences.

This won't be a usual sort of review, since I'll mainly concentrate on discussing the parts of Smolin's book that I found most interesting, and my perspective here is kind of unique, having spent a lot of time writing about many of the same subjects that he covers. I will offer some capsule consumer advice: if you have any interest at all in what is going on these days in fundamental physics, you should buy and read both books. If you really are on a tight budget, and your main interest is in the relation of mathematics and physics, you should get mine. If your main interest is in quantum gravity or the foundations of quantum mechanics, you should get Smolin's. His is more appropriate for someone with little background in this area, mine contains some significantly more demanding material which requires some expertise to appreciate.

What most fascinated me about Smolin's book is the personal story behind it. He was a graduate student at Harvard during the same years that I was an undergraduate there, and describes well that place and time. The standard model had just been formulated a few years earlier, and experimental confirmation was pouring in. Many of the people responsible for the standard model were there at Harvard, and there was more than a bit of justifiable pride and arrogance. Smolin was of a philosophical bent, and initially put off:

The atmosphere was not philosophical; it was harsh and aggressive, dominated by people who were brash, cocky, confident, and in some cases insulting to people who disagreed with them.

He studied the philosophy of science and was very struck by Paul Feyerabend's *Against Method* (there are also has some amusing tales of later personal encounters with Feyerabend). Feyerabend's philosophy of science has been described as "anarchistic"; he sees no one "scientific method", but science as a very human activity, in which all sorts of different tactics are used to make progress towards better understanding. Smolin recognized that much as he would prefer a more deeply philosophical approach, it was the much more pragmatic tactics of people like Coleman, Glashow and Weinberg, who wouldn't be caught dead talking about the nature of space and time, or foundational problems of quantum mechanics, that was what was really having success.

Smolin begins his book by explaining what he (and I) see as the most important fact about the past thirty years of theoretical particle physics research. We're in a historically unprecedented situation, with virtually no progress being made on the fundamental problems of particle physics for a very long time, despite huge efforts. In his description, the field has "hit a wall"; I like to describe it as a victim of its own success. The standard model is just too good. It's too hard to find an experimental result that disagrees with it, and too hard to come up with theoretical advances that will address some of the things it leaves unexplained. Smolin sees the source of the problem in the field's insistence on sticking with a way of doing science which worked until 30 years ago, but now has become dysfunctional, with string theory only a symptom of the underlying problem. He writes:

I have mentored several talented young people through crises very similar to my own. But I cannot tell them what I told my younger self – that the dominant style was so dramatically successful that it must be respected and accomodated. Now I have to agree with my younger colleagues that the dominant style is not succeeding.

Elsewhere he writes:

My hypothesis is that what's wrong with string theory is the fact that it was developed using the elementary-particle-physics style of research, which is ill-suited to the discovery of new theoretical frameworks… This competitive, fashion-driven style worked when it was fueled by experimental discoveries but failed when there was nothing driving fashion but the views and tastes of a few prominent individuals.

Smolin was a student of Stanley Deser's, and during his graduate student years supergravity was a field that was just taking off. He describes getting to know Peter van Nieuwenhuizen and Martin Rocek and being offered a chance to get into the field at the ground floor, one he passed up because he couldn't believe that the kind of lengthy algebraic calculations they were doing could give real insight:

It was like being offered one of the first jobs at Microsoft or Google. Rocek, van Niewuwenhuizen, and many of those I met through them have made brilliant careers out of supersymmetry and supergravity. I'm sure that from their point of view, I acted like a fool and blew a brilliant opportunity.

Smolin didn't join the Stony Brook supergravity group, but found that he could make a place for himself in the physics community working on quantum gravity, but using particle physicist's methods:

… an easy opportunity opened up while I was a graduate student, which was to attack the problem of quantum gravity using recent methods developed to study the standard model. So I dould pretend to be a normal-science kind of physicist and train as a particle physicist. I then took what I learned and applied it to quantum gravity.

Smolin ended up with a post-doc at the new ITP in Santa Barbara, which luckily was running a program on quantum gravity that year. His career tactic almost didn't pay off:

One day, as we were waiting for the results of our applications, a friend came by to tell me that I was unlikely to get any jobs, because it was impossible to compare me with other people. If I wanted a career, I had to stop working on my own ideas and work on what other people were doing, because only then could they rank me against my peers.

The most powerful parts of the book are the chapters entitled *How Do You Fight Sociology?*, and *How Science Really Works*. They give a detailed and clear diagnosis of the problematic way string theory research is being conducted, and decisions are being made about who deserves a job. Smolin has an insider's point of view, particularly because he himself worked on string theory:

… during the years I worked on string theory, I cared very much what the leaders of the community thought of my work. Just like an adolescent, I wanted to be accepted by those who were the most influential in my little circle. If I didn't actually take their advice and devote my life to the theory, it's only because I have a stubborn streak that usually wins out in these situations. For me, this is not an issue of "us" versus "them," or a struggle between two communities for dominance. These are very personal problems which I have been contending with internally for as long as I have been a scientist.

So I sympathize strongly with the plight of string theorists, who want both to be good scientists and to have the approval of the powerful people in their field. I understand the difficulty of thinking clearly and independently when acceptance in your community requires belief in a complicated set of ideas that you don't know how to prove yourself. This is a trap it took me years to think my way out of.

Smolin gives many examples of the "groupthink" behavior of the string theory community, while characterizing string theorists as "almost all more open-minded and self-critical and less dogmatic than they are *en masse*." He describes string theorists as:

… supremely confident both of the truth of string theory and of their superiority over those unable or unwilling to do it. To many string theorists, especially the young ones with no memory of physics before their time, it is incomprehensible that a talented physicist, given the chance, would choose to be anything but a string theorist.

…Anyone who hangs out with string theorists encounters this kind of supreme confidence regularly. No matter what the problem under discussion, the one option that never

comes up (unless introduced by an outsider) is that the theory might simply be wrong. If the discussion veers to the fact that string theory predicts a landscape and hence makes no predictions, some string theorists will rhapsodize about changing the definition of science.

Some string theorists prefer to believe that string theory is too arcane to be understood by human beings, rather than consider the possibility that it might just be wrong.

Smolin finds in the string theory community a sense of entitlement and disdain for anyone who works on alternatives to the theory, with major string theory conferences never inviting people who work on alternatives to speak. An editor from Cambridge University Press told him that one string theorist said he would never consider publishing with the press because it had put out a book on LQG (I see why their publishing my book was out of the question…). At string theory conferences Smolin would be asked "what are you doing here?" or told "It's so nice to see you here! We've been worried about you." Some friends explained to him that if he wanted to be considered part of the string theory community he had to work not just on string theory, but on the particular string theory problems that were fashionable at the moment.

One problem for physicists trying to get tenured positions that Smolin mentions is that most universities now require letters from 10-15 people evaluating their work, with a small number of negative evaluations sufficient to sink their chances. If you're working on something other than a mainstream topic, finding 10-15 people who can comment knowledgeably on your work can be impossible. He describes string theorists as mostly submitting the same two or three research proposals. This narrow concentration on a small number of problems is defended by some senior theorists as a "disciplined" approach, one that will more surely lead to progress than encouraging people to pursue a variety of different research directions.

Very recently, Smolin sees things changing:

Until last year I had hardly ever encountered an expression of doubt from a string theorist. Now I sometimes hear from young people that there is a "crisis" in string theory. "We have lost our leaders," some of them will say. "Before this, it was always clear what the hot direction was, what people should be working on. Now there's no real guidance," or (to each other, nervously) "Is it true that Witten is no longer doing string theory?"

One can quantify this new situation by noting that there have been virtually no heavily cited new papers during the past few years, except perhaps for the KKLT one that is part of the landscape story.

Smolin notes that many string theorists (including himself) have often been ill-informed about the exact state of knowledge concerning crucial conjectures about string theory. One example he discusses in detail is that of the finiteness of multi-loop string amplitudes. The state of the subject is that one knows how to precisely formulate them and can show lack of divergences only up to two loops (this is due to the work of d'Hoker and Phong). At higher genus d'Hoker and Phong have a conjectural definition, but have not yet been able to show that divergences cancel. Few string theorists seem to be aware of this, and some of them react with great hostility and shower with insults anyone who mentions this issue (as I've done here on this blog).

There's much else of interest in Smolin's book, including a lot of material about what he sees as promising ideas in quantum gravity, discussion of research on the foundations of quantum mechanics, and a chapter on "seers", people doing original work on foundations. These include 't Hooft, Penrose, and many others less well-known.

While I agree with just about all of what Smolin has to say about string theory, my own background is different and I see promise in very different lines of research than he does. I'm much more skeptical than him about our ability to get useful experimental data on quantum gravity, and see questions about quantum mechanics rather differently. My prejudice is that, lacking experimental guidance, the thing to do is to try and better understand the mathematical structures underlying the standard model. In the past, better physical models have gone hand in hand with deeper mathematics, and I'll bet this will continue to be true in the future. Quantum mechanics has deep connections to representation theory, a part of mathematics that unifies many different subfields. It seems likely to me that a better understanding of quantum mechanics will come from better understanding representation theory and its connections to physics.

There's a lot of other sorts of material in the book that I haven't discussed, and I strongly recommend that people read the whole thing. It's very, very good, and anyone interested enough to follow this blog will find it highly rewarding.

Comments

1. **Aaron Bergman**
   August 28, 2006

   Well, if I had a free copy….

   Anyways, regarding

   Anyone who hangs out with string theorists encounters this kind of supreme confidence regularly. No matter what the problem under discussion, the one option that never comes up (unless introduced by an outsider) is that the theory might simply be wrong.

   I can't say I know every young person in the field, but I've talked to a fair fraction, and I have to say that with a few notable exceptions, I don't see that sort of dogmaticism anywhere. Saying that string theory might be wrong isn't a particularly interesting observation, so I can see why it doesn't come up, but I've rarely met someone who doesn't think that string theory could be the wrong theory of quantum gravity.

2. **Jimbo**
   August 28, 2006

   Peter paraphrases Smolin as saying, "being offered a chance to get into the field(SUGRA) at the ground floor, one he passed up because he couldn't believe that the kind of lengthy algebraic calculations they were doing could give real insight"…

   Einstein would agree whole-heartedly with Smolin's reluctance, and this goes to the heart of the problem in theoretical physics in my opinion. In the absence of experimental evidence for SUSY and/or a driving paradigm, string theorists have chosen to hack their way thru the jungle with a `mathematics machete', obscuring any hope of real physical insight, and venturing off into the surreal world of mathematical physics, which with few exceptions, has seldom yielded any deeper insight into nature's secrets.
   Now, we are paying dearly for this obsession with math, with no end in sight. Only the LHC can level the playing field, as the recent astro-observations of Dark Matter did with the alt.theories of DM, MOND & TeVeS, which are now history.

3. **Stefan**
   August 28, 2006

   "My prejudice is that, lacking experimental guidance, the thing to do is to try and better understand the mathematical structures underlying the standard model."

   I agree with you 100%!! So, may you kindly do the undergrads a favour and tell us what those specific topics are.

   Stefan

4. **Kea**
August 28, 2006

Well, I must say that this is very entertaining.

5. **nigel**
August 28, 2006

Stefan, see http://www.math.columbia.edu/~woit/repthy.html for general background course on relevant maths and see http://arxiv.org/abs/hep-th/0206135 for some specific physical ideas explaining the Standard Model tentatively using such ideas. At top of page 51, the Standard Model particles are obtained with electroweak symmetry properties. This should be impressive if you are interested in the links between Standard Model and advanced mathematics without resorting to extra dimensions.

The taste in ideas for extreme abstraction in particle physics is set by symmetry principles more recently, and laws of nature further in the past. This is not the same approach that you use if you are dealing with relatively simple phenomena where you might try to guess a full mechanism and write down the relevant basic equations straight off, and then solve those equations.

If you take nuclear physics, where you have the shell model and the liquid drop model of the nucleus, you can use visual analogies to help formulate a model that makes checkable predictions, which are semi-classical. But with particle physics, it looks more promising to search for symmetry principles and other abstract laws than to guess a sem-classical model.

I mean, suppose you were crazy and guessed that the particle is like a piece of string, and then you found that to get it to work just as an ad hoc model you needed to make make it extra dimensional string, and then you still failed to predict anything with it for 20 years? How embarrassing!

6. **wkrn**
August 28, 2006

From your semi-review I do not understand what is the main point of the book (I do not have it).
In my opinion The Trouble with Quantum Gravity is that it is disconnected from experiments: sociological difficulties are not the problem, but a consequence of it.

The Trouble with High Energy Physics is different: experiments have not told us what we hoped, and we are waiting for the verdict from LHC. So, better to discuss after LHC.

7. **TheGraduate**
August 28, 2006

Peter:

How likely do you think it is that the LHC might actually contradict string theory and end its dominance?

8. **TheGraduate**
August 28, 2006

Peter:

I had one more question. What kind of impact has string theory had on modern mathematics? I have lately been becoming more and more impressed with the number of branches of mathematics that have been impacted by string theory.

9. **Fabien Besnard**
August 28, 2006

Thanks for this review, this all seems very interesting.

A sidenote : Feyerabend emphasized on the sociological aspect of science, but more : he saw scientific truth as a sociological phenomenon. In this respect the situation in today's physics clearly disproves Feyerabend's views since string theory is by no way acknowledged as a scientific truth, despite the sociological dominance of string theorists.

A question : does anyone know about another historical example of the "groupthink" Smolin describes ? I can think of the "N-rays" affair but it did not went that far, no lasted for so long.

10. **D R Lunsford**
August 28, 2006

Thanks for that Peter, I look forward to this book. I get impatient with Smolin because he's such a nice guy, and it seems to me what physics needs is some "tough love" and a few bitch slaps now and then – but it's obvious he is a sincere researcher and representative of all that's good in science.

For contrast, one should poke over to Motl's blog and read the page upon page of incoherent, basless ranting there. It's mind boggling.

-drl

11. **D R Lunsford**
August 28, 2006

RE the effects of sociology in physics, check this out:

http://www.theaustralian.news.com.au/story/0,20867,20192903-2703,00.html

I've long understood that the problem is not just string theory.

-drl

12. **Peter Woit**
August 28, 2006

Graduate,

The LHC can't contradict string theory, since string theory predicts nothing about what it will see. If the LHC sees something new and exciting, most theorists will be trying to figure that out, paying less attention to string theory.

The question of the impact on math of string theory is complicated. I did write something about this in my book, although that only scratched the surface. One complication is that much of the most important math that has come out of string theory research is really results from 2d QFT calculations.

13. **Stefan**
August 28, 2006

Nigel,

Thanks for the links. I'm having a look through the Rep Thry paper; seems quite interesting. Do you know of any (introductory) texts you think are worth studying to get up to speed in this area?

Peter,

If you recall our discussions some time before, i.e. on gauge fields and Dirac operators, I have been combing through Amazon.com for relevant introductory texts. The best ones I could find were:

"An Introduction to Dirac Operators on Manifolds" — Jan Cnops

and

"Operator Methods in Quantum Mechanics" — Martin Schechter

If you have any added suggestions please let me know.

Among the ideas you place forward, studying "geometric quantization" holds, atleast from my neophyte vantage point, (potentially) fundamental insights into QM and QFT, and may even (with the passage of time and careful consideration) displace string theory as the most important approach to fundamental physics research… I do think IT'S THAT IMPORTANT!
I also wonder if von Neumann's QM work would be helpful to study here? Do you also feel Jouko Mickelsson's work is important enough for the afore-mentioned programme to warrant careful study? [No offence intended to the author, but the book's cost is WAY too high for your average student to afford… unless it's worth all my three months' summer savings. ;-)]

Thanks.

Stefan

14. **Michael Bacon**
August 28, 2006

Here is a link to an NPR "debate" between Smolin and Greene:

http://www.npr.org/templates/story/story.php?storyId=5670911

Smolin's new book was the "trigger" for this piece.

15. **TheGraduate**
August 28, 2006

Peter,

I will definitely be looking for your book in the book store and I will investigate what you have to say about the connection between mathematics and string theory in more detail.

I have been becoming more and more surprised at what little evidence there is for the validity of string theory.

Are they really basing all this on mathematical and historical coincidences?

It sounds a bit limp to say that a line of reasoning is good because a particular model has nice properties you had not expected it to have. Is this really the extent of their reasoning?

16. **Thomas Larsson**
August 28, 2006

Stefan, if you want to have an idea what's in Jouko's book, have a look at his papers from anno dazumal, e.g. this, this, this, this, and this.

However, note that there might be some problems, see e.g. this and this.

17. **Thomas Larsson**
August 28, 2006

Sorry, missed a double quote. The links should be
this,
this,
this,
this,
this,
this, and
this.

18. **SFB**

August 28, 2006

We should be calling it STRINGTHEOLOGY.

19. **tg**
August 28, 2006

I'd like to thank Michael Bacon for the link to the Smolin-Greene interview. Having listened to the interview now, I'd like to post my thoughts on it as a layman to the subject whose academic life has only been tangentially touched by string theory. I, like many other mathematicians (I presume) have no vested interest in whether strings are bogus or not — I'd just like to know some arguments in both directions, just for my general scientific edification.

Frankly, Michael Greene was far more polished (understandably, he's had practice) and presented a more compelling point of view on general grounds. Smolin, in my opinion, was at times longwinded in his presentation, which hurt his debate.

Smolin has a difficult task to begin with. People, and scientists aren't really interested in unclear claims about what doesn't work, or "negative campaigning". Smolin stresses the lack of experiments. Greene says things take time. (What's wrong with that?) Moreover Greene asserts the mathematical consistency of things, which not knowing the specifics, sounds like reasonable evidence. These are things, Greene says, keep people inspired.

Smolin asserts that string people are holding back non-string people. He seems embarassed to make this claim directly, but rather hints at it in various ways. Greene counters that his _own_ graduate students sometimes work on non-string physics. Personally, I don't find it surprising that one field in a subject looks down at other fields, or consumes resources. It's annoying to be on the wrong side of the tracks, I know, but a priori, I have to admit this doesn't mean to me that the ideas in the field per se are faulty.

So I summarize that string theory is an with high degree of mathematical consistency but which clearly needs experimentation. Its difficult math and difficult physics, so therefore time is necessary to come to an appropriate conclusion.

Is there something I'm missing? I'd be glad to be better informed on this debate…

20. **Peter Woit**
August 28, 2006

TheGraduate and tg,

Both my books and Smolin's contain very detailed answers to your questions, I don't want to try and rewrite a lot of this here. As for the Smolin-Greene radio show, it's really not possible to sensibly debate these issues in a few minute long radio program for the general public. About all you can get off in that time are quick sound bites which will be highly misleading in one way or another. The quick answer to tg's summary is that:

1. string theory does not have a high degree of mathematical consistency, actually no one knows exactly what the theory is.

2. the problem with string theory is not that it needs experimentation. The problem is that it can't predict anything, at any energy, including being unable to retrodict things being studied in current particle physics experiments.

3. the theory is more than 30 years old, has been intensively studied for 22 years, with the result that it now looks a lot less likely you can get physics out of it than people thought 22 years ago. The problem is not that progress is slow, it is that progress is negative.

21. **Peter Woit**
August 28, 2006

Stefan,

I don't know the books you mention. One nice short introductory text on representation theory is Graeme Segal's lectures in Carter, Segal, McDonald, Lectures on Lie groups and Lie algebras.
Unfortunately I don't know a really good introduction to geometric quantization, in its relation to representation theory. There is a recent book by Kirillov, called "Lectures on the orbit method", which may be the closest thing.

22. **JPL**
August 28, 2006

Hi Aaron:

Saying that string theory might be wrong isn't a particularly interesting observation, so I can see why it doesn't come up, but I've rarely met someone who doesn't think that string theory could be the wrong theory of quantum gravity.

This observation of yours, however, sounds particularly interesting! If it never comes up how can you be so sure that you never met anyone who thinks it is an impossibility, I mean, that String Theory could simply be wrong? Telepathy? Groupthink? Pray let us know…

23. **Doug**
August 28, 2006

The title of the book refers to what's wrong with physics. The failure to recognize string theory's shortcomings, because of its dominance, no matter how disturbing to skeptics, and no matter how unfair to competitors, is only a small part of the story of what's wrong with physics.

On the Science Friday show, Smolin was just getting into the real problem, when Flato cut him off to take a call from a lady suggesting that what is needed is "thinking out of the box," but before that he was addressing Green's statement about the consistent calculations.

As Green explained how that extensive testing the consistency in the calculations of string theory, and its consistency with the established concepts of past physics, shows that the "theory comes through with flying colors every step of the way and keeps us thinking that things are at least headed in the right direction," Flato turned to Smolin and asked, "Well, Lee, what would be wrong with that, if things are working like that?"

Smolin said:

> If you really put quantum mechanics together with the description of space, then we know, from general considerations, that the notion of space should disappear. Just like the notion of the trajectory of a particle disappears in quantum mechanics, …the same thing should happen to space and the geometry of space.

I’m sure he was referring to background independence here, but he didn’t get to explain it very well, just that string theory doesn’t address this very directly, while other approaches do.

My question is, Peter, does he explains this well in the book, and does your work address this problem?

24. **Peter Woit**
August 28, 2006

Doug,

There’s a lot in Smolin’s book about the issue of background independence, not much in mine. In general I’m mostly writing about particle physics, Smolin is much more concerned with quantum gravity, and he certainly writes well and clearly. I don’t have any particular wisdom on this subject myself.

And please, if you want to discuss your favorite ideas about quantum gravity, don’t do it here.

25. **TheGraduate**
August 28, 2006

Peter,

I understand your reluctance to summarize complex arguments that you’ve already put years into expressing better elsewhere and I do promise that I will read your account in full when I get the opportunity.

At the moment though, I am a bit curious about whether the pro-string theory argument can summarized into the phrase: “The mathematical coincidences are too surprising to be chance.”

Could you say whether this is very, very roughly the crux of the pro-string theory argument?

26. **woit**
August 28, 2006

TheGraduate,

The pro-string theory argument for unification is mainly that the theory provides a quantum theory of gravity.

The argument about mathematical coincidences is more that an argument that calculations show that things are happening in string theory for reasons that are not understood. This is just an argument that there is something going on in string theory that is not yet understood. That’s an argument for further research into the theory, but not an argument that it is going to give you a useful unified theory.

27. **dan**
August 28, 2006

““Is it true that Witten is no longer doing string theory?”

well is it true?

have you or lee read lubos top 12 results from string theory?
http://motls.blogspot.com/2006/06/top-twelve-results-of-string-theory.html

28. **anonymous**
August 28, 2006

TheGraduate,

some mathemacians indeed say that string theory is an activity which produced some very beatiful and important mathematical ideas, which justifies this activity. Whether you want to call that activity ‘physics’ is another matter…

29. **Aaron Bergman**
August 28, 2006

This observation of yours, however, sounds particularly interesting! If it never comes up how can you be so sure that you never met anyone who thinks it is an impossibility, I mean, that String Theory could simply be wrong? Telepathy? Groupthink? Pray let us know…

Conversations in social situations rather than in researchy situations.

30. **JC**
August 28, 2006

I wouldn’t be surprised to see another “string theory” style sociological phenomena popping up decades later in particle physics, if string theory falls by the wayside in the near future. Seems like these sorts of things happen frequently enough every few generations or so.

31. **TheGraduate**
August 28, 2006

Peter,

Ah okay I think I see now. And I think I’ve read elsewhere that you definitely don’t agree that it provides a quantum theory of gravity.

Anonymous,

I guess I figure they are saying they have a degree in physics and they get paid by physics departments and therefore they are doing physics … probably a good enough argument 99% of the time

32. **David**
August 28, 2006

Stefan,
I really liked Schechter's book. You may be interested in some more recent work that follow's Schechter's. All of this can be downloaded from the following websites:
Barry Simon, Math Dept, Caltech
Gerald Teschl, Math Faculty, U of Vienna (both research articles and Lecture Notes [actually book preprints]),
Susanne Teschl, U. of Applied Sciences,Technikum Wien (thesis).
You can find all of these by using Google with the person's name.
Best,
David

33. **Egbert**
August 28, 2006

My copy of NEW just arrived today. At the back it says the URL of your blog is
http://www.math.columbia.edu/~woit/worelpress

Apart from that it looks good.

About the supreme confidence of string theorists – from the experience that I've had, it seems that there really are some amazing things that string theory has done. Counting rational curves is a good example; AdS/CFT and the dualities are impressive too, and it sure looks like it has something to do with quantum gravity, getting the entropy A/4 right for at least some black holes, and having Einstein's equation in it and so on. These things are, quite frankly, too amazing to be just a coincidence.

The problem is that these things have been interpreted as evidence that string theory is the one true theory of the universe, but in fact it is evidence that there is some underlying self-consistent mathematical structure, which may or may not be a theory about physics. All of the signs seem to point towards the conclusion that it is not a theory about physics, but is rather a mathematical structure which relates 2d CFT to various gauge theories in various manifolds.

Still, though, the string theorists, like Ed Witten, like to oversimplify and say that they think string theory has produced too many fascinating insights to be "wrong".

34. **ark**
August 28, 2006

To Dan and Anonymous
I am one of those whose work was selected into top 12 results in string theory (e.g. read #6 in this list) mentioned by Dan and, indeed, I have to say that, for instance, work by Grisha Perelman would never be completed (most likely) should he in his 3 Fields medal winning papers not use some basic string-theoretic results obtained in 80ties. This fact alone is sufficient for justification of string theory existence. But, surely, I can mention many other things, e.g. read my hep-th/0608117 and references therein. Clearly, sooner or later the dust will precipitate and there will be a lots of good bits and pieces of work for grabs by everybody who likes to work more than to talk.

35. **D R Lunsford**
August 28, 2006

Peter, why did you delete my comments?

-drl

36. **woit**
August 28, 2006

drl,

Because they had nothing to do with the posting, which is a review of Lee's book. I'm tired of explaining this to people, but this is not sci.physics, it's not a place for people to discuss their favorite ideas about physics. I don't have the time, energy or interest to moderate something like that.

I'm pretty annoyed about this comment section today anyway, I've been spending a huge amount of time deleting comments from idiots who want to denounce Yau or Tian, using endlessly changing pseudonyms. I really, really don't have time for this these days.

Egbert,
I noticed that typo and told the publisher about it. I've also set up my web-site so it responds properly to the incorrect URL.

37. **D R Lunsford**
August 29, 2006

Peter, I agree with you about that, but my comments were to the point and on topic, or so I thought. Relax, you are on target and have nothing to fear.

-drl

38. **hongbao zhang**
August 29, 2006

Peter,
Have you a free electronic copy of it? If you have, could you send it to me? We want to translate this great and intriguing book into Chinese so that more young guys in China could read it.:)

Thanks in advance.:)

My email: hongbaozhang969@hotmail.com

39. **MathPhys**
August 29, 2006

tg,

Do you know what the problem with string theory is? It's Edward Witten. If you think Michael Green is polished, you should listen to Witten.

When Witten speaks, he's extremely convincing. It's very difficult to refute an argument that he makes. It's very difficult to resist following in his footsteps.

Is that a good thing or a bad thing? Should non string theorists argue that having Witten on string theory's side gives the latter an unfair advantage? I think they should.

40. **Thomas Larsson**
August 29, 2006

Stefan and Peter,
When Stefan referred to Jouko Mickelsson's work and a way to expensive book, I assumed that he meant Current algebras and groups, which AFAIK is the only book he has written. Since I have read (although perhaps not understood) both the book and the papers I linked to, I can claim that there is considerable overlap.

41. **nitin**
August 29, 2006

Hello Peter

Your short review sure got me very interested in Smolin's latest book! I have ordered both his and yours, and I am eagerly waiting to read them. hehe.. It's very likely that I will be among a very few from my country, Mauritius, to read them (maybe Sanjaye Ramgoolam, a "string theorist" countrymate, at Queen Mary, will read one or both of them). Last time I tried convincing a friend about Physics, I ended up giving him Penrose's "A Road to Reality" as birthday present!

Cheers.

42. **tg**
August 29, 2006

Dear MathPhys,

"If you think Michael Green is polished, you should listen to Witten."

In fact, I have heard him speak (live) — but I can hardly compare. One was a technical talk to a group of mathematical string theory people, and the other was a popular talk.

43. **Peter Woit**
August 29, 2006

hongbao zhang,

Sorry, but I don't even have an electronic form of the book, and couldn't distribute it for free if I did. Anyone interested in acquiring the rights to publish a translation in another language needs to contact the people at Jonathan Cape, my deal with them was that they hold world-wide rights to publish the book.

Mathphys + tg,

The Science Friday show involved Brian Greene, not Michael Green. Brian is a lot better at this than Michael, I would think.

44. **Michael Bacon**
August 29, 2006

My take was that Smolin did well given the format. In any event, it was refreshing to listen to them discuss the issues in a civil way. Lubos would do well to draw the appropriate lessons.

45. **Gina**
August 30, 2006

Peter, is it possible to state the main (say, 10-15) points for the "case against string theory" with 4-5 sentences on each?

This will be very helpful. Please consider doing it.

It will be useful to

a) separate the stricktly scientific points from more sociological and philosophical points

b) to separate points that say that (*)"string theory is not (yet) successful" from those saying that (**) "this and that aspects of the theory are fishy" from those saying that (***) "string theory is not the right direction for dealing with "final theory/grand unification", from those saying that (***) "the whole endeavor of final theory/grand unification is misguided".

c) To hint if physics requires more back-tracking or it is just string theory that is problematic.

Thanks!

46. **a**
August 30, 2006

dear Gina, I think that the main scientific problem is: since we cannot directly probe quantum gravity, a useful theory of quantum gravity must predict something at the lower energies where we can do experiments. Strings allow something like 10^500 different possibilities: this seems practically equivalent to allowing everything and predicting nothing.

47. **nigel**
August 30, 2006

Stefan,

I hope Peter will produce an introductory textbook on Representation Theory and Particle Physics.

Gina,

Your idea would I fear produce a list of string theory claims with the same boring label 'uncheckable speculation' beside each.

‘Extraordinary claims require extraordinary evidence.’ – Carl Sagan.

You can see plenty of extraordinary claims in string theory (it solves almost all the big problems of unification, quantum gravity, the nature of particles, black holes). You don’t see any stringy evidence, let alone extraordinary evidence, and nobody expects to find much.

The string theory failure has some weak precedents in science: check the detailed history in peer-reviewed physics journals on the “Vortex Atom” and “Aether” (both the subjects of intricate mathematical speculation and wild claims of ad hoc success from mathematical physicists including Kelvin and Maxwell, who both died firmly believing flawed theories).

However, string theory is more dangerous. At least Kelvin and Maxwell’s ideas could later be checked by experiment. String theory deliberately speculates about practically uncheckable phenomena (Planck scale unification, etc) so remaining safe from experimental refutation, so becoming a religion:

‘Whatever ceases to ascend, fails to preserve itself and enters upon its inevitable path of decay. It decays … by reason of the failure of the new forms to fertilise the perceptive achievements which constitute its past history.’ – Alfred North Whitehead, F.R.S., Sc.D., Religion in the Making, Cambridge University Press, 1927, p. 144.

48. **RA**
August 30, 2006

“We’re in a historically unprecedented situation, with virtually no progress being made on the fundamental problems of particle physics for a very long time, despite huge efforts.”

That’s a convenient and shaky rewriting of history upon which to build your foundation.

‘Extraordinary claims require extraordinary evidence.’ – Carl Sagan.

That goes both ways.

49. **Peter Woit**
August 30, 2006

Gina,

At this point, I’m way too busy, and writing another explanation of what the problems are with string theory isn’t at all something I want to spend time on. The article I wrote back in 2001 is still a good short version of the argument, all it is missing is a discussion of how things have gotten much, much worse for string theory since then, because of the landscape.

The issues involved here are pretty complicated, and I don’t think short sound-bites, or me countering people’s “10 quick reasons why string theory is great” with “10 quick reasons why string theory doesn’t work” is going to be very enlightening. Partisans of one point of view or the other aren’t going to be convinced by this, and people who want to seriously understand the issues and make up their own minds should read both the pro-string theory point of view put forward in several books, and the other side of the story, as explained in my book and in Lee’s. I do believe that the problem is not just string theory, but more generally the idea of supersymmetric grand unification, these issues are discussed extensively in the book.

50. **Peter Woit**
August 30, 2006

Lubos,
While, unlike you, I’m not banning those who disagree with me from posting comments here, I’m not going to tolerate and will continue to delete any comments like your last one that attack people other than me.

51. **Gina**
August 30, 2006

Thanks for considering my suggestion and the comments, Peter
(and thanks nigel and a for the 2 items.)

Certainly my suggestion was not meant to replace reading your book or Smolin’s but it could be helpful for me to understand what is the essentials, e.g. while reading these books or the elegant books on the other side.

Your review on Smolin’s book (which is of the same size as what I would like to read) is very personal/philosophical almost like a gathering of two veterans on the same side of a battle but not so useful to understand the essentials.

Is the critique of string theory is similar to the critique on biologist for not understanding/finding cure for cancer? or is it stronger.
Is the idea that string rather than point particle can, in principle,
lead to a “grand theory” a-priori senseless, or just not yet successful, or reasonable-to-start-with but by now clearly a failure?
10^500 looks fishy but is 10^500 possibilities really that bad?

I think the main reason for me to be suspicious with “string bashing” is that it did not lead (yet) to interesting science: namely
to scientific papers (not popular reviews and books). Why is that?

52. **Lubos Motl**
August 30, 2006

Dear Peter,

you completely missed my point. My point was not to attack other people than you. It was, on the contrary, meant to prove that you are the #1 moron on this crackpots’ discussion forum. 😉

Best
Lubos

53. **Peter Woit**
August 30, 2006

Gina,

Again, for the short version, I recommend reading my 2001 article. Yes, 10^500 possibilities really is that bad. All indications are that it makes it completely impossible to ever extract a real prediction from the theory, which is deadly.

The situation of string theory is very different than that of cancer research, an analog would be if current cancer treatments not only didn't help at all with the disease, but made it much worse.

"string-bashing" by itself doesn't lead to interesting new science, except in the sense that encouraging people to stop working on a failed idea and look for something else to do may have a positive effect. Lee is one of the leaders of a very active research program that is working on new and different ideas, and he has published a long list of scientific articles on this. I have my own ideas about alternatives to string theory, have written much less about this. For some of what I have written see my long 2002 paper on the arXiv.

54. **Peter Woit**
August 30, 2006

Lubos,

Actually I did get that that was the point of your comment. But, sorry, if you want to write comments here about what a moron I am, you have to avoid at the same time attacking other people as morons, since I'm not going to allow that.

55. **Tom Killick**
August 30, 2006

I am not a physicist. I am an engineer. It has always seemed to me that postulating un-testable hypotheses is more the domain of religion or philosophy than of Physics. I would like to draw you attention to work being done by Charles Francis that is evolutionary, exciting and that does belong in the realm of Physics. This work claims to have bearing on dark matter, the age of the universe and more. It is also eminently testable and appears to explain currently anomalous data and makes specific predictions about future data.
I am intrigued by work being done by Charles Francis for much the same reason as you concentrate in your review of Smolin's book on the areas that most interest you, the personal story. I went to high school (in fact a very prestigious private catholic boarding school) with Charles Francis the late sixties and early seventies. I can unequivocally say that he was the most brilliant mathematician and logical thinker I have ever met. At sixteen he could produce elegant, concise and original proofs that allowed me, other classmates and his teachers to begin to understand the power and beauty of mathematical physics. I know he went on to Cambridge and Birkbeck college in London to finish a number of degrees. He is a very eccentric individual which has allowed him to focus on solving what he calls "the really important problems" for the last 40 years. His eccentricity has also isolated him from much of what I assume to be the hubris of modern physics.
His paper which in his characteristically un-self effacing way is titled

"Does a Teleconnection between Quantum States account for Missing Mass, Galaxy Ageing, Lensing Anomalies, Supernova Redshift, MOND, and Pioneer Blueshift?"

56. **Gina**
August 30, 2006

Here is my a priori take on this before reading any of these books.
———————————————————————

1. The question of quantum gravity and this grand unification is a
major intellectual/scientific challenge.

2. String theory offers understanding for this problem as well as deep and interesting insights on various issues from physics. Motl list of 12 appears to be very impressive. (And apropos Motl, I even conjecture, perhaps contrary to this example, that most string theorists are neither bullies nor male Chauvinist.) Not many other scientific theories can match such a list. String theory is the only major theory that offers such understanding for the unification problem. It also led to great mathematics.

(Well, there is some amount of over-sale, and discussions with serious faces of all this multiple universes stuff but this is not that unusual.)

3. There are serious problems with string theory concerning the possibility to draw concrete predictions that can be verified. There are also many possible string theories. (I do not share the interpretation that these limitations of the theory are fatal. And maybe we cannot hope for more.)

4. String theory is still rather tentative. It is quite possible that the
theory will fade away because of its difficulties and it is also possible that it will be replaced by a different theory which does a better job. It is possible it will prevail.

———————————————

As for the discussion, I cannot see, nigel, how string theory can be "dangerous" and I cannot see, Peter, how things can between 2001 and 2006 have gotten "much much worse" (but I can see you being much much more excited.) And "dying (like Maxwell) with a firm belief in a flawed theory", nigel, can serve as a nerd's curse
but it is not significantly more terrible than just dying. (Unless the death is caused by the theory.)

57. **Lubos Motl**
August 30, 2006

Virtually all of string theorists are nice people who never argue with anyone else, they're not chauvinists, and most of them are feminists. Most of them also think that string/M-theory are robust twin towers that are not threatened by any social effect or passionate proponents of alternative theories or proponents of no theories, and they almost always try to avoid interactions that could lead to tension which also gives them more time for serious work. Almost no string theorists drive SUV and they produce a minimum amount of carbon dioxide.

58. **Peter Woit**
August 30, 2006

Gina,

Discovering that your theory has 10^500 times more solutions to it than you thought it did really does count as "much worse". Virtually every string theorist will admit that the "landscape" is a huge problem for the theory. Things really are much worse now than they were 5 years ago.

59. **anonymous**
August 30, 2006

Hi,

I came across this weblog after Amazon automatically recommended me Peter Woit's new book. I went through it and I was amazed to see the extend that some disagreements can take and the way that people, affiliated with high profile institutions, behave when they should be models for the rest of the community and their students. Congratulations!

Personally, I find the situation rather interesting and I really hope something good will come out of this, whether it is in favour of string theory or not. I would like to make a comment, however, on the situation the way I see it. Please keep in mind that I am not a string theorist and I wouldn't even call myself a physicist in general. Nevertheless, here it is.

Let's see how long it takes for someone to get a PhD. Usually it is 4 years as an undergraduate and 5 years as a postgraduate. Most of the young people interested in string theory feel that they should start studying the subject while undergraduates. I guess that is why MIT introduces string theory classes and Zwiebach publishes books on "undergraduate" string theory. It has to do with demand, the customers have to be satisfied somehow. Blame it to the hype. In the graduate school you are forced to publish something, as if the rest of the thousands of people that form the "community", or physics as a science in general, is going to be saved by the students' publications. I may be wrong on this and it might be indeed necessary to publish as many papers as possible although I really doubt it. So, what is left? Narrow minded people, they have been doing strings or whatever all their life so you can't expect anything better, or disgusted and bored people who realize that life can be exciting without physics and go work for the industry, capitalizing their PhDs by getting nice flats and nice cars and going in nice places for their vacations.

I recall an undergraduate telling me that he wants to get his degree as soon as possible and go do a PhD in string theory without doing a masters first and that is why he chose to study for a 3 years bachelors (this senario is possible in some countries). I mean, how is that possible? Senior people are very well educated, no doubt about that, but what about the undergraduate/graduate folks? I am looking at the well known QFT book by Peskin and Schroeder sitting in my bookself right now. How long does it take for someone to read it, solve the excercises and be able to reproduce the results mentioned in it? In other words, master it? What about general relativity? Cosmology? Non relativistic quantum mechanics? Particle physics (with a phenomenological bend)? Statistical physics? Catch up with the rest of the community? Interact with people working in other fields like for example condensed matter or mathematics. Two or three years? If not, then how does someone attempt to solve a problem when he doesn't even know what the problem is in the first place? How is critical thinking going to be developed the way the educational system works?

60. **Ted Fails**
August 30, 2006

Im an amateur at this, but it seems to me that if there is already a
huge uproar over explaining the number "one" as arising naturally
in physics (ie, the CC), then how is it that anyone is comfortable with a finite number like 10^500? If this number is not infinite then
isnt it really wierd? (If it IS cardinal C, then why is it refered to as
10^500, which, by the way, is a very long way from C.)

In a unital algebra, "one" will frequently be present, but I find 10^500 a much more curious number.

Anyone please comment.

61. **ak**
August 30, 2006

tg claimed:

'So I summarize that string theory is an with high degree of mathematical consistency but which clearly needs experimentation. Its difficult math and difficult physics, so therefore time is necessary to come to an appropriate conclusion.'

I point out that the bare fact that string theory centers a discussion of dominating sociological and/or 'philosophical' character which in fact can be led largely decoupled from scientific arguments 'disproves' the above sentences. In fact there seems to be nearly a consent, even across the frontiers, about a certain lack of 'predictions' implied by string theory, while the disagreement centers mainly about the degree of this absence (which puts a claimed-to-be 'theory of everything' in a rather ironic light) and/or the 'interpretation' of this generally undenied fact ('it takes time'). I think that philosophy is fine as long as it supports or manifests the 'explaining' aspects of a mathematical formalism which shares as its necessary property 'prediction', I remember to have learned this years ago being still an undergraduate from a popular book by David Deutsch, his point was more or less that in purely logical terms a theory wouldn't need to be able to explain phenomena AS LONG as it makes the right (i.e. verifiable) predictions about them. To summarize the above discussion one can only conclude that string theory goes the opposite way, it more or less seems to suggest that prediction is fine but nothing compared to the intriguing implications suggested by the theory's 'explanation', this culminates obviously in this 'landscape' argument, where prediction is intrinsically senseless and explanation (the anthroposophic principle, the multiuniverse) puts itself into the perspective of initiating a 'new era' beyond Kopernikus, Einstein et al. I mention that one could parallelize these observations with not-quite recent arguments of german philosophers/sociologists (!) Adorno and Horkheimer whose 'Kritische Theorie' predicted and observed exactly the above discussed failure and tendency of modern rationalism and science in general to become 'mythological regression', even not as a corollary of the scientific method but as an intrinsic principle hidden in rational progress, danish philosopher Kierkegaard already observed in the 19th century that 'this century has produced more myths than any era before', I wonder what his comment would be today.
I could add that concerning my personal experience with 'modern physics' and mathematics I already doubted the mathematical rigor and consistency of 'the standard model' which was the point where I changed to mathematics, from the constant efforts of mathematicians to understand recent and non-recent concepts in 'modern physics' (only to mention the 'path integral', mirror symmetry) I can only doubt the above mentioned term of the claimed 'mathematical consistency' of string theory, even the meaning of this word combination is unclear (what does it mean: a theory which is logically consistent, should this be a particularly 'nice' feature of string theory or is ist just the most necessary condition for a mathematically formulated physical theory to become science?) and as it seems it is relatively hard to 'believe' that it is physics at all, it might be difficult and 'in some sense' consistent, but possibly neither mathematically consistent nor difficult as a physically theory, so maybe one could say it is an extremely difficult and sociologically 'consistent' metaphysical theory ?
(I apologize if this became a bit polemic.)

62. **Lee Smolin**
August 31, 2006

Dear Peter,

Thanks for the very thoughtful review. I have been distracted by some great personal blessing but see tonight that my book is available on Amazon.com and that Lubos has posted a two star review as you predicted. I am not interested in playing a game with Lubos or anyone else whose modus operandi is ad hominum attacks rather than serious engagement about ideas. (His attribution of some positive comments about my book made by Sabine Hossenfelder to the intellectual inferiority of women is for me so far beyond the pale, I really have no energy to further engage with him.)

I wrote a book which treats those with whom it disagrees with a great deal of respect and admiration. The point is not who is a member of what community or who is esteemed by whom, it is about which ideas are right about nature and which are wrong. I wrote about string theory, not to demean it, but because it was the best idea we had about unification and if it is in crisis then we have reason to reexamine our presuppositions which led us to believe in it so strongly (and I do mean us.) My book arose out of such a re-examination and its value, for me, is that it contains proposals for what are the wrong ideas that took such a promising idea to its present crisis. So I am not willing to engage with people who are not willing to recognize good faith and respond in kind. But I am of course happy to discuss with those who takes the time to read it and responds in the spirit in which it was written.

Thanks,

Lee

63. **Who**
August 31, 2006

Congratulations on the aforementioned blessing. Hope all are well:
though I and others know you only through your work, many must be wishing you joy.

64. **Gina**
August 31, 2006

Rather than attacking string theory directly a more promising way for trying to see what is wrong (if anything) with it to try to question the basics of extremely successful theories which preceded it. Peter, Lee is there some "QED bashing" in your books? (Even "QCD bashing" is already considered bad sportsmanship.)

65. **10^500**
August 31, 2006

Oversimplified answer to Ted Fails: "how 10^500 comes out?"

Strings want 6 or 7 extra dimensions, and to predict anything at low energy you must know their geography. Some complicated geography (holes and branes here and there) seems needed to try getting the complicated physics we observe. Strings tell that all of geography is dynamically fixed by vacuum expectation values of fields. There are many fields: a few fields describe the size and shape of extra dimensions, others tell the amount of each magnetic-like fluxes that can wind around each hole, etc, etc, etc. With a normal potential, each field has a few possible minima, and thousand of fields can have few^thousand minima.

10^500 is so many that, whatever we measure, string theory might have 10^100 solutions that practically look like it, altough finding them might be practically impossible.

Despite all impressive achivements, and despite Lubos Motl, this seems the end of the story.

66. **Gina**
August 31, 2006

It was a pleasant surprise that I could read Peter's 2001 paper
feeling that I understood most points Peter had made. This paper is almost disjoint (or orthogonal) to what I asked Peter. (Maybe 2-3 specific "anti string theory" claims can be extracted.) Part of the paper is a sort of philosophy of science look at particle physics and string theory of the last 3 decades geared towards "philosophy of funding of science". Philosophy of funding of science is an interesting and important subject worthy of discussions and debates but it is a separate issue to the "case against string theory" (as a scientific theory). I would still be happy and greatful to see a summary of the "case" against string theory along the lines I asked.

67. **TheGraduate**
August 31, 2006

To Gina:

(Well this is by no means authoritative but the anti-string theory case seems to be roughly as follows:)

1. String theory does not predict anything

2. There is currently no obvious way to modify it so it would predict something

3. String theory is reducing the probability that other (possibly more predictive approaches) will be tried.

All of these points can be expanded into sub-cases but I think they cover all the categories of objections.

68. **Lubos makes me puke!**
August 31, 2006

Gina- I would suggest that you visit the archives of this Blog starting with March of 2004 which has a good article about Peter and his education and qualifications. You can skim the articles and read the important articles about String theory fairly rapidly. This would help you understand that this is a very complicated problem that has arisen from virtually a idea that never had any of the empirical physical evidences that is required for the scientific method. The beauty and complexity of the mathematical calculations necessary to explore the extra dimensions of string theory lured a lot of our most brilliant and gifted students to work for many years only to find that they had invested their time unwisely. Rather than admit their mistake, some like Motl will do anything to keep this dogma a science. Something it has not been for a long time. We all owe our gratitude to Peter for making us aware of this problem.

69. **ak**
August 31, 2006

I have to admit that I still tend to get headache reading papers about particle physics whatever their background and philosophy might be, at least from my point of view they mostly tend to involve a considerable amount of mathematical sophistification but themselves completely lack the beauty and conisistency of the mathematical results and theories involved, in contrary they tend to mix up rigorous mathematical results with speculative ideas and concepts from mathematical and/or physical 'folklore', which makes ist extremely difficult for 'non-insiders' to decide what is still logically consistent deduction and what is wishful thinking or black-box deduction. I assume, and Peter seems to indicate that, that the 'problems' modern physics faced in the development since the 1970s derive as much from its desynchronization with mathematical justification of the concepts involved as with with its disconnectedness from experimental evidence. Motls list seems to reflect either intrinsic features of the theory which seem to be nearly tautological ('unity of supergravities') or concepts which are as interesting as yet poorly understood from a mathematical viewpoint (AdS/CFT/ mirror symmetry). From this point of view Peters attempt to re-view the mathematical concepts of the standard model seems to be promising; one could finally hint to an article of Berhelm Booss-Bavnbek, who judges post-war mathematics to be 'deformed' in a characteristic manner by aims of 'fictional warfare', this point of view is possibly not completely irrelevant to the discussion here (unfortunately in german):

'Symptome der militärischen Deformation: undurchdringliche Komplexität, rücksichtslose Kreativität und täuschende Vertrautheit'

http://www.uni-muenster.de/PeaCon/wuf/wf-90/9021101m.htm

70. **Gina**

August 31, 2006

Motl:

Can you please tell me (just a few sentences understandable to a laywoman) what is your opinion on the two claims:

1) That string theory cannot predict anything and will not be able to.

2) That there are over 10^500 possibilities which makes things worse.

many thanks in advance –Gina

71. **Who**
August 31, 2006

"The Trouble with Physics" (topic of thread) continues to be #1 on the Amazon general physics bestseller list
http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

at least it was 9AM to 4PM pacific time today, could of course be different at 5 PM—list changes hourly.

72. **Ming**
September 1, 2006

"My prejudice is that, lacking experimental guidance, the thing to do is to try and better understand the mathematical structures underlying the standard model."

I disagree with you 100%. The farce of string theory has shown definitively that more mathematics isn't the way forward for physics. I think the way forward is that we need to re-examine the basic foundation of the whole edifice of theoretical physics and look for the missing key pieces (of physical concepts, not mathematics) that everybody has so far overlooked. We need to question the foundation of everything and take nothing for granted. Looking for the easy half-hearted way out by using ever fancier mathematics simply won't work.

Unfortunately this kind of work is despised by most practising theoretical physicists, who're almost all of the "problem solver" variety. What we need desperately are more "seers" as Smolin described them, or "thinkers" may be a better word for it because it doesn't have the superstitious connotations of seers. If we look back at the history of theoretical physics, the most prominent advances were almost always made by thinkers and not problem solvers. Einstein being the best example of a great thinker (though he's also a darn good problem solver). Thinkers can think outside of the box (i.e. the existing formalism of theoretical physics), while problem solvers can only work within the box. The almost complete stagnation of theoretical physics for the past half century is due to an almost complete lack of quality thinkers, with all physics jobs going to the best problem solvers. As long as this extreme imbalance between thinkers and problem solvers persists in theoretical physics, I'm afraid there's no hope for true advances… IMHO

73. **Ming**
September 1, 2006

I just noticed that a recent 5-star review of Smolin's book has been deleted while Lubos' 2-star review is getting suspiciously high number of "helpful" votes. It looks like someone is actively (and desperately) "reporting" positive reviews of Smolin's book while artificially generating helpful votes for Lubos' review. I've only seen this kind of behavior from the site of another "science" book, the author of which is a total crackpot, and he is writing fake 5-star reviews for his own book while trying to report and delete all negative reviews (sad thing is he succeeded). Didn't know that string theorists/supporters can also fall so low…

74. **Eugene Stefanovich**
September 1, 2006

Ming,

I agree with you 100%. The usual paradigm:

Standard model and general relativity are great. The only thing left is to put them together.

lead us in a corner. The only way out of this corner is backward. No amout of clever problem-solving will help.

75. **ak**
September 1, 2006

I remark that it could be already 'mythological regression' to assume a GUT would actually exist. Periods of extreme idealism were quite frequent not only in science but also in philosophy or art (Hegel, Kant, Nietzsche) and Lubos gives an example of even biologists thinking of 'their' path to the universally saving GUT. The crisis of modern physics is not their lack of progress towards idealism, it is its implicit contact with the natural limitations of human (experimental) insight into nature itself (only to mention the energy scale of reasonably realizable accelerators). Possibly the non-existence of 'seers' derives from the fact that there 'is nothing to be seen' which would not go beyond the intrinsic limitations of human insight, at least derived from the standards of current technical ability. Apart from the fact that Einstein 'knew' what would have to be predicted, there in fact was experimental data (Michelson-Morley) giving at least subtle traces of the directions to choose, are there any comparable 'traces' today (Neutrino mass, dark matter?), one could doubt this. Apart from this, from a non-mathematical point of view idealism and regression were always closely connected, I only mention german idealism and its consequences for the history of the last century, it could be a characterizing property of a 'theory of everything' that it predicts in fact NOTHING, so string theory follows the 'dialectic principle' of human rationalism maybe in its purest form. I am personally quite happy about the existence of small-scale problem solving which, as a matter of exactness of the techniques involved, has from my point of view at least the potential to be of 'practical use' in human scale, that is, in human 'everyday life'. I objected a tendency in theoretical physics to substitute the reality of existence as human beings and the diversity of (even physical) reality by concepts of extreme idealizing, at the same time simplifying, potential. The current status of absence of 'predictiveness' of string theory is possibly just a corollary of the wish to include apparently uinversal 'explanation', under whose regime details as 'mathematics', 'logic', 'rationalism', 'predictiveness', in the end maybe 'science' itself seem to lose their relevance or status as guiding (and limiting) principles they acquired over the thousands of years of growth of human knowledge (at least in the 'exact sciences'). Possibly it is the moment where the exact sciences lose their insight into their own limitations where they end to exist as 'exact sciences' and turn themselves into mythology, I already said this above. For my own part, I am quite happy to consider exact sciences as 'exact' but limited and the other disciplines of human thinking (which EXIST, even to me as a mathematician) as 'inexact' but potentially unlimited, possibly the status of modern physics gives a hint towards the growing disassociation of the scientific worlds or human thinking in general, which would in the end lead again towards the concept of 'thinker': maybe it would be Einstein, knowing the history of post-war physics and societies in general, to conclude that there is a certain whisdom in preserving 'mythology' as 'mythical' and 'exact sciences' as 'exact', maybe this is what would have to be 'seen' from his perspective.

76. **Lee Smolin**
September 1, 2006

Ming,

I'm not doing anything, it is very sad to watch. I wrote two books before, there was a lot of disagreement, for example from string theory friends who told me that my idea of the landscape of theories was silly and there would soon be a principle of vacuum selection that gave unique predictions. But no one behaved badly. What is really sad is that there are many string theorists who are ethical and act and talk in good faith, if I were them I would be appalled to let me field be so represented. Besides which this kind of behavior provides strong evidence for the claim that there is something pathalogical in the sociology of the field.

Thanks,

Lee

77. **ks**
September 2, 2006

Just for the record. String theory has nothing to do with the philosophy of "german idealism". Attributing the believe in hypothetical stringy objects that are not detectable but shall be present for complicated theoretical reasons to Kants critics of pure reason or his categories of mind *a priori* or Hegels self-reflection of absolute mind and its projections into history, is hilarious. I do not even want to imagine what Nietzsche had made out of this drive into self delusion and science-as-cult beyond its empirist tradition. Maybe an appraisal of John Horgans writings as being *sound*? German idealism is close to the contemporary radical constructivist/deconstructivist philosophy, to existentialism, phenomenology etc. not to a naive believe in the objective existence of ones own intellectual phantasies.

78. **anonym.**
September 2, 2006

" Didn't know that string theorists/supporters can also fall so low… " – Ming

They are unable to respond any other way, they have no other responses to give.

79. **MoveOnOrStayBehind**
September 2, 2006

"German idealism is close to the contemporary radical constructivist/deconstructivist philosophy, to existentialism, phenomenology etc. not to a naive believe in the objective existence of ones own intellectual phantasies."

Here is a good example to what conclusions german philosophers are lead to, in this case concering the Higgs mechanism:
http://www.dpg-tagungen.de/archive/2006/dortmund/akphil2.pdf
Unfortunatly this is in german. What is being said there is that "neither an ontological, nor an epistemological interpretation of the Higgs mechanism is tenable"; this follows fram a "critical analysis".

The link given above, "Symptome der militärischen Deformation: undurchdringliche Komplexität, rücksichtslose Kreativität und täuschende Vertrautheit'" in another beautiful example of political ideology mixed up with science.

Good that there are other parts of the world where science is moving on, although I am getting concerned about the US too, after reading the opinions in this blog here.

80. **anon**
September 2, 2006

For translation try: http://babelfish.altavista.com/

81. **ak**
September 2, 2006

no, there seem to be some 'misreceptions', I did not compare string theory to 'german idealism', the argument was that to believe in the existence of a GUT could be a form of idealism, there is no such thing as pure 'naive believe in the objective existence of ones own intellectual phantasies', string theory takes place on a sociological/philosophical background and I just point out that it was Einstein co-initiating the belief in the existence of a GUT. My point was that real progression in modern physics could mean to be a little bit closer to Kierkegaards criticism of Hegel (opposing his dominant position, claim of unifying logical concepts etc.) and from what I understood, Peter and Lee move a bit in this direction. By the way I don't think that 'german idealism', as a philosophical phenomenon, is very close to existentialism or deconstructivism and 'to move on or to stay behind' is exactly what this discussion is about.

82. **ak**
September 2, 2006

I have to correct myself in the sense that the point is that to rethink modern physics with the explicit aim of a GUT remains pure 'idealism' as long as there are no fundamental experimental guidelines to show what exactly a new theory should predict or explain BEYOND the capabilities of the existing models. The unexplained constantness of the speed of light in a vacuum was Einsteins starting point, maybe I am not quite informed, but I do not see that there are any compareable fundamental facts pointing beyond the existing models today. In this situation the string theorists can hardly blame non-string-theorists to develope alternative pictures, one could for instance raise the question why not anyone seems to be interested in the notion of 'symplectic spinor' or symplectic Dirac operator, from a physical point of view the symplectic Dirac equation could possibly be the starting point for a geometric theory of bosons (since it involves the 'symplectic Clifford algebra'), a not quite new paper

http://www.mathematik.hu-berlin.de/~klein/ftr.ps

shows that there is a natural notion of pseudo-differential quantisation involved over sections of a certain line subbundle of the sympletic spinor bundle, on the other hand the metaplectic representation implies the Schrödinger equation for linear hamiltonian systems on R^{2n} and is reflected in some sort of Lie derivative

http://www.mathematik.hu-berlin.de/~klein/liabl.ps

the picture is admittedly not quite coherent, but as a physicist, i could possibly just 'couple', for instance over Kaehler or Calabi-Yau manifolds, the Dirac operator over the ordinary spinor bundle with the symplectic Dirac operatior over the sympl. spinor bundle (taking tensor products and operator 'sums)' and see what 'happens', for Calabi Yau manifolds a natural notion of Maslov index would be involved and would give rise to some notion of 'quantisable' Lagrangian foliations, which would correspond to the dimension of the kernel of the square of some restriction of the symplectic part of the coupled operator etc etc, maybe a new 'TOE', who knows.

83. **ks**
September 2, 2006

> I just point out that it was Einstein co-initiating the belief in the existence of a GUT.

Actually this goal must be attributed to Newton and all his followers. Einstein and other quantum theorists of the first generation destroyed the old worldview and broke it into two incompatible parts without losing the researchers inherent destination of a complete and consistent physical explanation of the whole world. There is no point to make in the inexistence of a GUT because its existence is undecidable unless it exists. It can't be disproved by reason. We can only get stuck. Hence demystification doesn't help us because there is no other side of true reason but just a decision to make for everyone when its time to give up, which is finally subjective.

What really happened with the desire of a GUT is that it became an aspect of mass/pop-culture and its proponents rock-stars of popular science magazines ("Einsteins legacy" etc.) and books. Physicists and to a lesser degree mathematicians are our last heros the last people who truly "transgress the boundaries" which is properly mythological and part of the fascination. Besides the person Stephen Hawking it was ST that had been in the focus of the *economy of attention* of fundamental science in the last decades. String theory is both a highly esoteric and speculative branch of mathematical physics and the pop culture of the TOE. This tension makes it interesting even for visitors who are by no means "active researchers" in the sense of Distler. I'm not claiming that depressing the public about the TOE wouldn't be healthy for the theoretical physics community even if it's going to shrink to the size it had at Einsteins time.

> I have to correct myself in the sense that the point is that to rethink modern physics with the explicit aim of a GUT remains pure 'idealism' as long as there are no fundamental experimental guidelines to show what exactly a new theory should predict or explain BEYOND the capabilities of the existing models.

This is undisputable. Reason without experience is empty, experience without reason is blind, as Kant said.

84. **D R Lunsford**
September 3, 2006

ks said

What really happened with the desire of a GUT is that it became an aspect of mass/pop-culture and its proponents rock-stars of popular science magazines ("Einsteins legacy" etc.) and books. Physicists and to a lesser degree mathematicians are our last heros the last people who truly "transgress the boundaries" which is properly mythological and part of the fascination.

Well I don't really agree. The desire for unity is completely justified, as is seeking it in geometry. All three major developments since Newton – Maxwell, Einstein, and Dirac – are based on geometry and the idea of unity, or rather as Finkelstein would say, "relativization", which amounts to simplification of the underlying Lie algebra of observables by decontraction. The problem seems to be that the current practitioners are just uncommonly bad at finding the key physical ideas, because they are too enmeshed in arcane mathematics. Klein, Courant, Weyl, all warned us this would happen.

-drl

85. **ak**
September 4, 2006

I am afraid not to understand the dialectic principle of these two points:

'The desire for unity is completely justified, as is seeking it in geometry.'

'The problem seems to be that the current practitioners are just uncommonly bad at finding the key physical ideas, because they are too enmeshed in arcane mathematics.'

The 'desire for unity' is claimed to be derivable from purely mathematical reasoning, at the same time to be 'enmeshed in arcane mathematics' is attributed to the unability of finding 'the key physical ideas'. This could hint to some key misunderstanding of string theorists reasoning, taking on one hand mathematics as a guideline for fundamental aims and on the other hand attributing subsequent experimental deficits of the theory to the unability of 'practitioners' to find the key physical ideas while being absorbed in mathematical reasoning. To resolve this one should possibly follow the contrary strategy: to take experimental facts as the origin of thinking (not taking experiments as the corollary of mathematical idealism) and to use on the other hand plain mathematics as the tool to derive a theory from this experimental starting point (I point out that my above statement about a possible 'TOE' derived from symplectic spinors was of substantial ironic character).

86. **D R Lunsford**
September 4, 2006

ak said

The 'desire for unity' is claimed to be derivable from purely mathematical reasoning, at the same time to be 'enmeshed in arcane mathematics' is attributed to the unability of finding 'the key physical ideas'.

No one ever claimed it was derivable from "purely mathematical reasoning". Indeed the intuitionists firmly believe that such a thing does not exist, and that both math and physics are stimulated by mutual interaction. Finding the right physical idea is an irreducible activity – finding its mathematical realization is not. By "arcane" I mean – disconnected from "physical reasonableness". Certainly there are many complex mathematical structures that are eminently reasonable. The main activity of the physicist is to come up with physical ideas that are reasonable and tractable. That is what is completely missing these days.

-drl

87. **ak**
September 4, 2006

'The main activity of the physicist is to come up with physical ideas that are reasonable and tractable. That is what is completely missing these days.'

I still do not agree on the form of this conclusion. It is a myth Einstein derived Relativity from pure physical intuition, there was an experimental guiding principle which lead to concepts like 'Lorentz invariance' and Minkowski space (Michelson-morley). From THIS point, it was in fact a pure 'thought experiment' to generalize to curvature, geodesics and so on, but the experiment could in fact qualify the result of these thought experiments to be true. In the current situation of modern physics there seems to be neither a clear physical guiding principle derived from experiment nor a possible way to judge the result of a wide variety of thought experiments, so one cannot in fact blame the state of string theory to the absence of 'thinkers' producing reasonable physics. It is exactly this belief in 'new physics emerging from human brain' which lies at the esoteric origin of string theory and potentially also of related concepts.

88. **amused**
September 5, 2006

Thanks for this review Peter, I'm looking forward to reading the book
(and yours).
Smolin makes some astute observations, but it's one thing to describe the problem and another thing to find a viable solution. As Smolin points out, young peoples' job prospects in formal particle theory are determined by how they are viewed by senior influential physicists, and since most of the latter are string theorists (at least at the

leading US uni's) it puts the non-stringers at a huge disadvantage. As far as I can tell from reviews of the book and what he has written elsewhere, Smolin's solution for this seems to be some kind of "democratisation" where funding and jobs get distributed over various areas in proportion to the number of people working in them. What do you think about this? Personally I'm against it. One reason is that it just replaces preferential weighting for string theorists by preferential weighting for people working on some broader selection of areas. What if my preferred research area is not among these? Or if the representative for my area on the "committee" is not very eloquent (he neglected to develop his salesman skills through hyping of our area to the public) and therefore can't get us a decent share of the pie? Or if I suddenly find that there is something exciting in a non-represented area that I want to work on? Besides that, I do think these kind of things should be left as much as possible to "market forces". The problem is that at the moment we don't have a genuine free market; it's more like a monopoly a la Microsoft.

Anyway, if Peter will indulge me I'ld like to propose a different solution: How about just letting people work on whatever they like, without preferential weightings for any particular areas, and evaluating them solely on the basis of the progress they make? This requires of course some objective measure for evaluating "progress". We need something that can be used to evaluate and compare people across different areas. The normal thing in academia is to base this on journal publications. Problem is that people don't care much about journals in theoretical hep these days. When you write a paper you stick in on the archives, where it gets seen by the senior influential people in your field, and your stock goes up or down depending on what they think of it. Subsequent publication of the paper in a supposedly major journal is usually routine and doesn't mean much. This situation is ok for evaluating and comparing people within the same area, but how are you supposed to compare people across different areas? Although they publish in the same journals there is no way to tell the relative quality and significance of their works just from "major" journal publications, since it doesn't take much to get published. Similar things can be said about citation counts (which not only measure the significance of the paper but also the well-connectedness of the author and the size and popularity of the area in which the paper lies).

However, there remains one physics journal which is still non-trivial to publish in: Physical Review Letters. So how about using number of publications there as the evaluation measure? (The weight of each paper should of course be normalised according to number of co-authors, and with a further appropriate reduction for young people who are just going for a ride on the coattails of seniors.) While it is true that some areas of physics (e.g. condensed matter) are easier for getting published in PRL than formal particle theory, within the latter area there doesn't seem to be any biases (e.g. it is not unusual for both string theory and LQG papers get published in PRL) so it would seem to be a level playing field for all. The string theorists surely won't have any objection to this – since they are so brilliant they will surely welcome the opportunity to prove it in an objective setting. In fact I'm sure it's only their natural modesty which has prevented them from filling up the pages of PRL already. It will also give a chance to the hardcore younger stringers to finally silence those "penis envy"-afflicted cynics out there, who go around disparaging them for being mindless clones, absorbing what they are spoonfed like sponges but incapable of doing anything original and significant on their own.
(Whoops, seems like I might have slipped into string-bashing mode at the end there ;))

89. **Ron Macnaughton**
September 9, 2006

I'm a high school physics teacher who just yesteray was asked what I thought of String Theory. My student had trouble understanding what he thought was the deepest theory developed so far.

I explained how most astronomers used to believe planets moved in circles or circles on circles. Eventually Kepler showed only elliptical orbits explained the observed positions of Mars.

I gave the opinion that String Theory makes some assumptions and it might come close to explaining reality, but I didn't think it would ultimately be successful, just as epicircles went into the dustbin of science.

I said that's only a high school teacher's opinion, but many brilliant people worked on it and believed it.

I think the main problem is that String Theory doesn't seem to include General Relativity.

I find the sociology of science rather interesting. We talk about heroes who have a pure drive for understanding, but Tycho Brahe gave Kepler the Mars problem, because he thought it would be too hard for the young whipper snapper to solve. Correct theories (plate tectonics) get rejected for decades. Wikipedia still lists only string theory as a theory for quantum gravity, even though many alternatives are out there.

I read "moron" comments on this blog which I find embarrassing when I hope to inspire young people to take up science as a career.

I can't wait for my copy of both books to arrive.

90. **woit**
September 9, 2006

Hi Rob,

An important thing to explain to students about science is that it makes testable predictions that can be checked. Things like string theory are very speculative ideas that some people someday hope will become legitimate, testable science, but they're not there yet. Some of us think it never will get there, some are more optimistic.

Links, Links, Links…

August 30, 2006
Categories: Uncategorized

Wired has an interview with Lee Smolin.

The French internet site Arte has interviews with various physicists, including one with Carlo Rovelli. If you don't want to watch the videos, there's a text summary (in French).

Mel Schwartz died earlier this week. He won the Nobel prize in 1988 for his 1962 co-discovery of the muon neutrino at the AGS at Brookhaven. Schwartz left physics for a while and founded his own company near Stanford. He returned to Brookhaven and worked on the plans for RHIC, then came back here to Columbia where he was a professor in the physics department, so I had the pleasure of meeting him a couple times. After his retirement he moved to Idaho.

Freeman Dyson's 1951 lectures on QED have been put in TeX and posted on the arXiv.

This fall Graeme Segal will be visiting Columbia as "Eilenberg Chair", a visiting position we have that was funded by the sale of part of Sammy Eilenberg's collection of South and Southeast Asian art to the Metropolitan Museum. Segal will be giving a course on The Mathematical Structure of Quantum Field Theories, which I'm very much looking forward to.

Another course I'd like to attend, but it's too far away, would be Dan Freed's one this semester on Loop Groups and Algebraic Topology. The web-site for the course includes a reproduction of Bott's wonderful lecture notes dealing with the topology of compact Lie groups.

There's a new paper out by Thomas Thiemann summarizing the technical state of LQG. I haven't had time yet to read it, but hope to spend some time soon doing that. A good place to discuss it would be here, where Aaron Bergmann has already started, also see some comments by Robert Helling. A not so good place to discuss it would be here.

Eckhard Meinrenken has an interesting new paper entitled Lecture Notes on Pure Spinors and Moment Maps, which promises a more detailed forthcoming paper by him, Alekseev and Bursztyn.

Some recent and ongoing conferences that have talks online are at Ahrenshoop and Santa Barbara.

Comments

1. **Attila Smith**
   August 31, 2006

   Dear Sir,
   Yakov Perelman, a Soviet physicist without a Ph.D., wrote wonderful books on popular (but very accurate) physics, always firmly based on low-tech experiments.
   Do you know of a relationship with Grigori?
   Yakov couldn't be his father, because he died in 1942.
   Thanking you in advance, I remain

   Yours faithfully A.Smith

2. **D R Lunsford**
   August 31, 2006

   The typesetting of the Dyson lectures is an absolute prize! Thanks to Mr. Moravcsik!

   -drl

3. **Luboš Motl**
   August 31, 2006

   Dear crackpot Woit,

   this is one of the reasons why you're crackpot. You "haven't yet had time to read a paper" – any technical paper, for that matter, and at least for 18 years – but you already offer the other idiots who visit your discussion forum of morons a precise prescription which analyses are correct and which are not.

   The cleverest 10% of the chimpanzees will figure out that your opinions are just a worthless piece of garbage, much like 10% of the cleverest visitors of your crackpots' discussion forum.

   Best
   Lubs

4. **D R Lunsford**
   August 31, 2006

   I should mention that the three books Dyson mentions are all available (Pauli from Springer I believe – Wentzel and Heitler from Dover) and are all wonderful books.

   -drl

5. **D R Lunsford**
   August 31, 2006

   Did someone just sit on a whoopee cushion?

   -drl

6. **Lubos and the Bogdanovs**
   August 31, 2006

Hi Peter

I visited a bookstore in the capital city today, and I saw a copy of the Bogdanovs' "Avant le big-bang" ("Before the Big-Bang"), recently (May 2006) published by "Le livre de poche" editions, in the french section bookshelf. Guess my surprise when I picked it up and had a look at the back! A comment by Lubos figured there. I quote:

"Les frères Bogdanov proposent quelque chose qui, d'□un point de vue spéculatif, a le potentiel pour représenter une alternative à la gravité quantique.
Professeur Lubos Motl, physicien théoricien,
Université de Harvard.",

which translates into:

"The Bogdanov brothers propose something which, from a speculative point of view, has the potential of being an alternative to quantum gravity.
Professor Lubos Motl, Theoretical Physicist,
Harvard University."

Go to this (http://www.livredepoche.com/index.html) for proof that I am not making this up.

Wow! I guess I should not be surprised by now when it comes to Lubos; only the latter can say such a thing about whatever the Bogdanovs are up to. Did he actually read this book?

I know this is not the place to post this, and I apologise if this is too much.

7. **LDM**
August 31, 2006

Thank you for posting the very nice Dyson link…

BTW, American edition of NEW, page 99 — you might be interested in knowing that Kharkov is in Ukraine, not Russia …
(though it is true most people in Kharkov might prefer the Russian lanuage to the officail Ukrainian language and Kharkov has strong ties with Russia)

8. **Thomas Love**
August 31, 2006

drl wrote: The typesetting of the Dyson lectures is an absolute prize! Thanks to Mr. Moravcsik!

Evidently he didn't read all of the typist's notes, Moravcsik did the rewriting from the first edition to the present one. The current TEX typesetting was done by David Derbes, a PhD student of Higgs.

It will be an interesting read, reading Dyson and then NEW. I ordered the package of NEW and Smolin's book from Amazon and I haven't received either.

9. **RandomSurfer**
August 31, 2006

I can't resist pointing to a link for the "eerie similarities" department. It's written by a reasercher in computer graphics, but… well, read and see for yourself: Leaving.

10. **Chris Oakley**
August 31, 2006

> Moravcsik did the rewriting from the first edition to the present one. The current TEX typesetting was done by David Derbes, a PhD student of Higgs

This gives the lie to the commonly-held belief that Ph.D. students are no good to anyone.

11. **woit**
August 31, 2006

LDM,
Thanks for pointing out that Kharkov is in the Ukraine, a fact that I wasn't aware of. The inaccuracy this caused in the book is minor. I refer there to Golfand, Likhtman, Volkov and Akulov as "Russian physicists in Moscow and Kharkov". Looking into this, while Volkov and Akulov were in Kharkov, Volkov was born and mostly educated in Leningrad, moving to Kharkov at age 26, so referring to him as Russian is not inaccurate. I don't know about Akulov, who was Volkov's student, quite likely he is Ukrainian.

12. **lostsoul**
August 31, 2006

Thanks for the Dyson link; latex is better than a hand written scrawl.
All round, you're keeping it real; and I keep coming back.

13. **nontrad**
August 31, 2006

The Dyson link, and the rest that are all QFT related, is one of the primary reasons why I continue to return to Peter's blog.

Simply put, there is a fond place in my heart / mind for all things QED!

Feynman, simply put, has been a hero of mine since at least I was 16. Sam Schweber's 'QED and the Men Who Made it' (http://www.pupress.princeton.edu/titles/5524.html) is a book that I literally poured over when I was first studying QFT… searching for inspiration for the how's and why's of that strange and wonderful place called 'quefithe'.

Dyson and Schwinger and Tomonaga, Pauli and Oppenheimer and Bethe and Wheeler and Dirac all became heros too…as a result of Schweber's book.

These notes then are a blast from the past that strike, apropos, of the leaves just now starting to change with the coming Autumn…

Scanning the notes, I am reminded of Dover's Principle of Relativity (containing the original papers 'On the electrodynamics of moving bodies', 'Mathematical aids to the formulation of generally covariant theories', 'The theory of the gravitational field' and 'Hamilton's principle and the general theory of relativity'); which I first bought in a used book store in a dog eared copy printed in 1952 that still sits on my book shelf.

The days when physics was still physics….

14. **YBM**
August 31, 2006

Dear "Lubos and the Bogdanovs", if you had a look *inside* the book, you could have read the same quote this time translated as :

"Les frères Bogdanoff proposent quelque chose qui, d'un point de vue spéculatif, a le potentiel pour être une alternative à la théorie quantique."

which translates into :

"The Bogdanov brothers propose something which, from a speculative point of view, has the potential of being an alternative to ***quantum theory****."*

Motl is falsified in the worse and more delusional "anti-String theory" book ever published in France (the cranky brothers are not proposing a "alternative to quantum theory" , but to "string theory" on the basis of bogus math and pre-graduate faulty physics), and is quite happy with that as long as he think he can use that against Woit. What a pity.

Motl couldn't (yet) be called a crackpot (at least on physics issues, anything he writes on computing on his blog is a joke), only a phycho. I guess it won't last long before he'll fall into the first category.

15. **YBM**
August 31, 2006

in case Lubos 'psycho' Motl would like to check the cover

16. **Gina**
September 1, 2006

Dear Lubos Motl:

Can you please tell me (just a few sentences understandable to a laywoman, even just a friendly link will be useful) what is your opinion on the two claims:

1) That string theory cannot predict anything and will not be able to.

2) That there are over 10^500 possibilities which makes things worse.

many thanks in advance –Gina

17. **woit**
September 1, 2006

Gina,

Lubos has his own blog at motls.blogspot.com. If you want to start a discussion with him, best if you do it there.

18. **dan**
September 1, 2006

dear anti-woit lubos,

if peter is materially wrong about something, that certainly deserves to be aired, but if peter is factually correct on his statements on string theory, then as a scientist, wouldn't you agree this kind of skepticism is to be valued?

Peter, have you seen Lubos top 12 top stringy results, and do you think that those results are sufficiently impressive so as to justify the current effort in the string theory research program?

19. **Chris Oakley**
September 1, 2006

As Peter quite rightly does not allow self-promotion here I will keep this short, but I have just added a section on the spin-statistics theorem to my on-line text book (preamble here). Intelligent feedback (by e-mail) would be appreciated.

20. **Stefan**
September 2, 2006

I came to know via an e-friend that his commentary to Lubos' post on Connes' new paper was removed. What more, he was permanently banned from posting there again.

He told me that what he wrote was neither personal in nature, nor degrading to Lubos' work or intellectual abilities (well, maybe a little), nor in any way offensive to anyone else.

His primary criticism was the following [in paraphrased form]:

Lubos, until you prove your value as a researcher (by say writing something which either: a) gets the attention of leaders in the field – say Witten or Gross or 't Hooft etc. or b) gets 500+ citations within a year of publication; or both) you will not get many to listen to your constant (and never ending…) spiel on random and (mostly, not always) off-topic issues that have nothing to do with physics…

in this case: your review of Connes' paper.

He goes on to add [in subtle language]: To critique someone like Connes' may be a bit out of your league…

I guess that probably offended Lubos' sky-high ego… and in the true spirit of scientific dialogue he banner him ever from posting again.

Wow, it's a side of Lubos he rarely reveals to anyone else…

…interesting… and intriguing I must add.

21. **Stefan**
September 2, 2006

Hi Peter,

Don’t quite understand why you blocked my previous post.

Stefan

22. **woit**
September 2, 2006

Stefan (and others),

The WordPress spam filter is idiosyncratic, often working very well, sometimes deciding to mark comments as spam for no obvious reason. When this happens, I don’t get a notification of it, but they go into a queue. I check the queue periodically, but often only once a day.

dan,

No.

Amazon Reviews

September 2, 2006
Categories: Uncategorized

I'd really much rather ignore the activities of Lubos Motl, but his unethical behavior recently has sunk to new lows, and it seems necessary to point this out and encourage others to take appropriate action.

When Lee Smolin's new book The Trouble With Physics first became available recently on Amazon, Lubos immediately posted a "two-star" review of the book, one that immediately had a large number of votes that it was "helpful", likely generated by Lubos himself. The review is thoroughly dishonest and designed to mislead anyone who might consider buying the book ("Lee reveals his intense hostility against all of modern physics", "Lee proposes a truly radical thesis that it is wrong for mathematics to play a crucial role in theoretical physics", "He also denies the difference between renormalizable field theories and the rest", "one of his rules says that the conclusions must be accepted by everyone if their author is a person of good faith", etc., etc., etc…). The dishonesty includes the use of two stars rather than one, since Lubos is well-aware that Amazon is more likely to immediately delete one-star reviews.

After a while, another review appeared, a positive 5-star review. At some point, it seems that Amazon deleted Lubos's review, perhaps because some people had, quite justifiably, clicked on the link that allows one to report a review as inappropriate. Lubos then posted on his blog a rant about this. Later on, he somehow managed to get the 5-star review deleted, and his own one reinstated (and removed his blog posting). At the present time, the only review of Smolin's book on Amazon is the dishonest one by Lubos. This situation provides yet another example of the kind of disturbing behavior of parts of the string theory community that Smolin has detailed in part of his book. Unfortunately, if people just ignore what Lubos is up to, we end up with situations like the current one at Amazon, so I encourage people to consider what action they can take to do something about this. As for Amazon, the answer to dishonest speech is honest speech, so I encourage people to post honest reviews there of the book, I've just done so (and if you want to review my book while you're at it, that's fine too…).

Lubos still has up on his blog an offer to pay people $20 for writing bad reviews of my book. I've complained to people in the Harvard physics department that this kind of professional behavior by one of its faculty members is unethical and not the sort of thing protected by academic freedom. I've also pointed out to them that Lubos regularly publicly claims that his colleagues share his views (most recently in the Amazon review where he goes on about Smolin visiting "us", and what "we" "mainstream physicists" think). While it appears that at some point an attempt was made by someone at Harvard to get him to suppress his extreme political views, I've seen no evidence whatsoever that anyone in the string theory group at Harvard has a problem with his behavior in defending string theory. This is also true of the larger string theory community, which remains almost unanimously (Aaron Bergman is the one exception I can think of) unwilling to publicly criticize Lubos's tactics. A common recent defense of string theory against its critics is that its proponents hold power because they have triumphed in the "marketplace of ideas." It's not a pretty sight to see how this triumph is being defended now that there are other voices in the marketplace.

Update: About an hour and a half after I posted this, my positive review of Smolin's book had accumulated a bunch of "helpful" votes, Lubos's a bunch of "unhelpful" ones, and, I'm guessing, a bunch of reports as "inappropriate". His review then disappeared. My sympathy goes out to whoever it is at Amazon who has to moderate this kind of controversy. Since Lubos is such a poster boy for the problems of string theory, I should say that I'd be happier if his review had not been deleted, but remained there, countered by other, more honest reviews.

Update: I see that Lubos's "one-star" review of my book is now back up (carrying the original date, why's that?) with the comment:

My review has been erased four times because the author keeps on encouraging other enemies of science on his discussion forum to report my review as inappropriate. This is not fair and is a reason why I returned to 1 star.

Well, his review is inappropriate, so I can see why people click on the link that reports this. Again, I'd prefer that it stay up there to show how string theorists behave, but that others with more honest reviews submit them also. Besides, like most authors these days, I do periodically check my Amazon sales ranking, and, as far as I can tell, when his review is there, sales improve. Go, Lubos!

Update: OK, now his review of my book has disappeared, and the one of Smolin's has reappeared. Depressing, my sales should soon head downward, but I'm glad Lee's will do better.

Update: Lubos is indefatigable, both his reviews are back, mine now says:

My review has been erased five times because the author keeps on encouraging other enemies of science on his discussion forum to report my review as inappropriate. This is not fair and is a reason why I returned to 1 star. Please don't trust the counter of helpful votes either. It is being distorted by the visitors of Peter Woit's blog who are directly controlled by the author of this book.

It seems that I "directly control" visitors here. Wow.

I'm guessing Amazon must have some sort of automated system, which apparently deletes reviews that receive a certain number of "inappropriate" votes, but allows the review to be edited slightly and resubmitted.

Update: Lubos seems to have managed to get my review of Smolin's book deleted, as well as one of the 5-star reviews of my book. I can't compete with him in terms of fanaticism, so will just have to take people's advice and ignore what he is up to in terms of manipulation of Amazon reviews. Smolin is a new father and also doubtless too busy for this. People who don't like this situation are free to try and do something about it, by writing reviews, or contacting Amazon, Lubos's employer, or the people he refers to as "us" in his review to make them aware of what is going on.

Comments

1. **Arun**
   September 2, 2006

   I really think you pay too much attention to Lubos Motl. I think once his current position with Harvard ends, his string theory "friends" will distinguish themselves by their enthusiastic lack of support for his future career. Here today, gone tomorrow – why would anyone risk any political capital in this fight anyway?

   Please don't make this to be about physicists, keep it about physics. Don't let it be said that the opposition to certain ideas arises from an underlying dislike of the people involved.

2. **Peter Woit**
   September 2, 2006

   Arun,

This isn't about dislike of Lubos, who I hear is a charming fellow, and I'm sure that's right. It's about the tactics being used by string theorists to suppress criticism. Quite a few physicists have told me that they don't dare say anything publicly critical of string theory, because of fear of what will happen to their careers if they do. One reason for this fear is the apparent support of the string theory community for Lubos and his tactics. Your own question "why would anyone risk any political capital in this fight anyway?" demonstrates the problem: if Lubos has so little support, why would anyone think they would be risking political capital in this field if they complain about his unethical behavior?

The problem isn't so much Lubos as the community he is a part of and its willingness to tolerate his behavior.

3. **Kris Krogh**
September 2, 2006

Now I see what Lubos means by his "free marketplace of ideas." One buys and sells the scientific truth with money.

4. **Anonymous Coward Michael**
September 2, 2006

Do you really think people at Harvard take you seriously enough to consider your complaints as anything more than spam? I wish you could hear the many little side remarks made by serious scientists mocking and disrespecting you in various everyday situations. You got the attention you were craving, all right, but you also paid the price for it. Congratulations, you *are* the class clown!

5. **Yatima**
September 2, 2006

M. Motl really needs to get professional help. The amazing shrillness of his commentary reminds me of posts on alt.alien.visitors or the grating sounds that one unfortunately (and increasingly) hears coming from unhealthy parts of the religious spectrum. Of course, all this is not entirely unexpected, after all, this involves Faith, in a big way. Faith cannot permit doubts. And as always, you have onlookers nodding approvingly from the sidelines while the unbeliever is being given a good and somewhat amusing trashing. Same old, really.

"Nature and Nature's laws lay hid in night; God said, Let Newton be! and all was light." … but this is said only because Newton was a really really nasty customer. He was also wrong in the end.

6. **Peter Woit**
September 2, 2006

Michael,

Thanks for the insider info on what the "serious scientists" up there in Cambridge such as yourself have to say. Always a pleasure to hear from you.

7. **anon**
September 2, 2006

"Concerning Feynman's misunderstanding of string theory, he was just too old … he was saying such a nonsense … he was already too old and a bit senile and slow…"

– http://www.math.columbia.edu/~woit/wordpress/?p=446#comment-14825 which is Dr Motl's attack on Feynman's alleged incompetence in 1988, when Feynman worked out the cause of the space shuttle explosion in 1986, as the physicist on the Special Pesidential Commision. Motl the added that discovering the O-ring failure problem was nothing compared to string theory.

So even if Feynman was around today, he would be dismissed as senile, and nobody would ask Motl for evidence or tell him to be more objective and less political. Everyone who contradicts Motl, regardless of the lack of evidence Motl has, will be dismissed as a moron, so you cannot discuss anything (unless you agree with him). This is why people don't really want to argue with Dr Motl. So authority in particle physics continues sliding into his hands.

8. **richard**
September 2, 2006

While I am not anti-string, Lubos does seem to be doing for the image of string theory what the movie "Deliverance" did for canoeing holidays.

9. **ks**
September 2, 2006

Michael, I guess Harvard takes public mind serious when it starts to considers ST to be a self delusion with high aggression potential and psychopaths as its promoters. Lubos actions are somewhat of a tragicomical attempt to correct the impression that ST is a failure by scientific standards that prevailed at least for a couple of centuries. On the other hand I believe public mind is tolerant about some artistical branch of theoretical physics that got stuck in "mathematical science fiction" (J.Horgan). No one really knows what to expect.

10. **Who**
September 2, 2006

as of today 12:15 pacific, or 3:15 PM your time, the Amazon general physics bestseller list was

#1 TwP
#2 Elegant Universe by Brian Greene
#3 NEW
#4 Douglas Giancoli college physics text
#5 a Stephen Hawking book

http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

at the moment TwP has 5 stars, perhaps because of Peter's review and the many positive votes it has received (I havent followed that, so can't say.)

11. **Anon**
September 2, 2006

Everyone else (both inside the string theory community and outside) ignores Lubos's rantings. The only person paying attention to them is Peter Woit.

Without Lubos to hold up as an exemplar of what's "wrong" with the string theorists, where would Peter ever find material for his blog?

12. **Peter Woit**
September 2, 2006

Anon,

Lenny Susskind.

13. **Anon**
September 2, 2006

What about Lenny Susskind?

Are you saying he's a lunatic like Lubos?

14. **Peter Woit**
September 2, 2006

Anon,

No, while from what I hear his reaction to my book and Lee's is not that different than Lubos's, he's not as nuts as Lubos. But he is, with a striking degree of success, devoting his energies into turning particle theory into a pseudo-science.

15. **Anon**
September 2, 2006

So, aside from the fact that he disagrees with you about the scientific status of the Landscape, is Susskind really the worst example you can come up with (besides Lubos)?

No wonder you spend all your time talking about Lubos, then.

16. **Peter Woit**
September 2, 2006

I don't think there's much worse you can do to go on a campaign to trash the whole idea of what it means to do science, so, Susskind is an impressive case.

Lots of other examples to talk about, but Motl and Susskind really are about the best, since each in his own way embodies well different things that have gone wrong with the field, and they have the backing of very influential institutions. Without them, there still would be plenty to write about: particle physics at its highest levels these days is pretty full of followers of Susskind and other varieties of Landscapologists. For arrogance, dishonesty and unethical behavior, besides Lubos there's always Jacques Distler.

It would be harder to come up with good material for this blog without Lubos and Lenny, but I think I'd manage fine.

17. **mathdude**
September 2, 2006

I have one question: How mathematical are both Lee Smolin
and Peter Woit's books?

I'm hoping they are not too layman oriented but instead make some attempt to
explain the basics in fairly clear fashion with some actual mathematical
discourse?

18. **woit**
September 2, 2006

mathdude,

Mine is significantly more mathematical than Lee Smolin's. But it is written without equations, more in the style of a book for laymen, so mathematically sophisticated people may find this annoying to deal with. It has some fairly challenging material in it, but in a form different than the way I would have written it for an audience of mathematicians.

19. **Lubos makes me puke!**
September 2, 2006

I just ordered both books. I have read most of what peter has written here and was not that interested to read the book. I did it just to say to that nut lubos Motl, that his slimy campaign against real science and scientist will never hide or destroy the truth. For those that can see and understand will stop at nothing to see it through to the light of day! I will do all I can to see to it that his attempts to slander those who try and have a open and honest discussion and find the truth in science, will in the end destroy his academic career.

20. **nigel**
September 2, 2006

mathdude,

As an example, Chapter 3 (Quantum Theory) in N.E.W. is where you begin to find a completely different – and more realistic – explanation of the fundamentals than you get in most popular books. There is an historical-context discussion of how the Hamiltonian operator works on vectors in Hilbert space or wavefunctions, Weyl's work in Lie groups such as U(1), the "unitary group of transformations of one complex variable" which is illustrated simply by an Argand diagram (Fig 3.1 in N.E.W.), SU(N), etc. It isn't a textbook, but is a vital supplement.

21. **Benni**
September 2, 2006

Peter, Lubos offer of 20$ for negative reviews of your book seems to be erased from his blog

http://motls.blogspot.com/2006/08/20-award-to-fight-against-review-fraud.html
or at least this link does not work anymore!

congratulations Benjamin

22. **Benni**
September 2, 2006

Lubos review on Smolins book got deleted
http://www.amazon.com/gp/product//0618551050

23. **John A**
September 2, 2006

I objected to Lubos' review. It is not for him to say what is science and what is not because someone objects to the fact that ST has not produced a single empirically testable claim.

24. **anon**
September 2, 2006

Dear Peter

I find your use of Lubos rather disingenuous. Lubos is not a poster boy for string theory. Rather, he is clearly a rather sad case of a brilliant and kind but disturbed and paranoid individual. You are making yourself ridiculous by taking him seriously. The community doesn't 'tolerate' his behavior, but mostly tries to ignore it like any reasonable person would, although a few people try to figure out how to help him.

Lenny is not crazy at all, but he loves to be provocative. But he is not a posterboy either.

Most string theorists are no more interesting/arrogant/crazy than the rest of us, (which may not be saying a lot) just more formal and mathematically inclined.

25. **CapitalistImperialistPig**
September 2, 2006

Motl's deranged reviews are annoying, but I agree that it is more useful to have them up than not. Lubos may not be the posterboy for String Theory, but he is the most visible combatant in the blogosphere. There are many prominent string theorists who read Motl's blog at least occasionally, and the failure of any of them to denounce his tactics is very telling. Either the Stalinist streak in String Theory is scary enough that even the most prominent dare not denounce it or they agree enough with him that they find him a useful tool.

I find it reminiscent of the power of the Ku Klux Klan in the old South. Most prominent citizens would mouth words like "the Klan goes to far" while being secretly pleased that these hoodlums were doing their dirty work.

On a completly different note, the review LM has up of Alain Connes newest paper is pretty interesting. Lubos may be nuts, but there is still a powerful mind in there somewhere.

26. **Aaron Bergman**
September 2, 2006

I find it reminiscent of the power of the Ku Klux Klan in the old South. Most prominent citizens would mouth words like "the Klan goes to far" while being secretly pleased that these hoodlums were doing their dirty work.

Oy vey.

Is this what it's going to come to? The 'failure to denounce' game? I get enough of that on political blogs.

27. **LDM**
September 2, 2006

The problem, it seems to me, is that the layperson reading Amazon reviews is not really in a position to analyze the scientific quality of any of Lubos opinions. If NEW was aimed a target audience of professional Physicists only — people who are quite capable of analyzing Lobos' statements –then Lobos' review would largely serve to discredit himself.
But, because NEW has a target audience that is non-technical, Lubos' reviews are unfortunate. Similary with the Trouble with Physics. However, no quantity of Lubos reviews can prevent serious researchers from reading these books and drawing their own conclusions. So in that sense, Lubos reviews are irrelevant and NEW and The Trouble with Physics are important contributions.

Based on what Lubos has posted here, I have a different opinion of Lubos' understanding of Physics, and "brilliant" is a ridiculous use of the word in his case.

28. **RA**
September 2, 2006

CIP,

Your comparison of Lubos to the KKK is stunningly inaccurate and would be laughable if it weren't so malicious and offensive. How could you? With that smear of a totally wrong stereotype about Southerners being racists, you've not only offended me but most other Southerners. If this is your and Peter Woit's idea of a way to promote an alternative to 'popular' physics then you're not going to be very profitable.

29. **Gina**
September 3, 2006

Lubos Motl had posted a few highly inappropriate comments on this blog, but I do not see why his review on Smolin's book and, in particular, the quotes you gave from his review are inappropriate. Perhaps Lubol's interprrtations on Smolin's views are incorrect but I do not understand, Peter, why you call them inappropriate.

30. **CapitalistImperialistPig**
September 3, 2006

RA and Aaron,

OK, Lubos is not the KKK. His lynchings are strictly verbal, and thus not reasonably comparable to real ones. That doesn't mean that they are harmless. His vicious attacks on Lee, Peter, and Christine, for example, are clearly intended to cause pain and destroy careers.

Those in power who witness such attacks and don't intervene (Polchinski, Maldacena, and the whole Harvard String mafia for for example), or actually join in, like Susskind, cannot pretend to be blameless.

31. **CapitalistImperialistPig**
September 3, 2006

RA – Also, I don't plan to lose much sleep over whether people might be offended by my calling the KKK (and those who supported it) racist.

32. **gunpowder&noodles**
September 3, 2006

On the whole, I have to say that I find it very educational to follow LM's activities. People often remark on the astonishing disjunction between the personae of people in real life and on the web, eg A. Bergman's claim that J Distler "isn't the man [PW] thinks he is". Sadly, all the evidence suggests just the reverse: that the blog persona is the real one, and the nice-guy-when-you-meet-him-in-person is just a facade which the individual finds it expedient to assume. I'm afraid that I think that when we deal with our fellow physicists remotely, we must brace ourselves to deal with the Mr Hyde rather than the Dr Jeckyll. Case in point: how many times have you heard, especially recently, jaw-dropping stories about the kinds of remarks people put into referee reports? Last year I had a paper accepted by a famed journal on the basis of a referee report which praised my work in fulsome tones, especially for one particular result. The catch was that I had not proved this result, nor claimed to. Clearly the referee had just skimmed the paper, got the impression that I was agreeing with his prejudices, and approved it on that basis alone. Now turn to LM's favourite activity these days: he tries to "review" all the papers on hepth on a given day; recently, even more astonishingly, he has tried it for gr-qc. The modus operandi was the one I have described: he reads enough to try to judge whether the author is a Good Guy or a Bad Guy in the interminable Western he directs in his head, and judges the paper accordingly. I think all young students should attend to his methods, because less transparent versions of the same thing are going to determine their professional fate. You can learn a lot from LM. Not about physics, admittedly…..

33. **Aaron Bergman**
September 3, 2006

"Intervene"?

This is silly. Lubos isn't a child. There's no big string theory boss who tells us all what to do. Frankly, I don't see why anyone should stop him. I don't like what he has to say most of the time, but he's got every right to say it.

Grow up and ignore him. And stop blaming an entire community for your inability to do so.

34. **Stefan**
September 3, 2006

I recently had an e-friend tell me the following:

He came upon Lubos' review of Connes' recent paper. He read what Lubos had to say. He then offered the following comments [paraphrased]:

Lubos, you seem to hold many (firmly-held) views on many topics (most of them off-topic in nature), but – despite your vitriolic remarks – very few will take you or your endless commentaries seriously until you prove your merit as a professional scientist. This can be done via: a) writing some paper that gets the attentions of people like, say, Witten, Gross, or 't Hooft, etc. or b) accumulating 500+ citations on a paper within a year of publication, or both. Until you achieve this you will only be viewed as a rabbler and nothing more.

And by the way, critiquing someone like Connes might be a wee-bit out of your league.

[Stefan]: Guess what happened next? He was banned from ever posting at Lubos' site again. Well, I guess the defender of free thought and critical dialogue wasn't feeling like his usual (over-)confident self…
Figures!

35. **Ebgert**
September 3, 2006

🙂 He he. I think it's great fun to see physicists fight in front of the public. Peter and Lee on the one hand say that Lubos's behavior is inappropriate and that Susskind is abandoning science, and Lubos and Susskind on the other hand engage in name calling, "glub glub glub" and crazy rants.

So the public is supposed to watch this and think that physicists are clever, respectable people who work hard and adhere to strict standards of professionalism and scientific integrity. And Peter and Lee can say "What are we supposed to do when Lubos and Susskind behave like that?", and Lubos and Susskind can say "Those people aren't even scientists; they're just crackpots who are jealous because we're right and they're wrong."

Beautiful. This is dragging the reputation of physicists everywhere into the gutter. Once physicists gained the world's respect when Einstein changed our views of space and time and then atomic physics showed the world what power truly is. Now you are all a bunch of squabbling children and the lesson the public will take from it is that nobody's right and nobody understands and astrology is just as good as the latest physics theories because the physicists themselves say so (although one bunch of physicists says it about one theory and a different group says it about a different theory).

So the physicists have brought disgrace upon themselves, like the philosophers did before them. Now follows a century when physicists can no longer behave so arrogantly and dismiss the other sciences like chemistry and biology as being beneath them. Serves you right, you arrogant assh*les.

36. **Anon**
September 3, 2006

Hi Peter,

You mentioned Lubos Motl, Lenny Susskind and Jacques Distler by name to illustrate what you dislike in the string community as well as Aaron Bergman as a positive figure. By chance (or not) these four are a subset of the people who write blogs and/or are very visible to the general public and the laymen.

In my opinion this fact alone confirms that your main points against string theory are sociological in nature and not necessarily accurate as such since the sampling is not representative (if you think otherwise, what are you arguments?)

If you would have technical points to raise you would name people writing influencial technical papers with whom you disagree. Now Lenny Susskind is certainly influencial but his landscape related ideas are in my opinion not in the string theory mainstream. I'm sure you could also name a handful of equally influencial string theorists who either ignore the landscape philosophy or expressly disagree with it. That is exactly what I mean when I say that your sample is not representative.

In addition the fact that you are fighting with "blog-people" explains why the mainstream ignores you. That is simply because blogging is not a scientific activity according to the majority of physicists and so whatever happens on blogs is not a concern to them. You are of course entitled to think that your blogging activity is a scientific activity but you should also acknowledge the fact that according to the majority it is not. Thus there is no reason to be surprised that your complains to the Harvard faculty are ignored. It is simply because people do whatever they please in their spare time, it's not the business of their collegaues.

To illustrate my point (it's just an illustration, don't take it literally please): if you find your wife (suppose you have one, I don't know) and Lubos having sex in one of the seminar rooms of Harvard you are 100% entitled to be mad at him. You may also think at first sight that the academic community of Harvard should somehow condemn this activity and you might also think that because the whole thing took place at Harvard the whole Harvard community has something to do with it. After one or two clear days you will however find (I guess) that this issue has to be dealt with exclusively by you, your wife and Lubos. That is because nobody broke any laws and the academic community has nothing to do what some of the faculty members are doing in their spare time and certainly I haven't seen signs in any classrooms that having sex is not allowed. Thus it would be foolish to demand a condemnation, public outcry, official statements against Lubos etc.

With your blog, Lubos's blog, your book, Amazon reviews, etc, etc, 99% of the things you are busy with the issue is exactly the same. This kind of stuff is not science thus people who care about their scientific integrity do not get involved in such issues on a professional basis, only as a out-of-office activity. Again, you may think that it is part of your professional activity but most don't and it would be much better for you if you grasped that point and would not fight with windmills.

Anon

37. **Ebgert**
September 3, 2006

gp&n:

You're absolutely right, people do tend to become more aggressive and nasty when they're writing on the internet, and Lubos is an extreme case (I've met him many times and he was pleasant every time, always very very very smiley). The one thing which doesn't change is that he sounds in person just as much like a party-political spokesman as he does on the internet.

But I would disagree that the aggressive internet persona is more "real" than the one we meet face to face. The facade we show to people when we meet them in person is just part of the collection of strategies we have for dealing with people and getting along with them, while satisfying our emotional needs. The aggregate of our strategies is our personality, so I think the techniques we use to avoid offending people who are standing in front of us have to count as a part of our personality, even though we might prefer not to have to use them, and when we are on the internet, the person isn't standing in front of us so we don't have the same pressing fear of causing offence.

Lubos is a wonderful case. Thanks to his internet behavior, Lubos will be remembered much more than Witten, when future historians of physics tell the story of string theory and its decade or so in the limelight. The internet is the human collective consciousness, and Lubos is there, visible and loud, while Witten is nowhere to be seen.

Also, congratulations to Peter (and Lee). Thanks to you, the string bubble has burst, like the housing bubble and the dot-com bubble before it. Your voices have been heard – the emperor has no clothes. Every scientist outside of string theory has been waiting for somebody to do this, and it is truly a joy to see Michael say that "serious scientists" don't agree with you, because he just means "string theorists". In fact, the string theorists are the only ones who don't agree with you. And they may be the only one who truly count in their own minds, and this is wonderful as well, because they will soon realise how little their opinions count in the real world.

From now on, every mention of string theory in the public media will include a comment or two from "the critics", "for balance". No more uncritical praise for string theory from the media. The tide is turning against them. The word is out – string theory has nothing to say about the real world. The only thing that can save them now, in the eyes of the media, is a prediction about the result of an experiment. But the string theorists know, even more than everybody else, that this will never happen. All of the pep-talks at conferences, "One day, we'll all be heroes!", it was all propaganda, and it may have stirred your heart, Aaron, but you can't avoid that sinking feeling in the same heart – you've wasted your life, and it's too late to stop, too painful to admit defeat.

Hahahaha! 🙂

By the way, the string bubble was on top of a physics bubble. It was the culture of arrogance and aggressiveness in particle theory that allowed a bunch of cocky cultists to take over: "We don't need no rigor; we don't need no experimental results to back up our indubitably correct guesses and hopes. We are the string theorists and the proof of our correctness is our manifest superiority over every other human mind." Well, unfortunately the culture of physics is such that people who say things like that become the "glorious leaders" of all of physics, and when they go down, the respectability of all of physics goes down with them, glub glub glub.

38. **Gina**
September 3, 2006

Stefan,
Something does not add up in what you wrote: Lubos' comments on Connes' paper were overall very positive (a bit funny to read though).

39. **Ebgert**
September 3, 2006

Anon,

You are right; it is not science, but that is beside the point. The battle is being fought in full view of the public, and it is a battle for the public opinion. It used to be the case that the public opinion was that string theory is right because the people on television and in the newspaper say so. Now the people on television and in the newspaper don't say so any more. The string theorists have lost the battle.

The fight over scientific issues, like whether the landscape exists, will take place within string theory, and the conclusion, in case you haven't been paying attention, is that the landscape does indeed exist, at least in so far as anything within string theory "exists". Lubos and the people who "disagree" with Lenny are not saying that the KKLT vacua aren't solutions of string theory. They're saying that maybe maybe maybe some as yet unknown and unpredictable something might possibly happen that will make everything OK again, so that string theory can go back to the way it was *before* Lenny pointed out the landscape.

Hahahahaha! That's like saying maybe maybe maybe somebody sometime will find some way to evade Goedel's theorem, so that all of mathematics will become provable again. That's why they're in the minority. They're the die-hard, have-faith, never-give-up-and-never-look-facts-in-the-face hardcore believers. Faith over reason. That's who the people who "disagree" with the landscape are.

The “moderate” people like Aaron try to strike a middle ground, saying that the majority of string theorists “don’t work on the landscape”. Hahahahaha! That’s like saying “Well, I know that it’s been proven that what I’m trying to do is impossible, but I don’t concern myself with what has and hasn’t been proven to be impossible.” Hahahahaha! 🙂 Keep up the good work, guys.

40. **Farrold**
September 3, 2006

As best I can tell from Amazon’s data system, the tag “crackpot” has been used 13 times on Amazon, 11 of them by Motl. Fascinating.

Meanwhile, in the Physics bestseller category this hour, the rankings run Smolin, Woit, Greene, Penrose.

http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

41. **David**
September 3, 2006

Stefan,
I learned from CIP that Lubos can only ban about 20 people at any one time. I was banned for a while but it was lifted when, I guess, he was madder at 20 other people than he was at me. This comment may free up someone else again. I don’t comment on Lubos’ own postings any more; he just deletes or refuses to discuss things in a rational manner. His treatment of Bee recently was, in my view, over the top. I do like to, sometimes, comment on what others are saying.

Peter,
As I’ve said before I got the UK edition of NEW early and I thought it was great in the sense of making me think about and appreciate some things I hadn’t thought about before. Thank you. I should be getting my copy of Lee Smolin’s book in the next few days. I look forward to a similar experience with that. WRT Motl he talks about what it means to do science and be a scientist but then ignores that in many of his posts. He knows what others should do but he doesn’t do it himself. If I was in string theory or at Harvard, I would regard him as super embarrassing. With his public postings and his record on SPIRES, I don’t see how he’ll get tenure but I’ve been wrong about that before. In any case, “This too shall pass”. In the long run, people will appreciate your behavior esp. in comparison to LM. You’re wise to leave his comments here for others to see.
Best wishes,
David

42. **Ebgert**
September 3, 2006

David,

Indeed, Lubos is a hypocrite. But if you are an American living in the McCarthy era, you can get tenure even if you spread lies saying that communists eat babies. Similarly, if you are in the Soviet Union, you can get tenure while saying that Americans eat babies. Today you can get tenure in Iran for saying that Israelis eat babies or you can get tenure in Israel for saying that Iranians eat babies. So Lubos’s statements that string theory critics eat babies will not do any harm to his career. Anybody who thinks that string theorists are more capable of a balanced judgement than Americans were during the McCarthy era is naive. Human nature has not changed for hundreds of thousands of years, let alone a single generation.

PS. Nobody eats babies.

43. **Gina**
September 3, 2006

Ebgert,

Your Godel’s theorem example goes the opposite way. There was a “foundational crisis” in mathematics based on the fear that its foundations are not provably sound. Godel’s theorem confirmed this fear but strangely this was the end of the “crisis” and mathematics continued as before. There were people like Brower that thought and taught that mathematics should be done completely differently in view of these problems but his views did not prevail. So if “landscape” to “string theory” is like “Godel’s theorem” to “mathematics” you can expect a bright future for string theory.

44. **Alejandro Rivero**
September 3, 2006

I think you people are getting a tendence to exagerate 🙂

“Human nature has not changed for hundreds of thousands of years” —> there is not evidence of language skills from human species 200 000 years ago. And remember that mithocondial Eve and Y-chromosome Adam are estimated between 80000 and 35000 years ago.

“There was a “foundational crisis” in mathematics based on the fear that its foundations are not provably sound”.@ –> actually there was some different approachs to foundations, Godel incorporated early into formalism and proved was doomed.

” Lubos’ comments on Connes’ paper were overall very positive ” —> Early comments were possitive. A longer (but not deeper) reading of the paper, done without the background of axiomatics of ncg spaces, led him to a fast dismiss.

45. **MathPhys**
September 3, 2006

Peter,

I think I’ve noticed something quite interesting taking place over the past two weeks, which I think coincides with the appearance of discussions of Smolin’s book in addition to yours.

Namely a number of people who normally won’t think at all about particle physics, approached me, and undoubtedly others, asking me if I’ve heard of or read two recent books that claim that string theory is totally wrong, particle physics is a mess, etc.

What I want to say is that I’m struck by what seems to be a sudden awareness amongst the public of what’s going on.

It seems to me that in spite of what people like Lubos and Susskind are saying on public forums, or doing on amazon, you and Smolin, rightly or wrongly, are winning the public vote.

46. **Ebgert**
September 3, 2006

Gina,

I agree with most of your points. In fact, Brouwer was correct, and mathematicians didn't pay as much attention to him as they should. Instead, they said "What's that weidro talking about? Never mind, who cares? Let's ignore him and call him a crackpot."

But mathematics lost a lot of its pomposity thank's to Goedel's theorem. They became humbled and realized that they were not talking about the Platonic ultimate truth, but rather about what results could be obtained from specific starting points using specific procedures. Al least, the logicians realized this. Perhaps it hasn't penetrated the thick skulls of many mathematicians yet (it has been less than a hundred years). Certainly we can't expect the physicists, with their little brains and their big egos, to understand the consequences for many generations. The physicists, string theorists in particular, will continue to insist that they are talking about the ultimate truth for the foreseeable future, but thanks to Lenny Susskind, we have a proof that they (the string theorists) are not.

> "Human nature has not changed for hundreds of thousands of years" —> there is not evidence of language skills from human species 200 000 years ago. And remember that mithocondial Eve and Y-chromosome Adam are estimated between 80000 and 35000 years ago.

Please see the wikipedia entry on [human evolution](). My statement stands. Humans have *not* evolved significantly since the McCarthy era; if you think otherwise you are wrong.

47. **Ebgert**
September 3, 2006

Mathphys,

Let me offer a different interpretation. The public can't remember or distinguish between Lee, Peter, Lubos or Susskind. To them, they're all "some guy". But what they can do is smell blood. And they like it. 🙂

48. **Gina**
September 3, 2006

Ebgert,

hmmm, I am glad that you agree with most of my points. You come across as being rather hostile towards physicists and mathematicians. This looks unfortunate to me. Overall, it is not dificult to create public hostility against intelectuals, scientists and various other minority and/or "elite" groups. But I do not think this is a very good path to follow and I doubt if this is what Peter or Lee intend to do in their critiques. Brower, right or wrong, was also a mathematician and he probably also had an "ego" (a term coined by yet another intelectual).

" The physicists, string theorists in particular, will continue to insist that they are talking about the ultimate truth…"

So is this what all this debate about? that physicists/string theorists/mathematicians/ will concede that they are not talking about the ultimate truth, and when they do they can continue developing their scientific disciplines as before?

49. **Ebgert**
September 3, 2006

Gina,

Do you know why Brouwer started constructivism? Or why intuistionistic logic was considered a desirable thing at all? The mathematicians who I declared to have "thick skulls" are the ones who say "No, we do not know and we do not care, and because we don't know about it, it isn't worth knowing." It is this attitude that I am hostile to, and I will continue to be hostile to it, because there is a difference between the arrogance of the ignorant and the intolerance of the educated. If those who are well-informed bow down before the ignorant and say "Ignorance is just as good as knowledge, and the ignorant are as just as qualified as the knowledgable" then the present situation will continue, with the ignorant becoming glorious leaders who can lead whole disciplines astray. I dare say that if Brouwer had been a little less tolerant towards his contemporaries then mathematics might be in a much better state than it is today.

I am not, of course, arguing that everybody should be intolerant towards everybody else. What I am saying is that there is a *clear* difference between well-formed arguments and proven theorems on the one hand and vague assertions of intellectual superiority and dismissiveness on the other hand. The two are not equally acceptable. Intolerance and hostility *should* be shown towards those who say "We have not proven what we say, but we are right because we are great and intelligent", which is the message of the string theorists. They deserve to be greeted with intolerance and hostility. Bowing down and submitting and saying "Oh, why yes, whatever you say must be right, and you must be more intelligent than me because you say so" is *not accepable* and it must *never* happen again, because it has brought (fully deserved) humiliation to physicists.

> So is this what all this debate about? that physicists/string theorists/mathematicians/ will concede that they are not talking about the ultimate truth, and when they do they can continue developing their scientific disciplines as before?

You do not understand. The mathematicians/physicists are following a certain procedure. If they think that they are discovering the ultimate truth, then they will do one thing. If they understand that they are not, then they will do another (different!) thing. Consequently they will not simply "continue developing their scientific disciplines as before". Do you not understand that there is a difference between mathematics as practiced today and constructivist mathematics, for example? Do you not think that it is even worthwhile to be aware of this difference?

You think that when somebody says: "You are doing the wrong thing because you are making a mistake. Please understand this particular point …", that is the same as saying "I want you to unthinkingly repeat this sentence, and then carrying on doing what you were doing before." There is a difference between these two things.

50. **Ebgert**
September 3, 2006

Gina,

I realize I'm blabbering on a bit much here (actually, I'm kind of enjoying swapping between blog blabbering and blabbering at an actual party, which may explain why I'm so incoherent). But the stuff about Brouwer isn't really that relevant. The point I was originally making about Goedel isn't that his theorem killed off any possibility of future progress in math, but rather that it would be foolish to spend the careers of thousands of people on the hope that somehow sometime somebody will find some way to return to the way things were before we knew about Goedel's theorem.

Lubos's argument about the landscape, and, implicitly, the thinking of the string theorists who "don't work on the landscape", is that maybe somehow things might someday return to the way they were before we knew about the landscape and the discretuum of vacua. That's why I was making the analogy. Your criticism of my analogy seems to be based on the interpretation that I was saying that Goedel's theorem killed math, so the landscape should kill string theory. I wasn't saying that.

51. **TheGraduate**
September 3, 2006

Studies in Human evolution is another area of science filled with much 'religious' fervor. It is also another branch of science that wants to provide a TOE (for all things human) and tends to try to make it's case to the public directly. It is also interestingly enough a group that makes its case by arguing that its opponents are idiots and don't deserve to be listened to or even argued with.

I have never waivered in my support for evolution as the explanation for why we are all here and yet I have never waivered in my conviction that this mode of argument is beneath 'real' scientists.

52. **Fort Knox**
September 3, 2006

Just for the record, a recently deleted blog entry about a $20 prize:

http://tinyurl.com/jmsca

53. **Gina**
September 3, 2006

Ebgert,

I see. Also while I did feel some hostility towards math physics etc. in your earlier comment I did not notice it in your later one so maybe it was not really there. From what I heard, while mathematicians regarded Goedel's Theorem as a beautiful piece of mathematics it almost did not influence they way mathematics is practiced and not so much the way mathematicians regard mathematics. In any case, any way you want to take your nice analogy it comes pro-string theory.

I also think from what I heard that while Brower ideas and constructivism had some influence in some areas of computer science, they did not have significant influence on mathematics itself. But at the time when Brower went lecturing about his ideas he was accepted almost as a hero and people where excited from the prospect of a revolution in mathematics.

Of course, I cannot say if you are right when you say that Brower was right. It looks that this is not what most mathematicians think and probably very few really know the details of constructivism. Maybe others can comment more about it.

Be that as it may be, Brower had some alternative theory. Is the anti-string movement came to the point where there is some alternative theory (even if very partial or incomplete or a little strange)?

54. **Stefan**
September 3, 2006

So, after all these posts and endless arguments–counter-arguments we (most of us atleast) seem to have arrived at a broad concensus:

Ignore that little guy from Cambridge, and move on to real discussions!

Topic 1:
Where is Connes heading with his programme?

Topic 2:
What background is necessary to work on the "Yang-Mills and Mass Gap" problem outlined by Witten and Jaffe [Clay Millenium Problems]?

55. **Alejandro Rivero**
September 3, 2006

Topic 2 amusingly should be in Woit's early training, according the wikpedia: "Peter Woit's earliest work verified Edward Witten's 1979 quantum chromodynamic formula for the eta-prime mass in terms of the second derivative of the vacuum energy.". So perhaps a blog entry will appear some day. The question of course is not about a mass sum rule, but about a proof of a gap in the mass spectrum of QCD, ie about the fact that QCD-binded objects get some mass.

Topic 1 will move this week, as Connes will take a couple hours in the Newton Institute to expand about this preprint. Connes his goal I can not tell, it is a sort of cross fertilisation between gauge theory and geometry and in principle it is one of a series of examples to clarify which is the right generalisation of (differential) geometry to algebras beyond the commutative case. Physics could benefit because the axioms seem to impose restrictions to the possible gauge groups and its representations.

In deeper layers this programme would help to understand foundamentation of QFT. For instance it is amusing that the absence of anomalies, which is a quantum requeriment, appears here as a geometric requeriment, Poincare duality. Also, there is a paralell programme running, by Connes Kreimer Moscovici and some other interested people, looking at the renormalisation group from new perspectives.

56. **Tim Swanson**
September 3, 2006

"It is being distorted by the visitors of Peter Woit's blog who are directly controlled by the author of this book."

That's why I always wear a tin-foil hat.

57. **Boaz**
September 3, 2006

anon raises an interesting question that I see cropping up here somewhat regularly. Namely: "what is the relationship between the online blogging community and the rest of the physics world?"
He (she?) says that the conflict between Peter and Lubos is irrelevant because the activities going on in the blogs are not a part of the professional activities of being a

scientist or academic. But there is clearly some overlap and this relationship is changing and being negotiated on a daily basis. If that poster is still reading, I wonder if he could comment on what he thinks a more appropriate role for blogging would be in professional scientific discourse?

58. **Bob McNees**
September 3, 2006

Peter,

It's silly to claim that the "larger string theory community" is "unwilling to publicly criticize Lubos's tactics." Most string theorists are unaware of, or don't care about, the exchanges between the two of you.

All of your exchanges with Lubos have two things in common: you and Lubos. If you don't like him, ignore him. If he goes to far then deal with it as you see fit, but don't expect someone else to do it for you.

There are lots of string theorists who read your blog. I think that most of them, like any good scientist, are genuinely interested in and open to criticism. Treating them as if they are part of some nefarious string theory conspiracy, intent on silencing a critic, will only convince them that you don't have anything worthwhile to offer.

59. **Thierry M.**
September 3, 2006

I was considering buying Woit's and Smolin's books, since I am deeply interested in theoretical physics in general and string theory in particular. I changed my mind. Reading the last post on this blog, what I see is some childish dispute about negative reviews being pushed in and taken out of Amazon.com. Who do you think cares about this? This is ridiculous. Where are the deep, noble thoughts and demeanor of people like Einstein and Bohr? Are they being replaced by ridiculous disputes and insult exchanges between angry bloggers? This is very disapointing, very sad, because I thought that blogs would bring a real progress in the exchange of ideas in physics. But this proves I was wrong. Just the opposite is happening. After all, you are no Einstein nor Bohr, a thing I should have realized from the beginning. I will now remove all physics blogs from my browser favorites. I don't want to have my mind polluted anymore by such rubbish.

60. **CapitalistImperialistPig**
September 3, 2006

String Theory is not falling into disrepute in the public mind despite the antics of Motl and Susskind, it's falling into disrepute because of them. The public may not be able to understand anything about the scientific (or, more realistically, philosophical) questions at issue, but they can tell who is wearing a black hat. When one side bullies and blusters, and the other responds with measured argument, it's obvious who the good guys are.

A bully usually has a crowd of hangers on. They don't seem to stick around long after he takes a punch, though.

61. **woit**
September 3, 2006

Woke up late today, then after a lazy morning logged in to find far too much here to deal with, and in my e-mail a link to a review of my book and Lee's by Susskind. I'll write a bit later about the Susskind review, and, just to brace you for the shock, I'll have some positive things to say about it.

I deleted a bunch of the more off-topic comments here, will try and respond briefly to a few of the others:

anon (who thinks I'm being disingenuous about Lubos):

I'm using the term "poster boy" in the sense not of someone who is representative of a problem, but an extreme, exaggerated case of a problem, as in posters of exceptionally cute and suffering children used to get people to contribute money for medical research or to alleviate poverty. Lubos is in no way representative of string theorists, but his arrogant conviction that people who disagree with him are idiots, that string theory is the only possible way forward for particle theory, that string theory has been hugely successful, etc. are exaggerated forms of attitudes that I've found to be all too prevalent among string theorists.

Anon (who thinks I don't have a representative sample),

I mentioned certain people explicitly because they are ones who publicly make their views known, thus inviting a public response. I don't think I've anywhere claimed that they are typical of string theorists. Among string theorists whose views I'm aware of, but haven't mentioned, one large group consists of perfectly reasonable people who are responsible scientists, with whom I just happen to have a scientific disagreement about the prospects for string theory. There also are a significant number of others who, in one aspect or another, exhibit Lubosian behavior. Some of these do this as anonymous commenters here and I don't know who they are. In other cases I've witnessed or have reliable first-hand accounts of such behavior, but I don't think it's appropriate to identify such people here. I'm not in any way claiming to know what fraction of the string theory community thinks what.

I don't think that your claim that Lubos's blog and his attacks on critics of string theory is, like his sexual behavior, purely part of his private life, and not part of his professional life, is in any sense supportable. I doubt he feels that way, for one thing. The whole question of what role blogs play in the professional activities of scientists is a complicated and interesting one. I see this blog as part of my professional activity, if you ask Jacques Distler, I suspect he sees his the same way (although he sees mine differently…).

As for what I think Harvard or his colleagues should do about Lubos, let me make clear that I complained to them specifically about his offer to pay people to write bad reviews of my book (and this offer seems to recently have been taken down). For the rest, all I'll say is that if one of my colleagues in my department was behaving the way he is and I was aware of it, he'd get an earful from me about what he was doing, and if he kept doing it, I would take steps to make sure that the targets of his behavior knew I did not support what he was doing.

62. **John A**
September 3, 2006

Lubos' disgraceful review of Lee Smolin's book has been removed. I wonder how long it will be before a) Lubos posts yet another review and b) Amazon gets tired of hosting him.

63. **CapitalistImperialistPig**
September 3, 2006

Aaron – *"Intervene"?*

This is silly. Lubos isn't a child. There's no big string theory boss who tells us all what to do. Frankly, I don't see why anyone should stop him. I don't like what he has to say most of the time, but he's got every right to say it.

Grow up and ignore him. And stop blaming an entire community for your inability to do so.

Lubos has every right to engage in a campaign of libel and personal vilification? I have my doubts. Since Peter has made himself a public figure in this debate, winning a libel suit might be difficult, but Lubos has been extreme enough that it might be possible. It is possible that Harvard is liable for any damages too, since they give him the platform, let him use their logo on his web site, etc.

Maybe you should check it out Peter. The publicity should be great for your book. And Harvard has really deep pockets if you win!

Is ignoring and permitting a crime itself criminal? I'm not sure, but I'm sure it isn't innocent.

64. **woit**
September 3, 2006

CIP,

I'm not the litigious sort, generally of the opinion that if one finds oneself hiring lawyers for anything other than routine paperwork, one has made a bad mistake of one kind or another. As for suing Lubos, my father was a lawyer during the early part of his career, and one piece of wisdom he imparted to his sons was "never sue anyone who doesn't have any money". On the question of my extremely wealthy alma mater, I seriously doubt that, if push came to shove, the administration there would support illegal behavior by one of its faculty members that might be tied to the institution. The fact that Lubos removed the offer that I complained to them about may be evidence for this. I kind of doubt that he did this unprompted.

65. **Aaron Bergman**
September 3, 2006

Lubos has every right to engage in a campaign of libel and personal vilification?

Libel has a very specific legal meaning. Being a dick is not included, the last I checked. I don't think much of the internet would survive if it were.

66. **woit**
September 3, 2006

Aaron,

My layman's understanding is that, legally, libel is the act of writing and publishing untrue things with reckless disregard for whether or not they are true, for the purpose of defaming someone's reputation. It appears to me that there is a good argument that Lubos's reviews of my book and Lee's on Amazon are examples that fit the definition.

I suspect though, that there is an insanity defense against libel accusations, and it appears that Lubos would have no trouble finding colleagues who would testify on his behalf.

67. **David**
September 3, 2006

The Graduate,
I don't think it's fair to compare an evolutionary biologist to LM. Remember evolution has 150 years of data supporting it, while string theory, at present, has zip. Michael Shermer's "Why Darwin Matters" clearly points out the differences.
Best Wishes.

68. **Gina**
September 3, 2006

In my opinion none of the strong sentences that Peter quoted
from Lubos' amazon critique:

"Lee reveals his intense hostility against all of modern physics", "Lee proposes a truly radical thesis that it is wrong for mathematics to play a crucial role in theoretical physics", "He also denies the difference between renormalizable field theories and the rest", "one of his rules says that the conclusions must be accepted by everyone if their author is a person of good faith"

Can be regarded as libel. (I am not a legal expert though.) As I said, I even did not find his Amazon critique inappropriate unlike some of his posting on this blog. (And some comments on him do sound like libel.)

I cannot understand the obsession with Lubos. It certainly does not support in any way the case you are trying to make, Peter. (OK maybe I can undesrtand it. Still I think it does not support the case you are trying to make and the quality of this blog.)

On the other hand, Stefan cncensus is good and his topic 2 looks like a great topic.

(I have a wonderful romantic theory though based on maximum nicyhood principle to explain why for Lubos who gave up old strings with his home, family, neighborhood and homeland for the sake of new strings in string theory, his vested interest in these string theory is unusually high which explains everything.)

69. **TheGraduate**
September 3, 2006

To David:

Please re-read my comment.

I wasn't saying either string theorists or evolutionary theorists are ultimately wrong. All I said was supercilious dismissal of those that disagree with you is a tactic that should be beneath every serious scientist … unfortunately, it's not.

70. **Benni**
September 3, 2006

peter your review of lees book was deleted

71. **King Ray**
September 3, 2006

I think that if Lubos keeps spending all his time blogging and reviewing other people's work, instead of doing research as he is being paid to do, he is going to perish academically. If he goofed off that much at a real job, he would be laid off for lack of productivity.

72. **David**
September 4, 2006

The Graduate,
Sorry, I must have been unclear. What I meant was that the biologists aren't taking the point of view you suggest but rather the ID/creationists are misrepresenting what science is and often the known facts as well. The public can often be fooled about technical issues and people in science don't know everything either. Further, misrepresentation is, in my view, what LM is often about.

73. **MathPhys**
September 4, 2006

Thierry,

I thought you left us a long time ago. I hope all is well.

74. **Arun**
September 4, 2006

I'm beginning to be confused about who represents what. Lubos is a charming fellow, we're told, though his blog output, which is all that I have available to make a judgement, doesn't reveal any hint of charm. That means Lubos's own blog does not represent himself, unless when we say "Lubos is charming" we really mean "Lubos is charming, except when he is obnoxious". If on-line activities do not represent a person, then no wonder no one cares about whether various tactics used are ethical or unethical, because they do not represent the person.

75. **Roger Schlafly**
September 4, 2006

Peter, please don't get paranoid like Lubos. You don't know whether Lubos got your review deleted. It could have been other string theorists, who may or may not have been reading his blog. With you and Lubos publicizing this deletion war, others may get into the act.

76. **Eli Rabett**
September 4, 2006

It is very simple, when Motl goes up for tenure send copies of his very best to the Harvard Corporation.

77. **Christine**
September 4, 2006

Motl's most impressive record of bad behavior is probably this one (see the comment's section), attacking a quantum gravity student in the occasion of her grandmother's death.

Peter Woit, please do forget about this tragic personage. I know this theme can be quite entertraining for many, but I think it is enough for some time already.

Best wishes,
Christine

78. **woit**
September 4, 2006

Roger,

Sure, you're right, there's no way to know. But, mysteriously, in the past, Lubo's Amazon reviews have often appeared with an almost simultaneous large number of "helpful" votes. Maybe he has a large number of people who agree with him and closely follow his activities, maybe he's pretty adept at manipulating Amazon's system.

Christine,

I pretty much do ignore what Lubos writes on his and other blogs (the example you gave of grotesque behavior is far from the only such), but the (succesful) attempt to maniupulate Amazon rankings is kind of different and seemed worth pointing out to people.

79. **David**
September 5, 2006

Christine,
While, of course, Lubos was nasty in the example you gave he can be, and often is, much worse than that. While by searching archives, you could find these worse examples (Mahndisa points one out in your example), I suggest you save yourself the time. Just watch The Reference Frame from time to time and you'll certainly see more of these. BTW I'm sure Mahndisa appreciated your comment. I also agree with Peter about the Amazon rankings. There LM is using misrepresentation and misinformation to try to keep information from as many people as possible. This is not what one expects from a Harvard Asst. Professor.
Best Wishes.

80. **Christine**
September 5, 2006

Then if this post is to be really useful for the general public, I suggest you make it appear as on of the first links when one searches google on LM.

Best wishes,
Christine

81. **D R Lunsford**
September 5, 2006

This is very interesting:

http://en.wikipedia.org/wiki/Right_Wing_Authoritarianism

One can easily see evidence of this behavior point by point in LM and his ilk's behavior (see "Significant Correlations").

-drl

82. **Anonymous**
September 10, 2006

BTW, a similar situation to the particle physics is beginning to develop around Big Bang, dark matter and black holes in astrophysics.

83. **Who**
September 10, 2006

BTW, a similar situation to the particle physics is beginning to develop around Big Bang, dark matter and black holes in astrophysics.

Anonymous, I don't understand what you are saying. Could you be a bit more specific?

84. **Who**
September 10, 2006

The last amazon sales rankings on this thread on 2 September over a week ago, when it looked like this:

**as of today 12:15 pacific, or 3:15 PM your time, the Amazon general physics bestseller list was

#1 TwP
#2 Elegant Universe by Brian Greene
#3 NEW
#4 Douglas Giancoli college physics text
#5 a Stephen Hawking book**

http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

As of 9:30 AM pacific this morning 10 September the lineup was roughly the same. Some titles added for comparison

#1 TwP
#2 NEW
#3 Elegant Universe (Greene)
#4 Douglas Giancoli college physics text
#7 BHoT (Hawking)
#10 RtR (Penrose)
#27 Cosmic Landscape…Illusion of Intelligent Design (Susskind)
#28 Hyperspace…Parallel Universes, Time Warps… (Kaku)
#30 Warped Passages (Randall)

http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

85. **dan**
September 11, 2006

Hello Peter and Lee,

Where would be the best place to discuss Lubos' review of your books (NEW, TROUBLE) from amazon, and your response to Lubos' criticism?

Issues to consider:
Is Lubos criticism of NEW/TRouble substantitve?
Does he offer strong counter-arguments?
Is Lubos factually correct in his criticism?
What are some counter-arguments to Lubos' claims?

For example on Smolin's & Woits book Lubos claims
"For example, he dedicates dozens of pages to speculations about the divergent amplitudes at finite orders of the perturbation theory – amplitudes that have been proven to be finite. "

Is Lubos factually 100% correct, and if so, then Smolin & Woit would be incorrect?

Another Lubos critique of Woit and Smolin

"There are also frequently repeated speculations that string theory and M-theory don't exist and many other similar "ideas", together with the most popular myth that string theory can't be experimentally tested. Neither of these things is supported by any results in the scientific literature"

Incidentally Lee, if you are reading this Lubos says

"The interactions between Lee Smolin and mainstream physicists are interesting. Lee often visits us. We smile at each other and Lee is being politely explained why his newest theories can't really work. Lee says that he understands these arguments. Then he returns to a conference or a journalist and repeats that all of his theories have been perfectly proven, while offering even more unusual theories. The newest theory says that the neutrinos are octopi swimming in the spin network. Believe me, we like him but it is not always easy to take him seriously."

Is Lubos factually correct about Lee? Has the unnamed Harvard string faculty taken the time to explain to Lee why he's wrong, and Lee ignores it?

86. **woit**
September 12, 2006

dan,

If you want to discuss Lubos's views, please do it at his blog. I won't be participating, because it would be a huge waste of time, and, in any case, I'm banned from posting any comments on his site. In the past he has deleted anything I've written there (which should give you some idea of how interested he is in discussion with anyone who disagrees with him and knows what he talking about).

As for the points you bring up, Lubos is just lying. Superstring amplitudes have not been proven to be finite above two-loops, and string theory cannot be experimentally tested. From past experience, there's no point in trying to discuss such questions with him, he's a pure ideologue with no interest in what might actually be true. I've written a posting here responding to his absurd and dishonest "review" of my book, but am not going to spend more time on this.

I definitely agree with commenters who have complained that there is too much Lubos discussion here. Please don't submit more unless there is something extremely new and interesting to be said on the subject.

Hold Fire! This Epic Vessel Has Only Just Set Sail…

September 3, 2006
Categories: Not Even Wrong: The Book

The August 25th issue of the Times Higher Education Supplement has a feature article by Leonard Susskind about my book and Lee Smolin's entitled "Hold Fire! This Epic Vessel Has Only Just Set Sail…" (unfortunately only available to subscribers). The bulk of it consists of two parts: an extended analogy designed to show what he thinks the current state of string theory is, and a long ad hominem argument about why people shouldn't listen to me and Smolin.

In Susskind's analogy, the current state of particle physics is like the 15th century European view of the world, aware that there was a large Atlantic ocean out there, but with no idea of what lay beyond it. String theorists are like ship-builders, building vessels that intrepid string theorist explorers will courageously pilot out into the risky unknown. I'm a "Chicken Little" figure, telling people that if they do this they'll fall off the end of the earth. Smolin is a builder of ships that don't float.

Susskind mostly ignores the contents of my book and Smolin's, which, in his analogy, both provide detailed analyses of the history and current state of a shipbuilding project, which, despite massive investment, has led only to a huge, overweight vessel which can't even get out of the harbor. Both of us are arguing that this project needs to be restructured and largely abandoned, and investigation of other ship designs supported and encouraged.

The part of Susskind's long ad hominem argument that attacks Smolin is just stupid, vicious, and offensive and I won't repeat it here. Given how limited the successes of string theory have been, his attacks on Smolin's work as ideas that are not working out is completely indefensible.

Susskind devotes a surprisingly long part of the article to discussing me and my career, and I have to admit that what he has to say is, while less than completely accurate, far more sympathetic than I would ever have suspected, especially given the many harsh things I've had to say about him here and elsewhere. He describes me as "one of those Princeton mavericks, who had the guts to work on other questions, in particular modern nuclear physics ", and criticizes (during the mid-eighties) "an unusual degree of hubris in Princeton, a smug, arrogant dismissal of any ideas that didn't fit the string theory agenda."

Susskind's interpretation of my early career is sympathetic, but a bit off. I actually left Princeton in the summer of 1984, just before the string theory "revolution" hit. I spent the early years of the era of string theory dominance at Stony Brook, with limited contact with what was going on in Princeton. Susskind doesn't quite directly say so, but he strongly implies that my criticisms of string theory are motivated by bitterness at not being able to have a successful career in a physics department due to the domination of string theory. What actually happened is that in 1987, after my postdoc at Stony Brook, I did find myself unemployed, and at the time wasn't too happy that string theory dominance was one of several reasons no one was much interested in hiring me. I spent a year as an unpaid visitor in the Harvard physics department and got a part-time job teaching calculus as an adjunct instructor at Tufts. During this year I had plenty to live on, but did face an uncertain future and wasn't so happy about it. People at Harvard and at Tufts were quite helpful, and in the spring I was offered an excellent job for the next year at MSRI, the math institute in Berkeley. After that I came to Columbia, and from my time at MSRI on, I have no complaints whatsoever about my career, feeling I've probably done better than I deserved, living in the places I most want to live, working with excellent colleagues under good conditions. So, as far as the embittered part of my career goes, it was pretty much limited to a short period of about a year, almost 20 years ago, during 1987 and 1988.

Susskind ends his discussion of me with something positive:

But Woit is correct to remind us how important diversity and humility are in the face of the vast sea of ignorance.

and ends his review by quoting ‘t Hooft as a sceptical critic of string theory, finishing with:

This leads ‘t Hooft to another important point: diversity of viewpoints is to be cherished, not suppressed. This is something that Woit and Smolin have properly reminded us of, and string theorists should not be allowed to forget it.

So, all in all I've quite mixed feelings about this piece. Susskind's attack on Smolin is highly reprehensible, and the way it ignores discussion of real issues, concentrating on dubious analogies and ad hominem argument, is disappointing. But, I have to admit that in his more than charitable discussion of one of his fiercest critics he shows a capability for gentlemanly behavior I wouldn't have suspected (and wish he had shown Smolin), and, in the end he recognizes and admits that Smolin and I are making an important point that string theorists need to take note of.

Update: Several people have pointed out that the same issue of the Times Higher Education Supplement also includes a quite positive review of Not Even Wrong by Philip Anderson. On the whole it's accurate, although I think Anderson neglects to mention that, lacking experimental results, I'm much more of a believer in the possibility of using mathematics to make progress in particle theory than he is. There are quotes from and discussion of the review at a new blog here.

Update: The THES in a later issue has a letter about Susskind's article, which correctly points out that answering criticisms of string theory by claiming they come from a "mid-level theoretical physicist" or a member of the "Chicken Little Society", didn't address the fact that in the same issue these criticisms were coming from an extremely distinguished theorist and Nobel Laureate (Anderson). The letter writer's reaction to Susskind's article was:

Moreover, Susskind's defence of string theory not only failed to address Anderson's key criticism of string theorists – namely that their theorising is not grounded "on the acute observation of nature" – but rather reinforced this impression.

Comments

1. **JC**
   September 3, 2006

   Peter,

   How big was string theory at Harvard around 1987-88?

   When did the string people take over Harvard's particle theory group?

2. **Ebgert**
   September 3, 2006

   > Susskind's attack on Smolin is highly represensible

   I think that's a typo.

   Is it fair to say that Susskind is attacking Smolin more than you because Smolin is offering a competitor to string theory, while you aren't?

3. **Boaz**
September 3, 2006

I haven't read the article, but I wonder if Susskind's ship metaphor comes from the following quote by V. Weisskopf (if so, its a pretty bold move to try to put string theorists into the role of the ship builders!):

There are three kinds of physicists, as we know, namely the machine builders, the experimental physicists, and the theoretical physicists. If we compare those three classes, we find that the machine builders are the most important ones, because if they were not there, we could not get to this small-scale region. If we compare this with the discovery of America, then, I would say, the machine builders correspond to the captains and ship builders who really developed the techniques at that time. The experimentalists were those fellows on the ships that sailed to the other side of the world and then jumped upon the new islands and just wrote down what they saw. The theoretical physicists are those fellows who stayed back in Madrid and told Columbus that he was going to land in India.

4. **Carl**
September 3, 2006

I ordered the two books by Woit and Smolin from B&N about a week ago. They arrived by UPS about 48 hours later. I started reading them two days ago. Very hard to put down. I highly recommend both books. The effect is to invigorate me to learn more about string theory and the alternatives.

So far my only complaint about Woit's book is that in his description of QM he suggests that it is not possible to use vectors to represent quantum states, but that spinors are required. This is commonly believed but it is not true. The density matrix representation of a quantum state can be written as a combination of scalars and vectors (i.e. multivectors of Clifford algebra) with no spinors needed. One can then split the density matrix into things that act like spinors by pre and post multiplying by a constant "vacuum" spinor.

This was what Julian Schwinger used as the foundation of QM in his classic book "Quantum Kinematics and Dynamics". While Schwinger did not write his book from a multivector point of view, the application will be obvious to any Clifford algebraist who reads it. Also David Hestenes has a geometric theory of QM that is written entirely in Clifford algebra and hence has no spinors.

Smolin's book is written towards a more general audience and is quite sympathetic to string theory. I've already got a half dozen margin notes in each and I'm not 1/3 through.

5. **Benni**
September 3, 2006

I have asked Suesskind in munich in person, if he thinks that alternatives should be considered when stringtheory is in this devastative state.
He said: "Of course. If you have any alternatives to this landscape, tell us of it."
He also said that the landscape picture is unwanted and unlikely but if stringtheory is true it might be the only thing they have.

I think this was honest.
To say that alternatives must be considered amlostly includes that he admitts stringtheory might be wrong.

6. **MathPhys**
September 3, 2006

Re Peter's outline of his career, I wish to point out to those who didn't live through the early 80's that these were very bleak years for young people looking for jobs in theoretical high energy physics, particularly if they wished to stay at major universities in the US AND were not high achieving string theorists.

I personally know of a number of PhD graduates from Harvard and Princeton from that time, at least one of whom was a student of, and strongly supported by Witten, who couldn't find positions, and eventually quit science, and these were all people who would have very easily become full professor, at good universities, within 10 years in the 60's.

What I want to say is that, it's not like Peter has failed. In fact, relatively speaking, he's one of the very few academic survivors of that era (yours truly included :-)).

7. **woit**
September 3, 2006

JC,

It's been a long time, but from what I remember there were fewer people there working on string theory than there are now, especially at the senior level.

Ebgert,

Thanks for mentioning the typo, fixed.

No idea what Susskind's motivation is. He has critical things to say about Princeton, maybe that's part of the story in some obscure way. Perhaps he sees me as the enemy of his enemy (Gross and Witten).

8. **John Baez**
September 4, 2006

JC wrote:

> How big was string theory at Harvard around 1987-88?

It was big. I was a math postdoc at Yale then, coming from a PhD at MIT, and string theory was all the rage both in physics and in math at all these schools. In math, people were excited about conformal field theory and topological quantum field theory. In physics, I seem to recall that the heterotic string had everyone all agog.

Of course, there probably weren't as many tenured string theorists in the physics department at Harvard back then as there are now – there hadn't been time for the excitment to translate into hires.

9. **Thomas Larsson**
September 4, 2006

Susskind should have credit for one thing: admitting that ST has very little or no chance of ever saying anything definite about our universe – this is the essence of the anthropic principle, isn't it? In a way, Susskind's anthropism is dual to Friedan's defection.

Admitting defeat is not easy, and perhaps the anthropic phase was psychologically inevitable. According to Stephen Hsu, Susskind now "said very clearly that string theory might be wrong, but that we could still learn from it." A few years ago, a senior string theorists contemplating that ST might be wrong would have been notable.

10. **Jason**
    September 4, 2006

    Dude, you have too much free time.

11. **alejandro riveroi**
    September 4, 2006

    does the analogy mentions archimedes?

12. **Ebgert**
    September 4, 2006

    Alejandro,

    It doesn't. You can read it online if you get a "free 14 day trial". I used to be suspicious of these things because they might want a credit card number which they start charging if you don't cancel. Many web sites will make it a difficult task to cancel the "subscription". In this case they just ask for a name and email address and then verify the email address and then give you access.

    You have to opt out of the spam, though.

13. **MathPhys**
    September 4, 2006

    String theory was big at Harvard in 1987–88, not because the older, tenured people, such as S Coleman, H Georgi and S Glashow, worked on it (they never did), but because the younger untenured (Harvard Society Fellows) types did. There were L Alvarez-Gaume, P Ginsparg and C Vafa. There were also even younger people like G Moore, and a number of others. Haravard was definitely a hotbed of string theory then.

14. **Arun**
    September 4, 2006

    String theorists are shipbuilders who believe they have proved that ships must have 26 masts.

15. **comentator**
    September 4, 2006

    good comparison to what String theorists are now brought by boaz of the famous remark by V.Weisskopf

16. **Doormat**
    September 4, 2006

    I'm surprised you didn't mention (by maybe I can't read or something) that the same issue of the Times Higher carried a review of NEW (see pages 22-23) which was *very* much in agreement with the main points of the book. Worth a read if your in the UK…

17. **King Ray**
    September 4, 2006

    Peter,

    I received both your and Lee's books from Amazon recently and haven't started reading them yet, as I am finishing up a novel. I was wondering if you had an opinion on which book would be best to read first?

18. **RA**
    September 4, 2006

    Of course, this being your blog and all I can see why you'd interpret Susskind's article as more 'favorable' to you and less so to Smolin, but I have to ask how you can see this an not an insult:

    "Woit seems to not realise (or not care) that
    this would rob the subject of all the romance of exploration and leave it
    to the dullest plodders. The brightest, bravest and boldest young
    explorers want to go where no one else has gone before. But Woit is
    correct to remind us how important diversity and humility are in the face
    of the vast sea of ignorance."

    Do you not realize that he's calling you a 'dull plodder' who needs a good dose of humility?

    Perhaps your misreading of Susskind's article is very indicative of your general ability to understand other ideas and concepts. In reading the complete article it is pretty clear that Susskind is kinder in his descriptions of Smolin than he is of you. I realize it is painful to read criticism and to try to make the best of it, but to present Susskind's appraisals as you do seems pretty dishonest, if only to yourself.

19. **Who**
    September 4, 2006

    Doormat, by whom was the recent review of NEW on pages 22-23 of the 25 August (London) Times Higher?

    So far I am reluctant to sign up for the two-week trial which Egbert suggests at
    http://www.thes.co.uk/current_edition/story.aspx?story_id=2032023

    But apparently doing so would permit one to read not only the Susskind blarney-piece but also an actual review.

It's possible that Susskind's colorful talk of others' books boosted sales of his own, which has risen abruptly and is currently #6 on amazon general physics bestseller list. A week or two ago I saw it bouncing around in the #40-#85 neighborhood.

Arun, I was laughing aloud at your comment:

String theorists are shipbuilders who believe they have proved that ships must have 26 masts.

20. **a**
September 4, 2006

Who,

It isn't funny, it's quite tragic. So a better nautical analogy is the Titanic (an unsinkable ship on its maiden voyage designed by the best people, carrying most people's blessings and endorsements, admiration and praise).

21. **TheGraduate**
September 4, 2006

(Thank you Egbert for pointing out that a 14-day subscription was enough to gain access to the article. I had not previously wanted to comment on something I had not read.)

This article had too much ad hominem for my tastes. The author often refers to credentials and not to arguments.

So for him it's a problem that "in the blogosphere everyone is an expert, everyone has an opinion and all opinions are equal." To me this is just normal democracy.

Every one of his critiques of string theory's critics had a ranking attached:

1. Journalists with "no more than high-school physics"

2. Woit who is "a computer administrator" and an "untenured mathematics instructor"

3. Smolin, the "mid-level theoretical physicist"

4. t'Hooft is the "most renowned physicist of our age" which is pretty high praise but I did think it was odd to omit that t'Hooft had won the Nobel Prize. Also his discussion of what Dr t'Hooft disagreed with suggested he wasn't really a critic.

The piece is even organized in order of increasing credentials.

I also didn't think the name calling was helpful: "Chicken-Little" and "Don Quixote"

22. **MoveOnOrStayBehind**
September 4, 2006

>String theorists are shipbuilders who believe they have proved that ships must have 26 masts.

They are those who believe that a ship must swim. Others propose to build alternative ships out of stones…despite lots of applause from the blogosphere's experts, those have not left off, and this for good reasons. The story Susskind writes is quite accurate, with regard to science as well as to the people involved.

23. **TheGraduate**
September 4, 2006

Peter,

"Loose ends and Gordian knots of the string cult" by Philip Anderson is in the same issue of Higher Education

I don't know if you've seen it before. It's pretty favourable. I would almost call it a resounding endorsement.

http://www.thes.co.uk/search/story.aspx?story_id=2032027

24. **a**
September 4, 2006

To extend the Titanic analogy, the stringy landscape (10^500 vacuua solutions) is the ice berg straight ahead.

The Captain's critics want to immediately put the ship into reverse, to slow it down (reducing hype) and reduce the risks of an embarrassing disaster. Critics also demand the full filling of all lifeboats (alternative theories) in good time.

But the Captain thinks this is unnecessary (since the ship is unsinkable) and that starting to fill lifeboats will seem like defeatism, and may cause panic problems.

The Captain on balance decides that changing course very slightly will prevent disaster, while giving the passengers a spectacular glimpse of the ice berg (landscape) in passing …

25. **A**
September 4, 2006

Susskind makes nice analogies but does not address physics: string theory turned out to predict 10^500 different things. This is why it is now seen as a metaphysical speculation; this is why people in Princeton and elsewhere initially hoped in mono-vacuism or, at least, in oli-vacuism.

26. **MathPhys**
September 4, 2006

RA,
I haven't read Susskind's article yet, but when I read the passage that you just quoted, my interpretation agrees with that of Woit rather than with yours.

27. **TheGraduate**

September 4, 2006

RA,
I have read the entire article and my interpretation is also closer to Woit than yours.

28. **Carl**
September 4, 2006

I signed up for the 14 day free trial. Arguments that are intended to be read by non specialists always descend first to claims about what "4 out of 5 dentists" believe, and then to ad hominem attacks on the expertise of the other side. The human condition is that 99.9% of what any human knows he knows only on the basis of what he has been told.

29. **nitin**
September 4, 2006

I have something written about Philip Anderson's review of NEW on my blog ;)

30. **woit**
September 4, 2006

Doormath, TheGraduate, Nitin,

Thanks for pointing out the Anderson review. I wasn't aware of it, someone had just mentioned to me the Susskind review, and I didn't notice the Anderson one. Anderson is a well-known string theory skeptic, so I'm not suprised he has positive things to say about the book. I met him when I was a student at Princeton and have corresponded with him about this topic.

RA,

You've removed that quote from context. Susskind was noting that I have positive things to say about string theory as a dual to gauge theory and a possible way of solving QCD, while claiming it doesn't work as an idea about unification. He was correctly saying that string theory wouldn't continue to get people as excited if that's all it is. He's missing the point that I'm all in favor of people pursuing ideas about unification, just not ones that don't work.

I have no doubt that he thinks some rather negative things about me, with "dull plodder" the least of it. I was in no way claiming that he's a fan…

King Ray,

Kind of depends on your background, although for most people who don't know quite a bit about the subject, Smolin's
may be a better starting place, since it explains more about the basics of string theory. One exception would be if you're a mathematician, since in that case my book should do a better job of giving you the background of the subject.

31. **Who**
September 4, 2006

that is a thoughtful blog, nitin. I just spent a while musing at it
http://commeappeleduneant.blogspot.com/

32. **King Ray**
September 4, 2006

Peter, thanks for the advice. I have sufficient math and physics background, so that is not an issue.

33. **woit**
September 4, 2006

King Ray,

OK, in that case it doesn't matter much which you start with, I think both books reinforce each other, with two different perspectives that lead to similar conclusions. Mine is shorter, but it's more dense and a few parts are a significantly more technical than Lee's, so may require more concentration to follow. Kind of depends what you're in the mood for…

34. **D R Lunsford**
September 5, 2006

I thought the Anderson review was right on the money, and was gratified to see that Peter is being taken seriously among real physicists at the highest level. Thank God this business is almost over.

I didn't detect any bias from Anderson against math, rather, against second-rate mathematicians taking over physics departments and thus displacing what might be first-class physicists. I've always felt that string theory is a sort of revenge wrought on physics by matheists who can't do the problems in Resnick and Halliday 🙂

-drl

35. **D R Lunsford**
September 5, 2006

Susskind's diatribe is filled with the usual self-congratulatory, unctuous sarcasm one finds in these people, the narcisstic self-absorption of the fey dilettante. I remember Gell-Mann giving a lecture about string theory years ago, in the late 80s, during which he managed to insult his hosts before everyones' coffee was poured (the UMd Physics and Astrophysics Dept. whom he called "half-asstrophysicists"). The article is much in that manner. And what he says about Peter is anything but gentlemanly – rather, highly condescending and completely ad-hominem. A truly repulsive read.

-drl

36. **King Ray**
September 5, 2006

Peter, thanks, I think I'll read yours first since I've been eagerly awaiting reading it for months. Don't let Lubos get you down; he's only helping your cause and proving your point.

37. **Chris W.**
September 5, 2006

The following quote seems apposite. I'll leave to the reader to determine its source:

> *If in this book harsh words are spoken about some of the greatest among the intellectual leaders of mankind, my motive is not, I hope, the wish to belittle them. It springs rather from my conviction that, if our civilization is to survive, we must break with the habit of deference to great men. Great men may make great mistakes; and as the book tries to show, some of the greatest leaders of the past supported the perennial attack on freedom and reason. Their influence, too rarely challenged, continues to mislead those on whose defence civilization depends, and to divide them. The responsibility of this tragic and possibly fatal division becomes ours if we hesitate to be outspoken in our criticism of what admittedly is a part of our intellectual heritage. By reluctance to criticize some of it, we may help to destroy it all.*

38. **MathPhys**
September 5, 2006

The Open Society and Its Enemies, Karl Popper.

PS Oh, the miracle of google.

39. **hack**
September 5, 2006

Well if we're gonna go with this mideaval shipbuilding analogy, we must cast Witten as the brilliant mathematician who realized, by gluing opposite edges together, the flat earth model becomes a torus, thereby elegantly solving the age old problem of what kept all the water in the ocean from draining over the edges. Toroidal earth theory has become all the rage in educated circles because of its mathematical elegance. Captain Susskind, who calls himself the father of torus theory because he co-invented the donut during his earlier career as a pastry chef, wants to prove the theory by sailing due north, and coming back home from the south. Lee Smolin is a heretic who thinks it's not such a good idea to send all the ships in the same direction. Lubos is Susskind's pet monkey, trained to fling dung at his enemies. There, now we have a proper analogy.

40. **LDM**
September 5, 2006

It would be interesting to know if Susskind was this way *before* string theory and his subsequent fame. If the theory is good, it can stand on its own merits and he certainly does not need to belittle its detractors. It is sufficient to point out the weakness, if any, in their arguments.

Also, NEW, page 69, there is the statement that the electromagnetic force is carried by the photon…
Of course, this is the way QFT describes it…as long as you make it clear it is the exchange of *virtual photons*, where all 4 polarizations are needed, and not real photons where only 2 are used…
Granted, the layperson probably does not care about this distinction, but a physics student pondering how it is an exchange of (virtual) particles can create an attractive force might.

41. **Peter Woit**
September 5, 2006

LDM,

There's much that is imprecise in the book. When writing something like this, at each point one has to decide whether to say something imprecise, or to tell a more accurate and more detailed version of the story. Telling the more accurate version requires writing something longer, and one has to decide if it's worth the extra demands on the attention of the reader to get it right. This is one case where going into more detail would have taken me too far afield.

It seems to me that ad hominem argument is what one resorts to when one doesn't have an argument on the facts, and I would claim this is what is going on here. If I had my facts about string theory wrong I'd have heard quite a lot about it from string theorists, their silence on this issue is significant.

42. **Anon**
September 6, 2006

Susskind was famous long before string theory became popular in 1984/85. He made important contributions to all branches of particle physics, from lattice gauge theory to …

It is a testament to the power of the internets that he is now best known as the person Peter Woit characterizes as an enemy of science.

"If I had my facts about string theory wrong I'd have heard quite a lot about it from string theorists, their silence on this issue is significant."

Maybe, aside from Lubos, the rest of them are just too polite.

43. **Peter Woit**
September 6, 2006

Anon,

Yes, maybe what I have to say about string theory is wrong and string theorists (other than Lubos), are just too polite to point this out. It's also possible that string theorists (other than Lubos) don't discuss the predictions string theory makes because they're just too modest.

44. **Anonymous Coward from UCSB**
September 6, 2006

`Yes, maybe what I have to say about string theory is wrong and string theorists (other than Lubos), are just too polite to point this out.'

OK, Peter, just this once. From your comment on the NC thread:

`As for the single bit of info here, I remember a time when string theorists were going on about no-go theorems that showed that you couldn't have string theory in deSitter space, i.e. with a positive cosmological constant. When a positive cosmological constant was found, they seem to have come up with a way of dealing with that problem. If the spatial curvature comes out positive, I'm sure they'll come up with something.'

The Maldacena-Nunez no-go theorem, hep-th/0007018, was specifically for supergravity backgrounds without branes or string corrections. In fact, it was already well-known (beginning with Becker & Becker, hep-th/9605053) that such corrections allow much more general solutions. The MN theorem was well-known at the time not to be relevant, and in fact the BB paper is the ancestor of most stabilized string vacuum constructions.

And, you have your chronology wrong: the cc was well-known when the MN paper appeared. As usual, you have chosen to misconstrue what you know in the most negative possible way.

As to why more string theorists do not post (or do so anonymously) I suggest that it is a matter of time: it is much more time-consuming to actually do research than to sit back and criticize the efforts of others.

45. **woit**
September 6, 2006

AC from SB,

"you have your chronology wrong"

I don't see where I said anything about the Maldacena-Nunez paper, that's not what I was referring to. And, by the way, the comment was largely a sarcastic joke, probably not worth trying to explain it to you though.

Thanks for showing us that all string theorists other than Lubos are not just too polite. I'm kind of missing your point though about how serious string theorists are so busy doing important research into the landscape that they can't type in their actual e-mail addresses (the one you left "woitisabozo@physics.ucsb.edu" was very clever, I guess it's right that string theorists are just smarter than the rest of us).

46. **Anonymous Coward from UCSB**
September 7, 2006

Please do tell us which no-go theorems you were referring to. And it is a pretty sad escape to claim to have been joking.

47. **amused**
September 7, 2006

Susskind's article is a blast. "Could it really be that a secret cabal of scientific priests have plotted to overthrow the rules of good scientific method and have absconded with the nation's scientific funding?" Yes they have, the bastards ;). But underneath the colourful ship-building and exploration analogies it's just the same tired old line that string theorists always take in response to critics: "Any physicist with an ounce of ambition and adventure should be directly working toward discovering a glorious Theory of Everything, and string theory is the best (or only) hope for this at present." This is supposed to justify the continued pursuit of string theory irregardless of what it requires (e.g. changing the definition of science) as well as its continued domination of the formal theory end of particle physics.

The main issue for some of us critics is not whether or not string theory is a waste of time, but whether people should have no option but to work on it if they want a career in formal hep theory. We don't have anything against people trying to construct an "epic ship" if they want to; in fact we wish them best of luck. But there is still much of interest that can and should be done in "near coast exploration" using currently developed ships; it's a mistake to just drop all of this and concentrate exclusively on building an epic vessel. We are certainly interested in the prospect of finding the New Land, but suspect that there are other ways to get to there besides sailing straight out into the ocean. E.g., by gradually and carefully pushing out the boundaries of near coast exploration it may be possible to find a series of landmasses that will allow us to get to the New Land in a series of steps. The ship-building innovations required sail from one landmass to the next might be more within our capacities than those required for an ocean-crossing "epic vessel".
(Some of us suspect that not only may it be possible to reach the New Land in this way but it will also happen sooner than by epic ship across the ocean. But we don't doubt that if and when the epic ship eventually arrives they'll trumpet it with great fanfare and proclaim themselves the true discoverers…)

In any case, isn't it sensible to allow near coast exploration to continue while the epic ship project is underway? That way, if the ship ends up ensconced in a multiverse/flotilla of floating icebergs there will still be hope of eventually reaching the New Land by other ways, even if the epic ship-builders themselves prefer to give up and simply redefine "New Land" to be an iceberg flotilla.

(As an aside, we find it most amusing when the epic ship-builders use their constructs with great pride to (attempt to) give low-resolution mappings of near coast areas in already-known regions, pretending to really care about these areas while at the same time being totally uninterested in and indifferent to breakthroughs in traditional ship building which open the possibility of exact mappings…)

48. **Peter Woit**
September 7, 2006

AC from UCSB,

Maldacena and Nunez were not the first ones to point out this problem.

Not much point in trying to explain a joke to someone who doesn't realize that it's hilarious having one of the leading figures in theoretical physics giving a series of prominent lectures about a theory of everything that is nearly infinitely complicated but only predicts the sign of one quantity (and acknowledges that there's probably a way around that, given that other ways of populating the landscape can likely be found).

49. **Anonymous Coward from UCSB**
September 7, 2006

Since you can't cite any references, and are changing the subject, I assume that you are conceding the original point: you created a fictitious chronology out of your own imagination to make string theorists look bad, when in fact the chronology was that reverse of what you claimed. String theorists were not in any sense `going on about de Sitter no go theorems' before the cc was seen.

You were right about one thing, calling you a `bozo' was inappropropriate. But `one who distorts the facts to make others look bad' is just so clumsy.

Your `joke' is no different from the vitriol that you pour on string theory every day, and insulting people and then claiming to be joking is not at all an original strategy, it is classic behavior.

I think we are finished with this subject.

50. **woit**
September 7, 2006

AC from UCSB,

The history of the no-go theorems you're talking about goes back to the mid-eighties. I'm not going to waste my time getting together references and arguing about their significance with someone who has nothing better to do than to spend his time insulting me in a cowardly fashion from behind the cloak of anonymity

You're the one who is evading the point here, that Landscape research is a really bad and depressing joke. Yes, I pour vitriol on this kind of research, it's destroying the field of particle physics. I'm far from the only one who thinks this.

As for who goes around personally insulting people, look in the mirror. The only person whose name I've invoked here is Susskind, and I am criticizing his argument, not him personally. You're unhappy that I've perhaps unfairly characterized some people's scientific arguments. Maybe I have, maybe I should have dealt more seriously with them, and explicitly stated the well-known fact that, in this case as in essentially all others, there's no such thing as a real no-go theorem in string theory, you can get whatever you want. I didn't happen to think it was worth the trouble. But you really need to learn the difference between personally criticizing someone and criticizing a scientific argument.

Yes, I think we're finished with this subject, unless you're willing to put your name and reputation behind what you have to say.

51. **A**
September 7, 2006

adding to "amused" above:

Philosophy, after trying many epic vessels, remained at the starting point.

Physics, thanks to near coast navigation, progressed even too far.

52. **Anon**
September 7, 2006

"The history of the no-go theorems you're talking about goes back to the mid-eighties. I'm not going to waste my time getting together references … "

We're all capable of using SPIRES to find the exact references. How about some names and approximate dates to get us all started?

53. **Chris W.**
September 7, 2006

Amused,

Your characterization of the situation reminds me of what is now happening to science within NASA; it is being sacrificed in favor of massive engineering boondoggles—a manned return to the moon (and establishment of a base there) and a manned expedition to Mars. These projects will probably collapse into a mess of cost overruns and missed milestones before they attain their stated objectives, but the aerospace and defense contractors involved (and the federal officials and members of Congress promoting their interests) will certainly benefit in the meantime.

[By the way, you nearly ruined a great comment by contaminating it with the pseudo-word "irregardless".]

54. **UCSB**
September 7, 2006

Hi Anon,

Let me help. The papers did indeed exist, but the precise issue is when `string theorists were going on about them', to use PW's words. They existed but in fact were so unknown that Maldacena and Nunez were not aware of them (and did not cite them) when they rediscovered and generalized their results. One paper is

RESIDUAL SUPERSYMMETRY OF COMPACTIFIED d = 10 SUPERGRAVITY.
B. de Wit, D.J. Smit (Utrecht U.) , N.D. Hari Dass (NIKHEF, Amsterdam) . NIKHEF-H-86-15, May 1986. 44pp.
Published in Nucl.Phys.B283:165,1987

This was cited a grand total of 6 times in the ten years before the dark energy was found, then 4 more in the next two years, and then 78 times when it was rediscovered after the MN paper (which itself was cited 289 times). So this clearly shows at what point string theorists were `going on about this'. By the way, MN themselves discuss the limitations on their assumptions, and cite a list of aready-existing papers that lie outside these assumptions and are not constrained by the theorem (beginning with Strominger in 1986, long before the dark energy was found). So there was no point in time at which those actually working on the construction of superstring vacua regarded these theorems as a constraint.

The other early paper was

Aspects Of Supergravity Theories.
G.W. Gibbons (Cambridge U.) . Print-85-0061 (CAMBRIDGE), Jun 1984. 53pp.
Three lectures given at GIFT Seminar on Theoretical Physics, San Feliu de Guixols, Spain, Jun 4-11, 1984.

which was even less cited and less known (it appeared only in a conference proceedings, before the days of the arxiv).

55. **woit**
September 8, 2006

I've wasted my time and found the precise reference I was thinking of when I wrote the lines that so upset AC from UCSB, they're from a paper but also correspond to what I remember hearing in a talk around this time

"This means that there is no classical way to get de Sitter space from string theory or M-theory… In fact, classical or not, I don't know any clear-cut way to get de Sitter space from string theory or M-theory. This last statement is not very surprising given the classical no go theorem. For, in view of the usual problems in stabilizing moduli, it is hard to get de Sitter space in a reliable fashion at the quantum level given that it does not arise classically".

Funny that we're told that the no go theorems were "well-known to be irrelevant", I guess this particular string theorist wasn't very well informed. It's true that he didn't claim that string theory “predicted” a non-positive CC, he’s not the sort to go on about bogus string theory “predictions”. And, by the time of this quote, the experimental evidence was in favor of a positive CC.

People who are very concerned with the issue of whether I’ve got this right still seem to insist on ignoring the main point: is Susskind’s “prediction” a bad joke or not?

56. **Anon**
September 8, 2006

“I’ve wasted my time and found the precise reference …”

And the precise reference is …?

“Funny that we’re told that the no go theorems were ‘well-known to be irrelevant'”

They weren’t well-known at the time.

And by the time they became well-known, they were already understood to be irrelevant (in the sense that there were already well-known string backgrounds that violated the assumptions of the theorems).

UCSB has the history right.

57. **Anonymous Coward from UCSB**
September 8, 2006

E. Witten, hep-th/0106109, 3 years _after_ the supernova data.
So you did distort the chronology: you stated that string theorists made a wrong prediction and then backtracked when the data came out the other way, whereas this was an honest statement of a research puzzle. And Witten was indeed out of the loop on this subject, since the first dS solutions (E. Silverstein hep-th/0106209) appeared at virtually the same time.

58. **woit**
September 8, 2006

Anon and AC from UCSB,

I think the Witten quotation speaks for itself, your statement that the no-go theorem was “well-known to be irrelevant” is simply wrong. Witten was giving talks at places like Strings 2001 saying what I quoted. He did not consider these theorems “irrelevant”, and if there’s one thing Witten rarely is, it’s “out of the loop”. Yes, it’s true that the way I stated things exaggerated the true situation. As I said, it was somewhat of a joke made in the context of responding to Susskind’s absurdity.

For the N’th time, will you or won’t you address the issue of whether or not Susskind’s argument is an absurdly bad joke? I understand very well why you would rather argue about something else, but sooner or later people in this field will have to face up to what is going on. You can devote your days (and nights) to anonymously attacking anyone who tries to point this out, but it won’t change what is happening.

59. **amused**
September 8, 2006

Chris,

Although I can well understand the temptation I think you’re being too harsh comparing string theorists to sleazy aerospace/defense contractors and congressmen. As individuals they aren’t really doing anything different from other physicists or academics in general by promoting their own area and supporting the people in it to the exclusion of others. But in this singular situation of string dominance the leaders of that field need to realise that with great power comes great responsibility. I.e. the responsibility to act in the best interests of physics. I wonder if they ever give any thought to this – it’s certainly not something they like to talk about. Do they really think it’s in the best interests of physics that the string program is pursued to the exclusion of everything else in formal hep theory?
The current protestations from string critics are nothing compared to the merciless slamming that awaits them from future historians of science if they don’t exercise their responsibility wisely.

(And as for the pseudo-word contamination, it was a quite mild contamination level by my usual standards ;))

60. **Anon**
September 8, 2006

“Yes, it’s true that the way I stated things exaggerated the true situation.”

You wanted to know why none of the string theorist bother to respond to you.

That’s why.

UCSB chose one recent example, but we could do the same with just about everything you say on this blog.

If you want them to take you seriously, then you’ll have to take them seriously, too, and not engage in this kind of deliberate distortions and obfuscations.

61. **woit**
September 8, 2006

Yet another anonymous string theorist,

“we could do the same with just about everything you say on this blog.”

Whoever you are, hiding behind anonymity, you’re completely dishonest.

Look, what is going on here is that, in a throwaway line in the comment section here, in the context of discussing the completely insane state that string theory has come to, I made a reasonable analogy to another similar issue in string theory. I wrote two sentences, both of which are completely accurate, but which could be read together as making a misleading accusation. I shouldn’t have done this. I’ve written probably a thousand pages of stuff here. Every bit of it is signed by me, and if I’m wrong about something I will admit it.

In response to this, I've gotten mostly dishonest responses from what appear to be three separate string theorists, none of whom dare put their name to what they write. AC from UCSB's claim that what I wrote was untrue because these no-go theorems were "well-known to be irrelevant" was shown to be not true by the quote from Witten.

Sure, sometimes in a comment section here I say something that is not quite right, sometimes for rhetorical effect. Anyone who wants to is welcome to correct me, but doing so in an insulting and dishonest way, hiding behind anonymity, is just kind of pathetic.

For the N+1th time, will any one of the three of you actually address the question at issue: is Susskind's "argument" a bad joke, deserving of being made fun of, or not?

62. **UCSB**
September 8, 2006

`is Susskind's "argument" a bad joke, deserving of being made fun of, or not?'

Actually, this is an interesting question, thank you for asking it. What if the curvature is measured to be positive? There is natural explanation: tunneling from nothing (Hartle-Hawking, Vilenkin) produces universes with positive spatial curvature, whereas tunneling from a higher dS state produces universes with negative spatial curvature. Either of these processes is sufficient to populate the landscape, so indeed neither observation would falsify the environmental solution to the cc problem, it would tell us something important about quantum cosmology.

The relation between these two processes has never clearly been understood by quantum cosmologists working in the effective QM-GR: every one has their own person favorite for the wavefunction. A complete theory of cosmology should explain the relation between them, most likely both occur in some form but presumably, barring an accident, one or the other is much more likely. Indeed, providing a theoretically convincing solution to this old area of controversy is a target for string theory, though observational test depends on having few enough e-foldings for the curvature still to be visible.

63. **z3**
September 8, 2006

Peter finally grudgingly admitted to an inaccuracy in an off-the-cuff, un-researched remark in a comment section — this is actually a pretty hard thing to do, especially in public view. One can respect him for that.

USBC finally calmed down and start to address physics — for a while there he was latched on to a secondary topic, attacking in full rage, and in so doing exposed the fact that he himself is not quite in full command of the history of the topic either.

Anon just extrapolated Peter's one error by 3 orders of magnitude to show that the entirety of Peter is in error. I hope he is more conservative in his main areas of study.

And we all now see how memory is not 100% reliable and why one must always research the facts before making quick strong statements.

64. **dan**
September 9, 2006

Hello Peter,

Even if string theory is unable to make predictions at any level, could you accept it as legimate scientific research as a way of explaining (but not necessarily predicting) fundamental physics, such as 3 generations of fermions?

There are many instances of a scientific theory (i.e meterology, evolutionm, n-body problem, fluid dynamics) that cannot predict (because too much is unknown or equations poorly understood or impossible to solve) but are considered scientific for other reasons.

thanks

65. **TheGraduate**
September 9, 2006

dan:

I could be wrong but I think that you can get predictions out of meteorology and mathematical analyses of the n-body problem and of fluid dynamics over a small enough time scale and in special circumstances.

Also in each case, there are actual physical systems that can be catalogued. Collection, arranging and categorization is also a scientific activity. For instance, a meteorologist can observe and classify types of clouds and the conditions under which they are known to form.

In the case of evolution, you can easily apply selection pressure to bacteria, viruses, protozoa etc and observe the affects on different allelles. eg. exposing bacteria to antibiotics and observing the shift in the population to antibiotic-resistant bacteria

66. **TheGraduate**
September 9, 2006

dan:

I think a good way to figure out if one is doing science or religion is to ask yourself whether your approach is vulnerable to the sorts of errors that religious approaches to finding truth are vulnerable. I think in the case of string theory, they certainly are.

Without accountability, the reasoning process tends to go awry.

67. **dan**
September 9, 2006

so TheGraduate Says:,
would u c string theory as a scientific theory in the sense of explanatory rather than predictive?

68. **nigel cook**
September 9, 2006

dan,

String theory probably “explains” why the science fiction of extra dimensional spacetime, superpartners, gravitons, multiple universes, and time travelling in wormholes is so popular.

There is a list of hyperspace films at http://en.wikipedia.org/wiki/Hyperspace_(science_fiction) and we owe a great debt of gratitude to Professor M. Kaku, who is the “anchor” of hyperspace, according to the first reference on that Wiki page.

Without string theory “explanation” hype, where would science fiction be today? It would still be half-plausible fictional stories based upon science. String “explanation” is not just replacing half decent religions (thanks to Susskind’s new book on the anthropic principle, landscape and intelligent design), it is also replacing decent science fiction with brilliant stringy films.

I wonder if Speilberg and others in Hollywood will become stringy defenders, joining Motl, Distler, and Susskind? 😉

69. **woit**
September 9, 2006

dan,

To have a scientific explanation of anything, you have to be able to check that the explanation is correct. That’s why you need a testable prediction. “Explanations” that can’t be checked and tested are not part of science.

All the sciences you mention make many testable predictions, meteorology is all about learning how to make better and better predictions.

70. **dan**
September 9, 2006

Nigel, I find what you say amusing.

Peter, string theorists have claimed that the fact string theory can derive BH-entropy a priori in agreement with Hawking-Berkenstein calculations, predict the spectrum of the standard model and three generations and particle masses, provide a theory of gravity in agreement with general relativity at long distances, are examples which check if the explanation are correct.

71. **woit**
September 9, 2006

dan,

This kind of discussion is pretty off-topic, and has taken place a hundred times here before.

1. The BH entropy calculations don’t apply to physical black holes, so don’t predict anything you can observe, even if you do find a black hole you can examine.

2. It’s just a complete bald-faced lie to say that string theory predicts the standard model, 3 generations and particle masses.

3. The idea that it gives GR at long distances is the strongest argument in favor of string theory. This is not a prediction, at best a postdiction (and you can argue about this..), it’s the recent people picked this theory to look at. As Lisa Randall has been known to point out: “Yes, string theory predicts GR. In 10 dimensions.”

72. **dan**
September 10, 2006

Peter,
Sorry if it’s off-topic, I should say “output is MSSM”

73. **Peter Woit**
September 10, 2006

dan,

It’s every bit as much nonsense to claim that string theory predicts the MSSM as to say that it predicts the standard model.

Noncommutative Geometry and Physics

September 5, 2006
Categories: Uncategorized

This week the Newton Institute in Cambridge is running (with funding from the Templeton Foundation) a workshop on the topic of Noncommutative Geometry and Physics: Fundamental Structure of Space and Time. The program is here, some of the talks are online here, and Paul Cook is blogging here (with truly scary pictures of a Newton Institute restroom). Thursday evening they will be having a public panel discussion, entitled The Nature of Space and Time: An Evening of Speculation.

For someone interested not in quantum gravity but in particle physics, the most interesting of these talks is doubtless that of Alain Connes, entitled Noncommutative Geometry and the standard model with neutrino mixing. He has a new paper out, with the same title, with more details promised in a forthcoming paper with Chamseddine. I've been carrying the paper around for a while now, hoping to understand exactly what he's doing, but it's rather dense and some of the calculations are involved and don't carry much of an explanation. I really wish I'd been at the talk to hear his exposition of what he's up to here.

Among the other talks at the conference I would like to have heard would be that of Samson Shatashvili, who is doing some very interesting things with 2d gauge theories. He has a new paper out (with Gerasimov) which looks quite readable, entitled Higgs Bundles, Gauge Theories and Quantum Groups.

Another conference going on that is finishing up this week is the Erice "International School of Subnuclear Physics", this year entitled The Logic of Nature, Complexity and New Physics, and dedicated to Richard Dalitz. Back during the sixties, seventies and eighties, the Erice School was an important yearly event, featuring the best theorists around giving expository lectures on the latest ideas, often including spectacularly beautiful lectures by Sidney Coleman. This year's school just looks profoundly weird, with an interesting and reasonable set of lectures on the experimental side, but the theoretical side mostly devoted to "complexity and the Landscape", featuring an opening lecture and mini-course about the Landscape by Susskind, a mini-course about the Landscape and its computational complexity (i.e. why it is hopeless to ever use it to predict anything) by Denef and Douglas, and more complexity from Zichichi, Beck, Gell-Mann and Tsallis. Many of the lectures are available here. Steve Hsu is blogging from the conference, and he reports that Susskind says the Landscape program is science since it now gives exactly one bit of information about the universe (the sign of the spatial curvature k), although he expects Andrei Linde to be able to make that bit disappear if he wants to.

Update: Urs Schreiber has an excellent discussion of the Connes program here and here.

Comments

1. **PPCook**
   September 5, 2006

   Hi Peter, thanks for the link. The conference here is very interesting, but what has been very tough is the realisation that to understand the Connes paper it does not help to hear him speak. It is, of course, a pleasure to hear him speak. And he is an excellent speaker. It's just to benefit one really must hear him speak while holding all the ideas behind the approach in your head. I think there is much promise in his approach, but it does seem to be very constructive. It seems the most important point to start learning about the Connes approach would be his paper with Chamseddine The Spectral Action Principle. Perhaps after spending years staring at the standard model one might gain the same feeling as Connes and one might feel that the starting points are intuitive, although perhaps not. One must bear in mind Alain Connes' own words when assessing his approach to physics:

   "There are two fundamental sources of 'bare' facts for the mathematician. These are, on the one hand the physical world which is the source of geometry, and on the other hand the arithmetic of numbers which is the source of number theory. Any theory concerning either of these subjects can be tested by performing experiments either in the physical world or with numbers. That is, there are some real things out there to which we can confront our understanding."

   Treating experimental data on the same footing as number theory seems to remove the need for motivating certain assumptions. The standard model just is – we have observed it, that is good enough. At the end of his talk he described string theorists who look for more than is apparent as living in "a dreamworld". I think this is understandable, but the dream is alluring. It is also very natural for scientists to seek explanations, but yet it is proper to respect experimental evidence. Nevertheless the formulation of Connes is extremely compact, and one hopes it is not just exquisite engineering.

2. **John Baez**
   September 6, 2006

   Connes surely doesn't get the right *dimensionless constants* in the Standard Model Lagrangian – if he did, folks at the Newton Institute would be drinking champagne and dancing naked in the streets. So, how does he manage to come so close yet not that far? And, what is his attitude towards these constants? Did his audience press him on this point?

3. **steve**
   September 6, 2006

   Peter,

   I never thought I'd be defending string theory, but I think your remark above is too negative.

   Lenny didn't claim victory because there is a single robust prediction from the Landscape. He's seems disturbed that most low-energy observables are unpredictable, even in an anthropic framework.

   However, it does mean the Landscape is falsifiable — if Planck measures a positive curvature it will strongly disfavor the scenario.

4. **Peter Shor**
   September 6, 2006

   John,

   It looked to me from his abstract that the dimensionless constants were parameters that Connes could put in his model, and the only actual predictions (so far) came at unification scale? I don't understand any of this, so could somebody verify whether this is right?

   Anyway, it seems to me that if the string theorists got this close (for whatever values of close he got), there clearly would be dancing in the streets.

5. **A**
   September 6, 2006

Following your link, I tried to read the Susskind lectures, but they contain a few words, almost no equations, plenty of cartoons.
My child got interested.

6. **amanda**
September 6, 2006

“However, it does mean the Landscape is falsifiable — if Planck measures a positive curvature it will strongly disfavor the scenario”

It will disfavor the *particular* scenario that LS pushes, with Coleman-de Luccia instantons. Of course, LS has the bad habit of pretending that his way of doing things is the only way — cf his repeated declarations that black hole complementarity, which is nothing but the wildest of wild speculations, is a “law of nature”!

7. **MathPhys**
September 6, 2006

amanda,

What do you mean by “black hole complementarity” more precisely?

8. **steve**
September 6, 2006

Amanda,

I’m under the impression that if there are many metastable vacua (almost all with much larger vacuum energy than our own), then it is highly likely that our universe must have originated in a tunneling (bubble nucleation) event. If so, the curvature has to be negative. It so happens that Coleman-Deluccia worked out the bubble form, but I don’t see that Lenny is making a nontrivial assumption.

Am I missing something?

9. **D R Lunsford**
September 6, 2006

Has anyone improved on Brout’s paper as an exposition for physicists?

-drl

10. **Peter Woit**
September 6, 2006

steve,

I didn’t say he was claiming victory, just that he was using this to answer certain people who argue that this is not science. It’s good to hear that he’s disturbed by not having any low-energy predictions, but the problem with the Landscape is not just at low-energy, it doesn’t give predictions at any energy.

As for the single bit of info here, I remember a time when string theorists were going on about no-go theorems that showed that you couldn’t have string theory in deSitter space, i.e. with a positive cosmological constant. When a positive cosmological constant was found, they seem to have come up with a way of dealing with that problem. If the spatial curvature comes out positive, I’m sure they’ll come up with something.

11. **urs**
September 6, 2006

John Baez said

> Connes surely doesn’t get the right dimensionless constants in the Standard Model Lagrangian – if he did, folks at the Newton Institute would be drinking champagne and dancing naked in the streets. So, how does he manage to come so close yet not that far? And, what is his attitude towards these constants? Did his audience press him on this point?

I don’t think the point of this quest for the spectral action of the standard model is to predict the standard model’s properties.

I think the main point is first of all to understand which spectral triple precisely is the one whose spectral geometry describes the standard model.

It would of course certainly be a nice side effect if some properties of the standard model were derivable this way, maybe in the sense that they might turn out to be forced to have a certain value to admit a spectral description at all.

So I think the point is that if you want to understand something deeply, you should first try to find its most elegant/powerful/compact description. And Connes rightly points out that encoding the entire standard model into a spectral triple does achieve such a description.

And we learn by that, for instance, that we observe a world of metric dimension 4 and KO-dimension 4+6 mod 8.

While not a prediction, that looks like a remarkable insight.

I am going to say more about that at the n-Café. So far there is an introductory entry.

12. **wolfgang**
September 6, 2006

I did not see much discussion on the quantization of the theory in the latest paper of Alain Connes. Is renormalization etc. already taken care of automatically in this approach (or described in another paper) ?

13. **urs**
September 6, 2006

> I did not see much discussion on the quantization of the theory in the latest paper of Alain Connes. Is renormalization etc. already taken care of automatically in this approach (or described in another paper) ?

As far as I can tell, so far this is just a way to rewrite the standard action functional in a different form.

See section 6 of Connes' latest paper for remarks on what might be expected as a "UV completion" of this spectral action.

In fact, his collaborator Chamseddine once began trying to understand if the spectral action obtained from a 1-Dirac operator could be the limit of a UV-complete one obtained from a 2-Dirac operator

14. **Alejandro Rivero**
September 6, 2006

wolfgang, one intriguing thing of Connes approach is that some details that are supposed to come from quantisation appear here as a consequence of the axions. Paricularly, Poincare duality imposes anomaly cancellation.

15. **A**
September 6, 2006

Actually the prediction of negative curvature has nothing to do with string theory: as shown by Coleman-deLuccia, a negative curvature arises from vacuum decay, for any potential studied so far. Next, anthropic arguments are used to argue that inflation maybe does not need to suppress the curvature down to unobservably small values.

16. **PPCook**
September 6, 2006

John,
No one in the audience asked about the dimensionless constants…
Best wishes,
Paul

17. **urs**
September 6, 2006

The Yukawa coupling constants are encoded in the "metric" of the internal (d_KO = 6)-dimensional space, namely in the Dirac operator associated with it. See equation (1.21) of http://arxiv.org/abs/hep-th/9606001

I talk about that here.

18. **urs**
September 6, 2006

Furthermore, apparently the bare gauge coupling is related to the volume of spacetime by equations (2.20) and (2.29) of the review. More details are here

http://golem.ph.utexas.edu/category/2006/09/connes_on_spectral_geometry_of_1.html#c004520

So, in a word, Connes does not *predict* the parameters of the standard model. What he does instead is to identify the "geometry of a non-geometric KK compactification" that does reproduce the standard model (including the dimensionless constants).

19. **steve**
September 6, 2006

A: Yes, but as I mentioned in a Landscape scenario our universe likely originated in a negative curvature bubble nucleation, so there is a definite sign prediction for k. Inflation might have flattened things out, but it would be hard to explain a positive measurement of k.

It isn't that negative k, or a nucleation origin, require string theory, but rather the converse.

20. **TheGraduate**
September 6, 2006

Hi,

I know people are in the process of figuring out what Connes' paper means but I was wondering if anybody could summarize in a sentence or two of nontechnical language what issues are in play. So far I understand the following:

1. This paper says something about the number of dimensions being equal mod 8 … and since (26 = 10) mod 8 … that says something about string theory … perhaps

2. It also makes progress in a group theoretic description of the standard model …

I apologize for my abyssmal ignorance and would greatly appreciate some breakdown.

21. **Andy Neitzke**
September 6, 2006

`What he does instead is to identify the "geometry of a non-geometric KK compactification" that does reproduce the standard model (including the dimensionless constants).'

Does he explain what theory one has to "compactify" on this nongeometric background to get the Standard Model?

22. **urs**
September 7, 2006

First of all, many thanks to "anon" for pointing out a stupidity I said. The corrected statement is here

http://golem.ph.utexas.edu/category/2006/09/connes_on_spectral_geometry_of_1.html#c004536

> Does he explain what theory one has to "compactify" on this nongeometric background to get the Standard Model?

Well, his theory is that given by the action functional which you cook up from the spectral triple.

So, given any spectral geometry, Connes considers the action functional obtained from its generalized “heat kernel expansion”.

That’s his way to formulate things. There are indications, though, that, if applied for instance to the Dirac-Ramond operator, this generalized “heat kernel expansion” reproduces precisely the effective string theory equations of motion.

That’s at least what Ali Chamseddine claims to have shown. See p. 10 of

http://arxiv.org/abs/hep-th/9701096

as well as

http://arxiv.org/abs/hep-th/9705153 .

23. **urs**
September 7, 2006

> summarize in a sentence or two of nontechnical language what issues are in play.

The idea is this:

To any given ordinary Riemannian manifold X, we may associate the Einstein-Hilbert Action. That’s gravity.

We know from Kaluza-Klein, that if X looks like Y x S^1 that gravity on X looks like gravity coupled to electromagnetism on Y.

Now, Connes comes along annd says that there is something like a “generalized” Riemannian manifold. And he provides a way to define a notion of action for that, such that for X an ordinary Riemannian manifold that action reduces to the ordinary Einstein-Hilbert action (plus correction terms of higher order).

So, he says, assume our spacetime is not an ordinary Riemannian manifold with extra forces in it, but just some generalized Riemannian manifold with only gravity.

Using his generalized notion of the Einstein-Hilbert action, we may check for each such generalized Riemannian manifold what its “gravitational” action looks like.

In a similar fashion, fermionic matter propagating on an ordinary Riemannian spin manifold (= coupled to gravity) may be generalized to fermionic matter propagating on a generalized Riemannian manifold.

So, Connes says, what I want to do is to find a generalized Riemannian manifold X of the form Y x Z, such that my generalized theory of gravity coupled to fermions on X looks like gravity coupled to our standard model on Y.

He plays around a little and finally finds Z such that this works.

24. **anon.**
September 7, 2006

urs wrote:

There are indications, though, that, if applied for instance to the Dirac-Ramond operator, this generalized “heat kernel expansion” reproduces precisely the effective string theory equations of motion.

Is this a leading-order statement in both g_string and alpha’, or are higher terms in the “heat kernel expansion” that should reproduce some or all of the corrections?

25. **urs**
September 7, 2006

> Is this a leading-order statement in both g_string and alpha’, or are higher terms in the “heat kernel expansion” that should reproduce some or all of the corrections?

Yes, Chamseddine checks leading order only.

I don’t know if the higher orders “should” agree. But if they do, we’d get a nicely coherent picture of what is going on.

Maybe to emphasize that, allow me to reformulate Connes’ approach somewhat suggestively this way:

He is considering the theory of a superparticle (*worldline* susy) in d_KO = 10 dimensions, determined by a Dirac operator D.

He takes the interactions of this superparticle to be such that the effective target space action is Tr(f(F)) + (psi,D psi).

Then he looks for compactifications

D = D_0 + D_F

such that this effective target space action agrees (to lowest order) with the standard model coupled to gravity.

That’s not precisely how Connes states it. And maybe it’s wrong. But maybe it’s right.

26. **PPCook**
September 7, 2006

I Just noticed the paper A Lorentzian version of the non-commutative geometry of the standard model of particle physics by John Barrett today (who has also spoken here at the NCGW, but not on this topic). It seems strange that, to my knowledge, it hasn’t been mentioned amongst the online discussion of the Connes paper even though it appeared on the same day as the Connes paper and in fact even a few entries earlier on the archive. The results appear at first to be identical.

27. **Chris W.**
September 7, 2006

From Connes (hep-th/0608226):

8. Acknowledgements
The detailed computations and extension of this work to the left-right model will appear in a joint work with Ali Chamseddine and Matilde Marcolli. The need to have independence between the KO-dimension and the metric dimension already emerged in the work of P Dabrowski and L., Sitarz on Podle´s quantum spheres [10]. The results of this work were announced in a talk at the Newton Institute in July 2006, and the fear of a numerical error in the above computations delayed the present publication. *It is a pleasure to acknowledge the independent preprint by John Barret (A Lorentzian version of the non-commutative geometry of the standard model of particle physics) with a similar solution of the fermion doubling problem which accelerated the present publication*.

28. **urs**
September 7, 2006

Coming back to the question on what, if anything, the spectral thing predicts, there are a couple of intersting remarks in

hep-th/9605001

(which is in general a pretty good account of the details involved).

In the introduction, the authors cite a couple of papers that argued that "most" models of Yang-Mills-Higgs type that one could write down are not obtainable by spectral action.

In the concluding section, it furthermore says that

– assuming a Higgs, the spectral stuff predicts a minimum of two generations

– including also the measured top mass then also predicts a maximum of 5 generations

– existence of the Higgs forces the electroweak sector to be chiral

– "the choice of possible gauge groups is very much restricted" it says

– finally: the huge span of fermion masses can *not* be explained.

So, there are a couple of internal consistency conditions, but the compactification metric (hence the set of dimensionless parameters) is essentially free input.

—

I have tried to expand on the superparticle point of view mentioned above:

http://golem.ph.utexas.edu/category/2006/09/connes_on_spectral_geometry_of_2.html

29. **anon.**
September 7, 2006

– existence of the Higgs forces the electroweak sector to be chiral

!

So the claim is that any time there's a spectral geometry formulation of your theory, you can only Higgs a gauge group with chiral fermions? There's no way to e.g. add a Higgs for SU(3)_color to this model?

Is there a heuristic explanation of why this should be true? It seems very strange.

30. **John Baez**
September 8, 2006

Connes writes:

> It is a pleasure to acknowledge the independent preprint by John Barret (A Lorentzian version of the non-commutative geometry of the standard model of particle physics) with a similar solution of the fermion doubling problem which accelerated the present publication.

Heh. The French never, *never* spell John Barrett's name right.

But, it's great that he scooped Connes. He'd been working on this for quite a while – he was quite excited about it when I saw him this April in Marseille, for the thesis defense of Alejandro Perez.

31. **urs**
September 8, 2006

> Is there a heuristic explanation of why this should be true?

I am not entirely sure. What I know is this:

the Higgs arises as the "internal" component of the connection, A_int.

By the general logic of the approach, this term is given by the commutator of the internal Dirac operator with an algebra element a

A_int = a [D_int ,a'] .

The algebra and its representation is such that projected on the anti-particle sector [D_int,a'] = 0, so we just get a contribution on the particle sector.

This is computed for instance on p. 26 of hep-th/960353 .

After the computation, you impose the self-adjointness condition and find that the term A_int is exactly given by a quaternion-valued function, which is to be interpreted as the Higgs.

A similar discussion must be in hep-th/9605001 somewhere, I guess.

32. **nontrad**
September 8, 2006

At the risk of disrupting the ongoing discussion, I flipped through Douglas' 08.31.06 Erice lecture on 'computational complexity and fundamental physics'.

It all seemed mostly innocuous until I arrived at the slide inwhich Douglas' began to discuss protein folding, and that made me pause.

Douglas makes some comments about evolution and the random order of amino acids in protein chains, which don't strike as anything other then a crude cartoon of the reality of proteins.
How and why such a cartoon-ish sketch of proteins appears in a talk on cosmological questions is then troubling to me since it suggests a situation of cartoons built on cartoons. Or analogies built on analogies. Or a castle made of sand.

Douglas then refers to Smolin's 'cosmological natural selection' and call's it 'bizarre', which immediately made me realize that perhaps Douglas is unaware of how his own thesis appears just as bizarre as those Douglas perceives as being bizarre. The pot calling the kettle black typically indicates bad news for someone.

Susskind's talk similarly features loops of DNA appearing more then once.

Hard to say that it isn't troubling to see what appears to be a tower of cartoons based on cartoons.

Who knows, perhaps all of these folks are super geniuses who have really figured is all out and are on the verge of a tottally complete and unified theory of everything.

I guess I'm caught up on the fact that their road to that end seems so fantastically unreasonable!

33. **cox**
September 8, 2006

Anybody see this:
http://english.ohmynews.com/articleview/article_view.asp?no=315855&rel_no=1

34. **Chris W.**
September 8, 2006

Reiterating PPCook's report, as a follow-on to 'nontrad' and 'cox':

At the end of his talk he [Connes] described string theorists who look for more than is apparent as living in "a dreamworld".

Indeed. And they talk about this dreamworld as though it was as solidly grounded in reality as the Atlantic Ocean, DNA, proteins, or the universe outside our solar system. To paraphrase Descartes, "I speculate that it exists, therefore it does".

35. **Alejandro Rivero**
September 8, 2006

appear here as a consequence of the axions.

Er… axioms

36. **A Babe in the Universe**
September 8, 2006

Have you boys forgotten what anniversary September 8 is? I haven't.

37. **D R Lunsford**
September 8, 2006

Urs – you might like this:

D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, Principle of general Q-covariance, Journal of Mathematical Physics 4, 788-796 (1963)

-drl

38. **kristo**
September 10, 2006

Is there somewhere an account available of the 'discussion' "The Nature of Space and Time: An Evening of Speculation" on Thursday?

39. **Alejandro Rivero**
September 11, 2006

kristo, sorry there was at least three bloggers+PhysicsForums dwellers and nobody of us has taken time about it, perhaps swept by the main happennings. The evening run smoothly without surprises, but with some anecdote. Hawkings computer reboot I have described in dorigo his blog. Amazing also the transmutation of Heller from a quantum theoretist in the morning to a teologist in the evening, these guys at vaticano have a good hiring board.

40. **Alejandro Rivero**
September 11, 2006

Baez says: *Heh. The French never, never spell John Barrett's name right.*

Actually I am having a hard time trying to distinghish if he thanks John Barret or John Baez in the first minutes of his talk :-DDD

(between minutes 20…21 of the MP3 file. Not at the end, where Barret is clearly named)

Open Access Publishing

September 9, 2006
Categories: Uncategorized

There's a big debate within the scientific community in general about how and whether to move away from the conventional model of scientific publishing (journals supported by subscriptions paid by libraries, only available to subscribers) to a model where access to the papers in scientific journals is free to all ("Open Access"). The main problem with this is figuring out how to pay for it.

In his latest This Week's Finds, John Baez gives a link to some information about the Open Access movement. One of the main actors here is SPARC (the Scholarly Publishing and Academic Resources Coalition). There's an associated SPARC Open Access Newsletter and a blog, Open Access News.

Inside Higher Ed has a recent article about this, and last week's Science magazine also has an article. The Science article discusses a new proposal put out by a task force from CERN that can be found here. The CERN task force has gathered a lot of interesting data about the particle physics literature, counting roughly 6000 papers/year, of which about 80% are theoretical. They found that about half of the journals publishing most particle physics papers are willing to move to an open-access model, with a cost per paper of between \$1-3000. These included APS and IOP journals, but did not include Elsevier journals like Nuclear Physics B. The APS has announced a program that would make papers in its journals open access at a cost of \$975-1300 per paper, and Elsevier has announced something similar at around $3000/paper. The CERN task force proposes raising \$6-8 million/year over the next few years to start supporting the half of the journals (not including Elsevier ones) that it has identified as ready for Open Access.

What is being proposed here is basically to give up on what a lot of people have hoped would develop: a model of free journals, whose cost would be small since they would be all-electronic, small enough to be supported by universities and research grants. Instead the idea here is to keep the current journals and their publishers in place, just changing the funding mechanism from library subscriptions to something else, some form that would fund access for all. The CERN task force suggests various sources for funds over the next few years, in a transition period, but doesn't address the long term funding problem. If you fund these things out of, say, NSF grants, when Congress decides to cut the NSF budget, there's a serious danger of the plug getting pulled on a field's entire scientific literature. One popular idea is that researchers themselves should pay the cost. The problem with this is that the bulk of the literature is theory papers, mostly from people who can't afford this. When there is a mixture of journals that require authors to pay the cost and those that don't, authors abandon the ones they have to pay for. The Elsevier journals like Nuclear Physics B achieved dominance over the APS journals during the 70s when the APS journals were financed by "page charges" paid by authors, but Nuclear Physics B cost nothing to publish in.

The CERN task force doesn't seem to me to be providing a viable long-term plan for moving to the kind of open access model they are supporting. It doesn't address the fundamental problem of keeping a system where physicists hand over the scientific literature to Elsevier, then have to figure out how to buy it back. Even if a willing organization is found that will give \$3000/paper to Elsevier, what will keep Elsevier from deciding to keep publishing more papers? What if the organization in question gets tired of this and decides to stop paying?

The CERN report also contains a lot of highly debatable arguments. It claims that the current refereeing process is extremely important, valuable, and must be maintained at all costs, ignoring the fact that virtually everyone accesses papers at the arXiv, not at the journal. It's true that the refereed version in a journal may be improved and have errors fixed, but authors are generally free to replace the original preprint version by a corrected one on the arXiv. The description given in the report of the "high standards of peer review" doesn't agree with the reality of what is going on (see the Bogdanov affair). The mathematics literature still has a functional peer-reviewing system and it plays a very important role of keeping the number of incorrect proofs and unreliable results to a minimum, but the particle physics literature is very different. The report does continually make the point that the refereed journal system is crucial to the ways institutions evaluate people and decide whether to hire or promote them, but it doesn't address the issue of whether this is a good thing.

The report also tries to claim that the advent of LHC data will somehow make the refereed particle physics literature and open access to it much more important. I don't see this at all. The experimental results from the big LHC detectors will come out only after very careful vetting by the groups themselves, and I don't see how a referee is likely to have much of a useful role there. If surprising experimental results are found, there will be a frantic battle among theorists to get a preprint out that explains the new data, and everyone will be following this on the arXiv. By the time such papers get through refereeing and are published, few people will still be paying attention to them.

Update: Nature Physics also has a recent article about peer review and open access.

Comments

1. **anonymous**
   September 9, 2006

   Peter, isn't the democratic solution is to have an alternative to arXiv, i.e., to have two independent electronic servers with different referees etc?

   This existed in the CERN Document Server up until 8 October 2004, when:

   "The CERN Scientific Information Policy Board (SIPB) closed the CERN CDS EXT preprint series, thus depriving me of preserving my work by posting it on EXT, as I had done with some papers that blacklisting had barred me from posting on arXiv…" – Tony Smith, http://www.valdostamuseum.org/hamsmith/jouref2.html

   Mainstream string theory and other evidence-lacking orthodoxies survive and indeed flourish just because they acquire dictatorial power to suppress other options by force, i.e., where you have one electronic preprint server with one set of referees, and no alternative. Imagine Lubos Motl or a close friend of his in charge of arXiv, and you see why people are so cautious about attacking the mainstream. Externally-submitted CERN Doc Server papers (accepted up to 2004) now can't even be revised/updated!

2. **CapitalistImperialistPig**
   September 9, 2006

   Peter,

   The ArXiv model is orders of magnitude cheaper to run than most of the open access models cited. I doubt that peer review does a lot for physics, but couldn't it be replaced by an open commentary scheme? Authors could solicit commentary from either known or hidden reviewers. A hidden review panel could probably be managed by an almost entirely automated process. Review comments could be either provided to the author or public.

3. **MathPhys**
   September 9, 2006

   I think that nowadays when a typical mathematician or physicist does a literature search, they start by searching arXiv. When something relevant is found, they use it.

   The main criteria to verify correctness is whether the author is known, and whether the work is cited in other preprints. I doubt if being published in a printed journal is a criterion at all.

Even if the preprint is a few years old but unpublished, one thinks “Oh, maybe it was in a conference proceedings”.

4. **D. Eppstein**
September 9, 2006

I don’t know if I’m typical, and my research area is far from physics, but I usually start my literature searches with Google Scholar. ArXiv is a great resource but still does not contain most research in my area, and anyway Google indexes it well along with some other resources such as the ACM Digital Library that cover my area better.

5. **werdna**
September 9, 2006

MathPhys says:

>I think that nowadays when a typical mathematician or physicist >does a literature search, they start by searching arXiv…The main >criteria to verify correctness is whether the author is known, and >whether the work is cited in other preprints.

Actually, mathematicians do not verify correctness of results based on the reputation (or lack thereof) of the author(s). Neither do citations matter much to them either. What matters is the correctness of the proof. That’s why the peer review process serves a much more important function for math journals than for other disciplines.

6. **Scott**
September 9, 2006

Thanks, Peter! To me there’s something astounding — and in need of explanation — about the academic community’s spinelessness and timidity on this issue. “Sure, we should experiment with open-access, but *obviously* we can’t abandon the core idea of donating all our work to Elsevier and then spending a fortune to buy it back.” What is the source of this craven conservatism, among people who in other contexts are (1) trained to question things, (2) generally left-leaning and distrustful of corporations, and (3) finely attuned to the ironic and absurd? I speculated about this in a book review I wrote in April, but didn’t reach a satisfying answer. I’ll be grateful if anyone else has a conjecture.

7. **jeremy**
September 9, 2006

Actually, peer review is important for all areas of scientific research, unless you don’t want to know the correctness of the previous results that you are referring to.

8. **John Baez**
September 10, 2006

Scott writes:

> To me there’s something astounding — and in need of explanation — about the academic community’s spinelessness and timidity on this issue. […] What is the source of this craven conservatism, among people who in other contexts are (1) trained to question things, (2) generally left-leaning and distrustful of corporations, and (3) finely attuned to the ironic and absurd?

As an academic who has rebelled against the system of academics working without pay for media conglomerates who charge high prices for the resulting journals, the source of the conservatism is **obvious**.

Reed-Elsevier, Springer Science+Business Media, and other conglomerates now own many of the most prestigious journals in science. They’ve spent the last half century buying them up.

“Prestige” may seem like an abstract and subjective concept, but it plays a deadly serious role in the career of any academic. Hiring, promotions and other decisions are based on it.
If a mathematics department has a choice between hiring two candidates, otherwise equal, one of whom has published a paper in *Inventiones Mathematicae*, while another has published in *Transactions of the American Mathematical Society*, everybody knows who will get the job. Every mathematician, that is – for we all know which journal is more “prestigious”.

So, even as the university library is crying for help, struggling to pay the ever-growing costs of journals run by the big media conglomerates, the science faculty continues to publish in these journals, because their careers depend on it.

The science faculty also work as editors for these journals, typically for no pay – just for the prestige of being on the editorial board. They also work without pay refereeing articles for these journals. They write papers that appear in volumes published by the same media conglomerates, again just for the prestige of having a paper in a prestigious volume. And, they write books for presses owned by the same conglomerates.

I find *these* activities to be a bit more craven, because I haven’t seen people getting hired just because they do these things. But, just as millionaires work their ass off to become billionaires, a lot of the most prestigious scientists engage in these activities to polish their reputations to an ever finer sheen. This is especially true of people who have given up trying to do original research.

I get lots of invitations to write books and papers for various collections, because people know I can write. These days I almost always turn them down. I’ve learned a key fact: when someone gives me an *honor*, it’s usually a way to get me to do work for free. I still give lots of talks, because I get free travel out of it, and I really enjoy explaining stuff. But writing review papers for volumes published by prestigious publishers – that’s something I’ve come to really dislike.

Each time I turn such an offer down, I feel a little ache, because I know I’ll miss out on a little piece of *prestige*. For example, I could have contributed to the forthcoming “Princeton Companion to Mathematics”. I almost did – what a great opportunity! But I didn’t. I’d rather do whatever the hell I want on a given day – usually thinking about math and physics. I’m in an incredibly lucky position where I can afford to do this; it seems insane not to.

In short, to understand what’s going on, you have to realize: big companies care about profits, academics care about prestige.

9. **Scott Aaronson**
September 10, 2006

Thanks, John! I agree that prestige is part of the answer — but if it were the whole answer, then wouldn’t we expect academics to agree *in principle* that the system sucks, even as they vied to publish in the top journals? (Much like they agree in principle that global warming sucks while driving their SUVs.) Yet I have friends who get up at conferences to *defend* the system, who make arguments like “maybe if we’re nice to Elsevier they’ll give us a 5% discount…” That’s the part I don’t understand: are they trying to reduce cognitive dissonance? Or is it just inertia?

10. **jeremy**
September 10, 2006

John Baez writes:

"In short, to understand what's going on, you have to realize: big companies care about profits, academics care about prestige."

Companies, big or small, have to make profits, if only to survive. Academics, however, don't need to gain prestige by publishing in a prestigious journal, if they can make breakthroughs in their research. Recent work of Perelman on Poincare conjecture would be an excellent example.

11. **M**
September 10, 2006

I do not see any Open Access problem: having papers on arXiv plus a line on SPIRES adding "published on…." already is Open Access.

The real problem is that it is expensive, because hiring committees and libraries keep traditional journals alive.

What we would need is not arXiv + Open Access journals, but arXiv + refereeing agencies i.e. somebody who certifies the quality of papers.

This somebody should be accepted by hiring committees (that already accepted JHEP), not be managed by physicists (just like a typical journal), not spend money in re-typesetting papers (possibly unless for those few authors who have serious problems with grammatics or with LaTeX).

JHEP retypsetted everything and could not survive as a free journal, was managed by physicists and political battles sometimes distorted even the refereeing process.

12. **amused**
September 10, 2006

Following on from what John Baez wrote above, and providing one possible answer to Scott:
In theoretical hep these days, people working in the dominant area don't have much need for publishing in prestigious journals – their
standing, job prospects etc are determined by the opinions of the senior influential people in their area on the papers they put on the archives. On the other hand, for people outside the dominant area, especially those from obscure backgrounds and without the "right connections", the possibility of publishing in a prestigious journal is practically the only way to gain some visability and the possibility of a research career. I owe my own career to date, tenuous as it is, to the existence of such journals and am therefore not much inclined to say that the system sucks. More objectively, well-run "prestigious" journals have an important role to play in research communities by providing objective "quality stamps" in the midst of all the political/sociological stuff. Private publishers who have been able to maintain such journals over longer periods deserve some respect and appreciation for this, regardless of what one might think of their pricing policies.

13. **Gebar**
September 10, 2006

You need a new model –or rather an extension of the present one. This will seem utopic, of course, but it is not unfeasible.

First you have to have some kind of "committee" who will actually try to find solutions and implement them. For example, sparc. The more prestigious, the better.

Academic publication has two main players. Arxiv and the "prestigious" journals. Arxiv could and perhaps should be extended (see rumors of "censorship" of papers that do not agree with its administrators' beliefs).

Ideally, it could be extended through a partnership with a big player in the open access field. The committee could talk with Creative Commons, Internet Archive, OurMedia, Wikipedia, and Google, to provide servers and bandwidth.

Any of them would be interested, I think, in participating in such a project. Right now, I, as an individual, can publish anything I want for free in OurMedia or the Internet Archive, with the material retained there indefinitely.

The printed part covered by the "prestigious" journals is much more important. Only electronic access through solutions such as arxiv is not enough to overthrow the present paradigm and mindset. You need also printed journals with a physical presence in the libraries that will gradually "push aside" the existing exorbitantly priced ones.

For this you will need a partnership with a self-publishing company (e.g. lulu.com). The cost of self publishing a black & white 100 pages book through lulu is $6.53, plus shipping, which is extra and obviously varies by area, but is not unreasonable. So what is this $3000/paper published in a journal, when the editorial board and referees all work for free?

Amazon also has a self publishing program. It is more expensive, but again this is a matter of negotiations. Amazon undoubtedly would like to be associated with such a prestigious project, as would any company.

With such a model, you could charge libraries reasonable amounts, and be even able to pay the editorial board and the referees. And you could sell subscriptions too, again with reasonable prices.

So you set up this partnership with the self-publishing company, you get the prestigious editors and referees from the prestigious journals to work on the open access journals too, and then gradually only on the open access journals, you persist in this, and you may have something.

You just need important papers from important people to get published in your open access journals, and the rest will take care of itself. This is not so difficult. People with established reputations will not have much resistance in publishing in open access journals, and if they do, the rest will follow.

Well, I did say that it will seem utopic. However, it only seems so, it isn't. You can't do open access with the old closed bureaucratic mindset. You need to get into the project players with the new open mindset.

That said, I know it is quite improbable for something like this to actually happen. And this despite the fact that you, the academics who participate in these science blogs, plus your interested friends and acquaintances, could get together, review a number of worthy papers from arxiv, and have a printed journal out by the end of the month.

You already do this when you post in your blogs links to arxiv papers you consider important. The step from that to have these papers printed in a journal of good print quality is trivial. You could even start by publishing Pelerman's papers. You can't get more prestigious than that.

14. **Peter Erwin**
September 10, 2006

For what it's worth, paying for journal publication is somewhat the norm within astrophysics, and has been for years (pre-dating the whole "open access" dicussion). Three of the four top journals have "page charges" (slightly more than $100 per page); these are sometimes paid by authors' institutions, and sometimes by the authors out of their grants (this is a standard budget item in grant proposals, like money for attending conferences). The two US-based journals, which always have page charges[*], do not suffer relative to the British journal *Monthly Notices of the Royal Astronomical Society*, which has no page charges.

(These are in addition to institutional subscriptions.)

[*] The European journal *Astronomy and Astrophysics* has a system where "sponsoring countries" contribute yearly sums, in return for which all scientists from those countries can avoid the page charges.

15. **ObsessiveMathsFreak**
September 10, 2006

> The report also tries to claim that the advent of LHC data will somehow make the refereed particle physics literature and open access to it much more important.

What is it with all the appeals to authority of the LHC? It's not even finished, but every second argument in the theoretical physics blogosphere and beyond seems to add in that; "LHC will probably confirm/disprove/support/question this theory/argument/viewpoint".

It's only a particle accelerator, albiet an extremely big one.

16. **Gina**
September 10, 2006

While the prices of commercial journal is often too high and scientists should play a role in pushing towards lower prices, I would be very cautious to try to move to an entirely different system where everything is controlled and run by scientists and/or based on publically-financed infra-structure.

On another matter, I would like to suggest to Peter to devote a special post on the "not even wrong" blog to allow for comments, questions, reviews, critiques, and perhaps a discussion on his book "not even wrong".

17. **Sailor Moon**
September 10, 2006

And where does the funding for arXiv.org come from? The discretionary budget of a library with a frozen (shrinking if you account for inflation) budget. It competes with armies of librarians in public services, an undisciplined program of one-off digital libraries, armies of public services librarians, a content-management system implementation undertaken for political reasons, and competition with Project Euclid, an essentially for-profit effort in math publishing.

The future isn't bright.

18. **TheGraduate**
September 10, 2006

In order to have open access I think the following changes are key:

1. All professors should be paid for their work: I think that professors working for free introduces economic inefficiencies because it makes it difficult to access the true cost of printing the journal.

2. Specific institutions should offer temporary contracts to have their journals managed by the companies in the private sector and the institutions should then offer their journals to the public for free.

The competition is necessary to keep prices down. Without competition, the prices would inevitably rise. By having specific institutions offer this service, it also allows variation eg. governments, universities.

I think it is essentially impossible to offer something to the public for free without having a benefactor.

19. **Juan R.**
September 10, 2006

Academia would focus on the true issue: optimization of the publishing process, developing a solid scientific language for publishing (LaTeX is **not**) being learned by young scientists and enginners at University. Any other approach is in my opinion –and history is proving me- a waste of time. If you do not reduce publishing costs, them translating them from readers to authors (PloS approach) or to mixed grants/funds/fees approaches or to 'free' systems (e.g. ArXiV) will not work as time has proved in many ocassions:

– PloS is economically unsustainable and currently they are losing a lot of money and survives thanks to millionary funding from third philantrophy bodies.

– ArXiv model has failures with administration, peer-review and is only working for simple publication process (PDF articles served online) in theoretical disciplines. It does not work for other disciplines. ArXiv-like approach fiasco in chemistry is well-known, i.e. the fiasco of the chemical preprint server: CPS.

– About SPARC. I find interesting they omit to say the free PloS journal cited in its site relies on $13 millions philanthropic grants and that charge $2,500 to authors. A look to SPARC membership dues is also interesting.

– Etc.

There is also a myth about that electronic journals are of small cost. Well, PDF versions of printed articles are cheap but that is not all one needs. In disciplines as chemistry publishing is something more than a PDF article of a theoretical work. Moreover, the myth of low cost vanishes with full electronic journals doing use of advanced web technologies: semantic web, XML specific scientific languages for datuments, databases…

In my opinion, there is another myth on private publishers being too expensive –that myth was base of finantial failure of PLoS and related approaches-. Well I have purchased 10 pages article from 80s on APS journals (official body for physicists) at $25 and 3 pages 2006 articles on ACS (official body for chemists) journals at $25 (the online version). Both APS and ACS are **non-profit** official bodies and publishs their own journals. Still $8,3 per page for a recent article or $2,5 per page for a 20 years old article are very expensive rates. This indicates that excesive profit from private publishers is not all of the problem, is it? Note: i agree that profit rates of Elsevier, Nature, and other private publishers could be lower and journals less expensive.

Another point of disagreement is in the double attitude of academia. At the one hand, they critize expensive journals and claim for open or free approaches but when publishing most of people choses a highly respected journal (i.e. those usually at $30 per article) because their colleagues give it more prestige and posibilities for a career. Would not prestige be offered in basis to quality of works instead where they were published?

Baez said:

> I've learned a key fact: when someone gives me an honor, it's usually a way to get me to do work for free. I still give lots of talks, because I get free travel out of it, and I really enjoy explaining stuff. But writing review papers for volumes published by prestigious publishers – that's something I've come to really dislike.

I buy each word of this!

jeremy Said:

> Companies, big or small, have to make profits, if only to survive. Academics, however, don't need to gain prestige by publishing in a prestigious journal, if they can make breakthroughs in their research. Recent work of Perelman on Poincare conjecture would be an excellent example.

You are right, but in some countries (e.g. Spain) position in Academia follows strict buroucratic (stupid?) rules. You receive many points for publishing in journals of class A, and little points for journals of class B, and C. After all points are computed for the committe and you can obtain a career or not.

Gebar said:

> For this you will need a partnership with a self-publishing company (e.g. lulu.com). The cost of self publishing a black & white 100 pages book through lulu is $6.53, plus shipping, which is extra and obviously varies by area, but is not unreasonable. So what is this $3000/paper published in a journal, when the editorial board and referees all work for free?

This highlights another of myths between academics. I managed a printed bulletin for chemists some years ago -Galicia química for the official body of chemists in Galicia (Spain)- the real cost was not in printing and distribution (we optimize size and shape for printing costs and weight for minimizing distribution charges). The real cost was in administrative and meta-publishing costs. Therein today you buy a 10 pages PDF article by $30 today on a non-profit ACS journal. Elsevier for instance has developed their own XML language for its internal publishing workflow. Even reusing available standards –e.g. MathML for mathematics from W3C, character encodings from Unicode- they still were forced to develop extensions to the official MathML spec for special needs on its journals and continue working with the Elsevier matrix for encoding because Unicode does not cover all of academia publishing –this will change when STIX project for scientific and enginnering fonts was finished-.

Suggestion: The 'open' or 'free access' initiative would also address the problem of books and monographs. They are very, very expensive doing scientific data was only accesible to rich scientists (or scientists in a rich University). How could someone at some humble University pay more than 1000 euros -more than $1270- for a specialized handbook on molecular physics and quantum chemistry from Whiley?

Juan R.

Center for CANONICAL |SCIENCE)

20. **Juan R.**
September 10, 2006

    Nice post!

21. **LDM**
September 10, 2006

    The mathematics literature still has a functional peer-reviewing system and it plays a very important role of keeping the number of incorrect proofs and unreliable results to a minimum, but the particle physics literature is very different.

    For example, the classification of finite simple groups (alluded to in NEW by the way), completed in the 80's, required ten thousand journal pages. Some of the papers were so dense it is probable that they were read only by the author himself and the referee.

    Would anyone believe this classification if it was based on pages only in the axXiv?
Probably not.

22. **TheGraduate**
September 10, 2006

    LDM:

    It seems to me we probably shouldn't believe it if it was only read by two people. Wouldn't you agree?

23. **LDM**
September 10, 2006

    TheGraduate:

    I understand your point and it is a good one.

    For me, it is more interesting to consider if such a proof could have been done using the arXiv as a means of publication…before we get rid of the journals, let's first be aware of and have a clear understanding of what they have accomplished.

24. **TheGraduate**
September 10, 2006

    LDM:

    I don't think open access and peer reviewing are mutually exclusive. For instance, one could have a review system where reviewers have rankings and a paper derives a reliability rating from the generally esteem in which a reviewer is held.

    In order to control the reviewing process, one might require that only AMS members can review AMS approved journals or something like that.

There is really no reason that open access has to mean a completely anarchic process.

Incidentally, I think it's important to pay reviewers for their work.

25. **D R Lunsford**
September 10, 2006

I used to spend countless hours pouring over the journal stacks in my library, while an undergraduate. I spent more time in the library than in class. Every now and then I'd come across a fascinating idea that was far off the mainstream but intrinsically interesting. Such papers have no chance on the arxiv. Open access is worthless if a battery of censors with an explicit agenda circumvents it. I still think I am the only person to have an already peer-reviewed and published paper censored from the arxiv. I have three more and a fourth underway which are IMO at least interesting. I dare not send them to the arxiv. Is this openness? If so it is a strange definition.

-drl

26. **Alejandro Rivero**
September 10, 2006

but couldn't it be replaced by an open commentary scheme? Authors could solicit commentary from either known or hidden reviewers.

Hey, this is an idea. The soon to be abandoned physcomments site was accused of SPAM because for every paper in hep-th* he asked two persons to comment about. Of course every referee request from every refereed journal is SPAM according the definition "spam=unsolicited email". But it could be diffferent if the Author is asked to write, or at least to sign with his email address, his own cover letter, which in turn is to be sent to two anonymous referees who can shoose about CCing back to the author or sending directly to the journal.

27. **RingZero**
September 10, 2006

>Academics, however, don't need to gain prestige by
>publishing in a prestigious journal, if they can make
>breakthroughs in their research. Recent work of Perelman on
>Poincare conjecture would be an excellent example.

The problem is that not only breakthroughs like Perelman's are important. In fact, those are rare and far apart. Incremental progress and consolidation of acquired knowledge is enormously important and, in time, is what leads to breakthroughs.

In fact, the progress that has been made in particle physics over the last 15 years is mostly incremental: a truly impressive mass of theoretical and experimental results that sets the stage for the next large breakthrough.

But the merits of incremental work are to some extent more debatable and open to subjective, and even "political," criticism. There's where the seal of approval of a leading journal enters the game.

28. **TheGraduate**
September 10, 2006

D R Lunsford:

There is always the internet. I think the issue is really not censorship at all. We are in an age where we can always find information if the author wants to make it available.

I think the issue is really information that can not be accessed.

When something is published by a journal, it obtains the seal of approval of that particular publisher. It is this seal of approval that we care about …

The price of this seal of approval is the information now can not be accessed by those of us too poor to obtain it.

I think we are in an age where it is within our grasp to overcome this disadvantage. Youtube, Wikipedia and descendants of Napster are proving this over and over everyday.

I would hope there would be a sort of large system where all papers could be found but those that had not been reviewed by a respected reviewer could be filtered out IF AND ONLY IF the searcher wanted them filtered out.

29. **Energex42**
September 10, 2006

I think a viable model can be a non-profit foundation run journal system. A donation from a famous person can get this thing started, then a break-even fee would be paid from the academic subscribers to keep it running.

30. **Scott Aaronson**
September 10, 2006

drl: Would you mind linking to the published paper of yours that was rejected from the arXiv? I think that, because of the arXiv's central role in disseminating science, the moderators ought to avoid doing anything that even *looks* like censorship, even if it means the rest of us need to scan the titles of a lot of bad or irrelevant papers. So I'd be curious to see what it is that they rejected.

31. **anon**
September 10, 2006

Scott:

http://cdsweb.cern.ch/search.py?recid=688763&ln=en

32. **Gebar**
September 10, 2006

> This highlights another of myths between academics. I managed a printed bulletin for chemists some years ago -Galicia química for the official body of chemists in Galicia (Spain)- the real cost was not in printing and distribution (we optimize size and shape for printing costs and weight for minimizing distribution charges). The real cost was in administrative and meta-publishing costs.

That's exactly what I mean when I say that you cannot do open access with a bureaucratic mindset. You let your "partner" (Amazon, or Lulu, or whoever) handle the administrative and meta-publishing costs, because they have already in place the infrastructure to do it.

Managing your subscriptions could be as simple as logging into your journal administration account in Amazon and clicking a few buttons so that the last issue of your journal will be sent to a list of recepients, in the same way you order a book to be sent to a friend.

The subscriptions could also be sold be Amazon, as well as individual issues. I can bet you they will cost a ridiculously small fraction of these hair raising prices charged by elzevier and sparc.

Any big publishing house would jump at the opportunity to partner with a formal body of academics who want to set up such a project. However, it only makes sense to go with a company that already functions according to the open access model, so that you control the material instead of the publishing house. Else you will end up with the same situation you have today.

Here's a challenge. Propose ten important papers and get permission from their authors to be published. Anyone of you could set up an account in lulu or some other such publisher, set up the issue, and have it sent to the authors or anyone else who wants it. The price again would be ridiculous.

The only thing missing to make this viable is a respected group of people who will manage this, and a respected editorial board and group of referees, perhaps the same people who now slave away for Elzevier.

33. **John Baez**
September 11, 2006

Scott Aaronson writes:

> Thanks, John! I agree that prestige is part of the answer — but if it were the whole answer, then wouldn't we expect academics to agree *in principle* that the system sucks, even as they vied to publish in the top journals?

Hi, Scott!

That's a good question. I spend a fair amount of time trying to teach people about the the problems with the system. Academics who read my stuff usually *do* agree that this system sucks. If they're idealistic enough, they even stop publishing in evil journals. Others agree "in principle" but feel their careers can't afford that much idealism.

But the people who are being most directly injured are not the academics – it's the librarians. They're the ones who have to keep cancelling journal subscriptions or buying fewer books in order to hang onto subscriptions to the "prestigious" journals. If you talk to them, you'll find they're *livid*.

You might say the academics are being hurt by a system where they do lots of work for free while big companies make money off their labor. This is true – but I think most academics value the prestige they get from association with prestigious journals more than the money they might earn.

(There's something so nice about seeing your name on the front cover of a famous journal, right there with the bigshots in your field. It's like seeing a bronze bust of yourself. The feeling itself is worth thousands of dollars, even apart from the useful connections and influence such a position gets you.)

So, I think most academics see the problems as abstract, until their favorite journals get cancelled when the libraries can't afford them anymore – or until they notice that their libraries can't afford to buy many books anymore.

34. **John Baez**
September 11, 2006

Jeremy writes:

> John Baez writes:
>
> "In short, to understand what's going on, you have to realize: big companies care about profits, academics care about prestige."
>
> Companies, big or small, have to make profits, if only to survive. Academics, however, don't need to gain prestige by publishing in a prestigious journal, if they can make breakthroughs in their research. Recent work of Perelman on Poincare conjecture would be an excellent example.

Academics need prestige to survive – that's how we get jobs, and that's how we get promotions. Yes, we can get this prestige by making breakthroughs like proving the Poincare conjecture – gee, why didn't I think of doing that? But, most of us don't make such breakthroughs.

So, most academics will do whatever they can to collect scraps of prestige: giving talks at conferences, organizing conferences, serving on advisory boards, publishing in prestigious journals, publishing books at prestigious presses, etcetera.

If you want to see all these scraps of prestige lined up neatly and organized, just look at job applications. Anyone on hiring committees will know what I mean.

In short: to get an academic to do something, just dangle a bit of prestige in front of him. Companies aren't dumb: they know this.

35. **TheGraduate**
September 11, 2006

My feeling is that the publishers aren't really offering anything special when it comes to journals so economics dictates that prices have to fall. Academia is sort of a communist universe (lifetime jobs, free work) so maybe that's why it is taking so long for the journals to drop in price.

For instance, I am sure Wikipedia is affecting encyclopedia sales and Wikipedia is written by random people.

36. **Florifulgurator**

September 11, 2006

So, why not opening a prestigious free electronic journal with a prestigious editorial board like Inventiones Mathematicae, and the main server (feeding the mirrors and printers) at some prestigious institute?

I guess we need to wait for that till the old pre-internet big names have died out and leave space for 21st-century-ready internet literates…

37. **Moshe**
September 11, 2006

John, I am with Scott on this (I think), that is, I just don't get it, including the prestige part. For example if the editorial board of "Topology" that just resigned got together and established the free-access electronic journal \tilde{topology} or topology' or something, which would be now the prestigous journal? I'd like to think an academic community would look beyond brand names and such when attaching prestige to publications, for example by quantifying it using "impact index" or other such criteria.

38. **Juan R.**
September 11, 2006

Gebar,

Let me doubt that partners like Amazon or Lulu have the infrastracture to do it. I already wrote a bit about technical details above.

> Here's a challenge. Propose ten important papers and get permission from their authors to be published. Anyone of you could set up an account in lulu or some other such publisher, set up the issue, and have it sent to the authors or anyone else who wants it. The price again would be ridiculous.

Well, the true is that we talk with Google Scholar people and even they can offer just a small subset of search and indexing capability needed (e.g. nothing at level of CAS functionality). The Googlebot directly indexes PDF files and extract metadata to databases, but it is limited since work in PDF files. Yes, pdf files is everything in ArXiv but is not in other disciplines where c3d and csm files or cif
databases are needed.

It is true that high cost of journals is an issue, but i think that academicians are losing the main point. In fact, all alternatives i know to the standard model either provide just a subset of functionality (e.g. online journals managed by academicians via web server) or failed in other disciplines (e.g. fiasco of ArXiv in chemistry) or are economically not viable (e.g. academicians abandoned Elsevier because was too expensive -they claimed- and funded PloS are losing a lot of money even when soliciting $2500 fees for each article).

Juan R.

Center for CANONICAL |SCIENCE)

39. **Ken Muldrew**
September 11, 2006

It may be worth keeping in mind that this discussion extends beyond theoretical research. The vast majority of scientific papers communicate experimental results (vast is too small a word!). The utility of experimental results, even rather trivial, incremental results, lasts for decades (centuries, even). This is especially true in fields like biology and medicine where there are no theoretical structures that can be used to figure out what the result of some experiment ought to be. Refereeing is essential for experimental papers to ensure that enough methodological details are provided for the experiments to be repeated, as well as for technical issues. This refereeing is done by scientists, without charge, and run under the auspices of scientific societies (also run by scientists and paid for by scientists). Archiving, though, involves print journals and library subscriptions. And high quality archiving with widespread distribution is essential to the scientific enterprise.

My point is that there is more to this than just prestige; a scientist is a member of a community that has extended temporal boundaries. One can do work that extends something done by a scientist who died a century ago, but only if you know the details of what that person did so long ago. The integrity and honesty of scientific work is currently maintained through the journals and this has to be retained. For experimental work, something like the arXiv is sort of a bridge between conference talks and publications; a new phenomenon that doesn't replace the old ways but rather adds to them, like email as it relates to phone conversations and page-written letters.

I think we can solve the archiving problem through the interaction of scientific societies with librarians (and possibly the creation of some new organizations who have a passion for the integrity of information, much as those who join scientific societies have a passion for learning the nature of things). But this solution will probably take the form of a phase change, and few can afford to submit to a process that doesn't happen very quickly. For those commenters who disparage scientists for their lack of spine, it's actually a pretty big deal to lose your livelihood and your career. By the time you put maybe 10 or 12 years of university study, 4 years of post-doc, a bunch more years as a semi-faculty ghost before finally getting hired, you have quite a bit invested in your career. Getting to this point means that you have a bunch of ideas and projects that you want to get done to share with the scientific community that you belong to. It's just not very tempting to stuff it all and become a martyr to the cause at a point where it's not clear how effective your effort might be. Because if you don't keep pumping out the papers in decent journals, you're down the road.

40. **Chris W.**
September 12, 2006

Ken,

This recent Wall Street Journal article is relevant to your comment about access to experimental results, as well as the general concern with open access, although the focus here is on a shorter time horizon:

Gates Won't Fund AIDS Researchers Unless They Pool Data
(WSJ, 7/20/2006)

> There's no guarantee these particular grants, or the Gates foundation's efforts in general, will lead to a working vaccine. But since fragmented vaccine efforts have yet to protect a single human from the pandemic that rages out of control in many regions, some supporters argue it's time for a new approach. Grant recipients and outside observers were unsure whether data-sharing requirements of the grants could pose potential legal or patent conflicts with Mr. Gates's vow to respect intellectual property. Foundation officials said this week researchers would still be free to commercialize their discoveries, but they must develop access plans for people in the developing world.
>
> . . . .
>
> There are four major goals to be funded by the grants: vaccines that spark neutralizing antibodies to block initial infection by HIV; vaccines that make stronger T-cell response to kill infected cells; creation of standard criteria to measure success or failure; and a new, secure Web site for sharing all the data in real time.

“Whether in academics or industry, scientists want to protect intellectual property. … With the alliance, the shift is to say: ‘No, the large enterprise is more important than the position I keep by holding my data close,’” says Steve Self, a biostatistician at the Fred Hutchinson Cancer Research Center in Seattle, which is lead investigator of a $30.1 million grant to create new adjuvants, ingredients that boost a vaccine’s power. Dr. Self got a $10 million grant to create a secure central data repository to be named Atlas.

Enforced data sharing, Dr. Self predicted, “increases the pace of discovery enormously rather than waiting for the process of writing formal journal articles, waiting for them to be published, and [confirmed] by other labs.” As efforts funded by the Gates grants get under way, other funders must not be lulled into complacency, warns Mitchell Warren, executive director of the New York-based AIDS Vaccine Advocacy Coalition, a nonprofit community group. Activists recently have voiced concerns that the National Institutes of Health budget is flat in real terms.

41. **Chris W.**
September 12, 2006

Posted as a test; please feel free to delete after an hour or so.

(My last comment vanished into thin air…)

42. **Peter Woit**
September 12, 2006

Chris W.,

Your previous comment ran afoul of the WordPress automated anti-spam software. It is highly suspicious of links in comments. Maybe it also noticed that your comment is getting fairly far away from the topic of the posting…

43. **Chris W.**
September 12, 2006

Can you suggest any guidelines about including links, other than “don’t do it” or “include them at your own risk”?

44. **Peter Woit**
September 12, 2006

Chris,

Hard to say, the anti-spam feature is called “Akismet”, most of the time it does a remarkably good job, which is why I keep using it. But it does make mistakes, often hard to tell why, except that links are something that make it suspicious (much of the spam consists just of links). It seems to use some proprietary algorithm running on their server, which they don’t divulge. As far as I can tell, it also doesn’t allow any configuration choices, e.g. I can’t whitelist people

45. **Exploited**
September 13, 2006

I do not have a research position, and probably will not have one in the foreseeable future. At the same time, I continue to publish papers in very prestigious peer-reviewed journals, at the top of their range. In this research, I invest most of my waking existence not taken up by my actual paid duties, and I get paid nothing for it.

In most publishing models, the author gets paid by the publisher, or in royalties, or both. Yet I am expected to provide my work, from which the publisher derives obvious economic benefit, entirely for free. The fact that I collaborate in my own oppression, in order to be taken seriously, probably shows me for the idiot I am. Still, something is wrong with the model. It may have worked better in the past, but for people like me it fails miserably.

46. **jeremy**
September 13, 2006

John Baez writes:

“So, most academics will do whatever they can to collect scraps of prestige: giving talks at conferences, organizing conferences, serving on advisory boards, publishing in prestigious journals, publishing books at prestigious presses, etcetera.
If you want to see all these scraps of prestige lined up neatly and organized, just look at job applications. Anyone on hiring committees will know what I mean.
In short: to get an academic to do something, just dangle a bit of prestige in front of him. Companies aren’t dumb: they know this.”

The price of the prestigious, and not so prestigious, journals and the prestige sought by academics to advance their careers are two different things. One is created by the publisher, the Companies; the other is the product of the academics. All the prestige collecting activities listed above are in fact have nothing to do with the Companies. It is fair to argue about the sky rocketing journal prices, but it would be unfair to accuse the publishers for dangling “a bit of prestige in front of” academics.

It is not too difficult to find that although the Companies publish the prestigious journals, but none of the editors and the editorial board members of those journals comes from the Companies. In fact, the Companies do not even have much to say about who should be in the editorial board. It is the academics that serve as the editors of the journal choose other academics as editorial board members. It is also the decision of these people to publish or not publish a submitted paper. In many cases, it is the editors who will do things such as inviting “bigshots” writing review papers and organizing special issues for the journal to make the journal more prestigious. The survival of the academics is also decided by other academics based on the, sometime unspoken, rules by the academics. The real problem is the system. A system created by academics themselves. The Companies, at most, take advantage of the system, but who wouldn’t. We can hardly blame them.

The price of the journal, as the price of anything else, is controlled by the market. It is a problem that can, and will be, solved by the market force. (Game theory?)

47. **Stinker**
September 15, 2006

If you believe in open access, how come your book is not available for free on the web?

What’s the difference between commercial journal publishers and commerical book publishers besides that the former don’t pay authors?

48. **woit**
September 15, 2006

Stinker,

The main problem with commercial scientific journals is the high cost, which can be $1-2 per page. You can buy my book on Amazon for about $18, which is around $.06 per page (and the paperback will be cheaper). If commercial journals were charging $.06 per page, no one at all would be complaining. It's the very high costs which are causing serious problems for library budgets, causing them to have to cancel journals and not buy books, ending up with journals only being accessible at a small number of wealthy institutions.

Personally I also find books and journal articles to be quite different in that it often makes sense to read a journal article on line, or print out a copy to look at, but I neither want to have to print out, nor try and read on a computer screen, a 300 page book. For books like mine, the amount publishers charge to produce a well-made printed version seems reasonable. What is more of a problem are technical monographs that are often sold for $100 and up, raising the same kind of problems of affordability and accessiblity as journal articles.

49. **Stinker**
September 15, 2006

Sounds reasonable. I think the main difference is who bears the cost. The buyer of the book presumably wants to buy the book; the temporary instructor laid off because his university finds it more important to maintain a subscription to (insert your favorite crap Elsevier journal here) presumably does not want to maintain that subscription. Which is to say when one submits articles to commercial journals the cost is felt by someone else; when one publishes one's book on blah-blah it is only the publisher and the buyer who face any costs.

To put it still another way – a lot of the more expensive commercial journals are closer to vanity presses than a lot of folks like to admit – their quality is low, the barrier to publication is low, and the main reason for submitting to them is to pad one's CV, which as we all know is very important for the good of humanity (at least a small part of it).

50. **TheGraduate**
September 15, 2006

I think there are definitely some externalities concerning the buying and selling of journals.

The publishing company provides two services to the academic community, the actual physical journal and the prestige of association with that journal either through publishing in that journal, editing that journal or reviewing for that journal.

I think this situation is somewhat similar to the fashion industry where one both pays for the article of clothing and the prestige (or branding) or that article.

I think this situation almost certainly guarantees that the academic community is overpaying relative to the actual cost of manufacture and administration of the journal.

The prestige end of things functions like a tournament. In other words, a large number of researchers support the system, so that a few can excel by publishing in the most exclusive journals. But frankly, statistics on the number of papers published by most academics suggest that most academics don't benefit at all.

The journal end functions in a monopolist manner as one can not buy equivalent product from different producers.

In other words, all around this system is pretty horrible.

Note, the benefit to an individual academic in terms of submitting to a prestigious journal and advancing his career is paid for by the ENTIRE academic community. Isn't this madness?

I think the way to achieve low prices is pretty well explored ground. Many producers, equivalent product across producers, transparency in terms of determining costs and benefits.

51. **Ivars**
September 16, 2006

All scientific information must be free and accessible to users; It does not matter whatever model is used as long as it serves the purpose and moves in right direction of increased availability.

If it stops to serve its purpose, it will be changed or eliminated. No need to worry about the RIGHT solution. When time will come, old style publishers will dissappear, but to achieve that, competition with open access is needed.

It is very OK to start it by funding the current publishers into their own elimination. Why not?

Various Links, Latest From Kaku

September 10, 2006
Categories: Uncategorized

Seems like everyone is getting a MySpace site, first Michio Kaku, now GLAST.

There was a conference this past week in Madrid honoring Nigel Hitchin's 60th birthday. The program is here.

The latest issue of Nature has an article by Barry Mazur about recent progress on the Sato-Tate conjecture due to Mazur's Harvard colleague Richard Taylor and collaborators. My meager understanding of this result is that it involves extending the Taniyama-Shimura-Weil conjecture from the case of the two-dimensional representation of GL(2) to symmetric powers of this representation.

Mark Trodden and Christine Dantas both have well-done reviews of Alex Vilenkin's Many Worlds in One, which I wrote about here. Mark implicitly compares the book very favorably to Susskind's recent one promoting similar ideas. I kind of disagree with him about the book, feeling that, no matter how well done, promoting to the general public science consisting of highly speculative ideas that seem to be untestable is not a good idea. It's true that the multiverse cannot be simply dismissed on the grounds that one can't directly observe it, but if the idea is to be considered part of science one has to come up with some way to test it. So far no one has been able to come up with a plausible proposal for how to do so, and there are solid arguments that this is inherently impossible.

Update: A commenter points out that on his MySpace site Kaku has posted a copy of a forthcoming article by him that is supposed to appear in New Scientist. It is about the controversy over string theory, but doesn't at all deal with the criticisms of the theory contained in my book and Smolin's. It does contain a thoroughly dishonest paragraph about me, misrepresenting my position at Columbia (Kaku is well aware than I am a faculty member and teach graduate courses here, as well as administering the department computer system), and describing me as a "former particle physicist" (he's well aware I have recently written a book on the subject of particle physics and continue to conduct research on the subject; then again, many people consider him to be a "former particle physicist"). He ascribes my criticism of string theory to jealousy over having been turned down for tenured positions at prestigious universities in favor of string theorists, and misquotes something I wrote about string theory:

String theory has only a "poetic relationship" to reality.

I never have said or written anything like this. He is misrepresenting a point I made in the book that string theory is a quite complex mathematical structure that only has a very distant relationship to musical notes and vibrating physical strings:

Once one starts learning the details of ten-dimensional superstring theory, anomaly cancellation, Calabi-Yau spaces, etc., one realizes that a vibrating string and its musical notes have only a poetic relationship to the real thing at issue.

The paragraph about me is dishonest and misleading, and so is much of the rest of the article. Kaku claims that string theory is being criticized because it cannot be directly tested by observing vibrating string modes. Critics of string theory are well aware that many theories can only be indirectly tested, and the arguments we are giving are about lack of any predictions at all. He describes five "indirect tests of string theory", neglecting to mention that string theory makes no definite predictions about what the five kinds of experiments being described will actually see. In particular, his claim that "string theory makes specific, testable predictions about the physical properties of dark matter" is simply untrue.

Some of the article is devoted to criticizing "media hype", and a "spoiled society, always demanding immediate results". Given his own role over the last twenty years in over-hyping and over-promising results from string theory, this is kind of funny to read. In the end, his response to the critics is similar to that of Susskind: less than honest ad hominem attacks, misrepresentation of criticism, and insistence that any evaluation of the success or failure of string theory be postponed to the far distant future, at a time when he will no longer be around.

Update: From Stanley Deser, perhaps the shortest arXiv theory paper ever.

Comments

1. **anon**
   September 10, 2006

   I notice from your link that Michio Kaku has a new blog entry dated today here which gives the complete test of a New Scientist article of his to come out in mid-November:

   "Civil War Erupting over a "Theory of Everything"
   "By Dr. Michio Kaku

   "… The debate is so white hot that some physicists have traitorously switched sides. …

   "… It's a sign of the vitality of theoretical physics that people are so passionate about the outcome. Science flourishes with controversy. …

   "… Basically, any unified theory must:

   "1) unify gravity with the quantum theory (whose most advanced version is the Standard Model, with its bizarre collection of quarks, leptons, gluons, W-bosons, etc.)

   "2) yield finite answers. …

   "The Standard Model of particles simply emerges as the lowest vibration of the superstring. And as the string moves, it forces space-time to curl up, precisely as Einstein predicted. Hence, both theories are neatly included in string theory. And unlike all other attempts at a unified field theory, it can remove all the infinities which plague other theories.

   "But curiously, it does much more. Much, much more. …"

   Professor Kaku goes on to decribe you as "former" particle physicist, and tells the world his reaction to your case:

   "Personally, I smile when I hear this criticism. … My own personal point of view is that we live in a spoiled society, always demanding immediate results. We pop pills, push buttons, flip channels, and demand instant gratification. The media whips this up, lavishing praise when you are on the rise, and dumping on you when you are down."

The most important thing is that Kaku only tries to refute your case with a list of very “indirect tests” for string theory, without once mentioning Smolin or his work. The indirect tests could be also consistent with many other different theories, so confirmed predictions mean nothing, particularly since alternative theories are suppressed.

2. **Mark Trodden**
   September 10, 2006

   Hi Peter. I just wanted to correct you that I don’t necessarily compare the book to any specific other piece of work by anyone else. I merely comment on Vilenkin’s attitude. I haven’t actually read the other book you mention.

3. **woit**
   September 10, 2006

   Thanks Mark,

   I’ve added a note to correct this.

4. **D R Lunsford**
   September 10, 2006

   Peter – in the vernacular – you rock.

   -drl

5. **Christine Dantas**
   September 10, 2006

   Woit wrote:

   I kind of disagree with him about the book, feeling that, no matter how well done, promoting to the general public science consisting of highly speculative ideas that seem to be untestabe is not a good idea.

   You are certainly right on this. However, I have given Vilenkin some “discounts” because he, in fact, is careful at some points:

   – Page 61: “Another important question is whether or not such scalar fields really exist in nature. Unfortunately, we don’t know. There is no direct evidence for their existence”.

   – Page 91: “A physical theory can be suported by the data, but it can never be proved. On the other hand, a single well-established fact that contradicts the theory would be enough to disprove it.”

   – Pages 116-117: “Do we really have to believe all this nonsense about our clones? (…) First of all, there is always a chance that the theory of inflation is wrong. (…) Even if our universe is the product of inflation, it is conceivable that inflation is not eternal.”

   And, this one, which probably is not a consensus, but gives some sense that things might be confined anyway to the realm of internal theoretical consitency, at least for a long time to come, and these theoretical ideas could also end up as completely wrong when experimental facts come into play:

   – page 193: “(…) quantum cosmology is not about to become an observational science. The dispute between different approaches will probably be resolved by theoretical considerations, not by observational data. (…) This issue is not likely to be settled any time soon.”

   So, I tend to believe that the public for this book, namely, people interested in science, will be able to get the warning messages. But I do think that he could have been much more emphatic on the warning signs, yes.

   Christine

6. **MathPhys**
   September 10, 2006

   I want you all to stop trashing Michio Kaku. Anyone who can figure iceskate that good at the age of 59 has my support.

7. **TheGraduate**
   September 10, 2006

   Peter,

   I was curious about why you were so hard on Kaku, calling him “dishonest”. I was thinking that “former particle physicist” was much more positive than what Susskind had said.

8. **Renormalized**
   September 10, 2006

   “For ten dark years, string theorists wandered in the wilderness. Only the true believers, those willing to suffer severe deprivation and humiliation, kept the home fires burning.these dark years.”

   This reminds me of a story from the bible where another religion was born. Nothing like suffering to bring out castrametation, fortification and entrenchment.

9. **woit**
   September 10, 2006

   TheGraduate,

   It’s remarkable that both Susskind and Kaku won’t answer the criticisms of string theory in my book and instead engage in ad hominem argument. Susskind had a lot to say about me, both in a radio interview and in the Times Higher Education Supplement. Some of it was stupid and dishonest, some of it wasn’t.

   Kaku is well aware that, besides administering the department computer system, I’m a full-time faculty member in the math department at Columbia, teach graduate and undergraduate courses, and continue to engage in research in particle theory. His description was designed to try and dishonestly discredit what I have to say. I think it’s pathetic. It’s highly tempting to respond in kind by quoting what some of his colleagues have to say about him, but I’ll restrain myself.

10. **woit**
September 10, 2006

Renormalized,

He also refers to string theorists as “defenders of the faith”.

I wonder why some people think string theory has become a cult…

11. **who is Kaku?**
September 11, 2006

dear Peter, maybe you should clarify to the readers outside US: why M. Kaku is so important? SPIRES tells that he wrote a paper in 1999 and various string papers many years ago, so the answer remains unclear to me.

12. **MathPhys**
September 11, 2006

I think Kaku is great. You have to admire a man who has the guts to call himself “one of the founders of string theory” AND look up people like Witten in the eyes.

13. **Johan Couder**
September 11, 2006

I rather liked the Feynman joke

‘(Schwarz remembers meeting Richard Feynman in the elevators during these dark years. Feynman would say to him, with a smirk, “And how many dimension are we in today, John?”)’

You should definitely add this in your next edition.

14. **John Baez**
September 11, 2006

who is Kaku? asks:

> dear Peter, maybe you should clarify to the readers outside US: why M. Kaku is so important?

His importance has nothing to do with his research. Just look at his website. He’s famous as a popularizer of science – a kind of media star. He has a call-in radio show. He’s written a number of popular books – you can buy autographed copies from his website! And, he writes columns for influential papers like the Wall Street Journal. So, he affects the opinions non-physicists have about physics.

One can argue about how this matters – but I think it matters a lot. For one thing, every physicist was at one stage a non-physicist. If students hear some branch of physics is cool, they will want to enter it. For another thing, some non-physicists make policy decisions that affect physics.

15. **MathPhys**
September 11, 2006

John,
I actually agree with what you say. Someone has to the job that Kaku is doing: bringing science to the layman in a glamourous way, and he’s definitely good at it.

16. **stupid**
September 11, 2006

“PARALLEL WORLDS
A Journey Through Creation, Higher Dimensions,
and the Future of the Cosmos

“Parallel Worlds [has been] selected as a Finalist for the Samuel Johnson Book Prize for Non-Fiction…” – http://www.mkaku.org/

See? It’s “Non-Fiction”. So extra dimensions are fact.

17. **Chris Oakley**
September 11, 2006

> Parallel Worlds [has been] selected as a Finalist for the Samuel Johnson Book Prize for Non-Fiction…”

I would be prepared to nominate it for the *Humour* prize.

18. **Chris Oakley**
September 11, 2006

Oh – and by the way, Peter, Happy Birthday and keep up the good work!

19. **LDM**
September 11, 2006

Regarding the behavior of scientists who have managed to become famous (whether this fame was acquired through genius of research or self-promotion is moot)…

Once a scientist becomes famous, he/she is then in a very fortunate position to positively influence events outside of science…i.e., affect public policy.

We need only remember Feynman and the shuttle disaster…and before him, you can find Linus Pauling getting atmospheric testing banned
(a nice video on this if interested: http://video.google.com/videoplay?docid=257544512953282441&q=linus+pauling&hl=en)

I think it would be very foolish for a scientist to tarnish his/her reputation for veracity just to promote a book and make a few dollars…because once you have lost this reputation, it is very difficult to recover.

Also, Happy Birthday Peter!…life begins at N=40
(http://www.newscientist.com/article.ns?id=mg19025450.500)

20. **hack**
September 11, 2006

Everyone knows that Kaku is THE FATHER OF STRING (field) THEORY.

Anyone know how to make "field" appear in a smaller font?

21. **A Babe in the Universe**
September 11, 2006

Happy Birthday, and many more! Your criticism of strings is needed to balance hype of Kaku, Greene and others. Whip eternal Inflation Now!
After that, how about "dark energy"?

22. **Peter Woit**
September 11, 2006

I've just heard from Michio Kaku, who has had his webmaster take the article in question down, explaining to me that it was a very preliminary version of something he was writing, he had yet to check any of the facts in it, and had not intended it to be made public. My thanks to him for his gracious behavior when this was pointed out to him.

23. **werdna**
September 11, 2006

And kudos to you, Peter, for your gracious acknowledgment of your critic's proper behavior. Giving credit where credit is due (even to your "enemies") is a necessary characteristic of a good scientist.

24. **Mahndisa**
September 11, 2006

09 11 06

Happy Birthday Peter, and the links you provided on the non commutative geometries were quite useful. Thanks. As to the other stuff, sometimes I wonder if you spend too much time dealing with naysayers but perhaps the controversy can generate book sales. In any event I wish you success and more book sales!

25. **Ron Avitzur**
September 11, 2006

John Baez wrote: *some non-physicists make policy decisions that affect physics*. XKCD illustrated this recently.

26. **Stefan**
September 12, 2006

Post I:

Hi Peter,

Peter, regardless of all the neverending cocophony surrounding HEP these days, *ultimately* it will be experiments and solid research which will decide the validity of respective theories.

Can't critics (of any theory – strings, LQG, or what-not) take the intellectual moral high-ground and just post a message (similar to the one above) on their websites and *move on* to conduct research.

27. **Stefan**
September 12, 2006

Peter, your site is no longer accepting my comments. So, perhaps later.

28. **nigel**
September 12, 2006

Stefan,

Feynman and Glashow did that. They spoke out against the domination of HEP by stringy speculations, but were ignored. It did not help to create a greater diversity of research directions, because nobody listened.

Groupthink doesn't fall that easily. If Peter used this blog mainly to explain his own research in representation theory, he would just be another person with an axe to grind, or "pet theory".

Dismissing critics for promoting alternatives to string is more effective that dismissing critics for having no alternatives to string. In the former case, the critic is deemed an egotist, while in the latter case, the critic is deemed a dull plodder. Better dull plodder than egotist.

The inclusion of the Calabi-Yau manifold with its many variable parameters makes M-theory inherently insoluble and speculative. The response to such criticism is a Watergate-type cover up by a Nixon-like stringy leadership.

29. **D R Lunsford**
September 12, 2006

Happy integer T sub P, Peter – my friend the high school physics teacher ordered both the UK AND the US editions of your book for his library – and Smolin's, and to be fair, Susskind's. Here's hoping T sub P = k for large k.

-drl

30. **woit**
September 12, 2006

Stefan,

My blog software decided you were a spammer after you submitted several short comments in quick succession. Please don't do this, and please avoid submitting long comments (in one part or many) that aren't directly related to the postings here.

Yes, critics of string theory should, now that the problems with string theory are finally getting a proper airing, devote more of their time and energy to working on alternatives and explaining to others what they are. Now that my book is done and out, that certainly will be more the focus of my attention.

31. **Stefan**
September 12, 2006

Peter,

Thanks for the guidelines; I'll try not to post in succession from now on.

As for your second paragraph: yes, it's time to discuss alternatives. If you recall (?) our previous discussions, I have browsed through Amazon looking for works in Operator theory (for QM), Gauge-theory (the best ones I could find), and other areas in HEP. If you don't mind may I share the list with you to elicit your feedback? (I don't have much expertise in the areas covered).

If you should agree please send me a confirmation e-mail at my supplied address.

p.s. I know, the post is somewhat out-of-topic, but I don't have your e-mail. Please forgive… 🙁

Your friend,
Stefan

32. **dan**
September 12, 2006

I personally have read Kaku's books, and I object to how Kaku presents string theory as established fact.

From what I am reading now, it sounds like Kaku is another Lubos.

33. **MoveOnOrStayBehind**
September 12, 2006

"Can't critics (of any theory – strings, LQG, or what-not) take the intellectual moral high-ground and just post a message (similar to the one above) on their websites and *move on* to conduct research."

Right, critics and alternatives always occupy the moral high ground, just for contradicting the main stream, isn't it, no matter whether it makes sense or not (a classic german attitude).

Alas…. hmm.. there may be a little little bit of a problem…. while it is easy to convince laymen that the mainstrem needs to be abandoned just for the sake of it, the experts need to be convinced too, of any viable "alternative"… and, lets face it…. where is such a thing? The string community eagerly waits for tips and guidance as to what those alternatives were!

It's like NASA engineers being told that they should abandon their trade and rather consider alternative rockets and engines, out of old dishwashers, car motors, etc while carpenters propose to build rockets out of wood. I am sure if it came down to a vote on the internet, the mainstream rockets would be eliminated in favor of such "alternatives" everyone with zero education can have an idea about. It doesn't matter what the engineers say, as everyone on the internet is an expert and counts equal, as Lenny says so nicely. Indeed this is democracy at its finest!

34. **Peter Woit**
September 12, 2006

MoveOn,

The problem with your analogy is that NASA engineers have designed rockets that do what they are supposed to do. String theory, as an idea about unification, doesn't do what it is supposed to (explain anything about the standard model). In your analogy, string theory would be a heavily financed program to design a rocket that, after more than 20 years of effort, had only produced a sequence of more and more complicated test rockets, each performing worse than the last, with the latest versions (the anthropic landscape ones) blowing up in people's faces at launch.

String theorists should stop whining that no one is presenting them with a well-worked out alternative that can do the things string theory has failed to do, and either get to work on an idea about string theory that goes somewhere if they have one, or help look for an alternative.

35. **Eugene Stefanovich**
September 12, 2006

MoveOnOrStayBehind:

Your comparison of string theorists with NASA engineers would be correct if string theorists had at least one flying rocket. They hadn't. Isn't it a better analogy to say that string theorists are trying to build a rocket out of yarn? Then switching to old dishwashers does look as a big step forward.

36. **dan**
September 12, 2006

Incidentally Peter,
one reason I come to this weblog is that I find your comments about string theory entertaining.

"In your analogy, string theory would be a heavily financed program to design a rocket that, after more than 20 years of effort, had only produced a sequence of more and more complicated test rockets, each performing worse than the last, with the latest versions (the anthropic landscape ones) blowing up in people's faces at launch."

LOL

Incidentally, Peter, I read your book, including your anecdote about seeing Witten at the library and then disappearing, and musing he's an extra-terrestial alien with teleportation, wouldn't the quickest, easiest, most economical and most straightforward way to get string theorists to work on other research programs (such as LQG & Sundance-Bilson preon models of the standard model) would be to convince just one man, Edward Witten, to work on them? I would imagine if Edward Witten could be convinced to work on LQG, a lot of other string theorists would follow suit.

"String theorists should stop whining that no one is presenting them with a well-worked out alternative that can do the things string theory has failed to do, and either get to work on an idea about string theory that goes somewhere if they have one, or help look for an alternative. "

Where Witten goes, string theorists follow.

37. **TheGraduate**
September 12, 2006

dan,

That's an interesting point. I would be interested to read what Witten had to say about the current situation. I read that his undergraduate degree was in history. I bet he's a pretty good writer.

38. **dan**
September 12, 2006

TheGraduate,

I forget if I read that in Peter's or Lee's book (that Witten was a history major.) It amazes me I would think Witten was a super-child prodigy, math olympiad, etc. I wonder if he's like a Good Will Hunting.

I would be interested to read what Witten had to say about the current situation as well. I would be interested to read what Witten had to say about Peter Woit's NEW or Smolin's Trouble books.

One wonders whether Witten's pursuit of strings is like Einstein's pursuit of a unified field theory, and Smolin-Sundance-Fotini's preon model might be the equivalent of Tominiga-Feynman's pursuing quantum field theory.

39. **ak**
September 12, 2006

move on said:

'Indeed this is democracy at its finest! '

it seems it wouldn't be quite that simple to translate the failure of a 'TOE' to layman's terms if string theory wouldn't obviously fail on layman's grounds, one could call this a problem of perspective: for to judge rocket launchers not-launching-rockets you don't even have to see the rocket launcher.

40. **Chris Oakley**
September 12, 2006

> I would be interested to read what Witten had to say about Peter Woit's NEW

I think that we already know the answer to this: he did not want the book published as he felt it was airing the HEP theory community's dirty laundry in public.

41. **dan**
September 12, 2006

Dear Chris Oakley,

Did Edward Witten specifically state he did not want NEW published, and if so, where does he say this? And Peter, if Witten expressly did make such statements, how do you feel about that?

Curious
Dan

42. **Peter Woit**
September 12, 2006

About Witten,

As far as I know, it's not at all true that Witten opposed publication of NEW in any way. After I ran into trouble with Cambridge about publishing it, I sent Witten a copy and asked him what he thought. I don't want to quote personal communications here, or put words into his mouth, but he certainly didn't in any way tell me he didn't think it should be published. He definitely disagreed with my point of view, gave some reasons why, and wasn't offering to call up Cambridge and tell them they had to publish my book, but he also was not at all telling me that he objected to its publication in any way.

Chris's characterization does describe one of the referee reports that Cambridge got, but from the very little I was told about the referee, it definitely wasn't Witten.

43. **dan**
September 12, 2006

Dear Peter

"He definitely disagreed with my point of view, gave some reasons why"

I understand that your communication with Witten is private and personal, and that you will not inclined to share them, which is entirely understandable, but generally speaking, (not necessarily speaking about Witten) what reasons do respectable, seriously scientific string theorists who are familiar with the arguments and criticisms levelled in your book (i.e landscape, SUSY-cosmological constant, etc.,) and Lee's, continue to research string theory?

String theorists in general disagree with your viewpoint, and what are the general reasons they give, that are non-polemical rationally defensible, scientifically respectable?

44. **Peter Woit**
September 12, 2006

The most recent thing Witten has written about his views on string theory that I am aware of is from late last year, a piece in Nature:

http://www.sns.ias.edu/%7Ewitten/papers/Unravelling.pdf

I think it reflects well the views of sensible string theorists and is scientifically respectable and rationally defensible. It's not really "polemical", but it definitely takes the most optimistic possible view about string theory that can be defended and, as readers here are well aware, mine is rather different.

45. **Dan**
September 12, 2006

Well thank you Peter. Do you whether he's made any public statements about LQG?

Thanks
Dan

46. **Peter Woit**
September 12, 2006

Dan,
I don't know of any place Witten has made any public statements about LQG. At least in the past, rumors were that he wasn't very enthusiastic. No idea what he thinks now.

47. **dan**
September 12, 2006

Thanks. I think Witten's research interest is the single most influential decision into the direction of physics.

48. **woit**
September 13, 2006

dan,

Not always. For some reason, very few people are following Witten in the direction of his latest research interest, relating QFT and the geometric Langlands program.

49. **a**
September 13, 2006

in the above link, Witten writes: "And where critics have had good ideas, they have tended to be absorbed as part of string theory, whether it was black-hole entropy, the holographic principle of quantum gravity, noncommutative geometry, or twistor theory".

Did he forgot anthropism (e.g. Weinberg 1987)?

50. **stupid**
September 13, 2006

I notice that the first Google hit on the Geometric Landlands Program states prominently that it is partly funded by DARPA, the Defense Advanced Research Projects Agency:

http://www2.math.northwestern.edu/langlands/

Perhaps this military sponsorship is putting some people off following Witten?

51. **Benni**
September 13, 2006

Suesskind has told me in Munich, that "Witten hates the Landscape of String Theory because we cannot get predictions from it"

52. **Jeremy**
September 13, 2006

I find myself increasingly unhappy with Dr. Kaku the more I hear him speak in public as I get older. I knew how he was personally from a family connection and I can't mesh the personal with his large public profile.

After A Brief History of Time, one of my earliest popular physics books was Hyperspace, which I enjoyed very much as a child. However, after rereading it last year, I actually found it quite disappointing, filled with ephemera and run-through with torrid poetic descriptions.

I looked up his television appearances on YouTube about six weeks ago and just had to laugh at the situations he put himself in. From speculative fiction to futurist, his commentary was so filled with mayhap and possibility. More humour than anything else. I didn't understand why, with the kind of mind and content he's supposed to have, that's what he was coming up with.

And the "preliminary" version of his article very much got my head scratching. While obviously my opinion of him as a popularizer and scientist has changed over the last few years, I never once thought of him as mean. I can understand the need to respond to challenges, and the desire to scar opponents, and even those thoughts coming to the fore, but to actually write that down for publication and have it go online is disappointing to me.

53. **Alejandro Rivero**
September 13, 2006

De Broglie was also majored in history, wasn't it?

54. **TheGraduate**
September 13, 2006

Peter,

You said "very few people are following Witten in the direction of his latest research interest, relating QFT and the geometric Langlands program."

I was curious about the mathematical background of the string theory community. OF what does the typical mathematical education of a string theorist consist ? It seems as if they cover more ground than even most mathematicians.

55. **TheGraduate**
September 13, 2006

Let me rephrase my previous question a bit. Do most string theorists have the mathematical background to contribute to relating QFT and the geometric Langlands program?

56. **Peter Woit**
September 13, 2006

TheGraduate,

The mathematical background of string theorists varies widely, from no more than that of your average non-string theorist particle theorist to quite a lot more. But very few do have the background to work on the geometric Langlands stuff Witten is working on. To be fair, extremely few mathematicians themselves have the background necessary to follow the mathematics involved.

57. **Sebastian Thaler**
September 13, 2006

Peter-

You will probably enjoy Monday's edition of the comic strip THE FLYING MCCOYS:

http://news.yahoo.com/comics/uclickcomics/20060911/cx_fmc_uc/fmc20060911

58. **dan**
September 13, 2006

"in the above link, Witten writes: "And where critics have had good ideas, they have tended to be absorbed as part of string theory, whether it was black-hole entropy, the holographic principle of quantum gravity, noncommutative geometry, or twistor theory".

did he forget LQG or preon theory?

59. **A.J.**
September 13, 2006

Three comments in one:

One for dan,

did he forget LQG or preon theory?

I don't think Witten is unaware of the existence of these theories.

One for stupid:

People who don't like DARPA-funded projects probably should avoid using the internet.

One for Peter:

I enjoyed the book. Thanks! (I guess further commentary belongs on some other thread.)

60. **dan**
September 13, 2006

one for AJ "I don't think Witten is unaware of the existence of these theories. "

He hasn't embedded LQG or preon theory into string theory AFAIK.

I wonder if Witten's apparent ignoring of LQG/Preon theory is like Einstein's pursuit of unified field theory in a time when he ignored quantum field theory and the standard model was being formed.

Incidentally Peter, as a particle theorist, since strings is an obvious failure, how promising do you think preon models such as the Sundance model is, to explaining the standard model, possibly simplifying it, and then relating it to spin foam LQG?

http://arxiv.org/abs/hep-th/0603022

61. **A Babe in the Universe**
September 13, 2006

PW, do you think that the many universes proposed by "eternal inflation" relate to the landscape?

62. **Peter Woit**
September 13, 2006

dan,

Unlike the case of Einstein, in Witten's case there is no analog of the great advances in quantum theory and quantum field theory that Einstein was ignoring. Idea about preons, etc. are still extremely speculative, a long way from something solid.

Louise,

"Eternal inflation" is the mechanism that anthropic landscapeologists generally assume will populate the landscape.

63. **Gumbi**
September 14, 2006

Do you consider it a factual matter whether we live in an anthropically-selected landscape universe? That is, is it a question of fact whether the laws of physics which govern our universe have this nature, that there are very many or even infinitely many parallel universes, and we live in one that is compatible with our biology? Or can physicists choose the laws of physics, and if they find the landscape unpalatable or frustrating, they can choose different laws of physics for our universe to be governed by?

In other words, what if the landscape is true but unprovable, and physics as we know it has in fact reached its end? Is this possible, or can physicists prevent it from happening by their efforts and research?

64. **Peter Woit**
September 15, 2006

Gumbi,

It's true but unprovable that our universe may just be a simulation in a computer run by a higher intelligence, but speculating about this is no more science than the anthropic landscape. Until now the scientific method has done very well as a way to learn more and more about the universe in a reliable way, and I think will continue to work, although the difficulty of doing higher energy experiments does make things tougher. I certainly see no need to do what many of the landscape people are doing, abandoning the scientific method for no good reason.

65. **Jason**
September 16, 2006

Re: Trouble with Physics and Not even wrong.

I may have missed something, but I notice that unfortuntely Peter indexes Lee but Lee does not behave but reciprocally. Too bad especiallly from theorists who have in common their seeking attention outside the stringy mairnstream. How lame is that. Get it together, you guiys.

PS Not that there's anything wrong with lameness.

Yet More Links

September 13, 2006
Categories: Uncategorized

Frank Wilczek, besides his other accomplishments, is also the star of an opera, entitled Atom and Eve. More commentary on this from Betsy Devine, and Jennifer Ouellette, as well as a report here, and a review here.

Paul Cook has a report on the Templeton-sponsored panel discussion at Cambridge on The Nature of Space and Time.

In October 2004 the French magazine Ciel et Espace published an article about the Bogdanovs entitled The Bogdanov Mystification (English version here). They sued the magzine in December 2004 for defamation. Evidently a French court has now decided the case against the Bogdanovs, fining them 2500 Euros for frivolous litigation and requiring them to pay the magazine's costs.

The Institute for Advanced Study has been famous in recent years for the emphasis of its theoretical group on string theory. They seem to be moving a bit more towards phenomenology these days, and there will be a workshop on axions there next month.

The Tevatron is performing well, recently achieving new record luminosities. You can keep track of their progress here.

LBL will soon be hosting a conference to celebrate the 50th anniversary of the Particle Data Group.

Chad Orzel has a perceptive review of Not Even Wrong at his blog, Uncertain Principles.

Update: There's a tradition among bloggers of "carnivals", collections of the more interesting recent blog postings in a certain area. The physical sciences now have one of their own, the first edition is now available, and it's called Philosophia Naturalis.

Update: Note added about the Bogdanov court case, giving claims about this by Igor Bogdanoff.

Comments

1. **hack**
   September 13, 2006

   That's too bad about the Bogdanov brothers. Did Lubos testify on their behalf?

2. **Apropos**
   September 13, 2006

   BTW I have not seen Lubos posts lately, given the turmoil with ST is that a signal that they are abandoning their trenches and running for cover?.

3. **Geon**
   September 13, 2006

   Dr.Woit,
   I respect your view on current status of hep-th but do you not think maintaining this blog(and repetitive nature of articles) and publishing books for 'general public' is just 'not good enough'? If you despise string theory, are you working on any alternate theory? As long as you spend your day 'putting links on webpages' I don't think your opinion will be given much credit.

4. **TheGraduate**
   September 14, 2006

   Geon,

   I can't speak for anybody else but I find these links excellent. I am always hearing about things I wouldn't know about otherwise.

5. **Thomas Larsson**
   September 14, 2006

   The Tevatron is performing well, recently achieving new record luminosities. You can keep track of their progress here.

   I am under the impression that 2 fb^-1 is an important limit for the Tevatron – is this not the point where a light Higgs, and thus indirectly susy, could be ruled out at 95% CL (a 5-sigma discovery would take much more, though)? With 1.9 fb^-1 and counting, Tevatron data are starting to be really interesting.

6. **Geon**
   September 14, 2006

   Sure, I have nothing against useful links. But I guess my point to Dr.Woit was that convincing 100 lamers(via non technical argument with no single equation, referring to 'personal' matter, criticising string theory and yet he himself can only 'speculate' on alternatives) is not only easy but is pointless. In that regard I strongly think doing "something" even though it may not be 'real' physics is more productive.

7. **Alejandro Rivero**
   September 14, 2006

   Geon, it is a regret than the english language does not have two different pronouns for "you" in plural and singular situation, because if it had you cound have considered if it is meaningful to ask a particular person about a global issue. And I would say yes, collectivelly we are working towards other approaches and yes, Woit (and Smolin) sort of political trenches are part of the fight.

8. **Heineken**
   September 14, 2006

   i really have nothing constructive to say, but thought i'd take a moment to talk about the misappropriation of terms from philosophy and the arts. Lubos has called

Smolin’s viewpoint “postmodern”. now i have not read Smolin’s latest, but i am familiar with some of his nontechnical works including 3 Roads to Quantum Gravity. getting to the zero dimensional point, it is string theory which strikes me as postmodern, not any other alternative approach to quantum gravity. one main idea in postmodernism is a rejection or questioning of the rational approach e.g. the scientific method (post Enlightenment hegemony).

string theory, at least to a layman like myself, has become disconnected from experiment. this hardly strikes me as rational, Enlightened, or even scientific. Lubos (the master of puppets) states that Smolin’s ideas are postmodern, yet a theory that misses the mark by 55 orders of magnitude is not?

“Radical as the fundamentals of quantum mechanics were, it’s easy to overreach when applying them in nonscientific contexts. I find the most bothersome example to be the frequently abused uncertainty principle, which is often misappropriated to speciously justify inaccuracy.” (Warped Passages, L. Randall)

philosophers and layman get blasted when they misuse terms from science, yet when it’s the other way around …

i think i’ll order a copy of Alan Sokal’s new book “Fashionable Nonsense (Pulling Your Strings Since 1984)”. i enjoyed Dawkin’s review of it, entitled “Lubos Disrobed … And It Ain’t a Pretty Sight”.

Lumo, the matrix theory has you.

9. **Geon**
September 14, 2006

What has coming up with an alternative theory got to do with political fight?? I thought we are doing physics here?

10. **TheGraduate**
September 14, 2006

Geon,

Not every argument requires an equation to be valid.

11. **Santo D'Agostino**
September 14, 2006

Geon,

Criticizing a proposed theory is an essential part of the scientific enterprise.

Heineken,

Very nice point about postmodernism, and your last line was amusing.

All the best,
Santo

12. **Geon**
September 14, 2006

Santo D’Agostino and to others(Is Dr.Woit ignoring my comments?),
I have nothing against criticism on string theory. I myself don’t yet believe in string theory too. But! I would rather be ‘deeply unsatisfied string theorist’ rather than ‘complaining non-string theorist’ for the reasons I have described above. Heineken said “a theory that misses the mark by 55 orders of magnitude ” do you really think string theorist are all happy about this? I think what drives them despite these nonsensical results are 1. There are ‘indications’ which gives them a strong feeling that ‘something’ is going on with string theory(yes it might be just some miraculous coincidences or maybe not!), 2. Simply this is the most developed work in progress we have at the moment as far as quantum gravity is concerned. I don’t know how could someone be as ‘confident’ as Dr.Motl but still it ‘does’ produce some signs which especially at this time of the century when there is hardly any experimental lead, is all we can rely on. Getting to point, string theory ‘definitely’ has worthy to be studied+alternative ‘could’ be addressed but you can still do this with your mouth shut+publicly advertising such situation to ‘general public’ is not ‘physicists’ job but that of reporters.

13. **Peter Woit**
September 14, 2006

Geon,

I’m not ignoring your comments, I was waking up, having a long breakfast, during which I was starting to read a new book that just arrived (“Dirac Operators and Representation Theory”), which looks like it should be quite helpful in relation to ideas about BRST that I’ve been working on.

Sure, I’d rather be spending time making positive progress on alternatives to string theory than criticizing string theory. But for twenty years physicists who were well aware of the problems with string theory kept their mouths shut while particle theory was taken over by people endlessly repeating the same overhyped claims for string theory, including the ones that you are echoing. I think this has done a huge amount of damage to the field, and someone needs to point this out and give an accurate picture of what is going on here. This is not something that a reporter can do, it requires someone who knows the subject. I’d much rather it be someone else, and I hope to spend more of my time working on positive alternatives, but I’m not about to shut up, no matter how much string theorists would like that to happen.

14. **Geon**
September 14, 2006

I apologise for using ths strong language there I didn’t imply that to you if you know what I mean. I certainly admire your view and I think it’s as equally valid as any other string theorists view of this nature at this stage. In some sense I think you are more ‘concerned'(in a positive way) and true ‘physicist’ in the sense that your moral is to put mother nature first. Your book did have some positive effect on me personally and gave me an opportunity to think about what made me up until recently to be brainwashed about string theory. In that process I did realise that it wasn’t really due to my deep understanding of these subjects but rather due to flash campaign presented by Dr.Greene. Now, I think I am in the state of neutral, but certainly I’m not a critic like you simply because I don’t have much knowledge yet. But certain things that I pointed out above is really addressing different ‘attitude’ taken by you and by string theorists, and I think the latter are in a way ‘trying’ whereas I get impression that you focus bit too much on publicising this situation.

15. **comentator**
September 14, 2006

It should be mentioned also the brain drain; Taking enthusiastic graduates to study ST and shutting off other areas of study for alternatives . this point has been mentioned before and is really troublesome not to mention the publications wall.

16. **King Ray**
September 14, 2006

Einstein once said in his later years words to the effect that the feeling of conviction you have that a theory is correct has absolutely no correlation with whether it is actually true or not. I think the string theorists need to understand Einstein's comment, which came from his being totally convinced he was on the right track a number of times in his search for a unified field theory, only to realize that he was going in the wrong direction. Now we know he didn't have enough information. The string theorists refuse to accept that they are on the wrong track.

17. **Loopy**
September 14, 2006

I can't wait to see tomorrow's post about Gregg Easterbrook's article on Slate.

18. **SFB**
September 14, 2006

Geon et. al.,
string theory takes an enormous amount of money away from other approaches. This happens via suported graduated students, faculty positions, grants, etc. Many of the people in charge of such money are not experts in string theory, other approaches, nor the philosophy of science. They have to make decisions based, in part, on things like the zeitgeist. Therefore, it is only fair to popularly express the many doubts about string theory, as string theorists have popularly expressed the many hopes for it.

The brian drain is secondary in my view: these people would perform calculations within *whatever* approach was most popular at the time in their careers they have to show something.

None of the string theorists have been able to approach the real problem: the foundations of quantum mechanics. They should be exploring the mathematical concequences of quant-ph/0506228. The fact that they are working on string theory is a commentary on how superficial their thinking is: string theory relies on mathematical coincidences instead of principles that must be true. Of course, workers in other approaches are guilty of this too.

I realize that it *looks* like unification is essential to string theory. But I don't see why this might not be an artifact of counting the number of parameters of the theory in a conceptually anthropomorphic way. Is the number of dimensions of the theory a free parameter? Is the number of kinds of dimensions (compact, etc.) a free parameter? Is **each** dimension to be counted as a parameter??? Until there is a theory (in the philosophy of science) that removes anthropomorphic biases and is universally applicable that gives the number of parameters (free and bound) of a physical theory I believe no argument can be made that the dimensions are not each parameters– meaning that string theory might assume just as much as the assumption of the trivial union of GR and QFT in the first place. (I could do any of this myself, given the salary of any one person paid to work on string theory.) I have not even mentioned the "landscape".

I would like to see a return to common sense (as understood within physcis). See the paper mentioned above.

19. **Chris W.**
September 15, 2006

I just skimmed [Easterbrook's piece](). With friends like him, you don't need enemies. I would suggest ignoring him.

20. **Whoman**
September 17, 2006

Peter,
Talking about Tempelton… I thought you would be interested to know that fqxi.org has announced grant winners. The two top winners are Louis Crane (A New Approach To Quantum Gravity, With Possible Applications) and Steven Giddings (Observation And Non-Locality In Quantum Gravitational Physics).

21. **Shantanu**
September 17, 2006

Peter, sorry to change topics, but I am wondering if you looked at the videos
of talks at the string phenomenology workshop at Santa Barbara ?
any particular talks look interesting or such?

22. **woit**
September 18, 2006

Whoman,

I did see that announcement, wrote a bit about it here when it came out at the end of July.

Shantanu,

I have a pretty low opinion of the whole concept of "string phenomenology", but did look at some of the talks on the KITP website. Nothing I saw there changed my mind. That field continues to produce a lot of hype and nothing close to a real prediction.

Reviews in The Economist, Slate and the Times

September 15, 2006
Categories: Not Even Wrong: The Book

This week's issue of The Economist has a review of my book and Lee Smolin's, entitled All Strung Up. It's quite positive about the point of view on string theory that Smolin and I share, and correctly identifies where we see things differently about the role of mathematics. Nothing in it that will be news to readers of this blog.

Yesterday I also saw two reviews that I don't think much of. The first is Gregg Easterbrook's piece at Slate, The Trouble With String Theory. It's a very enthusiastic review of Smolin's book, and when I started reading it my initial reaction was positive, although it did seem a bit over the top. As I read on, besides wondering "Hey, is he going to mention my book too?", I started to remember who Easterbrook is, and how stupid some of his previous writings on physics were. By the end of it, I was very glad Easterbrook had left me out of it. One sometimes depressing aspect of being on this side of the string theory controversy is seeing who some of one's allies are.

Easterbrook is best known as a sports writer writing about the NFL, but for some reason various prominent publications feature his writing on other topics. The biggest mystery of all is why places like Slate and the New Republic have him writing about science, a topic he seems to know nothing about, and be actively hostile to. For once, Lubos Motl's paranoid rantings about "anti-science" people who dislike string theory do actually have someone they legitimately apply to. This latest Easterbrook effort isn't even original, he's plagiarizing himself, writing:

Today if a professor at Princeton claims there are 11 unobservable dimensions about which he can speak with great confidence despite an utter lack of supporting evidence, that professor is praised for incredible sophistication. If another person in the same place asserted there exists one unobservable dimension, the plane of the spirit, he would be hooted down as a superstitious crank.

which isn't very different than what he was writing in the New Republic three years ago:

Ten unobservable dimensions, an infinite number of invisible parallel universes–hey, why not?

Yet if at Yale, Princeton, Stanford, or top schools, you proposed that there exists just one unobservable dimension–the plane of the spirit–and that it is real despite our inability to sense it directly, you'd be laughed out of the room.

The second new review that I don't think much of is one that I got a copy of late last night (after a party held to celebrate the US publication of my book). It will appear this Sunday in the New York Times Book Review and is the first really hostile review of the book by a science writer that I've seen. I've been very pleasantly surprised by how positive the reviews of the book have been so far, since I initially expected much more of a mix of sympathetic and hostile ones. Most science journalists have seen years and years of string theory hype go by, with no progress towards any of the promises made for the theory ever actually being fulfilled, and this has left them with a more and more skeptical attitude towards the theory. The Times reviewer, Tom Siegfried, most recently wrote a book entitled Strange Matters: Undiscovered Ideas at the Frontiers of Space and Time, and somewhat earlier a book called The Bit and the Pendulum: From Quantum Computing to M-theory. Both books feature a breathless, gee-whiz, completely credulous take on the most speculative ideas around, thoroughly mixing science fiction and fact, with little interest in distinguishing the two. At the time I was writing my book, ones like Siegfried's were models for me of the opposite of what I was trying to do, so I'm not surprised he didn't much like what I wrote.

Unlike most authors who don't have any viable way of responding to reviews they consider unfair and misleading, it's all too easy for me to do so here, so a response to the review follows.

Siegfried complains that I use technical jargon, for example by discussing "perturbation expansions". While there are certainly places in the book that have some technical material in them that most people would be best advised to skip over, this isn't one of them. To understand anything at all about the current state of string theory, you need to have some idea about what a perturbation expansion is. This is carefully explained at one point in the book. It's not clear to me what Siegfried's point is. Does he not know what a perturbation expansion is? If so he shouldn't be writing or reviewing books on this subject. Does he think that the audience for this kind of book is not capable of following such an explanation? If so, he has a profound lack of respect for the people who read these books. As far as I can tell, they cover a very wide range of backgrounds, but most of them have had a good high school or college education, and many have taken a calculus class where they have been exposed to power series expansions, and I explicitly refer to this in my explanation.

One can write a book like this by refusing to try and explain anything that can't be explained to someone with only a grade school education, but that's not what I was doing. I don't think you can honestly communicate much about the current state of particle theory and string theory if you follow this tactic. My decision was to first see which topics I wanted to try and write about, then do my best to give an honest explanation in the simplest and clearest terms that I could manage. Some topics end up being pretty accessible to everyone, others do require significant background to understand and appreciate. I think most readers of the book will learn some things from it, while not understanding everything. But they won't go away from the book being fooled into thinking they understand something that they don't.

Siegfried's review is unremittingly hostile, with virtually everything he has to say about what is in the book a misleading and less than accurate characterization. According to him I allege that people only do string theory because Witten has "mesmerized" them, mainly use quotes that reflect what people thought 20 years ago, engage in irrelevancies about masturbation, etc. This last has to do with a quote from Gell-Mann (He used to say, "physics is to mathematics as sex is to masturbation", changed his mind after 1984) that I discuss because it reflects well the attitudes of particle theorists towards mathematics, and the relation between the two subjects is one of the central concerns of the book. This discussion may be tasteless, but it is not at all irrelevant to what I was writing about.

Siegfried claims that my central accusation is that string theory makes no predictions and that I am flat-out wrong about this. He writes:

*…string theory **does** make predictions — the existence of new supersymmetry particles, for instance, and extra dimensions of space beyond the familiar three of ordinary experience. These predictions are testable: evidence for both could be produced at the Large Hadron Collider, which is scheduled to begin operating next year near Geneva. These predictions are not of the specific quantitative kind that would definitively prove string theory true or false, but their confirmation would certainly be taken as impressive support.*

The fact of the matter is that string theory makes none of the "predictions" Siegfried has been led to believe by the hype about string theory that he seems to have swallowed whole. It predicts nothing about what extra dimensions might be visible at the LHC, not even their number. Similarly, it does not predict that superpartners will be visible at the LHC or what their properties will be. His use of the term "supersymmetry particles" indicates how little familiarity he has with the subject, while still feeling quite comfortable accusing me of getting this all wrong.

Siegfried is rather more kind to Smolin's book, but also manages to mischaracterize it, insisting that Smolin is not content with favorable evidence for string theory, but is demanding some much higher standard of definitive proof. He ends by comparing both of us unfavorably to Schwarz and other 1970s string theorists, noting that they didn't complain about the dominant research program in particle theory during their day. The problem with this argument is that the dominant research program was gauge theory and the standard model which, very much unlike string theory, had a huge and increasing amount of experimental evidence backing it up. If it hadn't had this, I strongly suspect that Schwarz and many others would have also been complaining, loudly.

Update: There's a short, but very well-done, review of the book in today's Guardian. Also a mention in the Toronto Star, where science journalist Jay Ingram describes how:

A few years ago, the occasional physicist would confide in me that string theory — the idea that matter is composed of super-tiny vibrating strings — would one day be seen to *be wrong, a big mistake*.

Update: The review in the Times is here.

Update: The Boston Globe has a quite positive review of my book and Smolin's here.

Update: More coverage of this in USA Today. This piece includes a quote from John Schwarz that experiments will verify string theory in the future, and implies this will happen at the LHC. Lubos has his trademark insightful commentary.

Comments

1. **andy**
   September 15, 2006

   Does Tom Siegfried have any science education? I mean, is he confusing himself as a science journalist with an actual scientist?

2. **optimist**
   September 15, 2006

   Hello, Dr. Woit,

   This is my first time to leave some message here. Although I strongly agree with your critics about superstring theory I would be rather optimistic in different direction.

   These days I am studying AdS/CFT to learn AdS/QCD which is interesting topic these days. In doing these I realized that AdS/CFT did give theoretic predictions which were not clear from field theory sides. Even though they are just theoretical consistency I think they give very good reason to study string theory.

   Maybe understaning quantum gravity is too early for us but thanks to string theory it could be true that we happen to have unexpected tools to investigate nonpertavative regime of gauge theory.

   If people insist that they are doing "theory of everything" with accepting anthropic approach then you can criticize them and I agree you. If people see string theory as a tool for a strongly coupled guage theory then I think you should be encourge them to do it especially if they are fresh graduate students. Understanding strongly coupled guage theory is very important and useful thing, I think. Too critical mood could be harmful even to this kind of important research.

   Thanks.

3. **Who**
   September 15, 2006

   Peter, just for reference here is a link
   to a free copy of the 11 August Science magazine article by Tom Siegfried you mentioned in an earlier post.

   http://susy06.physics.uci.edu/press/susy06_science_naturalness.pdf

   He was covering the SUSY 06 conference and called the piece "A Landscape Too Far?" IIRC you gave a link to the subscribers-only version of the article but not to this copy at the official conference website.

   Readers may remember the article has a special sidebox where Joe Polchinski recounts how he used to be skeptical of the Landscape but then "got religion".

   This article shows something of how Siegfried works as a science journalist. He is evidently on chummy terms with many eminent string people and chats easily with them—obtaining seemingly valuable spontaneous quotes that enable him to tell their story in a personalized "insider" way.

   I am not approving or disapproving—especially since I havent seen his NYTBR piece about the two books. I merely remark that Siegfried is a sympathetic trusted insider science journalist who could be effective as informal spokesperson for the string community.

   He must have a fair amount invested in his access to and relation with important stringfolk. Should be an excellent person to voice the community's interests, rebut it critics, and tell its story to the public.

   Here is the KITP bio
   http://www.kitp.ucsb.edu/community/ITPBios/Siegfried.html
   His MA in journalism at U Tex Austin had a Physics minor.
   This is in answer to andy's question about science education.

4. **Thomas Love**
   September 15, 2006

   Finally finished reading NEW over lunch today. While I agree with you about the errors of string theory, there is quite a bit I disagree with you on, especially the correctness of the standard model.

   As you discovered, "The enemy of your enemy is not necessarily your friend."

   Now on to Smolin's book!

5. **Tommaso Dorigo**
   September 15, 2006

   Hi Peter,

   this continuing claim that string theory predicts supersymmetry and large extra dimensions, which the LHC will be able to find, reminds me of the following sentence from Kathy Mansfield's "The Fly": "we cling to our last pleasures as the tree clings to its last leaves".

   I think the tree will be bare soon – Give the LHC a couple of years.

   Cheers,

T.

6. **PPCook**
September 15, 2006

Hi Peter,

“Physics is to math what sex is to masturbation.”

I thought this excellent quote was due to Feynman.

Best wishes,
Paul

7. **woit**
September 15, 2006

Who,

The first entry for “Tom Siegfried” under Google is his listing as a journalism fellow at the KITP in Santa Barbara. I guess that’s where he learned about “supersymmetry particles” and all the string theory predictions for the LHC.

PPCook,

Gell-Mann does explicitly claim priority on this, although many internet sources list Feynman. It is only one of many discoveries the two of them have fought over credit for…

optimist,

I agree with you that AdS/QCD is interesting and that string theory is a promising approach to studying strongly coupled gauge theories. But I don’t think there’s the slightest danger that too few theorists are working on this subject.

8. **Carl**
September 15, 2006

Siegrfried complaining about talk of arcana such as “perturbations” misses the point. When one of my relatives asked for a recommendation, I sent them to Smolin’s book because it is written for a more general audience (and will probably sell a lot more copies).

9. **CapitalistImperialistPig**
September 16, 2006

Peter,

I think that if evidence for a sparticle was found at the LHC a lot of people, myself included, would count it as suggestive of the value string theory. Lubos is apparently confident enough of SUSY at the LHC that he has put a dime ($1000) on it. At least he puts his money where his mouth is!

Ditto if evidence for any extra dimensions, never mind how many or what size is found.

Either of these discoveries ought to point the way to new physics, but string theory would be an obvious first resort.

I’m jus sayin…

10. **Dan**
September 16, 2006

CapitalistImperialistPig I agree with what you say, if LHC finds either SUSY or extra dimensions then interest in string theory will continue to mount, and other approaches will either dry up or attempt to merge with string theory.

However, what-if,
what if LHC finds neither the higgs boson nor SUSY nor higher dimensions? What would happen to physics and string theory?

My prediction:

Non-SUSY 4D LQG will continue to languish, and interest in String theory will continue unabated.

11. **A**
September 16, 2006

if SUSY is there, LHC would see some new hadrons, new leptons, new higgses, but telling if they are SUSY would be difficult. E.g. LHC can measure some of their masses, but SUSY does not predict any mass. SUSY predicts couplings, telling that sparticles decays are too fast to be measured.

About extra dimensions at LHC: Dorigo could bet 100000$ against them, and likely no physicists would accept the bet. Extra dimensions are considered too unlikely.

12. **Nathan Myers**
September 16, 2006

Who really believes Lubos would pay if LHC results came out against him? If you do, offer to hold the money in escrow, before the experiment is performed, and see what happens.

13. **gambler**
September 16, 2006

Dudes,

Betting is *the* way to measure how firmly somebody is convinced of something. Not long ago in England people would talk about those who lacked the “courage of their convictions", where "conviction" is what you have if you're convinced of something.

Is there a web site where people can place wagers about things like sparticles? String theorists and string-theory critics could each make a lot of money, and humiliate their enemies, if only they're right. Although, maybe the string theorists would chicken out because maybe string theory is right even though supersymmetry will never be seen. Maybe string theory is wrong even if the sparticles are found, but I don't think so …. supersymmetry is one thing as a mathematical feature of a qft; it's quite another thing to say that it's a part of "the universe"; the second statement is a religious one, part of the same religion as strng theory.

Nathan is right, though. Escrow is what we need, and a Trusted Third Party. There might be legal problems if it's in the United States, but there are places in Europe and maybe in Vegas where it should be legal. I think this is something to push for – raise the stakes and then raise them further – checking for susy at LHC is the closest thing to a test of string theory we'll see in our lifetimes. Let's make it as spectacular as possible. After LHC, we'll all be fired anyway.

14. **Scott Aaronson**
September 16, 2006

Go to http://www.ideosphere.com (unfortunately, it doesn't let you bet with real money)

Robin Hanson is a major proponent of betting on scientific questions, and has lots of writings and links on the subject.

15. **PPCook**
September 16, 2006

Hi Peter,

Thanks for the information about the quotation. I didn't realise they also disputed quotations.

Best wishes,
Paul

16. **CapitalistImperialistPig**
September 16, 2006

A,

I don't know how firm the evidence is on this, but my understanding is that the lightest supersymmetric partner is supposed to be stable, as it must be if it is to account for, say, dark matter. No such discovery would "prove" string theory, but it would be a potent clue that ought to lead somewhere – maybe even to ST, which is the first place everyone would look.

Betting may give us insight in to peoples strength of conviction, but it doesn't give any into their soundness of judgement! Out of respect for Peter's policies I will avoid mentioning any of the examples that occur to me.

17. **Gina**
September 16, 2006

A week ago I wrote, especially for this blog, a critical pre-review of Peters book "not even wrong". Among other things, it contained a clever analogy between string theory as a theory of everything and mathematical logic which is the mathematical theory of everything and a critical assessment of Peter's ideas on sociology and funding of science ending with the strong statement: "As there is no such thing as a riskless risk, Woit's ideas on this front may deserve the title 'not even wrong'".

I explained why, in my opinion, Peter does not really have a "case" against string theory.

Since the pre-review was not ready in time to be included in the comments following the post I aimed at (one categorized by "not even wrong – the book"), I posted it in a later and unrelated post. There it lasted 5-6 hours before being deleted. It was read by at least two people (or entities), the "renormalizer" who suggested to delete it and Peter who deleted it. In spite of its short time under the sun I was satisfied with my effort and outcome, but in a world where some are trying to find an ever-lasting theory of everything which will reduce later physics to just filling the details, and a few are trying to prove that two decades of efforts of thousands string theorists amount to zero if not less, my short lived pre-review cannot be considered as highly ambitious nor as a success.

I thought of resubmitting my clever yet ill-fated pre-review, but now, my mood is different. Rather than reviewing the matter at hand let me make a few remarks how can, in my humble opinion, the overall nice book by Peter (after all it is justa book not a "case"), be made nicer.

Most of the chapters of the book are very good. I think this is a very good popularization of particle physics all the way to the "standard model". Popularization of science is a tricky business and deserves a whole separate discussion. There is no way to avoid some "cheating" but one should still try to be honest, useful and non-manipulative and Peter does a good job. The description of the connections with mathematics are especially good. The story about the Seiberg-Witten discovery is told very vividly. With the exception of too strong rhetorics most chapters on string theory are also well-written. I am learning a lot reading the book. Thank you, Peter!

What could make this book nicer?: I can see the temptation to include the Bogdanov brothers story (and to mention the (overplayed) Sokel's hoax), and Peter had a personal record to set for this case. But overall this story does not belong to this book. The story about the string theory guy who became a Maharishi scientist also does not belong here. The same goes for the refereeing process for Cambridge University Press. I have quite a few scientists friends, and complaining about referee reports is one of the few drawbacks in their sweet lives. Beside, they are the referees themselves! (and they also complain about the burden of refereeing.) The rhetorics against string theory, and string theorists, as Peter himself noted (p.225, l. -5) is indeed too strong. This does not add to but rather reduce the value of the book.

The concluding chapter starts with a beautiful quote from Bob Dylan's song "Absolutely sweet Marie" – "But to live outside the law, you must be honest". When I saw it I thought that this is a self reference and that Peter set a standard for himself: If you want single handedly, coming from the outside, to claim that one of the hottest scientific area of our time failed and the efforts of thousands of string theorists worthless, you better be honest about the details, presentation and even your own motives.(And overall Peter is indeed quite honest.) Peter's intention quoting Dylan was different as he referred not to himself but to string theorists — that without empirical support to their theory should be honest. It is good to remember that nobody is or can be completely honest. So in view of Dylan's quote, the laws and traditions of conducting science as well as debating science are better respected.

And Dylan's cryptic line from "Love Minus Zero/No Limit" also comes to mind:
" There's no success like failure, and failure's no success at all."

18. **TheGraduate**
September 16, 2006

Gina:

I was another entity that read your review. I think it contained a mistake of perspective that I think is also contained in your current post. In both, you consider Peter's book a popularization of string theory. As far as I understand Peter's intentions, it is not. It is an argument against string theory being the "only game in town".

On a side note, why do you keep saying how clever your review was?

19. **Peter Woit**
September 16, 2006

CIP,
One can certainly reasonably claim that the discovery of supersymmetry at the LHC would provide some encouragement to the research direction that has led people to superstring theory, but that's a far cry from claiming that superstring theory "predicts" that evidence for supersymmetry will be found at the LHC. As for superstring theory "predicting" that the LHC will see evidence for extra dimensions, that's just complete nonsense. Some of the fanatics like Jacques and Lubos are willing to put some money down on supersymmetry at the LHC (it will be interesting to see if they pay off), but I don't know anyone willing to put money down on the LHC seeing extra dimensions.

Gina,

Your earlier comment was deleted because you posted it in a completely inappropriate place. As for this one, obviously I disagree with you about the relevance of some chapters in the book to my argument. In particular, the refereeing story at Cambridge was a very unusual one, involving two referees who strongly backed publication, and two string theory partisans trying (successfully) to stop Cambridge from publishing the book, while lacking any arguments against its content. Given that one of the main reactions from string theorists to the book has been that "string theory has won legitimately in the marketplace of ideas", I think it is important to explain how this marketplace sometimes operates.

I should perhaps have made more explicit what I meant to convey with the Dylan quote. It's not specifically about me or about string theorists, but about the situation particle physics finds itself in. Lacking the discipline enforced by experiment, theorists now need to be a lot more self-critical and honest in evaluating the results of the speculative work they are engaged in.

20. **John A**
September 16, 2006

> One sometimes depressing aspect of being on this side of the string theory controversy is seeing who some of one's allies are.

It's not just a problem in theoretical physics 🙁

21. **John A**
September 16, 2006

The only big result I expect to see from the LHC is conformation of the Higgs Boson. Anything else is a bonus.

22. **A**
September 16, 2006

CIP: one difficulty in distinguishing supersymmetry from alternative theories, is that almost all of them have a stable neutral particle, because all theorists want to explain dark matter. "Dark matter at LHC" is the safest bet. If dark matter will be found, understanding why it is stable might lead to fundamental progress.

23. **Gina**
September 16, 2006

TheGraduate said: "I was another entity that read your review. I think it contained a mistake of perspective that I think is also contained in your current post. In both, you consider Peter's book a popularization of string theory. As far as I understand Peter's intentions, it is not. It is an argument against string theory being the 'only game in town' ".

The Graduate: I think I was quite accurate. The first half of the book is indeed a popularization of particle physics all the way to the "standard model". The second part contains a popularization of string theory plus an argument against it. This argument refers to string theory "stand alone" as well as to the aspect of string theory being "the only game in town".

Peter: My main (mild) critique of today was not about irrelevancy but that the items I mentioned (and a few others) reduce the quality of your book. Of course, a book may have many qualities and I am mainly referring to the quality of the book as a serious discussion and debate of science (of the kind appropriate to a university press). For example, if you refer to the string theory community as a "mafia" this statement is, of course, highly relevant, but making such a statement reduce the value of the book, at least in my opinion. I think you are wrong to consider your experience with Cambridge university press as very unusual. Many authors had similar experiences even with much less controversial (and more important) books and papers. (This is an empirical issue that, in principle, can be tested.)

I think the story of the Maharishi string theorist is irrelevant. You do discuss a little in a straight way the comparison between string theory and the occult. This is fine. But on top of it you add further insinuations and stories towards such a comparison and, again, this reduce the quality of your book. Overall, it is a nice aspect of your book that you are not "part of the story" and the few places where you add yourself to the story, e.g., the Bogdanov e-mail, seem somewhat artificial.

"… theorists now need to be a lot more self-critical and honest"

Yeah, we all need to.

24. **D R Lunsford**
September 16, 2006

It's a shame the Times has fallen so far as to not be able to field a single good science writer (Overbye included). *Are* there any good science writers?

-drl

25. **TheGraduate**
September 16, 2006

Gina,

I haven't read the book yet so my objection was a limited one. I just thought that you were critiquing a book that Peter didn't write. I know it's not the book he intended to write BUT I do not know for sure it is NOT the book he wrote. Unless, it was stated in the preface, introduction and on the dust jacket that it wasn't a popularization, I think it is perhaps a fair assumption that the average reader would consider any review of particle physics contained within the book as a popularization.

Hopefully if the book makes it to second edition, all the stuff that people are pointing out are things that are going to go into the book. It does appear from the objections I've seen people making that the book needs a little more framing so people know why certain things have been included.

I think in the pantheon of string theory books, it is obviously to be read after some of the others. It really makes no sense to read 'Not Even Wrong' if there had never been any such books as "The Elegant Universe" and "The Fabric of the Cosmos".

26. **Chris Oakley**
September 16, 2006

TheGraduate says:

> I think in the pantheon of string theory books, it is obviously to be read after some of the others. It really makes no sense to read 'Not Even Wrong' if there had never been any such books as "The Elegant Universe" and "The Fabric of the Cosmos".

The purpose of *Not Even Wrong* is to point out that something (string theory) commonly regarded as a success is actually, as a scientific theory, a failure. Popular books on the subject, such as Greene's, are only one aspect of this.

27. **woit**
September 16, 2006

Gina and the Graduate,

I did not refer to string theorists in the book as a "mafia", I said that some of the people who wrote to me did so. It is a fact that many people in the physics community feel this way and I was reporting this. It's not a word I would use to describe my own perception of string theorist's behavior.

All the things that Gina objects to are things that I was reporting that are factually accurate. I didn't do much "framing" because I think the world is a very complex place, and there are many possible lessons to be learned from phenomena like Hagelin, the Bogdanovs, or my experiences with Cambridge. I explained a bit about some of the lessons that I personally drew from these stories, but everyone can make up his or her own mind. Aaron Bergman felt that I was making unspoken implications, but that's really not the case. If I have something critical to say about anyone, I say it explicitly and clearly.

At various times while writing the book I thought that I should shade what I was saying to make it more palatable to others, for instance to ensure that referees would agree to let me publish it. In the end I decided this was a bad idea, that I should put down on paper exactly what I thought. For better or worse, that's what's in the book. It is in no way at all a political document.

28. **TheGraduate**
September 16, 2006

Peter,

Thanks for the clarification.

29. **Garrett**
September 16, 2006

I recently bet two young string theorists that superparticles wouldn't be found by the LHC.

The more reasonable of the two thought the odds would be around 50%, because much recent theory involves supersymmetry breaking at an inaccessible energy scale. So it looks (from an unenlightened viewpoint) like string theorists are aware of the threat to their models imposed by the LHC and are back peddling further away from prediction of observables superparticles.

I encourage others to take up bets like this one. Understanding and, consequently, prediction is what physics is supposed to be about. And bets are fun.

30. **dan**
September 17, 2006

Dear Peter,
It seems clear that should LHC find any SUSY, it will be a big boost for string theory, but if LHC does NOT find SUSY, do you think interest in string theory will wane, or do you think it will continue unabated?

Curious
Dan

31. **A**
September 17, 2006

let me try to answer to dan: if LHC finds nothing new, somebody will try the string-anthropic landscape, but bigger changes will happen: somebody will leave the field, somebody will move to neutrinos, cosmology, astropysics, most collider experimentalists will shift to astroparticles, laboratories such as CERN could survive studying neutrinos or flavor.

We do not need bets to make LHC results interesting.

32. **Garrett**
September 17, 2006

I'd like to see an interesting Higgs sector.
(But, err, this is getting off topic.)

33. **Thomas Larsson**
September 17, 2006

It seems clear that should LHC find any SUSY, it will be a big boost for string theory, but if LHC does NOT find SUSY, do you think interest in string theory will wane, or do you think it will continue unabated?

Dan, you have sort of given the answer yourself: [Strings should continue to funded & research, at least at present levels if not more, should LHC discover evidence of SUSY particles or extradimensions or both. — obviously, should LHC fail to find such particles then the answer would be no.].

34. **Fabien Besnard**
September 17, 2006

Speaking about bets : surely no one would accept to play a game where, if X happen you lose 1 $ and if X does not happen you don't gain anything. This is precisely the sort of game that some string theorists would like others to play : if susy or extra dim are seen at LHC, string theory wins, and other theories lose, but if they are not seen string theory doesn't lose and others do not win ! One can certainly conjecture about loss or gain of confidence, but since there is no quantitative prediction of the needed energy level to see such phenomena, this won't come any close to a Popperian falsification procedure.

35. **dan**
September 17, 2006

Hi THomas,
I was wondering if Peter agreed with what I thought — and what should happen and what Peter thinks might happen are two seperate issues, I think what SHOULD happen is that not finding SUSY at LHC SHOULD be grounds for decline interest in strings. Whether that does happen, given the dominance of string theory in academia seems unlikely.

36. **Gina**
September 17, 2006

Peter,

It is a crucial quality for a book to reflect the author's thoughts and point of view so in this respect I do not have any argument with your choices on what to include in the book, even if my taste is different.

I think that the parts of the book with non technical explanations of physics and mathematics related to the standard model and further to string theory are very good and could serve as platforms for several alternative books. It could have been (and still can be) a good platform for a scholarly discussion and critical evaluation of string theory from the point of view of history, philosophy and sociology of science which would be suitable for a university press publication.

The path that you took makes the book perfectly appealing for a commercial publisher but indeed not appropriate for a university press publication. Part of it is the rhetoric and selection of issues to discuss, and part of it is the clarity and strength of your overall argument.

Concerning the small issue of using the word "mafia". Indeed it is a quote from somebody else, but the discussion in the following sentences gives the impression that you endorse what is behind this term if not the term itself. This I find unfortunate.

37. **dan**
September 17, 2006

Hello A,
Given the cost of LHC, somwhere north of $8 Billion US $, if LHC does not find nontrivial new physics, it's hard to imagine the urge to build even more expensive colliders. In the US, the SCSC was cancelled. I do wonder, if the only new physics LHC discovers is the higgs boson, at a price tag of $8 billion, I cannot imagine that even more expensive particle accelerators will be built to achieve higher luminosities. (If LHC sees lots of new interesting and nontrivial physics, then I could see the push to build even larger, more expensive particle accelerators).

As you suggest, a null result would probably result in the shift might be to astrophysical particles and neutrinos. In the event of a null result, what I would like to see is particle physics taking non-SUSY, non-string approaches, such as preon theories.

For me, a null result would be strong experimental support for Woit's NEW thesis, that string theory is probably the wrong approach, and is taking resources away from perhaps more promosing approach. There are theoretical problems with SUSY-MSSM, and should LHC not detect SUSY, I think continued dominance of string theory at the expense of alternative approaches would be experimenally unjustified.

38. **woit**
September 17, 2006

dan,

I'm not very good at prediction of what will happen to string theory. Personally, several years ago I would never have predicted that serious scientists would keep working on string theory after they accepted the existence of the landscape.

My best guess is that string theorists will keep doing string theory no matter what unless another bandwagon starts up for them to join. This might come about because of an exciting unexpected LHC result, because Witten comes up with a promising non-string theory idea, or some other reason. If the LHC doesn't turn up supersymmetry or extra dimensions (which I think is very likely), string theorists will concentrate more on black holes and cosmology (this has already been happening). As they do this, they'll slowly lose the support of their colleagues in other physics subfields, their funding will get cut, and, to a large extent, they'll take the whole field of theoretical particle physics slowly down with them.

39. **MoveOnOrStayBehind**
September 17, 2006

"As they do this, they'll slowly lose the support of their colleagues in other physics subfields, their funding will get cut, and, to a large extent, they'll take the whole field of theoretical particle physics slowly down with them."

That's what you dream about, isn't it. But dream on.

40. **woit**
September 17, 2006

MoveOn,

No, it's not my dream, quite the opposite. It's just an extrapolation from what is already going on, which is sad and depressing to watch.

41. **Gina**
September 17, 2006

"My best guess is that string theorists will keep doing string theory no matter what unless another bandwagon starts up for them to join. This might come about because of an exciting unexpected LHC result, because Witten comes up with a promising non-string theory idea, or some other reason. If the LHC doesn't turn up supersymmetry or extra dimensions (which I think is very likely), string theorists will concentrate more on black holes and cosmology (this has already been happening). As they do this, they'll slowly lose the support of their colleagues in other physics subfields, their funding will get cut, and, to a large extent, they'll take the whole field of theoretical particle physics slowly down with them."

This is an excellent generic guess for any prominent theory:

My best guess is that X-theorists will keep doing X-theory no matter what unless another bandwagon starts up for them to join. This might come about because of an exciting unexpected EMPIRICAL result, because some prominent X-theorists comes up with a promising non X-theory idea, or some other reason.

If the EMPIRICAL support (or another exciting thing) doesn't turn up (which I think is very likely), X- theorists will concentrate more on possible application to theory Y (this has already been happening). As they do this, they'll slowly lose the support of their colleagues in other subfields of their sup-sup field , their funding will get cut, and, to a large extent, they'll take the whole sup-field slowly down with them.

42. **woit**
September 17, 2006

Gina,

Yes, but in the generic case, a real possiblity is that "X-theory will achieve one or more of its major goals, making it a solid and permanent part of science, opening up new areas to work on that build on this success". That's not at all in the cards in this case…

43. **Dan**
September 17, 2006

Since thus far there is no experimental support for proton decay, a generic feature of all known GUT models, do you think theoretical particle physics has been dragged down by GUT hopes?

Curious
Dan

44. **woit**
September 17, 2006

Dan,

Lee Smolin has an interesting anecdote about a discussion with Eddie Farhi, who told him he left particle physics after proton decay predicted by GUTs was not observed, figuring there was no way to get experimental info about unification, thus one shouldn't work on it.

Supersymmetry GUTs push the lifetime up, but even they may be in trouble with experiment soon.

Personally, GUTs always bothered me, because they don't solve the main problem of the standard model, the Higgs sector, just making it worse by having to introduce more Higgs fields to break the GUT symmetry. They do have some interesting features, e.g. all particles fitting into one SO(10) rep, and coupling constant near unification, but I think one definitely needs some quite different ideas to make anything like a GUT really work out.

45. **Renormalized**
September 17, 2006

Gina- Do you always just regurgitate what others have written? I can't see you are adding anything to any discussion you have been involved with. Your responses are more in line with a common online troll.

46. **Dan**
September 17, 2006

Dear Peter,

Thanks for responding. Sounds like Eddie Farh had the right idea. In someways GUT's foreshadow strings.

"Supersymmetry GUTs push the lifetime up, but even they may be in trouble with experiment soon."

Kamiokande proton decay and LHC together can give four scenarios regarding SUSY and GUT and string theory.

Obviously, the experimental scenario that would most favor your NEW would be LHC fails to find SUSY, proton decay is not observed and rules out all SUSY-GUT's at 99.7% confidence. You and Smolin would be "heros", although I'm unsure whether string theory will continue its dominance in academia.

If Kamiokande failure to proton decay rules out even SUSY-GUT's, would that be enough to falisfy the string theory unification project?

It would be pretty exciting if LHC discovers SUSY, but Kamiokande rules out all forms of SUSY-GUT (as the proton does not decay), or Kamiokande discovers proton decay consistent with SUSY-GUT, but LHC fails to discover SUSY.

If LHC discovers SUSY and Kamiokande discovers proton decay consistent with SUSY-GUT predicted proton lifetimes, string theory will continue its dominance as SUSY-GUT can be embedded in the string framework. Lubos and Jaques believe this will be an outcome, Lubos is willing to bet money, and I think if it is the outcome, I think their confidence in strings will have some experimental support.

In this scenario, I can't imagine either your NEW or Smolin's Trouble gaining any traction whatsoever.

47. **Yatima**
September 17, 2006

Dan says:

> You (Woit) and Smolin would be "heros"

Why should that be so? A hearty expression of frustration that turns out to be justified when the brick wall is finally reached does not a 'hero' make – and I do not think it is the authors' intention to acceed to hero status. 'Hero' would be the research group that comes up with the Next Good Thing, whatever that is.

> I think if it (SUSY + proton decay consistent with GUT) is the outcome, I think their confidence in strings will have some experimental support.

Hmm… how much experimental support would that actually be (I am unable to judge this). If you succeed in circumnavigating the earth, your conjecture that the earth is round, hollow and has big holes at the poles has some experimental support, but you still need to check those poles. Now if these are too difficult to reach, what then?

Btw. are preons still being worked on?

48. **jeremy**
September 17, 2006

Yatima write:

"Why should that be so? A hearty expression of frustration that turns out to be justified when the brick wall is finally reached does not a 'hero' make – and I do not think it is the authors' intention to acceed to hero status. 'Hero' would be the research group that comes up with the Next Good Thing, whatever that is."

To express such frustration so publicly, to challenge the status quo so vigorously, and to argue for alternatives so heartily is courageous and heroic. Who knows, the hero "that comes up with the Next Good Thing" may only become hero after being inspired by NEW.

49. **LDM**
September 17, 2006

Regarding the Siegfried review…it is pretty obvious he doesn't really understand what he is writing about…which I find irresponsible, since he must certainly realize that some people might make a decision to purchase or not based on his review.

But I suppose it is a two-edged sword, since I now would never consider buying or even recommend to anyone his upcoming book…

50. **Gina**
September 17, 2006

"That's not at all in the cards in this case…"

Wow, so you have them cards, Peter! Boy, we have a lot of questions to ask you…

51. **Gina**
September 17, 2006

"Who knows, the hero 'that comes up with the Next Good Thing' may only become hero after being inspired by NEW."

A better bet yet is that she will be a string-theorist.

52. **Gina**
September 17, 2006

And here is my own last-week review.

"NOT EVEN WRONG", by Peter Woit; A review

(Replies and comments are very much welcomed, also you ReNorm)

Peter Woit is wrong claiming that "string theory" is "not even wrong". It is questionable if the distinction between "right", "wrong" and "not even wrong" coined by Wolfgang Pauli should be taken as a serious way to classify scientific theories (and even if this is the correct translation to English of Pauli's terms). It is a nice gimmick, though, and a great name for the book. In any case, the insights and truths offered by string theory, one of the most daring intellectual endeavors of our time, may well be wrong. They may also prevail as an important and unique part of physics. We cannot tell which way string theory will go.

"Landscape", the possibility of a huge number of theories that we may never be able to choose between, may be an artifact of string theory itself, or just of string theory in its present form. But it can also be an "impossibility result" which reveal a genuine problem with our ability to describe physical reality at some scales, if we like it or not.

Whether string theory will prevail or not, it seems hard to argue that string theory contributed important insights and technical infrastructure to mathematics and to physics.

It is important (also for string theorists; at least on weekends) to be aware, that even if "string theory" is a "theory of everything", string theory is not "everything". In mathematics we see many examples of such a distinction. Mathematical logic is a "mathematical theory of everything" that was developed in order to understand the foundations of mathematics. But while mathematical logic formally includes all other mathematics, in reality it is a beautiful field which is one out of many fields of mathematics and, as a matter of fact, a rather separate field. It took many decades before important links between mathematical logic and other mathematical disciplines were found.

There is no a priori advantage or importance to the theory which study the most fundamental and general rules. (In my own mind, relevance is of key importance which is why from all sciences my own heart goes to Chemistry.) But there is no a priori disadvantage either.

Peter Woit is a scholar. Modern science and academic life do not give sufficient incentives for true scholarship and large parts of the book exhibit both genuine scholarship as well as Woit's gifted ability to present and discuss in non-technical terms complicated mathematics and physics.
Another advantage of the book is that Woit does not offer alternatives of his own to string theory. Woit does present a few nice ideas and observations that deserve to be pursued,

When it comes to string theory, Woit has concerns (some shared by string theorists), complaints (a few justified), suspicions, and unrealistic expectations (like everybody else); but Woit does not have a case.

Recommendation: For a layman wanting to read about string theory I would recommend Brian Greene's "The Elegant Universe" over Woit's new book. An intelligent reader should use plenty grains of salt to any new scientific theory and any popular book describing it. Woit's "case against string theory" may be of some interest to string theorists and other theoretical physicists. I suspect that this is the audience Woit really wants to address.

As I explained elswhere, some of the choices Woit has made as for what to include as well as some of his rhetoric are disappointing.

I am also not happy with Woit's analysis of sociology, politics and funding of science. For a scientist, trying to explore something completely new (e.g., a replacement for string theory,) is a very very risky business. Woit aims at a
system which allows scientists to take riskless risks. As there is no such thing as a riskless risk, Woit's ideas on this front may deserve the title "not even wrong".

Gina

(And thank you Ebgert for the analogy with mathematical logic.)

53. **Gina**
September 17, 2006

Gina–do you have a background or degree in physics?

54. **TheGraduate**
September 17, 2006

Gina,

One thing I've disliked about many of the reviews so far, including yours, is that they spend a lot of time arguing the string theory case for the string theorists instead of reviewing the actual book.

I just don't understand how it makes sense to review a book with a polemic about why it makes sense to continue doing some theory that the book is critiquing.

I would have thought the more sensible thing to do (in a review) is to analyze the author's main points and outline:

1. The facts that the author states which are incorrect

2. The deductions the author makes with correct facts but incorrect logic

3. Correct deductions made by the author with correct facts which are nevertheless constitute an incomplete analysis

By doing this, one gets at the quality of the author's arguments.

In that sense, my feedback to your review would be to be more specific and support your points … but that is my liberal arts education coming out …

As your opinion, I guess it's fine. There is only so much one can put into a random post on a blog. But as written, your point of view seems unsubstantiated.

55. **TheGraduate**
September 17, 2006

Gina,

I normally wouldn't have commented on your review but you did ask for commentary.

56. **woit**
September 18, 2006

Gina,

Please stop submitting repetitive and non-substantive comments here, as well as ones that show little understanding of the issues involved. I'm happy to debate people with a serious background in string theory who want to discuss the arguments in my book, but you're just wasting my time.

57. **Thomas Larsson**
September 18, 2006

will continue its dominance as SUSY-GUT can be embedded in the string framework. Lubos and Jaques believe this will be an outcome, Lubos is willing to bet money,

Of course, this also means that somebody else is willing to bet money against them – in Distler's case, somebody with inside information from the Tevatron. Also note that Motl's original bet involved the date 2006. Depending on the exact conditions and his success in renegotiating them, he may already have lost his bet in less than four months.

58. **Dan**
September 18, 2006

Dear Mr. Larsson, I've not heard of someone with inside information from TEV betting against Distler on SUSY. Could you elaborate?

Peter, when you say that string theory cannot be falsified, stringy unification theories are built on top of SUSY-GUT, which predict proton decay half-times. If the current null result from Kakimone continues, then at some point in the future SUSY-GUT's can be ruled out by experiment as SU(5) have been, hence string theory unifcation scenarios built on SUSY-GUT's would be experimentally falsified. (stringy unification scenarios is the one reason to take string theory seriously).

59. **Thomas Larsson**
September 18, 2006

Here.

Of course, nobody knows the outcome of future experiments for sure, but I would take Dorigo in 2006 a lot more seriously than Motl in 2001. Not only because he is in the loop, but also because he formulated his bet after five more years of experiments yielding null results for susy (Tevatron, SuperK, PEDM, muon g-2, …).

60. **Renormalized**
September 18, 2006

"I am also not happy with Woit's analysis of sociology, politics and funding of science. For a scientist, trying to explore something completely new (e.g., a replacement for string theory,) is a very very risky business. Woit aims at a system which allows scientists to take riskless risks. As there is no such thing as a riskless risk, Woit's ideas on this front may deserve the title "not even wrong". "

This shows a total lack of understanding or insight into what Peter has said from the beginning. He is not asking for a riskless risk (I feel dumb even copying riskless risk), He is asking for strings to be tied to reality in even the smallest way.

61. **Thomas Larsson**
September 18, 2006

If the current null result from Kakimone continues, then at some point in the future SUSY-GUT's can be ruled out by experiment as SU(5) have been, hence string theory unifcation scenarios built on SUSY-GUT's would be experimentally falsified.

Can you be more specific about when this is supposed to happen?

In hep-ph/0005095 Pati wrote on p 4:

"strongly suggests that discovery of proton decay should be around the corner. In fact, one expects that at least candidate events should be observed in the near future already at SuperK."

This was written in May 2000. Is September 2006 still in the near future from that?

62. **Steve Myers**
September 18, 2006

Peter,
I read the review in the NYTimes. Yes, sounds like the guy has an agenda & knows little math-physics. But a point I made months asgo : reading reviews, for an author, is not worth the time. Best criticism comes from editors & trusted friends in the field.
Another general point: if someone's grounds for doing or believing something is not based on reason, you can't reason them out of it.

63. **Peter Woit**
September 18, 2006

Dan,

My impression is that you can get a fairly wide range of predictions of the proton lifetime out of different supersymmetric GUT models, so unfortunately experiments won't be able to rule out the whole class of such models anytime soon.

LDM and Steve,

I'm not very interested in criticism from Siegfried, his own books are models of what I think is wrong with popular books on physics, and he seems to understand very little about this subject. His arguments in his review don't make much sense, they seem to be purely motivated by distaste for the fact that anyone would criticize the subject he has been spending his time promoting. I don't think he would see himself as being irresponsible, he clearly hates what I was doing with my book, and wants to discourage anyone from buying it.

In practice, his review completely trashes my book, but is much kinder to Smolin's. The net effect I think is that it encourages people with any interest in the subject to buy Smolin's book (which is doing extremely well since the review). As far as getting the news out to the general public about what the situation with string theory is, fine with me if Smolin ends up being much more successful at doing that than me.

64. **Alejandro Rivero**
September 18, 2006

I'd not say that Dorigo his bet is based on "inside information"; in fact his blog is one of the best efforts to pass experimental information towards a wider public community (I hope you will mostly agree; I think I am not biased by his quoting of my preprints).

What surprise me of his bet is that it is not restricted to SUSY but also to any non minimal Higgs. Still, I think that the terms of discovery put on his side of the bet the composite Higgs and 4+0 dimensional model if not new particles are needed.

65. **D R Lunsford**
September 18, 2006

Alejandro – I would accept a single bet of up to $1000 bucks that the Higgs will not be seen at all as such, rather something strange and interesting with neutrino physics (e.g. very massive majoran) that will require a retooling of the SM.

-drl

66. **A**
September 18, 2006

if somebody is interested in proton decay in supersymmetric models: it is caused by a) triplet Higgsinos and b) vectors. a) is uncertain and typically too fast, but it is easily avoided in more complicated models. b) is more precisely predicted and small, beyond the reach of Super-Kamiokande; it can be reduced only with dirty tricks. Hopefully, the next two rounds of proton decay experiments will test b) closing the issue before 2030.

67. **Quantoken Impersonator**
September 18, 2006

One criticism I have with not even wrong, which is excellent in enthusiasm and for inspiring the reader to learn more about the study of the real mathematical physics subject of quantum field theory, is this:

Woit offers no analysis of the possibility of external refutation of string theory by another theory being successful.

To understand this, consider other failed theories like phlogiston, caloric, epicycles, mechanical gear cog ether.

In each case (let's call these 'religious theories' for simplicity), the theory was incapable of falsification within itself because it made no checkable predictions per se, just as is the case for string.

But in each case the theory was eventually INDIRECTLY FALSIFIED by alternative theories being extended to do more stuff than the religious theory.

Woit rightly takes a conservative line and doesn't make grandiose claims that alternatives are likely to overthrow string anytime soon in the way that alternatives overthrew the 'religious theories' mentioned above.

Woit should however acknowledge some probability (however low it seems subjectively) that the end of string will come by some paradigm shift away from M-theory and towards phenomenological modelling, rather than through the failure of the large hadron collider and experiments.

Because string speculations swamp arXiv and provide so many contradictory "predictions" about the number of "branes" and the mechanism for the strength of gravity, not to mention the landscape of vacuum energy solutions, I'm confident that string theorists are perfectly capable of reading ANY experimental data from LHC as "evidence" for some version of string theory.

Since the "predictions" are so vague, almost any data from LHC could be read as a suggestive hint that superpartners could exist. People WANT to see confirmation + Woit and Smolin put pressure on the mainstream => the mainstream cooks up some false evidence. Easy! Critics sorted.

Woit will find it harder to say that the Emperor's clothes are threadbare convincingly (which will be the case when string celebrates evidence from LHC), than to say the Emperor is naked (the case today in 2006). You're cornering a wild beast in string theory; it will put up a frenzied fight rather than surrender.

Woit will be deemed by the media to have a vested evidence in making negative noise and being dismissive towards string, so you'll be ignored, and I fear that fiddled LHC or other "evidence" for big branes in string theory will sink all atlernatives forever. Even epicycles and phlogiston had some alleged evidence. Can't Woit survey some alternatives and write an arXiv paper objectively categorising alternatives to string according to how many facts they encompass, and how few speculations they require? (At present Motl is the only one who repeatedly claims to do this, but his scheme has only one category for alternatives: crackpot.)

68. **TheGraduate**
September 18, 2006

Finally got my copy of NEW and T w/ P at the local bookstore this morning.

69. **Yatima**
September 18, 2006

Quantoken Impersonator wrote:

> In each case (let's call these 'religious theories' for simplicity), the theory
> was incapable of falsification within itself because it made no checkable
> predictions per se, just as is the case for string.

Well, according to Thomas Kuhn (in 'The Structure of Scientific Revolutions') that would be the normal state of affairs. You don't need 'checkable predictions' as everything is 'explained' by your current approach.

The prevailing theory (e.g. "phlogiston") would explain any observations made and proponents would push observed anomalies to the boundary, either ignoring/cataloging them or explaining them away ("this could easily be explained by high concentrations of tightly bound phlogiston…needs more study")

After anomalies accumulate and remain unexplained, a new 'paradigm' is proposed (generally by someone with no vested interest in the prevailing paradigm). That new paradigm then has to win over the existing one. Kuhn writes: "In the sciences, the testing situation never consists simply in the comparison of a single paradigm with nature. Instead, testing occurs as part of the competition between two rival paradigms for the allegiance of the scientific community".

Kuhn refutes the existence of a Popperian falsification procedure, any prevalent paradigm having to be flexible enough to (within bounds) accomodate anomalies. Not unreasonable: If there is no competing paradigm, people will just shrug in response to an anomaly and say "we know it doesn't always work"

Now, String Theory does not seem to fight, it just assimilates. It looks like an exploration of a mathematical space with the hope that something in that space will be found that, with some specialization/parametrization is able to generate all existing observations – and then some. Unfortunately one does not know whether there is such thing at all and whether it is anywhere in the 'vicinity' of the target, M-theory ('vicinity' could actually be defined more rigorously here) Most probably though there are many many such things out there, so you want to find the one that is least generalized… I will stop now. Sorry sorry Peter.

P.S. Kuhn used 'paradigm' first, before marketeers misappropriated it, to describe achievements "sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity (and) sufficiently open-ended to leave all sorts of problems for the redefined group of practiioners to resolve".

70. **TheGraduate**
September 18, 2006

What does Kuhn say about the role of needing to get tenure in the propagation of faddish scientific theories?

I've heard a lot about the physics job market being very, very tight and how only the more 'elite' physicists get academic jobs these days … I've also heard just being able to say you work on string theory carries with it a presumption of being an elite physicist …

I suppose I'm a bit surprised that there isn't more discussion of that aspect of things ie the very real possibility of putting a lot of these chaps out of work.

71. **King Ray**
September 18, 2006

I think that as Kuhn describes in his book, The Structure of Scientific Revolutions, that we are in a period between paradigms where physicists are wandering in the wilderness, doing things such as string theory. It will take a revolutionary new idea to create a new paradigm. Kuhn stated that physics and science do not advance monotonically, but in leaps and bounds when new paradigms emerge. In between paradigms, there is stagnation, like now. It's been 25 years since I read Kuhn's book, but certain points he made have really stuck with me.

72. **Chris Oakley**
September 19, 2006

> It will take a revolutionary new idea to create a new paradigm.

I am sick of hearing that a new "revolution" is required in physics. The two most important "revolutions" in physics in the last century were special relativity and quantum mechanics, and I have not even seen the results of these applied honestly and consistently.

73. **Thomas Larsson**
September 19, 2006

Alejandro, I admit that "inside information" was for rhetorical effect. However, Dorigo's post did confirm my suspicion that past experiments are tightly constraining all kinds of post-SM physics at the LHC. I agree that his site is a wonderful place to learn about experimental HEP.

Anyway, my point was that Distler's and Motl's bets are not strong evidence for SUSY, since insightful people are betting against them.

74. **Gina**
September 19, 2006

In my review there is a typo which makes one sentence ambigious , when I wrote

"Whether string theory will prevail or not, it seems hard to argue that string theory contributed important insights and technical infrastructure to mathematics and to physics."

I simply meant, of course

"Whether string theory will prevail or not, it is a fact that string theory contributed important insights and technical infrastructure to mathematics and to physics."

(To your question, Gina, I do have an academic background but not a degree in physics.)

75. **dan**
September 19, 2006

"Thomas Larsson Says:
Anyway, my point was that Distler's and Motl's bets are not strong evidence for SUSY, since insightful people are betting against them."

That's a good point.

I actually think that string theory does make "soft predictions" that can be "falsified", by soft I mean qualitative falsification not quantitative predictions. String theory, since it requires SUSY, and a SUSY theory consistent with known observation. Peter has objected to Distler and Motl's claim strings "predicts" or "outputs" MSSM (I mistakenly said SM originally) but perhaps the word "embed MSSM in the string theoretical framework" would be more accurate?

Mr. Larsson, to answer your question,
I am under the understanding that SUSY-GUT that would be consistent with current observation would be either MSSM- SUSY-GUT SU(5) or SO(10), it's my understand current observed proton half-life of 10^35 rules out 95% confidence MSSM-SUSY-GUT SU(5)

"arxiv.org/abs/hep-ph/0302272 – It is widely believed that minimal supersymmetric SU(5) GUTs have been excluded by the SuperKamiokande bound for the proton decay rate."

"arxiv.org/abs/hep-ph/0108104 "We make explicit the statement that Minimal Supersymmetric SU(5) has been excluded by the Super-Kamiokande search for the process $p \to K^{+} …

That leaves us with MSSM- SUSY-GUT SO(10), which I think can be said to be embedded in the framework of heterotic E8 type string theory (If Peter objects to the word prediction or output). MSSM- SUSY-GUT SO(10) would be the most minimal SUSY-GUT known consistent with current observations of the SM. It also predicts proton decay although I am unsure what half-life it predicts, and predicts masses for superpartners that is within reach of LHC energies.

This paper
http://arxiv.org/PS_cache/hep-ph/pdf/0004/0004266.pdf
states it is highly model dependent.

In summary: If no proton decay is ever observed + no SUSY seen at LHC energies (or future colliders) + Loretnz violation observed at GLAST + absence other string-inspired observations such as cosmic strings = falsification of SUSY GUT SO(10) = other MSSM models are unlikely to agree with experience == unlikely string theory is correct as a result of experimental and observational evidence.

76. **Peter Woit**
September 19, 2006

Dan,
As far as I know, the SO(10) SUSY gut doesn't predict masses of superpartners within reach of the LHC, it doesn't predict them at all. With current Tevatron bounds, some degree of fine-tuning is needed to make supersymmetry work, so one can worry that one has already lost the supersymmetry explanation of the hierarchy problem. If superpartners have such high mass that you can't see them at the LHC, you need a lot more fine-tuning. But then again, if you believe many string theorists these days, the hierarchy problem is resolved anthropically anyway.

77. **dan**
September 19, 2006

Dear Peter,

I probably will need to do some research, and possibly ask the usual suspects who are familiar with the relevant literature in arxiv

I thought the reason Lubos & Distler are *publically* willing to bet money is that they have reviewed the relevant literature in arxiv and unpublished, on SUSY and SO(10) SUSY GUT and believe that SO(10) SUSY GUT predicts superpartners (i.e neutralino) accessible at LHC ranges (with SO(10) SUSY GUT representing low-energy phenomenology of string theory consistent with known facts) otherwise, they stand to lose money. I suppose the bet could be extended for proton decay.

The article: "http://arxiv.org/PS_cache/hep-ph/pdf/0004/0004266.pdf" does mention fine-tuning SUSY to match observation.

It seems to me that most string theorists feel since LHC's results are still several years into the future (Distler commenting it takes time for LHC to run at spec.) I think for string theorists, it's business as usual.

78. **Christine**
September 20, 2006

Gina wrote:

[The Landscape] can also be an “impossibility result” which reveal a genuine problem with our ability to describe physical reality at some scales, if we like it or not.

For what is worth, maybe an “impossibility result” *for string theory*, like it or not.

BTW yesterday I received Smolin’s book from the assistant director of publicity and just from reading the first pages it made my day. Smolin is the voice of lucidness in a sea of… of… *hep-th nightmare* (yes, except for very few exceptions, doesn’t it look more and more like some kind of physics nightmare?? Why not rename hep-th to str-math, “string mathematics”?). If string theory proves to be correct, I’ll also be one of the first to applause the effort. (I am not particularly against string theory — or “against theories” anyway.). But while the situation is so far from success, other coherent, promissing alternatives must be encouraged. Otherwise, this is not how science must proceed. Even my 7 year old son can understand this.

It’s difficult to add something to what has already been so cleverly said elsewhere, except to acknowledge, based on individual experiences, that the sociological effect is real and damaging and somehow must be reduced as much as possible from the scientific activity.

I look forward to Woit’s book with great interest as well, and see how both books complement each other. It’s a very special moment in the history of science, and all I can hope is that I’ll be able to write a responsible review on these historical books.

Christine

79. **Gina**
September 20, 2006

Let me make a few more remarks on the discussion above.

One frustrating thing about this science business is having to keep double checking not only those matters of disagreement, but also matters of complete agreement. There was a single item in the discussion I certainly agreed with Peter about, the need of an honest and self-critical approach, and even this item, on closer examination, is not that simple.

We do not pay scientists to be just honest we pay them also to be gullible. They have to be gullible mainly regarding their own abilities and also regarding their beliefs on the foundation of the current theory they are working in, the prospects for its success, the relevance of the methods, and the overall importance.They have to be a little blind to the frustrating nature of their profession and to the overwhelming probability of a sense of missing out on their lives down the road.

To me, Peter, the signal concerning the state of string theory coming from Gross’ “never never give up” statement is not that much different from what you are saying. (I could imagine how I would feel if my personal physician, Dr Gross had told me: “The outcomes of your tests have now arrived, and I will never never never never give up on you, Gina.”) I think Gross’ massage to the young scientists is the more correct one. What I’d tell to them young (and old) brilliant guys and gals is: “Do not give up and always keep your mind open, (and get a life, and put things in proportion, and remember, it is a struggle all right, but it is not really, really, really a war.)”

So if you feel we are in a bottleneck here, wish to cut some corners, and do not have the patience to wait for the emergence of the next paradigm as beautifully explained by Yetimar (or persistence of this one), maybe it will be a good idea to follow Quant Imp’s suggesting having an arXive-quality document summarizing the alternatives. And maybe my suggestion from a few weeks ago to have a scientific short document with the main point for the “case against string theory” which will elaborate on the main problems and can get a little technical but without the maharishi, and the brothers, and summer salaries and the job market, can also be a useful idea.

“but in the generic case, a real possibility is that X-theory will achieve one or more of its major goals, making it a solid and permanent part of science, opening up new areas to work on that build on this success”

One has to be very very gullible (much beyond what is needed for a scientist) to believe that this is the case for a generic scientific theory.

“Gina, … but you’re just wasting my time”.

Well, as they say, “A scientist who is not waisting his time is waisting his time”. Anyway, I will try to improve the quality of my remarks (and reduce their frequency), and I hope they will eventually become interesting to you. But please do not delete me from these infinite cyber valleys.

80. **Yatima**
September 20, 2006

Gina, this pertains to your interests, I think.

Just now I’m burrowing through Lee Smolin’s book (in the middle of the night…hmm double special relativity? tasty!) but I will stop for now and quote from a novel by Arkadi and Boris Strugatsky (not to be confused with the Bogdanoffs) called ‘Far Rainbow’ which on the face of it is about an physical experiment going horribly wrong as the underlying governing laws are unknown (a classic, recently explored by Greg Egan) but it actually seems to echo the Great Patriotic War. Anyway, towards the end, Camille, the greatest physicist of Rainbow, is heard to say (he has worked too hard not to mention died a few times):

“The Great Logician. Logical methods demand absolute concentration. To do anything in science, day and night you have to think about one and the same thing, read about one and the same thing, talk about one and the same thing.. And where can you go from your psychic prism? Away from the inborn capacity to love… You’ve got to love, read about love, you’ve got to have green hills, music, pictures, dissatisfaction, fear, envy… You try to limit yourself – and you lose an emormous part of your happiness. And you know very well you’re losing it. So then to blot out that consciousness and put an end to the torture of ambivalence, you castrate yourself. You tear away from yourself the whole emotional half of your humanity and you leave yourself with only one reaction to the world around you – doubt. Then loneliness lies in wait for you.”

Hopefully most physicists are not so bitter. But stil, one gets what he means.

Some Links

September 19, 2006
Categories: Uncategorized

John Baez is encouraging people to join in a campaign to "save New Scientist" from itself, i.e. to get them to stop publishing so much scientific nonsense. This seems to me like a worthwhile goal; maybe if they stop writing articles about crackpots and their "electromagnetic drives", they'll also stop promoting bogus over-hyped claims from prominent theorists about cosmology, string theory, etc….

Shing-Tung Yau is fighting back against the New Yorker article "Manifold Destiny", which was very critical of him, essentially claiming he was trying to steal credit for the proof of the Poincare Conjecture from Perelman. He has hired a lawyer and set-up a web-site. The web-site includes a long letter from his lawyer to the New Yorker, making his case that the article has many inaccuracies. There will be a webcast tomorrow at noon giving his side of this story. Many other blogs and newspapers are discussing this, see here , here, here, here, and here. Unfortunately for Yau, he has strong support from Lubos Motl, who seems a tad obsessed, ranting about how the quality of the New Yorker article:

resembled the style and ethical standards of many jerks in the blogosphere, including a colleague of Sylvia Nasar at Columbia University.

People who want to engage in bashing of Yau or of his opponents are warned that they should do it elsewhere. Only comment on this here if you have something to say that is substantive and respectful of all parties involved.

Besides Yau's webcast, tomorrow you can also listen to me on the SETI Radio Network program, broadcast on Discovery Channel Radio. This will also be on their web-site, more info here.

The Harvard Crimson has an article about Nima Arkani-Hamed, who evidently made Popular Science's "Brilliant 10" list for

his research on the idea that our universe may be only one of many "multiverses" and that additional dimensions may exist.

(many "multiverses"???) Arkani Hamed promotes the anthropic landscape and split supersymmetry as a test for it:

He recently proposed a model for new physics, called split supersymmetry—which theorizes that half of all particles in the universe have partner particles. He said that if the results of the LHC experiment reveal split supersymmetry, "it would be a tremendous push in the direction of a multiverse."

"Right now a lot of people are on the fence," about the theory of a multiverse, Arkani-Hamed said. "I think if the LHC sees split super symmetry it's over."

Also on the multiverse front, Gibbons and Turok have a new paper out on The Measure Problem in Cosmology. They claim to have a way of determining a measure on the "multiverse". Only problem is that with their measure, the probability of having inflation work out the way it is supposed to is about e^{-180}.

Update: Another radio appearance today, on the program This Week in Science.

Update: To view today's webcast, go to www.premierewebcast.com, get your software working, and enter room 150144. I'll be skipping this myself, partly because I'll be in a faculty meeting.

Update: If you want to read a lot of incredibly ill-informed and worthless comments on the Yau story, there's always Slashdot.

Comments

1. **Me**
   September 19, 2006

   «John Baez is encouraging people to join in a campaign to ?save New Scientist? from itself, i.e. to get them to stop publishing so much scientific nonsense.»

   Last time I read New scientist was arround 1994. Back then I was still in high-school and even so I recognized it mostly (not all) as vapid at best, deceitful at worst.

   Apparently it has gotten worse.

   «The writer of this article, Justin Mullins, seems aware that conservation of momentum is violated, but then churns out a lot of meaningless double-talk about ?reference frames? which he seems to think demonstrates that relativity somehow comes to the rescue:»

2. **Peter Orland**
   September 19, 2006

   New Scientist is too far gone to save. If I were to get a paper cut from an issue, I would immediately seek a tetanus shot.

   What worries me more is the fate of Scientific American. Scientific American inspired generations of people to become scientists and engineers. Though it is still miles above New Scientist, its quality has significantly declined.

3. **Peter Orland**
   September 19, 2006

   It seems pointless for people to take sides in the Yau vs. Perelman
   fight. With so much attention focused on the debate, the truth will
   win out, no matter who champions either side. What a silly waste of time. The people who will (and should) decide the issue is the mathematical establishment. The papers are there, with dated submissions, in black and white – so where is the debate?

   The argument does not seem to even be a matter of intellectual courage, since neither side needs defenders from the sidelines. It is
   courageous to take an unpopular position out of conviction, not to argue who deserves more credit.

4. **Thomas Love**
   September 19, 2006

   The campaign to save new scientist from itself is much like your campaign to save science from itself.

As far as string theorists go: Just give them enough string and they will hang themselves.

5. **Dick Thompson**
September 19, 2006

I don't think the campaign to save New Scientist will go anywhere. Reed Elsevier is surely not going to change their ways unless profits fall, which is not likely to happen.

On Yau, it's just sad to see this man, to whom math and physics are so indebted, embroiled in a he said, she said scandal at this point in his life.

6. **nigel**
September 19, 2006

New Scientist's success in popularizing physics in the UK is demonstrated by hard, factual statistics:

"Physics has declined in popularity among pupils at school and students at university, research suggests.

"A-level entries have fallen from 55,728 in 1982 to 28,119 in 2005, according to researchers at Buckingham University." – http://news.bbc.co.uk/1/hi/education/4782969.stm

Cause for real celebration by New Scientist! I wrote an opinion leader in Electronics World saying that stringy hype is destroying British physics years ago, but the only responses were from moronic crackpots who claimed the decline in interest in physics was due to TV or computers or anything but stringy stuff. Nobody cares.

7. **Johan**
September 19, 2006

New Scientist is published by Reed Elsevier?! In that case, I don't particularly care what happens to it.

8. **werdna**
September 19, 2006

Peter Orland,

Just to clarify one thing: The so-called "Yau vs. Perelman fight" does not exist at all. Yau, in his attorney's letter, calls it a "fictitious battle." Cao and Hamilton also testified publicly that there was never a controversy surrounding questions of credit, etc. (As it turned out, the 50-25-30% quote was made up by a third-world reporter!) The only people who decided to make it a problem were the irresponsible authors/"fact-checkers" of that NYer article. As we now know, that piece apparently contains a lot of fiction.

There is a so-called "Tian-Yau affair," however, but it is a different matter entirely and should best be left to the two parties involved to sort out.

9. **MathPhys**
September 19, 2006

I read the various commentaries that followed The New York article, and I'm shocked by how Nasar seems to have enticed then misinterpreted every single academic that she interviewed.

I still have to read one academic that was quoted in her article come out and say "Hell, yeah, that's my opinion. What will you do, Yau? Not invite me to your birthday party?"

10. **gunpowder&noodles**
September 19, 2006

Regarding Gibbons and Turok: Lubos had a posting about this paper which reveals something about his way of thinking. Basically he thought that they were criticizing inflation, which is The Establishment. He even went so far as to say that the GT paper should not be published, and should not even have been posted on the arxiv, because it criticizes an Established Theory.

He doesn't like *any* criticism of The Establishment, and that is the sole reason for his support of Yau. See? It all hangs together. Sort of.

Regarding Yau: To many people, I think the most shocking thing in the original Nasar article was the allegation about how he rammed through the paper in the Asian JM. I note that this crucial episode is not mentioned in the long letter. Why? I think we all know the answer, and we all know what it implies.

11. **Peter Orland**
September 19, 2006

To MathPhys,

Perhaps I mischaracterized the matter as a fight. Yau's talk at the meeting in China, however, certainly seemed like a shot across the bow. Usually when a breakthrough occurs, attention should be (and usually is) focused on the research articles where the breakthrough happened. I don't work in the field, but the breakthrough in this case seems to be Perelman's, from what I can tell.

Incidently, I have not read the New Yorker article yet, but now I am very curious to know why some people are very upset about it.

12. **Peter Orland**
September 19, 2006

Oops, I meant werdna, not MathPhys. Please excuse me.

13. **Roy**
September 20, 2006

Werdna,
Slightly off topic– but I was curious what you meant by the following– "(As it turned out, the 50-25-30% quote was made up by a third-world reporter!)"

Why is the adjective "third world" important?

14. **werdna**

September 20, 2006

I think that if people have an objection to anything Yau might have said or done, they should personally write him at his Harvard address using their real identity and give the professor a chance to reply to it.

It pains me to see such a hardworking and accomplished mathematician who's contributed so much to the field to be dragged through the mud by an outsider like Nasar. He does not deserve it. And neither did those people who have been deliberately misquoted in the article. The judging of all relevant works, including who contributed how much to what part of the PC, should be left entirely to the mathematical community. And this community likes to take its time to rigorously go through everything to ensure correctness and fairness. This is something that certain writers/researchers/journalists could learn from.

15. **MathPhys**
September 20, 2006

To gunpowder&noodles,

The episode in Nasar's article regarding the publication of Cao and Zhu's paper is not mentioned in Yau's lawyers' letters because it has nothing, and I repeat nothing, to do with this entire sad story.

As werdna said earlier, the Yau-Tian conflict has nothing to do anything else.

16. **jeremy**
September 20, 2006

Peter,

First time listen to your radio interview (This Week in Science). The messages are certainly there, but if you can make the sentence shorter and not stop in the middle of it, it will sound much better. Still, very well done.

In the letter of Yau's lawyer, it mentioned the relationship between Phillip Griffiths and Nasar. What is the relationship? Wife, girlfriend or mistress?

17. **werdna**
September 20, 2006

Roy,

You've asked a fair question. I added that adjective to suggest that the authors should have known better than to rely solely on a source of information that might not be completely reliable, given that journalism standards were generally considered lower in developing countries. I certainly did not mean to suggest that reporters there were less honest than those in wealthier nations. There are many factors (such as educational level, likelihood of being sued, job pressures, etc.) that may cause or force some reporters to be sloppier than others. But again, that is the general impression.

18. **Chris Oakley**
September 20, 2006

Re: This Week in Science podcast.

Helpful note for those wanting to listen to Peter and not the less hot broadcaster's notes about his Hawaii vacation: start the audio exactly 30 minutes after the beginning.

19. **Thomas Larsson**
September 20, 2006

"I think if the LHC sees split super symmetry it's over."

???

If the LHC sees sparticles, it would prove nonsplit SUSY, wouldn't it? What is the signature for split SUSY, and how can it be distinguished from no SUSY?

20. **geometer**
September 20, 2006

werdna,

1) I personally think that this is a free country and people can say whatever they want without providing their names, but you seem to follow a different standard (and I respect that). So if you wish to critisize Nasar you should write to her using your real identity, so she has a chance to respond to the accusations.

2) I welcome Yau's letter (the more information the better), and I am especially glad the quote of credit distribution seems to be false (as it was ugly). The way I see it Yau knew the "50/25/30 quote" went arount the world like fire, and he knew it was reported by Xinhua, which is a news agency millions of people pay attention to (I am sure New Yorker has much smaller audience). So if Yau wanted to rectify the situation he could've posted/distribute a correction saying that "Xinhua report was incorrect, and here is what really happened". Yau did not do that. Now some people might think that "Xinhua report" benefited Yau's case (and it did, at least initially), and this is why he did not compain.

21. **comentator**
September 20, 2006

I found very interesting the words by Perelman in an interview after he talked with J.Ball in St. Petersburg. "It seems that Cao did not understood my arguments and he rewrote them, but everything is there".

Now at this point is hard to say who contributed what; But a good indicator will be the Clay prize, since they will be hearing from the people who actually has been working on Perelman's papers and I guess only one or at most two persons will receive it, being Perelman one of them of course.

22. **mathjunkie**
September 20, 2006

I don't believe the New Yorker's report on Yau!

Parts of the report are hard to be proved.

23. **MathLover**
September 20, 2006

geometer,

About your point 2, it's actually the case. The counsel's letter clearly states that the misquoted deputy director Yang Lo refused the idea of share of credit. Unfortunately only the first Xinhua news goes through the world, not the second one. But those facts were available to Nasar. Nasar was even warned by Yau that there were opponents to his fight against corruptions in Chinese universities / higher education system. Those opponents have obviously their fingers in the press.

BTW, we all support free speach and free press, but not free slandering. Every one is responsible for his / her own words. I don't see werdna using any other standard here.

The counsel's letter really shed light on this subject. It's clear, Nasar's article damaged the math community severely. Hints point to conspiracy that those who have an interest to against Yau are at work. Nasar may become involved knowingly or unknowingly.

24. **TheGraduate**
September 20, 2006

Yau should be doing the webcast in approximately 11 minutes.

25. **geometer**
September 20, 2006

MathLover,

Unfortunately I cannot read Chinese, but I did run the Yang Lo's intervew through the web translator and could not see any clear claim that the previous news was incorrect. In fact, the whole Yang Lo's interview was a celebration of "Cao-Zhu success", and it implied, as far as I could see, that their effort was no less significant that Hamilton's or Perelman's (which is false in my professional opinion).

I agree that "The counsel's letter really shed light on this subject", but I see no damage to the math community. On the contrary I think we shall all gain at the end because of this extra publicity.

26. **werdna**
September 20, 2006

geometer,

There are anonymous commentators who post responsibly, and then there are those who hide behind a cover to hurl personal attacks and baseless accusations at others. The postings of the latter usually get deleted – at least in this forum anyway. As has been pointed out, free speech does not mean free slandering.

Personally, I would not write to Nasar myself. Those who have been misquoted by her had made repeated requests to correct inaccuracies and distortions in the past, but their attempts were unsuccessful. If the messages of these victims were ignored, what good would my letter to her do? Do you think she would even respond?

As for your other point, I believe MathLover has addressed it satisfactorily. Note that the June 4th Chinese news report was quickly rescinded on June 9th.

27. **Curious Bystander**
September 20, 2006

There were interesting comments signed by "Anon at
http://motls.blogspot.com/2006/09/shing-tung-yau-goes-after-shoddy.html
They are now partially removed?!

Lubos objected that my first post here didn't address the individual points of the letter, so I'll try to do a bit of that presently. This may be boring, but hey, our host asked for it.

The letter does not provide 12 pages of evidence of libel–it provides a few pages of evidence that Nasar was less-than-ideally fair, along with a whole lot of petty bitterness and innuendo. A general issue that I would mention first is that, as I indicated earlier, I think that Nasar probably got the impression by talking to a lot of people that Yau wasn't trustworthy. Whether or not that's a correct impression is not something that I feel competent to judge, but if she had that impression then I do not believe that Yau saying, "I told you X and you didn't print it" or "You didn't let me give my version of X" is actionable, because a journalist is free to judge whether or not certain statements and sources are credible.

The specific objections don't start until page 3. We see a long list of sentences beginning with "You gave [Yau] no chance to respond to…" As I indicate above, all these can be explained by Nasar growing to distrust Yau over the course of researching the article. If there is a law stating that journalists are obliged to solicit and reproduce all of their subjects' opinions on everything they discuss in an article (even when they distrust the source in question), I'd be interested in seeing it. Nasar may be guilty of jumping to conclusions or being somewhat unfair here, but I seriously doubt that she's guilty of libel.

Page 4 first complains about the subtitle and the picture–as I said I think the picture is rather problematic, but again it's not clear to me that Nasar is responsible for it. There's then a complaint about the famous 50-25-30 distribution of credit that the Chinese state media reported in early June (mysteriously, this seems to have disappeared from the online stories). Nasar and Gruber quoted from a press conference, attributing the distribution to Yang Le and giving a quote from Yau (which Yau now says was fabricated–by a Chinese reporter, not by Nasar) which seems to endorse it. Now Yau is quoting an e-mail that he sent to the fact checker to the effect that "I did not say it and people put that into my mouth." Again, perhaps Nasar doesn't trust Yau about this–and perhaps for that matter that fact-checker looked into this and found that he did in fact say such a thing at the press conference; there surely were witnesses that could have been consulted.

Lubos, you presumably remember very well the 50-25-30 distribution of credit, since you wrote a blog post featuring it. It seemed to make a very real impression on you and many other people around the world. If Yau didn't believe in this, the time to distance himself from this was immediately afterward–but he didn't see fit to do so at the time. (Yang a few days later apparently gave an interview–AFAIK only ever published i

AFAIK only ever published in Chinese–saying that he (Yang) wasn't qualified to give such a distribution of credit…but Yau is someone who is qualified to do so, and I'm not aware of him retracting anything prior to his interview with Allyn Jackson in the September Notices, if you even count that.) Instead, Yau went on to give the talk at Strings '06 which, again, trumpeted very heavily what Cao and Zhu did (and also everything that Yau did that could at all be connected to the conjecture–remember a certain blogger who explained that Yau had given "a talk about the Poincare conjecture and how he proved it!"?). Only later, when criticism of this distribution started mounting, did Yau suddenly decide that it would be prudent to move away from it. Throughout June, Yau was presenting the proof of the Poincare conjecture as a triumph of Chinese mathematics. Nasar and Gruber had every right to report on this, and to view Yau's modest reversal on this issue as insincere.

There follows a weird reference to Nasar and Griffiths' "relationship" and the confident declaration that the negative comments obtained from other mathematicians about Yau only resulted from Nasar poisoning their mind with nasty rumors about him. Well, I seriously doubt that every mathematician I've talked to who has a negative opinion of Yau has talked to Nasar and got it from her. These opinions circulate widely throughout the community, and it's much more likely that the community passed them on to Nasar than the other way around.
Much of page 6 discusses the Cao-Zhu paper and the amount of credit it gives to Perelman. The statements are framed very carefully in order to make Yau (and Cao-Zhu) look like they're giving as much credit to Perelman as possible and that Nasar was denying this, but it also doesn't bother to provide any actual statements in the article that contradict anything being said here (there is a reference to a sensationalistic subtitle and to the picture–probably neither of which was Nasar's work). These paragraphs also commit what I would say is a slight misdirection–what's at issue in the New Yorker article is not how much credit the Cao-Zhu article gives to Perelman (indeed neither Cao nor Zhu come off at all badly in the article, I thought) but rather whether Yau's public behavior was aimed at giving an unreasonably large amount of credit to Yau and his proteges at Perelman's expense. Note that the Cao-Zhu article was never even publically available until July (or maybe late June in paper form), and the number of mathematicians (and laymen) who had some exposure to Yau's actions in the press in June is much larger than the number who have looked at the text of the Cao-Zhu paper. And Yau's actions in June (which admittedly may have been misrepresented by the Chinese press–but that's something for him to take up with them, not the New Yorker) sure looked a lot like he was claiming credit for himself and his proteges

–the only debate anyone was having back then was whether said claim was valid; I don't recall any of Yau's defenders saying at the time that the claim wasn't being made.

Starting on page 7 there's a discussion of the Givental flap. The letter of course neglects to mention that the article says, "Liu maintains that his proof was significantly different from Givental's," but does complain about the fact that the article didn't refer to specific arguments that Liu made to support this statement. I don't know a lot about this specific dust-up, beyond the fact that at least a significant number of people believe that Yau's behavior was inappropriate here and that the Lian-Liu-Yau paper claimed more credit than was fair. When Givental came up for tenure at Berkeley, the department of course consulted experts in the field about this, and you can guess what the outcome was based on where Givental is now. This was certainly natural background for the authors to give, and while again I can understand if Yau thinks the description of the Givental conflict was unfair to him I can't imagine what could be considered libellous about it.

Then we get into the issue of the authors' statement that although Yau had been publishing prolifically it has been a decade since his last major result. Yau may not like this statement, but it's so subjective (what's a "major result"?) that once again I just don't see anything that deserves a libel suit here. I'm not an expert on Yau's work, but I'm sure not aware of anything that he's done in the last decade that has been anywhere near as influential in mathematics as a lot of the work he did in the period from, say, 1975-86. Or as influential as the proof of the Poincare conjecture. Of course Schoen and Smoller (in the former case talking about one of his pet sub-subjects (special Lagrangians) and in the latter case talking about his own joint work) are entitled to their own opinions here. Quoting Stroock (a famous probabilist) of all people about string theory doesn't exactly do wonders for the credibility of the argument being made here.

OK, if Mike Anderson's account is true then the magazine should probably run a correction removing those words from his mouth. The letter then mentions Stroock's (or rather "Strook's") complaint that he was taken out of context. That may be true, but I strongly suspect that every magazine and newspaper in the country would go bankrupt if they were sued every time they took someone's statements out of context. Like so many other things in the letter, this is just filler that is not remotely cause for a lawsuit. And it's also worth mentioning that Anderson's and Stroock's widely-circulated letters that are critical of the article's treatment of Yau appear were directed to Yau, seemingly at his insistence. Stroock did write a letter to the editor of the New Yorker, the published version of which had a quite different focus.

The first section of page 10 raises a subjective matter of interpretation–again, this is filler.

The implication of the start of the next section is that it's libellous and/or defamatory to point out (truthfully) that Yau has never spent more than a few months at a time in mainland China. Hmm…

"Yau's efforts to combat institutional corruption at the highest levels of academia in China," referred to many times in the letter, have nothing to do with the question of whether the article in question is libellous.

Presumably if this whole thing goes anywhere (it probably won't), we'll find out whether the mysterious Chern relative really exists, and whether the claims about Yau trying to move the ICM to Hong Kong (which the article acknowledged that Yau denied–of course the letter doesn't mention this) have any basis in fact. I'm getting tired of going through this page by page by now, but I think I've addressed most of the main points. It may be 12 pages, but there's really not much there beyond lawyerly intimidation that I suspect the magazine will easily see through.

Now that I think about it, Yau's main goal here may not be to extract anything in particular from the New Yorker, but rather to get his version of the story out in wide circulation. And I guess he's succeeded in that.

28. **Peter Woit**
September 20, 2006

Curious Bystander,

Anyone who frequents Lubos's blog should be aware that he has a policy of deleting comments he disagrees with, especially if they are substantive and present facts that he doesn't like.

29. **geometer**
September 20, 2006

werdna,

You wrote: "There are anonymous commentators who post responsibly, and then there are those who hide behind a cover to hurl personal attacks and baseless accusations at others… As has been pointed out, free speech does not mean free slandering".

I fully support polite intellegent discussions. However, it seems to me that you and MathLover confuse "slander" with the "difference in opinions". One is yet to PROVE in court that Nasar's article is slander, and I think, this might be difficult. I assure you there are many very respectable mathematicians that feel the article is basically correct. You may well disagree, just please do not call this slander. And there is nothing wrong in anonymous postings as long as they are polite and responsible.

You wrote: "Personally, I would not write to Nasar myself. Those who have been misquoted by her had made repeated requests to correct inaccuracies and distortions in the past, but their attempts were unsuccessful. If the messages of these victims were ignored, what good would my letter to her do? Do you think she would even respond?"

She would not, and this was exactly my point. It makes no sense for a Prof. Nobody to openly critisize to Prof. Yau, who would not even respond. However, Prof. Nobody can still express her opinions anonymously in a forum, as long as she is polite.

You wrote: "As for your other point, I believe MathLover has addressed it satisfactorily. Note that the June 4th Chinese news report was quickly rescinded on June 9th."

So I figure you can read Chinese. I truly envy you. Could you kindly translate for me the exact quote where the 1st news report was rescinded?

30. **werdna**
September 20, 2006

geometer,

The slandering I was talking about were the "personal attacks and baseless accusations" that have been deleted in this blog by Peter. Please read my entire paragraph again. I have always been supportive of polite, fair, and meaningful discussions here and elsewhere, and have never tried to silence anyone for a difference of opinions.

I did not say Nasar's article was "slander." Please read my earlier post above. I said it "apparently contains a lot of fiction." This is based obviously on the new information provided by the attorney's letter and the letters written by the academics who were (mis)quoted in the article.

You said that "I assure you there are many very respectable mathematicians that feel the article is basically correct." If that is the truth, then let the truth be told honestly. Why does a writer need to use deceptive interviewing methods and questionable editing styles to twist people's words? Wouldn't it be easier to just tell it like it is?

Finally, the June 4th and June 9th dates were taken from the attorney's letter. It is written in English, not Chinese.

31. **jeremy**
September 20, 2006

Curious Bystander writes,
"A general issue that I would mention first is that, as I indicated earlier, I think that Nasar probably got the impression by talking to a lot of people that Yau wasn't trustworthy."

You think? Nasar probably? So, you just went on writing everything based on your imagination. I thought that we are here to talk about irresponsible writers.

32. **Moeen**
September 20, 2006

MathPhys said: "The episode in Nasar's article regarding the publication of Cao and Zhu's paper is not mentioned in Yau's lawyers' letters because it has nothing, and I repeat nothing, to do with this entire sad story."

I thought what the authors were trying to get across was that Yau was trying to get more than his fair share of credit, and ramming the Cao-Zhu paper through the journal and bypassing the usual procedures was an example of him doing that. How then does it have "nothing" to do with this story?

33. **geometer**
September 20, 2006

werdna

"Finally, the June 4th and June 9th dates were taken from the attorney's letter. It is written in English, not Chinese."

Oh, so you simply believe what is stated in the attorney's letter. I have no more questions. If you ever wish to research the original sources try the direct links in Chinese listed at
http://boards.newyorker.com/thread.jspa?threadID=721&tstart=0
I wish someone could translate all this for me.

34. **Curious Bystander**
September 20, 2006

Jeremy writes: "You think? Nasar probably? So, you just went on writing everything based on your imagination. I thought that we are here to talk about irresponsible writers."

1. Though I agree with most comments made by "Anon" in my post I do not want to attribute the credit for his thoughtful remarks on the subject to myself.
2. We are not talking about imagination but of opinion. This opinion agrees we the opinion of a lot of machinations in this and other blogs. I, like "Anon" also believe that "These opinions circulate widely throughout the community, and it's much more likely that the community passed them on to Nasar than the other way around."
3. The previous posting was not about responsible or irresponsible writes (though I believe that Sylvia Nasar is a responsible one) but of the grounds for a legal dispute.
4. Talking of responsibility, why does nobody complain about completely irresponsible remark in Dr Yau's letter on "relationship between Ms. Nasar and Mr. Griffiths"?

35. **jeremy**
September 20, 2006

Moeen writes,

"I thought what the authors were trying to get across was that Yau was trying to get more than his fair share of credit, and ramming the Cao-Zhu paper through the journal and bypassing the usual procedures was an example of him doing that. How then does it have "nothing" to do with this story?"

How did Yau get his share of credit? It is Cao-Zhu's paper, right? If there is redit to get, it would be credit for Cao-Zhu. Do people really give credit to someone who talks about a problem being solved rather than give it to the ones who wrote about how to solve it?

How fast a journal wants to publish a paper is solely decided by the editor of the journal. In this case, it was Yau. If everything in the Cao-Zhu paper is correct, he can take credit for publishing a good paper. But if something turned out to be wrong in the paper, he will be humiliated publicly at least in the mathematics community. So, it is a fair game and there is nothing wrong with it.

36. **TheGraduate**
September 20, 2006

Mooeen,

I think you've got it slightly wrong, at least according to my reading of the article, it implies that Yau was motivated by trying to get as much credit for China as possible.

They are implying that he wanted credit but what they are implying that he wanted credit for was as a leader in promoting chinese mathematics not for his intellectual efforts.

37. **Peter Shor**

September 20, 2006

New Scientist has always been pretty flaky, but whatever happened to the other science magazines on a similar level? There seem to be a lot fewer of them now than there were ten years ago, and they all seem to be increasing their flakiness content. Maybe it's just that I'm increasing my standards, though. Other opinions?

38. **TheGraduate**
September 20, 2006

P. Shor,

I think people that are fans of science magazines like for science articles to be inspirational in a sort of pseudo-religious way. The commercial entities that sell these magazines are aware of it and twist the content to suit.

In other words, the job of New Scientist is to make money and I'm sure it does what it is made to do.

From the business perspective, that is what a business is supposed to do — make money. Anything else is superfluous and naive.

39. **Curious Bystander**
September 20, 2006

Oops, how stupid of me, I meant mathematicians, not machinations.

40. **geometer**
September 20, 2006

Jeremy,

You wrote: "How did Yau get his share of credit? It is Cao-Zhu's paper, right?"

In addition to what TheGraduate said, you should know that Cao is Yau's former student, and any success of a former student is (quite naturally) considered a success of his advisor. (Just imagine that you have had several students who are now tenured at top-5 universities?)

41. **CS Guy**
September 20, 2006

1. The first responsibility of any publication is to its readers. It has no responsibility to make anyone "look good" or to ensure that everyone written about is perfectly happy with how they are portrayed. Their responsibility is to inform their readers about interesting things that they would have otherwise been ignorant of. I think Nasar and Gruber did this admirably doing a lot of original reporting and conducting dozens of interviews. This is the very definition of a good magazine article.

2. Obviously S.-T. Yau has a right to present his own view. Indeed this is very much a good thing, so people can get both sides. But I do question the logic of a law suit. Imagine if GW Bush sued every news paper that portrayed him as an idiot or Bill Clinton sued every one who portrayed him as a liar? The fact that people say something bad about you does not mean it would be wise to sue them.

Here is a wikipedia article on libel. Based on the information presented there, I don't think S.-T. Yau has a chance unless he shows that the New Yorker *presented as fact something that they had no reasonable basis to believe was true*. Since the most damaging parts are public statements of Yau and quotes from fellow mathematicians, I don't see how he can win this.

3. Lastly, here is the response of The New Yorker:

> "Manifold Destiny," a 10,000-word article by Sylvia Nasar and David Gruber published in the August 28, 2006 issue of The New Yorker, is the product of more than four months of thorough, careful reporting and meticulous fact-checking. Ms. Nasar and Mr. Gruber spent over twenty hours interviewing Dr. Yau; they conducted approximately 100 other interviews with people in the field; corresponded by email with Dr. Yau and many others; and traveled to China where they conducted interviews and attended speeches and events discussed in the article. In addition, the magazine's fact-checkers spoke with Dr. Yau for approximately eight hours, they examined notes, tapes, and documents gathered by the authors, and the checkers conducted their own thorough research. Contrary to Dr. Yau's assertions, the article is nuanced and fair, and was prepared using ethical standards of journalism. Dr. Yau, his supporters and his point of view were given ample space in the article.

Basically, after reading this latest law suit, I am even more inclined to believe truth of The New Yorker article. We should be wary of covering up for famous people. That will only tell them that they can get away with anything and lead to more and more outrageous behavior.

42. **TheGraduate**
September 20, 2006

CS Guy:

Where is that quote of the New Yorker's response from?

43. **King Ray**
September 20, 2006

This whole Perelman-Yau thing kind of reminds me of the Einstein-Hilbert episode where Hilbert was going around discussing Einstein's ideas for GR and not attributing them to Einstein; this was right before his 1916 paper came out, if I remember correctly. Einstein had to write him a nasty letter to ask him to stop presenting Einstein's work as his own. Hilbert nearly beat Einstein to his Lagrangian for GR, or maybe he did find it a little before Einstein. There was also something where Einstein was publishing some work similar to what Cartan had done so Cartan wrote him and they ended up being great friends and collaborating on their teleparallel theory. Anyway, that is my recollection from reading about these things 20+ years ago, so I may be a little off.

44. **Molnar**
September 20, 2006

In answer to Peter Shor's question, I had the same impression about 20 years ago when I stopped reading Scientific American. I can't resist generalizing the observation to other periodicals, such as the New York Times, which I also no longer read. Given my advanced state of mental decay, I find it hard to believe my own standards are rising; I'm afraid the mainstream publishing business is going to hell in a handbasket. And speaking of the New Yorker, I still read it, but I think it, too, has declined in recent years (think of what A. J. Liebling would have written about Yau and Perelman).

45. **John Baez**
September 20, 2006

Peter Shor writes:

> *New Scientist* has always been pretty flaky, but whatever happened to the other science magazines on a similar level? There seem to be a lot fewer of them now than there were ten years ago, and they all seem to be increasing their flakiness content. Maybe it's just that I'm increasing my standards, though. Other opinions?

I don't think *New Scientist* was always so flaky. In the 70's it used to be pretty good, except perhaps with a left-wing bias (which some people like more than others).

Scientific American took a big slide downhill a while back. When I complained about it in This Week's Finds, one of the editors asked me to write some articles for them. I never got around to it. I didn't think it was a deliberate trick to prevent me from complaining further, as in "hey! how can you complain about us when *you're one of the writers?*" I even sort of wanted to do it, since Martin Gardner had been one of my heroes. But I just couldn't get up the enthusiasm to spend my expository energies in old-fashioned media where the readers have to pay and the writers don't get much of the money. I don't want a middleman.

And maybe I'm not the only one – maybe all the scientists busy blogging these days would once have been trying to write articles for pop science magazines! Maybe that's part of the reason for the downhill slide?

Or is the main problem the takeover of magazines by media conglomerates, who demand a certain percent of revenue growth per year? I always thought it was that.

Or, maybe, as you suggest, we're getting to be old crochety academics who whine about standards have dropped since the good old days when *we* were young. This reminds me of that *New Yorker* cartoon where two balding gentlemen are slouching in over-stuffed chairs in some faculty lounge, and one says:

"At least we never stooped to popularizing science!"

I don't really know. Whatever happened to *Omni*, and *Discover*, and the much better *Science News*?

Does anyone even care?

46. **CapitalistImperialistPig**
September 20, 2006

Peter,

I have posted a review of NEW on my blog.

47. **CapitalistImperialistPig**
September 20, 2006

John Baez,

While I can't disagree with anything you say, I have to say I'm sorry you never wrote those articles for *Scientific American*. There is something to be said for writing for the intelligent layman. It's certainly true that the magazine has gone downhill, but as a subscriber since approximately the time they started running the "25 years ago" column, I'm not going to give it up now.

48. **jeremy**
September 20, 2006

geometer writes:

""""
Jeremy,
You wrote: "How did Yau get his share of credit? It is Cao-Zhu's paper, right?"

In addition to what TheGraduate said, you should know that Cao is Yau's former student, and any success of a former student is (quite naturally) considered a success of his advisor. (Just imagine that you have had several students who are now tenured at top-5 universities?)
""""""""

Interesting. But do we all get jubilant about the achievements of our children, our students and our friends (well, forget about the friends part), no matter how small the achievements may be? Are we all proud of how they thrived in their life, in their work and in their research? Are these the most glorious feelings that any parents and teacher could ever have? Do we all have the urge to go out to tell everybody that we know about their success? And do we sometimes even exaggerate a little? Is that the natural behavior that made what we are as human? There is nothing wrong about it, isn't?

Ultimately, whatever we say about our children and about our students does not mean too much, because their achievement and success will be judged by the society and by their peers. Likewise, Cao-Zhu's contribution will also be judged by the mathematics community and by their peers, no matter what Yau says. And whatever Yau says, as a proud teacher, to praise his students is not wrong.

49. **Murray**
September 20, 2006

I am a Chinese but not a mathematician. I do read Chinese. I read of the 50%-25%-30% news on a Chinese News website a few months ago. The quot was attributed to S.T. Yau as I recalled. It's clearly aimed at bolstering Chinese self-confidence on Yau's part. I personally felt it's a rather silly act. Yau was doing a disservice to the very Chinese people he was trying to help in my opinion. However, I do not doubt Yan's motive. Yau is a Chinese patriot. Cantonese are among the most patriotic people in China.

About 80 years ago or so, the great Chinese writer Lu Xin wrote that the most damaging act to the Chinese people would be for teh Nobel committee to award a Nobel literature prize to a Chinese writer for the sake of being Chinese. I concurred 100%. If you read Chinese News from China today, you can feel the intense yearning from the public and academic circles alike for winning a Nobel prize for the Chinese people. Unbelievable!

50. **gunpowder&noodles**
September 21, 2006

By the way, this quote is interesting:

"Howard M. Georgi '68 wrote in an e-mail that Arkani-Hamed's work on split supersymmetry could be just as important as his research on large extra dimensions"

I have to admit that I suspect a touch of subtle Georgian humor here……..

51. **Chris Oakley**
September 21, 2006

I think that Georgi has got it right here. Arkani-Hamed's work on split supersymmetry *is* just as important as his research on large extra dimensions.

52. **Me**
September 21, 2006

« Hilbert nearly beat Einstein to his Lagrangian for GR, or maybe he did find it a little before Einstein.»

Yes, Hilbert got the action first.

53. **John**
September 21, 2006

I can't help being impressed by Einstein's note to Hilbert which is conciliatory without giving up the point at issue:

"There has been a certain resentment between us, the cause of which I do not want analyze any further. I have fought against the feeling of bitterness associated with it, and with complete success. I again think of you with undiminished kindness and I ask you to attempt the same with me. It is objectively a pity if two guys that have somewhat liberated themselves from this shabby world are not giving pleasure to each other." (from Corry, Renn & Stachel, Science, 1998)

54. **DMS**
September 21, 2006

As someone with no dog in this fight(Yau-Nasar), I must say that the mathematical community does not looking good.

Sure, there are always fights over credit in all areas, as in physics. At least in physics, they come out openly and say it(like Gell-Mann and Feynman), rather than say things behind someone's back(and maybe disown it later, if one accepts the New Yorker version).

And, as Mathlover says, if Perelman's papers were so clear and complete, why did others bother writing much longer papers to "explain" it—simply give Perelman the half million(other half to Hamilton).

55. **Peter Woit**
September 21, 2006

DMS,

Nobody is claiming Perelman's papers were "clear", as for "complete", the question is just whether they contained all the ideas necessary for an expert to work out the details of a proof, without having to do major original work and come up with new ideas themself.

The Clay foundation has specific rules about what one needs to do to claim the million dollars. A refereed paper, in a major journal, not necessarily by the person who came up with the idea of the proof, is part of the requirement. Perelman's preprints definitely didn't qualify. The two contenders for the required paper would be Cao-Zhu and Morgan-Tian. It's up to the Clay panel to decide if those who wrote up the proof did original work of a sort that deserves part of the $1 million. In this case I'd be surprised if they decide to give any of the money to either Cao-Zhu or Morgan-Tian.

56. **Anonymous**
September 21, 2006

Peter, members of the Clay panel have already indicated that they may waive the requirement of refereed papers in Perelman's case, or at least that's what I heard from second-hand but reputable sources at the ICM.

57. **Peter Woit**
September 21, 2006

Thanks Anonymous,

I was under the impression that Clay was refereeing and publishing the Morgan-Tian manuscript precisely so that there would be an unambiguous satisfaction of the refereed paper requirement, and that they were in no hurry to deal with the question of what to do about the million dollars, quite happy to have a couple years to let the dust settle. But, maybe they do want to get this over with…

58. **TheGraduate**
September 21, 2006

Concerning the part of the contraversy which involves corruption in China, I was thinking that it's not really a big deal if an academic allows himself to be listed as associated with more than one university as long as he is fulfilling whatever requirements are being asked of him.

I somewhat think of it like having more than one job. It seems to me that if I have more than one job, I'm not necessarily obligated to tell everybody I'm working for that I have more than one job as long as I'm fulfilling all my contractual obligations …

What is the academic concensus on this sort of behavior?

59. **Peter Woit**
September 21, 2006

TheGraduate,

It's not at all unusual for academics to have affiliations and be collecting checks from more than one institution. Sometimes this is part of a specifically negotiated deal a person has with the universities involved, sometimes it is part of the normal situation of someone visiting one place while being on temporary leave or not having to teach at another. Most universities have various specific regulations about what sorts of other paid work their faculty can take on without needing special permission.

I know nothing at all about the Tian situation that Yau is complaining about, but presumably both Princeton and the people in China know that he is involved with and being paid by another institution and are allowing this. I guess Yau is claiming that Tian's arrangement in China is unusual and "corrupt", but I have no idea what it is, and this just seems to be part of the unusually hostile and bitter relationship between the two of them.

60. **Thomas Love**
September 21, 2006

Now if we can just get Physical Review to stop publishing nonsense.

61. **MathLover**
September 21, 2006

What could be the Nasar-Griffiths relationship mentioned on page 5 of Yau's attorney's letter? Little facts can be found in internet, including following one:

In 1995-96 Nasar was a Direct's Visitor at IAS in Princeton, IAS director at time was Griffiths. There Nasar has done the research and interviews for her book "A Beautiful Mind".

Other connections between people around the New Yorker article can be verified in internet as well:

Shing-Tung Yau, PhD student of S.S. Chern at Berkeley (1971)

Phillip A. Griffiths, secretary of IMU 1999-2006, director of IAS 1991-2003, math faculty of IAS since 2004; a collaborator and good friend of S.S. Chern; holds an honorary degree from Peking University (Beijing, China)

James Carlson, the president of Clay Math Institute, was a PhD student of Griffiths at Princeton (1971)

Gang Tian, graduate student of Peking University, PhD student of Yau at Harvard (1988), faculty member of Princeton

Deane Yang, PhD student of Griffiths Harvard (1983), professor at Polytechnic University in Brooklyn, N.Y.

Prof. Yang was among three readers of New Yorker published letters on September 11 issue. Other two are Prof. Daniel W. Stroock from MIT (quoted in article) and Prof. Solomon Golomb from University of Southern California. Their letters see the link:

http://www.math.poly.edu/~yang/letters.html

Prof. Yang is also one of commentators using own real name at Peter's blog.

62. **TheGraduate**
September 21, 2006

Peter,

Yes, I would not have thought there was very much wrong with that sort of behavior. I guess there could be other complications that I am not qualified to assess: perhaps some rule in Chinese culture or specifically in Chinese academic culture. But to my mind, there is no way to secretly be on the faculty of two universities so it seems like the kind of thing each institution must be satisfied with.

Also, I am in the process of reading your book. It's been a very interesting read so far. Questions soon to follow.

63. **geometer**
September 21, 2006

Peter Woit wrote: "The two contenders for the required paper would be Cao-Zhu and Morgan-Tian".

You forgot Kleiner-Lott, whose writings were also supported by Clay Institute. Incidentally, there three papers are somewhat different. Kleiner-Lott's is a companion to Perelman's first and second paper, i.e. they have to be read together with papers of Perelman. Cao-Zhu covers the same ground and is self-contained; also Cao-Zhu could not understand some of the Perelman's arguments and it seems they have substituted their own arguments. These two papers cover the full geometrization including Poincare Conjecture. Morgan-Tian only give a proof of Poincare's Conjecture (but not the geometrization) in which they follow Perelman's three papers, so a unique feature here is that they cover the 3rd paper, which is a shortcut to the Poincare's conjecture and is not discussed by Kleiner-Lott and Cao-Zhu.

64. **Peter Woit**
September 21, 2006

geometer,

I didn't forget Kleiner-Lott. They certainly have a good argument that they were the first to work out the details of Perelman's arguments, but they did not submit their paper to a journal and it was not refereed, so it doesn't now satisfy the requirements of the Clay prize. They may yet submit it for publication and get it refereed.

65. **MathLover**
September 21, 2006

[Various accusations against Tian deleted]
I hope this post will add some background to the drama. If there is anything mistakenly stated, please correct it. Hope this stays as objective and Peter will not delete it.

66. **woit**
September 21, 2006

MathLover,

Do not even think of trying to carry on the vicious Yau-Tian fight in the comment section of this blog. I want no part of it, I think the whole thing is a complete disgrace and has done huge and continuing damage to mathematics. If you really think you must participate in that ugly mess, do it elsewhere.

67. **werdna**
September 21, 2006

I don't think any of the mathematicians who have contributed one way or another to the solution of the Poincare Conjecture cares about the prize money. Yau himself certainly doesn't seem to care about money, although he could use some of that now for a possible lawsuit…

What mathematicians in general care a lot about is the proper credit given for their original contribution to mathematical research.
For a famous problem like the PC, it is understandable that one might want to be acknowledged properly that he has contributed some part in solving it, even if it is, for example, "only" a lemma that is used in another lemma that is used to prove an important theorem. Just exactly how this credit is shared will be left to the math community, who will undoubtedly go through all the documents carefully and fairly, and this will take some time. Whoever made up the 2-year rule was wise indeed.

68. **TheGraduate**
September 21, 2006

I think pretty much everything that can be said about this Poincare buisness has been said. Information is limited. This is a bit surprising as we live in the information age and a lot of this stuff should theoretically be in the public domain. But I think all reliable information sources and conclusions have been exhausted. (And no random emails from unverified sources are not reliable information sources.)

My other thought is this stuff is blown way out of proportion. As werdna pointed out, this is probably going to get sorted out eventually and accurately. Even if we make the wrong call now, there will probably be some historian in 50 years who will read all the original documentation after everybody is dead and then he or she will sort it out.

69. **TheGraduate**
September 21, 2006

Peter,

In the intro. to your book, you said you moved to France as a kid. Do you speak french? I have often wondered that from your frequent links to french articles.

70. **Peter Woit**
September 21, 2006

The Graduate,

Yes, I speak French fairly fluently, although my standard joke is that, yes, I speak French, but like a 12-year old (which is how old I was when I moved back to the US).

The strange thing about this whole Poincare story is that it isn't really a case where there's any significant disagreement about who did what in solving the problem. Pretty much everybody agrees that:

1. Richard Hamilton (encouraged by Yau) had a program for finding a proof using Ricci flow, made a great deal of progress towards the solution, but got stuck.

2. Perelman came up with new ideas and techniques that overcame the difficulties Hamilton couldn't resolve, writing up a detailed outline of a proof.

3. Several mathematicians worked on filling in the details and checking the proof to make sure that it really worked. Kleiner-Lott did much of this, recently Cao-Zhu and Morgan-Tian produced two independent completely written up versions (both used the work of Kleiner-Lott).

Unlike many cases in science where there are real priority disputes over who did what first, in this case I don't think any of the above is controversial. What has generated controversy is people trying to simplify the above story and/or spin it for various purposes, and/or accuse others of trying to spin it.

71. **geometer**
September 21, 2006

I think what generated controversity is the statements made by Yau in June (or more precisely statements attributed to Yau by Chinese media). For example this one (in Chinese, which I have read via a web-translator)
http://gb.magazine.sina.com/chinanewsweek/20060710/2006-07-16/225714056.shtml
where he supposely said that "Chinese contribution is at least 30%". Yau now says that he made no such statements; well there seems to be no record that he publicly objected to any of the statements back in June/July.

Now it appears that after the ICM Yau has changed his mind (even though the only evidence of this is the statement of his attorney, which is not the same thing as Yau's own statement). So people say "there is no controversity, everything is fine".

I personally think that allegation of unethical behaviour of this magnitude should be investigated while it is still fresh in people's mind. This is because Yau is a public figure and eventually he may run for President of AMS etc, and it'd be helpful to know if the allegations are true.

72. **MathLover**
September 21, 2006

geometer says:

werdna,
……

You wrote: "As for your other point, I believe MathLover has addressed it satisfactorily. Note that the June 4th Chinese news report was quickly rescinded on June 9th."

So I figure you can read Chinese. I truly envy you. Could you kindly translate for me the exact quote where the 1st news report was rescinded?

To geometer:

I can give the link of English news items by Xinhua as follows:

http://news.xinhuanet.com/english/2006-06/03/content_4642313.htm
http://english.people.com.cn/200606/04/eng20060604_270860.html

http://news3.xinhuanet.com/english/2006-06/04/content_4644754.htm
http://english.people.com.cn/200606/05/eng20060605_271113.html

http://news3.xinhuanet.com/english/2006-06/21/content_4724497.htm
http://english.people.com.cn/200606/21/eng20060621_275840.html

The news on June 9th in Chinese:
http://www.gov.cn/jrzg/2006-06/09/content_305248.htm
has no English version from Xinhua. One reason is that I can't find the infamous 105% quote in English neither, hence no need to retreat from. I don't feel comfortable to translate it. Please use a translator program in internet (by yahoo or google). That will give you some idea. (Or believe what werdna said, I can confirm it is true.)

The journalist of the June 4th is called LI Bin. He was born in 1972 but has earned a good credit among his peers in Xinhua news agency. I can't explain what really happened. A post by "Columbia Chemist" may give you a clue. Nevertheless, it was unprofessional on Li's part first and very unfortunate to be spreaded by New Yorker's article, when we believe the letter by Yau's attorney and Nasar just ignored Yau's complain.

73. **Peter Woit**
September 21, 2006

geometer and MathLover,

The debate over the 30% is exactly what I had in mind here when I wrote that the problem is people trying to oversimplify and spin things. What Hamilton contributed to this proof is different than what Perelman contributed and these are both very different than the contributions of Kleiner-Lott-Cao-Zhu-Morgan-Tian. Trying to assign relative numerical values to these things is silly, you're comparing apples and oranges.

The contributions of Kleiner-Lott-Cao-Zhu-Morgan-Tian involve careful exposition and checking of details. This kind of work is considered an important service by the math community, but it is not considered the highest level of mathematics research, which is coming up with new mathematical ideas. What Hamilton and Perelman did was creative mathematics, in both cases they had to come up with something fundamentally new, something that wasn't there when they started. Hamilton came up with and did a lot of foundational work on what turned out to be a successful program, this is something research mathematicians value highly. Perelman also came up with unexpected new ideas and created new mathematics. In addition, he was the one to find the new ideas needed to finally solve the problem.

I think almost all mathematicians value much more highly what Hamilton and Perelman did than the work of Kleiner-Lott-Cao-Zhu-Morgan-Tian. Comparing the two of them though doesn't make sense. Hamilton had the correct vision and worked out a large amount of what was needed. Perelman was able to get around an obstacle that Hamilton couldn't surmount and get to the end. This kind of achievement is traditionally the one that ensures your name goes on a theorem. What to name theorems is also kind of silly, requiring huge oversimplification, but, no one is going to argue that Perelman's name doesn't belong on this theorem.

74. **werdna**
September 21, 2006

MathLover and geometer,

Again, I got the dates of June 4th and June 9th from the lawyer's letter. Whether the details there are correct, I do not know. It seems to come from a reputable source, and unless new information emerges to prove otherwise, I have no reason to doubt its accuracy for now.

75. **Gina**
September 21, 2006

Wouldnt it be a better use of both time and space to talk about these exciting three dimensional manifolds themselves and how they are now understood and what is perhaps left to understand (if anything) rather than about lawyers and credits and newspapers articles and prizes and slanders and power struggles etc??

76. **geometer**
September 21, 2006

MathLover said: "One reason is that I can't find the infamous 105% quote in English neither, hence no need to retreat from.

Well, "Columbia Chemist" gave a direct link for http://news3.xinhuanet.com/newscenter/2006-06/04/content_4644722.htm
for the 105% quote. It is in Chinese, and here is how google translates it:

"Reporters ask questions mathematician Yang Le. The IMC said that if divided by 100%, then the United States and in more than 50% of the contribution mathematician Hamilton, the Russian mathematician Perelman resolve the principal suspect in the 25% contribution. "Chinese scientists, including Qiu, Zhu Xiping and Cao Huaihu East, 30%. " Yang said that in a century, a major problem worldwide, 30% of the people of China can play a role, it will not be easy, which is a great contribution.

I also translated the link you provided, and here is what I got:

"Yang Le academicians believe that a quantitative description of scientists from various countries in proportion to the contributions made to break the Poincare Conjecture, or more than 300 pages of papers analogy of our scientists as" novel "and" not entirely accurate, I do not agree. However, Chinese scientists have indeed made 'outstanding contribution'. "

77. **geometer**
September 21, 2006

Gina said:
"Wouldnt it be a better use of both time and space to talk about these exciting three dimensional manifolds themselves.."

Well, the proof of Poincare's Conjecture means precisely that there is no exciting (simply-connected closed) 3-manifolds: all of them are copies of the 3-sphere. It'd be much more exciting if the Poincare Conjecture were false; unfortunately this is not the case.

78. **Gina**
September 21, 2006

Actually, I have a specific question that may be you guys can help me with. I vaguely remembered the wonderful story of this humble mathematician whose nick-name was "Papa" who worked on some things related to manifolds in dimension three and after years of effort managed to prove something really big. Thanks to Google and Wikipedea I found his full name – Christos Dimitriou Papakyriakopoulos and appearently he proved the "Dehn's lemma". I am curious if all this exciting new works also give a new proof to what "Papa" have done or is it still also "on his shoulders"?

79. **Gina**
September 21, 2006

Thanks, geometer,so are you telling me that this three dimensional saga is done and over with and we can go ahead to four dimensions?

80. **geometer**

September 21, 2006

Gina,

I am not a 3d-topologist but as far as I know Ricci flow arguments do not imply Dehn's lemma (as well as many other results of 3d-topology). To date Ricci flow only implies the geometrization (and all its corollaries), and unfortunately there are very few known application beyond that. On the other hand, I suspect that Dehn's lemma is used at the very last step of the Perelman's proof when he gets a collapsed 3-manifold and concludes (using collapsing theory and some topological results) that this must be a graph manifold.

As for whether "the three dimensional saga is done and over with and we can go ahead to four dimensions", well, there is still a lot of work for 3d-topologists, but the whole area has now become somewhat less exiting, and no so central anymore. Which is okay, in fact the area where Perelman was working all his life has never been a central area of math (until recently anyway).

81. **Gina**
September 21, 2006

Dear Geometer,

Many thanks for the interesting information. I do not know what a garph manifold is (never mind that) but I am very happy to hear that the proof of the Poincare conjecture still relies on the work of that dear man "Papa", Christos Dimitriou Papakyriakopoulos. From what I heard he was a very special person.

You said, "It'd be much more exciting if the Poincare Conjecture were false; unfortunately this is not the case. "

I beg to disagree with you on this point. The way I see it, it is exciting that the Poincare conjecture was proven true and it would have been exciting had it was proven false and perhaps, the most exciting thing is that we could not have known in advance. Not what will the answer be and not even if people will be able to crack this problem at all. Probably sometimes it looked going this way and sometimes it looked going the other way and sometimes it looked stucked.

By the way, before going to dimension four, is everything known about manifolds in dimension two?

82. **Peter Orland**
September 21, 2006

To Me:

Hilbert did get the action first, but not the right variational principle. He first found wrong field equations. In so doing, he duplicated an earlier wrong result for the field equations with matter, which Einstein had already published. The wrong result sets the Ricci tensor (not the Einstein tensor) proportional to the energy tensor. Since the Ricci tensor isn't covariantly conserved, these equations aren't consistent with local energy and momentum conservation.

The story I heard is that while Hilbert's paper was in press, he heard that Einstein had found different (and correct) field equations. These were obviously right because they WERE consistent with energy and momentum conservation. Hilbert then changed his paper's proofs before Einstein's article came out. In this way he was the first to publish the right result, though not the first to obtain it.

Of course the vacuum field equations (without matter) were published out by Einstein and his assistants a few
years earlier (1913).

83. **geometer**
September 21, 2006

Gina asked: "is everything known about manifolds in dimension two?"

Their classification is classical (pretzels with many holes and all that), but there are still some mysteries about surfaces, eg the studyng the mapping class group (ie the group of self-homotopy equivalences of a surface) is a very active area of research involving several brunches of mathematics, and there is an enormous literature on the subject.

84. **jeremy**
September 22, 2006

I really hope this Yau-Nasar fight will end soon and will end without being in court. It will be painful to see all the quoted mathematicians in the New Yorker article being dragged to the witness stand and to have them been quoted again, under oath this time. It will be unbearable to see their "beautiful mind" being fried by the lawyers in front of the whole world. It is better to leave this depressing saga behind without further damaging the mathematics community.

85. **Thomas Larsson**
September 22, 2006

The Einstein-Hilbert story (at least Todorov's version of it) can be found in physics/0504179.

86. **Juan R.**
September 22, 2006

Disputes of this class are often frequent on science. Usually one obtain a better perception of the history years after when historians do their work and check for all documentation they can find.

Take the case of Hilbert-Einstein. During decades people asked why Hilbert, if obtained the GR action first, did not claim priority. Well, the reply is that Einstein said not the true to Hilbert then as proven in this recently discovered mails:

(15 November 1915) Einstein to Hilbert:

Highly esteemed Colleague,
Your analysis interests me tremendously, especially since I often racked my brains to construct a bridge between gravitation and electromagnetics. The hints your give in your postcards awaken the greatest of expectations. Nevertheless, I must refrain from travelling to Göttingen for the moment and rather must wait patiently until I can study your system from the printed article; for I am tired out and plagued with stomach pains besides. If possible, please send me a correction proof of your study to mitigate my impatience.
With best regards and cordial thanks, also to Mrs. Hilbert, yours.

Popular physisicist's claim that Einstein was pionner in the search of an unified field theory is plain wrong. Hilbert was already working in unification!

Hilbert send a copy to Einstein of his paper on general relativity presented on November 16 at the Göttingen Mathematical Society. Hilbert's paper was submitted to print on Nov 20. Einstein replied:

[…] The system you furnish agrees – as far as I can see – exactly with what I found in the last few weeks and have presented to the Academy […]

BUT Einstein's reply was not accurate! Einstein did not obtain the correct equations of gravitation weeks ago like he claims in above correspondence, because Einstein presented a paper to Academia the day 4 (Nov 1915) containing the incorrect equations, and the day 11 submitted another paper containing again the incorrect field equations.

During years, Einstein agonized without obtaining a relativistic gravity. Only after of reading correct equations on Hilbert paper (presented the day 16, a copy sent to Einstein the day 18, and published the day 20), Einstein corrected his wrong equations and submitted the famous paper of day 25 containing the correct field equations. Moreover, as correctly noted by I. Todorov in above cited preprint, Einstein proposed *without any derivation or rationale* the correct equations of general relativity in his final paper of day 25. What is more, Einstein just 'forgot' cite or even acknowledge Hilbert crucial assistance.

Each one can obtain her/his own conclusions.

About the Science article cited above. It has been critized to be inaccurate and even sensationalist. It claims that the recently discovered gallery proof did not contain Hilbert action but the article failed to explain that proof was mutilated. That is, the paper did not contain the Hilbert equations because someone cut a third of the piece with the equations here!

Therefore together above petitions for NS and PR, i add Science journal also!

Juan R.

Center for CANONICAL |SCIENCE)

87. **Cynthia**
    September 22, 2006

    Does anyone know why Arkani-Hamed thinks that the doors to the landscape must open if split supersymmetry is revealed in the next run of LHC experiments? I'm uncertain as to why he's absolutely closed to the idea that the "other part of split supersymmetry is detectable at even higher energy levels. By the way, I'm not surprised to discover that Arkani-Hamed made the top ten list of brilliant scientists. Not that my opinion has any worth, I do, however, find him to be one of the most intriguing thinkers in the field of theoretical physics.

88. **Anonymous**
    September 22, 2006

    Peter, regarding earlier comments, I do know that Kleiner and Lott will be submitting their paper to a refereed journal, if they haven't already, and although I had heard that the Clay people were considering waiving the "refereed publication" requirement for Perelman, the two year wait will (almost certainly) stand.

89. **TheGraduate**
    September 22, 2006

    I think the issue of who gets the million dollars is minor. A million dollars is a lot of money to be given but I think most of the people involved in this controversy could probably make plenty of money on wall street if they were so inclined.

    I bet many companies would pay through the nose to get mathematicians of such quality and distinction in their financial mathematics departments.

90. **MathLover**
    September 22, 2006

    Peter and TheGraduate,

    You were talking about two jobs from same person. Today's issue of Science magazine has two articles under their News Focus column:

    SCIENTIFIC WORKFORCE: Frustrations Mount Over China's High-Priced Hunt for Trophy Professors
    http://www.sciencemag.org/cgi/content/summary/313/5794/1721

    SCIENTIFIC WORKFORCE: Many Overseas Chinese Researchers Find Coming Home a Revelation
    http://www.sciencemag.org/cgi/content/summary/313/5794/1722

    This will be helpful to know some background for one issue raised by the New Yorker article.

91. **TheGraduate**
    September 22, 2006

    Mathlover,

    Sorry, I can't access those articles.

92. **MathLover**
    September 22, 2006

    TheGraduate,

    Here is a link to the first article:

    http://blog.sina.com.cn/u/4aaaf369010005t0

    Go to the bottom of this blog and click number 6 from number series 1 to 6. There you can simply search word frustrations and find the article in a number of subsequent comments.

    This is the place where a Peking University math professor (Weiyue Ding) sets up a blog for the Poincare conjecture's Yau drama. I hope you will find the English article in a sea of Chinese characters.

93. **John Baez**
September 22, 2006

CapitalImperialistPig writes:

> John Baez,
>
> While I can't disagree with anything you say, I have to say I'm sorry you never wrote those articles for Scientific American. There is something to be said for writing for the intelligent layman.

There definitely is, and I want to do a bunch of that sometime. But, I probably want to do it in some way that's either free for the reader (like on my website), or will make me some serious money (like a sensationalistic, overhyped best-selling book).

Right now I'm having fun writing expository stuff for people who already know a bit of math and physics. There's a kind of market niche here that I seem to fill: math and physics have gotten so complicated that even most mathematicians and physicists can use a lot of help understanding it.

94. **TheGraduate**
September 22, 2006

MathLover,

Thanks. Quite interesting article. It reminds me of the worries about losing manufacturing jobs to China that is prevalent on the American side of the ocean. In this case, you have people in China worrying about academics that collect salaries while spending most of their time in America and then you have companies like Walmart that produce massive quantities of product in China but still want to be seen as American companies.

Globalization is good. Nationalism is so 19th century. I say we leave it to the universities to figure out whether they are wasting their money or not. My guess is they aren't.

95. **Peter Orland**
September 22, 2006

Juan R.

Your version of the Einstein-Hilbert question is very different from news reports that come out a while back. Be that as it may, General Relativity was invented by Einstein and collaborators, not Hilbert, in 1913, when they published the vacuum field equations. The issue of whether Einstein or Hilbert has priority concerns only the modification of these field equations with energy. The most revolutionary physical and mathematical ideas were in the 1913 work. One could argue that the field equations in the presence of energy was then going to be found by someone eventually.

96. **Juan R.**
September 23, 2006

Peter Orland, in short

Einstein recognized in several writtings that his 1913 work (so called Einstein–Grossmann theory) was wrong.

Einstein published many contradictory theories in subsequent years, in early 1914 returned to a scalar theory but in the last part of 1914 returned again to the metric theory of 1913 with modifications.

Hilbert presented objections to Einstein theory (1914 version) and since 1912 he was working in a unified field theory.

Einstein learned from Hilbert and contacted with him waiting his review of Einstein new works. Einstein published a series of subsequent papers with different theories (rejecting his previous ones and embracing Hilbert's criticism to previous versions) until the final work of 25 Nov 1915 containing the right version of GR. **But** Hilbert had did first!

General relativity is an outcome of the work of many people including crucial contributions from mathematicians as Poincare or Grossman. The GR action and the correct field equations and basic principles such as that of general covariance (initially rejected by Einstein) were pionerized by Hilbert.

I wait many distorsions of the history of relativity in physicists' textbooks can be eliminated in a future thanks to more accurate historical presentations. My posting here is my small tribute to Hilbert, Poincare, and others mathematicians.

Juan R.

Center for CANONICAL |SCIENCE)

97. **Peter Orland**
September 23, 2006

Juan,

I am not sure I should reply and keep this discussion going, but the idea of introducing a curved metric into gravity and the vacuum field equations is not due to Hilbert. Hermann Weyl, Hilbert's Goettingen colleague, in his book "Space-Time-Matter" did not give Hilbert credit for the idea of general relativity in 1913. If you have a copy, take a look. Weyl certainly does give credit to Hilbert for the action principle, which Hilbert deserves.

The only debate is who first got the details of the equations with matter in 1915. I think you are wrong on this too, but even if you were right, the 1915 result is much less significant than the 1913 vacuum field equations. Eventually someone would have got this right, whether Einstein, Hilbert or somebody else. Hilbert did not invent the principle of equivalence. Hilbert did not introduce the metric and curvature into gravity. Hilbert was not the first to realize the importance of the stress tensor. Hilbert did not find (exact or approximate) solutions of the vacuum field equations, or find their experimental consequences.

Like many ideas, general relativity is the work of many people, but
Einstein's work was primary, even if he made mistakes and changed his mind from time to time. After nine decades, some mathematicians still can't stand the fact that Einstein's contributions to relativity and gravity soar above their own. Why? I don't claim Einstein invented Riemannian geometry.

98. **Juan R.**
September 23, 2006

Peter,

From my part i do not desire to continue this discussion with you, since you continue putting in my fingers stuff i never wrote (this is the second time you are doing this).

I never said that Hilbert introduced the metric theory the first time and i already remarked the contributions of people as Grossman (precisely regarding some incorrect thoughts of Einstein regarding the nature of the metric tensor). Of course Einstein was not piooner here!

I never said that Hilbert were the only father of general relativity just remarked the field equations dispute therefore I fail to appreciate your criticism.

Hermann Weyl, was not a historian (was him?) and his 'personal' opinions about facts is a secundary source when compared with direct historical analysis of papers published by all people currently in the historians target.

It is not true that the only debate was "who first got the details of the equations with matter in 1915." The whole point is a little more complex than you try to present here.

"Hilbert did not find (exact or approximate) solutions of the vacuum field equations, or find their experimental consequences."

I would recommend you updating your sources since in last few years a number of very interestings works analize with care the history of relativity. You could begin with some of those:

Jürgen Renn and John Stachel, Hilbert's Foundation of Physics: From a Theory of Everything to a Constituent of General Relativity.

arXiv:physics/0504179v1

Friedwart Winterberg. Zeitschrift für Naturforschung (2004) 59a, 715-719.

Daniela Wuensch, "zwei wirkliche Kerle", Neues zur Entdeckung der Gravitationsgleichungen der Allgemeinen Relativitätstheorie durch Einstein und Hilbert. Termessos, 2005, ISBN 3-938016-04-3

http://arxiv.org/abs/physics/0405075

"After nine decades, some mathematicians still can't stand the fact that Einstein's contributions to relativity and gravity soar above their own. Why? I don't claim Einstein invented Riemannian geometry."

Now i can see you clearly…

Juan R.

Center for CANONICAL |SCIENCE)

99. **Peter Woit**
September 23, 2006

Peter and Juan,
This Hilbert/Einstein argument is completely off-topic and not going anywhere. I'll delete any more attempts to continue this argument here.

100. **Mathematical Truth in Chinese**
September 23, 2006

On September 23, 2006

Here are some facts found in the Chinese world on the Yau case. Many puzzles in this case can be answered from these simple facts.

[Long comment including attacks on Morgan and Tian deleted]

101. **Curious Bystander**
September 23, 2006

1. Here is a quote from Cooper's (Yau's lawyer) letter:
"Professor Manin made clear that the mathematical community felt that work remained to be done to complete Givental's arguments"

2. And here is a post in the New Yorker forum:

"Although it may be uncertain what is the consensus, and remain unknown if Manin indeed suspected a flaw in Givental's arguments, it seems quite clear however why Yau's defenders refrain from quoting Manin's statement in full. It turns out that when the passage is read in its entirety, the conclusion that "some work remains to be done in order to complete his arguments" applies to the paper of Yau et al as well.
Here it is:
"Givental [Giv2] achieved a remarkable progress in proving the Mirror Conjecture for complete intersections in toric varieties where the precise construction of mirrors is due to Batyrev ([Ba1], [BaBo2]). He enriched Kontsevich's approach by passing to the equivarinat quantum cohomology. Some work remains to be done in order to complete his arguments."
As one can see, Manin is talking here about the Mirror Conjecture for complete intersections in toric varieties as formulated by Batyrev and his coauthors. As it is well-known to specialists this conjecture, strictly speaking, still remains open. The paper [Giv2] deals with complete intersection in projective spaces, i.e. simplest examples of toric varieties. In this case, the Mirror Conjecture cannot be attributed to Batyrev as it was known before his work (and this fact, no doubt, is known to Manin, as well as Yau). Thus, when taken literally, Manin's statement views [Giv2] as a "remarkable progress" toward a proof of the general Mirror Conjecture, but contrary to the claim of Cooper's letter, it does not indicate that [Giv2] per se is incomplete. "

102. **woit**
September 23, 2006

I'm shutting off comments on this posting. Too many people are trying to use it as a Tian-Yau battlefield. I think this behavior is disgraceful and I want no part of it.

The Goldilocks Enigma

September 22, 2006
Categories: Book Reviews

Paul Davies is an author of many popular science books, often dealing with topics in cosmology and particle physics. He has been based in Australia for the last sixteen years, but is now moving to the US, taking up a new position at Arizona State University, where he will establish a new center he describes as a “cosmic think tank”.

He also has a new book coming out, entitled The Goldilocks Enigma: Why is the Universe Just Right For Life?, and a major concern of this one is the multiverse and anthropic reasoning. I was asked to write a review of the book for the British Magazine New Humanist, and the review has appeared in their September/October issue. One reason I agreed to do the review (besides the fee in the upper two figures) was that I thought I might write about the book here anyway. Here’s the text of the review. It’s somewhat different than my other postings here, since it’s written for a much wider audience and constrained by space limitations to be rather short. As a result, it unfortunately doesn’t go as deeply as I would have liked into discussing some of the issues raised in the book.

Review for New Humanist

Paul Davies’ new book The Goldilocks Enigma wrestles with some of the deepest philosophical issues around, but concentrates on one in particular: “why is the world the way it is?” He approaches this question through a discussion of a hot topic in theoretical physics that most scientists refer to as the “Anthropic Principle”, but which Davies chooses to label the “Goldilocks Enigma”. This refers to the fact that the physical laws that govern the universe are “just right” for the development of life. Relatively small changes in certain parameters would make it uninhabitable by the likes of us and we wouldn’t be here.

What should one make of this? Religion has a quick explanation, that God set things up so that we can exist. “Intelligent Design” is the currently popular name for explanations of physics or biology that invoke a higher intelligence that chose to make the world the way it is. This explanation suffers from the lack of any way to ever test it.

Davies spends much of the first half of the book providing an introduction to the modern scientific view of physical laws and cosmology, working up to the latest and trendiest of these. For more than twenty years now, theoretical physics has been dominated by a very speculative idea known as “string theory”. Very roughly, this involves replacing elementary particles with objects more like loops, and it crucially requires six extra dimensions beyond the three space and one time dimension we’re familiar with.

One must do something like wrap up the six dimensions to make them unobservably small, but then the properties of particles and thus our physical laws depend on how this is done. Initially there was much optimism that there would be only a small number of consistent choices for how to handle the six dimensions, and one of these choices would agree with what we observe. Recent results in string theory appear to show that this isn’t the case; instead an unimaginably large number of possibilities exist. Indications are that if one can get our observed universe this way, one can also get just about any variation of it, and legitimate scientific predictions are not possible.

Instead of abandoning string theory as a hopeless cause since it can’t predict anything, some string theorists have chosen to promote the idea that our universe is just part of a “multiverse” of all the nearly infinite possibilities allowed by string theory. One of the few thingsone can then predict is that we must be in a part of the multiverse that is “just right” to allow our existence. Debate rages amongst physicists over whether or not this idea is really testable and thus scientific.

Davies provides a careful description of this currently popular multiverse scenario and its explanation for why things are the way they are, including some mind-boggling implications involving infinite numbers of copies of ourselves, and the possiblity that the universe is a simulation. He contrasts it with the common belief among many physicists that there is a simple unique mathematical structure underlying the physical laws that describe the universe. The problem he sees with this belief is that there’s no reason to expect that such a mathematical structure should pick out exactly the parameters that are “just right” for life. But then again, does it really make sense to have any expectations about this? It’s not clear that a sufficient answer to the question “Why is the universe just right for life?” isn’t simply: because otherwise we wouldn’t be asking the question.

The last chapter of the book moves away from conventional points of view among physicists to some much more speculative answers to the “why is the world the way it is?” question that Davies finds appealing. These involve some version of the idea that life itself is in some way or other built into the laws of the universe, that they inherently lead to the evolution of life. He looks to information theory and quantum mechanics for hints of how this might come about. Like the multiverse, this kind of speculation tends to suffer from a lack of any known way to test it. The hallmark ofthe scientific method is the insistence that theories have the property that one can confront them with experiment in a way that allows one to decide whether they work or not. One’s answer to the “why is the world the way it is?” question should be a theory of this kind.

Davies concludes with the admission that, in the end, he finds all the different answers he has examined to be wanting. He notes that we’re the evolutionary products of the pressures of a specific environment, and only recently beginning to be liberated from these. Our minds may still be far too crude and our knowledge of the universe too fragmentary to allow us to perceive the correct answers to these existential questions. In the meantime, Davies has provided an engaging and very readable account of the range of answers we have come up with so far.

Comments

1. **alex**
   September 22, 2006

   When you write:

   ‘It’s not clear that a sufficient answer to the question “Why is the universe just right for life?” isn’t simply: because otherwise we wouldn’t be asking the question.’

   Is that you stating your opinion, as opposed to you quoting Davies? I read it that way, but it’s anthropic reasoning…

2. **Peter Woit**
   September 22, 2006

   Alex,

   That’s my opinion, and I guess it’s anthropic reasoning, but the question in my mind is whether anthropic reasoning here is anything but tautology. Is it sensible to ask “why X?” when “not X” implies that you can’t even formulate the question.?

   My problem with anthropic reasoning isn’t that it is never a legitimate form of logical argument, the problem is that it may not be science, since you can’t use it to make falsifiable predictions.

3. **urs**
   September 22, 2006

   > a simple unique mathematical structure underlying the physical laws that describe the universe.

We can have such a simple unique mathematical structure underlying the *laws* of the universe, and still have many possible solutions obeying these laws.

And this is how it has been for all our existing theories – always. Anything else would be rather shocking.

Alain Connes proposes a unique mathemtical structure behind the laws of the observable universe (the spectral action principle [#](#)). And yet, our world is described by just one out of infinitely many possible solutions (here: spectral triples) of this principle.

4. **Cristina**
September 22, 2006

Hello, I wandered here from John Baez's blog 🙂

I agree with your comment above, because the anthropic principle has always struck me as a vicious circle.

So the physical laws are good for the development of life as we know it — good. But it's a mistaking of cause and effect, because the Universe could be entirely different, composed of other basic building blocks, obeying entirely different physical laws, and still contain life, albeit absolutely different from what we call "life". (And those beings would probably have an anthropic principle of their own! :D)

The Universe is not here to allow us to exist — it is we who are here because the Universe in which we live is the kind of Universe which enables life similar to us to exist.

It all depends on how one defines life, really.
It's like wondering how come that it was exactly my parents who got to be together and, as a result, how come that it was exactly me who was born 🙂

Cheers,
Cristina.

5. **Vicky**
September 22, 2006

Nice review. It makes me want to read the book. (I hope that was consistent with your intention.)

I was unclear, however, about your concern regarding the notion of building the life into the fundamental laws of the universe. I can see why this might be directly untestable, but isn't it possible that such a dependence could eventually be logically derived from other, testable propositions? Surely valid science can permit untestable conjectures that are inevitable consequences of testable ones.

I ask because it sounds, from your brief description, like a line of thought that may be worth pursuing, and it beats some of the alternatives. I will have to read the book to see if his musings are pure conjecture or if they may have some merit.

6. **alex**
September 22, 2006

Thanks Peter,

If I may give my answer to the question in your reply…

In general whether or not "why X?" is a sensible or fruitful question is unrelated to any implications that "not X" may have for ones existence. For instance, I can learn something from asking why I survived the accident that might have killed me a few years ago. But I probably won't learn much if I am content with the answer that if I hadn't I wouldn't be asking the question. The question may or may not have a useful answer, but that's a different issue.

"Why is the universe just right for life?" and its big brother "Why is there anything at all?" may or may not have answers within human reach, but "Otherwise we wouldn't be asking" isn't an answer. It probably isn't even much of an indication as to whether they are likely to turn out to be fruitful questions.

7. **Tim**
September 22, 2006

May I make a brief comment on your claim "Religion has a quick explanation, that God set things up so that we can exist… This explanation suffers from the lack of any way to ever test it."

This suggests an unreasonably limited epistemology. A claim of Christianity, for example, is that God has intervened in history, supremely as a person (Christ), and communicated with people on topics including why we exist. Whether God has indeed intervened in world history can be subjected to tests in a similar way to a claim such as "Emperor Hadrian intervened in British history." For example, we can examine the quality of witness accounts and other documents, and we can look at the results of alleged interventions. Additionally, in the case of Christianity, one can ask whether the revelations amount to an unreasonably profound understanding of the human condition and whether prophesies have been fulfilled. If one concludes God has indeed intervened and spoken to us, one can seek an answer to whether He set things up so that we can exist, within what he has said.

Granted, this is not straight-forward! But I simply want to make the point that there are other reasonable routes to knowledge than the scientific method and those routes may be pertinent to the question you are addressing.

The anthropic principle is an emotive subject because it relates to the presence of a greater purpose. Perhaps one more example (due to John Lennox) might help in this context. Imagine Aunt Joan bakes a cake. Chemical analysis can determine the ingredients. You can write papers on the material properties of sponge and icing sugar. But whatever tests you perform on the cake, you will not discover why Aunt Joan baked it until she tells you it was to celebrate her grandson's birthday.

8. **Who**
September 22, 2006

http://www.amazon.co.uk/Goldilocks-Enigma-Universe-Just-Right/dp/0713998830

this says that the amazon.co sales rank of Goldilocks Enigma is currently 745 in the UK (among all books)

this is a pretty high standing. people in the UK are pre-ordering the book which is supposed to be available 28 September.

the amazon price is about 13 pounds

amazon.co.uk have paired it with Dawkins "the God Delusion" to make the usual two-for-less package deal.

9. **A Babe in the Universe**
September 22, 2006

Paul Davies CB (Order of the Bath) will be a huge addition to ASU and the USA too. He believes that the fundamental parameters are the result of a deeper principle. He has also written in Nature about the changing speed of light!

10. **John Baez**
September 22, 2006

I wonder if Davies raises the question: to what extent is the universe really "just right" for life?

If we saw life teeming throughout the universe, on every planet, asteroid and comet, then I'd say the universe was "just right" for life. In fact we've only seen it on one planet.

Maybe in fact the universe is not "just right" for life. Maybe life is not so tender and delicate as we think, either. Maybe life arises whenever conditions permit a sufficiently complex set of reactions. Maybe the Earth is the only place in the universe where this happened, maybe not.

It seems way, way premature to start wondering about *why* the universe is "just right" for life, before we know whether it *is*.

11. **TruthSeeker**
September 22, 2006

What a beautiful, well-written message, Tim. You have a way of explaining abstract concepts to the general public that even children can understand.

Sometimes we scientists do have a tendency to overanalyze everything. But perhaps – just perhaps – there is a simple explanation of why we are here, and of other related questions?

They say that it is incompatible to be a good scientist and believe in a Creator at the same time. But I don't see why it needs to be the case. The scientific method has been proven to be an appropriate tool to study the natural world. But perhaps it is the wrong tool to use to test anything in the supernatural world, where supreme beings reside? It would be like using a ruler to measure temperature, for example. Just because something cannot be tested by science does not mean it does not exist.

12. **John**
September 22, 2006

The scientific method can't hope to answer every question in fact one can't show, via the scientific method, that any question pertaining to the to the universe can be answered via the scientific method.

In simpler terms, one cannot use the scientific method to prove the validity of the scientific method, as a tool to answer questions about the universe. Hence, the answer to some questions about the universe may reside outside the scientific method.

13. **dave tweed**
September 22, 2006

@Tim: I think you're missing the point whe you say "The anthropic principle is an emotive subject because it relates to the presence of a greater purpose." The anthropic principle is emotive because it's not remotely clear whether it contains such a big vaguely defined concept (that we know what properties life has and what lower-level physics leads to/is incapable of leading to them) that any current _application_ of it seems to many to be untenable.

To give a completely impractical thought experiment: if we could somehow run simulations of a suitably huge sampling of all possible "rules for existance substrates" for suitably long "simulated times" AND we had artificial intelligences that were able to hunt through for any sign of "life" even if it didn't have the form we're used to AND they found no sign of anything like life, THEN I'd be more comfortable with the anthropic principle. (Note the point of the simulations isn't to test possible physics but to see if we can generate something that we'd class as life that doesn't have the characteristics of all the life we've seen so far.) If you'd firmly established this level of understanding that is presupposed by the anthropic principle, I wouldn't have any problems accepting as legitimate questions about whether this leads to "greater purpose" or "supreme beings".

The discomfort with the anthropic principle for many people is the jump from "I can't imagine any life which doesn't have this property" to "it is not possible for there to be life which doesn't have this property" in order to "get answers to question x" rather than accept "we can't get the true answer to the question x yet".

14. **Tim**
September 22, 2006

TruthSeeker – thank you for your comment.

dave tweed – I think the anthropic principle is found to be emotive by different people for different reasons.

15. **George**
September 22, 2006

RE: The Goldilocks Enigma.

I think the right question is really – Why is life just right for the universe?

16. **Gumbi**
September 22, 2006

Seems to me that the anthropic principle is not there so much to offer an explanation, as to suggest that no explanation is needed.

We don't wonder too much why earth is suitable for life, because we see that there are at least a few and probably very many other planets out there that are suitable to life to varying degrees.

But if there were only one planet in the universe, and it were earth, then the question of why this one planet was suitable for life would demand an explanation.

The anthropic principle, combined with multiverse concepts, suggests that some mysteries are not mysterious at all, and don't need explanations, just like the case of earth being so suitable for life.

17. **TheGraduate**
September 22, 2006

I think in general the anthropic principle is somewhat redundant and political. We all know that we may fail to find an answer to any question we seek to find an answer to. In that sense, the anthropic principle is redundant. Because it is so redundant, the only reason to bring it up is to convince people not to try and in that sense it seems political.

I consider the anthropic principle something like the 'god did it' principle. As long as this kind of idea is not part of making concrete progress, it seems a waste of time to focus on it.

18. **Chris W.**
September 23, 2006

TheGraduate,

Right. More specifically, it seems to me that the underlying premises of Christianity in treating these questions have been (1) apologetics – convincing unbelievers and doubters that God really exists, and (2) that rational investigation of nature without presupposing God's existence is tantamount to sin, and is the sort of thing that got us kicked out of Paradise. Therefore we have no business doing it. The existence of the world and ourselves as part of it ceases to be a mystery. Instead, it becomes a moral drama.

Returning to the subject of whether the existence of life tells us anything about the structure of physical law, the key point which always seems to be ignored is the role of physical law in accounting for any kind of *stability* in physical systems, and also the limits of that stability. Clearly, living things need some degree of stability or reproducibility in their environment and in their own constitution, combined with an allowance for change, the latter requiring some degree of *instability*. Some sort of balance needs to be struck, and the laws of physics are central to how it is struck and whether such a balance is possible. Of course there is something deeply reflexive about this, because the laws of physics are, by assumption, stable themselves. This begs the question, how do we account for the stability and universality of the laws?

There has never been an absolute answer to this question. When putative laws have been discovered to be limited in scope, ie, to fail under some circumstances, we have sought to understand this failure and *their prior success* in terms of a deeper and more all-encompassing law. The central question about a failed law is always "why and how did it ever work?" The relevance of relative velocities and the speed of light only became fully apparent with the advent of special relativity. Without it, the problems of reconciling Newtonian mechanics and Maxwell's electrodynamics and various observational anomalies could only be puzzled over.

I think we're at a point in the history of physics where the question of why nature has any law-like structure at all must be squarely confronted if we are to understand why it has the *particular* law-like structure that it does. In this connection, I should note that John Stachel has argued that Einstein's well-known objections to the indeterminacy of quantum mechanics were not due to its indeterminacy as such, but rather to the fact that the *degree* and form of the indeterminacy was unexplained. If we're going to admit some indeterminacy, then why not go all the way? Again, the subtle balance between indetermininism and determinism is the core issue, and calls for an explanation. Life seems to require it, almost by definition, but this fact throws little light on the fundamental basis of the balance.

19. **Arun**
September 23, 2006

The questions which currently interest us may not be answerable by application of the scientific method at this time. Progress in science as it is at a particular time depends on asking the right questions. If science is seen to be turning into philosophy, it is perhaps because we are not asking the right questions. Perhaps it is because the right questions at this time are relatively tame and boring compared to having theories of everything, and HEP theorists are no longer psychologically suited to tame and boring.

20. **TheGraduate**
September 23, 2006

Chris W. ,

Great comments. I think that to tackle the anthropic principle scientifically is at the moment a very lightweight approach and one really needs to read some of the analyses put forward by the philosophers to get to ideas with any kind of heft.

In defense of some christian philosophers, I think the motivations for their investigations are as you describe. However, I would not say that all their arguments fall into the categories you outlined.

One thing that Christians sometimes argue is that their way of looking at things is a complete theory of everything in that it attempts to describe all phenomena both physical and mental. In the sense that science has nothing to say on the question of 'what is a good life?' which is of incalculable importance to most human beings, Christians view the scientific enterprise as a quest toward a partial theory of everything where 'everything' for them includes intellectual, sensory, moral, emotional and physical phenomena in both the objective and subjective perspectives.

In a way, the scientific conclusion of the 'consistency' of physical systems is quite conditional in that one may take certain drugs; one may have dreams; one may have what is defined as a 'mental illness'; one may simply experience a state of mind that one tends to retroactively define as 'confused'; and during these times the rule of 'consistency' is thrown out the window.

To even approach the scientific method, one must lay down much conceptual frame work. For instance, one has to hypothesize that human beings have a certain frame of mind in which it is possible to do science and that the 'reality' of other frames of mind are invalid.

21. **John**
September 23, 2006

The question itself raises some problems; we are assuming that "natural processes", as we know them, held before or at the "creation" of the universe. In addition, physics can only describe the behavior of preexisting processes and material, but not "creation" where creation means something out of nothing. This goes deeper than the appearance of particles out of the quantum void, for you need laws to govern that void before anything can come from it, hence something exists.

Modern science always presupposes the existence of an underlying law governing the behavior of material objects. The problem is what happens when we ask about the origin of those very laws? We are in a quandary, we presuppose an underlying law, but wait the origin of the universe is the origin of all physical laws, so what kind of underlying law can govern the "creation" of the universe? We can't say its a physical law for ontologically speaking it must come before the physical laws, hence its a "meta-physical" law. Even if we did find an explanation via physical laws, we can always ask why those laws came about as they did and so on to infinity.

This is what I mean when I say the scientific method cannot validate itself; it always needs a preexisting underlying structure.

22. **Neznaika**

September 23, 2006

Peter,
Thank you for the interesting review. My question is about your earlier comment that one cannot use anthropic principle to make falsifiable predictions.
How about "Principle of Mediocrity"? I found a very interesting recent article on the subject by A. Vilenkin on Edge.com
(http://www.edge.org/3rd_culture/vilenkin06/vilenkin06_index.html). In that article and apparently in his new book, Many Worlds in One –
(http://www.amazon.com/gp/product/0809095238/qid=1151507616/sr=1-1/ref=sr_1_1/104-5849948-3641559?s=books&v=glance&n=283155),
Vilenkin argues that one can make testable STATISTICAL predictions using the Principle of Mediocrity. He also claims that his (and Weinberg's) prediction for the cosmological constant has already been confirmed.
What do you think about those arguments? Is that Science in your view?

23. **Neznaika**
September 23, 2006

Correction to my post:
Vilenkin's article has been published on Edge.org, not on Edge.com. Sorry about that.

24. **Who**
September 23, 2006

Amazon.co.uk has The Goldilocks Enigma in stock and is shipping.
http://www.amazon.co.uk/Goldilocks-Enigma-Universe-Just-Right/dp/0713998830

they guarantee delivery by 1 PM Tuesday 26 September, if ordered now, so I guess they mean business.

Goldilocks has UK salesrank #716 at the moment, which is very high for a physics book. For comparison, Not Even Wrong had UK amazon salesrank #1318 last time I looked.
And Hawking Brief History of Time (paperback) had rank around #4700.
so Davies book having UK salesrank 716 is really quite good

25. **Chris W.**
September 23, 2006

Neznaika,

The strength of Weinberg's argument has been questioned a number of times. See hep-th/0407174 for a recent example. It has also been questioned whether the argument is truly "anthropic", ie, whether the existence of life plays anything more than an incidental role in the argument.

TheGraduate: *To even approach the scientific method, one must lay down much conceptual frame work. For instance, one has to hypothesize that human beings have a certain frame of mind in which it is possible to do science and that the 'reality' of other frames of mind are invalid.*

This statement strikes me as quite unfounded. As my comment should have indicated, science can be seen as a natural outgrowth of grappling *consciously* with a problem that faces all living things. We must find and learn to rely upon some stability in the world for the sake of simple survival, if nothing else. The growth of science was galvinized by the gradual and surprising discovery that a certain kind of stability—stability of certain deep patterns of change—could be found in the world, and could be precisely and testably described, well beyond what seems practically necessary.

26. **TheGraduate**
September 24, 2006

Chris W.,

I am not quite sure what you mean. Could you elaborate? I understand that by stability you mean being able to replicate the results of experiments. But I think there is definitely a difference between the sort of investigative method that leads to science and the sort of reasoning people employ when they conclude things like: the sun will rise tomorrow, God is always on the side of the righteous and that poor people always steal. Nevertheless, on a subjective level, I think there is a sense for the people that believe these things that these are stable, replicable rules.

27. **John Bussoletti**
September 24, 2006

I've always wondered why those who espouse Intelligent Design seem to get so involved with questions of natural selection and the ideas of Darwin, when there really are some very basic aspects of nucleosynthesis that have even given me pause on occasion. Not that I'm espousing anthropic or Intelligent Design principles, but consider the following:

There is evidence that the chemical elements that make up our universe are created through nucleosynthesis processes in stars. "Hydrogen burning" creates helium. "Helium burning" critically depends on a "triple alpha" process, which is predicated on the existence of a 7.65 Mev excited state of Carbon to allow the production of Carbon. And from Carbon, production of all other elements follows

So there is in the nucleus of Carbon an excited state with zero net angular momentum at an energy level of 7.65 MeV, which is juist above the dissolution state of the nuclues into Berillium 8 (itself unstable to decay into two Helium nuclei) and an alpha particle. This excited state has an electromagnetic decay branch which emits a photon and decays to the 2+ excited state of the Carbon 12 nucleus, which is a bound state and itself decays by photo emission to the ground state.

Were the energy of this state of the nuclear system somewhat higher, the probability for a triple alpha interaction within solar interiors would drop precipitously, greatly reducing (eliminating?) the production of Carbon in our universe. Were it lower in energy, there might be no path for creation of Carbon. And without carbon, most other nuclei don't get produced.

The fundamental electromagnetic, strong and weak nuclear forces, and the values of the various undetermined constants in our various theories of particles and the universe as we know it, are just so, to allow the existence of this excited state, allowing synthesis of Carbon. Without the interplay of the various strong, weak and electromagnetic forces, there would be no carbon, let alone carbon-based life anywhere in this universe.

So, the Intelligent Design contingent, rather than worry about Darwinism, really ought to call greater attention to the very existence of Carbon.

Now, as much as the scientific method would like to argue against such an anthropic point of view, there is another fundamental problem, which was first explained to me by Jeremiah Ostriker in an Astrophysics course I took many years ago. In the early days of the course he explained the Copernican point of view by saying there are two fundamental assumptions that we make in Astrophysics: "First, we do not live an any special place in the universe. Secondly, we do not live at a special time."

With these two assumptions, we can make observations of that universe, build theories based on those observations, and have some confidence that those theories reflect some elements of truth.

The trouble with this is that these two assumptions are basically unprovable. That is, we're not able to subject them to the experimental test that the scientific method would dictate must be done. So as much as we might like to subject the universe to test, we're limited in time, space and even energy to explore only the very lowest excitations of the system. Our theories are nothing more than models that reflect those observations.

Robert Pirsig in "Zen and the Art Of Motorcycle Maintenance" quoted Bertrand Russell's description of science as "If bread is a stone and stones are nourishing, then I can eat bread and be nourished. So I do the experiment, I eat some bread and find that, indeed I am nourished, thus proving my theory that bread is a stone."

This is an extreme caricature of the scientific method, but unfortunately, it's also pretty accurate.

No matter what "fundamental" theory that one might propose, even should it explain "everything", the reality is that all we know how to do is construct models of the low level excitations of whatever it is that is in our universe at the particular time that we occupy it and in the particular place where we observe.

Landau had it right. All we can ever observe are effective interactions and so we're free to model them in whatever way is consistent with our observations.

But we can never prove correctness. We can only achieve utility.

That is, if our model is "good", it will allow us to build or control something. Basically, allow us to be engineers. But "Truth" and "Proof" elude us always.

So, the anthropic point of view directly opposes the Copernican assumptions. Both are unprovable points of view and both are largely irrelevant with respect to "utility". That's why Peter and others like him (I include myself among them) are so adamant that one must make predictions, testable ones, with whatever theory one creates. Without some ability of a model to make predictions, it has no "utility" and is, in the end empty. As much as science would like to establish proof, the reality is that the scientific method is largely a consistency argument, and in the end, is not provable.

But it can be very, very useful (sic).

28. **Adrian Heathcote**
September 26, 2006

"It seems way, way premature to start wondering about why the universe is "just right" for life, before we know whether it is."

Just a comment about John Baez's post.

I agree with this wholeheartedly. This is the point that no one seems to make in discussions of the Anthropic Principal. The most obvious thing about the Universe that we observe is that we seem to be alone in it. This may not be the case, of course. But that is the current evidence. People who talk about the fittingness of the universe to life must confront the difficult truth that it doesn't seem to be.

29. **Adrian Heathcote**
September 26, 2006

I think most people can see that the Anthropic Principle is a fallacy, the only real disagreement is what kind of fallacy it is. So let me add my 2 cents on this question. I think it is an example of what logicians call a "modal fallacy"—of which there are many examples in ordinary thinking. It is confusing a fact about the universe—that intelligent life exists—with a modal claim—that the universe *must* be such as to sustain life. But this is a case of inserting the necessity operator ("must") into the wrong position in the sentence. Fallacies that are generated by misplacing necessity and possibility operators are called modal fallacies.

The traditional statements of fatalism ("what will be must be") are likewise thought to be modal fallacies.

30. **Adrian Heathcote**
September 26, 2006

There is a second strand to the AP, evident in the post of John Bussoletti. The claim is that if the universe were different in some particular respect X then life would not be able to exist. Therefore since it does exist the universe *must* have this property X. This is the modal fallacy that I mentioned. The only thing that really follows is that the universe does have this property X.

31. **Neznaika**
September 26, 2006

To Chris W. and Peter,
Thank you very much for responding, Chris, since Peter is simply ignoring my question.
Actually, I don't care "whether the argument is TRULY "anthropic", (it's "anthropic" enough for me). Vilenkin's claims that 'The Principle of Mediocrity' is testable, since it can make VERIFIABLE statistical predictions and some of those predictions, for the cosmological constant specifically, have already been confirmed. I very much want to know if those claims are correct. I read the article on Edge.org and thought that Vilenkin makes a very convincing case but I am just a layman, I can't judge…
I am interested in Peter's opinion very much – he is extremely smart and seems to be the expert, so I asked him this very specific and simple question in response to his comments to Alex. I didn't receive any answer. Is he hiding, or he doesn't have an answer?

32. **Peter Woit**
September 26, 2006

Nezmaika,

I didn't answer your question because Chris already did, and I've written many, many times about these issues here. I'm just really tired of repeating the same points in response to the same overhyped claims. Once more:

The anthropic principle by itself is useless. It gives "predictions" that are tautologously true, so can't be falsified. The "principle of mediocrity", or more generally, the use of a multiverse model that gives an a priori statistical distribution of values of observables, combined with the anthropic principle as a selection effect, can in certain cases give predictions. If your multiverse model predicts a statistical distribution strongly peaked at a point, and that point is in the anthropically allowed range, then you should observe something near that value or the model is (probably) wrong.

The problem is that people are working with models (like the string theory landscape) that they have no control over and seem to have no useful structure, so people are just assuming the statistical distributions are flat. This is exactly the same assumption you make when you throw up your hands and say "I have no idea what is causing this", so, a priori, the distrbution of expected values is flat. You can't get something for nothing, and claim to be doing a serious non-trivial test of a model when the model's "predictions" are identical to those of just admitting you have no idea what is going on, so anything is equally likely. (I should note that Wilczek et. al have a calculation involving axions where they have some control over the a priori distribution, and it isn't flat, so maybe there is something more there).

The Weinberg "prediction" has been seriously overhyped, in many ways. First of all, it involves a flat a priori distribution of the CC values, so suffers from the problem mentioned above. It "predicts" a generic value of the CC in the anthropically allowed range, but that is also the "prediction" that comes from the model "the CC is determined by something purely mysterious such that I'll never know anything about its origin".

You can argue that this "prediction" is falsifiable: you may find that the CC has a non-generic value, e.g. very close to zero. If you observe this, then, you do have some information about the origin of the CC, it's not something random, but some unknown physics is giving it the non-generic value.

Despite the over-hyped claims you hear, this appears to be what is happening in the CC case. If you fix all cosmological parameters except the CC, you find that the observed value is somewhat smaller than expected, since it is one to two orders of magnitude below the top of the allowed anthropic range. If you allow other cosmological parameters to vary, the CC is much too small, many orders of magnitude below the top of the anthropic range. Vilenkin and Susskind's claims that the observed value of the CC is decisive evidence for the multiverse and landscape are absurdly overhyped.

If you take the string theory landscape seriously, there are lots of other similar "predictions" it makes which are just completely wrong. There appears to be no reason for the proton lifetime to be anything in particular on the landscape, but it is observed to be not generic, many orders of magnitude below the anthropic limits on proton decay. Same for CP violation, and lots of other things. If the people hyping these anthropic landscape "predictions" were honestly willing to give up their model when these "predictions" failed, that would be one thing. They're not, they don't take their own "predictions" that they are getting from these vacuous models seriously, so I don't see why anyone else should either.

33. **Neznaika**
September 28, 2006

Peter,
Thank you very much for taking time and answering this truly primitive question. I didn't realize that you've done it many, many times before, and I apologize for that. Your explanation is very detailed and clear; with your help I now understand the issue much better.
I agree that people make exaggerated, over-hyped claims about anthropic predictions but I find Vilenkin and Susskind's positions to be very different: they are interested in different kind of models. Susskind is interested in superstring theory and landscape, Vilenkin doesn't care about that. His only interest is cosmology: he uses 'The Principle of Mediocrity' for cosmological predictions.
I went to Vilenkin's colloquium recently (I am an alumni) about The Principle of Mediocrity. He made 3 points which I found interesting. Vilenkin is using a 2-step process to calculate the statistical distribution. He does assume AT FIRST that the statistical distribution of all possible CC values is flat. His argument that the range of all possible CC values in the multiverse is enormous and the allowed anthropic range is tiny by comparison. If you select a tiny range of ANY distribution curve, it appears flat as Earth surface looks flat to us. Next he goes on to CALCULATE the statistical distribution based on the number of galaxies, and that one is NOT FLAT. The PREDICTED (since it was done way BEFORE any experiments) and the observed CC values agree at 75% confidence level which looks pretty impressive to me.
Peter, I am very interested in Davis's book which is not available yet. Your review is great, so I have a question: I read your old Vilenkin's book review, you didn't like it at all. Davis's book review is much better but the subjects look similar. What are the differences and/or similarities in your view? Do they have a difference of opinions? Which one is better written and easier to read? Thank you very much.

34. **Peter Woit**
September 28, 2006

Neznaika,

Both books are written at a very low level, for a very unsophisticated audience. If one has a serious interest in these subjects, I'm not sure that either one is all that helpful. The Davies book is more about philosophical issues than the Vilenkin book. Its virtue is that it's even-handed: Davies explains the whole range of views on these topics, as well as what their problems are. Vilenkin's book is hyping one specific kind of theory, one that I find highly problematic since it is not really testable and thus not really science. On the whole, he also doesn't really bother to explain what the problems are with what he is pushing.

I can guess at what the "75% confidence level" claim is about, and I think it's heavily overhyped. For one thing, it completely ignores the problem I mentioned that if you allow other cosmological parameters to vary, the CC comes out much too small (probably so small that the standard experimentalists way of characterizing the situation is that Vilenkin's scenario is ruled out at greater than 99% confidence level).

35. **Neznaika**
September 29, 2006

Peter,
It's for ME! I AM that unsophisticated audience. I am trying to understand the science behind it but I don't want to fall asleep in the process, I want an interesting, well written book.
On a different subject: I read in your earlier post that Lubos Motl wrote a 'one-star' review of your book and "when his review is there, sales improve". I've noticed that Motl gave Vilenkin's book 5 stars (which is extremely rare for him). Do you think that Vilenkin's book sales are in real trouble?

Links and Gossip

September 22, 2006
Categories: Uncategorized

Well, no, I'm not going to start putting up here the really interesting gossip that people tell me. If I did so they'd stop telling me such things.

The Theoretical Particle Physics Jobs Rumor Mill has moved yet again. First it was hosted at the University of Washington, then the College of William and Mary, now it's at UC Davis. No idea why it moved this last time, but earlier this year some gossip told me the entertaining story of why it was booted out of Washington. To be honest, I've now completely forgotten all the details, so even if I wanted to violate their confidence, I couldn't.

The new Rumor Mill site confirms previous gossip I had heard that shows UC Santa Barbara having great success in hiring people in mathematical physics. Is Singer has been a regular visitor there in recent years, spending part of the year in Santa Barbara, part at MIT. This year they've hired two very good people: Dave Morrison and Sergei Gukov. Morrison has a mathematics background (algebraic geometry), and Gukov was educated as a physicist (a student of Witten's), but they both do interesting things at the interface of the two subjects.

Also at UCSB, Michael Freedman has moved his Microsoft Research group down from Redmond, and it is now temporarily in residence at the KITP, waiting to move into offices in the building next door when it is finished and will house the California Nanosystems Institute. Freedman is a topologist and Fields medalist, who was hired away from UC San Diego by my ex-grad school roommate Nathan Myhrvold when he was running Microsoft Research. From what I remember, at the time Nathan told me some mildly entertaining gossip about this, but, again, I've forgotten the details, so can't violate his confidence even if I wanted to.

Also on the move is John Horgan's blog. His Scientific Curmudgeon blog is being shut down, re-opened as a blog hosted by Discover magazine (which has its own blog). The new blog is called Horganism, and he has some advice which I don't endorse for would-be scientists;

Also, don't go into particle physics! Especially don't waste your time on string theory, or loop-space theory, or multi-universe theories, or any of the other pseudo-scientific crap in physics and cosmology that we science journalists love so much.

Seed magazine has some interesting new articles: one by mathematician Jordan Ellenberg about Fields Medalist and MacArthur winner Terry Tao, another by Joshua Roebke about Jim Simons and his Math for America project, the inspiration for which came over a poker game (OK, it was a poker game to raise money for charity).

The Cern Council Strategy Group has put out a briefing book that gives an excellent survey of the prospects for particle physics and particle physics experiments, especially in Europe, during the new few decades. Very much worth reading.

Comments

1. **Thomas Love**
   September 22, 2006

   Thanks for the link to Horganism, I wonder what a Horgasm would be like.

2. **John Ryskamp**
   September 23, 2006

   I also sent you an email about this. If you want a different perspective on string theory as it relates to the history of mathematics and physics, I strongly suggest you learn more about the important work which has been done over the past decade and a half in the history of set theory. Above all, read A. Garciadiego's BERTRAND RUSSELL AND THE ORIGINS OF THE SET-THEORETIC 'PARADOXES.' Some of the set theory paradoxes have already had holes poked in them, but Garciadiego does a remarkably thorough job in laying the groundwork for understanding the most important aspect of set theory: the response to it. You should ignore the huge number of typos in the book and pay close attention to the footnotes.

   But to the response: "natural" mathematics was developed, and traded under different names, with a program of "avoiding" the "paradoxes." This polemical point of view is well summarized in Penelope Maddy's polemic, NATURALISM IN MATHEMATICS.

   Poincare was one of the first of the "natural" mathematicians. The program informs his vastly influence SCIENCE AND HYPOTHESIS. Einstein rhapsodizes about this book. For many non-mathematical scientists, it was influential, and I believe Einstein took over its program into his physics.

   You should certainly ground yourself in the avant garde mathematics in which Einstein developed, if you propose to understand his work and what came after it. I believe you will develop a very different understanding, if you do.

   Here is my own take on the influence of "natural" mathematics during the twentieth century:

   Ryskamp, John Henry, "The Unity of Twentieth Century Ideas" (April 14, 2006). Available at SSRN: http://ssrn.com/abstract=897085

3. **Stefan**
   September 23, 2006

   Peter,

   As a (especially) theoretical physicist / mathematician of course you will not endorse John Horgan's advice to potential entrants; but what then are the [\italics]actual[italics] *realistic* responses to his qualm?
   I have been thinking about this for quite some time now, and each time it seems that going into HEP research would likely result in (or atleast set a path towards) financial and professional destitution and destruction (unless of course one possibly chooses to make fundamental compromises to one's intellectual principles, i.e. join one of the bandwagons of current theory [especially string theory]; this seems – sadly – more like playing Russian Rullet with one's professional career than anything else).

   My hypothetical responses to this quandary are the following:

   a) go into mathematical physics; [more on this below]

   b) go into condensed matter physics (atleast some job prospects can be expected after finishing studies);

   c) go into mathematics proper (-same reason as (b)-)

d) forgo this area (math and physics) and study something like ‘computational science’ (atleast there is some relation – albeit indirect to my knowledge – with theoretical physics; this area includes ‘chaos and complexity theory’ I assume;

e) forgo all of the above and become a computer scientist / programmer; this should be sufficient to guarantee a job no matter what the prevailing circumstances in academia (I hope!).

As for mathematical physics, what are the likely scenario(s) from your vantage point?

Stefan

4. **Stefan**
September 23, 2006

P.S.:

Peter, as you can guess I don’t know how to work the font system. Any suggestions?

5. **MathPhys**
September 23, 2006

Stefan,

In a certain sense, string theory really has nothing to do with high energy physics, although (like many other topics) it was motivated by questions arising in that subject.

String theory is very inspired (and inspiring) mathematical physics (or even mathematics).

Just forget about trying to make connections with what we see in (and expect from) high energy particle colliders and everything will be fine.

6. **woit**
September 23, 2006

Stefan,
Best advice about the fonts is probably to not even try to get that kind of formatting into comments here. Most blogging software handles html inside comments in an inconsistent way.

It’s very difficult to give people career advice about HEP research at this point. I certainly have no idea what the field is going to look like in a few years from now. A lot depends on what happens at the LHC, and how the physics community deals with the continuing dominance of string theory despite its conclusive failure to give any insight into unification. But, even in healthier times, going into speculative HEP research is not something someone should do who is looking for a straightforward career path and a well-paid, secure job. If those are important to you, do something else. There are just too many smart people and too few good permanent jobs in this field. On the other hand, if one has reasonably wide interests, and is very flexible in what one does to earn a living, one can most likely sooner or later find some reasonable sort of job after starting out trying to do HEP research.

If one is interested in the mathematical end of particle theory, one is probably better off getting a Ph.D. in math than in physics these days. Mathematics is a much healthier field, both intellectually and in terms of numbers of jobs. There are a lot of interesting questions at the boundary of math and physics to work on, and a reasonable number of good people and research activity going on in this area.

7. **anthropologist**
September 23, 2006

This is my first post here. While not exactly in the string theory, I am very fascinated by this whole psychology (people’s) angle. And, I have seen both Brian Greene’s and Ed Witten’s public talks, certainly, polished they were! So, these are my thoughts. On the side of the actual (tangible!) physics, what we have now is no new experiments that prove or disprove existing theories. Everything is wishy-washy, and there are no breakthrough experiments on the horizon, at least according to predictions by ANY theory. Yet it is still very desirable for people in the field of theoretical physics to eat (so to speak), and do something. So, these are the constraints.

One could argue that the string theory has evolved exactly to fit into this set of constraints. It captures public’s imagination, thus providing continuing public interest (and funding!). It cannot be readily falsified, so nobody can ever claim it is wrong, which also guarantees a good grace period for people to bail out if they start to have private doubts. Researchers are still able to do interesting (if irrelevant) mathematical things, with a twist, that instead of math they call it physics. And for a good reason, if they called it math, life would be a lot less comfortable, since math is more like art in terms of funding (most of practically useful math is probably no less than 50-100 years old).

Were the string theory doubted on a large scale, so that the public does lose the faith and interest, the funding for the entire field of theoretical physics might very well shrink. It is a mistake to think that the freed up funds would end up paying for less fashionable physics research areas, they might just go away altogether if the overall public interest is lower. For example, these money would go toward the “war on cancer”, where it is hard to believe that small increment of expenditures (percentage wise) will ever lead to any significant breakthroughs not achieved otherwise.

Now, coming back to the prevailing “strong peer pressure makes everybody conform” criticism that has been voiced in 2 recent books. In any event, if a “rogue” physicist has any good new ideas, then it would be possible to get tenure somewhere, and then just start promoting them heavily. That is because the personal gain in the case of true success is very high, perhaps, the Nobel prize (damn the string theory!). For a tenured faculty the risks would be relatively low, and the potential reward would be very high. Otherwise, why give up such a great brand such as “the string theory” for something that needs to be promoted, especially, if this new theory has no present day technical means to be proven/disproven even if offering falsifiable predictions? Of course, it might be that ALL theoretical physics people are too deep in the same hole, with the same set of tools. But visionaries like Enstein come once per hundred years, so, the whole vision thing is just very rare regardless of whether it is the string theory or something else.

So, it appears that while the criticism of the string theory might very well be valid, it is still the best thing for the physics field at the moment, since it keeps it going, and going strong. The only important thing is that the young people entering the theoretical physics fully understand the implicit rules of engagement, or otherwise irreversible damage to one’s career might result.

8. **Stefan**
September 23, 2006

Peter,

Thanks greatly for the advice; I do appreciate it. 🙂

You write:

“If one is interested in the mathematical end of particle theory, one is probably better off getting a Ph.D. in math than in physics these days.”

Coming from you [as a theoretical physicist] this is a little saddening and disappointing – one would have hoped there existed some optimism within the community. (This may be valid (indirect) confirmation that the “Golden Age” of particle physics has expired, and the field has changed in character from its glorious heydays…).

Anyhow, my question(s) then are:

1) What are the topics you recommend we study to gain expertise in this area [mathematics of particle theory]? (especially in relation to QFT-math; and if relevant – I suppose?! – to the Clay problem… one can hope, can’t one?!)

[If you have recommended texts, please include as you like.]

2) What (research) topics do you feel may pay dividends for future mathematical physicists (especially for the area above)?

3) [somewhat off-topic] What is the Representation Theory relation to all this? (You previously wrote that you were studying / reading the new text: ‘Dirac Operators in Representation Theory’.)

Regards,
Stefan

9. **Aaron Bergman**
September 23, 2006

And for a good reason, if they called it math, life would be a lot less comfortable, since math is more like art in terms of funding (most of practically useful math is probably no less than 50-100 years old).

A lot less comfortable? There are no monetary differences that I can discern. People call themselves physicists rather than mathematicians because the cultures are distinct. The training, the way problems are attacked, the standards of rigor and the incentives are all quite different.

10. **woit**
September 23, 2006

Stefan,

I have no idea what kind of ideas will ultimately solve the Clay Yang-Mills problem (or any other of the Clay problems..)

My 2002 arxiv paper is kind of a sketch of ideas that I find promising, more recently I’ve mostly been working on BRST (and its relation to the Dirac operator and representation theory). There’s a lot of mathematics related to BRST that has yet to be exploited.

Finally, geometric Langlands has all sorts of interesting math and possibly physics associated to it. If Witten keeps working in that are and turning up more things, it may get more people willing to join him and could get very interesting.

11. **Dan**
September 23, 2006

Speaking of links, the physics forum has a review of your book and a string theorists disagrees as a matter of fact with your book. While I suspect the reviewer is yours truly, the profile says France, not Harvard.

Here’s a link:
Go to first new post Opinions of ideas in “Not Even Wrong”
http://www.physicsforums.com/showthread.php?t=133053

“RX”

[reposting of this long comment deleted]

12. **Stefan**
September 23, 2006

Peter,

Thanks for the response; (was hoping for a more substantive one though). It’s alright however: I know you have other duties at hand too.

🙂

Take care,
Stefan

13. **woit**
September 23, 2006

Dan,

Please don’t repost long things here from the middle of discussions on other blogs. It’s fine to point out discussions elsewhere that people here might be interested in joining, but in general they should do so where the discussion is already taking place.

“R.X.” is not Lubos Motl, he’s a string theory partisan who posts on various blogs using various pseudonyms (from what I remember, I think he’s “MoveOn” when he posts here). For some reason he doesn’t want his real name and identity attached to what he has to say, perhaps because it is often incorrect and full of nasty personal attacks.

The comment of his you quoted doesn’t appear to be a review, since he doesn’t actually say anything about the book or claim to have read it. It’s just the usual ad hominem personal attack on me, together with standard issue string theory propaganda and incorrect claims (no, you can’t falsify string theory by not finding 10 d string excitations at the Planck scale, since some versions of string theory don't have these, ever heard of M-theory?).

14. **anthropologist**
September 23, 2006

Peter, actually, R.X. does not really attack anybody personally, where do you see that? R.X. does disagree with you, but surely healthy dissent is a good thing. Or you do not think so?

Also, R.X. points are well-reasoned, specifically, it is just not plausible that some smart individual would not abandon the string theory in heartbeat if something more promising came up. So I think that the particular claim of yours that "everybody is too locked up in the string theory to look around" does not stand under detailed scrutiny.

It is much easier to disagree with something than to be constructive. So again, R.X. argument is well-made, if you had some constructive material on the subject, you'd probably be publishing it in scientific, and not popular literature in order to properly collect your credit.

15. **Peter Woit**
September 23, 2006

anthropologist,

Actually, R.X., begins his comment with "I suspect if he [Woit] would be able to contribute something positive to science, he would have already done so in the form of real publications. Instead he chooses to gain his 15 minutes of fame by discrediting the work of others." and then goes on to not address any of the arguments in my book. I don't see why I should spend much time answering anonymous criticisms from people who personally attack me, show no willingness to even read what I have to say, and just endlessly repeat the same incorrect hype about string theory.

I'm not claiming there is an obvious much more promising thing than string theory that all string theorists should start working on. I am claiming that what they are working on has conclusively failed and they need to admit this. I don't think one can defend continuing to work on a failed idea. If they want to argue about this, they need to deal with my argument that string theory has failed. It's given in detail in the book. I'd be happy to argue these scientific issues with R.X. or anyone else.

Sure, it's very difficult to come up with constructive new ideas. I've written some of mine up, in hep-th/0206135.

16. **Peter Orland**
September 23, 2006

Stefan,

As a theoretical physicist, I don't agree with Peter (W)'s advice about going into math as a path to high-energy physics. Mathematicians have made valuable contributions here and there to physics, but going to graduate school in math won't make you a physicist. You will instead need experience doing
research in physics to understand the issues. I predict that whoever solves the Clay problem you mentioned above won't be a mathemtatician.

17. **anthropologist**
September 23, 2006

Peter,

Your approach is idealistic, and not realistic.

"I am claiming that what they are working on has conclusively failed and they need to admit this. I don't think one can defend continuing to work on a failed idea."

So, how do you exactly see such an admission? Like the following: All talks at the string conference would go "we failed, the string theory is dead". Then, since there is nothing obviously promising out there, upon return everybody turns in their resignation letters, and goes to work at McDonalds. Would that be the plan?

The only way the string theory would die out if it was displaced by something more promising (and not less!). That is the only possibility for an implicit admission. Without a replacement, the efforts on ST will continue.

Naturally, one way to gauge the real ST community sentiment would be to go to a string conference, feed some younger attendants some beer, and then collect opinions. Often big shots express their sentiments to the students, and those propargate, so with beer one could extract that. Many younger people not too close to the critical points of their career would be rather willing to discuss the perceived state of things, at least that is what I found in my field.

18. **Peter Woit**
September 23, 2006

anthropologist,

Have you read my book or Lee Smolin's? Both books very explicitly deal with the questions you are raising.

I've had many conversations with string theorists, young and old, over beers, wine and other beverages. Many of them are very disturbed about the current state of the theory and we agree on much more than we disagree.

The question is: how do you make it possible for the kind of young, bright, ambitious physicists who might be able to develop alternatives to get the kind of training needed and spend the years of work necessary to make some progress on alternatives, while still having a decent chance at a successful career? It seems to me a necessary ingredient is admitting failure. As long as the field is dominated by people who claim that string theory is the best thing for people to work on, refuse to admit that it has failed, and provide no support for young people trying to do something else, things will not change.

19. **anthropologist**
September 23, 2006

Peter,

I agree that to have a career one needs a lot of support from the establishment. And, with alternatives to ST, it still would be very risky to bet a career on an alternative, even with some supportive senior people (unless those have such great connections that a tenured job would be nearly guaranteed at a later point). That is because if you get relatively nothing interesting in a new area (no flashy or popular publications), you have nothing to apply for jobs with, and nobody would spend any time to understand what you did. Thus you would have been much better off incrementally improving the string theory where you would do something OK and be relatively widely understood, with good support by the establishement.

In my field, situation in some ways even worse, since there is certain "mafia" in the establishement which gets all the interviews for their people, so then not even the area is constraining, but the choice of people to work with! If you did not forsee that before, and ended up on the other side of such a divide, tough luck.

20. **nc**
September 23, 2006

"… tough luck." – anthropologist

I wish Peter or someone would plot out in a flow chart the standard responses to criticisms of string theory. All the responses are unoriginal, and follow the following sequence:

1. Critics should shut up complaining or else talk about an alternative to string. They all just want 15 minutes of fame.

2. OK, the critics have some ideas about alternatives, but they aren't hyped as much as string, so they must be wrong.

3. If you made the error of not being a sycophant of the stringy mainstream from your early years, tough luck.

This is really interesting because to ride out criticism, the stringy mainstream gets ever more arrogant. How long can such political tactics divert attention from their lack of physics?

21. **jeremy**
September 23, 2006

Stefan,

I am going to add something to what Peter (W) and Peter (O) already wrote, but I wouldn't call it advice.

In case you want to do your research in a mathematics department and working on the Clay problems, but still have worries about the future job opportunities, Navier-Stokes equations would be a good problem to work on. There has been a great deal of effort tackling this problem in applied mathematics in the last thirty years. So, in the worst case scenario, you don't get to win the million dollar prize given by Clay Institute and you cann't find an academic position in pure mathematics, but you always have applied mathematics to fall back on.

22. **Stefan**
September 24, 2006

Peter,

Thought this might be helpful to look through:

http://arxiv.org/abs/hep-ex/0108040

'Results of the Survey on the Future of HEP' by Young Particle Physicists (YPP) during the Snowmass '01 Conference.

Stefan

23. **Alejandro Rivero**
September 24, 2006

Re

"So, how do you exactly see such an admission? Like the following: All talks at the string conference would go "we failed, the string theory is dead".,"

A smotther way should be to produce a lot of talks to discuss "where did we go wrong?" and "why?" . There are some Field Medals relating to string theory, and one sould wonder how is that string theory has got to reach new mathematical structures.

My current answer is that these structures were so general than any bold attempt to go beyond QFT was dammed to find them in some disguise. It could follow that even if string theory is wrong, they were wronged by a mirage of some objects really related to physics, and it is still wothwhile to locate these objects. Of course better mathematics than strings is needed (here a toast to representation theory, for instance 😉

24. **Observer**
September 24, 2006

Peter,

I was thinking about this issue whether blogging and publishing polemic books constitutes scientific activity or not (it was raised several times on this blog).

And my conclusion is that it can be determined easily. If we assume that scientific activity is the thing that if someone does very well then he/she is entitled to a good academic job at a respectable university, then my question to you is this:

Do you think that anyone should be hired as a faculty member at a physics department based on his/her blogging and polemic book publishing activity?

If your (or anyone's) answer is 'yes' then you (or anyone) think(s) that blogging, etc, is a scientific activity, otherwise you (or anyone) think(s) that it is not.

Cheers,
Observer

25. **Peter Woit**
September 24, 2006

Observer,

Obviously it all depends on what is in the blog and in the book. If someone were to put up a correct proof of the Riemann hypothesis on their blog or include it as a chapter in their polemic book, obviously a respectable university should offer them a good academic job. It's also true that someone with no blog and no book, but a lot of worthless publications in journals on, say the Landscape, should not be offered a job by a respectable university.

One example of a blogger whose blog is devoted mostly to his ongoing research is Urs Schreiber. Respectable universities have certainly hired people based on worse scientific activity than that on his blog. My own blog entries dealing with current scientific research are often devoted to showing what is wrong with a new scientific argument. I happen to think this is scientific activity, and of non-negligible value, but also that more positve scientific activity (such as hep-th/0206135) is of much greater value. This kind of positive scientific work is on the whole likely to be too long and technical to fit comfortably as a blog posting.

This is completely off-topic, and I'm having trouble believing it's not personally aimed at me. Anyone who wants to discuss this further at a minimum will have to be willing to not hide their identity and credentials behind the cover of anonymity.

26. **anthropologist**
September 25, 2006

nc –

"2. OK, the critics have some ideas about alternatives, but they aren't hyped as much as string, so they must be wrong.

3. If you made the error of not being a sycophant of the stringy mainstream from your early years, tough luck."

Point 2 is more valid because people still have to do something to get noticed, and have a record to apply with. If the area is only weakly promising, and not much progress is made by a job applicant to be, then that is too tiny of a program to run on, so to speak. It is better to be a mediocrity in an known field, rather than in an unknown one.

Point 3 is valid because the present day establishement is all string theory, and rule 1 of academia that you do not go against the establishment when applying for jobs. In any field.

So these are not just purely pro-string theory arguments, but they are more generically relevant for any dominant worldview, however dillusional it might be.

27. **Thomas Larsson**
September 25, 2006

anthropologist:

So, how do you exactly see such an admission? Like the following: All talks at the string conference would go "we failed, the string theory is dead".

They could say something like string theory pioneer Dan Friedan did in hep-th/0204131:

"String theory has no credibility as a candidate theory of physics. […] Complete scientific failure must be recognized eventually."

The only way the string theory would die out if it was displaced by something more promising (and not less!).

As I pointed out in my 1999 manifesto, gr-qc/9909039, the multi-dimensional Virasoro algebra is the correct quantum form of the correct constraint algebra of general relativity (in covariant formulations). It is thus to GR what the ordinary Virasoro algebra is to perturbative string theory. That is why this mathematical discovery is a necessary prerequisite for quantum gravity, and that is why I decided to discover it.

28. **Stefan**
September 25, 2006

Peter,

Great reply! 😉

As a person not affiliated (directly, at least) with either side of this debate (or for that matter any committed research direction), I found it rather sorry-like (and even somewhat pathetic) that Ph.D. scholars [professional string theorists, albeit not all] were stooping to such un-professional levels to defend their position(s); this perhaps more than anything else suggests that inside each [defender / critic] lies a perpetual fear that his / her much-touted intellectual-academic enterprise may have lost its once-sparkling lustre, or even its stature within broader academia. The time has rightly come to conduct a *sobering* study of string 'theory', and (perhaps more importantly), how it has been effectively able to gobble up the lion's share of funding for hep-research… at the cost of other well-meaning avenues (i.e. LQG, twistor theory, non-commutative geometry, Algebraic/Constructive QFT, etc.).

P.S.: Incidentally, I am now studying Ch. 31 of Penrose's book [The Road to Reality, UK version] – 'Supersymmetry, supra-dimensionality, and strings' – and I would recommend any undergrad physics major contemplating doing future studies in hep to read his views on strings and supersymmetry. (I can assure you it reads nothing like Lubos' unintelligible and sometimes offensive/crude/impolite, (but all-in-all a great source of diversion and amusement), rants.)

Stefan

29. **Steve Myers**
September 25, 2006

On jobs & careers: industry needs good math & physics grads. People would be surprised at the level of math & physics involved with modelling. The pay is good & often the problems are interesting. But make sure you take a good statistics course. And no one in industry would be left to work on something that was as disconnected from reality as string theory.

But if you have a problem you must work on, one that won't let you go, get your phD & follow Einstein's advice and be a plumber. I haed a fellowship at NYU when I woke up in the middle of an exam and walked away froim the academic life.

30. **nc**
September 25, 2006

'If the area is only weakly promising, and not much progress is made by a job applicant to be, then that is too tiny of a program to run on, so to speak.' – anthropologist

The higher standards of alternatives make them more vulnerable to dismissal as 'only weakly promising' mainly because the people working on them don't hype them up so much.

Caution is wrongly taken as a sign of incompetence, or a sign that the researcher is at least not confident in the alternative idea.

Contrast this to the mainstream, who justify their extraordinary claims using extraordinary hype.

31. **Thomas Love**
September 26, 2006

    Peter wrote:

    "If one is interested in the mathematical end of particle theory, one is probably better off getting a Ph.D. in math than in physics these days. Mathematics is a much healthier field, both intellectually and in terms of numbers of jobs. "

    The situation was the same when I was in graduate school. After a Bachelor's degree in physics(1968), I did one year of grad school in physics before my education was interrupted by the Vietnam war. After serving 5 years as a pilot, I returned to grad school, switching to math because I felt that my physics education had ceased and my indoctrination begun. In my experience, the physicists want us to sit in awe of their work and not question it. There is far more academic freedom in math than in physics. In the intervening years, the fads had changed but the math requirements for the newer fields were much greater. I wrote a physics dissertation (The Geometry of Elementary Particles, UCSC, 1987) for a PhD in Math. Some members of the physics department fought against its acceptance. I arrived at some conclusions the physicists didn't want to hear: Einstein was right about quantum mechanics;quarks are a mathematical fiction; the ultimate theory cannot be a lagrangian field theory. I've added more heresies since.

32. **ks**
September 26, 2006

    Maybe you got this already?

    http://www.fnal.gov/pub/presspass/press_releases/CDF_meson.html

33. **jeremy**
September 28, 2006

    Peter,

    Please have a look at what Hamilton has to say about Yau at

    http://www.doctoryau.com/.

    Check for the UPDATE.

Unstrung

September 25, 2006
Categories: Not Even Wrong: The Book

This week's New Yorker has an article about the controversy over string theory, written by Jim Holt, with the title Unstrung. On the web-site there's also a link to Woody Allen's 2003 humorous New Yorker piece on string theory, Strung Out.

The New Yorker article pretty much gets the story right, although the description of the Bogdanov affair isn't completely accurate. The Bogdanov papers were about quantum gravity, but were not string theory papers (although they claimed to be motivated by string theory, and at least one referee described their results this way). Holt also describes members of the Harvard string theory group as unsure whether the papers were a fraud or sincere, which does correspond to an e-mail that circulated at the time. However he doesn't mention that at least one member of the Harvard string theory group to this day not only believes the Bogdanov papers were written sincerely, but considers them to be serious scientific research (an opinion shared by very few others).

Holt accurately describes Smolin's book as more accessible than mine, then chooses a very good example of an "indigestible" sentence from my book:

The Hilbert space of the Wess-Zumino-Witten model is a representation not only of the Kac-Moody group, but of the group of conformal transformations as well.

That is an example of some of the very advanced material I tried to include in a few places in the book. It's the precise expression of the mathematical relationship of representation theory and QFT that has been worked out in recent decades in two dimensions, exactly the thing that I would argue we should be trying to understand in the physical case of four dimensions. To the extent that the book contains a positive argument about alternatives to string theory, my decision was not to over-hype it, but to try and explain a point of view about the history of the relation of mathematics and quantum field theory that implicitly leads to this way of thinking.

Also out today is an article by JR Minkel on the Scientific American web-site entitled That's Debatable: Six Debates at the Frontier of Science. The first of the debates listed by Minkel is Is String Theory Unraveling?, and it's largely about the landscape. It includes a couple quotes from me, as often the case a bit abbreviated to make them sound even more provocative than I intended…

Update: The usual sensible commentary on the New Yorker review from Lubos. Holt is a "cretin from the garbage bin of the journalistic colleges", I'm the "black crackpot" (due to the color of the cover of my book, Smolin is the "blue crackpot"). Lubos reports on the reaction to the review from "one of the leading physicists of the current world" (presumably one of his colleagues):

*What's wrong with these people? Why don't they choose f***ing instead of writing about things that they don't like and they don't understand?*

Update: The story has made it to Slashdot.

Comments

1. **Aaron Bergman**
   September 25, 2006

   This is a pretty disappointing article. Take

   "Nowadays," one established figure in the field has said, "if you're a hot-shot young string theorist you've got it made.

   for example. The quote is from a number of years ago. These days, if you're a hotshot young astrophysicist or phenomenologist, you've got it made. String theory jobs are getting harder to come by.

   And, regarding Friedel's e-mail about the Harvard string theory group, frankly I just don't believe it. Pretty much everyone I know who looked at the papers agreed they were nonsense. I find it almost impossible to believe that the Harvard faculty wouldn't agree. Even Lubos's ravings on the subject are to the extent that there might be some ideas in there, not that the mathematical content of the papers is at all coherent.

   I'm a bit tired of all this talk about how we should cultivate 'seers' or 'valley crossers' or perhaps 'people Smolin likes' without any concrete suggestions on how we change the current incentive system in physics. I don't think there is this great division of people between the mindless string theory computation-o-trons and the deep thinking everyone else. What there is is a lot of smart people working under a strong incentive to develop a long publication record. If you want people to spend more time on hard questions, you're going to have to find a way to have it not hurt their job prospects.

2. **Stefan**
   September 25, 2006

   Peter,

   You write frequently about representation theory and QFT, so might it not be a generally good idea to write something pedagogical for would-be physics grad students before they prematurely commit themselves to any specific research direction.

   In fact, I was thinking it may be even more helpful to write a 'Resource Letter' along with the topic-intro and post both on the arXiv. This may help to disseminate your idea(s) to a wider reader base, especially given the climate of doubt which seems to have settled within the string theory community.

   Adieu,
   Stefan

3. **Peter Woit**
   September 25, 2006

   Aaron,

   Do you really think that a young grad student or post-doc who came up with something new about string theory that got a significant amount of attention would have trouble finding a job now? That's not my impression, but maybe I'm wrong.

   Smolin and I do both make concrete suggestions about how to change the incentive structure. To get them taken seriously though will first require that leaders of the field acknowledge that there's a problem. Besides positive incentives, I also fear that negative incentives may be part of the solution. If people decide they won't be able to get a permanent job by producing a long list of unambitious papers on trendy subjects, maybe they'll do something else. I would love to see the NSF/DOE start to take this issue seriously and start a discussion about their role in the current incentive structure and how to change it.

I don't have a problem with Smolin's "valley crossers" analogy, but I will admit that I'm congenitally dubious about "seers".

The e-mail in question doesn't refer to "Harvard Faculty", it refers to the string theory group as a whole, and presumably the discussion being reported was largely among postdocs. Lubos was one of them at the time and, given his later behavior, I wouldn't be at all surprised if his comments were one of things the writer had heard and was referring to.

4. **Peter Woit**
September 25, 2006

Stefan,

Yes, I would very much like to write something expository about QFT and representation theory, starting with something basic about QM and representation theory. This spring it looks like I'll be again teaching the second half of our graduate course on representation theory. Last time I did this I wrote up some preliminary notes which are on my web-page. This semester I hope to have time to improve those, and add some more on the topic of the relation of QM to the subject.

5. **Stefan**
September 25, 2006

Let me know when you finish the work.

6. **Aaron Bergman**
September 25, 2006

Do you really think that a young grad student or post-doc who came up with something new about string theory that got a significant amount of attention would have trouble finding a job now? That's not my impression, but maybe I'm wrong.

Depends on how new it is. I can tell you that I know a fair number of long term post-docs who have done very good work, but are deeply worried about their job prospects.

Smolin and I do both make concrete suggestions about how to change the incentive structure.

Can you please tell me what they are, then? I think it'd be a lot more helpful than just continually saying how much string theory has failed to do. I'd love to hear some really specific ideas.

Lubos was one of them at the time and, given his later behavior, I wouldn't be at all surprised if his comments were one of things the writer had heard and was referring to.

And so again Lubos somehow becomes representative of the entire field? I swear, if Lubos didn't exist, you guys would be forced to invent him.

7. **Peter Woit**
September 25, 2006

Aaron,

Lee has various suggestions, I don't have his book at hand. I assume one of his main ones would be to basically implement what he has been doing at the Perimeter Institute on a larger scale, in more places.

Among the things I've suggested that people should consider are:

1. Giving people directly out of graduate school longer term postdocs (e.g. 5-6 years), so they have more than 1-2 years in which to come up with something for their next job.

2. Graduate student birth control, bringing the ratio of Ph.Ds to jobs to something reasonable, so that the job market is not so insanely competitive and people are more likely to feel that they can have a future in the field even if they don't work on the latest, hottest topic.

3. Senior theorists need to stop putting students to work on the latest, trendiest string theory topic, encourage their students to work on a wider variety of things. At the same time they need to change their standards for hiring postdocs and junior faculty, making it clear to applicants that they want to see original ideas, not the same thing everyone else is doing.

4. The NSF/DOE should explicitly admit that particle theory research is in trouble, give guidance to people reviewing proposals that copycat proposals on the latest string theory topic will not be funded, and emphasize that priority will be given to diversity, that proposing to do something different will be a lot more likely to get you funded. This applies to grants for workshops/conferences, as well as grants to individuals and theory groups.

As for Lubos, who said he's representative of the entire field? Unfortunately he does represent the Harvard string theory group, at least he often claims that he represents their opinions. If this is not the case and they're unhappy with him, I've heard nothing about this from any of them, directly or indirectly. They're the ones who, after dealing with the guy for a couple years as a junior fellow, decided he was the best young person available in the field and offered him a tenure-track position.

8. **TheGraduate**
September 25, 2006

Peter,

I had always kind of thought that the overproduction of graduate students was a means of keeping wages low and therefore reducing the number of full time professors one needed to work in a particular department.

I am not sure to what extent this reasoning is sensible but the overproduction of science PhD's probably lowers wages in all fields for which science PhD's are eligible.

As I understand it, the growth in wages of professors has not kept pace with the growth in wages of other middle class professionals such as doctors and lawyers.

I think rather than graduate birth control, there should be more simple truth from the beginning of the system to the end of the system. Isn't it fair to say universities overhype the potential job opportunities and leave it to the student to eventually stumble into the rather brutal truth?

Didn't you have to discover for yourself that there were no jobs for nonstring theorists? I find that kind of system fundamentally unfair. You could have probably started thinking about your transition to math years earlier and perhaps had a smoother transition.

9. **Bee**
September 25, 2006

Hi Peter,

Senior theorists need to stop putting students to work on the latest, trendiest string theory topic,

That does not only go for string theory. In general, senior theorists should not dominate over the next generation by applying selection criteria that primarily support their interest, instead of criteria that support the most talented researches – even if those might have been working on topics they don't find exciting at all. The problem is that this goes on long after graduation. Given the fact that the average postdoc is aged around 30, hiring institutions should trust in the candidates ability and responsibility to find promising research fields on their own.

Besides this, its kind of funny that the article picks out exactly the same sentence about the Kac Moody group that [I mentioned](). I also found the following remark *'actually, this is a serious over-simplification […]'* very amusing 🙂

Best,

B.

10. **Carl**
September 25, 2006

It is true that Smolin's book is designed for a more general audience. What is amazing is that Woit's book, despite being fairly technical, is selling enough copies to be number 2 in physics book sales on Amazon after Smolin.

And this concept that the next big advance in physics is going to come from a graduate student is a little iffy. The idea is based on observations of the past, but as time has gone on it has become more and more difficult to reach the frontiers of physics. This alone would suggest that older researchers are more likely to make the advance.

Smolin had an interview where he said that his "seers" and "craftsmen" wasn't as good as someone else's "peak climbers" and "valley crossers", where the peak climbers push the old ideas to their limits, and the valley crossers abandon old peaks for new ones.

11. **Peter Woit**
September 25, 2006

TheGraduate,

It's extremely difficult to get universities interested in the idea of reducing the number of Ph.D.s. The faculty like having large graduate programs so they can teach advanced courses, the universities like having huge numbers of people competing for jobs, they're basically flooding their own labor pool.

The situation in particle theory is rather unusual for a scientific subfield, with a much worse imbalance between numbers of Ph.Ds. and jobs than any other subfield that I know about. The situation is much much better in mathematics.

I certainly went into graduate study in particle theory knowing that the job situation was quite bad, and then, as now, I assume that most departments and advisors let prospective particle theory Ph.Ds know this (although if they can't figure it out for themselves, one wonders how bright they are…). String theory was a non-issue when I was a grad student (I finished my thesis and started a postdoc in mid-1984). I can't say that I was surprised to have trouble finding a job after my first post-doc. I'd say that string theory just turned what would have been a difficult situation into an impossible one, and it was just as well that it made it much more sensible to look for a job in math than to hang around trying to find not very good positions in physics.

12. **Peter Woit**
September 25, 2006

Bee,

Maybe because I'm older than you I'm less of the opinion that the problem is inherently in the judgment of senior people. As far as pushing people into working on bad ideas about string theory (or other trendy subjects), I just don't see that it's mainly the older people doing this.

Funny that you chose the same sentence as Holt (although quite possibly he reads your blog…). My book has gotten a lot of criticism from people for some of the more technical things I put in it, but I'm still quite glad I did this. The material is there to provide something new and challenging for almost every one, and Chapter 10 in particular is an effort to get down on paper the story of the successful interaction of math and physics over the last three decades. Not everyone is going to be able to follow this, but I still think it's a worthwhile effort.

I suppose I'd feel differently if hardly anyone was buying the book because of this. As it is, it seems to be selling all right, with Smolin's book doing better because it makes many of the same points pitched to a wider audience. This situation is fine with me.

13. **TheGraduate**
September 25, 2006

Peter,

I finished reading your book yesterday and I had a few questions. So is S-matrix theory the origin of the string idea in string theory? Do you view string theory then as the modern face of S-matrix theory? Also, it occured to me that the sorts of anthropic reasoning and interest in theological tendencies that afflicted S-matrix theory at the end of it's theoretical life seems to be the sort of thing affecting string theory now. Was this something you wanted to highlight?

Secondly, I felt like you were emphasizing group representation theory as perhaps having interesting and generally overlooked significance to HEP. Was part the goal of writing the book to encourage more curiousity in that relationship?

14. **andy**
September 25, 2006

I cannot agree with the idea of reducing the number of graduate students as some sort of solution to the problem of excessive competition among would-be professors. Some areas of research allow students to go on to productive careers outside of an university.

15. **TheGraduate**
September 25, 2006

andy,

I think most likely if the major universities coercively reduced graduate students then lower tier and fringe universities would start offering PhD's to any people that still wanted to persue graduate degrees. I think this is already happening. It would just happen more.

16. **Aaron Bergman**
September 25, 2006

The longer postdoc thing might be helpful. I'm not sure that just won't lead to people churning out lots of papers at the same place rather than multiple places, however. I'd be more inclined to enhancing a sense of job security, but that may be my own anxieties speaking.

The reason, I should say, why string theorists encourage students to work on popular topics is that they know that that's how jobs are obtained. Perhaps it's a viscious cycle, but in the current environment, it's good advice.

As for NSF proposals, I've never been involved in one, but in areas of theory are they really directed at specific research proposals? For one, I thought that it was often entire groups that were funded out of these grants. It doesn't make much sense to me to tie these grants to specific research directions given that things can change rapidly.

As for Lubos, who said he's representative of the entire field?

Well, somehow we go from Lubos thinks the Bogdanovs might not be completely and utterly full of crap to "even the Harvard string-theory group was said to be unsure" (in that wonderfully weaselly passive voice) to, apparently, the implication that string theory is barely distinguishable from nonsense.

Isn't it fair to say universities overhype the potential job opportunities and leave it to the student to eventually stumble into the rather brutal truth?

Every place I applied told me that job prospects were crap.

instead of criteria that support the most talented researches – even if those might have been working on topics they don't find exciting at all

How does one determine the 'most talented researche[r]s'? That's the rub, after all. Otherwise, we'd just give them all tenured jobs at 18 and let them do whatever they feel like.

17. **Peter Woit**
September 25, 2006

TheGraduate,

S-matrix theory was an approach to the strong interactions that led to the discovery of the Veneziano amplitude, and that led to string theory. The ideology behind S-matrix theory was that QFT could never give a theory of the strong interactions, so had to be abandoned. This turned out to be wrong. Another part of the S-matrix ideology was that symmetries were not fundamental, this also turned out to be wrong. My suspicion is that present day string ideology has the same problems: maybe there is a QFT that gives quantum gravity, and maybe to make more progress on fundamental QFT you need new and better ways of exploiting symmetries. Representation theory is precisely how one exploits symmetries, one point of the book is that this has been the right approach in the past, even when people called for it to be abandoned, and it may yet be the right approach for the future.

andy,

Particle theory is the only area where I am suggesting reducing the numbers of Ph.Ds. I've heard all the arguments about why the current system that produces 5-10 times more people trained to do particle theory research than there are jobs doing such research is a good one, but I don't agree with them. I think a system where it is much harder to get a particle theory Ph.D., but where the people who get ones have a better shot at getting a permanent research job if they do interesting research would be healthier than what we have now.

18. **Peter Woit**
September 25, 2006

Aaron,

NSF proposals don't just say "We're Prestigious U. and have done good stuff in the past, send cash", they include descriptions of the research directions people in the theory group intend to pursue. If word got out that the NSF was not looking kindly on certain research directions, that would have a big effect. In the past, I think the NSF has for good reasons been unwilling to ever do this, leaving things up to peer review.

As you know, the reference to the Harvard string theory group comes from an e-mail sent by someone visiting there at the time (I don't know who this was, do you?). Maybe he or she was making this up or exaggerating, but the fact that one of the Junior Fellows there at the time still can't see that the Bogdanovs are full of crap, and his colleagues thought he was a promising young genius and gave him a faculty job lends a certain amount of credence to the story.

19. **Aaron Bergman**
September 25, 2006

As you know, the reference to the Harvard string theory group comes from an e-mail sent by someone visiting there at the time (I don't know who this was, do you?).

In the e-mail the Bogdanovs sent to many people (so I think I can safely call it public knowledge, especially as I put it on my webpage at the time), it was quoted as follows:

> Date: Fri, 25 Oct 2002 10:10:29 -0400
> From: Laurent Freidel
> To: Laurent Freidel ,
> Marc Magro ,
> Philippe ROCHE ,
> Laurent GALLOT , Jean-Michel.Maillet@ens-lyon.fr,
> kgawedzk@ens-lyon.fr, francois.delduc@ens-lyon.fr,
> pierre.vanhove@cern.ch Subject: RE: Hoax: Alan Sokol phenomenon reversed
> (fwd)

> " What is going on??? guys?? the claim is now that the Bogdanoff brothers are not a fraud and that they not only won Phd's with these papers that no one can understand, that yesterday everyone was convinced were fraudulent, they won appointment as professors to a french university, Bourgogne!!!
>
> So no one in the string group at harvard can tell if these papers are real or fraudulent. This morning told that they were frauds everyone was laughing at how obvious it is. This afternoon, told they are real professors and that this is not a fraud, everyone here says, well, maybe it is real stuff".

Again, I wasn't there, so I can't speak of anything that happened there, but given that everyone I know who looked at the papers was ably to discern the problems, what I can do is express my disbelief of this particular e-mail.

20. **Peter Woit**
September 25, 2006

The e-mail was forwarded by Laurent Freidel to various people Undoubtedly he, like I, found it rather amusing. But I still don't know where he got it from.

21. **TheGraduate**
September 25, 2006

Peter,

Concerning the limiting of Phd's, I guess I was thinking if particle theory became any more elite, guys like Einstein and Witten probably wouldn't make it in. The former not being well rounded academicly at least in his youth and the latter being a history major. It seems to me that the natural way that people would make it more elite is to start expecting even higher grade point averages, more research at the undergrad level, even more stellar recommendation letters etc etc. In other words, the kind of measures that in large part probably describe many of the string theorists and seem to have contributed heavily to why string theory has come to dominate.

22. **Aaron Bergman**
September 25, 2006

Well, since this anonymous comment has now made it into at least one book and an article in the New Yorker, perhaps the writer of the e-mail could come forward and explain what they meant by it.

Until that happens, I don't have much reason to believe that it bears much resemblance to reality and a lot of reason to believe that it doesn't.

23. **Peter Woit**
September 25, 2006

TheGraduate,

I don't think Einstein or Witten would have had much trouble meeting a significantly higher standard than the minimum now needed to get a Ph.D. But, sure, the main problem is that the current system is incentivizing and rewarding behavior very different than coming up with good original ideas.

24. **Gina**
September 25, 2006

Here is a couple questions I had while reading NEW.

1. String theory in the first simplest version implies that the universe has 26 dimensions. Is it possible to explain in a few sentences or a couple of paragraphs why? (I heard many years ago a short 1-2 slides explanation in a half-popular talk but I completely forgot it.)

2. In the book there is a distinction (for very successful theories from physics) between "convergent series", "useful divergent series", and "useless divergent series". Is there any formal distinction between the last two types? Can it make sense in math?

3. The era before QCD and the standard model is described at least as confused and chaotic as the situation in string theory today. Maybe even more than today. (Peter push the analogy by telling also there about a physist who combined physics with eastern philosophy.) Yet people who promoted these unsuccessful but very dominant theories were not ask to admit failure; moreover, they did not fail: conceptual and technical ideas from these unsuccessful theories turned out useful later, and students of these scientists had crucial role in developing more successful and completely different theories. This looks like a good model to proceed, no?

25. **woit**
September 25, 2006

Gina,

1. Away from d=26 quantization introduces an "anomaly" in the symmetry of conformal transformations of the string worldsheet. Basically this means that the quantum string theory is more complicated and the metric on the world sheet becomes a dynamical variable you have to deal with. People study these "non-critical" string theories also. There are various different calculations that give you the 26, I don't know of any simple physical explanation for it.

2. Some divergent series are "asymptotic" approximations to some function, which means that, at a fixed order, the truncated series is a better and better approximation to the function as the expansion parameter gets small (even though, at fixed small parameter, as you go to higher order, the series sooner or later diverges). The perturbation series for QED is supposed to be such an asymptotic expansion. This kind of divergent series can be quite useful, giving very good approximations.

3. Actually, the bootstrap program did fail as a theory of the strong interactions, and was pretty much killed off by QCD. Many of the people who worked on it certainly admitted that it failed, other die-hards never did admit this, but stopped being taken seriously by most theorists. Certainly some things learned from this were ultimately useful, but the program failed to do what it was intended to, and sensible people admitted this.

26. **Tony Smith**
September 26, 2006

Maybe the superstring phenemenon in theoretical high-energy physics is NOT an aberration in the current world of science,
but

is an example of a more general phenomenon in which a given approach obtains by successful lobbying a dominant position with respect to funds and jobs, and then continues to expand, cancer-like, whether or not any real productive results are ever obtained.

For example, consider AI (artificial intelligence) and a 25 September 2006 article by Andrew Orlowski in the Register at http://www.theregister.co.uk/2006/09/25/bogus_ai/ which article says in part:

"… MIT's Professor Marvin Minsky's … linguistics-based approach to AI, symbolic AI, triumphed in the grants lotteries over an approach which preferred to investigate and mimic the neural functions of the brain. Minsky's non-stop publicity campaign helped ensure his AI lab at MIT was well-rewarded while neural networks starved. …

The Japanese … poured hundreds of millions of dollars into their own AI project, called Fifth Generation. They may as well have buried the money in the Pacific Ocean. Two decades later there are no intelligent robots, and "intelligent" computers are a pipe-dream. …

For the past week reports have again confidently predicted intelligent computers are just around the corner. Rollo Carpenter['s]… chatbot Joan won an annual AI prize for creating software that most resembles a human … Here's Joan, cited at the MIT's Technology Review … :
Judge: So what you're saying is that you are not just human but THE smartest human?
Joan: Yes, why, what did you think I was saying? …
Judge: How many degrees in a triangle?
Joan: 180 degrees.
Judge: Is it possible for more than 180 degrees to be in a triangle?
Joan: Probably not. You can change your opinion, but not your constitution.
Judge: It's not a question of opinion, it's a matter of physics and maths.
Joan: A five-ounce bird could not carry a one-pound coconut. …

Occasionally Joan will ask the interrogator if they are a robot, or chide them for being stupid. These are all pre-programmed rhetorical tricks. They may bore or bamboozle an interrogator, but this is no indication of intelligence. …".

It seems to me that the Minsky-worshipping AI community is similar to the Witten-worshipping SuperString community in at least the following ways:
1 – using PR-type publicity to get and maintain dominance in funding and jobs;
2 – starving competing approaches;
3 – no real results after two decades;
4 – calling critics "stupid" (AI using a computer program that is merely, as the Register article says,"a database of conversational snippets", while SuperStringers are using a Harvard physics professor).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – I am NOT saying that "artificial" intelligence is not possible (for example, maybe it could be achieved by closely studying the way the brain works and applying the results of those studies, or by using arrays of quantum computers, or by a combination of both such approaches, etc). I AM saying that it appears that the Minsky-type approach seems to have many of the same dysfunctional characteristics as SuperString Theory.

27. **Alejandro Rivero**
September 26, 2006

we'd just give them all tenured jobs at 18 and let them do whatever they feel like.

I agree with this. I think this was the goal, if any goal can be claimed, of the university uprising in Paris 1968 (also Madrid 1965 and others): never finish the university career, keep always being an student (and get food and shelter).

Also Peter, er, Kropotkine suggested to limit the job requeriments at a maximum of five hours per day, so the rest of the time can be dedicated to science or other leisures. This could be a solution of some appeal for the people forced out of academy but still interested: to try to arbitrate for non-research but part time jobs in the understanding that the rest of the day -say, the evening- one can keep research activity holding the same rights (travel, computer access, conferences, use of desk, coffee and blackboards) that a full time research professor.

28. **a**
September 26, 2006

Peter,

You say job prospects for math graduate students is very good, but how do number of applicants in math grad programs compare to that of applicants in physics programs. Perhaps there are lot less students applying for math programs? However, I think situation in physics (no. of jobs and no. of wanna-be physicists) is much better than humanities where apparently one needs to be already quite accomplished to get into graduate programs.

29. **Peter Woit**
September 26, 2006

a,

I didn't say job prospects for math Ph.Ds are very good, just that they're quite a bit better than for particle theorists. I don't know of any significant difference in how hard it is to get into physics and math graduate programs. I also know of no evidence that it is harder to get into graduate programs in the humanities. Again, what is at issue here is the very bad job situation in one particular subfield of physics, particle theory. The situation in math and in other subfields is mostly much better.

30. **TheGraduate**
September 26, 2006

a,

The job situation isn't that great for math PhD's as far as I understand it. The AMS has yearly reports so if you google a little bit you should find articles in the Notices about it.

There is a general oversupply of science PhD's and this keeps wages down not just for professors but for post docs and graduate students and research assistants both in the universities and in industry.

I think this state of affairs is not something the universities really have any incentive to address in any way whatsoever.

I know that people will eventually stop being interested in science as the bad career prospects become more generally known. The computer science departments have already had catastrophic losses in popularity.

31. **Stefan**
September 26, 2006

To All:

Does anyone here happen to know the current prospects for (any of the following):

a) computational science / mathematical programming;

b) software engineering / consultancy;

c) computer science;

d) computer hardware / electrical engineering and related;

e) biophysics;

f) physics-related Wall Street type jobs (is it called 'actuarial science' by any chance)?

Stefan

32. **Patrick**
September 26, 2006

Stefan,

I got lucky and am in my 2nd year in a tenure track position in biophysics. From my experience during my job search it seems to me that, if your goal is a tenure track position at a research university, the job market is pretty awful across the board. Part of this may be due to the fact that the biological sciences flood the market with Ph.D.'s to an even greater degree than physics. During 3 rounds of searching (3rd time's a charm), I interviewed at Chemistry, Physics, Biochemistry, Biophysics and Molecular Biology departments (all told I must have sent out >200 applications). On average there were ~300 applicants for every position and I'm told that this is normal.

Overproduction of Ph.D.'s is a problem in all of the sciences. Personally I think that part of the answer is to ecourage the creation of research options other than the holy grail of the tenure track position. Some of the most talented people I know simply want to do research and don't want the hassle of running and funding a whole group. As it is there are very few places for these people. The current system essentially forces you to find a tenure track job within a certain amount of time after your dissertation or else leave academic science entirely. I'd like to see the creation of more quasi-permanent research associate type positions. That way a person could pursue research and enjoy a reasonable degree of job security without landing one of the few available faculty positions. I suspect that science has lost a great deal of talent due to the "up or out" philosophy. And let's face it, the skills that make you good at building and funding a research group are not necessarily correlated with the skills that allow you to do important and original science.

33. **TheGraduate**
September 26, 2006

Stefan,

Concerning the outlook for engineering, I found this webcast very interesting: http://mitworld.mit.edu/video/321/
(I hear one can hire 10 Indian engineers for the price of one American one.)

34. **Timothy Clemans**
September 26, 2006

Andrew Wiles (algebraicnumber theorist) and Edward Witten (mathematical physicist) should write an article together about their work and lives as academics who have had a profound impact on their subject. There's a really good book on becoming a mathematician called A Mathematician's Survival Guide: Graduate School and Early Career Development which I've read at the UW Mathematics Research Library. I really like Dr. Woit's biography of Edward Witten in his book exspecially the idea that he might be from outerspace, because he disappeared on Dr. Woit.

35. **Peter Woit**
September 26, 2006

Timothy,

I certainly think it would be great if Witten would write a memoir about his professional life, that would be fascinating. The part in the book about Witten possibly being from a superior race of non-humans was a joke (I think…)

36. **TheGraduate**
September 26, 2006

Patrick,

I think all the signals seem to suggest that the US is planning to buy its science from China and India one day. Everything seems to point to significant overfunding of 'stuff' (super computers, huge new buildings etc) and significant underfunding of people.

Tim:

The survival guide is excellent. Krantz is interesting … kind of ideosyncratic … naming some of his hypothetical people in the book after porn stars etc … last information that I saw on him, he was working on the mathematics of plastic surgery

37. **Timothy Clemans**
September 26, 2006

Dr. Woit,

I liked your reason for the thought because it sounded reasonable and it was a little fun part to read. A mention of a person thought to maybe teleporting when no one was thought to be watching in a non-fiction book about the wonderful connection between physics and mathematics and the problems with string theory was a great thing to put on paper especially since it was Witten.

What I don't understand about Witten is why he believes that he can conclude that strings and extra-dimensions do exist and that tring theory is testable. To me it he is taking Einstein's view that General Theory of Relativity was just too beautiful to be wrong and applied it string theory which can be made to say just about anything. Couldn't someone have overlooked the mathematics and missed something like what happened with Andrew Wiles when Nick Katz was reviewing his argument for the construction of an Euler system to give the precise upper bound for the size of the Selmer group, in 1992? – http://www.pbs.org/wgbh/nova/elegant/view-witten.html

38. **Timothy Clemans**
September 26, 2006

If Witten writes a memoir about his professional life, I hope he talks about how he was able to conclude that strings really exist.

39. **Peter Woit**
September 26, 2006

Timothy,

I've never heard Witten claim that string theory in its present state is testable, or that he is sure strings exist. As far as I know, his point of view is that string theory is an idea worth pursuing for various reasons, in its present state it may very well be wrong, but there is enough interesting structure to what has been learned about string theory to believe that it is somehow connected to reality.

There may very well be something crucially wrong in the standard arguments about string theory. Unfortunately the theory is not precisely defined the way Wiles's argument was, so you can't just go through it and look carefull for holes in the argument. The problem is that there are lots of possible holes…

40. **lostsoul Ph. D.**
September 26, 2006

Maybe the Simons model is the way to go: make some money by deploying the natural smarts characteristic of the profoundly numerate, then get down and do what you want. (Did Gibbs do this?) All told, though, this looks like a harsh option; to be at once both Gauss and Brunswick, or Hardy and Ramanujan? Maybe science will revert to the hobby status it had 200 years ago. That, at least, would weed out the supernumerary doctors among us.

41. **Carl**
September 26, 2006

The hilarious part of all this is that right after hearing a bunch of academics complaining about how hard academic jobs are to obtain because of excessive competition, they go into what crappy jobs they are in terms of pay and work hours. You'd think that a freshman class in economics would correct their thinking, but I figure that what they really need is a couple years hard work in the real world.

42. **Timothy Clemans**
September 26, 2006

"NOVA: It seems like the standard criticism of string theory is that it isn't testable. How do you respond to that criticism?

Witten: One very important aspect of string theory is definitely testable. That was the prediction of supersymmetry, which emerged from string theory in the early '70s. Experimentalists are still trying to test it. It hasn't been proved that supersymmetry is right. But there is a very precise relationship among the interaction rates of different kinds of particles which follows from supersymmetry and which has been tested successfully. Because of that and a variety of other clues, many physicists do suspect that our present decade is the decade when supersymmetry will be discovered. Supersymmetry is a very big prediction; it would be interesting to delve into history and try to see any theory that ever made as big a prediction as that."

"NOVA: Do you think it's possible that string theory will turn out to be wrong, or at least some branch of knowledge that just isn't connected to nature?

Witten: I guess it's possible that string theory could be wrong. But if it is in fact wrong, it's amazing that it's been so rich and has survived so many brushes with catastrophe and has linked up with the established physical theories in so many ways, providing so many new insights about them. I wouldn't have thought that a wrong theory should lead us to understand better the ordinary quantum field theories or to have new insights about the quantum states of black holes.

The question reminds me a little bit of the question about interpreting fossils. When fossils were first explored 100 or 200 years ago, some people thought they were traces of past life that had survived in the rocks and others thought that they had been placed there at the creation of the universe by the creator in order to test our faith. So I guess string theory might be wrong, but it would seem like a kind of cosmic conspiracy."
– http://www.pbs.org/wgbh/nova/elegant/view-witten.html

43. **Timothy Clemans**
September 26, 2006

Dr. Woit has Witten said anything about your book?

44. **Peter Woit**
September 26, 2006

Timothy,

I didn't realize that Witten had ever made that definite a claim string theory was testable by looking for supersymmetry. It would be interesting to know if he still would claim this, will also be interesting to see what he has to say after the LHC results are in, if they don't see supersymmetry. He could try and weasel out of his claim by saying that the supersymmetry breaking scale might just be higher than can be observed at the LHC, but that kind of non-straight-forward slipperiness doesn't normally seem to be his style.

Maybe he will finally admit that string theory doesn't work if the LHC doesn't see supersymmetry.

I sent him an early version of the manuscript, he wrote back with some comments, basically saying he disagreed with me, but that there were a lot of books out there he disagreed with. He also gave some of the standard arguments for supersymmetry and string theory, but he didn't claim that string theory was testable by looking for supersymmetry. This was now a couple years ago, I haven't discussed this with him recently.

45. **Timothy Clemans**

September 26, 2006

Okay now I'm just confused. Is Witten saying that if he assumes that strings exist then one of the conclusions is that there are extra-dimensions? If so then why does he implie that the existence of strings is very likely because otherwise it would seem like nature was playing tricks with us?

46. **Peter Woit**
September 26, 2006

Timothy,
If you want to know more about the standard story about string theory that Witten is one of the main people responsible for promoting, you should read one of the many books on the subject, like Brian Greene's, which answer questions like yours.

47. **woit**
September 26, 2006

Timothy,

You're kind of taking over this comment section, please stop.

48. **Gina**
September 26, 2006

Thanks, Peter, for the explanations. On the third point you wrote

"Actually, the bootstrap program did fail as a theory of the strong interactions, and was pretty much killed off by QCD. Many of the people who worked on it certainly admitted that it failed, other die-hards never did admit this, but stopped being taken seriously by most theorists. Certainly some things learned from this were ultimately useful, but the program failed to do what it was intended to, and sensible people admitted this. "

Let's take the analogy between string theory and the "bootstrap pogram" a little further. Judging from your attitude towards string theory, it looks that you would have expected proponents of the "bootstrap program" to admit failure even *before* an alternative successful theory (QCD) came along. This seems unreasonable.

It looks that the "bootstrap program" was a reasonable line of attack before QCD and had important contributions in any case. (You can thus say that Chew and the bootstrap people's were "false" but they were not "wrong" in the sense that these people did, as scientists, the right thing.)

Moreover, a theory "like" QCD as the solution
for strong interaction was something people considered.(In NEW you described how Gross almost succeeded to prove this task is impossible… just before he proved it being possible.) So for the case of strong interaction there was a reasonable alternative possibility of some quantum field theory being successful. Some people declared prematurely) this possibility as dead (from reasons that were and probably still are of some interest and value), but they were wrong.

On the other hand for the case of string theory there isn't really any "shelf" alternatives so this makes the situation even more favorable to string theory.

49. **TheGraduate**
September 26, 2006

Peter,

Could you take a moment to define the 'bootstrap program' ? If I'm not mistaken bootstrap is a bit of bayesian statistics which also seems to be the thinking behind the landscape. Can you discuss the relationship between the bootstrap program and the Landscape if there is one?

50. **woit**
September 26, 2006

http://www.mth.kcl.ac.uk/~streater/lostcauses.html#XIV

51. **TheGraduate**
September 26, 2006

Thanks.

52. **Chris Oakley**
September 27, 2006

More press coverage of NEW and TTWP in today's New York Sun.
It pains me to see Professor Sir Martin Rees so unashamedly anthropic.

53. **Bee**
September 27, 2006

Hi Peter,

well if Holt doesn't read my blog, he should 😉

Maybe because I'm older than you I'm less of the opinion that the problem is inherently in the judgment of senior people. As far as pushing people into working on bad ideas about string theory (or other trendy subjects), I just don't see that it's mainly the older people doing this.

True, I didn't mean to blame age reasons. The problem is the judgement of established people who are in the power of selecting newly employed candidates. This however comes most often with an age gap. That doesn't mean though that this necessarily has to be the case, and I know several quite notable exceptions myself.

Then there is the issue of inertia. One person doing research might be willing and flexible enough to readjust his/hers projects over time. A group is less likely to do so. The larger the group the stronger the resistance to change. String theory is a prime example, but not the only one. It's not even a problem of science in the first place, but you'll find it in other fields as well. E.g. politics. How many of our current problems are caused by a generation of politicians who just keep doing what has been done, because that's what always has been done, and refuse to realize that things have changed dramatically in the last decades?

Best,

B.

54. **JPL**
September 27, 2006

Gina:

*Let's take the analogy between string theory and the "bootstrap pogram" a little further. Judging from your attitude towards string theory, it looks that you would have expected proponents of the "bootstrap program" to admit failure even *before* an alternative successful theory (QCD) came along. This seems unreasonable.*

Actually, unreasonable or not, that is more or less what took place. After the GWS model for Weak Interactions came along and Neutral Current experiments backed its perdictions, people began to believe that Strong Interactions were most likely also understood by a Yang-Mills theory (Sakurai had tried this in the early sixties before the Higgs mechanism came along); Deep Inelastic scattering experiments had essentially shown that quarks were massless point-sources at high energy which contradicted the whole "microphysical democracy" spirit of the bootstrap. By the late sixties early seventies the analytical S-matrix school had essentially lost steam even before assymptotic freedom was understood and QCD fully recognized.

Today we may be going through a similar stage where String theorising is losing steam on its own even if no obvious alternatives have been "shelved", as you put it! One thing does not imply the other, I am afraid.

55. **jeremy**
September 27, 2006

Peter,

This is a review on NEW, sort of.

http://www.nysun.com/article/40425

56. **Thomas Love**
September 27, 2006

Max Planck wrote:

An important scientific innovation rarely makes its way by gradually winning over and converting its opponents: it rarely happens that Saul becomes Paul. What does happen is that its opponents gradually die out, and that the growing generation is familiarised with the ideas from the beginning.
(New York 1949).

Unfortunately, his statement is true about untrue "innovations". Many of the major opponents of string theory are dying and leaving string theorists in charge.

The people who need to read NEW are the people who control the purse strings. Cut the financing and we will be able to hear the rest of the orchestra (good music requires more than strings).

57. **Gina**
September 27, 2006

WHY THE EMPHASIS ON TWO DIMENSIONAL MODELS?

Peter wrote

" It is the precise expression of the mathematical relationship of representation theory and QFT that has been worked out in recent decades in two dimensions, exactly the thing that I would argue we should be trying to understand in the physical case of four dimensions."

Indeed one very nice point raised in Peter's book is the fact that many of the successes of physics and mathematics related to string theory and earlier physics are coming from two dimensional model. I asked around among my friends:

" Why can't you do anything as impressive for D>2, after all nobody, not even strings theorists claim that our universe has two dimensions? "

It seems that for D>2 scientists are simply stuck and things look very gloomy. "Arn't there any ideas around," I asked. Well, there are a few. One guy told me with a spark in his eyes about an idea to move directly from D=2 to D=4 and to base models on "homological" notions which will extend important "duality" properties for planar model. He talked about things like "Poincare duality" and "signature" and was quite excited but then admitted that these are all just ideas. (I guess this is the same old Poincare.) Another guy had much hope from representation theoretic extensions of notions from conformal analysis which are prominent for two dimensional models. But this is also in a very premature state. A third guy praised the "Heisenberg Lie group" as a place to "be in high dimensions and to feel in two dimensions".

There are ideas but overall there is some feeling that studying higher dimensional models is a waste of time. Some of these guys actually spent a lot of time and got nowhere.

I tried to be tough on them and I asked if sticking to the cozy D=2 and looking for the coin under the lamp is all about the summer salary.

"No, Gina" they said "this is not the reason". They said they simply do not know what do. They need a tip of a string to hold to in order to start (Often it turns out they just hold their own shoe laces). In this case they have nothing, they said. The are quite savvy in failures they said. But doing D>2 leads to "not even a failure". They did sound convincing but you never know with these wise guys.

USEFUL DIVERGENT SERIES

Peter wrote to my question: "Some divergent series are "asymptotic" approximations to some function, which means that, at a fixed order, the truncated series is a better and better approximation to the function as the expansion parameter gets small (even though, at fixed small parameter, as you go to higher order, the series sooner or later diverges). The perturbation series for QED is supposed to be such an asymptotic expansion. This kind of divergent series can be quite use ful, giving very good approximations."

Hmmm, this sounds very good indeed. A sort of "Calculus I" way I can think about such a thing of a function which is described at any point by a useful divergent series is that the terms of the series are themselves only approximation to a correct description of the function by a convergent series. (and these approximations are worse and worse for higher terms in the series.) Is this naive way to look at it reasonable? Is there a better intuition for what these useful divergent series are?

UNIFORM OPINIONS

Peter asks in the book if mathematician will regard superstring theory as mathematics and writes: “They [mathematicians] would uniformly say: ‘certainly not!”

I do not know about mathematicians attitude about string theory but judging from my experience with mathematicians, I think that no set of mathematicians will have a uniform opinion about ANYTHING.

Just to make the mathematicians reading this absolutely happy let me state this deep insight more carefully

Every set of mathematicians will not have a uniform opinion about any issue except possibly for the following cases

1) It is the empty set of mathematicians

2) It is a set of a single mathematician over an infinitesimally small amount of time

58. **Thomas Love**
September 28, 2006

Gina Says:

WHY THE EMPHASIS ON TWO DIMENSIONAL MODELS?

As a PhD mathematician, I know the answer: because the math is easier there.

Gina, you and quite a few others seem confused about the meaning of higher dimensions. Think of a dimension as an entry in an inventory form; how many numbers are required to describe the situation? (the standard reporter questions) Clearly, we need to know where and when (x,y,z,y), but we also need to know the field strengths, electric, magnetic, gravity. etc. Each of those require another entry in the form, i.e. a dimension. We also need to know the field strength due to the presence of electrons, protons, neutrinos, pions, etc (some of thse numbers may be redundant). The list becomes interesting only when we can weave it into one coherent whole, one geometry. Then Einstein’s vision of particles emerging from geometry would be realized. After Ed Witten spoke at the 1987 AMS meeting in Salt Lake City, I asked him if strings emerged from the geometry or had to be imposed. He said they were imposed. I knew then that string theory would lead no where.

59. **Gina**
September 28, 2006

” Gina, you and quite a few others seem confused about the meaning of higher dimensions.”

You bet! I am very confused!!!

Anyway, I just wanted to say that people do try to study lattice models and other models for D>2 but somehow do not find there the miracles found for D=2 and they say it is very difficult.

60. **Roger Schlafly**
September 29, 2006

Peter, don’t let Motl bait you into name-calling. His name-calling makes him sound like someone who is upset because he has been exposed for what he is.

61. **Peter Woit**
September 29, 2006

Roger,

Thanks, the advice is good, but all I did was refer to him as “sensible”….

62. **Charles**
September 30, 2006

Dear Dr. Woit,

However much you might disagree with string theory, how can you justify endorsing an article written by a man who thinks that finding a final theory of everything will by an irrelevance hardly noticed by science.

You and Smolin give the outward appearance of caring very deeply about this subject, the solution to which could rightly be called the greatest scientific discovery of all time.

I understand you might have given the interviews in good faith, but I do not understand why you don’t wish to distance yourself from this man’s views. Instead, you proudly link to his text.

Is it the money from promoting the book? Or is your vendetta against the string community worth bringing down the whole of physics with it?

When physics is reduced to a branch of sociology in the public’s eyes, things like this happen. You and Smolin bear some of the responsibility.

63. **Charles**
September 30, 2006

My “this” link does not appear to work. It is supposed to link to an announcement made last week from Reading University, UK that they are closing down their physics department due to lack of students interested in physics.

64. **Peter Woit**
September 30, 2006

Charles,

I actually had nothing at all to do with the New Yorker article, never talked to Jim Holt and didn’t “endorse” what he writes, other than to note that he mostly gets his facts right. The last line of his text that you object to so strongly is just making the obvious point that most of science is decoupled from the details of any unified TOE. Go talk to any biologist and ask them what the impact of a TOE would be on their research work.

Your claim that the publication of my book and Lee Smolin’s this month are partially responsible for the closing of the Reading University physics department is just hysterical and absurd. Both Smolin and I care deeply about the field of fundamental physics and its health. We wrote our books not to make money (I'll remind you that mine was originally intended to be published by a university press, and was not aimed at the general public, but string theory referees stopped that), but because we are concerned about what we see as a crisis in the field and how it is being pursued. We're not the ones who created the crisis. You seem to think the answer to it is shooting the messengers.

65. **nigel cook**
September 30, 2006

By the way, my nearest university physics undergraduate-teaching department closed: see http://www2.essex.ac.uk/physics/ for the remains of the department and what it now does.

For the CAUSE of the decline see http://www.tes.co.uk/2268414

'… A-level physics entries fell 49.5 per cent between 1982 and 2005, from 55,728 to 28,119. Meanwhile, the proportion of 16-year-olds studying A-level physics fell from 6 per cent in 1990 to 3.8 per cent in 2004.'

See also http://www.google.co.uk/search?hl=en&q=A-level+physics+decline&meta= for much more info.

This correlates with string theory's rise to fame, not Peter Woit's activities (Not Even Wrong was only set up new in March 2004).

Stephen Hawking and other string supporters do raise public awareness of string theory to great heights in the UK, and physics books sell, but people don't study physics. Maybe there is a fear of extra dimensions and wormholes … or maybe they just don't believe it without some evidence.

66. **Who**
October 2, 2006

I've been looking for a way to gauge sales of NEW in the UK and found this:

http://www.amazon.co.uk/gp/bestsellers/books/278434/ref=pd_ts_b_nav/026-8303087-5718006

this is the UK amazon bestseller list in the "particle and high-energy physics" subcategory of "physics" in "nature and science"

as of Monday 2 October 1:00 PM eastern, when I looked, Not Even Wrong was #1 on that UK bestseller list

the UK edition of Smolin's book is scheduled to come out February 2007—as of now UK amazon is not selling the Smolin book, it simply refers potential customers to overseas dealers, presumably because this saves market for the UK edition next year.

interestingly at least at the moment with NEW #1 on the UK list, it is leading a number of other books with mass appeal such as Warped Passages (#5 on the list) and two of Brian Greene (Elegant, which is #13, and Fabric, which is #16)

I have no idea how this translates into numbers of copies sold, but it represents a strong comparative showing—-the other titles being aimed at a broader less technically sophisticated audience.

67. **Who**
October 3, 2006

to correct something I said yesterday,
as of today UK amazon is selling Smolin's book directly instead of referring the customer to overseas distributors
http://www.amazon.co.uk/Trouble-Physics-String-Theory-Science/dp/0618551050/ref=pd_bxgy_b_img_b/026-8303087-5718006?ie=UTF8

the price is 13-some pounds and the book is paired with Peter's book in a package deal for 26 pounds.

this is not how it was yesterday—then it looked like UK amazon was going to wait for the UK edition to come out in February. perhaps there was a cataloging error in the computer.

so far there are not many reviews of Smolin's book at its UK amazon page, only two that I saw

68. **Who**
October 6, 2006

after 3 days of selling the book, during which it shot up to #6 on the UK amazon physics bestseller list, and to the #2 place on the the narrower "general physics" list, UK amazon stopped taking orders.

It is now no longer selling the book but instead gives a link to some distributors in the USA where it can be ordered—this is back to how it was before 2 October.

it could be a "publisher turf" thing, or some hitch in logistics. for those three days UK amazon was trying to serve as a relay—"order it from us, we will get it from overseas and ship it to you" in an estimated 1 to 2 weeks. They did not have the book in stock. Probably got more orders than they could reasonably handle in that fashion.

Release of the UK edition of Smolin's book is planned for February 2007. I forget who the UK publisher is. The cover is not that nice electric blue color, so I reckon the US edition is way preferable and would recommend Brits order from overseas. 🙂

69. **Who**
October 10, 2006

I occasionally check the UK amazon physics bestseller list and have been seeing Peter's book at the top of it frequently during the past week or so, for instance today at 7 AM pacific, which I suppose is 2 PM greenwich

1. Not Even Wrong: The Failure of String Theory and the Continuing Challenge to Unify the Laws of Physics
by Peter Woit 1414

2. Wiring Systems and Fault Finding: For Installation Electricians
by Brian Scaddan 1824

3. The Illustrated Brief History of Time

by S.W. Hawking 1927

4. Physics (Revise AS & A2 (Combined) S.) 1935

5. God and the New Physics
by P.C.W. Davies 2197

6. The Mind of God: Science and the Search for Ultimate Meaning (Penguin Press Science S.)
by P.C.W. Davies 2205

7. University Physics with Modern Physics with Mastering Physics (International Edition)
by Francis W. Sears 2212

8. The Fabric of the Cosmos: Space, Time and the Texture of Reality (Penguin Press Science S.)
by Brian Greene 2550

9. The Art of Electronics
by Paul Horowitz 2705

the fourdigit numbers are store-wide sales rank, among all book sales.
the physics list, initial segment shown here, is obviously quite broad and includes exam review, pop phys, god-and-wonder, electronics, audio, straight physics textbooks. Yesterday it had a Terry Pratchett. And of course it has Brian Greene and Stephen Hawking. One can ask how a serious scholarly work like Peter's gets to the top of such a list.

2006 Nobel Prize for Physics

September 26, 2006
Categories: Uncategorized

No, I don't have any idea who will win this year, but the announcement will be a week from today, on Tuesday October 3. After my initial success in Nobel Prize prognostication, I've now retired from that game, but encourage others to play.

Comments

1. **Tommaso Dorigo**
   September 26, 2006

   I would vote for Giorgio Bellettini (with a few possible others) for the top quark discovery. Wishful thinking of course, but who knows ?

   And I know I am biased, but I think the man would deserve it for his lifelong effort, which indeed culminated in the CDF 94 evidence and then CDF-D0 95 observation.

   Cheers,
   T.

2. **Shantanu**
   September 26, 2006

   I would say someone from the COBE team for discovsering anisotropies in the
   CMB. This year they got the Gruber prize.

3. **Peter Orland**
   September 26, 2006

   The evidence for the acceleration of the expansion of the universe is now quite good. Why not someone involved in the supernova red shifts?

4. **BobD**
   September 26, 2006

   I think the COBE team did a beautiful job, but in my view they simply confirmed existing expectations.

   I consider the evidence for an accelerated expansion as still iffy, though that result remains an electrifying possibility. However I wouldn't bet against some recognition of dark matter this year. Though Zwicky is gone, Vera Rubin is still quite active…

5. **Jimbo**
   September 26, 2006

   Has everyone forgot ? Alan Guth (& probably Andre Linde) were smiling the entire week the WMAP 3rd yr. observational reports were announced. Alan is a shoe-in, & perhaps Andre as well.

6. **Analyzer**
   September 27, 2006

   Alan Guth (& probably Andre Linde) were smiling the entire week the WMAP 3rd yr. observational reports were announced. Alan is a shoe-in, & perhaps Andre as well.

   Pffft, no way. WMAP's results have kept inflation healthily afloat, but it's hardly as if the matter is closed and inflation is proven. The evidence for dark energy is more convincing, and even that is not up to Nobel snuff; the prize committee goes for sure things, often many years or decades after the actual research was done and after everyone agrees on the results.

   I don't know enough about the entire range of subfields of physics to make a guess, but the last few years looked like this:

   2005: Quantum optics
   2004: QCD
   2003: Superconductivity/superfluidity
   2002: High-energy astrophysics
   2001: Bose-Einstein condensation
   2000: Semiconductors
   1999: Electroweak interactions

   Draw what conclusions you will, but remember, when you cast your prediction, that there is more to physics than particle physics and speculative cosmology.

7. **Relativist**
   September 27, 2006

   Well firstly it won't be a string theorist (at least not for their work on string theory).
   The two outstanding ones not yet given:
   – Higgs, Yang and Mills for the Higgs particle and Yang Mills theory (Yang has it for parity but not for Yang-Mills – I assume it is the same Yang?) But maybe also Goldstone? Higgs and Goldstone one year, Yang and Mills another?
   – Aharonov and Bohm for the Aharonv Bohmn effect. Well Bohm is dead but Aharonov is still alive I think.

8. **Thomas Larsson**
   September 27, 2006

   If the Higgs is discovered, there will probably be two rapid prizes: a theoretical one to Higgs, Brout and Englert (Polyakov and Migdal probably not), and an experimental one for the actual discovery. But we are not there yet.

My personal favorites are Belavin-Polyakov-Zamolodchikov for the application of CFT to statistical physics, but I'm sure that they won't win this year neither.

9. **David Cobden**
September 27, 2006

With Aharonov they'd have to include Michael Berry of Berry's phase, which is a more general concept and surely deserves a prize in its own right.

10. **ObsessiveMathsFreak**
September 27, 2006

The guys who created the Bose-Einstien condensate deserve some kudos. That was a top class piece of experimental physics.

11. **A**
September 27, 2006

To Tommaso: not discovering the top would have deserved a Nobel prize. Discovering that top (or its right-handed component) is composite would deserve a Nobel prize. But, so far, discovering the top with the expected charge, expected strong interactions, expected weak interactions, expected spin, expected mass (from precision data) and expected name is not interesting enough.

12. **Navneeth**
September 27, 2006

ObsessiveMathsFreak,
http://nobelprize.org/nobel_prizes/physics/laureates/2001/index.html

13. **JK**
September 27, 2006

Eli Yablonovitch and Sajeev John for the theory of photonic crystals. Although 3D crystals are found in nature and 2D crystals are already technologically important, perhaps the prize will wait for an experimental synthesis of 3D crystals.

Victor Veselago and John Pendry for theory of negative refractive index. David Smith for experimental realisation.

14. **sean m.**
September 27, 2006

i have to second michael berry (and perhaps aharonov). this has been wildly influential work in condensed matter.

15. **ksh95**
September 27, 2006

JK says:

> perhaps the prize will wait for an experimental synthesis of 3D crystals.

They have been making 3D crystals since the very begining. Jesus man; experimentalists are infinitely more capable than that. 3D photonic Crystal fabrication is an undergraduate project at a low level non-research institution.

Anyway I dont think Photonic Crystals are Nobel worthy. It's basically only classical field theory in periodic space.

Try supersolid

16. **ksh95**
September 27, 2006

Analyzer says

> but remember, when you cast your prediction, that there is more to physics than particle physics and speculative cosmology.

Yes people, remember that the blogosphere does not represent the average physics department…In fact, I'm reasonable confident that the majority of physicists specialize in condensed matter.

17. **Belizean**
September 27, 2006

David Deutsch, founder of quantum information theory. Primitive quantum computations have been performed in labs. So you have an important theory confirmed by experiment.

18. **Zelah**
September 27, 2006

Hi!

The only outstanding candidates spoken about here are
M Berry and V Rubin!

My vote is for V Rubin, but the Crafoord prize 2005 was awarded James Gunn, James Peebles, and Martin Rees for Dark Matter (in my opinion scandalous!). So I do not have much hope.

Onto M Berry. The problem is that who would he share the prize with?

Does anyone care about Statistical Mechanics? And anyway, there is the small problem of R Baxter, McCoy, Wu et al……

Expect the unexpected!

Zelah

19. **Christine**
September 27, 2006

The evidence for the acceleration of the expansion of the universe is now quite good.

There are some evidences, but they are not unquestionable. For instance, we first must learn much more about Type Ia supernovae.

Christine

20. **DMS**
September 27, 2006

Yoichiro Nambu.

21. **Bee**
September 27, 2006

I'd also put my bet on WMAP/CMB, exact measurement of parameters in LambdaCDM, esp confirmation of cc nonzero. I don't know though who'd be the person to name.

22. **A.J.**
September 27, 2006

Nambu may deserve a Nobel, but I'd be surprised if he gets it this time around. He'll probably have to wait till after the LHC turns on.

23. **A Babe in the Universe**
September 27, 2006

I echo Kea and Christine in that evidence for inflation and cosmic acceleration is still shaky. The low-l data rules out inflation's prediction. Evidence for acceleration is based entirely upon redshifts.

24. **Dick Thompson**
September 27, 2006

Fadeev and Popov, who showed how to quantize gauge theory and discovered the "ghosts".

25. **King Ray**
September 27, 2006

Peter Woit and Lee Smolin for their efforts on behalf of the welfare of theoretical physics.

26. **Jeff**
September 27, 2006

How about Paul Ginsparg for his contribution to the development of physics?

27. **Renormalized**
September 27, 2006

Belizean Says:
"David Deutsch, founder of quantum information theory. Primitive quantum computations have been performed in labs. So you have an important theory confirmed by experiment."

Do you have a link to this experimental evidence? I don't believe quantum computations have been verified.

28. **CYD**
September 27, 2006

David Pines for plasmons and the random phase approximation, and Conyers Herring for spin waves and the orthogonalized-plane-waves method in solids.

29. **Tommaso Dorigo**
September 27, 2006

A said:

"not discovering the top would have deserved a Nobel prize. Discovering that top (or its right-handed component) is composite would deserve a Nobel prize. But, so far, discovering the top with the expected charge, expected strong interactions, expected weak interactions, expected spin, expected mass (from precision data) and expected name is not interesting enough"

Dear A,
I do not think some thing has to be intrinsically unexpected to deserve a nobel prize. The top quark discovery was largely expected, but it involved two decades of searches, at least one published wrong observation (UA2 1987), and theoretical predictions for the mass which scaled with the experimental lower limits for quite a while. The CDF experiment was conceived in 1980 or so, built by 1985, started taking data in 1987, saw the first top event in 1988, and had to be upgraded with a silicon detector to find the elusive top, whose mass kept it unreachable otherwise. I think the overall achievement of observing a picobarn-sized signal in collisions occurring with cross sections of 60 millibarns is quite a feat, and the people responsible for the discovery deserve recognition.
Cheers,
T.

30. **Belizean**
September 27, 2006

Renormalized,

Here's one that took 10 seconds to with Google:

http://domino.watson.ibm.com/comm/pr.nsf/pages/news.20011219_quantum.html

31. **Scott Aaronson**
September 27, 2006

Do you have a link to this experimental evidence? I don't believe quantum computations have been verified.

Small quantum computations have been performed — see here for experimental confirmation that 15=3*5 (with small probability of error). The challenge is scaling up to a nontrivial number of qubits.

Of course, the *interesting* experimental discovery would be a fundamental reason why quantum computing *isn't* scalable!

32. **Florifulgurator**
September 27, 2006

Here´s my tip:

Give it to Mathematical Physics,
not String Theory,
but Knot Theory,
(Knit String Theory would be O.K. for me….)
E.g. Witten or Drinfeld,
or you suggest a 3rd one.
Since Maths works slow,
it would be good to surpass the rule that only living ones be Nobel pontificated,
& sharing the Prize money could be Dadificated (randomized in a certain sense)
e.g. by awarding it to an element of a time-space-brainlard chain of a tree (or Feynman Diagram) of brainlard evolution:
E.g. give it to Witten for the Witten-Jones Generalization (skipping Kaufmann)
or e.g. give it to Drinfeld for his Quasitriangular Quantum Groups (generalizing the Yang-Baxter relations).
…

33. **Florifulgurator**
September 27, 2006

…(cont)
Since both had the Fields medal,
suggested Nobel Prize could probably form a noice antiparticle to Perelman

34. **Douglas Natelson**
September 27, 2006

I'd really enjoy M. Berry and Y. Aharonov. Great stuff. There's also a long-overdue prize to recognize the great materials growers (MBE?). Regarding Conyers Herring, they'd better hurry…. He was not exactly a spring chicken when I was a grad student at Stanford ten years ago. Supersolid is too new and too controversial. For those interested in chemistry, at some point I'd be willing to bet a fair bit of money on Whitesides for self-assembly.

35. **David Heffernan**
September 28, 2006

I'd like to see it go to Kobayashi and Maskawa for their theory of CP Violation. As of this summer the unitary triangle is looking very consistent, and their names have come up before, so I don't think it would be too unexpected.

36. **hongbao zhang**
September 28, 2006

If the prize were given to theory, I think there would be two possilbilities.
1 to C.N.Yang Dyson and Fadeev etc
2 to Hawking and Bekenstein etc

:)^_^

37. **David Cobden**
September 28, 2006

I don't think the prize has ever gone to a theory that was not already experimentally verified to a very high degree, such that it was essentially completely uncontroversial within the physics community. (Is asymptotic freedom an exception?)
Nor I think has it gone to a technical prediction (such as photonic bands) that hasn't already had significant practical consequences. That makes several of the above suggestions seem very unlikely.

38. **Physiker**
September 28, 2006

My bet:

Sir Samuel Edwards.

Among other things:

Together with Phil Anderson (Nobel laureate), he pioneered the replica theory of disordered systems which has found far reaching applications even beyond physics.

Together with P. G. de Gennes (Nobel laureate), he pioneered the field theoretic approach to polymer physics and placed soft matter physics in a solid theoretical foundation.

39. **Patrick**
September 28, 2006

Physiker,

If they gave it to Edwards would they have to include G. Parisi as well?

40. **anon.**
September 28, 2006

A.J. wrote:

Nambu may deserve a Nobel, but I'd be surprised if he gets it this time around. He'll probably have to wait till after the LHC turns on.

What idea of Nambu do you have in mind? I thought some of the people involved in the early development of QCD (Nambu, Bjorken…) deserved a prize, but I don't expect that one to ever be given now that the later development of asymptotic freedom got the prize.

Then there's Nambu – Goldstone, but it's hard to imagine why that would get a prize now since it hasn't in the past. (It is a fundamental idea that's experimentally verified and applicable to all sorts of physics, so it seems as Nobel-worthy as any theoretical development I can think of.)

41. **Kent G. Budge**
September 28, 2006

Since I can't find an email address on your page (probably for good reasons) please indulge me in asking an unrelated question here. I have a Ph.D. in astronomy, and I have some familiarity with the mathematics of the Standard Model, but I'm by no means a particle physicist. Can you recommend a book or books on post-Standard Model theories that would be at my level?

Everything I've found so far seems to be aimed either at nonmathematicians (and is therefore useless for trying to understand the mathematics behind the models) or at people who are almost as knowledgeable as the author (and is therefore an inpenetrable display of how smart the author thinks he is.)

I'm looking for something on the level of Cottingham and Greenwood's "An Inroduction to the Standard Model of Particle Physics,", which I enjoyed immensely.

If you think your own book is in this category, then I'll accept that datum; but my sense is that you have largely avoided formulas. I want formulas, but I want them patiently explained.

42. **A.J.**
September 28, 2006

anon.,

Yes, I was thinking of spontaneous symmetry breaking. Nambu was the first to introduce the idea into particle physics, so he's certainly a candidate for the prize if they find a Higgs boson at LHC. I don't think he's a sure thing though. There were a lot of people involved in the idea. I think Higgs is the only one I'd put money on.

43. **anon.**
September 28, 2006

It seems strange that a Nobel prize for general properties of spontaneous symmetry breaking should be contingent on the discovery of a Higgs boson. The understanding of SSB due to Nambu, Goldstone, and others is amply supported by superconductivity, by chiral symmetry breaking in QCD, and other applications. I would think a Nobel prize for the Higgs should go specifically to people who studied SSB applied to gauge theories; as far as I know, Nambu didn't specifically propose that, while plenty of others did.

I think Nambu and Goldstone probably do deserve a Nobel, but it would seem odd if it were only given now, decades after it could have been. On the other hand, Ginzburg just got the prize recently, so maybe it wouldn't be so unreasonable.

44. **Count Iblis**
September 28, 2006

What about the theory of granular media? This is a relatively new but very important field in condensed matter…

45. **Peter Woit**
September 28, 2006

Kent G. Budge,

My book does contain short descriptions of many ideas about beyond the standard model physics, together with suggestions for further reading about these subjects. I don't know of a single good book of the kind you are asking about. One thing a little bit like what you are asking for is a book by Pierre Ramond called "Journeys Beyond the Standard Model".

46. **Jimbo**
September 28, 2006

Dear Kent,

THE book you're seeking is entitled:"The Ideas of Particle Physics, an introduction for scientists", by Coughlan & Dodd, 2nd ed., Cambridge press, ~ 200 pgs. A good companion book for it(at the same math level, but in more depth) is `Elementary Particles', by Hughes, 3rd ed.,also on Cambridge, ~ 400 pgs. Both are in paperback.

47. **Scott Aaronson**
September 28, 2006

Kent: If you want something more discursive than a textbook, try Penrose's *The Road to Reality*. It doesn't cover the post-Standard-Model theories with any pretense of rigor, but it does have formulas and it doesn't aim low.

48. **Kent G. Budge**
September 28, 2006

Thanks to all for the suggestions. It happens I live and work in Los Alamos, so if the local library doesn't have these works, it shouldn't take much of a nudge from me to get the library to acquire them And there's always the LANL technical library.

I am particularly curious about doubly special relativity. Is it covered by any of these books?

Thanks again. Sorry about the thread hijack.

49. **Jimbo**
September 28, 2006

Kent,
DSR is largely covered by the work of Smolin & Magueijo; just do an arxiv search & I'm sure you can pull up a review article. I don't think its covered in any recent texts.

50. **Kea**
September 28, 2006

I feel there is a Nobel Prize in the WMAP evidence. Of course, the theory must match it accurately. Like Bee, I wonder who the best candidate is?

51. **A.J.**
September 28, 2006

Anon,

I agree that it's weird that Nambu & Goldstone should be made to wait till after the discovery of a Higgs boson. But I can't think of anything else the Nobel committee could be waiting for. Perhaps they prefer to give the prize out for less abstract achievements..?

52. **JoAnne**
September 29, 2006

Kobayashi and Maskawa, for their prediction of the 3rd generation (before it was observed) as a means of explaining the observation of CP violation.

53. **Shantanu**
September 29, 2006

One more possibility which no one has pointed out.
Irwin Shapiro, Pound and Rebka (for proposing & measuring
"shapiro delay" and gravitational redshift of photon which
confirmed GR.)

54. **A**
September 29, 2006

answering to Dorigo: we agree that microvertex is an important technology. But my point was different. An analogy with leptons might clarify it: somebody first detected that nu_tau exist, and somebody discovered their oscillations: who got the Nobel prize?

55. **Physiker**
September 29, 2006

Patrick,

Parisi is one of the fathers of the replica symmetry breaking idea which is still contentious. Edwards-Anderson proposed the replica trick for the treatment of disordered systems which is something more fundamental and more widely accepted (not without critique, though). Edwards' contribution to physics is broader. Together with de Gennes, he transformed into physics what used to be a kind of "stamp collecting" (the study of polymers, membranes, interfaces, etc.).

Someone mentioned granular matter. We still don't have any breakthough of Nobel caliber there, but Edwards' mark in the major developments in the area is clear.

56. **anon.**
September 29, 2006

David Cobden wrote:

"I don't think the prize has ever gone to a theory that was not already experimentally verified to a very high degree, such that it was essentially completely uncontroversial within the physics community. (Is asymptotic freedom an exception?)"

Asymptotic freedom is definitely not an exception. In fact there were good experimental reasons for wanting it to be true before it was theoretically discovered, and the experimental evidence has continually improved in the past 30 years. In fact, Bjorken had proposed a related property called "scaling," which Feynman interpreted in terms of "partons" (small pointlike constituents of hardons). This idea was confirmed experimentally by deep inelastic scattering experiments at SLAC, a few years before asymptotic freedom. These ideas pointed to flaws in the "dual resonance" (i.e. string theory) model of strong interactions. So when asymptotic freedom was found, it was accepted fairly quickly because there was already good experimental evidence.

57. **dir**
September 29, 2006

why not adler et al. for their work on the anomaly?

58. **Count Iblis**
September 29, 2006

A. Belavin, A. Polyakov and A. Zamolodchikov

59. **Peter Orland**
September 29, 2006

Adler and Jackiw won’t get the Nobel because anomalies are mainly used as a guideling for model building. By themselves, anomalies predict nothing. It does help solve the U(1) problem in QCD, but only says why the eta and eta-prime are heavy. There is no way to use the anomaly to compute their masses.

Belavin et. al. won’t get it. Most conformal field theories are testable by computer, not by nature, except a few which are fixed points of models which are solvable anyway.

Baxter and co. won’t get the prize because their work is too specialized to specific theoretical models. Special cases of the 8-vertex model (besides the Ising model) can be realized (I think helium adsorbed onto graphite is well described by the hard hexagon model). This work is not general enough yet.

Anyway, any speculation that the above could win is wishful thinking on the part of theorists.

People can’t get the prize for great mathematical ideas alone, even
if somewhat relevant experimentally. A prizeworthy idea has to solve a crisis in the field or make a stunning experimental prediction.

Perhaps Nambu and Goldstone probably won’t get it for chiral symmetry breaking because Feza Guersey, who is the third person responsible for the idea (he invented the sigma model) is no longer living.

Someone said that the dark energy discover is not yet well established because the type 1a supernova candles can’t be calculated precisely theoretically. This was true some years ago,
but I thought they were now better understood (a stellar astrophysicist would know better). There is also other data, found by completely different means which confirms dark energy, as I understand it. Perhaps it won’t get the Nobel this year, but this work seems prizeworthy.

60. **Haelfix**
September 30, 2006

Another shout for the COBE experimentalist teams, they’re shoe ins for a noble prize at some point (though it might have to wait till Planck).

Vera also will get it eventually, but still too early IMO.

It would be a scandal if Guth and Linde got a prize for inflation, as its an idea and not a specific model (and the original model was falsified). One day in the year 2300 they very well might still have their names on it, but I very much doubt they’ll ever have a Nobel in their lifetime. I can think of several people more deserving for ‘accepted theories but not a specific model’ off the top of my head, including Adler/Jackiw and Hawking/Penrose.

Kobayashi and Maskawa should have received a Nobel a long time ago, but as usual there is some confusion there, so they’re perenial contenders.

I suspect the Nobel will be outside Astrophysics and Particle physics this year again so that ends my 2cents.

61. **A**
September 30, 2006

actually, only very recent B-physics experiments could test if the single Kobayashi Maskawa phase accounts for more than one CP-violating process.
As the result was yes (some anomalies present in past years data largely disappeared), as these experiments are almost completed, as more experimental tests would need many more years, I dare telling omedetoo gozaimasu for the Nobel prize that Kobayashi-Maskawa will get within 3 years.

62. **Ben oit**
October 2, 2006

Anatole Abragam and Richard Geller for their pioneering works on Nuclear Magnetic Resonance and Electron Cyclotron Resonance and their application to Medecine.

Too much criticisms were made against Physics. It has become crucial that people also see that fundamental works also save lives every day !

63. **Paul**
October 2, 2006

I heard some rumor that the prize will be awarded for research in biophysicists; in particular the names of Carlos Bustamante from Berkley (he is apparently a pioneer in single-molecule visualization) and Hermann Gaub from Ludwig Maximilians University of Munich have been mentioned.

64. **biophysicist**
October 2, 2006

Well if Paul is right I (as my name uhh no doubt indicates) would be pretty ecstatic. It’s probably still a little early for biophysics to be awarded physics Nobels, but that day will come (and lest we forget that biophysicists have won biology Nobels and even the Dirac medal already). Also Dr. Bustamante would be totally deserving – his work is inspirational.

65. **Thomas Larsson**
October 3, 2006

My wife thinks that the medicine prize is cool. One of her former students is a postdoc in Fire’s group.

66. **Mark Callaghan**
October 3, 2006

Hi folks – this just in- The Nobel prize for physics goes to Snoot and Mather for their work on the cosmic microwave background

67. **MathPhys**
October 3, 2006

Years ago, I tried to read Smoot’s book “Wrinkles in Time” but found it too boring. Maybe I should try again.

68. **Christine**
October 3, 2006

[Nobel prize laureates 2006]

John C. Mather

and

George Smoot

Congratulations!

Christine

69. **TTT**
October 3, 2006

I cannot believe it. Even schoolkids know that the Russians disovered anisotropy several months earlier using their “Relict” satellite.

So here goes another BS Nobel. What a waste!!:-)

70. **Checkmate**
October 3, 2006

TTT,

Please provide references for your assertions!

71. **Who**
October 3, 2006

COBE is the single most widereaching bunch of results since a long ways back—it is superNobel class.

that oval map of the CMB mottled blue and red for temperature was more ikonic of discovery than the first images of earth from space

it has become the face of the universe

when Smoot and the other COBE team presented the perfect fit of the all-sky spectrum to a perfect Planck blackbody for the first time to an international body of astronomers—-when they showed that slide—there was a standing ovation.

when you have a slide of the blackbody curve with your data superimposed, and it gets the whole IAU to give a standing ovation, it is something.

I only mention this because some misguided person said “BS nobel”.
On the contrary giving Nobel for mapping CMB validates Nobel and is overdue.
==============

simply detecting anisotropy was done using U2 airplane years before COBE, we are not talking about “discovering anisotropy”

please correct me if I have misremembered some facts here, am rushed and dont have time to check

72. **LDM**
October 3, 2006

Who,

Good post.

73. **TTT**
October 3, 2006

I’m retracting my criticism.
Was confused by:

http://hea.iki.rssi.ru/pazh/

Astronomy Letters,
Volume 18, 1992, Issue 5
Anisotropy of the microwave background radiation
I.A.Strukov, A.A.Bryukhanov, D.P.Skulachev, and M.V.Sazhin (pp.387-395)

74. **Relativist**
October 3, 2006

The interesting thing is that they excluded the theorists who first predicted the broad nature of the anisotropy patterns – R K Sachs and A M Wolfe, Ap J (1967).

75. **Relativist**
October 3, 2006

p.s. – Sachs and Wolfe did not predict the accoustic peaks in the power spectrum, but then COBE did not observe those peaks (its resolutin was too low). The peaks were confirmed by later observations (BOOMERANG, WMAP in particular), which will surely now be in the line for a Nobel prize. And again the theorists who predicted those peaks before they were observed will probably get left out. But that was a triumph of cosmological theoretical prediction, confirmed by extraordinary delicate observations.

This Week's Hype

September 28, 2006
Categories: Uncategorized

Fewer and fewer science writers these days are credulous enough to keep promoting string theory, but there still are some around willing to keep writing overhyped stories about how theorists have finally found a way to get some sort of prediction of something observable out of string theory. One of these is Tom Siegfried, who has a new article in Science magazine entitled A Cosmic-Scale Test for String Theory? which reports that "some string theorists now believe they've found a way to make superstrings observable."

Siegfried reports for Science from PASCOS 2006, where he finds two results worth writing articles about. One of these is the recent preliminary neutrino oscillation results from MINOS, which certainly are worth reporting, but the second is the cosmic superstring hype that has been around for nearly three years now, and which I've commented on in various places, including here and here. The hype surrounding this topic first got seriously going with a press release from UCSB more than two years ago, in which Polchinski claimed that cosmic superstrings were "potentially visible over the next year or two" at LIGO. Now that this time period is up, the hype has to be modified, and Siegfried informs us that:

LIGO may not be sensitive enough to detect them, but a planned set of three space-based gravitational wave detectors known as LISA would be a good bet.

As is always the case with string theory, there aren't any real predictions here. The hype is based on the fact that, among the nearly infinitely complicated string theory models people have studied, it is in principle possible to come up with ones in which superstrings created in the early universe would expand to a very large "cosmic" scale and thus be observable. They would show up in various astronomical observations, but no one has yet seen the slightest evidence of such a thing. One can claim that it is logically possible that such things exist, with exactly the right properties to have escaped observation so far, but to be visible to the LISA experiment if it really does manage to get funded and operate sometime in the next decade. While this is logically possible, saying that "it would be a good bet" is pretty absurd; I doubt that any physicist would be willing to put money on this unless given very high odds.

The hype surrounding cosmic superstrings tends to completely confuse the kind of cosmic strings that occur as defects in the Higgs field in some GUT models (which have been studied for about 30 years now) with the kind that are supposed to come from elementary strings. Siegfried's article includes a graphic purporting to show a "network of enormous 'superstrings'". As far as I can tell, this is nonsense, since the same graphic occurs here, in an article from 2000, long before the "cosmic superstrings", where it is described as showing "cosmic strings form from a random initial distribution of phases of a hypothetical field called a Higgs field."

Oh, and the fact that I think this is a pretty sad example of bad science reporting by someone completely taken in by the string theory hype machine has nothing to do with the fact that its author recently wrote an extremely hostile, unfair and inaccurate review of my book…

Update: For an example of the kind of misinformation spread by stories like this, see this blog entry by another science journalist, over at Seed's ScienceBlogs.

Comments

1. **D R Lunsford**
   September 28, 2006

   This byzantine logic should cast doubt on all cosmological speculation.

   -drl

2. **Timothy Clemans**
   September 28, 2006

   Every review for NEW on amazon.com is gone, gone and amazon.com also says thar it has not been released yet.

3. **woit**
   September 28, 2006

   Timothy,

   Looks like some sort of mix-up at Amazon, they never did get the publication date of the book right. I've asked the people at Basic Books to look into it.

4. **anthropologist**
   September 29, 2006

   I went to see Smolin at his book presentation in Princeton just this week. I think since the guy is taking on the string theory, his salesman skills ought to be better. His presentation definitely lacked the passion that would ignite the masses. Sometimes he was getting carried away and talked too much about specifics, I do not think most of the audience was up for that. Even worse, he started to make excuses on the subject why he discussed the sociological factors in the book at all in an unconvincing sort of way.

   I think that you guys (critics) must concentrate your efforts on making your message well defined, coherent, and easy to understand. Then just keep hammering it. OK, you are not gonna win the admiration of the string crowd, that is clear, but what about just giving great presentations? Sell your criticism, you've chosen to do this, so stick to it. That is why some reporters may not understand it, because you are not impressing them enough. Give them drama, they will chose to promote it for their own selfish reasons.

5. **Seth**
   September 29, 2006

   To anthropologist,

   I think it's a real shame if it turns out that scientific truth has to be debated in sound bytes rather than reasoned arguments.

6. **Stefan**
   September 29, 2006

   Peter,

While I fully support (and applaud) all your committed effort(s) to keep theoretical physics from spiraling into the void of (unverifiable) speculation and flights of imagination, I think inevitably the only realistically *result-driven* way to attract individuals to your cause is to provide a reasonable alternative (or alternatives) to the current paradigm [i.e. string theory]. If you are harping – irrespective of what you criticise against – about how all of string theory has been unable to predict any new physics without providing a meaningful alternative to the prevailing circumstance(s) your voice will not resonate as much as it would otherwise; yes, it's true there are valid and substantive reasons for not believing in string theory "hype", but show us an alternative: until then, you are only trying to tell people to do 'nothing' instead of 'something' – and perhaps that may not go down well with generally motivated young people in hep…

[To further remark, it is to my knowledge (YPP Survey) that most people who join HEP research do so for intellectual satisfaction; therefore, to tell them to remain idle – instead of working on what is perceived as one of the best approaches currently available for unification – is probably not going to go down well… The only solution is to provide a viable alternative and demostrate verifiable results… 'string theory' may not do that as yet (or perhaps it may never be able to do so) but as Urs Schreiber writes in his review of NEW, it has many fascinating properties that *may* be of significant value in the future… none of us can really tell as yet…]

Stefan

7. **Tom Siegfried fan**
September 29, 2006

Peter, how can you know for certain that strings haven't already been discovered, and merely have been misunderstood?

Atoms were long around before people knew for certain they existed.

For example, maybe widely-observed 'UFOs' are actually the ends of cosmic strings, flying about in the Earth's upper atmosphere?

Extra-dimensions have evidence long broadcast on TV (see the Twilight Zone and other programmes). Maybe ghosts and psychic phenomena, widely reported, are the really solid evidence for string theory. Nobel Laureate Professor Josephson has long said so, and has a paper on arXiv: http://arxiv.org/abs/physics/0312012

8. **Geon**
September 29, 2006

Stefan, I agree 100% with you.

9. **Stefan**
September 29, 2006

To add, I am greatly encouraged by your research-topic. I have perused Amazon[.com] for relevant titles. I am quite interested in RT-QFT connections.

Where should I start? Give us [non-experts] clear guidance…

Post-Script:

As per your suggestions I have included the various titles you recommended to my 'To Purchase Now' Wish List. If you want to include other titles please feel free to e-mail me.

Regards,
Stefan

10. **TTT**
September 29, 2006

Guys, this blog did an excellent job demoting string theory, so that whenever I hear the word 'string" I wanna scream, it makes me wanna p**k. The problem with all this is that having reached this critical disgust mass I begin to p**k every time you push your bashing further. I think it is important to stop from time to time and direct your energy (blog's pages, that is) to something really really different and interesting. We have already heard everything we need to know about how ****ed up the string theory is, so, please, for a change, just to keep your fans sane, do something different.

It is just the nature of human psychology at work here. Now, I'm a little bit nurvous everytime I open your page: I'm simply afraid to find another dragging about the string theory, about someone reporting something from some string conference, etc etc, and it doesn't matter anymore whether it turns out negative or positive.

How about a month without the word "string"?? would that be too hard?

sorry but you begin to seem like a cult of its own, and that's not good. This is just a friendly remark:-)

Something different! Pleeeease?:-)

11. **Peter Woit**
September 29, 2006

anthropologist and TTT,

Let's see, one of you wants me to simplify the message and keep hammering on it, the other one thinks I've gone too far and that I should stop mentioning string theory…. I'm tempted to conclude that maybe I've got it about right. The posts of the last couple weeks actually have had relatively little string bashing, although there has been a lot of string related stuff because I'm mentioning the reviews of the book (that topic should die down soon). It seems to me that the amount of string hype appearing in serious publications has definitely decreased, but as long as it's still appearing in places like Science, I intend to keep commenting on it.

Stefan and Geon,

Yes, of course it would be much better if I had an alternative TOE that made testable predictions that I could explain to everyone here, thus giving them something obvious to work on. Unfortunately I don't have that. If I did I probably wouldn't be spending time writing this blog, but instead would be enjoying the high life of fame, fortune and groupies that attends Nobel-prize winning theoretical physicists. I really think people who want to work seriously on particle theory at this point should just get over trying to find someone to tell them what to do, string theorist or anti-string theorist, and just try and come up with their own ideas. If you don't want to do this, but want to join a promising, healthy research program where you can make useful contributions to science by following someone else, you probably shouldn't be trying to work in this particular field at this particular time.

12. **Stefan**
September 29, 2006

Peter,

Thank you for your feedback, but you did not give me any titles for RT-QFT connections. Are there any (yet)?

13. **David Tong**
September 29, 2006

Peter,

I think your criticism is severely misplaced.

Firstly, you're right that it's important to draw a distinction between cosmic strings and cosmic superstrings. It's a shame the author didn't make more of an effort in this regard. It's kind of a shame that you didn't either because at times I can't quite work out what you're complaining about. In particular, the line

>

seems to apply to cosmic strings of all types. It also seems to miss the main point that cosmic strings have a strong and distinctive gravitational wave spectrum. LISA, should it fly, will give us a new window on the universe. At the very least, it will bring down the bounds on the possible existence of cosmic strings by many orders of magnitude. Spergel and other prominent cosmologists find this exciting. Maybe you disagree, but I think it's right that this work is hyped in the popular press, especially given the current situation at NASA and the fact that LISA is one of the most important science projects in the pipeline.

As for string theory, the excitement of cosmic superstrings comes partly because they are a generic prediction of large classes of string models. Not seeing cosmic strings will therefore rule out large classes of string models. (You should be happy about this although no doubt you will compain that it will not rule out all string models). But mostly the excitement comes because the properties of cosmic superstrings differ from the properties of gauge theory strings.

Now one can certainly have a discussion about whether these differences are potentially observable (by LIGO, by LISA, depending on string tensions, etc). One can also discuss whether it's possible to cook up gauge theories to mimic the behavior of cosmic superstrings. If you wanted to make any kind of constructive criticism of this work, there are plenty of opportunities to do so in the usual scientific fashion — by long calculations and hard work. Instead you prefer to simply bash out another skewed polemic from the sidelines, glossing over the key issues just as glibly as the science journalist you're complaining about.

14. **TheGraduate**
September 29, 2006

I think probably nobody is more qualified to figure out what is good for the site than Peter. I say this because he has access to the number of visitors per day and we don't. Don't forget the lesson of the string theory debacle. We must be data driven!

It can be hard to stand in front of a bunch of people and criticize others! And I have more respect for people that don't come to this kind of thing easily.

15. **David Tong**
September 29, 2006

I'm not sure what happened with your comments section, but the line I quoted didn't appear: it was

"One can claim that it is logically possible that such things exist, with exactly the right properties to have escaped observation so far, but to be visible to the LISA experiment"

16. **Peter Woit**
September 29, 2006

David,

Sorry if I wasn't clear enough, but I thought that it should have been clear that what I was criticizing were the claims being made about fundamental superstrings, not the traditional kinds of cosmic strings that occur in gauge theories coupled to Higgs fields.

I have no trouble with Spergel or anyone else promoting LISA or other experiments for their ability to set better lower bounds on cosmic strings, although I disagree with you about the desirability of scientists who want to keep their credibility hyping things to the public to get them to be willing to finance experiments. What I seriously have a problem with is what this writer was doing, based on the hype he was being fed by string theorists, which is to promote the idea that LISA is going to "test string theory". You're engaging in exactly the same kind of overhype here, with your claim that "not seeing cosmic strings will rule out large classes of string models". By this argument, since one can find a string theory that predicts just about anything, any particle physics experiment that measures something new is doing this. The Tevatron has "ruled out large classes of string models" and the LHC will rule out more. This isn't why these machines are important or should be financed.

I think the superstring models being used here are convoluted, ugly, and there's not the slightest evidence they have anything to do with reality. Obviously I thus don't think I or anyone else should be wasting their time on them. People who think otherwise are welcome to do so, they should just stop dishonestly hyping this work to overly credulous reporters, as well as complaining when someone points out the dishonesty.

17. **Tony Smith**
September 29, 2006

Peter, you said that if you "… had an alternative TOE that made testable predictions that [you] could explain to everyone here … [you] probably wouldn't be spending time writing this blog, but instead would be enjoying the high life of fame, fortune and groupies that attends Nobel-prize winning theoretical physicists. …".

Or,
you might find that:
1 – you are labelled a "crackpot" by Harvard, represented by a distinguished Harvard professor;
2 – you are blacklisted from posting your "alternative TOE" on the arXiv; and
3 – your offers to "explain" your "alternative TOE" at seminars etc are ignored by the physics establishment.

Oh, wait … 1 has already happened to you even without an "alternative TOE".

Maybe your reference to "fame, fortune and groupies" is more telling than you consciously intended, as it is consistent with the real objective of the present-day theoretical high-energy physics community being, NOT a "TOE …[with]…testable predictions",
but in fact
winning the game of pursuit of grants, funding, jobs, and bureaucratic empire.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

18. **Peter Woit**
September 29, 2006

TheGraduate,

Actually I don't think that the amount of traffic here is a good measure of whether what I'm trying to do is successful. I'm not trying to reach the widest possible audience. One goal is to expose string theory hype, thus reducing the amount of it.. Over the last couple years the amount of this has definitely gone down, for whatever reason, and I'm glad to see that. I also look forward to a near future in which there is so little of such hype in the serious scientific press that it's not a problem that needs to be paid attention to.

19. **Peter Woit**
September 29, 2006

Stefan,

The connection between representation theory and QFT has only really been developed in the 1+1 d case. Most relevant here are WZW models. An example of where this is explained is

Fuchs, Affine Lie algebras and quantum groups

There's also a lot of relevant material in the books by Mickelsson, and Pressley and Segal that I've recommended. Another place with a lot of material about this kind of thing is the Goddard-Olive reprint volume on Kac-Moody and Virasoro algebras.

20. **hack**
September 29, 2006

Uh oh, you've been Slashdotted.

21. **TheGraduate**
September 29, 2006

hack,

Is this the link you mean?

http://science.slashdot.org/science/06/09/29/1735237.shtml

22. **SD**
September 29, 2006

Dr. Shellard was my advisor a long time ago, and I'm pretty sure that picture is just "regular" cosmic strings. 😉

"The hype is based on the fact that, among the nearly infinitely complicated string theory models people have studied, it is in principle possible to come up with ones in which superstrings created in the early universe would expand to a very large "cosmic" scale and thus be observable. They would show up in various astronomical observations, but no one has yet seen the slightest evidence of such a thing."

Try not to get too upset by hype. I think workers in the field, but outside the string theory cathedral, know that it's interesting to see a prediction made, but not to take it too seriously. Indeed, if the bet were too good the observation would be boring!

It's a bit unfair to yell at people for hype, and then engage in hyping the other side as well. That "no one has yet seen evidence" (the slightest evdience, even!) is really not a way to make a substantive remark.

23. **Thomas Love**
September 29, 2006

I was disappointed that neither Peter nor Lee referenced:

Constructing Quarks: A Sociological History of Particle Physics by Andrew Pickering

The same sort of thing is going on now.

"He who does not know the past is condemned to repeat it"
—George Santayana

And physicists are notorious for their ignorance of history, prefering to pass on historical myths to their students.

24. **woit**
September 29, 2006

SD,

That no one has seen the slightest evidence for cosmic superstrings is not "hype", but an accurate, substantive statement. If there were some very slight evidence for such things, one might think it was a good bet that more sensitive observations would produce conclusive evidence. But there isn't.

25. **Yatima**
September 30, 2006

> "Constructing Quarks: A Sociological History of Particle Physics by Andrew Pickering"

Reference:

http://www.amazon.com/gp/reader/0226667995/ref=sib_dp_pt/002-4545818-0805654#reader-link

1984…a venerable age. Good book? I remember once reading a review of a book with
a similar title in which it was claimed that the book under review was
'post-modernist'/'relativist' in the sense that it denied the objective existence of
quarks at all.

The Economist's latest edition cautiously moves the spotlight to LQG:

http://www.economist.com/science/displayStory.cfm?story_id=7963608

26. **Arun**
September 30, 2006

NPR, Weekend Edition Saturday had a about two minute mention of string theory, Not Even Wrong (and Smolin's book) – it was at about 8:30 on WNYC.

27. **TheGraduate**
September 30, 2006

Peter,

I think that the public is an important part of ending the hype becasue the opinion of the public matters to:

1. those who ultimately fund physics

2. those who run universities

3. those who print magazines and newspapers and air tv shows

I think a lot of activities surrounding the hype are prestige seeking activities and therefore, a lower opinion of string theory in the public sphere will probably diminish the returns of such prestige seeking.

The String Vacuum Project

September 30, 2006
Categories: Uncategorized

Last week at the KITP, Keith Dienes gave a talk on A Statistical Study of the Heterotic Landscape. He gave a good idea of the state of the art of the investigation of the Landscape, focusing on one special type of models, heterotic models. The results he presented gave statistical distributions for just two very crude aspects of these compactifications, their gauge groups and cosmological constants. These models remain highly unrealistic, since the cosmological constants are of order the Planck scale and the compactifications are not stable.

The models studied have gauge groups of rank 22, and while many of them contain the standard model SU(3)xSU(2)xU(1), they also contain many more gauge group factors, with typically not one, but about seven SU(2) factors. These models, with their instabilities, far too large gauge groups and cosmological constants, are extremely far from anything like the standard model. It's not at all clear what the point is in enumerating them and studying their statistics, but Dienes describes in detail various problems that arise with the whole concept of generating "random" models of this kind and trying to get sensible statistical distributions. He also looks for correlations between gauge groups and cosmological constants, finding that at small cosmological constant one is somewhat more likely to get many factors in the gauge group (although in his case, both the gauge group and the cosmological constant are very different than in the real world).

Despite the very crude state of these calculations, Dienes reports that a group of 17 prominent string theorists have banded together to form the "String Vacuum Project", with the goal over the next few years of accumulating a database of 10s of billions of string models, with the hope of finding within this mountain of data about 100 models that have crude features of the standard model. I don't at all see what the point of this is, but it certainly is a computationally intensive project that could keep many people occupied for a long time. It also appears to be just the beginning, with the longer term goal being to devote the next decades to expanding from 10s of billions farther into the 10^500 or whatever exorbitantly large number is thought to be the number of all string models.

The String Vacuum Project submitted a proposal to the NSF last year, which seems to have been turned down, and they appear to be planning to resubmit the proposal. They have a Wiki, with all sorts of details about the project. Most recent additions to the Wiki are from Bert Schellekens in August, who discusses a proposed "String Vacuum Markup Language" (SVML) format, with links to a web-page that produces data in this format for certain sorts of models. There's also a European String Vacuum Project web-site.

Comments

1. **anon**
   September 30, 2006

   This is tragic and hilarious news. Thanks God I work on quantum computing. We do real experiments

2. **MathPhys**
   September 30, 2006

   This is sad.

3. **Benni**
   September 30, 2006

   I see, Dieter Luest from munich is associated with this "String Vacuum Project" http://strings0.rutgers.edu:8000/MemberGroup
   (even Lisa Randall is, why that? I thought of her not as a String Phaenomenologist).
   In Munich theres now a very big string group. Compared to the size of the theoretical phenomenologist groups, a very big group:
   http://www.theorie.physik.uni-muenchen.de/~luest/stringgroup.html
   Luest was one of the first who pointed out that string theory has many many solutions.
   Now, it seems that they led german students into the business of vacuum counting. At exactly the time, when LHC will come up with new data…..

4. **LDM**
   September 30, 2006

   What a boondoggle. I feel like writing my congressman.

5. **astro**
   September 30, 2006

   And I thought the people who play the Lotto were numerically challenged …

6. **Garrett**
   September 30, 2006

   Peter, sometimes your job is too easy.

7. **Tony Smith**
   September 30, 2006

   The SVP wiki page at http://strings0.rutgers.edu:8000/ProjectOverview says in part:
   "… Our primary goal is to construct string compactifications which lead to the Standard Model …
   A related goal is a broader study of standard-like models, by statistical and other approaches, …
   and
   … to be able to estimate the number of SM's which should exist among all known types of constructions …".

   So, if the SVP succeeds, it will not only be the authoritative catalog of superstring models, but will also be THE authoritative catalog of ALL physics models.

   Further, the SVP wiki page at http://strings0.rutgers.edu:8000/ProjectOverview/DiscussModelFormat says in part:
   "… Suppose one of us, or some independent group of string theorists, establishes the existence of a string compactification leading to a concrete low energy model, and wants to submit it to the SVP database.
   What should he/she do?
   … the first thing to do is to write a standard research paper and submit it to the arXiv …

… Having done this,
the key thing to do is to provide, or be able to provide, the information about the model in a standard format, which we might refer to as "String Vacuum Markup Language" or SVML . …".

Suppose somebody, say, Peter working on representation theory, comes up with a model that is consistent with the Standard Model plus Gravity,
and
suppose Peter meets the first requirement of posting his model on the arXiv.
Then,
if the SVP has obtained dominance roughly equivalent to the physics-community dominance of the arXiv,
Peter MUST fit his model into the SVP strait-jacket of "String Vacuum Markup Language" if he wants his model to be considered at all by the physics establishment.

If Peter's model were to be so unconventional (from a superstring point of view) that it woudl not fit happily into the "String Vacuum Markup Language" strait-jacket, then no matter how realistic and useful it might be, it would never be accepted or used.

Since the SVP proposal at http://www.physics.rutgers.edu/~mrd/SVP-v2.ps says in part
"… A group of theorists in Europe … have come to similar conclusions, and are writing a similar proposal to European agencies. We would join with them to strengthen the entire activity further. …",
it is clear that the objective of SVP is not merely dominance of the USA theoretical high-energy physics community,
but is to achieve global domination (as is now in fact the case with the arXiv).

Therefore,
I think that the SVP proposal is quite dangerous to the future of physics,
because
it seems to me that gate-keeping by the SVP powers-that-be, using such things as requiring "String Vacuum Markup Language", will be so restrictive as to make the blacklisting-by-moderators practice of the arXiv seem pale by comparison.

Tony Smith
http://www.valdostamusuem.org/hamsmith/

PS – My opinion in this comment is substantially objective, since the SVP proposal will probably not have much impact on acceptance/rejection of my work because blacklisting by the arXiv has already done the damage to me and my work that might be done by SVP.

8. **Gina**
September 30, 2006

This is a very nice project for the following reasons:

1) It can help to demonstrate that string theory can provide "a theory of everything" even if not "the theory of everything". This will be a great intellectual achievement. (As far as I understand from reading Peter's book, there is no guarantee that even a single one out of the 10^500 different string theories (or more, infinitely many??) will have the desired properties.)

2) It can go both ways. It can also lead to (mild but important) negative conclusions about string theory. E.g. if certain unrealistic or problematic features cannot be avoided in all the proposed models.

3) It does not have the sex-appeal/snob-appeal of hyper/super 23-century mathematics but is rather a down-to-earth programming-intensive project.

4) Projects of this kind are very demanding and often lead to difficult and thankless work. They are also rather risky.

So based on what Peter wrote I would recommend the NSF to support this project very very strongly.

9. **Jean-Paul**
September 30, 2006

Thanks to referees and panelists who were able to protect NSF from wasting money on ridiculous String Vacuum Project and String Vacuum Markup Language. You don't have to be a physicist to recognize that it is complete nonsense. Actually, I don't believe that theorists who are proposing this activity are acting in good faith — they must know that the only goal would be to keep the ball rolling… The logic is: let's get postdocs, whatever it takes… but then, would they really hire a programming expert? No, they would try to hire somebody working on more serious stuff listed on "daily specials"="menu du jour". They must be completely burned out and desperate or plain delusional like the person who proposed SVML.

10. **Carl**
September 30, 2006

That was an amazing wiki. Absolutely no comprehension of the magnitude of the problem. Just trying to figure out what sorts of biases are present in the data would be insane.

11. **Farrold**
October 1, 2006

If I may be excused for asking a question about the broader issue of vast size of the Landscape:

What is the standard response to the argument that any theory that does *not* predict a vast number of possible vacuua, but instead picks out a unique set of physical laws, would leave us with the problem of explaining the apparent fine-tuning of the observed set of laws to be consistent with life? From this perspective, the Landscape would seem to be an asset, rather than a liability.

12. **a**
October 1, 2006

to Farrold: about 10^10 vacua would be enough to interpret the apparent fine-tuning you mention: with intensive computer work one could find the true one. However, if the cosmological constant is small only due to a fine-tuning (that makes it practically uncomputable), about 10^100 vacua are needed, and this already seems a hopeless situation. If you have 10^20 buckets you can empty the ocean, but with 10^500 vacua what can you do? If you find 10^400 realistic vacua, where do you put them?

Despite this, computer tools that manipulate generic Lagrangians are being developed to study LHC data: if after LHC there will be nothing better to do, why not using them to explore a bunch of vacua?

13. **Peter Orland**
October 1, 2006

Gina Wrote:

1) It can help to demonstrate that string theory can provide "a theory of everything" even if not "the theory of everything". This will be a great intellectual achievement. (As far as I understand from reading Peter's book, there is no guarantee that even a single one out of the 10^500 different string theories (or more, infinitely many??) will have the desired properties.)

Gina:

It isn't clear to me that finding some string vacuum among many which describes nature would be a great intellectual acheivement. There is no prediction in such an endevour unless EXACTLY ONE vacuum of string theory describes nature at experimentally accessible energies. If two are found, no prediction can be made. Even if exactly one realistic vacuum is found, there may be no testable prediction. I suspect that people think that they can find a distribution of vacua whose predictions have a peak around observable values. This probably won't happen and isn't a proper
application of probablilty anyway.

Such a program MAY be of value. But I would go along with the
NSF on this one. There is a big risk for them that nothing substantive will result. Furthermore, since string theory hasn't been properly formulated yet, all these vacua could disappear in the future, which would really embarass whoever funds the project. If people really believe in the project, they may have to commit their
own time to it, without external funding. The NSF doesn't have much money these days, and they have to be careful how they spend it.

I suspect that in the end, this project will be funded by sheer lobbying, and some good competing proposals will lose out.

14. **A Babe in the Universe**
October 1, 2006

Einstein would say that the definition of insanity is doing the same thing 10^500 times expecting a different result.

15. **Who**
October 1, 2006

From Farrold: **What is the standard response to the argument that any theory that does not predict a vast number of possible vacuua, but instead picks out a unique set of physical laws, would leave us with the problem of explaining the apparent fine-tuning of the observed set of laws to be consistent with life?**

I think you mean the observed set of CONSTANTS which occur in the laws, and not the overall format of physical law. It is the set of dimensionless constants that people sometimes think presents us with a problem of fine tuning.

the distinction between the numerical parameters and the laws in which the occur is fuzzy but nevertheless useful. explaining the format of physical law would be a deeper puzzle than just trying to say why the CC is such a small positive number or why alpha is around 1/137.

However I think you are mistaken. A theory which picks out a particular set of fundamental constants would NOT present us with a fine-tuning problem. It would explain why the constants are what they are—perhaps all derivable from the value of some deeper numerical relationship. They would not be "tuned"—they would simply have to be what they have to be.

Life, an accident, would be constrained to be whatever can work within the context of those necessary predetermined constants. It's abundance or rarity would not explain anything about the constants because they would be explained by the theory which you have imagined.

A theory already exists which makes testable predictions and which explains observed values of fundamental dimensionless constants without making reference to life.

See for example pages 167-168 of Smolin's new book **The Trouble with Physics**
http://www.amazon.com/gp/bestsellers/books/14560/ref=pd_ts_b_nav/102-4540543-7840144

16. **Jean-Paul**
October 1, 2006

Babe in the Universe — calling 17 prominent string theorists insane is not justified (except for the SVML author). It would be a compliment — after all, Galileo, Copernicus et al were also considered insane. The problem with SVP is that it is proposed by a group of hypocrits and opportunists who want to suck up money that would normally go to serious hep-th research and spend it on 1) self-promotion of their old (wrong, 100% excluded) models by packaging them in a useless catalogue 2) taking control of the postdoc market.

17. **outside_observer**
October 1, 2006

This is insane, its not science at all

18. **dan**
October 1, 2006

Dear string theorists,

Do you seriously want NSF grant and research money being spent in this way?

Curious
dan

19. **Arun**
October 1, 2006

http://en.wikipedia.org/wiki/The_Nine_Billion_Names_of_God

20. **TheGraduate**
October 1, 2006

I don't know if it's valid physics but it seems like a pretty interesting computer science project.

21. **D R Lunsford**
October 1, 2006

    ABITU – that comment was priceless :))) -r

22. **Farrold**
October 1, 2006

    Who remarked:

    > A theory which picks out a particular set of fundamental constants would NOT present us with a fine-tuning problem. It would explain why the constants are what they are—perhaps all derivable from the value of some deeper numerical relationship. They would not be "tuned"—they would simply have to be what they have to be.

    I take your point. There would, however, still be a degree of mystery similar in kind (but not in magnitude!) to the mystery in Sagan's novel, *Contact*, that is, the encoding a large, pixel-based image of a circle deep (yet far too early) in the digits of pi.

    > A theory already exists which makes testable predictions and which explains observed values of fundamental dimensionless constants without making reference to life. [Smolin 2006, *The Trouble with Physics*]

    I assume that you mean it explains how one *could* derive observable values, rather than that it explains (by deriving) *the* observed values.

    In his 1997 book, *The Life of the Cosmos*, Smolin suggested that universes spawn new universes with different physical constants, and he proposed this as an answer to the fine-tuning problem. Does he now argue that the form of the laws, if understood deeply, would imply the constants?

    For example, one current idea (as I understand it) describes particles as braids in structures in loop quantum gravity, which in turn emerges from structures in a spin-foam model. If the spin foam model were as parameter-free as theorists would like, then the theory would derive all the physical constants from its form. I'd be happier with a theory that had several arbitrary (i.e., tunable) parameters that enabled (in principle) precise calculation of the many arbitrary parameters in the Standard Model.

23. **TheGraduate**
October 1, 2006

    The fine-tuned universe: http://www.xkcd.com/c10.html

24. **Farrold**
October 2, 2006

    (continuing)
    One would of course want a theory of this sort to explain how these N parameters appear through a spontaneous symmetry-breaking process which results in a multi-dimensional continuum (or fine-grained distribution) of possible sets of parameter values.

    In the multi-dimensional continuum case, the theory would be consistent with an actual infinity of possible vacua. Nonetheless, taking measurement of the Standard Model parameters as the experiment, the theory would be falsifiable: It would predict that the parameter values are constrained to an N-dimensional surface ($N << 29$) embedded in the 29-dimensional space of unconstrained parameter values. This surface either would or wouldn't intersect the error-box of our measurements.

    I agree with many of Peter's criticisms (and I usually root for the loop/foam team), but I think that a similar case can be made for a *possible* outcome of Landscape studies. One scenario would be **(a)** that the Landscape includes a set of vacua with 3+1 macro-scale dimensions and with physics that can be approximated by parameterizations of the Standard Model, and **(b)** that in every member of this set, the 29 parameter values fall on an N-dimensional surface ($N << 29$) embedded in the 29-dimensional space…and so on, as above.

25. **gina**
October 3, 2006

    Dear Peter O.

    Many thanks for your comment.
    I think that demonstrating that string theory can "in principle"
    give a theory (or 2 or 1,000,000) which is consistent with the standard model and can be regarded as a "theory of everything" will be a big step forward inspite of having no prediction power.

    (If a unique such a theory can offer some predictions it is quite possible that some predictions can be offered even if more than one found.)

    A nice feature of this proposal is that unlike most proposal in the area, the outcomes can go both ways as far as string theory is concerned. In some (weak) sense this project is an empirical tests for the ideas of string theory.

    "This probably won't happen and isn't a proper application of probablilty anyway."

    This is a very nice sentence. (We do get a lot of milage using probability in ways which are not entirely proper,…probably.)

    "But I would go along with the NSF on this one. There is a big risk for them that nothing substantive will result. Furthermore, since string theory hasn't been properly formulated yet, all these vacua could disappear in the future, which would really embarass whoever funds the project."

    I diasagree with you on this point. I think NSF (and also individual scientists) should take risks.

26. **copa**
October 3, 2006

    Are you sure it isn't called the
    VACUOUS STRING PROJECT?

27. **Lubos Motl**
October 3, 2006

Dear crackpot Peter, you are a damn asshole. I will sue you for the lies those crackpot commenters telling on me on your crackpot blog.
I hope you will die soon. The sooner the better.

So: be prepared to hear from my lawyer.

Best Lubos

28. **Shocked and Saddened**
October 3, 2006

This comment above by Lubos is beyond the pale. Here he wishes physical harm for Peter for hosting a mostly-open forum where sometimes some nasty comments are posted about Lubos (invariably in response to Lubos' own writings), while on the other hand his own writing has a take-no-prisoners style — his own blog posts are riddled with name calling and put-downs, and he makes no attempt to moderate nasty comments about Peter by others. From my perspective Lubos has lost a a certain connection with objective reality, and it appears to me that it is getting worse with time. He seems to be unaware of the great disconnect between his complaints and his own actions, for example complaining (in the past) about censorship on Peter's blog while practicing censorship to a much greater degree himsef (witness the on-going thread Peter started for those comments); he frequently rails against political correctness, yet people who do not behave in a politically correct way on *his* blog (i.e., according to *his* politics) typically meet with either censorship or intimidation tactics. There are other examples that I see as hypocritical behavior on his part, but no need to enumerate them.

I am no lawyer, but it seems obvious that Lubos has absolutely no chance of winning any kind of legal action against Peter. A quick read of his blog speaks volumes about how his verbally abusive tactics invite unsavory comments about him. Importantly, he has willingly made himself a public person, and his blogging style promotes the kinds of comments he deplores here. His reaction above to those kinds of comments look to me like yet another example of loss of objectivity. Presumably his lawyer will inform him that different standards for slander, etc. apply to public figures than others.

I hope Harvard University becomes aware of his comment. They need to know how a person who prominently puts their banner at the top of his blog is reflecting on them.

29. **Shocked and Saddened**
October 3, 2006

I should clarify one statement I made above, especially given Lubos' current litigous mood:
… he frequently rails against political correctness, yet people who do not behave in a politically correct way on his blog (i.e., according to his politics) typically meet with either censorship or intimidation tactics.
By "intimidation tactics" I am referring to ridicule, name calling, and other forms of put-downs that often await the hapless person who openly disagrees with him. I consider these to be intimidation tactics because their end result is often that the target of the verbal abuse feels intimidated and afraid to disagree in the future. In its worst form, the person may be afraid to take a public stand on a related issue in the future, even far away from Lubos' verbal reach, just because they are afraid of a repeat of the bad experience. This kind of intimidation-based suppression of free speech (in the sense above) is not healthy for science, nor for a democratic socieity in general.

30. **M-theory**
October 4, 2006

In his blog Lubos proposed to scan all 10^500 string models to show that string theory is predictive. In the comment section of his blog I answered: let's ask 10^500 monkeys to type 10^500 random field theories; both approaches will likely lead to something like 10^300 models (string models or monkey models) compatible with all data we have.

My comment was deleted.

I think Lubos understood the scientific value of the Monkey-theory Vacuum Project: in case DOE or Harvard are going to develop a cluster of monkeys my lawyer will start a legal action for plagiarism.

31. **Another TheGraduate**
October 4, 2006

I don't know if it's valid physics but it seems like a pretty interesting life-science project.

32. **Juan R.**
October 4, 2006

String theorists promised answers to current problems and misteria in fundamental physics. After many decades, they offered not a single consistent and good solution. Then now the hype is that whereas string theory is not physics it has been good for mathematics.

If this project start (i do not wait), that would we heard in next 40 years, that string vacuum project failed but was useful for developing new markup and computational capabilities?

Juan R.

Center for CANONICAL |SCIENCE)

33. **Chris W.**
October 4, 2006

The post mentioned above is here. It is an interesting and forthright exposition of how at least some string theorists view the question of falsifiability as applied to string theory and its offshoots. Consider the following quote:

> As Barton Zwiebach wrote, the prescription to decide about the validity of string theory is straightforward: simply list all possible vacua of string theory, calculate their properties with a sufficient precision, and compare them with reality. Either one of them will match the reality or not.
>
> The answer just can't be ambiguous just like the answer whether "2^32,582,657-1" is a prime integer cannot be ambiguous. And you bet it is a prime. The greatest known prime as of today. A naive critic of mathematics could say that the question whether the number is prime is not even wrong because one would have to test the potential divisors up to the square root of the number i.e. up to "2^16,000,000" or so. One would need "2^16,000,000" units of time. It is even more than the number of the flux vacua and it's not possible to check it. That's why, the critic would argue, it's not science, it's not even wrong, there are troubles with mathematics, blah blah blah. The reality is that one computer in GIMPS needed one month or so to verify that the number is indeed prime (and a faster computer rechecked it in a few days). There exist more sophisticated and faster methods to decide about the validity of a statement than the primitive critic of mathematics could imagine. The case of physics is analogous.

Ah, yes. Now, if we could just discover those more sophisticated and faster methods…

34. **Andy**
October 4, 2006

I keep getting "Cannot find server" error when I try to link to the SVP Wiki-page. Has it been removed or firewalled?

35. **dark-matter**
October 8, 2006

If you believe in the anthropic Landscape, then the next logical step is to statistically analyze 10^500 vacua. Indeed, analyzing 10^1000 would be better and would produce more scientifically convincing results. I am convinced it will spit out the nature of dark matter, calculating from the first principle of ST. However, it is somewhat risky to apply to NSF for funding for this vital project – it may cause the US Government debt to hit $15T. I suggest instead apply directly to CERN to divert all of its massive computing facilities to the String Vacuum project. After all, ST is far more important then LHC. The LHC can wait. I nominate Prof Motl as project lead to get the necessary funding from CERN, and scientific approval from NSF and his superior at Harvard. As the certified Clown of String, he is uniquely qualified. This is his chance for a Nobel! You know, after 25 years I still find ST so exciting ….

36. **Bee**
October 10, 2006

Hi Arun!

Thanks so much for the link to the short story! I read that story an eternity ago, forgot author and name, and have been trying to find it ever since.

Every once in a while, these comments are actually worth reading 😉

Best,

B.

37. **Bee**
October 10, 2006

About the svp: even though I think that the appearance of the st landscape is a serious indication that either a) we've misunderstood something about st or b) it's not the TOE, the project itself I find worth an investigation. I mean, even if it's not clear why THIS point in the landscape, wouldn't it be good to know it, so we can work with it? We also don't know why spacetime has Lorentzian signature, but still we can work with it. (Or if you know, tell me.)

What scares me however are the dimensions of the planned project, the potentially low level of possible knowledge gain, and what it means when I recall that we are living with finite resources, money, people. Questions to ask: Are there other, better, proposals that would suffer from a support of the above? Is the project likely to draw people away from other fields? Is it? I mean, look, those who support the svp have a research interest, so they write a proposal, whats wrong about that? Did they make any scientifically wrong claims about what knowledge gain it would yield? And, no, I wouldn't finance the project — of course I would support my own proposals…

B.

PS: regarding the above nasty comment allegedly written by Lubos, I seriously doubt he wrote it. I have seen very similar sounding comments appearing in his comment section, but signed with Peter Woit. I found myself thinking, no way Peter would write that. However, these comments disappeared within some minutes, so I guess Lubos just deletes them.

38. **M-theory**
October 10, 2006

Bee, maybe you should ask Lubos. I suggested him to state that he did not wrote that, but my comment was deleted with no answer.

39. **Bee**
October 10, 2006

Hi M, to be frank, I don't really care. I don't want to spend my time trying to psychoanalyze Peter's and Lubos' virtual realities. I found myself thinking recently it would be really interesting to lock the both of them together in a room for 48 hours, with a live webcam. –B.

And now that things are changing for the worse,
See, its a crazy world we're living in
And I just can't see that half of us immersed in sin
Is all we have to give these –

Futures made of virtual insanity now
Always seem to, be govern'd by this love we have
For useless, twisting, our new technology
Oh, now there is no sound – for we all live underground
~ Jamiroquai

40. **Who**
October 10, 2006

hi Bee, according to my understanding both the two persons you mention are nice and polite in reality, but little motivated to converse with each other—-so the locked room experiment would not be what you call "very interesting", but would, on the contrary, turn out to be quiet, and uneventful.

It is true that Lubos has a "Wildman" personality on the web. But I suppose that this is merely his web persona. Maybe you know him in real world, and can contradict me

41. **Arun**
October 10, 2006

Bee, a new version of Schrodinger's Cat?

42. **Bee**

October 10, 2006

Who, that’s what I had in mind 🙂 You think I would want to put them in a room together on the danger that only one of them comes out again? Hey, I am a nice girl! No, I think, there’s too much energy loss in friction here that could be better used. Since neither Peter nor Lubos are stupid they probably have noticed that as well.

Falsifying String Theory: Not

October 1, 2006
Categories: Uncategorized

Back in April a paper appeared on the arXiv from string theorist Jacques Distler and collaborators that made a rather outrageously overhyped claim to have found a way to "falsify string theory". The paper was entitled Falsifying String Theory Through WW Scattering, and was discussed extensively here. After the Wall Street Journal published an article in June about the problems of string theory, Distler wrote them to complain that the article was incorrect, because he and his collaborators had shown that string theory was falsifiable.

I had heard that this paper was going to be refereed, and was wondering whether a referee would really let the authors get away with the outrageous claim of their title. Well, it appears that the answer is no. A new version of the paper is now on the arXiv, with a new title: Falsifying Models of New Physics via WW Scattering. The abstract, which originally claimed that violations of the bounds they described "would falsify string theory" has now been modified to no longer make this claim; the new language is "would falsify generic models of string theory".

The paper has acquired a new co-author and been extensively rewritten. I'm assuming many of the changes were made to satisfy a referee. Besides changing the misleading, overhyped title, criticisms of earlier work embedded in one reference have been removed, and nine new references to earlier work have been added.

Comments

1. **optimist**
   October 2, 2006

   It sounds to me that any model from string theory should obey Lorentz Invariance, Unitarity and analyticity in S-matrix.

   So if one believes that these properies are essential for any realistic theory then this paper is tautology, giving potential overhyped claim to layman.

   But if one believes that nature could show strange behavior from the view point of conventional physics then string theory is indeed a falsifiable theory because string theory can not admit those kinds of strange behavior.

   The point is that how many people expect nature would not obey those properites. If the number is too small then it could be overhyped claim. If the number is not too small then it is a reasonable claim, I think.

2. **Who**
   October 2, 2006

   you mention the new co-author Rafael Porto.
   I see that Distler et al reference [22] is
   http://arxiv.org/abs/gr-qc/0402118
   A relational solution to the problem of time in quantum mechanics and quantum gravity induces a fundamental mechanism for quantum decoherence
   Rodolfo Gambini, Rafael Porto, Jorge Pullin
   13 pages
   Journal-ref: New J.Phys. 6 (2004) 45

   "The use of a relational time in quantum mechanics is a framework in which one promotes to quantum operators all variables in a system, and later chooses one of the variables to operate like a "clock". Conditional probabilities are computed for variables of the system to take certain values when the "clock" specifies a certain time. This framework is attractive in contexts where the assumption of usual quantum mechanics of the existence of an external, perfectly classical clock, appears unnatural, as in quantum cosmology. Until recently, there were problems with such constructions in ordinary quantum mechanics with additional difficulties in the context of constrained theories like general relativity. A scheme we recently introduced to consistently discretize general relativity removed such obstacles. Since the clock is now an object subject to quantum fluctuations, the resulting evolution in the time is not exactly unitary and pure states decohere into mixed states. Here we work out in detail the type of decoherence generated, and we find it to be of Lindblad type. This is attractive since it implies that one can have loss of coherence without violating the conservation of energy. We apply the framework to a simple cosmological model to illustrate how a quantitative estimate of the effect could be computed. For most quantum systems it appears to be too small to be observed, although certain macroscopic quantum systems could in the future provide a testing ground for experimental observation."

   (P.W. the original post had a typo where it said WWW scattering instead of WW scattering, you may already have corrected it by the time I submit this comment.)

   Gambini and Pullin do a variant of non-string QG related to LQG. they and Porto have more recent papers than the one cited which discuss the minimal rate of decoherence that necessarily attends using real clocks.

3. **MathPhys**
   October 2, 2006

   I don't say why the words 'string theory' show up there at all. They seem to be making very general statements that have nothing to do with strings.

4. **Chris Oakley**
   October 2, 2006

   > the original post had a typo where it said WWW scattering instead of WW scattering

   I hope it is not too late … there may already be an entry on the Reference Frame on the lines of "Anti-Science Crackpot Unable To Tell The Difference Between The World-Wide Web And The W Boson".

5. **SFB**
   October 2, 2006

   Who, I don't know what the relational post has to do with the rest of the thread, but the paper is interesting. However if you look at what I had thought was at least implicit in quant-ph/0506228 the calculations in the paper you mentioned seem confused. I tried to tell people about this in the mid 90's. Instead everybody ran with Rovelli's better publicized version, which is fine, but the understanding is superficial and they make all these fun but philosophically unmotivated calculations.

   I have started writing yet another paper explaining the old ideas, one that I hope will clarify this situation once and for all. Physicists like to calculate, whether in a philosophical vacuum or not, and string theory is just an extreme form of this calculate-itus.

6. **Chris W.**
October 2, 2006

SFB,

At some point you may have encountered an anecdote about Einstein's exasperation with a certain theorist, which Einstein expressed by the remark "the man can calculate, but he can't think." I believe I came across it in a book published during the centennial year of Einstein's birth (1979).

7. **Crimson**
October 2, 2006

I think there must be a confusion. Well, my confusion anyways. I just checked hep-ph/0604255 out of
curiosity, and I can only find version 3. I cannot find the v4 you link to and discuss in your comment. None of the changes you mention are there in v3.

8. **Crimson**
October 2, 2006

Ok, now v4 is actually there….

9. **Who**
October 2, 2006

Crimson, try
http://arxiv.org/abs/hep-ph/0604255v4

You say
**I think there must be a confusion. Well, my confusion anyways. I just checked hep-ph/0604255 out of
curiosity, and I can only find version 3. I cannot find the v4 you link to and discuss in your comment. None of the changes you mention are there in v3.**

it sounds like you just used the first link Peter gave and not the second

10. **Benni**
October 2, 2006

Lubos Motl has a new blog entry on this:
http://motls.blogspot.com/2006/10/falsifiability-in-physics.html
He calls Peter a nutcase….

11. **Jimbo**
October 2, 2006

I would strongly suggest that anybody who has seen Lubos' most recent rant, would agree that he is totally out-of-control, foul-mouthed, and should be severely disciplined by Harvard for utterly unprofessional behavior. He is beyond doubt, academia's best approximation to a human pit-bull, and should be de-clawed & neutered.

12. **Tony Smith**
October 2, 2006

Benni refers to a new blog entry by Lubos Motl.
In it, Lubos Motl says in part:
"… Lorentz symmetry breaking, breaking of unitarity, locality, rotational invariance … can't be embedded in string theory …",
and concludes that string theory is falsifiable because any one of those things, if shown experimentally, would therefore falsify string theory.

Lubos Motl goes on to say, in part:
"… String theory … has fixed Lagrangians and quantitatively accurate and rigorous formulae … it predicts various masses and cross sections. Moreover, it has no continuous non-dynamical adjustable parameters.
On the other hand, it has a large discrete set of classical solutions – possible universes. The number of them that have a chance to be compatible with the basic features of reality is probably finite.
Each of them accurately predicts the values of the usual quantities used in quantum field theory – masses and couplings. …".

Note that Lubos Motl does NOT show EVEN ONE example of a string theory solution that makes such predictions that are consistent with the experimental observations of the Standard Model.

Further, Lubos Motl goes on to say:
"… given the obvious progress and unique results with the supersymmetric vacua, we are confident that an answer to these questions exists in the non-supersymmetric case and it will be unique just like it was in the supersymmetric cases. …".

THIS IS AN IMPORTANT CHANGE IN HIS POSITION.
Back in 2004, acting as a sci.physics.strings moderator, Lubos Motl said:

"… "String theory" is a shorthand for "superstring theory" …"

His statement was made in the context of his opposition to my (non-supersymmetric) string version of my physics model that I put on the web as CERN-CDS-EXT-2004-031 as an example of a physically realistic (although non-supersymmetric) string theory.

Perhaps Lubos Motl has realized that the smart money is betting that the LHC will rule out the type of supersymmetry upon which "superstring theory" relies,
and
is now hyping non-supersymmetric string theory as a way to preserve the Bureaucratic Empire of SuperString Theory.

Even though my CERN-CDS-EXT-2004-031 could be seen as at least one concrete example of a realistic string theory,
something that Lubos Motl and his ilk have been unable to come up with themselves,
it is my guess that his hatred of me (he describes me as a "moronic crackpot") will outweigh his desire to exhibit it as such an example.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

13. **Vogelsang**
October 2, 2006

Wasn't it the observational discovery of the acceleration of the Universe's expansion that has led String Theory to its current sorry state of confusion? If so, it would be fair to say that it was Astronomers who showed that String Theory is not phenomenologically viable. That, in turn, would explain why Lubos hates Astronomy to such an irrational extreme.

14. **TheGraduate**
October 3, 2006

John C. Mather and George F. Smoot are our newest Nobel prize winners in physics!

15. **Stefan**
October 3, 2006

Lubos has written something on this topic [string theory falsifiability, or lack thereof] on his blog. As may be expected, it's the usual rant with semi-relevant and -valid technical points and arguments to give his post the requisite scholarly-authoratative lustre. All in all, I think he did a fairly decent job.

Moving on to other topics:

Peter,

Please help me!! [… or atleast help me to help myself!] make some sense of the Amazonian labyrinth that is hep textbook titles at Amazon.com [no pun intended : –)]. I feel like Bush ; –) in the White House when I surf through Amazon[.com] trying to figure out EXACTLY *which* titles are the best buys.

P.S.:
If anyone else wants to assist me, by all means send a 'yes' reply and I will provide access to my 'To Purchse Now' Wish List to you.

Stefan

16. **Who**
October 3, 2006

we could be entering a new stage of the game.
maybe stringery CAN be falsified as a fundamental picture of nature
and yet not falsified as an EFFECTIVE theory, permitting calculations.

one can have an effective theory which one knows is wrong, but which provides good approximate calculations.

so far one cannot calculate anything from stringery so one cannot check if it is a useful effective theory.

but at least one can imagine being able to show that it is fundamentally incorrect as a picture of nature. I think this is the direction that Distler et al paper is going:

Distler et al begins to persuade us that stringery might actually be falsifiable in a certain PHILOSOPHICAL way by general reasoning like, for example, this:

Gambini Porto Pullin showed on general grounds that realistically speaking time-evolution cannot be unitary. It is only approximately unitiary. There is a certain minimum necessary rate of decoherence.

Perhaps the most accessible paper is not the one which Distler et al cite but these more recent ones:
http://arxiv.org/abs/quant-ph/0608243
Relational physics with real rods and clocks and the measurement problem of quantum mechanics
Rodolfo Gambini, Jorge Pullin
19 pages

"The use of real clocks and measuring rods in quantum mechanics implies a natural loss of unitarity in the description of the theory. We briefly review this point and then discuss the implications it has for the measurement problem in quantum mechanics. The intrinsic loss of coherence allows to circumvent some of the usual objections to the measurement process as due to environmental decoherence."

http://arxiv.org/abs/gr-qc/0603090
Fundamental decoherence from quantum gravity: a pedagogical review
Rodolfo Gambini, Rafael Porto, Jorge Pullin
9 pages

We present a discussion of the fundamental loss of unitarity that appears in quantum mechanics due to the use of a physical apparatus to measure time. This induces a decoherence effect that is independent of any interaction with the environment and appears in addition to any usual environmental decoherence. The discussion is framed self consistently and aimed to general physicists. We derive the modified Schroedinger equation that arises in quantum mechanics with real clocks and discuss the theoretical and potential experimental implications of this process of decoherence."

Relational physics, using realistic clocks instead of presumed absolute time, is primarily an invention of Carlo Rovelli (that is, from the standpoint of Jacques Distler, the devil) and it has been most actively developed by Gambini, Pullin and by Distler's new co-author Rafael Porto.

Based on general arguments of Gambine et al, it is a NO BRAINER that string is falsifiable. Indeed it is already falsified as a fundamental picture of how nature is. So Distler wins the argument that it is falsifiable, right? String cannot be right because it assumes time evolution is unitary.

Conceivably also beginning next year one might be able to show that string cannot be a correct picture because it lacks the very slight energy-dependence of the speed of light appearing in other post-string QG theories—something that may be observed by the GLAST.

But these are very tiny effects which would in any case only falsify string as a fundamental theory. If it could be made to produce numbers, then it could still be tested as an effective theory. In this context I think Distler et al paper can be seen as a kind of shifting, which could be called progress.

It is why I made comment about the paper's reference [22] earlier, with the abstract of the 2004 Gambini Porto Pullin paper. I think Rafael Porto could be a big help to

Distler, if he is disposed to listen to reason 🙂

please excuse the rank speculativeness of this comment 🙂

17. **Garbage**
October 3, 2006

I think the paper didnt change its goals at all, indeed it made them broader!
I think the new version has effectively new results for WW scattering, which was unclear in the old version, and was discussed here as far as I recall. A violation of the new given bounds, in the absense of a light Higgs, will still falsify string theory in all of the forms currently known, or uknown if they obey the S matrix constraints discussed in the paper. One could argue that M theory (whatever that is) could get around this bounds but I pretty much doubt it if the latter desires any reality check. Even though experimentally speaking it is extremly unlikely the bounds will be violated, or the existence of a heavy Higgs for that matter, the idea remains appealing and deserves further study…

18. **Clark**
October 4, 2006

You may be interested by Burton Richter's Reference Frame in the October Physics Today: Theory in particle physics: Theological speculation versus practical knowledge
"…String theory was born roughly 25 years ago, and the landscape concept is the latest twist in its evolution. Although string theory needed 10 dimensions in order to work, the prospect of a unique solution to its equations, one that allowed the unification of gravity and quantum mechanics, was enormously attractive. Regrettably, it was not to be. Solutions expanded as it was realized that string theory had more than one variant and expanded still further when it was also realized that as 3-dimensional space can support membranes as well as lines, 10-dimensional space can support multidimensional objects (branes) as well as strings. Today, there seems to be nearly an infinity of solutions, each with different values of fundamental parameters, and no relations among them. The ensemble of all these universes is known as the landscape…
What we have is a large number of very good people trying to make something more than philosophy out of string theory. Some, perhaps most, of the attempts do not contribute even if they are formally correct…."
Richter ^

19. **Clark**
October 4, 2006

Corrected Link to Richter

20. **Tony Smith**
October 4, 2006

Clark mentions Burton Richter's Reference Frame article in October 2006 Physics Today.
In that article, Richter also discusses supersymmetry, saying:
"… If, for example, the Higgs mass is quadratically divergent, invent supersymmetry to make it only logarithmically divergent and to keep it small. The price of this invention is 124 new constants, which I always thought was too high a price to pay … a conceptual nicety was accompanied by an explosion in arbitrary parameters. However, the conceptual nicety, matching every fermion with a boson to cancel troublesome divergences in the theory, was attractive to many …
The Large Hadron Collider at CERN will start taking data in 2008 and we will know in a couple of years if there is anything supersymmetric there. If nothing is found, the "natural" theory of supersymmetry will be gone. …".

Richter's present-day view of supersymmetry is consistent with what he said 6 years ago in hep-ex/0001012 :
"… supersymmetry … was introduced to stabilize the Higgs mass which is quadratically divergent in the standard model and only logarithmically divergent in the supersymmetric variants of the standard model. Supersymmetry does reduce to the standard model at "low" energies, but it also introduces 80 real and 44 complex constants. The theorists who are fans of supersymmetry are groping for variants that reduce these 124 new constants to a handful. If the supersymmetric successor to the standard model cannot reduce the total number of constants, it would seem to me to be a step backwards rather than an advance.
To the experimenters I would say that supersymmetry is a pure "social construct" with no supporting evidence despite many years of effort. It is okay to continue looking for supersymmetry as long as it doesn't seriously interfere with real work (top, Higgs, neutrinos, etc.). …".

As I mentioned in comment 12. above, it seems that now even Lubos Motl seems to ahve lost his blind faith in the validity of supersymmetry,
possibly because he realizes that it is likely that the LHC will not find supersymmetry,
and that, as Richter said, "… the "natural" theory of supersymmetry will be gone. …".

With respect to the LHC, it may be worthwhile to quote some excerpts from Richter's hep-ex/0001012 :

"… LHC … experiments are huge and the sociology will be complex.
Beware of too many boards and committees … a bureaucratic overlay to the science with committees that decide on the trigger, data analysis procedures, error analysis, speakers, paper publications, etc.
The participating scientists are imprisoned by golden bars of consensus….".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

21. **Stefan**
October 4, 2006

Hello to All:

I am currently nearing the end of Ch. 31 of Penrose's mega-tome-mini-encyclopedia – 'The Road to Reality'. Chapter 31 is especially fun and informative as it relates to his views on 'strings', 'supersymmetry', and 'extra dimensions'.

I am hoping to start a lively discussion forum of interested individuals at my newly-started blog: 'Space-Time-Matter' [http://faustscorner.blogspot.com]. Interested readers are welcome to share their thoughts on the text.

Stefan

p.s. I haven't started posting on the topic yet; that will be tomorrow. (I have other things to do today.)

22. **Anonymous Coward Michael**
October 4, 2006

Hey Peter,

how's your own research coming? We are still waiting for you to explain to us all the subtleties of the BRST formalism. Surely you must be about to finish up your long awaited paper, right!?

23. **kramnik**
October 4, 2006

I'd just like to mention this paper hep-th/0610043 (and no I'm not one of the authors). It seems to be suggesting that N=8 super gravity might not be as plagued by infinities as first thought, which leaves the door open to a quantum field theory of gravity.

Any opinions?

24. **Peter Woit**
October 4, 2006

Garbage appears to be a sock-puppet…

I urge everyone who feels the need to discuss Lubos Motl's rants here to keep in mind John Baez's excellent advice that while it is difficult to ignore Lubos, it always repays the effort. I suspect John would have the same advice about Lubos's fan Michael.

25. **Who**
October 4, 2006

[b]Garbage appears to be a sock-puppet…[/b]

in that case I reckon Distler odds-on favorite
for the puppeteer.

26. **woit**
October 4, 2006

Who,

No, your guess is wrong.

27. **Garbage**
October 5, 2006

Woit & Who appear to be not even Wrong….;p

G

28. **Stefan**
October 6, 2006

Dear Friends,

I have posted my first entry at my very own site: 'Space-Time-Matter'.

Please make my site a success, by:

a) visiting it (thought this was obvious, but you never can tell these days);
b) leaving a helpful commentary.

Thank you.

Stefan

Controversy, Controversy….

October 4, 2006
Categories: Uncategorized

There's almost too much to keep track of the last couple days on the string theory controversy front:

Burton Richter of SLAC has a Reference Frame piece in the latest Physics Today entitled Theory in particle physics: Theological speculation versus practical knowledge. Richter shares my point of view that the Landscape studies currently popular in string theory are not science:

To me, some of what passes for the most advanced theory in particle physics these days is not really science. When I found myself on a panel recently with three distinguished theorists, I could not resist the opportunity to discuss what I see as major problems in the philosophy behind theory, which seems to have gone off into a kind of metaphysical wonderland. Simply put, much of what currently passes as the most advanced theory looks to be more theological speculation, the development of models with no testable consequences, than it is the development of practical knowledge, the development of models with testable and falsifiable consequences (Karl Popper's definition of science)…

The anthropic principle is an observation, not an explanation… I have a very hard time accepting the fact that some of our distinguished theorists do not understand the difference between observation and explanation, but it seems to be so…

What we have is a large number of very good people trying to make something more than philosophy out of string theory. Some, perhaps most, of the attempts do not contribute even if they are formally correct.

The issue of Nature that just came out today has an article about the controversy by Geoff Brumfiel with the title Theorists snap over string pieces: Books spark war of words in physics. He describes Lubos Motl's reviews of the Smolin book and mine on the Amazon web-site, and quotes Polchinski and Susskind. The reaction of string theorists to the books is said to be:

Few in the community are, at least publicly, as vitriolic as Motl. But many are angry and struggling to deal with the criticism. "Most of my friends are quietly upset," says Leonard Susskind, a string theorist at Stanford University in California.

and

The books leave string theorists such as Susskind wondering how to approach such strong public criticism. "I don't know if the right thing is to worry about the public image or keep quiet," he says. He fears the argument may "fuel the discrediting of scientific expertise".

Susskind will be giving a public lecture October 17 at UC Davis on String Theory, Physics and the "Megaverse".

Polchinski avoids the problems associated with the failure of string theory as a unified theory, and promotes in a somewhat overhyped way the idea that string theory explains the RHIC data.

Finally, Smolin makes an offer to string theorists that I feel I should try and match, hoping they will read his book to better understand exactly what he has to say:

If they don't want to buy it, tell them to get in touch with me and I'll send them a copy.

One thing Brumfiel gets a bit wrong is that my problem with string theory is not quite what he says "a fear that the field is becoming too abstract and is focusing on aesthetics rather than reality." The problems I see are rather different, with mathematical abstraction one of the few tools still available to theorists trying to make progress.

The same issue of nature contains an editorial Power and Particles lustily repeating much of the standard hype about string theory, noting that there are problems, but ending with:

Critical-mindedness is integral to all scientific endeavour, but the pursuit of string power deserves undaunted encouragement.

The editorialist definitely does not seem to be of the opinion that alternatives also deserve to be encouraged.

Finally, lots of reviews of Lee Smolin's book:

Unburdened by proof by George Ellis, also in Nature. Ellis takes the opposite point of view from the Nature editorialist, calling for more research on alternatives to string theory.

A loopy view by Michael Duff, in Nature Physics. Duff is extremely hostile to Smolin's book, sneering at Smolin and claiming that his book will "leave the reader rooting for strings" (funny, but this doesn't seem to have been its effect on most reviewers…). Duff agrees that there are problems with string theory, but claims that the problems Smolin correctly identifies are exactly the ones that he himself first identified back in 1987. String theorists like Duff seem torn between claiming that criticisms of string theory are crackpot nonsense, and that they themselves made them first. He goes on to furiously attack various straw men, accusing Smolin of "denying that any progress has been made!" (something I don't think Smolin does at all), and answering the criticism that string theory makes no predictions despite more than twenty years of effort by discussing how theories that did make predictions have sometimes taken a long time to be confirmed (or remain unconfirmed).

The string theorists were scammed! by Peter Shor on Amazon.

The Trouble With Physics by Sean Carroll at Cosmic Variance. If I can find the time, I may write about some of my problems with this review as a comment over there.

Comments

1. **fooltomery**
   October 4, 2006

   Peter,

   If the quarter you're looking for didn't fall near the streetlamp, looking for it near the streetlamp won't help. Perhaps the truth about nature lies far outside the circle of light cast by current theory's streetlamp. Perhaps that truth is stringy, perhaps not, but, as Peirce was wont to say, do not block the way of inquiry. Perhaps the exhaustive investigation of string theories will at least have the merit of revealing where the truth about nature is not.

2. **Peter Woit**
   October 4, 2006

fooltomery,

I think the exhaustive investigation of string theories has already had the merit or revealing where the truth about nature is not. Now the question is what is to be done to make progress on figuring out where it is.

3. **Michael Nielsen**
October 4, 2006

John Walker, founder of AutoDesk (makers of AutoCAD), has a review of Lee Smolin's book at: http://www.fourmilab.ch/fourmilog/archives/2006-09/000758.html

4. **Thomas Love**
October 4, 2006

Burton's article was great. Thanks for the link.

When I read the following in "Cranks, Quarks and the Cosmos" by Jeremy Berstein, I thought immediately of how the comment applies to string theory. Bernstein was discussing the errors Schroedinger made in his book "What is Life":

"What is Life" is proof that a brilliant but wrong idea in science can often have more of an impact than a dull but correct one. (page 63)

As I reread that, I realized that it only partially applies to string theory: String theory is wrong, but is not brilliant. Neither is it science.

5. **Kea**
October 4, 2006

Will any of my pro-M theory comments remain undeleted?

To say that String theory is not brilliant is a tall order, mathematically speaking. Personally, I am very, very tired of all this pointless bickering masquerading as intelligent conversation. It should be perfectly obvious that *both* the broad String formalism and some elements from alternative approaches will prove to be relevant to the eventual picture. Also perfectly obvious is that none of the approaches typically mentioned is correct.

6. **Michael Nielsen**
October 4, 2006

Peculiarly, I had forgotten that Walker also reviewed Peter's book:

http://www.fourmilab.ch/documents/reading_list/indices/book_502.html

7. **Who**
October 4, 2006

a propos controversy
in the long run I think Vilenkin's CNS paper, posted today on arxiv, will be important because of opening up the controversy over CNS—-which I think will prove significant as a falsifiable conjecture bearing on the determination of fundamental constants.

http://arxiv.org/abs/hep-th/0610051
On cosmic natural selection
Alexander Vilenkin
4 pages

"The rate of black hole formation can be increased by increasing the value of the cosmological constant. This falsifies Smolin's conjecture that the values of all constants of nature are adjusted to maximize black hole production."

Vilenkin goes to considerable lengths to refute the CNS conjecture: his argument involves black holes coming into existence by a "quantum fluctuation" and indeed at one point he invokes SOLAR MASS black holes coming into existence by quantum fluctuation

some heat could develop around Vilenkin's paper

8. **Garrett**
October 4, 2006

"If they don't want to buy it, tell them to get in touch with me and I'll send them a copy."

Oh great — I read this just after buying both books.

9. **Thomas Love**
October 4, 2006

Kea Says:

To say that String theory is not brilliant is a tall order, mathematically speaking.

–exactly the point! Some great mathematics has been done along the way, but there is no physics coming out!–

It should be perfectly obvious that both the broad String formalism and some elements from alternative approaches will prove to be relevant to the eventual picture.

—It is obvious to me that most of string theory will fall by the wayside. But I guess we won't know for sure until the eventual picture is revealed—-

10. **Dan**
October 4, 2006

hi Peter,

I did read your book, and have some thoughts:

1- I might have expanded as a full-length chapter on the history of extra-dimensions in physics, starting with Kalula-Klein, and how they failed, and what that might mean for string theory

2- I might have expanded as a full-length chapter on the history of GUT theories in physics, such as SU(5) and SO(10), and how they were falsified, and, what's more, how *conservative* they are, in comparison to string theory, and how given several candidate GUT's have been falsified, what this means for string inspired unification scenarios.

3- I might have expanded as a full length chapter history of physical theories that were "falsified" or found to be inconsistent, and what this might mean for string theory.

4- Maybe with a co-author (i.e Rovelli, Ashketar, Penrose, etc.,) I would have written a full-length chapter on quantum gravity, and how supergravity failed, and then the status of string theory as a theory of quantum gravity, and how well it succeeds in this claim.

5- Given more treatment on failed predictions of string theory. Go into detail string's version of particles and the standard model.

6- delete the chapter on string theory and mathematics and bogandov affair. Replace it with the SM and GR as to why they are a physics theory, and string theory, why it is not a physics theory.

7- explain that historically speaking, staying with 4D and observation and experiments have resulted in science, as opposed to speculation.

Dan

11. **Peter Woit**
October 4, 2006

Garrett,

It's an offer for string theorists….

Dan,

Thanks, some of the things you suggest are things I would have done if I had the energy and desire to write a longer book. Other things you mention I'm just not that interested in, and in that case think people are better off learning about these things elsewhere (and there are plenty of other places).

I really wanted to write about my own take on the subject, which concentrates not on quantum gravity, but on particle physics, and on the relationships to mathematics.

Luckily for people, there's Smolin's book too, which covers a lot of what I didn't.

12. **nontrad**
October 4, 2006

For what it's worth, a reply to Carroll's review would, at the very least, be appreciated by at least this one reader.

In the mean time, with Richter's juxtaposition of Thomas Aquinas and the Anthropic Principle, I can't help but wonder how Weinberg fits with all this…

13. **Peter Woit**
October 4, 2006

nontrad,

I hope that Smolin will find time to respond himself to that review, since it's a more serious criticism than many others. I took the time to write about one part of it, don't have time for more than that now.

14. **Aaron Bergman**
October 5, 2006

For whatever it's worth, I've only skimmed Smolin's book in a B&N, but from what I read, I pretty much agree with Sean's review.

I'd probably end up being a bit more snide, though.

15. **a**
October 5, 2006

it seems to me that Richter promotes the opposite unreasonable extremism.

First, he omits other successes of naturalness (corrections to the electron mass, to the mass difference between charged and neutral pions, etc). Anyhow, this sort of discussion would have been useful 10 years ago, but is now useless: LHC should soon conclusively tell if the weak scale is natural, or if we lost 30 years on a wrong track.

Second, no experiment tells that the cosmological constant is unnaturally small, because we never probed gravitons at energies above 10^-3 eV. Of course, we all expect that the theory of general relativity can be extrapolated at such "high" energies, giving a naturalness problem.

Third, like it or not, some (but not all) constants of nature really seem fine-tuned for "life". Since long ago this suggested that a theory that predicts everything does not exist, but it does not suggest that theory should predict nothing; the situation is not necessarily as bad as string theory suggests. For example, LHC data might allow us to reduce the number of free constants by confirming SUSY-GUT.

About RHIC: strings as an approximate model for RHIC is a minor result. One can easily predict that, in the present situation, any attempt of over-over-over-over-hyping it will fail.

16. **Bee**
October 5, 2006

reg. the Nature articles see also [The inverse problem]

17. **Thomas Love**
October 5, 2006

When I told my wife that Richter had compared String Theory to Theology, she replied: “What an insult to Theology”.

18. **bill lama**
October 5, 2006

Peter,
I’m a retired physicist (PhD in quantum optics, 1971) who would love to read the string and quantum gravity literature. While the popular books (including yours) are wonderful reading, they do not help one who wants to follow the calculations.

The research papers are incomprehendable, unnecessarily so in my opinion. Great physicists have found it possible to write advanced research papers that could be followed by a graduate physicist who was not a specialist in the sub-field. That quality is missing in the string and LQG literature.

I believe that could be corrected and wrote an “Open Letter to the Theoretical Physics Community” with a few suggestions on my blog http://palosverdesblog.blogspot.com.

Am I asking for an impossibility?

Keep up the great blogging.

Thanks,
Bill

19. **Gina**
October 5, 2006

DAN’S SUGGESTED CHAPTERS

I’d love to read the chapters Dan suggested to add. In particular, I will be happy to have any more detailed but accessible information (or links) on why the high dimensions (26 or 10). Actually, if Dan himself can say more on items 1-3 and 5 on his list and Peter wouldn’t object this will be great!

Some remarks on the controversy:

DO WE REALLY HAVE A CONTROVERSY (YET)?

A striking fact about the debate concerning string theory is that there is almost a complete agreement on factual matters between what string theorists say and what people who attack string theory say. (Can you list some real disagreements?) The interpretation of the facts is sharply different but many of the issues concerning the interpretation are not specific to string theory and are of very general nature.

CAN PHILOSOPHY OF SCIENCE HELP DOING SCIENCE?

This is a fascinating aspect of the discussion here and in Peter and Lee’s books. Philosophers will probably be the most skeptical about such “practical” applications of philosophy, for example, of Popper’s point of view. It is hard to consider various theories in philosophy of science as normative and it is hard to consider them as descriptive. (They also are in conflict, of course.)
These theories can be regarded as a way toward understanding and discussing in a scientific way what science is.

IS STRING THEORY FALSIFIABLE?

Well, I am not sure it is clear what string theory IS. But from the rough description of what it is, it seems very clear that string theory is falsifiable. For example, as Peter explained in the book the 26-dimensional model without supper-symmetry was rejected because it have consequences to physics that are regarded unreasonable. This, in principle, can happen to the super symmetric string theories.

THE BUSINESS OF FALSIFIABILITY

Falsifiability is not just a box you have to check and then forget about but rather it is a major part of the whole endeavor. It is not enough that the theory can be falsified in principle but researchers in the theory should devote considerable amount of efforts in this direction.

DESTROY STRING THEORY AND SAVE PARTICLE PHYSICS?

Peter’s noble reason for his attack on string theory is the desire to save particle physics. Well, one has to be very skeptic about claims of the form “Destroy X to save Y”. In this particular case one can be quite skeptical as well. But, in science, (appropriate) efforts to falsify a theory are as noble as efforts to prove it.

20. **nontrad**
October 5, 2006

Peter,

Fair enough; re the reply. I’ll look for Smolin’s comments in respective arenas. Thanks for the link none the less.

Aaron B,

With all due respect, I freely admit that I’m one of the worst sorts of skimmers myself, but surely, to review someone’s work requires perhaps more then simply skimming?

All the best,

nontrad

21. **Aaron Bergman**
October 5, 2006

Which is why I didn’t write a review.

HTH!

I’ve been listening to Lee on WGN radio for a while now, and he hasn’t said anything that makes me like him any more, though.

22. **anthropologist**
October 6, 2006

Well, one ought to give both of you guys (Peter and Lee) some serious credit. You forced the issue about the string theory into the open, and since "Nature" noticed it, many other people will take a notice too. That is a remarkable achievement, and not easy by any means! Sociology is not an easier science than physics, influencing masses takes some serious skill. No less than juggling some string equations around 🙂

Is this another episode in the series "Revenge of the Nerds" ? I do not know, but it surely does look so. Oh, the sweet revenge, on the establishement that is always right.

Guys, wow in it, you deserved it 🙂 There is so much political BS in the sciences, at least periodical upset of the status-quo somewhere should remind the establishment that it is not always right.

Signed, "closet revolutionary" 🙂

23. **Aaron Bergman**
October 6, 2006

Controversy is easy. The anti-establishment storyline is an especially easy sell.

And who's claiming that string theory is right?

24. **Tommaso Dorigo**
October 6, 2006

Hi Peter,

"Susskind will be giving a public lecture October 17 at UC Davis on String Theory, Physics and the "Megaverse"."

For some reason, while reading this sentence I could not help smiling, as this reminded me of those kinds of threats faced by people who become the target of fundamentalist muslims, like "we know your address"… As if you were disclosing where Susskind will be talking in order to unleash your adepts against him.

Because this is becoming more and more like a war. A good war – Free Science against String Theory fundamentalists. Should good wars be fought ? Is the risk that one "fuels the discredit of scientific expertise" worth running ?

I think so, but this is a rather odd situation in science, so we have little guidance from the past… The risk exists.

Cheers,
T.

25. **Alejandro Rivero**
October 6, 2006

Well, you do not need to look for fundamentalism muslims. Here in Spain, the pubs of RightWind and LeftWind fundamentalists had, pasted in their bulletin boards, this kind of lists of names of addresses of the opposite band.

Hmm, we have Spires anyway.

26. **Peter Woit**
October 6, 2006

Tommaso,

I should make clear that encouraging people who disagree with Susskind to attend his talk and in any way bother him was not my intention at all. I mentioned his talk just as an example to show that the pro-Landscape publicity machine is still in action.

These days, the anti-string theory point of view has been getting so much attention that I'm sure much of the audience for his talk will have heard that there are reasons to be skeptical of what he has to say. I don't think there's any reason at all for people who disagree with him to go out of their way to attend his talk.

27. **Ari Heikkinen**
October 6, 2006

Hi,

I'm en engineer for training and I understand your critisism about string theory. However, to mention one, I've read Greene's book about string theory and I think he's as honest as anyone could ever be about it. He clearly states that string theory might be wrong, and judging from his interviews I think he's the most reasonable physicist I've ever heard. Same goes for Kaku. Now, I don't really know if they're right or wrong. They might very well be wrong. Same goes for Newton and Einstein. Their stuff could very well be only an approximation. What do we really know about the universe? Nothing. We have space telescopes and we can look into the past and make educated guesses about things around us. Now, string theorists have their mathematical models that make sense to them and they make educational guesses based on them. Isn't that really what science is all about?

28. **Tommaso Dorigo**
October 6, 2006

Sure, Peter, mine was just a remark to share the funny feeling I had reading your post. But you did well in pointing out you are in no way encouraging people to disagree with Susskind… There indeed is already enough garage fights going on over string theory these days that it is better to state very clearly one is not interested in participating.

Cheers,
T.

29. **Who**
October 6, 2006

BTW the UK edition of Peter's book was #1 on UK amazon's physics bestseller list today.
Their physics category is quite broad: their next larger category up
"nature and science".
I'd say a #1 physics bestseller is worth celebrating.
http://www.amazon.co.uk/gp/bestsellers/books/278409/ref=pd_ts_b_ldr/026-8303087-5718006
At one point today I noticed it's overall sales rank among all the books they sell was #662 which looks pretty good for a book of this sort.

Here's to booksales Peter. Cheers!

30. **a**
October 7, 2006

dear Ari,
Greene forgot to tell that, unlike Einsetin and Newton, string theory seems intrinsically unable of telling anything about physics: whatever you ask, you get 10^500 alternative possible answers.

For example: does string theory predict that we will see supersymmetry?
Answer 1: yes. Answer 2: no. Answer 3: no. Answer 4: yes. etc etc etc 10^500 times.
In practice this can be summarized as: maybe, I don't know.

There are attempts of getting something out of this bad situation, but so far everything failed. Giving up and starting something else is the common-practice when doing research; but in the case of string theory this process is more difficult, because strings were publicly over-hyped.

31. **Spear Mark the Second**
October 8, 2006

Burt's article was interesting reading… he has been repeating that basic content for some time. Among particle experimenters, he has long been one of the most sympathetic to string theory.

There is a whole generation (maybe even two generations) of particle experimenters who came of age contemporaneously with string theory. Experimenters of all types tend to be skeptics and non-believers, and they are hard to characterize. Generally there is a lot of tolerance for our theoretical friends from grad school, who we often know, and know the talents of, pretty well.

I was turned into skeptic about even SUSY when no new particles were found at PETRA, PEP-I, and the SPPS. That the symmetry breaking scale was near 100 GeV and not a single member of the new particle families was less than that mass seemed (and still seems) suspicious. Of course, we're not done yet… a neutralino as light as 10 GeV is still a possibility, technically.

Which is a very important point: conclusively ruling out a conjecture as basic as the MSSM is a very strenuous, detailed, and exhausting experimental endeavor. Our theoretical colleagues long ago stopped providing us solidarity in that venture, and the decimation of budgets in HEP is related to our theorists losing interest in the actual physical world. Look at all the cancellations in the accelerator world…. KAMI, CKM, KOPIO, RSVP, BTeV, Braidwood (technically a reactor experiment), not to mention a witchhunt over the problems in Run II at the Tevatron. Add the sunset of PEP-II and CESR. On the one hand, many of these experiments don't seem to be at the energy frontier, and the energy frontier is always the most fruitful. On the other hand, crucial information has come from off the energy frontier… that SUSY generally provides large, flavor-changing neutral currents is a huge problem, and the experiments that failed to see those neutral currents are now over 40 years old, and were not even then performed at the energy frontier.

Let me go back to Burt for a second… there are significant details about him. They concern this oft-repeated statement that experimental particle physics has been in stasis for 30 years… with Burt, remember, that puts his discovery as the last truly significant one on the experimental side (well, maybe, the tau lepton discovery makes the cut too… done by his colleague Martin Perl, although Burt took his name off Perl's initial paper).

As someone who worked hard in experimental accelerator-based particle physics over those past 30-years, I'd say Burt minimizes the impact of other work (that he did not do) during that 30-year period. Of course he would minimize the impact of others' work… acknowledging others' work in those 30 years does not shine his star at all.

Many, many times during those 30 years the Standard Model seemed in jeopardy. Experiments that pursued the apparent shortcomings of the Standard Model during that period were crucial, and IMHO, way, way more important than all of the string theory developed contemporaneously. First, because experiment does provide a measure of empirical certainty, that string theory has not provided (so far). We don't have to argue any more about whether the Standard Model is mostly right anymore… had the experiments *not* been done, we'd have to constantly add conditions or admit the possibility that the Standard Model might be wrong about heavy quark mixing, low energy parity violation, etc.

Second, many of the experiments done in the last 30 years were the best chance at the time to actually find violations of the Standard Model. Effort was focused where sensitivity seemed apparent to new phenomenon, and actually, the sensitivities so achieved provide very strong constraints on anything new.

Turns out Burt was not very successful at doing most of those experiments. The SLC (his main project) was a fabulous and visionary effort, but was also marred by his organizational shortcomings and tendency to be arrogant.

The SLC got blown away by LEP (and I'm a SLAC alumnus). But now Burt says with some relish that it didn't matter, since LEP found nothing fundamentally new.

What else happened in those 30 years? I had to think hard to remember all the stuff, because, after all, the bottom line is right that the Standard Model was resoundingly verified. It is easy to just remember that, and forget the tortuous journey we all took. That journey (as I remember it) has a bunch of twists and turns, from excellent and ingenious experiments… I'll list a few, but I'm sure I overlook many…. the neutral charmed meson lifetime looked very long, but was finally nailed down by a terrific Fermilab experiment… the b-quark lifetime looked an order of magnitude longer than predicted, and turned out to be right… then neutral B mixing turned out to be way larger than predicted, also turned out to be right… these measurements led hard core phenomenologists (not string theorists) to successfully predict a very large top quark mass… the very large top quark mass is probably the central feature of the fermions in the Standard Model, and not a single string theorist predicted it, and indeed, they have reduced the phenomenon to a simple arbitrary parameter… well, then the Tevatron doggedly pursued the top and succeeded due to incredibly dedicated and ingenious work… the top quark discovery dwarfs all of string theory's contributions (so far) IMHO, and I was not involved in it. Then in CP violation, direct CP violation was an incredible source of controversy throughout the 1980's and 1990's, and was finally resolved again through terrific and ingenious experimentation. CP violation in the B meson system turned out to be observable, because of the surprisingly long lifetime and the large mixing, and again fantastic experiments were done on that topic.

Of course, there is the amazing story of neutrino oscillations and mass, that starts in the 1960's and still has not resolved itself. The major clues were *not* from accelerator based experiments, however, the final decisive experiments *will* be done at accelerator (or reactor) supported facilities.

BTW, is neutrino mass is *not* part of the Standard Model, and
is the first clue of physics beyond it. And many of the key experiments took place during the `30 year hiatus' that seems to have crept into the string theory apologias.

Of course Burt wants to reduce all that to a wish-sandwich, he was not involved. But the entire experimental community is still there, and knows. They know, they know, and they don't stand still, and they don't give up.

32. **Ari Heikkinen**
October 8, 2006

A couple of quotes from Sean Carroll's piece:

"It seems worth emphasizing that the dominance of string theory is absolutely not self-perpetuating. When string theorists apply for grants, they are ultimately judged by program officers at the National Science Foundation or the Department of Energy, the large majority of whom are not string theorists. (I don't know of any who are, off the top of my head.) And when string theorists apply for faculty jobs, it might very well be other string theorists who decide which are the best candidates, but the job itself must be approved by the rest of the department and by the university administration. String theorists have somehow managed to convince all of these people that their field is worthy of support; I personally take the uncynical view that they have done so through obtaining interesting results."

and

"To be clear, the scientists working on LQG and other non-stringy approaches to quantum gravity are not crackpots, but honest researchers tackling a very difficult problem. Nevertheless, for the most part they have not managed to convince the rest of the community that their research programs are worthy of substantial support. String theorists are made, not born; they are simply physicists who have decided that this is the best thing to work on right now, and if something better comes along they would likely switch to that. The current situation could easily change. Many string theorists have done interesting work in phenomenology, cosmology, mathematical physics, condensed matter, and even loop quantum gravity. If a latter-day Green and Schwarz were to produce a surprising result that convinced people that some alternative to string theory were more promising, it wouldn't take long for the newcomer to become dominant. Alternatively, if another decade passes without substantial new progress within string theory, it's not hard to imagine that people will lose interest and switch to other problems. I would personally bet against this possibility; string theory has proved to be a remarkably fruitful source of surprising new ideas, and there's no reason to expect that track record to come to a halt."

I think those are excellent points.

33. **woit**
October 8, 2006

Spear Mark II,

Thanks for the excellent, informative and thought-provoking long comment!

Ari,

Please don't take up space here putting in long quotes from material I've already linked to and encouraged people to look at. You're not adding anything at all to the discussion.

I responded over at Cosmic Variance to part of Carroll's piece, for the piece that Ari quoted, there's an interesting response here:

http://angryphysics.blogspot.com/2006/10/hiring-string-theorists.html

If there's an interesting discussion going on at other blogs, it would be best if people not try and move it over here, but carry it on at its original location, perhaps providing pointers from here.

34. **Jonathan Vos Post**
October 8, 2006

Strung along

[Los Angeles Times, Book Review section, Sunday, 8 Oct 2006]
http://www.latimes.com/news/science/la-bk-cole8oct08,1,4278240.story?ctrack=1&cset=true

Not Even Wrong The Failure of String Theory and the Search for Unity in Physical Law Peter Woit Basic Books: 292 pp., $26.95

By K.C. Cole,
K.C. Cole, who teaches science journalism at USC's Annenberg School, is the author of "The Universe and the Teacup: The Mathematics of Truth and Beauty."
October 8, 2006

The Trouble With Physics: The Rise of String Theory, the Fall of a Science,and What Comes Next
Lee Smolin
Houghton Mifflin: 392 pp., $26

IN physics, truth and beauty often walk hand in hand. Physicists describe theories as "ugly" or "beautiful," talk about ideas that "smell" or "feel" right. Often, aesthetic judgments lead to discoveries: as in Einstein's theory of gravity and Paul A.M. Dirac's discovery of antimatter. Aesthetics, French physicist Henri Poincaré said, is a "delicate sieve" that sorts the true from the misleading. Or as Dirac famously put it: "It is more important to have beauty in one's equations than to have them fit experiment."

To mathematician Peter Woit and physicist Lee Smolin, however, the search for beauty is ruining physics. Their ire is directed at "string theory," a magnet for physicists because it is so, well, beautiful, and has such great promise for solving what may be the central mystery of the universe — the incompatibility between the two grand laws that describe everything we know.

Quantum theory — which explains the subatomic world with exquisite precision — reveals that at close range, matter, energy and motion are a choppy mosaic of jittery bits. Think pointillist painter Georges Seurat on a triple espresso. Einstein's theory of gravity, which describes the large-scale cosmos with exquisite precision, tells us that space and time are woven into a smooth, seamless surface that warps under the influence of massive objects — a universe painted by Salvador Dali. Where the two realms meet, the quantum jitters shatter the glassy surface of space-time like a child cannonballing into a pool.

String theory is the first approach that seems to bring the two together naturally, and such unification of opposites, like electricity and magnetism, has driven physics for more than a century. Simply put, string theory does this by replacing point-like particles with tiny strings of some fundamental stuff vibrating in 10-dimensional space — their harmonies creating everything from quarks to galaxies. The loops of string don't let anything get small enough to let quantum fidgeting rip space and time apart.

String theory has its troubles, which the authors analyze in great and sometimes lucid detail: It appears to be untestable because the strings are too small to be seen, and recent research suggests that the theory may have an infinite number of solutions, so it can't make predictions. And string theory is so ill-defined that even ardent supporters admit they don't know what, exactly, it is. This is why Woit calls the theory, and his new book, "not even wrong," a play on a put-down by the late physicist Wolfgang Pauli.

These issues are well worth addressing, which makes it all the more disappointing that Woit, and Smolin in "The Trouble With Physics," write mostly about how string theory has ruined their careers — and physics as well. It has "choked off" investigation of "equally promising approaches," Smolin says. It is a "cult" in which "believers don't care about evidence." Physicists who don't work in string theory are rejected and shunned. "The ability to do mathematically clever work … [is] valued over the possession of original ideas," he complains. As for beauty, he writes that "elegance" is irrelevant, and "more sober minds" should insist on "a connection to reality."

Although Smolin's book is fairer and far more readable, both suffer from an overflow of jargon. And their language is telling: String theory is described as a "fad," "fashion" or "trend," its culture as "brash, aggressive, and competitive." String theorists "swagger." References to Smolin's kind of physics, on the contrary, are accompanied by words such as "deep" and "thoughtful."

Smolin is a respected physicist, having earned a PhD at Harvard University and written several delightful popular books, including "The Life of the Cosmos" and "Three Roads to Quantum Gravity." He helped found the Perimeter Institute in Waterloo, Canada, as well as a perhaps promising theory called "quantum loop gravity." He's also a former string theorist, so his book is well-informed.

Woit is a different story. As a postdoctoral fellow at State University of New York at Stony Brook, he couldn't find another position because, he says, he wasn't working on string theory. Woit then moved to Harvard, where the physics department "let me use a desk as an unpaid visitor." He's now a math lecturer at Columbia University.

The authors are right to say that physicists can get cliquish; that some of them swagger; that they frequently fool themselves and that science has become too risk-averse. On the other hand, dozens of astrophysicists, cosmologists, relativists and people who study fundamental particles and interactions in ways not related to string theory do quite nicely; some even dip into theory now and then. In fact, many highly esteemed physicists who formerly disdained string theory (Nobel laureates Steven Weinberg and Murray Gell-Mann among them) have become fans.

So it's hard to believe, as both authors charge, that physicists have been led like sheep to pursue a "failed theory," mostly by Edward Witten, now at the Institute for Advanced Study in Princeton and generally acknowledged genius — and also a nice guy. (In addition to his multiple prizes in physics, Witten also won the Fields Medal — the "Nobel Prize" of mathematics.)

True, Witten is highly influential. But it's hard to imagine him ruining an entire generation of physicists. They are not, in general, followers; getting them to agree on anything is like herding cats. They love nothing better than to prove each other wrong.

The claim that string theory can't be tested is serious; experiment is the ultimate arbiter of truth. But it's impossible to know what is ultimately testable. When the ghostly neutrino popped up in one of Pauli's equations, the physicist admitted he'd done "a terrible thing. I have postulated a particle that cannot be detected." Then in 1956, traces of neutrinos were seen in the wash of radiation spewing from newly commissioned nuclear reactors.

As for Woit's claim that string theory has "absolutely zero connection with experiment," experiments already planned for a new European particle accelerator will look for the existence of extra dimensions and extra families of particles — both predicted by string theory. In fact, many statements about string theory in these books are plain wrong. To say, as Smolin does, that string theorists are not trying to figure out how space and time came into being will surprise the dozens who do just that. To say, as Woit does, that fundamental mysteries about neutrinos are being ignored will come as news to the dozens of physicists who've been working on these problems for years.

So what good, ultimately, is beauty? As the late physicist Victor Weisskopf said, "What's beautiful in science is that same thing that's beautiful in Beethoven. There's a fog of events, and suddenly you see a connection."

Neither Woit nor Smolin sees the beauty in string theory. But perhaps they haven't spent enough time in the fog. Theories often seem impenetrable at the time they are being discovered — and clear and simple (and beautiful) only in retrospect. One of the strangest charges against Witten is that he's often openly muddled. Asked his opinion about a recent turn in string theory, he answered: "I just don't have anything incisive to say. I hope we will learn more." Smolin interprets this as Witten being "stumped." Perhaps it's a sign that he's thinking.

In the end, Smolin admits that he hasn't managed to do much better than string theorists, and his book is "a form of procrastination." One hopes he will soon dive back into the fog and start making connections.

=== end quotation from L.A. Times ===

35. **Norman Stevens**
October 8, 2006

String theory: Is it science's ultimate dead end?

http://observer.guardian.co.uk/uk_news/story/0,,1890340,00.html

36. **D R Lunsford**
October 9, 2006

Well Smolin made an excellent rebuttal here:

http://cosmicvariance.com/2006/10/03/the-trouble-with-physics/#comment-123907

-drl

37. **D R Lunsford**
October 9, 2006

Spear Mark, that was really inspiring and somewhat scary.

-drl

38. **Tony Smith**
October 11, 2006

Spear Mark the Second said:
"… the very large top quark mass is probably the central feature of the fermions in the Standard Model, and not a single string theorist predicted it …".

Ari Heikkinen said:
"… Sean Carroll …[said]… "… If a latter-day Green and Schwarz were to produce a surprising result that convinced people that some alternative to string theory were

more promising, it wouldn't take long for the newcomer to become dominant. …".

In the present climate of controversy over superstring theory, I think that one who produced a "more promising … alternative" would NOT "become dominant", but would be attacked and ostracised.

In support of my view, I offer my personal experience:

For over 20 years

(published as early as two papers in the International Journal of Theoretical Physics (vol. 24 (1985) 155-174) and vol. 25 (1986) 355-403) , the papers having been received by IJTP on 27 February 1984 and 16 October 1984, respectively, around the time that CERN was claiming to have found the T-quark at around 40 GeV, prompting John Maddox to write a Nature article (Nature 310 (12 July 84) 97) headlined "CERN comes out again on top")

I have espoused a physics model that in fact did and still does explain "the very large top quark mass"
(in terms of combinatorics and physics-related geometry motivated by, but not identical to, the ideas of Armand Wyler)
but,
far from me being a "newcomer" who has "become dominant",
I have been blacklisted by the Cornell arXiv and characterized on the web as a "moronic crackpot" by Harvard's superstring theory web-spokesman, Professor Lubos Motl.

One result is that, when I recently made an attempt at explaining the basic ideas of my constituent mass calculations, it was not posted on the arXiv, and can only be found on the web from my web site at
http://www.valdostamuseum.org/hamsmith/July2006Update.html#factorsphysics
and
another result is that, as Peter mentioned in another entry in this blog,
Michael Green is able to say (without fear of contradiction from the establishment) "There is no alternative to string theory. It is the only show in town – and the universe.".

Still another result is that, if even only some part of my model turns out to be correct and useful, it will remain unknown to the established physics community because its members will not risk damage to their careers by investigating the work of a blacklisted "moronic crackpot".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

39. **Ari Heikkinen**
October 11, 2006

Come on, Lubos' hardly a spokesperson for physicists or even string theorists. As far as I know most physicists aren't string theorists anyway.

If someone says a writer of a paper is a crackpot claiming he's wrong even thought it's right then he'd ruin his reputation. It's amusing to say that one person could ruin someone's career. I think everyone's well qualified to do that themselves.

I think the general public is more or less amused of physicists and theoretical physicists calling eachothers crackpots. It's just a shame that good science projects get cancelled because scientists can't get along.

And lets not forget that mostly taxpayers pay science. It's just sad to know tax money goes for salaries of people who, instead of doing good work, waste their time calling eachothers crackpots.

40. **Robert Ehrlich**
October 13, 2006

As a retired academician / statistician and having just read Smolen's book, a major premiss is not the physical unreasonableness of ST but that ST is a means of replacing the parametric problems in QM with another set in ST. The result is that essentially no progress is made. Smolens' physical arguments are cogent, but to one who has been burned before, the charge of legerdemain of the parameters is striking, if true.

Navier-Stokes Equation Progress?

October 5, 2006
Categories: Uncategorized

Penny Smith, a mathematician at Lehigh University, has posted a paper on the arXiv that purports to solve one of the Clay Foundation Millenium problems, the one about the Navier-Stokes Equation. The paper is here, and Christina Sormani has set up a web-page giving some background and exposition of Smith's work. I should emphasize that I know just about nothing about this kind of mathematics, but I'm reporting on this here for two reasons:

1. It looks plausible that this really is important.

2. Penny Smith tells me that she is a regular reader of this weblog.

Update: There's an informative news article about this on the Nature web-site.

Comments

1. **TheGraduate**
   October 5, 2006

   very fascinating.

2. **astro**
   October 5, 2006

   With Cao and now Smith, is Lehigh becoming a new powerhouse in math?

3. **Jud**
   October 5, 2006

   "With Cao and now Smith, is Lehigh becoming a new powerhouse in math?"

   Becoming? 🙂

   (I grew up in Bethlehem, PA, where Lehigh is located, and live nearby.)

4. **fh**
   October 5, 2006

   I heard Prof Ehlers talk about her work on GR late last year, it sounds as if her methods are capable of solving some long standing problems in mathematical physics. Very impressive.

5. **Demonic Gerbil**
   October 5, 2006

   This certainly looks promising. I wish I was better versed in the relevant details so I could attempt to judge it for myself.

6. **nc**
   October 5, 2006

   Thanks for reporting on this 9 page paper, 'Immortal Smooth Solution of the Three Space Dimensional Navier-Stokes System'.

   Wikipedia's page http://en.wikipedia.org/wiki/Navier-Stokes_equations says:

   'A $1,000,000 prize was offered in May 2000 by the Clay Mathematics Institute to whoever makes substantial progress toward a mathematical theory which will help in the understanding of this phenomenon.'

   Wiki on another page http://en.wikipedia.org/wiki/Navier-Stokes_existence_and_smoothness lists the exact problems. So it is solved unless there is an error in http://arxiv.org/PS_cache/math/pdf/0609/0609740.pdf

   'We prove the existence of a smooth solution for all time – under physicially reasonable hypothesis on the initial data – for the Navier-Stokes System in three dimensions.'

   How long will that take to be properly peer-reviewed before publication? (I trust it will be checked far more carefully than the Bogdanov's physics papers on string theory…)

7. **fh**
   October 5, 2006

   nc, this is building on a long string of well established, peer reviewed papers Smith has written over the last few years.

   This should be "relatively" straightforward to check.

   BTW, just for fun, her Mathematical genealogy traces back through Weierstraß to Gauß, and in a different branch via Hilbert and Klein to Poisson and Fourier and on to Lagrange, Euler, Bernoulli and Leibniz.

   http://genealogy.math.ndsu.nodak.edu/html/id.phtml?id=47125

8. **John Baez**
   October 6, 2006

   It would be really great if Penny Smith – or anyone! – made some serious progress on the Navier-Stokes equation.

Does anyone know whether this is the same “Penny” who used to post to sci.math and sci.physics?

9. **jamzik**
October 6, 2006

Yes,
PSmith9626 seems to fit the bill, Lehigh University, an interest in the Navier-Stokes equation – but it appears she stopped posting in early 2005. She must have found something that occupied her time.

10. **MathPhys**
October 6, 2006

John,

Yes, it’s the same Penny.

11. **erno**
October 6, 2006

Katherine Hayles summarizes Luce Irigaray:

“The privileging of solid over fluid mechanics, and indeed the inability of science to deal with turbulent flow at all, she attributes to the association of fluidity with femininity. Whereas men have sex organs that protrude and become rigid, women have openings that leak menstrual blood and vaginal fluids… From this perspective it is no wonder that science has not been able to arrive at a successful model for turbulence. The problem of turbulent flow cannot be solved because the conceptions of fluids (and of women) have been formulated so as necessarily to leave unarticulated remainders.”

And indeed, it takes a woman to solve it! (Maybe) Physics? HA! Postmodern 5th Wave Feminism has the answers.

12. **Yatima**
October 6, 2006

May I suggest some intense sessions with a good Freudian analyst (not that I think that psychanalysis is anything other than ‘not even wrong’)

13. **csrster**
October 6, 2006

Oh gawd, that was something that also lurched unwanted from my suppressed subsconscious when I first read this item. (At the time I first heard it I was a PhD student in Cambridge Applied Maths, a powerhouse of mostly-male fluid dynamicists.)

14. **Dick Thompson**
October 6, 2006

I have always cited the state of fluid mechanics, where the equations have been known forever, but no real analytical progress ever seems to occur, as my comeback to dreams of a final theory.

Do I have to change my tune now?

15. **Gina**
October 6, 2006

GO PENNY GO!

(Of course, we have to remember that the NS problem is infamous for
“fighting back” as the Hungarian mathematician Paul Erdos used to say about famous problems.) I wonder if the approximation of NS by certain hyperbolic equations used by Smith have physics relevance/intuition on their own, or are just technical tools in the mathematical proof.

16. **TheGraduate**
October 6, 2006

I wish somebody knew enough to place odds. This is intriguing.

17. **Michael**
October 6, 2006

Hey Peter,

this Navier-Stokes paper is quite interesting. I don’t dare to judge its contents, though. I hope some math colleagues will enlighten me with their expert opinions soon.

Mind if I ask how your own research is coming? You ignored my question last time I asked.

You announced in the spring that a paper of yours on some BRST-subtleties would be fourthcoming this year. Can we hope to read it soon?

18. **Walt**
October 6, 2006

Erno: I forgot all about that Irigaray quote. If Smith has really solved it, that would be an incredibly funny development.

19. **Peter Woit**
October 6, 2006

Michael,

I ignored your question because, from past experience, I don’t think you’re interested in the answer. The answer though is that my work on BRST is not moving as fast as I would like since the response to the book has been kind of overwhelming and it’s taking all of my free time to deal with it. I hope this will die down soon and I’ll get back to making progress on that project.

20. **John Baez**
October 6, 2006

Various unnamed sources say the proof doesn't seem plausible. Probably best to cool it and wait until the paper is refereed.

21. **penn smith**
October 6, 2006

John, it's penny. Can you email me and tell what these "unnamed sources" think is wrong?

Here is one (not important) error found in "Perron's Method for Hyperbolic Systems Part I": In Theorem 21 One needs POSiTIVE
initial data and positive g. IT then takes a little more work to prove the
comparison principle for sub and supersol with the SAME initial data.
Similarly, in Theorem 22. Otherwise counterexamples occur.

However, in both the Einstein and NS papers, I provide a different comparison principle (used in three space dimensions only) with a different proof.

22. **penn smith**
October 6, 2006

The point is that in the proof of theorem 21 of that paper, to apply the
Protter maximum principle to the parabolic approximating equation, one needs postive (not non-nonegative) initial data. In my handwritten version, I had that, and the inequality got miscopied in typeing.
Again, in the two later papers, a comparison principle with the SAME initial data, is proved and used only in three space dimensions by
a different method.

23. **penn smith**
October 6, 2006

And what is this " Unnamed sources tell me that something is implausible" nonsense.
If they have a real mathematical issue, they should (out of courtesy and out of mathematical professionalism) email it to me, so I can look it over.

That is what someone did with the issue that I just posted. I thanked them too!!

THAT"S HOW MATH WORKS. We are grateful for error correction.
We are not a cult.

And I personally care only about truth. I don't care about ego nonsense.

24. **mathjunkie**
October 6, 2006

Does Penny's solution mean that it is the solution describing turbulent flow?

25. **Brooks Moses**
October 6, 2006

Well, the various sources that I've seen claiming that this "doesn't look plausible" are one that are responding to claims that this is a constructive proof (with implications that it will be directly useful in simulations of fluid flow). That claim is, in my opinion, quite implausible — but I don't have the math background to back it up. However, I'm pretty sure that's not the claim Penny is making; at the least, she's certainly not claiming that implication, and that's the really implausible part.

Meanwhile, to answer mathjunkie's question: Given that it's possible to define a non-turbulent initial condition that will lead to turbulent flow in an infinite domain (namely, a sufficiently strong shear flow, at least in a localized region), the solution that Penny's paper describes does, indeed, have the potential to contain turbulence. Since it's not a constructive proof, though, it's not too clear to me what relevance that will end up having to the study of turbulence — though the actual smoothness-of-solution result may well be useful.

Now, my curiousity is how difficult it will be to apply this to flows with boundaries — and whether the method of doing that will involve the analytical equivalent of the computational "immersed boundary method", in which a flow with a boundary is simulated by taking a flow in an infinite domain and applying forcing functions to it that cause the velocity to match a prescribed velocity along a certain manifold. Though that may be a bit tricky, given that the forces are usually Dirac delta functions, and thus the velocity isn't smooth on the manifold. (I think there are ways around that, but I'm a computationalist, not a mathematician.)

26. **penny smith**
October 6, 2006

More detail, for those reading that paper:
In theorem 22 we require $w_{1}(x) =w_{2}(x)$ and
$L(u_{1}}>0.
This arises because to get nonstrict inequalities Protter (for the parabolic max principle) adds an exponential NOT IN MY HYPOTHESIZED FUNCTION SPACES.

IN THEOREM 21 we require $w_{1}= w_{2}$.

That kills the counterexamples.

To get Theorem 21 from Theorem 22, on page 365 line 7, we apply the
proof (not the statement) of Theorem 22 with a sequence of positive inhomogenous terms and initial data decreasing to zero.

AGAIN, NONE OF THIS USED OR NEEDED in either the Einstein Cauchy or the NS papers!

27. **penny smith**
October 6, 2006

Thanks Brooks,
It is NOT a constructive proof. It is an existence theorem.

The Perron method gives a solution as a lower envelope of a transfinite set of supersolutions and are then doing a triply countable sequence of those approximations to get the NS solution.

It is NOT applied computational math.

28. **Brooks Moses**
October 6, 2006

Gina: the approximation by these particular hyperbolic equations does indeed have physical relevance on its own. The incompressible Navier-Stokes equations have some variables (specifically, pressure) that have effectively zero relaxation time. In an incompressible fluid, a pressure wave travels at infinite velocity. One can add a bit of "compressibility" to the equations by giving the pressure a finite relaxation time, and thereby make the problem hyperbolic — and that's what Penny's formlation of the hyperbolic equations does. This was a relatively common computational solution method some decades ago, and is referred to as an "artificial compressibility" method. There are also more modern methods that are based off of that, but do a convergence to a truly incompressible solution.

Since Penny's forumulation also breaks things up into a first-order set, there's a similar issue (without as much physical significance to the problem) in the stress tensor, and so she adds a relaxation constant there; this, I haven't seen before in exactly that form, probably because it doesn't map to a computational problem the way the zero relaxation time on the pressure does.

29. **Richard**
October 6, 2006

"Various unnamed sources say the proof doesn't seem plausible."

I can't believe that John actually posted this comment. "Various unnamed sources" has the ring of a Bush administration leak, and "doesn't seem plausible" is dismissive while having no other content or value of its own.

This is more typical of a Motl drive-by shooting. I'm demoralized.

30. **John Baez**
October 6, 2006

I should not have mentioned vague second-hand doubts about Penny's proof on this blog. My only sensible point was that people should calm down and wait until some experts have had a chance to go through this proof.

31. **John Baez**
October 6, 2006

I should add that I would like nothing better for Penny's proof to be right!

32. **nontrad**
October 7, 2006

To John's last comment: Hear, hear!

Beyond the fact that NS is a intricately interesting system of equations (I encourage all to muck around with these equations if they never have; since everything from GR to QCD to EM and, infact, huge swaths of PDE can pop up in suprising ways depending on one's background) — it would be really truly *great* if Dr Smith's work really did knock over this historical giant…

Looking forward to learning more, and hoping for the best on this concern!

33. **penny smith**
October 7, 2006

Brooke is very smart. He understands.
By the way, I have posted the stuff I wrote here about the corrections to the earlier paper at arxiv. It will appear late on Monday EST. You people got first dibs.
Thanks for the interest in my paper.

34. **Johan Richter**
October 7, 2006

What would the physical relevance, if any, be if this paper held up?

Also how long do you think it will take before the paper is checked? It is short so we should not have to wait as long as we did with Poincare should we?

35. **MathPhys**
October 7, 2006

Perelman's papers are not that long. That was the trouble 🙂

36. **surlygrad**
October 7, 2006

pardon my ignorance, but is the analogous result known for the IBVP, on some reasonable domain?

37. **jeremy**
October 7, 2006

Penny,

Can you write an "expository paper" for your work on NS equation, meaning to have all the details in one paper? It makes a difficult read having to check all the references; some of them are yet to appear. Hopefully this would not require too much of work.

38. **Michael**

October 7, 2006

Peter,

no need to doubt that I wanted to know what I was asking. Yes, I dislike your populist anti-science crusade. But that makes me all the more interested in what you have might have to say on a more technical level.

Let me freely admit that I don't believe you are doing any research at all. I believe you are pretending to make yourself look more serious and competent than you are. I would happily stand corrected, however, if you could demonstrate the ability to write a decent research paper — no matter if it's math or physics. I'll be patient and check back with you every few months to see if you are making any progress. Please keep us posted. Thanks.

39. **penny smith**
October 7, 2006

This doesn't affect either my proof of NS or Einstein Cauchy.

I posted some fixups here of theorems 21 and 22 of "Perron's Method for
Quasilinear Hyperbolic Systems PartI" and have now found another fixup needed
so I have decided to ditch those theorems and replace them by Theorem 4
of my Einstein Cauchy Arxiv paper math.DG/ 0605352.

This gives the results of the Part I Perron paper above in all space dimensions that are odd and bigger than or equal to three. Lately, I seem to care only about three space dimensions (smile).

The correction will appear at arxiv late on Monday evening.

40. **Peter Woit**
October 7, 2006

Michael,

Thanks for your interest, although I think you have me confused with someone else (Lenny Susskind?), since I've never been on a "populist anti-science crusade".

41. **penny smith**
October 7, 2006

Dear Jeremy,
The Corrigendum to the old paper is at both JMAA (electronically through science direct), and at Christina's Somani's webpage for me.

When the dust settles and I get more than two hours a night sleep, I may indeed write something expository, but odds are that someone else will do it.
best
Penny

42. **jeremy**
October 7, 2006

Penny,

Thanks for the information. I hope you will get some more sleep and get refreshed.

All the best.

43. **Michael**
October 7, 2006

Peter,

believe me: I wouldn't confuse you with Susskind in a million years. You see, he is a maverick with incredible skill and talent. I don't agree with many of his views, but I sure admire his many important and beautiful papers.

44. **Marty Tysanner**
October 7, 2006

re: "Michael"

There is an interesting discussion of a potentially very important mathematical result going on here, and what does this "Michael" contribute? Pure noise and jarring distraction that has absolutely nothing to do with the topic at hand; his demonstrating thatcomments here are unsubstantive by any measure. As usual, "Michael" is acting like someone who is clueless about what is going on around him, someone who has an adolescent fixation on heckling Peter because he disagrees with Peter's broader viewpoints about the direction of research in string theory and particle theory in general.

But this is what we have come to expect from "Michael" — nothing of substance, just air-headed commentary that is almost invariably on the same topic: *So, Peter, how's your research coming? I think you are a charlatan. Blah, blah, blah…* I don't know what he *thinks* he is accomplishing by this immature mode of discourse, but to rudely interrupt a meaningful discussion with mindless, irrelevant blather like his above comments is bound to convince others that he is nothing more than an insensitive, anti-intellectual boor.

I wish Peter would be more merciless about deleting such worthless comments.

45. **Peter Woit**
October 7, 2006

Marty,

For better or worse, I've had a policy of allowing string theorists to post whatever comments they want here, without deleting them. Some people are convinced that I'm deleting all the intelligent comments with substantive responses to my criticisms of string theory, and just leaving the juvenile, worthless ones. Not so at all.

46. **surlygrad**
October 7, 2006

why isn't anyone asking the important question: how are we at columbia going to sucker penny smith into coming to present her work?

while the N-S solution will be fantastic if true, these results on a comparison principle for hyperbolic equations are interesting in their own right and i'd love to see them.

47. **David Purvance**
October 7, 2006

Sounds incredibly coincidental, but a "Fer Product" solution to the 3-space periodic Navier-Stokes was posted on arXiv on 2Oct2006. Has anyone looked at or commented on this paper?

48. **Nets Katz**
October 7, 2006

Perhaps this is not the right place to have a philosophical discussion on the theory of PDE. I confess that I have not attempted to verify the proof line by line, but having looked at the paper, I will nevertheless go out on a limb and say that I have doubts.

In order to solve a hard problem, generally you have to overcome the difficulty. The difficulty in Navier Stokes has little to do with the linear terms – the parabolicity of the equation – but has more to do with the nonlinear terms – supercriticality. For fixed energy, if we scale down a pulse, the nonlinear term dominates the linear ones.

What I find very curious about this claimed solution is that supercriticality is never addressed. We replace the equation by a 1st order system which can always be done by introducing derivatives of the original unknowns as unknowns. We add a tiny bit of differentiation in t to make the equation hyperbolic. We claim that hyperbolicity is all that matters, and we even get to take a limit making the terms that caused hyperbolicity to disappear.

But there are genuinely hyperbolic problems where supercriticality is the issue. Why not try to do the same thing for, say, the 3d quasilinear wave equation with 7th power defocusing nonlinear term.
\Square u – |u|^6 u= 0.
It is an open problem whether this has "eternal solutions" even in the radial case.
For that matter, why not study the focusing version of this equation
\Square u + |u|^6 u=0
(I may have switched the signs.)
Is there any reason the same method can't take care of both? That would be odd because the focusing one is known to have short time blow up.

Can anyone explain why my doubts are unfounded?

Nets

49. **mathjunkie**
October 7, 2006

I have some doubt about the result that an immortal smooth solution can exist. But, first, I have to admit that I am no expert in turbulence. Navier Stokes equation describes turbulence which is usually chaotic, then it is unlikely that an immortal smooth solution exists.

50. **Navier**
October 7, 2006

I think the paper needs a lot of work to be more readable. And one can check the details. Independently of the validity of the previuos results of the author.
For instance, based on the definition of the prolongation of the Heywood force, the pressure is given as part of the force F. But then in Theorem 2 an assumption of the regularity of F (continuous and bounded) makes the pressure continuous (by assumption). However it is well known that if the pressure is continuous then the solution (the Leray solution which known to be "immortal") is regular.
May be it is a typo and the pressure is not in F, but if it is we are faced with a circular argument.

51. **Euler**
October 7, 2006

It seems that the same proof works for the Euler equations and the Burgers equations!!!!!! If it doesn't, it would be nice to get an insight on the role of the dissipation in this proof.

52. **NS Fan**
October 7, 2006

1. Does the proof of existence of the solution also mean that the analytical solution is also availble or available soon? If Penny's solution turns out to be a valid one, she should be awarded $10Million.
2. Can we construct solutions to some special cases using her method to compare with the known solutions in order to gain more confidence, excitement?

Penny, no matter what, great job!

53. **Brooks Moses**
October 8, 2006

Nets: This particular paper is essentially one of the "we reduce the problem to a previously-solved problem" sorts of papers — where the previous problem is the question of smooth solutions for hyperbolic equations with this sort of term. You'll have to read Penny's other papers (given in the references) to find the things you're looking for. Specifically, this is handled in the "proof" section of Theorem 2, I believe, which references her previous papers Sm2 and Sm4.

MathJunkie: Turbulence, so far has been observed, does not contain any cusps or other non-smoothness (and certainly there's no reason why it would need to!); thus, it's not at all incompatible with an "immortal smooth solution".

Navier: A valid point! However, upon inspecting the equations, it appears that the inclusion of p in F is indeed erroneous; there should not be an undifferentiated p term anywhere in the Navier-Stokes equations, and certainly not in the equation that puts it in.

Euler: It can't work in the Euler or Burger's equations, because both of these have well-known counterexamples involving shock waves. I suspect you'll find the role of viscosity to show up in the referenced things that I pointed Nets at.

NS Fan: 1. No, it regrettably does not mean that. 2. The only parts of Penny's method that construct a solution are the limit-of-hyperbolic-approximations part of the proof. That portion of the method is closely related to existing computational methods which are already quite well-tested. Unfortunately, the rest of the proof doesn't appear to offer any opportunities for such testing.

54. **Mr. T**
October 8, 2006

>Navier Says:
>
>October 7th, 2006 at 8:51 pm
>I think the paper needs a lot of work to be more readable.
>…

I agree with Navier. It is not a good idea to use boldface to emphasize what is important and to emphasize. This is mathematics, not literature.

It seems that she is not familiar with TeX. She should just calm down and do a bit of work to make her paper look fairly neat.

In addition, I would not advise her to argue mathematically technically on the internet. I do not want to tell why.

With regards,

Mr. T

55. **Euler**
October 8, 2006

Brooks Moses: I am sorry to report that the Burgers equation does satisfy the conditions of the hyperbolic systems considered by Prof. Smith. In fact the presence of the dissipation complicates things for NSE and she had to augment them to make NSE fit with her previous theory. My guess is after a day or two, this result will be proven to be incomplete, since Burgers equation to known to be blow up in finite time.

56. **Benni**
October 8, 2006

Penny has withdrawn her paper. It seems that euler is correct.

the dreams are gone…

(sometimes it is good, to talk to more people and hear opinions before submitting anything to arxiv or journals. It can damage reputation to be "another one, who tries to get a clay prize"…..)

Hopefully Penny can correct her paper and make it complete…..

Good luck, Penny!

57. **anon**
October 8, 2006

The paper has been withdrawn, it seems.

58. **Lianhong Gu**
October 8, 2006

Don't give up, keep working, Penny!

59. **D R Lunsford**
October 8, 2006

A friend with long experience in turbulence comments as follows:

"I recently had an interesting discussion with someone who described his own interesting but unsuccessful work on this problem. (He tried to bound the "enstrophy" production – basically dissipation / viscosity).

"He told me that the solutions absolutely must exist, because if there really were some singularities, they would signal the breakdown of the NSE into something Boltzmann-like, anyway some kinetic level description.
But the fact is, there are no known breakdowns in any fluid phenomenon known to be governed by the NSE. In other words: non-existence of solns some sort of singularity breakdown of NSE, which we would
know about.

"Of course, it's good to look for real proofs, but this "argument" more or less settled the issue for me.

"Anyway, it's curious that according to your first link, the paper was withdrawn by the author due to fatal flaws. Also, it was interesting that she also looked at GR. I heard a talk about the GR equations as a hyperbolic system, with a view to existence of solutions, by Arthur Fischer from UC something or other, years and years ago. This seems more interesting to me — existence of
solutions to some system the validity of which we really have no independent knowledge of. An existence proof for NSE would very likely have zero impact on turbulence. But who knows, maybe not. Maybe it would reveal some new property."

-drl

60. **Euler**
October 8, 2006

Every serious mathematician who worked on the Navier-Stokes equation spent at least one night thinking they have the proof of the regularity problem, but they usually sober up after a day or two. Smith's idea to look at NSE as a singular perturbation of a slightly compressible fluid is a well known technique numerically and analytically. However, the slightly compressible NSE is as difficult as the NSE, and may be more. Another problem is the convergence of the solutions to the slightly compressible to the incompressible is not trivial. I hope Smith will take the responsability of a good researcher and clears the status of her preprint with Nature and all the news media who jumped on this.

61. **woit**
October 8, 2006

Polite comments explaining what the problem or problems with Penny Smith's work are are welcome here, rude ones containing no substance aren't.

62. **Chris W.**
October 8, 2006

From an interested bystander:

Leaving aside its apparent failure (pending possible revisions) does this attempt explore significant new territory? Does it suggest ways to approach the problem that haven't been considered before?

63. **penny smith**
October 8, 2006

Dear Euler,
I certainly did.
I loathe the way they wrote articles so quickly.
Arxiv is supposed to be a preprint file—not a journal or a newspaper.
This has hurt me a lot.
I might quit math.
Penny

64. **penny smith**
October 8, 2006

Well, no. I wont quit math.
But, I do feel rather depressed.
Anyway, life is about ups and downs.

65. **Michael**
October 8, 2006

Hey Penny,

please keep your head up. See Lubos Motls comment, who is in the business of being blunt and honest, rather than polite:

"Nevertheless, I think that Prof. Smith has nothing to be ashamed of: serious thinking sometimes requires a trash can."

I think Lubos speaks for many colleagues, certainly including myself.

Surely you were very brave to make this public before a thorough peer review. Oh well — live and learn…

66. **mathjunkie**
October 8, 2006

(sometimes it is good, to talk to more people and hear opinions before submitting anything to arxiv or journals. It can damage reputation to be "another one, who tries to get a clay prize"…..)

Submitting an article to arxiv is fine. The experts in the field can soon point out the problems with the article. The author soon knows what goes wrong with the article or otherwise it is actually a complete proof.

67. **Benni**
October 8, 2006

mathjunkie wrote: Submitting an article to arxiv is fine.

But not such thinks. If you are doing this twice or more times, nobody will take you seriously anymore. Such work is best send to a journal with long referee reports before publishing it at arxiv.org.

68. **Marty Tysanner**
October 8, 2006

I agree with the comments of "Michael" above. I think it was brave of you to put your results out to a wider audience to get feeback, even though it exposed you to potential public embarassment. Fortunately you discovered your error before things got even more public. In any case, you could do a lot worse than to make errors on such a difficult problem!

(To "Michael": It was very refreshing to see a reasonable comment from you. I don't mean that in a condescending way — I am sincere. If you would take such a reasonable tone when you want to debate Peter, leaving behind the taunting and mocking behavior and ad hominem attacks and sticking with well formed arguments, I and probably others would be more interested in what you have to say.)

69. **Michael**
October 8, 2006

Marty,

Penny clearly distinguished herself from the likes of Peter Woit by making an honest attempt at solving a very hard problem. Peter only mocks other people's work and caters to the prejudices of the half-educated. I just do not believe that he deserves the same kind of courtesy — next best thing is to try and expose him for what he really is.

Is it really so difficult to understand this basic difference? What good are well-formed arguments if the addressee is disingenuous? They are about as effective as stickers at the cockpit door "Please do not hijack this aircraft" against Islamic terrorists. Incidentally, for well-formed arguments you might try the arxiv instead of this blog.

Take as an example my recent inquiry about Peter's alleged research activities. Want to make a bet that no research paper of his is forthcoming within a year from now?

70. **Christine Dantas**
October 8, 2006

To Penny Smith:

> We are made to persist. That's how we find out who we are.

— Tobias Wolff

Best wishes,
Christine

71. **Navier**
October 8, 2006

To Penny Smith:

I understand very well you frustration.
The mathematicians who thought they proved the regularity of NSE were depressed. In any case there published papers using previous papers which are known to be wrong. This is science, and you need to continue your work.

I would really like to know what is the error that led you to retract your previous papers?

72. **MathPhys**
October 8, 2006

Isn't true that Boltzmann killed himself because he couldn't solve the Boltzmann equation? Well, that was silly.

I think posting a paper on the archive is just as good, or as bad, as talking to a colleague next door about your work. It's no big deal.

73. **Arun**
October 8, 2006

Penny Smith,
Nothing ventured, nothing gained. Don't give up, please!

74. **surlygrad**
October 8, 2006

so is the earlier work on the comparison principles for hyperbolic equations also void or is it just the NS proof?

75. **Richard**
October 8, 2006

Has anyone else wondered if medals and large monetary prizes in mathematics may have a dark side? I suppose that that they do place mathematics more prominently in the public eye, which is probably a good thing, but recently lawyers were dragged into the picture in another infamous case involving egos and lack of egos, and now Penny has been a victim of premature media hype and heated internet propagation, which probably would not have happened without this "famous" problem having been given such a high monetary profile.

And Penny — the heartbreak of mistakes is part of the territory. Get right back up on that horse!

76. **Daniel Grumiller**
October 8, 2006

Penny, I don't know if this will cheer you up, but it is a well-known story about Werner Heisenberg, who graduated on turbulence and later became one of the co-founders of quantum theory and a famous physicist:

He said he wanted to ask God two questions:
1. Why Relativity?
2. Why turbulence?
He was optimistic to get an answer on 1.

Don't give up. And maybe find a simpler and less depressing problem in the meantime.

77. **CapitalistImperialistPig**
October 8, 2006

I have read (in P. A. Davidsons *Turbulence*) a version of the questions Daniel Grumiller attributed to Heisenberg attributed to Horace Lamb. In that version there were two things he wanted God to explain:

1. Quantum Electrodynamics
2. Turbulence

Since this was dated 1932, you have to say he had a nice insight into at least the order in which God would provide explanations!

78. **Timothy Clemans**
October 8, 2006

One thing to note about the Nature article about Smith's work is that is clearly addressed the issue of how review would be done. Look problems have come up in many manuscripts that looked like they had promise. The important thing is that the serious flaw was found now and not two years from now. The news media is very important in these kinds of issues. They are real developments that need to be covered.

"Has anyone else wondered if medals and large monetary prizes in mathematics may have a dark side?" The Wolfskehl prize for Fermat's Last Theorem generated a lot of manuscripts which overwelmed universities.

79. **D R Lunsford**
October 8, 2006

Dear Penny,

Thank you for taking on this problem. Hang in there.

-r

80. **D R Lunsford**
October 8, 2006

PS let us know what you find out

81. **Marty Tysanner**
October 9, 2006

Michael,

Out of respect for everyone else, I don't want to belabor this point, but I think it is important. You said about Peter,

I just do not believe that he deserves the same kind of courtesy — next best thing is to try and expose him for what he really is.

I see two different issues here. The first is whether or not you should show courtesy to Peter. The second is that you want to try to expose him for what you think he is. Key questions are, who are you trying convince in your exposure, and what is the best way to go about it?

The main problem I see with your past tactics is that you come across as someone who wants to make a lot of noise by heckling Peter rather than someone who has something useful to say. If you were at a talk listening to a well known speaker and somebody in the audience stood up and started shouting and making a lot of noise would that make you sympathetic to that heckler? I would be very surprised if you welcomed it and changed your opinion of the speaker's point of view because of the outburst. Why should it be any different here? If you want to show anyone how wrong Peter is, you need to use reasoned arguments and counterexamples rather than just trying to "put him in his place." For one thing, whether or not Peter is right about the role string theory should play in particle physics is quite orthogonal in most people's minds to whether or not he has an alternative program of his own. (In fact, if he did have his own program, others would probably accuse him of trashing string theory to make his own ideas look better — the "hidden agenda.") For another, evaluating the merit of a scientific program or idea is meaningful in its own right — grant administrators need to do it, and so does anyone who referees a paper. That's the way I see it, anyway, and that's a key part of why I don't think your tactics of heckling, ad hominem attacks, and ridicule of his scientific activities will resonate much with most impartial observers.

The other question is, who you are trying to sway with your tactics? If you are trying to use rude comments and interruptions as a way of gaining the praise of other people who already agree with you, then perhaps your tactics are appropriate. But catering to people who already agree with you seems like a really pointless thing to do. Displaying bad manners and disrespect while identifying yourself with string theorists could easily look to everyone else like a reflection of the attitude of some string theorists. In the eyes of the less educated public and other decision makers who ultimately control funding, "some string theorists" could easily become "all string theorists," and their general perception of string theorists could spill over into attitudes about the viability of string theory in general. Again, that's the way I see it. Of course, the fraction of the public and decision makers who frequent this blog is probably not substantial, but you are already aware that this blog is a magnet for some journalists who can interpret what is going on here, and thereby disseminate their preceptions to a much wider audience. What kind of perception of string theorists do you personally want to help create in the minds of others?

So it seems to me that the real audience you should be thinking about is the people who aren't likely to ever be string theorists: scientists other than physicists, physicists who aren't string theorists, opinion makers, and the general public who frequent this and other blogs out of interest in science or just a chance to watch a little controversy. There are a lot more of those people than there are theorists in fundamental physics, and they ultimately are a lot more important to the future of string theory funding. And those people aren't going to be the least bit impressed by tactics like the ones you have favored in the past.

82. **TheGraduate**
October 9, 2006

Navier:

The information concerning the nature of the error can be found on her website. Perhaps it would be best to look there …

83. **mathjunkie**
October 9, 2006

A side issue.

Penny said she submitted the article to Journal of Mathematical Analysis and Applications (JMAA).

Is JMAA's rejection rate high? Is it a prestigious journal in pde? I just wanted to submit an article on pde.

84. **Sacha**
October 9, 2006

I think possibly every maths person has had moments where they think they've solved some hard problem, and then later found an error in some assumption or something with the effect that their proof instantly collapsed. Painful, yes, but helpful, as it means that that path was not the right one to take! And in trying, you learn a huge amount.

Keep up the research Penny.

85. **a**
October 9, 2006

By the way, why arXiv does not have a trackback to this post, that contains very informative comments?

86. **penny smith**
October 9, 2006

As to earlier work on hyperbolic comparson principles:
With a necessary (based on counter examples) additional condition,
I can still prove these results for short time depending on the initial data.
I am going to submit a paper on that: fixing my JMAA paper to a journal where it will get a decent referee job. In math that can take a while.
I have written it in manuscript form already.

This means the Einstein Result will also be true for short time
depending on the initial conditions.

87. **penny smith**
October 9, 2006

The necessary condition is on the sub and supersolutions and only such sub and supersolutions will have a comparison principle.
That's true for systems.

For the semilinear wave equation, no extra condition is needed, but there my comparison principle was proved for short time only.

88. **penny smith**
October 9, 2006

This means that my Perron method for hyperbolic systems also works for short time depending on the initial data. And, that is the paper that I am rewriting for submission to a journal.
Step one: Write and send that paper, with the corrected comparison
principle included.

89. **penny smith**
October 9, 2006

As to JMAA. It is a great and prestigous journal. It has a high rejection rate.
Nevertheless, they (and I) screwed up on that paper. So did the reviewer for Math Reviews.
This sort of thing happens. It is my bad luck. If the error had been
noticed by a referee, I would have fixed it in a few days and resubmitted and that would have been that.
Now, I look terrible and feel worse.

90. **penny smith**
October 9, 2006

The support here is helping me heal though!

91. **woit**
October 9, 2006

a,

The arXiv policy, set by its moderator Jacques Distler, is to censor any and all trackbacks to this blog, whether they're about math or physics. I tried for a while to do something about this, finally gave up since it wasn't worth the time or energy.

92. **D R Lunsford**
October 9, 2006

Penny,

How did you become interested in the NSE in particular? If you want to discuss offline I am at antimatter33 at yahoo dot com.

-drl

93. **Michael**
October 9, 2006

Marty,

thanks for your effort of explaining. I disagree with your fundamental assumptions, and therefore reach very different conclusions.

>> would that make you sympathetic to that heckler?

Are you able to understand the difference between academic discourse and deception for personal gain?

>> If you want to show anyone how wrong Peter is

I don't! Some very accomplished people think that string theory is a waste of time and effort. Plurality of opinion is generally good. What I despise is the way Peter makes no scientific contribution of his own while exploiting his populist criticism for attention and personal gain.

>> What kind of perception of string theorists do you personally want to help create in the minds of others?

Do I have to care? Is string theory a public relations affair? The moment this determines more than the level of background noise I have to endure is the moment I quit.

>> The other question is, who you are trying to sway with your tactics

Nobody, of course. You are free to believe in a democratic approach to science, I don't. I have seen that some people dislike my comments here and stick up for Peter as a result. (You are probably in that group.) Maybe it makes you a good buddy, but there would be less positive things to say about it.

94. **Andreas**
October 9, 2006

Oh boy, this blog documents is a real tragedy.

95. **jeremy**
October 9, 2006

Michael,

If you want to sneer at Peter, he has given you plenty of opportunities. But you as everybody else here know that this is not the place. Can you please show some respect for the decencies?

96. **John Baez**
October 9, 2006

Don't worry, Penny, no reasonable being will think ill of you for making this mistake. On the contrary, they should be impressed at your ability to accept it and move on.

97. **Chris Oakley**
October 9, 2006

Good luck, Penny.

A whole lot of annoying aphorisms spring to mind, like the fact that a theoretician's best instrument is his/her waste paper basket, and that nothing of value comes easily.

Don't give up!

Someone expressed reservation earlier about the whole concept of a Millenium Prize, and I agree with these reservations. A lot of problems remain unsolved because they are – with existing techniques, anyway – insoluble. Encouraging people, using large cash prizes, to continue to bang their heads against brick walls when they might otherwise be going off in orthogonal, and perhaps more productive directions may not be such a good idea.

98. **John Baez**
October 9, 2006

On a wholly other note, I wish Michael would spend a few days pondering Marty Tsysanner's wise and well-meaning advice. I'm sort of amazed at the careful thought Marty put into writing that.

99. **a**
October 9, 2006

I agree with Baez (2 posts above): in my view scientists that admit their mistakes are more trustable than ones that don't. Unfortunately withdrawing wrong papers is not the common-practice among us physicist.

100. **Editor**
October 9, 2006

Why is P. Smith publishing all of her results in JMAA.
I just noticed that P. Smith is on the editorial board of JMAA.
Does that help get a paper accepted without a thorough review?
I do have serious issues with this.

101. **penny smith**
October 9, 2006

Maybe, they trusted me too much because of that.
Not my doing.
I have plenty of papers not published at JMAA
Penny

102. **dave tweed**
October 9, 2006

@Editor:
I don't know any of the facts in this case. However, in general people are on the boards of journals in subject areas they understand well (even though they may not be detailled reviewers themselves any more). The areas they understand are likely to be the areas they are working in, so it's quite likely that they'll be submitting to those journals.

As to submitting to journals with less thorough reviews, I don't know of anyone who believes the work they do isn't actually correct, so thoroughness isn't a reason for choosing where to submit. (There are more "debatable" reasons guiding submission choices, such as not submitting to journals that are so popular that they can afford to reject papers that are merely "good" and not "exceptional". But that's different from hoping incorrect work will be accepted.)

I think the saddest thing about this is as an instance of the "I was there" phenomenon: we want to have been there when something amazing happened, so we force the interpretation that something groundbreaking _has definitely_ happened rather than leaving it to experts to figure out "at a natural pace". Given the history of things like Kempe's flawed proof of the four colour theorem, I'm not surprised that it takes a while for erroneous steps to be spotted; however Kempe chains are (so I'm told) an important element used in the correct proof of the four colour theorem.
So lets let things "cook" for a few months and then make a considered judgement of things.

103. **penny smith**
October 9, 2006

Dear John,
Well, first thing is to fix any wrong results, and get that published.
Also, I have other papers to write.
Thanks for the supportive comment.
All that really matters is getting the correct results in print, because that is SCIENCE.

104. **penny smith**
October 9, 2006

It's going to take more than a few months, because most math papers are reviewed VERY slowly.
I wish JMAA had been much slower.

105. **Marty Tysanner**
October 9, 2006

(Once more, I apologize to others for my part in keeping this subthread alive. I expect this will be the last time.)

Michael,

You said that

What I despise is the way Peter makes no scientific contribution of his own while exploiting his populist criticism for attention and personal gain.

Clearly you are putting yourself in the position of being a judge of Peter and his character. Who made you a judge of others? What makes you so certain of your infallible judgement of others and their motivations that you can make such pronouncements without any doubt? Clearly there are many others here who don't share your conclusion about Peter's motivations by the fact that they are participating in various discussions under his auspices. Even Penny, whose efforts you praised, is apparently a regular visitor, and also someone who felt that the quality of the audience here was sufficient to discuss her results. Do you think she and others would have done this if they thought Peter was doing all of this just for his own personal gain? Yet you seem to place yourself above others here in your belief that Peter is really just a fraud, and furthermore that your True Judgement empowers you to use disruptive tactics and otherwise reflect badly on string theorists in general. Your behavior may negatively affect a lot of people, but Peter is only one of them.

You said,
Nobody, of course.
in response to my question about who you were trying to sway with your tactics. If this is true, then your entire participation here is completely pointless and a waste of your time. But I don't believe that you don't care about swaying anyone, because in your first response to me you said,

I just do not believe that he deserves the same kind of courtesy — next best thing is to try and expose him for what he really is.

If you want to expose him, then you clearly want to expose him to others, to convice them that Peter is the fraud that you think he is. At least that's the way I read what you wrote. So I think you still need to think about who you are trying to convince, and what is the best way to go about it. I have already explained why I think your past tactics are a completely counterproductive approach, so I guess I really don't have anything else to say on the subject that is any more likely to reach you.

106. **Navier**
October 9, 2006

Dr Smith,

Please tell us what is error in your previous publication!!

107. **Michael**
October 9, 2006

>> Clearly you are putting yourself in the position of being a judge of Peter and his character. Who made you a judge of others?

I did. What do you want? You criticize me — what's your entitlement?

>> What makes you so certain of your infallible judgement of others and their motivations that you can make such pronouncements without any doubt?

You are getting weirder by the minute. I'm not infallible. Just voicing an opinion and acting on my personal convictions.

>> your entire participation here is completely pointless and a waste of your time

No. It makes Peter's unethical behavior less rewarding. I find it quite worthwhile…

>> But I don't believe that you don't care about swaying anyone, because […]

Swaying people's opinions in a scientific matter is just completely different from exposing a crackpot. I find it hard to take you seriously if I have to explain this to you.

>> I have already explained why I think your past tactics are a completely counterproductive

Yes, in essence it's because you believe in persuasion and democratic voting on scientific matters. And I don't.

108. **jeremy**
October 9, 2006

Michael,

OK, but why do you have to be a jerk?

109. **a**
October 9, 2006

Peter, this off-topic discussion is annoying. If you do not want to censor Michael, what about creating a separate posting where people can insult you without disturbing other readers?

110. **Peter Woit**
October 9, 2006

a,

Sorry about this, presumably “Michael” will get tired of this soon, and in the meantime he’s an excellent example of the reaction of the string theory community to the appearance of my book. The unwillingness to deal with what I have to say there, coupled with personal viciousness and cowardice, are quite telling. It seems that “Michael” divides his time between UMass in Amherst and the HEP group at Brandeis. Someone familiar with one of these places might want to tell us who he is. I strongly suspect that if he had to sign his name to what he writes, he’d stop doing it.

111. **TruthSeeker**
October 9, 2006

I don’t understand what is the rational reason for putting other people down because they’re not making good progress in research or hit a snag when solving a very difficult millennium problem. Such actions do not enlarge a person, further their own career, or enlighten them in any shape or form. So why do it? To get a cheap kick out of insulting others that lasts maybe 5 seconds? And then you’d have to pile on more insults to get the same “high” the next time; this is addictive. Nip it in the bud, Michael.

112. **Ray L**
October 9, 2006

Wow! Not hard to find every one of the seven deadly sins on this blog. Who says math is boring?

113. **mathjunkie**
October 9, 2006

From my experience, making mistakes is almost inavoidable in research. Don’t give up, Penny!

114. **Michael**
October 9, 2006

Dear TruthSeeker,

why don’t you try reading this:

http://www.math.columbia.edu/~woit/wordpress/?p=470#comment-17132

You might just advance to OccasionalTruthFinder.

115. **TruthSeeker**
October 9, 2006

Michael,

You must be talking about the first sentence of my previous post. Well, the first part of it refers to people such as yourself, and the second part of it refers to people who posted rude things that subsequently prompted the Oct 8th comment above by Peter. (That rude person has apparently stopped posting here.) I hope that clears up your misunderstanding.

116. **King Ray**
October 9, 2006

Penny, never ever quit. If you never quit, you’ll be astounded at what you can accomplish.

117. **Richard**
October 9, 2006

Geez, this thread was begun to discuss the NSEs and Penny’s heroic attempt at it, and then this boring and boorish “michael” intrudes once again with his own agenda. Sorry Peter, but I think that “michael” behaves like a pathological crank caller, and he will never grow tired of this.

118. **Daniel**
October 10, 2006

Dear Penny,

Thank you for telling us the truth. I have a question
and a suggestion:
1. Where did you get wrong? How did you find it out?
2. You possibly need someone to help you to keep the mistakes (I mean the serious ones) from you.

Best Wishes

119. **Timothy Clemans**
October 10, 2006

According to http://comet.lehman.cuny.edu/sormani/others/SmithNavierStokes.html and http://comet.lehman.cuny.edu/sormani/others/smith.html an anonymous mathematician found an error and reported it to her in one of her published papers which her preprints “Immortal Smooth Solution of the Three Space Dimensional Navier-Stokes System” and “Eternal Continuous Viscosity Solutions of the Einstein Cauchy Problem” relie on. Based on the following Smith said in this thread, “It’s going to take more than a few months, because most math papers are reviewed VERY slowly. I wish JMAA had been much slower.”, I believe that the paper with the flaw is one that was published in the Journal of Mathematical Analysis and Applications.

Your seggestion does not make sense to me, because I do not think that mathematician such as Smith would benfit from not having the details on any flaws in their papers.

120. **desA**
October 10, 2006

Prof. Penny,

I salute you & your brave efforts. You will come back to fight another battle. I wish you well.

I am currently researching the N-S from a physics perspective & have observed a slow-fast phenomenon which allows a suitable decomposition of the system into esentially 3 equation groups (eg. Reynolds decomposition).

How would your proposed solution affect such decomposition concepts? This is one of the typical approaches used in turbulent model development – bulk + fluctuating + closure.

Thank you & best of success…

desA

121. **anonymous expert**
October 10, 2006

"Heroic attempt."

Isn't this a bit of an overstatement?

I don't blame Penny for trying unsuccessfully to obtain global solutions to NSE and other nonlinear equations. I don't even blame her for deluding herself into thinking she had done it.

But the sycophancy of this thread is starting to grate. The reason anonymous experts said early on that the proof did not look plausible without knowing the exact line in which the error was is that the proof did not seem to be based on any propert of NSE or overcoming any of the known difficulties.

Everyday, there are experts with a deep knowledge of NSE looking for ways of making incremental progress against those difficulties. The reason they aren't making bold announcements isn't that they are further away from a solution than Penny Smith. It is that they better understand the field. Let us not forget their brave attempts while commending the attempt by Smith.

AE

122. **desA**
October 10, 2006

"anonymous expert", you are very cruel & unkind.

desA

123. **MathPhys**
October 10, 2006

No, I don't think AE is "very cruel and unkind". I think he's honest and fair. After all, this is science.

124. **Chris Oakley**
October 10, 2006

> The support here is helping me heal through!

Good … but think how much people would have hated you if you had been right.

125. **Testy**
October 10, 2006

I was wondering if Navier Stokes had any relationship to Quantum Gravity?

126. **Andreas**
October 10, 2006

Testy:

Navier Stokes (NS) describes momentum conservation in the classical limit of a quantum field theory (the diffusion coefficient in NS is a remainder of this quantum heritage); and quantum field theory is likely to be a low energy limit of quantum gravity. voila!

127. **Andreas**
October 10, 2006

…and because of this connection it wouldn't suprise me if turbulence itself is a phenomenon grounded in the quantum realm.

128. **Deane**
October 10, 2006

To do good research, you must have the utmost respect for those who have also tried to atack the same questions. And you must also have the utmost respect for those who will study at your work.

While you're doing your research, you don't have to know what others have done or what ideas they have tried. Usually, it's better to know, but sometimes it's good not to.

But the respect for others is critical after you think you have done something. At that point, you have to ask yourself, "why have I succeeded when others have failed", and it is absolutely critical to educate yourself and answer this question carefully and honestly before you announce anything to the world.

Testing your work against the simplest examples (as described so well by Feynman and suggested in the comments above) is the first thing every serious mathematician does, and it demolishes 99% of our efforts.

And if that test passes, it is only the beginning. There is an illusion that the responsibility for checking a proof lies in the referee of your paper. But that's just not true; very seldom do referees have the time to check a proof in detail. The ultimate responsibility lies in the author of the paper. So before you submit a paper to a journal or even arxiv.org, you have to sweat blood to find every error in your paper and correct it.

And after you've done all you can, you have to enlist as much help as you can manage to do another round of checking, because it is very difficult to find all the errors by yourself. But the help has to come from experts in the field, so posting your work on arxiv or putting up a web site is *not* the right way to do this.

And you do this for papers that don't come within light-years of proving a Millennium Problem.

Doing anything less than this wastes the extremely valuable time of those who will study your paper.

I consider both Penny and Christine friends, but I have to say that I think they both got a little too excited by the signficance of the problem and strayed from the fundamental rules of doing research just when they needed them the most. Normally, the consequences of this error are small, but, coming right after Perelman's proof, everything got blown out of proportion. I wish Penny the best and hope that everyone involved will proceed with a little more caution and level-headedness in the future.

129. **Aaron F.**
October 10, 2006

Prof. Smith — Just wanted to put in my hopes that this will get cleared up. I've never been very interested in fluid dynamics or differential equations, but the news about your paper was, and still is, very exciting for me. I'm sure something good will come of this whether or not Navier-Stokes is solved, but in the meantime, I'm hoping for the best. I know we can all think of at least one famous result that had to be retracted and repaired before it was accepted. 😉 Good luck!

130. **ksh95**
October 10, 2006

Dear Michael,

A good way to determine if some one is speaking nonsense is to take n -> infinity. If, after we take an argument to the extreme, we find nonsense. We have to conclude that, in the absense of some critical point, the whole argument is nonsense.

****
A pogo-stick theorist decides that the best way for a human to visit mars is to hold ones breath, get on a pogo-stick, and jump as high as one can.

The naysayer says, "*that's crap, human lung capacity is to small and it's impossible to generate enough energy to bounce all the way to mars on a pogo-stick. Everybody look at me and give me attention. I want to be famous.*

The pogo-stick theorist replys, "*screw you, you don't have a better idea and you are clearly seeking fame. Therefore, you can't criticize my idea*"………

I leave as an exercise for the reader to find the nonsense.

131. **ksh95**
October 10, 2006

BTW, Peter's opinion is clearly an opinion. I could be correct or incorrect, but it is not illogical.

132. **Anonymous**
October 10, 2006

one year ago Peter posted about a big theoretical claim in cosmology: No Cosmological Constant?. In a comment Sean told that the claim seemed suspicious. Indeed, it turned out to be wrong for that reason. The wrong paper still is on arXiv.

Deane: I see something to be blamed here, not in the paper that Penny has promptly widthdrawn.

133. **One Comment**
October 10, 2006

At least one Fields medalist first announced a false solution to the conjecture he later became famous for solving, then a serious flaw was found, and a year or so later he solved the problem in spectacular and career-making fashion. The jury is still out on Prof. Smith's work and failing to solve a big problem is no big deal.

See also Alain Connes remarks in one of his papers on Riemann Hypothesis. As a young student, he got advice that by publically attempting a famous problem one risks embarrassment. Connes then writes, that as he got older, he realized that the opposite approach, avoiding the embarrassment by not trying the hard problems, was equally self-defeating.

134. **SteveM**
October 10, 2006

The Navier-Stokes equations won't give up their secrets without a fight, but people putting a lot of hard work into honestly trying to solve these very diffciult problems deserve a round of applause even if the work is found to be flawed. However, important new ideas often emerge in an incomplete or flawed form and, like anything worthwhile with big payoffs, it takes a lot of effort and pain to get there. The millenium problems also attract a lot of attention and a lot of media attention though and this can be detrimental. But I hope Penny gets back to work on the problem with renewed determination.

My own encounter with Navier-Stokes was in the 90s when I began a Phd on "Turbulence in Blood", which was to be a detailed piece of applied math/mathematical biology. Unlike the "simple" problem of turbulence in water, the density and viscosity are variable and determined by the fractional violume of red cells in suspension– which are essentially deformable bi-concave discoids– and by the blood proteins and clotting factors/thrombin and their chemistry, or what state they are in. Blood tends to be remarkably laminar until it encounters an inhomogeneity like a clot and so on. I gave up after about 3-4 weeks and was allowed to switch to something infinitely more tractable!! (Maybe it could be simulated on a supercomputer, or has been). My knowledge of hydrodynamics is mostly in statistical hydrodynamics so I have interpreted turbulence as a stochastic process or stochastic flow with dissipation. (Does anyone know how this or the well-known Kolmogorov 5/3 law fits into this millenium problem?). Anyway Navier-Stokes=damn hard. I would like to learn more though about what people like Penny are doing in this field. More knowledge of the N-S equations would be useful in applied science, engineering and biology.

Someone said: "does anyone know how Navier-Stokes are related to quantum gravity". Both the Navier Stokes and Einstein equations could be interpreted as nonlinear, large-scale continuum approximations to deeper underlying miscrocopic descriptions. The N-S equations are continuum approximations describing the dynamics of a fluid which is really a statistical mechanical ensemble at shorter distances. The Knudsen number for example is K=L_{m}/L, where L_{m}=molecular mean free path and L=distance scale of interest. When L_{m} > L_{p}. This is very much like the Knudsen number of a fluid. The idea is that quantum gravity (maybe string theory) will take over before things get too "turbulent" as we approach the Planck scale or strong coupling regime. (I believe Briane Green had some computer graphics demonstrated something like this in his nova series). There is a lot of talk of "emergent spacetime" and "emergent gravity" bandied about but the comparison with the N-S equations could be useful here since they are clearly "emergent" too. In this sense I have never thought it made any sense to take the nonlinear Einstein equations as a starting point (as in LQG) and "quantize" them–the Einstein equations/GR should arise naturally in quantum gravity as a large-scale or continuum limit of the theory with no apriori assumptions of what the classical gravitational theory is like. At any rate, both the N-S and Einstein equations are among the most fascinating and difficult in mathematical physics, with the fun coming from the nonlinearity and the possibilities of blowups and singularities.

135. **Deane**
October 10, 2006

I am certainly not advising people not to try to attack the big unsolved problems. I think everyone should! It's a lot more fun than nibbling around the edges of a subject. What I am advising is that before you announce a solution, make sure you know where the critical issues are and understand why your approach deals with them properly (what is it that you know that no one else did?).

Penny's effort (a proof of the Navier-Stokes problem with a one month effort without ever having attacked the problem seriously before) cannot be seriously compared to Andrew Wiles' (a seven year effort, which probably included at least 3 man-years of checking his own work and which followed many more years of work torwards his proof).

To believe so easily that the Navier-Stokes equation can be solved with such little effort is, frankly, a huge insult to many top mathematicians who have sweated blood to prove much less, including but not limited to Fields Medalists such as Jean Bourgain and Terence Tao.

Unfortunately, there is a prevalent misconception that doing mathematics involves thinking up lots of ideas until the right one pops into one's head. The top mathematicians are certainly usually brilliant, but they also devote day and night to uncountably many failed calculations and proofs before they stumble onto anything worthwhile. I would even go as far to say that many if not all top mathematicians are at the top not because they are smarter than everyone else but they are fearless and relentless in their work. In other words, they succeed not because they know a good idea when they see it but because they have simply tried to work out the details of a hundred times more different calculations and proofs than anyone else.

Neither Penny nor Christine should have dismissed Penny's proof out of hand; it was certainl possible that the right idea had somehow popped into Penny's head. But the relative ease with which she obtained the "proof" should have been a clear signal for proceeding with extreme caution and skepticism. It should also have been a clear signal to everyone reading about their announcements for extreme caution and skepticism (it certainly was for me).

Caution and skepticism are not the same thing as dismissal. You can be extremely cautious and skeptical and yet hopeful at the same time. But those of us who have made serious attempts to attack difficult questions in mathematics know all too well how often our brilliant and ingenious solutions turn out to be mud. But sometimes it turns out to be gold, and the feeling you get when it does is like nothing else in the world.

136. **geometer**
October 10, 2006

I second what Deane has said. In fact a few days ago I showed Penny's proof to our local NS-expert and within 5 minutes he found the same difficulty with the proof that was later pointed out in this blog by Euler. Here is the rule I follow: before making any significant work public, I show the stuff a trusted expert, and if things go wrong, it just passes as a silly question…

137. **SteveM**
October 10, 2006

Part of my post (3rd paragraph) seems to have gotton mangled/lost when I pressed submit.

138. **SteveM**
October 10, 2006

Here it is again in case anyone is remotely interested:)

Someone said: "does anyone know how Navier-Stokes are related to quantum gravity". Both the Navier Stokes and Einstein equations could be interpreted as nonlinear, large-scale continuum approximations to deeper underlying microscopic descriptions. The N-S equations are continuum approximations describing the dynamics of a fluid, which is really a statistical mechanical ensemble at shorter distances. The Knudsen number for example is K=Lm/L, where Lm=molecular mean free path and L=distance scale of interest. When Lm is much less than L then the continuum approximation provided by the N-S equations holds, and so these equations (and similarly the equations of continuum mechanics) can be studied without worrying about the underlying molecular physics. When L is of the order of Lm, or L is less than Lm then you are in the realm of statistical mechanics.

Similary, the nonlinear hyperbolic Einstein vacuum equations ought to be a continuum or infrarad limit or consequence of a deeper microscopic description of spacetime structure. Of course, the Einstein-Hilbert action, coupled to additional fields, emerges as the long wavelength or low-energy effective field theory limit of string theory. (Regardless of what people here might think of string theory this remains a very interesting result). Also, the old idea of spacetime foam, which is a kind of "turbulence" occurs when Lp/ L is of the order of unity, where Lp is the Planck scale, and classical general relativity is recovered when L is much greater Lp. This is somewhat like the Knudsen number of a fluid. The idea is that a quantum gravity (maybe string theory) should take over before things get too "turbulent" as we approach the Planck scale or strong coupling regime. (I believe Briane Green had some computer graphics demonstrated something like this in his nova series).

There is a lot of talk of "emergent spacetime" and "emergent gravity" bandied about but the comparison with the N-S equations could be useful here since they are clearly "emergent" too. In this sense I have never thought it made any sense to take the nonlinear Einstein equations as a starting point (as in LQG) and "quantize" them–the Einstein equations/GR should arise naturally in quantum gravity as a large-scale or continuum limit of the theory with no apriori assumptions of what the classical gravitational theory is like. At any rate, both the N-S and Einstein equations are among the most fascinating and difficult in mathematical physics, with the fun coming from the nonlinearity and the possibilities of blowups and singularities.

139. **Christine**
October 10, 2006

If I understand this correctly, it seems that my name is being cited here because of my words of support to Penny Smith. I would like to mention that these words of support do not mean at all that I agree with her submition procedure to the arXiv. In fact, one can clearly notice from the records that I only post to the arXiv after my paper is accepted by a refereed journal. And it is clear that the responsability of the results published is of the authors. That is my procedure, and one can also verify that I am the first (or only) author in all my papers so far, so I am quite aware of the responsability envolved.

I do not wish to judge Penny Smith or anyone else's scientific posture here. But as a scientist colleague, I understand her genuine efforts, and having recognised a mistake in a moment when all attention was over her was a very difficult situation. That is why I have given her my words of support and they stand.

Best wishes,
Christine

140. **Christine**
October 10, 2006

Deane Says:
I consider both Penny and Christine friends, but I have to say that I think they both got a little too excited by the signficance of the problem and strayed from the fundamental rules of doing research just when they needed them the most.

As I read this again, it continues to be very strange. I have nothing to do with the research by Penny Smith. How can someone judge that I have "strayed from the fundamental rules of doing research"? Based on my words of support to Penny Smith?? What does it mean to consider "friends"??

Christine

141. **Deane**
October 10, 2006

Christine,

I don't claim to have judged the situation correctly, but it does appear to me that you went to considerable effort to promote Penny's work without the caution and skepticism that I think should have been warranted under the circumstances. I think your efforts to help Penny were well-intentioned but misguided.

Regards,
Deane

142. **Christine**
October 10, 2006

OK, after reading again Woit's initial post, I can only conclude that Deane confused Christina Sormani with me, Christine Dantas. Well, at least I hope so.

Christine

143. **Deane**
October 10, 2006

Christine ,

Oops! I do mean Christina Sormani and not you! I should check names as carefully as I check my proofs! I am very sorry!

Deane

144. **Christine**
October 10, 2006

Dear Deane,

That is all right.

Christine

145. **ttt**
October 11, 2006

I was wondering if Penny was interested in writing a textbook?
You seemed to have garnered alot of genuine interest in your work!

I would be interest in buying that book!

146. **csrster**
October 11, 2006

SteveM re: Kolomogorov: I would think the answer is "not at all" in the sense that the challenge is to prove that solutions to NSE remain finite precisely on the smallest, dissipative, scales where Kolmogorov breaks down.

Of course we computational fluid dynamicists all know the result, but we'd still like to see a proof 🙂

147. **oub**
October 11, 2006

Hello,

The paper is withdrawn, but can somebody point out to me the
regularity of the solutions, as stated in her original work?
Are they supposed to be classical solutions?

Thanks

148. **MathPhys**
October 11, 2006

I just looked up Sormani's web site, and there is a statement there that reads

"Regretably the journal referee did not catch the error or Smith would have corrected it before publication, either restating the result or rewriting the theorem.

If the restatement wasn't strong enough to obtain the immortal smooth solution to the Navier Stokes, she never would have made a claim to have found one.

It is possible it was a very subtle error that could only be picked up due to the combined efforts of all the mathematicians who looked at Smith's work".

So it's the fault of the referee and/or all the mathematicians who read the paper. I didn't know that!!!

149. **MathPhys**
October 11, 2006

Seriously, Smith had an idea, and it turned out to be wrong. No big deal, we all make mistake. Just don't blame the poor referee.

150. **penny smith**
October 11, 2006

It was indeed a VERY subtle error. And, I can mostly fix the paper
(except for Navier StoKES) if people would stop beating me up for makeing a subtle error. It is not as if I have been dishonest.

Moreover to answer some people nasty comments. I have been working on nonlinear PDE my whole 30 year working career–and have scored big sucesses. My first paper solved a major problem in compressible gas dynamics including three dimensions. My second Paper gave a smooth family of transonic flows in a plane exterior problem disproving
a famous and incorrect theorem.
I didn't get to Institute for Advanced Study by being a moron,
even if today I do feel like a moron.

Second, not everyone has experts on tap to check their work on NS.
I submitted to a preprint file, and was glad that an error was found.

I am not dishonest. I am not arrogant. I do not seek publicity.
I just try to do math the best I can, and have endured considerable sacrifices to do that.

I have some big successes. I now have some failures. Please don't beat the shit out of me. Ok?

Moreover, working on hard problems makes errors more likely.
Newton, Gauss, Poincare, Einstein, Hilbert all have published false
theorems. Were they morons?
It's hard enough as it is.

151. **MathPhys**
October 11, 2006

Penny,

No one says you were dishonest or arrogant. But when Christina Sormani implies that it's basically the referee's fault, I find that that's unfair on her part.

152. **jeremy**
October 11, 2006

I don't know how many of us have gone through all Penny's relevant papers, but suddenly some of us decided to give advices to Penny on how to conduct her research. I don't know if that is necessary. Every researcher has his/her own style of doing research, hardly anyone can claim one is better than the other. Remember Perelman, if he has waited for the expert's opinion, he would never have waited who knows how many more years to post his papers on arXiv.

Penny made a mistake in her research. We all make mistakes in our researches, mistakes that sometimes were also pointed out by others. If she decides to admit the mistake, which she did, and move on, I believe that all everybody else needs to do is to show respect.

153. **Deane**
October 11, 2006

I agree that Penny made an honest mistake and should be given a break here.

But I'd also appreciate a little more appreciation of how difficult it is to prove these big theorems. And, again, difficult in the sense of grueling labor over a long period of time and not in the sense of being brilliant.

For instance, someone has now cited Perelman as someone who doesn't do what I described. Well, besides the fact that Perelman is a pretty unique singularity that no one should try to emulate, Perelman did in fact do at least some of what I advised. He worked on the Poincare conjecture for approximately seven years. Again, I do not know but am confident that nearly half that time was devoted to checking his work. (why do I think that? Because whenever I've proved a new theorem, by the time I'm finished, 90% of my time was spent on checking my work. I'm assuming Perelman is much better at it than I am). Finally, he *did* send emails to other experts consulting them and asking them questions. I concede that he didn't actually show them his proof, but he did try to verify critical points in his proof with experts. Hamilton did not respond to Perelman, but others did.

No one should have the illusion that Perelman wrote those short sketchy papers without spending years (yes, years!) verifying the details that underly those papers. I am willing to bet that thousands of pages of calculations and proofs were written down by Perelman before he wrote those papers. And that's not counting all the calculations and proofs that didn't work out.

I think we mathematicians have been all too successful in hiding the fact that doing mathematics is 99.9% grueling labor and approximately 0.1% brilliant ideas (I hope I got the math right).

154. **penny smith**
October 11, 2006

Guess what people.
Now that my head has cleared.
The JMAA paper on PERRON is CORRECT!
The counterexamples were for nondiagonal systems, and that paper was for diagonal systems.

I was worried about I theorem of Ladyzenskya that I used, but it wasn't even necessary.

Several experts wrote saying they couldn't find an error in that paper.

I OWE THE REFEREE AND THE JOURNAL A DEEP AND ABJECT APOLOGY!!!!!!!!

IF HE/SHE IS READING THIS, ALL THAT I CAN IS THAT I WAS TERRIBLY DEPRESSED AND HURT AND I OWE THEM AN APOLOGY
AND ANY FAVOR THAT THEY WANT.

The error was in the use of unpublished theorem 4 of the Einstein Paper. That theorem had a very subtle error in the infinite time
comparison. I have extended it to show that the time of comparison depends (for the experts) on the C^{1} norms of L(
sub super solution).

155. **Christina Sormani**
October 11, 2006

I wrote:

"Regretably the journal referee did not catch the error or Smith would have corrected it before publication, either restating the result or rewriting the theorem. If the restatement wasn't strong enough to obtain the immortal smooth solution to the Navier Stokes, she never would have made a claim to have found one. It is possible it was a very subtle error that could only be picked up due to the combined efforts of all the mathematicians who looked at Smith's work."

I do not see this as blaming the referee.

On another note: the writer of the poem above doesn't even know undergraduate ordinary differential equations if he doesn't understand how one takes a second order equation and turns it into a first order system.

156. **penny smith**
October 11, 2006

Deane, I also spend thousands of hours doing calculations, just like you and just like everyone who does math research. That "month" alluded to by Nature was based on a misquote by me. I said:"
I have spent years (in fact about ten years) on these hyperbolic
papers and about a month on NS" –(most of which was checking NS, by the way!).
I posted on A PREPRINT FILE to get possible priority. I didn't claim a
theorem that was unpublished.

What exactly is your beef with me?

157. **penny smith**
October 11, 2006

And since when, DEANE, did I claim to be brillant?
What is your need to beat me up?
Penny

158. **woit**
October 11, 2006

Please, everyone. Stick to discussing math here, not who to blame about what. I can't even figure out how to moderate this if people start doing things like writing verse that may or may not contain personal criticisms. Enough already.

159. **Deane**
October 11, 2006

My only point is that it is highly unlikely that anyone can come up with a solution for the Navier-Stokes problem, write it up, and check it properly in less than a year, never mind a month. My guess is that I couldn't do it if the proof were dictated to me.

I'm willing to forgive you for your haste, since, as you say, you wanted priority and probably got a little overexcited. But I am dismayed by how many people without seeing your paper appeared to be so willing to believe that this was a reasonable scenario. One of your friends or colleagues should have grabbed you and said, "Penny, wait! Let's go slow and careful on this."

I was not trying to beat you up. I was trying to beat everybody else up. I'm sorry that my aim was so poor.

160. **Daniel Grumiller**
October 11, 2006

Penny: Honestly, I don't think Deane is beating you up. We all love you for your attempt (there you go – consider how many people would have hated you if you had succeeded…), but the issues Deane mentioned ring true.

161. **TruthSeeker**
October 11, 2006

Penny,

As an impartial observer reading his comments here, I, too, did not get the impression that Deane was beating you up. His (constructive) criticism was apparently based on the fact that he mistakenly thought you had worked on the problem for only one month. If there's anyone to blame, it would have to be the journalist (again?) who misunderstood or misquoted you for whatever reason. I hope you will focus your energy on math again and not give up on your passions. The journey of discovery could be just as exciting as the discovery itself. It all depends on your perspective.

162. **penny smith**
October 11, 2006

Dear Truethseeker,
I love doing math. I just hope people are still willing to read, referee and publish my papers.Thanks.
As the example of Yau shows beware of journalists.
Even well intentioned ones like mine. She just missed a point in what I said. All humans make errors–Journalists included–and the MOST important thing in life is not intellect but kindness to others.

163. **Brad Stone**
October 11, 2006

Penny,

Rock on with your math self! I have been fascinated to witness this exchange, and depressed to watch the nature of the media attention around you. I hope that you have the oppurtunity to get away from all of this nonsense and get back to peacefully attacking your PDE research.

As for Deanne, I simply have to say that his argument about the amount of time you spent on NS research is without force. It saddens me to have to remind her/him of the productivity of one summer in 1905. 3-months and 3-earth shattering papers. Even though you were misquoted by the reporter, Penny, I find some observers' close-mindedness troubling.

Good luck with the corrections!

164. **Benni**
October 11, 2006

Dear Penny
I think the problem is not the nature article, or that it was published so fast. The problem was, that you told the reporter the following words:

"I'm pretty confident that my result is right, or I would never have submitted it anywhere," says Smith. She hopes to serve as a role model for women in mathematics. "On the other hand, I certainly want the prize,"

Dear Penny, why don't you just have told nature:
"Sorry. This is only a preprint, which might be wrong. I posted it on Arxiv because it maybe something but I don't have an Navier stokes expert in my department who I can ask. So it has to be emphasized that my paper might well be wrong"

Perelman is a good example. He never wrote in his papers something like: "Here I solve a millenium problem"….
Instead, in the perelman papers, everywhere is caution!

No mathematician has the right to say, "he is certainly right" when he is attempting a millenium problem, otherwise he is likely to make a fool of himself because in 99% the "proof" will be wrong!

165. **TruthSeeker**
October 11, 2006

Penny,

There is no reason why people would not want to read or referee your papers, since you've already established a good reputation before all this. Mathematicians are generally fair-minded individuals who like to see proof and evidence in everything, rather than going by rumors or gossip. And frankly, there hasn't even been much of the latter going around anyway. You mentioned that you were depressed upon discovering the mistake in your NS paper. I hope you feel better now.

166. **geometer**
October 11, 2006

According to mathscinet, during 1982–2006 Penelope Smith has published 18 papers which were cited 11 times by 8 authors. Of course mathscinet does not pick up all citations, but surely it picks up more than half. This is a decent record, but I wish Prof. Smith were a bit more modest while talking about her accomplishments.

BTW, Perelman so far published 17 papers cited 198 times by 111 authors (and this of course does not include his yet unpublished work on Ricci flow).

167. **Genghis Cohen**
October 11, 2006

Mathscinet picks up only reviews since 1998 or so. So geometer's statistics are pretty irrelevant.

I don't know why you're all being harsh on her. So many mathematicians announce big results that are wrong. Think Poincare Conjecture or Jacobian Conjecture or so many others.. It's true they spend more time and exercise more caution than she did.. but many of the people behind the false proofs don't admit making mistakes easily and cause much more of a nuisance for mathematicians than this did. This whole episode lasted what, 3 weeks, and the paper (including its predecessors) was short. So it's not like the whole field was ground to a halt by this.

168. **Richard**
October 11, 2006

"Penny: Honestly, I don't think Deane is beating you up."

I disagree. There's a lot of snide condescension in the tone of many of these posts by some who for whatever reason do not have the confidence and backbone to identify themselves, their own backgrounds, and their own accomplishments and failures. Frankly, this discussion has become awfully disgusting, despite the attempt of some to discuss math, and I'm sorry that Penny, who I do not know at all, has been dragged through this.

169. **woit**
October 11, 2006

This endless discussion has become both unpleasant and tedious. No one seems to have anything else to say about mathematics, rather they just want to personally criticize someone or another for something or other. As a result I’m shutting off comments here. If you’ve got something new and substantive to say about this, e-mail me and I’ll consider turning them back on.

Richard,

Many people here are posting anonymously and you are right to point this out. Deane is not such a person, he has often identified himself here, he’s Deane Yang, a rather distinguished geometer, one who has worked with Penny in the past.

Schroer’s “Samizdat”

October 6, 2006
Categories: Uncategorized

Bert Schroer has a new version of his paper that was discussed here earlier this year, now with the amended title String theory and the crisis in particle physics (a Samizdat on particle physics). He claims that the version reflects a change in viewpoint due to his participation in this and other weblogs, and I believe he would like the opportunity to discuss this further here. There’s also a posting about this at the weblog of Risto Raitio.

Update: Schroer, agreeing with his critics that his paper had too many typos, has sent me a corrected version, which is available here, for use until the arXiv version gets updated. He also agrees that an “s” should be a “z” in Samizdat…

Update: Schroer has a new paper out, which contains a review of AQFT and a discussion of light-front holography, with further comments on the relation to the Maldacena conjecture.

Comments

1. **Thomas Love**
   October 6, 2006

   Peter, Thanks for the link, that is why I visit daily. As I scannned Schroer’s article, seeing the section “The only game in Town”, this story flashed into my mind: After the Titanic hit the iceberg, a woman was looking for her husband and found him in the poker parlor. “What are you doing here”, she asked. He responded “It’s the only game in town.”

   Physics has hit an iceberg: string theory. Hopefully some will be smart enough to get off before they go down with the boat.

2. **TheGraduate**
   October 6, 2006

   sam iz dat

   1. a clandestine publishing system within the Soviet Union, by which forbidden or unpublishable literature was reproduced and circulated privately.

   2. a work or periodical circulated by this system.

   (Thought others might also wonder what a samizdat was.).

3. **LDM**
   October 6, 2006

   It was a good read until the suggestion that only Witten is able to follow all mathematical developments in string theory.

   Von Neumann once expressed the opinion that the body of mathematics had become so vast that he only understood maybe 50% of it. If string theory uses “almost all areas of mathematics”, then Schroer should make a stronger statement than he makes — and include Witten in the group who is unable to follow all developments in string theory.

   It is difficult to understand the deference to Witten.

4. **MathPhys**
   October 6, 2006

   LDM,

   Have you met him?

5. **Tim**
   October 6, 2006

   The distinguished Bert Schroer must have a very strong opinion well worth publishing if he changed his viewpoint after some random rants and raves on a weblog.

   Best,
   Tim

6. **Tim**
   October 6, 2006

   Sorry guys, this is just too obvious, I can’t let it pass:

   page 16. “The message from this illustration is that one theory can only be asymptotically contained in a more fundamental one if the structures harmonize.”

   According to Bert Schroer the phrase “the structures harmonize” is not a metaphore but terminus technicus and on top of that he made rigorous and not metaphorous conclusions about the ST -> QFT limit based on the QFT -> QM limit, in other words he has proven rigorously that these two limits are analogous even though according to him ST does not even make sense.

   page 17. “The fact that thousends (sic!) of publications were written about this problem and that even the still ongoing research has not been able to come any closer at proving/disproving this conjecture is a unique mind-bending phenomenon in the history of particle theory.”

   Unique? May I suggest Bert Schroer to substitute “the existence of QCD as a non-perturbatively defined QFT with a mass gap” in place of “this conjecture”? Please don’t respond with the experimental relevance of QCD as opposed to AdS/CFT since clearly the AQFT crowd can not possibly be further from experiments than it actually is.

   I guess I’ll read on just for the fun of it but quit reporting the most hillarious parts here, there will probably be just way to much.

Best,
Tim

7. **Tim**
October 6, 2006

I promise this is the last comment, the only reason I add it is because of a genuine desire to aid Bert Schroer in his future publications: please, please, get a spell checker installed on your computer.

Best,
Tim

8. **Thomas Love**
October 6, 2006

Tim Said:

"The distinguished Bert Schroer must have a very strong opinion well worth publishing if he changed his viewpoint after some random rants and raves on a weblog."

The ability to change one's mind when confronted with new evidence is called the scientific mindset. People who will not change their minds when confronted with new evidence are called fundamentalists.

The comments here are, for the most part well reasoned arguments. Except for the quotes from "he who must not be named".

9. **Bert Schroer**
October 6, 2006

I suggest to read the text in a more careful manner.
The analogy with QFT—QM is taken as a warning because in d=1+2 in the presence of braid group statistics there is no QM in the nonrelativistic limit; the maintenance of the spin&statistics theorem prevents the emergence of QM!! This is an illustration of a structural disharmony. I am only rejecting scale sliding arguments if they are unaccompanied by a prior structural conceptional reasoning .
The comparision with the state of QCD is misleading because QCD is not in an epsilon environment of a rigorous theorem but the M-conjecture is!!
Fortunately these misunderstandings have nothing to do with misspellings.
My greatest change of mind is the way I now view the Harvard professor Lubos Motl

10. **MathPhys**
October 6, 2006

Can someone please summarize to me what Schroer new point of view is? I find it difficult to see the point in 49 pages of polemic.

11. **H. K.**
October 6, 2006

I found the "(possibly infinite) Russian matryoshka" metaphor interesting.

12. **Stefan**
October 7, 2006

Peter,

All this time you were searching for an alternative [to string theory] to work on: why not AQFT?

Stefan

13. **Tim**
October 7, 2006

After carefully reading the manuscript I concluded that Bert Schroer simultaneously thinks the following about the AdS/CFT correspondence:

1. it does not make any sense
2. it is a conjecture to be proven or disproven
3. it is trivial, its rigorous form was known for a long time
4. it was recently proven rigorously
5. no more papers should be written about it
6. not enough papers are written about its rigorous proof

Did I miss something?

Best,
Tim

14. **Egbert**
October 7, 2006

It is a thought-provoking article, but it can really be separated into an examination of string theory and its situation and an advocacy of the axiomatic approach instead.

I think the identification of the prevalence of metaphors is the most important and relevant point at the moment, though. Metaphors can play their role in communicating proofs, but they are not in themselves valid proofs of anything. If one has the correct metaphors, however, one can know the procedures that one should perform, without understanding why you are doing those procedures.

So if you are a "shut-up and calculate" type, who say that you understand once you knows what to do, then metaphors will be good enough for you, provided they get the message across. On the other hand if you demand that you can say "X literally has the relation Y to Z" before you agree that you understand, then metaphors will never be enough for you.

And if somebody who only knows the metaphors and knows how to do what is expected of him has to teach an enquiring young student, then the young student will be told what is expected of him and given some metaphors to help explain what is expected of him. The student will then have three choices.

He can accept that he is now in a society where “understanding” means being able to execute the procedures and recount the metaphors, and that he will never have any deeper understanding than this.

He can pester his teacher to explain in a satisfactory way why he is supposed to do these computations, without using metaphors. This will irritate the teacher because the teacher himself has abandoned any attempt to understand in a non-metaphorical way, and the teacher will be ashamed of this and will react aggressively to the student to cover his shame, putting the blame for the confusion on the student’s lack of intelligence.

Or he can say that the arguments the string theorists give are unpersuasive.The string theorists will point out that he is not qualified to judge, since he doesn’t know string theory, and he can only know string theory if he’s been accepted by the community, and he won’t be accepted by the community unless he is able to “understand” string theory, that is, unless he finds the arguments persuasive.

15. **Bert Schroer**
October 7, 2006

Yes you did, I said the structural theorem which was proven is in serious contradiction with any possible interpretation which one can attribute to the (not precisely formulated) Maldacena conjecture. Your statement that the rigorous proof in the two cited papers support Maldacena’s conjecture and that I claimed that there are any proofs of it is your fancy, it has nothing to do with my essay.
You also take the word “trivial” out of context. It refers to the fact that the rigorously proven correspondence is a change in the spacetime encoding of the same algebraic substrate (pretty much like a change enzyms cause in the substrate of stem-cells by enforcing different spatial differentiation).
I think that you already belong to the misdirected post-string generation which I allude to, and as I told you already before you do not seem to be able to read a scientific article whose content does not fit your prejudices.

16. **Bert Schroer**
October 7, 2006

Egbert, you really got it. And is you correctly suggest the ideas in the last section (which partly come from AQFT) are meant to counteract the no other game claim and are not to be considered a commercial for any other fad.

17. **Egbert**
October 7, 2006

The distinction that I draw is related to Lee’s distinction between the seers and the craftsmen, or, perhaps, is the same distinction.

The seers, in my interpretation, are engaged in the task of arranging what they know into literal statements, so that they can be regarded as being actually true. This is important for theorizing because we can only perform logical inferences with literal statements. Metaphors and logic don’t mix.

The craftsmen, on the other hand, can make do with the metaphors once they know how to carry out their tasks. And they can (and do) dismiss any attempt to seek a “deeper understanding”, beyond being able to do what’s expected of you, as philosophical nonsense.

Seers and craftsmen are still metaphors, of course.

18. **Egbert**
October 7, 2006

Bert,

I see the connection now.

19. **MathPhys**
October 7, 2006

I must say I find Maldacena’s conjecture not only stunning, but also very deep and beautiful. Of course nothing so deep in physics can be ‘proven’ in the mathematical sense of a proof, but people can keep on working out examples, and verifying it incrementally, which is what they have been doing.

I find invoking ‘the anthropic principle’ disheartening, and I find the behaviour of a certain junior string theorist to be unacceptable. But to rope in the AdS/CFT conjecture into that too goes a bit too far.

If we ask string theorists to be open minded about alternatives, we should equally expect non string theorists to appreciate certain aspects of string theory.

20. **Bert Schroer**
October 7, 2006

MathPhys
If the incremental verification would lead to a clarification how this conjecture has to be formulated in order to agree with the rigorous structural theorem (the same rigor as TCP, spin&statistics, but may be of lesser physical relevance) I would agree with you!
But I am willing to make a bet that this is not what will happen. Its too late, the caravan (~4000 papers) has passed already and the conjecture has turned into one of these metaphoric legendary flying dutchmen who will circle above our heads without ever finding a landing place.

21. **MathPhys**
October 7, 2006

Bert,
I’m not qualified to respond to what you say in a way that would settle this discussion, but on this very point I think most of what you say is polemic nonsense.

22. **Arun**
October 7, 2006

Quote:

“A profound mathematical theorem reveals that there is even a unique correspondence between Local Quantum Physics {QFT both Lagrangian and non-Lagrangian} models in n+1 AdS spacetime with a n-dimensional conformal invariant Local Quantum Physics model…{Being a structural theorem, it does not identify the models} it only relates their LQP algebraic structures…..I have tried all possibilities of what Maldacena could have meant and none of them seem to be consistent with the above structural theorem."

End quote.

I, for one, could use some help here, with a further elaboration.

23. **Stefan**
October 7, 2006

Bert,

Is there a freely available introduction to LQP? or do I *have* to buy Haag's book if I want to study it properly?

Stefan

24. **Renormalized**
October 7, 2006

Tim and Lubos both sign there work with "Best". That is an unusual practice.

25. **Bert Schroer**
October 7, 2006

MathPhys
A better way than to make such unqualified statements would be to familiarize yourself with the content of the references where the correspondence theorem is proven and then to compare it with what you think is the content of Maldacena's theorem. This would force you to enter a new non-metaphoric and non incremental verification state of mind in which conceptual clarity and mathematical rigor reigns. It is not easy, and I could help you if you made a serious attempt, but I have no cure against prejudice and community-caused infections. Why do you use the pseudonym MathPhys?

26. **Peter Woit**
October 7, 2006

Renormalized,

It's not that unusual, and I am sure that Lubos and Tim are not the same person. I urge everyone here to avoid bringing Lubos into the discussion, or behaving in any way like him.

27. **Bert Schroer**
October 7, 2006

Arun, if you study the cited papers (not mine!) and come up with some questions I am willing to answer them. But it is futile to argue about the result of years of subtle thinking (left on the wayside by the great caravan) on a weblog. If I read (some) papers coming from the string community people like you (this also includes MathPhys) should also look at references outside their main research.
Stephan: the area of LQP is a rapidly developing research subject and Haag's book (which is very nice to make a first brush) only covers the area up the the end of the 80s. There is presently no up to-date book perhaps because the whole area is in a process of rapid change. A recent not so difficult reference on the powerful new localization concept (by which QFT sets itself apart from ST and LQG) is http://br.arxiv.org/abs/math-ph/0511042

28. **Rae Ann**
October 7, 2006

Renormalized, signing off with "best" is very common, especially on academic blogs.

Bert S., what exactly is your goal in your attack of Lubos Motl? To embarrass and shame him into shutting up because you disagree with his manners? Above, you say, "I have no cure against prejudice and community-caused infections." It looks to me like you have become a source of "prejudice and community-caused infections." Your apparent change in view of Lubos is caused by what exactly? His political and social views? The way he speaks? The people he associates with? His 'faith' in his work? If he is acting as a filter of papers for others it is because his assessments are considered valuable to them. This is how an efficient community works, by delegating duties, so to speak.

29. **Bert Schroer**
October 7, 2006

Stefan don't do this. These things are much better understood now. If you just follow the chronological order of the articles in http://unith.desy.de/research/aqft/ including thesis and Diploma publications you get an excellent view about the state of the art. There is also a book by Logunov, Todorov,..in which Bogoliubov is a co-author which is better than the original Bogoliubov-Shirkov books.
But it is very tough going, especially if you do it on your own and have nobody to consult.
Best

30. **Bert Schroer**
October 7, 2006

Rae Ann
No my main problem is with the string community and with the chairman of the Harvard Physics Department who tolerate the posting of death threats against the owner of this weblog.

31. **woit**
October 7, 2006

I will delete any further efforts to discuss Lubos Motl here. Please do this elsewhere if you must. It should be made clear that Bert is wrong about this, the chairman of the Harvard Physics department is not someone who has been willing to tolerate Lubos's recent behavior.

32. **Arun**
October 7, 2006

I left professional physics more than a dozen years ago; but I will try to read the cited papers and understand. The idea that the Maldacena conjecture cannot be made consistent with a general structural theorem, and yet has many, many people pursuing it is one that commands attention.

33. **Bert Schroer**
October 7, 2006

I would be extremely interested to have some comments about my essay from the Great Inquisitor.
Bert

34. **Tim**
October 7, 2006

Bert,

I trust you I will make an appearance in the acknowledgments of the new preprint version for drawing your attention to the untolerable number of typos. Please use the following formulation if possible: "[…] and also would like to thank Talicska Tim for a careful reading of the manuscript."

Best,
Tim

35. **Egbert**
October 7, 2006

Bert,

I understand the importance of the role that metaphors have played in the patterns of thought that have led people to invest so much of their self-respect in their "understanding" of string theory, but I think that there's another important factor at play.

The notion of "fundamental physics", or of what is going on "at the fundamental level" has played a very large part in string theory – more than any other theory so far. On inspection, it appears to be a mythological-religious notion pervading modern theoretical physics. I am informed, by theoretical physicists in general, that the mathematical description of what is "happening at the fundamental level" is actually what God sees when he looks at "the Universe". It is supposed to describe what truly exists, and is the cause of everything else.

This is rather far from the usual understanding of science. I think the notion of "how the world looks to God" has had a lot of influence in the minds of a group consisting mostly of athiests and people who profess to despise religion. It seems to me that the reason for this is a confusion over what constitutes a cause and what constitutes an explanation. What they want is a complete understanding, that is, an explanation of everything, but this has become confused with the idea of a most fundamental cause.

It may well be that a metaphor can be "understood", but not "literally understood", if it contains within itself a confusion between a cause and an explanation. But I think that this confusion must always be present in any discussion of "fundamental physics".

36. **Daniel Grumiller**
October 7, 2006

I have read Bert's paper as part of my editorial duties and I enjoyed it a lot. Naturally, any work of such a nature – a mostly historical paper on subjects whose history has not ended yet – is biased by the author.

It would be interesting to press the "save" button right now, fast forward a century or two, and compare with future historians their assessment of 20th/21st century string theory. I really don't know what I should expect to read, but I imagine one of the four scenarios:

1. An experimentum crucis has been performed which convincingly provides evidence for string theory. Bert's paper will be a historical example of misjudgement (because string theory was right after all).

2. An experimentum crucis has been performed which convincingly falsifies string theory. This blog will be a historical example of misjudgement (because string theory could be falsified after all).

3. No experiments, but an alternative has been discovered which is more convincing than string theory. Either it will be celebrated as the successor of string theory, from which it emerged, or as its opponent.

4. No experiments, no alternatives, string theory has become a quasi-religious dogma.

I think we all agree that 4. ought to be avoided by all means, and this, in my opinion, is the main point of Bert's paper.

37. **AdamBalm**
October 7, 2006

I completely agree, Dan.

Best,
AdamBalm

38. **Bert Schroer**
October 7, 2006

Egbert,
we both seem to agree that the phenomenon we are trying to understand cannot be solely explaine in terms of the action of some very intelligent and charismatic individuals who succeeded to inspire or mislead (according to standpoint) a whole community. If I understand your point correctly you attribute the present confusion to a clash between cause and explanation and consider it as an inevitable consequence of the quest for knowing what is going on "at the fundamental level". In other words the path of unraveling the fundamental truth will end in mysticism. In a way this sounds to me like an adaptation to science of the more sociological oriented "critical theory" of Adorno and Horkheimer ("ënlightenment must convert into mythology") which I refer to in section 3 of my essay (with an interesting footnote quoting from a passage of one of Horkheimer's essays).
I personally think that it is more a millennium phenomenon. After the cold war when globalized capitalism strengthened its grip on hegemony there was the new ideology of an "end of history" and the coming of an era of peace and happiness. The physics ideology of a theory of everything unfolded parallel to these sociological

developments in complete chronological analogy. In may essay I argue that this is not accidental, it is rather a manifestation of the millennium Zeitgeist. This will give the hope that after the passing of time the pendulum may sway into another direction.

39. **Egbert**
October 7, 2006

I think there is more to Bert's paper than Daniel has stated.

There is a value to attempting to seriously examine the relationship between string theory and QFT while simultaneously considering how communities communicate and affect one another's perceptions and motivations. After all, string theory is undeniably a community of people bound together by a commitment to a specific guess, and each of them has taken a gamble, and they communicate with each other quite a lot, and it is an interesting and valid and very relevant and topical question to ask:

When you have a community like this, where every member of the community has taken a gamble, and there is no way to know, for the foreseeable future, how that gamble will turn out, and the members of the community constantly talk to each other about the gamble that they have taken, how do they affect each other's perceptions of the likelihood of their position turning out to be right?

It would also be nice for each string theorist to state, for the record, that he has never allowed the confidence of other string theorists to influence his estimate of the probability of string theory being the One True Theory of the Universe. If there is even a small effect whereby seeing the confidence of others in the Truth of string theory leads to having confidence oneself in the Truth of string theory, then this effect will be amplified through a positive-feedback loop as the members of the community talk to each other.

So the question for the sociologists is: Do communities of people who have all taken the same gamble always eventually convince one another that the gamble will pay off, and that the people who share this assessment are more intelligent than the people who don't?

If the answer is yes, then the argument can be presented to the string theorists:
Given what we know about sociology, and given that spending one's doctorate on string theory is a gamble which people freely take, and given that the truth will not be known for a long time, it is 100% certain that the string theory community will end up declaring themselves to be sure of the truth of string theory and declaring people who disagree with this assessment to be less intelligent than themselves.

I think the response of the string theorists will be to say that the described sociological effect does not occur or string theorists are immune to it (due to their exceptional professionalism and intelligence). Are there any string theorists who would accept that this sociological effect is real and affects them?

40. **Egbert**
October 7, 2006

Bert,

The millennium Zeitgeist seems to be a recurring feature of society. It would then appear to be no coincidence that string theory is dominant more in the United States than any other country, and that it developed here. The millennium Zeitgeist is particularly strong here.

But the argument from the string theorists would be that the mainstream of science (which they consider string theory part of) is not so strongly influenced by the fads and fashions of society, and that theories of physics in particular, do not get swept away by changes of mood accompanying world wars.

So the question is what is it that distinguishes string theory from other areas of science by making it more vulnerable to Zeitgeists?

With regard to viewing things as superstition and mythology, my grandparents would talk about people (in my family and my neighborhood) as being posessed by spirits, and I have talked to them about it and understand what they mean. A "spirit" is something like an attitude, so that a person is said to have the "spirit of charity" if they are charitable and is said to be in "high spirits" when they are happy. A man is said to be "possessed by a spirit" when he has gotten some idea into his head and he has adopted some attitude and behavior. The idea of a spirit may even be a folk recognition of the concept of a meme, recently introduced to the world of scientists by Richard Dawkins and widely celebrated as a great insight.

But the traditional scientific rejection of mythology has tagged the idea of possession by spirits as completely insane gibberish. I think that string theorists may view Zeitgeists in much the same way. As my grandparents would say, they're posessed by spirits.

41. **ZZZ**
October 8, 2006

Tim said:

… drawing your attention to the untolerable number of typos …

Tim, it's no biggie to make a few mistakes when using big words. The correct English word here is "intolerable". Try to build a tolerance for typos.

Best,

ZZZ

42. **Egbert**
October 8, 2006

Thinking more about Zeitgeists, I think you may be right. It's not a question of being vulnerable to them, but the attitude of the population at large was unusually suitable for the emergence of string theory. It certainly seemed as though there was a role which the entertainment industry needed filled and string theory auditioned and got the part.

So if I understand you then, the Zeitgeist causes confusion for the individual string theorist because it provides confidence that humans in general have basically figured everything out, and the string theorist translates this into confidence that the leaders of the string theory community know what they are doing, even though he is not able to find satisfactory proofs that this is the case, but must rely largely on trust, or metaphors.

43. **amanda**
October 8, 2006

If BS wants people to turn from string theory to AQFT, he will have to find a way of presenting the latter that does not make it look very, very, *very* boring.

44. **Bert Schroer**

October 8, 2006

amanda,
In an ealier comment to Egbert I stated that this is not my intention. Anything which tries to compete with ST will sooner or later inherit all those aspects which I criticize in my essay. The strength of AQFT is its secrete charm, it is only accessible to people who are willing to make a series effort. It is our most precious post millennium investment and it is still in the process of growth and should not be thrown onto the market. That you find it boring confirms that I did not sex it up.

45. **Tim**
October 8, 2006

ZZZ,

Thank God I didn't write that down in a paper only in a weblog! The reason for that is simply that I run papers through a spell checker which is the thing I suggested to Bert Schroer as well. Everybody makes typos after all but nobody can stop you from installing a spell checker 🙂

And since according to Peter Woit Bert Schroer corrected his typos as a result of my suggestion I truly believe I have a reserved seat in his acknowledgments. Otherwise I must be lead to think that he similarly neglects other sources from where he is drawing inspiration while writing a paper.

Best,
Tim

46. **Thomas Larsson**
October 8, 2006

we should equally expect non string theorists to appreciate certain aspects of string theory.

Some of the work done by *string theorists* is certainly being appreciated – Peter obviously admires Witten's work in mathematics, and the success of CFT in 2D statphys has largely formed my own worldview; my research circle around generalizations of algebraic structures from CFT to higher dimensions.

However, being on the receiving end of 20 years of unchallenged pro-string propaganda is a formative experience. It is possible that AdS/CFT is an physically important development, but since every claim in the past about string theory's relevance to the real world has been a vast exaggeration at best, I would instinctively assume the same about AdS/CFT.

Especially since both sides in the duality seem unphysical. AdS/CFT relates a string theory in AdS space, with negative cc, to an N=4 supersymmetric gauge theory. Last time I checked, neither a negative cc nor N=4 susy were particularly good descriptions of reality. I do find quite disturbing is when string theory partisans like Sean Carroll claim that there is overwhelming evidence for gauge/gravity duality, but don't mention that both the gauge and the gravity side are unphysical.

47. **Bert Schroer**
October 8, 2006

Tim, here you have your way:
I am particularly thankful to Talicska Tim (or whoever hides behind this name) for suggesting the use of a spellcheck to improve my essay on string theory and the crisis in particle physics.
You can print it out and stick it onto your office-wall. If you surface mail me a printout to my Rio address, I will hand-sign it and send it back.
But now you owe me an acknowledgement. If your misrepresentation of the content of my essay on one of your previous blogs (especially the issue of M-conjecture) was not intentional then you thank me for correcting your misunderstanding.

48. **TheGraduate**
October 8, 2006

Thomas Larsson:

You said "It is possible that AdS/CFT is an physically important development".

I don't know very much about the mathematics of this but AdS/CFT is a conjecture not a theorem right?

49. **Egbert**
October 8, 2006

Media mention of string theory alert

50. **Bert Schroer**
October 8, 2006

Dear Thomas Larsson,
although I agree with the content of your statements, I want to stress that my criticism of the Maldacena affair is not based on some physical gut-feelings but on the use of rigorous mathematics guided by a physical conceptual frame (i.e. not that amok-running mathematics which combines well with the physics metaphors). The issue of physical relevance of this superoverhyped issue was not the main issue. Actually if you read my essay careful you will realize that it is not that superoverhyped aspects in itself which deeply worries me but rather the fact that there exists a structural theorem on AdS—CFT which the caravan did not bother to address.

51. **elan**
October 8, 2006

I don't believe that an enemy of my enemy is necessarily my friend. Not even if his essay is free of typos. Bert Schroer is notorious for bizarre behavior at research seminars, conferences and other public occasions, not unlike this forum's favorite Harvard faculty member. His science (algebraic field theory) suffers from the same ailments as string theory and, in addition, nobody cares about it. His fascination with Pascual Jordan, a discredited Nazi physicist, who (after his "rehabilitation") pushed for remilitarization of Germany and questioned postwar borders in Europe is truly puzzling: it is like emphasizing that Hitler was really a decent painter…

52. **Bert Schroer**
October 8, 2006

Yes, I find it fascinating that the protagonist of QFT (the unsung hero of QFT according to Schweber's historical book on QED) who was point-right on quantum physics could be so dead-wrong in his political ideology (by the way, diffent from Heisenberg who was the leader of the uranium club, Jordan was an absolutely useless figure for

the Nazis, that is why they banned him to Rostock).
Sorry for keeping a distance to the "good guys (we) against the bad guys (them) or "who is not with us is against us" . I think that human nature is more complex than that. I have not attended any conferences for ages and I am not responsible if I appear in the blogges fantasies.

53. **TheGraduate**
October 8, 2006

Bert,

Can I ask you whether AdS/CFT is a conjecture or a theorem?

54. **Bert Schroer**
October 8, 2006

The AdS-CFT is a rigorously established structural theorem of QFT.
The Maldecena idea that particular (beta=0 and hopefully conformally invariant) SUYM models link up with certain strings in 5 AdS dimensions in a certain limit N –> infinity is a conjecture which is in apparent conflict with what the structural theorem says.
What renders the situation difficult is that even if there is a conformal invariant SUYM CFT, the N—> infinity is an object which does not enjoy the status of a QFT. The big question is whether people who do some approximate calculations have the correct interpretation as the AdS correspondent of their (alleged) conformal QFT.

55. **Lolka**
October 8, 2006

Dear Bert,

If the N -> infinity limit produces something that is not a QFT than how is the rigorous methods of AQFT are in any position to judge the Maldacena conjecture? Why don't honest AQFT people say that "Okay, this Maldacena guy is talking about something which is not a QFT, so we can't say anything about it, since we are only able to make statements about QFTs and similarly our rigorous structural theorems don't apply in this case, since our structural theorems only apply to QFTs. Hence we are not in a position to judge the Maldacena conjecture because our tools are not appropriate to this kind of problem." ?

56. **David B.**
October 8, 2006

Dear Graduate:

The AdS/CFT is a conjecture with a lot of supporting evidence going for it. Wether you cal lthe evidence overwhelming or not depends on personal opinions. So far no one has found a "contradiction", and the number of tests that have beeen performed is huge.

The "structural theorem" that Bert refers to makes no mention of quantum gravity, so it explains nothing in the AdS/CFT.

The structural theorem, as far as I understand it says: the conformal group is SO(d,2). Therefore
the physical spectrum of the CFT is classified by unitary irreducible representations of SO(d,2). A field theory in AdS willl have the same symmetry group, therefore the two are equivalent. It also gives no information of the dictionary between both sides.

I'm sure Bert can give you a more technical essay on what the theorem means and he will very likely disagree with my assesment of those results.

If the correspondence were just that, it woud have been discovered very quickly and people wouldn't be working hard trying to understand it.

I think regarding the AdS/CFT, Bert is just wrong.

Sorry to spoil the anti-sting party.

Best,

David B.

57. **Bert Schroer**
October 8, 2006

No, the theorem is not just a relation of spacetime symmetry groups (this and the ensuing equality of the spectrum was already known way back by Fronsdal) but it is a correspondence between the full QFTs in all their details. It is a bit difficult to express this in terms of standard Lagrangian QFT setting because one of the two sides is non-Lagrangian if the other is. But there is a more general concept of QFT (which is already in constructive use because most of the 2-dim. factorizing models are non-Lagrangian http://br.arxiv.org/abs/math-ph/0601022, http://br.arxiv.org/abs/hep-th/0609130)
Although one side is non-Lagrangian it is still a QFT and not a ST.
So the only saving grace is that the incremental evidence for Maldacena is not given the correct interpretation; perhaps another case demonstrating the conceptual confusion which originates from premature terminology.

58. **Bert Schroer**
October 8, 2006

Lolka o.k., but isn't the Maldacena conjecture one which is claimed to involve CFT on one side?

59. **Moshe**
October 8, 2006

I don't have the time or inclination to get into a long discussion. However, I did find that wrong statements on the internet have a tendency to percolate to unexpected places, most importantly to the minds of young students. Let me then refer anybody that is interested in true statements about ads/cft (as opposed to various polemics) to the classic review hep-th/9905111, that is a good starting point.

60. **Bert Schroer**
October 8, 2006

Actually to disconnect this discussion from any attributions of values, if you say that the incremental evidence indicates something deep and contains an important messages, I have no counterargument. Whatever it is, it is defintely not an illustration for a AdS-CFT correspondence about which there exists a rigorous proof (a structural theorem on par with TCP, Spin&Statistics…). If you distingush a conjecture called the Maldacena conjecture (even if I do not understand its true meaning in terms of QFT concepts) from a structurally established AdS–CFT correspondence then all my problems (and may be even yours) are solved as far as the discussion on this weblog goes.

61. **optimist**
October 8, 2006

Thomas Larsson Says:

Especially since both sides in the duality seem unphysical. AdS/CFT relates a string theory in AdS space, with negative cc, to an N=4 supersymmetric gauge theory. Last time I checked, neither a negative cc nor N=4 susy were particularly good descriptions of reality.

=====================================

Physical reality of both sides is not the point. If at least one side is realistic then it is OK because it can be just a tool. Now you complain that for AdS/CFT neither side is realistic. String theorists and nuclear theorists know it and try to make "some gravity/QCD" version. I think these approaches have given qualitatively satisfactory results so far. It's not exaggeration at all.

62. **Bert Schroer**
October 8, 2006

I repeat, there is the maldacena conjecture about a relation between a duality relation between a N–> infinity gauge theory and some form of 5-dim. gravity and there is a mathematical theorem about a AdS–CFT holography (or better correspondenc) and both have, according to our best knowledge nothing to do with each other. From a conceptual point of view the correspondence is rather trivial because the same substrate of matter is only changing its spacetime encoding (for more details see may essay) and olthough the physical interpretation changes it does not change miraculously (e.g. a spin 2 particle must have been there already on the CFT side). If the more than 400 people who worked on this problem would in addition to their computations have leaned back a while and looked at the published theorem may be we would have known by now what the Maldacena conjecture really mean conceptually. But I would predict that nobody at this late time will do this, the right time has passed. Certainly I would not loose time on such a physically fruitless project, but on the other hand as mathematical physicist I find this change of spacetime encoding in holographic projections very interesting; most interesting if the smaller spacetime is not a brane but rather a null-surface (see my last section and the cited literature).

63. **Yatima**
October 8, 2006

I don't understand much of your learned discussion but I liked the reference to Leibniz. And please — no smearing by convoluted association. Why should the fact that Pascual Jordan was card-carrying NS party member be at all relevant (except if he threw out important research avenues because they looked 'jewish' or something)?

An old Usenet rule went that the first mention of H*tler or He*nlein would immediately end the discussion thread. Beware!

64. **Bert Schroer**
October 8, 2006

He did not. In fact the reason why despite all his ouvertures he remained a suspicious character to the Nazis was precisely because his publications up to 1934 were overwhelmingly with jewish collaborators. But he was a Nazi and a militarist.

65. **MathPhys**
October 8, 2006

Vafa and collaborators considered Maldacena-type dualities between simpler gauge theories and string theories.

This led to Chern-Simons/topological string dualities, which can be tested in great detail. It all works just fine. The results are so far purely mathematical, but the inner consistency of the underlying ideas is undeniable.

66. **Matti Pitkanen**
October 8, 2006

Some comments to Bert Schroer about quantum holography, Chern-Simons action, and null surfaces, or lightlike surfaces, as I have used to call them.

3-D lightlike surfaces in 4-D space-time, which itself is a surface in 8-D space-time H=M^4xCP_2, can be seen as fundamental quantum dynamical objects in Topological Geometrdynamics. They are identified as parton orbits. The effective metric 2-dimensionality with ensuing super-conformal symmetries makes D=4 as a space-time dimension unique.

The infinitesimal transformations respecting null surface property form a Kac-Moody type algebra of conformal transformations of H localized with respect to X^3 decomposing to representations of 1-D Kac Moody algebra.

The cones H_+/-= delta M^4_+/-xCP_2 are also crucial for the formulation of theory and the 3-D lightlikeness of delta M^4_+/- makes possible super-conformal symmetries of new kind based on canonical algebra of H_+/- and its super-counterpart. General Coordinate Invariance predicts quantum holography at level of H_+/-apart from effects implied by the failure of the complete classical determinism of the classical theory.

The resulting theory at fundamental parton is *almost* (absolutely important physically!) topological CFT defined by Chern-Simons action for Kaehler gauge potential of CP_2 projected to X^3. The second quantized fermionic counterpart of C-S action is fixed by the requirement of super-conformal symmetry. The theory allows N=4 super-conformal symmetries of various kinds broken for lightlike 3-surfaces which are not extremals of C-S action (have CP_2 projection with dimension D>3). No space-time (Poincare) super-symmetries and thus no sparticles are predicted.

Super-symmetrization of super-canonical algebra is possible for a sub-algebra of superconformal symmetries for which Noether charges defined as 2-D integrals over partonic 2-surface reduces to 1-D integrals as duals of closed 2-forms. The elements of this algebra has vanishing spin and color quantum numbers and thus leaves invariant the choice of various quantization axis. The vertices of the theory are described by almost topological having stringy character.

Correlations between partons (propagators) involve interior dynamics determined by a vacuum functional defined as a determinant of the Dirac operator and assumed to reduce to an exponent of Kaehler action for absolute extrema playing the role of Bohr orbits for particles identified as 3-surfaces: this is quantum holography at the level of space-time surface. Interior dynamics of space-time surface codes for non-quantum fluctuating classical observables allowing to realize quantum measurement theory at fundamental level.

The mathematical methods of string theory can be applied to TGD and one can see the target space of string theories as a fictive concept associated with the vertex operator construction assigning to the Cartan algebra of Kac-Moody algebra a target space. In TGD framework spontaneous compactification can be seen only as an ad hoc attempt to give physical content to the theory.

For more details see my blog and homepage, in particular What's New sections to get view about the recent rapidly evolving situation in TGD.

With Best Regards,
Matti Pitkanen

67. **Bert Schroer**
October 8, 2006

MathPhys
the theorem about the AdS–CFT correspondence does not apply to topological field theories because they lack the notion of localization which is the Faraday-Maxwell origin of the meaning of "field". Top. field theories strictly speaking are not field theories, they are only called this way because Witten and other people (who view QFT through the functional integral glasses) started to use this metaphoric name. Intrinsically they are combinatorial theories based on tracial algebras and this is also the name mathematicians as Vaughn Jones, Anthony Wassermann,… as well as people in local quantum physics call them. They are so to say the bones of a QFT after you remove the localization flesh to use a metaphoric language. The process how this works is very rigorous but not suited for a weblog.
David B.
I have given more details in may essay and there are 3 important references (not to my own papers). Believe me I would not say something like this and wreck my reputation in public if I would not be completely sure. The theorem has been carefully checked by experts and it has (as I mentioned) the firm status of TCP or Spin&Statistics. I am not disputing that there has been perturbative evidence for something called the Maldacena conjecture, but the interpretation of these calculations in terms of a model illustrating the AdS–CFT theorem (which is a structural theorem valid for all CQT) is metaphoric, it is something else which only a more series conceptual investigation can tell. But this is the problem of the people who do such computation, certainly I will not loose any time on computational details one something which I consider as unphysical as supersymmetry (a social construct in the words of Burt Richter and I agree with him).

68. **Thomas Larsson**
October 9, 2006

Optimist, I did not object AdS/CFT per se, only that people who promote it neglect to mention that in the well-established case, both sides are unphysical. The attitude seems to be that non-stringers should be kept blissfully ignorant about such petty details.

69. **MathPhys**
October 9, 2006

Bert,

Your entire career and that of your best friends, such Swieca, was devoted to the study of simple 2-dimensional models.

These models are at least as far removed from 'real' 4-dimesional quantum field theories, as topological field theories are removed from the usual ones.

However, no one stopped you. You did what you could do, and what you wanted to do, and you learnt something from that.

Your critique of Vafa et al is not only irrelevant, but also unfair.

70. **Bert Schroer**
October 9, 2006

elan
Thinking about when I ever have gotten into an public argument with somebody at a conference, I could only recall one incident at a conference at Lake Tahoe at the begiining of the 90s when there was some brawl with Peter Goddard, the present director of the Princeton IAS. Although he may still hate me for that, I cannot image that he would compare mediocre paintings with Jordan's creation of QFT and hide behind a pseudonym which is reserved for graduate students and non-tenured colleagues who may fear retaliation as a result of speaking their mind. This would be so absurdly off the standard set by the great Robert Oppenheimer (with whom I had the priviledge of personal contacts during my one year visit of the IAI at the beginning of the 70s) that I have dismissed this idea. And by the way my interest is not fixed on Jordan, I am interested in general in the complexity of humans which show up if a genius scientist outside his area of competence is dead wrong e.g. by supporting the Iraq war (which destroyed an entire country and destabilized the whole world order) http://xxx.lanl.gov/abs/physics/0603095
It is fascinating because it tells us something about the complexity of our own nature (not "them").
MathPhys and David B.
Your attitude with respect to the Maldacena conjecture and its controversial relation to the AdS—CFT correspondence theorem confirms the main thesis in my essay: when big caravan's of hundreds of people writing thousands of papers have manufactured a fact then its all over. Nobody will return to a deep problem where he runs the risk of not enjoying the impact increasing community support. An individual researcher in the old-fashioned sense would never ignore such a theorem because it would show his lack of professionality (maybe even lack of intellectual honesty). The problem is not ST in itself as Peter and Lee often state it but rather the stringy metaphoric way of doing particle physics. As anti-Semitism can exist even without Jews, this new accepted metaphoric way of a scientific discourse (and probably will) will be self-feeding without ST. I ask you honestly, would you besides a weblog contribution really sacrifice time and make an effort to study the theorem, its proof and its physical implications? of course not. Why should you, after thousands of publication it has converted into a fact. This is only one illustration of the many conceptual craters which have been left in the landscape of particle physics. As long as things were progressing smoothly, there was no reason to worry about this and now it is not only difficult to cover them in order to be on less swampy conceptual ground, but the present system of scientific production does not support such an activity. Attempts to point out some of the holes are considered to be not sexy (on the scale that ST is).

71. **Bert Schroer**
October 9, 2006

MathPhys
Sorry, the papers with Swieca (and in part with Voelkel) do not contain minimal models (this was done 10 years later by BPZ) but they do contain the decomposition theory into conformal blocks (called nonlocal components of the local field) in n-dimensions with particular emphasis on the chiral decomposition in two dimensions. This came out from the resolution of a rather deep paradox, the causality paradox of (global) CFT (discussed in previous work) The only model known at that time was the massless exponential Bose field and everything was checked in that example. If you call that unphysical that is your privilege. Whereas ignorance can be combatted, There is simply no remedy against (probably wanton) misrepresentation.

72. **Bert Schroer**
October 9, 2006

But where did I ever criticize Vafa? I said (in connection with L.B.s death threat against the owner of this weblog) that the ST people should keep their house in order, is this what you mean? It seems that they meanwhile tried, because the real foul ranting has not appeared for some time. Lets see whether they had a sustainable success. Fortunately physics is one of the most democratic human endeavours.

MatPhys, I have wield no power, I am a retired but still curious physicist, so why do you need the protection of a pseudonym? Be a man and have your outing!

73. **David B.**
October 9, 2006

Dear Bert:

I have no qualms about the results you were quoting. It's just not a proof of the AdS/CFT correspondence.

You want to call it algebraic holgraphy, that 's fine by me to.

I meant you were wrong with regards of a proof of the AdS/CFT being available. It's not. A proof has to contain gravity, or show that it is impossible to do that. Neither of the papers that you quoted explains this to any level.

It is clear that you misunderstood my post and now you are attacking my professionalism and trying to put me down with a rethorical statement. I will not have that, nor will I respond in kind.

It is exactly this attitude in weblogs that makes it very hard to have a real discussion. I might not be the most articulate person, and sometimes what I write can be misinterpreted. That is a risk l take when I post in this kind of forum.

The reason I posted in the first place is to make it clear that the AdS/CFT has not been proved yet, and that you were wrong saying that it has been proved. That is a misleading statement. Something elese was proved. I did not say you were being dishonest, nor lacking in professionalism.

Even professionals can be wrong, and that happens quite often in my experience.

David Berenstein.

74. **Moshe**
October 9, 2006

Hey David, hope you and yours are doing well, looking forward to seeing you soon. Just a word about your comment- as I indicated above and previously, it is important to correct wrong statements since they spread. I am glad you did that for that "proof of ads/cft". On the other hand, it is probably not necessary to get into extended discussions with people not really interested in exchange of ideas. Hopefully sometime soon there will be more forums adequate for precisely such exchange, hope to chat with you there.

75. **Bert Schroer**
October 9, 2006

Dear David, you seem to think that "algebraic" distinguishes it from "field theoretic" but here you are (probably in a large company) misunderstanding what this framework stands for. It has been known that pointlike fields are just algebraic coordinatizations, so it is not a change of content of QFT but just a conceptually very powerful way of avoiding to make a commitment to any particular (composite) field (see Weinberg who way back also became aware of this problem). In your work you have accepted this important step to "intrinsicness" in differential geometry, but you obviously have no familiarity with a more autonomous formulation of QFT which is nothing else but doing the analogous step in QFT. This is not surprising since the new setting (without changing a jota in the physical interpretation of QFT) has taken 2-3 generations to come into the present shape and if one is involved in serious computational work on incremental perfection of the Maldacena conjecture (please no cynism) one does not have the time to immerse oneself into subtle conceptual matters, so this is not an accusation! If you try to find the AdS correspondent of the free massless Bose field (to which the theorem of course also applies) you will understand exactly why one does it that way. I think in the other paper and the lecture Rehren is more reader-friendly and indicates how this can be translated into the setting you are more familiar with. And let me encourage you (since you have strong computational power) to find out what the Maldacena conjecture (as a computational support for a relation of a new object to a particular object called the N–>infinity limit of a CFT, not itself a CFT) could conceptually mean; it is definitly not an illustration of a AdS—CFT correspondence of QFTs (not even in the widest sense of the use of "QFT")!
And by the way I do not mind if you are a little rough with me (I like young enthusiastic scientists) as long as you do not behave as L.M.

76. **David B.**
October 9, 2006

Dear Bert:

I will read those papers carefully.

Regarding notation: I was calling it algebraic holography to distinguish it from gravitational holography. I was not trying to make anything else implied. I was trying to separate the objects of the discussion to avoid confusions.

Now I'll be signing off this discussion thread.

Best,

David B.

77. **MathPhys**
October 9, 2006

Bert,
The reason I use a pseudonym has nothing at all to do with this discussion which will not get anywhere. Signing off too.
I wish you all the best.

78. **Arun**
October 9, 2006

David B., hope to hear of your conclusions whenever you have the time.

79. **Bert Schroer**
October 9, 2006

David, I understand, this time I misred you. Let me point out that the null-surface holography is much much more interesting than that on (possibly infinitely distant) branes. This type of holography is explained in my CQG article

http://br.arxiv.org/abs/hep-th/0507038
I also reworked the second part which I will send as an replacement probably during this week. This is the case of double cone holography which is closer to the problem of black hole entropy (with some remarks about black hole entropy which could interest you). If you want I can send a copy to your private address, only the reference list has still to be ordered (hope to find time soon). You probably realized that I am a little bit of a loner in the sense that that I do not represent a community and only occasionally collaborate with people who have a similar conceptual-mathematical background. My critical attitude towards string theory is less its content and more the metaphorical way of thinking which it (and other allegedly fundamental proposals) supports. In my essay the fact that it did not lead to predictions is hardly mentioned.

I now understand that you had t'Hoofts vision of holography in the context of gravity in mind; but my use of the word applies to bulk matter in Minkowski spacetime hologaphically projected onto its causal horizon. I think it is not so difficult to understand why this cannot be done in any Lagrangian setting (quantities as entropy have nothing to do with a particular "field-coordinatization). The most important result is that you get a notion of entropy ("localization entropy") which is consistent with the new quantum local covariance principle (related to background independence) which the string theoretic calculation (and also Weinberg's vacuum polarization estimates of the cosmological constant) is not.

80. **Arun**
October 9, 2006

In 2002, the two big problems were
http://www.esi-topics.com/brane/interviews/MichaelDuff.html

"What is the theory? How do we make realistic predictions?"

81. **Bert Schroer**
October 11, 2006

I am somewhat surprised that there was no comment on my emphasis on new contributions (not my own!) in papers which throw considerable doubts on the standard (ST and LQG) mantra that gravity is irreconcilable with QT. In view of
http://br.arxiv.org/abs/gr-qc/0603079
and the development over the last 5 years quoted therein, discussions like that between Sean and Lee look obsolete (and appear a confirmation of my suspicion in may essay that communities are a hindrance to progress).
The main point here is that ,although the emphasis on background independence is very important (the original sin of ST), its implementation through diffeomorphism-invariant states (as in LQG) is misleading. Rather the local covariance principle (diffeomorphism-independence) reflects itself in an algebraic property which (since states are dual to algebras) in terms of states is the invariance of a whole folium of states, but not that of its individual members (these things are explained in previous contributions of the authors). It is precisely the operator Einstein-Hilbert equation which brings about the background independence (it allows to make "symmetry-transformations" around the original background see B-F) and this is independent of whether the perturbative implementation has a finite number of parameters or not ((no)nrenormalizable). The recent perception that massless finite helicity representations have natural string-localized fields with excellent short distance behavior
http://br.arxiv.org/abs/math-ph/0511042
generates new hope that the last word on finite versus infinite parameters has not yet been spoken.
A similar tendency (a return of reconciliation of gravity with QFT) is evident in
http://br.arxiv.org/abs/gr-qc/0610018
the problem in that approach is how one can reconcile the asymptotic safety requirement with the continued validity of background independence for short distances (or whether one should give up b. i. for s.d.)
Finally I would like to express my wholehearted sympathy with Nakanishi's recent critical view on these problems
http://br.arxiv.org/abs/hep-th/0610090
His postulate: In the ultimate theory, any concept of classical physics must not"appear logically prior to its quantum-theoretical construction" is nothing else than Pascual Jordan's plea to walk (in QFT) without classical crutches which is also the Leitmotiv of AQFT.
In fact the characterization of QFT in terms of relative modular positioning of a finite number of copies of a "monade" in the last section of my essay (not a metaphoric picture but a rigorous theorem!!) is a nice illustration that even the last vestiges of classical thinking can be removed from QFT.
I do not understand his distinction between on- and off- shell supersymmetry but I am in complete agreement with his emphasis on un-naturalness of this concept (for me it is even too rigid for finding any beauty in it). I don't think that SUSY can be broken in any meaningful sense of this word (it is much too rigid for that). Already in the most soft way of symmetry breaking (by bringing the system into contact with a heat bath) it collapses instead of suffering a spontaneous breaking
http://br.arxiv.org/abs/hep-th/9812179

82. **dan**
October 11, 2006

Dear Peter Woit,

For your second-edition NEW (perhaps published after LHC results?)
have you considered

1- adding lots of pictures and diagrams, pictures of GUT, standard model particles, SUSY-breaking scenarios, Feynman diagrams, Kaluza Klein etc.? Pictures fo SM and then MSSM, picture of GUT SU(5), etc., A picture of how large a number 10^120 (for cc) and 10^500 (for vacua) is helpful in comparison to pictures and photos of deviations from observation QFT anomlous moment of electron, and GR on binary neutron stars, is. pictures of Higgs, pictures of Higgs triplets. picture of how string theory is supposed to make up the SM, and pictures on why this scenario is probably wrong.
pictures of W Z Y bosons and how Higgs is supposed to break them. Picture of Higgs self-energy and fine tuning.

2- have a chapter written by someone else (i.e Rovelli) on why strings fails as a theory of quantum gravity? (with pictures of course).

3- Whether KKLT is plausible or unphysical (with pictures on what these flux wrapping look like) along with pictures of epicycles.

4- A chapter first on the ethical responsibilities of scientists, the need for scientists to be skeptical, and printing colorful email and weblog responses from string theorists (i.e lubos, jaques, and possibly others) — For the reader's entertainment and insight on what some string theorists fall from the idea of what a scientist should be. pictures of the major string theorists might be helpful. pictures of obnoxious string theorists who write obnoxious emails also helpful.

5- FAQ chapter of standard string theorist objections and your counter-arguments with pictures, if applicable. Jaques Distler for example, compares string theory vaccua to infinite QFT'S.

6- Summary of predictions of string theory (or lack thereof) perhaps in table format.

Such a book could reach a wider, popular audience.

83. **Bert Schroer**
October 12, 2006

Dear David,
you said:
"I will read those papers carefully.
Regarding notation: I was calling it algebraic holography to distinguish it from gravitational holography. I was not trying to make anything else implied. I was trying to separate the objects of the discussion to avoid confusions."
Take your time because the conceptual and mathematical complexity is not lesser than in ST (and if I would take notice of the existence of ST for the first time, it would take me much more than a week only to get a rough impression of what it is about).
My problem with 't Hoofts "gravitational holography" is that I simply don't understand it (too vague). The post 't Hoofts paper which comes my mathematically and conceptually very precise form of (Minkowski and CST) holography close is one of Susskind's paper where he connects it with the so-called "lightcone quantization" without anything gravitational. The problem is that this was not even done correctly in the case of free fields (there he is in the company of all the lightcone quantizers). But whereas in that special case there really exist a linear relation between the free field in the bulk and its appropriately defined lightfront restriction, this ceases to be the case and I do not know any other way than to convert the bulk field into the operator algebra it generates (also generated by all its composites) and to carry out the holographic projection with this algebra. At the end you may express the net of algebras indexed by the natural compact regions on the lightfront (which are nonlocal relative to those in the bulk) again in terms of one of its generating fields (these are the fields which obey the transverse extended chiral field relations similar to W algebras). The conceptual and mathematical structure is explained (as well as I am presently able to) in
http://br.arxiv.org/abs/hep-th/0511291
whereas the technology has been done in the first part of this 2-part paper. If you look into it you will see the subtlety of statements like the degree of freedoms "live" on the horizon or in the bulk.
Compared to the null-surface situation the AdS–CFT case is easier (from a conceptual point of view) because that relative nonlocal behavior is absent. In that case the AdS—>CFT direction can be done directly in terms of local fields. For those CFT which come from a "standard" (this can be made very precise) AdS the inversion in terms of pointlike fields is tricky but still can be done (and the result is a nonstandard CFT), whereas the inverse CFT—>AdS for a standard CFT is best done in terms of algebras. It is very instructive to start with a conformal free field. Then you get one of those "hollow" nonstandard AdS theories (see the the paper of Duetsch-Rehren and the lecture notes of Rehren).
Bert

84. **Bert Schroer**
October 12, 2006

I always considered the Kaluza-Klein idea to be incompatible with the structure of internal/external symmetries (external is short hand for spacetime) of QFT. The reason why younger physicists do not see any problem with K-K I ascribe to the fact that the kind of deeper knowledge about particle physics has already been eroded by decades of string-caused metaphoric thinking. I feel very comfortable in the company of Noboru Nakanishi who says in his recent article "Spacetime in the Ultimate Theory":
"It is the cheapest idea to extend the spacetime to the one having a dimensionality higher than 4. Nevertheless, recently, it has become quite fashionable to investigate the theory of extra dimensions. The grounds for considering it seem to be the excuse that any possibility is worth investigating as far as it is not completely denied by experimental evidences and the expectation that it might be possible to resolve the hierarchy problem stated in [A] by introducing new parameters in the extra-dimension space. It is quite disgusting to revive the old Kaluza-Klein theory without any essentially new idea for resolving the fundamental problem of how to expel the extra dimensions from the physical world. Furthermore, the higher-dimensional theory makes ultraviolet-divergence difficulty out of control; the extradimension people merely hope, without showing its ground, that the divergence problem might be resolved by a non-perturbative treatment.
The dimensionality of the spacetime where we live is undoubtedly 4. There is no other observation more manifest than this fact. The extra dimensions, whose number is denoted by □, are qualitatively different from the physical spacetime dimensions from the outset. Recently, the superstring people have proposed the hypothesis that all fields other than the gravity are confined to a soliton-like (according to their claim) object called the "□-brane", but it would be quite difficult to formulate such an idea within the framework of the (4+□)-dimensional quantum field theory. A theory is worth being called a (4+□)-dimensional one if and only if its fundamental action is (4+□)-dimensionally symmetric. But such a theory remains (4 + □)-dimensionally symmetric unless one breaks it artificially. In order to make the □-dimensional extra space invisible, the extra-dimension people are forced to make it round into a tiny one by hand. This procedure implies that they are actually considering the 4-dimensional spacetime accompanied with an □-dimensional internal space. If so, they should honestly claim that the extra dimensions are internal. Then there is no logical basis for adopting a (4 +□)-dimensionally symmetric action. It is quite non-scientific to pretend as if such an action were a privileged one according to the symmetry principle.
There may be the objection that the extra dimensions are made round not by hand but "spontaneously". Certainly, there is no "proof" of the no-go theorem stating that the desired spontaneous compactification can never take place. But if they assert the possibility of such compactification, the responsibility of verifying it must be attributed to those who assert it. That is, the extra-dimension people should construct at least one model in which the spontaneous compactification of the extra dimensions takes place in a natural way. I cannot believe that such a model can be constructed without greatly changing the framework of the conventional quantum field theory, because the situation encountered here is qualitatively different from the ordinary spontaneous breakdown of symmetry.

In fact this is corroborated by the most profound results about internal symmetries from AQFT. In that conceptual framework of QFT the relation of inner symmetries (including statistics) with the spacetime localization structure arises from the representation theory of local observables (bosonic by definition). In 4-dimensional theories this leads to the field algebra with a compact group symmetry (the structural DR theorem admits in principle any compact group). Supersymmetry is the extremely artificial (and unstable under total collapse, not symmetry-breaking) situation in which the observable algebra is fine-tuned and amalgamated together with one of its fermionic representations.
This representation theoretical origin of internal symmetries is incompatible with "little curled up spacetime dimensions". Such a picture is only possible in a classical setting i.e. one has to do it there and then quantize. This is in complete agreement with what Noboru Nakanishi (and I think every particle physicist who made his career before the great string theory sell-out) says.

85. **Rickkkk**
October 13, 2006

I have not yet read the paper or any of the comments in this post, but I must say I'm excited to hear this news. Bert's other paper earlier in the year was very enlightening for me as an outsider and deals with things that to the uniniated such as myself seem to not be given due scrutiny. Can't wait to read it.

86. **egbert**
October 16, 2006

Bert says:

> Finally I would like to express my wholehearted sympathy with Nakanishi's recent critical view on these problems
> http://br.arxiv.org/abs/hep-th/0610090
> His postulate: In the ultimate theory, any concept of classical physics must not"appear logically prior to its quantum-theoretical construction" is nothing else than Pascual Jordan's plea to walk (in QFT) without classical crutches which is also the Leitmotiv of AQFT.

I disagree with this postulate. The following is the reasoning: What is quantum mechanics? Is it something which tells us what the Universe is? Or is it a prescription for saying what the probability of getting experimental results is?

The idea that we need to get rid of "classical crutches" supposes the former. The overwhelming consensus of the physicists who took the time to think carefully about the issues involved (the Copenhagen school) is that the latter is the case. Everybody can have his own opinion of course, and the people who do not carefully go through the arguments which lead to the Copenhagen interpretation will probably not arrive at the same conclusion. The people who like to go with their "gut feeling" will choose the other conclusion, because it is nicer to think that the mathematics we use describes the Universe itself rather than encapsulating our knowledge of the statistics of experimental results. But the only reason I have ever heard for dismissing the idea that the mathematics describes statistics of measurement results is "gut feeling", and perhaps an expression of contempt for careful thought.

In fact, in answer to the question of where the confusion in modern physics comes from, it seems clear to me (and I think Lee Smolin, among others, agrees with me) that it starts with quantum mechanics, and after quantum mechanics nobody knows what's real and what isn't. That's when metaphors replace literal statements. And we have aggressive insistence from various people that there's nothing to be confused about and everything is clear, but different people are selling different interpretations and they disagree with each other violently and dismiss each other's views as not worth considering.

Question: Does the string theory project implicitly assume that the Copenhagen interpretation is wrong? Remember that Copenhagen is still the official interpretation of mainstream physics.

So, we have a clear and unambiguous interpretation of quantum mechanics as a formula for predicting results of measurements. But this needs the classical concept (or "crutch") of a measurement and its result. People propose to get rid of the measurements and their results and what do they have left? Just a Hilbert space and an element of it (the hypothetical "wave function of the Universe"), and no way to relate it to observed phenomena (stories about unobservable parallel worlds are supposed to make up for it), the same problem that string theory suffers from.

87. **Bert Schroer**
October 16, 2006

Egbert
there is a fundamental misunderstanding here; what Jordan had in mind and what I mean has nothing to do with Bohr's (and the Kopenhagen school) emphasis that our language for interpretating QM must be the classical one which relates to our perception of the material world. What was at issue is the classical parallelism called (Lagrangian) quantization. QT is more fundamental than classical theory and quantization is a very limited and entirely metaphoric way to access QT. The deep conceptual insight is to understand how the classical world re-emerges from the quantum setting, but this is the opposite of quantization.

88. **Bert Schroer**
October 16, 2006

Egbert
there is a nice old bom mot from the mathematician and mathematical physicists Ed Nelson:
(first) quantization is a mystery but second quantization (without interactions) is a functor.
mystery=metaphor

89. **John Gonsowski**
October 16, 2006

Bert, is what Jordan had in mind by any chance found in Jordan Algebra? John Baez in a coversation with Tony Smith once said that what you want is something that is both a Lie Algebra and a Jordan Algebra. Very loosely I think this could mean Lie Algebra is the classical-like thing and Jordan Algebra is the quantum-like thing. The relationship between Lie Algebra and Jordan Algebra might then be what you called a deep conceptual insight. Lee Smolin wrote a nice paper on the relationship between Jordan Algebra and String Theory.

http://xxx.lanl.gov/PS_cache/hep-th/pdf/0104/0104050.pdf

One of the references in this paper is a bosonic M-theory paper by Horowitz and Susskind. If only Susskind would work more on this than the Landscape!

90. **Bert Schroer**
October 16, 2006

John Gonsowski
certainly not. The plea for a new access to QFT without "classical crutches" is from 1929 and as far as I remeber the propsal about Jordan algebras is from 1934/35 (the best is the joint work with von Neumann and Wigner). Jordan algebras play an important role in generalizing QM (in particular to get a good understanding of the relation algebra—states, see the book by Bratteli and Robinson). But I think there is a theorem that (under very general condition) each infinite dimensional Jordan Algebra is a von Neumann algebra (see also a paper by Jochum). The 1929 Jordan plea goes into a quite different direction namely to liberate QFT freom those nasty (intrinsically singular) "field-coordinatizations" in analogy to the liberation of geometry from coordinates in modern Diff. geometry. This has been achieved in AQFT, in fact AQFT came into beiing precisely as a result of removing the undesirable features of the (Borchers) singular composite field classes (i.e. to have instead of infinitely many field coordinates just one object: the net of local algebras (which any one of them generates)

91. **Bert Schroer**
October 23, 2006

If my apprehensions that years of co-existence with stringy metaphors breeds ideological prejudices still needed confirmation, the reader can find it now in the writings of Distler and Motl. (see also remarks in my lecture notes which aooeared on Mondays hep-th posting)
Distler: "The troubling aspect is the totemic power of the word "Theorem," and its ability (in the minds of some) to trump sound physical arguments and abundant calculational evidence." Have string theorists (after their sociologically successful magic trick three decades ago to liberate themselves from the bonds of observational control) now also abandoned any commitment to mathematical checks because they feel threatened by Rehren's theorem?
"…were it not for the peculiar amplifying nature of the internet that has, apparently, given these ideas a certain currency among impressionable young students."

Perhaps we have to go back to Socrates and his contemporaries; what about threatening Rehren with the cup of hemlock (after all one of the opponents of rigorous theorems is also a specialist in death threats)?
"when we met in person, he seemed like a very nice guy",
does Distler expect that opponents of string ideas look like teeth-grinding monsters?
Putting the re-hashing of a non-continuous quantization of the Weyl algebra (which in the beginnings of QM was invented to show that not every representation of it allows a return to the Heisenberg commutation relation) on par with a recent deep AdS—QFT holography theorem can only be done by somebody who does not understand the theorem or willfully wants to exorcise it because goes it may cross the path of his holy grail; in the present case the motivation seems to be a mixture of the two.
The problem why Rehren has these difficulties with the string community is that they apparently are not aware that holography is not a manifestation of gravity (it can be derived with or without gravity). They do not seem to know that there is now a generalization of QFT which incorporates the local covariance (diffeomorphism covariance) principle and which also shows that a perturbative treatment of quantum gravity is consistent with background independence (if the QFT obeys the quantum Einstein-Hilbert equations). It would be interesting to say something more about these recent results on this on this blog (in case of interest) since this new insight destroys the mantra of ST and LQG that QFT and gravity are not compatible.

92. **Anon**
October 24, 2006

What's the difference between "diffeomorphism covariance" and "diffeomorphism invariance," and which is one the correct notion to apply in quantum gravity?

93. **Thomas Larsson**
October 24, 2006

Bert, sorry, but this case I fail to see what's wrong with what Distler/Motl say. In physics, a theorem can fail not only because it is technically wrong, but also because the axioms are wrong. E.g., a century ago people rigorously proved that Bohr's model of the atom was wrong – according to classical mechanics, the electrons will lose energy an eventually fall into the nucleus. The problem was that the theorem was wrong because the axioms of classical mechanics do not apply to the hydrogen atom.

Similarly, it is possible that there are some exotic systems that obey the axioms of QM in a generalized sense, but that does not mean that such systems are realized in nature. It seems to me that LQG's failure to reproduce the harmonic oscillator as we know it is a show-stopper, plain and simple. There is hardly any system better studied that the oscillator, neither theoretically nor experimentally. Convincing physicists that there is a new kind of quantization which gives different result for the oscillator is not going to happen.

About Rehren duality, I really have no opinion, because I don't understand it. However, if what Motl write is correct, that a bulk field psi(x,y,z) is dual to a boundary field psi_z(x,y), then it seems pretty trivial to me. Besides, I think that the value of dualities are highly overrated. A duality is only interesting if one side is physical and the other is tractable, something which seems to be the case neither for Maldacena nor Rehren duality.

Rather than giving up established knowledge, progress is obtained by finding the loopholes. The loophole relevant to quantum gravity can be found here. Note how Robert reluctanty agrees with me, and how Motl and Distler refrain from explaining that I am an incompentent moron who does not understand the difference between a gauge and a global symmetry, which they undoubtedly would have done if I had given them a chance.

94. **Thomas Larsson**
October 24, 2006

In the unlikely case that somebody cares, I did point out to Lee Smolin that the LOST theorem might be wrong because its axioms are wrong here, before Distler and Motl did so.

95. **Bert Schroer**
October 24, 2006

Anon
Diffeomorphism covariance (or simply local covariance) is a concept that Einstein introduced. In special relativity, even though Newton's absolute space-time has been abandoned, it was still meaningful to talk about events as spacetime points in a given background.
In general relativity spacetime points however loose this apriori meaning because the principle of general covariance forces one to consider a point as belonging simultaneously to many different spacetimes (an unusually abstract idea which most students of GR became aware only very late). The reason for this abstraction is as follows. Imagine that you have a classical system of electromagnetic field strength and metric tensor satisfying together the Maxwell-Einstein equations. Suppose these fields are known prior to some spacelike surface Σ then, indeed, without conditions for the coordinatization of future points one has no Cauchy problem. As is well known, the Einstein equations give four constraints for the metric field and its normal derivative on the surface Σ and only 6 "dynamical" equations for the 10 quantities $g_{\mu\nu}$. If the observer establishes the
labelling of future points of M (coordinatization) by means of conventions using only $F_{\mu\nu}$ and $g_{\mu\nu}$, then the mentioned diffeomorphism will have no effect on his observations. The "change of the physical situation" is exactly compensated by his
change of the labelling of the points. The field equations together with such coordinate conditions provide a deterministic scheme.
The active interpretation of general covariance demands therefore that we imagine the possibility of decoupling the labelling of (future) points of the manifold from the prevailing configurations of
fields which obey a closed system of intrinsic field equations. The upshot of this is that a point which was contained in a spacetime region is as well belonging to any other spacetime region which is isometric to it. In other words if you live in a world in which only a (causally complete) part is physically accessible then you cannot distinguish this situation from any other world which has a patch which is isometric to the original region. This is a consequence of the active interpretation of transformations (i.e. diffeomorphisms and not coordinate transformations) and therefore comparisons with gauge theories (where one was forced to introduce to introduce redundant descriptions) may be misleading (those do not have an active interpretation).
The phantastic progress in QFT in CST was the recognition that with a more abstract notion of what constitutes a QFT (a new functorial concept which contains the old one as a special case) you can implement local covariance in QFT in CST
http://br.arxiv.org/abs/gr-qc/0603079
(look also at the literature cited therein)
This is after Einstein and black holes the third great step in gravity!
If in addition you quantize gravity (say perturbatively) the already established local covariance will automatically pass to background independence i.e. the change of one background to another one becomes a symmetry transformation in the full quantum theory (the argument on page 120). The problem whether there are other ways of quantizing which make QG a theory with a finite number of parameters (the standard way leads to an ever increasing number with increasing perturbative order) is not entering here because any perturbative approach will lead to a perturbative fulfillment of the Einstein-Hilbert like operator equation and hence the B-F argument applies. Hence the claim that QT and gravity are not compatible has finally been shown incorrect. This also is relevant to Lee Smolin who seems to have internalized this mantra during his years as a ST.
I have bothered Peter to have a special blog on this important matter which changes the rules of the game in town, but he is immersed in combating the ST backlash via embedded journalists. What criticism can be stronger than this invalidation of the ST mantra?

96. **Bert Schroer**

October 24, 2006

Thomas Larsson
I will return to your question later on. Meanwhile you may read what I wrote for Anon, because the Maldacena issue is not independent of these new developments in QFT.

97. **Bert Schroer**
October 24, 2006

Thomas Larsson
the short answer to your remark is the following question:
Is the starting point of Maldacena (not what he actually argues but how he wants his conjecture to be understood) a conformal field theory (even one having a Lagrangian description: SUYM) a 4-dim CFT, yes or no?
If the answer is yes then Rehren's theorem applies, he does not have any other assumptions. But then how can M be correct? If the answer is no than Maldecena may be correct but the boundary value he imagines cannot be a CFT. I do not know whether the N->infinity limit is conformal, in fact I have never seen a proof that one obtains conformality for finite N. I think the only safe statement seem to be the vanishing of the beta function in lowest order. Lets for the sake of the argument assume that not only beta=0 but also conformality (stronger than beta=0). Then one would think that this property is maintained for N=infinity. Here one has to be a bit careful because that double limit (large 't Hooft and large color) is not a naive limit which maintains field theoretic properties. But to imagine that bluff!!, suddenly conformality is lost in the limit is a bit far-fetched.
As Rehren has pointed out in his discussion with Distler (he stopped that discussion after it became obvious that there was no point in a continuation) that "restriction" and what they call"duality" in that discussion is the same (this to me very surprising and highly nontrivial statement was shown by Duetsch and Rehren).
I think that the origin of this misunderstanding of Rehren's work by ST is the way in which 't Hooft conceived "holography" as a mysterious property of gravity. This picture (the metaphoric part of Distler's discussion) is incorrect, holography is defined with or without gravity. In addition this picture would contradict the unity achieved via that fantastic extension of QFT which I sketched in my comment to anon.
The whole situation is very tragic because it shows that with the market force and embedded journalists the survival of individual researchers against corporate groups is endangered, at this point Lee is 100% right. This weekend I have watched the KITP glee club with the embedded cantor. While I was listening I laughed the lung out of my body. But as every indulgence breeds remorse I thought afterwards in bed, my God how could this happen to a once proud and purposeful science.
All the tangible progress (including the work mentioned before) has been done by individual researchers and nothing has come from those globalized communities. Let them fight it out, while we move on.

98. **Anon**
October 24, 2006

"comparisons with gauge theories (where one was forced to introduce to introduce redundant descriptions) may be misleading"

So you're saying the 10 components (d(d+1)/2 components in d dimensions) of the metric tensor are not a "redundant description" along the lines of the 4 components (d components) of the vector potential of electromagnetism?

99. **Bert Schroer**
October 24, 2006

Anon
No I am not saying that; if you work with pointlike metric tensor quantum fields you presumably have to use some BRST ghosts in order to reduce to the physical cohomology even literally speaking the theory is not a gauge theory. I understand that such a formalism is presently being worked out by Brunetti and Fredenhagen. I am looking at this problem (together with Jens Mund and Jakob Yngvason) without using ghosts by setting up a perturbation theory with our string-localized metric potentials.
What I had in mind in that phrase was that in the classical theory as well as in QFT in CST the local covariance principle forces one to interpret diffeomorphisms in the active sense (this is what they mean to mathematicians) and not as a change of coordinates whereas the gauge transformations in electrodynamics are purely passive.

100. **Anon**
October 24, 2006

Active or passive, the bottom line seems to be that you allow to compute observables that are not diffeomorphism invariant, where everyone else says only diffeomorphism invariant observables are physical.

101. **Bert Schroer**
October 24, 2006

I tried to be very careful about diffeomorphism covariance and diffeomorphism invariance. The first property is the local covariance of Einstein and its very non-trivial recent adaptation to QFT (necessitating a very novel and surprising generalization of being forced to think about all globally hyperbolic manifolds at the same time even if for defining just one QFT in the new sense). Once this problem was solved the second problem of invariance (background independence) follows almost without additional sweat by converting the metric tensor into a quantum field (you only have to accept that by cohomological descend you get the operator Einstein-Hilbert like equations similar to the way you argue about the Maxwell operator equations).
It would be helpful if questions could be limited to things which are not carefully explained in the papers.

102. **Anon**
October 24, 2006

All I'm asking is for a clear explanation of why, if you allow to compute non-diffeomorphism-invariant observables, all but 2 (d(d-3)/2 in d dimensions) of the components of the metric tensor still decouple.

I don't see why the situation is any different from a gauge theory where you allow to compute non-gauge-invariant observables.

103. **Bert Schroer**
October 25, 2006

Anon
the remark about 4 constraints versus 6 propagating components was made in connection with a well-defined Cauchy problem for the classical Maxwell-Einstein equation. The purpose of that discussion was not to make analogies with gauge theories but rather to argue for the active interpretation of diffeomorphisms (in contrast to the passive interpretations in genuine gauge theories). This is basically the upshot of the Einstein hole argument
Norton, J.D.: The hole argument. Stanford Encyclopedia of Philosophy, http: //plato. stanford.edu/
entries/spacetime–holearg
The result is quite revolutionary recognition that The principle of general covariance forces one to regard spacetime points simultaneously as members of several, locally

diffeomorphic spacetimes. It is rather the relations between distinguished events that have a physical interpretation.
The adaptation of this principle to QFT (a very nontrivial step) led Brunetti, Fredenhagen and Verch to their paradigmatic change in the definition of what constitutes a QFT.
Concerning the analogy to gauge theory the following remarks are in order. The zero mass finite helicity class of Wigner representations permits to introduce pointlike field strength. For h=2 the field strength is a linear version of the Lorentzian Riemann tensor. But as for h=1 one cannot write the Wigner inner product in terms of an integral over a local density in these Field strength and last not least it is not possible to formulate ultraviolet-tame (or better "finite parametric classes" of) interactions with field strength, rather one needs "potentials" (for h=2 the analog of the metric tensor). Classically this is not a problem at all. Although I have not done the calculations (perhaps one finds this in the literature) I believe that the connection between the potentials and the field strength for the genral helicity case can be encoded into a gauge principle. For the QT the conceptual situation is very different since there is the unitarity (positivity) requirement. There are two ways to handle this problem: either you convince yourself that although pointlike-localized potentials are not possible, there are string-localized potentials (this is the way Mund, Yngverson and I propose in our joint work). In that case you face the new conceptual problem to adapt the Epstein-Glaser iteration to string-like fields. The standard way is to insist in pointlike potentials and forget about QT (the Hilbert space) in intermediate steps hoping that by some ghost extension with cohomological properties you can use all the advantages of the standard pointlike formalism and still return to physics at the end. This has worked for h=1. Whether it does so for interactions involving h-2 is the subject of present investigation. In such an approach the analogy to gauge theory in Minkowski spacetime is perfect.
This analogy is however obfuscated since GR requires an active interpretation of diffeomorphisms in the above sense of the new BFV notion of QFT demanding that the algebraic substrate of QFT is "living" on all isometrically equivalent spacetimes simultaneously! (this includes submanifolds of two given global ones which are isometric i.e. local diffeomorphism).
I ask you Anon, what is for you an observable in gravity? Is it a gauge invariant in the above setting of an h=2 particle theory? In that case the potential (metric tensor) would be gauge-dependent but the field strength (Lorenztian Riemann tensor) is gauge invariant hence observable in the sense of gauge interpretation. But according to the (always active) diff-interpretation the latter is presumably not observable.
I can of course answer your question in one sentence, but that answer would cause more conceptional damage than enlightenment.

104. **Bert Schroer**
October 25, 2006

Peter,
my response to Anon got again stuck in the spam filter

105. **Arun**
October 25, 2006

It seems to me that AQFT practitioners find the Lagrangian path integral method of QFT to be unreasonably successful. Well, if AQFT is indeed the way to do QFT, then the success (and limitations) of the Lagrangian path integral should be explainable within AQFT. Presumably the successful Lagrangian formulations are taking care automatically somehow the setting up of the unique hyperfinite type III_1 factor algebra, the modular positioning of a finite number of abstract monades, etc., etc.

Since "For higher than 3 spacetime dimensions the presently known descriptions still look somewhat concocted", but we have highly successful 4 spacetime dimensions Lagrangian path-integral QFTs, presumably making the connection suggested above will enable us to see what a not-concocted example looks like.

IMO, this kind of demonstration is the only way to convince a large number of physicists that AQFT is actually useful.

106. **Anon**
October 25, 2006

I'm sorry to say that I find Schroer's long and rambling responses impossible to follow. Peter Woit clearly thinks that Schroer's work is of such importance that he posts something on his blog, every time Schroer posts or revises one of his papers on the arXivs. Since Peter is such a clear writer, maybe he could boost the physics content of his blog by posting a summary/explanation of this work by Schroer and collaborators.

It would be a great public service.

107. **Bert Schroer**
October 25, 2006

arun,
this is all deja-vue, we (maybe without you) discussed this in this blog in April/May.
From the phrasing of your question I sense that it is declamatorical, but nevertheless I will ignore this and answer it to the best of my knowledge
1) Funct5ional integrals are extremely limited; you cannot treat QFT in CST in this setting (that is why in mathematical physics presentations like those of Wald you wan't find it) and as far as I remember i have explained why this is not possible. Question: do you think the QFT in CFT is not interesting?
2) you cannot solve any 2-dim. massive system, even in case it admits a Lagrangian presentation. Most of the systems do not even have a Lagrangian; they are characterized by a crossing symmetric unitary S-matrix (just like string theory, except that it is not metaphoric). And for chiral CFT even you would not use Lagrangians but rather the methods of AQFT (represent6ation theory of observable algebras, that is what AQFT is if you do not artificially limit it to 2 dimensions).
3) Most free fields cannot be characterized as being of Euler-Lagrange type (above spin 2 I do not know any) but their use is perfectly legitimate in causal perturbation theory (where you only use interaction polynomials in free fields) although the most important ones can
4) The Lagrangian approach where it works in your sense is an artistic device: you write something on paper in good faith, develop it in perturbation theory, see that it does not make sense (the integrals diverge) and use your hindsight and good physical sense to repair it (renormalization) and at the end you realize that you have a perfectly reasonable result which however (if you try to check whether it its into your original functional representation) fails to satisfies your original functional representation. There is nothing wrong with artistic arguments as long as one keeps some awareness about their nature. Imagine that Heisenberg would not have come up with QM in 1925. Then we would have presumable have learned clever artistic tricks (using our physical hindsight) to go somewhat beyond the H-atom and it would have been quite disastrous if we would have been satisfied with artistic explanation.
The artistic device of functional integrals (in QM it is mathematical) works because the whole idea of locality is so strongly incorporated that you cannot fail to extract the right result (if you have a good covariant formalism which was not available before 1949).

108. **Bert Schroer**
October 25, 2006

Anon
neither Peter nor I are willing to serve as your Nanny. Do you really expect that these subtle recent developments in QFT can be explained in a weblog. The only thing one can do is cite the literature and make some guiding remarks and that is what I did. The rest you have to do; there is nothing in my commentaries which is not in detail explained in the existing literature. But if metaphoric tales are more to your taste than autonomous mathematical physics, then you really should stay with string theory or something alike and join one of those globalized communities where you find people who are very experienced in telling you nice-sounding metaphors which have a higher entertainment value than what you can get from me.

109. **Anon**

October 25, 2006

OK, so give me a reference (e-print and page-number) where it is explained why all but d(d-3)/2 components of the metric decouple, despite the fact that you allow to compute non-diffeomorphism-invariant observables.

This isn't a complicated question and, if it is answered in the literature, you could avoid writing paragraphs and paragraphs of gibberish by giving a detailed reference to the answer.

110. **Bert Schroer**
October 25, 2006

Anon,
That the propagation of the metric tensor according to the E-H equation leads to 4 non-propagating constraints (Cauchy problem) is in most texts on classical gravity (e.g. Wald's book). Its use in connection with establishing the active interpretation of diffeomorphisms (in contradistinction to the passive interpretation of gauge transformation) you find in the introduction of
Fredenhagen, K., Haag, R.: Generally covariant quantum field theory and scaling limits. Commun. Math. Phys. 108, 91 (1987)
and the nontrivial adaptation to quantum matter in
http://br.arxiv.org/abs/math-ph/0112041
and a formal argument that background independence (diffeomorphism invariance) follows from diffeomorphism covariance automatically upon quantizing the metric potential (and fulfilling the operator form of the E-H equations) in
http://br.arxiv.org/abs/gr-qc/0603079
It would not be advisable to eliminate some g components (in gauge theory you would not work with the Coulomb gauge either), you rather prefer to find a BRST ghost extension and encode the elimination job into the BRST cohomological descend (or if you, like I, do not like ghosts in intermediate steps one works with string-localized metric potentials). Ergo in QFT in CST you may eliminate, in a full QFT including gravity you never do that.
The length of my previous contribution resulted only from trying to point out some subtle differences between gauge theories and background independent quantum gravitation since I had the impression you looked for a more intimate contact than a vague analogy.

111. **Anon**
October 25, 2006

10-4=6, not 2. I am asking how you propose to get from 10 down to 2.

Brunetti and Fredenhagen (gr-qc/0603079) do not address this issue. In fact, except for the very last page of their paper, they do not address the quantization of the gravitational field at all.

Nota, when they do turn to some tentative remarks on the quantization of the gravitational field, they say

"A possible obstruction could be that locally the cohomology of the BRST operator is trivial, corresponding to the absense of local observables in quantum gravity. Another problem is the fact that the theory is not renormalizable by power counting. Thus the theory will have the status of an effective theory."

And they do not try to depart from the usual condition that only diffeomorphism-invariant (BRST-invariant) observables are to be computed.

You, on the other hand, say you can compute local, non-diffeomorphism-invariant, observables.

So, I'll ask again, how is that compatible with the decoupling of all but d(d-3)/2 components of the metric? A reference to one of your papers, where this is explained (a page number, please), will suffice.

112. **Bert Schroer**
October 25, 2006

Anon
in any theory based on BRST you compute of course also non-diffeomorphism invariant objects, it is only after the cohomological descend that you find the physical objects (diffeomorphism covariance passes to diffeomorphism invariance). The elimination procedure you seem to like so much is a special treatment of constraints by solving them explicitly. This in QED would amount to the (non-covariant) Coulomb gauge approach and I have never seen any honest computation in that setting. The Arnowitt-Deser-Misner Hamiltonian framework faces this problems of constraints (but I know to little about it in order to say that what they do is an explicit elimination of components).
Could I ask you what is your interest in such horrible elimination procedures (I never advocated them, it is only in the famous classical "hole argument" that one implicitly uses them but you seem to have a monomanic interest in them).
The two helicity degrees of freedom are only manifest if you use our string-localized matrix g-potentials (in the BRST treatment they are masked through the presence of the BRST ghosts). String-localization is explained in
http://br.arxiv.org/abs/math-ph/0511042
We meanwhile know their two-point functions but we are still struggling with the conceptual problems of formulating a perturbation theory for string-localized fields.
The remark of B-F about the effective theory means that it is diff-invariant but cannot be used up to the Planck length (since the number of parameters increase with perturbative order).

113. **Anon**
October 25, 2006

Brunetti and Fredenhagen:

"A possible obstruction could be that locally the cohomology of the BRST operator is trivial, corresponding to the absense of local observables in quantum gravity."

Are you saying that they (and not just they) are wrong, and that there are BRST-invariant local observables?

Or are you saying that you can extract sensible physics (e.g. achieve the desired decoupling) while working with BRST-non-invariant local observables?

In either case, could you give me a reference for the precise statement?

114. **Bert Schroer**
October 25, 2006

Anon,
I have no opinion whether the diff-invariant theory possesses still localizable flesh (localizable monades) or whether it only contains topological (or combinatorial according to once point of view) "bones" (type II_1 algebras, the ones Vaughn Jones uses in subfactor theory leading to the Jones polynomial. And I do not advocate a

physical interpretation of BRST non-invariant objects.
The reason I am so much interested in the string-localized formulation of metric potentials is precisely to clarify this issue. In that case the g's do act in the physical Hilbert space and the only thing which could go wrong is that they generate semiinfinitely extended spacelike strings which have horrible domain properties (e.g. create infinite energy states from finite energy ones). If this is not the case then the theory has in addition to E-H diff-invariant state other states in its Hilbert space. I am pretty confident that this picture is an alternative formulation to ordinary gauge theory but I would not make any bet in the gravitational case. String localized fields are not local observables in the strict sense, but neither are the highly useful fermion or anyon fields.
In any case if that "bone"scenario of B-F turns out to be true then the Maldecena conjecture in the sense of an AdS–CFT correspondence could not be true apriori, even if you go along with that weird idea that in addition to the known holography there is another gravitational one.
I told you that I believe that B-F are working on a BRST formulation, so we have to wait and see.

115. **Anon**
October 25, 2006

Doesn't the AQFT approach rely on the existence of BRST-invariant local observables?

What can you hope to achieve in that approach, if there aren't any (which is the conventional wisdom)?

"In any case if that 'bone' scenario of B-F turns out to be true then the Maldecena conjecture in the sense of an AdS–CFT correspondence could not be true apriori …"

To the contrary: the Maldacena conjecture relies on the fact that there are no local BRST-invariant observables in quantum gravity. If there were, then the Maldacena conjecture would most certainly be false. But I don't see the point of discussing the Maldacena conjecture with you. There are plenty of experts on it, who could do that.

"I told you that I believe that B-F are working on a BRST formulation, so we have to wait and see."

Above, you made some very grand claims for the AQFT approach to quantum gravity. In the absence of a proof that the conventional wisdom, about the absence of BRST-invariant local observables, is wrong, I think those claims are overblown.

116. **Bert Schroer**
October 26, 2006

The idea to use a BRST approach is not an idea emanating from AQFT, rather certain formal analogies to gauge theory (but not a naive identification of formalism). Since it has not been done it seems reasonable to do it before entering the blue yonder of amok running speculations. There are also other conservative ideas to formulate perturbative QG which are expected to lead to an operator version of E-H-like field equations on suitably determined states.
The nice aspect of the B-F argument (only on the level of string theoretic rigor one would call it a theorem) is that any implementation which achieves that is background independent i.e. background independence is a free ride and the hard work was done before while establishing diffeomorphism-covariance.
I appreciate your admission that you do not know much about the AdS–CFT correspondence and the Maldacena conjecture (your statement about a relation between BRST and the M-conjecture is rubbish) and that you prefer to leave that matter to experts.

117. **Anon**
October 26, 2006

Perturbative quantum gravity was studied exhaustively, starting with de Wit and Feynman, and followed by many others. The BRST approach to it was introduced by de Wit. Stating results found many decades ago is not "blue yonder of amok running speculations."

As to Brunetti and Fredenhagen, as I said, the only "quantum gravity" in their paper is a page of speculations at the end. Most of the paper is about quantum field theory in curved spacetimes, a subject that was well developed when Birrell and Davies wrote their book. In their AQFT approach, B&F miss (gloss over?) almost all of the subtleties that make the subject an interesting one.

"your statement about a relation between BRST and the M-conjecture is rubbish"

No, my statement is correct.

All I'm saying is that, if you want to discuss AdS/CFT, you should do it with someone like Distler (who seems to have written quite an acute summary of Rehren's proposal), not with me.

118. **Bert Schroer**
October 26, 2006

Egbert,
the concept of internal symmetries was since its inception (the SU(2)isospin of nuclear physics introduced by Heisenberg) one of the most mysterious proposals. Whereas it is natural to accept spacetime symmetries (since they accompanied us in the classical setting since the time of Newton) the understanding of internal symmetries is a mysterious concept in QT (in classical physics you can only get them by reading back QT concepts into classical physics i.e. they are classically unnatural). This problem was finally solved in the work of Doplicher, Haag and Roberts during 1970-1990
There idea was to abstract internal symmetries by taking a dichotomic view about QFT: local observable algebra

119. **Bert Schroer**
October 26, 2006

continuation
The representation structure of interests coming from the DHR theory in low dimensions is richer, in this case the observable structure leads to representation sectors which carry a representation of the braid group (a generalization of the symmetric group) and instead of the field algebra you find something which does not permit a clear-cut separation into inner and outer (spacetime) symmetries.
Suppose now that you have gone through this analysis in a higher dimensional QFT and you compactify certain spatial dimensions and make them small. Do the correlation functions of such a QFT in the limit approach those of a QFT with smaller spacetime dimensions and a larger symmetry? Of course not. The observable algebra on such a brane becomes awkward (it develops those disagreeable physical properties which Rehren mentioned in his discussion with Distler) and its representation theory of physical interest will not develop new sectors which come from the small spatial coordinates.
What people mean by KK is a Fourier decomposition in the small coordinates in the classical theory and retaining only the lowest component; it is only there (i.e. before quantization) that KK works but it does so in a completely childish way.
What Lunsford attributed to Pauli is something slightly different (this was the reason for my interest). If I understood it correctly it says that you cannot apply KK to a higher dimensional Einstein-Hilbert theory (diff-covariant) into a lower dimensional E-H (lower dim. diff-covariance) and remaining gauge part. This sound very plausible, but it seems to be a classical statement.

120. **Bert Schroer**

October 27, 2006

My complete contribution to KK is now available at
http://kaluza-klein.blogspot.com/
I am mentioning this here because there was a mistake in my previous blog here which now has been corrected.

Corrections…

October 8, 2006
Categories: Not Even Wrong: The Book

I'm well aware that there's far too much these days on this blog about the controversy over string theory, but two things have appeared today in the press about this that aren't accurate, and I can't resist using this platform to issue corrections. Readers who have had enough of this are warned to move on to some other blog with fresher material.

The Observer (the Sunday version of the British newspaper the Guardian) has an article today by Robin McKie, entitled String theory: Is it science's ultimate dead end? On the whole, the article is a well-written piece about the controversy over string theory. I talked to McKie on the phone, and he quotes me as saying something that is probably an abbreviated version of what I actually said.

'Too many people have been overselling very speculative ideas,' said Woit – author of Not Even Wrong – last week. 'String theory has produced nothing.'

The first part of this quote is fine, but "String theory has produced nothing" is not what I think, and presumably was part of some longer statement. String theory has certainly produced some very interesting mathematics, as well as some promising ideas about strongly coupled gauge theories. It has produced nothing useful about unification and how to get beyond the standard model.

The McKie piece also has some strong quotes in defense of string theory from David Gross, Samjaye Ramgoolam and Michael Green:

'String theory is on the right path,' said David Gross, of the University of California, Santa Barbara, and another Nobel prize winner. 'But this path is quite long. Further breakthroughs are required.'

I'm kind of wondering why he claims that definitely string theory is on the right path. Perhaps he also had some caveats that got dropped.

'said Sanjaye Ramgoolam, of Queen Mary, University of London. 'There are a number of ways that we could prove – or disprove – string theory. For example, Europe's new Large Hadron Collider may well be powerful enough to provide evidence that suggests we are on the right road.'

This kind of invocation of the LHC as being able to prove or disprove string theory always strikes me as less than honest.

According to Green:

"There is no alternative to string theory. It is the only show in town – and the universe.'

Again, perhaps some caveats have been dropped here.

The second piece with inaccuracies that appeared today is a review of my book and Lee Smolin's in the LA Times by K.C. Cole. It's entitled Strung Along and is basically a hit-job on me and Smolin. Some of the things in it are so dishonest and incompetent as to be pretty hilarious:

In fact, many statements about string theory in these books are plain wrong… To say, as Woit does, that fundamental mysteries about neutrinos are being ignored will come as news to the dozens of physicists who've been working on these problems for years.

At first I couldn't figure out why she was attributing to me the insane statement that "fundamental mysteries about neutrinos are being ignored", but after taking a look at all the references to neutrinos in the book, I finally figured it out. On page 93 of the US edition I write, after giving a description of the things the standard model leaves unexplained, including a parameter count that ignores neutrino masses:

One complication that has been ignored so far involves neutrinos.

and then go on to explain about the experimental evidence for neutrino masses. The "ignored so far" obviously means "ignored so far in this chapter", not "fundamental mysteries about neutrinos are being ignored" by physicists. This recalls some of the hilarities in Lubos's review of my book. It's absolutely amazing that a supposedly serious journalist would do this kind of thing.

There are plenty more claims in the review that are pretty much the opposite of reality:

To mathematician Peter Woit and physicist Lee Smolin, however, the search for beauty is ruining physics.

Actually my view is quite the opposite: what's ruining physics is pursuing very unbeautiful theories (Susskind is fond of calling them "Rube Goldberg machines") for which there is no experimental evidence.

I've never met Cole and she knows nothing about me personally, but she seems intent on painting me and Lee as embittered failures:

Woit, and Smolin… write mostly about how string theory has ruined their careers.

I don't think there's anything in Smolin's book about how string theory has ruined his career (and he's had quite a successful one). As for me, there's no such sentiment expressed in the book and my feelings about this are quite the opposite. If it weren't for string theory, most likely my academic career would have led at best to a job at a not very good institution in a place I really wouldn't be very happy living. Because of string theory I moved into mathematics early on, and have ended up with an academic position I'm extremely happy with, living in my favorite place in the world. String theory didn't "ruin my career", it made a very happy one possible.

As I said, I don't know Cole, so I don't know why she decided to write this kind of dishonest hit-job. Perhaps it has something to do with her professional association with string theorist Clifford Johnson at USC. I've long suspected that Clifford was the author of the referee report for Cambridge which compared doubting string theory to doubting the theory of evolution, and constructed evidence that I didn't know what I was talking about by taking a sentence in my manuscript out of context and changing a word. One is often wrong about such guesses, probably I'll never know…

Update: Amazing how quickly one finds out things one thinks one will never know. Over at Clifford Johnson's blog, Capitalist Imperialist Pig asked him if he was the referee who tried to stop Cambridge University Press from publishing my book. His answer: "that's all just silly and irrelevant". OK, now I know…

The funny thing about this is that Clifford has been bitterly complaining about the fact that the book is being marketed and publicized to a wide audience, but it appears that he was the one who stopped it from being published a couple years ago in a form where it would have reached many fewer people. Priceless.

Update: Thanks to "Another Grad Student", who in the comment section over at Clifford Johnson's blog did a better job than I could of explaining to him why I was no longer bothering to respond to his endlessly condescending, sneering and dishonest comments. Anyone who thinks there is anything to the accusations Johnson and Distler are making about me over there is encouraged to read for themselves some of the many comment threads where I have tried to have serious discussions with them.

More substantively, it's clearly a waste of one's time to try and debate these issues with someone who is on record as claiming that criticizing string theory is like criticizing the theory of evolution.

Update: Clifford Johnson has denied being the CUP referee in question, or having anything to do with the Cole "review", saying here that he has not even read the book. My apologies to him for incorrect suggestions made in this posting, and my misunderstanding of his later comments.

Comments

1. **CapitalistImperialistPig**
   October 8, 2006

   Having read Cole's "reviews" I'm left wondering what books she actually read. Many of the knocks against both you and Lee Smolin seem too weird to have been generated from the contents of the books.

   I am left wondering what the nature of her "professional association" with Clifford is though. I thought she was a journalist and he was a string theorist. Can you clarify?

   Finally, have you asked Clifford if he reviewed your manuscript?

2. **J.F. Moore**
   October 8, 2006

   Naturally, I can only speak for myself, but I don't think that you are writing too much about the bubbling controversy. I think it's fascinating to see how people behave when forced to justify their beliefs.

3. **Peter Woit**
   October 8, 2006

   CIP,

   K.C. Cole also teaches at USC, and she runs an evening program about science called "Categorically Not!" which is often discussed in Clifford's blog. If you look at the web-site for the program

   http://physics.usc.edu/catnot/

   you'll see that the text is written by her and her picture is on the front, but that:

   "This page made by Clifford V. Johnson and is brought to you by the Department of Physics and Astronomy at USC."

   As I wrote, I know very little about Cole, actually the main place I had heard about her was on Clifford's blog.

   No, I've never asked him if he was the Cambridge referee.

4. **LDM**
   October 9, 2006

   It is surprising that somebody that TEACHES science journalism writes such scientific nonsense. I was of the impression that journalists subscribe to something call "journalistic ethics and standards".

   As for Woit's claim that string theory has "absolutely zero connection with experiment," experiments already planned for a new European particle accelerator will look for the existence of extra dimensions and extra families of particles — both predicted by string theory. In fact, many statements about string theory in these books are plain wrong.

   In fact, the opposite is true…and both these books are well reasoned and generally correct to the extent allowed in a general presentation aimed at a layperson audience.

   If in fact the European particle accelerator does manage to muster a meaningful test for an extra large dimension…so what? There will be a negative result (or do you really believe the world is 10 dimensional?), and so one can predict string theorists will just maintain the extra dimension is not large, and so we will be back to the case of having an untestable theory.

5. **Spear Mark the Second**
   October 9, 2006

   I know KC a little… generally, I've liked her, and I've found her to be fair and smart. But her review in this case is off base, don't know why.

   A small point about the LHC… most scattering experiments since Rutherford have looked for an unusually large cross section at high momentum transfer. Rutherford discovered that nuclei were in essence point particles, Hofstadter probed the nuclear form factors, and Friedman, Kendall, and Taylor found quarks.

   Does anyone seriously think that the only reason experimenters will look for unexpectedly large cross section at LHC is extra dimensions from string theory? Large cross sections are always sought after… don't need a weatherman (or a string theorist) to know which way the wind blows.

   Since Sam Ting in the 1970's, everyone also looks for leptons in the final state, and since the W was discovered, missing energy too. Those are by no means signatures of string theory alone, and we did not need string theorists to know to look for those signals.

6. **Spear Mark the Second**
   October 9, 2006

   Just read KC Cole's review… she messed up on the neutrino discovery as well… the real key experiment was that of Ellis and Wooster, predating Pauli. Ellis and Wooster (part of the Cavendish group) proved there was missing energy, and that the energy was not electromagnetic in nature. Given that experiment, there were only two serious choices… that energy/momentum/spin conservation was wrong, or that a new particle was carrying off the energy/momentum/spin. All the experimentalists knew that at the time, but Pauli grandstanded and made a big deal about postulating the more detailed properties of the neutrino.

7. **MathPhys**
   October 9, 2006

> It is surprising that somebody that
> TEACHES science journalism writes such
> scientific nonsense.

Why?

8. **Bourgeois Nerd**
   October 9, 2006

   Very disappointing to read that review, since I quite enjoy K.C. Cole's science writing. I have a feeling that you're right, Peter, that her relatonships (not just with Clifford, but with a lot of other string theorists) clouded her view you and Smolin's book. Also, string theory really is rather up her alley; she's all about "elegence" and "aesthetics," and, since a lot of people think that's what strings bring to the table (I, being a total outsider to such high-level math and science outside of popular articles, books, and blogs such as this, have no way to judge whether it is or, as you've argued, is not), I can see her being very, very attached to it.

9. **CapitalistImperialistPig**
   October 9, 2006

   Peter – Well, I did ask my question (re: the manuscript review) of Clifford and his response was:

   As for the other thing about Peter's guesswork… that's all just silly and irrelevant.

   In the political blogosphere, I think we call that a non-denial non-denial.

10. **Arun**
    October 9, 2006

    CIP, many thanks for asking the question!

    All of this has been extremely illuminating. Thank you, Peter, for persisting and publishing your book.

11. **wolfgang**
    October 9, 2006

    Peter,

    in the comments on the Asymptotia thread you wrote about Lubos
    "Remarkably, his colleagues at Harvard promoted him and so far have been willing to tolerate his behavior (although his recent public calls for my imminent death may change that situation)."

    When and where did Lubos call for your imminent death?
    Can you provide a link or something similar?

12. **King Ray**
    October 9, 2006

    I never liked string theory precisely because it is not beautiful or elegant. Just look at all the fields that they have living on the string worldsheet. I once computed all the maximal subalgebras of a single E8, using Dynkin diagram techniques, and it was amazing what all was in there. At that time E8xE8 was in vogue. The ultimate theory should be simpler than current theories, not orders of magnitude more complicated. To use Einstein's phrase, string theorists can calculate but they can't think. I also think there is a lot of hole drilling where the wood is thinnest.

13. **andy**
    October 9, 2006

    The webpage at USC's Anenberg School for Communication obfuscates the background of its faculty. On a science department's webpage the faculty listing will tell you what degrees the professor has and where he or she earned them. I can't seem to find that for that department.

14. **Peter Woit**
    October 9, 2006

    Wolfgang,

    The latest occurence of this was

    http://www.math.columbia.edu/~woit/wordpress/?p=467#comment-16818

    and a few days earlier he discussed his opinion that death would be too good for "the black crackpot" (that's me, color of cover of my book, Lee is "the blue crackpot") given my sins against physics. After I complained to people at Harvard, Lubos did edit this first occurrence to remove the reference to my death. A few days later he posted the above comment.

    I brought this to the attention of people at Harvard, and can report that some people there (not members of the theory group) agreed that this behavior was not tolerable.

15. **d.liman**
    October 9, 2006

    I'm assuming, since Peter brought it to the attention of folks at Harvard, that he was reasonably confident that it was likely an authentic post.

    There is a remote chance that it could just be someone who signed it with Lubos' name. On his blog he claims that this happens often.

16. **Peter Woit**
    October 9, 2006

    d.liman,

    I'm confident this came from Lubos. After it appeared I wrote in to his blog to ask him if it really came from him. He did not deny it.

17. **Anon**
October 9, 2006

Peter religiously checks the IP addresses of his commenters. I'm sure he checked this one, before reporting the incident to the Harvard authorities.

Right?

18. **Tim**
October 9, 2006

Peter,

I find it amusing that in the very same post you complain about journalists taking words and phrases out of context and interpreting them according to their own tastes without being very careful about the original intention and yet you do exactly the same by reminding us of a short phrase by Clifford Johnson without giving us the opportunity to review the original circumstances and intentions of those words.

Most funny!

Best,
Tim

19. **Peter Woit**
October 9, 2006

Tim,

Good point. Problem is I've been advised it's not really kosher to make public documents written by people who assumed what they wrote would not be made public. I'd suggest you ask Clifford to post a copy, it would be all right for him to do so.

20. **d.liman**
October 9, 2006

Thnx for the response, Peter. Keep up this great blog. Its disappointing that people like Lubos can't argue calmly, and not lose their cool. Hopefully someone in the Theory Group at Harvard will advise him how to behave properly.

21. **Gina**
October 9, 2006

Peter and Tim,
If you are talking about the referee report that Peter is attributing (with little evidence) to Johnson, I think, in this case, ethics is going the other way around. Trying to reveal the identity of an anonymous referee, certainly in public, is not so ethical. But I see no problem in posting (without distortion) the content of a referee report. (But I am not sure on this point, I'd be interested to hear if I am missing something.)

Of course, these are conventions and we can imagine a system where both the identity of referees and the reports are in the public domain.

22. **anon**
October 9, 2006

Gina, this sentence is quite fuzzy.
"Trying to reveal the identity of an anonymous referee, certainly in public, is not so ethical."
Who is trying to reveal?
Peter didn't ask Clifford. It was CIP who asked him. Are you accusing CIP of being unethical, for asking Clifford, IN PUBLIC? Clifford didn't have to answer, but he did answer. Peter FIRST quoted Clifford exactly, and then reached his own conclusions about the meaning of the quote. I don't think that is unethical.
I think for a reporter to write an article, with so many errors, and so one-sided in favor of her firend Clifford, now that is unethical.

23. **Gina**
October 9, 2006

Dear Anon

Hmmm, interesting comment. I certainly do not "accuse" anybody of anything. For a scientist to try to speculate in public about the identity of a referee is somewhat unethical, or perhaps a better word is unconventional.

Actually, Clifford did not confirm that he was the referee at all and he reacted in the appropriate way when he was asked – whether he was the referee or not – not confirming and not denying. So for Peter to reach a conclusion that Clifford was the referee, and to continue the discussion based on this assumption is not a very good logic and not very conventional/ethical. Again, I am referring to conventions for a scientist. The conventions for journalists are different.

My point was that I do not see any reason why posting a referee report is unethical. (I agree that it is not particularly common.)

This is a just little point. I suppose there is also the issue of ethics and conventions for weblog behavior. This looks like a truly a fascinating subject but I do not have anything to contribute. What can be the ethical rules for a CapitalistImperialistPig? I suppose whatever he do he cannot really be kosher.

24. **Anon**
October 9, 2006

I agree with Gina. It is definitely unethical to publicly accuse (or state that one "supects") someone of being the anonymous referee.

That puts the accusee in the untenable position of being unable to either confirm or deny the accusation.

It is not unethical to print an (unfavorable) referee report about one's own work.

The former is highly corrosive of the anonymous refereeing process (on which we all depend). The latter, not at all.

25. **Peter Woit**

October 9, 2006

I’m not the one who put Clifford on the spot about this, blame that on CIP.

I’ve suspected that referee was Clifford for quite a while now (since one day after seeing his behavior in response to challenges to string theory, and looking up over my desk and seeing a copy of his book on my bookshelf. I realized it was published by Cambridge, and a light dawned…). I finally decided to mention this publicly because I really had enough with the way he was going on about what a money-grubbing publicity hound I am, promoting my book to the general public who can not understand the subtler points of what I am saying. If it weren’t for him, the book would have been published two years ago by Cambridge, in a form aimed at and marketed to a small audience. He decided to stop that, and leave me no choice but to find a trade publisher. He has no business at all complaining about how this book was published, and it was unethical of him to do so knowing full well that he was responsible for this.

I’m pretty sure I know who the second string theorist referee was, but won’t say anything publicly about that. Unless he gets a blog and starts complaining about how the book was published….

26. **Anon**
October 9, 2006

“I’m not the one who put Clifford on the spot about this, blame that on CIP.”

Why?

You are the one who publicly stated the accusation (excuse me, the “suspicion”). CIP merely brought it to Clifford’s attention, which someone else would have done sooner or later, anyway.

And then you were the one who took Clifford’s non-committal response (the only ethical one he could make) as a confirmation.

27. **CapitalistImperialistPig**
October 10, 2006

I don’t really think it’s unethical to speculate on who anonymous reviewers are – I’ve certainly heard a lot of reputable scientists do so. Is it unethical to ask? It’s a bit hard for me to believe that it is, but if it is I apologise, abjectly but not profusely, to the gods of ethical physics, if such there be. Generally speaking, the baloney I serve on my blog *is* strictly Kosher.

In any case, Clifford had several options – to ignore my question, criticize my question for violating the sacred bond between a publisher and the authors it already publishes, or to say something noncommital. He chose the last option, but in a rather strange fashion – one that I thought looked like a politician’s non-denial non-denial.

In any case, I’m not much impressed with anonymous review anyway. I’ve had the experience of running technical review for a publication system with all reviewers public, and I don’t think the loss of anonymity was a major problem.

28. **Gina**
October 10, 2006

The whole hype issue is very much over-hyped. Let’s say that string theory had the blessing (which can quickly turn into a curse) of unproportional media attention, so what? It will not make the next major scientific step any easier (or any harder). I do not understand this issue.

29. **Marty Tysanner**
October 10, 2006

Peter,

Having occasionally “defended” you in the past, I would feel a little hypocritical in keeping silent on this. From what I have seen, I agree with Gina and Anon that you should not have publicly aired your hunches this way about Clifford’s possible role as The Referee. It tends to create a suspicion of him without objective evidence. I also agree that you should not have publicly concluded that Clifford’s non-answer to CIP showed that he really was that person. The whole thing just looks too much like dirty politics, with one candidate creating suspicion of ethical failure by an opponent, and then taking the opponent’s refusal to confirm or deny the accusation as “proof” of guilt. I didn’t think you were running a political campaign.

While I can understand how some of your experiences with Clifford could make you resentful, and it doesn’t seem unreasonable to *privately* suspect him of the role you imply, the whole idea of publicly throwing suspicion on him this way is distasteful to me. It doesn’t seem like one of your finer moments.

30. **Arun**
October 10, 2006

That LA Times review would make anyone wonder.

31. **Clark**
October 10, 2006

Jim Weatherall at [The CSW Blog] provides some numbers that, for me at least, added interesting perspective to the discussion. This will not be news to Peter, who has been the chief interlocutor there, but may be of interest to some readers.
If this falls under the prohibition of not mixing blogs, please feel free to delete this post.
One of the most illuminating of recent developments has been hearing from the experimentalists, such as Richter, Spear Mark II,
and Weatherall, who may not have been as concerned as theorists in publicizing their viewpoint and observations.
For instance, SM II’s appraisal of Pauli’s role in the neutrino hypothesis suggested a consensus among the experimentalists
of which I must concede having been unaware.

32. **Thomas Love**
October 10, 2006

Searching for K C Cole led me to:

http://www.ncswa.org/archive/workshops/2004/3cole.html

Opening talk by K.C. Cole, Los Angeles Times

By Elise Kleeman

"Really good science writers need to lie, cheat, and steal, said K. C. Cole in the first plenary of the workshop. She outlined 15 rules for writing in her talk, but focused most on the value of lying."

That says a lot about her review, does it not?

33. **woit**
October 10, 2006

Marty,

Perhaps you're right. It's certainly true that I was highly annoyed about Clifford's recent behavior on his blog and because of that my judgement at the time may have not been the best. But still, I think mentioning my suspicions can be justified, even beyond the grounds I gave earlier that Clifford should not be criticizing how this book was published unless he is willing to have his own role in that story examined.

Part of the story of string theory is the story of how it has acquired and kept its dominant position. Some string theorists claim that this is purely through its "triumph in the marketplace of ideas", but, especially recently as a critical point of view on string theory has gained traction, less than ethical tactics have been used to try and discredit me and Smolin. I suppose I should be extremely careful of my own ethics. The question about Clifford is bit of a tricky one, since it has to do with what tactics are acceptable in order to expose less than ethical behavior and attempts to censor legitimate scientific views.

34. **woit**
October 10, 2006

Thomas,

That's pretty funny. One hopes that her "rules" (which seem to include "eschew objectivity and quote out of context") are tongue-in-cheek, but I'm not so sure…

35. **TheGraduate**
October 10, 2006

"Quotes are always out of context"

I guess that explains the way she used the neutrino quote which for me was bizarre.

36. **LDM**
October 10, 2006

The problem is Cole is writing on topics she probably does not deeply understand, and hence cannot really explain well…and instead of lying and cheating, better to write nothing at all…but if she were to need an example of honest science writing, she should read Feynman.

The Feynman Lectures are maybe the best examples of how to explain ,without lying and cheating, advanced concepts — accurately — to an not particularly advanced audience.

…When explaining things, Feynman did omit the details of lengthy calculations (that would normally be found in advanced texts or papers on the topic he was discussing) so he could concentrate on the PHYSICS…
(For a concrete example, see the discussion of the Molecular Hydrogen Ion in Feynman vol 3– all results are complete and correct, but many details of the standard calculations, normally laborious (Born- Oppenhemeir, etc) are simply not given)

37. **Michael Bacon**
October 10, 2006

"And then you were the one who took Clifford's non-committal response (the only ethical one he could make) as a confirmation."

I know I'm missing something here, but why was it the only ethical statement Clifford could make? Even if there are certain prohibitions on publically acknowledging that one was a referee, I believe that there must be limitations on this type of prohibition in the context of subsequent controversies that may arise; perhpas like this one. In such cases, a simple acknowledgment or deniel would settle the issue, and no real violence to ethics would be done.

38. **Peter Woit**
October 10, 2006

Michael Bacon,

In general I don't think there are prohibitions against a referee revealing his or her identity, either publicly or privately. Anonymity is there to allow the referee to give an honest and unbiased opinion without having to worry about having to suffer repercussions because of this. That's why Marty is right it is not a good idea in general to try and pierce this anonymity.

On the other hand, anonymity of referees has a dark side, with anonymity allowing a referee to behave unethically, knowing that he or she won't have to answer for less than completely professional behavior. So, it's not always so simple….

39. **Gina**
October 10, 2006

I agree that the issue of refereeing is not simple and sometimes loaded. In fact, almost every negative referee report is a little controversy. A completely open system may be an option, a "double blind" system is another option. But I do not see a simple solution except the universal advice: "take it easy".

I read Cole's review and it does not look to me as that negative. It is critical both to string theory and to the books by Lee and Peter, and this looks reasonable.
There were 20-30 reviews on the book, most are positive, a few (like mine) are mixed or negarive. I do not think it is right or wise, Peter, to regard negative reviews as "hit jobs" (it is also completely meaningless) and to "go after" the people who write them. If you want that people will listen to your criticism you should be ready to accept criticism, and if your success rate with string theorists or other sientists will be as high as with science journalists you will be in a good shape :).

40. **John Ramsden**
October 10, 2006

Apologies if it's already been mentioned, above or in another article in this blog, but last week Bert Schroer issued a new version of his anti-string-theory polemic (or "samisdat" as he calls it – I must look up that word) at http://www.arxiv.org/abs/physics/0603112.

Before rereading the paper, I was rash enough to mention its revised release in a footnote to one of my replies on Lubos Motl's blog (http://motls.blogspot.com/) but this reply mysteriously disappeared within minutes. However, the mystery was solved when I did get round to reading it the next day, and reached page 21 😉

Cheers

41. **Arun**
October 10, 2006

"Woit, and Smolin in "The Trouble With Physics," write mostly about how string theory has ruined their careers" – balance is not being equally negative or positive about both sides – i.e. "It is critical both to string theory and to the books by Lee and Peter, and this looks reasonable."
It wouldn't matter if Cole was uniformly critical of Lee and Peter and uniformly supportive of string theory, as long as she didn't misrepresent what the books were about.

Whether or not a book is worth reading is opinion, and one can have any opinion. But what is in the book or not in the book should not be lied about.

42. **Gina**
October 10, 2006

Arun,

indeed this quote striked me as very unfair but I double checked and it is not the full quote. Here it is:

"These issues are well worth addressing, which makes it all the more disappointing that Woit, and Smolin in "The Trouble With Physics," write mostly about how string theory has ruined their careers — and physics as well."

I think it is correct to characterize Peter's position as claiming that string theory ruined particle physics and this is what Peter mostly writes about, in the polemic part of the book. (As I already said the, 80% of the book is an excellent and inspiring description of particle physics and related mathematics.) Now, the part about string theory ruining Peter's career is an interpretation of Cole of what Peter tells in the book about himself. This is what Cole reads between the lines. It is a legitimate interpretation (for a journalist). I do not agree with this simplistic interpretation and would not have written it myself – but I am not a journalist.

Overall, the sentence is somewhat unfair but not nearly as much as seen from cutting the last words "– and physics as well".

Anyway, overall Cole's review is not that negative, and overall she is doing a good job describing the controversy, and overall, even if there are a few cheap shots here and there it is wrong and unwise (and a waste of time) to "go after" people who write negative reviews.

43. **anon**
October 10, 2006

Gina, I've never, ever heard Peter say that string theory ruined his career. In fact, Peter sounds to me as happy with his career as a pig in mud. Cole's sentence is groundless, a calumny, a character assassination; what she means is that Peter criticizes string theory because he is bitter; that his criticism has almost nothing to do with any shortcomings of string theory .

44. **Gina**
October 10, 2006

OK, I agree that Cole should not have written as a fact that the books are about string theory ruining Peter and Lee's careers even if she believes or just conjecture that this is what is 'going on'. (I criticize Peter himself for mixing too much the factual matters, his own interpretations, and unrealeted "gossip items", and I certainly do not like when journalists do it. But they do it all the time.)

45. **jeremy**
October 10, 2006

If someone wrote a review for my book, I would first thank him or her for at least taking time to read the book. I agree with Gina that it is not wise to "go after" the reviewer. It is reasonable to correct reviewer's misquotes and, sometimes calculated, distortions, but consider a review with different opinion as "a hit-job" is, for me, not prudent. It is really unnecessary for Peter being overly sensitive to criticism. Any review, positive or negative, will only make more people become aware of "the failure of string theory", and increase the possibility of bringing in fresh air to the research. It is a good thing that someone is willing to read your book and writing a review about it. I would suggest Peter, borrowing a line from Gina. "take it easy" on the reviewers.

46. **CapitalistImperialistPig**
October 10, 2006

Gina,

Your defense of KC Cole's review does not conform to a reasonable interpretation of the facts. Neither does her review. It wasn't merely biased, it was deeply dishonest.

Writers (and actors, painters, and others) are totally justified in criticizing their critics, especially those who don't evaluate them honestly.

Since you have strongly insinuated yourself in this debate, including questioning my ethics, let me ask the following: what is your expertise? Are you a string theorist? A physicist?

47. **LDM**
October 11, 2006

I went to the library, examined one of Coles books, "The Hole in the Universe" for myself just so I could accurately gauge her level of science writing first hand — and the content is watered down to the extent of either being meaningless or, unfortunately, just physically misleading if not wrong…

Also telling is that on the cover was a blurb by string theorist Briane Greene…So obviously an endorsement by Greene is not worth too much.

Essentially, she is a reviewer who does not understand the subject , has no problem distorting (lying) the subject, and seems to be strongly associated with string theorists. Interesting.

48. **John A**

October 11, 2006

> More substantively, it's clearly a waste of one's time to try and debate these issues with someone who is on record as claiming that criticizing string theory is like criticizing the theory of evolution.

Actually that's been done before in climate science. To criticize Greenhouse Warming as a hypothesis that makes no useful testable predictions gets the critic compared to creationists (and deniers of evolution therefore) and deniers of the Holocaust. It doesn't get more tasteless than that.

Now again, I do not make the case that Peter Woit agrees with my views on climate science, only that for reasons that appear to me obvious, the same dynamic of attacking the person rather than engaging in the actual questions of substantial scientific import are occurring in disparate fields.

BTW Peter, I bought Lee Smolin's book rather than yours because I'm frankly not up to the math and I felt happier dealing with Smolin's more general approach. I'm also reading QED by Richard Feynman, and find myself chuckling over Feynman's direct attack on theories that don't predict anything no matter how beautiful they are.

49. **Arun**
October 11, 2006

Gina,

If you are correct then it should have read:

"These issues are well worth addressing, which makes it all the more disappointing that Woit, and Smolin in "The Trouble With Physics," write mostly about how string theory has ruined physics, though they disclaim any impact on their careers.

50. **Gina**
October 11, 2006

Well guys, appart from the controversial sentence and misunderstanding about neutrinos, Coles review is good:

Take this one sentence explanation of string theory:

"Simply put, string theory does this by replacing point-like particles with tiny strings of some fundamental stuff vibrating in 10-dimensional space — their harmonies creating everything from quarks to galaxies. The loops of string don't let anything get small enough to let quantum fidgeting rip space and time apart."

and the critique on string theory

"String theory has its troubles, which the authors analyze in great and sometimes lucid detail: It appears to be untestable because the strings are too small to be seen, and recent research suggests that the theory may have an infinite number of solutions, so it can't make predictions. And string theory is so ill-defined that even ardent supporters admit they don't know what, exactly, it is."

and

"The authors are right to say that physicists can get cliquish; that some of them swagger; that they frequently fool themselves and that science has become too risk-averse."

Arun,
maybe she should have change the ordering and write

"These issues are well worth addressing, which makes it all the more disappointing that Woit, and Smolin in "The Trouble With Physics," write mostly about how string theory has ruined physics — and their careers as well."

It is legitimate for Cole to think that the books express personal frustration
even if not explicitely expressed. She does not have to prove it, and it is common not to distinguish facts from interpretations, this is journalism and not science. (But I agree that even in this form it is unfair to Peter and Lee and not referring to the careers would be a much better choice.)

51. **Bert Schroer**
October 11, 2006

"Simply put, string theory does this by replacing point-like particles with tiny strings of some fundamental stuff vibrating in 10-dimensional space — their harmonies creating everything from quarks to galaxies. The loops of string don't let anything get small enough to let quantum fidgeting rip space and time apart."

Well, this mantra with which string theorists used to start their talk is totally metaphoric, the fact is that the intrinsic quantum localization of the Nambu-Goto string is pointlike and not stringlike (see respective remarks in my samisdat contributio).

52. **Renormalized**
October 11, 2006

"Simply put, string theory does this by replacing point-like particles with tiny strings of some fundamental stuff vibrating in 10-dimensional space — their harmonies creating everything from quarks to galaxies. The loops of string don't let anything get small enough to let quantum fidgeting rip space and time apart."

This would be wonderful if it were true. The truth is we have never seen a string, never had a experiment which inferred there were strings, never have seen a dimension beyond the 3 space and one time. We have never seen this so called "fundamental stuff". We have never heard the so called "harmonies". We have never found a loop of string. This is what you have never gotten in your head Gina. It is all a fantasy! This is what happens when you get one good idea about how the universe "might" work and then have many brilliant people working on it for many years, teaching brilliant students to work on the idea and then forgetting it was just a idea that "might" work.

53. **DMS**
October 11, 2006

Peter,

No point wasting time "debating" these public string theorists(I liked InvestorBanker's comments as well). You are winning the argument, both on the substance, and on style.

On the one hand we have Jacques, who thinks his use of straw man arguments, italicizing comments, and use of "Huh?", makes him sound so brilliant.

And then we have Clifford, who makes the initial pretence(a nice front) of trying to be “serious” and “respectful” and then go down the same path.

No point in bringing on the others.

Frankly, this dishonest conflation of string theory for RHIC and string theory for the Theory of Everything is pathetic.

Simply ask these public string theorists one question: What exactly does string theory say we will see at the LHC???

Finally, these string theorists think they are the among the most brilliant people on the planet. With the singular exception of Ed Witten, again this is hype.

DMS

54. **Bert Schroer**
October 11, 2006

As a mathematical theorem this is true for the quantized N-G string.
The proof is actually quite simple, you only have to understand a deep theorem which relates the positive energy representation of the Poincare group inexorably with localization of states (totally intrinsic)
http://br.arxiv.org/abs/math-ph/0203021
http://br.arxiv.org/abs/math-ph/0511042
and convince yourself that in the decomposition of the highly reducible representation of the Poincare group of the N-G model (take the supersymmetric version in order to avoid tachyons) there is no massless “infinite spin” component (which would introduce string-like localization). Since the the physical field (after BRST descend) is a c-number, the quantized N-G “field” is actually a generalized free field with the well-known mass-tower particle spectrum. The confusion of classical localization with intrinsic quantum localization is an unfortunate heritage of the Atiyah-Witten geometric way of looking at QFT.
For mor on this I refer to my eassay.
And by the way, the only polemic I permitted myself was to the Lord of Misuse. My illustration of how ST manufactures facts is no polemic at all, it is based on 3 original refences to ST sources.

55. **Bert Schroer**
October 11, 2006

Sorry for the two misprints

56. **Ari Heikkinen**
October 11, 2006

Just to point out that not every negative review of your book is a conspiracy of string theorists and their friends, Peter.

57. **Bert Schroer**
October 11, 2006

The corrected phrase for
“Simply put, string theory does this by replacing point-like particles with tiny strings of some fundamental stuff vibrating in 10-dimensional space — their harmonies creating everything from quarks to galaxies. The loops of string don’t let anything get small enough to let quantum fidgeting rip space and time apart.”

should read:
simply put, string theory does this by replacing point-like particles by quantum objects which are obtained from the quantization of classical supersymmetric Nambu-Goto-like strings (which still remain pointlike-localized in the autonomous quantum sense) whose spectrum in ten spacetime dimensions is described by an infinite tower of pointlike particles…….

58. **The Graduate**
October 11, 2006

Peter,

How come you haven’t mentioned your interview with Horgan?

59. **Peter Woit**
October 11, 2006

The Graduate,

Partly because there’s been a lot to keep track of, so some things like this haven’t gotten mentioned, partly because I find the idea of myself on video kind of appalling. Bad enough that there’s audio out there of me, I just got used to that. Anyway, rumor is there’s an interview by Horgan of me on this web-site

http://www.stevens.edu/csw/cgi-bin/index.php

I’m not going to watch it, I suppose others could if they must…

60. **TheGraduate**
October 11, 2006

Peter,

It was a good interview (better than I would have done). I don’t like watching myself on video either!

61. **Boaz**
October 11, 2006

Peter,
I enjoyed reading your book.
One section I was particularly interested in were your comments on John Hagelin/TM/String Theory etc. You mentioned that particle theorists used to put up the MIU/MUM posters connecting fundamental physics to Maharishi’s Hindu concepts on their doors. Was this mostly done because they actually thought it was cool/interesting/insightful, or was it more a joke- as in “look at all the craziness out there!”?

Thanks,
Boaz

62. **woit**
October 11, 2006

Boaz,

It was definitely a joke. I think…

63. **Boaz**
October 11, 2006

Thanks…
I figured so, but that was the 80's, so you never know.

64. **Chris Oakley**
October 12, 2006

The Maharishi, who used to place full-page ads in national newspapers, was an important bellwether in deciding what was was "cool" in particle physics. It started with SU(5) GUTs, moving on to Supergravity, and finally to Superstrings. If you were a HEP theorist, but not working on the topics he mentioned, then your academic career was probably going nowhere.

65. **Boaz**
October 12, 2006

That's interesting Chris.
I wonder if it was solely due to John Hagelin, or whether the Maharishi actually knew anything technical about this stuff. I know that he studied physics at least to the undergraduate level.

Rube Goldberg's Instruction Manual

October 11, 2006
Categories: Uncategorized

Since string theory first became popular in 1984-5, attempts to connect it to particle physics have suffered from various problems. One of the most severe of these goes under the name of "moduli stabilization". Six dimensional Calabi-Yau manifolds come in families, parametrized by "moduli". The dimensions of these moduli spaces can be of order 100 or so.

Naively it might appear that string theories are characterized by a choice of a topological class of Calabi-Yaus (no one knows if the number of these is finite or infinite), and then a choice of each of the 100 or so parameters that fix the size and shape of the Calabi-Yau. According to the standard string theory ideology, this is not the right way to think, instead there is really only one string theory, with different moduli values corresponding to different states. The moduli parameters are supposed to be dynamical elements of the theory, not something parametrizing different theories.

The problem with this is that if you promote the moduli to dynamical fields, they naively correspond to massless fields, and thus give new long-range forces. So you have to explain away why we don't see 100 or so different kinds of long-range forces, and the experimental bounds on such forces are very good. Some kind of dynamics must be found that will "stabilize moduli", giving them a non-trivial potential. The moduli fields will then be fluctuations about the minima of this potential. If the quadratic piece of the potential is large enough, their mass will be high enough to have escaped observation.

One needs a potential with non-trivial minima, and has to ensure that the dynamics is not such that the moduli will run off to infinity. In recent years, ways of achieving this have been found that typically involve "flux compactifications", i.e. choosing non-trivial fluxes through the topologically non-trivial holes in the Calabi-Yau. On the one hand, this seems to provide a long-standing solution to the problem of how to stabilize the Calabi-Yau, on the other hand, it appears that there is an exponentially large number of possible minima. This is the origin of the "Landscape" and the associated claims of 10^{500} or more possible vacuum states for string theory.

The constructions involved are famously exceedingly complex and ugly, with Susskind referring to them as "Rube Goldberg machines", and one of their creators, Shamit Kachru, the "Rube Goldberg architect". Very recently a new Reviews of Modern Physics article by Kachru and Douglas called [Flux Compactification] has appeared. It can be thought of as a manual describing how to construct and count these Rube Goldberg machines.

Many string theorists had long hoped that whatever method was found to stabilize moduli would have only a small number of solutions. Then, in principle one would get only a small number of possible models of particle physics for each topological class of Calabi-Yaus. If the number of these was finite and not too large (the known number of constructions is something like 10^5-10^6), then to see if string theory could make contact with particle physics, one would just have to do a moderately large number of calculations, check them against the real world, and hope that one matched. If it did, it would then be highly predictive.

The existence of the flux compactifications with stabilized moduli described in the Douglas-Kachru article has convinced many string theorists that this old dream is dead. Some have tried to claim that this is a good thing, that the exponentially large number of states allows the existence of ones with anomalously small cosmological constants, and thus an anthropic explanation of its value. The problem then becomes one of how to ever extract any prediction of anything from string theory. Small CCs are achieved by very delicate cancellations, and it appears to be a thoroughly calculationally intractable problem to even identify a single state with small enough CC.

Many string theorists are now claiming that this is not really a big deal. So what if there are lots and lots of string theory vacua, it's just like the fact that there are lots and lots of 4d QFTs! For arguments of this kind, see recent comment threads here and here. There's something fishy about this argument, since discussion of flux compactifications has from the beginning focused on whether it is possible to use them to make predictions, whereas no one ever was worrying about whether (renormalizable) QFTs were predictive or not.

The source of the problem lies in the combination of the large numbers of string theory vacua with their Rube Goldberg nature. Consistent 4d QFTs are characterized by a limited set of data (gauge groups, fermion and scalar representations, coupling constants), and it has turned out that among the simplest possible choices of such data lies the Standard Model. String theorists commonly describe the Standard Model as "ugly", but it is among the simplest possible 4d QFTs, and is extremely simple and beautiful compared to something like the flux compactification constructions. One could hope that while flux compactifications are inherently rather complicated, one of the simpler ones might correspond to the real world. As far as I know there's no evidence at all for this, such a hope appears to have nothing behind it besides pure wishful thinking. Some string theorists like Douglas and Kachru don't seem to think this is possible, focussing instead on statistical counts of more and more complicated flux compactifications, hoping to find not a simple one that will work, but a statistical enhancement of certain complicated ones that would pick them out.

4d QFT is a predictive framework not because the number of possible such QFTs is small, but because our universe is described extremely accurately by one of a small number of the simplest of such QFTs. A few experiments are sufficient to pick out the right QFT, and then an infinity of predictions follow.

Is the QFT framework falsifiable? One could imagine that things had worked out differently, that instead of the Standard Model predictions being confirmed, each time a new experimental result came in, one could only get agreement with experiment by adding new fields and interactions to the model. It might very well be that the QFT framework could not be falsified, since one could always evade falsification by adding complexity. This happens very often with wrong ideas: they start with a simple model, experimental results disagree with this, but can be matched by making the model more complicated. As new experiments are done, if the original idea is wrong, it doesn't get simply falsified, but the increasing complexity of the models needed to match experiment sooner or later causes people to give up on the whole idea.

This is very much what has happened with string theory. The simple models that got people excited about the idea of string theory unification don't agree with experiment, with the moduli stabilization problem just one example. It appears one can solve the problem, but it's a Pyrrhic victory: one is forced into working with a class of models so vast and so complicated that one can get almost anything, and never can extract any real predictions.

Douglas and Kachru do address the question of whether one can ever hope to get predictions out of this class of models, but their answer is that they can't think of any plausible way of doing so. They mention various things that people have tried, but none of these ideas seem to work. The best hope was that counting vacua with different supersymmetry breaking scales would lead to a statistical prediction of this scale, but this has not worked out for reasons that they describe. In the end they conclude:

For the near term, the main goal here is not really prediction, but rather to broaden the range of theories under discussion, as we will need to keep an open mind in confronting the data.

This acknowledges that no predictions from this framework seem to be possible, and that continuing work in this area just keeps producing yet wider and wider classes of these Rube Goldberg machines. They are suggesting basically giving up not on string theory, which would be the usual scientific conclusion in this circumstance, but instead to for now give up on the theorist's traditional goal of making testable predictions. They advocate not giving up on string theory no matter how bad things look, instead just continuing as before, hoping against hope that an experimental miracle will occur. Maybe astronomers will find evidence for cosmic superstrings, maybe the LHC will see strings or something that matches up with characteristics of one of the Rube Goldberg models. There's not the slightest reason to believe this will happen other than wishful thinking, which has now been promoted to a new program for how to do fundamental science.

Update: Via Lubos, for those who don't know what a Rube Goldberg machine is, two examples are here and here.

Comments

1. **Michael**
October 11, 2006

>> This is very much what has happened with string theory.

You are factually incorrect. The flux vacua were always there, it just took researchers some time to realize it. No one has ever added a single complication to string theory, because it is not possible. If you want to discover something in string theory it had better be there in the first place.

2. **anon**
October 12, 2006

Michael said (sort of):

You are factually incorrect. The flux vacua were always there, it just took our brothers some time to realize it. No one has ever added a single complication to the Bible, because it is not possible. If you want to discover something in the Bible it had better be there in the first place.

Hallelujah, brother Michael

3. **M-theory**
October 12, 2006

Michael is right: this problem could have been realized or advertized many years ago, and he would have spent all these years working on something better. This situation can be extremely disappointing, but it is not Peter's fault. So, please stop fighting on each word.

4. **Joseph Conlon**
October 12, 2006

Dear Peter,

I strongly disagree with your claim that

>one is forced into working with a class of models so vast and so complicated >that one can get almost anything, and never can extract any real predictions.

In the IIB flux framework there is by now a substantial literature on studying supersymmetry breaking, computing soft terms, running them down to the TeV scale and analysing the phenomenology of the resulting sparticle spectrum.

There are also `generic' results: if moduli are stabilised by non-perturbative effects, as occurs in all these models, the moduli mass is lifted above the gravitino by a factor ln(M_P/m_{3/2}) while the corresponding gaugino mass is lowered by a similar factor.

Generally different methods of moduli stabilisation are quite specific, and have typical outcomes and mass scales, and I do not see any evidence at a technical level for the claim that one can get almost anything.

Best wishes
Joe Conlon

5. **Bert Schroer**
October 12, 2006

Peter,
"It might very well be that the QFT framework could not be falsified, since one could always evade falsification by adding complexity."
The framework of QFT is of cause falsifiable. One of the proud achievements of particle theory of the 50s and 60s was the derivation of structural consequences of micro-causality: the adaptation (rigorous derivation from the principles, not just perturbative checks) of the Kramers-Kronig dispersion relation. The experimental check in high-energy scattering experiments was carried out and QFT passed with flying flags. Micro-causality is the epitome of QFT and the experimental test of dispersion relations is one of the finest falsifiability tests of QFT (would have been worthwhile to be mentioned in your book, but by the time you entered physics this nice conquest was already taken for granted).
With specific models of QFT this is of course harder since in the present state of developments there are no hard (nonperturbative) facts one can abstract from Lagrangian names (or similar model characterizations) if one still does not know whether there is a living child behind that name (this is of course infinitely worse in ST).
A good illustration of this difficulty is the previous discussion (with David Berenstein) about the AdS–CFT structural theorem (crystal-clear) and the Maldacena conjecture (which is a statement about one of those baptized models in the limit N–>infinity where it slips out of the framework of QFT).

6. **Arun**
October 12, 2006

There are myriads of QFTS, need experiment to make the choice. There are myriads of flux compactifications, need experiment to make the choice. It sounds fishy but the fishiness is elusive. If we forget that low energy physics was supposed to be a prediction of string theory, and simply say, here are two methods of constructing models – QFTs, string compactifications – and there are many choices in each – where is the fishiness in that?

Somehow there is a difference between pre-Revolution and post-Revolution thinking, but I can't put my finger on it.

7. **Arun**
October 12, 2006

Let us imagine that string theory is true and we live in a multiverse. Let us imagine, in a science-fiction way, we are able to communicate with intelligent agents in another universe. Suppose our task is to find out what is the low energy content of that other universe. We are provided the results of collider experiments from that other universe and to request specific experiments be performed. We would build the model via QFT, not via flux compactification – even though flux compactification is the "true" !

8. **D R Lunsford**
October 12, 2006

The clear lesson of this entire episode is that one cannot do physics by exhaustion of a mathematical idea. The world has to guide the choice of ideas, the ideas cannot be forced onto the world. A corollary is that a successful idea must be backed up by a clear metaphysics – both quantum theory and relativity needed Democritus first.

My personal hero in the history of science is Kepler. He did not allow his personal love for his shell model, based on the regular Platonic solids, to stand in the way of dealing with the facts. Even more than Bacon, this set the tone for how to do science. Invent, but pay attention.

-drl

9. **woit**
October 12, 2006

Joe,

Are you claiming that you always will get the relation between moduli, gravitino and gaugino masses that you mention? So, if we measure these masses and don't see this relation, string theory is just incorrect?

Or, are there some vacua where this relation isn't true?

10. **Joseph Conlon**
October 12, 2006

Dear Peter,

My comment applied for the IIB flux vacua, where complex structure moduli are stabilised by fluxes and the K\"ahler moduli are stabilised by non-perturbative effects. It is these vacua which are the principal basis for the claims about the landscape. If this small logarithmic hierarchy does not exist, then I do not see how these vacua can describe nature.

The point is that although there may be 10^500 ways of turning on fluxes, that doesn't mean that every feature of the low energy theory can take 10^500 values: there can be, and are, aspects that are insensitive to the fluxes.

There are string vacua where this relation isn't true – for example exactly supersymmetric compactifications or if you stabilise moduli perturbatively.

Best wishes
Joe

11. **T.**
October 12, 2006

As a person who used to work on this subject and switched to more formal problems in ST, let me make the following comments.
1) Flux compactifications exist in the literature since the 1996 paper of Polchinski and Strominger and were ignored for a long time for a very good reason: they have no CFT description, therefore they are not "calculable" by using standard ST techniques.
2) Around 2001 several respected string theorists, including Kachru, largely ignored this problem (or argued that in some special cases one can circumvent it) and started using the effective field theory description (EFT) for moduli stabilization etc. Then Douglas started counting such EFT vacua, and the landscape bandwagon took off (taking with it a whole generation of students).
3) It should be made clear that 99.9% of work on flux compactification, including those of the authors of the review, KKLT, Joe's etc, is made within (quite an eclectic) EFT.
4) There is no reason to believe that EFT is a good (or any type of) approximation because alpha' and loop corrections are out of control — we simply do NOT know how to compute them, so there is no reason to believe that the landscape of 10^??? vacua really exists. I can guarantee the existence of only one "non-standard-model" ST vacuum: maximally supersymmetric toroidal compactification.
4) In conclusion, the landscape has no solid theoretical foundations and can be safely ignored. If you are seriously interested in these topics, you should work on developing a full-fledged ST description of flux compactifications (RR backgrounds etc…)

12. **Joseph Conlon**
October 12, 2006

Dear T.,

I find this statement a bit odd,

>4) There is no reason to believe that EFT is a good (or any type of) >approximation because alpha' and loop corrections are out of control — we >simply do NOT know how to compute them, so there is no reason to believe >that the landscape of 10^??? vacua really exists.

Effective field theory has been used in discussions of string compactifications ever since the subject started, so I don't see any principled objection to its use in flux compactifications that doesn't also apply to its use in (say) old-style unfluxed heterotic models.

With regard to alpha' and loop corrections, I don't see what is wrong with the conventional statement that in a regime of large volume and weak coupling, the alpha' and g_s expansions are ordinary weak-coupling perturbation expansions, and so can be controlled.

Best wishes
Joe

13. **T.**
October 12, 2006

Joe,
Even if alpha' corrections are "small" in what you call "large volume" limit, you cannot prove that they do not destabilize the vacuum. Landscape counts all vacua, with {\em massless} moduli — and these can be destabilized by "infinitesimal" corrections. On the other hand, if you construct a model with all moduli (including large volume) stabilized (i.e. massive) then your EFT calculations make sense, similarly to old heterotic constructions involving gaugino condensation (assuming that the dilaton is stabilized in the weak coupling regime, which is very difficult to achieve). But then it's not landscape — it's model-building.

14. **r hofmann**
October 12, 2006

Dear Joe,

being (for a good reason) quite ignorant about how fluxes and nonperturbative effects stabilize the moduli of a type IIB model my naive question is: Where do the fluxes come from, what governs THEIR dynamics (I guess only their existence is guaranteed by topological arguments) and what precisely hides behind the mysterious-sounding term `nonperturbative effects´?

By the way, the reason why I don't even believe in supersymmetric field theory and the SM's Higgs mechanism
is the emergence of spin-1/2 excitations in the completely
confining phase of pure SU(2) or SU(3) YM. The mass of the stable and charged excitation essentially is given by the Yang-Mills scale . I have not yet an answer to the question why Yang-Mills scales are set into a hierarchical pattern but I'm pretty much convinced of the nonexistence of gauginos, gravitinos, sfermions, and fundamental scalars and SCALARINOS.

With my best regards, rh

15. **Ahmad**
October 12, 2006

Hi Peter;
Im an electrical engineer, but Im really fascinated by physics. I have read your book and that of Smolin. I really find it hard to imagine that even with (Sociology) and its dynamics, the physics community can be so naive. How can Witten et al, be allured and not know it, if the facts are so clear to you and others. What do they have to say to what you call a naive assumption about the moduli? if even a Fields medalist agrees to use it, how come ?

16. **Peter Woit**
October 12, 2006

Ahmad,

I don't know what Witten thinks of these flux compactifications. He hasn't worked on them, my unsubstantiated guess would be that he would like them to not exist or find a reason to show that they aren't real vacua of string theory, but he doesn't know how.

I think the whole idea of trying to get physics out of these things is clearly unworkable, and I suspect more than a few string theorists feel the same way. Quite a few of them definitely don't work on this area, and generally don't want anything to do with it. The problem for them is how to answer the argument that these things are something that string theory seems to inevitably lead to. Commenter "T" here gives one attitude towards this.

17. **anonymous**
October 12, 2006

Actually, Witten wrote one of the foundational papers on flux
compactifications (arXiv:hep-th/9906070) with Gukov and Vafa.

And almost all string theorists have worked on the subject, as the
gravity duals of field theories arising in AdS/CFT are flux vacua, the
simplest being AdS five times a 5 sphere.

18. **Peter Woit**
October 12, 2006

anonymous,

I was referring not to general flux compactifications, which would include AdS/CFT, but to the ones discussed in the Douglas-Kachru paper as providing a way to stabilize moduli.

Thanks for the reference to the Gukov-Vafa-Witten paper. Again, I was thinking of the more recent constructions with stabilized moduli, but you're right to point out that that is part of the story.

19. **anonymous**
October 12, 2006

The GVW paper is actually about precisely the kinds of fluxes used in the moduli potentials. Witten wrote more papers about this (eg about G2 models with flux). So did Vafa.

And, the fluxes that are crucial in confining examples of AdS-CFT, are precisely used in the moduli fixing constructions. See Klebanov-Strassler.So, the subject cannot be divided up between AdS CFT fluxes, and the ones used in the constructions you dislike.

20. **Jean-Paul**
October 12, 2006

You are wrong. The GVW abstract reads:
"We consider F/M/Type IIA theory compactified to four, three, or two dimensions on a Calabi-Yau four-fold." Lanscape studies are based on IIB orientifold compactifications to four dimensions, focussed on totally different aspects of compactifications.

21. **anonymous**
October 12, 2006

Look up the meaning of f theory. The f theory compactifications to 4d are another name for IIB orientifolds in 4d at generic points in their moduli space. The GVW superpotential is the one used in ALL these models, with equal justification.

22. **Joseph Conlon**
October 13, 2006

Dear T.,

>On the other hand, if you construct a model with all moduli
>(including large volume) stabilized (i.e. massive) then your EFT
>calculations make sense,

But this is what almost all/most/a lot of the work on the `landscape' is about. As soon as you have the Gukov-Vafa-Witten superpotential, you are stabilising the dilaton and complex structure moduli, and then nonperturbartive terms come in to stabilise the K\"ahler moduli. You have to check the EFT for consistency.

I do agree with you that this is basically model-building, and I think that is a good thing,

Best wishes
Joe

23. **r hofmann**
October 14, 2006

… not even wrong, Joe…

24. **Jean-Paul**
October 14, 2006

To F-theory/orientifold expert: your attitude of deciding what research direction is good based on associations with W et al is exactly what ruined ST. Just try to think, maybe it's not late for you to start thinking independently. Otherwise hide in Langlands. Good luck.

25. **Vogelsang**
October 14, 2006

Peter, nice post, and thanks for the link to the review by Douglas and Kachru. It's very nicely written. The authors definitely do not hide behind technichalities, so even non-experts like myself can learn much about the ideas and the status of the field from them.

26. **T.**
October 14, 2006

Joe,
Of course, model-building is very interesting and useful. However, most of the remaining 10^??? vacua will never be investigated in such detail, and the debate is whether they really exist in full-fledged ST. The 10^??? counting includes all possible vacua with massless moduli: my hope is that they will turn out to be inconsistent, in a similar way as non-supersymmetric closed ST is inconsistent beyond one loop (due to the dilaton tadpole).

27. **anonymous**
October 15, 2006

T:

A cursory reading makes it clear that the vacua being counted have no massless moduli, and even looking just at those with weak (stabilized) coupling, there are a huge number. They aren't counting moduli spaces with unfixed moduli.

28. **anonymous**
October 15, 2006

Jean Paul:
Actually I am just going through the review, I don't know or care what W thinks. The logic seems clear, and also seems to extend beyond f theory to other regimes of string theory.

29. **T.**
October 16, 2006

My cursory reading of the review is very different from yours, so let's try to clarify it. Let's look at page 47, subsection VB of section V "Statistics of vacua". It considers IIB on CY with no complex structure moduli, ignoring Kahler moduli. I am no expert in CY, so can you tell me how many CY have no complex moduli, and zero Kahler moduli? I see that you are a big fan of GVW, but their superpotential can stabilize only the dilaton and some complex structure moduli. In order to stabilize Kahler moduli, you need D-terms, so you need magnetized D-branes, Joe's non-perturbative effects etc. It's not so simple. As far as I can tell ALL vacua treated so far statistically contain massless moduli, so please correct me if I am wrong. All what I am trying to say is that one needs a better understanding of flux compactifications before jumping to a conclusion that ST in inherently unpredictive and needs a special treatment by antropic reasoning etc.

30. **anonymous**
October 16, 2006

T:
The KKLT construction fixes the Kahler moduli; explicit e.g.s are
given now by several groups cited in the review. The statistics Douglas et al give for complex moduli can be used in any of the models with fixed Kahler moduli, and as long as the flux potential isn't too large, doesn't destabilize the Kahler moduli. That is indeed the entire point of the KKLT construction of the landscape. As I read it, in the IIA theory, even just the fluxes suffice to stabilize all of the moduli; the analogue of the GVW superpotential depends on all moduli at tree level.

31. **anonymous**
October 16, 2006

To be clear: this isn't just my reading of the review , see their citations to
Douglas Denef Florea; Douglas Denef Florea Grassi Kachru;
Lust Reffert Scheidegger Schulgin Stieberger; Balasubramanian, Berglund, Conlon, Quevedo;
and for IIA
DeWolfe Giryavets Kachru Taylor; Acharya Benini Valandro;
Derendinger Kounnas Petroupoulos Zwirner; Camara Font Ibanez.

These papers have lots of equations and seem to do what the review says they do. Which is stabilize all the scalars.

32. **Gina**
October 17, 2006

This is an excellent post, Peter, and so is the discussion.

If I understand correctly the non-technical parts it appears that people
agree that the landscape is a problem for (or a challenge to) ST. There is a
comment though that the evidence for the huge number of vacuua is

not yet theoretically solid. There is a suggestion (Joe) that some predictions will be possible common to all these theories. (And even some specific suggestions for such predictions were raised.) There is also a suggestion/hope (T) that most of the 10^** possible theories will be disqualified based on contradictory physics consequences. In any case, the fact that the damaging evidence was discovered by string theorists themselves speaks for the integrity of the ST endeavor.

33. **Peter Woit**
October 17, 2006

Gina,

You're adding nothing to the discussion here, just repeating in garbled form what others who do understand the issues have written. In particular, Joe does not claim to have predictions common to all theories, he was discussing something that is a feature of one class of compactifications but not others. Please stop adding to the noise level here.

34. **anonymous**
October 17, 2006

I am just an intermediate grad student, but I agree with Gina.
As far as I can tell, the situation is: there are "old" string theorists
who are stuck in 1985 and hope the whole thing will go away;
more modern string theorists who realize this is a real issue with
the whole theory (but the theory may well still be correct and one
has to deal with the issue); and finally a bunch of people like
Peter and Smolin who really dislike string theory, for reasons you
can judge yourself (they don't seem very sound to me). Anyway
I need to read my next review now (about supersymmetry), so
I'll sign off.

35. **Jean-Paul**
October 17, 2006

Hi anonymous,
Before you run away, I am just curious why do you want enter a research field which is in the state of crisis, at least according to some "old" people. For 20 years, these old people led the pack, and only a handful of younger people (like Kachru) made a real impact (Douglas is still an older generation). What do you expect to learn? If you take landscape seriously, ST is no longer a TOE, so it doesn't seem to contain this "romantic" flavor of fundamental theory that brought in the previous generations of students. I am also curious what is your advisor's point of view.

36. **TheGraduate**
October 17, 2006

I think taken in the best light, where we assume some of the strongest claims for what has been so far achieved, one would have to argue that string theory was a theory of properties of a final theory but I think it seems a bit unreasonable to claim there is anything that will force uniqueness … am I right?

I think the stuff about all possible universes having to exist just because the equations say so seems… dumb … that's like saying because we can define all possible humans using DNA sequences then all possible humans exist …

The other issue is, that one can never be sure that one has discovered all the possible restrictions …

It's an assumption (and a silly one at that) that there must be a direct correspondence between the universes being studied and actual universes …

37. **anonymous**
October 17, 2006

Jean-Paul:
I talked to many people, and it seems the people who entered
the field in the 80s and 90s (before say 1996) all felt strongly discouraged from doing so (apparently the senior theorists at Harvard openly ridiculed the theory in the late 1980s). I feel now, maybe, like they did then: there is criticism of this theory, but it is based on irrational factors. The theory unifies gravity and quantum mechanics. Given that inflation occurs, any theory with many vacua will have the landscape issue to deal with. And it is hard to imagine a theory with a unique vacuum giving our bizarre word (even grand unified theories have many vacua). Maybe I'm wrong. We'll see.

38. **ak**
October 18, 2006

possibly adding to the 'noise level' here, I pose the question if 'flux compactification' has actually a rigorous mathematical background and if so if this would fit into a yet-to-be-established picture of mirror symmetry, for instance: is there an algebraic counterpart of 'flux compactification' in Kontsevichs picture or what could it be? Furthermore, I wonder if 'monodromy'-questions are involved in questions of 'fluxes', families of Calabi-Yau manifolds being topologically trivial but symplectically nontrivial etc. or related questions of stability of special lagrangians in families. If someone knows a reference relating mathematical and physical research in these respects I would be thankful.

Links

October 12, 2006
Categories: Uncategorized

This evening a very interesting paper appeared on the arXiv, entitled [Instantons Beyond Topological Theory I] by E. Frenkel, Losev and Nekrasov. The authors are studying theories with a topological sector (supersymmetric quantum mechanics and 2d sigma models on a Kahler manifold, N=2 supersymmetric YM in 4d), but are interested in sectors of the theories that are not purely topological. I'm looking forward to reading the paper over the next few days, but it is a bit daunting. This paper is nearly 100 pages long, and it is only part I of three parts, and actually just the simplest part, that involving quantum mechanics.

HEPAP is meeting today and tomorrow, here's the agenda. From the slides of the talk about NASA, the budget situation there for fundamental science missions doesn't look good, and there is discussion of the upcoming NRC committee charged with figuring out which of the "Beyond Einstein" missions to allow to go forward. At Dynamics of Cats, Stein Sigurosson has been writing about this in terms of the missions being sent to Thunderdome, only one to emerge alive.

Slides from the talks last month at the conference in honor of Nigel Hitchin's 60th birthday are available.

Joe Lykken has a nice review article about the standard model, in which he notes:

There is only one diagonal Yukawa coupling that is of order one, and that is the top quark Yukawa. But even this case is mysterious. The top Yukawa is not really of order *one: it is equal to one! For example, using the 2005 combined Tevatron value for the pole mass of the top quark, the corresponding Yukawa coupling is 0.99 +/- 0.01. The entire particle physics community has chosen (so far) to regard this fact as a 1 per cent coincidence. I should point out that similar percent level equalities, e.g. supersymmetric gauge coupling unification or the ratio of the total mass-energy density of the universe to the critical density, have spawned huge theoretical frameworks bolstered by thousands of papers.*

Difference is that, as far as I know, nobody has an idea why this Yukawa coupling should be one. Maybe this is a big clue…

Over at Backreaction, there's an excellent posting about Does String Theory Explain Heavy Ion Physics?, one of the very few places to find a non-overhyped discussion of this topic.

Davide Castelvecchi has a well-done review of my book at his sciencewriter.org web-site.

At this week's physics colloquium at Penn, Andre Brown reports that Robert Cahn emphasized that "half the particles needed for supersymmetry have already been discovered." He also recalled a quote from another colloquium about supersymmetry: "Supersymmetry has stood the test of time. There is no evidence for supersymmetry."

Update: A couple people have pointed out the following rather accurate cartoon.

Comments

1. **A Babe in the Universe**
   October 12, 2006

   Concerning Beyond Einstein: Judging from the crowd Goddard sent to HEAD, Constellation-X will win. As Harvey Tananbaum pointed out:
   1) CON-X is considered first priority after JWST
   2) CON-X and LISA are approved programmes.

   In his Saturday talk, Roger Blandford urged us to attack fundamental problems directly instead of assumption-fitting model building. He described current theory as epicycles and avoided mentioning "dark energy."

   The COSMO 2006 Meeting a week before in Lake Tahoe was more fertile ground for JDEM support. The SNAP team sent just one speaker, and the poor woman was asking around for a ride because she had no way of getting back to Berkeley. Is she still there? Is SNAP running low on petty cash? Watching the Blue Angels was the most fun of the week.

2. **TheGraduate**
   October 12, 2006

   Concerning the reduced budget etc … does anybody have some insight into why the US gov is determined to spend less and less on basic science? It all seems quite weird.

   Compared to other types of investment, it seems like scientists are a rather cheap resource and the rewards of a larger scientific community are so great.

   Even in the face of fears of greater competition from China and India and almost daily fretting about outsourcing of jobs, absolutely nothing seems to be done and day by day, less and less is spent … year after year of decreasing budgets …

   Is there something I am missing?

3. **Peter Woit**
   October 12, 2006

   TheGraduate,

   The US government actually is increasing the budget for fundamental physics research. NASA seems to be a different story, with all the manned space flight issues.

4. **King Ray**
   October 12, 2006

   At the KITP (then the ITP), someone from the NSF once gave a talk on science budget issues, and they showed a graph that demonstrated how entitlements were squeezing everything else out of the budget. You didn't have to project the trends very far forward to get to a point where there was no money for science.

5. **TheGraduate**
   October 12, 2006

   King Ray:

You said “they showed a graph that demonstrated how entitlements were squeezing everything else out of the budget”

What kind of entitlements did he mean, like health benefits, sabaticals and tenure?

6. **TheGraduate**
October 12, 2006

Peter,

When you say it is increasing, do you mean it’s increasing this year or it has always been increasing and does this increase still hold up if we correct for inflation?

I hope it actually is increasing. That would be great.

7. **Tony Smith**
October 13, 2006

Peter said “… Joe Lykken has a nice review article about the standard model [at hep-ph/0609274], in which he notes: … The top Yukawa … is equal to one! …”.

Peter goes on to say: “… as far as I know, nobody has an idea why this Yukawa coupling should be one. …”.

One natural reason for the T-quark Yukawa coupling to be “equal to one” would be for the Higgs to be a T-quark condensate, as in a modified Nambu-Jona-Lasinio (NJL) model such as that described in hep-ph/0311165 Michio Hashimoto, Masaharu Tanabashi, and Koichi Yamawaki.
In that paper, they say “… the scalar bound state of tbar-t plays the role of the Higgs boson in the SM …”.
To avoid some of the problems of a simple NJL model, they consider Kaluza-Klein type “TeV-scale extra dimension[s]” and “… found that the bulk QCD coupling can … become suffciently large to trigger the top condensation for … D = 8 …[and]… predict the top quark mass
mt = 172 – 175 GeV for D = 8 …”.

Since their result is close to an observed value of a T-quark mass peak in Fermilab data, and
since their work is consistent with 8-dimensional structures in my physics model, and
since related structures seem to me to be related to some Fermilab experimental data in the ranges of mt around 130 GeV and around 225 GeV that seem to me to be important,
I described in a talk I gave at the April 2005 Tampa APS DPF meeting the above results of hep-ph/0311165 by Michio Hashimoto, Masaharu Tanabashi, and Koichi Yamawaki.

Joe Lykken chaired the session at which I spoke, and he seemed interested while I was giving the 10-minute talk. At the end of the talk he told me that he would take the ideas in my presentation back to Fermilab, and see if the people there had any interest in working on such approaches.
In the year that has followed, no interest has been expressed by Fermilab, and I see that the work of Hashimoto, Tanabashi, and Yamawaki (who are from Pusan, Tohuku, and Nagoya) is not mentioned in Joe Lykken’s paper at hep-ph/0609274, which is a subject of this blog entry by Peter.

I am disappointed and puzzled at the lack of interest in such an approach with obviously realistic contact with experimental results. Sometimes I think that maybe my status as a blacklisted “moronic crackpot” may discourage interest, and sometimes I think that maybe there really is a bias in the USA against the Nagoya group (after all, Kobayashi and Maskawa have yet to get a Nobel, even though it has long been obvious that they deserve it)).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Since the DPF meeting this year (around Halloween) is a joint meeting in Hawaii of the APS DPF and the JPS, and is designated “Joint Meeting of Pacific Region Particle Physics Communities”, I looked at the list of participants at http://www.dpf2006.org/DPF06%20Participants.pdf
to see whether MIchio Hashimoto, Masaharu Tanabashi, and Koichi Yamawaki might be participating.
To my regret, I did not see any of them listed (the list was dated 10-12-2006) as participants.

8. **TheGraduate**
October 13, 2006

I think with the issue of string theory, it is best to look at things the way a banker would. When one goes to a banker looking for money, he looks at your financial history, if in the past you have not made money, it will be difficult to get more money in the present. It is evidence based decision making.

I think while we all tend to sympathize more with the person in search of funds, we want someone like the banker in charge of our children’s college fund. Last thing you want to hear is that the banker put everything on something with no track record!

I think its pretty easy to get wrapped up in present details about how it will ALL BE DIFFERENT FROM NOW ON. (That’s how people talk when they are all caught up in any activity! It doesn’t really mean anything that people feel that way.)

What matters is waiting for a potential project to build up a record before funding in large amounts. Of course small amounts are okay. Now, the other thing to think about is if concrete measures show that two approachs are on equal footing then they should both be funded with equal probabilty (and with small amounts!).

I liked Smolin’s discussion of venture capitalism. I think he gets it.

9. **Capitalist Wanker**
October 13, 2006

The top Yukawa coupling is convention dependent and runs with energy. Who cares if it is exactly one in some conventions at some energy scale? The other quantities that Lykken mentions people working are not of this type.

10. **woit**
October 13, 2006

TheGraduate,

In HEP US funding has been flat or decreasing for quite a few years, but is supposed to increase significantly for the coming year and for the next few years. For some reason, Bush made physics research a central part of his “American Competitiveness Initiative”.

11. **Alejandro Rivero**
October 13, 2006

Capitalist Wr, the idea about the coupling being a convention emanates from the ideology of GUT, that says that every prediction must be imposed at the GUT scale and then run down to the electroweak scale (or even downto the IR limit?).

What we have here is that there is low energy relationship between Fermi constant (which sets the electroweak vacuum) and the top quark mass (which relates to the electroweak vacuum via its coupling constant).

12. **D R Lunsford**
October 13, 2006

For all its success, it's good to be reminded of just how balled up and complex the SM really is, and how critically tuned to the Higgs mass it is. That has always seemed to me to be odd.

-drl

13. **Thomas Love**
October 13, 2006

Peter said "Joe Lykken has a nice review article about the standard model".

I greatly enjoyed Lykken's article "Standard Model: Alchemy and Astrology," especially his statement:

"A decade from now, we will look back on our current understanding of the Standard Model and be amused at its lack of sophistication."

Peter, I thought you liked the standard model and I was surprised that you would provide a link to an article which compares the Standard Model to Alchemy and Astrology!

14. **D R Lunsford**
October 13, 2006

Thomas,

The SM occupies a strange place in physics history. The basic idea is gauge theory – which in itself, requires long-distance interactions. To make the SM work, you have to remove the very thing that characterizes the main idea. No other real theory is like that. In other theories the main idea assumes a prominent role. When this issue is resolved, the Higgs mechanism will indeed look outmoded.

One should not slight either alchemy or astrology. The former lead to psychology, and the latter, to astronomy.

-drl

15. **Bert Schroer**
October 14, 2006

Instantons Beyond Topological Theory I
This paper is just another exhibit of a misconceived relation between mathematics and physics which presents impressive mathematical results using metaphoric particle physics terminology. The potential harm of just papers is exclusively on the side of physics (with newcomers of QFT who are still struggling with its conceptual basis). The physics side of this much heralded new age between math. and phys. which ever since Atiyah-Witten (for lack of a better name for this math. phys. trend) has been metaphoric on the side of QFT (its relation to QM was more balanced than that to local quantum physics) left particle physics empty-handed. Where at the beginning it had the positive effect for serving as a kind of innovative catalyzer for new mathematics, it now flows back emanating from mathematicians who obviously think they are doing mathematically rigorous QFT whereas they are using physical metaphors to present excellent mathematical results. QFT is unitarity (positivity), causal locality and stability properties (energy positivity, KMS thermal states) and its hallmark which separates it from QM is interaction-caused vacuum polarization. Free field like constructions whose only physical nontrivial aspect is due to using non Lorentzian living space for fields may be mathematically interesting but they do not help for a mathematically controllable construction of interacting quantum fields.
The Standard Model: Alchemy and Astrology
The main source for the presence of Alchemy and Astrology in the SM is the unfortunate (but apparently ineradicable) metaphoric presentation of the Schwinger-Higgs mechanism of charge-screening (the abominable "fattening of the photon"). A slightly less metaphoric way consists in starting with pointlike massive free vectormesons: http://br.arxiv.org/abs/hep-th/9906089
In order to remove the last metaphoric vestiges one has to substitute the BRST gauge formalism by a different description of massive vectormesons in terms of a string-localized field with a very nice short distance behavior (without change of physical content). I have described this in the last section of my essay (see the Samizdat weblog) but did not find the time to elaborate it.

16. **Bert Schroer**
October 14, 2006

I forgot to explain why the Schwinger-Higgs mechanism has nothing to do with symmetry-breaking (already known through Elitzur's old paper) but rather describes screening. This becomes obvious if one takes the decoupling limit for the Higgs particle (can be done perturbatively) which leads to charge liberation (the opposite side of the medal).

17. **Alejandro Rivero**
October 14, 2006

The point is, one starts from Fermi Constant, as measured eg in muon beta decay, G_F=1.16637(1) 10-5 Gev-2, and from this one extracts an "EW vacuum", sqrt(G_F sqrt(2)) = 246.221. You can see this quantitiy in the [standard report](). The Yukawa coupling is calculated from a quantity differing by sqrt(2) and that we also call the "EW vacuum". It is thus 246.221 sqrt(2) = 174.104(1) GeV. Both quantities are used in the standard model because the later is more simple when attaching to fermions, but the former gets a nicer expresion against the mass of W. Nicer only in the sense that people is used to it.

So ok, we have 174.104(1) Now go again to the pdg and take the values of the top mass, either directly measured, 174.2+-3.3, or from the SM global fit, 172.3+10.2-7.6. Ok, from this global (conservative) fit you get only a quotient .9896+0.059-0.044. But from the *measurement* you get 1.0005 +- 0.019

Dorigo has also tryed to champion on this regularity. What is worse is when theoretist say "it is of order one". I mean, if you think it is just a casual thing, you should not mention it ever; it you think it is not casual, it is not of order one, but it is is exactly one plus, if you wish, corrections of about order alpha (or your favorite coupling constant here). What happens is that a log of GUT, SusyGUT and higher models (including some from the string with "free fermions") get to relate the scale of EW breaking with the mass of the Top (hey, even Connes's models do it), and the people presenting results find more convenient to think on "order one" from the theory than to face the experimental result. You can argue that the experiment still allows a 3% deviation easily, but even so, we have already not a o(1) but a o(0.1) gained, and with a huge probability of being o(0.01)

18. **Alejandro Rivero**
October 14, 2006

Hmm sorry, it is 1/sqrt(G_F sqrt(2)) (as obvious from the units).

19. **Capitalist Wanker**
October 14, 2006

So, Sqrt(2) is supposed to play a fundamental role? What if you needed Pi Sqrt(3) to get the ratio to be 1? Would that be as exciting? As I said before, it is convention dependent. Putting in the Sqrt(2) just cleans up some formulae. It has nothing to do with GUTS. And of course it runs, so at higher energies this relation would not be true.

20. **Tim**
October 14, 2006

Peter,

It's really entertaining to read your blog when I have a minute or two to kill. I have no idea what the purpose of the comment section is but it definitely contributes to certain individuals calling you 'crackpot', 'crank', etc. Please see Bert Schroer's comment. If I as an outside observer see such a comment on a whatever blog I — no matter how impartial, unbiased, etc I am — would certainly conclude that this blog is a gathering spot of leisure crackpots.

Sorry to bring bad news but you would do yourself a favour if you would simply delete crackpot comments instead of contributing to publishing them and afterwords complain that people accuse you of the exact thing you actually do: contribute to publishing crackpotism.

Best,
Tim

21. **Peter Woit**
October 14, 2006

Tim,

This is a real problem, but since one person's "crackpotism" is another person's serious scientific idea, I'd prefer to describe it as a problem of too much noise. All physics blogs that I know of that attract a significant number of comments have to struggle with this. It's frankly a huge pain in the ass: how would you like to spend a significant amount of your time examining nearly 50 comments a day, trying to figure out what to do with them, and fielding outraged e-mail from people who feel that something they took a lot of trouble to write has been deleted?

Partly to keep this from completely taking over my life, I've tended to adopt a policy of erring on the side of not deleting comments. Undoubtedly this would be a better blog if I put more effort into moderating things and keeping the noise level down. But one thing I should point out to you, is that, from what I recall, you submit a significant number of comments that are non-substantive and off topic, definitely adding to the noise level here. If I do find time to be more aggressive about deleting comments, you might find many if not most of yours not appearing.

And please, this is off-topic. I'll delete any more comments here about this. Stick to discussing what is in the postings. If you have a brilliant idea about what I can do to reduce the noise level without having to spend a lot more time on it, send it to me via e-mail.

22. **Alejandro Rivero**
October 14, 2006

CW, I do not know that to do of your last comment. Initially it seemed to me the typical damage from the [misinterpretation of, if you wish] doctrine of effective theories: As the Langrangian of Fermi beta decay is not the real thing, why to bother to learn it? And for the same token, as the formalism of the standard model is not the real thing, why to bother to learn it? On second reading, it seems that you are perfectly aware that these square roots come from the usual notation for that lagrangians and for the vector of electroweak breaking (the "higgs vacuum", if you prefer), and that one puts them in advance to aliviate notation after normalisation of some vectors. But then you should know they come forcefully from these normalisations, and you should not to play with "pi sqrt(3)" as a pausible ratio (incidentally, pi is specially misleading, as you can not get a trascendent number as solution of an algebraic equation).

So, are you being serious in your question, or just rethoric?

Some old authors, by the way, never were happy with these squarerootings, and they used to say that the value of the electroweak vacuum is about 175 GeV

23. **newcomer to QFT**
October 14, 2006

"This paper is just another exhibit of a misconceived relation between mathematics and physics which presents impressive mathematical results using metaphoric particle physics terminology."

I don't understand this criticism since the paper in question is about QM, which is mathematically well defined. QFT is mentioned, but is to be treated in the forthcoming sequels.

I'm sure I'll learn much about QM by studying this paper. However I've never seen thermal KMS states teach anything about quantum statistical mechanics that wasn't already known by traditional (although less mathematically rigorous techniques). Even the treatment of the thermal harmonic oscillator by Narnhofer and Thirring (Letters in Mathematical Physics, Vol 23 No 2 pg 133-142) seems to be similar to the coherent state treatment, but dressed up in fancy mathematical clothing. Do thermal KMS states teach us anything (physically interesting) about BEC or the Ising model that isn't already widely known?

24. **Bert Schroer**
October 14, 2006

My comment is about the use of these methods in QFT. Let us see whether there will be anything including interactions (and the inexorable vacuum polarization going with interactions) coming from such methods. If there is, I of course will correct myself in public. I do not care about the spacetime dimension as long as it is at least 2. Oscillators do not impress me.

25. **Bert Schroer**

October 14, 2006

It is precisely that balance between mathematics and physical concepts which one finds in Thirring's writing (and more generally in most of math. phys. coming out of Europe with the exception of Amsterdam and from string theory groups) which has been lost in that part of the particle physics community which takes its math from Atiyah and Witten.
By the way, there are meanwhile also quite nontrivial 2-dim. QFT which have been shown to be well-defined (most recently a very original AQFT method to show exitence for massive factorizing models has been discovered).

26. **Arun**
October 14, 2006

Another side to metaphorical thinking (perhaps?) is in the Jaffe/Quinn paper referenced in the n-Category Cafe:
http://golem.ph.utexas.edu/category/2006/10/wittgenstein_and_thurston_on_u.html>here

This is from 1993:

"When reliability of a literature is uncertain, the issue must be addressed. Often "rules of thumb" are used. For example, mathematicians presume that papers in physics journals are theoretical. This extends to a suspicion of mathematical-physics journals, where the papers are generally reliable (though with dangerous exceptions). Another widely applied criterion is that anything using functional integrals must be speculative. One of us has remarked on the difficulties this causes mathematicians trying to use solid instances of the technique [J]. These kinds of rules are unsatisfactory, as is the caveat emptor approach of letting each paper be judged for itself. Proponents of this latter view cite Witten's papers as successful
examples. But a few instances can be handled; it is large numbers that are a disaster. Also, it is a common rule of thumb now to regard any paper by Witten as theoretical. This short-changes Witten's work but illustrates the "better-safe-than-sorry" approach mainstream mathematics tends to take when questions arise."

27. **Jonathan Vos Post**
October 15, 2006

Not having a Science subscription, I can only get this teaser online, and wonder whether it relates to one of your recent references, and what it's about:

Science 13 October 2006:
Vol. 314. no. 5797, p. 248
DOI: 10.1126/science.314.5797.248

News Focus
PARTICLE PHYSICS:
Tidy Triangle Dashes Hopes for Exotic Undiscovered Particles
Adrian Cho

Physicists have proved that their explanation of matter-antimatter asymmetry is essentially the whole story–even though many hoped the theory wouldn't add up.

28. **Bert Schroer**
October 15, 2006

In my use of the attribute "metaphoric" I should have differentiated between a benign connotation and a malignant one. Clearly the use of the Higgs mechanism (the mass-dressing of the photon through the Higgs "condensate) and the gauge theoretic use of ghosts (which are not there in the beginning nor can they be tolerated in the final physical objects) are benign metaphors. Whether you compute using these metaphors or follow an autonomous path does not matter, you end up with the same renormalized expressions for the physical (gauge invariant in the metaphoric treatment, pointlike local in the autonomous derivation) quantities.
An example for a malignant metaphor is the statement that the Maldecena conjecture is an illustration of a mathematical theorem about the AdS—CFT correspondence. A pharaonic constructed erected by hundreds of people in thousands of publications cannot be undone by any rational argument. The only hope is that as a result of the unphysical nature of SUYM and AdS it will cease to circle over our heads and eventually become exclusively a topic for historians and sociologists of science.
For those among you who have not lost their sense of humor I attach a parody which Noboru Nakanishi (with his consent) wrote in 1986 i.e. at a time when skeptics as myself (and perhaps also Peter Woit) were still in a "lets give it a try" state of mind.

This is a faithful reproduction by LaTeX of a joke paper written in May, 1986 (unpublished).

COMMENTS ON THE SUPERSTRING SYNDROME

Noboru Nakanishi
Research Institute for Mathematical Sciences
Kyoto University, Kyoto 606, Japan

A recent epidemic among the Elementary Particle Theorists is the Superstring Syndrome, which causes the Kaluza-Klein symptoms. The pathogen is said to be much much smaller than any virus known and in shape of a string or a ring. Nothing certain is known about this virus, however, since nobody has ever seen it. There is a rumor that this virus has been cultivated under strict supervision of Drs. Green and Schwarz
By the way, it is said that these doctors are characters in the German-English translation of a famous novel by the distinguished writer Stendhal. It is also said that the translator is red-green blind.

and was released to outside by a researcher at the P-University (the author has chosen not to name him). This disease, unlike AIDS, does not have a tendency to prevail among homosexuals. In general, however, young and cheerful people are observed to be more susceptible to this disease. The infection occurs usually through mouth, as is true for the influenza. But since the infection occurs occasionally through eye, special precaution is necessary.
The String Syndrome had also been epidemic about ten and some years ago. At that time, the damage was a rather minor one, since it was not associated by the Kaluza-Klein symptoms. Eventually, the Yang-Mills was found to be an excellent remedy, and thus most of the patients were said to be completely cured. However, the current Superstring Syndrome is a much severe disease, for which the Yang-Mills may cause undesirable side-effects rather than remedy it. Adding further to the worries is the fact that brain might be affected. Under this disease, it is said that the patients fall in to a belief that something called "anomaly" is at the root of everything. In contrast to healthy people, who regard normal states as they are and then observe anomalous states as something deviating from them, the patients credit anomalous states and then observe normal states as deviations from them. Researchers of abnormal-psychology are therefore quite interested in this symptom.
The patients of the Superstring Syndrome believe in a Miracle. This is not a small miracle like the one in which Moses divided the Red-Sea into halves, but is the Great Miracle of the whole universe that the 10-dimensional space-time is divided into the 4-dimensional space-time and the 6-dimensional space. Although this separation has to be done completely, homogeneously and permanently throughout the whole universe, they cannot understand at all why it can be so. The patients just simply believe in it. It is said that a magic spell "Calabi-Yau" enables them to see clearly the extremely varied structures of the six-dimensional space. In fact, they must have experienced the wonderland stranger than that of Alice. In the very, very, very, very tiny world that is 20-orders as small as the scale of a nucleus, the classical geometry is claimed to hold to extreme accuracy. In that world, the mini-dragon Pyrgon must be striding in the vicinity of the Planck Length District wearing the top-mode of Anomaly-Free. The patients can easily see this hallucination without LSD. They use new drugs called SST I and II. Although it is not yet known what exactly they are, it can be guessed

from their pronunciations that they are similar to LSD. Since their safety is not confirmed by any clinical studies, it is advised that people should avoid their habitual use. The trouble about the Superstring Syndrome is that, like drunks never acknowledging their drunkenness, the patients do not acknowledge their own abnormality. They regard the Kaluza-Klein symptoms as normal and lose the sight of the fact that this real world we live in is the genuine four-dimensional space-time. The present author is deeply concerned about the possibility of a disastrous situation in case an effective remedy is not invented in near future.
[Translated by Hideaki Aoyama. The original article is published in Soryushiron Kenkyu (Researches of Elementary-Particle Theory), Kyoto, 72 vol.6 (1986) 345, in Japanese.]

29. **D R Lunsford**
October 15, 2006

Bert, pretty tame compared to Warren Siegel's magnum opus "The Everything of Theory", about TOENAIL (theory of everything not appearing in laboratories 🙂

-drl

30. **anon**
October 15, 2006

Joe Lykken writes that in 10 years we'll look back and be amused at how unsophisticated the Standard Model of today is.

Is anybody amused by the lack of sophistication of the Standard Model circa 1996? What measurable quantities can we calculate now that we couldn't calculate then?

31. **arivero**
October 15, 2006

Jonahtan, it is an article about the status of the unitary triangle after the recent measurements of B-meson mixings. Surely you can find more in the public reviews of the Particle Data Group.

32. **MathPhys**
October 15, 2006

Bert,

No one ever said that the Maldacena *conjecture* amounts to a *mathematical theorem* — If it were, how come everyone uses the word *conjecture*????

What more can people do than to keep on saying *conjecture* ????

It's statements like these that are malignant and obfuscating and hurts the cause of any meaningful criticism of the current status of string theory as a physical theory which is far from being in good shape.

Please don't confuse things. If anything, it surely hurts your cause.

Further, I find it unreasonable on your part to claim priority in discovering aspects of conformal field theory which you say is very physical (namely conformal blocks) before BPZ when it suits you, then criticize string theory for being a theory of free fields.

As I'm sure you know, string theory is based totally and completely on conformal quantum fields in 2 dimensions. Interactions come in via vertex operators, multi-genus Riemann surfaces, etc, etc. That's the whole point.

Further, I don't think that you have discovered conformal blocks (in the accepted definition of this concept) at all, since you didn't know anything about Virasoro algebras, central extensions, and you definitely didn't know about the minimal models.

There is another physicist who also likes to make such claims, but similarly, they are strictly unfair.

You all missed working at c smaller than 1.

33. **Walt**
October 16, 2006

MathPhys: It is far from the case that the Malcedena conjecture is always referred to as a conjecture. I was confused on this very issue until recently, simply because people are not that careful.

34. **MathPhys**
October 16, 2006

Walt,

Show me one serious string theorist who refers to it a mathematical theorem, and I'll be very happy to admit being wrong.

35. **Thomas Larsson**
October 16, 2006

Further, I don't think that you have discovered conformal blocks (in the accepted definition of this concept) at all, since you didn't know anything about Virasoro algebras, central extensions, and you definitely didn't know about the minimal models.

Claiming that one can understand conformal symmetry in 2D without knowing about the Virasoro algebra is about as absurd as claiming that one can understand diffeomorphism symmetry without knowing about the analogous Virasoro-like extension of the spacetime diffeomorphism algebra. But then again, absurdity has never prevented people from making such claims.

36. **MathPhys**
October 16, 2006

I think they were dealing with 2D massive models, which they can now say are off critical deformations of conformal field theories.

But then again the proper treatment of the latter requires knowledge of the underlying deformed infinite dimensional algebra, something that was studied only after BPZ, and I'm not so sure is well understood to this day.

Aside from that, I'm afraid I will not believe a word of what you say about a central extension of spacetime diffeomorphism algebras until you write a paper on that, and it gets refereed and accepted for publication in a regular math journal.

37. **Thomas Larsson**
October 16, 2006

The extension proportional to c_2 was published in
T.A. Larsson,"Central and non-central extensions of multi-graded Lie algebras"
J. Phys. A. 25 (1992) 1177–1184.

The extension proportional to c_1 was published in
S.E. Rao and R.V. Moody, "Vertex representations for N-toroidal Lie algebras and a generalization of the Virasoro algebra"
Comm. Math. Phys. 159 (1994) 239–264.
The paper is available online.

The representations were understood geometrically in
T.A. Larsson, "Extended diffeomorphism algebras and trajectories in jet space"
Comm. Math. Phys. 214 (2000) 469–491.
It is available online as math-ph/9810003.

It might be worth pointing out that Bob Moody is somewhat famous, e.g. for the codiscovery of something called Kac-Moody algebras back in 1968. But then again, maybe you don't believe in Kac-Moody algebras neither.

38. **MathPhys**
October 16, 2006

Thomas,

Thanks for the references.

It's nothing personal, but I'm wary of claims that are not published.

PS I know who R Moody is.

39. **Thomas Larsson**
October 16, 2006

Mathphys, sorry for blurting out. After the search for, and discovery of, the multi-dimensional Virasoro algebra met with nothing but silence and ridicule for almost 20 years (from physicists, mathematicians have acted very differently), I have become quite hostile per default.

And why should physicists care about genuinely new math, when there are so many exciting new ideas around. Like the anthropic principle and LQG.

40. **Matti Pitkanen**
October 16, 2006

Thomas Larsson Said:

"Mathphys, sorry for blurting out. After the search for, and discovery of, the multi-dimensional Virasoro algebra met with nothing but silence and ridicule for almost 20 years (from physicists, mathematicians have acted very differently), I have become quite hostile per default."

Dear Thomas,

I share you belief on higher dimensional Virasoro and Kac-Moodies although I have a different realization in mind. I share also your frustration. Average colleague does not seem to have the necessary minute or two (or perhaps it is ability after all) to concentrate to a new idea unless it is proposed by authrority.

Lightlike 3-D surfaces X^3 possess X^3-local conformal transformations of imbedding space act as symmetries respecting lightlikeness. Ordinary 1-D Kac Moody algebra emerges naturally as a Kac-Moody algebra associated with the lightlike coordinate and the algebra decomposes into subspaces remaining invariant under the action of this algebra. Infinite number of representations ordinary Kac-Moody are fused to a larger structure. One might think that this could stimulate some interest but landscapeology seems to be more fascinating.

Best Regards,
Matti Pitkanen

41. **Bert Schroer**
October 16, 2006

MathPhys
I found the commutation relations of the chiral energy-momentum tensor pursuing John Lowenstein's (the idea came from Wally Greenberg) of "Lie Fields". in 1973 and presented the results at the January 1974 conference in Rio de Janeiro. In his thesis John did not find any higher dimensional illustration (we now know that there is a no-go theorem) and my finding was the first natural illustration of this concept.
B. Schroer , A trip to Scalingland, Brazilian Symposium on Theoretical
physics, Rio de Janeiro, January 1974, Vol. I, ed. Erasmo Ferreira, Livros T´ecnicos e Cientificos, Editora S. A.
if you give your mailing address I send you a copy.
The Wit-Virasoro algebra, contrary to popular opinion (nobody seems to look at the original Virasoro paper), does not appear in Virasoro's paper, together with Wit Virasoro does not have the central term and different from Wit he only has the positive frequency (perhaps the reason for missing the central term).
My paper is a structural theorem always including the interacting case. Doing two-dimensional QFT in those days was extremely unpopular (the reason for not sending such things to international journals).
I tell you a funny story which underlines this. At one of the international conferences Swieca was teased by a well-known theoretician (whose name I will not reveal): Andre why do you work on that 2-dimensional stuff? Swieca's reply: I get lost in higher dimension. Without his sudden death in 1980 the story could have had the following cintinuation: Hy Andre what are you doing these days? S: I am looking at charge screening problems (the Schwinger-Higgs mechanism) in 4-dim. gauge theories. The unnamed physicists: but dont you know Andre that everything happens in two dimensions, in order to understand higher dimensions one only must understand 2-dim. conformal field theory!
MathPhys if you are honestly interested in the rich pre- Wess Zumino-Witten history about 2-dim. models I refer you to a review (published in AOP)
http://xxx.lanl.gov/abs/hep-th/0504206

it is very unfortunate that the connection to the representation theory of current algebras (later loop groups) was cutoff by introducing a Lagrangian terminology which is totally metaphoric and added nothing in substance (in fact the WZW people only pay lip-service to that Lagrangian and continue to compute with the prior current algebras representation theory).
but I genuinely doubt that you are; you seem to be one of those folks who only use weblogs primarily to denigrade others.

42. **woit**
October 16, 2006

Let me remind everyone to please avoid personal attacks here. There is all too much of this going on, it is just annoying and very unenlightening.

Also, Matti and Thomas. Please stop continually trying to use my blog as a forum to promote yourselves and your ideas.

43. **Garrett**
October 16, 2006

http://xkcd.com/c171.html

44. **Walt**
October 16, 2006

MathPhys: You're moving the goalposts. They don't call it a theorem, but they don't always call it a conjecture. And seriously? I'm supposed to show you an example of a serious string theorist? What, are you paying me? I am not an anti-string partisan: I do not have a definitive judgement of the subject. I was sharing with you my personal experience, that I was confused on its status as a conjecture until recently. If that means nothing to you, then what does that say?

45. **Bert Schroer**
October 16, 2006

There is an established theorem which completely covers the AdS-CFT correspondence, but it has nothing to to with Maldacena (the theorem has been established by Rehren). In fact in my Samizdat essay I have provided additional arguments why the growing suspicion that Maldacena's conjecture (which links a N–>infinite SUYM with a classical limit of a) 5-dimensional AdS string theory) can be sharpened to an outright contradiction to the theorem.
The confusion comes from interpreting the M-conjecture as an illustration of AdS–CFT correspondence in the sense of the theorem. The M-conjecture maybe correct, the problem is with its interpretation.
What makes the arguments on this weblog so painful is that the sociology of 4000 or so papers wins against a rigorous and clearly presented mathematical theorem. There is no reason to continue this discussion, you can read the paper about the theorem and my arguments in may essay but it is futile to invoke democracy against a theorem.

46. **Peter Woit**
October 16, 2006

I encourage people who want to discuss the relation between the Maldacena conjecture and Rehren's theorem to do so where it is on-topic, at Jacques Distler's new posting on the subject:

http://golem.ph.utexas.edu/~distler/blog/archives/000987.html

Based on my experience with him, what Distler writes on this kind of subject is often highly misleading (for a recent remarkable example, see what his comments on a recent thread at Clifford Johnson's blog). I know nothing about Rehren's argument, so I have no idea what is going on in this case, but encourage people who do know about this to discuss it over there.

47. **Bert Schroer**
October 16, 2006

Dear Peter,
Thanks, and let me add that this subject is really not appropriate to be discussed in any weblog. Way back in the Sci.PhysicsResearch Archive Rehren tried once, only to be cut down by the ST pittbull. It was that incident which made me aware of the exitence of new barbarians and their attampted sell-out of QFT.

48. **A.J.**
October 16, 2006

Jacques' summary looks quite accurate to me.

49. **A Babe in the Universe**
October 16, 2006

Dr Scroer, your input is appreciated. Please don't let some cowardly pitbull discourage your contributions. That person has attacked many of us and GM=tc^3 is still here.

50. **Peter Woit**
October 16, 2006

Please, when I referred people to Distler's weblog, I was encouraging them to read what he has written, and if they want to discuss it, do so over there. I was definitely not trying to start a discussion of this here.

51. **Tommaso Dorigo**
October 16, 2006

I think the coincidence of the top quark mass with the Yukawa coupling is not numerology, but a quite interesting observation – very few other "coincidences" of this kind exist, and Alejandro Rivero has noted a couple in recent papers.

Should we shrug our shoulders at these coincidences ? I think we should take all clues we have from experimental data very seriously. However, there seems to be a sort of immobility in theoretical trends, and these clues do not get picked up with enough momentum… I Guess I know the reason.

T.

52. **barbarian**
October 16, 2006

>Is anybody amused by the lack of sophistication of the
>Standard Model circa 1996? What measurable quantities can
>we calculate now that we couldn?t calculate then?

I, for one, am. Let's take 1994 instead of 1996, for the sake of argument.

In 1994 we didn't know whether the top quark actually existed, if it was standard, and what its mass was. We didn't know that neutrinos have mass, that they oscillate, and whatever happened to the missing solar neutrinos. We believed that the cosmological constant was either small and negative or zero.

In 1994 we thought that ST unambiguously predicted SUSY, and that the SSC was probably going to observe the MSSM.

In 1994 most (if not all) of the 7 parton QCD amplitudes were not yet known. Unquenched lattice calculations were virtually unheard of. Two-loop computations in the SM were nowhere near as widespread as they are now.

I'm just quoting from the top of my head. If I'd taken the trouble to actually checking the literature I'm certain I'd be able to write a much longer and precise list.

A lot of progress happened in the last ten years. And a whole lot of stuff is going on right now. Lykken is definitely right.

53. **barbarian**
October 16, 2006

By the way, in 1996 nobody knew how to renormalize fully relativistic baryon chiral perturbation theory at the one-loop level…….

54. **Anon**
October 16, 2006

Bert wrote:

"Thanks, and let me add that this subject is really not appropriate to be discussed in any weblog."

How many posts have you made on this weblog, about Rehren's duality versus the Maldacena conjecture?

Maybe you mean it is not appropriate to be discussed on any other weblog but this one.

55. **QWERTY**
October 16, 2006

HELLO PETTER HAVE YOU MISSED ME? I HAVE MISSED YOUR WEBLOG AND ALSO YOU. I HAVE BEEN ON A HOLIDAY!!

ANYWAY I WAS JUST WRITING TO SAY THAT YOU SHOULD ALSO MAKE LINKS TO TERENCE TAO AND HIS BRAND NEW AND VERY CLEAR EXPLANATION OF TEH POINCARE CONJECTURE.

http://www.math.ucla.edu/~tao/preprints/Expository/perelman.dvi

TERENCE TAO IS A GENIUS AND IT IS ALWAYS GOOD WHEN ONE GENIUS EXPLAINED THE WORK OF ONE MORE GENIUS. ALSO HIS COSMIC DISTANCE LADDER ARTICLE IS VERY ILLIMUNATING EVEN FOR PHYSICISTS.

http://www.math.ucla.edu/~tao/preprints/Slides/distance_ladder.pdf

WHAT DO YOU THINK?

56. **woit**
October 16, 2006

QWERTY,

I can't honestly say that I've missed you and your defective keyboard, and in general I try and discourage off-topic comments. But I hadn't heard about the Tao piece on the Poincare proof, which appears to be extremely good. The three available detailed proofs are hard to read, this looks like a much more accessible summary, an excellent contribution to the subject.

57. **nontrad**
October 16, 2006

re: our long lost cravings for QWERTY's long lost defective keyboard…

Too funny!!!

Keep up the good work Peter: Such a healthy sense of humor is well worth it's weight in gold.

p.s. Don't get me wrong, Tao is a large pile of papers in the corner of my office that I have yet to even begin to skim through. So good job QWERTY (even though it's got nothing to do with nothing in this comment section).

58. **MathPhys**
October 16, 2006

Bert,

Indeed, the central extension is not in Virasoro's original paper. It was discovered by Joe Weiss right after Virasoro's work became public.

How come we don't say "Virasoro-Weiss algebra"? Weiss died (he got himself killed on a weekend trek in the Juras while visiting CERN, or so I've been told).

Anyway, these are historical comments of no scientific content.

Re the discussion of Rehren's work on Distler's bolg: most illuminating (damn it Henning, you let the formalism confuse the issues, yet again).

59. **Bert Schroer**
October 17, 2006

MathPhys
correct, as far as I remember Weiss died in an alpinist accident (similar to Renner who was Gell-Mann's student).
Besides the ST line there is also a field theoretic line via the c.r. of the energy-momentum tensor. In that case it is impossible to miss the correct structure, even for the case of free Fermions it is there. Follow the advice and do not confuse names of ideas with the protagonists of ideas. Strictly speaking the Born probability interpretation for the Schroedinger wave function appears the first time in Pauli's work. But Max Born's proposal to interpret the scattering amplitude in probabilistic terms of a cross section is morally the same as Pauli's later contribution.

60. **MathPhys**
October 17, 2006

Bert,

Thank you for the advice.

Actually, Virasoro's derivation (and presumably Weiss' and everyone else's after that) is precisely what you call "the field theoretic line". It's all 2-dimensional field theory. Virasoro was just careless.

Historical quiz: When and where was Weiss' central extension mentioned for the first time? In what context?

61. **Thomas Larsson**
October 17, 2006

Gelfand-Fuks 1968?

62. **MathPhys**
October 17, 2006

Is that so? Not Feigin-Fuks?

63. **Thomas Larsson**
October 18, 2006

There are many hits for Gelfand-Fuks cocycle, e.g. this. Among other things, I think Feigin and Fuks were the first to publish a proof that the Kac determinant is singular where it is, but that is a different story.

John Baez has mentioned somebody who found the cocycle in characteristic p. Perhaps that was even earlier.

64. **Bert Schroer**
October 18, 2006

MathPhys
maybe some brief remarks concerning your doubt about the conformal decomposition theory (into conformal blocks) in my 1974/75 paper with Swieca and Voelkel could help you.
I think you were expecting something similar to BPHZ (where the conformal block decomposition arose as an important tool for analyzing the minimal models) and you were disappointed. Since we did not have those models (only exponential Bose fields and the closely related massless Thirring model were at our disposal) we had to argue on purely structural grounds. Our decomposition theory was based on a prior observation called "the global causality paradox of CFT" which in turn originated from the observation that for certain zero mass fields with anomalous scale dimensions (e.g. the massless Thirring field) the Huygens principle was violated (the anticommutator was nonvanishing in the timelike region called the "reverberation" phenomenon). In such cases the global causality notion had to be adjusted to the covering of the compactified Minkowski spacetime (something which was already done before by Irvin Segal). The decomposition theory simply resulted from the realization that anomalous-dimensional conformal fields (in any even spacetime dimension), although behaving irreducibly under "small"conformal transformation, are highly reducible under the action of the center of the covering group; the resulting decomposition is the block decomposition theory. We also payed special attention to the 2-dim. situation for which one obtains a block decomposition for each chiral component. We were somewhat surprised about our findings because the component fields were not Wightman fields since they came with a source and a range projector. We did not continue our research (the minimal models could have been found by just pressing ahead, one does not really need to know anything about Kac-Moody algebras) because we thought that our rich decomposition theory had no genuinly nontrivial realizations.
Let me tell you that I was impressed as anybody with BPHZ and it took me and Rehren almost 2 years to make the bridge from the old structural theorems to the new wealth of very nontrivial models (we published several joint papers on this subject).
I have no problem with the way history developed. It is not only those who are too late who are punished by history (Gorbatchov); this is a fact of life.
But it hurts me a bit if somebody claims that the old papers contain nothing. Deep ideas almost always have predecessors and the older results were usually obtained through very different arguments.

65. **MathPhys**
October 18, 2006

Bert,

When you say BPHZ, I'm sure you mean BPZ as in conformal field theories, and not BPHZ as in momentum cut-off renormalization theory.

Yes, I know of your earlier work (before BPZ) and I'm aware of the fact that you didn't press ahead long enough.

You know of course that others, most notably Gervais and Neveu were in a very similar situation. They also didn't persist.

I definitely have a lot of respect for your earlier works, and I didn't mean at all to belittle it. I just wish to be very precise about what has been done before and after BPZ.

I'm also familiar with your 2 papers with Rehren, and with Rehren's paper on his own that followed your joint work.

All the best.

66. **MathPhys**
October 18, 2006

PS So do you know where the first reference to Weiss' central extension of Virasoro appeared in the physics (not mathematics)literature?

67. **Bert Schroer**
October 18, 2006

MathPhys
sorry, I meant BPZ. I do not know the work of Gervais and Neveu, is it published?
The paper with Rehren on the exchange algebra of the conformal Ising model was the turning point when I really understood how the old stuff fitted together with BPZ. And it also increased my appreciation of Leo Kadanoff's work (which in my opinion has been underestimated relativ to Wilson's contributions).

68. **MathPhys**
October 18, 2006

Bert,
Yes, Gervais, together with Neveu and/or others have an almost infinite series of papers, in which some aspects of BPZ were "anticipated".

Tell me if you, and no one around you, knows when and where was the first mention of the Virasoro central extension in a physics paper. Don't be too proud 🙂

69. **Bert Schroer**
October 18, 2006

MathPhys
I think it was around 1972 for free Fermions via an explicit calculation by somebody around Dave Olive (Peter Goddard?). I am sure that I was not the first, but I may have been the first who derived it for the general case by an entirely structural argument. In any case my motivation was to find a class of "Lie fields" in the sense of Greenberg and Lowenstein; at that time my knowledge about string theory (the dual model) was closed to zero.
Concerning Joe Weiss. I met him at CERN but I think his deadly accident did not occur in the Alps but after his return to the US in the Rocky Mountains. When you suggested that he may have seen the Virasoro algebra I agreed instinctively because he was extremely bright and certainly up to the task. But I do not know a reference. Alpine accidents were quite common within the particle physics community; besides bruno Renner I think that also heinz Pagels died under similar circumstances.

70. **MathPhys**
October 18, 2006

Bert,

The first reference to Weiss' result was in a footnote in a paper on strings by Fubini and collaborators (the Italian mafia at CERN). That's when strings were a theory of hadrons in 4 dimensions.

After using the Virasoro algebra without a central extension, the footnote reads

"We have been informed by J Weiss that there is an extra term in the above equation. However, this does not change any of our conclusions".

The String Wars

October 18, 2006
Categories: Favorite Old Posts, Uncategorized

I noticed recently that George Johnson will be journalist in residence and giving a talk on Friday at the KITP in Santa Barbara about "The String Wars". Somehow I don't really think that it's a good thing that this is now being perceived as a "war". Johnson is the author of an excellent biography of Murray Gell-Mann and writes for the New York Times.

For controversy on the East coast, tonight the Center for Science Writings at Stevens Institute for Technology in Hoboken will be hosting a panel discussion and debate on The End of Science?, featuring John Horgan and Michio Kaku.

This week's New Yorker has a couple letters to the editor responding to their recent article about the string theory controversy. One points out that particle theory and quantum gravity is not all there is to theoretical physics. The second is by Lisa Randall, and mainly concerned with claiming that there is now a healthy interaction going between string theory and phenomenology, with most particle physicists eagerly awaiting the LHC.

Update: Today's New York Times has an Op-Ed piece entitled The Universe on a String by my Columbia colleague Brian Greene, in which he responds to recent criticism of string theory. As you might guess, Brian's piece doesn't really convince me to change my mind (as my book and Lee Smolin's don't seem to have convinced him).

Brian mentions the possibility of seeing supersymmetry or extra dimensions at the LHC, and possible effects of quantum gravity in the CMB, but acknowledges that these are not definitive predictions of string theory that can be used to falsify it. He also mentions the recent attempts to apply AdS/CFT to heavy ion physics, but these don't address the use of string theory as an idea about unification.

He deals with the landscape only by making an argument I've heard him make before: that just having a unified theory of gravity and particle physics would be a big accomplishment, even it this theory didn't explain any of the things about the standard model that one would like it to explain. Besides the fact that string theory still doesn't provide a fully consistent unified theory (since it has no non-perturbative formulation), I've always found this point of view problematic. If string theory can't make any definitive predictions about particle physics, it's very unclear that one can ever test it, which is a huge problem.

Brian does, unlike some string theorists, acknowledge that it's possible that string theory is wrong and will have to be abandoned, in particular if "future studies reveal an insuperable barrier to making contact with experimental data". My argument is that if string theorists accept the existence of the Landscape, such an insuperable barrier appears. He describes string theory critics as calling for research on string theory to be dropped, which really isn't accurate. Neither Smolin nor I have ever called for this, rather our argument is that research into alternatives to string theory needs to be encouraged.

Update: The George Johnson talk is now available here. It seems that many of the string theorists at the KITP are not very happy about my book and Smolin's, although it's unclear if any of them have read either of the books. Amanda Peet claimed that both books have many errors (invoking the NYT review by Tom Siegfried), while Johnson repeatedly told her that it would be a good idea for her to actually read one of the books. She also kept claiming that there is "a backstory" that explains why Smolin wrote his book, but she was dissuaded from elaborating on this when someone pointed out that the talk was on video and would be on the web.

The experience of watching the talk was pretty odd, since Johnson began by connecting to my blog and discussing the fact that I was discussing his upcoming talk. I watched a lot of the talk during commercials of an episode of Numb3rs, and during this episode "Larry" the physicist was working on calculations involving branes, and playing hooky from a string theory conference.

Update: Davide Castelvecchi has put up an interview with George Johnson on his web-site.

Update: Clifford Johnson and Lubos Motl have their own takes on the KITP video.

Update: It appears that there will be a second talk by George Johnson about this, String Wars 2. After the first one, I'm having trouble figuring out why anyone at KITP thought a second one would be a good idea.

Comments

1. **Jonathan Vos Post**
   October 18, 2006

   The End of Science? When exactly did that happen?

   Stephen Wolfram thinks it began when Newton invented the Calculus (soory, Fluxions) to do Physics, if the cosmos is actually a cellular automaton.

   The late 19th Century thought it had come, with only more decimal points to be added to measurements.

   Deterministic materialists think it came in with Quantum Mechnics.

   Aether theorists think it came with Michelson-Morley, or maybe Einstein and Poincare.

   Then there's that pesky String Theory, which certainly blocked the rest of Physics from getting equal access to funding and tenure-track employment.

   As J. D. Bernal wrote in "The World, the Flesh, and the Devil"): "we are still too close to the birth of the universe to be certain about its death."

2. **Timothy Clemans**
   October 18, 2006

   "Will science ever solve the riddle of the universe once and for all?"

   "This should be able to prove or disprove string theory. Personally, I feel no need to prove the theory experimentally, since I believe it can be proven using pure mathematics." – http://www.longbets.org/12
   Kaku

   Well according to Kaku he thinks that question will be answered by deducing string theory from some physical axioms so to him it may not be science that answers the question but mathematics.

3. **TheGraduate**
   October 18, 2006

"For controversy on the East coast, tonight the Center for Science Writings at Stevens Institute for Technology in Hoboken will be hosting a panel discussion and debate on The End of Science?"

What, no live webcasts?!

4. **King Ray**
October 18, 2006

I think fitting the universe with string theory is like fitting a sine wave with square waves, only it's much worse.

5. **Aaron Bergman**
October 18, 2006

Then there's that pesky String Theory, which certainly blocked the rest of Physics from getting equal access to funding and tenure-track employment.

The "rest of Physics"? Snort.

For controversy on the East coast, tonight the Center for Science Writings at Stevens Institute for Technology in Hoboken will be hosting a panel discussion and debate on The End of Science?, featuring John Horgan and Michio Kaku.

Ugh.

6. **Peter Woit**
October 18, 2006

OK, folks, I realize this "string controversy" coverage is getting old, and I promise to put up something on a different topic soon, but can we aim for a somewhat higher level of discourse than the above?

7. **Thomas Love**
October 18, 2006

Peter, The string controversy is not getting old. You fired the opening salvo in the string wars. You can't cut (the strings) and run! I'm hoping to hear about the final demise of string theory and I expect to read it here!

8. **Gina**
October 18, 2006

Goodbye!

This is a little off-topic, but I hope Peter will allow it, just this time. Over the last eight-ten weeks I made occasionally comments on several issues discussed here and in Peter Woit's book. I did put a lot of thought into my comments, although in the blog speed I could not be always on par. My comments were mainly on issues regarding philosophy of science and the practices and ethics of debating science. Without the technical background but with some comon sense and at times help from more informed friends (e.g. my comment on 2- and 4 dimensional lattice models,) I tried sometimes to relate in a non-technical way to specific scientific matters. Following my attempt to summarize the recent landscape discussion and Peter's rather negative comment 10 minutes later, Peter informed me that as this weblog "is largely intended for people who work professionally on the issues in math and physics that I'm interested in" and since my contributions drew a lot of criticism by other participants, a criticism that he shares, I will not be able to post here without prior monitoring. This practically means that I will not be able to comment here anymore.

It was an interesting experiece, even if unsuccessful. best wishes everybody.

9. **woit**
October 18, 2006

About Gina's comment:

As the volume of comments has increased here, it has become more and more difficult to keep the noise level down. There are far too many people who think it is a good idea to repeatedly post comments that are off-topic, unsubstantive, uninformed, and too often a waste of everyone's time. Dealing with this is taking up more time than I can afford, especially difficult is the phenomenon of people who sometimes post something sensible and interesting, but all too often something that isn't. I then have to spend my time trying to decide what to do with each of their comments, and dealing with complaints from them about the ones that I deleted. I don't have the time for this.

Please help me by only posting comments that are on-topic, substantive and well-informed. "String theory sux" or "string theory rules" comments are not welcome. If it's a technical issue that is the topic, I strongly encourage people with expertise to contribute and enlighten us, and equally strongly discourage people who are not informed about the issue from adding to the noise level by telling us all what they think.

Thomas,

I'd like to make clear that, as far as string theory goes, I have a simple goal here, and it's not to vanquish string theorists in battle. It's to provide accurate information about string theory, both about those parts of the subject that are not working, and about those that are more promising. My belief is that if the physics community were operating with a much more realistic and accurate view of what parts of the string theory program are getting somewhere, and which have failed, particle theory would be a much healthier subject. One aspect of this would be greater willingness to encourage research on non-string theory formal quantum field theory research.

10. **surlygrad**
October 18, 2006

Re: "The Rest of Physics"

God forbid some of us should be interested in topics (many particle systems, heterogeneous materials, etc.) that you physicists have deemed unworthy of your time and tossed to the engineers.

11. **Walt**
October 18, 2006

String theory sux.

12. **Walt**

October 18, 2006

Just kidding.

13. **TheGraduate**
October 18, 2006

I think a lot of what is missing in the comment section of this blog in general is an honest structured discourse. For one thing people tend not to respond to what other people have said. But even so this is a minor trait common to most blogs.

A more major problem, in my opinion, is the tendency of the discourse to careen off into minutae. It often has the form of a general discussion beginning on whether fast food is good for you and ending in a furious debate about the third decimal place in the solubility constant of FD&C Red No.40.

Isn't it fair to say there is a lot of ambiguity about the status of different propositions in any string theory discussion? Either a proposition is logically proven, a conjecture or substantiated by experiment. Either a proposition is marginal or it's critical to the argument. If one can already be satisfied on whether a particular proposition of string theory is marginal or critical and it's proof status then to me this is progress compared to how discussions usually progress on the blog.

Maybe there is a lack of good argument standard. I think in a physics argument there can only be two objective standards, substantiated experiment and mathematial proofs. Everything else is just what you feel inside (which you are allowed to feel but it's hard to say why what you feel is more important than what anybody else feels. And I think just because your feeling has an equation attached to it that doesn't make it suddenly more valid.)

(I often read the blog. I don't always comment on everything I see but I think I might have something sensible to say on this issue.)

14. **anon**
October 19, 2006

Peter, I think the problem is that you have outgrown your blog tools. In my opinion, any blog that gets more than 5 comments per article is already too burdensome. What you need is a blog in which (1)each article you post can grow threads (2) commenters grade each other. Free Software for this "uberblog" already exists; viz., Slashdot.

15. **Garbarge**
October 19, 2006

This blog could be an interesting place, and not just Lubos' trash can as once became, if one would be able to read the first and last comment and make any sense out of them. I dont read here that often, but I am amazed by the rapidity with which it degenerates into totally off topic discussions. In the other hand, and on the edge of toss my words into the garbage disposal too, I think the String Theory war has become a big parody/comedy with little or no contain. As long as the theater prevails over the science, I see no glean of fruitful thinking emerging from here. It is a pitty, this media has become such a powerful mean….

16. **A.J.**
October 19, 2006

One solution to the noise problem: Post on more technical topics! It's relatively easy to form and offer an opinion on controversies and pop sci reporting. But it takes a bit more effort to come up with something to say about e.g. stacks or elliptic cohomology.

Well…I guess there's always "stacks sux" or "Hopkins is teh rul3z0r".

17. **Thomas Love**
October 19, 2006

In "The Rise of the Standard Model'', Mark Bodnarczuk has an interesting article, "Sociological Consequences of the Standard Model", with this footnote:

Using numerous case studies, David Hull claims that not only are infighting, mutual exploitation, and even personal vendettas typical behavior for many scientists, but that this sort of behavior actually facilitates scientific development. David Hull "Science as a process"

So the rise of string theory is really not that different.

18. **ksh95**
October 19, 2006

Peter,

Find people you trust and give them admin access. This is similar to running a business, when you grow you have to add managers.

Gina,

I have a Ph.D in physics, yet I would never be arrogant enough to go to n-category cafe, start posting uninformed nonsense, jump in the middle of discussions I can't fully understand, and then sulk when my posts get deleted. **Instead**, I would read every day, try to learn as much as I could, and feel lucky that I was privy to such a high level discussion.
Any post I would make would be to thank the blog owners for making such interesting discourse public. OR, to ask for simple laymen explainations.

19. **Peter Woit**
October 19, 2006

Thanks for the suggestions. For a while I was interested in the idea of having a Slashdot-like self-moderated comment section. Then I read the comments associated with some of the items there about string theory. As far as I could tell, moderation was not working at all, with huge amounts of idiocy prominently displayed and substantive comments buried way down. I'm also skeptical of threaded comments, partly because they make it much harder to see what is new since one last looked, partly because they encourage people to get on off-topic threads, instead of encouraging a discussion of a single topic.

20. **John A**
October 19, 2006

Peter,

Can I say with the benefit of experience, that you cannot choose what level your commentators write at. As Aaron Bergman has amply demonstrated, sometimes people just simply don't want to express themselves in a rational way, but simply want to disrupt, flame or just be an asshole, hoping to drag down the discussion and turn away interested posters and shut down proper debate.

That said, my suggestions to you would be to shorten your posts to one or two key points, and invite your visitors to make discoveries/comments/reviews for themselves.

In my view you should take the initiative, talking about short topics in your book, or within other books and looking at alternatives to string theory in the same critical light.

You are not using categories on WordPress which is a mistake. This means that when a topic or topics covers a particular subject, and those topics disappear from the front page, then it disappears from your active discussions as well. Try to make good references both to your previous posts and to interesting comments in your newest postings.

In my view, its not the noise that's increasing, its the signal getting a little weaker.

21. **Hal**
October 19, 2006

To add something missing from ksh95's comment I would like to wish Gina well since she did, after all, give her best wishes to everyone.

22. **Aaron Bergman**
October 19, 2006

As Aaron Bergman has amply demonstrated, sometimes people just simply don't want to express themselves in a rational way, but simply want to disrupt, flame or just be an asshole, hoping to drag down the discussion and turn away interested posters and shut down proper debate.

So that's why I'm here.

I've been trying to figure it out myself….

23. **A.J.**
October 19, 2006

John A,

What are you talking about? Aaron's consistently been one of the most reasonable and well-informed commenters here.

24. **Peter Woit**
October 19, 2006

John,

Please stop with the attempt to start a flame-war. I can understand why Aaron had the reaction he did to the things he quoted, just wish he'd ignore some things, make more substantive comments on others. There's a lot of less than completely intelligent stuff posted here in the comment section, even after a fair number of deletions. People are encouraged to ignore such stuff and not feel that it is necessary to respond.

25. **Kent G. Budge**
October 20, 2006

"is largely intended for people who work professionally on the issues in math and physics that I'm interested in"

When I was a new astronomy graduate student at Caltech, I sometimes wondered if I ought to switch over to theoretical physics, since I believed I had much more aptitude for mathematical analysis than for observational and experimental technique.

However, during my first year, I had the poor judgement to ask a dumb question at a physics department seminar. I wasn't trying to start a flame war and I wasn't being *deliberately* ignorant; I was a young graduate student struggling to understand some of the basics of quantum field theory. However, the speaker thought it appropriate to take five minutes to respond to my question by berating my stupidity and wondering out loud how I ever got to Caltech.

At the time, I was humiliated and deeply hurt. However, in retrospect, I think the speaker may have done me a favor, by disabusing me of the notion that the physics department at Caltech saw itself as a teaching institution, and by steering me away from what would probably have been a poor career choice, given my dislike for feeling stupid and for getting in arguments.

Perhaps you are doing me a similar favor here. S'long.

26. **woit**
October 20, 2006

Kent,

I'm not berating anyone for stupidity or questioning their credentials. If someone who doesn't know what is going on asks an uninformed question in a seminar, it's perfectly appropriate for the speaker to say that he or she doesn't want to take everyone's time by answering it, especially if the answer is not something that can be given in a sentence or two. It's not appropriate to do this in an insulting way.

Similarly, here I also need to deal with uninformed comments, sometimes by telling the commenter that this is not an appropriate place for the kind of comments they are making. I hope that I do so in a polite way.

27. **ak**
October 20, 2006

anyway, I point out that a seminar circles by definition around a well-formulated subject or theme, it is by definition designed to fit persons with similar 'background', knowledge base and interest. At least as I understood the internet blogosphere is of a fundamental different character and this does not exclude excpetions as 'the n-category cafe' as it was mentioned. Undenied the fact that any blog owner might design his or her blog in a given way or directed to a chosen public and might do so by explicit forms of moderating, at least from my point of view this was not the initial aim of the phenomenon 'blog'. That certain characteristics of this initially 'liberal' form of electronic discussion also carry over to scientific forms cannot be regarded as a pure 'noise phenomenon'. The more informal and liberal form makes it possible to share information flow between possibly poorly overlapping subjects and standpoints and people with very different knowledge backgrounds. As far as physics and mathematics as classically highly impenetrable subjects for non-experts share responsibility to communicate with the 'outer world', the scientific blogosphere possibly

could realize this with relatively low levels of formal and financial effort. To be more concrete: as it ssemed to me Gina, fo instance, did have a scientific background, which while it aparently did not quite overlap with particle physics (chemistry?) does not quite exclude her form any form of rational reasoning.
The scientific blogosphere could well be aware of even much more radically differing forms of scientific reasoning having potential and justified interest in informal discussions with the mathematical or physical world, I mention philosophers, sociologists as already discussed, not to mention cultural scientitists or even artists, baring viewpoints which could 'possibly' be of interest or benefit for physicists or mathematicians themselves and who up to now share mostly very deformed and wrong views of scientific knowledge in the mathematical or physical sphere, I think the most 'distant' views shared in this blog came from engineers, so I do in fact think the situation could be much 'worse' (or better, what you prefer).

28. **Peter Woit**
October 20, 2006

ak,

A "blog" is just a software tool, and people are using it with many different purposes. Many scientists have blogs designed to try and explain and promote science to the widest possible public. Many people would like this blog to be a forum for the discussion of their and other unconventional ideas about physics. Those are fine things to do, I'm just not interested in doing them. One reason is purely a lack of time: maintaining this blog is already taking up too much of my time and I'm trying to figure out how to change that.

Sorry to be a bit obnoxious about this, but one of the whole points of the blog technology is to allow people to try and create an information source and discussion forum of whatever kind they want. I'm trying to make one for people who share some of the same interests as me, with the highest possible level of substantive material. Doing this requires deleting comments from people who want to use this to discuss something I'm not interested in, as well as comments that are uninformed. One obvious thing to point out to people who don't like this is that the technology is free, you're welcome to create your own blog, and there you can do exactly what you want.

29. **Jud**
October 20, 2006

I thought the suggestion about allowing some trusted folks to perform moderation was not an awful idea, though of course I don't know what your supply of such folks may be.

30. **Tony Smith**
October 20, 2006

Brian Greene's 20 October 2006 New York Times Op-Ed says:
"… at high energies, the electromagnetic and weak nuclear forces seamlessly combine … at even higher energies the strong nuclear force would also meld …
For decades, however, the force of gravity stubbornly resisted joining the fold. …
Time and again, attempts to merge the two theories resulted in ill-defined mathematics
…
Such was the case until the mid-1980's, when a new approach, string theory, burst onto the stage. Difficult and complex calculations … gave compelling evidence that this new approach … unified gravity and quantum mechanics …".

There is a very significant omission in Brian Greene's recital of historical "context":
In fact,
the first approach that successfully "unified gravity and quantum mechanics" was supergravity, in which gravity and other forces were unified by the use of Lie superalgebras and description of gravity by mechanisms of the MacDowell-Mansouri type, using either the anti-deSitter group Spin(2,3) = Sp(2) or the conformal group Spin(2,4) = SU(2,2).

Interest in supergravity waned in the mid-1980s when it seemed difficult to get it to contain the Standard Model groups and it also seemed that it might not have the cancellations needed for finiteness (although the difficult calculations have yet to be done).

Then, as Brian Greene said, in the mid-1980s conventional superstring theory became dominant in the high-energy theoretical physics game, and it maintains its dominance even now.

As Brian Greene said, the basic idea of conventional supertstring theory is to change the view of "matter's fundamental constituents" from "point-like dots of virtually no size" to "minuscule, vibrating, string-like filaments".

The competing basic idea of supergravity, ignored in Brian Greene's discussion of history, is to change the view of forces from being based on the generators of Lie algebras to being based on the generators of Lie superalgebras.

While the conventional superstring theory approach focuses on a new view of matter as filaments rather than points,
the supergravity approach focuses on a new view of the setting for force generators.

Even if changing the setting from Lie algebras to Lie superalgebras might not have been totally successful,
there are some other natural settings for the force generators (one of which is to look at them as root vectors, which is what I like to do)
and
I feel that such generalized/modified-supergravity approaches should be investigated as alternatives to conventional superstring theory,
especially
since supergravity (despite Brian Greene's omission of it from his recital of historical "context") was actually the first theory that successfully "unified gravity and quantum mechanics".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

31. **Gumbi**
October 20, 2006

In some ways the dispute reminds me of the old debate about the Many-Worlds Interpretation of QM. The MWI is just like textbook QM except there is no wave function collapse. Instead, proponents argue that in a universe with no collapse, macroscopic observers would see the illusion of collapse, which is consistent with what we observe.

This may be good philosophy but the problem from the scientific perspective is that the MWI is too perfect. It reproduces the predictions of regular QM so well that no experiment can distinguish them.

We are left with philosophical arguments – MWI proponents advocating its somewhat more parsimonious axiomatic basis, while opponents object to its version of the "landscape". All those parallel worlds, all those alternate histories and variations on ourselves, all as real as we are. It's a lot to accept.

Although the analogy is not exact, we might end in a similar situation with regard to string theory. If the string theory landscape is not falsifiable, then we can't prove the string theory landscape is real; but we also can't prove that the idea is wrong. In the end it may be a matter of philosophical taste as to which theoretical approach produces the most attractive and elegant model of the universe.

32. **Peter Shor**
October 20, 2006

I'm going to stick my neck out and say you're being too harsh on Brian Greene in at least one respect. I think that having a unified theory of gravity and quantum mechanics would be a big step forward (not that string theory is actually one yet).

Why do I say so? Because I suspect that all consistent unified theories of gravity and quantum mechanics are going to share a lot of properties, and if we could figure out what some of these properties are, then it might help us to figure out the right theory of quantum gravity. I believe that it is quite possible that any consistent theory of quantum gravity is going to lack one of the things we think are absolutely essential, for example Lorentz invariance, unitarity, or locality. (All three of these seem to be crucial in formulating the black hole information loss paradox.) One possible reason we haven't come up with any reasonable theories of quantum gravity might be that we're looking in the wrong place. So if we could formulate a non-perturbative theory of string theory (even in 10 or 11 non-compactified dimensions), it might teach us something something about all theories of quantum gravity.

I'm sure that one of the string theorists reading this will object and say that string theory has all these properties, and is also a consistent theory of quantum gravity. But my understanding is that we only "know" that perturbative string theory is unitary, and that this theory lives in the limit where the curvature of space-time doesn't change. The black hole information loss paradox is perfectly consistent with unitarity in this limit.

In fact, this is where I first lost some of my trust in string theorists. Shortly after quantum fault tolerance was discovered, I was talking to a famous string theorist, and trying to explain to him Kitaev's result about error correction with anyons, which I was very excited about at the time. This result led Preskill (I believe) to speculate that the laws of nature might be non-unitary at bottom, but that this non-unitarity is masked by an inherent error-correction process, which makes everything look unitary at any scale significantly larger than the Planck length. He was very dismissive, and said that it had been proved that any non-unitarity at small scales would also show up at large scales. Since this result was exactly contrary to the one I was trying to explain to him, I asked where this proof was, and how it worked. He couldn't tell me exactly who had proved it, or whether it was written down, and he muttered something with the words "at least in the generic case …" in it. At this point, I gave up.

I still think that Kitaev's result shows that it's possible that the true theory of quantum gravity is not unitary. Unfortunately, I have come to realize that it gives very little guidance as to where to look for a non-unitary but self-correcting theory of quantum gravity, so it seems unlikely that anybody will come across the correct theory of quantum gravity starting from this direction.

33. **Garbage**
October 20, 2006

To Peter Shor,

http://arxiv.org/abs/gr-qc/0603090

http://arxiv.org/abs/hep-ph/0604255

and why not.

http://arxiv.org/abs/quant-ph/0507262

I agree, unitarity is not such a sacred concept. Also, axiomatic constraint may be very well tested.

34. **Peter Woit**
October 20, 2006

Peter Shor,

My comment just reflects my often expressed worry about quantum gravity: what if there are lots and lots of them, and there is no way to ever get any experimental evidence about which one corresponds to reality? If you believe the most optimistic claims of string theorists these days, they have not one consistent theory of quantum gravity, but 10^500, and nothing at all about these theories is experimentally testable. This seems to me to be highly problematic.

It's certainly true that you may learn something generic about quantum gravity by thinking about all these models. But, I'm really a particle physicist, and you're not going to learn anything about particle theory by thinking about them. Where string theory has failed is not as a theory of quantum gravity, but as a theory of particle physics. If string theorists would just admit that, and formal particle theory was no longer so heavily dominated by string theory, we'd be a lot better off.

35. **anonymous**
October 20, 2006

I have never heard a string theorist say string theory gives rise to 10^500 theories of quantum gravity. Many of them think there is one theory with 10^500 solutions. It is a very big difference.

36. **Garbarge**
October 20, 2006

"Many of them think there is one theory with 10^500 solutions."

Well, it might be 5 or so after all

Peter, I think the main purpose of ST is to provide a theory of quantum gravity, which happens to include all of particle physics [Something inevitable, if you believe "Georgi"]
String theory has some achievements in that area. Like the entropy calculations/predictions for extreme BH, although it fails in more reasonable circunstances. Also one could say the ADS/CFT correspondence opens the bridge to understand gravity as a field theory as well. I agree ST does sort of what Peter Shor says, although still falls short to get the M-picture. I personally think the "background independence" issue is a big *problem* not yet fully addressed/understood. I asked once Ed Witten about this, his answer was:

"…another point is that though string theory was discovered historically in the context of a fixed spacetime background, it turns out that the string describes fluctuations in the geometry, though we don't understand very fully how this happens."

37. **woit**
October 20, 2006

anonymous,

I'm well aware of what the string theory ideology is, with its "one unique string theory", which no one actually knows the definition of. The fact of the matter is that current known definitions of string theory depend on a choice of "background", and many string theorists believe there are 10^500 or more different such choices.

38. **Chris W.**
October 20, 2006

> I have never heard a string theorist say string theory gives rise to 10^500 theories of quantum gravity. Many of them think there is one theory with 10^500 solutions. It is a very big difference.

Given that any one of those solutions is potentially an effective theory for describing the entire *observed* universe, and a failure of any one of them in fulfilling that goal merely sends us off on a wild goose chase through the other 10^500 looking for a solution that does work, one can say that we have 10^500 theories, for all intents and purposes.

We would be in somewhat the same predicament in atomic physics (non-relativistic quantum theory, shall we say) if it allowed for millions of solutions (stable or meta-stable configurations), we only had only example of such a system to test the theory against, and we couldn't independently observe the configuration to prevent the theorist from saying "no, wait, it's not a helium atom in state X," or "it's actually a boron atom in state Y [or an iron atom in state Z, etc, etc]". Now think of the options afforded us by a vast array of vacua, not to mention braneworld scenarios, and on and on.

Arguably, the central lesson of investigations into quantum gravity is that the issues are *inherently* cosmological in character. This enormously raises the stakes and and increases the risks of running off the rails epistemologically and methodologically. I can see why a sober particle physicist would just as soon avoid the entire territory while attempting to understand the existing problems of the Standard Model. The question is, is this really possible? What could a satisfying solution of these problems look like without involving gravity, or the dynamical structure of spacetime, in an essential way?

39. **Aaron Bergman**
October 20, 2006

The fact of the matter is that current known definitions of string theory depend on a choice of "background", and many string theorists believe there are 10^500 or more different such choices.

If you want to be a stickler about it, none of the examples with all moduli stabilized have a description as a perturbative string theory at all because of the presence of Ramond-Ramond fluxes. However, if you believe that string theory has supergravity as a low energy limit, you can tunnel between vacua and see that they cannot be considered separately.

On the other hand, Tom Banks might argue that none of these things will change the asymptotics of the solution and that different choices of asymptotics should be considered different theories.

Frankly, I'm not sure why anyone should care whether string theory should be thought of as a framework (like QFT) or as one theory with zillions of (not-so-rigorously constructed) vacua. What matters are the results of the calculations that you do.

If you believe Steve Carlip, there are plenty of inequivalent quantizations of 2+1D quantum gravity, so perhaps the same holds 3+1 also. I still think that string theory is interested because, at least with AdS/CFT, I believe that it really is a theory of quantum gravity. As such, even if it's not the right theory of quantum gravity, we can learn things about how the usual problems associated to quantum gravity might be solved. String theory has succeeded towards that end in some respects (black hole entropy, for example) but not entirely in others. (The black hole entropy calculation does not 'fail' in far-from-extremal situations as in give the wrong answer, in case there was some misunderstanding; it is just not doable yet.)

For Peter Shor, we know that AdS/CFT is unitary, and there the metric is fully dynamical. This argument is essentially what convinced Stephen Hawking as I remember it.

40. **Moshe**
October 20, 2006

Peter Shor, in my mind there are some indications in string theory that black hole physics does not require modifying QM, but there is nothing I know of that amounts to a watertight proof.
\
I am curious though about something related, in particle physics circles it is common belief that QM is "rigid" in the sense that there are no consistent "small" modifications of QM (e.g adding small non-linear terms to Schrodinger equation), example that is cited often is Weinberg's attempt to modify QM.
\
On the other hand it is my impression that in other communities the sitaution is different. So, out of curiosity, are you aware of any model that modifies QM and reduces to it for observable physics? Do you feel like something like that ought to exist?

thanks,

Moshe

41. **nontrad**
October 20, 2006

Aaron B,

Thanks for bringing attention to 2+1 QG. In the above posts discussing the posibility of several QG's I immediately thought of 2+1, which is an odd place / 'laboratory' in more ways then one.

Again, thanks for bringing attention to this point.

Regards,

nontrad

42. **anon.**
October 20, 2006

For Peter Shor: one of the arguments that is sometimes referred to in the context of claims that modifications of unitary QM are dangerous is a paper by Banks, Peskin, and Susskind, motivated by the black hole information problem:

http://www.slac.stanford.edu/spires/find/hep/www?key=1135198

They argue that modifying QM to try to get around the black hole information problem leads either to acausal signal propagation or to violations of energy-momentum conservation. They further argue that these effects are not suppressed by powers of the Planck scale. Their argument applies to a class of modified QM theories with a "superscattering matrix."

I'm not expert in these things and I don't know what the space of all reasonable modifications of QM looks like, so I don't know whether there is any danger for the sort of thing you have in mind. I am fairly confident, though, that this is the paper that has devolved into the sort of "folk theorem" you mention that "any non-unitarity at small scales would also show up at large scales."

43. **Tony Smith**
October 21, 2006

Peter Shor said "… Kitaev's result about error correction with anyons … led Preskill (I believe) to speculate that the laws of nature might be non-unitary at bottom, but that this non-unitarity is masked by an inherent error-correction process, which makes everything look unitary at any scale significantly larger than the Planck length. …".

John Preskill said in his paper Fault-Tolerant Quantum Computation, quant-ph/9712048:
"… In Kitaev's spin models, we might imagine that localized processes that destroy quantum information are quite common. Yet were we to follow the evolution of the system with coarser resolution, tracking only the information encoded in the charges of distantly separated quasiparticles, we would observe unitary evolution to remarkable accuracy; we would detect no glimmer of the turmoil beneath the surface. …".

Stephen Adler said, in his book Quaternionic Quantum Mechanics and Quantum Fields ((Oxford 1995):
"… If the multiplication is associative, as in the complex and quaternionic cases, we can remove parentheses in … Schroedinger equation dynamics … this … fails in the octonionic case, and hence one cannot follow the standard procedure to get a unitary dynamics. …
[so there is a]… failure of unitarity in octonionic quantum mechanics…".

Would the Preskill idea of non-unitarity at Planck energies be consistent with an octonionic-non-unitary model at Planck energies that becomes quaternionic-unitary by "freezing out" a preferred quaternionic subspace at sub-Planck energies ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

44. **Garbarge**
October 21, 2006

hey anon,

you might like to read this,

http://arxiv.org/abs/gr-qc/0305098

along with the other references I pointed out before.
There is no real *danger*, unitarity isnt such a sacred principle. One can conserve probabilities and charges (including H) without it. This has been known for a while in the theory of open systems (Lindblad formalism) . The nice thing about these papers is that the modified Schroedinger dynamics is derived from quantum gravity ideas, and applies independently of any other environmental decoherence effect. Also that the black hole paradox is rendered *practically* unobservable

http://arxiv.org/abs/gr-qc/0501027

http://arxiv.org/abs/hep-th/0406260

and provides a conceptually satisfatory solution to the problem of time,

http://arxiv.org/abs/gr-qc/0402118

There are indeed attempts to experimentally detect this type of effects,

http://arxiv.org/abs/quant-ph/0406007

nice reading… 🙂

bed time…

45. **Peter Shor**
October 21, 2006

Anon,

Many thanks for the reference. I've looked at it now, and I'm convinced you're right: this is probably the paper that turned into the folk theorem. As for AdS/CFT, I'm not going to be completely convinced by this until somebody can explain exactly why the black hole information paradox goes away in AdS/CFT. I've discussed this with a couple of string theorists, and they admitted they didn't understand it completely themselves.

Peter Shor

46. **Lee Smolin**
October 21, 2006

Dear Peter Shor,

If it really is the case that fault tolerant quantum computing can provide a counterexample to the Banks-Peskin-Susskind argument that is very worth working through carefully and writing up.

As to small testible modifications from quantum theory, it has been known for a long time that one can add terms to the schroedinger equation that are non-linear in the wavefunction Psi, which violate unitarity but not conservation of probability and then use precision experiments like the Lamb shift or neutron diffraction to bound their coefficients. (I wrote such a paper in the mid 80s but it was already an old idea then.)

As to consistent quantum theories of gravity which show that general principles like Lorentz invariance must be modified, let me recommend the recent Freidel-Livine work on 2+1 gravity with matter, which shows that Poincare invariance is necessarily quantum deformed as a result of including quantum gravity. (btw, not for the first time, I have no idea what the ambiguity is in 2+1 quantum gravity some people keep refering to. Carlip wrote about different appraoches to 2+1 gravity but my understanding is that with the same matter content they all give the same theory.)

Thanks,

Lee

47. **Aaron Bergman**
October 21, 2006

Let me quote from Carlip:

> Perhaps the most important lesson of (2+1)-dimensional quantum gravity is that general relativity can, in fact, be quantized. While additional ingredients—strings, for instance—may have their own attractions, they are evidently not necessary for the existence of quantum gravity. More than an "existence theorem," though, the (2+1)- dimensional models also provide a "nonuniqueness theorem": many approaches to the quantum theory are possible, and they are not all equivalent. This is perhaps a bit of a disappointment, since many in this field had hoped that once we found a self-consistent quantum theory of gravity, the consistency conditions might be strin- gent enough to make that theory unique. In retrospect, though, we should not be so surprised: quantum gravity is presumably more fundamental than classical general relativity, and it is not so strange to learn that more than one quantum theory can have the same classical limit.

48. **Lee Smolin**
October 21, 2006

Carlip concludes however (p 46).

"A more general problem is to understand which of the approaches described here are equivalent. In particular, it is not obvious how much of the difference among various methods of quantization can be attributed to operator ordering ambiguities, and how much reflects a deeper inequivalence."

Here is why I suspect that it is the former. Let us consider the case of no matter, to which his comments mostly refer. Given a compact two topology and a value of Lambda, there is a fixed finite dimensional physical phase space for 2+1 GR. Given that the physical phase space is finite dimensional, there are only two ways that one can get inequivalent quantizations. First, from different choices of subalgebras of the physical phase space algebra to be quantized. Second, from different representations of those algebras. Both of these kinds of ambiguities are well understood in conventional quantum mechanics. One cannot expect the situation here to be better than in ordinary QM, but if it is no worse than it is misleading to say that different approaches lead to different theories.

49. **Hmm**
October 21, 2006

Peter Shor–

Talk to better string theorists; the understanding of the information paradox is now relatively standard. The process of black hole formation and evaporation in AdS space can be desribed in the unitary boundary CFT. Furthermore, for the case of eternal black holes in AdS, there is a sharp form of the information paradox and its resolution as discussed by Maldacena in 2001, hep-th/0106112; this was the paper that convinced Hawking to capitulate. The sharp form of the paradox in this case is this: correlation functions for (say) a scalar field in the BH background naively fall exponentially to zero–whereas a unitary description would say that it can't get smaller than exp(-S). Of course the dual CFT tells you precisely this. There is a separate question of how to understand the breakdown of Hawkings original argument. Maldacena (and subsequently Hawking) suggested that this arises from a sum over all bulk geometries, including ones where the BH doesn't form, but this is still controversial. This does not change the fact that AdS/CFT provides the correct answer-no information loss.

With all due respect to you and your great accomplishments in quantum computing, your questions and comments on quantum gravity are somewhat naive. This also goes for a number of your "the world in a quantum computer!" colleagues too. This makes your insinuations that the string theorists don't know what they are talking about a mite annoying. Perhaps in the future you should consult some of the standard literature. Of course actually being an expert in some subject and actually knowing what one is talking about is completely at odds with the ethos of the blogosphere, especially here…

50. **woit**
October 21, 2006

Garbage,

My apologies. Your last comment ended up in multiple copies in the spam queue (presumably because it had multiple links, something this mysterious software is very suspicious of). I was trying to delete most copies, keep one, managed to irretrieveably delete them all. Please repost if you still have a copy.

Peter

51. **Aaron Bergman**
October 22, 2006

One cannot expect the situation here to be better than in ordinary QM, but if it is no worse than it is misleading to say that different approaches lead to different theories.

I can't quite understand this sentence — perhaps it's missing a word? Anyways, I think the point is that there are physically different theories of quantum gravity in 2+1D, ie, they have the same classical limit but different quantum predictions. For whatever reason this may be true, it would be very interesting to know how true it is in 3+1D (or more).

52. **Jean-Paul**
October 22, 2006

Johnson's talk was completely disgusting. I did not know that he was the journalist who wrote the (in)famous article on "probing string theory". Actually, he mentioned it,

and seemed quite proud of it. The seminar was from "we journalists know…" to "you string theorists you know" how to fool the public. What was most striking that there was no mention or even speculation on what is the impact of sensational journalism on public who reads it. Both Johnson and the audience did not care at all about readers' perception. No mention of the basic journalists' obligation to be objective. Instead, contempt for "ignorant" public. Very cynic remarks on "to be sure" paragraphs with a very fine print "it's just a hype". The moral was: we journalist look for a story, and the more hype and confusion you physicists create, the more stories we can write.
We are used to the sensational coverage of the entertainment world, but science writers are no better…

53. **Arun**
October 22, 2006

Started watching Johnson's talk and was quickly reminded of Gartner's Hype cycle, which should be familiar to people in the technology sector.

http://www.floor.nl/ebiz/gartnershypecycle.htm

I wonder if String Theory maps onto this cycle. Perhaps it is past the Peak of Inflated Expectations, but where it is beyond that point is beyond me.

54. **Arun**
October 22, 2006

I'm halfway into the Johnson talk, and I'm not very impressed by the audience either.

55. **Who**
October 22, 2006

I disagree with your take on it, Arun. The talk by Johnson was informative and increased my respect for his professionalism as a science journalist. One got a sense of what journalists do to successfully communicate their interest in science to the public—and the constraints within which they work.

Also the Q and A, the running dialog he had with KITP, really helped me to better understand the points of view of the string theorists in the room. And I think that Johnson spoke very well (and basically reassuringly) to their concerns.

I found that I could not watch the video very well in STREAMING mode because it kept stopping—-maybe due to heavy traffic. So based on my experience I woul recommend that anyone interested in watching should DOWNLOAD the whole movie (in my case "quicktime") file so that they can watch continuously and at their convenience.

this conversation is extremely informative and interesting—-and, as I say, gives me the highest impression of Johnson's cool, senses of humor, likeablility, and professionalism. One also gets to know KITP people much better, and their viewpoint and concerns. Instead of reacting scornfully, or churlishly, to this excellect video we should thank and congratulate KITP on contributing to improved understanding of all these issues. (sorry to disagree so strongly, Arun—I usually appreciate your posts very much.)

56. **Kris Krogh**
October 22, 2006

I agree with Who that Johnson's talk was excellent. His standards for scientific objectivity are much higher than Amanda Peet's.

In the comments afterward, David Gross complains about Lee Smolin's book. He says Smolin privately acceded to his argument that a background-independent version of string theory exists, but wrote the opposite. I'm hoping Lee will give his side of that.

57. **LDM**
October 22, 2006

I also enjoyed the Johnson talk…some comments (on the need for higher dimensions…) were very funny.

It is unbelievable that one theorist had not read the books, but still had strong opinions about their accuracy.

At one point, it almost seemed like there was an attack on Smolin and loop quantum gravity, which was a little strange, because the arguments against string theory do not rely on loop quantum gravity in anyway.

58. **Arun**
October 22, 2006

Who,
No need to be sorry for disagreeing strongly with my opinion. At most, I am put to the trouble of explaining what I heard.

Of the persons who spoke and could be heard, Johnson was the best. I did appreciate his telling someone to read the book first. The crowd was dismaying, with its "Swollen" and its challenge to Johnson to poll the room on whether Smolin is a crackpot. Also, the (perhaps my misinterpretation?) that Smolin and Woit belong to the same category of marginal people as John Horgan. I too want to hear Smolin's side of his alleged two-facedness.

The whole thing was vaguely reminiscent of the discussions I have seen on the Pakistani GeoTV channel about Pakistan's problems stemming from its bad image, the right PR formula is yet to be found. (I'm not Pakistani, fyi). Bringing string theory to the public cannot be called popularization of science, at least by the standards of my youth, where the popular science books (that were available to me) were always about well-established science. Kaku, Greene, etc., did the first disservice by hyping these in public, and if the string theory community did not feel discomfort then (did it?) it is a bit much if they protest now that there is a dehype. If Woit and Smolin were the first to bring string theory to the public, I'd agree that the ST folks irritation would be justifiable. However, from the POV of "what will the public think?", Woit and Smolin are a correction to the backdrop set by Greene, et. al.

——

If it is science we want to discuss and not PR, then the root problem is not yet addressed. Let's agree that primarily nature and secondarily limited funding have reduced the ability of particle physicists to have the close interplay with experiment that they used to have. Then we need new criteria on how to objectively say that we are not wrong so far in any particular line of research. Perhaps it is there implicitly within the community, it now has to become more explicit. Then one will also know what to tell the public.

59. **Peter Shor**
October 22, 2006

Dear Hmm,

Let me say that if string theorists routinely insult people who are trying to understand aspects of string theory, they are not going to get very many converts to their side.

I am aware of the "universe is a quantum computer" contingent, and I am not ashamed to say that I am not very impressed with their reasoning (unlike some string theorists, who will never criticize each other in public).

Best regards,

Peter Shor

60. **Jean-Paul**
October 22, 2006

Arun, Kris and Who,
With all due respect, I am afraid that you are missing the point. Johnson is a journalist. He can listen to tirades on background independence, attacks on the competence of Smolin/Woit but he cannot make his own judgment, and he admits it. Similarly, I would not discuss with him the issue of scientific objectivity. What I would like to hear is not a bundle of cynical jokes how journalists work, but a serious discussion of JOURNALISTIC objectivity and responsibility. When you write an article about "probing string theory" you would better ask 5 or so experts who would give you a wide range of opinions. Similarly when you write about a "crisis in ST".
Do you think that Johnson does a better job than somebody writing for the Enquirer? Certainly, science should be popularized, but at what expense? Is it OK to mislead the public? I wish Johnson could address these questions.

61. **Lee Smolin**
October 22, 2006

Hi, to respond to David Gross's claim mentioned by Kris Krogh above, I have been both clear and careful in the book. For example on p 240:

"Thus many quantum-gravity theorists believe there is a deeper level of reality, where space does not exist (this is taking background independence to its logical extreme). Since string theory requires the existence of a background-independent theory to make sense, many string theorists have indicated that they agree. In a certain limited sense, if the strong form of the Maldacena conjecture (see chapter 9) turns out to be true, a nine-dimensional geometry will emerge out of a fixed three-dimensional geometry. It is thus not surprising to hear Edward Witten say, as he did in a recent talk at the Kavli Institute for Theoretical Physics at UC Santa Barbara, that "most string theorists suspect that spacetime is an 'emergent phenomenon,' in the language of condensed matter physics."2
Some string theorists have finally begun to appreciate this point, and one can only hope they will follow up by studying the concrete results that have already been obtained. But in fact, most people in quantum gravity have in mind something more radical than the
Maldacena conjecture.

The starting point is nothing like geometry. What many of us in quantum gravity mean when we say that space is emergent is that the continuum of space is an illusion…."

In more technical language, what has usually been meant by background independence is that no classical metric, field or global symmetry appears in the definition of a theory. Thus, by definition, formulations of a theory with asymptotic boundary conditions are excluded. This definition, and the motivation for it is discussed many places in the literature, for example in my recent hep-th/0507235. For a discussion of why the Maldacena conjecture does not satisfy what is usually considered background independence, see pages 23 and 24. There I am responding to various discussions with string theorists including David Gross, who insisted that string theory does not need a background independent formulation, because the AdS/CFT correspondence gives string theory a non-perturbative definition.

However, in my most recent conversation with Gross, he argued instead that the AdS/CFT correspondence should be considered to satisfy a form of background independence. Recently others made the same argument to me. Their claim is that if the strong form of the AdS/CFT conjecture is true, a dynamical ten dimensional asymptotically AdS spacetime will have arisin out of a theory defined on 4d Minkowski spacetime with global Poincare symmetry. This does not satisfy the definition just given. But it is true that 6 of the ten dimensions transmuted from the space of scalar fields in the N=4 SYM theory to dimensions of space in the dual theory. In this limited sense a weak form of background independence will have been achieved. This is what I agreed to in conversation with David and others.

But, as I thought I made it clear in these discussions, there is a big difference between this and what has usually been meant. Classical GR achieves this usual meaning, as do LQG, spin foam models, causal sets, causal dynamical triangulations, and not, at least so far, string theory.

In a recent discussion, Brian Greene proposed a new terminology to straighten out the confusion. Brian proposes to use manifest background independence for what quantum gravity people and philosophers have up till now meant by background independence and background independence for the weak form satisfied by the (strong form of the) AdS/CFT conjecture.

If it helps to distinguish between a strong or manifest form of background independence and a weak form, then I can agree to use this language. But I don't see that changing the meaning of technical terms advances the issue. Brian and others (perhaps not David) agree that string theory should have a manifest or strong background independent formulation, and that it does not yet.

By the way, each time I note that string theory does not have the property of strong or manifest background independence, this is more than anything a criticism of myself as, to my knowledge, few others took the problem of making such a formulation of string theory seriously enough to spend years working on it, as I did.

Thanks,

Lee

Ps Just in case anyone thinks it is plausible that I and other people in LQG be considered crackpots, they can consult my cv which is at
http://www.thetroublewithphysics.com. I didn't listen to the tape, but if someone actually said that they should be ashamed of themselves.

Indeed, what is remarkable to me is that no one in this or, to my knowledge, any other discussion of my book says something like, "On page x of Chapter 12, Smolin says A and that is false for this reason. This is not just because some string theorists have not read the book, because if no one at KITP has, I know of others who have read it in detail, and if they had found errors of fact they would have said so. I wouldn't be surprised if this had happened, because in the chapters leading up to the evaluation of string theory I give in Chapter 12 a large body of technical results is summarized in non-technical language. It is very hard to do this and not get something wrong, even in one's own field, and I worked very hard, and checked and double checked with experts to be sure of having the facts right. Of course, if anybody finds such an error I would be grateful.

62. **Arun**
October 22, 2006

Perhaps it would be best to download and listen to the podcast (15 MB, quicktime player would suffice to view/listen).

63. **Kris Krogh**

October 22, 2006

Thanks Lee!
—
Jean-Paul,

For me, there's no distinction between scientific and journalistic objectivity. Isn't there only one truth? By urging a string theory audience to read Smolin and Woit's books, I think Johnson showed more concern for objectivity and the "scientific method" than many of the scientists he spoke to.

Especially in theoretical physics, there seems to be an unhealthy tendency to decide issues on authority, instead of blindly weighing evidence. (Like the woman with the scale.) Just vote on the smartest person (or who's a crackpot) and call their ideas reality.

64. **Lee Smolin**
October 22, 2006

Just to be sure, regarding David Gross's comments, I also wrote on page 189 of my book: "…if the strong form of the Maldacena conjecture turns out to be true — which is also consistent with the present evidence — then string theory provides good quantum theories of gravity, in the special case of backgrounds with a negative cosmological constant. Moreover, those theories would be partly background-independent, in that a nine-dimensional space is generated from physics in a three-dimensional
space.

There is other evidence that string theory can provide a unification
of gravity with quantum theory…."

The text was finalized before David and I had the conversation he is refering to in July and, as you can see, the text is consistent with his memory of what I said. Shall I conclude that he also commented on my book without checking to read it first?

Thanks,

Lee

65. **Tony Smith**
October 22, 2006

Arun says: "… The crowd was dismaying, with … its challenge to Johnson to poll the room on whether Smolin is a crackpot. …".

Lee Smolin says: "… Just in case anyone thinks it is plausible that I and other people in LQG be considered crackpots, they can consult my cv which is at http://www.thetroublewithphysics.com. …"
and goes on to say:
"… no one in this or, to my knowledge, any other discussion of my book says something like, "On page x of Chapter 12, Smolin says A and that is false for this reason. …".

Kris Krogh says: "… Especially in theoretical physics, there seems to be an unhealthy tendency to decide issues on authority, instead of blindly weighing evidence. … Just vote on … who's a crackpot …".

Since Kris Krogh is in the psychology department at UC Santa Barbara, which is the host institution for KITP where the talk in question took place,
maybe he could shed some light on the psychology of why the KITP superstring community is relying on authority and on "vote[s] … on …. who's a crackpot", instead of
on substantive discussion such as "… Smolin says A and that is false for this reason …".

If Kris Krogh were to prefer to defer from shedding such light himself (possibly saying something like he is a "Senior Electronics Technician" rather than a practicing psychologist),
perhaps he could prevail upon some of the practicing psychologists in the UCSB psychology department to shed such light.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

66. **Garbage**
October 22, 2006

Thanks Peter, it's alright, I was trying to point out, in particular to Smolin and Shor who brought the subject in, that unitarity can be violated without outrageous consequences. I mentioned in my answer to anon a few papers they might like reading. I think these provide a very satisfatory solution to the problem of time and black hole paradox, and perhaps even more, of the measurement problem itself. The following is a nice review:

http://arxiv.org/abs/gr-qc/0603090

thanks again.

67. **Aaron Bergman**
October 22, 2006

Lee —

[emergent geometry]

Some string theorists have finally begun to appreciate this point, and one can only hope they will follow up by studying the concrete results that have already been obtained.

I don't think the idea that spacetime would be emergent was remotely controversial. Even before AdS/CFT, there were various matrix models. I'd be surprised if there weren't papers speculating along these lines well back into the 80s. Holography is certainly a sort of emergent geometry, and I think you'll find that a fair fraction of string theorists believe that there is something fundamentally holographic about string theory.

But it is true that 6 of the ten dimensions transmuted from the space of scalar fields in the N=4 SYM theory to dimensions of space in the dual theory.

This is probably not a good way to think of AdS/CFT. It is true on the D-brane that the vevs of the scalars correspond to moving the brane around, in the AdS/CFT conjecture, ie, after you take the near-horizong limit, you have a five sphere and a five dimensional AdS. The fifth dimension of AdS corresponds to a RG scale in the field theory. The field theory can usefully be thought of as living on the boundary. Thus, the lack of background dependence in the YM field theory exactly corresponds to the fixing of the boundary of AdS. It's probably best to think of this (minor in my opinion) background dependence as necessitated by the need to do something there to make sense of anything in AdS.

I notice you mention CDT as a background independent approach, but last I checked, they fix a spatial topology (somewhat akin to fixing a boundary condition as in AdS/CFT). Has this restriction been removed and they sum over topologies now?

By the way, each time I note that string theory does not have the property of strong or manifest background independence, this is more than anything a criticism of myself as, to my knowledge, few others took the problem of making such a formulation of string theory seriously enough to spend years working on it, as I did.

How do you know? People have certainly considered it in the context of nonperturbative approaches. There's not terribly much to say in the perturbative approach after all. What most string theorists are interested in is finding a general nonperturbative approach. It is only in that context that the question can be usefully addressed.

I should mention that I just spent some time reading chapters 16-20 of your book in the bookstore (actually, I skimmed 17). I can't help but wonder, especially with all the blind quotes in chapter 16, who are these people? They certainly don't seem like the vast majority of the string theorists I know. Just to pick one example, the issue of whether or not string theory is correct or not is hardly verboten. Most of your list of seven facets of string theorists (which I confess to not remembering very well — perhaps someone with the book could summarize them again) didn't seem particularly representative of the people I know either.

When your book wasn't angering me (for reasons we've talked about in the past and which I won't get into here), it was immensely frustrating because you circle around some real problems but completely miss, in my opinion, the real issues. You base your solutions, it seems to me, on the idea that there exist these two types of people, the seers and the craftspeople. That seems to me to be fundamentally misguided. What we have are smart people doing their best to get by in the incentive structure that exists in theoretical physics. This structure rewards production. It rewards brilliant results also, of course, but there is a strong incentive to write lots of papers so as to get a job. Obtaining more seers is not a matter of identifying the iconoclasts (who in physics isn't a bit iconoclastic, after all?), but a matter of figuring out how to make it less dangerous for a young person to devote a significant amount of time thinking about extremely difficult problems that may not get solved.

People are doing their damnedest to get by in a market where there are scarce resources, and, contrasted with your proclaimed dissident seers, the lack of respect you evince (to my mind) towards the people who are willing to work and survive in this framework is dismaying.

68. **Lee Smolin**
October 23, 2006

Dear Aaron,

Just to address your last point. There is absolutely no disrespect involved. I just recognize that there are different types of scientific characters, who approach research differently and I emphasize that science needs several different types. I have a great deal of sympathy for the situation you describe, as I am much more like you in my skills than the people I am discussing here. I did fit into the incentive structure and did, as you put it, work and survive. This is because most of my work has been straightforward applications of QFT to gravity. LQG is after all, not radical, its just gauge field theory studied using techniques that are compatible with diffeomorphism invariance. It was also easier 30 years ago,.

Nonetheless, I recognize that there are people who either decline to or are incapable of fitting into the incentive structure you describe. They are , as you put it, unable to "work and survive in this framework."

Some of the people I am referring to do not believe the kind of work that the incentive structure rewards will be able to solve all the problems we face, because they believe the current conceptual framework requires revision. For example they may see the necessity of first altering the foundations of quantum mechanics or obtaining a conceptually and mathematically consistent understanding of what an observable is in GR. You and I are not such people, but they do exist and when they do succeed they make crucial contributions to science. I gave some examples in the book, such as Barbour, Deutsch and Valentini.

There is no incentive structure for such people to fit into, no matter how good they are and how important their work turns out to be, because they are working on problems either that were not clearly problems before they solved them or were not thought important. So in some cases they end up working outside the academy, in others they survive in small colleges or in math or philosophy departments.

A philosopher I know recently said to me that sometimes you have to recognize that an important problem cannot be solved by you, because it requires a person with a different kind of mind. Related to this is the advice that you are most likely to succeed at anything if you choose to work on problems that are compatible with how you think best.

All I am doing here is making the case that the progress of science would proceed faster if we made room for a small number of these individuals, who presently fall outside the incentive structure. This requires broadening the incentive structure a bit to recognize the fact that the progress of science requires a diversity of kinds of minds and skills. There is no disrespect meant or implied towards anyone.

Thanks,

Lee

69. **a**
October 23, 2006

unfortunately the word "war" that Peter does not like seems appropriate. So, why the first public scientific debate degenerated into a war?

First, Peter's criticism was perceived as a preventive war. Peter uses the word "failure" when some (little?) hopes remain open: maybe LHC will discover large extra dimensions, maybe some landscape statistics is peaked, maybe LHC will discover supersymmetry broken in some stringy way…

Second, the string hype. A naive extrapolation suggests that without the preventive criticism, some string theorists would have mispresented as a triumph of strings whatever LHC will see.

Third, somebody tried to close the traditional scientific channels to Peter's criticism. Ten years ago this could have been the end of the story, but nowadays this attempt transferred a private debate into a public internet war.

Fourth, Lubos Motl.

70. **Aaron Bergman**
October 23, 2006

Lee — I strongly disagree with that point of view. Just because someone is unwilling to work within the strictures that currently exist doesn't make them any better than those who are willing to do so. It just makes them more stubborn. Or flaky.

And I stand by my statement about the lack of respect you show. It comes across to me (and others, I would guess) in almost all your writing on the subject.

71. **ksh95**
October 23, 2006

This *"war"* is exceedingly healthy. Never before have such specialized and technical issues been debated in such a public manner. Force the naysayers to answer their critics, make the orthodoxy defend their ideas. And at the end of the day we will find truth, or we will witness theoretical high energy physics turn into philosophy…..You gotta love it!
/
Anyway, it strikes me as odd that the awe-inspiring, fabled and feared string theory propaganda machine is this ineffective. Woit and Smolin make substantive points and the machine calls Woit and Smolin poo-poo heads, babbles about conspiricy theory "back-stories", and accuses the people of seeking fame…..weird!
/
Even more queer is the fact that there exists an exceedingly convincing paper, in my opinion, explaining why string theory is the best(only) game in town. (the paper is recent and was mentioned on this and other blogs…I don't have the time or desire to find it).

72. **LDM**
October 23, 2006

Aaron,

You complain about a perceived lack of respect.

There are many physicists who simply do NOT respect what string theorists are doing in any way…who think it is wrong, and do not believe what you are doing is physics. Yet, string theorists still teach physics (and thereby influence a new generation of students), still have access to university physics resources, still consume tax dollars that would otherwise be spent on physics.

Weisskopf said it is a privilege to be a physicist, and I would contend that the way *publicly supported* string theorists have over-hyped and misrepresented any successes of string theory is just an abuse of that privilege.

73. **Aaron Bergman**
October 23, 2006

You miss the point. This isn't about respecting what people are doing; it's about respecting the people. I don't get angry when people attack string theory in an honest manner.

74. **Peter Woit**
October 23, 2006

Aaron,

I think this whole argument about who is dissing who is kind of ridiculous, more appropriate to teenage street gangs than serious scientists. In any case, given the attitudes and behavior of all too many string theorists, claiming that the problem with string theory's critics is their lack of respect for others is really a case of the pot calling the kettle black.

I think Lee has a point, but I'm not a huge fan of his "seers" vs. "craftspeople" distinction, since my own prejudice about the current problems of particle theory is that they are extremely hard, and making progress on them will require people who are some of both: iconoclastic enough to work on something different, with the technical skills and persistence necessary to get something beyond a vague, speculative idea. Particle theory needs new ideas and people ambitious and visionary enough to come up with them and pursue them (Lee's "seers" if you will), but the most likely way for someone to come up with these ideas is by immersion in the difficult technical issues surrounding quantum field theory and the standard model.

The great breakthroughs I've seen in math have come from people like Wiles and Perelman, who combined deep new insights, great technical skill, and seven or more years of dedicated single-minded work, during which they had nothing to show publicly for their efforts. How does one change the culture of particle theory to encourage this kind of effort?

75. **Aaron**
October 23, 2006

claiming that the problem with string theory's critics their lack of respect for others

I'm not claiming that. I never mentioned it in my review of your book, for example. This is a problem I have with Lee's writing on the subject.

As for the rest, I don't disagree with your last paragraph.

76. **DMS**
October 23, 2006

Aaron Bergman Says:
October 23rd, 2006 at 12:03 pm
You miss the point. This isn't about respecting what people are doing; it's about respecting the people.

While I appreciate the times you have clarified aspects of string theory on this blog(and I hope you get a faculty position soon…), I think you are way off the mark here. I have not read Lee's book, but nowhere on the blogs have I seen him be "disrespectful" to string theorists. On the other hand, there have been **numerous instances** on blogs where some(and no, not just LM) do not show the same courtesy to either Lee and **especially** Peter Woit.

And what I find bizarre is that neither of them are asking for string theory to be unfunded; they just ask for some other avenues of research to be funded. What is so wrong with this idea that gets some string theorists in apoplectic rage(when that could be directed to the "Rube Goldberg architects", for instance)???

Now I have copies of GSW, Polchinski, and Zweibach next to NEW on my bookshelf (I might even get the new Becker-Becker-Schwarz book IF it offers a new insight; getting bored with the same old discussion of the Polyakov action…). There is no contradiction. String theory has some astonishing successes and has offered some deep insights to mathematics(some of which I learn from this blog), even if it has failed miserably in its original goal.

On the other hand, one is always free to dismiss outsiders as “crackpots”…

77. **Aaron Bergman**
October 23, 2006

Courtesy and respect aren’t the same thing, and I should probably leave it at that.

78. **Lee Smolin**
October 23, 2006

Dear Aaron,

I don’t say that the people who are not working within the incentives you describe are better. I say only that the contributions of such people have from time to time been essential for the progress of science, and that this is one of those times. I do not see how an argument for greater diversity in academia is an argument against anyone. And I agree with Peter, there are a few people doing great work in ordinary QFT who are both visionary and unapreciated, and these are in the same category I called “seers.”

I do take your complaint seriously as I do think that respect among people who disagree is essential for science to work. I apologise if you feel disrespected as this was the opposite of my intention. It was rather to bring attention and appreciation to a kind of scientist who is often disregarded and disrespected. I am sorry if this had the unintended effect of making you feel disrespected.

While we are talking about disrespect, I hope you agree that to call serious scientists “crackpots” and to spread wrong information about their books without reading them, is outside of the bounds of acceptable professional behavior? No one is obligated to read a book or paper or comment on one, but if you do comment on a publication you have an obligation to have read it first. Do you agree?

Thanks,

Lee

79. **Aaron Bergman**
October 23, 2006

I do not see how an argument for greater diversity in academia is an argument against anyone.

As best I can tell, you think that there are different sorts of people and that we need to find these out of the mainstream people and anoint them with positions to do their out of the mainstream things. In contrast, I think there are lots of smart people, and we need to broaden the mainstream that they work in. In particular, it would be nice to find a way to make it safer to work on longer term projects with less a probability of success. I just don’t think it’s an easy thing to do.

As for the issue of respect, it has little to do with your attempts to “bring attention and appreciation” to other sorts of scientists. Without your book in hand, I can’t get into too much detail, but you could read this for a little example of what I’m thinking about.

And, finally, I think you’ll agree that a tit for tat on who is disrespecting who else the most is hardly productive. As for myself, I did sit down and read a fair chunk of your book.

80. **Chris Oakley**
October 23, 2006

Re: Lee’s article linked by Aaron above.

I was enjoying it until I read about the need for background independence. In particular, this:

> Meanwhile, many of those who continue to reject Einstein’s legacy and work with background-dependent theories are particle physicists who are carrying on the pragmatic, “shut-up-and calculate” legacy in which they were trained. If they hesitate to embrace the lesson of general relativity that space and time are dynamical, it may be because this is a shift that requires some amount of critical reflection in a more philosophical mode.

… which seems to ignore the fact that there are several orders of magnitude between the levels of experimental verification for SR and GR.

I have to say that criticism of String Theory will be seen as more legitimate if it is not combined with an advertising campaign for a particular alternative.

81. **egbert**
October 23, 2006

Aaron said, about Lee:

> As best I can tell, you think that there are different sorts of people and that we need to find these out of the mainstream people and anoint them with positions to do their out of the mainstream things. In contrast, I think there are lots of smart people, and we need to broaden the mainstream that they work in.

I think a part of the problem is that there is disagreement about what counts as mainstream. It was once the case that, for something to be mainstream science, it had to satisfy the usual conditions of making predictions and so on. Now string theorists have not yet made a prediction from string theory but hope to, and they insist, forcefully, that what they are doing is mainstream (without saying what the new criteria for mainstream science are), and are quite explicit about the fact that they do not regard any other approaches to quantum gravity as mainstream.

82. **Kris Krogh**
October 23, 2006

Lee wrote:

“Some of the people I am referring to … believe the current conceptual framework requires revision. For example they may see the necessity of first altering the foundations of quantum mechanics or obtaining a conceptually and mathematically consistent understanding of what an observable is in GR.”

It’s also possible the foundations of GR may need modification to agree with quantum mechanics. I agree with Chris Oakley that GR is not so well tested, and it hasn’t been shown the world is background independent. I predict the results from Gravity Probe B will shed some light next April.

83. **Aaron Bergman**
October 23, 2006

I clarified what I meant by ‘mainstream’ there in the sentence right after the ones you quoted.

Nobody working in quantum gravity has made any prediction, so perhaps it’s all out of the mainstream by your thinking.

84. **Kris Krogh**
October 23, 2006

    Aaron,

    There are two predictions here: http://arxiv.org/abs/astro-ph/9910325

85. **egbert**
October 23, 2006

    Aaron,

    Thanks for making references to my thinking. The sentence you say provides a clarification of what you mean by mainstream is:

    > In particular, it would be nice to find a way to make it safer to work on longer term projects with less a probability of success.

    Less than what? Longer term project than what?

86. **jeremy**
October 23, 2006

    On the way back from a recent trip to Cambridge, MA, sitting next to me on the plane was a middle age lady reading NEW. Curious, I made an effort to start a conversation. A friendly chat revealed that she was going home after visiting her child who is studying physics in one of the universities in Cambridge. Her child is planning to go to graduate school for theoretical physics and now is very worried what to do after reading the “black book” and the “blue book” (her own words, it took me a few seconds to realize what she meant, but not before she pulled out the “blue book” from a bag). As a mother, she wants to find out the details that made her child worry, although she knows little about physics. Is there a “war”? I don’t know. For a brief moment, I could almost smell gunpowder. As for the rest of my trip, it was very pleasant indeed.

87. **Kea**
October 23, 2006

    Jeremy

    You should have told the lady that it couldn’t possibly be a *better* time to go into Theoretical Physics, with so many great ideas in development.

88. **Chris Oakley**
October 24, 2006

    I have “The Elegant Universe” and “Not Even Wrong” next to each other on my bookshelf. I have surrounded the bookcase with detectors in the hope of seeing some interesting new quark states when they annihilate. Unfortunately, though, nothing yet has happened. I wonder if this is because I did not actually pay for Brian Greene’s epic, having got it as a present?

89. **Thomas Larsson**
October 24, 2006

    Is there a “war”? I don’t know.

    It is the third string revolution.

90. **Ari Heikkinen**
October 24, 2006

    Well, if anyone read that piece by Brian Greene, he’s pointing out the same things he’s wrote in his books and other pieces (and I apologize for the quotes in advance, but I feel I can’t make my point without posting them here, so I’ll make them short) like:

    “Nevertheless, mathematical rigor and elegance are not sufficient to demonstrate a theory’s relevance. To be judged a correct description of the universe, a theory must make predictions that are confirmed by experiment.”

    and

    “Nonetheless, should an inconsistency be found, or should future studies reveal an insuperable barrier to making contact with experimental data, or should new discoveries reveal a superior approach, I’d change my research focus, and I have little doubt that most string theorists would too.”

    Now, I’ve read again and again from some critics accusing that the advocates plain “lie” about the theory and some even claim they “falsify its results” to keep it going, but atleast of what I’ve read of the more prominent figures over the years they’ve basicly gone along Brian’s lines here.

    And I also tend to agree with this:

    “But to suggest dropping research on the most promising approach to unification because the work has failed to meet an arbitrary timetable for complete success is, well, silly.”

    I think both the advocates and the critics (atleast the sensible ones) are right on atleast the points that other research shouldn’t be abandoned in the name of string theory and also that it would be silly to abandon all that tens of years of promising research on string theory simply because the work hasn’t met some arbitrary timetable.

    I guess some progress is made when the fiercest critics and the foremost advocates can atleast respectfully disagree without calling eachothers crackpots. Now, if they could just go on with their research and start writing interesting papers again to prove their points would be even better.

91. **Tony Smith**
October 24, 2006

    After downloading and listening to George Johnson’s KITP talk about the books of Peter Woit (“Not Even Wrong”) and Lee Smolin (“The Trouble With Physics”) at http://online.itp.ucsb.edu/online/resident/johnson2/ the following excerpts (transcribed from my hearing, with George Johnson denoted by "Johnson" and various audience members denoted by "KITP" because I am not sure of the various audience voices) seemed to me to be interesting:

"…
Johnson – I don't think anyone would call Lee Smolin a crackpot.

KITP – … Would you like to take a vote on that? …

Johnson – Did you read the books?

KITP – Me? No. No.
…
KITP – I think that what bothers me is that people in the media are reading these books and thinking that what's in them is true. …

Johnson – I think that you should read at least one of the books.
…
KITP – You know, if you know Lee [Smolin], it's because he wants our money. It's because he doesn't have it that he wants to cut us down. …

KITP – It is clear that a lot of people are upset with ths book and feel damaged by it. …

KITP – these criticisms … the ones that are correct are the ones that are "of issue" that are being discussed within our community.
That [blog] is really not the right forum or the right set of antagonists to discuss … the problems we face in the current stage of string theory
so
I still think that the right attitude is not to engage in this kind of blog war … or public debate with marginal figures. …".

It is striking to me that:

1 – The KITP superstringers regard debate about superstring theory, NOT as a debate about physics, but as a fight over money;

2 – The KITP superstringers regard the funding they receive as "our money",
when in fact all their USA government support is taxpayer money, and therefore is as much my money as it is theirs;

3 – The KITP superstringers concede that some of the criticisms are "correct"
but
they say that even "correct … criticisms" should be discussed ONLY "within our community"
and
should NOT be discussed in blogs or public debate with "marginal figures";

4 – The KITP superstringers describe even well-credentialled people such as Peter Woit and Lee Smolin as "marginal figures"
(perhaps in the eyes of KITP superstringers everybody outside "our community" is "marginal").

I will leave it to readers (perhaps even some with Congress/NSF/DOE influence) to draw their own conclusions from the above statements coming directly from the mouths of the KITP superstringers.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

92. **TheGraduate**
October 24, 2006

Tony,

This is a horrible transcription and very, very inaccurate. I strongly suggest to anyone that reads this to be aware that these are isolated quotes from different speakers.

I do want to stress that the spirit of the conversation is accurate in my opinion but the combined effect of representing several speakers as one speaker and omitting large amounts of the conversation both within quotes and between quotes is to render the whole transcription a gross distortion of what actually occured.

Your inclusion of "…" just does not do justice to the huge swathes of conversation between some of the quotes.

93. **Tony Smith**
October 25, 2006

TheGraduate says that, in my comment 91, I was "… representing several speakers as one speaker …".

That is NOT true.
I explicitly said that my transcription was with
"… various audience members denoted by "KITP" because I am not sure of the various audience voices …".
Further,
each line beginning "KITP" contained excerpts from a single speaker.

It is true that I did not quote the entire hour-long talk, but I DID include a link from which the entire hour-long talk can be downloaded by anyone interested in hearing the exact context of each excerpt.

i am happy that it is the opinion of TheGraduate that, from my comment 91, "the spirit of the conversation is accurate".

As to whether or not the excerpts I quoted "render the whole transcription a gross distortion of what actually occured", I leave it to the readers to download the entire talk and decide for themselves,
bearing in mind that my stated purpose was NOT to transcribe the entire talk, but only to list some "excerpts" that "seemed to me to be interesting" and to comment on them.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – If TheGraduate, or anyone else, can identify each of the various KITP audience members that I denoted by KITP, please feel free to do so. I would be interested in knowing who they are.

94. **TheGraduate**
October 25, 2006

    My point is really just that bad quotes leave people with wiggle room later.

95. **Tony Smith**
October 29, 2006

    Lubos Motl, in his “take… on the KITP video” posted yesterday (28 Oct 2006), said:

    “… David Gross and other famous physicists …[hold an]… opinion [that] is a qualified extrapolation of the old good times in which the foes of science were irrelevant, an era in which the enemies of well-established physical theories … could be humiliated or ignored by the scientists, according to the scientists’ choice.
    But if you listen to David Gross more carefully, you can tell that he is not so certain that his assumption continues to hold.
    We arguably live in the first decade of the human history in which … online technologies including the blogosphere have made …[the failure of his assumption]… possible. …”.

    It is interesting to me that:

    1 – Lubos considers that it would be “good” if “enemies of well-established physical theories … could be humiliated or ignored by the scientists”; and

    2 – Lubos laments that “online technologies” make it difficult for “the scientists” to “humiliate… or ignore…”
    those who offer alternatives to “well-established physical theories”
    (and are therefore perceived by “the scientists” as “enemies”).

    Maybe that explains Lubos’s ad hominem attacks on physicists who don’t blindly follow the “well-established physical theor[y]” of superstrings (for example, calling the books by Peter Woit and Lee Smolin “the two slanderous books” and talking about “the intellectual dishonesty of the authors of the books collectively referred to as Swolin”, and calling me a “moronic crackpot”).

    Tony Smith
    http://www.valdostamuseum.org/hamsmith/

96. **woit**
October 29, 2006

    Tony,

    The funny thing here is that Lubos considers string theory to be a “well-established physical theory”. Thinking that one’s pet theory for which there is no scientific evidence is well-established physics sounds like crackpotism to me…

King of Infinite Space

October 19, 2006
Categories: Book Reviews

Back when I was a graduate student trying to figure out how to define and calculate topological charge in lattice gauge theory, at one point I went over to the math department to ask some people I knew if they had any idea about how to calculate the volumes of spherical tetrahedra. I was taken to the math department lounge to consult with the master of 3 dimensions, Bill Thurston. Thurston explained to me that this could be done by breaking the tetrahedra into "double-rectangular tetrahedra", whose volumes were then expressed in terms of the angles defining them using something called Schläfli functions, defined back in 1860. This experience helped cure me of my prejudice that modern mathematicians were probably ignorant of the older more concrete mathematics of the 19th century.

Thurston also pointed me to a more modern reference for this, a paper by H.S.M. Coxeter from 1935 entitled The Functions of Schläfli and Lobatschefsky. I ultimately found a much simpler way of computing topological charges, but I always wondered about this early 20th century mathematician, whose parents had given him a set of initials reminiscent of a British naval vessel. Later on in life, I learned a bit about some important algebraic constructions called Coxeter groups, and also heard that there was an active mathematician in Toronto named Donald Coxeter. I assumed that there were at least two and maybe three mathematicians named Coxeter out there, perhaps relatives.

It turns out that these are all the same Coxeter (the M. is for MacDonald), and there's a very nice new biography of him that has recently appeared, writtten by Siobhan Roberts and entitled King of Infinite Space. Coxeter only died quite recently, in 2003 at the age of 96, and Roberts was able to get to know him while writing the book. It contains a wealth of information about pieces of mathematical history I was not aware of, often buried in the very extensive footnotes.

Coxeter's main interest was in "classical" geometry, the geometry of figures in two and three dimensional space and he wrote a very popular and influential college-level textbook on the subject, Introduction to Geometry. Much of this subject can be thought of as group theory, thinking of these figures in terms of their discrete symmetry groups. This subject has always kind of left me cold, perhaps mainly because these groups play little role in the kind of physics I've been interested in, where what is important are continuous Lie groups, both finite and infinite-dimensional, not the kind of 0-dimensional discrete groups that Coxeter mostly investigated.

One theme of the book is to set Coxeter, as an exemplar of the intuitive, visual and geometric part of mathematics, up against Bourbaki, exemplifying the formal, abstract and algebraic. Bourbaki is blamed for the New Math, and I certainly remember being subjected by the French school system in the late sixties to an experimental math curriculum devoted to things like set theory and injective and surjective mappings. On the other hand, I also remember a couple years later in the U.S. having to sit through a year-long course devoted to extraordinarily boring facts about triangles, giving me a definite sympathy for the Bourbaki rallying cry of "A bas Euclide! Mort aux triangles!". To this day, both of these seem to me like thoroughly worthless things to be teaching young students.

Actually Bourbaki and Coxeter ended up having a lot in common. They both pretty much ignored modern differential geometry, that part of mathematics that has turned out to be the fundamental underpinning of modern particle physics and general relativity. Coxeter's most important work probably was the notion of a Coxeter group, which turns out to be a crucial algebraic construction, and ended up being a main topic in some of the later Bourbaki textbooks. A Coxeter group is a certain kind of group generated by reflections, and Weyl groups are important examples. Coxeter first defined and studied them back in the 1930s, part of which he spent in Princeton. Weyl was there at the same time giving lectures on Lie groups, and used Coxeter's work in his analysis of root systems and Weyl groups.

Coxeter groups and associated Coxeter graphs pop up unexpectedly in all sorts of mathematical problems, and Roberts quotes many mathematicians (including Ravi Vakil, Michael Atiyah and Edward Witten) on the topic of their significance. There are quite a few places where one can learn more about this. These include various expository pieces by John Baez (see for example here, based to some extent on this), as well as a web-site set up by Bill Casselman. The AMS Notices had an interesting series of articles about Coxeter and his work, written shortly after his death. The proceedings of a recent conference at the Fields Institute in Toronto entitled The Coxeter Legacy – Reflections and Projections have recently been published. In a couple weeks there will be a special program in Princeton about Coxeter, aimed at the general public.

One reason I'd started reading the book about Coxeter was to get away from thinking about string theory, but this was definitely not a success, since the book contains a rather extensive discussion of string theory. Coxeter was aware of string theory, it seems it reminded him of Jabberwocky, and he's quoted as follows:

It's like reading about a part of mathematics that you know is beautiful, but that you don't quite understand. Like string theory. That's as much a mystery to me as it is to anyone else who can't make head nor tails of the eleventh or sixteenth dimension.

Roberts quotes Witten (who she says is known as the "pope of strings") about the possible relevance of Coxeter groups and E(10) to string theory. She describes string theory in somewhat skeptical terms:

But rumblings are that if a bigger breakthrough doesn't occur soon, and in the form of streams of empirical evidence, string theory will at best be a branch of mathematics or philosophy, but not part of physics.

She quotes Amanda Peet as proposing that string theory become "a faith-based initiative", and Susskind as "There's nothing to do except hope the Bush administration will keep paying us."

Update: Siobhan Roberts has set up a web-site for the book, and she tells me that she'll soon be starting up a blog there.

Update: There's a very good expository paper by Igor Dolgachev that discusses Coxeter groups, and generally the way reflection groups appear in algebraic geometry.

Comments

1. **Thomas Love**
   October 19, 2006

   It is hard to get away from the topic of string theory, it pops up in the most unexpected places, like the comic page of the Times. But then, perhaps the comic page is where string theory belongs.

   Thanks for the review, I'll have to look up the book.

2. **Xerxes**
   October 19, 2006

   Your comment about spherical tetrahedra reminded me of a project I worked on where I needed to find the volumes of convex 3d polyhedra and 4d polytopes. I know how to work out simplex volumes and arbitrary convex 2d polygons, but I searched the literature for a long time on the higher-dimensional problem and never came up with anything. Eventually I resigned myself to estimating them using Monte Carlo techniques. Maybe somebody knows a better solution?

3. **D R Lunsford**

October 19, 2006

I remember reading "Regular Polytopes" as a kid and discovering empirically a fact about the Platonic solids. I was eating a lot of Dannon Yogurt at the time – the container was environment-friendly wax paper which however was rather weak. To strengthen the top, a circular cardboard disk was inserted. I pried out a bunch of these identical disks and used them to make the five solids by inscribing regular polygons in them etc. In the end, each face of each solid thus could be inscribed in the same circle. When I set them on my desk, I noticed that they paired up in altitudes, the cube and octahedron having the same altitude, likewise the icosahedron and the dodecahedron, while the tetrahedron was paired with itself, being self-dual! I went on to prove this little factoid by brute force. I often wondered if a simple proof could be had. I always thought Coxeter would have been delighted by that.

-drl

4. **Ben Recht**
October 19, 2006

Xerxes,

Surprisingly, Monte Carlo techniques are your best bet for high dimensions. Computing the volume of polytopes is #P-Hard.

http://en.wikipedia.org/wiki/Sharp-P

On the other hand, there are Monte-Carlo techniques that yield approximations to the volume in time polynomial in the dimension. See

Simonovits, M. "How to compute the volume in high dimension?" Math. Program., Ser. B 97: 337–374 (2003)

for a survey and lots of references.

Cheers,
Ben

5. **D. Eppstein**
October 19, 2006

One point I found odd in the book's discussion of the Coxeter-visual-geometry vs Bourbaki-formalist-algebra dichotomy was that it seemed to be placing Hilbert on the formalist side. The author of Geometry and the Imagination, an enemy of visual thinking in geometry?

6. **D R Lunsford**
October 19, 2006

Eppstein – yes, absolutely. Courant in fact to some degree rebelled against his teacher Hilbert. Hilbert once said "physics is too hard for physicists" – this was the beginning of a bad trend in math/physics, that Courant lamented in later life. Hilbert believed in axiomatics – Courant, like Weyl, had a more metaphysical approach and did not like drawing fine lines between the subjects.

-drl

7. **Alexandre Borovik**
October 19, 2006

Yes, I agree that Bourbaki and Coxeter ended up having a lot in common. The treatment of Coxeter groups by Bourbaki (N. Bourbaki, Groupes et Algebras de Lie, Chap. 4, 5, et 6. Hermann, Paris, 1968.) is perhaps the best written chapter in his treatise (it even contains an illustration!). It was written by Pierre Cartier; a very revealing account of his work on the text is given in his interview:

M. Senechal, The continuing silence of Bourbaki—An interview with Pierre Cartier, June 18, 1997. The Mathematical Intelligencer 1 (1998) 22–28.

8. **Steven H. Cullinane**
October 19, 2006

Another Weyl-Coxeter connection is furnished by the *quincunx lattice*. See "For Sir Thomas Browne" and "Geometry's Tombstones."

9. **Steve**
October 20, 2006

What's E(10)? I thought there was only E(6), E(7) and E(8).

10. **Tony Smith**
October 20, 2006

A web page at http://planetmath.org/encyclopedia/NicolasBourbaki.html says in part:

"… Once Bourbaki had finally finished its first six books, the obvious question was "what next?". … Pierre Cartier was working with Bourbaki … Its second series … consisted of two very successful books :
Book VII Commutative algebra
Book VIII Lie Groups
… Bourbaki was now becoming involved in a battle with its publishing company over royalties and translation rights. The matter was settled in 1980 after a "long and unpleasant" legal process, where, as one Bourbaki member put it "both parties lost and the lawyer got rich" …
In 1983 Bourbaki published its last volume : IX Spectral Theory. …".

I am sad that Bourbaki did not continue. As Alexadre Borovik said, Book VIII Lie Groups contained some of the best material by Bourbaki, and it would have been nice to have seen further development of such material described by Bourbaki.
Maybe such further description might have shown clearly how, as Peter said, "… Bourbaki and Coxeter …[had]… a lot in common …".

It is interesting to me that the law of "intellectual property" had a role in the end of Bourbaki.
It reminds me of a legal conference (in the 90s) in which an audience member said to a panel something like "the internet is a medium for free exchange of ideas", whereupon a panel member (corporate lawyer) said "Corporate interests are taking over the net and anarchists like you will be put down".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

11. **Aaron Bergman**
October 20, 2006

What's E(10)? I thought there was only E(6), E(7) and E(8).

Add two dots to your Dynkin diagram and hope for the best. You get an infinite dimensional Kac-Moody algebra which seems to be related to the compactification of M-theory down to one dimension on a T^10. This looks like an introduction.

12. **Thomas Larsson**
October 20, 2006

E(10) is not just infinite-dimensional, but **very** infinite-dimensional (of exponential growth). This makes it quite untractable mathematically, unlike the affine Kac-Moody algebras which only are of polynomial growth. To quote Victor Kac:

"It is a well kept secret that the theory of Kac-Moody algebras has been a disaster."

13. **Curious minds want to know**
October 20, 2006

That Amanda Peet?? http://www.adoring.net/amandapeet/

14. **Steve Myers**
October 20, 2006

A note on volumes in many dimensions: Hamming has an amusing proof (in "Numerical Analysis for Scientists & Engineers") that as dimensions increase, volume goes to the surface.

15. **h**
October 20, 2006

Xerxes:

You can subdivide any convex polytope to simplicies. There are trivial and not-so-trivial algorithms to do that, which are easy to find in the literature. 3 or 4 dimensions is low enough, so this should be fast, and also gives exact results (for rational polytopes).

16. **Fabien Besnard**
October 20, 2006

About the new maths and triangles : I've also been exposed to both, since i was about 10 at the end of the new math era, and had a teacher in high school that was in love with triangles (I suspect he had an affair with the Euler line). I did not find this boring at all, and both were very useful in learning rigour and the joy of discovery. As a child I remember a day when our teacher showed us how to count in the octal and binary systems. It was really exciting. In high school there was nothing more thrilling to me than working on a geometric problem for the all week end. Later, the French education changed and they got rid of both the new maths and almost all of elementary geometry. I had to teach in a high school for four years : now everything is more practical and "useful", less abstract… and so damn boring !

17. **D R Lunsford**
October 20, 2006

Curious, no this one.

-drl

18. **Andy**
October 20, 2006

(OT: There's an Op-Ed on ST by Brian Greene in today's NYT.)

19. **Tony Smith**
October 20, 2006

Peter says "… Siobhan Roberts … quotes Amanda Peet as proposing that string theory become "a faith-based initiative" …".

However, on her web page cited by D R Lunsford, Amanda Peet herself says:
"… I study … the fundamental dynamics of quantum gravity … using string theory …
Thus far, significant progress has been made … [i]n the search for string theoretic mechanisms for resolution of spacetime singularities. …
I will also be very interested to see how new ideas about the Landscape may inform the old question of vacuum selection in string theory …".

Peet's web page says that it was "Last updated: 2006/09/22", so it seems to me that it is probably a current statement of her views, and not an out-dated web page containing stuff about which she has changed her mind.

Since I think that Peet herself is more likely to give an accurate description of her own opinions than a third party, it seems to me likely that
Roberts has substantially distorted Peet's view of string theory,
and
that makes me very skeptical about the accuracy of other statements made by Roberts.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Maybe my opinions above are affected by my opinion of some journalists (the Amazon page says "Siobhan Roberts is a journalist"), which is exemplified by an interview of Joe Namath shortly after he became a New York Jet. A reporter asked him if, at the University of Alabama, he had majored in basket-weaving. Joe replied: "No, basket-weaving was too hard, so I majored in journalism."

20. **Peter Woit**
October 20, 2006

Tony,

The quote from Amanda Peet was clearly a joke by her.

Andy,

Maybe I'll add a couple comments about Brian's op-ed to the previous posting.

21. **LDM**
October 20, 2006

(OT: There's an Op-Ed on ST by Brian Greene in today's NYT.)

The interesting thing about this letter, is, that in another 20+ years, Briane will be able resubmit it to the NYT almost verbatim.

22. **Siobhan Roberts**
October 20, 2006

Tony,

Yes indeed, Amanda Peet has a good sense of humour. She made the comment at the Strings '05 conference in Toronto at a public session on "The Next Strings Revolution," which was dubbed as an "airing of our dirty laundry" session— downloadable proceedings are available at the Fields Institute website, http://www.fields.utoronto.ca/programs/scientific/04-05/string-theory/strings2005/, though if memory serves I don't believe Peet's comment, which was made during the Q&A afterward, was included on that recording. Her comment, and the jocularity of the event in general, was nicely covered by Dennis Overbye in the NYT Science Times, "Lacking Hard Data, Theorists Turn to Democracy," August 2, 2005 — http://select.nytimes.com/search/restricted/article?res=FA0E10FA355B0C718CDDA10894DD404482

As far being a journalist goes, accurate observation, documentation, and portrayal of events — as with science — is of utmost importance!

Best,
Siobhan

23. **DT**
October 20, 2006

How, in a group this nerdy (in a good way) could you mention Lobatschefsky in the post and have nobody comment on the eponymous Tom Lehrer song? Here's a link for the lyrics: http://members.aol.com/quentncree/lehrer/lobachev.htm

If you don't know the song, you should. NB: same guy, different spelling.

24. **Siobhan Roberts**
October 20, 2006

There is also a Lehrer song about the Bourbaki-inspired New Math!

http://members.aol.com/quentncree/lehrer/newmath.htm

25. **Tony Smith**
October 20, 2006

Siobhan and Peter,
my apologies for being too dense to realize that Peet's "faith-based initiative" remark was merely a joke intended to reflect her true views.
In light of that, I will consider Siobhan to be one of the (all-too-few) good-guy journalists,
and
not in the class of "some journalists" such as the Joe Namath interviewer (and all-too-many of the partisan talking-heads on TV "news").

As a concrete manifestation of my changed opinion, I have ordered a copy of "King of Infinite Space", and Amazon says I should get it on Monday.

A question:
Could a student today get a PhD and a job (and grants) by following in Coxeter's footsteps ?

If so, would the student's success be more likely:
1 – by working in string theory physics ?
or
2 – by working in a pure math department ?
or
3 – by some other path (if so what)?

If not,
does that mean that it is now unlikely that we will see any more ideas that are as out-of-mainstream as Coxeter's have sometimes been considered ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

26. **Tony Smith**
October 20, 2006

typo correction:
In my previous post
"… merely a joke intended to reflect her true views …"
should have been

"… merely a joke NOT intended to reflect her true views …".

My apologies again.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

27. **Siobhan Roberts**
October 20, 2006

Tony,

That's an interesting question. A number of people remarked when I was researching the book (and I may or may not have included it, can't recall) that Coxeter's students, all very talented mathematicians (Willy Moser, Asia Ivic Weiss, Barry Monson, Chris Fisher, to name a few), have not approached anything near Coxeter's stature. And Coxeter's position at the UofT was not filled with a "pure geometer" when he retired — and you'd think if anywhere, then there. Coxeter was one of a kind in many ways. But I'd be inclined to say 1) or 3), and I'm not sure what form the the latter category would take — perhaps a computer animation scientist at Pixar, or a mathematician in residence at Microsoft or At&T (and in that, their Coxeterian interests might take the shape of a nicely subsidized sideline for the most part, occasionally breaking through with an application). I talk about this in part two of the book, discussing where Coxeter's style of classical geometry pops up in "modern" and applied contexts. Hope you enjoy it!

Siobhan

28. **Siobhan Roberts**
October 20, 2006

Addendum:

Re 2) can Coxeterian geometers finding positions in pure math depts: John Conway at Princeton is in many ways considered Coxeter's spiritual successor as a geometer, in terms of the breadth and depth of his interests and work, and then there's the aforementioned Bill Thurston at Cornell. Many "Coxeterian" geometers find positions in math depts, but the nature of Coxeter's classical geometery, so to speak, has transcended its origins in terms of current research paths.

Siobhan

29. **yasir**
November 3, 2006

Its not the mutha-string – its the cosmic humor you seem to be striking upon repeatedly that is at the bottom of everything, and keeps everything going !

bravo.

o.0

^_^

The Hype Goes On

October 23, 2006
Categories: Uncategorized

The Ottawa Citizen today has an Op-Ed by string cosmologist Jim Cline, headlined The Big Idea That Won't Die, with a subtitle "The fact that string theory is suddenly under attack only underscores its success as a path to a unified description of nature." There's a lot that it outrageous about this piece, beginning with the subtitle. Normally scientists don't start going on about the success of their theories until they have some experimental evidence for them.

Most outrageous are Cline's claims that Smolin's book and mine are written in a "defamatory style", and are "slandering" string theory. Since he gives no evidence for either of these claims, there's not much to say about them except that they're defamatory and slanderous.

Cline makes the standard claim that string theory should be accepted since it has legitimately triumphed in the marketplace of ideas, while clearly being rather upset about the success that critics of string theory have recently been having in this same marketplace. Somehow, overhyping string theory is a legitimate marketplace activity, pointing out its problems is not.

He makes many of the by now standard bogus claims about supposed predictions and tests of string theory. At some point I suppose I should write a FAQ about these, since the string theory hype machine keeps promoting these things in a less than honest way to a public that is not well-equipped to see through the hype. Here's a pretty complete list of the bogus "predictions"

String theory predicts supersymmetry and extra dimensions. The LHC will test these predictions.

The problem is that there is no prediction of either the scale of supersymmetry breaking or the size of the extra dimensions; in string theory these could be anything. All we know is that the energy scales involved are at least a TeV or so, since otherwise we'd have seen these phenomena already. There's no reason at all to expect the extra dimension scale to be observable at the LHC, even most string theorists think this is highly unlikely. There is a standard argument that the hierarchy problem could be explained by a low supersymmetry breaking scale, but this is already starting to be in conflict with the lack of any observations of effects of supersymmetry in precision electroweak measurements, and now string theorists seem very willing to say that supersymmetry may be broken at an unobservably high scale.

String theory predicts observable effects in the CMB or gravitational waves.

If you look into this, this is based on very specific cosmological scenarios such as brane inflation, and again string theory doesn't tell you even what the energy scale of the supposed predictions is. Undoubtedly you can get "predictions" from specific models, once one chooses various parameters, but not observing these "predicted" effects would not show that string theory is wrong but just that a specific scenario is wrong, with many other possible ones still viable. There's a new review article by Henry Tye where he claims that "string theory is confronting data and making predictions", which isn't true. It is only certain specific scenarios that he has in mind, he admits that other, equally plausible, scenarios (such as using not branes but moduli fields as the inflaton) make no predictions at all. For more about this, one can watch recent talks by Tye and Polchinski at the KITP.

The anthropic landscape predicts the value of the cosmological constant and will make other predictions.

The latest contribution to the anthropic landscape hype is from Raphael Bousso and is entitled Precision Cosmology and the Landscape. I've written many times about the problems with the cosmological constant "prediction". Bousso claims that "there is every reason to hope that a set of 10^500 vacua will yield to statistical reasoning, allowing us to extract predictions". He doesn't give any justification at all for this, neglecting to mention arguments about the inherent computational intractability of this question, and the failure of the program to try and predict the answer to the one question that seemed most likely to be approachable: is the supersymmetry breaking scale low or high?

String theory makes predictions testable at RHIC.

There are lots of problems with this, but the main one is that the "string theory" involved is a different one than the one that is supposed to unify particle physics and quantum gravity.

Update: For more promotional material about string theory, you can buy a set of lectures by Jim Gates entitled Superstring Theory: The DNA of Reality. I haven't seen the videos, but Gates is probably not indulging in the kind of claims about "predictions" of string theory being made by many others.

Update: A couple people have pointed out that a new paper has appeared pointing out that the one "prediction" of the landscape claimed by Susskind, that of the sign of the spatial curvature, isn't sustainable. This issue was discussed here with Steve Hsu, who was blogging from a conference where Susskind made that claim, and wrote about it in more detail here. Hsu is one of the co-authors of the new paper.

Update: There's a rather critical review of Lee Smolin's book in this week's Science magazine by Aaron Pierce entitled Teach the Controversy! Somehow I suspect Pierce did not write the headline, since he doesn't seem to think much of opposition to string theory or that it is a good idea to encourage any dissent about it. In his review, he pretty much completely ignores the fact that string theory is supposed to be a unified theory, explaining the standard model as well as quantum gravity, discussing just the question of string theory as a theory of quantum gravity. This is rather odd since quantum gravity isn't even Pierce's specialty. I'm somewhat curious what he might think of my book, which is pretty much all about string theory's failure as an idea about particle theory.

Comments

1. **anonymous**
   October 23, 2006

   Cline correctly writes that (tenured) theorists can choose to work on the ideas that look more appealing. Maybe he could also explain why some theorists work on brane collisions: the idea of putting two perfectly parallel branes looks to me so childish (sorry, I cannot find a more polite word) that I never got interested in addressing its problems in getting inflation and problems in getting scale-invariant perturbations.

2. **Arun**
   October 23, 2006

   A FAQ is an excellent idea.

3. **hack**
   October 23, 2006

   Uh oh. Looks like another string theory "prediction" bites the dust.

http://arxiv.org/abs/hep-th/0610231

4. **M**
October 24, 2006

hack: interesting work, but you are too drastic; it just means that strings can now make statistical predictions for the sign of the curvature. (Just to be sure: this is a joke).

There is another interesting paper today: hep-th/0610231.

5. **AdamBalm**
October 24, 2006

Yeah, I'd second the call for an FAQ. It's a bit frustrating trying to bring this issue up with some people unfamiliar with the subject, and when they ask for more information or clarification, the best I can do is point them to this blog or tell them to read the book. So yeah, having it all in one place would certainly make it easier to introduce people to the current points of contention than expecting them to wade through hundreds of articles over the past year. (Not to mention it might force some other people to try and think up some fresh arguments for a change.)

6. **Chris Oakley**
October 24, 2006

> I suppose I should write a FAQ about these, since the string theory hype machine keeps promoting these things in a less than honest way to a public that is not well-equipped to see through the hype.

Great idea.

7. **r hofmann**
October 24, 2006

M,

that is, indeed, witty.

Peter,

FAQ is a very good idea. Maybe it is helpful to invoke simple parables to get the points across.

8. **LDM**
October 24, 2006

I agree with the FAQ idea.

I only wonder why Peter should have to waste his time doing this, when the string theorists themselves should be exercising the required level of scientific scrutiny and mentioning both the pro and con in their ideas.

If the FAQ could get permission to reprint Feynman's "Cargo Cult Science" essay– the antithesis to hype in my opinion — the layperson could have a first-rate understanding of what it means to be honest, as a scientist, in ones scientific claims.

9. **dark-matter**
October 24, 2006

Jim Cline writeup is a reasonable defense of ST. The good part is he didn't propose to redefine science in order to accommodate ST. He suggested a number of predictions to be tested by experiments. But he should know suggestions is very different from writing them up in a scientific paper in collaboration with the experimentists to specify meaningful prediction parameters. No such papers have appeared with regard to any experiments confirming or falsifying any claim of ST. The way ST have developed, no such papers will likely ever appear. Is Jim prepared to do another writeup proclaiming ST to have entered the realm of metaphysical religion?

10. **relativist**
October 24, 2006

Lubos Motl is back in the amazon.com reviews, thinly disguised as Non-String Physics Grad Student, complete with a series of fake endorsements of his review of Lee's book. He certainly has time on his hands. Interesting he classifies himself as a graduate student.

11. **woit**
October 24, 2006

relativist,

It doesn't sound to me like Lubos, who is not the only person with a very hostile attitude toward Smolin's book (and mine). The review reads to me exactly like what it purports to me, the work of a student who knows little or nothing about string theory.

12. **LDM**
October 24, 2006

I may be mistaken, but I don't recall Lubos showing knowledge of decoherence in QM…but perhaps it was off topic so just never came up.
It is quite true, as the grad student alludes to, that there is a body of research and experiment indicating that QM is now understood (contrary to the famous Bohr dictum to the contrary), using decoherence, and Smolin did not mention this in TTWP.

I concur that the grad student review seems legitimate.

13. **steve**
October 24, 2006

One point in this proposed FAQ that could be helpful would be to clarify the relationship between theories and models. I sometimes have trouble tracking the claims here– is the theory non-testable or incoherent and hence not even wrong, or is it falsified? It seems that some of the confusion here comes from mixing together claims about theories versus specific models within those theories.

Is it common in theoretical physics to insist on the testability of a theory, or is it OK, if less desirable, to be able to test specific models (or classes of models) within that theory? I bring this up because a theoretical framework or approach may be a fecund source or inspiration ("the context of discovery," as the philosophers say) for specific testable models, without itself being testable in toto. So is the claim: a) Individual models made of stringy components may be testable (and may have been falsified), but because there are zillions of such models you can't experimentally rule all of them out, b) No individual stringy model has ever been produced which is testable, or c) No testable individual stringy model CAN ever be produced? Each of these claims is distinct and has distinct implications.

In particular, if a) is the claim, then it might be reasonable for a theorist to say "String-type theory is the best idea I have for how to construct testable models that might explain quantum gravity (or whatever). I can't prove that some other starting point might not be a better place to start looking, but this is where I want to spend my time." If enough theorists agree with this point of view, it will look like string hegemony, but in this hypothetical case it would just be convergent individual judgments about research prospects.

On the other hand, if c) were the claim, sticking with strings would suggest serious problems with the field. And of course an unwillingness to consder alternatives seriously would also hinder progress.

I think of science as working like the stock market–in the short run it is a voting machine, but in the long run it is a weighing machine. My sense is that the critics of string theory believe that the voting period is extending well beyond the point where weighing should have determined the outcome. The string theorists don't agree, and the discussion is really about when to give up and why. I would find this discussion clearer if the theory/model distinction were more apparent.

14. **Lee Smolin**
October 25, 2006

To LDM: Decoherence is of course a real phenomena, and is mentioned briefly on p 252 of TTWP. While there are claims in the literature that it by itself solves the measurement problem this has, in the view of many experts, been shown wrong, indeed quite a long time ago, for example in papers of Abner Shimony. Decoherence does play a role in some formulations of quantum theory that address the measurement problem, for example the consistent histories approach. But I believe that I correctly characterized the situation in Chapter 1, which is that there is no consensus among experts as to whether any of the proposed interpretations or reformulations of quantum mechanics so far proposed are completely satisfactory. You can find arguments in the literature pro and con many different viewpoints and approaches. Had I represented one of them as a consensus view I would not have been accurately representing the field.

Thanks,

Lee

15. **Aaron Bergman**
October 25, 2006

For whatever it's worth, I agree with Lee on this point. Decoherence makes it a pain in the neck to figure out what on earth is going on with the quantum to classical transition, but it doesn't actually solve anything.

16. **a)**
October 25, 2006

Steve,

a) is true, and it is the big problem: it means that whatever we observe, among the 10^500 string models, 10^300 or so should be able of "explaining" it.

Presumably string theorists will try for a few years to study if the situation is so bad. Unless some good surprise will be found, anybody who wants to spend his time on this approach should put his 10^300 models in a big bag and move to a philosophy department.

17. **LDM**
October 25, 2006

Lee,

Yes, decoherence is even in the index in TTWP — I should have checked instead of trusting my memory. Sorry.
Also, thank you for the Abner Shimony reference. The book I am reading — "Decoherence and the Appearance of a Classical World in Quantum Theory" has 51 pages of references at the back , but I am unable to find Shimony mentioned at all. However, if you have a reference to the particular paper of Shimony that you are referring to, would you mind posting it? Thanks.

18. **steve**
October 25, 2006

a): If in fact a) is correct, then it's hard to argue that string theory is some sort of plague. It's just an urn into which some people are reaching hoping to draw out a black ball (testable and correct model) instead of a red ball (untestable or falsified model). Other people get to draw from other urns. My concern would be if the community somehow had different standards for what is black or red depending on which urn the ball came out of.

19. **Milan**
October 25, 2006

The absurd argument that the fact that idea X is criticized underscores its success is a bizarre form of scientific stalinism. In its original form, J. V. Stalin emphasized that the resistance of the capitalist class will increase as the march toward communist utopia goes on closer and closer to its perceived goal – and it was the major pretext for all purges in 1930s and 40s, with the tragic loss of millions of lives. Now, we here the same moronic nonsense invoked in defense of string theory!? I suppose the author of that nonsensical piece of propaganda also "predicted" that the criticism of stringy nonsense will also mount and increase in ferocity as the time goes on, offering even more "confirmation" to its successes, and so on, ad infinituum and ad nauseam.

20. **Derek Teaney**
October 25, 2006

Dear Peter,

I have been reading your blog for a bit and like it a lot. Regarding RHIC (which is my field) it is certainly true that the connection between the AdS/CFT correspondence and the actual experiment is very vague and wildly exagerated. However, the qualitative features of the kinetics of strongly coupled plasmas predicted by the correspondence are so wildly different from their perturbative counterparts, that if the QCD plasma were to behave anything like the AdS-Super Plasma there surely must be observable consequences at RHIC or the LHC. Hopefully someday we'll find a nice crisp observable which can probe these qualitative differences.

21. **Kea**
October 26, 2006

Interesting point, Derek. Do you have any handy references for AdS/CFT plasmas? I am actually working on this myself now.

22. **Peter Shor**
October 26, 2006

I would agree that the graduate student review is legitimate. I've had discussions with Lubos Motl on quantum mechanics which make me think that his views on decoherence and his objections to the book would be quite different than this reviewer. In fact, I'm quite sympathetic to the graduate student on this. While I don't think the quantum-to-classical transition is a solved problem yet, I expect that it will be solved through a more complete analysis of decoherence, unlike Smolin's other four problems, which are likely to need new physics for their solution.

On the other hand, since Penrose and 't Hooft side with Smolin on this one, I am nowhere near presumptuous enough to claim that any of them show a deep misunderstanding of physics.

23. **Bert Schroer**
October 26, 2006

Peter Shor
I don't think that decoherence can explain the process of factualization of potentiality, I think this remains outside present QM. What it can do is find an excuse which is sufficient for all practical purposes.

24. **Mike**
October 27, 2006

Hi Peter, just wondering if you'd seen this from a week ago.

http://online.itp.ucsb.edu/online/resident/johnson2/

25. **Peter Woit**
October 27, 2006

Mike,

There's a long discussion of it two posts back, the one on "The String Wars"

26. **Bert Schroer**
October 28, 2006

Of all the criticism of ST I find that of Phil Anderson the most penetrating.
Of course I 'have tremendous admiration for Peter's courage and I did not trust my eyes when several years ago I saw his critique on the lanl server. Lee's criticism had this important sociological component from the very beginning, this risk in his case was more to become an enemy of his former colleagues without desiring this role (as the director of an institute he did not suffer a direct career threat).
Not that there were no other critical minds; among my QFT colleagues I do not know any one who did not think of ST as freakish. But they have interesting projects and do not want to be sucked into controversies (especially since most of them have ST colleagues which, as Glashow at Harvard, they could not prevent the course of metaphoric physics). I myself belong to that class of cowards who only after retirement open their mouth.
But in the present situation in which we find our-self the criticism of Phil Anderson is the most fitting one. The hubris and prepotence state of mind as a result of the undeserved luck which led to discovery of the standard model is certainly the powerful combustion force which has brought us into the present mess. It would be very difficult for anybody to say this because it involves some of his Nobel colleagues and a field medal winner, but he can do it. It is a deep shame (and certainly be an issue for historians of science) that the particle physics community was unable to keep its house in order.
Of course neither Phil Anderson nor all those other critical minds have any illusions, there is nothing which can stop that collective madness, it just has to play out. This is not different from similar phenomena in the political realm, in fact some proponents of ST are at the same time apologist (or at least were) of the destruction of Iraq in the name of regime change and a new order of the near east (despite the many voices which predicted the present mess). Given the human nature, in both cases it has to play out to the bitter end. But it testifies to the power of human self-reflection to have articles as that by Phil Anderson.

27. **LDM**
October 28, 2006

The 'Non-String Physics Grad Student' review is gone from Amazon. It would be interesting to know if he has re-thought his views and so deleted the review himself, or whether Amazon removed it.

28. **egbert**
October 29, 2006

Re: factualization of potentiality. Of the people who believe that quantum amplitudes and wavefunctions represent information about the physical world (rather than representing physical states of affairs directly), nobody thinks there is a measurement problem, or anything unusual about collapse. As Heisenberg said, the world doesn't change discontinuously, but our information about it does.

On the other hand, of those who believe the opposite, namely that the quantum amplitudes and wavefunctions represent the actual state of affairs, everybody agrees that there is a problem which needs to be solved. Decoherence was presented as a candidate solution but most agree that it doesn't really help.

Over the course of the twentieth century, the number of physicists in the former camp has dwindled and the number in the latter camp has grown to include almost everyone. I asked Penrose, after a talk, about why he thought this had happened, and if he knew of any proof or argument implying that the view of the former camp was wrong (since he's firmly in the second camp along with almost everyone else). He said that he didn't know of any such proof but that the wavefunction "seems objective".

29. **Bert Schroer**
October 29, 2006

(This is probably not the right place, but perhaps Peter permits me to come to a closure with my previous somewhat sketchy remark)
The decoherence solves the problem for all practical purposes, but being within QM it cannot explain the closure of this process namely the irreversible factualization which happens at the moment of registration (which may be done by a machine).

30. **Christine Dantas**

October 29, 2006

Bert Schroer wrote:

(…) the irreversible factualization which happens at the moment of registration (which may be done by a machine).

Could you please elaborate? If Peter Woit thinks this is not the right place to continue the discussion, allow me to announce that I will be accepting guest posts to commemorate the [1 Year Anniversary of Christine's Background Independence] (refer to my last post over at my blog for instructions).

Best wishes,
Christine

31. **Bert Schroer**
October 29, 2006

Christine,
I think Peter will not mind a very short answer, since it comes with a citation of a lovely personal account of a giant of QFT, the creator of AQFT Rudolf Haag (which, I am sure, also Peter will read with great interest)
http://br.arxiv.org/abs/hep-th/0001006
there is also a whole section on factualization i.e. the conversion of potentialities (e.g. the process of decoherence) into events. Since irreversibility is outside QT, there is as yet no theory about it.
voce vai gostar
cordialmente
Bert
cor

32. **Christine Dantas**
October 29, 2006

Dear Bert Schroer,

Thanks! Muito obrigada!

Christine

33. **Bert Schroer**
October 29, 2006

After that short excursion on factualization and the problem of reality from the perspective of QT, let me return to the main topic of this section: hype.
All my colleagues from QFT who know that framework of particle physics (say beyond e.g. the Peskin-Schroeder level) agree with me that holography from d+1 to d dimension is nothing else than changing the spatial encoding of a specified algebraic substrate which was given in the natural spacetime labeling of a d+1 spacetime to the labeling which associates naturally with its horizon (in the highly symmetric case of AdS one takes a brane at infinity). Of course physics depends not only on the substrate but also on its spacetime organization. It is a bit like stem cells which by encymes can be forced to organize in different ways (organs). Localization in QFT, independent of whether it can be physically realized (as in case of black holes) or just thought about (Localization in Minkowski spacetime a la Unruh), leads to thermal manifestations which are caused by vacuum polarization near the causal (or event) horizon. The Hawking effect is fully accounted for by quantum matter in a Schwartzschild spacetime. Any state which extends from the outside into the black hole will lead to Hawking radiation at the Hawking temperature (in particular the state which is invariant under the Killing motion). Nowhere in the existing derivation are gravitons or QG entering. Since the state is thermal, it has also an associated entropy. To compute that localization entropy, I have developed a formalism of holography on null-surfaces (which led to extended chiral QFT) which explains why the entropy follows an area law. This area behavior is totally generic and has nothing to do with Bekenstein. In fact one obtains a one-parametric family of entropies depending on the chosen thickness epsilon of the vacuum polarization "atmosphere". This family corresponds to the family of boxed Gibbs systems which one introduces to define the thermodynamic limit. Of course one can use Bekenstein's classical formula and equate it with this microscopically computed entropy to determine epsilon (I have not done this, but there can be no doubt that at this point the Planck length enters). The calculations are in two papers (the first one is published in CQG and the second has been submitted there)
http://br.arxiv.org/abs/hep-th/0507038
http://br.arxiv.org/abs/hep-th/0511291
The reason I did not do the calculation in curved space time directly for the Schwarzschild model is very simple: by doing it for double cones in Minkowski spacetime I have a chance to slip through the prejudice of a possible string referee who would immediately reject a calculation of black hole entropy because there is no use of QG but who does not mind to do some (in his mind irrelevant) calculation on the thermal manifestation of double cone localization in Minkowski spacetime.
The important application of holography to the AdS—CFT and its connection with ST hype will be explained tomorrow in a separate blog.

34. **Bert Schroer**
October 29, 2006

Addition
I forgot to highlight the most important lesson: the localization entropy which I compute is not a counting entropy. Counting entropies (QM-like level counting in a global QFT) contradict the principle of local covariance (mentioned in earlier blogs) and the same holds for the energy caused by localization through vacuum polarization. This is also the reason why no expert of QFT in CST (as e.g. Hollands and Wald) accepts the Weinberg kind of counting estimates for CC (those which led to those absurd values).
The mysterious connection of holography with QG process came through 't Hooft and it is interesting to note that Susskind in one of his first papers had the right intuition that holography has something to do with the lightfront. Since he was very impatient he never got beyond the old lightcone quantization which is contradictory and has to be replaced by the much more subtle lightfront holography. If he would have been more patient and careful he would not presently find himself stuck in that metaphoric lanscape trao from which he will never get away any more.

35. **Bert Schroer**
October 30, 2006

Here is the continuation:
As the null-surface holography is a change in the spacetime encoding of an algebraic substrate which is given in bulk form (explained in my previous blog), so is the AdS—CFT correspondence. In that case the change of the spacetime ordering-device is more gentle and has a unique inverse (without necessitating additional assumptions). This has been elaborated with great clarity by Rehren in a series of papers, including the demonstration that it agrees with Witten's version of a prescribed conformal source in a functional integral representation (to the extend that the artistic functional integral approach dan be used for proofs). I do not know any competent quantum field theorist who does not accept Rehren's work as the correct formulation of AdS—CFT holography (Hollands, Wald, Brunetti, Fredenhagen, Verch, Buchholz,….)
On the other hand the Maldacena conjecture alleges to address the AdS—CFT correspondence, but it burdens the AdS side with that vague 't Hooft idea of holography as

a consequence of QG (in Maldacena's case the QG attributed to ST) which it is not capable to carry.
As argued abobe this is not what holography can deliver, but since the computation is so vague (involving not only perturbation theory but also additional limiting assumptions), and there is no clear structural expectation on the AdS side, the ST people think that their computational massaging has supported whatever their conjecture is. One should say in addition that even their assumption that their SUYM is conformal has never been shown (apart from conformality it is not even clear that the beta-function vanishes in all orders, and in addition the N–>infinity limit is not really a QFT). We are seeing in front of us precisely that situation which Phil Anderson describes: the unmerited success of the SM has created an era of hubris and self-delusion fueled by two Nobel laureates and one Field medal winner (but if I may add even Nobel laureates would not be able to do this without the foot soldiers being ready for a new age physics). I would bet a case of good wine with any reader of this blog that the Maldacena conjecture will never be proved, it will just fade away as all conjectures coming from ST.
In a weird attempt to maintain the Maldacena construct Distler and Motl have ignited a conceptual stink bomb (using the truism that in applying theorems one has to check prerequisites) accusing Rehren of something which they and not Rehren committed. This is to burden holography with the requirement of producing a quantum gravity Fatah Morgana on the AdS side.
The metaphoric approach to particle physics will certainly continue (on Friday there was a paper where the conjecture is used as the bases for meta-metaphoric excursions into the blue yonder). There is nothing which can stop it, similar to things in the political era hubris has to play out all the way.

36. **anonymous**
October 30, 2006

Smolin's post is full of misleading statements. For starters, he claims to mention decoherence in his book.

"Decoherence is of course a real phenomena, and is mentioned briefly on p 252 of TTWP."

This is completely misleading. He mentions decoherence in a context completely unrelated to quantum mechanics and measurements, and without any explanation whatsoever. The term decoherence appears as an offhand remark about particles propagating through spin networks. Sorry, but merely mentioning a word in a book just so that you can point to it in your index doesn't count.

Then, just to make clear to everyone why he doesn't understand quantum mechanics, Smolin says:

"While there are claims in the literature that it by itself solves the measurement problem this has, in the view of many experts, been shown wrong, indeed quite a long time ago, for example in papers of Abner Shimony. Decoherence does play a role in some formulations of quantum theory that address the measurement problem, for example the consistent histories approach. But I believe that I correctly characterized the situation in Chapter 1, which is that there is no consensus among experts as to whether any of the proposed interpretations or reformulations of quantum mechanics so far proposed are completely satisfactory. You can find arguments in the literature pro and con many different viewpoints and approaches. Had I represented one of them as a consensus view I would not have been accurately representing the field."

First of all, the papers he is referring to by Abner Shimony (from 1974) are deeply flawed and completely miss the point of decoherence. They prove a theorem that there will generally be cross terms in the final density operator for an experimental apparatus after a measurement takes place, and thus the experimental apparatus isn't a mixture of definite eigenstates corresponding to definite results.

Well, DUH! That completely misses the point of decoherence. In decoherence, there will certainly be cross terms. The point is that all cross terms have coefficients that go like ~e^(-t/tau) for some small rate constant tau, and thus go to zero exponentially fast. The cross terms aren't literally zero, but in an extremely rapid time interval (for an object like a table sitting in a room filled with air molecules, at time scale of order

37. **anonymous**
October 30, 2006

10^-43, which is essentially instantaneous since time scales that small don't really make sense anyway) the cross terms become completely undetectable by any other observer, directly or indirectly, and thus are really gone. If no other observer could ever even in principle distinguish between a density operator with such infinitesimal cross terms and a density operator without them, then you are pretty crazy to demand that they exist in any physical sense any more than invisible pink elephants.

Look, there are certainly issues when quantum mechanics is applied to the universe as a whole, to the big bang, etc. Quantum mechanics may need to be revised, extended, or replaced by a deeper, more general framework. But there are simply no remaining issues with quantum mechanics as applied to "every-day systems". And it's time physicists stop telling the public otherwise.

And no, I'm not Lubos.

38. **Bert Schroer**
October 30, 2006

anonymous
I cannot speak for Smolin, but I agree with you (see my contribution here before Christine); decoherence leads to a loss of phases for all practical purposes and hence solves the measurement problem fapp. However the event of observation (blackening of a photoplate, click in a counter) is irreversible (factualization) and QM (decoherence is part of QM) is reversible.

39. **Peter Woit**
October 30, 2006

I really wish people would take discussion of the foundations of QM elsewhere, it's off-topic, and definitely not something I want to moderate a discussion of here.

Anonymous,

I don't think you're Lubos, but you share some really obnoxious behavior with him (and you are at Harvard, what the hell has gotten into the drinking water there?). Your claim that Smolin "makes clear to everyone why he doesn't understand quantum mechanics" is just rude and idiotic. From my reading in this subject, the opinion that decoherence does not completely solve the interpretational problems of QM is a pretty conventional one among experts on this.

I don't happen to share Smolin's opinion that thinking about these foundational issues of QM is probably necessary to make progress on quantum gravity, but there are quite a few very prominent physicists who agree with him, and until the question of quantum gravity is settled it's definitely a valid position.

If you want to argue with Smolin about QM, find somewhere else to do it. You might also consider whether anonymously insulting people is really an acceptable thing to be doing, no matter what the circumstances.

40. **Eric Dennis**
October 30, 2006

Assuming this doesn't overtax Peter's patience with the digression, it ought to be mentioned that one of the very prominent physicists who would presumably agree with Smolin's opinion that foundational issues may be important for further progress in fundamental physics is the guy who first came up with the very idea of decoherence way back in 1952 (Phys Rev). That guy would be David Bohm. And he certainly did not think the mechanism of decoherence *by itself* could resolve the foundational problems in QM.

Prospects For a String Theory Phenomenology

October 24, 2006
Categories: Uncategorized

Yesterday at the KITP Michael Dine gave a very good survey talk on [Prospects for a String Theory Phenomenology](). It's pretty much hype free, has a much more realistic point of view than most talks on string phenomenology that I've seen, and gives a good idea of the current state of the subject.

Dine claims that almost all string theorists now accept the existence of flux vacua, although only some have adopted the anthropic landscape philosophy of Susskind et. al. He describes some string phenomenologists as closing their eyes to the problems represented by the large number of these vacua, and just working on some of the more tractable examples in the hope that something will turn up that will allow them to make some connection to the real world. He himself is convinced by the Denef-Douglas argument that even identifying a single vacuum state with sufficiently small cosmological constant is impossible, so that one has to make statistical arguments. He tries to have some optimism that perhaps this statistical study will allow one to make some kind of prediction, perhaps about whether the scale of supersymmetry breaking is low or high, although so far this has turned out to be impossible.

The discussion at the end of the talk is very interesting, with Dine acknowledging that there are lots of reasons he may be barking up the wrong tree and saying that he would be happier if this turned out to be the case. He quotes Witten as telling him that what he is doing can't work, that there isn't much point in trying to do the calculations he is trying to do because:

A.: "You are probably not going to succeed", and

B: "If that is all you can do it would be a great disappointment. We have this beautiful theory and we are going to get everything out of it" .

Comments

1. **Peter Orland**
   October 25, 2006

   Hi Peter,

   As you and I have privately discussed, I don't object to string theory as a laboratory to study gravity or unification per se. On the other hand I find myself very frustrated with the many scenarios devised to do phenomenology. The problem is not that these scenarios are complicated. It is that the complications cannot be forseen.

   Imagine if early 19th century chemists, cooked up an atomic theory and tried to explain the bulk properties of matter (hardness, thermal conductivity, phase diagrams). They would certainly fail because they would lack statistical mechanics and the basic ideas of spontaneous symmetry breaking (crystallization). The only property they would have understood is the density of compounds and alloys (as Dalton did). The mathematical concepts needed to understand the rest would never been discovered without experiments. I am not a historian, and for all I know, some brilliant chemists in 1810 actually tried this. I admit am guilty of pseudo-historical speculation to justify my point, but I think it is obvious regardless of this speculation.

   Often when experimentalists probed some higher orders of magnitude in energy scale, collective phenomena were discovered which were not forseen. This doesn't only apply to particle physics, but from the first investigations of matter to the study of crystals and kinetic theory, to
   atoms, etc.

   If string theory (or something like it) is right, it seems far too ambitious to think that we can pick out the right scenario to describe nature. Nature is smarter than any of us.

2. **gunpowder&noodles**
   October 25, 2006

   Would I be right in saying that this is the first time Witten has been quoted as saying definitely that he thinks the landscape approach is probably wrong/not worth working on?

3. **Benni**
   October 25, 2006

   Suesskind also said in Munich to me in person that Witten hates the landscape because "we cannot get any predictions from it". "Witten hates it" Suesskind said.

4. **AdamBalm**
   October 25, 2006

   I'd definitely second Peter (Orland, not Woit). But it irks me whenever someone says that ST is far too advanced to be probed with current technologies.

   This seems to me to be an example of petitio principii, or 'begging the question' in an argument. It's a false analogy because it is proved by its own premise. If I chose to construct any theory that I said was the most advanced of all, but that could not be experimentally verified, I could easily state that it cannot be falsified or verified precisely because it is so advanced. There's no way to argue against it because this is circular logic. This form of argument is used by Intelligent Design advocates fairly often. For example, I've actually heard someone say: "Looking at the complexity of the natural world and saying that there is no designer is like looking at Mount Rushmore and trying to find out what geological processes created such human-like rock formation". You see how deceptive such logic games are, while essentially proving or saying nothing at all. I could make the same argument that you gave, for any form of quantum gravity that doesn't make testable predictions. If one chose to play by those rules, one could say that the Flying Spaghetti Monster is so advanced so as to escape detection. The theory that music originated before language was brought up by Dennett as an example of theories that might sound great, but since there's no way to verify that in the present, they are essentially of no use to us.

   So essentially, we could choose to 'take it on faith' that ST is so advanced that all we can do is sit and wait for a few hundred years and that it will eventually proved correct, once the rest of science catches up to the level of brilliance that ST theorists are already working at. But this is something that has never been asked in the history of science. Theories, like definitions, sometimes aren't so much right or wrong as useful and useless. ST can provide a useful context and has helped people in understanding gauge theories and so forth (or so we are told), but overall has it been very useful to theoretical physics as a whole? I think this is a more important question than if it is right or not, is asking the old 'What has it done for us lately?'

5. **anonymous**
   October 25, 2006

   I find it very amusing that while you accuse string theorists of being a cult of Witten followers, they rationally look at the equations of the theory, while you guys fixate on what he “likes” or “hates”. This is physics: who cares what someone likes? Truth matters!

6. **milkshake**
October 25, 2006

P. Orland: Your analogy about chemists is misleading. Using atomic theory, they could make all kinds of qualitative and quantitative predicticon: exact composition of yet-undiscoverd salts. Behaviour of gases. Connectivity rules/molecular shape of carbon-based chemicals. Periodic table.

First you guess. Then you figure consequences of the quess and compare them with experiment. If it’s wrong…

7. **woit**
October 25, 2006

gunpowder,

As far as I know, Witten’s only public comment about the landscape so far has been something like “I wish it weren’t true, but I have no good arguments against it”.

anonymous,

I’ve repeatedly, in great detail here, made the scientific case that the kind of computations Dine is talking about can’t possibly lead to a legitimate scientific prediction. In response I’ve been denounced as a crackpot and repeatedly informed by string theorists that I don’t know what I’m talking about. I’m not going to apologize for reporting that Dine says Witten agrees with me on this point. As you might have noticed, I have an extremely high opinion of Witten, while at the same time being quite capable of disagreeing with him when I think the truth of the matter is not on his side.

8. **anonymous**
October 25, 2006

Peter:

A legitimate scientific prediction, in model building, consists of writing down your model, giving a convincing argument that it isnt yet ruled out, and making at least *one* prediction that can be verified by future experiment. Tye’s
inflationary models and Tye/Polchinski’s cosmic superstring ideas certainly fall within this rubric. So do many ideas for particle physics models motivated by the string constructions. None of them are a “prediction of string theory,” they are models which came out of string theory and which are predictive at the normal level of particle physics or cosmological model building. I find it very disingenuous to try and claim string theorists need to derive the whole world from top down to be doing legitimate model building. No one else ever does that, in science. Dine was, unfortunately, apparently focused on that unlikely goal

9. **woit**
October 25, 2006

anonymous,

If Tye/Polchinski would stop talking about “predictions of string theory”, and be careful to explain that they are talking about specific models, not string theory itself, there would not be a problem. That’s not what they’re doing.

People are welcome to work on specific models, they just shouldn’t sell the implications of these specific models as “predictions of string theory” or “tests of string theory”.

10. **anonymous**
October 25, 2006

Hi Peter:

If one of their models were verified, and the resulting string network had
the low P or multiple species of (p,q) strings characteristic of the
warped throat models, I would consider this evidence for string theory.
That is the sense in which such people call searches for such things tests of string theory; verification would provide evidence for a natural string based model, while falsification means that kind of string model is wrong. I completely agree (as would they I am sure!) that none of these models are inevitable consequences of string theory. Most string theorists I have heard go out of their way to stress this when presenting any model building type results. But verification of some prediction of this kind of model would be a big clue for string theorists, maybe telling them which limit of the theory is relevant to nature (if any). This is how all of science works; this process also led to the correct quantum field theory description of particle physics (it was not some instantaneous top down epiphany, but rather winnowing of a large structure to limits that seemed consistent with data). Anyway this view seems to be in partial agreement with yours. But I do think that because of this kind of model building, combined with future experiments, string theorists may well eventually learn which kind of vacuum is relevant, and make some predictions.

11. **a**
October 25, 2006

anonymous: can you give concrete examples about which possible developments could lead to which predictions?

12. **George Giles**
October 25, 2006

When Einstein was working on the General Theory of Relativity he had some experimental facts, and a load of intuition. He had the physics but not the math. Marcel Grossman and he then went looking for it and found it in Riemannian Geometry. It seems that String Theory is taking the opposite approach, a whole load of math (formally undecidable theories at that) and are searching for the physics.

Now even a blind pig finds an acorn every now and then, but it seems that the current approach is from a historical prospective, unlikely to be successful. My gut feel is Newton followed an approach like Einstein, not one like Witten et al.

I have to wonder if String Theory has gone from diminishing returns to vanishing returns to paraphrase Milton Friedman.

13. **anonymous**
October 25, 2006

a:

Well, I already mentioned, discovery of cosmic strings with certain network properties would give evidence for a class of models of inflation discussed in many papers over the past few years, and some of these properties were first suggested by string constructions (not field theory ones). There are other kinds of inflationary experiments (measuring nonstandard features of the density/temperature fluctuation spectrum) that will be launched in the next five years and could test another class of string inflation models.

In particle physics, obviously large extra and warped extra dimensions drew much attention because they have spectacular signatures at LHC (though I think they are quite unlikely). Verification of either would again be a big hint about which kinds of string models matter; both warped and large extra dimension models have recently been found in detailed studies of string compactification with flux.

5th force experiments may eventually find moduli with small mass. In supersymmetric string models, one class of gauge mediated models would predict moduli in the sub eV range. If both the moduli and the spectrum of gauge mediation were seen, this would be a hint.

Other people have found mediation scenarios where distinct spectra of sparticle masses result from specific extra dimensional structures (anomaly mediation, gaugino mediation, mirage mediation,…) which you can read about in review articles.

Altogether, the discoveries of LHC, future CMB experiments, and 5th force type experiments (together with gravity wave detectors) have all kinds of ways of giving us hints about high energy physics. A confluence of several hints could select the right kind of string model. Of course luck would also be required (how lucky would we be if there are extra dimensions at LHC??). But thats always been the way experiment helps theory; a lot of hard work developing the theory, and eventually, a decisive stroke of good luck in what is discovered. Arguing that one cannot be sure that experiment will give big hints about the right string model in the next decade is silly; that is obviously true, and has been equally obviously true for all really interesting theoretical developments in the past century. We will need to get lucky, but there are many ways to do so, some of which we haven't thought of yet. Thats why I am excited about this field.

14. **steve**
October 25, 2006

The following statement from our host clarifies what I asked about models vs. theories on the previous thread:

"If Tye/Polchinski would stop talking about "predictions of string theory", and be careful to explain that they are talking about specific models, not string theory itself, there would not be a problem. That's not what they're doing.
People are welcome to work on specific models, they just shouldn't sell the implications of these specific models as "predictions of string theory" or "tests of string theory"."

If string "theory" is just a framework for generating specific models, and it is specific models rather than the framework as a whole that are to be tested empirically, then the dispute collapses to a much smaller and more manageable level. The only residual problem would be if, regardless of their merits, models developed out of the string framework received more favorable evaluations than models constructed some other way. Absent evidence of that, I'd be inclined to think that the hype about strings is silly but not too damaging.

15. **anonymous**
October 25, 2006

All of the models of TeV scale physics I mentioned (large extra dim, warped
extra dim, gaugino mediation, anomaly mediation,…) were actually developed by people who received PhDs that are NOT in string theory. They all classified themselves as particle phenomenologists or model builders. Those works are all famous and well cited, and their authors received faculty positions (those who did not already have them). String theorists were very happy to build on their ideas, or try to find them in string theory. So there has been interplay, but I think it would be grossly unfair to say that only models that are constructed by string theorists, or first derived from string considerations, have been "hyped". It would be hard to imagine more hype than that surrounding the extra dimensional models that were NOT conceived of (first) by string theorists. More generally, however, the interaction between strings, cosmology, and particle phenomenology has been very healthy in the past decade. It is now hard to even classify some people who work in two or three of the fields. This is a good thing.

16. **Dick Thompson**
October 25, 2006

Just on the topic of a nineteenth century calculational atomic theory, Daniel Bernoulli in the eighteenth century used basic "corpuscles" acting elastically under conservation of momentum to derive PV = const. He or someone else might have taken the next step, identified "heat" with the summed *vis viva* of the corpuscles and the temprature with their mean *vis viva* and derived the virial theorem. I don't think a limited "kinetical chemistry" theory was impossible at that time, and historical analogies are always questionable because of the great influence of individual personalities on both past and present.

17. **Peter Orland**
October 25, 2006

Milkshake,

I think you missed the point of my analogy.

As you say, you make a guess. Calculate. Check against experiment. Make another guess. And so on. The point is that none of us is smart enough to start from a very high (low) energy scale and guess how nature should be at a very low (high) energy scale without a LOT of guesses and calculations and experiments
in between. Well, I think it can't be done.

The reason it can't be done is that every time you go to smaller scales, there is the possibility of new statistical/collective/nonperturbative phenomena happening. In
my foolishly naive analogy, I was trying to say that this is like
starting from the notions of atoms and calculate properties
of matter. It's impossible without investigating the molecular
level (kinetic theory), van der Waals forces (electricity
and magnetism), electrons and nuclei (quantum mechanics), and
the mean field theory of crystals (spontaneous symmetry breaking).

Continuing this analogy (since I don't have a better one), string theory would be like the quantum theory of electrons and nuclei. Imagine trying to extract bulk properties of matter from a many-body theory of electrons an nuclei! It can only be done in hindsight. Too many sophisticated phenomena happen between an Aangstroem unit and a centimeter.

I'm not saying that it isn't worthwhile to study the theory we guess is fundamental. I just have no confidence that one can stretch that theory from the Planck mass to 10 TeV and make sensible predictions. That doesn't mean the theory is wrong, but that we aren't smart enough to guess what happens at intermediate energy scales.

18. **a**

October 25, 2006

anonymous, you answered mentioning many possible natural solutions to the Higgs mass hierarchy problem. Do you still hope that dark energy will turn out to be something natural, rather than an unnaturally small cosmological constant?

If not, it seems better to see nothing new at LHC and get a second stroke on naturalness: a defeat naturalists vs anthropists = 0 : 2 would carry a strong message, while an impact 1 : 1 would not clarify this big issue.

19. **anonymous**
October 25, 2006

Hi a:

There are as yet (as far as I know), no good "natural" solutions to the
cc problem. But logically speaking, all the evidence for the string landscape just means that there are many vacua, some of which plausibly have small enough lambda. It could be that they are "populated" by cosmology in such a way that some dynamics enters in explaining the observed value. Needless to say, despite very many attempts in this direction (with various modifications of the Hartle/Hawking wavefunction, or more…questionable…approaches by Turok and Steinhardt), nothing less tuned or more convincing than just a small cosmological term explained by Weinbergs argument has emerged. But again, the evidence for the landscape just means small lambda can be accomodated in string theory, it does not necessarily mean anthropics has to be the explanation.

Arkani-Hamed and Dimopoulos and various others have tried to think of ways that LHC could give evidence for un-naturalness. The attempts are kind of forced, since there are good explanations for the higgs mass that come pretty easily out of string theory (at least as easily as their more contrived, though very testable, proposed "anthropic" models). But I agree if one of those models, or "higgs only," came out at LHC, it would be another big pointer to a new notion of naturalness, very possibly indicating the landscape again.

20. **relativist**
October 26, 2006

Hi anonymous

"the evidence for the landscape just means small lambda can be accomodated in string theory" – the word `evidence' here has taken on a rather unusual meaning. You presumably mean something like `theoretical support' rather than evidence in the usual sense of the word. Indeed Vilenkin tries to run it the other way and say that the small value of Lambda (which is evidence in the usual sense – it is an observed piece of data!) is evidence for the landscape – and for the multiverse, which is not observable in any ordinary sense of that word. This seems to be stretching ideas of scientific proof to a great degree – compare with what is taken as adequate proof in say solid state physics or quantum optics.

21. **Thomas Larsson**
October 26, 2006

OT: I suppose everyone has noticed that physics/0610168 references our host.

22. **anonymous**
October 26, 2006

Relativist:

Yes, I just meant evidence for many vacua in string theory, using standard physical techniques to abnalyze the vacuum structure. I did not mean there is experimental evidence. But it is true that the many vacua may well turn out to offer the best hope of accomodating (explaining??) The small cc in a quantum gravity theory. Of course we would need other more traditional tests of string theory to make this at all convincing, as all reasonable string theorists would acknowledge.

23. **Who**
October 26, 2006

OT: I suppose everyone has noticed that physics/0610168 references our host.

I noticed that. It is Hedrich's paper reporting on a study he did for the German equivalent of NSF, about the state of theoretical physics. I think our host had a blog sometime back that was partly about Hedrich and some coverage in the German press.

24. **LDM**
October 26, 2006

Referring to footnote 12 of the physics/0610168 about string theory and GR…

If you actually check what Feynman said in the "Feynman Lectures on Gravitation", page 30…you will see that the (so far undetected) graviton, does not, a priori, have to be spin 2, and in fact, spin 2 may not work, as Feynman points out.

This elevation of a mere possibility to a truth, and then the use of this truth to convince oneself one has the correct theory, is a rather large extrapolation.

25. **Anonymous 17**
October 26, 2006

Has anybody noticed hep-th/0610241, which seems to be the start of non-commutative geometry phenomonology. Connes and co-authors predict the mass of the Higgs boson. String theory appears to be starting to fall behind.

26. **Bert Schroer**
October 26, 2006

I find this point which Hedrich revives quite interesting. The massless finite helicity Wigner representation (say integer spin for simplicity) have all one property in common, their inner products cannot be written in terms of integrals over local densities. In order to do this (and also to implement interactions) you have to introduce "potentials"(the vectorpotential for h=1, the metric potential for h=2 where the field strength would be the linearized Lorentzian Riemann tensor etc.). This creates a dilemma in the quantum theoretical setting (none if one stays classical) since there is a No-Go theorem: there are simply no covariant pointlike fields in the Wigner-Fock Hilbert space. There are two ways out. Either you give up the Hilbert space in intermediate calculation and admit ghosts (which of cause can only be used as "catalyzers" because you have to get rid of them at the end of the calculation); this is the BRST cohomological setting. The alternative is to use covariant potentials which however describe string-localized fields (which fluctuate simultaneously in Minkowski- and the string-directional de Sitter space). In both cases you end with potential fields whose

short distance behaviour is not worse than that of a scalar free field (in contradistiction to the field strength whose scale dimension increases with the helicity). The renormalizable interactions are precisely of the "gauge" kind (although that language is not very appropriate in the second setting since you never leave the physical Hilbert space) but for the sake of this discussion it would be justified to call them all "gauge theories" (not only the case h=1).
In some sense the Einstein-Hilbert gravity interaction should be in this family of interacting h=2 theories. But there is a profound conceptual problem: the diffeomorphism invariance (according to Einstein an active transformation which also changes the points) is not the same as gauge invariance (phase transformation) which only effects the fibers over points.
I think it would be very interesting to re-investigate the issue of ref. 12 of Hedrich using the gain in conceptual understanding.
By mere coincidence some of this points were discussed yesterday in the samizdat blog.

27. **D R Lunsford**
October 26, 2006

Bert said

In some sense the Einstein-Hilbert gravity interaction should be in this family of interacting h=2 theories. But there is a profound conceptual problem: the diffeomorphism invariance (according to Einstein an active transformation which also changes the points) is not the same as gauge invariance (phase transformation) which only effects the fibers over points.

Yes, this is exactly the key point and why all attempts to make the gauge potentials part of the metric via KK ansatz are doomed. Pauli already knew that.

-drl

28. **D R Lunsford**
October 26, 2006

The paper by Hedrich is outstanding and long overdue.

-drl

29. **Bert Schroer**
October 26, 2006

The word "phase transformation" may be misleading (I was already thinking about coupling these higher helicity potentials as external fields to quantum matter). I would expect these transformation to be linear (generalization of h=1 gauge transformations). I would be surprized if this issue has not been already adressed in the literature. What is however important here is to start from the Wigner representation theory in order to avoid playing formal games with ad hoc higher tensor fields.

30. **Bert Schroer**
October 26, 2006

D R Lunsford
I did not know about Pauli's criticism, very interesting! Where did you see this?
There is an independent much more profound argument against KK coming from algebraic QFT. This is based on the observation that inner symmetries result (the DHR analysis) from the representation theory of local observables. It is the representation theory (modulo isomorphisms) of these local nets which leads to the statistics of particles as well as to the existence of charge-carrying fields which intertwine the charge sectors and encode the statistics into inner symmetry properties of these charge carrying fields such that the original observables are reproduced as the fixed point algebra of the field algebra. In 4 spacetime dimensions the emerging symmetry concept is that of compact groups. If you would not have locality the possibilities of representations would be too big and not be classifiable. The conceptual situation of observables—charge carrying field algebra (i.e. its construction from the observables) is precisely following Marc Kac's dictum: "how to hear the shape of a drum". It is totally absurd to think that such a situation can emerge from little rolled up spatial dimensions (KK is however possible in the classical setting, but is does not solve any physical problem). But there is nothing which one can do against collective madness; one just has to let it play out.

31. **egbert**
October 26, 2006

DRL said:

> Bert said
>
> *In some sense the Einstein-Hilbert gravity interaction should be in this family of interacting h=2 theories. But there is a profound conceptual problem: the diffeomorphism invariance (according to Einstein an active transformation which also changes the points) is not the same as gauge invariance (phase transformation) which only effects the fibers over points.*
>
> Yes, this is exactly the key point and why all attempts to make the gauge potentials part of the metric via KK ansatz are doomed. Pauli already knew that.

Is there a simple and obvious way to get from the first observation (diff inv is not gauge inv) to the second (KK is doomed)? I'm probably missing something that's obvious to both of you, but it sounds like Witten is missing that same thing as well, among others, so perhaps I can be forgiven for not seeing it right away.

32. **D R Lunsford**
October 26, 2006

egbert – Witten is a mathematical genius but Pauli was connected at the belly button to the Universe, and his arguments win in my book. Having great skill at math does not confer physical insight.

-r

33. **woit**
October 26, 2006

Please, this is not sci.physics. Stop trying to turn it into a discusssion forum for your favorite ideas about GR. This is completely off-topic.

34. **Bert Schroer**
October 26, 2006

I agree with Peter that this does not fit the headline of this blog, sometimes an interesting issue throws a weblog of topic.
It is however a subject which I addressed in my Samizdat article. So may be Egbert I could answer your question over there. Another possibility is that you use my email

address. But then an interesting subject will be lost to the typical reader of Peter's weblog which is not fitting the purpose of a scientific weblog. Since Peter is very interested in the use of representation theory for particle physics he may actually also be interested to know arguments which show that in the quantum setting there is a deep clash with the KK idea (independent of Witten's opinion).

35. **egbert**
October 27, 2006

Let's continue at my new blog.

In fact, I propose that, whenever a conversation wanders into realms that aren't appropriate for the host blog, a new blog post should be instantiated somewhere and a link posted to the original blog, so that those who wish to follow the conversation can do so.

36. **Bert Schroer**
October 27, 2006

Egbert
when I tried to follow your suggestion to move my 2 last contributions under Samizdat over to your new blog (as a kind of branched-off sub-blog to Peter's) and to start a more extensive discussion from there I realized that there must have been a malfunction with Peter's weblog because the list of names on the right margin does not correspond to the content after opening. What worries me even more is that only the continuation of my yesterday's contribution appears fully, the blog I wrote immediately before is incomplete (it lost 2/3 of its content) and unforyunately I did not make a copy. So before moving over, let us wait and see if Peter can check what happened.

37. **Peter Woit**
October 27, 2006

Bert,

I have no idea what the problem might be. What you see in the comment section of the blog is all that there is.

38. **Bert Schroer**
October 27, 2006

O.K., maybe there was a malfunction in the uploading. Of course I can reconstruct what I wrote and place it as a comment into Egbert's comment section, but I need some time (I will be able to do this today). So I invite all reader's who are interested in that particular issue to move later in the day to Egbert's weblog
http://kaluza-klein.blogspot.com/
(which I do not want to see as a competition to Peter's but rather as a means to continue with interesting points which are slightly off-topic).

39. **C**
October 27, 2006

Hi anonymous

"both warped and large extra dimension models have recently been found in detailed studies of string compactification with flux."

Could you provide a link to this work, or a hint such as the names of the authors, to help me find the papers?

Thanks very much.

C

40. **anonymous**
October 27, 2006

Hello C:

There are by now lots of papers on these things, but I think it is reasonable to point to:

For scenarios involving exponential warping like Randall/Sundrum, a standard reference is now arXiv:hep-th/0105097 by Giddings et al. The references in that paper and the citations to it provide a much larger guide to the relevant literature. Those models are important in hep-th/0301240, the original "KKLT" paper on flux compactification.

String derived models of large extra dimensions are described in
hep-th/0505076 by Conlon et al which focuses on phenomenology, and the citations/references of that paper.

41. **C**
October 27, 2006

Hi anonymous

Thanks very much.

C

42. **Thomas Love**
October 27, 2006

Peter, Thanks for the link to Michael Dine's talk. I listened to it and it was just like being there. I fell asleep 15 minutes into his presentation and was awakened by the applause at the end of his talk. I did go back the next day and listen to the whole thing. His comments at the end seem to reflect the reality of the problems with string theory, he was discussing the many states of string theory:

At 1:08:12 into the recording(slightly edited):

"You line up your graduate students. You give them each a barcode for one of these states. They start calculating. They come back 6 months later and they've calculated the second order correction. It looks pretty good. They come back two years later they've calculated the third order. It looks pretty good. Twenty years later they come back with the eighteenth order—it didn't work. Ok, throw this out."

I only listened because Dine is at UC Santa Cruz, where I received my PhD. I'm glad I got out in time. I wouldn't want to spend twenty years getting a PhD! His comments reminded me of why I left physics after one year of grad school and switched to math: the whole paradigm of perturbation theory makes no sense whatsoever!

Finite Generation of the Canonical Ring

October 26, 2006
Categories: Uncategorized

The last few weeks have seen the appearance of two papers giving very different proofs of a quite important result in algebraic geometry, resolving a question that had been open for a very long time, and in the process helping to make progress in the classification of higher dimensional projective algebraic varieties. Readers should be warned that this doesn't have anything to do with physics, and my knowledge of this kind of mathematics is highly shaky, so I'm relying largely on second-hand information from people much better informed than myself.

The theorem in question concerns the "canonical ring" of a smooth projective algebraic variety X, which is the graded ring R(X) defined by

$$R(X)=\oplus_{n=0}^\infty H^0(X, nK)$$

Here K is the canonical line bundle (top exterior power of the cotangent bundle) of X, nK is its n'th tensor power, and $H^0(X, nK)$ is the space of holomorphic sections of the bundle nK. This is also called the [pluricanonical ring.]

The new theorem says that this graded ring is finitely generated, and this implies quite a few facts about projective algebraic varieties of any dimension. In particular it implies the main goal of the "minimal model program" (also known as the [Mori] program) for classifying higher dimensional algebraic varieties.

A proof of this theorem was [claimed] back in 1999 by Hajime Tsuji, but it appears that there are problems with this proof. The arXiv preprint went through many revisions, but was never refereed and published. A couple weeks ago, a group of four algebraic geometers (Caucher Birkar, Paolo Cascini, Christopher Hacon and James McKernan) posted a [preprint] on the arXiv claiming a proof. Yesterday, Yum-Tong Siu, a well-known complex geometer from Harvard, posted another [preprint], giving a very different, more analytical, proof of this theorem. Siu notes that he has been lecturing on this proof for over a year, first at last year's Seattle [conference on Algebraic Geometry].

The mathematicians involved in creating these two proofs are well-known experts, and it seems likely that both proofs are correct. Given that there are two of a quite different nature, it now seems extremely likely that this theorem has been proved.

For more detailed explanations of this result and its implications, I'm afraid that you're likely to require someone who knows a lot more about algebraic geometry than I do. Perhaps some of my more expert readers here can help out.

Comments

1. **Aaron Bergman**
   October 26, 2006

   I know next to nothing about this, but I believe that Proj of the canonical ring provides a nice model for the space.

2. **Walt**
   November 3, 2006

   I'm planning on spending some time looking into this later, since it's not my area, but it looks like this gives a minimal model for varieties of general type (i.e. most varieties given by high-degree polynomials). This is a big step forward for the minimal model program, which (in my understanding) splits birational classification into two steps: find minimal models for varieties, and then determine when varieties have multiple minimal models. I think that it's hoped that varieties of general type only have one. But as I said, it's not my area.

3. **xyz**
   November 3, 2006

   That is not quite true. There can be more than one minimal model, but only one canonical model (=Proj(R(X,K_X)).

4. **Walt**
   November 5, 2006

   Even for varieties of general type?

5. **xyz**
   November 5, 2006

   Yes. Why not? they are connected by flops and they are easy to construct. If a variety is of general type, then it follows that the # of minimal models is finite.

Some Links

October 26, 2006
Categories: Uncategorized

Earlier this month there was a workshop on twisted K-theory held at Oberwolfach. Here is a report, also slides from a talk there by Greg Landweber about the Freed-Hopkins-Teleman theorem. Freed is giving a course on the subject this fall, and Hopkins is giving a series of lectures about TQFT in Gottingen this week. Urs Schreiber has reports on the lectures here and here. Also at the n-category cafe is an advertisement by John Baez for the work of my new Columbia colleague Aaron Lauda on TQFT, which I'll second here. For yet more on TQFT, see notes by Kevin Walker here, and the book by Bakalov and Kirillov, an early version of which is on-line.

Last week in Paris there was a conference dedicated to Joel Scherk, celebrating 30 years of supergravity.

There's an interesting interview with Alain Connes on the French TV network ARTE here. For his recent work on non-commutative geometry and the standard model, see this preprint, and talks here from the on-going workshop at the Newton Institute in Cambridge.

Comments

1. **Adam Balm**
   October 26, 2006

   Great. The first chance I had to see Alain Connes and it's in Real Audio.

   Anybody ever heard of youtube?

2. **nit**
   October 27, 2006

   Nice Alain Connes interview.
   Somehow, Connes' way of talking, maybe it's the gestures, reminds me of Feynman. Relaxed, confident, playful, enjoying the questions…
   My god, I must say the interviewer is very charming!

3. **Alejandro Rivero**
   October 27, 2006

   There is audio (in English) for talk II and III in the Newton Institute website. I strongly recommend to download them into your MP3 machine so you can heard them while in the tube. Every talk recapitulates the previous one, so if you ony can allow for one, start with III. In England, the series will continue November the 9th, plus some another "one-short" talks around. I do not know if there is some similar, or even more gossip-rich, presentations in France. Hints anyone?

   There is detailed coverage in the October posts of the n-category cafe, and some more sparse coverage in Motl's.

4. **Bert Schroer**
   October 27, 2006

   "Nice Alain Connes interview"
   this is indeed a very enjoyable style, remarkably different from that of those car-salesmen which we often are exposed to.
   As a physicist one notes that all mathematicians (perhaps with the exception of Vaughn Jones and some others who entered QFT from subfactor theory) continue to have that lighthearted attitude with respect to the Euclidean—Lorentzian relation (and the related issue of localization) which for the mature quantum field theorists are among the conceptually most subtle and delicate (and even not generally valid) issues of particle physics.

5. **Justin**
   October 27, 2006

   Sorry for the somewhat off topic post, but here is a concrete sign that the opinion of string theory is changing in the public's collective unconscious. I was reading an article on realclearpolitics about a controversial interpretation of the polling data:

   > A few people did not like that argument. They accused me of "rationalizing the results," which, to an extent, is understandable. It is not appropriate to start applying theories before you have offered some initial testing of them – unless of course you are a Marxist or a string theorist, neither of which I happen to be.

   http://time-blog.com/real_clear_politics/2006/10/a_republican_meltdown_part_ii.html

   Ouch!

6. **Clay Aiken**
   October 27, 2006

   Thanks for the links Peter! I've been meaning to catch up on TQFT for some time, finally my schedule will allow me to do this.

7. **Matti Pitkanen**
   October 27, 2006

   I read the articlle of Connes and like Bert Schroer I was astonished that he mentioned only in brackets that the spacetime metric has Euclidian signature and made no further comments about the significance of this. As a physicist having at least second foot in this world I find it very difficult to understand how a physicist proposing a unification of all known interactions can have such a light-hearted attitude.

8. **Tony Smith**
   October 27, 2006

   Connes explicitly states in the pdf version of his talk "Spectral action and gravity coupled with matter III" that is on the web at
   http://www.newton.cam.ac.uk/webseminars/pg+ws/2006/ncg/1011/connes/all.pdf

   "… The spectral action principle asserts that the fundamental action functional S

that makes it possible to compare different geometric spaces at the classical level
and is used in the functional integration (after Wick rotation to euclidean signature) to go to the quantum level,
is itself of the form … Trace (f(D / /\ )) …
where D is the Dirac operator and f is a positive even function of the real variable while the parameter /\ fixes the mass scale. …".

Maybe some people don't like such use of Wick rotation, but Connes is model-building,
and if he can construct a physically realistic model using such Wick rotation
then
in my opinion it will be so useful that it will be (and should be) as widely accepted as are
some not-yet-rigorously proven aspects of the Standard Model
and
any objections as to rigor should be motivation for further work on the model
rather than justification for trashing the model.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

9. **Bert Schroer**
October 27, 2006

well Tony, that is what I meant by lighthearted. Something like Osterwalder Schrader is not lighthearted.
But I agree with you that it is not illegetimate to do that and (as long as results are interesting and one does not forget that there is a conceptual problem) defer such questions to a later date (although this has been going on for more than a decade). However it is only a theory after these quations have been settled.

10. **Tony Smith**
October 27, 2006

As to objections about localization,
Connes said in the pdf file that is on the web at http://www.newton.cam.ac.uk/webseminars/pg+ws/2006/ncg/1011/connes/all.pdf

"… Assuming first that we deal with a classical manifold, one can form a number of such invariants (under suitable convergence conditions) as the integrals of the form
[Integral over M] F(K) sqrt(g) d^4x (9)
where F(K) is a scalar invariant function … of the Riemann curvature K.
Such invariants, of the form (9) appear as the single integral observables i.e. those which add up when evaluated on the direct sum of geometric spaces.

Now while in theory a quantity like (9) is observable it is almost impossible to evaluate since it involves the knowledge of the entire spacetime and is in that way highly non localized.

On the other hand, spectral data …
(the data of spectral lines are intimately related to the Dirac Hamiltonian, hence to the geometry of "space") …
are available in localized form anywhere,
and
are (asymptotically) of the form (9) when they are of the additive form
Trace (f(D / /\ )),(10)
where D is the Dirac operator and f is a positive even function of the real variable while the parameter /\ fixes the mass scale. …".

Again,
Connes is NOT ignoring the issues of localization and Wick rotation,
but is explicitly stating how he is building his model,
and
if his model is realistic, I think that it is useful.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

11. **Alejandro Rivero**
October 27, 2006

The goal in that paper was to get coincidence with Veltman's Diagrammar. Let me note that one of the first impulses, fifteen years ago, was Daniel Kastler, who did a lot of the calculations of the Higgs potential in the old models.

12. **Matti Pitkanen**
October 27, 2006

The idea about spectral data of Dirac operator as a manner to code nonlocal data locally is beautiful. I cannot avoid temptation to briefly mention (Peter can delete this posting if it is too much about pet ideas) an approach in which Dirac operator associated with lightlike 3-surfaces which by Diff^4 invariance can be chosen to be fundamental dynamical objects. Vacuum functional is coded as a Dirac determinant (appropriately defined) having interpretation as action exponential characterizing interior dynamics of space-time. In this case lightlikeness is the source of all superconformal symmetries, it defines parton level dynamics as almost topological CFT, and it gives rise to quantum gravitational holography. Everything would be lost by Euclidization trick.

13. **C**
October 28, 2006

On the topic of the conference in memory of Joel Scherk. Obviously Joel Scherk did very great work, and it's very tragic that he died. The proceedings of the 1979 Stony Brook supergravity conference, which were dedicated to his memory, said he had diabetes, and got stuck somewhere without his insulin, and went into a coma. But the Wikipedia article, which is just a stub, doesn't mention diabetes, and contains a link to a review of a book by Leonard Mlodinow, which contains a paragraph that provides different information. Could anyone provide more historical information?

Fit To Be Tied

October 27, 2006
Categories: Uncategorized

This week's Science News has a quite good article about the string theory controversy by Peter Weiss, unfortunately available on-line only to subscribers. The title is "Fit to be Tied: Impatience with string theory boils over". There's nothing much in the article that will surprise anyone who has been following this story here. It includes some accurate quotes from me and some from Lee Smolin, with the string theorists represented by Zwiebach, Polchinski and Strominger.

Polchinski claims that experiments will soon probe some elements of string theory, promoting the possibility that the LHC will observe extra dimensions. Zwiebach points to work on black holes: "In string theory, the black hole can be seen as built from strings and branes. It's a spectacular insight." Strominger on the one hand is quoted as finding it inappropriate that Smolin and I are criticizing how string theory research is conducted, while also saying he thinks that the way string theory has been promoted has given the public the wrong impression: "I've felt for a long time that the general public's impression of what string theory had accomplished and how much of it was correct was too positive."

Comments

1. **ObsessiveMathsFreak**
   October 27, 2006

   > "I've felt for a long time that the general public's impression of what string theory had accomplished and how much of it was correct was too positive."

   I think the general public deserve a little more credit. I've spoken to many "lay" people who regarded the string theory programs or newspaper reports they read with a good degree of skepticism.

   Now, the general public does regard things like Relativity and Quantum Mechanics with skeptisicm when they first hear about them(who doesn't), but at least in these cases they can be shown experiments and effects which give credance to the theory. Show anyone the double slit or Michealson-Morely experiments and they can at least appreciate that the universe is not always as logical as we think it is.

   But with string theory, there are no experiments. It really is all just symbols on paper, with only its supposed mathematical "beauty" holding it aloft. Faced with this, all that can be presented to the public is 3D animations of vibrating strings, and appeals to various scientific authorities(minus Feynman). Increasingly, more dishonest methods like violin concerts and cooking shows used as well.

   But I think general man on the street is not as guillable or as confused as certain people take him to be.

2. **r hofmann**
   October 28, 2006

   I entirely agree with the statement that the general public in a more or less working, technologically, morally, and culturally highly ambitious society should not be regarded as an unknowing, unsuspecting, and uncritical something that scientists can impose their fashion-of-the-day ideas on while standing on a disconnected, elevated platform. After all, scientists, being themselves a part of society, enjoy the privilege to unadulturatedly conduct their research just because the broad public is materially supporting them. The only way to leave a lasting impression on our collective conscience is the unearthening of scientific truth that, by definition, is backed up by strong empirical evidence, that is, by numerous and independent experimental confirmations of theoretical predictions (Smolin's ring of truth) and that, by historical experience, turns out to feed back into future everyday life in one way or another.

3. **anon**
   October 28, 2006

   ObsessiveMathsFreak, I disagree.

   I've had many friends approach me on this subject (I rarely initiate these discussions on my own devices) and my general impression is quite the opposite. The people I have discussed this with are usually people with a broad set of interest and usually with some basic knowledge of math and physics and good knowledge in say economics or history.

   My perception is the opposite, I am usually received with a lot of distrust and sometimes down right disdain when I say that string theory:

   *hasn't had any kind of experimental verification (both direct and indirect) attributed solely to it
   *doesn't replicate all previous physical theories, not even a reasonable subset of those (uniquely at least)
   *is not a coherent theory with a well set of fundamental principles, but so far only a set of rules glued together in an ad-hoc way

   The reaction is usually one of the following:

   *If it's so bad, why is it so popular? Seems to me you are just ignorant about it and just dismiss what you don't know of (I am far from knowledgeable about string theory but I think what I wrote so far is essentially true)
   *If all these uber-smart people and prize-winners do it, things can't be as you say
   *But I read an article in a top notch newspaper/magazine where it said string theory explains almost everything

   I've even had one you didn't know of the standard model (not serious sonce this was not his field) but thought that what explained all those particles was string theory and not the standard model (now this is serious).

   The truth is, humans are generally stupid about things they have barely any expertise on (myself included, and sometimes even about things they have expertise on) and usually rely on authority to make judgements about the validity of those subjects (this I try not to follow).

   Sad but it's the reality of things..

4. **TheGraduate**
   October 28, 2006

   One of the more interesting aspects of this debate has been the frequent appearance of anti-democratic tendencies. Basically, one designates a population whom one thinks is too ignorant to comment and then any comment from that group is unwelcome. I suppose my point is that, there is nothing god given about who gets to decide who is too stupid to possibly have anything useful to say. The defacto result is that whoever has the power decides.

The only recourse for those excluded from the conversation, if they so choose, is to make overt attempts at control such as using the media to amplify their agenda.

In other words, without conversation one is left with an environment where ‘might makes right’.

The other points I would like to make are:

1. Too many people perform a bait and switch in this argument where they claim it is about the good of science whatever that means. *This debate is about money and resource allocation.*

2. Essentially string theorists argue that they deserve their allocation due to their brilliance. Those opposing argue that 20 years without testable results negates this argument.

3. Those opposing argue that equally failed (experimentally unsupported and unverifiable) theories should be funded equally.

Overall the structure of the argument is very simple even if the details are admittedly complicated. Even somebody with no background in physics should be able to understand the flow of this argument.

5. **Arun**
   October 28, 2006

   Sorry for the digression, but there is no open thread here. Delete if appropriate. I’m posting this because Richard Dawkins is invoking the anthropic principle as an explanation (and as an explanation much superior to God. IMO, the anthropic principle is no more scientific than God).

   “We explain our existence by a combination of the anthropic principle and Darwin’s principle of natural selection. That combination provides a complete and deeply satisfying explanation for everything that we see and know.”

   http://www.huffingtonpost.com/richard-dawkins/why-there-almost-certainl_b_32164.html

   Because the arguments are all mixed up with religion, Peter may not want to talk about it here. But it hardly constitutes the “complete and deeply satisfying explanation for everything we see and know”.

6. **Bert Schroer**
   October 28, 2006

   My comment: atheism is no protection against amok running metaphysics. To the contrary, mixed with ST it even seems to facilitate the rise of metaphors at the expense of scientific autonomy.

7. **Ajax Minor**
   October 28, 2006

   This recent paper by Anthony Zee and collaborators might be appropriate to point out in this thread (or maybe in the one about ST phenomenology),

   hep-th/0610231
   Does string theory predict an open universe?
   Authors: R. Buniy, S. Hsu, A. Zee
   It has been claimed that the string landscape predicts an open universe, with negative curvature. The prediction is a consequence of a large number of metastable string vacua, and the properties of the Coleman–De Luccia instanton which describes vacuum tunneling. We examine the robustness of this claim, which is of particular importance since it seems to be string theory’s sole claim to falsifiability. We find that, due to subleading tunneling processes, the prediction is sensitive to unknown properties of the landscape. Under plausible assumptions, universes like ours are as likely to be closed as open.

8. **Juan R.**
   October 29, 2006

   Anon,

   Your collection of reactions is very interesting and highlights one of the true problem of string theory; i mean social issues and misinformation of people and noticeable consequences such as lack of funding of promising (but not so popular) approaches and others.

   I think that we can offer a reasonable reply to each one of reactions you listed. Peter woit, maybe this could be interesting for the FAQ.

   ***If it’s so bad, why is it so popular?**

   Popularity is not synonim for scientific trust. Electromechanical classical models of atom were very popular, but all popular models failed and just a single revolutionary model fitted exp. data.

   ***If all these uber-smart people and prize-winners do it, things can’t be as you say**

   Smartness and prizes are not proofs for scientific statements. The history of science is full of Nobel winners and other smart guys were wrong regarding some scientific statement. There is also well documented cases of entire comunities of brilliant scientists promoting a wrong vision of the world. The next paradigm is then labelled like scientific revolution.

   ***But I read an article in a top notch newspaper/magazine where it said string theory explains almost everything**

   The same that above. People also read in top magazines about Pons’ cold fusion but it was a sound scientific fiasco.

The Proof is in the Blogging

October 29, 2006
Categories: Uncategorized

Seed has a new article out by Stephen Ornes, called The Proof is in the Blogging, about the way the story of Penny Smith's solution to the Navier-Stokes problem played out here. I'm quite fond of the photo included in the Seed article.

I'm not sure there's anything more to be said about the Navier-Stokes story. One of my colleagues pointed out that mathematics is one of very few subjects in which bringing together a bunch of people with opposite views on what is true generally leads to one or more of them agreeing that they were wrong.

There's also a short article about this on Slashdot. Taking advantage of the arXiv trackback mechanism, the author found the discussion of this on Lubos's blog. I was going to take the opportunity to complain about the arXiv censoring links to this blog, but it turns out in this case there is one there. The ways of the arXiv are endlessly mysterious, I have no idea what their trackback policy is these days.

Maybe it's also relevant to mention that for some reason the hot news retailed here about the proof of finite generation of the canonical ring is not attracting the kind of attention indicated in the Seed picture.

Comments

1. **Anon**
   October 30, 2006

   Perhaps it's only hep-th which doesn't give you trackbacks?

2. **Chris Oakley**
   October 30, 2006

   It is interesting that the recent Navier-Stokes controversy was played out here rather than sci.physics.research or sci.math.research, where it more naturally belonged. I think that this is partly because the moderation takes too long on these newsgroups (some of my posts on SPR have taken days). If they adopted Peter's policy of allowing posts to appear before deleting them rather than just preventing what they see as irrelevant/obnoxious/uninformed posts at the outset, the whole thing might work better. Also, the collective quality control exercised by the moderators seems to be less effective than Peter's personal control. A bit like Communism vs. Capitalism.

3. **TheGraduate**
   October 30, 2006

   I didn't have much to say during the Navier-Stokes case as I couldn't think of anything helpful to say. I'm sorry about the way it played out though. My thinking is it wasn't as bad as it seemed to the people most emotionally involved in it, but I guess it must have felt that way.

   Discussion boards take a lot of getting used to. People probably take them more seriously than they should. (Some grow out of this and some do not.)

4. **ObsessiveMathsFreak**
   October 30, 2006

   I honestly don't see what all the fuss is about. The system worked *exactly* as it was intended. A preprint was posted on the arXiv, people viewed it, discussed it, found problems, and gave the author quick and concise feedback. The preprint was withdrawn, and the author can begin working on any problems found.

   The system works.

   Compare the quick turnaround to the tradiational publishing ringmarole. It could have taken Smith over 18 months to find out her paper contained a flaw. The flaw may never have been found at all. And if the paper, or the next version, had no flaws, the journal may have had problems publishing it anyway.

   The only problem in this paticular case is that Nature rushed the story to print. If some less than professional scientific publications choose to base their stories on *preprints*, that could deter people from posting preprints online so as to avoid any publicity and potential embarrassment, and that would be a big setback for the very good thing arXiv has got going.(Aside from unrelated censorship issues)

5. **Deane**
   October 30, 2006

   The system, in the sense of arxiv.org, works. I don't think any of the fuss is about that.

   What stunned me was seeing how too many bloggers, commenters on blogs, and "official" media such as Nature jumped on the bandwagon and touted Penny's work well before anybody with serious credentials had a chance to do a serious assessment of her claim. People who appear to have no expertise in the subject and have no way of understanding what was in Penny's paper posted their own personal (always positive) evaluations about Penny's work.

   You might ask what's the big deal, but some of the people who did this promote themselves as people who know math and how to judge what good math is. So their misleading statements create a higher level of FUD than what we sheltered pure mathematicians are used to.

   All in all, I think as math gets more publicity and attention, mathematicians like me are going to have to get used to the extra noise. Overall, I'd rather have the attention. But I think a lot of my colleagues feel differently.

6. **MathPhys**
   October 30, 2006

   Yesterday, there was a retraction of a paper on math.ph from 2004 on smooth solutions of 3D Navier Stokes.

7. **MathPhys**
   October 30, 2006

   PS That photo is great. You should have it framed 🙂

8. **anonymous**
October 30, 2006

This was an extraordinary case. What I find annoying are the many small mistakes present on arXiv. They get corrected in final published versions, but i) the most read version is the first one; ii) too many authors think that fixing errors in revised version without indicating them in the "comments" line is a good idea.

Please, instead of just writing "Comments: 26 pages, 7 figures, LaTeX" add "sorry, when we posted version 1 we were drunk, now version 2 is ok" and I will trust more your future works.

9. **anthropologist**
November 4, 2006

Now that I have read through all the comments on NS it does appear that Penny overhyped things quite a bit. And while the system of peer review did work as intended, the negativity she got is purely of her own making.

Again, Is N=8 Supergravity Finite?

October 29, 2006
Categories: Uncategorized

The main argument generally given for working on string theory is that it's the only way to get a finite theory of quantum gravity. One often hears claims that gravity can't be quantized using QFT, that string theory is needed to "smooth out the violent space-time fluctuations at the Planck scale", or some such explanation for the inherent non-renormalizability of quantum field theories of gravity. From the earliest days of their study, it was hoped that supergravity theories would have better renormalizability properties, with the maximally extended supergravity, N=8 supergravity, the most likely to be well-behaved.

For years the general belief has been that N=8 supergravity is non-renormalizable, based on the existence of possible counterterms at high enough order. The problem has always been that calculating the coefficients of these counterterms is too difficult, so one cannot be sure that one would not get zero if one actually did the calculation. Last year I wrote here about a talk by Zvi Bern in which he mentioned that twistor space methods for doing these kind of calculations were giving indications that these coefficients might be zero. Tonight there's a new paper out by Green, Russo and Vanhove suggesting the same thing. Their arguments involve M-theory and consistency conditions relating supergravity and the low energy limit of 10-d superstring theory.

It would be quite remarkable if it turns out that this work by Michael Green, using string theory and M-theory techniques, ends up shooting down the main argument for why one has to abandon QFT if one wants to do quantum gravity.

Update: Next month at UCLA there will be an entire workshop devoted to this question, entitled Is N=8 Supergravity Finite?

Comments

1. **anon.**
   October 29, 2006

   "It would be quite remarkable if it turns out that this work by Michael Green, using string theory and M-theory techniques, ends up shooting down the main argument for why one has to abandon QFT if one wants to do quantum gravity."

   I don't see that they would shoot down the argument; one has to be able to break SUSY without reintroducing nonrenormalizability. String theory allows one to have reasonable, realistic soft SUSY breaking. I'm not sure that N=8 SUGRA can do that.

   (Not that the question isn't interesting, and I would be happy to be corrected with pointers to references!)

2. **arnold**
   October 29, 2006

   Hi,
   a "naive" question. Why is it so important that there are no divergencies in a quantum theory of gravity?

3. **anon.**
   October 29, 2006

   arnold,

   That isn't precisely what's important — the technical property is "renormalizability", which means essentially that the theory is completely specified by a finite number of constants. Certain divergences are OK in a renormalizable theory, but in gravity they are usually too severe. One would need infinitely many parameters to quantize general relativity naively, and hence one would not really have a theory. You would always know at most finitely many parameters, and thus could never actually calculate anything.

4. **A.J.**
   October 29, 2006

   Arnold,

   One other reason that anon. didn't mention: The sort of divergences that appear when you try to quantize classical GR and supergravity have appeared elsewhere, e.g. in Fermi's theory of the weak interaction. (Although gravity is a considerably more broken theory than Fermi's ever was.) They usually indicate that the degrees of freedom we've chosen — in this case, gravitons — might not be valid at arbitrarily high energies.

5. **Kea**
   October 30, 2006

   I don't see much evidence of them taking the MHV twistor techniques seriously, so this paper can't have much to do with that excellent talk by Zvi Bern.

6. **MathPhys**
   October 30, 2006

   Why can you softly break SUSY in a string theory, but not in a finite supergravity theory?

7. **arnold**
   October 30, 2006

   thanks anon and a.j.

   Let me try to explain again my doubt : in any theory one has all possible operators allowed by symmetries, whose coefficients have to be fixed at some energy scale by experiments.

   Now, why does it matter if this coefficients are divergent or not?

8. **Gordon**
   October 30, 2006

I have about 8 papers on derivative corrections in IIB superstring theory and N=8. Including some matlab code that computes the coefficients. You might find this one interesting:

On the finiteness of N=8 quantum supergravity.
Gordon Chalmers (Argonne) . ANL-HEP-PR-00-83, Aug 2000. 11pp.
e-Print Archive: hep-th/0008162

9. **Gordon**
   November 2, 2006

   By the way I have completed software in matlab that allows for loop calculations of amplitudes. I dont plan on publishing all of them for a while. Available on request for various theories.

10. **Gordon**
    November 29, 2006

    Recent article by Green, Russo, and Van Hove:

    hep-th/0611273 [abs, ps, pdf, other] :
    Title: Ultraviolet properties of Maximal Supergravity
    Authors: Michael B. Green (Cambridge U., DAMTP), Jorge G. Russo (ICREA, Barcelona & Barcelona U., ECM), Pierre Vanhove (Saclay, SPhT)
    Comments: 10 pages

    (reproduces my earlier arguments)

11. **Johny**
    November 30, 2006

    Gordon: *(reproduces my earlier arguments)*

    Yet, they don't seem to cite you.

12. **Gordon**
    November 30, 2006

    Well, Johny, it only took about 6 years and then sum.

    Lets cite it then. I do.

Some Early Criticism of String Theory

October 30, 2006
Categories: Uncategorized

From the “First Superstring Revolution” on, there have always been skeptics, even though they often were not very vocal. Perhaps the most well-known piece of such criticism was Paul Ginsparg and Sheldon Glashow’s [Desperately Seeking Superstrings], which appeared in the May 1986 issue of Physics Today. I recently became aware of some other similarly critical articles by Noboru Nakanishi, and copies of them have been made available to me. They are:

[Comments on the Superstring Syndrome] (also from May 1986)

[“Superstring Theory” Syndrome] (published in the popular magazine “Parity”, September 1986)

[Can the superstring theory become physics?] (January 1993)

This last paper claims that “the bubble of superstring theory has … bursted”, which, in 1993, was rather premature.

Comments

1. **D R Lunsford**
   October 30, 2006

   Nakanishi makes an extraordinarily accurate point!

   “I believe that any correct quantum theory should be able to be formulated in the Heisenberg picture. As long as one does not give up the idea of adding the interaction part to the free-field theory, one will never attain the fundamental theory.”

   There is so much truth in these two sentences!

   -drl

2. **Christine Dantas**
   October 30, 2006

   G. Borner, in his book “The Early Universe – Facts and Fiction” (1988), also cites Ginsparg and Glashow on page 223:

   Let us hope that from its phase of floating around in the imagination, superstring theory will be able to find some connection with reality. Let us wait and see whether it succeeds or whether it ends up as the kind of thing that Pauli would have characterized as being “not even wrong”.

   Christine

3. **Conrad**
   October 30, 2006

   Speaking as an interested layman with a degree in philosophy, I have never understood how it came to be that superstring theory became so entrenched as THE theory. I always knew that the theory made no testable predictions. I always suspected it to be something like Alfred North Whitehead’s theory of gravity(anyone out there there ever hear of this- but it was once a big deal)- an elegant theory written by a brilliant mathematician that ultimately goes no where.

4. **Chris Oakley**
   October 30, 2006

   DRL – agreed. Also known as “Haag’s theorem”. In between progamming bouts for the client, this is what I am looking at now.

5. **Kea**
   October 30, 2006

   This last paper claims that “the bubble of superstring theory has … bursted”, which, in 1993, was rather premature.

   Does that mean you think it has burst *now*?

6. **Peter Woit**
   October 30, 2006

   Kea,

   I don’t know. There’s certainly a lot of noise, but is it the noise of a bubble bursting or something else? Time will tell…

7. **hack**
   October 30, 2006

   There’s a saying that goes something like “Bubbles always last longer than you think is possible, even after taking into acount for the fact that they always last longer than you think is possible”.

8. **ralf**
   October 30, 2006

   Couldn’t one say that superstring theory has never been a part of physics as a (once) serious science, but has rather always been a phenomenon of the sociology of physics and the politics of academia and science funding? The exact moment of the bursting of its “bubble” may have been/be (hopefully) different for different people. For me there never was a “bubble” in the first place.
   I think nobody who even considers the mere possibility that the entire physical reality is a consequence of the super-Virasoro algebra should be considered a serious physicist.

9. **King Ray**
October 30, 2006

String theory is a compendium of failed ideas… Kaluza-Klein, string theory of hadrons, supersymmetry, supergravity, etc. Why would you expect to get the correct theory by combining so many failed theories? I never found any of those ideas aesthetically pleasing anyway, and all mixed together they are even less so.

10. **Thomas Love**
October 30, 2006

Are we now replacing string theory with bubble theory?

11. **Kea**
October 30, 2006

Are we now replacing string theory with bubble theory?

Exactly! By showing that string theory is merely a moduli space technique for doing rigorous QFT one removes the necessity of landscapes, higher dimensions, KK and all that.

12. **JC**
October 30, 2006

I vaguely remember the early 1990's being a slump for string theory. Besides the people who were working on mirror symmetry stuff, it looked like a number of the hardcore string people were banging their heads on the heterotic string (attempting phenomenology) without much success.

At the time I wondered whether the string bubble had actually bursted. I never thought that string people would become interested in 11d supergravity or supermembrane theory again. (In the mid-late 1980's, some string folks thought supergravity was a dead end).

13. **D R Lunsford**
October 30, 2006

King Ray – agreed! It always struck me as curious that something that appeared so physically ad-hoc could generate so much interest. My conclusion was that the interest was not coming primarily from physicists, rather mathematicians. I've always thought that the popularity of string theory may have derived simply from creating a large number of mathematicians during the Cold War, in a world short on things remaining to be proved – and so they fell on string theory because it was a fertile playground for mathematical speculation.

-drl

14. **dark-matter**
October 30, 2006

Amazing predictions from Ginsparg/Glashow and Nakanishi, 20 years ago.

Conrad:
For a philosophical essay see: http://www.arxiv.org/abs/physics/0610168

ST will proceed by sheer momentum and the power of denial. Maybe there are Miracles. LQG, having successfully dealt with quantization of GR (see http://www.arxiv.org/abs/hep-th/0605052) is now engaged in developing couplings to matter fields and taking a crack at unification. It may or may not produce something in the next few years. The best guide to developing a replacement for ST is the LHC, gravitational waves experiments, and satellites/telescopes. In another words, let the experimentalists take the baton and bring in some much-needed facts.

15. **arnold**
October 30, 2006

The article by Glashow and Ginsparg is really good.
Thanks Peter.

I always wondered how a theory which makes no conceptual breakthrough (it is just a naive hypothesis that matter is made out of strings) can have anything to say about the world 16 orders of magnitude smaller than what we know!

If you think about it, special relativity was a conceptual earthquake, as general relativity, and quantum mechanics.

String theory is just some naive generalization of Feynman diagrams to extended objects. And in order to even hope to make sense it requires so many adjustments (supersymmetry, more dimensions, some funny
compactifications to cure problems that were created by the theory itself,…) that it becomes almost ridiculous.

16. **King Ray**
October 30, 2006

DRL,

Someone once told me to beware of people that mathematicians think of as physicists and physicists think of as mathematicians.

I think also a lot of people have invested a lot of their careers in these failed ideas and want to see them survive in some way, so they have jumped on the ST bandwagon.

The string theorists just lack the nose for beauty and truth that Einstein had, that tells one if one is on the right track or not. Going from the SM to ST is like trying to deduce the shape of the whole iceberg from the part above water; it is extrapolation, and that rarely works well. Maxwell only added one term to one equation when he unified E&M, he didn't add hundreds of more forces. Likewise with EW theory.

17. **The Anti-Lubos**
October 30, 2006

Conrad: *Speaking as an interested layman with a degree in philosophy, I have never understood how it came to be that superstring theory became so entrenched as THE theory.*

Read Peter's book. Seriously!

18. **Tony Smith**
October 31, 2006

Noburu Nakanishi is professor emeritus at Kyoto University and co-author (with Izumi Ojima) of a well-known book, "Covariant Operator Formalism of Gauge Theories and Quantum Gravity" {World Scientific, 1990).

His most recent paper is hep-th/0610090 "Spacetime in the Ultimate Theory" in which he is critical of both non-commutative geometry and superstring physics models, saying:

"… Recently, the quantized spacetime has been revived owing to the fashion of the non-commutative geometry. But the quantized spacetime seems to be investigated in favor of the mathematical interest rather than the physical requirement. … Hence the calculations concerning it often become no more than mathematical exercises. Of course, one may claim that the quantized spacetime is an approximation of string theory, but such an assertion implies that the quantized-spacetime theory itself is not a candidate of the ultimate theory.
…
Some of the superstring people believe that it is fruitless to consider any other theory because the superstring theory is the unique candidate of the ultimate theory. I believe, however, that such an assertion is too much prejudiced, because there are several fundamental difficulties in the superstring theory and nothing of them have yet been resolved … there are absolutely neither theoretical nor experimental evidences which justify the huge extension of the theoretical framework done in the superstring theory.
…
utterly no superpartner-like particles predicted by SUSY are discovered. It is quite unnatural
to assume that all superpartners have a mass so large that no present-day accelerators can
produce them without exception … If NATURE does not adopt SUSY, supergravity and superstring must be abandoned, and many people researching them will become greatly embarrassed. …".

His paper is NOT merely criticism of non-commutative geometry and superstring models,
since
in it he propose a concrete model for spacetime (although not, in that paper, extended to describe all the particles and fields of the Standard Model). You should read the paper hep-th/0610090 and the book mentioned above for details, but here are a few excerpts that might give an indication of his ideas:

"… The quantum theory of gravity thus constructed turns out to have a 16-dimensional supersymmetry … based on the "16-dimensional supercoordinates" x^mu, b_nu , c^r, c'_p , where c^r and c'_p denote the Faddeev-Popov ghost and anti-ghost, respectively
…
More precisely, its superalgebra is the (8+8)-dimensional inhomogeneous orthosymplectic superalgebra consisting of 144 generators. Of course, the affine algebra is its subalgebra
…
In the framework of indefinite-metric quantum field theory … many unbroken symmetries are available without predicting the existence of extra physical particles, so that one can construct the action having a large (super)symmetry which may unify the spacetime symmetry and internal symmetries without contradicting the no-go theorem for the extension of the Poincare symmetry …
…
both the physical spacetime and the Lorentz symmetry of particle physics are the
secondary concepts appearing as a consequence of the spontaneous breakdown of symmetries … the Nambu-Goldstone boson corresponding to the symmetric-part generators of general linear transformations is nothing but the graviton. This fact guarantees the exact masslessness of the graviton. …".

The final sentence of his abstract in hep-th/0610090

"Any criticism on my opinion is welcome."

is a welcome contrast to some of the attitudes seen in the USA high energy theoretical physics community.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

19. **Jeff**
October 31, 2006

Peter,

Nice links. 🙂

String theory – for all its grandeur – is still a work in progress, even though you validly claim that 30+ years is a long time for a theory to be in development. Ofcourse, maybe the unspoken understanding may be that so long as Edward Witten thinks strings are a viable approach to QG and unification, the program will continue on unhindered.

But my post is not on strings per se; I wanted your view on what 'twistor theory' has to offer for QG (or even unification). Roger Penrose writes in his latest work (rather convincingly) that twistor theory has deep relations to physics that has not been fully explored as yet (due in large measure to string theory's dominance of hep-th research). Do you think twistor theory might have good potential in the future?

Regards,
Jeff

20. **1995**
October 31, 2006

Things started going wrong in 1995.

Around 1985, string theory seemed to be what we need: a theory of quantum gravity able of predicting its low energy QFT limit. Strings correctly become the mainstream.

But in 1995 people working on string physics had achieved nothing, while people working on string theory gave interesting results, showing that the theory was more rich than initially envisaged. This complication was a bad news, but many people were happy. Indeed, at this point the community was large enough to allow a self-referential study of the mathematical aspects of a theory that was loosing the initial physical motivation that turned it into mainstream.

String theorists now understood that they must start delivering truly good physics. Either this happens within a few years, or will not happen.

21. **D R Lunsford**
October 31, 2006

King Ray – Smolin says that ST is probably a consequence of the Feynman/Dyson "hard-nosed" approach to physics, where calculational virtuosity is the main skill, and contemplation of fundamental issues is secondary. Naturally, physical mathematicians are going to have a bigger role in such an enterprise than mathematical physicists. And I agree on your comments RE beauty – that is, beauty is a good guide, but you have to be able to see it first!

-drl

22. **Bert Schroer**
October 31, 2006

D R Lunsford
I don't think it is quite that simple because for a long time (up to the early 80s) ST did not have a mathematical attraction. It was Witten who showed that it can be used as a vehicle which carries metaphoric physical ideas into interesting mathematical conjectures.
Physically it had no historical connection; its birth was a shot from the hip, a strange combination of the most crude phenomenology (of saturating the intermediate state completeness property by an infinite tower of intermediate one-particle states) with Beta-function engineering. It unfortunately happened at a time when the success hubris from the standard model (see Phil Anderson's criticism) was in ascend and the conceptual control of of speculative proposals out of the blue were on the decrease. Only in this way one can understand that the result of a prescription was called S-matrix without ever checking whether it even satisfies the most crude properties (e.g. multi-particle clustering) without which that physical name is completely undeserved.
Feynman's and Schwinger's computational power was conceptually controlled and not rampart.

23. **Alejandro Rivero**
October 31, 2006

Amusing coincidence that the criticism of Ginsparg/Glashow were published in the "Reference Frame". I guess that this article substantiated some of the gossip about why hep@xxx was divided in hep-th and hep-ph (yep, and hep-lat, but that can be more obvious). And actually it is the first physics/… paper, is it?

24. **anonymous**
October 31, 2006

a posteriori one should now admit that crackpot Noboru, crackpot Paul and crackpot Sheldon correctly guessed many of the present problems. Still, I think that their criticism was too early and that strings had been a very good attempt, altough unfortunately the criticism of crackpot Peter and crackpot Lee might be close to the end of the story.

25. **D R Lunsford**
October 31, 2006

anonymous – I don't see how you can maintain this position in the light of Nakashini's extremely pointed criticisms. His essay is wonderfully to the point and succinct and hits the main issues of *principle*, without even going much into the technical details that Peter stresses. The paper linked by Tony Smith is also very much to the point and filled with physical insight. His insistence on having a Heisenberg representation for any final quantum theory is the best point in physics I've seen made in public for a long time. It should be pointed out that quantum theory was born this way. I think students should get a history course taught out of "Sources of Quantum Mechanics" by van der Waerden.

-drl

26. **Chris Oakley**
October 31, 2006

Anonymous,

I am surprised that you failed to mention that crackpot Popper, who required that physics should be based on mathematical models that are open to being proved wrong by experiment.

27. **Chris Oakley**
October 31, 2006

Oh, and another thing, in case non-native English speakers are getting confused: "burst" is the same in the past tense, and is therefore not a regular verb.

28. **D R Lunsford**
October 31, 2006

Chris – it is too bad the crackpot Dirac died in the same year as the Green-Schwarz coup. I wonder what lame comments he would have made?

-r

29. **Eamon**
October 31, 2006

I don't think that the bubble, if you can indeed call it that, will burst. I mean – how would such a thing play out? Too much reputation, energy, and money has gone into this.

What these older articles prove is that the shortcomings of string theory were already observed and noted long ago. They were ignored then, and will probably be ignored now. It even *took off* without a good connection to experiment. Why would that hurt it now?

30. **Chris Oakley**
October 31, 2006

Here is an exercise for the reader. Subtitute one the following for "the bubble" in Eamon's first sentence:

(i) Enron
(ii) LTCM

(iii) Ptolemy's epicycles
(iv) Alchemy
(v) Communism
(vi) Star Wars SDI
(v) The Hindenberg
(vi) Sony Betamax

31. **Chris Oakley**
October 31, 2006

… and finally

(xlci) my ability to count in roman numerals

32. **Bert Schroer**
October 31, 2006

Eamon
I agree with your pessimistic assessment.
Unless the most charismatic updater of ST returns to particle physics, i.e. to that what he was doing before he met Atiyah, and hence through this very act renounces ST (which we all agree is not particle physics) there will be no change. However I doubt very much that ST will survive the passing of its protagonists and Nobel supporters. As a result of its predictive vagueness it will most probably survive LHC, especially if new data resist satisfactory theoretical interpretation for a longer period of time. According to Phil Anderson one should expect that the duration of ST is direct proportional to the hubris in the aftermath of the unmerited success of the SM, but it is difficult to quantify hubris apart from registering (looking at its aggressive supporters) that it is still going pretty strong.

33. **Ari Heikkinen**
October 31, 2006

Yes, exactly, there's always been critics of string theory since its very beginning, yet many researchers have found it interesting enough to continue working on it over the years.

As far as I can recall, initially string theorists were called crackpots by physicists, not vice versa.

I still don't quite understand what's the big deal about the critism now. There's, for instance, Glashow's critism of string theory even in popular books about string theory like Greene's that's been there long before Lee or Peter wrote their books.

My point is, the critism is nothing new.

34. **Bert Schroer**
October 31, 2006

Can anybody explain why the title "crackpot" is so popular around Harvard? In an earlier reaction of Peter to anonymous he suggested that there may be something in the water around Harvard.

35. **egbert**
October 31, 2006

It's not just Harvard. It seems to have become a fundamental part of the string culture that, since nobody really understands what's going on, reputation is everything. When a string theorist meets some physicist, he rushes to judge that person's intelligence, and if that person says things that coincide with what the string theorist has heard from people he respects, then the person is intelligent and if he says the opposite he's a crackpot.

In the KITP video where the journalist in residence talks about Lee's and Peter's books, somebody in the audience proposes "taking a vote" about whether Lee Smolin is a crackpot. The vote didn't go ahead, perhaps because they knew they were being recorded and broadcast on the internet. But from the general hostility the audience was showing (even though they hadn't read the books), I think there can be little doubt about what the result of a vote would have been. The grad students learning string theory at UCSB will develop their opinions of who is an actual scientist and who is just some crazy crackpot in this environment.

Susskind does something similar in his review of the books. He tells stories about shipbuilders and explorers but in the end all he does is attack reputations.

36. **Bert Schroer**
October 31, 2006

Ari Heikkinen
I think there is a big difference if some individuals were called crackpots by the particle physics mainstream (I don't recall that this expression was in use in those days, probably the majority used to think that this was "crazy stuff" or something similar) or if string theorists think that everybody else is a crackpot. The last situation reminds one of a group of ghost drivers who drive down the highway in the wrong direction and shout at everybody who is driving the right way because they are not yet aware of their mistake.
And by the way, this is one of the few instances where the majority was right.

37. **Energex42**
October 31, 2006

In my mind, this round of String Theory criticism is more credible. LQG is making rapid progress, and creating a set of predictions. String Theory still have nothing to say about predictions.

Ironically, String Theory may be saved one day because LQG manages to see strings in an effective field theory.

38. **Bert Schroer**
October 31, 2006

No, please, it is not very effective.

39. **steve**
October 31, 2006

As an interested outsider (not a natural scientist), it has fascinated me how strong the social policing within physics (and to an extent astronomy and cosmology) is about crackpots. The structural reason for this is obvious–there are a lot of people who don't know what they are talking about who want to participate in the conversation and/or fantasize themselves as the new Galileo. In order for the conservation to be useful, the idiotic stuff has to be filtered out.

But the policing of crackpotism seems to me to go well beyond that. Even well-credentialed people who go off the reservation get cast into the outer darkness. It looks like most fields of science have foundational weak spots that everyone agrees to leave alone while working on superstrucural puzzles (if you'll pardon the metaphor). Everyone in the field has some existential worry that those weak spots will fail and bring down the whole structure, and they don't like people reminding them and pushing these issues to the foreground. So they declare anyone who worries about these matters to be a crackpot as a way of eliminating the cause of their anxiety. And by "shunning" these critics (in much the same way closed religious communities like some Mennonite sects do) they can exorcise the threat to communal well-being. Out of sight, out of mind. The shunned are rendered powerless to influence anything during their lifetimes.

Objectively, the shunners may well be correct in their scientific beliefs more often than not. But given a) the importance of the results if the "heretics" are in fact correct, b) the stimulus to mainstream theory created by the heretics, and c) the self-censorship that must cripple creativity when people want to avoid being shunned, I think a bit more tolerance for others' crazy ideas would be good right now. Personally, if the core theory in my field had problems like getting the vacuum energy wrong by 120 orders of magnitude, I'd be open to creative suggestions, even if they seemed a little bit cracked.

40. **TheGraduate**
October 31, 2006

steve:

I like what you said. Of course, what you say is just a general scenario that may or may not apply in individual circumstances. But I think it is a scenario that does seem to play out often.

However, I am surprised that so few physicists are willing say, "Yes there is no absolute proof of what I believe but I'm going to work on it anyway and since I believe it's the best idea, I am going to tell everybody else to work on it too."

They seem to prefer quarreling over the subjective value of the inconclusive evidence.

41. **Chris W.**
November 1, 2006

[*My apologies; it looks like I neglected to properly close an <a> element in the previous comment. Let's try again…*]

Steve,
I think one can interpret the tendency to label people crackpots in economic terms, especially these days. It *costs* something to sympathetically and carefully consider someone else's ideas, including time taken away from pursuing whatever research agenda one happens to be interested in. There is a strong incentive to latch onto reasons for quickly dismissing an idea, if only to save time for investigating other options, any of which may demand considerable effort. Like most successful and ambitious professionals, scientists hate to waste time, and look askance at (erstwhile?) colleagues who appear to be investing their time carelessly.
In contrast, Michael Nielsen quotes Freeman Dyson, speaking of J. Robert Oppenheimer:

> … we can see the nature of the flaw which made his life ultimately tragic. His flaw was restlessness, an inborn inability to be idle. Intervals of idleness are probably essential to creative work on the highest level. Shakespeare, we are told, was habitually idle between plays. Oppenheimer was hardly ever idle.

In this context, one should recall Aaron Bergman's repeated references to (and laments on) the relentless pressure to produce substantive research output, especially for younger scientists, and the general antipathy that most working scientists have to philosophical discussion, which they regard as mostly fruitless, if not altogether sterile and pedantic.
What I think many people are looking for at this stage in quantum gravity is a galvinizing idea—something that is both original and clearly relevant, ie, appears to be getting at the heart of the matter. (They also fear the paralyzing effect of knowing too much, ie, tending to immediately think of convincing reasons to suppose that any new idea couldn't possibly work.) Until such an idea appears there will be a lot of simmering frustration and some short tempers.

42. **Garbage**
November 1, 2006

Peter,

This is why this blog misses the oportunity to be a good place to discuss physics. You have an interesting post on finiteness of N=8 SUGRA, which if true would certainly produce a (though unrealistic) more *traditional* finite theory of gravity, that doesnt count more than 8 comments. In the other hand, this one on early critics of ST clicks 40 in a few days. It seems to me like you keep repeating yourself over and over, and even though I agree ST has grown perhaps way too far it should have ever been, and also particle physics needs to hit a *refresh*, I dont see the point of constantly hammering the same nail to get a swarm of mostly pointless comments. Do you really think that by doing that anything is going to change by doing that? I guess it is fair to say there is a lot a ST hype out there but jumping into the anti-hype wagon isnt going to help much.
I, as many physicists do , expect the LHC to clear the way for science soon.
And btw, the articles are fun reading 🙂

G

43. **Jeff**
November 1, 2006

Garbage:

You hit the nail! :-)) with your remark. The problem is that with excess time to spare, and not enough data to process, there really isn't much else to discuss except the latest gossip on our favorite blogs; everything else lies suspended in animation. Only when LHC gives experimental results will the field move forward.

As regarding string theory, I've heard rumors that even Ed Witten is beginning to have doubts (I can't exactly pinpoint where I heard this from; maybe it was from Peter himself – referring to conversations between L. Susskind and Witten).

Continuing, if you believe ST is a dead-end (or overhyped) then perhaps you may want to have a look through 'twistor theory'. Roger Penrose, in his new book 'The Road to Reality' quite elegantly argues that physicists have grossly overlooked [and sometimes undervalued] the twistor approach in favor of strings (citing similar reasons as others, i.e. Witten's overarching impact on current hep-th research, the sometimes unethical behavior of some string theorists when advocating the theory to lay audiences, string theory's lion share of research funding – frequently at the expense of other meaningful approaches, the media hype over string theory, etc.); he also adds that twistor theory has some fairly deep and robust properties – grounded primarily on physical! (NOT mathematical) considerations – to uncover new insights into fundamental

physics.
[Penrose, as many may know, is twistor theory's progenitor; however, given that he has spent more than 40+ years working on the topic, and given his calm and objective approach to writing on physics (as evidenced remarkably accurately in 'Road to Reality') I would not dismiss his claims as merely self-promotion; anyway, this *is* Roger Penrose we are talking about.]

I guess if you are a serious grad student (or young researcher) and want a clearer understanding of the current (theory) research landscape, Penrose's viewpoint may well not be an altogether bad start.

Regards,
Jeff

44. **Bert Schroer**
November 1, 2006

Garbage and Jeff
I agree with you that there is to much gossip and not enough scientific substance in weblogs including this one.
But I think that N=8 Supergravity is not a good illustration of this point. Why should a social construct like that be the subject of a meaningful discussion? Rumors about its possible renormalizability appear cyclically as reports on the sight of Nessy. Perhaps if somebody really succeeded to show nth order renormalizability it is worthwhile to talk about the physical significance of such an result but not in this stage.
The monomaniac preoccupation with quantum gravity siphons power away from post SM innovative research and at the end also harms progress about gravity.
If you look at my contributions of the recent weeks you will notice that I have been trying to get people interested in conceptual issues of actual research such as
Is KK consistent with QFT (and ST for that matter)
Why the CC computations of adding energies contradict local covariance
Why the alleged connection of holography with QG is misleading and how this led to wrong conjectures about AdS—CFT (a la Maldacena)
Why the objects of ST cannot be string-localized in any quantum-intrinsic sense
I admit that all these points require a certain amount of conceptual sophistication and mathematical knowledge. But is this the reason why nobody took them up or even asked meaningful questions?
I tried to adapt myself to the typical small talk on this blog with the subversive intention to lure people into more serious questions which could be the starting point of interesting discussions.
I do not see any point in repeating the lack of observational content of ST over and over again. Most participants and readers know this deadly flaw and the ST community has developed a thick skin.

45. **JC**
November 1, 2006

Bert,

Many folks I knew over the years felt that the biggest barrier to understanding algebraic QFT, even on a "qualitative" level, is the level of mathematics involved. Even with some knowledge of the mathematics involved, I've found that I still don't have a good understanding of it.

46. **Tony Smith**
November 1, 2006

Garbage said that Peter has "… an interesting post on finiteness of N=8 SUGRA, which if true would certainly produce a (though unrealistic) more *traditional* finite theory of gravity, that doesnt count more than 8 comments.
In the other hand, this one on early critics of ST clicks 40 in a few days … a swarm of mostly pointless comments. …
This is why this blog misses the oportunity to be a good place to discuss physics. …".

Garbage fails to note that the first comment on this thread was NOT "pointless" with resepect to substantive physics, but in fact dealt with Nakanishi's view of the fundamental status of the Heisenberg picture.
Also, other substantive physics points have been mentioned in this thread, such as:
Penrose's twistor theory;
Nakanishi's use of indefinite metric;
the consistency of Kaluza-Klein with QFT; and
soft predictions from spin networks of deformed special relativity, elementary particles as coherent excitations of quantum geometry, and disordered locality, all discussed in hep-th/0605052 which was mentioned in comment 14 by dark-matter.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – I note that Garbage him/herself has NOT submitted a comment to the thread on the finiteness of N=8 SUGRA, and HAS submitted a comment to this thread, and, further, posted the above-quoted comment as an AC.

47. **Bert Schroer**
November 1, 2006

JC
This is not the whole truth. The mathematical prerequisites are certainly not more demanding than those in string theory where these days people use category theory, gerbs, algebraic geometry, noncommutative geometry…..
I think the basic difference is that in ST you do not have to know their deeper mathematical content; the metaphoric physical nature of ST allows you to operate with a basically verbal knowledge and the rest can be done by improvisation and massage.
AQFT continued von Neumann's tradition on the mathematical side (but on a meanwhile infinitely refined level) and tries to implement the Jordan plea of disconnecting a more fundamental theory (QFT) from its classical bonds (the AQFT as conceived first by Haag). Its mathematics consists only of refinements of concepts which in their mode crude version you are certainly familiar with: bounded and unbounded operators, operator algebras and their spacetime encoding. It is the only setting of QFT in which there has been an independent development on par with mathematics on a very deep level of conceptual coalescence (the modular Tomita-Takesaki theory with an independent development from the center of QFT: the thermal theory of open systems by Haag Hugenholz and Winnink and the concept of modular localization which started with Bisognano and Wichmann). Using metaphors of physics for the advancement of mathematics or using existing mathematics to expand metaphors of physics is not the same thing.
The distance which people maintain from AQFT has little to do with its high demands on mathematics; it is more related to sociology and history. The same people who have to take responsibility for the post SM hubris are also those who consider it as a superfluous distraction from their grand design.

48. **Particle Physicist**
November 1, 2006

"The same people who have to take responsibility for the post SM hubris are also those who consider it as a superfluous distraction from their grand design."

It is also the people who developed the SM who consider AQFT as a superfluous distraction, and fortunately so. Otherwise, we wouldn't have QCD and a theory of electroweak interactions today but were still stuck in the 50's, roughly speaking. Or can you point to any insight into the SM that was provided by AQFT?

49. **Bert Schroer**
November 1, 2006

I was not talking about the ideas which led to those important discoveries, the hubris started afterwords. I never felt any antagonism before but rather a division of labor. There are those who go out and find our truffles and there are others who like to clarify and secure these findings in order to build a reasonable trustworthy basis for starting new discoveries and this has never had a stifling influence on the truffle seekers. The discovery of gauge theories was very important but the situation is a long way off from being in the possession of a trustworthy basis for the conquest of new phenomena (and I am reluctant to say this, in the present conceptual quicksand even the future results of LHC may not indicate the way out).
The point of view you are presenting is part of that post SM hubris. I suggest to you to read what Phil Anderson has said about this problem.

50. **Particle Physicist**
November 1, 2006

Let me repeat the question: what insights related to standard model physics, and beyond, would we miss if no one ever had dealt with AQFT?

51. **Bert Schroer**
November 1, 2006

What one is missing has more to do with how to get out of the present crisis which already lasts for more than 3 decades. There are indeed suggestions of how to get some conceptual progress on gauge theory in general and the Higgs issue in particular. Some ideas you find in the last section of my Samizdat essay. But the only presently already worked out innovations are in the area of QFT in CST and black hole issues in particular the entropy of the quantum state which causes the entropy associated with the Hawking radiation (also explained there).
But from the way you phrase your question it seems that you think of particle physics in terms of a sport match with winners and losers (this seems to be your attitude with respect to AQFT) or even worse in terms of a eliminationist anti-conceptual ideology. Perhaps if there would be more than a handful of researchers in algebraic QFT you would not still find yourself in that already 30 years discovered best quasiclassical straitjacket for the physical reality behind the SM (from which all the globalized communities of enormous size have not been able to get away).
It has been my experience in more than 40 years of professional activity that particle physics needs many different talents to make progress.

52. **Tony Smith**
November 1, 2006

Bert Schroer compared Superstring Theory with Algebraic Quantum Field Theory, saying:
"… in string theory … people use category theory, gerbs, algebraic geometry, noncommutative geometry…. I think the basic difference [from AQFT] is that in ST you do not have to know their deeper mathematical content; the metaphoric physical nature of ST allows you to operate with a basically verbal knowledge and the rest can be done by improvisation and massage …
AQFT continued von Neumann's tradition on the mathematical side … and tries to implement the Jordan plea of disconnecting a more fundamental theory (QFT) from its classical bonds … It is the only setting of QFT in which there has been an independent development on par with mathematics on a very deep level of conceptual coalescence … ".

With respect to AQFT and Standard Model model-builders, Bert Schroer went on to say that he " … felt … a division of labor.
There are those who go out and find our truffles … gauge theories ….
and
there are others who like to clarify and secure these findings in order to build a reasonable trustworthy basis for starting new discoveries …
The distance which people maintain from AQFT has little to do with its high demands on mathematics; it is more related to sociology and history …".

It seems to me that it would be a good thing to build bridges across the "division of labor" between AQFT approach and the "truffle hunter" model builder approach in which models are often built from gauge theory Lagrangians using path integral quantization.

In a comment to another thread on this blog, Bert Schroer said "… hyperfinite Type III_1 von
Neumann algebras … are the heart of local quantum physics …",
but
I don't see a road-map for exactly how the various parts of the von Neumann algebra correspond to the various parts of a gauge Lagrangian / path integral model.

Maybe a reason that ST people seem to have more political influence than AQFT people might be that the ST people (even if by "improvisation and massage") take pains to describe purported connections between their ST models and gauge Lagrangian / path integral models.

Without a road map to connect gauge Lagrangian / path integral models with the relevant von Neumann algebras, it is understandable why the model builders would feel distant from the AQFT people,
especially
if the main voice that the model builders hear from AQFT people is harsh criticism of basic model builder tools such as path integrals, as opposed to discussion of what kind of limiting processes etc might connect the two approaches.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

53. **woit**
November 1, 2006

I kind of agree with Garbage that I wish people would post more substantive comments, especially ones relevant to the more technical postings. I recognize that this may be too much to expect; the number of people out there really knowledgeable about some of these subjects is vanishingly small and undoubtedly they have better things to do than contribute to this blog.

But, please avoid posting repetitive comments that just add to the noise level. It's true that many of the postings here deal with the same issue, the current status and problems of string theory. This is largely because this is an active story and new material keeps coming in, but the same general comments about what's wrong with string theory are not enlightening. I'm already deleting quite a few of these, and will delete more in the future.

54. **Ari Heikkinen**
November 1, 2006

Peter,

It's interesting that you encourage people to post more technical comments instead of purely philosophical, yet the very first of the links to the papers you posted (Glashow et al.) about early criticism of string theory is purely philosophical without a single equation or proof. Yet they manage, in addition to the criticism of string theory in that very paper, call Einstein, probably the greatest thinker since Newton, incompetent (Quote: "He had to fail, simply because he didn't know enough physics.").

I'm sorry to say, but it seems more and more to me as if different standards are applied when evaluating the work of string theorists compared to evaluating work of non-string theorists.

55. **Peter Woit**
November 1, 2006

Ari,

My comment had absolutely nothing to do with string theory vs. non-string theory, other than noting that I'm already deleting a lot of repetitive comments people write in here criticizing string theory, and encouraging people to stop doing it. As far as comments here go, yes I do have a double standard: I virtually never delete anything people write in that is positive about string theory, and/or critical of my point of view on it, but often delete things that are negative about string theory or just endorsing my criticisms.

Also, when I link to papers, it is definitely not because I agree with everything in them. The ones mentioned here were mentioned for historical interest, just because I only very recently became aware of the Nakanishi ones.

56. **Bert Schroer**
November 1, 2006

Particle Physicist
I think that there has been a misunderstanding, my statement should have been:
…..who consider it as a superfluous distraction from their grand new designs"…."The word new was missing. If there was some hubris to their achievements to discover the SM this is human, understandable and did not cause any harm. I meant the hubris that one could get much more without any additional conceptual physical investment. I think it is clear by now that those ideas which were involved in the discovery of the SM and which people have used for 30 years to get beyond are sapped.
Tony Smith
All these things you mention have been discussed in previous blogs of mine. They have appeared in a specific context and taking them out of this context does not move me to explain things for the nth time again just because weblogs do not seem to have a memory for more than one week. I have got to know your point of view (basically use the old ideas perhaps with slight updating) and I am not trying to proselyte. There is a high entrance fee for AQFT and I am not offering a cheaper rate to anybody (and answering all your questions and statements for the nth time does not help anybody).
It is strange that you remind me of the only thing which I have in common with ST, namely the conviction that more of the QFT as it is in the books is not taking us out of our present misery.

57. **TheGraduate**
November 1, 2006

Peter,

It's unfair of you to blame other people when you're the one who hasn't been clear in a prominent place about what you want on your blog.

You said "But, please avoid posting repetitive comments that just add to the noise level."

What is 'noise' supposed to mean? If you have not defined it in an easy to find place on your blog, how are people supposed to know? And on the issue of repetitiveness … so agreeing with people is disallowed?

"As far as comments here go, yes I do have a double standard: I virtually never delete anything people write in that is positive about string theory, and/or critical of my point of view on it, but often delete things that are negative about string theory or just endorsing my criticisms."

This approach is very, very curious indeed. I am not how sensible it is to make insulting you the easiest way of making sure a comment is not deleted but to each his own.

58. **Peter Woit**
November 1, 2006

TheGraduate,

I probably should post somewhere some kind of "policy", but the problem is that it isn't so easy to precisely formulate such a thing. In practice the main problems have always been people who don't know what they're talking about, people who want to change the topic of discussion to their pet interests, and people who think it is a good idea to post comments that repeat others or their own, adding nothing new. The best way I know of characterizing what I'm trying to achieve is keeping the signal/noise ratio as high as possible. Sure, what I consider "noise" someone else may consider signal and vice-versa. But if you're writing a comment that is adding nothing new to the discussion that people are likely to find interesting, you shouldn't do it, it's that simple.

59. **TheGraduate**
November 1, 2006

Peter,

This may not seem plausible to you but asking others to live up to a standard which you refuse to articulate truly is the definition of 'unreasonable'.

60. **Arun**
November 1, 2006

Ari writes:

"Yet they manage, in addition to the criticism of string theory in that very paper, call Einstein, probably the greatest thinker since Newton, incompetent (Quote: "He had to fail, simply because he didn't know enough physics.")."

Ignorance is not the same as incompetence. Unless you mean to say that the discoveries since 1955 are irrelevant to producing a theory of everything (e.g, anomaly cancellation), Einstein certainly did not know enough physics. And it is very likely we still do not know enough physics to produce a theory of everything, despite all the ST claims to the otherwise.

Think about it – how would you express the fact that Einstein did not know about quarks and QCD, among a great many things? "He did not know enough physics" is certainly a legitimate expression of Einstein's ignorance.

61. **Peter Woit**
November 1, 2006

TheGraduate,

I just articulated it as best I can: don't post comments unless they're adding something new to the discussion that will be interesting to other readers (who you should assume are smart and well-informed…).

And the only reason I'm not deleting your last comment arguing with me about this is that you're insulting me by describing me as unreasonable…

62. **TheGraduate**
November 1, 2006

Peter,

Well I certainly did not get into posting on your blog to insult you or any one else. I only aim to accurately describe. I apologize. (You may delete this post after you have read it, as it will have served its purpose.) I will try my best in the future not to add to the noise. You may also delete the 'unreasonble' comment if you wish. Although, it may not seem like it, most of my comments are aimed at promoting better conversation.

Cheers.

63. **Tony Smith**
November 1, 2006

In my comment 52, I asked Bert Schroer "… exactly how the various parts of the von Neumann algebra correspond to the various parts of a gauge Lagrangian / path integral model. …".

Bert Schroer, in reply said "… All these things you mention have been discussed in previous blogs of mine. … I have got to know your point of view (basically use the old ideas perhaps with slight updating) …".

I agree that my point of view is to stay close to "the old ideas" of a gauge Lagrangian / path integral model,
but
I do not see where Bert Schroer has in "previous blogs" explained "exactly how the various parts of the von Neumann algebra correspond to the various parts of a gauge Lagrangian / path integral model.".

However,
I HAVE seen a dialogue in the thread in Peter's blog "Schoer's Samizdat" in which:

Arun in comment 106 said something substantially equivalent to my present question:
"… It seems to me [Arun] that AQFT practitioners find the Lagrangian path integral method of QFT to be unreasonably successful.
Well, if AQFT is indeed the way to do QFT,
then the success (and limitations) of the Lagrangian path integral should be explainable within AQFT.
Presumably the successful Lagrangian formulations are taking care automatically somehow the setting up of the unique hyperfinite type III_1 factor algebra, the modular positioning of a finite number of abstract monades, etc., etc. …"

and

Bert Schoer replied in comment 108:
"… "… arun,
this is all deja-vue, we (maybe without you) discussed this in this blog in April/May. … I [Bert Schroer ' will ignore this and answer it to the best of my [Bert Schroer's] knowledge
1) Funct5ional integrals are extremely limited …
2) you cannot solve any 2-dim. massive system, even in case it admits a Lagrangian presentation …
3) Most free fields cannot be characterized as being of Euler-Lagrange type (above spin 2 I [Bert Schroer] do not know any) but their use is perfectly legitimate in causal perturbation theory (where you only use interaction polynomials in free fields) although the most important ones can

4) The Lagrangian approach where it works in your sense is an artistic device:
you write something on paper in good faith, develop it in perturbation theory,
see that it does not make sense (the integrals diverge) and
use your hindsight and good physical sense to repair it (renormalization) and
at the end you realize that you have a perfectly reasonable result which however (if you try to check whether it its into your original functional representation) fails to satisfies your original functional representation. …
The artistic device of functional integrals (in QM it is mathematical) works because the whole idea of locality is so strongly incorporated that you cannot fail to extract the right result (if you have a good covariant formalism which was not available before 1949). …".

In my view, Bert Schroer's reply is only a negative attack on "the old ideas", and is NOT responsive to the issue raised then by Arun and now by me:

If the von Neumann algebras of AQFT describe physics as well as "the old ideas" of the Lagrangian / path integral Standard Model,
then
what are the exact correspondences between von Neumann algebra and the structures of "the old ideas" ?

Or,
does AQFT in its present state suffer from an ailment similar to that of the numerous vacua of superstring theory, that is:
are there a lot of von Neumann algebras and nobody in AQFT can identify a single one that corresponds to the Standard Model ?

From reading Bert Schroer's hep-th/0610225, it seems to me that the latter is the case, as he says there:
"… the physical reality of relativistic local quantum matter in Minkowski spacetime originates from the (modular) positioning of a finite number of copies of one abstract monade …
The rich content of a quantum field theoretical model including its physical interpretation (particles, spacetime and inner symmetries, scattering theory….) is solely encoded in the relative positions of a finite number of its monades …

The requirement is that the positioning should be natural within the logic of modular operator theory (using the concept of modular inclusions and modular intersections). For higher than 3 spacetime dimensions the presently known descriptions still look somewhat
concocted …
there are some still rather vague ideas of how the fact that the S-matrix has the interpretation
of a relative modular invariant … may be used in model constructions.
They are based on … the hope that its use (i.e. in a perturbative bootstrap-formfactor program) could have a chance to improve this situation. I [Bert Schroer] expect that by combining some ideas of the old S-matrix approach with this recent
framework of modular wedge localization one will obtain new insights into the
construction of QFT.
It is my [Bert Schroer's] intense impression that … it is not possible to make progress in
particle physics without a new problematization of the bootstrap-formfactor idea. …".

Bert Schoer's appeal to bootstrap ideas to try to construct models in more than 3 spacetime dimensions indicates to me that AQFT suffers from a problem similar to that of the many solutions of Superstring Theory:
a large and poorly described Landscape / Swamp of solutions,
with no known correspondence of any of them with the Standard Model.

In the absence of a concrete answer to the question raised by Arun in comment 106 of the "Schroer's Samizdat" thread and my question in comment 52 of this thread,
I will plod along using "the old ideas perhaps with slight updating".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

64. **A.J.**
November 1, 2006

Bert,

I think what Tony was suggesting is that it's time for someone to write up an "Introduction to Algebraic QFT for Particle Theorists", showing how the essential ideas of effective field theory (regularization, renormalization, anomalies, gauge symmetries, and perturbative expansions) are interpreted in the "local quantum physics" framework, in a language suitable for particle physicists. I don't mean that one shouldn't use Tomita-Takesaki theory, of course, but that one should treat it as something within the grasp of most particle physicists, rather than claiming that "there is a high entrance fee" to AQFT. The essentials of factors can be explained to anyone with a basic grasp of Hilbert spaces.

65. **steve**
November 1, 2006

Chris W: I like your formulation of the crackpot-policing issue in terms of time allocation. From an econ background, it makes a lot of sense.

If passive ignoring were the only behavior directed at well-credentialed heretics, though, it wouldn't be such a striking phenomenon. The aggression or venom involved in ridiculing, cutting off the funding of, and otherwise ostracizing the people who keep picking at anomalies almost seems to take as much time as would be required to address their concerns. I guess the number of active persecutors relative to passive ignorers is pretty low, though, so your theory still has a lot of power.

66. **Chris W.**
November 1, 2006

I, as many physicists do, expect the LHC to clear the way for science soon. (from Garbage)

To the extent that one wants the in question science to be about quantum gravity and unification, I wonder how much it will help. If the current theoretical situation allows for millions of potentially applicable models, and fundamental assumptions remain up in the air as one or the other of the models is refuted by the data, then we'll still be groping for a way to narrow the field on other than purely empirical grounds. (I'm assuming here that the individual models will be of limited interest by themselves.)

On the other hand, if one is mainly interested in taking the next small steps beyond Standard Model, with some interesting anomalies to chew on, then a return to doing science seems more likely…although that depends on what the LHC turns up.

67. **Bert Schroer**
November 2, 2006

Dear contributers and readers
When I actively participated in the weblog for the first time in April this year, the problem of alternatives to ST was an issue of considerable common interest. It was in that context that I started to mention the algebraic approach to QFT which most participants have only met (if at all) in the context of chiral theories or factorizing models (since in both cases there is no Lagrangian Lagrangian construction of such models) and whose contributions to 4-dim QFT are meanwhile very impressive and everytime mor constructive and concrete.
I tried to convey a bit about its content but as time went on it got a bit out of hand and I now realize that a weblog is not the right place. What caused me to write these lengthy contributions was the lack of a good reference; the book of Haag which was written in the late 80s as a nice motivating text (and it still continues to be) which however does not intend to connect readers to the technical level which would be necessary to reach a working knowledge (not to mention the impressive progress during the last 2 decades).
I think the lack of a textbook which includes in addition to a presentation of the concepts also sufficient technical-mathematical details makes any discussion on a weblog very difficult. Even though there are only very few people working with these methods, one would hope that this will change in the not so distant future. Since weblogs are not the right place I will stop right here admitting: mission not accomplished.

68. **Sailor Moon**
November 2, 2006

Ha! It looks like Ginsparg will let you make trackbacks to ~his~ papers!

69. **Arun**
November 2, 2006

While waiting for a up-to-date AQFT textbook, I'd just point out that the nature of successful scientific theories is that they explain the apparent success and limitations of previous scientific theories. Thus,in this sense, Special Relativity contains Newtonian mechanics, General Relativity contains Newtonian gravitation, and so on.

Anything that claims to supplant the Lagrangian/ path-integral/ perturbation-theory Standard Model of electro-weak & strong interactions, which is our most successful theory so far, needs to show how, in the appropriate limit, the Standard Model emerges.

70. **Tony Smith**
November 2, 2006

Arun said that if AQFT (or anything else) "… claims to supplant the Lagrangian/ path-integral/ perturbation-theory Standard Model of electro-weak & strong interactions, which is our most successful theory so far,
[then it] needs to show how, in the appropriate limit, the Standard Model emerges. …".

Alain Connes is certainly very knowledgeable about von Neumann algebras etc
and
he does take pains to connect his Non-Commutative Geometry (NCG) models to the Standard Model (although I don't think that he seems to be opposed to using Lagrangians etc in his model-building).

In his paper with Chameseddine and Maroclli at hep-th/0610241, Connes says
"… The input of the model is extremely simple. It consists of the choice of a finite dimensional algebra … a direct sum C + H + H + M3(C)
… geometric considerations on the form of a Dirac operator … lead to the identification of a subalgebra … of the form C + H + M3(C) …"
(where C = complex numbers, H = quaternions, and M3(C) = 3×3 matrix algebra).

Is there a way to show how that particular choice of algebra, in the context of that NCG model,
would correspond to
a particular "(modular) positioning of a finite number of copies of one abstract monade" from which, according to Bert Schroer's hep-th/0610225, "the physical reality of relativistic local quantum matter in Minkowski spacetime originates"
??

If there is, then it would help me to understand AQFT.

If not, then it might still be helpful to know why not,
and what are the difficulties in making such a correspondence.

If Bert can shed any light on that, I would appreciate him making at least one more appearance on this blog thread to do so.
In any event, I thank Bert for a lot of interesting comments, which I have enjoyed reading, although I regret that my understanding (although expanded from what it was before I read) is limited.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

71. **Marty Tysanner**
November 3, 2006

Bert,

You commented:
I think the lack of a textbook which includes in addition to a presentation of the concepts also sufficient technical-mathematical details makes any discussion on a weblog very difficult.

Given your wealth of knowledge and experience, have you considered writing something like a review article of the "state of the art" of AQFT, complete with sufficient mathematical content to offer a path for those who might consider it a serious subject for study? The advantage of an in-depth summary of this kind is that you would have complete editorial control over the content and style. Publishing costs are free, and so is the cost to the student…

This would be a significant undertaking, but it would be a potentially lasting contribution, and one which would have far greater potential to interest the wider community in the possibilities of AQFT than anything a weblog could offer. Plus, even if you didn't have time to finish the article you could post whatever you have, and that would still offer something of substance to a potential student of AQFT. Anyway, it's just a thought…

72. **Bert Schroer**
November 3, 2006

The situation in the absence of a good pedagogical text is not that desperate as it appears in my last blog. Different from ST talks and articles often start with simple historically well-known results and problems. I have never seen a paper or participated in a talk on AQFT which started with hyperfinite type III_1 factor algebras, but I already found myself lost in talks (when I missed an early chance to get away) which started with type II_1 superstrings (with most of the audience nodding their heads approvingly). So it is not surprising that there are some very good reviews of some central concepts in AQFT (modular localization, charge confinement/liberation, local covariance of QFT in CST,…), but reading them outside the reach of experts may be frustrating because there is nobody whom you can ask if you get stuck. Certainly a normal weblog like this is not the right place and it may be worthwhile to think about a better solution.
Concerning the question of writing a book about algebraic methods, I once started to write some notes but then concluded that a time of rapid changes is perhaps not the right one. I do intend to return to this project next year (after having cleared up some pending problems as well as my desk).
What has been a bit frustrating to me is that that people always try to construct an antagonism between the functional integral approach and the perturbation theory resulting from AQFT ideas. In my earlier contributions (April-June) I took great pains to clarify this issue of AQFT being inclusive; renormalized physical correlation functions just don't care about the (even mathematically illegitimate) way you obtain them because you can check their correctness independent of the sins you committed on the way to obtain them. The number of renormalizable Lagrangian field theories is quite limited, there is a finite number of abstract actions and the rest is obtained in terms of families by playing around with varying numbers of field components (the last classified family was the gauge theory family which was the only one which possesses the asymptotic freedom property). But the number of non-Lagrangian families is infinitely larger as the studied 2-dim. factorizing setting shows. In that case you classify your theories by its simplest algebraic structure (as in case of chiral theories). I don't see any sense in craving for Lagrangian in 4-dim. while living quite comfortably with algebraic classifications and constructions in lower dimensions.
Concerning contributions to particle physics of people with great mathematical rigor, insight and talents like Connes and Witten I prefer to be silent because anything critical I say, as objective as it may be, will be misconstrued as regicide (whereas in reality I have an immense admiration for their mathematical knowledge and skills). The furthest I went was in a weblog discussion in (I think) May at this weblog with an anonymous participant named "the great inquisitor". He understood precisely what I wanted to convey and did not misuse it, neither did anybody else (perhaps I have been just lucky).

73. **N. Nakanishi**
November 4, 2006

Looking at the discussions between Bert Schroer and the outsiders of Algebraic QFT, I would like to comment that there is a standpoint in between theirs. I emphasize that any of non-superstring, non-path integral formalism, and non-perturbative approach does not necessarily implies Algebraic QFT, that is, there is the most traditioinal approach, canonical operator formalism of Lagrangian QFT. Recently, Abe and myself have developed a method for finding the solution to the canonical operator formalism of QFT in the Heisenberg picture. I believe that this method is quite natural, and several models have been explicitly solved by this new method. For a review, see N. N., Prog. Theor. Phys. 111 (2004), 301-337.

74. **Chris Oakley**
November 5, 2006

Nakanishi-san,

I am not aware of your work, but it sounds interesting and will look it up when I am in the library next.
I agree with the approach you describe above, although I would like to see the Lagrangian formalism replaced by something that emanates entirely from quantum considerations. I am (sort of) working on it…

75. **Bert Schroer**
November 5, 2006

Noburo
AQFT is a different method of implementing QFT it is not a different theory which modifies any of the underlying principles of QFT (as ST or LQP). There is no antagonism and I have tried (without much success) to get this message across. Fortunately QFT is a totally intrinsic theory (it even contains the concepts of its own interpretation) and the correctness of a result can be checked without using the same methods which were used to derive it, it is definitely not just a bunch of recipes as ST.
There are however problems for which the standard canonical quantization methods fail. Take e.g. chiral theories, factorizing models, QFT in CST or the question of computation of localization entropy (which I addressed in two of my publications). If you know how to deal with those problems in a canonical setting go ahead and do it; people on this weblog who are reluctant to learn trans-canonical methods will be thankful.
I think we both are conservative. You are conservative about the formalism whereas I am extremely conservative about the principles but care less about what formalism you use to implement them. I have no problems to agree with your critique of ST , supersymmetry and the so-called noncommutative theory.
all the best
Bert

76. **N. Nakanishi**
November 5, 2006

Bert:

I do not understand the contents of what you mean by "problems for which the standard canonical quantization method fails". In the method of Abe and myself, the representation of the operator algebra is given by constructing the set of all Wightman functions for the fundamental fields. If your problems can be discussd in the Wightman framework, I guess that they can also be dealt with my method.
The important characteristic common to both canonical operator formalism and Algebraic QFT is the existence of the operator level analysis. Izumi Ojima, my colleague, who previously established canonical operator formalism of the Yang-Mills field and is presently working in Algebraic QFT, has recently emphasized the fundamental importance of avoiding the theories in which the representation level is directly considered (such as path-integral formalism). I expect that he makes some comments in this debate.

(By the way, my first name is Noboru.)

77. **Bert Schroer**
November 6, 2006

Noboru
For the three problems I mentioned previously I do not know how to compute without using the setting of algebraic QFT.
The Wightman framework may be a nice way to present the structure of QFT, but I am not aware of any model computation which has been carried out in terms of them. The nonlinear positivity requirements are just too hard for any explicit construction and all successful calculations I know start with a Hilbert space and operators therein.

Particle Physics in Hawaii

November 2, 2006
Categories: Uncategorized

This week in Honolulu there's a major particle physics conference going on, a joint meeting of the Division of Particles and Fields of the APS, and the Japanese Physical Society, called the Joint Meeting of Pacific Region Particle Physics Communities. Slides from the talks have started to appear here.

The conference is huge, with hundreds of talks (for some reason, attending a conference in Hawaii seems appealing to many people), and I haven't had time to look at more than a few of them. Barry Barish gave a talk on international cooperation in HEP, Dave Schmitz one about the status of MiniBoone, which is "in the endgame" of a blind analysis of their neutrino experiment, with the black box containing their results to be opened in the not too distant future.

Lots of talks about string theory, including a plenary talk by Polchinski partly about AdS/CFT and attempts to use it to get information about QCD, partly about the landscape and string vacua. There was also a remarkable talk by Wati Taylor entitled Can String Theory Make Predictions for Particle Physics? Taylor begins by noting that "If we could do experiments at greater than 10^{19}Gev, answer would probably be Yes". "Probably" is different than the usual claims about this… His summary of the current state of string theory and particle physics goes like this:

- String theory need not make predictions for particle physics below 100 TeV
- We can't define string theory yet
- The number of suspected solutions is enormous, and growing fast
- Nonetheless, constraints on low-energy physics correlated between calculable corners of the landscape may lead to predictions
- If not, probably need major conceptual breakthrough to have any possibility of predictivity for low-energy particle physics
- *Raison d'etre* for string theory: quantum gravity

I don't know why he chose 100 TeV here, presumably just because it is probably an upper-bound on the likely energy scales particle physicists will be able to explore during the lifetimes of anyone now living. He could just as well have picked a much higher number. The only hope he sees for getting any kind of prediction using current versions of string theory is by finding correlations between things like numbers of generations and gauge groups when you examine large numbers of string vacua (this is similar to the conclusion reached by Michael Dine, described here). In work with Michael Douglas, he has found no evidence for this. Taylor also explains that the standard 10^{500} number often given for the number of string vacua seems to be a dramatic underestimate, and that it is even quite possible that the number is infinite when one takes into account non-geometric compactifications. Fundamentally, his conclusion seems to be that there is only a vanishingly small hope remaining of getting any predictions about particle physics out of string theory, so it has to be sold purely as a theory of quantum gravity, unless a miracle happens.

Taylor does make the case that string theory has found potential uses not in unification, but in studying strongly coupled gauge theory (AdS/CFT) and in suggesting new structures to try out in model-building. But at this point, he characterizes low energy physics predictions from string theory as unlikely, their appearance would just be an "unexpected bonus". So, I guess the answer to the question of his title is basically "No". Despite this, he does end by advertising the String Vacuum Project and listing the 17 prominent theorists who are asking the NSF to fund it.

Comments

1. **relativist**
   November 2, 2006

   Taylor has an excellent summary of the state of string theory on his webpage at

   http://web.mit.edu/physics/facultyandstaff/faculty/washington_taylor.html

   He starts off by saying "String theory is currently the most promising candidate for a framework in which to understand quantum gravity. It is still not possible, however, to define string theory in a space-time background compatible with the physics we see around us, and string theory cannot yet be used to make specific predictions".

2. **JC**
   November 2, 2006

   Peter,

   (slightly offtopic)

   Was there ever any "landscape" type thing in the days of supergravity, before string theory (ie. late 70's and early 80's)?

   (I looked up some old supergravity review papers the other day, and couldn't find much resembling the "landscape" silliness).

3. **Alejandro Rivero**
   November 2, 2006

   Due to a happy cumulation of circumstances, including or starting from the fact of the mass of the tau lepton being 1.78 GeV, we happen to have one of the outsiders of physicsforums giving a talk during a paralell session there. He went to Polchinski's, and did a brief comment here

4. **King Ray**
   November 2, 2006

   I agree that string theory is currently the most promising candidate for a framework in which to understand quantum gravity; it keeps promising and promising, but never delivers.

5. **Kea**
   November 2, 2006

   One of the speakers is blogging from the conference here:

   http://www.physicsforums.com/showthread.php?t=140402

6. **woit**

November 2, 2006

Kea and Alejandro,

If people want to discuss this speaker's talk, they're encouraged to do so with him over at PhysicsForums.

JC,
No, there never was anything like the Landscape back in the 70s or 80s. If anyone had suggested working on classes of models that complicated and unpredictive I doubt anyone would have taken them seriously.

7. **Ralph Q.**
November 3, 2006

No, there never was anything like the Landscape back in

I don't think people knew how, or had the motivation, to study non-susy vacua back then. Maybe the thrust in that direction came from the definitive astronomical evidence that the CC is not zero.

8. **Carl**
November 3, 2006

After the talk by Washington Taylor , one of the questioners started out her query with "I noticed that you avoided using the "A" word …"

Of course my mind leapt to the possibilities. Antimatter? Axion? Anomaly? Asymmetry? Atheism? Astronomy? Angular momentum? Algebra? Accuracy? Adultery? Ab initio? Aardvark?

Apparently the locals knew what was being talked about and the author, dared by the questioner, used the word "anthropic" in his response. I guess that that's a bit of a dirty word among the stringers. This (parallel) talk was well attended with standing room only.

The Polchinski (plenary) talk had two questions. The first was a request to comment on Susskind's alleged suggestion that we could be at the end of physics in that we are at the "end of the reductionist paradigm". The speaker wisely avoided commenting on Susskind, but said he himself does not have a paradigm, and that serious cosmologists have been worrying about this for >30 years, that is, what is it that allows life.

The second question was about the abilitiy of a large extra dimension allowing solving of the hierarchy problem. He speculated that maybe someone in the audience knew the answer to this but he didn't call on me by name (LOL), and mentioned the embedding of 5-D theories in 10-D.

9. **Matti Pitkanen**
November 3, 2006

For some mysterious reason Susskind equates the lack of predicitivity with giving up reductionism. Giving up reductionism in standard sense (reducing physics to Planck length scale) need not mean lack of predictive power. For instance, a loss of reductionism in the sense that there exists a hierarchy of scaled up variants of standard model realized as dark matter hierarchy, does not mean the loss of predictivity since fractality allows precise predictions using scaling arguments.

The additional bonus is that there is no need to continue repeating that there is no empirical input to guide the theoretician. Consider only biology: it serves as fantastic gold mine of effects if one accepts the possibility that biology is something more that mere complexity. I dare guess that the historians of physics will see the dogmatic belief in reductionism as the deepest reason for the recent crisis in theoretical physics.

10. **Tony Smith**
November 4, 2006

A potentially useful result that is contrary to conventional wisdom of the physics community appeared in the Hawaii DPF meeting in a section on Low Energy Tests of the Standard Model.

A contribution by Goran Senjanovic entitled "Grand unification and proton decay: fact and fancy" has an abstract that stated:
"… I review the minimal grand unification based on SU(5) and SO(10) groups, with and without supersymmetry. I discuss the predictions for the proton decay and show how they depend crucially on the fermion (and sfermion) masses and mixings. …".

Since the conventional view has been for years that proton decay experimental observations have ruled out SU(5) GUT,
and since I saw no full copy of Senjanovic's Hawaii contribution,
I looked up his arXiv postings, and found hep-ph/0204311 by Borut Bajc, Pavel Fileviez Perez, and Goran Senjanovic with an abstract that stated:
"… We systematically study proton decay in the minimal supersymmetric SU(5) grand unified theory. We find that although the available parameter space of soft masses and mixings is quite constrained, the theory is still in accord with experiment. …".

A couple of years later, coauthor of hep-ph/0204311, Pavel Fileviez Perez, wrote a paper (with Ilja Dorsner) at hep-ph/0410198 whose abstract stated:
"… We investigate model independent upper bounds on total proton lifetime in the context of Grand Unified Theories with the Standard Model matter content. … Our result implies that a large class of non-supersymmetric Grand Unified models, with typical values alpha_GUT = [about] 1/39, still satisfies experimental constraints on proton lifetime. …".

In an even more recent paper, hep-ph/0601023, Pran Nath and Pavel Fileviez Perez say in section 5.6:
"… In this section we discuss the possibility of finding an upper bound on the total proton decay lifetime … one may focus on the gauge d = 6 contributions since all other contributions can be set to zero in searching for upper limits …
any non-supersymmetric theory with alpha_GUT = 1/39 is eliminated if its unifying scale is bellow 4.9 x 10^13 GeV regardless of the exact form of the Yukawa sector of the theory.
Further, a majority of non-supersymmetric extensions of the Georgi-Glashow SU(5) model yield a GUT scale which is slightly above 10^14 GeV.
Hence, as far as the experimental limits on proton decay are concerned, these extensions still represent viable scenarios of models beyond the SM. …
For example in a minimal non-supersymmetric GUT based on SU(5) the upper bound on the total proton decay lifetime is …[less than or equal to]… 1.4 x 10^36 years …".

If non-supersymmetric SU(5) and SO(10) GUT models ARE consistent with experimental observations,
and
if N=8 supergravity might indeed be finite (see the thread in this blog citing the UCLA workshop on 11-15 December 2006 described at
http://www.physics.ucla.edu/tep/workshops/supergravity/index.htm)
then
maybe they could be useful in building a physically realistic unified theory/model.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – The proton decay ideas of Senjanovic, Perez, et al seem to be independent of the ideas of Adarkar, Krishnaswamy, Menon, Sreekantan, Hayashi, Ito, Kawakami, Miyake, and Uchihori expressed in hep-ex/0008074 that experimental backgrounds in proton decay experiments may have been incorrectly defined, and that different (and possibly realistic) background definitions would render experimental results to be consistent with SU(5) and SO(10) etc GUTs. If Adarkar et al are also correct, then the case for such GUTs is even stronger.

PPS – The UCLA workshop abstract says in part: "… The intimate connection of N=8 supergravity to N=4 super-Yang-Mills theory will also be discussed. …".
As is well known, quaternionic structure is key to the special properties of N=4 super-Yang-Mills theory,
and
N=8 supergravity has octonionic structure that may be useful in studying finiteness of it and related physics models.

11. **Not A Nobel Laureate**
November 4, 2006

"The first was a request to comment on Susskind's alleged suggestion that we could be at the end of physics in that we are at the "end of the reductionist paradigm".

It's not the end of physics, just the end of Susskind.

12. **dir**
November 4, 2006

100 Tev may come from the ultra high energy cosmic rays. their energy can be as high as 10^{19} electron volts, or above, which means a center-of-mass energy 100 Tev when they hit on protons in the air.

Events

November 4, 2006
Categories: Not Even Wrong: The Book

Because of the book, people have contacted me with various requests to do one sort of event or another, and I've agreed to do a few of these. Here's a list in case any readers of the blog are interested in showing up and saying hello:

- Monday November 6, 7pm, Princeton University Store. This one is coming up soon, and it will be interesting to be back at the location of my graduate student days, doing something I never would have believed was possible if you'd told me back then that this would happen.
- Thursday November 16, 7pm, University of Minnesota, Campus Atheists and Secular Humanists.
- Tuesday February 6, 7:30pm Cafe Scientifique, NYC.

Comments

1. **anon**
   November 4, 2006

   Very prudent not to do a book signing at Harvard or Stanford 🙂

2. **Alejandro Rivero**
   November 4, 2006

   Princeton seems risky enough to me 😀

   Btw, Peter, if during your signing tours you are pressed by Squarers of the Circle and they have some kind of formulae beyond 1% experimental agreement, remember I collect such, do not hesitate to pass them my email (sounds as a good excuse to get free of them, too).

3. **Peter Woit**
   November 4, 2006

   I encourage people with the kinds of interest Alejandro describes to, in the interest of efficiency, bypass me and contact him directly…

4. **anthropologist**
   November 4, 2006

   The book signing on Monday sounds like it could be worth attending. Is that gonna be just about the book, or other things as well that have transpired since that time?

5. **Peter Woit**
   November 4, 2006

   anthropologist,

   I'll probably be talking a bit about what's in the book, but mainly about how the book came to be and what is going on now. Also, it depends a lot on the audience. I'll try and discuss whatever they seem most interested in.

6. **M**
   November 4, 2006

   for Alejandro from M-theory: g2^2 = sinThetaW cosThetaW

7. **luke**
   November 5, 2006

   so atheists are against strings

8. **Who**
   November 5, 2006

   luke: *so atheists are against strings*

   I expect you're joking, and it's ridiculous to generalize that broadly but
   to make the obvious point: it is also possible that atheists are
   interested in knowing **how things actually are** and want
   to make up their own minds about it.

   Peter's book was #3 on the UK amazon physics bestseller list this morning
   when I looked just now. It often is near top among all physics books, still after being out in UK market for over five months. The book clearly has sales potential and should be worth doing some talks and signings to promote.

9. **Alejandro Rivero**
   November 5, 2006

   to M:
   :-DDD. Nah, you probably mean the criteria in formulae (10.6) of the pgd review.

   But indeed if someone likes to indulge in such games, email directly me, not to Peter, at al.rivero in my gmail.com account.

10. **Thomas Love**
    November 5, 2006

Recieved a mailing from Scientific American Book Club which has NEW on the list of available books. TWP was in last month's mailer.
It is cheaper than what I paid, but I've already read it so the extra money was worth it.

11. **M**
November 6, 2006

maybe sW cW = g2^2 seems too good to be true, but has nothing to do with the usual definition of the weak angle as function of the *ratio* g2/g1 (reexpressed in (10.6) of PDG in terms of observables).

To avoid that this gets deleted as off-topic numerology, let me try a strategy that was quite successful in the old good pre-Woit era:

The prediction sW cW = g2^2 is conjectured to result (in the Neveu-Ramond twisted formalism) from the Suguwara-Witten affine Lie algebra developed by generalized super-M theory compactified on toric varieties of weighted complex projective spaces associated with reflexive polyhedra.

12. **Tommaso Dorigo**
November 7, 2006

Hi Peter,

as far as events are concerned: I'm sure you know this already, but just in case you don't, there is a Discover.com online chat with Lisa Randall next Thursday 11/9 at 2pm,
"and the focus is string theory. Your insights would be valuable to this dialogue" recites the email I just got from a Coco Ballantyne, inviting me to participate.

Can't help laughing at my "insight" into string theory. But I will give the chat a look!

Cheers,
T.

13. **Peter Woit**
November 7, 2006

Tommaso,

I got the same e-mail. Not sure if I'll have time to tune into this, we'll see.

14. **whoman**
November 12, 2006

Peter,
I'm a sciambookclub.com memeber and at last they have your book in the club magazine. Below are the last two lines of the magazine review:
"String theorists, who have invested most of their careers to the topic, are loath to abandon it – and some have criticized Woit in distinctly unprofessional terms."
"In the face of many books from enthusiasts for string theory, NOT EVEN WRONG presents the other side of the story."

Of course I have put in my order.

15. **Thomas**
February 19, 2007

Hi

after reading your great book, it is absolut clear that string theory is a religion, with the "flying string monster", formerly known as flying spaghetti monster (www.venganza.org), as their good 😉

Regards

16. **Thomas Larsson**
February 20, 2007

The previous poster is not me. Aren't family names meant to be used?

String Wars, Part Deux

November 4, 2006
Categories: Uncategorized

Yesterday at the KITP in Santa Barbara, George Johnson gave a second talk and led a discussion on the subject of the "String Wars". The rather remarkable first session was discussed here, here and here. This time people were much better behaved, and the main topic was the media coverage of physics in general, and the past history of the media interest in string theory, and what effects this might have had.

Johnson has put on his web-site copies of various articles from the NYTimes about string theory. The first mention of superstrings was in a piece by Walter Sullivan back in May 1985, just a few months after the "First Superstring Revolution" really got going. This piece included cautionary comments from C.N. Yang about the lack of even "a single experimental hint" and from Michael Green that "I've seen many bandwagons come and go." Interestingly, already at this time the main suggested test of string theory was astrophysical or cosmological, with the Times referring to a recent Nature article about the possibility of seeing effects of the "shadow matter" that one gets from the other E8 in the E8 x E8 model popular back then (and still popular to this day).

Much of the KITP discussion concerned what effect news stories and popular books promoting string theory have had, with several people noting that they think they have been responsible for the large number of students they have seen wanting to do graduate work in string theory. Someone in the audience also pointed out that the continual use of the modifier "super" seems to get people's attention, with students showing up wanting to study "supersymmetry" even though they didn't know what it was, and it was much harder to get them interested in, say, "diffractive scattering."

The latest Nature Physics has a fairly sensible editorial (Tied Up With String?) about the string theory controversy. Popular promotion of string theory continues today at Stanford, where the Wonderfest Festival of Science is featuring Raphael Bousso and Leonard Susskind discussing "Is the World Made of Strings?"

Comments

1. **Kea**
   November 4, 2006

   The second Johnson talk was positively dull compared to the first. I found myself nodding off. And what's the big deal with a 1000ft vertical of ice? That's not that high.

2. **Kris Krogh**
   November 5, 2006

   ". . .the continual use of the modifier 'super' seems to get people's attention, with students showing up wanting to study 'supersymmetry' even though they didn't know what it was. . ."

   That's how you win in the "free marketplace of ideas." Advertising.

3. **Bee**
   November 5, 2006

   Hi Kris,

   exactly. 1000ft or not, Steve Giddings made a very important comment. Founding agencies care about media coverage. That's not good but likely true. I also find public outreach an essential part of science, but its not good to distribute *research* grants according to the efforts invested in advertisement. Something is going wrong there…

   Best,

   B.

4. **Kea**
   November 6, 2006

   No offence to Steve Giddings intended. In reality, he is quite a nice guy.

5. **Tony Smith**
   November 7, 2006

   The blog of Christine Dantas now contains only format material, and this statement

   "Monday, November 06, 2006
   The end.
   I'll only leave the post by Daniele Oriti on his upcoming book on Quantum Gravity and on his current research."

   and the post by Daniele Oriti which discusses, inter alia,
   "… Group Field Theory … as field theories describing the quantum dynamics of the fundamental discrete building blocks of (quantum) space … they offer an altogether new perspective on the issue of the continuum approximation and on the emergence of a continuum spacetime. … It is the problem of showing that in certain situations (to be identified) the fundamental 'atoms of space' described by some GFT -condense-, so that a continuum spacetime emerges as a sort of liquid …",
   which ideas sound very interesting to me.

   Christine, thanks very much for leaving up the interesting ideas of Daniele Oriti,
   and also thanks for an interesting blog as well as comments to other blogs.
   I also liked very much Pedro's "Outer Space".

   Your contributions will be missed, and I hope that you will come back when you want to.

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

6. **urs**

November 7, 2006

> the continual use of the modifier 'super' seems to get people's attention, with students showing up wanting to study 'supersymmetry' even though they didn't know what it was

Are students also lured into solid state physics this way? By superconductors? Or by superfluidity? Maybe by the desire to learn about the supersolid state of helium? Maybe even by studies of superconducting superfluids?

7. **Who**
November 7, 2006

> Christine, thanks very much for leaving up the interesting ideas of Daniele Oriti,
> and also thanks for an interesting blog as well as comments to other blogs.
> …
>
> Your contributions will be missed, and I hope that you will come back when you want to.

I share Tony's sentiments.

Thanks for a beautiful and informative blog, Christine. Sorry it got too troublesome. I hope we continue seeing you here and at physics forums.

8. **Peter Woit**
November 7, 2006

Christine explains why she closed up her blog here:

http://www.physicsforums.com/showpost.php?p=1153475&postcount=13

and a copy of Schroer's latest that she mentions is here:

http://www.math.columbia.edu/~woit/schroer2.pdf

I'm sad to see what happened here. Her blog was an excellent example of a positive contribution and one of the best in this are. The fact that the controversy over string theory made it so difficult for her to keep doing what she was doing, mainly about LQG, without getting caught up in an environment she didn't have the temperament for, is very unfortunate.

9. **Bee**
November 7, 2006

Hi Peter:

Thanks for the link to PF, it makes me feel slightly better. I just sent Christine a long email apologizing for my last comment. I feel really bad about it since it seems I did upset her quite a lot. I can't even remember exactly what I wrote, just that I didn't like the insulting side remarks in Schroer's paper, that this is not going to solve any problem, and that it surprised me she would post this writing. 1/2 hour later there were several other comments whose exact content I can't recall, and a last comment from Christine saying it's been enough, or something.

It makes me very sad to see her go, since her blog has been very balanced on various topics, and I've always liked to read it. On the other hand, I can relate to her problem with the journalist… – and I guess most of you around here know that at some point it just takes too much time, and effort, to set every misconception straight, and one wonders whether it's worth it.

I also want to add for those of you who follow the so-called string wars only via the media and the internet: the picture you get from what is actually happening is badly distorted. There is some truth in what is written, but for most of the time we get along pretty well and peacefully, and just do our research. How it comes across in journal articles, or blogs, is like putting magnifying glasses on a spot on your nose and then getting obsessed about it.

Yes, there's some discussion about where the money goes and how to hire people, but that discussion has always been there, and probably will always be there, unless some government realizes that theoretical physics needs a grant increase of at least two orders of magnitude. Yes, there are reasons not to like string theory, there are also reasons not to like LQG, but there's no reason to retreat to name-calling and insults. If anything, I'd wish we'd focus on our similarities, the scientific goals that we want to achieve, the reason why we went to study physics in the first place.

I think, this was roughly the last sentence of my comment on Christine's blog.

Best regards,

Sabine

10. **Bert Schroer**
November 7, 2006

I have participated in this blog for a couple of month always trying to stick to physics, I think almost all my contributions had a physics content. Having been insulted by anonymous bloggers and by you know who, I have nevertheless stuck to the scientific content.
Please will anybody let me know where in my last contribution I insulted somebody? I break taboos (but so does the owner of this blog), but this has nothing to do with insults. Bee, you have all the advantages of anonimity on your side which I do not enjoy, please let me know the insult I committed so I can correct this or appologize.

11. **Tony Smith**
November 7, 2006

Peter linked to Christine's explanation of why she ended her blog. In it, she said that she
does "… not have the right temperament for "living in the blogosphere".. …".

It reminds me of a scene in the movie Batman in which Joker (Jack Nicholson) says about Gotham City:
"Decent people shouldn't live here. They'd be happier someplace else.".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Bee said "… most of the time we get along pretty well and peacefully, and just do our research …". That does not apply to those of us who are blacklisted by arXiv, unless Bee considers blacklistees to be outcast nonpersons.

12. **Bert Schroer**
November 7, 2006

I sent a corrected version of my paper to Peter, and I am sure that if he will be back from his book presentation tour he will do the change. According to my best knowledge neither version contains insults. The new version has some more remarks about the history of the recent great profound discoveries of the implementation of diffeomorphism covariance and the sufficient condition for obtaining diffeomorphism invariance. The story starts with the construction of the local covariant energy-momentum tensor by Wald and reaches its present culmination in the two papers I cite (unfortunately not my own!). I am a particle physicists and the advanced particle physics methods have impressed me more than anything else. Is that insulting to the Perimeter or to KITP?

13. **Bee**
November 7, 2006

Dear Bert,

Yes. I did not say your paper had no physics content, and I am aware of your contributions, which are interesting in many regards.

I made it explicitly clear that I was not referring to the scientific part of your text. What I can't approve of are general and unbalanced accusations of 'mass psychosis', 'mass cult', 'the scientific version of Stalinism' and 'faith based mantra', etc. This is just plain insulting for many string theorists, who are highly qualified serious researchers. A picture of string theorists being blind worshippers of some mass cult is as far off of reality as that of 'alternatives' eating muesli and reading Faust. Both of which are prejudices that I don't want to see distributed, no matter what reason you have to justify your claims: it's not going to help matters — if anything it will increase the gap that I sense dividing our community (meaning: theoretical physicists). If we are going to disagree, we should stay on scientific ground.

Dear Tony,

There are definitely things at odds in the way science is pursued today, and the arxiv policies are a mystery not only to you. Regarding the question whether the community today does too easily 'outcast nonpersons', Lee Smolin makes some good points in his book there. Yes, these are issues that we should work on, but not by calling other's theories crackpotism or faith based science. This is not constructive. My remark above referred to the fact that the string theorists I have met are not more or less weird, aggressive, naive or stubborn than theoretical physicists of other research directions (and I should add, there is more to theoretical physics than strings and loops).

What bothers me, and you can already see this from this comment section, is that name-calling gets far more attention than any kind of constructive criticism. E.g. to come back to the original topic of Peter's post: how come there's been so little discussion of this second part, but an enormous fuss about the first one? In this second part there were some important (though hardly new) points raised: as I wrote above, Steve pointed out that founding agencies might care about the media coverage – this is certainly of some concern. Someone mentioned (what I've also pointed out before), that the 'string wars' currently are a very US-centered phenomenon. Then there was a very interesting contribution about whether or not such media coverage as on the string-issue today does influence young students. And if it does influence, in which way, etc.

Best,

B.

PS: *Bee, you have all the advantages of anonimity on your side which I do not enjoy,*

My name is Sabine Hossenfelder, as you can easily find out from my blog. You get plenty of details about me by googling my name. In fact, I do think that anonymity is one of the big problems in this equation. E.g. it seems to me that there are only a hand full of people echoing always the same silly sentences (it's quite ironing that George Johnson began his first talk trying to make that point but nobody was really listening, they were all too busy echoing).

14. **Kris Krogh**
November 7, 2006

Hi urs,

The "supers" you cite are not theories, but physical phenomena that are actually above others. The theories for these things have not-so-exciting names like "BCS."

I think marketing considerations do go into the naming of some theories, a classic example being "quintessence."

15. **Christine Dantas**
November 7, 2006

There is one thing that I would like to point out in this discussion. Bert Schroer was absolutely *not* responsible for the ending of my blog! I encourage everyone interested to read his paper. It is a polemical paper, yes. But it does have important scientific comments that are deserved to be widely read and studied.

I pointed out over at PF that one of the reasons that I decided to end the blog was that I was not having a good time dealing with polemics. So after posting Bert's paper over at my blog, this feeling that I just was tired of dealing with polemics came out and I decided it was time to stop. This was a process that was already happening for some time.

So, my last word on this issue here is: *I appreciate Bert Schroer's criticism on string theory and his paper should be read with care. I do think there are some points that he could have written differently, but that's not what matters here.*

I publicaly apologize to Bert Schroer in the case that this was the impression I have given (that he was responsible for my decision to end my blog). Not at all.

Best wishes,
Christine

16. **Bee**
November 7, 2006

and isn't it ironic…

17. **Bert Schroer**
November 7, 2006

Dear Sabine,
Now I know who you are (inasmuch as this is possible from weblogs). There is a generation gap between us. I have seen impressive progress in particle physics when there was no necessity to think about sociological aspects of science and nobody (myself included) would use the style of communication as nowadays in weblogs but even in papers, whereas you have grown up already in this deep crisis of particle physics in the times of market values.
I spend 6 years under Stalinism (my neighboor in school was Helmut Karasek). When we use the adjective “Stalinist” nowadays we mean total reality denial coupled with the internalized belief that the worse things get so much the better the glorious destination. I know that we have entered such a situation in physics, I am not saying this to insult you or anybody else. People can be very intelligent and serious and nevertheless being misled. Seriousness and intelligence are of course necessary but by themselves no guaranty for innovative insights. You misunderstood me completely if you think that I am denying the intelligence and dedication to people in ST or LQG for that matter.
I used to think that hype, mass psychosis and collective delusion can only occur outside of science (Nazism, Stalin ism.. can be updated) but never in science. Well this is not true; we were only lucky that it hit us so late (see the remarks of Phil Anderson). If you feel that this does not concern or affect you that is fine, then just forget it and look at the scientific aspects. Maybe you can see that the scientific world is not yet completely dominated by those communities which are doing their best to steer particle physics into their barren mono cultures, but there are still some concrete individuals (which Smolin describes abstractly), which are not only in the possession of an underlying philosophy, but also have very balanced harmonizing conceptual and technical skills (as in the good times of Feynman. Schwinger…), but maybe not for long anymore; they cannot be globalized and they cannot have descendants.

18. **Christine Dantas**
November 7, 2006

Hi Bee,

Yes, it is ironic… That’s a *blogostuff fever* I suppose….

And again, to make it clear: **No one is responsible for the ending of my blog, except myself.** I openly apologize to anyone who believe that I have damaged them for some reason, and that includes you, Bee.

I have created the blog and I have destroyed it. It was just a blog. It was useful, nice, interesting? Good, I am happy I did something useful in this life! Now it’s gone, like many things in life I suppose…

All the best,
Christine

19. **Bee**
November 7, 2006

Dear Bert,

Thanks for this statement. Please understand that in my perception your writing can easily be misunderstood as being insulting, even if you didn’t mean it to.

whereas you have grown up already in this deep crisis of particle physics in the times of market values.

Well, there will always be parts of science that flourish and others that people loose interest in because it doesn’t seem to go anywhere. So far I haven’t considered myself as growing up in a scientific crisis. As you also say, if there’s a crisis then it is not simply one of scientific research, but a much more general one. Many of the concerns about ethic values, or competition on the ‘marketplace of xxxx’, people blindly following fashions, etc definitely aren’t specific for string theory, just apply them to any other field you can think of, from politics to the cosmetic industry.

If you feel that this does not concern or affect you that is fine, then just forget it and look at the scientific aspects.

It does certainly concern and affect me, otherwise I wouldn’t be here spending my time writing this comment. I just think it would be better to disentangle the sociological criticism from the actual research.

Best,

B.

PS: My comment from above overlapped with yours, so I hadn’t read your comment when I wrote mine.

20. **Arun**
November 7, 2006

Of the three – “hype, psychosis and collective delusion”, I think the only one applicable to string theory is hype. Since Stalinism and Nazism involved physical coercion of their opponents, they are utterly inapplicable and inappropriate to physics. Perhaps a more appropriate analogy, if one needs to be found, is that of a stock market bubble. It is not necessary to elaborate on the analogy.

This : http://www.floor.nl/ebiz/gartnershypecycle.htm
is an even better analogy, in my opinion.

It is a pity that Christine Dantas decided to end her blog. But, Bee, don’t feel badly.

21. **Bee**
November 7, 2006

Dear Christine:

My rather cryptic line from above referred to a typo in my earlier comment — obviously, it didn’t mean to write ‘ironing’… 😉 There’s no need to apologize, and I don’t feel specifically damaged. I hope this is the right decision for you, and you’ll be around. Best,

B.

22. **Christine Dantas**
November 7, 2006

Hi Bee,

I am alleviated that you are OK. I am also OK, and I believe Bert Schroer is OK as well. Peter Woit, are you OK? Good! So everything and everyone seem to be in their proper places. Good!

Now you can continue. 🙂

Christine

23. **Bert Schroer**
November 7, 2006

Arun
O.K. Stalinism killed people, whereas in particle physics only careers but not opponents of the dominating fad are destroyed. I think hype alone could not do that, but let us get back to work.

24. **TheGraduate**
November 7, 2006

Bert,
One of the sad but unavoidable things about science on a limited budget is that someone's career is going to get 'destroyed'. Someone, somewhere, is going to have to quit and leave science. It might happen in high school, or graduate school or somewhere along the tenure track. But it more or less has to happen somewhere.
Given that physics jobs are neither being created nor destroyed but only transformed from one form to another, I have often been confused about what element of this string theory debate isn't just a isolated power struggle between different physicists. On the one hand you have string theorists who are presumably trying to make advances in theoretical physics. On the other hand, you have other non-string theorists who feel the same way and are doing the same thing. Where is the symmetry broken?
You could call what the string theorists are doing similar to Stalinism (ie organization for malicious purposes) or you can call it Fordism (after Ford of Ford motor cars, or organization for positive and effective purposes).

25. **Tony Smith**
November 7, 2006

Bee said "… What bothers me … is that name-calling gets far more attention than any kind of constructive criticism. …".

Maybe the lack of constructive criticism is a direct consequence of the validity of Feynman's criticism:
"… I don't like it that they're not calculating anything. … why are the masses of the various particles such as quarks what they are? All these numbers … have no explanations …".

If superstrings predicted a quark mass and LQG predicted a different quark mass,
then
the old physics process of comparison with experiment (which old process did lead to the last Big Thing in theoretical particle physics, the Standard Model) could settle the question of which (if either) model is realistic.

In the absence of what Feynman wanted, the only thing left for superstrings and LQG in their fight for funds is to engage in "name-calling".
Further,
if an alternative model DOES produce realistic numbers, the only way they can fight for funds against it is to "outcast" alternative model-builders as "nonpersons".

Therefore, it seems to me that, given human nature,
and the present state of superstring and LQG models (which get about 90% and 10%, respectively, of the theoretical particle physics funding in the USA),
it is inevitable
"… that name-calling gets far more attention than any kind of constructive criticism. …"
and
that blacklisting exists.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

26. **nontrad**
November 8, 2006

An unfortunate turn of events….

With repeated mention of the 'good old days' (calculation and so forth) it seems that this situation reiterates, in a round about way, the argument we've heard many times before: Data!

My 2 cents, physics has always been about data; data historically drove the most remarkable results…and I, for one, am convinced that it will again. Maybe not tomorrow, but one of these days.

In the mean time, I suppose it's a question of waiting this situation out. Hopefully, we won't let the tension get to us. The waiting room can be so boring.

In any event, all the best and best regards to you Christine.

27. **Tony Smith**
November 8, 2006

nontrad said "… physics has always been about data …".

That is true, and experiments are still providing relevant important data.
For example,
as Tomaso Dorigo said on his blog at http://dorigo.wordpress.com/ by post dated 26 October 2006:
"… WZ production discovered!
… Yesterday the CDF collaboration blessed a new result on electroweak physics: the measurement of cross section of the production of WZ pairs. …
we want to test the Standard Model … in [all] the available realm of predictions it makes. The rarer a process, the better the chances that something does not agree with the theory …
Now that we start seeing the very rare standard model processes yielding multi-leptons … we still see no new physics …".

In short,
new data is coming out, and it confirms the Standard Model,
with no non-Standard-Model-stuff like the supersymmetry of superstring theory.

The superstring and LQG models are not failing to meet Feynman's standards for want of data.
They are failing to meet Feynman's standards because,
even after decades of work by many very smart people,
they have failed to produce calculations that describe ANY of the particle masses, force strengths, Kobayashi-Maskawa parameters, much less to predict the WZ results stated by Tomaso Dorigo.

That indicates to me that alternative approaches should be given substantial support.

I would prefer that superstring and LQG funding remain level on the chance that maybe somehow those workers might some day figure out a way to do some of the calculations that Feynman wanted,
and
that new money be appropriated for alternative approaches.

The quest for new money for high energy physics theory alternative approaches
should (in my opinion)
be as important as the quest for new money for high energy experiments such as the ILC.

Since the amounts of new money needed for theory are far less than the amounts needed for such things as the ILC,
it should be at least as easy to get additional theory money as to get ILC money.
Perhaps the theory increase could be folded into ILC proposals.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – Now that the Democrats have won control of the USA House of Representatives,
Dennis Hastert (Illinois Republican) will probably be replaced as Speaker of the House by Nancy Pelosi (California Democrat).
The USA high energy experimental community has strongly advocated an Illinois (Fermilab) site for the ILC, in large part to get support of Hastert as Speaker of the House.
Maybe the USA high energy experimental community should rethink its advocacy of an Illinois site in favor of a California site, which is not only the home of Speaker-to-be Pelosi, but is also on the Pacific Coast and therefore possibly more acceptable to other Pacific nations such as Japan.

28. **M**
November 8, 2006

TheGraduate,

speculations about quantum gravity have been fruitless: this causes struggles among physicists. In healthy fields (like cosmology, neutrinos and hopefully particle physics in 2010) struggles are about physics.

29. **urs**
November 8, 2006

> The "supers" you cite are not theories, but physical phenomena

The "supers" I cited were names for properties. So is the "super" in "supersymmetry".

> I think marketing considerations do go into the naming of some theories, a classic example being "quintessence."

I agree, that's a good example (since it is such a bad example: what a horrible term).
Whether similar considerations made people say "super" instead of "graded" back in the old days I can't say. But I am somehow doubting it.

I know that, as a a student, I found the plethora of "supers" in physics appaling. (But then, that's also my reaction to most kinds of advertisement, so maybe there is a point 🙂

30. **Bert Schroer**
November 8, 2006

The "super" in superconductivity and superfluidity has the meaning of "ultra" (trans, alem) whereas in supersymmetry it is hegemony, arrogance and domination; this was its only purpose and everybody knows that. It in no way goes beyond symmetry and contains it in a limit or by a phase transition. Once such a misleading name has been given and used for some time it cannot be changed (the word "string" in string theory is another case, from the point of view of quantum localization it still continues to be point-like localized, it has many more degrees of freedom as the irreducible object of QFT which is a free field). I wish Urs would stop posting such nonsense on this weblog which after all is dedicated to a critical evaluation of string theory and not about stupid names.
Peter, can you please finally exchange the first version of my draft (the one which I sent to yesterday); on a server I could make a replacement on my own, but here I cannot do it. My original purpose (when I dedicated it to the anniversary of Christine's weblog) was to present a profound analysis of all the claims in ST (I think I did not forget any) in order to get a scientific discussion going and I thought that this would have been a good place because it is also visited by LQG researchers. All the people who bothered her should address me directly after having red the corrected version (which I again kindly ask Peter to post).

31. **anon**
November 8, 2006

Bert,

"but there are still some concrete individuals (which Smolin describes abstractly), which are not only in the possession of an underlying philosophy, but also have very balanced harmonizing conceptual and technical skills (as in the good times of Feynman. Schwinger…), but maybe not for long anymore; they cannot be globalized and they cannot have descendants."

Could you give us some names please just to make things concrete?

32. **TheGraduate**

November 8, 2006

As far as I know, Smolin named names but maybe I don't know quite what you mean … he had names like Penrose and Rovelli and others.

33. **Bert Schroer**
November 8, 2006

Anon
you just have to look at the updated last section of my paper (which Peter hopefully will substitute today). A fascinating story started at the beginning of the 90s by Bob Wald's work about how to define the correct energy-momentum tensor in QFT in CST and culminated tentatively in startling results about diffeomorphism- covariance and its expected quantum invariance. The story is certainly continuing and I (as somebody with a particle physics background) am very excited about it. It is completely within an (albeit advanced, you may not like this) particle physics (QFT) setting and does not create those large distances with particle physics as we know it from the other approaches. It is certainly closer to LQG than to string theory (that was the reason why I thought that Christine's weblog would have been a good place) if only the LQG people would notice it. Just be patient for a couple of hours.

34. **Bee**
November 8, 2006

oh no! I lost my comment… what a waste of time… what was I about to say? … I'll try again…

Dear Tony,

high energy particle physics just might not be the way to proceed. Imo it's currently much more promising to focus on cosmology/astrophysics. We already have evidence for physics that can't be explained within the standard model, the data is good, and getting better every year. If you think about it, collider experiments aren't exactly the smartest thing do to. It's like examining leftovers of a car accident and trying to figure out what the driver ordered for dinner the evening before. Yes, that might not be good news for particle physicists, and yes, everything was better in the old days, when we were all younger, when the gasoline was less expensive, and the music was still music, but that's the way life goes.

it is inevitable "… that name-calling gets far more attention than any kind of constructive criticism. …"

It is not. Name-calling is a cheap and easy way to get attention, whereas constructive criticism requires time and thought. Blogs seem to favor an atmosphere of fast and unreflected comments and reward upsetting statements far more than qualified opinions. Unfortunately, this is more and more used by the media – in print as well as online – to strongly polarize discussion and to artificially enhance disagreements that otherwise wouldn't excite anybody. This then feeds back into the discussion in an upward spiral.

You decide for yourself what you pay attention to. That's a good starting point I'd say.

Best,

B.

PS: If that comment vanishes again in virtual Nirwana, I give up.

35. **Bert Schroer**
November 8, 2006

Maybe Bee, but I do not see how you can base your Astrophysics/Cosmology on such a shaky particle physics basis as we have now (i.e. how you can live with everytime more precise astrophysical observations and a shaky basis of fundamental interactions which according to you does not need improvements).
I like your comparisions with car crashes but none of the big car-manufacturers would sell anything without the security improvements of controlled car crashes. The LHC may not do miracles expected by ST (perhaps less so by LQG), but at least the hope to close some of the open ends seems to me realistic.

36. **Tony Smith**
November 8, 2006

Bee quoted me out of context as saying "it is inevitable "… that name-calling gets far more attention than any kind of constructive criticism. …""
and
then hit the out-of-context straw man by saying "It is not. …".

The very important omitted context was
"given human nature,
and the present state of superstring and LQG models (which get about 90% and 10%, respectively, of the theoretical particle physics funding in the USA),
it is inevitable
"… that name-calling gets far more attention than any kind of constructive criticism. …"".

In that context, I stand by my statement because:

1 – Human nature in fights over funding in the present-day USA, where money is GOD, is that anything goes. Just as in political campaigns (for example the Tennessee Senate race), name-calling etc can be very effective. In fact, the physics funding fights look a lot like political campaigns.

2 – The present state of superstring and LQG models is that neither of them can make any substantive particle physics predictions, so that "constructive criticism" fails because neither class of models has any concrete particle physics calculations that can be constructively criticized. As John Baez said in a blog comment on Ars Mathematica:

"… The unpleasant nature of the whole extended argument can be seen as a collective cry of agony on the part of physicists trying and – so far – failing to find a theory that goes beyond the Standard Model and general relativity. Both string theorists and their opponents are secretly miserable over this failure. …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

37. **Bee**
November 8, 2006

Dear Bert,

how you can live with everytime more precise astrophysical observations and a shaky basis of fundamental interactions which according to you does not need improvements).

I never said that our basis of fundamental interactions does not need improvement. This is not my opinion. I said that I find it very possible that astrophysical/cosmological data can give us the clues that collider physics hasn't yet delivered.

The LHC may not do miracles expected by ST (perhaps less so by LQG), but at least the hope to close some of the open ends seems to me realistic.

Yes. There's a reason why I've worked on LHC physics for the last years.

Dear Tony,

sorry for cutting your sentence in a confusing place. I still disagree with you. *Human nature in fights over funding in the present-day USA, where money is GOD, is that anything goes. […] the physics funding fights look a lot like political campaigns.* . Indeed, but there's no reason to throw hands up and say: well, it's in the human nature to adore money as GOD, and its inevitable to retreat to name-calling, that's just how we are. Maybe my picture of humankind is just much more optimistic, or call it naive, but I don't believe it's actually money that we are searching for — Instead, it's what we think it represents. Especially in the scientific community we should be able to realize that on the long run the winner won't be the one who has the best rhetorical skills. Everything else is shortsighted, and hinders progress. Think future. Sadly enough, I agree on what Baez said.

Best,

B.

38. **r hofmann**
November 8, 2006

Sabine, I agree with Bert when he says that particle physics is in a deep crisis considering the huge industry that went into virtually no gain of robust knowledge and deep understanding over the last twenty years. Your present opinion reminds me of my first three postdoc years when I didn't see anything wrong either in scientifically dealing with extra dimensions, susy and the like. In my case I only slowly started to develop a belly ache about what was happening around me, slowly but steadily. I would say people like Bert naturally have a more global view on things because of their richer experience. My opinion is that in Bert's case this experience serves constructively and courageously to change things for the better, he has demonstrated his scientific and social integrity in a number of ways …

Viele liebe Gruesse von Ralf

P.S.: Schon mal was mit selbstdualen Yang-Mills-Feldern, Donaldson-Invarianten, der Poincare-Vermutung, oder dem dreidimensionalen Ising-Modell zu tun gehabt?

39. **Tony Smith**
November 8, 2006

Bee says that her "… picture of humankind is just much more optimistic …".

I hope that Bee is right and I am wrong.

I would like very much to see (especially in the scientific community) that

competition is among IDEAS (all relevant ideas being considered) with the outcome being decided by comparison with experiments

rather than

among people saying things like "You know, if you know Lee [Smolin], it's because he wants our money. It's because he doesn't have it that he wants to cut us down. …" (quote from a KITP person in the first Johnson String Wars talk) with the outcome being decided by dictates of authority / political power.

Maybe my relatively dark view is colored by my experiences.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

40. **anonymous**
November 8, 2006

Bee, what is the "evidence in cosmology/astrophysics for physics that can't be explained within the standard model" (apart from dark matter and cosmological constant)?

41. **Bee**
November 8, 2006

Hi Ralf,

Sabine, I agree with Bert when he says that particle physics is in a deep crisis considering the huge industry that went into virtually no gain of robust knowledge and deep understanding over the last twenty years. Your present opinion reminds me of my first three postdoc years when I didn't see anything wrong either in scientifically dealing with extra dimensions, susy and the like.

I see a lot of things going wrong, and I do agree that there's been an unfortunate pursuit of research directions that didn't pay off. However, I don't see this as reason to get depressed, and proclaim a crisis in physics. If the LHC doesn't fulfill our dreams, well, then lets look elsewhere. E.g. in the CMB.

Even though I've worked on extra dimensions, that's been mostly politically motivated. I admit that I'm not particularly proud of that, but at least I still have a job. As far as I am concerned, that is what's going wrong. I don't see a crisis in high energy particle physics, I see a problem with the way research directions and people are chosen and supported, for all further details, read Lee's book, he writes roughly what I also think, just skip the first three parts (and take off the cover).

Ralf, Ralf, Ralf… give me a hint, have we met?

Best,

B.

PS: 1. Yes, 2./3./4. No.

42. **Bee**
November 8, 2006

Hi Tony,

I am genuinely sorry to hear about your bad experiences. I was stupid enough to listen to the first talk, so I've heard all that crap. *I would like very much to see (especially in the scientific community) that competition is among IDEAS (all relevant ideas being considered) with the outcome being decided by comparison with experiments*

So would I! I encourage you to make proposals at to how the present situation can be improved in your opinion, or to distribute suggestions made elsewhere. If we have to echo something, then that's what we should echo.

Anonymous,

Isn't that enough to begin with? I think one shouldn't mix up the diverse data that lead us to postulate DM/DE, I find that very dangerous. E.g. the evidence for DE from high redshift supernovae, and that from WMAP. Then there's (with some statistical significance) the alignment of the low multipol moments ([Joao's axis of evil]()), evidence for DE from structure formation, rotation curves of galaxies (yes, it's handy to explain both by the same stuff, but do we actually know that?) and why is DM clustered in which galaxies how, do we understand that? Then (if I recall that correctly) there's a problem with the voids between visible structures (they are too large), what about the Pioneer anomaly, the missing GZK cutoff (okay, data isn't too good here, but should become better soon)? I also think there are some mysteries about the WMAP data, see e.g. [the north-south asymmetry…]() Hey, I am not even a cosmologist and can come up with all that!

Best,

B.

43. **Bert Schroer**
November 8, 2006

The corrected version of my article ST "deconstructed" is now available where Peter yesterday placed the previous version:
http://www.math.columbia.edu/~woit/wordpress/?p=489#comment-18997
For starting a critical evaluation of its content (a critical evaluation of all claims ST made which I am aware of) this may not be a good place, so I should perhaps ask Peter to post it at a more visible and accessible place.
There is of course no insult there since you can only insult individual persons and not areas of research as ST or LQG. When the subject of critique is an area of research (and naturally the associated monoculture of a globalized community) then one may be breaking a taboo, but this could hardly be called an insult. Critique and even taboo breaking is the oxygen of science (not only particle physics) and anybody who wants to regiment this may as well close down physics departments and research institutions. Nobody is served by acting according to the maxim: Friede, Freude, Eierkuchen (I do not know how to translate this into English)
My formulation is sometimes a bit shrill, but it is my experience that if one does not penetrate the elevated noise level of the globalized market, the message will not be noticed, as interesting and important it may be (I gave an illustration of this in the epilogue of my article). Of course the level must be proportioned to the content of the message, and I hope that I did not trespass this elementary rule.
It would be very productive if one could get a discussion about those 7 or 8 claims from ST which I exposed as being flawed in the above paper. This would give me the chance to (in a later stage) post something onto the hep-th server which has been brought to a public test. If the details of my arguments are to scars or unclear I can try to be more explicit. Of course string theorists are very welcome, especially in case I misunderstood any of their claims.

44. **Christine Dantas**
November 8, 2006

Ok, a (partial) backup of the blog is available there:

http://christinedantas.blogspot.com/

The blog morped into a Frankensteinian repository.

Anyway, now I think I can sleep well.

Christine

45. **Christine Dantas**
November 8, 2006

…morphed…

46. **Bert Schroer**
November 8, 2006

Dear Christine,
we had several email exchanges and there was at no time a problem or tension between us. Nevertheless I think it is nice to mention this also publicly and to express my sadness about having lost the most informative and equilibrated Brazilian weblog.
Already before you closed down your weblog you mentioned the problems outside your control which caused doubts whether the effort is worth it. You told me that you are not located at one of the great scientific centers in Brazil and that your have virtually no contact with the national physics community. I advised you to only continue as long as you get some satisfaction and enrichment of knowledge but that you should not do this for years only because your weblog became institutionalized. I also mentioned that Brazil as a result of the large distances between places of scientific activities and the grown number of researchers should have 3-4 weblogs and it would be nice if you could continue up to the appearance of others. Nevertheless, considering the circumstances a couple of days later, I think that your decision was the right one. I of course still have the hope that your example may animate other Brazilian physicists, although they would have to work very hard to build up those connections you had to the leading LQG people (or to other groups for that matter). But I also have the advice that if you should find yourself ever in a situation of "saudade" for your past activities then please return. It is not necessary to become enslaved by a weblog, it is not like a pet which needs to be constantly fed.
com um beijo e um grande abraco
Bert
ps. I just saw that you have a new post which I will look at immediatly after sending this one

47. **anon.**
November 8, 2006

"Especially in the scientific community we should be able to realize that on the long run the winner won't be the one who has the best rhetorical skills. Everything else is shortsighted, and hinders progress." – Bee.

It is not shortsighted but PERFECTLY LOGICAL for leading physicists to simply rename the concept of "shortsightedness" as "TAKING ONE DAY AT A TIME", ie, rely on name calling of alternatives (crackpots, cranks…), which enables them to keep what they perceive to be the "noise" level down (any alternative to M-theory = noise, by definition).

Similarly, the rulers of the church on 17 Feb 1600 burned the monk Brother Bruno to extinguish the noise he was making about astronomy. They weren't shortsighted, they didn't pay any penalty! They were long dead and burned by the time things changed. The censors only profit.

48. **Christine Dantas**
November 8, 2006

Dear Bert Schroer,

You told me that you are not located at one of the great scientific centers in Brazil

Well, I am, but I am not working in a physics department.

Thanks for the blogging advice. Blogging is not for me. Not for the faint of heart.

Why don't you set up your own blog?

Best wishes,
Christine

49. **Bee**
November 8, 2006

Dear Anon,

It is not shortsighted but PERFECTLY LOGICAL for leading physicists to simply rename the concept of "shortsightedness" as "TAKING ONE DAY AT A TIME", ie, rely on name calling of alternatives (crackpots, cranks…), which enables them to keep what they perceive to be the "noise" level down (any alternative to M-theory = noise, by definition).

What do you think these 'leading physicists' are searching for? It is money, is it fame? Or is it wisdom, knowledge, insights into the mysterious ways our universe works. And what is the way to a fulfilled life? It is my believe that many researchers have just lost track of their original motivations to become a scientist. And it is awfully easy to loose out of sight what you were really looking for if you are faced with what I like to call 'reality constraints' : you need a job, a place to sleep, income to get your family through. It hurts me if I talk to my colleagues and many, too many of them, tell me they'd rather work on this and that, but they are sitting on that boring grant number xyz, and they need to come up with a publication on a topic that's completely outdated since at least a decade. This IS unnecessary, and it is due to a mismanagement in our community, which can be improved. And if we don't do so, it IS shortsighted on every level you can think of: start with Bush's prime concern, the international competitiveness. It's not going to be helpful in any regard if theoretical physicists call each other names, and all advertisement in Nature, Discover or Physics Today isn't going to make a promising and promising theory into reality.

There is something at odds with the values in our society, more seriously though in the US, and it reflects not only in science. It is not logical to follow this path. Do I really have to point out that money and a high citation index doesn't make you happy?

Best,

B.

50. **Tony Smith**
November 9, 2006

Bee says that she "… believe[s] that many researchers have just lost track of their original motivations to become a scientist …
There is something at odds with the values in our society, more seriously though in the US …".

As to my opinion about "how the present situation can be improved",
I am (in my dark view) pessimistic,
but
here is my best guess at a course of action:

Some institution (not an individual) should set up a program (maybe for 3 to 5 years duration) to study and compare and evaluate ALL approaches to constructing unified physics models.

All the really obviously wrong ones should also be evaluated. Since it is easy to specify flaws in obviously wrong models, not much time would really be wasted doing that, and it would guarantee that EVERY approach got some consideration.

The only rules would be:
the evaluator should not be a worker on the approach being evaluated (thus eliminating self-praise)
and
the evaluator should actually listen to (or read) and understand the approach (at least enough of it to be accurate in making an objection to an approach)
and
an approach advocate should be allowed to append a supportive statement (the only limitations being as to length and being in a reasonable format such as pdf or LaTeX).

The evaluations (including statements etc) should then be put up on an evaluation web site for the world to see and make comparisons.

Such a project would create some physicist jobs, which is good.
It should be made clear that an evaluator gets as much credit for working on a flawed approach (and showing its flaws) as for "discovering" some approach that turns out to be useful and realistic.

I don't have enough experience in the world of grants to know how much it might cost,
but here is a rough guess:
50 jobs at $200,000.00 per year total cost including overhead, taxes, benefits, etc, would be $10 million per year;
for a 5-year program, that would be $50 million.

Since JoAnne Hewett said over at Cosmic Variance about the HEPAP committee on which she serves:
"… our committee is only charged at looking at experiments that cost $20 Million or more. …".
it seems to me that $50 million over 5 years is a reasonable, even cheap, amount to spend to get some sort of objective evaluation of the various approaches to unified theory models.

Of course, if the dominant paradigms of superstrings and LQG were really successful now, such a program would not be necessary,
but
as John Baez said (and Bee agreed): "… Both string theorists and their opponents are secretly miserable over this failure … to find a theory that goes beyond the Standard Model and general relativity. …"
and
such a program might uncover a realistic prospect that is currently off-the-radar of the physics community.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – As to what institution might be good to do such work, I don't know.
A problem with the biggest and richest North American universities and institutes (Harvard, Stanford, Institute for Advanced Study at Princeton, Kavli Institute at Santa Barbara, Smolin's Perimeter Institute)
is that their theoretical physics groups seem to be effectively monocultures for Superstrings (or
Loop Quantum Gravity / Spin Foam in the case of Smolin's Perimeter Inst.),
so they may be unable or unwilling to undertake a fair comparative evaluation.
Maybe the work could be done outside North America, say in Brasil or Japan or Europe … ???

PPS – As an example of current interesting results that seem to be ignored by most of the physics community,
that might be picked up and evaluated by such a program,
consider a contribution in the recent Hawaii DPF meeting in a section on Low Energy Tests of the Standard Model by Goran Senjanovic entitled "Grand unification and proton decay: fact and fancy" whose abstract stated:

"… I review the minimal grand unification based on SU(5) and SO(10) groups, with and without supersymmetry. I discuss the predictions for the proton decay and show how they depend crucially on the fermion (and sfermion) masses and mixings. …".

Since the conventional view has been for years that proton decay experimental observations have ruled out SU(5) GUT, and since I saw no full copy of Senjanovic's Hawaii contribution, I looked up arXiv postings for him and his coworkers (fortunately they are not blacklisted) and found hep-ph/0601023 in which Pran Nath and Pavel Fileviez Perez say in section 5.6:

"… For example in a minimal non-supersymmetric GUT based on SU(5) the upper bound on the total proton decay lifetime is …[less than or equal to]… 1.4 x 10^36 years …",

so, it seems that neutrino proton decay measurements have NOT in fact ruled out SU(5) GUT,
thus
contradicting a statement in the 2006 Particle Data Group Review 15. GRAND UNIFIED THEORIES (Revised October 2005 by S. Raby (Ohio State University)):
"… Recent Super-Kamiokande bounds on the proton lifetime severely constrain … with [lifetimes for varous decay paths greater than]… 5.0 x 10^33 yrs …[and]… 5 x 10^33 yrs …[and]… 1.6 x 10^33 yrs …[and]… 1.7 x10^32 yrs … These constraints are now sufficient to rule out minimal SUSY SU(5) …"
and
thus making even non-SUSY SU(5) GUT a viable component for building unified physics theory models.

PPS – Nothing in the above is intended to be an attack on S. Raby or Ohio State. The above-cited Review by Raby is very interesting and important. Even so, it should be possible for civil people to "agree to disagree", and any disagreement over how such things as dimension-6 operators work should be seen as furthering understanding, NOT personal competition.
Also, I like Ohio State and hope they win the USA football national championship (oh dear, now I have to say that I really don't fundamentally dislike Michigan, Louisville, Rutgers, Boise State, etc).

51. **JC**
November 9, 2006

Bee,

Most people don't know what they want out of "life".

By default, many people end up choosing obvious things like money, power, etc … In general, it tends to be things which can be "measured", and which have a "currency" in a particular group of peers, such as citations. Outside of physics, or academia in general, a person's citation numbers has very little to no meaning. The average anonymous person walking by on the street does not care about how many citations a particular physicist has.

I don't have any idea as to what the "meaning" of life is. For me, it is certaintly not string theory nor physics/math these days. I suppose if I still had a "fanatical" mindset, I probably could make something like string theory the "be all and end all" of my life. When I was younger, I was fanatical about all kinds of stupid things like rock bands (ie. Pink Floyd, the Scorpions, KISS, the Michael Schenker band, etc …).

52. **Chris Oakley**
November 9, 2006

Bee,

What you describe is unfortunate, but is hardly inexplicable. It follows from the fact that most academic research is, indeed, academic: in other words, society does not really care about the result. You will notice that when it does – e.g. semiconductors, or some kinds of medical research, a system more based on results and innovation emerges. The fact that research in theoretical physics is infinitely kind to useless, tenured old fossils and infinitely harsh to bright, young, sceptical people is because the first group have been allowed to dictate the terms.

Until about 1850 Oxford did not allow dons to marry: when they married they would leave, typically becoming country parsons or lawyers. This was healthy for the research environment, as it kept the average age of researchers down. Whilst I am not recommending a similar system for theoretical physics – which would exclude you already (and I doubt in any case that anything will really change) I have no doubt that a system that gave the whip hand to young people like yourself would produce better research.

I am only sorry that I am not in a position to do anything about it.

53. **r hofmann**
November 9, 2006

Dear Sabine,
but I like the cover of Smolin's book, and in particular 't Hooft's
comment, and I started reading the book as soon as it became
available. Otherwise I stand in awe of your honesty.

Gruesse von Ralf

P.S. Wir sind uns kurz ueber den Weg gelaufen, als ich anfing, von Frankfurt Geld zu beziehen und Du nach Arizona (?) gingst.

54. **Chris Oakley**
November 9, 2006

Apologies in advance for my rotten German, but …

Ich suchte ein anderes String theory Wortspiel hier, aber war enttäuscht.

55. **Bee**
November 9, 2006

Hi Chris,

Thanks for the link. I think I've just learned a new word: to denigrate. I should try to memorize it, it might be useful at some point. Totally off-topic: My first association to 'strung out' was this song (which nobody else seems to know). Which is not a bad song, but maybe explains why I find the title totally whack.

I actually agree that the society mostly doesn't care about what we do or don't. I think G. Johnson made a comment like this in the first part of his talks which went like: the public doesn't care one way or the other. Which caused quite some laughter, he looked pretty puzzled. I don't think this was meant as a joke.

But if you look on the 'large scale structure' of our daily struggles, and forget about the nasty details, the 'society' does of course care about our aims, trying to find questions to answers like: where do we come from, where do we go to, what are we made of? Of course that more general part of our work has to be communicated, that's why I think Peter Woit, Lee Smolin, Lisa Randall, Brian Greene, etc do a very important job with their books.

On the other hand, this 'not caring about the details' gives us fortunately room to organize our own community, set our standards, and ensure quality – which is hard for the public to judge on. Just that we haven't payed sufficient attention to that lately.

Dear Ralf,

AAH 🙂 🙂 Jetzt faellt der Groschen! Klar, Du hast mal an Stabilization of RS models gearbeitet und in Frankfurt irgendwas ueber Kinks erzaehlt (und keiner von den Heavy Ion Fritzen wusste wirklich worums geht)? Spaeter waren wir zum Nach-Colloq Dinner im Bastos? Bist Du noch in Frankfurt? Gibts das Bastos noch?

Best,

B.

56. **anonymous**
November 9, 2006

Various people here complain that the job market is too tough. But this is the same as complaining that salaries are too high: the two things are linked. Positions that would allow joung scientists to do long-term research without worrying of being competitive must have a salary a few times lower that what Tony Smith suggests. Otherwise people would start competing for these positions.

57. **Who**
November 9, 2006

Ralf Hofmann says
but I like the cover of Smolin's book, … Otherwise I stand in awe of your honesty.

the color is the blue of links. If you look at the date underneath the poster's name—right here—the book is that blue.

about things that matter, I admit she is awesomely honest, but if it doesn't matter she is able to feign insults for reasons of diplomacy and vocal balance.

when you point out 't Hooft's mention of "the sweet vagueness of quantum mechanics" you cause me to admire your ear for English style

high regards

58. **Bee**
November 9, 2006

Hi JC,

Most people don't know what they want out of "life".

By default, many people end up choosing obvious things like money, power, etc …

Yep. Many people go where money goes. That's why there's a discussion about the funding in theoretical physics… I don't know what I want out of life either, but I think that many scientists have a pretty good idea what they want out of science. Just that they don't have the opportunity to realize that. And sometimes just are afraid to speak out, in order not to appear naively idealistic, unrealistic, philosophically biased, or – well, maybe "pot-smoking hippies" 😉

I don't have any idea as to what the "meaning" of life is.

Maybe its better this way. What if you knew the meaning of life but didn't like it?

There is a theory which states that if ever anybody discovers exactly what the Universe is for and why it is here, it will instantly disappear and be replaced by something even more bizarre and inexplicable. There is another theory which states that this has already happened. ~ Douglas Adams

Hi Who,

the color is the blue of links. It's not. It has a definite green shade, I think its close by RGB#009dfc. In fact, I think there's an oil color that comes close: Coelinblau (sorry, don't know the English translation).

Best,

B.

59. **TheGraduate**
November 9, 2006

The internet says this is Coelinblau:

http://www.schmincke.de/media/images/news_ac_struktur/farbflaechen/coelinblau.gif

60. **TheGraduate**
November 9, 2006

further update: it means "Cerulean blue" (a greenish blue pigment)

61. **a.k.**
November 10, 2006

to quote Bee:

"Just that they don't have the opportunity to realize that. And sometimes just are afraid to speak out, in order not to appear naively idealistic, unrealistic, philosophically biased, or – well, maybe ''pot-smoking hippies."

I remark that I disagree to the implicitly given perspective that the main problems of modern physics root in the problem of formally preventing scientists to transform their original ideas of what 'life' should be into ideas of what 'science could be'. The search for an answer to 'large scale questions' of life or existence by using the method of 'exact sciences' could in itself be the germ of corrupting both the method and the aims. I tend to cite Horkheimer and Adorno in these respects who already objected 50 years ago a tendency of 'modern rationality', of the phenomenon 'Aufklärung' itself, to contain in a sense self-induced and inevitable regression, the point where it is undecidable where science turns into mythology and mythology turns into science. I strongly support the viewpoint that high energy physics today, i.e. string theory have an inherent tendency to represent mythological symbols of human existence in a quasi-rational language and that it is to a large degree undecidable where the scientific methods being constrained by falsifiablity and predictiveness, turn into belief. In this sense, I support the more radical view of Bert Schroer in his judgement of the state of string theory or JC's viewpoint of identifying a 'fanatical mindset' to judge the borders of what can still be called science and believe at the same time that these borders will define what was originally meant to be 'progress'.

62. **Bert Schroer**
November 11, 2006

Fortunately the "perfect" world of Bee never took hold of physics, not now and certainly not in those days of great progress in particle physics of Pauli, LSZ et al.
The big difference from pre- ST times and now is that irony, sarcasm and good polemics served the course of particle physics. When my boss Harry Lehmann told us (at the time when the Heisenberg nonlinear spinor theory entered the media): great news, the spinor theory was able to successfully compute the relative sigma-lambda parity up to a sign. Pauli was know for his acid humor, his own personality included. 4 weeks before his death, after a beautiful colloquium he gave in Hamburg, when he worn out and tired sat in his rotating chair which he used in the 20s (called the Pauli chair) and fell into his swaying movement like an orthodox Jew (the Pauli eigen-frequency) obviously in physical pains (4 weeks later he died of stomach cancer) he said: "I think that Heisenberg lies still heavily in my stomach". This was the condensed form of his previous withdrawn support of Heisenberg's spinor theory which he still presented and defended during a previous lecture tour through the US and it had more scientific content than 1000 words.
In the times of ST this has been substituted by foul language ("crackpot") void of any meaningful content. Can you imagine Witten using irony? Even Lee Smolin's "bel-esprit" soft-spoken style is a new phenomenon. Contrary to the pre-ST ways it is verbose and often repetitive. No Bee, I prefer not to be part of your nice new world.

63. **Bert Schroer**
November 11, 2006

I forgot to add:
without the information-laden irony of Pauli, the title of Peter's book would be at least as long as that of Lee Smolin's. There is nothing in the ST nomenclature which could match this, even the new ironic use of "a theory of everything" does not quite match the old style (and runs the risk of being misunderstood by the young ideologues).

64. **Bee**
November 11, 2006

Oh, great, I see you've promoted me into proclaiming the perfect world. I should maybe go and sympathize with the pot smoking hippies?

Dear Bert,

Fortunately the "perfect" world of Bee never took hold of physics,

*The big difference from pre- ST times and now is that irony, sarcasm and good polemics served the course of particle physics. […] Pauli was know for his acid humor, […]In the times of ST this has been substituted by foul language ("cra*kpot") void of any meaningful content. Can you imagine Witten using irony? Even Lee Smolin's "bel-esprit" soft-spoken style is a new phenomenon. Contrary to the pre-ST ways it is verbose and often repetitive. No Bee, I prefer not to be part of your nice new world.*

I completely fail to see how our personal sense of humor influences the way science works. I hate to point it out, but did it ever occur to you that sense of humor, and the way to lead arguments, differs from country to country? Your problem might just be that the particle physics community has changed in its composition. E.g. if you considered your text to be ironic, I am afraid I've been in the US for too long. On the danger of distributing prejudices, in my opinions Americans are just POLITE (in capital letters), and if I should describe their sense of humor, I'd call it nice and naive. I definitely do prefer British humor. Imo *Lee Smolin's "bel-esprit" soft-spoken style* isn't so much a new phenomenon, but an American phenomenon. You wouldn't believe how many times I stepped on somebody else's toes trying to make a joke I considered to be fairly innocent, but which was perceived as plain offending (or not a joke at all).

I'll give you another example where cultural differences play a big role. As I had to learn some while ago (correct me if I'm wrong), it's apparently not polite for a Japanese to say 'No'. So, they'll try to get around saying so in a straightforward way. If you don't know how to interpret that, you'll get a completely different impression of their way to answer questions, which might come off as incompetent. On the other hand, they'll likely be pissed off because they feel misunderstood.

Another point: yesterday there was a seminar here, which I missed, but several people excitedly reported to me that X had a fairly loud argument with Y. So. Back in Frankfurt that happened all the time. I've seen Russians working together, which apparently just wasn't possible without yelling at each other, and occasionally slamming doors. Go try that with an American, and say good bye to your paycheck (alternatively, you might end up in therapy, not a joke, I know a case where that happened).

I would summarize it by saying that globalization has changed the scientific community. If we don't take cultural differences into account, it won't work. In that respect, scientific research isn't so much different from the industry. Again, this is another point where our community needs a management that's missing (how about giving seminars on cultural differences to your international coworkers and how to acknowledge these).

But that's definitely not the problem I tried to point out, and it seems to me you're talking about something different altogether.

I agree with you that our community suffers from a lack of criticism. The reason for which I see in the necessity to advertise the own work as good as possible, to make a big fuss about successes, and keep silent about problems. Read a randomly picked abstract of a paper. How likely will you read a sentence like: We've tried the approach zing-zong to the problem soundso, and show that it doesn't work for the reason blahblahblah. Trial AND ERROR is a big part of science, but in times of short funding it's become common practice to just not mention any drawbacks (or if so, then not in public) .

That is a problem. And it's a big one. I agree on that. It is necessary to have constructive criticism. But it's definitely not necessary to do that in a specifically 'Paulian' way.

Best,

B.

65. **Bee**
November 11, 2006

Bert, regarding humor, see also The Stupid Title List. Maybe we're not such a hopeless case, eh*?

*trying to feel Canadian

66. **Bee**
November 11, 2006

Hi a.k.

I remark that I disagree to the implicitly given perspective that the main problems of modern physics root in the problem of formally preventing scientists to transform their original ideas of what 'life' should be into ideas of what 'science could be'. The search for an answer to 'large scale questions' of life or existence by using the method of 'exact sciences' could in itself be the germ of corrupting both the method and the aims.

Did I say that? If it came across as such, this is a misunderstanding.

What I tried to communicate is that I strongly oppose to shoulder shrugging when one faces a mismatch between 'that what is' and 'that what could be', by attributing it to an undefined concept like 'human nature'. Thereby one implicitly assumes that the own opinion is a minority opinion, and as a single person one can't do anything about it one way or the other. It's like cursing capitalism because the health care of low-income families sucks, but then shrugging shoulders and saying 'that's the way it is, and capitalism is in the human nature', instead of remembering that democracy means you can do something about it.

To come back to science, when I talk to colleagues I notice that there is a broad consensus that 'what is' is not 'what could be', but then again, who does something about it? This applies to issues raised in Lee's book (eg. who hires whom how, and are the presently used criteria optimal? why has it become necessary to work on 'hot topics' with somebody-famous), but in every day's work also e.g. to the peer review problem, or the completely ridiculous requirement that research proposals essentially have to explain what results you will have after 5 years. The recurring question about what the citation index actually says. Or, to come back to what Bert has addressed: that the importance of constructive criticism isn't sufficiently acknowledged.

It is in this context that I think most scientists do indeed have an original idea that is currently in conflict with reality, and I want to encourage them to do something about it. My remark above referred to the way science is pursued, not to the content of actual research fields (in which I agree that a scientist's original idea can very well be in conflict with reality, and that matching the one with the other would mean *corrupting both the method and the aims*)

Best,

B.

67. **Bert Schroer**
November 11, 2006

Well Bee, that's really funny especially in those cases where the humor is involuntary. In Sid Coleman's case it is probably functional humor related to the content, one remembers very nice titles of his work in the 70s like "the double well done doubly well". In Susskind against Laughlin it is probably an attempt to ridicule an unloved detractor of ST (but I did not open the article).
In your previous blog you put to much weight on US hypocritical behavior. Although you do not use this term it is precisely this what you have in mind. That is of course quite singular because it is such a normal everyday behavior in the US that it almost goes unnoticed. When Bush says after Abu Ghraib in front of international cameras with this slight cough in his voice: "we don't torture, the US does not torture" and the surprised rest of the world asks itself: well if the US did not do it, who was it? Didn't they have the supervision over Abu Graib?
this is an illustration of hypocrisy driven to perfection; the guy does not even notice the contradiction between his claim and the facts. However this sort of behavior I have never experienced during my 10 year stay within the academic surrounding in the US; and although one may criticise string theorists in the way of Phil Anderson, I do not think they can be accused of hypocrisy this is not part of the US academic cultural traditions. Japanese politeness is a cultural trait which includes academia but it has nothing to do with hypocrisy. Russian academic discussion tend to be a bit more rough than German, but as a result of the long reign of the Soviet system there was always a private political frankness. During the time of the Russian Afghanistan campaign it would have been inconceivable to be told by a Russian conscript in private that he fights in Afghanistan for the protection of his country or any other "just" course. But when the preemptive war in Iraq started probably most of the GIs internalized that they were bearers of freedom and democracy; even Witten believed that the war was justified by the argument of regime change.

68. **Bee**
November 11, 2006

In your previous blog you put to much weight on US hypocritical behavior. Although you do not use this term it is precisely this what you have in mind.

I don't. I think I made quite clear that I was pointing out there are different ways to argue, and I am not willing to start a discussion about George W and torture here, which doesn't have to do anything with my concerns about theoretical physics. Best, B.

69. **Bert Schroer**
November 17, 2006

Dear fellow physicists
I stop and say good by to all who engaged me in interesting discussions. It is not only the spam-filter which irritated me but I find this whole enterprise unworthy, regimented to rules which never were spelled out, and at the end and plainly ridiculous.

70. **Benni**
November 25, 2006

Edward Witten admits indirectly that string theory is in trouble:
He writes:
http://schwinger.harvard.edu/%7Emotl/witten-nature-letter.pdf

He Writes about, what scientists can answer to critics of science, indirectly referencing the case of string theory.
He writes, that he cannot answer the critics because this would add fuel to the discussion (maybe because he has no arguments). And explains that he thinks scientists should keep doing what they are doing and he hopes, some kind of science may might even be useful one day. He is clearly indirectly referencing to stringtheory here. And the case that "it even might be useful one day" is not an optimistic one.
The words from wittel below could be understood as a confession of failure. Or at least, as a not optimistic at all.

He writes:

The News Feature concerns radical environmentalists and
animal-rights activists, but the problem covers a wider area, often involving more enlightened criticism of science from outside the scientific establishment and even, sometimes, from within.

Responding to this kind of criticism can be very difficult. It is hard to answer unfair charges of élitism without sounding élitist to
non-experts. A direct response may just add fuel to controversies. Critics, who are often prepared to devote immense energies to their
efforts, can thrive on the resulting 'he said, she said' situation.
Scientists in this type of situation would do well to heed the advice in Nature's Editorial. Keep doing what you are doing. And when
you have the chance, try to patiently explain why what you are doing is interesting and exciting, and may even be useful one day.

71. **Peter Woit**
November 25, 2006

Benni,

I saw the Witten letter. Hard to know what exactly to make of it, since he's leaving it very unclear what this all has to do with the string theory controversy. In particular, the argument I'm making against string theory is not that the problem is elitism. I'm actually all in favor of elitism myself.

72. **Benni**
November 25, 2006

I think the sentences on elitism are simply because he seems a little bit depressed on what others say on his beautiful theory. They are simply because he is in bad mood.

I further think Witten says clearly what he wants. He says that string theorists should not discuss with critics like Smolin (this is the one who makes criticism from within science, because he has published actively on Quantum Gravity).
String theorist should, according to Witten, not discuss with critics because then, the situation for their subject would even go worse.

Witten answered to this nature article, because there was advice on scientists what to do with critics. Witten seemingly feared, that some string theorists would answer Smolin…..

All in all, when the only thing you can do is: proclaiming that "what you do might even be usefull at one day"…
You are actually saying (with the phrase "even might be") that there is a very good possibility that it won't be usefull at all. The man who writes here has clearly lost the last bit of optimism and admitts that he does this only becauese he likes the math.

73. **anon.**
November 26, 2006

"I'm actually all in favor of elitism myself."

Indeed.

74. **ScienceLover**
November 29, 2006

How can someone be elitist who leads a widely known public weblog? These media are exactly what blurr the distinction between a small elitist, self-selected community and a public audience delivered by press-speakers and mass media journalists. The silent readers as well as the participant form a new kind of public. I wonder a little about Wittens paternalistic attitude. What is science becoming after abandoning the englighted reader? A secret society with clan tattoos and corporatist ideals that tries to fascinate the people by the high fence they are living behind?

75. **Who**
November 29, 2006

ScienceLover you make several good points in a short post. I look forward to seeing more.

"…paternalistic attitude…abandoning the enlightened reader…fascinate the people by the high fence…"

To try to answer your rhetorical first question, perhaps one can distinguish between open-debate elites and sheltered inscrutable elites.

In both cases, the top people get to guide policy and influence how the money is spent, but in the open case top scientists recognize they have a responsibility to discuss differences of opinion in public—to educate the public, to explain, to reason honestly in the open.

Someone who says he favors the research establishment being run by elite, but who also has a blog that spotlights valid differences of opinion within the expert community and in a sense forces more open discussion, may be acting consistently.

Links

November 8, 2006
Categories: Uncategorized

Starting tomorrow there's a workshop in London entitled M-theory in the City, in some sense celebrating the 11th birthday of M-theory. There will be a reception on Thursday evening, and the organizers of the workshop are noting that:

Recently there have been a variety of publications presenting a sceptical view of string and M-theory. These have been reported extensively in both the national press and various popular science journals.

and encouraging journalists interested in this topic to attend the reception and use:

the opportunity to discuss with the participants and question where string theory is heading and address the recent criticisms string theory has faced.

Various and assorted quantum gravity news:

The latest Physics Today has an article by Lee Smolin entitled Quantum Gravity Faces Reality (available only to APS members). People concerned about open access to the scientific literature should note that sometimes professional societies like the APS are among the worst offenders. It appears that Physics Today is one of relatively few scientific publications that universities and other institutions are not even allowed to buy electronic access to. I've been told that this restriction of electronic access to subscribers is an intentional tactic of the APS to keep up its circulation figures and thus advertising rates.

The latest Nature Physics has a report from Ashtekar about recent developments in loop quantum gravity.

There's a new paper on the arXiv by Baratin and Freidel that looks quite interesting. It's too bad that Christine Dantas has given up her blog that provided an excellent location for discussion of this kind of quantum gravity research. I hope someone else will pick up where she left off. Blogger Sabine Hossenfelder has a recent arXiv preprint on Phenomenological Quantum Gravity.

I heard from my sister-in-law that NPR yesterday ran a segment on string theory, but it was mostly about soccer. I found this hard to believe, but she was right, the story is on-line here. NPR's Richard Harris covered a soccer game in Santa Barbara between visiting string theorists and laser physicists. The string theorists were trailing much of the game, but finally won on a penalty kick they got due to a misunderstanding by the laser physicists. The story does have some remarkable quotes from string theorists about the prospects for the theory. Steve Giddings "is actually feeling somewhat more optimistic about the fate of string theory these days", arguing that maybe the LHC will start producing strings (the article does note that "even most string theorists say this is a real long shot"). David Gross says that the reason to do string theory is that "…there's nothing else. There's no other game in town." He acknowledges that string theorists don't even know what the theory is, and are out on a limb and trusting in faith:

Even those of us who work in the field aren't really sure what string theory is or what it's going to be, Gross says. *So when you're in this kind of speculative, exploratory science, it's important to have faith because you're out on a big limb. So I think it's really a question of whether we believe this is the right direction; and that I do believe rather firmly.*

Update: Lee Smolin has put up a letter on his web-site in response to queries and criticisms he has received in response to his recent book.

Comments

1. **Jonathan Vos Post**
   November 8, 2006

   Hard to keep score. I'm not sure I understand the relationship between the icosahedral symmetry of soccer balls, Lie groups for glueballs, and the apparent confusion between theory and GOOOOOOoooooAAAAALLLLL!

2. **Bee**
   November 8, 2006

   Hi Peter,

   thanks for the link to my paper. It's not really a paper though, but just a writeup of a talk I gave at the SUSY06. The full paper is hep-th/0603032, and there's also a brief summary of it's content on my post The Minimal Length Scale. Best,

   B.

3. **anon**
   November 9, 2006

   The fact that Physics Today follows such restrictive circulation policies shows. It's sooo umbearably dull.

4. **M**
   November 9, 2006

   a criticism for Smolin, about physics.

   He carefully explains why unification is in trouble with proton decay. Later, when discussing "phenomenology of quantum gravity", he does not mention the Crab nebula constraint (astro-ph/0212190 and later works), that forbids some "quantum gravity" effects suppressed by one power of the Planck mass.

   I would say that both scenarios start to have trouble with experimental data. And of course in both cases one can avoid trouble in the standard way: if the simplest realization has problems, add complictations.

5. **Ejjay**
   November 9, 2006

   you know Peter, the string theorists are like these dilluded pot smking hippies, thinking they have this beautiful theory that will one day cure cancer and bring world peace. And you are like this nasty party-pooping right wing bastard, who slaps them around and telling them that their dreams are unrealistic and silly.

6. **Matti Pitkanen**

November 9, 2006

I have been wondering for more than two decades when people start to take the absence of proton decays seriously and be ready to consider separate conservation of B and L. I am also still wondering why the huge discrepancies in mass scales of fermions (think of top/neutrino mass scale ratio) do not raise the question whether mass scale might be a discrete dynamical variable. The attempt to force quarks and leptons into same multiplets might have looked natural when theoreticians discovered gauge groups but not anymore. In light of proton stability and huge mass scale differences for leptons, I am really amazed hearing people to claim that there is no data to guide theory building. What is data if this is not it?

7. **Tommaso Dorigo**
November 9, 2006

Good point, Matti…
There is indeed tons of data to speculate on. I see a tendency of self-constraint in the theorists in this respect. Why lepton and quark masses are what they are ? Why the values of the CKM ? Oh, sure, the landscape…
T.

8. **Lee Smolin**
November 9, 2006

Dear M,

Thanks for mentioning that. I agree I did not give a complete survey of the field of Planck scale phenomenology in either TTWP or the Physics Today article. My own view, which is consisstent with what you say, is that order l_P effects comingn from Lorentz symmetry breaking are already constrained by existing data, especially the lack of a polarization odd variation in the speed of light with energy. (The Crab nebula constraints are impressive but purists argue that there could be loop holes due to the modeling required.) However, as I hope I've made clear, the experiments leave room presently for possible order L_P effects coming from deforming special relativity.

Thanks,

Lee

9. **data lover**
November 9, 2006

To Tommaso and Matti:

A lot of well known theorists work on/have worked on models attempting to explain masses and mixings of the fermions out there. For instance to name a few, barbieri, hall, murayama, etc… High energy theory is a fast moving field based on what are the most promising and interesting directions at the time, so one shouldn't generalize that theorists aren't interested in these things just because you aren't seeing a whole host of flavor symmetry papers at the current time. However theorists are still interested and well aware of the shortcomings of the SM, however from EFT reasons there are more pressing issues to be solved in the SM, and presumably any succesful mechanism that explains the value of V_{cs} won't be able to be probed at the LHC.

As to the whole point of theorists not having data to guide them, this is a matter of perspective. There of course is a lack of data saying this is what physics there is beyond the SM, because up until now every experiment has always agreed with the SM. On the other hand we have tons of data and everything agrees with the SM which does inevitably guide model building in many ways. What most people outside of the theory community(and some theory savy experimentalists) don't realize is that it is very hard to solve your favorite issue in the SM without already having your solution be completely ruled out by other existing experiments if it predicts new phenomena at energies the LHC will probe!

10. **Tommaso Dorigo**
November 9, 2006

Datalover, I think I do know a little bit about those studies of mass generations. My point is that it is a minor branch of current theoretical effort, while it should by all means be a major one.
And I know it is very hard to formulate things that fit something without being at odds with some other data point. That, in fact, is the whole heart of the matter. The standard model rulez! I bet we will not break it in ten years (I have already, actually). But theory should try to fit the data, try and try. Present-day attempts appear a bit like philosophy to me.

Cheers,
T.

11. **Peter Woit**
November 9, 2006

Ejjay,

Funny, I thought I was the pot-smoking hippie telling the up-tight right-wing bastard string theorists that they needed to see the light, give up their uncool and unworkable rigid ideology, let a thousand flowers bloom, loosen up and try something new and different…

I do like the idea of Lubos as the pot-smoking hippie though.

12. **Mahndisa**
November 10, 2006

11 09 06

Well thanks for linking to Lee's letter, Peter. Although I found it a bit verbose, I was happy that he took the time to clarify his positions. As a student who has learned a lot of this stuff from personal study, it was rather difficult to figure out whose opinions I should listen to or not, as I relied upon more advanced persons for input. After seeing many of his comments on other blogs and reading a few of his papers, I appreciate the sanity of his approach, and his willingness to say that stringy options aren't a total impossibility, but that limiting our minds to only string theory isn't a good idea. Sounds reasonable to me:)

Have a nice day

13. **Pindare**
November 10, 2006

In the meantime Witten is still working on the Langlands program
http://math.berkeley.edu/index.php?module=announce&ANN_user_op=view&ANN_id=63

14. **Pot Smoking Hippie**
November 10, 2006

Maybe if we all got together and got high we'd get along better.

15. **anon.**
November 10, 2006

… because extra dimensions are created by pot.

16. **Tommaso Dorigo**
November 10, 2006

There you go guys, if you put yourself into it you can create a hilarious thread for once! (Of course I read and love all threads here, but I laughed at the last few comments…)

And whoever was it that signed himself as a "pot smoking hippie" I bet it was not what he faked to be… Good try at peace on earth, buddy, but we prefer to keep fighting 🙂

T.

17. **Pot Smoking Hippie**
November 11, 2006

Actually, I'm not a he but a she, and I don't fake anything. 😉

18. **Mahndisa**
November 11, 2006

11 11 06

Hello Peter:
I wanted to bring something to your attention that has been bothering me. You have made a crusade out of pointing out the flaws in string theory methodology and even dedicated threads to Motl. But I have not seen you address his posts which are more pernicious than any 'wrongness' with string theory. If I am mischaracterizing you, or am incorrect, pls forgive. But I am sick of visiting blog sites where people are linked to this guy and his is spewing this racist nonsense. As a physicists, I am really offended that more people haven't delinked him or taken him to task. How in the world can he mentor anyone with these attitudes?

I think that tunnel vision is a bane to science at large, which is why I can appreciate this blog. However, tunnel vision in scientific ideas doesn't even compare to the evil of saying that one racial group is inherently more intelligent than another. If any of the physicists who read this comment have heart, they will consider what I have said because by linking to him, they are tacitly endorsing his screwed up positions.

Lastly, to keep in line with the thread, LQG's newest loopy oscillator by Corichi is quite interesting and gives us some insight into regulating and approximating the fundamental rep.

19. **Benni**
November 11, 2006

Peter, I agree with mahndisa. I also think it is time to delink him…

20. **Peter Woit**
November 11, 2006

Mahndisa,

Lubos is a grotesque racist, and his views on almost every topic are completely idiotic. He does however occupy a very prominent position as someone that string theorists have judged to be among the best young scientists in their community, and many continue to support him. I suggest you discuss his appalling behavior with them, not here.

The fact that I have a link to something in no way indicates any sort of approval of it, in many cases quite the opposite.

21. **Mahndisa**
November 11, 2006

11 11 06

Peter thanks for the response. I left the comment here because I knew it would be read. Also since you link to him I had no issue bringing this up to you. As to the position he occupies among string theorists, I will be leaving another comment on other blogs about this.

Take Care.

22. **anonymous**
November 11, 2006

Mahndisa,

As a very junior string theorist, I can't help but laugh at the contrast between what peter said and what I hear from real string theorists. Lubos is widely viewed as a lunatic in the string community, and people think it was only gross midjudgement that got him a (relatively undesirable, non tenure track) juniorjob at Harvard. I'm sure his colleagues there find him an embarassment. He hasn't been even a junior leader in this field for many years now.

23. **Peter Woit**
November 11, 2006

To correct “anonymous”

Lubos may be “widely viewed as a lunatic in the string theory community”, but this doesn’t appear to be true at Harvard.

As for his job, I believe it is technically tenure-track, although Harvard does rarely tenure people from these positions. The “gross misjudgement” appears to be an on-going one: Lubos was hired as a Junior Fellow (not a job I’ve ever seen referred to as “relatively undesirable”), then after dealing with him for a few years, they promoted him to a junior faculty position, and seem to be keeping him on.

Doubtless many string theorists do consider him an embarassment, but it’s remarkable how hard it is to get any of them to say so publicly and put their names to it.

Remarkably, the publicity material on Amazon for the forthcoming book by Michael Dine about string phenomenology contains a blurb by Lubos. Evidently Dine and the people at Cambridge consider him not to be a “lunatic”, but instead a leading member of the community.

Please though, enough about Lubos here.

24. **TheGraduate**
November 12, 2006

The connection between science and marketting is getting so strong. I can just see the thinking behind this soccer game story. Perhaps I am cynical but it comes across as an ad for string theory. It is not unusual for a company being maligned to then want to humanize itself.Think oil company executives hugging their kids and watering plants.

Apparently now scientists have wised up about this. There was a similar story with Yau in the Times “The Emperor of Math”. It was again basically a sort of soft piece meant, I think, to prop up Yau’s maligned profile. It’s potentially a good cause but is this the right method?

25. **rumor**
November 12, 2006

> ..then after dealing with him for a few years, they promoted him to a junior faculty position, and seem to be keeping him on.

During one of Lubos’s rants he mentions that he only has a few more months left to endure in the Republic of Cambridge.

I think his time at Harvard is done. I’m curious to see what department will be willing to take on such a polarizing figure.

26. **Who**
November 12, 2006

Though age from folly could not give me freedom,
It does from childishness: can Fulvia die?

*
[Antony and Cleopatra Act I scene 3]

27. **William**
November 12, 2006

Pot-smoking hippies? I thought all the string theorists were getting high on LXD (Large eXtra Dimensions).

28. **Tony Smith**
November 12, 2006

Peter, about the cover of the UK edition of your book:

Isn’t the cover particle physics event
the same event
that is shown on the cover of Freeman Dyson’s new book “The Scientist as Rebel” ?

Since Dyson’s book is not (according to Amazon USA) to be released until Tuesday 14 Nov 2006 (2 days from now), I have not yet read it,
but
the Amazon book description says in part:
“… An illuminating collection of essays …
His topics fall into four groups.
The first takes up contemporary issues in science, from cosmology to nanotechnology to global warming.
The second group deals with questions of war and peace, particularly questions of nuclear weapons and disarmament.
The third group is concerned with the history of science, especially physics, with essays ranging from Isaac Newton, to Sir Ernst Rutherford and the discovery of the structure of the atom, to Einstein and Raymond Poincar, to Norbert Wiener, Richard Feynmann, and string theory.
The final section contains more personal and philosophical essays, dealing with such questions as the differences between science and religion, and the relation between science and the paranormal …”.

With respect to the “third group … essays ranging from … to … string theory”:
a 2003 web page at http://www.thirteen.org/bigideas/printable/about.html says:
“… Freeman Dyson returns to provide an alternate, skeptical assessment of string theory. …”.

Since both you and Dyson have “alternate, skeptical assessment”s of string theory, I wonder:

Does the common cover photo event on the two books signify that Dyson regards you as one of the constructive rebels of science ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

29. **Peter Woit**
November 12, 2006

Thanks Tony,

Yes, the two covers ultimately are based on the same source, although I kind of doubt that Dyson was trying to make the point you suggest. One reason this happened is probably just that that image is in the public domain and easily accessible. Look at the interactions.org ImageBank, it's at

http://www.interactions.org/imagebank/search_detail.php?image_no=CE0057

The UK cover of my book was the third try. The first one involved a drawing of a piece of string and seemed a bit too silly, the second one I vetoed since it looked too much like the cover of "The Elegant Universe". At that point I suggested that they forget about strings and Calabi-Yaus and find a purely particle physics image, since that would be more appropriate for the theme of the book. I pointed them to the interactions.org ImageBank web-site, and that's what they came up with.

30. **Who**
November 13, 2006

There is now even a third book cover that has appeared this year with the same image!

http://backreaction.blogspot.com/2006/11/not-even-zong.html

31. **Anonymous 17**
November 13, 2006

I see Lubos Motl's racist comments have been removed from his blog … there are apparently some things that his colleagues (or possibly his administration) at Harvard are unwilling to tolerate.

32. **Who**
November 13, 2006

NEW and TTWP have been #3 and #2 on the UK amazon physics bestsell list whenever I looked today. It's nice to see them doing well. #1 on the list is a new book by Paul Davies called "The Goldilocks Enigma" about why the universe is "just right" for life. Hard to beat a title like that.

33. **Eric Dennis**
November 13, 2006

I am no fan of Lubos, but I've seen no evidence that he is a racist. (Linking to a psychometric study and stating simple statistical facts does not make one a racist. In Lubos' case, his target is not black people but rather leftist intellectuals. Despite much incredulity on the part of the latter, it is in fact possible to disagree with a leftist intellectual for reasons other than that one hates black people.)

This issue is obviously way off-topic but the charge of racism is probably the single most fear-provoking and consequential one in the modern acadamy, and it should not be made lightly.

34. **Peter Woit**
November 13, 2006

Eric,

I don't think Lubos hates black people, just that he thinks they're stupid. He also thinks women are stupid, Democrats are stupid, LQGer's are stupid, environmentalists are stupid, anyone who doesn't believe in string theory is stupid, and that I'm stupid.

But please, enough about him, on the whole it's a really stupid topic, and the last thing I want here is to have to deal with a stupid discussion of racism and "leftist intellectuals".

35. **woit**
November 13, 2006

I'm shutting off comments on this posting due to the behavior of "TheGraduate", who insists on repeatedly reposting the same comment about Lubos, using different names, no matter how many times I delete it. Sometimes dealing with the comment section here is just incredibly annoying due to the obnoxious behavior it attracts.

More Links

November 13, 2006
Categories: Uncategorized

For some insight into the difficulties associated with being an untenured professor and trying to get an NSF grant in astronomy, see this post (and comments) by Rob Knop. For those innocent of the ways of academia, there's a post by Doug Natelson on what a well-organized faculty search is like (both of these via Angry Physics).

This week Witten will be giving two series of talks on gauge theory and geometric Langlands, one at Berkeley, and one at Harvard. Unfortunately I won't be able to make it to either, perhaps someone who does attend some of the talks can report on what Witten has to say, in particular on what might be new since his long paper with Kapustin on the subject earlier this year.

Some of the talks from last month's workshop on axions at the Institute for Advanced Study are now on-line.

Honeywell is sponsoring an educational program involving Nobel Laureates in chemistry and physics, called the Honeywell-Nobel Initiative. They already have material on-line from Leon Lederman and Horst Stormer, and from one of their promotional ads it looks like Frank Wilczek will also participate.

The Perimeter Institute has a new, improved web-site, with many online talks, both scientific seminars and talks for the general public.

The Templeton Foundation also has a new web-site. This month the featured "Life's Big Question" is Do we live in a multiverse?

The European Mathematical Society has a newsletter.

Several people have noticed that the cover of the UK edition of my book is being widely emulated, see here and here.

The folks at Axes and Alleys have a review of my book in their latest issue. It seems that the members of the Royal Tractor Repair and Maintenance Society of Outer Mongolia found the book quite confusing. I guess this is only fair, since I've always found them quite confusing…

In case anyone is worried about the supply of unadulterated hype about string/M-theory drying up, there's a new book out by John Gribbin which I took a look at yesterday in the bookstore.

Since the KITP does such a great job of providing a video record of the activities there, informal talks at the string theory phenomenology program have provided a fascinating public record of the way in which well-known string theorists are struggling with the all-too-obvious collapse of any reasonable hopes for getting predictions out of string theory. Last week there was an informal discussion with Michael Douglas of the String Vacuum Project, which was quite fascinating to listen to. I had trouble hearing what David Gross had to say, which was a shame, maybe someone with better ears who listens to this can report what they hear. Douglas encountered objections from the audience when he claimed that if one could get virtually any low energy physics out of one string vacuum or another, there was not any point to the whole project, with someone making the now standard claim that this situation is no worse than that of QFT.

Comments

1. **anon.**
   November 13, 2006

   John Gribbin's expertise in scientific predictions was first confirmed when at Nature in 1974, he came up with the money-spinning 'Jupiter Effect', which bears an uncanny similarity in some respects to biblical prophecy, alas without the confirmed predictions.

   His book with Plagemann, 'The Jupiter Effect' (1977 edition paperback), stated: 'in 1982, when the Moon is in the seventh house, and Jupiter aligns with Mars and with the other seven planets of the solar system, Los Angeles will be destroyed.'

   Obviously, Dr Gribbin merely omitted to mention that this prophecy was fulfilled in an alternative parallel universe of the cosmic landscape of string theory.

   See http://www.gi.alaska.edu/ScienceForum/ASF5/526.html

2. **Bee**
   November 13, 2006

   Hi Peter,

   Thanks for mentioning the Honeywell-Initiative. Coincidentally, I came across their site today, but didn't realize it's new (I just assumed I had never seen it before.) I found it a bit disappointing though, both in design as in content, but then I guess they'll stock up their database.

   […]a global education initiative designed to connect students across the globe with Nobel Prize winners in Chemistry and Physics… […] The Honeywell – Nobel Initiative establishes a forum for students worldwide to learn directly from Nobel Laureates in Chemistry and Physics

   Does somebody know if their concept of 'global' and 'worldwide' goes beyond those who have an high-speed internet access?

   Thanks also for the link to my blog, I hope you don't mind the joke.

   Best,

   B.

3. **anon.**
   November 13, 2006

   some other anon. wrote:

   "His book with Plagemann, 'The Jupiter Effect' (1977 edition paperback), stated: 'in 1982, when the Moon is in the seventh house, and Jupiter aligns with Mars and with the other seven planets of the solar system, Los Angeles will be destroyed.'"

   Funny, I thought that was when peace would guide the planets and love would steer the stars. I guess that prediction got superseded.

4. **anonymous**
November 14, 2006

it is highly surprising that the "now standard claim that this situation is no worse than that of QFT" is standard. If the situation is no better than in QFT, what is the purpose of studying strings?

5. **Thomas Larsson**
November 14, 2006

with someone making the now standard claim that this situation is no worse than that of QFT.

I think I finally understand what is wrong with this statement. It is true that there is an infinite landscape of QFTs, but it is the simplest ones that are realized in nature. Already free theories give a qualitatively correct picture of photons and phonons, and the simplest interacting theories are also realized in nature. phi^4 theory (Ising model) is the generic critical point and phi^6 theory (tricritical Ising) is the generic tricritical point, and among gauge theories, nature has selected the ones with the smallest gauge groups.

You can in principle realize models deep inside the QFT landscape in nature. E.g., phi^300 theory would be the generic 150-critical point, which can be realized experimentally by fine-tuning 150 parameters, and the 150th RSOS model exhibits such behaviour. But although such complicated models exist in theory, they don't exist in reality.

In contrast, the simplest string theories are deeply unphysical: neither 26 flat dimensions and tachyons, nor 10 flat dimensions, unbroken SUSY and 496 gauge bosons are compatible with observation. There might be complicated models in the ST landscape which agree with experiments (or rather, have not yet been ruled out), but the big difference compared to QFT is that the simple string theories are wrong.

6. **spacepig**
November 14, 2006

Anonymous and TL:

It is implicit that by going to string theory one has solved also the problem of quantum gravity. And, the people making this claim, all have in mind that (as with QFT) one should look for some new "stringy" phenomenon that determines which class of vacua is correct. Having observed this phenomenon, string theory fits the data (including gravity), QFT does not, and one used experiment instead of magical divination to find the correct description of nature.

Many people try to get you to falsely see the issue as: "strings have a landscape, therefore we'll never derive the correct vacuum without unforeseen breakthroughs." That is not the issue. We never derived the correct vacuum in QFT either, we found it by experiment.

The only obvious tuning that is required is for the CC. And that is NOT just a feature of string theory, it is a feature of ANY theory we currently have for the CC. So to say it is analogous to requiring a highly tuned phi to the 150th QFT is just wrong. Strings do not add any tuning that isn't there in all approaches we have to the current data. And it solves problems that other approaches don't.

The whole landscape discussion in this sense has been deeply misleading. A feature (that one can make more realistic models) has been turned into a bug (how do we ever derive which of the much better models we now have is correct??). Of course that has been done on purpose, both by Peter and Lee (who dislike string theory),and by those string theorists whose dreams of changing science into an exercise in mathematics ("derive the world from thought alone!") has been seemingly stymied.

7. **woit**
November 14, 2006

pig,

You're engaging in pure sophistry, not science. A scientific theory is supposed to predict the result of an experiment. String theory doesn't do this, and the nature of the landscape is the main reason why.

Here and at Clifford's blog you spend your time anonymously attacking not only me, but those string theorists (Dine, Taylor, Douglas) who are actually trying to do science and get a real prediction out of string theory. They're not dreaming of "changing science into an exercise in mathematics", they're trying to do what scientists are supposed to do, come up with a prediction of what an experiment will see so the theory can be tested. What they're finding is more and more evidence that this is impossible.

8. **John Baez**
November 14, 2006

Peter writes:

> A scientific theory is supposed to predict the result of an experiment. String theory doesn't do this, and the nature of the landscape is the main reason why.

I think it's wiser to take spacepig at his word. He says the problem of determining the correct vacuum in string theory is comparable to the problem of finding the right Lagrangian in quantum field theory: we should give up on "deriving" it; it should be determined by experiment. That's a consistent position, though a strong retreat from the long-held hope that string theory would relieve us of this need.

If we take this position, we should ask whether it's *possible* to find a string theory vacuum that matches existing experiments.

As far as I can tell, the vacua most people like to study don't work: they are manifestly supersymmetric, so they predict that for each boson (e.g. the photon) there should be a fermion (e.g. the "photino") of the same mass, and vice versa. None of these "superpartners" have been seen. So, for string theory to have a chance to match existing experiment, people need to find vacua that aren't supersymmetric.

One approach is to come up with a theory of "spontaneous supersymmetry breaking" which could save the vacua that people like from being irrelevant to physics – but as far as I can tell, this is very hard, so not much progress is happening in this direction.

It seems the hard-core theorists mainly leave this crucial problem to "phenomenologists", who solve it by throw in *ad hoc* "soft supersymmetry breaking terms" into the field theories that can be derived from string theory vacua. To get theories similar to the Standard Model, this requires choosing over 100 new numbers. I don't like all those free parameters. And what I like less is that nobody knows where these "soft supersymmetry breaking terms" should come from. As far as I can tell, they're just stuck in by hand, out of desperation at being unable to solve the supersymmetry breaking problem in a principled manner.

In short: if string theory now demands that we choose a vacuum to match existing experiments, it would be really nice if someone could *find* a vacuum that match existing experiments!

Until then, Michael Douglas' claim that string theory is "no worse" than ordinary quantum field theory seems overoptimistic. Yes, we can get gravity, but no, we can't get matter that matches what we observe – not without extra *ad hoc* assumptions that nobody is able to derive from string theory.

Saying we can get "virtually any low energy physics out of one string vacuum or another" also seems overoptimistic. We can get lots of different choices of low energy physics, but none of them match *our universe* – until one throws in extra "soft supersymmetry breaking terms" by hand.

This at least is my impression. If progress has been made on this since I last checked, I'd love to hear about it.

9. **Bert Schroer**
November 14, 2006

The view of spacepig about QFT is a caricature seen through stringy eyes. It has nothing to do with what QFT really is. It starts with his metaphoric inflationary use of the terminology "vacua".
In QFT you have a dichotomy of algebraic structure and states. A theory is given by the spacetime-indexed net of observable algebras and its "states of physical interests" can be classified and hence belong all to the same theory. If there is a spacetime symmetry group acting on the algebraic net one can define a distinguished invariant state, the vacuum state, if there is no global symmetry (as in the generic QFT in CST situation) there is no distinguished reference state at all (in particular no vacuum). The total state space of the theory is a direct sum of "superselection sectors" (no coherent superposition between sectors) and the vacuum (if present at all) only belongs to one sector.
In a reasonable way of counting there is only a finite number of perturbative renormalizable theories and renormalizability in the standard sense does not go beyond spin 1. There are quadrilinear couplings between scalar fields and Yukawa as well as gauge couplings up to spin 1, i.e. 3 types of couplings. The only remaining feature which one can play around with are multiplicities (different SU(n) or O(n) symmetry groups) but nobody with a rest of physical reasoning left would compare this to the different physical principles which underly the different string vacuua. QFT (even outside that perturbative setting) is bound together by testable physical principles (e.g. dispersion relations for scattering amplitudes); what bind string vacua together is metaphoric garbage.
In order to save QFT from such a metaphoric sellout by ST, I posted a paper in todays hep-th.

10. **woit**
November 14, 2006

John,

As Thomas pointed out, and I've argued repeatedly, the difference between the string vacuum case and the QFT case is that the simplest of a beautiful class of QFTs (gauge theories) works incredibly well and makes a huge number of confirmed non-trivial predictions, while the simplest string vacua are ruled out, and one has to go to ever more complex constructions, just in order to match some of the crudest features of the standard model.

The problem with supersymmetry breaking terms is that you don't even know what they are, just that they have to be big enough to avoid conflict with experiment. People seem to have string vacua that do have large enough amounts of supersymmetry breaking. It's true that the state of current technology is that no one can do accurate calculations of most things in these vacua, but as far as I know there is no known reason that these things can't give the SM at low energy. And if one vacuum gives the SM, all indications are that there will be essentially an infinity of others, ruining the hope of making any predictions.

One could argue that one should pursue these studies as a way of potentially falsifying string theory. If one could understand all string vacua and see that none agreed with the SM at low energies, that would be something. Unfortunately, understanding in that detail "all string vacua" looks hopeless, and no one has seen any kind of argument that would allow a general conclusion that some aspect of the SM can not be matched by a sufficiently complicated construction.

11. **spacepig**
November 14, 2006

John;

Thanks, you got my point exactly. I do not mean to be attacking anyone in a personal sense, I really think the quest to "derive" predictions from string theory would be rather like the quest to "derive" the correct Lagrangian for nature. (It is a bit different, in that the different Lagrangians are a superselection issue while in string theory the vacua are part of one theory, but this is irrelevant given the cosmological issues involved in probing one vacuum from another).

It is not true any longer that string theorists study only supersymmetric vacua. Much of the fuss about the KKLT paper was that they proposed ways to break supersymmetry while nominally retaining computational control (and keeping the scale of breaking quite low, perhaps low enough to explain the hierarchy). This has been expanded upon in many works which explain how to break supersymmetry, in an apparently controlled way, in the framework of string theory. By now there is a huge literature on this, and most of the landscape papers that Peter and others deride, are actually working steadily to improve the technology for supersymmetry breaking in string theory. The most recent burst of activity in this regard has been tying the Intriliator-Seiberg-Shih models to string constructions, but there are many other approaches too.

12. **Anon**
November 14, 2006

Bert,

What do you make of QFT's like pure N=2 supersymmetric SU(2) Yang-Mills, which have continuous families of inequivalent choices of vacuum state (superselection sectors with inequivalent physics)?

13. **Bert Schroer**
November 14, 2006

John, it is simply not true that hard-core theorists do not look at the question at spontaneous SUSY breaking. The result is that it cannot be spontaneously broken in the sense as e.g. the Lorentz symmetry is broken in a thermal medium. They found such under such circumstances it "collapses" (i.e. there is no averaging which can save the symmetry in a highly mixed state, see my hep-th paper for more details and references).
Again I emphasize that QFT models are bound together by common model-independent testable (and tested!!) common principles. This is a hell of a difference to the "vacuum problem of ST".

14. **spacepig**
November 14, 2006

John:

I don't know what Bert is talking about, but standard references which
discuss the issue of supersymmetry breaking include reviews like:

Poppitz and Trivedi, hep-th/9803107
Shadmi and Shirman, hep-th/9907225

The subject developed after Edward Witten described the possibility of dynamical breaking of supersymmetry in the early 1980s, in the very famous paper Nucl Phys B 188 p. 513.

As I said a significant recent effort has been devoted to making such models in string theory in conjunction with other moderately realistic features, and it seems pretty clear that one can do this, by now.

15. **Bert Schroer**
November 14, 2006

The word "spontaneous breaking"has a very precise conceptual and mathematical meaning. Its consequence is that by forming mixed avaerage states one still can recover symmetry (albeit in an artificial mixed state which violates cluster properties). Such an averaging does not exist for supersymmetry (the only known exception).

16. **spacepig**
November 14, 2006

Well, Bert, the references I gave are for the standard usage. These theories are thought (by e.g. the thousands of people who work on string theory and also go to SUSY 200X conferences each year) to be theories whose Hamiltonians are supersymmetric, but which do admit admit a supersymmetric ground state. Or in some cases (those with metastable susy breaking vacua), theories which possess both supersymmetric ground states and long lived metastable quasi-ground states.

It is quite possible you have some "rigorous" meaning in mind, which I do not know about, but which is also not thought by the many phenomenologists who work on the MSSM, to be all that relevant. I suspect when they discover SUSY and some structure of soft terms at LHC, no one will care much about the axiomatic QFT existence of the definition of breaking which you are worried about.

17. **spacepig**
November 14, 2006

erratum:

I am sorry, I meant "…but which do not admit a supersymmetric ground state." Somehow I typed admit twice.

18. **Bert Schroer**
November 14, 2006

There is nothing "axiomatic" about a clear conceptual distinction between spontaneous symmetry breaking and no symmetry (=broken symmetry), you should not try to push common sense into some axiomatic or high-brow corner.

19. **spacepig**
November 14, 2006

The standard usage of "spontaneously broken" that I know of, states that a theory exhibits spontaneous breaking if the hamiltonian exhibits a symmetry that is broken by the ground state.

That is the sense in which I was using the term.

If you were using it in a different or more precise sense, the theories I mention may not exhibit spontaneous breaking by your definition. They do by the definition I gave.

This is important: it means that in the theories I mentioned, the divergence structure is not sensitive to very high energies for certain processes. E.g. corrections to the vacuum energy, are cut off at the scale of supersymmetry breaking. So in principle it is finite and calculable.

20. **Bert Schroer**
November 14, 2006

This is not quite the correct definition. The diffeomorphism group of the circle e.g. does not leave the conformal vacuum invariant (only the Moebius subgroup has this property) but this group is by no means broken in chiral theories.
Your definition does not cover the breaking in thermal states. The problem there is that the ground state Hamiltonian is not the same as the one which leaves the thermal average state invariant (one cannot argue here with quantization boxed and Gibbs states because they break the supersymmetry in an explicit manner).
Coming back to the statement of John, there is no mathematical theorem that supersymmetry can never be spontaneously broken in vacuum state (presumably you want it in such a way that the Poincare-group is conserved) but that special "collapse" aspect in a heat bath (where any other symmetry is at most spontaneously broken) makes the whole issue of spontaneous SUSY breaking extremely suspicious. Fermions and Bosons are separated by the strongest superselection rule in this world: the univalence superselection rule. My physical explanation for the thermal collapse phenomenon: you only get SUSY by an extremely fine tuning of coupling strength and such a fine tuning is extremely unstable (more unstable than the fine-tuning problems which SUSY is supposed to solve).

21. **spacepig**
November 14, 2006

OK. Well, supersymmetry can be spontaneously broken by my definition; that is the definition the phenomenologists who work on the MSSM and its extensions use; and theories with that kind of breaking of SUSY lead to the successful prediction of unification of couplings, good dark matter candidates, and a solution of the hierarchy problem. The recent landscape constructions incorporate more or less this kind of supersymmetry breaking into string theory, quite successfully. (Though by no means producing a fully realistic model of the world — who knows if that is possible).

I have no knowledge of, or intelligent comment to make, about your definition.

22. **Bert Schroer**
November 14, 2006

No, the situation remains undecided. Whatever our definitions are, we don't spontaneously break a symmetry rather the question is whether nature permits such phases (symmetric and phases with order parameters).
I think that your picture of breaking "by hand" is caused by a too formal reading of the Schwinger-Higgs mechanism which is not a symmetry-breaking (even though people use this misleading terminology). Local gauge transformations have nothing to do with symmetry and hence cannot be broken. There is a hell of conceptual distinction between the Goldstone spontaneous broken phase and the Schwinger-Higgs description of interacting massive vectormesons even though formally (in the

Lagrangian approach) they look very similar.

23. **spacepig**
November 14, 2006

At this point, I can only make the sociological comment: the rest of the field
has already moved on, so in practice, the issue is decided. Spontaneously broken susy plays a central role in many papers that appear each day on the arXiv. I think this is well justified, but other readers will have to judge for themselves.

Just as the field has moved on (correctly or not), I will too. I have nothing else to say about this subject.

24. **Bert Schroer**
November 14, 2006

o.k. I know that there is a difference between science and sociology and the only thing what happened in this long discussion (which is now finished) is that you confirmed this.

25. **Peter Woit**
November 14, 2006

As far as I can tell, this discussion of the formal status of supersymmetry breaking is irrelevant. Partly because you don't actually have a real underlying theory that does what you want to discuss (no non-perturbative version of string theory), partly because even if it does what you want (break supersymmetry), you end up with all sorts of different classes of models. The simplest of these models that doesn't violate what you already know (the MSSM) has 105 extra parameters in it, and no known reason these cannot have arbitrary values.

Using the word "success" in this context is a bit absurd. The bottom line is that you can't even tell us if the LHC will see anything at all. If it does happen to see exactly the superpartners, you can extract predictions about some of their behavior, but if these predictions don't work out, all you've done is falsified the simplest model, and you have an infinite array of other models you can use to match the data. In that case you're not testing a theory or predicting anything, just parametrizing observations in a complicated way.

26. **Bert Schroer**
November 14, 2006

Fine, so there is after all a scientific conclusion.
Spontaneous supersymmetry-breaking is conceptually extremely suspicious and phenomenologically (in the form of superstrings) totally futile.

27. **JC**
November 14, 2006

Years ago I first bought into the SUSY thing largely from the standard arguments in favor of it, such as dealing with the hierarchy problem, unification of coupling constants, etc … When I came to the realization it would take around 100 free parameters to incorporate SUSY into the Standard Model, that's when I was turned off from SUSY initially. (Before that time, I thought the Standard Model had too many free parameters).

When string theory became popular, I initially thought it could find an easy way to reduce the number of free parameters in the MSSM. But so far that has not happened yet in a convincing manner. At times I wonder why some theorists are tolerant of the idea of zillions of free parameters. Do any string theorists today believe in the idea that string phenomenology can completely determine the free parameters of the Standard Model?

28. **Anon**
November 14, 2006

Finite temperature breaks supersymmetry, just as it break Lorentz invariance (there's a preferred reference frame, in which you are at rest, with respect to the thermal bath).

This is not what people are talking about, when they talk about the spontaneous breaking of supersymmetry (at zero temperature). As spacepig points out, there are plenty of theories, in which the Hamiltonian (or Lagrangian) is supersymmetric, but there is no supersymmetric ground state. Instead, there's a massless Goldstone fermion, which transforms inhomogeneously under supersymmetry, etc…

The real setting, for phenomenology, is one in which one is looking at supergravity, rather than global supersymmetry. The massless Goldstino is eaten by the gravitino, in an analog of the Higgs mechanism.

Bert Schroer will complain that this statement is not solidly grounded in quantum field theory. He is correct. Supergravity (or any kind of quantum gravity theory) is not a QFT, as he understands the term. And, to really define supergravity, it needs to be embedded in some UV-complete theory (like string theory) that he would consider completely beyond the pale.

But I think he misunderstands even the case of globally supersymmetric QFTs, in which supersymmetry is spontaneously broken.

29. **spacepig**
November 14, 2006

Hello:

Wow, the opinions here, about SUSY and its breaking, are totally nonstandard. Anyone trying to "learn" about physics from blogs, beware!
Mr. Schroer has lightly dismissed the work of thousands of papers over a period of 20 or more years, finding theories that exhibit spontaneous and dynamical (spontaneous, nonperturbative) SUSY breaking.

JC: The MSSM does have approximately 125 parameters. However relatively trivial symmetries, which are also required by data (things like R-parity, needed to avoid proton decay), cut down the number. There are a few well motivated scenarios with just a handful of parameters.

It is however a problem of some interest, usually now called the LHC inverse problem, to try and figure out which version of the MSSM with soft breaking parameters is the correct one, given some hypothetical LHC data that confirms SUSY. It is not an easy problem, and indeed because of the large number of parameters, one might have to wait for ILC or some other machine to really nail down the model.

Most string theorists do not believe the use of the theory will be to determine these (kinds of) parameters. It will be useful for phenomenology if and when some stringy signature, like cosmic strings or moduli, shows up in experiments. This is something peter continually refuses to acknowledge, but is a far more likely scenario for contact,

than classifying string vacua and then finding the right one by brute force. It has been useful for more conceptual issues (like singularity resolution and black hole entropy), solving gauge theories, motivating new classes of models, etc. already. In fact it would be hard to gauge the strength of its impact on these subjects over the past 20 years, it has been enormous.
And, from the viewpoint of many established, contributing theorists in
many top departments, it has been very positive.

30. **Bert Schroer**
November 14, 2006

In my case it was not lightly; it would be much simpler for me if I permit myself to use what Feynman called "excuses".

31. **Peter Woit**
November 14, 2006

pig,

I agree that the most likely scenario for making contact between string theory and phenomenology would be if astronomers find a cosmic string, study its properties, and find that it has the properties elementary strings of cosmic size are suppose to have distinguishing them from cosmic strings coming from a Higgs field.

But there is not the slightest evidence for this, and there is no reason to expect it other than pure wishful thinking. It also would be strong evidence for string theory if, when LHC collisions begin, an angel appears out of the interaction region with gold tablets explaining that the multiverse exists and is governed by the laws of string theory. I just don't see any reason to expect this to happen, but you're welcome to put your hopes in it. Just doesn't seem like science to me.

32. **spacepig**
November 14, 2006

The cosmic string scenario seems more likely than the one with angels,
to me. As do scenarios with 5th forces (which is why several groups are doing experiments to detect them). Or scenarios with low string scale (which is why Lisa Randall wrote a popular book that recently won her the Lilienfeld prize, about string-inspired scenarios which solve the hierarchy problem).

Particle physics has always progressed by "model building," not by epiphanies which predict the structure of the whole theory. You can make fun of this and say "there is no guarantee any interesting model will be selected by the data," and you're right. But there was no such guarantee for the past 100 years, and yet, we did fine. The problem for the last 25 has been the lack of experiments that push the energy frontier, the SSC cancellation didn't help, and with LHC that will finally be corrected. I expect the bottom up approach will continue to succeed, with no need for angels, and interesting phenomena will continue to turn up.

33. **Peter Woit**
November 14, 2006

If we see a 5th force it will be quite interesting, but far from evidence for string theory.

I certainly hope and expect that interesting phenomena will turn up at the LHC or elsewhere, I just see no reason at all to expect that they will have anything to do with a 10/11d string/M-theory with something very complicated done to the 6/7 extra dimensions.

Particle physics has not progressed due to model-building itself, it has progressed due to people coming up with models that actually have to do with the real world, based on finding testable, compelling models that both explain things already seen, and predict things not yet seen, which are then confirmed.

That Lisa Randall's popular book has received a prize is absolutely irrelevant to the question of the validity of the models she discusses. You seem very concerned with sociological rather than scientific evidence for models. If someone decides to award a prize to my book, will that prove that string theory has failed as an idea about unification?

34. **spacepig**
November 14, 2006

Peter, you fail to acknowledge that in fact string theory has been an inspiring source of models that have to do with the real world, have testable and compelling features, and predict things not yet seen. None of these models have yet been confirmed. If one is, it will not be proof of string theory. But to pretend string theory has not had this role, is really strange.

35. **Peter Woit**
November 14, 2006

pig,

String theory has inspired all sorts of things over the last 35 years, some of them very important, some of them worthless. One thing it has definitely inspired is some great mathematics. I don't happen to be a big fan of the kind of phenomenological models I think you have in mind; they appear to me to be complicated, and don't actually seem to really explain anything that we observe. Maybe I'm wrong and the LHC will provide experimental evidence for them, we'll see. If so, that would be a positive thing to attribute to string theory research. It wouldn't mean though that the world is 10/11 dimensional, or that string theory ideas about unification have anything to do with reality.

36. **JC**
November 14, 2006

spacepig,

How would you feel if the LHC and all other near-term future colliders (ie. built and running before 2100), only found the standard model Higgs particle and nothing else?

37. **anon.**
November 15, 2006

"… the opinions here, about SUSY and its breaking, are totally nonstandard. Anyone trying to "learn" about physics from blogs, beware!" – Spacepig (comment number 19,194)

Spacepig, you mean to write, surely, "… Anyone trying to 'learn' about orthodox string theory which we claim is physics, from blogs, beware!"

"String theory has inspired all sorts of things over the last 35 years, some of them very important, some of them worthless. One thing it has definitely inspired is some great mathematics." – Peter Woit (second to last comment).

Can you actually substantiate this claim, please? Please give a great equation from string, and show how to solve it. Kaku in a New Scientist essay a year or two ago wrote that he cried when he first saw the elegant simplicity and predictive power of Dirac's equation. Is there anything like that in the stringy Calabi-Yau's? I thought that the whole point is that the failure of string theory is a mathematical failure to actually solve the extradimensional equations because they're so inelegant? Surely there is nothing useful in equations which can have so many solutions?

38. **Bert Schroer**
November 15, 2006

Anon,
this is an issue on which I agree with you; perhaps one can bring it more to the point by the following remarks.
It is not mathematical physics which is helpful for unifying mathematics but rather "physical" mathematics i.e. metaphoric ideas about particle physics which are produced most abundandly in the present crisis of particle physics. This is pretty evident in the way physics ideas enter the Langlands program. It is not the world (say the SM) as it is in nature, but rather the way Atiyah, Seiberg and Witten… want it to jump over their e-m duality stick which is useful for mathematics. The reason why this is successful is that mathematicians need once in a while (especially for unification of their field) to get away from the rigor and activate their free-floating imagination; metaphorical ideas from particle physics are the ideal catalyzer for this.
What is fruitful for mathematics is a dead end for particle physics.

39. **anonymous**
November 15, 2006

spacepig,

you abandon the hope of predicting the vacuum. You abandon the hope of making statistical predictions. You suggest that what theorists did so far is enough. I agree that some scenarios would smell more stringy than others and that finding just the SM at LHC would smell anthropic. But selecting a QFT means selecting a QFT and does not mean proofing that string theory is the theory of quantum gravity.

If you wish to abandon the scientific method, we can close now the issue about string theory with the following pool:

a) it is 26th-century physics that fell by chance into the 21th century;

b) it is 21th-century archeology;

c) it is a fart in 11d space-time.

If instead you want to follow the scientific method, you must provide testable predictions. In my opinion, if this cannot be done, string theory has to dissolve as in c).

40. **Tony Smith**
November 15, 2006

Now that I have a copy of Freeman Dyson's book "The Scientist as Rebel", I see in it some passages that, like its cover, remind me of Peter's description of superstring theory.

Dyson said:
"… At the beginning of the seventeenth century, the birth of modern science had been proclaimed by … Bacon in England and … Descartes in France.
…
According to Bacon, scientists should experiment … and collect facts … until the accumulation of facts would make clear the way nature behaves.
…
According to Descartes, scientists should deduce the laws of nature by pure reason … Cartesian vortices were supposed to fill space … pushing celestial objects along their orbits.
…
At the time when Newton made his discoveries, the learned men of England were mostly doing science in the empirical styple of Bacon, but … believed in the Cartesian theory of vortices because it was the only theory available.
…
In 1667 Newton … resumed his solitary existence in Cambridge. … He spoke to nobody about his alchemical studies, and to almost nobody about his discoveries in physics.
For him, alchemy and physics and theology were parts of a single enterprise, three aspects of a single search for knowledge that God had placed within his grasp. Since he was not free to talk about his theology, he saw no reason why he should talk about his alchemy or his physics.
He might never have talked about his physics, if his friend Halley had not come to Cambridge in 1684 begging him to publish what he knew. Then, once he had started writing … he did not stop until he had finished the three volumes of the Principia. …
In the first two volumes …[of] the Principia …[Newton]… built a grand edifice of mathematics … and then in the third volume he … demonstrat[ed] with an abundance of observational facts that nature danced to his tune.
As soon as the Principia was published and widely circulated,
the Cartesian vortices were dead. …".

If Halley had not convinced Newton to publish the Principia, what would have been the course of science ?
Would others (using Hooke gravity, Leibnitz calculus, etc) have produced a Principia,
or
would Hooke gravity have remained as empirical physics and Leibnitz calculus have remained as mathematics reasoning, with the Newtonian synthesis never occurring ?

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – I guess I should say explicitly that the similarity that I see is that:
Baconian accumulation of facts = Standard Model plus gravity
and
Cartesian vortices = superstring theory
and
"the only theory available" = "the only game in town"
and
maybe a Newtonian synthesis (a model using new or unconventional math techniques that enables calculation of particle masses, force strengths, etc) might be necessary to unify Baconian standard model plus gravity physicists with Cartesian beautiful-math physicists.

PPS – As Dyson's essay also said:
"… Newton himself was at heart a Cartesian …".
That is borne out by the fact that Newton lived his life in accord with an Ovid quotation on Descartes's tombstone:
"Bene qui latuit, bene vixit."
Sadly,
there exist today pressures against unconventional physics similar to the pressures against unconventional theology in Newton's time, and it may be that even today some feel that they have a better life by hiding their true feelings.

41. **Bourgeois Nerd**
November 15, 2006

I've been trying to look at books that might have something to say about strings and say to myself, "What would Peter think?" to evaluate it. I'm not very far into it, but I was actually thinking that Gribbin's book, though undoubtedly pro-strings, was more skeptical than I'd thought it would be.

42. **Jeremy**
November 17, 2006

If you want to read the Axes & Alleys review online and not just in the PDF, you can find it here:

http://www.axesandalleys.com/book-review/

43. **Bert Schroer**
November 17, 2006

Dear friends
I stop and say good by to all who engaged me in interesting discussions. It is not only the spam-filter which irritated me but I find this whole enterprise unworthy, regimented to rules which never were spelled out, and at the end and plainly ridiculous.

More On Geometric Langlands (a Grand Unified Theory of Math?)

November 14, 2006
Categories: Langlands

After mentioning in the last posting that Witten is giving talks in Berkeley and Cambridge this week, I found out about various recent developments in Geometric Langlands, some of which Witten presumably will be talking about.

Edward Frenkel has put a draft version of his new book Langlands Correspondence for Loop Groups on his web-site. In the introduction he describes the Langlands Program as "a kind of Grand Unified Theory of Mathematics", initially linking number theory and representation theory, now expanding into relations with geometry and quantum field theory. The book is nearly 400 pages long, and to be published by Cambridge University Press. Frenkel also notes that recent developments in geometric Langlands have focused on extending the story from the case of flat connections on a Riemann surface to connections with ramification (i.e. certain point singularities are allowed). He has a new paper out on the arXiv about this, entitled Ramifications of the geometric Langlands program, and he writes that:

in a forthcoming paper the geometric Langlands correspondence with tame ramification is studied from the point of view of dimensional reduction of four-dimensional supersymmetric Yang-Mills theory.

The title of the forthcoming Gukov-Witten paper is supposedly "Gauge theory, ramification, and the geometric Langlands program."

Presumably people attending Witten's talks in Berkeley and Cambridge will get to hear about this new story for the ramified case. For the rest of us, on his web-site David Ben-Zvi has notes from talks this summer by Witten at Luminy where he describes some of this. Ben-Zvi also has an announcement of a series of lectures on geometric Langlands that he'll be giving at Oxford next April. The summary of the lectures says that he'll "describe upcoming work of Gukov and Witten which brings together geometric Langlands and link homology theory." Link homology theory is also known as Khovanov homology, and I wrote about this two years ago here, advertising Atiyah's speculation that there may be a 4d TQFT story going on, something I always have found very intriguing. Ben-Zvi has recently lectured on Khovanov homology at Austin, and began his lecture by saying that this material relates "themes in 21st century representation theory" to 4d TQFT. He goes on to cover some of the ideas about 4d TQFT and "categorification" that I was very impressed by when I heard about them from a talk by Igor Frenkel a few months ago (described here).

At first I thought Ed Frenkel's claim that geometric Langlands was going to give a Grand Unified Theory of mathematics was completely over the top, but seeing how some of these very different and fascinating relations between new kinds of mathematics and quantum field theory seem to be coming together, I'm more and more willing to believe that investigating them will come to dominate mathematical physics in the coming years.

Update: Slides from Witten's Berkeley lectures are here. And many thanks to David Ben-Zvi for the informative comments!

Comments

1. **A.J.**
   November 14, 2006

   Hi Peter,

   Witten has only delivered one lecture so far, and it was devoted to reviewing background material: mostly S-duality and a few words about topological twisting, all of which can be found in the Kapustin-Witten paper.

2. **Peter Woit**
   November 14, 2006

   Thanks A.J.!
   It would be great if you could keep us informed about the rest of the lectures…

3. **SFB**
   November 14, 2006

   It sounds like they are doing interesting math, but leaving physics to the LQG crowd.

4. **atrings**
   November 14, 2006

   I agree with SFB for the "interesting math ",but not for the"LQG crowd".

5. **Richard**
   November 14, 2006

   "At first I thought Ed Frenkel's claim that geometric Langlands was going to give a Grand Unified Theory of mathematics was completely over the top, but seeing how some of these very different and fascinating relations between new kinds of mathematics and quantum field theory seem to be coming together, I'm more and more willing to believe that investigating them will come to dominate mathematical physics in the coming years."

   Perhaps a domination of mathematical physics, but the claim of a grand unification of mathematics is in fact way over the top unless you believe that mathematics is nothing but mathematical physics. It probably all depends on your own personal values, biases, points of view, and even whom you believe owns mathematics. Recall Lubos' wild claim that someday mathematics will be completely subsumed by string theory?

6. **onymous**
   November 14, 2006

   I expect many of the people who have been working on geometric Langlands for years would be kind of shocked to be called mathematical physicists, Richard. Do they all instantly become mathematical physicists just because Witten got interested in what they're doing?

7. **Richard**
   November 14, 2006

   Onymous – I don't believe I said that.

8. **onymous**
November 14, 2006

Apologies, I misread Peter's original statement — didn't notice that he specifically singled out mathematical physics — and so misinterpreted your "…unless you believe that mathematics is nothing but mathematical physics" as an implication that geometric Langlands is mathematical physics. Never mind.

9. **David Ben-Zvi**
November 15, 2006

Hi and thanks for the references! (all notes on my page should be taken with many grains of salt..)
I should point out that the preprint by Gukov and Witten doesn't actually talk at all about link homology, so my talk description was perhaps premature, but a connection between
geometric Langlands and some kind of link homology is to be expected following their ideas (cf Gukov's Strings talk).
Cautis and Kamnitzer also have very interesting work in progress on such a relation.

After all, geometric Langlands is a very general categorification
program in representation theory, so one would expect it to
relate to the kinds of categorifications that give rise to Khovanov homology. There just aren't too many fundamental structures associated with a semisimple Lie group, and they all connect..

Of course it's a joke to speak of geometric Langlands as
a grand unified theory… but the Langlands duality is certainly
among the broadest themes in math, a kind of nonabelian
generalization of the Fourier transform, and it's extremely exciting
that we can view it in the geometric setting as electric-magnetic duality in four dimensional gauge theories!

10. **relativist**
November 15, 2006

David Ben-Zvi says

"it's extremely exciting that we can view it in the geometric setting as electric-magnetic duality in four dimensional gauge theories!"

Can you expand on that? Sounds very interesting.

11. **urs**
November 15, 2006

> it's extremely exciting that we can view it in the geometric setting as electric-magnetic duality in four dimensional gauge theories!

Can you expand on that? Sounds very interesting.

This is the insight of the Kapustin-Witten paper.

You can find a summary here.

12. **urs**
November 15, 2006

> a kind of nonabelian generalization of the Fourier transform

Is it a nonabelian generalization, or isn't it rather a categorification of the Fourier transform?

It seemed to me that much of Langlands can be nicely understood as taking place in categorified linear algebra. I have made remarks on how the Hecke operator looks like a 2-linear map for instance here.

13. **urs**
November 15, 2006

> It would be great if you could keep us informed about the rest of the lectures…

If anyone feels like reporting on interesting lectures online, we have a guest account for that over on the n-Café.

For instance we had David Roberts guest-reporting from a lecture by Brian Wang here, similar to the many guest reports we had # at the string coffee table.

14. **David Ben-Zvi**
November 15, 2006

Urs:

"Is it a nonabelian generalization, or isn't it rather a categorification of the Fourier transform?"

well it's both.. the main difficulty is the nonabelian nature
rather than the categorification, and that is where Langlands tells
us what to do (in the geometric or classical, noncategorified setting).
Categorifications of the Fourier transform have been
used for almost 30 years I think (starting with the Fourier-Deligne
transform, see eg Laumon's first ICM), and the geometric Langlands
program suggests that one can extend this to nonabelian settings
(G-bundles on curves).

By the way maybe this is an excuse to air one of my pet
peeves, the use of the term "Fourier-Mukai"
to refer to any functor between derived categories given

by an integral kernel.. I would be surprised if an analyst
referred to any map on function spaces given by
integration against a kernel (or any matrix) as a Fourier transform, and the same should hold in the categorified setting — in some
precise sense (due to Toen and which I'm badly paraphrasing)
all functors between derived categories are given by integral kernels!
"Honest" Fourier-Mukai transforms should have additional
structure and properties (for example taking convolution
to tensor product). Similarly not any duality is a T-duality!

15. **urs**
November 15, 2006

> well it's both.. […] Categorifications of the Fourier transform have been used […] and the geometric Langlands program suggests that one can extend this to nonabelian settings […]

Great, thanks! That's what I was hoping some expert would say. Probably I just talked to the wrong experts so far!

Because each time I'd ask a question along the lines

"isn't an eigenbrane just a categorified eigenvector in some 2-vector space"

the answer I'd get would be something like

"no, 2-vector space only appear after we categorify Langlands itself, like Kapranov discussed."

http://golem.ph.utexas.edu/category/2006/10/quantization_and_cohomology_we_1.html#c005444

16. **Peter Orland**
November 15, 2006

I don't know much category theory, but I thought that the
non-Abelian generalization of the Fourier transform is the character expansion (or Plancherel transform in the non-compact case) for functions on non-Abelian groups. Aside from a character formula, that is the simplest generalization.

Obviously, I am missing the point and something deeper is meant. Can anyone explain this to a dumb theoretical physicist?

17. **Peter Orland**
November 15, 2006

I just wanted to add that the sort of examples I mentioned don't help much with non-Abelian duality in classical or quantum field theory. To perform a duality transformation, a zero-curvature condition is Fourier transformed and the parameter integrated over is the dual field. This only really works in the Abelian case. There are non-Abelian generalizations of duality done this way, but they are rather messy, and not obviously useful.

18. **A.J.**
November 16, 2006

Well, Witten finished his lectures, but ran out of time to say much of anything about ramification. There's just too much information to be covered in (somewhat less than) 3 hours. Most of what he said is pretty well covered in David Ben-Zvi's notes, and in Urs's posts on the subject, or in the Kapustin-Witten paper for that matter.

We did get scans of his notes, so perhaps those will be available online one of these days.

19. **urs**
November 16, 2006

> Can anyone explain this to a dumb theoretical physicist?

One way to get an intuition for what is going on with these Hecke operators and similar transformations is to consider the drastically oversimplified baby toy example situation where the underlying spaces are in fact just – finite sets.

A vector bundle over a finite set is then just an array of finitely many vector spaces.

Think of that as a vector whose entries are vector spaces. Such a beast is known as a (Kapranov-Voevodsky) 2-vector.

The categorification involved here is that which takes the monoid of complex numbers and replaces it by the monoidal category of complex vector spaces.

So we can imagine doing linear algebra with these vectors whose entries are vector spaces by replacing sums of complex numbers by direct sums of vector spaces and products of complex numbers by tensor products of vector spaces.

In particular, let X any Y be two finite sets and consider a vector bundle L over X x Y .

By the above, this is now like a |X| x |Y| matrix with entries being vector spaces. Using the above dictionary, we can define the categorified matrix product of L with a 2-vector over Y, simply by using the ordinary prescription for matrix multiplication but replacing sums of numbers by direct sums of vector spaces and products of numbers by tensor products of vector spaces.

One can convince onself, that this categorified action of a 2-matrix on a 2-vector can equivalently be reformulated in a more arrow-theoretic way as follows:

We have projections p1 and p2 from X x Y to X and to Y, respectively. This makes X x Y into a span

http://golem.ph.utexas.edu/category/2006/10/klein_2geometry_vi.html#c005232

Given a 2-vector V -> X over X, we may pull it back along p1 to X x Y, tensor the result componentwise with L and push the result of that back along p2.

This operation produces precisely the naive categorified matrix product that I mentioned above.

But the nice thing is that this pullback-tensor-pushforward along a "correspondence" like X x Y generalizes to vastly more interesting situations.

There is an entire zoo of well-known operations of this kind. The Fourier-Moukai transformation is one example. The Hecke transformation that appears in geometric Langlans is another.

In the above sense, all of these operations can be understodd as linear maps on 2-vector spaces.

A description of what I just said, including some helpful diagrams and links to further material can be found here:

Fourier-Mukai, T-Duality and other linear 2-Maps.

20. **urs**
November 16, 2006

Concerning the abelian vs. nonabelian categorified Fourier transform:

there is something called the "classical limit" of geometric Langlands, as decribed for instance here:

Pantev on Langlands, II

The Hecke operation in geometric Langlands is a generalization of the categorified Fourier transformation: is a "2-linear map" in the sense of my comment above

http://www.math.columbia.edu/~woit/wordpress/?p=492#comment-19258

such that it coincides with the Fourier-Moukai transformation in this "classical limit".

In other words, the Hecke operation is a deformation of the Fourier-Moukai transformation.

21. **Bert Schroer**
November 16, 2006

I never understood what is the relation of elliptic cohomology (not that I don't know what it presents mathematically since I have followed the area with an ever increasing distance since the days of the Atiyah-Singer index theorem) with particle physics except that Witten has generated a certain enthusiasmus with some particle physicists. Since I have learned to make a distiction between physics and what (some) physicists are doing and since this blog (as Peter's book) is primarily about the present state of particle physics I think it is a legitimate question to ask about its relation to particle physics. If this is not permitted then this will be my last contribution to this blog.

22. **Anon**
November 16, 2006

To Peter Orland:

You are correct about the Plancheral theorem. But that tells you that if you know the irreducible representations, and their dimensions/characters, you know how to decompose functions. It doesnt tell you what the characters are.

In the first instance Langlands is a parameterization of irreducible reps, and a determination of their character; roughly they are in bijection with conjugacy classes in another group.

The categorification nonsense is an elaboration of this, to say *all* information you can extract comes from this dual group.

23. **Peter Orland**
November 16, 2006

Urs and Anon,

Thanks for the responses. I understand that a character formula of some sort is need to make Plancheral meaningful. What I worry about is that even with such a character formula, there isn't enough for non-Abelian electromagnetic duality. In fact, I am skeptical a USEFUL duality for pure Yang-Mills theorists exists.

To carry out a duality transformation, the Bianchi identity needs to be imposed by integrating over a new field (in 3+1 dimensions, this field is a one-form). Then we would like to integrate out the orginal gauge field to obtain a action in this new field. Doing this in practice is tough. There are tricks for doing it with certain character formulas, but the dual theory is a mess, since the dual fields are discretely valued (o.k. on the lattice, but without a good continuum interpretation).

Are these new techniques are somehow better? If so, it would be very interesting.

24. **urs**
November 16, 2006

> In the first instance Langlands is a parameterization of irreducible reps, and a determination of their character; roughly they are in bijection with conjugacy classes in another group.

That's the original "algebraic" Langlands thing.

> The categorification nonsense is an elaboration of this, to say *all*

I think the categorification nonsense comes in when you pass from the original to the *geometric* Langlands correspondence.

In the original Langlands setup, the Hecke operator is an ordinary linear map, acting on a space of modular forms.

In the geometric version of the theory, it becomes the Hecke operator that acts on derived coherent sheaves on some moduli space. And that guy is no longer an ordinary linear map. But it is a categorified linear map, if you like (and also if you don't like it).

In particular, in a special limit it is nothing but a certain categorification of the Fourier transformation.

25. **urs**
November 16, 2006

> I never understood what is the relation of elliptic cohomology […] with particle physics

Elliptic cohomology is not about particle physics. It is about string physics.

Elliptic cohomology is to strings like particles are to K-cohomology [#](#).

But what is the direct relation of elliptic cohomology to geometric Langlands, that made you bring this up here?

26. **Bert Schroer**
November 16, 2006

Interesting, so after all elliptic cohomology isn't about particle physics it is rather about ST. That's precisely what I expected.

27. **Bert Schroer**
November 16, 2006

Urs
Interesting, so after all elliptic cohomology isn't about particle physics it is rather about ST. That's precisely what I expected. I guess I got into the Langland's column by accident, but without this accident I probably would not have received such a precise answer.

28. **urs**
November 16, 2006

> Interesting, so after all elliptic cohomology isn't about particle physics it is rather about ST.

Yes, check out the table at the beginning of the introduction of [those notes](#).

Generalized cohomology theories are labelled by something called their "chromatic filtration".

The idea is that a cohomology theory of chromatic level p comes from the physics of "p-particles" – otherwise known as (p-1)-branes.

K-cohomology has filtration 1. It corresponds to 1-particles (0-branes). Ordinary points, that is.

Elliptic cohomology has filtration 2. It corresponds to 2-particles, otherwise known as 1-branes or strings.

Ordinary (singular) cohomology has filtration level 0. There is a precise sense in which it corresponds to 0-particles (or (-1) branes).

I expect this table is open ended. But I have never seen anything about cohomology theories of chromatic filtration larger than 2.

29. **urs**
November 16, 2006

> I am skeptical a USEFUL duality for pure Yang-Mills theorists exists.

It is a famous conjecture that 4-dimensional Yang-Mills theory has a duality called *S-duality*.

Yang-Mills theories (in a given dimension, for a fixed number of supercharges) are parameterized by a complex number

tau ,

the coupling constant, and a Lie group

G,

the gauge group.

For N=4 supersymmetric Yang-Mills, there is conjectured to be an isomorphism between Yang-Mills theory for

(tau,G)

and that for

(-1/tau , G^L) .

-1/tau is, roughly, the inverted coupling constant (therefore: "weak-strong coupling duality") and G^L is the Lie group that is Langlands dual to G.

See the first few paragraphs of [this](#), for instance.

That this is indeed an isomorphism of field theories is not a theorem, but it is supported by enough evidence that makes everybody assume it is indeed true. This is the *S-duality conjecture*.

Since the Langlands dual group appears in this conjecture, it has long been speculated that there is indeed a relation between S-duality and the Langlands program. But until recently nobody could really substantiate this.

The achievement of the Kapustin-Witten work is to show that for the special case that the 4-dimensional Yang-Mills theory is suitably compactified down to two dimensions, the S-dualiy conjecture for Yang-Mills theory is essentially equivalent to the geometric Langlands conjecture.

All the ingredients of geometric Langlands, like those moduli spaces of bundles and the derived coherent sheaves on them, can be understood in terms of field configurations and boundary conditions of compactified N=4 super Yang-Mills theory.

Notice that this amounts to further support for the S-duality conjecture, because it increases the number of people that truest the S-duality conjecture by those mathematicians that trust the geometric Langland conjecture.

But it might also be noteworthy that this suggests that the geometric Langlands duality is only a tiny aspect of a much bigger story – since it is (apparently) just the special case of S-duality applied to a very specific compactification of Yang-Mills theory only.

30. **Peter Orland**
November 16, 2006

Urs,

Yes, I know about the S-duality conjecture (I would much more interested in a similar conjecture about pure Yang-Mills than N=2 or N=4 Yang-Mills. Theories with adjoint matter are very different from those we know about in nature).

Though a conjecture is nice, to really prove it operator equivalences are needed. The procedure I discussed before, character expansions of the Bianchi identity, etc., is the first step to find such equivalences. In Abelian theories, this is how Kramers-Wannier duality works. There are some non-Abelian constructions due to Sharachandra and Anishetty, they haven't proved useful yet.

31. **A.J.**
November 17, 2006

Urs,

Their's a mild caveat to be added to your statement that

> All the ingredients of geometric Langlands, like those moduli spaces of bundles and the derived coherent sheaves on them, can be understood in terms of field configurations and boundary conditions of compactified N=4 super Yang-Mills theory.

The geometric Langlands correspondence is stated in terms of D-modules on the moduli stack of not-necessarily stable G-bundles. Kapustin & Witten's work doesn't quite give full information about the moduli stack, but only its semi-stable locus. As I far as I can tell, the relation between N=4 SYM and the Langlands correspondence for D-modules on the full stack hasn't been completely spelled out.

32. **ks**
November 17, 2006

Question to Urs. First of all thanks a lot for all your explanations. You work on cool stuff anyway (though it is a little over my head at this time). Do I understand your research program correctly when I assume that You try to link the standard model and ST in purely algebraic terms by means of higher category theory? Hence when changing the algebraic setting they do not look much different but are connected through certain higher morphisms?

33. **Bert Schroer**
November 17, 2006

Peter Orland
Conceptual realism demands to separate Kramers-Wannier duality (and its structural extension the order-disorder issue) from speculative ideas. The o-d duality is a local quantum physical phenomenon which has no known analog in higher dimensions. Whereas o-d is a phenomenon which has a solid operator algebraic intrinsic understanding (if you want I can provide you with recent literature) there is nothing like this for the S conjecture.
By now Wikipedia has more material on wild conjectures than about genuine results. There is the danger that we may be fooled to our own simulacrums and metaphors in particular that conjectures solidify because they comes from somebody with a high status in the community or because they have been hanging around for a long time so that several generations have stepped on them.

34. **urs**
November 17, 2006

> wild conjectures

S-duality is certainly a conjecture, but hardly a wild conjecture.

I mean, that's the point: S-duality is apparently as wild as geometric Langlands.

35. **urs**
November 17, 2006

> Kapustin & Witten's work doesn't quite give full information about the moduli stack, but only its semi-stable locus.

Right, thanks. There are probably a couple of such technicalities. I am not working on this stuff, so it's hard to keep them all in mind.

So what about that "classical limit" in which, apparently, geometric Langlands is only proven so far. Does compactified SYM exactly coincide with the geometric Langlands data in that limit?

36. **David**
November 17, 2006

A couple of comments: Kapustin-Witten's theory does (as far as I
understand) cover the full stack of bundles, not just the semistable locus. The sigma-model/mirror symmetry description
fails outside the semistable locus, but they emphasize
in the paper that the gauge theory sees the entire stack
of bundles — I think the problem is us geometers
have only been able in the past really to process
the classical aspects of the theory (solns of the equations of motion
etc) but quantum gauge theory is a lot smarter than we
are (speaking for myself at least). As far as I know they
can't completely say what S-duality predicts off the semistable locus, but the important point is it does actually apply there.

The classical limit of Langlands is only proven
generically, missing the hardest locus — it's a beautiful
result and one of the best in the subject, but saying classical
geometric Langlands is understood is on the same

level as saying you understand (noncompact) Lie groups
when you understand their diagonalizable elements —
the hardest part involved unipotents..

Also I'm not sure I would think of Hecke operators as Fourier transforms – the Hecke operators are the symmetries of moduli
of bundles (and sheaves on them), while the Fourier-Mukai
type transforms relate G and G^ the dual group.

One sense (of many) in which geometric Langlands is a nonabelian
categorified generalization of the Fourier transform is that while Plancherel helps you decompose spaces of functions on a group,
geometric Langlands type results help you decompose the
CATEGORY of all representations of a group —
since these categories are not semisimple
there's a big difference between listing irreducibles and their characters and actually describing the structure of general
representations. (Geometric Langlands ideas
can be used to study for example the category
of Harish-Chandra modules for a real semisimple Lie group).

37. **Peter Orland**
November 17, 2006

Bert,

I cannot understand your explanation especially well. In my attempt to translate your statement into simple language, I conclude you mean more conjectures than solid statements dominate our field. I don't need to be reminded of this, since I have
seen it all over the literature for the last decade or so.

I was asking if the experts on Langlands believe a useful concrete electric-magnetic duality transformation can be constructed from non-Abelian Fourier transforms (character expansions). I suspect the answer is no, since no one gave me a simple "yes".

38. **John Gonsowski**
November 17, 2006

I'm probably mixing algebraic number theory with analytic number theory but is there a relationship between elliptic cohomology and elliptic Mobius transformations?

39. **urs**
November 17, 2006

> Also I'm not sure I would think of Hecke operators as Fourier transforms – the Hecke operators are the symmetries of moduli
> of bundles

Oh, sorry, I misspoke if I said that. The Langlands correspondence is analogous to the Fourier transform, exchanging skyscraper sheaves (analogous to delta-functions) with Hecke-eigensheaves (analogous to plane waves). So, in this analogy, the Hecke operator is like a categorified derivative.

40. **Bert Schroer**
November 17, 2006

I am afraid the sad truth is the answer is "no". It is better to live in quantum reality than to become complacent with a Disney version of it.
I was not trying to explain anything in technical terms but only pointing to the obvious observation that Kramers-Wannier on a microscopic level (achieved by Leo Kadanoff) was quantum from the beginning whereas the Seiberg Witten duality is from a physical Disney dreamland which precisely of this is so useful to a large part of mathematics. The kind of mathematics for which it had no use is the operator-algebraic mathematical setting of QT which dates back to von Neumann and has been enriched by the locality principle in AQFT. By the way the manner Kadanoff has extracted (noncommutative) operator commutation relations for the (what we nowadays call) the Ising primary fields from the Euclidean lattice setting (via a partially guessed properties of the transfer matrix formalism) had my deep admiration; the Leitmotiv of all my work with Swieca in the early 70s was related to adapt ate Kadanoff's order/disorder ideas to the continuous setting of QFT; in many cases we even succeeded to read this back into a continuous functional integrals setting by using an Aharonov-Bohm analog language. Later, when I was working with Rehren on an algebraic approach to chiral conformal QFT I remembered those Kadanoff ideas and we found a completely explicit operator version of an "exchange algebra" for the conformal Ising field theory from which it was possible to compute its n-point Wightman functions. A historical review can be found in
http://br.arxiv.org/abs/hep-th/0504206
but thinking about this now, I should have written much more about Leo Kadanoff's contributions; he really deserved a Nobel prize together with Wilson.
In those days we also convinced ourselves that this order-disorder idea has no electric-magnetic counterpart in the full QFT setting.

41. **Peter Orland**
November 17, 2006

Bert,

I also worked extensively on duality. Like you, I concluded that
there is no simple operator equivalence between a non-Abelian
gauge theory and its dual. But there are intriguing exceptions
of systems with non-Abelian systems which do have duality
transformations and disorder operators. In my Ph.D. thesis I found lattice systems with permutation-group S_{N} symmetry which have nontrivial duals. But I will spare people here from a list of more publications on the subject.

Regards,
Peter (O.)

42. **Thomas Larsson**
November 18, 2006

Conceptual realism demands to separate Kramers-Wannier duality (and its structural extension the order-disorder issue) from speculative ideas. The o-d duality is a local quantum physical phenomenon which has no known analog in higher dimensions.

???

The 3D Ising model on a cubic lattice is Kramers-Wannier dual to Ising gauge theory on the same lattice. Why is this not o-d duality in higher dimensions?

43. **urs**
November 18, 2006

> I concluded that there is no simple operator equivalence between a non-Abelian gauge theory and its dual.

Is this saying that you consider the S-duality conjecture to be in fact false?

If so, I'd be interested in the details of the assumptions that go into this.

I recall that Bert Schroer was (similarly ?) claiming that the AdS/CFT duality conjecture (in the sense of Maldacena) is false, and that the correct duality statement was along the lines of Rehren's work.

In that case I got the impression that two rather different concepts were being compared, and that in fact Rehren's work had little relation to the setup considered by Maldacena et al. Compare for instance Jacques Distler's account.

The crucial difference in this case is that Rehren's work was based on a fixed and precise axiom set, while Maldacena's work uses notions of quantum field theory that have not been axiomatized yet.

For people like Bert Schroer this is reason enough to completely reject all QFT that does not fit into the AQFT axioms. For other people, in contrast, the restrictive applicability of the AQFT axioms is reason enough to reject those.

To some extent it is a matter of taste concerning which role of rigour you find useful in physics research. I can easily tolerate both these standpoints. But I would like to know in each case which one is assumed by which participant.

44. **woit**
November 18, 2006

Urs,

You keep ignoring the fact that Peter Orland is asking about pure YM theory, not N=4 SYM. There's a beautiful story about duality in non-supersymmetric abelian gauge theories, and many people (including Peter) have tried hard to generalize this to the non-abelian case. I gather that he's trying to understand whether geometric Langlands gives any insight into that problem, and as far as I can tell, the answer is just no.

45. **Peter Orland**
November 18, 2006

Urs,

Sorry that I am giving long-winded answers to your questions. I am
mainly interested in advancing methods in asymptotically-free field theories and in constructions which could eventually facilitate calculations. I try to learn other stuff, because I can't predict what I may need to know in the future. But I am more interested in theoretical, rather than mathematical physics (as people abuse use the term nowadays, to study mathematical techniques, rather than to prove theorems).

I believe (after some years of trying to show the contrary) there is no USEFUL version of Kramers-Wannier duality which is true for PURE non-Abelian gauge theories. There are non-Abelian dualities for some special \$S_N\$-invariant systems, which I mentioned above (there is also non-Abelian Bosonization in two dimensions).

The general problem for duality in non-Abelian theories is constructing dual fields with local commutation or anti-commutation relations. Supersymmetric or other theories with adoint matter have some sort of charge-monopole duality – but such theories are effectively Abelian. These theories are interesting in their own right, but to my way of thinking, they are not as important as Yang-Mills theories coupled only to fundamental (not adjoint) Fermion color charges, or pure Yang-Mills theories.

There are other notions of duality in QCD. The 't Hooft loop is the disorder operator. Unfortunately, there is probably no useful local dual-field-theory formulation for which it is the order parameter.

46. **urs**
November 19, 2006

> You keep ignoring the fact that Peter Orland is asking about pure YM theory, not N=4 SYM.

In as far as I am ignoring anything, it is not on purpose. I'd be glad to be enlightened.

Maybe I found Peter Orland's statement

> I concluded that there is no simple operator equivalence between a non-Abelian gauge theory and its dual.
>
> #

seemed to refer to arbitrary gauge theories.

> I gather that he's trying to understand whether geometric Langlands gives any insight into that problem, and as far as I can tell, the answer is just no.

Hm, maybe here is the source of the misunderstanding. Kapustin-Witten show that geometric Langlands does give insight into the type of duality present in N=4 SYM. So in far as this is different to other types of duality, geometric Langlands apparently does not apply to these.

> Supersymmetric or other theories with adoint matter have some sort of charge-monopole duality – but such theories are effectively Abelian.

Could you expand on what you mean by "effectively abelian" here? Thanks!

47. **Peter Orland**
November 19, 2006

Urs,

By "effectively Abelian", I mean that that the magnetic-monopole charge is well-defined and quantized. In QCD or pure Yang-Mills, there is no precise definition of magnetic-monopole charge.

In the Georgi-Glashow model (an the related deformation of N=2 supersymmetric gauge theory) a Higgs field breaks the gauge group down to the Cartan subgroup. Thus there are Abelian monopoles, with quantized charge, etc. These theories have a confined phase for sufficiently small monopole mass, which goes back to Polyakov's observations in the 70's. Duality for such theories is not so different from those of Abelian Wilson lattice gauge theories. They are, however, quite different from QCD.

Now there is an old result made by many people (Fradkin, Shenker, Rabinovici and others) that there is little difference between a Higgs field in a gauge theory and a scalar field in that gauge theory without a Higgs potential. The basic point is that the operator creating a massive vector Boson in the Higgs theory looks just like the operator creating a "meson" built from scalars in the confined phase. From this point of view, any theory with scalar matter is not so different from a Higgs theory. In particular, it is possible to define magnetic charge, no matter what the scalar potential happens to be. So in such theories charge-monopole duality is a sensible concept.

The reason why the possibility of duality for Yang-Mills theories is interesting is because it could yield insight into the confinement phase. Some sort of magnetic condensation occurs, producing confinement and a mass gap, as simulations show, but we want to know why.

48. **anonymous**
November 19, 2006

Off-topic mathematical physics fun: Andre LeClair is claiming there's a physical system, which, on physical grounds, suggests the Riemann hypothesis is true. Are there any experts around to comment on whether it's plausible?

http://www.arxiv.org/abs/math-ph/0611043

49. **relativist**
November 22, 2006

For those like me who don't know much about the Langlands programme but would like to, a useful account is an older one by Frenkel: `Lectures on the Langlands Program and conformal field theory', at
http://www.arxiv.org/PS_cache/hep-th/pdf/0512/0512172.pdf

Esquire On The State of Particle Theory

November 15, 2006
Categories: Uncategorized

You know things are getting strange when Esquire magazine starts running an article on the current state of particle theory. As you might expect, their take on this is rather odd. It centers around Nima Arkani-Hamed and begins with:

For a hundred years, physicists have been scraping away at the strange and complicated phenomena obscuring the true face of our universe. Finally, a few brillant young thinkers may be on the verge of getting the first real glimpse.

which is pretty much complete nonsense, totally ignoring the huge success of the standard model in favor of the latest extremely speculative models promoted by some people.

The Esquire writer talked to several theorists, including Lee Smolin and Laurent Freidel at Perimeter, where he describes young postdocs as hanging out at a local hipster bar, with one of their number describing string theorists as "the post 9/11 theocons", afraid of anything new: "The string theorists just masturbate to their same ideas." The postdocs do note that at Perimeter string theorists and non-string theorists get along fine. Freidel, a faculty member at Perimeter, is described as not having slept for two weeks straight when he was working on a solution of QCD, with his wife asking a colleague "Can you do something? He's going insane."

After describing Perimeter, the article then moves on to Witten and Maldacena at the Institute in Princeton. Witten's comments about the current state of things go as follows:

Well, you can't have your best year every year… I've lived through two periods, the mid-eighties and mid-nineties, where for about six or seven years, roughly, there were a lot of really interesting results that were also relatively easy. And I've also lived through several periods by now where you have to work a little harder to get something interesting.

Witten goes on to say that he is putting his hopes in the LHC and the idea that it is likely to tell us something about the nature of electroweak symmetry breaking.

There's some attempt to describe Maldacena and his AdS/CFT conjecture, which is characterized as "a mind fuck, but not crazy." The article then moves back to Arkani-Hamed at Harvard, with "For crazy, you have to go about 250 miles north." His view of the current controversy over string theory is said to be "forget the antistring polemicists! They're just reactionaries! This could be the greatest discovery of our time!", and he heavily promotes the anthropic landscape and the idea that the LHC will provide evidence for it. He says:

The mantra of string theory ten years ago was that the theory was smarter than you… Well, exactly that–just follow the theory where it leads you and it leads to this precipice. And now we have to decide what to do. So now a number of people are deciding to jump… And I think that those of us that decided to take the plunge are staring at the true nature of the beast for the first time.

Personally, I think if a scientific theory with no experimental evidence for it takes you to the edge of a precipice and tells you to jump, the sensible thing to do is to say "No Thanks!", back away, and go find another theory. But that's just me.

The latest New Scientist also has something about the string theory controversy, an article by Michio Kaku entitled Will we ever have a theory of everything?, part of a series of articles on "The Big Questions". Kaku describes the controversy dramatically:

It's all-out war. The hostilities have begun. With guns blazing, daily salvos are being fired by both sides. Welcome to the conflict raging within the rarefied world of theoretical physics, where a civil war has erupted over string theory and a theory of everything.

While I disagree with the far too rosy picture he paints of the prospects for string theory, Kaku takes a very sensible attitude towards the whole thing:

So who's right? Actually, both have a legitimate point of view. But far from signalling a collapse in physics, this debate is actually rather healthy. It's a sign of the vitality of theoretical physics that people are so passionate about the outcome. Science flourishes with controversy.

and ends, reasonably enough with:

One day, some bright, enterprising physicist, perhaps inspired by this article, will complete the theory, open the doorway, and use the power of pure thought to determine if string theory is a theory of everything, anything, or nothing.

New Scientist also asked various well-known physicists what they thought might happen in physics in the next 50 years. Weinberg says that he hopes for a final theory of particle physics, with discovery of superpartners a first step. Tegmark also hopes for a final theory, one which will have us living in just one of many "parallel universes". 't Hooft hopes for a deterministic model that would unify quantum mechanics and gravity, Randall for a new understanding of space and time, Carroll for a theory of the big bang, Wilczek for a new golden age of particle physics catalyzed by the LHC, Kolb for the discovery of gravitational waves and Vilenkin for the discovery of a cosmic string. The most popular question on many these people's minds is that of whether or not we live in a multiverse (Tegmark, Rees, Krauss mention this). Among mathematicians, Marcus du Sautoy suggests we'll have a proof of the Riemann hypothesis, Timothy Gowers favors P=NP.

Among all these and other scientists, I think the most plausible prediction comes from my graduate school roommate, Nathan Myhrvold, who thinks a revolution will come from materials science, with the development of new "metamaterials", substances with new, intricate synthetic structures.

Update: Somehow I hadn't noticed that New Scientist also had a prediction from Witten:

String theory will continue to be an extremely fertile source of new ideas. It will still be viewed as the interesting candidate for quantum gravity, and may even be more or less understood by 2056.

Interesting that he thinks that 50 years from now the situation will be much the same, with string theory still just a "candidate" for quantum gravity, and he doesn't predict that we will have string-based unification of particle physics and gravity.

Comments

1. **Aaron Bergman**
   November 15, 2006

   "One of PI's initial faculty hires, Smolin, fifty-one, began his career in string theory before becoming fed up with the lack of progress and turning instead to loop quantum gravity, an alternate possible unified theory."

2. **Troublemaker**

November 16, 2006

and use the power of pure thought to determine if string theory is a theory of everything, anything, or nothing.

Mmmm, Aristotle-icious! I prefer the power of comparing empirical results to theoretical predictions, but sure, pure thought is fun, too.

3. **Chris Oakley**
   November 16, 2006

   This is my favourite bit of the *Esquire* article:

   > A different, even more extreme explanation for the symmetry breakdown is known as supersymmetry, which theorizes a set of counterparts to our known subatomic particles that are embedded in the architecture of space-time. Besides explaining electroweak interactions, the discovery of supersymmetric particles, with cool names such as squarks, sleptons, and selectrons, would be a huge boon to string theorists, whose model of the universe depends upon them.

   … so now I know something I was previously unaware of, i.e. that supersymmetry explains something.

4. **a**
   November 16, 2006

   Actually, the precipice or feature or bug has little to do with smart string theory: all models with extra dimensions have multiple vacua and/or moduli.

   By the way, the most interesting observation about "is the weak scale anthropic?" was made in hep-ph/9707380 (1997: when it was not a fashionable string theory topic!), but this really brillant work is mostly ignored by popular journals.

5. **Mary**
   November 16, 2006

   Another pop-culture mention of the string theory controversy: this Zippy the Pinhead comic strip.

   (Zippy is a weird, surrealist sort of strip, and doesn't usually make even this much sense…)

6. **Pot Smoking Hippie**
   November 16, 2006

   "Personally, I think if a scientific theory with no experimental evidence for it takes you to the edge of a precipice and tells you to jump, the sensible thing to do is to say "No Thanks!", back away, and go find another theory. But that's just me."

   So, has your theory actually taken you anywhere besides another precipice? Does it offer any more experimental evidence than any other theory? Perhaps the string theorists are rappelling instead of free falling. Maybe the loop people could try rappelling with chains made of their loops? It's just a wild thought.

7. **Bert Schroer**
   November 16, 2006

   Pot Smoking Hippie,
   How many joints are necessary to write a blog like yours?

8. **Who**
   November 16, 2006

   Hippie, I watch post-string QG research and have seen no indication that those you call "loop people" have come to any metaphorical precipice. Progress appears steady along several lines of investigation. Preliminary contacts with matter and with classical gravity have been made (see recent papers by Rovelli et al and Freidel et al). Lately some problems with the ground state appear to have been gotten around (see Randono's papers of this past week for some recent results).

   So you seem to be generalizing needlessly—-just because somebody like Nima might see string research at a precipice which he believes the adventurous are jumping off and meeting "the beast", does not mean that non-string QG is in a similar situation. I think, instead, it is beginning to unearth more fundamental degrees of freedom from which space time and matter emerge at large scale.

9. **Alejandro Rivero**
   November 16, 2006

   this one is from 1994:
   space to let

10. **Garbage**
    November 16, 2006

    "Weinberg says that he hopes for a final theory of particle physics, with discovery of superpartners a first step. Tegmark also hopes for a final theory, one which will have us living in just one of many parallel universes. t Hooft hopes for a deterministic model that would unify quantum
    mechanics and gravity, Randall for a new understanding of space and time, Carroll for a theory of the big bang, Wilczek for a new golden age of particle physics catalyzed by the LHC, Kolb for the discovery of gravitational waves and Vilenkin for the discovery of a cosmic string."

    There is a high chance any of this will happen. I mean:

    1) No SUSY

    2) One Universe

    3) QM is not ultimately deterministic

    4) I assume Lisa awaits XD, so here I'd so no more than 4 (and please no "four more"! 😉)

    5) I'm guessing a better theory of big bang is to understand the initial singularity, keep waiting…

6) LHC will single out just the Higgs.

7) LISA will never launch, but perhaps/hopefully LIGO II will see something.

8) Cosmic strings would be fun watching, in purple and red ;). Even if we find such objects, how do we know they come from M-theory? magic?

And then what?

There is a high chance we'll just see the Higgs and then new physics will have to come in indirect ways, that is still very interesting. I would support ILC anyhow, there is a lot of stuff to do/check, but not a completly mind f*ck I'm afraid…:)

Like Witten says, just a "little harder" 🙂

In the mean time, keep on gnitabrutsam…

11. **Bee**
November 16, 2006

well, I can report the local hipster bar isn't so hip anymore. besides this, I like the sentence:

Taking up residence here is a bit like joining the priesthood. You're segregated from the rest of the world, and your job is to get into God's head and figure out how the big damn machine works.

So, that's then a priesthood that hangs out in the local hipster bar.

Sneakers and jeans rule

That makes a priesthood in sneakers and jeans that hangs out in the local hipster bar. I should add, I learned that Canadians call their sneakers 'runners'. But better:

It's like exploring a forest[…] as if it were a fort, planning a means of attack […] as if the search for a unified theory were the world's biggest game of sudoku.

And let's combine that with what The New Yorker wrote:

a sort of Menshevik cell of physicists in Canada

Okay, so we have a Menshevik cell of priests in runners and jeans that – when it doesn't hang out in the local hipster bar – plans attacks on forests and plays sodoku.

Also completely priceless: our universe is *[…] a giant DVD floating among an infinite number of other DVDs* and gravity is *like a bottle of whiskey that has been passed around the galaxy a few too many times.*

Now I'll go back into God's head. Deus vobiscum.

12. **language minder**
November 16, 2006

'There's some attempt to describe Maldacena and his AdS/CFT conjecture, which is characterized as "a mind f***, but not crazy."'

Please asterisk-out the major parts of poor language, or your site will probably be put on a parental hot list by search engines (as unsuitable for teenage kids who need to know this physics controversy).

13. **Tommaso Dorigo**
November 16, 2006

Well said, garbage.

I already placed a bet which covers your points 1, 4, and 6, 1000 US$ says they hold.

I am willing to bet 25,000 US$ on point 2, one and only one universe. I will make the check payable in another universe, of choice of the winner, however. No, seriously, I cannot bet more than a year of salary, but I would be happy to lose that one bet too. Any takers ?

Cheers,
T.

14. **Gil Kalai**
November 16, 2006

When brilliant and hilarious Scott Aaronson came to town last month he was much more enthusiastic to tell us about the recent physics controversies, the new books about string theory, and the related blog excitements, than discussing quantum computer's skepticism. Scott surely got us interested!

What might happend for physics in the next 50 years? Good question! And what the future be for string theory? Below are six alternatives:

This (light) piece is inspired by the (deep and serious) classic paper by Russell Impaglliazzo on the five possible universes regarding computation and cryptography. (Russel's paper is also one of Scott's favorite papers.)

Apart from the illustrative details (which are meant to be amusing) I regard each of the six alternatives below as realistic. The second alternative can be regarded as the current default cautiously-optimistic main-stream approach of the scientific community which, perhaps, make it the most plausible.

Six Alternatives For String Theory's Future

1. UTOPIA

String theory continues to progress and converges in a few decades to become a solid part of our scientific understanding with plenty of empirical direct and indirect confirmations and many applications to all other areas of physics. Some of the landmarks after the "Maldacena conjectures" (1997) were the "Johnson Postulate" (2009), the "Motl Ansatz" (2014), the "Diestel Paradigm" (2017) followed by the powerful "E-F-W Calculus" (2022). String theory becomes the "language of physics" perhaps

even “the language of nature”. Every graduate student
in physics is able to make string theory computations, and this is what most physicists do. String theory represents a sound mathematical theory, in fact, mathematics is now considered just as “the special case of string theory for Plank constant 0”. A few exciting problems remain.

Peter Woit’s book “TRUE!” tops the NYT best sellers lists for 24 weeks.(In his book Woit advises caution concerning applications of string theory to the area of finance.)

2. TRIUMPH and ISOLATION

String theory continues to progress and converges in a few decades to a solid part of our scientific understanding with convincing empirical direct and indirect confirmations but with little applications and relevance to other areas of physics. Computations with string theory are extremely hard. (Computations based on the E-F-W calculus are computationally infeasible even on the newly built “quantum computers”.) Mathematical foundations of string theory as of earlier high-energy physics remain shaky.

Peter Woit’s book “NOT WRONG!” hits the market,

3. PERPETUUM MOBILE

String theory continues to progress but it does not converge. String theory thus remains a “useful divergent theory” whatever this means. More and more exciting connections to mathematics are found. More and more conceptual revolutions in the theory itself are taking place. (The latest is the “13th superstring revolution”.) String theory leads to a whole new way to look at physics and, even more, it is a scientific experience not seen before. The best most brilliant minds are attracted to this theory as before.

The 17th edition of Woit’s “Not even wrong” appears.

4. DECAY

String theory continues to progress but the progress is slower, the attractiveness of the theory seems smaller. String theory indeed looks more promising than ever but while the success looks around the corner, string theory is not sufficiently promising to attract the best people. Interest in physics is shifted to other directions.

5. GLORIOUS FAILURE

A brilliant string theorist from Vanderbilt University discovers a potential feature of supersymmetric string theory which contradicts basic physics insights. Massive computations in the “String Vacuum Project ” confirm her discovery. After several years of extensive research (with beautiful new connections to mathematics found) it is now commonly accepted that string theory was falsified and is no longer an option for a theory of everything. No alternative is seen at sight. 20 prominent string theorists declare string theory as part of “mathematical physics”, rather than a viable physics theory, and within 72 hours, 18 of them get lucrative offers from top mathematics departments.

Woit’s biography of Ed Witten “WRONG!!”, is the basis for a successful Hollywood movie featuring Will Smith as Witten in the main role.

6. ALTERNATIVA

The alternative theory was discovered by cheer coincidence and like string theory itself is based on a technical rather than conceptual idea. The starting step was by an elderly mathematician from Bristol University seeking for mathematical explanation for QEC suggests to replace the term(1/137)^k with (1/137)^k (1-z)^(k(k-1)) where z is a constant smaller than and very close to one. Strangely, this has led to some consistent theory and this technical variation made quantum gravity easier. The next step came when a researcher from the University of Tehran (inspired, in parts, by some rather general suggestions of P. Woit, and the mathematical notion of “noise sensitivity”,) relates dark matter and dark energy with representations of unbounded weights and dimensions. Such representations are prominent in the new theory. (This new type of mass/energy is called “the mess”.) The theory is then developed and brought to completion by a New-Jersey based physicists N. Seiberg and E. Witten.

An extremely surprising feature of the new alternative theory is that the universe is 3+1 dimensional.

The translation of the new addition of Woit’s “Not Even Wrong” to Czech has just appeared.

(My subjective probabilities for the future of string theory, Utopia – 10%, Triumph and isolation – 40%, perpetuum mobile – 10% decay – 20% failure – 15% alternative found – 15%. Of course, some combination or an entirely different scenarios that I missed are also possible.)

15. **Jonathan Vos Post**
November 16, 2006

Esquire has commented on Physics and Mathematics before. But in a fairly superficial way.

Examples:

[The Physics of Immortality: Modern Cosmology, God and the Resurrection of the Dead (Paperback)
by Frank J. Tipler]
“A doozy of a book… it’s 2001: A Space Odyssey meets The Divine Comedy.” –Esquire.

From Wikipedia, the free encyclopedia
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Cartoon physics is a joking reference to the fact that animation allows regular laws of physics to be ignored in humorous ways or dramatic effects. For example, when a cartoon character runs off a cliff, gravity has no effect until the character notices and mugs an appropriate reaction.[1] Students of animation may hear of the “commandment” “Animation follows the laws of physics – unless it is funnier otherwise,” which is attributed to Art Babbitt.

The phrase also reflects the fact that many of the most famous American animated films, particularly those from Warner Brothers and MGM studios, unconsciously developed a relatively consistent set of such “laws” that have become regularly applied in comic animation.

The idea that cartoons behave differently, but not randomly, than the real world is virtually as old as animation. Walt Disney, for example, spoke of the plausible impossible (see The Plausible Impossible, 1956), deliberately mispronouncing the second word so it rhymed with the first.

Specific reference to cartoon physics extends back at least to June of 1980, when an article "O'Donnell's Laws of Cartoon Motion"[2] appeared in Esquire magazine.
[2] O'Donnell's Laws of Cartoon Motion", Esquire, 6/80, reprinted in IEEE Institute, 10/94; V.18 #7 p.12.

Philip Rosedale: Building a world entire
By Tom Colligan
December 2006, Volume 146, Issue 6

"What's that great line at the beginning of Metropolis? 'Every epoch dreams its successor'?" says Philip Rosedale, sitting at his cluttered desk in downtown San Francis co, at the center of a large white room filled with white desks identical to his own. "I think people have dreamed correctly, that the dream of something like Second Life has always been true. I've just always felt as if this was coming, and I just wanted to see it. I was just bored without it…."

A Physics Lesson: The Meniscus : Esquire : Jun 01, 2006. A Physics Lesson: The Meniscus. By Barry Sonnenfeld. topic: Science …

Angry Physics: Can You Guess Which Sexy Woman Has a crush on Einstein?
Presenting the "other" side of academic physics, where people backstab and … but I came across this teaser in Esquire about the sexiest woman of 2006. …

Nice to be noticed by Esquire. But does that mean that we should dress better and hang out at the right watering holes to keep getting noticed? Kind of like citing Einstein as a fasion expert on hairstyles, who also plays the violin, and does something f***ing crazy with unified field theory in Princeton, and hangs out with noted food minimalist Kurt Godel.

16. **Who**
November 16, 2006

Gil, I believe the scenario 4 "Decay" to which you attach subjective probability 10 percent has already occurred. The rate of publication has declined since 2002 and quality measured by objective standards has plummeted. Recent published papers are no longer innovative enough to garner much citation. Judging by new research, the signs are that independent and ingenious young people are getting out of stringy research, or not entering in the first place.

What you say seems to apply to the present: "string theory is not sufficiently promising to attract the best people. Interest in physics is shifted to other directions."

Perhaps Scott can give you some hints as to where those other directions might be.

17. **anon**
November 16, 2006

Nima Arkani-Hamed…"Well, exactly that–just follow the theory where it leads you and it leads to this precipice. And now we have to decide what to do. So now a number of people are deciding to jump… And I think that those of us that decided to take the plunge are staring at the true nature of the beast for the first time. "

Wow, his glee and excitement are palpable; it's like hearing the excited thoughts of a lemming as it takes the plunge into icy artic waters

18. **Aaron Bergman**
November 16, 2006

Lemmings don't actually do that, for whatever it's worth.

19. **amused**
November 17, 2006

"Last year, when Freidel discovered a possible rigorous mathematical solution for the strong force—which acts as the glue between protons, neutrons, and nuclei, and which to that point had been studied only by approximation—he didn't sleep for two weeks straight."

Can we expect a rigorous analytic proof of the mass gap in the coming future then?
The low-key, understated way that LQG'ers describe their work makes such a refreshing contrast with the giddy hype of string theorists.

20. **anon**
November 17, 2006

Bergman…"Lemmings don't actually do that, for whatever it's worth."

Thanks for the reference. Disney's pogrom of Mickey Mouse's family. Looks like they do jump, with the kind help of humans.

21. **a**
November 17, 2006

Tommaso, do you accept "just the SM at LHC" as evidence for an anthropic multiverse? Notice that it is not a joke, it could be the hot issue in 2014. I cannot accept your bet because this outcome might have a negative impact on your 2014 salary.

22. **Thomas Larsson**
November 17, 2006

The alternative theory was discovered by cheer coincidence and like string theory itself is based on a technical rather than conceptual idea.

Gil, there seems to be one possibility that you don't consider. That an alternative theory based on a conceptual rather than technical idea is discovered, not by coincidence but by following the idea's internal logic. The theory predicts four spacetime dimensions and m_p/m_e = 1836.

23. **Bee**
November 17, 2006

Hi Gil,

A 7th alternative:

Sorting out

String theorists will thoroughly focus their efforts towards a successful description of nature, dropping all their nice but irrelevant distractions that are maybe interesting maths but not physics. They will very possible go back to the very basics, and try looking for sideways they might have missed. Very likely, they will discover similarities to other approaches towards quantum gravity. Hopefully, they will be open to them, and everything will converge towards the same direction.

Best,

B. (always the optimist)

———————

The girl went through the back door into the garden, and called out, "You tame pigeons, you turtledoves, and all you birds beneath the sky, come and help me to gather:

The good ones go into the pot,
The bad ones go into your crop."

Two white pigeons came in through the kitchen window, and then the turtledoves, and finally all the birds beneath the sky came whirring and swarming in, and lit around the ashes. The pigeons nodded their heads and began to pick, pick, pick, pick. And the others also began to pick, pick, pick, pick. They gathered all the good grains into the bowl. Hardly one hour had passed before they were finished, and they all flew out again.

24. **Who**
November 17, 2006

Good point Thomas Larsson. It's a possible scenario.

> Gil, there seems to be one possibility that you don't consider. That an alternative theory based on a conceptual rather than technical idea is discovered, not by coincidence but by following the idea's internal logic. The theory predicts four spacetime dimensions and m_p/m_e = 1836.

A sizable fraction of the crowd here seems momentarily reduced to sarcasm or to heckling Bert. The basic cause, I think, is simple: non-string QG, it currently happens, is where the interesting results are being obtained.

Let's have a challenge: can anyone think of a 2006 string paper as consequential as the pair Andy Randono just posted generalizing the Kodama state?

25. **woit**
November 17, 2006

Bert and others,

Stop posting comments that contain personal attacks on people and little else. If one person does this, it's bad enough, but then everyone else wants to join in the fun and the comment section degenerates into hostile noise. I just deleted a half dozen such comments, please stop posting them.

26. **Who**
November 17, 2006

"Die guten ins Töpfchen, die schlechten ins Kröpfchen."

Aschenputtel (always the optimist)

27. **plank**
November 17, 2006

I can't believe no one found this yet!
—————
(My subjective probabilities for the future of string theory, Utopia – 10%, Triumph and isolation – 40%, perpetuum mobile – 10% decay – 20% failure – 15% alternative found – 15%. Of course, some combination or an entirely different scenarios that I missed are also possible.)
—————-
Well, (10 + 40 + 10 + 20 + 15 + 15)% = 110% > 100%

This can't be a good sign.
People seam to be ignoring basic fundamental consitency checks here. Where else?

28. **plank**
November 17, 2006

consitency checks => consistency checks

People seam => People seem

(I too make mistakes, although these are only "formal", sorry)
🙂

29. **Anon**
November 17, 2006

"Let's have a challenge: can anyone think of a 2006 string paper as consequential as the pair Andy Randono just posted generalizing the Kodama state?"

Kapustin and Witten
Frenkel, Losev and Nekrasov
Hofman and Maldacena
Intriligator, Seiberg and Shih
Arkani-Hamed, Motl, Nicolis and Vafa
Herzog, Karch , P. Kovtun, Kozcaz and Yaffe

to pick 6 random papers.

Peter might object that some of these papers aren't sufficiently "stringy" to count. But all of them use, in an essential way, ideas and techniques developed in a stringy context. And, for the purposes of "bean counting," all of them have author lists who Peter would consider to be "string theorists."

30. **A String Theorist**
November 17, 2006

"Let's have a challenge: can anyone think of a 2006 string paper as consequential as the pair Andy Randono just posted generalizing the Kodama state?"

Ummm, how about hep-th/0511286, in which Witten's open string field theory was finally solved analytically, or hep-th/0610251, in which planar N=4 Yang-Mills was finally solved analytically…

[OK, I generalized "2006" to "within the past year".]

31. **woit**
November 17, 2006

who/A String Theorist,

Please take this kind of partisan LQG/string theory argumentation elsewhere.

32. **MoveOn**
November 18, 2006

"(I too make mistakes, although these are only "formal", sorry)"

Plank => Planck

33. **Chris Oakley**
November 18, 2006

MoveOn,

You don't know that that is a mis-spelling. In England we say that someone is "as thick as two short planks" when they are not bright.

34. **plank**
November 18, 2006

"(I too make mistakes, although these are only "formal", sorry)"

Plank => Planck

— — –

That is deliberate. I have explained this but Woit censored it. Maybe he will censor this post too, I am yet to understand the criteria.

Nice to see people are concentrating on really important things (spelling) vs real mistakes where probabilities are greater than 1 and no need for normalization is hinted.

I for one would like the author to update the "probabilities" with the "correct" values, preferably summing less than 1 to accommodate some kind of other option such as the one (quite relevant I might add) brought up by Larsson.

Again, on my censored post I wrote that if these "probabilities" have been discussed without people making the most basic consistency checks, just imagine what probably happens on more esoteric math/QFT that is sometimes discussed here.

But then again, maybe I am "as thick as two short planks"
🙂

P.S: I would like to know where I can find the criteria used for deleting posts or is it just discretionary.

35. **You-know-who**
November 18, 2006

plank wrote:

P.S: I would like to know where I can find the criteria used for deleting posts or is it just discretionary.

— — — — —

Criteria is simple, if your post help to sold more copies of Not Even Wrong then is archived, otherwise it is deleted.

36. **woit**
November 18, 2006

Plank and I-don't-know-who,

I spent much of yesterday on planes, and at various airports was kept busy using my laptop to log in to my blog software and delete large numbers of comments people were submitting that were mainly devoted to insulting someone or other. I don't remember Plank's, but presumably it was part of one of several such threads that I deleted wholesale.

Whether I delete comments depends on lots of factors, including what mood I'm in. But if you submit a comment anonymously that's not about the original posting and that is insulting to anyone, there's a good chance it will be deleted.

I of course strongly encourage people to post comments that will sell lots of copies of Not Even Wrong and make me filthy rich, but if my policy was to delete comments that don't do this, the comment sections here would be extremely short….

37. **Tommaso Dorigo**
November 20, 2006

Dear a (nonymous? gnostic? pologetic? posteriori? bominable? to name just a few),

the future can't be so bad as to both provide no further clues on fundamental physics at the LHC experiments AND set the stage for yet another abominable string of useless years of pondering on the sex of angels…

Cheers,
T.

38. **plank**
November 20, 2006

"the future can't be so bad as to both provide no further clues on fundamental physics at the LHC experiments "

——-

Care for a justification as to why the future can't be so bad?

Seems to me you are trying to end a faith based initiative with a belief. That's ironic to say the least.

But I'd love to hear WHY the future can't be so bad. I sure hope it won't, but hope in something doesn't guarantee it happening.

39. **Arun**
November 20, 2006

Question – isn't the strong CP issue and whatever the cure is, say axions – also a unconfirmed part of the Standard Model?

40. **Peter Orland**
November 20, 2006

Arun Says:

November 20th, 2006 at 11:24 am
Question – isn't the strong CP issue and whatever the cure is, say axions – also a unconfirmed part of the Standard Model?

Yes it is. The axion is the only simple-minded solution to the strong
CP problem.

There was an intriguing alternative suggestion that the CP violating term (which is F*F) is a non-renormalizable operator if QCD is treated non-perturbatively (perturbatively, it is marginal and perfectly okay). If that were true, such a term would disappear at
low energies. Since we can't do a non-perturbative renormalization of the theory with this term, it isn't clear whether this idea still has merit.

41. **Tommaso Dorigo**
November 20, 2006

I would also like to make a general remark here. If a theory predicts the M_p/M_e ratio it does not mean it is The Theory, no more than if I come up with an a posteriori explanation of a set of funny events in my data the explanation has to be correct.

Don't let's forget. PREdictions have to PREcede observation, else a theory is just a good try. Do we need more tries ?

So please come up with values for the neutrino mixing angles, Bs->mumu decays, and other pieces of future reachable measurements, and not non-existing stuff that we will not see. But fast, they are about to get measured.

T.

42. **Jonathan Vos Post**
November 20, 2006

I partly agree and partly disagree with Tomasso. There is still considerable scientific value in a theory which RETRODICTS.

Retrodiction is making conclusions about previously gathered data, once theoretical advances in the specific field of research have happened.

An example of this was the explanation of the already-observed perihelion shift of the orbit of the planet Mercury. Newton's theories of the laws of motion, which in a deeper sense retrodicted Kepler, were unable to explain the phenomenon. Einstein's theory of general relativity was successful in the retrodiction.

Give me a retrodiction of M_p/M_e ratio, neutrino mixing angles, Higgs mass, or the fine structure constant, and I'll certainly pay attention. In some sciences, we have huge data sets, computational problems, and retrodiction is the main stream. Climate analysis, continental drift, there are many such examples.

43. **Tommaso Dorigo**
November 21, 2006

Ok Jonathan,

I have to agree with what you wrote above. My point was roughly that retrodiction by itself is not enough. You also need predictions that can be falsified.

Cheers,
T.

44. **J.F. Moore**
November 21, 2006

I've detected quite a tone of moderation in Kaku's recent interviews. You may recall this summer that he came out fervently (in the NYT?) bashing the Steorn free energy loonies. Easy target, but Kaku in the past has talked so much about wooly-headed nonsense that I was surprised to see him amongst those throwing stones. The cynical part of me thinks he's just mining new markets of less credulous people to sell his backlog of books; but maybe, just maybe he is cluing in to the winds of history and appropriately tacking?

As for Esquire, well, one might as well ask a theorist how to tie a Windsor knot or for the name of their tailor.

45. **Hitchhiker to the Galaxy**
November 23, 2006

I'm reading NEW (Not Even Wrong), a brave book. Has Prof Sokal ever been involved in string theory? He should write "Fashionable Nonsense II." If strong theoriests have not been able to decide on what M of M-theory should stand for, what about Mirage-theory?

46. **Who**
November 23, 2006

Malarky

http://www.worldwidewords.org/qa/qa-mal1.htm

47. **Jonathan Vos Post**
November 24, 2006

With that clarification, I agree with Tomasso.

Given earlier estimates by various writers here on the possible outcome of the String Theory debate, might there not be revisions in the light of Tomasso? That is, (1) by what trajectories with what likelihood will String Theory or M-Theory give apparently good retrodiction; and then ; (2) by what trajectories with what likelihood will String Theory or M-Theory give a prediction as such; and finally (3) by what trajectories with what likelihood will String Theory or M-Theory give a prediction which is tested and found at least approximnately right?

An example, admittedly not Physics at all, gives a good example of how an author who insists that she's doing science, produces controversy with a brilliant book of retrodiction. Only weakly can there be falsifiable prediction following: if traces of the lost colonists are found inland, if letters or other documents germane to the case are uncovered, for instance.

http://www.jhu.edu/%7Ejhumag/1101web/roanoke.html

Rethinking Roanoke

Seeking to solve the mystery of the "Lost Colony" at Roanoke, anthropologist Lee Miller, MA '87 (pictured at left) looks beyond the conventional culprits to spin a tale of sabotage and intrigue.

By Dale Keiger

— Jonathan

Postscript: what would really put this on the map, and boost Woit to bestsellerdom, would be a murder mystery involving String- and anti-string theorists. If some of the people blogging here met face-to-face and one thing led to another. Motive + means + opportunity = murder. Ladies and gentleman, one of us here in this room is the murderer. [the light go out] [gunfire]

Langlands on Langlands

November 21, 2006
Categories: Langlands

In a forthcoming issue of the AMS Bulletin, there will be a long review by Robert Langlands of the book p-adic Automorphic Forms on Shimura Varieties by Haruzo Hida. The book itself is on a very technical subject, but the review includes long sections by Langlands that are much more generally about the current state of the so-called "Langlands Program". While this inspired the "Geometric Langlands Program" that I've written about here recently, it's a quite different subject, one that is very much central to research in number theory. Basically it deals with number fields (extensions of the field of rational numbers), and the function fields of geometric Langlands involve very different issues.

At the same time as making available his review, Langlands also made available commented copies of his correspondence with various experts in the subject about a draft of the review that he had sent them. Much of the review itself is likely to only be accessible to experts, and this is even more true of the correspondence. Casselman comments:

I also have the impression that you have edited this review for the pleasure of experts, and that therefore the cutting-room floor is filled with the sort of stuff The Naive Reader might appreciate.

The response to this from Langlands is:

I had in mind explaining more, but the editing was not a matter of choice but of necessity. I did not understand enough to say more.

I suspect few people will be able to follow the discussion here, but it gives a good idea of what is going on in an active but very difficult area of mathematics.

Both of these documents are from a fantastic resource, a web-site set up by Bill Casselman which contains pretty much the complete works of Langlands on-line. If you want to know more about the Langlands program and where it comes from, there's lots of material worth reading on the site. One of the more readable sources for a beginner is the 1989 Gibbs symposium lecture on Representation theory- its rise and its role in number theory.

For a lower form of entertainment, there's another book review, of Leonard Mlodinow's *Euclid's Window*, which appeared in the AMS Notices. The review is pretty much completely over the top, beginning with the sentence:

This is a shallow book on deep matters, about which the author knows next to nothing.

Update: I should also have mentioned that last month there was a small conference at the IAS on The L-group at 40, in honor Langlands' 70th birthday.

Comments

1. **Peter Orland**
   November 21, 2006

   Since I haven't read Lenny Mlodinow's book, I don't know
   if the review is a fair one. Apropos of nothing, I took classes
   with Lenny at Berkeley before he turned into a writer for "Star
   Trek" and author of pop-science books. The review reminds
   me of the proverbial referee report, "This paper fills a
   much needed gap in the literature." (attributed, no doubt
   falsely, to Paul Halmos).

   But at any rate, I was impressed by Langland's breadth of
   knowledge of the classics of mathematics and of the history of
   science in general.

2. **Scott Aaronson**
   November 21, 2006

   I haven't read Mlodinow's book either, but I feel certain that if I disliked it, it wouldn't be for the reasons given by Langlands. Lewd and tawdry jokes? Cartoon caricatures of complicated historical narratives? A relentlessly late-20th-century perspective? Sounds pretty good to me…

3. **luny**
   November 21, 2006

   On a somewhat unrelated note, check out the closing talk of the Quark Matter conference,
   http://www.sinap.ac.cn/qm2006/ppt/Nov.20/Plenary-15/Larry-McLerran-QM061.ppt
   slide 19 onwards.
   The slides do not convey the extremely caustic sarcasm the speaker presented them.
   Considering the extremely political nature of this conference, this is interesting.

4. **fh**
   November 22, 2006

   "Sounds pretty good to me…"

   You are joking, right?

5. **Gumbi**
   November 22, 2006

   Langlands ironically characterizes his own review as "the pedantry of… one priggish mathematician" and I have to say that is pretty accurate. I have seldom seen a more blatant or arrogant display of a reviewer showing off his superior knowledge and mastery of every aspect of the subject covered by a book.

   I suggest that Langlands put a tiny part of his amazing mind to the study of what economists call "comparative advantage". He will learn that even if he is better than everyone at everything, there is still a role for the lesser minds of the world to make their own efforts, poor though they might be compared to what the great Langlands would have done if he had condescended to devote time to such matters.

6. **Kea**
November 22, 2006

When I met him, I thought Langlands was very nice.

7. **Scott Aaronson**
November 23, 2006

You are joking, right?

Yes, but I also had a serious point. When we talk about the history of science (or anything else), I *don't* agree that we're forbidden from generalizing, making moral judgments, telling stories with heroes and villains, cracking jokes, or asking questions that wouldn't have seemed pertinent at the time but do seem so now.

So for example, anyone who's not a total philistine "knows" that Bertrand Russell's *History of Western Philosophy* and E. T. Bell's *Men of Mathematics* are worthless as intellectual history: "biased," "simplistic," "reductive," "romanticized," and so on. In other words, the trouble with these books is that they're not dry as dust.

Wherever Russell and Bell get their facts wrong, one can and should take them to task for it. But beyond that, they're every bit as entitled to their romantic visions of history as their critics are to their own.

8. **Mark Hillery**
November 23, 2006

When Robert Langlands's review of Leonard Mlodinow's book came out, the two opinions I heard most frequently expressed, which coincided with my own, were that a review that is this over the top says more about the reviewer than the book, and that the AMS Notices should never have published it. Since I have known Leonard for 30 years and enjoyed his books, I wrote to the then editor, Harold Boas (who, weirdly enough, went to high school with Leonard Mlodinow – six degrees of separation anyone?), criticizing the decision to publish the review. Much to my surprise, I received a rather lengthy response. The main thing I remember about it is that I was accused of making an ad hominem attack on Robert Langlands. Why? I had suggested that in order to see what people who want to write popular accounts of mathematics and physics are up against, it might do Prof. Langlands some good to take a little time off from the Institute for Advanced Study and teach a precalculus course at a large state university. It's a tough audience out there and capturing and keeping their interest is not easy.

Writing about science, and especially mathematics, for a popular audience is a very different task than doing research in mathematics or writing scholarly accounts of it. Doing it well takes hard work and a great deal of writing skill. I have read several books by very good scientists, meant for a popular audience, that were quite dull, because the writing was flat. Those of us who work in science benefit from the efforts of those who work to make the public interested in our subject.

9. **fh**
November 23, 2006

"But beyond that, they're every bit as entitled to their romantic visions of history as their critics are to their own."

Whatever became of "Everyone is entitled to their own opinion, but not their own facts."?

Now we all are entitled to have our own mythologies, but just basic scholary and intellectual ethics/honesty forbid the kind of trivializing and falsifying of history for the purpose of entertaining.

Russel no doubt was very aware of the perspectives of the respective times he writes about. This does not mean that we can not or should not analyze them through a modern perspective, but to actually provide anything with any sort of valuable insight one needs to understand first. Their internal consistency and perspectives (as far as possible).

Something which by all appearances the author failed spectacularly at.

Russel uses western philosophy as a canvas to communicate certain insights and structures in philosophy. That is, the book tells us (to some degree at least) about Russell's philosophy not about the historical philosophies. Therein lies (one of) it's value(s).

Also there is a difference between "biased," "simplistic," "reductive," "romanticized," and "flat out wrong," "ahistorical," "without any understanding of the matter" etc… In so far as Langlands review is accurate the author also committed errors in terms of scholary precision that are unforgivable.

Then just as Russels use of a mythizised but essentially insightfull analysis of history is remarkable and praiseworthy, this use of an essentially wrong mangled and disfigured history for the sake of cheap entertaining is abhorrable.

10. **lostsoul Ph. D.**
November 23, 2006

The Langlands assessment of Euclid's Window is renowned as an example of OTT and sarcastic review – it must be (in)famous because even I have heard of it. It owes its celebrity to its acerbic style (which other AMS reviewers have cultivated in the past), and not to whatever it may convey about the book in question (which I have not read, but have heard people speak quite highly of). How different is it from some of the ordure heaped on Peter's book of late? Professional competence and, still less, genius do not guarantee civility and self awareness; just because a fellow is an ass-hole, it doesn't mean he's wrong. But, then again, it doesn't mean he's right either. The perils of popularisation, I suppose.

11. **Jonathan Vos Post**
November 23, 2006

Mlodinow wrote a PhD dissertation on how QM would work in a universe with an infinite number of spatial dimensions. This dazzled Gell-Mann, who had him appointed to a major faculty position at Caltech. Then the story gets interesting.

Mlodinow has had successful teleplay writing during and after his tenured Physics professorship at Caltech, a marvelous biographical book (Feynman's Rainbow), and collaboration with Steven Hawking, that will yield a new movie next year. Might there not be a sour grapes effect here? I admire Langlands' math very much, so far as I understand it. But this reminds me of Carl Sagan being blackballed from the National Academy of Sciences by those jealous of his popular success in books, TV, and film.

12. **L group**
November 23, 2006

It's great that AMS published the review. It will have no impact on the popularity of the book, as the readership of the Notices can form their own opinions and are a drop in the ocean compared to the mass market. More exacting viewpoints, such as Langlands' or that of Weil in earlier times, are much less visible, and it is good that they are represented from time to time.

13. **JC**
November 24, 2006

Jonathan Vos Post,

Was Mlodinow a postdoc or tenured faculty at Caltech? From reading his book, I got the impression he was only a postdoc. What was amusing were Feynman's anti-string tirades, he wrote about.

14. **Peter Woit**
November 24, 2006

The talk mentioned by "luny" seems to be stirring up trouble, see

http://asymptotia.com/2006/11/23/nuclear-guy-goes-nuclear/

and

http://backreaction.blogspot.com/2006/11/more-on-adscft-and-rhic.html

where there's the usual Lubos attack on anyone who points out that string theory is being over hyped.

Mark,

I also think the Langlands review of the Mlodinow book has more to say about Langlands than about the book, and I think the AMS Notices should not have published it.

Reading the many reviews of my book has provided a new perspective on book reviews, and why they often have little to do with reality. While many reviewers misunderstood what I had to say in the book, I can't complain, since on the whole, their misunderstandings led them more often to write positive things than negative things. The one time I tried to contact a publication about whether they were willing to correct something completely factually untrue that a reviewer had written (KC Cole in the LA Times) was an eye-opening experience. The people there made clear to me that their policy (and presumably the policy of many places) is that:

1. While in the rest of the newspaper they care about factual accuracy, reviews are opinion pieces and thus they have no problem with factually inaccurate statements appearing in them.

2. The only standard for these reviews is whether the reviewer has some sort of argument, no matter how irrational and crazy, for what they wrote.

15. **D R Lunsford**
November 24, 2006

Langlands says about his thesis

Examining again, after forty years, the verification of the basic estimates of the second part, I found a large number of misprints, so that it would have been difficult if not impossible to follow my arguments line by line. I have tried in the present version to correct the misprints, but cannot be certain to have fully succeeded.

The thesis remains, to my regret, my only active encounter with partial differential equations, a subject to which I had always hoped to return but in a different vein.

That is remarkable in a way. I wonder what he meant?

-drl

16. **D R Lunsford**
November 24, 2006

Mlodinow was apparently a writer on the Picard version of Star Trek. I think that is the harshest review I've ever seen, of anything! Well no, Ebert's 0-star movie reviews are a lot like this..

I can't say a word about Langlands as a mathematician but he is certainly an entertaining writer. The review leaves one with an uneasy feeling for some reason, as if culture itself, and not just mathematical culture, were in decline, and he is lamenting the passing of it.

-drl

17. **Jonathan Vos Post**
November 24, 2006

JC:

From the Preface to "Feynman's Rainbow: A Search for Beauty in Physics and in Life" [Warner Books, May 2003):

"… this book is about my time just after graduation in 1981, when I was on the faculty of the Calfornia Institute of Technology…."

"I had been given my fellowship…. Did I really fit in here, with two Nobel prize winners down the hall…."

"This book tells the story of my first year on the Caltech faculty"

I admit to the ambiguity: does he mean that "faculty" includes "postdocs"?

I don't think that Caltech had adjuncts, but they also had Instructors, such as my friend Dr. Thomas McDonough. Instructores teach classes, so I suppose are faculty, but not tenure track.

I'd ask Mlodinow directly, but he is sufficiently busy that he answered an email of mine when I invited to speak on a track of an international conference where I chaired 3 sessions, but explained why he had to decline the attractive offer due to how busy he was.

Good question. I'm still not sure.

Do you agree with the possibility of my Carl Sgan / sour grapes analogy? Amusingly, Brian Green had a bit part in the science fiction film “Frequency” and now seems to have been an executive consultant or the like on the big-budget feature “Dej Vu.”

Here we go again with Esquire Physics, or People Magazine Physics.

18. **Heavy Ion Guy**
November 25, 2006

Dear all,

At quark matter this year Larry did make some strong statements about string theory. This was a response to one paragraph in Brian-Greenes Op-Ed piece in the New York Times.

The full paragraph which makes reference to the heavy ion work is here “And in a recent, particularly intriguing development, data now emerging from the Relativistic Heavy Ion Collider at the Brookhaven National Laboratory appear to be more accurately described using string theory methods than with more traditional approaches. ”

This statement in the public press should be called for what it is — a wild exaggeration of the state of the theory in the heavy ion community. Larry’s main point was that if such claims are to be made in the public press they should be held to the same standards of scientific scrutiny as the “more traditional approaches”. Larry was correct to sharply criticize this remark.

That being said, a number of people (who normally think about heavy ion collisions) have recently been calculating transport coefficients with AdS which they really wanted to know in QCD. These transport coefficients have been inferred from hydro and kinetic models of the heavy ion reaction and generally are smaller than what would be expected from a weakly coupled plasma. It is important to emphasize that each of hydro/kinetic models have a number assumptions that can certainly be challenged. Nevertheless to reproduce these small transport coefficients with perturbation theory consistently requires an extrapolation outside the domain of validity into the strong couplign regime. The AdS/CFT provides a useful foil to extrapolations based on perturburation theory into this regime. Many aspects of the strongly coupled N=4 plasma are markedly different from the qualitative features of perturbation theory. It would be nice to determine a relatively clean observable which would probe these qualitative differences. I think that AdS/CFT is a phenomenological long shot, but it is good (and relatively easy) fun.

19. **JC**
November 25, 2006

Jonathan Vos Post,

The Sagan/sour grapes analogy sounds a bit like what happens in many fields and/or niches. “Serious” folks who find success in the popular media/culture, are seen as “sell outs”.

Do you think it is related to how people like to “compare” themselves to others? Many “serious” people see popular culture as something “low brow”.

20. **Juan R.**
November 25, 2006

> This statement in the public press should be called for what it is — a wild exaggeration of the state of the theory in the heavy ion community. Larry’s main point was that if such claims are to be made in the public press they should be held to the same standards of scientific scrutiny as the “more traditional approaches”. Larry was correct to sharply criticize this remark.

Ok, but would be remarked that calls for holding to the same standards of scientific scrutiny in public press are not new. They are part of well-known scientific ethics guidelines in other communities.

About stringers reactions, Clifford is right when states that there is nothing wrong with being publicly excited by the possibility, though.

In the same vein, there is nothing wrong being publicly excited by the possibility string theory leave us nowhere in main goals.

Still, Clifford fail to understand that Greene was critized not because claimed possibilities but because claimed more accuracy becoming from the string part when even there is not qualitative agreement with experimental data.

About Motl comments, i would cite

> But physics is not just about the fundamental Lagrangians as some of the idealized theorists could think. It is about the understanding of all possible phenomena and about predicting of the outcomes of experiments in a wide variety of physical situations.

That is just the reason because string theory is in trouble.

Unfortunately, Motl presents the same class of disturbed reasoning about string theory that Greene. For instance Motl writes

> The most far-reaching new technique in the research of strongly coupled gauge theories is undoubtedly the AdS/CFT correspondence.

Just a few words after you discover that by most far-reaching technique means that many people have very good reasons to expect that this tool will be the most efficient one to understand questions about the strong force. But nobody proved this, just again, like in last 40 years, we receive good hopes from stringers.

21. **alfons**
November 25, 2006

Also interesting (wrt NEW):

http://www.cbc.ca/thecurrent/2006/200611/20061124.html

22. **Cynthia**
November 27, 2006

I somewhat agree with Prof. Langlands’ criticism of Leonard Mlodinow’s “Euclid’s Window”. Unquestionably, Mlodinow is considerably sloppy at conveying “this math/physics pathway towards strings” to the lay public. He’s especially bad at explaining this connection between Non-Euclidean geometry and Einstein’s geodesics of spacetime.

Needless to say though, I’m still rather in shock to discover that Mlodinow holds a PhD in physics. Yet I’m in ever greater shock to find out that he has a specialized degree in perturbation theory, no less! I'd surmise, however, the poor-quality of Mlodinow's work is perhaps more of a reflection of his sub-optimal skills as a pop-physics writer, and less of an intrinsic indicator of his physics know-how.

Just think about it for a moment though, being a writer for "Star Trek" films, Mlodinow isn't required to compose meaty, substantive material; he simply needs to create a script with lots of golly-gee-whiz wizardry.

23. **foo**
November 27, 2006

Why is the review of Mlodinows book by Langlands over the top? Is it factually wrong?

If a book is as badly written as Mlodinows book seems to be, then a slamming is appropriate, I'd say.

24. **Bob**
January 5, 2007

Nice discussion – very enlightening.

Media and Other News

November 25, 2006
Categories: Uncategorized

There's filming going on outside my office window today, right at the entrance to the Columbia Mathematics building. The film is Brief Interviews With Hideous Men, with a screenplay based on the David Foster Wallace book of the same name.

On the way in here I stopped at a bookstore and took a look at the new Thomas Pynchon novel *Against the Day*. Over at Cosmic Variance, Mark Trodden and Sean Carroll are Pynchon fans and have postings about this. I was quite fond of *Vineland* and enjoyed some of Pynchon's earlier books, but he lost me with *Mason and Dixon*, and this new one doesn't look promising. From flipping through it, one important topic seems to be quaternions and their relation to 4d space-time geometry, and a group of characters are called the Quaternioneers. I almost bought the book, thinking that it was my duty as a chronicler of the nexus of math, physics and popular culture to read the thing. But when I picked it up, its sheer heft caused an immediate feeling of discouragement, so I put it back down and will wait for reports from others.

There's a new movie out this week called Deja Vu, and evidently string theory play a significant role in its time-travel/multiverse based plot. My colleague Brian Greene was scientific consultant on the film, and the Cosmic Log MSNBC blog has a story about this, noting that he's also involved in another time-travel movie project (Mimzy), and appeared in yet a third (Frequency). The MSNBC story does explain that time-travel is not a big topic of current physics research, but describes physicists as "intrigued by the trippy concepts spawned by string theory – indicating that the universe could follow any of 10^{500} possible courses, and that our course seems to be going down just the right path to allow for the development of stars, galaxies and life" (the story does note that some people have a problem with this and gives "Not Even Wrong" a mention). While I gave up on the idea of spending $35 on the Pynchon book and devoting endless hours to reading its more than 1100 pages, spending $10 and devoting a couple hours to watching a cheesy movie seems like a much more viable way of fulfilling my blogger duties, so I think I'll be doing that this evening.

Continuing on the science fiction theme, next year's Les Houches summer school will be on the topic String Theory and the Real World.

In further media news, last week I talked with someone from the CBC radio program The Current, and supposedly they were going to use some of this in a program on the controversy over string theory that aired yesterday. Also someone tells me that this past week's issue of Der Spiegel has an article on this.

Finally, for some non-media science fact, the week before last there was a workshop in Paris on High Energy Physics in the LHC Era. There were quite a few interesting talks, including one by Albert de Roeck on post LHC accelerator possiblities (mainly the SLHC, a luminosity upgrade of the LHC), by Alessandro Strumia on astrophysical neutrino experiments, and by Fabio Zwirner on supersymmetry (see page 18 of his slides for a good reason not to believe in supersymmetry). The summary talk was given by Luciano Maiani, who argued that the next machine after the LHC should be a larger proton-proton machine, on the SSC size scale, to be built in the US (since it wouldn't even fit at CERN), with an electron-positron collider to be built at CERN.

Update: For a more general discussion of the question of whether new physics that solves the naturalness problem will be visible at the LHC, see a recent posting by Tommaso Dorigo, who is reporting on a conference going on in Bologna, especially the talk by Andrea Romanino.

Update: The movie is completely generic, including no strings, but just a standard-issue wormhole.

Comments

1. **Moshe**
   November 25, 2006

   Someone in the New Yorker seems to agree with you on Pynchon's latest opus:

   http://www.newyorker.com/critics/books/articles/061127crbo_books

2. **Jud**
   November 25, 2006

   Re the New Yorker review, "Pynchon writes for Pynchon," "Pynchon writes that way just to show he can," and "Pynchon is too damned obscure" are common complaints among Pynchon non-fans, famously including the Pulitzer jury that reviewed Gravity's Rainbow. Lots of these accusations of egotism and playing with readers seem to stem from Pynchon's use of humor. When the heck did we start requiring our artists to be so solemn? Joyce's "Ulysses" has pages and pages of extended humorous vamps; what got him into trouble wasn't the humor, it was closing the book with 50 pages of a woman giving herself an orgasm. So 80 years ago, sex was the sin; now, apparently, it's laughter.

3. **M**
   November 26, 2006

   It is exciting that, while this epoch of endless speculations is coming to an end, we have no best guess for what LHC will see. Discovering if supersymmetry anticipated LHC results by 20 years, or if we lost 20 years on a wrong speculation will be a sensible issue, like discovering that strings predict things univocally up to a 10^500 ambiguity.

   Concerning colliders, a reasonable point of view is that now we don't know if we will need to do precision physics with a 500 GeV ILC or explore higher energies with a SSC-like collider. So it would be reasonable to keep the second option open, by working on the needed magnetic technologies, and by finding a better name for it.

   Moving all US resources to the first option will put the US in first position, if the physics case for a linear collider will turn out to be correct. If not, it will be difficult to switch to the SSC-like option. The possibly wrong physics case for ILC has been already presented to the congress by somebody who got a recent Pinocchio award: are we sure it is a good strategy?

4. **r hofmann**
   November 26, 2006

   My personal feeling about the ILC is that there will be a lot of very clean new-physics signals to be seen at such a machine nothing to do with supersymmetry. I believe that hadron colliders at higher energies will be more or less redundant judging from the lessons learned with the linear collider and LHC.

5. **K.**
   November 26, 2006

   It is very remarkable that the MSSM adds 100-odd parameters to the SM, yet it seems to need a 1% level tuning in order to evade LEP bounds. In principle it should be able to fit about 30 elephants wagging their tails, if one believes Fermi 🙂

As first light nears, the LHC saga is building up more and more suspense. Who says particle physics is not thrilling?

6. **Tommaso Dorigo**
   November 26, 2006

   Indeed, it is thrilling.
   And, that were not enough, there's more. One of the most thrilling things is that many of the people working in the experiments (Atlas and CMS) do not seem to realize, as close as 1.5 years from serious data taking, how complex are things at a hadron machine.

   At the workshop I attended in Bologna Anna Maria Zanetti gave a very nice talk on the experience in CDF with the start of Run II and the first problems with data taking. I knew all of the stuff, so I could lay back and enjoy the terrified look in the face of many at the sight of what a lost proton beam can do to your apparatus, what fraction of the silicon sensors were burned by beam incidents as the first few nanonarbs of data were being collected, and the ugly effects resonance (not a particle, but the nasty effect of elastic forces at work) can do to sensitive parts of your electronics – exemplified by wirebonds broken in scores in the silicon readout as data taking causing lorentz forces passed through the resonant frequency of the bonds.

   I think it will really be exciting…

   T.

7. **King Ray**
   November 27, 2006

   On the topic of science fiction, I don't know if many people know that Harlan Ellison and Sidney Coleman are childhood friends. I was at an LSC lecture given by Ellison back in the 80's at MIT in Kresge Auditorium and Harlan introduced Sidney as a close friend and had him stand up in the audience.

8. **D R Lunsford**
   November 27, 2006

   Science fiction was much more entertaining when science fact had its act together.

   -drl

9. **King Ray**
   November 28, 2006

   Has anyone seen this?

   http://www.physorg.com/news83924839.html

   Cosmologists expose flaws in anthropic reasoning

   Many scientists never liked it anyway, and now Glenn Starkman from Oxford/Case Western and Roberto Trotta from Oxford show that too many details—and too many unknowns—mean that anthropic reasoning gives inconsistent values of the cosmological constant, some that are far from current estimates. In their recent paper, "Why Anthropic Reasoning Cannot Predict Lambda" (Physical Review Letters), Starkman and Trotta find that different ways of defining the probability of observers in different universes leads to vastly different predictions of the cosmological constant.

   "The significance of our work is to offer a concrete example of how anthropic methods of reasoning can be used to reach conclusions contradictory to those usually arrived at," Starkman told PhysOrg.com. "This suggests to us that anthropic explanations of fundamental questions should be treated very cautiously."

   …

10. **Thomas Love**
    November 28, 2006

    If "anthropic methods of reasoning can be used to reach conclusions contradictory to those usually arrived at" perhaps it is not anthropic methods of reasoning which are wrong. Perhaps the usual modes of reasoning are wrong. Not that I believe in "anthropic methods of reasoning ".

11. **Chris W.**
    November 28, 2006

    TL,

    I think the point that Starkman and Trotta are trying to make is that anthropic reasoning provides way too much room for fudging, to the point where it can't be relied upon to yield coherent conclusions. That is, it can yield one set of conclusions that contradict another set of conclusions previously arrived at by anthropic reasoning.

12. **Chris W.**
    November 28, 2006

    More on the media front: A story for NPR by Robert Krulwich on manufacturing universes in the lab, with Brian Greene and Andre Linde featured prominently:

    "Just imagine if it's true and there's even a small chance it really could work," [Linde] said. "In this perspective, each of us can become a god."

13. **Chris Oakley**
    November 29, 2006

    Re: the Universe Machine, I was not clear on how much customisation is possible. Presumably you can adjust basic things like the total mass, amount of dark matter and Hubble constant, but what about the finer adjustments? Could you, for example, create a universe where the majority of what theoretical particle physicists say to reporters is not total bullsh*t? *That* would be a challenge.

14. **Ari Heikkinen**
    December 1, 2006

So, the movie has to be cheesy because Brian Greene was their consultant on science? I'm sure Greene, even though he's a string theorist, is competent enough to be a credible consultant for a film.

Just pointing this out, because you keep pointing out that people should read your book first before commenting it, so wouldn't it be only fair to first watch the film and comment it afterwards?

15. **Peter Woit**
December 1, 2006

Ari,
You're completely making things up I never wrote. I didn't characterize the movie as cheesy because Brian was a consultant, I characterized it as cheesy because I think pretty much all big-budget thrillers with time-travel themes are cheesy. I had seen the trailer for the movie, read reviews and other articles about it, and all the evidence was that that this would be a cheesy movie. I often like to see cheesy movies, so happily paid my money expecting to see a cheesy movie, and was not disappointed.

16. **anonymous**
December 1, 2006

"I often like to see cheesy movies,"

(Ahem, perhaps you shouldn't be so frank, or some bits of what you say in jest will be quoted out of context by your enemies to make you sound cheesy.)

17. **Chris W.**
December 3, 2006

More on the media front: In the December 1 Science Journal (*Wall Street Journal*) Sharon Begley focuses on anthropic reasoning, with special attention to recent criticisms and research that bears on its alleged effectiveness in answering substantive questions about things like the value of lambda.

In particular she discusses work published in August (in Physical Review D) by Graham Kribs, A Universe Without Weak Interactions (hep-ph/0604027, 4 April 2006).

(Kribs is also a co-author of another interesting paper posted last April, on an alternate derivation of some important AdS/CFT predictions — hep-th/0602110.)

18. **Benni**
December 5, 2006

Job Openings:

2 String theory professorships available in munich:

http://www.asc.physik.lmu.de/MATH/job_openings.shtml

We expect to fill one W1 Junior professorship position (Junior research group) within the Excellency Cluster "Origin and Structure of the Universe" on Extra Dimensions, Strings and Branes (see here for more information). The announcement will appear soon.

We expect to fill one W2 Associate Professorship position as well as one W1 Junior professorship position within Elite Master Study Programme "Theoretical and Mathematical Physics" (see here for more information). The announcements will appear soon.

There aren't many string theory professorships available in europe, and especially in munich, theres lots going on on high energy theory:

especially here:
http://www.theorie.physik.uni-muenchen.de/asc/

here
http://www.mppmu.mpg.de/english/index.html

and here:
http://www.universe-cluster.de/

I think these are worthy positions to take.

Benni

Two Reviews

November 29, 2006
Categories: Not Even Wrong: The Book

Two reviews by physicists of my book and Lee Smolin's have recently appeared.

The first is by David Lindley in the Wilson Quarterly. Lindley has written some excellent popular books about physics, including one about quantum mechanics entitled Where Does The Weirdness Go?, and he has a new one about the history of the uncertainty principle that I look forward to reading. He is also the author of The End of Physics: The Myth of a Unified Theory, which appeared back in 1993, and was the first popular book I know of that explained that the project of finding a unified theory of particle physics had started to run into trouble and was not making progress. In some ways Lindley's book was a precursor to John Horgan's later The End of Science, and Horgan acknowledges Lindley's influence. Lindley notes that I say a bit about his book in mine, saying I misstate one of his arguments. He has to be right about this, so I'm rather curious to know what I got wrong (an internet search shows that he has been pretty successful at keeping his e-mail address non Google-accessible, so I haven't yet contacted him to ask him about this). His description of the books is reasonably accurate and straight-forward, and he ends with the following observation:

As for string theory, it's likely to unravel only when its practitioners begin to get bored with their lack of progress. Like the old Soviet Union, it will have to collapse from within. The publication of these two books is a hopeful sign that theoretical physics may have entered its Gorbachev era.

December's Physics Today has a review of the same two books by Kannan Jagannathan, under the title Scrutinizing string theorists and their future. One unusual aspect of the review is the peculiar description of the reviewer, unlike any I've ever seen before in Physics Today:

Kannan Jagannathan is a professor of physics at Amherst College in Amherst, Massachusetts. Though his background is in high-energy theory, he has no strong stake or expertise in string theory.

It's an indication of the highly partisan nature of the controversy these books have stirred up that Physics Today seems to have found it necessary to include this sort of unusual disclaimer. It appears true that Jagannathan is no partisan, but his disclaimer of expertise on the subject covered by the books is associated with a rather superficial take on the arguments these books are making. His only attempt to evaluate whether there is anything to the claims Smolin and I make about the problematic behavior of some string theorists is to have read Lisa Randall's recent popular book Warped Passages, and found that she doesn't seem to share our concerns.

While avoiding saying anything about the substance of my arguments, Jagannathan does take exception to the style of some of them, suggesting I should use more "temperate rhetoric", and avoid "anecdotes and private communications." Perhaps he's right that tactically it would have been better for me to write a more impersonal book, bending over backwards to appear to not be expressing personal opinions. For better or worse, I chose to do something quite different; to write a very personal book, expressing precisely what I think, and describing experiences that have led me over the years to these opinions.

Comments

1. **Alejandro Rivero**
   November 29, 2006

   The comparision with the Soviet Union is interesting. Acording Lenin, or more properly according a reported talk of Lenin with one of the leaders of Spanish anarchist union CNT, the system was foreseen to allow "about 50 years" (this was 1927 or so), after which it would fall, expectedly towards anarcho-socialism. This bit or the prediction was a sort of failure, but except on this detail Lenin mechanism worked as a clockwork: not having internal renovation of leaderships, the system burnt out itself.
   Now, the hope with physics, or with string theory, is that science self-implemented destruction mechanisms, rooting a theory without experimental input, will work.

2. **Thomas Love**
   November 29, 2006

   Lindley's new book: "Uncertainty: Einstein, Heisenberg, Bohr, and the Struggle for the Soul of Science" sounds like an interesting read, but I'll bet he doesn't mention that the uncertainty principle is classical physics, coming from any wave phenomenon. It is in the classical theory of light and the classical theory of acoustics.

3. **D R Lunsford**
   November 30, 2006

   Peter – you were entirely correct to write in the style you did – it reminds me of Klein's highly personal and very instructive style and humanizes the whole thing. It's an excellent book regardless of your point of view because the arguments are logical and the facts are clear.

   -drl

4. **Tony Smith**
   November 30, 2006

   An Amazon UK synopsis of Lindley's 1993 book "The End of Physics: The Myth of a Unified Theory" says in part:
   "… the particle physicists …[in]… their attempts to reach … for a unifying theory … and unable to subject their findings and theories to experimental scrutiny … have moved into a world governed by mathematical and highly speculative theorizing,
   none of which can be empirically verified.
   David Lindsey argues that a theory of everything derived from particle physics will be full of untested – and untestable – assumptions.
   And if physicists yield to such speculation, the field will retreat from the high ground of science, becoming instead a modern mythology.
   This would surely be the end of physics as it is known today. …".

   Since superstrings were dominant in high-energy physics theory in 1993, it is obvious that Lindley was referring to superstring theory as the "… modern mythology … the end of physics …"
   Now. over a decade later, Lindley says that superstring theory
   "… [i]s likely to unravel only when its practitioners begin to get bored with their lack of progress …"
   which indicates to me that Lindley disagrees with the hopeful view of Alejandro Rivero that
   "… science self-implemented destruction mechanisms, rooting a theory without experimental input, will work …".

   Further, given such things as the videotaped reactions at KITP to Johnson's first talk, the blogs of Lubos Motl and Clifford Johnson, etc, it seems to me that it is very unlikely that superstring "practitioners" will "get bored with their lack of progress" at any time in the foreseeable future,
   so

it seems to me that Lindley's view is that if you want to do high-energy theoretical physics in the forseeable future, your options are either:
do superstring theory or;
work in a relatively small niche field (tolerated as long as it stays in its place as a small niche) like LQG or;
somehow make a living outside the established physics community, record your work for posterity as best you can, and hope that, if it is correct, somebody 100 years or so from now might stumble across it.

In short,
superstringers own the City Hall of high energy theory, and you can't fight City Hall.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

5. **A**
November 30, 2006

Surely past years look like a Breznevian stagnation era. Since 1960 a failure seemed likely but not unavoidable, so a whole apparatus of people who got power thanks to communism, togheter with a few strong believers, could force everybody to keep going for 30 more years in doing something that almost nobody believed.

And the end can take different forms.

Susskind could be a Gorbachev, who destroyed communism in an attempt of changing it. Maybe Motl dreams to be a Kim, who still keeps pure communism in North Korea. More likely, the end could be something constructive like in China.

6. **Thomas Larsson**
November 30, 2006

according a reported talk of Lenin with one of the leaders of Spanish anarchist union CNT, the system was foreseen to allow "about 50 years" (this was 1927 or so)

Nitpick: Lenin died in 1924.

7. **Alejandro Rivero**
November 30, 2006

Nitpicked. You forced me to check notes. Effectively, Angel Pestaña was sent to Moscu in 1920, while Nin was sent in 1921 (1922?). I think the note I remember was a comment from Buenacasa on the report of Pestaña back in Spain. Victor Serge explains he was with Pestaña when they first meet Lenin, but he does not mention this remark about the temporality of the dictatorship.

8. **D R Lunsford**
November 30, 2006

Tony – the entire thesis of the Lindley book is absurd, because we have working example from 1865 – Maxwell's unification of electricity and magnetism. This introduced a new scaling constant called – let me think – oh yes, the speed of light.

-drl

9. **King Ray**
November 30, 2006

I don't think string theory will die until the funding is cut. I knew a guy in grad school who got several postdocs doing string theory who told me he didn't even believe in it but he was getting all these great jobs doing it. I think it is wrong to work on something in science you don't even believe in just to make money and get a job. Science is supposed to be a higher calling.

10. **L group**
November 30, 2006

Experimental input, although the ultimate source of the theory, is itself overhyped in the discussions of string theory and theory in general. If ultimately the only output of string theory is a more efficient formalism for QFT with no additional physical predictions, that is itself a major contribution. Such an existing or expected contribution may or may not justify the current levels of interest in string theory, but neither does harping on the lack of current relation to experiments.

11. **Carl Brannen**
December 1, 2006

On the topic of what will destroy string theory, a very interesting 800+ page book is Collins' on gravity waves. I would never have picked up a book on physics by a "sociologist", except that I took two graduate physics classes from Joe Weber who is the subject of a good bit of the book.

The main subject of the book is how it is that communities of physicists reach agreement on subjects that are debated. The author's well supported contention is that debate does not always change minds and that in the worst case debates both sides are supported by well respected physicists. Since 99.99% of what we know, we know not by direct experience but instead because we trust the judgement of others (for example, I've never measured the mass of an electron), the physics community has no way to decide the issue (think Einstein versus QM).

Eventually, truth is decided when one side or the other gets an advantage at minting graduate students, and the other side dies off. My guess is that the advantage comes about partly by numbers, and partly by way of attractiveness of the calculational techniques, but the author does not discuss this. After one side or the other wins, history is then rewritten so as to make the side that died off look like idiots and old fogies. And a new generation is taught that science is 100% correct.

One can argue that physics has done well with this technique of fighting over the truth — most physicists believe that the foundations of physics are solid. But from a sociological point of view, even if string theory is wrong, we can expect its proponents to go to their graves unrepentant, and to dominate theory until they retire.

12. **Peter Orland**
December 1, 2006

To Carl Brannen:

There are examples of scientific controversies which were resolved by
data and not by sociological forces (such as one side dying off). I don't find Kuhn's thesis especially convincing.

People are naturally reluctant to abandon their theories and ideas for notions suggested by others. Thus science tends to stay a conservative enterprise, in which only the best thought out ideas eventually predominate. This may sound overly idealistic, but it's often true.

As an example, consider Harlow Shaply who refused to believe that spiral nebulae were galaxies external to our own. In spite of his heated arguments with Hubble and others, he eventually changed his mind (and wrote a popular book entitled "Galaxies"). The point is that Shaply didn't have to die for the galaxy concept to win acceptance. There are other examples (Hoyle and the steady-state vs big-bang universe is one).

Anyway, people really can be persuaded by evidence. observational or otherwise, if only to save their own reputations (or in some cases, to get a post-doc).

13. **Johny**
December 1, 2006

Peter Orland: let me add one more example to your list. Schroedinger refused for a long time to accept Einstein's photon idea. He was convinced by Bohr that there was no other way to interpret the photoelectric effect. According to an account by Heisenberg, the discussions were long and difficult. So much so that Schrodinger fell ill, but still Bohr sat by his bed and went on discussing the issue. In the end, Schrodinger didn't die and accepted the particle nature of light.

14. **Tony Smith**
December 1, 2006

King Ray said that he ".. . do[esn]'t think string theory will die until the funding is cut. …"
and
that he "… knew a guy in grad school who got several postdocs doing string theory who told me he didn't even believe in it but he was getting all these great jobs doing it …".

As King ray went on to say, "… it is wrong to work on something in science you don't even believe in just to make money and get a job …".

In light of King Ray's comment, it seems to me that the most accurate USSR analogy for present-day superstring theory is Lysenkoism, which:
got dominant government funding;
viciously attacked competing approaches; and
had worker/supporters who knew about the flaws in Lysenkoism but supported it anyway in order to continue getting funds and jobs, and to preserve positions of prestige and political/funding power.

From a wikipedia entry at http://en.wikipedia.org/wiki/Lysenkoism on Lysenkoism:
"… mass-media presented him [Lysenko] as a genius who had developed a new, revolutionary technique.
… Lysenko … was … supported by the … propaganda machine, which overstated his successes and omitted mention of his failures.
Instead of making controlled experiments, Lysenko relied upon questionnaires …".

To me, that sounds a lot like the present-day superstring cult of personalities, every one relying on praise from each other, similar to Lysenko's "questionnaires" whose replies consited mostly of sycophantic praise.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS –

15. **Tony Smith**
December 1, 2006

Sorry about the omitted PS –
I was going to remark about how long Lysenkoism lasted and how difficult it was to bring down, but I found writing it to be too depressing if it is an indicator of how long the superstring cult is to last and how difficult it is to bring down, so I stopped writing after "PS -".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

16. **Graduate**
December 1, 2006

I also though of Lysenkoism as a possible analogy to what is going on with string theory. I didn't find it satisfying.

1. For one thing, Lysenko was viewed as providing metaphorical evidence for the validity of certain national policies (As Lysenko sought to vernalize wheat, the soviet people where thought to be vernalized by the harsh policies of the Soviet Union, becoming stronger and hardier with more harsh treatment; or so the theory went). There is no such unholy alliance between string theory and politics.

2. Another important distinction is that Lysenko ignored the results of actual experiments. In string theory, there are no experiements for the theorists to ignore.

I have one question however. I have been wondering about the way theoretical physicists argue mathematics. I understand the mathematical way of proving statements. But I've been confused by the way physicists argue in mathematical sketches. How does one decide which of the debaters has 'won' ? It seems to me entirely possible to string together plausible conjectures and get several contradictory statements. And that indeed seems to happen often. I would appreciate some insight into this aspect of theoretical physics culture.

17. **Peter Woit**
December 1, 2006

TheGraduate,

Physicists characterize all sorts of different things as a "proof at physicist's level of rigor". These range from cases where one has overwhelming evidence from numerical calculations that something is true and cases where something is true for sufficiently well-behaved functions (and the exact characterization of "well-behaved" has not been sorted out), to cases where the evidence is very fragmentary and far from convincing. Traditionally physicists haven't worried too much about whether they really have a sold argument for a conjecture, since it has often been the most efficient tactic to just assume things will work, and go ahead and calculate under that assumption. If the calculations lead to inconsistencies or disagreement with experiment, that's the time to go back and check assumptions.

One big problem with string theory is that the lack of any experimental tests means that all you have to go on is the lack of inconsistency, and given the way the theory is built on a lot of unproved conjectures, this can get problematic.

18. **Tony Smith**
December 1, 2006

Graduate said
"… Lysenko ignored the results of actual experiments.
In string theory, there are no experiements for the theorists to ignore. …".

Actually (in my opinion) there are substantial experimental results that show absence of supersymmetry,
and
those results (in my opinion) have been effectively ignored by superstringers choosing successively heavier scales of supersymmetry breaking as successive experimental results failed to see supersymmetry.

The superstringers (in my opinion) are acting in a way that Feynman considered unscientific in the book Feynman Lectures on Gravitation" (Addison-Wesley 1995) at pages 22 and 23, where Feynman said:

"… a famous professor … asked "… how are you sure that the photon has no rest mass? …
I [Feynman] answered … "… I would be glad to discuss the possibility that the mass is not of a certain definite size. The condition is that after I give you arguments against such mass, it should be against the rules to change the mass."
The professor then chose a mass of 10^(-6) of the electron mass.
My [Feynman's] answer …[was given]…
Then he [the professor] wanted to know what I would have said if he had said 10^(-12) electron masses …
[again, Feynman's answer was given]…
After this, the professor wanted to change the mass again, and make it 10^(-18) electron masses …
[again, Feynman's answer was given]…".

Feynman went on to say
"… We do not want to do similar things in attempting to construct a theory …
we should be prepared to put forth definite theories … and be prepared to reject them if they are inadequate. ".

I think that superstringers have been in violation of Feynman's standards for some time,
and
that superstringers are, and have been for some time, effectively ignoring experimental results indicating that superstring-type supersymmetry does not exist.

It also seems relevant that now, with indirect evidence indicating that even the LHC will not see superstring-type supersymmetry, superstringers such as Lubos Motl are attempting to move the goalposts yet again, exemplified by Lubos Motl saying on his blog, about superstring theory, such things as:

"… we are confident that an answer to these questions exists in the non-supersymmetric case and it will be unique just like it was in the supersymmetric cases. …".

Contrast that statement
with Lubos Motl's past view of supersymmetry as a fundamental part of superstring theory, exemplified by his rejection (in 2004) of a non-supersymmetric string theory model by saying "… "String theory" is a shorthand for "superstring theory" …".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

19. **King Ray**
December 2, 2006

Tony, very interesting analogy. I hadn't heard of Lysenko before, but he definitely reminds me of some people I have known.

I'm sure there are many people working on string theory that don't really believe in it; the one person I mention was just the one honest enough to admit it.

I think string theory is just a mutual admiration society; if you agree with everything they say you are a super genius, otherwise you are stupid and a crackpot. I don't think many of them are actually that bright, and none of them have any aesthetic sense or physical intuition such as Einstein had. They are clearly not independent thinkers like Einstein either, but conformists.

20. **Thomas Larsson**
December 2, 2006

In string theory, there are no experiements for the theorists to ignore.

String theory predicts supersymmetry – not seen.

String theory predicts 496 gauge bosons – 12 seen.

String theory predicts extra dimensions – not seen.

String theory predicts a large and negative cosmological constant – the cc is small and positive.

String theory predicts proton decay – not seen.

Aether theory predicts aether wind – not seen.

But then again, Lorentz and Poincare didn't count negative experiments any more than Gross and Witten.

21. **TheGraduate**
December 2, 2006

"Between 1934 and 1940, under Lysenko's admonitions and with Stalin's blessings, many geneticists were executed (including Agol, Levit, and Nadson) or sent to labor camps. The famous Soviet geneticist Nikolai Vavilov, was arrested in 1940 and died in prison in 1943."

This is a quote from Wikipedia.

Look, there is really no comparing Lysenkoism and string theorists. There are lots of historical examples of legitimate scientists who used their academic power to exclude people who disagreed with them. One of these incidents would be a more appropriate comparison.

22. **Peter Woit**
December 2, 2006

TheGraduate is right. Please stop with the highly exaggerated attacks on string theory and string theorists. These are off-topic, not enlightening, and kind of repetitive.

23. **Ludmilla**
December 2, 2006

>String theory predicts supersymmetry – not seen.

The jury is still out on susy, extra dimensions, the Higgs boson, etc. At least until 2008-2010.

Actually, it's not so clear to me that string theory really *predicts* susy. One of the most disappointing features of string theory (or, should I say, of string theorists) is that no clear-cut predictions are made. That's why some say it's *not even wrong*.

In my opinion, string theory is here to stay. Most likely not as a TOE, but as a math-phys framework. Personally, I don't believe in TOEs, stringy or otherwise, I think it's just a silly idea.

24. **anonymous**
December 3, 2006

Maybe the behavior of string theorists is more predictable than string theory: here are my testable clear-cut predictions (or maybe just my worries) about their official reaction to some possible future LHC results:

if LHC sees supersymmetry, eureka we have up to N=8 of them.

if LHC sees extra dimensions, eureka we have 6 or 7 of them.

if LHC sees technicolor, eureka we have AdS/CFT.

If LHC sees dark matter, eureka we have discrete gauge symmetries.

if LHC sees nothing new, eureka eureka we have the anthropic landscape.

Sorry for this non politically-correct comment, but I believe it's true. Hopefully the next few years before LHC will allow to improve the physics and/or the sociology of string theory.

25. **Thomas Larsson**
December 3, 2006

The jury is still out on susy, extra dimensions, the Higgs boson, etc. At least until 2008-2010.

Sure. Every experiment that does not see evidence for susy or extra-dimensions is irrelevant, such as muon g-2, B_s oscillations, permanent electric dipole moment, no deviations from Newton's law, etc.

H. A. Lorentz did not give up on aether theory that easily neither. For years he discovered increasingly complicated, Rube-Goldbergesque constructions, instead of accepting the obvious conclusion of the Michelson-Morley experiment. He never went as far as making anthropic arguments, though (a large aether wind might be incompatible with human life).

Of course, he had his reasons. Aether theory was the only game in town – it follows from Newtonian mechanics and the Galilei group, which had passed every test for 200 years. And he certainly did not care about some crackpot patent clerk, who was ignored by everybody until Max Planck made him his assistent in 1910.

26. **dan**
December 3, 2006

Dear Thomas Larsson,

"String theory predicts a large and negative cosmological constant – the cc is small and positive."

Hasn't this been successfully addressed by the KKLT paper, which shows that string theory can account for a small positive cosmological constant?

27. **Ludmilla**
December 3, 2006

> Sure. Every experiment that does not see evidence for susy or extra-dimensions is irrelevant, such as muon g-2, B_s oscillations, permanent electric dipole moment, no deviations from Newton's law, etc.

No, every experiment that does not see susy, extra dimensions, technicolor, preons, strings, whatever, is not irrelevant. It just puts tighter bounds on each of those possibilities. At some point those bounds can completely exclude one of the possibilities, or at least make it phenomenologically irrelevant.

As far as I know, all known models of new physics are currently pushed into a rather uncomfortably tight region of their respective parameter spaces by experimental data. But

we are not there yet. Ruling out susy (in its
phenomenologically significant variants) will take LHC data.

If you want to know my personal opinion, the MSSM and extra
dimensions will not be seen at the LHC. But then, opinion
is not fact. Also, that point of view does not solve any
problems and, in fact, makes them worse. Without susy, how
do you solve, for example, the LEP paradox and the dark
matter problem? I don't have an answer, do you?

28. **D R Lunsford**
December 3, 2006

TL – that was hysterical. I can see that paper,

"Aether-Wind und Lebensmöglichkeit"

Poor Lorentz, he really was very brilliant. Relativity was so much simpler than what he was doing.

-drl

29. **Chris Oakley**
December 3, 2006

> TL – that was hysterical. I can see that paper,
>
> "Aether-Wind und Lebensmöglichkeit"

I cannot. I doubt that, even in his darkest dreams, he would have contemplated anything so ludicrous, and if he had, he would have being a laughing stock. Now some prominent physicists try to make a laughing stock of those who do *not* believe in the Anthropic Principle, which I suppose only shows how far we have fallen.

30. **anonymous**
December 3, 2006

http://www.kuro5hin.org/story/2006/10/31/161746/39

"String Theory and the Crackpot Index

"…Certainly, Dr. Greene has been been working for a long time (10) on a paradigm shift (10), towards which Einstein struggled on his deathbed (30). For his effort, his theory has no equations (10) and no tests (50). With the starting credit, that much makes 105 points.

"Is Dr. Greene a crackpot? No. But is this how physics should be presented to the public?"

Wonder why Woit doesn't discuss this stuff?

31. **Peter Woit**
December 3, 2006

anonymous,

I saw the story you mention, but the main answer to why I didn't think it was worth discussing is embedded in what you quoted. I think there's a lot wrong with string theory and how it is pursued, but Brian Greene is not a crackpot, and neither are most string theorists.

I disagree with Brian about a scientific issue, the prospects for string-based unification, and, as a result, also don't think the kind of public promotion of this idea he has engaged in is wise. But he's a perfectly reasonable person, willing to admit that string theory may be wrong, just trying to promote and pursue ideas he believes in. I've known him for a long time, work in the same department, and talk to him regularly. I think both of us see our disagreements as scientific ones and want to avoid personalizing them. If you want to engage in Brian-bashing, do it elsewhere.

32. **Thomas Larsson**
December 4, 2006

Hasn't this been successfully addressed by the KKLT paper, which shows that string theory can account for a small positive cosmological constant?

With 10^500 parameters one can fit 2*10^499 tail-wriggling elephants. Or the value of the cosmological constant.

33. **Thomas Larsson**
December 4, 2006

*No, every experiment that does not see susy, extra
dimensions, technicolor, preons, strings, whatever, is not
irrelevant.*

Ludmila, I was responding to Graduate's claim that "In string theory, there are no experiements for the theorists to ignore." So evidently he, as many other string theorists, thinks that none of these experiments is relevant.

34. **Ari Heikkinen**
December 4, 2006

Speaking of books..

I'm soon ordering some books from amazon and just out of curiosity, was thinking of buying a book about string theory and another that points out problems with it.

I'd basicly prefer something with actual equations for backing up any claims instead of merely philosophy (I already have Greene's book about string theory, but it was, although excellent read, a bit too basic without any equations whatsoever), so while I'm leaning towards that Zwiebach's "first course" book on strings (based on searching on amazon), which one should I pick for a rebuttal for it, Lee's or Peter's? What's the difference? Wouldn't want to end up buying another book with just claims and nothing to back them up, be it critics or advocates.

35. **Steve Myers**
December 4, 2006

I hope this isn't entirely off topic, but I've been thinking about popular books and articles on science. How good are they? When I read one on something I know well I find they are too thin or skip the "hard" stuff that you need to understand to even know what's going on. There is a good book by Longair on physics but would you classify it as popular? Scientific American, for example, is often too thin and can give a wrong impression of a field.

When I think about explaining my own rather simple work in a popular way I find that I have to explain FFT and signal processing, etc. What bothers me that even a Dirac prize winner can make a mistake about entropy and the arrow of time, what kind of mistakes are in stuff that I'm not familiar with — like Biochemistry?

This is a serious questions: can one even write a good popular book on a subject — like theoretical particle physics without simplifying too much. I've discussed this with my computer scientist son who says he knows of nothing in his field. I know of & enjoy as light reading books by Gamow, Pagels, Einstein, Feynman, etc., but to even have a vague idea of what's going on in a field I don't know of any better or easier way than reading the technical stuff and working on a problem.

36. **Peter Woit**
December 4, 2006

dan,

The problem with KKLT is that it "accounts for" a small CC by using a very complicated Rube Goldberg setup (Susskind's description), and if you believe this works, you can "account for" just about anything, your theory is unpredictive, and you're not doing science. Thomas inaccurately characterizes KKLT as have 10^500 parameters, which is not right, it's 10^500 solutions, but this doesn't change the problem.

Ari,

My book has some more technical material than Smolin's, but it is still written without using equations, and it was not possible in this format to explain in technical detail many things. I also included quite a few suggestions for more technical further reading. One problem with Zwiebach's book is that it only covers a small part of the string theory story, not taking you very far at all into the issue of how to connect string theory to real physics.

Steve,

I think there's a wide variety of "popular" books out there. Quite a few oversimplify and don't even try and give a legitimate explanation of what is going on. But some do an excellent job of giving an accurate picture of a field, even if a true understanding of the subject requires spending a lot more time and working out some basics techical details for oneself. To my taste, the best books I've seen often take a historical point of view, showing how a scientific subject evolved to the point it is now. Knowing the history of a subject is often a good place to start when trying to learn the basics.

37. **King Ray**
December 4, 2006

I really enjoyed Abraham Pais' Subtle is The Lord, a biography of Einstein with equations along the way to his discovery of GR.

http://www.amazon.com/Subtle-Lord-Science-Albert-Einstein/dp/0192806726/sr=8-4/qid=1165253958/ref=sr_1_4/002-0203434-5813668?ie=UTF8&s=books

38. **John Gonsowski**
December 4, 2006

A lot of things that string theorists talk about exist outside the context of string theory. Brian Greene might talk about ten spacetime dimensions for string theory but John Baez can talk about them outside string theory. Greene might talk about exceptional algebras for string theory but Baez can talk about them outside string theory. Green might talk about string theory worldsheets but Urs Schreiber says there's a deep connection between a worldsheet and Feynman paths (Feynman was not a string theorist). Even the supersymmetry of superstrings exists outside string theory (and there are other types of supersymmetry too). The one nice thing about the anthropic landscape is that it has quite a few critics who are string theorists. Given the politics of physics, sometimes I think it might be easier just to pursuade string theorists to work on different things under the string theory umbrella than to get them to downsize their empire.

39. **Who**
December 4, 2006

I met a PhD student of Steve Giddings just in the past month—he was up from Santa Barbara to hear some talks. He mentioned what his thesis research was about: it wasn't string theory though it was somewhat LIKE string theory—IIRC it didnt need extra dimensions, maybe it lived in 4D like normal people. This is, of course, vague gossip—but I remember getting a
sense of drift, of a program morphing into something else. It wasnt quite cool
anymore to be doing core string, so you emphasized the distance.

40. **Ludmilla**
December 4, 2006

> Ludmila, I was responding to Graduate's claim

Thomas, you quoted from *my* message, so I thought
reasonable to assume you were responding to me. I still
think so.

> *The jury is still out on susy, extra dimensions, the Higgs boson, etc. At least until 2008-2010.*
>
> Sure. Every experiment that does not see evidence for susy or extra-dimensions is irrelevant, such as muon g-2, B_s oscillations, permanent electric dipole moment, no deviations

41. **King Ray**
December 4, 2006

Giddings is Witten's student, isn't he?

I agree that we should explore 4D theories more fully. Higher dimensional theories have never been successful, going back to KK. I think by going to higher dimensions people are taking the easy way out. There should be a theory entropy statistic that is a measure of how complicated a theory is, and very complicated theories should be avoided.

42. **Thomas Larsson**
December 5, 2006

Ludmila, your first post was a response to my statement "String theory predicts supersymmetry – not seen." Evidently you found this objectionable.

String theory predictions have always been wrong in the past. Why should the LHC change this good old tradition?

43. **Ludmilla**
December 5, 2006

String theory predictions have always been wrong in the past. Why should the LHC change this good old tradition?

🙂 That's a good joke. But the same thing could be said of you and every other guy around. So, why should the LHC confirm any of *your* predictions, this one in particular?

44. **Thomas Larsson**
December 5, 2006

Why susy or extra-dimensions are unlikely to be seen at the LHC? Perhaps because other experiments put quite tight constraints on any deviation from the SM – unitarity triangles etc. Susy already requires fine-tuning at the percent level.

45. **Tony Smith**
December 5, 2006

Thomas Larsson said "String theory predictions have always been wrong in the past. ".
Ludmilla replied "… the same thing could be said of you and every other guy around. …".

No.
Some models do make predictions that have NOT always been wrong.
Some aspects of my personal example can be found in the abstract at http://arXiv.org/abs/physics/0207095 and in that paper.
My further work along those lines has been blacklisted by the Cornell arXiv,
but can be found on my web site.

I am not saying that my model is the only model that makes some correct predictions, only that it is one example and that the one example is all that is needed to refute Ludmilla's allegation that predictions of "every other guy" have "always been wrong".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

46. **Ludmilla**
December 6, 2006

one example is all that is needed to refute Ludmilla's allegation

Tony: "every other guy" means roughly "one out of every two". I have always considered you as part of that privileged predictive half.

47. **Tony Smith**
December 7, 2006

Ludmilla, my apologies for misunderstanding what you meant by "every other guy"
(half of the population,
as opposed to all of the population except Thomas Larsson)
and thanks for considering me to be part of the "predictive half".

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – My misunderstanding may have been an example of a linguistic version of the ambiguous image phenomenon illustrated by the images at
http://www.questacon.edu.au/html/tg_p8a.htm
The way the human brain works with respect to such things is interesting, but probably off-topic for discussion here.

Lots of Links

December 5, 2006
Categories: Uncategorized

Various things that I've run across recently that seem worth mentioning:

The proceedings of the big yearly lattice gauge theory conference that was mentioned here, Lattice 2006, are now available here.

The New York Times today in its Science Times section has a very interesting article by Dennis Overbye entitled China Pursues Major Role in Particle Physics. It tells some of the history of particle physics in China, describes the BEPC accelerator in Beijing which has just had a luminosity upgrade, and discusses the role China may play in future accelerator projects, especially the ILC. A US physicist who sometimes works at BEPC, Frederick Harris, is quoted as saying "The rate China is growing, this is something they could contemplate hosting in 10 years." Perhaps the future of high-energy frontier accelerator projects really will be in China.

There's also an associated article about the spring 1989 physics conference in Beijing that overlapped with the Tiananmen Square massacre, with David Gross quoted as saying "Until the shooting began, the visit was delightful." He and Vafa describe the bloody van that was supposed to be their transport, after it had been used to pick up wounded students, two of whom died.

Physics World has an article about physicists willing to make bets, called Physicists who fancy a flutter, featuring Tommaso Dorigo's recent $1000 bet with Gordon Watts and Jacques Distler over what the LHC will see.

New Scientist has a feature article Physics Goes Hollywood, about Costas Efthimiou and a course he is teaching at the University of Central Florida. The idea of the course is to have students watch movies, often ones with a sci-fi theme, then use real physics to critique the accuracy of scenes in the movies. Costas is a particle theorist who has worked on conformal field theories, and was a visitor here at Columbia for a while, from what I remember. He has several papers about teachng physics using films, most recently this one.

The Cao-Zhu paper giving the details of the proof of the Poincare conjecture that originally appeared in the Asian Journal of Mathematics has now been posted in revised form on the the arXiv. The revised version includes an apology to Kleiner and Lott for not acknowledging the use of their work in the original version.

Geometric Langlands is definitely the hot topic of the moment, I just learned about two more conferences about this that will take place soon. One is a Gottingen Winterschule, on January 4-7, the second is a program on Langlands Duality and Physics, to be held at the Schrodinger Institute in Vienna from January 9-20.

A couple weeks ago in Hamburg there was a conference on Kahler Geometry and Mathematical Physics, held to celebrate the 100th birthday of Erich Kahler.

Princeton will be hosting a conference next year entitled Geometry and the Imagination in honor of Bill Thurston's 60th birthday.

Dmitry Vaintrob, son of mathematician Arkady Vaintrob, has won a $100,000 scholarship from the Siemens foundation based on a research project in string topology. For more discussion of this, and what it means for string theory, see here.

Update: Two more.

Giorgios Choudalakis took a poll back in August of grad-students, postdocs and professors associated with Fermilab, asking them what they expected the LHC to find. Here are the results. More about this at Fermilab Today.

A commenter points out that this week's Zippy the Pinhead deals with one character's doubts about string theory. Over the last few years, the comic has often dealt with string theory, to see this try typing "string" into this search page.

Comments

1. **Tony Smith**
   December 5, 2006

   Since the New York Times requires registration, I read an article on the freeinternetpress.com web site that said in part:
   "… China Pursues Major Role In Particle Physics 2006-12-05 … Dennis Overbye ..
   The proposed I.L.C. would shoot electrons and positrons at each other with 500 billion electron volts of energy through a tunnel 20 miles long. An approximate price tag will be announced when the international collider planning team meets in Beijing this February.
   … the jockeying for where to put the machine has already begun. The host country for the collider would have the advantage of being the center of 21st-century physics, but would have to bear a larger share of the cost.
   Last spring, a report from the National Academy of Sciences urged the United States to do what it takes to get it built here rather than in Europe or Asia, or face the prospect of relinquishing traditional leadership in physics.
   … Frederick A. Harris, a professor of physics at the University of Hawaii, who works often at the Beijing collider … said, "The rate China is growing, this is something they could contemplate hosting in 10 years."
   … Given the explosive growth of China's economy and the vow of the country's leaders to emphasize science and technology, it is natural to wonder whether some future particles will have Chinese names the way many of the bright stars in the sky have Arabic names. …".

   It is interesting that the USA lobby to get the ILC located in the USA has been proposing a site at Fermilab in Illinois, rather than a site on the Pacific Rim such as California.
   Since the USA lobby's choice of Illinois was based heavily on the political influence of Illinois Republican Dennis Hastert as Speaker of the House
   and
   since the November 2006 elections changed the 2007 Speaker of the House from Illinois Republican Hastert to California Democrat Pelosi,
   it will be interesting to see whether the USA lobby will change its focus to a California site, or even whether the new political situation will weaken the USA lobby so that the eventual site for the ILC will be in Asia, such as Japan or China.

   As Peter said, "… Perhaps the future of high-energy frontier accelerator projects really will be in China. …".

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

2. **SSC**
   December 6, 2006

I am not so sure that we will need a linear collider with center-of-mass energy equal to 500 GeV, not much above the old LEP. Maybe LHC will tell that it is enough for studying spetroscopy of supersymmetric particles. Or maybe not: LHC could instead suggest that we need to explore higher energies. While the US probably have psychological problems with a SSC-like project, China might like the idea of a SinoSuperCollider.

3. **Mary**
December 6, 2006

Since the readers of this blog are possibly the only people in the entire world who will think these comics are funny, I can't resist leaving another link: Zippy the pinhead is doing string theory again.

I have no affiliation with Zippy, just an appreciation for the deeply surreal…

4. **Chris Oakley**
December 6, 2006

Re: Zippy: I don't think that twins should get involved like that – or maybe the inbreeding of its advocates is the key to the success of String Theory – ?

5. **Garbage**
December 6, 2006

I dont understand, how can "the SM Higgs" and "no new Physics" be separated options?
Unless "no NP" means *nothing at all*, not even the Higgs, and that would be more than New!!! 🙂
I mean, somehow the EW Theory has to be unitarized, so *something* HAS to happen, or is *nothing* a logical option? Could the theory break unitarity, at a scale lower than the Planck scale so no QG effect (forget XD) and still make sense, without *anything* ELSE happening?

I'd like to know what was in the minds of those 9%…

6. **J**
December 6, 2006

I'm not sure whether China would play an important role in future particle accelerator programs, but I am thinking that China will definetely be part of it.

7. **Thomas Larsson**
December 7, 2006

*I mean, somehow the EW Theory has to be unitarized, so *something* HAS to happen, or is *nothing* a logical option?*

Was no aether wind a logical option for H. A. Lorentz?

AFAIU, no Higgs at the LHC would disprove QFT. Now **that** would be interesting, but perhaps not very likely. But then again, what is most unlikely, no Higgs at the LHC or no single top production at the Tevatron?

8. **Garbarge**
December 7, 2006

"AFAIU, no Higgs at the LHC would disprove QFT."

which means that *some NP* has to appear to make up a consistent theory. I am talking about the scenario where NOTHING is found.

"Was no aether wind a logical option for H. A. Lorentz?"

Jap, and that's how Relativity was born, new physics at the time 😉

9. **Who**
December 7, 2006

Cosmic Variance has a short review of the two books
dated 07 December
by Polchinski
that will be appearing in American Scientist

http://cosmicvariance.com/2006/12/07/guest-blogger-joe-polchinski-on-the-string-debates/

10. **Who**
December 7, 2006

Pankaj Jain (SUNY Buffalo and Kanpur IIT) thinks he has detected an axion

http://www.physorg.com/printnews.php?newsid=84633896

and the article has been published in an IOP electronic journal

Search for new particles decaying into electron pairs of mass below 100 MeV/c2

P L Jain et al 2007 J. Phys. G: Nucl. Part. Phys. 34 129-138 doi:10.1088/0954-3899/34/1/009

article is available FREE for a limited time, but you have to create an account and get logged in

http://www.iop.org/EJ/abstract/0954-3899/34/1/009

http://www.iop.org/EJ/article/0954-3899/34/1/009/g7_1_009.pdf

it does not seem to have been posted at arxiv.

11. **Peter Woit**
December 7, 2006

Who,

The “axion” result looks highly unbelievable. I find it very hard to believe that if such a particle with that mass existed it wouldn’t have been seen before. Maybe someone more expert can give a more informed opinion about this.

12. **Matti Pitkanen**
December 7, 2006

It is not long time when an axion candidate from optical rotation experiments see posting of Lubos. The mass of this axion was in millieV range so that there is a difference of 12 orders of magnitude as compared to this axion. This makes me wonder why the term “neutral pseudoscalar particle” is not used instead of “axion”: is the reason political?

These two are not the only candidate for neutral pseudoscalars. I add here my comment to the blog of Lubos. The first evidence for pion/axion-like particles created in heavy-nucleus collisions near Coulomb threshold in MeV range came for more than 20 years ago. I developed a TGD based model for their strange production characteristics for more than 15 years ago.

TGD explanation was based on what I called leptohadron physics with quarks replaced by color octet excitations of electrons allowed by TGD view about color. The production mechanism was creation of leptopions in strong non-orthogonal E and B with action given by E.B. Coupling to photons is dictated by partically conserved axial current hypothesis and identical with the coupling of pion/axion. The mass of the lowest leptopion state is very nearly to 2 times electron masses. Also heavier excitations on Regge trajectory are predicted: actually entire spectroscopy of leptomesons and leptobaryons.

Exotic quarks with MeV mass scale and corresponding to Mersenne prime M_127=2^k-1, k=127, characterizing electron, play a key role in TGD based model of nuclei as string like structure with threads connecting nucleons having quark and antiquark at their ends. The model explains tetraneutron and predicts a new exotic states of nuclei.

Quite generally, TGD predicts entire hierarchy of p-adically scaled up variants of standard model physics with mass scales coming as powers of half octaves. p=about 2^k. k prime or power of prime, are favored. Hence pionlike states should exist at various length scales and serve as a signature of various scaled variants of QCD like physics.

*Mass scale 8 MeV (compare with 7 MeV) would correspond to k= 11^2, and thus power of prime.
*Mass scale of 16 MeV (compare with 19 MeV) to p=about 119=7*17.

Below a list of references about early evidence for pionlike states which people for some reason probably related to shortcomings of ancient theories(want to call axions.

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1328.

13. **Thomas Larsson**
December 8, 2006

“Was no aether wind a logical option for H. A. Lorentz?”
Jap, and that’s how Relativity was born, new physics at the time

This is not correct. The proponents of aether theory discovered much of the math of SR, e.g. Lorentz transformations and Poincare algebra. But abandoning aether theory, i.e. saying that Newton’s mechanics is wrong, was a step that they could not take. It took Einstein to do that.

Noone denies that QFT has passed every test for 60 years. But Newton passed every test for 200 years, and was nevertheless superseded by something better. 4D QFT has its problems (incompatible with gravity, renormalization mathematically dubious), and if not even a Higgs shows up at the LHC, it will have problems with unitarity as well. If so, a modification of QFT which preserves unitarity seems necessary.

I don’t seriously think this will happen. But saying that the LHC must see at least a Higgs to save unitarity is similar to saying that Michelson-Morley must see an aether wind to save Newtonian mechanics.

14. **Lux**
December 9, 2006

If no higgs particle were observed, then the higgs sector of the SM would be wrong. It would mean that the mechanism of SSB is different in nature from the simplest possibility implemented in the SM. There are many Higgsless models around, the LHC could tell us if any of them would be right in that case.

But it is the “Higgsless SM” that has problems with unitarity, not QFT. QCD is a QFT, and it’s perfectly unitary whether there’s a HIggs in nature or not.

15. **anonymous**
December 10, 2006

Lux, the trouble is any “Higgsless” model necessarily requires a new particle at or below 800 GeV-ish, which the LHC should be able to find. But 10% of the people in the poll said “no new physics”, which seems logically inconsistent — do those 10% of people really expect violations of unitarity? More likely they meant “only an SM Higgs” and didn’t notice the other option?

16. **Lux**

December 10, 2006

"Nothing at all" is clearly not an option. If anything, without a Higgs electroweak boson physics is going to get quite interesting.

17. **Bob**
January 5, 2007

"New Scientist has a feature article Physics Goes Hollywood, about Costas Efthimiou and a course he is teaching at the University of Central Florida."

Efthimiou is a well respected professor at UCF and an all-around good guy.

Polchinski Review at American Scientist and Cosmic Variance

December 7, 2006
Categories: Not Even Wrong: The Book

This month's American Scientist has a review entitled All Strung Out? of *The Trouble With Physics* and *Not Even Wrong* by prominent string theorist Joe Polchinski, and he has posted a slightly edited version of the review with some explanatory footnotes at Cosmic Variance. I assume there will be a lot of discussion of it over there, perhaps with Polchinski participating, so, even though I wanted to write some sort of response here, I'll leave comments off and encourage people to discuss this over at CV.

First of all I should say that I was quite pleased to see Polchinski's review. While I disagree with much of it, it's a serious and reasonable response to the two books, the kind of response I was hoping that they would get, opening the possibility of a fruitful discussion. Unless I've missed something, the only review by a string theorist to appear in a publication so far has been Susskind's almost purely ad hominem one in the Times Higher Education Supplement. There also are two other (not published in conventional media) reviews by string theorists that I know of, a serious one by Aaron Bergman, and a nutty one by Lubos Motl that Princeton University Press paid him to write for some mysterious reason. I've heard that several publications have had a hard time finding a string theorist willing to write a review of the books, which I guess is not too surprising. It's not obviously a rewarding task to involve oneself in a controversy that has become highly contentious, and where some of the main points at issue involve very real and serious problems with the research program one has chosen to pursue.

Much of Polchinski's review refers specifically to Smolin's arguments; some of it deals with the endless debate over "background independence", and the "emergent" nature of space-time in string theory vs. loop quantum gravity. I'll leave that argument to others.

Polchinski notes that I make an important point out of the lack of a non-perturbative formulation of string theory and criticizes this, referring to the existence of non-perturbative definitions based on dualities in certain special backgrounds. The most well-known example of this is AdS/CFT, where it appears that one can simply define string theory in terms of the dual QFT. This gives a string theory with the wrong number of large space-time dimensions (5), and with all sorts of unphysical properties (e.g. exact supersymmetry). If it really works, you've got a precisely well-defined string theory, but one that has a low-energy limit completely different than the standard model in 4d that we want. This kind of string theory is well-worth investigating since it may be a useful tool in better understanding QCD, but it just does not and can not give the standard model. The claim of my book is not that string theories are not interesting or sometimes useful, just that they have failed in the main use for which they are being sold, as a unified theory of particle physics and gravity.

The lack of any progress towards this goal of a unified theory over the past 32 years (counting from the first proposal to use strings to do unification back in 1974) has led string theorists to come up with various dubious historical analogies to justify claiming that 32 years is not an unusual amount of time to investigate a theory and see if it is going to work. In this case Polchinski argues that it took about 50 years to get from the first formulation of QED to a potentially rigorous non-perturbative version of the theory (using lattice gauge theory). The problem with this analogy is of course that in QED non-perturbative effects are pretty much irrelevant, with perturbation theory describing precisely the physics you want to describe and can measure, whereas with string theory the perturbative theory doesn't connect to the real world. When QED was first written down as a perturbative theory, the first-order terms agreed precisely with experimental results, and if anything like this were true of string theory, we wouldn't be having this discussion. For the one theory where non-perturbative effects are important, QCD, the time lag between when people figured out what the right theory was, and when its non-perturbative formulation was written down, was just a few months (Wilson was lecturing on lattice gauge theory in the summer of 1973, having taken up the problem earlier in the year after the discovery of asymptotic freedom).

Polchinski agrees that the key problem for string theory is its inability to come up with predictions about physics at observable energies. He attributes this simply to the fact that the Planck energy is so large, but I think this is misleading. The source of the problem is not really difficulties in extrapolating from the Planck scale down to low energy, but in not even knowing what the theory at the Planck scale is supposed to be (back to that problem about non-perturbative string theory…).

Weinberg's anthropic argument for the size of the cosmological constant is described by Polchinski as a possible "prediction" of string theory, and he recommends Susskind's book as a good description of the latest views of string theorists. I've been far too rude to Polchinski in the past in expressing my views about this "anthropic landscape" philosophy, so I won't go on about it here. He neglects to mention in his review that many of his most prominent colleagues in the string theory community are probably closer in their views on this subject to mine and Smolin's than to his, and that our books are the only ones I know of that explain the extremely serious problems with the landscape philosophy.

Recently string theorists have taken to pointing to attempts to use AdS/CFT to say something about heavy-ion physics as a major success of string theory, and Polchinski also does this. I'm no expert on this subject, but those who are like Larry McLerran have recently been extremely publicly critical of claims like the one here that "Physicists have found that some of the properties of this plasma are better modeled (via duality) as a tiny black hole in a space with extra dimensions than as the predicted clump of elementary particles in the usual four dimensions of spacetime." My impression is that many experts in this subject would take strong exception to the "better" in Polchinski's claim.

Finally, about the "sociological" issues, Polchinski disagrees about their importance, believing they are less important than scientific judgments, but I'm pleased to see that he does to some extent acknowledge that there's a serious question being raised that deserves discussion in the theoretical physics community: "This convergence on an unproven idea is remarkable. Again, it is worth taking a step back and reflecting on whether the net result is the best way to move science forward, and in particular whether young scientists are sufficiently encouraged to think about the big questions of science in new ways. These are important issues — and not simple ones."

Again, my thanks to him for his serious and highly reasonable response to the two books.

Comments

1. **Michael Bacon**
   December 7, 2006

   I thought he treated you better than Lee. Apart from literary comments, and a couple of other hits, his focus on "predictions" was correct, but a conclusion that they are impossible seems a little over the top. Some of this other comments were fair, others were not. How's your book doing anyway?

2. **LDM**
   December 7, 2006

   Just because it is not ad hominem does not mean it is a good review…and it is not…in the sense that a layperson reading it would get an incorrect view of the challenges faced by string theorists.
   Polchinski makes little effort to be accurate and his bias is clear. For example, he incorrectly minimizes the lack of convergence (also mentioned by Penrose, incidentally) of the pertubative expansion. He mentions black holes as an application of string theory, when in fact these black holes are not ones we observe, and scarcely qualifies as an application as is normally understood by the meaning of the word.
   I must say after reading this review, I unfortunately lost a certain amount of respect for Polchinski.

3. **r hofmann**

December 8, 2006

From my own experience with him I agree with the statement that Polchinski is a fair-enough person and surely one of the fastest thinkers available today (not necessarily implying an important role in furthering our understanding of fundamental physics). Since he likes to point out that there are nonperturbative formulations of superstring theory I think it would be reasonable to mention the resolution of the vacuum instability of perturbative bosonic string theory in the presence of D-branes (Sen's work) which to my mind takes away a major motivation to introduce supersymmetry on the world sheet (just like the absence of a naive Higgs sector in the SM may forbid mention of a so-called gauge hierarchy problem).
As far as ADS/QCD as an 'explanation' for certain strong-interaction effects in heavy-ion collisions is concerned I am convinced that these claims are completely unfounded and dangerous because misleading.

4. **MoveOn**
December 8, 2006

"…which to my mind takes away a major motivation to introduce supersymmetry on the world sheet…"

Are you sure you know what you talk about? Susy on the world-sheet is needed to describe fermions, even in a theory that is not supersymmetric in space-time.

"As far as ADS/QCD as an 'explanation' for certain strong-interaction effects in heavy-ion collisions is concerned I am convinced that these claims are completely unfounded and dangerous because misleading."

Is this conviction based on a similar deep understanding of the matters you attempt to judge here?

5. **r hofmann**
December 8, 2006

MoveOn, I believe I know exactly what I am talking about! I was talking about the vacuum instability of a purely bosonic string theory which seems to be cured in the presence of D branes. My personal feeling about fermions is that they are emerging phenomena in certain field theories (YM to be precise) and thus even as a concept should not enter into something soooooo fundamental as a string theory (be it supersymmetric or not in spacetime). I appreciate constructive remarks for one thing. On the other hand, I have developed a certain immunity against put-down attempts of hot-shot string educators.

6. **wolfgang**
December 8, 2006

Peter,

I am just curious; How many copies of your book have been sold until now?

7. **MoveOn**
December 8, 2006

R Hofmann: " My personal feeling about fermions is that they are emerging phenomena in certain field theories"….

Well, if you can provide a model in which the fermions we observe, quarks and leptons that is, emerge from a YM theory, then surely many would appreciate it I do know that via the spin-from-isospin mechanism, fermionic excitations can occur in monopole backgrounds, do you mean something like that? But that hasn't to my knowledge lead to anywhere. And in fact there had been ideas around a while ago where the superstring spectrum arises as some kind of solitons from the bosonic string. Also that AFAIK didn't lead to anywhere.

For the time being, such speculations are much more far fetched than speculations about ordinary supersymmetric field and string theories. Same applies to your apparent disbelief in the HIggs mechanism (I think it was you who expressed that?). The moment you or someone else comes up with any remotely working "alternative" model, that would be great and hundreds would follow right away. But criticising string and/or susy field theories in a generic manner, in the sense "I don't like any of that" for ideological or whatever personal feelings without having anything concrete in hands, is too cheap. Just saying "fermions should be emergent" is in my humble opinion not enough to constitute "constructive criticism".

But constructve criticism is something I haven't seen in this debate anyway. And that's no surprise.

8. **Peter Woit**
December 8, 2006

Not sure what happened here, thought I had turned off comments…

Still, I encourage people to discuss the Polchinski review over at CV. And stop the off-topic and un-enlightening sneering and hostility.

As for how the book is selling, I have only a vague idea, based on the Amazon rankings. As far as I can tell it's doing better than the average book in this area, but is far from a best seller. In other words, somewhat better than Susskind's, nowhere near Brian Greene's. I'm quite happy that the book seems to be doing better than I expected, but, no, it's not going to make me rich. Publishers report sales to authors I guess quarterly, with reports taking quite a long time to be prepared. So, at this point the only sales report I have is from the end of June in the UK, soon after the book went on sale.

9. **r hofmann**
December 8, 2006

MoveOn, ok let's behave … Although Peter doesn't like people being too explicit about their own brain childs at this location I sketch you hereby what I believe a lepton is. SU(2) Yang-Mills theory has a confining phase whose ground state is a condensate of paired, massless magnetic center-vortex loops. Excitations above this ground state are unpaired or twisted center-vortex loops, each selfintersection point constituting a Z2 charge of mass Lambda (the YM scale). If you want to read more about this I refer you to our papers. Peter, please don't see this as an advertisement, its just answering MoveOn's question. In the absence of any experimental evidence of susy but in the presence of anomalies in z pinch experiments, PVLAS, and CMB low multipoles I don't quite see why you believe that admittedly speculative proposals of the kind sketched above are wilder than the MSSM for example.

10. **michael**
December 8, 2006

Dear Ralf,

What you say about the bosonic string is not quite correct. Sen's work has to do with open string tachyons, the closed string tachyon instability remains.

Michael

11. **M**
December 8, 2006

The practical reason why studying non-perturbative phenomena is important is that simple perturbative string models are not realistic. The fact that we now know the extreme non-perturbative limit and that it turned out to be dual to some other perturbative string (i.e. something unrealistic again) means no progress on the above crucial issue.

12. **huh?**
December 8, 2006

Actually, in many models of physics that explain the electroweak scale,
nonperturbative effects are thought to explain the ratio M_W/M_P which is
tiny. This is true in models where susy explains the higgs mass, for instance.
That has driven a lot of interest in studying nonperturbative phenomena.
It is simply not true that people study them only when perturbation theory
"seems to give the wrong answer"; often, as in the case of susy mentioned
above, one carefully looks for circumstances where an effect is
nonperturbative, in order to explain a small ratio in nature. Many such
dimensionless small ratios exist in our standard models.

13. **Michael Bacon**
December 8, 2006

Somewhat better than Susskind's, nowhere near Brian Greene's sounds like just the right spot.

14. **M**
December 8, 2006

huh?, I mean that string models in the weakly-coupled limit predict many non-existing massless moduli (e.g. dilaton gravity instead of gravity): a careful engineering work allows to (meta)stabilize all moduli, but this resulted in the infamous landscape. Years ago the hope was that non-perturbative effects could qualitatively improve the situation. However the present understanding of non-perturbative effects via strong/weak dualities means that in the opposite strong coupling limit one gets the same physics as in the weak coupling limit, and so again the same problems.

15. **Yatima**
December 9, 2006

Well, "The Trouble with Physics" is in the list of "Best Books of 2006" in this week's "Economist". Here's the list for the heading "Science and Technology":

The Revenge of Gaia: Earth's Climate Crisi and the Fate of Humanity. By James Lovelock.
The God Delusion. By Richard Dawkins.
The Omnivore's Dilemma: A Natural History of Four Meals.
The Trouble With Physics: The Rise of String Theory, the Fall of Science, and What Comes Next. By Lee Smolin.

Not an uncontroversial book selection, I would say.

16. **Damysus**
December 9, 2006

The edge.org in its last enquiry asked its correspondents to write a few lines about what they believe, but cannot prove. I believe, but cannot prove, that Ad/CFT with a dual QFT string definition will be found to be correct, but will need a redefinition of dimensions (4+1) and the realisation of a very heavy Higgs. Polchinski may be seeing a rather dim light at the end of a stringy tunnel…

Yet More Links

December 12, 2006
Categories: Uncategorized

The January Notices of the AMS is out. Two quite interesting articles, one of which is an interview with my colleague Joan Birman. She just recently officially retired, but, at the age of 79 is still very active in research and a major presence in the department, an example to us all. The second is an article by Anatoly Vershik about the Clay Millenium Prizes. Vershik argues that these million dollar prizes are not good for mathematics, and that the story of the proof of the Poincare conjecture shows why. Top mathematicians who think they have a chance of solving one of the Clay problems are going to work on them whether or not the prize exists (the money certainly didn't motivate Perelman). The prizes give the public a deformed view of what is important about mathematics and encourage unseemly squabbling about how "credit" for a solution will be assigned. Vershik writes:

In my opinion, all this clamor and fuss show that this method of promoting mathematics is warped and unacceptable, it does not popularize mathematics as a science, on the contrary, it only bewilders the public and leads to unhealthy interest.

There's an excellent article about James Clerk Maxwell in the December Physics World. It's the 175th anniversary of Maxwell's birth this year. He lived only to age 48, dying in 1879. The author of the article speculates that "Had he not died so young, Maxwell would almost certainly have developed special relativity a decade or more before Einstein."

For an update on the US federal budget situation for science, see this AAAS web-page. As far as I can tell, the situation is that (as often happens) the Congress has not yet passed FY2007 appropriations for most of the government, including the DOE and NSF, even though we're now more than a couple months into the fiscal year. As a result, these agencies are operating under a continuing resolution, without access to the increased funds that were supposed to flow because of the "American Competitiveness Initiative". The new Congress will have to deal with this after it convenes in January, and news reports I heard today said that Congressional leaders were considering not producing new appropriations bills but running the government on a continuing resolution for the rest of FY2007. Unclear to me what this means for science funding, but it doesn't sound good. Over the next few years, if the new Democratic Congress makes a serious effort to bring the US federal budget deficit under control, science funding may be under pressure.

At the Scientific American blog, J R Minkel has a story called Comic Books Looove String Theory, about developments in the Ex Machina comic, which is about "a retired semi-super hero turned Mayor of New York City who can control machines with his mind." In issue 10 a lunatic starts ranting

It's not about the branes, it's about the bulk. You were supposed to tell people… Witten is close, but we're closer.

Minkel doesn't mention the recent string theory themes in Zippy the Pinhead.

Witten's new paper with Gukov mentioned here is now available. It is about 160 pages long and generalizes the earlier Kapustin-Witten paper to the ramified case. This involves constructing "surface operators" in the 4d gauge theory, operators attached to surfaces in much the same way 't Hooft operators are attached to curves. Unfortunately it doesn't discuss connections to Khovanov homology that Gukov described in his Strings 2006 talk "Surface Operators in Gauge Theory and Categorification" (I'd provide a link, but the Strings 2006 site seems to be down). The authors also note that Frenkel and Gaitsgory have a "unified approach" to this ramified case, but that "Unfortunately, we make contact here neither with the use of conformal field theory nor with this unified statement. We hope, of course, to eventually understand more." So, there's lots more to do…

If you want to get an idea of what it costs to run a theoretical physics center, check out the report of the Michigan Center for Theoretical Physics. The interim director of the MCTP is Gordon Kane, and they will be hosting a symposium next month to celebrate his 70th birthday.

Over at Cosmic Variance, there's a discussion of the new Martin Scorsese film String Kings, which features "a scene showing work on an extension of the New Jersey turnpike, involving string henchmen (disguised with hard hats and overalls) a large cement truck and Peter Woit." I guess this doesn't seem like such a great plot idea to me for some reason. Personally I've been thinking that the whole recent controversy over string theory would make a great comic novel. The thing to do is to somehow get David Lodge interested…

Update: The Strings 2006 site is back up, and the Gukov talk mentioned is here.

Update: More about the FY 2007 science budget situation from Science and from FYI.

Comments

1. **a**
   December 12, 2006

   "Had he not died so young, Maxwell would almost certainly have developed special relativity a decade or more before Einstein."

   That article states:

   "A velocity appeared in his theory also, but with a different numerical value that had no obvious physical meaning. Maxwell plugged Weber's force ratio into his equations and discovered to his utter astonishment that the velocity exactly equalled the speed of light, which was then known experimentally to an accuracy of 1%. With excitement manifest in italics, he wrote, "We can scarcely avoid the inference that light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena."

   This isn't true, (1) the ONLY numerical value by dimensional analysis using the electric and magnetic constants is was 300,000 km/s, (2) Maxwell did not predict c.

   Dr Alan F. Chalmers' article, 'Maxwell and the Displacement Current' (Physics Education, vol. 10, 1975, pp. 45-9). Chalmers states that Orwell's novel 1984 helps to illustrate how the tale was fabricated:

   '… history was constantly rewritten in such a way that it invariably appeared consistent with the reigning ideology.'

   Maxwell tried to fix his original calculation deliberately in order to obtain the anticipated value for the speed of light, proven by Part 3 of his paper, 'On Physical Lines of Force' (January 1862), as Chalmers explains:

   'Maxwell's derivation contains an error, due to a faulty application of elasticity theory. If this error is corrected, we find that Maxwell's model in fact yields a velocity of propagation in the electromagnetic medium which is a factor of [root 2] smaller than the velocity of light.'

   It took three years for Maxwell to finally force-fit his 'displacement current' theory to take the form which allows it to give the already-known speed of light without the 41% error. Chalmers noted: 'the change was not explicitly acknowledged by Maxwell.'

Weber, not Maxwell, was the first to notice that, by dimensional analysis (which Maxwell popularised), 1/(square root of product of magnetic force permeability and electric force permittivity) = light speed.

The whole story is an insult to any physicist of integrity. Fairy tales distorted from historical facts don't have a place in physics.

Maxwell never wrote the Maxwell equations in vector calculus. He had 20 differential equations and never summarized 4 or 5 key equations, which were written by Heaviside. Hence the doubtfulness of the popular claim made http://www.mathpages.com/home/kmath103/kmath103.htm that Maxwell considered the need for a divergentless field using vector calculus and was led to his discovery in the same way Einstein invented general relativity:

'Just as the inclusion of the "displacement current" in Ampere's formula was the key to a Maxwell's self-consistent field theory of electrodynamics, so the inclusion of the "trace stress-energy" in the expression for the Ricci tensor was the key to Einstein's self-consistent field theory of gravitation. In both cases, the extra term was added in order to give a divergenceless field.'

The most important thing Maxwell did do, which he isn't given credit for, is predicting the electron in the 3rd ed of his Treatise on Electricity and Magnetism. (J.J. Thomson, editor of Maxwell's Treatise, is given credit for discovering the electron when he just measured the charge to mass ratio.)

2. **D R Lunsford**
December 12, 2006

The claim that Maxwell would have discovered relativity is utter nonsense. There is nothing in Maxwell's work that even discusses these issues, and with good reason – there were too many other basic problems to work on (and not just in electromagnetism) and no experimental evidence at all for the relativity of simultaneity. And just because Maxwell discovered that light is an electromagnetic phenomenon, this in no way "points forward to Einstein and relativity". If anything, it points forward to Lorentz. But even that is a stretch. What it does it to point into the mirror – at Maxwell himself, who needs no context other than the one he provided himself.

-drl

3. **Daniel Biss**
December 12, 2006

I have to say that I don't really understand Vershik's argument. The prize had nothing at all to do with stimulating mathematics research in the short term, which he essentially admits in various points in the article; thus, explaining that Perleman didn't do what he did because of the prize is totally irrelevant.

What the prize did do was to generate a ton of media attention for topology and Poincare. Vershik also addresses this and manages to conclude that it's a bad thing, mostly by inexplicably trying to blame the prize for the bad behavior of various mathematicians. Um, I guess that's possible, but I don't really see it.

Here's something I *did* see, though: in the weeks surrounding the Poincare/Perleman media explosion, I had probably more than a dozen long conversations about topology and the conjecture with non-mathematicians in social situations — and I was never the one who brought it up. Sounds like the prize generated press coverage which, in turn, generated a lot of discussions about math. Which sounds like a success to me…

4. **Peter Woit**
December 12, 2006

Hi Daniel,

I have mixed feelings about it, posted about Vershik because I thought it was an interesting and not often expressed point of view. The main thing about the Clay prize that bothers me is the way it depends on picking a person to give the $1 million to, or even worse, trying to decide how to split it among several people. Great mathematical results almost always involve the efforts of more than one person, with no sane way of assigning percentages to each person's contributions.

If I had my druthers, the way it would work would be for the $1 million prize to be awarded at the point the Clay committee agrees that the problem has been solved (or maybe wait a couple years), but that the money should not go to the people who solved it (who, after all, are likely to be rewarded by the university star system with amounts adding up to over $1 million). Instead, the comittee would give the money to some organization or organizations that could use it. In the case of Poincare, giving the money to the support of Russian mathematics research might be appropriate.

5. **Chris W.**
December 12, 2006

Almost any media phenomenon that attracts the attention of millions of people will end up leaving one with reasons to be ambivalent about its effects.

That's inevitable in a democratic mass culture, with enormous diversity in the exposure of the audience to mathematics, not to mention the diversity of individual temperaments and talents. In the case of the Clay Millenium Prizes it seems quite reasonable that the balance might lean to the negative where the culture of mathematics is concerned, for reasons like those Peter mentioned, while leaning in the positive direction where the general public's awareness and interest in mathematics is concerned.

Whenever I reflect on the experiences that first excited my interest in the sciences and mathematics, my positive appreciation for them is also mixed with a certain rueful appreciation for their vulgarity or superficiality, as seen in retrospect. It's enough that one eventually attains that level of understanding and is willing to talk about how one arrived at it. From this point of view there is little point in deploring the original experiences and the ignorance and credulity that might have accompanied them. I'm therefore inclined to agree with Daniel's apparently positive assessment, but then again I'm not a professional mathematician.

That said, it is just as well that misgivings are publicly expressed, because that can also contribute to public understanding of the practice of mathematics and the motivations for doing it. Any thoughtful person with a genuine interest in the subject can learn from the diverse viewpoints expressed in the discussion of the Millenium Prizes.

6. **Turkey Royale**
December 13, 2006

Vershik, who is a serious mathematician, articulates a viewpoint not often enough heard in The US (and western Europe?), namely that one studies mathematics or physics or whatever for the love of the thing and the interest in knowing and aesthetic reasons, and that the best mathematics/physics/etc. comes out of research so motivated rather than out of programmatic, externally directed, or `practically' motived research.

Vershik expresses himself with care and with nuance; he is not dogmatic and it was my impression that he takes seriously the motivations advanced by the Clay Institute for its prizes. He certainly does give credit to the motivation of stimulating youths with an interest in math, and he does not disparage the need for educating the general public about math (judging from his extensive professional involvements, he is well aware of the complexities of funding mathematics research). As an aside he seems to want to put on record an authoritative debunking of some of the claims made about Perelman, while still respecting Perelman's privacy (that is not providing correct information, simply pointing out the incorrect); whether he succeeded, I don't know, since I don't know the true state of affairs vis-a-vis Perelman.

Vershik does not agree with Paris Hilton than any publicity is good, whereas many mathematicians in `the West' do. One might crudely summarize his critique as – perhaps amateur pornography is not the best way to promote oneself.

Vershik operates with an implicit distinction between `unhealthy interest' and healthy interest in mathematics. He also worries that placing a priori value on the solution of certain problems accords unnecessarily importance to those problems, and perhaps makes the study of non-anointed problems, or themes, more difficult. By the way, he clearly does not view mathematics as the business of solving problems already clearly posed; I suspect for Vershik the activity of researchers like Gromov or Thurston (who in some sense are anything but problem solvers) is more congenial than was the activity of Erdos, although he respects also Erdos.

His is not a simplistic `mathematics for mathematics sake', although I may have made it sound that way. On the other hand, it seems to me he is implicitly critquing the programmatic, business part of scientific research in particular in the US.

7. **plank**
December 13, 2006

“Maxwell never wrote the Maxwell equations in vector calculus. He had 20 differential equations and never summarized 4 or 5 key equations, which were written by Heaviside.”

— — — —

actually I think he did manage to summarize the equations using a quaternionic formalism. It’s been quite a while since I’ve read this so I could be wrong.

8. **David Corfield**
December 13, 2006

On page 29 of this I expressed similar sentiments to Vershik’s. If the prizes have any effect on mathematicians, it won’t be towards the greater openness to which we aspire at the n-category café.

9. **D R Lunsford**
December 13, 2006

plank – That is right – that’s what really annoyed me about the relativity statement – it ignored Heaviside, Gibbs, Lorentz, Hertz, Roentgen, and on and on, all the people who discovered the phenomena and developed the theory before Einstein came along and put it all right.

For the record, Maxwell had his own phrases for div grad and curl (*con*vergence, slope, and *twirl*) and did in fact apply quaternions to his theory, sometime in the early 1870s before his book was published. I don’t recall if it is in the book but I think it is at least mentioned.

However he did not have grad – i d/dt, which you need to really do his theory with quaternions. Had he had that, THEN he would have been one step away from the Lorentz transformation.

-drl

10. **Peter Shor**
December 13, 2006

Of course, a novel about the string theory controversy (if it’s not one of those postmodern novels) requires some kind of ending. Here are some possibilities.
(1) The string theorists are unexpectedly vindicated by LHC. (Many string theorists clearly are hoping for this ending, even though it seems rather unlikely.)
(2) The hero has a mental breakdown, quits theoretical physics, and writes a semi-autobiographical best-selling novel. (Also unlikely.)
(3) Finer examination of the cosmic microwave background reveals a message from God, in which He reveals the true workings of the universe. None of the competing theories come even close to the right answer. (I’m not betting on this outcome either.)

11. **Chris W.**
December 13, 2006

At risk of beating a dying horse, I’ll ask yet again what “vindication” of string theory by results from the LHC is supposed to mean, in light of the string theory Landscape, which seems capable of “vindication” by just about any result one pleases.

Is this not the case? If so, why not? My assumption is that the current efforts to statistically survey the structure of the Landscape represent fairly desperate and ever more unpromising attempts to answer these questions. Without such answers the above-mentioned hopes of many string theorists would seem to be simple delusions, regardless of what the LHC reveals.

(All this reminds me of undergraduate attempts to prove some mathematical assertion in the wee hours of the morning, which end pathetically in “establishing” some tautology.)

12. **Jonathan Vos Post**
December 14, 2006

Is there really a Physics equivalent of this?

By definition, a [first order] Celebrity is someone who is famous for being famous. Paris Hilton is a second order Celebrity: someone who is famous for being unjustifiably a first order Celebrity. More and more, morning major network “news” programs feature interviews with second order Celebrities who have appeared on some “reality TV” show on the same network as the “news” program.

Although there have been Physicists who became first order celebrities, although treated as second order, such as Einstein, Sagan, and Hawking, I am not aware of any true second order Celebrity Physicists, aside from TV personality engineers masquerading as scientists, such as Bill Nye Science Guy, and the debate raging about String Theory. Am I wrong?

13. **Arun**
December 14, 2006

Maxwell’s treatise on electricity and magnetism is viewable on books.google.com

14. **Lame**
December 15, 2006

Peter:

We are begging, BEGGING you to give us an actual technical argument here; why do you ALWAYS rely on authority when any physics issue comes up? Much easier playing pretend physicist on your blog huh? Sure impresses all your fans, particularly convenient that it requires NO INTELLECTUAL EFFORT OR SKILL on your part! Nice deal.

You remind me of those annoying undergraduates who talk a big game but don't actually KNOW anything. How about backing up your words with some content for once? Can you do that without running away to Mommy/Daddy/d'Hoker and Phong to bail you out when the discussion requires actual knowledge? Tell us what is wrong with AMS with an argument other than "people who know tell me there is something wrong with AMS".

Lousy science journalists will fall for your act Peter, but you don't fool a single decent theorist.

15. **Jonathan**
December 15, 2006

The files from the strings 2006 talks still seem to be online and at least here in China I can access them.
http://strings06.itp.ac.cn/?id=agenda_arr

If you really can't see them from the US then e-mail me and I can speak to those in charge of the site.

J

16. **Peter Woit**
December 15, 2006

Jonathan,

Thanks! The site seems to have just been temporarily down. I'll add the link to the Gukov paper.

Lame,

Thanks for the hilarious parody of the behavior of Jacques, Clifford, Lubos and various of their anonymous allies over at Asymptotia. Very, very funny!

17. **D R Lunsford**
December 15, 2006

Arun – that is very nice – I see he does mention quaternions in passing but does not attempt a systematic development (articles 618 and 619 in ch IX). That is good because none was possible. It does make one wonder if he had considered adjoining d/dt to grad.

-r

18. **Ari Heikkinen**
December 15, 2006

"Vershik argues that these million dollar prizes are not good for mathematics"

I couldn't disagree more. I mean, if there's a million dollar prize, it'll make the brightest minds pursue those ideas worth the prize.

I think the most useful problems to the human kind should have prizes worth even more.

Surely, even Peter, as a mathematician, would gladly work on something worth a million dollars prize, if that's something he'd have a good intuition to work on.

19. **Ari Heikkinen**
December 15, 2006

And my only hope is that Peter Woit and Brian Greene can go have beers and disagree with respect.. 😉

If they can't, then science isn't probably worth any funding from the government's perspective..

20. **Peter Woit**
December 15, 2006

Ari,

I'm not so sure the million dollars makes much difference on whether people work on these famous, high-profile mathematics problems. If I thought I had a good idea about how to solve one of them, I'd work on it, even if it didn't have the million dollars attached. Unfortunately I don't have any promising ideas about any of those problems, so it's not an issue….

And don't worry, I and Brian have never had a problem disagreeing respectfully, and this is true of just about all the string theorists I know. The weird unprofessional behavior of a small number of them in response to scientific disagreements has very much surprised me in recent years.

21. **nontrad**
December 15, 2006

I've posted here several times, because I hang out here quite a bit; because I appreciate whatever it is that I appreciate about it…

But, for whatever reason, it seems appropriate at this juncture to say that as far as I am concerned NEW is well worth the effort and not *just* because I have similar dispositions to concerns in, for a complete lack of anything better to call it, the philosophy/economics/policies of today's physics but because I find this blog to consistently be a collection of excellent / affordable / cogent pieces of writing about topics I find fascinating. It's damn good blogging… is what I'm saying!

But, I think it speaks volumes that unnecessarily aggressive posts repeatedly appear here. Not that authors shouldn't think twice about their statements: This is the internet afterall and we all speak freely here. Instead, the record speaks for itself.

22. **Neville**

December 15, 2006

In my experience, much of the general population views mathematics as a dead subject, which reached its zenith with the discovery of long division. Many scientists and engineers of my acquaintance seem to think mathematical discovery was finished in the 19th century. The Clay Millenium Prizes publicize mathematics as a living subject. Perhaps some youngsters will find inspiration in the notion that mathematical mysteries abound.

23. **mathjunkie**
December 16, 2006

The author of the article speculates that "Had he not died so young, Maxwell would almost certainly have developed special relativity a decade or more before Einstein."

What is the basis of saying that?

24. **Steven H. Cullinane**
December 17, 2006

The January AMS Notices now has online a letter from Birman on the Yau-vs.-New-Yorker controversy.

25. **I Care**
December 18, 2006

The entire academic community needs to police itself (field by field) to ensure that the same (more or less) review process be applied to every paper submitted to a particular journal, regardless of how influential/famous/well-established its author(s) or editor(s) may be. That may sound a bit idealistic, but if one is not, one shouldn't be working in the ivory tower anyway. Academic politics is oxymoronic to me, if the area one studies is indeed objective and rigourous.

26. **Peter Orland**
December 20, 2006

I Care:

If large communities of people police themselves, they will
soon become victims of police brutality. Academic freedom
has drawbacks; nastiness, unfairness, etc. But policing academic
communities (even self-policing) will have ten times as much
of these drawbacks. If you think academic politics is bad, imagine
how it would be with acedemic aristocracy.

27. **Moeen**
December 23, 2006

Peter,

I'm not sure how you come to your conclusion. What's wrong with setting ethical standards in academic communities and making sure there are consequences for people that violate those standards regardless of their fame or influence? I don't see how this would create an "academic aristocracy", this is about ethics, not politics, and with few people outside academic communities with an interest in policing them, self-policing seems like a pretty good idea.

28. **Peter Orland**
December 24, 2006

Moeen,

There are such ethics. There are consequences. I know
pesonally of cases where famous people have had their
papers rejected because they did not meet certain standards (unfortunately, I can't tell you who who). There is peer
review. Sure, the system isn't perfect. But yes, it IS about
politics.

In free academic communities, just as in free societies,
people often get away with unethical behavior. This problem
won't be cured by imposing more control. A "self-policed" academia is inevitably going to be policed by some selves
and not by other selves.

What people who advocate more control on any
society overlook; in a more rigorously-policed society, just
as in a self-policed state, MORE people get away with WORSE unethical behavior. Rigorously-policed societies have more corruption, not less.

29. **Moeen**
December 26, 2006

Peter,

I'm not sure what you had in mind by self-policing, but all I was aiming for was merely setting higher ethical standards for the entire community. It won't help if only some parts of the community impose higher standards an others don't, there has to be a consensus.

It is because the mathematics community failed to uphold better standards for themselves that, as Joan Birman said, "the entire profession has received a very public and very bad mark." If academic communities fail to do so in the future, the loss is their own.

I don't think the analogy to police-states is a good one since the only examples of such governments I'm aware of are usually responsible for various human rights violations. Regardless, I was not trying to promote the academic equivalent of a police-state, as that would have just been silly.

30. **Maroc**
December 27, 2006

Of course, a novel about the string theory controversy (if it's not one of those postmodern novels) requires some kind of ending. Here are some possibilities.

(1) The string theorists are unexpectedly vindicated by LHC. (Many string theorists clearly are hoping for this ending, even though it seems rather unlikely.)
(2) The hero has a mental breakdown, quits theoretical physics, and writes a semi-autobiographical best-selling novel. (Also unlikely.)
(3) Finer examination of the cosmic microwave background reveals a message from God, in which He reveals the true workings of the universe. None of the competing theories come even close to the right answer. (I'm not betting on this outcome either.)

31. **Peter Orland**
December 27, 2006

Moeen,

How can the community impose (your word, not mine) higher standards without leading to abuse of authority? My point is that the current system is flawed, but it does work. As long as journal editors, referees and reviewers are human, there will be mistakes and abuses. Do you really think this can be resolved by some new system of authority?

The consequence of imposing good science is bad science, just as the consequence of imposing good art is bad art.

32. **Moeen**
January 1, 2007

Peter,

I still do not see how if the community as a whole decides to set higher standards for everyone, then that would lead to an abuse of authority. All this means is that no matter how much influence you have, you're still subject to the same rules as everyone else. Sure the system works, but that doesn't mean it can't be better. There doesn't have to be a new system of authority just an improved one.

The analogy to art doesn't work because what makes "good" art different from "bad" art can be pretty subjective, whereas in science, and certainly math, there's a very clear difference: good science is accurate, bad science isn't.

In any case, as far the events that Birman was referring to, Yau violated the normal peer review process by pushing the Cao-Zhu paper into the AJM journal by bypassing the editors, and no one called him on it. As a result, they seemed to be implying that they were okay with this kind of behavior, damaging the integrity of the profession. Seems like Yau is the one responsible for an abuse of power here, not anyone else.

33. **Peter Orland**
January 1, 2007

Moeen,

I don't think we are going to agree on this, so this is my last effort against more stringent control of science, though I am happy to hear your response.

How is this the system of authority going to be improved? I don't see how to do it reasonably. The final authority is the scientific community. As long as the main goal of people in that community is the truth, that is the best authority. Changing the power structure will make things worse, not better.

I still think my anology with art is a pretty good analogy. Consider how poor the general quality of science, as well as art has been under totalitarian regimes (they both fare somewhat better under authoritarian regimes, but not well).

It seems to me that the system worked very well on the Poincare-conjecture affair. The Cao-Zhu paper was published, as you say, but the reaction of the math community is clear. This is a simple matter of democracy in action.

34. **Jeff Holtz**
February 1, 2007

Peter,

I agree. The math community has responded quite effectively to the manner in which the Cao/Zhu article was published. But, I don't think it has responded very effectively to the downright plagiarism of Lemma 7.1.2 .

This document clearly shows that it was not just a accidental side step, but deliberate copying from Kleiner and Lott. I'd be interest to hear opinions on why there has been such a tempered response.
See this file:
http://www.cds.caltech.edu/%7Enair/pdfs/CaoZhu_plagiarism.pdf

02138

December 16, 2006
Categories: Uncategorized

There's a new magazine aimed at Harvard alums, named 02138 (after the local zip-code), and its second issue has just appeared. Personally I've never quite understood the phenomenon of people who retain a lifelong fascination with the fact that they attended Harvard, but it seems that there are a lot of them, and the magazine is partly aimed at them or at anyone with an interest in the place or its alumni. The university already has an alumni magazine that it sponsors, but 02138 appears to intend to provide something edgier and not so much along the lines of promotional material.

This latest issue contains an article about the string controversy, written by John Sedgwick and with a focus on the Harvard angle, including me, fellow Harvard grad Brian Greene, and current Harvard faculty member Lubos Motl. The piece is called Unstrung Heroes, and for the full thing I guess you'll have to subscribe to the magazine. I fear that Sedgwick has done an excellent job of accurately putting together the most outrageous statements that he could find on this topic, including some things I told him when he came down to New York a couple months ago. He also got some interesting quotes from quite a few physicists about the current state of string theory. These included Glashow, who "said he considers a big book like Woit's long overdue, because *string theory has gone exactly as we imagined*. If anything, he adds, *it's even worse than it was*." Weinberg is quoted as saying:

The critics are right. We have no single prediction of string theory that is verified by observation. Even worse, we don't know how to use string theory to make predictions. Even worse than that, we don't really know what string theory is.

Cumrun Vafa "calls string theory *the major leagues* in the field of quantum gravity. As for other theoretical pursuits, he derides them as *little efforts here and there*." Barton Zwiebach promotes string theory as possibly being able to "see the origin of the universe, and the very meaning of how space and time are born and what they are." Michael Peskin claims that we might discover a universe that existed before time as we know it began, while noting "But there is a big debate as to whether this idea makes any sense."

Sedgwick tells the story of Lubos Motl's reference to me as the "black crackpot", and Lee Smolin as the "blue crackpot" (because of the colors of the covers of our books), and his discussion of the desirability of my death. Lubos has evidently been told he's not supposed to say things like that anymore, and responded to a request for an interview with "I don't enjoy elementary human rights right now." There's a quote which I think originated as a comment on my blog to the effect that Lubos has done for the image of string theory "what the movie *Deliverance* did for canoeing holidays."

Perhaps the most outrageous quote is an accurate one from me characterizing some of my experiences criticizing string theory from a position outside the field's standard rigid hierarchy as being analogous to what happens when one messes with the dominance hierarchy of a chimpanzee troupe. This leads to a lot of strange behavior, flinging of shit, showing of behinds, and all sorts of bizarre behavior. In order to avoid offending people I wasn't referring to, I should explain that I had in mind specifically some of my experiences when first starting this blog, see in particular the comment section of this posting.

It's a bit embarassing that I'm made out to in some degree be the hero of this piece, the oppressed underdog that the author tries to set up in contrast to overlord Brian Greene. Sedgwick sees the story of how string theory dominates an academic field despite very limited achievements as quite analogous to the phenomenon he had personal experience with of how "theory" came to dominate the humanities in academia. I think there is something to the analogy, with both kinds of "theorists" starting out as an insurgent minority needing a certain amount of fanaticism to survive and expand their influence. Both groups revel in the complexity and obscurity of their work, convinced that those who disagree with them are stuck in the past or just too dumb to appreciate the great achievements of the difficult ideas involved in the two kinds of "theory".

Chris W. has pointed me to a site that brings together the two sorts of "theory". It's called Scriblerus Press, is run by Sean Miller, who has a blog and is working on a PhD thesis in English on the topic of "the cultural currency of string theory." Scriblerus is sponsoring and now looking for contributions to an anthology of short creative works that deal with string theory in one way or another.

Comments

1. **Alejandro Rivero**
   December 16, 2006

   one messes with the dominance hierarchy of a chimpanzee troupe.

   Er, you have a curious instinct to put the finger on bogus science, if you are referring to "Chimpanzee Politics", of Frans de Waal. In the revised edition, he explains (in the small letter) that the chimpanzee causing all the history was received in the Zoo as a donation of "Disney on Ice" or a similar circus show.

2. **luke**
   December 16, 2006

   Peter can you get permission to post the article on this website?

3. **Kris Krogh**
   December 16, 2006

   For my money, Weinberg is among the most honest traders in the "marketplace of ideas." His advocacy of string theory has troubled me more than anyone else's. So this turn is a relief — maybe there's a place for normal scientific standards in theoretical physics.

4. **D R Lunsford**
   December 17, 2006

   I should point out that I have been making this analogy of humanities to physics since the 80s, and that they both lurk in the dark shadow cast by postmodernism.

   -drl

5. **Anonymous**
   December 17, 2006

   Both groups revel in the complexity and obscurity of their work, convinced that those who disagree with them are stuck in the past or just too dumb to appreciate the great achievements of the difficult ideas involved in the two kinds of "theory".

   Isn't the situation broadly similar in cosmology, with Big Bang's dark matter, dark energy etc.?

6. **Big Crimson**
December 17, 2006

“Personally I’ve never quite understood the phenomenon of people who retain a lifelong fascination with the fact that they attended Harvard, but it seems that there are a lot of them, and the magazine is partly aimed at them or at anyone with an interest in the place or its alumni.”

The university teaches this habit as part of its famous core curriculum, now under revision. The mental habit of self-adulation provides an important basis for the mental habit of donating money to wealthy institutions.

7. **Troublemaker**
December 17, 2006

Isn’t the situation broadly similar in cosmology, with Big Bang’s dark matter, dark energy etc.?

Experimental cosmology is a field that…well, exists. Experimental string theory doesn’t.

8. **Michael**
December 17, 2006

Dear Peter:

I just read one of your earlier posts (post=3 actually) giving some idea of your academic background; it’s nice to know that while you have the requisite background to provide thoughtful and meaningful commentary on current theoretical physics (trends) and related areas, you don’t engage in the usual ramblings and shouting that sometimes characterize other commentators. (I always felt the mark of genuine scholarship was objectiveness, sound reasoning and clarity of thought… something not particularly paid much attention to in the current sub-culture of particle physics a la string theory.)

Well, anyhow, the reason for writing was to ask you the following:

1) You seem to view Representation theory an integral part of mathematical investigations of current particle physics. Exactly how, I wonder? [I am new to hep-th.]

2) You further view Atiyah’s works as fundamental to QFT-related studies. Can you explain how? [Only an outline would do.]

3) I am reading “The Road to Reality”; there, Roger Penrose advocates twistor theory; why, for example, do you not share the same level / degree of excitement as Roger Penrose regarding his program? I would have thought that you, among a handful of individuals who take objectiveness in research rather seriously, would want to assist Sir Roger in his investigations as much as possible.

4) [Optional] Can you send me an e-copy of Coleman’s QFT notes (if you have one)?

Thanks.
Michael

9. **CapitalistImperialistPig**
December 17, 2006

Peter,

Thanks. You just saved me $65. (I read the linked comments from Mark Srednicki.)

10. **Jonathan Vos Post**
December 17, 2006

I suspect that 02138 is “edgier” (not in the Feynman diagram sense, which would have some magazines “loopier”) to appeal to younger alumni. My father got his degree at Harvard, cum laude, and maintained a lifelong love for his alma mater, including dining almost daily at the Harvard Club in New York, buying me Harvard neckties, rooting in “The Game” (Harvard-Yale football), and snailmailing me articles cut from the harvard alumni magazine. However, my father was 82 when he died, and the mainstream of magazine publishing appears to have shifted considerably. I’d hypothesize that 02138 has “edgier” graphics (further along the spectrum towards “Wired”) and a slant towards controverial human interest stories.

Now, should Caltech change its almni magazine’s name from “Engineering and Science” to “91125”?

11. **D R Lunsford**
December 17, 2006

CIP – Peter won that argument flat-out, and with dignity rather than arrogance.

-drl

12. **CapitalistImperialistPig**
December 17, 2006

D R – absolutely.

Bending over backwards to be fair to Mark, he’s probably not the first person to go off half-cocked. The deliberate obtuseness on the SU(2) question was a bit much though.

13. **Tony Smith**
December 17, 2006

Michael asked Peter “… [Optional] Can you send me an e-copy of Coleman’s QFT notes (if you have one)? …”.

Coleman’s QFT notes were published in a 1999 Cambridge University Press book entitled
“Quantum Field Theory for Mathematicians”
by Robin Ticciati, who, as the preface states, “… had audited Sidney Coleman’s outstanding Harvard lectures and had taken very good notes. Equally fortunate, I [Robin Ticciati] had Robert Brandenberger’s official solutions to all the homework sets. … “.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

14. **woit**
December 17, 2006

Michael,

Your questions are good ones, but to answer them in detail would take more time than I have now, and get into topics far afield from the posting. I'll try and say a bit though, in reverse order…

Lots of physicists, like me, probably have a yellowing set of notes from Coleman's course around, and many people have taught courses from these. The Harvard web-site for the course (this semester taught by Lubos Motl, who seems to be following Coleman's notes) has a copy of such notes, see:

http://my.harvard.edu/icb/icb.do?course=fas-phys253a&pageid=tk.page.phys253a.dir.96baa9f5f565ad359215beb46d7685a9

About twistors: I've always been very interested in the geometry of twistors, and long ago even wrote a paper about what they might have to do with the standard model, one that I now think of as very naive. I think the fundamental problem with twistors is that, while they provide a different and more interesting set of variables to use in thinking about space-time geometry, no one really knows how to relate these to particle physics, or how to use them in a quantum field theory of space-time. This is a very deep problem, I'll bet that someday it will be an important part of future progress, but no one has been able to solve it yet.

About Atiyah. Atiyah's work on index theory and K-theory is one of the great advances of mathematics in the last half of the last century. It brings together geometry, algebra and analysis in a fascinating way, with the Dirac operator playing a crucial role. If you believe like me that the deepest ideas about mathematics are related to physics, this looks like a promising place to be searching for new ideas about physics. Atiyah himself did some things related to QFT (work on anomalies), and a lot on the geometry of gauge theories. He retired a while back and has not been as active as he was during the 70s and 80s. His student Graeme Segal has been someone thinking deeply about QFT and CFTs more recently.

I highly recommend reading Segal's notes on the subject that you can find on the web. Trying to understand the full implications of this kind of mathematics for QFT is still very much a work in progress, with many interesting things known, but we're far from a clear picture about this.

In general, Atiyah is also a master expositor, so his papers are well worth reading. But as for his actual work on QFT, it's fragmentary. One of the most impressive things he did was to come up with the idea of a topological QFT, convince himself that there had to be a TQFT for Donaldson theory, and for what are now known as Gromov-Witten invariants, and then badger Witten about this until Witten actually figured out that such theories really exist and how to define them.

Representation theory is a huge subject, and it's a great unifying idea in mathematics. Some beautiful 1+1 d QFTs can be understood in terms of the representation theory of loop groups and the group of diffeomorphisms of the circle. These groups are infinite dimensional and their representation theory was not well-understood until people started thinking about the relation to QFT. My belief is that higher dimensional QFTs may have a close relation to the representation theory of higher dimensional gauge and diffeomorphism groups, but very little is known about this subject. We seem to still be missing some crucial ideas. One idea that I've thought a lot about is that of using equivariant K-theory, a subject that was developed by Atiyah and Segal, and brings together representation theory and K-theory.

15. **Memetician**
December 17, 2006

Thanks for the post. I can't say I'm going to run out and subscribe 02138, but my interest is definitely peaked and I will check out the article next time I'm at the Coop. I got a good laugh from your blog, and have been clicking on links going deeper and deeper down the rabbit hole into the battle of string theory land. How I love the chimp throwing shit analogy. And even better is the quote, "I don't enjoy elementary human rights right now." HA! This is such great fodder . . . very bloggeriffic.

16. **woit**
December 17, 2006

CIP,

Srednicki was the one at the George Johnson KITP talk to raise his hand when Johnson said something like "no one thinks Smolin is a crackpot", generating much merriment from various chimps in attendance. From a quick look, his QFT book appears to be detailed, competent, but not in any way original or particularly insightful. Distler's behavior in that comment section, and many other later similar exchanges, is just bizarre.

One thing that has struck me is how it seems to be people like Srednicki, Motl and Distler, who are nowhere near the top of the string theory hierarchy (Srednicki is not even a string theorist), who get most frantic at the very idea that someone who they see as an outsider or lower than them on the hierarchy challenging in any way those at the top. They seem to be the ones with the most emotionally invested in the maintenance of hierarchy. I'm not sure what this means.

17. **gunpowder&noodles**
December 17, 2006

The fact that all three have given up research might also be relevant.

18. **D R Lunsford**
December 17, 2006

Pter – these people were always around. The Internets and the Googles give them a voice.

-drl

19. **D R Lunsford**
December 17, 2006

Peter – yes twistors are fascinating, but hopeless for the current model of matter with its insistence on point particles. The idea is very simple – make a line geometry for spacetime rather than space – but with that, the point particle becomes a derived object, and so there is no hope of twistors having an intrinsic meaning for the usual model of matter. A new model of matter might have a better expression in terms of line geometry. What twistors really point to is a new model of matter.

-drl

20. **Peter Orland**
December 17, 2006

Just a quick comment to Gunpowder and Noodles:

Better not to write people off as having given up research, unless you are sure. I'm not sure it hurts anyone, but it poisons the dialogue (yes, I know that other people have already pumped it full of toxin). I wouldn't seriously write "hey, that Gunpowder and Noodles sure doesn't explode pasta like in the olden days".

Anyway, I expect you get my point.

I think Mark wrote a few papers recently.

21. **woit**
December 17, 2006

I agree with Peter Orland. Enough with the less than accurate hostile comments.

22. **Michael**
December 18, 2006

Peter:

As regarding the [Optional] question, thanks! I'll look through them; just wish they had been type-written though.

Ahem… might you have his "Erice Lectures" in e-format as well? :-(=)

23. **Michael**
December 18, 2006

Sorry, wanted to make [grin] symbol.

24. **MathPhys**
December 18, 2006

"They seem to be the ones with the most emotionally invested in the maintenance of hierarchy. I'm not sure what this means."

I think it's their way of being "more royal than the king".

25. **MathPhys**
December 18, 2006

Coleman's Erice lectures are copyrighted.

26. **Michael**
December 18, 2006

MathPhys:

I just asked if he [Peter] had them in e-format?

27. **Michael**
December 18, 2006

And by the way, sometimes authors make their works available free of cost, e.g. Connes' NG or Warren Siegel's 'String Field Theory'.

28. **anonymous**
December 18, 2006

it's a pity that the Coleman lectures are too old to be freely available on arXiv; maybe you can find the following file:

Coleman S. Aspects of symmetry (CUP, 1985)(L)(T)(207s).djvu

29. **Peter Woit**
December 18, 2006

I don't have a copy of Coleman's Erice lectures in electronic format, and if I did, I wouldn't be making it publicly available, since this can't legally be done without the publisher's permission, and I see no reason they would give it. This web-site is hosted on university equipment, and I'm sure the university does not want me opening them up to possible legal problems by violating a publisher's copyright.

In any case, I think having articles all available on-line is great, but for books and monographs I hate trying to read such things online and would much rather have a real book in hand. It's also a shame that students are getting the idea that anything that is not available on-line is not worth looking at. They should get used to using whatever libraries they have access to (I realize there are some people for whom this is a problem). Especially if you can get yourself to a research physics or math library, they have an immense number of very valuable books there which you're not going to find on-line, and looking into these is time well-spent.

30. **anonymous**
December 18, 2006

bringing dozens of physics textbooks in a laptop is so useful that many physicists would buy good pdf copies of books that they already have. For the moment it cannot be done legally, but it is reasonable to expect that authors and/or publishers will realize that this pre-internet system is unconvenient for everybody.

31. **Roy Lisker**
December 18, 2006

In regards to the Harvard Magazine "02138". Residences of Cambridge recognize that Harvard snobbery extends even to its Zip Code. Proles who live at an 023139 Zip Code address are definitely looked down on.

32. **MathPhys**
December 18, 2006

" —- for books and monographs I hate trying to read such things online and would much rather have a real book in hand."

Peter, you're so —- behind the times 🙂

Here is something I recently found (which is not entirely, entirely off topic)

" —— Grothendieck is now convinced that the Devil is working to falsify the speed of light. Schneps ascribes his concerns with the speed of light to his anxiety about the methodological compromises physicists make. He talks constantly, however, about the Devil, semi-metaphorically, sitting behind good people and nudging them in the direction of compromise, of the fudge, of the move towards corruption."

33. **a.n. onymous**
December 18, 2006

when they make a book-like (tablet type laptop) PDF reader to replace books, then electronic books will take off more. Something as cheap as a book, no heavier or bulkier than a book, and whose batteries will last long enough to actually read a whole book…

34. **MathPhys**
December 18, 2006

But can you take it to bed?

35. **Tony Smith**
December 18, 2006

Roy Lisker said about "Harvard snobbery" and the Harvard Zip Code 02138:
"… Proles who live at an 023139 Zip Code address are definitely looked down on. …".

Poor MIT.
Its address at web.mit.edu is given as
"… 77 massachusetts avenue cambridge, ma 02139-4307 …".
I guess engineers are too close to the real world
to fit in with the "Warped Passages" among the "Hidden Dimensions" of Harvard Intelligentsia.
Oh, wait !!
Harvard is planning to move its SuperStringers to Allston, which is 02134 !!!
(see http://www.allston.harvard.edu/)
The indignity must be intolerable.
Maybe they can get their own magazine as a consolation prize.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

PS – However, at least MIT can hold its head up about its area code, 617.

36. **LDM**
December 18, 2006

Regarding electronic books…Sometimes when one purchases the print version from Amazon, there is an option to upgrade to an e version…which allows you to highlight, underline, and add notes…as you would a print version.

The fact that 02138 is reporting the controversy on string theory as it is leads me to believe that the mass media, thanks to NEW and TTWP, has pretty much figured out that they were originally oversold on strings…
Now…when a salesperson oversells a product, he eventually loses credibility,and eventually sales. I would expect that any granting agencies that have been funding string related research adopt a similar tightening of the purse strings.

37. **Peter Orland**
December 18, 2006

LDM,

Don't bet on it happening soon. Funding doesn't work
that way. Panels and reviewers make the final decision
as to who gets funded – and these are mercurial. People
at the top have to get heavy-handed before there is change.

It is unfortunate (for who?, we might ask) that funding
agencies have been tough on non-string particle theory
(other that hard-core number crunching or phenomology).
I have to add that I think it would be just as unfortunate
for agencies to get tough on someone just because he or
she happens to do string theory. There is some really
interesting string stuff out there, in my humble opinion,
though almost all of it fits in the AdS/CFT (NOT AdS/QCD)
category.

But I reiterate, don't hold your breath. Hell didn't freeze over
in a day, as somebody famous said.

38. **J.F. Moore**
December 19, 2006

Regarding the theory/postmodern parallels with string theory, is there a physics analog to Finnegan's Wake? Specifically, an indecipherable monograph which appeared to be (or perhaps was) written by a madman but was later adopted and heralded as a masterpiece, and around which this dominant movement coalesced? Just curious.

39. **D R Lunsford**
December 19, 2006

JF – I would say the bad times started with Bourbaki and their mouthpiece, Dieudonne. Weyl, like Klein before him, strove mightly – and unsuccessfully – to stem the tide of formalism. (See his book 'The Continuum".) I think now the works of Bourbaki are thought of highly among academic mathematizers (for reasons that are unfathomable to a poor physicist like me). Without the triumph of formalism, string theory, inflation etc. would be inconceivable.

-drl

-drl

40. **JC**
December 19, 2006

D R Lunsford,

It's amusing coming across some old K-6 math textbooks from the 1960's which covered the "New Math" program of that era. "New Math" attempted to cover things like modular arithmetic (ie. clock math), set theory, groups, relations + functions, limits, base-n arithmetic (where n is a base other than 10), etc … to kids from kindergarten to grade 6 or 7. (Nowhere was there anything like multiplication times tables). The guys who came up with "New Math" must have caught the Bourbaki "virus".

41. **anonymous**
December 19, 2006

MathPhys, your objection ("But can you take it to bed?") was an important one a few years ago. With current laptop technology, and up to copyright problems, you can even take it to the toilet.

42. **Michael**
December 19, 2006

anonymous:

Hypothetically speaking… [kinda' like O. J.'s 'hypothetical' new book soon to be not out ;-)]

maybe I kinda' did search for the file, but maybe I couldn't find any freely available copy. Just maybe where can I find a FREE copy… in a hypothetical sense?

43. **Peter Woit**
December 19, 2006

About Bourbaki,

They definitely did very much influence the "New Math" and you certainly can blame them for inflicting things like Venn diagrams and injective mappings on grade-school kids. But the problems with string theory are not at all their fault, or in general, not the fault of mathematicians at all. Just take a look at the great majority of string theory papers these days. Many of them don't involve mathematics more sophisticated than what we teach our first and second year undergrads, and the calculations are often very informal, based on very unclear definitions, the complete antithesis of the Bourbaki philosophy, much more like what Bourbaki was reacting against.

It's also not true at all that the works of Bourbaki are now thought highly of among research mathematicians. Their heyday was in the fifties and sixties; by the late seventies and eighties, this kind of formalism was not so popular. My impression is that now most mathematicians don't ever look at most of the Bourbaki volumes except every so often as a reference for some specific definition or result, and no one thinks they're a good place to try and learn something from. The volumes on Lie algebras and groups, which were mostly written by Borel, are probably the ones that people think most highly of and still use. Even the people most interested in formalism are not interested in Bourbaki, instead they care about Grothendieck's work and use categories, a point of view that Bourbaki never adopted (and this is one reason that the Bourbaki project ground to a halt).

44. **JC**
December 19, 2006

Hopefully I'm not the only one who actually liked Bourbaki's "theory of sets" book. 😉

45. **J.F. Moore**
December 19, 2006

Peter,

Thanks for illuminating the topic. I was a bit confused by drl's prior response. In addition to what you say, my impression of Bourbaki is that what they wrote wasn't wrong, or nonsensical, or 'not even wrong', in the sense that the "two theories" you refer to in this post just might be, but it just turned out not ultimately to be possible to wrap it all up, which was the initial point. I am not well read at all on 'pure' mathematics, though, so I could be totally off on that. Maybe if I had been in grammar school in the 60s …

Anyway, I was probably asking about something that just doesn't exist (a Rosetta Stone of postmodern physics), which is probably a good thing.

46. **Peter Woit**
December 19, 2006

J.F. Moore,

I don't know of any Rosetta Stone of postmodern physics, no analog of Finnegan's Wake for string theory. Pretty much ALL string theory papers were being ignored by most theorists before 1984.

Bourbaki accomplished a lot of what it initially set out to do during the 30s, which was to provide the details of a logically rigorous framework for certain kinds of mathematics that people were working on. They did this for analysis and algebra, but not for geometry (they couldn't even agree on exactly how to define a manifold, from what I remember). The problem is that mathematics evolves, and as it does, ideas about what's the right foundational material change. Grothendieck's new ideas about the foundations of algebraic geometry changed the way mathematicians look at the subject, and made some of Bourbaki not wrong, just incomplete and less interesting. I suspect a project like Bourbaki needs to be reinvented and redone from the ground up every few generations.

47. **D R Lunsford**
December 19, 2006

Peter – what I know of Bourbaki comes from a mathematician friend who was forced to endure it at Columbia and MIT. He had a satori about it and completely retaught himself math from a non-formalist point of view, so he had both an insider's and an outsider's perspective. I myself could never tolerate excessive formalism so it never entered my consciousness directly. His awakening moment came when he realized that after proving some complex existence theorems for PDEs in his thesis, he discovered in his new career as an applied mathematician that he couldn't easily do a simple surface integral 🙂 This fact struck me like a thunderbolt. Excessive formalism had led to a situation where a brilliant person could not tackle a simple problem. I do see parallels with this and the rise of unreasonable physics ideas. There is some input that must come from the intuition about what makes physical sense. This intuition can't be taught, and without it, what is one going to say? The answer is to hide behind formalist axiomatizing – the string hypothesis is really a formalist conception that is not stimulated by physical necessity or experimental evidence. The postmodernist will say – it doesn't matter what you say, only the social context in which it is said. This is very much at the bottom of what Smolin describes when he talks about the milieu of string theorists.

-drl

48. **Peter Woit**
December 19, 2006

drl,

Abstract formalisms are just one kind of tool, and they can be used well or misused. It's certainly true that some people learn formalism without understanding what it's useful for, and just get wrapped up in it. Mathematics suffered through a lot of that in the 50s-70s, and some people still suffer from this. But most really good mathematicians know when to use abstract formalism and when not, when this is an appropriate tool and when it isn't. These days, you'll often hear mathematicians refer to certain formalisms as "abstract nonsense", as in "at this point in the proof, such and such can be shown by abstract nonsense". This terminology comes from an appropriate skepticism about overvaluing abstract formalism.

It's also true that what research mathematicians are often trying to do is to come up with new tools. People spend a lot of time playing with new abstract formalisms trying to understand what you can do with them (a good example would be the n-categories you see people investigating these days). It's not that someone like John Baez doesn't know how to do much more concrete things, it's that the more concrete ideas have been around a long time, and it's pretty well understood what you can and can't do with them. If you want to find something new, you have to try out different things, including new abstract ideas.

49. **PatrickNewYork**
December 19, 2006

Can Witten do hard integrals? Could he ever do hard integrals and solve tough blood and guts PDEs by hand? How would he do on the putnam exam?

I'm not trying to put Witten down. I am just trying to get a handle on how he thinks about math and physics. Does Witten just divine the closed form solution to tough integrals and PDEs without doing all the hard algebra tricks.

I read some where that when Witten was a grad student, he solved Quantum Field Theory course problems with a few few lines of deep conceptual reasoning rather than grinding out a calculation over several pages. If this is true, maybe someone should teach all theoretical students to solve QFT course problems this way.

Same question about Graothendieck. CAn/did he do hard integrals and Putnam type problems with ease.

50. **Peter Woit**
December 19, 2006

I don't know much about Witten's skills at doing integrals, but I'd guess that he can do them as well as most theorists, while there are some people out there much more skilled at this than him. His talents definitely lie in finding elegant conceptual solutions to problems, rather than in finding brute force computational solutions or being a master of computational tricks. Unfortunately I think this is something that is hard or impossible to teach.

51. **Neville**
December 19, 2006

I have fond memories of Bourbaki's Commutative Algebra. Some don't. I seem to remember some comment of Grothendieck to the effect that EGA started where Bourbaki's Commutative Algebra left off.

An Iranian interviewer asked Alain Connes about his very brief participation in Bourbaki meetings. He said:

"Another reason to leave was that they had a life style which was unpleasant. People would leave without saying good bye, being rude was the main feature of the founders they seemed to cherish. I found it very irritating. Clearly the founders did great things.."

This is the same interview in which Connes told the famous anecdote about a physicist not thinking for himself:

"I went to Chicago in 1996, and gave a talk in the physics department. A well known physicist was there and he left the room before the talk was over. I didn't meet this physicist for two years and then, two years later, I gave the same talk in the Dirac Forum in Rutherford laboratory near Oxford. This time the same physicist was attending, looking very open and convinced and when he gave his talk later he mentioned my talk quite positively. This was quite amazing because it was the same talk and I had not forgotten his
previous reaction. So on the way back to Oxford, I was sitting next to him in the bus, and asked him openly how can it be that you attended the same talk in Chicago and you left before the end and now you really liked it. The guy was not a beginner and was in his forties, his answer was "Witten was seen reading your book in the library in Princeton"!"

Speaking of Alain Connes, he told me something to the effect that the most trivial paper he ever wrote involved string theory. Because of the string theory connection, it's also his most highly cited paper.

Any inaccuracy in the statement above is purely due to my own feeble memory. However I'm pretty sure I'm recalling the gist of his remark correctly. The phrase "most trivial" sticks in my mind.

52. **yagwara**
December 19, 2006

It has been fashionable for the past 20-30 years to bash Bourbaki – an attitude promulgated particularly by Russian mathematicians. But it is difficult for a mathematician trained today to appreciate how *confused* mathematics was at the time. Each subfield had its own language, unspoken assumptions, and rules of thumb. Simple things like the definition of a vector space, or even the definition of a function, while they certainly existed in the research literature, were not common currency.

The whole picture of mathematical objects as *sets with structure*, which we can scarcely imagine thinking without, was the creation of Bourbaki. One can argue that we need to move past it, but at the time, that foundation desperately needed to be laid.

It is worth noting that Weil, one of Bourbaki's prime architects, had a particularly historical and organic vision of mathematics, and was appalled that today's mathematicians have not grown up reading Euler, Gauss, and Riemann.

53. **Tony Smith**
December 19, 2006

Peter said "… Bourbaki['s]… heyday was in the fifties and sixties …".
yagwara said "… it is difficult for a mathematician trained today to appreciate how confused mathematics was at the time. Each subfield had its own language, unspoken assumptions, and rules of thumb. Simple things like the definition of a vector space, or even the definition of a function, while they certainly existed in the research literature, were not common currency.
The whole picture of mathematical objects as sets with structure, which we can scarcely imagine thinking without, was the creation of Bourbaki. One can argue that we need to move past it, but at the time, that foundation desperately needed to be laid. …".

Having studied math in the early 1960s, and having found Bourbaki to be a useful anchor in my studies, I agree with the comment of yagwara.
It should be noted that the exercises in Bourbaki provided some useful concrete realizations of the abstract main body of each volume.
It should also be noted that the Bourbaki Seminars published much interesting work that did not all find its way into the formal Bourbaki series of books.

Peter said, further: "… Bourbaki['s]… volumes on Lie algebras and groups, which were mostly written by Borel, are probably the ones that people think most highly of and still use …".
I also agree with that statement.
I would add that, almost in the spirit of the Lie Groups and Algebras volumes of Bourbaki, and the exercises,
a later pseudonymous group of French mathematicians, Arthur L. Besse, wrote such useful books as Einstein Manifolds (Springer-Verlag 1987).

Tony Smith
http://www.valdostamuseum.org/hamsmith/

54. **Richard**
December 19, 2006

Peter – "I suspect a project like Bourbaki needs to be reinvented and redone from the ground up every few generations."

I agree, although this becomes more difficult as mathematics becomes ever larger in scope. On the other hand, as time passes and we continue uncovering unexpected connections between different areas of mathematics, the need and desire for updated and more sophisticated formal frameworks will become great. It should be interesting.

55. **Neville**
December 19, 2006

I agree completely with yagwara's statement "… it is difficult for a mathematician trained today to appreciate how confused mathematics was at the time."

Looking at old mathematics books and papers, I sometimes felt lost in a forest of words, trying to find the actual statement of a theorem, never mind the proof.

56. **Neville**
December 20, 2006

I seem to recall reading the Deligne started reading Bourbaki when he was 14 years old. Didn't seem to do him any harm.

57. **geometer**
December 20, 2006

Does the term *abstract nonsense* really "comes from an appropriate skepticism about overvaluing abstract formalism", as Peter Woit suggested? I am not so sure. Certainly, this expression has been in use for a while, e.g I saw it in Lang's *Algebra* written in early 60s. Was anyone skeptical back then?

I never really read any Bourbaki's book, even though I own quite a few of them, but I suspect many math textbooks that I did read were greatly influenced by Bourbaki, and it sounds like a good history project to trace those influences. (Maybe this has been done?)

I personally dislike math research written in Bourbaki's style (which may be okay for "dead math"). Unfortunately, this is how many (especially French) mathematicians still write: theorem-proof-theorem-proof. This style of writing has been the greatest of Bourbaki's influences, I think. Rather I prefer the authors who motivate everything, and explain what they want to accomplish even when they cannot do it. I think, brevity is not always a virtue, and all too often people hide behind brevity when they do not want to admit they do not understand something.

58. **Alejandro Rivero**
December 20, 2006

Bourbaki's group theory is still widely used for practical purposes, at least I have seem high-calibre mathematicians to walk the library while checking against Bourbaki.

It should be mentioned that not only Bourbaki come to the "new mathematics" textbooks. Also the Groethendiek method to get a group out of a semigroup is the preferred way to define integer numbers and then rational numbers in the "new math" textbooks. Actually I liked it, and it is useful when one comes to K-theory. The only think I do not like about the new mathematics is the algebraic aproach to the proof of Pythagoras theorem.

By the way, if I recall correctly, Groethendiek's father and Weil's sister fought in the same war. Probably they never met, because Simone was sent home after some months, she was assigned to a anarchist patrol (in the aragonese border?) and she accidentally put the foot inside the boiling pot they were using to cook the food. On the other hand, both Groethendiek and Weil are famous because of their anti-war postures.

59. **Walt**
December 20, 2006

A pet peeve of mine: The Grothendieck method to get a group out of a semigroup is not by Grothendieck at all, but discovered years before. (The basic idea goes back to Dedekind, but the specific construction of group from a semigroup was pointed out by someone — I forget who — well before Grothendieck.)

60. **mclaren**
December 20, 2006

You have to wonder if the drift into vacuous untestable abstractions in both literary and string theory forms part of a larger breakdown by Western culture into an essentially medieval mindset.

We find untestable wallows in idle speculation not just in literary crticisim and string theory, but also in music (the mania for set theory, aleatoric composition, and other psuedoscientific conjurations which reduce the role of the composer to nothing and consistently contradict the available data from psychoacoustics experiments), in the arts ("conceptual" art in which, no matter how negative or contemptuous the reaction from thepublic, the artist always claims "That's the entire point of the piece") politics (phoney think tanks set up to deny global warming, teaching creationism rather than evoution, claims that "We found the WMDs in Iraq") religion (rapture frenzy), economics (rational choice theory which confidently predicts that MLK's voting rights drive of the early 1960s could not have happened), finance (the disastrous reign of Merton & Scholes at Long Term Capital Management) and almost everywhere else you look in modern Western society.

Over the past 20 years, our brightest minds seem to have drifted into self-delusion, arguing about vacuous terminology with no verifiable connection to observed reality. We've seen it just about everywhere in Western culture over the past 20 years, from politics to theoretical physics. It's a form of dementia which induces people to ignore measurable reality and disappear into word games and mathematical calisthenics with no objectively verifiable connection to anything we can observe.

New Dark Ages?

Whatever is going on, it's disturbing. When simple straightforward statements like "Science has to involve experimentally testable hypotheses" become the focus of fierce controversky by some of the most respected minds in physics, we're in big trouble.

61. **Jeremy**
December 20, 2006

Y'know, we had considered interviewing Brian Greene and Mr. Motl for our piece on Not Even Wrong, but we felt the book stood on its own (and I really didn't want to interview Lubos). I understand the need to create controversy, but it's stupid. It's already there. Why make more?

62. **D R Lunsford**
December 20, 2006

mclaren – ah men. see what you lost.

beyond postmodernism is narcissism, and for that, no answer.

-drl

63. **LDM**
December 21, 2006

"Science has to involve experimentally testable hypotheses" become the focus of fierce controversky by some of the most respected minds in physics, we're in big trouble.

There is no controversy…
Can you imagine something similar in Biology, Chemistry, Medicine, etc adopting a similar view. I cannot. Anybody who thinks an eventually testable hypothesis is not needed has stopped doing science — by definition. Period. And anybody who advocates such a view has just become a non-respected mind in physics.

64. **Aaron Bergman**
December 21, 2006

Anybody who thinks an eventually testable hypothesis is not needed has stopped doing science — by definition. Period. And anybody who advocates such a view has just become a non-respected mind in physics.

Good thing nobody thinks that, then.

65. **Alejandro Rivero**
December 21, 2006

Walt, indeed I had always suspected of Dedekind (and his methods to define numbers). But I was actually very surprised in the graduate courses in K-theory when they finally explained us about these virtual bundles, then the teacher menaces us with "The Groethendiek Construction", and next day he comes with a argument we knew from the elementary school 😀

66. **ME**
December 21, 2006

"Can you imagine something similar in Biology, Chemistry, Medicine, etc adopting a similar view. I cannot."

— — — — —

I can imagine, it's happening.

* Take for instance all the people in those fields who say clinical trials regarding the efficiency of ADD medication is not needed (or that they exist when they don't).

* the thousands of people "diagnosed" as manic-depressive (oh! sorry, "bipolar") on the basis they have a brain chemical imbalance that never gets measured. Similar things (worse) for the physiological reasons behind schizophrenia.

* I could find others

The current situation in HEP is bad, but not nearly as bad in some of the fields you mention…

At least in HEP most people agree there is a problem and (I think) are trying genuinely to address it. Other fields usually don't even see the problem.

Why do you think most scientific frauds happen in the so called "life sciences". I know, most research happens in those fields, but I "think" that even after normalizing in some meaningful way, it would still be quite higher than in Physics.

Bottom line: things are rough in Physics but they haven't even reached that point on the other fields you mention and thats not a good thing.

67. **woit**
December 21, 2006

Aaron is correct, in that few if any string theorists believe it is all right if string theory doesn't eventually produce a testable hypothesis. They're not saying that, and it isn't fair to say that they are.

But what many of them are saying is much the same thing, and almost as dangerous. The way I read Susskind and many other string theorists, they are claiming that string theory remains the most promising approach to a unified theory and deserves to continue to dominate research in this field, even though they believe it inherently leads to an exponentially large number of different kinds of physics, and there is no plausible known way to use it to legitimately predict anything. While they are not claiming it is fine for the the theory not to predict anything, they are saying that it is an acceptable situation to have a theory which now can only be used to make "anthropic" predictions (and they claim the CC "prediction" as such a success). While not having any ideas about how to ever get real predictions out of the theory, they acknowledge the importance of ultimately doing so, making dubious historical analogies like "well, it took more than 2000 years to get predictions out of the theory that there are atoms". In practice I don't see much difference between saying it is all right that your theory doesn't predict anything, and saying that you hope that some day it will, with these hopes not appearing to be much more than wishful thinking.

68. **a**
December 21, 2006

actually, the analogy with ancient greeks seems a good one. For some time it was nice to debate theories about atoms, water, fire, etc, etc, etc. At some point greeks realized that the net result of all these speculations was empty words and get bored. Real progress started 2000 years later, when it was possible to experimentally explore atoms.

Quantum gravity seems in a similar situation. String theory shows that quantum gravity does not need to imply a predictive theory of everything able of predicting something at low energy, and that does not need to imply new detectable effects (such as violations of Lorentz invariance from spacetime foam). If today we have no way of experimenting quantum gravity, people will continue trying fascinating speculations until they will get bored by the lack of real progress.

69. **John Gonsowski**
December 21, 2006

In the beginning, string theory was supposed to have a GUT and GUTs do make predictions. I still think a GUT is the best way to get predictions out of string theory. Course anything other than a minimal GUT would worry me cause the GUT could be tweeked to fit the data maybe. One can have a minimal GUT even in a SUSY model, right? One could maybe have a correct GUT and an incorrect overall string theory but the correct GUT would be quite wonderful just by itself.

70. **Peter Orland**
December 21, 2006

John,

The main purpose GUTs have served was to promote the construction of huge neutrino detectors. This was timely, but there are reasons not to take this kind of unification too seriously.

There were two theoretical "successes" of GUTs. One was the prediction that the sum of electric charges in each generation of quarks and leptons is zero. The other was that there seems to be a common unification scale for the U(1) and SU(2) of the Schwinger-Glashow-Ward-Weinberg-Salam model and the SU(3) of QCD.

The first "success" is a fake – it is necessary to insure that anomalies (quantum breaking of gauge invariance) are absent. It turns out that the sum of electric charges in each generation has to be zero anyway, to eliminate anomalies from the standard model. So GUTs buy nothing here.

The second "success" just tells you that there is probably some sort of unification at 10^{16}eV. This is a huge scale, not as lofty as the Planck scale, but still way up there. And it is by no means clear that the wherever the theories unify, the unification is a GUT.

I think even Georgi and Glashow are skeptical about GUTs these days, though I wouldn't want to put words in their mouths.

71. **James Graber**
December 22, 2006

John, Peter O., Peter W. and anyone else:
About GUTs: Long ago, people used to talk about SO(10), SU(4)xSU(4), and flipped SU(5)xU(1), after SU(5) was ruled out. Now you almost never hear about them. Does anyone care any more? Could LHC produce any results which would significantly favor or disfavor these GUTs? Or is only proton decay relevant?

By the way, there was a Harvard magazine called "Cambridge 38″ way back in 1958. This old chestnut never dies.

Best,
Jim Graber

72. **Peter Orland**
December 22, 2006

James,

The main problem is that the unification scale is very high.
I don't know if the LHC will have any bearing. There are,
however, new generations of astrophysical neutrino
detectors which might (Ice Cube for example). The details
of neutrino mixing does tell us something about what
happens at scales of the order of the unification mass.

A neutrino experimentalist or phenomenologist would know
more about all this than I do.

73. **mclaren**
December 24, 2006

Dr. Woit astutely remarked:

"While they are not claiming it is fine for the theory not to predict
anything, they are saying that it is an acceptable situation to have
a theory which now can only be used to make `anthropic' predictions
(and they claim the CC `prediction' as such a success)."

Several questions:

Q: What is the difference between a HEP theory which predicts anything
you could possibly observe, and a HEP theory which predicts nothing?

Q: What is the difference between an elegant scientific theory with
so many adulterations and encrustations and baroque modifications, like
current string theory which has now turned into M-theory, that it becomes ugly and intractable…and an outright kludge that's ugly and intractable to start with, like the Ptolemaic epicyclic system?

Q: What is the difference between saying "it took 2000 years to
experimentally verify the existence of atoms, so it could conceivably
take that long to experimentally verify string theory" and "it took
2000 years to experimentally verify the existence of atoms, so it
could conceivably take that long to experimentally verify [feng shui /
ufology / orgone energy / (ad nauseum)]"?

Q: What is the difference between a theory like the phlogiston
theory of heat, which was pursued for several hundred years
without success, and string theory if we pursue it for another
50 or 100 years without producing any experimentally falsifiable
predictions?

Q: If people like Motl claim that it's okay to continue to
pursue string theory for decades or perhaps generations
before getting testable numbers out of it, what's the time
limit? How long is long enough? 50 years? 70 years? 100
years? 200 years? Longer?

Q: How can the statements "String theory is currently the dominant
theory in HEP" and "there are no viable scientific alternatives
to current string theory in HEP" be falsified, given that
current HEP grad students find themselves forced either to work
in string theory to get tenure, or find another profession?

Bonus question: Isn't this like saying to a young Russian
economist circa 1970 "Marxist-Leninist dialectical materialist
theory is (and must be) the one correct theory of economics,
for there is at present no other viable theory of economics in
the Union of the Soviet Socialist Republics"?

On a more serious note…

The big question remains whether string theory can produce any slight but experimentally observable departures from the Standard Model at energies much lower than those required to reach the Planck scale. If so, we have a real shot at observing something that might confirm or disconfirm string theory. No possible accelerator built by humans could reach the energies required for unification — but are there subtle phenomena which would emerge at energies reachable by either the LHC or its successors, or astronomical observations, which string theory predicts, but which lie outside the Standard Model?

At present I'm not aware of any. Are there any?

Planck's quantum hypothesis implied the photoelectric effect, which was observed. De Broglie's matter wavelength implied Bragg diffraction, which was observed. Einstein's general theory of relativity implied the bending of starlight around massive objects, which was observed. Quantum chromodynamics implied the Casimir effect, which was observed.

What slight but experimentally detectable effects does string theory imply? Are there any (which are not predicted by the Standard Model, that is)? If not, can we call it a scientific theory?

74. **Walt**

December 24, 2006

Does anyone else feel sorry for the Ptolemaic epicycle system?

75. **Jonathan Vos Post**
December 25, 2006

Excerpt from Pharyngula science blog
http://scienceblogs.com/pharyngula/2006/12/blank_post.php

(medieval): Nature abhors a blank post.

(modern): Blank posts are spontaneously filled by virtual posts and antiposts.

(postmodern): String Theory (strings of alphanumerics) has failed to make any useful predictions on a Blog Theory of Everything. Or, more properly, Character String Theory predicts a "landscape" of over 10^500 possible blogs in a blogmultiverse.

See, for instance,
Not Even Wrong
http://www.math.columbia.edu/~woit/wordpress/

Posted by: Jonathan Vos Post | December 25, 2006 02:41 PM

76. **woit**
December 25, 2006

Walt,

Some commenter here pointed out that Ptolemaic epicycles are at least much better than string theory. They make loads of experimentally verifiable predictions.

77. **Jonathan Vos Post**
December 27, 2006

J.F. Moore's question, and the mention of ancient Greeks, and Ptolemaic astronomy are connected.

For instance, J.F., Aristarchus may not have been thought mad, but his heliocentric theory was ignored for a millennium, until Copernicus rediscovered it.

www-history.mcs.st-andrews.ac.uk/Mathematicians/Aristarchus.html

Democritus was not thought mad, and was seriously discussed by Epicurus (who sort of predicted chaos theory in cosmology) and Lucretius (so long ago that science was disseminated in poetry). But most people thought that atoms were only a philosophical construct, until Dalton updated them. Until Einstein's quantitative analysis of Brownian motion, many scientists STILL thought atoms a mere calculational convenience.

Odds are good that some other obscure ancient pre-Socratic Greek theorists will be rediscovered in some exciting future way. Those folks were actually in favor of experiments. That died out, for reasons unclear to me (Aristotle?) and ivory-tower theory dominated until the modern era.

The attack on String Thory is, in part, a historical analogy to the debate between pre-Socratics and later natural philosophers on the value of empirical methods.

I can't recall the author and title of a science fiction story about someone who created a hierarchical theory of genius. He systematically searched the trash heaps of insane asylums for ideas thought mad that turned out to be true, but ahead of their time. In the story, he found a few. Wasn't Grassman thought mad, and eventually accepted the claims, with his algebra filled with nilpotents. Turned out right, but ahead of its time.

78. **Jonathan Vos Post**
December 29, 2006

So the cosmos is not a dodecahedron embedded in a 5-dimensional composition of 4 elements plus Quintessence, but rather of strings of phlogiston?

And these string can be plucked in a Pythagorean mathematical harmony so as to become the electric fluid?

I have not the Humor to follow this, but wonder what the soothsayers determine from the portents on how long such a theory will be accepted.

As pointed to by Ars Mathematica, here's a wonderful quotation, and fascinating paper, on the
misapplication of scientific statistical methodology, which might just as well be applied to String Theory by its critics.

"… a potent but sterile intellectual rake who leaves
in his merry path a long train of ravished maidens but
no viable scientific offspring…."

From:
"The Earth is Round (p 'less than sign' .05)" by Jacob Cohen, 1994.
http://camargue.unibas.ch/cohen1994.pdf

79. **XPM**
January 1, 2007

> I can't recall the author and title of a science fiction story about someone who created a hierarchical theory of genius. He systematically searched the trash heaps of insane asylums for ideas thought mad that turned out to be true, but ahead of their time.

The story (or more precisely, a "review" of a fictitious story) "Odysseus of Ithica, New York." in Stanislaw Lem's *A Perfect Vacuum*.

Scott Aaronson For Sale

December 21, 2006
Categories: Uncategorized

Scott Aaronson has adopted a sensible attitude towards the controversy over string theory, announcing in a new posting entitled [Mercenary in the String Wars]() that his allegiances in this "War" are for sale to the highest bidder. I encourage all my extremely wealthy financial backers to take him up on this.

He seems to have reached this decision after enjoying an all-expenses-paid vacation in the Bay Area courtesy of the Stanford string theorists, despite having a great deal of sympathy for the criticisms being made of string theory. While there, he gave a talk for which he makes his notes available, on the topic of [Computational Complexity and the Anthropic Principle](). It's quite entertaining, although the fact that anyone is seriously debating the kind of issues Scott discusses is a good indication of how far off the rails string theory has gone.

Scott seems surprised to discover that, in private discussion, string theorists are far more reasonable than he expected from their writings, from the behavior of string theory bloggers like Lubos, and from his conversations with Greg Kuperberg, who is convinced that string theory critics are "intellectually non-serious" (I forgot to mention in my last posting that Kuperberg was someone else I had in mind when quoted in 02138). His experience agrees with my own, that in private conversation I find that most string theorists and I agree much more than one would guess. In such a context I've just about always found them more than willing to admit that the current situation of string theory is disturbing, progress has ground to nearly a halt, and that the whole landscape business is extremely problematic. That these attitudes are not well reflected in the public utterances of string theorists I think is due to several factors. Given the problems facing the theory, many find it best to just avoid being quoted publicly, and those who do talk to the press feel that their field is to some extent under unfair attack in the media and they should make their best effort to defend it. Those who spend their time vigorously defending string theory as a healthy research program, attacking its critics on blogs and elsewhere, often represent only a tail in the statistical distribution of views and behaviors of the string theory community.

I also suspect that one reason Scott found the Stanford string theorists behaving more reasonably than he would have guessed is that the last year or so has not been kind to their early hopes that statistical calculations would allow some sort of real predictions to emerge from the anthropic landscape. It has become increasingly clear that this kind of idea just can't work, for reasons that have been extensively discussed here.

I predict a lively discussion in the comment section over at Scott's blog, and encourage people to use that venue. Already John Preskill has weighed in with what he thinks is an unintentional double entendre about Susskind: "When I listen to Lenny Susskind, I really believe that information can come out of a black hole."

Update: Scott is pretty funny, but I have to admit that Lubos is completely [hilarious]().

Comments

1. **hack**
   December 21, 2006

   string theory critics are "intellectually non-serious"… this sounds alot like the language used by neoconservatives against critics of the Iraq war. Interesting parallel.

2. **M**
   December 21, 2006

   I think that

   a) judging string theorists from Lubos, and
   b) judging string theory from our anthropic vacuum

   are equally misguided approaches. One event is not enough to meaningfully test a statistical distribution.

3. **Peter Woit**
   December 21, 2006

   hack,

   Because of my desire to keep politics out of this blog I've avoided so far giving into the temptation to draw parallels between the string and Iraq fiascos, but it is hard to resist. In both cases most sensible people agree things are going badly, with those in authority resisting calls for pulling out and accusing their critics of not providing a tolerable alternative. Also, in both cases most people I know think not much is going to improve until Nov. 2008, when US presidential elections, and LHC experimental results, both promise about a 50% chance of some positive changes in the situation.

4. **stringhater**
   December 21, 2006

   It is easy to criticize a scientific endeavor, if it is speculative it is crazy, if it is not speculative, it is going nowhere, or it is not cutting edge, the speculations might or might not develop into facts, if they dont they an easy target, crazy speculations which were obviously never going to work, if they do, they are often incremental, something you can brush off as insignificant, it doesnt achieve all you had hoped, well that happens all the time, science is normally incremental, Peter Woit, you find it so easy to criticize the science of others, what about your own, has it progressed more than all those whom you so easily criticize? Oh, wait a minute, you don't seem to have any science, well that explains a few things.

5. **wazoo**
   December 21, 2006

   The anthropic principle seems to have weird effects on people. I wonder whether people really argue in those terms? And, at Stanford?

   What's going to happen when those graduates start reaching the higher rungs at Hewlett Packard? I don't know… I think those slides gave me a horrible hangover…

6. **Yatima**
   December 21, 2006

   wazoo said:

What's going to happen when those graduates start reaching the higher rungs at Hewlett Packard?

What do you _think_ will happen? The usual stuff: Winning contracts, eliminating co-workers, mooching and smooching, the odd superfluous management book; maybe some pretexting and an invitation to a board of enquiry. Business is quite unlike physics. It is even quite unlike engineering.

While "Computational Complexity and the Anthropic Principle" is certainly not a great contribution to string theory (IMHO of course), it certainly is fun reading. Though I'm not sure whether, assuming that "There is no physical means to solve NP-complete problems in polynomial time" holds in the universe (which I agree with), the sentence "There is no way to reach a solution to an NP-complete problem in polynomial time in the multiverse" is necessarily true or even meaningful. Where is Greg Egan when you need him?

…but for this evening I have the Complexity Zoo to admire.

7. **anon**
December 21, 2006

Scott Aaronson's talk mixing the everett multiverse and the anthropic principle is…breathtaking…Not-Even-Wrong "physics" of monumental proportions.

The huge complexity zoo makes it starkly clear that complexity theory is a very young field. Those guys need a classification theorem that will reduced the number of complexity classes by a factor of 10, at least.

8. **Scott Aaronson**
December 21, 2006

Those guys need a classification theorem that will reduced the number of complexity classes by a factor of 10, at least.

I've heard that complaint more often than I can remember, but I've never found it particularly well thought-out. Do chemists need a classification theorem that would reduce the number of elements — say, by collapsing nitrogen with helium? The bottom line is that there are a lot of complexity classes because there are a lot of models of computation. Furthermore, we *know* that collapse can't be the general rule of the Complexity Zoo — for example, since P!=EXP, we must have either P!=NP or NP!=PSPACE or PSPACE!=EXP (and most likely all three of them).

If you're serious about wanting a classification theorem, can you tell me anything concrete about what such a theorem should look like? This blog is no place for critics and naysayers — I want to see a positive proposal! 🙂

9. **Not Even Not Even Wrong**
December 21, 2006

Scott,
chemistry has the periodic table, which is a classification and probably just the sort of "reduction" that anon was talking about.

10. **Scott Aaronson**
December 21, 2006

Yes, but it's a *messy* classification, where some rows contain 2 elements, others 8, and others 18, and the properties of an element can be deduced from the column it's in except when they can't…

I could give you a "classification" of complexity classes in exactly the same sense. Here, I'll even start: P is to NP is to BPP, as E is to NE is to BPE, as EXP is to NEXP is to BPEXP…

11. **Chris W.**
December 21, 2006

Actually, Hack, neoconservatives have started directing the accusation of "non-seriousness" against the Bush administration. Don't ask me why it took so long. From "Neo Culpa" in Vanity Fair:

> **Kenneth Adelman**: "The most dispiriting and awful moment of the whole administration was the day that Bush gave the Presidential Medal of Freedom to [former C.I.A. director] George Tenet, General Tommy Franks, and [Coalition Provisional Authority chief] Jerry [Paul] Bremer—three of the most incompetent people who've ever served in such key spots. And they get the highest civilian honor a president can bestow on anyone! That was the day I checked out of this administration. It was then I thought, There's no seriousness here, these are not serious people. If he had been serious, the president would have realized that those three are each directly responsible for the disaster of Iraq."

12. **Arun**
December 22, 2006

Thomas Friedman, NYT pundit and pontificator, had a bunch of rules for the Middle East in his column, which I have presented here removing one sentence.

"Rule 1: What people tell you in private in the Middle East is irrelevant. All that matters is what they will defend in public in their own language. Anything said to you in English, in private, doesn't count. …. In the Mideast, officials say what they really believe in public and tell you what you want to hear in private."

I'm wondering whether Friedman Rule 1 is applicable in a wider circle than just the Middle East.

(w.r.t. "… in private conversation I find that most string theorists and I agree much more than one would guess.").

13. **Chris Oakley**
December 22, 2006

Stop! Stop! Enough politics! This is getting like Lubos's blog!

14. **Robert Musil**
December 22, 2006

Surely there can be few means of evidencing the "non-seriousness" of a scientific discussion than for its participants to decend into political analogies of which they generally have little expertise. "Not Even Wrong" is an admirable, serious discussion maintained by a remarkable man.

The political infection is a mistake. A very big mistake.

15. **Peter Woit**
December 22, 2006

OK, OK, no more politics. But at least no one has brought up global warming….

16. **A String Theorist**
December 23, 2006

Hi Peter,

In a comment above you indicate your belief in the possibility that experimental results from the LHC could lead to an improvement in the string theory situation.

Do you believe we might actually discover TeV scale strings, black holes, or extra dimensions?

It seems that if we "only" discover some kind of supersymmetry, or Higgs (even if it is not the standard Higgs but something more exotic), that will have no impact on string theory (other than sociological) because any such scenario can likely be embedded in some version of string theory.

To summarize: it's really hard for me to imagine any experimental result that would have an impact on string theory.

17. **Peter Woit**
December 23, 2006

A String Theorist,

What I meant about the LHC was not that it would have anything to say about string theory, but that it might tell us something exciting and unexpected, most likely about electroweak symmetry breaking. If this happens, many theorists would just drop string theory and work on whatever new direction the experiments are indicating. This would improve a lot the situation of particle theory, not the situation of string theory.

18. **Thomas Larsson**
December 23, 2006

To summarize: it's really hard for me to imagine any experimental result that would have an impact on string theory.

For almost 20 year, Ed Witten repeatly stated that string theory makes one prediction, supersymmetry (and one postdiction, gravity). Hence if SUSY is disproven at the LHC, there are two possibilities:

1. Witten was right and string theory is wrong. Then there seems to be no point in pursuing string theory.

2. Witten was wrong. Since Witten's enthusiasm was the main reason for being interested in string theory in the first place, there seems to be no point in pursuing string theory.

19. **Arun**
December 23, 2006

Perhaps the so-far-unsuccessful scientific effort to detect gravitational waves – which I think is at least as old as the first string revolution – can be examined for clues as to how patient one needs to be with respect to scientific discovery?

20. **no**
December 23, 2006

general relativity was proposed in 1915, made a firm postdiction for the perihelion of Mercury and a few firm predictions; the first one was quantitatively confirmed in 1919 (just after WWI, that caused some delay).

String theory predicts supesymmetry, maybe around the weak scale, maybe a bit above, maybe around the string scale (which could be anywhere below the Planck scale), maybe at any intermediate scale, maybe split, maybe super-split (on 1st April).

Arun, I think there is some difference.

21. **Arun**
December 24, 2006

no, I specifically mean LIGO!

22. **MathPhys**
December 24, 2006

Am I reading Lubos right? Is he preparing himself for leaving academia?

23. **YBM**
December 24, 2006

I'd guess that Lubos is actually searching desperately for an excuse, feeling he's likely to be fired soon.

24. **PPKR**
December 25, 2006

$2 for scott

25. **gw**
December 25, 2006

lubos's low-degree of psycosocial intelligence (esp., inability to detect sarcasm/clearly identifiable marks of non-seriousness) leads me to conclude that he has asperger's syndrome. he should be checked.

26. **Eli Rabett**

December 25, 2006

Two short comments. First, the interesting question about Motl is what next. He is not stupid, which either means he has some plan for the future that fits in with his behavior, or he is stupid (see a) or he has a cushy job lined up somewhere else (Czech Republic??) where his behavior and lack of production will not make a difference so he can indulge himself.

Scott Aaronson makes a fundamental mistake. While the periodic table may appear messy to a physicist, it is a huge reduction from the number of possible molecules, and the key organizing principle in chemistry. As a minor note, the original table, based on reactions with oxygen and hydrogen WAS rectangular, however there are a large number of other forms which may appeal to you (in roughly chronological order)

27. **Jonathan Vos Post**
December 25, 2006

"There is no way to reach a solution to an NP-complete problem in polynomial time in the multiverse" is necessarily true or even meaningful. Where is Greg Egan when you need him?"

Or Charles Stross, or Rudy Rucker, or Vernor Vinge, all of whom write great fiction about such questions.

28. **Hans**
December 25, 2006

I have Aspergers Syndrome myself and studying theoretical physics. And I can say, Lubos is a hard case of this desease.

I have run in similar problems on internet forums as he does on his blog.
When discussing about things in which feelings of other persons would be involved, everyone could see my "mathematical thinking style" unable to recover emotions of other people (at least as fast as a normal people should)

An extreme case, where this lack of empathy is seen, is this post of lubos:
http://mrigmaiden.blogspot.com/2006/08/zero-divergence-blogroll.html

Although the grandma of the poster has died, lubos attacks the physical ideas of this female student. He relies on physics having no connection to how the attacked person might feel.

From my experiences, I have learned, that it is best for a person with AS, only to comment seldom on things where feelings of toughts of others are involved. And when, then one has to put words like "I think" or "in my opinion" Or "maybe" or "as I understand" in ones sentences. This is, because the mathematical thinking style makes a person with AS believe, he/she knows the world for sure.

But this isnt. Since persons with AS have only a rather small connection to others feelings, and so, they miss important facts in their analysis.

One can live with AS almost without being seen as excentric. But only if one represses oneself on discussions that could be connected with others feelings.

If I would open a blog and discuss opinions of sociology or personal feelings on what others are doing (calling people working on LQG as crackpots is not a scientific statement), It would be my death.

But exactly this is the problem here:
It is correct, that LQG is not developed that far as string theory is (because of the lower number of researchers). So lubos is calling researchers in this field as crackpots. Because they are working on a theory that is not developed that much. His mathematical analysis leads to a conclusion and then he insults the researchers.

It is correct, that most females have not so much scientific interests than males. This statement can be based on sociological data as on brain functional analysis. Lubos sees this, and now tries to find more facts. He then looks for IQ studies of woman, and states this loudly. Without thinking, that he will insult some women. (But however, it is not intelligence, but different interests, (it can be shown that woman are, on the average, more interested in living objects), why, regrettably, so few woman study physics. Here lubos has lack of knowledge of sociological data, but he presents his poor evidence loudly as facts. With no care, of insulting anyone. Sentences, like "I think" or "Maybe it is" or "this might be an interesting result", or any sign of open-mindedness for other meanings are not present.

This is typical for AS. A person with a mathematical mind takes some data as axioms. And then, all other opinions are wrong.

And he will present his thinkings without any knowledge how others feeling.

I think for him it would be the best, simply to SHUT DOWN HIS BLOG IMMEDIATELY and concentrate himself working on some papers. He has ruined his life with his blog.

In Munich, I've heared Suesskind complaining personally about Lubos Blog! If Lubos would not have this blog, almost everyone would employ a former Harvard professor. (But of course, Lubos won't shut down his blog, because he thinks that he was always right, seeing no insults he has made. He writes that he "does not enjoy elementary human rights now", and this shows no insight, that he has done something wrong)

Lubos should furthermore only comment on sociological items, when he has thought of seriously, what others might think of his sentence.

This is something, one can expect from an intelligent man, even if he has AS or not.

And then, when he finds out, that he has no or not much ideas of, what others might think on his sentences, he should go to a therapist, who gives him some hints).
Then he should try to train this ability.

For Lubos, this is the only way to go.

A first check if he should go to a doctor is this test from Cambridge:

http://www.wired.com/wired/archive/9.12/aqtest.html

29. **Eeyk**
December 25, 2006

This kind of "diagnosis-at-a-distance" doesn't strike me as particularly serious. On the other hand autism, in any of its different forms and degrees, does not explain, for example, why should anyone write a long piece of gratuitous, deceitful publicity for Micros*ft in their own blog. Just plain stupidity of the garden variety is more than enough for that.

Demographically speaking, people afflicted with Asperger syndrome are relatively rare. Guys like Lubos, look for one and you'll find dozens.

30. **John A**
December 25, 2006

Hans, that's very insightful. I'd never thought of Lubos like that but it makes a lot of sense. Lubos does not recognize certain behaviors that he displays are exactly the same sorts of behaviors he criticizes in others.

I've had some run-ins with some climate scientists (and one or two journalists and mad bloggers) but I have never, ever wished them physical harm. The idea that Lubos could wish that of Peter Woit is beyond reprehensible, its inhuman.

I think Lubos could shut down the blog, if and only if, he could be convinced that the blog was detrimental to his work even if it is remarkably popular. If I was looking to employ Lubos, then I'd insist that he shut down the blog.

I know a professor at Harvard who told me that he is not allowed to run a blog nor respond to people on blogs. I've no idea how or why Lubos gets away with it.

There IS a crisis in physics and nobody knows how to begin to solve it. A theory of quantum gravity that makes testable predictions is as elusive today as it was in Einstein's time.

31. **Hans**
December 25, 2006

Eyek wrote
>Demographically speaking, people afflicted with Asperger syndrome are relatively rare.

No. It affects 0,05% in the general population.

Most of these people become mathematicians or physicists or engineers.
In physics they aren't rare. At least, they are as rare as people in academic positions on western universities whishing others to die because they have other opinions.

Again: This post here:
http://mrigmaiden.blogspot.com/2006/08/zero-divergence-blogroll.html
indicates behaviour that only someone without empathy can have.

For example, these points of behaviour would led to a diagnosis by a doctor (DSM-IV):

#1) Restricted repetitive & stereotyped patterns of behavior, interests and activities, as manifested by at least one of the following:
1A) encompassing preoccupation with one or more stereotyped and restricted patterns of interest that is abnormal either in intensity or focus.
(Lubos hates LQG and does String theory only. I think he is preoccupied with string theory to an abnormal intensity. Some would say he is a "String theory fanatic". That does count here.)

He fullfills one criteria of #1. so we go to step #2

2 The disturbance causes clinically significant impairments in social, occupational, or other important areas of functioning.

(When others think his behaviour is rude, or excentric, and telling him this often, and Harvard forbids him to write what he wants, this could count as a significant impairment. At least then, when Lubos becomes unemployed and does not get a job, because of his blog. It is to say here, that many professors with AS do not fulfill the point above. But this point is not present in many other diagnostic criteria. That is, we could use the criteria by Gilbert or ICD10, were this is not present, if we want him diagnosed)

3 There is no clinically significant general delay in language
(Lubos speaks fluently and theres no reason to believe he had suffered from language delay in the past)

4 There is no clinically significant delay in cognitive development other than in social interaction
(Lubos is a bright guy. Otherwise he would not be able to do string theory)

5 Qualitative impairment in social interaction, as manifested by at least two of the following

5A) lack of social or emotional reciprocity
(Aaronsons post was humour. This is seen by most healthy adults. But not by lubos, who lacks appropriate emotional reciprocity and takes Aaronsons post in fact by word)

To fully qualify for a diagnosis, he must now fulfill one of the following points:

5B) marked impairments in the use of multiple nonverbal behaviors such as and gestures to regulate social interaction
5C) failure to develop peer relationships appropriate to developmental level
5D) a lack of spontaneous seeking to share enjoyment, interest or achievements with other people

Of course, I cannot check B,C,D online! But I think he fulfills 1A, 2, 3, 4, 5A.

For a doctor, it would be important now, if he also fullfills 5b or 5c or 5d. He only needs to fullfill one point and then case closed.

Or one could look at maybe DSM-IV or ICD-10 where other diagnostic criteria are stated. Maybe there IS case closed then.

32. **MathPhys**
December 25, 2006

I must admit I was stunned to read Lubos' reaction to Aaronson's post. Something's wrong with that young man.

33. **woit**
December 25, 2006

Please, please, enough about Lubos's supposed psychological problems, this isn't on-topic or worth this much discussion, and mostly seems to me rather ill-informed anyway. I have no idea what he's alluding to about leaving academia. The most amazing thing about him has always seemed to me the fact that his string theory colleagues seem to consider him a serious scientist, the best young one they can hire, and to this day, as far as I know, have not taken any steps to try and bring his point of view on string theory into some sort of alignment with reality.

34. **woit**
December 25, 2006

Arun,

I don't really see any relation between LIGO and string theory. One has known what sorts of gravitational waves GR predicts for a long time, they have been indirectly observed in binary pulsars, and direct observation is a very tough technical problem. The current LIGO has always been known to not be sensitive enough to definitely see a signal.

35. **Hans**
December 25, 2006

Sorry, for these discussion about Lubos. It will never occur on this Blog anymore.

I made this post because people out there, who are to give him a new job, after his time at Harvard is over, can understand Lubos better.
Lubos may feel, that no one would give him a new job in academia because of his behaviour. He may feel, that he is in some sense "outside" of the community. And therefore, he writes that he looks forward to leave it.

It is important for future employers to recognize, that they deal with someone, whose papers should be read with greatest interest, but his insulting writings on other people are simply not to be taken seriously or as rude. It is important for employers, that Lubos does not intend to be rude, naive, or odd in any way.
I think, Lubos just thinks he makes (mathematically) correct statements.

One should judge him on his papers and on nothing else.

I would give him a research scientist position, were he can write papers without influence of anything social other than simple situations as lecturing, for example (here one speaks logically derived sentences to a silent audience. He will have no problems with lecturing).

It is important to have a non perturbative string theory. And this part of his research might be the reason for giving him a job at Harvard.

He should be given a position, were he can develop this further.

36. **Scott Aaronson**
December 26, 2006

Thanks, Hans — I have no idea if your analysis is correct, but it did help to put Luboš's apparent vitriol against me and others in a different light.

Scott Aaronson makes a fundamental mistake. While the periodic table may appear messy to a physicist, it is a huge reduction from the number of possible molecules, and the key organizing principle in chemistry.

In exactly the same sense, the "zoo" of complexity classes is a huge reduction in the number of possible computational problems, and the key organizing principle in theoretical computer science. The zoo and the periodic table are both relatively messy, but still much simpler than the alternatives.

37. **Graduate**
December 26, 2006

Hans,

No offense intended but maybe you should go post all this stuff on Motl's blog where he might have a think on it. Whether he has AS or not, you do seem to have some helpful insight into his behavior so maybe you will know how to get things across to him in a way that will help.

Cheers.

38. **Hans**
December 26, 2006

I think this won't help. He would simply delete my comments, without any thinking.

The problem is, that he belives he is strictly correct. He thinks simply:

String theory=most developed theory of Gravity
=>String Theory=Science
=>critics of String Theory=anti-scientists.
Anti scientists=crackpots
As a scientist one must fight against crackpots
=> one must fight strongly against any critique of String theory.

That is all. There is no space for other opinions, meanings and thinkings or even humour in his mental structure.

Maybe his colleagues tried often, to explain him, that there are allways different opinions out there. Maybe they have given up, because telling him this has no effect on him. (At least Susskind, who, in Munich, projected a big photo of a prof. Lubos Motl, Harvard, with his beamer, that was signed with the phrase "Susskind is senile", seemed to found it strange, that such statements are in a blog from a Harvard professor. I think, in Stanford, he won't get a job these days).

To say that there's a point in life, which he misses completely is, in my opinion best done by a friend (if he has such) or a colleague, whom he has some respect of. At best of course a String theorist.

And not an anonymous crackpot coming from a crackpot blog of the internet.

Also: In a one to one conversation, these problems might be not present. Since here, one has a wider context (as facial expressions), which makes it easier detect, if some words are not appropriate as in online writings.
It might be, that in a one to one conversation, he is seen by his colleagues as merely a "shy" man. So one could forgive his colleagues, when they dit not tell him something.

Also, I think although Lubos is not allowed by Harvard, to comment on "not even wrong", he even might read this postings here, just for couriosity. Here, he can read them twice, without deleting them.

Aaronson might complain to Harvard because lubos called him "the ultimate example of a complete moral breakdown of a scientist"
and "a corrupt piece of moral trash. "

I think such a complain should be made to Harvard. In it it should be emphasized, that no one can take Lubos seriously after reading his texts. Aaronson could write, that Harvard should protect Lubos from himself by shutting down his blog which continues to contain insults.

39. **Eeyk**
December 26, 2006

No. It affects 0,05% in the general population.

Five in ten thousand is what most people would call "relatively rare".

At least, they are as rare as people in academic positions on western universities whishing others to die because they have other opinions.

"Empathy-impaired" persons are not necessarily afflicted with AS. Many other conditions can lead to a lack of empathy. Even more so, non-empathic people are not necessarily mentally ill, just as non-musical people are not necessarily deaf.

I understand your viewpoint, and your opinion on this might well be correct, but I think it takes more than bad net.behavior and over-the-top blogposts to diagnose someone with a mental illness or personality disorder.

40. **Hans**
December 26, 2006

Well, I think this is rather going off topic here.
I simply wanted to give Lubos or employers of Lubos some advice. Not more. And this will definitely be the last reply on that topic.
We should discuss on physics now.

But to answer Eeyek shortly:
He wrote:
"non-empathic people are not necessarily mentally ill"

It is a spectrum. One says, that "non-empathic" people are ill, when they run into problems with environment or their lack of empathy is simply striking. That's all.

To answer the second point:
I have met people diagnosed with AS on internet forums. Unfortunately some hard cases behaved in a way, exactly with the same writing style (when it comes to social naivity, misunderstanding, (unfortunately) aggression, insults, fanaticism on topics, collecting data, repeating facts, repeating contents) as I see it from this Harvard professor. The reason for my conclusion comes from at least 70 samples with AS. The fact that I've seen exactly this behaviour only and I emphasize ONLY in harder cases with AS, is the reason for calling him unfortunately a "hard" case in the post above.

I don't want to go into detail here, even if I could do it easily, because this is a physics blog.

Maybe Lubos has no AS. But then he would be a very good imitator of a harder case of this disease. It is difficult to imitate a disability.

Just for fun: http://www.math.columbia.edu/~woit/wordpress/?p=111

41. **Hans**
December 28, 2006

Lubos now says, that he might go to Kenya
http://www.haloscan.com/comments/lumidek/6952878876388094292/?src=hsr#689412

Wonder, if they need there any sort of a stringtheorist. At least, it was Czech fighter jets which they definitely wanted in Kenya.
http://en.wikipedia.org/wiki/Corruption_in_Kenya
Would have cost them Sh 12.3 billion. Maybe they think Lubos gives a good warlord (or what so ever).

Well, as Lubos has some experience with countries whereis not much freedom of speech as was in former communist czech and as is in Kenya till today:
http://news.bbc.co.uk/2/hi/africa/4765250.stm

I don't think one really has to fear about him there.

42. **Chris Oakley**
December 29, 2006

> Lubos now says, that he might go to Kenya

Fine by me, especially if he has no access to the internet there.

43. **Hans de Vries**
December 29, 2006

> Lubos now says, that he might go to Kenya, "Wonder, if they need there any sort of a string theorist "

Do not underestimate Kenya's head of Internal Security Michuki. He knows very well that String Theory is currently our only hope for the development of gravity guns, or wormholes generators for rapid troop deployment, just to name a few.

Be sure that Security chief Michuki did read the literature and books on advanced physics directed to the general public. Without doubt he has the means and will to provide Dr. Motl with his own secret lab in the jungle to lay the basis for a grandiose vision of a great pan African empire.

For Dr.Motl, Africa will be just as good as any starting place to free the world of Climate fanatics, Islam, feminist, liberals, democrats, communist and worst of all: crackpots (String theory skeptics). There are already signs of an emerging united African front (including Mogadishu warlords) in the struggle against the latter.

(Read the text associated with the Image here)
http://motls.blogspot.com/2006/12/intellectuals-beat-allah-in-somalia.html

Regards, Hans

Holiday Links

December 26, 2006
Categories: Uncategorized

A random collection of links, on the whole not having anything to do with the holidays:

A Stanford Physics Student in Berkeley is now An American Physics Student in England, and reports from the DAMTP Christmas party, where people were supposed to be wearing "Sci-Fi" costumes, that one physicist came in a black t-shirt with the following printed on the front:

The Anthropic Landscape of String Theory

Leonard Susskind. hep-th/0302019

As far as I can tell, of string theory papers written during the last four years, this is the second most heavily cited (the first is the KKLT one that inspired it). How dare these English people act as if this is some sort of joke?

Raymond Streater's Lost Causes web-site has always been a wonderful source of anecdotes and opinions. He has a new book coming out any day now from Springer entitled Lost Causes in and Beyond Physics which I've just ordered and am looking forward to reading. Streater's web-site also includes a pretty hilarious commentary on Lubos Motl's typically absurd review of one of Streater's earlier books, the deservedly famous PCT, Spin and Statistics and All That, written with Arthur Wightman. I had never realized I was in such good company.

From Streater's web-site I also found a link to an interesting talk by Guralnick on some of the history he was involved in of work on symmetry breaking in QFT during the sixties which ultimately led to the Glashow-Weinberg-Salam model and what is now known as the Higgs mechanism. The talk tells how leading physicists discouraged work on these ideas as "junk" that wouldn't lead anywhere and would ensure that one couldn't get a job. During these years the dominant opinion was that S-matrix theory was the route to future progress, with QFT a dead-end.

Back when I was a physics graduate student I remember every so often picking up a copy of the journal Foundations of Physics and flipping through it, trying to read some of the articles. From what I remember, at the time it struck me as a semi-crackpot phenomenon, mixing a few serious attempts at thinking about foundations with large heaps of nonsense. It seemed clear to me then that serious theorists worked on very different things, trying to understand gauge theories and the Standard Model. A friend of mine who was also a graduate student back in those days recently told me that now the current mainstream literature strikes him as much like that found in the old days in journals like Foundations. I don't know what this means for physics, but Springer recently announced that Gerard 't Hooft (one of the main creators of gauge theory) is taking over as editor-in-chief of the journal. Maybe in times like ours in which there is no experimental guidance, work on foundations should get new emphasis (I think this is one of the points in Lee Smolin's recent book).

If one wants an overview of recent developments in the interaction of math and physics, one could do a lot worse than read the proposal from various mathematicians and physicists in the Netherlands entitled The Fellowship of Geometry and Quantum Theory (via Klaas Landsman's web-site).

John Baez's student Derek Wise has a well-written paper about Cartan connections, and John provides some commentary in his latest This Week's Finds in Mathematical Physics. I've always been fascinated by Cartan connections, since they provide a framework linking very general ideas about geometry with Lie groups. As John notes, they provide a joint generalization of the Riemannian and Kleinian points of view about geometry. They also seem to provide a natural mathematical framework for thinking about the relation between GR and gauge theory. Besides the references given by Wise, one should also note that Kobayshi-Nomizu, the standard reference text among mathematicians on geometry from the point of view of connections, is very much inspired by the idea of a Cartan connection. It seems likely to me that if we ever figure out how to properly understand geometrically how to unify gravity and the standard model, these ideas will be part of the story (although much else will also be required, including an understanding of the role of spinors, and of the geometry behind quantization).

Finally, for comic relief, Kris Krogh pointed me to a talk by Michael Berry from a few years ago, where he describes his experience back in 1985 at CalTech when he was working on quantum physics and zeta-functions, and met up with some of the local string theorists:

I met one of them, who asked what I was working on. When I told him, he fixed me with a pitying stare. "Yes, we have zeta functions throughout string theory. I expect the Riemann hypothesis will be proved in a few months, as a baby example of string theory."

Update: Several people have pointed out that the Susskind t-shirt or the report about it contain a typo. The correct reference is hep-th/0302219

Update: There's an interview with me posted on Scienceline, the web-site of the NYU Science, Health and Environmental Reporting Program, with the title Stringing Up String Theory.

Update: Yet another interview, this one with Lee Smolin at IEEE Spectrum on-line, called Thread-bare Theories.

Comments

1. **andy**
   December 26, 2006

   I once talked to our librarians about the peculiar inclusion of "foundations of physics" with the other physics books because it appears to be a philosophy journal. Apparently the LOC decides where it is shelved for most libraries.

2. **Peter Shor**
   December 26, 2006

   Can I put in a plea for less scorn in physics? I don't really want to defend the foundations of physics, as much of the research in this field really was, and probably still is, junk. But the dismissal of the whole field as garbage by the vast majority of mainstream physicists is, in my opinion, a large part of the reason that the profound differences between classical and quantum computation weren't discovered earlier. The field of quantum computation was thus ignored by the mainstream and founded by crackpots, outsiders (such as me), and the occasioual genius who didn't care what other people thought. This, of course, was greatly to my benefit. (I've changed my mind. More scorn! More scorn!)

   If the string theorists are wrong, the scenario will undoubtedly be repeated in the case of quantum gravity. Is anybody paying attention to the work of Alain Connes, a genius *and* an outsider?

3. **Tony Smith**

December 27, 2006

Peter Woit said "… Kobayshi-Nomizu, the standard reference text among mathematicians on geometry from the point of view of connections, is very much inspired by the idea of a Cartan connection. It seems likely to me that if we ever figure out how to properly understand geometrically how to unify gravity and the standard model, these ideas will be part of the story (although much else will also be required, including an understanding of the role of spinors, and of the geometry behind quantization). …".

In that context, might it be useful to revisit the 1980s work of Meinhard Mayer, A. Trautman, et al, on the geometry of gauge theories, which work extended the 1960s Kobayashi-Nomizu material from a gauge physics point of view ?
Examples of such work include:
Hadronic Journal 4 (1981) 108-152;
and
New Developments in Mathematical Physics, 20th Universitatswochen fur Kernphysik in Schladming in February 1981 (ed. by Mitter and Pittner), Springer-Verlag 1981, especially the articles entitled
A Brief Introduction to the Geometry of Gauge Fields;
The Geometry of Symmetry Breaking in Gauge Theories; and
Geometric Aspects of Quantized Gauge Theories.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

4. **Tony Smith**
December 27, 2006

Peter Shor asked "… Is anybody paying attention to the work of Alain Connes, a genius and an outsider? …".

Actually, Peter Woit has had at least two entries on this blog about Alain Connes:

December 2005 at http://www.math.columbia.edu/~woit/wordpress/?p=313
which has links to a Tehran interview and workshop

and

October 2006 at http://www.math.columbia.edu/~woit/wordpress/?p=482
The latter entry has links to
http://arxiv.org/abs/hep-th/0610241 (a paper by Chameseddine and Connes in which they calculate "… a Higgs mass around 170 GeV and a top mass compatible with present experimental value …", clearly (in my opinion) the type of results that superstringers would love to have but have been unable to get)
and
http://www.newton.cam.ac.uk/webseminars/pg+ws/2006/ncg/ (a Cambridge workshop on noncommutative geometry)
as well as
a link to an interview with Connes at
http://www.arte.tv/fr/connaissance-decouverte/science/Paroles_20de_20chercheur/1350636.html

Tony Smith
http://www.valdostamuseum.org/hamsmith/

5. **Paul Jackson**
December 27, 2006

Peter Shor is right; we need more tolerance in Physics. On the other hand, there may be wisdom in Henry Ford's aphorism: "History is bunk" (or did he mean Philosophy?). But the equivocal stance is a futile one. So I say — strength to Peter Woit, who is anything but equivocal.

There is also room in physics for folk like Eduard Prugovecki, a Toronto physicist-mathematician-philosopher who explored the prehistory of the String Wars (aka the Renormalisation Wars). A long and informative article of his is at:

http://individual.utoronto.ca/prugovecki/EpistemicPerspectives.html

It does not appear in the archives of this Blog, but should be more widely known.

6. **Michael**
December 27, 2006

Hi Peter,

[this is somewhat off-topic but closely related to your post]

You once blogged that Graeme Segal would conduct a seminar at Columbia during December. Has he held his seminar yet?

7. **woit**
December 27, 2006

Michael,

Segal gave a long lecture at a one day conference organized by Dennis Sullivan at the CUNY Graduate Center in December. He also gave a long series of lectures here at Columbia this past semester.

The lectures were about various topics in QFT, and he is writing a book on the subject. I've been trying to think of various tactics that might encourage him to get the book done more quickly. One of them would be to put my own notes from his lectures on-line, but given their state, I decided that that would be mean. Some of this material is already in lectures that you can find on-line written up by Segal himself.

8. **Matt**
December 27, 2006

First of all, I can name the Anthropically attired physicist, but I'm not sure if he would want to be "outed", so I'll just send him a link to the post.

Secondly, Andy said, “I once talked to our librarians about the peculiar inclusion of “foundations of physics” with the other physics books because it appears to be a philosophy journal.”

It’s a physics journal alright, just not one that only publishes papers that calculate the cross-section of somethingorother. I realize that makes it not recognisable as physics to a large section of the physics community. Compare with Stud. Hist. Phil. Mod. Phys. if you want to see what a philosophy journal looks like. In my opinion, creating artificial boundaries between “physics”, “philosophy”, “mathematics” and even “computer science” these days is part of the problem with modern physics. Foundational questions are studied mainly by philosophers, physicists and mathematicians, but they each bring a different approach to the subject and have different goals in studying it, so we’re best off treating it as an interdisciplinary subject rather than cordoning it off as “philosophy”. In any case, who cares where Found. Phys. goes in the library? Is there anyone left who wouldn’t prefer to access it on the web instead?

On the other hand, I agree that Found. Phys. has had a rather checkered history. It has gone through eras where editorial policy was particularly bad, but right now it seems to be more or less on track, so have a look at a recent issue and see what you think. I’d urge caution in judging it too harshly though. After all, even the esteemed PRL contains a large proportion of misguided papers. Even if technically correct, they often do not have the significance for the subject claimed by the authors. The main difference is that PRL covers technical work at the forefront of modern physics, so it is difficult for a casual reader to judge the quality of papers outside their field. On the other hand, Found. Phys. covers the foundations of subjects that any educated physicist ought to know something about, so it is much easier to spot the garbage.

9. **Doug Natelson**
December 27, 2006

Found. Phys. is not the only journal that has gone through, shall we say, interesting periods. Look up Nature **251**, 602 (1974) for an example.

10. **Perry**
December 27, 2006

If a paper uses the word epistemology, it is philosophy!

11. **Kris Krogh**
December 27, 2006

Michael Berry’s talk reminds me very much of Richard Feynman. The full text is [here](#).

12. **Chris Oakley**
December 28, 2006

Re: Streater/Wightman, I would be grateful if some kind reader of this blog would find it in their hearts to update the [Wiki entry for Haag’s theorem](#) with authoritative, up-to-date information. This is, after all, one of the main things tackled by this book.

Re: Eduard Prugovecki – thank-you, Peter Jackson, for drawing attention to Prugovecki, who arguments against renormalization are more learned and eloquent than my own. I was sorry to see that he died relatively young.

13. **Chris Oakley**
December 28, 2006

Sorry: Paul, not Peter Jackson.

14. **D R Lunsford**
December 28, 2006

This was very nice to see in print:

http://www.mth.kcl.ac.uk/~streater/EPR.html

The whole Streater page is just pure fun!

-drl

15. **D R Lunsford**
December 28, 2006

Peter Shor – I don’t see Streater as being scornful at all, just wickedly sarcastic, almost like H. L. Mencken. You get the strong feeling he has looked deeply into many of these lost causes himself and knows why they fail, and like any good explorer, is pointing out the potholes and cliffs to avoid.

His comments about quantum cosmology, wave fn of the universe, many worlds etc are however genuinely and rightfully scornful. Perhaps there is not enough scorn in physics!

-drl

16. **Mauricio**
December 28, 2006

the correct hep-th number of Susskind’s article is hep-th/0302219

17. **LDM**
December 28, 2006

Kobayshi-Nomizu is a standard reference and needs to be more widely known among physics students, but look as you may, you will not see semi-Riemannian manifolds discussed. Also it is somewhat weak in that this standard reference on geometry contains not one picture in either of its 2 volumes. It reminds one of the criticism of Lamb’s Hydrodynamics — you can read it and still not know that water is wet.
Volume 2 of Spivak is vastly superior for a physicsist in that it gives a good analysis and motivation of the different approaches to connections, which is more what a physicist needs…in other words, if you really want to understand the history and motivation of the subject, read Spivak.

18. **woit**
December 29, 2006

LDM,

I agree that Spivak is much more readable than Kobayashi-Nomizu. It's much longer and does things in much more detail and at leisure. However, one thing I noticed when teaching a course on this and using both as a reference is that there are some pretty sizable chunks of Spivak where the proofs are more or less directly copied out of Kobayashi and Nomizu.

On the whole, Kobayashi-Nomizu is not a good book to learn the subject from. But once you know what is going on, it is an excellent reference, with beautifully concise arguments.

19. **D R Lunsford**
December 29, 2006

peter – a friend pointed that out about the Feynman lectures – useless didactically, but invaluable once you knew what was happening. I never reconciled myself to this idea, having learned math from Klein and physics from Sommerfeld. There is no reason to be obscure when writing a text.

-drl

20. **Matt**
December 29, 2006

Perry said, "If a paper uses the word epistemology, it is philosophy!"

I know this is meant as a joke and I can assure you that the humor is not lost on me. Similarly, you can tell you are in a philosophy talk if the speaker ever talks about "cashing out" an idea.

However, you can find an increasing number of papers that use the word epistemology, or at least its cousin epistemic, in good old Phys. Rev., since they are beginning to take foundations seriously again in the wake of quantum info and quantum computation. That either means that Phys. Rev. is now publishing philosophy, or that it is genuinely possible to write a paper that bridges the gap. I'll leave you to decide which is the case.

21. **Louise**
December 29, 2006

If you can't see the stars from Manhattan, then check out ASTRONOMY magazine: "WHAT IF STRING THEORY US WRONG? If it is, the dark matter, dark energy and cosmic inflation are in big trouble."

22. **Peter Woit**
December 29, 2006

I'll take a look at the latest Astronomy, but dark matter, dark energy, and inflation really have nothing to do one way or another with string theory.

23. **anon**
December 29, 2006

Not sure where to post this, but you and your book were mentioned in a year-in-review episode of Talk of the the Nation's Science Friday. One of the panelists admitted to having written one of the most negative reviews of your book and spewed some ignorant nonsense. Thought you might enjoy listening to it and getting annoyed.

24. **Peter Woit**
December 29, 2006

anon,

Thanks, but I think I'll pass on the opportunity to hear more from K.C. Cole. Her review was by far the most amazingly bizarre one of the many I've seen. I made the mistake of wasting my time complaining to her that she had misrepresented what I wrote in the book about neutrinos. In response she wrote back to explain that she, unlike me, was an expert on neutrino physics and had lectured to physicists on the subject. Yesterday I was talking with someone who has worked her, but will resist the temptation to repeat what he had to say about her.

25. **Sebastian Thaler**
December 29, 2006

The webcast of the Science Friday segment to which anon refers above can be found at http://www.npr.org/templates/story/story.php?storyId=6696466 and the discussion of Woit's and Smolin's books begins at 30:40.

26. **LDM**
December 30, 2006

K.C. Cole is not a scientist — and certainly could not do a real neutrino calculation if her life depended upon it. NPR/sciencefriday should have its funds cut for exposing our children to her incompetence.

27. **Michael**
December 30, 2006

Peter,

[again, partially off-topic but relevant nonetheless]

Do you know when, approximately, Segal will post his notes on the arXiv and/or when his (presumed) book will come out?

Michael

28. **Peter Woit**
December 30, 2006

Michael,

I didn't get the impression Segal was close to having a book done, and I don't think he intends to write up notes from these lectures, so unfortunately I think it will be a while before this material appears in a finished, public form.

29. **Alejandro Rivero**
December 30, 2006

"Is anybody paying attention to the work of Alain Connes, a genius and an outsider?"

I am 🙂 and it is a very difficult work to follow, in the side related to gravity. He is up to conjuring all his previous experience on KMS states plus the new work with Marcolli on Qlattices, in order to get a vision of gravity "as symmetry breaking".

Is anybody paying attention? Yes. Will anybody dedicate the effort needed to follow him? I doubt it. At least in the gravity related issues. I am a bit more optimistic in the question of producing examples and or classification of spectral triples. And then there is the part about the standard model. Barrett at Nottingham was putting attention recently, and it receives sporadic attention from here and there, more or less at the level of any other GUT model.

30. **TomH**
December 30, 2006

K.C. Cole was a so-called "science reporter" for the Los Angeles Times a few years ago. I found nearly everything she wrote to be exceedingly annoying, both in style and content.

31. **fh**
December 30, 2006

From what I've seen people are very aware of Connes work, but nobody really knows what to do with it.

I know that there are a couple of guys in Denmark trying to apply Loopy techniquees to Connes style geometry, but it's tough from a technical point of view AND doesn't really seem to conect to the conceptual physical questions that seem appropriate….

Ah yes, the reference is:
hep-th/0601127

32. **Shantanu**
December 30, 2006

Peter and others, happy holidays. Have a look at Steinn's blog
http://scienceblogs.com/catdynamics/2006/12/dissed_by_the_best.php
on how he shifted from string theory to astrophysics

33. **a**
December 31, 2006

it would be interesting to have a presentation of Connes works in a style more accessible to physicists, focussed about their physical motivation and predictive power (does it restrict the QFT particle content and couplings?), rather than about applications to quantum gravity.

34. **Peter Orland**
December 31, 2006

I don't mean to be too discouraging, but I don't see what is
so exciting about Connes' approach to the standard model.
He takes a complicated, successful theory and makes
it even more complicated, just to facilitate his ideas. It
all seems badly motivated.

35. **D R Lunsford**
December 31, 2006

Peter – you actually should listen to Cole. I have never in my life heard such indescribable bullshit. How does this non-entity, who without doubt cannot even understand Aristotle, not to say Kepler or Galileo, much less Newton or Einstein or beyond, presume to tell an interested listener what physics is about?

It is extraordinary. Imagine if such a person gave medical advice, or piloting advice, or firefighting advice. "Well it's only physics." Wrong – physics is important. Most of modern life came from it.

I am stunned.

-drl

36. **Jonathan Vos Post**
January 1, 2007

Michael Berry's 1985 CalTech anecdote is extremely funny. But one shouldn't overgeneralize. Caltech has, for a century or so, been a place where there are always SOME Mathematicians who listen to what Physicists are saying, and vice versa. John Schwarz is not a universal template, and is a gracious host at swimming pool parties I've attended.

The intersection once included, to pick a few luminaries almost at random, Harry Bateman, Dr. Robert A. Millikan, Dr. Theodore von Karman, Linus Pauling, Feynman, Gell-Mann, Witten, Stephen Wolfram, Hawking, and at the moment includes Barry Simon and Kip Thorne. The fact that it produced people second rate in both, such as myself, should not be held against it.

37. **Peter Shor**
January 3, 2007

Peter Orland:
Connes actually has a prediction for the Higgs mass in his latest paper (hep-th/0610241). True, it seems to assume the probably incorrect "big desert hypothesis" that there's no new physics between the Higgs scale and the Planck scale, but I believe this is still more than any of the alternative approaches can achieve. So it seems to me

(who knows nothing about it) that Connes' approach must be introducing some new constraints somehow. And since I know Connes is really smart, my opinion is that it's worth paying attention to.

On the other hand, it looks very hard and I'm not planning to try to figure it out myself, so I probably shouldn't be criticizing other people for not trying to figure it out themselves, either.

38. **Peter Orland**
January 3, 2007

Peter Shor,

Please understand that though I admire Connes,
I don't find the prediction of one number especially
impressive. Fits are not physics.

One needs a new idea (or a dramatice experimental
result) which is fundamentally simple. This is different
from being mathematically or calculationally simple.
By fundamentally simple, I mean a theory with few
hypothesis leading to a strong consistent framework
(like Maxwell's equations or General Relativity).
Connes idea, to my way of thinking, is not simple
in this sense. No principle points to his non-commutative
scheme the way the principle of equivalence pointed
to Riemannian geometry into gravity.

39. **Peter Orland**
January 3, 2007

P.S. (by which I mean Peter Shor)

I think it is better to find an idea interesting and
not pursue it (as you are doing), than dismiss an
idea because you don't want to learn about it.
I hope that that is not what I am doing.

Many people in high-energy theory arrogantly
dismiss an idea as nonsense because they don't
want to make an effort to understand it. I only
add this remark because I do not want my writings
above to reinforce this contemptuous behavior.

40. **Peter Shor**
January 3, 2007

Peter Orland,

I didn't mean to imply that you are one of the people who arrogantly dismiss ideas because they don't understand them.

I have assumed that worrying about how to quantize space-time led to non-commutative geometry. Since I don't understand any of this, I could be completely wrong.

Peter Shor

41. **Peter Orland**
January 3, 2007

Peter,

I don't think that quantization of space-time definitely
implies that geometry is non-commutative. Non-commutativity
is just one proposal.

All we are sure of is that there is some cut-off scale less
than or equal to the Planck scale 10^{-33}cm, at which
GR breaks down. This may have no implication for the
standard model (I can't imagine how it wouldhave such an
implication).

Unless there is some fundamentally simple idea that
points to what should happen (by "fundamentally
simple", I don't mean easy to understand. See my above
remarks) there are lots of ways to cut off gravity.

Some schemes for cutting off gravity are more
philosophically attractive than others (depending
on your philosophy) but there is no compelling principle
yet which tells us how to chose. Some people would
chose string theory because of renormalizability or
finiteness, but it isn't clear to me that it's the only way.

An interesting problem would be to put in just a silly
momentum cut-off into gravity at the Planck scale and
ask if there are any observable consequences. For example,

there would be Planck-sized unitarity violation. That could conceivably imply some nonconservation of probability that one might be able to test. Maybe Kuzmin has thought about this, since he and his collaborators had suggested that violations of certain precepts at the Planck scale could have macroscopic consequences.

One aproach would be to ask what sort of cut-off scheme would naturally explain flatness. By this I mean both approximate cosmological flatness and the fact that space-time fluctuations don't seem to grow to macroscopic size (this is a problem in path integrals of random manifolds). I had a crazy proposal to solve this problem, which I called the "critical solid", but some of the assumptions of the proposal need to be changed slightly. I eventally would like to look at this problem again, but I am too busy with non-perturbative aspects of gauge theories.

42. **Peter Orland**
January 3, 2007

I just looked up V. A. Kuzmin's papers on SPIRES. It seems he has looked at violation of CPT at the Planck scale, but not of unitarity.

43. **Alejandro Rivero**
January 4, 2007

The requeriment of a "new idea" is sort of misleading. Gauge theories were stressed by Pauli, the nonabelian version from Yang-Mills can not score as a "radical new" idea, and the point of using SU(2) times U(1) would not score even as "new".
It is more about ideas evolving in paralell to experimental input, and in this sense Connes´s model is performing well: it transmuted ten years ago to go from electroweak to include colour, and now in transmutes again to include neutrinos. Still, it is not definitive, because it is not really exploiting the new mathematical setup (for instance, the fact of SU(3) being separated from the electroweak part in the bivector field formulation, as it appears in the red book, would add some insight about the quarks and it doesn't – yet).

44. **Peter Orland**
January 4, 2007

Alejandro,

I didn't say that I am skeptical of Connes's idea because it is not new. I am skeptical because it makes the standard model more complicated and solves no problems. In this sense I don't think the idea is a fundamental advance.

What Are You Optimistic About?

January 1, 2007
Categories: Uncategorized

Every year John Brockman's Edge Foundation asks a large number of people in science and technology to write a short piece answering a chosen question, and this year the question is [What Are You Optimistic About?]

Among particle physicists, the overwhelming thing to be optimistic about is the LHC. For instance, [Lawrence Krauss] writes:

I am optimistic that after almost 30 years of sensory deprivation in the field of particle physics, during which much hallucination (eg. string theory) has occurred by theorists, within 3 years, following the commissioning next year of the Large Hadron Collider in Geneva, we will finally obtain empirical data that will drive forward our understanding of the fundamental structure of nature, its forces, and of space and time.

Others who also mention the LHC include [Lisa Randall], [Charles Seife], [Lee Smolin], [Adam Bly], [Maria Spiropulu], [Karl Sabbagh], [Frank Wilczek], [Paul Steinhardt] and [Corey Powell].

Wilczek describes himself as optimistic that "physics *will not* achieve a theory of everything", taking the point of view that he hopes nature will continue to surprise us. He also denigrates the search for a fundamental theory of everything by noting what it has led to in the case of the string theory landscape:

At this point the contrast between the grandeur of the words "Theory of Everything" and the meager information delivered becomes grotesque.

[Alexander Vilenkin] on the other hand is optimistic about the multiverse and the anthropic landscape, saying it is implied by string theory, "our best candidate for the fundamental theory of nature", and that he thinks that statistical predictions will be possible.

The person I agree with most is [Gino Segre] who writes:

So why am I optimistic? Because I believe that controversy, with clearly drawn out opposing positions, galvanizes both sides to refine their opinions, creates excitement in the field for the participants, stimulates new ideas, attracts new thinkers to the fray and finally because it provides the public at large with an entrée into the world of science at the highest level, exhibiting for them heated arguments between great minds differing on questions vital to them. What could be more exciting?

That sort of optimistic point of view on the whole string theory controversy is one that I hope more theoretical physicists will take, with string theorists acknowledging that there are serious questions that have been raised and that are worth debating.

Personally I'm a lot more optimistic now than I was a year ago that a more realistic view of string theory has started to take hold in many quarters, and that perhaps particle theory will move towards a healthier state. Like the Edge contributors, I see the fact that the LHC is now not far off as a cause for optimism. Perhaps it will produce the sort of surprising new insight into electroweak symmetry breaking needed to show the way forward. Even if it doesn't do this, the likely failure to see superpartners or extra dimensions may encourage theorists to give up on ideas that don't work and try and strike out in other directions.

Comments

1. **mclaren**
   January 1, 2007

   With minds like this on the job, there's every reason to be optimistic about string theory:
   http://www.theonion.com/content/node/38718

2. **Renormalized**
   January 1, 2007

   That story from the onion has been floating around the internet for 5 years and was totally fabricated by a student.

3. **hack**
   January 1, 2007

   A fabricated article in the Onion? Say it isn't so!

4. **Tony Smith**
   January 2, 2007

   In his 2007 Edge answer to "What Are You Optimistic About?", Steve Grand said:

   "… The Strong Possibility That We've Got Everything Horribly Wrong

   The thing I'm most optimistic about is the strong possibility that we've got everything horribly wrong. All of it. Badly. …

   Sometimes we manage to convince ourselves that we have a handle on what is going on, when in fact we're just turning a blind eye to a mass of contradictory information. We discard it or ignore it (or can't get funded to look at it) because we don't understand it. It seems to make no sense, and it can take us a while before we realize that the problem doesn't lie with the facts but with our assumptions. Paradigm shifts are wonderful things. Suddenly the mists clear, the sun comes out and we exclaim a collective "aha!" as everything begins to make sense. What makes me so optimistic about science right now is that there are plenty of these "aha" moments waiting in the wings, ready to burst energetically onto the stage. We've got so much completely wrong … and I think a lot of our standing ideas and assumptions about the world are about to turn inside-out, just as our much older, religious ideas did during the Enlightenment.

   My guesses for prime candidates would include quantum theory and our understanding of matter, but those aren't my field …

   My field is artificial intelligence, but I'm sad to say that this subject started on the wrong page of the map many years ago and most of us haven't woken up to it yet. We keep our eyes firmly on the route and try not to look to left or right for fear of what we might see. … the digital computer has dominated the AI paradigm, through failure after dismal failure. …".

   I suggest that Steve Grand's selection of "quantum theory" as a "prime candidate" may be accurate,
   and

that the last paragraph in the quote from him would be just as accurate if
"artificial intelligence" were changed to "quantum theory"
and
"the digital computer" were changed to "superstring theory"
and
"the AI paradigm" were changed to "the high-energy theoretical physics paradigm".

It would be interesting to compare the sociology of theoretical physics and of AI studies over the past couple of decades or so.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

5. **normalized**
January 2, 2007

There is no cause for optimism. People shouldn't go into science if they're prejudiced, because nature doesn't conform to most prejudices. Ideally to the mainstream of the 19th century, the geological record should have turned out to support Biblical Creationism. It didn't. Tragic for all those mainstream guys.

String theory is insulated too well to be disproved directly, but if the predictions of an alternative theory were confirmed, then that would make string theory look a waste of time. Even though string theory can't be disproved, it would be discredited.

Therefore, it is guaranteed that all alternatives that might make contact with reality will be attacked most strongly as crackpot by the mainstream.

Weaker alternatives, which look as pathetic as string theory regarding experimental falsification, will be looked at slightly more tolerantly by the mainstream.

Similarly, lame attacks on string theory will be tolerated but convincing attacks will be answered with strong abuse. There is no way anything can improve. What if people don't like nature? Suppose string theory turns out wrong. What will stringers do? Call nature 'crackpot' and storm off in a huff? Obviously, they want to postpone that evil day indefinitely.

6. **Michael**
January 2, 2007

Question:

With the LHC about to begin its first run, and supposing that both the Higgs boson and superpartners are discovered, respectively, what then would theoretical physics look like for the next few years?

Answer, anyone?

7. **Ari Heikkinen**
January 2, 2007

Or, it could turn out that LHC does nothing but make the widely adobted we-need-a-bigger-accelerator syndrome even stronger.

My question about LHC is that has there ever been any realistic estimate of energies required to arrive at anything even remotely useful (I mean, worth the cost, considering the huge amounts of money these things cost to build) ?

To me it somewhat appears that everyone's just guessing and no one even knows what's it supposed to reveal.

8. **QM**
January 2, 2007

Ari, that's why research is more interesting than burocracy.

9. **nontrad**
January 2, 2007

Back in the day, when I was doing physics (around '97-ish) and listened to various pros and cons of matrix / M / this and that theory bandied about in various hallways, the killjoy phrase I heard most often was 'If LHC sees no SUSY then that'll be it for ST '. Almost invariably, once that card made it to the table even the most zealous proponents tended to stare down at their shoes while the conversation turned to other matters.

Most important for my young ears: This magic phrase didn't come from minor weights; some of the speakers had seen more then one 'revolution' in their time.

So….as far as I am concerned….based on my very limited experience….it's ALL about LHC. ALL about LHC.

10. **Seth**
January 2, 2007

To Notrad: what I've heard, most recently, is that the LHC can't prove String Theory and can't kill it either. You can always move the Supersymmetry up to a higher energy scale.

And speaking of killjoys, this isn't the year to be optimistic about LHC. We'll be months into 2008 before real data and analysis of energy frontier physics start to arrive.

11. **Hans**
January 2, 2007

In the 10 dimensional theory, ST is certainly supersymmetric. However: LHC won't see this length scale. The compactified theory that it faces to must not be supersymmetric.

But when SUSY is not seen at LHC, there would exist no clue at all, that ST is the correct way. "When we don't see SUSY at LHC, then we need good advice" said Dieter Luest.

So, LHC can't kill ST but in can reduce remaining hopes that one can describe physics with it, which can be observed.

12. **Yatima**
January 2, 2007

Tony Smith says:

It would be interesting to compare the sociology of theoretical physics and of AI studies over the past couple of decades or so.

This has probably been done somewhere. Sociology should be similar in a general sense. There are the 'Notable People': Minsky comes to mind. Apparently his paper on the limits of "perceptrons" was a major fator in funding cuts for artificial neural networks. Everyone piled into symbolic computation after which some new results and disillusionment with the symbolic approach led to a revival of the ANN approach etc. There are the 'SSC' programs like 'Ffth Generation' and 'Strategic Computing Initiative'. These yield little empirical data but generally lead to nowhere and/or mutate into something else due to changing goals and the realization that one has aimed too high.

Thing is, the problems are of fundamentally different nature. While ThPhys pulls into the direction of an overarching and hopefully elegant/compact description, "Intelligent System Studies" (to avoid the AI moniker, which has become overly stale lately) has given up the delusion of finding 'The Intelligence Algorithm' a long time ago. Said delusion has been called 'Physics Envy' – by whom I don't remember. Some still ride against qualia strawmen or sundry shadows in Searl's Room or they go off orthogonally into QM or whatnot (no disrespect to Penrose intended) but this gets tiresome quickly to anyone who remembers his brain is such a polynomial kludge that the may be unable to find his own socks in the morning.

I'm always ready to be surprised by fundamental shifts but at this point I would be surprised to be surprised if I may say so.

A paper about the "messiness" by Cherniak. Still good:

"Undebuggability and Cognitive Science," Communications of the ACM, 31 (1988) 402-412

My excuses for this digression into non-physics.

Happy new Year to Peter Woit … and the rest of the crew.

13. **nontrad**
January 2, 2007

Seth,

Thanks for the thoughts; see you after LHC 2007.

Best regards,

nontrad

14. **Chris W.**
January 3, 2007

As I recall, some time ago on Cosmic Variance JoAnne Hewett said that if the LHC doesn't turn up something pretty interesting within a few years then experimental particle physics in the U.S. is in for some very dark days. Actually, she may have said this in a comment. Perhaps she can offer a reprise (or a correction) here…

15. **from EU**
January 3, 2007

some time ago there was a post at CosmicVariance about this topic, and I was very surprised in hearing that apparently everybody thinks that the past and present situation in the US is fine, that SLD had been as successful as LEP, etc. Elsewhere one hears different opinions. Let me present two facts, that can be checked

1) A few years ago, many experimentalists in Europe didn't like that a large number of US experimentalists joined LHC without giving a proportionally large contribution to its construction. One of the arguments in favor of this decision was the danger of a collapse of the high-energy experimental community in US before LHC.

2) Tevatron, "the biggest accelerator in world", actually has a circumference smaller than HERA, than LEP/LHC, than the old SppS.

16. **Michael**
January 3, 2007

Don't worry if particle physics bites the dust within the next few years… there's Financial Engineering to go into for all those out-of-work young postdocs.

17. **island**
January 4, 2007

> Actually, she may have said this in a comment.

In *this* forum:

http://www.math.columbia.edu/~woit/wordpress/?p=445#comment-14668
If the worst case scenario plays out, and the LHC discovers nothing, then that is the end of particle physics as we know it. And that includes string theory. They may think they are immune, but they are not – they will fall due to lack of funding with the rest of us.

I can't wait, because I'm fairly certain that you'll be able to hear John Horgan's shouts of vindication from just about anywhere on Earth.

18. **Thomas Larsson**
January 4, 2007

If the aether wind does not turn up in the Michelson-Morley experiment, it just means that something like split aether theory is right.

19. **Chris W.**
January 4, 2007

Island,

Yes! I'm sure that's the one. Thanks for tracking it down.

20. **Steven H. Cullinane**
January 4, 2007

For a rather different "entrée into the world of science," see page 56 of this week's New Yorker (issue dated Jan. 8, 2007).

21. **Eli Rabett**
January 4, 2007

Steve I just put the NYorker cartoon up Great minds, etc…

22. **MDL**
January 4, 2007

I'm optimistic that someday one of these searches:

site:http://www.math.columbia.edu/~woit/ "minimum description length"
site:cosmicvariance.com "minimum description length"

or one of the first ten links (I gave up after that) returned from this search:

"string theory" "minimum description length"

will find a discussion of the relevance of the minimum description length (MDL) principle to the issue of the scientific status of string theory. MDL is, after all, a common criterion for selecting among theories in machine learning, when all of the theories can fit all of the data.

That discussion would likely start with the argument that — provided both theories can be made to fit all the data! — the criterion for preferring one physical over the other should be lesser size, as expressed in some reasonably primitive and unbiased representation in which all expressions can be reduced to bit strings. (See also Kolmogorov complexity.)

Someone then would doubtless speak up and claim that MDL sidesteps objections that the use of anthropic arguments destroys scientific criteria, then someone would object that having even one real number could make complexity unbounded, and someone would respond that this suggests that comparing the number of free real-number parameters in each theory is a better basis for preference than is MDL in the conventional sense. Then I would expect to see arguments about the status of real numbers, particularly the unbounded-complexity sort, no doubt leading someone to assert that all parameters are actually rationals, citing Pythagoras.

It will be a lot of fun to see all that, and to see where it leads. Maybe some day.

23. **J.F. Moore**
January 4, 2007

I can't wait, because I'm fairly certain that you'll be able to hear John Horgan's shouts of vindication from just about anywhere on Earth.

I think he is more reserved than that. For one, unless LHC science is a total bust, ILC will certainly follow, so that is another decade of pining enabled, with a couple of years for someone to win (or not) a Nobel Prize in time to resolve the Horgan-Kaku long bet:

http://www.longbets.org/12

24. **wab**
January 5, 2007

Let's say that all LHC finds is a very broad HIggs at well over 500 GeV. I would not bet on an ILC in that case.

25. **Ari Heikkinen**
January 5, 2007

Interesting comments from Kaku on that "longbets" link. Lot's of "if"'s and "might"'s tho.

26. **plank**
January 5, 2007

"Personally, I feel no need to prove the theory experimentally, since I believe it can be proven using pure mathematics." (Kaku)

Interesting indeed!

How does one prove a theory about reality using pure mathematics?

You may prove that it reproduces all phenomena you've observed by now at best. But for that there exist good theories already. So what is really needed is predicting something unknown and getting EXPERIMENTAL verification of that.

I for one would like to know how to prove, say Newton's laws of motion using only pure mathematics.

27. **Thomas Larsson**
January 6, 2007

You may prove that it reproduces all phenomena you've observed by now at best. But for that there exist good theories already.

There are many interesting numbers not explained by current theories, e.g. the spectrum of particle masses.

28. **plank**
January 6, 2007

"There are many interesting numbers not explained by current theories, e.g. the spectrum of particle masses."

Right, I wasn’t precise enough. I should have written that many theories explain a lot of the data we have. Those theories are “incompatible” but they still explain a lot.

So yes, retrodiction of unknown relations already experimentally verified, say mass ratios, mixing matrices constraints or something like that, would also be a breakthrough.

My point is, even if your objective is explaining all that has been observed, you can’t do it with pure mathematics alone. You need experimental data in order to check that theory. And if you predict anything new, to be “sure” you need to verify that too.

When a physicist writes “Personally, I feel no need to prove the theory experimentally, since I believe it can be proven using pure mathematics.” (and this seems like not a very uncommon idea among his peers) I think there is genuine reason to be concerned.

I understand that theoretical physicists need to be hardheaded.
Imagine if Galileo didn’t ignore air friction and all that! To this day we would have only Aristotelian Physics. A feather doesn’t fall like a stone. Everyone can see this on their daily experience. But, Galileo made an hypothesis (model) and verified that model experimentally with controlled conditions he though reproduced the assumptions his model made.

You can’t expect to say that the distance of falling bodies varies with time like the sum of the first n odd integers (he did say this, because $\sum_{k=1}^n (2k-1) = n^2$) and expect that reality conforms to your intuitions because the fact that summing the first odd integers you get a perfect square is so beautiful.

FY 2007 Funding Issues

January 5, 2007
Categories: Uncategorized

As mentioned here earlier, the last Congress decided to not pass most new FY 2007 spending bills before leaving town, putting these off until the new Congress convenes, and running the government on a continuing resolution, mostly at last year's spending levels. There is speculation that the new Congress may decide to not even try to put together and pass FY 2007 spending bills (the fiscal year started Oct. 1), instead just funding things by a continuing resolution for the rest of the year, mostly at FY 2006 levels. The Fermilab director Pier Oddone has issued a statement about the implications of having to run Fermilab at the FY 2006 funding level for the rest of the year. These would be dire, including having to take such measures as completely shutting down the lab and furloughing its employees for a month.

This would be extremely bad news for Fermilab, coming at a time when they have been having great success with getting the Tevatron to run at ever greater luminosity. The machine has just set new records for weekly luminosity, monthly luminosity, and initial luminosity (you can follow their progress here). While everyone is concentrating on the LHC, the Tevatron remains the only machine in the world running at the high-energy frontier, and the most likely source of any surprising new information about beyond standard model physics during the next couple of years. It would be a great shame if budget problems were to have a negative impact on this.

I don't have any information about what the impact of these budget problems might be on particle theory or on mathematics. For mathematics, the impact may not be so great since, after several years of sizable budget increases at the NSF, the FY 2007 budget request for mathematics at the NSF contained only a 3.2 percent increase.

There's a quite interesting interview in the latest (February) issue of the Notices of the AMS with William Rundell. Rundell was the director of the mathematics part of the NSF until last summer. He describes how during his tenure the NSF emphasized "single-researcher" or "PI" grants, saying that:

If you take any block of time from NSF's beginnings to now and you ask, what were the best years for the DMS single-investigator grants or for senior researcher increases?, the answer is the period of 2001 through 2005.

Rundell notes that during this time the number of grants went up by only a small amount, maybe 10 percent, but that the value of each grant "went up enormously". Before 2001, people were being given at most one month of "summer support", now junior people get two months, and senior people often a month and a half or two months. While inflation and average university raises have been around 2 to 3 percent, the academic star system has had stars (the people most likely to be getting these grants) receiving 6 to 7 percent raises, 10 percent promotion raises, and big hikes in salary when they move. So, the bottom line is that a lot more money has been going to a small segment of the mathematics research community.

The interviewer states that "Most mathematicians believe PI grants are the most important part of the DMS", but I wonder whether that is really true. Rundell also explains that the current system leaves most mathematicians with not much motivation to lobby for an increased NSF budget, especially if most of the increase is going to go to a small number of well-paid people:

I think it is probably true that the mathematicians who get the money aren't pulling their weight for justifying us to get more. And on the other hand, those people who are disenfranchised have no incentive to do that.

Personally I've never understood the logic of devoting such a large part of the NSF research budget in math or theoretical physics to increasing the salary of the best paid people in the field, although I hear that once one achieves such a status the reasons become much clearer. Besides the "summer salary" though, these grants do fund many things that are important for the health of university math departments, especially supporting graduate students. Rundell claims that over this same period the NSF has doubled its support for graduate students. This is probably reflected in the data contained in another article in the new Notices, an annual survey of new doctoral recipients. This survey finds the number of Ph.D.s awarded last year in mathematics to be 1245, the highest number ever recorded. Four years ago this number was at a local minimum, with 948 mathematics Ph.Ds awarded.

Also supposedly suffering from funding problems is the high energy theoretical physics group at Harvard, where, according to one of its faculty members, because of feminism the university has been unable to afford competent computer support. As a result the group has recently had to shut down its web server (schwinger.physics.harvard.edu), and evidently has had several of its machines broken into, with no administrator around to deal with this. There's a huge on-going problem with university computer systems which seems to be the same thing that happened at Harvard. Many groups of hackers have broken into a large number of insufficiently well-protected university unix systems, often installing trojanned versions of the SSH software. The trojanned SSH client programs then gather people's usernames and passwords as they are typed in when SSH is used to login to another system. These are used to break in to yet other systems. Since SSH is the fundamental tool used to manage logins between different machines at most universities, this is a very difficult problem to deal with.

One reason I've mentioned this is to warn people to be very careful about using SSH, especially using it to login from a system not at your home university, since the SSH program on the machine you are using may be trojaned. Better to use your own laptop, with its own SSH software. I'd like to discourage posting of comments about computer security here, since most such comments just spread misinformation of one kind of another, just making problems worse. There are many other places on the internet to get information about and discuss these issues.

Update: There's more about the 2007 NSF budget here.

Update: Today's NY Times has an article here. It seems that many other labs, including RHIC and Jefferson Lab, are facing similar problems.

Comments

1. **plank**
   January 5, 2007

   just regarding ssh. Try using private/public key (DSA for instance) authentication when possible and most of those problems go away. And don't login *from* a computer you don't trust (even when using keys)

   If LHC finds nothing people are expecting (SUSY, higgs) nor anything completely unexpected (my favourite) then budgets will get far more stringent in the future.

2. **Grammar Nazi**
   January 5, 2007

   "Congress may decide to not even try and put together and pass FY 2007 spending bills"

   "try and"? Shame

3. **Peter Woit**

January 5, 2007

OK, OK, Grammar Nazi,

When I proofread the posting I did think that the writing was worse than usual, but also that I was too busy to do much about it. And, hey, standards on internet blogs are low in general, right?

4. **Kevin**
January 5, 2007

Simply put, I do not believe Fermilab's Pier Oddone. Inflation is about 3%/yr. Thus, if 2007 funding is at 2006 levels, they will have to cutback by 3%, not close down for a month (an 8.3% decrease).

Besides, when was the last time Fermilab produced any good work? 1976? They have hardly established a good track record in the last few decades, and better they live within a budget than that my taxes go up.

5. **Peter Woit**
January 5, 2007

Kevin,

Oddone gives a detailed justification in his letter to the DOE. This far into the fiscal year there are strong constraints on what he can do to save money, especially since supposedly there are no major capital spending projects going on that could be stopped or slowed down. His estimate for what they can save by shutting down the lab for a month is 3 percent of the budget. You're not going to get an 8.3 percent savings since what you save by turning off the lights and furloughing people is only part of your operating costs. You'll continue to need some people to stay there, and you morally can't do things like shut off medical benefits to your furloughed employees.

If you really don't think Fermilab produces any good work, you shouldn't be arguing for saving $11 million by shutting the place down for a month, but for saving billions by shutting it down permanently (along with the rest of US HEP research, which hasn't done any better than Fermilab). And please, everyone, take the aggrieved arguments pro or con about taxation levels to other blogs hosted by people willing to tolerate them.

6. **Peter Orland**
January 5, 2007

In defense of Farm 'n Lab…. I agree that nothing earthshaking is has come out in a long time. An important consideration, however, is that there are standard nuts and bolts measurements, such as more accurate values for masses and widths, soft-scattering measurements, etc. This is physics, even if not the exciting kind.

I have a question for experimentalists or phenomenologists who might be reading. Will measurements at the Tevatron be useful for interpreting LHC data? I think they should, since the background at the LHC will be hard to subtract otherwise. Someone else would certainly know for certain.

7. **Peter Orland**
January 5, 2007

If Grammar Nazi is reading, there is no need to chastise me. Re-reading my last sentence fills me with shame.

8. **anonymous**
January 6, 2007

Kevin:
1976? For most of us, the discovery of the top quark was certainly good work. Perhaps you meant 1996? In any case, the Tevatron has put lots of limits on new physics, and has precisely measured a lot of Standard Model parameters. It also does good B physics.

Grammar Nazi:
"Try and" is perfectly grammatical in typical English usage. Surely you have better things to do than carry on a pedantic crusade based on arbitrary rules? For instance, you can go to [this post at Language Log] to see "try and" being used by a coauthor of the Cambridge Grammar of the English Language.

9. **pheno answer**
January 6, 2007

backgrounds have already been measured, and will be much worse at LHC.
Observing tops, Z, W… at LHC will help in calibrating LHC detectors.
People moving from Tevatron to LHC will bring their expertise.

The main goal of Tevatron was and is discovering something new just before LHC. This would have some scientific value, because Tevatron detectors and backgrounds are already understood. Nevertheless it seems to me an example of poor competition: Tevatron runII is not really needed for physics and needs a lot of resources. For comparison, CERN preferred to close LEP (despite the hints for a higgs, leaving the next strike to Tevatron) and go on with the next big project. Hopefully this will turn out to be a good and brave decision.

10. **Michael**
January 6, 2007

The more I read your blog the progressively more skeptical I become about a meaningful career in hep; maybe 'financial engineering' isn't such a bad move to make if you want a remote chance to make a decent living *AND* use all the new tricks and skills you pick up during your studies.

How say you, Pete?

11. **Chris Oakley**
January 6, 2007

> The more I read your blog the progressively more skeptical I become about a meaningful career in hep; maybe 'financial engineering' isn't such a bad move to make if you want a remote chance to make a decent living *AND* use all the new tricks and skills you pick up during your studies.

I am not any of the Petes, but I have done a fair amount of Financial Engineering after doing a PhD in HEP theory. What you need to be a Financial Engineer is knowledge of probability theory and the ability to solve PDEs. Also, programming skills are essential. However, you are unlikely to be using any techniques you learned after your first degree. The reason that employers choose PhDs is that there is hot competition for the jobs and PhDs look better on paper.

12. **D R Lunsford**
January 6, 2007

Peter – do you think there is a niche for physicists/mathematicians with professional-level computer knowledge to run these university systems? I would think such a person could better serve the needs of physics/mathematics faculty.

-drl

13. **Peter Woit**
January 6, 2007

drl,

I do think it is a good idea for physics/math departments to consider having their systems managed by a permanent person with a Ph.D. who teaches and has some sort of faculty position. This has worked well here in the Columbia math department, with me and my predecessor (but then I would think that..). The main problems I've seen with departmental computer systems occur when one tries to run them with grad students or poorly paid/part-time computer administrators, who don't stay in the job very long. If you want a reliable computer system, you need to have someone smart in charge of it who is around long-term, and has the authority to enforce standards and spend the money needed to ensure reliability. The security and spam difficulties associated with running mail servers and multiuser unix systems are huge, and the day is long gone when a faculty member or grad student could set these things up and get them working reliably in their spare time.

14. **Peter Woit**
January 6, 2007

Michael,

I think Chris is right that most "financial engineering" jobs don't use that much sophisticated mathematics. But they certainly are much better paid, with much better job prospects than in academia, especially if you want to live in a place like New York or London. At the moment half of all personal income in Manhattan comes from the financial industry, and extrapolating from past trends, that should soon approach 100 percent. Not clear to me what the future holds…

Mathematics is a perfectly healthy field, theoretical physics much less so, with the problems I discuss ad nauseam here. Experimental HEP in the US has serious questions about its future. For both theory and experiment, the future should be a lot clearer 4-5 years from now after initial results from the LHC. If the LHC doesn't find something that disagrees with the standard model, experimental US HEP will be in big trouble, theoretical physics will also be in huge trouble if it continues down the same road it has been following.

15. **C**
January 6, 2007

Hi Peter Orland,

"Will measurements at the Tevatron be useful for interpreting LHC data?"

"Tevatron-for-LHC Report: Preparations for Discoveries," hep-ph/0608322, has some information about this.

Regards,
C

16. **ssh?**
January 6, 2007

Peter, sorry for asking a question about computers, but maybe you are in a unique position also for this issue. I would like to understand if forbidding direct ssh access (to everything, including the http server) is an exaggeration. To users like me it seems more annoying than having some hackers, but our professional administrators have a different opinion, and, being not competent enough, I cannot understand if they are right, or if they like to have a too easy life.

17. **Peter Orland**
January 6, 2007

C,

Thanks!

18. **Peter Woit**
January 6, 2007

ssh?,

Unfortunately, at the present time there is a good case to be made for severely restricting people from logging in to accounts that can run a shell from machines that one doesn't trust. There are just too many compromised machines out there, and if someone logs in from one of them, a group of hackers now can and will get into the account. From a shell account, even if they don't know much they can cause significant trouble. If they are very knowledgable they can probably get root access and cause huge amounts of trouble (compromising many accounts on the system, trojaning ssh so they can get into other systems, trojaning the system software so that the server needs to be completely rebuilt, etc. etc.).

Security is always a balancing act between making it convenient or at least possible for people to do things over the network that they need to do, and protecting systems by minimizing the number of potentially dangerous network connections. There's no easy answer to this…

19. **Ari Heikkinen**
January 6, 2007

"I do think it is a good idea for physics/math departments to consider having their systems managed by a permanent person with a Ph.D. who teaches and has some sort of faculty position."

That wouldn't work. Having someone who teach as system administrator "when they have time" only means the systems will basicly be left unmanaged and the only thing they end up doing is add or remove a few accounts now and then.

It's a full time job trying to keep up to date with security updates and especially trying to anticipate any possible future attacks (then again, just avoid windows and you'll automatically avoid most of the security problems of today).

20. **Peter Woit**
January 6, 2007

Ari,

"That wouldn't work."

Well, my experience is definitely that it can work. My colleagues here in the Columbia math department seem to be quite happy with how the computer system runs, and it's basically run by me, at the same time that I teach one course per semester (some semesters a seminar for the graduate students giving them practice teaching, which requires little preparation). Before I took over this job, it was done on a similar basis by someone else, and people were also happy then with how it worked. It would not work if the person involved had a much larger teaching load. It also can take quite a few years to get a computer system stabilized, secured, and working in a reliable, secure and easy to maintain manner.

And no, avoiding Windows is not the solution to all problems. The problems the Harvard theory group seems to have had which required shutting down their systems had nothing to do with Windows. And Windows/Mac/Linux arguments and evangelism are completely off topic. Don't even think of it…

21. **Ari Heikkinen**
January 6, 2007

Just to add to my previous post, I use them all. There's different sets of problems each one solves the best.

22. **Chris W.**
January 7, 2007

Regarding computer and network security see this sobering article:

Attack of the Zombie Computers [botnets] Is Growing Threat
(NY Times, 1/7/2007)

(Windows XP Home Edition, not to mention Windows 98, has for years been considered a botnet disaster waiting to happen.)

23. **Bastard Operator from Hell**
January 7, 2007

Ok, Peter's idea to take discussion about security issues elsewhere sure is a good thing:

1) "trojanning" is a wrongly spelled verbing of a bastardization of "installing a trojan horse program". What happens is that the SSH client is either replaced or "debugged" by something that captures password information. Thus, "compromising the SSH client" is the phrase to use.

2) Plank says: "Try using private/public key (DSA for instance) authentication when possible and most of those problems go away." They do not and DSA is NOT a recommended algorithm for public key cryptosystems. However, "two-factor authentication" will alleviate some problems.

3) Ari says: *'Having someone who teach as system administrator "when they have time" only means the systems will basicly be left unmanaged'* Ab-so-lutely. It's fun to do half-assed system administration from time to time (don't we all love to tinker) but how can you ever dot the i's and cross the t's on a network with more than a handful of servers, workstations and users ? This is a high-dimensional space and the inevitable random walk will quickly take you into the regions that are labelled 'here be downtime'. Not to be insulting but I have noted that often people are somewhat optimistic regarding the true state their network is in, especially if they have an academic background. Ah the good old times when all our data was on a single harddisk under the teaching assistant's desk (no, it was not in the backup pool either).

Also…where's the documentation?

4) Two words: *Tripwire* or *Radmind*.

Bu enough of this, enjoy the Sunday.

24. **Eeyk**
January 7, 2007

"trojanning" is a wrongly spelled verbing of a bastardization

It's interesting how wrongly spelled verbings of bastardizations can be absolutely clear and meaningful. "Trojanned ssh" simply says it all in just two words…

25. **CD318**
January 7, 2007

I can speak to life sciences. An NSF proposal on which I was PI scored in the top 15% was declined; less than 10% of submissions can be funded at present (in recent years around 20% have been funded). Fortunately we can scrape by for awhile on other funding sources but for others this will be the difference between surviving and not.

26. **CD318**
January 7, 2007

Kevin typed: "Simply put, I do not believe Fermilab's Pier Oddone. Inflation is about 3%/yr. Wellllll… belief != data, and the Consumer Price index != inflation of scientific equipment and supplies (which are increasing faster), or of the cost of electricity required to run the experiments. Moreover, the cost/time of running a scientific experiment is not constant, even in constant dollars. In addition, nothing in the above post indicates that Fermilab itself is going to see a 3% cut. It is common, even when agency budgets are flat to implement across-the-board cuts that are considerably more severe.

In other words, it is obvious that you are operating in a severely data-deficient state, and that your beliefs, while strongly felt, are not based on much beyond strong feelings. Finally, I would note that the Republican congress and the President were planning to DOUBLE the NSF physical science budget over the next several years, and I doubt that the Democrats will scuttle that particular plan. So you might want to avoid getting too excited about a one-year 3% cut. Over the longer term, what you want is simply not going to happen.

27. **Peter Woit**
January 7, 2007

    OK, OK, will fix the bad spelling…

28. **Who**
January 7, 2007

    I suggest
    trojan'd

    the double n doesn't look right—it puts the stress on the second syllable
    but neither does **trojaned**.

    Verbing is just fine in this case.

29. **Jimbo**
January 7, 2007

    So how much funding for the Tevatron would be required to keep it running until the new budget is cranked out ? I'm guessing on the order of ~500 M$$…..
    Perhaps Jim Simon could talk to Bill Gates, Steve Ballmer, Matt Lesko, Sir Richard Branson, and other high-tech philanthropists, and they could in turn network to their peers….shortly, in a geometric progression, the bread might be made available if these parties were made aware of how dire the straits are….?

Astronomy Posting

January 8, 2007
Categories: Uncategorized

When I was young, my main scientific interest was in astronomy, and to prove it there's a very geeky picture of me with my telescope on display in my apartment, causing much amusement to my guests (no way will I ever allow it to be digitized, I must ensure that it never appears on the web). By the time I got to college, my interests had shifted to physics, and since that time I've hardly at all kept up with what is going on in astronomy. Like everyone, I'm still fascinated by the amazing pictures coming out of the field, and like most particle physicists, I'm deeply jealous of astronomers for the fact that they have a wealth of exciting new data to work with, together with promising prospects of lots more to come.

This week there's a big meeting of the American Astronomical Society going on in Seattle, producing lots of astronomy news. Many bloggers are in attendance, including Rob Knop, Steinn Sigurdsson, Phil Plait, and C.C. Petersen. Rumors that celebrity couple Sean Carroll and Jennifer Ouellette were there turned out to be partially unfounded. Lots of press releases are being generated, including one from the University of Washington full of the usual overhyped claims about cosmic superstrings.

This week's Science has a special issue on particle astrophysics, with lots of articles worth reading, including a nice summary of the exciting things happening in the field by Adrian Cho. He reports that many experimental particle physicists have moved into the field, partly because of the opportunities there, partly because of the difficult situation of experimental particle physics, especially in the U.S. Michael Turner is quoted explaining that the particle physicists have brought to the field some ambitious ideas, due to their habit of "thinking big":

These are not people who are afraid to ask for big things, and they're used to people saying yes.

An example of this is the IceCube neutrino experiment being put together under the ice in Antarctica, employing 400 researchers and costing $271 million.

Turner also has an article summarizing the situation in cosmology, where he notes that many string theorists are now pinning their hopes on making some connection to the real world in this context:

Nowhere in particle physics are the stakes higher than for string theory. If string theory is to live up to its billing as "the theory of everything" rather than, as some say, a theory of nothing, it needs a home run. Because most of its current predictions exceed the reach of terrestrial laboratories, many string theorists are pinning their hopes on a cosmological home run, such as a fundamental understanding of inflation (or a more attractive alternative), a solution to the puzzle of cosmic acceleration, or insight into the nature of the Big Bang itself.

For something truly bizarre, check out the cover story of the February issue of Astronomy magazine, entitled "What if string theory is wrong?" (mentioned earlier here). It confirms me in my opinion that I shouldn't write about things I don't know much about, like astronomy, since it's by an astronomer who clearly knows very little about particle physics, especially about supersymmetry:

Supersymmetry is a mathematical principle that allows force-carrying particles, such as photons and gluons, to transform into one another. It also allows the unification of gravity with other forces because its particle, which some call the graviton, can transform into one of the other force-carriers…. If extra dimensions don't exist, then supersymmetry doesn't either… Without supersymmetry, some physicists have proven that the energy of empty space would be so enormous the universe would instantly collapse. Only by understanding physics beyond the standard model can we hope to understand how the vacuum works and the universe's dark side. And only string theory appears able to serve as a reliable mathematical guide to that larger universe.

Lenny Susskind provides the usual over-the-top outrageous quote:

It is hard to find a serious paper about particle phenomenology that doesn't in some way use the tools of superstring theory.

The author seems to believe that there's some sort of experimental evidence of string theory and that it is just like general relativity:

While string theory is sparse on experimental validation, the situation is not so different from general relativity in its early days, when difficult mathematics made calculating a prediction extremely challenging.

and somehow thinks that string theory is the only hope for the future of physics:

Without superstring theory, we'd lose the intriguing prospects for the multiverse, with its infinite and eternal creativity in spawning new universes… More immediately, dark matter and dark energy would remain imponderable enigmas, shorn of any clues about where they come from. Astronomers can live without knowing the quantum properties of gravity. But to learn that 96 percent of the cosmos is unknowable would be a bitter pill to swallow. It would be even worse for physicists. Without a logical framework in which to pose and answer questions, our inquiries into the fundamental aspects of the physical world would devolve into semantic quibbles.

Some days I think that there's definitely a more realistic view of string theory out there, other days I'm not so sure…

Update: It seems that Edward Witten is attending the AAS meeting, although not speaking there. See the comment from David Cobden, and Steins Sigurdsson's blog entry from the conference Trendspotting, where he reports:

On a completely unrelated note, Ed Witten was spotted wandering the halls…
Now there is always some cosmic string or quantum cosmo thingy going on here, but what we ask (and, yes, I did actually ask), was he doing in the extrasolar planet session?

Ed likes exoplanets!
Dood.

Update: Science a Gogo has an article about this, String Theory? Knot!, which uses my characterization of Susskind's quote as "over-the-top", but then uses the wrong quote, using something from the Astronomy magazine article which wasn't written by Susskind.

Update: The University of Washington press release on cosmic superstrings, based upon a poster presented at the AAS meeting, has made it to Fox News (via Lubos).

Comments

1. **gunpowder&noodles**
   January 8, 2007

   "Without superstring theory, we'd lose the intriguing prospects for the multiverse, with its infinite and eternal creativity in spawning new universes…"

I don't know why, but I find that quote laugh-out-loud funny. One pictures mournful physicists lamenting the good old days when the multiverse had infinite and eternal creativity.

2. **Chris Oakley**
January 9, 2007

Google Translate

Translate Text

Original text:

> Without superstring theory, we'd lose the intriguing prospects for the multiverse, with its infinite and eternal creativity in spawning new universes…

Breathless Popular Science Hype to English

> Superstring theory requires more than 3+1 dimensions, which is unfortunate as there is no compelling reason to believe that there *are* any extra dimensions.

3. **M**
January 9, 2007

maybe all your quotes are meant to be somewhat overhyped, so I should not add that IceCube was built thanks to the experimentalists who spent many years developing the necessary technology with smaller experiments (from Baikal to Amanda), and because astrophysics suggests that a km^3 detector is needed to probably start seeing neutrino events from astrophysical sources.

4. **Tommaso Dorigo**
January 9, 2007

Thank you for the quotes from Astronomy. I have always preferred Sky and Telescope to it, but it does not harm to be given additional good reasons now and then…

Cheers,
T.

5. **John A**
January 9, 2007

> While string theory is sparse on experimental validation, the situation is not so different from general relativity in its early days, when difficult mathematics made calculating a prediction extremely challenging.

General Relativity: published 1915.
First exact solution (Schwarzschild)- 1916
Prediction of perihelion shift of Mercury – c. 1916
Bending of spacetime by gravity (Eddington) 1919
Gravitational Red Shift (Pound/Rebka) 1959

String Theory: [this line unintentionally blank]

6. **Bee**
January 9, 2007

It is hard to find a serious paper about particle phenomenology that doesn't in some way use the tools of superstring theory.

What exactly might he mean with 'the tools of superstring theory'?

7. **LDM**
January 9, 2007

Susskind, by making such unfounded statements, must realize he only succeeds in destroying any scientific reputation he may have enjoyed.
As far as Physics is concerned, if he truly believes his statement and is not just engaging in some form of self-promotion, then he is truly delusional.

8. **Ari Heikkinen**
January 9, 2007

"Without superstring theory, we'd lose the intriguing prospects for the multiverse, with its infinite and eternal creativity in spawning new universes…"

Actually, you don't lose anything. I don't need string theory (or any other theory or even any math) to conclude that we're in a bubble called the "universe" (or anything you want to call it) and anything (if anything) out there outside this "bubble" could be called another "dimension" (again, whatever you like to call it).

And even with this purely philosophical reasoning I'm not going to be any more right or wrong than any other reasoning of whatever might be out there or not.

It's kind of like what's going to happen after death. There's lots of theories of what might happen with each one as likely to be right or wrong, as no one's came back to tell what really happened.

So my message to all physicists who happen to read this is: instead of wasting all your life on theories which aren't even remotely useful for anything practical why not instead do something useful, like figure out how to get fusion reactors working (and when it comes to funding, this is where the money should go).

9. **Thomas Love**
January 9, 2007

Lenny Susskind said:

It is hard to find a serious paper about particle phenomenology that doesn't in some way use the tools of superstring theory.

Obviously Susskind doesn't know how to do a literature search.

10. **Chris W.**
January 9, 2007

But to learn that 96 percent of the cosmos is unknowable would be a bitter pill to swallow.

Is Susskind referring to the multiverse here? If so, shouldn't that be "99.9999999…. percent of the cosmos is unknowable"?

11. **Dan**
January 9, 2007

Dear Peter & co,

How successful has been string cosmology, including Randall's & Sundrum braneworld models? How successful has loop cosmology been? I understand that there probably isn't much in the way of hard quantitative predictions, but what about soft or generic predictions, that would differ and be potentially observable, from current SM+GR? (i.e I am aware string cosmology offers SUSY-partners as DM candidates. Anything else?)

curious
Dan

12. **Peter Woit**
January 9, 2007

Dan,

As far as I know, none of "string cosmology", braneworld models, loop cosmology make any predictions, hard, soft, generic or whatever. The SUSY dark matter candidate is a feature of SUSY, not of strings, branes, loops…

13. **Dan**
January 9, 2007

Hi Peter,
Ok thanks. When I google there are a lot of hits, such as arxiv.org/abs/astro-ph/0303499 and arxiv.org/abs/astro-ph/0303499 but I am unsure what real astronomers think

14. **Robert Musil**
January 9, 2007

"[T]o learn that 96 percent of the cosmos is unknowable would be a bitter pill to swallow. It would be even worse for physicists. Without a logical framework in which to pose and answer questions, our inquiries into the fundamental aspects of the physical world would devolve into semantic quibbles."

Perhaps the author is suggesting that if string theory must be abandoned for whatever reason, its cultists will (must!) Drink The Coolaid instead of soldiering on in a purposeless world! The IAS as Jonestown! Well, in that case, maybe it will be sherry, not Coolaid.

15. **Peter Orland**
January 9, 2007

Paul Says:
January 9th, 2007 at 6:03 pm
a) If three-dimensional space (let's omit the "fourth dimension of time", for a moment, reason given below) is "curved" by gravity, what is it curving into? For example, if you place a bowling ball on a bedspread, it is curving *from* the second dimension *into* the third dimension.

Paul,

The answer to your question is very subtle. To understand curved
space, it isn't necessary to curve that space into anything. This was discovered by Gauss and Riemann, and is a fact of "intrinsic geometry".

To see how this works, imagine a curved sheet of rubber. Draw
coordinates (that is a graph) on the rubber. Now mash it flat into the plane. The coordinates will remain on the sheet, but won't be
flat. The intrinsic curvature is still there, due to the stresses
rubber. On the other hand, if you take a flat piece of rubber
and bend it (without otherwise straining it), it remains intrinsically
flat, though there is now what is called extrinsic curvature.

Einstein's equations of gravity relate the intrinsic curvature of
spacetime to matter. They say nothing about extrinsic curvature,
i.e. how space-time is embedded in some space of more than
four dimensions.

Implicit in your question is another about curvature of
three-dimensional space, as opposed to four-dimensional
space-time. Many popular books incorrectly explain
gravity as being due to curvature of space. The bowling-
ball/bedspread picture is NOT really the right way to think
about gravity. In the most natural (either comoving or
Schwartzchild) coordinates, the spatial curvature is
extremely small. Newtonian gravity is not at all related to
spacial curvature, in these coordinates, but in space-time
curvature. There is some curvature of three-dimensional space, responsible for some of the shift of the perihelion of mercury,
but it is very small.

The physical way to think about gravity is this… In so-called
comoving, or inertial coordinates, space falls into energy
(which by E=mc^2 includes matter). Particles in this space fall
with it. The bedsheet is slightly curved by the bowling ball, but
Newtonian gravity is due to the bedsheet being sucked into the

bowling ball!

One can choose other coordinates, where spacial curvature
is big, but they are not particularly useful.

16. **anonymous**
January 9, 2007

Dan, there are plenty of specific models of inflation in string theory. They can involve things like a brane and an anti-brane in the extra dimensions which move toward each other and annihilate. While they move, the universe is expanding (inflation); when they annihilate, lots of particles are produced. Contrary to what Peter says, any specific model of this type does make specific numerical predictions for things like the CMB power spectrum. The trouble is that there are a lot of models of this type, and not enough numbers measured well by experiments to select among them, at least so far.

17. **woit**
January 9, 2007

Please Peter O. and others. This kind of discussion of the basics of GR is off-topic and has nothing at all to do with the posting. This is not intended as a general physics discussion forum, which is something I have no interest in hosting or trying to moderate.

anonymous,

Again, there is no such thing as a "prediction" of string cosmology, or a "prediction" of brane world scenarios about cosmology. These ideas cannot tell you what the next generation of experiments is going to see, or the one after that. There are so many variants of these models and lots of parameters, so much so that these ideas are vacuous in terms of making anything anyone would call a legitimate prediction. If you disagree, tell us what "string cosmology" or brane-world scenarios says the next generation of CMB experiments will see, and be willing to agree that string theory is wrong if they don't see this.

18. **Aaron Bergman**
January 9, 2007

Specific models make specific predictions. Nothing's going to say that string theory is wrong, but various scenarios make unambiguous predictions and are falsifiable.

19. **Peter Woit**
January 9, 2007

Aaron,

"string cosmology" is not falsifiable
"brane world cosmology" is not falsifiable
I have no idea about loop cosmology.

Sure, you can come up with highly complicated, highly speculative models that don't really explain anything about observable physics but make "predictions". I could make up a "scenario" which involves God writing the lyrics to Lucy in the Sky With Diamonds in Morse code and imprinting these in the CMB at a certain amplitude. This would make definite predictions, and be completely falsifiable.

20. **Robert Musil**
January 10, 2007

I'm not an astronomer, but I sent your quotes to the astonomers I know (there are not that many – but they do include people working at Harvard-Smithsonian). They weren't even aware that string theory offered any explanation whatsoever for Dark Energy and Dark Matter. They tell me the most likely candidate for Dark Matter is "axions", which are hypothetical particles that arise from supersymmetry in the standard model (no strings there). The most likely candidate for Dark Energy is said to be some kind of small positive vacuum energy due to something about the vacuum or fields that we don't understand (again, no strings needed). These astronomers agree that while positive vacuum energy many constrain string theory, there are no constraints of any sort from string theory to observations.

For the record: The astonomers I asked are actually now directly involved in observation projects regarding Dark Energy and Dark Matter. Just thought I'd mention that.

21. **LDM**
January 10, 2007

Aaron Bergman,

"Nothing's going to say that string theory is wrong."

Please. Look up the definition of the science method somewhere and then rethink your statement.

There is another quote by Pauli, lesser known, but perhaps with more scientific content and less sarcasm than the title of Peter's book. Pauli pointed out that there is always a *continuum* of theories that agree with a given phenomena, but the test of a theory is that it predicts something new.

Note Pauli did not say postdict.
Note that string theory fails this test miserably.

Pauli arrived at this observation, in my opinion, because his genius was so great that he could and did easily discover alternate formulations and approaches.

There is so far no compelling reason to believe string theory in its current form and considerable reasons not to, that is unless you are like Susskind and have invested your professional identity with it…and also consider yourself one of the fathers of the theory.

22. **anon y mus**
January 10, 2007

Dan & other anonymous,

models with parallel branes in extra dimensions have been studied hoping that a) their potential energy naturally gives the flat potential needed for inflation; b) their final collision generates the particles we see.

However a) turned out to be false, after that the size of the extra dimensions is stabilized. Furthermore b) when inhomogeneties in the matter density can be computed, their spectral index comes out very tilted, unlike the observed one. And c) in any case the idea that some God liked to start the Universe putting two perfectly parallel branes looks as naive as the ancient oriental cosmology where the Earth stays above a turtle above a turtle…

23. **anon y mus**
January 10, 2007

correction: probably this cosmology is not naive from the modern point of view of String Naturalness: perfectly parallel branes can be justified by invoking the Anthropic Principle.

I am sorry, but for a moment I forgot that Susskind told us that we must use the tools of superstring theory to do something serious.

24. **r hofmann**
January 10, 2007

Dear Ari,

`So my message to all physicists who happen to read this is: instead of wasting all your life on theories which aren't even remotely useful for anything practical why not instead do something useful, like figure out how to get fusion reactors working (and when it comes to funding, this is where the money should go). '

I agree with you. All philosophizing about mere matters of belief,
such as the multiverse, is even culturally irrelevant if our very culture won't survive the looming climatological catastrophe we
will have to face in the absence of a viable alternative to the present way of fueling or energy needs. I personally believe that fusion with
magnetically confined plasmas will be viable if we manage to
apply genuine knowledge gained about electroweak symmetry breaking (theoretically and at the LHC) to this problem.

High regards.

25. **Lee Smolin**
January 10, 2007

Dear Dan,

Regarding loop quantum cosmology, this is a class of models, not yet the full quantum field theory. Nonetheless within them, there is a universal mechanism for eliminating the initial singularity. That mechanism implies modification of the power spectrum, at a level that may be observable in future CMB observations, as argued in astro-ph/0411124. Whether this leads to predictions from the full theory rather than models is the subject of ongoing work, see for example astro-ph/0611685.

26. **Dan**
January 10, 2007

Dear Lee Smolin & both anonymous

thanks. It seems string cosmology is also a class of models as is loop cosmology? Incidentally Lee, I've been hoping you and Bilson publish a followup preon paper 😉

27. **Aaron Bergman**
January 10, 2007

"string cosmology" is not falsifiable
"brane world cosmology" is not falsifiable
I have no idea about loop cosmology.

Sure, you can come up with highly complicated, highly speculative models that don't really explain anything about observable physics but make "predictions".

Actually, some of them aren't particularly complicated, and they do make honest predictions. I don't understand what your problem with this is. The models can be ruled out.

28. **Landscape vegetable**
January 10, 2007

How long will it take to rule out 10^500 models? How many per second can be ruled out? To do it within 1000 years (3*10^10 seconds) would mean ruling out ~10^490 models per second. The universe is only 4*10^17 seconds old, so to do it over the age of the universe would require ruling out only ~10^483 models per second.

It reminds me of SDI in 1985. Objectors to SDI dismissed it by working out that all the computers needed to shoot down Soviet missiles would take thousands of years to program. It's too bad that nobody influential is using this argument against the Landscape.

29. **anonymous**
January 10, 2007

Peter says the models "don't really explain anything about observable physics," but for most of us having a reasonably simple model of how inflation could work is pretty nice. As for what "anon y mus" says, I suggest reading more of the literature — it's not always about exactly parallel branes, and there are some viable scenarios. It turns out one doesn't even necessarily need slow-roll, brane inflation has suggested scenarios where the kinetic term matters more than the potential. Anyway, Dan, yes, brane cosmology suggests various models, which as Aaron says are falsifiable. What it doesn't do is suggest a unique model, but neither did old, field-theoretic inflation. So I don't really know what Peter's so worked up about.

30. **LDM**
January 10, 2007

anonymous ,

If string theorists have to resort to an extremely speculative idea like inflation as evidence for an even more speculative idea, string theory, than that itself is telling. I do not consider such models meaningful.

31. **Lee Smolin**
January 10, 2007

Dear Dan,

Loop quantum cosmology must be a set of models for two reasons: 1) they incorporate diffrerent models of the inflaton. 2) They describe reductions to different homogeneous cosmologies, i.e. FRW, Bianchi I, Bianchi IX etc. These are harmless, they do not represent different low energy theories or low energy phenomenologies as do the string cosmologies. Moreover there are no fine tunings in the main results, such as the removal of singularities or corrections to the CMB spectrum.

At the same time, one would like to do better and know if the predictions found are genuine predictions of full quantum gravity rather than a quantization of a reduced set of degrees of freedom. This is the aim of some current work.

There is also a more speculative direction of work in which we postulate that, if geometry emerges from a low energy limit of a purely quantum geometry it may do so in a phase transition which leaves observable signatures. This work is very recent, see hep-th/0604120, and hep-th/0611197, but it appears that one can make distinct predictions for CMB observations on the basis of a few simple assumptions about that conjecttured phase transition, see astro-ph/0611695.

Thanks re preons. Work is presently underway, in collaboration with several people.

Thanks,

Lee

32. **Peter Woit**
January 10, 2007

Aaron,

"I don't understand what your problem with this is. The models can be ruled out."

You decided to not quote the part of my comment that answers this, here it is again:

"I could make up a "scenario" which involves God writing the lyrics to Lucy in the Sky With Diamonds in Morse code and imprinting these in the CMB at a certain amplitude. This would make definite predictions, and be completely falsifiable."

There's more to doing science then coming up with "scenarios" based on pure speculation, you actually need some sort of evidence for them. What string theorists are doing now is saying that string theory can't tell us anything about low energy physics, but it is science because it can tell us about cosmology. When asked what it tells us about cosmology, the answer is "in general, nothing, but it leads to all sorts of scenarios". And the only evidence for these "scenarios" is that they are somehow compatible with the idea of string theory unification, determining low energy physics. The whole thing is kind of absurd.

33. **Peter Woit**
January 10, 2007

anonymous,

I just don't see how introducing branes improves the situation at all as far as "having a reasonably simple model" is concerned. It seems to do quite the opposite.

34. **Aaron Bergman**
January 10, 2007

There's more to doing science then coming up with "scenarios" based on pure speculation, you actually need some sort of evidence for them. What string theorists are doing now is saying that string theory can't tell us anything about low energy physics, but it is science because it can tell us about cosmology.

Not everything has to be seen through this prism of string theory, spawn of satan or harmless diversion. If I just called them string inspired models, would that be ok? Take DBI-inflation, for example. Outside of its string theory inspiration, is there anything that makes it worse than other models for inflation?

35. **Peter Woit**
January 10, 2007

Aaron,

Working on speculative ideas that really have nothing to do with string theory, and justifying this not because of evidence for the ideas, but because they are "string-inspired" doesn't seem to me like a healthy way to do science. It's also kind of intentionally misleading, intended to get people believing that "string theory makes predictions", and writing completely absurd articles for Astronomy magazine.

36. **anon y mus**
January 10, 2007

Dear anonymous-who-wants-literature-for-problem-a): see pages 22,23 of hep-th/0610102 for a short review. In my opinion the fact that the generic idea does not help is more important than the fact that one can avoid some problem by cooking up specific models.

37. **Aaron Bergman**
January 10, 2007

Working on speculative ideas that really have nothing to do with string theory, and justifying this not because of evidence for the ideas, but because they are "string-inspired" doesn't seem to me like a healthy way to do science.

People aren't working on string-inspired models because they're string-inspired, but because they are new models. There are lots of models for inflation, some string-inspired, some not. I think they're all worth investigating, don't you?

38. **Aaron Adams**
January 10, 2007

If ever there was a gauntlet thrown at the feet of an apartment visitor with a cellphone camera, this is it:

> "…there's a very geeky picture of me with my telescope on display in my apartment, causing much amusement to my guests (no way will I ever allow it to be digitized, I must ensure that it never appears on the web)."

39. **Ari Heikkinen**
January 10, 2007

I don't think it's entirely fair to say that just because you're not making any predictions you're not doing science.

Take early days of aerodynamics, for example. All they were doing was trying to understand what's happening in their wind tunnels, that is, trying to understand real world phenomena.

Even though they weren't making any predictions (at the time), everyone in the field still seem to think they were doing science.

40. **kuos**
January 10, 2007

Susskind is right.

String theorists occasionally use algebra, hence algebra is one of the "tools of superstring theory".

Very few serious particle phenomenology papers avoid the use of algebra. QED.

41. **Peter Woit**
January 10, 2007

Ari,
Experimentalists don't need to be making predictions, but theorists are supposed to, and it's theorists I was talking about.

Aaron B.,

Well, in general I don't think all possible models (of inflation, or of anything else) are equally worth investigating. One is supposed to be concentrating on those for which one has a good scientific reason. If your only reason for deciding to work on one class of models is that it's "string inspired", that seems to me to be a problem.

42. **wow**
January 10, 2007

This discussion of inflation is quite interesting. I can't think of *any* development that was important in yielding the eventual standard models of cosmology and particle physics, that Peter couldn't criticise along the same lines.

Gauge theory? Just a model. Can't make a unique prediction, because it isn't a unique model. Since you can't falsify gauge theory, its not science.

Einstein's theory of gravity? Has an infinity of solutions. If your favorite FRW cosmology doesn't match data, you could choose a different solution. Thats not science.

In both cases, people chose a framework, built models as best they could, and eventually found that ONE describes nature pretty well. For gauge theory that happened to be 3-2-1. For Einstein gravity, initially, the flat FRW solution (which has since been augmented). Other people chose other frameworks, tried to build good models, and failed. String theorists are working in exactly the same way: they choose a framework, and try to build a model that is itself predictive and falsifiable. Smolin uses his framework. Thats fine. As long as each model is testable, there is no problem with the plethora of models being suggested.

Claiming that these are string theory models of inflation, is no more or less misleading than saying that the various Lambda CDM cosmologies (specified by many parameters) are all simple versions of GR + the standard model. The model is far from unique. So what?

43. **Chris W.**
January 10, 2007

(from Ari) *All they were doing was trying to understand what's happening in their wind tunnels, that is, trying to understand real world phenomena*.

And that doesn't involve making predictions? I don't mean predictions of the "future," but just predictions of, say, what velocity field you'll get in the neighborhood of an specific airfoil positioned in a certain way in the wind tunnel. You may have such observations already in hand, which you try to reproduce with an application of the Navier-Stokes equations or some less general model. If you succeed, then you say, okay, let's *predict* what will happen if we reposition the airfoil or change its shape in a certain way, whose presumed effects we know how to calculate. It's the results of this follow-up testing that are of the greatest interest; we want to know if the initial agreement was merely a fluke.

Admittedly, the results of such tests may reflect more on our methods of approximating a solution to the model, or on our choice of relevant free parameters, than on the fundamental assumptions of the model itself, but that's part of the problem of testing the model—find ways to confront the central assumptions with *crucial* tests, ie, tests whose evaluation is not overly dependent on incidental aspects of the model's formulation.

String theory seems to have evolved in such a way that crucial observational tests, as opposed to tests of internal consistency or correspondence to (at least some) important aspects of the Standard Model, are impossible to formulate. The best criticism then becomes a meta-criticism—that string theory seems *designed* to make such testing impossible or in some sense beside the point. (Of course the apparent freedom to choose a background spacetime has played an essential role in all this.)

In the face of this situation one can continue to imagine that one has understand something about nature that wasn't understood before, but the same can be said of any number of metaphysical ideas. Susskind seems to be protesting the abandonment of this "opportunity" to understand nature. Perhaps there still remains such an opportunity in string theory, but the current situation demands a much deeper response than a proliferation of convoluted variants on Kaluza-Klein models, an interminable exploration of the Landscape, invocations of the Anthropic Principle, or exploration of the vast array of mathematical ramifications of the theory's formalism.

44. **Peter Woit**
January 10, 2007

wow,

Sorry, but I find these claims that there is no difference between the predictivity problems of string theory, and those of gauge theory or GR to just be absurd, and I can't believe that anyone makes these claims seriously.

For about the 100th time, the difference is not hard to understand. Gauge theory and GR are quite simple, rigid structures, and at the time they were suggested, they correctly postdicted huge amounts of observed physics, and made huge numbers of predictions, some of which were quickly testable.

String theory unification, whether applied to particle physics or cosmology postdicts virtually nothing and predicts nothing. Unlike the case of gauge theory or GR, there simply is no scientific evidence for the idea, nor any plausible prospects for getting any soon. Unlike the case of gauge theory or GR, attempts to connect string theory unification to real world physics aren't based upon solid predictions that follow logically from certain simple principles. Instead there's a whole industry of complicated and ugly Rube Goldberg constructions which don't explain anything, but are are designed purely to avoid contradicting the rules of logic or what is already known about physics.

45. **dan**
January 10, 2007

Dear Lee Smolin,
I am aware that the symmetry reduced loop quantum cosmology may have good semiclassical description but not LQG.

Incidentally would you mind if I cut and paste your comments to wiki here

http://en.wikipedia.org/wiki/Loop_quantum_cosmology?

A hobby of mine is to update wiki's entries on LQG
(I.e I added material on preons and Kodama state, for example)

http://en.wikipedia.org/wiki/Loop_quantum_gravity
http://en.wikipedia.org/wiki/Preon

46. **David Cobden**
January 11, 2007

I heard about something quite funny and moderately relevant today at the AAS meeting. One of our newer students was giving a poster on detecting gravitational waves from cosmic strings. He described to me how someone with a high-pitched soft voice came up to talk to him, and how the other students around him suddenly went completely silent. The stranger was quite interested and said something like "you've got to look at radiation from cusps". Student: "Kinks?" Stranger: "No, cusps." Student: "You seem to know quite a lot about string theory." Audience: smiles! Yes, the man himself, on the lookout for experimental verification!

47. **F.**
January 11, 2007

It seems strange that every harmless motion and remark of a
prominent physicist seems worth commenting on here.
What happens?
F.

48. **woit**
January 11, 2007

F.,

Well, one role of this blog is to retail gossip and keep people up to date on the activities of the celebrities of our field, and for those of us interested in particle theory and mathematics, Witten is bigger than Britney Spears, Paris Hilton and Lindsay Lohan all rolled into one. He has a very large influence on the direction of the field, so for instance if it turns out he has given up on particle theory and decides to devote himself to the study of exoplanets, that would be explosive news. No, I don't think it's happening. More likely, like many of us, Witten just thinks a lot of this astronomy stuff is cool.

49. **David Cobden**
January 11, 2007

Dear F.,

What seems strange to me is that anyone could think that EW's remark was the point of the comment. Oh well.

Dave

50. **FFF**
January 11, 2007

Can Witten go to the toilet while attending a string theory seminar without worrying that it might be interpreted as an explosive news?

51. **wow**
January 11, 2007

Actually, gauge theory (and field theory as a whole) was criticized in much the same way as you are criticizing string theory, for about 20 years following the original Yang-Mills paper. This culminated in the bootstrap/S-matrix movement where the irrelevant technical "rube goldberg" details of field theory, which was itself not unique and far from well defined, could be swept under the rug.

Of course this was badly misleading. Field theory is difficult and nonunique. It is also deep, beautiful, and correct. My guess is that this is true of string theory as well. A difference seems to be that string theory is unique, but has many solutions, while field theory is fundamentally non-unique. But this is not an important difference.

52. **D R Lunsford**
January 11, 2007

Peter,

Like you I started with astronomy and dropped it in college. Recently I acquired a good 10″ telescope and let me tell you, the thrill is still there! You'll enjoy having one, I recommend it. It's also more fun now that I understand light and the universe a lot better than I used to.

-drl

53. **D R Lunsford**
January 11, 2007

Tommaso,

“Astronomy” has always been a miserable sensationalist rag. It also had the bad effect of debasing Sky&Telescope by forcing them to compete on a more glossy level. In a sense it annihilated with the old S&T and they both disappeared in a burst of advertising dollars.

-drl

54. **woit**
January 11, 2007

wow,

The difference between QFT and string theory has nothing do with questions about uniqueness. The simplest QFT in the class of gauge theories (QED) accurately describes nature to absurd precision, makes an infinite number of non-trivial predictions, all of which turn out to work when you test them. This remains true of the only slightly more complicated non-abelian gauge theories and the standard model.

People did not argue against QFT that is was untestable, they were testing it all the time. They argued that it couldn’t describe the strong interactions, and that appeared to be the case until the discovery of asymptotic freedom. There has never been any time when people studying QFT were claiming that it was impossible to get predictions out of it because there were too many QFTs.

If you want to make an argument that string theory will someday triumph the way QFT did, you could try and claim that someday we’ll better understand string theory, and realize that it has some surprising property like asymptotic freedom that will make its current problems describing nature go away. Maybe, but there’s no evidence for that now.

55. **woit**
January 11, 2007

drl,

I still have an old 8″ telescope, don’t get to use it very much in New York City though…

56. **wow**
January 12, 2007

The history of qft is not as you say. There were two periods of significant “discouragement” due to people who thought the structure should be easier and more unique than it is.

If you read about QED (see Dysons many accounts), it is clear that by the mid 40s, after 15-20 years of worrying about relativistic quantum mechanics, most of the old heroes of qm (Dirac, Schrodinger,…) Thought a fundamentally new structure to replace qm (not QED) was needed. It was the new generation (feynmanm schwinger etc) that saw that the old complicated theory, could indeed be made to work. They needed rube goldberg contraptions to explain loop effects and it took 25 years before the correct wilsonian understanding of renormalization was attained.

Then, later, the chicken littles appeared when the theory of strong interactions required novel features of qft.

The analogy is quite clear. Understood poorly, qft is very difficult, nonuniqe, and even seems ambiguous (due to misunderstandings of renormalization). The aesthetic worries were irrelevant (science doesn’t demand uniqueness, just that one avatar of the theory be a good approximation of nature).

57. **Thomas Larsson**
January 12, 2007

Wow: Funny, I used exactly this summary of the history of QFT in the introduction to my contribution to this book. However, I don’t see how it can be construed to support string theory. Adding tons of unconfirmed (and increasingly unlikely) “new physics”, like susy, extra-dimensions, fundamental extended objects, 496 gauge bosons, or 10^500 universes is more like the new physics that Oppenheimer and others thought was necessary to cure the infinities of QED.

58. **Peter Woit**
January 12, 2007

Wow,

I have read a bit about the history of QFT… Earlier I was referring to the post 40s history of the subject, but I don’t think your interpretation of the earlier history holds water.

From the very beginning in the late 1920s, QED had a huge amount of experimental backing. First-order perturbation theory was well-defined, and gave extremely accurate, testable predictions of a huge variety of experimentally measurable effects. The problems with higher order perturbation theory were a matter of principle, but practically of little significance. The measurement of the Lamb shift made clear that higher-order effects were real and the theory had to explain them, which was rather quickly done.

It’s true it took 15-20 years to sort out the problems of how to renormalize QED, but for a large chunk of this period, most physicists were either trying to save their skins or working on war-related projects. People were worried that QED might very well be inconsistent, but I don’t know of anyone who was worrying that it was consistent, but experimentally untestable.

And the reason people took seriously Feynman and Schwinger’s renormalization methods, whether or not they found them to be of a “Rube Goldberg” nature, was that these methods gave extremely accurate and experimentally confirmed predictions for things like the anomalous magnetic moment of the electron. If Feynman and Schwinger had been doing higher order perturbation theory calculations using complicated methods, but getting no predictions out of them, just a bunch of excuses for why QED couldn’t make predictions, absolutely no one would have taken them seriously.

59. **wow**
January 12, 2007

I haven’t actually heard any string theorists say the theory will not lead to predictions, once (by whatever fortuitous experiment) the correct class of solutions is found. I have heard you say this many times, but apparently the rather large community of technical experts on string theory, does not believe your statement. To me, this looks like what critics of field theory would have been saying in the 30s and 60s. And the response of the string theorists, is similar to what the wise folks who kept working in the natural extension of existing theory (QFT) did.

The absence of data at the relevant energy scale is indeed different. This is not a problem of string theory, and has haunted beyond SM physics for 20 years. To attack string theorists and not the huge number of phenomenologists who worked on supersymmetry, technicolor, large extra dimensions, randall sundrum scenarios, little higgs models, or whatever, seems silly to me. One could just as easily make ad hominem remarks about the 10s of thousands of papers there with no experimental confirmation of the ideas. And these remarks would be just as silly: the goal of the endeavor is to divide up the space of models so * when interesting signatures are SEEN *, one will know what this means about where we live in theory space.

String theorists do the same thing with their framework. If Smolin had a framework I imagine he would also do this, though I have never understood in what sense he does have one.

60. **Peter Woit**
January 12, 2007

wow,

I'm sure people working on string theory hope that it will some day lead to predictions, but there aren't any now, and the problem is that at this point there's not a plausible scenario for how this is going to happen. You seem to be saying that you believe in a "fortuitous experiment" that will indicate that the string theory landscape picture is correct by providing evidence for a certain class of string theory solutions. That's logically possible, but I don't see any evidence for this scenario other than wishful thinking based upon a firm conviction that string theory has to be true. Yes, this judgement about the plausibility of your scenario is a personal one, but I'm pretty sure that it is shared by a large fraction of particle theorists who are not string theorists.

Beyond the standard model particle phenomenology has, like string theory, had very little success over the last 20 years. With no experimental guidance, it's a very hard problem. Like string theory, some ideas about particle phenomenology are overhyped by their enthusiasts. But I just don't see the same unhealthy domination of the subject by a single idea that is not working. Phenomenologists are at least trying lots of different ideas (even if none of them are especially promising). The idea of getting beyond the standard model physics out of some kind of string/M theory on a 10/11 space has not just failed to work out the way people hoped, but it continues to dominate a large fraction of particle theory research. Given the lack of any other successful ideas, there's no reason some people shouldn't work on it, but people who do so should acknowledge that things are not going well, and that probably other quite different ideas are needed.

The day technicolor models are pursued by large groups of theorists who create institutes devoted their study, won't hire people who don't work on these models, write books and TV shows about the glories of technicolor, and start up blogs devoted to calling anyone who doesn't believe in technicolor an imbecile, I'll start writing postings pointing out the problems with what they are doing.

61. **Thomas Larsson**
January 12, 2007

To me, this looks like what critics of field theory would have been saying in the 30s and 60s. And the response of the string theorists, is similar to what the wise folks who kept working in the natural extension of existing theory (QFT) did.

"The critics are right. We have no single prediction of string theory that is verified by observation. Even worse, we don't know how to use string theory to make predictions. Even worse than that, we don't really know what string theory is."
-Steven Weinberg

62. **anonymous**
January 12, 2007

"and start up blogs devoted to calling anyone who doesn't believe in technicolor an imbecile"

Hey, I bet Ken Lane can be persuaded to do that!

"But I just don't see the same unhealthy domination of the subject by a single idea that is not working. Phenomenologists are at least trying lots of different ideas (even if none of them are especially promising)."

Wah? I guess by one idea you mean "the right theory is a string theory." At the same level of generality phenomenologists generally go from the idea "the right theory is a QFT for which the SM is a low-energy effective theory." There is of course lots of diversity in what phenomenologists do, but at the same time there is diversity in stringy model-building (intersecting brane models, flux compactifications, etc.). I don't see what you're doing as an apples-to-apples comparison.

63. **M**
January 12, 2007

wow, many phenomenologists are interested in if/which new physics keeps the weak scale naturally small (supersymmetry or technicolor or…?) because colliders are now exploring physics at the weak scale. This is not only an important issue, it also is an issue that will be clarified, probably soon at LHC. The present attempts could be right, or could be wrong, but cannot be not-even-wrong.

64. **wow**
January 12, 2007

M:

Many of the things explored by model builders (including the most influential ideas of the past decade or so) were either directly or indirectly but obviously influenced/inspired by string theory ideas. I am not saying this as "an arrogant adherent of string theory" — this is a fact that the originators of the models would freely admit. It is true of the Randall/Sundrum works on warping and anomaly mediation; it is true of the large dimensions works which not coincidentally blossomed right after D-brane gauge theories became a topic of interest; it is true of new ideas in SUSY model building which took off from Seiberg's classic works, which in turn were inspired in part by thinking precisely about string theory moduli spaces. It is true of the recent resurgence of interest by cosmic string theorists, who see the reasonable motivation for new possibilities of strings with smaller tension that play a very subleading role in structure formation. It is true of most attempts to build models relevant for Planck, that would show measurable non-Gaussianity or gravitational waves (specially the former; ideas about the latter go back to the origin of inflation, but have been considerably influenced by theoretical considerations of quantum gravity more recently).

So, M, even the things that you say are defensible because in some very short term they are "relevant" to near term experiments, have a history that shows the value of the more formal research going on in particle/string theory.

65. **Chris W.**
January 13, 2007

The aesthetic worries were irrelevant (science doesn't demand uniqueness, just that one avatar of the theory be a good approximation of nature). (- wow)

Right, I got it; that's good. So, one should make sure that one's theory has as many "avatars" as possible, with as much diversity as possible, to maximize the likelihood that one of the them will be a good approximation of nature. After all, nature can be anything at all, and science is fundamentally trial and error, so we want to make sure the options available for those trials really cover the bases. Works for me. It looks like string theory and its offshoots are doing a heckuva job then.

(Karl Popper used to talk about how important it was for a theory to *restrict* the observations that one might anticipate making. Man, what a crock of s*** that was…)

[🙂]

66. **M**
January 13, 2007

wow, what you say about extra dimensions is true, and probably their connection with strings also is the main reason why these ideas attracted so much attention. Experiments will tell if it was worthy.

I prefer to look at physics rather than at physicists: recent decades will be likely remembered for the following fundamental discoveries: the cosmological constant, inflationary cosmology and structure formation, solar neutrino oscillations, atmospheric neutrino oscillations. Strings played no role there.

67. **Kyrie**
January 14, 2007

> recent decades will be likely remembered for the following fundamental discoveries: the cosmological constant, inflationary cosmology and structure formation, solar neutrino oscillations, atmospheric neutrino oscillations

I would add to this list the definitive observational proof of the existence of dark matter. And maybe also the experimental proof of the CKM model of CP violation.

68. **M**
January 19, 2007

I hope that next years will be the crucial ones for both topics.
Dark Matter could be seen in direct detection experiments and produced at LHC.
The second topic you suggest could be part of a more general issue: since 30 years theorists think that the SM must be replaced at the weak scale by some new physics that solves the hierarchy problem, but since 30 years experiments (colliders, CP,…) fail to find it. If this negative trend continues at LHC, I would consider it as a fundamental discovery, more fundamental than e.g. supersymmetry. But I don't know how this could be explained to funding agencies…

69. **Ari Heikkinen**
January 23, 2007

"cosmic superstrings"

So any "fluctuation" whatsoever on whatever measuring device and it's evidence for cosmic superstrings? Come on, even Hoagland would have more reasoning for his theories than that.. 🙂

Various Stuff

January 11, 2007
Categories: Uncategorized

This week I'm getting ready for the start next week of the spring semester. I'll be teaching the second half of our graduate course on Lie Groups and Representations, something I also did a few years ago, at which point I wrote up some notes and put them on-line. This year, since the students covered somewhat different material during the first semester, I'll be covering some different topics, hoping to both write up some notes on the new topics, and improve the older notes. We'll see how much of that I have time for. Throughout academia, others are also trying to figure out what they'll be talking about during the new term, for example see Clifford Johnson's recent posting. He's teaching a course on string theory, something about which he seems to be a tad bit defensive. Actually his outline syllabus doesn't really indicate what he will cover, referring to aspects of perturbative and non-perturbative string theory, gravity and quantum field theory, which pretty much includes most of modern physics. Perhaps, like some of the rest of us, he hasn't quite yet decided what exactly to talk about…

A future course that some people might be interested in is a summer school to take place in Seattle on Lattice QCD and its Applications.

An American Physics Student in England has a review of QFT textbooks for beginners. He neglects to mention a couple of my favorites (maybe just because they are ones I learned from during my student days): *Quantum Field Theory* by Itzykson and Zuber, and Pierre Ramond's *Field Theory: A Modern Primer*.

I saw the above link first at Dorigo Tommaso's blog, which also contains all sorts of news about interesting results coming out of the Tevatron, including a new, more accurate value of the W-mass. See for instance here, here, here, and here. About the new W-mass measurement, there's also a Fermilab press release, and an article in Nature. It may yet turn out that the Tevatron is the place where the Higgs is first seen.

Also in Nature is an interesting article by Frank Wilczek about recent lattice QCD results showing that QCD leads to a nucleon-nucleon potential with hard-core repulsion.

Notes from the talks at last week's Gottingen Winterschule on Geometric Langlands are now available.

From Peter Teichner's web-site, a new preprint by him, Hohnold and Stolz describing 8 different models for real K-theory, one of which is in terms of supersymmetric quantum mechanics. The paper is dedicated to the memory of Raoul Bott, whose periodicity theorem is a large part of this story.

From Michael Douglas's web-site, there are slides from his recent colloquium talk here at Columbia on Supersymmetric Gauge Theory: an overview. He also has a new preprint out with Denef and Kachru entitled Physics of String Flux Compactifications. The autthors go over the arguments for the Landscape and devote significant space to discussing whether or not string theory is testable. They explain why hopes that one could use a statistical, anthropic argument to predict whether supersymmetry breaking happens at low or high scales haven't worked out. There's a somewhat mystifying claim that "in fact string/M-theory does predict a definite distribution of gauge theory and matter contents", referring to various papers which don't contain anything like a definite string/M-theory prediction of such a distribution.

As for the testability of string theory, the authors first note that while there are all sorts of exotic phenomena that one might imagine finding that are consistent with string theory, none of them are required by string theory, so:

Thus, while string theory can offer experimentalists many exciting possibilities, there is little in the way of guarantees, nor any clear way for such searches to falsify the theory.

They then go on to give what they see as four possibilities for testability:

1. "Swampland" arguments showing that string theory can't possibly lead to a low energy effective theory that agrees with what we see. Unfortunately, there seems to be no such plausible argument, with all arguments of this kind so far only ruling out string theory as a source for very different physics than what we observe.

2. String theory must be true because there is no other possible theory of quantum gravity. They completely ignore LQG, but do admit that "one should not take this too seriously until it can be proven that alternatives do not exist", mentioning the possibility of finiteness of N=8 supergravity.

3. Maybe the LHC will discover new physics that clearly is the result of a string theory compactification.

4. Maybe they will be able to make statistical predictions using the landscape.

These seem to me extremely weak and problematic arguments. 3 appears to be little more than wishful thinking that a miracle will happen and save the day, and all efforts over the last few years to pursue 4 seem to lead to insuperable difficulties for very fundamental reasons. In the end, the authors acknowledge this, writing "ultimately convincing evidence for string theory will have to come from observing some sort of exotic physics", and putting their hopes in string cosmology, especially the hope of seeing networks of cosmic superstrings or signals in the CMB corresponding to non-linearities in the DBI action.

After this dismal summary of the situation and of prospects for the future, the authors decide to end with conclusions more or less directly opposite to the ones their arguments naturally lead to:

We conclude by noting that while the present situation is not very satisfactory, there is every reason to be optimistic… There are many well-motivated directions for improving the situation, and good reasons to believe that substantial progress will be made in the future.

Update: One more. There will be a public debate over the anthropic principle later this month, involving David Gross, Lenny Susskind, and others. More information here.

Comments

1. **A.J.**
   January 11, 2007

   American Physics Student in England also forgot to mention Zinn-Justins's *Quantum Field Theory & Critical Phenomena", which about as good a QFT textbook as currently exists. I always appreciated the fact that they explain Wilson's perspective on effective field theory in the introduction, before doing even a gaussian integral.*

2. **Mark**
   January 11, 2007

   Hi Peter,

   Whats your favorite reference for Lie algebras? I remember you once mentioned Bourbaki.

3. **Peter Woit**

January 11, 2007

Mark,

Kind of depends what you want to do with them. Bourbaki is unusually readable, but better might be a standard mathematical textbook, best-known algebraic one may be Humphreys. More for geometers, there's Fulton and Harris. If you're just learning the subject, a concise discussion is in the first part of the short book by Carter, Segal and McDonald. For physicists, who don't care about arbitrary fields and really want to compute things, there are lots of references, and they might find Fulton and Harris readable. The beginning chapter on Lie algebras by Jurgen Fuchs in his book Affine Lie Algebras and Quantum Groups is nice, lots of computational details in Howard Georgi's book.

4. **Thomas Love**
January 11, 2007

I cut my teeth on Robert Hermann's Lie Groups for Physicists (which I really cannot recommend) and Robert Gilmore's Lie Groups, Lie Algebras and Some of their applications (now from Dover). Gilmore has a new version at:

http://www.physics.drexel.edu/~bob/LieGroups.html

Thanks, Peter, for making your notes available. I'll read them soon. My major complaint about most books on Lie algebras is that they deal with matrix reps and ignore reps via differential operators as is done is courses in differential geometry.

5. **Chris W.**
January 11, 2007

In line with the variety of items above, I thought I would pass along a blog posting about a new open access journal, mentioned in a comment on CV:

PhysMath Central

(..open access, but *commercial*.)

6. **Thomas Larsson**
January 12, 2007

As a student, I preferred Ramond over Itzykson and Zuber because it wasn't so prohibitively big. IZ is a better reference, though.

A second drawback of IZ, which I have become increasingly aware of during recent years, is that small print is unfriendly to middle-aged eyes.

7. **Peter Orland**
January 12, 2007

The book by Sattinger and Weaver on Lie groups and algebras is not bad. It's pretty rigorous, and much of it is in language physicists can understand. The best thing about it is that it's short. The chapters on classification of semi-simple Lie algebras are pretty good (except the proof of Engel's theorem is impenetrable – I had to find that someplace else).

I also second Peter W.'s endorsement of Segal's lectures, in the book with Carter and MacDonald.

8. **amused**
January 12, 2007

A minor quibble with Wilczek's nice article: When he mentions the limitations of the calculation of Aoki & co (e.g. quark masses larger than their physical values) he neglects to mention that the calculation is also "quenched", i.e. fermion determinant in the path integral is set equal to 1 and hence the effects of vacuum polarisation by quark-antiquark pairs are ignored. I've no idea to what extent this affects the result; presumably its qualitative features – in particular the hard core repulsion – would remain, but quantitatively the inter-nucleon potential could be quite different. At any rate, going from quenched to full (unquenched) QCD certainly affects the results for hadron masses, reducing the discrepancies with experiment from ~10-30% to ~1-2% in the high-precision lattice calculations that Wilczek mentions (but surprisingly doesn't give a reference to). The problem with having to do calculations at larger quark masses is not so serious since one can extrapolate to physical values (via chiral perturbation theory), but there is no similar solution for unquenching – you just have to get a powerful enough computer and do unquenched calculations from the beginning. So this seems a more serious challenge as far as removing the limitations of Aoki & co's work goes.

Btw Peter, do you ever think about returning to the lattice? There are interesting things going on these days, including mathematical things. E.g. it is now understood how to formulate fermions on the lattice with chiral symmetry, exact chiral zero-modes etc, so the notion of index makes sense and one can ask whether it coincides with the (your!) lattice topological charge of the background gauge field, as it should (for sufficiently smooth fields) according to index theorem. Numerical studies indicate that this is the case, but there is no analytical proof so far. So if you ever feel like returning to this topic there's a research problem for you right there.

9. **liealgebras**
January 12, 2007

Jacobson's old book on Lie algebras is quite readable and very cheap (its a Dover). It also goes into more depth than most the current standards (it's an algebra textbook not a geometry textbook).

10. **Peter Woit**
January 12, 2007

amused,

I actually got interest in the things I've been working on for many years when I started thinking about chiral lattice fermions. It has always seemed to me that which lattice gauge theory does a beautiful job of taking advantage of the geometric nature of gauge fields, the discretization of fermions completely ignores the geometry of spinors, and maybe there is a better way of doing things.

Anyway, this led me to other questions and problems I'm still struggling with, going back to the lattice problem to see if anything I've learned about spinor geometry can be useful there is on my ToDo list, something I should try and find time to work on.

11. **a**

January 12, 2007

Off topic, but talking about books, does anyone know a good introduction to differential geometry? Last time I looked at the first chapter of Do Carmeno, it seemed to have bunch of errors. Spivak is very nice, but perhaps a bit thick. Any other suggestions?

12. **amused**
January 12, 2007

Peter,

Sorry if this is getting off-topic, but chiral fermions on the lattice are in pretty good shape these days in the overlap/Ginsparg-Wilson formulation. It certainly doesn't have the obvious beauty and naturalness of the lattice gauge field formulation, but on the other hand, given the obsticle of the Nielsen-Ninomiya no-go theroem, it might be as good as it gets. At any rate, many things work out as they should, including chiral gauge anomalies and their relation to families index theory. People haven't tried to describe general aspects of spinor geometry in this formulation though – it's all been usual spinors coupled to gauge fields on flat spacetime so far.

13. **Peter Orland**
January 12, 2007

Peter,

As amused said, the problem is solved for QCD. The status of spectra and some other numbers of QCD is actually looking very good.

On the other hand, regularizing the standard model with the lattice is in poor shape. Genuine chiral fermions are a problem. The Nielsen-Ninomya theorem is a potential problem with any cut-off, not just the lattice (in fact, Nielsen and Ninomiya did not use a lattice). It may be that this problem points to some new physics beyond the standard model.

14. **anonymous**
January 13, 2007

Peter (Orland),

Can you elaborate on that? I was under the impression that Nielsen-Ninomiya comes from some index theorem based on putting the theory on a torus. So it looks to me like it has more to do with IR regulation than with UV regulation. So couldn't you just take the finite-volume regulator to be on some other topology with a different value of the characteristic class appearing in the index theorem, and take the infinite volume limit of that?

(I realize I might have some major misunderstanding here, since I've only heard the Nielsen-Ninomiya theorem discussed briefly a couple of times.)

15. **Jimbo**
January 13, 2007

Neither American Student in England, nor any of the respondees has mentioned the new player in town, by Mark Srednicki, chair of the physics dept. at UCSB: http://www.physics.ucsb.edu/~mark/qft.html

In my humble opinion, it will shortly blow Peskin & Schroeder Out of the proverbial water. Our QFT instructor, immediately upon discovering Srednicki's new text, not even due out until Feb., dropped P&S immediately in favor of it. Check it out !

16. **amused**
January 13, 2007

Peter Orland,

It is true that the lattice formulation of the Standard Model hasn't been accomplished yet, but nevertheless I still consider the lattice formulation of chiral gauge theories (not just vector ones like QCD) to be in relatively good shape. The fundamental obsticle represented by the N.-N. no-go theorem has been overcome by the overlap/Ginsparg-Wilson approach, and it has been shown to correctly reproduce the anomaly structure of the continuum formulation. What remains is to construct the chiral fermion measure (or "perfect phase" in the overlap terminology) such that the lattice theory is gauge invariant when the usual (continuum) anomaly cancellation conditions are satisfied. This has already been done for gauge group U(1) but not yet for the nonabelian cases (including Standard Model). There are no fundamental obsticles to accomplishing this; it is "just" a hard technical problem. The prospects of this being sorted out any time soon are bleak though since no one seems to be working on it. (Some of us would like to but then we'ld soon be out of a job…)

17. **Peter Orland**
January 13, 2007

Amused,

I am not so sure that the non-Abelian case is just a technical problem. Also the U(1) solution is very complicated, and it is not so clear that it is satisfactory.

18. **A Rivero**
January 13, 2007

I-Z was my textbook, and I do not like it. Actually I wonder if someone learning mainly from it got to do research on QFT. It was a transition book, for a generation who was too late to use the Bjorken et al books and too early to enjoy the new books.

19. **Chris Oakley**
January 13, 2007

Re: Srednicki, I too thought it was better than average, and it is nice to find a QFT reference that is more the thoughtful, useful pedagogical text and less of the cookbook.

Having said that, I did feel the need to send an e-mail to the author pointing out a problem with the common SL(2,C) conventions used in the supersymmetry community. As in some other areas, I seem to be the only one who worries about these things, but this particular problem cost me at least a day of my not-very-valuable time when I was a graduate student.

20. **amused**
January 13, 2007

Peter O.,

"I am not so sure that the non-Abelian case is just a technical problem."

All I can say is that it's my strong impression after having worked on it and tallked to others who have worked on it.

"Also the U(1) solution is very complicated, and it is not so clear that it is satisfactory"

If you have found something unsatisfactory in Martin Luscher's argument I suggest you write and tell him about it. (It seemed fine to me when I studied it.)

21. **Peter Orland**
January 13, 2007

amused,

Just so I understand what you are saying….

Are you stating that it has been resolved that chiral fermions, e.g. neutrinos have a good lattice formulation?

22. **Peter Orland**
January 13, 2007

… and that everything works when coupling said particles to gauge fields?

23. **Peter Orland**
January 13, 2007

I am looking over Luescher's papers to say exactly what he says.

24. **amused**
January 13, 2007

Peter O.,
Before coupling to gauge fields, chiral fermions such as neutrinos do indeed have a good lattice formulation now. And they continue to do so if you couple to U(1) gauge fields in an anomaly-free representation. For coupling to nonabelian gauge fields the situation is still unresolved at the nonperturbative level. (At the perturbative level things have in fact been resolved and everthing is fine.) As I mentioned above, the remaining problem in this case is to construct a chiral fermion measure such that the theory is gauge-invariant when the fermions live in a representation of the gauge group which satisfies the usual (continuum) anomaly cancellation conditions. (There is a priori an arbitriness in the phase of the chiral fermion measure, and the challenge is to determine a suitable phase factor (as a function of the lattice gauge field) such that the chiral fermion determinant becomes gauge invariant.) Actually there has already been substantial progress on this for electroweak gauge group U(1)xSU(2) but not yet a complete solution. It's a difficult technical problem – as you mentioned, the U(1) case itself is already pretty complicated – but i don't see any fundamental obsticle to things working out.

This is getting quite off-topic from Peter W.'s original post, I hope we aren't testing his patience too much… Anyway I have to go to bed now (it's 4am here) so won't be able to get back to this until tomorrow…

25. **Louise**
January 15, 2007

Dr Woit, you may enjoy Sharon Begley's column in the WSJ January 5: "Giant Swiss Collider May Reveal Secrets About Origins of Mass"

"If the LHC creates superpartners, the results will be spun as fiercely as a political campaign debate. String theory, which asserts that the basic constituents of matter are tiny vibrating strings that exist in 11 or 12 dimensions, requires supersymmetry. Thus stringsters may hail signs of superpartners as their long-sought vindication. But rival theories posit superpartners too, so if the LHC finds them it wouldn't uniquely support string theory."

"Supersymnetry is a vital part of string theory, so if the LHC doesn't find it, that would argue strongly against string theory," says physicist Lawrence Krauss of Case Western Reserve University, Cleveland. "If it is observed, you can say that string theory has not been disproved, but not that it has been validated."

The article also contains quotes by Lee Smolin. I can post the whole thing if you wish.

26. **lostsoul Ph. D.**
January 15, 2007

More or less off topic – Anjana Ahuja (the London Times 15th Jan.) flags up NEW (and Lubos' Reference Frame) as an 'exciting arena for gladitorial spats … over string theory'. Somehow, she manages to make the Sage of Harvard appear to be the more reasonable of the combatants: his mildest ever invective ('crackpot') is paired with what must ahve been Peter Woit at the end of a very hard day.

27. **King Ray**
January 15, 2007

Chris Oakley: thanks for posting a link to your explanation. I will have to think about this some more. Do you have a link or reference for Penrose's conventions? I don't have his book handy. I want to make sure that my conventions are consistent.

If we take the contravariant symbol to be the inverse of the covariant symbol (a la the metric in GR), which would make them negatives of each other when viewed as matrices, we have eps^(ab)*eps_(bc) = delta^a_b. Then we have eps^(ac)*eps_(cd)*eps^(bd)=delta^a_d*eps^(bd)=eps^(ba)=-eps^(ab). Thus it doesn't seem that having them be inverses of each other and obtaining each other by raising and lowering indices are consistent, assuming raising and lowering are done with the second index. I hope my notation is clear. contravariant indices are preceded by a ^, and covariant indices by a _. I think if you had them not be inverses of each other, but be the same then you would get eps^(ab) by raising the indices of eps_(ab). Is that what Penrose does?

28. **Peter Woit**
January 15, 2007

For those wondering what "lost soul" is referring to, see

http://www.timesonline.co.uk/article/0,,20909-2547760,00.html

The quote is from here

http://www.math.columbia.edu/~woit/wordpress/?p=490

and taken out of context. It was part of a response to commenters who wanted me to remove links to Lubos's blog because of the racist nonsense he spews.

29. **Chris Oakley**
January 15, 2007

I think that String Theorists should be grateful to Lubos for his efforts in keeping public interest alive in a subject that might otherwise die for lack of experimental support. It would appear that many of them are.

King Ray: I will try and keep this short, but Penrose's handling of SL(2,C) – which is of course a pre-requisite to twistors – I got from lecture notes, not a text book, although I am sure that there will be printed references somewhere. The notes on my web site spell out my understanding of this matter. The covariant-contravariant relationship does not require the ε identities as you give them – it only requires that the transformations under group action are inverses of each other. Your identities in fact almost apply with Penrose's conventions, apart from a few sign differences. Yes, obtaining ε^{AB} by raising the indices of ε_{AB} is the thing that matters, and it is indeed what Penrose's conventions guarantee. My point is that if one does not have the same rule for raising/lowering indices of preserved tensors as other tensors, then all hell will break loose.

30. **King Ray**
January 15, 2007

Chris, if the covariant and contravariant 2D Levi-Civita symbols are not inverses of each other, and you use the second index of each to raise or lower indices, then if you raise a spinor's index and then lower it you get back the same spinor. If your LC symbols are not inverses of each other (i.e., are the same), then you get the negative of the spinor you started with (like a complex structure squared acting on the spinor since eps^2=-I). I think the way others are doing it is ok so long as you know that when you raise the indices of the covariant LC symbol you get the opposite of the contravariant LC symbol. Maybe they should have two different symbols. Maybe all could be made good if you raise with one index and lower with the other and make covariant and contravariant LC symbols equal? I'll have to see what Penrose does.

31. **King Ray**
January 16, 2007

I looked up Penrose's raising and lowering conventions in Penrose and Rindler, Spinors and space-time, Vol. I, and Penrose has covariant and contravariant LC symbols the same, and lowers with the 1st index and raises with the 2nd. I guess a good mnemonic would be L1R2.

32. **King Ray**
January 16, 2007

Chris, thanks for bringing up this issue. I may adopt Penrose's convention. The only problem I have with it is remembering which issue to raise with and which to lower with, which is why I always preferred to raise and lower with the 2nd index.

In summary, I think Srednicki's conventions are consistent, you just have to keep in mind that eps contravariant is not gotten from raising the indices of eps covariant. I can't see any problems if you keep that straight.

It must be 20+ years since I looked at Penrose's book, so thanks for getting me to do that. I also saw a nice set of identities in there that are all worked out. Penrose and Rindler also have a nice appendix on spinors in n dimensions in Vol. II.

33. **Chris Oakley**
January 16, 2007

King Ray – right. But one must have ε_{AB} = ε^{AB} or the bad things alluded to in my note will happen. See also here, eqs. 3.31-3.35.

34. **Chris Oakley**
January 16, 2007

> In summary, I think Srednicki's conventions are consistent, you just have to keep in mind that eps contravariant is not gotten from raising the indices of eps covariant. I can't see any problems if you keep that straight.

Yes, but that means that ε_{AB} is not a tensor. I would definitely call that a problem. It means that every time you raise or lower its indices you have to remember to change the sign as well.
PS: If you are who I think you are (I have only just twigged), then I apologise for a somewhat brusque reply to an earlier post from me. Peter, fortunately, deleted it.

35. **King Ray**
January 16, 2007

Chris,

Thanks for the link to your document, I will look at it more carefully later.

I agree with you 100% in that it is an abuse of notation to use epsilon for both contravariant and covariant LC symbols if you use Srednicki-like raising and lowering. Like I said above, they really should have different symbols in that case, but using the same symbol is ok if you don't assume that they are related by raising and lowering indices. Penrose's convention is definitely less confusing as far as that goes, although harder to remember since you raise and lower with different indices.

I did not see the post you referred to, so no harm was done. I am not sure who you think I am, but I am probably not that person unless Peter told you who I am. I prefer to be anonymous, one reason for which is that I have many friends and acquaintances in the string community. In the past, I have gone on long walks with Penrose and Newman on a number of occasions.

36. **King Ray**

January 16, 2007

Also, yes, you must have eps^(AB) = eps_(AB) in Penrose's convention as I alluded to previously. Otherwise raising and then lowering gives you a minus sign as I stated above.

37. **peter_pan**
January 17, 2007

Peter,
I don't follow your blog, so there's no way that I know whether you know or you don't know about the following workshop in KITP. with both string theorists and numerical GR and looooooooooooooooooooooooooooopists.
Here is the link:
http://online.kitp.ucsb.edu/online/singular_m07/

News From All Over

January 16, 2007
Categories: Uncategorized

Last summer the entire editorial board of the prestigious journal Topology resigned, in protest over the high prices that Elsevier was charging. It was announced today that a new journal called the Journal of Topology is being launched by many of the same people. It will be published by the London Mathematical Society, printed and distributed by Oxford University Press, and the first issue should appear in January 2008.

There's a recent report from HEPAP evaluating how far along the field is towards reaching certain set "long-term goals" (where "long-term" here is not a very long time-scale).

The New York Times Science Times section has a new columnist, John Tierney. Tierney has been with the paper for a long time, writing columns about New York and on the Op-Ed page, typically from a consistently Libertarian perspective. He also has a blog (where he promises to "rethink conventional wisdom about science and society") and explains his conversion to science journalism by writing that he "always wanted to be a scientist but went into journalism because its peer-review process was a great deal easier to sneak through."

The Templeton-funded magazine Science and Spirit, dedicated to bringing science and religion together, has a new issue out. It contains an interview with Max Tegmark about the Foundational Questions Institute. There's also an article called The World on a String about the anthropic landscape and the problems with string theory. Susskind and Wilczek are quoted saying positive things about the multiverse, Krauss and I on the other side of the question. Finally there's a review of my book by David Minot Weld with the title Stringing Us Along. It's pretty accurate, although it's not true that the book describes string theory as "totally without scientific merit" (that would be the string theory anthropic landscape…). Weld appears to be the son of ex-Massachusetts governor William Weld.

The Templeton foundation has a new web-site, and has announced a moratorium on new proposals over the next few months while they change their grant-making process. The web-site gives various information about the grants they have made in the past. I hadn't realized that they make grants in mathematics. There was one last summer for about $16,000 to W. Hugh Woodin for research in mathematical logic.

New institutes devoted to "foundations" appear to be popular, with Templeton Prize winner Paul Davies starting up one at Arizona State University to be called *Beyond: Institute for Fundamental Concepts in Science*. This was announced by ASU president Michael Crow, who before he left for ASU was Executive Vice Provost here at Columbia and in charge of overseeing research and various "strategic initiatives".

In the bookstore this past weekend I saw a new glossy book from National Geographic called Theories For Everything: An Illustrated History of Science. Lots about physics, but as far as I could tell, no mention of the Standard Model, Glashow, Weinberg, QCD, etc, but a whole page about string theory. In their version of physics history, one skips from Feynman to black holes, Hawking and string theory.

The coverage of string theory in popular media these days is decidedly mixed. A couple weeks ago I attended a performance of the play "Strings" by Carole Bugge, for a review, see here. It wasn't bad as a play, and reminded me of another similar one from a couple years back, String Fever. But I'm kind of dubious that this sort of thing actually communicates any accurate understanding of physics to anyone. The play deals with themes of adultery, loss and 9/11 with a plot based on the train ride supposedly during which Steinhardt and Turok came up with the ekpyrotic scenario (the play's train ride is jazzed up with a woman cosmologist, who is sleeping with the two other physicists). Unfortunately the playwright's understanding of all this seems to be based on little more than watching a British TV show on the topic. In the pamphlet distributed to the audience various popular books on string theory and physics are recommended, together with much more dubious sources, like the film "What the Bleep Do We Know?"

There does seem to be a much more skeptical take on string theory getting out into the media these days. A recent episode of Numb3rs featured Judd Hirsch telling his genius mathematician son Charlie that string theory is "bogus", more or less the same insight into the universe as that of late sixties hippies, everything is "vibes". String theorist Larry has been shot off into earth orbit for some reason.

As mentioned here and at Cosmic Variance, the New Yorker recently actually ran a cartoon about the string theory controversy. If that's not an indication that something has made it into the zeitgeist, I don't know what is. Besides the New Yorker, string theory features in Zippy the Pinhead and recent Doonesbury cartoons, as well as one from Rodrigo Alonso entitled Pulling Strings that he sent me recently.

Update: What is it with Harvard string theorists and climate change?

Comments

1. **Peter Orland**
   January 16, 2007

   As I skimmed Tegmark's interview, I learned that he hopes his foundation will merit new Einstein's. It reminds me a bit of Smolin's question as to why there are no new Einsteins, indicating that the current scientific environment discourages ultra-creative people.

   What strikes me as misguided about all this is that the real Einstein met obstacles every inch of the way until 1905 (when he published all his results in a mainstream journal). I don't see today's scientific environment as being worse than 1905's. I think it's much better. In 1905, many people who did science had to be independently wealthy (not most of the people we read about – they were lucky enough to have their talents recognized).

   I also wonder how these people would react if a new Einstein finds some breakthrough in an area of physics other than quantum gravity or multiverses?

2. **island**
   January 16, 2007

   Davies is an atheist… just to clear the air of any possible misunderstanding.

   Interesting factoid:

Davies has pre-released and then withdrawn the book that you reviewed Peter, on two separate occasions now, in the U.S.

It is now scheduled for its third release sometime in mid-April under then new title of, **Cosmic Jackpot: Why Our Universe Is Just Right for Life:** instead of **The Goldilocks Enigma**, like it was in the U.K. and elsewhere.

http://www.amazon.com/Cosmic-Jackpot-Universe-Just-Right/dp/0618592261/sr=1-1/qid=1167149428/ref=pd_bbs_1/002-8082944-3852846?ie=UTF8&s=books

3. **hack**
   January 16, 2007

   Thompson is indeed a string theorist from Harvard, but a cursory glance does not reveal any obvious tendencies towards Lubos style crackpotism on the issue of climate change. It would be interesting to hear what he has to say on the issue.

4. **Jason**
   January 17, 2007

   On climate change I don't see how Lubos is a crackpot. He clearly knows more about it than Al Gore for example.

5. **Anti-Lubos**
   January 17, 2007

   On climate change I don't see how Lubos is a crackpot.

   Hahahahahaha. Hahahahahahahahahaha. Hahahahaha.

6. **Ari Heikkinen**
   January 17, 2007

   "On climate change I don't see how Lubos is a crackpot."

   Ok, seriously, that must be the funniest thing I've ever read here. I'm actually laughing. 🙂

   Would actually be interesting to hear what Thompson has to say about the climate change considering in another Harvard string theorist's dimension greenhouse effect apparently don't exist at all while that same person apparently considers himself the allknowing expert on the subject dismissing actual experts in the field as charlatans and anyone who disagrees with his conclusions a crackpot.

   Surely if Thompson's that bad I'll never take anything anyone from Harvard says seriously no matter what degrees they might have.

7. **Peter Woit**
   January 17, 2007

   I don't know anything about Thompson, and have no reason to believe that he has anything other than sensible and well-informed views on climate change. It just seemed remarkable that there's this much interest in climate change in the fairly small community of Harvard string theorists.

8. **Ari Heikkinen**
   January 17, 2007

   I actually have a question about this:

   Can string theory actually be used to (or tell) anything about the earth climate, perhaps temperature changes as correlated to measured amounts of greenhouse gases in the atmosphere considering known amounts of carbon dioxide emitted due to burning of fossil fuels worldwide (according to EIA in the US that was 27000 million metric tons of carbon dioxide worldwide total in 2004) ?

9. **Peter Woit**
   January 17, 2007

   Ari,

   No.

10. **Chris W.**
    January 17, 2007

    Looks like some young string theorists are keeping their options open, and working on something whose relevance is unquestionable. More power to 'em…

    (LM, on the other hand, is probably consulting for Exxon-Mobil.)

11. **anonymous**
    January 17, 2007

    Ari and Peter,

    Dave is a very bright guy who had an interest in environmental issues long before coming to Harvard. While I can't speak for him, I do know that he is a very reasonable fellow and, as far as I could tell, has a sensible take on climate change.

12. **Clark**
    January 17, 2007

    Cal State's eccentric Professor Larry Fleinhardt (Peter MacNicol) is moonlighting as senior presidential advisor on the new season of 24, which is also a Numb3r.

13. **Robert Musil**
    January 17, 2007

    I don't think a diversion here into climate warming is a good idea. And I don't think calling anyone a "crackpot" is going to be any more effective a rhetorical technique for commenters on this blog than it has been for Lubos. That's the kind of weapon that mostly backfires.

For what's its worth, Harvard has pretty high profile people on various sides of the climate warming issue. For example, Willie Soon and Sallie Baliunas, both at Harvard Smithsonian, have published papers suggesting that solar variability is more strongly correlated with variations in air temperature than any other factor, even carbon dioxide levels. Obviously, this is not a view held by a majority of climate scientists. But these people are far from "crackpots."

Here's a Wikipedia article with some links:

http://en.wikipedia.org/wiki/Sallie_Baliunas

Of course, I don't want to be construed as endorsing her views. But I do want to be construed as opposing calling people "crackpots" without justification far exceeding what Lubos or his critics have mustered to date and further opposing bringing up global warming here in the first place.

14. **Aaron Bergman**
January 17, 2007

Soon and Baliunas? Seriously?

15. **Peter Woit**
January 17, 2007

Please, all. The global warming debate is definitely off-topic here, unless you have a specific insight into the interest certain string theorists are taking in it.

From what I can tell, there's a long list of other blogs where those interested in this debate can discuss it, sometimes even with people who actually know something about the subject. Please take such discussion there.

16. **Jonathan Vos Post**
January 17, 2007

Close, but no cigar, Clark.

The school on the hit show where professors include Charlie Eppes (David Krumholtz), Dr. Larry Fleinhardt (Peter MacNicol), and Amita Ramanjuan (Navi Rawat), is 'Cal-Sci' (based on Caltech). Caltech's IP attornies were ready to approve this; but CBS legal staff had last-minute cold feet and invented 'Cal-Sci' which, in any case, is not "Cal State." I'm quite clear on this, as I was a Physics-turned-Math graduate of Caltech, one of my professors was NUMB3RS's Math Advisor Gary Lorden, and my son is about to graduate with a double B.S. in Math and Computer Science from Cal State L.A.

Oddly enough, in real life, Judd Hirsch (who plays Alan Epps) has a bachelor's degree in Physics! At least, that's what he told me…

17. **nontrad**
January 17, 2007

Yikes,

In an attempt to get back on topic, (and this may be off topic…sort of reaching out on a limb here) did anyone read Banks' most recent lanl posting http://arxiv.org/abs/hep-th/0701146 discussing entropy in the early universe? I ask since this paper repeatedly discusses various 'beliefs' and 'religions', which seems relevant to Peter's post above.

Now, I only ask since I do and have, to some degree, followed Bank's work for reasons related to some of his early work in nuclear physics and some vaguely related matters…

Prior to reading / skimming Bank's recent article (which I went over very early this morning), I was unaware that a certain someone posted on their blog about this article. So this might be hot water to raise this topic. And forgive me if I'm stirring the pot here by raising this subject: That's not my intention.

Instead, in reading this article I was reminded of the time when (over a decade ago) Coleman, Polchinski, Susskind and others wrote articles about the 'Attack of the Giant Wormholes' and 'Revenge of the Giant Wormholes' etc in NPB. A time when…well things didn't look promising at all.

Even Banks states in his article that there are concerns about 'calculating the number of angels that dance on the head of a pin' in these matters. By comparison, the recent HEPAP report seems far more cogent, sober, constrained, rational and realistic…

Aren't these two very very different in nature?

18. **woit**
January 17, 2007

nontrad,

I also took a look at the article by Banks, but decided it wasn't worth the time needed to try and figure out exactly what he was talking about, life is too short. This kind of absurdly speculative discussion of anthropic/cosmological topics seems to me highly unlikely to ever lead anywhere, and it's questionable whether it's science at all. I also noticed that at times Banks appeared to be to some extent making fun of the subject, which is encouraging, because the whole thing does seem a bit nuts.

Anyway, sorry but I can't help you understand this paper. There's always Lubos, but his endless posting about this appears to just be the usual stew of misinformation about physics and claims that anyone who disagrees with him is stupid. I wonder what Banks thinks about how his student turned out…

19. **Peter Orland**
January 18, 2007

nontrad

Banks did not do nuclear physics in the old days. He worked on lattice gauge theories (and did primarily analytic, not numerical research).
This subject is part of nonperturbative, relativistic quantum field theory.

20. **Steve Myers**
January 18, 2007

I glanced at Banks paper. Immediately I thought of Wells' "Dr. Moreau" & the monkey-man's Big Thinks & Little Thinks. It seems real science has more to do with the little thinks than those Big Ideas.

Does it come to this: Second Law implies no low entropy implies no consciousness implies no me. But I only know that because I am; therefore, I am not if and only if I am. Will that get me a Templeton grant?

21. **CapitalistImperialistPig**
January 18, 2007

Tierney a science columnist? I predict that he will vigorously contest with Gregg Easterbrook for lamest science columnist award – if his political columns provide any hint.

22. **Ari Heikkinen**
January 19, 2007

If there's a transcript or perhaps audio (mp3?) somewhere of that Thompson's talk perhaps post a link here? Thanks.

23. **Ari Heikkinen**
January 19, 2007

"While teaching at Harvard, he developed software that allows blind students to read papers in physics and mathematics. As the lead researcher in Harvard's Alternative Fuel Vehicle Project in 2001, David assembled recommendations that led to the use of environmentallyfriendly biodiesel in all of Harvard's trucks and buses."

I'm sure he's a very reasonable guy based on all this and I acknowledge it's unreasonable to prejudice solely based on another person's musings on the subject.

24. **Maynard Handley**
January 21, 2007

"But I'm kind of dubious that this sort of thing actually communicates any accurate understanding of physics to anyone."

I think most physicists would agree that the way in which "relativity" transmuted to "relativism" in the first half of the 20th C
(a) had buggerall to do with physics
(b) provided apparent support for a set of occasionally dubious ideas, which, right or wrong, should have stood on their own, unbuttressed by big words hijacked from physics.

OK, we all agree with this — and yet we are supposed to cheer, or at least shut up, when other physics ideas (entropy, chaos theory, string theory, [coming up next: phase transitions and spectroscopy]) are misused as hooks for artworks. Personally I think the tolerance of the scientific community for this sort of thing is quite daft.
If you can write a kickass play about phase transitions, well you're obviously some sort of genius and good for you. But if what you are writing is a play about how life is sort of like phase transitions as you misunderstand them, well let's leave it to the theater critics to talk about the play part, but we, the physicists, should be criticizing misunderstood physics wherever we see it not be, with with the subtle condescension of diminished expectations, praising a playwright for using some of our words, and look, he almost got them in the right order, how precious.

Simile and metaphor have their place in art, fine, but we don't tell botany students to make a special point of reading "My love is like a red red rose".

2006 Topcites

January 18, 2007
Categories: Uncategorized

Every year the people running SPIRES put together a list of the most heavily cited papers in their database. I've discussed here in the past the listings for 2003, 2004 and 2005. Up until 2003 these appeared with a discussion by Michael Peskin of many of the papers on the list and their significance, but he hasn't done this for the past couple years. This year, instead of waiting for the SLAC people to put together the list, I decided to generate one myself. I'm not enough of an expert with SPIRES to get it to just give me the list for 2006, but it was an interesting exercise to go through the lists generated by various searches just using their "topcite 50+, topcite 100+, etc…" feature, together with restrictions on dates. I think I was able to compile a complete list of papers with 150 or more citations, and post-1990 papers with 100-150. I was just looking at papers in particle theory (hep-th, hep-ph, hep-lat), not experimental (hep-ex) papers or astrophysics (astro-ph) papers, and was not counting survey articles. I've put the full list on a separate web-page, Most Heavily Cited Theoretical Particle Physics Papers 2006.

There are of course lots of caveats about any conclusions drawn from counting citations, but these numbers do give some solid data about what is going on these days in particle theory research. Two topics from nearly a decade ago continue to dominate these citation counts: AdS/CFT and brane-world models. By far the most heavily cited paper is the original 1997 one by Maldacena (546 citations), and the number of such citations has actually increased significantly over the number in 2004 (451) and 2005 (436). Research into AdS/CFT heavily dominates current particle theory research, but, remarkably, this research has not led to any recent heavily-cited papers on the subject. After a flurry of activity in 1998-2000, the only 21st century paper on the topic with over 100 citations in 2006 is the 2002 paper on pp-wave backgrounds by Berenstein et. al.

Overall, the list provides a very depressing view of the first six years of 21st century theoretical particle physics, with only eight post-2000 papers getting over 100 citations. These break up neatly into 4 hep-th string theory papers and 4 hep-ph phenomenology papers. Besides the 2002 pp-wave paper (hep-th/0202021) the other three string theory papers are all about the landscape, with the KKLT paper (hep-th/0301240) getting by far the most citations (238), followed by hep-th/0105097 (Giddings, Kachru, Polchinski) with 150, and Susskind's hep-th/0302219 ("The Anthropic landscape of string theory") with 109.

The heavily cited phenomenology papers are mostly compilations of theoretical fits to experimental data: hep-ph/0201195 (parton distributions, 193 citations), hep-ph/0405172 (neutrino oscillations, 133 citations), hep-ph/0406184 (CKM matrix, 118) and hep-ph/0506083 (neutrino mass matrix, 103 citations).

While getting this list together, I also accumulated some other data, including lists of recent papers with citation counts in the range of 50-100, and will try and put this together and write about it sometime soon.

Some other data one might want to take a look at is the arXiv monthly count of submissions (I found out about this from a posting at physicsforums). It shows the number of HEP submissions growing until about 2002, more or less flat since then, although each of the last two years have shown slight declines.

I'll avoid the temptation to make extensive editorial comment on the meaning of these numbers, but I find it hard to believe that anyone could claim that they reflect a healthy field. The domination of non-phenomenological particle theory research by landscape studies is especially disturbing.

Comments

1. **Tony Smith**
   January 18, 2007

   Peter said "… a posting at physicsforums). It shows the number of HEP submissions growing until about 2002, more or less flat since then, although each of the last two years have shown slight declines. …".

   2002 was the year that the Cornell arXiv began (or greatly expanded) its blacklisting actvity. A 14 October 2002 e-mail message from register-query@arXiv.org (not addressed to me, but it was forwarded to me which is how I know the quote is accurate) referred to:
   "… a large pool here – typically flagged by reader complaints …".

   Without regard to whether or not any particular person (me or any other) should have been blacklisted,
   it appears that 2002 marked the establishment of a policy of using "reader complaints" as a basis for rejection of submissions from people in the "large pool".

   I am not surprised that the establishment of such a policy coincides with a flattening of the number of submissions, followed by slight declines.

   I (for the record I am not disinterested in such matters) feel that this does indeed not "reflect a healthy field".

   Tony Smith
   http://www.valdostamuseum.org/hamsmith/

2. **anon**
   January 18, 2007

   As far as I know, which is very little, there are a few "referees" working ad-honorem for the arXiv. Submissions are not evaluated in detail but, rather, by their abstracts. The number of preprints actually rejected is allegedly extremely low, and consists of obviously nonsensical submissions. I don't think those very few rejections should have any impact on the global statistics of a given research field.

3. **student**
   January 18, 2007

   Hi Peter,
   "Overall, the list provides a very depressing view of the first six years of 21st century theoretical particle physics, with only eight post-2000 papers getting over 100 citations."

   I'm not sure how you did the search but you somehow missed all these papers with over 100 citations:

   hep-th/0011089, hep-th/0109152, hep-th/0308089,
   hep-th/0308055, hep-th/0309187, hep-th/0404257,
   hep-th/0404116, hep-th/0403047, hep-th/0312171,
   hep-th/0301006, hep-th/0301006, etc.

   So, it's not so depressing 🙂

4. **anon**
January 18, 2007

"…with only eight post-2000 papers getting over 100 citations" during the year 2006.

For example, the famous paper by Maldacena got 546 citations in 2006, and 436 in 2005.

5. **woit**
January 18, 2007

student,

I was talking about number of citations/year, specifically the number in 2006. For instance, the first paper you mention has more than 100 citations, but over more than six years. It looks like during 2006 it had only 7 citations.

6. **matt visser**
January 18, 2007

Peter: When did you compile your data from Spires? — There was an (unannounced) bug with their database search routines for most of Wed 17 Jan which caused large numbers of articles to be unfindable using the standard search interface… As "student" points out:

"I'm not sure how you did the search but you somehow missed all these papers with over 100 citations:

hep-th/0011089, hep-th/0109152, hep-th/0308089,
hep-th/0308055, hep-th/0309187, hep-th/0404257,
hep-th/0404116, hep-th/0403047, hep-th/0312171,
hep-th/0301006, hep-th/0301006, etc."

Spires now seems to be functioning normally, and it would be worth double-checking your searches.

7. **woit**
January 18, 2007

Matt,

I did the searches the evening of the 17th, and a few this morning (the 18th). Earlier in the day I had been trying various things on SPIRES and once or twice noticed some anomalous behavior. But while I was doing the searches, SPIRES seemed to be fine, and all the numbers I was getting appeared to be consistent. In particular I was going through the 2005 topcites list as a starting point, and the 2005-2006 numbers tend to be similar with a couple interesting exceptions (interest in pentaquarks dropped like a stone in 2006…).

Again, this is 100 citations/year, not total citations. If anyone can point to a paper that isn't on my list that got over 100 citations, please let me know.

8. **anonymous**
January 18, 2007

Peter, why do you think lots of recent papers should have over 100 citations in a year in a healthy field? It's not at all clear to me that this is a measure of the health of a field. One could argue the opposite, that it's a sign of a healthy diversity of research topics.

Also, it seems to be a stretch to say that non-phenomenological theory is "dominated" by landscape studies. A quick look at the past week of hep-th shows about 4 or 5 landscape-related papers (things like moduli stabilization) out of 66 total papers. That is hardly "domination."

9. **Yatima**
January 18, 2007

Dont' know whether this link has shown up already, but here goes:

Interview with Lee Smolin in the IEEE Spectrum Magazine:

http://www.spectrum.ieee.org/jan07/4812

Nothing unusual.

10. **woit**
January 18, 2007

anonymous,

Particle theory has always been a field where promising new ideas quickly attract a lot of attention, with lots of people soon writing papers trying to exploit the new idea and see what they can do with it. I just don't see any evidence for the claim that what is going on here is that there are as many promising new ideas as there used to be, but people are not pursuing them, rebelling against particle theory's traditonal trendiness and going for diversity. It seems to me that the evidence is overwhelming that the post 2000 period for particle theory has been characterized by an historically unprecedented dearth of new ideas.

The dominance of landscape studies that I was writing about is not an absolute dominance in terms of numbers of papers being written, but a dominance in the area of new ideas that the community has chosen to pursue. The data about which recent papers are getting most heavily cited is unambiguous.

To put it crudely and bluntly: sure, lots of people are not working on the landscape, far more are working on AdS/CFT or on other topics. But they're not writing papers that many other people think contain promising ideas worth pursuing. The landscape people are the only ones recently to be writing papers that inspire many others to follow them and work on the same thing.

11. **Who**
January 18, 2007

anonymous, here is something to help you decide about the health issue that concerns you
in year 2000 there were **twenty-one** recent string papers (papers that appeared in the past five years 1996-2000) which got 100+ cites in 2000

in year 2006 by Peter's list there were **three** recent string papers (papers that appeared in the past five years 2002-2006) which got 100+ cites in 2006

we are talking healthy field and comparing. You ask:

Peter, why do you think lots of recent papers should have over 100 citations in a year in a healthy field? It's not at all clear to me that this is a measure of the health of a field. One could argue the opposite, that it's a sign of a healthy diversity of research topics…

Maybe it will help make it more clear to you if you compare 2000 with 2006 and see if there is a more healthy diversity of research topics now in 2006 than there was in 2000, since the cite numbers are so much less.
I think if you go back and look at the titles and abstracts of some of those 21 papers you will decide not. But here is the 2000 list if you want to try.

http://www.slac.stanford.edu/library/topcites/top40.2000.shtml

12. **anonymous**
January 18, 2007

It's unfortunate that we don't have a good database of complete particle theory citation counts stretching back to the 40s or earlier; I strongly doubt that it's always the case in any given year that there are lots of good new ideas that get *over 100 citations* in one year, within a year or a few of their publication.

The "100 citations" mark is a bit high to extract meaningful information from, I think. Let's look at one recent paper that "many other people think contain[s] promising ideas worth pursuing": Intriligator, Seiberg, and Shih, hep-th/0602239, already has 52 citations. Surely by any reasonable standard this is a paper that has inspired other people to follow up on it. Other recent trends, just off the top of my head, include the study of energy loss in quark-gluon plasma, integrability in N=4 SYM, and trying to understand general constraints on infrared modifications of gravity. There are plenty of papers on all of these topics, but not necessarily one extremely highly-cited and very recent paper that kicked off the trend. They don't meet your criteria for what is trendy, but I think most people in the field would say they are healthy trends.

For that matter, hep-ph/0507005, "Bootstrapping multi-parton loop amplitudes in QCD" by Bern, Dixon, and Kosower, has 57 citations. That might not match your standards, but 57 citations in a year and a half for a highly technical paper is impressive, and I don't think *anyone* can argue that this is not good and highly relevant physics.

13. **anonymous**
January 18, 2007

Who, your comment appeared as I was writing mine: I don't think it's fair to expect any given year to match the explosion of work that followed AdS/CFT and the beginning of extra-dimensional model building. The fact that a few major new ideas appeared at that time doesn't mean that more recent new ideas are completely useless. For that matter, many of the most important papers in the history of theoretical physics failed to provoke the sort of flurry of new papers that AdS/CFT did. AdS/CFT is a sociological anomaly.

14. **woit**
January 18, 2007

anonymous,

Personally I happen to think the various work you quote is among the best work going on in particle physics, but it's not very promising that it will ever lead to important progress in the field. That's just my opinion, and you're welcome to dismiss it as such, and instead claim that in your opinion the best current work in particle theory is every bit as promising as that of any other period.

But the numbers I'm discussing here are not matters of opinion but solid facts about exactly how particle theorists are voting with their feet about what's an idea worth working on and what isn't. If you have facts to back up your claims, let's hear them. SPIRES data does go quite a ways back, I just noticed that they now have "topcites" lists going all the way back to 1974. If you look at any of the lists from before the last few years you'll see large numbers of recent papers amongst the most heavily cited ones. For something really dramatic, take a look at the first one, 1974. The overall number of papers was much lower, with the most heavily cited papers getting 150-200 citations, unlike the 3-500 we see now, but virtually every paper on that 1974 topcite list is a recent one from the previous six years.

1974 was an unusual year, just after the birth of the standard model, but the data for more than 30 years is there. If you can find some evidence in it to support your claims, lets hear it.

15. **woit**
January 18, 2007

anonymous,

If you think 2000 is an unfair year to pick, pick any other one. For instance pick 1997, right before AdS/CFT. Of the 6 most heavily cited theory papers, guess how many were written during the previous 4 years? ALL of them.

16. **Graduate Student**
January 19, 2007

It seems that the paper by Minahan and Zarembo hep-th/0212208 has around 100 citations in 2006.

17. **woit**
January 19, 2007

Graduate Student,

My SPIRES search gave 94 citations for the Minahan-Zarembo paper in 2006.

18. **Bee**
January 19, 2007

Overall, the list provides a very depressing view of the first six years of 21st century theoretical particle physics,

That's probably because you've excluded astro-ph?

Anyway, looking at your list I should have cited at least half of the papers you listed at one point or the other, not sure if I actually did though. E.g. the PYTHIA, CTEQ, global neutrino fits etc, it's fairly easy to accidentally cite a paper referring to an older version, and those guys usually don't complain. Its still puzzling me why journals have no policy on what has to be cited and what hasn't. Admittedly, I think it would be as important to have a policy on what papers are not to be cited. There are just too many people who cite papers that have only a vague connection to their work, or because they know the authors, or because the author is one of those who will sent you an email saying 'I want to draw your attention to MY VERY INTERSTING PAPER… '

Interesting post, thanks.

Best,

B.

19. **Kasper Olsen**
January 19, 2007

Peter,

your measure seems quite arbitrary; for example, some new ideas generate a lot of new papers since many "relatively simple" things can be worked out in the beginning (like what has happened with AdS/CFT), other ideas take longer to generate new papers since the ideas can involve things which at least **some** people have a harder time understanding (like with the geometric Langlands program).

So,
(1)how would you quantify your claim, that *"but I find it hard to believe that anyone could claim that they [those numbers]reflect a healthy field*.?

(2)Have you compared with another field of research which you consider as being "healthy"?

(3) Have you tried to do the same "statistical" analysis in some sub-field of mathematics, for example? And what would then be your conclusion?

-Kasper

20. **Peter Woit**
January 19, 2007

Bee,

Sure, astrophysics and cosmology are quite healthy subjects, unlike particle physics. While I was making up this list, I was looking at papers that appeared in astro-ph that had large numbers of citations to see if any had much relevance to particle physics. The only candidates were papers about the vacuum energy and things like "phantom energy". It's certainly true that astrophysical observations of a CC pose a serious challenge to fundamental particle physics, but unfortunately I don't think anyone has a promising idea about what to do about his.

Kasper,

I very explicitly quantified my claim: a healthy field is one that is generating significant numbers of promising new ideas, and the best objective standard I can think of is to look at what work people are citing in their current work, and how much of it is recent. If you have a better objective standard, let's hear it and let's see some objective evidence that particle theory is doing well these days.

Your conjecture seems to be that maybe the current situation is different than the one of the past in that now there is plenty of progress and promising ideas out there, but, unlike in the past, few people are working on these ideas. I don't believe this, and don't see any evidence for it. If you have some evidence that this is what is going on, that this explains the difference in numbers between now and the past, let's see the evidence.

I'm a great fan of the recent work on geometric Langlands, but I don't think large numbers of people are going to work on it anytime soon. This is not because it is difficult (which it is), but because it doesn't appear to show any obvious promise as an idea about solving particle physics problems.

If you want an example of a healthy field, as Bee suggests look at astrophysics. I don't have time to generate numbers, but if you look at the 2005 SPIRES topcites list, you'll see that most of the astrophysics papers listed there are recent ones.

I haven't tried to do the same exercise in mathematics, it would be complicated because there are lots of sub-fields, lots of overlap of sub-fields, and much smaller communities working on the same problems. Mathematics definitely has more and less healthy sub-fields; one could probably identify some that are pretty moribund, with few promising recent ideas. The subfields of math that dominate major research universities appear to be quite healthy though, with for instance the solution to the Poincare conjecture and Fermat's Last Theorem in recent years. I know of no reason to suspect that you would see a big difference in the fraction of heavily cited papers that are recent now versus in the past in most subfields of math.

21. **Michael**
January 19, 2007

I recently read that Steven Weinberg considered the most important task ahead in theory research was to understand better the various effective field theories in hadron physics. He felt deep insights – and perhaps important new physics – lay within these structures.

I'm wondering if any theorist(s) are paying much attention to his views (sufficiently more than to AdS/CFT research)?

Michael

22. **Michael**
January 19, 2007

Oh, and by the way, I was curious about another issue:

Besides string theory research, were there any other theory paper(s) [hep-th, hep-ph, hep-lat, or (also) math-ph] that generated interest within the field?

I think the era of effective field theories is beginning to dawn on the research horizon. Any comments to this view?

Michael

23. **Peter Woit**
January 19, 2007

Michael,

Weinberg's views about the importance of effective field theories are the dominant paradigm in particle physics, and this has been true for 30 years or so. There is no danger that these views (or others of Weinberg) are being ignored.

24. **wow**
January 19, 2007

Hi:

Actually, if you look for *why* the so-called landscape papers you mention are well cited, the reason is not much to do with what you regularly bash as "landscape studies."

A few of the citations are from papers about anthropics or the multiverse.

The vast majority are from papers that try to build realistic models: that is papers that are trying to achieve realistic inflation, or particle physics, or both, in a concrete string model. While you can call this "landscape studies," most people would probably call it model building. One can argue about whether one should try to build models of realistic physics all the way from the planck scale down using string theory, but this is a pretty traditional goal of particle physics (starting with GUTs where speculations about ridiculous energy scales became fashionable). Arguably almost all papers on SUSY are motivated by this goal also (with unification being one of the pillars that drives SUSY model building).

25. **Graduate Student**
January 19, 2007

So Minahan and Zarembo are almost there. Nevertheless I think this is an active area of research in hep-th. I don't know if you consider this area to be part of string theory (via AdS/CFT) or QFT…

26. **foo**
January 19, 2007

If you think arXiv rejects papers which are obviously nonsense, search for papers by Mueckenheim. I've spotted a few others, but can't remember right now.

27. **woit**
January 19, 2007

wow,

One of the papers is Susskind's anthropic manifesto. Sure, the citations of the other two often come from "landscape" model-building that doesn't necessarily involve anthropics. But I still think this kind of model-building is highly problematic. The models are inherently complicated and ugly, unmotivated by any physical evidence that points to them, and seem able to reproduce virtually any extension of the SM that one wants, leading to a real question about whether this kind of model-building is normal, falsifiable science at all.

28. **gunpowder&noodles**
January 19, 2007

"There are just too many people who cite papers that have only a vague connection to their work, or because they know the authors, or because the author is one of those who will sent you an email saying 'I want to draw your attention to MY VERY INTERSTING PAPER… '"

I don't see anything particularly harmful in this. I'm sure you know how irritating it is when people who ought to cite you fail to do so, and I just don't want to be one of *those* people who only cite the works of famous people. Look at the cites for Witten's Geometric Langlands papers: most of them just seem to think that having that name on their paper will miraculously render it more likely to be published. That is pathetic, but, again, what harm does it do? There is a lot of talk these days about how there are too many bad papers on the arxiv. By people whose papers are, of course, uniformly excellent.

29. **Michael**
January 20, 2007

Peter,

If this is the case – that Weinberg's view(s) have remained, and continue to remain, the most important paradigm in particle theory – surely his research program is worth persuing for young grad students and postdocs. Why spend so much of ones efforts on 'string theory' and such when (arguably) the most important research direction is staring us in the face? Am I [simply?] missing some information / insight into the field or does such a view seem quite obvious and relevant; I still don't know why SO MANY of the brightest in the field spend time with strings and other highly speculative ideas when EFT may be the most important direction of research for the current generation (to perhaps the next or beyond) of physicists.

Any idea, feedback, etc?

Also, would you happen to know where CP violation studies currently stand? Is CP violation studies directly related to EFT research?

Michael

30. **David I. Santiago**
January 20, 2007

Peter, I am somewhat puzzled that you did not consider theoretical condensed matter physics papers. After all, they are not only an important part of theoretical physics, but they have for a long time been influenced
and have influenced particle theory and quantum and conformal field theory.

31. **woit**
January 20, 2007

David,

One reason for this is that condensed matter theory papers aren't in SPIRES, but more importantly, I just don't know very much about the subject, which seems to be a good reason for me not to write about it.…

Michael,

Most "string theory" papers ARE effective field theory papers. Effective field theory is an extremely general idea which almost everyone is using. The problem in this field is that nobody has a good idea WHICH effective field theory will get us beyond the standard model (which most people believe is an effective field theory itself).

32. **Michael Bacon**
January 20, 2007

Daved Deutsch is joining the debate as well regarding string theory. In the December 9 issue of New Scientist he comments on string theory as follows:

"I think it's unlikely that a research programme of that kind can work. Even if you found the right mathematical object, you probably wouldn't even recognise it because you wouldn't know how it corresponds with the world. For example, if someone had invented quantum theory purely as a mathematical model, how would they ever guess that its multi-valued variables correspond with quantities that we measure with single values? After all, it assigns multiple values to observable quantities simultaneously. I would warn against expecting the answer to come from a new mathematical model. It should be the other way around: first find what you think might be the solution to a problem, then express it as a mathematical model, then test it."

33. **woit**
January 20, 2007

About the Deutsch quote. I haven't looked at the original article, but if he's really refering to string theory, I don't think his argument makes any sense. To defend string theorists, they are not working with "purely mathematical models", with no idea how to connect them to quantities we can measure. Their problem is quite different: when they do try and use string theories to model a unified theory of gravity and particle physics, the simplest forms of string theory are inconsistent with what we observe, and making the theory more complicated to get around this makes the theory consistent with almost anything and thus unpredictive.

34. **anonymous**
January 20, 2007

Michael wrote: "I recently read that Steven Weinberg considered the most important task ahead in theory research was to understand better the various effective field theories in hadron physics. He felt deep insights – and perhaps important new physics – lay within these structures."

Do you have a link or a reference for this? I feel like there's a miscommunication between you and Peter in this discussion, because I don't think Weinberg would say that the most important task ahead is for people to understand "effective field theory" in the general sense Peter is interpreting it as. Pretty much everyone in the field understands that. I expect Weinberg means *particular* effective field theories (hence, "in hadron physics"), probably formalisms like SCET.

35. **Peter Orland**
January 20, 2007

Peter,

I know that this is not your meaning, but to many people the number of times a paper is cited is the main indicator of how valuable the work is. I would not dispute that the number of citations is a parameter which has some use, but the emotional reliance on this number is, in itself, unhealthy. Are we to evaluate ourselves the same way chairs, deans and provosts evaluate us?

I was at a large string theory conference some years ago and one speaker who had been giving many talks "challenged" the audience with the question, "What will be the next string theory paper to garner 500 citations?". Everyone seemed highly animated and thrilled by this disgusting, prurient line of thought. I mentioned to a young string theorist, who had been very excited by this question, that the number of citations is much less significant than an experimental prediction. He replied that if this were to happen in string theory, he would switch to pure mathematics. I interpret this to mean he was actually indimidated by the thought of confronting real science.

I realize that you are trying to make a particular point with SPIRES and the time-dependence of citations. Part of the reason for the phenomenon you mention is that people to believe that highly-cited papers must be good, so those are the papers which garner more citations. This is symptom of the short-term scholarship in our field.

36. **Chris W.**
January 20, 2007

Speaking of citations, would you believe a spill-over of eternal inflation and the multiverse into evolutionary biology? See [q-bio.PE/0701023](q-bio.PE/0701023):

> … I argue that the many-worlds-in-one version of the cosmological model of eternal inflation implies that emergence of replication and translation, as well as

> the major protein folds, by chance alone, as opposed to biological evolution, is a realistic possibility and could provide for the onset of biological evolution.
> …

37. **M**
January 21, 2007

Nobody presently knows what future good physics is, and citations allow to do a democratic pool. It will be interesting to wait some decades and measure the correlation between good physics and highly cited physics. Probably the result will be what Feynman described as:

"Nobody was permitted to see the Emperor of China, and the question was, What is the length of the Emperor of China's nose? To find out, you go all over the country asking people what they think the length of the Emperor of China's nose is, and you average it. And that would be very "accurate" because you have averaged so many people. But it's no way to find anything out; when you have a very wide range of people who contribute without looking carefully at it, you don't improve your knowledge of the situation by averaging."

38. **Q**
January 21, 2007

M, *it is more democratic to take a consensus by averaging guesses* than by imposing a particular person's guess. At least this process has the benefit of eliminating all extreme guesses, which are crazy enough to have a chance of being correct.

39. **M**
January 21, 2007

Q, ultimately some experimentalist will decide for everybody, and many top-cited papers will end up in the trash. Science is not democracy.

I agree that in the meantime citations might be a good alternative, but without forgetting its limitations, like the one you mention. Quoting Winston Churchill: "democracy is the worst form of government except all the others that have been tried".

40. **Kyrie**
January 21, 2007

> Nobody presently knows what future good physics is, and citations allow to do a democratic pool.

I guess you meant a "demographic poll"? But I agree with you: since nobody knows yet what new physics is going to be, citations reflect what most people in the field is working on, but not necessarily *where* the good physics is.

> and many top-cited papers will end up in the trash. Science is not democracy.

Once a paper has a few thousand citations, it will remain in the top cited list for a very long time. If it's more than 2000, then it will remain there for decades to come, maybe even forever…

Funny thing is, if susy and/or extra dimensions are not seen at the LHC, several papers on either subject will be stuck in the top-cited list in HEP, even though nobody is going to be working on that anymore…

41. **Kyrie**
January 21, 2007

On a related note, there's not only the list of top-cited papers. I read in the December issue of Physics Today that there are *five* new books on susy and/or susy phenomenology. I don't doubt all of them are very good books. I wonder how many good books on susy and its phenomenology are there now in the market. One dozen? Several dozens?

In this respect, I think there are two scenarios for LHC physics, and both look equally ludicrous to me.

In the first scenario susy is observed at the LHC. The idea that the discovery of new particles has not only been predicted, but actually preceded by more than a dozen books about them is definitely preposterous.

Just imagine that between 1946 and 1976 people had been very busy writing *large numbers of books* about the Upsilon and b physics… does that make sense from a historical perspective? It'd be like a movie played backwards, and equally hilarious 🙂

The second, no less ludicrous, scenario is that susy is not observed at the LHC. Then, what are historians of physics going to make of the last 30 year period? What are they going to have to say about an entire community working for decades on a field, to the extreme of writing several dozen books about it, when actually there will be no hint of the relevance of that field to nature? Again, it'd be a very funny situation…

42. **Peter Woit**
January 21, 2007

I'd like to make clear that it certainly is not my opinion that number of citations indicates a paper's value. These citation numbers are interesting purely for providing an objective measure of what topics particle theorists are working on, and what past ideas people have decided are worth pursuing and are using to write new papers. It's no secret that I think there's a serious problem with the choices that are being made and would like to see very different ones….

43. **Amos Dettonville**
January 21, 2007

Regarding the Deutsch quote, I think he may have been trying to make a more general point, since he referred to "a research programme OF THAT KIND". He apparently thinks string theory is (or at least was) a research program whose modus operendi was to rely heavily on purely mathematical criteria in seeking a "mathematical object" that corresponds with the world. I guess it's debatable whether the string program fits that description, but this seems to be the KIND of program that Deutsch thinks is unlikely to be successful.

As I understand it, Peter thinks this criticism doesn't actually apply to the string program. Rather, he says, the string program has been driven by empirical factors, and those factors have falsified the notions arrived at by purely mathematical criteria, so the program can't be criticized for total reliance on abstract criteria, ignoring empirical facts. He thinks the problem with the string program is in how it has responded to the (what he sees as) empirical falsification, by introducing so much flexibility that it becomes essentially unfalsifiable – thereby rendering it incapable of making any definite predictions.

I think the comments of Deutsch and Woit describe two different problems that can afflict a research program. The string program has probably suffered from both at different times. The fundamental premises underlying the string model, with the extra dimensions, etc., seems (to me) to have been motivated largely by abstract considerations of the kind that Deutsch argues are unlikely to lead to genuine increased understanding of physical phenomena. But I also agree with Peter that, at the present time, the string model seems to have morphed from what once was hoped (and hyped) to be a rigidly unique structure into an almost infinitely flexible curve-fitting formalism (like Ptolmey's epicycles and deferants) that could be fit to just about any conceivable set of facts.

I suppose, in a way, the two problems are related, i.e., any program devoid of new physical ideas and relying heavily on mathematical criteria, is likely to arrive (at best) at a more sophisticated curve-fitting model.

As an aside, I don't think there is necessarily anything wrong with devising a better curve-fitting formalism for expressing physical theories. My problem with the string program is that I think a lot of its prominence and funding, etc., has been and still is driven partly by the aura generated by the original hype, which presented string theory as having this marvelously rigid and unique structure, e.g., it ONLY works in this many dimensions, and it REQUIRES this and that, so the impression was given that this program was leading to the unique logically viable structure, which must therefore be true. It isn't surprising that this prospect got a lot of people (even those with no knowledge or expertise in physics) excited, and the news was on the "front page" of the popular press. Now, decades later, very few people in the field would make those such claims, but the retractions and back-pedaling have much less loudly publicised than the original claims. As a result, I think the string program has continued to benefit from some residual public excitment and hype that, in retrospect, was not justified. So, I think the books of Penrose, Smolin, and Woit are useful in countering some of that effect, and highlighting the fact that the feature of the string program that was originally its main selling point (unique prediction of the logically necessary structure of the universe!) is now very much in doubt.

44. **King Ray**
January 21, 2007

String theory is definitely Ptolemaic… it is to the standard model as Ptolemy's cycles and epicycles are to Kepler's laws and Newton's theory of gravity. The ultimate theory is supposed to be simpler than the standard model…

45. **Chris W.**
January 21, 2007

I must say I think Deutsch is right on target in the following sense: At this point in the development of string theory, the problem has become, at best, *a combination of* (1) "what mathematical structures should or must the theory contain" and (2) "what is the right way to set up a correspondence of these structures with observations we can make, or theories that have already tested [ie, the Standard Model]". The theory has evolved in such a way that it allows enormous latitude for fiddling at both levels, as if its developers imagined that such room for fiddling is what one ought to have in a good theory. In short, the apparent conclusion has become that there is a vast multiplicity of options, any one of which might become "the flavor of the month" as observations come in, ad infinitum.

The seeds for this situation arguably existed in the theory from the start, inasmuch as it began as a stab at understanding the strong interaction, and evolved—based on some formal features of the theory—into an approach to unification and quantum gravity. A clear *a priori* motivation for the relevance of those formal features to the new context has never been articulated. Similarly, the linking of internal symmetries and spacetime (Poincare) symmetries formally described by supersymmetry has never been understood physically.

In contrast, Einstein's motivations for replacing the spacetime of Minkowski by a Riemannian spacetime were clear, because he took pains to make them so. He tried very hard to minimize the arbitrariness of his choices. Neither experiment nor mathematical insight (much less mere technical virtuosity) is enough for this. What Einstein had at that time, it seems to me, was a *deep insight into what physics was about*. An essential aspect of such insight is appreciating the empirical facts of prior theories' successes, while knowing that these theories cannot be fundamentally correct.

46. **Thomas Larsson**
January 22, 2007

What Einstein had at that time, it seems to me, was a deep insight into what physics was about.

It might be added that the really big breakthroughs of the previous century (SR, GR, QM and renormalization) all involved thinking deeply about observers and observability. I see no reason why QG should be different.

47. **Jester**
January 22, 2007

One could look at these results from an optimistic side. Apparently, there was recently no hype that could fool a large part of the community (like the 2004 paper about split supersymmetry, hep-th/0405159, 104 citations in 2005).
But seriously, i really think there is more interesting work going on now than in the last few years, though it does not pass the 100+ trigger. Two examples from my domain: a composite Higgs of hep-ph/0412089, 47 citations in 2006, and a bottom-up approach to AdS/QCD, hep-ph/0501128, 53 citations in 2006.

48. **Travis**
January 23, 2007

Good to see that you are making use of the SPIRES database, however, as someone else mentioned, there was an anomaly in our citation data for about 12 hours during the time you were searching. This was due to an error in some new code that we had to release to be able to track citations to the upcoming new arXiv.org ids, as well as tracking other citation forms. For this reason I would be a bit suspect of your data, and I'm sorry that our unfortunate bug caused a problem at the wrong time for your analysis. Beyond that bug there can be some other anomalies, since data mining the database can actually be a bit tricky and it helps to really understand the data and the system.

This year we plan to release the topcites lists on the 31st of January, so your wait will not need to be too long. In the future, if you (or anyone reading this) have some statistical questions like this that we don't answer via the regular lists, feel free to contact us directly at spires AT slac.stanford.edu and we can often generate such lists and statistics much faster, and more accurately than you can via the web interface. We can't devote too much time to these things, but we can certainly help when we can. See http://www.slac.stanford.edu/spires/play for examples of this sort of thing.

Best Regards,
Travis
SPIRES Database Manager

49. **Peter Woit**
January 23, 2007

Thanks Travis,

I've spot-checked the numbers I found earlier, and the ones I checked seem to agree with what SPIRES reports now. Looking foward to seeing your results. I did also gather data on theory papers from the last few years with citations in the range 50-100 in 2006. Before writing about that I guess I'll wait and see if your numbers are any different than mine.

Various and Sundry

January 22, 2007
Categories: Uncategorized

Later this week there will be a mini-workshop at City College organized by some of the CUNY particle theorists, on the topic of Yang-Mills Theories: nonperturbative aspects. The schedule of talks is here, I'm planning on attending some of them.

Also this week, Witten is speaking at the IAS on Wednesday with the title "Operator Expansion Product of 't Hooft Operators". I'd like to go down to Princeton to hear this, but have to teach here around the same time, so won't be able to attend the talk. Maybe someone who does attend will tell us about what Witten had to say.

There's an interesting new particle theory blog, called Resonaances, and written by someone in the CERN Theory Group (who for now is operating anonymously as "Jester", also commenting here). It includes reports of talks at the recent Winter School on Strings, Supergravity and Gauge Theories, discussion or recent ideas about supersymmetry breaking using metastable vacua, and scary photos from the Christmas party, which included someone playing a Borat/Theorat character and Wolfgang Lerche as the string pope, intoning the following prayer:

Our Witten, which art in Princeton,
Hallowed be thy name.
Thy Nobel come,
Thy will be done,
In CERN as it is in the US.
Give us this day our daily string,
And forgive us our theory,
As we forgive those who do phenomenology.
Lead us not into experiment,
And deliver us from tests.
For thine is the arXiv,
Hep-th and math-AG,
For ever and ever,
Amen

Over at Tommaso Dorigo's blog, he's spreading wild rumors about a Higgs signal seen by CDF. He does acknowledge that this "signal" is not the sort of thing one should take seriously, almost certainly a statistical fluctuation. With the Tevatron getting closer to the point where it might actually see the Higgs, and the LHC sooner or later starting to produce data, I look forward to the prospect of lots of rumors being put out by bloggers of Higgs or SUSY signals. I remember many years ago that there were always new rumors of things being seen at experiments, which just about always turned out to not actually be there. In recent years the large experimental collaborations have done a better job of acting responsibly and not letting wild rumors get out. Maybe the blogging phenomenon can play a useful role in getting the irresponsible rumor game going again. Any CDF/D0 people who want to send me rumors that I can then irresponsibly help propagate are encouraged to do so.

I just got a copy of a new textbook about Lie groups and their representations, called Compact Lie Groups, by Mark Sepanski. I had been frustrated that there wasn't a book out there of just the right level with the same perspective I'm taking during the next few weeks of my graduate course, but Sepanski looks just right. From what I've seen so far of it, I recommend it highly as a place to learn about things like the Peter-Weyl and Borel-Weil theorems.

Another interesting book I recently acquired is Terry Gannon's Moonshine Beyond the Monster, which is highly readable as well as entertaining, and contains a wealth of information about affine lie algebras, "modular moonshine", vertex operator algebras and conformal field theory, and much more.

There are two new textbooks now out about string theory and attempts to get the a unified theory of particle physics out of it, by Michael Dine, and by Katrin and Melanie Becker and John Schwarz. I haven't had a chance to look at either very carefully, but they both seem to neglect to mention that this idea doesn't work. The thing that most amazes me though is Dine's choice for one of the three luminaries of the field to get a blurb from that might convince people to buy the book: Lubos Motl.

Update: John Conway, the CDF experimenter whose potential Higgs signal was mentioned here, has joined the Cosmic Variance team, and his first post is one of a series giving the details of this story.

Comments

1. **A quantum diaries survivor**
   January 22, 2007

   🙂 spreading rumors is fun indeed!

   But I will defend strenuously my reputation if put to trial on this one! I just about posted blessed results (shown at conferences) of a reasonable analysis observing a 2-sigma bump _somewhere_ in one spectrum… I am not guilty, your honor!

   Of course, part of the fun in discussing these kind of bumps lies in the fact that usually only those who know better understand that a 2-sigma bump like that has _zero_ significance (because the mass is unknown, and a bump _has_ to happen somewhere).

   All others, who are fascinated by the chance that a higgs is there, of course want to dream on, and I let them do so by presenting some additional suggestion… I claim innocence on this count too, your honor.

   Cheers,
   T.

2. **yagwara**
   January 22, 2007

   Thanks for the pointer to Terry Gannon's book.

   On lie groups, do you dislike Brocker and tomDieck for some reason? I ask because I have been wanting to go back and (re)learn some of this stuff and that was one of the books I was planning to use.

3. **Peter Woit**
January 22, 2007

yagwara,

Brocker, tomDieck is probably my favorite book among the older ones, although unfortunately it doesn't discuss Borel-Weil theory. The Sepanski book is in some ways similar to Brocker, tomDieck, but simultaneously shorter, covering fewer things that are not so important, and getting to Borel-Weil theory.

4. **kuos**
January 22, 2007

Lot's of SUSY/String textbooks coming out, each one destined to become a classic. Perhaps a last ditch effort to cash in before LHC makes 20 years of theoretical research obsolete?

5. **wolf**
January 23, 2007

Regarding the Cuny workshop, it seems to be entirely devoted to two spatial dimensions. Maybe with an eye on applications to critical phenomena/statistical mechanics?

6. **Shantanu**
January 23, 2007

Peter did you find anything interesting/controversial at the KITP workshop
on singularities at : http://online.kitp.ucsb.edu/online/singular_m07/

7. **Peter Woit**
January 23, 2007

wolf,

I don't know if there are any applications of 2+1d YM to stat mech. I think the main reason people are interested in 2+1d YM is as a toy model for trying to solve 3+1d YM non-perturbatively. In 1+1d YM is exactly solvable, in 3+1d there's very little we know how to do analytically. 2+1d is intermediate in difficulty…

Shantanu,

My main interest is in particle physics, so I fear the kind of quantum gravity discussions going on at that workshop just aren't something I'm interested enough to follow closely. The workshop does appear to have both LQG and string theory people participating on an equal footing, so I guess that's something worth noting.

8. **Thomas Love**
January 23, 2007

Peter, I just noticed this on your side bar:

Categories
Not Even Wrong: The Book (24)
Uncategorized (476)

That's a total of 500 posts, a landmark which should be acknowledged. Congratulations!

9. **A quantum diaries survivor**
January 23, 2007

Congratulations ??? What a lazy bum! I have 670 posts out and I've been around for a third of the time…

Of course… Hehm… My posts are not quite as interesting, and… Hhruumph… They get a hundredth of the traffic…. And hmmm… I do not have a tenth of the comments to answer. Oh well.
Congratulations, Peter!

T.

10. **Who**
January 23, 2007

Yes! Congratulations on reaching the 500 mark, Peter, with a blog that clearly makes a uniquely valuable contribution.
Also must say I was very glad to see the home-made 2006 topcites. I wouldn't be surprised to learn that their appearance accelerated Spires and prompted them to decide on bringing out the official list by 31 January, the date Travis mentioned in this post:

http://www.math.columbia.edu/~woit/wordpress/?p=508#comment-21399

11. **Peter Woit**
January 23, 2007

Thanks T. and T., although you may be slightly premature, I think there might be two posts that are in both categories…

Just upgraded the blog software to WordPress 2.1. If anyone notices any anomalies, let me know.

12. **z3**
January 23, 2007

Heads up on Distler and Rothstein's paper in the upcoming issue of PRL discussing possible LHC tests of string theory, or rather it's generic assumptions — analyticity, unitarity, and Lorentz invariance — through W boson scattering events.

13. **stevem**
January 23, 2007

As regards the CUNY Yang-Mills workshop, what exactly is the current status of non-perturbative analytical calculations of things like mass gap, string tension and confinement? I had heard/read somewhere that the string tension can be estimated for (2+1) YM and is comparable to values obtained from lattice sims. The talks will probably cover such things. Hopefully, they will appear online as the first one looks especially interesting. Or maybe someone attending will report on them. The emphasis seems to be on (2+1) YM. It still seems unlikely that (3+1) YM for SU(3) can ever be solved exactly. Maybe a string dual can be found to pure qcd, but again this seems unlikely, at least right now. Also, does anyone know if any progress has been made on the YM millennium problem? I presume you would have to prove existance of quantum YM and a mass gap strictly in (3+1) and not in (2+1)?

14. **wolf**
    January 23, 2007

    z3, those generic assumptions of string theory — analyticity, unitarity, and Lorentz invariance — look vaguely familiar to me. Could it be that I have seen them some place else? Maybe some theory not involving strings?

15. **Peter Orland**
    January 23, 2007

    Stevem,

    There will be several things discussed at the CCNY meeting.

    Right now, what most people are doing in 2+1 dimensions are new strong-coupling methods. In 2+1 the strong-coupling mass gap is proportional to the coupling squared, and the string tension (determined by working out the vacuum state) is proportional to the coupling to the fourth power. These results were shown by lattice strong-coupling methods (not computer simulations) a long time ago, but some new mathematical methods which are quite interesting have been developed to study them.

    I will also present some work I have been doing for the last few years
    on a weak-coupling method for understanding confinement, which
    works only in 2+1 dimensions. The method used two coupling constants, which are arbitrarily small in units of the UV regulator. The
    gauge theory studied this way is not rotation invariant, but quarks
    are confined and adjoint sources are not (for a finite number of
    colors. There is also an interesting picture of string excitations.

    I think what most people hope to gain from the meeting (at least I do) is an approach to confinement in 3+1 which works for weak bare coupling constant.

    Since Peter W. will be going to some of the talks, perhaps he can give
    his evaluation of the meeting.

16. **z3**
    January 24, 2007

    Wolf, these principles are not shared by all theories of QG. So a test of them amounts to a weak test of string theory, in that there's a very low probability string theory would be falsified thru an observed violation of these requirements, but the lack of any violation also only increase our confidence in string theory by a negligible amount. There is quite a bit of publicity stunt in calling this a test of string theory, but there is some physical motivation in it as well.

17. **John Conway**
    January 24, 2007

    "Wild rumors" ??

    I gave the talk at the Aspen particle physics conference where we broke the news of the results of our search. I was the one who "let the wild rumor out."

    We showed what we saw, pure and simple. It's not anything to get excited about statistically, clearly. People did get excited, though, because this is about the Higgs after all!

    You try to suggest it's "irresponsible" to show your results honestly in public, and discuss them afterwards in the hallway or blogs or wherever…well, that's just irresponsible of you. So there.

    And, T., there certainly does not *have* to be a bump somewhere. For us we would only expecet such a thing about 2.5% of the time; we are working on refining that number, as I said at Aspen. It's tricky, as you know.

    What are results do show is that *if* the Higgs is there, at an enhanced rate at high tan beta, we could nail it at the Tevatron before the LHC (on which I also work) does. Now wouldn't that be interesting?

18. **Peter Woit**
    January 24, 2007

    John,

    My comments about Tommaso's posting were a bit tongue-in-cheek. I do think HEP experimentalists are behaving incredibly responsibly these days, rarely if ever promoting signals of marginal statistical significance. Maybe I'm wrong, but I remember this happening much more often 20-30 years ago. I admit to a bit of nostalgia for this kind of activity, for seeing people get excited by possible signals (even if they almost always have their hopes dashed later). So I think maybe a bit more irresponsibility would be a good idea, and I was trying to encourage any inclinations in that direction that Tommaso might have. The only problem is how to keep it out of the science coverage in the press that goes out to the general public…

19. **Tommaso Dorigo**
    January 27, 2007

    Yes, John, the mentions of "rumors" and "irresponsibility" were made with half-joking tone, both above and in my blog.

    As for the significance, I totally agree, it is tricky indeed. My opinion is that the CDF signal would be the typical way a 160 GeV Higgs would show up in a statistically-limited analysis such as ours, but it is just as well the typical bump due to a weird fluke in the data.

    We search the Higgs in tens of different mass distributions, in various final states. We HAVE to have a 2-sigma bump somewhere at some point! Of course, you can shrug your shoulders and claim that you are only concerned with your own analysis, but as long as rumors spread only when signals grow above the (say) 2-sigma mark (or if more vocal people start crying wolf at 1.5-sigma, as in LEP II – hruuumpf), we are giving the world a wrong perception of the data if we do not qualify those 2-sigmas.

Not your job, of course, so no criticism is implied to you, but rather on the way we publicize our scientific output.

And I agree with Peter when he says these days we are a bit over the mark with our “protection” of our own data. We’ve been colleagues for 14 years, and you certainly have a very well informed opinion on how I think of the whole matter… Just think of the superjet affair 😉

In any case, congratulations for your intriguing result, John!

Cheers,
T.

20. **Tommaso Dorigo**
January 27, 2007

And, Peter, the problem of discussing results among peers without the matter getting known by newspapers is not solvable.

During the last four months I have been contacted to comment or explain new results posted in my blog by no less than five journalists from Nature, New Scientist, Physics World, Scientific American, Physics Today… (And as you know I am grateful for your directing traffic to my blog now and then, without which I would be a lesser known blogger).

These people do look for “leaks” in the blogosphere. They have grown smart!

That, combined with the tough bylaws of my experiment, totally prevents me from real leaks in my blog. What I can do is to hint at things to come. As I did in the post on John’s signal, where I hinted at the fact we will bless a Z->bb signal in CDF soon, and that a few tens of higgs decays could be there if the MSSM signal were true…

Cheers,
T.

21. **Peter Woit**
January 27, 2007

Tommaso,

One solution of course is that if you can’t leak on your blog, you could write me, and I could then spread rumors from my blog… If journalists called me I would deny everything, claim I just made it up.

A major problem with this whole blogging thing is that some of the best rumors I hear from friends and colleagues I just can’t use, mainly because it would be too obvious where they came from, and the people involved would get annoyed and never tell me anything again. My hopes to become a leading physics/math gossip columnist are unfortunately difficult to realize.

Physicists Develop Test For "String Theory"

January 24, 2007
Categories: This Week's Hype

Press releases claiming that a "test for string theory" has been found appear with some regularity, notwithstanding the fact that no one actually knows how to test string theory. The latest one comes from the University of California at San Diego, where the press office today put out a press release entitled Physicists Develop Test For "String Theory". The story has been picked up by the media, appearing here and here and probably soon in many other places.

This latest claim about a "test for string theory" is quite remarkable and even more bogus than usual. It is based on a paper which has nothing to with string theory and doesn't do a string theory calculation at all. The paper first appeared on the arXiv last April with the title Falsifying String Theory Through WW Scattering, and was extensively discussed here. In October a new version of the paper was put on the arXiv, with a changed title Falsifying Models of New Physics via WW Scattering (and this was discussed here). I'm guessing that the removal of the claims about string theory from the title was due to a referee at PRL not being willing to go along with such a title, although maybe there's more to the story and if so I'd be curious to know what it is.

The year is just beginning, but I'm already willing to award this press release the title of "most outrageously misleading string theory hype of 2007". It is going to be extremely hard for anyone else to match it.

Update: The Distler et. al. overhyped press release continues to spread misinformation to the public, getting more and more ridiculous as it spreads. The blog Tech.Blorge.com reports about string theory that:

Until now, experimental verification has not been possible; but researchers at the University of California, Carnegie Mellon University, and the University of Texas are planning a definitive test with the future launch of the Large Hadron Collider…

This then made it to Slashdot, which put out a story under the headline String Theory Put to the Test, which starts off with:

…scientists have come up with a definitive test that could prove or disprove string theory. The project is described as…

and then goes on to give a description of the LHC project.

I think the people responsible for this should be ashamed of themselves.

Update: Not to be outdone by UCSD, Carnegie-Mellon has also issued a press release about this. More also here and here.

Update: More at Digg, SpaceDaily, Science Frontline, etc., etc.

Update: Yet another major university issues a misleading press release about this: from the University of Texas Team of Theoretical Physicists Develop a Test for String Theory.

Update: The Resonaances blog has a posting explaining what is actually in the Distler et. al. paper, while describing the press releases, with their pretensions that the authors have found a way to test string theory at the LHC, as "hilarious".

Update: Sabine Hossenfelder wrote in to point out that New Scientist now has an article about this, with the title New particle accelerator could rule out string theory. The article quotes hype from string theorist Allan Adams as well as from Distler, ignoring Distler's co-authors and describing him as "leader of the team" that solved the problem no one else had been able to solve, figuring out how to test string theory at the LHC. Funny, but as far as I can tell, this great advance in the testability of string theory is not being covered at any of the string theory blogs. I wonder why…

Comments

1. **andy.s**
   January 24, 2007

   The authors are saying it tests Lorentz invariance, analyticity, and unitarity, the ingoing assumptions for ST.

   I guess the first one means relativity is valid, the last one means all operators are unitary (?); I have no idea what analyticity means.

   Are any of these likely be violated at the LHC?

2. **r hofmann**
   January 24, 2007

   In general, unitarity of the S matrix puts a bound on the growth of cross sections with energy, the so-called Froissart bound. In particular, if the SM-calculable bound is heavily violated by experiment (LHC) then the SM is inferred to be embedded into something more general which takes over at high energies. Personally, I strongly believe that the latter will happen on grounds that have nothing to do with susy, Xdim, and string theory for that matter.

3. **Jester**
   January 24, 2007

   It's a pity the authors chose to advertise this way since, after all, they did a very interesting work. Most of us used to think that in a low-energy effective quantum field theory any higher-dimensional operators consistent with the symmetries are allowed. Now we know that at least the signs of certain operators are constrained by very general arguments. Only very bizarre theories could violate these constraints (and string theory is not bizarre enough). There is also the earlier paper by Arkani-Hamed et al. [hep-th/0602178], which discusses these matters from a somewhat more general perspective.

4. **Bee**
   January 24, 2007

   You have a funny typo there, never heard of WWW Scattering 🙂

   In October a new version of the paper was put on the arXiv, with a changed title Falsifying Models of New Physics via WWW Scattering (and this was discussed here).

5. **Peter Woit**

January 24, 2007

Thanks Bee. I guess I thought the author's paper has more to do with scattering bogus claims via the World Wide Web than scattering W particles…

6. **Tony Smith**
January 24, 2007

The UCSD press release by Amy Pavlak and Kim McDonald has as its title
"Physicists Develop Test for 'String Theory'"
The press release said in part:
"… "Since we don't have a complete understanding of string theory, it's impossible to rule out all possible models that are based on strings," said Rothstein. …".

The title and the statement by Rothstein seem to be contradictory.

Further, the press release said that the "… test sets bounds on … three mathematical assumptions –
Lorentz invariance (the laws of physics are the same for all uniformly moving observers),
analyticity (a smoothness criteria for the scattering of high-energy particles after a collision) and
unitarity (all probabilities always add up to one). …",
so
it seems that the press release could just as well be entitled "Physicists Develop Test for 'Bootstrap Theory'",
about which Peter Woit said in blog comments in July 2006:
"… the bootstrap program … was wrong, and wrong in very much the same way that string theory is … If you look at Chew's writings from that era, there's a lot of the same kind of wishful thinking you see in string theory. Not knowing about asymptotic freedom of gauge theory, you could argue that the bootstrap was "the only game in town" … To some extent, lots of people who had worked on the bootstrap and early versions of string theory just picked up where they had left off when string theory came back into fashion in 1984. There really only was a period of about 10 years (1974-1984) when QFT was completely dominant. …".

It is also interesting that Ed Witten studied under David Gross and
David Gross studied under the leading advocate of bootstrap theory, Geoffrey Chew.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

7. **A quantum diaries survivor**
January 24, 2007

Well, the first link quoted above is a rather imprecise but not dishonest account IMHO. In particular, I appreciated the sobering quote by Distler:

"If the bounds are satisfied, we would still not know that string theory is correct," said Distler. "But, if the bounds are violated, we would know that string theory, as it is currently understood, could not be correct. At the very least, the theory would have to be reshaped in a highly nontrivial way."

Of course, there is a small step still needed to fill the gap from what Distler acknowledges and the fact that nobody really takes seriously any theory implying violation of those three principles, and the article fails to make it.

T.

8. **Peter Shor**
January 24, 2007

So does this mean that now any test of general relativity is now also going to be a test of string theory? And that Hulse and Taylor received their Nobel prize for a successful test of string theory?

9. **Ari Heikkinen**
January 24, 2007

I guess it's not just a string theory problem, I mean, how do we know that any "new breakthrough" that'll be claimed even on LHC will be anything but a desperate attempt to make something out of some nonsense just to give even some justification for all the money spent on building it?

10. **Tony Smith**
January 24, 2007

Ari Heikkinen said: "… how do we know that any "new breakthrough" that'll be claimed even on LHC will be anything but a desperate attempt to make something out of some nonsense just to give even some justification for all the money spent on building it? …".

LHC is almost certain to produce results with respect to Higgs and the Tquark that will substantially improve understanding of the Standard Model.
The Standard Model is very far rom "nonsense".
Increased understanding of it is very important with respect to real (i.e., testable by experiment) physics,
and (in my view at least) fully justifies the cost of building the LHC so that its enegy/luminosity regions can be explored and better understood.

As to "how do we know" claimed results of LHC will or will not be "nonsense",
it is the same as with all accelerator experiments:
You read the papers, look at the data, and study the analysis techniques. If you find something that doesn't look right to you, then say so and if your criticism is correct it will eventually become part of the results.

There WILL be data that if properly analyzed WILL increase our understanding of the Standard Model, and, since that is what the LHC was designed to do, it WILL be worth the money spent on it for that purpose.

There is a big difference between deciding to spend money on a machine to do a specific task (for the LHC, to explore the Standard Model in a given region of energy and luminosity)
and
spending 90% of high energy theory money on something that is substantially divorced from experimental results, and has produced no such results for decades, and is sociologically operated like a private club.

Tony Smith
http://www.valdostamuseum.org/hamsmith/

11. **Peter Woit**
January 24, 2007

Tommaso,

I think there actually is an honesty problem with the Distler quote. His paper does not involve any string theory calculation in any way, so claiming a "test of string theory" is very problematic. In addition, as far as I can tell, what seems to have happened with that paper is that a PRL referee made them change it to remove the claim from the title about having a "test of string theory". If this is right, to issue a press release about the PRL paper describing it as a "test of string theory" takes real chutzpah. If there's another explanation for why the paper's title was changed, I'd like to know what it is.

12. **Peter Woit**
January 24, 2007

Ari and Tony,

Please try and keep your comments on-topic.

13. **anonymous**
January 24, 2007

Tommaso said:

"the fact that nobody really takes seriously any theory implying violation of those three principles"

But at least a few people take seriously loop quantum gravity, which as I understand it does violate at least one of these principles….

14. **A quantum diaries survivor**
January 24, 2007

Ok, I confess I did not read the paper 🙁 shame on me!

I guess I am getting dangerously close to having to rely on incorrect press accounts to know what is going on, at least in string theory.

As a matter of fact, I remember discussing in my blog (guess with who, with Jacques Distler) the very issue of theoretical papers being too cryptic for a "normal" experimentalist to even conceive reading. If I recall correctly, he claimed that it was not string theory's fault, because it has never been easy for experimentalists to read cutting-edge theoretical papers.

T.

15. **Peter Woit**
January 24, 2007

Tommaso,

Actually that paper is pretty readable, partly because it doesn't involve string theory. Some experimentalists might actually find it more readable than many theorists, since it is a story about the behavior of scattering amplitudes. This is closer to what experimentalists have real experience with these days, unlike theorists who often spent much of their careers working on more speculative stuff.

16. **Ari Heikkinen**
January 24, 2007

Peter,

The point I was trying to make (ok, perhaps it was badly worded as an example) was that upfront announcements of scientific "breakthroughs" is nothing new. It's been done throughout the history by scientists, the media and by others, not just by string theorists.

Speaking of the announcement in question, don't they claim this one could only prove string theory wrong, not right (Quote: "But, if the bounds are violated, we would know that string theory, as it is currently understood, could not be correct.") ?

So if they think they can prove string theory wrong this way then what's the problem? Also, the pdf you link (on arxiv) still reads "Falsifying String Theory Through WW Scattering" so what's wrong with that?

17. **Peter Woit**
January 24, 2007

Ari,

I put two links there. One is to the original version of the paper, with the claim about string theory in the title, the second is to a revised version of the paper uploaded several months later, where the title has been changed to remove the reference to string theory. Given the timing of this, it seems likely that the revised version is the refereed one, and that the referee is responsible for insisting on the changed title.

You might want to go back and read the extensive discussion about this whole topic that occurred here and that I linked to, involving one of the authors of the paper. I don't want to repeat all those arguments, but the gist of the matter is that the assumptions made by these authors are actually the fundamental ones made in QFT. So, what is actually true is that if their bounds are seen to be violated, it is QFT, not string theory that is falsified. These same assumptions apply to perturbative string theory, but since one doesn't really know what non-perturbative string theory (M-theory) is, they don't apply to string theory in general.

18. **onymous**
January 24, 2007

My attempts to comment anonymously keep getting rejected, I'm not sure why.

Here are two things that are being overlooked in this discussion:

1. Yes, both QFT and string theory have these basic properties, but not necessarily every theory does. LQG, for instance, is sometimes claimed to violate Lorentz invariance.

2. These bounds reveal ways that low-energy physics is sensitive to very high-energy physics. It is true that in QFT these positivity criteria are always satisfied, so QFT would be falsified by violations of them. However, it's not the case that in low-energy *effective* QFT these criteria are always satisfied. So what is falsified is that at very *high* scales, the theory describing our world is not a QFT. But we already know that, because of gravity! So QFT is already falsified. Given that QFT is falsified, we can ask what are other sensible theories of high-scale physics. One is string theory. Violation of these positivity conditions would, then, falsify string theory. This is not trivial. They would not necessarily, for instance, falsify LQG.

19. **Peter Woit**
January 24, 2007

onymous,

Sorry about your problems with the spam filter (Akismet). The new version of WordPress has a newer version, which may have different behavior. Unfortunately it is is completely unconfigurable, and it does sometimes tag comments as spam for no obvious reason (sometimes it does this to MY comments…). I do generally check the spam queue once or twice a day, so if you just wait a while, your comment should appear after a while.

I just don't think it's accurate to say that violation of these bounds would falsify string theory (and, if you look at the wording in the published version of the paper, I think you'll see the authors don't say this, perhaps because a referee wouldn't allow them to). What might be falsified is perturbative string theory (and presumably this is what the authors mean by the locution "generic models of string theory" used in their paper), but that is already falsified. One needs some sort of non-perturbative string theory or M-theory to get the kind of string theory backgrounds needed to get realistic physics. Not knowing what this M-theory is, you don't know if it will satisfy the relevant assumptions.

20. **Ari Heikkinen**
January 24, 2007

Ok, I got your point, the first three versions of the paper in question are titled "Falsifying String Theory Through WW Scattering" and claim "If no light resonances are found, then a measured violation of the bound would falsify string theory.".

But in the fourth version of the same paper the title was changed as "Falsifying Models of New Physics via WW Scattering" and the claim was changed as "As a corollary, if no light resonances are found, then a measured violation of the bound would falsify generic models of string theory.".

As far as I can recall from Greene's book, there wasn't any mention of "generic models" of string theory, so exactly what's meant by that?

21. **Peter Woit**
January 24, 2007

"generic models" of string theory doesn't actually have any well-defined meaning, as far as I know. It's a bizarre locution I've never seen anywhere else, and was presumably introduced here to satisfy a referee who told the authors that their assumptions were not obviously satisfied by all string theory models.

As I explained in my previous comment, the problem is that only for perturbative string theory (and some unrealistic non-perturbative versions) do you have any kind of definition of the theory, and thus can check whether it satisfies the assumptions being made. For the kind of fundamental theory that string theorists hope exists and produces standard model physics, the existence of such a theory is purely conjectural, and one doesn't know if it will satisfy the assumptions.

22. **mclaren**
January 24, 2007

Yes, I was just about to post about this misinformation. You have to admire the sophistry involved in these kinds of press releases. Since string theorists claim that string theory susbsumes all the rest of physics, it therefore follows that any test which confirms any theory anywhere in physics necessarily confirms string theory.

Good thinking. That kind of incisive reasoning will surely get us to a Theory Of Everything "real soon now"…that is, if we don't experience The Singularity first. Or find WMDs in Iraq…

23. **Aaron Bergman**
January 24, 2007

In related news, I recently saw a non-black non-raven.

24. **Jack Lothian**
January 24, 2007

I have not read the paper (& probably would not understand it) but the press release reads as a form of lying to me. In the release, facts are selectively stated in such a way as to mislead people who are not experts in the field. The release is written in such a way that they encourage the layman to jump to erroneous conclusions. In my opinion, here is an analogy of what they are doing.

I claim that I have an investment strategy that can return your investments plus 100% in as little as 2 months. Furthermore I have a scientific test that was used to validate my strategy. It showed scientifically that the return on your investment will always be positive. Of course some investors will be more successful in using my strategy than others. So just send me a few thousands dollars & I will give you my scientific investment strategy.

Is this a form of fraud? I say yes & the press release uses a similar argument to imply the test will validate string theory when in fact it will just show string theory is one many different possibilities and outcomes. And one of the possibilities that will be still open is that string theory is not even wrong.

25. **Johan**
January 24, 2007

Why would LQG be Lorentz-invariant? It's not a theory of flat spacetime. For an enlightening discussion, see http://www.lns.cornell.edu/spr/2003-11/msg0056792.html . Someone well known to NEW readers makes a cameo appearance.

26. **gunpowder&noodles**
January 24, 2007

I guess what people mean by "Lorentz invariant" is "Lorentz invariant in the limit of flat spacetime". By the way, are you sure that that thread is not some kind of forgery designed to suggest that LM doesn't understand basic GR?

27. **student**
January 25, 2007

GR (as well as string theory, which is also generally covariant) contains Minkowski spacetime as a solution. On the other hand, it is not clear if LQG can produce a flat Minkowski spacetime as a solution. In fact, there is a claim that the DSR is a limit of LQG and that the Lorentz violating effects in DSR can be tested experimentally (Lee Smolin keeps mentioning GLAST and other experiments). Hence, if the Lorentz violating effects are found experimentally it would be an indirect evidence of the LQG. The Distler, et. al. paper claims that the Lorentz violating effects can be tested at the LHC as well. If such effects are found the perturbative string theory would be invalidated. If they are not found, theories like LQG and DSR would get a big blow but string theory would still survive, although this would not "prove" string theory.

28. **Garbage**
January 25, 2007

I find rather amusing Peter's insistence on the inquisitor referee cutting off any reference to ST. It looks as if he was that referee ;). The paper was finally accepted with a revised title AND (quote from the latest version): "argument completely revisited, stronger and more pertinent bounds given".
This version thus make stronger connection to tests of New Physics at LHC, via WW scattering, including ST and any other candidate for a High Energy completion of the SM which obeys LI, Unitarity and Analyticity. There are a few technicalities which are not the case to discuss now (like a heavy Higgs scenario). However, as a matter of principle, the logic I think is clear.
Namely, tests of GR are of course tests of low energy ST (and also low energy LQG), but also tests of any other *classical* theory of spacetime and low energy physics. What's propossed in this paper is a test of the high energy behavior of scattering amplitudes and hence of high energy ST, since the latter is one of the few theories (and some claim perhaps the only one) we have to complete the SM (including gravity) in the UV. Furthermore, if the way out of ST (as Peter claims) is thru a yet non-existent M-theory, violations of the bounds will put strong constraints on the possible non-perturbative definitions of ST, which by itself is, needless to say, extremly important/interesting.

G

29. **milkshake**
January 25, 2007

The old filter in WordPress was triggered by multiple links or some notorious subject lines.

The new Akismet goes after comments that have multiple exclamation marks. It also filters out very short posts that look like they were bot-generated for driving up ranking of single website (typical one is "interesting point, I agree")

30. **Thomas Larsson**
January 25, 2007

I guess what people mean by "Lorentz invariant" is "Lorentz invariant in the limit of flat spacetime". By the way, are you sure that that thread is not some kind of forgery designed to suggest that LM doesn't understand basic GR?

From what I recall of that spr thread (you might be able to find some comment by myself there, I don't remember), this was one of the few occasions when I agreed with LM. He disagreed with people who claimed that diffeomorphism symmetry is empty just because you can choose curvilinear coordinates in Newton theory. Like a theory with global phase symmetry is a gauge theory, because you can choose different coordinate systems in different fibers 8)

A freely falling observer sees a Minskowki spacetime. GR has a local Lorentz symmetry in this sense.

31. **press release**
January 25, 2007

Garbage, the fact that the test needs a heavy Higgs is not a tecnicality, since data suggest a light Higgs.

Anyhow, violations of unitarity etc at the Planck scale would give effects 1000000000000000000000000000000000 smaller than what LHC can measure.

Furthermore, hep-th/0602178 presented the basic idea earlier and correctly.

32. **Matti Pitkanen**
January 25, 2007

For quite an interesting idea about a possible reduction of Poincare group to its subgroup as a fundamental symmetry (implying that CP also ceases to be a symmetry) see the [Very Special Relativity] of Glashow and Cohen. See also the little article [here].

33. **D R Lunsford**
January 25, 2007

Shame is the main thing missing in all areas of modern culture. It's one of those passe' emotions like love and loyalty. But don't you *feel good* about yourself?

-drl

34. **Arun**
January 25, 2007

Is this the reasoning?

Suppose the Standard Model is an effective theory valid upto some scale A.

We can add higher-dimension terms to the SM Lagrangian and put bounds on the coefficients of these terms.

Suppose those bounds are found to experimentally violated, and suppose our assumption that the SM effective theory is valid to some scale A is still true, then it must be that one or more of Lorentz invariance, analyticity of the S-matrix, etc., is violated and string theory is cannot be true.

The assumption that the SM effective theory is valid to some scale A and not some lesser scale B is presumably checked by the same set of experiments that find violation of the bounds on the coefficients of the higher-dimension terms?

Thanks in advance for clarifications!

35. **Peter Woit**
January 25, 2007

Please, this is not the place to discuss various people's confusions about the role of the Lorentz group in GR. In the paper in question here the Lorentz group has to be a global symmetry, this is not the case in GR.

36. **Peter Woit**
January 25, 2007

Garbage,

So, it's true that a referee wouldn't allow the paper to be published with a title claiming that its authors had a test that could falsify string theory? If so, don't you see a bit of an ethical problem with the authors having their universities issue press releases based on publication of the paper in PRL entitled "Physicists Develop Test for "String Theory""?

37. **press release**
January 25, 2007

Arun, these constraints come from Quantum Field Theory. So, if LHC can only probe energies at which some QFT applies (the Standard Model or whatever else), these constraints are automatically satisfied. To test if quantum gravity violates locality/unitarity/etc, one must do experiments sensitive to quantum gravity. But if we knew how to directly test quantum gravity, we would have no need of inventing indirect tests.

38. **onymous**
January 25, 2007

Trying again to post this:

"So, if LHC can only probe energies at which some QFT applies (the Standard Model or whatever else), these constraints are automatically satisfied."

No, this is not true, unless I misunderstand what you're getting at. An effective QFT can perfectly well describe everything happening at LHC energies, but the constraint can still be violated because the UV completion is something more exotic. In that case the effective QFT will have some unusual signs, but this doesn't mean it's not a valid effective QFT for low-energy physics. This is sort of the whole point of this type of study, as emphasized in the Adams et.al. paper (hep-th/0602178): analyticity tells you about how far-UV properties manifest themselves in IR behavior. In this way it is possible (but not likely!) that properties of quantum gravity could make themselves known at much larger distances, very indirectly. Unfortunately this only happens if the theory of QG is exotic enough.

Also, above: "Anyhow, violations of unitarity etc at the Planck scale would give effects 1000000000000000000000000000000000 smaller than what LHC can measure." This isn't clear to me either: do you have some particular example of a nonunitary theory in mind? In general modifying quantum mechanics is dangerous, e.g. the Banks/Peskin/Susskind argument that superficially plausible-looking modifications of QM lead to order-one violation of unitarity even at low energies (http://www.slac.stanford.edu/spires/find/hep/www?j=NUPHA,B244,125).

39. **amused**
January 26, 2007

onymous's point in his first comment above seems fair enough: Basically, if I understand rightly, if physics at high (Plank scale) energies is described by a quantum theory (of point particles, strings, or whatever) then Lorentz invariance, analyticity and unitarity imply certain positivity constraints in effective QFT descriptions at lower energies, and in particular in the Standard Model. These can apparently be tested in WW scattering, and if they are found to be violated it implies that a quantum theory description at Plank scale (with the aforementioned 3 properties)doesn't exist. Since string theory is supposed to have these properties, it in particular gets invalidated. Peter's objection that string/M-theory hasn't been constructed yet (at least at the nonpertubative level) is strictly speaking correct, but to me it seems excessively trench warfare-like. If the constraints are found to be violated in WW scattering it would certainly be a heavy strike against string theory as a road towards describing Plank scale physics.

Kudos to the authors for coming up with this interesting and potentially important result. But the string-hype way in which it is being presented in the press is lamentable. Casual readers will come away with the impression that string theory has finally made a testable experimental prediction, just like e.g. electroweak theory predicting the vector bosons and their masses. The limited nature of the "prediction" in this case should have been emphasized, and the authors can't escape some responsibility for this not having been done.

40. **amused**
January 26, 2007

"…imply certain positivity constraints in effective QFT descriptions at lower energies, and in particular in the Standard Model."

Er, I guess that should have been "…and in particular at energies above the standard model ones but still accessible at the LHC."
The SM itself, being a QFT, automatically satisfies the positivity constraints…

41. **Garbage**
January 26, 2007

"Anyhow, violations of unitarity etc at the Planck scale would give effects 1000000000000000000000000000000000 smaller than what LHC can measure."

As pointed out by onymous (although the reference to Banks et al. deserves further comments on), this is by no means totally clear. For instance, violations of LI generates dim

42. **Garbage**
January 26, 2007

Peter,

I posted a longer comment. Not sure what happened. There is something strange going on here. Unfortunately I posted it as I wrote it and I dont really feel like writting it again. Hope it can be retrived, otherwise it will be left to the reader's as an exercise to fill the (much longer) gap…

G

43. **press release**
January 26, 2007

Let us assume that quantum gravity is at the Planck scale (10^16 TeV) and violates unitarity/analiticity/causality/etc: it would give effects suppressed by 10^16 TeV.

But LHC is sensitive to new operators suppressed at most by 100 TeV. Operators induced by quantum gravity are too small to be seen. Whatever is their sign, it can be approximated with zero: LHC is not testing quantum gravity. LHC is testing QFT. Any QFT (whether or not it comes from strings and whether or not it is the Standard Model) predicts no violation of causality, no Vedic spirits, etc.

Discovering Vedic spirits at LHC would be very interesting, but a paper that tells "if LHC sees Vedic spirits we would have to rethink string theory" is not very interesting.

44. **N. Nakanishi**
January 26, 2007

I'd like to comment that Lorentz invariance can be violated spontaneously in the ordinary framework of local QFT based on the manifestly Lorentz-invariant action. As is well known, the regulator theory can remove all ultraviolet divergences, but it is usually rejected as unphysical because it violates the unitarity of the physical S-matrix. However, this belief is not true if the regulator masses are complex. That is, what is violated in the relativistic complex-ghost field theory is not unitarity but Lorentz invariance! This fact was found 36 years ago. Please see N. N., Prog. Theor. Phys. 116 (2006) 873 or hep-th/0609206. Earlier references are contained therein.

45. **Arun**
January 26, 2007

amused wrote:

Basically, if I understand rightly, if physics at high (Plank scale) energies is described by a quantum theory (of point particles, strings, or whatever) then Lorentz invariance, analyticity and unitarity imply certain positivity constraints in effective QFT descriptions at lower energies, and in particular in the Standard Model. These can apparently be tested in WW scattering, and if they are found to be violated it implies that a quantum theory description at Plank scale (with the aforementioned 3 properties)doesn't exist.

To which I add : doesn't this argument assume that there is nothing beyond the standard model until the Planck scale? If WW scattering at LHC shows something new, then (in the absence of string theorists) wouldn't we be normally looking for new particles at the LHC energy or a little higher rather than drawing implications for the Planck scale?

46. **Peter Woit**
January 26, 2007

Sorry about that G, but the rest of your comment seems to have vanished into the ether. I'm afraid that this blog software, like most others, has problems when you try and use the "less than" symbol. It inteprets it as opening an HTML tag, which never gets closed, so I guess it just deletes the symbol and everything after it. If someone knows of some way the software can be modified to not behave like this, let me know. Otherwise, people should just be warned to be very careful using "less than" or "greater than" symbols, realizing that WordPress may do something very undesirable when it encounters them.

47. **Chris Oakley**
January 26, 2007

Hi Garbage,

One can of course use < for < and > for > as per standard HTML.

48. **amused**
January 27, 2007

Arun,

As I understand it, the argument in the paper doesn't make any assumption about where the new physics shows up; it could be anywhere between the SM and Plank scales (although it shouldn't be too close to the SM scale, otherwise some complications arise according to the paper). The point is that if the physics at some or other high energy scale scale is described by a quantum theory with S-matrix having the usual properties, then that alone implies certain constraints on the effective QFT descriptions of physics at lower energies (specifically, on the coefficients of a couple of terms in the general expression for a chiral Lagrangian describing a spontaneously broken electroweak gauge theory provided the Higgs mass isn't too light). These bounds can apparently be probed by WW scattering at the LHC. Say the bounds are found to be violated in some future experiment at energies above the SM. That would imply that the physics at any higher energy scale, and in particular at the Plank scale, can't be described by a quantum theory with S-matrix having usual properties (otherwise the bounds wouldn't have been violated). Since string theory is supposed to be (a road towards) such a quantum theory, it would be pretty much dead as a consequence.

"press release",

I don't get your point in your last comment. The paper doesn't claim to be saying anything specific about the structure (operators etc) of whatever theory describes physics at the Plank scale.

49. **press release**
January 27, 2007

amused,

here are a few other observations that would similarly falsify current theories (including string theory): a 4th generation of quarks only, a W that moves faster than light, new vectors that do not fill an adjoint representation, perpetual motion, an anti-commuting boson, a new lepton doublet without its neutrino, nonconservation of energy, antigravity, time machines, a fundamental particle with spin 3, etc.
Why do you think that WW scattering is more interesting?

50. **Arun**
January 27, 2007

The abstract says:

"We show that the coefficients of operators in the electroweak chiral Lagrangian can be bounded if the underlying theory obeys the usual assumptions of Lorentz invariance, analyticity, unitarity and crossing to arbitrarily short distances. **Violations of these bounds can be explained by either the existence of new physics below the naive cut-off of the the effective theory, or by the breakdown of one of these assumptions in the short distance theory.** As a corollary, if no light resonances are found, then a measured violation of the bound would falsify generic models of string theory. "

I repeat what I posted earlier, that in the absence of string theorists, we'd greet the violation of the bounds with gleeful cries of new physics below the naive cut-off.

51. **Amos Dettonville**
January 27, 2007

I must say this is an ingenious approach to establishing the falsifiability of a theory, one that I had not thought of before. I wonder if Popper ever thought of it, or commented on it. There are some extremely fundamental premises underlying essentially all scientific thought, and so an observed violation of one or more of those premises would automatically falsify just about every scientific theory. For example, according to my theory, the electrostatic force of a charged particle falls off (at least approximately) as the square of the distance, so if someone discovered that it actually falls off as the cube of the distance, my theory would be falsified. Likewise, according to my theory, it's impossible to construct a perpetual motion machine, so if someone ever constructs a perpetual motion machine, my theory is falsified. I can dream up endless "tests of my theory" this way… but does this really count as falsifiability in the sense of a robust scientific theory?

Maybe the notion of falsifiability should include an extra condition: If a particular empirical finding would falsify not only my theory, but also virtually every other theory, then it shouldn't be counted as a test of my theory in particular. For example, a test of Lorentz covariance, or analyticity, or anything of this fundamental nature, ought to be simply described as tests of those properties. It seems inappropriate to label such tests as "tests of string theory".

I'm actually surprised that the press releases haven't taken the opportunity to call these "Tests to Falsify Einstein's Theory of Relativity!", since a violation of Lorentz covariance would surely be more significant as a violation of relativity (an actual existing theory) than of "string theory" (a hypothetical theory that may or may not actually exist, and that may or may not actually be falsified by this result, depending on what, if anything, the theory turns out to be).

52. **Aaron Bergman**
January 27, 2007

I repeat what I posted earlier, that in the absence of string theorists, we'd greet the violation of the bounds with gleeful cries of new physics below the naive cut-off.

I'd think you'd find that, if you were to talk to string theorists, they would be extremely excited if new physics, any new physics, would be found.

—

[If something would falsify] virtually every other theory, then it shouldn't be counted as a test of my theory in particular.

I'd think a modicum of research would show that there are plenty of attempts by people to construct theories that violate most subsets of unitarity, Lorentz invariance and analyticity. Lee Smolin, to pick an example, seems to believe (at least some portion of the time) that LQG is not Lorentz invariant.

53. **Amos Dettonville**
January 28, 2007

It doesn't take any "research" to know that people have examined the possibility that (for example) Lorentz invariance might be violated. Indeed, Lorentz himself theorized about this. But surely Lorentz invariance is (and has been for quite some time) accepted as one of the best supported empirical facts, and it is implicit in all the *standard* (for lack of a better word) modern theories (as opposed to research porgrams or ideas for theories) of physics. I'd think a modicum of thought would suffice to realize that if Lorentz invariance were ever found to fail, any implications it may (or may not) have for the highly speculative research program known as "string theory" (or LQG etc) would be utterly insignificant compared with the implications for the very foundations of modern physics, i.e., actual theories that actually exist today. A falsification of relativity would be staggering news… but not because of what it does or doesn't say about "string theory".

My point is that it's silly to tout failure of Lorentz invariance as falsification of the hypothetical "string theory" (whatever that may or may not turn out to be), when in fact it would falsify the foundations of all the successful EXISTING theories of physics.

54. **amused**
January 28, 2007

press release,

The odds are against any of those things turning up, don't you think? If it turns out, as many people seem to expect, that string theory can accomodate whatever new physics is found at the LHC, then these bounds at least give one way that it could still potentially be falsified.

Arun,

"I repeat what I posted earlier, that in the absence of string theorists, we'd greet the violation of the bounds with gleeful cries of new physics below the naive cut-off."

If the violation isn't accompanied by the mentioned "light resonances" then the conclusion about breakdown of one of the assumptions on the short-distance theory is inescapable. (And existence of string theorists was not one of those assumptions…)
If the light resonances are there, then the bounds would need to be modified – that's what I was alluding to in the "some complications arise" part of my last comment.

"

55. **N. Nakanishi**
January 28, 2007

One should not confuse the Lorentz invariance of the fundamental action with that of the physical S-matrix. As I emphasized previously (January 27, 2007 at 6.14 am), Lorentz invariance can be violated SPONTANEOUSLY in the ordinary framework of QFT without contradicting the fundamental principle that the action integral is Lorentz invariant. Therefore, this theory can be a theory which coincides with the standard theory in lower energies. Furthermore, if one accepts the violation of Lorentz invariance, it is possible to remove all ultraviolet divergences without violating the unitarity of the physical S-matrix.

56. **woit**
January 28, 2007

Amos,

I think that pretty much everyone except Distler et. al. who has ever thought about what it means to test a scientific theory understands that your test of the theory is supposed to involve characteristic features of that theory, and that you're supposed to actually do a calculation involving such characteristic features in order to come up with anything that can legitimately be called a "test".

The way some string theorists recently seem willing to throw out the most basic things about what it means to do science in order to make rather absurd claims for their research never ceases to amaze me.

57. **Garbage**
January 28, 2007

Peter & Amos,

I disagree with the claim that the bounds are not a test for whatever New Physics (NP) is out there in the UV. The magic of the EFT formalism is that one doesnt really need to know what is it like but what sort of basic principle respects. If the bounds are violated these principles must be re-thought, or the other possibility is NP at a much lower scale than we think.
To partially answer press release at the same time. If these basic assumptions break down naively at the Planck scale, it isnt a priori true that they wont show up at lower scales. For instance, the breaking of LI could generate operators in teh EFT which are not Planck supressed (This is easy to understand once the 1/Mpl gets killed with the UV cuttoff scale).
Unitarity in the other hand goes tied to the notion of evolution. Therefore, it is also possible that Planck scale violations get enhanced by
a mixture of its time and energy dependence (recall the disp relation will not get thru unless SS*=Id).
Yet another possibility is that the fall off of the scattering amplitude does not obey the Froissart bound and, as in the cae of LI, small violations will get amplified by the cutoff scale.

In spite of all the possible scenarios we could come out with, the EFT spirit tells us we can still do some predictions based on simple assumptions without a complete knowledge of the UV completion. If the bounds are violated we will know one or more of these assumptions are wrong, or there is a light resonance out there we havent found. This is a robust prediction which will falsify all the models of NP which rely on such premises. All we know so far of ST falls into that class. LQG or others, might not. This is a non trivial test of the High Energy behavior of scattering amplitudes, needless to say does not conflict with classical GR since, for instance, the assumed LI in LQG are of quantum origin. As such, this is a test of quantum gravity or GUT if you wish, for which ST claims to give a description of…

G

58. **confused**
January 28, 2007

I don’t understand what you’re all so upset about with this paper. The authors note that not all low energy effective field theories can be UV completed to some string theory. This is important and it runs contrary to what most anti-string people claim. Furthermore, it’s not true that any quantum theory of gravity which anyone takes seriously will automatically satisfy unitarity, lorentz invariance, etc – LQG doesn’t. So this seems to provide some nontrivial information about physics beyond the standard model. Obviously this can’t be used to verify string theory but it could, in principle, falsify it (modulo assumptions which look quite reasonable to me). This is EXACTLY what you guys are always complaining about: string theory can’t be falsified. But when somebody presents some small progress in the direction of showing that string theory can be falsified you people all complain about it, rather than praise the authors for at least trying.

As for the hype, yes, of course, the title somewhat overstates the claim but it’s not an outright lie (they don’t need to do a full string theory calculation to know that string theory respects unitarity, etc). All the authors have done is what everybody does: try to motivate people to actually read their paper. Do you also have “ethical” objections to giving your paper a playful/funny title in an attempt to attract attention?

59. **Peter Woit**
January 28, 2007

confused and Garbage,

My problem is not with the actual content of the paper. The authors are claiming to have some bounds which, in the extremely unlikely event they are violated, would indicate the existence of some new physics. The problem is that this is very different than having a “test of string theory”. They don’t actually use string theory in any way at all in their paper, in particular they don’t show that string theory satisfies the assumptions they are making (and no one knows if whatever non-perturbative string theory is, whether it will satisfy these assumptions). It’s quite simple: the paper is simply not about string theory at all, so it can’t in any sense be “progress in the direction of showing that string theory can be falsified”. There’s nothing wrong with trying to show string theory can be falsified, there is something wrong with claiming you can show this when you can’t.

“All the authors have done is what everybody does: try to motivate people to actually read their paper. Do you also have “ethical” objections to giving your paper a playful/funny title in an attempt to attract attention?”

Unfortunately the title of the press release doesn’t seem to be a joke, although it really is one. Yes, I do see an ethical problem with giving a paper a misleading, dishonest title in order to get people to read it and it appears that a PRL referee had the same problem. I see an even bigger problem with issuing misleading and dishonest press releases, which is what the authors have done here.

60. **onymous**
January 28, 2007

no one knows if whatever non-perturbative string theory is, whether it will satisfy these assumptions

You keep saying this, but we do know that string theory is a theory satisfying the rules of quantum mechanics, so I don’t see how unitarity could *ever* be violated by nonperturbative effects. It also seems hard to believe that analyticity would ever be violated, because it encodes causality properties. Lorentz invariance, you might be able to raise questions about, I suppose (maybe you can turn on a small background B field or something?). So if you were going to try to argue that a realistic string theory could violate one of these principles, I guess that Lorentz invariance would be the place to start. In any case, within the paradigm of string theory on 4D Minkowski space times a compact manifold, the assumptions seem pretty much unimpeachable, right? No matter what nonperturbative physics is happening to stabilize the compact manifold, it doesn’t change the far short-distance properties of the theory. “Nonperturbative” doesn’t automatically mean “we have no clue.”

61. **Peter Woit**
January 28, 2007

onymous,

Getting 4D Minkowski space X a stabilized compact manifold is just one aspect of what you hope your non-perturbative string theory, whatever it is, will look like at large distances. You don’t know what is going to happen at arbitrarily short distances. With very little trouble you should be able to find many places where prominent string theorists such as Gross, Witten, Seiberg, etc. go on about how understanding non-perturbative string theory will require us to give up our conventional notions of what space and time are. I don’t see how you can reconcile this with claiming that you know that non-perturbative string theory involves a 4D Minkowski space structure and associated analyticity properties down to arbitrarily short distances. It seems to me that Gross, Witten, Seiberg, etc are saying the exact opposite, and that they would be happy to claim that violation of the bounds in question is evidence for string theory, not that it falsifies it.

62. **Amos Dettonville**
January 28, 2007

Just to be clear: I don't dispute that the violation of Lorentz invariance would falsify one of the basic premises of any theory or any research program that takes Lorentz invariance as one of its basic premises. I'm just observing that this includes essentially all *existing* successful theories, as well as many/most research programs for new theories. Lorentz invariance is a very fundamental feature of existing physical theories, and the fact that its violation would undermine some speculative directions of research into future theories ("new physics") strikes me as secondary to the fact that it would falsify the foundations of all existing theories.

Perhaps one can take the phenomenological view that existing theories are not based on any degree of Lorentz invariance beyond what has been experimentally verified, so if a violation were found outside those limits, it wouldn't affect existing theories. But that seems like a rather obtuse interpretation of current theories. Surely the point of a scientific theory, like relativity, is not just to be a catalogue of known empirical results, but to predict things beyond what has already been measured, and to place the phenomena in a coherent conceptual framework. If a measurement conflicts with the prediction of a theory, even in a region beyond what has been tested before, then the theory is falsified. We may still accept that the theory "works" approximately within a limited range, but it is nevertheless falsified in its previous sense.

I hope we can all agree that relativity predicts that Lorentz invariance is never genuinely violated (more or less by definition), so if a violation is found, relativity is falsified. All I'm saying is that, to me, the falsification of relativity itself would be far more significant than the undermining of one or more speculative research programs based on (among other things) relativity. Notice that the paper in question points to Mattingly's 2005 paper in Living Reviews in *Relativity* for a discussion of possible Lorentz invariance and its implications.

Note to N. Nakanishi: You commented about something that might be termed "violations of Lorentz invariance" in existing QFT, but the authors of the paper in question seem to conceed that the kind of Lorentz invariance they are talking about would in fact conflict with QFT, so they seem to be talking about what I would call a genuine violation of Lorentz invariance (see Mattingly's paper for the meaning of "genuine").

Note to confused: I'm uneasy about giving playful/funny titles to scientific papers "in an attempt to attract attention". I suppose if the title is understood to be an inside joke, then it would be okay, but I don't think it is being presented as a joke when reported in the popular press. Reporters don't seem to "get" the fact that it is a joke, i.e., that the falsification being discussed would undermine not just some avenues of string research but essentially all existing theories of physics. My objection to this practice is that it doesn't just attract attention, or serve as an amusement, it also seems to mislead. I'm all for whimsy, but I think we should strive to avoid using whimsy to mislead people… and there seems little doubt that people have been misled in this case (see all those press releases).

63. **Garbage**
January 28, 2007

"…It seems to me that Gross, Witten, Seiberg, etc are saying the exact opposite, and that they would be happy to claim that violation of the bounds in question is evidence for string theory, not that it falsifies it."

At least not the kind of ST we have so far and that's definitely a powerful claim. If the bounds are violated, and ST wants to stay up there, better start taking those 'give-up' ideas seriously.

In the other hand, there is people who would say ADS/CFT provides a
non-perturbative definition of ST. If that is the case, violations of the bounds would certainly throw that option out of the window, since we know the FT side is well behaved (In fact that is what convinced Hawking that BH evaporation was unitary after all. Although that might not be the case).

The paper uses assumptions about the UV completion of the SM. So far ST is *perhaps* the only theory at hand, which happens to obey these in most of its current forms. Therefore, the bounds are a test for what we know or hope of ST. I agree there is too much of a hype behind this, but yet I see no incorrect statements in the paper nor in the authors' claims in the press.

I would be happy to see more paper like this coming out, and certainly it would be great if the bounds are violated and the string community starts to 'give-up' in any way 🙂

64. **Garbage**
January 28, 2007

Amos,
LI can be violated in the vacuum (or a given state) and yet be there as a full symmetry of the action. In LQG the starting action is Einstein theory, its quantization introduces the foamy structure which might break LI. The LQG people would claim Einstein theory is recovered in the (h goes to 0) classical limit. The same people would tell you that the space of physical state is invariant under the whole diff group, and gauss law (local Lorentz). The same could happen to Unitarity depending on the choice of time variables even though there is an underlying unitary theory.
For the scattering amplitudes, String theory so far doesnt seem to violate LI nor the other assumptions (see the original Adams et al. paper).

G

65. **woit**
January 28, 2007

Garbage,

"At least not the kind of ST we have so far and that's definitely a powerful claim"

The "kind of ST we have so far", perturbative string theory, AdS/CFT, and a few versions of M-theory in special backgrounds, are already falsified. You can't get the standard model as low energy physics out of any of them. Claims that string theory can lead to the standard model have to invoke some hoped for, but not yet understood non-perturbative theory. The question of "is string theory falsifiable" is only non-vacuous if it refers to conjectural versions of string theory that solve the problems of known versions of string theory. For these conjectural versions you can't say one way or another whether they satisfy the conditions at issue.

66. **onymous**
January 28, 2007

"Claims that string theory can lead to the standard model have to invoke some hoped for, but not yet understood non-perturbative theory."

You do know about the work of Braun et al on the heterotic MSSM, yes? Or of Verlinde and Wijnholt? There's not exactly "not yet understood non-perturbative theory" that's crucial there. There's no mystery about the short-distance physics. The models aren't perfect yet, true, but they're not as far from reality as you seem to think.

(Cosmology might require not-yet-understood physics, but I don't see any obvious connection to the short-distance questions we're talking about here.)

67. **woit**
January 28, 2007

onymous,

The models you discuss involve an ad-hoc choice of background, in particular an ad-hoc choice of a 4d Lorentz invariant background. This background is supposed to be determined by the unknown non-perturbative theory fundamental theory. As far as I know, they are not yet successful at reproducing the standard model.

The question is not whether people have looked at models with 4d Lorentz invariance, the question is whether string theory has to lead to models with this invariance, and there is no argument for this.

68. **Garbage**
January 28, 2007

"The question is not whether people have looked at models with 4d Lorentz invariance, the question is whether string theory has to lead to models with this invariance, and there is no argument for this."

There will be if the bounds are violated….forget about it….

G

69. **Bob McNees**
January 28, 2007

I know I'm late to the party, but I feel like I need to comment on this. There are four versions of the paper available at the arXiv. None of them contains the phrase "test of string theory". They do, however, refer to the potential for "falsifying string theory".

I'm going to assume that everyone here understands the difference between these two things.

The "press release" appears to have been put together by UCSD's news service and subsequently picked up by their counterparts at CMU and UT. In fairness, I can see how some of the quotes, taken without reference to either the rest of the press release or the actual paper, deserve to be argued with. But Peter's "most outrageously misleading string theory hype of 2007″ also contains these quotes:

"If the bounds are satisfied, we would still not know that string theory is correct," said Distler. "But, if the bounds are violated, we would know that string theory, as it is currently understood, could not be correct"

"In other words, string theory—as articulated in its current form—would be proven impossible."

Wow. So basically, the most outrageously misleading statement about string theory for 2007 is that an experiment can not confirm string theory, but it can falsify it. How did such a radical idea ever make it through the peer-review process?

I can't imagine why Peter – who is, of course, free to delete this post if he likes – would be upset by the prospect of a legitimate proposal for falsifying string theory (Yes, I know … along with several other theories). I mean, it's not like he's getting royalty checks from a book whose title implies that this is not possible, even in principle.

70. **N. Nakanishi**
January 28, 2007

Reply to Amos Dettonville: You seem to misunderstand my claim.
There are two, mutuall equivalent, perturbation theories, "old" and "Feynman". The latter is manifestly covariant, that is, if the action is Lorentz invariant then the "Feynman" S-matrix is automatically Lorentz invariant, as long as no new situation is encountered. The necessary new situation encountered is the introduction of complex delta function. This is not an artificial procedure. Indeed, if one calculates the S-matrix by means of the "old" perturbation theory, one obtains exactly the same Lorentz-noninvariant result without introducing any new concept.

71. **Amos Dettonville**
January 28, 2007

Reply to N. Nakanishi:

Maybe you can help me understand what you're saying if you would comment on the following quote from Mattingly's 2005 review paper on Lorentz Invariance (which is cited in the "string falsification" paper):

"As we have seen, over the last decade or two a tremendous amount of progress has been made in tests of Lorentz invariance. Currently, we have no experimental evidence that Lorentz symmetry is not an exact symmetry in nature. "

Do you agree or disagree with this statement? Are you saying there IS evidence of violation of Lorentz Invariance? Or are you saying current theory entails such violation, even though it has not yet been detected? Or are you saying something else completely? If the latter, could you explain the relevance to the subject under discussion?

72. **N. Nakanishi**
January 29, 2007

Reply 2 to Amos Dettonville:
I never positively assert the violation of Lorentz invariance, but I think that the main subject of this debate is the possible violation of Lorentz invariance. I believe that the spontaneous violation of Lorentz invariance is much more probable than the extra dimensions, string theory, etc. My aim is to remind people of the fact that even if LHC experiment indicates the violation of Lorentz invariance, it is still possible to explain it in the framework of the manifestly Lorentz-covariant QFT.
(As an experimental evidence of the possible violation of Lorentz invariance, some people point out the observation of extremely high-energy cosmic-ray particles, because they should collide with 3K cosmic background photons.)

73. **woit**
January 29, 2007

Bob,

You totally ignore the scientific argument repeatedly made here that the Distler et. al. paper does not provide a way of falsifying string theory (an argument that the PRL referee seems to have agreed with). While ignoring the scientific issue, your only argument is to make a sleazy accusation that I'm just trying to make money.

Completely pathetic.

74. **Bob McNees**
January 29, 2007

I don't ignore the argument, I disagree with it. Perturbative string theory gives you an S-Matrix with well-defined properties. If you demonstrate a violation of these properties, you have falsified the theory. Yes, a lot of other things go along for the ride. If you see the kinds of violations they discuss, it would be hard to reconcile those observations with conventional QFT. One way of falsifying string theory is showing that properties it should have at all scales are violated at the scales you have just gained access to.

That is a scientific argument. You are free to say "they should come up with something that targets string theory more precisely, as opposed to such a wide-swath of theory space". Sure, that would be great. But it doesn't make their argument unscientific.

Is it the press release you disagree with? The way it might be interpreted in the popular media? Because that's where I think you are being hypocritical. Where was your indignation when the press releases were being issued on your behalf? You just wrote a book describing your views on string theory, and you gave it a title implying that string theory cannot be falsified. The publisher issued press releases and advertisements, as publishers are wont to do, claiming that you *show* that string theory cannot be falsified. I read your book and considered your complaints, and I don't agree that you have demonstrated a "lack of falsifiability". Why do you get to announce that in a press releases?

I don't think you are "just trying to make money". I think you genuinely want people to know what you think about string theory. You just chose a forum that uses advertising and press releases to drum up interest, and now you're upset that it might cut the other way. At least people can download and read the peer-reviewed article for free.

75. **Peter Woit**
January 29, 2007

Bob,

You continue to ignore the scientific argument I was making. Perturbative string theory is already falsified, it makes predictions that disagree with experiment.

I gave my book a title implying that string theory cannot be falsified, because, in its current state, the way it is being pursued, it can't. I'm willing to stand by that claim, and have spent a lot of time discussing and defending it here and elsewhere and will continue to do so.

The marketing materials prepared by a publisher trying to sell a book are not directly comparable to a press release issued by a university on behalf of one of its faculty members, but if you want to make that comparison, fine. I don't think I've seen everything the marketing people at my two publishers have written and may be using, but I am willing to stand behind everything that they've shown me and asked me to look over. This is mainly the jacket copy on the books. I argued with the US publisher and insisted that they use language that, while it was not the way I would want to present my arguments, was language that I could stand behind and back up. I assume that the authors of this paper similarly are willing to stand behind what has been issued in their name. If they're not, it would be a good idea for them to say so publicly.

It's a very simple distinction: String theory IS "not even wrong", it is not testable in its current state, and honest researchers in the field acknowledge this.

76. **D R Lunsford**
January 29, 2007

Bob McNees said

None of them contains the phrase "test of string theory". They do, however, refer to the potential for "falsifying string theory". I'm going to assume that everyone here understands the difference between these two things.

This is just sophistry. There is no difference other than one of language. I am going to assume you know the difference between actual physical experimentation and the vagaries of language.

-drl

77. **Jack Lothian**
January 29, 2007

My wife has had hundreds of press releases issued in her name & she has read everyone of them before release & edited most of them before release. This is a pretty common practice. The idea that press releases are generated by faceless gnomes who never communicate with the so-called authors of the release is not a true picture of how most press releases are issued. Shame on you for suggesting this, even our lowest politicians know that they can not get away with this kind of defense. It ranks right up there with the dog ate my homework excuse. I believe the odds are that these authors saw this press release before it was sent out & they approved it. Thus the authors publically endorsed the view that their paper presented a "test of string theory".

I agree with Lunsford that your arguments are a form of sophistry.

78. **urs**
January 30, 2007

Peter wrote:

> Perturbative string theory […] makes predictions […].
>
> #

🙂

79. **Q**
January 30, 2007

The first […] is 'is already falsified,'
The second […] 'that disagree with experiment'.

It's supersymmetric unification scheme predicts a massive cosmological constant.

The SU(3) force predicted by string for low energy, using the known SU(2) and U(1) force strengths, is wrong by current experimental data (four times outside the experimental standard deviation).

Should Peter have called his book simply ‘Wrong’? ‘Not Even Wrong’ refers only to the non-falsifiable ‘predictions’ like unification of all forces at an energy of 10^19 GeV, extra dimensions rolled up in Planck scale manifolds, prediction of gravitons, superpartners, branes, etc., that can’t be checked falsifiably by experimentation…

80. **woit**
January 30, 2007

Urs,

If you actually have an argument, it would be helpful if you would make it. I’ve made one: purely perturbative string theory doesn’t give standard model physics, it’s wrong. Attempts to get around this by invoking complicated backgrounds, generated by non-perturbative effects like branes are non-predictive and not even wrong. If you have an answer to this argument, let’s hear it.

81. **Thomas Larsson**
January 30, 2007

Wrong, completely wrong, not even wrong.

Bosonic string, superstring, M-theory.

82. **John Gonsowski**
January 31, 2007

Let’s say Gross comes up with some very good math to link string theory to QCD (perhaps giving QCD it’s own emergent spacetime while string theory has some other spacetime)… would this then allow Gross to justifiably claim QCD as a test of string theory?

83. **Peter Woit**
January 31, 2007

The “test of string theory” discussed here has nothing to do with a string theory dual for QCD. There may very well be such a thing, and if it can be shown to be exactly dual to QCD, then every test of QCD would be a test of that string theory. The claims in the Distler et al. paper are not about this, but about testing the idea of string theory as a unified theory of particle physics and gravity

84. **Arun**
February 2, 2007

What’s your opinion of this “verification of string theory” method?
http://link.aps.org/abstract/PRL/v98/e051301
doi:10.1103/PhysRevLett.98.051301

85. **woit**
February 2, 2007

Arun,

I haven’t looked at the paper very carefully, but I guess Shiu/Underwood. are claiming that in certain brane-inflation models, for certain ranges of parameters, you can in principle see effects of the warped compactification on the CMB. I have no idea whether these effects are something one could imagine practically measuring anytime in the forseeable future for realistic parameter values.

They don’t claim in the PRL paper to be able to “test string theory”, but they have issued a press release, which seems to me an unwise thing for theorists to do to promote this kind of very speculative result:

http://www.news.wisc.edu/13422.html

The press release is entitled “Physicists find a way to “see” extra dimensions”, which is nowhere near as misleading as the Distler et. al. press releases. It does contain claims about string theory that are likely to mislead people, especially Shiu’s quote that “This provides a rare opportunity in which string theory can be tested.” It would have been a good idea for him to make clear that string theory is not being tested, but a very specific “string-inspired” model of extra dimensions, and that not seeing the effects they study (which is extremely likely) doesn’t in any way provide evidence against string theory.

This is kind of like the endless claims one heard after Randall-Sundrum that the LHC would “test string theory” since in principle extra dimensions could be of TeV scale, and in principle they could come from string theory. People seem to have stopped making that particular “test of string theory” claim for one reason or another.

It will be interesting to see how many inaccurate press stories are generated by this press release.

Links, Links, Links…

January 25, 2007
Categories: Uncategorized

Too much going on, hard to keep up with all the things that seem worth mentioning. Here are some quick ones:

There was a meeting on Geometric Langlands involving mathematicians and physicists last week at the Schrodinger Institute in Vienna. David Ben-Zvi has notes for the talks.

There's an interview with John Baez and Urs Schreiber, partly about blogging. I personally take credit for first referring to him as a "proto-blogger", thus making him feel bad (although it was intended as a compliment…) and encouraging him to modernize:

So, I started getting a little frustrated about being called a 'proto-blogger'. I would joke that I felt like being introduced as like, 'Homo erectus: Very smart for its time, with the first stone tools'. It made me feel sort of old!

Victor Rivelles reports from the Latin American String School in Bariloche. Notes are online.

Michael Creutz has a new paper claiming that rooted fermions are not just "ugly" or "bad", but "evil". See here for some commentary on The Evil That is Rooting.

William Fulton is giving an excellent series of lectures here at Columbia every Friday afternoon, on the topic of *Equivariant Cohomology in Algebraic Geometry*. Ex-Columbia undergrad Dave Anderson, now Fulton's student at Michigan, is writing up notes.

There's a proposed new newsgroup "sci.physics.foundations", which some readers here might find interesting, and which would be a better place for a lot of the discussions which people try to start here, which I then try and stop because I don't want to moderate them. More about this here, and some discussion here.

Comments

1. **Cynthia**
   January 25, 2007

   Most ironically, perhaps, the Earth's axis of evil (the stuff of mankind, no doubt) is spilling over into the greater realm of Universe. God only knows, I suppose.;)

2. **Jay R. Yablon**
   January 25, 2007

   Thanks, Peter, for letting folks know about the new Usenet group "sci.physics.foundations" which we are seeking to start up in the near future.

   A primary goal of our group is to use the power of the internet as a platform for scientific collaboration on an international basis among serious physics researchers seeking to gain a deeper understanding of nature. We are hoping that this group will evolve into a model of enlightened, mutually-supportive scientific collaboration, going beyond the "gotcha" and one-upsmanship mentality that has so pervaded many other physics discussion groups.

   The moderators and proponents all believe that conservative scientific method is fundamental to scientific discovery and advancement. This includes the persistence to conduct trials, the courage to make and admit errors, the open-mindedness and flexibility to correct errors, and the recognition and acceptance that in the end, nature, not personal philosophy or predisposition, popular fashion or paradigm, or greatest availability of research funding, is the ultimate arbiter of whether our theories are correct. In part, nature presents herself via other knowledgeable people of good will who are willing to look at someone else's theories-in-trial, collaboratively point out what nature has already validated to be true which is possibly being overlooked or contradicted, and kindly suggest corrective measures. As such, with the goal of contributing to the advancement of scientific knowledge, sci.physics.foundations will make use of the newly-available power of the internet to augment "feedback" to serious research during its formative, stages of trial, error, and correction, from people of like-interest around the world.

   Please review our RFD at http://vacuum-physics.com/spf/, post your support at http://groups.google.com/group/news.groups.proposals/browse_frm/thread/aa4bfd6d8bf61111/6f20a6d40961a4b6#6f20a6d40961a4b6, and then join us the collaboration once sci.physics.foundations begins.

3. **mclaren**
   January 25, 2007

   Apropos of nothing in particular, a pop sci discussion of string theory in Wired magazine (that notable peer-reviewed scientific journal :->) which once again gets it mostly wrong and glosses over far too much, here:
   http://www.wired.com/wired/archive/15.02/bigquestions.html?pg=3#messy

   (Scroll down to "Why Is fundamental physics so messy?")

   At least the author sounds more skeptical than was the case for pop sci reporters a few years ago. Though how anyone can call "fundamental" current models of physics, which systematically fail to successfully unify the Planck scale with GR, remains unexplained.

4. **Christine**
   January 26, 2007

   For what is worth, at LASS07 I could only count 2 women participating out of 100 people (although I could have missed some with abbreviated first names)… Is this the usual rate at other string schools?

5. **Chris W.**
   January 26, 2007

   McLaren, note the author of that little piece—science writer George Johnson. Brian Greene and James Gleick also contributed to the article ("The Big Questions") among others.

6. **Q**
   January 26, 2007

   Proof that female physicists value predictions and evidence?

7. **D R Lunsford**
January 27, 2007

   Thanks Peter for mentioning SPF – any interested readers should go to news.groups.proposals and offer support.

   -drl

8. **D R Lunsford**
January 29, 2007

   What we need is a few good patent clerks.

   -drl

9. **Jonathan Vos Post**
January 29, 2007

   Regarding what Christine says, my wife is a Physics professor, also with first name Christine. She and I anecdotally suspect that Physics is still the most male-dominated field of science. Does anyone have current data which supports this (or alternatively disproves it), such as APS surveys?

10. **Chris W.**
January 29, 2007

    In keeping with the eclectic nature of this post, there is a new paper on the arXiv with a peculiar single-word title, **O'KKLT**
    . The authors are well-known, and the subject matter is quite interesting.

11. **Chris W.**
January 29, 2007

    Correction: It's not that new. (*Duh*) It was just updated, but was originally posted on 16 November.

12. **alex**
January 30, 2007

    Sorry to interrupt such an intelligent discussion about women in physics, but…

    The message that pops up when I put the cursor over a topic in the "Latest Comments" sidebar now says the last comment was "37 years and 1 month ago".

13. **woit**
January 30, 2007

    [comment about Lubos Motl's views on women deleted]

    alex,

    Will look into that bug. If anyone expert on wordpress and the "latest comments plugin" has an idea what the problem might be, let me know.

14. **mclaren**
January 30, 2007

    The problem is obvious. The comment in question was travelling at very nearly the speed of light.

Short Items

January 30, 2007
Categories: Uncategorized

Discover Magazine has just announced a competition, calling on people to submit videos to them that "clearly explain perhaps the most baffling idea in the history of the world: string theory". The challenge is called String Theory in Two Minutes or Less, and the winner gets featured in an upcoming issue of Discover. In related news, one of my correspondents suggested making my postings available as YouTube videos, but I think I'll resist any temptation to go that route.

Michael Peskin was here at Columbia yesterday, giving the physics colloquium, which was mainly about prospects for detecting at the LHC the kind of supersymmetric WIMP that is supposed to make up dark matter. On his web-site there are slides which more or less correspond to the talk he gave. The bottom line is that he believes that over the next 5-10 years we'll be seeing evidence for such a WIMP from all of three different sources: astronomy (GLAST), direct detection experiments, and the LHC. The claim is that the LHC should be able to detect the existence of such a particle (although it's not easy…) and maybe even measure the mass to 10 percent.

Experimental HEP bloggers keep putting out gripping multi-part stories about what it's like to be dealing with collider data that is not conclusive, but has anomalies that promise the possibility of something new and exciting. See the latest from John Conway and Tommaso Dorigo.

There's a new mathematician's blog out there, John Armstrong's The Unapologetic Mathematician. He promises "I'm sure I can come up with a good rant once a week or so. Actually, I'll set that as a goal."

NPR's last Science Friday program dealt with experimental HEP physics, featuring David Barney (CMS), Jacobo Konigsberg (CDF) and Barry Barish (ILC).

I managed to get to a few of the talks at last week's City College workshop on non-perturbative Yang-Mills that was mentioned here recently. Unfortunately I couldn't get up there on Friday and missed talks by Maldacena and Freidel that I would have liked to see, but did make it to some of the talks on Thursday and Saturday. It appears that progress in 3+1d remains limited, but quite a lot of work is going on with new analytical methods for dealing with 2+1d, which can be tested by comparison with extensive results from lattice gauge theory computer simulations.

Max Karoubi has a new paper on the arXiv, Twisted K-theory, old and new. It traces the origins of the subject back to nearly 40 years ago, explaining the original mathematical motivations, old and new results, and relations between them.

Over at edge.org, my friend Nathan Myhrvold has his photos and an essay about penguins. OK, besides Nathan's background in the quantum gravity business, this has nothing to do with math or physics. But the things are damn cute…

Update: A commenter points out that I should also advertise a bit an event taking place downtown here in New York next week. I'll be talking at the Cafe Scientifique, which will take place at 7:30 next Tuesday evening, at the Rialto Restaurant in Soho.

Comments

1. **Long John Silver**
   January 30, 2007

   >Discover Magazine has just announced a competition, calling on >people to submit videos to them that "clearly explain perhaps the >most baffling idea in the history of the world: string theory". The >challenge is called String Theory in Two Minutes or Less

   Sounds a bit like All-England Summarize Proust Competition.

2. **Kea**
   January 30, 2007

   The competition is only for Americans (read the terms).

3. **John Armstrong**
   January 30, 2007

   Thanks for the nod. Hopefully I can manage to keep up with this thing.

   and LJS:
   Green, in his first book, wrote about, wrote about fa la la
   Green, in his first book, wrote about, wrote about
   He wrote about
   Green, in his first book
   He wrote about….

4. **Q**
   January 30, 2007

   'The winning video will be selected by Columbia University physicist Brian Greene, best-selling author of The Elegant Universe and The Fabric of the Cosmos, and broadcast via a prominent spot on the homepage of Discover.com.' – http://www.discover.com/twominutesorless/

   I hope that someone who is really deserving of the publicity and hype wins. (Say, someone really expert, with a Czech accent.)

5. **Sebastian Thaler**
   January 30, 2007

   Peter-

   Another item worth mentioning for the benefit of your readers in the NYC area: a certain critic of string theory is scheduled to appear at Cafe Scientifique in lower Manhattan on the evening of February 6th. It should be a good time; the speaker has an interesting blog….

6. **woit**

January 30, 2007

Thanks Sebastian,

I'd forgotten that I did intend to mention that again here, will add something on to the posting.

7. **mclaren**
January 30, 2007

Really, this sounds like Monty Python's "Semaphore version of Wuthering Heights."

8. **Chris Oakley**
January 31, 2007

Peter –

I would love to go to your talk, but unfortunately will not be in New York on that day. This title: "Not Even Wrong: The Failure of String Theory And the Search for Unity in Physical Law" is a bit of a mouthful. Is it too late to change it to just "Is String Theory bullsh*t?"

9. **Bee**
January 31, 2007

well. that's how progress is made. why study a decade when you can just look at some 2-min videos at YouTube, and everything is explained? it seems to me the adjective 'baffling' was very carefully chosen.

Thanks for the penguin-link 🙂

10. **Thomas Love**
January 31, 2007

I thought the objective was to put the entire final theory on the back of a T-shirt.

11. **Seth**
January 31, 2007

As a student on ATLAS, I'm curious what you meant by your comment that "it's not easy" to detect the existence of supersymmetric WIMPs at the LHC. I admit it's very possible I'm missing something, but I had thought that simply finding WIMPs was a much easier (and quicker) study than many others at the LHC.

It's true in some sense that nothing in particle physics is easy, but as the slide in Peskin's talk notes (on page 58), we do think we have a handle on kinematic variables that will point to the existence of such particles. In particular, the effective mass (usually the scalar sum of missing transverse energy and the top four jets) plots often look rather different than the Standard Model, even with relatively little data.

There's a lot of active work being done on how best to do these studies. There are refinements that may improve the situation considerably, like playing with the details of the cuts or the exact variable one plots. There are also problems that need to be understood better, in particular having a firm estimate of the standard model background and the error on that estimate.

The search for heavy non-interacting particles is one that can be done even with fairly early LHC data, and might yield promising results quite quickly. Other studies, like the ones analagous to the Tevatron "bump-hunting" that you've linked to, will require much more care and better estimates of systematic errors, and so would seem to be much harder.

Figuring out what kind of WIMP we're dealing with, and maybe measuring its mass—now *those* things will be hard.

12. **Peter Woit**
January 31, 2007

Seth,

The reference to "not easy" was just a reference to the general difficulty of extracting this kind of signal, not a claim that it was difficult compared to lots of other signals people are looking for, which are even harder. Peskin in his talk was emphasizing the huge sizes of the backgrounds to be dealt with at the LHC (perhaps because he does want people to keep in mind the case for the ILC).

Thanks for your informative comment about this!

13. **Tommaso Dorigo**
February 2, 2007

From Chad Orzel's blog:

"You'll be happy to know that Peter Woit has already bowed out (suggested concept: standing in front of a whiteboard, hopping up and down, and yelling, "It's crap! Crap crap crap crap crap!" for two minutes)."

Where did you suggest that Peter ? I must be missing something.

Cheers,
T.

14. **D R Lunsford**
February 2, 2007

Chris O – or "Not Even Brown"

-drl

SLAC 2006 Topcites

February 1, 2007
Categories: Uncategorized

A couple weeks ago I generated a list of the theoretical physics papers that were most heavily cited during 2006, according to the SLAC database, and discussed it here. Today the people at SLAC put out their own lists for 2006, which are quite interesting to go through. Their data is quite consistent with mine, although the numbers are very slightly larger, since a small number of new citations have been added into the database over the last couple weeks, I think mainly from papers which for example appeared on the arXiv in January 2007, but carried 2006 dates (e.g. write-ups from 2006 conferences).

The main list covering all HEP papers is dominated these days by astrophysics-related papers. Out of the 50 papers on the list I count only about 15 particle theory papers, and 3 review articles. The only post-1999 hep-th paper that makes the list is the KKLT landscape paper. To make the top 50, a paper needed to get 152 citations or more.

The list I put together goes deeper, down to papers with 100 citations, but SLAC has also put out something even better: 2006 lists of 50 most-heavily cited hep-th and hep-ph papers. To make the hep-th list, a paper had to have 62 or more citations. Looking over the 20 or so post-2000 papers on this list gives a good idea of what topics have been popular in recent years: the landscape, dark energy, and various aspects of AdS/CFT, and a small number of other topics. There are also several review papers on the list. Anyone interested in understanding what topics are attracting attention in particle theory research these days should find it quite interesting to go through this list, and spend some time taking a look at and learning about any of these papers that are unfamiliar.

Comments

1. **Bee**
   February 1, 2007

   thought you might be interested, in case you haven't yet seen

   New particle accelerator could rule out string theory
   22:04 01 February 2007
   NewScientist.com news service

2. **Peter Woit**
   February 1, 2007

   Thanks Bee, I hadn't seen that particular piece of string theory hype, will add it to the relevant posting. To relate to the current topic, the paper in question has been cited a total of 5 times since it appeared last April.

3. **gunpowder&noodles**
   February 1, 2007

   Do many string theorists really deny that the subject is in poor shape these days? The only one I can think of is Clifford Johnson. Most of the ones I know freely admit that there is a stalemate, but claim [I think rightly] that there is no alternative on the market……

4. **woit**
   February 1, 2007

   gunpowder,

   I've found the last few years that when I speak to string theorists privately they're willing to admit that things are not going well, but they are very loathe to say this publicly. The active string theorists with blogs (Clifford/Jacques/Lubos) all publicly claim that things are fine with string theory, that at most what is going on is that the field is a bit less active than at some other times.

   I agree there's no obvious alternative on the market (as far as particle theory goes, for quantum gravity there's LQG). If there were a promising alternative idea particle physics unification, string theorists by now would be willing to jump ship. The problem is how do you encourage people to do the kind of work necessary to come up with alternatives?

5. **Garbage**
   February 2, 2007

   "To relate to the current topic, the paper in question has been cited a total of 5 times since it appeared last April."

   Well, the final (published) version if I recall right came out in october…LHC yet to come!
   Nevertheless, it is kinda fun Peter makes such remark. I cant even imagine the type of ranting he would spit out if it had more than 100+ by now 🙂
   The article in the New Scientist is however really poor I admit. If it makes it into 'the economics' then I would start to worry 🙂

   G

6. **Pindare**
   February 2, 2007

   Those who think there is no alternative on the market should perhaps have a closer look at the recent work of Connes, Chamseddine, Marcoli, Barrett, Cartier…

   http://arxiv.org/abs/hep-th/0610241
   http://arxiv.org/abs/hep-th/0608221
   http://www.ihes.fr/IHES/Scientifique/Seminaires/seminaire.php?theme=3

   In a recent video interview available here
   http://www.arte.tv/fr/connaissance-decouverte/science/1350636.html

   Connes says (chapter 18 at 3:50, translated from french): "[…]when you look at the table which I have made […] you realize that the dimension of a point is not zero, it is six! […] This is somehow consistent whith what people do in string theory *but* they look for an ordinary space of dimension 6 while the space that one finds with the formula is not an ordinary space, it is a non-commutative space, so they could not find it. […] it is of metric dimension 0 but of dimension 6 in the sense of K-theory […]"

He then says they make predictions (Higgs, Top) which must be tested in two years time by LHC data to see wether or not their model is right.

7. **Who**
February 2, 2007

as a way to track the quality of string research activity we can count the number of recent string papers that made the "top 50" list last year and compare this with how many made it in previous years.

As you mention, in 2006 only one recent string paper made the top 50. By recent I arbitrarily mean published in the last five years: 2002-2006.

However in 2000 I count TWENTY that made the top 50 list. Here by recent, published in the past five years, I mean a 1996-2000 publication date. Going from twenty down to one is a remarkable decline.

8. **Peter Woit**
February 2, 2007

Who,

To be fair, only in recent years has SPIRES included the astrophysics data. If you exclude astrophysics papers, I suspect a couple more particle theory papers would make the top 50. Still, the change since 2000 has been extremely dramatic.

9. **mclaren**
February 2, 2007

"The active string theorists with blogs (Clifford/Jacques/Lubos) all publicly claim that things are fine with string theory, that at most what is going on is that the field is a bit less active than at some other times."

That parrot's not dead! He's just sleeping!

10. **Who**
February 2, 2007

lovely and apt Pythonism 🙂
John Cleese: This is an EX-PARROT!
http://www.mtholyoke.edu/~ebarnes/python/dead-parrot.htm

11. **woit**
February 2, 2007

I can't resist referring to an ancient piece of creative writing of my own:

http://www.math.columbia.edu/~woit/wordpress/?p=9

12. **Who**
February 2, 2007

From the looks of it, astrophysics has been included since 2001 (and in Peskin's reviews for 2002 and 2003 what gets top billing is already cosmology, after which neutrinos. So I think it is fair to compare the years
2001-2006 (the top 50 have been determined on about the same basis in each year)

2001: 14 recent string papers in top 50
2002: 13 papers
2003: 8 papers
2004: 3 papers
2005: 3 papers
2006: 1 paper, the one you mentioned by KKLT.

recent defined as published in the past five years, e.g. in 2001, published sometime in 1997-2001
Over this time period I don't think the decline can be ascribed to the inclusion of astro-ph because that was done throughout.
Good point though. I will exclude 2000 and earlier, since on quick inspection astro doesn't seem to have been included before 2001.

Shameless Enthusiasm

February 1, 2007
Categories: Uncategorized

The write-up of Larry McLerran's summary talk at Quark Matter 2006 has now appeared. This talk created a bit of a stir since McLerran was rather critical of the way string theorists have been overhyping the application of string theory to heavy-ion collisions.

McLerran explains in the last section of his paper the main problem, that N=4 supersymmetric Yang-Mills is a quite different theory than QCD, listing the ways in which they differ, then going on to write:

Even in lowest order strong coupling computations it is very speculative to make relationships between this theory and QCD, because of the above. It is much more difficult to relate non-leading computations to QCD… The AdS/CFT correspondence is probably best thought of as a discovery tool with limited resolving power. An example is the eta/s computation. The discovery of the bound on eta/s could be argued to be verified by an independent argument, as a consequence of the deBroglie wavelength of particles becoming of the order of mean free paths. It is a theoretical discovery but its direct applicability to heavy ion collisions remains to be shown.

McLerran goes on to make a more general and positive point about this situation:

The advocates of the AdS/CFT correspondence are shameless enthusiasts, and this is not a bad thing. Any theoretical physicist who is not, is surely in the wrong field. Such enthusiasm will hopefully be balanced by commensurate skepticism.

I think he's got it about right: shameless enthusiasm has a legitimate place in science (as long as it's not too shameless), but it needs to be counterbalanced by an equal degree of skeptical thinking. If shameless enthusiasts are going to hawk their wares in public, the public needs to hear an equal amount of informed skepticism.

Another shamelessly enthusiastic string theorist, Barton Zwiebach, has been giving a series of promotional lectures at CERN entitled String Theory For Pedestrians, which have been covered over at the Resonaances blog.

Zwiebach's lectures are on-line (both transparencies and video), and included much shameless enthusiasm for the claims about AdS/CFT and heavy-ion physics that McLerran discusses. His last talk includes similar shameless enthusiasm for studying the Landscape and trying to get particle physics out of it. He describes intersecting D-brane models, making much of the fact that, after many years of effort, people finally managed to construct contrived (his language, not mine, see page 346 of his undergraduate textbook) models that reproduce the Standard Model gauge groups and choices of particle representations. Besides the highly contrived nature of these models, one problem with this is that it's not even clear one wants to reproduce the SM particle structure. Ideally one would like to get a slightly different structure, predicting new particles that would be visible at higher energies such as will become available at the LHC. Zwiebach does admit that these contrived constructions don't even begin to deal with supersymmetry-breaking and particle masses, leaving all particles massless.

He describes himself as not at all pessimistic about the problems created by the Landscape, with the possibility that there are vast numbers of models that agree to within experimental accuracy with everything we can measure, thus making it unclear how to predict anything, as only "somewhat disappointing". He expects that, with input from the LHC and Cosmology, within 10 years we'll have "fully realistic" unified string theory models of particle physics.

The video of his last talk ran out in the middle, just as he was starting to denounce my book and Lee Smolin's, saying that he had to discuss LQG for "sociological" reasons, making clear that he thought there wasn't a scientific reason to talk about it. I can't tell how the talk ended; the blogger at Resonaances makes a mysterious comment about honey…

Finally, it seems that tomorrow across town at Rockefeller University, Dorian Devins will be moderating a discussion of Beyond the Facts in Sciences: Theory, Speculation, Hyperbole, Distortion. It looks like the main topic is shameless enthusiasm amongst life sciences researchers, with one of the panelists the philosopher Harry Frankfurt, author of the recent best-selling book with a title that many newspapers refused to print.

Update: Lubos brings us the news that he's sure the video of the Zwiebach lectures was "cut off by whackos" who wanted to suppress Zwiebach's explanation of what is wrong with LQG.

Update: CERN has put up the remaining few minutes of the Zwiebach video.

Comments

1. **Robert**
   February 2, 2007

   I am not saying that everything is solved in intersecting brane worlds and I would agree that they are only understood as long as susy is unbroken. But if you say that all particles are massless you should say the same about the all the fermions in the standard model. Only the Higgs has a genuine mass terms.

   But just as in the standard model, there are yukawa type couplings between the higgs and the fermions in intersecting brane worlds. Once the Higgs takes a vev (and this is allowed by the equations of motion) the fermions are massive.

   It is one of the features of intersecting brane worlds that left and right handed fermions and the higgs all live at different intersections of pairs of stacks of branes and there is a triangle of branes which has these three particles living at its corner. You can stretch a (euclidean) string (instanton) over this triangle and this gives the yukawa coupling in the low energy theory proportional to exp of minus the area of this triangle. As the families are supposed to arise from multiple intersections of the stacks as they wrap around the torus where all this is living, triangles of roughly equal geometric area give rise to exponentially different yukawa couplings, a feature many find attractive.

   You can say many things about why intersecting brane worlds are not realistic (susy, loads of scalars from bulk moduli) but the mass pattern is one of the pros of these models.

2. **D R Lunsford**
   February 2, 2007

   "String Theory for Pedestrians"

   I suppose this is a typically post-modernist reference to "Lie Groups for Pedestrians", the semi-famous and mega-annoying book by Harry Lipkin. I remember thinking the title offensive, did not regard myself as any kind of pedestrian, and didn't need a boy scout to escort me across the street of math-physics to the far curb of understanding. I think that book was the beginning of the end of my positive attitude about modern mathematics and its practitioners. Instead I decided to learn from fellow pedestrians Lie and Klein.

-drl

3. **D R Lunsford**
February 2, 2007

Looking through the transparencies, one finds the following typical example of sophistry in action:

The String Theory Landscape

String theory shares with Einstein's gravity a problematic feature.

Einstein's equations of gravitation admit many cosmological solutions. Each solution represents a consistent universe, but only one of them represents our observable universe.

Hmm, the last time I checked, this is just an initial-boundary value problem for a set of differential equations combined with a set of massively iffy assumptions about the early conditions in the Universe. What this has to do with the metaphysical ethos of the string landscape is beyond my pedestrian ability to understand. Hey! what did I just step in? TAXI!!

-drl

4. **urs**
February 2, 2007

But that's precisely what that "landscape" is: a space of solutions to something generalizing Einstein's equations.

5. **Peter Woit**
February 2, 2007

Urs,
That's great news. Perhaps you can let us know exactly what the equations are that generalize G=8piT so we can use them to make predictions about the results of experiments.

6. **urs**
February 2, 2007

A string vacuum is a certain 2dCFT. "Geometric string vacua" describing 2dCFTs that can be written as sigma-models are determined by what is called the "string background field equations", which are something like Einstein-Dilaton-Maxwell gravity with various other fields — like those infanmous "fluxes" — and various higher correction terms.

In principle, the "landscape" is the space of solutions of these field equations.

Like in any KK-like theory, physics in 4d is sdetermined by how these solutions look like in the compact directions. There are many such solutions, hence many ways to have physics in 4d.

7. **woit**
February 2, 2007

urs,

From your description, it appears to me that not only do you not know what the exact equations in question are, but you don't even know what exactly what space the equations live on. Perhaps, instead of just telling us that the equations that determine a string background are called the "string background field equations", you could write them out explicitly, or point us to somewhere where this has already been done.

8. **The man**
February 2, 2007

Hey Peter,

Urs is spewing nonsense, but your sneering attitude towards him in the last few posts can't help but remind me of Clifford's attitude towards you in his "tempest" posts.

9. **woit**
February 2, 2007

The man,

Thanks for the good advice.

I do get frustrated by the continuing mystification that surrounds the issue of the current state of attempts to get unified particle physics out of string theory. Opinions can reasonably differ about hopes for the future, but the question of what is now known about string theory ground states is a straight-forward one, and string theorists should stop "spewing nonsense" on the subject.

10. **Q**
February 2, 2007

Asking questions for clarification is not sneering. Clifford, Lubos and Jacques certainly attack because they can't psychologically face string theory's failure as physics.

If you want to see real sneering, ask one of those string theorists about alternatives like Smolin's work.

11. **z3**
February 2, 2007

Another “test of string theory” paper appearing on PRL, by Gary Shui et al. This time, it’s using CMB data to measure the geometry of compact dimensions.

12. **woit**
    February 2, 2007

    z3,
    See my comment about this week’s “test of string theory” in the other thread.

13. **paul**
    February 3, 2007

    Has anyone read this article?
    http://www.ccnmag.com/news.php?id=4789

    Sounds very promising!

14. **Ari Heikkinen**
    February 3, 2007

    You actually make a very good point about the “mystification” issue.

    As far as string theory is concerned, if you ask for something with actual equations, they’d give you a long list of big (and expensive) books to read and then suggest no one would understand them anyway unless they take 5-10 years courses in somewhere like Harvard (and it works, I’m still curious about what’s so special there in string theory math).

    So my question still remains: is there any single (self contained) book (or whatever) with equations, say something like 500+ pages that would explain what’s in Greene’s book with actual equations? There’s been many suggestions, but buying them all would surely be waste of money..

15. **a.n. onymous**
    February 3, 2007

    No, it wouldn’t be a waste of money from the perspective of the authors of those books. You can’t have too much learning…

16. **woit**
    February 3, 2007

    Ari,

    I haven’t actually seen the book, but from its table of contents the new Becker-Becker-Schwarz book sounds like the closest thing to what you are looking for. It is 700 pages of difficult material, and really understanding this is definitely a multi-year project. Then there’s the problem that the book doesn’t actually seem to really explain how bad and generic the problems of this research program are, or that any contact with experiment is at best far in the future, at a time-scale that is receding into the distance…

17. **luzo**
    February 3, 2007

    “It should be said at the outset that, as of yet, there has been no experimental verification of string theory. Also, no sharp prediction has been derived from string theory that could help us decide if string theory is correct.”

    Quote from Zwiebach’s book (chapter 1.3 right at the beginning).
    This seems very reasonable to me and in stark contrast with say Kaku’s claim that one could prove string theory using only pure mathematics.

    The part I’ve seen from his lecture at CERN follows the same spirit as this quote from the book.

18. **Uncle Enzo**
    February 4, 2007

    About this CMB test of string theory, first of all you need sensitive enough data. Let’s suppose you have this. Doesn’t the article assume there are extra dimensions in the first place? Obviously you can come up with another explanation for the CMB patterns which don’t assume extra dimensions. Plus, let’s suppose you narrow down the possibilities of geometries with the data. If the resulting 4D physics (at low energies) doesn’t match what we observe, then there’s a piece of evidence falsifying string theory.

19. **Q**
    February 4, 2007

    ‘If the resulting 4D physics (at low energies) doesn’t match what we observe, then there’s a piece of evidence falsifying string theory.’

    But string theorists can take all falsification to be a mere ‘anomaly’, as they already do with failures of string theory already (supersymmetry falsely suggests a really massive cosmological constant, for example). Science doesn’t come from experimental disagreement, because the theory gets modified, just as general relativity getting a small positive CC in 1998 to fit observations:

    ‘Scientists have thick skins. They do not abandon a theory merely because facts contradict it. They normally either invent some rescue hypothesis to explain what they then call a mere anomaly or, if they cannot explain the anomaly, they ignore it, and direct their attention to other problems. Note that scientists talk about anomalies, recalcitrant instances, not refutations. History of science, of course, is full of accounts of how crucial experiments allegedly killed theories. But such accounts are fabricated long after the theory had been abandoned. … What really count are dramatic, unexpected, stunning predictions: a few of them are enough to tilt the balance; where theory lags behind the facts, we are dealing with miserable degenerating research programmes. Now, how do scientific revolutions come about? If we have two rival research programmes, and one is progressing while the other is degenerating, scientists tend to join the progressive programme. This is the rationale of scientific revolutions. … Criticism is not a Popperian quick kill, by refutation. Important criticism is always constructive: there is no refutation without a better theory. Kuhn is wrong in thinking that scientific revolutions are sudden, irrational changes in vision. The history of science refutes both Popper and Kuhn: on close inspection both Popperian crucial experiments and Kuhnian revolutions turn out to be myths: what normally happens is that progressive research programmes replace degenerating ones.’

    – Imre Lakatos, ‘Science and Pseudo-Science’, pages 96-102 of Godfrey Vesey (editor), ‘Philosophy in the Open’, Open University Press, Milton Keynes, 1974.

20. **Robert**

February 5, 2007

In case anybody is really interested in how to compute corrections to G=8pi T here is what you have to do: Write down the sigma model for a string propagating in an arbitrary background geometry given in terms of G,B and phi (let's take RR fluxes to be 0 for the moment or use Berkovitz' formalism). Then do a world sheet loop computation (i.e. expansion in alpha' which plays the role of hbar in that theory) of the beta functions (in terms of G, B, and phi). For any order of alpha' this is a well defined procedure and there is no arbitrariness. Then you get the background equations of motion by requiring the beta functions to vanish. This is explained in detail in GSW Ch. 3.4 at the one loop level (i.e. the first alpha' correction) and has been computed if I am not mistaken up to four loops.

This should answer your question of how string theory generalised the Einstein equation. I described a perturbative computation but this is not any worse than elsewhere in particle physics.

21. **woit**
February 5, 2007

Robert,

The problem with the perturbative calculation you suggest is that it doesn't lead to something realistic with broken supersymmetry and stabilized moduli. For this you need non-perturbative phenomena: where are branes in your calculation?

This is the difference between string theory and standard QFTs used in particle physics. In string theory the perturbative ground state doesn't give you what you want, so you make some assumptions about what possible ground states may come out of the full theory, ending up with an incredibly complex set of possibilities. In the Standard Model, the electroweak ground state is exactly the unique perturbative one, which you understand well, and for the QCD ground state you have a well-defined non-perturbative theory that you can use to calculate numerically what it is, and get a unique answer.

What if you didn't know how to formulate QCD non-perturbatively, perturbative QCD didn't correspond to anything observed, and people went around claiming that non-perturbative QCD, whatever it was, led to a huge number of different possible ground states, so many that you couldn't use the theory to predict anything. Would anyone take this seriously?

22. **andy**
February 5, 2007

I really "liked" this comment of Lubos's, in reference to Zwiebach's lectures:

The goals of high-energy theorists have always been, are, and will be much more ambitious and abstract than a particular dirty experiment, whether or not an unrefined and uncultural observer of science likes it or not. Barton's focus only reflects the experimental character of the location where he gave the lecture.

23. **Thomas Larsson**
February 5, 2007

The problem is not that string theory disagrees with a particular dirty experiment, but that it disagrees with pretty much **every** dirty experiment, to the extent that it says something about reality at all.

I realize that I might sometimes sound wacky, but in one respect I am very conservative: I simply don't think that disagreement with experiment is a good thing.

24. **andy**
February 5, 2007

I hope it's clear that "liked" = ROTFL!

25. **Thomas Larsson**
February 5, 2007

Andy, sorry!

26. **andy**
February 5, 2007

Thomas, no problem. It's hard to be nuanced on a blog.

27. **wolf**
February 5, 2007

much more ambitious and abstract than a particular dirty experiment,

Delubosional… 🙂

28. **Coin**
February 5, 2007

> They're trying hard, using various tools from Germany of the 1930s they can find, to hide the simple fact that every informed person knows about the status of that theory, namely that loop quantum gravity almost certainly doesn't work – and the two recent popular books are cheap porn for undemanding consumers. It has become effectively impossible to both report about the actual current status of physics as well as to keep the talk publicly available.
>
> I wonder what percentage of the talks will have to be doctored, preordained, or censored before other people start to realize that there is a problem. Be sure that once more than 50% of talks are doctored or censored, it will already be too late because those who don't exactly believe that science should be made by free scientists who are not intimidated will already be in charge.

Huh?

29. **woit**
February 5, 2007

"cheap porn for undemanding consumers"

People have complained that the book is too hard for the average reader, maybe I should get the publisher to put the above blurb from Lubos on the back cover.

30. **Robert**
February 6, 2007

Peter, I don't understand your reply. I fail to see how the choice of vacuum influences this beta function calculation which is in some sense local at a point on the world-sheet. It is one way to obtain the low energy field equations and all discussions of KKLT etc of the many vacua actually talk about the _solutions_ of these equations. There is little doubt about the equations.

I had thought the standard line of attack here would have been to mention that the corrections are alpha' corrections and are thus important only if center of mass momenta get close to the Planck scale and are thus unlikely to observe in the real world soon. Which is followed by a philosophical discussion if this theory is really a different theory from GR from which it doesn't differ at everyday energies.

31. **woit**
February 6, 2007

Robert,

You can choose any background you want and try and do perturbative string theory on it, but string theorists keep telling us that string theory is a unique theory with no adjustable parameters. The theory is supposed to tell us what the possible lowest energy (or metastable) states of the theory are. Doing this requires a full non-perturbative string theory, and no one knows what this is.

32. **urs**
February 6, 2007

> Doing this requires a full non-perturbative string theory…

That might be.

But, by definition, the "landscape" of string theory is the space of "perturbative vacua" which, in string theory, is a space of certain 2d CFTs. For a subset of these (the geometrical ones), this space is the space of solutions of a generalization of Einstein's equations.

DRL's quote, which got this discussion started, is therefore accurate in the realm of perturbative string theory.

33. **woit**
February 6, 2007

Urs,

The "landscape" of string theory is supposed to be the set of all vacuum states (or metastable states) of string theory, this is what is physically relevant. Again, you can't actually know what this set is until you have a non-perturbative string theory.

34. **Robert**
February 6, 2007

Peter, you are moving the goal posts. The question was

> Perhaps, instead of just telling us that the equations that determine a string background are called the "string background field equations", you could write them out explicitly, or point us to somewhere where this has already been done.

I gave you the answer and now you complain that I did not answer the different question "What are the vacua of string theory?". The corrections to Einstein's equations can be computed without knowing the answer to the second question.

35. **Peter Woit**
February 6, 2007

Robert,

These are questions about physics, so what I was asking for is something that answers a question about physics. I don't doubt that you can write down equations that no one knows how to connect to physics, I'm claiming you can't write down equations that answer physical questions.

In particular, I don't believe that, from string theory, without knowing which vacuum state we're in you can write down a universal modification of Einstein's equations. Without knowing the vacuum state you don't even know how many large dimensions there are or what the string coupling is.

As Lisa Randall once pointed out: "sure, string theory predicts GR… in 10 dimensions".

36. **urs**
February 6, 2007

Since it seems we are arguing about a definition, let's look at the literature to see what people actually do when they say they study the "landscape".

The review
Fux Compactifications
http://arxiv.org/abs/hep-th/0610102

by Douglas and Kachru might help.

They go through lots of examples, write down the generalized Einstein actions of string theory background fields and find the solutions to their equations of motions.

For instance take the first example, p. 29. The generalized Einstein-Hilbert action of relevance is equation (36), the ansatz for solving its equations of motion (finding its extrema) starts in equation (41).

You can search for "Einstein equations".

They also do discuss (p. 4) that invoking "nonperturbative effects" is one aspect done in this business, but necessarily so. So if you don't believe in these nonperturbative effects there is still a lot of ordinary landscape remaining.

Finally, maybe one comment: unfortunately this review does seem to take that as granted, but it might be worth pointing it out: the very reason for calling these solutions to the string background equations — which, recall, determine conformal sigma-models — “vacua” is that given any such 2-dimension CFT in perturbative string theory we imagine regarding its n-genus correlators to be the n-loop amplitudes of a perturbative expansion of something about that very “vacuum”.

37. **Peter Woit**
February 6, 2007

urs,

The equation you point to has an undefined term in it that the authors call “S_loc” which they don’t specify other than to say that this is where you can put in things like D-branes and orientifold-planes. A non-perturbative theory is ultimately required to know what consistent choices can be made for such terms, as well as to understand whether these equations actually correspond to a valid approximate calculation in a ground (or metastable) state of the theory.

38. **urs**
February 6, 2007

I’d think D-branes and orientifold planes would still be part of the perturbative landscape, in that they are captured just by looking at 2-dimensional CFT: backgrounds with these features still correspond to points in the space of 2-dimensional superconformal QFTs with central charge 15.

But I do completely agree that whenever some author invokes aspects of “nonperturbative string theory” it might at best be a well-motivated guess.

I also agree that it might be that perturbative string theory is phenomenologically unviable, that there is no hope to get reasonable physics from studying the “perturbative landscape”, i.e. the space of c=15 2d SCFTs. Might be.

All I wanted to say is that the “perturbative landscape” of string theory — despite its silly name and the fact that we have no good real understanding of it at all and no matter how much we trust its prospects regarding phenomenology — is a well defined mathematical entity — and that many points in this space do indeed correspond to solutions of (natural) generalizations of Einstein’s equations.

39. **Robert**
February 6, 2007

The number of large dimensions or the value of the coupling constant are properties of the solutions to these equations not of the equations. And these equations definitely hold outside the branes in the bulk. And this equation is easy to check: just create some Planck scale curvature using some mass/energy distribution, and measure the graviational (and other) fields. Then you can check if they obey these alpha’ corrected equations. This should be independent of the vacuum you select at infinity. And if the coupling (in terms of the dilaton) is not too big, non-perturbative corrections should as well be controlable (being of order e^-1/g or smaller).

40. **Peter Woit**
February 6, 2007

Robert,

So, your modification of Einstein’s equations depends on which ground state you’re in, but knowing what these ground states actually are depends on knowing what non-perturbative string theory is (which you don’t).

What people are talking about often when they talk about a”string theory background” is some sort of semi-classical approximate ground state built on a specific 10d geometry, adding branes, fluxes, etc. The problem is, without knowing the underlying non-perturbative theory, you don’t know how well these reflect an actual ground state of the theory. You can argue that for special regions of parameter space you expect the semi-classical approximation to be good, but generically there’s no reason to expect this.

So, the equations you are trying to sell as analogous to G=8piT come in an infinite number of sets of equations, you don’t know what space parametrizes this infinite number, and whatever it is, on most of it these equations reflect an approximation which is probably no good and has nothing to do with physics.

I still think there’s a problem with the analogy of solutions in GR and string backgrounds….

41. **Aaron Bergman**
February 6, 2007

So, your modification of Einstein’s equations depends on which ground state you’re in, but knowing what these ground states actually are depends on knowing what non-perturbative string theory is (which you don’t).

How exactly did you go from what Robert said to that?

42. **woit**
February 6, 2007

Aaron,

Actually this discussion has gone far from the original origin which was Urs’s claim that the Landscape is just like solutions to G=8piT.

As for what I wrote, is it or is it not true that the modification of Einstein’s equation in string theory depends on the background (i.e. ground state)?

43. **Aaron Bergman**
February 7, 2007

In the perturbative string? No. The condition is just that the sigma model be conformal. You write down a general sigma model and write down the beta-functions. It’s done in Polchinski I.3.7, for example.

44. **urs**
February 7, 2007

And the beta functions one gets are, incidentally, for strings in pure gravitational and dilaton backgrounds, those used by Perelman to complete Hamilton’s program of solving the Poincaré conjecture.

45. **Peter Woit**
February 7, 2007

Aaron,

I know the argument in Polchinski, which deal with backgrounds large compared to the string scale. My question is whether this holds once you introduce other structures a la KKLT to stabilize the moduli.

46. **Aaron Bergman**
February 7, 2007

The perturbative string should properly be thought of as giving equations on the set of 2D QFTs of which sigma-models are only a part. The solutions to these equations are the 2D CFTs. (This is background independent on the classical level. To do background-independent quantization is trickier.)

When you start talking about moduli stabilization, however, one is talking about the 4D effective theory. This, by necessity, depends on the choice of compactification. Nonetheless, the theory is what you get from compactifying the string theory equations on your choice of CY. The final issue is the question of nonperturbative corrections. These are dealt with on a somewhat ad hoc basis in string theory because we a nonperturbative formulation of string theory in general. These do depend on the choice of background, ie, solution to the equations of motion. But this isn't particularly different than how one deals with instantons in ordinary QFT.

47. **Amos Dettonville**
February 7, 2007

Is Lorentz invariance optional in any of the known approaches to string theory? J. Distler's paper implies that Lorentz invariance (among other things) is a necessary attribute of string theory, or at least of what he calls "canonical string theory", whereas others seem to argue that string theory could be pursued in a non-Lorentz-invariant context. Can anyone clarify this? Does the "landscape" of possible string theories include some in which Lorentz invariance does *not* hold?

A related question: Is the "landscape" outside of what Distler calls "canonical string theory"? If so, at what point would the "landscape" become canonized?

48. **Nature**
February 8, 2007

"and that many points in this space do indeed correspond to solutions of (natural) generalizations of Einstein's equations."

Urs, what do you mean by 'natural'?

49. **urs**
February 8, 2007

> Urs, what do you mean by 'natural'?

On the one hand, nothing deep. I just added this remark to indicate to those who didn't know it that its not a bunch of weird generalizations in all kind of silly directions, but just the natural kind that you would expect: everything remains generally covariant, with various fields (p-forms, spinors, etc) all appearing coupled to GR in terms of scalar invariants cooked up from covariant derivatives.

While I think there is a also a deeper sense in which the string background equations are "natural", this is here not the place to discuss that. Suffice it to say that these generalizations are not chosen by hand but all appear automatically from an underlying principle. And not just in string theory #.

50. **Robert**
February 8, 2007

Urs, while I agree with your notion of naturalness you should not forget to point out that the naturalness is of the form of the equations but there are numerical coefficients for which a range of values would be natural but which get specific values in string theory. How many of them there are depends of course on the symmetries you assume your theory to have (for example N=8 susy fixing a lot of relations).

51. **Peter Woit**
February 8, 2007

Urs and Robert,

You both insist on discussing features of string theory backgrounds that are known not to correspond to the real world. The real world is not N=8 supersymmetric. It's highly misleading when you promote string theory by promoting aspects of it that are known to be completely inconsistent with its use as a unified theory that corresponds to the real world.

52. **urs**
February 9, 2007

Robert,

right. I vaguely indicated that there is more to the effective string background action than just being a form of GR coupled naturally to various sorts of matter. As you indicate, it is not just any coupling of dilaton, p-forms, spinors, etc to gravity, but a very particular one.

53. **urs**
February 9, 2007

> You both insist […]

We are just ansering a technical question:

"In which sense is a point in the landscape like a solution of Einstein's equations."

Answer: "From the point of the perturbative formalims: in every sense."

Clarifying this technical question is independent of evaluating its implications for phenomenological physics.

> It’s highly misleading when you promote string theory […]

I think in this thread neither Robert, Aaron nor me has done any promotion. I need to promote string theory no more than I need to promote, say, the tricategory 3Grp.

In order to clarify facts (facts like: “what *is* a point in the landscape, *technically*“) it helps to pause for a while with promoting this or that.

54. **Robert**
February 9, 2007

Maybe Peter is right and there is no point of discussing technical facts about theories. Maybe it’s better to make just claims and not discuss if details of these claims are right or wrong.

But still, just for the record, since there still seems to be some confusion: You have to differentiate between the theory (i.e. the equations of motion) and the state. All we are saying is that there are certain parts of the theory that are considered to be well understood (here: corrections to the classical equations of motion) while Peter keeps saying “but you don’t know the state” which is true but irrelevant for the first question as we try to argue. In addition, I see no reason why the world cannot be in a state with completely broken supersymmetry of a theory which has N=8 susy.

All the trouble with (absence of practically performable) experimental checks of string theory is that generically low energy questions depend crucially on the state (which as I said is unknown) while high energy questions (e.g. Regge scaling, higher curvature corrections) tend to be less dependant on the vacuum you are in.

55. **Peter Woit**
February 9, 2007

Robert,
I never said there is no point in discussing technical facts about theories. My only point is that if you are going to make claims about how the equations of string theory are “natural” and much like the equations of GR, you need to make clear if you are talking about some approximate version of string theory that can be rigorously shown to have nothing to do with the real world, or about a version of string theory that is a candidate for a unified theory.

Urs did helpfully reference a precise source for the equations he was discussing as “natural”. I pointed out to him that those equations contain explicitly an undefined term the authors call “S_loc” where they put in ad hoc contributions which are necessary to stabilize moduli, and which I don’t believe can reasonably be called “natural”.

As for you argument that string theory predictions at high energy don’t depend on the state, I don’t buy it. There seems to be an ideology among string theorists that the vacuum state, which governs what we can actually m easure, is a non-perturbative phenomenon (and thus we don’t know what it is), but that higher energy states (at energies we have no hope of probing) can be studied perturbatively. I see no reason for this to be true about the universe other than wishful thinking.

56. **onymous**
February 9, 2007

There seems to be an ideology among string theorists that the vacuum state, which governs what we can actually measure, is a non-perturbative phenomenon (and thus we don’t know what it is), but that higher energy states (at energies we have no hope of probing) can be studied perturbatively. I see no reason for this to be true about the universe other than wishful thinking.

Why not? It’s true of plenty of field theories. A lot of the nonperturbative stuff invoked for moduli stabilization is really just ordinary field theory (e.g. gluino condensation).

57. **Peter Woit**
February 9, 2007

onymous,

It’s not true for theories like QCD where the vacuum state is a truly non-perturbative phenomenon, and you can’t understand the higher energy states just using perturbation theory.

There are other arguments for why you won’t necessarily see 10d weakly-coupled string behavior (Regge trajectories) at high energy. What about M-theory? What about black holes?

The problem is that, not knowing what non-perturbative string theory is, string theorists are invoking it when perturbative string theory gives them something that disagrees with observation, then saying it can be ignored when they claim they can make predictions (safe from ever be confronted with experiment) at high energy.

58. **Anon**
February 9, 2007

“It’s not true for theories like QCD where the vacuum state is a truly non-perturbative phenomenon, and you can’t understand the higher energy states just using perturbation theory.”

The high energy behavior of QCD isn’t given by perturbation theory?

Really?

59. **Amos Dettonville**
February 10, 2007

On the question of whether Lorentz invariance is necessary for string theory, here’s what Smolin says in Ch 14 of The Trouble with Physics, where he talks about the possible breakdown of special relativity:

“String theory predicts that no matter how distant their sources are from one another, photons of different frequencies travel at the same speed. As we have seen, string theory does not make many predictions, but this is one; in fact, it’s the only prediction of string theory that can be tested by present technology… [[Interesting that Smolin wrote this prior to the recent announcements about the possibility of testing string theory]] …

“Would string theory survive…? Certainly all known string theories would be proved false, since they depend so heavily on special relativity holding. But might there still be a version of string theory that could be consistent with [the breakdown of Lorentz invariance]? Several string theorists have insisted to me that even if special relativity were seen to break down or be modified, there might someday be invented a form of string theory that could accommodate whatever the experiments see… There are a lot of ways to change the background of string theory so that there is a preferred state of rest… What puzzles me is why string theorists think this helps their cause… To me it’s more an indication that string theory is unable to make any predictions…”

Well, not being a specialist in this field, I must say that while I appreciate Smolin at least addressing the question directly (which hardly anyone else seems willing to do), the answer is slightly odd, because Smolin has worked in the area of string theory, and yet his only way of even trying to answer this fundamental question is to refer to what some un-named string theorists have insisted to him. The way he phrases it, it seems clear that he himself doesn't feel qualified to answer this question, although he does add the remark that "there are a lot of ways to change the background so there is a preferred frame". If this is true, then why does he need to rely on the un-named string theorists to "insist" to him that it is possible?

I suppose this just illustrates that one can't ever really falsify a research program, since a program can also change course and accommodate new information. Theories may be falsified, but research programs just fade away if and when people lose interest. Still, I find it troubling that even respected specialists in string theory don't know such a fundamental thing about their theory, as whether or not it must be Lorentz invariant.

One other comment: Smolin says the known versions of string theory ASSUME Lorentz invariance. He does not say they predict it, nor have I ever heard anyone else claim that string theory predicts Lorentz invariance. We don;t start with a tabula rosa, and miraculously deduce Lorentz invariance from the string principle (or some such thing). Rather, Lorentz invariance is simply assumed, as a postulate. Hopefully it's clear that the possibility of falsifying a postulate or assumption of a theory is quite different from falsifying a prediction of a theory. Likewise, when one says "string theory makes no falsifiable predictions" this is quite different from saying "the known string theories don't assume any physical principles that can be falsified". The former is the charge of the critics of string theory, whereas Distler et al are really only addressing the latter, which is at best only tangentially relevant to string theory.

60. **woit**
February 10, 2007

Anon,

Yes, really. The spectrum of states in QCD above the ground state consists of protons, neutrons, pions, etc., not free quarks and gluons. This spectrum is the analog of the Regge trajectories people claim that string theory predicts.

Asymptotic freedom and the ability to use perturbation theory to study scattering in certain high energy regimes is a different issue.

61. **anon.**
February 10, 2007

The spectrum of states in QCD above the ground state consists of protons, neutrons, pions, etc., not free quarks and gluons. This spectrum is the analog of the Regge trajectories people claim that string theory predicts.

In fact, the spectrum of QCD *does* at least approximately consist of Regge trajectories, if you go to large N_c. But real-world QCD has 3 colors, and violations of quark-hadron duality die off: at large s, rho(s) ~ constant. So in real-world QCD, perturbation theory does work at least for many quantities at large momentum, whether Euclidean *or* Minkowski.

(This is not to say that perturbation theory is any good for things like the total proton-proton cross section at high energy, of course. But you said the spectrum.)

62. **woit**
February 10, 2007

anon,

I was giving QCD as an example of a QFT where non-perturbative QFT determines both the ground state and the states above the ground state. Perturbative QCD just doesn't work at all here, for either the ground state, or for the states above the ground state (these states cannot be understood in terms of gluons and free quarks).

Whether QCD states can be understood in terms of perturbative string theory is a completely different issue. The problem there is not that you don't know the ground state because it is determined non-perturbatively, the problem is that you don't know what the string theory is.

63. **anon.**
February 10, 2007

By "higher energy states", I assumed you meant "states well above the QCD scale", since that's what the other Anon seemed to be talking about. And perturbation theory is perfectly good there, in a precise sense.

The states in QCD are, obviously, created by gauge-invariant operators acting on the vacuum. A two-point function of gauge-invariant operators can be expressed in terms of a spectral function rho(s). For s >> Lambda_QCD, perturbation theory calculates this up to small corrections by analytically continuing the Euclidean result and taking the discontinuity across the cut.

Of course resonances appear at small s and are not calculable in perturbation theory. But large s is perturbative. So it is not true that perturbative QCD doesn't tell you about states above the ground state, if you go to sufficiently high invariant mass.

64. **Peter Woit**
February 10, 2007

anon,

I don't think

"the vacuum state is a truly non-perturbative phenomenon, and you can't understand the higher energy states just using perturbation theory."

or

"The spectrum of states in QCD above the ground state consists of protons, neutrons, pions"

was in any way ambiguous. I was talking about the states of the theory above the ground state, precisely the analog of the low-lying states in Regge trajectories that string theorists always say are a prediction of string theory, despite the non-perturbative nature of its ground state. The question of what happens at very large invariant mass is a quite different one. I was obviously not making claims that there is no kinematical regime in which perturbation theory is useful in QCD, but was referring explicitly to a regime where it clearly isn't.

Again, I seem to never be able to get string theorists to address this point: if the ground state is determined by some unknown non-perturbative string theory mechanism, why do string theorists often claim that they can reliably use perturbative string theory to predict the energies of the low-lying states (the ones lying on a Regge trajectory) above the ground state?

65. **Moshe**
February 10, 2007

Strong feeling of deja vu, but let me try…if the string coupling is zero there are a bunch of massless modes (moduli) and a bunch of massive modes lying on Regge trajectories, giving characterisitc soft scattering etc. etc., This is the picture to zeroth order in the string coupling.
\
Working in small but non-zero coupling and including the tiny corrections due to instantons, gluino condensation, small fluxes etc. all those masses shift a tiny bit. This does not change the picture much, except that the would be massless modes are now massive, with a tiny mass compared to the string scale.
\
The analogy to QCD is misleading since the coupling is small, and consequently all non-perturbative effects are tiny. The vacuum and all massive modes are still approximately the perturbative ones.

66. **Peter Woit**
February 10, 2007

Thanks Moshe,

One comment is just that your argument assumes very small string coupling, for generic values of the string coupling there's no reason to believe that Regge trajectories etc. persist.

But I'd really like to understand exactly what the status of the KKLT backgrounds is. Besides the effects you mention, they need to invoke the introduction of a specific set of anti-D3 branes. With these, is everything really under control? Gross seems to be fond of claiming (see his remarks in Toronto a couple weeks ago) that since one doesn't understand non-perturbative string theory, there is no reason to believe that these constructions give you consistent vacuum states, especially in a cosmological context. Is he wrong? If he's right, and non-perturbative effects can destabilize these states, how do you know they won't destabilize higher states?

I'm willing to accept that it's possible that string theorists can come up with a self-consistent scenario involving vacuum states whose properties are under control, modulo some assumptions about the true non-perturbative theory, and in such a scenario you get Regge trajectories, etc. But even so, this is just going to then be a small corner of the landscape. Generically you're not going to have perturbative control, so can't claim that "if we just had a big enough accelerator, string theory predicts we'd see Regge trajectories".

67. **Moshe**
February 10, 2007

I am not sure what Gross referred to, I would have to look at the talk. It is true that one is always limited to constructions with calculational control. In such perturbative constrcutions Regge behaviour is a robust feature. Even if there are non-perturbative corrections invalidating the claims about the vacuum, the Regge behavior would still be there as long as the coupling is small.

68. **Peter Woit**
February 10, 2007

Moshe,

I'm not sure I'm convinced that if non-perturbative corrections make your vacuum state no longer a vacuum state they won't have significant effects on higher energy states. And, if unknown non-perturbative effects need to be understood to find the true vacuum state, in general I see no reason why they might not affect the Regge behavior of higher states.

Gross's comments were in the context of a public debate over anthropics. He said what he has said in many other talks: no reason to accept the landscape since we don't know what string theory really is, suspect that space and time are emergent phenomena but don't know what they really are, don't understand time-dependent backgrounds necessary for a consistent cosmology.

69. **Arun**
February 10, 2007

You may want to listen to Nima Arkani-Hamed's Perimeter seminar of Feb 8, where the claim is that given the small positive cosmological constant and the neutrino masses where they are, and the Standard Model in 3+1 dimensions, there is a landscape of vacua with one dimension compactified. i.e., given the Standard Model Lagrangian, you cannot compute the ground state.

70. **Peter Woit**
February 10, 2007

Arun,
I did watch the beginning of that talk, but didn't have time to watch the whole thing. Not clear to me what the point was, perhaps that you can ruin the Standard Model and make it as useless as string theory by replacing our 3 large flat spatial dimensions by various compactifications. Just seems like a good demonstration of why you shouldn't study these kinds of backgrounds.

71. **Arun**
February 11, 2007

Peter,

My initial reaction was the same as yours. But!

If the arguments hold up, it means if given } a 3+1D QFT + General Relativity + small positive cosmological constant + massless/light degrees of freedom like the Standard Model (i.e., the photon, graviton and light neutrinos)}, then that model has an infinite number of compactified vacuums, apart from the 3 large flat spatial dimensions vacuum. I presume that the only reason we didn't see this before is because we didn't look, we assumed that the 3 large flat spatial dimensions vacuum is the natural and the only vacuum solution.

I also gathered that this is not a generic problem with QFT + GR, but rather a problem with models that are like the Standard Model at low energy and that have a small positive cosmological constant. In other words, we are fine-tuned to have a landscape in the Standard Model!

Without any reference to string theory, the Standard Model + GR has a landscape problem. Of course, the anthropic solution is simple in this case, it involves only one choice, we pick the vacuum with the large flat spatial dimensions, because that is the one we live in, and that is that. But unless there is a physical/mathematical argument that rules out the compactified vacua, philosophically, we are not very far from the string theory landscape issue.

-Arun

72. **Q**
February 11, 2007

'… the Standard Model + GR has a landscape problem. Of course, the **anthropic solution is simple in this case, it involves only one choice, we pick the vacuum with the large flat spatial dimensions, because that is the one we live in,** and that is that. But unless there is a physical/mathematical argument that rules out the compactified vacua, philosophically, we are not very far from the string theory landscape issue.'

I disagree; the case of knowing the right solution from observations (three flat spatial dimensions) is not an anthropic selection. That's observational input, not anthropic reasoning.

Anthropic solutions are entirely different. Suppose a metastable vacuum which produces the SM is discovered in the string theory landscape. You would defend that solution by the anthropic argument, not because you have directly observed the Calabi-Yau manifold on the Planck scale and determined the right solution by direct observation.

Anthropic solutions are based on *indirect reasoning* not directly observing the type of dimensions. I.e., Hoyle said predicted the cross-section for the triple alpha fusion to create carbon because he ruled out other paths and knew carbon exists, not because he observed the triple alpha process in the lab.

There's no similarity between the anthropic selection of a SM from the string theory landscape, and the landscape of GR or SM where you aren't using the anthropic principle to select a solution, you are using direct empirical evidence about the dimensions etc. to choose a solution.

73. **student**
February 11, 2007

"I disagree; the case of knowing the right solution from observations (three flat spatial dimensions) is not an anthropic selection. That's observational input, not anthropic reasoning."

To me the "observational input" argument sounds just like the anthropic agrument. Here is why: We know the right solution from observations because we can make those observations in the first place. If the SM vacuum were AdS_3XS_1 instead of dS_4 we would not be here to make those observatons and use the three flat spatial dimentions as the input.

In other words, the "observational input" argument still DOES NOT EXPLAIN "Why?" the SM vacuum is dS_4 and not AdS_3XS_1. "Observational input" must have an explanation, don't you think?
According to Nima, the SM Largangian plus Gravity with a tiny CC has a huge landscape of vacua. Finding a selection mechanism, other than "observational input", such that the dS_4 vacuum is selected by dynamics would be a much more compelling answer.

74. **Amos Dettonville**
February 11, 2007

I agree with "Q" that the GR and SM solutions are based on direct empirical observation, not on indirect anthropic reasoning. On the other hand, I think Arun's comment highlights the need for one other ingredient, namely, Occam's razor. We can't claim that if there were compactified dimensions we would be able to directly observe them. If they existed, we would only be able to infer their existence indirectly, by the effect(s) they have on observable physics. Now, we have not, so far, observed any physical phenomena that imply the existence of compactified dimensions, but it doesn't follow that we can confidently extrapolate this to make predictions about new (previously unexamined) phenomena. I guess what Arun is saying is that, lacking some kind of generic argument ruling out compactified dimensions, the ability of GR/SM to make *predictions* outside out existing base of experience is placed in doubt. I understand that argument, but I don't think it carries much weight against GR/SM, because they adhere fairly closely to Occam's razor, i.e., they propose the simplest vacuum consistent with observation. Granted, we *could* postulate more complicated vacuua with unseen dimensions, but we choose the simplest one that works. This choice is fairly unambiguous, and although it is vulnerable to falsification (i.e., we can't be absolutely certain that the results extrapolate into new territory), this falsifiability is a good thing. Each time GR and/or SM avoid falsification in a new observation, it is justifiably counted as a success for those theories.

In contrast, if we have a theory that is intrinsically committed to the existence of many unobserved compactified dimensions, but which provides no unambiguous criteria for specifying those dimensions, then we are in a much more problematical position, because we are precluded from applying Occam's razor to simplfy down to a falsifiable theory. We face a hugh number of highly complex theories, with no clear way to choose between them. This is especially disappointing considering that the original excitement surrounding string theory was that it seemed to be leading toward a unique mathematically consistent theory. I know some people still hold out hope that a unique solution may emerge (a position that Susskind has characterized as "faith-based"), but as it stands today, it seems that the most salient attribute of string theory is its NON-uniqueness.

75. **woit**
February 11, 2007

Arun,

I still think that trying to claim that the most successful physical theory ever developed, which makes an absurdly large number of very non-trivial predictions which all agree completely with experiment, is on the same footing as a theory which predicts nothing is just silly sophistry.

I've made this point repeatedly, but here it is again in this context: the point is that the SM vacuum state is exceedingly simple and symmetric. It is not some random background of huge complexity based on choosing a complicated compactification, fluxes, D-branes, etc., etc. It is this simplicity that makes the theory predictive. There would be a compelling string model worth taking seriously if a simple choice of background led to something that didn't disagree with what we know about physics. The problem with string theory is that getting it to look like the real world involves adding in a Rube Goldberg-like array of complicated mechanisms designed to evade the fact that simple mechanisms don't work. This is the hallmark of what happens when you start with a wrong theory and try and make it agree with experiment.

Sure, you can ruin the predictivity of the Standard Model and make it not agree with experiment by putting it in a complicated background. So what???

76. **student**
February 11, 2007

"I don't think it carries much weight against GR/SM, because they adhere fairly closely to Occam's razor, i.e., they propose the simplest vacuum consistent with observation."

SM +GR do not propose/select the vacuum. The observational fact that we live in dS_4 is used as input but not explained. To me, the Occam's razor argument would select a 4D Minkowski vacuum as the simplest, not a dS with a tiny cosmological constant – indeed, a very unnatural choice!

"Sure, you can ruin the predictivity of the Standard Model and make it not agree with experiment by putting it in a complicated background. So what???"

Peter, what exactly do you mean by the "complicated" background?

Is 4d Minkowski less complicated than dS_4? So, why do we live in dS _4 and not in Minkowski vacuum? The "absurdly large number of very non-trivial predictions which all agree completely with experiment" would be "ruined" if our vacuum were AdS_3XS_1 instead of dS_4, according to Nima. The point is that there is no dynamical mechanism in the SM + GR which would select the vacuum. So, the ony option so far is to use the "observational"/anthropic argument to "explain" it.

77. **student**
February 11, 2007

Correction:

The "absurdly large number of very non-trivial predictions which all agree completely with experiment" would NOT be "ruined" if our vacuum were AdS_3XS_1 instead of dS_4, according to Nima.

78. **woit**
February 11, 2007

student,

As I wrote, I didn't watch all of Arkani-Hamed's talk, and I still have no idea whether he had a more serious point to make than the silly one about "the standard model has a landscape problem just like string theory" which is all I was getting from Arun's summary.

deSitter space is not that much less simple than Minkowski space, (it's also a homogenous space).

79. **student**
February 11, 2007

Peter,
The statement that "the standard model has a landscape problem" is not silly. Nima's discovery that the SM is just as happy to live in AdS_3XS_1 as opposed to dS_4 with a very tiny CC begs for an explanation as to why we ended up in the latter. To me, a 4d Minkowski vacuum seems to be a more "simple"/"natural" outcome than either of the two choices above. This "SM landscape" problem may be in the same category as the "string landscape" problem. It is the question of identifying a correct vacuum selection mechanism.
There is nothing "silly" about this question. If one can find such a mechanism for the SM+GR, the same mechanism may apply for the string landscape.

80. **Peter Woit**
February 11, 2007

student,

The "string landscape problem" is just caused by the fact that people have taken an idea that doesn't work (string theory based unification), added huge amounts of structure to it to evade the fact that it doesn't work, and now are trying to find some way out of this mess other than the obvious one (admit this doesn't work and try something else).

There certainly are specific choices that go into the standard model that distinguish it from other equally simple theories: why the specific dS cosmological background we are in (rather than AdS3xS1 or whatever)? Why SU(3)xSU(2)XU(1)?, etc. If you can solve any of these problems you'll be very famous. And not because you've figured out something that may help understand the string theory landscape…

81. **Amos Dettonville**
February 11, 2007

student,

I don't understand what you mean when you say GR does not propose a vacuum (specifically 3+1 dimensional spacetime, with a cosmological constant very close to zero). In alleged contra-distinction to this, you assert that "The observational fact that we live in dS_4 is used as input but not explained." Well, that's true, but it doesn't conflict with what I said. It's just repeating what I said. To "propose" is not to "explain". And we're in agreement that this proposal is based on observational facts. Indeed this is the whole point: the attributes of a theory like GR can be pinned down rather fully by observable facts. GR doesn't contain a large amount of free structure that is unconstrained by observable facts. I think Peter's point is that, in contrast, string theory has taken on a great deal of structure that is unconstrained by observable facts. This is what makes it problematic.

You wrote:
"To me, the Occam's razor argument would select a 4D Minkowski vacuum as the simplest, not a dS with a tiny cosmological constant -indeed, a very unnatural choice!"

Well, if 4D Minkowski spacetime was sufficient to save all the phenomena, I imagine it would be the preferred model. But it is pretty much impossible to reconcile the equivalence principle with mass-energy equivalence in the context of 4D Minkowski spacetime, so these observationally-based principles motivate a more general spacetime, and this generalization allows for the possibility of a non-zero cosmological. The economy, logical coherence, and connection to observable fact of general relativity is a far cry from the extravagant constructions of string theory.

Having said that, there's no denying that general relativity is an incomplete theory, as Einstein himself often emphasized. The stress-energy tensor is really (at best) just a place holder for some more meaningful representation of the non-gravitational constituents. Also, being a differential theory, GR obviously requires the specification of boundary conditions of some sort, and the imposition of energy conditions, etc., things the theory itself doesn't seem to constrain. So nobody would argue that GR is the last word. But I do think it presents a striking contrast to string theory.

82. **Anon**
February 11, 2007

"The 'string landscape problem' is just caused by the fact that people have taken an idea that doesn't work (string theory based unification), added huge amounts of structure to it to evade the fact that it doesn't work"

The existence or nonexistence of a large number of vacua of a theory is a property of the theory, not of the motivations of those studying it. If I understand correctly, Nima claims that the existence of a large number of vacua is a property that the SM+GR shares with string theory.

If so, then you can't wish the landscape away, by blaming it on the nasty string theorists.

83. **Q**
February 12, 2007

'The point is that there is no dynamical mechanism in the SM + GR which would select the vacuum. So, the ony option so far is to use the "observational"/anthropic argument to "explain" it.' – Student, February 11th, 2007 at 2:54 pm

Observational facts are just not anthropic arguments, and shouldn't be confused with them. The string landscape effort is based on neither observational facts nor anthropic predictions. The anthropic argument says that one set of observed facts (eg, the existence of carbon) predicts something else (eg, the rate of fusion of three alpha particles into carbon) which can be checked and confirmed in the lab.

For example, Hoyle used the anthropic principle to correctly predict a nuclear energy level at 7.6 MeV, which was then confirmed!

" "Most anthropic insights are made with the benefit of hindsight. We look at the Universe, notice that it is close to flat, and say, 'Oh yes, of course, it must be that way, or we wouldn't be here to notice it.' But Hoyle's prediction is … is a genuine scientific prediction, tested and confirmed by SUBSEQUENT experiments. Hoyle said, in effect, 'since we exist, then carbon must have an energy level at 7.6 MeV.' THEN the experiments were carried out and the energy level was measured. As far as we know, this is the only genuine anthropic principle prediction…"

– http://www.novanotes.com/jan2003/anthro1.htm

Even if a solution to the string theory landscape which was in ad hoc agreement with the SM was found, that is not an anthropic prediction. To get an anthropic prediction, you would have to find a solution to the landscape which is not merely consistent with the SM, but which says something further than can be checked in the lab.

With as many as 10^500 solutions, if any do include the SM, there would have a wide spectrum of nearby solutions which also do that, but which have variations regarding the extra physics they include that can be checked. So the problem will persist that the theory is sufficiently vague (due to having so many slightly different solutions) that it won't be falsifiable by the method of Hoyle's successful anthropic prediction.

84. **Arun**
February 12, 2007

I think I appreciate better Peter's and Amos' points, after some thought. The SM has a few dozen things put in by hand, if we have to add another, a vacuum selection principle, no big deal, we still have a theory from which we get a lot more back than we put in. We have a predictive theory.

If the anthropic principle led to a theory from which we get out more than we put in, I don't think anyone would be complaining very loudly. However, with the landscape, to put it dramatically, the sum total of human knowledge is insufficient to determine the vacuum.

85. **Peter Woit**
February 12, 2007

Anon,

"If so, then you can't wish the landscape away, by blaming it on the nasty string theorists."

I don't wish the landscape away, I'm agnostic about it. Gross thinks the calculations that lead to it are not reliable, other string theorists think they are. My point is that if you believe that you have reliable calculations that show that your theory has extremely complicated "backgrounds" in it, that the simple ones don't look at all like the real world, so you have to go to very complicated ones just to evade contradiction with experiment, it means your theory is wrong, and scientific ethics say you should abandon it.

This is the landscape problem, and any claim that the SM has the same problem is just sophistry.

86. **Anon**
February 12, 2007

"My point is that if you believe that you have reliable calculations that show that your theory has extremely complicated "backgrounds" in it…"

What does "extremely complicated" have to do with it? ("complicated", being very much in the eyes of the beholder)

I thought the "landscape problem" was that the theory has a large number of vacua. Period. No qualifiers about how "complicated" they are.

87. **Peter Woit**
February 12, 2007

Anon,

"I thought the "landscape problem" was that the theory has a large number of vacua. Period. No qualifiers about how "complicated" they are."

The number of vacua is not really the point, the problem is the nature of the vacua combined with their number. If the theory had one free parameter and you could calculate everything in terms of this parameter, you could say it had "an infinite number of vacua", but this would not be a problem. You just need one measurement to fix this and get a predictive theory. If there were a small number of complicated vacua, you could separately calculate things in each vacuum, compare to experiment, and see if one matched.

It is the combination of the complexity and the large number of vacua that both allows the string theory landscape to evade being confronted with experiment, and makes the whole setup non-predictive.

And no, "complicated" is not purely in the eye of the beholder.

88. **anon**
February 12, 2007

"It is the combination of the complexity and the large number of vacua …"

I don't see what the alleged complexity has to do with the issue.

If your theory has a large number of inequivalent vacua and if the low-energy physics is different in these different vacua, then you run the risk of losing predictivity.

I don't see why it matters whether these vacua are "simple" or "complicated."

89. **Peter Woit**
February 12, 2007

anon,

"If your theory has a large number of inequivalent vacua and if the low-energy physics is different in these different vacua, then you run the risk of losing predictivity."

You're ignoring the example I gave of a simple set of an infinite number of vacua. Again, the problem is not inherently the number of vacua, it's the combination of large number with their complicated structure. Simple versus complicated matters because simple situations are easier to do calculations in than complicated ones.

The other reason "complicated" matters is because "complicated" is what happens when you try and evade failure. If you start out with a simple model, find it disagrees with experiment, then add new more complicated structure to evade this, as you keep doing this it becomes more and more clear that your starting point was wrong. That is exactly what has happened as people have constructed more and more complicated string theory backgrounds.

90. **anon.**
February 13, 2007

It's kind of puzzling that you're so hostile toward Nima's talk. I guess you stopped watching before he explained that there are new ways to access deSitter solutions that don't involve the usual picture of tunneling and eternal inflation?

91. **Anon**
February 13, 2007

"Again, the problem is not inherently the number of vacua, it's the combination of large number with their complicated structure. Simple versus complicated matters because simple situations are easier to do calculations in than complicated ones."

If you need to compute the physics of 10^500 vacua, before you can determine which one we live in, and extract predictions for low-energy physics, then it really doesn't matter whether each individual vacuum is "simple" to do calculations in.

Conversely, if you can extract predictions without going to such extraordinary lengths, why does it matter that each individual vacuum is "complicated"?

" … failure …"

I think you are mixing up your personal distaste for anything related to "string theory" with whatever scientific point there is to be made about Nima's lecture and the landscape.

92. **woit**
February 13, 2007

anon.,

My hostility to the talk was just due to the claims about "the standard model has the same problem as the landscape". Perhaps there was some actual substance to the talk, I haven't heard any from the commenters here. A "new way to access deSitter solutions" sounds more substantive, but just isn't the sort of thing I'm especially interested in, maybe other people are.

Anon.,

You seem to just be unwilling to acknowledge that I've given you an example showing that in cases of simple theories a large or infinite number of vacua is not a problem at all. The point is simple: simple theories are simpler to calculate in, for complicated theories, the calculations are, well, complicated…

93. **Anon**
February 13, 2007

"You seem to just be unwilling to acknowledge that I've given you an example showing that in cases of simple theories a large or infinite number of vacua is not a problem at all."

Your example, with a continously degenerate vacuum, is in flat contradiction with experiment (because it contains a massless scalar field with dangerous couplings to ordinary matter, exactly as in a string compactification with unfixed moduli). Therefore, it is relevant, neither to the discussion of Nima's talk in particular nor to the landscape problem in general.

"The point is simple: simple theories are simpler to calculate in, for complicated theories, the calculations are, well, complicated…"

A tautology which, again, is irrelevant to the landscape problem.

94. **Peter Woit**
February 13, 2007

Lots of theories have parameters in them, my example is only "in contradiction with experiment" if you want to insist on the string ideology that your unknown non-perturbative string theory will have no free parameters. If you want to make my example consistent with the ideology, just have the free parameter only take discrete values. The issue is whether observable physics depends in a simply calculable way on the parameter, not whether it is continuous or discrete.

Again, the landscape problem is not that there are lots of vacua, as the above example shows, it's that the vacua are very complicated, it's difficult to calculate relevant physical observables reliably in them AND they come in essentially infinite variety.

95. **Anon**
February 13, 2007

"If you want to make my example consistent with the ideology, just have the free parameter only take discrete values."

A theory with free parameters (whether or not those parameters take on only discrete values) is not the same thing as a theory with many (discrete) vacua.

So, no, your example doesn't shed any light on the landscape problem.

“Again, the landscape problem is not that there are lots of vacua, as the above example shows, it’s that the vacua are very complicated,…”

That’s not what everyone else , including Nima, means, when they use the phrase “the landscape problem.”

Thanks for clarifying that thats what you mean.

96. **Peter Woit**
February 13, 2007

Anon,

If you don’t like my calling them “free parameters”, replace “free parameters” with “vacua” and read what I have to say again.

When I repeatedly write here that the landscape problem is due to both the number and complexity of the vacua, your deleting one half of this (about the number), putting what I have to say in quotes and saying that my definition of the landscape problem is different than other people’s is just a ridiculous tactic.

Since you seem to be on first-name basis with Nima and invoke him in everyone of your comments, perhaps you might want to contact him and ask him if all there is to the landscape problem is that the number of vacua is large.

97. **Lee Smolin**
February 13, 2007

Dear Q,

The Hoyle argument is not a “prediction” of the anthropic principle. The Hoyle argument is based on a fallacy in which an extra statement is added to a correct argument, without changing its force. The correct argument is as follows:

A The universe is observed to contain a lot of carbon.
B That carbon could not have been produced were there not a certain line in the spectrum of carbon.

Therefor that line must exist.

To this correct argument Hoyle added a statement that does no work, to get:

U Carbon is necessary for life.
A The universe contains a lot of carbon
B That carbon could not have been produced were there not a certain line in the spectrum of carbon.

Therefor that line must exist.

You see that U does no work. One way to see it is that if the prediction turned out to be wrong one would not question U, one would question the calculations leading to B.

I have found that every single argument proported to be a successful prediction from the AP has a fallacy at its heart. See my hep-th/0407213 for details.

What has been so disheartening about the current debates re the landscape is that all this was thought through a long time ago by a few of us and it has been clear since around 1990 what an appeal to the landscape would have to do to result in falsifiable predictions. The issue is not the landscape per se but the cosmological scenario in which it is studied. The fact that eternal inflation can’t yield anything other than a random distribution on the landscape is the heart of the impass, for that leads to the AP being pulled in in an attempt to save the theory and that in turn leads to a replay of old fallacies.

Thanks,

Lee

98. **Anon**
February 13, 2007

‘If you don’t like my calling them “free parameters”, replace “free parameters” with “vacua” and read what I have to say again.’

I did. Now it makes no sense at all.

“Since you seem to be on first-name basis with Nima and invoke him in everyone of your comments, perhaps you might want to contact him and ask him if all there is to the landscape problem is that the number of vacua is large.”

Nima’s argument is that fairly generic theories (like the Standard Model), when coupled to gravity, have a landscape of vacua.

You want to claim that what Nima finds is not a landscape because all those vacua are “simple,” whereas, in the landscape, the vacua are “complicated” (whatever the heck that means).

Clearly that means that you and he disagree about the definition of “landscape.” I’m just pointing out that the majority of high energy theorists, who use the term, are referring to his definition, not yours.

99. **woit**
February 13, 2007

Anon,

I didn’t define what the landscape is (everyone agrees we’re talking about the space of vacua of a theory), I was telling you what the landscape PROBLEM is. You believe the problem is the large number of vacua, not their complex properties, I’m telling you that it is both.

For the N’th time: If the landscape consisted of an infinite number of vacua, labeled by a discrete parameter (say an integer), and we could calculate all physical observables easily in terms of that integer, there would be a landscape, but no landscape problem. You just would have to compare the result of that calculation to observation, and see if, for any integer, the results matched. An infinite number of vacua is not inherently a problem, the problem is whether you are able to calculate things and match to experiment.

I don’t know what Arkani-Hamed’s “landscape of SM vacua” looks like and whether it’s one in which you can easily do calculations or not, and thus whether or not it suffers from the same problem as the string theory landscape does. Whether or not it does, claiming that "the SM has the same landscape problem as string theory" is sophistry.

100. **Q**
February 13, 2007

Dear Lee,

Thank you. As you write, the observation that people exist does not go into Hoyle's prediction, so the anthropic argument is not involved in the actual prediction.

The observations of the amount of carbon and of the beryllium bottleneck (that prevents significant carbon being formed by reactions other than the fusion of alpha particles) are no more anthropic than other astronomy or nuclear physics observations.

Hoyle's prediction was claimed to be 'the only genuine anthropic principle prediction', according to John Gribbin and Martin Rees's *Cosmic Coincidences*, quoted at http://www.novanotes.com/jan2003/anthro1.htm

Your link to http://arxiv.org/abs/hep-th/0407213 is extremely helpful and it surprises me that, in fact, there aren't any genuine anthropic predictions that have been confirmed. I thought it was just weak physics but not it looks like rubbish. My motto in future will have to be *nullius in verba*.

101. **Anon**
February 13, 2007

And, for the N'th time, the problem has nothing to do with the alleged complexity of the individual vacua in question.

"If the landscape consisted of an infinite number of vacua, labeled by a discrete parameter (say an integer), and we could calculate all physical observables easily in terms of that integer, there would be a landscape, but no landscape problem. … An infinite number of vacua is not inherently a problem, the problem is whether you are able to calculate things and match to experiment."

In your alleged non-example, one can calculate, once and for all, the values of all physical observable in these different vacua as a closed-form function of a single (or several) integer(s).

If that's your definition of "simple," then I'm afraid that there are no simple theories of any physical relevance to the real world.

Some quantities (super-renormalizable couplings, like the cosmological constant) will vary wildly between different vacua, even if (as Nima and others have argued) other, renormalizable, couplings do not vary appreciably.

There are plenty of field theory examples (much simpler than the Standard Model) that exhibit this behavior, when coupled to gravity.

I'm sorry that this observation causes you such psychological distress.

102. **woit**
February 13, 2007

Anon,

You are completely devoted to missing the point here. Obviously my example was over-simplified, but the fundamental point is simply that of whether or not you can calculate things and compare them to experiment, i.e. do science.

In the SM you can, extremely successfully. In the string theory landscape people have failed utterly to calculate anything that can be compared to experiment in any way. Examining why that is might be worthwhile, claiming there's no real difference is absurd.

Thanks for your concern about my psychological well-being, I assure you that I'm feeling just fine, the only psychological distress coming from mild annoyance that I'm wasting my time trying to have a serious discussion with someone who doesn't seem interested in this.

103. **urs**
February 14, 2007

> In the string theory landscape people have failed utterly to calculate anything that can be compared to experiment in any way.

While I don't want to disagree with the general statement that there are no useful predictions yet, the above statement is not quite strictly true.

One thing that is rather easily determined for most vacua is for instance the gauge group of the effective 4-dimensional theory. That can be compared to experiment.

And that's one of the main properties that people in string model building (= landscape point finding) check. They discard lots of vacua with the wrong gauge group and find vacua with gauge groups that come closer to the real thing by passing from simple to more involved vacua.

It is true that those vacua with the right gauge group that have been found so far are rather involved, as far as their "algorithmic complexity" goes (i.e. concerning how many pages you have to fill to define it). Although that does not preclude (nor suggest, of course) that there might not be a complicated looking Rube-Goldberg vacuum which is later realized to be "algorithmically non-complex" in that it, say, is the unique one with a certain property, whatever.

Please note well: I am not discussing whether or not we should be happy or non-happy with string theory and its space of solutions. We all know each others opinion on that already. All I want to address here is a technical point, namely if or if not people have managed to compute from a given string vacuum something that can be compared with the real world.

104. **woit**
February 14, 2007

Urs,

You're well aware that one can get just about any gauge group one wants out of the landscape. When I wrote about comparing theory with experiment, I was referring to the standard scientific notion of testing one's theory by making experimental predictions and checking them, something that is impossible in the landscape situation.

105. **Thomas Larsson**
February 14, 2007

Urs, would you say that a negative, or at least non-positive, cosmological constant is a falsifyable prediction of AdS/CFT?

106. **urs**
February 14, 2007

Peter, Thomas,

let me recall what I was commenting on:

Peter described how to proceed with a physical theory that has more than one solution

(http://www.math.columbia.edu/~woit/wordpress/?p=514#comment-22316)

> [for each solution] […] calculate all physical observables […] compare the result of that calculation to observation, and see if, […] the results matched [observation]

Then he remarked that

(http://www.math.columbia.edu/~woit/wordpress/?p=514#comment-22319)

> [for string theory solutions] people have failed utterly to calculate anything that can be compared to experiment in any way #

I was just pointing out that this is not quite true.

Lots of string solutions are known, lots of properties have been computed for them, like gauge group, number of generations, particle content in general, number of large dimensions.

Most choices one has tried don't match observation. To match even these basic properties with observation one has to look for rather peculiar solutions. (Some that do match these basic properties do exist, though. Most famously maybe the solution by Braun et al.)

I do agree with you all, though, that no useful prediction has come out of string phenomenology so far.

107. **woit**
February 14, 2007

urs,

If you look at the history of this, I think you'll find that what has been going on is not at all what I was talking about: computing the predictions of a theory, comparing to experiment, and admitting failure if they don't match. It has been something rather different: computing gauge groups + representations in simple examples, finding they don't match the observed SM, choosing more complicated examples, computing again, finding they still doesn't match, etc, reaching a present point of working with complicated examples that still are very unphysical, and not even having any idea whether the gauge groups + representations one has laboriously made match by ad hoc methods are even the ones that you want (what if the LHC discovers a new force or new particles?).

108. **urs**
February 14, 2007

Peter,

in your example,

http://www.math.columbia.edu/~woit/wordpress/?p=514#comment-22316

how many integers would you check before you "admit failure"?

I should say that I agree very much that checking lots of convoluted examples, one by one, with no guarantee that there is at least one that works is not particularly uplifting.

What is, at times, much more enjoyable are more bottom-up approaches, where one tries to identify some hidden underlying structure of our standard model, which maybe suggests where to look for a UV completion.

I am particularly thinking of Connes' way of encoding the entire standard model in the data of a spectral triple – and now rather elegantly so.

http://golem.ph.utexas.edu/category/2006/09/connes_on_spectral_geometry_of.html

Sometimes I imagine that if I were a model builder, I would try to use that information to look for string backgrounds that have a chance of reducing to Connes's spectral triple.

Notice that string backgrounds do tend to ordinary spectral triples on target space in the point particle limit:

http://golem.ph.utexas.edu/string/archives/000844.html

Riordan Review of The Trouble With Physics

February 3, 2007
Categories: Uncategorized

Lee Smolin's *The Trouble With Physics* will be out soon in the UK, available February 22 according to Amazon.uk. This month's Physics World has a very good review of the book by Michael Riordan, under the title Stringing physics along. Before his current incarnation as an historian of science, Riordan worked as an HEP experimentalist for many years, going on to write one of my favorite books about the development of the Standard Model, The Hunting of the Quark.

The review is very well done, and I especially like his description of string theory as not a theory, but "instead a dense, weedy thicket of hypotheses and conjectures badly in need of pruning." The one place where I really disagree with Riordan, is where, like many other people, he explains the landscape issue and characterizes the problem with string theory as "it got caught up in its own mathematical beauty." I don't think that that's the problem with string theory in general, and it's certainly not the problem with the landscape arm of string theory research, which is explicitly devoted to the idea (see for example Susskind's book) that the universe is something of spectacular mathematical ugliness.

Riordan goes on to make the claim that the way string theory is being pursued is of danger to science in general, since its continual evasion of any possibility of confrontation with experiment is of the same nature as "intelligent design". Riordan writes "To me, string theory and intelligent design belong in the same speculative, unproveable category." He ends with the recommendation "*The Trouble With Physics* deserves a wide, careful reading by all physicists concerned about the future of our discipline."

Comments

1. **Keith Thompson**
   February 3, 2007

   Speaking of string theory:

   A cartoon.

2. **Thomas Larsson**
   February 3, 2007

   Science Fashions and Scientific Fact
   Michael Riordan
   Physics Today, August 2003, page 50

3. **woit**
   February 3, 2007

   Keith,
   That cartoon appears as a graphic in the Riordan article.

4. **Aaron Bergman**
   February 3, 2007

   To me, string theory and intelligent design belong in the same speculative, unproveable category, and Smolin apparently agrees.

   I think I've already used this line somewhere, but the fundamental difference between string theory and intelligent design is that string theory aspires to be science.

5. **woit**
   February 4, 2007

   Aaron,

   It's not that simple, intelligent design also aspires to be science.

   I do agree there's a fundamental difference: physicists should know better.

6. **Aaron Bergman**
   February 4, 2007

   Intelligent design pretends to do so, but we all know that it's a facade.

7. **mclaren**
   February 4, 2007

   ",,,the fundamental difference between string theory and intelligent design is that string theory aspires to be science.."

   Alas, all pseudoscience aspires to be science. The difference twixt science and pseudoscience is that genuine science offers us a reliable guaranteed way of detrermining whether those aspirations are destined to be fulfilled.

   I keep asking the same question over and over again, but no one ever gives a credible answer — specify the experimental results which can be performed and will categorically disconfirm string theory's predictions.

8. **John Gonsowski**
   February 4, 2007

   Physicists should know better than to go too much past what the known math is saying. Philosophers can go wherever they want but the two disciplines should be kept more separate than they sometimes are.

   String theory is an attempt to link some fairly well known ideas. If the math to do this linking was well known, there'd be plenty of existing experiments to use. Adding extra stuff has to either explain new things or link old things. String theory seems to be overly trying to find new ways to verify old things that they hope to link to other old things some day. That’s kind of not all that important at this point (except maybe for public relations if it gets interpreted to be more than it is).

9. **mclaren**
   February 4, 2007

   “String theory seems to be overly trying to find new ways to verify old things that they hope to link to other old things some day.”

   Occam’s Razor.

10. **King Ray**
    February 5, 2007

    There seems to be an Orwellian (1984) aspect to string theory…

11. **amused**
    February 6, 2007

    I’m no string lover but have to take issue with Peter’s description of Riordan’ review as “very good”. From the review:

    “To evade comparisons with dross reality, for instance, string theorists have invoked an unseen “metaverse” of parallel universes corresponding to the “landscape” of 10^500 possible solutions that might exist”

    No, the landscape was a derived consequence of the theory, not something invoked or put in by hand to “evade comparisons with reality”. I’m sure most string theorists would have much preferred there to be a unique vacuum from which unique predictions could be derived. (And yes, I hate the landscape just as much as everyone else here, but that’s beside the point.)

    “But at least these ideas [loop quantum gravity and others] lead to firm, testable predictions by which they can be judged and – if found lacking – rejected. This is science”

    String theorists talk about the possibility of evidence for ST from cosmic strings — I don’t want to defend the credibility of that, but are the “firm, testable” predictions from LQG any more credible?
    Also, has it been shown that LQG doesn’t have a landscape problem similar to string theory? Has it been shown that LQG can be coupled to matter in such a way that a unique low energy effective theory is obtained which contains the Standard Model in Minkowski spacetime?

    Riordan writes “To me, string theory and intelligent design belong in the same speculative, unproveable category.”

    Having once called on string theorists to denounce Lubos for comparing ST to the theory of evolution (see here for the outcome 😉) I now feel obliged to denounce Riordan for comparing ST to ID, and also Peter for his uncritical reporting of it. Nothing good will come from this derision of the research efforts of serious scientists who are following their best judgement in a difficult situation. (Disagreeing with their judgement is quite different to mocking them by comparisons to the ID nutters.) Much better imo to keep hammering the stringers on their weakest point: the unearned hegemonic position of ST in formal particle theory, and the ills that follow from it. Such as string theorists having been hired into faculty positions “at a disproportionally high level not necessarily commensurate with ability in all cases” (to quote SLAC’s JoAnne Hewett), with some of them having chosen ST for the reason that “well, you know, that’s the way things are going” (an actual quote from an acquaintance of mine who switched to ST as a postdoc and got a good faculty position out of it. I can hardly blame him, he had a family to support…)

12. **woit**
    February 6, 2007

    amused,

    I didn’t intend to imply that I support everything Riordan writes, some of it is highly oversimplified.

    Actually I don’t think Riordan has it completely wrong to say that the Landscape arises from trying to avoid problems with experiment. The point is that simple string backgrounds have massless moduli fields and so predict new long range forces. If we saw these, this would be considered a huge confirmation of string theory. But they’re not there so people have constructed more and more complicated “backgrounds” to stabilize the moduli, leading to the landscape problem.

    As for ST and ID, I don’t agree with Riordan’s broad-stroke characterization of string theory, there are plenty of string theorists who are not fanatics and are intellectually honest. But there are also some who are highly intellectually dishonest and/or complete fanatics, as bad as any of the IDers (and this includes the two most prominent string theory bloggers). The anthropic landscape is no more based on legitimate science than ID is, and the way that research program has gained serious attention is disturbing.

13. **amused**
    February 7, 2007

    Peter,

    “The anthropic landscape is no more based on legitimate science than ID is”

    The anthropic landscape isn’t science in the usual sense, but is still quite different from ID. String theorists who work on this can reasonably claim that they are simply following to its logical conclusion what in their judgement was the best road forward in particle physics at the time. Our response to that would be that they were following what in hindsight was a bad road, and should try to find another one. But still, the fact remains that the anthropic landscape, although not science itself, was nevertheless arrived at in a reasonably scientific way by serious scientists. In contrast, ID is nothing more than a cover for promoting religion.

14. **Aaron Bergman**
    February 7, 2007

    It’s not as if string theorists put the landscape of vacua into the theory by hand; it’s always been there.

    I really think you should reconsider your characterization here. It is both false and tremendously offensive. What you’ve never seemed to realize is that the vast majority of the people working on landscape related things have done so for the express purpose of being able to make predictions. Now, you might not like their philosophy, and maybe you think they’re out to destroy science in order to save string theory, but to compare them to the intellectual dishonesty that is ID is something I’d hope you would think long and hard about before doing.

15. **Peter Woit**
    February 7, 2007

Aaron and amused,

Yes, the anthropic landscape was arrived at by standard deductive scientific arguments. The problem is what happened when people got to it. Many physicists have properly acknowledged that if your theory leads to something grotesquely ugly and complicated that can't provide legitimate scientific predictions, you have to acknowledge that your theory is wrong and try something else, and that this is the conclusion the anthropic landscape forces on one. Some people refuse to acknowledge that this is what has happened, and I don't see their behavior as fundamentally different than that of the IDers. They are giving up on the standard scientific method because it conflicts with their ideology.

I've repeatedly made the scientific argument about why the anthropic landscape is not science, I don't see any point to trying to make claims about what is in the hearts of either string theorists doing this or IDers promoting their views. That said, I think the recent flurry of press releases issued by universities about "tests of string theory" are every bit as dishonest as the press releases coming out of the Discovery Institute.

16. **Aaron Bergman**
February 7, 2007

I hope you would consider all the implications of what you are saying when you make comparisons with ID and ask whether such comparisons are necessary for you to make your arguments.

Reading a bit more on the history of the ID movement might help, too.

17. **Thomas Larsson**
February 7, 2007

to compare them to the intellectual dishonesty that is ID is something I'd hope you would think long and hard about before doing.

It seems much more fair to compare string theory to aether theory. The aether theorists were serious scientists – Lorentz won the Nobel prize and Poincare was one of his generation's best mathematicians, like Gross and Witten. They had good reasons to view aether theory skeptics to be crackpots – aether theory is more or less an inevitable consequence of Newtonian mechanics and a finite speed of light, and the latter was experimentally proven beyond reasonable doubt. And they kept ignoring experimental data contradicting aether theory (Michelson-Morley, photo-electric effect), just like string theorists ignore experiments (no SUSY or extra-dimensions, positive cc contradicts AdS/CFT). It is an interesting empirical fact that the physics establishment kept believing in aether theory for 20 years after Michelson-Morley, i.e. for some five years after the crackpot patent clerk's annus mirabilis.

18. **Thomas Larsson**
February 7, 2007

BTW, no LQGist can IMO can be compared to the top aether theorists.

19. **Peter Woit**
February 7, 2007

Aaron,

I'm well aware of the implications of what I am saying. I really wish string theorists would seriously consider the implications of issuing dishonest press releases and pursuing pseudo-scientific research programs. This is doing real damage to the credibility of science among the general public.

20. **Aaron Bergman**
February 7, 2007

I'm well aware of the implications of what I am saying.

I'm not sure you do. That's why I suggested reading up on the history of the ID movement.

21. **1uk3**
February 9, 2007

LOL Peter when you compare string theory and its advocates to ID and its advocates you are just as ridiculous as certain string theorists who say that skepticism about string theory is analogous to "skepticism" of "darwinism".

Here's what Lubos said once:

"I think that the analogy between evolution and string theory is a rather fair one. In neither case, we have a practical method that would indeed allow us to kill the whole framework by a single observation. In both cases, there are hundreds of details that must be answered by a more detailed work. In both cases, someone's emotional feelings that evolution is too cruel or extra dimensions are too religious are completely irrelevant for science's opinion."

22. **Peter Woit**
February 9, 2007

1uk3,

Yeah, sure, the idea that some string theorists behave like intellectually dishonest fanatics is just ridiculous….

23. **Aaron Bergman**
February 9, 2007

I see. Reductio ad Lubos.

24. **Peter Woit**
February 9, 2007

Aaron,

Lubos is often the best example, embodying many of the problems with the behavior or string theorists in a pure form, something one rarely sees in nature.

But, even among those string theorists with blogs, he's not the only example. At least he doesn't have Harvard issuing press releases…

25. **Aaron Bergman**
February 9, 2007

You know, I'm not particularly interested in playing this game. We're not talking about all the people you dislike or people who dislike you; we're talking about whether it is fair to compare string theorists to proponents of intelligent design. This relates to very specific questions about motivation, methods and intellectual honesty. If you think those comparisons are appropriate, I think you've gone beyond the pale. If you just want to talk about how the bad bad people are doing bad bad things, then I've got better things to do.

26. **Peter Woit**
February 9, 2007

Aaron,

I'm not making blanket accusations about "string theorists are as bad as IDers", and I'm not making any judgements about who is "bad, bad" or likable or dislikable. You and others assure me Distler can be a charming guy, I've heard the same from many people about Lubos, and from what I've seen of Susskind, he certainly has his charm too.

This doesn't mean I don't have serious problems with the scientific arguments that they make, and the tactics they sometimes employ in making them. There are well-understood norms of what an intellectually honest scientific argument is, and my claim is that at times these people have violated these norms, and all the evidence that I've seen is that they have done so for ideological reasons.

What's "beyond the pale" I think is not my views about this (which, by the way are shared by a sizable number of people in the physics community, not just Riordan, try asking around), but the kind of unprofessional behavior that some string theorists have decided to engage in. It's doing a lot of damage to physics and to the credibility of the field. I'm not going to apologize for or stop pointing this out.

27. **Amos Dettonville**
February 10, 2007

Aaron, in view of your rejection of any sort of comparison between string theory and Intelligent Design (ID), it's interesting to recall this from Smolin's book:

"In a recent interview, Susskind claims that the stakes are to accept the landscape and the dilution in the scientific method it implies or give up science altogether and accept intelligent design (ID) as the explanation for the choices of parameters of the standard model: 'If, for some unforeseen reason, the landscape turns out to be inconsistent … as things stand now we will be in a very awkward position. Without any explanation of nature's fine-tunings we will be hard pressed to answer the ID critics. One might argue that the hope that a mathematically unique solution will emerge is as faith-based as ID.' "

Granted, he phrases this in the ambiguous form "one might argue", but it seems clear that he is saying that, barring some kind of anthropic guide through the landscape, string theory is as faith-based as ID. Was Susskind "beyond the pale" in making that statement? Was Smolin beyond the pale for paraphrasing it as he did?

28. **Aaron Bergman**
February 10, 2007

I suggest you reread the passage. I don't agree with what Susskind says, but there is no way to go from the text you quoted to the paraphrase, "barring some kind of anthropic guide through the landscape, string theory is as faith-based as ID."

29. **Amos Dettonville**
February 10, 2007

Aaron, I'll conceed that my paraphrase may have been off the mark. Susskind said "if the landscape is inconsistent" whereas I said "barring some kind of [should have inserted "consistent"] anthropic guide through the landscape". Admittedly those are two different conditionals. On the other hand, I don't think they are as unrelated as you do. Susskind's "if the landscape is inconsistent" could be taken to mean different things, especially in view of his other statements, where he refers to faith in the emergence of a unique solution. From what would a unique solution emerge "if the landscape is inconsistent"? He also talks about having nothing to determine the constants of nature in that case… and it's well known that he thinks the anthropic principle is the key to selecting from among the landscape of possibilities. But if the landscape itself is inconsistent (whatever that means), we wouldn't need any kind of guide to select the right one. But anyway… disregard my paraphrase…. the point of my message wasn't to paraphrase him, but to note that he made a comparison between string theorists (admitedly in a hypothetical situation) and proponents of ID. (By the way, I'm a bit surprised you didn't also object to Smolin's paraphrase, which if anything was even more tendentious than mine.)

Forget the paraphrases. Just focus on what Susskind actually said. I see that you don't agree with him, but my question was whether Susskind went beyond the pale by even suggesting that those who believe string theory may somehow produce a unique solution are comparable to proponents of ID. Do you disagree that this is what he implied? I guess it all depends on what he meant by "if the landscape is inconsistent". It's a rather muddled conditional, because even if the landscape IS consistent, surely there could still be theorists who think a unique solution could somehow emerge. In fact, we might more easily believe this if the landscape IS consistent than if it is not.

Taken in its entirety, I admit that it's not easy for me to assign a self-consistent meaning to Susskind's remarks, but I do think it's clear that he was, in some way, comparing how some string theorists might behave in some circumstances to how the proponents of ID behave. I suppose you could argue that his comments don't apply, because his conditional (landscape inconsistency) is both counter-factual and inconceivable, but then what was the point of his comment? I think you have to admit that he was acknowledging that string theorists could end up seeming like ID proponents. Does this put him "beyond the pale"?

30. **Aaron Bergman**
February 10, 2007

Do you disagree that this is what he implied?

Of course. He didn't say anything close to that. What Susskind is saying (and he thinks he's being clever — sigh) is that the universe looks fine tuned for the existence of life. Either we have to accept that the universe was designed for us or we have to accept some sort of multiverse and anthropic explanations. This is an old, old argument. Regardless, Susskind is using ID in a nonstandard way, referring to the design of the universe rather than the usual critiques of evolution. It is the people propounding the latter that are subject here.

31. **Amos Dettonville**
February 10, 2007

Here's an exact quote:

Susskind: "One might argue that the hope that a mathematically unique solution will emerge is as faith-based as ID."

Then I asked Aaron if he thought "Susskind went beyond the pale by even suggesting that those who believe string theory may somehow produce a unique solution are comparable to proponents of ID. Do you disagree that this is what he implied?”

And now Aaron replies:

“Of course. He didn’t say anything close to that.”

This is fascinating, in a peculiar sort of way. We have two statements:

(1) “One might argue that the hope that a mathematically unique solution will emerge is as faith-based as ID.”

(2) Susskind suggested that those who believe string theory may somehow produce a unique solution are comparable to proponents of ID.

Now, I will conceed that (1) and (2) are not identical, but I find it hard to accept that they are *not even close*.

By the way, Susskind isn’t using ID in a non-standard way. The idea of intelligent design (of the universe) has been debated for thousands of years, going back at least to the ancient Greeks, and has been featured prominently in the thinking of many great scientists and philosophers. The adoptation of these arguments by anti-evolutionists is what is non-standard.

32. **Aaron Bergman**
February 11, 2007

Oh good. Now we can play definition games. Why don’t you google “Intelligent Design” and find me the first link that doesn’t deal with anti-evolutionism of some sort. (What you’re referring generally goes by ‘the argument from design” or the “teleological argument”. In modern usage, “intelligent design” almost invariable refers to the argument against evolution, and, more importantly, before we started playing Susskind-gotcha-games — isn’t it fun? — it was the subject of this thread.)

Regardless, Susskind is not making a comparison between string theorists and IDers in general. What he is saying is that he believes that the belief in a single vacuum of string theory is not currently supported by the preponderance of the evidence, so that belief is “faith-based”. He hides behind a “one might argue” because he’s not actually making the comparison; he just thinks its clever to juxtapose the hope of some that string theory may have a unique vacuum with the faith that IDers have that a designer exists.

LHC Prospects

February 3, 2007
Categories: Uncategorized

Just to show that New Scientist doesn't always get it wrong, there's an unusually good article in this week's issue by Davide Castelvecchi about prospects for new discoveries at the LHC. Besides the usual story, he concentrates on the question of how the data will be analyzed.

For one perspective on the problem, he gives an amusing quote from Ian Hinchliffe of Atlas who says:

People always ask me, "If you discover a new particle, how will you distinguish supersymmetry from extra dimensions?". I'll discover it first, I'll think about it on the way to Stockholm, and I'll tell you on the way back."

Nima Arkani-Hamed optimistically claims that "The most likely scenario is that we're going to have a ton of weird stuff to explain,' and Castelvecchi quotes him and others as promoting a new sort of "bottom-up" data analysis. Here the idea (various implementations exist under names like VISTA and SLEUTH) is that instead of looking "top-down" for some specific signature predicted by a model (e.g. the Higgs, superpartners, etc.), one should instead broadly look at the data for statistically significant deviations from the standard model. Castelvecchi mentions various people working on this, including Bruce Knuteson of MIT. Knuteson and a collaborator have recently promoted an even more ambitious concept called BARD, designed to automate things and cut some theorists out of a job. The idea of BARD is to take discrepancies from the standard model and match them with possible new terms in the effective Lagrangian. Arkani-Hamed is dubious: "Going from the data to a beautiful theory is something a computer will never do."

While the article focuses on the LHC, the role of such "bottom-up analyses" may soon be explored at the Tevatron, where they have a huge amount of data coming in, and have already put a lot of effort into the "top-down" approach (for the latest example, see Tommaso Dorigo's new posting on CDF results about limits on Higgs decays to two Ws.) For the next few years all eyes will be on the LHC, but it will be the Tevatron experiments that will have a lot of data and be far along in analyzing it. Maybe lurking in this data will be the new physics everyone is hoping for, and it will be of an unexpected kind that only a broad-based "bottom-up" analysis might find. Doing so may raise tricky questions about what is statistically significant and what isn't, and require a lot of manpower, at a time when these experiments will be losing lots of people to the LHC and elsewhere.

Comments

1. **AGeek**
   February 3, 2007

   "Beating a grand master in chess is something a computer will never do."

   Sorry, couldn't resist.

2. **luzo**
   February 3, 2007

   «"Beating a grand master in chess is something a computer will never do."

   Sorry, couldn't't resist.»
   ———–x——–

   Well, you should have!

   Chess has a very deterministic set of rules which the computer has to follow to reach an optimal well defined state (winning the game).

   Extracting Physics from raw data implies induction and not deduction from well defined rules and defining the optimal outcome may actually be part of the problem.

   This is why I think this analogy is flawed.

3. **Jonathan Vos Post**
   February 3, 2007

   Von Neumann and Morgenstern invented Game Theory. John Forbes Nash, Jr., took it deeper, using polytope theory in his famous (but rarely read) PhD dissertation.

   John H. Conway et al then produced a Theory of Everything for games, including the transfinite and non-standard, of which Chess is merely a single point on the landscape of all possible games.

   John Baez has explained by Category theory what ("^" means exponentiation) is the structure of Chess^Go and Go^Chess.

   In a tournament, you don't care what your opponent's metatheory of Chess is, or whether thery consider Checkers to be in the same Calabi-Yau space with Chess. You simply want to falsify their theory, by winning!

   The analogy is thus: in the World Tournament of Physics, who wins the interminably protracted String Theory versus Everyone Else International Grandmaster Championship.

4. **Tommaso Dorigo**
   February 4, 2007

   Well, Luzo… "the computer", one day (quite soon, some say) will have induction, too.
   The singularity is near…
   Cheers,
   T.

5. **luzo**
   February 4, 2007

   "Well, Luzo… "the computer", one day (quite soon, some say) will have induction, too.
   The singularity is near…"

————x————-

It has been near for some 20 years now without almost any concrete evidence that it's getting closer.

Kind of reminds me of a TOE that has also been very promising for about a litle over 20 years and has delivered very litle when one demands concrete substance or even something to back one's "faith" in it.

Sorry, now I was the one that couldn't resist 😉

Induction is not the problem, the biggest problem (in my opinion) is that knowing when you have an optimal outcome is not even evident.

Bottom up approaches work in order to indicate a possible theory and then work top bottom to verify it.

6. **Seth**
   February 4, 2007

   Going from data to a *beautiful* theory is something a computer will never do, sure. Going from data to an effective Lagrangian, though, seems a more possible—at that point, though, the theorists will still have to step in.

   I don't expect BARD to be useful so much as a tool for discovering new physics, as for pointing people in the right direction on what to look for via more traditional methods with the next round of data.

7. **AGeek**
   February 4, 2007

   Beauty is in the eye of the beholder. Writing down all terms of a QFT action consistent with Lorentz invariance and renormalizability can certainly be reduced to a perfectly mechanical task; fitting experimental data to them too. As noted by Seth, the result may not be "beautiful" to some – but then, so what?

   luzo: the term "Singularity" does not refer to old-style AI (though your claim that the latter has "delivered nothing" is wide off the mark), it refers to the extrapolation of long-term historical trends in a wide range of technological and economic areas, all pointing to the year 2050 or thereabout as the time when they go essentially vertical on the time scale of (contemporary) humans.

   If you are an optimist like Kurzweil, you take this to mean that we will then break through to a whole new regime (human minds transferred to immensely faster electronic substrate, or whatever technology Intel will be peddling by then). If you are a pessimist, you take it to mean that 2050 or so the latest possible time when our civilization becomes unsustainable (runs out of gas and other resources maybe, Peak Oil and all that). Me, I prefer to be an optimist, since I fully plan to be around well after 2050. 😉

8. **luzo**
   February 4, 2007

   AGeek, thanks for clarifying the singularity point. I was indeed referring to old style AI (since it's what is related with the post) but didn't actually say it delivered nothing. I intended to say initial prospects were FAR too optimistic. Go and ask some working in AI in say the early 1980's whether they ever thought that by 2007 human computer interaction would be taking place using such contrived tools as the ones we are using right now. Most if honest will say they probably though by 2007 things would be much more advanced. Some would even thing that my doubts regarding things like BARD would be as irrelevant by 2007 as doubting the earth isn't flat.

   From my life experience I have to say things look to be slowing down and I still haven't seen the often mention (and desired) "paradigm shift". This in almost every field from Physics, social structure, Biology, etc. Nothing really interesting and corroborated for decades now. Physics at leats had this in the early 20th century, other fields are still lacking.

   Regarding Physics, I'm not a pessimist and I do hope the time we are living now is analogous to say the time when Kelvin was very old. Neither do I think the things that need explanation currently will remain without one for much longer, neither do I hope that explanation can be made with the tools we currently have. I'm not a big fan of singularities in human behaviour.

   Regarding the LHC, I tremendously doubt automatic data analysis alone will give us any theory that predicts yet unobserved phenomena. I could be wrong though.

   What I am really hoping for is for some really unexpected event observed at the LHC. Without a good wake up call from mother nNature this stagnation will most likely persist.

   One criterion for beauty is efficiency and conciseness (for me at least):)

9. **Alexey Petrov**
   February 6, 2007

   Strange point. I thought that all the people who do "low" energy phenomenology (including myself): B or D-physics, Peskin-Takeuchi parameters stuff, flavor-changing neutral currents in quark and lepton sectors, do just that — advocate a bottom-up approach by providing new low-energy observables which should be used in a combined analyses with direct studies at the LHC… after all, that's how any particular model's parameter space is constrained…

10. **Tommaso Dorigo**
    February 6, 2007

    About NS getting things wrong, a friend (David Orban) pointed me to a short article on the Higgs boson in the latest edition of the New Scientist, also online, which quotes me…

    In it, my name is misspelled! 🙁

    T.

Odds and Ends

February 3, 2007
Categories: Uncategorized

John Baez has a new issue of This Week's Finds. He has a lot of interesting things to say about Euler characteristics and how you measure sizes in a category. Among many things I learned from Graeme Segal when he was here last semester was the idea of thinking of the Faddeev-Popov prescription in the path integral approach to gauge theory as reflecting the fact that one should think of gauge fields as a category. The category is the category whose objects are bundles with connection, and gauge transformations are the automorphisms of these objects. The general principle that when you count objects in a category you need to divide by the number of automorphisms provides a sort of motivation of the Faddeev-Popov calculation.

The US FY 2007 budget situation for math and science, which was looking very bad just recently, has taken a huge turn for the better as the House has voted to increase funding for the DOE Office of Science and the NSF. More information about this here, here, here, and here. Looks like Fermilab and RHIC will emerge from the budget process unscathed. Soon the president's FY 2008 budget proposal will be unveiled, and we'll see what the new Democratic majority will do about science funding.

This week's press release announcing a "test of string theory" is from the University of Wisconsin (also here, here, and undoubtedly elsewhere). It's no competition at all for last week's spectacular press releases about "string theory tests", but like the many other examples of the genre it is designed to make claims about string theory highly likely to mislead unsuspecting readers. I made some comments about this here. It's not really that hard to come up with these "tests of string theory", since "string theory" now has been invoked to justify studying a huge variety of different kinds of models, and is compatible with just about anything. All you have to do is find one, no matter how complicated, obscure and lacking any evidence or motivation, where you can choose the parameters so as to create effects not visible to current experiments, but perhaps visible to potential experiments, even ones many decades down the road. Without too much trouble you should be able to get a paper about this published, and at that time your university press office will surely be happy to put out a press release for you announcing that "Researcher(s) at University X have discovered a way to test string theory." This has been going on for years, and people seem to never tire of it.

Sean Carroll seems to find it amusing that many articles on the arXiv can now be thought of as a new form of performance art. John Horgan, who got a lot of grief years ago for accusing physicists of engaging in "ironic science", should really enjoy this.

Update: The Distler et. al. media juggernaut rolls on, informing the world that : The LHC, due to be finished by and running by the end of the year, may rule out the string theory, as well as the work by Distler and his colleagues offers something profound – a way to actually test string theory, and that if Distler's bounds are satisfied, it would provide a weak support for string theory. The last article quotes Distler to the effect that string theory is just an "effective theory", which I'm sure will clarify this for the public.

Update: The Shiu et. al. "test of string theory" press release has also led to lots of misleading stories. For an example check out Physicists devise test for string theory, where you'll learn that "the University of Wisconsin theorists predict that upcoming experiments on the European Space Agency's Planck satellite will have the sensitivity needed to prove the case for string theory." Somehow I suspect string theorists will not give up on string theory if the Planck data doesn't work out, and there won't be any press releases…

Comments

1. **Sean Carroll**
   February 3, 2007

   I do find it amusing, although (of course) it has nothing to do with "many," or "now," or irony, etc etc.

2. **Eli**
   February 3, 2007

   FaDDeev

3. **newcomer to QFT**
   February 3, 2007

   It would be great if you have a chance to report more on Segal's Eilenberg lectures.

4. **Jonathan Vos Post**
   February 3, 2007

   Sean Carroll actually wrote about arXiv "Poetry" as opposed to "many articles on the arXiv can now be thought of as a new form of performance art" as summarized.

   Here's the difference: the paper (whether on the page or electronically published) can be poetry. The presentation at a conference (and on digitized video) can be performance art. Some physicists write great papers but give boring, confusing lectures. Some write trivial papers but give exciting oral presentations. A handfull (again, I'm a protege of Feynman, and coauthored poetry with him) can do both outstandingly well.

   Over in the Math world (but he also did Physics) Sato won the Wolf prise, but was famously bad at presentations, so that his work was essentially ignored for many years, despite his time at the Institute for Advanced Study.

5. **Peter Orland**
   February 3, 2007

   Peter

   It is good that mathematicians are thinking about
   how to study gauge-orbit space, via stacks or
   category theory, or whatever they think is
   useful. Having said that, the best progress on
   this front (in the last ten years), has been made
   by physicists.

   There are ways to parametrize orbit space which are
   more effective than the old methods – Karabali and
   Nair deserve some credit for this, and I will claim some

for myself as well. It is now clear how to solve Gauss's law in axial gauge for a lattice gauge theory. So far these techniques physicists have developed have been applied only to 2+1-dimensional QCD, but we all thinking hard about 3+1.

It's always good to have new points of view, but mathematicians working on this problem might do well to familiarize themselves with the progress made by the small community of physicists who still work on non-perturbative methods.

6. **woit**
February 4, 2007

Eli,

Thanks, fixed.

Sean,

Sorry for any misrepresentation of your views. Maybe I'm just in a lousy mood today, but the kind of thing you are finding amusing here I find depressing.

7. **Ralf**
February 4, 2007

"Performance art" or "ironic science" are nothing but euphemisms for fraud. If one does not believe in the scientific merit of one's projects one is committing fraud whenever one applies for funding and whenever one delivers a report on these projects. That is not a question of personal opinion. Any judge is well aware of this.

Fundamental physics is not merely in a crisis, it has ceased to exist. At some point it will need to be completely rebuilt on an entirely new foundation. But can do that when the entire field has been taken over by frauds, conmen, Witten adulators and Einstein heir wanna-bes? Everything is suffocated by an atmosphere of dishonesty, fundamentally faked convictions and the need to keep silent about the most obvious facts that rule out the b—s— to avoid losing the funding. Today, fundamental physics is nothing but lies in order to preserve funding and employment. *It never occurred to me that one day I would have to be ashamed of being a theoretical physicist*. What honest physicist could let any Ph.D. candidate pass who cannot debunk 99.9% of the subject matter of today's "research?" And if someone spells out this truth, he finds himself publicly contradicted *by people of whom he knows perfectly well that, in private, they agree with him completely.*

8. **Richard**
February 4, 2007

If Sean had given no clue that this was in fact a real abstract, I would have assumed that it was either a joke or fake physics babble in a science fiction story. I wonder what would happen to a math paper submitted to arXiv with an abstract that read like this.

9. **John Baez**
February 4, 2007

Thanks for the plug! [Euler characteristic and homotopy cardinality] are really cool, and I love how Leinster has begun unifying them using the notion of the [Euler characteristic of a category].

I first ran into the idea of "dividing by the number of symmetries" when learning how to compute Feynman diagrams. At the time I thought it was really weird. Why should a more symmetrical thing count for less. Later I saw it made sense and was part of a [big story] involving groupoid cardinality and ultimately homotopy cardinality.

By the way: I don't think Sean was trying to list abstracts that qualify as "performance art" or "ironic science". The [first one], by Jackiw and Pi, looks like good solid condensed matter physics (at first glance anyway).

10. **Peter Woit**
February 4, 2007

John,
The counting for Feynman diagrams with symmetries is Segal's favorite example of the principle. I was less impressed by this than the Faddeev-Popov idea, since Feynman diagrams are pretty much completely understood, whereas the non-perturbative quantization of gauge theories still seems to me to be something we haven't gotten to the bottom of (as Peter Orland also notes).

It's also intriguing to me that there are these generalizations of the "counting in a category" idea that seem to apply to spaces and use volumes (not just counting). I first saw this idea in the long Atiyah-Bott paper on Yang-Mills on Riemann surfaces, where they note interesting relations to Tamagawa numbers. This has always intrigued me, never known quite what to make of it…

About Sean's posting: I just mentioned it because I was very much struck by how he was amused by and seems to think highly of something like studying brane cosmologies where the branes "zoom and twirl around like multi-dimensional figure skaters", whereas this seems to me a typical example of what's wrong with theoretical physics these days. Nothing against this paper in particular, but the fact that there's a huge subject of research out there so outlandishly speculative and divorced from standard notions of scientific testability is not funny.

11. **Jonathan Vos Post**
February 4, 2007

First, I loved John Baez's latest posting about Euler characteristic of a category. Coincidently, I had just supplied my wife with some material on Euler characteristics for a Physics course she was teaching to university students majoring in fashion design. I supplied pages from B. Datta and A. K. Upadhyay, "Degree regular triangulations of the double torus", Forum Mathematicum, 18(2006)6, pp.1011-1025. These are to be treated as clothing patterns to be cut from cloth and sewed with edges identified, and the genus of the 3-D finished assemblage observed; after first sewing patterns for Platonic solids, asked to fill out a table of numbers of vertices, edges, and faces, and being led to discover Euler's formula for convex polyhedra.

Second, "If one does not believe in the scientific merit of one's projects one is committing fraud whenever one applies for funding and whenever one delivers a report on these projects." Fraud is defined on nolo.com as "Intentionally deceiving another person and causing her to suffer a loss. Fraud includes lies and half-truths, such as selling a lemon and claiming 'she runs like a dream.'"

Bringing funding into the picture allows one to determine a specific dollar figure for damages. But who has "standing" to sue? Whose ox is gored? A friend of mine was several consecutive years denied funding for an astronautical engineering concept he'd presented at conferences, then on his web site, and finally as an award-winning refereed paper in an engineering journal. The next year, another person (PhD in Physics) won exactly the Federal grant in question, for substantially the same material, even using the keyword which my friend had in fact coined and first published. The grant-winner (as I witnessed) both admitted and denied having read my friends's earlier work, could not explain where he got the keyword for the title of his grant application, and has been extremely hostile my friend and me subsequently. The grant in question was $75,000. My friend hired an attorney, who cocluded that proving fraudielent intent was allmost impossible, that intellectual property rightd were diminshed by open online and journal publication, and that the matter was not worth pursuing in court. My conclusion: the well-publicized frauds in science are about faked research data. The less-publicized but more common are about "lies and half-truths" and theft of intellectual property, but that these are diffult to pursue in court.

Remember the Math teacher who sued McDonalds, disputing how many billions of burgers had been sold? The judge agreed with the arithmetical argument, but denied that the teacher had been injured through the lies or half-truths, and thus had no standing to sue or prevail.

For string Theory, to use the Talmudic formulation, "whose ox is gored?" How can a Physicist hope to prove in court that he or she has been actually damaged by being denied hiring, promotion, tenure, or grants?

12. **Ralf**
February 5, 2007

The damaged parties are the federal government as represented by the funding agencies like the NSF and the DoE. I don't see how it is much different from, say, welfare benefit fraud or tax fraud.

13. **relativist**
February 5, 2007

"The Distler et. al. media juggernaut rolls on, informing the world that : The LHC, due to be finished by and running by the end of the year, may rule out the string theory, … the work by Distler and his colleagues offers something profound – a way to actually test string theory".
So if this is true, a genuine test as advertised, then if it comes out negative, presumably Distler, Green, Greene, Motl, Johnson, et al. will all give up string theory and do something else.
Does anyone believe that??? Not likely!

14. **King Ray**
February 5, 2007

String theory would make a strategic retreat to higher energies…

When theories retreat to higher and higher energies when confronted with experiment, they are probably wrong. Or not even wrong.

15. **Michael**
February 5, 2007

Why are you in such a lousy mood, Peter? Did your research suffer a setback?

16. **woit**
February 5, 2007

Michael,

Nope, research is fine, but progress is slow. I'm spending a lot of time working on the graduate course in representation theory that I'm teaching. It's time well spent since it is giving me new ideas and deepening my understanding of the subject. Always a pleasure to hear from charming anonymous string theorists such as yourself…

17. **Amos Dettonville**
February 6, 2007

The Feb 5 issue of Time magazine includes a full-page article under the banner **"The Geometry of Music: A composer has taken equations from string theory to explain why Bach and Bebop aren't so different".** Tymoczko has gotten a lot of coverage in the press over the past few months (for reasons that aren't obvious to me), but I'm just now noticing how prominently the "string theory" angle is being played. According to Time, *"Borrowing some of the mathematics that string theorists invented to plumb the secrets of the physical universe, he has found a way to represent the universe of all possible musical chords in graphic form"*.

They note that Tymoczko, a composer from Princeton University, is the first music theorist to have a paper published in the journal *Science*. His idea, for those who have somehow not heard about it yet, is that the universe of chords consists of "weird multi-dimensional spaces known as orbifolds". The simplest chords, consisting of just two notes, reside in a space like a Mobius strip. More complex chords *"inhabit spaces that are as hard to visualize as the multi-dimensional universes of string theory"*.

Harvard Magazine reports that *"Tymoczko spends time with scientists and it was a physicist who suggested the orbifold upon hearing his description of the musical map's properties. Tymoczko notes that physicists are forever using music as a metaphor for their work; now, he says, music can reciprocate."* This article goes on to say Noam Elkies helped Tymoczko with the math.

It occurs to me that this might be bigger news than people have realized. For years, string theorists have lamented the fact that "we don't really know yet what string theory is about". Maybe now we know. It isn't a theory of physics at all… It's about music! Or more specifically, it's a way of encoding traditional musical styles and pleasing chord progressions. But seriously, to the extent that "orbifolds" (in the sense used by Tymoczko) were invented by string theorists, this seems consistent with my impression that the mathematical machinery of string theory is actually a giant curve-fitting framework of almost limitless flexibility that can be fit to almost any subject. Does this marvelous applicability of mathematical techniques to music theory imply that this is the true final theory of music? Or is it a case of "When you have a hammer, every looks like a nail"? Interestingly, though, Tymoczko acknowledges that his theory won't really help anyone write better music. It essentially is just an attempt to represent formally some of the knowledge that composers have always known intuitively. Oddly enough, all the articles talk about chords, but none of them talk about rhythm, timbre, or any of the other aspects of music.

18. **Jun Xian**
February 6, 2007

I am the writer of one of the last articles quoted and I have a few issues that I would like to see clarified.

Firstly, Distler's meaning is not correctly interpreted in this quotation. I quote from the article directly:
– "Distler said that a successful outcome in the experiment, however, should not be seen as anything more than a weak support for string theory. The results of the experiment can effectively rule out certain quantum field theories but not serve as verification."
The full quote speaks for itself.

Secondly, an "Effective Field Theory" is not simply a field theory that is 'effective'. The reason why the article specifically placed these words in quotations is because it is a specific term referring to an approximate theory describing physical phenomena. I would hope the authors do their research adequately before quoting in a manner meant to misconstrue others' opinion.

Thank you.

19. **Ralf**
February 6, 2007

Amos,

I stipulate that absolutely nothing of any denotative content about string theory (which is not a theory) has ever been or could be conveyed to anyone who is not a physicist or a mathematician (or graduate student therein). There is absolutely no shortcut around the shift in the conception of physical reality of 1926 (and the less drastical ones between then and 1974). The physics illiteracy of the general public is a necessary condition for the rise of propagators of patent nonsense to the echelons of academia. People not just don't know anything about it, they don't even know enough to be aware that they don't know anything. (That includes anyone who doesn't know about Lagrangian and Hamiltonian mechanics.) And this won't change until it is acknowledged.

20. **Ralf**
February 6, 2007

String theory is the tantrum that spoiled rotten brats throw because they had to face the reality that they are not the *Übermenschen* that their inferiority complexes compell them to see themselves as.

21. **Astro**
February 6, 2007

You have all heard of NURBS: nonuniform rational B-strings.

22. **woit**
February 6, 2007

Jun Xian,

Seeing WW scattering that satisfies unitarity, Lorentz invariance and analyticity would not in any sense provide weak support for string theory. It would say nothing at all about string theory one way or another, and Distler's claim otherwise is just dishonest and intentionally misleading.

I know what an "effective field theory" is. Your text contains several inaccuracies about the relation between string theory and quantum field theory. It is not true that "String theory is the most successful of various quantum field theories..", string theory is not a quantum field theory. It is also not an "effective field theory". It's not surprising that you're confused about the relation between QFT and string theory, given that Distler et. al. have been on a campaign to try and confuse the issues in this case (their results apply to QFT, have nothing to do with string theory).

Instead of repeating unskeptically misleading claims about the significance of his work by a scientist, it would have been a good idea in your reporting to consult some other particle theorists than the authors of this paper, in order to see see if the author's claims are supported by their colleagues in this field.

23. **Steven H. Cullinane**
February 6, 2007

Some further details on "dividing by the number of symmetries"… The "general principle" mentioned by Peter and John sounds a lot like old-fashioned Polya-Burnside enumeration. (See John's "Week 147.") The "big story" paper mentioned by John is "Categorified Algebra and Quantum Mechanics, by Jeffrey Morton, which cites two papers by Joyal dealing with the combinatorics of "structure types" (as does John's "Week 185"). For details of the connection between old-fashioned Polya-Burnside enumeration and Joyal's structure types, see a 1999 dissertation, "Graphical Enumeration: A Species-Theoretic Approach," by Leopold Travis. The Travis paper is pure mathematics rather than physics, and so may appeal to a wider audience than the "big story" paper.

24. **Jun Xian**
February 6, 2007

Well, if you understood what an effective field theory is, perhaps you should not have stated "The last article quotes Distler to the effect that string theory is just an "effective theory", which I'm sure will clarify this for the public.", which is a rather overt attempt of yours to mislead the public, in my opinion.

Not to mention that I clearly stated in my article that Distler's words were to the effect that it could ONLY serve as a refutation if the WW scattering did NOT satisfy the bounds. Looked at from a different angle, failure to disprove can be taken as an extremely weak support.

I admit to making some technical mistakes in the article, which have since been corrected. However, I do not appreciate the exploitation of my personal failure at understanding as a method to attack Distler's work.

I have nothing for or against either side of the issue. As such I have no quarrel with you concerning the truth of his theory or otherwise. I simply would not like my article to be misquoted and hence cause undue misrepresentation of Distler's work, no matter it's value.

Thank you.

25. **woit**
February 6, 2007

Jun Xian,

My quotes from your article were accurate, and included a link to the actual article so that anyone who wanted to could see them in context.

Now you've turned the "weak support" from your article into "very weak support'. Again, the accurate statement is that if these bounds are satisfied, this has nothing to do with string theory. It would not provide "weak support" or "very weak support", it would have nothing to say at all about the subject.

I don't blame you for technical mistakes. This is a difficult subject and non-experts are going to have trouble understanding it. The inaccurate statements in your article seem to me to be the direct result of Distler's misleading you about the significance of his work.

26. **Big Red**
February 6, 2007

Amos Dettonville,

Tymoczko's father was a sort of logician/philosopher of mathematics and his sister is an assistant prof in math at Michigan. This might be the simplest explanation for any affinity he might have for mathematical ideas.

27. **hooman**
February 6, 2007

I know this is a bit off the topic, but your title is Odds and Ends and I could not resist…
Is this what Lubos is driving these days?
http://www.toyota.com/vehicles/2007/matrix/m_theory.html
the add says " a theory you can't wait to put to test! "

Has anyone studies the impact of string theory on consumer products yet, or vice-versa?

28. **mclaren**
February 7, 2007

Hesitated to mention Tymoczko's paper because it leads to such an obvious swamp of pseudoscience. The ultimate end point is Keely's Sympathetic Vibratory Physics:
http://www.svpvril.com/musicuni.html#TOP%20musicuni

And of course Bruce Cathie's series of ufology books, including "The Harmonic Conquest Of Space" (1998):
http://www.amazon.ca/s?ie=UTF8&rh=n%3A952076&page=15

Tymoczko's twaddle is such obviously vacuous pseudoscience it requires little discussion. But the fact that Tymoczko saw fit to trumpet some alleged connection with string theory seems ominous. It suggests the thin membrane (or should I say "brane"?) which separates mathematically valid but entirely speculative science from "that Serbonian bog," as John Clerk Mawell remarked in his Rede Lecture of 1876, "which has swallowed whole armies of scientific music-makers and musical men of science."

I suppose we must draw a distinction twixt speculative untestable nonsense which is nonetheless based on known laws of physics and contains valid mathematical procedures (though never leading to a testable calculation) and the outright gibberish purveyed by the John Keelys and Hans Keysers and Bruce Cathies of the world. But frankly, if you can't test any of it, I don't care for such fine distinctions.

Incidentially, there's a veritable Himalayan mountain of musical pseudoscience dealing with harmonics and eigenfucntions and various and sundry crackpottery. Tymoczko is just the tip of the iceberg.

http://www.altered-states.net/barry/update95/index.htm
http://www.sacred-texts.com/eso/sta/sta19.htm
http://www.sacredscience.com/archive/Kayser.htm

And so on. A tidal-wave-sized sewage spill of superstition masquerading as a mathematico-musical "theory of harmonics." Classic neo-Platonic number mysticism, acoustical gematria pure and simple. Perhaps string theory could be renamed "cosmological gematria"…?

29. **Chris Oakley**
February 7, 2007

I suppose you need a compact vehicle to navigate the compact dimensions. And you would need pretty good cornering to handle the 10^-35 m (or thereabouts) radius. No doubt Toyota have thought of all of that.

30. **Alex Nichols**
February 7, 2007

King Ray Says:
February 5th, 2007 at 4:26 pm
"When theories retreat to higher and higher energies when confronted with experiment, they are probably wrong. Or not even wrong."

Strictly speaking, the alchemists were NOT WRONG, even if they never did find the Philosopher's Stone
– Lead CAN transmute to Gold.

Upon discovering thorium changing to radium, Rutherford said to his assistant.
"For Christ's sake, Soddy, don't call it transmutation. They'll have our heads off as alchemists."

About 60 years later, Gold was produced in reactors. Without a handy Supernova, you'll never make a living at it though.

31. **Thomas Larsson**
February 7, 2007

Has anyone studies the impact of string theory on consumer products yet, or vice-versa?

It is not yet as successful as cold fusion.

32. **Thomas Love**
February 7, 2007

I've been aware of the connections between music and mathematics for a long time. Back in 1999 I was walking through a university book store and saw a book on harmonic analysis in the music section. I already owned the book, so I put it in the mathematics section. The next day, it was back in the music section. I returned it to the math section. The next day, it was back in music at which point I gave up.

33. **Chris W.**
February 7, 2007

Thomas Love,

You should have affixed a Post-It Note (or something) that sarcastically suggested to the bookstore staff that they open the book and flip through a few pages before jumping to conclusions about the subject matter. Then again, it probably wouldn't have helped……

34. **Chris W.**
February 7, 2007

It seems that in the minds of many people, including many journalists (and even, I suspect, more than a few mathematicians) that theoretical physics and mathematics have become an undifferentiated mush.

An interesting question: How does one explain to somebody that applying an aspect of a physical theory's *formalism* to a problem in an otherwise unrelated field is not the same as applying the theory to said field, and that identifying and characterizing the abstract structures underlying such cross-applicable formalisms is what mathematics—as opposed to the natural sciences that employ mathematical formulations in their theories—is all about.

[By the way, Dmitri Tymoczko evidently has a sister who is a mathematician (Michigan).]

> But this long history of learning how not to fool ourselves—of having utter scientific integrity—is, I'm sorry to say, something that we haven't specifically included in any particular course that I know of. We just hope you've caught on by osmosis.
>
> The first principle is that you must not fool yourself—and you are the easiest person to fool. So you have to be very careful about that. After you've not fooled yourself, it's easy not to fool other scientists. You just have to be honest in a conventional way after that.
>
> I would like to add something that's not essential to the science, but something I kind of believe, which is that you should not fool the layman when you're talking as a scientist. I am not trying to tell you what to do about cheating on your wife, or fooling your girlfriend, or something like that, when you're not trying to be a scientist, but just trying to be an ordinary human being. We'll leave those problems up to you and your rabbi. I'm talking about a specific, extra type of integrity that is not lying, but bending over backwards to show how you are maybe wrong, that you ought to have when acting as a scientist. And this is our responsibility as scientists, certainly to other scientists, and I think to laymen. ***—Richard Feynman, "Cargo Cult Science"***

Quick Links

February 7, 2007
Categories: Uncategorized

Alexey Petrov, a particle theorist at Wayne State University, has a blog worth following called Symmetry Factor. He has news about the 2008 budget request for HEP at the DOE, which according to him includes a 12.7% increase in the final 2007 number and a 3.7% increase for 2008 above that (not sure where his numbers come from). This would be a very healthy increase over these two years. Research in the physical sciences has become a big priority for the Bush administration for some reason, it's a major part of the "American Competitiveness Initiative". The NSF is also seeing a large increase in its FY2008 request: 8.7%. For various news stories about this, see here, here, here and here. Still unclear what will happen to this budget request in Congress where the Democratic majority will be in control. They have been sympathetic to science research spending in the past, but may or may not want to go along with the emphasis on the physical sciences embodied in this request. Then there's the small matter of the huge US government deficits to consider. Somebody, someday might decide to try and do something about them.

Maybe I've been a bit unfair in the past to the Templeton Foundation, which recently issued this statement.

The IAS held a workshop last month on Homological Mirror Symmetry, notes are available here. Next month there will be a part two, which will mainly concentrate on Geometric Langlands. The schedule is here.

Via Jonathan Shock, the news that particle physicist Nick Evans has written a particle physics murder mystery entitled The Newtonian Legacy, and is making it available on-line for free. I'll definitely be reading it soon.

Last evening I gave a talk here in New York downtown at the Cafe Scientifique. I think the talk went quite well: the place was packed, the audience attentive and asked quite a few good questions. Up next month is Glennys Farrar of NYU, who will be talking about dark matter. This event is pretty new, organized largely by Stefanie Glick who just got it started last fall. Also in New York are two other similar monthly science events: Secret Science Club, organized by Dorian Devins at Union Hall in Brooklyn (Janna Levin will be there tonight), and Columbia's Cafe Science, which features Columbia faculty members (my colleague John Morgan will be speaking with Sylvia Nasar next week).

Comments

1. **Peter Orland**
   February 8, 2007

   Hi Peter,

   I don't think you have been so unfair to the Templeton foudation in the past. In my experience most seriously religious people may accept certain scientific facts on the surface, but have a very unscientific point of view at their core. I have even heard more compromising religous types talk about how God guides evolution, that the Big Bang was a conscious act of Creation, etc. I don't want to be nasty or contemptuous of people who want to experience religion in some
   way through science, but to start putting the Big Kahuna(s) in the middle of scientific facts is wrongheaded at best, dumb at worst. To an atheist like me (most scientists as well, I think), this seems to be lot of excess baggage.

   Now I'll probably get email threatening my death by cancer.

2. **Alexey Petrov**
   February 8, 2007

   Dear Peter,

   Thank you for referencing my blog! My numbers regarding the budget increase are from the Under Secretary Orbach's presentation of the 2008 budget request, see footnote on page 4. The pdf file of this presentation is here: http://www.science.doe.gov/Budget_and_Planning/Budget_Rollouts/DrOrbach-FY2008-SCBudgetRollout-Feb05-stakeholders.pdf

   Regards,

   –Alexey.

3. **QWERTY**
   February 8, 2007

   DEAR PROFESOR WOIT,

   PLEASE DO NOT ENDORSE SYLVIA NAZAR. SHE HAS SOILED THE REPUTATION OF THE ENTIRE MATHEMATICS WORLD WITH HER UPSIDE DOWN ARTICLES ABOUT SHING TUNG YAU.

   ALSO (ABOUT THE MIRROR SIMMETRY) I KNOW ABOUT THE SYZ CONJECTURES BECAUSE THEY ARE JUST ABOUT SPECIAL LAGRANGE MANIFOLDS, BUT WHERE SHOULD I LEARN ABOUT TRIANGLED AND DERIVED CATEGORIES TO LEARN HOMOLOGICAL MIRROR SIMMETRY? AND WHERE CAN I LEARN WHAT IS A STACK AND WHY I NEED IT IN GEOMETRICAL LANGLANDS PROGRAMS?

   AND HAS EDWARD WITTEN AND ANTONY KAPUSTIN AND SERGAY GUKOV ACTUALLY DONE NEW MATHEMATICS ABOUT GEOMETRIC LANGLANDS OR IS IT JUST TALKING ABOUT IT IN THE LANGUAGE OF STRING THEORY

4. **Peter Woit**
   February 8, 2007

   QWERTY,

   I'm not endorsing anyone, please don't engage here in pro or anti Yau diatribes, I want nothing to do with that and will delete any further comments of this kind.

   Derived categories are what you get when you generalize homological algebra to consider not just the homology groups of complexes, but complexes up to homotopy. For an introduction, see

   http://www.arxiv.org/abs/math.AG/0001045

Stacks are useful whenever you want to work with objects with symmetries. Instead of thinking about the points of the space, you think about the category whose objects are points, morphisms symmetries. There's a collaboration underway to write a new textbook on the subject, with preliminary versions of some chapters here
http://www.math.unizh.ch/index.php?pr_vo_det&key1=1287&key2=580&no_cache=1
You might try reading the introductory chapter.

I don't know geometric Langlands well enough to know whether the Witten/Kapustin/Gukov work provides mathematical insights that the mathematicians working on this were not already aware of. The recent work of Witten and Gukov does seem to correspond to topics that mathematicians in this area are actively working on. I hope to make it down to the IAS in March to learn more about this.

5. **island**
February 10, 2007

I've never been able to completely uncover the true underlying motivations of Templeton, but Paul Davies is a publicly professed atheist who was forced to accept the Templeton prize only because they fund research into strong interpretations of the anthropic principle, and somehow, I just don't see the Discovery Institute doing this. Davies and others are forced to take money from them because nobody else will fund research into the most apparent implications of the observed universe, so I'd imagine that he'd tell people to get their dogmatic anticentrist priorities straight, or get over it, because they're unfounded belief system is no better than the idists.

I know for a fact that "leading" IDists, including Bill Dembski, do not like, nor do they support the position of the Templeton Foundation, so Templton certainly does not fit the profile of your standard fanatical ID proponents who would abuse science in order to force religion into the high school curriculum. Other than that, this isn't high school, children, so if a person wearing a cross around their chest is also holding science in their hand, then you should get over it and listen to them, because your otherwise unfounded paranoia will destroy ya…

> The "leap of faith" needed to accept the existence of a multiverse is greater than that normally expected of scientists, which is to assume the unexplained existence of the laws of physics, although it is perhaps less than that required for belief in a cosmic designer who made a universe fit for life.
> -Paul Davies

Perhaps? … 😉

ILC Price Tag

February 8, 2007
Categories: Uncategorized

The “Reference Design Report” for the ILC was released today, and here’s a presentation about this from Barry Barish, the director of the ILC GDE (Global Design Effort). The most closely held numbers in the report have been the cost estimates (see here for a document about the status of cost estimates that warns “don’t post cost estimates on public web or wiki sites!”).

The cost estimate comes out to $4.87 billion for the technology components, $1.78 billion in site-specific costs, 13000 person-years of labor, and two detectors (no cost estimate for these). In round numbers, roughly $10 billion. The machine would consist of two 11km linacs end-to-end, with an interaction region in which two detectors could be moved in and out. The biggest part of the cost is the cost of the linacs, which would accelerate electrons and positrons to tunable energies with collisions at center of mass energy between 200 and 500 Gev. A possible future upgrade of the machine would take it to 1 Tev.

The plan for the future is to start working on a “Technical Design”, a much more detailed design that would show exactly how to build the machine. The hope is to make a decision on whether to build the ILC around 2010, based on what the LHC has found, and on how much progress CERN has made on the much more ambitious CLIC design. Construction would take 7 years, so the earliest such a machine could be in operation would be around 2017.

The full report is here.

Update: More at Science magazine and the New York Times.

Update: Joanne Hewett has an excellent detailed posting about this over at Cosmic Variance.

Comments

1. **kuos**
   February 8, 2007

   A small price to pay to test string theory and find our place in the multiverse!

2. **Tony Smith**
   February 8, 2007

   Peter said “… The “Reference Design Report” for the ILC … cost estimate comes out … [i]n round numbers, roughly $10 billion …”.

   A competing way to spend a comparable amount is on a new aircraft carrier ($8 billion in construction cost alone) .
   According to a navydata web page:
   “… The CVN 21 Program is the future aircraft carrier replacement program for USS Enterprise and CVN 68-Class aircraft carriers. …
   The total cost to build the lead ship … CVN 78 … is $8.1B in FY08$.
   The Navy expects to award the CVN 78 construction contract in FY08 with an expected delivery in FY15. …
   the total ownership cost for CVN 78 … over its 50 year service life … is expected to be $26.8B …”.

   In short, by foregoing a new navy aircraft carrier, the USA fund its share of the ILC and have around a couple of billion left over (maybe to fund high energy theory alternatives to superstring theory?).

   According to an 8 February 2007 Reuters article by Edmund Blair:
   “… Iran’s Revolutionary Guards test-fired missiles on Thursday …
   “These missiles, with a maximum range of 350 km (220 miles), can hit different kinds of big warships in all of the Persian Gulf, all of the Sea of Oman and the north of the Indian Ocean,” senior Revolutionary Guards naval commander Ali Fadavi said.
   Fadavi was also quoted by the state broadcaster’s Web site as saying that the 500-kg (1,100-lb) warhead of this missile had the capacity to sink “all kinds of big warships” …”.

   A military commentator, Gary Brecher, said back around December 2002:
   “… A few years ago, a US submarine commander said, “There are two kinds of ship in the US Navy: subs and targets.” …”.

   Why doesn’t the USA forgo building a new big target for enemy missiles,
   and
   use that money toward building the ILC ?

   The carrier construction cost alone is over 80% of the total ILC cost estimate, and should by itself more than cover the USA contribution toward the international ILC project.

   Tony Smith

   PS – Here is a bit more from the military analysis web page mentioned above:
   “.. The fact that big surface ships are dinosaurs is something that’s gotten clearer every decade since 1921. … the aircraft carrier is now: a big, proud, expensive…sitting duck. Aircraft carriers came out of WW II looking powerful, but that was before microchips. …
   what about Iran? … The Iranians are … smart, they’re dedicated, and they hate us like poison. … Give the Navy the benefit of the doubt and say they get 90% of the incoming missiles. You still end up with a dead carrier. …
   … if Iran gets involved, those carriers won’t last one day. … And the sickest part is that the admirals and the captains and the contractors all know it. …
   it won’t be the brass who die. It’ll be the poor trusting kids on those carriers who’ll die, the poor suckers who thought they’d get free training and a world tour, or even get the chance to “defend America.” They’ll die not even believing what’s happening to them as the whole giant hulk starts cracking up and sliding into the water. …”.

3. **Q**
   February 8, 2007

   ‘Construction would take 7 years, so the earliest such a machine could be in operation would be around 2017.’

   Who is predicting that the LHC won’t provide the evidence for the real electroweak symmetry breaking mechanism within 10 years?

Or is this new project really more concerned with searching for massive stringy bosonic superpartners?

4. **Peter Woit**
February 8, 2007

Q,

The LHC will actually reach higher energies than the ILC, but if it finds something interesting, it may be very difficult to study it due to the much more complicated nature of proton-proton vs. electron positron collisions.

Basically, if the LHC finds nothing new in its accessible energy range except the expected SM Higgs, it will be hard to make the case for the ILC. If, as people hope, something new shows up, depending on what it is, the ILC may be needed to study it. One popular example is supersymmetry. Some people expect the LHC to see all sorts of superpartners, and the ILC would be a much better machine for studying these (IF their masses are low enough) than the LHC. In general, if the LHC finds that there's something more interesting going on in electroweak symmetry breaking than a scalar Higgs, again the ILC may be the right machine to study the phenomenon. All depends on exactly what it is….

5. **Tony Smith**
February 8, 2007

Q asks "… is this new project [the ILC] really more concerned with searching for massive stringy bosonic superpartners? …".

No.
Although the ILC might see stringy stuff such as superpartners if they exist (my opinion is that they do not exist),
that is not why I think that a high-energy lepton collider such as the ILC should be built. Here is why a non-superstringer like me wants to build such a thing:

The ILC could do a lot more precision tests of the Standard Model than LHC can do, so the ILC could either:
1 – find more subtle new stuff than can be seen at the LHC
or
2 – verify the Standard Model to much greater precision.

Either result would be significant.

For example, the EPP2010 report said in part:
"… The ILC will probe the Standard Model with unprecedented precision … involving the masses, lifetimes, and reaction rates of W and Z particles, top quarks, possibly Higgs particles, and others. If the Standard Model survives these tests, physicists will gain a new level of confidence in its validity and scope.
…
Even such a basic property of the Higgs particle as its spin cannot be easily measured at the LHC. The Standard Model requires that the Higgs particle has no spin … If a Higgs particle is discovered, the spin can
be measured straightforwardly by determining the rate at which it is produced at different energies at the ILC. …".

Therefore, I disagree with Peter when he says "… if the LHC finds nothing new in its accessible energy range except the expected SM Higgs, it will be hard to make the case for the ILC …".

Tony Smith

6. **wab**
February 8, 2007

My colleagues also argue point 2) about high precision measurements of the standard model. That arguement may sell to high energy physicists but I think it will be a hard sell to the other parts of the APS, let alone the general public… so we'd better hope for positive discoveries from the LHC.

7. **graviton383**
February 8, 2007

The $6.7 billion number is in `European' accounting & does not include overhead, contingency or any money for detectors. In `US accounting' the total is closer to $15B…

8. **Robert Musil**
February 8, 2007

At this point it seems quite unlikely that the ILC will be funded unless it is adopted by one or more Asian economic power (most likely China, but maybe India and/or Japan) seeking to make an essentially nationalist political statement about having "arrived" in the advanced world. The problem is that even the Standard Model, with all of its vaunted (perhaps excessively vaunted) cleverness and accuracy of prediction, has not made much difference on any politically significant issue … still less to anything that a Western politician could point to as justification for spending over $10 Billion in tax money … or even a substantial portion of that inevitably-to-bloat figure. Once one moves to questions outside the Standard Model things get even flakier and harder to justify financially and politically. So what's the point from a political perspective? That's the main reason why the old Super Conducting Super Collider eventually came to be seen as mere "pork" in the US Congress. That "pork" perception led to its defunding, since there were other pork projects with far more influential constituencies than any that supported the SCSC. (The LHC builders were lucky that Europe loves this kind of pork, but even Europe's appetite for physi-pork seems sated by the LHC itself.) It's hard to imagine what question the ILC possibly could settle or illuminate that would justify its cost in the political theater. Unfortunately, it's all too easy for academic researchers to imagine just such a question that matters to them … which speaks mostly for the political, economic and historical detachment of the current crop of academic researchers. Undemocratic Asian nationalism appears to be the ILC's only hope, which is one reason why China is a better hope than democratic Japan or India. But that is slender hope indeed.

9. **heh**
February 8, 2007

And here I thought Robert Musil wrote *The Man Without Qualities*, not Comments Without Quality. The most likely location for the ILC at this point seems to be Fermilab. There has been no serious talk, as far as I am aware, of building it in China or Japan. The way to sell the US Congress is to point out that (a) the energy frontier will have been in Europe for some time, and (b) the US tradition of particle physics experiment will be practically dead if we do not build the ILC. Appeal to the fear that the US is slipping far behind in science and technology. Talk about the technological spin-offs that come from accelerator R&D.

10. **heh**
February 8, 2007

Sorry, I meant "no serious talk… of building it in China or India." Japan is a possibility but does not seem likely.

11. **David Heffernan**
February 9, 2007

In response to Robert Musil and heh, Japan is a very serious contender to host the ILC. The Japanese High Energy Physics Advisory Panel agreed that investing in R&D on the linear collider, and then hosting the machine, was the "highest priority" for HEP in this country. The government has already formed a committee to investigate funding and advise how to bring the machine here.

It goes without saying that the host nation will have to make a significant investment in the project, and as far as I am aware the Japanese government is the only one seriously prepared to pay for the privilege at the moment.

12. **Robert Musil**
February 9, 2007

David Hefferman:

I agree that Japan is certainly a possibility as an ILC host to the extent there is any such possibility. Japan has the money and the brainpower – and in some quarters of the electorate an unfulfilled desire to show the West that Japan has "arrived," perhaps by using ultra-expensive particle physics as a demonstration. But any such desire is uninformed atavism. Japan has nothing to prove – it "arrived" in technology and science a long, long time ago. It may be possible to politically exploit such atavism to support the ILC, but that's a slender reed indeed.

Forming a committee to investigate funding and advise how to bring the machine to Japan is very far indeed from actually spending a major piece of $10 to $15 Billion and (probably) way up. In my opinion it is very premature to suggest that the Japanese government is seriously prepared to pay for the privilege. The Japanese High Energy Physics Advisory Panel does not make the decision as to whether the government of Japan is going to spend that kind of money on the ILC or instead on, say, a suspension bridge to some fishermen's island that happens to lie in the district of some well connected national representative.

It would also be premature to count China out. The New York Times article to which Peter linked above points out:

"The location of today's announcement, at the Institute for High Energy Physics in Beijing, underscores the growing role and ambition of Asia, particularly Japan and China, to become major players in high-energy physics, a field that has been dominated by the United States and Europe in the last century."

The fact is that if China were willing to host and fund the project, there is a very good chance it would be built there. Moreover, Chinese government processes are almost completely non-transparent, so there is no way to know if senior government people might at some point latch onto the ILC as a nationalist vanity project. That would be quite consistent with many other things the Chinese government is now doing. Of course, the price tag for the ILC is pretty big compared to almost anything.

Europe, on the other hand, is extremely unlikely to host ILC with the LHC already up and running. Unlike China's non-transparent system, the absence of any serious voiced interest from Europe tells a lot about what the Europeans are likely willing to do. If there were serious interest, some sort of response along the lines of those of Japan described by Mr. Hefferman would be in view. Nothing is in view.

What about heh's argument that "The way to sell the US Congress is to point out that (a) the energy frontier will have been in Europe for some time, and (b) the US tradition of particle physics experiment will be practically dead if we do not build the ILC. Appeal to the fear that the US is slipping far behind in science and technology. Talk about the technological spin-offs that come from accelerator R&D." Well, it's hard to take this argument seriously given historical precedents. All of these arguments were made repeatedly in support of the Super Conducting Super Collider and they did not work. Nothing has changed since then for the positive.

The fact is that there are many forms of science and technology, and it is a very hard political sell to explain to the public and their elected representatives why it means much if the US "falls behind" in this particular technology – which means virtually nothing to the lives of the people writing the tax checks. Other forms of technology clearly matter to taxpayers. A few years ago the EU came together in the so-called "Lisbon Accord," which stipulated that Europe would become the world capital of intellectual property development. Silicon Valley laughed, and the Europeans don't talk too much about that silly accord and its ambitions now. Falling behind in that technology clearly means a lot to elected officials, as the existence of the Lisbon Accord demonstrated.

13. **Haelfix**
February 9, 2007

As much as I would love to see the ILC become a reality, and would gladly donate generously out of my pocket to achieve such, I do see the political baggage such a thing costs.

There really isn't a great justification for spending those sums of money, outside of the simple answer 'just to know more'. A lot of the claimed side effects of spawning new technology and so forth tend to be overblown. Nasa gives this line everytime they have a new plan to send people to Mars/moon/etc and after decades of this, the politicians have grown a little skeptical (justifiably so too).

You know the usual line we get at cocktail parties
'Exactly what use is your field, and how does it benefit me?'
I now simply state '99% chance that it leads to absolutely no pragmatic app in our lifetime, and perhaps never'

14. **Steve Myers**
February 9, 2007

HEP got spoiled after WWII because of the fear of the bomb & the Russians & so $$$ weren't hard to get. There's money for crazy stuff like Reagan-Bush-Bush Star Wars but not for basic research; and the Pentagon (Carriers, planes, etc.) always gets what it wants no matter how useless. Maybe getting across the idea that basic research is important for its own sake has to start in grade school?
But if ILC is built my guess is it will be in China. Also, a not irrelevant note: as of Nov 2006 major foreign holders of US Treasury securites (in billions): Japan 637.4, China 346.5, UK 223.5. So whatever funds the U.$. contributes will be borrowed from Japan & China.

15. **dark-matter**
February 10, 2007

Whether ILC proceeds or not depends on reality in the 2017 era. First, the technical case: no ILC-class machines will be built by anybody unless LHC discovers something absolutely astonishing (like the discovery of nuclear energy decades ago) that has the potential to disrupt human fundamentals, opening up a new direction, and points to major military implications.

Suppose such discovery occurs. Then the major powers will want to jump in and build the next machine to exploit it. What machine depends on what is discovered. The proposed ILC design is based on current theoritical knowledge and its projections. BUT these projections have largely been discredited in the eyes of the politicians and a sizable portion of the public. That's why everybody's looking for the real thing, LHC, for guidance and not theories. So if the next machine is to be built, it is very unlikely to be this ILC.

As for China, it certainly wants to participate in the next big machine but won't insist having it on its soils. China knows it just doesn't have the ability to build any of the sophisticated components and support systems, including computing facility. Those who can actually design and build it have the greatest say on location.

Unless there's a dramatic change in the reputation of USA in the next decade, an ILC type machine will not be built there. The mess with the Space Station construction have just about destroy the chance of any country, even friendly ones, wanting to do such an international project again. USA is looked upon with high suspecion – everything must have a military angle, there's the American way or the highway, unreliable funding from Congress even after treaties and contracts were signed, high-handed politics at every turn. So if US wants the next machine it on its soil, it must do it alone and take the lead on everything. But if Congress does see a justification for this huge investment, it will do it. Afterall, $20B is chump change if the politics is right. An game-changer discovery by the LHC, supported by theoritical work, will make the politics right. Otherwise, the money will continue to go to space-based instruments for fundamental physics.

16. **mclaren**
February 10, 2007

By way of comparison, America spends 5 billion a year on potted plants. We can easily afford it. The tragedy is that we'd rather build mythical "missile defense" systems that don't work (40 billion so far and counting, no end in sight) than create machines that explore the basic constituents of the universe.

Robert Musil remarked: "The problem is that even the Standard Model, with all of its vaunted (perhaps excessively vaunted) cleverness and accuracy of prediction, has not made much difference on any politically significant issue … still less to anything that a Western politician could point to as justification for spending over $10 Billion in tax money … or even a substantial portion of that inevitably-to-bloat figure. Once one moves to questions outside the Standard Model things get even flakier and harder to justify financially and politically."

And there's the irony. Neutrino mass is arguably outside the Standard Model. Although the SM can be fudged and fiddled to include neutrino mass, the SM never predicted it. And how was neutrino mass discovered? With a relatively dirt-cheap experiment. A big tank of cleaning fluid and some photomultipliers. No 10 billion dollar investment, no 20 mile long superconducting accelerator.

Maybe we're better off w/o the ILC. Maybe we need to follow Rutherford's example and experiment smarter, instead of experimenting bigger.

Many have said it before, but it bears repeating: the 3 biggest experimental results over the last 20 years didn't come from giant accelerators. They came from a tank of cleaning fluid in a mine shaft and a couple of relatively inexpensive satellites. Maybe the glory days of the giant accelerators are past.

17. **Tommaso Dorigo**
February 10, 2007

Dear mclaren,

the Standard Model could not "predict" the neutrino masses, any more than it allows the prediction of the mass of charged leptons or quarks. I personally think that the neutrino masses force an extension of the Standard Model more or less as revolutionary as a rearrangement of the furniture in the kids' room forced by the arrival of a new baby.

Mind you, I am not trying to derate the significance of a measurement that we had all been waiting for, a confirmation of tens of years of chlorine experiments and helioseismology – although I believe the discovery of the top quark, just as unsurprising, was not less important -and indeed a confirmation of the Standard Model.

Anyway, I concur – maybe we do not need the ILC. And I personally hold that it won't be built, despite the US ambitions to bring back HEP frontier experiments to their soil or Japanese or Chinese pride. That is because the LHC will, IMO, not discover anything else than a regular Higgs boson…

Cheers,
T.

18. **mclaren**
February 10, 2007

"This discovery shows that it is likely that the Standard Model, proposed in the 1970s to describe the fundamental forces and particles that make up all matter, is incomplete."

(..)

"Theoretically, neutrinos can change from one flavor to another only if they have mass. The Standard Model, however, assumes neutrinos are without mass."

Global Team of Scientists Upends Standard Model With Discovery of Neutrino Oscillation, Mass
9 Jluy 2004
http://www.sciencedaily.com/releases/2004/07/040709082752.htm
——
"Within the Standard Model, the neutrino has a zero mass…"
Didier Verkind, 5/20/1999
http://wwwlapp.in2p3.fr/neutrinos/ankes.html
——
"Neutrinos are exactly massless in the Standard Model, but recent experimental observations show neutrinos oscillating between different flavors."
Beyond the Standard Model, 2007, Glenn Elert
http://hypertextbook.com/physics/modern/beyond/
——
"However, the Standard Model predicts that neutrinos have no mass!"
The Latest Experiments Detecting Neutrinos Suggests They Have Mass,
http://www.hep.phys.soton.ac.uk/archive/15f4.pdf
——
I'd be delighted to be proven wrong, since in that case I would learn something.

So I'd ready and willing to believe that all of the above statements are incorrect and the SM does indeed predict a non-zero mass for neutrinos. All you have to do is prove it.

Please provide four (4) peer-reviewed journal articles showing that the SM predicted non-zero neutrino mass prior to the discovery of oscillating neutrinos. Please provide the volume number, issue number, page number, principal author and co-authors for each of the peer-reviewed HEP journal articles. Since I have cited 4 articles, you'll need to provide 16 counter citations.

I look forward to seeing them.

19. **Peter Woit**
February 11, 2007

mclaren,

While, before there was convincing evidence for neutrino masses, people often thought of the standard model as the model with zero neutrino masses, it was clear to everyone that adding in neutrino mass terms was a very simple extension of the model, one that experimental evidence might some day require. When this evidence appeared I don't remember anybody being especially surprised. It's kind of a game with language whether you want to emphasize the importance of neutrino masses (they show the standard model is wrong!), or denigrate their importance (they're a trivial extension of the standard model!).

20. **Tommaso Dorigo**
February 11, 2007

Ditto, better than I could have myself.

Cheers,
T.

21. **a**
February 11, 2007

I don't see how neutrino masses can be considered as part of the Standard Model, since we do not yet know what the theory of neutrino masses is.

More likely neutrino mass are of Majorana type, and give us a window at physics around 10^14 GeV. This physics might "just" be a few right-handed neutrinos: it would be less exciting than what nowadays theorists like to dream. But it is not a dream.

Otherwise, neutrino masses could be of Dirac type. While this possibility just needs a few more more Yukawa couplings, their extreme smallness and the lack of a Majorana masses makes this possibility the unlikely one: crazy new physics is needed to justify it.

22. **Aaron Bergman**
February 11, 2007

The standard model doesn't have any renormalizable operator that gives rise to a neutrino mass, but there's a dimension five (?) operator that does so. By the usual rules of effective field theory, this operator is suppressed by the mass-scale of the new physics whatever it may be.

23. **woit**
February 11, 2007

a,

Unless the LHC discovers something really new, I think the neutrino sector is by far the most promising place to look for new insights, but I think it confuses things to state this as "neutrino masses are not part of the standard model". These days the standard everday model of particle physics is the version with neutrino masses, and when people refer to the "Standard Model", that's often what they mean. I think a better way to describe the situation is to make the point that the neutrino sector is the part of the standard model that we understand least well. We don't know the mass matrix completely, there are lots of interesting questions about it, and answering these questions may give insights into degrees of freedom that really are completely separate from the standard model.

So, I think it's just a question of language, with my own preference being to think of neutrino masses as the most poorly understood aspect of the Standard Model. It's of course also accurate to insist on thinking of them as an extension of the Standard Model. Both choices of language refer to exactly the same physics and mathematics.

24. **a**
February 11, 2007

Peter and Aaron,

years ago QED was the theory, Fermi described radioactivity adding non-renormalizable operators suppressed by the weak scale, and this lead to the Standard Model. Today the Standard Model is the theory, we (likely) have to add non-renormalizable operators suppressed by a new scale, 10^14 GeV, and we don't know to which new "Standard Model" this could lead.

It is true that neutrino masses were theoretically expected, but it is also true that almost all theorists expected smaller neutrino masses (suppressed by the GUT scale) and with small mixing angles. Theorists suggested "build Kamiokande to discover proton decay", and Kamiokande discovered atmospheric neutrino oscillations with large mixing angle. This is the most recent triumph of theory.

25. **Tommaso Dorigo**
February 12, 2007

Dear a,

neutrino masses were not just theoretically expected. They were predicted from experimental measurements of neutrino capture rates that date far back in the past. Indeed, the first chlorine experiment results to claim a deficit in solar neutrinos was Homestake in 1968.

About the reason for building super-kamiokande: I would like to read it in SK's technical design report. I think that the discovery of neutrino oscillations was one strong motivation, no less than proton decay.

Cheers,
T.

26. **a**
February 12, 2007

dear Tommaso,

the name is enough: the "nde" in kamiokande stands for "nucleon decay experiment".

About the Chlorine anomaly: ironically it started to be accepted after that it was realized how small-mixing-angle oscillations could explained it; this explains the importance of 2002 experiments, that established that it was due to large-mixing-angle oscillations.

About the top: its non-discovery would have been very important, as it is one example of those crazy things incompatible with accepted theories (including string theory, as some people like to emphasize).

27. **Thomas Larsson**
February 13, 2007

Neutrino oscillations were certainly a mainstream idea many years before the discovery. At least it was so in the spring of 1981, when I gave an undergraduate seminar about the topic, in a course which nominally was about nuclear physics but was called CERN physics by the students (CERN-Physik statt Kernphysik).

28. **wolf**
February 13, 2007

Neutrino oscillations were first proposed by Bruno Pontecorvo in 1957, if I'm not wrong. They were not included in the SM at the time of its formulation simply because then there wasn't any experimental clue as to whether neutrinos really oscillate or not

29. **a**
February 13, 2007

Potentecorvo 1957 paper was wrong: nu nubar oscillations don't exist.

Anyhow there is a big difference between doing things and talking about doing things. Today we talk about supersymmetry, extra dimensions, etc:, but what would be really interesting is discovering them.

30. **wolf**
February 14, 2007

Well, Yang and Mill's 1954 paper was also wrong since isotopic gauge invariance does not exist. Isospin is just a global broken symmetry.

Wrong ideas often turn out to be the best ones. That's why string theory might well have a bright future.

31. **Eugene Stefanovich**
February 14, 2007

wolf:

Wrong ideas often turn out to be the best ones. That's why string theory might well have a bright future.

That's the best endorsement of string theory I've seen so far.

32. **mclaren**
February 14, 2007

Tommaso;
Thanks for pointing out that nuetrino masses were theoretically predicted by at least some members of the HEP community long before neutrino oscillation was discovered. I didn't know that. It's always great to learn something new.

You remarked: "It's kind of a game with language whether you want to emphasize the importance of neutrino masses (they show the standard model is wrong!), or denigrate their importance (they're a trivial extension of the standard model!)."

I have to disagree with that claim because a number of cosmologists have suggested neutrinos with non-zero mass as the mechanism behind dark matter. While I don't know the details on the theoretical status of this idea, and there has been a good deal of debate about the exact mechanism behind dark matter, it's safe to say that anything that might make up 80-plus percent of the mass of the universe is not just "a game with language." Let's put it another way: when the SM assumed zero neutrino mass, oscillating neutrinos were out as an explanation for dark matter. When the SM was extended to include neutrino mass, that became a possible source of the missing mass. That seems like a significant difference to me. (OF course the missing mass could still be superpartners or who-knows-what-all, but the big difference there is that we have hard experimental evidence for neutrinos with mass, while to date we have no hard experimental evidence at all for superpartners.)

BTW, it's also fair to say that whatever extensions are required to the SM to accomodate neutrino mass do seem to be minor. My point wasn't that the SM is significantly "broken" in any way by the discovery of neutrino mass. Rather, I was trying to point out that there are areas in which the SM can be tested and in which it has to be modified, leading to potentially fruitful new physics.

Common sense would suggest that spinning out mathematical computer models of imaginary multiverses which can never be experimentally tested fails to qualify as fruitful physics.

33. **AGeek**
February 21, 2007

Lessons learned?

http://www.npl.washington.edu/av/altvw84.html

Just something which I happened to stumble upon tonight.

The Newtonian Legacy

February 10, 2007
Categories: Uncategorized

Instead of doing the work I had planned, I spent much of today having a very enjoyable time reading the mystery novel *The Newtonian Legacy* by particle theorist Nick Evans. A copy is available at his web-site here, and there's a FAQ about the book here (I pretty much agree with his LHC predictions).

The book is well-done, very entertaining, and a good read that keeps you wanting to know what will happen next. It includes lots of popular-level explanations about particle physics and the ideas particle theorists are studying these days, so it might be an excellent way to introduce someone to these ideas. It is set at a fictional theoretical physics research institute in England, and many of the characters are particle theorists of one stripe or another (string theorists, phenomenologists, lattice gauge theorists).

The novel includes quite a few amusing portrayals of characters embodying the current sociology of particle theory: a postdoc trying to decide whether to write into the Rumor Mill to tell them he is on a short list at a place he'd rather not go to in hopes of getting other places to offer him a job, a lattice gauge theorist who stalks out of a string theorist's talk in disgust, postdocs comparing the string theory landscape to religion, a self-satisfied American physicist from the West Coast convinced that string theory has the answers to the ultimate questions of science, and quite a few others.

The main character, Carl Vespers, is a particle theorist who, besides getting involved in the investigation of a mysterious death and having people trying to kill him, has to contend with more than one attractive woman throwing themselves at him, tempting him away from his long-distance girlfriend. All in all, a highly accurate portrayal of the life of a typical particle theorist. Highly recommended.

Comments

1. **Eric Baum**
   February 11, 2007

   Hi Peter,
   Rudy Rucker's Mathematicians in Love is an amusing sci-fi set around the academic world.

2. **s k san**
   February 11, 2007

   "has to contend with more than one attractive woman throwing themselves at him, tempting him away from his long-distance girlfriend. All in all, a highly accurate portrayal of the life of a typical particle theorist."

   I just love your sarcastic sense of humour

3. **Jonathan Vos Post**
   February 12, 2007

   I agree with the Rudy Rucker novel recommendation. His PhD, by the way, was in Mathematical Logic.

   Going through old emails, I found that I'd made a wry commentary on TV popularization of String Theory, and the human side of the academic world, as follows:

   BLACK HOLE BIRTHDAY
   ——————-
   by Jonathan Vos Post
   copyright (c) 2003 by Emerald City Publishing
   ——————-

   At midnight, you turned 50,
   alone, in your underwear,
   eating a bowl of Cheerios
   and wondering — where did all your friends go?

   What happened to that commune
   with the famous scientists
   who liked to keep you around
   to remind themselves how smart they all were?

   What if you had taken her up,
   that reactor engineer
   from the 747
   who propositioned you, mid-funeral?

   The years blurred past you, passing
   like the billboards high above
   Koreatown — ideograms
   you can't understand, don't remember.

   Palm trees blaze, torches roaring,
   dropping Kentucky Fried rats
   into the scummy hot tub,
   where you once played underwater Scrabble.

   All your extra dimensions
   are compactified, rolled up
   too tight to be detected.
   In String Theory, your strings are out of tune.

They all ran away from you,
galaxies of hangers-on,
accretion disks of neighbors.
What remained imploded to a black hole.

The light of your former world
drools down the gravity well
and, even if you got it,
you can’t escape the event horizon.

— — — — — — —
1200-1230
7 Nov 2003
— — — — — — —

That’s my complex emotional response
to the NOVA series on String Theory, combined with an interview of Art Garfunkel in Modern Maturity magazine. Poetry, and most art in general, attempts to reflect the complexity of the real world…

— Professor Jonathan Vos Post

4. **r hofmann**
February 12, 2007

Congratulations to Nick Evans. I had an enjoyable weekend reading his book. The book is manifestation of his talent to use very subtle terms and intelligently arranged combinations thereof to bring his convictions across. The entertainment frame is professional novel writing.

I like Dr Evan’s decision to make his brainchild freely available.

5. **Scott Aaronson**
February 13, 2007

Thanks, Peter — I read *Newtonian Legacy* over the weekend, and it was superb! But for the accurate discussions of electroweak symmetry breaking, one would think Evans turns out potboilers for a living. Unlike many academic novelists, he actually knows how to develop interesting characters and (especially) keep a plot moving. I hope the fact that the novel is free won’t deter people from reading it.

The similarities between the Phi and PI (Perimeter Institute) are a bit too striking to be dismissed as coincidental.

6. **Peter Woit**
February 13, 2007

Scott,

So the financial backer of PI and his wife are just like their analogs in the Evans book, and live next door to the Institute? Wow….

7. **anon.**
February 13, 2007

Peter is suddenly finding that vacation to Waterloo more appealing 😉

8. **Scott Aaronson**
February 14, 2007

So the financial backer of PI and his wife are just like their analogs in the Evans book, and live next door to the Institute? Wow….

Mike Lazaridis does live right here in Waterloo (his company shares a parking lot with the Institute for Quantum Computing), and comes to some of our events to give speeches about how amazing we are. Admittedly, I don’t think he’s involved with any “Historical Society,” and I’ve never met his wife.

9. **Cynthia**
February 14, 2007

Hi Peter!

Come to think of it, we, as avid consumers of pop-physics, oughta just throw out all those cheesy authors from the lesser sciences–including from the humanities, of course–that have this incredibly annoying tendency to write about physics with utter recklessness, not to mention with sheer listlessness.

After all, it’s becoming increasingly clear that physics is blessed with a whole host of brilliant writers which–believe it or not–just so happen to be active workers (including players, of course;)) in the field.

Needless to say, thanks for sharing this story written by a genuine fellow in the field. In fact, I’ve gotta confess, what Nick Evans has put together truly makes a most enjoyable read!

Thanks again!
Cynthia

Today’s Links

February 12, 2007
Categories: Uncategorized

The March issue of the Notices of the AMS is out. Excellent summaries of the mathematical work of the 2006 Fields medalists and interviews with three of them (everybody except Perelman). By the way, does anyone know if the 2006 or 2002 ICM proceedings are available on-line (the AMS is advertising the published books for the 2006 ICM, 3 volumes for $428)? There’s also an interesting article about Jim Simons and “Math for America”, the New York City based program designed to encourage people with a mathematics background to go into teaching. The program is partially funded by a yearly charity poker tournament attended by various people in the financial industry. I’d heard from one of them about this, but didn’t realize the scale on which they were operating. Last year’s tournament brought in $2 million.

This year’s Fields Medalist Terence Tao has an article submitted to the Bulletin of the AMS entitled What is Good Mathematics? See here for commentary from David Corfield.

Last month McGill University sponsored a large public symposium on the Anthropic principle that attracted overflow crowds, and featured Paul Davies, George Efstathiou, David Gross and Lenny Susskind. Gross and Susskind made more or less precisely the same points they have been making publicly about the string theory anthropic landscape for the last 4 years. You can watch the video for yourself here. Susskind seemed a bit less of an aggressive salesman of the anthropic point of view than in the past, acknowledging that the question of how you put a measure on the multiverse (this is needed if you want to make even statistical predictions) still has no solution. Gross made his usual points that accepting the landscape is premature, since we don’t know what string theory is, don’t understand the “emergent” notions of space and time it seems to lead to, and lack consistent time-dependent states describing something consistent with what we know about cosmology.

Besides the Becker-Becker-Schwarz and Dine fat textbooks on string theory that have just come out, another one is due out soon. It is by Elias Kiritsis and is called String Theory in a Nutshell (at nearly 600 pages, kind of a big nut). Princeton University Press is bringing it out in May. One of the leading physicists chosen to give a blurb is Harvard’s Lubos Motl, who also features on the Dine book. Evidently people who write string theory textbooks and their publishers feel his endorsement will do a lot to sell the books. Some of his recent postings refer to me as a “Communist” of a more “primitive and fanatical” sort than the ones he had to contend with during the Soviet era. I’d like to make clear that my political tendencies lean more toward some combination of anarcho-syndicalism and Clintonism than Soviet Communism. He also refers to the loyal readers of my blog as “human waste” (that’s you, folks…)

Comments

1. **John Stanton**
   February 12, 2007

   Dear Peter,

   keep your way!

   The agressions of a certain Harvard professor are typical of
   frustrated people that did not defend themselves in situations
   when it was necessary, and then, out of shame, pass
   on the received agression to others in situations where they
   are not appropriate.

   I guess it would do him a lot of good to have a wife and kids.

   Continue on your way!

   John

2. **Chris Oakley**
   February 12, 2007

   > He also refers to the loyal readers of my blog as “human waste” (that’s you, folks…)

   Funny that he should say, specifically, *human* waste.

   It does rather support the notion that he might be an extra-terrestrial.

3. **Peter Woit**
   February 12, 2007

   I forgot to add that, besides Princeton and Cambridge University Presses, the other media organization getting its material from Lubos is the Drudge Report. See http://www.drudgereport.com/flash.htm

4. **Clark Goble**
   February 12, 2007

   Thanks for the text book list. There are lots of people who were physics majors who’d love to be able to try and get up to speed on some of the latest in theoretical physics but are unsure how to proceed. I knew Weinberg had a series of QFT texts, the fourth of which dealt with string theory. However I’d gotten some negative comments on them.

   I wonder if there are any good ones on Loop Quantum Gravity?

5. **Oleg Tchernyshyov**
   February 12, 2007

   Greetings to all,

   I happen to be the one to whom Dr. Motl recommended this blog in those rather strong terms after I spent a week at his blog. The recommendation was apparently triggered by my comment in which I pointed out that calling your opponent a communist is unwise for a couple of reasons (e.g. communists can be good scientists, too, as exemplified by Lev Landau, who was a communist at heart). That comment was promptly made to disappear by the freedom-loving host.

Anyway, here I am looking forward to learning some things from your blog.

Oleg Tchernyshyov

6. **c.w.**
February 12, 2007

on the subject of jim simons and math for america, see also the item on CBS evening news and a video interview:

http://tinyurl.com/23mxzr

7. **wolf**
February 12, 2007

Clark, Weinberg's book consists of three volumes with the third discussing supersymmetric field theory. I haven't read the third volume in any detail. The first two, however, are the best QFT texts I know of in terms of technical accuracy, depth, and conceptual insight. Personally, I don't like his choice of notation and conventions, but that's a minor issue to me.

8. **Clark**
February 12, 2007

I have his first volume although I confess I wasn't a huge fan of it. I think it's one of those texts that might have been better for folks already familiar with the topic rather than learning from it. (Just IMO – others disagree but I know a few people I've talked to came away with the same opinion) Sorry about getting that volume number wrong. I could have sworn there were four, not three.

Anyway, I just figured I'd drop a note of appreciation from the masses no longer in schooling but who maintain their physics interest. Textbook recommendations are always welcome.

9. **Lee Smolin**
February 12, 2007

The now standard textbook on loop quantum gravity is "Quantum Gravity" by Carlo Rovelli, Cambridge University Press. There are older books by Ashtekar and Gambini and Pullin and a book in press with a rigorous approach to LQG by Thomas Thiemann, also CUP.

10. **gunpowder&noodles**
February 12, 2007

I don't agree that the Susskind et al symposium was the same old same old. In particular, Susskind was *very* much more tentative, with no declarations that his stuff is just straightforward, *standard* QFT and cosmology, and no claims that there is absolutely no alternative. For him, this was a very subdued performance. And on the other hand, Gross was much more specific than usual, pointing out for example that LS's proposal for creating new universes probably doesn't work. I wouldn't be surprised to see LS start putting out papers to the effect that the landscape doesn't work and that this is the greatest crisis in history.

11. **Amos Dettonville**
February 13, 2007

In the McGill discussion, I found it interesting that Davies and Efstathiou both posed essentially the same question to their fellow panelists, and yet the answers they elicited were quite different. Prefacing his question, Davies pointed out that if we imagine the "pre-existence" (in some Platonic sense) of a large set of physical laws, one of which is operative in our particular universe, then this set of laws is being imposed by some system of meta-laws, by a process that is, by definition, outside of the physical laws of our particular universe. His alternative is some kind of co-emergence of the universe with its laws, rather than imagining them to be imposed from some unknown (and presumably unknowable) "outside". He then asked the other panelists to comment on this. Susskind at first demurred, and Gross basically just said the question was not (presently) within the realm of science. He likened it to the philosophical question "Why is there something rather than nothing?" Susskind concurred, saying that Davies's question was profound, but couldn't really be addressed scientifically…

But here's what I found interesting: The next question was from Efstathiou, who asked the other panelists what they thought the prospects were for being able to compute the prior probability of various sets of physical laws within the landscape. Now this was a question that Susskind grasped immediately, noting that he himself had included this very question on his list of important areas of study during his earlier remarks. He didn't seem overly confident of being able to solve it any time soon, but he definitely recognized it as a legitimate and important scientific question. But…

Am I wrong, or was Efstathiou's question really just a re-phrased version of Davies' question – the question that had just been declared unscientific? Surely to determine prior probabilities for all the alternative universes, what's needed is an understanding of the very meta-laws and outer machinery that Davies had asked about. The difference between the two questions was just that Davies expressed the idea verbally in terms of laws being imposed from outside, whereas Efstathiou expressed the idea in the form of an equation involving what he called the prior probabilities (corresponding to Davies' imposition of laws from the outside). I guess Susskind was just more used to thinking about it in the terms that Efstathiou used, and didn't recognize it in the terms that Davies used.

Gross gave a fairly consistent answer to both questions, i.e., he responded to both by saying they were not scientific – at least not at the present time. He didn't fall for the "delta function" trap, i.e., he wasn't phased when Efstathiou suggested that if we discount the possibility of a (non-trivial) prior *distribution* of possible laws, then we are basically arguing for a delta-function distribution, aligned perfectly with the needs of a universe that has the seemingly special properties of our universe. Gross seemed to recognize this as essentially a repeat of Davies' question, so his answer (again) was that we don't know enough about the whole realm of possible modes of existence (why something rather than nothing?) to even think about this in a scientific way.

I didn't sit through the all audience questions, but I was bugged by the first guy who attributed the "why something rather than nothing" question to Leibniz. It's been a long time, but I distinctly recall this question being famously bandied about by the Scholastics in the Middle Ages ("Why be there soe much something and not much more nothing?"), and it was an ancient question even then.

12. **human waste**
February 13, 2007

About Lubos, his behavior (blogging instead of working on strings) suggests that he agrees with Peter, but cannot accept that a macho like him is scientifically impotent. Maybe he could find the solution to his problem in some spam e-mail.

More seriously, my opinion about Weinberg books:

Volume 1 (QFT introduced in a non standard way) looks worse than the standard way.
Volume 2 of Weinberg (advanced QFT) is very good.

Volume 3 (supersymmetry): people who worked on supersymmetry wrote better books.

13. **island**
February 13, 2007

> I wouldn't be surprised to see LS start putting out papers to the effect that the landscape doesn't work and that this is the greatest crisis in history.

This being the case, and if he was true to his word, then he'd be obligated to join the Davies/Wheeler side of the "debate"… or maybe he'll take-up preaching… 😉

As things stand now, we will be hard pressed to answer the IDists if the landscape fails.
-Lenny

Surely it would make more sense for him to ask Lee Smolin what we have in common with black holes.

14. **stinking human waste**
February 13, 2007

As another sample of stinkin' human waste, of the worst kind – I have been a communist in my early days, although I have successfully recycled myself into a social-democrat- I should say that we all need to be wary of the anti-communism of such patriots as Lubos Motl, who comes -let's not forget it- from a ex-communist country, and was certainly the subject of brainwashing. Beware!

Cheers,
QDS

15. **Greg Biffle**
February 13, 2007

Hello Peter,

I was wondering if you and Smolin were planning on appearing on a panel with Gross, Davies, et al.

I think that it's time that the physics establishment starts inviting you guys to String Theory conferences.

Smolin has written several papers on string theory, and a bestselling book.

Do you have any reflections on how long it will take before you are invited to participate in conferences, and add you valuable insigts to the debate?

Indeed, I imagine that this blog is read far more often than the books of Davies, Gross, et al combined.

Either your book or Smolin's pointed out that the average age of the tenured professors is approaching sixty.

As creativity is the domain of the young, and arrogance the domain of the old who have failed to create when they were young, should not Lenny Sussking ste down?

16. **Peter Woit**
February 13, 2007

Greg,

I think it would be a good idea for talks at string theory conferences to include a more self-critical take on the subject and more serious discussion of the difficulties it faces, but I think this should be done by string theorists themselves, not by me. Lee Smolin often has pointed out that string theory conferences rarely include discussion of alternative approaches to quantum gravity, and that it would be a good thing for them to do so. I think he's right about that, and that this is starting to change.

As for things like the Toronto symposium, which feature more philosophical talks by elder statesmen of the field like Gross and Susskind, obviously I'm not remotely in their class. They have had long careers of tremendous accomplishments, and have earned the right to have a prominent role in such things and have people pay attention to what they have to say (and possibly disagree with it…).

Some people have invited me to speak in various contexts. I've already done some of this and there are several events planned for the future that I'll be involved in. I think I'm personally getting more than enough attention, and I'd prefer to see more public debate within the particle theory community about the problems of string theory, with other people doing this, not me.

I also don't think the problems with string theory are generational or have anything to do with age. The anthropic landscape is not something being promoted just by an older group of physicists; many if not most of its proponents are younger. If anything, the problem of string fanaticism is most pronounced in the generations of physicists who have gotten their Ph.Ds. since 1984 and sometimes don't know much about the parts of particle theory that have nothing to do with string theory. Older physicists often have seen ideas come and go, and have a more skeptical attitude about the claims made for string theory than do less-experienced youngsters.

17. **The Anti-Lubos**
February 13, 2007

This may not be appropriate here, and I hope that Peter will delete this comment if he feels that way, but I would like to advertise for a new blog that I've just created, Deleted by Lubos. I invite anyone who has posted a comment to Lubos's blog only to have him subsequently delete it to come to this new blog and post what they had to say.

18. **Chris Oakley**
February 13, 2007

> As creativity is the domain of the young, and arrogance the domain of the old who have failed to create when they were young, should not Lenny Susskind step down?

Whether Susskind is uncreative, superannuated and arrogant or not, I think that I speak for the majority of the human waste who post here in saying that if the Anthropic Landscape is the best you can come up with, then you should at least have the decency not to brag about it.

19. **Eugene Stefanovich**
February 13, 2007

human waste:

More seriously, my opinion about Weinberg books:

Volume 1 (QFT introduced in a non standard way) looks worse than the standard way.

I would like to defend Weinberg's Volume 1.

First, I should note that QFT is a tough subject. Much more difficult than QM. You can easily teach yourself to write Lagrangians, calculate Feynman diagrams, renormalize things, and still don't understand exactly **what** you are doing. I tried to learn QFT by myself. After 10 years, half-a-dozen textbooks, and countless journal articles I still couldn't grasp the big picture. This all changed after I read Weinberg's Volume 1: "the lightbulb" turned on in my head. The difference was made exactly by Weinberg's non-standard approach. He places particles first and fields second. He doesn't worship the gauge invariance principle, but uses it as a pragmatic tool. This makes everything logical and crystal clear.

Some Weinberg's papers written in the 1960's are very good as well, e.g.,

S. Weinberg, "The quantum theory of massless particles", in Lectures on particles and field theory, vol. 2 (Brandeis Summer Institute in Theoretical Physics, 1964).

Though, I would agree that Weinberg's Volume 1 should not be your first QFT textbooks. Sometimes, especially in the 2nd part of the book his writing becomes too abbreviated for a beginner to follow. For starters, I would recommend earlier books by Schweber and Bjorken & Drell.

20. **Greg Biffle**
February 13, 2007

Hello Peter,

I understand taht you have to be nice on your blog, but what have Gross and Susskind really achieved?

What is the Gross eqution and Sussking equation?

And too, even if they did achieve great things (in higher dimensions, as we can't see them here), does not their arrogance trump their acheivement?

For as Einstein and Socrates reminded us, are not a man's greater achievements contained in the way they humble themselves before the unknown?

A certain arrogance has reigned the culture, and they're all in it togther. Kaku praises Greene who praises Gross who praises Susskind, but eventually a Lubos shows up, and the bluff gets called.

You would think that Susskind would have the courage to denounce Lubos, but he doesn't–now whay is that?

21. **defense**
February 13, 2007

Greg: What on earth are you talking about? Gross has a nobel prize and I don't think anyone of sound mind could question that both Gross and Susskind have made major contributions. I have met both and find them each to be quite modest and friendly, not at all arrogant as you claim. Also, I can assure you that at the McGill symposium Gross most certainly did not excessively praise Susskind.

About Lubos: did it ever occur to you that maybe Gross just doesn't care about Lubos? Maybe he just prefers to spend his time doing something other than badmouthing his collegues.

22. **Peter Woit**
February 13, 2007

Greg,

Well, Gross has a well-deserved Nobel prize for co-discovering asymptotic freedom and thus QCD, for a start. His achievements are undeniable.

Susskind has nothing on that level, but still quite a lot to his credit. One example would be Kogut-Susskind fermions.

I don't think Susskind does not denounce Lubos out of a lack of courage. Is he in any position to denounce someone for being arrogant, not very concerned about the usual standards of what is science and what isn't, fanatically devoted to string theory, and prone to over-the-top statements in public? This is kind of many string theorist's problem with Lubos: as far as physics goes, he just takes many of their attitudes and beliefs to an extreme.

23. **Peter Woit**
February 13, 2007

defense,

I agree with you that it's likely that Gross's attitude towards Lubos is just that it's best to try and ignore him. Do you have any insight though into why at least some string theorists think so highly of Lubos that they put endorsements from him on the cover of their books?

24. **Peter Shor**
February 13, 2007

Do string theorists actually get to decide whose endorsements get put on the cover of their books? Or is this the publisher's prerogative?

25. **Peter Woit**
February 13, 2007

Peter Shor,

In my limited experience, publishers do allow authors to vet jacket copy, meaning that they send it to you and ask you if there is anything you strongly object to. In this case, there are two different publishers involved. I find it very difficult to believe that both publishers, besides having editors so clueless as to think that a Motl endorsement would be a good selling point, also did not let the authors know what endorsements were going on their books.

26. **anon.**

February 13, 2007

Blogdom has reached a new low when an an uninformed commenter can call Gross and Susskind arrogant and claim they haven't accomplished anything. Greg Biffle, you might want to reconsider your own arrogance: you clearly know nothing about the last four decades of particle physics and have no right to judge those who do. You might want to read up on the development of the Standard Model in the 70s and reconsider whether to take Gross and Susskind seriously.

27. **defense**
February 13, 2007

Peter,

I really can't speculate as to why some string theorists might want Lubos' endosement on their books. It may be, as you would argue, that they endorse some toned-down version of his position. It may also be that they simply don't follow his blog and aren't familiar with the kind of stuff he says there. Or maybe they respect him as a theorist and are simply willing to disentagle his published work from his blogging activities.

28. **LDM**
February 13, 2007

Evidently people who write string theory textbooks and their publishers feel his endorsement will do a lot to sell the books.

Well perhaps, but personally, I tend to ignore *anything* written by Lubos. Any endorsement by him I could never trust to be well reasoned or balanced, so I simply ignore it. I would also question the business acumen of any publisher that used him as an endorser, given the negative comments on his blog and elsewhere.

29. **woit**
February 13, 2007

One of the wonderful aspects of running a blog like this is that every so often someone decides to engage in a campaign of harassment by repeatedly posting racist or otherwise obnoxious comments. By using different names and anonymized internet addresses, they can keep this up for quite a while.

Someone from the University of Toronto at Scarborough has been doing this quite a bit today, and you may see their comments here every so often when I'm not logged in and able to delete them. The computer people at the University of Toronto tell me that they think they have identified the person doing this, so perhaps some action can be taken that will convince them to stop this harassment, if necessary through lodging a complaint with the police.

Ah, the joys of the internet….

30. **Chris W.**
February 13, 2007

Amos,

What you pointed out—the differing responses by Susskind, but not Gross, to Davies' and Efstathiou's distinct formulations of essentially the same question—is revealing of what seems to be a widespread syndrome. It can be described as a propensity to mask philosophical confusion behind what appears at first to be a well posed physical or mathematical question.

Consider the phrase "prospects .. for being able to compute the prior probability of various sets of physical laws within the landscape". The obvious question to ask here is, *compute with **what**, if not a prior set of physical laws (or, if you like, meta-laws)?*. If one admits this, then one would have to explain what point there is in talking about "sets of physical laws" within in the landscape, instead of talking about a single physical law (or single set of laws?) that gives rise to the landscape, with its implications—or lack thereof—for the observable universe.

This episode strikes me as indicative of the sheer dismissiveness and indifference that Susskind and many of colleagues seem to show towards any kind of serious critical philosophical reflection on what they're doing. This seems pretty darn close to what Einstein had in mind when he referred to a certain physicist as someone who could calculate, but couldn't think.

31. **Jeff Moreland**
February 14, 2007

Maybe the publishers of these stringy textbooks feel that Lubos is well known for controversy, and that his name will help sell the books, regardless of what people think of him.

32. **mclaren**
February 14, 2007

Doesn't anyone find it disturbing that idle speculations about the landscape can be described as a "theory"? Maybe there is a multiverse, maybe there isn't. Maybe we're only one of many universes floating in higher-dimension space, maybe we aren't. And maybe there are pink unicorns on some planet circling a star in the Andromeda galaxy and maybe there aren't. None of that baseless daydreaming qualifies as a "theory."

Show us how oi disconfirm the hypthesis. Then you've got a theory. If there's no way to experimentally disconfirm it, it's not a "theory," it's idle speculation, smoke and mirrors, all hot air with no substance.

The other thing I find objectionable is calling all those empty slots for physical constants "parameters." If we called 'em what they really are, "fudge factors," I bet people's opinions would change in a hurry.

VERSION 1: "By proper selection of parameters, one of the 10^500 possible universes can be made to match experimental observations."

VERSION 2: "By proper selection of fudge factos, one of the 10^500 fudge factor outputs can be made to match experiment observations."

Version 2 doesn't sound so good, does it?

33. **Pindare**
February 14, 2007

Most of the Proceedings of the 2002 ICM have been posted to the arxiv. Some for the 2006 one too, try
http://front.math.ucdavis.edu/search?a=&t=&q=icm&c=&n=40&s=Listings

Also note that Tao's "What is Good Mathematics?" will appear soon on the arxiv with feedback incorporated (at least he says so on his website, the version you're pointing to being dubbed 'old version').

34. **anon on the hudson**
February 14, 2007

Clintonism!!! God help us. Did you enjoy Clinton's bombing of the Serbian civilian population(and the chinese embassy which could have trigered WW3)

Back to Math/Physics. Okounkov claims he never participated in the high powered russian math olympiads I thought this was an interesting comment. I wonder what percentage of current elite mathematicians and physcisists participated in Math olympiads. Did Thurtston and Witten participate in math olympiads. If so how well did the do.

Supposedly, Stallings entered Princeton graduate school with an English degree. I was told he majored in English at arkansas. Maabe I got this story wrong. It t's been a while since I was told this by one of his Princeton classmates(a classmate who has since passed on)

35. **jeff**
February 14, 2007

In defense of lubos motl, his criticisms of lqg etc are interesting because they're always rather specific about the physics.

36. **poochie**
February 14, 2007

You realize, folks, that Lubos-related discussion accounts for at least a third of the traffic on this site. Peter, if you banned Lubos related discussions, your hit counts would probably drop like a rock.

Peter-vs-Lubos is the "Itchy & Scratchy Show" of the Quantum Gravity world.

37. **M**
February 14, 2007

jeff, not always. For example the answers by Smolin in the (undeleted!) comments to

http://motls.blogspot.com/2006/02/doubly-special-relativity-is-just.html

show that he missed the point. And maybe lqg experts stopped answering to Lubos when he later started putting too many insults.

38. **woit**
February 14, 2007

Jeff,

M is right, I think if you try and seriously read what Lubos has to say about LQG (as about most topics), it's a mishmash of some valid comments and some complete misunderstandings (with a heavy helping of insults thrown in). He does do a good job of acting like he knows what he's talking about, even when he doesn't, and has impressed many people this way.

poochie is right that the Lubos discussion is pretty absurd. The guy is incredibly entertaining though, and the phenomenon of string theorists putting endorsements from him on their books still strikes me as weird beyond belief.

39. **Greg Biffle**
February 14, 2007

Hello All,

I understand that the seventies were a great time for physics.

But it saddens me that that gives Gross & Susskind license to monoplize the future of physics too, when the truth is, they have stood idly by while the vast string theory juggernaut rolled itself up into a ball that has so much gravitation mass that it sucks the lion's share of NSF funding.

Do we really need more string theory books with blurbs from lubos?

Why is it that theoretical physicists–the supreme gate keepers of the Laws of Nature and Mind of God–have not the power to remove Lubos's comments?

Lubos is far more famous for his politcial, and sometimes racist, rantings, than he is for physics. He is far more famous for leaving Harvard than perfoming any physics while there.

And yet, in Lenny & David's ST world, his endorsement is worth being printed on the back of a book. . .

Jut what is going on here?

40. **Chris Oakley**
February 14, 2007

> He is far more famous for leaving Harvard than perfoming any physics while there.

I'd hate it if Harvard were to kick him out. It would make the job of anti-stringers *so* much harder.

41. **CapitalistImperialistPig**
February 14, 2007

Peter,

I bought Michael Dine's book, and, so far at least, like it quite a bit.

I have a theory as to why Lubos's blurbs wind up on such books. Suppose we stipulate that it's impossible to have a rational discussion with Lubos (except maybe for the elect) and that his views on a whole range of issues are bizaare, fanatical, wrong, and expressed in terms that would lead to a short life in less civilized neighborhoods. The fact remains that he is a very bright guy who is willing to read books very carefully and give the authors detailed and helpful advice.

If he likes your book, you get a nice blurb. His blurb on Dine's book told me exactly what I wanted to know about it. Sieberg and Arkani-Hamed wrote nice blurbs too, but their info was far less relevant to my decision to buy it.

42. **Q**
February 15, 2007

Lubos does have a sense of humor. He has the funny Antiworld physics song playing in the background on his blog:

He was sitting there,
floating in the air….
With a smile on his face …
we jumped into hyperspace. …
He was an anti-man.
He lived in an anti-world.
He had an anti-dog.

Full lyrics and download: see http://musiclub.web.cern.ch/musiclub/bands/cernettes/songs/antiworld.html

43. **Steven H. Cullinane**
February 15, 2007

Corfield, in the remarks Peter cites, says, "I've been pushing for philosophers of mathematics to address the problems of values other than truth." Such values lead naturally to… Hollywood!

44. **woman**
February 15, 2007

"but cannot accept that a macho like him is scientifically impotent."

scientifically? Why does he hates women?

45. **human waste**
February 15, 2007

"scientifically impotent": I mean that he likes hard-core science (where everything is either right or wrong), string theory started as a hard-core science, but it now looks not-even-wrong. Since getting hard-core results seems now unlikely, barking against the landscape and biting Woit and Smolin is a good alternative.

46. **Greg Biffle**
February 15, 2007

Questions to ponder are:

Would the advancement physics be helped or hurt if:

1) logic and reason were held superior to hype and speculation
2) lubos motl had to back up his words with facts
3) kaku/davies/susskind, now in their sixties, stepped down with their religious prophecies to make way for simple logic and reason
4) NSF funded proposals based on logic, reason, and elegance, instead of how many time the words "m-theory" or "String theory" appeared
5) Ed Witten/Kaku/Susskind/Gross/Davies/et al. took some responsibility for the crisis and provided some moral leadership, humbling themselves before physics and reality, and admitting, as Smolin and Woit point out, ST abject and complete failure to advance physics so far
6) Prestigious university presses stopped using Lubos Motl as a referee and blurb-generator until he published a single legible paper in physics, or even a blog post that advances physics
7) Physicists such as Woit and Smolin were given more weight in the realm of physics than journalists such as KC Cole et al, who freely attack the truth of their books, and still find employment at prestigious institutions of learning and publications.
8) Polanski, Greene, Witten, Kaku, Davies, Susskind, or any one of thousandn of string theorists or NSF agents who read this blog stepped forth to stand up for science, reason, and truth, and pass judgment on KC Cole, Lubos, and the decaying bureacracy that was just yesterday the tragic String Theory juggernaut.

We all know what will be best for science, and it brings to mind the words of Max Planck,

An important scientific innovation rarely makes its way by gradually
winning over and converting its opponents: What does happen is that the opponents gradually die out. -Max Planck

The true genius of the string theorists is that instead of making ST the domain of an indvidual, as the advancement of science has ever been, they wed it to groupthink and immortal government bureaucracies. Thus even when they are gone, thousands of snarky postcos, who don't even believe in ST, will yet defend it as a means to get a job.

The only prerequisite to become a string theorist is the iwllingness to sacrifice a life of truth and beauty and physics for power and prestige, and that is the trap lubos has fallen into.

For what happens when the false reality comes crumbling down?

They lash out at not those who are bringing it down, but those who are noticing that it is tumbling on down, such as Smolin and Woit.

There's a lot of nervousness out there, as King Sussking and Witten and Gross still wield so much bureaucratic power. Every young physicists is asking themselves,

"I know it's all BS, but if I want a job and a family–I've got to go with the flow, and echo the ST mantra–give us a year, or two, or thirty, or a thousand, and we will figure it out, because we are smarter than you."

Imagine the poor, tormented souls, but such is the price one pays for going into physics and then playing politics to get ahead. For "those who live by the sword shall die by the sword." Let's wish them all the best–let's hope that NSF stops breeding this malicious dependency on groupthink and snarky, mean-spirited mythology at conferences in Aspen and Prague.

And let's all look forward to tomorrow's renaissance in physics that shall go not to politicians, but to Smolin's "seers"–those who grappble with the big questions–those who walk right on by the meaningless, masturabatory math as Odysseus sailed on by the sirens, and start approaching physics as that which it is–physics–simple, elegant physical models underlying reality.

47. **Peter Woit**
February 15, 2007

Greg,

Please don't post rants like this here, it's really not helpful at all, since so much of what you write is highly exaggerated and caricatured. Besides getting some people's names wrong, you are completely misjudging many people's influence. For example, I don't think there are any physicists at all who care in the slightest what KC Cole thinks. This kind of thing just convinces people that critics of string theory are as far gone and fanatical as Lubos, just in the opposite direction, please don't do it here any more.

48. **Greg Biffle**
February 15, 2007

Thanks Peter.

Thanks for running this blog and your kind, even-handed, level-headed spirit.

You're doing a lot more for the advancemnt of physics than perhaps you even know.

Your spirit brings to mind a poem by Rudyard Kipling–"If."

If
A poem by Rudyard Kipling

If you can keep your head when all about you
Are losing theirs and blaming it on you;
If you can trust yourself when all men doubt you,
But make allowance for their doubting too;
If you can wait and not be tired by waiting,
Or, being lied about, don't deal in lies,
Or, being hated, don't give way to hating,
And yet don't look too good, nor talk too wise;
If you can dream – and not make dreams your master;
If you can think – and not make thoughts your aim;
If you can meet with triumph and disaster
And treat those two imposters just the same;
If you can bear to hear the truth you've spoken
Twisted by knaves to make a trap for fools,
Or watch the things you gave your life to broken,
And stoop and build 'em up with wornout tools;

If you can make one heap of all your winnings
And risk it on one turn of pitch-and-toss,
And lose, and start again at your beginnings
And never breath a word about your loss;
If you can force your heart and nerve and sinew
To serve your turn long after they are gone,
And so hold on when there is nothing in you
Except the Will which says to them: "Hold on";

If you can talk with crowds and keep your virtue,
Or walk with kings – nor lose the common touch;
If neither foes nor loving friends can hurt you;
If all men count with you, but none too much;
If you can fill the unforgiving minute
With sixty seconds' worth of distance run –
Yours is the Earth and everything that's in it,
And – which is more – you'll be a Man my son!

Best,

Greg

49. **Ari Heikkinen**
February 16, 2007

"He also refers to the loyal readers of my blog as "human waste" (that's you, folks…)"

I don't think anyone should take his rants too seriously, as he seems to consider anyone who disagrees with his conclusions on any matter (and I don't even mean string theory here) a crackpot or an idiot.

It's freedom of speech folks, ofcourse, but you don't have to take everything you read seriously.

Updates

February 15, 2007
Categories: Uncategorized

Terry Tao's article What is Good Mathematics?, written for the Bulletin of the AMS, is now available at the arXiv.

There's a new article also on the arXiv by Zvi Bern et al. explicitly constructing the 3-loop 4-point amplitude of N=8 supergravity. They find various extra cancellations beyond those expected from supersymmetry, and argue that this and other calculations "strongly suggest that N=8 supergravity may be finite." The innumerable claims made over the years at the beginning of pretty much every string theory book or popular article that you definitely can't quantize gravity just using QFT are now no longer operational. There is quite a lot of interest in this topic, with an array of possible computations that need to be done in order to sort this out. Just at the Mathematics Institute at Oxford the past few weeks there have been talks related to this by David Dunbar, Kellogg Stelle, Bo Feng, and Michael Green. The Resonaances blog has a report about a talk by Lance Dixon, one of the co-authors of the new paper. Also a nice report on a talk about supersymmetry by David Kaplan, who gives an excellent definition of fine-tuning of parameters: "a model is fine-tuned if a plot of the allowed parameter space makes you wanna puke" (accompanied by an illustrative plot of the situation in mSUGRA).

I'm waiting for the above news about supergravity finiteness to hit the media, perhaps some of the people working on this need to get to work on their press releases. There's yet more press about the Distler et. al. "test of string theory." The Daily Texan has an article called Test May Prove Far-out Theory, where Distler explains how the LHC seeing effects consistent with unitarity, analyticity and Lorentz invariance will provide "more evidence" for string theory. The reporter who wrote this called me up, and includes a garbled version of what I had to say. I was trying to be polite and stick to just pointing out that the paper at issue was not a string theory calculation and its title had been changed to remove reference to string theory, something I suggested the reporter might want to ask the paper's authors about. He seems to have not taken up my suggestion. I probably should have just used more straight-forward language, something along the lines of "dishonest bulls**t".

The US Congress finally finished dealing with the FY2007 budget, sending a bill to the president which he'll sign. It restores money to the NSF and DOE, avoiding a freeze at FY2006 levels that looked possible for a while and would have forced shutdowns at Fermilab and RHIC.

Finally, Capitalist Imperialist Pig informs us that Princeton is taking the occasion of shutting down its parapsychology lab to also close down its string theory program and redirect the funds to research on global warming. Somehow I suspect this may be a bit of an exaggeration, but they do seem to be making moves away from string theory and towards phenomenology, sponsoring workshops next month on Physics at LHC: From Experiment to Theory, and Monte Carlo Tools for Beyond the Standard Model Physics.

Update: Here's a link to the Kaplan talks on SUSY.

Comments

1. **Greg Biffle**
   February 15, 2007

   Lance Dixon was a TA of mine when I was an undergrad.

   I think he was a postdoc at the time, but he was better than the professor, and we always went to his sessions.

   I look forward to reading his paper! Thanks!

2. **kuos**
   February 15, 2007

   Question for discussion: how many of these attributes identified by Tao apply to string theory?

   • A field which becomes increasingly ornate and baroque, in which individual results are generalised and refined for their own sake, but the subject as a whole drifts aimlessly without any definite direction or sense of progress;
   • A field which becomes filled with many astounding conjectures, but with no hope of rigorous progress on any of them;
   • A field which now consists primarily of using ad hoc methods to solve a collection of unrelated problems, which have no unifying theme, connections, or purpose;
   • A field which has become overly dry and theoretical, continually recasting and unifying previous results in increasingly technical formal frameworks, but not generating any exciting new breakthroughs as a consequence; or
   • A field which reveres classical results, and continually presents shorter, simpler, and more elegant proofs of these results, but which does not generate any truly original and new results beyond the classical literature.

3. **John A**
   February 15, 2007

   At last, Princeton has the guts to shut down PEAR aka "if you stare hard enough at random numbers you'll see a pattern"

4. **Q**
   February 15, 2007

   "… Princeton is taking the occasion of shutting down its parapsychology lab to also close down its string theory program …"

   These are unifiable, at least according to the paper of Nobel Laureate Professor Brian Josephson of Cambridge University, http://arxiv.org/abs/physics/0312012 :

   "String Theory, Universal Mind, and the Paranormal … A model consistent with string theory is proposed for so-called paranormal phenomena such as extra-sensory perception (ESP). Our mathematical skills are assumed to derive from a special 'mental vacuum state', whose origin is explained on the basis of anthropic and biological arguments, taking into account the need for the informational processes associated with such a state to be of a life-supporting character. ESP is then explained in terms of shared 'thought bubbles' generated by the participants out of the mental vacuum state. The paper concludes with a critique of arguments sometimes made claiming to 'rule out' the possible existence of paranormal phenomena."

   I suspect that those who don't like string theory have formed a conspiracy to suppress research into paranormal extra dimensions, or ESP mental vacuum states, in case paranormal research come up with some evidence to support M-theory. Hopefully string theorists like Lubos Motl (who is deeply concerned about global warming issues) will take great comfort from the fact that the money being spent by Princeton on string will in future go to global warming.

5. **JustAnotherInfidel**
   February 15, 2007

   From what I recall, people tried to do model building with N=8 SUSY a long time ago because it automatically contained a graviton, and so was automatically N=8 SUGRA. There are 28 spin = 1 bosons in the multiplet, which look suspiciously like the adjoing of SO(8). But SO(8) doesn't contain SU(3) x SU(2) x U(1), and I don't know any more than this. I guess one could study, say, N=8 SUSY SO(10) or something, and have SUSY break at the planck scale to N=1 SUSY.

   I dont know—does anyone know about any phenomenology which has been studied along these lines?

6. **kramnik**
   February 15, 2007

   The point with this is not that N=8 might be the TOE that we want. Rather, if it is finite it's probably due to some as yet unknown deep symmetry which we don't know about. Determining whether it is finite, and if so why, will be a major clue to uniting gravity with the rest of physics.

7. **andy**
   February 15, 2007

   At first I was sure that it was a joke, but the phrase "mental vacuum state" does in fact appear in the abstract referenced by Q.

8. **CapitalistImperialistPig**
   February 15, 2007

   Peter,

   My remark about Princeton's getting out of the string theory business was unmitigated bullshit. I thought I had implied that pretty clearly in the post, and apologize to anyone who was thereby misled.

   Mea culpa!

9. **Jean-Paul**
   February 15, 2007

   The only reason to be excited about finitness of N=8 sugra is that it's an existence proof of a finite QFT with spin 2. From the particle physics point of view this is as irrelevant as the vanishing of c.c. in N=1 case.

10. **Gordon**
    February 15, 2007

    CapitalImperialistPig:
    No need to apologise–we all know your comments are always
    unmitigated bullshit.

11. **Thomas Larsson**
    February 16, 2007

    I don't see why one should be excited if N=8 sugra were finite. That theory certainly has as least as much problems with experiments as string theory. Besides, QFT infinities are not just a nuisance, but something which explain some experiments via anomalies, e.g. pion decay. Could really that effect be reproduced if the SM were embedded in some finite field theory?

12. **A String Theorist**
    February 16, 2007

    Hi Peter,

    In the abstract, at least, they were very careful to quantify the word "finite" with by saying "perturbatively finite." I don't know anyone who even would speculate that N=8 might actually be finite also NON-perturbatively. Presumably the non-perturbative completion of N=8 supergravity would be some string theory.

    Take N=4 Yang-Mills theory as an example. It is perturbatively finite, and its non-perturbative completion is a string theory (namely, type IIB string theory on AdS5 x S^5).

13. **CapitalistImperialistPig**
    February 16, 2007

    Ah Godon, coming from you, that means so much!

14. **Tim**
    February 16, 2007

    Peter,

    you write "on the arXiv by Zvi Bern et. al. explicitly" but "et" means "and" in latin and is not an abriviation thus needs no dot.

    Tim et al.

15. **woit**
    February 16, 2007

    A String Theorist,

    I've heard many claims about string theory providing an "ultraviolet completion" of a QFT, but you are the first person I've heard claim that it provides a "non-perturbative completion” of QFT. Actually, I think you just got this completely backwards: every string theorist I’ve heard say anything like this has said it in the other direction. No one knows what non-perturbative string theory is, but AdS/CFT can be interpreted as saying that the N=4 SYM QFT (which is believed to make sense non-perturbatively) provides a “non-perturbative completion” of string theory, where we only have a perturbative definition.

Thomas,

I think kramnik has it right. The point is not that N=8 supergravity is a viable TOE, it is that this result says there is some structure in it we don’t understand that is causing unexpected cancellations improving its ultraviolet behavior. The conventional wisdom that QFT’s involving gravity inherently have bad behavior in the ultraviolet looks like it is just wrong. This may be quite analogous to what happened with asymptotic freedom: up until 1973 the conventional wisdom was that QFTs in general had bad ultraviolet behavior, behavior that furthermore was inconsistent with observed high-energy scaling. That change in conventional wisdom killed off string theory once, it could happen again…

Tim,

Thanks, I always forget that. Fixed.

16. **Robert Musil**
February 16, 2007

According to the NY Times, various “legitimate” scientists visited PEAR’s and even gave them money from time to time. There is a unconfirmed rumor that Ed Witten was hard at work trying to move thought bubbles with his mind when the Princeton Dean rudely turned off the PEAR’s lights, plunging the special ‘mental vacuum state’ into darkness and making Ed stub his toe on the way out.

17. **A String Theorist**
February 16, 2007

Hi Peter,

Yes I realized right after submitting the post that I was being imprecise.

The essential problem, as you point out, is that we don’t have a non-perturbative definition for “N=8 supergravity”. I would bet that if someone someday finds such a definition, it will end up being a string theory.

18. **Hmm**
February 16, 2007

It is a stunning indication of americas descent into mediocrity that you are now being contacted by journalists to give views on *scientific* rather than sociology-of-science matters. Wow. Its enough to have a blog; you don’t need to have a record of research accomplishment or to even understand the technical claims being made. God help us all!

19. **A String Theorist**
February 16, 2007

Oops, I hit submit before I was finished.

About N=4 Yang-Mills, I never said that string theory provides the non-perturbative definition of YM.

I said that the non-perturbative completion of the theory (which you may take to be a Feynman path integral, or lattice I suppose) is a string theory (i.e., in a certain regime of parameter space, it describes relativistic strings).

20. **Scott Aaronson**
February 16, 2007

*The reporter who wrote this called me up, and includes a garbled version of what I had to say … I probably should have just used more straight-forward language, something along the lines of “dishonest bulls**t”.*

Peter, I’m having the same problem these days talking to journalists who want to bullshit up their quantum computing stories (“the world’s first commercial quantum computer has been unveiled, able to solve NP-complete problems in a heartbeat…”). I patiently explain to them why the story doesn’t mean anything like what they think it does, but I get the sinking feeling, even as I’m talking, that the article will have zero mutual information with anything I say. If you ever figure out how to dissuade a journalist from writing the misleading piece he or she wants to write, please let me know.

21. **woit**
February 16, 2007

A String Theorist,

Thanks for the clarification. I still think it’s kind of misleading to talk about a “non-perturbative” completion of N=4 SYM. By itself N=4 SYM is inherently a non-perturbative theory (even if we don’t completely rigorously understand it). One doesn’t talk about the “non-perturbative completion” of QCD, QCD is just inherently a non-perturbative theory, and going to a supersymmetric version doesn’t change this.

I haven’t thought much about this, but it’s not clear to me that you can’t just define N=8 supergravity non-perturbatively as the path integral, and try and make sense of this. If this works, you would have a purely conventional QFT for quantum gravity. Like many QFTs it might have some regime where a string theory approximation was useful, but I don’t think that would justify calling it a string theory since it is fundamentally defined in terms of quantum fields, not strings.

Hmm,

My, some of you string theory partisans are charming, especially the ones too cowardly to put their names to their ad hominem attacks. I’ll leave it to informed people to decide who was giving this reporter honest, accurate information, me or Jacques Distler.

Scott,

Similarly, if you figure how how to stop journalists from writing the “quantum computers solve NP-complete problems in a heartbeat” story, let me know and maybe it will help me figure out how to dissuade them from the “string theorists figure out how to prove string theory” stories which they are being continually led to write.

22. **Thomas Larsson**
February 16, 2007

My concern about combining anomalies, which are experimentally confirmed consequences of QFT infinities, with a finite theory are not original. I essentially took the argument from Roman Jackiw, hep-th/9911071, which ends with

"one very important lesson we should take from quantum field theory is not to banish all its infinities. Apparently the mathematical language with which we are describing Nature cannot account for all natural phenomena in a clear fashion. Recourse must be made to contradictory formulations involving infinities, which nevertheless lead to accurate descriptions of experimental facts in finite terms. It will be most interesting to see how string theory and its evolutions, which purportedly are completely finite and consistent, will handle this issue, which has been successfully, if paradoxically, resolved in quantum field theory."

23. **anon**
February 16, 2007

"I'll leave it to informed people to decide who was giving this reporter honest, accurate information, me or Jacques Distler."

Since you complain that the reporter misquoted you, are we to assume, in contrast, that he quoted Distler accurately?

24. **Peter Woit**
February 16, 2007

anon,

I don't think people need to or should base their decisions about this on what the reporter has either of us saying, since he for good reason had trouble understanding what is going on. In my case, you can judge from what I've written here on this topic as to its scientific accuracy. In Distler's you can read the press release the university issued for him, or consider the many other press stories quoting him on this topic. My scientific claim is simple, and it seems that a PRL referee agreed: Distler's paper isn't about string theory so can't claim to have a test of it. Distler seems to be quite clear in his claims to the press that he has a test of string theory that can be performed at the LHC.

Up to you to consider which of those two claims is more scientifically accurate.

25. **Robert Musil**
February 16, 2007

Peter,

"The reporter who wrote this called me up, and includes a garbled version of what I had to say … I probably should have just used more straight-forward language, something along the lines of "dishonest bulls**t"."

In general, those who deal frequently with the media come to understand that when a reporter calls asking to speak one has two choices: Speak, and be misquoted – or refuse to speak, and be misinterpreted.

I pray you have the widom to choose correctly.

26. **Chris W.**
February 16, 2007

Scott and Peter,

I'm afraid that the people you need to dissuade are the editors that have final say on what gets published. (Maybe it goes higher than that.) I suspect that in most cases a reporter who is conscientious in this sense would have trouble continuing to report on science-related stories; he or she would be told that they can play ball, take other assignments, or get fired.

Perhaps there is an editor out there who would like to take issue with this. If so, please post a comment, preferably with some helpful explanation. Try not to blame it on the great unwashed public, ie, your alleged readers.

27. **Tim**
February 16, 2007

Peter,

You write "My, some of you string theory partisans are charming, especially the ones too cowardly to put their names to their ad hominem attacks." and this is not the first time you bring up such a remark.

Did I miss the sign "If you post here, you are obliged to give your real name!"?

You chose this type of forum yourself and you know very well that posts can remain anonymous, so why complain? If you chose a different forum (refereed journal, arxiv, conference, etc) for letting the world know what you think, all responses would bear the real name of the respondent. However it is by your own free choice that you use a blog which implies by the very nature of this forum that responses can and will remain anonymous.

Your complaining is analogous to a writer who refuses the advice of his publisher to order high quality paper for a new book but later on complains that the books are worn out too quickly.

Tim

28. **Ari Heikkinen**
February 16, 2007

"close down its string theory program and redirect the funds to research on global warming"

Heh, that's funny, as I thought the biggest web-advocate of string theory thinks that global warming is utter nonsense (sorry, couldn't resist:).

29. **island**
February 16, 2007

Tim, Peter often knows or can find out who the anonymous posters are, yet he generally keeps their secret from the public, (unless he is being severely harassed), while calling them out for being too chicken to claim responsibility for their unsubstantiated bullshit.

Shut up.

30. **Peter Woit**
February 16, 2007

Tim,

I don't encourage people to post anonymously here; I think it would be best if people always used their real names. Personally I don't post comments on other blogs without using my real name, although I can imagine some unusual situations in which I might choose to do so. But my policy certainly is to allow anonymous comments, and I realize there are many legitimate reasons for people to choose not to use their names. If someone wants to engage in anonymous attacks on me, I'm allowing that. I also have every right to refer to people who decide to do this as "cowardly".

31. **Peter Woit**
February 16, 2007

island,

Thanks for the supportive comment, but do try and keep the hostility level down…

It is also true that it's often not too hard for me to figure out who anonymous commenters likely are. For instance, in the case of "Hmm", his comment was posted from a somewhat unusual and precisely identifiable location, making it not hard to, with a small amount of research, make a pretty good guess as to who he is.

32. **Rugs**
February 16, 2007

I have a friend that is an editor for a newspaper (I will leave his name, my name and his paper anonymous so we can remain friends) and when we had a mutual friend die in an accident I got to watch how he dealt with reporters. He wrote out every statement and read the statements to reporter's one word at a time making sure they wrote each word as he had intended. He was very concerned about how the circumstances of our friends' death were reported. He not only understands how sloppy note taking can lead to bad reporting he also knows that a sensational story can help a reporters career better than a boring one. Though he doesn't believe it is done intentionally (I feel a little differently) he does feel human nature will push a simple story in the wrong direction. I don't know if knowing how a newspaper editor deals with reporters is any help but I sure found it enlightening.

33. **Peter Woit**
February 16, 2007

Chris W.,

I should say that my experience over the last year or so with quite a few reporters is that they try hard to get things right. They're not experts in the science involved, and, faced with people making contradictory or hard to evaluate claims, they do the best they can. With one exceptional case of behavior that was worse than I would have guessed goes on, I've been pleasantly surprised by the competence and degree of professionalism of almost everyone I've dealt with.

The problem with the "physicists finally discover how to test string theory" stories mostly doesn't come from the people reporting these stories, it more often comes from the scientists involved.

34. **Michael**
February 16, 2007

Peter,

suppose N=8 sugra is finite and exists at the quantum level. In order for your conclusion that it is a theory of quantum gravity besides string theory to hold, you'd still have to show *that*, namely that it cannot be realized as a string theory. Absent such a proof you could make that claim already about N=4 SYM, which is a sugra theory on an AdS space written in funny variables.

Until someone does this for you (clearly you are not capable of it yourself), it is much more plausible to say that N=8 sugra resisted embedding into string theory so far simply because it doesn't exist beyond perturbation theory.

So even if I swallow all the ifs buts and maybes you interjected, your line of reasoning has more holes than swiss cheese. Your thinking is really very flaky.

35. **Eric**
February 16, 2007

Michael, the gravitational degrees of freedom in a SUGRA theory are pretty straightforward to isolate (in contrast to the surprising case of AdS/CFT). If N=8 SUGRA were shown to be finite and a sensible quantum theory, no convoluted rewriting of the theory in a dual string picture, or showing that you can get N=8 SUGRA in particular backgrounds ,will change the fact that you have found a non-string theory of quantum gravity.

36. **Michael**
February 16, 2007

Dear Eric,
How can you say that isolating the degrees of freedom is "pretty straightforward" if you cannot even show that the theory exists? There might just be another "surprise" in stock for you.

The smart money remains on the reverse logic: N=8 sugra cannot be embedded into string theory, because it doesn't exist beyond PTB.

37. **Peter Woit**
February 16, 2007

Michael,

You anonymous string theorists are all such polite folks, always a pleasure to try and have a discussion about scientific issues with you.

"N=4 SYM, which is a sugra theory on an AdS space written in funny variables"

That's just not true. Even if you believe everything conjectured about AdS/CFT, N=4 SYM is supposed to be exactly dual to a full, non-perturbative string theory, not to supergravity on AdS.

It's kind of hilarious to see string theorists claiming that N=4 SYM should be understood as a string theory, when they don't know how to define non-perturbative string theory, except as N=4 SYM.

I don't think the argument "OK, QFT can also give you finite terms in perturbation theory, just like string theory, but it may have problems non-perturbatively, unlike string theory (wait a minute, we don't know what string theory is non-perturbatively, other than through the QFT…)" is going to fly.

38. **Michael**
February 16, 2007

"such polite folks"

Stop whining and talk about the issue!

"That's just not true."

For large radius, my statement is perfectly accurate; the generalization is obvious. My point was well taken.

You have failed to address the hole in your logic, which I pointed out. Thanks for nothing!

39. **Eric**
February 16, 2007

Michael,

I'm not even anti-string theory, but you don't make sense, and your point is definitely not well taken. Since one of the things you do in SUGRA is quantize the metric, you've got a perturbative spin-two field staring you in the face, giving you gravitational interactions, just like the relevant closed string mode in string theory: this is very easy to isolate.

For the large radius limit of an AdS space, yes it is supposed to be well-described by a SUGRA, but this is just an approximation, there's nothing really to "generalize."

40. **Peter Woit**
February 16, 2007

Michael,

Despite your rudeness I'm trying to have a serious discussion with you. I have addressed the issues here, you haven't. The discussion is not about a limiting case of large radius. All you seem to be able to do here is talk about "the smart money". If you know of an argument showing that N=8 supergravity can't be defined outside of perturbation theory, but string theory can, let's hear it.

41. **Michael**
February 16, 2007

Peter,
I may have an opinion, but I do not know if N=8 sugra exists. I simply pointed out that under the assumptions you made your logic still had a gaping hole, and it remains so.

You are trying to sidestep the issue and claim `string theory isn't better`. Perhaps. Bananas. Whatever. It does not reduce the number of holes in your argument.

42. **CapitalistImperialistPig**
February 17, 2007

Ari – Yes, that was whatever point there was of my little "joke." Lubos had just deleted a global warming comment of mine when I wrote the post.

43. **CapitalistImperialistPig**
February 17, 2007

Michael said: "suppose N=8 sugra is finite and exists at the quantum level. In order for your conclusion that it is a theory of quantum gravity besides string theory to hold, you'd still have to show *that*, namely that it cannot be realized as a string theory. "

If this is the "hole" you keep talking about, you are pegging your argument on a pretty flimsy support. Whether some QFT can be realized (I think you really mean represented) as a string theory is irrelevant to the question of whether there is a QFT of gravity. Perhaps the second law of thermodynamics can be realized as a game of parchesi – if so, it would certainly be amusing, but it would hardly show that parchesi is essential to the description of the world.

44. **gunpowder&noodles**
February 17, 2007

Michael's comments are so funny — especially "My point was well taken" [Haha, I thought *other* people are supposed to say that!] that I suspect he is pulling Peter's leg. Unlike, sadly, "Hmm", whose determination not to have a sense of humor is known far and wide.

45. **Steven H. Cullinane**
February 17, 2007

Q: Regarding thought bubbles– Thanks for sharing.

46. **ObsessiveMathsFreak**
February 17, 2007

And for examples of What is Bad Mathematics, see Jean-Pierre Serre's How to Write Mathematics Badly.

47. **mathjunkie**

February 17, 2007

What is the importance of that N=8 supergravity exists? Does it mean that it can replace String theory?

48. **Alex Nichols**
February 18, 2007

There are now two papers:
1) http://www.arxiv.org/pdf/hep-th/0702112
-based on point particles

2) http://arxiv.org/abs/hep-th/0610299
-based on strings

Both claim to be u-v finite.

Question: At what sort of energy levels would it be possible to exclude either 1 or 2?

49. **Arun**
February 18, 2007

If I understand correctly, crucial to the Bern et. al. argument is the use of the KLT (Kawai, Lewellen, Tye) tree-level relations, orginal expressing tree-level closed-string scattering amplitudes in terms of pairs of open string ones, which in the low energy limit of compactifaction to four dimensions relate N=8 supergravity and N=4 super YM tree amplitudes.

Unless this step could be discovered independently, it would appear that a digression into string theory is essential to the (possible) discovery of viable QFT gravity theories.

🙂

50. **Peter Woit**
February 18, 2007

Alex,

This isn't an experimental question, it's a purely theoretical one about whether these higher order terms are finite or not. In principle it's a straightforward calculation, and one can just calculate. The problem is that the calculation is so difficult that people have to use indirect arguments and make some assumptions in order to get an answer at high orders.

51. **onymous**
February 18, 2007

Alex, from your comment it seems you think that the papers you link to are about two different theories, one stringy and one involving point particles. This is not the case. Both are about N=8 supergravity in four dimensions, they are just approaching it from different directions: you can get to this theory by compactifying 11D maximal supergravity.

In general the question of whether this is a "field theory of gravity" or a "string theory of gravity" is not well-defined. As with any interesting field theory, there are nonperturbative effects. In gravitational theories these contribute exp(-1/g) terms, which are understood in string theory to be associated with branes. So solitonic objects are inescapable in this theory, even if it is perturbatively finite. (The solitons are associated with the divergence of the *sum* of the perturbative theory, which will diverge even if the individual terms are finite.) N=8 supergravity is part of the same family of theories as M-theory (the UV completion of 11D sugra), which includes both theories with light strings and those without.

In any case, it certainly isn't a typical local QFT of spin-2 objects, the usual difficulties with constructing local observables in quantum gravity apply to N=8 supergravity even if it is finite.

52. **JustAnotherInfidel**
February 18, 2007

"The conventional wisdom that QFT's involving gravity inherently have bad behavior in the ultraviolet looks like it is just wrong. "

I don't think this is the conclusion that the authors are claiming—in the last paragraph they seem to imply that stringy effects are still needed to get all of the cancellations needed:

"The result presented here, in conjunction with the allloop-order evidence from unitarity [13] and string theory hints of additional cancellations [7, 8, 9], strongly suggests that N = 8 supergravity may be ultraviolet finite."

53. **woit**
February 18, 2007

JustAnotherInfidel,

The reference is to independent duality arguments of Green et al. that do have to do with string theory, but have implications for pure supergravity. What is being discussed here is not string theory, it is a QFT, N=8 supergravity, and the question is whether the terms in this perturbation series are finite. String theory methods may be useful for showing this (although the Bern et al. paper under discussion does not use string theory), but the claims are being made about a theory that does not involve strings, and is a conventional QFT. It is precisely this conventional QFT that people have in the past believed had bad high energy behavior, but now it appears this may not be true.

54. **Gordon Chalmers**
February 20, 2007

I havent checked the blog for a while, but I would like to point out that it is straightforward to obtain a nonperturbative formulation of N=8. Take the nonperturbative formulation of IIB string theory compactified on a six torus and decouple the massive modes. I believe the exact S-matrix of IIB string theory is known, and for that matter, after decoupling the massive modes so is the exact matrix of N=8 sugra. Of course the exactitude of the S-matrix relies on the belief of S-duality
so consider it a hypothetical exactness until duality is shown.

By the way in case you are interested there do seem to be some early articles on obtaining the standard model with some technicolor type of arguments but that is all. The theory seems to be nonviable unless the standard model gauge group changes, which is in fact possible if you read some more about string bits.

Also, just as a plug, I am in fact the first person to argue for finiteness in a concrete manner, and to this day my arguments are
repeated almost by everyone except for Bern et al, who use the
no-triangle hypothesis in a convincing manner.

55. **Vogelsang**
February 20, 2007

> Also, just as a plug, I am in fact the first person to argue for finiteness in a concrete manner, and to this day my arguments are repeated almost by everyone

Could you please provide some references?

56. **gordon chalmers**
February 20, 2007

Would yould you like that with at symbols or what?
I could give you the references, which you could look up, or I could give you the 200 papers that I have catalogued which are unpublished.

Which is which is what I know of.

Gordon

57. **gordon**
February 22, 2007

There are further cancellations at 3 loops and beyond which are not accounted for by the no-triangle hypothesis. These cancellations occur in two and three loops. They are not accounted for by any symmetry and the integral functions are absent and they are many. So the theory is probably secretly hiding a symmetry which noone has encountered before. The theory is probably finite, and if you dont like my comment why dont you ask me or Zvi Bern for details: but they are unproven.

Richter, MiniBooNE and an Eternal Feast

February 18, 2007
Categories: Uncategorized

At the big AAAS conference held in San Francisco the past few days there was a session on "A New Frontier in Particle Physics", about the LHC and the promise of physics at the TeV scale. Burton Richter's talk there on Charting the Course for Elementary Particle Physics is now available at the arXiv.

Richter uses the "It was the best of times; it was the worst of times" line from Dickens to characterize the current state of particle physics. He expects the LHC to begin operations at full energy in summer 2008, with physics results beginning to appear in 2009. As for the ILC, at the earliest it would be completed in 2019, and he sees no chance of it being built if the LHC doesn't find something new by 2012. He also sees a luminosity upgrade of the LHC being considered next year, one which would take place after the machine had been running for 5 years or so.

He also discusses the MiniBooNE experiment at Fermilab, a neutrino experiment that is late in reporting results. They are doing a "blind analysis", not "opening the box" and looking at the final answer from their data until the last moment. Richter is dubious about this, saying: "I never did like blind analyses", claiming that they prevent experimenters from seeing problems in the data, and he worries that the MiniBooNE result (which is trying to check results from the LSND experiment that disagree with the standard picture of neutrino physics) will be inconclusive.

We should know soon, I see that on March 1 there is a colloquium scheduled at Princeton by one of the MiniBooNE experimenters on "New Neutrino Oscillation Results from the MiniBooNE Experiment". For a recent talk about MiniBooNE given at Columbia, see here. This talk did also definitely mention the possibility of an inconclusive result, requiring a more sensitive experiment that might take place at the SNS (Spallation Neutron Source) in Oak Ridge.

Also at the AAAS meeting was yet another session on the wonders of the multiverse called "Multiverses, Dark Energy and Physics as an Environmental Science," featuring the usual Stanford team of Linde and Susskind, with Lawrence Krauss brought in to provide a little bit of reality. Stanford has put out a press release promoting Andrei Linde's talk at the meeting. Linde goes on about what he calls 10^{1000} vacua, and how they are "an unexpected gift from string theory… an eternal feast where all possible dishes are served." He seems to be positively gleeful about the "Alice's Restaurant" aspect of this pseudo-science, where "you can get anything you want…"

Update: For the YouTube generation, Stanford has a video here.

Update: Jon Bagger's talk at the AAAS is available here.

Update: Various reports on the AAAS multiverse session including here, here (Wired blog, couldn't get in the room, too full), and here (New Scientist blog, describes the session as "you might have mistaken the proceedings inside for a stand-up comedy act").

Comments

1. **Q**
   February 19, 2007

   Linde goes on about what he calls 10^1000 vacua, and how they are "an unexpected gift from string theory… an eternal feast where all possible dishes are served."

   A string miracle, hallelujah! Regarding the eternal feast, Christians should be impressed with this blessing, for it way exceeds the miracle of Jesus feeding the four thousand (Mark 8:1-9),

   In those days the multitude being very great … they did eat, and were filled … And they that had eaten were about four thousand: and he sent them away.'

2. **Steven H. Cullinane**
   February 19, 2007

   Christians, as a web search shows, use the phrase "eternal feast" to refer, not to an earthly repast, but to Heaven. Few of them are likely to find the Heaven of string theory more appealing.

3. **A**
   February 19, 2007

   Interesting reporting about MiniBoone. You may already know this but the co-spokesfolk for this experiment, Janet Conrad and Mike Shaevitz, are both at Columbia.

4. **Hans de Vries**
   February 19, 2007

   Christians should be impressed with this blessing, for it way exceeds the miracle of Jesus feeding the four thousand.

   Christians use the phrase "eternal feast" to refer, not to an earthly repast, but to Heaven

   Then the joy must be because the number of the beast, 10^666, has been finally beaten. This makes all the difference between a Universe expanding into the eternal Heavens or one collapsing back into Hell fire. :^)

   Regards, Hans

5. **Bee**
   February 19, 2007

   A string miracle, hallelujah! Well, you don't actually need to turn water into wine, you just have to convince everybody that it is wine.

6. **Arun**
   February 19, 2007

   "Well, you don't actually need to turn water into wine, you just have to convince everybody that it is wine."

   Does this have to do with the question – what is reality?

7. **Steven H. Cullinane**
February 19, 2007

Or “What is Kool-Aid”?

8. **Chris W.**
February 19, 2007

It sounds like Andrei Linde has irrevocably *jumped the shark*.

9. **Louise**
February 19, 2007

You would have enjoyed hearing Larry Krauss' intro to the AAAS session: “Why scientists have gone mad.” He said that supernova data “naively implied” that the universe was accelerating. Introducing Lenny Susskind, Krauss asked rhetorically if strings were a sinking ship. The room was very crowded!

10. **Bee**
February 20, 2007

“Well, you don't actually need to turn water into wine, you just have to convince everybody that it is wine.”

Does this have to do with the question – what is reality?

Well. Everything has to do with the questions what is reality, no? But in this case I was more thinking about ‘marketplace’ tactics. Like, you don't need to actually produce the best pizza, you just need to convince everybody it IS the best pizza (alternatively, you can make sure all competitors go broke, then you have the one-and-only best-and-worst pizza).

PS: I don't want to get spam-filtered, so I won't include a link, but check Lubos for a 2007 edition of the landscape, I had to laugh quite a lot.

PPS: And make sure to turn down the volume before you do.

11. **a quantum diaries survivor**
February 20, 2007

By 2012, the upgrade of the LHC (SLHC) will probably be almost completed. So we will have many years to delve in Standard Model boredom, undisturbed by new R&D for the suppressed ILC…

T.

12. **Vogelsang**
February 20, 2007

So we will have many years to delve in Standard Model boredom,

If the SM is boring to you, why not switching to some other area of physics? Astrophysics and nanophysics for example are currently very active, with lots of experimental/observational results to feed theory.

13. **Thomas Love**
February 20, 2007

I just read on a google group:

Lubos Motl (a Czech national) pressured to resign from Harvard's physics department
because of his vicious shotgun public polemical attacks on Woit, Smolin,
Penrose and even Susskind!

Is it true?

14. **Peter Woit**
February 20, 2007

Thomas,

That's from Jack Sarfatti, and I think he's about as reliable a source of information as Lubos, and in this case the origin of this claim is some comment from Lubos about resigning from Harvard. No idea whether he is leaving Harvard, in the last few years he has repeatedly talked about it.

As far as I've ever been able to tell, the theorists at Harvard are fine with Lubos's attacks on people who criticize string theory (or his much more mild criticism of Susskind). On the other hand, his spending all his time on a blog devoted mainly to global warming and other right-wing causes, while not doing much in the way of physics research, may make their decision about whether to keep reappointing him much more difficult.

Sarfatti also has been distributing a description of the AAAS talk, which evidently he and Sean Carroll both attended. He claims that Susskind called his talk “Rats Leaving the Sinking Ship”, in this case the sinking ship of the “reductionist” view of string theory, that it is based on simple mathematical principles and makes unique predictions.

15. **the+ed**
February 20, 2007

Newton delayed the publication of his gravity law for many years in order to get a quantitative confirmation. Other scientists who proposed religion-sensible theories similarly waited for scientific evidence.

Now, we have no evidence for the String Anthropic Eternal Fest, no tests, no ideas about how to devise a test, a few years of attempts resulted into interesting papers but no physics, all this could end up with a failure. But we have some guys who publicly promote this new religion.

This is more dangerous than the old-fashioned string hype, that only irritated people within the physics community. Suppose that LHC will find nothing new: this would support anthropic views, but are we going to tell “we are the new pope, give us 10Giga$ for new experiments”?

16. **Garbage**
February 20, 2007

“But in this case I was more thinking about ‘marketplace’ tactics. Like, you don’t need to actually produce the best pizza, you just need to convince everybody it IS the best pizza”

Wouldnt that make it the BEST pizza anyhow? 😉
There seems to be some degree of objectivness around these sentence which might not apply to the REAL world….

G

17. **Peter Orland**
February 20, 2007

I want to echo Vogelsang’s song. There is a lot of interesting physics to do. So why do so many people stay in string theory?

I think one reason is that string theory is a field in which it is relatively easy to come up with a tractable problem. Some of these problems have more intrinsic value than others, but in any case, writing papers, getting grants, etc. is much easier if you know exactly what to do. Some of the work done is very interesting and most of it isn’t.

It is also interesting that string theory appears to be a bigger field than it is. You can do almost anything formal and call yourself a string theorist. This isn’t a joke. I haven’t published anything string-theoretic in a while, but when I attended a lattice-gauge theory workshop at the KITP a few years ago, a postdoc there said, “but you aren’t a lattice-gauge theorist. You’re a string theorist”. My experience suggests that in some eyes , almost any high-energy theorist not doing phenomenolgy or numerics is a string theorist.

18. **Phil**
February 20, 2007

Dr. Woit,
Your book is good, but too technical for a mass audience and not technical enough for specialists. Maybe if you really want to do damage to string theory, you should write a more academic book. I did get a sense you were trying to do that with this book, but ran into too many referees.

19. **Peter Woit**
February 20, 2007

Phil,

The book as published is pretty much as originally written. Referee problems kept Cambridge University Press from publishing it, but commercial publishers agreed to publish it in the form I wrote it. It’s not for everyone, and was intended to be more the sort of thing an academic press would publish than something really commercial. Given that, I did try to make it as readable as possible, and have been pleasantly surprised at how many different kinds of people have told me they enjoyed it and got something out of it. I think there is a market out there for things that are more intellectually challenging than a lot of the standard popular science fare.

As for more technical discussions, the blog is aimed at a narrower audience and I’ve probably beaten to death here the question of what the technical problems of string theory are. Many of them are not anything abstruse: you don’t need to have a lot of technical knowledge to see what the problems are with the anthropic landscape, for instance. I certainly hope in the future to write some much more technical things, but they won’t be about string theory…

20. **Q**
February 22, 2007

Stanford’s video http://news-service.stanford.edu/news/2007/february21/videos/180_flash.html only has a few seconds of Andrei Linde talking, about three-quarters of the way through, about the failure of string to accommodate observed cosmology. The eternal feast is edited out.

21. **D R Lunsford**
February 22, 2007

th+ed,

I believe Newton delayed publication because he was just disgusted with Hooke and the environment he represented, and generally a sour and suspicious person, and basically quit and went home with his football. He already knew he was right.

-drl

22. **the+ed**
February 22, 2007

as far as I know, Newton wanted to check that the gravitational field that explains the orbit of the Moon is quantitatively the same that explains the fall of apples. So, Newton had to do a difficult integral over the volume of the Earth, and (without knowing the Gauss theorem) this computation needed years.

Anyhow “Hypotheses non fingo” is a better slogan than “Hooke is a jerk”.

23. **D R Lunsford**
February 24, 2007

the+ed – re this last comment..

This illustrates in stark terms the real problem with physics. A lot of (supposedly) smart people have taken over, with no knowledge of either history or, really, the real world.

Ah! now i get it. You rotated the real world by pi/2, and made it imaginary.

-drl

Assorted Links

February 22, 2007
Categories: Uncategorized

Alain Connes is now a blogger, contributing to the new Noncommutative Geometry blog. There will be a conference in his honor next month in Paris.

Today and tomorrow there's a HEPAP meeting going on in Washington, with some of the presentations available here. Excluding the SLAC linac, which is getting moved around in the budget, the DOE HEP FY208 budget request is for $782 million, a very healthy more than 16% increase over FY2006. There's also an HEP demography study being discussed, which somehow involves "tagging" and "tracking" HEP physicists, and they seem to be having trouble with the tagging. Not clear what sort of analysis will be performed on the data once all HEP physicist tracks have been reconstructed.

Next month on the 28th there will be a debate on the topic of

Does string theory merge general relativity and quantum mechanics to explain the origin of time, space, and the universe? Or is it extraordinarily complex mathematics that has nothing to do with physics, and so explains nothing?

between Lawrence Krauss and Brian Greene at the Smithsonian Natural History Museum in Washington. I recall attending a similar debate at the Museum of Natural History almost exactly six years ago here in New York, also featuring Krauss and Greene. Some things seem destined to never change…

Comments

1. **Jimbo**
   February 22, 2007

   Hopefully, the Krauss/Greene debate will be a more or less balanced affair. In the aftermath of David Gross's colloquium here at the U. of Oregon, it is clear that the stringers are on the defensive, big-time, & are sending out their heaviest artillery to ramrod their case to the general community of physicists & grad students.
   Gross' lecture was notable only for its ineloquence, non-technicality, and being conducted very much from a bully pulpit here, cutting off a faculty member in mid-sentence who asked a reasonable question. All else listened, intimidated into silence for the entire 90 mins.
   The chair (a former Princeton colleague) allowed him to run ~30 mins over the normal hour, effectively crushing opportunities for questions, and virtually all bolted after the obligatory applause for a nobelist. It was a sales pitch thruout, and not even colloquial.

2. **woit**
   February 22, 2007

   Jimbo,

   My first substantive post on this blog 3 years ago was about what sounds like pretty much the same talk from Gross. Things definitely haven't changed at all. That talk, here at CUNY, also went on for 90 minutes, but there was time for questions. I don't think that going over time is something Gross does to thwart questions about string theory. same with cutting someone off; my impression is that's the sort of thing he has always done, mainly because he's quite quick, not very patient, and at least thinks he knows what a questioner is talking about before they've finished stating their question.

3. **Thomas Love**
   February 23, 2007

   Since Lawrence Krauss and Brian Greene are repeating a debate they had 6 years ago, I have to conclude that there in good money in it for them.

   I heard David Gross speak at the March 13, 2005 conference on The Legacy of Einstein's Science. Totally unimpressive.

4. **Peter Woit**
   February 23, 2007

   Thomas,

   I doubt that either Krauss or Greene are doing this for the money. What's striking to me is how incredibly static this field has become, with string theorists like Gross just digging in their heels, refusing to admit that there's any problem that needs to be addressed. The attitude these days seems to be to just give up, sit on one's hands and talk about how the LHC will solve all problems by giving us wondrous insights into the nature of space and time, etc… I see very little hope of positive change over the next couple years, and predict that in 2009 the same debates will be going on and Gross will be giving the same talk. After that, maybe the LHC results will start changing things. All depends on what they are…

5. **Thomas Love**
   February 23, 2007

   The debate on the question:

   "Does string theory merge general relativity and quantum mechanics to explain the origin of time, space, and the universe? "

   should be interesting since it is impossible to "merge general relativity and quantum mechanics". The two theories are mutually incompatible. A new theory which includes some aspects of each is necessary.

6. **a quantum diaries survivor**
   February 23, 2007

   Things will change, Peter. Just give it time… As much as I wish them a long life, the two debaters will die one day. And old ideas die with those who support them…

   Cheers,
   T.

7. **Zathras**
   February 23, 2007

So what was the Krauss/Greene debate 6 years ago like?

8. **woit**
   February 23, 2007

   Tommaso,

   Unfortunately what I find scariest these days are the ideas supported by people who are likely to outlive me… The fact that in a few short years we may no longer have to listen to as much propaganda about "new dimensions of space and time are right around the corner" is definitely something to look forward to.

   Zathras,

   It also included Glashow and Jim Gates on opposite sides, with Lisa Randall there to espouse an in-between point of view. The discussion was polite, but very much the same as what one hears these days, minus the Landscape, which hadn't yet come on the scene. Krauss even quoted at one point from the writings of an unknown guy who had just posted a polemical article about string theory on the arXiv. As I said, not much has changed.

9. **Greg Biffle**
   February 23, 2007

   I see a lot of troubling passisivity which is in part to blame for the "trouble of physics."

   Many people are of the mind–oh–it will come to pass.

   Oh–it makes good entertainment, year in and year out.

   Oh–they are fine fellows, and nice, and provide good PR for NSF.

   Oh–let them be. Of course it makes no sense, but join the group and get a postdoc and trips to Aspen.

   I have been a bit shocked to see all too many SCIENTISTS sit idly by as science is redefined to suit the political aspirations of entertainers and showmen.

   Our major crisis is not that new ideas aren't being accepted (even though they aren't), but that false ideas are being institutionalized.

   I thought that Not Even Wrong and The Trouble With Physics would have had a greater impact, but now Greene is celebrating with Discover Magazine's contest to see who can come up with the best two minute summary of ST.

   I wonder how many entries trumpeting the wisdom and truth in NEW and TTWP will be considered.

10. **mclaren**
    February 25, 2007

    A conjecture: "In the limit as new experimental results approach zero, the state of HEP approaches Dianetics."

11. **D R Lunsford**
    February 25, 2007

    Biffle said

    Our major crisis is not that new ideas aren't being accepted (even though they aren't), but that false ideas are being institutionalized.

    Yes, that is the exact point.

    -drl

12. **Johan Richter**
    February 26, 2007

    Terence Tao has a sort of blog as well. Check out "whats new" under his hoempage.

The Dynamics of Cats on Yarn Theory

February 23, 2007
Categories: Uncategorized

Over at Dynamics of Cats, Steinn Sigurdsson has a posting on Yarn Theory, to some extent a review of Lee Smolin's *The Trouble With Physics*. Steinn is an astrophysicist, but started out life as a string theorist before getting disillusioned with the subject. His account of his early career is well worth reading, doing a good job of capturing the atmosphere around 1990 at Caltech, a major center of string theory research. He was working on orbifold heterotic string compactifications, discouraged by how many there were. Not much has changed…

His general point of view on string theory is that it has led to the discovery of a family of unified theories, but that it is missing some basic principle that would tell us which is the true theory and why. He is sympathetic to Smolin's criticisms of the dysfunctional way theoretical research is being pursued, focusing efforts on one very speculative idea and making it unlikely that people who chose to try and work on others will be able to make careers for themselves:

… a lot of people will not only not believe that this is a real problem, they will make sure nobody else believes you either. Someone out there quite likely is already on the right track to the true theory, but their odds of survival in the current academic system are not wonderful. We may just have to wait a generation or two for a good approach to be rediscovered, which is a shame, cause some of us want to know! Now!!

… points on groupthink, and the systematic bias which discourages innovation and risk taking by young researchers hit painfully home – it is all too true, and yet it self-perpetuates because the mechanisms which reinforce conservatism in science are there for reasons. The system is flawed, and possibly broken, but the fix is not as simple as Smolin suggests – funding agencies are terrified of funding bad science, since there is so much pretty good science it is safe to fund, and as a community scientists are very harsh when bad science is mistakenly given precious resources.

It is the same market flaw that gives us beautiful flawless large red apples in supermarkets – with no taste. To get the old intense flavour varieties that everyone loves when they taste, we would have to choose small bruised discoloured apples when we shop, and leave the flawless big red apples with no taste in the bins. But collectively we do not, and the market responds. All for the fear of being the one ~~department head~~ consumer to go home with an occasional rotten apple. The real shame is that the big red shiny tasteless apples are rotten just as often, they just look so good sitting there, waxed and sprayed, in the bin. 'Course if you only get to buy one apple every three years you learn to be very conservative in your choice; don't want a rotten or even tart apple this decade.

I think Steinn gets to the rotten core of the problem here. There are very good reasons for conservatism in deciding what kinds of research to encourage, but with the very difficult situation that particle physics now finds itself in, the standard mechanisms for making these decisions have led to a seriously problematic situation. There are various things one could imagine doing to help get out of this, but even getting started on a discussion of them first requires that the powers-that-be in the field acknowledge the existence of a problem, and that has yet to really happen.

On a completely different subject, there's a new preprint by Michael McGuigan which manages to cite both *Not Even Wrong* (the book), and a Lubos Motl blog entry. The citation of my book seems unnecessary, surely there are other sources critical of string-based unification that have priority. The article is about the "see-saw" mechanism for getting the right magnitude of the cosmological constant, and it is for this that Lubos's blog gets a citation. This does seem to be a more promising idea about the CC than many. I for one think it will be a wonderful development if the field of particle theory turns around, stops promoting pseudo-science justified by the Weinberg anthropic CC "prediction", and heads instead in a more promising direction, all based on an entry in Lubos's blog…

Comments

1. **D R Lunsford**
   February 23, 2007

   fear that is where string theory went. It discovered a family of theoretical approaches and solutions which clearly encompass an essential truth of quantum gravity and its union with the standard model and post-standard model physics, but we missed on the deep guiding principle which tells us how to select the true theory (such as it is) and where the exact theory came from and leads to.

   A comment like this is simply mysticism. You don't discover "essential truths" in physics. There are no "inessential truths". If something is right in context, then it is right. String theory has never been right in any context. In fact, to coin a phrase, it's not even wrong.

   The current blog is essentially unique. It is empty of both mysticism and shameless egotism. It is wonderful that we have at least one physics blog with these properties.

   -drl

2. **D R Lunsford**
   February 23, 2007

   Oh Peter, I saw your book in Barnes and Noble. It made me smile. Well done!

   -drl

3. **David Corfield**
   February 24, 2007

   We're having a discussion relating to 'groupthink' over at the Café. I agree that its unacknowledged presence is at the root of the problem. You may enjoy the Thom quotation in this comment.

4. **mclaren**
   February 24, 2007

   Delightfully tongue-in-cheek take on string "theory":
   http://arxiv.org/abs/hep-th/0011065

5. **Ralf**
   February 24, 2007

   The citation of my book seems unnecessary, surely there are other sources critical of string-based unification that have priority.

This is an unwarranted condescension towards the author who probably knows best what has “priority” for him. Your “modesty asides” are becoming annoying. This is about the last thing one wants to hear from someone who put himself in the public sphere, after all. The fact that he cites you does not mean that he grants you the authority to tell him whom to cite. There are good reasons for prefering to cite you rather than those “other sources”, and I don’t think I have to spell them out. You’ve been overrating these sources all along.

6. **a quantum diaries survivor**
February 24, 2007

Ralf.

I think there is a good point in not putting oneself at the forefront – you are not the first one to fall if things turn awry, while in case you are running for the prize you can always make that extra step later.

I do not think that is the reason of Peter’s acknowledgement that others have been doing what his book does before him or more authoritatively. But it is a good reason nonetheless…

T.

7. **Peter Woit**
February 24, 2007

Ralf and Tommaso,

It just seems a bit peculiar for me to be getting credit for the fairly obvious idea that string theory based unification is an idea that hasn’t worked out. Many particle theorists have always thought this, and some have expressed themselves publicly on the topic over the years. I am happy to take some credit for successfully drawing a lot more attention to this issue, but that’s a bit different. If the LHC finds supersymmetry, with a pattern that identifies a particular compactification of string theory, producing real successful predictions, that will certainly make me look like a fool. I’m willing to take that chance…

8. **Chris Oakley**
February 24, 2007

Yes, but here is an example of string theory based unification that *has* worked out.

9. **srp**
February 24, 2007

One modest proposal to attack the groupthink problem, and the red apple problem, not just in physics but in the rest of the US scientific community: Establish a source of money (say a private foundation) that makes grants to research proposals that have been rejected by the NSF (or NIH) BUT have extreme variance in ratings from the panelists. The benefits of this idea are;

1) Breakthrough research (as well as plausible but wrong ideas) is likely to polarize expert opinion.

2) Disagreements about the prospects of a research proposal suggest that there might be high information content in the outcome of the research.

3) At least some qualified people consider the research interesting and feasible.

4) Nobody has to rewrite their proposals! Anything that gets scientists to spend more time doing science and less time writing proposals to do science makes me a happier citizen.

A colleague once said to me that if two academics have the same opinions, one of them is redundant. I wouldn’t go that far, but the spirit of the idea applies to this proposal. Maybe Templeton should shift some of its resources in this direction…

10. **Peter Orland**
February 24, 2007

The groupthink problem is inherently unsolvable. Any foundation or organization which is designed to encourage ideas outside the norm will have its own set of preferences (or prejudices). A foundation which rejects NSF proposals will have its own criteria, which will either quickly conform to groupthink or fade into obscurity.

The situation isn’t new. There is always resistance to new ideas. Organizations which attempt to change the situation disappear or merge into the mainstream in order to survive (the Nobel foundation is a good example. Nobel prizes were origninally supposed to encourage junior people who hadn’t yet made their most significant contributions).

The only way to deal with groupthink is make an effort to change what the group thinks. That is a realizable goal, but only after a lot of yelling and screaming.

11. **srp**
February 25, 2007

Peter: I’m sorry if my suggestion wasn’t presented clearly.

I’m not proposing a foundation to “reject NSF proposals.” I am proposing that someone fund NSF-rejected proposals that had high variance in the ratings they received from the NSF review panels.This agency wouldn’t have groupthink–they wouldn’t have to have any-think–because they would work off the scores already generated by the NSF evaluators. And since they would fund proposals where the reviewers for the NSF DISAGREED with one another, groupthink in evaluation would be ruled out from the start.

This idea may be impractical for a number of reasons, but I don’t think an ex cathedra statement that groupthink is inevitable, without showing specifically why this exact mechanism would fail, is very persuasive. And “changing what the group thinks” isn’t a big help if it just creates a new set of unsupported orthodoxies.

12. **Arun**

February 25, 2007

srp, it sounds like a good idea. Maybe bring it to the attention of the Bill and Melinda Gates foundation?

13. **Nathan Myers**
February 25, 2007

Arun: very funny. George Soros, perhaps?

14. **Nathan Myers**
February 25, 2007

How appropriate that someone would move from string theory to astrophysics. Astrophysics is in much the same state, today, except in lacking eloquent voices like Smolin's and Woit's to puncture it.

15. **mclaren**
February 25, 2007

I would disagree strongly that astrophysics experiences anything like the degeneration we observe in string "theory" (how can it be called a scientific theory when it fails to make any experimentally faslsifiable preidctions?).

Astrophysics remains vibrant and fertile because _new experimental data keep coming in_. This is the real Achilles' heel of HEP right now. The data at energy ranges and luminosities and cross sections required to disconfirm even low-range estimates on the superpartners, let alone the Higgs, has simply not been there. We reached the limits of current accelerators and hit a wall. You can only do so much, and then you have to build a whole new machine — which nowadays takes decades. Calibrating the detectors on these accelerators is a gigantic undertaking. The computer technology involved is simply staggering. You're talking about events so rare, and amounts of data so large, that it boggles the mind.

By contrast, with astrophysi9cs you just send up a new satellite. Or sometimes not even that — dump some cleaning fluid in a tank underground and set up photodetectors. That's cheap! It's quick. It's easy compared to building an ILC or LHC. So astrophysics has been awash in new experimental results, while HEP has been starved for 'em. That to me is the core of the problem in HEP right now.

16. **Q**
February 25, 2007

Nathan, I recall that Peter answered this point before. The small positive cosmological constant, or some alternative idea which does the same job, is required to fit GR to the observations of supernovae redshifts, explaining why gravity isn't slowing down the recession.

String theory by contrast provides speculative explanations for things which are not observed, such as unification at the Planck scale (inventing supersymmetry to explain that speculation), and inventing gravitons (inventing supergravity to explain gravitons). String theory thus 'explains' speculations, not observations.

The point Peter made before about cosmology is that at least it is being led by observations, unlike string theory.

Mainstream models for observations might turn out wrong, but that's better than being not even wrong. Approximations like flat earth theory, caloric, and phlogiston could later be disproved by evidence. Epicycles could provide an endlessly complex mathematical way of representing observed data, but were eventually replaced by Kepler's simpler laws for convenience, which in turn could be explained by a simple inverse square force law.

String theory doesn't even model or duplicate any existing observations, so it is worse than Ptolemy's epicycles: Feynman's criticism of string theory was largely that it doesn't address the parameters of the standard model. It's not even wrong because it doesn't even model anything known to exist.

17. **Peter Orland**
February 25, 2007

srp,

You took my misprint too seriously. I garbled the message earlier.
Yes, I know you didn't mean another round of rejections.

I also have no objection to your idea of some private foundation funding proposals rejected by the NSF (or DOE). The more funding, the better.

My point was that such measure would not cure the groupthink sickness, whatever their other virtues might be.

18. **Peter Orland**
February 25, 2007

Just to add. The reason I don't think groupthink would be cured
is because any such private funding would quickly succumb to
the same pressures the NSF and DOE have to deal with. Maybe not
in the first year or two, but before the funding could alter the
course of the field.

19. **Peter Orland**
February 25, 2007

Nathan,

To add my voice to those above…

Experimental cosmology is thriving. The theorists have not caught up
with the data yet. Explaining dark matter wil take more investigation
(maybe particle physics experiments), and nobody has a clue about
dark energy. This is a healthy field, even if theorists haven't satisfactorily
explained what is going on.

20. **Kyrie**

February 25, 2007

>This is a healthy field, even if theorists haven't satisfactorily
>explained what is going on.

I'd change "even if" to "because".

21. **Christine Dantas**
February 25, 2007

Hi,

John Baez has just released his Week246 writing about your book and Smolin's.

Christine

22. **srp**
February 25, 2007

Peter: Still not reading what I wrote. The proposed innovation isn't really a new funding SOURCE. It is a new funding RULE, a rule which explicitly prevents groupthink by funding ideas which draw high-variance, divergent responses from reviewers. If you have a theory as to how this RULE could be corrupted by groupthink, I would be interested.

I tossed out the idea that a private foundation might implement this rule only because I see significant political obstacles to its adoption by the standard funding agencies. The essence of the proposal is not the funding source, however.

23. **Amos Dettonville**
February 25, 2007

The idea of a rule that forces a group of people not to make decisions based on "groupthink" is sort of interesting… in a paradoxical sort of way. As described by "srp", it seems to rely on at least some pre-existing non-groupthinkers, because he says to award money to proposals that meet with very divergent responses. In a pure "groupthink" environment, there wouldn't BE any divergent responses (more or less by definition), so the scheme would only have a chance if we suppose there are some non-group thinkers in the system with votes that matter.

But this raises the interesting question of whether a scheme could be devised that would actually enable a *pure groupthink environment to free itself from its own groupthink. Suppose everyone agrees on what they should do… but they are worried that they might all be suffering from groupthink. How could they address this worry? I'm reminded of a Seinfeld episode in which George decided that, from now on, he was going to do the opposite of whatever he thought he should do in every situation.*

But seriously, I think the folks at google and other search engine designers have considered how groupthink can cause their search criteria to go unstable, because they feed back to themselves. (Give high ranks to pages that have lots of links, and those pages then tend to get even more links, which gives them even higher rankings…) If you notice, they sometimes "dither" their rankings, to "shake things up" a bit, and see if other pages would rise to the top after a re-shuffle. (Dithering is a technique carried over from control theory.) Maybe what the funding agencies need is a little dithering. (Off topic, but I suspect a healthy human mind must have some degree of "dithering" (not to be confused with blithering), or it would just become fixated on a small number of specific things.)

24. **Ralf**
February 26, 2007

Why is it that nobody is taking down string theory from its very inception and input? Why bother with any of its "content" in the first place?

Doesn't "supersymmetry" really mean not understanding the nature of fermions and bosons? Doesn't "GUT" really mean not understanding the nature of the observed gauge interactions? Isn't the idea that virtually any observed structure is due to some kind of spontaneous symmetry breaking (and the "anthropic principle") transparently the cynical nihilism of intellectual bankruptcy?

Up to now progress in the understanding of fundamental structures has always been marked by an increase in simplicity and efficiency and explanatory prowess.
The principles associated with string theory are not only "not even wrong", they are of fundamental stupidity and lack of sophistication. The undeserving heirs squandering the wealth the fathers worked so hard to gain. To truely believe in string theory one must have never understood any physics in the first place. It's so 19th century!

25. **Chris W.**
February 26, 2007

Amos,

Another way to describe that healthy tendency to dither might be a tendency to get bored and frustrated with whatever rut one has gotten into, due to social influences, financial inducements, or one's own overripe enthusiasm.

mclaren,

I think that allegedly tongue-in-cheek take on string "theory" (hep-th/0011065) was intended fairly seriously. What the hell—it beats someone's posting yet another goddawful braneworld scenario.

26. **srp**
February 26, 2007

Amos: My understanding, not based on systematic data but anecdote, is that in real life there are many grant proposals to the NSF and NIH that receive high-variance scores–some judges love them and some hate them. Supposedly, these usually get rejected with only the proposals that get high scores from all evaluators funded (and not always all of those, depending on budgets). So whether or not it can happen in theory, divergent evaluations appear to be an empirical phenomenon. Maybe a moderate psychological level of groupthink is institutionally amplified by the current rules of only funding proposals that everyone agrees on.

Dithering, simulated annealing, and other ideas for randomly jiggling things to get away from being trapped on local peaks may be good analogies for what I'm suggesting. One common approach to avoiding groupthink is to use dialectical processes, such as an adversary system, to surface as many points as possible for and against something. But that only works if the judges of the dispute aren't subject to groupthink. My proposed rule finesses that problem.

Another approach is to cultivate multiple funding sources drawn from sociologically separate communities. The military and ARPA used to function this way to a degree, but I'm not sure that still holds true for basic science. There are lots of people in Congress and the bureaucracy who argue for "rationalizing" the system by centralizing decision-making in a body like the NSF.

27. **Peter Orland**
February 26, 2007

srp,

I am still skeptical. How does one re-evaluate proposals with diverging scores to encourage diversity of ideas? One institution which does essentially such a re-evaluation is the Physical Review, as you probably know. I don't think their refereeing policy has specifically encouraged more directions of research (nor, do I think it was intended to).

Though re-evaluation proposals might not be a bad idea, I can't see it altering the dominant culture.

28. **Benni**
February 26, 2007

John Baez sais something on NEW and TTWP

29. **Benni**
February 26, 2007

http://math.ucr.edu/home/baez/week246.html

30. **srp**
February 26, 2007

Peter O: Here's one way to do it: A scientist whose grant proposal got rejected at the NSF submits the same proposal to the hypothetical new funding source, along with the reports and scores of the NSF review panel that rejected him. The new funding agency sorts these proposals according to the within-panel variance in NSF ratings. It then funds the highest-variance proposals first and works its way down the list until it exhausts its budget (or the proposals start to look too gamy).

I am not aware of the Physical Review process (I'm not a natural scientist), so if they do something similar I would be interested to hear how it works (in both senses).

31. **Ralf**
February 26, 2007

How do you write a proposal if what you have is an idea that might lead to a breakthrough, but you can't possibly put the idea itself in the proposal because that would effectively mean giving it away? Doesn't the funding process virtually exclude anything that doesn't fall under "normal science"?

32. **srp**
February 26, 2007

Ralf: Wow! Are these panelists really that unethical that you can't report your ideas without them ripping you off? That would be a lot more serious problem than groupthink.

In any case, I'm not saying that my proposal would guarantee funding to all breakthrough ideas, merely that current proposals that have more breakthrough potential than average could be funded.

33. **Peter Orland**
February 26, 2007

srp,

Here is the Physical Review refereeing process (as far as I can tell from my experience submitting and refereeing papers). Most papers are sent to two referees. If there is one rejection and one acceptance it goes to a third referee. If that referee rejects it, it usually goes back to the author with a rejection from the editors.
The author can (and often does) challenge this assessment.
Sometimes the editors can make a clear judgement of rejection or acceptance – but if they can't, it goes to another referee. And so on.
If the author can get two acceptances, the paper is usually published.
So at some level, the process resembles your suggestion.

While in principle I like your idea, I am not sure it would work effectively if real money is involved. Financial considerations will eventually make the people running your system unwilling to take too many risks.

34. **lostsoul Ph. D.**
February 27, 2007

An insight into peer review, as practised in the UK, circa 1980, long before science became as venal as it is today. (And this is tawdry applied physics/engineering, in which the stakes are not that high, and one is thus less likely to compromise one's mortal soul.) A barely post-doctoral physicist puts together a paper, relating to a finer point of detail relating to laser operation, and submits it to a respectable journal. Nothing, beyond the usual politesse of receipt, is heard for several months; thereafter discreet enquiries elicit only the reassurance that due process is in hand. Some nine months on, the second referee's report arrives, which grudgingly accepts that the substance of the MS is sound, but would benefit from some experimental verification (contact with reality, hep-th dudes) which the referee would be happy to provide, in a joint publication. Honour amongst thieves or what? And if a trip to Stockholm was up for grabs? Oh dear.

35. **Chris Oakley**
February 27, 2007

Hi lostsoul,

The private sector is much better. If my own experience is anything to go by, then I cannot say that people behave any more honourably, but at least they are scrabbling for something more tangible than glory. It is also a thing that can be exchanged for goods or services.

36. **srp**
February 27, 2007

Peter O: You're right that any attempt to institutionalize behavior at a non-profit foundation is quite difficult. Random personnel turnover eventually leads to a critical mass in management and among trustees who want to circumvent or change the deceased founder's original vision. John MacArthur probably wouldn't be too thrilled with today's MacArthur Foundation, and I wouldn't even want to try to estimate the rpms in Henry Ford's grave over the antics of the Ford Foundation.

On the other hand, if a founder's mandate is fairly narrow and specific, it can only be overturned by the trustees going to court and proving that changing conditions have made that mandate non-practicable or absurd. That doesn't happen very often. So, for example, a foundation that was tasked to give out college scholarships to people residing in a given town would be hard to mess with unless the town became depopulated or colleges became obsolete and disappeared.

Anyway, I am off this thread. Thanks for the feedback.

37. **amused**
March 1, 2007

"There are very good reasons for conservatism in deciding what kinds of research to encourage, but with the very difficult situation that particle physics now finds itself in, the standard mechanisms for making these decisions have led to a seriously problematic situation."

Yep. As best I can tell, the main citerion for determining who gets jobs is: "Which influential senior physicists are you buddies with, and what have you done to help advance their research programs?" This might work fine when the way forward is clear, e.g. during the development of the Standard Model. After all, senior influential physicists usually get to be that way for a reason, and young people could do much worse than to follow their lead. But in a situation where, to paraphrase Polchinski, there is "remarkable convergence on an unproven idea" it might be good for physics if there was a possibilty for people to work on other things and still have the chance of a career. Btw, "other things" doesn't just mean the QG alternatives of Smolin&co but also the various topics in formal theory that Susskind refers to as "near-coast exploration". Physics advanced quite far by near-coast exploation in the past, so it might be a good idea to allow it to continue in the present rather than forcing everyone who wants a career to go off on some or other epic voyage.

Actually there is an easy way to fix this situation if people really want to: Take away the NSF/DOE funding which currently goes to individual faculty members or research groups to hire postdocs and use it to fund postdocs directly via an open competition. Schemes like this already exist, e.g. the EU's Marie Curie fellowships. A crucial feature of the scheme would be that the applications are assessed by a broad committee with representatives from all areas of theoretical physics. Then the string theory member of the committee will have to explain to colleages from condensed matter and atomic physics why string theory applicant X, who has never done anything on his own but whose famous advisor has declared him to be "brilliant", is more deserving of support than non-string applicant Y who has published single-author papers in PRL. Many, perhaps most, of the people supported under such a scheme will be the same ones who find support under the current system, but there will also be others following more independent paths who currently don't have a chance. For it to be worthwhile the scheme would have to be supplemented with a similar one at the junior faculty level, e.g. along the lines of the UK's PPARC Advanced Fellowships, but with assessments again being made by a broad committee with representatives from all areas. If the reps are all from particle theory it will just be buddies rubbing each others backs and the outcomes will be no different than under the current system.

Various…

February 26, 2007
Categories: Uncategorized

The Princeton Physics department colloquium on "New Neutrino Oscillation Results from the MiniBooNE Experiment" scheduled for this Thursday has been canceled. I wonder what is going on with that. Have they not opened the box yet on their result, or did they do so and have a problem with what they found? Other talks about MiniBooNE results are still scheduled for March 14th in Manchester ~~and March 17th in Montreal (the abstract of this last one is ambiguous about whether there will be results "MiniBooNE's oscillation path and what may lie beyond, will be presented")~~ . The MiniBooNE colloquium has been replaced by a Nikita Nekrasov talk on "The Mathematics of String Theory". Not sure if I'll make a trip down to Princeton that day; Nekrasov's talks are usually worth hearing, but I'd prefer to hear a more technical talk about his recent work, which is quite interesting.

John Baez's latest "This Week's Finds" is largely about the controversy over string theory and my book and Lee Smolin's. As usual, John's take is quite level-headed. Faced with people who claim that string theory is "the only game in town", John advocates leaving town, striking out and looking for another place to live and work, maybe even starting a new one. Some comments about this at his blog.

On Friday at Fermilab Michael Chanowitz gave a talk on Precision Electroweak Data and the Direct Limit on the Higgs Mass. He updates previous work on these fits, including recent CDF and D0 values for the top and W masses. The new, presumably better, top quark mass values from CDF and D0 are lower than those of a couple years ago, with recent CDF results about 170 GeV. The main interest in these fits is that they give you a predicted value of the Higgs mass, although not a very accurate one. The lower top mass drives down the predicted Higgs mass, to the point where it is starting to get in trouble with the fact that LEP showed it had to be heavier than 114 GeV at 95% confidence level. The fits to all data give a most likely value for the Higgs mass of 85 GeV, with only an 18% probability of it being over 114 GeV.

Chanowitz devotes a lot of attention to the one measured value that deviates the most (3 standard deviations) from that predicted by the other data: the forward-backward asymmetry in b-quark production at the Z-pole. If you throw this out from the fit, on the grounds that it is less reliable than the other data since it doesn't match the SM as well, your Higgs mass really goes down, to a most likely value of 48 GeV, with only a 2% chance of it being above 114 GeV. Hard to know what to make of this, evidently it's hard to come up with a model that would explain the anomalous forward-backward asymmetry while not ruining the rest of the fit. Maybe this is a first indication that the SM is not the full story….

Update: This just in, from Michael Nielsen. Seems like this is the month for Fields Medalists to become bloggers. First Alain Connes, now it's Terry Tao. Will Grisha Perelman be next?

Update: Well, not Grisha Perelman, but today the latest mathematician blogger is David Goss. Goss is a specialist in the mathematics of function fields over finite fields, and his first post includes comments about how ideas have entered this very different field from physics, as well as remarks about the work of Bost and Connes that expresses zeta-functions in term of partition functions of statistical mechanical systems.

Update: A couple more. The talks at the recent AAAS session on A New Frontier in Particle Physics are available (~~some in a weird format I've never seen before which works on Internet Explorer, not Mozilla~~). Also some remarks by DOE's Raymond Orbach at the HEPAP meeting where he seems to be raising an important question: even if the ILC is built, it may not be ready until the mid-2020s or later, and the Tevatron and SLAC B-factory will be closing down relatively soon, so "I would like to re-engage HEPAP in discussion of the the future of particle physics. If the ILC were not to turn on until the middle or end of the 2020s, what are the right investment choices to ensure the vitality and continuity of the field during the next two to three decades and to maximize the potential for major discovery during that period?"

Update: Fermilab director Pier Oddone discusses the Orbach letter and the need for planning for the possibility of a long wait for the ILC in today's Director's Corner at Fermilab Today:

I am requesting a steering group composed of members of our laboratory and the community under the leadership of Deputy Director Kim to produce a detailed plan for positioning the field and Fermilab in the next two decades for a robust program of discovery based on accelerators. I am asking for such a plan by August 1st.

Comments

1. **Kea**
   February 26, 2007

   Perhaps you could mention Tommaso's excellent ongoing commentary on the Higgs.

   Maybe this is a first indication that the SM is not the full story…

   Well, hardly. Cosmology has been troubling for a while now.

2. **LDM**
   February 26, 2007

   For the last few decades, astrophysicists have been making amazing discoveries in fundamental physics: dark matter, dark energy, neutrino oscillations, maybe even cosmic inflation in the very early universe! — John Baez

   Baez should confine his commentary to math. Dark matter, dark energy, and cosmic inflation are still speculative ideas, not confirmed discoveries.
   What has happened to scientists? The need to publish and self-promote must never be allowed to overtake the scientist's responsibility to disseminate accurate information to the public.

3. **Marco**
   February 27, 2007

   If I read well, the MiniBooNE talk at McGill that you mention already took place in March 2006 (i.e. one year ago).
   By the way, I'd be a bit surprised if the collaboration announced results in an individual invited talk at some university. They might choose a more formal way, e.g. at Fermilab, so I am keeping an eye on the Fermilab events list.
   Please keep covering news&rumors on this topic, thanks!

4. **1uk3**
   February 27, 2007

   why not Grisha Perelman? It would be a nice way for him to spend some of his free time.

5. **Q**
February 27, 2007

The hyperlink to David Goss's first blog post in your second update doesn't work because "http://" is duplicated…

6. **Peter Woit**
February 27, 2007

Q,
Thanks, fixed.

7. **island**
February 27, 2007

Baez should confine his commentary to math. Dark matter, dark energy, and cosmic inflation are still speculative ideas, not confirmed discoveries.

My experience with the thinking of John Baez tells me that he's not making the kinds of assumptions about the true nature of our observations that many string theorists make when he says that. Which means that he'll now step-in to prove that I don't know what the heck he thinks… 😉

What has happened to scientists? The need to publish and self-promote must never be allowed to overtake the scientist's responsibility to disseminate accurate information to the public.

It has also been my observation that there is a rather large gap between the rigor that is expected for accepting conclusions made about Relativity, and those of QM.

It isn't pretty in the latter case… and "Katie bar the door"… beyond that.

8. **Ari Heikkinen**
February 27, 2007

It's kind of nice that experts begin to start blogs. That way the general public can ask questions from them and get expert answers on their time and for free.

9. **Coin**
February 27, 2007

The lower top mass drives down the predicted Higgs mass, to the point where it is starting to get in trouble with the fact that LEP showed it had to be heavier than 114 GeV at 95% confidence level. The fits to all data give a most likely value for the Higgs mass of 85 GeV, with only an 18% probability of it being over 114 GeV.

What kinds of bounds will the LHC be able to fix on the mass of the Higgs; and is the maximum mass it will be able to probe at enough that if the LHC *doesn't* find the Higgs, we can start asking questions like what if the Higgs as we think of it doesn't actually exist?

10. **srp**
February 27, 2007

Is there any significant effort in the physics community to develop new accelerator concepts that can get to higher energy without costing as much as a couple of aircraft carriers? I recall reading years ago about plasma-wave concepts, but haven't heard much since. A physicist (whose identity I can't remember) once told me that getting that idea to work would be as hard or harder than magnetic-confinement fusion, but I have no way of knowing if that opinion is general among accelerator physicists.

It seems to me that if there are any radical ideas out there, now is the time to break 'em out. The public is daunted by the prospect of building multi-kilometer, multi-billion dollar extravaganzas for such esoteric (to the public) purposes. (Personally, I'd rather build you guys a giant accelerator than pave over more of West Virginia for the glory of Robert Byrd, but my priorities are not shared in the broader political system.) Anything that mght make higher energy more affordable, even if it takes time to develop, strikes me as something the physics community should embrace.

11. **Martin Griffiths**
February 28, 2007

There has been some recent progress with plasma wakefield technology: see [here].

12. **Peter Woit**
February 28, 2007

srp,

I've written about this at least a couple times on this blog, see for instance:

http://www.math.columbia.edu/~woit/wordpress/?p=339

As far as I can tell, turning these exotic ideas into workable HEP accelerators is conceivable, but still a long way off (50 years?). The idea I've heard that sounds closest to something that might work is to use this kind of acceleration as an "afterburner": at the end of a long conventional linac like the ILC, double the beam energy using this kind of technology.

13. **Thomas Love**
February 28, 2007

Just found a jewel:

"It is very important that we do not all follow the same fashion… It's necessary to increase the amount of variety… and the only way to do this is to implore you few guys to take a risk with your lives that you will not be heard of again, and go off in the wild blue yonder to see if you can figure it out." Richard Feynman(1965), Nobel prize in physics award address.

Take a risk or string yourself up.

14. **A.J.**
February 28, 2007

SRP,

There's a lot of work being done on plasma wake acceleration being done right now. See for instance this expository piece in Nature, from Feb 15 2007.

http://www.nature.com/nature/journal/v445/n7129/full/445721a.html

Still a lot of problems to be solved, of course, but they're making progress. And I don't imagine that the energy losses would be any worse than in synchotrons.

15. **Neville**
February 28, 2007

Maybe I'm blind, but I couldn't find the *wild blue yonder* text in Feynman's terrific Nobel lecture. I discovered a slightly expanded version of the quotation elsewhere. It would be nice to have the complete original text.

16. **Neville**
February 28, 2007

For obvious reasons, I should have described Feynman's address as *mostly* terrific.

17. **srp**
February 28, 2007

Thanks for the references on plasma wakefield technology. Those articles were actually more encouraging than I thought. Problems, but the sort of thing that could actually be solved, and some signs of substantial progress.

So let me ask: Is this research at the point that it is primarily resource limited or idea limited? In other words, if more people and apparatus were directed to developing these technologies, would things get sped up significantly? Or is this still a matter of waiting for inspiration to strike on some currently unsolvable problems?

Because if these technologies could be brought on line much faster with more resources, I would definitely try an argument like, "Well, if you don't want to fund the ILC, will you at least give us (pick your percentage) of it to work on plasma wakefields and afterburners?" From an advocacy standpoint, it's a lot easier to sell research on cool new breakthrough technology than on expensive applications of old technology. It also has the advantage that it might be the smarter policy.

18. **JoAnne**
March 1, 2007

The box (MiniBooNe) is still closed. I heard they were on step 2 of a 6 step process to open the box. It will be another month or two…

The LEP results on the forward-backward asymmetry of the b-quark are puzzling. This measurement depends on the product of the electron's couplings to the Z boson and the b-quark couplings to the Z-boson, and is 3-sigma off from the expected Standard Model value. However, individual measurements of both the electron's (at both LEP and SLC) and the b-quark's (at SLC) couplings are bang on the Standard Model expectation.

19. **Peter Woit**
March 1, 2007

Joanne,

Thanks for the MiniBooNE news (or rumors…) and the comment on the anomalous forward-backward asymmetry result. It does sound like this anomaly can't be new physics, but then, where is the Higgs???

20. **anonymous**
March 10, 2007

see http://neutrino.pd.infn.it/conference2007/Talks/Shaevitz.ppt for the most recent MiniBooNE talk.

Symmetry Magazine

February 28, 2007
Categories: Uncategorized

There's a new issue of Symmetry magazine out. It is a bimonthly magazine about particle physics put out by SLAC and Fermilab, and often has interesting and informative articles. But, even though I generally read the whole thing when it comes out, I've always had a feeling that, somehow, there was something missing. This latest issue kind of explains why.

In a very well-done article about the BaBar experiment and B-physics, John Ellis is quoted, explaining the origin of the name "penguin diagram" as follows:

That summer, there was a student at CERN, Melissa Franklin, who is now an experimentalist at Harvard. One evening, she, I, and Serge went to a pub, and she and I started a game of darts. We made a bet that if I lost I had to put the word penguin into my next paper. She actually left the darts game before the end, and was replaced by Serge, who beat me. Nevertheless, I felt obligated to carry out the conditions of the bet.

For some time, it was not clear to me how to get the word into this b quark paper that we were writing at the time…. Later…I had a sudden flash that the famous diagrams look like penguins. So we put the name into our paper, and the rest, as they say, is history.

If you look up the original source of this, you find a bit more of an explanation of where that "sudden flash" came from. Here's the full second paragraph:

For some time, it was not clear to me how to get the word into this b quark paper that we were writing at the time. Then, one evening, after working at CERN, I stopped on my way back to my apartment to visit some friends living in Meyrin where I smoked some illegal substance. Later, when I got back to my apartment and continued working on our paper, I had a sudden flash that the famous diagrams look like penguins. So we put the name into our paper, and the rest, as they say, is history.

Lots of other articles worth reading in the magazine, including one by an undergraduate at Humboldt State in California about taking a science course that explained how physical reality is stranger than any fiction, ending with the anthropic principle and the theory of evolutionary cosmology. The course is called Cosmos, and you can check out its web-site. Maybe I'm wrong, but I get the impression that John Ellis is not the only physicist out there who may have at one time or other sampled the agricultural products of Humboldt County…

Comments

1. **island**
   February 28, 2007

   ending with the anthropic principle and the theory of evolutionary cosmology

   A true anthropic cosmological principle *necessarily* defines a Darwinian Universe… which indicates that physicists pursuing this idea should be looking for a mechanism that enables a universe with pre-existing volume to "leap/bang" to higher orders of the same basic configuration… now, pass the bong, "hippie"… 😉

2. **Steve Myers**
   February 28, 2007

   On Cosmos-type classes. Maybe it's possible to understand the physics without the math but I doubt it. I've found people who say that or "feel" that are just kidding themselves. If you can't do the problems, I don't think you understand the material. Would they say the same thing about their bank account?

3. **Chris Oakley**
   February 28, 2007

   I think that the likely explanation is that the game of darts was one of the things that Ellis hallucinated while smoking the illegal substances.

4. **Arun**
   February 28, 2007

   "Aspects of Symmetry", Selected Erice Lectures, Sidney Coleman
   The 1979 lecture, 1/N, on the large N limit of SU(N) theories:

   "For the baryons, things are not so good. Witten's theory is an analytical triumph but a phenomenological disaster".

   For some reason, I found it very funny.

5. **Jimbo**
   March 1, 2007

   Looks like High-Times at Humbolt High ! And we only thought that such libations were confined to UC Santa Cruz ! Without doubt, one of the best physics course advertisements I've ever seen, and I'll bet his enrollment saturates in the first week.
   And lets not forget, that Sagan & Glashow both were heads (am I leaving anybout out ?), so there must be somethin to it.
   Should be a lotta phun !

6. **Thomas Love**
   March 1, 2007

   The biggest difference between science and science fiction is that fiction has to make sense. The biggest difference between science and mathematics is that science has to match reality. Mathematics can be used to model fictional universes as well as the one we live it. That is the problem with the landscape: too many fictional universes and we are not sure that there is one which matches reality! My PhD is from UC Santa Cruz and I got high by thinking about higher dimensions, no agricultural substances necessary!

7. **island**
   March 1, 2007

   But it is *people* who carelessly abuse the counterintuitive aspects of some theories to rationalize their way round appearance of absurdity that appears with others, thereby enabling them to avoid the most natural extensions in order that they might dodge causality and first principles in order to use “selection effects” to choose from a hypothetical and mathematically derived woulda’ could’a shoulda’ “what-if” landscape, instead of doing real physics.

This goes WAY far back before ST became the only game in town though… That’s the kind of thing that Einstein used to bitch about… a LOT!

8. **Greg Biffle**
March 1, 2007

I spent the weekend reading original papers by Bohr, Einstein, and Heisenberg.

Oddly enough, there was no mention of drug use, nor cutesy-wootsie dart games, nor insider wink-wink-nudge-nudge let’s get some cash from NSF to meet again next year for more darts and drugs.

Physicists of yore seemed quite content to contemplate reality, and thus left us with lasting physics.

There seemed no peer-pressure back then to smoke up in those glorius original papers.

I hope that by and by we (and journals) can return to supporting physicists concentrating on advancing physics and contemplating the beauty and natural wonders of the world that surrounds us, rooting it all in humility, logic, reason, and a dedicated search forr the truth.

9. **Coin**
March 1, 2007

> I spent the weekend reading original papers by Bohr, Einstein, and Heisenberg.
>
> Oddly enough, there was no mention of drug use, nor cutesy-wootsie dart games, nor insider wink-wink-nudge-nudge let’s get some cash from NSF to meet again next year for more darts and drugs.

Clearly your mistake here was not including some Feynman in the mix.

10. **Peter Woit**
March 1, 2007

Greg,

I’m deleting your latest comment. Please do not post rants and personal attacks on people here. You’re just adding heat and no light to the discussion. There’s too much of this already.

Short-term and Long-term

March 1, 2007
Categories: Uncategorized

There's an article in this week's Science magazine by Adrian Cho entitled Dreams Collide With Reality for International Experiment. It's about DOE Undersecretary Orbach's warning to HEPAP (mentioned here) that current plans were too optimistic about the time-scale for the ILC, leaving a potentially dangerously long period with few US HEP experiments in progress.

Some physicists who had proposals for experiments (BTeV and RSVP) that were canceled in favor of going ahead full steam with the ILC were not amused:

*Meanwhile, Orbach's call for a program of smaller projects evoked jeers from researchers whose experiments had been cut. "This is really stupid and very frustrating because we **had** a program," says Sheldon Stone, a physicist at Syracuse University in New York who worked on an experiment called BTeV that would have run at the Tevatron collider at Fermilab.*

While experimentalists are worrying about the short-term, string theorists seem to be taking the long view. In his talk on String Theory: Progress and Problems at the recent conference celebrating the centennial of Yukawa and Tomonaga, John Schwarz ends with the conclusion

Even if progress continues to be made at the current rapid pace, I do not expect the subject to be completely understood by the time of the Yukawa-Tomonaga bicentennial.

I don't find his claims about a current rapid pace of progress very convincing, and the idea of the entire next century of theoretical particle physics being dominated by the kind of unsuccessful more and more complicated string theory constructions we've seen for the last 25 years doesn't seem like something to look forward to.

Turning from the difficulties of the future, Howard Georgi gave a wonderful talk on The Future of Grand Unification at the same conference, which was actually mainly about the past, largely devoted to telling the story of how he and Glashow came up with the first GUT models. He emphasizes the kinds of manipulations of representations of Lie algebras that he and Glashow were masters of and used to construct many different kinds of models. There's also an advertisement for his wonderful book on the subject, the text for his course where I first many years ago encountered this subject. This semester I'm teaching my own course on the same material, from a rather different point of view, emphasizing geometry. It remains one of the most beautiful and central parts of modern mathematics as well as physics.

Comments

1. **Peter Orland**
   March 1, 2007

   Hi Peter,

   I am surprised you are waxing so poetically about GUTs. GUTs have no real successes, with the noteworthy exception that experiments to detect proton decay were built (and found something else). The GUT unification scale isn't really all that convincing as physics – it isn't even completely clear that there IS unification, since the matching of the Weinberg-Salam and QCD scales is so rough. I remember the vanishing total charge of each generation (for anomaly cancellation) was considered a success. But later it was pointed out that it has nothing to do with GUTs specifically.

   Something's there at higher energies, but why should it be a GUT?

2. **Peter Woit**
   March 1, 2007

   Peter,

   I thought Georgi's talk was fascinating to read through, but that doesn't mean I'm a huge fan of GUTs. One nice thing about the GUT story is that Georgi shows how particle theory is supposed to work: he and Glashow came up with a tolerably simple model which does offer some sort of explanation of some of the questions the standard model doesn't answer (I do think the way fermions fit into the spinor representation of SO(10) is intriguing). They then made real predictions based on it (proton decay), and when these didn't work out they gave up (Georgi points out that he stopped working on GUTs in the early 80s). What they didn't do was keep coming up with more and more complicated models trying to evade failure. Once that seemed to be what was happening, I think they changed their attitude to "this doesn't seem to be working. Maybe it's true, but making more complicated models is a waste of time", and went on to try other things.

   The main problem with the GUT idea is that it's not a very satisfactory sort of unification, since you've just moved many of your problems into a new Higgs sector. The electroweak Higgs sector is already the source of most trouble, adding a new Higgs sector with similar problems doesn't seem like a promising way to make progress.

   It is a wonderful mathematical exercise in Lie algebra technique, even if these higher rank groups don't end up having anything to do with physics.

3. **Benni**
   March 1, 2007

   As to the ILC thingy…
   I find it somekind strange that physicists want it.
   The ministry of research in germany recently stated that it is unlikely for germany to apply for becoming Hamburg an ILC site.

   Well, after some asking around why, it becomes clear, that there is research underway at various universities in germany to increase the energy of particle beams with many new techniques (some, in fact optical).

   It seems that the german ministry of research has decided to leave the ILC to US america and then, after 20 years or such, build a new linar collider of much higher energy!

   At this time then, you in the US will have a wonderful ILC without any value!

Whatever State wants to get the ILC site, I wonder what to do with this rather low energy collider, when not a single hint of supersymmetry can be found. Is it not likely, that this will be a machine, which will have almost nothing to measure?

4. **Coin**
March 1, 2007

> There's also an advertisement for his wonderful book on the subject [Lie Algebras in Particle Physics]

If you don't mind me asking, what would you say would be the level and nature of required background knowledge for this book?

5. **Benni**
March 1, 2007

As to peter, what he says about GUT: I think that is absolutely correct. Glashow and Georgy are very honest researchers of the good old school.
The behaviour of some theorists, everytime when an idea does not work (in Feynmans words) to come up with a "fix-up" and look what happens if you add a parallel-world, hyperspace or a new unobserved differend standard-modell as hidden sector is simply what would be considered as scientific misconduct in experimental science. Expecially when they overhype these theories in press releases and refuse to mention any of their problems. When one looks at the Susy and sting propaganda one simply gets scared, since they often do not mention even the obiousliest problems

This is the same behaviour as of an experimentalist, who measured something and before publication deletes all his data which might be problematic.
For example why after the appearance of
Phys. Rev. D 20, 403 – 408 (1979) http://prola.aps.org/abstract/PRD/v20/i2/p403_1 does anyone continue to write propaganda material like: http://arxiv.org/abs/hep-ph/0203114 where it is statet that there would exist "a consistent pictures of breaking susy" and let experimentalists in fact believe, that such a theory would exist?

6. **Peter Woit**
March 1, 2007

Coin,

It's a textbook for a graduate physics course, basically what a mathematician would call a physicist's take on Cartan-Weyl theory, with lots of examples worked out. I took the course near the end of my undergraduate years, but I was a ridiculously ambitious student and took lots of courses that were over my head (this one was only a bit over my head… and Georgi was a good teacher).

The background needed to go through the book is basically quantum mechanics and a good grasp of how angular momentum and spin works in that subject.

Benni,

I think most people agree that the case for the ILC depends on the LHC first finding evidence of some new physics that can be studied at ILC energies. One reason I suspect Orbach is saying the ILC may take a while is that it may take a while for the LHC results to arrive. New accelerators and detectors often take a while to get working properly.

If the LHC does find something new that the ILC would be the right tool to study, it would make sense to build it, and building it in the US will solve the problem of what's next for US HEP research. This is what people are preparing for, but it is a bit of a gamble. Things like laser acceleration technologies are a long ways off, I strongly doubt having an accelerator of that kind ready 20 years from now is feasible.

7. **Coin**
March 1, 2007

Thanks.

8. **JC**
March 1, 2007

Peter,

slightly offtopic

What specific courses did you take in undergrad that were "over your head"? (Besides the Georgi group theory course).

9. **Peter Woit**
March 1, 2007

JC,

By far the most ridiculous thing I did was take a quantum field theory course in my second year (taught by Roy Glauber). I did come in with advanced standing due to taking a lot of AP exams, so technically I was a junior, but still… That year I started out taking a classical mechanics course from Arthur Jaffe, and sitting in the QFT course to see what it was about. A few weeks into the semester I realized that the classical mechanics seemed boring and required doing lots and lots of hard problem sets that had to be handed in on time, whereas the graduate course just had a few problem sets due every so often. So I went to see my advisor (Shelly Glashow), who, when I asked him if I could drop one and enroll in the other, irresponsibly said "Sure, whatever you want, I don't want you to be unhappy…". He would sign pretty much anything, if you could find him.

This is definitely getting off-topic, enough about misspent youth.

10. **Domenic Denicola**
March 1, 2007

Re: Schwarz's attitude, I too was rather perturbed by it when I went to talk to him last term about frontiers in physics. He said he didn't think we would have a Theory of Everything before he died, which seems to me to be overly pessimistic. Maybe it's just the naivete of youth (I'm an undergraduate freshman), but it seems to me there are enough people poking around in the right directions that we should stumble upon something, and probably in the next 10–15 years. Maybe working on string theory for so long engenders pessimism, heh.

11. **JC**
March 1, 2007

Domenic Denicola,

I use to think exactly like you 20+ years ago, when I was younger and naively bought into string theory. It took me a long time to eventually figure out that string theory wasn't all that it was cracked up to be.

12. **Peter Woit**
March 1, 2007

Domenic,

I'm about 15 years younger than Schwarz, so probably have a bit longer than him. If you extrapolate from the last 25 years, both Schwarz and I will be long gone before there's serious progress towards a TOE.

But, unlike him, I'm more optimistic. What strikes me about the Standard Model is both how incredibly good it is and how much beautiful, deep mathematics it involves. Besides gravity, most of the problems it leaves unresolved have to do with electroweak symmetry breaking. If either the LHC tells us what is really going on there, or someone figures it out without experimental guidance, we may very well end up with a theory that deserves to be called a TOE.

13. **Zing**
March 2, 2007

Undersecretary Orbach's remarks is his 'politically correct' way of saying the ILC is dead. Kaput. So forget about it. There is simply no justification for such an expensive machine. It is the wrong machine for the wrong time. We can try again in 10 years' time.

14. **a**
March 2, 2007

Maybe LHC will tell that ILC has a strong physics motivation. Today we have a strong argument against ILC (high-energy physics needs moving to more advanced technologies) and a strong argument in favor of ILC (it would keep the US high-energy physics community alive). Small experiments have been killed to emphasize the last argument (without explicitly mentioning it), but maybe it is not the best strategy.

It seems unavoidable that high-energy physics will become (for both experimentalists and for theorists) a part-time job: one big experiment every 30 years and in the meantime progress on astrophysics, neutrino physics, and whatever lower energy physics that happens to be interesting. Trying to do this transition in a smooth way could be the way of exploring physics up to the maximal possible energy.

Theorists often don't like near-cost navigation, but being interested only in big projects (such as a Theory of Everything) can be suicidal.

15. **Thomas Love**
March 2, 2007

In "Lie Algebras and Quantum Mechanics" (1970), Robert Hermann wrote theoretical physicists

"…are like greyhounds all chasing the same rabbit, never really catching it, but periodically convincing themselves that it is not worthwhile chasing this particular one any longer, and going off after another." (p. x)

It looks like Schwarz has given up.

16. **Benni**
March 2, 2007

BTW: Peter, do you often speak with Glashow? Because in your book, where you write about symmetry principles and group representation theory, these chapters look, as if they could be written from Glashow himself (At least it looks like this, when one looks at what Glashow is trying now with group theory). It seems indeed as if your book belongs to somewaht that could be called a "Glashow-school". Is this correct?

17. **Peter Woit**
March 2, 2007

Benni,

I haven't talked to Glashow since I was a student, and am definitely not a member of a "Glashow school". I suspect that, other than agreeing with me about a lot of the problems of string theory, we see things very differently. In his book Georgi (who definitely is in the "Glashow school") warns very explicitly about the seductive danger of making too much of representation theory, treating it as more than a device for doing approximate calculations. From his point of view, I've definitely gone too far and been seduced, since I see representation theory as a fundamental principle of physics. In general Georgi and Glashow are both pretty suspicious of abstract mathematics (one reason they didn't become string theorists), quite the opposite of my belief that progress in particle theory will require more mathematics, not less.

18. **Benni**
March 2, 2007

Well now, Glashow does such things involving group theory:
http://arxiv.org/abs/hep-ph/0601236
(I for me, of course find this somewat misleading and wrong. But it is refreshing to see that some people try their own new ideas).

19. **JC**
March 2, 2007

Peter,

How common is it for particle theorists to be suspicious of abstract mathematics? (Excluding string theorists).

Some of my previous colleagues who did string phenomenology, were not really interested in the overly abstract math stuff. On the other hand, a few other string colleagues were heavily into the abstract math stuff (ie. stuff related to mirror symmetry, etc …).

20. **woit**
March 2, 2007

JC,

I think particle theorists are all over the map on this issue these days. Some are highly suspicious, some are quite enamored of abstract math. For something related, see my next posting….

21. **D R Lunsford**
March 3, 2007

Peter said

The main problem with the GUT idea is that it's not a very satisfactory sort of unification, since you've just moved many of your problems into a new Higgs sector. The electroweak Higgs sector is already the source of most trouble, adding a new Higgs sector with similar problems doesn't seem like a promising way to make progress.

This is exactly the point – embedding the gauge group is not the real problem, and that is why SU(5) seemed doomed to me from the beginning. In the "real" theory this embedding will be explained naturally and perhaps the heroic effort will be seen in a new light and will not simply be recorded as a failure. I think in some sense the failure of GUT, which was certainly unexpected, coming hard on the heels of the failure to find a UFT for light and gravity, demoralized the community of physicists, and so string theory had a fertile ground of disillusionment in which to take root.

-drl

Weinberg in Physics Today

March 2, 2007
Categories: Uncategorized

The March issue of Physics Today is now available. It contains a piece by Steven Weinberg based on a banquet talk he gave to a group of postdocs. He describes his own memories of his time as a postdoc, writing that "Many of us were worried about how difficult it seemed to make progress in the state that physics was in then." This was the heyday of S-matrix theory and he comments:

Some people thought that the path to understanding the strong interactions led through the study of the analytic structure of scattering amplitudes as functions of several kinematic variables. That approach really depressed me because I knew that I could never understand the theory of more than one complex variable. So I was pretty worried about how I could do research working in this mess.

He describes envying the previous generation of 10-15 years before his time, that of Feynman, Schwinger, Dyson, and Tomonaga, thinking that all they had had to do was sort out QED, and speculates that they in turn had envied the generation before them since quantum mechanics was even easier. I can see that he's trying to provide encouragement, ending with

So the moral of my tale is not to despair at the formidable difficulties that you face in getting started in today's research… You'll have a hard time, but you'll do OK.

Postdocs in high energy physics these days do need some encouragement, but I also think they need some recognition from their elders that they're facing a different situation than that faced by earlier generations. Coming into a field that has not seen significant progress in about 30 years is a different experience than what Weinberg or previous generations of particle physicists had to deal with. High energy physics is now facing some very serious problems, of a different nature than those of the past, and I think these deserve to be mentioned.

In the same issue, Weinberg makes another appearance, in an exchange of letters with Friedrich Hehl about the torsion tensor in GR. Hehl takes him to task for his comments in a previous letter that torsion is "just a tensor", pointing out that it can be thought of as a translation component of the curvature. Weinberg responds that he still doesn't see the point of this:

Sorry, I still don't get it. Is there any physical principle, such as a principle of invariance, that would require the Christoffel symbol to be accompanied by some specific additional tensor? Or that would forbid it? And if there is such a principle, does it have any other testable consequences?

Actually, Weinberg is rather well-known for taking the point of view that GR is just about tensors, and that their geometrical interpretation is pretty irrelevant, so it's not surprising that he doesn't see a point to Hehl's comment. In his well-known and influential book on GR, he explicitly tries to avoid using geometrical motivation, seeing this as historically important, but not fundamental. To him it is certain physical principles, like the principle of equivalence, that are fundamental, not geometry. There's a famous passage at the beginning of the book that goes:

However, I believe that the geometrical approach has driven a wedge between general relativity and the theory of elementary particles. As long as it could be hoped, as Einstein did hope, that matter would eventually be understood in geometrical terms, it made sense to give Riemannian geometry a primary role in describing the theory of gravitation. But now the passage of time has taught us not to expect that the strong, weak and electromagnetic interactions can be understood in geometrical terms, and too great and emphasis on geometry can only obscure the deep connections between gravitation and the rest of physics.

This was written in 1972, just a few years before geometry really became influential in particle physics, first through the geometry of gauge fields, later through geometry of extra dimensions and string theory. I recall seeing a Usenet discussion of whether Weinberg had ever "retracted" these statements about particle physics and geometry. Here's an extract from something written by Paul Ginsparg, who claims:

back to big steve w., when he wrote the gravitation book he was presumably just trying to get his own personal handle on it all by replacing any geometrical intuition with mechanial manipulation of tensor indices. but by the early 80's he had effectively renounced this viewpoint in his work on kaluza-klein theories (i was there, and discussed all the harmonic analysis with him, so this isn't conjecture…), one can look up his research papers from that period to see the change in viewpoint.

Comments

1. **Sean Carroll**
   March 2, 2007

   Weinberg's point that the torsion is "just a tensor" is an attempt to draw a distinction with the Christoffel part of the connection, which is *not* a tensor. You can't just stick a term like (connection)^2 into the action, but there's no trouble at all sticking in a term like (torsion)^2, or any of a million other terms. There aren't any symmetries that give you any information at all about what the dynamics of the torsion should be, or how it should couple to other fields. So you can write down whatever action you like. (In particular, there's no obstacle to giving the torsion a mass at the Planck scale.) Consequently, there is no conceivable way that you could detect some new field and say "aha, that must be the torsion."

2. **Peter Woit**
   March 2, 2007

   Sean,

   Isn't this just like the curvature then?

   Won't your particles couple to it in a very specific way, since you're not changing the Dirac equation, just allowing spin-connections that are not torsion-free? I assume that's the kind of physical prediction that Hehl is talking about.

3. **Tony Smith**
   March 2, 2007

   Peter said that Paul Ginsparg said "… by the early 80's … big steve w. [Weinberg] had effectively renounced this viewpoint [of]… replacing any geometrical intuition with mechanial manipulation of tensor indices …".

   I think that Paul Ginsparg is correct about that. At the 1984 APS DPF Santa Fe meeting, Weinberg gave a talk "Unification Through Higher Dimensions" in which he not only discussed "The Kaluza-Klein Idea" but also concluded by discussing "Research Directions" including "The SO(32), N+4=10 Green-Schwarz theory".

   At that same meeting John Schwarz gave a talk (on work with Shahram Hamidi) entitled "A Unique Unified Theory That Could Be Finite And Realistic", in which he

discussed “SO(32) and E8xE8 superstrings” with respect to finding “the correct low-energy (compared to the Planck mass) theory in four dimensions with which to make contact”.
Swarz went on to say that “In collaboration with J. Patera, we have classified all the chiral N=1 theories that satisfy the one-loop (and hence two-loop) finiteness conditions. The list includes theories based on E6, SO(10), SU(5), and SU(6) that can describe three or more families without mirror partners. However, if we also require the occurrence of elementary Higgs fields in representatins that can give realistic symmetry-breaking patterns, then one unique scheme is singled out. …
The unique model that is potentially finite and realistic is based on the gauge group SU(5). …
To be perfectly honest, we are not sure how seriously this should all be taken. The three-loop calculation could result in a dramatic failure and is therefore of utmost importance.
The connection between superstring compactification and finite four-dimensional theories is only a speculation … It need not be true in general.”.

IIRC, during the talk by Schwarz, Weinberg stook in the rear door of the lecture room. Many of the physicists at the meeting were on the edge in deciding whether or not superstring theory would be the new fashion for theoretical physics, and were looking to Weinberg for a sign about which way to go. At the end of the talk, Weinberg commented that he was favorably impressed, and in my opinion that comment had some influence in the establishment of superstring theory as the fashion and paradigm for the next 20+ years.

It is interesting that in his 1984 Santa Fe talk Schwarz presented, on grounds related to what I think of as “real physics”, a “unique unifed theory”
and
that Schwarz proposed a direct test of that “unique” theory by doing three-loop calculations
and
that even though the “unique” theory failed to be a realistic model, superstring theory advocates refused to acknowledge the failure,
and even now, over two decades later,
they continue to “work” on stuff that has
more and more complex structure (to avoid theoretical failure)
and
less and less contact with experimental observations (to avoid refutation by experiment).

Tony Smith

4. **Eugene Stefanovich**
March 2, 2007

Weinberg is also famous for his non-orthodox views on QFT. For example, he writes on page 200 of his “The quantum theory of fields” vol. 1:

Traditionally in quantum field theory one begins with such [Klein-Gordon] field equations, or with the Lagrangian from which they are derived, and then uses them to derive the expansion of the fields in terms of one-particle annihilation and creation operators. In the approach followed here, we start with the particles, and derive the fields according to the dictates of Lorentz invariance, with the field equations arising almost incidentally as a byproduct of this construction.

To me this suggests that Weinberg does not consider fields as being fundamental physical quantities. However in another place (http://www.arxiv.org/hep-th/9702027) he seems to be contradicting himself:

In its mature form, the idea of quantum field theory is that quantum fields are the basic ingredients of the universe, and particles are just bundles of energy and momentum of the fields.

It is also interesting to see the evolution of Weinberg’s views on the local gauge invariance principle. On page 990 of Phys. Rev. 138B (1965), 988 he writes (after short introduction to what he calls “extended gauge invariance”)

The only criticism I can offer to this textbook approach is that no one would ever have dreamed of extended gauge invariance if he did not already know Maxwell’s theory.

I read it as an admission that he doesn’t know how to justify the gauge invariance principle, except by the fact that it works. I find this attitude somewhat refreshing when compared to the modern textbook dogmatic approach. He continues:

In particular, extended gauge invariance has found no application to the strong or weak interactions, though attempts have not been lacking.

This was in 1965. Long before electroweak theory and QCD. Do we know better today **why** there should be local gauge invariance? Besides the fact that it works.

5. **JC**
March 2, 2007

Tony,

Has anyone done this particular three-loop calculation mentioned by Schwarz?

6. **Moshe**
March 2, 2007

The way I read the torsion issue is that it is an irreducible representation of the Lorentz group (= a tensor), so when coupling to gravity or anything else you just write the most general couplings consistent with symmetries, since there is no symmetry that will prevent them from being generated.
\
On the other hand, treating this tensor like “torsion” entails presumably very specific way of coupling to matter, in other words setting some of the possible couplings to zero.

7. **Sean Carroll**
March 2, 2007

No, particles don’t *have* to couple in any specific way. Ordinarily we decompose the covariant derivative into (d+G), where d is the partial derivative and G is the connection coefficients. And they have to come along with each other, since each alone is not a tensor. But with torsion this becomes (d+C+T), where C is the Christoffel part of the connection and T is the torsion. But there isn’t any reason they have to come together! (d+C) by itself is a perfectly good covariant derivative, and T is a perfectly good tensor. In a low-energy effective theory, we would expect T to couple to anything it was allowed by symmetries to couple to. And there aren’t really any symmetries to prevent it from being massive and coupling indiscriminately.

More here: http://arxiv.org/abs/gr-qc/9403058

Geometry is fun for inspiration, but physics plays by its own rules.

8. **gunpowder&noodles**
March 2, 2007

On the other hand, it is clear that the generic connection does have torsion. The restriction to zero torsion connections is a very strong one, so it should be justified. Setting something equal to zero is not the same as saying that it *has* to be zero for physical reasons.

Traditionally, students were told that torsion had to vanish because non-zero torsion, being a tensor, would violate the equivalence "principle". [Basically, anything about GR that one doesn't understand is referred to that so-called principle.] People apparently didn't notice that the same idea can be used to "prove" that the curvature tensor also has to be zero.

Hehl had a very influential paper about torsion in Rev Mod Phys in about 1977. People interested in the subject should read that. Torsion certainly would be observable, but it hasn't been: for some reason the torsion tensor of our Universe is zero. Nobody knows why.

9. **Tony Smith**
March 2, 2007

JC asked "… Has anyone done this particular three-loop calculation mentioned by Schwarz? …" [for his "unique" string theory SU(5) "unified theory"]

Yes. As might be expected, after Schwarz made his 1984 Santa Fe talk, a lot of work was done on that SU(5) structure.

For example,
in Physics Letters B, Volume 160, Issues 4-5 , 10 October 1985, Pages 267-270,
D. R. T. Jones and A. J. Parkes wrote a paper entitled "Search for a three-loop-finite chiral theory". Its abstract stated:

"Grand-unified theories have been constructed out of supersymmetric SU5 theories which are finite at one and two loops. We investigate the three-loop divergences in these models and find that they can never be three-loop finite. We present an example based on E6 of a three-loop-finite chiral theory."

The Jones-Parkes paper was received by Physics Letters on 19 June 1985, about 7 months after the Santa Fe meeting (which was from 31 October 1984 to 3 November 1984).

Tony Smith

10. **Joseph Smidt**
March 3, 2007

I tend to agree with Weinberg. When I took GR I thought geometry was front and center in importance. Now taking QFT, I am beginning to think that geometry distracts from particles and fields and other fundamental quantum ideas.

I also liked what Sean Carroll said in a comment on his blog. He said you should be careful wanting to axiomatize relativity. I've given this a lot of thought and it seems every time we axiomatize a theory we end up having to alter things here and there to fit nature. I'm beginning to think it is more important to have an evolving theory which evolves with experiment.

11. **Levi**
March 3, 2007

"That approach really depressed me because I knew that I could never understand the theory of more than one complex variable."

Sorry to isolate this bit of quotation from Weinberg, but it reminded me that I have never seen a good clear text on multi-variable complex analysis, Does anybody here know of one?

12. **Peter Woit**
March 3, 2007

Sean,

You can certainly, as a consistent low energy theory, write down all sorts of things. But, if your Lagrangian is supposed to be a fundamental one, and your only fields are spinors and the spin-connection (metric, but not necessarily torsion-free), then there's one simplest Dirac equation (standard covariant derivative, i.e. "minimal coupling"), and you can ask what the implications of assuming that are. I haven't really looked at Hehl's papers, but am just assuming that's what he does. Forgetting questions of aesthetics, philosophy or whatever, Hehl claims to have predictions (even if not practically testable), and Weinberg seems to be saying he doesn't see this (although his wording is unclear). Is Hehl right?

I agree that the problem with torsion is that you don't know what its dynamics is. If you start looking at all consistent possible couplings to all physical fields, maybe you can't predict much of anything. Presumably Hehl is making some sort of assumptions about this dynamics.

Joseph,

The problem with the "GR involves geometry, QFT" doesn't point of view is that the it turns out that the very particular QFT that governs the real world (the Standard Model) is very much a geometrical theory (gauge fields=connections, field strengths=curvature, matter fields=sections of associated vector bundles with connection).

The point of talking about "axioms" is just so you make explicit what your assumptions are, and being clear about that is generally a good idea.

13. **Moshe**
March 3, 2007

Peter, interjecting again…GR certainly is not a fundamental theory, as it is not renormalizable.It is an effective field theory at low energies, so you can only make sense out of it if you allow all possible couplings (they run with scale, so it is meaningless to just set them to zero, zero at what scale?). So, if you add to your theory that particular tensor, there is no way to ensure it couples precisely like a torsion. If I got it right, that is the issue discussed.

14. **Sean Carroll**
March 3, 2007

I think Moshe and I are in agreement. Things like "simplicity" and "aesthetics" and "geometry" might inspire you to think about theories with certain symmetries and certain degrees of freedom. But once you have that, you should write down all of the possible terms consistent with the symmetries; if they're not there in the fundamental Lagrangian (whatever that means), they'll be generated at low energies by radiative corrections.

There are many specific models with torsion, and each individual one may very well make predictions. But there is no possible observable that would allow you to conclude that a certain new degree of freedom was the torsion. It's just a tensor, as someone once said.

(In fact, classic "Riemann-Cartan theory" doesn't even give the torsion a kinetic term, for the very bad reason that it's not there in the Einstein-Hilbert action; since the Einstein-Hilbert action didn't have torsion at all, that's just silly. If you don't give it a kinetic term, it's just a Lagrange multiplier and you can integrate it out, leaving some non-renormalizable contact interactions.)

15. **Peter Orland**
March 3, 2007

Levi,

There are few readable texts on several complex variables. I assume you mean subjects like polydiscs, Hartog's theorem, envelopes of holomorphy, etc. The best known texts are by Bochner and Martin and by Vladimirov, but these are quite old and it isn't clear how useful they are nowadays.

A short book which is to the point is the paperback by Narsimhan. A more
sophisticated text by is by Krantz.

16. **Peter Orland**
March 3, 2007

Sorry that's Narasimhan.

17. **woit**
March 3, 2007

Moshe and Sean,

I understand the effective field theory philosophy that our Lagrangians are just effective low energy theories, so all you have is what you think you know about the symmetries of the theory to constrain terms in the Lagrangian, and non-renormalizable terms like the Einstein-Hilbert one occur with powers of the energy scale at which the effective field theory is no longer valid.

But I'm just not convinced by it. The fact that asymptotically free gauge theories make sense at all energy scales makes me suspect that they're not just effective field theories for something more fundamental. I think it continues to be a valid thing to do to try and make sense of QFTs not just as effective low energy theories, but as fundamental theories, valid at all energy scales. If you try and do this, there are three sources of problems:

1. Elementary scalars like the Higgs. They're bad news for lots of reasons, and quite possibly are just effective fields, not part of the fundamental theory (e.g. technicolor). Maybe the LHC will clear this up…

2. The U(1) gauge coupling. Maybe there are GUTs, and the U(1) unifies with a non-abelian group, is asymptotically free above a certain energy scale.

3. GR. Maybe the people claiming N=8 supergravity is finite are right. Maybe there are fundamental Lagrangians that give consistent dynamics at all energies, with an Einstein-Hilbert term, other terms having some specific structure. There seems to be something going on in supergravity that we don't understand, causing extra cancellations. Maybe the LQG people are right, and if you quantize your theory differently you'll get something that makes sense. Maybe part of this story is that, just like SU(2) and SU(3) connections are fundamental fields in the non-gravitational part of the theory, the spin connection is a fundamental field, not necessarily torsion free, and its torsion components may have some well-defined dyamics, with measurable consequences.

I'm still curious about Hehl's claims of a prediction, and what assumptions they make. Any comments about those?

18. **Sean Carroll**
March 3, 2007

I'm happy to admit that there is some fundamental high-energy Lagrangian, but what we actually observe at low energies is the low-energy effective field theory. For example, the gauge field-strength tensor doesn't involve the torsion at all, so there is no "fundamental" coupling between torsion and photons. But there is a coupling between torsion and fermions, and between fermions and photons, so voila — at low energies you get an effective coupling, suppressed by the Planck scale, between torsion and photons. And so on — there's nothing in the dynamics of "torsion" that protects it from being just another propagating tensor field at low energies.

I believe that Hehl is referring to classic Einstein-Cartan gravity, in which the torsion is non-propagating. You can integrate it out to leave a four-fermion interaction, suppressed by the Planck scale. Observable in principle, but not relevant to any conceivable experiment.

19. **Peter Woit**
March 3, 2007

Sean,

OK, I think it's clear now that you and I were just talking about two different things. I was discussing a conjecturally fundamental Lagrangian, you the effective low energy Lagrangian relevant to what we actually measure. In principle the low energy Lagrangian is derivable from a fundamental one, should it exist, but I certainly acknowledge there's no problem-free candidate for such a thing at the moment.

20. **D R Lunsford**
March 3, 2007

I am so happy that Weinberg is still Weinberg!

Yes, torsion in GR proper is a lost cause because of reducibility. But, you can make a theory where torsion is a sort of gauged electromagnetic duality, and there it is interesting. Since a charge-pole system has an intrinsic angular momentum independent of spin, and since asymmetry of the connection is associated with intrinsic angular momentum, torsion becomes very interesting. But I have other things to work on.

The Weinberg book on GR is far and away the best one to be had, even now. The approach is thoroughly geometrical, in spite of the author's own statements. See this thread on SPR.

-drl

21. **JC**

March 3, 2007

Peter,

Is there a possibility that there does not exist a “fundamental” Lagrangian?

Or maybe higher energy physics (ie. higher than the electroweak scale) cannot be quantitatively described in a Lagrangian/path integral framework?

22. **D R Lunsford**
March 3, 2007

Sean Carroll said

Consequently, there is no conceivable way that you could detect some new field and say “aha, that must be the torsion.”

You could if you could find a magnetic monopole.

-drl

23. **Peter Woit**
March 3, 2007

JC,

By “Lagrangian” I was referring to the standard QFT setup, in your favorite formalism: path-integral, Hamiltonian, or whatever. Sure, at scales we haven’t probed yet, QFT may no longer describe things. Maybe it will need to be thrown out in favor of string theory or something else. But there’s no evidence for this, and, to me, the fact that asymptotically-free QFTs give mathematicallly beautiful theories that are highly constrained and perfectly consistent at all energy scales seems very striking. Maybe nature has got a better idea, and these are just low energy approximations, but I haven’t seen a convincing better idea.

24. **D R Lunsford**
March 3, 2007

JC,

This is the approach of David Finkelstein as I understand it – if you are philosophically comfortable with that, then you should investigate his work.

-drl

25. **Chris W.**
March 3, 2007

Can anyone clearly state the presuppositions behind the assumption that a “fundamental” Lagrangian exists? Is there a way to know that one doesn’t exist, or must we settle for simply observing that we haven’t found one—ie, all the candidates have problems—and their are some general reasons to be skeptical that we will find one?

26. **Moshe**
March 3, 2007

One more comment, possibly redundant…The use of EFT does not commit you to the form of the fundamental theory. Even when you have a complete description, such as in QCD, it is still useful to use various EFT (such as chiral PT, soft collinear EFT, etc.) in different regimes. That is why I don’t like the expression “EFT philosophy”. It is not a philosophy, it is just a self-consistent approximation scheme.

27. **D R Lunsford**
March 3, 2007

Chris,

My own view is that the Lagrangian is the embodiment of the idea of a conservation law, and insofar as physics is the study of conservation laws, it involves a Lagrangian. Finkelstein believes instead that physics is about processes, and that conservation laws are derived ideas in the limit of many processes.

-drl

28. **Peter Woit**
March 3, 2007

Chris W. and DRL,

Sorry, but this discussion has moved far off-topic into a kind of discussion I don’t want to have to try and moderate here. Please stick to comments relevant to what Weinberg had to say in Physics Today.

29. **Levi**
March 3, 2007

Peter Orland,

Thanks! I’ll take a look at those books.

30. **Tony Smith**
March 3, 2007

Levi asked “… Does anybody here know of … a good clear text on multi-variable complex analysis … ? …”.

In addition to the references given by Peter Orland,
from my old (1960s) college days, a book entitled “Analytic Functions of Several Complex Variables (Prentice-Hall 1965) by Gunning and Rossi is an introduction that I like.

The Gunning-Rossi book is based on polydiscs, which are different from unit balls,
so you might also want to look at the book “Function Theory in the Unit Ball of C^n” (Springer-Verlag 1980) by Walter Rudin. As Rudin said in his preface:
“… Around 1970, an abrupt change occurred in the study of holomorphic functions of several complex variables. Sheaves vanished into the background, and attention was focussed on integral formulas and on the “hard analysis” problems that could be attacked with them … The ball is the prototype of two important classes of regions that have been studied in depth, namely the strictly pseudocomplex domains and the bounded symmetric ones. …”.

If you are interested in the bounded symmetric domains, a standard reference is the book “Harmonic Analysis of Functions of Several Complex Variables in the Classical Domains” (AMS Translations of Mathematical Monographs Volume 6 1963) by L. K. Hua (whose life story in China is very interesting but probably off-topic here).

Two recent books with a lot of interesting further details are:
Analysis on Symmetric Cones (Oxford 1994) by Jacques Faraut and Adam Koranyi; and
Analysis and Geometry on Complex Homogeneous Domains (Birkhauser 2000) by Jacques Faraut,Soji Kaneyuki, Adam Koranyi, Qi-keng Lu; and Guy Roos.

Of course, the above is far from an exhaustive list, and you can find a lot more fascinating stuff to read if you are interested.

Tony Smith

31. **Levi**
March 3, 2007

“The Gunning-Rossi book is based on polydiscs, which are different from unit balls”

Right, and for various reasons I am more interested in the unit ball. Unfortunately, the book by Rudin is hard to get ahold of these days. Thanks for the help, though, and I know I’m off topic so I will leave it at that.

32. **nontrad**
March 3, 2007

I have to agree with Peter on his point about asymptotically free theories. QCD, for example, from it’s number of parameters to its mathematics and experimental verification is a very very interesting theory. To speak loosely, I’ve always regarded QCD as a paradigm.

GR, on the other hand, (where I’ve spent more then a few idle moments) is, as much as everyone has ever wanted it to be anything other, a much much more difficult situation (including the work of….insert bigwig buzzword names here from A to Z).

And here on this blog and elsewhere, we all know this! Bottom line, GR is a stumbling block that is not simply the speed bumb that the success of SU(2)XU(1) or the success of SU(3) might suggest. Instead, GR is more like an Everest of some kind… and like Everest just waiting for the right person (who as far as I am concerned will arrive one of these days).

When I was first learning GR I read Weinberg’s book and considered his comments about geometry and, simply, I was confused. A primary source of my confusion was Chandra’s comments about how GR is ‘indistinguishable’ from a geometric view point. If the two are indistinguishable then why have a preference? Why not use the tool for the job that gets the job done? After all, who really cares? Afterall, this isn’t philosophy…this is physics borne out of a history that includes Bethe’s calculations… meaning always have a calculation!

Well, it’s probably obvious that I have a dog in this fight…. and I suppose I do.

33. **Thomas Larsson**
March 4, 2007

IIRC, Schrödinger was very excited about torsion in the 1940s, thinking that it was a means to reconcile gravity with QM. Einstein, having thought about and dismissed torsion 25 years earlier, made him change his mind. There was a paper about this on the arxiv a number (5-7) of years ago.

34. **Benni**
March 4, 2007

Lubos has now a discussion on Weinberg vs Hehl.
http://motls.blogspot.com/2007/03/steven-weinberg-vs-weird-physicists.html

He calls The editor of “Annalen der Physik” and former chief of the workgroup “gravitational physics” in germany a “weird physicist” because Hehl’s theories would not come close to experiment….

(german gravitational physics was rather good, because not completely devoted to fashions like strings. In Germany, traditionally many approaches are considered. I do not like it, when our (small) gravitational physics is malighed this way. Hehls theories are not more weird and disconnected from experiment than string theory is. People like Lubos would be called as ill-suited here because Lubos follows standard-fashion, not able to create own ideas.

I do not know, If someone should bring this to attention of the professional association of gravitational physics in germany:
http://www.zarm.uni-bremen.de/GR/organisation.htm
maybe someone has the time to send an email to the responsible persons:
http://www.zarm.uni-bremen.de/GR/kontakt.htm

35. **woit**
March 4, 2007

Benni,

I don’t think people should be contacting the (large number) of targets of Lubos’s unprofessional behavior. If people see a problem with it, and don’t want to do what is generally considered to be the sensible thing (ignore him), it would make more sense to contact those of his colleagues who have promoted him, and now appear to tolerate if not encourage his behavior.

36. **Benni**
March 4, 2007

Yes. But this must be done by german gravitational physicists. That is, one must first inform the GRE group of the German DPG about Lubos, so that Ehlers, Hehl, Kiefer, Nicolai and Fraundiener can take the appropriate actions.

The point is:
German theoretical physics suffers since the second world war when Hitler had forbidden jewish physics and relativity.
Due to the speculative nature and risky business of this research, government was and is very sceptical of it and would to this day not create any new professorships studying relativity. In Germany, there are (without the AEI) only around five relativity groups. And this groups are generally under the pressure of a majority of experimenters who think that they simply do not need this research which has no connections to any sort of experiments.

So, it is very bad, if the small theory group in germany is maligned from someone outside.

Since the only reason to install new theoretical professorships for german government would be the pressure, that germany will lose important competence to the US. (This is the reason why they agreed to fund some strin projects recently).

If a Harvard Professor calls a german gravitational physicist as "weird" it could have consequences for the small group in Germany.

37. **woit**
March 4, 2007

Benni,

I doubt that scientific policy decision-makers in the German government are reading Lubos's blog, so presumably would be unaware of his nonsense about Hehl, unless you bring it their attention.

If they are reading Lubos's blog and taking it seriously, German science has a lot worse problems to worry about….

38. **Benni**
March 4, 2007

Of course, policy makers are not reading Lubos blog.

But maybe experimentalists, who are curious about theoretical physics and want a good reason to set an experimentalist on the chair when Hehl becomes emeritus.

Or other members of the Harvard group can read this, who are then, after some look at the publications of Hehl, finding out, how strange Germans theoretical physics is (not string theoretic).

In fact, German policy makers often get their referees from foreign countries. In this situations, such entries of Lubos blog do not help when one wants to enlarge a research group.

39. **gunpowder&noodles**
March 4, 2007

Benni: don't worry, LM will soon be leaving Harvard, because he is being persecuted by left-wing academics on the grounds that he thinks that GR is nothing but perturbation theory around Minkowski space. He also thinks that topologically non-trivial spacetimes are possible but they, too, are perturbations around Minkowski space, and this too is anathema among leftist academics. In short, LM's 1920s style understanding of GR is politically unacceptable in Cambridge, so he has to leave and that will solve your problem.

40. **Benni**
March 5, 2007

Haha,
Well, I know at least one Gravitational physics group in germany (the one of Dehnen in Konstanz) which was destroid because Dehnens chair was after his retirement not taken again.
This chair was stopped because a majority of experimentalists (solid state physicists and optical physicists who thought that theoretical research on cosmology and general relativity is not worth doing.

Well, and this here might have been a reason too:
(I've wondered everytime who the referee of the Bogdanoff Hoax paper in class quant grav was. In his Math-Zentralblatt review of the Bogdanoff paper, it seems clearly that Heinz Dehnen did not get their joke:
http://zmath.impa.br/cgi-bin/zmen/ZMATH/en/quick.html?first=1&maxdocs=3&type=pdf&rv=Heinz+Dehnen&format=complete

41. **Benni**
March 5, 2007

An interesting thing to note might be, that Hehl, whom Lubos calls a weird physicist has much more publications in prestigious journals than Lubos and even very much more citations!

and well, it seems that Lubos fears consequences. He writes now on his blog (I wonder what this is about. In every case, the german GR association should be informed about Lubos):

Lubos wrote:
There's just far too much organized influence terrorizing people in science. Whenever your results or conclusions of your work disagree with a sufficiently large group of ignorants, they will attack you personally in the worst possible ways. They will present the fact that your results reject their preconceptions as your moral flaw.

I am looking forward to be away from the focus of these intellectual bottom-feeders who exist not only on Not Even Wrong and who enjoy a silent approval by many of the leftist officials in the Academia.

42. **Peter Woit**
March 5, 2007

Benni,

Please, enough about Lubos. People who want to read his rantings can go to his blog. He generates a vast amount of this kind of stuff, devoting attention to it would overwhelm any sort of intelligent discussion. Enough.

43. **King Ray**
March 6, 2007

Peter, et al, thanks for posting on this topic. I found many of the links and comments very interesting.

Semi-precise Predictions

March 4, 2007
Categories: Uncategorized

Many string theorists seem to have decided to react to the criticisms of string theory that have recently been getting a lot of attention by going to the press with claims to have experimental tests of string theory that can be performed in the very near future.

The latest of these claims has nothing at all to do with the aspect of string theory that has come under criticism, its failure as a unified theory of gravity and particle physics, but instead involves the conjectural use of string theory as an approximation method in QCD. The main problem with this idea so far is that it involves not QCD but a related theory (N=4 supersymmetric Yang-Mills), and it is very unclear exactly what the relation is between the calculation and the real world. For some earlier comments about this, see here. John Baez also has a summary, and the Backreaction blog of Sabine Hossenfelder and Stefan Scherer has a very extensive explanation here.

Last week the AIP Physics News site carried a story entitled String Theory Explains RHIC Jet Suppression, which dealt with recent work by Hong Liu, Krishna Rajagopal and Urs Wiedemann concerning the jet quenching parameter, which describes how charmed quarks move through a quark-gluon plasma. In the AIP story, Rajagopal claims that their calculation "agrees closely with the experimentally observed value", and that other related calculations "make a specific testable prediction using string theory." This story was picked up by Scientific American, which has a story by JR Minkel. According to Scientific American, "trying to fit the QCD-like theory to reality makes the results only semi-precise, Rajagopal says," alluding to the problem of doing the calculation in the wrong theory. Maldacena is quoted about this as follows:

It's like saying you are trying to study water, but instead you are studying alcohol… We certainly know it's not the correct theory, but maybe it behaves in the same way.

Theorist Ulrich Heinz is even more skeptical:

Even if any of the numbers worked out by accident, I don't think it would validate the approach… If they predict the color of an apple, and somebody looks at a pear and finds it has the same color, would you say that the prediction was correct?

Other recent claims by Shiu et al. and Distler et al. to be able to make predictions using the string theory approach to unifying physics are covered in the latest issue of Plus magazine, in a story entitled Stringent Tests. According to this story

It seems that string theory, so far the strongest contender for a physical "theory of everything", may soon be put to the test for the first time. Two separate teams of physicists have just published work describing how to compare the theory's predictions with reality….

Neither of the two new tests will be capable of verifying string theory once and for all. If the results concur with its predictions, then this is just some further evidence for its correctness, not absolute proof. But the tests' ability to falsify string theory, or at least certain aspects of it, means that a philosophical barrier has been overcome.

On a somewhat related note, I'll soon be traveling to Italy, giving talks in Rome and Pisa on the topic of "Is String Theory Testable?".

Comments

1. **Kea**
   March 4, 2007

   The only way to sort out this mess is to REALLY figure out QCD. Why don't we spend our time doing that instead of playing cowboys?

2. **kramnik**
   March 4, 2007

   Fancy new string-theory-inspired methods to calculate amplitudes in supersymmetric theories do have relevance to the real world. For one thing, purely gluonic QCD tree amplitudes actually coincide with the analogous ones in susy theories. At loop level, one can add appropriately chosen susy amplitudes in a clever way and end up with a qcd amplitude.

   (i'm not for a minute defending the ridiculous pseudo-predictions you've reported on recently, Peter)

3. **Peter Woit**
   March 4, 2007

   There's no denying that string theory has inspired a lot of important ideas, both in math and in physics. The field would be a lot healthier if people would just admit that trying to get a TOE out of string theory doesn't work, and if they really want to keep doing string theory, instead focus on trying to better understand those parts of the theory that might lead to something interesting. Trying to better understand QCD via AdS/CFT is a perfectly reasonable thing to be doing, although over-enthusiastic claims about where that project is don't help matters. And it would be nice if there were more encouragement for people trying to understand QCD or other non-perturbative QFTs using non-string theory methods.

4. **A String Theorist**
   March 4, 2007

   "The field would be a lot healthier if people would just admit that trying to get a TOE out of string theory doesn't work."

   It depends on your definition of "TOE".

   If by "everything" you mean something like "the Standard Model, along with whatever (if anything) supplements it at the next 10 orders of magnitude (or so) in energy", then I think that the vast majority of praciticing string theorists (it's hard to think of any exceptions) would happily admit this.

   However, many string theorists have a broader definition of "everything", roughly something along the lines of "the set of all possible consistent quantum theories of gravity".

5. **Peter Woit**
   March 4, 2007

   A String Theorist,

Interesting to hear that almost all string theorists have given up on getting particle physics out of string theory. Maybe you're right, most of the ones I talk to are certainly pretty pessimistic about it, although not necessarily willing to give up on the idea.

Besides the usual arguments about whether string theory has actually produced a "consistent quantum theory of gravity", I'm not sure I see the point of a research program devoted to generating "the set of all possible consistent quantum theories of gravity" based on the idea that they're all extremely complicated, ugly and untestable. This will certainly keep one busy for a long time, but why do it?

6. **Peter Orland**
   March 4, 2007

   Kea,

   There is a very tiny group of people who still try to figure out QCD. Unfortunately, it's a tough problem. The best we can do now is try to solve related models which we hope are relevant, much as the string theorists do. Whose models are better is in the eye of the beholder, and will
   continue to be so until QCD is finally understood.

7. **Thomas Larsson**
   March 5, 2007

   String theory is testable, even
   supertestable 🙂

8. **Q**
   March 5, 2007

   If a theory predicts something, that only validates the theory if there is evidence the theory is the only one possible. Ptolemy's epicycles-based mathematical model of the universe allowed predictions of where the planets would be at any time. As a spin-off, it also led to new mathematical methods. It's practitioners were extremely enthusiastic about it being the final theory of everything, so they tried to block all nonsense of alternative ideas without investigating them (one turned out to be more predictive!).

   Falsifiability isn't the key. Newton's theory failed to predict the precession of the perhelion of Mercury, but that didn't lead to dumping the theory. Today the Pioneer anomaly doesn't falsify general relativity. An error in prediction is just an anomaly. This would be true for string theory if it made a predictive error. The real problem for string theory is that it isn't based on an facts.

   Time and again the scientific method in physics has been to

   collect data
   summarise the data by empirical maths
   check the maths by predictive extrapolations
   come up with a theory for the maths that predicts more
   check it experimentally/observationally

   The basis of M-theory is religious belief in spin-2 gravitons, planck scale unification, and the 6/7 extra-dimensions which are required to make string explain these speculations. It's not factual physics.

9. **Thomas Love**
   March 5, 2007

   Peter: You mentioned your trip to Italy which reminded me of a famous Italian story about strings: Pinocchio! In the Disney version, Pinocchio sings: "I've got no strings to hold me down…"

10. **Eric Mayes**
    March 5, 2007

    The best way to test string theory is to construct a realistic MSSM model derived from string theory that can be tested by LHC. In this regard, please see our upcoming paper (Chen, Li, Mayes, Nanopoulos).

11. **thomas**
    March 5, 2007

    This post conflates a number of questions that are
    really quite distinct:

    1) Can string theory predictions for physics beyond the
    standard model be tested? (Do such predictions exist?)

    2) Can string dualities (like AdS/CFT) be tested?

    3) Are AdS/CFT results useful for QCD phenomenology?

    I would argue that the answers are:

    1) Maybe. A definitive test in the near future would
    certainly seem to require a lucky break.

    2) Absolutely.

    3) Yes. I agree with you that "semi-precise" in is an
    unfortunate choice of words. At present, this is clearly
    a qualitative rather than a quantitative exercise.

12. **A String Theorist**
    March 5, 2007

"I'm not sure I see the point of a research program devoted to generating "the set of all possible consistent quantum theories of gravity" based on the idea that they're all extremely complicated, ugly and untestable. This will certainly keep one busy for a long time, but why do it?"

They are not *all* extremely complicated, ugly and untestable. Some of them, such as for example topological string theories or maximally supersymmetric ones, can be quite simple and beautiful. For the same reasons, these are also the most well-studied ("under the lamppost").

As far as testability, there is absolutely no reason to expect any observational consequences of quantum gravity below 10^19 GeV. We might get luck, of course (large extra dimensions, braneworld, cosmological signatures, etc…)

13. **Peter Woit**
March 5, 2007

Thomas,

I think some of the people promoting this work to the press would actually like to see some of these issues conflated in the public mind…

You're also conflating different things, by using the term "test" in quite different contexts.

I'll agree with you about 2), duality conjectures like AdS/CFT coming from string theory can be tested. Whenever you manage to compute things that correspond on either side of the duality, whether they match is a test of the conjecture. But this is something completely different from an experimental test of a theory. You're testing the conjecture that two theories are related, not testing whether a theory has anything to do with the real world. If you go around telling people "string theory is testable", meaning that you have tests concerning whether a certain string theory is related to QCD, you are being misleading.

About 1), that's what I'm writing a talk about. I'd claim that the conventional understanding of the phrase "test a theory" means to use distinctive features of the theory to make falsifiable predictions about what you will see if you do a specific experiment, acknowledging the theory is wrong if you don't see what you predicted. By this definition, string theory is not testable. Attempts to use different definitions of "testable" again sometimes seem to be motivated by a desire to obscure this point.

As for 3), sure, this is an uncontrolled approximation, maybe it's qualitatively useful. That's fine and an interesting thing to work on, but people should avoid misleading the public about what is going on.

14. **A String Theorist**
March 5, 2007

Oops, I clicked submit again before I was finished:

We might get lucky, but this fact has been clear for decades — it's not as if there has been some massive conspiracy on the part of string theorists to cover up elementary dimensional analyisis.

"This will certainly keep one busy for a long time, but why do it?"

Different people will give you different answers, but fundamentally the reason people study string theory is that it interests them. Some people study the distribution of prime numbers, others study pre-Columbian musical rituals. Most people find both of those topics, as well as string theory, to be uninteresting wastes of time. But they don't start blogs, or write books, about their prejudices.

15. **A Non-Scientist Observer**
March 5, 2007

The March 5 observation by AST misses the point. Of course, many people study many things for their own sake. The difference is that they do not make extraordinary claims about what they are doing. No one studying prime numbers has asserted that they are on the verge of explaining huge swathes of the natural world, at least, not since Pythagoras. If string theorists said "Hey, this is just esoteric math stuff that is fun" there would be no real issue. In fact, it appears that many make profound claims, and other string theorists implicitly endorse those claims by not opposing them.

16. **Thomas Larsson**
March 5, 2007

However, many string theorists have a broader definition of "everything", roughly something along the lines of "the set of all possible consistent quantum theories of gravity".

Some days ago, I stumbled across the Wikipedia entry about [Paul Ehrenfest](). I learned a number of things, e.g. that Ehrenfest's suicide probably had something to do with his youngest son having Down's syndrome. What really caught my eye, however, was the title of his 1912 inaugural lecture: "About the crises of the light-ether hypothesis". My point is that in 1912, i.e.

* 25 years after the [Michelson-Morley]() experiment,

* 12 years after Planck's formula for blackbody radiation,

* 7 years after Einstein's explanation of the photo-electric effect and discovery of special relativity,

mainstream physicists realized that there was a crisis in ether theory.

How could an obviously wrong idea keep its hold for such a long time? Clearly because most physicists considered it as "the set of all possible consistent theories of light".

It seems to me that string theory is following the trail of ether theory, exactly one century behind.

17. **Bee**
March 5, 2007

Hi Peter,

thanks for the link.

Trying to better understand QCD via AdS/CFT is a perfectly reasonable thing to be doing, although over-enthusiastic claims about where that project is don't help matters.

I think the sentence you quoted: ‘make a specific testable prediction using string theory’ has been very carefully formulated. Krishna Rajagopal gave a colloquium here at PI some weeks ago about his calculations, and he has definitely not made any over-enthusiastic claims. For me it looks like this: there is a model that has turned out to agree with some observables. Now people are using it to make actual predictions to see how far its range of application extends. Whether or not one can justify *why* this is (or isn't) a useful approximation (in certain regions of the phase diagram) is a different question. For sure one would like to understand that better.

In case somebody is interested, there's video, audio and slides at the PI websites

[Probing the Properties of Quark-Gluon Plasma]

(I hope the link works, they keep moving the item IDs which drives me nuts. In case the link is broken, search the seminar series by speaker.)

What concerns me most about the AdS/QCD hype is that suddenly so many people jump on the topic. For one, even if it turns out to be the greatest model for heavy ion collisions ever, there will be only limited demand on people working on it. But what's worse is that it means all these people have previously worked on something so fascinating that they are willing to drop it from one day to the next.

Best,

B.

18. **Peter Woit**
March 5, 2007

Bee,

Thanks for the explanation and link to Rajagopal's talk.

A String Theorist,

The problem is that the non-ugly string theory backgrounds provably don't relate to the real world. If you want to amuse yourself by studying string theory backgrounds that you know can't ever be related to the real world, purely for your intellectual entertainment, that's fine, but you should be honest about it, and then compete for resources with lots of other people who have their own arcane intellectual interests. Somehow I've never seen this description of string theory research on a grant proposal or research statement from someone trying to get a job.

Some of these things (especially topological strings) do involve some wonderful mathematical structures, and they're under active research by many people in the mathematics community. If this is what you want to do, great. I do suggest though that you might want to try and get a job in a math department. You'd find there some very smart people who know a lot about what is already known about the relevant mathematical structures, and an active, healthy research field.

19. **Benni**
March 5, 2007

Your talk in Pisa is somewhat funny:
https://indico.pi.infn.it/categoryDisplay.py?categId=24

March:
29 Raphael Bousso (Berkeley), Predictions in the Landscape
20 Paolo Gambino TBA
19 – 21 Supersymmetry, Supergravity, Superstrings
15 Peter Woit Is String Theory Testable?

First you, and then, when the audience has learned something, comes Bousso!

It is important, that you traven to europe. Since some departments think of installing LARGE string departments.
It would be important that you say, string theory should be installed, but only with small groups, for investigation of QCD or purely mathematical physics and under all circumstances, not as the only game in town for fundamental particle physics.

Otherwise it is the danger, that Europe makes the same mistakes as theorists did in the US.

20. **grant**
March 5, 2007

“Somehow I've never seen this description of string theory research on a grant proposal or research statement from someone trying to get a job.”

How often do you think it happens that people who study pre-columbian music rituals or prime number distributions write grant applications reading “my reseach is completely esoteric and without practical applications, focusing on some details of a field which most people consider to be an uninteresting waste of time”?

21. **Peter Woit**
March 5, 2007

grant,

Obviously my comment was a bit sarcastic. In grant or job applications, people try and make the case for the widest possible significance of their work. But the honest ones don't write things that they privately acknowledge to be untrue. Claiming that what you are doing is going to lead to predictions about beyond the standard model physics when you know very well this is not possible is not honest.

Look, I know people who study prime number distributions, and have seen their grant and job applications. These documents make a case for studying prime number distributions for their intrinsic interest and give legitimate arguments about the significance of this kind of research for the rest of mathematics.

22. **puzzled**
March 5, 2007

Peter, this is hilarious! Are you seriously saying that you will give talks on a subject on which you've never ever worked in your entire life? Writing a blog about it is fine because one can blog just about anything but giving a talk at a university??? And you complain about the lack of professional attitudes on the part of certain string theorists?

23. **Peter Woit**
March 5, 2007

puzzled,

If you're unhappy with the concept of my giving these talks, take it up with the physicists who invited me to do so, I can assure you that the talks were not my idea. Quite a few physicists read this blog, and many of them seem to find what I have to say well-informed and worth listening to. Also, I hate to break this to you, but it's not the first time I've given a public talk about this at a university.

As a relevant example, I don't see why only people who have written papers on the landscape should be giving talks about it and the problems it raises. It might be a good idea to hear from people who have experience as Ph.Ds and postdocs in particle theory, have spent a lot of time learning about the issue and debating it with others, coming to the conclusion that "working" on the landscape by writing papers about it is not a good idea.

And, you know, it's really tiresome how many string theorists seem to be unwilling to put their name publicly to their critical comments here. That is something I find highly unprofessional.

24. **Peter Woit**
March 5, 2007

Eric,

Sorry your comment initially got caught in my spam filter for some unknown reason.

If the LHC sees super-partners, how can you tell they came from a string theory.?In particular, what values of the MSSM parameter space are predicted by string theory, or does string theory say nothing about this?

25. **A String Theorist**
March 6, 2007

A propos your suggestion that string theorists should look for jobs in math departments:

Of course as you know several string theorists have gone to math departments. I think this movement will pick up steam in the next few years. In fact, this year is the first year that (barely) more than half of the "string theory" jobs listed on the rumor page are actually in math departments.

26. **Gina**
March 7, 2007

It is very nice to see the overall moderate tone of the post and remarks. I was impressed by Peter's sentence "There's no denying that string theory has inspired a lot of important ideas, both in math and in physics". Recognizing this while playing a role of "devil's advocate" regarding string theory is perfectly fine.

Attempts to make predictions via string theory should be welcomed by everybody. As we often try to make predictions on humans based on mice, predicting the behavior of water based on alcohol and of apples based on pears is reasonable.

27. **JUAN R.**
March 7, 2007

A String Theorist Said:

> It depends on your definition of "TOE".
>
> If by "everything" you mean something like "the Standard Model, along with whatever (if anything) supplements it at the next 10 orders of magnitude (or so) in energy", then I think that the vast majority of praciticing string theorists (it's hard to think of any exceptions) would happily admit this.
>
> However, many string theorists have a broader definition of "everything", roughly something along the lines of "the set of all possible consistent quantum theories of gravity".

When String Theory was initially promoted as "the only serious candidate to TOE", string theorists defined "everything" like *everything* (not just extension of SM or a quantum gravity theory) in papers, lectures, and books.

String Theory was promoted in that way to general public also.

> For this reason string theory is sometimes described as possibly being the "theory of everything" (T.O.E.) or the "ultimate" or "final" theory. These grandiose descriptive terms are meant to signify the deepest possible theory of physics—a theory that underlies all others, one that does not require or even allow for a deeper explanatory base.

It is fine if you want change now the meaning of "everything" in basis to the recent String Theory *fiasco* as a sensible TOE. It is fine if now the superb String Theory is now to be promoted like just some kind of QCD recipe.

It is not fine you want change history.

28. **Jonathan Vos Post**
March 7, 2007

The prime number theorist might well include this in the grant application:

My theory, with a little more work, will solve important problems in Physics. See:

"Applications of Statistical Mechanics in Prime Number Theory"
Marek Wolf

http://secamlocal.ex.ac.uk/people/staff/mrwatkin/zeta/wolfgas.htm

Wolf begins by briefly referring to the general phenomenon of the 'number theory – physics crossover'. Two references to the application of prime number theory to quantum chaology in particular are given [1,2,3]. Two mathematical properties which are normally associated with physical systems have been detected in the distribution of prime numbers by Wolf himself, namely multifractality [4] and 1/f noise [5]. A paper which numerically calculates Lyapunov exponents (familiar to chaos theorists) for the distribution of primes is mentioned [6]. Finally an article is cited [7] wherein the authors apply the Wiener-Khintchine formula, which relates spectral densities and the autocorrelation function, to the problem of the distribution of pairs of primes {p,p+2}.

29. **A String Theorist**
March 8, 2007

Juan R:

Your quote about the "theory of everything" is precisely in accord with my original post.

String theory IS a theory of everything, including many things (such as topological strings, an example I gave above) which do not actually "exist" in our universe.

But even though it is the deepest possible theory of physics, it's not going to tell you what the next winning lottery number is (any more than it will tell you what the mass of the electron is).

30. **Q**
March 8, 2007

No, it's not a theory because it doesn't explain anything about real phenomena. It links gravitons nobody has ever seen to Planck scale unification nobody can see, using extra-dimensions that have to be explained away by a Calabi-Yau manifold which gives the string a massive landscape of 10^500 solutions of particle physics.

Explaining (by further speculation) a few speculations about gravitons and unification isn't a theory of everything. It's not even a 'theory' about speculations, it's just vague hype that isn't tied down to any known facts. The claim it's the "deepest possible theory of physics" suggests you have a disproof of LQG and every suggested alternative. Where are these disproofs?

Kap

March 5, 2007
Categories: Uncategorized

There's a short memoir out this evening on the arXiv by Peter Freund about the algebraist Irving Kaplansky, universally known as "Kap", who died last year at the age of 89. Freund worked with Kaplansky at Chicago during the mid-seventies on the classification of Lie superalgebras, and comments about the relations between math and physics at that time. He also claims that Kaplansky told him with assurance that Hopf algebras couldn't possibly be of relevance to physics, and that Andre Weil was the greatest living mathematician, since he called all the courses he taught "mathematics", and lived up to this title. Freund also tells a well-known story about a talk by Weil at Chicago that he heard about first-hand from Kaplansky.

Freund's piece is based on his talk at the recent memorial event held for Kaplansky in Berkeley at MSRI. His student Hyman Bass has a wonderful presentation describing Kaplansky's life and work.

I have my own personal recollections of Kap since he was director of MSRI the year that I was a postdoc there (1988-9), a year during which I unfortunately had only a few short conversations with him. The conversation with him I remember best was our first, which occurred on the phone. In early 1988 I was working part-time teaching Calculus at Tufts and had applied for several full-time mathematics jobs for the next year, not at all sure that there was any chance this would work out, since my Ph.D. and first postdoc had been in physics. When the call came from Kap offering me a job at MSRI for the next year I was elated, partly because Berkeley is one of my favorite places to live as well as being an excellent place to work in both mathematics and physics. During our conversation Kap told me a bit about his work on and continued interest in supersymmetry. I didn't really have the heart to tell him that because my own inclinations are so geometric, I'd always found supersymmetry and superalgebras a tantalizing but very frustrating subject.

No Comments

Some Quick Links

March 7, 2007
Categories: Uncategorized

John Horgan keeps moving his blogging activities to different locations, the latest one is here.

For a truly sad and distressing story about what has happened to Billy Cottrell (who was mentioned snarkily by me long ago here), see this LA Weekly article and a Clifford Johnson posting. The abuse of people going on in this country associated with labeling them "terrorists" is just appalling and deeply shameful.

More from Steinn Sigurdsson on Yarn Theory.

Cern Courier reports on the recent Axion workshop at Princeton and the 2006 Quark Matter conference in Shanghai.

Lee Smolin's book is out in the UK, here's a review from the Financial Times.

The latest London Mathematical Society newsletter has a review of Not Even Wrong.

David Ben-Zvi was here last week and gave a wonderful colloquium talk on Langlands duality, loop spaces and representations of real groups. Much of was somewhat general philosophy about a new way of thinking about these topics, and this was quite compelling, although I need to find a sizable chunk of time to sit down and really understand what he is doing. If I can do this, maybe I'll then take a stab at trying to explain this here. He did convince me that one needs to think not only about stacks, but derived stacks. There's a long foundational document about this by Jacob Lurie (see here), something more readable from Bertrand Toen.

This semester Edward Frenkel is running a seminar on Topics in the geometric Langlands program. The slides of a talk by Ben Webster that are there are wonderful. One problem with this field has always been how unreadable much of the material about is. People like Webster and Ben-Zvi are starting to do a great job of explaining what is going on in a form that others have some chance of following.

Later this month the IAS will have a conference related to this topic, next year a whole program.

I'll be travelling most of the next week and a half or so, so blogging will be light to non-existent. Behave.

Comments

1. **LDM**
   March 7, 2007

   It is simply heartbreaking to hear of someone being denied their physics (and other) books and writing materials, being denied access to the kind of work where their skills might best be put to use – Clifford
   What rubbish.
   You break the law. You go to jail. You loose freedoms. Anybody who can understand string theory can undoubtledly understand this.
   However, the terrorist charge, based on the "evidence" in the links, looks wrong…yes, therefore the sentence should be reduced accordingly.

2. **Choo**
   March 8, 2007

   Uh. Yes, you lose freedoms. But if you read the article, this is not what they are complaining about. They object to the mischaracterization and voodoo fear of Billy's science training, taking his papers away as a "fire hazard" and inconsistent treatment on account of his "terrorism" label. Now, I actually lean towards calling him at least some sort of an economic terrorist, but not if it encourages this kind of result.

3. **yeti**
   March 8, 2007

   The notion that Billy Cottrell should have gotten leniency because of his intellect and talent is offensive.

   The notion that someone should get 7-8 years in jail for what he did is also offensive.

   The notion expressed in the LAWeekly article that ELF does not exist is ridiculous. Maybe it has no clear organizational structure, but the folks are out there. Go to Oregon.

4. **D R Lunsford**
   March 8, 2007

   Peter – bon voyage – I promise to behave 🙂

5. **amused**
   March 8, 2007

   "I'll be travelling… Behave."

   An incitement to wreak havok if ever there was one 🙂
   Proposals for discussion topics in Peter's absence:
   — Einstein, Dirac and Feynman were all wrong
   — discussion of papers banned from arxiv
   — vilification of prominent string theorists
   — psychoanalysis of Lubos
   — mysticism in physics
   — supernatural phenomena (have you seen ghosts?)

6. **Chris Oakley**
   March 8, 2007

Re: Billy Cottrell, I think that the authorities have got it right. If only one could use this “fire hazard” excuse to stop people working on String Theory at universities.

7. **joseph smidt**
March 9, 2007

It’s unfortunate what this country will automatically do to people if you are ever labeled a terrorist.

This kid deserves to be behind bars but it is terrible he is treated so differently just because he is smart and currently labeled as a terrorist. Also, if he could be productive with books why not give him some. We are we against prisoners making the world, or at least a small corner of it, a better place?

8. **Ralf**
March 9, 2007

Methinks, Tony Sudbery is no Alain Connes.

9. **relativist**
March 9, 2007

Seeing this section is labelled `quicklinks’, and given the broad interests of this blog, I think it is appropriate to point out new work by one of the best quantum theorists around, just placed on the arXiv, as follows:

quant-ph/0703060:
Title: A Topos Foundation for Theories of Physics: I. Formal Languages for Physics
Authors: A. Doering, C.J. Isham

This paper is the first in a series whose goal is to develop a fundamentally new way of constructing theories of physics. The
motivation comes from a desire to address certain deep issues that arise when contemplating quantum theories of space and time. Our basic contention is that constructing a theory of physics is equivalent to finding a representation in a topos of a certain formal language that is attached to the system. Classical physics arises when the topos is the category of sets. Other types of theory employ a different topos. In this paper we discuss two different types of language that can be attached to a system, S. The first is a propositional language, PL(S); the second is a higher-order, typed language L(S). Both languages provide deductive systems with an intuitionistic logic. The reason for introducing PL(S) is that, as shown in paper II of the series, it is the easiest way of understanding, and expanding on, the earlier work on topos theory and quantum physics. However, the main thrust of our programme utilises the more powerful language L(S) and its representation in an appropriate topos.

quant-ph/0703062
Title: A Topos Foundation for Theories of Physics: II. Daseinisation and the Liberation of Quantum Theory
Authors: A. Doering, C.J. Isham

This paper is the second in a series whose goal is to develop a fundamentally new way of constructing theories of physics. In this paper, we study in depth the topos representation of the propositional language, PL(S), for the case of quantum theory. In doing so, we make a direct link with, and clarify, the earlier work on applying topos theory to quantum physics. The key step is a process we term `daseinisation’ by which a projection operator is mapped to a sub-object of the spectral presheaf — the topos quantum analogue of a classical state space. In the second part of the paper we change gear with the introduction of the more sophisticated local language L(S). From this point forward, throughout the rest of the series of papers, our attention will be devoted almost entirely to this language. In the present paper, we use L(S) to study `truth objects’ in the topos. These are objects in the topos that play the role of states: a necessary development as the spectral presheaf has no global elements, and hence there are no microstates in the sense of classical physics. Truth objects therefore play a crucial role in our formalism.

quant-ph/0703064
Title: A Topos Foundation for Theories of Physics: III. The
Representation of Physical Quantities With Arrows
Authors: A. Doering, C.J. Isham

This paper is the third in a series whose goal is to develop a fundamentally new way of viewing theories of physics. In paper II, we studied the topos representations of the propositional language PL(S) for the case of quantum theory, and in the present paper we do the same thing for the, more extensive, local language L(S). One of the main achievements is to find a topos representation for self-adjoint operators. This involves showing that, for any physical quantity A, there is an arrow $\breve{\delta}^o(A):\Sig\map\SR$, where $\SR$ is the quantity-value object for this theory. The construction of $\breve{\delta}^o(A)$ is an extension of the daseinisation of projection operators that was discussed in paper II. The object $\SR$ is a monoid-object only in the topos, τ_ϕ, of the theory, and to enhance the applicability of the formalism, we apply to $\SR$ a topos analogue of the Grothendieck extension of a monoid to a group. The resulting object, $\kSR$, is an abelian group-object in τ_ϕ. We also discuss another candidate, $\PR{\mathR}$, for the quantity-value object. In this presheaf, both inner and outer daseinisation are used in a symmetric way. Finally, there is a brief discussion of the role of unitary operators in the quantum topos scheme.

quant-ph/0703066:
Title: A Topos Foundation for Theories of Physics: IV. Categories of
Systems
Authors: A. Doering, C.J. Isham

This paper is the fourth in a series whose goal is to develop a fundamentally new way of building theories of physics. The previous papers in this series are concerned with implementing this programme for a single system. In the present paper, we turn to considering a collection of systems: in particular, we are interested in the relation between the topos representation for a composite system, and the representations for its constituents. We also study this problem for the disjoint sum of two systems. Our approach to these matters is to construct a category of systems and to find a topos representation of the entire category.

10. **Vogelsang**
March 9, 2007

>new work by one of the best quantum theorists around,

Relativist, just out of curiosity, are you Doering or Isham?

11. **Peter Orland**
March 9, 2007

Fellow scientists,

Let’s maintain our sense of shame. No abstracts on Peter Woit’s
blog please! That’s what archive is for.

12. **Who**
March 10, 2007

The UK paperback edition of Peter's book is scheduled to come out 7 June 2007.
http://www.amazon.co.uk/Not-Even-Wrong-Continuing-Challenge/dp/0099488647/ref=pd_sbs_b_2/026-3303388-4063638

The publisher is Vintage. Evidently people are already advance ordering because the UK amazon sales rank (among all books) is currently #6,726.

The US paperback edition is scheduled to come out in September:
http://www.amazon.co.uk/Not-Even-Wrong-Failure-Physical/dp/0465092764/ref=ed_oe_p/026-3303388-4063638

Currently Smolin's book is #1 on the UK amazon physics bestseller list and Peter's book (hardcover) is #7
http://www.amazon.co.uk/gp/bestsellers/books/278409/ref=pd_ts_b_nav/026-8303087-5718006

Both are doing rather well, I'd say, judging by their sales ranking among all books. For example Smolin's book is currently #49
among all books that UK amazon sells, as can be seen here:
http://www.amazon.co.uk/Trouble-Physics-String-Theory-Science/dp/0713997990/ref=pd_ts_b_1/026-3303388-4063638?ie=UTF8&s=books
That is pretty astonishing for a physics book, because it is #49 in competition with popular genre—detective, cookbooks, selfhelp, humor, diet, fantasy, Harry Potter, and so forth.

13. **yagwara**
March 15, 2007

Off topic, but I just had to share this someplace:

http://www.tenthdimension.com/

What makes this so weird is that it is a crackpot site, but it presents no red flags (at least to me). It is a beautifully designed site (no CAPITAL LETTERS IN DIFFERENT COLOURS) and the animation is lovely. Moreover, it starts out completely sane in dimensions 1-4. Then it turns to complete garbage.

I've sometimes seen real physicists' websites that look like crackpottery at first glance, but this is the first time I've seen crackpottery that looks totally professional at first glance.

(Although in retrospect, the inclusion of a category for "songs" should have given me pause.)

14. **Ptolemy**
March 15, 2007

yagwara: he seems unaware that 10 dimensional superstrings are composed of a worldsheet of 1 time dimension and 1 spatial dimension, plus 8 other dimensions to include supersymmetric particle physics, 6 of which are accounted for the the Calabi-Yau manifold.

But if you examine anthropology objectively, you find the coincidence that we have 10 fingers (and also 10 toes) which is the same number as the number of superstring dimensions! Anthropic ideas are currently used (in default of anything else) in string theory to try to select the standard model from the landscape of 10^500 models string theory includes.

The anthropic principle implies that the reason for particle physics being the way it is, is due to the fact that we are around to see it. Anthropically, we have ten fingers and ten toes, and most commonly we use base ten. So, we have evolved with as many fingers/toes as there are supersymmetric dimensions. Coincidence?

Remember, as a great Harvard assistant professor once wrote, 'God wrote the world in the language of superstring theory'.

Thus, it's logical that God made the superstring from 10 dimensions, so that people can use their fingers to count them.

As for the eleven dimensional supergravity bulk in M-theory, can it be a coincidence that he square root of 137 is merely 6% higher than the number eleven?

But the anthropological fact we have 10 fingers is quite irrefutable. So it is pretty water-tight evidence for string theory being on the right track. In addition, because 2 of those fingers are really thumbs, we have 2 thumbs plus 8 other fingers. This is exactly like the 2 dimensional worldsheet, plus 8 added dimensions for conformal symmetry of particle physics. Don't try to tell me this is coincidence. It first suggests that the anthropic principle is valid (the world is the way it is because people exist!), plus it indicates that there are hidden clues in evolution which tell us about string theory.

15. **LDM**
March 15, 2007

yagwara,

Red flags are obvious, although it is a beautiful flash application.

In part 5, he mentions part five "waves of probability" from Quantum Physics.
There are no waves of probability in Quantum Physics.

16. **yagwara**
March 15, 2007

LDM,

Oh yes, certainly, if you get to section 5 you're already waist deep in crazy. I just meant there were no red flags at first glance.

Usually you can look at a website from across a room and tell in an instant if it is a crackpot site.

17. **Robert Musil**
March 16, 2007

According to the Economist magazine the LHC may lose out on the discovery of the Higgs (http://www.economist.com/science/displaystory.cfm?story_id=E1_RRTDJRR&login=Y – subscription required):

To find [the Higgs], scientists at the European particle-physics laboratory, CERN, in Geneva, are building what will, when it starts up later this year, be the world's most powerful particle smasher. This machine, known as the Large Hadron Collider, is designed especially to look for the Higgs boson. But the Europeans may be pipped at the post by rivals working at what is the highest-energy collider working today, the Tevatron, at the Fermi National Accelerator Laboratory (Fermilab) near Chicago.

18. **Anon**
March 16, 2007

On a totally unrelated note:

http://www.math.uchicago.edu/~arinkin/langlands/Illusie.wav

is an audio clip of Luc Illusie reminiscing about Alexander Grothendieck, his work, etc.

Just thought you'd like to know.

Is String Theory Testable?

March 17, 2007
Categories: Uncategorized

I've been traveling in Italy for the past ten days, and gave talks in Rome and Pisa, on the topic "Is String Theory Testable?". The slides from my talks are here (I'll fix a few minor things about them in a few days when I'm back in New York, including adding credits to where some of the graphics were stolen from). It seemed to me that the talks went well, with fairly large audiences and good questions. In Pisa string theorist Massimo Porrati was there and made some extensive and quite reasonable comments afterwards, and this led to a bit of a discussion with some others in the audience.

I don't think the points I was making in the talk were particularly controversial. It was an attempt to explain without too much editorializing the state of the effort to connect the idea of string-based unification of gravity and particle physics with the real world. This is something that has not worked out as people had hoped and I think it is important to acknowledge this and examine the reasons for it. In one part of the talk I go over a list of the many public claims made in recent years for some sort of "experimental tests" of string theory and explain what the problems with these are.

My conclusion, as you'd expect, is that string theory is not testable in any conventional scientific use of the term. The fundamental problem is that simple versions of the string theory unification idea, the ones often sold as "beautiful", disagree with experiment for some basic reasons. Getting around these problems requires working with much more complicated versions, which have become so complicated that the framework becomes untestable as it can be made to agree with virtually anything one is likely to experimentally measure. This is a classic failure mode of a speculative framework: the rigid initial version doesn't agree with experiment, making it less rigid to avoid this kills off its predictivity.

Some string theorists refuse to acknowledge that this is what has happened and that this has been a failure. Most I think just take the point of view that the structures uncovered are so rich that they are worth continuing to investigate despite this failure, especially given the lack of successful alternative ideas about unification of particle physics and gravity. Here we get into a very different kind of argument.

It was very interesting to talk to the particle physicists in Rome and Pisa. They are facing many of the same issues as elsewhere about what sort of research directions to support, with string theory often being pursued as an almost separate subject from the rest of particle theory, leading to conflict over resources and sometimes heated debates between them and the rest of the particle physics community. Many people were curious about how things were different in the US than in Europe, but I'm afraid I couldn't enlighten them a great deal, mainly because I just don't know as much about the European situation, although I've started to learn more about this on the trip. Several wondered if the phenomenon of theorists going to the press to make overhyped claims about string theory was an American phenomenon. I hadn't really noticed this, but it does seem to be true. While the hype starts in the US, it does travel to Europe, with the US very influential in this aspect of culture as in many others. In the latest issue of the main Italian magazine about science, there's an article explaining how certain US theorists have finally figured out how to test string theory with the new LHC…

Comments

1. **Levi**
   March 17, 2007

   "The fundamental problem is that simple versions of the string theory unification idea, the ones often sold as "beautiful", disagree with experiment for some basic reasons."

   This seems similar to the situation with Grand Unified Theories. I gather that SU(5) was the "beautiful" version, and when that version ran into problems much of the beauty went out of GUTs. It's interesting to contrast this with cosmic inflation, where Guth's original version didn't quite work, but Linde and others found forms of inflation which worked better, and WMAP data gives a reality check.

   I should mention that I'm not a physicist, just a casual reader, so if I'm misinformed I hope somebody will point it out.

2. **Arun**
   March 17, 2007

   It would be nice to know what Porrati said, if at all possible.

3. **Joseph Smidt**
   March 17, 2007

   Great Post. I thought your comments on US/Europe string culture were interesting. Thanks for the slides.

4. **Irish physicist**
   March 17, 2007

   Off-topic – but congratulations on 3 years of blogging and Happy St. Patricks day too!

5. **woit**
   March 17, 2007

   Irish Physicist,

   Thanks! I hadn't realized that the blog was started on a St. Patrick's day. Surely some sort of homage to the Irish was unconsciously intended.

   Arun,

   I can't recall exactly what Porrati's points were, except that he said that he had five of them, and none of them were things that I really had a substantive disagreement with. Some of them were (from memory, and in loose translation, surely he would express these differently)

   1. String theory shouldn't be thought of as a theory that leads to a unique, predictive model, but instead as a very general framework, like QFT, valuable for the different kinds of models it allows.

   2. He mentioned the "swampland" idea, that one could try and characterize those low energy theories that come from an ultraviolet completion like string theory.

   3. His main point I think was that as long as there was no alternative way to unify particle theory with quantum gravity, string theory would continue to be a main focus

for people to pursue. Kind of the “only game in town argument”.

4. He may also have mentioned the use of string theory in heavy-ion physics, in regimes where lattice gauge theory has trouble providing results.

I guess I’m missing at least one…

6. **anon.**
March 17, 2007

Porrati’s 1st point is with all due respect exactly the argument that defended the use of epicycles by both Ptolemy and Copernicus: it seemed to be a very useful framework of ideas. (Ptolemy used epicycles in the earth-centred universe, c. 150AD. In 1543, Copernicus used epicycles in his final model of the solar system.)

As a ‘general framework of ideas’, the false theory of epicycles was invaluable to Ptolemy, Copernicus and generations of physicists. But that useful approximate framework was really false, as Kepler eventually discovered. So in the end both the earth-centred universe and its general framework of ideas were discredited. Will the string theory framework of ideas similarly mislead generations?

What is so interesting is that it seems to be disconnected from reality not just with regard to its failure to make testable predictions, but also at the input end. Instead of having solid input, everything which has been put into string theory is completely speculative. It is less testable than either of the epicycle theories, and has less solid evidence.

People now laugh at the idea that a theory was once constructed in which the stars and planets were carried around the earth while imbedded in closed crystalline shells. At least that false model was an attempt to interpret data. Perhaps people will cry with pity in the future, reading how physicists defended 10/11 dimensional M-theory in the 21st century, without providing any evidence at all.

7. **Chris W.**
March 17, 2007

The pre-Copernican astronomers could be excused on the basis of epistemological naivete; their successors largely *invented* the understanding of science that is now being invoked in discussions of string theory.

String theorists can’t be so excused. They should have known better, and should know better now. Certainly ‘general frameworks of ideas’ are important; they set the context for *formulating problems*. This is why metaphysics is important in science, even though most metaphysics ultimately proves worthless.

The questions that must be asked now with respect to quantum gravity and unification concern the problem formulation. (Shiing-Shen Chern, who discussed the matter with Einstein in the 1940s, recognized this as the essential work of the physicist.) The alternatives to string theory in quantum gravity challenge the received wisdom in this regard, and for this reason alone are important. In this context Porrati’s main point (as stated by Peter, and echoed by many of Porrati’s colleagues) strikes me as a complete crock. The string theorists who adopt this attitude are the least likely to arrive at the crucial insights into the problem. One can hope they’ll at least have the good sense and simple honesty to recognize those insights when they appear, although I’m less and less optimistic about that.

8. **Vijay Shankar**
March 17, 2007

There seems to be many differences in opinions unfortunately based on nationality. What people would want Physicists to come up with is a theory that holds in all frames or an experimental method that would help us test all the theories. Until then, we can’t stop someone crying foul whenever there is a news about ‘revolutionary’ theories.

9. **tomj**
March 17, 2007

My question is: what is the difference between no theory and one which cannot be tested?

I cannot figure out why string theory is a theory. It barely ranks as a hypothesis, and a poor one, very close to what my teenager would come up with. It is 100% mental.

A theory, at minimum, should cover all the facts known, but as Einstein once said (something like): a theory should be as simple as possible, _but_ no simpler. The implication is that there has to be a careful balance, and the theory _must_ track data. How else could the complexity of theory be measured? Yes, you can predict new facts, but first you have to account for known facts. We have to start with the abilities of the observer. And the first ability is that of objectivity, and objectivity begins with the repudiation of belief.

If a theory cannot be any simpler than necessary, how … really … how can a theory be more complex than necessary? If over simplification is a sin, complexity is beyond sin. A ‘theory’ (or set of words and math) which can ‘explain’ everything ‘after the fact’ is useless. Can someone please explain to me this: do physicists really believe that it is possible to formulate a complete description of the universe which will be testable? Because one possible reality is that we are incapable of this. We have thousands of years of data to suggest this conclusion, and only wishful thinking to suggest otherwise.

I like the name of the book, it is important to echo prior thinking. But it might have been even more valid to call it ‘Beyond Reason’. Everyone seems to think that they have reason, that they think logically. And as long as we can avoid testing our reason and logic, we can continue to ‘think’ and ‘believe’ whatever we want. And if we become dogmatic in these untested beliefs, what is this? Science is not belief. Science is experiment. And experiment is based upon question, the antithesis of belief. Science is not an answer, science is a method.

10. **Ptolemy**
March 18, 2007

‘I cannot figure out why string theory is a theory.’ – tomj

Gerard ‘t Hooft:

‘Actually, I would not even be prepared to call string theory a “theory” – rather a model or not even that: just a hunch. After all, a theory should come together with instructions on how to deal with it to identify the things one wishes to describe, in our case the elementary particles, and one should, at least in principle, be able to formulate the rules for calculating the properties of these particles, and how to make new predictions for them. Imagine that I give you a chair, while explaining that the legs are still missing, and that the seat, back and armrest will perhaps be delivered soon; whatever I did give you, can I still call it a chair?’

– http://www.math.columbia.edu/~woit/wordpress/?p=258#comment-5030

Peter Woit’s argument of why a non-predictive framework is not science can be found on p211 of Not Even Wrong (UK ed.):

‘An explanation that allows one to predict successfully in detail what will happen when one goes out and performs a feasible experiment that has never been done before is the sort of explanation that most clearly can be labelled 'scientific'. Explanations that are grounded in … systems of belief and which cannot be used to predict what will happen are the sort of thing that clearly does not deserve this label. This is also true of … wishful thinking or ideology, where the source of belief … is something other than rational thought.'

11. **r hofmann**
March 18, 2007

`Several wondered if the phenomenon of theorists going to the press to make overhyped claims about string theory was an American phenomenon. I hadn't really noticed this, but it does seem to be true. While the hype starts in the US, it does travel to Europe, with the US very influential in this aspect of culture as in many others.´

There are many examples of theoretical physicists
working in the US (foreigners and US citizens) who do extraordinarily good work but on the short run are screened by those that produce overhyped newspaper headlines. My general impression is that the US culture supports the go for extremes in generating scientific opinion, publicizing of `results´, and network formation. This may be helpful in projects where a focus of resources is needed (Cobe, WMAP,…). On the theoretical side, however, it may at times just produce entropy, a lack of well-fermented orginality, and thus no gain in robust knowledge.

12. **Stacy**
March 18, 2007

A note on inflation, inspired by Levi's comment:
Actually, the situation with Inflation is quite analogous to that with string theory. The original idea was beautiful, and made a simple prediction (the universe should be flat) which solved the coincidence problem (to do with the evolution of the density of the universe). These together were compelling and propelled the theory to the dominance it enjoys today. But it did suffer problems (like a graceful exit from inflating) which have not entirely been solved. Worse, the compelling aspect of the flatness prediction – confirmed by the WMAP satellite – was that the density parameter should be unity – all in mass – in order to solve the coincidence problem. But it isn't all in mass – we have now to invoke dark energy. This makes the coincidence problem worse.

In other words, the compelling part of Inflation that led us all to believe it not only doesn't work, but has made worse the problem it originally seemed to solve. I can't help wondering if future generations of sociologists will debate whether speculative theories like string theory and Inflation were ever distinguishable from some sort of mathematically motivated religion.

13. **matteoeo**
March 18, 2007

I agree with Stacy, I think cosmology suffers just the same problems as string theory. Cosmologists can produce potentials that would suit to any possible dynamics of inflation and produce the desired spectrum of cosmic background radiation, without actually deriving them from the properties of the known QFT particles. The worse, cosmology at the moment is a melting pot of the most un-scientifical theories and hypothesis in town: dark energy, cosmological costant, strings and GUTs (early universe), inflation, higgs boson, quintessence, supersimmetry. In cosmology it seems one could just say whatever he wants without too much care about scientific estabilished facts. I was impressed once reading some articles that showed that accelerated expansion of the universe could be explained without any reference to cosmological costant and dark energy, but just owing to some very peculiar relativistic effect (I can give references if any of you is interested). The point is: before inventing theories about the universe, shouldn't we study general relativity a lot better? And, before unifying gravity and the quantum, shouldn't we try to understand the basis of QFT and the geometrical structure of QM, and the very profound implications of GR itself?

14. **woit**
March 18, 2007

Please, cosmology is off-topic. I'm not a cosmologist and don't want to moderate discussions about cosmology.

15. **Alex Nichols**
March 19, 2007

I don't think the epicycles analogy is correct.
That's an example of an incorrect theory that was disproved by subsequent observation, rather like to the ether theory.

The suggestion being made is that string theory is incapable of falsification because it can't be tested.
Possibly true, but there are compelling reasons for believing that extended entities that fluctuate are the only possible basis for observable space-time.
This could include strings, loop quantum gravity, spin foams, spin networks etc…

Were it found that the higgs boson is a fundamental particle by the the LHC, all of these would be disproved.
But aren't the problems of falisfiability at high energy (planck or horizon size) equally true for all the other theories?

Perhaps all the effort shouldn't be going into one avenue of research. When it comes to funding though, governments may simply decide that we need more effort in applied physics, such as energy production.

16. **Alex Nichols**
March 19, 2007

BTW, could this finding have any heterotic implications? :-

http://web.mit.edu/newsoffice/2007/e8.html

17. **CapitalistImperialistPig**
March 19, 2007

The problem with particle physics, if it is a problem, is that we don't have any new particles, and the very good theory we have for those particles looks pretty much like a kludge – all those undefined parameters hovering there like epicycles – which were very highly predictive, by the way.

String theory is a heroic attempt to go beyond the SM, but so far hasn't proven predictive in a confirmable sense. My guess is that we might be stuck without more input from the Universe, which is why everybody is pinning their hopes on the LHC.

Maybe it will provide some clue that makes it possible to turn ST into a predictive theory, maybe it will make it more unlikely that ST has any reality, and maybe it will be mute on ST and other subjects.

Only the last would be a bad outcome.

18. **off topic**
March 19, 2007

sorry to be off-topic, but let me point out that simplest inflation predicts Omega_total = 1: it is what we measure, and the fact that the total involves some components we don't understand has nothing to do with inflation. Simplest inflation models also naturally produce a spectrum of scalar adiabatic Gaussian cosmological perturbations with spectral index ns = 1 +- 1/60. Each word has a precise meaning, and it agrees with data. (The deviation of ns from 1 is not yet safely seen).

People tried and try to invent alternative to inflation, but it is not easy because inflation turned out to be good succesful physics. For example alternative models based on "simple string cosmologies" suggested wrong kinds of perturbations (isoentropic, ns not close to 1, etc), and a significant amount of additional complications seems needed to get what inflation naturally does.

19. **matteoeo**
March 19, 2007

I'm sorry I went off topic and I will retain from writing again, but nevertheless I think it's interesting to see how the scientific method has been mistreated and pseudo-scientific claims are made in almost any field of natural sciences and humanistic "sciences".
Or do you think that this bad string theory story is just an occasional mistake soon to be recovered?
My question was: do we know enough of the physics of the 20th century before adventuring in the physics of the 21st? I don't think this question is off-topic.

20. **Peter Woit**
March 19, 2007

matteoeo,

There are all sorts of problematic claims made in different sciences. I just don't want this blog turned into a discussion forum about all of them, but want to keep it focused on things I know about and am willing to moderate discussions of. The question of the evidence for inflation is an interesting one, and "off-topic" makes to-the-point comments, but I'm not an expert on this, and there are good blogs out there run by people who are, so that's where the discussion should really take place.

My point of view is certainly that the Standard Model QFT remains poorly understood in many ways, and that problem deserves more attention. There are lots of other issues in physics that aren't well-understood, but again, I don't want to moderate discussions of issues I don't know much about.

21. **Robert**
March 19, 2007

If your words were as reasonable as your slides congratulations for this nice presentation. For the philosophy of science section of the German Physics Societey I had indended to give a talk with a very similar subject (but of course slightly different conclusions). Unfortunately for personal reasons I could not attend the conference.

Just a minor point of nitpicking (and we have discussed this before): When you say there is no clear cut experimental prediction I would qualify that with "to be performed with currently available experimental technology". Otherwise I strongly believe your claim is wrong, at least if a weakly coupled description exisists (that is there is — possibly after a duality — a stringy description with g

22. **Robert**
March 19, 2007

Sorry for the sudden end of the previous comment. I wanted to say g less then less than (i.e. \ll in TeX) 1 but typing that froze my firefox (probably the script that does the preview. Luckily, I did not lose the post as after a few minutes it popped up a box asking me if I wanted to cancel a script. So I could still press the submit button. But there seems to be a bug either in the script or in firefox…

23. **Robert Musil**
March 19, 2007

Peter,

Please correct me if I mistake your views, but I believe you have several times made clear that while you harbor skepticism over many aspects of string theory as physics, you believe that much extraordinary and important mathematics has resulted from string theory. The "Mirror Conjecture" is one such example. Admiration for string theory mathematics spin-offs is widely shared by many of the worlds leading mathematicians.

But there are some very troubling aspects to even this, very real, admiration for string theory inspired mathematics – at least to my eye. It's trivial to formulate the Mirror Conjecture: Just flip the Hodge array on the diagonal and ask for a variety. But nobody bothered to ask the question before M-theory was posited. Moreover, the first few examples of the Mirror Conjecture are not hard to prove (although the entire conjecture is), yet nobody bothered to investigate them before M-Theory was posited. One main (or at least common) example that supposedly demonstrates the mathematical importance of the Mirror Conjecture – finding those curves – was being pursued (apparently) by exactly two Norwegians on a computer before the Mirror Conjecture came up. Yet the Mirror Conjecture is supposed to be ultra-important mathematics. There is something very strange here.

Perhaps what is strange here is reflected (oops! and unintentional pun) in the constant references to physics in all mathematical programs regarding the Mirror Conjecture (or at least the ones with which I am familiar). "Golly," the mathematicians seem to say, "What I'm noodling over has relevance to the real world! It must be important mathematics!" But if it turns out that string theory is not important physics, I believe it would be a first if the associated mathematics were really all that important – regardless of the level of enthusiasm it has inspired. After all, string theory inspired quite a lot of ill-considered, unchallenged enthusiasm as physics for quite a while.

In other words, I can't shake the sense that the enthusiasm over the Mirror Conjecture (for example) has itself a hall-of-mirrors aspect: Mathematicians (even very good ones) love it supposedly because it is "intrinsically" wonderful mathematics. But it's a strange kind of intrinsically wonderful mathematics that nobody gave a dam about before the physics came along in the form of string theory – even though it's wonderful mathematics whose formulation is trivial and whose first few examples are easy and whose supposedly important applications nobody cared about enough to work on but two Norwegians (not that I have anything against Norwegians, mind you).

Of course, on the other side of the hall of mirrors we find the string theorists reassuring themselves that their theory must be important (or even correct) because the mathematics is so wonderful. Bing, bing, bing goes the wonderful image across the hall – each time a little more distorted as it recedes.

Personally, I find this hall of mirrors aspect of things disturbing, perhaps because I associate halls of mirrors with lower-budget hotel lobbies trying to look bigger than they are. Somehow I get a similar feeling from the mathematical spin-offs of string theory.

Do you have anything to say on this?

24. **r hofmann**
March 19, 2007

Dear Robert Musil,

although I have no idea about the Mirror Conjecture what you say about it and its embedding into the modern relationship between physics and mathematics strikes me as an intelligent observation. Thanks for the info.

25. **David B.**
March 19, 2007

Dear Robert:

Mirror symmetry is not just about "flipping the Hodge diamond".

When you say
"But it's a strange kind of intrinsically wonderful mathematics that nobody gave a dam about before the physics came along in the form of string theory."

You are trivializing the contribution from physicists and mathematicians. The truth is that mathematicians had not suspected that the problem of counting curves in Calabi-Yau manifolds (a typical problem of enumerative geometry) could be related to the theory of deformations of the complex structure on the mirror geometry.

You are also trivializing the problem by making statements like "even though it's wonderful mathematics whose formulation is trivial and whose first few examples are easy".

The formulation is not trivial at all, and it took quite a while before someone produced a complete mathematical proof of the first few examples.

I don't like these misinformed statements about the relationship between research in string theory and mathematics. They seem to be crafted for purposefully misleading the public at large.

Many profesionals use simple statements like "flipping the hodge diamond" when giving presentations in order to explain the simplest aspects of mirror symmetry to an uninformed audience, and to try to give them something they might relate to. In this way they can share the excitement of the subject. Don't mistake those statements for the research that is done in the subject.

26. **Peter Woit**
March 19, 2007

Robert (non-Musil),

The slides pretty accurately reflect what I said. In this talk I wanted to just as clearly as possible state the facts of the matter and avoid any editorializing.

One thing that I should have put in the slides was a comment about the issue you raise, the claim that the testability problem for string theory only arises at low energy, that if we could do Planck scale experiments, it would be testable. I think we've probably discussed this before, but I would claim that the string theory framework continues to be not testable even at that scale. As you acknowledge, even a qualitative prediction of the kind I assume you have in mind (standard distinctive aspects of perturbative string spectra or scattering amplitudes) rely on the string coupling being small enough for the perturbation approximation to be good. Such a prediction is not falsifiable, since it could be evaded simply by saying "well, maybe the string coupling really is not small enough".

In practice, it is true that if we could do experiments at arbitrarily high scales, we'd presumably see what the structure of quantum gravitational effects is, and would see whether this looked at all like anything that had ever shown up in studies of string theory.

Robert (Musil)

David B. is right. The "Mirror Conjecture" and the associated mathematics it has generated go far, far beyond what you mention and are much deeper than "flipping the Hodge diamond". As an example of this, next week at the IAS they'll be an important mathematics workshop on "Homological Mirror Symmetry", focusing on relations to the geometric Langlands program. This is a very active and important area in matthematics. It has pretty much nothing to do with attempts to unify physics via string theory, but it's great mathematics, and maybe someday it will turn around and inspire some physics.

27. **Robert Musil**
March 19, 2007

Peter,

Thank you for your as-always thoughtful response. David B. seems a very intelligent and knowledgeable (if somewhat excitable) fellow, but he is certainly not right in mischaracterizing me as asserting that the Mirror Conjecture ends with the Hodge Diamond formulations. Indeed, I'm not aware of any comprehensive formulation of the Mirror Conjecture. Manifolds with mirror-symmetric Hodge tables are called geometrical mirrors. My point in this regard is (and was) that the Hodge Diamond formulation is trivial to state and notice and that nobody had bothered to do either prior to the positing of M Theory. Yet now that very formulation is deemed to be inherently wonderful mathematics. Of course, this is not an argument for dismissing or downgrading the significance of any version of the Mirror Conjecture. But to start the discussion it does help to get the question right.

Nor is David B.'s assertion that there are no easy examples of Mirror Symmetry right. Indeed, it is not that hard to find references to this fact in papers by central practitioners in the field. Of course, some of the known examples were by no means easy.

As for the geometric Langlands program, I'm not knowledgable in the area of mathematics. I realize that geometric Langlands is an active area of research considered promising by many very smart people. But promise and "rich" structure alone didn't make string theory great – or even important – physics. I'm not sure if I see why one can already conclude that Geometric Langlands is great mathematics – and evaluating the importance of Mirror Conjecture relationships to GL is another step after that.

28. **Peter Shor**
March 19, 2007

If somebody comes up tomorrow with a beautiful new theory which unifies gravity and QM and is much simpler than string theory, and if the LHC produces results that agree with its predictions, I assume that nearly all the string theorists will drop their current research and jump on the bandwagon.

The real question is (a) without any hints of an alternative, are any of them going to abandon string theory research, no matter how unpromising it looks and (b) whether a hint of a promising alternative is enough, or whether it takes a fully formed theory. For instance, if the LHC produces a Higgs mass close to that predicted by Connes, are any of the string theorists going to take this as a hint that maybe they're on the wrong track, and Connes on the right one?

Any wagers on this?

29. **Kea**
March 19, 2007

Any wagers on this?

What are we betting on? How long it will take the String theorists to figure out what's going on? Actual experimental outcomes at the LHC? Oooohhh, this is fun.

30. **A.J.**
March 19, 2007

Several comments for Robert Musil:

1) The relationship between Hodge diamonds predates M-theory by several years. It's part of the story physicists like to tell about M-theory and a simple example of a mirror phenomenon, but I don't think it's of deep importance. More of a decorative note.

2) I've never heard anyone claim that the existence of manifolds with mirror hodge diamonds was the important or deep part of the story. Complaining that others are calling it "inherently wonderful mathematics" seems like a bit of a straw man. Who exactly has said this?

3) What is important, as David B. more or less pointed out, is that we can relate moduli spaces of complex structures to moduli spaces of symplectic structures. This is incredibly non-trivial, and potentially very useful.

4) While I agree that "This shows up in string theory" isn't necessarily a good rationale for a mathematical research problem, I think it's a poor reason to dislike good mathematical ideas. And the notion that there's a topological field theory which carries information about the space of curves and maps to a fixed target has proven to be a fertile source of algebro-geometric ideas.

31. **A.J.**
March 19, 2007

Peter (Shor):

If the Connes et al prediction comes out right, I imagine that some people will take the hint and start working on it. On the other hand, I've also seen some stringy speculation around the fact that the noncommutative space in Connes, Marcolli, & Chamseddine has KO dimension 6.

32. **Robert Musil**
March 19, 2007

A.J.,

You are quite right in that current interest in mirror manifolds is due to the idea is that along with the equality h1,1(X) = h2,1(Y) of moduli numbers of Kahler structures on X and of complex structures on Y, the whole symplectic topology on X is equivalent to complex geometry on Y, and vice versa. In that sense perhaps I should have been more explicit about the means of establishing the Hodge equivalences. But the first examples of this equivalence are not hard, nobody was looking at them, etc.

I'm not sure what you mean by "The relationship between Hodge diamonds predates M-theory by several years," unless you are referring to the earlier computer results. I'm not aware of any general Hodge diamond conjecture that predated the positing of M Theory.

All that being said, I don't see why my points don't still stand. For example, while I don't mean to be snide or obtuse, neither do I see why the assertion that something is "a fertile source of algebro-geometric ideas" is a very good basis for concluding that those ideas or their source are important. The argument seems to completely assume its conclusion. Am I missing part of your point?

There is clearly great and broad enthusiasm for some mathematics derived from (or spun off from) string theory – much of it among very smart and accomplished people. But there was (and is) just such enthusiasm for string theory itself – an enthusiasm only recently seriously challenged. That challenge has been made from one redoubt: In physics one at least has the check on the products of such enthusiasms that at some point or other those products must be EXPERIMENTALLY TESTABLE (although, as this blog cogently points out, some string theory practitioners are struggling mightily to avoid even that check). There is no such check in mathematics. So how do we know that the mathematics spun off from string theory are not just empty enthusiasms? It's just silly to deny that a lot (as Peter points out, perhaps not all) of the enthusiasm is derived directly or indirectly from string theory itself. To make matters worse, some of the best mathematicians speaking to the public about mathematics spun off from string theory often make claims for its importance that are absurdly over the top (Michael Atiyah, for example). Certainly just asserting that one thing or another is "great mathematics" or the like doesn't advance matters, does it? What does?

33. **A.J.**
March 20, 2007

Robert:

OK first, most of the ideas of mirror symmetry predate M-theory by several years. The former is part of the body of evidence for the latter. If you want more direct evidence: Kontsevich's homological mirror symmetry lecture is from the summer of 94; Witten's M-theory announcement from the fall of 1995.

Second, why are we still talking about Hodge equivalences? This is a hint that there's something interesting going on, not the end goal of any major research efforts.

I don't understand what your metric for "importance" is. But it seems to me that algebraic geometers have judged Gromov-Witten theory to be important and interesting because of the ideas it's brought into their field, not because it's connected in some way to a much larger program in a different field. So, yes, by this standard, it's important. If you mean important in some other sense, I really don't have anything to say to you.

My point basically is this: You have a reasonable abstract point about a potential relationship between relative levels of enthusiasm about physics and mathematics, and a cute metaphor about hotel mirrors to go with it. But I think you're quite wrong to single out mirror symmetry as an example of the phenomenon you're talking about.

And I suspect you will have a hard time finding actual examples. It's true that some mathematicians like to talk and daydream about important physics connections, but I think you'll find that the physics-derived ideas which mathematicians have really taken the time to develop intensely have been those which are useful and interesting as *as mathematics*.

34. **Robert Musil**
March 20, 2007

A.J.,

You mention "I don't understand what your metric for "importance" is." Well, let's take that seriously. Terry Tao advanced a set of criteria for "bad mathematics" that I believe were discussed in this blog a while back:

• A field which becomes increasingly ornate and baroque, in which individual results are generalised and refined for their own sake, but the subject as a whole drifts aimlessly without any definite direction or sense of progress; or

• A field which becomes filled with many astounding conjectures, but with no hope of rigorous progress on any of them; or

• A field which now consists primarily of using ad hoc methods to solve a collection of unrelated problems, which have no unifying theme, connections, or purpose; or

• A field which has become overly dry and theoretical, continually recasting and unifying previous results in increasingly technical formal frameworks, but not generating any exciting new breakthroughs as a consequence; or

• A field which reveres classical results, and continually presents shorter, simpler, and more elegant proofs of these results, but which does not generate any truly original and new results beyond the classical literature.

Is it clear that the mathematics spun off from string theory has avoided each of these? It seems at least arguable that one, perhaps more, of these criteria fit uncomfortably well. Not that an answer to this would end the discussion, of course.

35. **Robert Musil**
March 20, 2007

A.J.,

I first want to be very clear that I appreciate your thoughtfulness and intelligent comments. I also want to apologize in advance for popping in this second post before you have a chance to respond to or digest the first.

With respect to Kontsevich's seminal address at ICM, Zurich 1994, it is worth keeping in mind that Kontsevich's himself characterized what he was doing as follows (I quote from his address):

"Mirror Symmetry was discovered several years ago in string theory as a duality between families of 3-dimensional Calabi-Yau manifolds (more precisely, complex algebraic manifolds possessing holomorphic volume elements without zeroes). The name comes from the symmetry among Hodge numbers. For dual Calabi-Yau manifolds V, W of dimension n (not necessarily equal to 3) one has
dim Hp(V,q) = dim Hn−p(W,
q). ….

"We describe here a not yet completely constructed theory which has potentially wider domain of applications than mirror symmetry. It is based on pioneering ideas of M. Gromov on the role of ∂-equations in symplectic geometry, and certain physical intuition proposed by E. Witten."

The relevant references to Witten's "intuitions" are to two papers: Topological sigma models, Commun. Math. Phys. 118 (1988), 411-449 and Two-dimensional gravity and intersection theory on moduli space, Surveys in Di□. Geom. 1 (1991), 243–310.

I believe these quotes address several questions and concerns expressed in your posts above (why we are talking about Hodge numbers, for example). I also believe these passages support my points.

36. **A.J.**
March 20, 2007

Robert:

Tao didn't give that list as criteria for bad mathematics. It's just a list of dangers (somewhat exaggerated as Tao admits) which *might* have detrimental effects on the development of a field. I think it's misleading to treat it as checklist for identifying "bad mathematics".

That said, the only danger I see being remotely applicable is is the 2nd one. But I don't think it's a particularly great danger. For one thing, judicious borrowing of physical intuition has a pretty good track record. (Donaldson theory, Chern-Simons, knot polynomials, mirror symmetry, Seiberg-Witten theory, and so on.) And for another, mathematicians have a habit of concentrating on problems they think are solvable. No one is butting heads with 4d Yang-Mills theory right now, because it's probably out of reach. But there's lots of motion in the Gromov-Witten theory of orbifolds right now; people are getting things done.

37. **A.J.**
March 20, 2007

Robert:

I don't see how the Kontsevich quotes support your points. Perhaps you'd care to explain? You'll probably have to take some care to spell out carefully what you mean, since we seem to be talking at angles.

Some of the confusion may stem from the term mirror symmetry. The symmetry gets its name from the duality of the hodge diamonds, but it's just a name. The actual set of ideas involved is considerably richer than the name implies. Most of it has been developed in the years since Kontsevich's lecture.

38. **David Williams**
March 20, 2007

Hello
Please have mercy on an old scocial science Phd.

I have had, basically, only a pragmatic and professional education except for a couple of biology courses and a stint as a biology teaching assistant (where I first encountered the scientific method) but I have indulged my interest in popularized science writing. I use this information in debating the champions of religion.

In debate the basic successful argument is that Science is not based on belief but on questioning and testing. Recently, String theory has become widely accepted in physics. I love the idea in the sense that it tells us that the universe is a symphony. HOWEVER, String theory appears to arrive at the position of a Unified Field Theory only by relying upon
a. mathematical solutions b. solutions that require positing multiple universes.

May I ask you these questions.

In your opinion are mathematical solutions the equivalent of an empirical test? Although I'm told (and I simply have to accept or not – at the level of my math and science skills) that M theory will offer an opportunity to empirically test String Theory. I cannot, to my satisfaction, imagine an empirical test for multiple universes.

And, If empirical tests are not available by the very nature of String Theory is this idea no better than religious belief?

I am inclined, therefore, to simply leave String theory to it's own devises and conclude that it lacks scientific credibility and that we are stuck with the contradictions between General Relativity and Quantum Dynamics. We would otherwise be as lacking in evidence as the religious. Why have physicists so departed from scientific standards?

Hope you will be able to spare the time answer this query.

David P. Williams, PhD
3181 Micmac St. Halifax
Nova Scotia Canada B3L 3W3
(902) 454

39. **tomj**
March 21, 2007

David,

I have similar concerns. String theory is hyped or hoped beyond belief. This is serious, because if a scientist is supposed to be exact and careful about their theory, their work, etc., why doesn't this carry over into their public descriptions?

I somehow stumbled across this site a few weeks ago. But for several years I have firmly believed that there was something not right about the 'theory of everything' crowd.

At that point I was trying to track down some more concrete details about these theories. But nothing concrete ever appeared. Instead, I ran across some made up cafeteria dialog between a string theorist and (I guess) a LQG theorist. I think the point of the dialog was to highlight the lack of evidence for either theory, but more important for me was another principle: science is about the unknown, not the known or the unknowable.

If science is expanded to cover the unknowable, you forfeit the ability to apply Occam's razor. Occam's razor isn't a theory, it isn't a law of nature, it is a check on logic: it requires experiment. If a theory has no experimental results, how can you compare it to one that does? If a theory predicts unknowables like multiple universes, how can this win out over a theory that predicts only the one we experience?

My problem with the proponents of string theory is that their ideas fall into the category of 'known' or 'unknowable'. That is, their statements lead me to believe that they know something (strings are the basic building blocks of everything) or their theory covers stuff we can't know (multiple universes, etc.). In the first case, they are lying, or using language in a very sloppy way. If they are sloppy with English, why should I think that they are not sloppy in their math or logic?

What I don't understand is that if a scientist makes wild statements that they 'know' something or that their theory implies 'unknowable' realities, why shouldn't I remember their unscientific approach? Either put up, or shut up.

Known = technology
Unknown = science
Unknowable = fantasy

40. **r hofmann**
March 21, 2007

A.J.,

it's of undeniable educational value to follow the debate between Robert Musil and yourself.

`No one is butting heads with 4d Yang-Mills theory right now, because it's probably out of reach.'

This statement is, however, outright false and confirms
pretty much the relevance of Terry Tao's above quoted criteria.

Best, RH

41. **Ralf**
March 21, 2007

David and tomj,

String theory/M-theory is a speculative research program which therefore would not be covered at all in the popularized science press if the latter were responsible. Officially, string theory is "accepted" exactly as that. In practice this doesn't stand in the way of string theorists taking over high energy physics, in part because the field has been short of new ideas for three decades. In such cases the subjective criteria for what can be regarded "reasonable" ideas become rather flexible. The "scientific method" exists only in the imagination of philosophers of science. "Occam's razor" cannot be "applied" like a theoretical analog of a lab test.

People can be honestly deluded about things that are crucial to their identity, like their love life, their social life or their professional world. In addition, string theorists view themselves as intellectually superior to everybody else, which automatically degrades any objections brought up by those.

Finally, in reality there is simply nothing about string theory that can be related to laypeople. Anybody who writes about it is only heaping nonsense on a foundation of nonsense which again rests on a foundation of nonsense. It is utter intellectual dishonesty to pretend otherwise. Supersymmetry, by itself, cannot possibly be assessed by a non-physicist. Every account makes it appear much more reasonable than it is. Grand Unification sounds almost like a no-brainer if one doesn't know the details. The technical details on which string theory is built—and which are never even mentioned in the popular press—render it, in my opinion, deranged and demented. And it is exactly this wide gap between actual physics and string theory that—perversely—facilitates the public's susceptibility for it. The public never registered anything from the Schrödinger equation onward because they don't like the absence of visualizability.
That is why they prefer the faux visualizability of General Relativity—and of string theory, of course. Physics is not the Riemannian geometry of the 19th century. It was Einstein, after all, who commented that one should explain everything as simply as possible—but not simpler.

42. **a**
March 21, 2007

dear David,

let me try to give an answer to your “Why have physicists so departed from scientific standards?”. It is oversimplified and caricatural, but I think it captures a relevant aspect of the question. Do you believe that an average rational human being would choose option A or B?

Option A is what is happening now.

Option B is “I spent my life working on strings, but, contrarily to what press said, initial hopes mostly disappeared. Maybe I could start doing some other physics, but I only have expertise in strings, that is a highly specialized topic: so I resign from my academic job”

43. **A.J.**
March 21, 2007

R. Hofmann:

Sorry about that. I was not expressing myself clearly. (Why can’t you people just read my mind?!) A precise formulation: “Few if any mathematicians are attempting to construct 4d Yang-Mills theory in the sense required by the Clay Millenium prizes.” Obviously plenty of people are thinking about 4d Yang-Mills in a non-rigorous fashion, or trying to work out various facts about its topological analogues. But no one’s managed to do anything interesting as far as construction & mass gap goes.

44. **r hofmann**
March 21, 2007

A.J.,

I see … That problem was formulated by E. Witten and a famous Harvard mathematical physicist, right?

Best, RH

45. **A.J.**
March 22, 2007

Yes,

I don’t know anything about how the Clay Foundation works, but at the least the problem description was written by Edward Witten and Arthur Jaffe.

46. **Ari Heikkinen**
March 22, 2007

Peter,

Just a couple of questions, when you say:

‘The fundamental problem is that simple versions of the string theory unification idea, the ones often sold as “beautiful”, disagree with experiment for some basic reasons.’

Do you mean by this that what’s in Greene’s book (that “beautiful idea”) of particles being tiny vibrating strings of which amplitude and wavelength corresponds to different masses and force charges of them and that those “extra” dimension are curled up in Calabi-Yau shapes are what disagree with experiment?

‘Getting around these problems requires working with much more complicated versions, which have become so complicated that the framework becomes untestable as it can be made to agree with virtually anything one is likely to experimentally measure.’

And by this something that Greene’s book don’t mention?

47. **Peter Woit**
March 22, 2007

Ari,

One of the main problems is that you have to do something to fix the size and shape of the Calabi-Yaus, and the only ways people have found to do this involve introducing a lot of complex, ad hoc structure. This is the “moduli problem”, and I don’t remember what Brian says about it in his book. His book was written now quite a few years ago, before people had any solution at all to the problem. Back then I suspect there was a lot more optimism that a simple solution could be found.

E8 Media Blitz

March 20, 2007
Categories: Uncategorized

Last Friday night when I was in Rome I received e-mails in quick succession from two science journalists asking what I thought about a new mathematical result, the "mapping of E8" that was going to be announced at a press conference on Monday. Information sent to journalists was embargoed until Sunday night at 11pm, but the first journalist sent me a copy of the brief press release and told me that there was a longer one available. Reading the press release left me still baffled about what this could be about: what was the "century old problem" that this group of 18 mathematicians had solved? The obvious interpretation of "mapping of E8", mapping it as a geometrical object, didn't make sense since that's a well-understood problem. The group E8 is a 248 dimensional space, but its local geometry is the same everywhere and completely understood in terms of its Lie algebra. The global topology is interesting, but also well understood.

I wrote back to both journalists that the best person I knew to comment on this and its possible relation to physics would be John Baez, and asked to see the longer press release. It wasn't much more enlightening, but it did have a link to a web-site with details. After spending a little time reading this I understood that "mapping of E8" was a calculation of the structure of representations of the split real form of E8, and decided that I was on vacation and not about to try and quickly write a blog posting about this.

Well, here are the press releases from MIT and AIM, and David Vogan did give a public talk about this yesterday at MIT. The media blitz was quite effective, getting the story into not just the usual suspects (there's a good version of the story by JR Minkel at Scientific American), but also achieving a wide distribution in much less usual places such as today's New York Times, the BBC, le Monde, and many, many others. I think this may be getting about as much attention as the proofs of Fermat's Last Theorem and the Poincare Conjecture. There are also a huge number of blog postings, and I'm very pleased with myself to note that by far the best is the one by John Baez (crucially supplemented by the first comment there, from David Ben-Zvi), so I at least sent the journalists to the right place.

For mathematical details, John's posting and the comments there are the best place to go besides the technical papers linked to from the AIM site.

While the calculation is a computational *tour de force*, and the computational methods may be useful elsewhere, the level of hype in the press releases, especially about the possible relations to physics, is somewhat disturbing. The AIM page on E8 and Physics contains statements such as

…once one adopts the basic principles of string theory, it can be argued that we live in the universe we live in because it is the only one that is possible.

as well as making the highly misleading claim that the new calculation has something to do with heterotic string theory.

What initially confused me about the press release is that, with the standard interpretation of what one means by "E8", the "E8" that appears in heterotic string theory, there is no open problem to be solved. The group is well-understood, and so is its representation theory. As a compact Lie group, the representation theory of E8 is part of the standard Cartan-Weyl highest weight theory, and was worked out long ago. To read about this, there's an excellent book by Frank Adams about the representation theory of E8 and other exceptional Lie groups, called Lectures on Exceptional Lie Groups. It is this representation theory that appears in the heterotic string story. For more about E8, and one of the stranger things I've seen in a math paper, you might want to look up a 1980 paper by Frank Adams called "Finite H-spaces and Lie Groups", in the Journal of Pure and Applied Algebra.

What the new result is about is something quite different, the "split real form" of E8. The classification of compact Lie groups proceeds by classifying their Lie algebras, giving a well-known list, with E8 the largest of the exceptional cases. In doing this, one complexifies (works over the complex numbers), studying the complex semi-simple Lie algebras, which are the Lie algebras of the complexifications of the compact Lie groups. In the simplest example, one studies SU(2) by complexifying its 3d Lie algebra (R^3 with vector product), i.e. studying the Lie algebra of SL(2,C) instead. Finite dimensional unitary representations of SU(2) correspond to holomorphic representations of SL(2,C), and the same correspondence works in general between finite dimensional unitary reps of compact Lie groups and holomorphic representations of their complexifications.

Given the complexified group, one can ask if it has other "real forms", i.e. subgroups other than the compact one which would have the same complexification. In the case of SL(2,C), there is another real form: SL(2,R). The representation theory of SL(2,R) is a vastly more complicated subject than the case of SU(2). One reason is that the group is non-compact. Geometrical constructions of representations like the Borel-Weil construction give infinite-dimensional irreducible unitary representations. The case of SL(2,R) is difficult enough (and a central topic in number theory), but the case of representations of general real forms of semi-simple Lie groups is extremely difficult and complicated. Representations are infinite-dimensional and labeled by "Langlands parameters" instead of highest weights. This theory has been pretty well worked out over the last 30-40 years or so, with the case of E8 one where it was known how to do calculations in principle, but they had so far been computationally intractable. Dealing with this is the new advance.

What actually is calculated are things called "Kazhdan-Lusztig" polynomials; for an explanation, see John's blog. These tell one how to build arbitrary irreducible representations out of something simpler which one does understand, certain induced representations called "standard" representations. The numbers involved here also have a beautiful geometrical and topological interpretation. This is a generalization of what happens in the compact case, where the cell decomposition of the flag variety governs how irreducibles are built out of Verma modules.

So, this is a result about the structure of the irreducible representations of one of the real forms of E8 called the "split" real form. As far as I know it has nothing to do with heterotic string theory. The only thing I can think of that physicists have worked on that might make contact with this result is the work of people like Hermann Nicolai and Peter West trying to get physics out of Kac-Moody algebras like E10 and E11. I have no idea whether they have run into the split real form of E8 subalgebras and the representation theory of these in their work. In Pisa I had the pleasure of meeting blogger Paul Cook, a student of Peter West's who is now a postdoc in Pisa and has worked on this kind of thing. Perhaps he would know about this.

Update: I hear from Jeffrey Adams that he has put together a web-page about this, aimed at mathematicians, and designed to explain the nature and significance of this result. It's quite clear and does a good job of this, accessible if you have a bit of background in representation theory. If not, you may at least enjoy his comment on the media attention:

This leaves the question of why this story took off in the press. For us, that is harder to understand than the Kazhdan-Lusztig-Vogan Polynomials for E8.

Comments

1. **Matti Pitkanen**
   March 20, 2007

   Dear Peter,

   I think that AIM link is to an older posting of John Baez which is not
   about the recent calculation but about compact form of E_8.

2. **Peter Woit**
   March 20, 2007

Matti,

Not sure which link you mean. The "E8 and Physics" link is on the main page announcing the new result. It does just refer to the compact case and links to an old Baez piece about this. I think it would be better if they made clear that this is rather different than the case they're issuing a press release about.

The analogy in SL(2,C) would be trying to promote a new result about SL(2,R) representation theory by claiming that it had do with the physics of spin (SU(2)). They're two very different things….

3. **Aaron Bergman**
March 20, 2007

The split real form of E8 shows up when you compactify 11D SUGRA on an 8-torus. In particular, all the fields organize them into representations of the group (except for the scalars which organize themselves into a sigma model into the homogeneous space E8/K where K is the maximal compact subgroup). This is the inspiration for the ideas of Nicolai, West, Ganor and others about exceptional symmetries in M-theory.

4. **Cody**
March 20, 2007

Is it common for mathematical results to be announced via press release? What kind of coverage could anything less than a result like FLT hope to get?

5. **Peter Woit**
March 20, 2007

Thanks Aaron,

Very interesting. Do you know if there's any possible application of this calculation to that story?

Cody,

No, it's not common to announce math results this way. I'm rather surprised at how successful it was in getting attention. Lots of universities and other organizations issue press releases about the work of their people, but normally these are mostly ignored by the general media. Sounds like AIM has a really, really good person handling this.

6. **John Baez**
March 20, 2007

Thanks for the plug!

One of the people on the Atlas team, Jeffrey Adams, just let me post an email explaining more about what they calculated.

7. **Matti Pitkanen**
March 20, 2007

Peter,

Yes,I meant "E8 and physics". By the way, the flag manifold discussed in the page of John Baez is similar to that appearing in the construction of modular representations in Langlands program.

8. **J**
March 21, 2007

I just tried to read that about 5 times and now I want to hit my head on my desk because I am lost after about the first 8 words. 🙁

9. **Turkey Royale**
March 21, 2007

The tone of your post is unfortunately negative. You have allowed your antipathy to all things stringy to mislead you into misreading what has been done and what has been claimed to have been done. Look at the Atlas webpage and you will find nothing about string theory, only a coherent project to describe representations of real reductive groups. The split real form of E8 being the largest exceptional simple real Lie group, the calculation of things like its character table is impressive both theoretically and computationally. The goal of representation theory is not to know in the abstract how to find representations, rather to know the representations and how to decompose their tensor products. When G has many, and their structure is complicated, it is at first bewildering how one might even organize a map (a page in an atlas) of the representations of G. This is what the Atlas project is after.

There is a certain tendency to wish to find the exceptional simple real Lie groups in nature, but this tendency has really very little to do with a mathematician's interest in these groups.

10. **Dug**
March 21, 2007

To assist readers in better understanding the E8 relation to the Leech Lattice, the Monster and [string] physics, consider:

1 – The Terry Gannon arXiv paper 'Monstrous Moonshine: the first 25 years' [33 pages with 124 references].
http://www.arxiv.org/PS_cache/math/pdf/0402/0402345.pdf

2 – This appears to form the framework of the Gannon book, 'Moonshine Beyond the Monster' [477 pages, 575 references].

Lieven le Bruyn refers to [1] as a survey paper at 'Never Ending Books'.
http://www.neverendingbooks.org/?p=133

11. **Robert Musil**
March 21, 2007

Peter,

This is a worthy but quite modest result. But the hype surrounding it that has been confected by the people involved is just embarrassing. That the media have fallen so badly for that hype is just one more testament to how intellectially impoverished reporters and media outlets have become.

The classification of irreducible unitary representations of reductive Lie groups has been a central focus of David Vogan's work for over 30 years. But no general picture has emerged, not even a conjectural one. For the last few years he has been making a computational assault. But for this kind of question a computational approach all but concedes defeat. The original goal was a computer program that would tell you whether or not a parametrically inputted potentially unitary representation is actually unitary. Hypothetically, if there were a conceptual description of the irreducible unitary representations for all representations other than the split real form of E8 (rather than computational description), the omission would be of much concern to no one. Conversely for a computational approach, a general answer would not add all that much to an answer for all cases other than E8.

The matrix that relates the irreducible (unitary or not) representation to the so-called standard representations is called the "Kazhdan-Lusztig matrix." This effort computes the Kazhdan-Lusztig matrix for E8, as well as the inverse of that matrix. The algorithm to do this in all cases is well known. The only problem was that some of the matrix entries are very large integers, so computation for E8 required too much computer memory. Someone I will not name – someone who has not even been credited publicly in all this hype — suggested to the Adams-Vogan group that they perform the E8 computation modulo several large (but not too large) primes. That reduces the amount of memory enormously and the actual answer can then be reconstructed. This is exactly what the Adams-Vogan group now has done. The program can run on a laptop for groups other than E8. The hype (although not the result) is risible because just knowing the Kazhdan-Lustig matrix is parsecs from solving the unitarity problem.

12. **Michael**
March 22, 2007

Peter, if I needed an opinion about a scientific breakthrough, you'd be my first choice as well. I'll keep you on speed dial…

13. **Peter Woit**
March 22, 2007

Turkey Royale,

To the extent my comments were negative, they referred not to the Atlas project or its web-pages, just to the AIM web-pages and press release. I think the material there about relations to physics is seriously misleading, and one of the main things I wanted to do in the posting was to clearly explain what the issues are, something I don't see getting explained anywhere else on the web or in the media.

As for the significance of this as pure mathematics, sure, it's an impressive piece of work, especially as a computational achievement. The fact that the AIM people managed to get it so much attention is quite a phenomenon. In one sense it's great: if every mathematical advance of this magnitude was widely covered in the press, that would be wonderful, and there would be a lot more press stories about math. But, given how traditionally only the most dramatic millennium-prize sort of advances normally get covered, I do think it's a good idea to offer some perspective on this one.

14. **Robert Musil**
March 22, 2007

I particularly enjoyed digesting this hi-calorie forkfull of hyperblather (http://news.yahoo.com/s/afp/ussciencemathematicsfrancegermany):

"Today string theorists search for a theory of the universe by looking at E8 X E8. The scientists said the magnitude of the E8 calculation invited comparison with the Human Genome Project. While the human genome, which contains all the genetic information of a cell, is less than a gigabyte in size, the result of the E8 calculation, which contains all the information about E8, is 60 gigabytes in size, they said."

But this implied claim that the mathematicians who "have successfully mapped E8" have accomplished something about 60 times as difficult and important as the biologists "mapping" the human genome raises a difficult issue: How is it that the marketing geniuses who developed and sold string theory failed to call it "Strand Theory" instead? After all, if it were called Strand Theory the facile equation of (1) the importance of this branch of physics without a testable hypothesis or prediction and (2) the ultra-important and practical structure of DNA (which comes in the most famous double strands of all!) could have been enormously facilitated in the mind of the public. As it is, the Adams-Vogan group (or at least their publicists) have to labor mightily to insinuate that their result is as epoch making as the mapping of the human genome at the same time they are laboring to imply that E8 contains – through a completely preposterous and non-existent string theory connection – something like the "genetic code" of particle physics.

Gosh, they must be tired. And all of that extra labor could have been eased so much by just one simple change in nomenclature!

It just makes you wonder where their heads were at when they were poking them into all those extra dimensions.

15. **turkey royale**
March 24, 2007

Peter,

My post did not intend to defend the hype-seekers and fame-mongerers, just to say that the culpability for the allusion to string theory did not appear to belong to the representation theorists in the Atlas Project. That is not to say that their efforts merit or do not merit press coverage; the folks actively seeking press coverage are the same folks all the time scheming to dominate this or that aspect of academia, and they are very tiresome for the rest of us who just want to modestly understand a few modest things within our comprehension.

In this sense, I think you are basically right to criticize the publicity seeking efforts of AIM. Overstating one's case undermines one's credibility in the eyes of those who know something.

I think the unnamed suggestor mentioned in Musil's post was Noam Elkies.

16. **Jeffrey Adams**
March 25, 2007

Now that the dust has settled I would like to make a comment about the attention this story has gotten in the press.

The goal of the Atlas of Lie Groups and Representations is to classify the unitary dual of a real Lie group G by computer. A step in this direction is to compute the admissible representations of G, including their Kazhdan-Lusztig-Vogan polynomials. The computation for E8 was an important test of the technology. While an impressive achievement, it is but a small step on the way towards the unitary dual, and not remotely as important as the original work of Kazhdan, Lusztig, Vogan, Beilinson, Bernstein et. al.

Nevertheless, because of the nature of the result, the Atlas team
and the American Institute of Mathematics decided this would
be an excellent opportunity to educate the public about research in pure mathematics. The intended audience of this campaign was the general public, and it was

undertaken for the benefit of mathematics awareness as a whole, and not for the Atlas project itself. We are happy to have been successful in raising awareness of mathematics research worldwide.

For more information see some details about the Atlas project.

String Theory Debates

March 22, 2007
Categories: Uncategorized

This seems to be the month for string theory debates, with two a couple weeks ago in the UK involving Lee Smolin, and another featuring Lawrence Krauss and Brian Greene scheduled for next week in Washington D.C. The Washington Times has an article about this.

Smolin's book has just appeared in the UK, and there have been lots of (very positive) reviews. See here, here, here, here, and here.

Besides talks (for a report on the one at Cambridge by a skeptical American physics student in England, see here), there were two debates. One featured Smolin, Philp Candelas, Simon Saunders and Frank Close and was held at Oxford; for a report, see here. It appears to have been a respectful and reasonable public airing of a few of the issues where string theorists and some of their critics disagree.

A couple days earlier though, a debate in London between Smolin and Mike Duff (also involving philospher Nancy Cartwright) had a very different nature. According to the report from one attendee, after Smolin started things out by arguing his case:

Smolin sat down. Duff stood up. It got nasty.

The trouble with physics, Duff began, is with people like Smolin…

Duff is described as "string theorist and man for whom, one imagines, the words 'self' and 'doubt' do not often rub shoulders", and seemed to think it was a good idea to answer criticisms of string theory with vociferous ad hominem attacks. Lubos Motl and Clifford Johnson both found Duff's behavior an excellent example for all string theorists, inspiring Clifford to write part VII of his extended attack on me, Smolin and our two books. He admitted somewhere around part V or VI that he actually hadn't looked at the books and had no intention of doing so, and he's pretty steadfast in that attitude. It never ceases to surprise me that people like Clifford don't realize that, much as they may enjoy engaging in or listening to personal attacks on me and Smolin, this just doesn't do a lot for the credibility of their field. String theorists often complain that Smolin portrays them as arrogantly dismissing any criticism, but they should realize that behavior like Duff's doesn't help them at all on this issue, quite the opposite.

Duff pretty obviously has a double standard for popular books about string theory. He's quite capable of being polite, writing a very respectful review of Susskind's The Cosmic Landscape for Physics World. His review of Smolin's book in Nature Physics is something very different, much more like his performance at the debate. The review begins by misquoting Smolin, based upon something that was in the proof copy of the book he had (which the author hasn't had a chance to look at), but was different in the published version. After the review, he had been informed about this, but still seemed to think it was a good idea to use this as ammunition in his personal attack on Smolin during the debate.

One of his main points was that it is ridiculous to claim that string theory has not made any progress since the 80s. Obviously there are some areas in which there has been progress in better understanding the theory, but, as far as the central issue, that of getting any predictions out of the idea of using strings to unify physics, it's interesting to follow the link that someone with a waggish sense of humor at Nature put at the bottom of the page of Duff's review. It's a story from 1986 entitled Where Now With Superstrings?, and it reports on the views of string theorists at the time, roughly one year after the early developments that caused so much enthusiasm for string theory as a unified theory. The problem of too many vacua was something people were starting to worry about, but the feeling was that:

… another problem of non-uniqueness in superstring theory, the variety (thousands) of possible four-dimensional worlds it allows, is showing some signs of resolution.

The "progress" on this more than twenty years later is that instead of "thousands", the number has moved up to the exponent, and we've now got the "Landscape" of 10^{1000} or so possible four-dimensional worlds. Any "signs of resolution" of this are long vanished. Just as physicists are now waiting for the LHC next year, those of 1986 were waiting for the Tevatron to start up the next year, with Weinberg claiming that the mass range to be explored by the Tevatron was "a very plausible mass for them to have". The reporter wrote that:

If the Tevatron sees no superparticles, supersymmetry will lose its value in the hierarchy problem, and hence half its motivation.

So, I guess Duff is right that it's inaccurate to say that things haven't changed with the prospects for string theory since 1986, since the situation now is a lot worse than it was then.

If you want to listen to the debate, audio is available on-line here, with a transcript to appear shortly. For another kind of audio showing what this is all about, see this posting from Sabine Hossenfelder.

Comments

1. **Bee**
   March 22, 2007

   Hi Peter,

   thanks for the link. You know what I really don't understand is why some string theorists apparently deliberately try to make the situation worse for themselves. In my opinion, the smart thing to do is definitely no to argue with their interpretation of the author's intentions or psychological problems. For the public opinion – what many pretend to be oh-so concerned about – they could just have turned it into a reasonable discussion about funding in theoretical physics, instead of getting upset about a would-be here or there.

   Best,

   B.

2. **Peter Woit**
   March 22, 2007

   Bee,

   I agree. One of the weirder aspects of being involved in these arguments is sometimes watching people on the other side engage in self-destructive behavior. I think both Lee and I have often had the experience of feeling that we could do a better job of defending string theory research than many string theorists seem capable of.

   Especially in private, most string theorists I know have no trouble admitting that the situation is pretty discouraging for string theory right now. Those who feel this way aren't likely to agree to review the books or get into a debate on the subject in public, since it's kind of a no-win situation. String theorists who do take on these assignments are often the most fanatical true believers around, and this is all too obvious. It would be very much in the interest of more sensible string theorists to speak up for themselves, and not let their least convincing colleagues be the ones to publicly represent their field.

3. **Joseph Smidt**
   March 22, 2007

   Great post. Thanks for the heads up about the debate.

4. **JC**
   March 22, 2007

   Sometimes folks will just hang themselves with their own rope, with very little to no encouragement from others. This is very common in politics and religion, than in science.

5. **Erik**
   March 22, 2007

   I'm an undergraduate—I've only been reading this blogs for about 6 months or so. It is generally quite informative and enjoyable, and I appreciate your updates (although the overly technical discussions obviously go over my head for the most part, which I expect). I occasionally read most of the blogs that you link to, so I'm familiar with many of the harsher posts, but this latest round following Dr. Smolin's and Dr. Duff's debate seems a bit worse than usual—particularly nasty and biting. I guess I'm just wondering to what extent this type of behavior is… "normal?" Is this most common in this particular branch of physics? I guess the claims being made are pretty weighty and the stakes are certainly pretty high in this branch. And finally, for all the little flurries of posts and responses that these things set off in the "blogosphere," do you think the theoretical physics community at large experiences something similar?

6. **Peter Woit**
   March 22, 2007

   Erik,

   I think you'll find that academics in general are no better or worse behaved than the population at large. String theory is a bit of a special case: a huge amount of effort has gone into it and a lot of people have a lot invested in it, while it hasn't worked out as they had hoped. Under the circumstances, lots of people in the field are unhappy with the current situation, but mostly quietly, and behaving reasonably in a difficult situation. Unfortunately, some are reacting with less than reasonable behavior. The blogosphere kind of encourages some of this, but it's not just a blogosphere phenomenon.

7. **DB**
   March 22, 2007

   It's interesting, resurrecting that 1986 story. It shows how some (by no means all) string theory practitioners have transformed a once promising speculative path into a quasi-religious quest. I think Einstein, with his fruitless 30 year quest for a unified field theory, ignoring the wonderful empirical successes of quantum theory along the way, is at least partly responsible for the emergence of this attitude. I think it is no coincidence that Princeton, where Einstein conducted this quest, is today the locus of string theory research. Today's practitioners of this doomed tradition could do with some of his humour and humility.
   I can't just blame Einstein though, and I often wonder why it is that other great physicists, Newton, Weinberg, Schrodinger – have shown a willingness to become side-tracked into the realms of metaphysics in their later years, albeit to very different degrees in each case. An age-related onset of rigidity of thinking, allied to a diminution of creative powers perhaps?

8. **Peter Woit**
   March 22, 2007

   DB,

   The problems with string theory unfortunately can't be blamed on people getting old. That's a well-known problem in the sciences, and biology has an effective way of dealing with it. Many of those most fanatically devoted to the string theory ideology are quite young, with new converts coming up through the ranks all the time.

9. **Bee**
   March 22, 2007

   Peter Woit said: Especially in private, most string theorists I know have no trouble admitting that the situation is pretty discouraging for string theory right now. Those who feel this way aren't likely to agree to review the books or get into a debate on the subject in public, since it's kind of a no-win situation. String theorists who do take on these assignments are often the most fanatical true believers around, and this is all too obvious. It would be very much in the interest of more sensible string theorists to speak up for themselves, and not let their least convincing colleagues be the ones to publicly represent their field.

   This is also my perception, and I hope some of the 'more sensible string theorists' take your advice.

   (Though I suspect that in some cases the stage for a debate is set such that controversy is expected or even hoped for. In a certain sense, it must be boring for the public to hear that in most cases theoretical physicists get along with each other pretty well.)

10. **Aaron Bergman**
    March 22, 2007

    The problem, I think, is that there are at least three different debates going on simultaneously. The first is a discussion of the merits of string theory. At least speaking for myself, I think this is a perfectly legitimate debate to have. There's a lot of misinformation out there, but these are generally scientific questions and can be discussed scientifically. I don't believe I've ever personally attacked anyone for raising a criticism of string theory.

    Secondly, there is the debate about how to best encourage risk-taking and new ideas in the field of theoretical physics. This is a much more difficult debate to have because I don't think there are any easy answers.

    The real problem, however, is the third "debate" which is really a series of attacks on string theorists and the string theory community. This is a personal debate and has often led to personal responses. I can't imagine anyone finds it surprising that people often respons as such when their reputations as scientists are attacked.

    I don't see any real hope of disentangling these debates.

11. **Aaron Bergman**
    March 22, 2007

BTW — let me take this opportunity to give a short version of the response I'd like to give on the issue of the cc over at Asymptotia — I have someone visiting so I don't have the time to do an extended version. The short story is that you can look at contributions to the cc in SUGRA and nonSUGRA situations:

SUGRA:

1) Bare cc — related to superpotential
2) vacuum corrections — related to susy breaking scale
3) symmetry breaking
4) condensates

nonSUGRA
1) bare cc — free parameter
2) vacuum corrections — divergent, presumably cutoff by Planck scale
3) symmetry breaking
4) condensates

(and probably stuff I'm forgetting)

Now, given these charts which I hope you agree with, I don't see how the statement that the cc-problem is worse in susy situations is supportable.

12. **Ari Heikkinen**
March 22, 2007

There's also the question wether string theory is physics or philosophy based on mathematical reasoning.

My observation is that even suggesting the latter seems to be taken as an insult by atleast some string theorists.

13. **Peter Woit**
March 22, 2007

Aaron,

I don't really disagree with your charts. The point I keep trying to get across is that, as you note in your chart, in the SUGRA case the scale that appears is the SUSY breaking scale, and that is something well-defined enough for us to actually measure it. We have an experimental bound of around 100 GeV, and if low-energy SUSY exists, maybe the LHC will give us not just a bound, but a number. Problem then is that this number is at least 10^60 times too big, and this is what pretty much everyone describes as the "CC problem".

In nonSUGRA the situation is just different, there is no such thing as a SUSY breaking scale, so you don't have this problem. You do have the problem that vacuum corrections are divergent and so the CC is ill-defined, but that's a different problem than in the SUGRA case. It's somewhat a matter of taste which is the worse problem, but I think it's a sustainable point of view that having a theory in which a question is well-posed, but the answer is completely wrong, is worse than having a theory in which the question is ill-posed. Being wrong is a worse thing to happen to a theory than being not even wrong, since at least you can hope that further understanding will move you from not even wrong to maybe right, but wrong is just wrong.

14. **Aaron Bergman**
March 22, 2007

I don't see how you can distinguish the divergent computation in nonsusy situations from susy situations; it's the exact same calculation. You can even send the susy breaking scale to the planck scale and interpolate between the models.

15. **Peter Woit**
March 22, 2007

Aaron,

About the "third debate". I think most string theorists are careful not to engage in personal attacks, and in particular I think that you're not someone who has done this, other than at times expressing exasperation with Lee, which isn't especially unreasonable. But I think it's highly unfair of you to characterize either Lee or me as being responsible for this situation because we started personally attacking string theorists. I don't think there's anything at all in either of our books that could be characterized that way. In his other public statements (as well as in all private discussions I've ever had with him), I know of no case where Lee has personally attacked anyone. I've by now written thousands of pages about string theory and string theorists on my blog and others, and undoubtedly have, in a small number of cases, out of exasperation, made comments about people that I regret. Recently, I have made very specific accusations about the personal behavior of some people, and those I don't regret.

Sure, both Lee and I think there is a wide-spread problem with how string theory research has all too often been conducted. He gives examples of what he sees as problematic in his book, I do so in my blog and in my book. These aren't "personal attacks" on people, they're specific complaints about specific behavior and decisions, which we see as coming out of, not personal failings of specific people, but an organization of research that is not working.

This is very different than the wholesale personal abuse both Lee and I have been subjected to. You know very well that I can give you large numbers of examples of this (Lubos Motl and his supporters at Harvard, Susskind, Duff, Peet, Srednicki and the other jeering bozos at the George Johnson talk at the KITP, Jacques, Clifford, "Hmm", "Michael", and others). The problem with this "third debate" is real, but it's not of our making.

16. **Chris W.**
March 22, 2007

Ari:
There's also the question wether string theory is physics or philosophy based on mathematical reasoning.

My observation is that even suggesting the latter seems to be taken as an insult by at least some string theorists.

That's because they have such a dismissive (and not particularly well-informed) attitude towards philosophy. Not surprisingly, string theory is not particularly coherent or well-considered as philosophy.

In contrast, Einstein's thinking was a thoughtful, careful, and original philosophical thinker, largely because he saw philosophical thinking as playing an essential role in physics. In particular he saw it as an antidote to ill-considered and uncritical reliance on mathematical formalism in physics. He paid a certain price for this; in 1921 the Nobel Committee looked askance at the philosophical cast of Einstein's writing on special and general relativity.

17. **Mike C.**
March 22, 2007

I'm a little confused about the big problem with the landscape. The Standard Model appears to have a huge landscape of physically distinct compactified 2+1-dimensional vacua, many with a radius around 10 microns. A tiny intelligent creature living in one of those vacua would be forced to deal with vacuum selection. We would laugh if such a creature tried to explain the mass of his electrons based on some fundamental necessary principle. And since the existence of these 2+1-dimensional vacua depend only on low-energy physics, any theory that contains the Standard Model at low energies would also have to contain this landscape of 2+1 dimensional vacua.

So why is it a surprise that a theory with extra dimensions should have a landscape of vacua? If it must contain a landscape of 2+1-dimensional vacua, why is it a surprise that it should have a landscape of 3+1-dimensional vacua? And if the Standard Model has a landscape, why are we so upset to see other theories with a landscape, especially since any theory that contains the Standard Model will already have to contain its landscape?

I guess I just don't see the big deal here, but perhaps I'm mistaken!

18. **Chris W.**
March 22, 2007

Mike C.,

1) Please be a bit more specific about the Standard Model's physically distinct compactified 2+1-dimensional vacua.

2) If we were the tiny intelligent creatures in question then we would eventually conclude that the theory is worthless, because our observers and theorists could play this game (until 'O' gets fed up):

> **O**: I have some observations I would like to account for within your theory.
>
> **T**: Given a choice of vacuum X, I can show your observations are fully consistent with my theory.
>
> **O**: Oops, wait a minute, I made a mistake. The data I just gave you was wrong. Here is the corrected data. Can you account for it?
>
> **T**: Sure, no problem. Give me a minute and I'll find another vacuum that will do the trick. (I've got lots of 'em.) ….. Okay, here you go. Vacuum Y will do it.
>
> **O**: What the hell is up? I made up all this data! Can you tell me what you *couldn't* account for? Describe some results that I should *not* observe under specified circumstances—circumstances that I can independently verify! I need predictions from you with minimal wiggle room—none of this vacuum selection crap.
>
> **T**: [Yawn…] Sorry, but that's not the way the game is played these days. There are some older ideas (limiting cases of my theory, in a certain sense) that you might find more agreeable, but I don't find them particularly interesting; they've already been tested. The goal is to achieve agreement with observations, and that's what I'm giving you. If you don't like it then maybe you should consider another line of research.
>
> **O**: You really don't get it, dude. I'll grant you this; you know a lot more mathematics than the average astrologer. A lot of good it does you…

19. **Mike C.**
March 22, 2007

Chris,

I understand that you don't like the landscape. But my point was that the Standard Model has one, whether you like it or not. (Check out a recent paper on the archive for this. Distler also discussed it in his blog recently.) This is the vanilla Standard Model, with its tiny cosmological constant and small (but nonzero) neutrino masses. No SUSY, nothing funny. And this landscape contains a near-continuum of 20 micron-sized 2+1-dimensional vacua. And a tiny intelligent creature living in one of these vacua would never be able to find a theory that predicted the values of the couplings and masses he saw, because we know full well that he's living in just one of many vacua, all of which have different laws of physics.

But since the existence of these vacua is independent of any UV completion of the Standard Model, any other theory that extends the Standard Model at high energies will have to contain this landscape of 2+1-dimensional vacua, and thus, in particular, contain a landscape, whether we like it or not. The only question is whether a UV completion of the Standard Model must contain a landscape of 3+1 dimensional vacua, or if there is a unique 3+1 dimensional vacuum. But that's a pretty strong assertion! (And would be of little consolation to our tiny 1 micron sized friends.)

20. **woit**
March 22, 2007

Mike,

The big deal about the landscape is that it's a framework in which you can't make any experimentally testable predictions, so it's not science. Not being science is a big deal. If you want to do science, you have to come up with predictions that can be tested to see if your theory is right. In the landscape framework, you inherently can't do that.

The Standard Model is a 4d flat space QFT that makes an infinite number of predictions, many of which have been tested to very high accuracy. This is completely different than the landscape, and claiming that it's all the same is just sophistry. We've already had this discussion here, I think a couple times. I haven't looked closely at the Arkani-Hamed et. al. paper about this that recently came out. Maybe there's something interesting there. But if the claim is that it shows that there's no difference between the Standard Model, which is highly predictive and testable, and the landscape, which is completely non-predictive, that is obviously nonsense.

21. **Mike C.**
March 22, 2007

Peter,

So sorry to keep bothering you about all this, but it came up in the comments, and I thought it would be an interesting discussion!

You should check out their paper. It doesn't say that the 3+1 dimensional vacuum of the Standard Model isn't unique. It is, as far as the Standard Model is concerned. The Standard Model predicts a unique 3+1 dimensional vacuum. But the paper does shows that there exists a landscape of physically distinct compactified 2+1 dimensional vacua in the Standard Model, many with physics almost the same as in our 3+1 vacuum for objects that are small enough, and intelligent creatures could conceivably live in them, if they were small enough (~ 1 micron).

So what are these creatures supposed to do? They live in a landscape! Are they supposed to say that landscapes are bad and hence spend eternity looking for a reason why their electrons have the precise mass they do? We large creatures would laugh at them!

But the other is that any theory that contains the Standard Model would have to contain this landscape of 2+1 dimensional vacua, since the existence of this landscape depends only on low-energy physics. So there's no question about theories having a landscape. The only question is whether they have a landscape of 3+1 dimensional vacua. Let's hope there are no 4+1 dimensional creatures laughing at us!

22. **woit**
March 22, 2007

Aaron,

About SUSY/non-SUSY. Sure, you can put in broken supersymmetry as a regulator of the vacuum energy in a non-SUSY theory, then take it to infinity. But in one case it's a regulator, in the other it's a physical, measurable scale. Having a theory with a divergence that you don't know how to regularize without introducing trouble is bad. Having a theory that makes a robust prediction that is off by a factor of 10^60 is also bad, in a different way. Again, personally I think making a flat-out wrong prediction is worse than not being able to make a prediction. If you feel the other way, fine. Hard to argue about it though, they're two completely different things. It just seems a bit ridiculous to me when I hear people saying it is an advantage of supersymmetry that you can calculate the vacuum energy in it, when the result comes out absurdly wrong.

23. **Aaron Bergman**
March 22, 2007

It's really not true when people say that you can't calculate the vacuum energy in non-susy theories. It's a free parameter. The cosmological constant is superrenormalizable, and the divergence can be dealt with in the usual manner. Thus, the cc problem is a fine tuning problem, not a problem of an incorrect prediction. In SUSY theories, the only difference is that the renormalization is finite and the bare term is constrained to be related to the superpotential. From the point of view of effective field theory (which is the only way I understand QFT), it all seems the same to me.

Putting things another way, would you agree that SUSY at any scale below the Planck scale helps the fine tuning problem for the Higgs mass?

24. **Chris W.**
March 22, 2007

The paper Mike mentioned is evidently the following:

Quantum Horizons of the Standard Model Landscape
(hep-th/0703067 — Authors: Nima Arkani-Hamed, Sergei Dubovsky, Alberto Nicolis, Giovanni Villadoro)

25. **woit**
March 22, 2007

Mike,

Sorry, but I still don't see why I'm supposed to be interested in these 2+1 d compactifications. They have nothing to do with the real world, and I don't buy the analogy with the string theory landscape. Sure, maybe we're some random point in some landscape of some 4+1d QFT, or some 10 d string theory or whatever. Could be. But if you want to claim that going on about this is doing science, you have to come up with a way of testing what you are doing. If what you are doing inherently can't lead to a testable prediction, I don't know what you want to call what you are doing, but it isn't science. Does the Arkani-Hamed et. al. paper suggest any way to test landscape ideas? If it does, let's hear it. If it doesn't, any claims it makes about the landscape are not science.

26. **woit**
March 22, 2007

Aaron,

I don't see the relevance of the philosophical argument about fine-tuning. I've repeated endlessly what I see as the difference here. One case involves a physically measurable number (that is wrong), the other doesn't. If that's not an important difference to you, fine, just say so. But then we're operating with different value systems, and aren't ever going to agree on what is "better" or "worse".

27. **Mike C.**
March 22, 2007

These 2+1 dimensional vacua appear to exist, in our real world. And any theory that contains the Standard Model should contain this landscape of 2+1 dimensional vacua. That's a prediction. The Standard Model predicts their existence. So I'm a bit confused about why you're not interested in them. The upshot is that any theory that contains the Standard Model contains a landscape. Do you disagree?

28. **Aaron Bergman**
March 22, 2007

No.

In both cases, there is not an incorrect prediction. One can tune the cosmological constant by balancing the quantum corrections against the bare term in both the supersymmetric and non-supersymmetric theory. (Or, more properly, in the sugra and nonsugra theory.) The cosmological constant problem is always a fine tuning problem. It's just whether you are fine tuning a formally divergent quantity (such as with the Higgs mass) or a finite quantity (such as the Higgs mass in supersymmetric theories.)

29. **Chris W.**
March 22, 2007

Mike, the fundamental issue remains:

> **O**: … Can you tell me what you couldn't account for? Describe some results that I should not observe under specified circumstances—circumstances that I can independently verify! I need predictions from you with minimal wiggle room—none of this vacuum selection crap.

In the presence of the string theory landscape *what is excluded*, and more to the point, what is excluded that isn't already excluded by the Standard Model? If the answer to the latter question is "nothing" then we have an untestable pseudo-answer to the questions left open by the Standard Model. Some people may find this pseudo-answer appealing, but if it had been known 20+ years ago that this is where we were going to end up, few people would have bothered to continue down this path.

(And for those who take general relativity as a theory of spacetime structure seriously, and understand how deeply this viewpoint on the theory challenges the foundations of quantum theory, string theory and its offshoots have very little to offer—unless some major new insights are achieved into string theory's bearing on this question. If so I doubt we'll still be calling it string theory.)

30. **woit**
March 22, 2007

Mike,

No, these 2+1 dimensional vacua do not exist in our real world. Our real world has 4 large dimensions. You're talking about some other worlds which have nothing to do with ours. Again, tell me how I am going to learn anything at all about the real world by thinking about these things. If there's a proposal for this, it might be interesting. If there's not, I just don't see any reason to pay attention to this.

31. **Mike C.**
March 22, 2007

These other vacua can be connected to ours by interpolating geometries. They have to be very far away, though, because their opening angles are small. But if the universe is large enough, then they really must be out there. That's a prediction. Though, obviously hard to test.

32. **Chris W.**
March 22, 2007

These 2+1 dimensional vacua appear to exist, in our real world.

Really? And how would you verify their existence? Let's assume the "Standard Model predicts their existence," as documented (allegedly) in hep-th/0703067. How would this prediction be checked? Bear in mind that most predictions of existence are inherently problematic; a failure of attempted verification can always be dismissed on the basis that one didn't look hard enough. (Question begged: How hard is hard enough?)

33. **woit**
March 22, 2007

Aaron,

Talking about it as a fine-tuning problem, as you say, in one case you are "fine tuning a formally divergent quantity" which isn't a well-defined thing to do, and you can't characterize the size of the fine-tuning. In the other you are fine-tuning something you can experimentally measure, and the size of the fine-tuning required is known and huge. If you think this is an acceptable thing to do, fine, do it and there's no CC problem.

If you think there is a CC problem, then in the second case it has a well-defined size (huge), in the first it doesn't. I just don't buy the argument that it's better to have a huge problem than an ill-defined one.

34. **woit**
March 22, 2007

Mike,

You're not telling me how to experimentally see these other vacua. Do Arkani-Hamed et.al claim that they have new, in principle observable, predictions based on the standard model? If so, that would be much more interesting than empty analogies about landscapes. Where in their paper do they make these predictions?

35. **Aaron Bergman**
March 22, 2007

Talking about it as a fine-tuning problem, as you say, in one case you are "fine tuning a formally divergent quantity" which isn't a well-defined thing to do, and you can't characterize the size of the fine-tuning.

Of course it is well-defined. How does this situation differ from the Higgs mass? The Higgs mass is quadratically divergent. The hierarchy problem is that the measured Higgs mass is much less than the cutoff scale. The cosmological constant problem is that the measured cosmological constant is much less than the cutoff scale. I don't see how you're distinguishing the two cases.

36. **Arun**
March 22, 2007

Mike C.

http://www.math.columbia.edu/~woit/wordpress/?p=412#comment-23310

37. **woit**
March 22, 2007

Aaron,

The difference between the two cases is, as I keep repeating again and again, that in one case you are talking about an experimentally measureable number, in the other case there is no such number (you don't know what the "cutoff scale" is).

In the case of the hierarchy problem, trying to use supersymmetry to resolve it makes sense since the electroweak symmetry breaking scale may not be too different than the SSYM breaking scale (although experiment is on the way to ruling this out). In the case of the CC, it doesn't work at all, in a spectacular way.

This has become a complete waste of time. In one case the problem is characterized by an experimentally measured number that we know to be completely of the wrong magnitude, and in the other, no such number exists. Sorry, but this is a difference. Again, If you don't think it's relevant, fine. I think it is, but there is no way to "prove" that it is or isn't.

This all goes back to your complaints that what I wrote in my book about supersymmetry was not accurate. I don't agree at all. The book gives an accurate characterization of the reasons why supersymmetry breaking is a problem. I'm not going to repeat these here, except to say that getting the completely wrong scale for the CC is one of those problems. This is not some weird idea I came up with, but conventional wisdom in the field.

38. **Mike C.**
March 23, 2007

I'm a little confused. If you have a well-tested theory (and no theory known to humankind is better tested than the Standard Model), and the theory, without modifying it in any way, predicts something that is difficult to observe (in this case, a landscape of 2+1 dimensional vacua, which would appear to us observers as cosmic black strings and other black objects if we found one), are we supposed to reject such phenomena? If the universe is big enough, these things will exist, at least if the Standard Model—with the presently observed c.c. and our best estimates for the neutrino masses—is in fact the correct low-energy description of the universe. Are we supposed to insist otherwise? Unless you modify the Standard Model in some way, then it predicts these phenomena. The Standard Model, as it now stands, predicts the existence of a landscape of vacua.

So, there are two questions here:
1) If the Standard Model predicts the existence of certain phenomena, though they may never be experimentally observed, why insist that they don't exist and that we shouldn't think about them? There may be intelligent life out there in the universe that we will never ever observe, but does that mean that they can't exist? The universe is probably much bigger than the part we will ever be able to see, but does that mean that if a given well-tested theory predicts that it's bigger, do we just reject that prediction?
2) Why do we live in this noncompact 3+1-dimensional vacuum, when there are a huge number of other vacua predicted by the Standard Model with the same physics all the way up to 20 times the Planck scale, at least for objects that are smaller than 10 microns, other than because of the anthropic principle?
3) If the Standard Model predicts the existence of a landscape of other vacua, which can be interpolated smoothly into our own over large spatial distances, and the existence of these vacua depend only on low-energy physics (as is the case), then doesn't any high-energy extension of the Standard Model have to contain a landscape as well, at least a landscape of 2+1 dimensional vacua? So why criticize a theory that has a landscape?
4) If any such extension must have a landscape at least of 2+1 vacua, why is it so hard to imagine that there is a landscape of 3+1 dimensional vacua as well? Doesn't the onus now turn to people to prove that it doesn't happen? Just because it hurts predictivity somewhat, does that mean nature doesn't work that way? I wish I could predict all the species of animals on the earth today, but I can't. It's a bunch of historical accidents. Nonetheless, zoology and paleontonlogy are still a sciences, because we can make predictions about a limited class of phenomena.

39. **Mike C.**
March 23, 2007

By "two questions" I meant four. I can't count—so sue me!

40. **C. G.**
March 23, 2007

Mike,

I think you're getting confused between theory and experiment. The thing is, the standard model is a hugely successful phenomenological theory of particle-particle interactions, but, at the end of the day, it is still just a model. To believe that anything you can derive from it is absolute truth implies an implicit faith in the idea that the model is completely and absolutely correct. This I believe is a mistake, because to render reality to a theoretical artifact is not really the way physics should be done; physics should be based on real experimental observations.

41. **Mike C.**
March 23, 2007

C.G.

You make a lot of demands. Quantum mechanics predicts a lot of stuff that has been experimentally tested and verified. So does GR. But they both also predict a lot of strange stuff—well within in the physical regimes (energy regimes, length scales, etc.) in which they are known to be valid—but that we may never be able to see, and that at least we are not guaranteed to be able to rule out. Are we simply to insist that any such predictions that they make simply don't exist? Do things only exist if they can defintely be observed, even if an astoundingly accurate model predicts that they should exist and they might but not definitely be observable some day? It's like a fortune teller who predicts everything perfectly, but also starts telling you about things that you can't be guaranteed to check with your own eyes.

I'm reminded of what Feynman used to say about all this. He asked why nature should care what we human beings liked. The behavior of nature simply isn't up to any of us human beings. All we can do is try to figure it out and not be too prejudiced.

42. **M**
March 23, 2007

dear Mike C.,
I tried to have a look at the first pages of the paper you suggest, and it seems that what they find is just that the SM compatified to 2d can have one or a few vacua plus a quasi-flat direction i.e. some light scalar. That vacuum arises because neutrino masses are comparable to the cosmological constant, and likely this is an accident that has nothing to do with the string landscape. I don't understand why they choose the name "landscape" for this simple situation. Probably their little 2d animals would have some fun in understanding what goes on, but I fail to see why we 3d animals should get interested in it.

43. **Mike C.**
March 23, 2007

So is this whole debate really about the demand that no prediction of a theory be believed—not matter how well-tested the theory—unless that prediction is guaranteed to be experimentally checkable? I suppose that's a demand you can put on science, but it seems rather restrictive. Who are we to demand that nature must behave that way?

44. **Mike C.**
March 23, 2007

I guess the reason I disagree with that argument is that any theory of nature that addresses really, really high-energy questions is going to make lots of predictions that we cannot be assured of being able to experimentally verify, hopefully along with many predictions that we can be guaranteed to be able to verify so that we can decide if the model is good. But the ratio of the first kind of prediction to the second kind is inevitably going to get bigger and bigger as we approach greater and greater physical extremes. That seems unavoidable. And so should we always pretend that the first kind of prediction—those that may be testable but that are not guaranteed to be testable—just don't exist?

An example. Suppose just for the sake of argument that string theory is correct. Suppose that it suddenly makes a huge number of extremely nontrivial predictions that we know we can test, and we test them and they turn out correct. But suppose that there's idea how to do an experiment that would literally let us see these strings floating around. (It might be possible, but it's not assured that such an experiment is possible, say.) Do we then insist in this case that there are really no strings?

45. **Mike C.**

March 23, 2007

Correction: missing “no”
But suppose that there’s *no* idea how to do an experiment that would literally let us see these strings floating around. (It might be possible, but it’s not assured that such an experiment is possible, say.) Do we then insist in this case that there are really no strings?

46. **Aaron Bergman**
March 23, 2007

I don’t see how we have an experimentally determined quantity anywhere here except for the actual value of the cc (or, analogously, the Higgs mass). The SUSY breaking scale gives us a cutoff with respect to which we can express the fine tuning of the cc. We don’t know what the SUSY breaking scale is. It could be at the TeV scale (which would be nice), but it could be elsewhere. It could be at the Planck scale. And, if there were no SUSY at all, we’d still have a cutoff at the Planck scale. Are you claiming that these two cutoffs should be thought of differently? They’re both experimentally measurable, either through the detection of supersymmetric partners, or quantum gravitational effects.

Are you saying that because we don’t know the physics beyond the QG cutoff, there could be some mechanism that natural accomplishes the needed fine tuning? If so, I don’t see why you couldn’t make a similar argument for the SUSY-cutoff cc. If we don’t know, we don’t know, susy or not. But from the point of view of effective field theory, I don’t think it matters.

47. **woit**
March 23, 2007

Mike C.

You are claiming that Arkani-Hamed et. al. have discovered that the Standard Model implies the existence of “cosmic black strings and other black objects”. What exactly are their properties, and how could we produce them? Where in their paper do they make this claim? I have trouble believing this. The SM is not a quantum gravitational theory and it appears to me that you need to calculate topology-changing transition amplitudes, but lack a theory where such a calculation makes sense.

If string theory make a lot of testable predictions, and they’re correct, the theory is verified, and this has nothing to do with whether you can “see strings”. The problem with string theory has nothing to do with conceptual difficulties about whether or not you can “see strings”. Fine if the strings are not observable themselves. But a theory has to make predictions of some kind, and string theory just doesn’t.

Aaron,

SM + GR tells you nothing at all about how this “Planck scale” cutoff is supposed to work and you have no hope of doing experiments to see what happens. SUGRA gives a very precise understanding of what happens at the SUSY breaking scale, and people are spending billions of dollars to go out and measure this. These are just different situations. Again, for the N’th time, having a theory in which the question is well-posed is different than having one where it isn’t.

48. **Arun**
March 23, 2007

The best analogy I can think of is that in the Standard Model 3+1 large flat dimensions is an experimental input/theoretical assumption/whatever – asking why 3+1 dim in the context of the SM is ill-posed (and maybe even with SM + GR).

It is in string theory that the question of the number of experimentally observable dimensions comes up, and in a nasty way, because the theoretical answer is different.

—
Mike C., the physicists in the 2+1 dimensions in the (SM+GR) world don’t have these interesting theological debates, because in their world, (2+1) quantum gravity is tractable, and so the biggest single motivation for string theory (that it retrodicts gravity) is simply not there.

49. **andrea**
March 23, 2007

Hi all,

assume tomorrow someone comes up with a Rube Golberg compactification of string theory, with a lifetime say about 10^10Gyrs, which fits everything we know about particle physics and cosmology: how’s that different from Newton’s “hypotheses non fingo” about the 1/r^2 dependence of the gravitational forces? It works, and this is all that matters… i’m not saying it’s likely (as a matter of fact, it don’t think it is), i’m just saying i see no reason to get worked up on the landscape issue.

On the other hand, it is very likely that string theory compactifications cannot yield all conceivable quantum field theories at low energies, but only a “small” subset of them (e.g. if something like the “gravity as the weakest force” conjecture holds). Should one prove a rigorous theorem about this and find this subset experimentally ruled out, string theory would be falsified.

To sum up: string theory is undoubtedly way overhyped and has probably slowed down the progress of more down-to-earth theoretical physics by attracting many smart guys, but i don’t find compelling reasons for saying it is not still worth a shot, expecially given the fact that the nonzero CC makes the landscape picture a lot less unreasonable.

50. **Peter Woit**
March 23, 2007

Andrea,

The problem with the Rube Goldberg compactifications is that they are insufficiently rigid to be predictive. It’s not just a matter of finding one that fits what we already know, you have to find one that does that and is rigid enough to make falsifiable predictions. If the failure of every “prediction” can be fixed by going to a slightly different compactification, all you’re doing is coming up with a really ugly way of parametrizing experimental results.

As for the “Swampland”, the problem is two-fold. All the things that people are looking at as supposedly not able to come out of string theory already don’t look like the real world, so you can’t use this to falsify string theory. In addition, swampland proponents have this problem that sometimes when they announce that something can’t come from string theory, experts write into their blog explaining to them how to do it. “You can’t get X from string theory compactifications” often just means that no one has tried hard enough to do so. Yet another addition to the Rube Goldberg chain may do the trick. As long as you don’t know what non-perturbative string theory is, your arguments that “you can’t get that from string theory” are going to be dubious.

51. **M**
March 23, 2007

dear Andrea,

have you read “The Library of Babel” by Borges?

String theory seems to be a similar story. If tomorrow you scan 10^200 string vacua, you can probably find one vacuum that fits all the physics we know, and you get a Nobel prize. If on sunday you finish the work scanning the 10^500 remaining vacua, you can find 10^300 more vacua that fit everything, and you get an igNobel prize.

52. **Mike C.**
March 23, 2007

Arun,

You are not correct. These vacua only look like they’re 2+1 dimensional to large creatures like us. Tiny creatures, however (~1 micron), would see 3+1 dimensions, with one dimension wrapping around if they move along it far enough. Their physics—including gravity—is 3+1 dimensional and looks almost exactly the same as ours, all the way up to energies 20 times the Planck scale (when you start making black holes bigger than 20 microns) except for variations of all their coupling constants and masses. Read the paper to see why. So gravity is no more tractable for them than it is for us, and they still would have no explanation for the values of those coupling constants.

53. **Mike C.**
March 23, 2007

I must say that I’m a little bit disappointed with many of the commenters here. It seems like a lot of people here are making assertions and demands without having done their reading. People read the first two pages of a paper, for example, essentially just to find things that support their argument, and then start spouting things that aren’t true. A lot of the comments indicate that people don’t understand or haven’t read the relevant material. I’m feeling a lot of prejudice here. Can’t we all be a little more open minded?

54. **woit**
March 23, 2007

Mike,

You’ve come here, posted 13 comments, full of sophistry about the landscape which you base on a 44 page paper, which you refuse to answer my questions about. You seem to expect me and others to drop what we are doing and go read this 44 page paper. Sorry, everything I heard about it indicates to me that my time would be better spent reading other papers. It’s not even the topic of this posting at all. This isn’t a general discussion forum where you can just appear, start going on about what interests you, and expect others to go out and spend their time thinking about what you want them to think about, unless you’ve got a lot more compelling argument.

55. **Brett**
March 23, 2007

I don’t think it’s especially interesting that there is a “landscape of vacua” in theories in which some of the space-time directions are compactified. I say “landscape of vacua”–in quotes–because at the quantum field theory level, the different compactifications correspond to different theories; the underlying space-time is an element of the definition of the field theory. In a quantum gravity theory, maybe these are different states of the same underlying theory, but the standard model is not a quantum theory of gravity.

It’s no real surprise that by choosing different compactifications, one can get significantly different physics, because the compactifications themselves are ingredients in defining the theory. The parameter space of the theory includes the space of compactification manifolds. In order for these theories to be interesting physically, some additional conditions must be met; otherwise, we’re just adding extra free parameters. If there were any experimental indications that some of the dimensions of our universe were compact (either one of the four we see, compactified on a large scale or additional ones on small scales), this would be a very interesting subject. Or if having compact dimensions allowed a natural solution to some kind of hierarchy problem. Of course, there are various proposed “solutions” of such hierarchy problems using extra dimensions, but they are not natural. If we clean up a known hierarchy by introducing unnaturally sized extra dimensions, we haven’t solved anything, just moved the fine tuning into a new set of free parameters we’ve introduced just for that purpose.

If we want to allow for the possibility that one of the four dimensions we see is actually compact, and that somewhere far away in the universe, the corresponding compactification scale is quite small, that’s fine. This introduces a new generalization of the standard model, and maybe it’s what the universe really looks like. But it’s very much like introducing a new sector of particles near the Planck scale, with new free parameters, but hardly any new predictions for feasible experiments.

56. **M**
March 23, 2007

Mike C., I don’t see what these tiny creatures can teach us. In the case of strings, 10^500 vacua come out because you start with fields with a few 10d Lorentz indices and compactify many dimensions. These ‘features’ are not present when you compactify the SM to 2+1 dimensions.

Anyway I agree that theories with compactified dimensions tend to have many vacua, as discussed e.g. in the book by Smolin.

The true problem is: can we get good physics from the big string landscape? If the best we can do is debating about tiny creatures, then better to move to biology.

57. **dan**
March 23, 2007

Hi Peter,

you wrote “As long as you don’t know what non-perturbative string theory is, your arguments that “you can’t get that from string theory” are going to be dubious.”

How do you feel about physicists pursuing a non-perturbative string theory as a frontier of HEP?

thanks

58. **Peter Woit**
March 23, 2007

dan,

Finding a useful non-perturbative string theory is the big problem in string theory, and many people have worked on this over the last few decades and continue to do so. It would be great if there were progress on this, I just don’t see much recently.

59. **andrea**
March 23, 2007

brett,

the paper on standard model landscape is actually on standard model + general relativity landscape, so spacetime geometry is genuinely dynamical… that said, it goes neither here nor there as far as physics is concerned.

peter,

i don't get the rigidity argument: a single unit switch on one of the handles of the Rube Goldberg machine will generically lead to very large changes of the low energy physics (different number of flavours, or different gauge groups or planckian change in the CC). Nearby vacua don't give nearby physics.
If at least one vacuum is found which matches the physics we know, then one can make all sort of predictions: if they are verified we keep it and make other predictions, if not one is free to look for another one (typically very far away). No need to say that there may exist no vacuum with the right properties, but if Nature actually worked that way i'd be somehow disappointed, but i would not be shocked.

I've been following this debate for a while, and some string theorists have way passed the line of a civil exchange of ideas, but aside from that, the string community rightfully shows off its achievements (overhyping is unavoidable in the free market of ideas) and attracts smart guys because it is a very fashionable subject. The only way to put an end to this fashion before it eventually dies out is to take a swim in the swampland and falsify it. Sort of a catch22…

60. **Peter Woit**
March 23, 2007

Andrea,

For the reasons I mentioned, I just don't think the "swampland" has anything to do with actually falsifying string theory. If it did, maybe I would work on it… The string theory backgrounds people have most closely investigated are the ones that look most like the standard model, there are all sorts of these and absolutely no argument I am aware of that suggests the possibility of ruling out the standard model as a low energy effective theory for some string theory.

I just don't believe that we're going to find a single specific background that matches the standard model (which would then predict other things). There are solid arguments that it is inherently impossible to even identify those backgrounds with the right CC, much less the right CC and everything else. If one figures out how to overcome this, it would have to be by being able to precisely figure out the implications of large classes of vacua at once. If one can do that, I don't see why one won't end up with a large number of vacua with the SM properties, not just one or a small number.

In any case, given the current state of the theory, this kind of discussion is pretty much a theological one. I just don't think one can deny that what is happening is a classic example of failure: simple models disagree with experiment, so you are forced to more complicated ones. These are designed to be so complicated that you can't analyze them and confront them with experiment. Once this happens you are supposed to give up, not conduct philosophical arguments trying to justify continuing.

61. **Anon**
March 23, 2007

"You seem to expect me and others to drop what we are doing and go read this 44 page paper. Sorry, everything I heard about it indicates to me that my time would be better spent reading other papers."

And you complain that Clifford Johnson won't read your book?

"It's not even the topic of this posting at all."

But you've devoted space in previous blog postings to trashing this paper that you haven't read.

62. **Peter Woit**
March 23, 2007

anon,

Actually, I haven't mentioned the Arkani-Hamed et.al. paper in any of my postings. You're quite right that if I was writing a posting about it and not reading it, that would be reprehensible.

What I have done is responded to various commenters here who have written in claiming that "Arkani-Hamed et. al. show that the SM has the same landscape problem as string theory" by telling them that, while I don't know what is in the paper, that statement is obvious nonsense, and explained why. I've also asked them to back up what they were saying, telling me what the evidence for this claim was from the paper, but haven't gotten an answer. Presumably this is because the paper doesn't really make that claim.

Again, there are lots of interesting things I don't have time to learn about, so the properties of the SM compactified on very non-physical backgrounds just isn't very high on the list.

People write in here everyday with all sorts of claims based upon all sorts of misunderstandings, quoting lots of different papers. I'm not about to read them all, but will continue to point it out when people are spouting nonsense.

63. **Anon**
March 23, 2007

"while I don't know what is in the paper, that statement is obvious nonsense, and explained why."

It seems to me that MikeC has both read and understood the paper.

Aren't you on pretty thin ice calling his account of what's in it "sophistry" ? Especially, since it mirrors what previous commenters have said and which you have previously called "nonsense."

64. **Tim**
March 23, 2007

Peter,

You quot a reporter from the event:

"The trouble with physics, Duff began, is with people like Smolin…"

This posting of yours demonstrates very well that you are no different than the bad journalists and popular science reporters that you often complain about. This whole blog is full of misquotations and misinterpretation like the one above just as there are loads of misquotation and misinterpretation in bad populer science papers. I hope this will convince you and the audience here that what you are doing is not science but bad popular scientific journalism.

If you would have cared to check your facts — indeed a minimal requirement for any journalist — you would have found that what Michael Duff has said is the following:

"The trouble with physics, ladies and gentlemen, is that there is not one Lee Smolin but two."

Michael Duff went on explaining that the speaker before him — Lee Smolin — presented arguments which are perfectly fine and one can only agree with them. On the other hand his book is not of this nature but has rather many inaccuracies and went on explaining what he disliked in the book. Hence his opening sentence "there is not one Lee Smolin but two", which in the light of true facts becomes something completely different than what you quoted in your posting. The mp3 audio from the event is available to everyone including good and bad popular science writers but of course only the good ones will take the initiative to actually check it.

Here it is: http://www.rsa.org.uk/audio/lecture050307.mp3 and it was avaiable at the time of your posting.

Now I'm sure you will be ready to explain us all that your blog consists of hundreds of postings and so few have misquotations and misinterpretations such as the one above so everything is okay. Just the same argument any journalist or popular science magazine would pull when confronted with lies and distortion in their magazines "oooh, we have been publishing thousands of articles in the last 20 years and it's actually a proof of our high standards that only a couple of articles are completely false" and of course they would not be discomfortable with pulling the same argument over and over again and no matter how many times they are caught they would still continue to publish their magazine because that is what they live on.

The same behaviour that one can observe in the attitude of corrupt politicians. Although they might know they are not telling the truth and they know that they will continue to be distorting events but they have no other choice because that is what they live on and they are not couregous to say "okay, I've been caught too many times, it's time to stop and actually look for a real job".

Best regards,
Tim

65. **Tim**
March 23, 2007

Oh, and I forgot the always winning argument "some of my opponents were telling bigger lies than me so why should I resign?"

Best,
Tim

66. **Chris Oakley**
March 23, 2007

> "The trouble with physics, ladies and gentlemen, is that there is not one Lee Smolin but two."

Or there could be just one Lee Smolin, the illusion of two being a result of lensing by galactic-scale superstrings.

67. **Peter Woit**
March 23, 2007

Tim,

What I put in quotes is not a misquotation. It is an explicit quote from the report by someone who was there that I linked to. It does not purport to be a direct quote of what Duff said. It is a direct quote of how that person chose to characterize what Duff said. It reflects precisely how that person interpreted what he was hearing Duff say.

If you can point to a legitimate misquotation, even one, anywhere in the thousands of pages I've written here, please do so. Otherwise you should not make the kinds of accusations you are making. The one you mention was completely accurate, and included reference to the source it came from, so people could determine for themselves its reliability. Sure, they can also listen to the audio file, which I also linked to, and judge for themselves whether that report was accurate.

68. **Peter Woit**
March 23, 2007

Anon,

For about the 10th time, what I called sophistry is the claim that "the SM has the same landscape problem as string theory". I explained repeatedly here and elsewhere why that claim is sophistry and nonsense.

69. **Tim**
March 23, 2007

Peter,

I think we are getting into a problem in linguistics. The statement "your posting contains a misquotation and is hence misleading" holds. I did not say that you quote someone inaccurately, indeed, you quote the bad reporter accurately. The bad reporter on the other hand does misquote Michael Duff and hence your posting does contain a misquotation.

A good journalist certainly does not want to quote people saying misleading statements without clarifying that the person just quoted was indeed saying something misleading because if he did so he would himself be misleading his audience.

This is just what has happen in your case, you uncritically have taken over a quotation from someone that is very misleading and even insulting to a third person. You did not research the subject of your post although you have known full well that the quotation you quote will put a third person into an embarrassing light.

It is indeed the duty of every good journalist to give an impartial view, give the chance to anyone attacked by any of his quoted respondents to defend him/herself and check whether his sources are accurate or not. Failure to do so due to the fact that the respondents and other sources of his article are supportive of his preconception results in poor, biased, partial and activism oriented journalism something that is very far from a professional stand point.

In simple terms: repeating a lie (although accurately giving the source) without actually checking it and commenting on its untruefulness is called: propagating lies.

Best,
Tim

70. **Anon**
March 23, 2007

"For about the 10th time, what I called sophistry is…"

You accused MikeC of engaging in "sophistry" when all he has done is (accurately) recount what is in Arkani-Hamed et al's paper.

That's pretty much the same thing as accusing Arkani-Hamed et al of engaging in "sophistry."

But you'd never do that … because you haven't read their paper. Right?

71. **Q**
March 23, 2007

'In simple terms: repeating a lie (although accurately giving the source) without actually checking it and commenting on its untruefulness is called: propagating lies.' – Tim,

Tim, you've just contradicted yourself. You earlier stated that you want Peter's quote to be corrected to Duff's lying claim to the effect there are two Lee Smolins!

If you want quotes to not include lies, then it would be impossible to quote much said by some defenders of string theory.

For example, is the following quote a lie, because string theory predicts unobserved gravitons (instead of really predicting real gravity):

'String theory has the remarkable property of predicting gravity.' – Dr Edward Witten, M-theory originator, Physics Today, April 1996.

Should I 'correct' Witten's remark when I quote it, so I'm not propagating lies myself. Say to something like:

'String theory has the stupid property of predicting unseen gravitons and nothing checkable about gravity.' – Dr Edward Witten, M-theory originator, Physics Today, April 1996.

Would that make things nice and accurate in your view? 😉

72. **Tim**
March 23, 2007

Q,

Thank you for taking time to respond. No, that would not be accurate. The following would, if I suppose that you think that the quote from Witten is wrong:

" 'String theory has the remarkable property of predicting gravity.' – the above quote is from Dr Edward Witten, M-theory originator and has appeared in Physics Today, April 1996. I don't agree with Dr Witten's opinion for this and this reason."

In order for everyone to understand, the professional way for Peter to write his posting would have been this:

""""
According to the report from one attendee, after Smolin started things out by arguing his case:

Smolin sat down. Duff stood up. It got nasty.

The trouble with physics, Duff began, is with people like Smolin…

Now it turns out that I have checked what Duff said and the reported quoted is above is in fact lying, because Duff said

"The trouble with physics, ladies and gentlemen, is that there is not one Lee Smolin but two."

My opinion is however that Duff is wrong in saying the above because of this and this reason.
""""

Is it clear now?

If not let me give another example. I'm living in Germany so let's imagine I hang posters all over Berlin with the following content "Jews are killing Christian babies – see: Protocols of the Elders of Zion". Am I quoting accurately? Sure I am. Can people check the facts? Sure they can and they will find exactly what I am stating, namely that if they look up the Protocols of the Elders of Zion, they will find the quote I chose to post. Am I propagating lies? Sure I am. Is it impossible to quote from the Protocols of the Elders of Zion? Sure it isn't, all I need to do is make clear that it is a forgery something along the lines: "Jews are killing Christian babies – as can be read in the well known forgery Protocols of the Elders of Zion which nobody takes seriously in a civilized country"

I'm going to so much detail because I still believe that people like Peter can be convinced to change track and do somthing useful for the physics community instead of propagating lies and becoming irrelavant laughing stocks.

Best,
Tim

73. **Peter Woit**
March 23, 2007

Anon (and why is it that all you guys are anonymous, anyway?)

For about the 11th time, the claim that "the SM has the same landscape problem as string theory" is sophistry, see earlier iterations for the explanation why. It's sophistry whoever is making it, but the ones I know about are the ones writing in anonymously to my blog.

Tim,

In the quote I repeated, Duff's words are not in quotes and it is pretty clear that the person who wrote it is not giving a transcription of what Duff actually said, but the impression it left him with. He makes clear that is what he is reporting, and he explains why. Listening to what actually happened I think one can understand why this person had that impression of Duff's remarks.

I'm extremely careful in what I quote here. If something is in italics or quotation marks, it is an accurate quote from a source that has been indicated, and I have taken trouble to make sure that it is accurate and not taken out of context. In this case, what I wrote starts with "According to..", and explicitly gives the source. Everything I wrote is completely accurate.

On the other hand, you choose to make the accusation "This whole blog is full of misquotations and misinterpretation" and are not able to give any evidence of this. I think you have no evidence for this, and for you to make that kind of accusation is dishonest.

74. **The man**
March 23, 2007

Peter,

You often mention that *most* string theorists are reasonable and the type of string theory partisans that (often anonymously) post here are a strong minority.

After seeing the sampling of anonymous posts here, and at Clifford's, I'm beginning to doubt that. Where's the evidence the most string theorists don't behave like this?

75. **Anon**
March 23, 2007

"For about the 11th time, the claim that 'the SM has the same landscape problem as string theory' is sophistry,…"

Nowhere in his comments here does MikeC use the phrase "landscape problem." Nor does the phrase appear anywhere in Arkani Hamed et al's paper.

And yet you accused MikeC of engaging in sophistry.

As far as I can tell, he has given an accurate rendition of what's in the paper. So, if anything, you are accusing Arkani Hamed et al of sophistry.

Which might be OK if you had read their paper.

76. **Aaron Bergman**
March 23, 2007

At the risk of being annoyingly socratic, let's say that there was susy at 100 TeV. Would you say that this ameliorates the fine tuning in the Higgs mass?

77. **Mike C.**
March 23, 2007

I'm a little disappointed. I visited this blog, and saw that there was a mention of the landscape in the original blog posting. I was curious if the folks here had seen the recent work showing that the Standard Model has a landscape of a very similar form to that found in string theory, consisting of a continuum of lower-dimensional vacua. I pointed out a recent paper on the subject, which demonstrated the existence of these physically inequivalent vacua, and showed that they are perfectly acceptable solutions to the Standard Model + gravity with interpolations to our 3+1 dimensional vacuum. The paper also had the upshot of showing that any theory that contains the Standard Model must contain this landscape of 2+1 dimensional vacua, since their existence depends only on low-energy physics, and in particular, any theory containing the Standard Model has a landscape, by definition of what the word landscape means.

Instead Peter has repeatedly called me a "sophist" and people have told me that they can't be bothered even to look at the paper and clearly aren't even reading my posts. Well, fine. Perhaps I'm a sophist. But Peter, you're a dick.

I've got better things to do with my time too. Perhaps one day you guys will stop just for a moment and question your assumptions and prejudices. More likely, you're too confident in your ideology. I feel sorry for you.

Cheers,
Mike

78. **Jean-Paul**
March 23, 2007

I looked at the paper on 2+1 landscape. It deals with another nonsense extension of SM. Nonsense in, nonsense out.

79. **woit**
March 23, 2007

Aaron,

Maybe it's just late at night, but I can't even tell what point you are trying to make, or how what you are asking is responsive to my last comment here (is it?). I think it has been quite a few iterations since any substantive new point was made by either of us.

Mike,

I spent a lot of time trying to have a discussion with you, involving 13 comments here by you on my blog. You completely refused to address the points I made in response to you or answer the questions I asked you about the argument you were making, instead just endlessly repeating yourself and insulting me.

The Man,

On days like this, I must say that it becomes hard to maintain the point of view that most string theorists are reasonable. I still choose to believe that there's a silent majority out there, and the anonymous commenters don't reflect the behavior of most string theorists. My experience dealing personally with string theorists is very different. Only on very rare occasions have I run into unreasonable behavior in personal interaction. Anonymous blog comments unfortunately I think encourage this. You can behave like a complete jerk at no personal cost to yourself or your reputation by hiding behind anonymity. That's why people generally choose to do this.

80. **Aaron Bergman**
March 24, 2007

I'm just trying to understand how your philosophy applies to the case of another fine-tuned parameter.

81. **a**
March 24, 2007

dear Mike C., I read the section about "The SM landscape" and I confirm that the landscape exists only in the title: what they find is one 2+1d vacuum with one light scalar. A honest landscape is much bigger.

82. **Peter Woit**
March 24, 2007

Aaron,

It seems to me you keep ignoring the point I'm making (you don't have to agree with it, you just have to acknowledge that it's there), and trying to discuss something else.

As for the hierarchy problem, it's different, but the existence of a physically measureable 100 Gev or so SSYM breaking scale is also relevant. In the hierarchy case it's a feature (you're trying to use supersymmetry to explain why the Higgs is around 100 GeV), in the CC case it's a bug because it's the wrong scale.

All Sorts of Links

March 23, 2007
Categories: Uncategorized

The LHC is the cover story on this week's issue of Science magazine, with three articles on the topic here, here and here.

Also in this week's Science is an article about the "spin puzzle", the fact that accelerator experiments with polarized particles give results for protons that are different than what one would expect from a naive quark model. The general assumption seems to be that this is a QCD effect, one that is tricky to calculate. I've always wondered if there is any chance that there is some sort of spin-dependent behavior of quarks different than that predicted by QCD. I don't know of any work by people trying to come up with such models, but maybe it's out there. I'd love to hear from some expert on this about whether the experimental results really do point to a serious possibility of something going on other than standard QCD.

A new book of interviews of scientists has recently appeared, Candid Science VI by Istvan and Magdolna Hargittai. It contains interviews with David Gross and Frank Wilczek. The authors ask both of them about their interactions with Wigner, and what they think of various other famous Hungarian scientists. Wilczek explains why he has made various moves over his career, that he was quite influenced by Peter Freund as an undergraduate, why he thinks it took so long to get the Nobel prize, and that his motivation for working on the beta-function calculation was to know if the electroweak model had the same Landau pole problem as QED.

Gross talks about his background and relation to Judaism, and also about his Nobel prize work. He remains enthusiastic about string theory, and characterizes opposition to string theory in many physics departments as due to people not wanting to learn it because it is hard work, as well as fear that if they hire string theorists, all the good graduate students will go work with them. There may be something to what he says, but I think it's out of date, and times are changing.

I hear from David Derbes, who put together Dyson's 1951 Lectures on Advanced Quantum Mechanics that were mentioned here earlier, that World Scientific is publishing them as a book this month. Profits will go to the New Orleans Public Library, where David grew up.

The two new Fields medalist bloggers each have fascinating blog entries on Millenium problems. Terry Tao writes a long explanation of Why Global Regularity for Navier-Stokes is Hard. He also comments about the recent New York Times piece about him and about math education issues. The comment sections of his postings have some very interesting discussions going on.

Alain Connes has a wonderful posting about Le reve mathematique, especially his mathematical dream of proving the Riemann hypothesis using non-commutative geometry. He notes that the first goal is to come up with a non-commutative geometry version of a proof for the function field case. More about this in a recent posting on the same blog by David Goss.

Comments

1. **Thomas Love**
   March 23, 2007

   I was delighted to hear that David Derbes, who put Dyson's 1951 Lectures on Advanced Quantum Mechanics into LaTex was contributing the proceeds to the New Orleans Library. I met David when I taught at Tulane University. He wrote his PhD under the direction of Higgs. He was teaching at the high school in N.O. from which he graduated, as a form of service. I was quite impressed by his knowledge and his discription of particles as broken symmetries help me in my work.

2. **Uncle Enzo**
   March 23, 2007

   description, not discription.

3. **wolfgang**
   March 23, 2007

   Peter,

   I am certainly not an expert on this, but I know that the so called "proton spin crisis" has been discussed for many years (at least since the 90s) and there are tons of papers and various proposals about it. You find them e.g. by searching for "proton spin crisis" on the arxiv or at Google scholar. I was always surprised that this "crisis" did not get much attention outside of the lattice-QCD and related communities, because (as you already wrote) it might very well indicate that something is wrong with QCD as we know it.

4. **Peter Woit**
   March 23, 2007

   wolfgang,

   I've also noticed papers on this over the years, periodically tried to read more about it to see if there was any chance there was something wrong with QCD. Never got far with this, so I was hoping to find a real expert who had some feel for the situation. I agree that it's surprising this problem has not attracted more attention in the particle theory community. Even if it's not evidence that will upend the standard model, it seems to be one of the least understood corners of particle physics, with experimental anomalies to guide one to better understanding, and new data coming in.

5. **Kris Krogh**
   March 23, 2007

   Peter,

   Quote from a web page on the standard model of particle physics:

   "So far every experiment test of the standard model has confirmed the predictions of the theory."

   I think I've seen similar statements on this blog. In light of what was initially called the proton "spin crisis," which has not gone away, how can this be said?

6. **Brett**
   March 23, 2007

A first principles calculation of how the spin of the proton is distributed among its constituents would be extremely difficult. So there is no really good prediction coming just from QCD to compare things to. Assuming that almost all the spin is on the valence quarks (or rather, appropriately dressed "constituent quarks") turns out to give quite good predictions for such things as the neutron/proton magnetic moment ratio. But if you look at the scattering off the quark spin, this model gives wildly wrong answers. So this very simple-minded model is wrong. What are needed (and what people have worked on developing) are calculationally tractable models that capture more of the essential features of QCD and explain why very simple models can give accurate results for specific quantities.

7. **Kris Krogh**
March 24, 2007

It seems when a simple-minded calculation agrees with standard model, that's taken as a vindication. If not, no crisis, we just need to do more work. Heads I win, tails we flip the coin again.

8. **lostsoul Ph. D.**
March 24, 2007

Ther is currently a paper that purports to disprove the RH, posted on the arXiv at

http://arxiv.org/abs/math.NT/0703367

This has not received the media attention that the cataloguing of E8 has (though there has been a rustle in the hedgerow of the blogosphere); is this because it may be wrong (I am not qualified to judge, though the approach looks pretty much 'old school' to me) or perhaps down to the author's lack of a good PR unit?

9. **modestproposal**
March 24, 2007

great blog but may I suggest that you put a description and some few key words in your html so that when searching on google you get an idea of what the blog is about – like lubos's "The best theoretical physics blog that the search engine can offer you".

10. **Chris Oakley**
March 24, 2007

Kris,

Yes, but since we do not really know how to do bound states for QFT in general and the Standard Model in particular the proton spin crisis is only a crisis for ad hoc, string-and-sealing-wax models used to plug this gap in our understanding.

11. **woit**
March 24, 2007

lostsoul,

That paper on the RH has all of the hallmarks of a wrong paper: dramatic claims to do something no one else has been able to do in over a hundred years, no evidence of a new idea that other people haven't looked at, a long technical argument, full of opportunities to go wrong. Remember, papers on the arXiv are not refereed, and even when they contain errors, the authors do not always acknowledge this and retract them. I have no idea if an expert has tried to go through that paper and the kind of problem most would expect to exist.

12. **Kris Krogh**
March 24, 2007

Hi Chris,

Your comment reminds me of an item from the Feynman biography by Gleick:

When a historian of science pressed him on the question of unification in his Caltech office, he resisted. "Your career spans the period of the construction of the standard model," the interviewer said.

" 'The standard model,' " Feynman repeated dubiously. . . .

The interviewer was having trouble getting his question onto the table. "What do you call SU(3) X SU(2) X U(1)?"

"Three theories," Feynman said. "Strong interactions, weak interactions, and the electromagnetic. . . . The theories are linked because they seem to have similar characteristics. . . . Where does it go together? Only if you add some stuff we don't know. There isn't any theory today that has SU(3) X SU(2) X U(1) — whatever the hell it is — that we know is right, that has any experimental check. . . . "

13. **Peter Orland**
March 24, 2007

The spin puzzle is neither a verification or a disproof of QCD. In the simple quark model, the quark masses in a nucleon are consituent masses. These are much greater than the current masses, which are actual masses. Most of the constituent mass is glue and virtual quarks – which should have an effect on any experiment measuring spin or magnetic moments. Nobody has calculated how this works, I believe.

14. **Doug Natelson**
March 28, 2007

Peter – Frank Wilczek gave a talk yesterday at Rice, and I asked him about the QCD spin situation. I have a new blog posting discussing this. In short, he says there is no problem with the QCD calculations or measurements; both say that the proton ends up as spin-1/2. Rather, the problem is that people's intuitions don't like that the calculations say that much of that spin comes from the gluon field.

15. **Peter Woit**
March 28, 2007

Thanks Doug!

(His posting is at http://nanoscale.blogspot.com/2007/03/frank-wilczek-talk-part-two.html)

16. **Jennifer Kovinski**
April 18, 2007

Navier Stokes Equations and Euler's Equations.
It's very brave of Penny Smith who persists in working on one of the hardest and still fruitless areas of mathematics–namely the Navier-Stokes and Euler's. From what I know, there are now only a handful of mathematicians who are still working on it: Peter Constantin is the most stubborn one among them. If you look at Clay Math's official problem description, you will notice that there are virtually no significant results. Humans have only moved a few steps in a thousand miles of the journey, yet, the problem seems to be that, the road doesn't exist, and one has to find a road to move forward. Sadly, all those leading experts in this field now become inactive simply because of the difficulty posted by the problems. I admire Dr. Smith's persistence. Her works are certainly good contributions to the field. I read about her papgers and preprints, left alone the mistakes, it doesnt seem to me that even the mistakes were abscent, each of the claims (correct or incorrect) still need to be proved, which is an extremely essential process. I, as well as my colleagues in Europe, don't believe that the claims have any usefulness in resolving the Navier-Stokes Equations. But nevertheless, good efforts. It's inspiring–hope more mathematicians would try it…

Is String Theory Testable? (Part II)

March 24, 2007
Categories: Uncategorized

I recently noticed that, around the same time I was preparing my slides for a talk about [Is String Theory Testable?](), Michael Douglas was doing something similar, preparing a talk on [Are There Testable Predictions of String Theory?]() There's a certain amount of overlap in our presentations, and people might find it interesting to compare them.

Douglas goes over much the same story I do, but reaches different conclusions. For him, string theory does "make predictions", just lots and lots of incompatible ones, so the problem is that:

none of the ideas which have been suggested so far are guaranteed signatures of string theory. We would be happier with one prediction, which could lead to a decisive answer either way.

This is the sort of thing I would call a prediction, so I guess we agree that they don't exist. Douglas ends by noting that the one way he can think of to get such a prediction is through a statistical argument based on counting vacua and the dynamics of eternal inflation. He doesn't mention the argument given in my talk that this is already falsified (by the lifetime of the proton), or that such arguments inherently lead to calculations that are inherently intractable and can never be done (this argument is due to him and Denef, it is surprising that he doesn't mention it).

Along the way, Douglas does make a couple claims about things that he thinks the statistical anthropic landscape arguments disfavor, especially varying constants and large distance modifications to gravity. Seeing these would falsify our current theory, but would not falsify string theory, since it can accomodate them, although perhaps not within Douglas's statistical framework.

One of the more surprising responses I've seen to my recent claims that string theory has failed as an idea about unification because it's inherently untestable comes from [Mark Srednicki](), who writes (in the context of mentioning an MSNBC interview with Brian Greene):

We see that the big issue for Brian, and for just about all scientists (though with the apparent exceptions of Lee Smolin and Peter Woit), is what is TRUE. Not what corresponds to some philosophy of what science is or is not. Lee writes that the landscape must be rejected because "it would mean the end of our field" (page 165). It should be obvious that this is not the basis that is traditionally used for accepting or rejecting a theory! Peter's (essentially the same) argument that string theory must be rejected because (at the moment) it does not appear to be sufficiently predictive (for Peter) is also irrelevant to the question of whether or not string theory is TRUE.

If the landscape is right, we may never get anything more than circumstantial evidence that it's right. But that's often the case in science. We've been spoiled in particle physics by having extremely precise data and highly predictive and quantitative theories for the past few decades. Most of the rest of science has not been so lucky. Perhaps we will not be so lucky going forward. The only way to find out is to do more work and see where it leads.

Srednicki's reaction to the lack of testability of string theory seems to be that testability is not what matters. What matters is what is TRUE, and it's perfectly logically consistent for string theory to be of such a nature that we can never test it. The problem with this point of view is that science is not so much about what is true as about how one knows what is true about the world. Religious believers are also interested in what is true and think they know what this is, but science is different since it provides a means for deciding what is true. Scientific ideas about the universe are true when they make predictions that can be checked in a convincing way. Ideas that can't be experimentally checked in some way, directly or indirectly, may or may not be true, but they're not scientific ideas, rather something of a different nature.

Comments

1. **Tim**
   March 24, 2007

   Peter,

   "….. the one way ….. to get such a prediction is through …….. this is already falsified (by the lifetime of the proton)"

   Ooooops, did you just say that string theory has been falsified? Consider changing the title of the blog to "Wrong".

   Best,
   Tim

2. **hack**
   March 24, 2007

   Srednicki is really making a fool of himself. History has not looked kindly on people who were sure they knew what was TRUE and didn't see the need to prove it.

3. **Robert Musil**
   March 24, 2007

   Lee Smilin has posted a fairly extensive rebuttal to Srednicki, including to Srednicki's (mis)use of the quote from Smolin's book and Srednicki's suggestion that scientific "TRUTH" can be divorced from falsifiable predictions – with most scientists favoring the divorce. Here:

   http://asymptotia.com/2007/03/13/more-scenes-from-the-storm-in-a-teacup-vii/#comment-34765

   Some excerpts:

   "Dear Mark,

   "Your selective quotation of me badly misstates my position. Not only do I acknowledge that the landscape is a possibility, I invented the idea, named it and was the first to explore its consequences, in papers from 1992 on. My issue, since then has been how we can continue to make falsifiable predictions if the landscape is true. …. I hope my point of view is clear: the landscape may or may not be a real feature of string theory-evidence is that I was right and it is. But if it is we are not relieved of our obligation to test the theory by making falsifiable predictions for doable experiments. There is at least one scenario that stands both as an existence proof that this can be done and as a challenge to observers to falsify. Any newer proposal for doing physics on the landscape then has to do at least this well.

   "Thanks,

“Lee”

4. **tomj**
March 24, 2007

I thought I had included this quote on the previous thread on this subject, but it isn’t there. Probably too long?

Anyway, from pg 22 of Albert Einstein’s _Relativity_:

“We thus require a definition of simultaneity such that this
definition supplies us with the method by means of which, in the present case, he can decide by experiment whether or not both the lightning strokes occurred simultaneously. As long as this requirement is not satisfied, I allow myself to be deceived as a physicist (and of course the same applies if I am not a physicist), when I imagine that I am able to attach a meaning to the statement of simultaneity. (I would ask the reader not to proceed farther until he is fully convinced on this point.)”

My question is that in the case of String Theory, if there are definitions being used, do they satisfy the above requirement? Can String Theorists point to any concept they use, and words they use whose definition includes a method of deciding something significant?

5. **John Armstrong**
March 24, 2007

I agree completely that the debate isn’t about what is true so much as how physics is done. You emphasize the epistemological nature of science as a method of coming to knowledge about the world.

There’s a parallel, altogether dirtier line, though: the debate is about what physics we will choose to fund. The danger isn’t that physics will wander in the Landscape with no guiding stars, it’s that we will spend all of our time (and money!) wandering while we could have spent it on other possible lines of research.

6. **Chris W.**
March 24, 2007

..and of course the assertion of the Landscape’s existence must be TRUE, because its existence follows from principles that we know to be TRUE, right? Just as a reminder, what are those principles again? And how, again, are we to be *assured* of their truth, and *know their proper application and interpretation*?

7. **Chris Oakley**
March 25, 2007

In abandoning Occam’s Razor in favour of brand loyalty to a particular speculative idea Srednicki drives another nail into the coffin of Scientific Method. To take the most optimistic analogy, supposing that Newton had proposed Lorentz Contraction and Time Dilation in 1700. This would be “true”, or at least more true than the dynamics he actually did propose, but with what justification could anyone living then abandon commonsense notions of space and time if they are not presented with a compelling reason to do so?

8. **Peter Shor**
March 25, 2007

When Marc Sredinski writes

> If the landscape is right, we may never get anything more than circumstantial evidence that it’s right. But that’s often the case in science.

what kind of science is he talking about? I wouldn’t think it could be any other kind of physics, chemistry, or biology – the fields commonly referred to as the hard sciences.

9. **Thomas Larsson**
March 25, 2007

I learned something new today from [this comment] of Mark Srednicki’s:

“For example, the string picture of black hole evaporation requires huge violations of macroscopic causality;”

Note the comment by “Elliot” a few lines below:

“If string theory predicts violations of causality, I am not sure this is type of assertion that would lead many “string agnostics” (of which I count myself as a member) to readily adopt the faith. Of all the scientific principles, causality is in my mind one of the fundamental cornerstones. “

10. **Peter Shor**
March 25, 2007

Is there actually a string picture of evaporation of black holes? I have not found any discussion of string theory and the black hole information paradox which looked more concrete than unfounded speculation.

11. **Peter Orland**
March 25, 2007

I think people are getting off track here and criticizing Mark
Snrednicki for something he didn’t say. He never said that the
landscape or even string theory is true. He said that it is
important to get at what is actually true. If you substitute
the synonym adjective “factual” for the word “true” in his
statement, perhaps, what he says seems less controversial.

There are other issues raised by Mark’s remarks, such as making
assertions about other Peter and Lee (which I disagree with), and
how one gets to the facts or TRUTH (which is where issues of falsifiablity are important). He’s absolutely right, however, in that science is about understanding reality, not the scientific method.
The method is only a method, not the goal – it is the proverbial
finger pointing to the moon. There are no rules in science beyond finding out what is.

12. **TCO**
March 25, 2007

Why don't the physicists figure out how to make a room temperature superconductor? And why (if they are so smart, and these problems so trivial) was High Tc found experimentally, rather than with theory or modeling?

13. **Peter Orland**
March 25, 2007

Sorry, misspelled Mark's name above – Srednicki.

Petre Drolan

14. **woit**
March 25, 2007

I don't think I'm criticizing Srednicki unfairly. I reproduced here his argument pretty much completely, and in later comments on the same thread he says that he strongly disagrees with the statement that string theory needs to make falsifiable predictions.

It's kind of absurd that Srednicki is criticizing Smolin for having too rigid a view of science, since, if you read Smolin's book, you'll see that he sees himself in some degree as a follower of the philosopher of science Paul Feyerabend, famous for his "anarchistic" philosophy of science, that there is no fixed "scientific method". Like Smolin I've also read Feyerabend and found myself in sympathy with this sort of idea. Sure, there is no fixed way to get to scientific truths, and all sorts of things should be tried. Personally, I'm partial to a semi-mystical belief that deep ideas about physics and about mathematics go together, so one should try and see if one can make progress in physics by concentrating on the deepest mathematical structures that seem to connect to physics. Other people have reasonable arguments about why this is a dumb idea that can't work.

In the end though, however one gets to it, the crucial thing about scientific truth is not that it is truth, but that the rules of logic together with experimental observation provide convincing evidence that it is truth, evidence that cannot be denied without violating logic or the evidence of one's senses. The current situation, with people like Srednicki promoting the idea that the landscape doesn't need to make predictions in order to become accepted as our dominant scientific theory of the physical world, is really dangerous to science, both internally and externally.

The attitude of Srednicki and too many string theorists these days seems to be that it is all right if the theory turns out to be non-predictive, that what is important is that it "has won out in the marketplace of ideas", i.e. achieved dominance in terms of things like NSF grants and faculty positions. The current situation is that he and others feel comfortable publicly jeering at those, like Smolin, who challenge this notion as "crackpots" (he has admitted to doing this).

I don't think this is about the landscape so much as it is about whether the conventional standard we insist on for recognizing a scientific truth will be upheld, or fall by the wayside because it requires some people to acknowledge failure. I disagree with Smolin about the landscape, but we agree about the crucial point: it has to make convincing, conventional scientific predictions, otherwise it's not science.

15. **matteoeo**
March 25, 2007

Peter Orland,

also bad science has no rules, beyond finding out what isn't. So, how can you distinguish them? I guess you can't or you know somebody upthere that tells you what TRUE is.

16. **Peter Orland**
March 25, 2007

Matteoeo,

I think you misread my message. I said nothing of the sort you
impugn to me (no prophet or messiah I!). In particular, I don't
think I was defending what you regard as bad science, which I
assume is the landscape (I also think the landscape is bad science).
Your question as to how to distinguish good and bad science is an important one, but seems more like a peccadillo directed towards me than a serious remark. But go ahead and metaphorically scream at me some more, if you like. I'm not proud.

Peter,

As you know, I also don't like the prevailing attitudes of string
theorists. I was not agreeing with the purpose of any attack on
you or Lee Smolin. I was just pointing out that one of Mark
Srednicki's points, which he was being criticized for here, was
a valid one. I don't think of scientific facts as TRUE in capital
letters, nor do I think the term has much meaning these days.
It has been abused by religionists beyond repair. In this I think Feyerabend (from whom I took a class at Berkeley) is absolutely right. Nor do I think ideas are scientific if they can't be either verified or falsified.

17. **matteoeo**
March 25, 2007

Peter (Orland),

believe me I'm not screaming at you. Maybe I'm mistaken, but then I don't really understand sentences like "There are no rules in science beyond finding out what is." What is what?

I think just the opposite, that science (from latin cognosco = I know) is the colletion of facts you know because their existence respects a few rules that were formulated long before Feyerabend was born, and that in a way belong to the logic of smart people without necessarily being taught of, al least until they stick up with an obsessive idea and direspect their own logic (but this is normal, since it's very hard to mantain a critical thinking just about anything. That's why phylosophers needed to formulate the Scientific Method as a painful rule). Scientific facts are a representation and a modelling of "what is" but they're not it – unless we embrace Pitagoreans' idea that the world is numbers.

I think it’s very important to distinguish “what is” from “what is knowable”. This is a problem that mathematicians were facing after Goedel’s theorem, and I think QG might encounter similar questions, indipendently of the approach – the AP in the Landscape is an example of that, of a strange and maybe circular overlap of thinking and meta-thinking. But that’s just a silly idea. Anyway something unknown (and unknowable, define it God or whatever) will always resist the attacks of scientists. Would you call that science?

As to Feyerabend, he was a great “dadist” thinker, but I think that his ideas on imaginative and inductive thinking had such great resonance because for half a century almost any field in science improved in a seemingly very fanciful fashion (owing to the technology mostly, which did very quickly “selective pressure”, to use biological comparison *à la* Smolin).

But now I think that String Theory failure is the demonstration that Feyerabend’s ideas are flawed. I almost regard him as (partly) responsible of all this. I know Popper isn’t as appealing, but he might end to be the only guy who really said something correct (besides the no-democracy-suicide statement).

Hope to have convinced you I wasn’t just yelling.

18. **Peter Orland**
March 25, 2007

Matteoeo,

Now that you have stated your position in more detail, I see you have something interesting to say. Maybe you are right that today’s trends indicate that Paul Feyerabend’s ideas are flawed. I’ll have to think about it…

19. **Q**
March 25, 2007

Chris, Newton’s motto, “hypotheses non fingo” (feign no hypotheses), kept him safe. Newton worked *from* data to formulate checkable laws, something that’s fast becoming a heretical approach today, and is the opposite to string theory.

20. **Who**
March 25, 2007

Matteo:
But now I think that String Theory failure is the demonstration that Feyerabend’s ideas are flawed. I almost regard him as (partly) responsible of all this. I know Popper isn’t as appealing, but he might end to be the only guy who really said something correct (besides the no-democracy-suicide statement).

What is the no-democracy statement. I did a search and came up with this quote of Popper:

“Dictatorship is morally wrong because it condemns the citizens of the state—against their better judgement and against their moral convictions—to collaborate with evil if only through their silence… It transforms any attempt to assume one’s human responsibility into an attempted suicide.

21. **CD318**
March 25, 2007

“I think people are getting off track here and criticizing Mark Snrednicki for something he didn’t say. He never said that the landscape or even string theory is true. He said that it is important to get at what is actually true.”

Well, even that much is wildly off-base. Science does not lead us to truth. At best, science allows us to generate models that, to a greater or lesser extent, are consistent with observation and experiment and allow extrapolation to observations and experiments yet unmade.

I wonder if Snrednicki has ever bothered to think with any seriousness at all about how one might define “truth.”

22. **matteoeo**
March 25, 2007

who,

I think this is a little off-topic here. Anyway I coined the epression no-democracy-suicide to make it short. Popper wrote that in a democracy people cannot vote the end of the democratic regime itself, and for example the beginning of a dictatorship – it might sound obvious but it’s not, it’s based on a logic argument and it would be an interesting task to read modern Constitutions and actually see if there’s some explicit article about this (I know italian has). Very boring reading from Popper, but foundamental, *The Open Society and its Enemies*.

As to QG and strings, I’d like to know if anyone has ever had the feeling I expressed, that there might be some foundamental inconsistency in unified theories, such as in mathematics Goedel’s theorem (and Chaitin’s reformulation), or if you think that there must be a theory, at all costs.

23. **A String Theorist**
March 25, 2007

Well we really have wandered off onto an epistemological tangent, haven’t we?

One of the marvelous things about scientific progress is that our understanding of which questions admit scientific answers evolves over time.

To use a somewhat tired, but very appropriate example: a long time ago there was interest in trying to derive the orbital radii of the planets from some fundamental principle.

Happily we now understand that this particular question is one which science does not, and CANNOT answer. This is NOT a failure of science! In fact, it represents a great triumph of science—Newton’s UNIVERSAL law of gravitation. By giving up the hope for scientific answers to a few of the “old questions” about the particulars of our solar system, we gain in exchange the answer to infinitely many “new questions”—namely the dynamics of any gravitating system anywhere in the universe.

Similarly, and this is the essence of Srednicki’s point: it is a logical possibility that, perhaps, there will NEVER be ANY scientific explanation for “why” the unbroken gauge group of the Standard model is what it is, or why the Yukawa couplings are what they are.

You are still welcome to work on those problems—and if you find a solution, no doubt fame and fortune will certainly be yours! But, again, you must accept the *logical possibility* that no explanation exists.

String theory finds itself in the awkward stage half way between realizing that the “old questions” that people used to be interested in, such as calculating the electron mass from first principle, are in fact unanswerable, and not yet being at the stage where we understand what the correct “new questions” are.

Historically some of the most exciting and productive times in physics have occurred as similar “boundaries” between old and new understanding were crossed.

24. **matteoeo**
March 25, 2007

who,

I’ve read my own post once again and I found that my bad english induced me to an error – I meant “including the no-democracy-suicide statement”, and not “besides”. Sorry for that.

25. **Peter Orland**
March 25, 2007

CD318,

I have a feeling this is getting off-topic, so I won’t blame Peter W. for deleting my comments. I just can’t resist to responding to what you have said about “truth”. As I said above, the term has lost its meaning, but maybe we should try to restore that meaning.

Outside of what anyone said, including Srednicki, Smolin or Woit, you have to be careful when you say science does not lead us to the truth.

If “truth” is something absolute, then it’s a rotten useless string of letters. If instead by “truth”, you mean facts (which are not immutable, but can change) then there is no problem with the term. A model which works well in predicting data is true, even if it doesn’t give us the whole truth.

Even a famous theorem could be untrue if the assumptions are found inconsistent, but I am willing to accept a theorem whose assumptions I accept and whose proof I understand as true. Most of us often use the word “true” in this sense (perhaps even you). Abuse of the term by religious types and even some scientists has led to our reluctance to use it. Perhaps we should try and reclaim the word from the abusers.

No good scientist or mathematician is willing to take for granted that no ideas are right. Newtonian dynamics is right – so why can’t we say it’s true, under certain circumstances.

I think scientists should not feel uncomfortable with refering to scientific facts as true. Otherwise they play into the hands of those who equate science with religion or a only a cultural phenomenon.

Anyway, I won’t say any more about this.

26. **Peter Woit**
March 25, 2007

A String Theorist,

The problem with the string theory landscape is not that it can’t predict the electron mass, it’s that it can’t predict anything at all. Nothing. If it made even one convincing standard sort of scientific prediction (for instance, allowing calculation of some experimentally measurable number to better than 1%, a number not calculable in the standard model), then it would be science. But the evidence is pretty strong now that it can’t do that. What I find remarkable is the way many string theorists and string theory partisans are reacting to this rather conventional sort of failure (it’s not really unusual to find that one’s speculative idea doesn’t work because it doesn’t predict anything). Instead of admitting failure, they’re trying to change the rules of what science is. It’s pretty amazing to see Srednicki and others arguing that a theory that can’t be experimentally tested is a viable scientific theory.

Sure, maybe Yukawa couplings are environmental and we’ll never be able to calculate them. But if you want to claim to be doing science, you have to come up with not just excuses, but some kind of predictions. What is going on now is that string theorists are turning Feynman’s criticism of them: “string theorists don’t make predictions, they make excuses” into a new philosophy of how to do science.

27. **matteoeo**

March 25, 2007

sorry Peters, I can't resist being nasty:

"Even a famous theorem could be untrue if the
assumptions are found inconsistent"

If the assumptions of a theorem are inconsistent (and thus the hypothesis is false), then the theorem is true (Ex Falso Quodlibet). The conclusion might eventually be wrong.

Anyway you can't just renormalize the meaning of words so as to be always in agreement with others. "Truth" as is meant by Srednicki is not "Facts", otherwise we would be talking about SM (as does the string theorist) instead of strings. Scientific facts cannot change with time, their interpretation might.

28. **Peter Woit**
March 25, 2007

Peter,

I didn't say that science doesn't lead us to truth. It certainly does, if there's any meaning at all to the notion of a truth about the physical world (and I think there is). My point is just that what distinguishes science from other human activities is not that it makes claims about what is true, but that it provides a method to reliably discover truths. I find it amazing to see supposedly serious scientists suggesting abandoning this in the case of the string theory landscape.

Religion also claims to lead to truths about the universe, and provides its own methods for getting there: consult the Bible, pay attention to what the pope says when he's speaking ex cathedra, etc. I think it will be very sad if physicists abandon conventional ideas about the scientific method in order to prop up a failed research program.

29. **anon.**
March 25, 2007

'I think scientists should not feel uncomfortable with refering to scientific facts as true. Otherwise they play into the hands of those who equate science with religion or a only a cultural phenomenon.' – Peter Orland

But happens when there is a radical overhauling of a scientific theory? Examples:

'Fifteenth century Europeans "knew" that the sky was made of closed concentric crystal spheres, rotating around a central earth and carrying the stars and planets. That "knowledge" structured everything they did and thought, because it told them the truth. Then Galileo's telescope changed the truth.'

– James Burke, 'The day the universe changed', Little, Brown and Company, Boston, 1986, p.9.

Other examples of 'false truth' abound: vortex atoms were the truth to Kelvin, mechanical aether was the truth to Maxwell, phlogiston, caloric, cold fusion, UFOs, alien abduction, etc., are all truth to cult believers.

The problem is that 'true' has always been emotional and hence poorly defined for science, yet well suited to religion. Critics of such 'truth' sound unreasonably, or immoral:

'What is truth?' – Pontius Pilate (Mark 15:2)

Popularising string theorists make a vague appeal to being the truth without having to actually make any checkable prediction.

30. **Peter Orland**
March 25, 2007

Well I was sort of finished with this topic, but a lot of people are
still respondingto my last comment, implying that I am defending
or attacking things that I am not, respectively, defending or attacking. It's very exasperating. I never said tentative theories (string theory or vortex theory included) should be considered true. I was just answering CD318's comments above about the word "truth".

My last point was just this: are we forbidden from saying
something is true? It all depends on what you mean by
"true". In common usage, the word just means factual.
Most of us say it every day. Are any of you prepared never
to use the word again in day-to-day life? If not, why can't
we use it as scientists? Just because some
people (and not everybody) attach a lot of extra meaning to
it doesn't mean we should. In particular, I think the word
needs to be rescued from those (perhaps some of the people
writing here) who take it too seriously.

Before responding to what you think I am saying, please
read twice.

31. **Peter Orland**
March 25, 2007

By the way Peter, I never implied that you said science does not lead to
the truth.

32. **dark-matter**
March 25, 2007

Come on guys. 21th century and here we are debating about 'truth'. 'Truth' can be defined any way. There are hundreds of truths on any one thing. Science is about being 'correct' A theory confirmed by experiments is correct, yielding correct predictions, the basis of reliable knowledge that have built our modern society. They give Nobel prizes for a theory being correct, not being 'true'. Leave the 'truth' stuffs to philosophers, theologians, etc.

33. **Chris W.**

March 25, 2007

The first sentence of a blog post by graduate student Jo Guldi:

> You might understand or not, but I write farce because I'm not convinced I have a handle, let alone a monopoly, on the truth.

Somehow this seems apposite.

34. **Chris W.**
March 25, 2007

(PS: Note the name of this particular blog.)

35. **Peter Orland**
March 25, 2007

Dark-Matter, isn't the conventional meaning of "true" in everyday usage is the same as "correct"? So why is one word better than the other? That is really all I am trying to say here.

36. **Arun**
March 25, 2007

Physics, then and now

A quote from C.N.Yang, seen by chance, to contrast with the quote from Srednicki.

37. **Jean-Paul**
March 25, 2007

String Theorist — Your argument about planetary orbits is yet another propaganda trick pulled by AP desperados, that appears in the introduction part of all Arkani-Hamed talks (at least of those that I attended). Yes, we do not understand initial conditions for many motions in the Universe, and its not a big deal that we do not understand them for the solar system which is in our nearest proximity. However, you cannot compare this ignorance to our lack of understanding of say electron's mass which is relevant not only in our proximity, but everywhere that we know in the Universe. If you want to consider the solar system at the same footing as the Universe, then you must subscribe to the multiverse faith which puts the solar system and the observed Universe at the same footing, as minuscule parts of a "bigger" story. Blah blah blah…

38. **Changcho**
March 26, 2007

Peter W. wrote: "The attitude of Srednicki and too many string theorists these days seems to be that it is all right if the theory turns out to be non-predictive, that what is important is that it "has won out in the marketplace of ideas", i.e. achieved dominance in terms of things like NSF grants and faculty positions. The current situation is that he and others feel comfortable publicly jeering at those, like Smolin, who challenge this notion as "crackpots" (he has admitted to doing this)."

This is also the viewpoint of people like LuMo. I agree that such a philosophy (if it can be called that) is a danger to science.

39. **King Ray**
March 27, 2007

String theory is no longer a science but a religion.

40. **mclaren**
March 28, 2007

Precisely. Once someone claims that what matters is not whether a claim is testable but whether it is true, we have passed out of science and into the realm of religion.

Did Jesus rise from the dead after he was crucified?

We can't test that hypothesis — but what matters is not whether it's testable, but whether it's TRUE.

Is Mohammed the one true Prophet of God, the only God, praise be to his Name?

We can't test that hypothesis — but after all what matters is not whether it's testable, but whether it's TRUE.

Is the Buddha the one Englihgtened one?

We can't test that hypothesis — but what counts is not whether it's testable, but whether it's TRUE.

This is the kind of self-deluded attitude that led people commandeer a pair of 757s and steer them into the twin towers. Thank you, but I've had quite enough of that kind of mindset.

If a claim is true, provide evidence for it. Otherwise, shut up.

41. **dan**
March 28, 2007

Hey Peter,
Since Lubos deletes your comments, have you considered debating it out over at http://www.physicsforums.com?

http://physicsforums.com/showthread.php?t=162163&page=2

"On the other hand, people like smolin and woit – especially woit – lost their battle in the arena of science long ago and are now trying to win it in the court of public opinion. Their books and comments are clearly meant for lay people who can be easily convinced of anything so that even if they prevail here – and it really doesn`t matter for research in quantum gravity either way – the victory would be a Pyrrhic one at best. Since they`re both no doubt already aware of this, one must wonder why they pour so much of their energy into this campaign of manipulation and misinformation."

42. **tomj**
March 28, 2007

"Precisely. Once someone claims that what matters is not whether a claim is testable but whether it is true, we have passed out of science and into the realm of religion."

This is a common theme, very understandable. But an interesting thing is that the religions that I am familiar with make lots of statements (similar to scientific principles) which can be applied to actual situations. The results can be judged, and the principle can be upheld or diminished. This process is not entirely personal, and therefore has some potential to be scientific. But religions have many facts which can never be verified. Maybe because they are singular events which occurred long ago.

From what I understand, in string theory there are no verifiable facts, at all.

Religion is already about something, maybe wrong, maybe mixed with unverifiable facts, but in general there is some useful advice, verifiable without the need to believe something you cannot test.

But equating string theory as it is being describe with religion, is quite the compliment for string theory.

43. **Peter Woit**
March 28, 2007

dan,

There's a limit to how much time I want to spend debating these topics. That's one reason I wrote the book, to get the case I wanted to make written down in one place. People can read it and judge for themselves.
While lots of string theory partisans are announcing they refuse to read it, quite a few physicists and mathematicians have, and I've been pleased with the response. String theorists were going on a lot at one point about how there was no longer an argument, since their theory had triumphed in the "marketplace of ideas". Now that it's not doing so well there, you hear this argument a lot less….

44. **dan**
March 29, 2007

Well thanks Peter,

Have you considered publishing technical articles in HEP/ARXIV critiquing string theory results such as string theory's derivation of BH entropy?

Peace.

45. **Peter Woit**
March 29, 2007

dan,

Lee Smolin has already done precisely what you suggest. As far as I can tell, his paper doing this (hep-th/0303185) has pretty much been ignored, especially by string theorists. I'm not so interested in quantum gravity, but have thought of writing up the arguments in my lectures in Italy in a more formal way to post on the arXiv. I suspect they would be ignored just as much in that form as in any other, which is an argument for spending my time writing up not that, but other things.

46. **Aaron Bergman**
March 29, 2007

Ignored? Say, rather, disagreed with, for reasons I'm sure you don't want to hear yet again.

47. **Thomas Larsson**
March 29, 2007

math-ph/0603024 more or less disproves string theory. Therein I make the simple observation that the Laurent or Fourier polynomial version of an algebra of gauge transformations is incompatible with nonzero charge unless there is an anomaly. Since nonzero charge evidently exists, and the relevant gauge anomalies in 4D do not exist in string theory, the latter is wrong.

E.g., for conformal symmetry the argument is simply this. If the charge L_0 != 0, then all L_m != 0, because
[L_m, L_-m] = 2m L_0.
Hence nonzero charge implies that there is no "local" part of the gauge algebra which is represented trivially, and unitarity then requires an anomaly. But this argument applies equally well to other gauge symmetries, provided that we consider the Laurent polynomial version of the gauge algebra.

AFAIK, this argument has never been disagreed with, only ignored.

48. **John**
April 1, 2007

"If it made even one convincing standard sort of scientific prediction (for instance, allowing calculation of some experimentally measurable number to better than 1%, a number not calculable in the standard model), then it would be science."

String Theory is Mathematics. Are u saying Mathematics isnt a science?Truth in science HAS to be mathematically consistent. String Theory maybe viewed as some mathematical religion but at least it satifies the law: Thou shall be mathematically consistent. (Unlike pretty much all the other theories proposed).

49. **anon.**
April 2, 2007

"Truth in science HAS to be mathematically consistent." – John

You're not even wrong! See http://www.cgoakley.demon.co.uk/qft/

"The shell game that we play … is technically called 'renormalization'. But no matter how clever the word, it is still what I would call a dippy process! Having to resort to such hocus-pocus has prevented us from proving that the theory of quantum electrodynamics is mathematically self-consistent. It's surprising that the theory still hasn't been proved self-consistent one way or the other by now; I suspect that renormalization is not mathematically legitimate."

– Richard Feynman, Nobel laureate 1965 (quote from Feynman's book, QED).

"[Renormalization is] just a stop-gap procedure. There must be some fundamental change in our ideas, probably a change just as fundamental as the passage from Bohr's orbit theory to quantum mechanics. When you get a number turning out to be infinite which ought to be finite, you should admit that there is something wrong with your equations, and not hope that you can get a good theory just by doctoring up that number."

– Paul Dirac, Nobel laureate 1933

Mathematics is used in science as a tool and language for patterns and quantities.

If a mathematical model for physical phenomena were completely scientific and consistent, it would be the end of scientific research!

50. **Peter Woit**
April 2, 2007

John,

"String theory" (interpreting those words to mean the version of the theory that tries to unify physics) is not mathematically consistent. It has no non-perturbative definition that works, the perturbative definition is based on a divergent series. There's a conjecture that a mathematically consistent definition exists for which the perturbative definition is an asymptotic series, but no one knows whether this conjecture is true.

51. **dan**
April 8, 2007

Peter Woit Says:

March 29th, 2007 at 12:19 pm
dan,

Lee Smolin has already done precisely what you suggest. As far as I can tell, his paper doing this (hep-th/0303185) has pretty much been ignored, especially by string theorists. I'm not so interested in quantum gravity, but have thought of writing up the arguments in my lectures in Italy in a more formal way to post on the arXiv. I suspect they would be ignored just as much in that form as in any other, which is an argument for spending my time writing up not that, but other things.

Peter,
Thanks for sharing this with me, I've looked at it. I infer from reading it that in string theory, spacetime is nondyanmical as it is in GR. Perhaps particle physics can be explained in terms of some for of quantum gravity. Have you had a chance to look at Wen's papers attemping to demonstrate that particle physics and arise as emergent states analogous to the phonon in condense matter physics? i.e http://arxiv.org/abs/cond-mat/0508020 implies that some form of quantum gravity gives rise to particle physics.

Censored Comments From Asymptotia

March 26, 2007
Categories: Uncategorized

Warning: There's not much science in this posting, just mostly people behaving badly. You would be well advised to skip this one, unless you find this kind of thing entertaining…

Long ago, as a public service, I set up a posting to hold comments censored by Lubos Motl, since his preferred way of dealing with people who make comments he finds hard to answer is to delete them. It appears that now a place for inconvenient comments censored by Clifford Johnson at his blog Asymptotia is also needed. If you have a copy of a substantive comment deleted from there or want to discuss one of these, you can use the comment section here.

Since the first thing censored was one of my comments, I'll provide a little background, then reproduce the censored material I have access to.

This exchange began with this comment from Clifford, which seemed to me to be little more than an out-of-control personal attack and rant, so my only response to it was:

Clifford,

As time goes on and the failure of string theory becomes more apparent, you are starting to rant in a manner which is converging with that of your junior colleague at Harvard. You should get a grip.

Not very long after I wrote this comment, the following comment appeared from Lubos Motl:

Dear Mr Woit,

your answer to Prof Mark Srednicki is absurd. The quark theory that Mark was writing about talks about physics at essentially the same energy scale as the effective theories with hundreds of hadrons from the first part of his story, namely hundreds of MeV. Also, the quark theory would be hard to test using the normal experiments at the QCD scale – which is essentially a low-energy scale – because one would have to calculate very complicated properties about bound states of quarks, and there are many of them etc. QCD is only easily testable at higher energies where it becomes weakly coupled.

Mark's gedanken experiment was designed to be isomorphic to the situation of string theory and if there is a difference, then the difference is that the natural scale of string theory is way above the observable scale so that the gap in string theory is greater than in the nuclear story, not in the other way around as you incorrectly wrote. Every physicist who has read Mark's comment knows it and understands it. The only reason why you argue that there is a significant difference between the two examples is that you don't understand how these theories actually work.

The fact that you find quantum gravity uninteresting is not surprising for me at all. At any rate, the key arguments – the mathematical robust ones – about questions such as the information loss came from string theory and everyone who was interested in these things – such as Stephen Hawking – knows this, too. Hawking admitted that the information is preserved primarily because of the AdS/CFT correspondence.

Concerning the anthropic principle, every scientist who has a sufficient talent and who has looked into it understands that there have emerged all kinds of reasons – not just pure string theory research – to think that the anthropic picture could be correct which is why this possibility must be seriously investigated, together with other possibilities. The people who are completely ignorant about everything could of course share your simple-minded and radical opinions but I think it would be a very bad idea if the people who are ignorant were deciding about the direction of the research done by the people who are not ignorant. You are effectively confessing that your goal is to manipulate people who can be easily manipulated – because they know nothing about the current state of knowledge in high-energy physics – and use them as a political force. I think it is deeply immoral and unscientific.

Dear Clifford, your value has increased in my eyes after the individual above compared you to me!

All the best
Lubos

This was followed by a similar comment attacking Smolin for what he had written about the black hole information paradox, and Smolin responded with a short and polite answer (I don't have copies of these).

Clifford has edited my original comment, removing "in a manner which is converging with that of your junior colleague at Harvard", and replacing it with "…snip … – personal reference deleted -cvj". He has also removed the comments from Lubos and the response by Smolin, replacing them with this comment. Here he claims to have deleted these comments without reading them, since, for undisclosed reasons, he has a policy of deleting all comments from Lubos. Whatever the reason for this policy, he does want to make clear to everyone that he has a high opinion of Lubos as a scientist, and for his work at the Reference Frame "widening the discussion" about physics:

Since I have a great deal of respect for his ability as a physicist, however, if he was making a physics point in his comments, perhaps he might make it on his blog and link to this discussion via trackback. I thank him for his physics contributions and widening the discussion.

Comments

1. **Chuckles**
   March 26, 2007

   You are quite correct of course; as demonstrating things to be true, or deduction from axioms and postulates is the province of mathematics and hardly that of science – even less of the wildly speculative endeavor in physics known as string theory. Perhaps Johnson misunderstood your point. Though how he could have is beyond me – since you pointed out so generously that logically consistency is hardly the defining feature of good science.

2. **Ari Heikkinen**
   March 26, 2007

   "I thank him for his physics contributions and widening the discussion."

   Heh, I thought his main contributions have been to denounce most of his colleagues (other than string theorists) as crackpots, idiots or otherwise incompetent people (kind of strong claims considering when you walk in say any city and look around there isn't anything that would have anything "string theory" in it).

3. **Chris Oakley**

March 26, 2007

I am posting this comment in the hope that it will be deleted, along with the other comments, and remainder of the post. Sorry, Peter, but I am sure that you have got better things to do.

4. **Hmmm**
March 26, 2007

I question any self respecting scientist whose policy it is to leave up a Lubos comment…..regardless of what is says.

5. **Nigel**
March 26, 2007

The following sentence among others 'snipped for brevity' from my comment http://asymptotia.com/2007/03/23/questions-and-answers-about-theories-of-everything/#comment-34707

'What's completely unique about string theory is that it has managed to acquire public respect and credulity *in advance* of any experimental confirmation.'

I'm not upset about this. It should have been written differently:

'String theory is completely unique in science because

(1) it's based entirely on unobservables (unobserved spin-2 gravitons, unobserved supersymmetric partners for all observed particles for unobserved unification near Planck scale, unobserved extra dimensions, unobserved branes),

(2) it fails to predict anything checkable after decades of research,

(3) it is hyped and celebrated *in advance* of success.'

6. **Peter Woit**
March 26, 2007

Chuckles,

Yes, that was my point. One can have rigorous theorems telling one about the properties of the mathematical formalism one is using to model the real world, but you can't have a "theorem" about the real world. You have to do experiments to test whether your formalism accurately models the real world.

7. **tomj**
March 26, 2007

It seems to me that the problem is exactly in widening the discussion. Maybe the tipoff should have been wide terms like 'theory of everything.' There is no method to distinguish what is in or out, that very idea seems to be the only thing not accounted for in the 'theory of everything'. Maybe string theory is 'wide science', much better than exact science.

8. **Peter Woit**
March 26, 2007

Nigel,

I don't want to encourage you or others to write repetitive critical comments about string theory either on Clifford's blog or elsewhere. If you do this on my blog I'm likely (depending on how busy I am…) to delete it, so I certainly don't criticize Clifford for deleting this kind of thing from his.

9. **Theo**
March 26, 2007

Surely the "great deal of respect for his ability as a physicist" comment referred to Smolin, who was the most recent antecedent?

10. **relativist**
March 26, 2007

"… he has a policy of deleting all comments from Lubos". Seems eminently sensible to me. Greatly improves the tone of the discussion.

11. **DB**
March 26, 2007

Theo,
I think Clifford's comment refers to Lubos, not Lee Smolin, as it goes on to mention "his blog". AFAIK, Smolin does not have a blog.

12. **Carl Brannen**
March 26, 2007

What I've been surprised by him in the recent tempest has been his willingness to criticize books that he's never read based on his reading of comments made by people who also refused to read it. I'm amazed that Clifford puts up with me as much as he does. Maybe it's because I'm a big Clifford algebra fan.

I might as well ask for you to add LaTex in comments like Clifford has. (Makes whining sound.)

13. **Peter Woit**
March 26, 2007

Theo,

It's quite clear he is referring to Lubos, not Smolin, partly because of the reference to the blog (Lubos has one, Smolin doesn't).

Despite repeated attempts, I never was able to get Clifford to criticize in any way Lubos's behavior as a string theory defender of the faith. It seems he approves of that, just can't tolerate what he sees as Lubos's racism and sexism. Many string theorists seem to find Lubos very sound when it comes to physics, a crackpot on other topics.

Personally I find him rather consistent.

Carl,

There is LaTeX in the comments (if not in the preview), but I'm a much less tolerant guy than Clifford when it comes to off-topic comments, even if they are about one of my favorite topics, Clifford algebras.

14. **LDM**
March 26, 2007

I tend to agree with Chris Oakley …

HOWEVER, Blogs are a new phenomena…and as many people put stock in what they read in blogs (not just physics blogs for that matter), there could be some real value in a site that tracks deleted comments to Asymptotia otherwise you can get this mechanism:

1) Somebody (a journalist?) searchs Google , finds Blog A
2) He finds some statement on Blog A that answers his query
3) Accept this answer as fact because he found it on the internet (sad, but this happens)

…but, now add:
4) Peter's proposed site would presumably show up in a Google search and thus be a partial remedy to 3)

Granted, this may not be Peter's original motivation, but such a site would be useful.

15. **John Baez**
March 26, 2007

I find it a bit tendentious to use the term "censorship" to describe what people are doing when they delete unwanted comments on their blogs. Maybe it's true according to some definitions of this term. But, at least in the United States, "censorship" has a connotation of "violation of the First Amendment right to free speech". This right is not violated when Prof. X deletes Prof. Y's comments on Prof. X's blog — just as it's not violated when a newspaper refuses to publish a given letter to the editor.

Of course, it's somehow more annoying to put a comment on a blog and then have it deleted, than it is to submit a letter or article for publication and not have it accepted. It's there — *and then it's gone! Censorship!!!*

This annoyance is part of the price we pay for a quick exchange of comments. The older, slower system of moderated newsgroups was closer to newspapers, where articles must be accepted for publication before anyone sees them.

But even in the old days, when I was moderating sci.physics.research and I'd refuse to accept an article, I'd sometimes get accused of "censorship". I found that a bit tiresome. Hence this mini-rant.

Go ahead — censor me. 🙂

16. **Peter Woit**
March 26, 2007

Hi John,

The "censorship" thing here is a bit tongue-in-cheek, maybe I should put smileys in there or something. Like you I've had all too much experience trying to moderate on-line discussions, have done plenty of "censoring" in my time, and think a certain degree of it is healthy and necessary.

This posting isn't really about "censorship", but just a little documentation of Clifford Johnson's behavior, which a few people fanatical enough to follow the details of some of these obscure battles in the string wars might want to know about (didn't you read the "Warning" at the top?)

Personally I thought it was pretty funny late last night, when, after responding to Clifford's Lubosian rant ("Do you actually teach any young people physics? Or any science? I dearly hope not.") by pointing out that it was, well, Lubosian, Lubos immediately wrote in with more of the same and a message of support for Clifford. Clifford's reaction to this, deleting my comment, claiming not to have read Lubos's, and praising him for his talents as a physicist and for "widening the discussion", was, as he says, priceless. I couldn't help myself, I had to share.

I keep telling people my next book is going to be a comic novel, cut and pasted from various blog entries and e-mails. I really need to keep this material somewhere….

17. **roy**
March 26, 2007

I really do think that you're making a fuss out of nothing here. Clifford is perfectly correct in removing personal attacks. If he does not do that consistently you could accuse him of hypocrisy though. Otherwise I think these string discussions are becoming more and more like pointless rants; someone–all of you– need to drag it back squarely to physics. It is not nice to see scientists wallow in this sort of personal attack laden filth. And Lubos should be kept away from all discussions– he is simply incapable of writing anything without insulting someone, and it seems, people like to write equally silly things about him.(Including probably this comment!) That said, I don't see why he should not be respected as a physicist. His papers are all right.

Please, please, all of you, get back to physics and mathematics and stay there. Much nicer that way.

18. **Peter Woit**
March 26, 2007

Roy,

Clifford is not "removing personal attacks", he is making them ("Do you actually teach any young people physics? Or any science? I dearly hope not."). You could ask him, but I'm pretty sure the reason he removed Lubos's comments (which were similar in tone to his own) had nothing to do with them being "personal attacks", but because he feels that Lubos is a racist and sexist (and it's pretty embarassing to have someone like that appear on one's blog, behaving in much the same way as oneself…).

19. **Roy**
March 26, 2007

I agree Peter, Clifford should have removed that bit as well. It was nasty and personal. I think in general it would be a good idea if people could simply control their tempers. I'm not taking any sides here– too many people are looking at this thing as something of a trench warfare.

On Lubos, even if Lubos weren't sexist or racist, he should be prevented from spewing his juvenile rants on anything. He has an uncanny ability to reduce a discussion to a brawl. That alone merits removal of his comments. One should perhaps give Clifford a benefit of doubt on the Lubos point.

All I am trying to say is that people should be a bit more respectful when in a scientific discussion– after all this is not, and should never be, personal.

20. **Peter Woit**
March 26, 2007

Roy,

I do agree with you that personal attacks don't belong in scientific discussions. When people start making them, it's often a sign that they're on the weak side of the scientific argument. I'm sure I've too often responded intemperately to a lot of the attacks that have come my way, I wish I had the much more even-tempered demeanor of Lee Smolin, who manages to respond politely no matter what sort of abuse he is subjected to.

But I really do wish people would read my warning, and only those who enjoy this nonsense would bother to pay attention to this posting.

21. **Chris Oakley**
March 27, 2007

Peter,

Since the post is still here, let me expand on what I was saying earlier, probably supported by LDM above. The battle (deflating ST hype) is won … when journalists write something about String Theory now, they generally call you or Lee Smolin up for the "sceptical" view. They did not do that before. The only purpose served by getting into pissing contests with String Theorists in the blogosphere is in getting them to reveal themselves as more crazy than those who criticise them. With Lubos that battle also is won. With the others I really do not think it is not worth the extra stress. Far better just to keep us updated on what is happening in the world of physics, plus your own efforts to use representation theory to solve QFT problems. I personally am sceptical of the latter, so prove me wrong!

22. **woit**
March 27, 2007

Oh no. If you don't find this entertaining Chris, it looks like I've lost what I thought of as my hard-core audience.

OK, no more. Off to Princeton to learn about the latest developments on the representation theory and QFT front….

23. **Chris Oakley**
March 27, 2007

For a blog to be good it has to be entertaining, informative, or both, and Not Even Wrong normally scores highly on both. I still savour the discussion about galactic-scale superstrings. The background noise here – which was considerable – mostly only enhanced the entertainment.

24. **tomj**
March 27, 2007

The Clifford post had this statement: "No-go theorems (and things in that spirit) are often the starting point for wonderful discoveries in science."

I've been wasting my time reading Einstein's _Relativity_, which says something of the antithesis of this. I'll quote in a second, but I just want to also ask if Peter believes that physicists don't have theorems, or if theorems are the same as theories? My concept is that we start with observation. This leads to speculation and maybe curiosity and prediction and more focused observation. Maybe a theory falls out. This is a process known as the scientific method. String theory, as described here and everywhere is stuck somewhere in this loop, but has never completed even one cycle, there is no feedback as of yet. So we have to have a theory to advance beyond where we are, but how to advance? Shouldn't we start with data which doesn't fit our current theory, or some principle which is easy to verbalize or quantify which could be applied to current theory. For instance, the anthropic principle doesn't fall into this category because there is no unexplained data, and it isn't clear how to apply it to current theory.

The quote from _Relativity_ (pg 66):

"No fairer destiny could be allotted to any physical theory, than that it should of itself point out the way to the introduction of a more comprehensive theory, in which it lives on as a limiting case."

The link between current theory and a future theory must be done via the clear statement of some principle. It is speculation if the principle is useful. But the starting point should be the immediate recovery of the current situation. It should explain why we are deluded to think our current theory is incomplete. There must be some benefit, maybe even some beauty.

I'm not sure why anyone has to understand string theory to ask for the principle upon which it is speculated to rest. It is like being given a map but not knowing where you are on the map.

25. **amused**
March 28, 2007

Peter, don't worry, some of your audience finds this stuff most entertaining. We want more! 😉
If stringers sling mud at you it is perfectly natural and appropriate to sling some back…

One thing I find striking in these "discussions" is the contrast between the knee-jerk reactions and simplistic views/arguments of the younger hard-core stringers (Aaron Bergman is an exception)
and the more reasonable, balanced and intelligent responses of senior stringers such as Polchinski and Harvey. Anyone have a theory for why that might be?

26. **Peter Woit**
March 28, 2007

amused,

Glad to hear someone else is amused by the spectacle. That people who consider themselves among the smartest in the world would behave this way and make these kinds of arguments seems to me quite a fascinating part of the human comedy.

People behave very differently in the face of legitimate criticism, a lot of this is just differences in character and emotional makeup. But string theory fanaticism does seem to be at its worst among the young. Lubos, Clifford and Jacques have spent their entire professional lives during the period of string theory dominance, trained in the subject at the heights of enthusiasm for the theory. I think they have a real problem even conceptualizing the idea that some of the core beliefs they were inculcated with could be wrong. To them, anyone claiming that must clearly just be ignorant, or not have learned the subject as well as they have.

Older people trained during the height of enthusiasm for gauge theory may often have more of a perspective on all this. Another factor sometimes at work is that, at least for some people, as they get older they get a bit wiser, and less often act like jackasses.

27. **Leonard Ornstein**
March 28, 2007

A scientist ideologically may be a 'Platonist': one who believes reality can be modeled by an ideal, yet-to-be-discovered set of absolutely TRUE, mathematical 'laws', which will exactly describe what can be observed;

or a pragmatist: one who looks at models as provisional guesses, that ALWAYS must depend upon realty checks.

Cliff Johnson and Mark Srednicki construct arguments that make it appear that they're Platonists…but not all of the time. If they were, it would be easy to see the source of their chagrin with critics of ST, and there wouldn't be much point in arguing the matter further.

I tried this out on them, on Questions and Answers on the Theory of Everything-Asymptotia #36. I wasn't censored…but drew no response!

(I was "trained (even before) the height of enthusiasm for gauge theory.")

28. **King Ray**
March 28, 2007

There is no doubt that string theorists are very intelligent, however they lack physical intuition, an aesthetic sense of beauty and simplicity, and are not independent thinkers (how could so many independent thinkers all think the same way?).

Einstein always felt that his greatest assets were his nose (physical intuition), his sense of beauty and simplicity, and his ability to think independently.

You could be the greatest machete brush clearing person in the world, but if you always go off in the wrong direction in the jungle, that is not going to help you. Your ability will just allow you to get further off track more quickly.

29. **JC**
March 28, 2007

Peter,

Do you remember any prominent bootstrap analytic S-Matrix old timers, being really vocal about string theory after 1984? (I can't think of any offhand). Folks like Geoff Chew seemed to be relatively silent on the subject of string theory.

30. **Nigel**
March 28, 2007

'I don't want to encourage you or others to write repetitive critical comments about string theory either on Clifford's blog or elsewhere.' – Peter Woit

My point is how *unique* string theory is in being the world's only grossly celebrated failed theory! Clifford earlier made the repetitive statement that string theory is *not unique,* so the controversy over it *could equally apply broadly in academic science to funding problems facing any non-mainstream projects*.

This is false since every other known mainstream theory in science has some kind of evidence behind it!

Therefore, it is a new point – not a repetitive criticism – that string theory is the *only* known example of a mainstream theory both unfounded on experimental fact and unable to produce checkable predictions, *existing anywhere in science*. Even the mainstream model of cosmology, the Lambda-CDM, is an ad hoc a fit of general relativity to the observed data, which is checkable to a certain extent by extrapolation and new data coming in. String theory is not even an ad hoc way to explain the standard model of particles, gravity or the small cosmological constant. It's a religion.

Has anybody ever emphasized before how *unique* the mainstream string theory crisis is? If string theorists do succeed in changing the definition of 'science' to mean uncheckable mainstream speculations about 'truth', this might infect science widely, supporting the merger of religion and mainstream science.

31. **Peter Woit**
March 29, 2007

JC,

By 1984 it was 20 years after the heyday of the bootstrap, and the kind of string theory and mathematics being used was completely different than the analyticity techniques of the bootstrap, so the two things had little to do with each other.

On the other hand, the younger people who during the late 60s and early 70s worked on dual models and early versions of string theory were quite enthusiastic about the rebirth of string theory after 1984 (Veneziano, Susskind, Mandelstam, Kaku, etc., etc….).

32. **adam**
April 2, 2007

I notice that Motl's wikipedia page is again up for deletion. Also that it appears that a mention of Motl can get comments deleted elsewhere or, at least it seemed that way to me when I made a reference to him in a CV comment that seemed to get deleted. But maybe I am just senile and forgot to submit it, or there was something else in my post that was so offensive that it Could Not Stand. Given the 'he who must not be mentioned' type of references in comments in, say, Chad Orzel's April Fool's blog entry, I guess that I just missed some collective attempt to starve Motl of the 'oxygen of publicity'.

I agree with John Baez that deleting blog comments isn't the Crime of the Century; it is, after all, that blogowner's own sandpit. I am not sure what is achieved by deleting all of Motl's comments, however; he might be considered to be unpleasant by many but his manner is not generally as offensive as his ideas (some will feel that that's damning with faint praise), although he is rude.

I personally find Motl's attitude to people that question string theory irritating: he appears like a 'You puny humans, YOU CANNOT UNDERSTAND!' movie villian. His social and political opinions, which I generally don't share, he is generally prepared to debate openly and in a more considered fashion, it seems to me. Clifford may have his objections the wrong way around, if it really is for Motl's beliefs that he's deleting the comments, rather than for the way that he expresses them. I don't know what Clifford's thinking, however. I certainly have no idea what Motl's thinking.

33. **Peter Woit**
April 2, 2007

adam,

Deleting comments, not just by Lubos, but by anyone who mentions his existence, I think is a tactic designed mainly to deal with the fact that he is a huge embarassment to string theorists, an embarassment that they would like to hide and eliminate any reference to. There is a case to be made for automatically deleting Lubos's comments because of his behavior, but I don't see the case for deleting comments that refer to this. The guy is a major figure in the string theory community, with many people in it having a high opinion of him, and not seeming to have a problem with his behavior. Two out of three of the very recent string theory textbooks carry Lubos's endorsement on their covers, a fact which speaks for itself.

Clifford Johnson makes it pretty clear that it's not Lubos's tactics in arguing for string theory that he has a problem with (he allows personal attacks in his comment section, often anonymous ones, engages in them himself, and writes approvingly of others). He goes so far as to praise Lubos as a physicist and thank him for "widening the discussion".

34. **adam**
April 2, 2007

Peter: It's a damn good thing that William Shockley never had access to blogs. He'd probably feature pretty high up anyone's* 'evil winners of the Nobel Prize in Physics' list.

If Motl really does get comments deleted because his other opinions are embarassing to people in his field, that's pretty silly. There have always been nasty or otherwise unloveable people in physics and every other area of human endeavour. One thing that is genuinely interesting to me about the string theory debate in which you are engaged is that the phony politeness (at least in the public eye) of the academic physics community is evaporating somewhat as the debate heats up. I know that there have been hot debates before, but this is the first one where I've met some of the combatants (rather briefly, in most cases, and not including yourself or Motl).

*Anyone except Motl, perhaps.

35. **Peter Woit**
April 2, 2007

adam,

The nature of the debate certainly has been an eye-opener about human nature. Especially seeing someone mild-mannered like Clifford Johnson descend into irrationality, Lubosian attacks and ranting as his view of the world is threatened has been something amazing to watch.

The number of people involved is still a small fraction of the string theory community, and the attitudes of most of the string theorists I know (adjusting for how likely they are to be polite in what they say to me) seem to be pretty different. While they're not happy about my activities, and annoyed especially at some of the unfair public criticism that has come their way because of this, they're well aware that string theory has serious problems and is not doing well (some are willing to characterize the field as "in crisis"). I've not run into any string theorist who, in private conversation, has told me that he thinks string theory research is doing fine.

Princeton Workshops

March 26, 2007
Categories: Uncategorized

Last week Princeton hosted a workshop on [Physics at LHC: From Experiment to Theory](). Many of the slides of the talks are available [here.]() The talks covered a wide range of ground, from theory not obviously relevant to the LHC to reports on the status of experiments. The talk by Michael Tuts of ATLAS reports that they may be roughly 5 weeks behind schedule, with nominal schedule setting the start of beam commissioning at 450 GeV late in November. 7 TeV beams won't be available until summer 2008, with first physics run perhaps in July, and about 10-100 pb^{-1} during 2008, giving similar statistics to what the Tevatron has today for some processes. It looks like it may be the 2009 run that will most likely produce data that will go beyond the Tevatron results, although if the Higgs mass is about 150 GeV, there may be evidence in the 2008 data. Plans for a luminosity upgrade in 2015 are already proceeding.

There was also a plan for a meeting of the String Vacuum Project at the workshop, with a discussion paper available [here]() (via their Wiki).

This week, also down in Princeton, but at the IAS, there will be a [Workshop on Homological Mirror Symmetry and Applications](), mainly devoted to recent work on geometric Langlands and its relation to mirror symmetry. Since I had to teach I couldn't make it down there today, so unfortunately missed the first talks by Kapustin and Witten, as well as that of Edward Frenkel, but I'm looking forward to going down tomorrow and maybe later in the week to hear some of the talks.

Comments

1. **Robert Musil**
   March 28, 2007

   So, what happened?

2. **Peter Woit**
   March 28, 2007

   Nothing too exciting to report I fear. I attended the talks by Witten, Kapustin and Gukov, which were just about the Witten-Kapustin and Witten-Gukov papers, not something newer. The Witten and Gukov talks were exceptionally clear. In both the Kapustin and Gukov talks, Witten was in the audience and often ended up being the one to respond to questions from other audience members.

   Witten was mainly talking about 't Hooft loop operators. He tried to jazz things up a bit by telling the mathematicians that studying moduli spaces of bundles in 2d using a 4d QFT was a way of dealing with a problem that they knew, that you have to think about not just the space of bundles on a Riemann surface, but the stack (geometric Hecke operators need to be defined using the stack). He also referred to the construction of the 't Hooft loops as a "categorification".

   I noticed few physicists there; the physicists in Princeton don't seem too interested in following Witten into this area. IAS math will have a whole program next year which will include more activity in this area.

   As always, lunch at the IAS was excellent, and many interesting people there to talk to.

3. **Robert Musil**
   March 28, 2007

   Peter,

   I'll be very interested to hear what comes of all this by the end of the week. As I mentioned previously, I'm no expert in Geometric Langlands, but I'm not aware of any spectacular results that have come out of this project to date. Of course, there are lots of indications (even to a non-specialist such as myself) that the project has lots of promise and talented researchers.

   But my impression is that the GL project was mostly interesting for its intriguing promise, not its existing results. More specifically, here's an outline of my understanding:

   Many attempts have been made towards a proof (or good understanding) of original Langlands conjecture, which of course applies to number fields. Those attempts have for the most part met with very limited success (one might say, that they've mostly "hit the wall"). My impression is that most of the researchers focusing on the original Langlands conjecture have come to believe that we're laking some essential geometric idea. So far, some new geometric ideas have emerged from the Geometric Langlands project – but these are ideas that pertain with some success to the function field case. However, I am not aware of a real, complete proof even in that case. Nevertheless, the ideas seem real, new and potentilly powerful, which is what I have understood to have generated high expectations for Geometric Langlands – expectations which may or may not be fully warranted.

   All that being said, I am not aware of any significant role played by Homological Mirror Symmetry in any of these new and promising geometric ideas – at least the ones that I have heard about.

   Am I wrong about the existing state of GL? Is some exciting new idea in Geometric Langlands predicated on Homological Mirror Symmetry expected to be revealed in this workshop?

4. **Robert Musil**
   March 28, 2007

   Just to be clear:

   My comments above contemplate Gaitsgory's fine work as well as the suggestive papers of Kapustin and Witten (Electric-Magnetic Duality and the Geometric Langlands Program) and Hausel and Thaddeus (Mirror symmetry, Langlands duality, and the Hitchin system). Thanks.

5. **Aaron Bergman**
   March 28, 2007

   I can talk a little bit about the role of homological mirror symmetry in geometric Langlands (although I'd probably be better off letting DBZ do it :)). In the physics formulation of the conjecture, one ends up with a mirror symmetry between the A- and B-models on certain Hitchin moduli spaces. This is in contrast with the mathematical conjecture which is an equivalence between categories of sheaves on the contangent bundle to Bun_G and D-modulies on Bun_Gdual (or something like that). To get that, Witten and Kapustin give some physics arguments that the A-model on a contangent bundle gives rise to D-modules on the base. I think this is a new idea (obviously, it is presaged somewhat by the work of Hausel and Thaddeus). Along the same lines, I don't think people had been aware that geometric Langlands has the structure of a 4D TQFT. On the math side, the connection between A-branes and D-modules has been proved in certain cases by Getzler and Nalder and Nadler. In the general situation, however, I think there's a lot more to be said, however, especially in regards to worldsheet instantons.

6. **David Ben-Zvi**
   March 28, 2007

   Robert,
   There are many things to say about your question, but I think it's
   quite fair to say

   1. the original Langlands program is doing spectacularly well, and is far from stuck at a wall.
   2. there are spectacular applications to representation theory as well as to the classical Langlands program that have come from geometric Langlands ideas.
   3. there are strong connections between geometric Langlands and
   homological mirror symmetry, at least the hyperkahler version thereof.

   I don't feel qualified to say much about 1, other than
   to mention Richard Taylor, Michael Harris and many others working in that area and proving fundamental conjectures in the area since
   Wiles' breakthrough – a recent meeting in Luminy on
   Langlands program left the uninformed spectators (eg me) in complete awe at the astonishing rate of progress in the area.

   For 2 I would focus for example on the work of Bezrukavnikov and collaborators, and that of Ngo and Laumon. Roman B. (together with Mirkovic, Rumynin, Ginzburg, Arkhipov, Finkelberg etc)
   has been proving a host of conjectures in "classical" representation theory, or what you might call Lusztig-world, in particular
   Lusztig's conjectures tying modular representaitons with
   representations of quantum groups or affine algebras. Also there's a profound understanding of (affine) braid group actions on various categories coming from this work of Roman that relates to many geometric problems, and work of Bridgeland and others.

   One of the most exciting things is the work of Ngo. Ngo was able to look at one of the fundamental geometric aspects of geometric Langlands, the Hitchin fibration, in a brand new way and use it with Laumon to prove the fundamental lemma for unitary groups (and maybe soon for other groups) — this being one of the central problems in the CLASSICAL Langlands program.
   There are a bunch of other concrete consequences of geometric
   Langlands ideas outside of its internal problems, but that's maybe enough for now.

   Finally for 3 I would point (besides the papers you mention)
   to the work of Donagi-Pantev, in which geometric Langlands
   is proved (in a certain degenerate limit, on an open subset)
   using T-duality (Fourier-Mukai transforms). I would also point
   out the work of Nadler and Nadler-Zaslow connecting Fukaya
   categories on cotangents to constructible sheaves,
   a key step in bridging the homological mirror symmetry
   picture of Kapustin-Witten to geometric Langlands.

7. **Aaron Bergman**
   March 28, 2007

   (Damn — why do I always confuse Getzler and Zaslow? Apologies.)

8. **A.J.**
   March 28, 2007

   Aaron,

   What do you have in mind about the worldsheet instantons? I thought they were supposed to be trivial?

   While we're on the subject of vague geometric Langlands speculations, I've always wondered what's supposed to happen to twisted N=4 at large N. Is there any sort of AdS dual theory?

9. **Robert Musil**
   March 28, 2007

   David –

   Many thanks for your very able and enlightening thoughts. That's a lot to think about, which I will try to do. Thanks again.

10. **Robert Musil**
    March 28, 2007

    Aaron –

    I also appreciate your thoughtful and reasonable response, which I am also going to try to think about.

11. **Aaron Bergman**
    March 29, 2007

    Hi AJ —

    I'm not sure I'm remembering this correctly, but I think the issue is the composition of cc – cc – A strings. There are no instanton contributions to the cc -cc strings or to the cc – A strings, but there might be to the composition law.

12. **Peter Woit**
    March 29, 2007

    Thanks David!

From many conversations with my colleagues here at Columbia, I can vouch for the fact that experts think that there have been big advances in the classical Langlands area during the last year or two, especially Taylor-Harris on Sato-Tate, and Ngo-Laumon on the fundamental lemma.

It also seems to me that there has been an increase in the interest of the rest of the math community in geometric Langlands ideas, and this is not just due to Witten's work and the relation to mirror symmetry. The connections to other subjects that David mentions have been part of this, as well as efforts by him, Ed Frenkel and others to make these ideas more accessible. A few years ago the widespread perception was that you had to speak Russian and hang out in Hyde Park to have any hope of understanding anything about geometric Langlands, but that has definitely started to change.

13. **Robert Musil**
March 29, 2007

Peter, Aaron and David –

A first, partial thought:

I don't mean to be harsh or dismissive, but my impression of the work concerning the relationship of mirror symmetry to geometric Langlands is that these papers strongly resemble the physics literature in this area: Written as fast as possible and largely motivated to stake a claim. Lots of claims to have "thrown up bridges" and the like, without too much in the way of bridge engineering detail. Of course, it is entirely possible that some of these ideas will yield real progress, but (for me, at least) it's far too soon to tell at this point in time.

I'm still thinking about your responses.

14. **Peter Woit**
March 29, 2007

RM,

My colleague Michael Thaddeus (with Tamas Hausel) was investigating this example of mirror symmetry several years ago, motivated I think partly by the hope that the Strominger-Yau-Zaslow picture could be worked out in this case, partly by the connection to geometric Langlands. That work was pretty standard mathematics, no connection to physics.

The more recent Kapustin-Witten and Gukov-Witten papers really are written from a physics point of view, and I think it's up to mathematicians to ultimately figure out how much insight they provide into the mathematics. This is not easy, given the very different point of view, grounded in difficult aspects of QFT. There have been amazing mathematical successes coming from this kind of activity in the past (Seiberg-Witten, knot invariants, counting curves on threefolds, etc.), we'll see what this leads to. I agree that it's still unclear…

15. **David Ben-Zvi**
March 29, 2007

I don't think there's anything tentative at this point
on the relation between geometric Langlands and T-duality.
Not being a physicist I can't vouch for the relation of this particular
T-duality with mirror symmetry, but Kapustin-Witten explain
it is precisely a hyperkahler instance of mirror symmetry.
From the math point of view if we take homological mirror
symmetry as our reference point, then geometric
Langlands is very close to fulfilling that requirement:
it relates B-branes on one space to D-modules on the dual.
Now you have to decide how close D-modules are to A-branes,
and that's a big part of the argument in K-W, and on
the math side is the topic of Nadler&Zaslow's work.
Unfortunately we don't yet actually have a definition of the category of A-branes outside of the Lagrangian objects (Fukaya category)
so it's hard to ask the question, but there's no doubt
the two are very closely related.. I'm not surewhat else you would
look for as "real progress"..

16. **Robert Musil**
March 29, 2007

David and Peter-

I'm not ignoring your most recent points, but one of David's earlier points is in my mind right now – and there's not that much room up there. I think it is unquestionable that Taylor-Harris, for example, represents real and tremendous progress in establishing the Langlands conjectures for local fields. But establishing the Langlands conjectures for local fields may (in my opinion as a non-specialist) be best viewed as finally making the global conjectures precise. That formulation is a kind of progress on the global case ("real" number field Langlands). But I think it is highly unlikely that Taylor would say that establishing the global conjectures (as now more precisely defined) has been moved significantly closer by his or anyone else's work under discussion.

17. **woit**
March 29, 2007

RM,

I believe you're thinking of earlier work by Taylor-Harris, not the recent Sato-Tate work I was referring to. I'll leave it to people more knowledgable than me to comment on the significance of their local Langlands stuff, but the recent Sato-Tate results are for number fields, not local fields. In my pitiful understanding of the subject, they generalized Taylor-Wiles to symmetric tensor powers.

18. **Robert Musil**
March 29, 2007

Peter –
Well, OK. Sato-Tate pertains to global fields and generally fits into the original Langlands program. But so does the Taniyama-Shimura theorem – and I think it's going rather far to view Taniyama-Shimura (and Fermat?!) as essentially features of the original Langlands program. The original Langlands program is vast and visionary, bold and sweeping. But – again speaking as a non-specialist – I don't think it's best practice to see all progress made in any area of mathematics the Langlands program touches

as significant progress in
the establishment of non-abelian global calss field for number fields
.
This all reminds me of a quip recently made by an Anglican bishop following some nasty theological dust-up at a bishops' conference that pretty soon even the Ayatollah might wake up and find that he's an Anglican!

19. **Robert Musil**
March 29, 2007

Peter –

I'm sorry if this post is redundant, but I'm concerned that my last one was obscure (in part, but not totally, because of its embarrassing typos, for which I apologise).

Sato-Tate deals with points on elliptic curves reduced mod p for all p – and definitely pertains to global number fields. Moreover, Langlands personally has had interesting things to say about Sato-Tate. But while Sato-Tate generally fits into the Langlands program and pertains to global number fields, it does not seem best practice (to me, as a non-specialist) to view Sato-Tate as a creature of the Langlands program. After all, Taniyama-Shimura (and Fermat!) also generally fit into the Langlands program and pertain to global number fields. In my opinion, Sato-Tate and Taniyama-Shimura can well be taken as evidence that the original Langlands program is on target. But I don't think any of these things significantly advances us towards a proof of a global nonabelian class field theory for number fields.

So, in summary, I don't think Sato-Tate is a significant advance of the classical Langlands program, even though Sato-Tate is a very significant advance!

20. **bb**
March 30, 2007

Disclaimer: I am, at best, an interested outside to number theory and might suffer from severe delusions about the mathematical content of what I say.

Robert,

I think it's incorrect to claim that Shimura-Taniyama does not represent a significant advance towards the global Langlands program, and to claim that it's "rather far" to view the former as being a "feature" of the former. Here's why:

The Langlands program relates automorphic representations and Galois representations over global fields (or, more philosophically, automorphic objects and geometric ones). Given class field theory, the first non-trivial case is the two dimensional one. In this case, there's a standard method to produce tons of interesting examples on both sides. For the automorphic side, one takes (weight 2) modular forms, and for the Galois side one takes the Tate modules (homology) of elliptic curves defined over such fields. Given this, the natural question to ask is if these two are Langlands-related (i.e: have the same L-function)? In this sense, Eichler-Shimura and Wiles' theorem provide the first non-trivial instance of the Langlands program — the former says that such forms have associated geometric objects, and the latter produces an inverse association.

Of course, I realise that this doesn't quite prove it (even for n=2, where it's still actively being researched by Kisin and company!), but it still seems like a fairly significant advance towards Langlands' conjecture to be able verify it in the first non-trivial case one can write down!

21. **Robert Musil**
March 30, 2007

bb –

As one non-specialist to another, I think I mostly agree with you. "Eichler-Shimura and Wiles' theorem provide the first non-trivial instance of the Langlands program?" Of course, although I might quibble about the "first" qualifier, since I think lots of earlier examples by Langlands and others were "non-trivial." Maybe Fermat is the first "spectacular" example. Sato-Tate? Ditto. These results are all – to my limited understanding – very good examples of the kinds of things Langlands and his school explicitly wanted to be able to get at with the classical Langlands program. That's why I think that "Sato-Tate and Taniyama-Shimura can well be taken as evidence that the original Langlands program is on target."

So, yes, seeing that various "predicted" by the classical Langlands program are correct and can be proved using techniques of the type Langlands and his school advanced is definitely progress. And in that sense I see David and Aaron as being definitely and completely correct in pointing out that classical Langlands techniques have NOT "hit a wall" in reaching such spectacular and wonderful examples.

But, as preliminary and possiblly epistomological quibble, Fermat rather predates Langlands, and lots of people made lots of progress on Fermat before Wiles – and not just Faltings. Sato-Tate is from 1960, Taniyama-Shimura from 1955, etc. Each of these conjectures have many lives and characteristic techniques that are independent of the Langlands project, even though the Langlands project had (has) something important to contribute to their resolutions. Wiles' critical use of Galois deformation in proving Fermat is not really part of the Langlands project, either – a fact that even the Langlands-imperialists (that a joke) at Columbia Math have admited in public writing! So I don't think these conjectures are best thought of as mostly creatures of the Langlands project. Rather, the Langlands project exists in large measure (but not solely) to contribute some techniques that address such conjectures!

But my second, more basic, point is that it is my impression (as an outsider) that the leading people in this field believe that (1) the classical Langlands program is a huge mass of conjectures, of which we are only beginning to scratch the surface, which tells us what the structure is, and (2) some powerful new geometric ideas are needed before real progress towards Langlands' goal of a class field theory for nonabelian global number fields. My further impression is that Geometric Langlands is thought to be a good place to look for such ideas.

None of that is intended as criticism. No one could reasonably critize Taylor or Harris or Shahidi or anyone else just because they don't yet have the tools to complete the clasical Langlands program and are looking at GL (among other places) for new ideas!

Anyway, I go on far too long and I don't mean to dominate Peter's blog. So I'll shut up now.

And, Peter, what's wrong with Hyde Park? I love Hyde Park!

Quick Links

March 28, 2007
Categories: Uncategorized

Shelly Glashow is traveling around, giving talks bashing his childhood hero Isaac Newton (and noting that he was "surely one of the greatest intellects the world has known"). He'll be at NYU tomorrow talking on The Errors and Animadversions of Sir Isaac Newton. For a copy of his talk, see here, (and here for a summary in Catalan).

Tonight is the Lawrence Krauss – Brian Greene string theory debate in DC, and next week in Berkeley Krauss is debating John Terning on the subject, at an event entitled Extra Dimensions and String Theory: Physics of the Future or Pure Mathematics? The event is organized by the FQXI funded organization Multiversal Journeys.

Krauss was at the cosmology conference that Tommaso Dorigo has been reporting on, giving a dinner talk dissing anthropic reasoning and pointing out that cosmology has a "miserable future", since everything is receding from us. His main conference talk was evidently not very optimisitic about near-term prospects for learning more about dark energy or dark matter.

For more about non-commutative geometry and number theory, David Kazhdan is giving a talk at Harvard on April 18 with the following abstract:

Discussion of Alain Connes formulation of the Andre Weil's theorem. . Alain Connes found a very interesting way to interpret the results as a computation of an asymptotic of a family of operators of finite rank.

Lieven le Bruyn has a discussion of Plato's cave and a recent paper Modular shadows and the Levy-Mellin infinity-adic transform, by Marcolli and Manin, who motivate their title by relating Plato's cave and holography in AdS/CFT.

Update: The Washington Post's publication Express has an article about Lawrence Krauss and the problems of string theory, entitled Frayed String.

Update: Science has a report on the debate. Also, there's a report (with audio) from a blogger at the Hooded Hawk blog.

Comments

1. **Chris Oakley**
   March 29, 2007

   Glashow's piece slagging off Sir Isaac makes interesting reading. I was aware of the fact that Newton was not above petty jealousies or obsessions with alchemy, astrology and other less-than-scientific pursuits, but one does wonder about Glashow's motivation in drawing attention to these … is he preparing the ground for some revelations about his own life, i.e. that he is a laudanum addict, a practising astrologer and necromancer and one who, despite the present evidence, secretly admires Superstring Theory? We should watch this space. Further revelations may be on the way.

2. **DB**
   March 29, 2007

   If his paper had included a proviso along the lines of "it's easy to criticise from the vantage point of 350 years of hindsight" I might be inclined to indulge Glashow. Rather than speculate on how Newton would have conducted himself in the modern era, Glashow might be better advised to imagine how he himself might have fared in the mid seventeenth century. A pity there are no new insights in this article, other than to learn that Glashow's childhood innocence has been cruelly undermined.
   Comparisons with Einstein are just silly. Einstein was no mathematician and no experimentalist, so why on earth compare them? And Einstein was no saint either! Instead of wallowing in the human defects of his subject, Glashow would serve his bright agenda (which I support) better by emphasising how scientific achievement enables humans to leave behind a legacy which outshines the feeble reality of their ephemeral personalities.
   By the way, Asberger should read Asperger (after Hans), somebody may be casting a spell!

3. **steveM**
   March 29, 2007

   Another of Newton's bizarre beliefs was that the law of inverse
   squares was already to be found in Pythagoras. He was also
   the first physicist to go into finance when he became Master of the Mint and sent forgers to the gallows without a second thought. Despite his intellect Newton also made a huge blunder when he lost £20,000 in the stock market–a colossal sum in its day, and still quite substantial even today–specifically when the South Sea Bubble collapsed. He then remarked " I can compute the motion of heavenly bodies but not the madness of people". The quote also suggests that dealing with people and the subtleties of human
   relationships was not his forte

4. **dan**
   March 29, 2007

   It's been claimed Newton had Asperger's Syndrome.

5. **Peter Shor**
   March 29, 2007

   Is attacking Newton for using way too many significant digits fair? The theory of and proper use of significant digits is second nature to scientists now, but I don't think it's a priori obvious, and I can't believe it was actually around at the time.

6. **Kea**
   March 29, 2007

   *Another of Newton's bizarre beliefs was that the law of inverse
   squares was already to be found in Pythagoras.*

   This is not bizarre. It is true.

7. **Chris Oakley**
   March 29, 2007

Inspired by Glashow via Peter, I went to the antechapel of Trinity College, Cambridge today where there is a statue of Newton. He looks like a P.E. teacher. Maybe all this grouchy behaviour was only because he missed a vocation here.

8. **Uncle Enzo**
March 29, 2007

How come my comment was deleted?

9. **woit**
March 29, 2007

Sorry Uncle Enzo, but there are certain topics which, once broached, lead to large numbers of comments by tedious ideologues, so I don't post about these and will ruthlessly delete comments about them. These include evolution, religion, climate change, and others I dare not even mention…

10. **Robert Musil**
March 30, 2007

Could Newton have been the model for Templeton the Rat in Charlotte's Web?

11. **Robert Musil**
March 30, 2007

I was a little disappointed that Glashow didn't mention any of the drubbing Newton took from Jonathan Swift in Gulliver's Travels. In fact, the original Frontispiece of "Gulliver's Travels" [a cartoon of Gulliver himself, reproduced here: http://www.jaffebros.com/lee/gulliver/morley/fp.jpeg%5D may be a likeness of Newton. [See: http://links.jstor.org/sici?sici=0035-9149(199607)50%3A2%3C191%3AITFO'T%3E2.0.CO%3B2-6%5D

Swift saw Newton as the essence of the immoral (or at least amoral), abstract reasoning scientist, and was especially annoyed at Newton's scheme to debase Irish coinage, which Swift believed was immoral and callous. In the book, Gulliver travels to the Flying (Floating) Island Laputa (Spanish "la puta": the whore – Swift considered excessive rationality to be whorelike). The inhabitants are distracted with very narrow interests – mathematics and music. Their clothes do not fit and are decorated with astrological symbols and musical figures. They listen to the music of the spheres, believe in astrology, worry constantly that the sun will go out and live in badly built houses. They are speculative and rationalistic philosophers who are pathetic failures as philosophers, reasoners and men. They are devoted to the most ethereal of abstract music and mathematics — but cannot play music well or figure accurately enough to build houses or tailor clothes (The tailor's mistake is directed at Newton based on the mistake made by a printer in one of the figures Newton used in computing the distance of the earth from the sun). They are completely incompetent in practical affairs.

Gulliver is then lowered from floating Laputa to Lagado, where the crops are poorly managed, people wear ragged clothing, and the houses are in bad condition – except for the house of the governor of Lagado. He tells Gulliver that 40 years before some Lagado residents visited Laputa and came away with a smattering of mathematics. They built an academy to carry out their projects they learned in Laputa. The Grand Academy of Lagado is a satire on the Royal Society, of which Newton was then President. At the Academy of Lagado, scientists are attempting to extract sunbeams from cucumbers, turn human feces back into food, erect buildings from the roof down, plow farmland with pigs, make marbles soft enough to stuff pillows and pincushions, breed sheep whose entire bodies are bald, and have students learn mathematics by swallowing wafers on which formulas are written.

And there's more.

12. **stevem**
March 31, 2007

I just read Glashow's interesting article right through. Despite his flaws, or perhaps even because of them, Newton remains a fascinating character. Yes, he got a lot of things wrong and was into a lot of strange stuff, but every so often his immense intellectual powers would just come to the surface. In 1696 Bernoulli challenged his colleagues to solve the "brachistochrone problem", which asked what is the curve between two displaced points along which a body acting under gravity alone would follow in the shortest time. The deadline for the problem was 6 months but Leibniz requested it be extend to one and half years. Newton, then 55, received the challange at 4pm on Jan 29, 1697. By the following morning he had invented the essence of calculas of variations, solved the problem and sent off the solution anonymously. But everyone knew who had solved it with Bernoulli remarking: "we recognise the lion by his claw".

Jonathan Swift's attack on the scientific community you mention in the third book of Gulliver's Travels arose from the discussions of the "Scriblerus Club". This was a group of disgruntled Tory writers who formed themselves into a literary society, and which met in the rooms of St James's Palace. The Scriblerus Club set out to redicule the learned societies of the day and Newton, being a big shot in the government establishment and the dominant scientific figure, was top of their hit list of course. A play in 1717 has a character called "Dr Fossile", who is a pompous wig-wearing scientist and the jokes clearly suggest it is a satire of Newton. For example, he is asked by a foreign philospher/adventurer who can't speak good English: "vat do you tink of de new methode of de fluxions of de quantity?". To which he replies" the greatest quantity I ever knew was three quarts a day". He is also asked if he can see a "star as big as the moon in the daylight". Looking through his telescope he (Fossile) replies" I can spy no other celestial body but the sun".

13. **anon.**
March 31, 2007

Newton isn't necessarily the only physicist or mathematician under attack since Gulliver's Flying (Floating) Island, Laputa, is powered by a loadstone (magnetite rock). This is satire on Kepler's theory that the planets orbit the sun due to magnetism, not on Newton who came up with gravity. Ever since Gilbert discovered that the earth is a magnet in 1600, it had been assumed to be the force acting between planets. Descartes had Cartesian vortices filling space to account for the planetary motions, while Kepler moved a step forward by suggesting magnetism. Newton killed off Cartesian vortices by showing that the inverse square law of gravity based on Kepler's planetary laws, supplied with Galileo's data for acceleration at earth's radius, correctly predicted the moon's known centripetal acceleration.

Newton's fluxions, not his physics, came under attack from those who claimed it is irrational and religion. Bishop George Berkeley in 1734 wrote *The Analyst; or a Discourse Addressed to an Infidel Mathematician: Wherein it is examined whether the Object, Principles and Inferences of the modern Analysis are more distinctly conceived, or more evidently deduced, than Religious Mysteries and Points of Faith.*

If you have a rectangular block height y and width x, then it's area is the integral of y*dx over the range of x: area = N*y*dx = (x/dx)*y*dx = xy, where N is the number of slices. Although this works, there was an argument that it is effectively multiplying infinity (ie. x/dx) by zero (ie. dx) which seemed irrational.

The full text of Bishop Berkeley's attack is online: http://www.maths.tcd.ie/pub/HistMath/People/Berkeley/Analyst/Analyst.pdf

14. **Robert Musil**
March 31, 2007

stevem –

One has to be a fascinating lion to have people fussing after 350 years over the mistakes and flaws – instead of just trying to remember the accomplisments!

15. **Jonathan Vos Post**
March 31, 2007

“Another of Newton’s bizarre beliefs was that the law of inverse squares was already to be found in Pythagoras”

Besides being true, this is part of a much more interesting point: that Newton, for all his battles on priority, seemed to believes that he was primarily rediscovering the “secrets of the ancients.”

Okay, his last published paper was on the Penny, that’s sensible, given his job in charge of the Royal Mint (and how nice for an alchemist to have access to all the nation’s gold!). But his last book was on the Secrets of the Pyramids.

A fine novel with Newton as protagonist is:

Dark Matter: The Private Life of Sir Isaac Newton: A Novel, by Philip Kerr

Paperback: 352 pages
Publisher: Three Rivers Press; Reprint edition (October 28, 2003)
Language: English
ISBN-10: 1400049490
ISBN-13: 978-1400049493

Publishers Weekly review:

Holmes and Watson provide the template for this very satisfying historical thriller from Kerr (The Grid, etc.), with Sir Isaac Newton acting as great detective and one Christopher Ellis serving as narrator. It’s 1696, and a series of murders are plaguing the Tower of London, where the middle-aged Newton has recently assumed (as in real life) the position of warden of the royal mint, with the younger Ellis (again as in real life) serving as his assistant. Like Holmes, the cold and cerebral Newton relies on rationalism the scientific method to solve the crimes, while Ellis, quick with sword, pistol and temper, brings the emotional counterweight provided by Conan Doyle’s Watson. The murders are accompanied by esoteric clues, most notably encrypted messages and alchemical references, that spur Newton to their resolution as forcefully as does his intense sense of duty, for the killings seem to involve not only a plot to disrupt a recoinage necessary to continue England’s war with France, but also a conspiracy to commit religious genocide against a backdrop of incessant tensions between Catholics and Protestants. The mystery elements of the novel provide a sturdy spine for the book’s main flesh: its robust recreation of life at the end of the 17th century. Ellis’s fluid narration sets the tone, illuminating a London beset by pestilence, poverty, whores and ruffians, noblemen grave or foppish, opium dens, brothels and grisly executions, and a bright array of historical figures including, in the role of blackguard, Daniel Defoe. There’s an erotic/romantic subplot involving Ellis and Newton’s niece, but the main focus is on the two leads. Both are well drawn, though Newton, ostensibly the novel’s center, is less compelling than Ellis’s full-blooded youth. That disparity, and an overly complex plot, are the drawbacks of what is, withal, a most gripping and well-appointed entertainment.
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Not Good

March 30, 2007
Categories: Uncategorized

This latest news from the LHC does not sound good. One hopes it won't affect the LHC schedule.

Update: Also discussed at the Resonaances blog from CERN.

Update: Latest (4/3/07) news here.

Comments

1. **Peter Orland**
   March 30, 2007

   Does anyone know if all nine of these assemblies can be modified? Or
   do they have to be rebuilt from scratch? How many will needed altogether? More than nine?

2. **tomj**
   March 30, 2007

   It sounds like the loads were never taken into consideration during design, and overlooked during multiple reviews, which seems weird because they were testing for the load, which shows up under several expected conditions during normal and abnormal operation.

3. **Yatima**
   March 30, 2007

   These things happen in engineering. (Independent) Verification and Validation _can_ miss things. Then costs go up (politically difficult) or the supplier will be asked to fix things at his expenses (leading to finger-pointing, more delays and lower quality). Grin and bear it.

   Just be happy that this project is not called "Galileo" (yes, I have a stack of GAL requirements on my desk)

4. **woit**
   March 31, 2007

   Please, stop with the off-topic comments that have nothing to do with the magnets at CERN, I've had to delete a thread of these.

5. **anon**
   March 31, 2007

   It's just another delay tactic of the string theorists.

6. **Haelfix**
   April 1, 2007

   The vibe from experimentalists in my department who work at CERN range the entire gamut of the map. So I don't really have any good inside information about it. One seems to think its not a big deal, whereas some others are furious about it and already booking plane tickets. Its quite hush hush though, as I can't coax much information out of them.

   My best guess from what I felt is some delays, but perhaps not as bad as some thought.

7. **Aymar resigns>>Resonaances**
   April 1, 2007

   […] the start of the LHC will be delayed by several years. Following these reports, the CERN Council had a special meeting today, during which Director General Robert Aymar resigned from his post. I assume full responsibility for what has happened, he said […]

8. **Chuckles**
   April 1, 2007

   I think its about time to start a String Theory Costs ticker, just like all those Iraq War Costs tickers on so many websites. Until people are convinced that we are actually spending good money day after day on what is quickly becoming cultic activity within the realms of natural science, the kind of public reevaluation we need will not be forthcoming.

9. **Ari Heikkinen**
   April 4, 2007

   "These things happen in engineering."

   Actually no.

   When you design a pressure vessel (as I understand it was) you have to be right, you can't just wait for it to explode and fix it later.

10. **Bob**
    April 5, 2007

    Well, of course oversights happen while developing any major project. What is very troubling here — and every project manager's nightmare — is that the "longitudinal forces" that caused the failure were, according to Fermilab, never accounted for in the specs and were not identified as potential problem by the external oversight reviews.

    The latest update from Fermilab is at (http://www.fnal.gov/pub/today/20070403_page01.html).

John Horgan Discussion With George Johnson

March 31, 2007
Categories: Uncategorized

Bloggingheads.tv now has some video up with an interesting discussion between John Horgan and George Johnson on a range of topics. One segment is entitled String theory deemed load of crap!, and discusses the controversy over string theory. Both Horgan and Johnson agree that things are not look good for a physical theory when there start being public debates on the subject. Johnson also discusses his time at the KITP and mentions this blog. He seemed to be particularly struck by the behavior of the participants at the session he ran there which involved a lot of bashing of Lee Smolin, with Mark Srednicki raising his hand when Johnson said he didn't think anyone would call Smolin a crackpot.

Johnson also described the "echo chamber" effect of blogs like this one. I guess I better keep this going by blogging about his commentary about my blogging…

Update: Sean Carroll has a posting about this over at Cosmic Variance entitled String Theory is Losing the Public Debate. Probably best if people join the discussion over there, which so far includes John Horgan and others.

Update: If you like this sort of thing, more blog discussions about string theory here, here, and here… and here and here.

Comments

1. **Gina**
   March 31, 2007

   When both Horgan and Johnson seem to agree that "string theorists are the smartesr people not going to wall-street" [a quote from their discussion] it makes you think that it is not just string theory [right or wrong] that is losing the public debate but the scientific endeavor as a whole.

2. **anon.**
   March 31, 2007

   Gina, seeing that string theorists can't calculate anything which turns out to concern the universe we happen to live in, I can understand why Wall Street wouldn't require them. Checkable predictions are often required in the stock market…

3. **Coin**
   March 31, 2007

   Johnson also described the "echo chamber" effect of blogs like this one. I guess I better keep this going by blogging about his commentary about my blogging…
   Quick! Somebody write a blog post responding to Woit's blogging about Johnson's commentary about Woit's blogging!

4. **Kea**
   March 31, 2007

   Unfortunately I can't view this clip on my computer. But it sounds very interesting. Thanks, Peter.

5. **Arun**
   April 1, 2007

   There is an interesting claim there in the comments. If true, on Monday, everyone who doubted string theory will have egg on their face.

6. **Chris Oakley**
   April 1, 2007

   I was forwarded this from a former HEP colleague, who allowed me to post it on condition that I remove the sender's & receiver's e-mail.

   I am not sure quite what to make of it. If it is what it appears to be, then I am surprised that nothing got out earlier. I guess that we can expect a public announcement soon, though. You heard it here first!

   > [Snip]
   > But for Lee, Peter Woit and the club of disgruntled losers that follow them, we would have made an announcement in January.
   > [Snip more personal attacks]
   > As you see, with Joe's insight, there is anomaly cancellation in 3+1 dimensions and the theory exists perturbatively at all orders, with no infinities – not even cancellations, just finite! The tree-level amplitudes agree with the SM, and the (finite) loops, which Joe calculated in December not only get the Lamb shift and electron anomalous magnetic moment absolutely right, but get a new value for the muon a.m.m. that suddenly puts it back into 1 s.d. territory vis a vis experiments … this really is one in the eye for the superstring critics, and just as they were beginning to get far too much air time.
   > [Snip more personal attacks]
   > the so-called "wild goose chase" has turned out to be nothing of the sort! – as for supersymmetric partners, the SS breaking puts the photino at 1.35 TeV, the selectron at 1.1 TeV, and the squarks between 1.2-1.5 TeV – all within reach of the LHC! So we will find out very soon, but personally I have no doubt.
   > [Snip lengthy comment about CERN politics]
   > It seems our intuitions were right … superstring theory doesn't just feel right – it probably IS right! And that's not the half of it – Mike's group have come up some amazing stuff on the QG side – wormholes, time travel – everything you ever fantasised about. He's is a bit cagey about this, and isn't likely to make an announcement until he's checked and double-checked the sums, but it seems that they'll soon have something to give to the experimenters, and if it turns out to be right, it extends GR in truly mind-blowing ways!

7. **Thomas Larsson**
   April 1, 2007

   Chris, don't forget which day it is today.

8. **Chris Oakley**
   April 1, 2007

Thomas! Don't spoil this before anyone in the US has even got out of bed! Peter – I implore you to delete this and the previous comment!

9. **Mondrian**
April 1, 2007

Chris,

that was too obvious anyway. Enjoy this one before Peter will get out of
bed and delete it:

http://video.google.com/videoplay?docid=706444693144627950&q=lubos+motl

10. **anon.**
April 1, 2007

Mondrian, can you provide an English translation from the Czech in that google video, please?

11. **Mondrian**
April 1, 2007

No, sorry.

12. **anon.**
April 1, 2007

I was wondering if it is Lubos' 2-minute explanation of string theory.

13. **Mondrian**
April 1, 2007

Seems likely. In that case I would really appreciate if he could do an english
version too, maybe including a quick enumeration of string vacua.

14. **Chris Oakley**
April 1, 2007

That clip with the prancing Lubos has only made me hungry for more string theorist music videos. What about, for example
• Brian Green as Alice Cooper, singing "I wanna be elected".
• Peter West as Pete Townsend, including the climactic destruction of his "axe" on stage.
• Susskind as a "Kiss" lead singer. He has the attitude – all he needs is the eye makeup.

15. **mclaren**
April 1, 2007

Left a comment at Cosmic Variance, but did want to mention here that Sean Carroll's analogy twixt the theory of evolution and string "theory" (so-called) seems unacceptably inaccurate.

For an analogy to hold, there must be some points of similarity between the two items being compared. Darwinian macroevolution, with libraries full of supporting evidence, does not make a valid comparison with string "theory" (so-called) because at present stringy speculations have not a single piece of hard experimental evidence to back 'em up. Indeed, unlike Darwinian macroevolution, string speculations at present make not even a single testable prediction.

Too, as usual Peter Woit seems to be getting quoted so grossly out of context that it's almost not worth correcting the record. It's sort of like taking a historian's offhand remark that his grandfather died in WW I but his granfather was only one man, and misquoting it to try to "prove" that the historian is denying the existence of WW I because "only one person died in the entire war."

Dr. Woit mentioned that he's surprised that string theorists have done such a poor job defending string theory in public.

I'm not.

To effectively defend a point in debate, you must cite evidence and logic to back up your point. The string theorists have no evidence. That leaves them with hand-waving, which is bound to be an extremely weak debating tactic. As Aristotle remarks in his *Rhetoric*:

if we have no evidence of fact supporting our own case or telling against that of our opponent, at least we can always find evidence to prove our own worth or our opponent's worthlessness.

And isn't this exactly what we find happening with string theorists?

16. **rho**
April 1, 2007

anon, there is nothing about string theory in that video. It is just the Czech title song for the cartoon series of "Tom and Jerry".

Anyway, there is something seriously irritating to see this…

17. **anon.**
April 1, 2007

Sean Carroll has now denied in a comment on his blog that he was comparing string theory to evolution.

18. **anon.**
April 1, 2007

On the subject of 1 April, Google has put up one at http://www.google.com/tisp/ and if you follow to http://www.google.com/tisp/install.html and then click on professional installation service you get the error report mentioning grand unified theories:

‘The requested URL was not found … (far less plausible, but theoretically possible, depending on which ill-defined Grand Unifying Theory of physics one subscribes to), some random fluctuation in the space-time continuum might have produced a shatteringly brief but nonetheless real electromagnetic discombobulation which caused this error page to appear. …’

– http://www.google.com/tisp/notfound.html

19. **anonymous**
April 1, 2007

I guess everyone has heard by now. There is a forthcoming paper (by Ludwig Poehlmann) concerning the fatal flaw in the implementation of extended objects such as strings and branes. This appears to be the nail in the coffin for some of the mainstream theories.

20. **wolfgang**
April 1, 2007

Ludwig Poehlmann = Archimedes Plutonium ?

21. **Michael**
April 1, 2007

“Sean Carroll has now denied in a comment on his blog that he was comparing string theory to evolution.”

Well, he clearly did compare the situations: public debate over string theory and public debate over evolution. I don’t know how to take his comment on evolution. Is he more skeptical of evolution due to the outcome of the debates, or does he simply not care anymore?

22. **Haludza**
April 1, 2007

I believe Sean Carroll was simply saying that the existence of public debate of a physical theory doesn’t necessarily imply that it’s on shaky ground. These debates will tend to happen if there’s some perceived public interest in what’s being debated!

So, clearly, evolution is debated because it touches some nerves and not due primarily to whether the evidence suggests it.
Carroll only compares string theory and evolution in so far as they’re things involving scientific questions which are being debated.

23. **Peter Woit**
April 2, 2007

Arun,

The amazing thing about that paper

http://www.arxiv.org/abs/hep-th/0703280

is that Sean Carroll and others seemed to be pretty convinced that it is an April Fool’s joke, but its authors seem to be serious (and it was submitted on March 29, not April 1). Looks to me like people in string theory have completely lost the ability to distinguish between a joke and serious science.

24. **anon.**
April 2, 2007

“The electron mass is about 6.5 times larger that the expected value, while the muon mass is about 40% smaller.” – http://www.arxiv.org/abs/hep-th/0703280 p4

What’s the experimental error bar/standard deviation on the electron and muon masses, nowadays?

25. **Peter Orland**
April 2, 2007

It doesn’t look like a joke to me. Assuming it is technically right, the
right question to ask is, “how many parameters are put into this
theory/model to get something realistic”. If the number of
parameters is more than the number of observables, the idea isn’t
useful (I think this is the case). If the numbers are equal, it is a parametrization.

26. **wolfgang**
April 2, 2007

anon.,

the reason (according to the paper) the electorn and muon come out wrong is their low mass and the fact that the calculation is tree-level only.
They provide a reference to a paper in preparation which shall discuss and “fix” this issue (I assume based on some approximation at 1-loop).

27. **Chris Oakley**
April 2, 2007

> They provide a reference to a paper in preparation which shall discuss and “fix” this issue (I assume based on some approximation at 1-loop).

I will look forward to seeing this paper on April 1, 2008.

28. **april fool**
April 2, 2007

Peter,

Doesn’t it seems more likely that the speculation about

http://www.arxiv.org/abs/hep-th/0703280

being a joke has more to do with the fact that it hadn't yet appeared on the arxiv and that nobody had read it when they were speculating that? I'm not defending (or, for that matter, deriding) the physical content of the paper but I just don't see what part of this paper could be interpreted as funny. I don't claim to have read the whole thing very carefully, but if the paper is a joke, then it doesn't seem to be a very funny joke to me.

29. **anon.**
April 2, 2007

Wolfgang, why would one lepton mass be underestimated while another be overestimated? I could understand if both lepton masses calculated were both too low because of some vacuum corrections.

Other questions: To what degree are the predictions subjected to the arbitrary assumptions of the model? How much variation is there in the predictions if you take other assumptions? Obviously if this is wrong, at most it will only falsify this particular model in a landscape of models.

On the other hand, can even this model be falsified? Or – by making small adjustments – can it be fine-tuned to fit virtually any LHC data? On p4 they list a lot of parameters they "choose" to plug into the model in order to make it generate the masses they want. To what extent it this just numerology?

30. **Peter Woit**
April 2, 2007

april fool,

Sure, if you look at the paper, you realize it's not funny, and thus not likely to be an April Fool's joke. But, based on the claims made in the comment section at CV about the paper by one of its authors, Sean Carroll and others assumed it had to be a joke. We have a situation right now where there's a whole field of "string phenomenology" full of researchers making claims like this, with most theorists well aware that there is no hope that these claims can be sustained, so assume they are some kind of a joke if made around April 1.

This is kind of like the situation with the Bogdanov papers. They clearly weren't a joke because they weren't funny. What was disturbing was that mainstream journals were publishing stuff that was pretty universally acknowledged to be nonsense by the physics community.

31. **Michael**
April 2, 2007

"What was disturbing was that mainstream journals were publishing stuff that was pretty universally acknowledged to be nonsense by the physics community."

there's also the Schon scandal (2001/2002).

http://en.wikipedia.org/wiki/Jan_Hendrik_Sch%C3%B6n

i don't know if this publishing "total nonsense" started with Sokal or not. while it's possible to dupe a bunch of lit crit postmodernists with a physics article, it should (in theory) be harder for a physicist to dupe another physicist.

32. **Phen Phen**
April 2, 2007

Mayes's comment on CV: "Eric Mayes on Mar 31st, 2007 at 11:54 pm"

Submission history: "Thu, 29 Mar 2007 20:03:58 GMT (11kb)"

unless there is a delay in the arxiv, the paper was sitting there the entire time people were labelling it a joke. or did i look at the wrong paper? all those stringy papers look the same to me.

33. **Peter Woit**
April 2, 2007

Phen Phen,

arXiv papers don't appear until a set time later when they are made publicly available in a batch. From the submission date, this paper would normally not be available until 8pm Eastern time on Sunday April 1. Someone did claim that the arXiv schedule did seem to be different this weekend, with that paper available earlier (or, maybe, if you accessed the paper by number you could get it, listings were just not yet available). I think most of the people who assumed it was a joke did so based on the description of the paper's claims made by one of its authors, don't know if any of them actually accessed the paper and looked at it. Since the claims in the comment were similar to previous claims by this author, I assumed it probably wasn't an April 1 joke.

34. **Eric**
April 2, 2007

Apparently, someone had access to the preprints before they actually came out. I made my post in reaction to previous posts such as one by PW that string theory couldn't make contact with particle physics. Hopefully, you guys will realize that it can. Can you admit it?

35. **april fool**
April 3, 2007

Peter,

"…based on the claims made in the comment section at CV about the paper by one of its authors, Sean Carroll and others assumed it had to be a joke. We have a situation right now where there's a whole field of "string phenomenology" full of researchers making claims like this, with most theorists well aware that there is no hope that these claims can be sustained, so assume they are some kind of a joke if made around April 1."

But, at least according to the authors, those claims were sustained! Presumably people thought it was an april fool's day joke because it's a bold claim. One might interpret this as meaning that the paper is extremely important…

"This is kind of like the situation with the Bogdanov papers. They clearly weren't a joke because they weren't funny. What was disturbing was that mainstream journals were publishing stuff that was pretty universally acknowledged to be nonsense by the physics community."

I don’t see how it’s anything like the Bogdanov affair. In that case a paper that was clearly nonsense got published in a peer reviewed journal. In this case people heard a bold claim about a potentially important result (nobody had actually seen the paper) on a blog and thought it might be a joke just because the problem the authors claim to have solved is a difficult one. In the case of the Bogdanov affair the problem, presumably, was a lazy referee who didn’t actually read the paper. In this case I don’t see why you think this reflects so poorly on Sean Carroll and friends. Nobody had actually read the paper.

36. **Peter Woit**
April 3, 2007

“One might interpret this as meaning that the paper is extremely important…”

It seems to me you’re joking here, making my point that this field has a real problem with telling the difference between a serious scientific argument and a joke.

I wasn’t criticizing Sean Carroll. He’s quite right to have thought that these kind of claims were a joke, because it’s not possible to take them seriously as science. Other commenters here have already pointed out why.

37. **Thomas Larsson**
April 4, 2007

It might be emphasized that it was a strong string advocate, Sean Carroll, which jumped to the conclusion that news of successful stringy phenomenology had to be an April’s fool. I don’t think that Peter ever claimed that the paper was a *deliberate* joke.

Various Stuff

April 3, 2007
Categories: Uncategorized

The list of speakers for Strings 2007 is now available. Titles of talks are not available, but as far as I can tell they're not taking up Lee Smolin on his suggestion that they have someone there to talk about developments in the LQG approach to quantum gravity that string theorists might find interesting. Also, no mathematicians on the list and fewer mathematically inclined string theorists than at Strings 2006. One experimentalist from CERN (Rolandi), presumably to talk about the LHC. Lots and lots of people who work on the landscape and various string compactification schemes, with the Stanford group well-represented.

If you're in Princeton tomorrow there are a couple of interesting math talks. The talk by Simons at the IAS on *Some Results in Differential Cohomology* (with Sullivan, presumably about this) should set some kind of historical record for the highest net worth of a speaker giving a technical math talk. Ed Frenkel is giving the colloquium at the math department on *Langlands Correspondence for Loop Groups*; I wish I had time to go down there to hear it.

The conference in Paris on non-commutative geometry in honor of Alain Connes is continuing this week. Fabien Besnard reports on the talk by Michael Atiyah, where evidently there was some commentary on the role of mathematical beauty in physics, and warning to the young that working on the kind of idea he was discussing would be dangerous for their careers. And no, in French "Physique Retardee" does not carry the same meaning as a naive translation to English would imply…

The web-site of representation theorist Ivan Mirkovic has lots of interesting things, including notes about geometric Langland and the recent work of Witten-Kapustin-Gukov. Another interesting representation theorist web-site is that of Alistair Savage, which includes various lecture notes and an overview about quivers and geometric representation theory.

See here for the program and some lecture notes from the recent spring school at Trieste. Especially interesting are the lectures by Martin Schmaltz about Physics Beyond the Standard Model and the LHC. The Resonaances blog also has a report about a recent talk by Schmaltz at CERN. The bottom line seems to be that, contrary to what was previously thought, in many of the kinds of supersymmetric models supposed to come from string theory, you can't run the observed scalar superpartner masses back up to the unification scale, so, even if you see such things, you won't get information about grand unification out of them. Schmaltz gives a graphical representation of the reaction of various people to this. I'm in category A.

Tommaso Dorigo has an excellent suggestion for experimental collaborations worried about the information that their blogging members are putting out. Don't fight them, join them! He suggests that large experiments like CDF, D0, ATLAS, CMS should be putting out a collaboration-approved blog, getting their story out to the public through this medium.

Update: I realized there is another event I should mention. This Saturday I'll be giving a talk at a symposium at the University of Central Florida in Orlando, organized by the Society of Physics Students and the Campus Freethought Alliance. Also speaking there will be Jim Gates, who presumably will be taking a somewhat more optimistic view of string theory. I'm still trying to figure out what to talk about, current plan is to cover some of the material in my book, emphasizing the parts everyone ignores that are not about string theory…

Comments

1. **Who**
   April 4, 2007

   the emblem on the main page of the Strings '07 website
   is a 1789 oil painting by Goya called THE BLIND CHICKEN
   (La Gallina Ciega) or translated into English "Blind Man's Buff".

   The blindfolded girl is in the center of a circle of dancers,
   who elude her attempts to strike them.

   There is a sunset, the players are aristocrats, it is pretty but
   inexplicably melancholy.

   To get further information about the Strings '07 conference,
   click on individual dancers in the circle.
   http://gesalerico.ft.uam.es/strings07/

   To me, the blindfolded young lady looks like M-theory, something which so far has never been seen, surrounded by elusive entities who joined by the duality of holding each other's hands. No one is like Goya.

2. **Peter Orland**
   April 5, 2007

   Among the list of speakers at Strings '07 are Bern
   (maybe N=8 supergravity is finite) and Beisert (advances
   in N=4 Yang-Mills and the AdS/CFT conjecture). These
   are important topics and both a bit out of the current
   string mainstream. Without this small diversity of focus
   (that is, something other than usual string stuff) the
   meeting would probably feel like Strings 1907.

MiniBooNE Announcement

April 6, 2007
Categories: Uncategorized

It seems that the MiniBooNE neutrino experiment at Fermilab is finally ready to announce results. A talk next Wednesday, 11am, at Fermilab by Janet Conrad and William Louis has been scheduled, with title *Initial MiniBooNE Oscillation Results*.

Via Alexey Petrov, who explains the significance and teases that there's a rumor that MiniBooNE sees "something interesting"…

Comments

1. **A**
   April 6, 2007

   Hi Peter, The results will also be released at Columbia by Mike Shaevitz – at the same time. It will be at 1pm EST in rm 428 Pupin Hall.

2. **Brett**
   April 7, 2007

   I wouldn't be surprised if every MiniBooNE PI is giving a talk on Wednesday. Rex Tayloe has one at Indiana.

3. **JA**
   April 8, 2007

   Your speech in Japan was terrific, Peter!

   http://www.youtube.com/v/qYgZYkTYUaQ

4. **Chris Oakley**
   April 8, 2007

   He's very persuasive … if I lived in Japan, I'd vote for him. Or not. Whatever he wanted.

5. **Peter Woit**
   April 9, 2007

   A,

   Thanks, I'll be there to hear the results first-hand!

6. **Marco**
   April 9, 2007

   Thank you for the heads-up. Any idea whether one of these talks (ideally the one at FNAL) will be available in streaming on the web?

7. **Marco**
   April 9, 2007

   Yes, it looks like the streaming will be available here:
   http://www-visualmedia.fnal.gov/live.htm

8. **V**
   April 9, 2007

   If MiniBooNe announces that they have evidence for a sterile neutrino, does anyone have an idea of how this fits in with the Standard Model? To me, the most natural explanation would be that this a a fourth generation right-handed neutrino which has a strongly suppressed Majorana mass so that the seesaw swings the other way and gives the fourth-generation left-handed neutrino a mass ~50 GeV.

9. **D. V. Ahluwalia-Khalilova**
   April 9, 2007

   If MiniBoone sees something like a sterile neutrino, its phenomenological consequences would strongly depend on the CP properties of the component(s) beyond the three mass eigenstates. For this twist to the problem, see hep-ph/0702049.

Talks at UCF

April 9, 2007
Categories: Uncategorized

This past weekend I was at the University of Central Florida, participating in a symposium organized by Costas Efthimiou of the physics department there. It was sponsored by two student organizations, the university's Society of Physics Students and Campus Freethought Alliance. There were two speakers, Jim Gates and myself. I suspect that the organizers and many in the audience were hoping for some fireworks between Gates and myself, taking opposite sides on the controversy over string theory, but I fear that we disappointed them.

My talk was entitled The Challenge of Unifying Particle Physics, and my intention was to avoid spending much time going over the problems of string theory, since I'm pretty tired of that, and instead to try and explain to the audience some of the basic facts about symmetries, representations and quantum mechanics, together with an outline of the current state of efforts to unify physics. Gates gave a very general talk about particle physics, unification and string theory, featuring a lot of very impressive graphics he has developed as part of a multi-media course called Superstring Theory: The DNA of Reality.

In the end, there wasn't that much for us to disagree about. My critique of string theory as a unified theory is based on the claim that the idea of using strings in 10d doesn't work because the variety of possibilities for handling the extra 6 dimensions makes predictions impossible. Gates has always been skeptical about extra dimensions and wasn't about to defend them, let alone the landscape. I take his general attitude to be similar to that of Warren Siegel, who he collaborated with in the past, and who explains his point of view here. Recently Gates has been very much interested in representation theory, in his case the representation theory of supersymmetry, where he and collaborators see fundamental problems still to be resolved, and have new ideas about using Clifford algebras to attack them. For one of their recent papers written from the more mathematical end of the problem, see here.

I very much enjoyed my time in Orlando; high points were getting to meet with and talk to some of the physics students there, meeting someone who sometimes comments here who came to the talk, and especially getting the chance to discuss things with Jim, who I found to be impressively knowledgeable and thoughtful about every topic that came up.

Comments

1. **Anton**
   April 9, 2007

   Siegel has many funny parodies on his web site. The one I liked a lot is "THe Official String Blog":
   http://insti.physics.sunysb.edu/~siegel/parodies/atchoo.html

2. **A.J.**
   April 9, 2007

   Hey Peter,

   Out of curiosity, is there anything Siegel says in his research summary that you disagree with?

3. **woit**
   April 9, 2007

   AJ,

   There are lots of different things in Siegel's research summary. On the whole I'd say his interests are just in a different direction than mine. Lots of questions about string theories and superspace, whereas I just think there are much more interesting things to think about, especially various aspects of gauge theory.

   I think Siegel gets it right that the 10 d string is a doomed idea about unification. I'd be willing to believe his work on 4d strings might someday lead to new insights into QCD. But I don't see any evidence that different ideas about string theory are going to give any insight into unification, and I'm basically agnostic as to whether string theory is particularly useful as s theory of quantum gravity.

4. **Hendrik**
   April 9, 2007

   Dear Peter,

   Concerning the mathematical problems involved with obtaining a consistent SUSY QFT, you may be interested in the obstruction theorem of Kishimoto and Nakamura at Commun. Math. Phys. 159, 15–27 (1994). A strategy for circumventing this obstruction recently appeared in http://arxiv.org/abs/math-ph/0604044

5. **A.J.**
   April 10, 2007

   Hi Peter,

   I wasn't expecting you to say anything about D=4 strings.
   But I was struck by how sober and even-handed Siegel's general comments on string theory are. No excited language, no wild claims. This sort of thing was the norm in conversation when I was a physics grad student, but seems rather rare in the blogotub.
   As for conclusions about string theory, I personally tend towards Siegel's 2nd option: There's something vaguely right about string theory, but no reason to believe that the various models which have been written down are correct. For one thing, they strike me as far too geometrical. I just can't bring myself to believe that our world is described by anything as neat and clean as a Calabi-Yau manifold, or a bunch of intersecting branes.

6. **Chris Oakley**
   April 10, 2007

   "Superstring Theory: The DNA of Reality" sounds a bit like "Superstrings are the language in which God wrote the world". Presumably this was not one of the things you agreed about – ?

7. **Coin**
   April 10, 2007

So, a question?

This jumped out at me from Siegel's research summary:

In fact, the only property of string theory not found in particle theory is Dolen-Horn-Schmid duality, which states that summing poles in one channel gives the same result as summation in another channel. This is also the only experimentally verified result of string theory.

I have to confess I don't really understand this… well… at all. A quick hit on Google indicates only that "channels" apparently means "momentum channels", whatever those are. However:

One of the most common (and common for good reason) criticisms of String Theory floating around right now is that it hasn't made any testable predictions. One of the running themes I've noticed reading blogs like this is seeing String Theory proponents offering up various results of String Theory as "predictions", but those results turning out under analysis to not fairly qualify as such.

Siegel carefully words his page in a way that seems to be almost intentionally avoiding calling the Doren-Horn-Schmid thing a "prediction", and I imagine if it were easily classified as such it would have been brought up elsewhere. But as he describes it, it *is* a result which is (1) predicted by String Theory (2) predicted by string theory uniquely, or at least not predicted by "particle theory" (3) borne out by experiment. These three things are generally the conditions I would think of when judging whether a scientific theory has made a "prediction". If it is a prediction, it certainly doesn't sound like a large or significant one, but based on the limited information I have it certainly does seem to be better than nothing.

What vaguely is this Doren-Horn-Schmid thing, if it's not too much trouble to ask? Would you say that it qualifies as a "prediction" made by String Theory? If not, why not?

8. **Doug**
April 10, 2007

Peter,

I just want to commend you for your efforts to shed light on this highly specialized subject. You seem to appreciate how difficult it is for non-specialists to grasp what is going on, and are willing to use your gift for clear articulation to help out.

However, your last slide is most revealing:

1) The math of the SM is poorly understood in many ways.
2) We don't understand the representation theory of gauge groups.
3) Future progress might require unification of mathematics.

What are the chances that you are willing to prepare a talk elaborating on those three points?

9. **woit**
April 10, 2007

Coin,

What Siegel is talking about concerns the idea of using strings to model the strong interactions. This goes back to the late sixties, early seventies, and there are good reasons to believe this is sensible. Some observed aspects of the strong interactions do appear to be well-described by a string model (but no one has a completely successful model). This has nothing to do with using strings to unify gravity and the standard model, which is what isn't working at all.

AJ,

Yes, the way string theory is represented in the blogotub does a disservice to the field. There's a well-known line from Yeats "The best lack all conviction, while the worst are full of passionate intensity"

I think Siegel and Gates see themselves as somewhat out of the mainstream of string theory. Gates at UCF explicitly said they he was in a minority, and not just because of the color of his skin…

Chris,

No, I don't agree with Gates about using that kind of metaphor. I think he sees himself as trying to be careful to distinguish speculation from solid science, but at the same time trying to transmit something about these ideas to a very large audience, including transmitting the enthusiasm of the scientists involved. This seems to me a very tricky business, all too easily giving people the impression that something that is very speculative and without experimental foundation is on a similar footing to something that we have a lot of evidence for.

10. **woit**
April 10, 2007

Doug,

At some point I do hope to write more on these topics, although at the moment I'm much more trying to find time to push my own ideas about these things further along. To make clear exactly what I had in mind with those points, here's a more specific version:

1. We don't understand non-perturbative gauge theories, especially ones with chiral fermion couplings very well at all. This is not just a mathematical problem, but a conceptual one, and new mathematical ideas might help.

2. The representations of a gauge group in 1d are understood, this is the theory of loop group or affine Lie algebra representations. This theory allows us to understand certain 1+1d QFTs, especially WZW models. In higher dimensions, very little is known, probably including the fundamental question of what the right sort of representations is to look for.

3. Absent experimental guidance, new ideas about unification may have to come from mathematics. Mathematics and physics seem to be connected in a very deep way, and understanding this connection may be our only way to make progress if we can't probe higher energy scales experimentally.

11. **King Ray**
April 10, 2007

Peter, great talk at UCF, and it was a real pleasure to meet you in person! Thanks also for autographing my book.

12. **JJ**
April 10, 2007

Hi Peter:

I would like to know if you have had the chance to read Tegmark´s last paper . It was discussed briefly at the N-Category Café . Although highly especulative I found it worth reading.

13. **Peter Woit**
April 10, 2007

JJ,

I did take a look at the Tegmark paper you mention. Philosophically I think I'm somewhat in agreement with him about the "Mathematical Universe Hypothesis", but to me it seems that to get anything out of it you need to understand what the fundamental mathematical structures out there are in a much more sophisticated way than he is doing.

14. **mclaren**
April 10, 2007

String theorists seem to be using the term "prediction" and "predictive" in a non-standard way. For example, Jacques Distler claims that "string theory is highly predictive" but when asked for published HEP journal articles making experimentally verified prediction, the string theorists refuse to provide any.

Moreover, the 3 big results from experimental HEP-related physics of the last 20 years were not predicted by anyone. Not by the SM and not by string theorists: neutrino oscillation, dark matter, dark energy.

Some will jump in to claim dark matter was predicted by the string theoretic superpartners but no one knows whether dark matter is due to superpartners. It could just as well be due to neutrino oscillation, or something else might be going on that we're just not aware of yet. And no published HEP experiment has yet verified the existence of superpartners. In some cases, we haven't even experimentally verified the existence of the original SM particle (graviton), let alone the superpartner (gravitino). So it sounds like that's really putting the cart before the horse.

So when people like Siegel say *"This is also the only experimentally verified result of string theory,"* it's not clear what they mean. In the sense that string theory collapses down into all of current physics at low energies, string theory can be technically said to have lots of experimentally verified results… But they're trivial results. The real test of a new theory isn't whether it reproduces the results of a perfectly adequate existing theory like the Standard Model. It's whether the new theory predicts phenomena which aren't predicted by the old theory, and which are then confirmed by experiment.

So far I'm not aware that string theory has done that.

15. **theoreticalminimum**
April 11, 2007

Peter,

Maybe you should fix the following in your slides:

pp 4, 5 : "different from", not "different than"
p 11 : -1 factor missing in Schrodinger equation.
p 21 : At the time "it" was unclear…
p 24 : As several generations of new accelerators have [been] come…
p 24 : "… everything they have seen is compatible with the predictions of the SM" – Even neutrino masses? Dark matter?(?)
p 29 : 1974 "Used" to quantize…
p 31 : .. as to whether any "is" possible.

Feel free to delete this comment after you've read it.

16. **Warren**
April 11, 2007

mclaren Said:

> So when people like Siegel say "This is also the only experimentally verified result of string theory," it's not clear what they mean…The real test of a new theory isn't whether it reproduces the results of a perfectly adequate existing theory like the Standard Model. It's whether the new theory predicts phenomena which aren't predicted by the old theory, and which are then confirmed by experiment.

Please see my previous sentence on that same webpage:

"In fact, the only property of string theory not found in particle theory is Dolen-Horn-Schmid duality…"

So far, no one has been able to derive DHS duality from any particle theory, the Standard Model in particular.

Coin Said:

> What vaguely is this Doren-Horn-Schmid thing, if it's not too much trouble to ask? Would you say that it qualifies as a "prediction" made by String Theory?

In particle physics, you need to sum over exchanges of particles in both the s & t channels:

Figure 1

In string theory, you only have to sum over just s or just t, not both: The 2 sums are equal. That's because either sum represents the exchange of a string worldsheet, which when stretched thin in one direction (s) or the other (t) looks like a particle worldline in that direction. Here's a more complicated worldsheet for string scattering (I guess I need more figures on my webpages):

Figure 2

I guess you could call DHS duality more of a “postdiction”: The 1st string (then called “dual model”) amplitude was derived by requiring this property.

17. **Peter Woit**
April 11, 2007

theoreticalminimum,

Thanks for the editorial assistance. I fear that those slides ended up getting created under too much time pressure, so there’s a lot wrong with them, some of which you’re helping fix.

Math and Physics Roundup

April 10, 2007
Categories: Uncategorized

The latest "This Week's Finds" by John Baez discusses Felix Klein and his "Erlanger Programm", which essentially was the idea that geometry should be understood as the study of Lie groups G, their subgroups H, and coset spaces G/H. This, supplemented with Cartan's notion of a connection, allowing things that only locally look like G/H, is very much at the heart of our modern view of geometry. John gives links to quite a few things worth reading by and about Klein here. Another very interesting document is Klein's own history of 19th century mathematics "Development of mathematics in the 19th century".

I'm very much looking forward to the next installment of TWF, where John promises some insights into Hecke algebras. He also has a wonderful posting that generated an interesting discussion at the n-category cafe on the topic of mathematical exposition, entitled Why Mathematics Is Boring.

For some more mathematics blogging of the highest possible quality, see Terry Tao's postings on his Simons lectures at MIT, here, here and here.

I wrote a bit about the LHC Theory Initiative here last year. They have just announced the award of two graduate fellowships and say that they will be awarding postdoctoral fellowships in the future. Unclear from this if they were successful in their efforts to get NSF funding, the solicitation of applications for the fellowship just mentions an older grant to Johns Hopkins.

NPR has run a two part series on the LHC (here and here). The first part features CERN theorist Alvaro de Rujula. I had the great pleasure of taking a particle theory course from him when I was a student at Harvard a very long time ago. He cut an impressive figure, and provided a survey of the subject that was both enlightening and entertaining.

Scott Aaronson provides quotes from someone else (Gian-Carlo Rota) whose lectures I attended around the same time, including one that ends "You and I know that mathematics, by definition, is not and never will be flaky". I kind of agree with the sentiment in the full quote, but my experience with Rota back then was a rather weird one. For some misguided reason I had decided that since category theory was the most abstract kind of mathematics I had heard of, it would be a good idea to take a course on it. The only course on the subject was a graduate course down at MIT offered by Rota, so I started going down there to sit in on it. A few lectures into the course Rota all of a sudden announced that he had decided that only those students actually enrolled for credit should be taking the course, and that the several of us who were just auditing should leave. So we did, somewhat mystified (it's not like the room was over-packed or anything). To this day, I still don't know what that was about. Perhaps Rota knew that he was doing me a favor by stopping me from thinking about category theory at that point in my education, when in retrospect it seems likely that it really would have been somewhat of a waste.

There's a lot more about Rota at this web-site. His capsule reviews in the back of the journal he edited, Advances in Mathematics, provided outrageous entertainment for many years (although some might at times think that they were, well, flaky…).

Comments

1. **Coin**
   April 10, 2007

   (Gian-Carlo Rota) whose lectures I attended around the same time, including one that ends "You and I know that mathematics, by definition, is not and never will be flaky"… although some might at times think that [Rota was] well, flaky.

   Ahh, you must be careful always not to mistake *mathematics* for *mathemeticians* 🙂

2. **ObsessiveMathsFreak**
   April 11, 2007

   I don't understand physicists' almost fetish like obsession with Lie groups. I know it's nice and all, but I'm a bit confused as to what use all this mathematical theory is to the physics community? I thought physicists "just want the numbers". I'm finding it hard to believe that you make use of all this when you compute electron shells, solve GR equations, etc, etc. Do you make use of it?

3. **Peter Woit**
   April 11, 2007

   OMF,

   Computing electron shells is a classic application of representation theory of the SU(2) group. Angular momentum operators correspond precisely to a basis of the Lie algebra of SU(2), all that stuff in QM books about how to use them to analyze atomic spectra is pure SU(2) group theory and representation theory.

   For particle physics, you definitely need SU(3), not just SU(2), and this is a quite non-trivial example of a Lie group. Then there's all that stuff about energy, momentum, special relativity: the Poincare group. Lie groups are extremely useful in physics.

4. **MathPhys**
   April 11, 2007

   Peter,
   While I never liked Rota's book reviews in Advances (I found the ones I read too superficial), I highly recommend his "Indiscrete Thoughts", which is a collection of biographical sketches of matehmaticians he knew and many shorts essays on various topics.

   If you have no time for anything else in the book, please read the 4 page biographical sketch on Alonso Church (who was a professor of logic at Princeton when Rota was a student there). And the one on Solomon Lefshetz. Pure gold.

5. **andy**
   April 11, 2007

   Well, back in the days when I was doing nuclear structure shell model calculations, it very often was the case that diagonalizing the two-body matrices in a basis of irreducible representations of SU(3) — the "nuclear SU(3) model" [not to be confused with QCD SU(3)!] — gave very nice and elegant results that had a physical "meaning" (rotational bands). However, that's probably not what you meant! 🙂

6. **Brett**

April 11, 2007

Except for 18.03 (Differential Equations–the largest class at M.I.T.), Rota disliked teaching large classes. I don't know exactly why this was, but it was something that he was completely open about. He would frequently try to convince anyone who thought that they weren't "getting it" to drop a class, as well as other tricks like asking those not enrolled to leave (but if they came back at the next lecture, he wouldn't actually care). To his common refrain, "If you do not see this, I cannot explain it!" he would sometimes add, "And you should drop the course now!" Other professors certainly tried to winnow their classes down to what they felt were more manageable sizes too; only Rota was the least subtle. Perhaps this had something to do with his popularity, since most of his upper-level classes were indeed partially populated with people not up to grasping the material.

7. **ObsessiveMathsFreak**
   April 12, 2007

   > Except for 18.03 (Differential Equations–the largest class at M.I.T.), Rota disliked teaching large classes.

   He didn't paticularly like teaching that either. Well, at least not as much as he would have liked.

   I always find it uplifting to encounter such diatribes from experts in the field, especially when one always found the subject opaque. It's nice to see mathematics presented in a more human light instead of the rather dry and almost dogmatic presentations you normally get. It's a bit like Feynman's lectures, almost half apologising for the subject instead of handing down edicts from above. I think it's better to learn a more fallable subject than a polished one.

8. **Coin**
   April 14, 2007

   He didn't paticularly like teaching that [link goes to fascinating Rota talk on teaching undergraduate Ordinary Differential Equations courses] either.

   Wow. I feel almost kind of ill reading this link. In a lot of ways it's like reading a laundry list of the reasons why the DiffEQ classes I took as an undergraduate did not go well.

   —

   Something that interested me in that link– in one brief section of that talk Rota appears to be advocating using differential algebra not as an advanced tool for those people who already have a firm grounding in working with differential equations, but actually as a teaching tool for giving introductory students a better understanding of what exactly is happening when differential equations are worked with. Is this tactic Rota proposes something which there are actually examples of out there in the world? I would be very interested to see an example of such a thing, since I personally find I have a much easier time with those mathematical subjects where I have been given a clear idea of how the math is to be viewed in the light of abstract algebra (or some other tool which similarly forces the foundational or axiomatic parts of the mathematical techniques at hand to be made explicit). Rota makes reference to "Cohen's book of the twenties" as an example of an introductory treatment of differential algebra; does anyone have any idea what book exactly is being referred to here, or even which "Cohen" he refers to?

9. **anon.**
   April 15, 2007

   Maybe: Abraham Cohen, 'An elementary treatise on differential equations', 2nd ed., 1933?

10. **Coin**
    April 15, 2007

    Sounds like it, thanks.

11. **D R Lunsford**
    April 27, 2007

    Klein and Weyl are somewhat like Maxwell and Einstein as a pair. As time goes on, my respect for them just grows and grows – the luckiest thing that ever happened to me academically was to be shown the books "Elementary Mathematics from and Advanced Standpoint" at an early stage.

    Thanks for that pointer.

    -drl

MiniBooNE Results

April 11, 2007
Categories: Uncategorized

I won't bother to write up something about the background of today's MiniBooNE results, since Tommaso Dorigo has already done a better job than I ever could. He also provides a link to where the live feed of the seminar will be, starting at 11am CDT. I'll be in class at that time, but an hour later will try and attend the local seminar here at Columbia featuring Mike Shaevitz discussing the results.

And the result is….

No mu-neutrino to electron-neutrino oscillations of the sort that would explain the LSND result and require an extension of the Standard Model (beyond giving masses to the 3 known neutrinos). MiniBooNE was designed specifically to look for this, and has successfully ruled it out at 98% confidence level. They do see something anomalous in their data at low energy, but it is not compatible with being due to the kind of neutrino oscillations they were looking for. It's also true that they just first got a look at this data two weeks ago, still have a lot of work to do to see if there is some sort of background contamination they hadn't expected at these energies, or something they didn't know about low energy cross-sections. Maybe it will take them a while to sort this out, but the bottom line is that what they were looking for is definitely not there.

Press release here, paper to come soon.

Update: For an excellent description of the result from Heather Ray, one of the MiniBooNE experimenters, see this guest posting at Cosmic Variance.

Update:

Warning: serious people should stop reading now, the rest is a low form of entertainment.

For something truly hilarious, you really should be following Lubos's continually evolving misunderstandings of this experimental result, which he has taken as a reason for launching into another bizarre rant about me and Lee Smolin. As near as I can figure out, Lee and I are responsible for the misguided idea of designing an experiment like MiniBooNE to check into the possibility that LSND was seeing evidence of a sterile neutrino. His posting keeps changing (its URL is miniboone-confirms-lsnd, title now "Miniboone Refutes LSND"), with the early versions saying:

Evidence for several types of neutrino oscillations have been known for a decade or more. That includes atmospheric neutrino oscillations, solar neutrino oscillations, and a lab experiment called LSND in Los Alamos.

A simple oscillation in between two neutrino flavors – electron neutrino and muon neutrino – was a natural candidate to explain the observations but it couldn't explain details of the LSND data which is why the LSND results were questioned. Another natural candidate was a two-flavor oscillation that includes a sterile neutrino, a new kind of neutrino without a charged partner.

Today, Fermilab's MiniBooNE experiment has confirmed that the LSND results were correct and a more subtle explanation than the simple two-flavor oscillation is necessary. The result rules out the possibility that the observed oscillation is a two-flavor oscillation involving a new sterile neutrino. Their results indicate that there is something surprising that doesn't fit the most obvious model.

He does seem to have more recently gotten a clue about this and noticed that it doesn't confirm the LSND results, editing his posting and adding the standard obsessive rant. I see that in his previous posting (about a Harvard faculty meeting), according to him the proposed new Harvard core curriculum states that "All of science education must lead to increasing food production for the working class in the next 5 years ".

It seems that I am not only determining the course of neutrino experiments, but also setting the Harvard core curriculum. My powers are truly immense…

Update: Lubos has now changed the file-name from "miniboone-confirms-lsnd.html" to miniboone-refutes-lsnd.html, and deleted the comments from people explaining to him that he was confused. The new version starts off with:

I have erased several comments that only increased the amount of confusion, changed the filename to break links from crackpots' blogs, and hope that the text below is now more or less OK.

Comments

1. **a quantum diaries survivor**
   April 11, 2007

   Well, Peter, thank you: as they say around here "drink what you like, it's on me".

   I think it will be quite interesting to see if they kill their own upgrade or what…

   Cheers,
   T.

2. **Shantanu**
   April 11, 2007

   Unfortunately I am getting a "User Limit Exceeded" message. looks like
   lots of people have connected.

3. **Thomas Love**
   April 11, 2007

   http://www.interactions.org/cms/?pid=1025099

   has the results

4. **andy**
   April 11, 2007

Nothing! (except for a weak anomaly at the lowest energy which they're trying to hype.)

5. **Doc Snyder**
April 11, 2007

http://www.fnal.gov/pub/presspass/press_releases/BooNE-box.html

6. **John**
April 11, 2007

Lubos is getting a little flak in his comments I see. ha.

7. **Peter Woit**
April 11, 2007

John,

Don't worry, he has dealt with that problem in the usual way.

8. **Bob Jones**
April 11, 2007

Peter,

I guess I am one of the less serious readers of your blog. And apparently you have REALLY pissed Lubos off. I only read his blog by linking from yours…and now he is blocking that with a hilarious message that reads:

"Error
Sorry but I really don't intend to share readers with that particular aggressive liar, parasite, and crackpot. If you find it appropriate to read Not Even Wrong, you're just not welcome on The Reference Frame. This type of direct links won't work. I've had huge problems with the scum pumped onto my blog by the Not Even Wrong website. I apologize if you're not a part of it but there's no way to distinguish.

You can open motls.blogspot.com if you wish.

L.M."

Wow!! Thanks as usual for the useful information and sometimes entertainment!!!

9. **gunpowder&noodles**
April 11, 2007

Still, I'd rather read LM's [current] summary than Heather Ray's hilariously anticlimactic and infinitely tedious explanation of the fact that they didn't find *anything*…..

10. **Heinrich**
April 11, 2007

This blog is not totally correct. True, miniBooNE ruled out standard neutrino oscillations as a solution for the LSND anomaly, but that possibility was already at the verge of being ruled out by combinations of results from solar, atmospheric, reactor and collider neutrino experiments. On the other hand we proposed a spectacular solution to the LSND anomaly involving neutrino shortcuts in extra dimensions, published in Phys.Rev.D72:095017,2005 [hep-ph/0504096]. If you look at Fig. 5 in this paper, you will see that for a choice of the resonant energy in the region 200-300 MeV we not only predicted the small counting rate for electron neutrino events above 475 MeV, but also the large rates in the 300-475 MeV region!
While the anomalous effect seen by miniBooNE might have a conventional explanation, it might well be the first hint for extra dimensions of spacetime!

11. **mclaren**
April 11, 2007

Like most people, as soon as I encounter the word "Lubos" or "motl" I stop reading.

Clearly something odd is going on with neutrinos. Does anyone have any take on whether this new result rules out neutrinos as the putative source of the missing dark matter? I.e., as I understand it, neutrino oscillation of the standard kind requires that neutrinos have mass, which in turn implies that they *might* be responsible for some or all of the missing matter.

Does this new result, in short, imply anything about neutrino mass?

12. **Kea**
April 12, 2007

mclaren

No, it doesn't rule out DM sterile neutrinos that appear in weird CP sectors, as Dharam points out.

13. **Matti Pitkanen**
April 12, 2007

The neutrino energies of LSND and MiniBoone are 60-200 MeV and
300-1500 MeV and oscillations in LSND mass range are found to be absent above 500 MeV. Evidence for oscillations is found below 500 MeV.

Hence LSND and MiniBooNE are consistent if one accepts that neutrino mass scale depends on its energy as TGD strongly suggests. For details see the posting at [my blog page](#).

Matti Pitkanen

14. **Kris Krogh**
April 12, 2007

We’ll also hear the preliminary results from another major experiment this week. Those from Gravity Probe B will be announced Saturday at the meeting of the American Physical Society in Florida, preceded by a NASA press/media event. (My prediction: general relativity fails.)

15. **woit**
April 12, 2007

mclaren (and others),

Remember, this is a null-result. All that happened is that this experiment was looking for something specific seen by LSND inconsistent with the standard picture of three massive neutrinos (the masses and abundances are considered too small to explain dark matter), and they showed it’s not there. You can’t claim this as evidence for your favorite exotic theory. The only anomaly they see is the low energy one, and this is based on data they just first looked at two weeks ago, and have not had the time to properly evaluate. They are in no way claiming this as a reliable measurement, their only claim is to have ruled out the supposed oscillations.

16. **Thomas Larsson**
April 12, 2007

OT: A review of NEW and TWP by Martin Gardner can be found here. He ends by quoting Glashow’s poem.

17. **tomj**
April 12, 2007

So if it rules out the inconsistent LSDN result, that is pretty important science, isn’t it?

Does this mean that the LSDN result is no longer considered a valid result, like oops, never mind?

18. **Peter Woit**
April 12, 2007

tomj,

Yes, the importance here is ruling out LSND, specifically the idea that it was seeing neutrino oscillations. The experiments were not the same, so you can still try and come up with something consistent with both LSND and MiniBoone, but that is very hard.

19. **Thomas Love**
April 12, 2007

The two experiments, LSND and MiniBoone, yielded inconsistent results. Doesn’t that mean we need a third experiment to see which was correct? It doesn’t seem right to accept MiniBoone uncritically just because it is more recent.

20. **Bee**
April 12, 2007

but didn’t LSND use anti-neutrinos where MiniBoone has neutrinos? I mean, yes, MiniBoone rules out the sterile neutrino scenario (which, as Heinrich pointed out was pretty much ruled out by the SNO neutral current data anyhow), but if one argues there might be an asymmetry between nu/anti-nu this does neither confirm nor refute LSND?

21. **Bee**
April 12, 2007

Thomas: the point is in the 3-flavor scenario LSND *is* inconsistent with the other neutrino data (lots of: solar, atmospheric, reactor). You need to come up with some ad hoc additional explanation to incorporate LSND (like e.g. sterile neutrinos) if you believe their data analysis is correct (LSND data is commonly excluded for global fits of neutrino osci). MiniBoone has re-checked the LSND energy sector and did not find any evidence against the standard 3-flavor scenario and its parameters (mix. angles, \Delta m^2).

22. **Peter Woit**
April 12, 2007

Bee,

Yes, in principle anti-neutrinos could behave differently, and I gather MiniBoone is now running with anti-neutrinos, but I don’t know if they will have the statistics to directly test LSND. Also, I don’t know how hard it is to construct models where antineutrinos would oscillate but not neutrinos, without violating CPT or running into contradiction with other experiments.

23. **tomj**
April 12, 2007

What is the importance of the results being collected in this tamper proof box? Is this just a metaphor for computer data that nobody can see while the experiment is being run?

24. **Peter Woit**
April 12, 2007

tomj,

It’s not that it’s tamper-proof. The idea of a “blind” analysis like this is to try and do the data analysis while not knowing how the choices one makes affect the final result, to keep oneself from subconsciously skewing one’s choices to get a certain result. The problem with a blind analysis is that you may miss problems with the data that only show up in the final result, not in the partial results you allowed yourself to look at. This may be the cause of the low-energy anomalies they found in their final result.

25. **Brett**
April 12, 2007

For neutrinos and antineutrinos to oscillate differently, you need CPT violation, and you can’t break CPT very easily. To have CPT violation, you need to give up Lorentz symmetry, or locality, or unitarity, or energy positivity, or something equally basic. You can’t just resolve the discrepancy between LSND and MiniBooNE by introducing a “CPT-violating mass”; no such operator exists. You instead have to introduce a neutrino mixing interaction that violates one of the conditions that I mentioned, and–very importantly–these kinds of exotic operators will not give oscillations with the same L/E dependence that is seen in mass oscillations. For example, with Lorentz violation, it would be quite possible to have oscillations only at lower energies (where they were seen by LSND) without actually violating CPT, and thus have the oscillations in both neutrinos and antineutrinos.

26. **M**
April 12, 2007

even LSND interpretations based on sterile neutrino(s) were already significantly incompatible with bounds from other neutrino experiments: this is why viable models for LSND already involved ingredients such as extra dimensional sterile neutrinos with CPT-violating varying masses. Using these tools, it is probably possible to write down models that fit both the anomalies in MiniBoone and LSND.

About the part of the post that serious readers should not read, I signalled to Lubos that what he wrote in the comments to his post contains mistakes in both neutrino physics and string theory. But instead of censoring his comments, he preferred to censor mine.

27. **H-bar**
April 12, 2007

I get forwarded to this site from the link to Lubos' posting: http://www.physics.harvard.edu/~motl/crackpot-not-even-wrong.html

28. **Wolf-gang**
April 12, 2007

> by introducing a "CPT-violating mass"; no such operator exists.

I think Gabriela Barenboim has found one though I don't have the arXiv reference handy now.

29. **Bee**
April 12, 2007

now how did that happen? I certainly haven't said I want to break CPT to explain LSND, it doesn't even seem to me remotely appealing. I was just wondering why an experiment set up to clarify LSND wasn't as close to the LSND experiment as possible to begin with.

30. **Ari Heikkinen**
April 12, 2007

Heh, that bit in your "entertainment" section nicely proves that people who think are experts at everything (no one is) will sooner or later end up making themselves look like fools.

31. **M**
April 12, 2007

Bee, because LSND and MiniBoone have many persons in common, so they cannot confirm LSND by repeating it. This is why they did a blind analysis and moved from neutrinos to higher energy anti-neutrinos, that are detected in a different way.

The present unclear situations arises because MiniBoone was planned to confirm or exclude LSND oscillations, but in the meantime the oscillations suggested by LSND had been indirectly disfavored by other experiments, and more exotic interpretations have been proposed.

For example, one proposal was oscillations among active neutrinos (without any sterile neutrino) with CPT-violating masses, but this was indirectly excluded by KamLAND. At this point both CPT-violating masses and sterile neutrinos were considered socially acceptable, so that desperate theorists could move to sterile neutrinos with CPT-violating masses…

32. **Thomas Love**
April 12, 2007

Brett Says:

"For neutrinos and antineutrinos to oscillate differently, you need CPT violation…"

But we live in a sea of neutrinos. We do not live in a sea of antineutrinos. It would be very possible "For neutrinos and antineutrinos to oscillate differently" if that oscillation is due to the interaction with the background sea.

We do not need CPT violation.

33. **Brett**
April 12, 2007

Barenboim's model is indeed CPT-violating, with a term that can be cast in a form that looks like a mass. It gets around the CPT theorem by being nonlocal, something which tends to cause a lot of problems when nontrivial interactions come into play. (I don't claim that this particular model necessarily suffers from those problems though; I myself have worked on similar Hilbert-transform-type nonlocal models that can circumvent some of the usual difficulties.)

I don't think this theory has a mass term in the usual sense though. The theory has non-canonical commutation relations for the field, which is problematic for a particle interpretation. More generally, in theories that violate one or more of the conditions that ensure CPT, different notions of the mass (which are equivalent in ordinary physics) may diverge. The inertial mass, gravitational mass, rest energy, and location of the pole in the propagator will not generally coincide.

34. **Bee**
April 12, 2007

Bee, because LSND and MiniBoone have many persons in common, so they cannot confirm LSND by repeating it.

This seems to me like a rather weird reason. If its a scientifically performed experiment it shouldn't depend on the persons.

so that desperate theorists could move to sterile neutrinos with CPT-violating masses…

That's what I am afraid going to happen. Everybody whose model is in danger now will jump and add some extra parameters, try to get published before also this window closes …

35. **Matti Pitkanen**
April 12, 2007

I would still emphasize that the neutrino energy ranges in LSND and MiniBoone 60-200 MeV and 300-1500 MeV. Low energy in MiniBoone means something still above LSND and at low energies the effect was found. For some reason this point has not been emphasized explicitly in blog discussions and has not prevented people from drawing quite strong conclusions in most blog discussions.

To me the result says that the two experiments are consistent if one accepts the possibility that neutrino mass depends on neutrino energy measured with respect to rest frame defined by laboratory. This combined with relativistic invariance of course means that neutrinos must have rather delicatate kind of interaction with matter. For my own proposal see my blog.

36. **Marco**
April 13, 2007

Bee,
I think the point in not repeating the exact same experiment is that you want to have different systematics. The important quantity that governs oscillations is L/E (= baseline of the neutrino beam / energy of the neutrinos). The strategy for MiniBooNE was to increase both L and E with respect to LSND, while keeping a similar L/E. At a different Energy, one has different cross sections, different backgrounds etc etc: in short, different possible sources of systematic errors. But since the L/E was similar, if the LSND result had been due to (oscillation) physics, it should have popped up. Which it did not.

As for more "extreme" theory models: well, what M was saying in the comment above is that CPT violation + sterile neutrinos has already been put on the market (hep-ph/0308299), because CPT violation alone or sterile neutrinos alone were already in trouble before MiniBooNE. It's up to your taste to like it or not.

Now let's see what these "desperate theorists" (as you said) will find to explain that little excess at low energies in MiniBooNE. For the moment, hats off to Heinrich's model (see his comment above)! It's certainly a bit of an extreme model, but they had predicted something like this sort of excess. I think he is celebrating with some good Alabama whiskey…

37. **Bee**
April 13, 2007

Hi Marco,

🙂 I've played around some years with extra dimensional neutrino models, and believe me, some of them were far more extreme. These models with mass varying neutrinos – well, interesting, but I admittedly find it very implausible. But yes, I see. I guess the intention was to check several points in which LSND was different but not mingling them all together?

Hi Matti,

thanks for pointing out that the energy range was different as well, I must have missed that. I would suspect though that if there really is an effect in this case it's less a neutrino effect, as a data analysis problem. As Marco mentions, for analyzing the detected neutrinos you need 3 factors: production (how many start), oscillation (where do they go), and detection (cross-section). I can't recall all the details but at some point I tried to figure out how the cross section for neutrinos are modeled in the monte-carlo analysis and found some are based on really poor data. Best,

B.

38. **Bee**
April 13, 2007

I mean, look at the available data points in the

39. **Bee**
April 13, 2007

sorry, I always forget that HTML doesn't like 'smaller than' …

What I meant to say, look at the available data points in the region smaller than .5 GeV

http://www-boone.fnal.gov/cross-sections/boone_reference.html

And this is for neutrinos. For anti-neutrinos it's even worse.

(Peter, feel free to delete the previous comment)

40. **Marco**
April 13, 2007

Hi Bee,
first of all, I have also played with neutrinos in extradimensions, but my models were perfectly natural and incredibly compelling. 😉

But since we are on the topic, here is my partial list of things proposed for LSND, and we could rate them on a scale of "extreme" or "plausible": it could be an interesting exercise in the sociology of science. I am not sure it is very scientific though. (I don't cite references but of course I can provide them. And if I'm forgetting some category I'd like to know.)

– the well known sterile neutrinos with about 1 eV mass (but they were disfavored by a number of things even before MiniBooNE, among which cosmology)
– Heinrich's model: sterile neutrinos + one extradimension with hawaiian waves (how does this score??), still it predicted the right thing for MiniBooNE.
– models with CPT violation in the usual 3 neutrinos: disfavored, so people moved to CPTv+sterile
– on another side: models with Mass Varying neutrinos (their mass varies according to the matter around them, or according to the density of other neutrinos), both for LSND and for a connection to Dark Energy
– and now: can you guess what's coming? yes, people also proposed Mass Varying Sterile Neutrinos

So you see you definitely need to work as soon as possible on a model of Mass Varying Sterile CPTviolating ExtraDimensional neutrinos, but maybe our friend M above is already on it.

More seriously, I think this shows that the LSND anomaly was really hard to reconcile with a number of things. So it's good that MiniBooNE settled the issue. Unless, of course, we have now something anomalous again from MiniBooNE itself…

41. **Bee**
April 13, 2007

Hi Marco,

but my models were perfectly natural and incredibly compelling yeah, yeah, that's what I say in my talks as well 😉 . Regarding your partial list, I would add the above mentioned uncertainties for the cross-sections. I mean, one could go so far to ask if they have actually measured the oscillation with knowing the cross-section or vice versa. I am generally sceptic about having only one extra dimension, not to mention that this most often doesn't really explain anything. It just gives you another parameter. For all points the question is: why would such an effect only be important for the neutrinos? Best,

B.

42. **Marco**
April 14, 2007

Hi Bee,
I tend to agree that neutrino cross sections (and their energy dependance) are a delicate point. I think however that the MiniBooNE people have been very careful on this, using data from other experiments etc. Still, if you find some flaw let us know.

If I understood correctly, your other comment is that it looks a bit artificial that people invoke all those exotic things always for neutrinos and not for other particles. I.e.: why should neutrinos always be the odd ones in town?

Well, yes, but I think that in reality there is a different reason for any one of the oddities invoked. Some are more convincing, some are less.
For example, when one says "sterile neutrino" one really is taking a shortcut to say: one of all the possible "spin 1/2 fermions that are neutral under the SM SU(2)L x U(1)Y gauge group"; and since there are many of these on the market (from SuSy, from additional simmetries, even from strings!…) a sterile neutrino is not toooo exotic. If in addition it is light for some reason, the mixing with the ordinary neutrinos is likely, so here we are.
For the case of extra-dimensions, you know, once you have bought a large extra-dimension you should make the most of it and put in it everything that can fit, so the graviton and also a sterile neutrino, because they are not tied to our brane by SM forces. So here we are again having neutrinos play the exotic.
For the case of Mass Varying Neutrino models, I think the motivation was the observation that the density of Dark Energy in the Universe today is similar to the energy density of massive neutrinos today, so DE and neutrinos could be coupled. So again neutrinos end up being singled out.
And so on.

But of course the underlying reason in all this might be (more mundanely) that neutrinos are the least tested of the Standard Model particles, so we still have freedom to play.
Sorry if I talked too much.
Best, Marco

43. **Bee**
April 14, 2007

Sorry if I talked too much. No problem. You can't beat me. I managed to give a talk today which I though would take one hour, but I wasn't finished after two hours.

Regarding extra dimensions and neutrinos, what I found intriguing about this approach is that it explains why the neutrino IS the odd one out. The reason being that a right handed neutrino (if there is one) does not carry any charges of the standard model (electrically neutral, electroweak singlet), and therefore – like the graviton – is not bound to the brane. One doesn't have to invent another sterile (left handed) neutrino (though one can of course, one can always make things more difficult).

But yes, it always seems to me kind of odd to include a new feature but then only include it for part of the particles. It would seem more natural to assume it exists for all particles (but it might be only observable for some).

Regarding the cross-section, I am sure the MiniBoone people have been careful! It is just not clear to me what the errorbar on that cross-section is (its a line! can it be there is no errorbar to it?). The point I was trying to make is if I look at the data points it seems to me there aren't so really many (in fact, none) in the relevant energy region. If I look at the paper from which the curve comes, it is not clear to me how large the theoretical uncertainty is (there is a lot of talking about Method I and II, and I admit I didn't look into to details), and whether this uncertainty can be fixed with the data points in the higher energy region? I'm not saying this is a flaw, this is just something I don't understand.

Best,

B.

44. **CG**
April 15, 2007

Sorry to cut in, but I have a rather naive question which seems somewhat tangential to your discussion, though theoretically based, rather than experimental. I was really wondering what the basic arguments were in favour of only having 3 flavours to each of the particle families (eg neutrinos) in the Standard Model. From a lay-scientist's point of view (mine), it does not seem at all obvious why this should be the case. i.e, why not 4? 5? 6? infinity?

Thanks.

45. **woit**
April 15, 2007

CG,

In the standard model, particles come in generations, with one neutrino in each generation. We've seen all the particles in three generations, no evidence at all for a 4th generation, although in principle there could be one (or more). In addition, the observed decay width of the Z particle implies that there cannot be more than three low mass neutrinos. If there is a 4th generation, it has to be unlike the others, having a neutrino of a much much higher mass than the other 3.

46. **CapitalistImperialistPig**

April 15, 2007

Poor Lubos,

I worry that he is losing touch with the real world. You would think Harvard could afford to find medical help for whatever it is that’s really bugging him. I fear that a rather brilliant mind is going largely to waste.

47. **D. V. Ahluwalia-Khalilova**
April 16, 2007

The L/E for the MiniBoone at 475 MeV is nearly coincident where KARMEN data starts, while LSND’s L/E range has an overlap with that very line for 2nu oscillation analysis (as chosen by MIniBoone).

The KARMEN, LSND, and MiniBoone data are hard to understand in the context of any oscillation analysis (irrespective of any mismatch between the CP properties of the SM and the sterile mass eigenstates). Indeed, neutrino sterility should come from some new physics; and one has a big playing field for that. But, in the absence of a constraining principle, or strong experimental data, such a game is at best a good mathematical science fiction.

48. **Thomas Larsson**
April 16, 2007

The MiniBooNE paper is now available on [the arxiv](#).

49. **island**
April 16, 2007

CIP said:
I worry that he is losing touch with the real world. You would think Harvard could afford to find medical help for whatever it is that’s really bugging him. I fear that a rather brilliant mind is going largely to waste.

Lumo’s behavior is highly typical of extremists and fanatics who grasp at any straw that they can reach in order to rationalize their belief system in the face of reality. He initially attempted to do exactly that when he first heard them talking about the low energy descrepancy and “other exciting physics”.

I would expect to find lubos trying to vindicate string theory long after it’s dead and gone to the rest of the world.

50. **Robert Oerter**
April 16, 2007

CG:

As far as the math is concerned, the Standard Model can accomodate any number of particle families, and hence any number of neutrinos. (Note: only one neutrino per family.)

There are two independent ways we know there are only 3 neutrinos in nature. One is the decay rate of the Z0 particle. Since the Z0 can decay into a neutrino and an antineutrino, the more families there are, the faster the Z0 must decay. The decay rate has been measured and shows there are 3 families.

Secondly, in the first millisecond after the Big Bang, extra neutrinos would affect the amount of hydrogen, deuterium, and helium that gets produced. Astrophysicists have measured the relative amounts of these in intergalactic space, so they can constrain the number of neutrino species out there. Their results also give 3 families.

51. **Bee**
April 16, 2007

Hi CG,

I was about to write the same as Robert. One should add that this applies for flavour neutrinos with masses smaller than 1/2 times the Z0 mass (somewhere around 45GeV) , you find [a very brief summary here](#). Best,

B.

52. **CG**
April 16, 2007

Thankyou very much Bee and Robert for your comments. I still admit that I find the idea of there only being 3 families very strange, though it is possible that, if there were heavier eigenstates, they might not be stable, ie only the first 3 eigenstates are stable, and hence are observed. Or it is possible that the notion of being able to think of a neutrino as a particle also becomes meaningless after reaching a certain energy because the local structure of space encompassing the object is no longer properly Lorentzian, but I am starting to wander into the realm of the highly speculative here.

Nonetheless, I just want to clarify– since the Z_{0} has a finite energy (~45GeV, you said I think)– surely this limits the analysis of which you speak to the measurement of neutrinos of mass less than or equal to this value? So you could have very heavy neutrinos that you are missing? Or is that a silly question? I understand that the first few neutrino flavours are [[extremely]] light, and a significant mass difference between families 3 and 4 (presuming there is a 4) seems very unlikely.

Alternatively, I suppose, you might have that the transition between Z_{0}’s and eigenvalue 4 neutrinos is forbidden by limitations on how the geometry can degenerate, hence making it possible for mass

53. **CG**
April 16, 2007

(continued from last post)

54. **CG**
April 16, 2007

less than 45 GeV evalue 4 neutrinos to occur, but again this seems very highly speculative.

(for some reason my last post got clipped too, I have no idea why >

55. **Peter Woit**
April 16, 2007

My spam filter ate my original response to CG, which I just liberated.

There is some number of generations above which you lose asymptotic freedom, but otherwise, any number of generations is possible. Particles in the second and third generation are already unstable, if there are higher generations they would also be unstable. The only way to get more generations of the same kind as we know now would be if their neutrinos are very massive so that the Z can't decay into them.

If you start speculating about new generations with neutrinos having completely different properties due to changing the structure of spacetime, you can get all sorts of things, but unfortunately I have to discourage attempts to engage in this kind of speculation here….

56. **Marco**
April 17, 2007

CG,
on the issue of the number of generations and number of neutrinos I can only add that the observations of the abundances of Helium and Deuterium (ie the predictions of Big Bang Nucleosynthesis: BBN) actually constrain the total number of active + sterile neutrinos (with some caveat). The SM generations are instead active by definition. So I would keep the Z decay as the main "argument" for having 3 light SM neutrinos, with BBN being a more general probe.

Incidentally, while it is true that BBN finds roughly 3 neutrinos, some analyses in these days claim that other cosmological probes give evidence for more than 3 neutrinos. The extra ones have therefore to be sterile. But it's still uncertain ground.

Bee, regarding the cross sections you may be right in having some uneasiness. Maybe one could ask someone in the collaboration for more details.
Best, Marco

57. **Bee**
April 18, 2007

Hi Marco,

Maybe one could ask someone in the collaboration for more details.

That's what I did, but haven't yet come to any conclusions. Check my blog every now and then, I will try to write something if I can answer that question. Best,

B.

58. **Marco**
April 19, 2007

Bee said:
Check my blog every now and then .
Of course! No need to say: I already do so…
Thanks,
Marco

59. **E.**
April 21, 2007

If there turns out to be a fourth generation, it will be discovered at LHC. There are strong constraints on such a fourth generation, but who knows?

60. **Bee**
April 23, 2007

so that desperate theorists could move to sterile neutrinos with CPT-violating masses…

That's what I am afraid going to happen. Everybody whose model is in danger now will jump and add some extra parameters, try to get published before also this window closes …

Here we go, these people really move fast:

http://arxiv.org/abs/0704.2593
CPT and lepton number violation in neutrino sector: Modified mass matrix of neutrino coupled to gravity
Authors: Monika Sinha, Banibrata Mukhopadhyay

Abstract: We study the consequences of CPT and lepton number violation in neutrino sector. […]

*The oscillations between different kinds of neutrino
and antineutrino flavor have been observed form solar,
atmospheric and LSND data. The three pieces of observation
indicate three values of mass-squared difference
of three different orders. With three families of neutrino,
one can obtain only two independent mass-squared differences.
Therefore, observations require the introduction
of fourth neutrino which must be sterile in the standard
model. But many difficulties arise with the introduction
of fourth neutrino as discussed in literature (e.g. see [1]).*

*As an alternate proposal to accommodate the results,
many authors have proposed CPT violation in neutrino
sector [2, 3, 4]. One can either introduce a new particle
(sterile neutrino) or allow CPT violation to take care of
all experimental results with present data. However, very*

recently, MiniBooNE results have been declared which shows that LSND experimental results can not be explained simply by neutrino oscillation.

61. **orlando**
    May 4, 2007

    Hi

    to comment about the Limit from Z0 decays. This limit did not say that you only have 3 neutrinos.
    The Z0 width count number of left neutrinos!!
    If you have any lighter massive neutrinos then half Z0 mass then you always have 3 effective neutrinos, because the the unitarity constraint.

    There is a mistake between flavor eigenstates and mass eigenstates.
    If you have very heavy massive neutrinos, above half Z0 mass, then you test steriles neutrinos because the heavier states are suppressed.

    The best, Orlando Peres

62. **orlando**
    May 4, 2007

    MINI-BOONE and LSND are correct. In this week appear a paper that says that that can made compatible the negative resulys of MINI-BOOne and positive results of LSND.

    They work in 3+2 scenario, with CP violation. why CP violation? because LSND measure anti-neutrinos and MINI-BOONE measures neutrinos.
    The paper is http://arxiv.org/abs/0705.0107.

    They compute for all parameters the best fit point and found out for a slight non-zero CP phase you can have a stronger suppression of nu_mu -> nu_e probability and non-zero anti nu_mu -> anti nu_e probability.

    The values for difference of masses is around 1 eV^2 and 2 eV^2.

    the best, Orlando Peres

String Kings – The Director's Cut

April 13, 2007
Categories: Uncategorized

I just learned from Cosmic Variance that a review of the Director's Cut version of String Kings is now out. It seems that the Director's Cut version includes more scenes featuring a certain "man on the edge" in New York City…

Comments

1. **milkshake**
   April 13, 2007

   In the String Kings III the crime syndicate with its new ruthless and increasingly decoherent Czech boss comes crashing down in a violent showdown and in the final visceral balcony scene the gutters of arXiv will be overflowing with freshly-spilled data

2. **Kea**
   April 13, 2007

   OK, I laughed a lot. But the film is a little light on its female casting.

3. **Jimbo**
   April 13, 2007

   If this does'nt cause you to crack up repeatedly, you are BAD CRAZY ! My only critique of the casting is that Segal is approx. 1/2 meter taller than Brian Greene !

4. **joe**
   April 14, 2007

   The movie "The Red Violin" could be used as parody of the futility/tragedy of ST. Links: 1, 2, 3

   From this review are some hauntingly relevant remarks on the hubris of "Beauty":

   "The soul is born in beauty and feeds on beauty, requires beauty for its life."
   — James Hillman

   "..is a spiritually rich and musically sublime drama about the soulful dimensions of beauty. It also presents some of the ways in which beauty has been perverted by people's hubris and their treating it as a commodity."

   "..predicting a long journey [Italy, Austria, Oxford/England, China, Montreal] characterized by moments of great happiness and disaster"

   "New York expert Charles Morritz (Samuel L. Jackson) appraises it as "the perfect marriage of science and beauty."

   As I understand it, the ST "gang" is fixated & in love with mathematical beauty?

   Is this movie an example of "Life imitating Art [which is itself imitating Life]"?

   "Theory & Experiment":

   I was impressed by one scene, where the violin teacher schools the student on the important of Theory:

   "violin..cheese.
   There's something about your playing that eludes me..I've decided to analyze it systematically with a scientifc method.
   First , your bowing. And your left hand phrasing. Your detache, ornamention, and your taste.
   And theory! Theory, too, is important
   You need a lot more than inspriation to play the violin.
   You need method, you must think.. and work.
   ..
   if you play well, you'll enjoy the finest fare. My boy, play well, and there will be cheese.
   violin..cheese"

   The ST critics are claiming that 2 decades later of "stringing", there is no sign of "cheese". I guess ST gang would claim the critics are guilty of "Whine", or as the popular phrase goes "If they bring the Whine, I'll bring the Cheese". As pointed out recently, ST conferences should have a "Alternatives" to ST for balance..thus making it a "Whine & Cheese" exhibition.

   BTW, there was a good sequence where the female Communist party leader said "Sometimes the teacher can learn from the student". This is reminiscent of Feynman's philosophy, of not taking a position where teacher-student interaction is non-existent (like at IAS/Princeton). Take this metaphor further, maybe a "counter-point" to ST by critics could be useful to ST development.

   There is also a good sequence about "dogma VS revolution":

   "The [Communist Chinese] authorities denounce Western music as decadent and the old music teacher reminds them that Beethoven was a great revolutionary."

   Is ST "revolution" (its intention), or just a dogma by the "Gang of 4"?

5. **Jonathan Vos Post**
   April 14, 2007

   It still doesn't sound like Space Opera. Close, but not hitting on all 20 criteria.

   Brian W. Aldiss, in his anthology "Space Opera" [Garden City NY: Doubleday, 1974]

identifies various key indicators of “Space Opera” as (if I may interpolate
from his delightful introduction):

(1) Style and Mood staunchly traditional
(2) Hitherto unknown places to explore
(3) Continuity between Past and Future
(4) Tremendous sphere of space/time
(5) A pinch of reality inflated with melodrama
(6) A seasoning of screwy ideas
(7) Heady escapist stuff
(8) Charging on with little regard for logic or literacy
(9) Often throwing off great images, excitements, aspirations
(10) The Earth should be in peril
(11) There must be a quest
(12) There must be a man to match the mighty hour
(13) That man must confront aliens and exotic creatures
(14) Space must flow past the ports like wine from a pitcher
(15) Blood must run down the palace steps
(16) Ships must launch out into the louring dark
(17) There must be a woman fairer than the skies
(18) There must be a villain darker than a Black Hole
(19) All must come right in the end
(20) The future in space, seen mistily through the eyes of yesterday

Well, not all these indicators are valid even for each of the stories he’s assembled, but his list is indicative.

For details, see:

http://www.magicdragon.com/UltimateSF/thisthat.html#spaceopera

6. **Kea**
April 14, 2007

…and they somehow missed the large Elephant walking through Manhattan.

News From the Landscape and Elsewhere

April 17, 2007
Categories: Uncategorized

At the big annual APS meeting, now going on in Jacksonville, of the 9 plenary talks, one is about particle theory. The talk is entitled “String Theory, Branes and if You Wish, the Anthropic Principle” and it was given by Shamit Kachru of the Stanford group. Here’s the abstract, which besides the usual claims that string theory is “our most promising framework for a unified theory of the fundamental interactions” and that “the underlying theory is unique”, also makes the claim to have “testable ideas about inflation and particle physics”. No clue what these ideas are, so I don’t know if they include the testable prediction the landscape makes about the proton lifetime. Also unclear why the Anthropic Principle is being demoted to “if You Wish”. Lots of experimental talks on particle physics at the conference, here’s a Fermilab press release on CDF and D0 results discussed at the meeting. Lawrence Krauss was speaking on “Selling Physics to Unwilling Buyers”, I wonder what that was about. More about the meeting at the Physics Meetings blog.

David Ben-Zvi has put up on his web-site his lecture notes from last week’s series of lectures in Oxford on geometric Langlands. As usual, a very readable survey of the subject, emphasizing links to representation theory.

For another source of material about representation theory and the (non-geometric) Langlands program, see the web-site hosted by the Clay Mathematics Institute devoted to the collected works of James Arthur.

There’s yet another round of discussion on bloggingheads.tv between science writers John Horgan and George Johnson. This week the LHC and the state of particle physics are some of the topics they consider.

From Fermilab, various new sources for discussion of the future of experimental particle physics include:

A web-site for the steering group tasked with developing a roadmap for future use of US accelerators. This week’s meeting includes a presentation on reconfiguring the Fermilab accelerator complex to produce larger numbers (factor of 3 more) protons, for use by neutrino experiments and others.

The Fermilab Physics Advisory Committee met on March 29-31, here are the presentations and report.

Last week there was a workshop devoted to considering what effect early data from the LHC would have on plans for the ILC (via Tommaso Dorigo).

Finally, Steven Miller, author of “String Kings”, has a new blog he is working on, devoted to essays on mathematical physics, theoretical biology and the history of science.

Update: Two more.

Seed magazine has a series of “cribsheets” about science. For physics, they cover nuclear power, the elements, and now string theory. The lack of predictivity of the theory is given a positive spin as being due to the “rich diversity” of string theory. At Cosmic Variance, Sean Carroll approvingly refers to this as “it only refers glancingly to the anthropic principle, which is a much more accurate view of the state of discussion about string theory than one would get by reading blogs.”

Nature has an article about the state of the LHC and the possibility that the Tevatron might be the first to see the Higgs. LHC project manager says that they were already running about 5 weeks behind schedule before the problem with the quadrupoles appeared, but says “In my view the magnet problem has been blown out of proportion… It is a very small part of a bigger picture.” If the schedule slips much more, there might not be time for an engineering run in 2007, and the first science run might be delayed until later in 2008.

Update: Thanks to commenter F. for pointing to the slides from Kachru’s talk. It’s a clear presentation of the moduli stabilization problem and the techniques that he and others used to solve it, while at the same time making the landscape problem much worse. The “testable” ideas mentioned in his abstract are the usual sort of thing behind claims like this: not actual tests of string theory, but effects in certain very specific models among the infinite variety of ones you can get out of string theory. Kachru doesn’t much address the issue of whether the landscape framework is testable science in the conventional sense, other than to describe people’s attempts to use eternal inflation to explain how the vacuum gets selected and try and get physics out of this as “notoriously confusing.” He also describes counting of vacua as favoring high-scale supersymmetry breaking, so maybe there is a prediction: no supersymmetry at the LHC.

Update: For the latest from FNAL on the LHC magnet problems, see here.

Comments

1. **Tim**
   April 17, 2007

   Peter,

   “No clue what these ideas are, so I don’t know if they include the testable prediction the landscape makes about the proton lifetime.”

   Would you ellaborate on that? The landscape makes an experimentally testable predition?

   Best,
   Tim

2. **Peter Woit**
   April 17, 2007

   Tim,

   One of many problems with the anthropic landscape is that conservation of baryon number is rather special, typical vacua will have baryon number violating interactions. The bounds on the proton lifetime are something like 10^32 years, whereas, anthropically, all that is needed is for only a small fraction of protons to have decayed since the big bang (10^10 years ago). So, unless you can find a landscape argument for why the GUT scale has to be so high (and I don’t know of any) protons should decay much, much faster than they do in the real world. The landscape folks seem to take the attitude that they can claim “testable predictions”, while ignoring the fact that similar or even better grounded “testable predictions” of the scenario are already falsified.

3. **dan**
   April 17, 2007

Peter,
Proton decay experiments have falsified SU(5) and SUSY-SU(5), I am unsure about SO(10), but if GUT as a class are all falsified, whether through proton decay experiments or lack of monopoles, would this falsify stringy unification scenarios, or for that matter, the need for SUSY in GUT unification?

Thanks
Dan

4. **woit**
April 17, 2007

dan,

I don't think you can falsify GUTs as a class, since you can always find models with longer proton lifetimes than can be measured, in particular this is true for some SUSY-GUTs. In stringy unification scenarios you can get any proton lifetime you want, so, again, you can't falsify these.

What you can falsify is the idea that we're at some generic, anthropically allowed point in the string theory landscape, and this has already been falsified by not seeing proton decay.

5. **Dan**
April 17, 2007

Hi,
thanks for answering my question. I infer though that all stringy unification scenarios are extensions of GUT scenarios, which may or may not be physical (and the simplest sort, SU (5) and SUSY (5) has been ruled out).

6. **anon.**
April 18, 2007

Dan,

Because SU(5) has been ruled out, it is at least wrong, which is a whole class better than the not even wrong status of string based unification, where there's a landscape of theories too big to ever rule out. However, the way that some unification schemes can be fiddled to keep falsification at the door, is familiar:

'Many physicists are working very hard trying to put together a grand picture that unifies everything into one super-duper model. …

'Somebody makes up a theory: The proton is unstable. They make a calculation and find that there would be no protons in the universe anymore! So they fiddle around with the numbers, putting a higher mass into the new particle, and after much effort they predict that the proton will decay at a rate slightly faster than the last measured rate the proton has been shown to decay at.

'When a new experiment comes along and measures the proton more carefully, the theories adjust themselves to squeeze out from the pressure. … The phoenix just rose again with a new modification of the theory that requires even more accurate experiments to check it.'

– R. P. Feynman, *QED*, London, 1990, p150.

This is the interim step between definitely falsifiable theories, and definitely unfalsifiable stringy M-theory. It must be a sociological issue. Mainstream physicists didn't want to risk to their careers of being experimentally refuted after working on and popularising a falsifiable theory, so in the 1980s they started getting excited about theories that were increasingly 'safe' from experimental tests.

The usual claim that extradimensional string theory started out with the hope of being checkable but gradually the initial optimism has proved exaggerated, is maybe bit too kind to the string theorists. It didn't have any experimental evidence at the beginning, just a lot of hype and 'hopeful possibilities' (which isn't worth a dime in science, where facts alone count and hope is just faithful religion).

7. **John A**
April 18, 2007

Peter,

what do you think of [this]?

> Although the theory is not definitive, Sparling explains that several major ideas in current physics would likely play a role (such as condensed matter physics, category theory, non-commutative geometry, string theory, and the structure of superfluids). Such connections might also point the direction to a unified theory, though currently speculative.
>
> "My work can be seen as a strong antidote to the present air of pessimism surrounding modern fundamental physics," Sparling said. "As is well-known, string theory has been roundly criticized for its lack of predictive power. String theorists have been reduced to an absurd reliance on the anthropic principle, for example. Here I have a clear-cut prediction, which goes against the common wisdom, which gives experimenters a target to go for: first find the extra dimensions, then decide their signature (a very tough homework assignment!). Of course I could be proved wrong, but the effort to decide is surely worthwhile.
>
> "Actually, in the area of philosophy, I am in opposition to string theory," he said. "It is a top down theory: dream up something that works in some high dimension and then try to finagle some way of reducing to fit in with the lower-dimensional theory. My approach is bottom up: take the existing four-dimensional theory seriously and try to build up from it. This is very tough to do. Hopefully my ideas work. Note that my work only constitutes a possible beginning at a more inclusive theory."

8. **woit**
April 18, 2007

John,

I'd really like to avoid turning this into a forum to discuss very speculative ideas like Sparling's, this is something I don't have the time or energy to moderate and would quickly get out of control. A couple quick comments though: in general I think the idea of using twistor geometry is promising, it is a very fundamental idea about 4-dimensional geometry. I haven't looked yet to see exactly what Sparling is doing, but all attempts like this I've seen in the past don't reproduce the standard model QFT set-up. My feeling is that you need not just the typical kind of kinematical idea about finite-dim symmetries that people are trying, but some new insight into the structure of gauge theories that would make non-trivial use of spinor geometry.

9. **Christine**
April 18, 2007

It must be a sociological issue. Mainstream physicists didn't want to risk to their careers of being experimentally refuted after working on and popularising a falsifiable theory, so in the 1980s they started getting excited about theories that were increasingly 'safe' from experimental tests.

I often find such statements disturbing. In the same line, it has been argued elsewhere that string theorists have invested their whole careers in string theory and with the lack of experimental confirmation (or lack of predictability, for what is worth) they are making use of desperate arguments like the anthropic interpretation for the landscape, as a new shift in paradigm of the scientific method, etc.

Well, what sounds odd to me is that there is a basic principle that *any* scientist learns very early: failure is much more common than success. Or, better yet, as Thomas Edison have (presumably) said:

> I have not failed. I've just found 10,000 ways that won't work.

Whether string theory will turn out to be right or not, is not the problem here, but simply the fact that *one should not make claims in advance before effectively succeeding*. So, if some string theorists are doing such a thing, yes, they would be wrong in a very basic sense.

On the other hand, independently of making anticipated claims or not, if things look bad enough so that there is a possibility that string theory will not be successful, that would not be the case for desperation.

What, if a scientist fails? How much is that bad? It's part of the game!

Winners x losers is a strong paradigm of the american society, and I often find it funny from my latin american perspective. Yes, it can lead to useful achievements in some aspects, but not always. But it makes no sense to see scientists as winners or losers. Great scientists had their moments of discovery and failure.

So I can only conclude that the problem stands on the following general *basic* issues:

1. You should never make anticipated promises in science. Things might work, or might not, and the latter is much more common than the former.

2. In science, there are no winners or losers, but the advancement comes, in general, from brilliant people, clever experiments, incremental work, some luck and opportunity windows, failure (yes!), and, finally, (using another quote from Edison):

> one percent inspiration, ninety-nine percent perspiration.

The bottom line is a very obvious thing: any good scientist does not fear failure, does not make anticipated claims, and are not winners or losers. They're are just… scientists.

I do not have doubts that there are many good scientists working in string theory. So I can only agree that the sociological issue has a share of importance in the whole issue, which, for what is worth, will be very interesting from the point of view of the history of science.

Christine

10. **Peter Woit**
April 18, 2007

Christine,

I agree that most scientific research is speculative and ultimately won't work out. So people should be expecting to spend most of their time on ideas that will fail, or lead to very modest advances, and there's nothing wrong with this. The problems people are addressing now in particle physics are extremely difficult, and one should expect essentially everything one tries to not work. What bothers me is that I don't see people trying a lot of different things, and giving up on ones that don't work. Instead, effort continues to pour into a narrow range of ideas, especially string theory, even when a huge amount of evidence has accumulated that the idea of getting a unified theory out of a 10 or 11 dimensional string theory is inherently unpredictive and can't work.

This seem to me to involve two big questions which I don't think are being addressed, a positive one and a negative one:

1. How do you encourage people to try something new, different than what other people are doing, given that it is most likely to fail? Can one provide incentives so that young people do not feel that they are probably committing professional suicide when they try and do something new and ambitious?

2. How do you get people to acknowledge that an idea that they have a huge amount invested in doesn't work, once it becomes clear that this is the case? In the past, experimental results provided discipline of this sort, now we see a whole field devoted to ideas that inherently cannot be confronted with experiment, and thus are immune to this kind of discipline.

11. **anon.**
April 18, 2007

'How do you get people to acknowledge that an idea that they have a huge amount invested in doesn't work, once it becomes clear that this is the case?' – Peter Woit

You can't do that. If people are irrational enough to believe in an elaborate guess which just interconnects assorted speculative unobservables (gravitons, supersymmetry, standard model unification at near Planck scale energy), without any connection to observables, and without any hope of making falsifiable predictions from the landscape, it won't be possible to get them to give up!

It's like trying to debunk religion or UFOs, it can't be done because there's no evidence to be discussed, there's nothing physical to be scientifically examined. In a free country, people are entitled to believe in failed theories, crazy ideas, and whatever they want. The basis of the problem is groupthink, is the illusion that because it has a lot more people than alternatives, you get the the 'so many people can't all be wrong' dogma being used to defend the mainstream, despite the lack of science. The only way they could be defeated is by losing research grants, with the media ignoring their arrogant, unsubstantiated claims.

12. **Christine**
April 18, 2007

Dear Peter Woit,

Yes, the two points you make are important.

1. The system must definitely be reformulated. Negative results are results. Lessons learned are results. New ideas should be incentivized up to the point that they can be scrutinized by the scientific method. Number of papers cannot be taken as the absolute measure of a scientist's achievement.

2. Such an acknowledgement must arise from two fronts: an internal one, in which a good scientist will eventually reach at, and from an external consensus, which depends on how the system is structured (item one above).

However, these are simple views that I have, from a simpler world. I understand the issue is in reality much more complex than the solutions above suggest.

Christine

13. **andy**
April 18, 2007

Christine: you said *Number of papers cannot be taken as the absolute measure of a scientist's achievement*.

A noble aspiration. Regrettably, the real world does not work that way.

Sorry if I come across like a cynic. But I'm old enough to be allowed to be one.

But I respect your idealism.

14. **Tim**
April 19, 2007

Peter,

You wrote:

"One of many problems with the anthropic landscape is that conservation of baryon number is rather special, typical vacua will have baryon number violating interactions. The bounds on the proton lifetime are something like 10^32 years, whereas, anthropically, all that is needed is for only a small fraction of protons to have decayed since the big bang (10^10 years ago). So, unless you can find a landscape argument for why the GUT scale has to be so high (and I don't know of any) protons should decay much, much faster than they do in the real world. The landscape folks seem to take the attitude that they can claim "testable predictions", while ignoring the fact that similar or even better grounded "testable predictions" of the scenario are already falsified."

Now I'm at a complete loss regarding your point of view. You published a book as well as hundreds of blog postings about string theory and landscapeology being 'Not Even Wrong' because no testable prediction emerges from these ideas. At the same time you acknowledge that there is an experimentally testable prediction from the landscape (right or wrong, doesn't matter at this moment).

You changed your mind? Or what's going on? Why not change the title of the blog to 'Wrong' from 'Not Even Wrong' if your motivation for the title is the lack of an experimentally testable prediction? Will you release an errata to your book?

Please explain what you think about these matters because until now I went into great pain of understaning what you really mean and just when I thought I get your point (with or without agreeing with it, that doesn't matter at this moment) I'm just completely lost again as to what the heck your message is.

Best,
Tim

15. **Christine**
April 19, 2007

But I respect your idealism.

Thanks. And I am sure I'm one of the last idealists living today. 🙂 Enjoy yourself!

In any case, it is really difficult to have an objective measure of a "good scientist" or the potential of a young researcher, something that you can only (in general) have some judgement in a posteriori cases or when you work closely with that person. So it's more a question of acknowledging these fragile points and to decide whether one is willing to take some risks and incentivize diversity or other means as a possible paradigm for a healthy scientific activity. This is something I tend to agree with Smolin.

One other thing is the question of being very precise in defining the status of one's subject of investigation. One should be extremely careful in making use of some established convention that specifies what a theory (with "T" or "t", whatever) is, what a hypothesis is, what a set of conjectures is, what a model is, what an idea is, etc. I believe these terms are at some level reasonably distinguishable, but sometimes are used indiscriminately under different meanings in papers, talks, etc. In this respect, I think that the high energy community should attempt to reach a consensus on the status of string theory. Somewhat in analogy, astronomers met and changed the status of Pluto as no longer a planet as it was realized for some time that it did not fit the convention of what a planet is.

Christine

16. **woit**
April 19, 2007

Tim,

The point is the following: the landscape allows virtually anything, so one can't use it to make predictions. The anthropic landscape idea is something more specific, it's that one can make statistical predictions based on the idea that we must be at a "typical" point of the anthropically allowed region of the landscape. The proton decay argument shows that we're not, and this is wrong. People pushing this I suppose can argue that they still haven't worked out exactly what all the states in the Landscape are, and until this is done, one can't be sure of what will happen. But all the evidence is that there's no reason for protons to be so stable.

So, oversimplified: landscape not even wrong, anthropic landscape wrong.

The situation is more complicated than just the true statement that "string theory makes no falsifiable statements". "string theory" is not a well-defined single thing. Many versions of string theory do make falsifiable predictions that are wrong, generically string theories predict several completely wrong things. In order to evade these predictions, people have come up with these very complicated constructions designed to evade falsification.

17. **Will**
April 19, 2007

Regarding Kachru's demotion of the Anthropic Principle to "if You Wish", this may be a nod to the cyclic model of Steinhardt and Turok, which of course uses string- and brane-theoretic ideas but is emphatically anti-anthropic (as is clear from their paper "The Cyclic Model Simplified").

18. **gina**
April 19, 2007

Peter: "1. How do you encourage people to try something new, different than what other people are doing, given that it is most likely to fail? Can one provide incentives so that young people do not feel that they are probably committing professional suicide when they try and do something new and ambitious?"

Well, my opinion is that you should NOT encourage people to do
things that are most likely doomed to fail with little chance for something to show for their efforts. If they do take high risk projects, there is no way to avoid the high chance of a serious damage to their career, the way they are judged by others, and most importantly their self-judgement. There are no riskless risks.

There is one thing you can do. It is to have a culture were high risk ideas are examined, and read, and people think about them and comment on them, and mainly criticize them. In this aspect, when it comes to the string theory debate the people on the string theory side are overall much better.

19. **Thomas Love**
April 19, 2007

Christine said:

"I think that the high energy community should attempt to reach a consensus on the status of string theory. "

I don't believe that's possible right now. Physicists don't like to abandon a theory until they have a new theory to work on (one must publish, even if it is junk) and it is unlikely that a new theory would be accepted quickly. There are (at least 10 fundamental issues on which quantum theory and general relativity are incompatible, even if our choices are choosing from column q or from column r, there are 2^10 =1024 possible choices (it is possible that neither is correct).

20. **E.**
April 19, 2007

Peter,
You like to claim that string theory 'doesn't work'. Do you have proof of this statement beyond arguments against the landscape? If the difficulty of connecting with experiment is your sole criteria, then I would counter that this is not a problem with string theory, but a problem of a lack of experimental data at high energies. ANY theory of quantum gravity will have the same problems. Hopefully, within the next year this situation will begin to change as LHC begins to produce data. The smart money is on TeV scale supersymmetry which, while not proving string theory, is strong evidence in it's favor. Precision experiments that follow should allow us to determine the exact mechanism by which SUSY is broken, information which will help the string theorist. You should keep in mind that what we call string theories are really just perturbative limits of M-theory, which is largely unknown. We need experimental data to get beyond perturbation theory.
PS: I look forward to seeing you eat crow within the next two years.

21. **Christine**
April 19, 2007

Thomas Love wrote:

Physicists don't like to abandon a theory until they have a new theory to work on

That is certainly *not* what I have suggested. Please read my comment again.

Christine

22. **Christine**
April 19, 2007

E. wrote:

If the difficulty of connecting with experiment is your sole criteria (…)

Hm. I think it's more than in time for Peter Woit to write a FAQ about his main arguments.

Christine

23. **E.**
April 19, 2007

Christine,
Difficulty connecting with experiment is the ONLY possible argument against string theory. String theory is the ONLY framework which allows one to combine quantum mechanics, gauge theory, and gravity in a consistent way. If Peter has the ability to refute this statement, then I'd like to see a paper by him on the arXiv which is also submitted to a refereed journal. He can pontificate all he wants, but this is not scientific.

24. **anon.**
April 19, 2007

E., you're missing the point even if all alternatives like LQG with its prion particle model really are dismissable; *the connection to experiment is the ONLY possible criterion of science*. String theory connects only to speculations; alternatives have less speculation in them, and fewer degrees of freedom. They're more factual, which is science. Eg, see Feynman video explaining

"It doesn't make any difference how beautiful your guess is…"

– http://www.youtube.com/watch?v=ozF5Cwbt6RY

25. **E.**
April 19, 2007

Anon,

Again, the problem is not with string theory, but the fact that the energy scales involved are so large that experiments are difficult if not impossible. String theory can make contact with experiment, however it is difficult at this stage to make predictions since we are working with approximations to M-theory. At present, we can only show that it's POSSIBLE for string theory to describe our world, and then try to work backwards. Yours and Peter's arguments would essentially argue against the entire enterprise of quantum gravity, not just string theory, on the basis that it's difficult to make experimental predictions. Any other theory you want to bring up will have the same problem. However, there is only one theory which is mathematically consistent with the potential to do the job, and that is string theory.

26. **Christine**
April 19, 2007

Gina,

Woit wrote:

> people should be expecting to spend most of their time on ideas that will fail, or lead to very modest advances, and there's nothing wrong with this. The problems people are addressing now in particle physics are extremely difficult, and one should expect essentially everything one tries to not work. What bothers me is that I don't see people trying a lot of different things, and giving up on ones that don't work.

So it is not that he is "encouraging people to do things that are most likely doomed to fail with little chance for something to show for their efforts". It has to do with the nature of the frontier of knowledge that high energy physics represents today in the search for a quantum gravity theory or the unification of fundamental interactions. It has to do with acknowledging the difficulties involved. So it is to encourage people to work in a difficult subject, yes, but with the expectation that ideas might turn out (more frequently than desired) to be wrong, so one has to be extremely cautious and willing to accept failure as part of such an endeavor. Such eventualities should not be seen as a failure in their scientific careers, but part of it. But yes, the system as structured today is not ready to identify such eventualities as part of the effort, nor is open to encourage new ideas. In consequence (at least, in part), the system ends up to be also insensitive to some undesired distortions.

Christine

27. **anon.**
April 19, 2007

"Again, the problem is not with string theory, but the fact that the energy scales involved are so large that experiments are difficult if not impossible. …" – E.

That is a pathetic defense and it *is* a fault of string theory. A good theory should not build upon unobservables that can't be checked. This is the whole trouble: string provides solutions for non-existent, imaginary problems on scales that can never be onserved, and in the process requires more uncheckable speculation.

28. **Christine**
April 19, 2007

E. wrote:

I'd like to see a paper by him on the arXiv which is also submitted to a refereed journal.

You are not alone in such a desire (but it should not necessarily be a paper by Peter Woit). For a long time I am willing to see a detailed technical argumentation in a refereed journal on the status of string theory, critically assessed. I would expect to see such a paper written even by a string theorist. In any scientific field, it is natural for the researcher to assess the pros and cons, the open problems and discoveries, of their current investigations.

Christine

29. **Thomas Love**
April 19, 2007

E. Says:

"String theory is the ONLY framework which allows one to combine quantum mechanics, gauge theory, and gravity in a consistent way. "

Can you prove that?

30. **E.**
April 19, 2007

Anon,
It isn't string theory's fault that nature has placed the unification scale at 10^{16} GeV. Perhaps those ancient Greeks theorizing about the existence of atoms were pathetic as well, since they couldn't possibly perform experiments to test this idea.

31. **E.**
April 19, 2007

Dear Thomas Love,
This statement was proved in 1984 by Green and Schwarz.

32. **Peter Woit**
April 19, 2007

I think I've made the arguments I want to make about what the problems with string theory are clearly and in detail in many places. For one version I recommend
http://www.math.columbia.edu/~woit/testable.pdf

Maybe I should write up this sort of thing as a formal paper to put on the arXiv, but I'd rather spend my time on other things. I simply don't believe that doing so would have the slightest effect on the views of string partisans like E., who have a long list of unshakable ideological beliefs that just don't correspond to reality (like the belief that the 1984 Green-Schwarz anomaly cancellation proves that "String theory is the ONLY framework which allows one to combine quantum mechanics, gauge theory, and gravity in a consistent way. ") For the less partisan, there are now plenty of places people can read what I and other string theory critics have to say, see how string theorists respond, and make up their own minds. A few years ago only one side of this argument was readily publicly available, that has changed a lot.

Some string theorists do still seem to believe that the LHC will see supersymmetry, with a pattern of supersymmetry breaking that will correspond to a specific class of string backgrounds, that will then used to make real predictions. From talking to many string theorists, my impression is that the "smart money" no longer has much faith this is going to happen and would not be willing to bet on it. What I'm seeing are people well aware that this hope is looking more and more unlikely to work out, so they are hedging their bets, to make sure that they won't be eating crow a few years from now.

33. **Christine**
April 19, 2007

E. wrote:

This statement was proved in 1984 by Green and Schwarz.

About this paper, here is an interesting comment from one of the authors:

[This week's citation classic](): the Consistency of Superstring Theory.

Green writes:

> A fundamental problem of all present formulations of string theory is that they are based on an approximation scheme in which the geometry of the space and time through which the string is moving is treated nonquantum-mechanically and is an input to the theory.

34. **anon.**
April 19, 2007

"It isn't string theory's fault that nature has placed the unification scale at 10^{16} GeV. Perhaps those ancient Greeks theorizing about the existence of atoms were pathetic as well, since they couldn't possibly perform experiments to test this idea." – E.

1. You don't know for sure that unification occurs at 10^{16} GeV.

2. 'Atom' is Greek for not splittable. All the Greek ideas about the atom turned out completely wrong.

35. **island**
April 19, 2007

The message that I continually get is that LGQ has failed and String Theory is pursued as the only avenue left, while Peter Woit points out that this isn't even science, yet nobody sees this as the first sign that quantum gravity and the standard model are in deep trouble, rather, they point back to the other failed solution as the better solution because it's the only "other" game in town that doesn't include *as many* unproven or semi-established assumptions.

People keep crying-out for a new Einstein, and how we need "seers", but their insights must be conditionally attatched to one of the only two games in town, or the consensus will kill Albert with its momentum to hell.

36. **Chris W.**
April 19, 2007

PS: (on anon's comment) Greek atomism was, from a modern perspective, a largely *metaphysical* hypothesis, notwithstanding some remarkably insightful comments on familiar observations by the Greeks, made in light of atomism. In the hands of Newton and his successors this metaphysical hypothesis was ultimately extraordinary fruitful, insofar as it inspired powerful ideas that were truly testable against observations.

I'll take E.'s comment as an admission that the founding assumptions of string theory are essentially metaphysical. That's fine; metaphysical ideas have had an important place in the development of scientific theories, as have criticisms of undisciplined metaphysical speculation by Mach and others. (Einstein arguably appreciated this creative tension better than anyone else ever has.)

However, by now it has become apparent that the putatively scientific spawn of string metaphysics is an interpretive mess. The appeal to observations that might be made with the LHC is cold comfort, because the interpretation of those observations will be as much a mess as the theoretical ideas employed in the interpretation. A whole generation of researchers have been encouraged to accept this state of affairs as normal, and even as defining a new way to do science. It's not; instead, it is a sign of a profound crisis, the response to which has been a largely myopic exercise in technically clever model-building that leads nowhere.

Nobody will be eating crow in the next two years. The string theory community should be eating crow now, but collectively they show no inclination to do so. Instead, I expect we'll see a slow dimunition of interest, and an increasingly restless search for alternative ideas. Edward Witten already seems to have moved on (to Langlands and other purely mathematical topics).

37. **Thomas Love**
April 19, 2007

anon. Says:

" 'Atom' is Greek for not splittable. All the Greek ideas about the atom turned out completely wrong."

No, the English borrowing of the word to describe the items in the periodic table was wrong. The atomoi of the greek philosophers are the proton, the electron, the neutrino and their antiparticles.

38. **a**
April 19, 2007

Peter, I don's see why the proton longevity falsifies anthropic arguments. If the weak scale is small for anthropic reasons, and if the Standard Model (or something like the SM) is the full physics below the Planck scale, then effective theory arguments imply that the proton decay rate is suppressed by 4 powers of the Planck scale i.e. that the proton has a uselessy long lifetime.

Certainly, anthropic arguments would have been much more convincing if the proton lifetime were just anthropically long.

39. **Peter Woit**
April 19, 2007

a,

Sure, but according to the statistical landscape philosophy, I see no reason why the SM (or something like it) should be all there is below the Planck scale. That's a very special theory: generically string vacua have larger gauge groups, and all sorts of Beyond SM physics. If you claim we are living in some generic, statistically likely such vacuum, it will have much faster proton decay, and anthropic considerations won't save you.

I think to avoid this argument, you have to come up with some reason that vacua like the SM are statistically favored over others. All that I've seen of actual attempts to look at large numbers of vacua don't show anything like this at all.

40. **E.**
April 19, 2007

Peter,
Are you against string theory or just the landscape? I can assure you that many string theorist do not particularly care for the landscape or anthropic reasoning either.

41. **A.J.**
April 19, 2007

E.

That paper doesn't even claim to prove the results you're ascribing to it. It argues that the SO(32) and E8 x E8 string theories _might_ be consistent, not that the string theories are the only possible such theories. You're really not helping anyone by making such wild claims.

42. **E.**
April 19, 2007

Dear A.J.,
Green and Schwarz proved that the string theories were quantum mechanically consistent and necessary included gravity, for two choices of gauge group, SO(32) and E8 x E8, as you say. I would agree that if someone came up with an alternative theory that had these properties, it would be important and people would pay attention to it, but certainly this is not the case to date. LQG is interesting, but I don't think it's possible to construct gauge theories within its framework.

43. **Peter Woit**
April 19, 2007

E.

It's not a question of whether string theorists "like" the landscape or not. Either pertubative string theory on these sorts of backgrounds makes sense, and you have a theory that is unpredictive, or else it doesn't, in which case you don't have a theory. Either way, the idea has failed.

44. **A.J.**
April 19, 2007

Dear E.,

I'm not very impressed by the argument that string theory must be the only consistent quantum gravity theory because it's the only theory we've found in the last 30 years which might be consistent. Quantum gravity is probably the hardest problem theoretical physicists have ever looked at, so we should probably let a few more centuries pass before putting much faith in arguments of this form.

At the very least, I want to see a compelling argument that Connes' non-commutative geometry theories aren't low energy approximations to a consistent theory of quantum gravity.

45. **E.**
April 19, 2007

Dear Peter,
I disagree with you that the idea is failed. We simply do not have the complete theory in our hands, only pieces. At present, it really is the only theory that has the potential to explain everything. That isn't to say that people shouldn't work on alternatives, as Schwarz and others did in the 1970's when string theory was decidely out of favor. Perhaps one of those alternatives will be shown to have the same special properties of string theory, but until that day string theory will hold the most interest. It's much more likely that additional progress will be made in uncovering nonperturbative aspects of string theory that will lead to unique predictions.

46. **E.**
April 19, 2007

Dear A.J.,
If there is another alternative, that would be great. I don't argue against other approaches, I only point out that at present string theory is the only contender.

47. **A.J.**
April 19, 2007

Dear E.,

That's the trouble with words, I guess. When you wrote

> String theory is the ONLY framework which allows one to combine quantum mechanics, gauge theory, and gravity in a consistent way.

many of us didn't realize that you were only referring to frameworks that we know of. Anyways, if what you're saying isn't really different from the standard explanation of string theory's current state, then we don't need to continue this conversation.

Best,

48. **Tim**
April 19, 2007

Peter,

You seem to be really losing it. You can't claim that string/M-theory is 'Not Even Wrong' and at the same time claim that string/M-theory is 'Wrong'.

Several people already have tried to point out to you what the problems are with your line of reasoning, but you failed to listen. I tried as well, but you failed to pay attention. In addition you think that you have something better to do than writing a technical research paper which clearly demonstrates that you have no intention of entering into real technical (or professional) discussions.

You are really like a classic internet troll. Everyone tries to explain to him that what he does is wrong, he doesn't listen, serious people give up explaining, others, mostly who are new to the internal keep explaining, they give up, yet new people come to whom the phenomenon is new, try to explain, doesn't work, give up, etc, etc, and the troll remains a troll and always finds innocents who try to talk to him without any progress in anything. No real audience, no real content.

Best,
Tim

49. **Peter Woit**
April 19, 2007

E,

"additional progress will be made in uncovering nonperturbative aspects of string theory that will lead to unique predictions."

People have been repeating this line for more than twenty years now, while everything that has been learned about non-perturbative string theory (branes, M-theory, AdS/CFT…) has led in exactly the opposite direction, to a wider variety of phenomena that don't look at all like they give the SM in any sort of unique way. As far as I can tell, string theorists have no reason at all for the belief that this is going to change except for pure wishful thinking.

50. **Peter Woit**
April 19, 2007

Tim,

As far as I can tell you're not even bothering to read what I wrote in response to your comment about "Wrong" vs. "Not Even Wrong", preferring to launch into a personal attack on me, all from the cover of anonymity. And I'm an "internet troll"….?

51. **Coin**
April 19, 2007

> " 'Atom' is Greek for not splittable. All the Greek ideas about the atom turned out completely wrong."
>
> No, the English borrowing of the word to describe the items in the periodic table was wrong. The atomoi of the greek philosophers are the proton, the electron, the neutrino and their antiparticles.

Hm. Wikipedia:

> In about 485 BC, the Greek philosopher Parmenides stated the ontological argument against nothingness, essentially denying the possible existence of a void. In c.460 BC, Greek philosopher Leucippus, in opposition to Parmenides' denial of the void, proposed the atomic theory, which reasoned that everything in the universe is either atoms or voids; a theory which, according to Aristotle, was stimulated into conception so to purposely contradict Parmenides' argument. **In the years to follow, specifically in about 450 BC, Leucippus' pupil Democritus went on to further develop the atomic hypothesis using the term atomos, which means "uncuttable".**
> …
> **The earliest views on the shapes and connectivity of atoms was that proposed by Leucippus, Democritus, and Epicurus who reasoned that the solidness of the material corresponded to the shape of the atoms involved. Thus, iron atoms are solid and strong with hooks that lock them into a solid; water atoms are smooth and slippery; salt atoms, because of their taste, are sharp and pointed; and air atoms are light and whirling, pervading all other materials.[2] It was Democritus that was the main proponent of this view. Using analogies from our sense experiences, he gave a picture or an image of an atom in which atoms were distinguished from each other by their shape, their size, and the arrangement of their parts. Moreover, connections were explained by material links in which single atoms were supplied with attachments: some with hooks and eyes others with balls and sockets.[3]**

I personally didn't know most of the things in that article so I'm finding it kind of interesting; either way, though, whether we view the greek atoms as atoms or some other class of even more fundamental particle, it doesn't appear that any versions of the atomic hypothesis that particularly resemble reality were proposed until Newton and Boyle. (Although the "hooks and barbs" idea, at least as Descartes formulated it after resurrecting the idea, seems to have a certain conceptual resemblance to valence electrons and shells).

So, E brought up the greek atomic theory as an analogy to String Theory, to observe that the greeks correctly postulated the atomic principle long before the ability to directly observe atoms was close to being available. Were I to try to make an analogy to String Theory here, it would be more to observe that even if the basic string hypothesis were someday found to be accurate, it could well be that *every single other idea* that today comprises String Theory is utterly wrong, just as the atomic hypothesis is right but the sensation of cold is not, as Democritus' version of Atom Theory would lead us to conclude, caused by atoms with pointy ends.

Another possible analogy to string theory suggests itself if you ask the question of *why* the greeks were able to correctly hit on the idea of fundamental atoms/particles, but then get all of the details wrong. One possible answer to that question is that the greeks were, as far as I can tell, working off of purely philosophical methods, with no particular effort being made to follow the modern strategy of developing testable or falsifiable hypotheses. Without at least some effort or ability to check their progress against reality, they were unable to see when at some point or other they started off on a wrong track in a minor way (for example, assuming atoms took simple geometric shapes), and after following that wrong track to its logical conclusion wound up with a theory of matter which was "elegant" and self-consistent but unfortunately, with the technology of a couple thousand years later, is observably obvious as totally wrong. There is probably a similar trap waiting in the modern era for anyone who tries to do too much by purely mathematical methods and does not at some point start seriously asking the question of how to verify against experiment…

It would be interesting to check a more in-depth history of the atomic hypothesis to see exactly at what point the idea of experimental science first became explicitly involved in the formulation of atomic theory, and see whether this had an immediate impact on the ability of the theorizers to come close to the way atoms or fundamental particles actually work.

52. **E.**
April 19, 2007

Coin,
I should point out that many scientists did not believe in the existence of atoms as late as the early 20th century. However, there were others, such as Boltzman, who used the concept of atoms to construct mathematical theories, e.g. statistical mechanics.

53. **E.**

April 19, 2007

Dear Peter,
I would disagree with you that the progress in understanding nonperturbative aspects of string theory has made the situation worse. The 'landscape' has always been with us, despite Susskind. String theory is a deep theory with a rich structure. It may take a much longer time before we really understand it. Thus, I would say your criticisms are premature.

54. **Coin**
April 19, 2007

E: Right, but Boltzmann's ideas– even if they were poorly accepted in their time– were not dependent on pure thought, philosophy or mathematics, but were fundamentally testable and falsifiable. Boltzmann's ideas about statistical mechanics were mathematical, but the math was such that it could predict things which could then be tested, even with the limited technology of his day. There are things about the behavior of gases which Boltzmann's mathematical theories based on the atomic model uniquely explain as an unambiguous consequence, and which mathematical theories based on other models of matter did not explain at all. In terms of *process* this is all quite different from or pre-Renniasance atomic theory or even modern String Theory.

55. **Peter Woit**
April 19, 2007

E.

The "landscape" has not always been with us. What was with us before the landscape was the moduli stabilization problem, and the hope that non-perturbative effects would stabilize moduli and lead to only a small number of consistent vacua. Instead they led to the landscape in its incarnation as an exponentially large number of stabilized vacua.

56. **E.**
April 19, 2007

Peter,
As I said, the problem of 10^{600} vacua has been with string theory from the beginning. The additional stabilized vacua via fluxes is just the same problem recast in a different form. In all probability, these vacua will turn out to be an artifact of our current perturbative formulation of string/M theory.

57. **E.**
April 19, 2007

Coin,
Perhaps a better analogy would be to DNA. From a single object, a wide diversity of living creatures are possible, a landscape if you will.

58. **Coin**
April 19, 2007

An analogy from what to what? That just seems unrelated to anything in this thread so far.

Anyway, a random question on a totally different subject:

Peter Woit wrote:

> Some string theorists do still seem to believe that the LHC will see supersymmetry, with a pattern of supersymmetry breaking that will correspond to a specific class of string backgrounds, that will then used to make real predictions. From talking to many string theorists, my impression is that the "smart money" no longer has much faith this is going to happen and would not be willing to bet on it.

What has to happen before the supersymmetry hypothesis can cease to be taken seriously by the scientific community entirely? My understanding is that supersymmetry is like proton decay– it can always avoid falsification by just tuning up the numbers. If an experiment fails to demonstrate proton decay, we can still say proton decay occurs but at a half-life so high the experiment wouldn't have been able to detect it; if an experiment fails to demonstrate the existence of supersymmetric partners we can still say the partners exist but at a mass so high the experiment wouldn't have been able to detect it.

However, there was a point where the lower bound on proton decay eventually got *so* high that people just stopped taking it seriously; I understand when we got to the current result of proton decay having minimum half-life 10^35 years, that was considered the same as demonstrating proton decay not to occur.

Does this same thing eventually happen to supersymmetry? While it would be great if they were found, if supersymmetric partners are *not* found by the LHC, will we start to assume they do not exist? What about the VLHC? What energy scale do we have to reach before physicists can no longer propose supersymmetric partners can still be found while keeping a straight face?

59. **woit**
April 19, 2007

Coin,

I don't think proton decay bounds are as good as you say, more like 10^33 at best. As far as I know, typical SUSY GUTs are not ruled out at this level. Typical non-SUSY GUTs are, and this caused people to lose interest in them. If you use the constraint of unifying couplings, that fixes the GUT scale, you can't just move it up.

For supersymmetry, you can move up the breaking scale, but at some point you lose the ability to claim that SUSY stabilizes the electroweak breaking scale/Higgs mass. This is actually already in trouble, the current bounds mean a significant amount of fine-tuning is required. I think if the LHC doesn't see supersymmetry, it will be hard to take seriously the idea of supersymmetry stabilizing the Higgs mass, but people may still keep claiming that the world is supersymmetric, just at unreachably high energies.

If you look at Tevatron predictions, you see a lot of people claiming that they were pretty sure the Tevatron would see SUSY. This failure didn't slow them down much, but an LHC failure might.

60. **E.**
April 19, 2007

Coin,
SUSY SU(5) has essentially been ruled out by the proton lifetime, however other SUSY GUT's such as flipped SU(5), SO(10) and Pati-Salam are still viable. As Peter

correctly points out, if SUSY is to solve the gauge hierarchy problem, then we expect TeV scale superpartner masses. If LHC doesn't see SUSY, then we're screwed. However, I don't think this is very likely.

My earlier comment was making an analogy of strings to DNA. DNA is a fundamental object, but can lead to many possible solutions. In this way, it is like the landscape. Even though we know the structure of the DNA molecule, we don't yet know how to predict exact features of the lifeform that would arise from this DNA. You could of course make a similiar analogy to atoms, which gives rise to a 'landscape' of molecules.

61. **a**
April 20, 2007

E., there is a little problem with your analogy: we can see cats and dogs and many animals and hydrogen and oxigen and many molecules, but I have never seen another universe. Can you post a picture of it?

62. **Lee Smolin**
April 20, 2007

Dear E,

You say, "LQG is interesting, but I don't think it's possible to construct gauge theories within its framework." In fact, there are a number of papers showing how to incorporate gauge theories coupled to gravity in LQG, see for one example in spin foam models, 0207041. For the hamiltonian framework there are earlier papers, for example Thiemann's papers which described coupling to gauge fields, fermions and scalars-and even earlier. There is also a literature about using LQG methods to study quantum Yang-Milsl theories, indeed the loop representation was first introduced by Gambini and Trias in this context, for the most recent work in this area see the recent papers of Florian Conrady. And before you ask, yes there are also papers on incorporating supergravity, in both path interagral and hamiltonian frameworks.

Please, could you be so kind in the future as to check the literature or check with someone in the field before making false statements.

Thanks,

Lee

ps as to how long the landscape has been with us, the earliest reference I know of to the issue of a vast number of equally consistent vacua is Strominger's 1986 paper on Torsion in Superstring Theory. He makes a clear statement that there is a problem with predictability. But Peter is right that many experts claimed for years afterwards that moduli stabilization and susy breaking would lead to a unique and predictive ground state. Those few of us who argued otherwise were definitely in the minority.

63. **F.**
April 20, 2007

Thanks for the link to the Jacksonville meeting.
Kachru's talk is a nice review of some of the current
directions in string theory. The very informative
slides of the talk are here:
http://www.aps.org/meetings/multimedia/upload/kachru.pdf

Cheers, F.

64. **E.**
April 20, 2007

Lee,
Thanks for the information on LQG. Has anyone been able to construct Standard-like models within this framework and can it make predictions for particle masses, gauge couplings, etc.? If not, then I'd like to see Peter et. al. hold it to the same standard that they hold string theory. From what I know, the main argument for LQG is that it is background independent, whereas string theory is supposedly not.

65. **E.**
April 20, 2007

Lee,
Just a thought, but is it possible that LQG is equavalent to Matrix theory? The quantized area and volume of LQG seem to me to be very similar to D0 branes….

66. **urs**
April 20, 2007

> literature about using LQG methods to study quantum Yang-Milsl theories

Here is a question I have:

any gauge theory may certainly be described by conceiving their configurations, which are bundles with connection, in terms of the corresponding holonomies. Smooth holonomy maps are equivalent to smooth bundles with connection.

In many (most?, all?) discussions in the context of LQG, people enlarge the configuration space from smooth holonomy maps to "generalized connections", namely to holonomy maps which need neither be smooth, nor even continuous.

Doesn't this step loose a lot of structure? For instance the possible nontriviality of the gauge bundles is lost (as every bundle in Set trivializes, as opposed to bundles in Top or in C^infty).

Isn't much of the difficulty in LQG of making contact with what are called "semiclassical" structures ultimately due to this enlargement of configuration space?

Can one expect to capture Yang-Mills theory — even classically — when connections are replaced by "generalized connections"?

Finally: are you aware of the work by Freed-Moore-Segal? This is in spirit very close to the concerns of LQG, in that one studies configuration spaces of connections and their quantization, but stays within the context of smooth connections. (Difference is that they consider only abelian connections at the moment, but also for higher degree.)

They find interesting, and rather subtle, quantization effects. I doubt that any of these interesting structures would survive a passage from smooth to "generalized connections”.

Or is there an idea of how to recover information about smooth connections from information about “generalized connections”?

67. **Peter Woit**
April 20, 2007

Urs,

This kind of technical discussion of LQG, while interesting, is completely off topic and not something I know know anything about. Unfortunately I can’t let this blog turn into a general discussion forum for whatever people are interested in, because I don’t have the time or energy to moderate such a thing. There are a small number of people in the world who can give you an informed answer to your question, I suggest you contact them directly.

68. **E.**
April 20, 2007

Peter,
I would argue that string theory is also something that you’re not an expert on (how many papers have you written on the subject?), but that doesn’t stop you. LQG should be part of the discussion, since it’s one of the supposed alternatives to string theory. The question is, is LQQ also ‘not even wrong’ since it doesn’t make any predictions either?

69. **Thomas Larsson**
April 20, 2007

E,

What is worse, not making any predictions after 2,000 man-years or not making any predictions after 20,000 man-years? If you put the bar at 10,000 man-years, it seems like funding to string theory to be nullified at once, whereas LQG has another 8,000 man-years to go.

Not that I believe much in LQG, but that’s another matter.

70. **Peter Woit**
April 20, 2007

E.,

I have a Ph.D. in particle theory, and have spent a sizable fraction of my time during the last 23 years learning about string theory, following the subject closely, and discussing it with many experts. I’m quite comfortable that I know what I’m talking about when I criticize it. String theorists seem to often feel that the way to respond to my criticisms is, when their scientific arguments fall apart (e.g that Green-Schwarz proved that string theory is the only possible theory of quantum gravity), to try and attack my credentials and my right to speak on this topic. This tactic hasn’t worked for them so far; it’s kind of transparent that it’s what one does when one has lost a scientific argument.

I’ve spent a much, much smaller time learning about LQG, and, as you must know if you read this blog, my main interests are in particle theory and in mathematics, not in GR or in quantum gravity. I leave the arguments about whether LQG or string theory is a better theory of quantum gravity to others, and stick to what I know about, particle physics. As a purely sociological matter though, the main problem with string theory is the way it overwhelmingly dominates the formal end of particle theory research, despite its complete failure to tell us anything about particle theory beyond the standard model. By contrast, the amount of effort being devoted to LQG seems to me quite reasonable and healthy.

71. **E.**
April 20, 2007

Peter and Thomas,
Don’t get me wrong, I have absolutely nothing against LQG. I merely make the point that it suffers from the same issues that you constantly ascribe to string theory, namely predictability. As for time and money allocation, if more people thought LQG gravity had the same potential as string theory, I’m sure that it would get more attention.

Peter: A quick check on Spires shows that you haven’t been an active researcher in nearly twenty years. At any rate, if you have legitimate arguments against string theory, write a paper on it and submit it to the arXiv and a peer-reviewed journal. That’s the proper place to have this debate, not in the public arena, where it is essentially just grand standing.

72. **dan**
April 20, 2007

Peter wroite “it will be hard to take seriously the idea of supersymmetry stabilizing the Higgs mass, but people may still keep claiming that the world is supersymmetric, just at unreachably high energies.”

Would there be any credibility to this claim, that there is SUSY at unreachably high energies, should LHC not find SUSY? A major reason for accepting SUSY to begin with was stabilizing Higgs mass against radiative corrections. Another was that it would allow GR to be renormalizable.

73. **Cecil Kirksey**
April 20, 2007

E:

Peter can defend himself, but I am a lay person (retired engineer) and I have a valid legitimate argument against string theory: After 22-23 years ST has not made single quantitative connection with the real world, none, nada, zilch, nil. Got it! It is based on two premises that were and still have not been validity, i.e.., > 4 dimensions and supersymmetry. Why continue to pursue some theory that has no connection with the real world?? I cannot think of a single instance in physics research where this has occurred for 20 odd years can you??

74. **Walt**
April 20, 2007

Wow, E, you just totally lost this argument.

75. **Ptolemy**
April 21, 2007

E.,

String is the mainstream theory, hogging the limelight. There's no mention of LQG in the popular media. However, LQG certainly sticks closer to trying to model phenomena that can be observed. This should really be a paradox for all to worry about: why have people have lost faith in simplicity?

If Peter discusses LQG as well as string, that will be convenient to string theorists, who will be able to say he's just against everything. This is the opposite of the facts, it seems, since the problem is that there needs to be more alternative ideas, not consensus on unchecked speculation like string.

String is unique – and thus singled out for criticism – because it is entirely based on unobservables and speculation of the uncheckable variety. Excuse unification problems and quantum gravity problems by introducing unobserved extra dimensions. Excuse the huge landscape of models resulting therefrom by using the anthropic principle. Excuses for speculative failures.

76. **Arun**
April 21, 2007

I wish we knew who E was – what I mean is his intellectual history. I'd like to know where E got the notion that Green and Schwarz proved in 1984 that ""String theory is the ONLY framework which allows one to combine quantum mechanics, gauge theory, and gravity in a consistent way". Was it in a physics classroom? Seminar? Who was the speaker? etc.

———

On a different note, it seems to me that the Wilsonian teaching about renormalization was that a landscape of high energy theories would all lead to pretty much the same low energy effective theory; and now string theory is telling us that a landscape of low energy effective theories results from a single high energy theory. We cannot bootstrap ourselves from having to examine each energy scale experimentally; there is no point at which we know enough that physics can become axiomatic and deductive like mathematics.

——

77. **Peter Orland**
April 21, 2007

It is interesting that E. questions Peter W.'s competency by discussing his lack of publications in SPIRES. I wish people who disagree with Peter would stop using this tactic, which ultimately undermines whatever else they have to say.

So E:

1) Peter is a mathematician these days. So what if his papers aren't on SPIRES?

2) Can't someone who is not working in an area be capable of making an evaluation of that field? By necessity, we all have to do this, to decide what to work on, or simply to maintain perspective. We often look at ideas in high-energy physics and classify them as useful or as dead ends, even if we haven't actually written papers on these ideas.

78. **woit**
April 21, 2007

I'll let E's arguments speak for themselves, but I've often been hearing from string theorists his complaint that criticism of string theory outside a peer-reviewed scientific publication is somehow illegitimate.

I would have some sympathy for this were it not for the huge amount of pro-string hype produced for public consumption over the last twenty-some years: from magazine articles to books to TV and radio programs, etc., etc. Much of this material is highly misleading and gives a completely inaccurate picture of the state of string theory research, so much so that it leads to people like E. repeating exaggerated and bogus claims like the one about Greene-Schwarz. One reason for my book was to provide scientists and the public with the other side of this story. One can argue pro or con about whether conducting debate on this topic publicly is a good idea, but I don't think one can argue that the earlier status quo, only pro-string hype allowed in public, was a healthy or honest situation. I know of a few string theorists who were privately unhappy with the public hype, no examples of any string theorists who publicly expressed any displeasure, until public criticism of the theory started to get attention.

79. **James**
April 21, 2007

It's quite sad, that someone really spends time writing anti-string theory books and blogs, when he could be doing real research instead. So far, no one has a unified theory that works, but that does not mean one should not pursue the goal. But I admit: philosophising and nagging about problems is easier than doing the real work!

80. **Peter Orland**
April 21, 2007

Hey James!

Read what I just wrote about E. above.

81. **woit**
April 21, 2007

James,

I'm not in any way opposed to pursuing the goal of a unified theory, quite the opposite. I just happen to think that the current way string theory is being pursued and promoted is very much hurting the pursuit of that goal and someone should speak up about this. Preferably someone other than me, but that wasn't happening. Sure, I'd rather be spending my time on more positive pursuits, but I just don't see the problems with string theory being addressed. Instead I see a refusal to seriously address legitimate criticism, continued outrageous and misleading hyping of the theory, coupled with ad hominem attacks on anyone who dares to make such legitimate criticism. Given these circumstances I'm not too motivated to shut up about what is going on.

82. **E.**
April 21, 2007

Peter,
To me, your complaints about the 'attention that string theory receives by the media' don't seem to have anything to do with science. It really just seems like you and others are envious of that attention. Particle physicists have had the same experience for years from condensed matter physicists who feel that it receives too much money and attention. In fact, such jealousy is partially what lead to the cancellation of the SSC in 1993. There are reasons that people pursue string theory having to do with science that have nothing to do with how 'glamorous' the subject is in the popular culture.

83. **Peter Orland**
April 21, 2007

E.

You are off the mark here. The issue isn't high-energy physics versus others in the science community. The issue is how to allocate time and resources within high-energy physics.

84. **E.**
April 21, 2007

Peter O.,
Very little money actually goes to theory at all when compared to other science disciplines. However, I don't think attacking string theory is going to help this situation. Personally, I would like to see all areas of high-energy physics receive more money, not just string theory. I do think that within the string theory community itself, too much influence over what topics are important is in the hands of too few people, in particular the Princeton/Harvard/Stanford groups.

85. **E.**
April 21, 2007

Also, for those who like to think of LQG as a viable alternative to string theory, the basic problem with it is perturbative renormalizability. As Lee Smolin pointed out, it may be possible to include gauge theory within the framework of LQG, but not in a unique way. One of the reasons for the initial excitement of string theory is that it was found that the anomalies canceled for only two choices of gauge group SO(32) and E8 x E8, in particular since E8 contains within it groups like SO(10) which hold great phenomenological interest.

86. **Peter Orland**
April 21, 2007

E.

When you say "Very little money actually goes to theory at all when compared to other science disciplines. However, I don't think attacking string theory is going to help this situation", it seems as though you mean we should shut up about our concerns for the field, because others might get wind something isn't right. Such arguments are used to persuade people not to rock the boat within every elite. Should goverment officials, police, etc., not dispute what their communities do, because it hurts their image? On the contrary, disputing how a community often helps that community.

I think a serious debate (even peppered with nasty insults, as this one seems to be), might increase positive public interest in high-energy physics. In fact, I think it is already doing that.

87. **E.**
April 21, 2007

Here's a suggestion, why doesn't Peter Woit denote all money he receives from his book and speaking engagements to people studying alternatives to string theory?

Peter O.: There's nothing wrong with debating the merits of string theory. I think it holds up well in an honest debate. It's the attacks that have nothing to do with science that are not helpful. They only serve to give the impression among the public that physicists don't know what they're doing and will in the end hurt all of high-energy physics. I do believe that the discovery of SUSY at LHC will provide some vindication, since this has been theorized on for thirty years 'without showing any results'.

88. **Peter Orland**
April 21, 2007

E.

Attacks are not helpful? Then what was all that about Peter W.'s publication record? Why this nonsense about his donating his money to alternatives?

89. **anon.**
April 21, 2007

'Here's a suggestion, why doesn't Peter Woit denote all money he receives from his book and speaking engagements to people studying alternatives to string theory?' – E.

Smolin and Woit, the biggest critics who have written books, are both working on alternatives to string. The revenue from their writing and presentations (if these are actually commercial ventures??) isn't a reliable or sustainable way to fund alternatives!

'It's the attacks that have nothing to do with science that are not helpful. They only serve to give the impression among the public that physicists don't know what they're doing and will in the end hurt all of high-energy physics.' – E.

It's not the *attacks* but *string theory* which isn't even a speculative-yet-falsifiable *theory,* let alone *science*. The physicists working on non-falsifiable ideas, whether these are in mathematics or telepathy like Josephson's http://arxiv.org/abs/physics/0312012 or supersymmetry, aren't acting as *physicists* or *scientists* when they do this.

Seeing the amount of public hype these people sell in books, it's fortunate there is some public-arena backlash, or the public would remain duped.

90. **Peter Orland**
April 21, 2007

By the way, E. a lot of theoretical physicists who work on supersymmetry
are not as optimisitic as you are about its vindication at the LHC.

http://www.strings.ph.qmul.ac.uk/~dsb/dbwager.pdf

91. **E.**
April 21, 2007

Anon: I respect Lee Smolin and the work he has done.

Peter O.: I would say that only a small minority are not optimistic about the discovery of SUSY at LHC. There are some very good reasons why we should see it. At any rate, we'll soon find out. Who knows, maybe there will be some nice surprises. For the critics of string theory, I would say wait a couple of years until we have the results from LHC. If there is no SUSY, it would indeed be a big blow to string theory.

92. **E.**
April 21, 2007

To add one note, as John Ellis has said, it is only the optimists who accomplish anything.

93. **Peter Orland**
April 21, 2007

E., said "I would say that only a small minority are not optimistic
about the discovery of SUSY at LHC. "

I think you are wrong about this. I don't have any hard
statistics, but I talk with a lot of theorists and most privately think
superpartners won't be seen. I don't know if they would admit to
this in public.

By the way, since you bring up the subject of optimists, I am an optimist. My guess is that the LHC will eventually show us
something nobody anticipated. It's not very optimistic to
think that all we'll find is what people have predicted for
decades.

94. **island**
April 21, 2007

> I would say wait a couple of years until we have the results from LHC. If there is no SUSY, **[or higgs]** it would indeed be a big blow to string theory **[and the standard model]**.
>
> Amen

95. **E.**
April 21, 2007

Peter O.,
Of course, there are the large extra dimensions people who believe TeV scale gravity will solve the hierarchy problem and the experimentalists at LHC will have black holes coming out of their ears, but don't count on it.

96. **E.**
April 21, 2007

As for what we'll find at LHC, I think we can say that if the Higgs mechanism is correct, then you have to solve the hierarchy problem. There are two ways to solve the hierarchy problem, either there is TeV scale SUSY or the Planck scale is really TeV scale and gravity only seems weak because of large extra dimensions. If the Higgs mechanism isn't correct, then we'll be back at the drawing board.

My guess is that there may be some new states discovered besides the superpartners, perhaps the quarks and leptons of a heavy fourth generation or something completely exotic.

97. **Reader**
April 21, 2007

After reading some of the comments over at Clifford Johnson's blog I just have to ask…if E. and James have actually bothered to read Woit and Smolin's books? And bets on their answers?

98. **E.**
April 21, 2007

Reader,
Yes, I've read them both.

99. **woit**
April 21, 2007

Peter O.

Thanks for the link to the betting document, very interesting. I suspect even fewer would now be willing to bet on SUSY at the LHC.

E.,

The total income so far from my speaking engagements would support one postdoc for about one week. As for the book, it hasn't quite yet earned back its advance, a sum which, after taxes would support a postdoc for about one semester. I suspect people's ideas about the amount of filthy lucre I'm making on the backs of hard-working string theorists are unfortunately exaggerated.

I don't think one or more postdoc- years to support people for whom there are no permanent jobs would change much. What's required is a change in what is going on at the places that train the next generation of theorists. If Harvard or Princeton will offer me a discount and endow a permanent faculty position for non-string formal particle theory using the funds generated by my book and speaking engagements, I'm willing to make the donation.

100. **M**
April 22, 2007

E.: actually, if instead LHC finds that the hierarchy problem is a fake problem (e.g. by discovering no new physics beyond the Standard Model), this would be a great victory for anthropists, and a great Phyrric victory for string-landscape-anthropists.

101. **N. Nakanishi**
April 22, 2007

I would like to make two comments.

1. Even if LHC does not observe Higgs boson, it is still possible to save the standard model by slightly modifying it within the framework of the manifestly covariant quantum field theory. See hep-th/07042645.

2. I believe that the Green-Schwarz anomaly cancellation condition is based on the claim of the existence of gravitational anomaly by Alvarez-Gaume and Witten. But they committed a fundamental mistake. They confused the T*-product quantities with the T-product quantities. Those are different for the matrix elements containing the energy-momentum tensor, because its expression contains time differentiation in contrast to the current in gauge theory. The argument based on the path integral is also misleading, because the path integral gives the T*-product quantities only. For detail, see Prog. Theor. Phys. 115 (2006), 1151 or hep-th/0503172 v2.

102. **E.**
April 22, 2007

M: I'm not sure how this would help the anthropists. If the Higgs mechanism is right, then you have the hierarchy problem. If the Higgs mechanism isn't right, then we have no idea how to break the electroweak symmetry and generate mass for the quarks and leptons. I don't think technicolor is a viable option.

For those who don't think LHC will see SUSY (or hope that it doesn't), all I can say is we'll see who's right within two years or so.

103. **Cecil Kirkey**
April 22, 2007

Peter:

Your last response gives me an opportunity to ask for further explanation on the funding for theoretical physics research. If I am a capable theoretical researcher all I need is pencil and paper and access to the internet. I could get a job teaching at a CC and work at night or even become a patent clerk. I heard that has worked in the past. Why does a post doc need outside funding?

I guess this goes back to the whole issue that you and Lee and to some degree Bee have written about, i.e., giving capable people the opportunity to pursue their OWN research interests. How are they being prevented from that? The financial rewards should come AFTER a demonstration of capability; before that you only have potential.

I know of no first job where one can pursue their own interests and get paid a good salary unless you are a professional athlete.

To a layperson this seems like a major issue driving the anti-ST dialogue that is why I am asking about it. Thanks.

104. **Eugene Stefanovich**
April 22, 2007

Cecil Kirkey:

I like your ideas.

If I am a capable theoretical researcher all I need is pencil and paper and access to the internet.

I would add to this list a good scientific library nearby.

I could get a job teaching at a CC and work at night or even become a patent clerk.

The great advantage of this arrangement is that you are free from various artificial pressures, such as the pressure to publish or the pressure to groupthink.

Eugene.

105. **M**
April 22, 2007

E.: the weak scale might be unnaturally small because of anthropic selection, see hep-ph/9707380. See you within 3 years or so.

106. **JC**
April 22, 2007

offtopic post

Cecil, Eugene

Teaching at a CC is ok, though a full time assignment may be quite heavy. (ie. You may have to do all the grading too).

What's NOT ok these days, is teaching high school physics and/or math. It's a complete nightmare these days. (Long story, which I won't go into).

107. **anon.**
April 22, 2007

'If I am a capable theoretical researcher all I need is pencil and paper and access to the internet.' – Cecil

'I would add to this list a good scientific library nearby.' – Eugene

Maybe you also really need to interact with other people in the field, attend conferences, give talks, put papers with preliminary results on arxiv and submit papers to journals? Otherwise – although in isolation you will be safe from the perils of groupthink and consensus – with no critical discussion and feedback, there's no motivation to tackle hard questions needed to make progress and generate interest? Patent clerks haven't been doing much theoretical physics since Einstein resigned as one in 1909 to become a lecturer.

108. **E.**
April 22, 2007

M,
Hmmm..I'm not sure how an unnaturally low weak scale keeps the Higgs from getting radiative corrections that give it a Planck scale mass…unless the Planck scale is small too. How do you explain this without large extra dimensions? Also, what about the logarithmic running of the coupling constants and their unification, with SUSY, at 10^{16} GeV? Coincidence and/or illusion?

109. **Peter Orland**
April 22, 2007

E. (and M.)

The radiative corrections to the Higgs mass don't have to be of order the Planck scale. This means that the bare Lagrangian has to be fine-tuned, but this is an esthetic problem, not a fundamental one (maybe this esthetic problem could be solved by condensation of top-prime pairs, should there be more generations).

And as to the last two questions I think extra dimensions and unification of running couplings are not illusions but delusions. There is no field-theoretic or string-theoretic derivation of dimensional compactification to four dimensions. The existence of a unification scale is not so obvious as people seem to believe.

110. **Eugene Stefanovich**
April 22, 2007

'If I am a capable theoretical researcher all I need is pencil and paper and access to the internet.' – Cecil

'I would add to this list a good scientific library nearby.' – Eugene

'Maybe you also really need to interact with other people in the field, attend conferences, give talks, put papers with preliminary results on arxiv and submit papers to journals?' – anon.

Yes, these things should be certainly present. However, being a patent clerk does not reduce ones ability to do all these things. Maybe just a little bit.

111. **E.**
April 22, 2007

Eugene,
I think the idea of some brilliant, young guy working out secrets of the universe in isolation is a nice, romantic idea. Sadly, it really isn't practical anymore. The complexity and sophistication of modern theoretical physics is such that it's almost impossible to work on it in one's spare time and without working with others.

112. **E.**
April 22, 2007

Peter O,
It's always possible that we're in for surprises. Perhaps it is only an aesthetic judgement, but I think that fine-tuning and the anthropic principle were made for each other. Personally, I believe that the simplest and most natural explanation is best. Clearly SUSY falls into this category.

113. **Peter Orland**
April 22, 2007

Ok, if you think the supersymmetric standard model is simple and natural. I think it's an abomination!

114. **E.**
April 22, 2007

Personally, I think the MSSM is very beautiful.

115. **Cecil Kirkey**
April 22, 2007

E said:

"The complexity and sophistication of modern theoretical physics is such that it's almost impossible to work on it in one's spare time and without working with others."

So why should anyone, before proving themselves, be paid a full time salary to set around and think about modern theoretical physics with or without the direct inputs of others?? Do the work first and then get compensated.

It seems like as soon as some person gets recognized and rewarded with a tenured or life time position the output really falls. One example that readily comes to mind is Alan Guth. One shot in the dark, great position and then essentially no significant output. At least from what I can judge.

116. **E.**
April 22, 2007

Cecil,
Students in theory are expected to start proving themselves in grad school. One usually needs a substantial publication record to just get a postdoc, which are highly competitive, but don't pay especially well. It usually then takes several postdocs before one is able to get a full-time untenered faculty job. It is not an easy road.

117. **Peter Orland**
April 22, 2007

Cecil,

Alan Guth did other important things besides inflation. I suggest Peter W.'s blog not be used as a forum to point out the shortcomings of individuals, whether real or apparent (which I would argue is the case, in this instance).

118. **urs**
April 23, 2007

> This kind of technical discussion of LQG, while interesting, is completely off topic and not something I know know anything about.

Sorry. I thought it would be okay to reply to Lee Smolin's comment.

119. **Gina**
April 23, 2007

Dear Christine,

You quoted Peter Woit who wrote: "people should be expecting to spend most of their time on ideas that will fail, or lead to very modest advances, and there's nothing wrong with this." and added among other things: "But yes, the system as structured today is not ready to identify such eventualities as part of the effort, nor is open to encourage new ideas."

You certainly raise interesting points, and I should think about them more. As a quick reaction let me say that the notion of success is a multi-scaled notion, so I think people are expected to be successful at least on some scale. Thinking only about the largest scale is not healthy. This is why I disagree with Peter's quote above. As for changes in the current system it can be interesting to discuss alternatives. But I am not aware of any good suggestion.

120. **Christine**
April 23, 2007

I think people are expected to be successful at least on some scale.

Dear Gina,

Well, most people would like to succeed in life. The notion of success, however, is highly subjective and varies from people to people, from culture to culture. What do you mean when you say "people are expected to be successful" — expected by "whom"?

I do agree with you (if I understood you correctly), that there are many "levels" of success. I think it is a healthy posture to acknowledge success as not an absolute thing. Also, it is not something to pursue "above everything", but only up to the point where you are not damaging someone else in order to achieve it.

In science the notion of success should be, in principle, formal, that is, related to the issues I have outlined previously. One should first be very precise about the terms "theory", "hypothesis", "idea", etc, and the level of success on them measured against an objective standard. Mainly, the scientific method sets the machine running, and a sober community of scientist evaluates the progress from time to time — *if* they are in the position to do so.

Under such a basis is where — at some level — the success of string theory is supposed to be evaluated. First, is it a theory? Second, having defined what it is, then in its present stage, what is the objective evaluation of it from the scientific community?

One question that often comes is: are really only string theorists able to evaluate string theory? Is the rest of the community really excluded from such an evaluation, even at the more conceptual level? Even given a set of objective criteria that string theory (or whatever) should fulfill, if is is to be considered a theory (or whatever)?

How deep are the intricated technical issues of string theory, how fundamental they really are in order to set the stage for its conceptual understanding, so that it fixes a clear frontier against debate to a larger community? What is exactly such a frontier?

I do not have really many suggestions to offer, but hope to read reasonable ideas in due time.

Christine

121. **Cecil Kirkey**
April 23, 2007

Peter Orland:

I was not picking on Alan just using him as an example of someone who apparently has not published much original work after his inflation idea. I did check AXIV. I think it only goes back to 1992 though. My comment was meant to indicate that sometimes researchers are rewarded with a good position with maybe the expectation that they will continue to make break throughs and then end up contributing very little. Maybe the person ends up being a great teacher or not.

122. **Thomas Love**
April 23, 2007

E said:

“I think the idea of some brilliant, young guy working out secrets of the universe in isolation is a nice, romantic idea. Sadly, it really isn’t practical anymore. The complexity and sophistication of modern theoretical physics is such that it’s almost impossible to work on it in one’s spare time and without working with others.”

Young? What’s youth got to do with it? It is a myth that only the young can do innovative physics. Just learning the necessary mathematics can take many years.

Why work with others? “Inspiration was never known to strike a committee.”

Really good ideas are radical and more than likely will be shot down in a group setting by mediocre minds (to use Einstein’s phrase).

123. **anon.**
April 23, 2007

‘As for changes in the current system it can be interesting to discuss alternatives. But I am not aware of any good suggestion.’ – Gina

The change needed to the current theoretical high energy physics is to bring it into line with experimentally-substantiated scientific disciplines by discouraging the complacency which string theory has introduced over the past twenty years. It was fine for people to investigate strings in the 80s, but it’s now well and truly failed. M-theory is not the crowning glory of physics, a grand unified theory of everything, but is more like a dunce’s tin foil hat. It’s simply better to wear no hat than that of a charlatan or, at best, loser.

High energy theoretical physics is the one science in existence which is currently crowned by a piece of ‘theory’ which isn’t a single theory but 10^500 theories, and which is neither based on experimental data, not falsifiable even in principle. Falsify one in the landscape of 10^500 string theories, and you get nowhere because there are always plenty more where that came from.

M-theory is to physics what parapsychology is to psychology. It will certainly be a success in generating overwhelming media interest and drawing in excited gullible crowds at circus time (attracting the losers who read horoscopes and believe in other faith-based-phenomena), but it is damaging to scientific discipline, a travesty to the objectivity needed by genuine physicists, and frankly it’s no more than mathematical metaphysics.

124. **E.**
April 23, 2007

Just out of curiousity, suppose that we find one string theory vacuum that completely describes the world we live in and even predicts something that is later discovered. Would this constitute proof that string theory is on the right track?

125. **Peter Orland**
April 23, 2007

Cecil,

That’s not the point – you shouldn’t use this blog as a forum for critically evaluating individuals. It just upsets people without contributing anything positive. It is not just that I don’t agree with your evaluation (use SPIRES, not arxiv).

126. **E.**
April 23, 2007

Cecil,
What did Einstein do in the last thirty years of his career?

127. **anon.**
April 23, 2007

‘Just out of curiousity, suppose that we find one string theory vacuum that completely describes the world we live in and even predicts something that is later discovered. Would this constitute proof that string theory is on the right track?’ – E.

No it wouldn’t prove it, because it would likely be a coincidence, seeing that it is just one theory picked out of a landscape of 10^500. If one monkey at a typewriter types out a poem, it’s impressive evidence for monkey intelligence. Not so if the monkey is just one selected from 10^500 monkeys at typewriters, because there is then the large possibility that it is merely coincidence.

The landscape of alternative’s to string theory is not that big. Even if everyone on earth has an alternative idea, that’s say 10^10 theories, which is trivial (one part in 10^490) compared to the oft-quoted string theory landscape population of 10^500 theories. It’s just a lunacy that people want to investigate the string theory landscape when it will be a failure whatever it produces, instead of trying new things, like modelling the facts of the particle physics using new techniques, to gain deeper insights into the masses and symmetry breaking problems before experimental results come in from the LHC.

128. **E.**
April 23, 2007

Anon,
I disagree that it wouldn’t prove string theory. If there was one string theory vacuum that completely discribed our universe in every detail and was predictive, I would hardly think this is a coincidence. We simply then have the problem of understanding why this particular vacuum is selected. If we ever find such a vacuum, perhaps the answer to this question will present itself.

129. **anon.**
April 23, 2007

E., if you write down 10^500 different sets of predictions at random, one of them may turn out to be ‘right’, without actually being the correct theory, merely because of the coincidence that such a large number of possibilities holds.

Vagueness, i.e., a big landscape, increases the probability of agreement to nature by pure coincidence. The larger the number of possibilities, the greater the chance of agreement by coincidence.

As for ‘proof’, physics is about *useful* theories. String has popularly claimed to be useful since it is claimed to have been proved a renormalizable theory of quantum gravity and perturbatively finite, but it turned out that Mandelsham out proved it finite for certain types of infinite terms, and only three out of an infinite number of terms in the perturbative expansion are really known to be definitely finite. The Maldacena conjecture, hailed as a great result, again is unproved. So M-theory is not even proved to do what it claims with mathematical rigor.

Whatever the best fit in the 10^500 theories of string is to the real universe, it will need to have its Calabi-Yau manifold fixed to prevent the moduli or parameters of the extra dimensions from drifting in nature due to instabilities in each particle containing the 6 rolled up extra dimensions. This requires moduli stabilization with a Rube-Goldberg type machine, involving fields to hold the extra dimensions in place. The whole thing is so artificial and convoluted, it's just epicycles, caloric or phlogiston all over again, minus the (faked) 'evidence' of experimental agreement that epicycles, caloric and phlogiston had.

What you hope for is that string theory will advance from being not even wrong to being wrong like epicycles, caloric and phlogiston. Dream on.

130. **JC**
April 23, 2007

What would be hilarious is if in the year 2100, some kid finds a "theory' which replicates all the "exact" solutions to the S-matrix of the Standard Model, with only high school math (ie. basic algebra).

131. **E.**
April 23, 2007

Anon,
You're completely wrong. If string theory can provide a complete description of our world and can make predictions that turn out to be right, it is progress, even if we cannot understand why a particular vacuum is selected. As I've pointed out in my previous posts, string theory as we currently understand is a perturbative formulation. We do not have the 'real' theory, only approximations. If we could go beyond the perturbative description, it may be that the vacuum that corresponds to our universe is selected. However, in the absence of the nonperturbative description, we can only work backwards.

Also, I should point out that much of what you say are inaccuracies quoted directly from Smolin's book.

132. **anon.**
April 23, 2007

"You're completely wrong." – E.

If I'm completely wrong, I take it you have just proved the Maldacena conjecture? And you've just found the long awaited proof that perturbative string theory is finite?

"If string theory can provide a complete description of our world and can make predictions that turn out to be right, it is progress, even if we cannot understand why a particular vacuum is selected." – E.

That's exactly Ptolemy's argument in favour of the 'progress' of using epicycles to model the earth centred universe in 150 AD. Except, string theory doesn't even model anything real and observable in a predictive way, unlike epicycles.

"If we could go beyond the perturbative description, it may be that the vacuum that corresponds to our universe is selected. However, in the absence of the nonperturbative description, we can only work backwards." – E.

No, you can work forward from empirical facts and build theories on those, like Newton's dictum, *hypotheses non fingo*. You can make checkable predictions from a theory which is built on a framework of solid facts. You can't do that with a theory based on unobserved extra dimensions, branes, gravitons, supersymmetric partners, unification at 10^16 GeV, and other wishful thinking or abject, non-checkable speculative guesswork.

"… much of what you say are inaccuracies quoted directly from Smolin's book." – E.

Now you are being vague and inaccurate, much like string theory. Name one inaccuracy, instead of making up such vague, guesswork speculation.

133. **E.**
April 23, 2007

Anon,
People like you really aren't worth my time.

134. **E.**
April 23, 2007

Anon,
Can you prove that perturbative string theory isn't finite? And what does Ads/CFT have to do with my arguments? It's clear to me that you really don't know very much except whatever propaganda Woit and Smolin have told you, which you just repeat.

135. **Michael Bacon**
April 23, 2007

E wrote: "If string theory can provide a complete description of our world and can make predictions that turn out to be right, it is progress, even if we cannot understand why a particular vacuum is selected."

Well yes . . if it could do those things it would be progress, but it can't, can it? I'd be curious who thinks it is even possible to provided a "complete" description of the world and make "predictions", while not be able to undersand why a particular vacuum is selected?

136. **woit**
April 23, 2007

I've been under the weather with a migraine headache the last day or so, in retrospect probably should have deleted most of the comments in this thread. If people have something substantive to say, please do so, but I'll delete any more repetitive and pointless bickering, whether it's pro or anti-string theory. And Peter O. is right, this isn't the right place for public expression of views denigrating particular people.

137. **Cecil Kirkey**
April 23, 2007

Peter:

Sorry to offend. You and Lee and Bee to some degree have suggested that part of the problem in fundemental theoritical physics is the alck of a system that allows a young researcher to pursue their OWN interests. My point in the posts was to try to understand exactly how these people are being stopped from doing whatever research they choose.

Tenured positions are rewarded on past accomplishments and maybe future expectations. But I think if an objective study was done in theortical physics that the output of tenured researchers is significantly less than that of non-tenured researchers.

I see NO reason why any researcher cannot work on something other than string theory and prove themselves worthy and then earn a coveted tenured position based on accomplishments and not potential.

138. **Peter Orland**
April 23, 2007

Cecil,

People can win tenure on either accomplishments or potential. Most
Universities want people who can win grant money. Generally speaking, potential wins out over accomplishment in this regard.

A small number of physicists have obtained tenured positions doing
neither exclusively string theory, phenomenology nor numerical lattice work, but instead pursuing their own ideas over their entire careers. They are lonely, ignored and happy.

139. **Peter Woit**
April 23, 2007

Cecil,

I don't think you're very aware of what the realities are for young theorists. It's not just that they need to get tenure. Typically they are in postdoc positions with terms of maybe three years. Starting such a position, knowing that in two years you'll have to apply for another postdoc (competition is stiff) or a tenure-track job (competition is extremely stiff), should you try and work on something outside the mainstream, assuming that in those two years you'll have impressive enough results to convince hiring committees to hire you? It's not an easy position to be in. I think anyone who knows how the system works, and looks at the pressures and incentives it puts on young people, has to acknowledge that this is a major reason why few young people work on very ambitious ideas.

I think you're also highly unrealistic about the possibilities for doing important work in this area in spare time after a full-time job. Few people have the energy for this, and contact with smart people (grad students, colleagues, visiting people from other institutions) with similar interests is invaluable and something you'll not get.

140. **Eugene Stefanovich**
April 23, 2007

Peter,

What you described is just a perfect recipe for groupthink.

You correctly pointed out that current system forces young people to stick with mainstream ideas. Then you suggested that the benefit of remaining within this system is the opportunity to interact with other people (who presumably are sticking with the same mainstream as well and pulling you inside the same vicious circle).

Can we assume that if these "smart people" have something interesting to say, they already published that in the arXiv? So that I can benefit from their ideas while keeping a safe distance from the deadly mainstream current?

141. **woit**
April 23, 2007

Eugene,

Universities are not so homogeneous, they often contain people who have found a way not to work on mainstream ideas. Even people who work mainly on mainstream ideas often cannot avoid having acquired some wisdom on one topic or another. One can make progress just by oneself, but having smart people who know things you don't know to talk to is often very helpful. And not everything they know is on the arxiv, or, especially for mathematicians, if it's there it may be in a form that is much much harder to get at than by talking to someone who understands the field and can explain the basic ideas. Mathematics is still in some ways an oral tradition.

The other great benefit of universities is getting the opportunity to teach, if you can do it at a quite advanced level. Teaching something to people is an invaluable way of learning it deeply oneself.

My own experience has mostly now been in math departments, where I have learned a huge amount from colleagues, talks and from teaching. It's true that this is an environment inherently pretty far removed from current mainstream particle theory and its concerns.

142. **anon.**
April 24, 2007

Peter, have you heard any whispers leaking from Brian Greene's office at Columbia about the progress of deciding a winner for Discover magazine's competition for a 2 minute explanation of string theory? The deadline was about 6 weeks ago. Must be a tough call to decide the best one. http://discovermagazine.com/

143. **Peter Woit**
April 24, 2007

anon.

For some reason, neither Brian nor the people at Discover have decided to consult me about the choosing of the winner for that competition…

144. **Dan**
April 24, 2007

Personally I think you should have been consulted as a dissenting voice in the PBS Nova special Elegant Universe, along with computer graphics with the problems with string theory and supersymmetry.

145. **Coin**
April 24, 2007

Um, wasn't that all the way back in 2003?

146. **island**
April 24, 2007

Coin and Dan together make a good point, though, I think. Peter and others have been saying the same thing for many years, but professional and popular recognition didn't occur with any real impact until Peter and Lee started writing books about it, so at least now, future "specials", (and his peers), will more seriously consider his, Lee's, and others' dissenting opinions are real concerns of physicists that should be considered with the rest, rather than to allow things to appear to be more than they really are strictly for the advancement of egos and popularity. That cannot be good for science.

147. **Shantanu**
April 26, 2007

Peter, sorry for the OT comment as this is FYI. I was looking at the webcast
of the on-going
black hole conference at STSCI. although all talks are superb, you will be interested in the talk by
Horowitz which is on black holes in string theory (and in particular the discussion) See the answers to Narayan and Livio's questions.

Witten on Gauge Symmetry Breaking for Mathematicians

April 25, 2007
Categories: Uncategorized

Edward Witten has a new expository article, aimed at mathematicians, to appear at some point in the Bulletin of the AMS, but now available here. It's based on colloquium-style lectures to mathematicians he has given over the last few years (including here at Columbia) and is entitled "From Superconductors and Four-Manifold to Weak Interactions". The paper is organized around describing various aspects of gauge-symmetry breaking, but pretty much sticks to aspects of the problem that don't involve the full quantum theory, just analysis of classical Lagrangians.

He begins with a description of the Landau-Ginzburg model of superconductivity, and various physical phenomena that it describes including the Meissner effect, Abrikosov-Gorkov flux lines, and Type I and II superconductors. Solutions for a special case are described using complex-analytic techniques. Exploiting an analogy to the Landau-Ginzburg case, he next takes up the Seiberg-Witten equations and their use by Taubes to get invariants for symplectic 4-manifolds and existence theorems for pseudo-holomorphic curves in them.

Witten's final topic is electroweak gauge symmetry breaking and the Higgs mechanism in the Standard Model. He ends by remarking that in the superconducting case the analog of the Higgs field is just an effective field for a different underlying physics, and mentioning technicolor as an implementation of something similar in the electroweak case, while noting that precision electroweak data shows no signs of anything other than an elementary Higgs field. He comments "But it is always possible that the right alternative has not yet been proposed" and explains how the LHC should definitively see a Higgs particle if the SM is correct since current bounds place its mass between 115 and 200 GeV.

The paper is purely expository, and aimed at mathematicians. It's interesting to see that, even though there aren't any really new developments in the area of gauge symmetry breaking, Witten clearly sees it as a fundamental problem every bit as deserving of being explained to non-physicists as string theory.

Comments

1. **Bee**
   April 25, 2007

   Hi Peter,

   don't forget to have a toast on the guy who said

   "Das ist nicht nur nicht richtig, es ist nicht einmal falsch!"

   Best,

   B.

2. **Jimbo**
   April 25, 2007

   Peter,
   Accd'g to the latest results (4/15/07), the new upper bound on the Higgs boson mass is ~144 Gev at the 95% CL:
   http://www.physorg.com/news96040503.html
   This and the lower bound U cite, put the mean at about 130 Gev.

   J

3. **woit**
   April 25, 2007

   Thanks B.,

   I'll definitely remember to toast Pauli on his birthday before the evening is out!

4. **DB**
   April 26, 2007

   Jimbo wrote:

   "Peter, Accd'g to the latest results (4/15/07), the new upper bound on the Higgs boson mass is ~144 Gev at the 95% CL:
   http://www.physorg.com/news96040503.html
   This and the lower bound U cite, put the mean at about 130 Gev."

   At the end of the quoted article is the link to the next article:
   "Mathematician suggests extra dimensions are time-like"

   Oh well, the real physics was nice while it lasted.

5. **Daniel Doro Ferrante**
   April 26, 2007

   Hi Peter,

   Just to point out a couple of things:

   (a) One can find a more detailed historical account of the facts leading to the discovery of Spontaneous Symmetry Breaking in the following link: A Physics History of My part in the Theory of Spontaneous Symmetry Breaking and Gauge particles with a mix of modern ideas (PDF, 194Kb).

   (b) Thus, in all fairness, there should have been a link [in Witten's article] to Global Conservation Laws and Massless Particles (GHK). As can be seen in Spontaneous Breakdown of Symmetry in Axiomatic Theory (page 515) and Spontaneous symmetry breaking in local gauge quantum field theory; the Higgs mechanism, there are some "subtleties" treated in the GHK which had not been previously considered — and those were important for the future development of the ElectroWeak symmetry breaking and the Standard Model.

Lastly, i would personally say that there have been some "new developments" in this area: Computational High Energy Physics Group at Brown. For instance, we have some new numerical methods (which we're still improving and sharpening) that are able to deal with Symmetry Breaking and different phases in a given QFT (something that ordinary Monte Carlo simulations cannot do): We've been using some Stochastic Quantization techniques, together with some techniques from Wiener Polynomial Chaos, and have been able to tackle the issue of symmetry breaking in simulations of QFT quite well. Also, we've been working on the relation between Symmetry Breaking and Topology Change (there will be a new [sequel] paper coming out on this topic shortly); a topic that has potential implications to Quantum Gravity at large, once it deals with Topological Transitions, explaining them in terms Symmetry Breaking (and Morse Theory). So, i think the playfield is still active… 😉

Cheers, Daniel.

6. **Jimbo**
   April 26, 2007

   DB,
   Suggest U keep an open mind. That mathematician is Penrose's former student, George Sparling. U might be impressed if U attempted to read the article. Itzhak Bars has also investigated Xtra time-like dimensions as well.

7. **Kea**
   April 26, 2007

   Jimbo

   That's fantastic! Thanks for the link. FINALLY, some decent physics makes the news.

8. **Daniel Doro Ferrante**
   April 27, 2007

   Just a quick note in order to stress points (a) and (b) of the comment above: Both, Weinberg's (PDF, 76Kb) and Salam's (PDF, 1.16Mb) Nobel Lectures credit GHK and, in fact, Salam goes as far as saying that he was tutored by Kibble [in the technique of Symmetry Breaking]. Furthermore, Glashow's (PDF, 59Kb) [Nobel Lecture] also cites GHK and Symmetry Breaking, albeit in a "funny" remark (see paragraph 3 of page 498 — here i mean "funny" under the context given by Bob Marshak's remarks on the 3rd Shelter Island conference).

   And, to drive those points home, note the acknowledgements in Spontaneous Symmetry Breakdown without Massless Bosons. 😉

   Cheers, Daniel.

9. **joseph smidt**
   April 27, 2007

   Thanks for posting this. I love these kind of papers.

10. **Jonathan**
    April 29, 2007

    Hello,

    We invite you to discuss science in our open forums. Subscription to the forum is anonymous.

    http://readingmen.org/forum/index.php

11. **anon.**
    April 29, 2007

    Jonathan, is your invitation to the 'reading men' science forum open to women as well as to men? Or are women excluded? I hope women are included, for a variety of reasons.

12. **Kea**
    April 29, 2007

    Too late. We're boycotting it.

13. **anon.**
    April 30, 2007

    Kea, 'boycotting' is a sexist term.

14. **Kea**
    April 30, 2007

    Oops, sorry! Personcotting.

15. **anon.2**
    April 30, 2007

    I like how some blogs make it difficult to tell when someone is joking. Charles Boycott is the source of the eponym.

16. **Jonathan**
    May 6, 2007

    as I wrote registration is anonymous. We do not check, gender:))

17. **Jonathan**

May 22, 2007

We have changed our domain name. Now, It is
http://advpubl.org/

and the forum is at
http://advpubl.org/forum/index.php

Thanks.
J.

Various Events and Other News

April 29, 2007
Categories: Uncategorized

Upcoming events in and around New York, including several I'm planning to attend:

The New York Academy of Sciences is having an evening of lectures this Wednesday, hosted by Frank Wilczek, on the topic of Expanding Frontiers of Physics and Cosmology. Speakers will be Max Tegmark and Nima Arkani-Hamed.

The YITP at Stony Brook is having a symposium to celebrate its 40th anniversary, and many former students, faculty and postdocs will be in attendance. I plan to definitely spend Thursday out there, maybe also Saturday.

One reason I likely won't be out at Stony Brook on Friday is that I'd like to attend at least some talks at another event that will be downtown at the new location of the New York Academy of Sciences. It's the 9th Northeast String Cosmology Meeting, co-sponsored by Columbia's ISCAP. Edward Witten will be among the four people speaking.

There will be an event entitled When the Scientist Becomes the Story at NYU next week, on May 8th, featuring a discussion about John Nash and Francis Crick with their biographers.

Much farther in the future will be next year's program on representation theory, algebraic geometry and physics at the mathematics division of the IAS in Princeton. This will include a conference November 26-30 with a title reflecting my favorite topic "Gauge Theory and Representation Theory". Presumably much of the focus will be on the Geometric Langlands program.

Closer in time, but farther in distance, I'll be speaking at a science festival called FEST in Trieste on May 18th. In June my book is supposed to be coming out in an Italian edition. I have to be in London the evening of May 23rd, then will head back to New York the next day. Currently trying to come up with a plan for how to spend the time in between, with the leading possibility a train trip through the Alps to Geneva, then a stop in Paris on the way to London.

In other news:

Lee Smolin has put up on his web-site a response to the review of his book and mine by Joe Polchinski.

On the Fields Medalist blogging front, there's a report from Terry Tao about a symposium at UCLA where he and three other Fields medalists gave talks. He gives a detailed description of the talks, including one by Richard Borcherds on QFT that sounds somewhat mystifying to me. Alain Connes at his blog gives his take on some of the talks delivered at the recent conference in his honor.

I've recently for no particular reason run into various interesting domain-names that some mathematicians and physicists are using for one purpose or another: monodromy.com, cohomology.com, and stringvacua.org.

A couple links mentioned by commenters here that deserve more visibility:

Neutrino Unbound is a site devoted to all things neutrino.

An interesting document concerning a bet made several years ago about whether supersymmetry will be found at currently (or soon-to-be) accessible energies is available here. Maybe someone can think of a way to get more particle theorists on the record about this…

Update: For upcoming events really far afield from here, I should mention that the new Kavli Institute for Theoretical Physics in Beijing is starting to get organized. Jonathan Shock reports that there will be an opening ceremony at the end of May, a two month program on Quantum Phases of Matter starting in June, and a program on String Theory and Cosmology in the fall.

Update: I've just heard that Discover Magazine has chosen the finalists in its "String Theory in Two Minutes or Less" contest. No, I didn't enter. Here they are.

Comments

1. **CapitalistImperialistPig**
   April 29, 2007

   Peter,

   Thanks for pointing out Polchinski's review and Smolin's response. This is dialog that actually is instructive – and which was unfortunately missing from some earlier reviews of your book and Smolin's by ST er's.

   If the protagonists will concentrate on the physics, as I think you and Lee did, and as Joe seems to have, the debate not only becomes more informative but more pleasant to observe.

2. **Peter Woit**
   April 29, 2007

   CIP,

   I think both Lee and I have been surprised at how little serious, substantive public reaction our books have gotten from string theorists, with Polchinski one of the welcome exceptions. I certainly have found that, in private discussions, most string theorists and I find lots of common ground, and that we agree about much more than we disagree. The kind of fanatical response that one sees on various blogs I don't think reflects the views of many serious string theorists, although it does provide evidence of some of the sociological problems afflicting the subject.

3. **Marco**
   April 30, 2007

   Just a short request: if your stop in Paris becomes reality, and if some public event is planned for it, please don't forget to mention the details here so that one can attend. Thanks.

4. **Thomas Love**

April 30, 2007

Peter said:

"Maybe someone can think of a way to get more particle theorists on the record about this…" (supersymmetry).

Suppose we speak our piece here? Supersymmetry will never be found. It was one of those crazy ideas that wasn't crazy enough.

5. **V**
April 30, 2007

My prediction: Low energy supersymmetry will be discovered within the next two years, as well as the Higgs. Huge triumph for the theoretical physics of the last thirty years. Certain people will be humbled.

6. **Ari Heikkinen**
April 30, 2007

About that Smolin's response, you still have to admit that those top string theorists (like Greene or Kaku) are still damn good at public relations and selling string theory to the public.

I wish I had as good speaking and presentation skills as say Brian Greene..

7. **Coin**
April 30, 2007

From Smolin's response thing:

the technical argument I had in mind… begins with the fact that supersymmetry requires that the cosmological constant be zero or negative
…wait… what?

Is this true? I'd never heard anything about this, and it seems like if it were true it would be a big enough deal to have come up before now– since there are indeed still people who expect we will see supersymmetry at the LHC, yet we've got this Dark Energy thing now that at is at least analogous to the positive cosmological constant. Right?

If this isn't too big a question to ask within a a blog comment: *Why* does supersymmetry require that the cosmological constant be nonpositive, if indeed it does?

8. **Aaron Bergman**
April 30, 2007

The condition is for unbroken supersymmetry. It arises because the only term in the SUGRA lagrangian that can give rise to a cosmological constant is negative definite. Once you break SUSY, however, you can have positive contributions and are able to get a positive cc.

9. **Coin**
April 30, 2007

That helps a lot, thanks.

10. **woit**
April 30, 2007

Coin,

There's more to it than what Aaron writes. If you want more of the story and some references to follow, it should be possible to extract the physics from the vitriolic attacks by multiple string theorists on me in this thread:

http://www.math.columbia.edu/~woit/wordpress/?p=454

11. **Thomas Larsson**
May 1, 2007

I once saw a quote by Witten in APS News, where he said that the discovery of the negative cc was the most disturbing fact he had ever learned. My impression is that it is not so much the magnitude as the sign which is troubling; de Sitter space apparently lack the right kind of asymptotic regions where you can put holographic data, cf hep-th/0106109. You can almost hear from its name that AdS/CFT will run into problems if spacetime is de Sitter rather than anti-ditto.

12. **Cecil Kirkey**
May 1, 2007

Peter:
Re: String Theory Videos

I looked at a couple of the videos and they finally inspired me to ask your opinion of the following.

Assuming ST to be correct for sake of argument are particles really strings or do the physics suggest that all interactions can be modelled AS IF the particles were strings?

This has always bugged me. What is the reality? Vibrating strings, tension, total unobservable properties. I suppose that this is really a big question on what constitutes physical reality, but don't physical theories attempt to model reality not actually describe what reality is?

In this same vein when the string is quantized it leads to the requirement of more 4 dimensions. Is there more than one way to quantize a system or is quantization unique? Do you know of any work that attempts to avoid quantization of a classical system and just start right away with an already defined quantized system? Thanks for the feedback.

13. **Jean-Paul**
May 1, 2007

You've got to live with it: the world is NOT holographic — it is truly 4-dimensional. QCD is NOT holographic, it is NOT topological. Standard model is NOT a

topological QFT.
Welcome to the world of real dynamics where you have to work harder than just to count instantons and everything is fixed by some analycity constraints. I wished we paid more attention to the failure of (bootstrap) S-matrix theory. Actually, the most interesting non-perturbative results in QCD were obtained long ago, just after it replaced the S-matrix approach to strong interactions. Later, holomorphic SUSY took the bandwagon on another funride, diverting attention from serious dynamical problems: the bootstrap lesson was quickly forgotten.

14. **Peter Woit**
May 1, 2007

Cecil,

People can have different philosophical viewpoints, but to me what is physically "real" at a fundamental level is whatever are the fundamental elements of our most successful physical model. Right now, that's the standard model, so certain quantum fields are the fundamental things. If string theory were right, it would be something different, depending on what was the successful string theory model (e.g. it might be quantized fields on the space of strings, or matrices if you believe some versions of M-theory, or something else).

It's somewhat of a mystery that our most successful quantum theories are formulated by "quantizing" a classical system. One can come up with quantum theories that don't arise in this way, the quantum mechanical formalism is inherently more general.

15. **V.**
May 1, 2007

Cecil,
Very good question. I think the idea of the elementary particles being extended objects is supposed to model the fact that the spacetime geometry breaks down at the Planck length. It may very well be the case that quantiziing a classical string is accurately modeling reality, provided that the different ways that it may vibrate are restricted, e.g. through compactification of extra dimensions. That is to say that the extra dimensions and their compactifed geometry aren't actually 'real', but rather a mathematical device required to model the 'true' reality with a classical string. In this view, their will be one and only one compactification that corresponds to the true theory, while the landscape of string vacua is a mathematical artifact left over from starting with a classical object.

16. **Chris Oakley**
May 1, 2007

Cecil,

Quantization is far from being unique. One can very easily get contradictory results by choosing the "wrong" Poisson brackets to turn into quantum commutators. What rescues the situation is the restrictions on the space of possible quantum solutions owing to group-theoretical considerations. For example, the basic Poisson bracket to commutator substitution for [x, p] follows from the notion that the Q.M. momentum operator is the generator for space displacements – Planck's constant merely being a scaling factor determined by our unit of classical momentum; similarly for time displacements and rotations to get the Q.M. Hamiltonian and angular momentum operator.

For quantum fields the basic Poisson bracket → commutator quantization fails for all but the simplest case – hence (e.g.) the need for Gupta-Bleuler quantization for helicity 1. But once again the situation is rescued by group theory – in this case the need to produce a spectrum of physical states that are an irreducible unitary representation of the Poincare group.

If you are interested, have a look at these notes on my web site that develop QFT without mentioning quantization.

17. **Chris W.**
May 1, 2007

Thomas,

In mentioning Witten's remark, did you mean to write "discovery of the *positive* cc"? See the exchange between Coin and Aaron Bergman.

18. **Chris W.**
May 1, 2007

V,

Would it be fair to say, given the viewpoint you sketched in response to Cecil, that the classical string might eventually be understood as a classical image of something embedded in spacetime, which arises from some more primitive "pregeometric" object in a dynamical substrate that generates spacetime as a kind of mean-field classical approximation?

Maybe this should become a central element in the effort to push string theory forward, instead of ongoing elaboration on differential geometry and its abstract connections with other areas of mathematics, in which questions of ontology (and the early misgivings of Green and Schwarz) have been largely pushed aside as irrelevant.

19. **Leonard Ornstein**
May 1, 2007

Peter:
We try to approximate aspects of reality with what our senses and instruments detect and measure. We appreciate that, at best, the measurements can only be incomplete approximations, subject to measurement noise and (in spite of our best efforts), possible undetected bias.

These observations are used to try to generate self-consistent models of that reality. And we use further observations to determine the closeness of match between the models' predictions and what we can access of that reality.

Against this background, it's difficult to understand "what is physically 'real' at a fundamental level is whatever are the fundamental elements of our most successful physical model".

Unobservable singularities and infinities, at least temporarily, would become part of your reality. That reality is a compromise of the moment (and might not easily provide motivation for going beyond present observation).

It may be the best we presently can do. (And I appreciate the compelling and amazing matches between QED predictions and magnitudes like that of the fine structure constant.) But QED still 'only' a model.

The Platonist believes that in the limit, the ideal model will ultimately provide more perfect descriptions of reality than our senses and instruments can ever provide; and that theoretical physics will be indistinguishable from a branch of mathematics..except for its 'content' of 'physical reality'.

Your 'definition' above implies that you are a Platonist. This seems to conflict with my understanding of your usual positions in this blog…and in your book!?

20. **V**
May 1, 2007

Chris,
Yes, I'm trying to say something similiar to this. Most string theorists would say that M-theory will be such a formulation. One of the striking things about perturbative string theory is that the strings are quantized, but the spacetime they live in is purely classical.

21. **Peter Woit**
May 1, 2007

Leonard,

Sure, I'm a Platonist, even often describing my views as radically Platonist. Many people don't seem to have read my book very carefully, a large part of it is about the relation of mathematics and physics, claiming that they are very deeply connected, and it ends by suggesting more attention to this.

We know the world through our senses and through experiment. These are how we know that our mathematical models of physics have to do with "reality", and are not just random abstract structures. I've always made clear that I don't object to string theory on the grounds that many people do, that it is "too mathematical". My objections to it are two-fold:

1. When you try and use string theory to make a model of reality, you are forced into huge amounts of complexity and ugliness.

2. String theory is inherently unpredictive, there is zero experimental evidence for it.

I see 1. and 2. as closely related problems.

22. **V**
May 1, 2007

Dear Peter,
I have to take objection with your first point that string theories models are complex and ugly. On the contrary, models in Type II theory involving intersecting D-branes are very geometric and beautiful, and the standard model is very naturally accomadated. Regarding your second point, perhaps there is some validity to this at present. However, this may not always be the case as further progress is made.

23. **a quantum diaries survivor**
May 1, 2007

Hi Peter,

since bets on supersymmetry are mentioned here, let me mention that I had a more substantive bet open on my site last year.

Jacques Distler and Gordon Watts have bet respectively 750$ and 250$ with me that new physics (SUSY, or something else beyond a SM Higgs) will be found at the LHC within two years from having collected 10/fb per experiment. If SUSY is found, I lose $1000. If
nothing is found, I win $1000 with which I can try to cheer up my saddened soul.

Cheers,
T.

24. **Peter Woit**
May 1, 2007

V.,

I'll just quote Zwiebach, page 346, about these models:

"the models seem contrived, at least in the sense that they are engineered to give the physics that we observe, rather than obtained naturally as the simplest solutions to string theory. The Standard Model is an intricate construct, and present-day attempts to describe it within string theory are not simpler."

25. **Nameless**
May 1, 2007

Smolin mentions in his reply to Polchinski's review that N=4 SYM has not been rigorously constructed yet. I wonder (and this is certainly the most stupid question ever asked in this blog) whether N=4 SYM is asymptotically free?

26. **Peter Woit**
May 1, 2007

Nameless,

The N=4 SYM beta-function is zero, so the coupling constant doesn't run, there's an ultraviolet fixed point at non-zero coupling. It's not asymptotically free, since the coupling doesn't go to zero.

One of the big problems with using string-gauge duality to understand QCD is exactly this: you don't get asymptotically free theories like QCD.

27. **V**
May 1, 2007

Peter,
If you'd ever get your hands dirty with real work, you'd actually have a better sense of these models, rather than relying on a comment in Zweibach's book. There are many, many constraints that one must satisfy in order to build realistic models, so that it very definitely not contrived. See hep-th/0612087 for a beautiful example.

28. **woit**
May 1, 2007

V,

Sorry, but that looks completely contrived to me. Then again, it may look less contrived to those doing the contriving….

29. **Nameless**
May 1, 2007

> One of the big problems with using string-gauge duality to understand QCD is exactly this: you don't get asymptotically free theories like QCD.

And even in the conformal case, only the large N limit, I gather?

Thanks for your reply!

30. **V**
May 1, 2007

Peter,
I doubt very seriously that you have the knowledge and experience in either string theory or particle theory to make such judgments. In any case, you will claim everything is contrived since it suits your purposes, regardless of the reality. I would have some respect for you if had something postiive and constructive to say, rather than just continually nagging.

31. **woit**
May 1, 2007

V.,

You've chosen to remain publicly anonymous, while making it clear to me who you are. I really don't think you're in a good position to attack my or anyone else's credentials, and in any case that's not something you have any business doing from behind the cover of anonymity. Please cut it out.

32. **kuos**
May 1, 2007

Wow V, the number of matter generations in that beautiful noncontrived model only differs from reality by 33%. You've made a believer out of me.

33. **dave tweed**
May 1, 2007

V wrote "There are many, many constraints that one must satisfy in order to build realistic models, so that it very definitely not contrived." I want to understand the logic implied by this statement. Are you saying (where "we" just means "people who understand string theory") "We understand genuine principles of physics (which aren't just "we need to end up with a realistic model" conditions in disguise) that give lots of constraints. If we build a model which satisfies those constriaints, we get a realistic model" or "We know various things must be constrained to get a realistic model. If we build a model satisfying those constraints, we get a realistic model"? I'm just trying to understand precisely what is being claimed.

34. **V.**
May 1, 2007

Peter:
By no means have I let you know who I am anonymously. You can make whatever assumptions you like. I point to this model as one that I'm familiar with which is very simple, yet from it comes the entire structure of the MSSM. All essentially from a set of numbers which are all plus/minus one.

kuos:
How do you know there won't turn out to be four generations, with the fourth generation being heavy? In any case, is this not an experimental prediction?

Dave:
In constructing models, there are many nontrivial consistency conditions which must be satisfied such as tadpole cancellation, conditions for supersymmetry, K-theory charge cancellation, etc.. Never mind getting the right matter spectrum.

35. **Aaron Bergman**
May 1, 2007

And even in the conformal case, only the large N limit, I gather?

No. The AdS/CFT conjecture in its most powerful form holds for finite N. Gauge/geometry duality is also not only restricted to CFTs.

36. **dave tweed**
May 1, 2007

V, I'm not a physicist so I don't understand what those terms mean in detail, and in the details you seem to have avoided commenting one way or another on whether these conditions are _primarily understood on their own terms_, or just understood as necessary in order to avoid producing features in concrete instances of the model which are not realistic. To put it another way, suppose tomorrow we were to observe something that implied, eg, "incomplete tadpole cancellation" happened in the universe (whatever that might mean), presumably the fact that it's a consistency condition of string theory means no variety of string theory would be unable to model this, right? (I'm just trying to clear up what seem like vagueness about which conditions are put in to ensure a match the observed world — and could be removed if experimentally falsifed — and which are logically inviolable parts of the mathematics.)

37. **dave tweed**
May 1, 2007

Ugh: double negative is just a typo, should be "no variety of string theory would be able to model this".

38. **anon.**
May 1, 2007

Peter wrote:

One of the big problems with using string-gauge duality to understand QCD is exactly this: you don't get asymptotically free theories like QCD.

Aaron wrote:

Gauge/geometry duality is also not only restricted to CFTs.

It might be worth expanding on these two statements for the benefit of the reader who doesn't know about gauge/gravity duality. I would say they are both correct. First, there are *non*conformal theories for which the duality is really under control. These are not asymptotically free theories, but theories which are strongly coupled even in the UV. The canonical example is Klebanov-Strassler, but there are many others.

For asymptotically free theories, the trouble is that the gravity side becomes strongly coupled when the gauge side is becoming weakly coupled. One can get interesting theories in the same *universality class* as gauge theories of interest; e.g. Witten found a supergravity black hole background in the universality class of nonsupersymmetric Yang-Mills. This background seems to have a tunable parameter that can be adjusted to reach the true non-SUSY Yang-Mills, but the problem is that in trying to take this limit one runs into a regime where calculations cannot be done. Similar almost-dual backgrounds exist for theories with flavor (Sakai-Sugimoto, etc.).

In that sense, gauge/gravity duality provides something much like the lattice strong-coupling expansion: a way of calculating in a theory very much like the theory of interest, weakly coupled where the dual is strongly coupled, with a limit one can take to reach the theory of interest (and no phase transition when taking that limit). In both cases the desired limit is not tractable, but qualitatively most of the properties of the theories are similar. In the case of the lattice, the desired limit actually *can* be done (with difficulty) on a computer, at least for calculating simple enough quantities. There's no analogue of that for the gravitational dual.

39. **dave tweed**
May 1, 2007

Third post: in case it's not clear, I'm not looking for an explanation of any of these things, just a clear statement "we have most of the model conditions as an unavoidable requirements of string theory" or "most of the model conditions are motivated by the need to obtain a realistic model", just in order to understand what level of claim for these sorts of string theory model is being made.

40. **Nameless**
May 1, 2007

Thanks, Aaron.

41. **V.**
May 1, 2007

Dave,
The requirements that I mentioned are basically for internal consistency at the quantum level, and these requirements must be satisfied apart from any considerations of matching the model to observed physics. For example, if the tadpoles are not cancelled there will be what's call a triangle anomaly. Once these conditions are satisfied, one would like to have the gauge group and matter representations of the MSSM, a full set of Yukawa couplings (necessary for generating mass), and gauge coupling unification.

In intersecting brane models, this is accomplished by having different stacks of D-branes which wrap around the compactified dimensions. Every time the different stacks intersect, there is chiral matter localized at the intersection. The idea is to have a configuration which gives the standard model. However, as mentioned above, it is not possible to have any possible configuration, as this is seriously constrained by the need for internal consistency. The model that I pointed out has basically the simplest possible configuration, where the different D-branes wrap around each compactified direction exactly once.

42. **Peter Orland**
May 1, 2007

V.

One of the reasons some people get frustrated with string theory as a fundamental description of the universe is that arbitrary scenarios are invoked. Intersecting D-branes are of academic interest for constructing models with particles of different colors and flavors; that doesn't mean that this is how quantum numbers arise in nature.

The problem is that there are no dynamical calculations in string theory. No dynamical calculation shows that there is Calabi-Yau (or any other, including KK) compactification. No dynamical calculation shows that certain brane configurations dominate (including the one you advocate). This is what I always found unsatisfying about attempts to do string phenomenolgy, even back in the eighties – there are plenty of scenarios, but no analysis. Landscape advocates go even further in this respect. They have openly given up on justifying these scenarios.

43. **V**
May 1, 2007

Peter,
You're right that at present there are no dynamics that would choose the specific vacuum presently known. However, I think this is because we don't have the complete theory. If we can find a vacuum which correspond very closely to our universe, we may be able understand why this vacuum is selected, which may in turn lead to a complete formulation of the theory. In any case, it would be remarkable if we can find a model which provides a complete description of physics as we know it, and it would be even more remarkable if this model made predicitions that were later born out.

At the present, we don't know why the Standard Model gauge group is what it is. Afterall, a large number of gauge groups are possible and it has many free parameters. Does that stop particle physicists from using it? Absolutely not! However, some people like Peter Woit would claim that the Standard Model is 'contrived' and arbitrary. It may very well be, but that does not mean it's useless or that it doesn't help us get closer to the truth.

It will probably turn out that we will find a string theory model that completely describes our universe which may be regarded as an effective theory just as the standard model is an effective theory. However, it will be at a deeper level. Only later we will understand why it is chosen.

44. **Thomas Larsson**
May 2, 2007

In mentioning Witten's remark, did you mean to write "discovery of the positive cc"?

Oops.

45. **woit**
May 2, 2007

V.
I don't think the Standard Model is contrived, quite the opposite. I think all attempts to get it out of string theory are contrived.

46. **Nameless**
May 2, 2007

Anon, thanks to you too. I'll refrain from asking further questions… that's what review articles are for.

47. **V.**
May 2, 2007

Peter,
String theory models are no more contrived than the Standard Model itself. The standard model can only make predictions after fixing it's free parameters by hand. We have no reason for choosing SU(3)xSU(2)xU(1) in the SM except that it seems to work. We have no explanation for the charges in the SM except they seem to work. We have no explanation for the masses in the SM except they seem to work. Any string theory model will essentially do the same thing, except that it will provide a geometric explanation of all of the above AND unify the SM with gravity. Perhaps we will not be able to understand why this structure arises for some time, but it will still be useful, especially if it can make other predictions.

48. **Peter Shor**
May 2, 2007

There are 19 parameters in the standard model. Assuming each of these *on average* needs around 4 significant digits to get similar physics to ours gives one chance in 10 ^ (-84) we would have a universe looking fairly close to this one. This chance appears to be significantly better than you get with the string theory landscape.

Is this a good measure of the "contrivedness" of a theory? I don't think so, but I don't buy the argument that string theory has no free parameters, so it's less contrived.

49. **V**
May 2, 2007

I think you guys would be a lot better off if you'd stop worrying about the ability to uniquely determine the universe we live in. We're clearly not there, yet. The goal should be to effectively describe our world in a predictive way, regardless of the uniqueness question. Making aesthetic judgements is rather stupid at this point. If it works, that's what's important. We can worry about the 'why' question later.

50. **King Ray**
May 2, 2007

V, if you require anomaly cancellation and that the global gauge group of the SM is S(U(3)xU(2))=SU(3)xSU(2)xU(1)/Z_6, you get a unique solution for the hypercharges (and hence the electric charges), assuming anomalies cancel out within a family (the "Poor Man's Unified Field Theory"). The SU(2) and SU(3) reps of the quarks and leptons are inputs though.

51. **V**
May 2, 2007

King Ray,
Sorry, the Standard Model by itself cannot explain the charges, although it's true that they are constrained by anomaly cancellation. To explain the charge quantization, you have to go to a Grand Unified Theory such as SU(5).

52. **Peter Orland**
May 2, 2007

I think it's interesting that V. says aesthetic judgements are stupid. The justification of string theory by those leading the field was largely aesthetic, way back in the eighties. Now that so many people are totally committed to the strings, this justification seems to have been dropped.

53. **King Ray**
May 2, 2007

V, anomaly cancellation gives two different solutions, one the true solution and the other a bizarre solution. The bizarre solution is not consistent with the global group S(U(3)xU(2)), leaving a unique solution if you assume that amomalies cancel within each family, and that the global group is S(U(3)xU(2)).

http://prola.aps.org/abstract/PRD/v43/i8/p2709_1

54. **V**
May 2, 2007

Peter,
The justification for string theory has always been it's potential for unification of quantum mechanics and the standard model with gravity, rather than aesthetics. There are some hardcore hurdles that string theory has been able to hurdle. Like it or not, it is in fact the only known possible candidate for unification at the present time.

King Ray,
It sounds to me like what you're doing might be equivalent putting the standard model reps into representations of SU(5). Perhaps you've discovered grand unification without realizing it?

55. **King Ray**
May 2, 2007

V, S(U(3)xU(2)) indeed embeds into SU(5) in block diagonal form. However, S(U(3)xU(2)) does not have the extra gauge bosons (from the off block diagonal entries) that SU(5) does that lead to proton decay. It was because S(U(3)xU(2)) embedded into SU(5) so well that made SU(5) look so attractive initially. S(U(3)xU(2)) is a U(3)xU(2) theory with the U(3) and U(2) determinants related.

The charge quantization and ratio prediction comes from intra family anomaly cancellation and taking the global structure of the SM to be S(U(3)xU(2)), which identifies six elements of SU(3)xSU(2)xU(1).

There are 13 possible choices for the global gauge group of the standard model, and of them only S(U(3)xU(2)) is consistent with the quarks and leptons that we have observed in nature.

It is well accepted that the global group of the SM is S(U(3)xU(2)) and not SU(3)xSU(2)xU(1). See O'Raifeartaigh's book, for example.

56. **Peter Orland**
May 2, 2007

V.

Yes, we all know that that was the reason people were attracted to string theory. But it isn't clear that it is the only way to have quantized gravity. Between the energies we study experimentally and the Planck mass, there are many possibilities. Quantum gravity can be said to be a theoretical success over this entire (enormous!) range. What string theory does beyond this range is provide a cut-off at the Planck mass.

Now there are other ways to cut off gravity at 10^{-33}cm. A very crude example is some sort of Regge calculus. So I don't find the unification of gravity with quantum mechanics to be a very convincing argument for string theory. Any judgement as to how one wants to cut off the theory at the Planck mass is going to be an aesthetic one.

I am not against studying quantum gravity using string theory (I have even written a few string papers). I have no opinion as to the best way to cut off gravity in the ultraviolet. But it's one thing to study something, quite another to think there is no alternative.

57. **V**
May 2, 2007

Peter,
If there is another (serious) way to study quantum gravity, then it would be great and I'm sure people will pay attention. One thing that should be kept in mind regarding the debate about string theory, is that one of the driving forces in it's development over the last several years is a lack of experimental data. Because of this, there has been no other way to study these problems. I wonder what the development of theory over the last 15 years would have been like if the SSC had been constructed? Perhaps it would

Three String Theory Textbooks

May 2, 2007
Categories: Book Reviews

Until very recently, someone who wanted to begin studying string theory seriously had really only three possible textbooks available:

- Superstring Theory (1987), by Green, Schwarz and Witten. This is a two-volume, massive 1000 page treatment of the quantization of the superstring and ideas about Calabi-Yau compactifications dating from right after the First Superstring Revolution in 1984.
- String Theory (1998), by Polchinski. In two volumes and 900 pages this covers most of what is in Green-Schwarz-Witten, while also surveying D-branes, the second Superstring Revolution, and much of what was learned about string theory during the decade after GSW.
- A First Course in String Theory (2004), by Zwiebach. This is the textbook for an undergraduate course, so is at a lower level than the other two books.

Very recently three new string theory textbooks have appeared, each aimed at providing a textbook for an advanced one-year graduate course, assuming a background in quantum field theory and the standard model. Each of them is quite a bit shorter, while trying to cover much more than Polchinski and GSW. This is a daunting task. Polchinski in his introduction noted how difficult it was to cover even in 900 pages a literature of size around 10,000 papers. These new books are trying to cover a literature probably twice as large in sometimes half as much space. As a result all three of them necessarily often have a rather telegraphic feel, more that of a review article than the usual sort of introductory textbook.

I've spent some time reading through all three books over the last couple months, and here are some impressions. Just as these books are too short to really cover the subject, my comments here will be much too short to do justice to the 1800 pages or so of material in the books.

Michael Dine's Supersymmetry and String Theory actually probably shouldn't be thought of as a string theory textbook (and on page 310 the author notes "This is not a string theory textbook"). The first 300 pages have nothing to do with string theory, instead consisting of an introduction to the Standard Model, beyond Standard Model Physics (especially supersymmetry), and cosmology. The last 175 pages of the book give a very sketchy survey of string theory, concentrating on prospects for getting unification and particle physics out of it. Dine starts out with the standard promotional material for this idea, but does clearly explain the fundamental problems such as that of moduli stabilization that have led to the landscape and the ever-more-clear failure of this idea. He ends with a chapter about this and about the anthropic landscape. The main concern of most string theorists over the past 10 years, AdS/CFT duality, gets just two pages. For other reviews of the book, see one by Jacques Distler, and one by Lubos Motl (whose endorsement of the book's contents as "state-of-the-art picture of reality" appears on the book's cover). One peculiarity is that when he turns to general relativity and string theory, Dine switches his convention for the sign of the metric. Perhaps the book is best thought of as mostly an introduction to supersymmetry in particle physics, with the string theory material an outgrowth of that central topic.

String Theory in a Nutshell, by Elias Kiritsis, is one of what I guess Princeton University Press intends to be part of an "in a Nutshell" series, beginning with Tony Zee's Quantum Field Theory in a Nutshell. Zee's is a wonderful book, although it's best for someone who has already taken a QFT course and wants to get further insight into the theory, or read as a supplement to a more detailed text like Peskin and Schroeder. The Kiritsis book is not much longer than Zee's (they are both somewhat less than 600 pages), but is much more intended as a standalone textbook for a one-year string theory course, replacing Polchinski. It contains a wealth of exercises, nearly 500 of them (and the author warns that some are hard enough to have been the subject of research articles). While the book begins with the standard promotional pitch, Kiritsis does acknowledge that it may turn out that the subject is "an intellectual classical black hole". He pretty much completely ignores the moduli stabilization problem and the landscape. AdS/CFT gets a long chapter of about 70 pages, with 62 exercises. I don't know of any other reviews yet, but Lubos Motl's endorsement (which doesn't appear on the cover) can be found in Princeton University Press's promotional material for the book.

The most complete of the three books is String Theory and M-theory, by John Schwarz and the Becker sisters. It is more than 700 pages long and is intended as the textbook for a year-long graduate course, taking students from the basics of string theory to the latest ideas about flux compactifications and moduli stabilization. Trying to cover such a huge subject in this space means that it is done in much the "in a nutshell" style of Zee's QFT text. As a result many sections of the book have more the feel of a review article for a general audience than that of a textbook for students. The calculations leading to the landscape are covered in some detail, and there's a discussion of anthropic arguments and statistical calculations. Like Kiritsis, a 70 or so page discussion of gauge-string duality is provided. There's a review by Capitalist Imperialist Pig, and a short mention from Lubos Motl. No endorsement from Lubos on the book, instead it carries endorsements from the leading figures of the subject (Arkani-Hamed, Gross, Strominger, Vafa and Witten).

I found all three books quite interesting to spend some time going through, as they each in their own way provided an overview of the current state of string theory as a unified theory of particle physics. Of the three, Becker-Becker-Schwarz I think gives the most complete coverage of where the subject is at. Dine is a separate case, since it's mostly about other things. As you might guess I'm highly dubious of the idea of teaching this sort of material in a standard class for graduate students. The fundamental problem is that the very speculative idea that these books are devoted to, that you can unify particle physics using 10/11 dimensional string/M-theory together with compactification and branes in order to make the extra dimensions invisible, is one that has by now pretty clearly failed. Dine comes the closest to explaining how problematic the situation is, Kiritsis is at the other end, choosing to not explain the nature of the problems. These books attempt to cover a huge literature which consists of failed attempts to make some sort of connection with the real world, and I can't think of any other field of physics or mathematics where there are graduate-level textbooks that could be characterized in this way. Unfortunately, much of what has been successful about string theory is ignored in these books. Mirror symmetry, which has had a huge effect on mathematics, is not even mentioned by Dine, gets a couple pages in both Kiritsis and Becker-Becker-Schwarz. While ignoring string theory's mathematically most interesting insights, these books lead students into a horrendously complicated thicket of speculative ideas that generally don't work, but provide enough grist for decades of research projects to come. Any student who chooses to follow this path will need to devote many years to mastering this material, a one-year graduate course is not going to do the trick. There's no particular reason to believe that this kind of training is one that will lead to a solid background in techniques that are likely to have more success in the future.

Comments

1. **King Ray**
   May 2, 2007

   Hmmm… The latest edition of Ptolemy's Almagest is 693 pages. It seems that these overly baroque theories need longer texts to explain them. You would hope that the ultimate theory would be explainable in 10's of pages.

2. **baroque**
   May 2, 2007

   "You would hope that the ultimate theory would be explainable in 10's of pages."

   I would? Are you aware of any reasonable treatment of any nontrivial aspect of physics which can be explained coherently in 10 pages?

3. **Peter Woit**

May 2, 2007

baroque,

These books aren't like Peskin and Schroeder, explaining in detail how to do calculations that can successfully be compared to experiment. Mostly they're quite sketchy. Dine does the Standard Model in under 20 pages, and all of cosmology in 9 pages.

But, the problem with these string theory textbooks is not their length. Peskin and Schroeder is 800 pages long, but it explains in detail how to do a wide range of calculations that can be successfully compared to experiment. Nothing in any of the thousands of pages in these books allows you to compute anything that can be compared to experiment. The Almagest is definitely better on that score.

4. **King Ray**
   May 2, 2007

   baroque, if you can't explain your theory in 10's of pages, I think you're on the wrong track, as is becoming slowly apparent to almost all on the string theory front.

   Look at Einstein's The Meaning of Relativity, Landau and Lifschitz's Mechanics, etc. for some beautifully simple expositions.

   Beauty and Simplicity should be the key characteristics of the ultimate theory.

5. **Arun**
   May 2, 2007

   "The latest edition of Ptolemy's Almagest is 693 pages. It seems that these overly baroque theories need longer texts to explain them."

   De Revolutionibus is 330 pages as per Wikipedia.

6. **ali**
   May 2, 2007

   Hi peter,
   What do you think of the princeton university's latest press release?
   http://www.physorg.com/news97339219.html
   Thanks

7. **Peter Orland**
   May 2, 2007

   Some years ago, I remember that Princeton supposedly dropped
   their field-theory course and taught string theory instead. I presume this has since been recognized as a mistake, but I don't know for certain.

8. **marketing**
   May 2, 2007

   I'd argue differently. If anybody is considering writing a book on these topics, they better make sure it comes out in the market right now. Two years from now will most likely be too late to try and sell this stuff. Whatever the LHC might bring, the name of the game will be phenomenology.

9. **Peter Orland**
   May 2, 2007

   Ali,

   This is interesting stuff, but the article fails to mention that the
   gauge theory which is (probably) equivalent to the string theory
   is N=4 Yang-Mills. This is very different (even qualitatively) from QCD, the gauge theory of the strong interactions.

10. **King Ray**
    May 2, 2007

    I think this is the paper of Klebanov's in question:

    http://arxiv.org/abs/hep-th/0611135

    I think that all the overhyping and distorting of results for press attention is hurting string theory, since they can't deliver on their hype. I'm sure some interesting math is going on in AdS/CFT, but it may not be related to the real world.

11. **Aaron Bergman**
    May 2, 2007

    *Some years ago, I remember that Princeton supposedly dropped
    their field-theory course and taught string theory instead.*

    I doubt that. Both classes were certainly taught when I was there.

12. **Peter Orland**
    May 2, 2007

    Aaron,

    This was a long time ago. I heard about it around 1987

13. **Jack**
    May 2, 2007

    http://discovermagazine.com/2007/feb/cover

I'm not sure I understand this, but from what I'm reading it sounds like parts of string theory was experimentally verified/tested. Is this so?

14. **Peter Orland**
May 2, 2007

Jack,

That is not how RHIC experiments should be interpreted. What they study is high-energy nuclear collisions, with goal being a plasma of quarks and gluons. There is no test of fundamental string theory (of gravity and matter) here.

Here is what is going on. The formalism of string theory is used by some theorists to try to describe QCD, the theory of these quarks and gluons. The problem is that it really only describes what is called the strong-coupling approximation. Such approximations are not new – since 1974, there has been the lattice strong-coupling expansion, which is a non-computer-based calculational scheme. Such approximations gave us the first hints that QCD might really confine quarks.

Unfortunately the strong-coupling approximation is not necessarily a good approximation. The old-time lattice people abandoned it in favor of numerical methods. There are unphysical aspects of this sort of approximation (lattice artifacts, or unphysical particles from the string).

Now people are claiming that the stringy strong-coupling methods are a good way to study QCD, and to understand RHIC physics. But the real problem with any strong-coupling approach is that you can't renormalize the theory – that is remove the ultraviolet cut-off. For this reason, I think such approximations are highly questionable.

I go to a lot of talks on this stuff, and I always raise this issue. None of the speakers have addressed it, at least not to my satisfaction.

15. **J**
May 3, 2007

What do you think of the 3 early books? Any comments?

16. **valon**
May 3, 2007

Peter,

In your review of the String books you wrote:
" Mirror symmetry, which has had a huge effect on mathematics, is not even mentioned by Dine, gets a couple pages in both Kiritsis and Becker-Becker-Dine."
Did you perhaps mean BBS?

Regards.

17. **woit**
May 3, 2007

valon,

Fixed

J.,

I haven't looked at the earlier books closely recently, so don't want to say much. In general, I think they are much less sketchy, actually work through a lot more of the details of how you quantize a string, which is an interesting thing to do. From what I remember GSW also has much more details about Kahler geometry, which could be useful. But I should look at the books again before making more comments.

18. **anon.**
May 4, 2007

How about other books dealing with string theory in slightly a less spiritual, and more practical, way? For example, check out the *Red String Book: The Power of Protection (Technology for the Soul)* by Berg, http://www.amazon.com/Red-String-Book-Protection-Technology/dp/1571892486

If you'd take a more holistic approach to string, instead of blindly Popperian one you take, you'd see it differently. The people who complain that religions are wrong or 'not even wrong', ignore the *great success* of religion as a cultural, groupthink activity.

19. **David**
May 4, 2007

Anon.
I'm not sure if your post was tongue in cheek or not. I'll assume not. That said, I'm reminded of Francis Crick's remark that over the last 2000 years philosophy has been spectacularly unsuccessful at dealing with science. I'm also reminded of attending a philosophy of science conference in the late 1970's where the discussion was about the importance of betweenness in groups of billiard balls as important for the philosophical understanding of atoms & molecules. They seemed to have missed quantum theory. Thus, while Popper may have contributed something to science, I don't think any of us (at least at this blog) believe that the standard philosophical approach of ONLY thinking will lead to great advances. Feynman often said that no matter how beautiful the theory, if it doesn't agree with experiment it's wrong. The point of science is to understand nature. We can only tell how well we are doing by comparing our theories with what nature actually does. If anyone is more spiritual and less practical, it seems to me he/she is on the string side of the debate. That doesn't mean thinking is unimportant but at some point one must compare the thinking with nature.
If I misunderstood your post, sorry.
Best,
David

20. **theoreticalminimum**
May 8, 2007

I recently came across a few shared folders of ebooks on esnip, from which I found links to some of the books mentioned by Peter, from which those interested can download the books in djvu or pdf formats. I would like to merely extend those links to this post, with the hope that Peter won't be having too many misgivings about this seeming act of "free-education-resource-for-all" *débauche*. They are:
Superstring Theory (Green, Schwarz, Witten)
Volume 1 [*http://www.esnips.com/doc/db4a2a3a-79f4-4dfb-9101-839a297f0ca3/Green-M.,-Schwarz-J.,-Witten-E.-Superstring-theory,-vol-1*]
Volume 2
[*http://www.esnips.com/doc/f904e73d-fb9c-4e06-907c-94e1a565cf4f/Green-M.,-Schwarz-J.,-Witten-E.-Superstring-theory,-vol-2*]
String Theory Volume 1 [DJVU] , Volume 2 [DJVU] **(Polchinski)**,

A First Course in String Theory [DJVU] **(Zwiebach)**,
String Theory and M-Theory: A Modern Introduction [PDF] **(Becker, Becker, Schwarz)**.

Michael Dine provides lecture notes of a course he taught in 2002, which carries almost the same title (Beyond the Standard Model: Supersymmetry and String Theory) as his book. They might be worth checking out, whether or not one unfamiliar with the book would be thinking about significant overlaps with the book's content (maybe Peter would be in a position to inform us here).

Eh ben voilà quoi!

Princeton Physicists Connect String Theory With Established Physics

May 2, 2007
Categories: This Week's Hype

The latest press release hyping a string theory paper in a misleading way comes from my alma mater Princeton, which I find quite depressing. According to yesterday's press release, entitled Princeton physicists connect string theory with established physics:

String theory, simultaneously one of the most promising and controversial ideas in modern physics, may be more capable of helping probe the inner workings of subatomic particles than was previously thought, according to a team of Princeton University scientists.

The theory has been highly praised by some physicists for its potential to forge the long-sought link between gravity and the forces that dominate within the atomic nucleus. But the theory — which posits that all subatomic particles are actually tiny "strings" that vibrate in different ways — has also drawn criticism for being untestable in the laboratory, and perhaps impossible to connect with real-world phenomena.

However, the Princeton researchers have found new mathematical evidence that some of string theory's predictions mesh closely with those of a well-respected body of physics called "gauge theory," …

This has nothing to do with the controversial failed project of using string theory to provide a unified theory of particle physics and gravity. What it is about is another check of something not very controversial at all: the pretty much universally believed idea that a very special un-physical quantum field theory, N=4 supersymmetric Yang-Mills theory, at strong coupling can be described by a weakly-interacting string. This AdS/CFT correspondence is now almost ten years old and a significant amount of evidence for it has accumulated. What the press release is referring to is this paper by Igor Klebanov and collaborators, which studies numerically an integral equation derived in this paper.

The press release has already led to stories here and here, with presumably many more to come. Should make Slashdot any moment now….

Comments

1. **anon.**
   May 3, 2007

   Maldacena's unproved conjecture of a correspondence between 5-d AdS (anti de Sitter) space and 4-d stringy Yang-Mills conformal field theory (CFT) is one of the best things to come from string theory related research. I just love the fact that by the holographic principle, that 4-d particle physics resides as a brane or surface on a 5-d anti de Sitter space.

   On the negative side,

   *AdS has a negative cosmological constant, instead of a positive one,

   *N=4 Yang-Mills CFT isn't consistent with 10-d supersymmetry.

   Other conjectured unproved correspondences which can under limited conditions model real phenomena to some extent include:

   *by using phlogiston theory, you can model combustion without oxidation, which was very handy at one time,

   *by using caloric you can model heat without needing kinetic and radiation theories, again, a useful simplification at one time (Ca/KR correspondence)

   *by using Ptolemy's epicycles, you can actually model planetary motions in the solar system, which was simple for astrologers (Pt/SS correspondence)

   *by using the FitzGerald-Lorentz aether you can model the contraction of moving bodies without needing special relativity, which is oh so useful for crackpots (FL/SR correspondence)

   So the evidence of adS/CFT correspondence conjecture holding fairly well, implies that maybe people should start taking seriously other models that are similarly based on totally unphysical assumptions?

2. **John W.**
   May 3, 2007

   Something is better than nothing. With all this arbitrary mish-mash of ideas being thrown around maybe ideas may evolve to bring more meaning into the current fundamental physics discourse; until that time more raving and ranting…

   Oh, and by the way, Peter, you still haven't written anything substantial on Rep Theory and Particle Physics for the advanced undergrad. May we expect anything of that sort within the next several months?

   Regards

3. **F.**
   May 3, 2007

   Thank you for the exciting news. I think the numerical confirmation of Beisert's inspired guess how a gauge theory in the asymptotically free high-energy limit is smoothly connected to a string theoretical description at lower energy is a tremendous step forward, well worth some publicity.
   Cheers, F.

4. **ruleman**
   May 3, 2007

   F., nobody –I think– doubts that this is interesting stuff. And potentially highly useful. Well deserving of publicity.

   But when the main author says,

   > "We have previously been able to study these interactions in detail only at the high-energy conditions within particle accelerators, but with these findings we may be able to describe what's happening inside the atoms that make up rocks and trees. We cannot do so yet, but it appears that the math of string theory

could be what we need to bridge this gap."

he's clearly saying something untrue. Much is known about the strong interactions in the low energy regime, and there are a variety of tools that have been successfully used to describe them.

After all, if the above quote is true, then, how well do they fit the Roper resonance? Can they predict the masses of the meson and baryon decuplets? Or, more to the point, what is the ratio of the rho to proton masses in their theory?

I mean, can they really do what has never been done before, as they claim? Can they do better than lattice QCD, chiral effective models, QCD sum rules, as they claim? Bring on the numbers then, we are eager to check 'em.

5. **andy**
May 3, 2007

The press release isn't all hype. Consider the following:

This is not to say that string theory is likely to become accepted as an overall explanation of subatomic physics anytime soon. Klebanov's team has found a bridge between established physics and a mathematical theory, which is only one step toward solid experimental proof that the world is actually constructed of tiny vibrating strings. And even this bridge applies to only one facet of gauge theory. Bridging this gap for other facets will be necessary to enable physicists to understand fundamentally the interiors of the protons and neutrons that make up the earth beneath our feet.

I think that's fair enough. "Other facets" clearly refers to QCD.

6. **Jean-Paul**
May 3, 2007

F., could you please explain what do you mean as "asymptotically free high-energy limit"? I though that N=4 SYM was a finite theory…

7. **Thomas Love**
May 3, 2007

Peter wrote:

What the press release is referring to is this paper by Igor Klebanov and collaborators, which studies numerically an integral equation derived in this paper

Both of the links lead to the same paper

8. **Peter Woit**
May 3, 2007

Thomas,

Fixed.

9. **F.**
May 3, 2007

Jean-Paul,
you are right, of course. I am referring to:

"Beisert said that his team's work provided a useful abstract proof of the transition between weak and strong interaction strength, but that numerical evidence had been lacking.

"The result of Klebanov's group gives beautiful numerical evidence for the validity of our proposal," "

Cheers, F.

10. **Kasper Olsen**
May 4, 2007

Peter,

Actually, string theory already is a "unified theory of particle physics and gravity"; whether it is a correct description of reality is yet to be seen…

best, Kasper

11. **Herbert**
May 4, 2007

Other folks will also find it depressing that Princeton is Peter Woit's alma mater. But the press release is exciting and Dr (!) Woit should be praised for promoting it.

12. **gunpowder&noodles**
May 5, 2007

Lubos Motl also objects to this story. On the grounds that somebody used the word "controversial" in connection with string theory.
By the way, LM has recently written a screed on the 2nd law of thermodynamics that is decidedly non-mainstream; borderline crackpot in fact. I mention this *not* to start another anti-LM tirade, *nor* to initiate a discussion on the 2nd law, but as a warning to those who, like me, previously thought that he always knows what he is talking about on technical issues of physics. Evidently he doesn't. Oh well. We'll just have to read his technical writings with a few more grains of salt.

13. **Web King**
May 5, 2007

Couple of week back I read the review of the Director's Cut version of String Kings. It seems that the Director's Cut version includes more scenes featuring a certain "man on the edge".

14. **John W.**
May 5, 2007

Peter,

[Somewhat off-topic, but I thought you may be one of only a handful of people to give an honest + critical feedback.]

What do you think is specifically important about the following works? (I'm assuming you may have perused through one of them in your academic studies; I apologize if you haven't):

"Topology, Geometry and Quantum Field Theory: Proceedings of the 2002 Oxford Symposium in Honour of the 60th Birthday of Graeme Segal"

and

"Geometry, Topology and Quantum Field Theory (Fundamental Theories of Physics)"
– P. Bandyopadhyay

Thanks.
John W.

15. **anon.**
May 5, 2007

'Actually, string theory already is a "unified theory of particle physics and gravity"; whether it is a correct description of reality is yet to be seen…' – Kasper Olsen

It's not even a unification of *particle physics* and *gravity*. It's merely a unification of the (ill-founded in view of the landscape) speculation that string can model particle physics, with the speculation that by M-theory you can include gravitons. To call string theory a unification of particle physics and gravity is misleading hype. Maybe instead you should call it a unification of unphysical speculations about particles and gravity.

Saying that string *is* does unify particle physics and gravity, but then afterwards adding that it has not yet been shown to be the correct unification, is the cause of all the confusing hype problems. You need to be completely clear and call string a speculative unification scheme.

16. **V.**
May 5, 2007

Dear Anon,
String theory does in fact unify particle physics and gravity into a consistent structure. This is not speculation. You are right that we cannot say at the present why the specific vacuum which corresponds to our universe is chosen. However, I think the defiinition of particle physics is more broad than just the Standard Model. Do you have any reasons within the bounds of quantum field theory for believing that the structure of the Standard Model is unique and no other vacuum structure is possible?

17. **Kyrie**
May 5, 2007

> You are right that we cannot say at the present why the specific vacuum which corresponds to our universe is chosen.

And what, exactly, it is that you *can* say at the present time? Does string theory predict susy or not? Does it predict extra dimensions or not? Does it predict light scalars or not? And what are the corresponding mass scales?

18. **X.**
May 5, 2007

gunpowder & noodles wrote:
"By the way, LM has recently written a screed on the 2nd law of thermodynamics that is decidedly non-mainstream; borderline crackpot in fact."

LM's opinions on thermodynamics are about as accurate as his ones on computers, numerical analysis, and climate science. BS.

19. **V.**
May 5, 2007

Kyrie,
What we can say is that string theory provides a consistent unification of quantum mechanics and gravity. Supersymmetry and extra dimensions are generic predictions. The physical properties of the universe that we live in corresponds to a specific solution. In order to make predictions, this specific solution must be found first. When and if this solution is found, we may be able to say why this solution is selected. However, even in the absence of such a selection principle, this solution would allow us to completely describe the physics of the universe we live in. So, you should give string theorist/phenomenologist time to discover this solution. Then, and only then, will we be able to make specific predictions about our universe.

20. **kyrie**
May 5, 2007

V., being on the right track to a consistent unification of quantum mechanics and GR is what most people would call an important research program. It is reasonable to expect that, if successful, such program will have a lot to say about particle physics — and gravity.

But saying that *string theory does in fact unify particle physics and gravity* is very different. It implies, among other things, that particle physics can be derived from ST. And also that there are concrete predictions for future experiments. I think it's still quite far from that. There are interesting generic predictions from ST, as you say, that have actually motivated a lot of model building. But no specific ones, as far as I know, not even about orders of magnitude.

And then, the possibility cannot (so far) be logically excluded that even if particle physics and gravity actually are unified in nature, that unification is not ST.

21. **AnOn**
May 5, 2007

Kyrie,

I think that you're missing the point. All V is saying is that string theory is a quantum theory of gravity which can incorporate gauge theories, etc. V is not arguing that it necessarily has anything to do with our universe (though, of course, it might) just that it successfully merges gravity and particle physics. Some people think that this fact alone alone is impressive. I'm not aware of any other research programme which can make this claim. The only thing which comes close is LQG which, of course, doesn't predict anything about particle physics either.

22. **matteoeo**
May 5, 2007

string theory does in fact unify particle physics and gravity

I think it might at most unify particles and the graviton, which is different from unifying particles and gravity and very far from unifying particle physics to gravitation.

23. **V.**
May 5, 2007

I think there should be define what we mean when we are talking about particle physics. Are we talking about all possible gauge theories in general or just the Standard Model gauge theory which describes our universe?

24. **Yatima**
May 5, 2007

A not-quite-serious Saturday Evening post:

http://www.angryflower.com/dating.gif

25. **kyrie**
May 5, 2007

I agree, semantics seems to be a problem here. By particle physics I understand what is measured in particle accelerator (and cosmic rays) experiments. And which is, so far, perfectly well described by the SM (minus some technicalities).

26. **Peter Woit**
May 6, 2007

John W.

I know nothing about the second reference, but the first is the proceedings of a conference I attended honoring Graeme Segal on his 60th birthday. It contains his notes on conformal field theory, which is a very interesting document. Most of it is research-level articles, as usual a mixed bag. Some very interesting, some not. But definitely a document that is more aimed at experts, not so much an expository book.

And it doesn't look like I'll have time in the near future to write up anything at an undergraduate level. At the moment, still trying to understand relations between Langlands stuff, representation theory and QFT, as well as continuing to work on BRST. Next year I'll be teaching our graduate course on representation theory for the full year, hope to find time to generate a better set of notes, covering a wider part of the subject.

27. **John W.**
May 7, 2007

Thanks Peter.

Regards.
John W.

All Sorts of Stuff

May 5, 2007
Categories: Uncategorized

For up-to-the-minute news about the Higgs, far better informed than any media source could ever be (and thus a great example of why blogs are changing the way the media works), your best bet is Tommaso Dorigo's blog. His latest posting explains well what the current state is, and predicts that, with the data expected from the Tevatron through 2009, they should be able to have 2.5-3 sigma evidence for a 115 GeV Higgs if it is there, or if it's not, rule it out at 95% confidence level up to 130 GeV. He shows a recent plot from D0 based on 1 fb^{-1} of data, and discusses the fact that D0's limits on a Higgs are not quite as good as expected at low mass. When similar data from his own experiment (CDF) becomes available, it will be interesting to compare the results. Not being able to rule out a low-mass Higgs at the expected level probably just means that it's harder to do than expected. But there's another possible interpretation: maybe there's something there….

Tommaso also has a posting about a new Physics World article discussing the recent blog-centered discussion of statistically-not-very-significant sightings of a possible new particle that could be a supersymmetric Higgs. Evidently these events have caused some consternation within CDF and D0 about the possible implications of bloggers in their midst and how this changes communication of their results to the public.

This month's Blog Life column in Physics World covers Not Even Wrong, accurately and well.

On the mathematical side of things, Terry Tao continues to come up with amazingly good blog entries. His latest is a series of three postings (here, here and here), reporting on my colleague Shouwu Zhang's lectures at UCLA on the topic of rational points on curves. This is a fundamental issue in number theory and arithmetic geometry, and the fact that Tao is a great mathematician, but not an expert, may have a lot to do with why his explanation of Shouwu's lectures is relatively easy to follow. One of the problems with academia is that one's illustrious colleagues (like Shouwu) get invitations to give lecture series like this elsewhere, but not at their home institutions. So, while I didn't get to hear Shouwu's lectures, Tao's account of them is excellent compensation.

For an interesting article by a young philosopher about the question of beauty in physics, see this article in Perspectives in Science (based on his doctoral dissertation).

On May 22 the CUNY Graduate Center program on Science and the Arts will host an event entitled String Theory for Dummies. Unfortunately I'll be out of town that day…

A couple weeks ago there was a workshop in Tel Aviv and Jerusalem on String Theory: Achievements and Perspectives, honoring the sixtieth birthday of Eliezer Rabinovici and Shimon Yankielowicz. Videos and some transparencies from the talks are available here. Susskind gives his usual propaganda for the anthropic string landscape, but seems rather defensive, starting off saying that he "feels like he's at the center of a circular firing squad" (which maybe does describe what is going on in string theory these days), and that "some people say I'm a traitor" or that "my ideas are dangerous."

Gross ended the conference with a remarkable discussion of the current state of string theory. He put up various cartoons illustrating the fact that the public perception of string theory has turned rather negative (including the recent one from the New Yorker: "Is String Theory Bullshit?"), but took solace in a recent use of string theory in an advertisement for women's bikinis. He declared that "I am still a true believer in the sexiness of string theory", and that he continued to think it is clearly on the right road. But, after giving the standard list of string theory achievements, he did admit that he was much less optimistic than 20 years ago, and spent some time discussing what he sees as the main failure to date: the continuing lack of a fundamental dynamical principle behind string theory. The question "what is string theory?" still has no real answer, and he has "the very uneasy feeling that we're missing something big, that semi-classical intuition fails", and that this will make the landscape disappear. Perhaps most remarkably, Gross admitted to some discouragement about AdS/CFT. He noted that the recent Klebanov et. al. results promoted by press release as connecting string theory with physics were actually due to an impressive gauge theory calculation. According to him, what has happened is that gauge theory techniques have proved more powerful than string theory techniques. He went on to discuss the landscape, explaining that he found the anthropic principle impossible to falsify, completely against the way physics has made progress in the past, and just "an easy way out". Gross ended his talk by pointing out that 90 percent of the conference talks used supersymmetry, and that currently there was a "really weird situation": supersymmetry was an essential tool, but there was absolutely no evidence for it. He said that he continues to believe that supersymmetry will be found at the LHC and has been willing to take 50/50 bets on the subject for bottles of wine, etc.

I haven't yet had time to listen to many of the other talks, it looks like there are quite a few worth listening to, although as usual recently a depressingly large amount of landscape-based rather philosophical and pseudo-scientific argumentation.

I spent Thursday out at Stony Brook at the celebration of the 40th birthday of the ITP. It was great to catch up with many people I haven't seen in nearly twenty years, hear what a lot of ex-Stony Brook people are doing, and meet some interesting new people (including some blog readers!).

Yesterday I spent much of the day downtown at the headquarters of the New York Academy of Sciences, which was hosting this semester's Northeast String Cosmology Meeting, organized by Brian Greene and others from Columbia. The setting was pretty amazing, up on the 40th floor of the new 7 World Trade Center building, which has a spectacular view of lower Manhattan. Richard Bond gave a talk on topics concerning inflation and the CMB. He ended with lots of detailed calculations of CMB effects due to cosmological models involving string theory compactifications, especially a "Roulette Inflation" model. The joke was that God does not just play dice with the universe, but roulette also. In the question period Neil Turok politely pointed out that he was randomly choosing initial conditions, and getting very different imprints on the CMB, so wasn't really able to predict anything. Nima Arkani-Hamed spoke on "Quantum Horizons and the Landscape", talking about very general philosophical issues of horizons in AdS, the landscape, whether there are any "sharp observables" in this context and associated limits on the applicability of effective field theory. He ended by claiming that the situation is like that of the quantum theory in 1911, with the angst people are experiencing due to the landscape just like the difficulties physicists faced early in the century in going from classical physics to quantum physics. He didn't mention that the old quantum theory was making lots of verified experimental predictions, whereas he is giving talks on whether, even in principle, the landscape can predict anything. Seems kind of different to me.

Among the many people there was Alan Guth, who, according to this blog entry someone pointed me to, has started "to have been converted over to thinking that anthropic arguments might have some merit."

While I found these two talks depressing and all too symptomatic of the sad state of this subject, there was a huge bright spot at the workshop. Witten gave a really amazing talk about 2+1 d gravity. He has some fascinating new ideas about this, but they deserve a completely separate posting, which I'll try to get to writing up tomorrow…

Comments

1. **island**
   May 5, 2007

   Leonard Susskind said:
   some people say I'm a traitor" or that "my ideas are dangerous."

   If he's talking about the fact that he wants to abandon the scientific method without a tested and proven theory, then yeah, he's dangerous.

Or the fact that he wants to "conditionally" abuse strong interpretations of the anthropic physics in order to blackmail others, maybe?

> If, for some unforeseen reason, the landscape turns out to be inconsistent – maybe for mathematical reasons, or because it disagrees with observation – I am pretty sure that physicists will go on searching for natural explanations of the world. But I have to say that if that happens, as things stand now we will be in a very awkward position. Without any explanation of nature's fine-tunings we will be hard pressed to answer the ID critics.

Anybody want to take bets on how fast Lenny retreats and becomes a traitor to his strong anthropic interpretation without the landscape, if the landscape does fail?

2. **Kea**
May 5, 2007

willing to take 50/50 bets on the subject for bottles of wine

Oh, goody! I'll take him on. There are some great pinot noirs from Central Otago that I don't think he'd turn his nose down at (although he won't be winning the bet).

3. **Aaron Bergman**
May 5, 2007

They certainly made it a challenge to watch the talks.

4. **M**
May 6, 2007

The complain that I hear is: "life was better before that Susskind promoted the anthropic landscape, he made a disaster".

This reinforces my opinion that many string theorists abandoned physics for something else. The physics of string theory is anthropic? Then better to address it.

5. **usuck**
May 6, 2007

even u can appreciate Witten's genius. You look so 60s', update your outlook man.

6. **Jeremy**
May 6, 2007

I'm a bit sauced off that The New Yorker contained a cartoon like that. I wish I could find it.

7. **Peter Woit**
May 6, 2007

Jeremy,

Here it is:

http://www.cartoonbank.com/item/123525

Yes, maybe you were ripped off…

8. **Jeremy**
May 7, 2007

Oh, probably not. That thought's not SO uncommon, is it? Plus actually looking at it, the most I could surmise is that if by the unlikely chance the writer saw our stupid little ad, it was merely an inspiration.

Witten on 2+1 Dimensional Gravity

May 6, 2007
Categories: Favorite Old Posts, Uncategorized

The high point of Friday's string cosmology workshop here in New York was Witten's lecture on his new ideas about 2+1 dimensional quantum gravity. I'll try and reproduce here what I understood from the lecture, but this (2+1 d quantum gravity) is not a subject I've ever followed closely, so my understanding of the topic is very limited. It does seem clear to me though that Witten has come up with a striking new idea about this subject, linking together some very beautiful mathematics and physics. He has yet to write a paper on the subject, but presumably there will be one appearing relatively soon. I also suspect this is what he'll be talking about at Strings 2007.

Witten began by stating his motivation: to study fully quantum black holes in an exactly solvable toy model. There's no exactly solvable model in 3+1d, and 1+1d is too simple, so that leaves 2+1d. Assuming 2+1d, for positive cosmological constant Λ he is suspicious that the theory is non-perturbatively unstable and one can't get precise observables, for Λ=0 one doesn't have black holes, so that leaves negative Λ, here the vacuum solution is anti-deSitter space, AdS_3.

Quantum gravity in AdS_3 is related to 2d conformal field theory. There have been studies of AdS_3/CFT_2 as a lower dimensional version of string/gauge duality, but here he uses not string theory on AdS_3, but a quantum field theory. In a question afterwards, someone asked about string theory, and Witten just noted that perhaps what he had to say could be embedded in string theory, and that the recent Green et. al. paper showing that one can't get pure supergravity by taking a limit of string theory did not apply in 3d. If one wants to interpret this new work in light of the the LQG/string theory wars, it's worth noting that the technique used here, reexpressing gravity in terms of gauge theory variables and hoping to quantize in these variables instead of using strings, is one of the central ideas in the LQG program for quantizing 3+1d gravity. Witten was careful to point out though that there was no 3+1d analog of what he was doing, claiming that one can't covariantly express gravity in terms of gauge theory in 3+1d (he said that LQG does this non-covariantly).

For negative Λ the theory has so-called BTZ black hole solutions, discovered by Banados, Teitelboim and Zanelli back in 1992, and it is for the quantum theory of these black holes that Witten is trying to find an exact solution. The technique he uses is one that goes back to the 80s, that of re-expressing the theory in terms of SO(2,1) (or its double cover SL(2,**R**)) gauge theory, where the action becomes the Chern-Simons action. More precisely, the Einstein-Hilbert action

$$I_{EH}=\frac{1}{16\pi G}\int d^3x\sqrt{g}(R +2/l^2)$$

(here the cosmological constant is $\Lambda=-1/l^2$) gets rewritten as an SO(2,2)=SO(2,1)XSO(2,1) gauge theory with connection

$$A= \begin{pmatrix}\omega & e \\ -e & 0 \end{pmatrix}$$

where &\omega; is a 3X3 matrix (the spin-connection), e is the 3d vielbein, and the gauge theory action is the Chern-Simons action

$$I=\frac{k^\prime}{4\pi}\int Tr(A\wedge dA+\frac{2}{3}A\wedge A\wedge A)$$

with $k^\prime=\frac{l}{4G}$ (that 4 may not be quite right…).

Witten wants to exploit the relation between this kind of topological QFT and 2d conformal field theory that he first investigated in several contexts (including one that won him a Fields medal) back in the late eighties. He notes that in this context the existence of left and right Virasoro symmetries with central charges $c_L=c_R=\frac{3l}{2G}$ was first discovered by Brown and Henneaux back in 1986, and he refers to this discovery as the first evidence of an AdS/CFT correspondence. If one really does have a CFT description, one expects that the central charges can't vary continuously, but that 2+1d gravity will only make sense for certain values of l/G, but Witten notes that there is no rigorous way to find the right values one will get upon quantization.

He then goes on to make a "guess", adding to the action a multiple of the Chern-Simons invariant of the spin connection

$$I^\prime=\frac{k}{4\pi}\int Tr(\omega\wedge d\omega + \frac{2}{3}\omega\wedge\omega\wedge\omega)$$

Now the theory depends on two parameters: l/G and an integer k.

Using the fact that SO(2,2)=SO(2,1)XSO(2,1), one can rewrite the total action as the sum of two Chern-Simons terms

$$I= \frac{k_L}{4\pi} \int Tr(A_-\wedge dA_-+\frac{2}{3}A_-\wedge A_-\wedge A_-)$$
$$ \\ + \frac{k_R}{4\pi}\int Tr(A_+\wedge dA_++\frac{2}{3}A_+\wedge A_+\wedge A_+)$$

for connections

$$A_{\pm}=\omega\pm e$$

Now instead of l/G and k we have k_L,k_R and these are quantized if we take the gauge theory seriously. By matching Chern-Simons and gravity the central charges turn out to be

$$ (c_L, c_R)= (24k_L, 24k_R)$$

and holomorphic factorization is possible in the 2d CFT for just these values

Looking at just the holomorphic part, we have a holomorphic CFT with central charge c=24k and ground state energy -c/24=-k (note, now a different k than before…).

The partition function is expected to be ($q=e^{-\beta}$)
$$Z(q)=q^{-k}\Pi_{n=2}^\infty \frac{1}{1-q^n}$$
The first term in the product is the ground state (AdS_3), the only primary state, with the other terms Virasoro descendants (excitations of the vacuum from acting with the stress-energy tensor and derivatives).

Witten then goes on to note that this expression is not modular invariant, so one expects other terms in the product, corresponding to other primary states. By an argument I didn't understand he claimed that these would be of order q^{1}, at an energy k+1 above the ground state, and his proposal was that it would be this modular invariant function that would include black hole states.

In these units the minimum black hole mass is M=k, but here one is getting states only at mass M=k+1 and above. This is because the Bekenstein-Hawking entropy of the M=k black hole is 0, so it doesn't contribute to the partition function.

Witten claimed that this proposal gives degeneracies of states that agree with the Bekenstein-Hawking entropy formula. As an example, for k=1 the partition function is given by the famous J-function

$$J(q)=j(q)-744=q^{-1}+196884q+\ldots$$

and thus for a black hole of mass 2 the number of primaries is 196883 and the entropy is ln(196883)=12.19, which can be compared to the Bekenstein-Hawking semi-classical prediction of 12.57 (one only expects agreement for large k,M).

The number 196883 is famous as the lowest dimension of an irreducible representation of the monster group, and this partition function is famous as having coefficients that give the dimensions of the other irreducibles ("modular moonshine"). There is a conjecture that there is a unique CFT with this partition function. If so, it must be the CFT that has the monster group as automorphism group. It has always seemed odd that this very special CFT didn't correspond to a particularly special physical system, but if Witten is right, now it has an interpretation in terms of the quantum theory of black holes in 2+1 dimensions.

Anyway, that's what I was able to understand of what Witten had to say and what he was claiming. Other people have worked on this problem in the past, for a recent review article on this topic by Carlip, see here. Carlip describes the understanding of the problem at the time as "highly incomplete", and one of the explanations he describes relates the black hole problem to the Liouville theory. A question from the audience after the talk asked about this, and Witten indicated that he thought the Liouville theory explanation did not work.

I'm no expert here, so unclear on the details, why some of these things might be true, and what the implications might be, but this does seem to be a remarkable new idea, involves beautiful mathematics, and seems to provide promising insight into a crucial lower dimensional toy model. I suspect it will draw a lot of attention from theorists in the future.

For this posting, I especially encourage any comments from people more knowledgable than myself who can correct anything I've got wrong. I also strongly discourage people who know little about this from contributing comments that will add noise and incorrect information. Bad enough that I'm trying to provide information about something I'm not expert on; if you can help that's great, but if not, please don't make it worse…

Update: Lubos has picked up on this, which he describes as having been "leaked", and gives the usual argument that this must be part of string theory.

Comments

1. **Coin**
   May 6, 2007

   Assuming 2+1d, for positive cosmological constant Λ he is suspicious that the theory is non-perturbatively unstable and one can't get precise observables, ***for Λ=0 one doesn't have black holes****, so that leaves negative Λ, here the vacuum solution is anti-deSitter space, AdS3.*

   Stupid question: Why not?

   What happens if you just take a certain amount of mass and drop it into an area of zero-cosmological-constant 2+1d space less than that mass's Schwarzschild radius, or whatever?

2. **Peter Woit**
   May 7, 2007

   I just deleted almost all the comments on this posting, since, besides one helpful comment pointing out a typo, they were doing what I specifically requested people not to do. Please try and resist the temptation to add to the noise level here. If you have an informative and substantive comment, please post it, but the level of not-worth-reading comments followed by people pointlessly bickering about them has gotten really annoying. This creates an environment I wouldn't want to participate in myself.

   Coin,

   I don't know what the physical interpretation is, but I assume the point is that the BTZ solutions don't exist for zero CC. Maybe someone more familiar than me with this has insight into why this is.

3. **anon**
   May 8, 2007

   2+1 is special: For lambda = 0, Ricci = 0 implies Riemann = 0; there's no Birkhoff theroem in 2+1. So, a vaccum equation in 2+1 is just flat space. No curvature, so no black holes.

   More intuitively, in 3+1 one can consider the lower bound of a tottally collapsed gravitational object (Schwarzschild radius = 0) as a critical point between wave dispersion and gravitational / self attraction. Consider a radially ingoing gaussian profile of energy; say some field. For small amplitude the dispersion will exceed the self attraction and the solution puts the field on the boundary at future infinity. With increasing amplitude, eventually one finds the critical point and black holes thereafter.

   In 2+1, for Lambda >=0 there is not the same mechanism of self attraction; so no black holes. It's all dispersive instead.

4. **Coin**
   May 8, 2007

   woit/anon: Thanks!

   Anon, I think that actually makes sense, but I do have one question. What happens if, in flat (i.e. zero cosmological constant) 2+1 space, you accumulate mass into an object so dense that its escape velocity is greater than the speed of light? Would it simply not be for some reason possible to construct such an object in flat 2+1 space? Or would it be possible for such an object to exist, but the object would not be or behave like a black hole (i.e. no singularity forms)? From what you're saying it sounds like the latter would be the case.

5. **anon**
   May 8, 2007

   Well, you're thinking very intuitively about the situation, which is fine.

   However, there's an important distinction between vacuum and matter solutions and an important relationship as well. In 3+1 there's a good understanding that collapse of a matter solution will produce a singularity and subsequently an asymptotic final state that is a pure vacuum solution (all matter confined asymptotically to the singularity and so no matter elsewhere; e.g., a pure vacuum solution). The Oppenheimer Snyder analysis points the way / motivates the passage from a matter solution towards a vacuum solution final state, which logically (but not historically) is a primary impetus for looking closely at the 3+1 vacuum space of solutions. Hence consideration of Ricci = 0 solns (vacuum by defn of the Einstein equation). Einstein's original motivation for looking at Ricci = 0 was different, perhaps explaining why it took the prescient work of Schwarzschild and then Oppenheimer and Snyder after that.

Thus, the motivation for immediately jumping to Ricci = 0 solutions assumes the given of tottal gravitational collapse (which in turn presumes ideas like escape velocity).

Interrogating 2+1 for Ricci = 0 finds a suprise: All vacuum Ricci = 0 are flat.

Hence no total gravitational collapse; hence no analogous escape velocity.

2+1 is different and has special arguments.

HIH

6. **rob**
May 8, 2007

Coin,

The idea of a black hole as an object near which the escape velocity is greater than one is a purely newtonian idea. Laplace famously wrote a paper about it, and someone else before him, but that's another story. The precise definition of a black hole in general relativity is considerably more complicated than that.

But anyway, let's think about black holes in 2+1d newtonian gravity. Whereas in 3 spatial dimensions the gravitational force from a point particle goes as the inverse of the square of the distance, in 2 spatial dimensions it just goes as just the inverse of the distance. This is just like the electric field of a line charge.

Anyway, if the force goes as one over the distance, then the binding energy goes as the log of the distance. So it would take an *infinite* amount of energy for a test particle to escape to infinity in 2+1d. Escape velocity isn't a meaningful concept in 2+1d newtonian gravity, because there just isn't any escape.

7. **John Baez**
May 9, 2007

anon writes:

> 2+1 is special: For lambda = 0, Ricci = 0 implies Riemann = 0; there's no Birkhoff theroem in 2+1. So, a vacuum equation in 2+1 is just flat space.

Gotta be a bit careful here. Everything you say is true, but you can have metrics that are vacuum solutions of (2+1)-dimensional general relativity everywhere *except* for a singularity. For example, there are solutions where space looks like a cone. A cone is flat except at the tip, where the curvature is infinite. These solutions act like point particles – not black holes.

The cool part is that there's an *upper bound on the mass of the particle* in these solutions. More precisely, and even more cool, the mass of a particle is only defined modulo the Planck mass! It makes no sense to talk about a particle with mass greater than the Planck mass… past this point, the mass just 'wraps around': a particle like this is the same as a very light particle.

By the way, this stuff is true classically: no quantum gravity required! The reason is that in 2+1 dimensions one can define the "Planck mass" – a quantity with units of mass – using just Newton's constant G and the speed of light c. One doesn't need Planck's constant!

All this is much more sensible than it sounds at first sight. For more details, try week232. For even more details, try my paper with Derek Wise and Alissa Crans.

But anyway, yeah: there are no black holes in (2+1)-dimensional gravity when the cosmological constant vanishes… or is positive. One doesn't need to understand quantum gravity to see this: it's just GR.

It's nice to see equations on your blog, Peter! Good, sound, serious stuff – not just people fighting!

8. **anon**
May 9, 2007

Very cool!
Thanks John

9. **John Baez**
May 10, 2007

coin writes:

> But anyway, let's think about black holes in 2+1d newtonian gravity.

This is interesting to do, but general relativity in 2+1 dimensions is funny.

Unlike the more familiar (3+1)-dimensional case, it *does not reduce to Newtonian gravity* in the limit of small masses moving much slower than the speed of light!

For example, it's pretty easy to see that in 2+1 dimensions, general relativity does not allow one small mass to orbit another in a circular orbit. Newtonian gravity does.

So, in 2+1 dimensions, 'Newtonian black holes' are a poor guide to the behavior of general-relativistic black holes — much worse than in 3+1 dimensions, where John Michell successfully computed what's now called the 'Schwarzschild radius' way back in 1784, using Newtonian gravity! (His calculation was repeated by Laplace around 1795.)

All this makes (2+1)-dimensional general relativity a rather odd subject to use as a warmup for real physics… but it's very pretty. Most researchers in quantum gravity have not been able to resist its charms — there are lots of papers about it.

10. **amanda**
May 10, 2007

"All this makes (2+1)-dimensional general relativity a rather odd subject to use as a warmup for real physics…"

Then why is Witten's work interesting? I'm not being sarcastic — I genuinely want to know what we might learn if people push on with this thing.

11. **Kochemasov Gennady,RFNC VNIIEF,Sarov**

May 10, 2007

A.V.Pushkin (1947-2004) from Russian Federal Nuclear Centre –VNIIEF (Sarov) used in nineties The Monster Group as hardware for calculations of important dimensionless physical constants, and not only them. Very succinct exposition of his approach you could find in his presentation on the Second Int. Sakharov Conf., Moscow, 1996 (Proc. of this Conf. were published by World Scientific Publ.,1997).Here I place the annotation of this work.
"MONSTROUS MOONSHINE" AND PHYSICS
A.V.Pushkin
The paper presents some results obtained by the author on the quantum gravity theory. This theory proves related to geometry of Cayley projective plane and the algebraic structure of the theory to the commutative nonassociative Griess algebra. The theory symmetry group is the automorphism group of Griess algebra: "Monster" simple finite group. Knowledge of the theory symmetry allows observed physical quantities to be computed in the "zeroth" approximation. Results of the calculations, including those for fine structure constant ~ 1/137 and proton to neutron mass ratio, are presented, with the theory-controlled accuracy of the "zeroth" approximation being higher for some of them by 1–1.5 orders of magnitude than the accuracy of modern measurements.
If it is interesting to somebody I can send full text and additional information.
Gennady Kochemasov, RFNC VNIIEF.
My home e-mail: ggk44@mail.ru..

12. **Peter Woit**
May 10, 2007

amanda,

In terms of physics, 2+1 gravity isn't what one ultimately wants to know about (although, for mathematics, Witten's claims may make it turn out to be very interesting). It's a toy model. If one needs to develop new ideas and new techniques for solving a problem (which is what I think quantum gravity needs), then most of the time you have to first start with simpler cases, with toy models, in order to develop these new ideas and techniques. Witten seems to have found a new sort of relationship between a fully quantum gravity theory, gauge theory, and a simple conformal field theory. Yes, it's in 2+1d, not 3+1d, but it's a new idea, and until one completely understands how it works and what one can do with it, whether or not it will help with the 3+1d problem is unclear. I think Witten would also argue that sorting out the conceptual problems of a fully quantum, exactly solvable, theory of 2+1d black holes may give insight into the more difficult conceptual problems in 3+1d, where there seems to be no hope of an exact solution.

About the work of Pushkin: this seems to have nothing to do with the topic of this posting, Witten's recent work, but I'll leave the comment, suggesting that people who want to discuss that contact the author.

13. **Kochemasov Gennady,RFNC VNIIEF,Sarov**
May 10, 2007

To Peter Woit. I think that your comment about the work of Pushkin and the topic of this posting is not exactly correct, The reason very simple: Pushkin really thought that he solved the problem of quantum gravity and had strong support for such conclusion.If this true(I am not expert in QG, but I had many talks with Sasha), then this posting had projection on wright direction.And it is good news. For explaining my point of view I place here one more citation from Pushkin book(in Russian):"In the superstring theory a striking progress has been achieved recently: as few as five theories remain which are actually based on two lattice types in 16-dimensional space. Moreover, it has been proved that they are non-perturbative equivalents and as such are the limiting cases of the more general single theory. What last step should be made in the superstring theory in the direction of the construction of a single theory? Of course, this step is the gravity quantization, which will result in disappearance of the last dimensional scale in the theory, viz. the fundamental string scale. Then the theory will possess the local scale invariance properties and may perform quantitative calculations with non-perturbative methods by translating algebraic relation between physical quantities to different scale levels, including to the level of ordinary phenomena appearing in continuum motions in standard conditions." Here I pass long but important part of text and proceed:"In geometrodynamics, i.e. upon the gravitation quantization, even this last dimensional scale disappears. In the language of the lattice theory in spaces this means that there should be the only possibility to avoid the need of appearance of dimensional scale: the fundamental root amount is equal to infinity (i.e. there is no separated cell) and the Weyl vector is therewith light-like, which also requires no a priori scale. It is evident that the last requirement is readily formalizable at the level of the elementary problem of Diophantine equation solutions . Having solved it, you will find those values of dimension of hyperbolic spaces , which determine the lattice arrangement of the geometrodynamics."

14. **urs**
May 10, 2007

> I genuinely want to know […]

It may be helpful to emphasize, as Peter mentioned above, that

a) there is a well-known relation between 2-dimensional conformal field theory and 3-dimensional Chern-Simons theory for a given gauge group G. This is a deep and powerful relation between two classes of field theories.

b) it so happens that for particular choices of gauge group, Chern-Simons theory becomes equivalent to 2+1 dimensional gravity.

This is probably a "coincidence of low dimensions": two classes of structures which are by themselves rather different happen to coincide when both are restricted to sufficiently degenerate cases (here: gravity restricted to sufficiently low dimensions), simply because there is not enough room for them to differ:

here both the Chern-Simons action and the Einstein-Hilbert action (in its Palatini formulation) must be suitably invariant functionals pairing a connection 1-form and a 3-manifold. There are not that many possibilities for that, in a sense.

So the idea is: (quantum) gravity in general is an awefully complex issue, which we arer having a hell of a hard time coming to grips with. After we are sufficiently frustrated about our lack of progress in the general case (or even the one case, 3+1 dimensions, which we started off being interested in), we fall back to the observation that if only we constrain one parameter (the number of dimensions) sufficiently, suddenly everything becomes tractable.

Before we embarked on the quest for quantum gravity we had been optimistic that progress is possible for the cases that we were really a priori intersted in and didn't care about that unphysical 2+1-dimensional case.

But now that frustration with the general case is high enough, the case we previously looked down on suddenly appears much more charming.

So we say: "Let's then at least fully work out everything in that toy example. Better than getting nowhere."

And then it turns out that even the toy example is demanding enough and that we should maybe have trained our skills at this from the very beginning.

15. **anon**
May 10, 2007

Hopefully, someone will correct me.

In 3+1 there are some deep problems with quantum gravity. One of which is the problem of time and others related to the problem of quantifying the geometry (or the arena) on which physics occurs.

Some writers originally approached 2+1 as a 'laboratory' (or as Peter calls it a toy model) where problems like the problem of time or the quantization of geometry could be considered towards the end of generating intuition / ideas about the 3+1 problem.

16. **Aaron Bergman**
May 10, 2007

it so happens that for particular choices of gauge group, Chern-Simons theory becomes equivalent to 2+1 dimensional gravity.

As I understand it, this is not at all clear. It is true that there are classical transformations that take one to the other, but the relation of the quantum theories (whatever they may be) is not obvious.

17. **urs**
May 11, 2007

> it so happens that for particular choices of gauge group, Chern-Simons theory becomes equivalent to 2+1 dimensional gravity.

As I understand it, this is not at all clear. It is true that there are classical transformations that take one to the other, but the relation of the quantum theories (whatever they may be) is not obvious.

Ah, is it not that Witten in his approach effectively *defines* 2+1D quantum GR to be G-Chern-Simons, for suitable G?

18. **urs**
May 11, 2007

(Hm, in the preview of the above comment the nested blockquote did work.)

19. **Jens**
May 11, 2007

> As I understand it, this is not at all clear. It is true that there are classical transformations that take one to the other, but the relation of the quantum theories (whatever they may be) is not obvious.

Indeed, even the classical theories are not equivalent, since in the CS formulation degenerate metrics are included. As a consequence, the gauge equivalence classes of Einstein-Hilbert gravity and "CS gravity" are not the same, see for instance gr-qc/9903040 for an elaborate discussion.

As Urs points out, Witten, in his -88 paper, rather takes the CS formulation as the definition of 2+1D quantum gravity

20. **John Baez**
May 11, 2007

One has to just take some definition of 2+1-dimensional quantum gravity, study it, and see if 'acts like a quantization of general relativity in 2+1 dimensions should' — bearing in mind that general relativity in 2+1 dimensions has a lot of weird features, and quantization will make things even weirder!

There are a number of BF theories and Chern–Simons theories that have a claim to describing gravity 2+1 dimensions, depending on the signature of the metric and the value of the cosmological constant. Derek Wise will give a thorough listing of these in his thesis (due in a few weeks).

As Jens notes, all these theories allow degenerate metrics, unlike Einstein-Hilbert gravity. That may be okay; one just need to see what happens, especially upon quantization.

The more realistic theories have Lorentzian signature, so they have noncompact gauge groups, which introduce a lot of technicalities that people don't fully understand yet. For example: the representation theory of noncompact quantum groups and noncompact affine Lie algebras.

21. **Rado**
May 12, 2007

For those you want to take a look on a simple example (among many) of 2+1 Gravity and 2-d CFT relation, you can take a look here [hep-th/0411060].
This papers starts with 2+1 CS gravity as used by Witten and derives 2-d Ward identities which are characteristic from CFT and known for long time. The key is to considers proper boundary terms, inpired by AdS/CFT correspondence.

22. **urs**
May 14, 2007

One way to think of a realization of the relation

G-WZW theory on 2D-bundary G-CS theory on 3D-bulk

seems to be to think of membranes in a Horava-Witten setup:

the membrane, propagating in some spacetime, couples to the supergravity 3-form C, which looks like

C = CS(A_e8) + CS(A_so(10,1)) + dB,

where A_e8 is an e_8 connection and A_so(10,1) an so(10,1)-connection and CS(…) are the corresponding Chern-Simons terms.

Varying the membrane configurations is a lot like varying the pulled back connections A_…, hence a lot like variation of a Chern-Simons functional on the membrane.

So we might expect a corresponding WZW theory on the boundary of the membrane. And indeed, for the e8 part we do: that should be the internal current algebra CFT of the heterotic string.

But what about the SO(10,1)-part?

At least naively, it should somehow split, locally, into the SO(2,1)-part tangential to the membrane and an SO(8)-factor transversal to it.

Possibly the SO(8)-WZW model may be merged with the E_8 one some way or other. What would be left is an SO(2,1)-CS theory on the membrane. Since that's, roughly, workvolume gravity — as discussed in the above thread — , it looks a little like part of the worldvolume kinetics of the membrane.

I am just saying this in the hope that somebody out there recognizes something in these observations that can actually be made a little more precise. I have here three Lie 3-algebras corresponding to the above three ingredients lying around, and I would like to fuse them back to the one thing they came from. It vaguely looks like understanding the above should help understand this problem. Or vice versa.

23. **Peter Woit**
May 14, 2007

Urs,

Do you really believe that a good way to think about the relation between the simplest 2d conformal field theories (WZW models), and simple 3d QFT with a single, beautiful term in the action (CS) is to embed everything in 11 dimensions, and try and relate things to 11d supergravity and an unknown M-theory?

24. **urs**
May 14, 2007

> Do you really believe […]

Yes.

By the way, maybe in what I wrote above I am being stupid with seeing a problem here where the formalism is trying to tell me that there is no problem:

On p. 79 of Wiaeo? the authors define a Chern-Simons theory parameterized by a G-bundle E->X to involve only those bundles with connection, which can be obtained from pullback of E along some map Sigma : M–>X. Then one only varies those maps Sigma.

That's *precisely* the situation we have for the 3-form coupling of the membrane!

Hm…

25. **R**
May 18, 2007

It is really nice to read unpublished newest ideas in physics as blog and to have a chance to post a comment though I am a simply beginner of string theory.
I cannot tell well but I am wondering newest 2+1 D doesnot mean usual spatial 2D + time D but spatial D + time D + extra D (or spin geometry?).

Regards,

26. **John Baez**
July 13, 2007

I say a bit more about Witten's new idea in week254.

In particular, since Urs and some of his collaborators know a lot about how to get 3d topological quantum field theories from 2d rational conformal field theories, some of them may enjoy trying to extract a 3d TQFT from the c = 24 conformal field theory Witten is considering as dual to 3d quantum gravity in his new work – the one that has the Monster group as symmetries.

If Witten's ideas are correct, this 3d TQFT could be a kind of "improved version" of Chern-Simons theory when it comes to describing 3d quantum gravity. This seems like a strange idea, but perhaps interesting.

(I'm not sure this field theory is a rational conformal field theory, or has been proved to be so. I believe Borcherds, Frenkel, Lepowsky and Meurman only consider it as a vertex operator algebra. I hope it extends to a rational CFT. But, this could be hard to show, even if it's true.)

Crash Course

May 7, 2007
Categories: Uncategorized

This week's New Yorker has a quite good article on the LHC and the state of particle physics with the title [Crash Course](). One of the main themes of the article is that of the rivalry between experimentalists and theorists. There's a quote from Leon Lederman:

If I occasionally neglect to cite a theorist, it's not because I've forgotten,… It's probably because I hate him.

CMS experimentalist Robert Cousins describes worries that triggers designed with too much attention paid to theorists could be disastrous:

There are famous high-energy-physics experiments that missed discoveries because they weren't writing them to tape… This is why we try not to be too specific about which theoretical speculations we care about. We add up all the energy, and if it's a huge number we write that event to tape. If on one side of the detector it's a not-so-huge number, but there is nothing on the other side, so it's a huge imbalance, we get excited about that, and we write that to tape, too.

The only theorist interviewed is Nima Arkani-Hamed, who, while consuming prodigious numbers of espressos, describes the perception of theorists by experimentalists as:

There is a sense among many experimentalists that theorists are a bunch of irresponsible little spoiled brats who get to sit around all day, having all these fun ideas, drinking espresso and goofing off, with next to no accountability.

and jokes that theorists will need to get a "Deep Throat" among experimentalists in order to get access to any raw LHC data.

As for the state of the LHC, the Resonaances blog at CERN [describes]() rumors from "well-informed sources" that the low-energy test run scheduled for late this year is likely to be cancelled, with a physics run at full energy not likely until summer 2008.

Comments

1. **Christine**
   May 7, 2007

   I read in the article that

   As of 2000, at least ten thousand scientific papers on supersymmetry, and several thousand more on string theory, had been written.

   So what has been learned from those thousand and thousand of papers? What is the best review paper available?

   Thanks,
   Christine

2. **anon.**
   May 7, 2007

   Christine, Einstein said if 100 geniuses all believe in the same thing, at least one of them should be able to provide some solid reasoning, and if they can't do that, you begin to wonder how ingenious they really are. That was in 1931 when *100 Authors Against Einstein* was published. Supersymmetry is pretty similar. So many people can't all be wrong in their prejudiced opinions, even though they have no solid evidence and furthermore – with the landscape problem – no conceivable way of ever getting any solid evidence. What's been learned is that the landscape undermines the credibility of the subject, but the inertia of groupthink continues.

3. **Chris Oakley**
   May 7, 2007

   > So what has been learned from those thousand and thousand of papers?

   I am sure that quite a lot has been learned, but I doubt that any of it is relevant to the LHC.

   Re: the *New Yorker* article, I am curious about some of the more wacko speculations as to what the LHC might produce, e.g. extra dimensions or mini black holes. *How would one know?* In the time-honoured particle physics tradition, all the LHC experiments use measured position and momenta of detectable particles to infer properties of the unstable particles that may have created them using relativistic kinematics. One may be able to enlarge the table of known particles by analysing these data, but how does one go from this to demonstrating the existence of extra dimensions or mini black holes? In these cases one would surely need to have a clear idea of how these esoteric ideas impacted the list of known particles and their properties first – something that at present is clearly lacking.

   Maybe I am missing something here. Does the LHC have an experiment specifically designed (e.g.) for catching mini black holes? (other than in the funding, obviously).

4. **Peter Woit**
   May 7, 2007

   Christine,

   As far as attempts to use supersymmetry and string theory to connect to the real world, the three books I wrote about recently each review this. Dine is strongest on supersymmetric field theory, Becker-Becker-Schwarz on string theory. Lots has been learned about string theory and supersymmetry that is irrelevant to the real world, but there's such a variety of things that I don't know a single source that reviews them.

   Chris,

   There are "brane-world" models in which the quantum gravity scale is low and you can estimate cross-sections for producing black-holes, and what the experimental signatures would be if you produced them. There's a whole industry of people doing this. Of course it's pretty much completely unmotivated, there's no reason at all to believe that the quantum gravity scale is around 1 TeV, carefully set so as to have zero effect at the Tevatron, but a dramatic effect at the LHC. Then there's the problem that these things maybe should already have shown up in neutrino experiments.

   As far as I can tell virtually no one believes that this kind of thing will be seen at the LHC, but a lot of people go on about how exciting it would be it this happens.

5. **Aaron Bergman**
May 7, 2007

Supersymmetry is pretty similar. So many people can't all be wrong in their prejudiced opinions, even though they have no solid evidence and furthermore – with the landscape problem – no conceivable way of ever getting any solid evidence.

There are many, many conceivable ways of getting solid evidence for supersymmetry. And what, exactly, does supersymmetry have to do with the landscape anyways? Can we please try to keep our attacks of various directions in high energy physics straight?

For Chris, in various extra dimension models, people have worked out the relevant accelerator signatures. As an example, for certain TeV scale gravity models, you can produce graviton that leaves the brane and thus manifests as missing energy. There are many others, but I'm not really up on this stuff. Similarly, small black holes have accelerator signatures as worked out, for example, by Giddings and Thomas (and again, probably many others).

6. **David**
May 7, 2007

Chris & Peter,
I heard Lisa Randall give a talk last year mod(details) when she said that some of the things in her book should be testable at the LHC. It might be interesting to have a guest posting to hear about the details.
Best,
David

7. **Peter Woit**
May 7, 2007

David,

I think Lisa Randall is at one extreme of the spectrum in terms of believing in these models, since she is one of the people who came up with them. But from what I remember of her book, even she doesn't claim a strong belief that these things will be seen at the LHC.

8. **Deep Throat**
May 7, 2007

Nima Arkani-Hamed must know something if he mentions me like that. Maybe my throat is too deep….

And I doubt theorists would be able to do anything with "raw LHC data" anyhow. Probably they would get as far as to re-discover partons, but not much more. Raw LHC data will be as raw as the tail of a live cow can be called "rare filet". Not easy to digest if you don't kill the cow first.

Cheers,
DT

9. **Peter Woit**
May 7, 2007

Deep Throat,

I think the LHC experiments should seriously consider providing "raw data" to any theorists who complain. The results might be amusing to watch.

10. **Anti-Crackpot**
May 8, 2007

Can someone please explain to me the reason for your skepticism about supersymmetry? String theory is one thing, but I think calling supersymmetry unmotivated and crazy is just plain wrong. Those of you who think this way must have never really studied particle physics. Otherwise, you would be aware of how simply and elegangly supersymmetry solves a hosts of problems.

11. **ruleman**
May 8, 2007

> simply and elegangly supersymmetry solves a hosts of problems.

Also, half the particle spectrum has already been observed, and about 20% of the couplings measured.

But it must be mentioned that at least one prominent staff theorist at CERN (who most certainly has studied particle physics) claims that the MSSM is *minimal* with respect to its credibility… We'll see, in due time.

12. **Chris**
May 8, 2007

Dear Peter,

I know it's forbidden to mention one's own paper on your blog, but my paper arXiv:0704.1476 is definitely an attempt to take TeV-scale gravity very seriously. There's unfortunately a very large amount of work still to do to have predictions ready for the LHC, but it's doable, and I'm going as fast as possible.

The most generic prediction of TeV-scale gravity is that the effects will turn on extremely fast at a certain energy, due to the very rapid increase with energy of graviton effects. Below a certain energy, that could very well lie between 2 TeV and 14 TeV, there will be nothing detectable, and above that energy, a large fraction of the large transverse momentum processes selected at the LHC will see large fractions of missing energy as gravitons are radiated into the extra dimensions. In this respect it will be similar to the J/psi.

With regard to why it should happen at the LHC:

(1) TeV-scale gravity makes it easier to fit a small cosmological constant; and

(2) There's a natural factor of 1/10 (a loop factor) between the topologically stabilized breaking of the Horava-Witten E8 to the Standard Model, and the radiative breaking of SU(2) x U(1) in modern versions of the Coleman-Weinberg mechanism, see e.g. hep-ph/0509122 by Chishtie, Elias, Mann, McKeon, and Steele.

With regard to neutrinos, Maltoni and Schwetz have shown in arXiv:0705.0107 that MiniBooNE and LSND can be in perfect agreement if there are two sterile neutrinos, i.e. a (3+2) scheme, due to a new CP violating phase. The E8 breakings in subsection 5.6 of 0704.1476 lead automatically to a number of Standard Model generations plus an unrelated number of sterile neutrinos, so can accomodate 3 + 2.

Best regards,
Chris

13. **Peter Orland**
May 8, 2007

There are a number of reasons why high-energy experimentalists have hostile feelings towards theorists (as expressed by Lederman in his book). Unlike, say, condensed-matter physics, there is rarely much contact between experimentalists and any but the most phenomenological theorists. That is because neither theorists nor expermentalists are doing something which has serious impact on the activities of the other group.

The experiments are very hard to do but produce no fundamental results (let's hope that will change soon). Hence theorists work on problems with low stakes (like unifying particle physics with quantum gravity) alienating the experimentalists.

14. **Hans de Vries**
May 9, 2007

One can appreciate Michael Peskin's recent effort to refocus the theorists attention to real world LHC issues, like finding traces of new particles in the rather complicated background of the jets.

(Although he lures them with an highly optimistic picture of finding both SUSY physics and dark matter particles at the 100 GeV scale.)

http://video.tau.ac.il/Lectures/Exact_Sciences/Physics/stringfest/OnDemand.html?6
(Needs Internet Explorer 5.5 or higher)

Regards, Hans

15. **Bee**
May 9, 2007

I heard Lisa Randall give a talk last year mod(details) when she said that some of the things in her book should be testable at the LHC. It might be interesting to have a guest posting to hear about the details.

Hi David,

regarding predictions of models with extra dimensions at the LHC, see

Extra Dimensions

and

Micro Black Holes

Best,

B.

16. **Bee**
May 9, 2007

Btw, Peter, thanks for the pointer to the article, it's really well written and worth reading. I didn't know that quotation by Wilson, I like it a lot.

am I the only one who finds the random comics in the middle of the page kind of disturbing?

17. **J**
May 9, 2007

Bee, certainly you are not. I totally agree with you.

18. **theoreticalminimum**
May 10, 2007

"*… while consuming prodigious numbers of espressos…*"
Apparently, Nima could gobble up "no less than six cups of espresso" during a 1.5-hour interview! Kolbert also reports a similar kind of feat.
Wow! Assuming it is not decaf, this is hell of a lot of caffeine in the bloodstream! This is almost literally turning oneself into a machine running on caffeine.
Now, I wonder whether this is the physics equivalent of the often-talked-about amphetamine intake of some mathematicians (if the latter is still happening).

19. **Steve Myers**
May 11, 2007

Being a guy who gets paid to gather & analyze data I was most impressed by the amount of data that has to be massaged. Always the hardest problem is extracting information from the data. With that problem even the crudest model helps a lot. Even a bad theory is better than none — so I guess even a bad theorest is worthwhile.

20. **David**
May 11, 2007

Bee,
Thank you.

David

21. **Alan Reifman**
May 14, 2007

Tonight, the NY Times website has a link to an article about the LHC that apparently will run in Tuesday's papers (the online version is dated May 15).

http://www.nytimes.com/2007/05/15/science/15cern.html

The Times article covers similar ground to the New Yorker one — i.e., both the physical construction and science of the LHC — and is similarly lengthy.

New Blogs and Other Stuff

May 12, 2007
Categories: Uncategorized

Here's a few new blogs I've run across recently:

- The FQXi organization now has a blog called FQXi Community.
- Rantings of an Angry Physicist is not another Not Even Wrong, but an interesting blog so far devoted to explaining what is going on in Steve Carlip's quantum gravity course.
- The new open access journal PhysMathCentral has a blog. It's "open access" in the sense that it promises to indefinitely provide free access to published articles. Funding comes from the authors of the articles, who have to come up with an "article processing charge" of around $1500. I'll be curious to see if this funding model works out, but have my doubts. From what I remember, back in the 1970s, the fact that APS journals were charging authors a similar "Page Charge" fee was one of the reasons why many prominent theorists stopped publishing in the Physical Review and started publishing in commercial journals like Nuclear Physics B, thus entrenching commercial publishers like Elsevier. It's unclear to me now how many authors will be willing to pay to publish when they can publish for free in other (often commercial) journals.

Robert Bryant, a great geometer in the Cartan-Chern tradition, now at Duke, has accepted the post of next director of MSRI at Berkeley. Robert was here at Columbia recently as a visiting professor, and I think he's a wonderful choice for leading MSRI.

The Geometry, Topology and Physics Seminar at UCSB has some material from talks there on-line. Last month there was a quite interesting talk by Sergei Gukov on gauge theory and "arithmetic topology", meaning some analogies between 3-manifold topology and number theory.

For the past few days in Brussels there has been a Solvay workshop on "Gauge Theories, Strings and Geometry" . Talks are available here.

From the Fermilab Steering Group trying to develop a strategic roadmap, there's a presentation about possibilities for higher energy colliders than the LHC or ILC. Ideas discussed include a doubling of the LHC energy using new 17 Tesla magnets, and a huge proton-proton collider called the VLHC to be built deep underground, in the Chicago area.

Next month in Paris there will be a Smolin/Damour debate about string theory, see Dispute chez les physiciens.

For an interesting article I just ran across about Geoffrey Chew and S-matrix theory during the 1960s, see here.

Comments

1. **anon.**
   May 13, 2007

   English translation of the link to the news of the Smolin-Demour debate (courtesy of babelfish).

2. **Thomas Larsson**
   May 13, 2007

   Re Geoffrey Chew, the latest issue of Cern Courier has an article about another favorite of yours.

3. **wolfgang**
   May 13, 2007

   anon.

   thanks for the translation, but when it says "theory of the cords of quantum gravity", do you think the original was strings or loops 8-?

4. **Peter Woit**
   May 13, 2007

   In French "theorie des cordes" is used to denote string theory, although "corde" is a word used to mean something more like a rope than a string. I've never understood why the translation isn't "ficelle" which would be more accurate.

   The French do have in their language the concept of "un string", but it means something a bit different, leading to snickering if one translates the term that way…

5. **theoreticalminimum**
   May 13, 2007

   > The French do have in their language the concept of "un string", but it means something a bit different, leading to snickering if one translates the term that way…

   Oh, come on Peter! I thought you would show us some of that French *audace*, and translate that term away (not that it needs translation for informed readers of this blog). I even found out that, may I say well-endowed, members of the so-called "string mafia" have made it to the Tour de France! I mean, could any unification program get that much sexy?! ;-P

   The debate between Smolin and Damour would, I believe, be worth listening to. It would be of a kind different from the ones that took place before, e.g. Smolin/Duff. Damour is one of the leading experts in general relativity and, though having written a few papers in string cosmology, is not, I would be inclined to say, a full-fledged string theorist. So, at least in that respect, it would be interesting enough to look forward to knowing to what extent Damour agrees and disagrees with Smolin.

6. **John Baez**
   May 13, 2007

   Speaking of string-loop debates, Hermann Nicolai is helping run a conference in Bad Honnef called "Quantum Gravity: Perspectives and Challenges", during April 14th-16th, 2008. It's supposed to be a kind of reprise of the 2003 workshop on "Loops vs. Strings" at the Albert Einstein Institute. It looks like I'll be going — maybe in my new role as someone who has serious doubts about both approaches.

   I'm not exactly relishing the prospect, but I think it'll be good to have people there who aren't committed to either approach, but know a little about both. Solving physics problems by gladiatorial combat between "teams" doesn't seem very productive.

7. **Markk**
   May 13, 2007

   "Solving physics problems by gladiatorial combat between "teams" doesn't seem very productive."

   Yes but from reading history it does seem to be productive at defining specific problems which can be attacked. Or at least agreeing on areas where those problems lie.

8. **Harry**
   May 14, 2007

   Concerning "cordes" and "ficelles", "ficelles" is in french associated with a few quite negatively connoted expressions : "celui qui tire les ficelles" = the one who is hidden and makes this others act, "les ficelles d'un art" = the hidden processes of an art. An old meaning (XVIIIth) is also "crafty" (i. e. "il est très ficelle" = he's very crafty).

   I guess that's the reason why "cordes" (which sounds solid and thick) has been chosen.

9. **Christine**
   May 14, 2007

   For what is worth, in portuguese the translation of "string theory" is "teoria de cordas", following the french term.

10. **Alejandro**
    May 14, 2007

    Same in Spanish ("cuerdas" rather than "hilos"). Perhaps the often-used analogy to vibrating strings in musical instruments can explain it? "Cuerda" normally (i.e. without context) would mean "rope" in Spanish, but string instruments are called "instrumentos de cuerdas". I gather the same happens in French.

11. **Aaron Bergman**
    May 14, 2007

    FWIW, Lee Smolin and Erik Verlinde will be appearing on a two person panel at the end of the month.

12. **Who**
    May 14, 2007

    Actually the October 2003 conference at the Albert Einstein Institute that Hermann Nicolai, Abhay Ashtekar and others put together was called
    "Strings meets Loops"

    and the emphasis, by all I could tell from outside, was on productive exchange. There was a fifty-fifty balance of talks. Nicolai gave the opener and Ashtekar gave the concluding talk.

    Seemed like a good idea. I'm glad that something along those lines is scheduled for 2008. Long overdue. Just want to correct a slight oversight int what John Baez said: the conference was not called "Strings VERSUS Loops", or Loops vs Strings, or anything versus. AFAIK the organizers' idea was a friendly meeting of equals.
    ==quote==
    Speaking of string-loop debates, Hermann Nicolai is helping run a conference in Bad Honnef called "Quantum Gravity: Perspectives and Challenges", during April 14th-16th, 2008. It's supposed to be a kind of reprise of the 2003 workshop on "Loops vs. Strings" at the Albert Einstein Institute. It looks like I'll be going — maybe in my new role as someone who has serious doubts about both approaches.
    ==endquote==

13. **Who**
    May 14, 2007

    Typo: the conference was called STRINGS MEET LOOPS
    http://www.aei.mpg.de/english/contemporaryIssues/seminarsEvents/pastEvents/index.html#2003
    I learned about it from John Baez, who posted the schedule sometime around the first week of October 2003, IIRC, but i can't find it in TWF.

14. **Viri**
    May 14, 2007

    Regarding theoreticalminimum's language reference links 🙂 I must say that in Portuguese String Theory is translated as "Teoria das cordas" "corda" being strong as opposed to "fio".

    Now the really funny part is that the pictures he linked where of "tangas" (the underwear) and that is a synonym of bullshit and has a derogatory meaning in many other contexts, usually when someone is trying to fool someone else.

    I too think the musical instrument analogy explains these translations to romance languages.

15. **Michael**
    May 14, 2007

    Peter,

    Sorry to go off subject. But I was wondering if you knew of any colloquium/seminar type events held regularly in the New York City area over the summer. Or if you knew about any upcoming lectures of interest in areas such as cosmology or particle theory. I'm having some trouble finding out about things that go on off the academic calendar. Thanks!

    -Michael

16. **Peter Woit**
    May 14, 2007

    Michael,

Sorry, but I don't know of any colloquium/seminar activities in the area this summer. People pretty much only schedule those during term time, assuming too many people are away and it is hard to get together an audience during the summer. In the New York area there are two summer programs I know of:

http://insti.physics.sunysb.edu/itp/conf/simonswork5/
at Stony Brook, and

http://www.admin.ias.edu/pitp/

at the IAS. The IAS program you probably need to have applied for to attend the lectures.

17. **Chris W.**
May 14, 2007

The conference that Aaron pointed to (re Smolin and Verlinde) also includes this intriguing talk:

Jürg Fröhlich (ETH Zürich) — "Atomism and quantization"

I wonder what exactly *that* will be about.

18. **Travis**
May 23, 2007

Regarding Open Access, while Open Access options are currently limited to "author pays" there is an initiative in Europe called SCOAP3 that would shift the payment for Open Access to the funding agency level. Few, if any, proponents of Open Access in HEP think that authors would choose to pay if the money could go elsewhere. But by earmarking, or simply paying as a consortia, funding agencies can engineer this shift.

This is important for physicists to think about, as one essential reason for moving this direction is the preservation of the peer review process against the cancellation of overpriced journals by underfunded libraries who no longer have much reason to spend money on HEP journals (why pay for articles already on arXiv?).

Salvatore Mele gave a colloquium at SLAC recently about this topic, see:

http://www2.slac.stanford.edu/colloquium/details.asp?EventID=204

for more information, including links to the SCOAP3 documents.

19. **Peter Woit**
May 24, 2007

Thanks Travis!

I was just as Oxford, where the editorial board of the Elsevier journal Topology all recently resigned, leaving to found another journal. The latest issue of Topology came in while I was there, a real collector's item. The page that normally lists the editors is blank. No one knows if Elsevier will try and keep the journal going…

All LHC, all the Time

May 15, 2007
Categories: Uncategorized

The LHC media blitz is in full swing, with last week's long New Yorker article now followed by an unusually long and detailed New York Times piece titled A Giant Takes On Physics' Biggest Questions. Dennis Overbye does an excellent job of covering the story. Besides the experimentalists actually involved in building the machines, he quotes theorists John Ellis, Joe Lykken, Nima Arkani-Hamed and Michelangelo Mangano. To distinguish this piece from the New Yorker one, here it's Mangano who is the one who consumes a lot of espresso. There are side-bars about the recent problem with the Fermilab magnets and about the implications for string theory (not much). There's a multimedia component to the Times coverage, with interactive graphics, a slide show, a podcast (an interview with Arkani-Hamed, described as "one of the physicists at the center of the project"), and a video.

I do fear all this LHC coverage is peaking too early. With still probably at least a year to go before the machine even starts taking data, the coverage may already be generating an LHC overexposure problem: see Chad Orzel's new posting Tired of the LHC. If Chad is already complaining about this, boy is he going to be grumpy about it by a year from now…

The New Yorker keeps its physics theme going this week with cover art that includes a blackboard full of basic equations from quantum mechanics.

The NY Times article includes the usual not very cogent explanation of the role of the Higgs. For something much better aimed at explaining Higgs-hunting to the general public, see the online interactive presentation Hunt for Higgs, part of a web-site about the LHC called Big Bang.

Blogging may be light the next week or so since I'll be traveling. First stop is Trieste, where I'll be speaking at 5pm on Friday as part of a large event there called FEST. From there I'll make brief visits to Geneva, Paris and London, back here in New York late next week.

Comments

1. **Ari Heikkinen**
   May 15, 2007

   All the better to hype it now and give it as much coverage as possible, as when it's running a year or two from now it'll probably end up finding nothing useful anyway.

   Then again, maybe some LHC hype makes it possible to collect some money for a few other projects while LHC is still being built..

2. **sinus**
   May 15, 2007

   > it'll probably end up finding nothing useful anyway.

   Maybe the notion of "useful" has been blurred by calling the Higgs boson "God particle", and string theory the "Theory of Everything". Those suggest that nothing short of finding "God" or "Everything" is good enough.

   I think discovery of, at least, new phenomena related to the EW interactions is guaranteed (by the usual arguments). Whatever is found, even if it's "just" the SM Higgs, will be a Real Natural Phenomenon (TM), infinitely more useful than any speculative fantasy about gods and everythings.

3. **Anti-Crackpot**
   May 15, 2007

   I think its likely that within two years, the particle physicists and string theorists will be sipping champagne, and all of the stupid people who think it will find nothing will dissappear into the vacuum.

4. **Walt**
   May 15, 2007

   It's rare you see such a pure expression of bravado outside of sports bars. Like: I think it's quite likely in two years the Philadelphia Eagles will have won two Super Bowls, and all the stupid Patriot and Colts fans will disappear into the vacuum.

5. **Anti-Crackpot**
   May 15, 2007

   Walt,
   Physics isn't sports. The people who doubt that LHC will find anything, don't do so for rational reasons. They actually hope it won't. The overwhelming liklihood is that there will be significant discoveries.

6. **jb**
   May 16, 2007

   I think you two should put some money on this.

7. **plank**
   May 16, 2007

   How can anyone seriously say whether or not new phenomena will be found at the LHC?

   Really, how? Either way it's "faith" based.

   Now saying string theorists will be sipping champagne is another thing since I would like to know of a possible discovery that would legitimately justify that. Just an example.

8. **anon.**
   May 16, 2007

The string theory landscape is so big, they probably *will* be sipping champagne in two years whatever the LHC discovers, because they will be able to pick out a vacuum which resembles the physics, whatever that physics turns out to be.

That's the whole brains of string theory! Whatever nature turns up, it's *impossible* not to come up with some version of string theory which can include that. String theory predicts everything remotely possible, so we it is right before doing the experiments. The experiments just help us identify the correct model. I can't understand why people (apart from Prof. Anti-Crackpot) don't understand.

Physicists can start celebrating string theory now, because it can't be falsified, there's no sensible alternative with as much hype as string, and even if there was a fashionable 'sensible' alternative, it would be likely to be *extremely boring* compared to the 11 dimensional 10^500 models of the string multiverse.

9. **Anti-Cackpot**
May 16, 2007

Plank,
If we weren't able to seriously say that there's a strong liklihood of discovery, LHC would not have been funded. The Higgs and supersymmetry will both likely be discovered. They are both well-motivated theoretically. You should understand that these are not just things theorists invent because they're nice. These are really necessary pieces that need to be added to the Standard Model.

10. **anon.**
May 16, 2007

These are really necessary pieces that need to be added to the Standard Model.

The Higgs or something else to unitarize WW scattering, yes. Supersymmetry, no.

11. **Bob**
May 16, 2007

I don't understand this discussion about the LHC. Surely it will find new phenomena, irrespective of whether they can be related to string theory. in some sense, failure to find a Higgs boson in the expected energy range would drive high energy physics forward even more strongly than would the confirmation of a mass within the currently favored limits. Either result would be great.

High energy physics is presently unbalanced between theory and experiment, and in serious need of experimental data in new energy regimes. *That* the LHC will provide one way or another.

12. **Anti-Crackpot**
May 16, 2007

Anon,
If there is a Higgs boson, then supersymmetry is essentially required to to stabilize the Higgs mass, otherwise known as the hierarchy problem. There may be other 'solutions' to this, but none of these is natural. The existence of supersymmetry is probably just as necessary as the existence of anti-matter, as a necessary symmetry of nature.

13. **observer**
May 16, 2007

It seems to me that there's been a curious drift in the rhetoric on this blog of late. It looks to me like SUSY is being taken as synonymous with string theory, which it's not (the latter clearly requires the former, the reverse is not true). SUSY is theoretically well-motivated and quite predictive. It should be possible to rule SUSY out with future experiments, unlike string theory. So why is everyone so pissed off about SUSY?

14. **Peter Woit**
May 16, 2007

observer,

I seriously considered deleting just about all the comments on this posting. Our string-theorist troll "Anti-Crackpot" generates lots of content-free responses. Please, no more of this on both sides. In general please resist posting contentless comments that do little except aggressively express an opinion, without even bothering to justify it.

Sure, SUSY is better motivated and more predictive than string theory. But I think there recently has been a reaction to the fact that SUSY has received and continues to receive an overwhelming amount of attention, despite the fact that SUSY scenarios are far from convincing (generally due to the fact that SUSY-breaking leads to all sorts of trouble). Many SUSY proponents way back when vigorously claimed that evidence for it would show up at the Tevatron. The fact that that didn't happen, and that effects also haven't shown up in precision electroweak data, I think has caused a lot more skepticism about whether it will be seen at the LHC.

But, it is very different than string theory, will get a real test at the LHC, and we'll find out one way or another within a few years. If the LHC sees nothing, the idea that supersymmetry stabilizes the electroweak scale will be conclusively dead.

15. **plank**
May 16, 2007

«These are really necessary pieces that need to be added to the Standard Model.»

OK, suppose both are discovered beyond reasonable doubt. In what way does that corroborate String Theory?

Neither of those ideas were created in a String context, or am I wrong?

If there were some specific predictions from ST regarding SUSY breaking or the higgs mass I could understand, but to my knowledge there is nothing serious close to this.

So my question stands, what should be discovered at the LHC in order to justify string theorists "sipping champagne"?

And what would justify its demise.

These are important questions that should be answered *before* the actual data is analyzed, otherwise I'm afraid anything will be hailed as a victory for ST.

16. **Peter Shor**

May 16, 2007

Since almost anything the LHC is likely to find will be shown to be consistent with string theory, I predict that the string theorists will be sipping champagne three years from now, no matter what the LHC finds.

17. **Anti-Crackpot**
May 16, 2007

Peter,
I could reply to your insult about being a string-theorist troll, but let's not go there. Let's leave that for Lubos. I'm really more of a particle physics person than a string theorist.

My opinion on the reason for the sentiment against supersymmetry on your blog is that it is due to the fact that it is a necessary component of string theory, which you strongly disfavor. Otherwise, it's hard to understand why anyone would be so against it., especially since it's well-motivated theoretically and testable. It seems to me that there is a strong current against mainstream particle physics on this blog, and I feel I must go against this.

18. **Anti-Crackpot**
May 16, 2007

Plank,
String theorists will be happy if supersymmetry is discovered because it is a necessary part of string theory. Don't forget that SUSY was actually discoverd in string theory first, before being formulated in normal particle physics by Wess and Zumino.

19. **Peter Woit**
May 16, 2007

Anti-Crackpot,

My opinion about supersymmetry in general is mixed, there are many things interesting about it. As for the MSSM, I've always been a skeptic, even back pre-1984 when string theory became popular. There's a whole chapter in my book about the problems with supersymmetric extensions of the standard model, this is an issue independent of string theory.

I'm getting really sick of the contentless bickering here. Please, all parties stop. Comments containing zero information, just some an expression of opinion backed by at best best a vague, tired talking point will be deleted. I can't believe anyone is getting anything at all from this kind of discussion, other than the idea that reading the comment section here is a waste of time.

20. **JC**
May 16, 2007

Anti-Crackpot,

Wasn't SUSY first discovered around 1970 by Likhtman and Golfand?

21. **Anti-Crackpot**
May 16, 2007

Peter,
I'm not sure that there has been much bickering in this thread, or that the posts have had zero content. If you don't like people disagreeing with you then perhaps you should shut down the blog altogether. Perhaps my statement about the 'stupid people dissappearing into the vacuum' was a little harsh, and I apologize.

As for supersymmetry, it's fine to be skeptical. The main point I want to make is that the subject is one that should be taken seriously and not just be dismissed as the musings of idle string theorists, whether or not it turns out to be right or wrong.

22. **Anti-Crackpot**
May 16, 2007

JC,
Yes, it may have been, but if I remember correctly they were in the Soviet Union at the time, so noone in the west knew about their work.

23. **Coin**
May 16, 2007

Don't forget that SUSY was actually discoverd in string theory first, before being formulated in normal particle physics by Wess and Zumino.

When and where?

24. **Anti-Crackpot**
May 16, 2007

Coin: Ramond, Schwarz, and Neveu in about 1971-1972.

25. **M**
May 16, 2007

Anti-Crackpot: I don't know if supersymmetry was first invented by string theorists or by communists, but surely the discovery of supersymmetry would not imply that communism is right nor that string theory is right.

The reason why LHC is interesting is not that we know that LHC must discover supersymmetry, but that we do not know what LHC will find, and exploring higher energies is the only way of sorting out physics at higher energies. Even if LHC will tell that we lost decades on a wrong idea, it will be progress, and this is why I prefer to work on testable weak-scale physics rather than on quantum gravity.

26. **dan**
May 16, 2007

Peter Woit wrote “But, it is very different than string theory, will get a real test at the LHC, and we’ll find out one way or another within a few years. If the LHC sees nothing, the idea that supersymmetry stabilizes the electroweak scale will be conclusively dead.”

Hello Peter, if LHC does not find SUSY-partners, and yes I’ve read your book NEW cover to cover, and therefore does not stabilize the electroweak scale, it doesn’t seem to me that there’s no compelling reason (other than string theory hype) to continue to pursue this as serious research. In otherwords, if SUSY doesn’t stabilize the elctroweak scale, while SUSY can be broke all the way up to the planck scale, there is no reason to believe it is a symmetry of nature if there is a LHC null result. (Presumably the theoretical difficulties of breaking SUSY and flavor changing neutral currents and large CP violations others in combintation to a LHC null result should pretty much discredit the theory).

27. **Anti-Crackpot**
May 16, 2007

M,
You’re right that the discovery of supersymmetry would not prove string theory. However, since SUSY is a necessary ingredient, it would certainly suggest that string theory is in the right direction.

28. **Michael Bacon**
May 16, 2007

The discovery of SUSY might suggest that string theory moving in the right direction. But that’s not the issue, is it? The problem is that the failure to discover SUSY wouldn’t disprove “string theory.” Apparently, that’s impossible!

29. **Anti-Crackpot**
May 16, 2007

Michael,
The honest truth is that if SUSY is not found at LHC, most interest and support of string theory will completely evaporate. Unless, of course one of the large extra dimensions/ brane world scenarios turns out to be true. In that case, there will be KK states and black holes galore, and string theory will be experimentally confirmed.

30. **anon.**
May 16, 2007

The honest truth is that if SUSY is not found at LHC, most interest and support of string theory will completely evaporate.

Nonsense — you can find plenty of string theorists who claim that high-scale SUSY breaking is actually favored, or at least not disfavored. It’s not at all clear at the moment what string theory has to say about what is likely at the TeV-scale.

Also, above:

then supersymmetry is essentially required to to stabilize the Higgs mass, otherwise known as the hierarchy problem. There may be other ’solutions’ to this, but none of these is natural.

Again, nonsense; technicolor is natural, for instance. It just happens to be highly disfavored by precision data. There is also the possibility that the Higgs mass is simply tuned. There is absolutely *no* argument that SUSY is “required” or “essentially required” by the SM. There are only debatable aesthetic or philosophical reasons for liking it.

31. **Anti-Crackpot**
May 16, 2007

Anon,
Sure, it’s always possible that the SUSY breaking scale will be high enough that the superpartner masses will be unobservable at LHC. However, then SUSY would not be able to solve the hierarchy problem, and so would lose much of it’s justification. If this happens, I don’t think people would take SUSY seriously, regardless of what ‘plenty’ of string theorists say.

I don’t think that technicolor is particularly natural. Besides the fact that it doesn’t work, it has always seemed rather ugly and contrived to me. On the contrary, supersymmetry solves the hierarchy problem, gives us gauge unification, and provides a dark matter candidate. Keep in mind that supersymmetry was not originally invented to solve these problems. This is what I would call natural. The fact remains that something beyond the standard model is required and supersymmetry is the best bet for that somethting, by far. Thus, all of your statements are complete nonsense!

32. **Mikka**
May 17, 2007

Hi Peter,

Peter, you clearly have little beyond trivial knowledge of what physics is really about… I have come across only ONE! paper by you in the last upmteen years that was worth reading through (‘Particle Physics and Representation Theory’). Clearly you spend way!! too much time trying to be critical of theories you have already decided upon are unworthy of further study… and yet you continue to quip over and over on them… sigh!

If you are genuinely a physics researcher you would engage in one of the following activities:

1. Doing physics research… of whatever kind there is to be done… (i.e. in condensed matter theory, astrophysics, biophysics, chaos theory, etc.);

2. Writing up beautiful notes for Weinberg’s QFT texts and distributing them freely on the net;

3. Doing both with one hand tied behind your back.

Regards,
Mikka

4

33. **island**
May 17, 2007

Anti-Crackpot Says:
The fact remains that something beyond the standard model is required…

The false assumption being that the resolution to the problems can't possibly come from something fundamental that was missed along the way.

I beg to differ.

34. **anon.**
May 17, 2007

On this question of distinguishing supersymmetry from string theory stuff, I thought that 'superstring' is an abbreviation of supersymmetric string theory, and since M-theory unified the five theories in 1995, string is in effect synonymous to superstring?

Many string predictions are supersymmetry predictions: unification at 10^19 GeV, s-particles with unpredictable energy, etc.

I don't see how any experimental result will be able to confirm or deny supersymmetry or string predictions.

By the time you have enough experimental data to be meaningful, you will have resolved the problem experimentally. What use is theory in that case? Dirac predicted the energy of antimatter particles, Pauli predicted the energy of neutrinos, and the Standard Model predicted the energy of weak gauge bosons. There's nothing like that kind of prediction from string/supersymmetry. String is a great theory for yellow bellies, scared of experimentally refutation.

35. **JC**
May 17, 2007

anon, Anti-Crackpot,

If string theory ever falls out of favor in physics departments, it will most likely still be pursued in math departments. Some areas of string theory have very interesting mathematics (ie. mirror symmetry, etc …), that some mathematicians would still be interested in.

On the other hand if supersymmetry is not seen at the LHC, it wouldn't be surprising to susy phenomenology fall out of favor and eventually die shortly thereafter. Though possibly some hardcore SUSY true believers will still work on it. Analytic S-Matrix theory falling out of favor in the early 1970's, didn't stop Geoff Chew from continuing work on it well into the 1980's.

36. **sinus**
May 17, 2007

"If" susy is found… "if" KK states are found… "if" substructure is found… Those are some big "if"s. The bottom line is that physics at the TeV scale is going to be experiment-driven. With exactly zero predictions, theory has lost the LHC train.

37. **L-Train**
May 17, 2007

I stumbled on your website, trying to find information about a theory about gamma ray bursts that has been written up in general interest publications, such as the Economist. I am a layman (although with a scientific background — chemistry), but I have a paranoid interest in particle physics.
The theory that I was wondering about was Dr. Clavelli's theory that GRBs are produced by a spontaneous SUSY transition in white dwarves. From what I understand, GRBs are generally thought to be caused by exploding stars, and if anything, GRBs tend to not occur in galaxies where there are not very many elements heavier than helium, which seems to negate this idea. I was wondering whether Clavelli's ideas (regarding GRBs and spontaneous SUSY transitions in general) are taken very seriously in the physics community as a whole.

38. **CW**
May 17, 2007

L-Train,

Cosmic Variance would be a better blog for you to monitor, given your apparent interests. It also links to other blogs that focus on astrophysics.

39. **Anti-Crackpot**
May 17, 2007

Sinus,
Supersymmetry makes a very specific prediction: for every Standard Model fermion, there is a corresponding boson. If these states are discovered, then supersymmetry will be confirmed. We cannot at this point make specific predictions about the mass and mixings of the supersymmetric specturm because we can't say exactly how SUSY is broken. However, if SUSY is to explain the hierarchy problem, then the states must be TeV scale.

40. **Walt**
May 17, 2007

Anti-Crackpot: You are clearly offended by the existence of skeptics. Whether or not SUSY is confirmed, would physics have been better off if there were no skeptics? Clearly not.

41. **Anti-Crackpot**
May 17, 2007

Walt,
What offends me is skeptics who spread misinformation.

42. **anon.**
May 18, 2007

What offends me is skeptics who spread misinformation.

So the landscape dwellers who are certain without hope of solid confirmation that there are 10^500 universes each with 6/7 extra dimensions curled up in different versions of the Calabi-Yau manifold, don't offend you? Bullshit with the stamp of mainstream consensus is inoffensive, while objections from skeptics of the mainstream which contradict the mainstream claims are offensive misinformation? Quite right too!

Maybe you need to increase your ability to take offense, however. Be also strongly offended by people with alternative ideas. That's spreading misinformation, if the ideas don't fit into the mainstream consensus, the superstring framework. One other thing to take offense from (when you get more sophisticated): experiments. Experiments are just an embarrassment to an irrefutable theory.

43. **a quantum diaries survivor**
May 18, 2007

Hello Peter,

sorry for dribbling most of this nonsensical thread about who believes what and why it is stupid to/not-to 😉 My comment is rather about the point made in the post, i.e. that there is an over-exposure of the LHC quite a bit too early.

There are physicists who have spent the better part of their last 15 years designing, building, simulating, testing, assembling the giant detectors that are now almost ready to take data. These people have invested maybe half of their career in the project. It is them who will arrive tired to the first day of data taking, unfortunately. These experiments are really too long shots in this respect. However, I really see no alternative, but can only hope that the next efforts will be based on more readily available technology and funds, such that the hiatus between the TDR and the data becomes more reasonable.

I agree, the media are overhyping an endeavour that is either too old or too young to be meaningfully in the headlines. It is now clear that LHC will have no chance of having produced enough data to see the Higgs boson before mid 2009. And even SUSY particles, which some simple minds believe will pop into view the moment the detector is turned on, will have to wait until then to be discovered, if ever.

My two pence ? The LHC is getting its share of spotlights because now is the time to start financing its upgrade, the SuperLHC. Projects on R&D for detectors to be installed in CMS and Atlas to sustain 10^35 luminosities are already under way…

Cheers,
T.

PS have fun in Trieste, and stay away from Casinos in Slovenia!

44. **Aaron Bergman**
May 18, 2007

Bitter much?

45. **Anti-Crackpot**
May 18, 2007

Anon,
I don't think that I've been making an argument for string theory or the landscape. My arguments have been strictly in favor of SUSY. It is quite possible for the world to be supersymmetric and string theory to not be right. As far as skepticism is concerned, I'd like to hear real alternative ideas rather than just arguing against supersymetry on the basis that we haven't yet observed the SUSY partners. If LHC does not observe them, then your skepticism will be justified and shared by many particle physicists. However, we know that the SUSY scale should be 1 TeV or less to solve the hierarchy problem, so there's no point being skeptical until the range of energy has been explored.

46. **anon. #1**
May 18, 2007

At the risk of feeling like I'm banging my head against a brick wall, let me point out some things that seem to be misconstrued or not known by at least some of the people discussing this here:

1) It would be nice if the hierarchy problem turns out to have a solution. On the other hand, it might not; the universe might just be described by a fine-tuned low energy effective field theory.

2) SUSY does solve the hierarchy, as does technicolor (whether you think it looks "natural" or not, it certainly is technically natural). Technicolor is *highly* disfavored by precision data. SUSY is also disfavored by precision data, just less so. (This is called the "little hierarchy".) You can't think seriously about TeV-scale SUSY models without running into LEP data. There is *no* beautiful model that can solve the hierarchy problem, fit the data, and that doesn't have some ugly kludges and/or fine-tunings built in somewhere.

3) There is a nice model that has gauge coupling unification, dark matter, and no real difficulties with precision constraints. It's called split supersymmetry, and it does not solve the hierarchy problem.

4) String theory does *not* have much of anything to say about whether there is TeV-scale SUSY at this point. There seem to be lots of vacua with high-scale SUSY breaking. There are some approaches that claim to favor TeV-scale SUSY (e.g. claims that M-theory on G2 compactifications tends to have TeV-scale SUSY breaking once the cosmological constant is tuned to zero), but there doesn't seem to be any reason the theory would prefer those vacua.

47. **r hofmann**
May 18, 2007

Anti-Crackpot,

seems there was quite some indoctrination going on during your scientific education.

If you had ever deeply thought about 4D Yang-Mills and its relation to real physics (meaning ideas, concepts and quantitative predictions that are experimentally falsifiable) you would have had your answers concerning alternatives to the present doctrines for solving the hierarchy `problem´.

I wont react on any of your comments.

48. **Anti-Crackpot**
May 18, 2007

Anon,
I don't really understand why you are so upset. I only make the well-known point that supersymmetry is currently the best candidate for new physics and that it is clearly testable. I do not claim that it's the only possible solution, only that it's the most plausible solution. As for string theory, I guess I need to repeat that my arguments in favor

of SUSY have nothing to do with string theory.

r hoffman,
I have a Ph.D. in theoretical physics, having learned from some of the very best particle phenomenologists and string theorists in the world. I think I know what I'm talking about it and have no trouble with your empty statements.

49. **anon.**
May 18, 2007

It is quite possible for the world to be supersymmetric and string theory to not be right. … I'd like to hear real alternative ideas rather than just arguing … – Anti-Crackpot

Well, your pseudonym doesn't encourage me to believe your claim about wanting to hear '*real* alternative ideas'. String and SUSY have had 25 years of mainstream support to work out the details. The common problem with *any* alternatives is that they are haven't had 25 years of mainstream funding, interest, and development. So you'll be able to dismiss them by definition as non-mainstream, for being less well investigated than string and SUSY. There are several real alternative ideas but this blog is not a free for all discussion of them. The question is, if you're an anti-crackpot, why are you so keen on invisible superpartners which increase the tunable Standard Model parameters from 19 to 125 or more, without delivering any useful additional predictions in return.

The worst type of physics is that which like phlogiston, a wonderful solution to why burned things weigh less than unburned things, but is always totally useless for quantitative predictions, and finally turns out to have misled everyone. So if you're really believe you're an anti-crackpot, I'm afraid you may need to buy a new mirror.

50. **King Ray**
May 18, 2007

The true crackpots may be those who believe in string theory, supersymmetry and higher dimensions. Time will tell who the true crackpots really are.

51. **David B.**
May 18, 2007

Dear anon.

There is one prediction of the MSSM that you seem to be missing and it is the most important prediction that makes the model testable: the lightest Higgs has to have a mass that is lower than the mass of the Z particle at tree level. With loop corrections one can make this number bigger, but not too big. The model barely gets by with the current precision data and bounds.

Indeed, one of the most important reasons that made supersymmetric models appealing was that the higgs selfcoupling constant was
unified with the gauge coupling constants of the standard model.
Unfortunately because the MSSM does not break supersymmetry spontaneously, that was not enough to fit data. The extra parameters that are added to the MSSM to break susy in the most "general way that does not spoil the advantages of having supersymmetry" do not change this prediction.

If the higgs that is found at the LHC is not sufficiently light, then the MSSM will be ruled out. I find that to be quite a useful prediction.

52. **Anti-Crackpot**
May 18, 2007

Anon,
Since we do we claim that the supersymmetric partners are 'invisible'. On the contrary, they should be observed at LHC if supersymmetry is right. In any case, if your alternative ideas are so good, then I would suggest writing a paper on them. If they turn out to be right, then you'll win the Nobel. You should really refrain from lumping SUSY in with string theory. There is some relation between the two, but they are not entirely the same.

If and when supersymmetry is discovered, the following work will be to understand the mechanism by which it is broken. At that point, we will be able to make some predictions, such as the relic neutralino (assuming this is LSP) density and from that get a handle on the composition of the dark matter. Are there any of you who believe dark matter isn't real?

53. **Christine**
May 18, 2007

Are there any of you who believe dark matter isn't real?

> Science is facts; just as houses are made of stone, so is science made of facts; but a pile of stones is not a house, and a collection of facts is not necessarily science.
> Jules Henri Poincaré (1854-1912)
>
> There are in fact two things, science and opinion; the former begets knowledge, the latter ignorance.
>
> Hippocrates (460 BC – 377 BC)

I do not see how this is a question of believing or not.

BTW, see http://arxiv.org/abs/0705.2462

(sorry: html tag for the link seems not to be working in the preview)

54. **Christine**
May 18, 2007

BTW, I have worked for some time with dark matter models. So, as I said, it is not a question of believing or not.

55. **Anti-Crackpot**
May 18, 2007

Hi Christine,
Sorry, I was a little loose with my language. What I meant was, is there anyone who denies that there is strong evidence for the existence of dark matter? There is a lot of evidence for it coming from many independent directions such as galactic rotation curves, microlensing and CMB. If it does indeed exist, then this is another line of

evidence that the Standard Model needs to be extended since the dark matter cannot be baryonic or composed substantially of neutrinos.

56. **Walt**
May 18, 2007

Anti-Crackpot, you originally said "I think its likely that within two years, the particle physicists and string theorists will be sipping champagne, and all of the stupid people who think it will find nothing will dissappear into the vacuum." There's nothing in this statement about skeptics who spread information (whoever that is supposed to be); it's about the existence of skeptics at all. It's not a healthy attitude. Nature will turn out to do what nature does, whatever our aesthetic judgement tell us.

57. **M**
May 19, 2007

If supersymmetry will be found, "stupid people" will start working on it rather than sipping champagne or disappearing into the vacuum. However, supersymmetry missed a number of oppurtunities for showing up, and LHC is the last call. If supersymmetry will not be found, then those people who spent their life working on supersymmetry will be really upset.

Peter thinks that it is unhealhy that so many string theorists have been hired, and here we could have a similar situation: various phenomenologists built their career working on supersymmetry, and LHC might tell that their research activity was irrelevant. Anti-Crackpot, do you guess who would pay for this situation? I mean: if you are a joung post-doc who works only on supersymmetry, you should consider extending your research activity.

58. **anon.**
May 19, 2007

Christine: thanks for that link to http://arxiv.org/abs/0705.2462 which is important. The 'evidence' for dark matter and dark energy is based on mainstream consensus in a very weak way, like claiming that the existence gravity is evidence for M-theory because M-theory predicts gravity, or claiming that SUSY must be real because, if you don't have SUSY, the three SM force strengths won't naturally converge at the Planck scale.

59. **anon.**
May 19, 2007

There is one prediction of the MSSM that you seem to be missing and it is the most important prediction that makes the model testable: the lightest Higgs has to have a mass that is lower than the mass of the Z particle at tree level. … – David B.

Unless the model makes a prediction which is *unique enough* (which usually means *precise enough*) that no other theories are likely to make the same prediction, then the prediction is not a convincing test for the model. Even if the MSSM was the only possible SUSY, the confirmation of that prediction would still be dubious. If the MSSM Higgs isn't found then they can just find another SUSY model which is even harder to experimentally refute.

Similarly, in the case the string theory landscape, *individual* vacua are testable, since each has a definite set of parameters and therefore makes concrete predictions; the problem is that there are so many different vacua that it isn't falsifiable. A theory which comes in many versions, like epicycles or string and SUSY, is likely to contain superficially impressive models by sheer coincidence.

60. **Michele**
May 19, 2007

Is there a way to get your talk at FEST? I would have liked to be there but I missed it. Thanks.

61. **a quantum diaries survivor**
May 19, 2007

Speaking of SUSY at the LHC, Marcela Carena gave a nice seminar in CDF yesterday. You might want to give a look at two plots she showed about discovery reaches of a MSSM Higgs by Atlas and CMS, in my blog.

Cheers,
T.

62. **Anti-Crackpot**
May 19, 2007

Anon and Walt,
For some reason I keep having to make this point, but supersymmetry is testable directly as the supersymmetric partners of the known fermions and gauge bosons may be produced and detected in collisons at the LHC. The present situation is no different than the previous search for the top quark, which took twenty years to be discovered after the bottom. Why? Because of it's very large mass! Now, you guys, arm-chair scientists that you are, should be aware that such a large mass was anticipated by those working with no-scale supergravity and radiative electroweak symmetry breaking, where a large top mass is required to drive the Higgs mass-squared to a negative value.

63. **sinus**
May 19, 2007

> such a large mass was anticipated by those working with no-scale supergravity and radiative electroweak symmetry breaking,

Today there are models with Higgs masses large, small, and intermediate. Those are not really predictions, since there are no solid reasons which one of those models is experimentally correct, if any. Some of those models may even give Higgs masses in the correct range, and still be completely wrong.

To give another example, naive SU(5) gives essentially all the correct low energy physics, since it contains the SM, yet it doesn't work.

If radiative SB and supergravity models (of which there are many) were validated by the large top mass, as you seem to suggest, why is people wasting their time with other approaches that ostensibly failed to predict the top mass?

64. **Anti-Crackpot**
May 19, 2007

Sinus,
Regarding SU(5), there is one piece of low-energy physics on which it is spectacular wrong: neutrino masses. As far as the Higgs is concerned, it's mass is very sensitive to such things as the top mass and the type of soft-SUSY breaking that is assumed. In the models, one can change the top mass by a few GeV and this causes the Higgs mass to change a lot. In the minimal supergravity models (mSUGRA), the gaugino masses (m_{1/2}) and scalar masses (m_0) are universal, but their actual values are

unknown. The Higgs mass and low energy superpartner spectrum depends very much on these values.

65. **Haelfix**
May 19, 2007

Susy is still fine, but its getting perilously close to where its not fine anymore. Everyone hopes the lightest superpartner as well as the Higgs arises somewhere before 140 GEV or else we run into major phenomology problems elsewhere. Things become much less well motivated, nonminimal and contrived.

If we find SUSY at say 500 GEV, and nothing other than a bare Higgs scalar before that, everyone will be horribly confused.

66. **L-Train**
May 20, 2007

CW:

Thanks for your reply, but this blog may actually be a useful place to get some info on the topic of SUSY, and whether a transition to a SUSY state is possible at this point in the evolution of the universe, as predicted by Clavelli. You guys, after all, are debating about SUSY.

67. **Anti-Crackpot**
May 21, 2007

For the anti-stringers, there is a guest post at CV by Joe Polchinksi in reaction to Smolin's reponse to his earlier review.

68. **Peter Woit**
May 21, 2007

I'm still traveling, won't be back home until late Thursday. So, for now I won't write anything much about the Polchinski posting. In any case, it's mostly specifically an on-going discussion with Lee Smolin, and I know I don't like it if, when someone is arguing a question with me, someone else tries to be helpful by taking my side, often making arguments I wouldn't really agree with.

It's also true that Polchinski is just writing about quantum gravity, not about particle physics, where he's well known to be a proponent of the anthropic landscape point of view. I'm not too interested in getting involved in arguments about who has the better theory of quantum gravity that can't be tested. It would be interesting to see Polchinski try and defend the idea of 10/11 d string-theory based unification against the claim that virtually no predictions are possible, except vague sorts of statistical "predictions" that are often wrong.

69. **elzoro**
May 23, 2007

Off-topic but worth mentionning: Fields medalist Richard Borcherds (known among other things for his work on the Leech lattice, Monstruous Moonshine and axiomatic QFT) now has a blog http://borcherds.wordpress.com/

70. **Peter Woit**
May 24, 2007

Thanks elzoro,

Maybe soon every Fields medalist will have a blog…

This Week's Hype

May 24, 2007
Categories: Uncategorized

Still traveling, but will be back soon. This week's bogus "test of string theory" is described in a [NASA press release]() about three satellite-based experiments that would look for violations of the equivalence principle. From the press release:

…it could provide the first real evidence for string theory. String theory elegantly explains fundamental particles as different vibrations of infinitesimal strings, and in doing so solves many lingering problems of modern physics… The equivalence principle could offer one way to test string theory…

"Some variants of string theory predict the existence of a very weak force that would make gravity slightly different depending on an object's composition," says Will. "Finding a variation in gravity for different materials wouldn't immediately prove that string theory is correct, but it would give the theory a dose of supporting evidence."

…string theory makes a range of predictions about how strong this new force would be, so it's possible that the effect would be too small for even these space-borne instruments to detect.

Does string theory predict violations of the equivalence principle? From a [posting]() on Lubos Motl's blog:

In reality, it will probably be impossible to falsify string theory because string theory is probably correct and you can't ever falsify correct theories. 😉 *But if string theory were wrong, there would be thousands of ways to falsify it, even in the very near future. Although string theory predicts many new phenomena whose details are not uniquely known, it also implies that many old principles are exactly valid. If string theory is correct, the superposition principle of quantum mechanics, Lorentz invariance, unitarity, crossing symmetry, equivalence principle etc. are valid to much higher accuracy than the accuracy with which they have been tested as of 2006.*

If you believe that string theory is wrong, just prove any of the theories predicting all the bizarre phenomena like Lorentz symmetry breaking, breaking of unitarity, locality, rotational invariance, and so on. I think that all these things are badly motivated – but it's mostly because I know that it seems that they can't be embedded in string theory. If you don't believe string theory, you should believe that anything can occur and every new test of Lorentz invariance has a potential to falsify special relativity. Every new test has a potential to falsify the equivalence principle. And there are dozens of such examples. Without string theory, all these laws are approximate accidental laws and symmetries. I assure you that string theory will pass every new test of this type and its foes will always lose. String theory allows us to redefine what proposals about new physics are reasonable and what proposals are not, even without the exact knowledge of the vacuum.

I guess it's all right that I don't have time to comment on this, since no comment seems necessary…

Comments

1. **Chris Oakley**
   May 24, 2007

   This [paper]() from a 2005 conference on tests of the Equivalence Principle has a table (table I, p. 3) showing some experimental limits on deviations from the E.P.; briefly, these are – neutral bulk matter: 10^-12, atoms: 10^-9, neutrons: 10^-3 and charged matter (electrons): 10^-1. I guess that the new experiment (STEP) will be able to bring the "neutral bulk matter" limit down to 10^-17 – or not, depending on what it finds.

   The neutral bulk matter tests involve systems where the electrical charges are in balance to a much greater extent than one part in 10^12, and I wonder whether one could not just say "case proven" here and concentrate on the limit for charged matter instead. Such experiments, though, seem to be hard to carry out as they require the measurement of tiny deflections of single electrons due to gravity. See [here](), for example.

2. **anon.**
   May 24, 2007

   'I guess it's all right that I don't have time to comment on this, since no comment seems necessary…' – Peter Woit

   Glad that you are not wasting time on fruitless arguments.

   Lubos is correct. Physics proceeds by asserting a theory is true and must be believed until it is disproved. Even when the theory is disproved, you must continue using it until a better theory comes along. It is very arrogant of certain people to assert that a person's defense of extradimensional dogma isn't physics. You must first prove string wrong, and provide the correct theory to go in its place.

3. **Rick**
   May 24, 2007

   anon, it should go without saying that what you've said is nonsense. Science doesn't proceed via theories that -cant- be disproven nor bring much to the table.

   I'd say a theory can't be taken seriously unless it can propose a way in which it could conceivably be disproven, even if it has some amazing utility and explains many things. Unfortunately string theory fails all 3 of these tests so it's even more useless than a generally unfalsifiable theory.

4. **Me**
   May 24, 2007

   annon is obviously nothing but a troll.
   Don't waste time answering him.

5. **anon.**
   May 24, 2007

   'There is an unwritten precept in modern physics… which states that in physics "anything which is not prohibited is compulsory".'

   – O. Bilaniuk and E. C. G. Sudarshan, *Physics Today,* May, 1969, p43.

6. **Yatima**
   May 24, 2007

You must first prove string wrong, and provide the correct theory to go in its place.

Cough. Gentlemen, it seems to me that the word you are looking for here is “sarcasm”.

7. **anon.**
   May 24, 2007

   Yatima, take the situation of the procession of the perhelion of Mercury, which was a weak point in Newton’s theory for centuries (it was eventually cleared up by general relativity).

   The situation with string theory will be identical if it is found to disagree with experiments: you have to go on using string theory until a new theory comes along which reduces to string theory as an approximation. So even if it is experimentally ‘disproved’, we will continue to use string theory until we resolve the problem, which may take centuries!

   However, as Lubos points out, it is more probable statistically that the landscape is not entirely false. Think of the 10^500 vacua as a lottery. Become a string theorist, and it’s like having 10^500 lottery tickets: you own virtually conceivable theory going. *It’s a monopoly!* 😉

8. **russellman**
   May 24, 2007

   > The situation with string theory will be identical if it is found to disagree with experiments:

   I agree. Problem is: there’s every reason to suspect that ST will never be found to disagree, nor agree, with any experiment. That’s why it’s not (even) wrong.

9. **King Ray**
   May 24, 2007

   I think it is stretching the term theory to call string theory a theory. Apparently wikipedia agrees:

   http://en.wikipedia.org/wiki/Theory

   “The term theory is occasionally stretched to refer to theoretical speculation that is currently unverifiable. Examples are string theory and various theories of everything.”

   Also, from wikipedia,

   “In physics, the term theory is generally used for a mathematical framework — derived from a small set of basic principles (usually symmetries – like equality of locations in space or in time, or identity of electrons, etc) — which is capable of producing experimental predictions for a given category of physical systems. ”

   Where are string theory’s predictions? I don’t think string ‘theory’ is even a theory!

10. **Steve**
    May 24, 2007

    anon., I’m not sure if you understand how the scientific process works in physics, so let me just give you a quick run-down:

    (1) Someone sees something and guys “Hmmm…why did that happen?”
    (2) Another someone sets up a series of experiments and comes up with a bunch of data about what happened. Publish.
    (3) A theorist sees this data and goes “Hmmm…I wonder if I can figure out why this happened…” Using existing theories, said theorist tries to explain the data.
    (4) If every theorist decides “We dunno”, you start inventing a “new” theory.

    The burden isn’t on experiments to falsify theory, it’s on theorists to try to come up with an explanation of an experiment using existing, thus far unfalsified, theories. String theory reverses the roles. Physics isn’t dictated by who has the cooler theory, it’s dictated by what God decided the universe should look like, and what the experimentalists consequently see.

11. **anon.**
    May 24, 2007

    “Physics … it’s dictated by what God decided the universe should look like, …” – Steve

    Well, maybe your understanding of physics is about 500 years out of date. But thanks for your attempt to enlighten me 🙂

12. **DB**
    May 24, 2007

    It’s interesting that a number of fairly eminent members of the general relativity community have made soothing overtures to the string community of late. Clifford Will and Thibault Damour are names to be reckoned with in the field of experimental tests of general relativity. I expect they believe variants of string theory can in principle be tested in the same way as Dicke’s scalar-tensor theory of yore. However, I fear they are getting involved in an elaborate and expensive version of whack-a-mole. They will be welcomed with open arms by a community desperate for external validation.

13. **kuos**
    May 24, 2007

    If correct theories are not falsifiable, then by Lubos Logic it follows that non-falsifiable theories must be correct.

14. **King Ray**
    May 24, 2007

    I don’t know why string theory doesn’t just declare that it is all possible theories and then declare victory… It is getting close to that point now… another 10 years maybe and then they’ll do that.

    String theory reminds me more of a Fourier series expansion that can fit anything than a physical theory.

15. **Peter Shor**
    May 24, 2007

Rick says

I'd say a theory can't be taken seriously unless it can propose a way in which it could conceivably be disproven, even if it has some amazing utility and explains many things.

Isn't it the case that any theory which has amazing utility can be disproved. If a theory doesn't predict anything, then shouldn't it be considered (like Borges' Library of Babel) by definition useless.

16. **Chris W.**
May 24, 2007

I guess one should have expected Lubos to misrepresent the established usage (in discussions of the philosophy of science) of the word "falsifiable." (*Sigh*…)

17. **pathetic and obnoxious mug of vitriol**
May 24, 2007

Great post. If Motl's original text was absolutely hilarious, quoted in this context it's irrepressible so… A masterpiece of polemical counterpoint…

18. **mclaren**
May 25, 2007

> Lubos is correct. Physics proceeds by asserting a theory is true and must be believed until it is disproved. Even when the theory is disproved, you must continue using it until a better theory comes along.

This is wise and accurate. Physics proceeds via proof by assertion. Not only is no evidence required to demonstrate that any assertion made in physics confirms with observables, the very attempt to require evidence for any assertion in antiscientific and, indeed, anti-rational. The very essence of the rational skeptical mindset which characterizes the modern Western world is credulous belief in any unsubstantiated claim regardless of how many experimental observations it contradicts.

In other breaking new, the earth is cubical in shape, 12 = -1, and plogiston will soon replace gasoline in our cars, effectively ending the Peak Oil crisis.

19. **Cecil Kirksey**
May 25, 2007

Peter:

I hope this is not considered OT but it is somewhat related to the issue being discussed. Suppose someone wants to become a theoretical physicist. The person obtains a PhD at a good university with a well known advisor; then he/she is able to obtain several postdocs and finally he/she is able to receive a position at a top notch university or research center. My question is this: How does this person judge the success of their research career? By the number of papers published in PEER reviewed journals? By being a great teacher of up and coming other theoretical physicists? By obtaining research grants based on proposals reviewed by PEER occupied committees? By having their name appear in lay publications? Write a successful textbook or book for the layperson? Or win a Nobel Prize?

Other than the last criterion couldn't everything also apply to say a person who desires to do research in say medieval Germanic literature?

Without some connection to the real world (RW) how does one define the success of one's career as a theoretical physicist? Is it just PEER review?

20. **woit**
May 25, 2007

Cecil,

If you manage to get a tenured position at a top-notch university, you're more of a "success" as a theorist than 90 percent of the other people who got Ph.D.s in the subject. If you don't consider that "success" you're basically an insecure neurotic. Beyond that, once you have a permanent job allowing you to pursue what you want, all the different things you mention are possible things to work on and aspire to. Up to you to judge how well you succeed at what you try and do. Different people value different things, and the approval of certain very specific groups of ones peers is something different people care about to different extents.

21. **tomj**
May 25, 2007

I wonder if it is possible to concede a point to the string theorists: their theory is true and can't be falsified. So why don't we just move on. I don't see the point of a theory which cannot be falsified. If a particular experiment can neither prove nor disprove a particular theory, then why even discuss the theory with respect to the experiment?

My daughter was required to design an experiment for her 8th grade science class. She decided to measure the some ill-defined difference between skimmed milk, 2% and whole milk, by bring the different samples to a boil. The measured variable was time to boil. If she were to claim that this experiment falsified string theory, how would a string theorist show that the experiment was not applicable?

22. **Cecil Kirksey**
May 25, 2007

Peter:

Thanks for the reply. But I was more interested in the research part of being a successful theoretical physicist. How does one judge that aspect of their career? After all discovering a new theory of nature does not happen that often. So in this age of ST, LQG and other estoric ideas how does one define being a "success" other than position? Which is usually governed by how your PEERS judge you.

23. **Aaron Bergman**
May 25, 2007

I can't contain my curiosity. What exactly does P.E.E.R. stand for?

24. **Chris Oakley**
May 25, 2007

Aaron,

I think it is “Person Expecting Endless Rubbish”

25. **Coin**
May 25, 2007

In other breaking news… 12 = -1

Ah, Z13.

26. **Simplicissimus**
May 25, 2007

When I clicked the link in “From a posting on Lubos Motl’s blog”
(which refers to http://motls.blogspot.com/2006/10/falsifiability-in-physics.html)
I arrived at http://www.physics.harvard.edu/~motl/crackpot-not-even-wrong.html.
I’m not familiar with string theory. But I am familiar with standards of civilized communication. Is this the famous Harvard university?

27. **Eugene Stefanovich**
May 25, 2007

Cecil:

Medieval scholars designed ingenious ways to count the number of angels dancing on the head of a pin. No doubt, they earned respect from their peers, well-paid positions, and all that… at about the same time when Galileo and Giordano Bruno were grilled by inquisition, one of them quite literally. So, in those times “success” and “progress” were not synonyms. Do you think they are synonyms now?

28. **Cecil Kirksey**
May 25, 2007

ES:
That seems to be the issue.

I guess I need to be more more clear. If you are a theoretical physicist and do not contribute to a theory that makes connections to the RW can you be successful? Yes or no? Exactly what does a theoretical physicist do if there are data to support his or her pet ideas? You can teach, but what about the research? Or maybe theoretical physicist needs a different definition. (Of course I am not sure what a theorteial physicist actually does so maybe someone who is such an animal can define his or her job description.)

29. **anon.**
May 27, 2007

‘If you are a theoretical physicist and do not contribute to a theory that makes connections to the RW can you be successful? Yes or no?’ – Cecil

That’s the same as asking if string theorists are successes. Consider dictators who think they have the answer to everything, and suppress dissent. Fellow dictators may respect them, and financially they may be a success. That’s all that counts to them.

What would be a failure in their book is someone like Mallory who set off to climb Everest in 1924 and didn’t return. A ‘success’, in the dictator’s book, is a label attributed by others to someone who makes a vast fortune and acquires power and through that power, respect (though that may be respect through fear, rather than respect from genuine admiration).

Newton and Darwin both came up with their discoveries and held back from publication for decades until they had built up a convincing set of evidence to support them. They didn’t build careers on the back of revolutionary discoveries. Copernicus wrote his book when he was an old man. String theory is the very opposite of this tradition; it’s got no solid evidence yet it is deemed a popular success because it has acquired mainstream credibility.

30. **philippe**
May 29, 2007

Since money is always involved, I tend to see theoretical physics as a big fashion show. You can’t tell what is or isn’t successful before a substantial amount of time or generations have passed. I don’t think anybody can claim enough hindsight at present to carefully judge the situation in theoretical physics or the “success” of anything in a less relative way. Let the fashion show continue…and its models trip.

The Empire Strikes Back

May 25, 2007
Categories: Uncategorized

After last month's posting at Cosmic Variance about how String Theory is Losing the Public Debate, Sean Carroll seems to have decided to go on the offensive (or defensive…), with a piece in New Scientist entitled String theory: it's not dead yet, which he reproduces and has a posting about here.

I can't really disagree with Sean about either title. Yes, string theory is losing the public debate, and no, it's not dead yet. Some of Sean's claims in the New Scientist piece are descriptive claims about the behavior of theoretical physicists:

String theorists are still being hired by universities in substantial numbers; new graduate students are still flocking to string theory to do their Ph.D. work…

Ideas about higher-dimensional branes have re-invigorated model-building in more conventional particle physics… Cosmologists thinking about the early universe increasingly turn to ideas from string theory.

All of these are true enough (although the word "re-invigorated" might not be the most appropriate one), but don't address the value judgment of whether any of this activity is a good thing or not. One could also come up with other evidence for continuing activity in string theory, such as the large number of press releases being issued claiming to have found new ways to "test string theory", but the fact that these have all been bogus is relevant to evaluating whether this activity is a good thing or not.

Sean's positive case for string theory is mostly about its role as a quantum gravity theory, acknowledging that the Landscape is a problem, and that progress has slowed since the mid-90s (although more accurate would be "come to a dead halt, now moving backwards.."). He describes that period as "it seemed as if there was a revolution every month", displaying the predilection for over-the-top hype that has characterized much string theory salesmanship over the years. His claims about the achievements of string theory vary from relatively modest exaggerations ("The theory has provided numerous deep insights into pure mathematics") to standard misleading propaganda:

"a promising new approach has connected string theory to the dynamics of the quark-gluon plasma observed at particle accelerators" (connected? wonder how strong the connection is…)

"it is compatible with everything we know about particle physics" (and also compatible with just about everything we know to not be true about particle physics…)

"Michael Green and John Schwarz demonstrated that string theory was a consistent framework" (there's a lot more to consistency than canceling that anomaly…)

"It was realized that those five versions of the theory were different manifestations of a single underlying structure, M-theory" (would be nice if we knew what M-theory actually was…)

In the comment section Sean explains how string theorists have no intention of standing behind what used to be considered the main "prediction" of the theory, TeV-scale supersymmetry:

If the LHC discovers supersymmetry, string theorists will be happy, but if it doesn't there's no reason to give up on string theory — the superpartners might just be too heavy.

So, prospects for string theory remain bright, since with each new experiment the situation is: heads they win, tails doesn't count.

Also at Cosmic Variance is the latest in an exchange between Joe Polchinski and Lee Smolin, entitled Science or Sociology? (some earlier parts of the exchange are here). I'm mostly resisting the impulse to get involved in various parts of that argument since Smolin doesn't need my help: the points at issue don't seem to me central to the claims of his book, and his positions and what he wrote in the book are perfectly defensible.

While I don't see the point of arguing about things like how conjectural the AdS/CFT duality conjecture is (pretty damn conjectural I'd think though, since no one even knows what the definition of one side of the duality is…), it is interesting to see what it is that Polchinski finds most objectionable about Smolin's criticisms. In the context of an argument about how much of a problem the positive CC was considered to be by string theorists in the late 90s, he strong objects to Smolin's description of "a group of experts doing what they can to save a cherished theory in the face of data that seem to contradict it", going on to describe the work on moduli stabilization that led to the landscape as "a major success" which Smolin is trying to paint as a "crisis". Ignoring the argument about who thought what back then (although if you really care about this, for some relevant evidence, see the Witten quote), in a larger sense "a group of experts doing what they can to save a cherished theory in the face of data that seem to contradict it" describes precisely the behavior of Polchinski, Susskind, Arkani-Hamed, and many others in the face of the disastrous situation created by the "major success" of moduli stabilization.

The "anthropic landscape" philosophy is nothing more than an attempt to evade failure, and it is an failure of scientific ethics of a dramatic kind. Once one understands a speculative idea dear to one's heart well enough to see that one can't make any conventional scientific predictions using it, ethics demands that one admit failure. Instead we've seen scientists announcing a new way of doing science, even writing popular books and magazine articles promoting this. Most physicists (including even a sizable fraction of string theorists) are appalled by this behavior. If you don't believe me, consult a random sampling of the faculty in your nearest physics department, or watch Susskind's recent talk in Israel where he describes himself as at the center of a circular firing squad.

Polchinski ends by claiming that Smolin's case for "group-think" and for a "sociological" problem with string theory is "quite weak". This problem is obviously hard to quantify and a matter of perspective. While I don't doubt that Polchinski sees himself as not suffering from "group-think", if he were, he obviously wouldn't think so. One thing I think is undeniable about the "sociology" of all this is that the blog phenomenon has put a lot of evidence out there for any unbiased observer to judge for themselves, and this is one of the main reasons for what even a fervent string theory proponent like Sean Carroll has noticed: string theorists are losing this debate.

Anyone who regularly follows the most well-known blogs run by string theorists pretty soon becomes convinced that they have a real problem. Lubos Motl is the Id of string theory on uncensored display. The fact that his colleagues promoted him and show signs of only having a problem with his politics, not his behavior as a scientist (if they have any problem with his calls for my death or other attacks on me, I've never seen evidence of it) is truly remarkable. Two out of three recent string theory textbooks prominently carry his endorsement. All another prominent string theorist blogger, Clifford Johnson, has to say about Lubos is "I thank him for his physics contributions and for widening the discussion." This was in the context of an eight-part personal attack on Lee Smolin and me for having written books that Clifford steadfastly refuses to read. The other of the three prominent string theory bloggers is renowned for his sneering attacks on the competence of anyone who dares to criticize string theory, issues press releases claiming tests for string theory that other physicists describe as "hilarious", while misusing his position of responsibility at the arXiv to stop links to criticism of string theory articles from appearing there. Among those string theorists without their own blogs who choose to participate in the comment sections of others, a surprising number seem to think that it is an ethical thing to do to post often personal attacks on string theory critics from behind the cover of anonymity. Less anonymously, a large group of string theorists at the KITP seem to have thought it was an intelligent idea to act like a bunch of jeering baboons, on video, for distribution on the web.

This kind of public behavior and the lack of any condemnation of it by other string theorists is what has convinced many physicists and others that, yes, string theory does have a "sociological" problem. I have to confess that my experience over the last couple years has caused me to come to the conclusion that the string theory community has a much greater problem with personal and professional ethics than I thought when I wrote my book. The fact that so many string theorists have decided to respond to my book and Smolin's not with scientific arguments, but with unprofessional behavior I think speaks volumes for the strength of their scientific case, and this has been noticed by their colleagues, science journalists, and the general public. While I applaud Polchinski for behaving professionally in his response to the two books, I suggest that he should take a look at the behavior of many of his colleagues and ask himself again whether or not there might be a sociological problem here.

Comments

1. **kasper Olsen**
   May 25, 2007

   Dear Peter,

   Who cares, if we physicists are losing the (public) debate?

   At some points it has seemed, that real scientists are also
   losing the debate in the US over the Intelligent Design movement.

   Progress will not — in the end — be driven by sociology, or superstition.
   But instead by something completely different. This something is what is usually called "truth"….

   -Kasper

2. **Coin**
   May 25, 2007

   relatively modest exaggerations ("The theory has provided numerous deep insights into pure mathematics")

   Do you disagree with that statement as written then?

3. **Domenic Denicola**
   May 25, 2007

   This was quite a polarizing post. And yet from what I've seen, I can't really disagree with any of it.

   Coin, re: pure mathematics: I think the issue may be one of degree. I don't know if "numerous" and "deep" are the appropriate qualifiers here. Although I'm not knowledgeable enough to say for sure, I think that string theory really hasn't contributed all *that* much to mathematics, at least from the perspective of mathematicians who are actively working toward "numerous deep insights" instead of developing new tools mathematical tools as needed.

4. **Peter Woit**
   May 25, 2007

   Kasper,

   What's going on is not "physicists losing the public debate", but string theorists losing the debate among physicists. You might try asking your non-string theorists colleagues their opinion of string theory these days…

   Coin,

   While string theory has contributed to mathematics in various ways, as I wrote, I do think that saying it has provided numerous deep mathematical insights is an exaggeration.

5. **J**
   May 26, 2007

   Peter,

   The problem here is that lots of non-string theorists know very little about string theory and just follow the trend of "hating" string theory. At least I konw many people of this type. Nevertheless, I agree with you that string theory is in big trouble and some string theorists are too biased about it.

6. **Peter Woit**
   May 26, 2007

   J,

   Sure, few people actually understand much about string theory. In the past the hype surrounding the subject led to a lot of people being very impressed by it. As it has become clear that much of this was hype, and the problematic "sociology" becomes more manifest, this is leading to a backlash and to people "hating" string theory. "Hating" isn't a reasonable reaction to a scientific theory, but it is a reaction to the hype and to the behavior of some string theorists.

7. **outsider**
   May 26, 2007

   …how conjectural the AdS/CFT duality conjecture is (pretty damn conjectural I'd think though, since no one even knows what the definition of one side of the duality is…)…

   How can they talk about this duality if they don't know both sides? What do string theorists say if they explain the duality??

8. **Aged String Theorist**
   May 26, 2007

   Peter….

   wheeze-exhale

   I ..am ..your ..father!

   wheeze-exhale

More seriously, I agree with you on the fact that the statement that string theory "is a fundamental theory of physics" is "not even wrong", but to suggest that string theory is completely useless (I don't know, maybe I am taking you out of context here) is perhaps a bit of an over exaggeration. I am sure that there are many stringy inspired pieces of mathematics that will be of great interest to mathematicians for years to come, and potentially many more reserves of mathematical complexity that have yet to be tapped. Though I must admit that I could be wrong here, I think certainly one has to acknowledge that there is a whole community of theorists working in the area that think that there is definitely SOMETHING there, so, maybe there is! Just maybe not the fundamental "theory of everything" that has been (dishonestly?) hyped in the media. Something to think about anyway.

9. **Math Grad Student**
May 26, 2007

As someone who knows little about the physics involved, but a lot about the math (I'm finishing my Ph.D. in representation theory & algebraic geometry this summer), I can say that from my point of view Peter is completely right about physicists losing the argument from a sociological standpoint. I'm a long-time reader of this blog as well as other physics blogs and it has been amazing to see Peter (as well as Lee Smolin elsewhere) being unflappably polite while being subject to the most absurd ad hominem and personal attacks. I can't speak to the substance of the posts arguing physics, but when string theorists claim that Peter has no authority since he isn't publishing academic papers (as though this has any bearing at all on his arguments) or call him a crackpot or worse, it only makes them look bad. Add to this the vicious postings by string theorists on their own sites that don't address the substance of Peter's arguments but only hurl insults in his direction, and consider Lubos' quite unhinged nature — my own personal favorite post of his is the one where he claims women can't do physics or math — and it's no surprise string theorists are losing on the public relations front.

As for the claims of physicists that string theory has greatly enhanced mathematics, I can only say that I am unaware of this impact in my field. I know that string theory has had an impact on enumerative algebraic geometry (eg work of Kontsevich on curve counting) but it's not valid to claim that the impact has been huge. In fact it seems like string theorists are taking the shiniest new baubles from mathematics and claiming them as part of string theory. A case in point is new claims that geometric Langlands has import in string theory — maybe I'm off base here, but can anybody tell me how the hell, say, Hecke eigensheaves on a moduli stack have anything to do with string theory? Undoubtedly QFT has had an impact on representation theory, especially the infinite-dimensional flavor, but string theory? Color me unconvinced.

10. **Peter Woit**
May 26, 2007

Aged String Theorist,

I'm quite sure I've never claimed string theory is "completely useless". It's a huge subject, many parts of which remain worth pursuing. But the idea of unifying physics with 10/11 d string/M-theory really has pretty conclusively failed, and string theorists should start publicly acknowledging this instead of allowing Landscape pseudo-science to flourish and continuing to promote misleading hype about the theory.

The relation of string theory to mathematics is a really complicated subject, with one reason for this being that a big part of the story is 2d conformal field theory, which is an independent subject. Just as it would be a really good idea for physicists to think clearly about what string theory ideas have failed and which have led somewhere, the same is true about the interaction of string theory and mathematics. Hype about "numerous deep insights into pure mathematics" isn't really helpful.

11. **Aaron Bergman**
May 26, 2007

I think it's a pretty silly exercise to bicker about the *extent* to which string theory has influenced mathematics. In certain fields (Gromov-Witten theory, for example), string theory has inspired many results; in others not so much.

To answer the specific question, the moduli stack can be approximated by the Hitchin moduli space which arises when placing the equations of a particular 4D TQFT on a Riemann surface. (It's been argued that working on the stack is equivalent to working in the full 4D context, but I don't think that's been made precise.) One then gets a 2D model of maps into this moduli space. Using ideas from topological string theory, we can identify boundary states of this field theory with objects in the derived category. There are specific operators in the 4D theory called 't Hooft loops which operate on boundary states and thus give functors on the derived category. The Hecke eigensheaves are the same things as "eigenbranes".

12. **Eric Mayes**
May 26, 2007

Peter,
As far as I can tell, the only people who believe that string theory has failed are you and your accolytes. This certainly is not the opinion of most high-energy physicists and you should stop misrepresenting this fact. Basically, this a pseudo-controversy created by you and others, just as creationists try to give the impression that there is a controversy over evolution. It's irritating, but in the end irrelevant.

13. **Peter Woit**
May 26, 2007

Aaron,

Referring to the 2d sigma model involved as a "string theory" seems to me to be rather gratuitous. You're not summing over surfaces, you're not summing over metrics, it's an interesting TQFT with little relation to physical string theory. As I wrote in the previous comment, it really would be useful to keep straight what is what, and not try and muddy the waters. The QFT/Langlands story is about gauge theory and TQFT, not about string theory.

Eric,

You really should get out more…

14. **Aaron Bergman**
May 26, 2007

Referring to the 2d sigma model involved as a "string theory" seems to me to be rather gratuitous.

I wasn't aware I did that.

15. **Peter Woit**
May 26, 2007

Aaron,

"Using ideas from topological string theory, we can identify boundary states of this field theory"

It requires fairly close reading of this to realize that there's no string theory here, just a use of "ideas from topological string theory", which aren't about string theory.

16. **Eric Mayes**
May 26, 2007

Ok, Peter, say that in a few years when you've been completely forgotten, and all of the emnity you've created comes home to roost.

17. **Aaron Bergman**
May 26, 2007

Ummm, yeah. Ok.

18. **Math Grad Student**
May 26, 2007

. . . so am I understanding this correctly, that geometric Langlands really pertains to QFT and less to string theory? I'm already aware that there are connections to QFT (cf Edward Frenkel's notes on the topic).

As for Eric Mayes' comments, they are a classic example of what Peter is talking about — even though I don't know a lot about evolutionary theory it's patently obvious that intelligent design and arguments against evolution by religious nuts are unscientific. However, although I don't know the physics involved, it is NOT patently obvious that Peter is being unscientific; in fact it doesn't seem like that's the case at all. It's pretty great that Eric has come along to underline precisely the point that Peter makes, which is that ad hominem attacks have taken the place of reasoned conversation, and that this is making string theorists look pretty bad to those of us who don't know physics on a deep level.

19. **woit**
May 26, 2007

Math Grad Student,

Witten (with Kapustin and Gukov), over the last few years has been exploring the relation between certain 4d topological QFTs and geometric Langlands. He relates the 4d TQFT to 2d TQFT based on maps from a 2d space with boundary (such as the upper half plane) to the Hitchin moduli space of bundles over a Riemann surface. Langlands duality then is related to mirror symmetry between two such TQFTs.

This is one sort of relation of QFT to Geometric Langlands, but there's another quite different one, that is explained in many of Edward Frenkel's articles and lectures. There use is being made of ideas from conformal field theory (and especially vertex operator algebras) to do explicit constructions in geometric Langlands. As far as I know the relationship between these ideas and Witten's newer ones is not understood, but maybe someone better informed than me knows something about this. If so, I'd love to hear about it…

20. **Aaron Bergman**
May 26, 2007

so am I understanding this correctly, that geometric Langlands really pertains to QFT and less to string theory?

Yes. As I said (which apparently Peter thought was deeply hidden in my opaque prose), some ideas that had their origin in string theory have gone into the analysis, but all the theories are topological QFTs. Strangely enough, these do not seem obviously related to the CFT stuff that Frenkel discusses in his notes.

In regards to the increasingly tiresome string PR wars, I'd suggest reading various more formal reviews of the relevant books as opposed blog comment threads which involve a vanishingly small fraction of the community.

21. **tomj**
May 26, 2007

One question which keeps coming to my mind is if physicists believe that you have to fully understand (or even partially understand) their theory to discuss certain grand ideas in scientific thought.

One is Occam's razor. One arm of this principle is that a new theory must fully describe the same results as another theory, with less assumptions or somehow more simply.

Another principle is the ability to predict results prior to experiment. This seems important because if the theory doesn't predict results, it is impossible to argue that the theory even applies to the experiment.

Underlying both of these principles is the bedrock assumption: physical experiments are required, and any theory, or any part of any theory, which does not deal directly with the observable world is by definition un-necessary, it is dead weight, and must be discarded using Occam's razer.

What I'm getting from the string theorist's side of the debate is that their theory is more important than these principles, so the principles themselves must be incorrect. It's a neat trick.

22. **Math Grad Student**
May 26, 2007

I see. Thanks for the information on TQFTs — I have Frenkel's paper and I'm planning on reading through it. It's good to have some sort of rough overview of the picture when reading about the interface between mathematics & physics, which I often find confusing. Frenkel's book on geometric Langlands & loop groups comes out in a month also, and I'm hoping it'll be readable — the preprint version of the book from his website included a forward in which he said that the book should be accessible to a bright undergraduate or beginning graduate student, which made me feel kind of dumb.

23. **woit**
May 26, 2007

Math Grad Student,

Frenkel's comment about that book being accessible to someone with an undergrad math background is one of the funniest things I've seen in the math literature. That he might be serious about it is one of the scarier things…

24. **Aaron Bergman**

May 26, 2007

The deal is this:

1) Quantum mechanics and general relativity are incompatible
2) There are currently no experiments that probe regimes where this incompatibility is important.

Given (2), there are two options. The first is just to not work on number one. This applies to string theory, loop quantum gravity, causal dynamic triangulations and any other theory you can think of. None of them had made anything remotely resembling a falsifiable prediction.

The thing is, people don't having a problem in front of them and not working on it. So people have their favorite ideas for working on (1). Most people who have thought about these questions think that string theory is the best of the various ideas out there. And so they work on it.

25. **Tom**
May 26, 2007

In the end, none of this matters. The LHC will turn on and either end the debate or end particle physics. If we find something, it will clarify the direction theory needs to head. If we find nothing, we will all be working at hedge funds. Personally, I'm hoping for the latter. I've always wanted a sailboat.

26. **Intellectually Curious**
May 26, 2007

Math Grad Student wrote:

"it's patently obvious that intelligent design and arguments against evolution by religious nuts are unscientific."

While it is true that some arguments by some 'religious nuts' are unscientific, it is not true that intelligent design and (all) arguments against evolution are unscientific. There are plenty of reputable scientists who believe that the universe was designed and created, and whose reasons against the evolution theory are based on scientific facts. See here for a list.

At any rate, I thought the comment was irrelevant to the current discussion and sounded like a 'cheap shot' coming from certain string theorists :-), which was out of character of your other posts. (Your first one, for example, was fair, unbiased, and well-argued.)

27. **Peter Woit**
May 26, 2007

Any further attempt to use this blog to carry on the ID vs. evolution debate will be ruthlessly suppressed. Don't even think of it…

28. **Coin**
May 26, 2007

Frenkel's book on geometric Langlands & loop groups comes out in a month also, and I'm hoping it'll be readable

Does "loop groups" in this context mean the same thing as holonomy groups?

29. **Peter Woit**
May 26, 2007

No,

a loop group is just the group of maps from the circle into a group, with point-wise multiplication.

The holonomy group is something different. Given a connection, it's the group generated by parallel transport around a loop.

30. **Coin**
May 26, 2007

I see, thank you.

31. **Amos Dettonville**
May 27, 2007

It's interesting that while string researchers themselves have the strong impression that trememdous progress has been made at various times (e.g., "it seemed as if there was a revolution every month" in the mid 90s), from an outsider's perspective there appears to have been no progress or change at all, at least for the past 20 years or so. I base this largely on the popular book "Superstrings, A Theory of Everything", edited by Davies and Brown, published in 1988. The book (in case anyone here hasn't read it) is a fascinating collection of interviews with prominent string researchers, conducted in 1987, exactly twenty years ago. Those interviewed were Schwarz, Witten, Green, Gross, Ellis, Salam, Glashow, Feynman, and Weinberg.

Re-reading this book recently, the overwhelming impression I got was that absolutely nothing has changed in the past twenty years. In fact, it seems to me that the popular chronology of string theory (first revolution, second revolution, etc.) is wrong, or at best misleading, because already in 1987 we find every aspect of the present discussion, including the unification of the various versions into a single over-arching theory, the idea of invoking higher-dimensional surfaces (instead of just one-dimensional strings), the huge number of possible vacuum states due to the various ways of curling up the extra dimensions, the prospect of needing to appeal to the anthropic principle as a way of choosing between them, the possibility of formulating the theory in four dimensions from the start, the issue of "background independence", the difficulty with the cosmological constant and the fact that we don't know how to break supersymmetry without producing an excessively large CC, acknowledgement of uncertainty about whether it has really been fully proven that string theory will be completely finite in every sense and to all orders, and the over-riding lament that "we don't understand what string theory is", and we seem to be missing some fundamental principle. And so on.

There was also a discussion of the sociological issues, noting the "totalitarian" aspects of the string research community, and questioning whether it's a good idea to focus so much one speculative idea. The book even contains a tinge of "flame war": The interviewer mentioned that Feynman was quite critical of string research, and Green began his response by saying "I would not have thought that this was the kind of approach to physics that Feynman would favor". (How did that get past the editors? It's the only ad hominen comment in the book.)

It may be that experts in string research could "date" those interviews, but my guess is that most people would find them indistinguishable from blog discussions typed this morning. (Just about the only give-away is that they say "the LEP stands a good chance of detecting the Higgs", whereas now one says "the LHC stands a good chance of detecting the Higgs".) And yet, as I said, the string researchers themselves have the impression that they have been progressing (at various times since 1987) at breakneck speed. I can only assume that the research has generated several internal problems that have been tackled and resolved, giving workers in the field the impression of progress, despite the fact that, in overall terms, the program hasn't budged since 1987.

Here's a nice succinct question and answer.

Interviewer: It seems at the moment that this must be a rather major obstacle to further progress in the theory – not knowing how the higher dimensions curl up.

Witten: We would be much happier if we understood how the higher dimensions curl up and therefore what the vacuum state of the theory is.

32. **marcus**
May 27, 2007

Amos Dettonville,
Thanks for the pointer to that 1987 book. It was recently republished in an attractive-looking paperback edition, I see.
http://www.amazon.com/Superstrings-Everything-P-C-Davies/dp/052143775X

It is available new for $20 or so, (not counting shipping) and I see that used copies are for sale for $0. 47 on the same basis. How can anybody resist an up-to-date book on string theory for 47 cents?

Though others may have missed your reference to 1986 anticipation of M-theory, I think that must be what was meant here:

…the popular chronology of string theory (first revolution, second revolution, etc.) is wrong, or at best misleading, because already in 1987 we find every aspect of the present discussion, ***including the unification of the various versions into a single over-arching theory****, the idea of invoking higher-dimensional surfaces (instead of just one-dimensional strings), the huge number of possible vacuum states due to the various ways of curling up the extra dimensions, the prospect of needing to appeal to the anthropic principle as a way of choosing between them,…*

33. **ali**
May 27, 2007

Hi Peter,
I have a kind of off-topic question. Do you have any idea why DARPA is funding geometric langlands program? I mean, it is a military agency and I cannot see the motivation behind it. I am not in this field but this is pure math as far as I can see

34. **LDM**
May 27, 2007

If Sean's claim

String theorists are still being hired by universities in substantial numbers; new graduate students are still flocking to string theory to do their Ph.D. work…
is true, then the perceived loss of the public debate is moot -it would still appear to be business as usual. I only care about the public opinion in this debate to the extent it can influence the allocation of academic and research funding…

If somebody wants to study string theory on their own — fine, just don't ask the tax payers to support their delusion under the guise of conducting physics research. Get a philosophy grant instead.

.

35. **Cecil Kirkey**
May 27, 2007

I would like to second Amos. I have had the book in question for several years as well as other string theory books (sorry Peter I have NOT yet bought your book but intend to). I can maybe see why some researchers would really be interested in ST, but how to get rid of all those extra dimensions!!!

Aaron:
It maybe the "only game in town" but could it just be a suckers game? Just asking.

36. **Aaron Bergman**
May 27, 2007

String theory could certainly not be the correct theory of quantum gravity. It's lucky, then, that it has inspired many new ideas in the field of phenomenology and mathematics. In addition, the AdS/CFT conjecture gives us a new way to understand gauge theories. It might be disappointing if gauge/geometry duality is the only connection between string theory and more traditional physics, but it is still a remarkable discovery that deserves further investigation.

37. **Peter Woit**
May 27, 2007

ali,

From the DARPA web-site:

http://www.arpa.mil/dso/thrust/math/fathm.htm

I think DARPA has traditionally been willing to sometimes fund basic research whose possible military applications are pretty far-fetched. This seems to be a pretty extreme example.

38. **David Ben-Zvi**
May 27, 2007

Peter,
There are certainly relations between the CFT type
approach to geometric Langlands and the 4d TFT one –
I think most of the fundamental structures in geometric
Langlands can (or will) be seen from this POV.

I think the key to this is understanding the Kapustin-Witten
TFT in codimension three, i.e. on the circle, but sadly I think
neither the algebra nor the TFT (I self-censored after saying "math" and "physics" 🙂) is sufficiently developed to make such statements precise. I'll just say that both sides are pretty definitely about loop groups.

In fact I think 2d gravity/topological strings will soon
become relevant to the geometric Langlands story —
at the very least the "space of string states" seems to appear when you try to relate representation theory of loop groups to that of groups..

39. **LDM**
May 27, 2007

Peter and Ali,

DARPA of course has zero interest in string theory as a physical theory…It is only interested in any mathematics that string theory may have influenced and whether or not such mathematics can be used in defense:
"The fundamental mathematics developed in this program is expected to have broad significance in basic science and several avenues of possible long-term defense impact, including quantum algorithms and devices; cryptography; fast structured algorithms for signal/image processing and other DoD-critical applications; and high-density data coding"

Public key encryption as a secure cryptosystem relies on the mathematical assumption of the difficulty of factoring large numbers — a difficulty which apparently has never been proved. Hence the DARPA interest in number theory and Langlands.

Quantum algorithms and devices (mentioned by the DARPA link) refer to quantum computers (and also quantum key distribution)—the development of quantum computers would give an exponential increase in factoring speed , rendering public key encryption broken.
On the other hand, a successful development of quantum key distribution would solve the problem of secure distrubition of encryption keys, which would immediately make the one time pad cryptosystem (known to be secure) practical.

The DARPA funding looks reasonable.

40. **ali**
May 28, 2007

LDM, thanks for the insight. I know NSA is also very interested in quantum computers too for obvious reasons. I just did not know geometric langlands program could have any such practical realization. I am not in this field

41. **Professor Doctor Galileo Galilei**
May 28, 2007

Galileo lost the public debate.

In much of the US so did Darwin.

Not sure the public debate is very relevant for sciencitific questions.

42. **anon.**
May 28, 2007

What I don't like about military funding of science is that if anything interesting comes up, it will be classified secret to stop enemies knowing about it. This probably won't stop the enemies finding it, because you always get leaks from the Dr Klaus Fuchs sort, but it will prevent the interesting applications being known widely.

43. **changcho**
May 28, 2007

"…but it will prevent the interesting applications being known widely.' Perhaps, but only for a little while…

44. **mclaren**
May 29, 2007

String theorists are still being hired by universities in substantial numbers; new graduate students are still flocking to string theory to do their Ph.D. work…

Ideas about higher-dimensional branes have re-invigorated model-building in more conventional particle physics… Cosmologists thinking about the early universe increasingly turn to ideas from string theory. — Sean Carroll

All of these are true enough (although the word "re-invigorated" might not be the most appropriate one), but don't address the value judgment of whether any of this activity is a good thing or not. One could also come up with other evidence for continuing activity in string theory, such as the large number of press releases being issued claiming to have found new ways to "test string theory", but the fact that these have all been bogus is relevant to evaluating whether this activity is a good thing or not. — Peter Woit

Fascinating. When I made essentially the identical point in a comment on the Cosmic Variance blog, Sean Carroll deleted my post.

In particular, I went on to make a further point in the comment which Carroll deleted. Namely, that there's nothing radically new about the peculiar properties of string theory as science. Irving Langmuir called this kind of activity "pathological science," and he gave a famous lecture on the subject:
http://www.cs.princeton.edu/~ken/Langmuir/langmuir.htm

Examples of pathological science include Blondlot's N-rays, mitogenetic rays, the Davis-Barnes effect, and the Allison effect. I would add to that list phlogiston, alchemy, hard AI, and the luminiferous aether. (Some will doubtless disagree about hard or GOFAI, but the evidence there seems pretty clear after 50 years. Look at the CYC project for a classic example.)

Sean Carroll claimed that string theory is unique because it has gone on so long without getting experimentally tested in such a way as to definitely disconfirm it, but that's actually not correct. Each of the above examples of pathological science never got an experimental disconfirmation which proponents of the theory accepted as definitive. The supporters of N-rays, for example, tried to wriggle out of Wood's report by claiming that N-rays don't operate according to Snell's law, that Wood didn't know how to read the scintillations properly, that Blondlot was tired during that particular experiment, etc.

We hear exactly the same kinds of excuses today whenever new data places a lower bound on the possible mass of the superpartners. I.e., the string supporters simply move the bar.

There's nothing new about any of this. We see this in pathological science for at least the last 100 years, and if you count phlogiston, going back several hundred years.

The scientists who came up with phlogiston weren't stupid. There were superificially convincing reasons to believe in it — metals lost weight when heated, gained weight when heated. It seemed credible. (Turned out to be oxides, not phlogiston.)

There are likewise some superficially convincing reasons to believe in string theory, particularly the mathematical ones. The problem is that regardless of how convincing it might seem, string theory hasn't generated testable falsifiable experimental predictions.

One of the biggest problems with string theory is that (like all patholgoical science) it has not been fertile in a scientific sense. It *has* proven mathematically fertile. Lots of new and wonderfully fascinating math has come out of it. But after 30 years, no new areas of physics have been opened up to investigation by string theory and explored with experiments. This is very different from the case with, say, quantum theory. Initially, quantum theory merely offered a way of explaining the absorption lines of the simpler elements like hydrogen and helium, in particular the Ballmer series. But then quantum theory got applied to crystalline solids and you got band-gap theory of semiconductors, and that led all sorts of fascinating research and a wealth of new experimental results. Likewise, LInus Pauling applied quantum mechanics to organic chemistry, and that led to incredibly productive areas of research, including X-ray crystallography and eventually the decoding of the structure of DNA. Again, quantum theory was applied theoretically by Dirac to predict negative energy states, leading to the prediction and soon the discovery of the postiron, and antimatter, which opened the door to a vast new realm of experimental research in particle physics. Dirac certainly did not imagine Gell-Mann's "eightfold way" when he made the prediction about negative energy states, but Dirac's work proved incredibly fertile.

All these cases involved experimental predictions which led to further epxierments, and opened up huge new areas of active and productive research.

What huge new areas of productive research has string theory led to in 30 years?

Where are the experimentally testable falsifiable predictions from string theory? Where are the experiments based on string theory predictions which have opened up vast new areas of active productive research, and led to new technologies and new realms of applied science?

There are none. String theory has not been fertile.

That's a big warning sign.

Another big problems is string theory's prediction of 10, or, possibly, 11 dimensions (depending whether you adhere to M-theory or not). Every scintilla of experimental evidence we have amassed in the entire history of physics so far converges on the conclusion that we live in a 4-dimensional world — 3 dimensional of space, one dimension of time. There is not a single shred of experimental evidence pointing to the physical existence of more than 4 dimensions in our universe.

There's no reason why compactificaiton *couldn't* be theoretically correct, mind you — it's just that there's absolutely no experimental evidence to support it. And, if compactification occurs anywhere near the Planck length, the energies required for an experimental test lie far beyond anything anyone can hope to reasonably produce in any possible particle accelerator. That makes compactification unfalsifiable. And that's yet another big problem for string theory.

The parallels between string theory and other types of pathological science, like N-rays or mitogenetic rays, run even closer. Drs. Woit and Smolin seem to have played a similar role to the physicist Robert Wood in the N-ray affair — except that Wood pointed out that Blondlot's experimental results were incorrect, whereas Woit and Smolin are pointing out that the string theorists aren't even making any predictions that anyone can provide experimental results to test. (That's even worse than the case with Blondlot's N-Rays.) And we're getting much the same kinds of responses from string theorists that the N-ray proponents gave back in the early 1900s.

Viz., critics of the theory "don't really understand it and thus are not qualified to comment." (Rejoinder: I don't need to understand the mathematical details of a theory to know whether it has produced an experimentally testable falsifiable prediction. This is the same faulty argument as claiming "You can't judge who won the Olympic sprint, since you're not a world-class runner." Yes I can. I don't need to be a world-class runner to see who crossed the finish line.)

And again, proponents of the pathological science claim to have tests of the theory — except that the tests turn out to be phoney. We're seeing this now with string theory. Each time a new "test" of string theory gets announced, it turns out to be pure twaddle.

And again, proponents of pathological science resort to censorship of critics.

None of these tactics ever works. Eventually a scientific theory **must** produce experimentally testable falsifiable predictions, or it dies. This is why it's hard to see how string theory can be accurately described as "alive and kicking." Its proponents continue to flail around in search of some way of generating testable falsifiable predictions from their theory. Jacques Distler's swampland approach with statistics on the vacua represents one approach, Ed Witten's latest toy model represents another. These are all valiant efforts, and there's certainly a great deal of furious activity going on…but the simple fact remain that all this furious activity on the part of string theory never seems to lead anywhere. We never get a unique falsifiable hard number out of string theory for, say, the mass of the Higgs — a number that's solid and unique and can be disconfirmed by experiment. We never get a single hard testable fasifiable number for the compactification length. Maybe Ed Witten's new 2d + 1 approach will finally give hard testable numbers. But I can tell you this…I'm not going to hold my breath, based on past experience.

The acid test of a theory of physics remains: ***What does it take to disconfirm it? What experimental results will disconfirm your theory?*** String theorists have never been able to answer this crucial question.

That's the sign of what Hubert Dreyfus called "a degenerating research program." It seems clear to most unbiased observers that the string theory reserach program is degenerating badly. Despite all the furious activity by string theorists, no falsifiable testable numbers are coming out of the equations.

Some string theory proponents have made an analogy with the early days of quantum chromodynamics. Initially, those equations blew up and produced infinities — then renormalization was discovered, and things settled down. However, it's important to remember that renormalization was invented and applied within a very short time. The gap between QCD and hard testable numbers was only a handful of years. (Gell-Man and Low actually invented the renormalization group in 1954, though Fisher typically gets the credit for it.)

It's now been three decades for string theory. Still no hard testable numbers. That doesn't seem analogous to the QCD situation. In fact, it's not analagous to *any* case in theoretical physics that comes to mind, other than pathological science.

That's my take on why string theory is losing the battle for credibility. As for the continued hiring of string theorists, institutional inertia is the simplest explanation. Linda Rosa's critique of the "therapeutic touch" studies appeared in 1996 and to sensible skeptical observers, that closed the case.
http://www.quackwatch.org/01QuackeryRelatedTopics/tt.html

But hospitals still hire therapeutic touch "experts" 11 years later.
Hubert Dreyfus' book *What Computers **Still** Can't Do* appeared in 1979…but universities continue to hire and grant tenure to researchers pursuing "hard" AI. In economics, Rational Choice theory is currently on the outs, and its record of failed predictions (Martin Luther King's voting marches in the deep south couldn't have occurred because of the low utility function) is leading to a widespread abandonment of that particular theoretical framework. But, once again, some universities do

continue to hire and grant tenure to Rational Choice theorists in their economics departments.

This is human nature. People aren't perfect. It takes time for groups of people to absorb information and reach conclusions, even more time for institutions to form a consensus.

A consensus about string theory apepars to be forming within the HEP community. I would be surprised if string theory is still around in any significant way in another generation.

45. **anon.**
May 29, 2007

'… Eventually a scientific theory must produce experimentally testable falsifiable predictions, or it dies. …' – mclaren

In order to debunk string, you need to bring a new game to town, or the gamblers will only play at strings. You seem to be assuming that the field is competitive. The only competitions are those inside mainstream string theory, because it's the only game in town.

46. **Eugene Stefanovich**
May 29, 2007

anon.:

In order to debunk string, you need to bring a new game to town, or the gamblers will only play at strings. You seem to be assuming that the field is competitive. The only competitions are those inside mainstream string theory, because it's the only game in town.

In my humble opinion, entire post-Standard-model theoretical physics looks like one big casino. People throw around outrageous ideas hoping to hit the jackpot. For some reason, the "string table" attracted more gamblers than other equally destructive games. The only solution is to stop gamble and to switch to some constructive work. It may be slow and boring, but that's the only way forward… in my humble opinion.

47. **mclaren**
May 29, 2007

> In order to debunk string, you need to bring a new game to town…

Apodictically wrong. If a scientific theory persistently fails to produce testable falsifiable predictions, it debunks itself.

48. **Zathras**
May 30, 2007

In order to debunk string, you need to bring a new game to town…

Apodictically wrong. If a scientific theory persistently fails to produce testable falsifiable predictions, it debunks itself.

These two quotes demonstrate the difference between the theoretical conception of the practice of science and science as actually practiced. Since science is a human enterprise, there will always be a disconnect between the two. The practice of string theory shows this disconnect. Science as practiced will continue to have these problems. You might define the problem away by saying that what they are doing is not science, but that does not correct the actual practice issue.

49. **Peter Shor**
May 30, 2007

I don't know that string theorists are behaving that differently from scientists in other fields. It is (and should be) very difficult to dethrone an established scientific theory. The problem in this case is that string theory somehow became an "established scientific theory" with what many people consider only the flimsiest circumstantial evidence in favor of it.

How did this happen? There were several decades with no experimental evidence for anything beyond the Standard Model, and during the 80's and 90's, string theory looked very promising, so it got established as the leading contender to unify gravity and quantum mechanics during this time. Now, even if somebody comes up with a promising alternative theory at this point, sociology means that it will be very hard to battle the particle physics establishment.

50. **anon.**
May 30, 2007

'… Now, even if somebody comes up with a promising alternative theory at this point, sociology means that it will be very hard to battle the particle physics establishment.' – Peter Shor

String theorists say that the problem of alternatives being starved of funds and prestige is the same sociological issue that always arises in all the sciences, and string theory is therefore not a special problem. But in other mainstream theories (e.g., the big bang, evolution, etc.) there is plenty of evidence for the theory.

String, despite lacking evidence, must be defended using aesthetic ('beautiful ideas') and romantic ('all the real geniuses believe in string theory') arguments, backed up with serious-sounding assertions that nobody has any other serious ideas.

This seems to be the reason why string is preventing the development of alternatives. Who can develop a theory to rival strings which will be taken seriously by anybody? Will an alternative need to be developed secretly for decades to accumulate as many results as string, before being published with an expensive fanfare of hype, press conferences, advertising, etc? How on earth can any conceivable alternative capture the limelight from a 10/11-d multiverse theory? Who will pay the slightest attention? Alternatives to rival string theory may require as much effort as has gone into string theory. How can that ever happen in the current high energy physics climate?

51. **Peter Shor**
May 30, 2007

It's possible that the LHC will find something interesting, and this will lead to the next 10 years of hiring in particle theory in physics departments being predominantly non-string theorists (unless the string theorists turn out to be the ones who explain what the LHC finds). This would greatly reduce the political power of string theory. However, I think this is only likely to happen if substantial new and different physics is required to explain the LHC results.

52. **Aaron Bergman**
May 30, 2007

However, I think this is only likely to happen if substantial new and different physics is required to explain the LHC results.

Have you looked at hiring patterns over the past few years?

53. **locrian**
May 31, 2007

What? Students are still flocking to string theory? Bummer for them.

Don't get me wrong, every grad student I know involved in it is having a blast. The thing is, every grad student I know trying to get a job afterwards is just freaking miserable. Saying that string theorists are still being hired by universities in substantial numbers (assuming we actually believe it) hides the fact that many of those universities are little better than high schools and many of the jobs are temporary – and don't even ask about the pay.

One soon-to-be-phd actually approached me the other day and told me flat out not to finish my degree in physics. I suppose the lesson here is: be careful studying string theory; it may be more than some public debate you lose.

54. **Chris Oakley**
May 31, 2007

> One soon-to-be-phd actually approached me the other day and told me flat out not to finish my degree in physics.

Bad advice. Finish your physics degree and then do something other than String Theory afterwards.

55. **Christine**
May 31, 2007

Peter Shor wrote:

unless the string theorists turn out to be the ones who explain what the LHC finds

Homework exercise: find a scenario that string theory *cannot* explain.

As far as I understand it, isn't it the main problem? Is it not the question that string theory is able to accommodate a huge plethora of solutions? So the only condition I see that would make string theory falsifiable would be in a situation were a physical phenomenon was found such that it *could not* be accommodated in string theory by any means.

Christine

56. **Peter Woit**
May 31, 2007

Aaron (and others),

I'd be interested to hear about it if anyone has any actual numbers about this they can point to, other than personal impressions that the job market for string theorists sucks (the job market for all theorists has sucked since 1970, one needs some numbers to see if there have been real changes in relative suckiness…).

My own impression from seeing who is getting hired is that, while leading string theorists are going around giving talks about how one should ignore the fact that current versions of string theory can't predict anything, because we don't know what string theory really is, if you actually work on the fundamentals of string theory, trying to make progress on figuring out what string theory "is", your job prospects are dim. On the other hand, if you work on "connecting string theory with real physics", doing things like "string phenomenology", then you have a better chance of getting a job, even though most people acknowledge that this kind of research is not likely to go anywhere.

57. **Aaron Bergman**
May 31, 2007

*Homework exercise: find a scenario that string theory *cannot* explain.*

As always, I fall back on my amazement that, given the inability of anyone to produce a single vacuum consistent with the real world, people immediately postulate that everything is permissible. But whatever.

For Peter: You can read the rumor mill just as well as I can. In fact, contrary to my expectations (and that of others I know), this year was surprisingly good for formal string theorists. By that, I mean four were hired to physics departments. In addition, there were three more positions (offered or hired) in math departments. Add to that 2 more, maybe, and you get the string theory jobs in this country and Canada. A rough count (I'm lazy) gives me at least twice times as many phenomenology/astro jobs as that.

58. **Peter Woit**
May 31, 2007

Aaron,

I did take a look at the latest rumor mill (hey, what's up with Frederik going to Harvard?) and I just lost all sympathy for string theorist's complaints about the job situation. By my count there are 32 research jobs offered, very roughly half to people who haven't worked on string theory at all, half to people whose research has some connection to string theory (often black holes), even if tangential. This is consistent with your count of formal string theorists.

This number of jobs is dramatically higher than it was a few years ago, both for string theorists and non-string theorists. I remember looking at these numbers carefully several years ago, and estimating 15-20 jobs/year, consistent with one I just picked at random (2000) and counted, giving 19 jobs.

So, looks like all the hullabaloo generated by my book and Lee Smolin's has caused a huge increase in hiring of both string theorist and non-string theorists (or maybe all the people hired 1960-70 are finally starting to retire or croak…).

Anyway, if you believe the Mill, it looks like the job situation for particle theorists, string and non-string, is better than it has been at any time for nearly forty years.

59. **locrian**
May 31, 2007

Aaron,

I don't understand. Is 9 positions really a good number? My university alone will produce 3 graduates by the end of the year. Peter was looking for numbers, so do you have an idea of how many string theory students are graduating and how that compares to the 9 positions that opened up in a good year?

60. **locrian**
May 31, 2007

Peter,

I am curious as to your source for your information. I'd love to compare other physics disciplines as well. To be honest, the job market still sounds pretty awful to me, regardless of how it compares to past years.

61. **Peter Woit**
May 31, 2007

locrian,

Yes, 9 positions is a good number. These are tenure-track positions at universities that support research, and such positions have always been very scarce in this field compared to the number of people getting Ph.Ds in it.

I looked very carefully at these numbers when I was doing research for my book about 5 years ago. The best estimate I could find was that there were about 80 students a year in the US getting particle theory Ph.Ds, with roughly ten of them ending up in a permanent position. This was based on the fact that the rumor mill was showing roughly 15 hirings into tenure track jobs/year, and an estimate that a third or so of those would not turn into permanent hirings (people would not get tenure or would move some place else).

So, the job situation in particle theory has been awful, and this has been true since 1970. It does appear though that the job situation may be moving from awful (10-15% of Ph.Ds getting permanent research jobs) to less awful (20-25% of Ph.Ds getting permanent research jobs). But, in any case, if you go into this field and get a Ph.D., you're still pretty sure to not be able to get a job working in it (and pretty much certain to not get one unless you're working on one of the hot topics in string theory or phenomenology).

62. **Aaron Bergman**
May 31, 2007

Where'd you get half and half? It looked around 2:1 to me.

9 is a good number compared to previous years. Objectively, most people who graduate in string theory won't get jobs.

63. **Chris Oakley**
May 31, 2007

Peter: yes, but that is not a reason to pass up the opportunity of doing a physics Ph.D. if one has the opportunity. There are employers outside academic research, and they, generally, are impressed by a Ph.D. in a hard science (although whether (e.g.) the anthropic landscape could be classified as such is debatable).
As for the increased hirings of String Theorists since the publication of "Not Even Wrong", I would attribute it to the aura of glamour that you have endowed the subject with by lambasting it publicly.

64. **Peter Woit**
May 31, 2007

aaron,

I'd characterize things as more like 1/3 string theory, 1/3 hard-core phenomenology, 1/3 less easy to characterize (often cosmology, black holes), but of this last 1/3 a significant number have at least one paper claiming to be related to string theory.

But, I don't think we disagree about the bottom line, the job situation for string theorists is better than ever (although that for phenomenolgists maybe even more so).

65. **Aaron Bergman**
May 31, 2007

I'd say that this year was a surprisingly good year, especially for formalists. Who knows what next year will be like?

66. **Chris W.**
May 31, 2007

As always, I fall back on my amazement that, given the inability of anyone to produce a single vacuum consistent with the real world, people immediately postulate that everything is permissible. But whatever.

Aaron,

Then tell the prominent theorists who have been acting like a Landscape containing vacua consistent with the real world (however few) is a foregone conclusion to put up or shut up. I'm getting pretty tired of your insistence that people like Leonard Susskind are unrepresentative of most string theorists, while the community of which he is evidently a part allows his viewpoint to gain so much traction. (One should recognize the efforts of David Gross to counteract this phenomenon.)

Let's restate the problem. Either:

(1) string theory leads inexorably to a landscape of solutions, none of which are consistent with observation or the success of the Standard Model, requiring the abandonment of string theory as a failed project (which is inherently nothing to be ashamed of);

(2) string theory leads to a landscape of solutions, some of which are consistent with observation and the success of the Standard Model, but offers nothing but shallow, after-the-fact empirical criteria for selection ("curve-fitting") of these solutions, leading to invocations of the Anthropic Principle;

(3) string theory leads to a landscape of solutions, and also incorporates *clear criteria* for ruling out all but a small family (an equivalence class?) of these solutions on *a priori* grounds, which thereby become testable, along with the underlying assumptions of string theory itself.

One possible source of the criteria for (3) would be a non-perturbative formulation, or at least some empirically fruitful insights into the required features of such a formulation.

For the record, (2) is logically possible, but is an abdication scientifically—a 21st century version of Ptolemaic astronomy. Physics could have resorted to such a resolution of its problems at any point in its history. On what basis should it become acceptable now? Skeptics of the significance of fundamental scientific discoveries have always invoked variations on the argument that alleged agreement of theories with observation is a fraud and a fabrication, an inscrutable accident of some practical use but telling us nothing about reality, or a kind of tautology, following inevitably from the structure of our minds and senses *or the structure of our theories*. The risk of vulnerability to these arguments is always present, as is the risk of outright refutation by observation for theories with genuine empirical import. If we choose to run the latter risk we can have no final guarantee of success.

[Thanks, Christine.]

67. **anon.**
May 31, 2007

I don't understand Peter's claims that comparable numbers of offers go to string theorists and phenomenologists. Let's look at the current rumor mill data. If we neglect job offers from math departments, there are only 6 string theorists with offers out of 32 total theorists with offers. If we count the ones from math departments, it's 10 out of 36: close to 1/3, but still substantially less. And the remainder is really dominated by phenomenology, not just 1/3: there are 22 offers to phenomenologists. Below I list the people with offers, categorized appropriately (all due apologies for filing people in boxes, but I want to counter the misperception that's being spread in this thread):

String theory (10 offers, 4 from math departments)
————–
Frederik Denef
Bogdan Florea (math dept)
Yang-Hui He (math dept)
Matthew Headrick
Chris Herzog
Liam McAllister
Michael Schulz
James Sparks (math dept)
Diana Vaman
Johannes Walcher (math dept)

Phenomenology (22 offers)
————–
Kev Abazajian
Kaustubh Agashe
Rouzbeh Allahverdi
Mu-Chun Chen
Antonio Delgado
Patrick Fox
Ayres Freitas
Michael Graesser
Yuval Grossman
Dan Hooper
Jamal Jalilian-Marian
Ryuichiro Kitano
Ian Low
Markus Luty
Alexander Penin
Tilman Plehn
Stefano Profumo
Yuri Shirman
Peter Skands
Philip Stevens
Tim Tait
Liantao Wang

Other (theoretical cosmology, mostly — 4 offers)
—————
Alfredo Lopez-Ortega
Alberto Nicolis
Dejan Stojkovic
Andrew Tolley

68. **woit**
May 31, 2007

anon,

You might consider actually reading what I write. I didn't write: "comparable numbers of offers go to string theorists and phenomenologists", I wrote

"1/3 string theory, 1/3 hard-core phenomenology, 1/3 less easy to characterize (often cosmology, black holes)"

Among your 26 "non-string theorists" are many whose work is not so easy to characterize, but is not what I would call "hard-core phenomenology". One example is Nicolis, who has been hired here at Columbia, whose recent work is in the "Swampland" program.

69. **anon.**
May 31, 2007

Notice I put Nicolis in the "other" category. I'm pretty sure that all 22 of the people I listed as phenomenologists are, in fact, phenomenologists, whether or not you think they are "hard-core." One could always further subdivide them as collider phenomenologists, astroparticle phenomenologists, model-builders, etc.…

70. **anon.**
May 31, 2007

Also, while Nicolis is not readily classifiable as either a string theorist or a phenomenologist, he is nonetheless a first-rate theorist and a good example of how the theory community doesn't exclude people with good, non-mainstream ideas.

71. **Aaron Bergman**
May 31, 2007

Chris W. —

String theorists fight about this stuff *all the time*, often quite vociferously.

72. **woit**
May 31, 2007

anon.

My definition of "hard-core phenomenologist" was more or less someone who interacts with experimentalists and computes numbers they care about.

I don't know Nicolis at all, but characterizing as non-mainstream someone who has been part of the Harvard theory group, one of Arkani-Hamed's main collaborators, and working on the "swampland" program that has been heavily promoted by leading string theorists seems to me rather bizarre.

The only reason I chose him as an example was that I started going down the list at the Rumor Mill and he was the first clear case of what I meant by a non hard-core phenomenologist who has done work related to string theory. So, he was an example purely because "C" for "Columbia" comes early in the alphabet. One could waste a lot of time going through the rest of the alphabet…

73. **dan**
May 31, 2007

Chris,
I agree with your scenario 1-3. How do we decide which of these are most reasonable and when?

Let's say the MSSM is true. Well LHC results are not yet online, and the MSSM is just as unpredictive as string scenario 2.

It might look like scenario 1 to Peter now, NEW, but maybe after another 50 years of intense sustained research, with LHC results and bootstrapping from that, 2 or 3 actually holds.

Of course it's possible string theory it's actually 1, and it is starving off alternative research directions which is what Lee says is the Trouble with Physics.

My own opinion: if LHC doesn't find SUSY-partners, or its results cannot be accounted for or embedded in, a string theory scenario, I hope the HEP community pursues alternative research directions.

2 alternative post-SM post-string HEP research directions I'd like to see investigated is Smolin's preon braiding, and condensed-matter analogues such as Volvovik's fermionic points.

Dan

74. **JC**
May 31, 2007

Sometimes a "negative" result can be just as interesting, such as if the LHC doesn't find anything at all. (ie. No Higgs particle and nothing else).

75. **Dipankar Ray**
May 31, 2007

"Coin,

While string theory has contributed to mathematics in various ways, as I wrote, I do think that saying it has provided numerous deep mathematical insights is an exaggeration."

Let me quote from Faddeev/Atiyah's description of Witten's work, in the Proceedings of the ICM
(you can see the text here: http://www.icm2002.org.cn/general/prize/medal/1990/Witten/page1.htm):

"…Although he is definitely a physicist (as his list of publications clearly shows) his command of mathematics is rivalled by few mathematicians, and his ability to interpret physical ideas in mathematical form is quite unique. Time and again he has surprised the mathematical community by a brilliant application of physical insight leading to new and deep mathematical theorems."

The citation goes on to detail Witten's insights into Morse Theory, the Index Theorem, Rigidity Theorems, and Knot theory. Essentially, by 1990 Witten had re-invented all of that century's work in geometry, using his string-theory heuristics.

By the way, this was back in 1990, so it doesn't count his later work on Sieberg-Witten, or the recent stuff on Geometric Langlands; forget his and other people's (Candelas, etc) work on Mirror Symmetry, which has completely blown open Moduli of Curves, and deeply influenced the work of Kontsevich and Okuonkov, to name just two Fields Medalists who've benefited from string theory…

Just to give one personal example: in 1990, I don't think anyone expected that there would be a connection between Weyl's asymptotic formula and Mumford's conjectures on the Moduli of Curves; the glue of this particular connection turns out to be Witten's conjecture, and the various proofs by Kontsevich, Pandharipande/Okounkov, and Mirzakhani. That one conjecture has also inspired great work in combinatorics, in representation theory, etc.

As you say, you yourself have written about some of this. Can you explain to me what your criterion for "numerous deep mathematical insights" is, as this certainly qualifies in my book…

76. **woit**
May 31, 2007

Dipankar,

Your Atiyah/Faddeev quote does not mention string theory, precisely because the work that Witten got the Fields medal for has nothing to do with string theory. To say "Essentially, by 1990 Witten had re-invented all of that century's work in geometry, using his string-theory heuristics." is non-sense, I don't believe that's what Atiyah or Faddeev actually wrote. In particular, the work on morse theory, supersymmetry and the index theorem predates Witten's interest in string theory and has absolutely nothing to do with it.

If you're interested in where Witten's work came from, you might want to try reading my book, which has a chapter on the subject.

77. **Chris W.**
May 31, 2007

Aaron,

That's good to hear…

78. **Dipankar Ray**
May 31, 2007

"Essentially, by 1990 Witten had…" was from me, not Atiyah. I almost didn't put it there, since I could guess what the response would be. And indeed, I made a mistake: I should have said "Essentially by 1995…," and perhaps I should have ended with the canonical locution "using his path-integral oracle".

Had I done that, then I could have made the following list of results:
– Morse theory
– Atiyah-Singer Index theorem
– Donaldson Theory
– (you get the point)

These are (I think you'll agree) several of the handful of crowning accomplishments of 20th century geometry. And I'll note that this is why Witten, at the AMS Alg Geo conference at Santa Cruz in 1995, made the (very compelling to everyone I knew who was there) argument that, in the future, QFT would be a field of mathematics alongside of algebra and analysis, and would probably subsume differential geometry, algebraic topology, and even parts of alg geo).

Certainly, this does _not_ mean that he re-invented algebraic topology, hodge theory, etc, in terms of the basic definitions. But he definitely re-cast our understanding of such things, in much the same way that Grothendieck re-cast our understanding of algebraic geometry. Atiyah and Bott have both written about how Witten completely re-organized the way that _they_ think about Morse theory, about their own work on Yang-Mills, etc. Similarly, Witten's results on Atiyah-Singer and Donaldson theory weren't just new proofs, they were fundamentally new ways of thinking about these problems, very much "deep insights". A flurry of work and consolidation and connections followed, in each case (as I'm sure your colleague John Morgan would agree).

You distinguish between this work of Witten's and "Witten's interest in string theory". I find this disingenuous, though perhaps it is exactly this point that distinguishes our world-views.

Green Schwarz and Witten is copyrighted 1987. That tells me that Witten was pretty busy thinking about String theory precisely when he made his great original math discoveries. And certainly the Seiberg-Witten work and the Geometric Langlands work is from his "string theory" era, if we are to try and force "periods" onto his career?

But I think this is a key point: I believe that String theorists are by and large engaged in a basic, long-overdue, formal, fundamental study of QFTs. QFTs are where the problem started, since they showed (1) the incredible power of perturbative methods, (2) the lack of mathematical rigor in the methods, and (3) underscored the depth of the problem Einstein had wasted the last ~30 years of his life pursuing (Feynman-Schwinger-Tomonaga showed us how successful QFT was, and thus how crazy it was to think that the problem of unification was a tractable problem in 1920).

The Jaffe-Quinn school was one approach, but it got bogged down. String theory has become the de facto place where a basic study of QFTs is taking place (and why not, since if you're a budding grad student hoping to learn QFT, you probably want to turn to your phenomenologists and your string theorists, since they're the experts on QFT in your department, no?).

The actual sociological problem might be this: Einstein was lucky; Gauss and Riemann had already discovered the necessary math for him, so he didn't have to invent it for GR. Unfortunately "mathematicians" like you and me are blogging when we should be proving hard math theorems; as an indirect result, physicists studying QFTs are having to invent the mathematics for themselves. One can easily imagine that Einstein and a handful of grad students would have taken >20 years to invent Riemannian Geometry; well, it's taking us >50 years to invent the necessary math for QFT. So it goes.

Because of this world-view of mine (shared by many, I think), separating the results that Witten obtained by studying QFTs from his work in string theory – well, this is very weird to me, since it was surely his interests in string theory that had him write his papers on TQFTs, etc.

79. **woit**
May 31, 2007

Dipankar,

I really suggest that you learn some of the history of this subject, you might find it enlightening. Just one example: Witten's work on Morse theory and supersymmetry (which is the foundation of his work on TQFT) dates from 1980, his interest in string theory began around 1983, with his first paper on the subject in 1984.

The "path-integral" is not "string theory". These are two very different things. QFT is not string theory either. You're welcome to your view that I am "disingenuous" or "weird" for pointing out that many of Witten's most important ideas come from another source than string theory, but this happens to be a fact. The interaction between different ideas about physics and mathematics is a complicated and extremely interesting subject, I'd suggest you try learning something more about it than that everything is due to "string theory".

80. **Dipankar Ray**
May 31, 2007

Your reply seems to prove my point: you distinguish between supersymmetry and string theory here, while I believe elsewhere the lack of experimental evidence for supersymmetry is an oft-repeated example of one of the failures of string theory (perhaps not by you? my problem here is undoubtedly that I've arrived late to a debate where all the principal players are already nursing wounds from attacks from all sides).

Certainly, when Zumino said (in the 70s) that supersymmetry implied SU(n) holonomy, that wasn't string theory either. And yet SU(n) holonomy and the exchange of supersymmetries are exactly the key features of Mirror Symmetry.

In ’81, Witten was writing about Kaluza-Klein theories, and the “Supersymmetry and Morse Theory” paper wasn’t published until 1982 (of course I don’t doubt you that it was written earlier – this is before my time). In general, most of these geometry results come from duality of one kind or another.

Your point is that I should read more and learn to distinguish between supersymmetry and string theory, whereas my point is quite clearly that I think these are false distinctions that ignore the fundamental point: in reality we’re paying people to study QFTs.

Couldn’t I use your same arguments to say that string theorists themselves are in a different field today than they were 20 years ago? The string theorests of today are basically studying a completely different subject (geometric langlands? category theory?) from the string theory of 20 years ago.

The commonality between string theory then and now? Some mix of supersymmetry, kaluza-klein, and the basic (distinguishing) notion that the fundamental objects of the universe are strings.

You seem to be saying that only the last element is string theory. I’m saying that all of it is QFT (my version of “can’t we all just get along?”)

I commented on this thread, because it seemed to me that you were saying that this interaction between math and physics hasn’t provided deep insights for mathematics, which sounded like bad advice for a beginning grad student. Now I see you’re drawing a particular distinction (one which I don’t agree with, and not because I’m so ignorant of the literature/history, as you suggest; rather my interpretation is fundamentally different).

If I were a beginning grad student in math, I might easily conclude from your comments that reading Witten’s papers wasn’t fruitful. That sounded really bad to me; in fact, Kontsevich’s ’94 ICM talk tells me that, if you’re interested in math, you might do very well indeed to think hard about physics in general, and string theory in particular.

With that, I should withdraw.

81. **Dipankar Ray**
May 31, 2007

…woops, memory failing me, I think Zumino only showed U(n) holonomy…

82. **woit**
May 31, 2007

Dipankar,

“you distinguish between supersymmetry and string theory here”

“If I were a beginning grad student in math, I might easily conclude from your comments that reading Witten’s papers wasn’t fruitful.”

This is just completely ridiculous. Supersymmetry and string theory are two different things, and I’ve written voluminously about both of them and about the importance of Witten’s work, on this blog and in multiple chapters of my book. In all of these places I’ve made it very clear that I have the highest opinion of Witten’s work, that the experience of reading some of his papers has been among the greatest intellectual experiences of my life. But, you know, he’s written more than 300 papers, despite what you think they’re not all about string theory, and, surprisingly enough, some are more interesting than others.

83. **Dipankar Ray**
May 31, 2007

I came to your website because a friend pointed me to your informative post about the abc conjecture. Because I liked to that post, I went to the previous post, and then read about how you thought Sean Carroll was mildly exaggerating string theory’s influence on math, followed by comments that were fairly condescending towards Polchinski, both of whom I have a lot of respect for. This surprised me – especially the comment about string theory’s influence. So I read the comments, to see if anyone else questioned this statement. Someone did, and your comment reinforced the point that string theory was no big deal to mathematicians.

So I didn’t read your voluminous writings about how great Witten is. I reasonably conclude that others might first encounter your website the way I did, and draw the conclusions about your opinions that I did. Hence my post.

My mistake to post in the first place – I didn’t know what I was up against. You clearly have long ago formulated very strong opinions on all of this, and seem to be very ready to jump down my throat when I question the validity or (more importantly) _usefulness_ of the distinctions that you’ve drawn.

You’ve clearly rejected my umbrella metaphor, that physicists are drawing false distinctions between what they do, when all they’re trying to do is understand QFT. I don’t see any point to string theory, or supersymmetry, or Kaluza-Klein theories outside of understanding QFT, and so hence it’s all QFT to me.

Fine, you reject this. But you still don’t address my other points: that Witten was _thinking_ about string theory, and hence using string theory heuristics to guide his work on moduli of curves, on seiberg-witten, on geometric langlands. that in itself is interesting and deep.

Second, that mathematicians studying QFT in general and (yes) string theory in particular (or at least conjectures that grew out of string theory) resulted in deep work not only in mirror symmetry, but in fundamental algebraic geometry (Mumford’s conjecture, which was proven by Madsen and Weiss largely because of the renewed interest around moduli of curves sparked by Kontsevich’s proof of the Witten conjecture), in combinatorics (Ravi Vakil’s article in the Notices gives a nice presentation of that work), and in representation theory.

There are more examples, obviously (Givental’s work is of course important, and there are a number of conjectures in that area that have fallen recently, and I think we’ll find that Okounkov’s work will soon combine with Werner/Schramm’s work to show the power of stochastic methods in general; certainly Okounkov is influenced by string theory). Renewed interest in old topics, new problems solved – all of this is at least in part due to mirror symmetry. I wouldn’t be surprised if the geometric langlands thing led to similar revolutions throughout math.

anyways…

Very harsh language in addressing me. Did I do/say something to deserve condescension like “Despite what you think they’re not all about string theory”? I apologize about the mistake I made about a Zumino paper from the 70s, but perhaps you could cut me a little slack on a comment on a blog, and consider that I’m not a complete idiot? I don’t think it requires stupidity and/or ignorance to disagree with you on the precise borders between gauge theory, supersymmetry, QFT, TQFT, and string theory. While clearly these are separate topics, they are interconnected in everyone’s head. It’s like saying that the fundamental theorem of algebra is only a theorem in analysis, cause there’s no algebra involved in the proof (unless you do the Galois theory proof, or somesuch…) – technically true but misses the point a little, I’d say.

Actually, forget what I'd say. Your views are also not representative of either the physics or the math communities, and your admonishment/directive for me to read your book are not convincing me that these communities have a reason to change their behaviours, nor does your condescending tone elicit in me an overwhelming compulsion to buy your book.

84. **Peter Woit**
May 31, 2007

Dipankar,

You're the one who came here, started posting highly misleading comments that showed you don't know what you're talking about (no, path integrals, supersymmetry and string theory are not all the same thing), and started insulting me as being "disingenuous" and "weird". Given that behavior, I've actually tried to respond as politely as I can manage, giving you some factual information you might consider before going on in the way you do about Witten, TQFT and string theory. If you read more than a couple postings on this blog, you might find, in addition to a point of view that challenges your prejudices, some accurate information of interest.

85. **amused**
June 1, 2007

Re. the discussion of jobs to string/brane theorists vs others, one curious aspect revealed by the rumor mill page is that the string/braners are generally being hired sooner after their PhD, and with fewer papers. Now why might that be?…
I guess their brilliance must just be so abundantly clear that there is no need for them to prove themselves over longer periods, in contrast to the non-string plodders.

86. **Aaron Bergman**
June 1, 2007

Care to cite evidence that string theorists are being hired younger than non string theorists?

87. **amused**
June 1, 2007

It was a pain, but here's some data: it's the people listed as having recieved job offers at research uni's on the current U.S. rumor mill page as of this moment. I've ordered it according to the year they got their PhD's, and in the format "Person – N – topic(s)" where N is their current number of publications according to Spires. The people in each PhD year are listed in order of increasing N. Simple data analysis follows below.
— — — — — — — — —
2006:
McAllister 12 string cosmo

2004:
Tolley 13 stringy cosmo
Profumo 38 astro, susy pheno
Skands 39 susy pheno

2003:
Headrick 16 string
Hooper 78 astro

2002:
Nicolis 16 cosmo (recently stringy)
Sparks 17 string
Fox 17 branes, some susy pheno
Florea 20 string
Delgado 25 brane models
Chen 34 pheno (beyond SM)
Herzog 35 string
Kitano 37 susy pheno
He: 55 string
Freitas 61 susy pheno

2001:
Low 27 pheno (branes & trad)
Stojkovic 29 cosmo (non-string)
Walcher 30 string
Vaman 33 string
Abazajian 37 astro

2000:
Allahverdi 42 cosmo (susy & stringy)

1999:
Denef 27 string
Graesser 31 susy pheno, some branes and astro
Penin 55 pheno (trad)
Tait 56 pheno (trad & branes)

1998:
Agashe 44 pheno (beyond SM, branes)
Plehn 70 susy pheno (brane-free)

1997:
Shirman 29 susy pheno, branes
Jalilian-Marian 58 pheno (trad)

1996 & earlier:
Luty 68 susy pheno, cosmo, a bit of branes

Grossman 90 pheno (mostly trad)

Excluded from data:
Wang (since he already got a job last year)
———————

Average PhD year for string theorists (including stringy cosmology): 2002

Average PhD year of those for whom strings and /or branes is a major component of their research: 2001

Average PhD year of those for whom strings/branse is not a major research component: 2000

That's a less pronounced difference than I had expected, but it's still definitely there. Also, there is a clearly discernable tendency for string/brane theorists to have fewer paper than other job recipients in the same PhD year, although there is admittedly a fair bit of fluctuation in that.

88. **anon.**
June 1, 2007

amused: your counting is also a little skewed because several of the phenomenologists are moving from one faculty job to another (Grossman, Luty, …)

89. **Coin**
June 1, 2007

Care to cite evidence that string theorists are being hired younger than non string theorists?

And if it is true, does it mean "string theorists are more likely to be hired" or "string theorists are more likely to be young"?

90. **LDM**
June 1, 2007

"I guess their brilliance must just be so abundantly clear"

Maybe somebody can explain this to me, because I just don't understand. How is it even possible to be brilliant in a subject like string theory as there are no experiments (and likely never will be) to validate or invalidate your work…The "Not even wrong" logic would to me imply, also, "Not even brilliant", since physical correctness of ones work is a *requirement* of ones work also being brilliant. Or have I missed something here?

Therefore, I conclude any hiring of string theorists cannot be based on brilliance, at least not in string theory, no matter how much they might wish to think otherwise…

91. **ok**
June 2, 2007

I have a dumb question:
Why can't String Theory be thought of as a 2-d QFT(CFT)? I mean, what makes ST to be different from just a particular 2-d CFT?

92. **woit**
June 2, 2007

ok,

A CFT is defined on a fixed Riemann surface with conformal structure. To get perturbative string theory, you need to integrate over all Riemann surfaces (i.e., sum over the genus, and integrate over the moduli space for each genus. This introduces a new parameter, the string coupling.

And this (divergent) series is supposed to be an asympotic series for the true string theory, whatever that is. You're supposed to be including branes, M-theory, etc….

93. **amused**
June 2, 2007

anon.: Ok, I wasn't aware of that, but I suspect it also applies to string theorist Denef. It surprises me that the rumor page doesn't list these people under `faculty shuffle'.

LDM: In fairness to string theorists I think there is a reasonable formal theory sense in which ST work can be "brilliant" despite the lack of connection to experiment. The ads/cft correspondence and dualities between various theories are examples (at least in my book). Whether brilliant string work is overhyped and oversold compared to brilliant work on non-string topics is a completely different question…

94. **LDM**
June 3, 2007

Amused,

The whole point is to get a number. When I was in grad school, I knew a very bright Chinese student who had devised a formulation of QFT in terms of measure theory, which he thought was significant because he viewed the concept of a measure space somehow more "fundamental". But unfortunately, he could not get any new predictions with his new theory.
Similarly, who cares about Maldacena's Ads/Cft, if it does not ultimately get related to an *experimental* number?
An example of a duality which I would currently consider superior to Ads/Cft, from the point of view of physics, would be the Legendre transformation. This is an example of a duality which actually does produce measurable numbers, in both classical mechanics and thermodynamics.

95. **Urs Schreiber**
June 3, 2007

> A CFT is defined on a fixed Riemann surface with conformal structure.

Usually, a 2-dimensional CFT is defined on all Riemann surfaces. (Except, possibly, in some older statistical mechanics literature.)

In a quite precise way, a CFT is a functional that reads in "Riemann surfaces with marked points" and spits out numbers (the "correlators").

The basic idea of perturbative string theory is that one defines a theory on some "target space" by looking at a sum of values of a fixed 2-dimensional CFT evaluated over all Riemann surfaces.

The choice of the fixed CFT here is the choice of "target space background".

This idea is supposed to be the direct generalization of how perturbative quantum field theory works: there we sum the correlators of 1-dimensional QFT (known as "relativistioc quantum mechanics") over all 1-dimensional graphs of sorts.

Instead of pairing 1-dimensional QFTs with graphs, as perturbative quantum field theory on target space does, perturbative string theory pairs 2-dimensional QFTs with surfaces (to be thought of as "smeared graphs").

96. **Peter Orland**
June 3, 2007

Urs,

Of course, you are right: it is the modern terminology to define
a CFT as something on any given Riemann surface. But it is not
an especially physical viewpoint, at least not for some situations. Should we define any quantum field theory on all manifolds of a given dimension? Why stop there, and not consider all dimensions? Just as with CFT, one could regard any QFT as a functional giving correlators from the manifold.

I think the utility of such a definition depends on the problem
being studied.

97. **ok**
June 3, 2007

Thanks everyone for the clarification.

98. **amused**
June 4, 2007

LDM,
Studying the structures of QFT's (of point particles, strings,…) with the aim of getting deeper insights into them, finding relationships between them etc is a valid activity in its own right, even if it doesn't lead directly to numbers that can be compared to experiment. It is intrinsically interesting (in the same way that pure math is interesting), and the ideas generated and lessons learnt may later turn out to be very important for "real physics". (Yang-Mills theory was an example.) Actually there is quite a bit this kind of activity going on in particle theory these days, ST is far from the only case. For example there is the "fuzzy physics" approach to QFT's, and a few years back there was quite a bit of "quantum groups" activity with luminaries like Faddeev being involved. I don't think any of that was likely to directly lead to new numbers to compare to experiments. The point was to generate new theoretical insights. The complaint that ST doesn't produce new (or any!) numbers applies just as well to those other activities. The reason no one complained about them though was that they never became so dominant in formal particle theory to the extent that people who chose not to work on them were putting themselves at a disadvantage careerwise.

99. **urs**
June 4, 2007

> it is the modern terminology to define
> a CFT as something on any given Riemann surface. But it is not an especially physical viewpoint, at least not for some situations. Should we define any quantum field theory on all manifolds of a given dimension?

Yes, indeed, we should!

That's one way to make precise what an n-dimensional quantum field theory is: it is a functor

nCob –> Hilb

namely a rule which reads in n-dimensional manifolds with boundary, assigns Hilbert spaces to the boundary components (the "spaces of states") and assigns linear maps between these to the manifolds themselves: this is the quantum propagator

U(t)=exp(it H)

in simple cases, or, more generally, the path integral for the given states on the boundaries.

If this sounds obscure and "remote from physics", notice that this is, read the other way around, nothing but a clean formalization of what the "path integral" defining the QFT should really be.

Ever wondered how physicist manage to start their papers by writing down an ill-defined expression called the path integral and then nevertheless extracting lots of interesting results at the end of their paper?

There is a simple reason behind this: while the definition of the path integral as an integral mostly makes no sense at all, it often happens that this definition is in fact never used at all! Instead, what is being used are a list of properties that the path integral supposedly satisfies.

The most important of these is "sewing": this says that evaluating the path integral for a process from A to C is the same as doing it from A to B and from B to C and "summing over all intermediate states".

It is common practice in math to isolate those properties of the objects under considerations which are actually used in the constructions, theorems and proofs.

Here, it is essentially the sewing law (and also the unitary condition). The technical term for something satisfying such a law is a "functor".

So, we notice that whatever the path integral actually is, it is at least required to be a functor that reads in cobordisms and spits out morphisms of Hilbert spaces (or numbers, "correlators" if the manifolds fed in have no boundary.)

Once we are at this point, we do the obvious and declare that every such functor qualifies as a "path integral", hence as defining a quantum field theory.

So an n-dimensional quantum field theory is — by definition — a functor from n-dimensional cobordisms to Hilbert spaces.

For a discussion of this geared towards physicists, check out some notes by Kevin Walker.

> Just as with CFT, one could regard any QFT as a functional giving correlators from the manifold.

And indeed, one does!

The slogan is this:

Quantum field theory is the study of representations of cobordism categories.

But it's tough working out in detail what these functors are like, obviously. Most of the progress has been made, of course, for the simplest cases: topological field theory in 0,1,2,3 dimensions and conformal field theory in 0,1,2 dimensions.

With the advent of Khovanov homology, people are now with more intensity attacking 4-dimensional topological QFTs in this precise sense, see the recent Conference on Link Homology and Categorification in Kyoto and especially Gukov's talk Gauge Theory and Categorification

> I think the utility of such a definition depends on the problem being studied.

Certainly. Especially, if the problem is "understanding what's going on" and "organizing one's concepts" this is very useful.

100. **Peter Woit**
June 4, 2007

'So an n-dimensional quantum field theory is — by definition — a functor from n-dimensional cobordisms to Hilbert spaces."

This is a starting point for TQFT, not for QFT in general.

101. **Peter Orland**
June 4, 2007

Urs,

It is not true that path integrals are not well-defined, except after the
fact. This is a comon misconception. In regularized QFT's, there is absolutely no problem with the path integral. They are often studied numerically (that's what lattice Monte Carlo people do) and, in some cases, even evaluated analytically!

As I said, defining field theories on arbitrary manifolds depends on
the problem under scrutiny. Standard analytic methods,
1. perturbation theory, 2. strong-coupling methods, 3. semiclassical approximations, 4. exact S-matrices and form factors, 5. the Bethe Ansatz, and 6. conformal FT methods using the trace anomaly, all of which have applicability in at least some field theories do not require any but flat manifolds.

102. **Aaron Bergman**
June 4, 2007

This is a starting point for TQFT, not for QFT in general.

TQFT is a particularly simple example of this sort of axiomatization, but it is not hard to extend to QFT in general. Just choose add more and more structure to the objects in your cobordism category. (I'm beginning to become somewhat skeptical of this particular axiomatization beyond the 1-categorical structure, but that's just me.)

In regularized QFT's, there is absolutely no problem with the path integral.

That's true, but I think it can be quite hard to regularize a QFT (nonpertubatively!). Even then, to really talk about the full QFT, one needs to prove the existence of the continuum limit (presumably varying the couplings as you decrease the lattice spacing.) This is, needless to say, rather hard.

103. **Peter Orland**
June 4, 2007

Aaron,

I am not sure to what extent we disagree on this matter (perhaps not
at all).

As I tried to explain above, path integrals can in some cases be
evaluated analytically. No one who works with them seriously
questions their utility. Proving the existence of the continuum limit
is always the issue, but, generally speaking, path integrals have a better track record on this than other methods do. A stunningly
better track record!

For example, mathematical physicists (Nelson, Glimm, Jaffee, Simon, Frohlich, Aizenmann, Balaban) getting nowhere with the Wightman axioms had some real success after switching to Euclidean path-integral methods in the 70's. Furthermore, semiclassical methods
(DHN techniques in 1+1, confinement in compact QED) are best
understood with the path integral.

Sometimes other methods work better for specific cases
(integrable QFT's in 1+1 for example) , but for all-around utility
the path integral has no serious rivals.

104. **Aaron Bergman**
June 4, 2007

I'm all for path integrals. There are, I'm led to believe, some reasonably tought no-go theorems about the nonexistence of interesting measures on spaces of functions, however, so it remains a very interesting question as to what exactly a path integral is. I sometimes can't help but feel that the lack of a proper definition after all these years might mean that we're missing something important. But, then, the success of lattice gauge theories certainly speaks against that.

In some sense the axiomatizations of Atiyah and Segal and co. are an attempt to do an endrun around the lack of a proper definition of the continuum path integral. What it is an attempt to capture, I think, is the local nature of the path integral, that it must obey gluing and the existence of states in a Hilbert space to express that gluing. The structures expressed in these axioms clearly are present, but I doubt capture the whole story. One possible extension is the idea of a hierarchy of n-categorical structure, but the lack of a proper definition of the objects involved makes it tough to figure out how to go from usual physics to these rather abstract notions.

(To quote Dan Freed: "I define a mathematician's category number to be the largest n such that he/she can think about n-categories without getting a migraine.")

105. **Kea**
June 4, 2007

"I define a mathematician's category number to be the largest n such that he/she can think about n-categories without getting a migraine."

LOL! Cute. Guess for physicists we can reduce n to, say, n – 3.

106. **Peter Orland**
June 4, 2007

Aaron,

Forget all the no-go theorems (or rather, take them with a grain of salt).
Lattice path integrals are rigorously defined. The mathematicians who
have problems with them are geometers, not analysts, and the real
basis of path integrals is analysis, not geometry. The only issue is how to renormalize the answer. If there is a critical point in the space of couplings, there is a continuum limit (and then one has to address issues
of triviality).

107. **Aaron Bergman**
June 4, 2007

I'm not sure we're arguing, although I'm under the impression that there are still issues with chiral fermions and global symmetries. I'd just really, really like to see a proof of the existence of the continuum limit in an interesting QFT — the triviality issues, as I understand them, generally reflect the existence of a Landau pole, right? I'm not sure it's fair to say that its only the geometers that have problems — no-go theorems in measure theory count as analysis in my book, and analysts like to prove things just as much as any mathematician.

108. **Peter Orland**
June 4, 2007

Aaron,

I repeat that path integrals are healthy, and the best way to
look at quantum field theory in most cases.

There are of course subtleties, but the issues you are raising
are not fundamental difficulties with the path integral. There
are problems with putting the standard model on a lattice,
for example, but the origin of these is related to the
Nielsen-Ninomya theorem, which is also a problem in any regularization. Luescher may have solved this problem.

The Landau pole only suggests triviality of some QFT's. It's
less meaningful than generally believed. There are better
arguments for triviality (Ginzburg's criterion, epsilon
expansions, 1/n expansions, polymer methods) for
scalar field theories.

There are no measure-theoretic no-go theorems, at least none
which are meaningful. Look at constructive field theory for
example (I listed a bunch of names earlier). The
measure-theoretic issues for Euclidean path integrals
were first addressed by Mark Kac a long time ago. Edward
Nelson also worked on the foundations of this subject.
Take a look at Simon's book on Functional Integrals.

109. **Aaron Bergman**
June 4, 2007

Look, I agree that path integrals are the way to go, but if there were no difficulties, there would be existence proofs for interesting QFTs in four dimensions. There aren't. I'd love to believe this is just a result of the technical complexities of the limit, but until someone actual proves something who knows?

110. **Peter Orland**
June 4, 2007

Aaron,

Yes, of course there aren't existence proofs of four-dimensional theories (though there are existence proofs of two and three dimensional theories).

Anyway, I don't think rigorous existence proofs are the first step towards understanding the continuum limit (I say this as someone
who has spent some time trying to see how rigorous methods in
lattice field theory work). What we want is a good way to calculate physical quantities in that limit, using some sort of controlled approximation scheme. This is where perturbation theory fails for theories like QCD. If that can first be done, and it is a big if, then
we can worry about cleaning up the arguments mathematically.

111. **Aaron Bergman**
June 5, 2007

I'd like both thank you very much. I can say that because I'm not actually working on the subject :).

112. **Thomas Larsson**
June 5, 2007

"This is a starting point for TQFT, not for QFT in general."

TQFT is a particularly simple example of this sort of axiomatization, but it is not hard to extend to QFT in general. Just choose add more and more structure to the objects in your cobordism category.

Is this so? My impression, admittedly based on what I saw 15 years ago, is that the point with topological theories is that they are locally trivial – there are no local dofs. With only finitely many global dofs, it is hardly surprising if you don't encounter the really difficult problems of QFT, which have to do with infinitely many local dofs.

113. **Peter Orland**
June 5, 2007

Thomas,

Actually there is a sense in which some topological field theories are nontrivial. As I understand it, their correlators are purely topological, but they do have nontrivial degrees of freedom. The original models of Floer and of Witten (not Chern-Simons) are examples.

114. **urs**
June 5, 2007

The functorial definition of QFT is not restricted to topological theories. Put any structure S you like (conformal, Riemannian, etc) on your cobordisms, and you'll get a corresponding S QFT (conformal, etc).

TQFTs are just the best understood cases. But 2-d CFT is now also pretty well understood. Full rational 2-d CFT is pretty much completely understood this way. And, actually, only this way. (See this for more.)

And, as I said, this is nothing but making sense of the concept of a path integral. If you happen to have a path integral which already makes sense by itself, all the better. Then you have a way to construct such a functor using functional integrals. But if your path integral happens to be ill-defined, you might still be able to construct your functor otherwise.

At our workshop taking place currently the main focus is on understanding functorial Riemannian 2-d QFT.

(That's because Stephan Stolz and Peter Teichner noticed recently that to get elliptic cohomology over points (knows as modular forms) from the landscape of 2-dimensional QFTs, one doesn't necessarily need to restrict attention to superconformal ones. Supersymmetry alone implies that the torus partition function is a modular form (see this preprint for more).

115. **urs**
June 5, 2007

> In regularized QFT's, there is absolutely no problem with the path integral.

In most QFT cases which are physically interesting, there is absolutely no clue what the measure on the space of fields should be which we would like to integrate over.

116. **urs**
June 5, 2007

> I'm beginning to become somewhat skeptical of this particular axiomatization beyond the 1-categorical structure,

We hadn't really mentioned extended QFT yet.

But it is true, that the idea is that in the end n-dimensional QFT is really an n-functor on extended n-cobordisms instead of just a 1-functor on n-cobordisms.

This, too, though, is closely related to the path integral: namely to evaluating the path integral with certain boundary data kept fixed. The idea goes back to Dan Freed, at least, who does present it in path integral language in his papers from the beginning of the 90s (see the references given here.)

Hopkins and Freed are proposing, at least in talks, refinements of this idea to infinity-functors. As mentioned here.

117. **Peter Orland**
June 5, 2007

"In most QFT cases which are physically interesting, there is absolutely no clue what the measure on the space of fields should be which we would like to integrate over."

False.

118. **Peter Orland**
June 5, 2007

Urs,

I don't want to be a churl, but I am a bit sensitive about this issue, since I have worked on it for most of my career, and often hear statements such as yours. So let me just mention that I gave examples above where the measure is perfectly fine. The problem with the field theories we'd like to understand better is not in defining the path integral (which can be regularized), but in removing the cut-off.

119. **Aaron Bergman**
June 5, 2007

With respect, Peter, that's like saying that taking the limit isn't important in the Riemann integral. The whole point of defining the path integral rigorously is to remove the cutoff.

120. **Peter Woit**
June 5, 2007

Aaron,

I don't see where Peter is saying that taking the limit isn't important. But there are physically interesting theories (e.g. Yang-Mills) where conjecturally we think we know how to take the limit, and there is a huge amount of numerical evidence to back up this conjecture.

121. **Peter Orland**
June 5, 2007

Just to add to what Peter W. said:

There are examples above where the limit CAN be taken. In particle scalar field theories in 3 Euclidean dimensions. One can also prove that the 5 dimensional case has no interacting limit (and argue the same in 4d, though not yet rigorously).

Since no one has solved QCD, we don't yet know what the limit is, and it has not proved to be an interacting theory. Nobody who works on this problem doubts that the limit exists, however. Formal renormalizability on the lattice (just perturbation theory with a lattice cut-off) certainly indicates that the limit exists, though it is not a proof. In any case, if you are theoretical physicist, you want a calculational scheme more than a formal proof.

I have some experience with this sort of problem, so I think I have some feeling for what doesn't work (which is most of what people have tried). Perhaps the formal ideas Urs is discussing are mathematically interesting, but they do not shed light on the real problems of four-dimensional theories.

122. **Aaron Bergman**
June 6, 2007

If you believe Borcherds's notes on QFT, there does not exist a Lebesgue measure on the relevant spaces, so assuming that such a limit exists, I'd like to understand precisely what this is.

The reason why this would be useful is that there are field theories that don't admit nice Lagrangian descriptions. It seems reasonable to me that there are plenty of "measures" that aren't simply exponentials of integrals of functions involving the fields and their derivatives. I'd like to know what they are.

123. **Peter Orland**
June 6, 2007

Aaron,

I don't seem to able to get my view through to you or Urs. That may have to do with the choice of problems we work on. Unlike the two of you, I actually work on non-perturbative aspects of gauge theories an other field theories, and have for some time. That's why I have been very insistent that much of the issues you raise are not the right issues.

I am aware that there are field theories which do not (at least yet) have Lagrangian descriptions. But…

The field theories we NEED to know about for the purpose of understanding experiments in high-energy or condensed-matter physics (possibly with the exception of some special CFT's) admit a Lagrangian description.

The measure-theoretic issues Borcherds worries about may be important in certain approaches to QFT. They may give insights of one sort or another. But they may not be relevant to real problems of QFT (confinement, for example). I repeat (for the fourth or fifth time) the path-integral measure on Euclidean lattices is fine (and mathematical physicists, by which I mean people who know measure theory agree). Trying to convince yourself it isn't won't lead to progress. Proving that there is a continuum limit is not the main issue – there is no doubt that an asymptotically free theory has a continuum limit as the bare coupling vanishes. The problem is to understand the properties of that limit. Then, once that is done, one can try to clean things up mathematically.

My philosophy on such problems is that we need to study them any way we can that yields a bit of progress. For myself, this usually (though not always) means inventing techniques rather than theorems.

124. **Aaron Bergman**

June 6, 2007

I think we're talking past each other. I agree that the questions you discuss are all interesting. I agree that lattice gauge theories are beautiful, well-defined things. I'm not convinced, however, that the existence of the continuum limit is necessarily a boring technical detail. I was trained as a physicist; you won't find me arguing that all progress must necessarily involve rigorous theorems. Nothing could be further from the truth. However, I think it is important that things do get cleaned up in the end, and it is somewhat disturbing to me that QFT has not yet been cleaned up. If it's just ugly technical analysis, I'd like to know that. If it's a sign of some deeper obstruction, I'd like to know that, too.

This all goes back to the old question of what is a QFT. I think Wilson has certainly led us in the direction of a possible answer in the case of Lagrangian field theories. I want to know what the general structure this leads to. As a physicist studying the standard model, perhaps this isn't so interesting, but as someone who wants to study thing beyond the standard model, understanding fully what a QFT is strikes me as very interesting.

Not to belabor the obvious too much, but this isn't an either/or proposition; there are lots of interesting questions out there. If I had more cajones, maybe I'd even work on it, but I don't at the moment, and I think I'll stick with my derived categories for the time being. Technical analysis was never my strong point anyways :).

125. **Peter Woit**
June 6, 2007

Aaron,

The reference by Borcherds is just to the well-known fact that the things physicists write as "Dx", a putative infinite-dimensional version of the translation-invariant Lebesgue measure, don't make sense, even for a free particle moving in R^n. But there is a measure in this case that does make sense, Wiener measure. Even in quantum mechanics, to make sense of the path integral as a measure, you need to do two things:

1. Euclideanize
2. Include some version of [tex] e^{-\frac{1}{2}\int |{\dot x}|^2 [/tex] in the measure to get Wiener measure, not the non-existent Lebesgue measure

126. **Peter Orland**
June 6, 2007

Aaron,

I never said the existence of the limit is a boring detail. I said
it is a secondary detail.

127. **Aaron Bergman**
June 6, 2007

Hmmm — I read Borcherds's statement as more general than that (ie, the nonexistence of measures on field spaces — besides the Gaussian case, of course), but looking around Wikipedia, it looks like he probably does mean the more basic statement you refer to, however.

I thought there were some more general no-go theorems, but I can't seem to think of where I heard about them at the moment.

128. **Aaron Bergman**
June 6, 2007

(And, after googling a bit, it doesn't look like the Gaussian measure exists in higher dimensions, but it's not too hard to get around that in a well-defined way)

129. **Arun**
June 17, 2007

Not to belabor the obvious too much, but this isn't an either/or proposition; there are lots of interesting questions out there.

Is there a list of well-formulated questions about QFT out there somewhere, kind of like a Hilbert's problems?

Proof of the abc Conjecture?

May 26, 2007
Categories: abc Conjecture

While I was traveling this past week, there was a conference held here entitled L-functions and Automorphic Forms, which was a celebration of the 60th birthday of my math department colleague Dorian Goldfeld. From all I've heard the conference was a great success, well attended, with lots of interesting talks. But by far the biggest excitement was due to one talk in particular, that of Lucien Szpiro on "Finiteness Theorems for Dynamical Systems". Szpiro, a French mathematician who often used to be a visitor at Columbia, but is now permanently at the CUNY Graduate Center, claimed in his talk to have a proof of the abc conjecture (although I gather that, due to Szpiro's low-key presentation, not everyone in the audience realized this…).

The abc conjecture is one of the most famous open problems in number theory. There are various slightly different versions, here's one:

For each $\epsilon >0$ there exists a constant C_ϵ such that, given any three positive co-prime integers a,b,c satisfying a+b=c, one has

$$ c < C_ \epsilon R(abc)^{1+\epsilon}$$ where $R(abc)$ is the product of all the primes that occur in a,b,c, each counted only once.

The abc conjecture has a huge number of implications, including Fermat's Last Theorem, as well as many important open questions in number theory. Before the proof by Wiles, probably quite a few people thought that when and if Fermat was proved it would be proved by first proving abc. For a very detailed web-site with information about the conjecture (which leads off with a quotation from Dorian "The abc conjecture is the most important unsolved problem in diophantine analysis"), see here. There are lots of expository articles about the subject at various levels, for two by Dorian, see here (elementary) and here (advanced).

As far as I know, Szpiro does not yet have a manuscript with the details of the proof yet ready for distribution. Since I wasn't at the talk I can only relay some fragmentary reports from people who were there. Szpiro has been teaching a course last semester which dealt a bit with the techniques he has been working with, here's the syllabus which includes:

We will then introduced the canonical height associated to a dynamical system on the Riemann Sphere. We will study such dynamical systems from an algebraic point of view. In particular we will look at the dynamics associated to the multiplication by 2 in an elliptic curve . We will relate these notions and the questions they raised to the abc conjecture and the Lehmer conjecture.

For more about these techniques, one could consult some of Szpiro's recent papers, available on his web-site.

The idea of his proof seems to be to use a and b to construct an elliptic curve E, then show that if abc is wrong you get an E with too many torsion points over quadratic extensions of the rational numbers. The way he gets a bound on the torsion is by studying the "algebraic dynamics" given by the iterated map on the sphere coming from multiplication by 2 on the elliptic curve. I'm not clear about this, but it also seems that what Szpiro was proving was not quite the same thing as abc (his exponent was larger than $1+\varepsilon$, something which doesn't change many of the important implications).

Maybe someone else who was there can explain the details of the proof. I suspect that quite a few experts are now looking carefully at Szpiro's arguments, and whether or not he actually has a convincing proof will become clear soon.

Update: I'm hearing from some fairly authoritative sources that there appears to be a problem with Szpiro's proof.

Comments

1. **CG**
   May 26, 2007

   This would be most remarkable and exciting if true! I seem to recall that the good professor Terry Tao mentions the opinion of another mathematician, Shou Wu Zhang, on how the abc conjecture could be proved, namely, saying that it would follow if one assumed enough variants of the BSD and RH conjectures.

   http://terrytao.wordpress.com/2007/05/04/distinguished-lecture-series-iii-shou-wu-zhang-"triple-l-series-and-effective-mordell-conjecture"/

   So if abc is true, while it is obviously not strong enough to provide any information about these other extremely famous conjectures, the remark of Shou Wu Zhang seems to indicate that at least it doesn't rule them out (in their full generality) by contradiction.

2. **David S-D**
   May 27, 2007

   I'm told by a mathematician friend that abc doesn't imply Fermat; it implies asymptotic Fermat (sufficiently large exponents).

3. **Steve**
   May 27, 2007

   I agree with David S-D…in particular, on the site about the ABC conjecture which you link to, the consequence is said to be:

   "that there are only finitely many solutions to the equation x^n+y^n=z^n with gcd(x,y,z)=1 and n> 3."

4. **Peter Woit**
   May 27, 2007

   I should have been more precise. It's true that abc only implies Fermat for big enough n, but Fermat is known to be true by other methods for a large range of n, so abc would finish the proof (without Taylor-Wiles). You do have to check the coefficient you get in abc to make sure the ranges overlap, they do for Szpiro's proof.

5. **kasper Olsen**
   May 27, 2007

   Peter,

   The way you phrase the abc-conj above, it appears that C_\epsilon depends on a, b and c. The way I would phrase it would be to say,
   that **given** any epsilon > 0, there exists a constant C_\epsilon
   such that for any triple of positive integers a, b, c with a+b = c and gcd(a,b,c) =1… [etc]. Agree?

-Kasper

6. **Peter Woit**
May 29, 2007

Kasper,

Yes, that's right, I should have written things in another order to make that clear. The constant doesn't depend on a,b,c. Maybe I'll change it now…

7. **gaddeswarup**
May 29, 2007

According to Wikipedia Szpiro announced a proof of the 'Weak abc conjecture'. The paper http://www.math.columbia.edu/~goldfeld/ABC-Conjecture.pdf mentions a 'weak abc conjecture'.

8. **surlygrad**
May 30, 2007

How does abc connect to dynamical systems?

9. **Peter Woit**
May 31, 2007

surlygrad,

No real connection to conventional dynamical systems. What is being used here is iteration of an algebraic map of the sphere to itself.

10. **Z**
June 2, 2007

Rumours at my institution say that it is Emmanuel Ullmo, from University of Orsay, who is currently proof-reading the manuscript.

11. **layman**
June 4, 2007

I think it would be informative to flip between this blog and that of Dr. Lubos Motl – after all, it doesnt hurt to listen to a range of opinions. Unfortunately, when you follow a link from this blog, you get a message refusing you entry and, worse, refusing return.

So flipping is difficult. Too bad. Hope he changes his policy.

12. **Peter Woit**
June 4, 2007

layman,

I think you should take that up with Lubos, not much I can do about it. I suppose you could try posting a comment about this on his blog and not mine (although he'll probably delete it, following his policy of deleting comments he disagrees with).

He put that page in place the day I pointed out here that his posting claiming the MiniBoone experiment had confirmed the LSND result was wrong (actually the opposite was true).

13. **blank**
June 7, 2007

Hi,
What is your opinion about the recent paper posted in archive by T Pati disproving Riemann Hypothesis?

14. **Peter Woit**
June 7, 2007

That the Pati paper actually disproves the Riemann hypothesis seems extremely unlikely, and I don't know of anyone who has taken it seriously. The arXiv contains a large number of claims about the proof/disproof the the Riemann Hypothesis which are incorrect. The Pati paper is presumably also incorrect, but to find out why you need someone with some expertise who is willing to spend their time reading the paper and looking for the error. Perhaps the paper will get refereed and this will happen.

15. **blank**
June 7, 2007

I heard rumors that the paper has been submitted for publication (may be annals). The interest lies in the fact that Pati is a respected figure in India.
Moreover, he is not known to have such false claims before.

16. **blank**
June 7, 2007

Speaking about Riemann Hypothesis , recentle some suggestions were made by Alain Connes towards solution of Riemann Hypothesis by borrowing ideas from physics and may be non commutative geometry.

Can you shed any light on this , are there people seriously trying to prove the most important problem in number theory by borrowing ideas from physics?
Because that will have great implications……

17. **Peter Woit**
June 7, 2007

Over the years people have tried all sorts of ideas related to physics on this problem. Connes has some ideas related to non-commutative geometry, but has not claimed to have a proof of RH. My understanding is that in the case of RH for function fields (where a proof by other methods exists), there is progress on finding a proof by

Connes's methods. For comments by him about this, see this posting on his blog:

http://noncommutativegeometry.blogspot.com/2007/03/le-rve-mathmatique.html

18. **Peter Woit**
June 10, 2007

via Ars Mathematica, here's a link to a discussion of what is wrong with the Pati argument:

http://guests.mpim-bonn.mpg.de/kroetz/RH.pdf

Even More Stuff Than Usual

May 31, 2007
Categories: Uncategorized

Here are various things of interest that accumulated while I was away:

Last week there was a conference in Florence on the early history of string theory, some of the talks are available here.

Lots of blogging activity among Fields Medalists: there's a lot worth reading in Terry Tao's reporting on a series of lectures by Yau at UCLA here, here and here. At the blog of fellow Fields Medalist Alain Connes, there's mention of on a recent conference at Vanderbilt (slides here), as well as a report from Connes about a recent conference on the philosphical ideas of Wolfgang Pauli. Finally, yet another Fields Medalist, Richard Borcherds, has a blog. Not only do Fields Medalists like to have blogs it seems, but they also like to use them to discuss physics…

Some other blogs I've run across include A Strange Universe, from gravitational wave physicist Warren Anderson, which includes his Dire Straits inspired Papers For Nothing. Also John Armstrong's The Unapologetic Mathematician, which has a lot of expository material, and Julie Rehmeyer's MathTrek, a blog at the Science News web-site.

Steven Weinberg has canceled a planned public talk at an event to be held in conjunction with PASCOS 2007 at Imperial College in London in July, an event celebrating the 50th anniversary of the arrival of Abdus Salam at Imperial. In a letter to Mike Duff (available here), Weinberg says that he is boycotting the event in response to news of a boycott of Israel by the British National Union of Journalists, due to his belief that there is no possible explanation of this other than widespread anti-Semitism in Britain "especially in the intellectual establishment" or "a desire to pander to the growing Muslim minority in Britain." *Note: any attempts to use mention of this news to justify attempts to carry on the Israeli-Palestinian conflict in the comment section of my blog will be ruthlessly suppressed*.

As usual, Tommaso Dorigo is doing a great job of making current collider physics actually seem exciting and interesting. He's now spreading rumors of a 4-5 sigma excess of multi-b-jet events being seen by D0. What does CDF data show? He's keeping his mouth shut about that… Maybe there will be some excitement at the upcoming summer conferences…

There's a new Harvard College magazine about math, run by undergraduates called The Harvard College Mathematics Review. The first issue contains an article by Noam Elkies about the abc conjecture, and one by Dennis Gaitsgory about how not to teach linear algebra.

Via Mathephysique, here's an interview with theorist Edouard Brezin.

I keep running across more and more web-sites of theory groups that are putting up material from their theory seminar talks. The latest is the HEFTI Seminar Archive at Davis.

Mike Hopkins gave a Distinguished Lecture Series in Toronto recently. Only audio from the talks is available on-line, and I can attest that forcing someone to try and follow a talk they are interested in like Mike's The Topological WZW Space of Conformal Blocks just by listening to the audio without always being able to tell what he is writing on the board is just cruel.

Via the n-Category Cafe, notes from a recent conference in Kyoto on Link (also known as Khovanov) homology and categorification. Lots of interesting talks to read, but I'm especially fond of the abstract of Dror Bar-Natan's talk, which begins:

I'm over forty, I'm a full professor, and it's time that I come out of the closet. I don't understand quantum groups and I never did.

Now I don't feel so bad. Also, for inspiration, check out his Dream Map.

Update: A Chicago network news show has a recent segment about Fermilab and the hunt for the Higgs.

Update: The D0 rumor has made it to Slate.

Comments

1. **Joe S**
   June 1, 2007

   Here's an interesting link in response to Wienberg's decision and providing an other side to the story:

   http://counterpunch.com/jensen06012007.html

   Regards

2. **Mike Wiest**
   June 1, 2007

   Hi,

   I'm not able to read your entire site, but I have a general question about your critical stance toward string theory. My superficial understanding has it that there are many possible vacua of the "theory," each corresponding to different physics, and we don't know which is the "true" vacuum, so one says the theory makes no predictions and cannot be falsified.

   However, I thought I had come across schemes that claimed to reproduce the standard model with very few parameters. (I don't remember details; the one author I can remember is Dmitri Nanopoulos; this was in the late 90's). Are these claims false? And if not, doesn't it count as huge progress to go from a 19 free-parameter theory to one with few parameters and a theory of quantum gravity to boot?

   Thanks!

3. **Peter Woit**
   June 1, 2007

   "Are these claims false?"

   Yes.

No one has been able to use string theory to calculate any of the standard model parameters in terms of the others. If you don't believe this, try and find a string theory prediction of the one still unknown SM parameter (the Higgs Mass) in terms of the others, or a string theory prediction of anything that will be seen at the LHC.

It would be huge progress if string theory (or any other theory) was able to reduce the number of free parameters in the SM, but this has not been done. Any claims otherwise are intentionally misleading.

4. **Eric Mayes**
June 1, 2007

Peter,
Your statements are not entirely correct. The model that we present in hep-th/0703280 is very close to reproducing the three-generation MSSM, including the standard particle masses and mixings. We can make predictions of the superparticle spectrum and Higgs mass with essentially only five parameters, the moduli VEVs and the gravitino mass. We can restrict the range of these parameters by demanding that the relic neutralino density is in the range to produce the observed dark matter density. To be fair, this is only one vacuum among many and we haven't completely addressed the issue of moduli stabilization, however the model is testable in that either this vacuum will correctly predict the pattern of superpartner and Higgs masses or it won't. By the way, the model generically predicts the Higgs mass to be in the range 117-121 GeV.
Just want to point this out. Hopefully, this doesn't trigger another round of angry post.

Cheers,
Eric

5. **M**
June 2, 2007

dear Eric, you do not have any string prediction for the Higgs mass. You just ASSUME tanBeta = 46 (page 3 of your paper) because you know that, according to a computation done 15 years ago in quantum field theory, this gives m_higgs = 91 GeV + (top loop correction) such that the Higgs mass is slighly above the LEP bound m_HIggs > 115 GeV. Had you ASSUMED a lower tanBeta, you would have obtained a Higgs mass that contradicts the LEP bound.

6. **A quantum diaries survivor**
June 2, 2007

Alas, M, your comment is at the very essence of the problem that plagues HEP today. What is the predictiveness of a model (say Supersymmetry, or string theory, or whatever) that contains (many) new parameters that can be adjusted to provide whatever outcome one wants ?

I remember years ago, when many expected SUSY would be discovered in a discrepancy in the sides of the CKM triangle… The Bs mixing delta M was expected to be 30/ps or so. Now it is 18+-1/ps, the triangle closes beautifully and what do susy theorists do ? They cook up a "MFV scheme" to keep away from inconsistencies. M is for "minimal", to imply they have grown accustomed to walking on tiptoes to avoid perturbing the beautiful consistency of the SM.

I wonder how much sense that does. Given the large phase space allowed to SUSY parameters years ago (masses of SUSY particles at 80-100 GeV, large effects in BR through loops, etcetera), and a "flat prior" assumption for their values, wouldn't it be reasonable to compute the "confidence level" of SUSY today ? We would find we have excluded it at well more than 95% confidence level.

Mind you, I do not usually give too much weight to "95% confidence levels". They are not very meaningful if you do not specify the probability distribution you have integrated. For instance, the direct limit for the higgs at 114 by LEP II is very strong, because if you go even slightly below 114 the probability becomes zero quickly – they would not have missed the higgs at 112. An example of the other kind is the "upper 95% cl limit" from indirect fits of SM observables at 166 GeV: even 200 GeV is not wildly unlikely – it just entails one or two wrong measurements of electroweak parameters.

My bottomline is: a theory which can be tuned to give just about any possible outcome is not a theory, it is an exercise. Not worth investing a large part of a budget of funding agencies!

7. **A quantum diaries survivor**
June 2, 2007

And I would like to add something to my previous comment. I remember that in the late eighties and early nineties, when the top quark had not been found yet, theoretical predictions for its mass used to follow closely the lower limits from experiments: some theoretical calculations indicated a mass of x+-x/3, then a direct limit would arrive at 1.2*x, and a new prediction then would be published at (1.5+-0.3)*x. New collider data would then exclude masses lower than 1.6*x, and guess what, new predictions would foresee masses around 1.8*x…

It looks like some theorists love to have a bet out which makes it possible for them to dream hitting the jackpot. I am not implying these are not serious scientists – just that the game is a bit silly.

8. **Peter Woit**
June 2, 2007

Eric,

Your model, like all such things, not only doesn't reproduce the SM with fewer parameters, but actually adds more undetermined parameters. And, in any case, it isn't a "prediction of string theory" anyway.

9. **The Professor in Black**
June 2, 2007

Cool Dire Straights song/link.

Many of you are familiar with Johnny Cash's song "San Quentin," which he perfomred in front of the inmates of San Qunetin, during one of his famous prison concerts.

It has recently come to light that he also traveled the world, singing to those who were placed in non-tenured postdocoral/lectureship postitions by string theorists–those everlasting prisons for today's freethinkers.

The Man in Black sang,

String Theory, you've been livin' hell to me
You've hosted me since nineteen eighty three
I've seen 'em come and go and I've seen them die

And long ago I stopped askin' why

String Theory, I hate every inch of you.
You've cut me and have scarred me thru an' thru.
And I'll walk out a wiser weaker man;
Mister Congressman why can't you understand.

String Theory, what good do you think you do?
Do you think I'll be different when you're through?
You bent my heart and mind and you may my soul,
And your m-brane walls turn my blood a little cold.

String Theory, may you rot and burn in hell.
May your walls fall and may I live to tell.
May all the world forget you ever stood.
And may all the world regret you did no good.

String Theory, you've been livin' hell to me.

Johnny Cash said to the exiled postdocs, "I was thinking about you guys yesterday. I've been here three times before, and I think I understand how you feel about some of the things. It's none of my business how you feel about some of the things, and I don't give a damn about how you feel about some of the things. But anyway, I tried to put myself in your place and I believe this is how I would feel about String Theory" — Johnny Cash @ The San Quentin String Theory Conference

10. **Ralf**
June 2, 2007

Exoneration. – Asked whether she agreed with the characterization of the present as the "age of the fakers," somebody insisted on the caveat that the popular rendering of string theory can be a source for subjective cosmological mysticism, along the lines of, say, a romance novel. Thus, it would not be fair to refer to string theorists as fakers, if they were marketing it like dime store romance novels.

11. **Chris Oakley**
June 2, 2007

Professor in Black,

I'm sure that this Johnny Cash ballad will take its place alongside that other classic of the genre, "He ain't heavy, he's my supersymmetric partner."

12. **Chris Oakley**
June 2, 2007

Ain't no selectron, at less than 10 TeV
Ain't no squark that a detector can see
But I won't give up on supersymmetry:
Just need more time, and more money.

(… or something like that)

13. **Yatima**
June 2, 2007

> A Chicago network news show has a recent segment about
> Fermilab and the hunt for the Higgs.

Umm… very nice. When did that annoying style of explaining everything through movie-teaser soundbites become so prevalent? "Shake the Higgs loose"? Oh well.

And there's an allusion to the ILC at the end! Let's not get carried away though, the Real World has a way of crushing dreams.

14. **Joseph Smidt**
June 2, 2007

"As usual, Tommaso Dorigo is doing a great job of making current collider physics actually seem exciting and interesting."

Thanks for the heads up.

15. **Mike Wiest**
June 3, 2007

Thanks very much for the helpful responses from different perspectives, and the bittersweet lyrics. I appreciate the specific examples without overwhelming technical detail.

So, to press a little further, I think I understand the point that the various string-derived particle spectra represent putative vacuum states that might not really be the true and stable ground state of String Theory, so we may want to avoid calling such theories "predictions" of string theory. But if we can find one or many of these theories that describe known physics and unify all the forces–can we afford to discard them? We would perhaps need to stop thinking of string theory as a fundamental theory, but isn't just having a unified description a major step forward? Aside from counting free parameters, if we have a theory that's in some sense arbitrary, like the standard model, but it does unify all the forces…isn't it superior (even if we KNOW it's not the true vacuum of the theory)?

Doesn't string theory unify the forces, if considered as merely a phenomenological description rather than a fundamental theory? Forgive my ignorance, but do we actually have any other theory that does this?

Finally, since you were all so straightforward, I have to demonstrate my ignorance again to ask this–is there any other theory of quantum gravity that is finite or renormalizable?

Muchos thanks!

16. **Peter Woit**
June 3, 2007

Mike,

The true ground state of string theory is presumably known, it's one of the supersymmetric ones like flat 10d space-time. Problem is that this ground state looks nothing like the real world. What string theorists are doing now is looking at metastable possible ground states that might look more like the real world. But they are extremely complicated and exist in huge number and variety. The bottom line is that string theory is not able to predict anything at all. To claim that you have "unified" physics with a theory that doesn't predict anything just makes no sense.

People now think N=8 supergravity might be finite, and there's a whole research program pursuing another idea about quantum gravity (loop quantum gravity) which arguably has been as successful as string theory in terms of coming up with a quantum gravity theory.

17. **Mike Wiest**
June 3, 2007

Peter,

When you say string theory is not able to predict anything at all, I'm taking you to mean that we have no principled way of choosing among the huge number of ground states that (might) look like the real world. But are you saying that even if we pick one of those adequate ground states we don't have a unified description of the the forces? Because the parameters are still too unconstrained? Or is it because we can't tell if any single one of the promising ground states is "adequate"?

I don't quite see why it makes no sense to think of each "adequate" quasi-ground state as a consistent unified description of all the forces. I mean, we can't derive the weak mixing angle in the standard model but we were pretty happy to get the weak interaction unified with the electromagnetic, right?

If our situation is that we have too many adequate theories, isn't that better than none?

Thanks for your patience!

18. **Peter Woit**
June 4, 2007

Mike,

There are two huge problems here, one of practice, one of principle:

1. In practice, one doesn't know what the full, non-perturbative string theory is, so one is working with various approximations and assumptions. As a result, for each conjectural metastable ground state, all one can get are various crude approximate "predictions", you can't actually extract any accurate predictions of SM parameters.

2. In principle, even if you had a full theory and could do reliable, accurate calculations in it, the number and variety of these states is so huge that no one has a credible proposal about how to get a prediction of anything out of studying them.

In electroweak unification, sure you can't predict the Weinberg angle, but you can predict a huge number of other things and test these predictions. The problem with superstring theory unification is not that there are things it currently can't predict, but that it can't predict anything at all. Again, trying to sell people on how wonderful your unified theory is, when it can't predict a single thing about anything, is obviously problematic.

19. **Mike Wiest**
June 4, 2007

Hi Peter,

I'm thinking of "predictions" as being an orthogonal dimension to "unification." If I accept that these theories (or "schemes") make zero predictions, do they still give me a unified description of the fundamental forces? Even if we have to fit every single parameter to experiment, don't we have something we didn't have before, namely, a way of seeing all the forces as arising out of one kind of stuff?

Sorry if I'm sounding like a not-too-bright broken record. You seem to feel that a unified theory is not a unified theory if it doesn't make any predictions. But isn't there a difference between a unified description as I'm imagining exists in string theory, as opposed to the mere aggregate of interactions that we have in the standard model?

Maybe your point is that if you can't do reliable calculations in any of these theories, then you don't have any unified description to speak of. Are you saying something to the effect that, even if I set all the parameters, we don't know how to calculate cross-sections and whatnot? So the indeterminacy is not just in the parameters, but somehow the theory turns to mush if you try to calculate anything even given the parameters?

In that case, would it not even qualify as a theory of quantum gravity? Am I wrong to believe there is a consistent quantum theory of gravity in there somewhere? ("No, Mike, you're NOT EVEN wrong…")

I do appreciate the importance of being able to predict things…but I also tend to see unification as valuable in itself.

20. **Peter Woit**
June 4, 2007

Mike,

As far as I'm concerned, you're just not doing science unless you can test your theory against experiment, i.e. make predictions with it and check them. One of the main reasons we want a "unified" theory is that by relating different parts of physics we should get more predictions we can check, not less. A "unified" theory that can't predict anything is just not a scientific theory.

21. **tty**
June 4, 2007

just to add a new stuff not included in your blog, there's a good article by Smolin in New York Review of Books, about some new books on Einstein.

22. **Peter Woit**
June 4, 2007

tty,

Thanks for mentioning that, I was going to include it on this list, but seem to have missed it. There's a detailed posting about this and discussion at Cosmic Variance.

23. **Mike Wiest**
June 5, 2007

Peter,

I think you've just about convinced me. It would seem that if one's putative unification doesn't constrain the different sectors by relating them to each other, then nothing really got unified. If I think of an example where the unification seems substantive but no new predictions are made, I'll get back to you…

But–is it a finite theory of quantum gravity? Or again, because it is so unconstrained, do you say there is no theory there?

24. **A quantum diaries survivor**
June 5, 2007

Hi Peter,

about PASCOS: I will attend the conference for a couple of days (so what, your readers are entitled to ask). I will be presenting CDF tests of the Standard Model (so what ?). Nothing, just to mention that I will try to blog about what I hear there. I think I will not understand much of the stringy talks, but at least I hope to give some report of the cosmology part.

Cheers,
T.

25. **Peter Woit**
June 5, 2007

Mike,

String theory really isn't a finite theory of quantum gravity. It only gives the terms in a perturbation series, and it is these that are supposed to be finite. The series doesn't converge (it's presumed to be a divergent, but asymptotic series). Conjecturally, there is some non-perturbative finite theory, to which this series is asymptotic. But, even if this conjecture is true, the problem is that the evidence then is that you'll end up with not one theory of quantum gravity, but 10^500….

26. **Mike Wiest**
June 6, 2007

Sweet!

Gotta love that.

What does divergent but asymptotic mean? Can you really be divergent and at the same time get asymptotically close to a finite number?

27. **Peter Woit**
June 6, 2007

Mike,

Asymptotic series are ones that become arbitrarily accurate for arbitrarily small coupling, also they get closer to the true answer as you compute more terms up to some point, where they start getting worse. Depending on how small your coupling is, in some cases they can give quite accurate results, even if there is an inherent limit to this accuracy. In QED the perturbation series is presumably just asymptotic, but quite accurate. In string theory you have no idea since you don't know what the coupling is. But, when you only have an asymptotic series, it's misleading to claim you have a finite theory.

28. **Mike Wiest**
June 11, 2007

Aha. Why would QED be "presumably" only asymptotic with a coupling of 1/137? Because you know things change at the Planck scale (or before)?

Thanks Peter.

29. **Peter Woit**
June 11, 2007

Mike,

There's an old argument (due to Dyson), that in QED the way things depend on the coupling constant [tex] \alpha [/tex] can't be an analytic function at [tex] \alpha=0 [/tex] since the vacuum is unstable for even infinitesimal negative coupling constant. Only for analytic functions will you get the power-series converging to the true answer. So, you expect to only have an asymptotic series in QED, this is independent of how big the coupling is. Since the coupling is quite small, even though the series is only asymptotic, at low order one expects it to be very accurate (which it is). More detailed arguments imply that the series expansion should get better and better up to order around 137, only for higher order calculations than that does it start to get worse. So, for all practical purposes, the series expansion is fine.

30. **Mike Wiest**
June 18, 2007

Interesting.

Thanks!

Endless Universe

June 2, 2007
Categories: Book Reviews

There's a new popular book about cosmology now out on bookstore shelves, [Endless Universe] by Paul Steinhardt and Neil Turok. The authors are the inventors of a competing model to inflationary cosmology, variously called "ekpyrotic" or "cyclic" cosmology. They describe coming up with the same idea simultaneously during a lecture at Cambridge on M-theory by Burt Ovrut back in 1999. Ovrut was describing his work on the Horava-Witten scenario, which involves two parallel branes (we live on one of them), and during the lecture both Steinhardt and Turok started wondering about whether one could explain the big bang as a collision of branes. They went up to discuss this with him after the talk, and continued the discussion on a train ride to London that evening to see a performance of the play Copenhagen. This train ride was a central part of a 2002 BBC TV program Parallel Universes and a recent play Strings by Carole Bugge that I saw performed here in New York late last year.

The book is very much an advertisement for cyclic cosmology, and devotes a lot of space to doing something which is rarely done, explaining the problems with inflationary cosmology. Steinhardt has worked extensively on coming up with viable inflationary models, and a large part of the book explains the story of this research. Perhaps the most interesting aspect of the book is the story that Steinhardt and Turok each tell about their careers and how they ended up working together on this alternative to inflation. The problems with inflation are described in the context of promoting their cyclic model of branes colliding, moving apart and then back together in a repeating pattern. They heavily sell the idea that a cyclic model with no beginning of time is conceptually much preferable to a standard inflationary model in which the universe emerges at a given time. Reading this made clear to me why, at the recent String Cosmology meeting here in New York, Turok was so persistently questioning one speaker about whether everything he was doing didn't just depend on an unmotivated choice of initial conditions.

Steinhardt and Turok do a pretty good job of demolishing the inflationary multiverse and the associated Anthropic Landscape philosophy that has become so popular in recent years. They correctly describe the main problem with the inflationary multiverse as the lack of any way to test the idea, even in principle, making it not really a scientific idea at all. As for the testability of their own theory, they devote an entire chapter to the question of contrasting its predictions for the CMB with those of inflation. They claim that both cyclic and inflationary cosmology make the same predictions for the WMAP results (implicitly criticizing the commonly made argument that WMAP provided strong support for inflation). The one possible test that they point to that could distinguish the inflation and cyclic scenarios is the expected more sensitive measurement in coming years of a possible B-mode polarization signal due to gravity waves in the CMB. They claim that inflation predicts a significant amount of B-mode polarization, whereas the cyclic model doesn't. Unfortunately, from what I can tell by looking at the recent literature on "string cosmology" (e.g. here), various inflationary scenarios can give a wide range of amounts of such polarization, with stringy models like "brane inflation" and "modular inflation" leading to essentially none, just like in the cyclic case. So, I guess the cyclic model is in principle falsifiable, if next generation CMB experiments turn up measurable B-mode polarization. But if this doesn't happen, I don't see how one is ever, even in principle, going to distinguish experimentally between the cyclic and inflationary scenarios, which will make this whole area of research highly problematic.

On the whole the book seems to me to be too much of an advertisement for a very speculative idea, and I don't think the public needs more of this in this kind of format. I didn't notice anything in the book about what the case against cyclic cosmology might be, so anyone who wants to find out the other side would have to go on a search of the scientific literature, something most members of the public might not be able to do. Most strikingly, since the cyclic model is based on brane ideas motivated by string theory, the book contains endless hype about string theory, without so much as a word about its problems. One would have to read extremely carefully to realize that there is not a shred of experimental evidence for string theory. As for recent public debates about the problems of string theory, the authors just pretend they don't exist. They give a long list of recent popular books in this area, such as those of Susskind and Vilenkin promoting the Landscape, but somehow neglect to include the two such books that have taken a critical point of view on string theory.

Associated with publication of the book, it looks like there will be various stories in the media promoting the cyclic vs. inflationary debate, trying to make it into a modern version of the old steady-state vs. Big Bang controversy. On the Edge web-site there's a recent piece by Turok, which gives a good explanation of his current research and point of view. From NPR, there's a very recent radio show about the cyclic model, entitled Forget the Big Bang Theory, where "renegade physicist" Turok's model is described as "fighting words in the halls of science."

Comments

1. **anon.**
   June 2, 2007

   They claim that inflation predicts a significant amount of B-mode polarization

   Many people would disagree with that. The well-known Lyth bound suggests that observable B-mode polarization implies Planckian field values. This is extremely difficult to obtain in string theory; see work by Baumann and McAllister, for instance. It's also not very plausible in effective field theory, where one expects potentials to get Planck-suppressed corrections and flatness at trans-Planckian scales would be difficult to explain. There have been attempts to circumvent this with, e.g., inflation driven by large numbers of axions, but nothing very compelling. An observation of large tensor signal would cause a real theoretical crisis, I think.

2. **Kris Krogh**
   June 2, 2007

   Peter,

   Glad to see you shine a little light on cosmology, another near-monopoly situation with many parallels to string theory. Who says the Big Bang is "the only game in town?"

3. **Coin**
   June 2, 2007

   Who says the Big Bang is "the only game in town?"

   Ah, be careful, that's the kind of statement that you'll tend to see quote-mined on Talk.Origins later 🙂

   My understanding of the "cyclic universe" model described here is that it does not supplant the traditional big bang model (at least the big bang model as the layman would understand it), but rather extends it. Is this not correct?

4. **Kris Krogh**
   June 2, 2007

   Hi Coin,

Haven't read the book, but the universe had a beginning in BB. Somehow "Endless Universe" doesn't sound like that to me.

5. **M**
June 3, 2007

Inflation predicted that primordial inhomogeneities are adiabatic, Gaussian and with a quasi scale-invariant spectrum. All of this was later confirmed by WMAP and other observations.

Bounce cosmologies have problems in postdicting these features. First, cosmological singularities in string theory are not understood well enough to know if a bounce really occurrs. Second, perturbations generated "before" the bounce do not need to be scale-invariant, in gross disagreement with observations (maybe some miracle happens during the bounce: we do not understand this critical phase). This can be fixed in specific models: by assuming ad-hoc potentials, by introducing extra fields.

This is why many cosmologists do not consider ekpyrosis as a serious competitor to inflation. And somebody jokes that ekpyrosis sounds like a beautiful name for a new illness…

6. **Alex Nichols**
June 3, 2007

'Ekpyrosis' is actually an old term derived from the Stoic philosophers, implying that the universe was created "out of fire".
Certainly, the observational evidence implies that the early universe was very hot.

S & T also revived the term 'Quintessence' in their cyclic model, originally the Aristotelean "5th element", through which the stars and planets were believed to move. They use it as an explanation for 'dark energy', suggesting that it's a slowing rolling force (currently increasing), which explains recent obervations on cosmic expansion.

Cyclic Cosomologies were quite popular in the early 20's, based on a closed universe with a negative cosmological constant.
For example, the Friedmann, Walker, Roberton, Le Maitre models.

In the 1930's, Richard Tolman showed that this would lead to an entropy build-up, so that each cycle would become larger than the previous one, eventually leading to a situation where gravitational attraction would be insufficient to cause re-contraction. This severely reduced the model's explanatory power and it went out of favour.

Then in the 1960's Penrose and Hawking showed that gravitational singularities would form in any big-bang scenario based on General Relativity. This would erase the information from previous cycles. They didn't accept that there was any evidence that the "big-bang" was preceded by a "big crunch". Penrose in particular, has argued that there is no reason why a 'crunch' would lead to a 'bounce'.

As current thinking is that the cosmological constant has a small positive value, gravitational cyclic models based on General Relativity and the Friedmann equations are not in favour.
A quantum or string based description of the events approaching a big bang singularity would be required to explain what actually caused such an event.
This is what Steinhardt and Turok are attempting to provide with the idea of a brane collision.

So far, it hasn't been disproved by the WMAP results and future experiments such as the Planck satellite should be able to provide tests of the polarisation of the CMB and Spectral tilt.

A more radical view of the situation would be that it's not meaningful to try and explain 'the universe' as a physical system and that valid physics can only be performed locally.

In such a view there is an extremal indentity between the singularity and cosmic horizon. As one approaches either, quantum and or string effects are essential in explaining what happens, but it's only possible to infer these theoretically, rather than directly measure them.

S & T certainly do list a series of observational consequences of their model that can be tested.

7. **island**
June 3, 2007

Then in the 1960's Penrose and Hawking showed that gravitational singularities would form in any big-bang scenario based on General Relativity. This would erase the information from previous cycles.

Interesting, if you project relativity backwards to the point where inflation is alleged to end, then that is also where you conveniently find "thermalization", reheating or, "Ekpyrosis".

The singularity arrises only if this isn't indicitive of a big bang in a universe that has pre-existing volume… which would *not* erase information from previous cycles.

8. **Peter Woit**
June 3, 2007

Alex,

At least in the book, their claim is that their model has the same observational consequences as inflation, except for the CMB polarization.

9. **Navneeth**
June 3, 2007

Associated with publication of the book, it looks like there will be various stories in the media promoting the cyclic vs. inflationary debate, trying to make it into a modern version of the old steady-state vs. Big Bang controversy.

It has made it to Slashdot. 🙂

10. **matteoeo**
June 3, 2007

I'm an italian physics undergraduate student. Although I took several classes in GR and cosmology, I'm no expert. But it doesn't take an expert to understand that the state of cosmology is pretty messy. I think that there is some very smart speculation going on, but on the whole it seems to me that cosmology is too young a science to really tell something about the universe. What mostly strikes me is that cosmology uses as ingredients all kinds of speculative (sometimes unscientific) hyphotesis and still unknown objects: higgsboson, SUSY breaking, superpartners, strings, branes, dark matter, dark energy, CC, quintessence, extended quintessence, GUTs an GUT simmetry breaking, and so on. As a result you can adjust inflation so to have almost any CMB, but this is not really scientific. If every ingredient had a 50% possibility to come out

true, which is the probability that cosmology makes sense at all?
I'll tell you why I feel so uncomfortable with cosmology. A few years ago italian cosmologists and theoreticians Matarrese, Riotto, Notari and american Kolb showed (here) that today's accelerated expansion might be explained as the effect of inhomogeneities in the matter distribuition (super-Hubble wavelenght fluctuation modes), which combined with standard GR act as a cosmological costant in the Friedmann equations (they assume inflation). So standard GR and a better comprehension of complex behaviour of matter coming out from the fact that the universe is not homogeneous at all might rule out ad hoc-CC and DE. But what is most interesting is to read the thread of arguments that these pepers raised, with all kinds of objections, mostly of a very general flavor: no go theorems for irrotational matter to cause negative pressure (doesn't apply to their case though), a discussion on whether or not these effects might suffice to give negative pressure and the need for a non-perturbative approach, whether or not one can use Friedmann equations and modify them while assuming an inhomogeneus universe and so on.
I'm not publicizing their idea, which I cannot evaluate; but I strongly believe that this whole thing shows that the very foundations of Cosmology (Cosmological Principle above all) are not understood and questionable, that all sorts of very general problems are not at all estabilished, and thus work in the field is easy to criticize (cosmologists dismantling others'proposals). I also think that before assuming all kinds of hyphothesis a much better understanding of GR dynamics, complex systems and collecive phenomena should be gained.

11. **Alex Nichols**
June 3, 2007

re Penrose/Hawking:-

During the 1990's Penrose suggested that the boundary condition of a collapsing universe could be distinguished by a large Weyl tensor.
Hawking agreed but said it was determined by the no boundary proposal.
Turok later worked with Hawking on an inflationary model based on this, but has moved on since then.

12. **Malo Juevo**
June 3, 2007

The paper by Kolb et al. was known to be obviously wrong from the moment it appeared. Working out precisely what the error was turned out to be not so easy, but perturbations around Robertson-Walker spacetime have been well understood long enough that something like this would not have been missed. The calculations in the paper are quite intricate, making it very difficult to keep track of what the range of validity of perturbation theory is at each step. However, if one looks at it very carefully, the perturbative technique is not valid. In some sense, this might be expected, since the whole point of the paper is to claim that a certain variety of perturbations are singular, yet the paper is done entirely in non-singular perturbation theory.

13. **Peter Woit**
June 3, 2007

If you have informed comments about Steinhardt-Turok, please post them. But this is not a general Cosmology discussion board, and I can't and won't moderate such a thing, so if it's not about Steinhardt-Turok or their book, don't post it here.

14. **Ari Heikkinen**
June 3, 2007

Peter,

It appears to me that "Endless Universe" is more like speculation on a new theory than it's cosmology, so could you suggest a good university level book (preferably self contained) on modern cosmology which would include all the relevant observations of the universe and their related math, but in a manner that it would explain what's seen, without trying to speculate on any specific theory?

15. **Cynthia**
June 4, 2007

In the "Edge" transcript, Turok uses the term "_mdash" not once but three times: "realistic&mdashspoils", "bring&mdashlike", and "argument&mdashhand". Although the meaning of the term "m&dash" is pretty obvious (to me, that is), does anyone know if it's really proper to be using this term in essay format?

16. **Chris W.**
June 4, 2007

Cynthia,

The occurrences of &mdash in the text are due to sloppy HTML editing on somebody's part; I doubt Turok had anything directly to do with it. **—** (—) is an HTML 4.0 *character entity*.

[Example: *Web developers do make mistakes—sometimes*.]

See this reference.

17. **clarity**
June 4, 2007

Peter,

I just want to make a few distinctions which are, I think, a little muddy in your posting. First off: inflation, brane inflation and the multiverse/anthropic landscape are all distinct. The simplest inflationary scenario is very well supported by the data and is certainly testable (it's already been tested!). Particular string theory embeddings of inflation (such as brane inflation) may or may not be falsifiable, though there isn't currently a way to rule out inflation from string theory (however, a positive signal for tensor modes would give stringy inflation a hard time). Eternal inflation and the multiverse are both controversial, even among proponents of inflation and are certainly not necessary parts of the puzzle. Inflation doesn't necessarily have anything to do with anthropic arguments and many cosmologists dislike anthropism as much as you do.

18. **Peter Woit**
June 4, 2007

clarity,

Sorry if I wasn't clear, I am aware of the distinctions you mention, and that simple inflationary scenarios do have some real experimental support (as well as problems that Steinhardt and Turok do a good job of describing). I didn't mean hear to be criticizing this kind of thing, I think it's serious science (brane inflation and eternal inflation/multiverse are a different story).

19. **clarity**
June 4, 2007

Peter,

Thanks, I just wanted to be sure that you weren't trying to lump all those things in together. The problems with inflation are largely conceptual and, to my mind, many of these problems have deep quantum gravity issues at their core. Personally (you are likely to disagree with this) I think that it will be hard to completely resolve all the problems of simple inflationary scenarios without coming up with some kind of UV complete, quantum gravity version of the story. So in this sense I don't think that brane inflation (and similar scenarios) are nearly as poorly motivated as you think they are. I'm not trying to argue that any of these string theory inflation models actually solves any of the outstanding problems of inflation, just that trying to embed inflation into a quantum theory of gravity is probably a step in the right direction. (I should probably mention that I don't think that the ekpyrotic/cyclic scenarios offer much, if any, resolution to these problems, but that's another story entirely…)

20. **Peter Woit**
June 5, 2007

clarity,

My own general view is that to understand inflation you will need to understand the beyond Standard Model physics that is driving it. Quite possibly this requires a good theory of quantum gravity. Historically I guess the hope was that maybe inflation came from some kind of Higgs sector, but I gather that doesn't work.

The problem with all string theory inflation scenarios that I've seen seems to me to be exactly the same as the problem string theory has with getting particle physics: compactification. You have to invoke complicated compactifications in order to avoid conflict with what is known, and this ruins predictivity. I've looked a bit recently at brane inflation papers (including a recent Klebanov et. al. one this evening), and the constructions just look complicated, ugly, and with no hope of leading to real predictions.

21. **clarity**
June 5, 2007

Peter,

"My own general view is that to understand inflation you will need to understand the beyond Standard Model physics that is driving it. Quite possibly this requires a good theory of quantum gravity."

We agree on this for sure. The thing that makes the quest to embed inflation into beyond-the-standard-model physics interesting is the fact that inflation is not at all a generic property of sensible theories of high energy physics. Typically the requirement of sufficiently many e-foldings puts stringent (often prohibitive) constraints on the underlying theory. This is one of the main points of the recent Baumann et al. papers – it's very difficult to tune the potential to be flat enough (even though a naive parameter counting suggests it should be simple). The fact that it's so difficult to come up with concrete, working models of inflation in string theory suggests, to me, that the models which do work will be at least somewhat predictive. (I'm still working through the paper you mention so I won't comment on how falsifiable this scenario is, though I think that a large tensor signal would rule it out for sure.)

Of course, as you mention, there are aesthetic issues and I won't claim that these constructions aren't complicated. It's not clear to me how complicated is "too complicated" when dealing with these kinds of theories, there's certainly some personal prejudice involved in making that call. Given that it's far from obvious that one can construct concrete, explicit, successful inflationary models in string theory, I think that any example (even a complicated one) is interesting.

22. **Peter Woit**
June 5, 2007

clarity,

I think even the authors of the paper I mentioned acknowledge how problematic the whole thing is, since they end their paper with:

"Finally there is a pressing need for a more natural model of string inflation than the one we have presented here".

which is equivalent to saying that they believe the model they are working with is highly unnatural and clearly they have little faith that it reflects the real world.

Personally, looking at the calculations done in this and similar papers, the whole thing seems to me completely absurd. You're invoking a huge amount of complicated structure, and trying to explain just a few observable numbers. This is hopeless. It's not an aesthetic question, it's a question of whether the number of assumptions that go into your calculation is larger than the number of observable predictions you can derive from it, and thus whether your framework is predictive.

Falsifiability is not enough to make a legitimate testable scientific theory. My theory that everything that happens in the universe happens because it is controlled by a malevolent being with 11 toes is falsifiable, because an omnipotent malevolent being with 10 toes may put in a public appearance at any moment. The fact that a complicated cosmological model predicts no measurable effects of primordial gravitational waves is not enough to make it a testable model in the usual sense. You have to come up with measurable effects that are characteristic of the model, and distinguish it from others. I don't see anything like that even conceivable with the string inflation models people are working on.

23. **wow**
June 6, 2007

Several experts now think cosmic strings from brane inflation (admittedly a very model dependent phenomena) may be distinguishable from the traditional abelian Higgs model networks due to different properties of intercommutation as well as (p,q) flavor structure; this is not obviously true or false, but is in any case a potential distinguishing signature that experts are sorting out.

A similar thing could be said about non-Gaussian signatures that arise from inflation with D-branes governed by the DBI action, in the regime where the kinetic structure is important (not slow roll). Experts on non-Gaussianity (including the WMAP team) use this model as a reference point to bound non-Gaussianities.

So I think your claim that characteristic measurable effects aren't "even conceivable" is not held by most of the people who actually try to build inflation models, or test them experimentally.

24. **Peter Woit**
June 6, 2007

wow,

Your first example starts out "assume we observe a network of cosmic strings…", something which most people would describe as an extremely unlikely event. From there you go on to note that this is a very model-dependent phenomenon and it's not clear if you can even distinguish it from non-string theory cosmic strings. Sorry, this just doesn't sound to me like a plausible test of brane inflation models: you're starting assuming something that isn't going to happen, then acknowledging that even assuming this, you can't necessarily make any predictions.

Your second example again assumes that we observe something that conflicts with simple inflationary models. Fine, but is this really a distinctive signal that one is seeing brane inflation? I can't help but suspect that if such an effect is observed, people will have no trouble coming up with many very different models that would explain it.

25. **wow**
June 6, 2007

By definition, a "distinctive" signature has to be something that
is not common to almost all models. So such things will be, at some level, unlikely. I suspect most working theoretical cosmologists would assign significantly greater probability to observing a cosmic string network, or non-Gaussianity, than they would to theoretical consistency (let alone experimental verification) of the Ekpyrotic/Cyclic scenario. I agree that most would also suspect absence of all these things; but that is just saying that vanilla inflation will look good, and we won't know the correct model at the level of a very specific potential or (in higher dimensional scenarios) compactification manifold.

My point was that signatures with roughly this likelihood constitute a major portion of what phenomenologists work on/hope for, and experimentalists try to measure, since by virtue of their somewhat rare nature they are very instructive when found. And it is quite conceivable to a majority of active workers in theoretical cosmology that such signatures are forthcoming sooner or later, to the extent that major current experiments use models motivated in this way as benchmarks. For me this puts your claim that there are "no conceivable" singatures of such models, in a more realistic context.

26. **clarity**
June 6, 2007

Peter,

You're way off about wow's comment about large nongaussianity. Many theorists have worked very hard to come up with inflationary models which have large nongaussianity and there are, currently, very few examples. Among those few examples most of them could be observationally distinguished by the shape and running of the nongaussianities which contain a wealth of information about the underlying theory. So a future observation of large nongaussianity (which is entirely possible) which is consistent with the results of DBI models would certainly be strong evidence for brane inflation. This is the position of most people who work on cosmological nongaussianity. It's not true that many other models could explain the same observation.

27. **Peter Woit**
June 6, 2007

clarity,

I'm making no claim to expertise in this area, so will take your word that there are few ways to get non-gaussianity. Can't help though being suspicious that if it's ever observed, people will find more….

28. **clarity**
June 6, 2007

Peter,

Of course there might be more ways to get big nongaussianity that nobody has thought of yet. I would be surprised if there are lots more ways, however, because there has been a lot of activity in trying to construct models with f_NL >> 1 and the list of models considered seems pretty exhaustive to me. It's clear that large nongaussianity requires something pretty novel to occur either during inflation or else shortly afterwards.

Also, it's worth pointing out that the bispectrum constains a lot of information and is rather model-dependent so if, as you say, many more models with big nongaussianity are discovered it still seems likely that they'll be (at least in principle) distinguishable from DBI.

All Landscape, All the Time

June 5, 2007
Categories: Uncategorized

There seems to be a peculiar trend going on in the particle theory community. Just about all theorists I talk to, correspond with, argue with on blogs, etc. claim to be quite unhappy with the Landscape, and insist that most of their colleagues share this view. On the other hand, all evidence is that Landscape research is becoming increasingly influential at the highest levels of the string theory community. The most prominent yearly string theory conference, Strings 07, will soon be taking place in Madrid, and titles of many of the talks there have just been announced. The largest contingent of speakers is from Stanford, and it appears likely that landscape studies will be the most popular topic at the conference, with various aspects of AdS/CFT running a close second. Just counting the number of times "Landscape" appears in the title of a talk, so far there are 4 such talks out of 31 with announced titles. Last year at Strings 06, out of about 50 talks, 2 had "Landscape" in the title. Naively extrapolating this eternally inflationary trend to the future, pretty much all Strings 1X talks should be about the Landscape…

Another indication of where the field is going is the yearly TASI summer school aimed at training graduate students in particle theory. This year the topic is "String Universe", and several of the lecture series are about the Landscape, with two having "Landscape" in the title. Videos of the talks are being made available now, even as the summer school is going on. I learned about this from Clifford Johnson, who writes that the talk he most wanted to look at and recommends to everyone is Raphael Bousso's on "Cosmology and the Landscape".

Harvard's Lubos Motl traditionally has been a landscape skeptic, but in recent months he has been writing more and more positive things about this subject. His latest posting advertises a new paper by Raby and Wingertner calculating statistics on (an extremely small piece of) the heterotic landscape.

Update: Lubos has written a posting entitled Landscape 2007 in response to this one. His point of view seems to be that although he doesn't like the Landscape, he doesn't have a workable vacuum selection principle, and as time goes on and no such principle is found, this makes the Landscape more and more likely to be correct. He doesn't seem to even consider the possibility that the existence of the Landscape and the lack of a vacuum selection principle means that string-based 10/11d unification is just a failed idea. I suspect his point of view may be widely shared among string theorists, explaining the simultaneous unhappiness with the Landscape and its increasingly widespread adoption as a research program.

Comments

1. **ObsessiveMathsFreak**
   June 5, 2007

   > Just about all theorists **I talk to**, correspond with, argue with on blogs, etc. claim to be quite unhappy with the Landscape, and insist that most of their colleagues share this view. On the other hand, all evidence is that Landscape research is becoming increasingly influential at the highest levels of the string theory community.

   Have you taken into account that you may be persona non grata among Landscape researchers. Of course it may also be the case that you have become ad hoc confessor for the String Theory community, whom they go to in secret outside the group to confess their doubts and heresies. I hope you're not abusing your position by giving them your own book to buy and read as a penance!

2. **anon.**
   June 5, 2007

   It's about a year since Lubos was converted from his heresy. In his June 18, 2006, blog posting entitled *Top twelve results of string theory,* Lubos' top result number 12 was:

   'The existence of the landscape, a large enough set of metastable solutions that the cosmological constant can adjust to a value small enough as to allow organized structures (which require many bits and many cycles).'

   He wrote that this result was added at the suggestion of Prof. Polchinski. It's kinda confusing to me to see the landscape hailed as a top result of string theory, something to be proud of.

   Should Prof. Baez now revise his crackpot index so that it doesn't include theories which are endlessly adjustable and so can't make falsifiable predictions?

3. **Peter Shor**
   June 5, 2007

   Could it be that everything else in string theory is just too hard to get any significant results in?

4. **Bhabha**
   June 5, 2007

   There seems to be a peculiar trend going on in the theoretical physics community. Just about all theorists I talk to, correspond with, argue with in letters, etc. claim to be quite unhappy with the theory of relativity, and insist that most of their colleagues share this view. On the other hand, all evidence is that relativity research is becoming increasingly influential at the highest levels of the theoretical physics community.

   Letter from journalist X.Y. to his friend Z.V. in 1920.

   (Note for the nitpicker: I do not think the landscape is comparable to relativity but merely would like to point out that the fact that a particular theory is unwelcome in scientific circles and yet it gains momentum can not and should not be interpreted as a defect.)

5. **Peter Woit**
   June 5, 2007

   Peter Shor,

   Yes, I think that's basically why landscape studies are so popular. The huge variety of possible "vacua" provide a huge number of possible calculations you can do, many of which are not very hard. So, instead of trying to solve a hard problem (actually find a legitimate way to unify physics via string theory, or work on an alternative), people do this, even though it can't ever lead anywhere. Seems pretty dysfunctional to me…

6. **Peter Woit**
June 5, 2007

Bhabha,

"I do not think the landscape is comparable to relativity"

Than why write a comment making the analogy?

I'll point out that my comment that most physicists are unhappy with the landscape is not my argument that there is something wrong with it. I've made that scientific argument at great length here and elsewhere.

7. **wolfgang**
June 5, 2007

Peter,

> Clifford Johnson, who writes that the talk he most wanted to look at and recommends to everyone is Raphael Bousso's on "Cosmology and the Landscape"

this is quite different from what Clifford wrote.
"I glanced for a while at Raphael Bousso's first lecture in the series "Cosmology and the Landscape", and it was clear and very well presented."

8. **King Ray**
June 5, 2007

Einstein particularly despised those physicists who looked for the thinnest part of a board and then drilled as many holes as they could there.

Sounds like the problem with the landscape, string theory, toy models, etc. is that physicists are looking for something that they can calculate and publish, regardless of whether it has any relevance to the real world. That seems secondary.

Physicists need to knuckle down, think hard for years and try to solve some real problems instead of darting off to higher or lower dimensions when things get too difficult. I think they publish too much. Just pick up any copy of Phys Rev D; 99% of the theories are inconsistent with each other, so therefore 99% of them are wrong. Or not even wrong.

9. **Ptolemy**
June 5, 2007

Wolfgang, your quotation is a bit incomplete, missing:

"(This is not entirely surprising – Raph is always an excellent lecturer.)

"This is great! I'm going to make some time to work through some of this wonderful material over the next month. Anyone else?"

10. **Peter Woit**
June 5, 2007

wolfgang,

I exaggerated slightly, but the fact remains that the Bousso talk on the landscape was the one Clifford chose to spend his time watching, not others, and I think his comments read as an encouragement to others to spend their time watching this talk. Since Clifford is known to complain that critics emphasize too much the role of the landscape and associated pseudo-scientific argumentation in current string theory, I thought it odd that he chose to watch and recommend the Bousso talk, which is about as pseudo-scientific as these things get, rather than something else, like one of the talks on aspects of AdS/CFT, which he often complains doesn't get enough attention.

Note: The above comment is as originally written, but it was not written as carefully as I would have liked. The clause "which is about as pseudo-scientific as these things get" was intended to refer to the advertised topic of Bousso's series of talks, not specifically to his first talk.

11. **anon.**
June 5, 2007

OK, I'm puzzled by your count of which topics are dominating string theory (but then, I usually am). I see four titles explicitly mentioning the landscape, maybe two more that are landscape-related. That's all. Are you counting anything related to, say, inflationary model-building as 'landscape'?

12. **anon.**
June 5, 2007

Also:

I thought it odd that he chose to watch and recommend the Bousso talk, which is about as pseudo-scientific as these things get

I watched the first Bousso lecture: it was a very straightforward description of deSitter space, FRW cosmology, and why there is a cosmological constant problem. Things that anyone interested in cosmology should know. Are you referring to a later talk?

13. **Eric Mayes**
June 5, 2007

I wonder what Einstein would think about those who like to sit and criticsize, but who don't ever do anything themselves.

14. **Peter Woit**
June 5, 2007

anon,

I chose to stick to counting whether "landscape" occurs in the title to avoid issues of interpretation, as well as not knowing what will actually be in some of these talks. I'll accept your count of 6 landscape related talks. I count 4 talks on AdS/CFT, no other single topic with more talks than this. I don't think my characterization of the landscape as the topic with the single largest number of talks is incorrect.

I didn't watch the Bousso talk, made the assumption that his series of talks with the title "Cosmology and the Landscape" would cover similar material to his recent papers on the subject. If the first talk didn't mention the nonsense about the landscape, and that's why Clifford was promoting it, my apologies to him, although I think it would have been a good idea for him to explain that the reason that it was a good talk was that it didn't deal with half of its title.

15. **Peter Woit**
June 5, 2007

Hi Eric,

And I wonder what Einstein would think of those who devote their energies to defending the practice of bogus pseudo-science by engaging in personal attacks on anyone who dares to point out that this is what is going on?

16. **John Baez**
June 5, 2007

> Should Prof. Baez now revise his crackpot index so that it doesn't include theories which are endlessly adjustable and so can't make falsifiable predictions?

No, this item on the crackpot index still stands:

> 50 points for claiming you have a revolutionary theory but giving no concrete testable predictions.

I'm not saying string theory is crackpot physics. Whether someone wins the 50 crackpot points depends on what claims they make for this theory.

17. **Eric Mayes**
June 5, 2007

Hi Peter,
I only point out that the people who criticize the most also happen to be those who contribute the least. In my experience, such people make noise in order to give the appearance of being relevant. I'm the not criticizing you, by the way. Generally, I have no problems with your arguments, only that sometimes your statements are too extreme, i.e. that string theory is 'pseudo-science' and string theorists are dishonest, etc.. You could make your points without this vitriol.

Anyway, you're right that not too many string theorists like the anthropic principle. However, most except the reality of the Landscape. It's just not possible to answer the vacuum selection problem right now, as your nemesis points out in his recent comments. This question will have to eventually be answered if string theory is too be viagle, and I think it's healthy to point this out.

Best,
Eric

18. **Joseph Smidt**
June 5, 2007

"Videos of the talks are being made available now,"

As always, thanks for the heads up. This bog is great!

19. **Joseph Smidt**
June 5, 2007

bog = blog (Above)

20. **Michele**
June 6, 2007

Dear Peter,

You are sure of the failure of superstring theory?
I think that if the Theory of String is "very elegant" also only from the point of view mathematical, it could not be wrong completely.
I am looking forward of receiving Your answer at this my opinion.

21. **Michele**
June 6, 2007

I'm studying string theory from about seven years. You think really that the string theorist, hence also me, are visionary?
For me You are a long way off from the truth and I'm sorry that You have not again understood the beauty and the elegance of mathematical verity.
I hope that You answer at these my two observations.

22. **Thomas Larsson**
June 6, 2007

I wonder what Einstein would think about those who like to sit and criticsize, but who don't ever do anything themselves.

Quite a good point, because Einstein's time was not completely unlike ours. It didn't take an Einstein to see that the Michelson-Morley experiment in 1887 was problematic for ether theory, but it did take an Einstein to discover special relativity. So what would Einstein think about those who liked to sit and criticize ether theory, but who didn't ever came up with special relativity themselves?

23. **matteoeo**
June 6, 2007

michele,

while you wait for Woit's answer (which surely won't come up until next US morning), I would like to ask you a few questions, just to warm the night up:
– why in your view is string theory such a beautiful theory? where does its beauty come from? I happen to think just the opposite, that string theory is a kind of messy zoo with no rules and principles in it, and no elegance at all.

– why should this questionable elegance be a piece of reality, if no experiment is ever going to falsify or validate ST?
– what is truth in science? You sound to me like a sort of mistic or religious when you talk about “verity” (which is not an english word by the way, maybe “truth” might work better)
– have you read Woit’s book or a wide selection of his blog entries?

24. **Yatima**
June 6, 2007

Is it really important what Einstain would have thunk before 1905? Maybe he would have been too busy examining patent applications?

Einstein just went for simplicity – he got rid of the Ether altogether (which was actually not disproved by the MM experiment series) and reused the Fitzgerald-Lorentz equations to good effect:

… the introduction of a light-ether will prove to be superfluous since, according to the view to be developed here, neither will a space in absolute rest endowed with special properties be introduced nor will a velocity vector be associated with a point of empty space in which electromagnetic processes take place.

http://www-groups.dcs.st-and.ac.uk/~history/HistTopics/Special_relativity.html

On the other hand, his post-GR opinions on force unification might be of more interest.

25. **matteoeo**
June 6, 2007

michele,

I have more questions for you. I understand you’re a young italian string theorist. Since a great deal of the string debate is on sociological issues, such as funding and student recruitment, I think these topics might be interesting:
– after seven years study, do you handle it well enough to work on it and publish?
– during these years, did you also try to take a look at alternative theories of gravity, or on advances in the mathematics of QFT, or any other subject?
– how did you get involved in ST? Did you choose by yourself or were you strongly addressed by some professor?
– when you chose, were you aware of the state-of-the-art of ST?
– did popular books such as Greene’s “L’universo elegante” have any role?
– in case SUSY weren’t to come up at LHC, would you keep studying ST? have you ever thought of abandoning the theory in case it didnt’ lead to any significant improvement, and try to study something new?

26. **Michele**
June 6, 2007

I think that is perhaps a big exaggeration suggest that string theory is completely useless.
I am sure that are many stringy inspired pieces of mathematics (with regard some sectors of string theory) that will be of great interest to mathematicians for years to come.
This is, for me, the mathematical “beauty” of string theory.

27. **Michele**
June 6, 2007

I’m not a young string theorist. I am a studious of mathematics and theoretical physics.
I think that also you have not again understood the mathematical beauty….

28. **Michele**
June 6, 2007

For matteoeo

Who are you, please?
I’m looking forward to read the answer of Prof. Peter Woit.
However, I’ve read many books of string theory (Polchinski, Green-Schwarz-Witten) and more than 500 papers concerning various sectors of string theory. I’ve also published 13 papers. I’ve “trust” in the mathematical beauty of string theory and I’m sure that the next experiments with LHC can be the confirm to it.

Message for Italian peoples

Purtroppo mi rendo conto che ci sono molte persone e molti studiosi di vedute limitate e che non hanno rispetto per quella che loro definiscono “astrattezza” della scienza matematica.
Ma chi ha fede fino in fondo verrà ricompensato degli sforzi di anni e anni di studi pazienti e sacrifici inenarrabili.

29. **matteoeo**
June 6, 2007

michele,

the point has been made very frequently in this blog’s threads that one should distinguish ST’s merits as a mathematical tool and as a theory of something physical. I also question the fact that the theory itself is beautiful; the study of strings has boosted a great amount of study in many areas of mathematics and of mathematical QFTs, and has revealed very beautiful truths (which are tautologies and have nothing to share with the platonic “truth” you talk about) belonging to these areas, but is ST itself a beautiful theory? I really don’t know, do strings themselves have any very simple and beautiful mathcmatical content? Maybe anomaly cancellation. Or is it just an instrument that reflects other theorys’ light?
If you distinguish among mathematical achievements and physical world description, you can also say that from the physiscs point of view ST is completely useless, since it cannot calculate anything.

30. **matteoeo**
June 6, 2007

LHC cannot confirm ST, since ST doesn’t predict anything hat has to do with LHC; what could happen is that LHC does not falsify it, which surely will be the case since ST is not fasifiable. as to your italian message, what stries me more is the use of the word “Faith”, which in my view shouldn’t belong to science. Will you still have faith after LHC will show no sign of SUSY?

Anyway I'll stop posting since this is becoming a personal argument and at the moment there is no moderation. And also the topic was "anthropic landscape" rather than ST in general. Sorry Peter.

31. **Peter Woit**
June 6, 2007

Michele,

I've never written anywhere that "string theory is completely useless". String theory is a mixed bag, ranging from some wonderful mathematics to some appalling pseudo-science. This posting is about the "Landscape", an increasingly active part of string theory research which is mathematically extremely ugly, as well as showing no signs at all of having any connection to physics. This is what I'm criticizing here as pseudo-science. If you're interested in my views on string theory in general, there's a huge amount of this here on the blog, and my book has just come out in Italian…

32. **Chris Oakley**
June 6, 2007

Like the host of this blog – and no doubt many others – I find Lubos's climbdown over the anthropic principle somewhat puzzling, especially as this does not seem to be based on any new evidence coming to light. I would advance the following explanation: although the String Theory community values Lubos's services as a cheerleader, they do, at the same time, recognise that he is a loose cannon, and have sent him to special clinic for reprogramming, a bit like Alex in *A Clockwork Orange*. The new Lubos, although just as hostile to critics of S.T. will now no longer resist the encroachment of anthropic arguments, or any other pseudo-science that comes his way, provided that it comes from the right sources.

33. **woit**
June 6, 2007

Apologies to John Baez, whose comment above spent all night in the spam queue of my blog software. It gets very suspicious when people use links in their comments, unfortunately it doesn't seem to be configurable in any way.

34. **Thomas Larsson**
June 6, 2007

Chris, the anthropic landscape is the language in which God wrote the world.

35. **Michele**
June 6, 2007

Dear Peter

I would Thank You for Your advices. I've determined to read Your new book.
Now, I'm writing You for ask You with regard the "Loop Quantum gravity", you think that it can be a good alternative theory as regards string theory?

36. **Brett**
June 6, 2007

I just want to point out that "verity" most certainly is an English word, one of which I happen to be rather fond.

37. **matteoeo**
June 6, 2007

strange, my dictionary doesn't quote it but I see on Word Reference it exists, must be very rare. sorry for that.

michele, would You like to answer my question about faith and science?

38. **Michele**
June 6, 2007

matteoeo

I believe in mathematics and this has not a metaphysics meaning.

39. **AGeek**
June 6, 2007

King ray wrote: "Physicists need to knuckle down, think hard for years and try to solve some real problems instead of darting off to higher or lower dimensions when things get too difficult. I think they publish too much."

True. Alas, this has a cause, known as "publish or perish". So either you pick/invent some easy problem which you can use to generate a lot of papers in a fairly limited amount of time, or you're out of academia and need to get a Real Job(TM) to put food on the table. And then you won't have much if any time to think about physics.

So academia ends up being populated by people doing things which they don't really believe in, holding on to the hope that some day, they'll end up having the time to do what they really would like to. Somehow. Someday.

Easy to criticize, but try to come up with a workable alternative.

Maybe somebody smart enough to do something really worthwile in theoretical physics should also be smart enough to become independently wealthy in a reasonable amount of time (a decade or so)? Then the best course of action might be to just fire all theoreticians and funnel the funds to experimental physics instead. An LHC is out of reach for individuals, so it can only be built using public funds. Not true of model building and theory.

40. **Leonard Ornstein**
June 6, 2007

Michele

"Believing" in anything for a while, is at least a temporary metaphysical act of faith.

Any social contracts to deal with something as "given", without proof, such as commitments to axiomatics of logic, mathematics, language, etc., are all equivalent to metaphysical acts of faith. So none of us, scientist or mathematicians, should decry metaphysic "belief" too vigorously. We can't avoid it.

But one shouldn't loses track of the difference between metaphysical, unprovable models, and those which (although also essentially metaphysical) are able to be supported or refuted by empirical observation. Because this distinguishes scientific models, and the scientific enterprise, from most other (including mathematical) models and disciplines.

The argument to which you should try to respond is that String Theories and the Landscape currently don't pass this test, as Science.

41. **fynn**
June 6, 2007

Thanks for pointing to the link to the TASI2007 lecture videos (via "Asymptotia"). Meanwhile I listened there to the first three lectures by Bousso on the c.c. problem, and found them very illuminating indeed. Without going into too much technicalities associated with string theory he makes a convincing point that the anthropic argument, taken seriously, is in fact not a mere tautology, and far from trivial, since it obliges to demonstrate that there are sufficiently many choices for the c.c. in the theory, and to derive from the theory a mechanism how these choices are populated in a way consistent with the observations about our universe.
Very recommendable lectures, also for non-experts.
Cheers, Fynn.

42. **Chris Oakley**
June 6, 2007

AGeek — you could be on to something there.

43. **Alex Mikunov**
June 7, 2007

Bousso's talk mostly based on his previously published papers, e.g.
http://arxiv.org/abs/hep-th/0702115
He's basically trying to substitute Weinberg's "observers require galaxies" with something like "observers obey the laws of thermodynamics"
What really bothers me is that, since superstring/M-theory is supposed to explain [among other things] thermodynamics as well, it's like saying "observers obey the laws of superstrings". This, of course, leads nowhere [at least logically].
(Correct me if I'm wrong)
Thus, it's obvious to assume that we need some sort of a meta-theory approach, maybe similar to what we have in mathematical logic/proof theory

44. **Raphael Bousso**
June 7, 2007

Peter,

A few years ago, you wrote about the Einstein issue of the Scientific American:

"One article in the magazine doesn't really have much to do with Einstein and I believe would make him gag if he were still around. The article, entitled "The String Theory Landscape" is by Raphael Bousso and Joe Polchinski. In it they claim credit for the pseudo-scientific idea of "explaining" the value of the cosmological constant by the existence of the "landscape" and the anthropic principle. It's sad to see this nonsense being purveyed by the most respected and well-known popular science publication in the US."

After Joe commented on this on Jacques Distler's blog (http://golem.ph.utexas.edu/~distler/blog/archives/000760.html), you offered an apology:

"First of all, you're right that the tone is quite objectionable and I'd like to apologize to you for it. I sincerely regret writing that posting in that way, and you're right to object to it. But while I agree with you about the civility issue here, I disagree with you about the issue of whether this kind of thing is legitimate scientific discussion. The example you give is a low point for my blog […]"

I am not qualified to judge the competition for low point on your blog, but the depth of your contrition is illustrated well by a more recent posting:

"I thought it odd that he chose to watch and recommend the Bousso talk, which is about as pseudo-scientific as these things get"

which, amusingly, prompted one of your readers to inform you of its content. I can't help admiring the frankness of your reply,

"I didn't watch the Bousso talk […]",

but the episode tempts me to quote the question that Welch asked McCarthy. You can google it as an exercise in checking sources.

45. **Peter Woit**
June 7, 2007

Raphael,

The only thing I regret here is that I did not express myself precisely in one of the comments I wrote, with the "about as pseudo-scientific as these things get" clause applying to your talk when I meant it to apply to the advertised topic of your lectures. I'll add something to the end of that comment to make that clear.

No, I didn't watch the talk, but I have read your recent papers, as well as a wide variety of other promotional material by you and others for the anthropic landscape, including your Scientific American article with Polchinski. I'm not about to apologize for referring to any of that as "pseudo-science", since that's what it is. I can assure you that, privately but perhaps not to your face, a large number of physicists express themselves in even stronger and less civil manner about this topic. After I apologized to Polchinski for some of the language used about the Scientific American article, I received several e-mails from people complaining to me that I had no business apologizing, since my description was completely accurate.

Many people besides me, including many of the leaders of the particle theory community, feel that what you and others are doing to (quite successfully) promote an inherently untestable research program is extremely dangerous for physics. I'm not going to apologize for forcefully making this case here.

46. **nigel cook**
June 7, 2007

If you think it permissible to bring up the McCarthy era as an analogy to criticisms of the cosmic landscape, you may escalate the hostilities because others will draw analogies between string propaganda and the propaganda of certain historical dictatorships, etc.

The following question in my opinion can more appropriately be directed to those who popularize pseudoscience, than to those who combat it:

'Have you no sense of decency?' – http://www.americanrhetoric.com/speeches/welch-mccarthy.html

47. **Bee**
June 7, 2007

well, the landscape is definitely a topic that attracts a lot of attention and very many interesting developments have been made in this field. I am presently at the String Phenomenology (this year near Rome) and we've just had a very stimulating talk by Keith Dienes who closed (among other things) with stating "just as in astrophysics, botany, and zoology, the first step in the analysis of a large data set is enumeration and classification" (slides not yet online, should be shortly). He made his points well, his talk was (as always) very good (as well as entertaining), and I understand the attempt of 'That's what we have, now let's deal with it' – which is imho as risky as courageous. Nevertheless I can't avoid finding the landscape botanics somewhat depressing.

48. **Peter Woit**
June 7, 2007

Bee,

The problem is that the landscape of supposed string vacua is NOT a "data set". Actual data about the real world is worth spending time enumerating and classifying. Endlessly complicated and useless constructions that reflect nothing except the failure of a certain research program are not worth spending time on. The fact that this is a rather easy activity to engage in, that you can keep yourself busy for years doing it, generating lots of papers, doesn't mean it's worth doing.

49. **Eric**
June 7, 2007

Peter,
If you think studying string theory vacua is is not worth your time, then you definitely shouldn't spend any time on it. However, leave the rest of us who think it's worthwhile alone. I don't see how it's any of your concern what the rest of us choose to study.

50. **Peter Woit**
June 7, 2007

Eric,

If you don't want to hear my opinions on "string vacua" and want to be left alone to pursue their study, all you have to do is not read this blog.

51. **Eric**
June 7, 2007

Ok, I won't read it anymore. I recommend this to everyone.

52. **Peter Shor**
June 7, 2007

In mathematics, I have the impression that the criterion for tenure is not counting papers, but counting really good papers. Is this different in physics? Do many mediocre papers get you tenure?

53. **Bee**
June 7, 2007

The problem is that the landscape of supposed string vacua is NOT a "data set".

Oh well. By now, you must know that I think the whole landscape is a bug not a feature. But I think the term 'data set' was just used in a somewhat vague context, referring not to 'data' as measurements, but as quantifiable features within that landscape of possibilities. Of course you can classify that stuff – isn't that what mathematicians do all the time? There's a vast number of possible mathematical structures, one can categorize them and count them, and find all kinds of interesting relations etc etc. If you like, you can try to calculate the probability for finding 9112001 within the first n digits of \pi or try to explain why there are more puddles than lakes, or why the number 17 is the most random number there is. See, I like maths, I also like zoology and botany, but I don't want it to be sold as physics. Plus I wouldn't want to categorize 10^500 things, whether vacua, herbs or butterflies. Best,

B.

54. **Peter Woit**
June 7, 2007

Peter,

People in physics do tend to write more papers than people in mathematics, but at good institutions, you're going to need to have written good papers to get tenure, not just a lot of mediocre ones. Of course, then the debate is about what is a "good paper", which is not so clear in a field like string theory unification, where no one is making real progress.

Of course, if it's too hard to write good papers, then, in terms of getting tenure, more mediocre ones is better than fewer.

55. **Peter Woit**
June 7, 2007

Raphael,

I did today watch your third TASI lecture on-line. In it you explain to the students that the Landscape is a "great success for string theory". After watching it, I'll stand by my characterization of what you presented to the students as "pseudoscience".

56. **Christine**
June 7, 2007

Bee wrote:

Of course you can classify that stuff – isn't that what mathematicians do all the time? There's a vast number of possible mathematical structures, one can categorize them and count them, and find all kinds of interesting relations etc etc.

It took 60 Gigabytes to store information on the exceptional Lie group E8… Analogously, if one considers the landscape as some type of mathematical structure *per se*, one would still have to deal — at some point — with an incredibly huge amount of information, no?

57. **Dave B**
June 7, 2007

What if the string theory is the correct theory of quantum gravity and the landscape is real? Can we really dismiss a theory just because it contains a feature we don't like? It's not impossible that the existence of our universe is completely random. Perhaps this is not a comforting thought, but it doesn't invalidate string theory. Rejecting string theory on this basis is as foolish as Einstein rejecting quantum mechanics because of its probablistic nature. Perhaps there are simply limits to our ability to know everything and make predictions.

58. **Peter Woit**
June 7, 2007

Dave B.

Yes, it's possible that the landscape is real, it's possible that we live in a Matrix overseen by superior beings, etc., etc. But an explanation is only scientific if it is possible to use it to make predictions which can be tested against experiment. This is scientific method 101, and it's something the Landscape people aren't doing. They don't have a plausible idea about how to test their theory, even in principle. This is why I claim it is pseudo-science, not science.

59. **Dave B**
June 7, 2007

I don't think it's fair to compare the landscape and string theory to some crackpot fantasy. It is a consistent body of work based upon solid mathematical underpinnings and well-motivated physically. I think your objections are philosphical rather than scientific. I don't think it's true that string theory is not able to make predictions 'in principle', it just may not be possible right now for technical reasons. If quantum mechanics has taught us anything, it's that it's impossible to know and predict everything. You would reject quantum mechanics as 'unscientific' because it cannot predict the outcome of every experiment and can only give statistical probabilities. Why is it so hard to believe that the properies and existence of our universe would be any different?

60. **Peter Woit**
June 7, 2007

The problem with the landscape is not that it doesn't predict everything, it is that it does not predict anything. QM makes a huge number of testable predictions, which have been confirmed, the landscape makes none (and the reasons for this are not "technical", but inherent in the whole concept). One of these is science, one is pseudo-science.

61. **Dave B**
June 7, 2007

No, the landscape predicts a discrete set of possible vacua, any one of which could be a description of our universe. QM does the same. For an invidual measurement, QM can only make statistical predictions by assigning probablities to a large set of possible outcomes. You can only test QM by performing a large number of individual measurements. IT CANNOT PREDICT THE OUTCOME OF AN INDIVIDUAL MEASUREMENT. I'm sure that the landscape of string theory is the same, only it's not presently known how to assign probabilities to the different vacua.

62. **Peter Woit**
June 7, 2007

Dave,

You have no idea what you're talking about. QM makes both exact and statistical predictions. The landscape of string theory is not the same. It makes no predictions whatsover.

63. **Eugene Stefanovich**
June 8, 2007

Dave B.:

I don't think it's fair to compare the landscape and string theory to some crackpot fantasy. It is a consistent body of work based upon solid mathematical underpinnings and well-motivated physically.

I would disagree about *solid mathematical underpinnings*. In my opinion, physicists were never interested in a rigorous mathematical (definition/axiom/theorem) formulation of their theories. Theoretical physics was always a patchwork of mathematical proofs, plausible guesses, and outrageous hypotheses. For centuries this worked fine, because experiment provided a mechanism of "natural selection": stupid theories couldn't survive experimental tests. Now we don't have the culture of mathematical rigor, and we don't have experimental censorship. In these conditions, the only factor of "natural selection" is sociological: if you managed to gather more supporters than your opponent, then your theory won.

64. **Anonymous**
June 8, 2007

In response to Raphael Bousso, Peter Woit wrote, "No, I didn't watch the talk, but I have read your recent papers, as well as a wide variety of other promotional material by you and others for the anthropic landscape … I'm not about to apologize for referring to any of that as 'pseudo-science', since that's what it is. I can assure you that, privately but perhaps not to your face, a large number of physicists express themselves in even stronger and less civil manner about this topic."

For those who weren't able to track down the question that Welch asked McCarthy, here's a clue: "in February 1950, an undistinguished, first-term Republican senator from Wisconsin, Joseph McCarthy, burst into national prominence when, in a speech in Wheeling, West Virginia, he held up a piece of paper that he claimed was a list of 205 known communists currently working in the State Department. McCarthy never produced documentation for a single one of his charges, but for the next four years he exploited an issue that he realized had touched a nerve in the American public." From "Basic Readings in U.S. Democracy",
http://usinfo.state.gov/usa/infousa/facts/democrac/60.htm

65. **Thomas Larsson**
June 8, 2007

Godwin's Law:
As an online discussion grows longer, the probability of a comparison involving Nazis or Hitler approaches one. Once such a comparison is made, the thread is finished and whoever mentioned the Nazis has automatically "lost" whatever debate was in progress.

It seems to me that McCarthy can be substituted for Hitler, and that prof. Bousso thus has lost the argument.

66. **Bruno**
June 8, 2007

Could it be that the Landscape is welcomed by the proponents of Intelligent Design? As soon as it is established by reputable physicists that the very delicate fundamental features of the universe were created as an extremely improbable choice out of an astronomical number of possible choices, the religiously minded will triumph.

I think science shouldn't give up trying to explain observations.

Best regards,
Bruno

67. **Peter Woit**
June 8, 2007

In response to "Anonymous" who left the address "anonymous@anon.com",

Raphael chose to write in here and say what he had to say under his own name, which I greatly respect. It's a very unfortunate aspect of this debate that many people feel unwilling (often for good reasons) to publicly get involved in it in any way. If anyone doubts though that there is strong opposition to the idea that the Landscape is a "great success" of string theory, or that there is a widespread opinion that it is not normal science and is a bad thing for the field, pick a random sample of physicists and ask them privately. Or note that Susskind, the most well-known proponent of the Landscape, recently described his experience as that of being at the center of a circular firing squad.

68. **wow**
June 8, 2007

The situation is considerably more nuanced than you describe, Peter.

Most string theorists now believe there is a landscape, in the sense that the semiclassical approximation to the theory has a huge number of consistent, metastable (or in supersymmetric or AdS cases, stable) vacua.

Most string theorists are not yet willing to buy into anthropic reasoning; they view the situation above as disturbing, but as one that can perhaps be "saved" by a selection principle. The detailed nature of this principle is however never discussed (some people talk about Hartle Hawking wavefunctions or whatever, but there is no indication at all that this helps).

The belief in a large number of consistent vacua is supported by a large body of mathematical work of varying levels of rigor, since the theory does not allow exact computations in most cases of interest. But the evidence is now accepted as fairly decisive by most theorists. This was clear even at Strings 2005, the location of the infamous anthropic poll which also demonstrates the point about hostility to anthropic reasoning. It has only become clearer over the past 2 years.

It will be interesting to see how many can continue to deny the efficacy of anthropic reasoning about the CC as the years pass, if the wished-for dynamical miracle does not materialize.

69. **Peter Woit**
June 8, 2007

wow,

I'm not sure what "most" string theorists think, it's certainly true that the situation is nuanced, with many people having trouble figuring out how to deal with it.

I suspect that your point of view may be somewhat influenced by the part of the world you are in, but, sure, what I was writing about in the posting was the increasingly widespread degree of acceptance of the Landscape as a well-founded aspect of string theory. Yes, more and more string theorists acknowledge that it is a feature of the semi-classical approximation to the theory in a metastable ground state. But there are two possible reactions to this:

1. These are physically relevant ground states for the full theory. To get the SM and actual physics out of this, then opinion splits between anthropic/statistical argumentation (also known as pseudoscience…) and belief that a cosmological selection principle will save the day (also known as wishful thinking…)

2. These are probably not physically relevant ground states for the full non-perturbative theory, whose properties we do not understand yet at all. From everything he says, I take this to be David Gross's point of view, and that of many other people, including almost all the string theorists I know personally and have discussed this with.

I find 2. to carry too much wishful thinking for my taste, although it's nowhere near as bad as the wishful thinking that Hartle-Hawking will save string theory by picking out a point in the landscape. The point of view of proponents of 2. is often that better understanding non-perturbative string theory is in any case an interesting research program that, even if it fails as a TOE, quite possibly will lead to other important results (a solution of QCD, new mathematics, etc.)

I would guess that there are more string theorists in camp 2. than in camp 1, but I don't have very good statistics. Maybe at Strings 2007, this question can be put to a public vote…

70. **wow**
June 8, 2007

Hi Peter:

Thats interesting. I don't know any who hold view 2; the
AdS/CFT correspondence pretty much kills this point of
view for many of the AdS vacua, since the field theories are well defined and hence show that string theory really does have these exact, well defined superselection sectors giving distinct stable solutions. One could hope all metastable
De Sitter or Minkowski vacua are just artifacts of the semiclassical
approximation, but I don't know any string theorists who really
believe this (well, maybe Tom Banks; you say D. Gross, though this doesn't coincide with my understanding from actual discussions I've witnessed).

Point of view 2. would require all kinds
of miraculous failures of perturbation theory, and in normal physics, we haven't seen this happen. (Almost our entire understanding of general relativity and quantum field theory is not "nonperturbative" in this sense, and we use them quite successfully every day; the Standard Model is not fully asymptotically free and this doesn't hamper us, GR cannot be quantized without invoking string theory, etc.).

Clearly, we must be talking to different (very large) samples of string theorists.

71. **Peter Woit**
June 8, 2007

wow,

My understanding of Gross's point of view is purely based on several of his talks I've attended, and an even larger number of talks of his accessible in one form or another on the net. His two main oft-repeated points are that "we don't know what string theory is", and "string theory will require us to replace space (and probably time) by something else". I take these to mean he has in mind something more drastic than a Hartle-Hawking or other cosmological selection mechanism for one of the metastable vacua in the landscape, but maybe I'm wrong.

One other confusing aspect of this question is that of identifying who is a "string theorist". Many people I talk to who have written quite a few papers on string theory, or even most of their papers on string theory, will say things like "maybe I'm not really a string theorist", or "I might be a string theorist, but I'm not part of the 'string theory establishment'". It's also true that my sample is weighted towards those working on the more mathematical ends of string theory. But I also notice that publicly many "establishment" string theorists answer criticisms of string theory by claiming that the theory is still extremely poorly understood, with some clearly having in mind a time-scale of another 100 years before real contact with experiment is made. They often make the point that multiple new "revolutions" may be needed, and I take this to mean that they see the currently known string vacua constructions as far from the final story about how unification will come out of string theory.

72. **Mark Srednicki**
June 8, 2007

Peter Woit says:
"To get the SM and actual physics out of [the landscape], opinion splits between anthropic/statistical argumentation (also known as pseudoscience…) …"

On what basis do you believe that it is *impossible* for the parameters of the SM to be randomly determined?

It will indeed be disappointing if it turns out that the parameters of the SM (possibly including the number and type of gauge groups and the representations of the matter fields) cannot be predicted from a more fundamental theory, because it turns out that they are in fact the result of a random process in some sort of multiverse. But I don't see how you can know, right now, that this *cannot possibly* be the case.

And if it *can* possibly be the case, then it is surely wrong to label investigations of this possibility as pseudoscience.

After all, we don't say that the physics of planet formation is pseudoscience because we can't use it to calculate the number of planets in out Solar System and their distances from the Sun; we don't say that geology is pseudoscience because we can't use it to calculate the shapes of the continents.

And if string theory turns out to be compatible with a great variety of models of low-energy physics, then in this regard it's no better or worse than quantum field theory. Should we declare that quantum field theory has failed because it cannot be used to calculate the parameters of the SM?

73. **Peter Woit**
June 8, 2007

Mark,

I'm pretty sure we've had exactly this argument before, and the answer to your points is in some of my responses to the comments above. But, again:

The problem with the landscape is not that it doesn't predict everything, but that it doesn't predict anything. This is very different than QFT, which makes a host of testable, verified predictions. Sure, it is possible that all parameters, gauge groups, representations of the SM are environmental. It's also possible that they are chosen by an omnipotent being who wanted us to have a cozy place to live. If you want to do science, not pseudo-science, your theory has to make distinctive, experimentally testable predictions that will allow it to be checked. If you know of such a prediction made by the anthropic landscape program, let's hear it. The only ones I know of are either

1. pure wishful thinking on the order of "maybe if we calculate observables in all 10^500 vacua, the statistics will have a narrow peak and we can make a prediction". There is not a shred of evidence for this, in any of the by now many calculations people have been doing, nor any argument at all why this should happen.

2. fairly solid arguments that completely disagree with experiment. The most well-known is the anthropic landscape prediction of the proton lifetime, which is many, many orders of magnitude too low.

The reason the landscape is pseudoscience is that you can't use it to make testable predictions. If you are willing to make some assumptions that do allow non-trivial predictions, they often come out completely wrong. This kind of activity is just not science.

74. **Eric Mayes**
June 8, 2007

Peter,
Name one prediction that QFT alone can make without any input information, i.e. gauge groups, gauge couplings, and matter content. All of these things must be put into QFT by hand, and then and only then can you make predictions. As Mark has said, this is no different than string theory, which for a given vacuum would be just as predictive. If the vacuum which corresponds to the physics of our universe could be found, then this would be just as predictive as the QFT Standard Model. In fact, it would likely be more so since it would include gravity. The real questions are 1) does such a vacuum exist and 2) how is this vacuum selected. I think we would be a lot better off if we focus more on the first question and leave the second question for later.

75. **Peter Shor**
June 8, 2007

Eric,

I don't see how you can possible call quantum field theory a failure given its successful predictions over the last 30 years. The success of quantum field theory is not that it actually predicts anything a priori, but that once you constrain your quantum field theory by using the results of experiments, it predicts the results of other, apparently completely unrelated experiments. Can string theory do this? Right now, absolutely not.

If string theory is to be able to do this, I believe you will have to address one of the following questions: (a) are there effective quantum field theories that are not allowed by string theory? or (b) is there any reasonable way to actually select the vacuum? I don't believe that the landscape studies that Peter Woit is disparaging address either one.

76. **anon.**
June 8, 2007

'… this [QFT] is no different than string theory, which for a given vacuum would be just as predictive. If the vacuum which corresponds to the physics of our universe could be found, then this would be just as predictive as the QFT Standard Model. In fact, it would likely be more so since it would include gravity.' – Eric

Yeah, all you need to do is to be sure that the 1 in a 10^500 vacuum is the right one. Easy:

1. choose the best vacuum you can identify (ie one with a small positive cc, assuming that the cc is a real constant and is not evolving with time, as some recent cosmological studies with gamma ray bursters suggest)

2. use it to make new predictions.

Now the clever bit:

if the things it predicts don't show up, then it's the wrong one.

You then pick out another one of the 10^500 and start again. Eventually you'll or some distant stringy descendent will get there, providing that string theory *is* the right theory…

Of course, if you're wrong, no worries. You'll be dead and buried for billions of years before the entire 10^500 possibilities have been experimentally refuted.

I think this is the point about the landscape. Of course, *if* some version of string theory *is* reality, and *if* the it could ever be identified, then yes, it might be able to predict things in principle. You're kind of missing the small problem that nobody has ever given any scientific evidence that this *might* be the case. Consistency with spin-2 gravitons and supersymmetry isn't scientific evidence since there is no evidence for either, they're just guesses.

QFT works because its easy to plug experimentally found data into it, which identifies the real vacuum, and get predictions. You can't do this with string theory because you don't know the state of the unobservably small rolled up dimensions in the Calabi-Yau manifold which determine the details of the particle physics. You have to guess one of 10^500 possibilities before plugging experimental data into it to get a prediction. That's why it's a total failure.

77. **woit**
June 8, 2007

Eric,

The bottom line is simple and you have no argument against it: your theory has to produce a distinctive, experimentally testable prediction, otherwise belief in it is a matter or faith, not science. The landscape doesn't do this, or, to the extent it does, people ignore disagreement with experiment. This is pseudoscience, not science.

Saying "But my unpredictive and untestable theory is better than the SM, because it also includes an unpredictive and untestable gravity sector!" doesn't change the fact that you are asking people to believe something about the world that can't possibly be checked. This isn't science.

78. **Eric Mayes**
June 8, 2007

Peter S.,
I think you misunderstand me, I have not called QFT a failure. However, it should be recognized that the standard model is but one QFT out of many possible QFT's. One has to build a model consistent with QFT and then make predictions within this model, but by itself QFT can do nothing. String theory is the same. Regarding whether or not string theory can make predictions of some parameters in terms of others once it has been constrained by experiment, the answer is yes. We are presently doing this. As far as the landscape studies that Peter W. disparages, I agree that they are not likely to go very far.

79. **woit**
June 8, 2007

Peter Shor,

The "swampland" program does try and address the problem of identifying effective QFTs that can't come from string theory. The problems with this program are that:

1. Showing that some class of models that don't look like the real world can't come from string theory doesn't provide evidence that string theory actually has something to do with the real world.

2. String theory is still so ill-defined that it's not clear you can say that string theory can't produce some model or other. People keep on coming up with new compactification schemes. Typical "swampland" arguments don't actually have anything to do with string theory, but try and argue that the existence of gravity rules out certain kinds of effective QFTs.

80. **Paolo Bizzarri**
June 9, 2007

Eric,

I understand that the problem of SM against ST is that SM is much more constrained.

You are saying that you are predicting some values, given some other values as parameter (a specific selection of the vacuum).

Is this a falsifiable prediction?

In SM, if an experiment would reveal a different value for some of its parameters, we could not simply say “Ok, so we have to tune this and this other parameter”.

Am I right?

81. **Eric Mayes**
June 9, 2007

Paolo,
The model is as constrained as the CMSSM. Most of the so-called ‘tunable’ parameters are the F-terms which define how supersymmetry is broken. It’s possible to get many different spectra with these five parameters, however the spectra are constrained by phenomenological considerations. If for a given set of values for these parameters the model can get the right values for ALL of the superpartner masses, then the model would completely describe low-energy physics. If it can’t reproduce the superpartner masses, then the model is falsified. Keep in mind that all of the parameters in the model are tightly constrained by requiring supersymmetry at the string scale, gauge coupling unification, and the correct CKM masses and mixing for the normal SM particles.

82. **Paolo Bizzarri**
June 13, 2007

Eric,

just a very dumb question. Which are the right values of the superpartner masses?

Thank you.

83. **alex**
June 19, 2007

“Which are the right values of the superpartner masses?”

My guess is that they should be compatible with things like the LEP lightest chargino bound ~104GeV, the LSP should be neutral and such that the relic density is within the experimental bounds. The”right” spectrum should result in radiative EWSB as well as precision gauge coupling unification at two-loops. It should give the correct value for the Z-boson mass, etc.

84. **Peter Woit**
June 19, 2007

alex,

In other words the “right values” are the ones compatible with experiment? OK, hard to argue with that…..

85. **Karla**
June 19, 2007

I think what’s meant by the ‘right’ values are the experimentally measured superpartner masses, once they have been found by LHC. Presumably, the spectrum of superpartner masses is calculable within the model given a set of input parameters (the soft terms), and the correct superparticle masses may or may not be obtained for some value of these input parameters.

86. **alex**
June 19, 2007

Sure, if your model predicts, for example, gluino LSP then it’s obviously not the “right” spectrum. But apart from experiments, the standard precision gauge coupling unification and REWSB should also be naturally realized in the model, according to my biased opinion.

Imposter String Theorist at Stanford

June 6, 2007
Categories: Uncategorized

In recent years many people in the particle theory community have been wondering what's going on with the Stanford theory group, as it has become dominated by work on things like the anthropic landscape. It turns out that, for a while now, there was someone there who even they were wondering about. Her name is Elizabeth Okazaki, and evidently for the last four years she has

attended graduate physics seminars, used the offices reserved for doctoral and post-doctoral physics students and — for all intents and purposes made the Varian Physics Lab her home

this despite the fact that she has no formal affiliation with the university. Some press stories about this are available from [The Stanford Daily]() (more [here]()) and the [San Francisco Chronicle]().

According to the Stanford paper, students interviewed said that Okazaki:

claimed to be a visiting scholar in the humanities, looking to provide an interdisciplinary perspective on string theory. On several instances, she has said that she was working with Physics Prof. Leonard Susskind, one of the world's most respected string theorists.

but

Susskind told The Daily that Okazaki was not officially associated with him or his lab in any way.

"As far as I know, she has no official connection with anyone in the physics department," Susskind said. "In fact, as far as I can tell, she has a very limited knowledge of physics itself."

The story in The Stanford Daily on-line has a long associated comment thread, containing (besides a lot of nonsense) some comments from people in the Stanford physics department that provide more insight into the situation.

The San Francisco Chronicle article quotes Stanford graduate student Surjeet Rajendran about the situation as follows:

A university has a lot of weird people… Some of the faculty are weird, some of the grad students are weird. So you don't really know who's who. And you feel rather, I guess, rude asking them, 'What the hell are you doing?'

For another perspective on this, see Scott Aaronson's posting on [The Groupies of Science](), where he makes the point that "Science Needs More Groupies, Not Less", and argues that:

When we discover a stowaway on the great Ship of Science, why throw her overboard when we could make her swab the decks?

Comments

1. **island**
   June 6, 2007

   Swab the decks???… Imposter???… Hell, she's bound to be the closest thing to a real physicist in the whole department… and they should put her in charge of it… 😉

2. **Professor Doctor Fatrear**
   June 6, 2007

   I find the title of your post completely disingenuous. This woman was the typical friendly crackpot/nutjob that can be found in the halls of every math/physics department in which I have ever spent even a few days. To call he an `impostor string theorist' suggests that string theory has something to do with her situation, which it clearly doesn't in any serious sense.

   These people survive because people's hearts are mostly kind and no one feels like going to the (sometimes considerable) effort of driving away a mostly harmless occassional nuisance.

   The nutjobs with tenure are different matter.

3. **Michele**
   June 6, 2007

   Dear Peter,
   I would you like to thank You for Your advices. I've determined to read Your new book.
   I'm now writing you because I want ask You with regard the "Loop Quantum Gravity", You think that it can be a good alternative theory as regards the string theory?

4. **Peter Woit**
   June 6, 2007

   PDF,

   The title of my post is not "completely disingenuous", it is only partly disingenuous. Sure, I'm making a bit of fun of the Stanford string theorists, perhaps somewhat unfairly. On the other hand, given Susskind's behavior over the past few years, I don't think it's a coincidence that this woman without a firm grasp on reality decided to claim affiliation with him, or that she gravitated to string theory rather than some other subject.

   Not knowing the details of the Stanford situation, I'm not expressing any opinion about what they should have done or not done, or criticizing them about this. Academia's tolerance of eccentricity and various hangers-on is a good thing and I see nothing wrong with this, quite the opposite. People like Okazaki do put one in a difficult position about what to do about them.

5. **Peter Woit**

June 6, 2007

Michele,

LQG is really off-topic here and I don't want to start up a discussion of it. Personally I choose to concentrate on particle physics, not quantum gravity, since only in that case does one have good hopes of testing one's ideas if they're successful. Among approaches to quantum gravity, LQG seems to me a very interesting one, with some appealing features. But again, this is another topic, and the last thing I want to do is have another bout of LQG vs. string theory warfare going on here.

6. **Professor Doctor Fatrear**
June 6, 2007

I've seen a woman without a firm grasp on reality gravitate to singular integral operators and a scrawny man who understood them very well. I remember a departmental stalker who was very into Penrose tilings, convinced half the department had it in for him, and spent a lot of time promoting basic physics on the web. These people are complicated, show up everywhere and in all disciplines, and have their good points as well as their bad ones. What happens in math and physics is that we can tell the difference between crackpots and people with wild ideas because we have some more or less objective criteria to work with (fortunately there are no objective criteria in music). On the other hand, you seem to be implying that Susskind also does not have a firm grasp on reality, and that seems a bit extreme; even were he a pathologically lying sycophant of the dean citing himself in every one of his two hundred repetetive articles, his behavior would be well within academic norms, and I guess he is not that bad.

7. **Aaron Bergman**
June 6, 2007

I've seen a woman without a firm grasp on reality gravitate to singular integral operators and a scrawny man who understood them very well.

I've seen things you people wouldn't believe. Attack ships on fire off the shoulder of Orion. I watched C-beams glitter in the dark near the Tannhauser gate. All those moments will be lost in time, like tears in rain. Time to die.

(sorry)

8. **Peter Woit**
June 6, 2007

PDF,

I don't disagree with your characterization of the various types that inhabit academia. But yes, I happen to think that Susskind and other promoters of the anthropic landscape (quite a few of whom are based at Stanford) don't have a firm grasp on reality, not in their everyday lives, but in the specific research program they have chosen to pursue. Sure, this is not really out of academic norms, which involve tolerating some degree of research of dubious sanity. But (see previous posting), the fact that this stuff is becoming increasingly influential and popular at the very center of the field is pretty disturbing.

9. **Stanford grad student**
June 6, 2007

There's been a bit of a mischaracterization in the media, I think, of Elizabeth Okazaki's situation. It's been made to sound as if she was a crackpot looking to rub elbows with the string theorists, hoping to push her theories. In reality, I think that her interest in science, if she even has one, was entirely peripheral to her actual goal, which was to live in the building. This was a point that the news reports tended to underemphasize, that she lived in, as in literally slept in and ate in and occupied space in, the physics building for four years, and that this was what was freaking out the grad students. She is a homeless person, and she saw a great opportunity to take up residence in a building full of people who, as we saw, wouldn't bother her, so she did. (And frankly, who can blame her? If I had to choose between the streets and the student lounge of the physics building, I'd do the same thing she did.)

Anything to do with string theory or attending seminars or claiming to work for Lenny Susskind was just part of her cover story. "Wait, who are you? What are you doing here?" "Oh, I, uh, work for Lenny Susskind, yeah, that's it." "Hmm, all right, carry on."

This is why I found it amusing that Lenny would focus on the fact that "she has a very limited knowledge of physics itself." He, like many others, missed the point entirely; she wasn't there for the physics, she was there for the roof.

10. **jhk**
June 6, 2007

Quite entertaining, but really nothing out of the ordinary. When I was at Stanford there were much weirder people hanging around the department.

If you're ever at Stanford, be sure to go to the physics library and look up a Ph.D. thesis by a Mr. Kenneth Uzo. And be sure to look at which faculty members signed off on it.

11. **anon.**
June 6, 2007

'Some of the faculty are weird, some of the grad students are weird. So you don't really know who's who.' – Surjeet Rajendran

All faculty and graduate students should wear suits (as was the case until the 60s hippy culture came in). Better still, they should be forced to wear a beard and smoke a pipe (as occurred a century ago).

12. **lostsoul Ph. D.**
June 6, 2007

I have always depended on the kindness of stringers

13. **stevenm**
June 6, 2007

From the article:
"If she were a large, intimidating man, there's no doubt that something would be done," he said. "There's a huge bias against appearances and it's prevented people from taking action. I can't see any reason why our department is special. This could really happen all over the place."

I can testify to this. My hobbies have always been weightlifting and weight training and when I studied in the US I slept in the offices overnight for a while at one point (with permission) while trying to find an apartment. Then one night I had an encounter with two security guys who were doing their rounds about 2am, and who quickly called for backup. I did have the appropriate ID and explained the situation. After that on subsequent nights they did'nt bother. I think they assumed I had something to do with the football team. This was a private university within the top 10 in the US and they had resources, and so security was quite tight. This was also back in the 90s before 9/11 and the paranoia that followed. There were also signs on the outside doors (and which you needed an electronic key to open) saying that anyone on the premises who had no right to be there would be prosecuted and probably jailed. The tolerance in the Stanford situation only persisted because it involved a small asian woman who no-one perceived to be a threat. A man of middle-eastern origin, a black man or scary white men (like myself :)) doing the same thing would be very quicky dealt with.

14. **Coin**
June 6, 2007

…so, um, is it bad that when initially confronted with a story about someone just persistently showing up at grad student seminars and pretending as if they belonged there, my initial response is "Whoa! That sounds like a pretty good idea! I wonder if I could get away with that?"

^_^;

I do want to agree with "Stanford grad student" that it is really important here to distinguish between what Scott Aaronson is calling "Groupies of Physics", and someone who is simply a homeless person pretending to be a student in order to gain access to the facilities. The distinction is important because the latter aren't fundamentally interested in science, just not freezing. I also think the latter are more common than most people realize (though I don't think most aim as high as Ms. Okazaki did)– I knew of at least one at my college who I eventually figured out was literally living in the 24-hour computer lab…

15. **anon**
June 6, 2007

I think Scott Aaronson's opinion is quite shallow. If academia wants to forge honest ties with society, they can do no better than to collaborate with private industry and entrepeneurs, so that real jobs, in the periphery of physics, are created. Scott wants groupies, and he wants to hire them to "swab the decks". Only someone who thinks he is so special he should have serfs to serve him would think that way. College Professors already have a bunch of poorly paid workers(graduate students) who write papers for them. Do these aristocrats need an additional class of poorly paid servants

16. **lostsoul Ph. D.**
June 6, 2007

Stevenm

She could have been a ninja in drag. Way back, when I was a post grad in Cambridge UK, a dude used to crash out at a course I attended. He was middle aged, smelly and, as the young might have it, gross. And he snored. And yet he took the whole course; he was given the benefit of the doubt. And, no, it wasn't Perelman. If it were not for the fear instilled by those who hope to govern us, we could all kick back and make , within reason, a little bit of space for others.

17. **Puny Geek**
June 6, 2007

Stevenm says: "The tolerance in the Stanford situation only persisted because it involved a small asian woman who no-one perceived to be a threat."

I generally agree with the comment above, except that #1 Asian women are not always small (but generally are). and #2 If she was a young attractive white woman, she wouldn't have been annoyed by others, much less getting kicked out.

I have seen so many cases when an attractive white/caucasoid woman was treated much better than an attractive asian woman, unless the asian woman is an object of fetishism. From my observation, the attitude towards women in male-dominated society seems pretty harsh, unless the women is super smart, "HOT" by their standard, or submissive to the males.

Of course, I cannot make a judgement about the case without knowing all the details, and I think a stranger *living* in a department is rediculous.

18. **Changcho**
June 6, 2007

Yes, I've met Susskind and I think the post was a just a little bit unfair to him. I believe I've seen situations similar to this in LeConte as well. The situation seems pretty much to be like what "stanford grad student" wrote.

To A. Bergman, that was great! Those lines from Blade Runner have always stayed with me ever since I saw the movie for the first time.

19. **Peter Woit**
June 6, 2007

jhk,

Googling out of curiosity turns up the following from the Palo Alto newspaper in 1997

"OUT OF THE CLOSET . . . When a Stanford math professor smelled smoke as he prepared students for a midterm exam last week, he followed its source to a nearby closet. Inside was a man sitting, smoking a pipe. "Can I help you?" the man asked as the math professor opened the closet door, according to Stanford police captain Raoul Niemeyer. The man–37-year-old Kenneth Uzo of Fremont–asked the professor if he would please close the door. The professor did so and then called the police. Niemeyer said that Uzo, who graduated from Stanford in 1992 with a Ph.D. in physics, is known to Stanford police as a habitual trespasser. He has been issued with a stay-away letter, Niemeyer said."

I wouldn't criticize anyone for signing off on the Ph.D. thesis of a problematic student. Put yourself in their position: you look at the thesis, you have some sort of thesis defense. Even if it's pretty bad, you then have two choices.

1. Pass the student, at which point the department has no more official connection to them and you can reasonably expect them to leave and not be heard from again.

2. Fail the student, in which case they'll presumably continue as a student, working on an "improved" thesis, and you'll have to keep dealing with whatever their undesirable behavior was.

20. **LDM**
June 6, 2007

Educators in academia who typically are supported by our tax dollars should be reminded that they are role models and one of their jobs outside of research is to develop the minds of their students. They are not being paid to exploit groupies… and encouraging and/or recruiting groupies would be ethically questionable.
NOW, to put this in perspective, some years ago, a young man named Steven Spielberg did a similar thing at Universal Studios. He would show up everyday and pretend to be an employee so that he could study his craft by watching films being made and by associating with people who were already doing what he wanted to do. The guards assumed he was an employee as did everyone else. And eventually, his knowledge of film making impressed Universal Studios sufficiently that he was given a seven year contract, before the age of 21. I don't think he was ever called a groupie.

21. **Ron**
June 6, 2007

I hope these two incidents back to back (Elizabeth Okazaki and Azia Kim) don't create a backlash to make Stanford a less trusting and open community. I attended as an undergraduate between '84 and '90, stopping out every other six months to work my way through school. Their policies were very student-friendly: one could leave at any time and return at any time, no questions asked. While working, I still spent a fair bit of time on campus between being enrolled. It is easy to see how Okazaki and Kim were able to take advantage of the situation. I've done so myself in other circumstances. http://www.PacificT.com/Story Stanford's trust in and respect for its students are a good thing.

22. **Fabien Besnard**
June 7, 2007

I also have some anecdotes about strange people in academia… One that I have witnessed myself is about a guy at Jussieu campus. He was always in the library whatever the hour, whatever the day. Nobody I talked to could tell me who he was. He read all kind of books, generaly in few minutes only… He had been nicknamed the ghost of the library. Someone told me, but I can't vouch for that, that he was seen someday on his knees in the middle of the street, holding a sing reading 'je veux une femme !" (I want a woman !).

23. **J**
June 7, 2007

Interesting. What is more interesting is that we have a similar guy here in China, a guy named Jia-zhong Chen. He stated that he is a professor of Harvard and somehow managed to get a position in one of the top universities (Zhejiang University) in China as a visiting scholar. He also claimed that he had published several "important" papers in PRL, which is not true. There are some other fake stuff that he has used for his cheating behavior. Oh, by the way, he even has never been accepted by any graduate school.

24. **jhk**
June 7, 2007

From the comments to the Stanford Daily article: " it sounded like she was regularly getting in and out of relationships with different guys around Varian as well as SLAC." I know being a grad student or postdoc can get pretty lonely, I wonder how many guys took the bait.

25. **Jonathan Vos Post**
June 7, 2007

Though Scott might not repair washing machines to keep groupies around, I suspect that he would repair Turing machines.

Are science fiction authors a kind of "science groupies"? Science fiction readers? People who subscribe to Scientific American? People who blog about science? People who watched "A Beautiful Mind" or "Goodwill Hunting" or "A Brief History of Time" or the hit CBS-TV show "NUMB3RS?" People who attend lectures for the free coffee and cookies beforehand?

Some "science groupies" — a small fraction, but not zero — become productive scientists. The outstanding examples include Fritz Zwicky.

There is an overlap between "science groupies" and crackpots. Howveer, it can be NP to distinguish these. One generation's crackpots includes the next generation's pioneers of the paradigm. Crackpots know this.

Living 5 miles from the Caltech campus, where I earned my first degrees, I visit campus at least once a week, attending seminars, chatting with professors, students, staff. Since I am not currently a professor (have been, in 2 fields) — does that make me a groupie?

Secretaries at Caltech, especially Math and Physics are trained to handle crackpots who walk in with manuscripts on proving Einstein wrong, squaring the circle, classifying solutions of Fermat's Last Theorem, and the like. The secretaries tell me that the crackpots sometimes explain that they hope, by dropping off their manuscripts, to instantly become professors, when their genius is recognized, a la Ramanujan.

Is any nonscientist who dates a scientist actually a science groupie? How do you know that?

The last time that I saw people living in a dorm who were not students, they turned out to be a pair of rogue cops doing an unauthorized undercover recreational drug sting, and busted an essentially innocent senior the day before he graduated, having had to have his parents, just arrived in town for the ceremony, bail him out of jail.

Which reminds me. Tomorrow is the simultaneous Caltech Presidential Inauguration and graduation, with Jared Diamond as commencement speaker. Dr. Jean-Lou Chameau, who has served as Caltech's president since September 1, 2006, will be inaugurated in a simple ceremony at the start of Caltech's 113th annual commencement on June 8. This bucks the tradition of university presidential inaugurations that involve a week of lectures and dinners, capped off with a large inauguration ceremony. Chameau felt his inaugural should reflect his priorities. Within the audience, how many of the people should one characterize as groupies?

Groupies? Drugs, sex, rock & roll, and quanta.

26. **Brain Greeen**
June 7, 2007

Clearly Peter is a string groupie, someone who tries to put meaning in his own life from string theorists' reflected glory.

27. **Michael**
June 7, 2007

The story would have been much more relevant (cooler) had Okazaki actually been interested in the physics at Stanford. Still, her situation is, though unknown to most, a minor but definitely important breach of a very expansive and secretive contingency within the halls of America's most heralded institutions:

Here we are then, countrymen. Artists who would be starving and homeless as well, we work by day in research-and we've come to love science: i do-but i am no scientist of your trimmings, Peter Woit. And yet the title hangs on my door as i type.

But what business do "they" have here?? Here? In this well-fenced depot of creativity? One might surmise they have merely been fortunate enough to mistake the gatekeeper for a teacher.

And so it goes that science brains reduced are, simply, obsessively objective artist brains who choose to model their reality with quantities over colors, tones, and words. Perhaps you will see it other way around. Modelers of observation. All of us. Just happens to be that the spies are justifiably more eager to spray their notings rudely, if even astutely on the walls of the Colluseum. Good clean, class penetrance I suppose. Wait. Is that what we're afraid of?

28. **MathPhys**
June 8, 2007

I liked very much the statement that "A university has a lot of weird people… Some of the faculty are weird, some of the grad students are weird".

How true. In fact, universities that have policies that one way or another bans such people, are not universities. They are businesses or something, but not seriously academic universities.

29. **Chris W.**
June 8, 2007

More or less in this vein, read and listen to Ron Avitzur tell "The Graphing Calculator Story," about developing software at Apple (for the first Power PC Macs) as an unpaid former contractor who had to sneak into the building in which he was working.

(By the way, Avitzur is a Stanford graduate; he was a physics major who subsequently moved into software engineering.)

30. **Ron**
June 8, 2007

While working sixteen hour days, seven day weeks, months on end, unpaid, folks at Apple occasionally said to me: "I see you here when I get in in the morning. I see you here when I leave at night. And I'm working twelve hour days! What are you doing?!" I looked them in the eye and said in a deadpan voice: *I'm in training for physics graduate school.*My Stanford experience prepared me for working at Apple. My fellow undergraduates had a theorem regarding the difficult nature of our studies: either we had been through worse already and could handle it, or if we had not been through worse before this would set the standard by which all future troubles would be compared so that after this nothing would phase us.

31. **More Asian Female Squatters, please.**
June 8, 2007

The Okazaki girl was kicked out as a byproduct of the A. Kim dorm-squatting case, i.e., people contacted the Stanford student newspaper about her case once the Kim story broke.

32. **Stanford grad student**
June 8, 2007

The Okazaki girl was kicked out as a byproduct of the A. Kim dorm-squatting case, i.e., people contacted the Stanford student newspaper about her case once the Kim story broke.

Well, it was the perfect opportunity. There had been complaints about Okazaki for years, and the department dragged its heels like hell.

33. **Chris W.**
June 9, 2007

*..nothing would **phase** us.*

That should have been "faze," but "phase" is an understandable substitution for a physicist or engineer. Perhaps it was a double entendre. 🙂

(No knock intended; as you'll see from his narrative, Ron is an excellent writer.)

34. **what?**
June 10, 2007

I've read every comment in this post and I still don't understand what a homeless girl living in the Stanford physics building has to do with string theory.

35. **Peter Woit**
June 10, 2007

what?

Instead of reading the comments, you might try readng the posting where I quoted part of the article that explained that this woman was passing herself off as a visiting scholar in the humanities, in the physics department because she was working on string theory with Susskind.

36. **MathGirl**
June 12, 2007

Better still, they should be forced to wear a beard and smoke a pipe (as occurred a century ago).
Oh yes, like in the good old days, when women weren't allowed at universities at all…

37. **surlygrad**
June 12, 2007

I totally understand the part about why no one bothered her. We had a student who, for reasons not to be discussed, lost his funding, and decided to just move into the grad student offices (eat, sleep, etc.). The rest of us really had no idea how to react or deal with the situation. It was unpleasant and there were hygiene issues

Eventually things got straightened out (the student got funding again), but it was very very weird and awkward for about six months.

38. **a.k.**
June 28, 2007

one could wonder if anyone was interested what happened to Okazaki after she had been 'kicked out' of the department, it is interesting to note the low degree of 'solidarity' (in a yet to be defined way) with her situation reflected in the above postings, whether she had been actually interested in physics from any valid viewpoint or not or did academic work in any valuable way in these four years or not- her fate (and those similar cases described described above) exemplifies what in europe would be cosidered as 'the darker side of america' and explains why hardly anyone, for instance in germany, tends to deny the value of certain achieved social standards, i.e. to formulate certain rights concerning appartment and minmum social funding.

39. **kathleen_conformal_strings**
July 12, 2007

Hmm…

I think someone like that could be a nice addition to any physics or math department… Her funny presence could be a form of relief as well as mourn all bundled up with a bit of curiosity, compassion, and disgust.

I've seen her in the department myself. I can't speak of this lady as being very distinct from any weirdo to whom I would go to interact with… There are others which appear and disappear with changing mood.

She's a confused person herself and it's remarkable how she tries to be as ecstatic as possible in that confusion. Alas, her confusion has seeped into the minds of all the geeks there and turned their being from inside out, rather unnecessarily.

Why oh why, just leave her alone, guys. Is it surprising that such people with their wierd (but reasonable) sets of intrigues exist? Don't let this dueling go on for decades, coz' who knows it wears someone out. Otherwise, let's just commit to confusion and see if it all can be reasoned necessary.

It all just leaves me laughing…

Back to quantum conformal gravity, Pontrjagin, and superpotentials…

|Kat>

Dispute Chez Les Physiciens

June 7, 2007
Categories: Uncategorized

Yesterday evening there was a public debate about string theory held in Paris, between Lee Smolin and Thibault Damour. So far, accounts of the debate have appeared in Le Monde and at Fabien Besnard's blog Mathephysique.

The Le Monde article is not very informative, but indicates that Damour defended string theory against charges that it was not testable by claiming that it predicted "possible classes of experimentally testable phenomena" at the LHC. Besnard gives a more detailed account, describing how Damour answered these charges of lack of testability with: "Lee, a subtle thinker, surely doesn't believe himself the naive Popperian position he is defending". He also evidently claimed that string theory was testable because it would be confirmed if a violation of the equivalence principle was found (he really should talk to Lubos, see here). Remarkably, he also claimed that observation of the kind of DSR dispersion relations that Smolin thinks LQG leads to would not be a problem for string theory, since one could also get them out of string theory (here I think he needs to talk to both Lubos and Jacques Distler).

Update: I hear that the event was recorded, and audio should be available by the end of the week at the web-site linked to above.

Comments

1. **Aaron Bergman**
   June 7, 2007

   This translation is from google, so I apologize if it gets it wrong:

   > LS explained why certain violations of the relativistic relation of dispersion were predicted by the quantum theory with loops, and that if they were not observed, that would refute the theory,

   Strange, isn't it, how Lee always leaves that (incorrect) impression.

   It's worth pointing out that while most of the scientific community works by some combination of positivism and falsificationism (which isn't nearly as contradictory as historically indicated), the philosophy of science has moved beyond both as I understand it. There's a lot of stuff out there about the problems with "naive falsificationism", for example — I'd guess that's what Damour is referring to.

2. **Peter Woit**
   June 7, 2007

   Aaron,

   The way string theorists are hiding behind philosophy of science is just ridiculous and less than honest. String theory makes no predictions, that's all there is to say about it right now. You can honestly argue that maybe once we understand non-perturbative string theory, this will change, but invoking subtleties about the philosophy of science to claim that string theory really is testable is just dishonest. And claiming that string theory can be tested by looking for violations of the equivalence principle is just absurd.

3. **Aaron Bergman**
   June 7, 2007

   And here I thought I was just clarifying a point of philosophy….

4. **anon.**
   June 7, 2007

   Babelfish translations: Le Monde and Mathéphysique.

5. **Lee Smolin**
   June 7, 2007

   Aaron,

   Please listen to the recording. I was quite precise. I said that LQG pointed to possible new phenomena which were characteristic quantum gravity effects. I gave as an example of such a possible phenomena, DSR , but I also said that there were no agreed upon precise predictions. I also said that I had written a paper deriving such an effect at a semiclassical level, but without a precise coefficient, and I also emphasized that my more rigorous colleagues were as yet unconvinced by this. I also said at least one other time that neither string theory nor LQG had so far made precise predictions by which they could be falsified.

   Lee

6. **Coin**
   June 7, 2007

   > **The cords of the discord agitate the cosmologists**
   >
   > C ' is a scientific argument with old. A controversy as the time offers any hardly any more. Wednesday June 6, in the City of sciences, in front of several hundreds of researchers, students or simple curious, two scientists of high flight discussed one of most attractive – and more inaccessible for the common run of people – constructions of physics: the theory of the cords. For the charge, the American Lee Smolin, researcher in Institut Perimeter of Ontario and signatory of the lampoonist *Nothing goes any more in physics* (Dunod, 2007).

   Oh, how I love Babelfish. I think their new name for Smolin's book is maybe actually an improvement.

7. **Aaron Bergman**
   June 7, 2007

   I don't know what you said; there doesn't appear to be a recording available yet. What I was commenting on is the continuing propensity (and the above is far from the only instance of this) of your listeners to come away with the impression that deformed dispersion relations are a prediction of LQG.

It seems odd, no?

8. **Yatima**
June 7, 2007

Babelfish serendipity:

…and signatory of the lampoonist ***Nothing goes any more in physics*** *(Dunod, 2007).*

That is actually the correct translation for the french title "Rien ne va plus en physique". Far stronger than "The trouble with…".

"lampoonist", however, is clearly wrong – "pamphlétaire" has the meaning of "written by a firebrand".

In other news, one can detect despondency at [Slate](), whereby the tought of the Standard Model soon being buttressed by a Higgs sighting and not much happening thereafter causes depression. Strings only feature on page 2:

But what happens if the Higgs turns out to be just right? Well, then the standard model predicts that you'd need a machine roughly a quadrillion times more powerful than the LHC to find anything new. (…) Though some theorists—proponents, for instance, of string theory—speculate about what such an accelerator might find, few other physicists take them seriously.

9. **Coin**
June 7, 2007

Yatima: Ah, thanks for clarifying.

10. **Garbarge**
June 7, 2007

"And claiming that string theory can be tested by looking for violations of the equivalence principle is just absurd."

I dont think that is absurd, I actually believe that is a very important venue. If GR is *wrong*, and not just due to higher order curvature effects, then we will know for sure ST, in its current form, cannot be right. The same about Lorentz Invariance, unitarity and analyticity.
The quibble though lies on how big of an effect can this be and whether can it be measurable. To attack the building blocks of a theory, otherwise adjustable to 10^500 different scenarios, sounds like the only possible route to success (or failure depending on which side you want to jump on). What's wrong with testing the only few properties all these vacuum solutions actually share!

Of course, violations of these sacred principles will radically modify our views and intuition about the world, but as a side dish will put the ST community on stage for a big change whereas LQG people may even celebrate after all 😉

11. **Peter Woit**
June 7, 2007

Garbage,

My objection is to what I take to be Damour's claim that an observation of a violation of the equivalence principle would be evidence for string theory. If you believe Lubos, string theory predicts no violation of the equivalence principle, if you believe Damour, it predicts a violation. They need to get their story straight, but can't, because string theory can accomodate virtually anything.

12. **theoreticalminimum**
June 7, 2007

I have a problem with this rhetoric:
"*"le désaccord sera toujours nécessaire à l'avancée de la science"***"** ["disagreement will always be necessary for the advancement of science."]
This statement is, I believe, vacuous and incorrect. I think that, within a community consisting of different groups of people motivated in exploring different ideas, disagreement will always naturally occur when these ideas are confronted with each other, or with any form of philosophical discussion. What really counts as progress in science is that, at some point, repeated confrontations bring the members of the community to realise that they have to agree on the correctness or incorrectness of an idea. This agreement is the advancement. Unless we can all agree that some theory is right or wrong, we can spend decades disagreeing, and science will not take a definite step forward. In other words, disagreement is necessary for the exploration of ideas, but ultimately, we have to agree on what makes sense so that we can move on.

13. **Fabien Besnard**
June 8, 2007

Lee, I wish I were more precise about your statements on my blog. Indeed you emphasised that LQG does not yet make precise predictions everyone agrees about, and I'll make an update. Still, I think the point is not really there, it is more a matter of principles. If I understood well, you wanted to show that in principle LQG is falsifiable, and when you asked Damour about the impact of DSR, should it be observed, on string theory, his answer was that ST could accomodate both DSR and no DSR. Would you agree with this version ? This is why I said that in principle, supposing some consensus is reached in the LQG community about DSR and supposing GLAST experiment sees it, one would be a in strange situation with in one hand a falsifiable theory passing a popperian test and on the other hand an incompatible, non-popperian one, claiming it does not care…

14. **Fabien Besnard**
June 8, 2007

I must also say that Thibault Damour never said that String Theory could be confirmed by some experiments at the LHC, contrarily to what the Le Monde article says. In fact, if I correctly recall, TD said he was not optimistic about this.

15. **Bee**
June 8, 2007

Hi Peter,
thanks for the links, should listen to that audio. Reg DSR, this is quite ironic. I am reasonably sure there is a comment of mine from last year somewhere in the blogosphere that says I'm waiting for a string theorist to claim they can have DSR as well. I'm just done with the String Pheno conference, giving a talk (half-ways) about

DSR – can't say I had the impression anybody was particularly interested in it (but then, it was before the first coffee break…). Best,
B.

16. **Brett**
June 8, 2007

The claim that string theory can't violate the equivalence principle or Lorentz invariance isn't justified either. It's very hard to study nonperturbative effects in string theories, but in off shell string field theory, there are operators that certainly could give rise to spontaneous breaking of these symmetries. In the twenty-six dimensional bosonic version, it seems that this does not actually occur, but at the same time, there doesn't appear to be a fundamental reason why this is so; it's just a matter of which local minimum happens to the global extremum of the effective potential. And for more "realistic" string theories, the question is still open.

17. **Question for Aaron**
June 8, 2007

Aaron Bergman wrote:

"I don't know what you said; there doesn't appear to be a recording available yet. What I was commenting on is the continuing propensity (and the above is far from the only instance of this) of your listeners to come away with the impression that deformed dispersion relations are a prediction of LQG.

It seems odd, no?"

I assume Aaron is just as scandalized by, and as prone to criticize, the much larger number of instances where string theory papers or lecture(r)s leave their audience with the impression that string theory makes predictions comparable to the above. That should go without saying, right?

18. **Aaron Bergman**
June 8, 2007

Everyone can be prone to overstatement, but string theorists haven't gone out writing books trashing their colleagues and setting themselves up as standard-bearers of scientific virtue.

19. **anon.**
June 8, 2007

'… but string theorists haven't gone out writing books trashing their colleagues and setting themselves up as standard-bearers of scientific virtue..' – Aaron

That's the problem! Instead of going out and trashing not-even-wrong hype and 'setting themselves up as standard-bearers of scientific virtue', they do the opposite…

20. **Question Answered.**
June 8, 2007

So the "overstatement" of LQG predictions is not different from string theorists' numerous overstatement of stringy predictions, but Aaron finds other reasons for criticizing only the first? Thanks for confirming that the diss was bullshit.

21. **woit**
June 8, 2007

QA,

I think I see what Aaron is saying. He's attacking someone from outside his group for doing something that he wouldn't criticize if it were done by someone inside his group, because that outsider accused people in his group of engaging in "groupthink" behavior.

Sorry Aaron, I know the above is a bit unfair. But, you know, string theorists would get a lot more sympathy if there were evidence of significant internal criticism by string theorists of the excesses going on in their own community. Some days I feel like sending a bill to some leaders of the string theory community for doing work that they should be doing.

22. **Aaron Bergman**
June 8, 2007

Actually, it seems to me that you're acceding that it was perfectly legit.

If your only response is that, well, string theorists make overstatements too, then perhaps we're all kinda the same. There are people who are overly enthusiastic and curmudgeon's all around. Nobody is more blinded by groupthink than anyone else, and there are mountain climbers and valley crossers everywhere. Lots of people are doing their best to figure out hard problems, and there's no monopoly on virtue centered in Ontario.

Which was sort of my point.

HTH!

23. **Aaron Bergman**
June 8, 2007

To PW: I don't like to criticize my colleagues in public because I generally don't think it's, well, collegial. LS, on the other hand, has chosen to write an book, a fair fraction of which, his protestations to the contrary, is precisely attacking his colleagues. I think that makes his own hypocrisies fair game, and I don't feel particularly collegial towards him.

24. **Chris Oakley**
June 8, 2007

Aaron & others,

The important issue for the select group to which most of you belong — a group financed by tax dollars — is to carry out your mission of discovering the workings of nature with the maximum of energy and integrity. If anyone wants loyalty, they should get a Cocker Spaniel. This so-called quality is totally contrary to the spirit of scientific enquiry and leads to the kinds of closed shops that Peter W., amongst others, has been justifiably complaining about all along.

25. **Aaron Bergman**
June 8, 2007

I don't think exposing the private fights of science (not all of which are substantive) to the public helps anyone.

(Of course, with the recent recording of talks at conferences and the like, the line between public and private has been somewhat blurred.)

26. **Coin**
June 8, 2007

I don't think exposing the private fights of science (not all of which are substantive) to the public helps anyone.

As a member of the public, I kind of have to say I disagree.

The public of course shouldn't be *settling* scientific disagreements or whatever, but I think keeping people informed is an unqualified good unto itself.

27. **tomj**
June 8, 2007

Here is an idea: when physicists use ordinary language, and pretend that the ordinary meaning somewhat applies to what they are talking about, then ordinary idiots which understand the ordinary meaning have every right to ask questions.

My main complaint with much of modern physics is that common words (not phrases or sentences) are used to describe or tag new ideas. The effect is very similar to teenagers overusing common language for new (private) meanings. Forget about the philosophy of science, how about the philosophy of language? People who call themselves scientists and who can't get these basic philosophies right probably shouldn't be proposing theories.

A philosophy is a framework, it contains principles, ways of comparing more concrete frameworks. Even theories are frameworks, but they are of a more concrete nature. String Theory and the Landscape bs appear to be meta-frameworks, above theory, similar to a philosophy. Viewed in this way, it is easy to see why you cannot disprove it: it is above theory. Only theory deals with data.

The problem with these meta-frameworks is that they use mathematical language as a means of definition. The effect is to mismatch the language with the level of use. Example: if the general principle of relativity can be described as a result of assuming that the laws of nature are the same in any inertial frame (as defined in GR), which is a pretty simple philosophical principle, how can a heavily mathematical theory be somehow above this? Mathematics is not going to be above philosophy. Experiment and the resultant data is not going to be above theory. And by above, I don't mean more important. Experiment and data are the foundation. You cannot throw out data based upon a new theory or philosophy. This is exactly what is happening with ST, etc. These are frameworks, above theory, somewhere near philosophy.

But the problem: philosophy and the resultant frameworks are relatively easy to describe, even if the implications are difficult or impossible to appreciate. ST, etc. somehow fail this test in a huge way. Somehow the details are chased around in an attempt to connect them with experiment or even theory. Unable to catch up with the facts, with data, the only thing left is a repudiation of the theories which are based upon the known facts. It is almost like concluding that the speed of light is infinite because nothing can catch up to it.

28. **Lee Smolin**
June 8, 2007

Dear Fabien,

Check the recording when it is available, but what I remember Thibault said is that string theory could accomodate broken lorentz invariance, as there are many vacua that have this property. Given that this was a non-technical debate in front of a popular audience I choose not to belabour the difference between broken and deformed lorentz invariance (which I suspect he understands well.) From what I know, no one has demostrated a form of interacting string theory consistent with deformed lorentz invariance, although Magueijo and I found evidence for consistent free bosonic string theories with this property. I know of string theorists who are now working on this problem, and from a much deeper point of view, but so far as I know they have not published yet.

Aaron, your whole discussion assumes that I exaggerated. I insist that a listen to the tape will show what I said was quite precise and true, indeed I went out of my way to emphasize that 1) many of my colleagues do not think there is strong enough theoretical evidence for the conclusion that 3+1 LQG is DSR and 2) even me who thinks there is agrees that I can only calculate the effect semiclassically up to an undetermined dimensionless constant.

29. **Aaron Bergman**
June 8, 2007

I insist that a listen to the tape will show what I said was quite precise and true

That may be true, but it's not my point. As I have already stated said point in multiple threads, I won't belabor it any further.

30. **M**
June 9, 2007

Aaron, around 1995 the LSND collaboration published a paper claiming a new discovery and one young member of the collaboration, instead of signing the collegial paper, published another single-author paper explaining why he did not believe in the result.

In the same years, no young string theorist questioned the mono-vacuistic ideology of string gurus, or complained about how the theory was irrealisticaly presented to the public.

Sometimes good science needs a departure from loyalty.

31. **Bee**
June 9, 2007

Hey Tomj,

philosophy and the resultant frameworks are relatively easy to describe, even if the implications are difficult or impossible to appreciate

Whether or not you think philosophy is 'relatively' easy is a very relative statement. I happen to think maths is much easier than philosophy for the 'simple' reason that it only deals with well defined objects and not to a considerable amount with interpretations of words or juggling with undefined things. I have no idea what you want to say with your comment, do you think physicists should not use words from the 'ordinary language' to describe objects because it might confuse the public?

Forget about the philosophy of science, how about the philosophy of language? People who call themselves scientists and who can't get these basic philosophies right probably shouldn't be proposing theories.

Well, the problem is that 'the public' most often doesn't understand that the theory is not made out of words but of equations. Every description that tries to avoid these equations might seem simpler but is never quite as accurate, and rarely manages to capture the elegance of an idea (my opinion). The obvious way to avoid this is just to learn the 'language' of mathematics, instead of accusing scientists to abuse 'ordinary' language. 'Reading' maths is neither a mystery nor complicated to learn (definitely not more complicated than, say, Polish), it's just a way to decipher equations. Whether you learn how to 'speak' it, is another issue. I'd wish the prejudice that maths is a nerdy thing (whereas French isn't) would end up in the trash bin. This trend however is supported by dropping equations from pop sci books, and articles etc. because it 'scares' the readers.

Best,

B.

32. **Bee**
June 9, 2007

Oh, and regarding DSR and so on, see e.g.

DSR
The Minimal Length Scale

or the slides to my yesterday's talk. Currently too tired to repeat what I've written there. The point is instead of understanding st as a theory of an extended object with standard SR, I am considering a model of 'standard' point particles with the additional property of reproducing the presence of a minimal lenght alias UV regulator or generalized uncertainty relation respectively. Thus, strings with standard SR are instead described by point particles with funny special relativistic properties (applies off shell). Plus point is, it's not necessarily an string-only approach (the only assumption I make is that the underlying theory has something like a minimal length). Differences to DSR Lee is talking about can be found in hep-th/0603032 (there are several, the most important one being my modification is offshell only, see also gr-qc/0412004 cmp. section IV about the Snyder basis and note that p^2 is the square of the four- not the three momentum). With greetings from Warsaw,

B.

33. **Aaron Bergman**
June 9, 2007

M — do please note the word "public". If you think there haven't been disagreements over just about every point of theory by string theorists young and old, then you haven't hung around string theorists very much (or physicists in general for that matter.)

The other point people seem to have missed is the difference between criticizing one colleagues and criticizing their *work*. There are papers on the arXiv contradicting other papers every day. A common pastime at lunches in various string groups is to try to figure out why the papers of the day are wrong without reading more than the abstract :). This is a good thing. Similarly, if someone wanted to write a book about the problems with string theory, I'd probably disagree on some of the substance, but I wouldn't throw a huge fit. Lee, on the other hand, isn't just disagreeing with string theory; he's attacking string theorists as scientists. Criticizing one's colleagues in this manner in a book aimed at the public is something I find extremely objectionable.

34. **anon.**
June 9, 2007

'… he's attacking string theorists as scientists. Criticizing one's colleagues in this manner in a book aimed at the public is something I find extremely objectionable.' – Aaron Bergman

You just need to lighten up a bit, Aaron. Ridicule is a tradition:

'Oh, my dear Kepler, how I wish we could have one hearty laugh together! … And to hear the professor of philosophy at Pisa labouring before the Grand Duke with logical arguments, as if with magical incantations, to charm the new planets out of the sky.' – 1610 letter of Galileo Galilei to Johannes Kepler, http://www.catholiceducation.org/articles/science/sc0043.html

35. **Chris Oakley**
June 9, 2007

Aaron,

I find your attitude to be a little too convenient. I see neither LQG nor String Theory as providing any insights other than the limited utility of signposting blind alleys, but whereas Smolin expresses himself with invariable politeness and tolerance, your own Motl throws insults around like they are going out of fashion. Why is Smolin "extremely objectionable" and Motl not? I would be interested to know.

36. **nardex**
June 9, 2007

Peter,

I've read that "the study of strings has boosted a great amount of study in many areas of mathematics and of mathematical QFTs, and has revealed very beatiful truths belonging to these areas".
Thence, for You this is not important for the string theory itself?
This is the "because" that you says that the string theory is "not even wrong".
I think that any theory that "has boosted a great amount of study in many areas of mathematics" is a useful theory.
Looking forward to receiving your kind answer

All my esteem

37. **Aaron Bergman**
June 9, 2007

Chris, why not do a little googling?

38. **Eric Mayes**

June 9, 2007

I'd like to propose the following theorem:

Any theory of quantum gravity which provides the UV completion of quantum field theory will have a landscape of consistent vacua.

The reasoning behind this is that quantum field theory can incorporate a large number of consistent models, and so it follows that a theory of quantum gravity which incorporates QFT will do the same. Thus, even if Lee's loop quantum gravity is able so someday encompass QFT, it will have the same landscape problem of string theory.

39. **Chris Oakley**
June 9, 2007

Hi Aaron,

OK.

Search #1A: "Smolin" "Insult"; top match

> In the debate, Michael said that "to put Stephen Hawking on the same page as Lee Smolin is an insult to the people of this audience" (which was more of an insult to Lee Smolin), yet even Hawking no longer believes there are singularities, and for the very same reason Smolin does not, and the very same reason Barrow does not, and the very same reason I gave in the debate: every going theory of quantum gravity, including Hawking's, eliminates them.

Search #1B: "Smolin" "Scorn"; top match

> Smolin points accurately to some of the real problems with the sociology of university life, and seems filled with scorn for those who think that nothing can be done about it.

Search #1C: "Smolin" "Vituperation"; top match

> It seems to me that the RANCOROUS VITUPERATION is confined to the minor figures.
>
> I don't see Smolin or Witten as rancorous squabblers at all, or David Gross…or JB for that matter. They all seem to be above the squabble.
>
> Smolin had some serious points about policy and science in general, which he made politely and respectfully (I thought)

Now let's try

Search #2A: "Motl" "Insult"; top match (on this blog 9/11/2005)

> Happy birthday Peter. I've enjoyed reading your blog, with the exception of Motl's insult-laden comments, and I look forward to buying a copy of your book when it comes out.

Search #2B: "Motl" "Scorn"; first relevant (also on this blog)

> Now that it's available again, the Rumor Mill has the striking news that Harvard has chosen for a faculty position one of its postdocs: Lubos Motl. Lubos is well-known as undoubtedly the most rabidly fanatic string theorist around, always willing to heap abuse and scorn on anyone who questions the idea that string theory is the language in which God wrote the world.

Search #2C: "Motl" "Vituperation" ; find the same as with "Smolin" – then this:

> lubos motl has published is objections to loop quantum gravity which lee smolin responded to.
>
> what are some common objections to string theory, esp in NEW, what do string theorists think of these objections? lubos motl calls peter's book crap, gives it 1 star on amazon. (then it got deleted).
>
> ——————————————————————————————–
>
> marcus08-27-2006, 04:30 PM
> what good does animosity and vituperation do?
>
> I don't think it does much good.

Need I say more?

40. **Aaron Bergman**
June 9, 2007

Say more? I have no idea what your point is. It's certainly not relevant to mine.

41. **The man**
June 9, 2007

Perhaps a fitting cousin of string theory for which "not even wrong" also applies:

http://itre.cis.upenn.edu/~myl/languagelog/archives/000024.html

42. **Kris Krogh**
June 9, 2007

Aaron,

The point seems clear enough and Googling was your idea. What can you come up with?

43. **Aaron Bergman**

June 9, 2007

Chris asked why Smolin was objectionable and Lubos was not. Seems to me that that's assuming facts not in evidence.

44. **Kris Krogh**
June 9, 2007

Hi Aaron,

Maybe Google is attesting to some facts here.

45. **Kris Krogh**
June 9, 2007

Are you agreeing now that Smolin is not objectionable?

46. **mclaren**
June 9, 2007

Haven't these people ever heard of Occam's Razor? The claim that some fabulously elaborate conjecture which makes no testable falsifiable predictions is nonetheless verifiable because the simpler proven theory of physics (The Standard Model) to which it reduces in the limit is testable, is just breathtakingly ignorant and shockingly foolish.
If we accepted this crackpot reasoning, then every crank conjecture without a shred of evidence would be considered "testable" because the standard physics on which the crazy conjecture gets piled (without a shred of evidence to justify it) is known to be testable.
Ridiculous but obvious example: ESP is good solid science because it's based on conjectured "ESP waves" (whose precise nature remains conveniently undefined — as undefined as the actual nature of the alleged strings in string "theory") which follow the familiar laws of physics, and since the familiar laws of physics are testable, ESP must be solid verifiable science too.
Other examples of this gross abuse of Occam's Razor in physics are so obvious it's insulting even to discuss them, but since these Ivy League physicists seem to persist in this bizarrely defective reasoning, I guess someone has to point out the obvious.
History is littered with elaborate conjectures that reduced to familiar solid physics in the limit, but which turned out to be unnecessary idle speculation. The most obvious example is the luminiferous aether. As late as 1925 reputable physicists were giving lectures abou "the crisis in the ether." Experiments were being done to try to detect the ether as late as the mid-1920s. None succeeded in showing any evidence of the ether. That was the crisis. The solution to the crisis turned out to be simple — throw out the ether as an unnecessary conjecture. Use Occam's Razor. Do not multiply superfluous entities. Problem solved.
It's getting unutterably wearisom to hear all this talk about string "theory." It's not a theory, people, it's a conjecture _at best_. Let's call it what it is: the String Conjecture. It's not a theory since it hasn't even advanced to that stage yet. It's not even an hypothesis yet. It's just people blwoing mathematical smoke up our asses.
Moreover, the String Conjecture is a baseless conjecture. There's not a single shred of evidence in all of physics for the conjecture that we live in a universe with more than 4 physical dimensions. There's not the merest scintilla of evidence in the entire history fo physics that there exist any universes other than our own, let alone that multidimensional "branes" can come into contact with one another in 11 (or 26) dimensional space and set off Big-Bang-type events.
There's not the smallest tincture of evidence from any physics experiment ever done that ultra-tiny strings exist in 11 (or 26) dimensions and give rise to all the elementary particles with their vibrational modes.
All this stuff is science fictional conjecture. There's no evidence for any of it. The science ficitonal part isn't fatal — after all, the notion that particles are also waves is science fictional conjecture, but that weird wave/particle duality conjecture was forced upon us by overwhelming masses of evidence. The string conjecture isn't required by any evidence, it's just some neat math. At this point, the string crowd will predictably point out there was no evidence for the neutrino when Fermi hypothesized it. But that analogy is fatally flawed. Fermi based his hypothesis on a rock-solid law of physics, the conservation of momentum. If we throw that out, we're all in trouble. So a strange conjecture like the neutrino was preferable to the much more unpalatable conclusion that momentum conservation was violated. This is the same principle David Hume used in his reasoning. Choose the conclusion that requires the least miraculous operation of the universe, and you're usually correct
But unlike the case of the neutrino, no such rock solid principle of physics requires us to posit the string conjecture. 11-D strings are just idle speculation, with no hard evidence to back it up.
Well, the problem is that 'the public' most often doesn't understand that the theory is not made out of words but of equations.
Once again, this string speculation is **not** a theory. It's a *conjecture*. It's a ***baseless*** conjecture, derived from no physical evidence, just math.
Anyone can make a wild conjecture from elegant mathematics and then posit outlandish disconnected results. For example:
"Green's Theorem requires that we live in an 11-dimensional universe in which all elementary particles are made up of vibrating strings." No it doesn't. Green's Theorem is just a set of equations, and they don't require that strings or higher dimensions exist, they're just empty mathematics which makes no predictions about observed reality. So how is that different from string "theory"? Like the vacuous nonsenical conjecture above, string 'theory" doesn't require that strings or higher dimensions actually exist, string "theory" only describes such hypothetical constructs **IF THEY DO EXIST.** Like my absurdly meaningless Green's Theorem example, string "theory" makes no testable falsifiable predictions about observed reality. Like my ridiculous example involving Green's Theorem, no basic physics requires us to make that conjecture.
Really, too many supposedly educated physicsts suffer from a weirdly superstitious awe of mathematics. Just because you write down an equation, however elegant, doesn't mean it has any necessary relationship with reality. The math can be true and beautiful and yet flagrantly contradict observed reality. The Banach-Tarski paradox is the most obvious example of beautiful and indisputably true mathematics which completely contradicts observed reality, but you find these kinds of weird results littered throughout mathematics. The kind of naive Platonism we get from the string conjecturists is just bizarre. Don't any of these string people know that there exist other options beyond naive Platonism? Did John Dewey and David Hume not exist? Where were these people during Philosophy 101? Asleep?
Wake up, people! There's nothing magical about equations, they're just mental models — not reality itself. Just because someone writes down a beautiful series of interlocking equations doesn't mean they must be true. Keats' claim "Truth is beauty and beauty truth, that is all ye know and all ye need to know" may sound passable for art or poetry, but it fails disastrously in science. A physics theory is NOT a set of equations, it's a testable hypothesis with a rational mechanism behind it that happens to be embodied in equations. The equations are not always required for a theory to be solid science, as the second laws of thermodynamics demonstrates. That law of physics was valid for quite a long time before Boltzmann and later Maxwell put the second law of thermodynamics on a firm mathematical footing.

47. **Peter Woit**
June 9, 2007

nardex,

You've already asked that off-topic question under another name and it has been discussed here. String theory is "not even wrong" as an idea about unification in physics, this has nothing to do with whether it is useful in mathematics or in other areas of physics.

All,

The comments here have gotten pretty much completely off-topic, not to mention not making much sense. If you have something substantive to say relevant to the posting, please do so, but otherwise please just stop it with the pointless argumentation, rants, etc.

48. **Eric Mayes**
June 9, 2007

Dear Mclaren,
This are many compelling reasons for introducing strings and it is far from being 'baseless conjecture'.

This really sad thing about this debate is that there are so many ignorant people out there who are easily manipulated by these fallacious arguments. If we're honest, you have to admit that what is being peddled is us-against-them -ism. In this picture, string theorist are elitists who live in their Ivy League ivory towers telling the common-folk who really no better what to believe, while the populist heroes Woit and Smolin are the defenders of rationally and common sense. The truth is that the serious people who are really doing physics will continue to study string theory because it offers the best chance to make progress, by far.

49. **Peter Woit**
June 9, 2007

Eric,

In blog comment sections you'll find some examples of uninformed populist nonsense, but neither Smolin nor I, with our Harvard and Princeton backgrounds are this kind of populist. I freely admit to a certain elitism, and work at an Ivy League institution (something there are problems with, but they don't have to do with the level of science and math done here).

My last comment trying to stop the flow of off-topic comments doesn't have seem to have succeeded, so I'll just start deleting such comments now. This may be elitist of me, but I'm not going to tolerate having the comment section here taken over by arguments so dumb that no informed person is going to read it or participate in it.

50. **Aaron Bergman**
June 9, 2007

Are you agreeing now that Smolin is not objectionable?

No.

51. **vicar**
June 9, 2007

Aaron said: "A common pastime at lunches in various string groups is to try to figure out why the papers of the day are wrong without reading more than the abstract :). This is a good thing."

Oh yes, that's a really good thing.
In reality, this sort of attitude is exactly what Smolin means by groupthink. You look at the abstracts and if they do not agree with your preconceptions you have a good laugh and move on to the next victim. The only papers you actually look at are those by names you recognise as being a member of the gang. I have heard some unbelievably naive things being said on the basis of this "let's just glance at the abstract and decide that the paper is wrong" procedure. This is essentially LM's approach, and it has led him to write some things that gave me a very bad case of vicarious embarrassment. As for example what he has said about equivalence principle violation over the years, things that would not be tolerated in an undergraduate in any respectable university.

52. **Aaron Bergman**
June 9, 2007

Here it was that I thought groupthink meant mindlessly agreeing with the status quo and not questioning things said within the group. Now, apparently disagreeing with papers counts as groupthink, too. I'm so confused.

(I hasten to add that the subject of such lunchtime conversations is most often string papers, but now, of course, string theorists will be accused of only looking at string papers and nothing else. Groupthink! And so it goes on….)

53. **mclaren**
June 9, 2007

Eric Mayes remarked:
There are many compelling reasons for introducing strings and it is far from being 'baseless conjecture'.

Compelling mathematical reasons. Not compelling experimental evidence. If I'm wrong, please provide the title, volume number, issue number and page numbers of the peer-reviewed HEP journal that has published experimental results confirming the predictions of string theory that would compel us to believe that string theory is the next necessary step in HEP.

This really sad thing about this debate is that there are so many ignorant people out there who are easily manipulated by these fallacious arguments.

Show me the evidence. That's all that counts. Show me the HEP experiments providing evidence for string theory. Everything else is irrelevant. The two biggest pieces of experimental evidence in HEP in th last 20 years, dark energy and oscillating neutrinos, weren't predicted by string theory.

If we're honest, you have to admit that what is being peddled is us-against-them -ism.

People are asking for hard experimental evidence. You can't have experimental evidence for or against a scientific theory if the theory doesn't even make testable predictions. Without testable predictions or experimental evidence pro or con, the "theory" is **not even wrong**.

In this picture, string theorist [sic] are elitists who live in their Ivy League ivory towers telling the common-folk who really no [sic] better what to believe, while the populist heroes Woit and Smolin are the defenders of rationally [sic] and common sense. The truth is that the serious people who are really doing physics will continue to study string theory because it offers the best chance to make progress, by far.

At some point, you have to recognize that a theory isn't making predictions and no one knows how to squeeze or tease it into making testable predictions. At that stage, the whole project has become what Hubert Dreyfus calls a degenerating research program. From the nonsensical philosophical arguments now being offered in favor of string theory and judging by its persistant inability to make testable falsifiable prediction, string conjecture seems to be a degenerating research program.

Eric Mayes' claim here is the same as Sean Carroll's faulty argument — string conjecture is valid science because physicists are still working away at it. That's faulty because it doesn't matter how many people work at an area of investigation if it produces no tangible results. Thousands of physicists worked worldwide investigating the ether in the 1920s but no results came from that line of investigation. It was a degenerating research program. Physicists eventually abandoned it.

It doesn't matter how many people investigate string theory if it continues to fail to produce testable falsifiable predictions. If you're not making predictions that can eventually be tested in a laboratory, you're not doing science. It's just that simple.

54. **CapitalistImperialistPig**
June 9, 2007

Peter,

You said *"My objection is to what I take to be Damour's claim that an observation of a violation of the equivalence principle would be evidence for string theory. If you believe Lubos, string theory predicts no violation of the equivalence principle, if you believe Damour, it predicts a violation. "*

Doesn't a dilaton violate the equivalence principle? I thought it had to be massive to confine the violation to short distances.

55. **CapitalistImperialistPig**
June 9, 2007

Aaron,

Your assertion that theoretical physicists should keep their disagreements private is pretty darn medieval. It's not in keeping with the history of physics or science generally, and it's dishonest to hide such disagreements from the people paying the bills.

56. **Aaron Bergman**
June 9, 2007

My god, does nobody read what I say? Or am I really that unclear?

Let me quote it again: *The other point people seem to have missed is the difference between criticizing one colleagues and criticizing their work.*

57. **Bee**
June 10, 2007

The other point people seem to have missed is the difference between criticizing one colleagues and criticizing their work.

hum… I wonder how the 'sociological' part of the book had been discussed had it not been written by a physicist but by someone working in, say, the sociology of sciences? I suspect, then the criticism would be it was *not* written by a 'colleague' who knows the field?

Hi Vicar,

In reality, this sort of attitude is exactly what Smolin means by groupthink. You look at the abstracts and if they do not agree with your preconceptions you have a good laugh and move on to the next victim.

nah, you're mixing up cognitive biases, what you're talking about is called confirmation bias:

`a tendency to search for or interpret new information in a way that confirms one's preconceptions and avoid information and interpretations which contradict prior beliefs. It is a type of cognitive bias and represents an error of inductive inference, or as a form of selection bias toward confirmation of the hypothesis under study or disconfirmation of an alternative hypothesis.'

and is btw also exactly what Polchinski criticizes about Lee's book *Regarding group-think: you interpret the reaction of string theorists to your book as more evidence for your point of view.*

Entertaining, eh? We can probably spend the rest of our days accusing each other of faulty thinky, or other logical and formal fallacies. I esp. like the Texas sharpshooter fallacy, anybody wants to write a book about it?

Sorry, Peter, couldn't resist – this comment section has just gotten too silly.

-B.

58. **Peter Woit**
June 10, 2007

Some of the most common automated spam robots operate by posting comments consisting of a generic phrase of indeterminate reference, or a snippet from a previous comment, followed by such a phrase. I'll just point out that the comments from some people here recently are indistinguishable from those of the spam robots (just that they don't have a link to a web-site trying to sell something in the "web-site" field).

59. **Peter Woit**
June 10, 2007

CIP,

In general, massless moduli fields can give violations of the equivalence principle. That's why some string theorists say "string theory predicts violations of the equivalence principle". But we don't see such long-range forces, so string theorists argue that the fields must be massive and short range, and concentrate recently on models where this is true. Some like Lubos, then start going on about how "string theory predicts no violations of the equivalence principle." The whole thing is kind of ridiculous.

US Particle Physics Planning

June 11, 2007
Categories: Uncategorized

Last week both SLAC and Fermilab hosted "Users Meetings", providing a forum to discuss the current status and future plans of the two laboratories. The SLAC agenda is here, and talks from previous years are available here, with this year's perhaps available later.

The Fermilab meeting was also celebrating the 40th anniversary of its first Users Meeting, which was held back in 1967 at a time when Fermilab was under construction, with plans for a 200 GeV fixed-target machine underway, led by director Robert Wilson. This year's talks are available here. The status of the Tevatron is described in Roger Dixon's talk. Already the machine has delivered nearly 3 fb^{-1} of luminosity to the two experiments there, half of this over the last year. They are projecting to have 6-7 fb^{-1} by the end of FY 2009 (a bit more than two years from now). The current plan calls for operation of the Tevatron only until the end of FY 2009, and a year or so ago there was even some discussion of shutting it down before then. With the machine operating well, a healthy US HEP budget, the LHC startup now not until 2008, and some cautious optimism that that the Tevatron might be able to accumulate enough data to see the Higgs under some scenarios, it looks like no one is about to shut the Tevatron down early, rather the question will be how much extra time to give it. There seems little point to shutting it down as long as the LHC is not producing results that make it obsolete, and no one knows yet how long that is going to take. While those running Fermilab would like to know what they will be doing several years in advance so that they can plan and budget, it may be difficult to do this since no one knows what will happen with the LHC.

Fermilab is in the middle of a long-range planning exercise, with a Steering Group meeting trying to put together a plan by August 1. They have many of their materials available on-line. Some of the discussion revolves around the question of the ILC, with talks showing that in principle it would be possible to start construction of the ILC in 2012 and have it built by 2019, but few people believe that things will happen this fast. Whether building the machine makes sense will depend on what is seen at the LHC. Other scenarios are under discussion, for example see here. Other than the LHC, the main things one could conceive of building at Fermilab would be a more intense proton beam (proton driver), or accelerating muons to provide a "neutrino factory" and perhaps ultimately a muon collider.

While US HEP has a difficult task ahead to figure out what to do after the Tevatron shuts down and the energy frontier moves to CERN, at least the budget situation is looking a lot better than it was a few years ago. At the Users Meeting, there was a presentation by the DOE's Robin Staffin showing budget figures that included a 6.8% increase planned for FY2008, after a 5.9% increase from FY2006 to FY2007. For some reason the federal government seems to have decided to put significantly more money into fundamental physics research, and HEP is benefiting from this. For more about the general situation with the Federal science research budget, see this recent talk by John Marburger, the director of Office of Science and Technology Policy.

For the budget situation in mathematics, see this report in the latest Notices of the AMS about the NSF budget numbers. After flat budget numbers for the past couple years, there was a 3.3% increase for mathematics research in FY2007, and the proposal for FY2008 has a 8.5% increase. Math is cheap compared to HEP, with the NSF spending on math (which is the bulk of federal math research funding) only about a quarter the size of the HEP budget. The AMS Notices article also computes numbers for what fraction of the NSF budget goes to different fields, noting that in FY2004 18.3% was for math, 20.9% for physics, while the FY2008 proposal goes 17.8% to math, 23.6% to physics.

Update: Also at SLAC, this week the DOE is there to review the lab. Presentations prepared for the DOE are on-line. Michael Peskin gave a presentation about the work of the theory group. He highlighted (besides hopes about the LHC) the work of SLAC's Lance Dixon on computing perturbative QCD amplitudes, including its relation to N=4 supersymmetric Yang-Mills and to the conjectural finiteness of N=8 supergravity.

Comments

1. **Eric**
   June 11, 2007

   Peter, Thurston is celebrating 60th birthday at Princeton
   http://www.math.princeton.edu/Thurston60th/

2. **DB**
   June 12, 2007

   Isn't it interesting that while the US is the undisputed world leader in string theory research, it has not led the way in experimental HEP since the 1970s.? Remember that the only reason the Tevatron is currently the world's most powerful collider is because CERN's LEP was shut down to build the LHC. Oh, and just before the LEP shutdown we were also told about events that "might" indicate that the Higgs had been seen there. MiniBoone was interesting, but a pale shadow of Kamiokande and Sudbury. By substituting "cheap" math for real physics, is it such a surprise that we end up with string theory?

3. **alex**
   June 12, 2007

   You say the NSF spending on math is only a quarter of the HEP budget, but go on to quote figures making the NSF budget for math not much smaller than for physics as a whole (17/18 % compared to 21/24 %). Are you talking about a different budget the second time?

4. **Peter Woit**
   June 12, 2007

   alex,

   Math is funded mainly by the NSF, and the NSF gives comparable amounts to math and physics. But HEP is mainly funded by DOE, and it is this funding that is much greater than math.

   DB,

   The US HEP problems can be traced back to the SSC debacle. I don't think that's something that can be blamed on the string theorists…

5. **Zathras**
   June 12, 2007

   Within physics, is HEP getting a smaller portion of the pie? Any other areas that are getting significantly more than they had before?

6. **Intellectually Curious**
   June 12, 2007

"By substituting "cheap" math for real physics, is it such a surprise that we end up with string theory?" -DB

Now, I understand why math is cheap, since one needs only paper and pencil (and usually a trash can) to do pure research. But what is "cheap math"?

7. **John H.**
   June 13, 2007

   Peter, with so much money being spent on HEP research, if neither LHC nor ILC provides new and meanigful data this may summon the end for particle physics… atleast for some time to come. Then what?! What will all those who devoted half of their lives (and sometimes more for senior researchers) do? What will they have to show for their lives' efforts…

   Just a thought…

8. **Coin**
   June 13, 2007

   Peter, with so much money being spent on HEP research, if neither LHC nor ILC provides new and meanigful data this may summon the end for particle physics

   Even if this is so, is it at all possible that this would be not the end of experimental particle physics, but just the end of collider experiments?

   I mean, if it were ever absolutely, truly necessary to do so, surely there's *some* way to do frontier fundamental particle research even if there's no funding for new ubercolliders.

9. **John H.**
   June 13, 2007

   Sure, HES [High-Energy Speculation]… 😉

10. **DB**
    June 13, 2007

    Experimental HEP research is increasingly using the astrophysics channel – which has a long and distinguished history of discovery from the muon (Anderson, 1936) to the charged pions (1947) to the neutrino oscillations at Kamiokande and Sudbury. Expect continued growth in the placing of sophisticated detectors in orbit – why the ISS is not being better exploited for this purpose is a mystery to me.
    The US remains the world leader in astrophysics with the infrastructure to design and deliver the best in class detectors so if, as I expect in maybe ten years time, non-terrestrial HEP once again becomes the dominant mode of particle physics research, it is perfectly placed to retake the lead. However, when I look at Nasa's funding priorities, its scaling back on its science budget, Europe and Japan's resolve, I just wonder.

11. **Peter Woit**
    June 13, 2007

    John,

    It's not the experimentalist's fault if it turns out Nature has decided to look exactly like the Standard Model up to 1 TeV or more. Finding out about Nature is what experimentalists do, and they have been and are doing it. Sometimes what they find out is dramatic and unexpected, sometimes not, but it's still valuable to know what actually happens. The fact that the Standard Model is so good is in itself a very interesting experimental result.

    What is worrisome about the post-LHC future is that, if the SM still holds, no one has a plausible idea about how to do what physicists have always done: keep trying to find out what happens at higher energy and shorter distance scales. The LHC will provide a jump of a factor of 7 in energy, but there's no affordable technology out there that could give us another such factor anytime soon.

12. **Matteo Martini**
    June 15, 2007

    Peter Woit wrote:
    " The LHC will provide a jump of a factor of 7 in energy, but there's no affordable technology out there that could give us another such factor anytime soon "

    What about the Super LHC?

    The maximum integrated luminosity increase of the existing options is about a factor of 4 higher than the to the LHC ultimate performance, unfortunately far below the LHC upgrade project's initial ambition of a factor of 10

13. **Matteo Martini**
    June 15, 2007

    Sorry,
    I did not write the sentence was quoted from Wikipedia:
    http://en.wikipedia.org/wiki/Super_Large_Hadron_Collider

14. **Thomas Larsson**
    June 16, 2007

    Matteo, SLHC gives higher luminosity, not higher energy. More collisions, but at the same energy.

15. **Matteo Martini**
    June 16, 2007

    Thomas,
    thanks for your reply.
    What about the VLHC?
    200TeV, instead of 7+7TeV?
    http://en.wikipedia.org/wiki/Very_Large_Hadron_Collider

16. **Peter Woit**
June 16, 2007

Matteo,

As the Wikipedia article mentions, the "VLHC" idea would require a machine hundreds of kilometers in size, and the cost would likely be prohibitive. There's little chance of such a thing being built within the next few decades.

17. **Matteo Martini**
June 16, 2007

Peter,
thanks for your comment.
I have read something the VLHC, in this file, http://vlhc.org/Limon_seminar.pdf, written by P. Limon, and, it is stated that:
1) the technology needed to build a VLHC, is available today; and
2) if we can afford a linear electron collider, we can afford a VLHC (do not understand the exact meaning of this sentence, though..)

What do you think?

18. **Peter Woit**
June 16, 2007

Matteo,

The problem with the VLHC is not the technology, but its size. It would be bigger than the failed SSC project. Given that the SSC was going to cost more (in constant dollars) than the ILC is supposed to, I suspect the VLHC would be significantly more costly than the ILC to construct.

19. **Matteo Martini**
June 16, 2007

OK
Let` s hope something completely new comes out from LHC, then.
Than, there will be maybe hope that the world` s governments could decide to go ahead, one day, with a bigger Hadron Collider.
Please, not that, last year the defense budget of the U. S. alone was over USD400 billions (http://www.whitehouse.gov/omb/budget/fy2007/defense.html)
Since the estimated cost of the ILC is around USD5 billions (http://www.linearcollider.org/pdf/RDR_Machine%20Overview_v5-1.pdf), I think there is still room, if there is any compelling reason for governments, to go ahead, and build a machine significantly larger than the ILC.
Note, that world powers did not have any problem to pop out some USD95 billions for the Space Station (http://www.space.com/news/spacestation/gao_iss_inquiry_010619.html).
I did not really read of any real practical outcome and scientific progress, from that expensive program.
Please, note that the figure I gave above (USD$400 billions+), is the budget of only 1 year, and, of only 1 country.
The cost of building any machine, such as the VLHC, would have to be split in many calendar years, therefore, reducing the cost per year to a fraction.
Also, collaboration and splitting costs between nations, would provide a further reduction of the expenses for each nation.
The bis point, in my opinion, is: will we really find something new next spring?
Will some theorist, come out with a brand new theory that requires a 50 TeV machine to be proven, but, that could change our lives and spur progress?
If yes, there could be reasons, in the near future, to work for an even bigger Collider.
If not, maybe, we will be stuck with the Standard Model for another 100 years.

20. **Nuttata**
June 17, 2007

>If not, maybe, we will be stuck with the Standard Model for another 100 years.

No reason to complain, it could have been much worse. Look at high Tc superconductivity, for example. Those guys have been stuck for more than twenty years, and they don't even have a theory. At least the SM does work.

But there's no way the LHC could possibly leave us entirely empty-handed. Some kind of EW symmetry breaking must be there, and/or some unitarity-restoring sector. "Nothing" is not in the menu, as soon as the experiment is up and running.

21. **Matteo Martini**
June 17, 2007

As far as I can see, human progress of the last 100 years, has been mainloy driven by breakthroughs in elementary physics.
The inventions of the:
– semiconductors (in the early 70s), which lead to the birth of the modern electronic and informatic business (Intel, AMD, Microsoft, Google and the modern IBM);
– the discovery of the DNA, in the early 50s (at the basis of genetics and modern biochemistry);
– laser optics (which lead to the development of current optical cables, and made it possible the birth of companies like Cisco systems and of the internet);
– the future quantum computing?
were, almost all, based on the discoveries of the behaviour of the atom, and, based on general relativity and quantum mechanics

If you look at the " old " industries (such as: avionics, rocket industries, car business, machines of all kinds), there has been nothing really new in these fields, in the last 40 years, at least.
The car engine, it has been the same in the last 100 years, and, after the man in the moon, in 1969, no big advancement in space exploration too.
Planes, are not inherentically new, after the invention of the jet engine (made in the sixties).
This is because, all the " old " mechanical industries, basically, use the 3 laws of physics, discovered by Newton in the late 1600s.

The same is happening with the " new " industries (informatics, semiconductor business, ..), there is still a lot of innovation, there, but, for how long??

In my opinion, if we do not advance, to the next level of knowledge of particle physics, human progress will probably, reach a halt in 20-30 years..

Just my opinion, though..

22. **sinus**
June 18, 2007

> The car engine, it has been the same in the last 100 years, and, after the man in the moon, in 1969, no big advancement in space exploration

I beg to disagree. It is true that the most basic ideas involved in the explosion engine have not changed. But today's cars, and their engines, are very different from those of 100 years ago. The technology involved in those differences is highly non trivial and, in many ways, revolutionary.

Regarding outer space exploration, we've been living a golden age of cosmology for more than ten years now. Cosmological and astrophysical knowledge has undergone many revolutions in the last two decades or so, due to enormous advances in space exploration which produced large amounts of high quality data. To quote but one example: several dozen extrasolar planets have been discovered, whereas none was known during the XX century.

23. **Matteo Martini**
June 18, 2007

Sinus wrote:
I beg to disagree. It is true that the most basic ideas involved in the explosion engine have not changed. But today's cars, and their engines, are very different from those of 100 years ago. The technology involved in those differences is highly non trivial and, in many ways, revolutionary

Matteo:
Yes, there have been many substantial innovations in the car engine, as well as in the commercial aviation business (look at the A380), but, the basic structure of the car engine, as well of that of the plane, has not changed in the last decades.
Current car engines use the Otto cycle, or the Diesel cycle, to provide energy, and, these cycles have been discovered more than 100 years ago.

Sinus wrote:
Regarding outer space exploration, we've been living a golden age of cosmology for more than ten years now.

Matteo:
I beg your pardon.
I meant human space exploration.
There is nothing revolutionary, in this field, since the man on the moon, dated 1969.
If we have to go to Mars today, we would still use rockets, and a lot of fuel.

Random Collection of Stuff

June 14, 2007
Categories: Uncategorized

Pretty much everybody in the math community seems to be getting a blog. Many of the new bloggers are quite good research mathematicians (including even some Fields Medalists). Two very new ones are:

Secret Blogging Seminar: Named after a “Secret Russian Seminar” at Berkeley, a group blog of several ex- and current Berkeley math graduate students (Ben Webster, A.J. Tolland, Scott Morrison, Noah Snyder and David Speyer)

Math Life: The blog of UT Austin’s number theorist Fernando Rodriguez Villegas

A few things I learned from the Secret Blogging Seminar postings and following associated links:

The Microsoft Research group at UCSB working on “topological quantum computation” is now known as Station Q, and has a web-site.

Googling “Secret Russian Seminar” led to the web-site of Scott Carnahan, a student of Richard Borcherds who will be a postdoc at MIT this fall. Carnahan has some interesting sets of notes there, including notes from Borcherds’ 2004 course on QFT. Back in 2001, Borcherds had taught an earlier version of this course, and notes taken by Alex Barnard are available. According to Carnahan, Borcherds began the 2004 course with the comment:

Some of you might remember I gave a class a few years ago on the standard model. It ran into a few technical problems, the main one being the fact that I didn’t know what I was talking about. I’ve learned a thing or two since then, and I’m going to try again.

I’ve finally been making some progress in understanding some mathematics associated to BRST; if this keeps making sense I hope to get something written about it this summer. I recently noticed that the pretty much incoherent Wikipedia entry on the BRST formalism, has been joined by another incoherent one on BRST quantization. Both entries contain the warning at the top “This article or section may be confusing or unclear for some readers”, which is an understatement.

A new issue of Symmetry Magazine is out, and it contains a report on the recent string theory debate in Washington between Brian Greene and Lawrence Krauss. An editorial noted how amazing it is that this sort of thing drew 600 people willing to pay $25. There’s lots of interest out there in fundamental physics in general, and this controversy in particular.

Can’t remember where I saw this earlier today, but there’s a famous quotation I hadn’t heard before from economist John Kenneth Galbraith which seems to apply well to the current situation in string theory:

Faced with the choice between changing one’s mind and proving there is no need to do so, almost everyone gets busy on the proof.

Update: One more. Adrian Cho at Science Magazine has an article about the debate going on over how long to run the Tevatron. The chair of the P5 panel is saying that they will recommend running through 2009, but that “It would take some unusual circumstances to justify running beyond 2009.” But, if the LHC takes longer to get working correctly than planned (there’s a history of this with new accelerators), and Tommaso’s Dorigo’s rumors of sightings of a Higgs at the Tevatron ever start to firm up, it’s going to be hard to justify starting to tear the machine down…

Update: Yet one more about math blogging. Lieven le Bruyn has changed his blog from NeverEndingBooks to Moonshine Math (also known as NeverEndingBooks, v. 2). He begins with a wonderful blog posting about the j-function which explains one of my favorite remarkable facts about numbers:

$$e^{\pi\sqrt{163}}=262537412640768743.99999999999925\ldots$$

Comments

1. **Aaron Bergman**
   June 14, 2007

   One thing that all this time on the internet has made clear to me is that physicists have done a pretty poor job of communicating that our understanding of QFT has changed. Renormalization is still communicated as this form of black magic that we do only because it gets the right answer. I would say that we now have a good understand of how it works. We even have a reasonable conjecture (see the previous discussion with Peter Orland) of how one might going about defining a quantum field theory via path integrals, and how the process of renormalization relates to that definition. Maybe someone should write a popular science book about effective field theory…

   On another note (again from reading the notes on Borcherds’s lectures), as I understand it, the idea of using the nonisomorphism of Pin(1,3) and Pin(3,1) to distinguish the signature of spacetime is wrong. There are eight ways (IIRC) to extend the Spin group to deal with the full orthogonal group of which two are the Pin groups. I don’t know of any physical reason to distinguish the various choices.

2. **anon.**
   June 14, 2007

   Regarding BRST: recently I was trying to understand the old paper by Thierry-Mieg that interprets the ghosts as vertical components of the connection on the principal bundle. The antighosts are still ad hoc. After this the literature appears to bifurcate in several directions, and I can’t tell which if any of them give a clear explanation of the full BRST structure with both ghosts and antighosts. Can you point me in the right direction? Is there one authoritative reference?

3. **Chris Oakley**
   June 15, 2007

   Aaron,

   I would love to be able to regard renormalization at something other than black magic, but it really does not help when someone of Borcherds’ standing comes out with a statement like this:

   > In general, renormalization is used to cancel out the terms you get from poles. This is a rather bizarre process by which terms are inserted that depend on the coupling constant, so that the resulting poles exactly cancel out the poles you get from the analytic continuation. The main justification for why it works is that the answers you get agree with experiment to some absurd number of significant figures.

(See the previously-linked lecture notes).
I nominate you to write this book on effective field theory. I will look forward to reviewing it on Amazon.

4. **matteoeo**
   June 15, 2007

   I think Michel Le Bellac "Des phénomènes critiques aux champs de jauge" (don't know the english name), part II (from chapter V on), is a great introduction to path integrals, renormalization and effective field theories ad could be studied in a QFT course (obiously also part I should be studied to undersand the statistical mechanical basis, but part II is pretty much independent).

5. **Thomas Larsson**
   June 15, 2007

   The canonical reference is [Henneaux and Teitelboim](#).

6. **Johan Couder**
   June 15, 2007

   Aaron Bergman says: "…Maybe someone should write a popular science book about effective field theory… "
   Chris Oakley says: "…I nominate you to write this book on effective field theory. I will look forward to reviewing it on Amazon."

   I second that. There is more than one book on quantum mechanics out there at an "intermediate level", i.e. in between the puerile pop science books and graduate level textbooks.
   But as for QFT ? Feynman wrote a nice book on QED for the math-disabled, and Schumm did his best to explain the Standard Model [at least better than Peter did 😉] to the lay person without using too many silly analogies (as in say Randall's "Warped passages"). Some of us laypersons have asked MacMahon to write a "QFT Demystified" book (he wrote fairly good ones on QM and GR.), but he seems more interested in doing a "String Theory Demystified" version.J :-))

7. **Peter Woit**
   June 15, 2007

   anon,

   Thomas is right that Henneaux-Teitelboim is about the best there is. But they don't have much to say about the geometrical interpretation of the formalism they are working with, that's what has always remained obscure to me.

8. **A quantum diaries survivor**
   June 15, 2007

   Hi Peter,

   please fix the link to Adrian Cho's article (need to remove one extra http). Curiously, he did not cite me in his piece although he interviewed me on the issue, while you do cite me in the same paragraph when you cite him. Your psychic powers at work ? :-°

   Cheers,
   T.

9. **Peter Woit**
   June 15, 2007

   Thanks Tommaso, fixed. I guess I do have psychic powers…

   This whole discussion about the Tevatron seems a bit odd. From Cho's article, the cost of running the thing is 4% of the US HEP budget. It's the only thing in the US producing HEP data at the high energy frontier, and it's not like anyone has an obviously better idea for an HEP experiment that is not getting funded. I'm not seeing why the plan is not to just keep running it, only starting to shut it down after the LHC has definitively started producing the kind of results that would make the Tevatron data of little to no interest.

10. **anon.**
    June 15, 2007

    Thomas, Peter: Thanks, I have heard of that book but I haven't obtained a copy yet. I am hoping to find something explicitly geometric, though.…

11. **A quantum diaries survivor**
    June 15, 2007

    Peter, I think the money issue is only part of the deal. By keeping the Tevatron alive, the US HEP program maintains that it is a good idea to invest on an alternative to LHC, while a lot of effort has been done and is currently ongoing to try and make the US participation in ATLAS and CMS as visible and clear as possible.

    There is thus a contradiction between keeping the Tevatron on the scene indefinitely and putting money and effort in the CERN endeavour. That is only embittered by the fact that scientists that could be useful at CERN are enticed into continuing to work at the Tevatron.

    From the outside, the choice of spending some 30-50M$ a year for a few more years of Tevatron running beyond 2009 is a no brainer – there are even things that the Tevatron is better at doing than the LHC at full power. But from the point of view of dictating the strategy of US HEP, it looks like the decision is a bit tougher.

    Cheers,
    T.

12. **Hendrik**
    June 15, 2007

    Dear Peter and Thomas, concerning BRST, a few remarks;-

    The classical (geometric) version of BRST is mathematically in good shape (cf. e.g. Stasheff arXiv:hep-th/9112002v1) and even connects with quantum theory through geometric quantization, cf. Duval e.a. in Commun. Math. Phys. 126, 535-557 (1990).

However a purely quantum BRST constraint reduction procedure
cannot rely on the classical theory (due to the problems with quantization maps – check out Gotay e.a. on Groenewald-Van Hove obstructions). For quantum BRST the theory is in much worse shape, with only a few sporadic rigorous papers, e.g. by Horuzhy.

There is not one BRST in method in heuristic physics, but several.
Broadly speaking, there is the Hamiltonian version (as in the book by Henneaux and Teitelboim or in the Russian BFV school), and the Lagrangian version (as in the papers by Kugo and Ojima), and these come in several different flavours.
The Hamiltonian version starts from a set of quantum constraints and give an algorithm for constructiong the BRST charge Q, whereas the Lagrangian version starts from a set of gauge transformations and "replaces" the gauge parameters by anticommuting ghost fields to obtain the BRST graded derivation d.

Whilst on the surface these two methods seem quite similar, (with an obvious connection in d = graded commutator with Q) there are subtle differences in the actual constructions involved. The Hamiltonian version is the more problematic one of the two;- it is quite easy to produce quantum constraint systems where Hamiltonian BRST produces different results from the usual Dirac method, or is inconsistent, e.g. by producing a nonpositive physical space, or the wrong physical observables. This has been noted in a number of places in the heuristic literature, but usually an ad hoc "fudge" is invoked to get out of such tight spots. In the rigorous literature the failure of BRST has also been noticed, cf. e.g. Landsman and Linden, Nuclear Physics B Vol 371, 415-433 (1992) or McMullan, Commun. Math. Phys. Vol 149(1), 161-174 (1992). I also have an (unpublished) preprint on the breakdown of Hamiltonian BRST which I can email to you if you're interested.

On the other hand, the Lagrangian BRST is applied almost exclusively to gauge theories (i.e. where there is a gauge potential, not just a nonphysical degree of freedom), and closely follow Kugo and Ojima. A good example is e.g. the book by Scharf "Quantum Gauge Theories: A True Ghost Story" where their threatment of QEM can be made rigorous, and it produces the correct results. Whether Lagrangian BRST can be made into a general quantum constraint reduction procedure still remains to be seen (but I have not followed the recent literature on BRST so might have missed it).

I am aware that the real appeal for BRST comes from the path integral approach, but from the operator point of view I still cannot see how quantum BRST is an improvement over a simple Dirac constraint approach. Unnecessary and problematic formalism.

13. **Hendrik**
June 15, 2007

Dear anon, I think that Bonora and Cotta-Ramusino in Commun Math Phys, 87(4), 589-603 claim to have an improved version of Thierry-Mieg's paper on BRST. This is fully geometric (hence classical).

14. **Scott Carnahan**
June 15, 2007

It's a bit surprising to see that those QFT notes got publicity – I had mostly intended them for fellow grad students who didn't feel like taking notes. Most of the comments that I took down (e.g. Allen Knutsen's remark about Pin groups) were basically off-the-cuff, so they should be taken with a grain of salt.

I should mention that I don't think Borcherds' ending comment about renormalization is completely representative of his viewpoint, and he may have been just tired and cynical. He is aware of effective field theories (and they are mentioned in the 2001 notes), and he has said several times that all of the ingredients for making perturbative quantum field theory (and in particular renormalization) mathematically rigorous already exist in the published literature, but that writing them down together is highly nontrivial.

Tao's blog entry on the Fields medalist lectures has a shorter but more recent representation of his work.

15. **theoreticalminimum**
June 16, 2007

For anyone who wants to have a look at the "Quantization of Gauge Systems" book, a djvu copy can be downloaded here.

16. **Chris Oakley**
June 16, 2007

Scott,

Borcherds' comment about renormalization may have been unguarded, but that does not mean that it did not accurately reflect what he thought. The argument about rigour is one that no-one ought to have. The rigour of a scientific theory one ought to be able to take for granted. Certain assumptions lead to certain consequences. The consequences may not be in accordance with experiments, but there should never be an issue about whether they connect with the underlying assumptions or not. Can you imagine a distinction being made between axiomatic and non-axiomatic classical mechanics? I once got an e-mail from an "axiomatic" field theory post-doc saying that I was trying to be "holier than the pope" in requiring rigour in quantum field theory, but why am I being unreasonable? Is there any mathematical cheating going on in going from Newton's universal law of gravitation and the derivation of elliptical planetary orbits obeying Kepler's laws, or between Maxwell's equations and the calculation of the force between two current-carrying wires? Why should there be two sets of standards, one for QFT and one for everything else?

17. **Thomas Larsson**
June 16, 2007

Hendrik, I would be interested in your preprint. You can find contact information on the arxiv.

When you talk about problems, is it the BRST method that breaks down, or do people make mistakes when applying it? One need to honor certain regularity conditions, but it is nevertheless possible to make subtle errors. E.g., thm 17.2 of H&T is flawed already for the harmonic oscillator, because it only works as stated if you say that the oscillator has a gauge symmetry. The reason is that there are "Noether identities" of the form (17.8) due to the fact that the equations of motion has solutions. The difference is that the index &alpha runs over a (d-1)-D manifold for solutions, and over a d-D manifold for genuine gauge symmetries.

18. **Hendrik**
June 16, 2007

Dear Thomas, expect my email on Monday. Regarding:

“When you talk about problems, is it the BRST method that breaks down, or do people make mistakes when applying it?”

I do mean the stronger statement;- if you take Hamiltonian BRST (as expressed in the book by H&T) seriously as a constraint algorithm, then it fails when applied to some simple constraint systems. To get the right answer additional assumptions are necessary (which of course means that you know the right answer by other means).

The input of a quantum constraint system is simple,- it is a *-algebra A of operators (on Hilbert or Krein space with some common dense domain), together with a set C of distinguished elements which you want to set to zero (constraints). It is easy to generate such pairs (A,C) for which the Hamiltonian BRST fails.
Dynamics (i.e. a one-parameter automorphism group of A) is independent from this;- just assume a dynamics which preserves the constraints. In any case Hamiltonian BRST is a kinematic procedure, dynamics adjustment is not part of it.

It is not even clear to me that if you start from equivalent constraints (i.e. they produce the same physical state space) that Hamiltonian BRST will produce the same physical algebra.

I have to emphasize again that Lagrangian BRST (which is closer to the classical geometric version) is not a general constraint algorithm, it is just for gauge theories. For these (at least the ones which can be analyzed rigorously) it seems to produce the right result.

19. **Johan Couder**
June 18, 2007

for those who thought i was kidding … McGraw-Hill is releasing David MacMahon’s “String Theory Demystified” on December 24, 2007. (search for the title at McGraw-Hill’s website if you like)
Apparently: “Using the proven Demystified format, this book elucidates the highly mathematical and complex topic of string theory, covering key topics such as particle physics, quantum field theory, D-brane physics, and different types of strings. YOu will learn the mathematical tools necessary to truly decipher string theory.”
How mainstream can you get ? Eagerly awaiting the “ST for Dummies book” 🙂

20. **anon.**
June 18, 2007

> David MacMahon’s “String Theory Demystified” on December 24, 2007.

Brilliant. I’m writing Santa for a copy for the book as part of my next Christmas present, plus a sexy supersymmetric partner and hopefully also an extra dimension (just to the new supersymmetric partner and I achieve perfect unification at high energy). 😉

21. **Chris Oakley**
June 18, 2007

Achieving perfect unification with your supersymmetric partner on an uncomfortable Planck scale must be very hard.

22. **Prestito**
June 18, 2007

Well, really a nice idea make a blog about this argument. My best compliment about that.

23. **Matthew**
June 18, 2007

“Is there any mathematical cheating going on in going […] between Maxwell’s equations and the calculation of the force between two current-carrying wires?”

As a matter of fact, there is. It’s a bit hazy in my mind, but I do know there’s all sorts of problems in defining various “fundamental” things in classical E&M. For example, the radiation-reaction problem. Fuzzy things like pre-acceleration.

All the same sorts of problems that show up in QFT as a matter of fact. It’s just that, as Aaron pointed out, we know a lot more about their origin and how to solve them.

24. **anon.**
June 19, 2007

It’s a bit hazy in my mind, but I do know there’s all sorts of problems in defining various “fundamental” things in classical E&M.

Put current into a transmission line (pair of wires) by connecting them to a battery, and you get a continuous flat-topped logic pulse propagating along the transmission line at light speed for the insulator.

This violated Ampere’s law of circuits because the current pulse doesn’t know in advance if there is a complete circuit at the far end of the line, or an open circuit.

Maxwell’s whole genius was adding an ‘extra current’ to Ampere’s law which can flow across space between the two wires (even across a vacuum), completing the circuit while a transient flows into an open circuit!

What happens when you do the experiment with sampling oscilloscopes is you find that the energy reflects back from the far end of the transmission line. If it’s an open circuit at the far end, the reflected current adds to the energy flowing in, so the transmission line charges up, a little like a capacitor.

All the same sorts of problems that show up in QFT as a matter of fact.

Maxwell’s extra current was supposed to be due to the displacement of virtual fermions in the vacuum, which polarize in an electric field. The vacuum ‘displacement current’ consequently flows in direct proportion to the rate of change of the electric field, dE/dt.

Nice theory, and it predicts light. Problem is, QFT involves a vacuum polarization due to pair production of virtual fermions, only at high energy (above Schwinger’s electric field strength threshold for pair production, or the IR cutoff energy for particle scatter). So below the IR cutoff, Maxwell’s displacement current mechanism is in difficulty. However, the correction is easy to see: electrons are accelerated by electric fields in the conductors, so they radiate transversely. Each conductor behaves as an antenna radiating an inverted version of the radio signal from the other one. At large distances from the power line, the superimposed radio signals cancel out perfectly. The conductors are therefore just swapping this radio energy, and the resulting effect of the swap is equivalent to having a ‘displacement current’. So you still justify the Maxwell equations when you dig deeply, though his original theory is wrong.

25. **Peter Woit**
June 19, 2007

Enough about E and M, this is completely off topic.

26. **Scott Carnahan**
June 19, 2007

Chris,

I think mathematical rigor in a physical theory is an admirable thing, but history seems to indicate that it is not strictly necessary for progress. In particular, your example of celestial mechanics illustrates that people like Newton could make useful physical predictions using the theory of fluxions without the precision of epsilons and deltas that appeared two hundred years later. For another example, much of early quantum mechanics used objects like the Dirac delta, which was not made mathematically precise until Schwartz formulated the theory of distributions in the late forties. Quantum field theory is currently in the intermediate stage, like Kepler's laws in the early 1800s.

I don't think this sort of work should be rejected for a lack of rigor, because in the end, physicists are communicating ideas and calculational heuristics to other humans, not a proof-verifying computer. If an idea allows us to build a simpler mental picture of a process, or a heuristic consistently yields answers that agree with experiment, I think it increases our understanding of the universe, and some sloppiness should be tolerated. It is also occasionally useful for mathematicians who require rigor to have some bold theoretical leaps charted by people who aren't burdened by such restrictions.

27. **Intellectually Curious**
June 19, 2007

Scott,

If I understand your comments correctly, you seem to suggest that mathematicians are "proof-verifying computers," and that they should occasionally make some "bold theoretical leaps." But mathematical proofs require imagination and creativity as well as rigor in thought, and mathematicians are far from the automatons that you seem to depict they are. They are more like poets in this regard. And in order to do creative research, they do have to take bold theoretical leaps sometimes.

Arghh, I guess even some physicists (assuming you are one) misunderstand the nature of the mathematician's work. Well, at least you didn't think – as many members of the public do – that their work involves crunching numbers all day long. 🙂

28. **Chris Oakley**
June 19, 2007

Scott,

The important thing is that, when have a botch-up, you recognise it as such. The problem is that most QFT books & lecture courses sell renormalization like dishonest salesmen, harping on the miraculous achievements and hardly drawing attention to the caveats (such as my comment #461 here).

29. **Hendrik**
June 19, 2007

Peter, following on from your quote:

"Faced with the choice between changing one's mind and proving there is no need to do so, almost everyone gets busy on the proof."

There is a recent article in "Reason" supporting this exact point: http://www.reason.com/news/show/120455.html

30. **Peter Woit**
June 19, 2007

Hi Hendrik,

Thanks for pointing to the "Reason" article.

Also, thanks a lot for the comments on BRST, quite helpful and interesting. I think you're right that Hamiltonian BRST has a lot of problems with it, and in its present state is a very unsatisfactory formalism.

It's closely related to Lie algebra cohomology, and there's the same philosophical issue there: why not just look at the invariant part of a representation, what's the point of looking at a complex, whose 0-cohomology is the invariant piece you want?. Anyway, Lie algebra cohomology has had some real uses in representation theory, I've been trying to understand those for quite a while, only recently have some promising ideas that seem to connect to BRST, we'll see what that leads to.

Mathematicians have some really interesting theorems under the slogan of "quantization commutes with reduction", but the connection of this to BRST remains unclear.

31. **emyl**
June 19, 2007

>"quantization commutes with reduction"

but that is not true, as far as I know. At least, not in gauge theories.

32. **Peter Woit**
June 19, 2007

emyl,

Not sure what you have in mind. I was being too telegraphic perhaps, here's a survey article about exactly what I meant:

http://www.ams.org/bull/1996-33-03/S0273-0979-96-00661-1/S0273-0979-96-00661-1.pdf

33. **emyl**
June 20, 2007

peter,

thanks for the reference, looks quite interesting. What I had in mind were some examples quite similar to the one given in that paper after the statement of the central “theorem”, as they call it. Regards.

The Triple-Scoop

June 15, 2007
Categories: Uncategorized

Experimenters at the Tevatron have released data showing the existence of a new baryon, built out of a down, strange and bottom quark, and having a mass of 5.774 +/- .019 GeV. It has been given the name "Ξ_b". The FNAL press release is referring to this as a "triple-scoop" baryon, since it contains one quark from each of the three generations. Tommaso Dorigo has a new posting about this.

Postings on this blog tend to involve a certain amount of discussion of bad behavior by theorists, so I guess I should point out here that this story comes with some accusations of less than good behavior by experimentalists in the way this was announced. The press release is all about the announcement of this discovery by the D0 experiment, and about their paper, which was submitted to the arXiv Tuesday night. It turns out though that D0's competition, CDF, also has had the same result for a while, and it had already been "blessed" but not officially announced. So CDF was "scooped" at the last moment for the "triple-scoop", and I imagine there are dark accusations going around about how D0 might have found out about this and rushed out the paper and press release. Supposedly there is an informal one-week notification period that the two experiments normally observe to keep this kind of thing from happening. Anyway, looks like they're trying to fix it up a bit: the FNAL web-site contains a "special announcement" of back-to-back seminars about this, D0 at 1pm, which will "immediately be followed" by a CDF seminar. The D0 seminar is called

First observation of a new b-baryon, Ξ_b

whereas the CDF seminar is entitled

Observation of a new b-baryon, Ξ_b

Comments

1. **Chris**
   June 15, 2007

   It should be down, strange, and bottom quark.

   Best regards,
   Chris

2. **Peter Orland**
   June 15, 2007

   I remember similar behavior concerning the upper bound on
   the number of light neutrinos in the late 80's. The SLC (sort of
   the ILC scheme, except the two beams came from the same machine)
   at SLAC had a very small number of events (I think less than 30). Some people at Fermilab were also working at the SLC detector (I forgot the official name) and made sure to get their results out first. Shortly
   therafter the SLC was dismantled.

3. **Peter Woit**
   June 15, 2007

   Chris,

   Thanks! fixed.

4. **Coin**
   June 15, 2007

   I guess I'm trying to figure out exactly what the result or relevance of finding this particular particle was. I think I get what they found, and I get that it's historically impressive, but I'm not sure what it means for physics.

   Is the idea that the fact this particle exists is a surprise, and gives theorists some new work in explaining how it's possible? Or was this something they were hoping or planning to find, which validates a prediction of some previous bit of theory? Is the idea that now that this weird crossgenerational baryon has been found, they can do some experiments to explore its properties and thus maybe learn something new about the exact nature of the mysterious quark generations? Or is the spirit just "Golly gee gosh, look at this weird-looking thing that we got to come out of the accelerator"? (Note that I'm *not* saying this last possibility is at all a bad use of research time.)

   Also, does the existence of this thing mean they have to modify any of those nonets or decuplets or whatever that they use to classify hadrons?

5. **stefan**
   June 15, 2007

   Coin,

   the place of this "cascade-bottom" in the baryon multiplets has been known since long – see at Physics News Graphics for a graphical representation of the respective multiplet:

   http://www.aip.org/png/2006/270.htm

   I would be surprised if anything had to be modified in this scheme. The point is, of course, that most of these baryons have not been observed yet, although probably no one doubts that they exist.

   Interestingly, the Particle Data Group already lists the Cascade-bottom, if only as a "1-star" particle (meaning "Evidence of existence is poor" – see pdg.lbl.gov/2007/listings/s060.pdf). These first hints at its existence were found about ten years ago in the DELPHI and ALEPH experiments at CERN, but there seems to have been no mass determination at that time. It would be interesting to know how the Fermilab results fit to these earlier data.

I don't know if this is possible in practice, but it may be interesting to compare the now measured mass of the Xi_b with lattice calculations – for the bottom-charm meson B_c, there has been a quite spectacular agreement between lattice results and experimental data (see e.g. Physics News Update 731, May 2005). Moreover, all kinds of quark potential models used to describe the properties of hadrons work best for heavy quarks such as the bottom. So, it may be possible to check some predictions of these models against experimental data.

6. **ObsessiveMathsFreak**
June 15, 2007

To paraphrase Tom Lehrer

> And then I write
> By morning, night,
> And afternoon,
> And pretty soon
> My name in Dnepropetrovsk Fermilab is cursed,
> When he finds out I published first!

Although, D0 is at Fermilab too….

7. **Coin**
June 15, 2007

Stefan, thanks!

8. **csrster**
June 20, 2007

Hmm. Triple-scoop. Must have ice-cream.

9. **andy**
June 20, 2007

I hope that you tolerate an off topic post, Professor Woit.
Someone claiming to be Lubos Motl says that he is leaving science. In the comment area on Tommaso Dorigo's blog
http://dorigo.wordpress.com/2007/06/19/a-small-peter-woit/
I apologize if it turns out to be a hoax. I think that I have always been surprised by the vitriol from this man. But at the same time I think that the criticism against string theorists should have always sound more like "you thought outside the box on a tough problem but you are a valuable scientist and you should move on to another approach" and less like "we think you are an idiot."

10. **Peter Woit**
June 20, 2007

andy,

Lubos has been saying for a while that he was leaving Harvard after this academic year, and I gather that that's what's happening. I don't know what he intends to do, but perhaps he'll keep doing physics, just not at Harvard.

11. **A quantum diaries survivor**
June 21, 2007

Yes, Lubos is "retiring", but he will continue to think about science. And I bet a dime or two he'll continue blogging about it too!

T.

12. **Jim**
June 21, 2007

So does this mean that Lubos was denied tenure at Harvard?

13. **woit**
June 22, 2007

Jim,

Lubos is leaving a tenure-track job at Harvard, but he hasn't been there long enough to have reached the point where they would have to decide about tenure.

14. **JC**
June 22, 2007

Peter,

I thought assistant professor jobs at Harvard were NOT tenure-track? (ie. To get tenure at places like Harvard, one has to actually apply and compete for a separate tenured job opening? At other less prestigious places, it's largely an administrative change of job title after a committee review.)

15. **woit**
June 22, 2007

JC,

I don't know exactly how things work at Harvard, but I was under the impression that assistant professor position's like Lubos's are nominally tenure-track, although mostly they don't lead to tenure, and people who accept them are well-aware of this. There are several highly prestigious places in the US like Harvard where most senior hiring is done from outside, but typically there is a tenure-track system in place, even if most people in it don't actually get tenure.

Assorted News

June 22, 2007
Categories: Uncategorized

Yesterday Sean Carroll and I appeared on the BBC Radio 4 program The Material World, in a segment on String theory – knot good enough?, about the controversy over string theory. The segment started with a piece from the play Humble Boy by Charlotte Jones, in which the main character is working on string theory. I don't think anything either of us said was particularly controversial or would be in any way surprising to a regular reader of either of our blogs. The same program had a segment on the multiverse three weeks ago.

Some more titles of talks at next week's Strings 07 have appeared. Witten's talk is entitled "Three-Dimensional Gravity Revisited" and presumably will be about the new ideas described here. So, at least one talk there will be about a non-string theory approach to quantum gravity more along the lines of the LQG program. The schedule doesn't seem to include any discussion session like the one at Strings 05 where the audience voted against the anthropic landscape.

A competing conference to Strings 07, Loops 07 will be taking place at the same time next week, but in Mexico, not Madrid. It's much smaller, with less than a third as many participants. There will be one plenary talk on string theory, Moshe Roszali speaking on "Background Independence in String Theory".

As the hunt for the Higgs is heating up at Fermilab, and CERN has officially announced the delay of LHC startup until next May, there's a group of filmmakers who may be well-positioned if something exciting is found at the Tevatron. For the last few years 137 Films has been making a film to be called The Atom Smashers, following scientists working at FNAL. Filmmaker Clayton Brown is keeping a blog about this.

Yet another bogus "possible signature for string theory". Even Lubos doesn't seem to believe this one, so I'll just quote his argument:

My personal guess based on our work on the weak gravity conjecture is that the black hole bound is also satisfied in string theory for localized macroscopic objects, up to small corrections. This belief of mine is supported by the observation that Gimon & Hořava don't have any explicit solution for their "superspinar".

Christina Sormani tells me she has created a Wikipedia article on the proof of the Poincare conjecture, see here. For the latest on Perelman, see here.

Comments

1. **anon.**
   June 22, 2007

   'String Theory – knot good enough?

   'Radio 4 Drama's science season continues on Saturday with Charlotte Jones' play Humble Boy. In it, a young physicist turns to String Theory in an attempt to unite the irreconcilable in his life. But is he doomed to failure?'

   Sounds fairly realistic … will have to listen in tomorrow.

2. **Michael**
   June 22, 2007

   hehehe

   http://xkcd.com/c171.html

3. **Aaron Bergman**
   June 22, 2007

   So, at least one talk there will be about a non-string theory approach to quantum gravity more along the lines of the LQG program.

   ???

4. **Peter Woit**
   June 22, 2007

   Aaron,

   If Witten is talking about the same thing I heard him talk about, it's an approach to quantum gravity that involves non-perturbative quantization of the 3d version of Ashtekar variables, no strings anywhere to be seen.

5. **Aaron Bergman**
   June 22, 2007

   I think it's unseemly to speculate on what other people are working on, so I probably should just be quiet. I'll just say that it's silly to frame this as a LQG vs. string thing. Witten invented the Chern-Simons quatization of 2+1D gravity way back when.

6. **Christine**
   June 23, 2007

   For the latest on Perelman, see here.

   Well, I know next to nothing of the russian language, but tried one of those online translators, and giving some discounts to the usually terrible literal translations and other funny effects, I did not like what I read. Ridiculous.

7. **AGeek**
   June 23, 2007

   Tried Babelfish. They are making fun of him for riding the subway in sneakers rather than driving a Lamborghini in Guccis, after he turned down a million dollar reward?

How profound.

8. **anon.**
June 23, 2007

[Babelfish translation]

9. **Peter Woit**
June 23, 2007

Aaron,

The LQG comparison may be silly (but only very slightly so…), but I think it's remarkable that for the second year in a row Witten's talk at Strings XXXX will not be about string theory. And, I strongly suspect his talk will be by far the one with the most interesting new ideas.

10. **Kris Krogh**
June 23, 2007

Peter,

Your Perelman link — how would you feel if people followed you with cell phone cameras and posted it to the web?

11. **Peter Woit**
June 23, 2007

Kris,

I don't think it would bother me, if it was a rare event. A lot of it would certainly get annoying. Perelman may feel quite differently, I have no idea. I couldn't read the Russian comments, and if they're stupid and disrespectful, that's a shame.

12. **Viri**
June 23, 2007

[in English via google language tools]

Can someone who actually knows russian translate this?

13. **Eugene Stefanovich**
June 23, 2007

Here is my try at translation of the Perelman "news".

Almost forgot. Recently I met Perelman in St. Petersburg subway. Yes, that same crazy scientist who proved the Poincare conjecture. They wanted to give him Fields medal and 1 million bucks, but he told everybody to fuck off.

He boarded at subway station Kupchino and left at Victory Park. During that time I discreetly took pictures of him on my cell phone. The picture quality is bad, but that's all I have.

He looked like an accomplished tramp; he also had a huge fingernail on his little finger that he used to constantly probe in his mouth. Terrible. Nobody recognized him except myself, that was somewhat strange. However, my surprise disappeared when I tried to tell about this encounter to my acquaintances: less than one out of five heard anything about Perelman. Although he was shown on TV many times.

14. **Aaron Bergman**
June 23, 2007

How'z'bout we wait for the talk and then see what's in it?

15. **Pavel Krapivsky**
June 23, 2007

Here is a translation of a note about Perelman:

I almost forgot! A few days ago I met Perelman in St. Petersburg's metro. Yes, that crazy scientist who proved Poincare conjecture. They wanted to give him a Fields medal and 1,000,000$, but he said — go fuck yourself. So Perelman got in on "Kupchino " station and he got off on "Park of Victory" and all the time when he was in the car I was recording him on my phone. Quality is poor, but it is what it is.

He looks like a homeless. He has a very long nail on his pinky, and he pecked in his mouth with this nail. Awful. Apart from me, nobody recognised him which was sort of odd. Yet I am not surprized anymore — when I described the story to my friends, at most one out of five ever heard about Perelman. Although there were many programs about him on TV.
%%%

Overall, the author uses quite illiterate Russian, and I am not surprised that it is hard to translate mechanistically. The following comments are totally useless, and many are indeed stupid and stinky, so after reading such comments one may want to take a shower.

16. **Peter Woit**
June 23, 2007

Aaron,

Well, I heard him give a talk with pretty much the same title a few weeks ago here in New York. During the talk he brought up the question of LQG, noting that in 3d you could covariantly express gravity in terms of a gauge theory, that the way this was done in 4d (LQG) was non-covariant. I'm not sure I really understand this point, maybe he will elaborate at Strings or in a paper.

In the question session afterwards someone asked him about whether what he was doing could be embedded in string theory. His only answer was to note that recent arguments claiming to show that you can't decouple supergravity completely from string theory in a limit failed in 3d, implying that even if you embedded what he was doing in string theory, it could be completely decoupled from the string theory. There was nothing at all about string theory in his talk. Perhaps he has come up with an important connection to string theory in the last few weeks, perhaps there is some less important connection to string theory which he'll mention in Madrid but didn't mention in New York. But, as far as one could tell from his talk here, the important new idea he has about 3d gravity doesn't have anything to do with string theory.

17. **Richard**
June 23, 2007

Not even having the benefit of the translation yesterday, I thought that these pictures and the picture taker — not Perelman himself — were a bit creepy.

18. **gunpowder&noodles**
June 24, 2007

I wonder who decides the list of invited speakers. There are some pretty odd choices there — some people whose recent papers have hardly been cited, and others whose talks are clearly going to be re-runs of talks given many times before……the only novel-looking talks are those of Verde and Vafa.

19. **Chris Oakley**
June 24, 2007

Er … Pavel, what was wrong with Eugene's translation?

To tell the truth, I am a bit disappointed with Perelman. I was hoping that he would be wearing a T-shirt with "F*** the IMU" on it.

20. **Viri**
June 24, 2007

«Er … Pavel, what was wrong with Eugene's translation?»

I think Pavel meant that the article's author used "illiterate Russian" not Pavel and thats why machine translations of it suck even more than usual. (seems clear to me)

Regarding the 1 in 5 ratio who knew of Perelman, I bet all of them know who Paris Hilton is. She achieved so much after all…

21. **Chris Oakley**
June 24, 2007

Eugene is a Russian speaker!

22. **Peter Woit**
June 24, 2007

gunpowder,

I presume the speakers are chosen by the "International Advisory Committee"

http://gesalerico.ft.uam.es/strings07/020_organization07_contents/021_international_ac.htm

Have you considered that the very small number of talks that sound like they'll have novel ideas might be due to something other than the advisory committee not doing the best possible job of coming up with speakers?

23. **Aaron Bergman**
June 24, 2007

Well, the paper is out, and it's about finding dual CFTs for 3D gravitational systems. So, I suppose it depends on whether you call AdS/CFT string theory or not.

24. **LDM**
June 24, 2007

The Pavel Krapivsky and Eugene Stefanovich translations are correct… with Eugene capturing just a little more correctly (for examples Eugene gives the translation "bucks", which is what the author wrote, whereas Pavel gives "$", normally understood as dollars, which is not what was written.

However, both I think did not capture the true flavor of the obscenity, because while there are a variety of ways to say "**** you" in Russian, the one chosen was one of the strongest…so strong in fact that some Russians will not utter it and will prefer a euphemistic, watered down form. It is also doubtful that a machine translation would get the obscenity, because нахуй correctly written should be two words, на хуй…which I will not translate here for reasons of decorum.

What is NOT correct is the statement that Perelman appears often on Russian TV. I get 16 channels of Russian and Ukrainian TV, and have yet to see Perelman on the news.

25. **Peter Woit**
June 24, 2007

Aaron,

Invoking the letters AdS/CFT doesn't make it string theory. This isn't string/gauge duality since there are no strings on the AdS side.

People have looked a lot at exactly this AdS_3/CFT_2 correspondence, trying to relate string theory on AdS_3 to 2d CFT. This gets very complicated and ugly from what I can tell. What Witten has done is throw out the strings on the AdS side, making things much simpler, and it seems, much more interesting.

26. **Aaron Bergman**
June 24, 2007

Strangely, most string theorists don't feel the need to live in such pigeonholes.

27. **Kea**
June 24, 2007

On page 37, Witten does mention the possibility of embedding this into a string theory.

28. **gunpowder&noodles**
June 24, 2007

“Have you considered that the very small number of talks that sound like they’ll have novel ideas might be due to something other than the advisory committee not doing the best possible job of coming up with speakers?”

I really wouldn’t presume to judge that. What I do know is that, if I were on that committee, I would find some diplomatic way of telling the invitees, in the strongest possible terms, that we don’t want to see the nth re-hash of a talk claiming that eternal inflation is absolutely guaranteed to work, that 3-dimensional gravity is really cool despite the abundant evidence that it is utterly unlike 4-d gravity, etc etc etc.

29. **M**
June 25, 2007

sorry Peter: I stopped after reading the 1st sentence of the abstract, because it looks one more paper that studies irrelevant physics in order to find interesting mathematics.

Is this recent theoretical trend healthy for physics? Please search “FIND A WITTEN AND TOPCITE 500+” on SPIRES: how many of these 46 super-papers should an experimentalist read? And what about other theorists that do Witten-style research without being Witten?

30. **joe**
June 25, 2007

There was a New Yorker article about Perelman, Yau & others pursuing the Poincare Conjecture proof:

http://www.newyorker.com/archive/2006/08/28/060828fa_fact2?currentPage=10
http://www.newyorker.com/archive/2006/08/28/060828fa_fact2?currentPage=11

> … and said that he was dismayed by the discipline’s lax ethics “It is not people who break ethical standards who are regarded as aliens,” he said. “It is people like me who are isolated.”
> …
> As for Yau, Perelman said, “I can’t say I’m outraged. Other people do worse. Of course, there are many mathematicians who are more or less honest. But almost all of them are conformists. They are more or less honest, but they tolerate those who are not honest.”
> …
> The prospect of being awarded a Fields Medal had forced him to make a complete break with his profession. “As long as I was not conspicuous, I had a choice,” Perelman explained. “Either to make some ugly thing”—a fuss about the math community’s lack of integrity—“or, if I didn’t do this kind of thing, to be treated as a pet. Now, when I become a very conspicuous person, I cannot stay a pet and say nothing. **That is why I had to quit.”** We asked Perelman whether, by refusing the Fields and withdrawing from his profession, he was eliminating any possibility of influencing the discipline. “I am not a politician!” he replied, angrily.
> ..
> Mikhail Gromov, the Russian geometer, said that he understood Perelman’s logic: “To do great work, you have to have a pure mind. You can think only about the mathematics. Everything else is human weakness. Accepting prizes is showing weakness.” Others might view Perelman’s refusal to accept a Fields as arrogant, Gromov said, but his principles are admirable. “The ideal scientist does science and cares about nothing else,” he said. “He wants to live this ideal. Now, I don’t think he really lives on this ideal plane. But he wants to.”

!!

He quit his job as a mathematician, because of his discovery..because his notoriety would force him to deal with the unethical idiots.

You now understand Perelman’s MIND (totaly Ideality, VS Reality=Corruption), which accounts for his isolation. The latter is perceived as eccentricity, the extreme language FU!.

This reminds me of the quote:

“An unreasonable man tries to Change the World, a reasonable man adapts to it..we need more unreasonable men”
— Revolution VS Evolution

“Captain Pike has an illusion, and you have reality. May you find your way as pleasant.””
— [“The Menagerie”](#)/Star Trek, Talosians final message to Capt. Kirk

Perelman changed the world of mathematics, & has “retired” into a world of mathmatical-ideality (“illusion”..mathematics is said to be imaginary by a famous mathematician)

ST is an “illusion” (all math, no connection with real-world experiment), & after 20 years needs to be “retired”. The Star Trek quote refers to 2 paths: Reality & Illusion. “Not Even Wrong” is accentuating that the Illusion is going no where.

31. **Thomas Larsson**
June 25, 2007

I am confused about what Witten tries to do. Wasn’t CS theory for general group G solved 20 years ago, with all correlators being framed knot invariants which can be readily computed from G. Why not just put G = SO(2,1) and be done with?

In my understanding, the difference between a spin connection and a SO(2,1) gauge connection is that you have a vielbein which can be used to write down the Einstein-Hilbert action,. But the CS action does not depend on the vielbein, so I don’t see what special about spin connections in this case.

Anyway, it is striking that there are two unphysical assumptions: D = 3 and a negative cc. Witten suggests in section 1.2 that a world with positive cc (like the one we may be living in) is always at best metastable, which is pretty close to the anthropic worldview.

32. **Peter Woit**
June 25, 2007

M,

I agree the Witten paper has no direct physical relevance, and there’s no reason experimentalists should be reading it. But it’s a fundamental new idea about gauge theory, gravity and mathematics, and as such it may some day lead to something directly physically relevant. I think a lot of Witten's best work should be thought of as analogous to some of the best work of experimentalists developing new detector technologies and acceleration techniques. There's no reason a theorist should be reading the papers of people who are making advances on using lasers to accelerate particles, and this work is not of any immediate use in HEP, but it is very important that someone is doing this kind of thing, and it is the sort of thing that may lead to important breakthroughs in the future.

Thomas,

CS theory is just not well understood at all for cases like this of non-compact space and groups. One of the interesting things about Witten's work is that I think it shows how little we understand about what many people think of as simple gauge theory QFTS which are completely understood.

33. **Peter Woit**
June 25, 2007

Aaron,

I was making no comment at all about string theorists, I was making comments about string theory. You seem a tad defensive…

I won't be surprised a few years from now when string theorists are working on 4d gauge theory approaches to quantum gravity using Ashtekar variable, insisting what they are doing should be called "string theory". I think Nati Seiberg made the comment to a reporter a couple years ago that whatever theorists had some success with in the future, they would call it "string theory".

34. **urs**
June 25, 2007

Classical Einstein gravity may be formulated as a theory of SO(n,m)- and/or ISO(n-m)-connections. This basic fact underlies the approach of LQG just as well as many developments in the study of supergravity, which is usually best understood as a gauge theory for the super Poincare Lie algebra.

The crucial point of LQG is 2-fold

1) conceive the space of all these gauge connections as a space of holonomy functors

2) pass from the space of ordinary (smooth) connections to the larger one of "generalized connections" (essentially dropping smoothness and continuity of the holonomy).

It is mainly the second point here which makes LQG different from other approaches of quantizing gravity. I don't think that Witten is talking about quantizing Chern-Simons using "generalized connections".

But I do very much agree that it is good that attention is being paid to understanding gravity as a theory of gauge connections. Of smooth connections, in particular. One look at the LQG program anew, without passing to generalized connections.

35. **urs**
June 25, 2007

> One look at the LQG program anew […]

One should look at […]

36. **Aaron Bergman**
June 25, 2007

I was making no comment at all about string theorists, I was making comments about string theory.

I just think it's amusing that anything you think is interesting you define to not be string theory. I think most of the rest of world would consider AdS/CFT as part of the circle of ideas generally called "string theory".

You seem a tad defensive…

Are you really going to play the "defensive" game? It doesn't put you in very good company.

37. **Pavel Krapivsky**
June 25, 2007

There is nothing wrong with Eugene's translation, I just submitted mine before I saw his. LDM rightly says that it is hard to "capture the true flavor of the obscenity".

38. **Peter Woit**
June 25, 2007

Aaron,

Most of the world considers AdS/CFT to be "string theory" because the AdS side of the correspondence is a string theory. I seem to be repeating myself, but it's a simple fact that in Witten's paper the AdS side of the correspondence is NOT a string theory. Claiming that what Witten is doing in this paper is "string theory" is simply dishonest.

39. **Aaron Bergman**
June 25, 2007

And I think that's all there is to say on the subject.

40. **marcus**
June 25, 2007

Peter said:
Claiming that what Witten is doing in this paper is "string theory" is simply dishonest.

Urs said:
One look at the LQG program anew, without passing to generalized connections.

One convenient way to do that—combining a fresh look with an illustrative comparison—is to follow Laurent Freidel's recent online seminar talk of 15 May.

Matter coupling to 3d quantum gravity and effective field theory

The link to the series of online LQG seminars is at

http://relativity.phys.lsu.edu/ilqgs/

The direct link to the PDF of Laurent's slides is
[url]http://relativity.phys.lsu.edu/ilqgs/freidel050807.pdf[/url]

The direct link to the MP3 audio of his talk is
[url]http://relativity.phys.lsu.edu/ilqgs/freidel051507.mp3[/url]

A key point which Laurent makes at the outset is that because matter is present the system has infinitely many degrees of freedom and that he is intentionally restricting himself to using analytical tools which apply to the 4D case (with matter) as well. He explicitly excludes AdS/CFT.
In the work described in the talk, the authors get Feynman diagrams of matter out of spinfoams (the "paths" of spacetime geometry evolving in a path-integral picture).

Since both talks are recent and concern 3D gravity, they make an interesting side-by-side comparison and allow the fresh look that Urs mentioned.

41. **Changcho**
June 25, 2007

Perelman is a very intriguing character. I liked the quote from joe about needing more "unreasonable men" to change the world.

LDM: I have been studying a bit of the Russian language, but strangely enough I could not find the insulting expression in my russian textbook ;-), thanks! However, from your post I should be very careful about that insult as you imply that it is very strong…

42. **urs**
June 25, 2007

Why is theoretical physics harder than mathematical physics? Witten gives a good explanation on p. 10:

> We make at each stage the most optimistic possible assumption. Decisive arguments in favor of the proposals made here are still lacking. The literature on three-dimensional gravity is filled with claims (including some by the present author [6]) that in hindsight seem less than fully satisfactory. Hopefully, future work will clarify things.

😉

43. **M**
June 25, 2007

Peter, I do not understand your analogy: nobody would worry that people who develop lasers might have lost contact with the reality. On the contrary, the physical relevance of a certain kind of theoretical work looks questionable, even taking into account that sometimes progress comes from unexpected directions. I wonder what Newton would have achieved by studying topological supergravity in lower dimensions.

44. **urs**
June 25, 2007

> One convenient way to do that—combining a fresh look with an illustrative comparison—is to follow Laurent Freidel's recent online seminar talk of 15 May.

What I meant is that it is still an open (though possibly also still intractable) problem to find the quantized configuration space of *smooth* connections in the case of either Yang-Mills theory or of Einstein gravity, or of any other non-trivial nonabelian gauge theory.

I am talking about the analog of the "LOST theorem

href="http://de.arxiv.org/abs/gr-qc/0504147

or the analogous result be Ch. Fleischhack

http://arxiv.org/abs/math-ph/0407006

by CH. Fleischhack — but for *smooth* connections.

In fact, this has been done in the case of abelian (but possibly higher) connections by Freed, Moore and Segal. They find a couple of very interesting — and very subtle — quantum effects. Precisely *this* kind of analysis ought to be done for Yang-Mills and/or Einstein gravity in Palatini form.

Clearly nonbody can do it right now. But that's the interesting problem to solved. I would appreciate if people started looking into this problem more seriously. Freed-Moore-Segal demonstrate that there is gold to be found here…

45. **elzorro**
June 25, 2007

Not sure if it has been mentionned on this blog already but there has been a quantum gravity school in Poland last march organized by John Barrett and Hermann Nicolai covering some string theory, some LQG, and some stuff related to noncommutative geometry and to quantum groups. Some of the lectures notes are online, all the talks being available as mp3s http://www.fuw.edu.pl/~kostecki/school.html

46. **LDM**
June 25, 2007

M:
The history of the laser is the opposite. Some very great physicists did think it was crazy. When Townes met Niels Bohr in Denmark and explained to him the idea behind the maser, he said "but that is not possible". Townes never could convince him.

Von Neumann had the same reaction (but later changed his mind).
Noted Columbia theorist Llewlen Thomas said flatly it could not work, and stopped talking to Townes. All this is recounted by Townes in chapter 5 of his book "How the laser happened".

So in some sense, the laser analogy is not too bad, but contrary to Peter's opinion, the theorists can learn a lot from experimentalists, as the laser history shows.

If I cared to refute Peter's analogy, I would prefer to cite an observation of Ulam's in regard to Von Neumann:
"Von Neumann was the master of, but also a little bit the slave to, his own technique. When he saw that something could be done, he let himself be carried away on tangents. My own feeling is that some of his work on classes of operators or on quasi-periodic functions, for example, is very interesting technically, but to my taste not terribly important; he could not resist doing it because of his facility." – Adventures of a Mathematician, pg. 78.

Witten results are technically interesting to mathematicians, but to my taste, not terribly important for real physics.

47. **Peter Woit**
June 26, 2007

M,

Another way of saying my analogy is that every scientific field is limited in what it can do by the limits of its tools. What Witten is doing is trying to come up with new understanding of the basic tools of fundamental physics, QFTs. Personally I think a deeper understanding of the relation of math and QFT may lead to new tools that might ultimately allow the solution of fundamental problems in HEP. Sure, this hope is very speculative and maybe wrong, but I think it's as promising a speculative idea as any others out there at the moment.

It's hard to tell where new ideas like Witten's will lead, maybe nowhere. But at least he's coming up with new ones, not endlessly working on the same tired ones that don't work like most people.

48. **M**
June 26, 2007

LDM, I didn't know this history, but I insist that inventing the laser might have been successful physics or unsuccessful physics. In any case, physics.

Peter, in some speculations I see the effort to come back with results (e.g. today the arXiv:0706.3688 paper by Chamseddine and Connes), while a too large fraction of hep-th now looks disconnected from reality.

Anyhow, let's wait 30 more years to see if we will better understand physics by studying things like gravity in 2+1 dimensions, the SM landscape in 2+1 dimensions, etc.

49. **Kea**
June 26, 2007

Witten's criticism of the gauge theory approach, mentioned by Distler (who was at the talk), would seem to apply to both strings and Woit's favoured QFT thinking.

50. **anon.**
June 27, 2007

Kea, I note that you quote Lubos' remark on your blog:

"Well, I happen to think that if Edward Witten started to work on loop quantum gravity, as defined by the existing contemporary methods and standards of the loop quantum gravity community, it wouldn't mean that physics is undergoing a phase transition. Instead, it would simply mean that Edward Witten would be getting senile. We all admire him and love him, if you want me to say strong words, but he is still a scientist, not God." – Lubos Motl

This kind of gives the impression that Ed Witten risks being deemed as "senile" and "not God" but merely "a scientist" if he did take more interest in Loop Quantum Gravity. Maybe that's why he doesn't?

51. **LDM**
June 27, 2007

M:
Since that particular laser history is not taught in the textbooks, it is safe to say more than a few physicists are unaware of it.

Regarding your comment:
Anyhow, let's wait 30 more years to see if we will better understand physics by studying things like gravity in 2+1 dimensions, the SM landscape in 2+1 dimensions, etc.

Well of course, there's no need to wait 30 years, you can simply decide now that these approaches are sterile, and so strike out on your own…and if these approaches are not likely to go anywhere in terms of physics, you have *drastically* increased your chances of discovering something interesting by not studying them.

52. **evankeane**
June 27, 2007

Ah i noticed this paper on the violation of the Kerr bound while perusing the arXiv … was wondering if you would mention it!

Evan Keane

Strings 2007

June 25, 2007
Categories: Strings 2XXX

Strings 2007 is starting today, and already there seem to be a large number of different laptops connecting to this blog from wlan.uam.es. I'm hoping that some of their owners will write in here with news of how the conference is going. I'll also try and add links here to any blog and press coverage of the conference that I see or hear about.

Witten's new 83 page paper entitled Three-Dimensional Gravity Revisited appeared on the arXiv last night and presumably this is the detailed version of what he'll be talking about in the first of tomorrow's talks at the conference. It corresponds pretty much to what he talked about here in New York a few weeks ago, which was described here. I assume many of those laptops at the conference are being used to download and read copies of Witten's paper. At his blog, Clifford Johnson notes that he won't be at Strings 2007, and hopes that this will ensure that there will be an exciting breakthrough announced there, just like what happened when he decided not to attend Strings 1995.

As I write this, I see that string theorist Jacques Distler is there live-blogging. Here's the first of his reports.

Update: Some of the slides of the talks are already on-line. The slides for Witten's talk are available here.

Update: There's an interesting posting here by Jacques Distler about the Witten talk, and, for those who enjoy such things, quite a rant from Lubos here, prompted by my comment that in this case Witten is investigating quantum gravity using non-perturbative QFT, not strings.

I've been reading Witten's paper a bit more carefully, and it raises all sorts of interesting issues. He makes the point that even in 3d, we really don't know exactly what "non-perturbative pure quantum gravity" is. He uses the Chern-Simons formulation of 3d gravity in terms of gauge theory to motivate his guess at the correct boundary CFT, and then once he has that he has something much more well-defined to study.

This is a bit reminiscent of the compact, non-gravitational situation. There Chern-Simons theory works fine perturbatively, but to understand the non-perturbative theory one connects it to a CFT on the boundary, in this case the Wess-Zumino model (this is the story that got Witten a Fields medal).

Update: B. Yen has set up a video-blog for Strings 2007, where there will be iTunes podcasts of the conference available.

Update: Latest report from Jacques Distler is that, since Witten's one, which he got to write about:

*there have been some **very** cool talks*

(emphasis in the original), but he can't tell us even which ones they were since his laptop is malfunctioning. Slides of the talks are available here. I've looked through them and, besides Witten's, don't see anything I would describe as "**very** cool", but maybe that's just me.

Update: All the talks are on-line now and I just looked through the last of them, and watched the summary talk by Gross. Lisa Randall discussed recent calculations of black-hole production at colliders, with the bottom line being that even in the unlikely event the gravity scale is within reach of the LHC, existing bounds already pretty much rule out the possibility of seeing the kind of dramatic effects from black hole production that have been widely advertised as something that might be seen at the LHC.

Gross noted that the conference was much less mathematical than last year's, possibly because Yau was not involved in organizing it. He was most enthusiastic about describing the many talks on AdS/CFT, especially the Beisert talk which told about recent progress in getting an exact solution of N-4 SYM. Some talks referred to possible applications of AdS/CFT not just in QCD and heavy-ion physics, but in condensed matter physics (using the duality to get info about relevant CFTs). He told about Polchinski's speculation that "maybe AdS/CFT will solve high T_c superconductivity", but dismissed it with "sounds great, but seems unlikely to me." He dealt with the landscape talks by flashing them by quickly, in a lower and less enthusiastic voice, noting that they made up at least a quarter of the talks at the conference. He dealt similarly with the cosmology/anthropic talks, describing Bousso's as "an attempt to make the anthropic principle precise if not respectable."

After the summary, he gave his own take on the state of string theory, saying that one had to be honest about the lack of falsifiable predictions and that now he had a slide headed "The Failures of String Theory". He continues to feel that the main failure is because we "don't know what string theory is", that something is missing, some principle that would pick out not a "vacuum" but a "cosmology", one perhaps using new ideas about what space and time are. He said he was not too upset by the landscape, because "we don't know what the rules are" in string theory, so one can't argue that string theory implies the landscape. He appeared to feel that he is losing the debate, complaining that this used to also be the opinion of his colleagues, but that they were going over to the other side because of the cosmological constant, saying that if another explanation of the CC was found 90% of the anthropicists would come back to his side. He tried to minimize the size of the CC problem, measuring it with respect to a supposed 1 TeV SSYM breaking scale and working in energy, not energy density units, so it is only too small by a factor 10^{16}. He compared this to Dirac's famous large number problem (which Dirac tried to solve not anthropically, but by time-varying constants, leading to a prediction that was falsified), which was finally "explained" by asymptotic freedom. His message to the anthropocists was "just because you don't know an explanation doesn't mean it doesn't exist".

Finally he mentioned that Strings 08 will be at CERN, Strings 09 in Rome, and no one has yet agreed to host Strings 10. He argued that the series of Strings conferences "must go on", because they are "like the canary in the coal mine", and if they stop that would be a very bad sign for string theory.

I'm curious to know what those in attendance thought of this; it wasn't exactly a rousingly optimistic portrayal of the state of the subject…

Update: There's an article in El Pais about the conference and about the state of string theory. My Spanish isn't perfect, but as far as I can tell the piece was pretty much pure unadulterated hype, of the sort that it is one goal of the Strings XX bashes to generate.

Update: Jacques Distler finally got his computer fixed, and posted about one example of what he considered a "**very** cool talk" with exciting new ideas. Unfortunately, it seems the ideas are not that new, since a commenter wrote in to his blog to point to papers from four and a half years ago that do pretty much the same thing.

Comments

1. **A quantum diaries survivor**
   June 26, 2007

   Hi Peter,

   I wonder why three international conferences dealing with strings one way or another are being held at more or less the same time around the globe. I of course would not have dreamt of going to Strings 07, while I am indeed going to Pascos next week, which has some strings flavor but admittedly is also devoted to particles and cosmology. But die-hard stringers might have had to make a difficult choice.

   By the way, I will try to report from there, but I will be there only for a couple of days so I can't really promise anything…

Cheers,
T.

2. **marcus**
June 26, 2007

here is a sample exerpt from the talk by Witten, slides 14 – 16:
==quote==
First of all, I am only going to consider the case of negative cosmological constant.
Currently there is some suspicion that quantum gravity with Lambda > 0 doesn't exist non-perturbatively (in any dimension) with positive cosmological constant. The reason for this is that it does not appear to be possible, with Lambda > 0, to define precise observables. This is natural if it is the case that a world with positive cosmological constant (like the one we may be living in) is always unstable.

If that is so, then a world with Lambda > 0 doesn't really make sense as an exact theory in its own right but (like an unstable particle) must be studied as part of a larger system.

Whether that is the right interpretation or not, since I don't know how to define any precise observables, I don't know what it would mean to try to solve 2+1-dimensional gravity with Lambda > 0, since it isn't clear what we'd want to compute.
==endquote==

Unfortunately this sounds like a rationalization. Perhaps it is a question of what tools one uses, as you suggested in comment to your previous post:
http://www.math.columbia.edu/~woit/wordpress/?p=570#comment-26394
If one set of tools for studying the world fails to provide observables, then before declaring the world we "may be living in" to be "like an unstable particle" it seems a sensible reaction would be to look around for better technical means. Am I missing some essential part of the message? Maybe others would comment on this passage.

---

3. **Thomas Larsson**
June 26, 2007

Marcus,

Witten's argument is exactly what leads to anthropic principle: if our dS universe is only a metastable vacuum, it is probably an ugly mess from a landscape of possibilities, and there is no beautiful selection principle to guide us. Declare defeat, and work on mathematically nice but physically wrong things, like 3D and AdS.

This is a unavoidable problam if all observables in QG are global, because dS space lacks the right asymptopia where the global observables can live. AFAIU, the only way around this dilemma is if there are local observables in QG after all; then we don't have to worry about the lack of global observables.

Indeed, we know for sure that there are local observables in classical GR – Rovelli's GPS coordinates. Now, these are only partial observables, but we could make them complete by reading a local clock. Without leaving my car (if I had one), I could use my fancy GPS equipment to read off my GPS coordinates, and read off the dials of my watch to get proper time, and get a local, complete observable. This observable is local, because I don't have to visit a holographic screen in an AdS region of the multiverse (i.e. outside our visible dS universe) to make this observation; I can stay in my car.

4. **Peter Woit**
June 26, 2007

Marcus,

I think this debate over dS observables is completely irrelevant to the Witten paper. All he is doing is referring to that debate and noting that the arguments in his paper involve the AdS case, and don't apply to the dS case. He's not making any claims in this paper of any new ideas or understanding about the dS case.

5. **Jim Clarage**
June 26, 2007

If I were still in theoretical physics I'd eagerly await next year's Strings 2008. For it will mark the 175th anniversary of Hamilton's article on optics and planetary motions where he invokes the spirit of Bacon:

In every physical science, we must ascend from facts to laws… and unity arises from variety: and then from unity must re-deduce variety, and force the discovered law to utter its revelations of the future.

Or if I can quote again with inlines relevant to science nearly two centuries later:

In every physical science [even string theory], we must ascend from facts to laws… and unity arises from variety: and then from unity must re-deduce variety [i.e., must reproduce the standard model], and force the discovered law to utter its revelations of the future [i.e., make novel and testable predictions of future experiments].

6. **Peter Woit**
June 26, 2007

Hi Tommaso,

One reason there are a lot of string theory conferences (20-30 a year) is that there are a lot of string theorists. And conferences are concentrated in the summer months when people don't have to teach. Typically there are a few smaller "satellite" conferences organized before and after the main Strings conference, at locations not too far away. I gather there are some of these going on around now. Will look forward to your reports from PASCOS!

7. **Eugene Stefanovich**
June 26, 2007

Jim Clarage:

I don't think that string theory should be blamed exclusively for the stagnation in modern theoretical physics. I think that the fundamental problem was best expressed by Mark Srednicki in his reply to Chris Oakley on Cosmic Variance

And I don't agree at all with your statement that while "it would be very nice to explain SM parameters, but establishing a mathematical framework free of inconsistencies has to be done first." Historically, progress in physics has almost never been made this way. It almost always came by somebody futzing around with some not-fully-baked theory, until something interesting popped out.

This “futzing around with some not-fully-baked theory” was very successful until 1960’s. But, apparently, it doesn’t work anymore. I think that the chance that something will “pop out” is close to zero. Maybe it is time to stop relying on random attacks and switch to a methodical and organized siege: Decide which physical principles are truly important (the principle of relativity? laws of quantum mechanics?…) and build a rigorous axiomatic mathematical framework around these principles.

8. **hmmm**
June 26, 2007

> Decide which physical principles are truly important (the principle of relativity? laws of quantum mechanics?…) and build a rigorous axiomatic mathematical framework around these principles.

Well that’s the whole problem isn’t it. That’s like saying *“…to become a gazillionaire we need to decide which business idea is the right one and build a rigorous business around that…”*

9. **Eugene Stefanovich**
June 26, 2007

hmmm,

That was my point. I have an impression (please correct me if I am wrong) that there is no much interest in designing a sound plan and building the “business” from ground up. Just look at talks at String 07 or any other similar gathering. People are trying to prove theorems without even knowing what their axioms are. These are conjectures based upon previous conjectures. Using your analogy, they try to become gazillionaires by gambling in a nearby casino. Srednicki thinks this is the way to go. I respectfully disagree.

10. **Coin**
June 26, 2007

Using your analogy, they try to become gazillionaires by gambling in a nearby casino.

Well, they have to. It’s the only game in town.

11. **Coin**
June 26, 2007

So I’m afraid a lot of this is beyond my current level of understanding, but I’m trying to figure out– it’s been commented several times that Witten’s Strings 2007 paper here doesn’t seem to have much to do with string theory. However, it does seem like it has a lot to do with AdS/CFT correspondence. In fact, it seems like he’s *using* the AdS/CFT correspondence; as I understand this he’s analyzing black holes in 3-dimensional AdS spacetime by reasoning about the equivalent 2-dimensional conformal field. That’s AdS/CFT, right? So here is basically what I’m trying to figure out:

1. This paper seems to somehow be using AdS/CFT without talking about strings. I didn’t actually realize you could *do* that. Is there anything “special” about CFTs that correspond to spaces with strings, or is it just a matter of whether you choose to describe the space as containing strings?

2. Although the paper doesn’t explicitly talk about strings, would it make sense for the ideas in this theory to be followed up, in either a later paper by Witten or somebody else, by linking these ideas to string theory or simply adding strings to the toy model described here? If so, what would that look like? Like, would it make any sense to drop strings into the toy model here and analyze their interactions with a BTZ black hole?

12. **Peter Woit**
June 26, 2007

Just a reminder. If you want to have general philosophical discussions about physics, please find some other forum. If you have something substantive to say specifically about the topic of this posting, String 2007, please do so.

13. **Peter Woit**
June 26, 2007

Coin,

When people talk about “AdS/CFT” duality, they normally are talking about a relationship between string theory on five-dimensional AdS space (really AdS times a 5-sphere, the strings need 10 dimensions) and a supersymmetric gauge theory on its 4d asymptotic boundary. More generally, one hopes that this string-gauge duality works for different sorts of geometries. One could try and do this with 3d AdS space, and people have worked on that.

But this is just not what Witten is doing. He’s not looking at string theory on the AdS space, he’s looking at pure gravity, and at a Chern-Simons gauge theory, and trying to understand their relation. Pure gravity and string theory on AdS are just two very different theories. What Witten is doing just isn’t a string/gauge duality. I don’t see what you’re going to achieve by adding string theory to this story, and I suspect if Witten thought one could get anything that way, he’d certainly be mentioning it in his paper or talk at the conference. Strings are mentioned zero times in his talk

14. **ori**
June 27, 2007

Let us leave alone for a moment the question of whether the recent paper of Witten has anything to do with String theory or not.

What it clearly implies, is that variable changes as those promoted by Ashtekar and used in LQG can not be correct, and have nothing to do with the quantum version of gravity (which is expected to have black holes to start with). Thus, in the most well understood situation of 3d gravity, where the variable change is clean and covariant (unlike 4d) Witten shows that this avriable change is menaingless in quantum level.

Should we still expect that 4d LQG tricks do makes sense ? certainly not. This is a complicated non covariant version of the 3d case, and one should not expect any conclusion besides that LQG has no meaning after quantization.

Thus, one of the conclusions of Witten’s paper is that LQG does not make any sense.

15. **Peter Woit**
June 27, 2007

Ori,

Witten does not claim that using gauge variables and the Chern-Simons action has nothing to do with quantum gravity. He actually uses these to produce the correct central charge of the boundary CFT. Here's what he actually does say about this:

> We know that Chern-Simons gauge theory is useful for perturbation theory, as was explained in that section, and we hope that it is useful for understanding some nonperturbative questions. We used the gauge theory approach to get some hints about the right values of the cosmological constant (or equivalently of the central charge) simply because it was the only tool available.

The variables that do seem to be useless for non-perturbative quantum gravity in 3d are string variables. Witten doesn't mention strings at all in his talk and gives no indication that he thinks they have anything to do with this problem.

Unfortunately I don't think this tells us anything one way or another about 4d, but if one could have the kind of success there starting with Ashtekar variables that Witten seems to have starting with Chern-Simons in 3d, that would be quite spectacular.

16. **Ori**
June 27, 2007

Peter,

I want to avoid misunderstanding and confusion. Forget for a moment how Witten derives whatever he derives, this is a matter of taste. The invariant physical conclusion is that the non perturbative complition of 3d gravity has nothing to do with the gauge theory !

Similar conclusion is expected to hold for the 4d case, where there would not be enough dof in the gauge theory to create black holes, as in the 3d case.

Most of the people here are laymen, and I want to make sure that they understand that Witten's conclusion is that the gauge theory variables are bad. This was known to Seiberg for some time, apparently. This leaves no hope that any interesting insight can be derived from gauge theory formulation of 4d gravity, for obvious reasons. Thus, LQG seems to have been falsified.

Unrelaed comment for experts : The way Witten derives some central charges does uses gauge theory variables. This is, however, incorrect since the gauge theory variables dont make much sense. Witten admits this is the case, and wishes he had a better way to derive the central charges. He could, as well, guess the correct values by some factorization and absence of gravitational anomalies requirements. This is the second way he motivates these charges, which is much more convincing.

One should not confuse the physics conclusion (which is that the Chern-Simons formulatoins have nothing to do with gravity) with the technical details (which is an incorrect derivation of some central charges, which is as good as a guess). Note that the reason CS has nothing to do with gravity is independent of the ability of disability to derive these central charges.

17. **Ori**
June 27, 2007

Another comment is that Peter is correct that there are no stringy variables in the story. However, the construction of the paper is a very particular example of something which was well known is string theory : AdS/CFT. This also shows that the results obtained in string theory in this context are much more general, because similar techniques can be applies in non supersymmetric non stringy cases.

18. **gunpowder&noodles**
June 27, 2007

"Thus, LQG seems to have been falsified."

Oh, come. It's extremely clear that all of Witten's [brilliant] work on this is strictly dependent on the 3D context. There is not a clue about how to extend *any* of this stuff to the enormously different 4D case. I have no time for LQG, but enthusiasts for it could quite legitimately point out that 3D gravity has been studied intensively for years, and so far it has told us exactly nothing about 4D physics. There's no reason to suspect that this is about to change.

For all my admiration for EW, and gratitude to him for the way he has changed the subject, I have to summarize the situation as follows: he has shown that gravity with the wrong sign of the CC in the wrong dimension is wrongly described by Chern-Simons. Next talk please.

19. **Peter Woit**
June 27, 2007

Ori,

Your claims that Witten has shown that "gauge theory variables are bad" etc. are directly contradicted by what Witten actually wrote, including the part I quoted above. You're putting lots of highly tendentious claims in his mouth which are not in his paper and were not in his talk (or in the one I heard here in New York). I think Witten is someone who is very careful to say precisely what he means. In this case he is being very clear that the question of how to properly understand 3d non-perturbative quantum gravity is very much still up in the air. Here's what he writes:

> We make at each stage the most optimistic possible assumption. Decisive arguments in favor of the proposals made here are still lacking. The literature on three-dimensional gravity is filled with claims (including some by the present author [6]) that in hindsight seem less than fully satisfactory. Hopefully, future work will clarify things.

This is very different than claiming that he has shown that "the non perturbative complition of 3d gravity has nothing to do with the gauge theory !" and falsified the whole idea of using gauge theory variables.

20. **Ori**
June 27, 2007

The conclusion from your comment is that you have misunderstood the paper completely. You have picked some quotations out of context, and that is it.

Witten clearly write that the CS theory can not be the correct non perturbative complition, he said it explicitly in strings and it is written in the paper. This is the whole point of the paper, if you claim otherwise, you have misunderstood the paper and the talk. Let me quote for you :

"There is, however, an even more serious problem with the idea "gauge theory=gravity in 2+1 dimensions"…. "

He has couple of such arguments against the 3d description via CS theory, which in the overall sum to what I said.

You may keep thinking whatever you want, I am the last to care, I was only willing to make sure that readers of this blog will be aware of the correct statements.

To gunpowder&noodles :

If you had read the paper, you would have understood that the arguments against the gauge theory description are completely general, and can be extended to 4d as well. Witten also says that theories with positive cosmological constant probably have no non perturbative complition (unless embedded in a more complete model), so it is not clear what is the point in trying to quantize de sitter backgrounds, without a stable ads ground state

21. **Ori**
June 27, 2007

Another way to proceed, is that u answer a question (Instead of putting more and more out of context quotes to defend yourself).

There are BTZ black holes in 3d. If CS is the correct way to quantize gravity than there should be extremely many states in the topological CS theory. What are they ? 🙂

Of course, there are no such states (since CS is a topological theory), which is the main point in Witten's paper (and the strings talk). This means that CS has nothing to do with the full quantum theory of pure gravity.

22. **dont be misleading!**
June 27, 2007

I would like to recall ori that is possible to written the BTZ black hole as a solution of CS theory. It was done many years ago by Malrof and other people. Look at ArXives.

Thus, the quantum CS theory also contains BH, as is part of they are part of its classical spectrum.

23. **woit**
June 27, 2007

Ori,

I certainly don't claim that Chern-Simons is the "correct way to quantize gravity". For one thing, as Witten states, non-perturbatively the situation of 3d gravity remains unclear. The problem is not necessarily even well defined: maybe there is more than one consistent non-perturbative theory that has the right classical limit. Witten has made a very interesting proposal about a distinguished boundary CFT that might deserve to be called the "right" boundary CFT for non-perturbative pure gravity. I don't think he claims at all to know what if any 3d variables and Lagrangian treated non-perturbatively with conventional path integral techniques would lead to this theory.

This problem corresponds to something that has always bothered me in the simpler compact Chern-Simons-Witten theory. The standard thing people say there is that, even non-perturbatively, the theory is defined by "integrating [tex] e^{ikCS[A]} [/tex] over the space of connections A". This is kind of nonsense. Try actually doing this integral, e.g. by putting it on a lattice, and you start to see what the problems are. While the CS action is fine for generating a perturbation theory, non-perturbatively it doesn't completely define the theory. What Witten does in that case is use the CS action to motivate what the boundary theory is (WZW), and then uses that to actually do computations. Even in that case, the CS action isn't the full story about the non-perturbative theory, but it definitely is true that you want to be using gauge theory variables.

Anyway, there's a very interesting story going on here, and gauge theory variables are definitely part of it.

24. **Peter Woit**
June 27, 2007

Thanks dbm,

I was wondering about Distler's claim that "We would not, for instance, ever see the BTZ blackhole from the gauge theory", which didn't seem to make sense since the gauge theory gives the same classical equations and the BTZ black hole is a classical solution.

25. **A quantum diaries survivor**
June 27, 2007

Peter,

your links to lubos' blog get dumped to a unwelcoming page, you should know this by now, or is it an attitude to ignore the fact ? 😉

This time though I will second the auto-embargo of Lubos. I intended to read his rant, but now I changed my mind… He is probably happier that way.

Cheers,
T.

26. **Peter Woit**
June 27, 2007

Tommaso,

I'm not ignoring it, just figure it adds to the entertainment value…

27. **Aaron Bergman**
June 27, 2007

What I think Jacques meant (although I haven't asked him) is the microstates of the BTZ black hole — it's hard to imagine where they are in a topological theory.

28. **urs**
June 27, 2007

> microstates of the BTZ black hole — it's hard to imagine where they are in a topological theory.

The Einstein-Hilbert action functional can be rewritten as a Chern-Simns action functional in 3d. So if the former has an extremum somewhere (like a black hole solution), the latter must have, too. After all, they are *equal*. No? What am I missing?

29. **urs**

June 27, 2007

> it’s hard to imagine where they are in a topological theory.

One more thing:

consider pure gravity in any dimension d. Consider a d-dimensional topological space, possibly wtih boundary. Suppose you could make sense of the EH path integral over all metrics on that space, possibly with prescribed boundary data. Shouldn’t the answer depend solely on the topology of the chosen space? Shouldn’t it in fact give you a functor from d-dimensional topological cobordisms to some flavor of vector spaces? Wouldn’t that be exactly as for Chern-Simons theory?

The only real distinction which I can see between the EH formulation and the corresponding gauge theoretic (Palatini) formulation of gravity is that the former vaguely suggests not to include degenerate metrics, while the latter vaguely suggests to include them.

30. **Aaron Bergman**
June 27, 2007

No? What am I missing?

Microstates. You’re thinking about the classical solution which is a macrostate.

31. **urs**
June 27, 2007

> No? What am I missing?

Microstates. You’re thinking about the classical solution which is a macrostate.

Hm.

Suppose I did the CS path integral over connections with the constraint that the vielbein be invertible.

What would be the difference between using the EH action functional in the path integral from using the CS action functional? Both are equal. Both are defined on the same domain. What’s the difference?

32. **urs**
June 27, 2007

(Sorry, there is a blockquote missing. The comment preview did correctly show the nested quotation which I did include.)

33. **Aaron Bergman**
June 27, 2007

Isn’t that question aking to, suppose I did the path integral for the EH action in 4D? Do you know how to quantize CS theory with that constraint?

(Witten talks about this in his original paper, BTW.)

34. **urs**
June 27, 2007

> Do you know how to

Right, of course I don’t.

The only thing I really know here (beyond knowledge of folk lore and plausibility arguments that all get so confusing after a while) is this:

for every modular tensor category, I can construct

A) a 3-dimensional TQFT

B) a 2-dimensional rational CFT

C) such that they are holographically related.

If that tensor category happens to be the representation category of loops in G, then

A) is G-Chern-Simons theory

B) is the WZW model on G

C) is the fact that states of Chern-Simons are conformal blocks (precorrelators) of WZW.

It is strongly assumed that this can be extended beyond rational CFTs. But only for rational CFTs this has been done in detail.

In any case, there should be other modular tensor categories, or variations thereof, and there should be other realizations of A,B,C. I would like to see if what Witten is talking about could be (classes of) other realizations of this. It ought to be, since he is in effect arguing that A should be defined in terms of B.

35. **joe**
June 27, 2007

The videos seem to be working for me (USA, west coast). Somewhere else, someone mentioned some download bandwidth issues.

I need to contact the program chair (name & email) to get permission to do some tests with their videos. Can’t find it on the Strings ’07 website, just the webmaster emails. Help? (I went to the IFT sponsor site, but the Miembros section has a generic list of staff.)

36. **ori**

June 27, 2007

Hi, “dont be misleading!”

You seem to completely misunderstand the point. It is correct that the classical BTZ solution was written long time ago, as any other classical solution (eg gravitons near the boundary). However, the BTZ entropy can not be accounted for by the quantum CS theory in any reasonable way (and here Witten says “although there were some attempts in the past…”).

Thus, the CS theory does not appreciate the thermodynamics of black holes and can not explain their entropy. Hence, CS can not describe in the quantum level anything like gravity (this is a correct invariant physical conclusion which means that the gauge theory variables are meaningless after quantization and can at best serve for some unmotivated guesses).

A similar thing happens in 4d as well, where the gauge theory variables of LQG dont make sense after quantization.

37. **ori**
June 27, 2007

Urs,

>”The only real distinction which I can see between the EH >formulation and the corresponding gauge theoretic (Palatini) >formulation of gravity is that the former vaguely suggests not to >include degenerate metrics, while the latter vaguely suggests to >include them.”

You are correct, and this difference is very very crucial !! if not this difference the theories were equivalent. In all known examples in string theory the non perturbative formualtion has never included degenerate metrics. Also here, this is the fundamental reason for the failure of CS variables in the quantum level. Similarly, in 4d, the gauge theory variables (and LQG) break down for the same reason.

38. **Arun**
June 27, 2007

In 4D, LQG has enough degrees of freedom to account for black hole entropy, therefore I do not buy Ori’s *A similar thing happens in 4d as well, where the gauge theory variables of LQG dont make sense after quantization.*.

It may very well be that LQG does not make sense for other reasons.

39. **dbm**
June 27, 2007

hi uri,

I think that despite phylosphical considerations, facts are facts.

And the first fact is that you don need to consider the CS formulation of GR to admit configurations wih a degenerated vielbein, is enough to consider the first order formalism in which the vielbein and the spin connection are regarded independent fields, this theory, automatically, admits a solution where all the fields are zero. Furthermore this first order formulation of GR can be done in any dimension.

The second fact is that it has been proved, long ago, that the first order formalism, is off shell equivalent to the usual metric formulation, that is: the set of configurations where the vielbein is not inverible is of measure zero.

I would say that all this bla bla, of witten and the degrees of freedom of GR, and that in string theory the invertibility of the metric is a serious thing, contradict not only his paper on the soulbility of GR, but also his papers on topological quantum field theory, and topological gravity of the 80’s.

I really dont like his arguments and I even think that his last paper deserve further analysis, for instance is no possible that the guy doesnt obtain the right value for the entropy for BH of mass=2 and says, ok, for larger k a better agreement will be obtained.

Come on!

40. **Carol**
June 27, 2007

I want to live in the dimension where a story can earn you a Fields medal, and shouting loud enough releases a graviton.

41. **ori**
June 28, 2007

dear dbm,

The entropy for k=1 is not expected to match since you compare a semi-classical entropy given by the BH formula to an exact calculation of microstates.

In this regime, semi classical analysis makes no sense since the black hole is very small and gravity is strongly curved. Therefore, the fact the results are similar is a good surprise.

The fact that for larger k there is a better and better agreement is a strong indication that what Witten did is correct.

Of course, this is correct regardless of you liking or disliking this.

42. **Arun**
June 28, 2007

How do black holes form in 2+1 classical gravity? (i.e., with no gravitational radiation)?

43. **dbm**
June 28, 2007

Dear ori,

So, please explain me, why in every realistic situation in which we can actually compute the entropy beginning with the microscopical description it COINCIDE with our thermodynamical result.

Thanks!

44. **ori**
June 28, 2007

Dear ori,

>So, please explain me, why in every realistic >situation in which we can actually compute the >entropy beginning with the microscopical >description it COINCIDE with our >thermodynamical result.

>Thanks!

I assume you refer to systems which are not gravitational. The explanation is trivial : The result coincides with the thermodynamic result in the limit where the system is large. For example, even in the system of free spins which contains N spins the result will not coincide with the thermodynamic result for finite N, there are logarithmic corrections (Stirling's formula) etc.

Note that to count states correctly one also needs to have a weakly coupled description of the degrees of freedom, and for the word thermodynamics to make sense there needs to be a classical description of the system.

In the case k=1 the black hole is very small, so it is not a macroscopic system and also the system has no good classical description. Thus, you basically extrapolate formulas illegaly and you should not be surprised that you get a somewhat different result.

I don't have any idea what you meant by your comment, anyone who has ever studied statistical physics know that there is never agreement with thermodynamics beyond the strict limit of infinite number of dof, sorry.

45. **dbm**
June 28, 2007

Ori,

You are right, in the theormodynamic limit.

However, I still insist in my point of the offshell equivalence of CS theory, and 2+1 GR, I mean the facts that I previously mentioned.

The first fact is that you don need to consider the CS formulation of GR to admit configurations wih a degenerated vielbein, is enough to consider the first order formalism in which the vielbein and the spin connection are regarded independent fields, this theory, automatically, admits a solution where all the fields are zero. Furthermore this first order formulation of GR can be done in any dimension.

The second fact is that it has been proved, long ago, that the first order formalism, is off shell equivalent to the usual metric formulation, that is: the set of configurations where the vielbein is not inverible is of measure zero.

46. **ori**
June 28, 2007

I am happy you agree (and you should withdraw your "come on" comment).

The mistake in your other statement is the sentence

"the set of configurations where the vielbein is not inverible is of measure zero."

Nobody knows how to formulate the non perturbative gravity path integral (it is not necessarily the naive one with Haar measures)
and hence, you can not deduce the measure of degenerate metrics correctly, sorry.

In fact the work of Witten CLEARLY shows that the effect is not a measure zero one, and leads to substantial non perturbative differences in the two theories (which naively differ by measure zero).

47. **ori**
June 28, 2007

btw, one should expect this set of non invertible metrics to be very important also in higher dimensions.

48. **dbm**
June 28, 2007

But, consider the paper gr-qc/0303113. My argument is as follows, if the first order, palatini, formalism admit naturally a degenerated viebein, and these people has shown that it coincide, off shell, with the second order metric formalism. ..

There is some level of tension between that paper and your last setence!

btw, thanks for the discussion, is fun.

49. **ori**
June 28, 2007

I will study the paper, perhaps tomorrow. I don't wanna give sloppy answers.

50. **amuseds**
June 28, 2007

Meanwhile at the pinnacle of the 'Superstringy Universe' edifying discourse on the subject can be found, such as

…. So PW is basing his monster conjecture on a single footnote! Well, I’ve always been leery of a man with a foot fetish. Such a man reminds me too much of a gay shoe saleman several years back who kept marveling at my feet as though I was Cinderella in the flesh trying on glass slippers. I swear, I recall feeling both deeply annoyed and genuinely embarrassed by the whole ordeal!

And since then I’ve come to realize that it takes a real man to become captivated by the more noteworthy parts of my body. In a way, I suppose, PW is nothing more than that faggot shoe salesman at Nordstrom’s.

….PW’s site would have already petered-out (no pun intended) if it weren’t for a few top contenders, such as Aaron. That being said, I must make mention that Lubos was (and always will be) Peter’s toughest challenger – without having a single close second coming from behind him!

Now I’m totally clueless when it comes to military matters, but it seems pretty obvious to me that the only sure-fire way to stamp out PW’s site is to battle it out of existence, not to simply let it fizzle out – like an open can of coke. Moreover, I’ll go out on a limb and say that because his site is essentially limited to a single narrow issue, this severe handicap will work to hasten its demise.
…

Now back to the discussion of the relevance of gauge theory to quantum gravity…

51. **hmmm**
June 28, 2007

> …. So PW is basing his monster conjecture on a single footnote! Well, I’ve always been leery of a man with a foot fetish. Such a man reminds me too much of a gay shoe saleman several years back who kept marveling at my feet as though I was Cinderella in the flesh trying on glass slippers. I swear, I recall feeling both deeply annoyed and genuinely embarrassed by the whole ordeal!
>
> And since then I’ve come to realize that it takes a real man to become captivated by the more noteworthy parts of my body. In a way, I suppose, PW is nothing more than that faggot shoe salesman at Nordstrom’s.
>
> ….PW’s site would have already petered-out (no pun intended) if it weren’t for a few top contenders, such as Aaron. That being said, I must make mention that Lubos was (and always will be) Peter’s toughest challenger – without having a single close second coming from behind him!
>
> Now I’m totally clueless when it comes to military matters, but it seems pretty obvious to me that the only sure-fire way to stamp out PW’s site is to battle it out of existence, not to simply let it fizzle out – like an open can of coke. Moreover, I’ll go out on a limb and say that because his site is essentially limited to a single narrow issue, this severe handicap will work to hasten its demise.
> …

Whoa!!! who said what now.

52. **Peter Woit**
June 28, 2007

Please, Lubos’s craziness is bad enough, but if you start discussing here the commenters on his blog, that way lies madness…

53. **gunpowder&noodles**
June 29, 2007

Ori declaims:

“If you had read the paper, ”

No need to remind us that you’re a string theorist…..

“you would have understood that the arguments against the gauge theory description are completely general, and can be extended to 4d as well.”

Wow. Do tell us, in mathematical detail, how to perform that feat. Or at least refer us to EW’s paper. Chapter and verse please. And no “clearly a similar argument” stuff please.

54. **ori**
June 29, 2007

Dear “gunpowder&noodles”,

Gauge theory decriptions contain in their path integral solutions which correspond to degenerate metrics in the gravitational description. These are not measure zero, as was previously thought (the absence of degenerate metrics gives rise to a different non perturbative complition).

You may or may not be convinced, it is not important. There will certainly be people who will keep thinking in the gauge theory variables direction although it was proved incorrect for d=2 and d=3. I presume that sensible people understand this point very well and will take it into account.

The situation has changed, since previously one could argue that the change of vairables is regular apart from a measure zero set of configurations. The same argument was presented for both 3d and 4d (in the 3d case even Witten himself was actually incorrect). Now that this argument is known to be incorrect for 3d and 2d it is most conceivable that the 4d case, which has the same singular locus , and essentially the same ambiguities, should not be described by gauge theory variables

55. **Peter Woit**
June 29, 2007

ori,

Can you point me to somewhere where anyone has actually done the Chern-Simons integral non-perturbatively (in a non-abelian theory)? As far as I’ve ever been able to tell, this integral is just inherently ill-defined, you’re integrating a phase over an infinite-dimensional space, and there is no “measure” here with any reasonable properties that would allow you to talk about some configurations being “measure zero”. The problem is very fundamental, it exists even in simple QM analogs of the CS theory. If you actually try and discretize the Chern-Simons functional integral and do the the (finite) integral, it appears that there is no way to get an answer which is not completely dependent on how you do the cutoff.

56. **urs**
June 29, 2007

> somewhere where anyone has actually done the Chern-Simons integral non-perturbatively

I [this lecture](), Mike Hopkins hinted at a way how to do it. But didn't really explain it.

57. **Peter Woit**
June 29, 2007

urs,

That's not at all what I'm talking about, since that kind of very abstract "integral" has no notion of "measure" or "measure zero". What I don't believe exists is the kind of standard integral used to define path integral quantization: an honest measure in the cut-off theory that, when you integrate appropriate quantities against it gives well-defined answers when you remove the cut-off.

58. **urs**
June 29, 2007

> that kind of very abstract "integral" has no notion of "measure" or "measure zero".

Yes, as I said. He hinted at a way how to do it, but didn't explain it.

59. **Arun**
June 29, 2007

Very brash out of ignorance, I'd say that in 2+1 pure gravity black holes have no way to evaporate (or to form) perturbatively, there being no radiation. So perturbation theory cannot take you from a non-black hole sector of states to a blackhole sector of states. I would guess that in 3+1 pure gravity neither condition holds.

60. **Yatima**
June 29, 2007

The presentation PDFs really need abstracts.

An LHC status report This I can understand more or less.

A talk from 'SUSY 2012' uncovered today!

Baryons from Instantons in holographic QCD Hmmm….

Andrei Linde in Amazing Mr Universe mode 😉

61. **Jonathan Vos Post**
June 29, 2007

A122505 Arises from energy spectrum of three dimensional gravity with negative cosmological constant, in analysis by Edward Witten.

n……………a(n)
–……………—–
1……………24
2……………24
3……………95
4……………..1
5…………143
6……………..1
7…………262
8…………-213
9…………453
10……….-261
11……………?
12……………?
–……………—–

Can anyone extend this integer sequence, to be properly credited for said extension in the Online Encyclopedia of Integer Sequences? If so, please have at it…

62. **gunpowder&noodles**
June 30, 2007

PW said: "don't see anything I would describe as "very cool", but maybe that's just me."

No doubt you are willing to make an exception for Bousso's talk. His theory makes "thousands" of predictions. That is so cool that it suffers from frostbite. It's almost as cool as the funereal garb and manner he affects in his classes. In fact I would put it right up there with Paris Hilton.

63. **anonym**
June 30, 2007

To Ori,

Really basic question.
Witten's work is for AdS background only.
I think LQG can handle dS case. (I am not an expert on LQG but just curious about your logic.)

Then how can you say EW's work rules out LQG?

I think you can not say anything definite like this one especially when you rely on very incomplete arguments.

64. **ori**
July 1, 2007

Well, dS is argued not to exist alone, but only when there is also some AdS around as well. So, Witten's work applies. You should think of dS as an unstable particle. Such a system can not be described completely without referring to the correct ground state (which is AdS).

65. **ori**
July 1, 2007

and btw, I don't know which of the arguments you call "incomplete". An argument is not a proof certainly. However, it is complete as an argument since it addresses the correct question, and suggests the correct answer convincingly or not, this depends on you (If you only "argue" you can not convince everybody, for this you need a proof. However, wise people often don't need a proof at all).

66. **hmmm**
July 1, 2007

> However, wise people often don't need a proof at all

In which case wise people are often wrong and usually less wise than they think.

> Well, dS is argued not to exist alone, but only when there is also some AdS around as well. So, Witten's work applies. You should think of dS as an unstable particle. Such a system can not be described completely without referring to the correct ground state (which is AdS).

A wise person should also add that the above statement may have nothing what-so-ever to do with the universe in which we live.

However, in Witten's opinion, it could be relevent to all those 2D beings living in rubber sheet world.

67. **Thomas Larsson**
July 1, 2007

Well, dS is argued not to exist alone, but only when there is also some AdS around as well.

IOW, if observations don't agree with your pet theory, observations are irrelevant. It might be true in this case, but it is nevertheless a classical crackpot argument.

If we take the positive cc seriously, it inevitably leads to the conclusion that QG must have local observables, since there are no global observables in dS. The lession from CFT, viewed as diff-invariant field theory on the circle, is that diffeomorphism symmetry is only compabitible with locality in the presence of diff anomalies (multi-dimensional Virasoro algebra).

68. **GH**
July 1, 2007

Jonathan Vos Post asked:
— — — — — — — — —
n……………a(n)
–…………… — –
1……………24
2……………24
3……………95
4……………..1
5…………143
6……………..1
7…………262
8…………-213
9…………453
10……….-261

Can anyone extend this integer sequence, …?
— — — — —

Here:

739
-833
1169
-1168
2172
-2505
3104
-3581
5255
-6449

69. **Viri**
July 1, 2007

Gross's talk:

http://s4.video.blip.tv/0440000555536/Strings07fri-DGrossKITPSantaBarbaraPerspectives815.flv

the real interesting part starts around 25:00.

70. **anonym**
July 1, 2007

Orid said

“You should think of dS as an unstable particle. Such a system can not be described completely without referring to the correct ground state (which is AdS).”

Maybe I don’t understand correctly the meaning that dS corresponds to metastable state. Anyway let me ask it to you. Do you think our universe is metastable? If observation tells me this “according to our present knowledge” then I would think that there is some problem in our understanding rather than think our universe is metastable. Also this is one of what I am saying as “many” incomplete arguments. You seem to overly trust a theory which is at best a consistent approximation to the reality. You should think your present understanding could be very wrong although you may think it is your best at this point.

71. **Urs Schreiber**
July 2, 2007

I do not understand in which sense it can be true that “gauge variables for gravity are bad”, as ori said here:

http://www.math.columbia.edu/~woit/wordpress/?p=571#comment-26430

I can imaging that there is some true statement sounding vaguely similar, but as such I am having a hard time making sense of that.

We are talking about two different sets of coordinates for one and the same configuration space: that of metrics.

It is true that there are ISO(2,1)-connections which correspond to degenerate pseudo-Riemannian structures.

But there are also metrics that correspod to degenerate Riemannian structure, clearly.

I am willing to consider the question whether or not to include degenerate metrics in the path integral. But formulating a metric in any of the many equivalent ways there are can hardly change anything about the physics.

Unless, of course, it suggests some *different* procedure for how to go beyond perturbation theory. But all this is shrouded in mystery right at the moment.

Also, it would seem that those interested in string theory should be fond of the idea of encoding Riemannian structures in terms of ISO(n,1)-connections. As far as I am aware, that is the only sensible way to handle supergravity.

72. **Haelfix**
July 4, 2007

Its definitely extremely puzzling, and also whats so damn interesting about the paper, at least to me.

Assume for a second you solve both methods exactly. The difference (if indeed there is one after careful analysis) should then sum up to the contribution of degenerate metrics in the path integral *I think*.

This suggests a strategy for 4D. Use gauge variables, then ansatz the form of the contribution from 2d, 3d and see what we get.

73. **Haelfix**
July 4, 2007

I should say ‘ansatz the form of the degenerate metrics from 2d, 3d intuition’ and then sum them together.

74. **Chris W.**
July 4, 2007

Speaking of taking things seriously, recall this remark of Steven Weinberg’s, in *The First Three Minutes* (1977):

“This is often the way it is in physics. Our mistake is not that we take our theories too seriously, but that we do not take them seriously enough. It is always hard to realize that these numbers and equations we play with at our desks have something to do with the real world.”

75. **M**
July 7, 2007

Actually, the article in El Pais mentions the problems of strings: here are the key sentences:

“… la verdad es que la complejidad de la teoría está resultando enorme. Los mismos especialistas creen que esto se debe a que no es aún una teoría terminada. Por ejemplo, de las diferentes soluciones que encuentran a sus ecuaciones no saben cómo elegir la que realmente corresponde a nuestro universo.

…no ha podido despegarse de la principal crítica: la falta de experimentos en perspectiva que verifiquen si es correcta. …

… Para otros muchos, la ausencia de predicciones experimentales de una teoría física después de 20 años de desarrollo, es un inconveniente que no se puede pasar por alto en modo alguno.”

76. **Indrajeet Patil**
July 15, 2007

Is it true that Witten is no longer working in string theory?

77. **Peter Woit**
July 15, 2007

Indrajeet,

During the last year Witten has written two long papers, neither of them about string theory. I don’t know what he spends his time working on. Besides the topics of the two recent papers (3d gravity and TQGT/geometric Langlands), quite likely he continues to spend some time thinking about string theory.

Too Much Good Stuff

June 30, 2007
Categories: Uncategorized

I've been finding recently that an increasing serious problems with blogs is that there are too many good ones with material worth reading. I've learned quite a lot recently from many well-informed blog postings, but the sheer number of these makes it hard to find the time for other things one should be doing.

I'll violate my usual rule of sticking to math and physics and report that my brother Steve is joining me in the family blogging business by being involved as Publisher in a new venture that just launched this week called Xconomy. Basically it's a blog based up in Cambridge, with offices in Kendall Square, devoted to news about what they call the "exponential economy", that part of the economy responsible for what is perhaps too optimistically described as exponential growth in certain areas. They're focusing on events and news relevant to new technology, especially bio-technology, businesses in the Boston area. For some interesting blog postings by their CEO Bob Buderi about what it's like to start up this sort of business, see here and here. For a nice posting about Doc Edgerton, see here.

Back to physics and math, over at Backreaction there's an excellent posting on the GZK cutoff and high energy cosmic ray experiments, and a report from Loops 07 sent in via Blackberry by Sabine Hossenfelder.

An American Physics Student in England tells about a recent conference on Heavy Flavour Physics, giving a very nice overview of what is going on in that field.

The latest This Week's Finds in Mathematical Physics from John Baez is out (available here, blog entry and comments here). It's a wonderful description of the various mathematical patterns that the standard model particles fit into. Most well known is what happens in SU(5) and SO(10) GUTs, where one can fit the fermion quantum numbers into something that can equivalently be described as the spinor representation in d=10, or the exterior algebra $\Lambda ^* (\mathbf C ^5)$. John goes on to explain various possible connections to the exceptional groups, including a recent idea from Garrett Lisi about how to use E_8 to get three generations.

The blog entry comments discuss two recent papers by Chamseddine and Connes about their non-commutative algebra approach to this question of mathematically characterizing the SM degrees of freedom. The papers are on the arXiv, entitled A Dress for SM the Beggar, and Why the Standard Model. Because of these papers and Witten's recent one, John seems to be getting a bit more optimistic about physics, writing "I get the feeling that theoretical physics may not be quite so stagnant after all!"

All sorts of interesting stuff at the Secret Blogging Seminar, including yet more about Connes: a "review" by A. J. Tolland of the first quarter of the new book by Connes and Marcolli (available here), which A. J. claims has the title *Noncommutative Geometry, Quantum Fields, Kitchen Sinks and Motives*. Like the earlier fat book on non-commutative geometry by Connes, it's an amazing document, ranging widely over physics and mathematics, covering ground from QFT to the Riemann hypothesis, at a level varying from expository sections on well-known subjects to more speculative research-level discussions. I've just started looking at it, may bring along a copy for summer vacation reading when I head up to a lake in New Hampshire tomorrow.

Other interesting things at the same blog include reports (here and here) from Ben Webster about talks by Sergei Gukov on categorification and gauge theory (about which he has a new expository paper here), as well as about an earlier talk by Gukov on Arithmetic Topology and Gauge Theory.

Also worth reading are posts from Ben Webster about centers of blocks of category O, various comment section discussions with David Ben-Zvi at both this blog and the n-category cafe, and a series of postings by David Speyer about quadratic reciprocity and geometric class field theory (I'm running out of energy to provide links…).

From Ben-Zvi (who could run a really great blog if he chose to…) there are notes from the recent conference at Northwestern on non-commutative geometry. These include an intriguing lecture by Beilinson, as well as lectures by Nadler and Ben-Zvi himself about their recent work which connects geometric Langlands with questions about more conventional representation theory using striking ideas about how to handle loop spaces. They have a recent paper about this, which has been very high on my list of things I wish I understood better ever since David gave an inspiring talk about this here a couple months ago.

Finally, one more thing definitely worth looking at in light of Witten's new work: an expository and historical article by Jim Lepowsky about the story of the relation of vertex operator algebras and the monster group. He explains what is so remarkable about the specific vertex operator algebra that Witten is connecting to 3d gravity on AdS, including the ways in which it is conjecturally uniquely the "smallest" such structure in a specific sense.

Comments

1. **Garrett**
   June 30, 2007

   Mathematicians are wonderfully nonlinear complex systems — one little perturbation and good stuff flies in all directions!

   Although relating the three fermion generations to E8 triality got John interested, I'm actually assigning ALL standard model and gravitational fields to elements of Lie(E8). There's a link to this description in TWF, but here it is directly:

   Deferential Geometry

   The above site (my research wiki) has a one paragraph summary and a link to a talk (slides and practice audio) I just gave here in Morelia. In five words… "everything in an E8 connection." This includes putting the gravitational spin connection and frame together in this connection too, which should be looking like a familiar thing to do right now… 😉

2. **Garrett**
   June 30, 2007

   Oops, bad link. This one should work:

   Deferential Geometry

3. **Bee**
   June 30, 2007

   Hi Peter,

   Thanks for the link. What I didn't write in the post is that there's some preliminary data from Pierre Auger which apparently confirms the presence of the cut-off – I expect it will be presented at that Cosmic Ray conference next week. Also, I have a second post about the Loops 2007 with some photos, but admittedly little content. I hope the slides to the talks will be on the website soon. E.g. Fotini's talk (about non-locality) was very interesting, and so was Garrett's 🙂

The conference closed today with a discussion session (moderated by Carlo Rovelli) where everybody was asked to give the most optimistic prediction what he/she will be talking about at the Loops 2017 – and what the probability is for that to actually happen. It was very interesting (much of it based on what the phenomenology will hopefully teach us in the soon future). It was recorded and I hope the audios will be available on the websites as well.

Best,

B.

4. **Kris Krogh**
July 1, 2007

Hi Bee,

Can you tip us off on the the cut-off energy seen? Is it exactly the GZK prediction?

Cheers,

Kris

5. **James**
July 1, 2007

Regarding the use of the word 'exponential': Any increasing function on a finite interval grows faster than a non-trivial exponential function. So I would say the use of the word above is not so much optimistic as it is next to meaningless.

6. **M**
July 1, 2007

Bee: various Auger talks at ICRC 2007 already appeared on astro-ph, including some energy spectra. For example see arXiv:0706.2096

7. **Fabrizio**
July 2, 2007

Hi Garrett.
I just saw your great wiki and started to have a look into E8: the idea of unifying all in a single connection is really nice.

I just wonder if you can really avoid the problem of mirror fermions. For example the Cl_7 model of Trayling suffers (at least to me) of that problem: there is a duplication of the fermions, since the (algebraic) spinor has 8×8 complex = 128 real components, that is twice the components of a SM family, 16×2 complex=64 real. (and antiparticles are usually already taken care by the 16 weyl fermions of the SM)…

Cheers,
Fabrizio Nesti

8. **Garrett**
July 2, 2007

Hi Fabrizio,

This is a good question. Before I answer it, let me tell the story of how the connection came together. I was working mostly with GR and spinors using Clifford algebra, when I saw Trayling's nice description in Cl(7). But his model made more sense to me in Cl(1,7), and after playing with it a bit I was able to combine Trayling's description with the spin connection and frame of GR, with the Higgs entering in a nice way. But this only had one generation of fermions. When I saw E8, it matched this exact structure, along with two more blocks of fermions. This made me very happy, so I've been telling people about it, even though there's still some things to figure out.

Now, your question. Trayling solves it by either dropping the "mirror" fermions, or including them as anti-fermions. Without looking at the big picture, there's no good reason to do one or the other. But in the big picture it's more clear what's going on. If we start with the compact real form of E8, there are no mirror fermions — each fermion block has the correct number of real degrees of freedom. But the real form has SO(4) as a subgroup, whereas we want SO(1,3) — the Lorentz group, with the corresponding action on the blocks of spinors. To fix this, we could start with a complex form of E8, such as would be built from complex Cl(8) bivectors — but this has twice the desired degrees of freedom for all particles, kind of like supersymmetry. This would include the "mirror" fermion problem you describe. Instead, we use a real form of complex E8, built from a complex representation of Cl(1,7). This has the same degrees of freedom as compact E8, with no mirror fermions, and has the Lorentz group as a subgroup.

Now, I don't know WHY it's right to start with this real form of complex E8, other than because it works. The answer to this will have to come from whatever even big picture emerges. 🙂

I hope this answers your question, but here's some more nitty-gritty detail. If you look at the big matrix, each generation of fermions is a 4×4 block, with each entry a 2×2 block representing a Weyl spinor. Each of these Weyl spinors has 4 real degrees of freedom, which can be reassembled into a column of two complex numbers — the way we usually work with them.

Best,
Garrett

9. **ok**
July 2, 2007

For those who are interested:

Geometrically Engineering the Standard Model: Locally Unfolding Three Families out of E_8

http://arxiv.org/abs/0704.0445

10. **ori**

July 2, 2007

μ is a nice letter

11. **Fabrizio**
July 3, 2007

Hi Garrett,
yes, that is what I was asking. One must have a sort of reality condition on the Cl_7 (or Cl_1,7) spinors, but this puzzled me, since spinors always carry a complex representation… On the other hand, mirror fermions can not be decoupled by making them heavy, because chiral fermions can not have a gauge invariant mass term.

Incidentally, this is what led me to consider that Cl_4 is actually enough, if one considers everything in LR-symmetric way: you can unify just isospin with gravity, and strong interactions are just internal geometrical symmetries (my [0706.3304]). This approach also leads back to Peter Woit's inspiring [NPB paper of 1988], where he introduced spinors on a 4-dim complex spacetime, and our world is a real section of that…

If now in E8 one can fit the fermions in the (OxO)3 part of the gauge field (64×3 real fields) I expect that doubling is no longer a problem because one can use the would-be mirror family (S8-) as the second generation… (and the vector (V8) as the third).
If this works (e.g. strong spinor charges, triality.. work well) it is really nice!

Fabrizio

12. **trd**
July 4, 2007

Hi Garrett.
I just saw your great wiki and started to have a look into E8: the idea of unifying all in a single connection is really nice.

I just wonder if you can really avoid the problem of mirror fermions. For example the Cl_7 model of Trayling suffers (at least to me) of that problem: there is a duplication of the fermions, since the (algebraic) spinor has 8×8 complex = 128 real components, that is twice the components of a SM family, 16×2 complex=64 real. (and antiparticles are usually already taken care by the 16 weyl fermions of the SM)…

13. **Venkatesh Rao**
July 4, 2007

Hello Peter:

I finished your 'Not Even Wrong' and Lee Smolin's 'The Trouble with Physics' a few weeks ago. I posted a review on my new blog that you might like to check out:

[Review]

I am a systems theorist by training, not a physicist, so I hope I haven't gone too far out on a limb in my assessment of the debate. Thanks for a great read, you've done the planet a service with this book (and this blog)! I spent several weekends ploughing through both books and it was very rewarding, if exhausting, work. I think my comprehension was between 10-50% through most of the read, but even that left me feeling illuminated.

14. **Aaron Bergman**
July 4, 2007

Don't believe everything you read. Check out the various reviews of Lee's book on cosmic variance, for example.

15. **nutcase**
July 7, 2007

More good stuff:

> arXiv:0707.0005
> Higgs Physics as a Window Beyond the MSSM (BMSSM)
> Michael Dine, Nathan Seiberg and Scott Thomas

Interesting read… but… is it just me or is it very unusual finding a purely phenomenological paper coauthored by N.Seiberg? Are the winds changing in high energy physics?

16. **Bee**
July 23, 2007

Here is the promised update:

[GZK cutoff confirmed]

Less Stuff Than Usual

July 10, 2007
Categories: Uncategorized

I was on vacation for a while, but haven't been posting much mostly since there's not that much to write about. Like every summer, there are huge numbers of string theory conferences going on all over the world, but from looking at the talks that are available, the subject is about as dead as it has been for the last couple years, with essentially no new ideas. The reaction of string theorists to all the criticism that they have been getting in recent years about the lack of any connection of string theory to the real world seems to have been to rename their conferences things like String Theory and the Real World.

One other reason I haven't been writing as much is that there are an increasingly large number of quite good math and physics blogs out there, run by other people who are doing a great job of writing about the kinds of news that I've often posted items about. There's a mini-revolution going on in research-level mathematics blogging, with various Fields medalists being joined by groups of some of the best graduate students and post-docs around. Following on the heels of Berkeley's wonderful Secret Blogging Seminar, there's Cornell's Everything Seminar and the Max Planck Institute in Bonn's Vivatgasse 7 (mainly about arithmetic algebraic geometry).

Also at the Max Planck Institute, a couple weeks ago there was the yearly Arbeitstagung, a conference devoted to recent mathematics research and run in a somewhat unconventional way. The concept is to mostly not schedule talks and speakers in advance, but to instead just try and get a group of the best possible people to show up, and then to more or less democratically decide on who should speak about what, depending on who has something new to talk about. The first Arbeitstagung was organized by Friedrich Hirzebruch exactly 50 years ago, back in 1957, partly with the goal of bringing Germany back into the mainstream of mathematics research after the post-WWII period. Hirzebruch remained the organizer for many years (and was also director of Max Planck), and was there this year to give the opening lecture, on The first Arbeitstagungen in 1957, 1958 and 1962. At the first Arbeitstagung in 1957 some of the talks announced dramatic new results in mathematics, including the birth of K-theory. Grothendieck's talk included the first definition and use of K-theory (of coherent sheaves on a projective algebraic manifold) in his proof of what is now known as the Grothendieck-Riemann-Roch theorem. Hirzebruch had worked some of this out more concretely before, and he reported on his work with Borel-Hirzebruch which links up representation theory, characteristic classes and topology in a fundamental way. Bott was not there, but had recently discovered Bott periodicity, and over the next few years Atiyah used this and the Dirac operator to reformulate Grothendieck-Riemann-Roch as a general index theorem in the context of differentiable manifolds, proving the theorem with Singer, and lecturing about it at the 1962 Arbeitagung. Hirzebruch gives an excellent description of some of this history. The talks are all available online, but I fear that there was nothing discussed this year that seems to reach the heights of what was being discovered back in those early days 50 years ago.

The latest news on the LHC schedule is: close the machine April 2008, beam commissioning starting May 2008, first beams at high energy July 2008, about exactly one year from now.

The Notices of the AMS has a wonderful set of articles this month about George Mackey, I wrote a bit about him here.

The Harvard string theory group will be minus not just Lubos Motl this coming year, but also Nima Arkani-Hamed, who will be on leave (I'm not sure where he's going or what his plans are). I hear that the two of them had a joint goodbye party.

Starting today, Arkani-Hamed is pushing the multiverse at the String Theory and the Real World conference. For the hundredth puff-piece about how wonderful the anthropic multiverse pseudo-science is, see the article Islands in the Sea, at fqxi.org.

Bloomberg.com is carrying a review of *Endless Universe*, the recent Steinhardt/Turok book I wrote about here. The author of the review seems to have noticed the same thing about the book that I did:

Given the recent controversy surrounding string theory — the publication last year of Lee Smolin's "The Trouble With Physics" and Peter Woit's "Not Even Wrong" — it's disturbing that Steinhardt and Turok don't even address their dependence on it. Does their model work if string theory is wrong?

My sense is that it doesn't, but they never even face the possibility — lending inadvertent weight to Smolin's and Woit's complaints that string theory is strangling physics. I hope the authors can someday publish a second edition in which they don't treat string theory as the only game in town.

Despite its problems, string theory does now have a shop and a blog.

Update: The news is that Arkani-Hamed will be moving from Harvard to the Institute for Advanced Study.

Update: In case any one was worried that Lubos's move back to Europe would end his entertainining rants about the stupidity of anyone expressing skepticism about string theory, don't worry. The latest is Bloomberg: another idiotic article, which begins:

> Elizabeth Lopatto is the name of the latest breathtaking idiot who was hired to write about theoretical physics for Bloomberg. I am periodically amazed that the newer journalists are always able to exceed the degree of mental breakdown of their predecessors.

and then goes on from there…

Comments

1. **JC**
   July 10, 2007

   The "fatigue" of some string conferences in recent years, looks a lot like the similar "fatigue" of those GUT and supergravity conferences from the early-mid 1980's (before string theory became very popular), when those subjects were losing their luster.

2. **Who**
   July 10, 2007

   Though age from folly could not give me freedom, It does from childishness: can Fulvia die?

   Could someone bring me up to date?

   > The Harvard string theory group will be minus not just Lubos Motl this coming year, but also…

   I've not been keeping track of the Motldrama. Why will Harvard be
   "minus" the abovenamed asset next year?

3. **anon.**
July 10, 2007

Thanks for the link to the article ‘Islands in the Sea’, which states on page 3:

> To grasp just how large a number this [landscape of metastable vacua] is, try writing it out longhand: a 1 followed by 500 zeroes.
>
> 100
> 000
>
> No matter how staggering a figure 10^500 is, though, it has the same status as any other number. In fact, if the multiverse is infinite, 10^500 possible vacua do not exhaust it. “Very large is still finite,” says Vanchurin of this strange state of affairs. “So there is a finite number of values, but an infinite number of regions, where those values are realized. If you take each one of those 10^500, there are infinitely many places in the multiverse where these values take place.”

Of course, they are right to emphasise that, despite the fact that string theory can’t be falsified, one of it 10^500 vacua might still be the real world.

Similarly, you can’t falsify religions because they are too vague and contain caveats like ‘Do not put the Lord your God to the test …’ (e.g., Deuteronomy 6:16 and Matthew 4:7).

Don’t you think that instead of being so negative about the failure of string theory to make falsifiable predictions, you should start to be more positive and point out its great success as an invincible religion? One of the great drawbacks to other religions is the lack of mathematical obfuscation in them, and the fact that anyone can read their secrets. A religion based around mathematical speculation has valuable advantages over existing religions: 1) Infidels can be dismissed by the high priests as merely mathematically illiterate ignoramuses; 2) Hollywood stringy sci-fi films can be made as powerful propaganda tools to convert the heathen masses to string theory; and 3) String theory, unlike regular religion, is a theory of everything unobservable, so it is like knowing the mind of God; hence it really is the one true religion, if correct (it is be impossible to disprove, so it is correct for religious purposes).

Suppose that the assumptions which make the landscape of string theory finite sized turn out to be false, and the landscape is really infinite in size. (This is quite possible if dark energy turns out to be a misunderstanding of experimental facts, like phlogiston theory.)

In that case, string theory will come in an infinite number of forms. Since infinity is the biggest possible number in mathematics, it then *must contain the real universe we see* because the probability that a set of infinite possibilities will contain reality must be equal to 1, mustn’t it?

On the other hand, the probability that any particular vacua, selected at random out of the infinite number of vacua, will be equal to 1/infinity = 0.

4. **Peter Woit**
July 10, 2007

Lubos has left his position at Harvard and returned to the Czech republic, not clear what his future plans are. I don’t know the full story of why he left Harvard.

5. **Anon**
July 10, 2007

Peter, you are and apparently always will be a critic, and I mean that in the worst possible way. To complain the way you do without adding anything to the solution of what you complain about only calls attention to your own inadequacies.
That being said I enjoy many of the math and physics links.
Good luck,
Anon

6. **Peter Woit**
July 10, 2007

Anon,

Well, I got into this critic business because no one else was doing it and it needed doing. I’d be happy to hand off the job to someone else, but there don’t seem to be many takers (possibly because of all the abuse about one’s inadequacies from anonymous and other sources that comes with the role…)

Anyway, another reason for fewer posts is that something I’ve been working on for a very long time finally seems to be working out and I’m spending more time on that. Maybe I’ll turn out to be not so inadequate after all, maybe not, we shall see…

Glad you enjoy at least some of the material here!

7. **Coin**
July 10, 2007

One of the great drawbacks to other religions is the lack of mathematical obfuscation in them, and the fact that anyone can read their secrets.

I take it you are not familiar with the works of William Dembski.

8. **gunpowder&noodles**
July 10, 2007

“but I fear that there was nothing discussed this year that seems to reach the heights of what was being discovered back in those early days 50 years ago.”

Exactly. Theoretical physics is far from being alone in its malaise. And some might argue that the influence of Grothendieck and people like him play the same role in mathematics that string theory plays in physics. Too much hype, too few real results.

9. **Peter Woit**
July 10, 2007

gunpowder,

I think mathematics and theoretical physics are both experiencing one problem that is similar: they’re victims of their own success. As you figure things out, you do the easier things first, what remains are more and more difficult problems.

But Grothendieck's story is quite different than that of string theory, in some ways it's more like the Standard Model. During the 50s and 60s he came up with ideas that are the foundation of the way algebraic geometers now think, and they do this because it actually successfully solves problems. It is true that the kind of abstraction Grothendieck pursued is something that led many less talented than him into fruitless areas. There was a reaction against this in the 70s and 80s, with a rather balanced view about abstraction emerging.

I actually find it surprising how much progress mathematics is able to make despite its increasing difficulty. The proofs of the Poincare conjecture and Fermat's last theorem in recent years are striking examples.

There's nothing like the landscape going on among top-level mathematicians, and I don't think the problem with string theory is that of too much empty abstraction. Rather the problem is the refusal to admit that a specific, concrete idea has failed, that of getting particle physics out of a 10/11d string/M-theory.

10. **ali**
July 10, 2007

    Hi Peter,
    I was not following Lubos's story either. Can it be that he was denied tenure or something like that?

11. **Tom Whicker**
July 10, 2007

    On the opening page for the "String theory and the real world" conference, they state:

    "String theory has given us new insights into quantum gravity, and has provided non-perturbative techniques for analyzing dynamical questions and computing observable predictions. It has suggested a variety of candidate solutions for all of the generally recognized problems of fundamental physics: the hierarchy problem, the cosmological constant problem, the nature of dark matter, and the origin of the fundamental constants. "

    Is there any support for any of this? Or is it generic ad copy?
    Regards to all, TW

12. **mathjunkie**
July 11, 2007

    Sad to hear that Lubos has left Harvard.

13. **mathjunkie**
July 11, 2007

    Was Lubos' leaving due to his radical way of defensing string theory on his personal web that caused Harvard to do something that Lubos hated very much?

14. **Peter Woit**
July 11, 2007

    ali + mathjunkie,

    Lubos wasn't there long enough to come up for a usual tenure decision so I don't think it was that. If his colleagues in the theory group had any problems with how he was defending string theory, I never heard about it.

    Tom,

    It's ad copy, based on some kernel of truth, but highly misleading. The part about providing a "variety of candidate solutions" to the great problems of physics is laughable in a way since it tries to make a virtue out of the theory's main problem, that of being consistent with almost anything.

15. **mathjunkie**
July 11, 2007

    Peter, thanks for your rely.

    Just found that Lubos put down on his personal web "Pilsen, CZ" as his current location. So, Lubos is back to his country where he grew up. Anyway, we can still watch how he defends string theory there.

16. **lyme**
July 11, 2007

    > # mathjunkie Says:
    > Sad to hear that Lubos has left Harvard.

    All the circumstantial evidence suggests it was the opposite that actually happened.

    Personally, I'm glad this long foreseen parting has at last happened.

    The Harvard tag gave Lubos a credibility he doesn't deserve and would never had gained by himself. His blog is full of lies, half truths and deliberately misleading information just about everything and everyone, from computers to politics, from physics to pedagogy, and everything in between.

    Right now, as far as his internet personna is concerned, he's just another nut with a blog, one among thousands, lost somewhere in Central Europe. Who could care less what he says?

17. **Joe**
July 11, 2007

    lyme,
    I agree with you. Lubos has become another Peter Woit.

18. **lyme**
July 11, 2007

> Lubos has become another Peter Woit.

That's what he would like to think… There's a long distance from Pilsen to Columbia, and not just geographically… Time will tell what he will become, it's still too early to draw conclusions.

But, as far as I'm concerned, the crucial difference between one and the other is that I'm able to answer your post because it's still there for me and everyone to read.

19. **woit**
    July 11, 2007

    Please stop it with the personally insulting comments, about Lubos or about anyone else.

20. **lyme**
    July 11, 2007

    As soon as I opened the "string theory blog" you cite, I knew there was something wrong… The photos there are just too good to have been shot by physicists…

21. **wow**
    July 11, 2007

    Arkani-Hamed will be joining IAS starting this fall. Apparently,
    the powers that be in Princeton think more highly of his work
    than you do, Peter.

22. **Peter Woit**
    July 11, 2007

    wow,

    Thanks for the news. Interesting to hear that the inner sanctum of what Polchinski describes as the "cult of monovacuism" will now include one of the most vigorous proponents of the anthropic multiverse. Susskind must be rejoicing at fresh evidence of the capitulation of resistance…

23. **IMHO**
    July 11, 2007

    In my humble opinion. From a political and marketing standpoint, Motl was a walking liability for both the department and university. The physics department, at arguably the world's most famous institution, housed a racist, sexist, attack-dog who would scream as loudly as he could to anyone and everyone….what an embarassment!

    I'm surprised, and somewhat disheartened, he lasted as long as he did.

24. **Peter Woit**
    July 11, 2007

    IMHO,

    Again, I wish people would stop it with the loaded characterizations of Lubos's personality.

    From what I hear, he can be quite charming personally, with the attack-dog screaming act restricted to his internet activities. Some people really don't do well when you give them a keyboard…

    It doesn't surprise me that Harvard put up with him so long (and for all I know, they were willing to continue to do so, with him being the one who decided he had enough of it there). Universities in the US, at least in principle, have a very strong prejudice in favor of not restricting free speech. My guess is that much of Lubos's problem was that he seemed to be putting all his time and energy into ranting on the blog, not doing research. If he had been producing impressive papers about string theory in between the ranting, I don't think he would have had much of a problem.

    What did surprise me was the apparent unwillingness of his string theorist colleagues to forcefully tell him that the kind of response to scientific criticism he was engaged in was both inappropriate and damaging to their interests, or to take any action to distance themselves from what he was doing. As far as I was ever able to tell, he felt he was expressing the views held privately by his colleagues, with the only difference between them and him being that he was more honest and willing to stick up for what he believed. I have some reasons to believe that he wasn't unreasonable to think this, in particular given a few of the anonymous Cambridge-based attacks I've been the target of.

    Unfortunately I think Lubos's conviction that anyone who criticizes string theory must be an incompetent idiot, with their criticism of string theory something morally reprehensible deserving of personal attack, is not unique to him, but shared by some influential people. When I first started publicly criticizing string theory, I was shocked by how many people told me they thought I was doing something very brave and foolhardy. There has for quite a long time been an ugly atmosphere in the particle theory community over this issue, and Lubos's behavior makes quite clear what the nature of the problem is.

25. **Ivone FitzGerald**
    July 11, 2007

    Hi Peter,

    I enjoyed reading your book and Lee Smolin's as well. I've just been reading your comments on the LHC, and do you know what? It's my considered prediction that there'll be a Higg's boson found within six months of the machine switched on in May next year, with considerable excitement. But come July 2009 – 10 there'll be string theory conferences saying where the hell are the predicted black holes?

    Will they still carry on with string theory then? Unfortunately, yes. They'll make excuses such as: The LHC wasn't powerful enough. Lets build one the circumference of the Earth and then we'll get a result. Dream on!

    Let's face it. Once the Higg's boson is proved to exist, it's a monumental waste of money building ever larger more expensive colliders for all they'll produce is yet more Higg's bosons. We're better off researching into the known anomalies produced by varying basic so called "constants".

    I'm not a physicist. I'm doing natural history and archaeology, but even I can see this hole that the physics community has dug itself into.

26. **Peter Woit**
July 11, 2007

Ivone,

I think it will be longer than that to find the Higgs. Even if all goes well, the machine won't start producing real data of any interest until July. And, if history is any guide, it may take quite a while to get the LHC working the way it is supposed to. Analyzing the data from these very complicated experiments is also a long and time-consuming business, and the Higgs is not the easiest thing to look for. Personally I think I would bet on a Higgs in 2010…

The string theorists aren't really predicting black holes, they don't expect to see them. That possibility is a remote one and doesn't happen in most popular versions of string theory.

What we're all hoping for is not just the discovery of the Higgs, but studying its properties and finding something unexpected, or finding not a Higgs, but something else that plays the same role. Depending on what the LHC sees, there may or may not be a convincing case for spending the money on higher energy accelerators.

27. **m.**
July 11, 2007

lyme, the distance from Central Europe (as well as from China, India and other places you look down upon) to Columbia is of the same order of magnitude as most places in the US – proved by many a faculty list.
Pilsen, in particular, is a nice city with more than thousand years of history, which has given us the Pilsner beer,
Skoda motor company, music of Smetana and goals of Pavel Nedved…

28. **Oles**
July 11, 2007

"From what I hear, he can be quite charming personally, with the attack-dog screaming act restricted to his internet activities."

Having known Lubos for the past few months in and out of the class room, I found him to be quite inspiring as a teacher and deeply considerate as a person. Of course on many non-physics topics he struck me, at times, as being hopelessly misguided or ill-informed. Yet I never recall him being arrogant and unwilling to listen. His web persona is something of a mystery to many. At any rate, his wonderful sense of humor, physical insights and, most of all, great kindness will be remembered by several of us.

29. **Arun**
July 11, 2007

Oles, that is good to know. Let us hope then he lands up at a place without an internet connection, so that the dark side find no expression.

30. **lyme**
July 11, 2007

> and other places you look down upon

I don't look down upon anything. I've been there, done that, and know exactly what I'm talking about. And yes, Czekia is at exactly the same distance from the Ivy League as Tennesee. But on the way out distances dilate very fast, much faster than on the way in. The clock is ticking quite fast for Lubos.

31. **Chris W.**
July 11, 2007

On the internet, nobody knows you're a dog.

On the internet, nobody knows you're really a nice guy.

32. **amused**
July 12, 2007

So Lubos finally made good on his threat to leave Harvard (and academia?)… it'll be interesting to see what his next move is. Maybe a job at his hometown uni, but considering the love and admiration he expresses on his blog for Czech president Klaus, and his interest in science-related political stuff in general, it wouldn't surprise if he has lined up some government job there. Perhaps we can look forward to the spectacle of future Education Minister Motl imposing string theory on the Czech high school curriculum… At any rate, good luck to the guy, he's been an entertaining character (modulo the repugnant sexist/racist stuff he sometimes spouts). Will be interesting to see if/how his blog style changes now that he is presumably "unleashed" and able to "enjoy basic human rights".

33. **Neville**
July 12, 2007

> something I've been working on for a very long time finally seems to be working out and I'm spending more time on that

Sounds like you're having a fine adventure. Good luck!

34. **mathjunkie**
July 12, 2007

Just had this thought:
If Peter hadn't pointed out the improper aspects of string theory (like being untestable), Lubos would not have fighted back and he would have more time doing research. He might have made breakthroughs and…then he would go to IAS…

But the above is just for fun. 🙂

35. **The Lone Haranguer**
July 12, 2007

Never mind all that – seen today's New Scientist?

36. **woit**

July 12, 2007

TLH,

Thanks, already on it. See next posting…

37. **milkshake**
July 13, 2007

Oles: If you are a team member in good standing, Lubos can be very nice to you. (But you are wrong about the sense of humor – Lubos likes the sarcastic put-downs but he laughs only to his own jokes, the jokes of his peers and to the slapstick kind of stuff.) A narcissic, brilliant and agressive guy with twenty chips on his shoulder is not the best kind of advisor that one can have in the grad school.

He seriously compares himself to Feynman and Jesus Christ (but not to Borat). He actualy wants is to be loved and admired by all the smart people while defeating the idiots who get in the way of progress.

38. **IMHO**
July 16, 2007

Peter Woit Said:

> It doesn't surprise me that Harvard put up with him so long (and for all I know, they were willing to continue to do so, with him being the one who decided he had enough of it there). Universities in the US, at least in principle, have a very strong prejudice in favor of not restricting free speech.

That's easy to say and well-and-good in principal, until it starts costing Harvard Corporation money…Prestige and image have value also.

Maybe I put to much faith in the ethics and morals of the physics community, but I can't imagine many condoning Motl's behavior (at least in private). You often mention most string theorists unwillingness to publicly *"take any action to distance themselves from what he was doing"*. That may be true, but a lack of public admonishment is not the same thing as approval.

Either way, at the end of the day it's hard to imagine any scenario where a Harvard Asst. Prof who *"decided he had enough of it there"* chooses to leaves Harvard and the American Scientific establishment behind to mingle with Borat.

39. **IMHO**
July 16, 2007

Actually, a simple google search gives a reasonably plausible explanation.

http://www.physicsbanter.com/particle-physics/79483-lubos-motls-forced-resignation-harvard.html

40. **woit**
July 16, 2007

IMHO,

I'm quite sure that the claims by Sarfatti that he was responsible for getting Lubos fired are nonsense. I would think that Lubos's colleagues would consider complaints from a source like Sarfatti a point in Lubos's favor, not a reason for firing him.

I don't claim that Lubos's string theory colleagues approved of how he was defending string theory, but I never saw any evidence at all that they disapproved of it enough to tell him to cut it out.

41. **IMHO**
July 16, 2007

Woit said:

> I'm quite sure that the claims by Sarfatti that he was responsible for getting Lubos fired are nonsense.

Oh yes, I'm sure those particular claims are nonsense.

What was of interest to me was the information that there was a forced resignation as far back as January. If true, it now seems that the Harvard Physics Department is not as morally and ethically bankrupt as they first appeared.

42. **milkshake**
July 16, 2007

The asset/liability ratio was decreasing for quite some time and finally even his suporters agreed that Lubos was not worth the constant aggravation.

I don't think Sarfatti can claim credit Lubos for himself – for example about 5 years ago there was a minor affair when Lubos discovered that a (politically-correct + pompous) lecturer of slavonic studies in Glasgow misrepresented his old czech degree as a Ph.D. – the degree in question was roughly equivalent something intermediate between MS and Ph.D. Lubos allerted Glasgow uni, called the lecturer a commie spy colaborator and suggested he should commit suicide. As a result the lecturer threatened a legal action and complained at Harvard. Lubos was reportedly on the occasion ordered by his superiors to take his extracurricular interests easy and to stay out of the spotlight. He managed, for about 3 weeks.

43. **Peter Woit**
July 16, 2007

milkshake,

I hadn't heard that part of the Lubos story before, presumably it has to do with this:

http://www.blisty.cz/art/9429.html

In Lubos's latest posting, it seems that I'm the villain of the latest Die Hard movie…

44. **milkshake**
July 16, 2007

Yeah, it is this stuff – for many months the same e-zine even had a free advert on its sidebar for the Motl's czech translation of the Elegant Universe and would post some Lubos articles there. You can imagine how it progressed from there.

45. **amused**
July 17, 2007

IMHO wrote: "…at the end of the day it's hard to imagine any scenario where a Harvard Asst. Prof who "decided he had enough of it there" chooses to leaves Harvard and the American Scientific establishment behind to mingle with Borat."

unless he/she is a free spirit who is also a fanatic and a bit of a nutter. Not everyone in this business is a careerist, and different people value different things. Just look at Perelman. Lubos has been writing for a long time on his blog about his distain for Cambridge and academia in general, so my bet is that it's more a case of him jumping than being pushed.

46. **IMHO**
July 17, 2007

amused said:

> my bet is that it's more a case of him jumping than being pushed

His entire self worth is wrapped up in an unwavering belief in his superior scientific intellect….and he chose to leave academia???

I'll take that bet any day of the week!

47. **anon.**
July 17, 2007

IMHO, you have got the facts a bit wrong. He was just an undercover secret agent working for an anti-string conspiracy, and his had to leave Harvard before his cover was blown (when his 'defense' of string theory had become obviously self-defeating).

48. **Chris Oakley**
July 17, 2007

It's true – Lubos was a gift to us String sceptics. He's going to be hard to replace.

49. **amused**
July 17, 2007

Ok IMHO, I'll wager US$100 on it (that he jumped rather than was pushed).
And Chris, I don't see the big difference between Lubos entertaining us from Harvard or from Czekia. Chances are he'll be even more entertaining from Czekia!

50. **Chris Oakley**
July 17, 2007

Amused,

It's not a question of that. The point is that now Lubos will just be another dissident like Nigel Cook, Jack Sarfatti, Thomas Larssen, Danny Ross Lunsford, Matti Pitkannen, Tony Smith, me and – for all I know – you. Much easier to ignore than a Harvard Assistant Professor.

51. **IMHO**
July 17, 2007

How do we determine the truth Amused?

52. **amused**
July 17, 2007

Chris,
You are of course right that the Harvard connection confers prestige, but, unless academia has become more of a joke than I realized, his Harvard position was a reflection of his status in the string community rather than the source of it. If he continues (or resumes) research in Czekia and it goes well, I don't see why his status should change. There are other examples of prominent string theorists working in offbeat places, e.g. Berkovits in Sao Paulo.

IMHO,
At some point stories of what happened will emerge, and if these converge to a fixed point re. jumped or was pushed then I think we can assume it's the truth. If they don't converge it will be more difficult. I suggest we ask Peter to adjudicate. Do we have a bet?

53. **gunpowder&noodles**
July 17, 2007

Gentlemen, gentlemen: False dichotomy alert. If you think that there is a sharp distinction between push and jump then you don't know much about academic politics.

54. **IMHO**
July 18, 2007

Amused,

No, I don't think so. It seems kind of Schadenfreude.

55. **amused**

July 18, 2007

IMHO,

Ok, but next time you say “I’ll take that bet any day of the week!” bear in mind that someone might actually take you up on it! 😉

56. **Vacuum**
July 23, 2007

Motl had a publication record of 23 papers with only 1253 citations to them (and no publications in the last year). I don’t think Harvard would hire a postdoc with such a record, let alone award tenure to Assist Prof (to compare, Clifford Johnson’s record is 75 papers with 2724 citations). So I think he was denied tenure at Harvard.

57. **Peter Woit**
July 23, 2007

Vacuum,

Clifford Johnson is quite a bit older, getting his Ph.D. nearly 10 years before Lubos, so you can’t compare those numbers. They’re more than acceptable for a Harvard postdoc, although his recent lack of publications might be a problem. Again, he hadn’t been at Harvard in a tenure-track position long enough to come up for a tenure decision in the usual way.

String Theory: The Fightback

July 12, 2007
Categories: Uncategorized

The public perception of string theory has definitely changed over the last few years, with the latest evidence this week's cover story in New Scientist, which begins:

> It's the theory everyone loves to hate.

The article (available fully only to subscribers, I fear) is entitled String Theory: The Fightback, and its story line is that, because of all this criticism, after nearly 25 years of work, finally:

> string theorists themselves have realised they must find ways to put their models to the test. They may still be far from being able to observe a string in a laboratory, but experiments planned for the near future – and even one currently under way – could provide tantalising evidence either for or against string theory…
>
> Now the string community is fighting back by devising creative, if indirect, ways to look for signs of strings – from hidden dimensions to ripples in space-time and other potential signatures of a stringy universe. The time has come to put string theory to the test…
>
> Critics should take heed. Experiments now show that string theory may be testable after all. One study at a time, string theorists seem to be homing in on models that will make specific, falsifiable predictions.

What follows is the usual misleading hype of bogus "tests" of string theory, of the sort I've written about extensively here. They are:

- The networks of cosmic superstrings promoted by Polchinski.

For one of my postings about this, see here. More than three years ago Polchinski and the KITP at Santa Barbara issued a press release about this trumpeting the idea that such superstrings could be observed by LIGO "over the next year or two." The problem with this kind of "test" of string theory is that you can easily come up with string theory models that produce lots of cosmic superstrings (already falsified), no observable amount of cosmic superstrings (can't use to test string theory), or precisely the amount of cosmic superstrings such that no hint of them would be seen until now, but there would be networks of the things lurking just below the threshold of measurability, to be discovered by the next generation of experiments (highly unlikely, pretty much pure wishful thinking). Polchinski has now stopped talking about LIGO and a year or two, and instead is promoting measurements of pulsars and five to ten years:

> According to Polchinski, though, our best bet for observing gravitational waves emanating from strings is to use pulsars. A pulsar is a rapidly spinning neutron star that fires out a beam of electromagnetic radiation as it rotates, like a lighthouse. These flashing beacons act as some of the most accurate clocks in the universe, and a gravitational wave rippling between a pulsar and Earth would disturb the otherwise precise timing of the pulses arriving here. The most likely cause of such fluctuations would be black holes colliding, but waves from strings would yield a unique timing pattern that would make them stand out. "Over the next five to 10 years," Polchinski says, "these will probe the most interesting models."

My colleague Brian Greene is quoted about this, making the essential point about this kind of "test":

> Sure, catching sight of a cosmic string would be a boon for string theory, but is there any observation that would serve as a death knell? For many sceptics, it's not that string theory is so hard to prove correct that puts them off, but rather that you can't falsify it. "I'm not aware of any test that if it fails will prove string theory wrong," says physicist Brian Greene of Columbia University in New York. "That's a real headache. You'd like to have a situation where you have a prediction, and if it's right the theory is right, and if it's wrong the theory is wrong."

- Quoting this recent paper, supposedly "according to Linde and Kallosh, no string-based inflation model can possibly create detectable gravity waves".

The problem is that the quoted paper doesn't actually say that. Here's the paper's concluding paragraph:

> However, a possible discovery of tensor modes may force us to reconsider several basic assumptions of string cosmology and particle phenomenology. In particular, it may imply that the gravitino must be superheavy. Thus, investigation of gravitational waves produced during inflation may serve as a unique source of information about string theory and about the fundamental physics in general.

Given the wealth of possible string theory scenarios, I have no doubt that if an imprint of gravity waves is found in the CMB, there will be string theory models that would "predict" it. As for the idea that not seeing such gravity waves would be evidence for string theory, here's what Glashow has to say:

> Not everyone thinks these tests will be useful, however. "Not seeing something is hardly evidence for string theory," says Nobel laureate Sheldon Glashow of Boston University, Massachusetts, an outspoken critic of string theory. He feels that such a result would mean very little. "String theorists are very wise. They can come up with a way to explain anything." String theory is simply not testable, he says. "There are an enormous number of string theories and they describe zillions and zillions of universes, none of them observable in any way. It sounds to me like angels dancing on the head of a pin."

- String theory is being tested at RHIC and, conjecturally AdS/QCD makes predictions about strong interaction physics.

> "We're still very far from being able to say, here is the exact string theory that describes QCD," Susskind says. "But the connections that show up between nuclear physics and string theory are fascinating. Sceptics won't consider this evidence for string theory, but nuclear physicists will use string theory and in time discover how accurately it describes these experiments."

What Susskind is neglecting to mention here is that these are tests of whether string theory is a useful way to do calculations in an already tested theory of the strong interactions, and has nothing to do with the question of testing the idea of string theory as a unified theory of quantum gravity and particle physics.

Also in New Scientist, there's a story about Neil Turok's recent talk at PASCOS entitled "Is the Cold Spot in the CMB a Texture?" which links to the string theory article. Evidently there's a patch of CMB where the temperature is anomalously low (the probability of this happening supposedly being 2 percent) and one can speculate that this may be due to a topological defect of some kind. Amusingly, the online version of the New Scientist article includes an interpolated editorial comment that someone forgot to take out before publication:

> Turok presented the findings at a conference on particles *Hi Anil. This threw me a bit. We say the team noticed previous work by Turok, and then before we know it Turok is the one presenting the work. When did he join them*, strings and cosmology at Imperial College London last week.

Comments

1. **Coin**
   July 12, 2007

   Evidently there's a patch of CMB where the temperature is anomalously low (the probability of this happening supposedly being 2 percent) and one can speculate that this may be due to a topological defect of some kind.

   The article is available only to subscribers. I was wondering– will it be possible to more easily tell whether or not this cold spot is coincidence after any particular future CMB observation experiment?

2. **Jonathan Vos Post**
   July 12, 2007

   "…New Scientist… interpolated editorial comment that someone forgot to take out before publication:

   Turok presented the findings at a conference on particles 'Hi Anil. This threw me a bit. We say the team noticed previous work by Turok, and then before we know it Turok is the one presenting the work. When did he join them…"

   Or, this tunneled through from elsewhere in the multiverse. Bound to happen now and then. Duality with Boltzmann brains. Or something else within epsilon of unfalsifiable, in the Landscape.

   "What's the frequency, Kenneth?"

3. **joe**
   July 13, 2007

   M. Disney (observational galactic astronomer) was an "alarmist/whistle-blower" to cosmology back in 2000. It seems to parallel P. Woit/"Not Even Wrong", in that large-scale (cosmological) questions are cursed with not having the proper data. Without data, you have only theory..hence "religion".

   The Case Against Cosmology

   http://arxiv.org/pdf/astro-ph/0009020
   [interesting references to particle physics & LHC..who can do actively do "experiments". Astronomers can only passively observe]

   Is not the below a striking parallel to the ST quagmire?

   > Given statements emanating from some cosmologists today one could be forgiven for assuming that the solution to some of the great problems of the subject, even "the origin of the Universe" lie just around the corner. As an example of this triumphalist approach consider the following conclusion from Hu et al. [1] to a preview of the results they expect from spacecraft such as MAP and PLANCK designed to map the Cosmic Background Radiations: ". . .we will establish the
   > cosmological model as securely as the Standard Model of elementary particles. We will then know as much, or even more, about the early Universe and its contents as we do about the fundamental constituents of matter".
   >
   > We believe the most charitable thing that can be said of such statements is that they are naive in the extreme and betray a complete lack of understanding of history, of the huge difference between an observational and an experimental science, and of the peculiar limitations of cosmology as a scientific discipline. **By building up expectations that cannot be realised, such statements do a disservice not only to astronomy and to particle physics but they could ultimately do harm to the wider respect in which the whole scientific approach is held. As such, they must not go unchallenged.**
   >
   > It is very questionable whether the study of any phenomenon that is not
   > repeatable can call itself a science at all. It would be sad however to abandon the whole fascinating area to the priesthood. But if we are going to lend this unique subject any kind of scientific respectability we have to look at all its claims with a great circumspection and listen to its proponents with even greater scepticism than is usually necessary. This is particularly true when the gulf between observers and theoreticians is as wide as it usually is here. Either side may be more inclined to accept the claims of the other than they should. As an extra-galactic observer addressing a mostly theoretical audience I want to emphasise the very many caveats that should always be attached to the observational side of this field

   The First Crisis in Cosmology Conference

   http://www.ptep-online.com/index_files/2005/PP-03-03.PDF

4. **anon.**
   July 13, 2007

   New Scientist uses the same article 'template' for lots of pseudoscience where old discredited ideas are being worked on, provided that the ideas seem exciting. It did an article some years back which was pretty much the same but about 'cold fusion'.

   (The synopsis for that PR stunt was something like: 'Everyone loves to hate cold fusion because it was overhyped a bit at first, but now people are starting to get tantalizing glimpses of real physics from it, blah, blah…', followed by some experimental work which gave results of no statistical significance, and a lot of wishful thinking from experts. I think that issue sold out fast. New Scientist should be commended for knowing how to write copy that sells. Pity it's totally misleading, but that's sci fi for you: it's popular, but it's also scientific fiction until some solid evidence shows up.)

   I'm just waiting for New Scientist to write a defense of creationism:

   'creationists themselves have realised they must find ways to put their models to the test. They may still be far from being able to observe the act of creation in a laboratory, but experiments planned for the near future – and even one currently under way – could provide tantalising evidence either for or against creation…'

   😉

5. **Nigel**
   July 13, 2007

   "String theorists are very wise. They can come up with a way to explain anything." – Sheldon Glashow

   I'm surprised to see such a sarcastic comment from a such Nobel Laureate like Sheldon Glashow being quoted. String theorists are (correct me if I'm wrong) just human beings, so presumably they have feelings and don't like sarcasm (unless, of course, the mental ability of string theorists to believe that the world has 6/7 extra dimensions without evidence, is linked to a genetic mental condition which makes them invulnerable to ridicule). Those string theorists who believe that they reside in the universe where the anthropic principle has selected the existence of the *Not Even Wrong* blog to occur on the internet, might feel upset and hurt by such sarcasm. But many of the other string theorists, believing that they spread over the segment of the landscape of the multiverse where physics is fairly close to our own but not quite close enough to include this blog, might remain deluded, if they really exist.

6. **Peter Woit**
July 13, 2007

Joe,

I'd rather people discuss mainstream cosmology on blogs run by cosmologists, but I'll just point out that, whatever its problems, mainstream cosmology is an extremely solid and respectable science compared to "string cosmology".

7. **M**
July 13, 2007

Joe, the "Case against Cosmology" is just a few months older than "String theory: An Evaluation" (physics/0102051). In these years cosmology made enormous progress, while the main development in strings has been the rediscovery of the anthropic principle.

8. **Bee**
July 13, 2007

sadly, the criticism you raise above holds for a lot of other theoretical approaches as well. I understand you are mostly concerned about string theory, but the problem is much more general than that. if I look at the arxiv, it seems it has become quite normal to cook up more or less meaningful 'theories' or 'models' that have a lot of 'motivation' but no connection to reality whatsoever (despite an impressive amount of calculations and references). now the fashion changes to "needs phenomenology". being a phenomenologist, I certainly don't mind, but in many cases there either is no phenomenology (a 'qualitative' prediction is no prediction, one can always shift parameters out of experimental range, so what's the point), or there is phenomenology but no sensible model (i.e. one that is in disagreement with the standard model, or worse, one can't be sure whether it is in agreement or disagreement with anything), or there is phenomenology and a model but one can't learn anything from it (e.g. there must be one hundred different 'models' that 'motivate' why a breaking of Lorentz symmetry is 'natural to expect' – so if we measure it what do we learn from that besides that it is broken?).

9. **Peter Woit**
July 13, 2007

Bee,

I agree that this is a more general problem, but the string theory situation still seems to be a unique one. There's lots of dubious theoretical work out there (one reason being that coming up with a good new idea in this field is extremely hard), but most of it is pursued by smaller groups of people with lower profiles in the community. What is going on here is outrageous overhyping and misleading of the public by a rather large group of people at the absolute top of the academic hierarchy. I don't know of anything quite analogous in other subjects.

It's interesting to note that the people New Scientist found willing to promote bogus "tests" of string theory are mostly the same ones promoting the anthropic landscape.

10. **Bee**
July 13, 2007

Hi Peter,

but the string theory situation still seems to be a unique one

I agree. I am just saying one doesn't cure the problem by demonising one field – there is the danger such unfortunate distractions from questions relevant to physics just shift somewhere else.

Best,

B.

11. **publius**
July 13, 2007

I just came accross a quote by chilean writer and filmaker Alejandro Jodorowsky (el Topo, Sacred Mountain, 'El Incal' comics (with Moebius)) that beautifully resumes the situation with string theory, the landscape and what it is doing to fundamental physics:
" Failed Theory:
A Philosopher who couldn't walk because he stepped on his beard, cut his feet"
It is interesting to notice the string-like nature of long beards…

12. **island**
July 13, 2007

Coin asked:
The article is available only to subscribers. I was wondering– will it be possible to more easily tell whether or not this cold spot is coincidence after any particular future CMB observation experiment?

This is already the second confirming indicator of the reality of the anomalies, and it's not just a coincidence when the motion of the Earth around the Sun, (the ecliptic), also traces-out the goldilocks zone of the observed universe.

So, give em another 20 or 30 years to explain it away without giving equal time to the most apparent indication of the evidence, since that's the way science is done now a-days!

http://arxiv.org/PS_cache/arxiv/pdf/0704/0704.0908v1.pdf
Extragalactic Radio Sources and the WMAP Cold spot

> To create the magnitude and angular size of the WMAP cold spot requires a ~ 140 Mpc radius completely empty void at z greater than or equal to 1 along this line of sight. This is far outside the current expectations of the concordance cosmology, and adds to the anomalies seen in the CMB.

Adds what?… a greater need for more time and excuses not to introduce the smoking gun into evidence?

13. **Peter Woit**
July 14, 2007

Coin,

The rest of the article says that Turok presented calculations about what the next generation Planck experiment should see in the CMP if these topological defects really do exist.

All,

Please take general attacks on and discussion of the conventional cosmological model elsewhere. I'm not an expert, not very interested, and not willing to moderate or host a discussion of this topic.

14. **jkowalskiglikman**
July 14, 2007

Hi,

Perhaps my question is a bit out of topic, but I am wondering what "quantum gravity" means in the context of string theory. What is being quantized? How one gets the semiclassical limit? What is the conceptual relation between quantized microscopic degrees of freedom and the semiclassical description in terms of gravitational field? Does string theory incorporate equivalence principle? Is it any direct way to get any prediction concerning, say, orbits of planets in Solar System and string corrections.

All these must be a textbook stuff, but I could not find any clear discussion.

Thanks

J

15. **Peter Woit**
July 14, 2007

J.

Unfortunately this is pretty much off-topic, and full answers to the questions you ask have long answers. You might do better to try this question on the blog of a string theorist like Jacques Distler or Clifford Johnson. These questions are addressed in the standard string theory textbooks, although the answers are confusing, largely because while it is clear what string theory is supposed to be for strings propagating on a fixed background, string theorists want to use this to deal with varying backgrounds.

16. **mclaren**
July 22, 2007

New Scientist's article about string "theory" proved particularly disappointing. The weasel words came thick and fast — "**may** allow for a test of string theory" and "**could** lead to verification of the theory." Same kind of deceptive wording you find in a prospectus for a dodgy hedge fund.
Why is it so difficult for reporters to simply write "string theory has not yet made a single falsifiable prediction"?

17. **anon.**
July 22, 2007

Mclaren, reporters must write what sells the paper or their editors won't publish it.

Journalism, even science journalism, is a little bit different from the scientific ideal.

There's also a sense of groupthink involved. New Scientist is not the only journal which reports fashion rather than real news.

At the end of the day, journals are there to put food on the tables of the journalists and editors who produce them. If they reported news that doesn't excite the crowds into buying it, they would lose readers, prestige, advertisers, distribution, prizes awards for popular science journalism, massive salaries, etc. (Plus, they would risk being strung up by all those who believe string is sacred writ.)

18. **Vladimir Trifonov**
July 22, 2007

(A part of) My paper "GR-friendly description of quantum systems"
(IJTP, DOI 10.1007/s10773-007-9474-3, http://www.springerlink.com/content/w3175m02836610t4/?p=e2f0a2261a4248e69cdb5eb78668af77&pi=2)
may be of interest to members of this community.
It is also available at http://members.cox.net/vtrifonov/ (PDF).
Best regards,
Vladimir

19. **mclaren**
July 23, 2007

With regard to string theory, Dr. Woit remarked:
What is going on here is outrageous overhyping and misleading of the public by a rather large group of people at the absolute top of the academic hierarchy. I don't know of anything quite analogous in other subjects.

Eh?
You don't?
I certainly do. Indeed, the examples have grown so flagrant it's hard to overlook 'em. "Hard" AI has been guilty of "outrageous overhyping and misleading of the public by a rather large group of people at the absolute top of the academic hierarchy" for at least 50 years. In 1967 tenured the Princeton computer science "genius" David Gelernter confidently predicted "Within 10 years, the best mathematician in the world will be a computer." In 1974, Marvin Minsky, head of the MIT AI Lab, predicted "human-level" intelligence from computer programs by the early 21st century,. and superhumanly intelligent AIs shortly thereafter. In 1984, Douglas Lenat (leader of the CYC project) predicted that by 1994 CYC would be going online and reading online text independently in order to increase its knowledge base.

None of these wildly overhyped predictions have come close to reality. Meanwhile, all the most respected AI researchers now admit AI has hit a "dead end" and call AI "brain dead" and "a degenerating research program."

For an excellent overview of the pervasive failure of "hard" AI, see:
http://www.skeptic.com/the_magazine/featured_articles/v12n02_AI_gone_awry.html

In his 1959 article "There's Plenty Of Room at the Bottom," Richard Feynman inaugurated the idea of nanotechnology, a subject expanded to science-fictional lengths in K. Eric Drexler's 1987 magnum opus *Engines of Creation*. Despite 20 years of incessant hype, Drexler has produced not one single scientific advance which would bring his fantasies of molecule-sized computers and "assemblers" closer to reality. In fact, Drexler hasn't even done any basic research — instead, he's spent all his time doing PR.

Undeterred by these unpleasant realities, computer science professor Vernor Vinge has confidently predicted "The Singularity," an alleged explosion of technology in which "hard" AI combined with Drexlerian nanotechnology and genetically engineered supermen to allow science fictional scenarios like mind uploading into computer, immortality, people with IQs of 20,000, nanomachines capable to tearing apart any object and rebuilding it atom by atom into any desired form.

Despite all this wild hype (including truly crazed predictions by spinsmeisters like Ray Kurzweil and Hans Moravec that "hard" AI is not only possible, but imminent, and that most of the people in his audience will live long enough to upload their minds into superhumanly intelligent computers), signs of actual progress in genetically engineering supermen, or producing human-level intelligent AI programs, or building Drexlerian nanomachines, have proven impossible to discern.

This does not seem to have daunted any of the spinmeisters, who continue to trumpet the alleged imminence of The Singularity "any day now" even while researchers in computer science and materials science and molecular biology have run into roadblock after roadblock and brick wall after brick wall. Indeed, the more research molecular biologists do into the genetic code, the more their results challenge previouly accepted models of how DNA works:
http://www.bioresearchonline.com/content/news/article.asp?docid=e87b8231-8a15-43bc-8ba4-bb7b65b028ee

Rather than being on the brink of genetically engineering mythical supermen with IQs of 20,000, it seems more likely that researchers are now finding out that almost everything we thought we knew about the genetic code is wrong, and we must go back to square one.

Jaron Lanier has written about the outrageous overhyping and misleading being done by the bizarre quasi-religious "rapture of the nerds"-style cult that has developed around these fantasies of "hard" AI that's supposedly "right around the corner" and purportedly imminent genetic engineering to enhance human traits like intelligence and allegedly soon-to-be-built nanomachines able to revive the dead and literally turn water into wine:
http://www.edge.org/3rd_culture/lanier/lanier_index.html

Lanier calls this crackpot cult "cybernetic totalism," and he points out that it has taken over surprisingly large parts of previously respectable computer science and materials science and molecular biology.

I'm seeing a direct parallel here between the increasingly wild hype and ever more bizarrely hubristic predictions (*"I expect the Riemann Conjecture will be proved within several years as a baby example of string theory"*) of string theory and the increasingly wild hype (*mind uploading! immortality! superhumanly smart AIs! genetically engineered uebermenschen!*) and ever more bizarrely hubristic predictions (*people in this audience will live long enough to live forever! — Ray Kurzweil*) touted by the crackpot cult of the Singularitarians.

For that matter, I see a direct parallel twixt the radical disconnect from reality we observe in American foreign policy (*"We have to fight them over there so we don't have to fight them over here"*), and these bizarre cults of string theory and "hard" AI and The Singularity in the sciences which have similarly cut themselves off from reality and now float into a fantasy realm of multiverses and branes, superhumanly smart AIs and nanoengineered diamond-based power plants the size of a matchbox allegedly able to power a city.

It's as though — in a wide variety of different areas of American life — otherwise sensible rational professionals (computer science professors (*Vernor Vinge's and Hans Moravec's delusion of The Singularity*), theoretical HEP physicists (*string theory*), economists (*The Laffer Curve*), foreign policy analysts (*Project for the New American Century*), materials scientists (*Drexlerian nanotechnology*), molecular biologists (*genetic engineering to enhance traits like intelligence which have never even been successfully defined or definitively measured*)) have drifted off into la-la land and now live in a fantasy world of their own imagining, immune to inconvenient facts and inappropriate logic.

Far from being an isolated example, string theory seems to me paradigmatic of a much wider dysfunction and pathology in American intellectual life.

20. **dave tweed**
July 23, 2007

`Meanwhile, all the most respected AI researchers now admit AI has hit a "dead end" and call AI "brain dead" and "a degenerating research program."' This is a difficult case because the scope of the field referred to by AI tends to be understood differently by different people: does, e.g., computer vision research fall within its purview? I think the hardcore AI people claim it as a branch of AI – partly so they can point to some of the deployed examples – whilst most people working in vision don't consider it AI. So there are lots of vigorous and, I think, non-degenerate research programs in areas that some might claim as AI. In addition, lots of computer research does get evaluated on (semi-) objective problems and is generally improving. Most is still a long way from being reliable and safe enough for real world deployment, but there is meaningful, measured progress.

The key differences from *Peter's view* of string theory is that AI people don't claim anything but a hard AI approaches to problems is not going to work, and there are lots of people working at problems hard AI "thinks it owns" using different techniques, eg, simple formula learning in data mining problems. (I've emphasised what I think is Peter's view just because I don't have the knowledge to know if he's right.)

21. **mclaren**
July 23, 2007

Dave Tweed makes some excellent points. However, the boundary lines twixt cosmology and HEP are fairly blurry too, so if we use your criteria a good argument could be made that "lots of people" in cosmology are actually working on string-related physics.

The problem with playing these kinds of word games is that they obscure everyday commonsense realities. Namely, sensible people know there's a whopping big difference between "hard" AI and machine vision, just as sensible pople know there's a whopping big difference between string theory and cosmology.

For one thing, cosmologists talk about things that can be ***measured***. And cosmologists construct models that can be ***falsified***.

Moreover, few string "theorists" have claimed to my knowledge that it's the only approach that ***can*** work – instead, they're claiming it's by far the most promising approach. "The only game in town" doesn't mean the only possible approach, just by far the best one in the judgment of the string supporters.

LQG research continues, just as artificial vision research persists in computer science indepddent of GOFAI. So Dave Tweed's remarks on that score constitute a straw man.

What Dave Tweed's verbal calisthentics tend to obscure, I think, is the hard cold fact that for both "hard" AI and string theory, it's clear to any reasonable person that these approaches have broken down. They're just not working. There are no practical results in the real world, and no sign of any path that would lead to practical results in the real world.

This seems true of other fields as well. Merton & Scholes' 1997 Nobel Prize followed by the bankruptcy of Long Term Capital Management, a hedge fund that used their economic theories to allegedly eliminate risk in investing but went broke instead, offers a prime example. By any definition Merton and Scholes qualify as the "absolute top of the academic hierarchy" in economics by reason of their Nobel Prize. The disastrous collapse of LTCM under the guidance of their economic theories surely fits the definition of "outrageous overhyping and misleading of the public."

Merton and Scholes represent only the tip of the iceberg. We've been seeing an awful lot of "outrageous overhyping and misleading of the public by a rather large group of people at the absolute top of the academic hierarchy" over the last 15 to 20 years, and in academic fields far outside physics. Dave Tweed's verbal gymnastics about the exact definition of "hard" AI tends to obscure this valid and significant point.

22. **Me**
July 24, 2007

«This seems true of other fields as well. Merton & Scholes' 1997 Nobel Prize followed by the bankruptcy of Long Term Capital Management,»
— — — — —
Maybe I'm just nitpicking but Merton & Scholes didn't win a Nobel prize in Economics, they couldn't since that prize doesn't exist.

They won the 1997 Swedish Bank prize in Economics in memory of Alfred Nobel. Regardless of that, it's considered the pinnacle of an economist's career. Nice info I didn't know.

23. **mclaren**
July 24, 2007

You're quite right about the Swedish prize for economics — Alfred Nobel never stipulated an award for economics. Thanks for the correction.

24. **Chris Oakley**
July 24, 2007

Er … excuse me, but Merton and Scholes were most definitely not responsible for the LTCM disaster, directly or indirectly … they were merely roped in to provide some kind of academic imprimatur to a venture that was always going to be risky. They had virtually no part on the day-to-day decision making, which was done by Meriwether and a handful of maths PhDs he brought with him from Salomon Brothers. The Nobel-equivalent prize they won was for developing an option pricing formula based on the notion that stock prices undergo a random walk — demonstrably not what happens but their formula nonetheless proved useful as a kind of reference point.

25. **dave tweed**
July 24, 2007

In Mclaren's original comment, the "hard" modifier to AI wasn't consistently there so it wasn't clear whether you were claiming "hard AI" or "AI" was dead. I don't like the name AI because it's not agreed whether it's merely generating results which humans generate by intelligence, or the computer should be doing this "in an intelligent way". However, there are lots of institutes around the world that have "AI" in their names that have people working on both "hard AI" and other problems so we're stuck with it. I was more pointing out to non-computer science readers that the situation isn't quite as black-and-white as you suggested, and certainly your viewpoint is not universally held.

Mclaren wrote "I think, is the hard cold fact that for both "hard" AI and string theory, it's clear to any reasonable person that these approaches have broken down." This may well be true. However, my understanding of the criticism of string theory is that string theorist's totally dominate *all* theoretical particle physics and quantum gravity research. This isn't the case for "areas of research that some people could be call AI": lots of people doing non-"hard AI"-approaches are working on some of these problems. It's certainly not more difficult to get an academic job working in these areas without being a "hard AI" person, something claimed to be difficult in theoretical physics for non-string theorists.

Basically, I understand Peter's objections to ST be based both on technical problems in their program and their excessive sociological power and abuses of it. The first may be true of "hard AI" but the second definitely isn't because in academic circles they don't have much power to abuse. Of course the general media is a different thing because talking about immortality or the singularity is going to get journalists' attention much more than talking about simple model learning for data mining against credit card fraud prevention, etc. I think this says more about journalists than it does about academic AI though.

26. **Who**
July 24, 2007

> However, my understanding of the criticism of string theory is that string theorists totally dominate all theoretical particle physics and quantum gravity research…
>
> Basically, I understand Peter's objections to ST be based both on technical problems in their program and their excessive sociological power and abuses of it.

Dave, this does not bear on your argument concerning Artificial Intelligence, but is merely a point of information.

From the statements of European non-string quantum gravitists, and my own observation of current research, I gather that the string dominance and abuse of sociological power which you mention is less of a problem **outside the US**.

A German PhD student now in the UK recently referred to the situation in the US as "extreme". I believe string virtual monopoly is primarily a millstone around the neck of US theory research, and only secondarily worldwide. In his expressed view, funding for non-string QG study and research is satisfactory in Europe and the UK.

What we are seeing is a kind of "brain drain", as new PhDs in quantum gravity migrate out of the US. A 2006 Penn State PhD won a European (Curie) fellowship and went to postdoc in the UK. Another recent Penn State PhD went to Canada. Penn State has the only non-string QG group in the US (more than one faculty) and is the only place in the country regularly turning out non-string QG physicists.

String has such a stranglehold on theory research support that these young people have limited options in the US. No matter how brilliant the trackrecord they have to leave the US to continue in their chosen line of research. But these are precisely the people one would want to keep if one has any hope of diversifying research in US departments!

The ESF (European Science Foundation) started a special QG arm in 2006 which is already well-funded and active.
http://www.maths.nottingham.ac.uk/qg/
US postdocs are currently working in Utrecht, Marseille, several places in Canada, and the UK.

So when one speaks of the crippling of theory research due to abuse of excess power, one should probably qualify that by saying that it is progress in the US that is retarded—and not necessarily elsewhere.

27. **mclaren**
July 25, 2007

Er…excuse me, Chris Oakley, but your claim is not supported by the available evidence:

Scholes's and Merton's involvement in LTCM has led commentators to ask whether the company's failure reveals basic errors in the assumptions underpinning their work with Black on option pricing. Dunbar, for example, says that these assumptions are 'flawed' – a common conclusion in discussions of LTCM. If Dunbar and the others are correct it would be a matter of real import, given how central these techniques have become to the global financial system. But the extent of the technical dependence of LTCM's trading on Black-Scholes-Merton reasoning is unclear until there is more information in the public domain.
http://www.lrb.co.uk/v22/n08/mack01_.html

Merton himself was interviewed on camera in the PBS Frontline special "The Trillion Dollar Bet." He himself states on camera that LTCM consittuted a test of his options pricing theory method of eliminating risk from investments. He went on to waffle and hedge that it was unclear whether the theory was wrong, or whether the boundary conditions were violated and thus it wasn't a valid test.

The assertion that Merton & Scholes had nothing to do with LTCM's collapse is the standard rewriting of history we find with all these kinds of huge failures nowadays. Ronald Reagan wasn't a senile crackpot who hired an astrologer to time his White House meetings, he was The Man Who Won the Cold War. Merton and Scholes weren't actually invovled with LTCM, they were jsut there for publicity purposes as window dressing. Ten years or so down the road we'll start hearing that the Iraq war wasn't a giant debacle but a huge success and papers will start appearing showing graphs to prove how marvellous the Iraq mess has been for the Palestianian peace process,, how much it has improved the Middle East, and so on. Standard stuff.

Merton was responsible not only for inventing the Black-Scholes formula, but also for founding LTCM
http://www.portfolio.com/views/blogs/market-movers/2007/05/22/bob-merton-sees-no-more-market-panics

Black, Scholes and Merton's solution to the problem was far-reaching, but their basic idea was simple and elegant. They showed how to construct a 'replicating portfolio': a continuously adjusted set of investments in both the underlying asset and government bonds or cash that would have exactly the same pattern of returns as an option. In an efficient market, the price of the option has to equal the cost of the replicating portfolio. If those prices diverge, there is risk-free profit to be made…
http://www.lrb.co.uk/v22/n08/mack01_.html

That's the explicit model on which LTCM claimed to have based their investments. Draw your own conclusions.

28. **Peter Woit**
July 25, 2007

mclaren,

This discussion of the problems of LTCM is both completely off-topic (and, from the little I know based on reading a book on the subject and talking to people in that industry, pretty far off-base). I'd really like this blog to stick to topics in math and physics I'm competent to moderate, the kind of discussion you want to engage in belongs on a very different blog.

29. **Chris Oakley**
July 25, 2007

Can't argue with that – but McLaren, you'll have to give me an e-mail address if you want to continue the discussion. Your comments, BTW, suggest that you have never worked in the business. I have.

30. **Ronald Mirman**
July 26, 2007

Physicists believe that if their theory disagrees with experiment they can just say it hasn't been possible to test it yet. Yet string theory has made a testable predication: the dimension. And that is wildly wrong. Moreover it has long been known that physics would be impossible in any dimension but 3+1 (which it totally uninteresting since it agrees with reality). For proof see my OAIU book in the booklist in my blog, which also has discussion of many other things wrong with string theory. Science blog
impunv.wordpress.com
or
impunv.blogspot.com

Political blog
randomabsurdities.wordpress.com

The politics should be ignored except by those who like nasty remarks about George Bush.

Carroll and Johnson on Bloggingheads

July 14, 2007
Categories: Uncategorized

There's an interesting exchange today between Sean Carroll and George Johnson on bloggingheads.tv. They cover a range of topics, including the controversy over string theory and the role of blogs. Johnson describes his time last fall at the KITP, telling about drinking very expensive Scotch with Steve Shenker, and many discussions with the string theorists there who were concerned that they were getting bad publicity and didn't know what to do about it. Some were concerned that Lubos Motl was not exactly representing them well.

Sean gives a fairly standard defense of the landscape ("maybe it really is the way the world works"), with no discussion of the main problem with the landscape, that it shows no signs of making falsifiable predictions that would make it legitimate science. Some other points of the discussion include speculation about "what if Feynman and Gell-Mann had blogs?", and Sean's analogy of the string/LQG debate with mainstream/"heterodox" academic economics.

Update: Sean has his own posting about this.

Comments

1. **Chris Oakley**
   July 14, 2007

   Well – it's over an hour, but I saw it through. Once again, we discover that Sean does not care whether the String Theory Landscape is ugly or not, he just cares about whether it is *right*.

   With all due respect to Sean, this is not the sort of view that a scientist should take. "Right" and "wrong" imply certainties, a luxury never afforded to the scientist. There are only good explanations and bad explanations. Invokes the Anthropic Landscape being a *bad* explanation, one should, at that point, or, maybe even before, be seeking something better, and if this involves discarding twenty years of work, then so be it.

2. **SnarkFest**
   July 14, 2007

   A mildly fanciful elaboration:

   "Maybe it really is the way the world works. If so, we'll never be able to *test* this entrancing conjecture. We'll have to content ourselves with asserting it, and reveling in the irrefutability of the assertion. It will be the greatest achievement of the human mind since G. W. F. Hegel proved that the solar system contains seven planets."

3. **mr.M.**
   July 15, 2007

   I'm glad this debate happened and i wish there will be more in future. It's more interesting if debaters don't agree and they debate the issue.

4. **Chris Oakley**
   July 15, 2007

   > "…It will be the greatest achievement of the human mind since G. W. F. Hegel proved that the solar system contains seven planets."

   Well … just get them to de-list Uranus and Neptune, just like they have already done for Pluto, and he will have been right!
   The S.T. equivalent, I suppose, would be Susskind (or whoever) altering the definition of the term "science" to allow the Anthropic Landscape to be respectable.

5. **AGeek**
   July 15, 2007

   Two related questions come to mind:

   1) If some string theorists are concerned that someone is not representing them well, why don't they represent themselves? Who's out there blogging, besides Motl, Clifford and Distler?

   2) How come Carroll is seen taking the pro-string side of the debate so often? Not only is he not a string theorist, it is questionable if he can even be considered a physicist. Is there really nobody more suited than him for the "job"?

6. **Peter Woit**
   July 15, 2007

   AGeek,

   Sean is definitely a physicist, although definitely not a string theorist. It is a somewhat weird situation that he's taking a leading role defending the theory.

   One of Sean's main arguments for string theory is that it has been victorious "in the marketplace of ideas", i.e. it has achieved its dominant position in particle theory by convincing theorists it is the way to go. My take on why more sensible string theorists than Lubos haven't tried to mount a serious defense of string theory against its critics is that they know that they would not fare well in open debate. String theory has not been doing well in the "marketplace of ideas" recently, and that's not because Lee Smolin and I are marketing geniuses. I've been told that one prominent string theorist has said that he wanted to write a book refuting Lee's book and mine, but couldn't bring himself to, since he would have to directly refer to us by name. For the moment, I think string theorists have made the tactical decision that they won't do well in an open marketplace of ideas, that entering it would just give serious criticism more exposure. If you were promoting an idea like the anthropic landscape, wouldn't you do what you could to stay away from forums where you would have to answer your critics?

7. **AGeek**
   July 15, 2007

   Carroll's academic degrees are all in astronomy http://preposterousuniverse.com/cv.html

Based on that, and on what I've seen of his work, I wouldn't call him a physicist.

His talk of "marketplace of ideas" has been criticized by others as trivially flawed: in academia, you don't have producers and consumers, with the latter only concerned with choosing the best product offered by the former; everybody is a producer. Imagine ten Apples all trying to sell iPods and iPhones to each other… the metaphore may reveal Carroll's level of understanding of economics, but little else.

If I were promoting an idea like the anthropic landscape, I guess I'd concentrate on the people who really count for the bottom line. That would be funding agencies and workers in more or less related fields, especially cosmology. I would probably not worry too much about the general public unless there were a real outcry about wasted funding threatening to turn into political action.

8. **DB**
July 15, 2007

Carroll is very slick. Very PR savvy. Very mediagenic. I think you'll see more of him as "the acceptable public face" of string theory. Motl was a PR disaster for string theorists – and a boon to critics. My bet (but what do I know anyway) is that Sean would like to carve out a niche for himself in the lucrative area of popular science presentation. A guy as smooth as Sean will be able to turn on a dime and dump the string theorists whenever he wants.

9. **Peter Woit**
July 15, 2007

DB,

"lucrative area of popular science presentation"

I know for a fact how much Sean made in one of his recent media appearances, since I was promised a check of the same size. After taxes, the sum is in the high two figures. I doubt bloggingheads pays even that well.

As far as I can tell there are extremely few physicists or cosmologists making enough money from "popular science presentation" to support even an extremely modest lifestyle. My guess is that you could count them on one hand, with fingers to spare. I doubt Sean is doing this because it's "lucrative", because it really isn't, except in very unusual cases (basically I think you need to have a successful TV show). People's motivations are complicated, and I think every scientist who makes it part of their mission to get ideas about science before the public has some mixture of an altruistic desire to enlighten others and a less-altruistic desire to get attention for themselves. Rarely does money have much to do with it at all, it seems to me.

10. **DB**
July 16, 2007

Peter,
Fair point. It doesn't usually pay well, although a bestselling book can be a real moneyspinner (e.g. Brief History of Time), especially if tied into a successful TV show (e.g. Sagan's Cosmos).

Anyhow, Sean's May rallying call in New Scientist

http://www.newscientist.com/channel/fundamentals/dn11882-string-theory-its-not-dead-yet.html
is having the requisite effect today:

http://space.newscientist.com/article.ns?id=dn12261&feedId=space_rss20

If I'm not mistaken, dark energy is Sean's area, so it will be interesting to read his take on this.

11. **Bee**
July 16, 2007

@AGeek above: Reg. the 'marketplace of ideas', you might find this interesting.

12. **Bee**
July 16, 2007

sorry, off-topic, but you just have to see this paper

Search for Future Influence from L.H.C
Authors: Holger B. Nielsen, Masao Ninomiya

"When the Higgs particle shall be produced, we shall retest if there could be influence from the future so that, for instance, the potential production of a large number of Higgs particles in a certain time development would cause a pre-arrangement so that the large number of Higgs productions, should be avoided."

what an ingenious idea! Peter, one shouldn't worry, I think ST just has a backward causation: Every potential observability of stringy effects just causes a pre-arrangement such that the observation is avoided. Thus, no observation is the correct prediction. Best,

B.

13. **Peter Woit**
July 16, 2007

Bee,
Thanks for mentioning the Nielsen-Ninomiya paper. I noticed it, was thinking about writing something about it since it is so bizarre, but couldn't think of anything sensible to say…

14. **gunpowder&noodles**
July 16, 2007

Maybe the NN paper is some kind of elaborate joke? Write on a piece of paper, "THE LHC WILL HAVE ITS FUNDING CUT OFF" and see what happens to Higgs observations……

15. **Eric Mayes**
July 16, 2007

I'm pretty sure this paper has absolutely no connection to string theory. Isn't it a little unfair to give lay readers the impression that it does?

16. **Peter Woit**
July 16, 2007

Eric,

My, my, you string theorists are mighty defensive these days.

I made no connection of this to string theory at all. But now that you mention it, one could note that this paper at least proposes a falsifiable test of their idea at the LHC, which is more that string theory is able to do…

17. **Eric Mayes**
July 16, 2007

Peter,
Bee made the connection with string theory. However, there are several falsifiable tests of string theory at LHC, namely supersymmetry and large extra dimensions. You may claim that a discovery of either of these does not prove string theory, however either of these would be very strong evidence in it's favor.

18. **Peter Woit**
July 16, 2007

Eric,

You seem to have a non-standard definition of "falsifiable". Supersymmetry and large extra dimensions are not falsifiable predictions. Their status is the usual one of string theory "tests": if we see them, that's great for string theory, if we don't, no problem. Heads we win, tails doesn't count…

19. **Eric Mayes**
July 16, 2007

Peter,
Sure, it's falsifiable. All we need is a big enough collider. Now, it may be that LHC isn't big enough, but this a technological liimitation not a fundamental one. However, it is clearly possible to find evidence in favor of string theory at LHC, even if LHC will not be able to rule it out.

20. **Peter Woit**
July 16, 2007

Eric,

"there are several falsifiable tests of string theory at LHC"

"Sure, it's falsifiable. All we need is a big enough collider. Now, it may be that LHC isn't big enough"

Funny the way these string theorists keep changing their story when you point out to them that they're wrong…

21. **Eric Mayes**
July 16, 2007

Peter,
Do you deny that it's possible, in principle, to find evidence in support of string theory at LHC?

22. **Peter Woit**
July 16, 2007

Eric,

Sure, although I don't think it's likely. But it's really a tedious waste of time dealing with the comments that you keep posting here which make completely false claims (e.g. "there are several falsifiable tests of string theory at LHC"), which you never acknowledge are untrue, instead continually returning here to make the same false claims again and again.

23. **Eric Mayes**
July 16, 2007

Peter,
If you don't think it's likely that supersymmetry will be found at LHC, then you are in for a rude awakening. Supersymmetry, by the way, is the smoking gun of string theory.

24. **Chris Oakley**
July 16, 2007

Sure, Eric. Superstrings being – let me guess – the language in which God wrote the world – ?

Just for the benefit of lay readers – Supersymmetry is, and always has been in conflict with experiment. No pair of known particles are classifiable as supersymmetric partners of each other (fermions and bosons of adjacent spin would need to have the same mass). So supersymmetry is *wrong*. It *does not agree* with experiment. The talk is therefore about *broken* supersymmetry, which means a supersymmetric theory that has been doctored according to a well-defined procedure known, through abuse of the word "spontaneous", as spontaneous symmetry breaking. However, the ways in which the broken supersymmetric theories can be constructed are legion, and however little evidence appears for supersymmetry in LHC and similar experiments, it is always possible to claim that supersymmetry is still there, but is broken at a higher energy.

25. **super**
July 16, 2007

Chris,

It seems to me a little dishonest to say that SUSY is “doctored” because it must be broken in nature. The gauge symmety of the standard model is also broken, which doesn’t mean that it’s wrong.

26. **Eric Mayes**
July 16, 2007

Chris,
Yes, we know supersymmetry must be broken at low energies. However, there are very good reasons for the superpartners to have 1 TeV scale masses, just in the range that will be probed by LHC. Now, it’s possible that the superpartner masses will be much greater than this, but in this case we are giving up naturalness and introducing fine-tuning into the Higgs potential. One could make the case that the reason for some people to introduce the anthropic landscape is to justify unnaturalness and fine-tuning.

27. **Thomas Larsson**
July 16, 2007

“However, there are several falsifiable tests of string theory at LHC, namely supersymmetry and large extra dimensions. ”

Theorem
Supersymmetry will not be discovered at the LHC.

Proof: String theory predicts supersymmetry (Witten 1984-2002). String theory predictions are always wrong. Hence supersymmetry does not exist, and will in particular not be found at the LHC. QED.

Corollary
Lubos Motl will lose his experimental-susy-by-2006 bet.

28. **Eric Mayes**
July 16, 2007

Thomas,
I thought string theory predictions were ‘not even wrong’, so doesn’t this invalidate the proof of your theorem?

29. **ori**
July 16, 2007

Regardless of whether string theory is right or wrong, it is consistent mathematically, and does decribe on the purely formal level unified quantum gravity. I think that any theoretical physicist interested in these ideas must know string theory, even if he does not believe it. It has proved useful in calculating highly non trivial objects in gauge theories, with lots of possible uses to real QCD (see Alday-Maldacena for instance) and needless to say, many results in mathematics.

30. **lyme**
July 16, 2007

Nielsen, who has made superb contributions to HEP in the past, has been saying that kind of crazy things for a long time. Somehow, scalar particles seem to trigger a kind of mystic madness in him.

Or maybe he’s just joking, which would be equally crazy. I’ve seen him speak at the ICHEP conference in 1996, saying the same kind of nonsense. Cracking the same joke time and again for eleven years would certainly not be a sign of mental health…

31. **woit**
July 16, 2007

ori,

The line “string theory is mathematically consistent” is stated as a fact in its favor quite often, but maybe you can tell me precisely what theory it is that is mathematically consistent. Is it

1. the string perturbation expansion?

according to d’Hoker/Phong, in flat space this is well-defined through two-loops, quite possibly it is well-defined at any order, and quite possibly you can extend this to other backgrounds. But all evidence is that it’s a divergent series. You can’t sum it, and you can’t cut it off at fixed order without violating unitarity.

2. non-perturbative string theory?

last I heard, no one knew what that was. In some special cases, via making the AdS/CFT conjecture a definition, I suppose you could define it in terms of a QFT, but then it’s not so clear why you shouldn’t just be studying QFT. In this context, string theory is just a conjectural approximate calculational method for certain QFTs, something which is quite interesting, but not necessarily fundamental, and not necessarily of interest to all particle theorists.

32. **Who**
July 16, 2007

this just out. Can anyone enlighten me as to its possible significance?
http://arxiv.org/abs/0707.2283
Conformal Gravity Challenges String Theory
Philip D. Mannheim
8 pages. Proceedings write-up of talk presented at PASCOS-07, Imperial College London, July 2007
(Submitted on 16 Jul 2007)

“The cosmological constant problem and the compatibility of gravity with quantum mechanics are the two most pressing problems in all of gravitational theory. While string theory nicely addresses the latter, it has so far failed to provide any compelling solution to the former. On the other hand, while conformal gravity nicely addresses the cosmological constant problem (by naturally quenching the amount by which the cosmological constant gravitates rather than by quenching the cosmological constant itself), the fourth order derivative conformal theory has long been thought to possess a ghost when quantized. However, it has recently been shown by Bender and Mannheim that not only do theories based on fourth order derivative equations of motion not have ghosts, they actually never had any to begin with, with the apparent presence of ghosts being due entirely to treating operators which were not Hermitian on the real axis as though they were. When this is taken care of via an underlying PT symmetry that such theories are found to possess, there are then no ghosts at all and the S-matrix is fully unitary. **Conformal gravity is thus advanced as a fully consistent four-dimensional alternative to ten-dimensional string theory.**"

33. **Peter Woit**
July 17, 2007

who,

Please, this has nothing to do with the posting, and this is not a general discussion forum for new ideas about quantum gravity. The idea of "conformal gravity" has been around for a very long time, with lots of people claiming to have ideas about how to deal with the problems caused by its fourth order action. This is one more, I have no idea whether it works. You should use another forum to discuss this issue.

34. **Bee**
July 17, 2007

Hi There,

I've put some remarks on the bizarre paper here. In case it wasn't clear, my above comment about string theory was meant as a joke.

@Eric,

you write *Do you deny that it's possible, in principle, to find evidence in support of string theory at LHC?*. It seems to me you are kind of missing the point. The question is not whether there is 'in principle' a 'possibility' to find something or the other, but whether there is a prediction it *has* to be found, or otherwise the theory the prediction was made with is falsified. The problem is if the LHC doesn't find anything like SUSY or extra dimensions, you can still argue ST has not been outruled.

– B.

35. **ori**
July 17, 2007

Peter,

it is string theory as a whole on all of its thousands of papers with independent calculations which have never contradicted each other. Surely, the whole object is not yet well defined mathematically and non perturbatively, but this does not stop people who are physics-minded to do many non trivial calculations and reach fascinating conciusions which have tought us many lessons in mathematics and physics (and were confirmed in many independent ways).

36. **Peter Woit**
July 17, 2007

Ori,

"string theory is consistent mathematically"

"the whole object is not yet well defined mathematically and non
perturbatively"

It's pretty misleading to go around telling people that you have something mathematically consistent when you don't have a definition of what it is…

37. **Eric Mayes**
July 17, 2007

Bee,
Science is about research and discovery. Sure, it's true that we can't guarantee that we'll find anything, but this is the case with any theory. This is what we like to call 'research'.

38. **anonymous**
July 17, 2007

Eric,

It is most definitely *not* the case with any theory. Einsteins theory of general relativity made specific concrete falsifiable predictions. If those predictions did not pan out then the theory would have been proved wrong.

String theory is not like that. String theorists have not offered any falsifiable prediction. Unless and until string theory can offer up a real prediction you can not lump it in with 'any theory', but of course you already know all of this.

Eric, why do you insist on tip toeing around the elephant in the room? It has been pointed out time and time again and you are obviously intelligent enough to see it, so why do you ignore it and use word games to prance around it like it isn't right in front of your face?

39. **Santo D'Agostino**
July 17, 2007

Eric,

The terms "theory" and "prediction" are sometimes used in non-traditional ways, and that has made some discussions of string "theory" unproductive. I note that recently there has been some movement towards using these terms in their traditional sense. For example, David Gross has said that we don't know what string theory is, and Barton Zweibach has said (in his textbook) that string "theory" makes no "sharp predictions". Traditionally, there is only one kind of scientific prediction, so the phrase "sharp prediction" is not a return to standard usage, but at least it acknowledges that there is a problem.

Is there a string "theory" calculation for the specific mass of a particle that has not yet been observed? Or some other specific quantity that has never been measured? If so, then we have a prediction. If not, then let's just admit this. And if there is no prediction, then it's difficult to see how anything measured in any experiment could provide support for string "theory".

I have no problem with any individual person pursuing research in string "theory". It has a number of features that seem to promise great riches. However, "we don't know what it is yet" precludes it being called a scientific theory. A possible scientific theory in formation, maybe, but not a theory.

All the best,
Santo

40. **anon2**
July 17, 2007

anonymous,

Suppose Einstein had instead invented a scalar-tensor theory. This theory can be made to look arbitrarily close to GR by making the scalar heavier and heavier. It will predict some fifth force which, if seen, would confirm the theory. However, the theory can never be ruled out, you just get stronger and stronger upper bounds on the scalar mass. Would you consider this theory "not even wrong" and accuse its advocates of promoting religious nonesense in the guise of physics?

41. **anonymous**
July 17, 2007

"Would you consider this theory "not even wrong" and accuse its advocates of promoting religious nonesense in the guise of physics?"

Yes, I would say they were "not even wrong."

OTOH, I wouldn't say they were promoting religious nonsense. String theory might well be a useful mathematical tool, but it is not a scientific theory. It offers the observer no predictions, no scaffolding on which to better understand the real physical world.

String theory is just speculation(s). It is a proposal for some 10^500 possible scenarios that -might- be happening at scales and magnitudes completely beyond our ability to penetrate. It does not offer one iota of explanation for the large mysteries of physics that have been discovered during string theories incubation.

When the special theory and quantum mechanics were being developed theorists had some shocking mysteries they were struggling to understand. The constancy of the speed of light… The mysterious nature of the photon… The theories developed during this time helped explain the what and why of these experimental phenomena.

The same can not be said of string theory. With string theory, if some new and shocking experimental finding or observation comes along the string theorists run along not long after and shout, "We can fit that in string theory too!"

Glashow's comment hurts the sensibilities of string theorists so much because it cuts so close to the truth.

42. **Peter Woit**
July 17, 2007

anon2,

When I accuse landscape proponents of engaging in pseudo-science, it is because they are promoting something very different than what you describe, something that makes no predictions of any kind (or to the minimal extent that it does, some are already falsified), not predictions that depend on a parameter.

Sure, it's conventional scientific activity to speculate about what happens when you extend tested theories by adding one or more parameters, and trying to put bounds on these. If there were a unique string theory, with just an undetermined scale, extracting predictions from it and putting bounds on the scale would be a standard kind of scientific activity. But that's not what string theory is like. Instead you have exponentially large numbers of possibilities, any of which still contains an undetermined scale (or scales). This is not conventional science, and resembles in no way a one-parameter scalar-tensor theory.

43. **Eric Mayes**
July 17, 2007

Does quantum field theory by itself offer any falsifable predictions? The answer is no, it does not. To get any predictions, you must input into the theory information such as gauge groups, particle masses, gauge couplings, all of which are not fixed by the theory itself. Does this mean that QFT is not science? Absolutely not. The same is true of string theory.

On the other hand, there are theoretical and mathematical criteria that may be used to falsify various candidate theories of quantum gravity. To date, string theory is the only such theory which passes these test, which is the reason it is studied.

44. **King Ray**
July 17, 2007

"The value of a principle is the number of things it will explain."—Ralph Waldo Emerson

45. **woit**
July 17, 2007

King Ray,

String theory promises to explain everything, but so far explains nothing. So, it achieves the highest possible quotient of promised value/actual value (infinity/zero). Perhaps this is why string theorists like to describe it as the "most promising" principle for unifying physics.

Then there's the landscape, which explains everything with the explanation that it can't be explained…

46. **Santo D'Agostino**
July 17, 2007

Eric,

You said:

"Does quantum field theory by itself offer any falsifable predictions? The answer is no, it does not. To get any predictions, you must input into the theory information such as gauge groups, particle masses, gauge couplings, all of which are not fixed by the theory itself. Does this mean that QFT is not science? Absolutely not. The same is true of string theory."

I agree with you, except for the last sentence. Would you please describe a calculation from string theory that goes through a process analogous to the one you describe for QFT and results in a (specific numerical) prediction?

If, at the very least, it is possible using string theory to calculate quantities that have already been measured, and whose values are well established, then string theory might qualify as a scientific theory (there are other criteria as well). If not even that is possible, then one might say it is a theory in development, and so work on it would certainly qualify as scientific activity, but how could you call it a theory?

All the best,
Santo

47. **anon.**
July 17, 2007

Santo, you are right. In quantum field theory, the few inputs to the standard model can easily be found from observed data, based on experiments. But for string theory, there are many vital inputs that can't be found from experiments, because they describe features of the 6 compactified dimensions which (are supposed to) exist at the unobservably tiny Planck scale. Hence string theory is a failure.

48. **King Ray**
July 17, 2007

Peter, you have hit the nail on the head once again.

As Feynman said, if string theory is so great then they should be able to calculate the mass of the electron. Forget about predicting superpartners, just show us a calculation for the mass of the electron!

I think the string theorists are dodging honest work; they are so divorced from reality that they don't even know when they are lost. They seem to be divergent rather than convergent thinkers. To get something done you have to limit the possibilities you're going to explore.

I would start by eliminating the extra dimensions, and stop spending so much time in 2 and 3 dimensions, which in a lot of cases seems to be just avoiding the difficulties in 4 dimensions. Also, if you can't handle 4 dimensions, how are you going to handle 11?

If there is no evidence for something, such as extra dimensions, then why should you believe in it with such vigor?

49. **ori**
July 17, 2007

Peter,
such a situation occured many times in history. I am sure that if you lived in the era of Dirac you would go around and say that whatever Dirac does does not make any sense since this strange delta function object is not well defined, and you can not say that you do something well defined when you don't have axiomatic understanding of it…

This is why physics is physics and math is math, and it is good that it is this way. The sole fact that people have been able to compute so many non trivial things (mutually consistent) without a rigorous definition of the theory clearly implies that there is such a definition, and it will be found in the future.

Your objection have been raised many times in the history, and was never relevant. Whenever something in physics works, even if it seems ad-hoc, it must make sense in some more well defined frame work. The examples are too many to list, but I am sure you understand this point very well.

I also wish to emphasize that all string theorist are very interested in finding a more natural and more axiomatic framework to unite whatever they were doing in the last 25 years – this is not something which is being ignored. The evidences for the existance of such a formulation is overwhelming, there is no doubt that the theory is mathematically consistent (in similar way that Dirac's delta functions turned out to be not an ad hoc object with no clear rules for the game) as I said.

50. **Peter Orland**
July 17, 2007

Ori,

Criticizing string theory is like criticizing the Dirac delta function? Are you kidding?

51. **Eric Mayes**
July 17, 2007

King Ray:
Regarding the calculation of the mass of the electron, please see hep-th/0703280.

Santo:
I agree, string theory has not reached the level of maturation of QFT with the Standard Model. Keep in mind that this is only because the Standard Model is operative at low energy where there is a lot of experimental data.

What is needed is a string theory model which completely describes the low-energy physics of the Standard Model and can make detailed predictions for physics beyond the Standard Model. Even if some parameters need to be fitted by hand to get agreement with the Standard Model, once this is done all of the other (new) physics should be fixed as well. The problem would be if there are many string theory models which reproduce the standard model exactly, yet have completely different predictions for new physics. I don't expect this to happen.

Again, I would like to make the point that it is possible for string theory to be falsified by theoretical criteria alone, although this hasn't happened. Peter's main argument that it isn't falsifiable is wrong for this reason alone. In addition, it is always possible that new, unexpected phenomena will be discovered that are not compatible with string theory.

52. **King Ray**
July 17, 2007

Eric, do you really think the true theory is that complicated? You put in a lot of fitting parameters and are still off by a factor of 6.5 on the electron mass; wouldn't that seem to say that your model cannot fit the SM?

53. **anonymous**

July 17, 2007

Eric,

"In addition, it is always possible that new, unexpected phenomena will be discovered that are not compatible with string theory."

Please describe these hypothetical phenomena.

54. **alex**
July 17, 2007

"Please describe these hypothetical phenomena."

I believe that violation of unitarity is incompatible with ST.

55. **Lee Smolin**
July 17, 2007

Eric,

"Does quantum field theory by itself offer any falsifiable predictions?" Yes, certainly it does: the existence of antiparticles, with the same mass and opposite charges as known particles, is a direct consequence of the axioms of special relativistic quantum field theory. So are the relation between spin and statistics and the CPT theorem. The existence of antiparticles and CPT were predicted before they were tested and they and the spin-statistics connection are confirmed by many experiment and to a large number of decimal places.

One can add to this the classic tests of QED, the magnetic moments of the electron and muon and the Lamb shift, which hold robustly to many decimal places, independently of the strong and weak interactions, higher generations etc. The fact is one can make extremely successful predictions without knowing very much at all about what the gauge groups and mass spectra are, using only the well confirmed fact that there is an unbroken U(1) gauge symmetry coupled to light fermions.

This gives us confidence that the axioms of QFT are reliable over a wide range of scales. There is not a single comparable success in string theory or any post standard model theory.

Thanks,

Lee

56. **Tim**
July 17, 2007

Hi Lee,
Sure, antiparticles are a necessary ingredient of any QFT. However, can you predict the specific particles that will be present in your theory from first principles? Absolutely not. You have to assume a gauge group, from which it will follow that the matter content will be representations of this gauge group, with the only constraint that all anomalies must cancel. You cannot determine from first principles what the matter content, masses, or charges of your QFT will be. The Standard Model is just that, a model which happens to be a QFT, and in this regard it is not unique.

57. **Eugene Stefanovich**
July 17, 2007

Alex said:

I believe that violation of unitarity is incompatible with ST.

Violation of unitarity is incompatible with common sense. Total probability for all alternatives simply cannot be greater (or less) than 1. Quantum mechanics + preservation of probabilities = unitarity (Wigner theorem).

58. **Chris Oakley**
July 18, 2007

> Sure, antiparticles are a necessary ingredient of any QFT.

They are only "necessary" if you require a charge conjugation symmetry such as CPT.

59. **Santo D'Agostino**
July 18, 2007

Eric,

You said:

"What is needed is a string theory model which completely describes the low-energy physics of the Standard Model and can make detailed predictions for physics beyond the Standard Model. Even if some parameters need to be fitted by hand to get agreement with the Standard Model, once this is done all of the other (new) physics should be fixed as well. The problem would be if there are many string theory models which reproduce the standard model exactly, yet have completely different predictions for new physics. I don't expect this to happen."

About your last sentence: Until there is an actual string theory that makes actual predictions, it is hard to know what can be expected.

You also said:

"Again, I would like to make the point that it is possible for string theory to be falsified by theoretical criteria alone, although this hasn't happened. Peter's main argument that it isn't falsifiable is wrong for this reason alone. In addition, it is always possible that new, unexpected phenomena will be discovered that are not compatible with string theory."

Yes, agreed, any theory can be falsified if it is logically inconsistent, and this does not require any experimental input. However, the point about falsifiability that has repeatedly been made by a number of people is that because no predictions have yet been made by string theory, it is impossible for any experimental result to falsify it.

This is what I take the "not even wrong" label to mean.

All the best,
Santo

60. **Eric Mayes**
July 18, 2007

Santo,

I'd like to emphasize that it may very well be possible to make definitive predictions in the near future and there very well may be experimental results in the future that may be able to confirm or falsify it. In my opinion, the claim that string theory is not falsifiable is itself 'not even wrong' because we really don't know what new developments or discoveries my be found in the future. Saying we shouldn't study string theory because we do know enough about it yet to make predicitions is rather dumb. How are we ever going to understand it well enough to make predictions if we don't study it? Why not give up on experimental high-energy physics as well since we'll never be able reach Planck-scale energies?

61. **Vince**
July 18, 2007

I think one reason why antiparticles are necessary in QFT comes from the desire to preserve causality. Look at Peskin and Schroeder.

62. **anonymous**
July 18, 2007

Eric,

"In my opinion, the claim that string theory is not falsifiable is itself 'not even wrong' because we really don't know what new developments or discoveries my be found in the future."

You seem to have a problems with the present tense. Right now, at this very moment, string theory is 'not even wrong' because it makes no predictions of any value. Saying that it 'may very well be possible in the near future' to make predictions doesn't obviate the fact that — right now — it does not.

The FSM 'may very well be possible in the near future' to falsify. However, the FSM, as it stands now is 'not even wrong.'

63. **Eric Mayes**
July 18, 2007

Anonymous,
This is the nature of research in progress. I'm really getting tired of this abuse of Pauli's phrase 'not even wrong'. He certainly didn't mean it in the strict, limited way that you and Peter use it. Essentially, he meant that a theory was 'half-baked', i.e. not consistent. The inability to test a theory due to current practical limitations is not a valid reason for dismissing a theory.

64. **Eric Mayes**
July 18, 2007

As an example, consider the theory that intelligent life exist elsewhere in the universe. This idea is almost certainly right, however I can't think any way to disprove the idea. Is this idea 'not even wrong'?

65. **anon2**
July 18, 2007

Eric,

Lots of perfectly reasonable theories will be "not even wrong" in the sense advocated in this comment section. Your theory about intelligent life and the scalar-tensor theory I mentioned earlier are examples. As Peter points out, string theory has more tunable parameters than scalar-tensor gravity. However, string theory also has stronger theoretical motivation: the desire to find a consistent quantum theory of gravity.

Like much of what is discussed here it seems to me to be really a question of personal prejudice. Does the potential future promise of string theory justify the effort that's currently being put into it. Many people think "yes". Some people think "no". This is fine because everybody is allowed to choose her own research programme. I'm don't see what all the fuss is about.

66. **Eric Mayes**
July 18, 2007

Anon2,
I agree with you completely.

67. **anon.**
July 18, 2007

'… everybody is allowed to choose her own research programme. I'm don't see what all the fuss is about.' – anon2

Silly me! I thought the whole problem was that string theory is hyped to be the 'only consistent theory of quantum gravity'.

I was evidently deluded, since I thought that such marketing of currently non-falsifiable string theory made life hard for those who are pursuing other research programmes. Of course, I see the light now! Everyone is free in spirit to *choose* their own research programme, they just aren't generally free in practice to actually pursue it, unless by chance they have chosen string theory…

68. **Santo D'Agostino**
July 18, 2007

Eric,

I have no argument with your last comment addressed to me, particularly the parts of it that are directed at others. (For example, I have never suggested that individuals should study or not study any particular subject.)

One ought to be painstakingly honest about the state of one's field of study, for the benefit of scientists outside the field, incoming graduate students, and also lay people. (And perhaps for our own benefit most of all.) I have met many lay people and students in the past decade with distorted views about string theory. Advocates of string theory could largely disarm many of its critics simply by being honest and direct about the state of the subject.

Scientific theories make predictions about the results of future experiments, then the experiments are carried out. If the experimental results match the predictions, within acceptable limits, then fine. If not, then the theory is discarded or modified. That is how the game is played.

You have not responded to my requests for predictions of string theory, although you imply that there are some, just at energies that are too high to access now. Are there any? If there aren't any, then fine, but let's be honest about that. Let us say, OK, it's not a scientific theory yet, it's in development, Eric Mayes finds it an interesting subject to work on, no problem. Best wishes to Eric Mayes, we hope he comes up with some good ideas that turn this into a proper scientific theory. Repeat, I have no quarrel with your research choices, Eric, and I sincerely wish you well.

But in the meantime, let's not delude ourselves into thinking that the next experiment can produce results that will either support or falsify string "theory", when there is no theory yet.

69. **Eric Mayes**
July 18, 2007

Ok, here are my predictions. Supersymmetry and the Higgs will be discovered at LHC with the Higgs mass between 116-121 GeV.

70. **Santo D'Agostino**
July 18, 2007

Eric,

Is your prediction for the Higgs mass based on a string-theory calculation? And your statement that supersymmetry will be found at the LHC does not qualify as a prediction unless it is quantitative.

All the best,
Santo

71. **alex**
July 18, 2007

ST predicts the existence of superpartners but it does not make a unique prediction about the SUSY breaking scale and mass spectrum.
It's clear and simple – the existence of superpartners is a generic prediction of ST.
Their mass spectrum depends on how SUSY is broken but that's a second question for which there is no unique ST prediction yet.

So, what! In the SM you need 19 parameters as input. Once the scale of m_3/2 is fixed, lots of SUSY models will be ruled out just based on this one input parameter.

In my biased opinion, SUSY is the best candidate to explain the dark matter so based only on this assumption the LHC has a pretty good chance to discover superpartners. There are of coures many other well known reasons (stabilizing the hierarchy, radiative EWSB, heavy top, precision gauge coulpling unification, etc) to expect that TeV scale SUSY is around the corner.

72. **King Ray**
July 18, 2007

Old news, but what about this?

http://www.cosmosmagazine.com/node/714

Which SUSY models does this screw up?

73. **alex**
July 18, 2007

"Which SUSY models does this screw up?"

"The MSSM does not lead to any significant deviation from the SM expectation for CP violating phenomena such as de
N, εK, ε□/ε and CP violation in B physics; the only exception to this statement concerns a small portion of the MSSM parameter space where a very light ˜t (m˜t

74. **alex**
July 18, 2007

"The MSSM does not lead to any
significant deviation from the SM expectation for CP violating phenomena such as d^e_N, ε_K, ε'/ε and CP violation in B physics; the only exception to this statement concerns a small portion of the MSSM parameter space where a very light ˜t (m˜t less than 100 GeV) and χ+ (mχ ~ 90 GeV) are present."

Sourse:
Flavour and CP violation in supersymmetry
Antonio Masiero and Oscar Vives

75. **Uncle Enzo**
July 18, 2007

'Source', not 'Sourse'.

76. **alex**
July 18, 2007

Thanx!

77. **Santo D'Agostino**
July 18, 2007

Alex,

You said,

"ST predicts the existence of superpartners but it does not make a unique prediction about the SUSY breaking scale and mass spectrum."

The existence of superpartners is an assumption of ST, not a prediction. And it would be more accurate to say that ST makes no prediction about the SUSY breaking scale and mass spectrum.

The word "prediction" has a precise meaning, and ought to be used with care.

All the best,
Santo

78. **Chris Oakley**
July 19, 2007

Vince,

Re: antiparticles, I don't agree. Causality, or at least spacelike commutativity is connected with the spin statistics theorem, but has nothing to do with the existence of antiparticles. Yes, naive quantization of the Dirac equation leads to particle/antiparticle pairs of the same mass, but one could with equal validity just 2nd quantize Majorana spinors which have no such property.

79. **Peter Woit**
July 19, 2007

I just deleted a sequence of comments by Santo and Eric, which, on Eric's part, mainly consisted of the sort of insults about incompetence and stupidity that some string theorists seem to feel is the best way to defend their point of view. Please stop this.

This discussion has pretty much degenerated, and I'll cut off any more comments that aren't well-informed and to the point. Before posting a comment here that carries on a pointless argument, ask yourself if you're writing something that a sensible person would find informative and worth spending their time reading. If the answer is negative, restrain yourself.

Various News

July 17, 2007
Categories: Uncategorized

There's a potentially important new paper on the arXiv from Terry Tomboulis, entitled Confinement for all values of the coupling in four-dimensional SU(2) gauge theory. Tomboulis claims to prove that SU(2) lattice gauge theory has confining behavior (area law fall off of Wilson loops at large distances) for all values of the coupling at the scale of the cutoff, no matter how small. This conjectured behavior is something that quite a few people tried to prove during the late seventies and eighties, without success. Tomboulis is one of the few people who has kept seriously working on the problem, and it looks like he may have finally gotten there. The method he is using goes back to work of 't Hooft in the late 1970s, and involves considering the ratio of the partition function with an external flux in the center of SU(2) and the partition function with no such flux. For a recent review article about this whole line of thinking by Jeff Greensite, see here. For shorter, less technical articles by Tomboulis about earlier results in the program he has been pursuing, see here, here, and here.

As far as I can tell, even if this holds up, it won't get Tomboulis the million-dollar Clay prize being offered for the solution of the Yang-Mills Millenium Prize Problem. There the problem is stated as rigorously constructing Yang-Mills for any gauge group, not just SU(2), showing that it has the expected properties, and a mass gap. I don't know whether Tomboulis's methods can prove that there is a mass gap. In any case, I also don't know why the Clay prize asks for a mass gap rather than confinement, which seems more physically relevant.

The latest HEPAP meeting was this past weekend in Washington and presentations are available here. The news from the DOE is that it looks like Congress will approve a FY 2008 HEP budget at or above the White House request of $782.2 million, an increase of at least 4%. This will include $60 million for ILC R and D (up from $42 million in FY2007). This will include funds for site surveys in the US, which in principle will include sites other than Fermilab, although I find it hard to believe a US site other than Fermilab would end up being chosen.

There's a report from the University Research subpanel that puts near the top of its recommendations "A higher priority in the overall HEP program should be given to funding directed at university-based theoretical particle physics for the purpose of increasing the number of HEP-grant supported graduate students." Given the continuing high ratio between students getting Ph.Ds in particle theory and jobs for them when they get out, I would have thought the priority would go to finding ways to fund jobs for students once they get out, rather than increasing the number of Ph.Ds.

There's a recommendation from the P5 panel that the Tevatron should definitely run through FY2009, and in September they'll start looking at the case for running even longer than that. There's also a discussion of "European reaction" to comments from the DOE's Ray Ohrbach that the US needs a plan in case the ILC doesn't start happening soon. Typical reaction from the Europeans was said to be that the US is not a reliable partner, and makes unilateral decisions without consultation. CERN has recently been promised a budget supplement over the next few years that could pay for an LHC upgrade (they were in hock over the LHC), and by 2016 that would be done and paid for. At that time CERN will have money to spend on a new big project and a higher energy linear collider using CLIC technology would be a possibility (the semi-joke made was that the work advancing this possibility is now going on mainly in the US, at SLAC).

A progress report from the Fermilab Steering Group, which is supposed to report to the Director on Aug. 1, included extensive discussion of "Project X", a proposal to dramatically increase the power of proton beams by building an 8 GeV proton linac. Part of the idea is that building this smaller linac would help get experience needed for doing the ILC.

Also at Fermilab, I noticed that they have a web-site devoted to the history of the place, and here is a recent talk by Adrienne Kolb on the subject.

According to CNN, one of the "Geniuses who will change your life" is Harvard's (soon to be Princeton's) Nima Arkani-Hamed, who is described as follows:

> Nima Arkani-Hamed thinks big. He has a theory that our universe is one of an infinite number of universes — meaning the largest thing we can wrap our minds around is actually pretty tiny
>
> He didn't pull the "multiverse" out of thin air, though. After becoming a Harvard professor at age 30, Arkani-Hamed first made a name for himself by suggesting that our universe is five-dimensional. Then he moved on to the multiverse, theorizing that our own universe has a hidden feature called "split supersymmetry," which means that half of all particles have partner particles.
>
> The theory will be tested soon in Switzerland's brand-new Large Hadron Collider (LHC), and if the LHC finds Arkani-Hamed's partner particles, it could prove that the multiverse is real — and that our place in it is that much smaller.

The claim that if you see split supersymmetry, this proves that the multiverse is real seems pretty much laughable to me.

Clifford Johnson is in Aspen, at what its director describes as a "summer camp for physicists". He has a posting about his experiences there, describing how the big topic of conversation is the recent Strings 2007 conference. He asks for people to write in telling about which of the Strings 2007 talks they found most interesting (although so far, a half day later, no one seems to have taken him up on this). Like Jacques Distler, after Witten's talk, Clifford seems to be most impressed by Seiberg's talk on BMSSM (Beyond the Minimal Supersymmetric Standard Model) physics, based on his recent paper with Dine and Thomas. The idea is that, even with its 105 extra parameters, the MSSM still requires a lot of fine-tuning to get around the LEP bound on the Higgs mass, so you should analyze adding even more terms to the Standard Model besides the minimal supersymmetric ones. Distler was very excited by the Seiberg talk, arguing that these results should have been found years ago but weren't. Various people wrote into his blog to point out to him that this wasn't right, that mostly these things had been done a while ago, including by several authors back in 2003 (see hep-ph/0301121 and hep-ph/0310137). Seiberg et. al. issued a revised version of their paper where they added quite a few references, including one to work back in 1999 by Alessandro Strumia (hep-ph/9906266) where he already discussed the impact of the two operators identified by Dine et. al on the Higgs mass.

Update: The debate over string theory really has reached the general public, at least in New York: a few months ago there was a cartoon in the New Yorker, this week, it has made the New York Magazine web-site celebrity coverage.

Comments

1. **Peter Orland**
   July 17, 2007

   Peter,

   As you say, the string tension (which is experimentally known through Regge trajectories) is more significant than the mass gap, or lightest-glueball mass (light glueballs have large widths, can mix with multi-quark states and so the experimental situation is not as clear).

Maybe the reason the Clay foundation set things up the way they did, is that most people suspected the gap would be easier to prove than confinement. If Terry Tomboulis is correct, however, extending the method to proving a gap is probably not out of the question. He uses a real-space renormalization group argument to compare quantities at strong bare coupling to those at weak bare coupling. The quantity he studies isn't the string tension, but another quantity, essentially the vortex condensate, which he uses to bound the string tension.

I am spending time reading the paper carefully…

2. **Bitter T**
July 17, 2007

I guess the reason why more hep phd fellowships are necessary
is because we may need, in the near future, even more of these data analysis slaves working for ongoing/upcoming experiments.
Or maybe we are just short of new permanent positions and, since it's so difficult to defend our area's research merit these days, it's much easier to appeal to 'education purposes' (what a dragon hunter does? on the very least, he teaches others to hunt dragons!)

Whatever it happens to most of these young physicists after phd is not of much interest; they may spend part of their most creative years with an illusion of a life in physics, but the fact is that, from the very beginning, there would never be enough places for all, just a bright tiny minority. It's a sad truth in these days of high specialization and changing technology, but these physicists are disposable. They won't be hired, they will be replaced by new, cheaper, phd students.

3. **Garbarge**
July 18, 2007

"Nima Arkani-Hamed thinks big. He has a theory that our universe is one of an infinite number of universes"

In which part of the landscale did that happen?

"Arkani-Hamed first made a name for himself by suggesting that our universe is five-dimensional."

They forgot to add he also discovered 3+1 and time travel.

"Then he moved on to the multiverse, theorizing that our own universe has a hidden feature called "split supersymmetry," which means that half of all particles have partner particles."

He's been busy, he also came out with SUSY, SUGRA, the Heterotic string and last episode of 'numbers'.

"The theory will be tested soon in Switzerland's brand-new Large Hadron Collider (LHC), and if the LHC finds Arkani-Hamed's partner particles",

Are we looking for the Arkino too?

"it could prove that the multiverse is real — and that our place in it is that much smaller."

This is too complicated to even attempt analysis…

Clearly the CNN people is an example that the reporter's brain is 'much smaller' than we would like it to be…fortunately there is a universe out there where Physics is financed by Ted Turner… 😉

4. **Chris Oakley**
July 18, 2007

BRITNEY: Forget Peter Woit. Forget Lee Smolin. They don't know sh*t. I've done the calculations. I have a unique compactification that gets all the correct gauge groups and fermion masses. Yeah, yeah … I know. Don't worry – I'll post my results on Arxiv later this month!

5. **amused**
July 18, 2007

"In any case, I also don't know why the Clay prize asks for a mass gap rather than confinement, which seems more physically relevant."

If I remember rightly, existence of mass gap implies confinement but the converse is not true. So mass gap existence is the stronger statement. It also seems like a perfectly physical statement – it means there are no massless physical states in QCD (in contrast to QED where there are massless photons). A simple intuitive way to see why mass gap implies confinement is this: If gluons were not confined then we would have free massless gluons in the physical QCD spectrum, in contradicton with the existence of a mass gap (i.e. nonzero lower bound on the physical energy spectrum). Same thing for massless quarks.
(Well that's how I understand it at any rate; if I've got something wrong then hopefully someone will correct me.)

6. **DB**
July 18, 2007

> There's also a discussion of "European reaction" to comments from the DOE's Ray Ohrbach that the US needs a plan in case the ILC doesn't start happening soon.

It seems Europe was also reacting to Orbach's statement: (quoted in Wormser's report)

> US wants « order of magnitude leadership »

7. **Alessandro Strumia**
July 18, 2007

hello,
thanks for signaling my paper hep-ph/9906266. It has no collaborators because nobody was interested in its main topic: testing a model of extra dimensions at the weak scale with precision data. While completing it, I started to believe that experimental bounds make extra dimensions an unplausible solution to the hierarchy problem, and furthermore another paper appeared with a similar analysis.

At this point, instead of being wise and trashing my little paper, I spent a week-end trying to improve it: adding the aside remark about how non-renormalizable operators raise the MSSM Higgs mass, and writing the introduction in a ironic mood, e.g. "string theory is currently an example of a theory with no parameters that makes no calculable predictions".

I hope that having anticipated something of Strings 2007 and something of anti-Strings 2007 can be considered as good irony.

8. **David**
July 18, 2007

If the gauge field is massless, one can calculate and see that the wilson loop satify the perimeter law. So masslessness implies lack of confinement. The converse then must be true. I agree that this is not the same thing as actually calculating the gap.

9. **J**
July 18, 2007

Ah, the CNN is speaking.

10. **Peter Orland**
July 18, 2007

David and Amused,

It isn’t quite that simple.

Gauge fields which are coupled to adjoint Higgs fields have no massless excitations, but do not confine.

Confinement implies a mass scale, but that isn’t quite the same
as a (mass) gap between the ground state and first excited state.
There is an example of confinement with massless particles. This is
the case for a version of compact QED in three dimensions (the reason is that this theory has two mass scales, which should not be the case for QCD in four dimensions.
One can tune the gap to be zero, for
fixed string tension, by choosing one of these scales).

On the other hand, any method yielding confinement ought to
powerful enough to settle the issue of the mass gap.

11. **David**
July 18, 2007

I agree it’s not so simple as that, Peter O.

But the counter example you give is that mass gap does not imply confinement. My point is that since absence of mass implies absence of confinement, confinement should imply mass gap.

This of course does not mean that mass gap implies confinement. So I do not think the Higgs case is a counter example.

To summarize, since masslessness implies the perimeter law, presence of a mass gap is a necessary but not sufficient condition for confinement.

The case of 3 D or 2 + 1 D compact QED is interesting. What are the two mass scales? The charge has a mass scale and there is also the cutoff. If we work on a lattice the cutoff is the lattice spacing. If we work on the continuum and obtain compact QED by spontaneously breaking and SU(2)
gauge theory with the helping hand from a Higgs field, the cutoff is the symmetry breaking scale. So I agree that 2 + 1 D has two mass scales.

What I do not agree with is that 2 + 1 D compact QED exhibits confinement with massless photons. First go to Polyakov’s 1977 Nuclear Physics B paper or his book “Gauge Fields and Strings” and you see that confinement is accompanied by mass generation for the photon.

That the photon cannot be massless if there is confinement can also be understood in simple terms from Wilson loop calculations. I will use latex notation and hope is not too confusing.

The Wilson loop is

\langle \exp{\oint_C A_\mu (x) ds^\mu} \rangle =

\exp{ \oint_C \oint_C D_{\mu \nu} (x – x’) ds^\mu ds’^\nu }

where D_{\mu \nu} (x – x’) is the gauge field propagator which is gauge variant of course. The Wilson loop is gauge invariant because the integrations over closed paths lead to zero for the gauge variant parts of the propagator.

Now to the point, if we use the massless propagator we do not get an area law for the Wilson loop in 2 + 1 D (the same thing happens in 3 + 1 D). Hence if the photon is massless there is no confinement.

So even though 2 + 1 D has two mass scales it is not true that one of them can be tuned to have confinement and a massless photon. It is possible that one of the mass scales can be tuned so that the photon becomes massless, but at such a point the theory does not confine.

12. **amused**
July 18, 2007

David, I don’t understand your reasoning at all. The gauge fields in QCD are necessarily massless – a mass term would break gauge invariance. However, the massless quanta of the gauge fields – the gluons – are not physical states of the QCD Hamiltonian (in contrast to photons in QED) since they don’t satisfy the nonabelian Gauss Law constraints (they aren’t color singlets). The question that then arises is: can one have color singlet states consisting of a number of essentially non-interacting free, deconfined gluons? Such a state would be massless (since the individual gluons are massless), implying that QCD does not have a mass gap. But there is no sign of such states in experimental low energy hadronic physics (which is where we would see them if they existed). Lattice studies indicate that color singlet purely gluonic states do exist in QCD – the so-called glueballs – but they are massive (quite heavy in fact) and have yet to be seen in experiments afaik.

The lightest state in hadronic physics is the pion, which definitely has nonzero mass, so if QCD is to describe hadronic physics like it is supposed to then mass gap is implied by experiment. This makes it very natural that proof of mass gap should be a criterion for the Clay prize.

13. **amused**
July 18, 2007

Peter O., my post crossed with yours, I'll go back and read what you wrote now.

14. **David**
July 18, 2007

To summarize if A implies B, then it is true that not B implies not A.

Masslessness implies no confinement, thus confinement implies mass gap.

I understand there are nontrivial issues with extracting certain information from a non-Abelian gauge theory and this is an understatement. And calculating the gap might be a herculean task. But as long as the lack of mass means no confinement, it seems that confinement implies a mass gap.

On the other hand like I said and Peter O.'s Higgs phase example shows perfectly, the presence of a mass gap certainly does not imply confinement!

15. **David**
July 18, 2007

Peter O.

I would love to hear your take on the paper. It's strengths and its weaknesses or open questions.

16. **amused**
July 18, 2007

If I understand correctly, Peter O.' counter-example to "mass gap implies confinement" involves spontaneous breaking of gauge symmetry, resulting in massive gauge field. In that case the intuitive argument I gave in my first post breaks down. Is there a counter-example where gauge symmetry is not broken (as in QCD)?

Btw, the "if I remember rightly, mass gap implies confinement" in my earlier comment was referring something I once heard Pierre van Baal say (in reference to QCD). It is possible I might be remembering wrongly though…

David, I will need to go and read up on these things again to check your claim that massless gauge field implies perimeter law (it's been a while..). Did Wilson really require massive gauge field when he derived the area law in strong coupling lattice QCD? I don't remember that!

17. **David**
July 18, 2007

amused,

As I said I have never stated that mass gap implies confinement as that is explicitly false.

What is not false is that confinement implies mass gap because masslessness
implies lack of confinement.

Once again A implies B is equivalent to not B implies not A, but does not say anything about not A implying not B, and in fact this last statement could very well be false.

No Wilson did not require a massive gauge field directly. But in his paper he did calculate that a massless gauge field implies no confinement. Therefore on can conclude that confinement implies mass gap.

Mass gap certainly does not imply confinement and a very good counter example is the "spontaneous breaking" of gauge symmetry mentioned by
Peter O. where there is a mass gap but no confinement.

18. **Peter Orland**
July 18, 2007

David,

I wrote a paper when a dumb grad student, publ. in
Nucl. Phys. 1982, where it is shown that in the
STANDARD continuum limit of 3D CQED, the mass
gap vanishes, with fixed string tension. This is
because there are two scales, namely the coupling constant
and the monopole fugacity. If one changes the monopole
fugacity, it is possible to keep the gap in the continuum
limit (though I did not mention this in the paper). Appropos of
nothing, the paper is mainly about generalized gauge theories of
p-form fields, their solitonic p-branes, duality and confinement
phases. Anyway, the masslessness of the (confining) continuum limit
was shortly therafter proved rigorously by Gopfert and Mack.

Now presumably there is only one scale in QCD, so this should not
happen. The question is what standard of rigor you demand. It is
not utterly inconceivable though that the gap could vanish
(with all other excitations massive), though I would be greatly
surprised if this happens.

Now essentially what Tomboulis does is show that one can
run the bare coupling all the way to the strong-coupling
region. If his paper is right, there is "no doubt" there is
a gap – I mean that I would be convinced, but that's
not yet a proof. What would be interesting is an estimate
of the gap (he does have an estimate of the string tension).

As far as the question of the paper being right is concerned, I don't have an opinion yet. I am spending all my spare time trying to figure it out. Right now I am a little
bogged down in how Tomboulis

understands and uses reflection positivity.

19. **David**
July 18, 2007

Peter O.,

I could buy what you say and I'll definitely check out your paper.

But I would like to know how do you scape the Wilson loop argument. This confuses me. If you calculate the Wilson loop with a massless propagator in 2 + 1 D and 3 + 1 D, one does not obtain an area law. I would take this to mean no confinement. So what gives?

Is it that we have confinement without an Area law? How does this come about?

Is it an order of limits issue because the point at which the gauge fields just goes massless is critical point and one needs to be very careful?

20. **amused**
July 18, 2007

David,

"As I said I have never stated that mass gap implies confinement.."

No, it was me who stated that.

"Once again A implies B is equivalent to not B implies not A, but does not say anything about not A implying not B, and in fact this last statement could very well be false."

I think we can agree on that.

"No Wilson did not require a massive gauge field directly. But in his paper he did calculate that a massless gauge field implies no confinement."

Now I'm really confused. I was under the impression that Wilson derived the area law using the "Wilson action" describing massless gauge fields, and without sneaking in a mass for the gauge field anywhere along the way. Is this correct or is it not correct (yes or no please).

"Mass gap certainly does not imply confinement and a very good counter example is the "spontaneous breaking" of gauge symmetry mentioned by Peter O. …"

I'm sure it is an excellent example, but unfortunately not so relevant for the situation I happen to be interested in which is where gauge symmetry is not broken (as in QCD).

21. **Peter Orland**
July 18, 2007

"But I would like to know how do you scape the Wilson loop argument. This confuses me. If you calculate the Wilson loop with a massless propagator in 2 + 1 D and 3 + 1 D, one does not obtain an area law. I would take this to mean no confinement. So what gives?"

OK, David, I think you are assuming that the Wilson-loop can be
reliably computed by gluon emission and absorbtion by a source. This is what perturbation theory tells us (there are of course corrections of more exchanges and higher loops), but it is very misleading for QCD, where gluon exchange is only meaningful at short distances. The contribution for large separations on the loop is not given by a finite number of gluon exchanges (in fact, most of the serious people in this field think of gluon exchange as a red herring).

In contrast, strong-coupling expansions don't give any gluons, just
glueballs. The problem with these expansions is that there is no
reason to assume they don't have a finite radius of convergence. They
are at best a caricature of the continuum limit, and they tell us little
about high-momentum physics.

22. **David**
July 18, 2007

amused, if you go to Wilson's paper he calculates his famous loop with massless gauge fields and shows that it does not satisfy the area law.

The bare fields are massless; you are correct in this. The question is whether the long distance renormalized gauge fields are massless. For the this last case there certainly is no confinement.

He did this to show that QED for weak coupling which has massless gauge fields does not confine.

He then goes on to argue strongly, not show, but good enough to convince me. that for strong coupling the Wilson loop satisfies the area law and does there is confinement. He did not calculate the mass gap nor explicitly state
that it ha gap, but given that masslessness implies no confinement it is certainly implicit.

Now, Peter O. tells me that one can show that 2 + 1 D CQED confines without a mass. Like I told him I could take his word on this, but it baffles me since one can calculate the Wilson loop for a massless gauge field
in 3 + 1 D and 2 + 1 D and it does not satisfy the area law. So it looks that the has a case where there is confinement without and area law.
How can this happen, I do not understand. I would love for Peter O. to clarify as I would learn something new. I can buy it but I remain sceptical until I understand it better.

Does this help amused?

23. **David**
July 18, 2007

Peter O., Thanks

Yes I agree that going to the strong coupling expansion is non trivial and in a a sense I was using perturbative thinking

24. **Peter Orland**
July 18, 2007

Amused,

Wilson's original paper did strong-coupling expansions for pure gauge theories. In these expansions there is a gap and confinement. It isn't that one implies the other, but that both are consequences of the the theory being weakly-correlated.

25. **David**
July 18, 2007

One question Peter O.

In your work on confining massless 2 + 1 CQED. Are the massless excitations gauge fields? I would think not, but I am not sure.

26. **Peter Orland**
July 18, 2007

David,

That's a good question. From what I remember, they are just photons – the correlation function of the field strength looks just like ordinary free QED.

27. **amused**
July 18, 2007

David, thanks for your efforts to explain. I will need to go take a look at Wilson's paper again to remember how this all hangs together. One immediate objection I have to your argument is that the renormalized gauge fields could acquire mass. Surely gauge invariance (Ward Identities) prevents this?
Anyway, it is well past my bedtime here on the other side of the world, so I'll have to take a break from the discussion for the time being.

28. **David**
July 18, 2007

Yes amused, Ward identities prevent this. I misspoke by saying that the aguge fields have mass. It is thought that the spectrum is fully gapped. So what is believed is that the gauge fields disappear from the spectrum, but you have things like Gluballs that have mass so the spectrum is gapped.

This is believed for many reasonable arguments and I buy them. But it has never been proven even semi rigorously I believe.

In Wilson paper all of this does not hang fully together. This beliefs and the reasonable arguments that point to them was built slowly through the mid 70's to the early 80's both from lattice gauge theory and from continium QFT.

29. **jhk**
July 18, 2007

It's pretty amusing how people like Distler get all excited over something when a celebrity like Seiberg says it, but not so much when a nobody said it years before. Same thing happened with the anthropic landscape. Modern physics is just a fashioin show.

30. **Peter Orland**
July 18, 2007

David,

I think if you read Wilson's paper carefully, it is quite sensible. He was very careful.

31. **amused**
July 18, 2007

Peter O. and anyone else,
I'm pretty sure I remember hearing from a very authoritative source (P. van B.) that mass gap implies confinement in QCD. If this is true it would explain why the Clay prize asks for proof of mass gap but doesn't mention confinement. Can anyone out there confirm this?

David,
what you wrote in your last comment seems to be converging to what I had written earlier. Is there anything we still disagree on?

32. **Kea**
July 19, 2007

Has anybody here actually read the Witten-Jaffe text?

33. **amused**
July 19, 2007

I just took a look at it now (the article is here). No mention there about mass gap implying confinement (or the converse)… A fundamental physical aspect of the mass gap that they emphasize is that it implies YM interactions are short-range (which they must be if QCD is to describe the strong nuclear force).

34. **Peter Orland**

July 19, 2007

amused,

I don't know what Pierre's argument was. There is no rigorous proof or estimate of the gap from the assumption of confinement (that I am aware of, and I have worked on this, on and off for a long time). There are lots of physical argument; I gave one above, assuming there is one scale. One expects that $K=M^{2}/\sigma$ is universal for this reason, where σ is the string tension and M is the gap. So can K be zero? I say certainly not, but my statement is not a calculation or an estimate of K.

By the way, Terry's argument, if it is right, does imply a mass gap for another field theory, namely the principal chiral SU(2) 1+1-dimensional nonlinear sigma model.

35. **David**
July 19, 2007

In Wilson's original paper if you go to the last paragraph of the second column of page 2447 and about two thirds of the first column of of page 2448. Wilson calculates his loop for a massless gauge field propagator in 4D (or 3+1 D) and shows that it satisfies the perimeter law.

Therefore massless gauge field leads to perimeter law, that is no confinement. At least in 4D masslessness implies no confinement and thus confinement implies mass gap. Now as amused said if is the gauge field
that acquires the mass there seems to be conflict with Ward identities. A possible resolution is that the gauge fields disappear from the spectrum and what is gapped are objects like glueballs, but no ones know for sure

Now mass gap in gauge theory does not imply confinment. The Higgs phase is a counter example. Now it is believed that if there is no symmetry breaking and there is mass gap in a gauge theory it has to confine. There are reasonable arguments for this, but no one knows for sure.

So amused, I think I agree mostly with you except on the statement that a mass gap necessarily implies confinement. Although I confinement probably implies a mass gap. Peter O. said that he has a counter example in his early paper. I have not gotten around to reading and studying the paper.

36. **Peter Orland**
July 19, 2007

"In Wilson's original paper if you go to the last paragraph of the second column of page 2447 and about two thirds of the first column of of page 2448. Wilson calculates his loop for a massless gauge field propagator in 4D (or 3+1 D) and shows that it satisfies the perimeter law."

David,

This is assuming one can calculate the loop by gluon exchange. Wilson is just pointing out that perturbation theory won't work,
not that a gap is necessary for confinement.

37. **Peter Orland**
July 19, 2007

If you use a massive propagator you will also find a perimenter law.

38. **David**
July 19, 2007

Peter O.

Wilson was pointing out that a massless propagator leads to a perimeter law.

I agree that if the theory is strongly coupled there are important corrections that might invalidate this thinking. But I would say that these corrections
would have to violently change the nature of the gauge field propagator
so that it does not correspond to propagation of a massless excitation.

The interpretation of the gap is my reading, but as I just said I agree that nonperturbative effects could invalidate this thinking. So I am not fully dogmatic and this reading could be wrong. I'll check out your paper too. I have just been caught up with other things.

39. **David**
July 19, 2007

Peter O. I agree with your comment of the massive propagator.

40. **Peter Orland**
July 19, 2007

OK David, I'd be interested in any other comments you have.

41. **amused**
July 19, 2007

Thanks for the replies Peter O. and David. I guess Pierre must have had some physical/heuristic argument in mind like you said. (Or it could be that I'm just misremembering what he said, in which case I'll be glad that I'm anonymous if he ever sees this!)

42. **Neville**
July 19, 2007

Peter Orland said:

> Maybe the reason the Clay foundation set things up the way they did, is that most people suspected the gap would be easier to prove than confinement.

That's the way I've got it figured. Connes, Jaffe, Wiles and Witten
were looking for problems with high probablity of solution. Just look at their their past behavior! Wiles picked Fermat's Last
Theorem for its tractability, while Connes and Witten pursued the low risk roads of Noncommutative Geometry and String Theory.

The Poincare Conjecture is another good example. Heck, the writing was on the wall before that Perelman character showed up.

As for the Riemann Hypothesis, a string theorist said it best to Michael Berry:

> Back in 1985, visiting CalTech, I had just started studying some quantum physics related to some mathematics called the zeta function, itself related to a famously untransparent mathematical speculation called the Riemann hypothesis… At the same time, CalTech was a centre of superstring theory… I met one of them, who asked what I was working on. When I told him, he fixed me with a pitying stare. 'Yes, we have zeta functions throughout string theory. I expect the Riemann hypothesis will be proved in a few months, as a baby example of string theory.'

Special note to Asperger sufferers: Try not to take this message too seriously.

43. **Peter Orland**
July 19, 2007

Neville,

It's not that I don't appreciate your light-hearted remarks, but both
problems – confinement and the gap – were always regarded as
super-tough. I can't read anyone's mind, but I was just trying to
guess the Clay F.'s motivation.

44. **Garbage**
July 20, 2007

Somehow out of topic, and maybe not on the various news theme:

I found there is a loops 07′ conference also in Czech republic, Lubos' land, in Charles U. indeed…

http://www.karlin.mff.cuni.cz/~loops07/

I am sure he will find this rather amusing too 😉

No idea how loops are related to Agriculture though… 😀

45. **amused**
July 21, 2007

Just a few final thoughts on mass gap vs. confinement in QCD (sorry I can't resist). A mass gap certainly implies that any particles corresponding to massless excitations of the fields must be confined in some way, since otherwise there would be states describing free massless particles in the physical spectrum. In particular, gluons must be confined; this ties in with the point emphasized by Jaffe & Witten that mass gap implies that the YM interactions are short-range. Similarly, massless quarks must also be confined. On the other hand, this reasoning does not imply confinement of massive quarks. I guess one can invoke a scale argument in this case, as Peter O. did, to see why confinement is expected, but it is of course not a proof.
Actually, for QCD to have a mass gap the quarks must be massive, since otherwise there would be massless Goldstone bosons associated with the spontaneous chiral symm. breaking (i.e. the mesons would be massless).
From looking at the Jaffe-Witten article it seems there are 3 reasons why mass gap rather than confinement was chosen for the Clay prize problem:
(1) Its physical significance: implies that YM interactions are short-range.
(2) Simpler to formulate than confinement: it just means the physical energy spectrum has a strictly positive lower bound.
(3) Potential mathematical significance: since mass gap implies that the interactions are short-range, it makes it potentially easier to extend results from YM on R^4 to YM on general 4-dimensional spacetime manifolds.

46. **Peter Orland**
July 21, 2007

Amused,

Confinement doesn't just mean that particles disappear from the spectrum. It is COLOR confinement. The excitations which do appear
are color singlets. There are no asymptotic particle states with color.

47. **Anonymous**
July 21, 2007

Peter Orland says:

I can't read anyone's mind, but I was just trying to
guess the Clay F.'s motivation

I suspect this may be right. In a conversation I've had with one of the problem's formulators (although we weren't specifically discussing the mass gap vs. confinement issue), he said that they tried to give the simplest formulation which would prove the type of result he wanted.

48. **amused**
July 21, 2007

Peter O,
I know that. But without knowing about QCD dynamics one could envisage color-singlet states describing a collection of deconfined gluons (as I mentioned in an earler comment). Something like a very dilute, spread out glueball. Such a state would have effectively zero mass, and is therefore excluded by mass gap.

49. **amused**

July 21, 2007

Anonymous,
"Simplest formulation" isn't the same as "easier to prove", which was what Peter O suggested. But yes, as mentioned above, I also got the impression from the Jaffe-Witten article that a reason they chose mass gap rather than confinement was because the problem was simpler to formulate.

50. **amused**
July 21, 2007

Whoops, just realized that it isn't possible to have states which are both color-singlet and describe a collection of free gluons or quarks at the same time. (There would have to be gluon strings between the quarks, or closed gluon loops, etc…). So I take back what I wrote earlier, sorry 'bout that.

51. **F**
July 22, 2007

Hi Peter,

Definitely off topic comment, so won't be offended if you delete this, but wanted to mention the ironic fact that you and Lubos share a lot in common these days; you both posit that a long held cherished theory receiving huge amounts of funding may have a hidden political agenda in the face of growing evdence that the theory may not be as infallible as once thought; hence the vehemence in attacks against dissenters. In your case it's string theory, and in his case, global warming theories driven by man-made CO2. Both of you risk ridicule by these powerful lobbies, but both of you assert rational and normal scientific inquiry have led you to these conclusions. A nice one-to-one mapping, don't you think?! Just food for thought….

52. **mclaren**
July 22, 2007

Delightful. With 105 parameters, the current string "theory" has made itself thoroughly unfalsifiable, since whatever gets observed can be back-predicted by tweaking one of those 105 parameters. Yet now an alleged "genius" proclaims that we need ***more*** parameters? What for, to render the "theory" even **more** unfalsifiable than it already is?
Everyone in the theoretical HEP community should be working overtime to **reduce** the number of free parameters in string "theory," not **increase** 'em.
Instead of BMSSM, they may want to shorten the abbreviation:
BS.

53. **Yatima**
July 23, 2007

> In your case it's string theory, and in his case, global warming
> theories driven by man-made CO2.

Oh Yeah. The first is an exploration of mathematical space with highly speculative application to the present reality. The second is an all-but-certain empirically validated internationally recognized and obvious-to-the-meanest intelligence ongoing modification of the supporting environment, with only Exxon-supported dead-enders and right-wing talk show hosts still in vociferous denial.

Definitely the same thing.

54. **anon**
July 23, 2007

If Woit, Lubos, Distler and Johnson were businessmen:

Woit:
Boys, here is our ledger; it shows that we haven't made any profit for decades. I don't know how to fix this, but something has to change, or we'll go broke.

Lubos:
Of course we are making a profit. Woit is a dumb bastard who doesn't know how to add. Anybody who says we aren't making a profit has an IQ of 80, or is a woman, or a communist. Look, just yesterday we sold 10 cents worth of string in Nebraska. You see, this shows we've been making a profit for decades.

Distler:
Wow, we are making a huge profit. The smartest people in the world have been saying we are making a profit for decades. So many smart people can't be wrong. I just read a paper by Seiberg et al where they point out for the first time ever that 2 + 2 = 4. You see, we are making progress in giant leaps.

Johnson
Boys, we might not be making *too much* of a profit *right now*(wow, you see, I am so open minded), but that is just a blip. It's just a tempest in a teapot. Wow, I enjoy the life of a string theorist so much; all that deep thought; did I tell you about my latest vacation?

55. **F**
July 23, 2007

Yatima,

Read your own comment again (the second part); do you notice that it has a remarkable similarity to the comments Peter Woit used to receive from Lubos in the early days when he espoused on the invincibility of string theory? I have no strong opinion on the content being discussed (in either field), since I am not qualified to do so, but I did want to highlight some rather _abstract_ similarities between the two bloggers. You've helped illustrate my point.

56. **Chris Oakley**
July 23, 2007

Weinberg:
Woit is right. We are not making a profit. No – I don't remember saying that Super String International was the most profitable corporation in the world two years ago.

Susskind:
This talk about Super String International – that I founded 40 years ago – being unprofitable is just nonsense. SSI has now completely broken away from the traditional accounting practises that hamper our competitors. The far more significant Mark-to-Ideas-Markets shows that the company is, and continues to be, an enormous success. Yes, I can give you a very good price if you want to buy my stake in the company.

57. **Coin**
July 23, 2007

> Definitely off topic comment, so won't be offended if you delete this, but wanted to mention the ironic fact that you and Lubos share a lot in common these days; you both posit that a long held cherished theory receiving huge amounts of funding may have a hidden political agenda in the face of growing evdence that the theory may not be as infallible as once thought; hence the vehemence in attacks against dissenters. In your case it's string theory, and in his case, global warming theories driven by man-made CO2. Both of you risk ridicule by these powerful lobbies, but both of you assert rational and normal scientific inquiry have led you to these conclusions. A nice one-to-one mapping, don't you think?!

Uh, I don't think I've ever seen Woit or anyone else claim that String Theory has a "political agenda".

This said, having encountered Lubos' interactions with climate-related blogs, I've actually noticed a mapping between these two things as well, but I'm not sure that I'm seeing the same thing you're seeing.

I don't exactly see the "dissenter" parallels you seem to be claiming here, because it seems to me that the nature of the consensus toward String Theory as a promising research program in physics, is very different in both scope and nature from the nature of the consensus in climatology that global climate change is real and serious.

But when I compare the string theory blog fights to the global climate change debate, I do kind of tend to identify a one-to-one mapping, with the string theory *doubters* mapped to the mainstream climatology *supporters*— because in each case, these are the sides who are spending most of the time talking about *evidence and rigorous connection to experiment*. This is, personally, the rubric I would tend to use to identify which side of a scientific controversy to tend to give the benefit of the doubt to when I am unqualified to make a determination as to which side is right.

58. **anon.**
July 25, 2007

> Uh, I don't think I've ever seen Woit or anyone else claim that String Theory has a "political agenda".

That which is blatantly obvious to all, need not be said by anyone.

Since string theory *doesn't have a scientific agenda, i.e., falsifiability*, it has a political agenda instead. (Politics is defined as the process of making decisions via groupthink and consensus.)

My Life With Stephen

July 23, 2007
Categories: Book Reviews

Jane Hawking, Stephen Hawking's ex-wife, has written a book about her life together with Stephen, which has recently appeared here in the US under the title [Travelling to Infinity: My Life With Stephen](). At around 400 pages, it's an abridged version of the 600 page [Music to Move the Stars: A Life With Stephen](), which appeared in the UK back in 1999. A US production company, Film and Music Entertainment (FAME), has acquired an option to make a movie of the book (weirdly enough, the contract is [on-line]()), but I don't know whether the movie is actually going to get made.

The Hawking's separation in 1990 and later divorce was widely covered in the media, and the book doesn't dwell on the depressing details. Stephen went to live with one of his nurses, Elaine Mason (who at the time was married), and later married her. Jane later married Jonathan Jones, a musician she had met a decade earlier, partly through her church choir, and who developed a close relationship with her and the rest of the Hawking family during the 80s. At the time of the UK edition of the book, there was little contact between Jane and Stephen, but it appears that Stephen is now in the process of getting divorced from Mason, and has re-entered Jane's life.

Up until the publication and huge success of *A Brief History of Time*, the Hawkings were not especially well-off, and dependent on others (including the MacArthur Foundation) for the high costs associated with Stephen's care. After 1989 though, the book and other projects brought in huge sums of money, which made him a wealthy man and perhaps played some sort of role in the collapse of the marriage.

The Hawkings were married back in 1965, at a time when Stephen's illness had already become apparent, and his prognosis for long-term survival was not good at all. For the next 25 years, Jane spent most of her time in the back-breaking labor of caring for an invalid husband while raising three children. While Stephen went from success to success, the center of attention due to his brilliant scientific work and triumph over his disability, Jane received little support, encouragement, or recognition for the sacrifices she was making, and one would have to be a saint to not develop some resentment for the situation and for the way it ended. She tells the whole story in some detail, and it's in many ways a rather sad one.

Among the sources of conflict between them were: religion (she was a believer, he a fervent atheist), his family (described as definitely not nice to her), and his devotion to physics:

> I sensed that there was yet another partner lurking in our already overcrowded marriage. The fourth partner first appeared in the form of a trusted and quiescent friend, signalling the way to success and fulfilment for those who followed her. In fact she proved to be a relentless rival, as exacting as any mistress, an inexorable Siren, luring her devotees into deep pools of obsession. She was none other than Physics, cited by Einstein's first wife as the correspondent in divorce proceedings.

She describes how, during his work on black holes leading up to the discovery of Hawking radiation, Stephen would isolate himself:

> For Stephen those periods of intense concentration may have been useful exercises in cultivating that silent, inner strength which would enable him to think in eleven dimensions. Unable to tell whether it was oblivion or indifference to my need to talk that sealed him off so hermetically, I found those periods sheer torture, especially when, as sometimes happened, they were accompanied by long sessions of Wagnerian opera, particularly *The Ring Cycle*, played at full volume on the radio or the record player. It was then, as I felt my own voice stifled and my own spontaneity suppressed inside me, that I grew to hate Wagner.

Jane also tells the story of Stephen's first public talk on black hole radiation, after which the chairman of the session, J.G. Taylor:

> …sprang to his feet, blustering, "Well, this is quite preposterous! I have never heard anything like it. I have no alternative but to bring this session to an immediate close!"

After this brusque cut-off of any questions, she describes how later she observed:

> Still blustering and indignantly muttering to his students, J.G. Taylor stood behind me in the queue, unaware of my identity. I was rehearsing a few cutting remarks in Stephen's defence when I heard him splutter, "We must get that paper out straight away!"

After she reported this to Stephen, he sent his paper off immediately to *Nature*, where the referee turned out to be Taylor himself. The article was first rejected, then finally accepted after a second referee was consulted. It appeared in the March 1 1974 issue with the title [Black Hole Explosions?](). Taylor's paper (with P.C.W. Davies as co-author) arguing that Hawking was wrong appeared a few months later as [Do Black Holes Really Explode?](). After writing many papers on string theory during the late eighties, more recently [Taylor]() has devoted his time to the study of neural nets and consciousness.

While Jane quite liked many of the relativist colleagues of Stephen's that she was meeting, especially if they weren't in a group talking about physics, she was much less impressed by the particle theorists that Stephen started spending his time with after the mid-seventies as his work concentrated on quantum gravity and unification:

> Nor, I have to confess, did the set of scientists with whom Stephen was now associating attract me in the least. On the whole, particle physicists were a dry, obsessive bunch of boffins, little concerned with personal contact but very concerned with their own scientific reputations. They were much more aggressively competitive than the relaxed, friendly relativists with whom we had associated in the past.

Despite Stephen's disability, the Hawkings did an immense amount of traveling, especially for professional purposes, and Jane describes her impressions of the places they went and people they met. One of the few things she gets wrong is the name of Andrei Linde's wife, Renata Kallosh, who they met on a trip to Moscow. Linde is now one of the most fervent proponents of the anthropic principle, which appears at one point in the book as Jane tells about early debates between Stephen and Brandon Carter during the late sixties and early seventies. She describes it as philosophically close to the medieval Ptolemaic universe, trying to put man at the center of all things.

The book is mostly not a book about physics though, but very much about what it was like to struggle with caring for someone coping with a grave disability, a difficult and not always rewarding task even in this remarkable case of someone who has overcome obstacles and achieved about the highest pinnacle of success possible.

Comments

1. **gunpowder&noodles**
   July 23, 2007

   This is what brings me back to this blog: well-written articles that I could not get elsewhere, even if I don't agree with many of them. No parochial poliitical crap that I could get anywhere [cf Cosmic Variance]. No boring, rambling diatribes about God-knows-what [Reference Frame]. Better watch out for Sabine H. though, she is catching up with you….

2. **anonymous coward**
   July 23, 2007

In my mind I need to qualify that final "success"… he achieved about the highest pinnacle of scientific success, yes. But shouldn't success in life have something to do with how you care for your family?

Out of curiosity, are there good examples of leading scientists who were noted for having great family lives?

3. **Peter Woit**
July 23, 2007

Thanks gunpowder,

Sabine does have a great blog, I'm glad to see that there are more and more of them around. It's not a competitive sport…

ac,

Actually most of the leading mathematicians I know well enough to know about their families seem to be quite successful there too. On the whole I think mathematicians and physicists do no worse in this area than other people. Hawking's story is a very unusual one, with unusually difficult problems both he and his wife had to face.

4. **Flerp**
July 24, 2007

Feynman's family life, his first wife's very untimely death notwithstanding, seems to have been mostly happy.

5. **Christa**
July 24, 2007

Hi Peter,

Good story. But am I being excessively critical or is Stephen Hawking (somewhat) overestimated as a professional physicist (by both general audiences and academic colleagues, albeit I presume less by the latter)? I can understand the general sympathy given him as someone suffering from ALS AND doing worthwhile physics, but how much has he actually contributed other than his earlier works – which by far aren't necessarily uncommon. It seems more and more that he is a celebrity / physicist than a genuine heir to Einstein, et. al. However, when credit is due one should receive it by all means, but excessive credit is going the wrong way.

Einstein, Dirac, Weinberg, etc. deserve to be regarded as major figures as they produced genuinely worthwhile results which have made major impacts on theory physics, but I am not quite as certain regarding Hawking's contributions.

Maybe it's just me…

6. **Large Atom**
July 24, 2007

ac:

"Success" is a relative and subjective term; relative in that, what is "success" for one may be unimportant to another; and subjective in that, there should be no way to quantify it.

What if one does not have a "family", by the way, and we define a part of success using such a term? Is this person somehow not able to ever fully achieve it?

7. **Arun**
July 24, 2007

Feynman's family life, his first wife's very untimely death notwithstanding, seems to have been mostly happy.

No, his infidelities were endured and caused pain.

8. **Chris Oakley**
July 24, 2007

Christa does have a point. I am not questioning his achievements as a mathematician, but unlike the other notables mentioned, nothing Hawking has done has made any impact on physics, and given the difficulties in establishing the existence of black holes, never mind checking for radiation from them, may never.

As regards Feynman's moral character, I refer the interested reader to my [Amazon review of "Surely You're Joking, Mr. Feynman"]. Feel free to add more unhelpful votes (currently 4 out of 4).

9. **Steve Myers**
July 24, 2007

It's easy to judge a life from the outside. Also a good rule to follow: never trust the opinion of an ex-wife.

10. **SteveM**
July 24, 2007

It is fair to say that if Hawking was able bodied then the general public simply would never have head of him. However, his condition coupled with the type of work he does has certainly captured the imagination of the public and created that rarest of things–the celebrity scientist. Easy to see how it must have put a huge strain on his marraige though. I like Hawking's way of thinking and doing things. His papers are usually clear and readable and his contributions to general relativity are important. Also, his radiating black hole work was the first time ideas from both quantum mechanics and gravitation came together in a consistent and very tantalising way; and of course, people are very much still thinking about that.

11. **A quantum diaries survivor**
July 24, 2007

You do not need my appreciation Peter, but I concur with gunpowder… Great post, and always a pleasure to read you.

Cheers,
T.

12. **island**
July 24, 2007

The highest pinnacle of success possible can easily mean that Hawking holds Newton's chair, which was held prior to him by Paul Dirac. It doesn't require any, "what have you done for me lately's" for the statement to be reasonable and correct.

I thought that the ideas that he presented at GR-17 were pretty interesting stuff though.

13. **Garbage**
July 24, 2007

Hey,

For anyone interested, there is an enjoyable paper by Page about Hawking calculation and some historical notes.

http://arxiv.org/pdf/hep-th/0409024

I think the credits should go to Zeldovich also, but as usual there is always a russian who did everything first…

I agree Hawking wont get upgraded to Einstein's level, but try to do math without pencil and paper, and let alone talk about not being able to do so! I even have troubles splitting the bill at the restaurant sometimes, I dont want to even try to imagine being in his shoes…

14. **anon on the hudson**
July 25, 2007

I wasn't going to respond to ths post. But the more I thought about Steven and Jane Hawking the angrier I became.

There is something morally rancid about Steven and Jane Hawking. As other people on this thread have pointed out, without the affliction of Amyotropic Lateral Scelerosis, 99.9 percent of the general public would have a clue as to who Steven Hawking is.

Amyoptropic Lateral sclerosis has given Steven Hawking massive celebrity status that he would never would have experienced if he did not have ALS-his enormous contributions to Black Hole theory notwithstanding.

The celebrity status that ALS has given Steven Hawking has made Steven Hawking a wealthy man. I have no problem with this.

However, Steven Hawking and his wife Jane have never-with the exception of a minor bleat last year about stem cells-used Steven's celebrity status to raise conciousness among the general public in the UK and the US and to call for a massive increase in goverment funding of ALS research and goverment support to his fellow PALS(people with ALS)

Instead we get a celebrity sob story loaded with stories about Steven's randy behavior. No doubt, a movie will soon follow, and Jane and Steven Hawking will be the wealthier for it.

In the mean time……ALS research will continue to remain underfunded and inefested with ego-maniac ALS researchers with marginal scientific talent(this is a good description of the Columbia Presbyterian Lou Gherigs center).

The suffereing for over 90 percent of PALS and their caregivers-CALS- is many orders of magnitude worse than Steven and Jane Hawking's suffering Under the circumstances, Steven and Jane Hawking have lived quite comfortably. Awash in $$$$$ these days, active social life, appearances on national TV and….a chance to act in a Star Tek next Generation episode!!!!.

For over 90 percent of PALS, every second, every minute every hour, every week,every month and every year is a living hell. Most PALS either commit suicide by not traching or are tricked into not traching by family members and hospice staff into not traching(One British legal scholar argues that under Anglo-American legal theory, tricking PALS into not traching, especially by medical profressionals, should be considered a very serious crime)

Steven Hawking was waited on hand and foot by a well paid nursing staff. In America, the majority of PALS are "taken care of by" by either a five dollar an indifferent home home health aid worker-many times not fluent in english-who often doze off in a chair for several hours or a nursing home where they decide to commit suicide after they become completely locked in.

I'm not intertsted in the "devout Christain" Jane's Hawking's sob story and tales about her husbands feynmanesque infidelitities.

Steve Hawking could have used his celebrity status-bestowed upon him by Amyotropic Lateral Scelrosis-to raise pressure both in the UK and the US goverment to massively increase funding for ALS research and goverment support for PALS. Steven Hawking choose not to do this.

Think off all the public appearances Stevn Hawking has made in the US. Steven Hawking has been on shows such as Larry King numerous times. So many blown opportunities. A real man in his condidtion would have used his appearances to make very strong statements about the lack of funding for ALS research and lack of goverment support for PALS locked in their bodies locked in a room with caregiver- year after after year in some situations.

Here is What Steven Hawking could have said on the Larry King show:"The US invasion of Iraq cost 11 billion dollars a week and will cost at least a trillion dollars when it is all over. These billions could have been spent curing Amyoptripic Lateral Sclerosis one of the worst diseases known to man. I am very forutnate to have a very strong support system. However, for my fellow American PALS, life is a living hell. This is outrageous. Put this money into curing Amyotropic Lateral Sclerosis and other underfunded small market disease. Eleven billion dollars a month for the invasion and occupation of Iraq? Eleven billion dollars could have cured this terrible disease many times over.

I don't care how many children Steven Hawking has sired. In my book, a real man-especially one whose his fame and forutne is a direct conseqence of being aflicted with Amyotropic Lateral Sclerosis-with Hawking's clebrity status and wealth would have demostrated a much higher degree of solidarity with his fellow PALS.

America, the nation with the largest number of PALS and their CALS, has made steven Hawking a wealthy man. His chidren and grandchldren will be economically secure for sure.

15. **Molnar**
July 25, 2007

I don't have a problem with people taking potshots at Hawking for his physics or his marital behavior (although I'm not about to throw any stones in those areas), but I think "anon on the hudson" has a misconception of what is considered good manners in most of the world outside the U.S. In most places with which I am familiar, it is considered perfectly acceptable to use one's fame to proselytize (politely) on behalf of unfortunates who do not share one's own affliction, but not on behalf of those who do; such a plea is typically considered self-serving, even if others are also in need, and may well backfire. Perhaps 25 years of Thatcher and Blair have brought the U.K. closer to U.S. standards on this, as well as the somewhat related (in term of social acceptance) behavior of aggressive self-promotion – I can't say. But as someone who has a 50% chance of getting ALS myself, I can say that I would never expect Hawking to do what anon asks.

16. **Kyle**
July 26, 2007

Anon coward,

John Bardeen had a good family life. About the only complaint his family sometimes gave was that he worked very hard. As Peter pointed out, I think most physicists have good family lives – at least as good as the profession will allow. Now that college and graduate education for physicists is breaking the decade mark regularly and they often have three to ten years of temporary employment afterwards, keeping a good family life is tremendously difficult.

As to the post,

Thanks for the review. However I generally disagree with the statement that Hawking has achieved "about the highest pinnacle of success possible." Obviously only he can tell us that for sure, but I have always considered his work at the same time very impressive, and not a fraction as important as people try to sell it as – much like Guiness world record holders. This includes his scientific and public outreach works.

I also consider the "weirdo super genius" identity that the public and physicists have conspired to create bad for the discipline, and both Einstein and Hawking are great examples of it in action. The public likes the idea that to be smart enough to work in something as esoteric as physics, we must have some terrible malady that makes up for the perceived difference in intelligence. Of course most of this difference in intelligence is fictional, but everyone deems it in their best interests to continue the charade.

In other words, I'm not overwhelmed with Hawking's accomplishments, and I feel he has done negative things for the public's understanding and perception of physics. This doesn't constitute a great success in my book.

I am quite sure others will vehemently disagree.

17. **M**
July 26, 2007

Hawking started quantum gravity, by showing that it could be a flourishing field. And that at least it sells books.

We will see if this field leads somewhere or if the final word will be a footnote in textbooks about astrophysical black holes: "we can ignore Hawking radiation with respect to radiation emitted by ambient matter".

18. **Who**
July 26, 2007

> In other words, I'm not overwhelmed with Hawking's accomplishments, and I feel he has done negative things for the public's understanding and perception of physics. This doesn't constitute a great success in my book.
>
> I am quite sure others will vehemently disagree.

Perhaps others do so, but I heartily agree and thank you for putting it succinctly.

19. **relativist**
July 26, 2007

Chris Oakley says "nothing Hawking has done has made any impact on physics". This is not true. According to Google Scholar, the black hole radiation paper has had 2999 citations. Hawking's next three best papers each have had over 1000 citations, and a further 8 have over 400 citations. No matter how you try to decry his work, this is a huge impact. The fact that he has had a major impact on physics rather than just mathematics is also made clear by the talks at his 60th birthday meeting (published in "The Future of Theoretical Physics and Cosmology", edited by Gibbons et al).

20. **Ck**
July 27, 2007

Molnar – what absolute rubbish.

Christopher Reeve campaigned relentlessly and tirelessly for his own cause – "self interest" as you so inappropriately put it. He was widely admired and supported for his extraordinary efforts.

How do you categorise famous people who have had breast cancer, for example, and then go on to help raise funds for that health problem? Or prostate cancer, and so on?

How to you view parents of sick children who make it their life's work to raise research funds and to gain gov't and public support for "their" cause, all out of "self interest", because they hold hopes of a miracle for their child?

Your comments are ignorant.

Look at the real world for goodness sake. People don't randomly "take up a cause", there is almost always a personal connection for them, whether it be a friend or a relative, or yes, even themselves! If this was NOT the case the arse would fall out of medical fund raising and research efforts in the blink of an eye.

You owe "anon on the Hudson" an apology, not for the rest of your opinion, but for being so blindly wrong about medical awareness and fund raising efforts.

21. **anon on the hudson**
July 27, 2007

CK

I am not interested in a knock down debate with molnar. To even write about Lou Gherigs disease causes me great pain.

In discussions on ALS websites, PALS and their CALS have expressed great diappointment that Steven Hawking and his ex-wife Jane have not used their celebrity status to bring much greater attention to the issue of the immorally low level of funding for Amyopropic Lateral Sclerosis research.

I don't want to be mean about this, but a case can be made that Steven Hawking has acted in a self-serving way.

Unfortunately, Steven Hawking gives the general public the impression that Amyoptropic Lateral scelerosis is a disese that a person can live with. If a PAL does not have a strong support system-which most PALS do not-Amyotropic Lateral Scelrosis is not a disease that most PALS can live with. As I mentioned previously, most PALS do not get trached(suicide).

In recent years, the "great" Steven Hawking has taken to making grand pronouncements about the future of humanity. Some of these pronouncements are ignorant and borderline stupid(humans will be replaced by robots in a few decades).

Steven Hawking probably thinks he is making a grand contribution to humanity by pontificating about the fate of humanity.

Well, if Steven Hawking is the least bit interested in making an important contribution to humanity beyond his work in quanum gravity, he may want to start with something as urgent and close by as the indescribable suffering of the 99.9 percent of his fellow PALS who are not in a position to use the terrible affliction of Amyotropic LAteral Scerosis to make themselves a wealthy celebrity.

Most PALS I know, if they were as wealthy and famous as Steven Hawking, would hammer home unrelentingly on TV and during public speaking engagements one point:99.9 percent of people with Amyotropic Lateral Sclerosis will die a horrible death or commit suicide unless the US goverment puts at least one billion dollars a year into ALS research.

For the last four months of his life, my brother was quadraplegic in a medical bed. He had a deep interest in cosmology. I bought several of Professors Hawkngs popular books. I would flip the pages for him as he read. I did this to distract him this horrific disease.

If Steven Hawking wants to make a nontrivial contribution to humanity outside of his work in physics he might consider making a very public statement along the following lines:the Iraq war costs the American people 11 billion dollars a month. If this money was spent on curing ALS-just one month of the Iraq war!!!!-a cure for for Amyotropic Lateral Sclerosis could be found very soon.

This would be a much better use of his celebrity status-bestowed upon him by Amyoptropic Lateral Sclerosis-than making stupid statements about human beings being replaced by robots in twenty to thirty years.

It's not too late Steven Hawking and Jane Hawking.

22. **Howking**
July 27, 2007

Relativist, Chris Oakley did mean "impact on stablished physics".

Just count how many Nobel Prizes for physics Hawking won.

23. **Chris Oakley**
July 27, 2007

There seem to be very few on this thread who are prepared to identify themselves. Obviously "Anon on the Hudson" has a tale to tell. It would be interesting to hear more, and although I respect his/her wish for anonymity, I do not quite understand it.

Re: Hawking and physics: If citation counts were all that mattered, then all of the Nobel prizes for physics in recent years would have gone to String Theorists. Luckily, the subject has not gone off the rails completely (although sometimes I wonder), and the Swedish Academy, at least, recognises that for something to be called "physics" it has to be verified experimentally.

24. **Molnar**
July 27, 2007

CK: Your sole counterexample to the theorem does not satisfy the conditions (Christopher Reeve was American), and the rest of your post is just handwaving: zero marks out of ten. Try to be a bit more rigorous next time.

Hudson: My only point about your post was that we should be sensitive to cultural differences. Being reserved about personal issues and keeping a stiff upper lip is a bigger part of British culture than American (or at least it was not too long ago).

25. **Juan R. González-Álvarez**
July 28, 2007

"Hawking started quantum gravity, by showing that it could be a flourishing field."

Are you sure M?

The first papers in quantum gravity were from Rosenfeld on the 30s. In the 30s the idea of the graviton was already introduced. Rovelli calls the prehistory of quantum gravity.

Hawking first 'important' paper on BH radiation was on 1974. But it was not about quantum gravity but an aplication of QFT on curved spacetime.

Four years latter Hawking revives the Wheeler-Misner-Feynman
approach in the form of "Euclidean quantum gravity". That is where Hawking enters in quantum gravity, a field however Hawking does not star.

Now if you have some time, count the multiple names between Rosenfeld 1930 and Hawking 1978: Bronstein, Feynman, Wheeler, Dirac, DeWitt, Pauli, Finkelstein, Weinberg, …

26. **Steve Demuth**
July 29, 2007

Several things trouble me about *Anon the Hudson*'s postings.

First, the equation of refusal to be "trached" with suicide, is I think, morally extreme. The right to refuse invasive life-exending medical procedures is widely recognized in the western world to be without the moral taint of suicide. It is a huge step backward in our understanding of autonomy in the context of medical care to assert otherwise.

Secondly, the explicit criticism of the Hawkings for not “raising consciousness … and advocating for massive increases in government funding” is at least problematic. The Hawkings may have made this choice for base reasons, but they may equally well have done so for morally sound and rational reasons. They may have concluded, for example, that however costly and devastating ALS is in their lives, it does not deserve to be a higher priority of government biomedical research that it already is. ALS after all is a rare disease (an average incidence in countries with primarily European populations of 4 per 100,000, and significantly less than that in non-European populations) that strikes people fairly late in life (average onset in the early 50s) that is not communicable or known to be environmentally triggered. In other words, as bad as it is, it appears to be a random act of nature that, compared with many conditions, destroys relatively few total years of productive life in society worldwide.

I know a little bit about ALS, and the impact it has on people’s lives, having seen a friends parent die of the disease, and having friends of my own with the disease. I certainly do not discount the real anguish *Anon on Hudson* no doubt feels through some close personal connection with the affliction. But I don’t think the response posted here with respect to Hawking is a productive use of that pain. Dying from a terrible disease by refusing treatment is not suicide, and could, for some, be a courageous and morally uplifting assertion of agency over a disease that has relentlessly ravaged their potential for agency. Choosing not to be a public advocate for spending on one’s own condition is not automatically a selfish act. It could well be a defensible, unselfish, decision that one’s own afflictions are not necessarily, by virtue of being one’s own, a priority for the world.

27. **anon**
    July 30, 2007

    In Hawking’s defense, I believe he has been an outspoken advocate for people with disabilities in general, even if not for people with ALS in particular. For example, in his various visits to developing countries he has been quite vocal in his support for improved wheelchair access and the integration of people with disabilities into society.

    So perhaps he has simply chosen carefully which battles to fight.

28. **Tanya A.**
    September 13, 2007

    Anyone who read Jane Hawking’s memoir, especially the first version of it, and paid any attention will be aware that they both did a lot of campaigning for ALS victims when they were married. After their divorce, Jane tried to become involved again but found herself blacklisted because of her separation from Stephen, since Stephen’s name carried more weight.

    I don’t want to say either one is a saint, or conversely to be too hard on either one of them because they were subjected to inhuman pressures (both of them) and turned out (both) to be only human. But they have always struck me as decent people and their bad moments as only human, and yes, it’s important to remember that charity is done somewhat differently in the UK. To accuse either of them and especially Jane of being indifferent to the potential uses of their fame to help others is simply ignorant. Jane Hawking continues to be very active in charitable causes, including for autistic children (she has an autistic grandson). And Stephen Hawking has been working to raise environmental awareness of late.

    Again, nobody’s perfect. But “morally rancid” is untrue and unfair. Please read the book before you make such comments.

Quick Links

July 24, 2007
Categories: Uncategorized

The New York Times has an article this morning by Dennis Overbye in its Science Times section about the hunt for the Higgs and the various rumors that were circulating earlier this year. It does a good job of accurately summarizing and reviewing the situation (although of course the blogs were and remain the place to go for breaking news, up-to-date and accurate information…). Steven Weinberg recalls the time back in 1977 when he quickly wrote up a paper with Ben Lee about a model concocted to explain rumored "trimuon" events (which turned out not to be there). There are quotes from bloggers Tommaso Dorigo, Gordon Watts and John Conway, and, in a new posting on his blog, Gordon is now trying to deny that he uses the term "Dude" in actual conversation. Unfortunately, anyone at D0 who knows anything seems to have clammed up, no more rumors that I'm aware of about whether they're seeing anything exciting.

The 2007 Europhysics Conference on High Energy Physics is going on in Manchester, England, and many of the talks are already on-line. This is a conference more aimed at experimentalists than theorists, so there doesn't seem to be much new in the theory talks. There are so many experimental talks that I think I'll have to wait for the summary talk to appear to figure out what to pay attention to.

There's a long list of things I was going to write about, but Sabine and Stefan at Backreaction got there first (here, here and here):

- Nature has a special section on the LHC. Very good and much more in depth than most of the huge amount of press coverage of this story. Especially interesting is the article by Chris Llewellyn Smith telling the history of how the LHC came to be.
- The LHC Theory Initiative, a now NSF-funded project that will provide some graduate fellowships and post-docs for people working in phenomenology relevant to the LHC, is being promoted with a University of Buffalo press release. It claims that currently Europeans dominate the field of LHC phenomenology, so the NSF funding is needed to stop this "outsourcing" of crucial high-tech employment to foreigners. HEP in the US is quite an amazing industry, the only one I know of that outsources technical work to countries where the labor costs more than it does in the US….
- This year's award for most ludicrous hep-ph paper is likely to be won in a walk by this one. Tommaso is even better than Sabine on the topic.

There's a new chapter out of the particle physics novel The Newtonian Legacy (blogged about here) by Nick Evans. Not often that the Cern Courier carries material about Higgsless models and lingerie in its pages…

Comments

1. **Gordon Watts**
   July 24, 2007

   Dude! I really don't use that word normally!

2. **Gordon Watts**
   July 24, 2007

   Dang it. How do you edit comments you've left?? 😉

3. **Kasper Olsen**
   July 24, 2007

   Concerning Holger and Ninomiya's paper:

   Yes the idea is crazy, and contrary to what we know, but I don't think it is fair to call it "ludicrous". If you accept some of the premises — which of course might be very hard — then certainly the idea is not foolish, or completely unreasonable.

   Try to read the paper, and present your own reason for why you think it is "ludicrous".

   Kasper

4. **DB**
   July 24, 2007

   Tommaso titles his review:

   > respectable-physicists-gone-crackpotty

   Could it be, that as an alternative to such an archaic and defective way of relieving oneself, they are in fact taking the piss?

5. **A quantum diaries survivor**
   July 24, 2007

   Kasper,

   the idea is dangerously bordering into the ludicrous, but I accept that it is still valuable science to test for backward causation.

   However, the idea that 5000 physicists financed by fixed fractions of the GNP of participating states accept to put at stake 20 years of efforts spent designing, building, and making operative the most daring enterprise in physics ever, to look for an effect preventing higgses from being produced, or worse, saving the LHC from a flood or an explosion, or people from being injured (sic) is
   O U T R A G E O U S
   and brings shame to their purporters and embarasses most of the reasonable physicists working for the CERN experiments.

   Cheers,
   T.

6. **matteoeo**
   July 24, 2007

holger-ninomyia's theory seems to be testable at least in principle, so it might still be better than ST in a sense…

anyway is everybody really sure it is not a joke? maybe they tried to write nonsense with the previous paper ("hit by bad luck") and, noticing no reaction, they have worsen it.

7. **A quantum diaries survivor**
July 24, 2007

Matteoeo, that would be a bit too much of a walk on the wild side. And if we have to live with either theorists who build non-testable theories or theorists who build theories whose testability depends on playing cards or "turn the bottle" kind of games, based on the chance of shutting down billion dollar experiments, I sincerely take the former.

Cheers,
T.

8. **Arun**
July 24, 2007

I liked this from Overbye (emphasis added)

*Joe Lykken, a Fermilab theorist who said he first learned of the rumored bump the old-fashioned way, over lunch in the laboratory cafeteria, said: "Pre-blog, this sort of rumor would have circulated among perhaps a few dozen physicists. Now with blogs **even string theorists who can't spell Higgs** became immediately aware of inside information about D Zero data."*

9. **Peter Woit**
July 24, 2007

Arun,

Funny, but the dissing of string theorists by their colleagues has become so common-place that I didn't even really notice Lykken doing this…

10. **Tony Smith**
July 24, 2007

Arun quoted Joe Lykken as saying
"… Now with blogs even string theorists who can't spell Higgs became immediately aware of inside information about D Zero data. …",
which supports my feeling that a lot of today's theoretical physicists are so wrapped up in superstring abstract math that they don't have the time and energy to really understand the Standard Model well enough (i.e., at a level of detail similar to that set out in the review sections of the Particle Data Group publications) to realize what fascinating stuff is being done at Fermilab and will be done at LHC.

Further support comes from the Resonaances blog, which in a 1 July 2007 post entitled "Nima's Marmoset" said:
"… Nima Arkani-Hamed [formerly at Harvard and now at Princeton IAS]… gave another talk …[at]… CERN … advertising his MARMOSET … a new tool for reconstructing the fundamental theory from the LHC data …
Nima pointed out …[that]… at the dawn of the LHC era we have little idea which underlying theory and which lagrangian will turn out relevant …
Nima says that this new situation requires new strategies … The idea is to study physical processes using only kinematic properties of the particles involved.
Instead of the lagrangian,
one specifies the masses, production cross sections and decay modes of the new particles. The amplitudes are parameterized by one or two shape variables. This simple parameterization is claimed to reproduce the essential phenomenology that could equally well be obtained from more complicated and more time-consuming simulations in the standard approach …
MARMOSET is a package allowing … Monte Carlo simulations of physical processes. As the input it requires just the new particles + their production and decay modes. Based on this, it generates all possible event topologies and scans the … parameters, like production and decay rates, in order to fit the data. The failure implies necessity to add new particles or new decay channels. In this recursive fashion one can extract the essential features of the underlying fundamental theory. …
Proffesionals say that MARMOSET does not offer anything they could not, if necessary, implement within half an hour.
On the other hand, it looks like a useful tool for laymen. …".

In short,
Nima says that the Standard Model Lagrangian should be ignored
because
"… we have little idea which underlying theory and which lagrangian will turn out relevant …"
even though the Standard Model has passed EVERY experimental test for over 30 years, and there is NO experimental observation whatsoever indicating that the Standard Model is not the relevant "… underlying theory and … lagrangian …" for physics at the LHC.

I disagree with Nima, and feel that the Standard Model Lagrangian (unless and until some experimental result disagrees with it) should be the primary basis for analysis of data from the LHC.

As a layman who supports the Standard Model, and whose physics model includes the Standard Model as a subset, I resent the characterization of MARMOSET as "… a useful tool for laymen …",
especially when
it seems to be primarily a tool designed by superstring theorists to enable them to avoid troubling their highly trained minds (a la Majikthise and Vroomfondel) with the details of the Standard Model.

Tony Smith

11. **ObsessiveMathsFreak**
July 25, 2007

> # This year's award for most ludicrous hep-ph paper is likely to be won in a walk by this one. Tommaso is even better than Sabine on the topic.

At one point I was regretting it, but right now I am ***so*** glad I picked mathematics instead of physics. The field is … a disaster.

What we have got here, is a failure of process.

12. **Eric Mayes**
July 25, 2007

Actually, physics is quite healthy. It's only because of the efforts of certain people to mislead and disparage that you have this impression.

13. **Xerxes**
July 25, 2007

This arxiv paper appears to have been cribbed from the plot of "Einstein's Bridge", a novel in which the reason for the SSC being canceled was that time-traveling physicists sabotage the effort in order to prevent a dire future catastrophe associated with particles produced there. Take SSC–>LHC and time-traveling physicists–>consistent histories interpretation of QM, and it seems a near match.

14. **H-I-G-G-S**
July 25, 2007

The amusing thing about the Lykken quote is that not that long ago
he was selling himself as a devoted string theorist on NOVA. He, like
the author of this blog, is an opportunist.

NOVA: What is so compelling about string theory to make you want to devote your career to it?

Lykken: I think when a lot of people go into theoretical physics, they're looking for the big answers to the big questions. String theory holds out the promise that we can really understand questions that you might not even have thought were scientific questions—questions about how the universe began, questions of why the universe is the way it is at the most fundamental level. The idea that a scientific theory that we already have in our hands could answer the most basic questions is extremely seductive. Of course, for this to really happen, string theory first of all has to be right, which we don't know, and then we have to be able to test it and understand it eventually in experiments.

String theory itself probably won't be understood even in my lifetime at the deeper level. But I do think that there are ideas coming out of string theory that we will test and we will confirm hopefully in experiments, and that's what I'm really hoping for. I want to see during my career that at least some of these big ideas coming out of string theory we'll actually get our hands on and see that they do happen in the real world.

15. **Peter Woit**
July 25, 2007

HIGGS,

OK, I see that there is one string theorist who can spell the word…

You may be right that there is a certain amount of rats/sinking ships going on here. As for the accusation against me of "opportunism", I've been saying things critical of string theory and the behavior of string theorists for a very long time. When I decided to start doing so publicly, many people told me I shouldn't do this, that it wouldn't be good for me at all, and that I'd become the target of personal attacks, often anonymous ones…

16. **Aaron Bergman**
July 25, 2007

Apparently people can't even make jokes these days.

17. **anon**
July 25, 2007

Aaron, you are not even funny.

18. **Tom Whicker**
July 25, 2007

Higgs,

To say "String theory itself probably won't be understood even in my lifetime at the deeper level" is to beg the question that there
is a deeper level to string theory. It is similar to Jackson Pollock
putting the last drips of paint on "Full Fathom Five" and saying
"I may never fully understand this art work."

TW

19. **anonymous**
July 25, 2007

It seems to me that being skeptical of string theory has become fashionable of late. I'm pretty sure this is a phenomenon of those who aren't skilled and smart enough to work in string theory. Some of these people are crackpots, and some are real physicists who work on topics which are a little out of the mainstream (i.e. 'alternative approacjes). I guess they think being skeptical makes them look smart, even though these people generally produce nothing of their own. It's always easier to sit back and criticsize than to do some actual work.

20. **Aaron Bergman**
July 25, 2007

Aaron, you are not even funny.

But do the lurkers support me in e-mail?

21. **Peter Woit**
July 26, 2007

anonymous,

Well, I'm pretty sure that you and other string theorists' conviction that skepticism about string theory "is a phenomenon of those who aren't skilled and smart enough to work in string theory" is a phenomenon of people crippled by a pathological degree of arrogance.

22. **wat happened to Lubos?**
    July 26, 2007

    Maybe it's inappropriate to ask here but I'm curious to know if Lubos has been fired or if he voluntarily resigned. If he did get fired, was it due to his blunt remarks and disrespect for other academic researchers?

23. **amused**
    July 26, 2007

    I'm pretty sure that if string theorists were as smart as they think they are then ST papers would be dominating the pages of PRL, just like the papers by the smartest mathematicians dominate the pages of Ann. Math. and Invent. Math. (Of course, string theorists profess not to care about journals any more, but it's hilarious to see how keen many of them are to submit to PRL whenever they think they have a chance.)

24. **anonymous**
    July 26, 2007

    Most string theorists don't like to publish in PRL because it's too restricitive. It's difficult to communicate results in string theory to a broad, general audience within the four page limit. Most string theory papers will be found in JHEP, Nuclear Physics B, Physical Review D, or Physics Letters B.

25. **amused**
    July 26, 2007

    Well, string theorists don't seem to have any problems when it comes to hyping their work in the general press, where the audience is even more broad and the restrictions more severe…
    It's no more difficult to communicate results in ST than in other areas of formal theory, and from what I've seen most string theorists love publishing in PRL just as much as the rest of us, even though they are a bit reluctant to admit it 😉

26. **DaveC**
    July 26, 2007

    My impression is that the peer expectations amongst string theorists don't push them towards PRL as they do the rest of us. At the same time there is surely some awareness that most of their results don't actually meet the PRL broad interest requirement. On the other hand the arrogance of anonymous is laid out as clear as day before us here. Does s/he think any physicist outside string theory (the vast majority of us, remember) is going to be impressed by moaning about the four-page limit?!

    More on-topic, Gordon has called me 'dude' quite a few times. He shouldn't be allowed to get away with denying it, but I also hope he doesn't stop.

27. **Amos Elberg**
    July 26, 2007

    I have the same question about Lubos…

28. **random dude**
    July 26, 2007

    Dudes, why does this Gordon Watts dude have a problem with the word `dude'?

29. **SnarkFest**
    July 26, 2007

    *Skepticism about **X** is a phenomenon of those who aren't skilled and smart enough to work in **X**.*

    Now that's a nice rhetorical trick! I think I'll start using it…

30. **mclaren**
    July 26, 2007

    Snarkfest's comment seems apropos. The comment by anonymous that skepticism of string theory is *a phenomenon of those who aren't skilled and smart enough to work in string theory* is nothing but the old long-debunked ad hominem argument. Anyone who disagrees with my arguments is too stupid to understand them, therefore not worth bothering to refute.

    The 2 obvious problems with this kind of ad hominem argument are:
    [1] it is demonstrably untrue, since highly skilled and knowledgeable phycisists like Peter Woit and Lee Smolin and Steven Wienberg have been vocal about their skepticism of string "theory"; and [2] the implication behind this kind of ad hominem argument is that we cannot apply the generally accepted requirements of the scientific method to string "theory" because string "theory" is so allegedly complex and so purportedly sophisticated.

    Let's review the generally accepted requirements for a scientific theory, shall we?

    (A) To qualify as a scientific theory, it must have a rational basis and must use a maethmatical framework to describe nature. String "theory" qualifies on this point.

    (B) To qualify as a scientific theory, the mathematics must yield testable falsifiable predictions. String "theory" fails on this point.

    (C) To qualify as a scientific theory, the theory must be fertile and give rise to additional insights about the physical world which in turn can be tested and confirmed or disconfirmed, and lead to additional areas of productive research. String "theory" also fails on this point.

    The entire purpose of the scientific method and the peer review process is to insure that people who not extreme insider experts in a narrow niche field can nonetheless make a general evaluation of claims by other researchers. To claim that each field of scientific knoweldge is so esoteric and so advanced that no one outside that tiny subspeciality is qualified to evaluate the validity of papers published in that field, is to deny altogether the utility of peer review and, in effect, to abandon the scientific method. If that were really the case, only the handful of specialists in any given field would ever be qualified to judge the validity of that science, which means that essentially everyone would be UNqualified to evaluate whether ANY science was valid.

    Nope. Wrong.

    Peer review works. The scientific method works. I don't have to know all the details of the latest experiments in shotgunning out blocks of conserved junk DNA in mice to know that it heralds some important new discoveries about the relationship between the various kinds of RNA and the traditiional paradigm of information transmission through the genome. I don't have to know these details in order to eb sure the science is valid, because these papers have been published in peer-reviewed scholarly joujrnals, and their results have been replicated by other researchers.

String "theory" does not even qualify as a scientific theory because it has made no testable falsifiable predcitions and no resarch data (obviously) have been published in peer-reviewed scholarly journals replicating those findings. It's not necessary to know anything more than that to become skeptical that string even qualifies as "theory." If we deny this chain of logic, we must abandon the peer review process and throw out the scientific method.

31. **mclaren**
July 27, 2007

> …Being skeptical of string theory has become fashionable of late. I'm pretty sure this is a phenomenon of those who aren't skilled and smart enough to work in string theory.

The old ad hominem fallacy.

Ad homneim arguments won't convince anyone that string is a "theory," but falsifiable testable predictions might.

Got any?

32. **anon.**
July 27, 2007

mclaren, if string ever made any truly falsifiable predictions, nearly everyone in that subject today would run away, for fear of it being disproved. You don't have the intelligence to grasp that not having any falsifiable predictions actually makes a science more attractive to slow thinkers. If they try working in a fast moving research program which makes falsifiable predictions, their future is insecure.

The brains behind all big corporate research efforts involving many people is lack of falsifiability. It is a great benefit in many areas, not just religion but also politics, and now string theory. You just need to become more broad minded, then you'll begin to understand how stupid your comments are. You're under the illusion that string theory should be a 'shut up and calculate' discipline. The whole point is that it's a 'shut up and speculate' subject. The less likely it is to ever make a falsifiable prediction, the stronger it becomes, since moronic critics can't disprove any of it.

33. **lyme**
July 27, 2007

> Steven Wienberg have been vocal about their skepticism of string "theory";

As far as I know, S. Weinberg is an advocate of ST, though not a practitioner himself.

34. **lyme**
July 27, 2007

Maybe I should give some basis to my opinion.
See hep-th/9702027 and hep-ph/0401010, for example. A short quote from the latter paper,

> I am emphasizing here that it took a long time before we realized what these ideas
> were good for partly because I want to encourage today's string theorists,
> who I think also have good ideas that are taking a long time to mature.

35. **schtirlitz**
July 27, 2007

"highly skilled and knowledgeable phycisists like Peter Woit and Lee Smolin and Steven Wienberg"
LOL, very amusing indeed!

36. **schtirlitz**
July 27, 2007

"To qualify as a scientific theory, the mathematics must yield testable falsifiable predictions. String "theory" fails on this point."

Is the existance of the Higgs boson a falsifiable prediction of the Standard Model?

37. **schtirlitz**
July 27, 2007

Oops, I meant EXISTENCE

38. **Peter Woit**
July 27, 2007

mclaren,

Please stop posting rants here. This isn't helpful and does nothing to encourage informed discussion, quite the opposite.

schtirlitz,

Yes the existence of the Higgs is a falsifiable prediction of the Standard Model. All its properties, including its spin, charge and all couplings to all known other particles are predicted. There is one undetermined parameter in the Standard Model, the Higgs self-coupling, which, if known, determines its mass. The LHC should be able to cover the entire energy range where effects of the Higgs are supposed to show up, no matter what the self-coupling. If the LHC sees no sign of the Higgs, the Standard Model is falsified. We're hoping this happens….

39. **schtirlitz**
July 27, 2007

Thanks Peter! I agree, although the Higgs mass cannot be predicted, there exists an upper bound which the LHC covers. BUT it only becomes falsifiable once the LHC

comes about.
OK, then a similar question:
Was the existence of the 3-rd generation of quarks a falsifiable prediction? It seems to me that there was a huge uncertainty about the top mass in the context of the SM. Am I wrong? If the top mass were ~O(few TeV), would the SM have been falsified?

40. **Peter Woit**
July 27, 2007

schtirlitz,

As I've often written about here, the notion of "falsifiability" is not such a simple one. Different theories contain different amounts of wriggle room, from one extreme where nothing is adjustable to another where almost eveything is. The Higgs is a good example of a conventional falsifiable theory, with one undetermined parameter, and a vast number of highly precise predictions with zero wriggle room that can be made and tested for each value of that parameter.

Once the tau was found in the mid-seventies, the SM predicted a bottom and top quark with specified properties, except for two undetermined parameters, the masses. The bottom quark mass was fixed by the observation of the Upsilon in 1977. After that, I believe there were various upper bounds on the top quark mass from precision electroweak experiments, but I don't remember the details of that story.

41. **schtirlitz**
July 27, 2007

Thank you for your answer Peter but to be honest, I'm not satisfied with it. I agreed with your answer about the HIggs mass but don't you think that the discovery of Upsilon was a lucky thing in itself?
I thought that the answer to my question was trivial:
The existance of the 3-rd generation was not falsifiable since, for example, the masses could not be fixed by the theory. It took about 20 years of searching to discover the top – a clear indication that it's existence, predicted way back in the early seventies was pretty much in the same category as either technicolor or superpartners or extra dimensions right now – unfalsifiable!

42. **Peter Woit**
July 27, 2007

schtirlitz,

I don't have time to look up the references, with a little effort you can find them yourself, but precision electroweak measurements implied a specific range of values for the top quark mass. Before its discovery there were only a range of values for the mass such that the SM was consistent with all the data. The SM made a falsifiable prediction that there would be a top quark with specific properties in a specific mass range. A particle with exactly these properties was found in exactly the predicted mass range. This is a textbook case of a conventional sort of testable, falsifiable scientific prediction.

The other things you mention are a mixed bag each with different degrees of wriggle room.

43. **lyme**
July 27, 2007

Without claiming any expertise in these matters, I'd say that without a 3rd generation there'd be no CP violation in the SM, and it'd go down the experimental drain.

With a non-standard 3rd generation, without a top, there's no anomaly cancellation in the SM and, again, it's falsified.

No top = no SM.

44. **schtirlitz**
July 27, 2007

"Before its discovery there were only a range of values for the mass such that the SM was consistent will all the data."

But the data you are referring to was only available a few years before it was discovered. It took 20 years of data collection to nail it down! It was unknown upriory what its mass range would be. The SM itself does not require it to be that much heavier than the other quarks, does it?

So, by the same token MSSM pheno people can say that there exists, for instance, a gluino with very specific properties and the upper bound on its mass still undetermined for the lack of precision data, just like was the case for the mass of the top quark in the 70s. If the LHC discovers a gluino it will be just like the dicovery of Upsilon you mentioned above – a lucky accident!

45. **schtirlitz**
July 27, 2007

"With a non-standard 3rd generation, without a top, there's no anomaly cancellation in the SM and, again, it's falsified."

Nope, the anomalies cancel in each successive generation.
It was CP violation by K_0 that prompted Kobayashi and Maskawa to suggest the existence of the 3rd generation.

But again, low scale superpartners have been suggested to stabilize the hierarchy, explain the electroweak symmetry breaking (why the Higgs mass parameter turns negative), dark matter, gauge couling unification, etc. The MSSM has many input parameters which can in principle be fixed by precision data, so what? It's just like the Standard model in the 1970s when a lot of its parameters were not fixed!

46. **Eric Mayes**
July 27, 2007

Schtirlitz,
Don't forget that the top mass was predicted to be very heavy by radiative electroweak symmetry breaking in minimal supergravity back in the early 80's, which is another strong piece of evidence for the 'not even wrong' theory called supersymmetry.

47. **Peter Woit**
July 27, 2007

1 undetermined parameter or 105, it's exactly the same! A theory that has been tested precisely 1000s of times or one that predicts almost nothing, exactly the same!!

Geez….

48. **Eric Mayes**
July 28, 2007

Peter,
It seems to me that supersymmetry predicts much more than ‘almost nothing’. In addition to radiative electroweak breaking and a correspondingly large top mass, it predicts the existence of superpartners to the known particles, which are estimated to be in the TeV scale range. Your point seems to be that this idea is useless since we cannot predict what the precise masses and mixings of these states will be, since this depends on the exact mechanism by which supersymmetry is broken. I and others have pointed out that this is no different that the situation in the 60’s and early 70’s when the Standard Model was just beginning to emerge from the fog. You seem to be in denial about this and oblivious to the way science actually works.

49. **Peter Woit**
July 28, 2007

“oblivious to the way science actually works”

Eric,

You really should consider whether ad hominem attacks and insulting people is the right way to carry on a scientific discussion.

I’m not going to review the history of the Standard Model here, it’s a complicated story, one that is very different than the story of supersymmetry.

50. **lyme**
July 28, 2007

> Nope, the anomalies cancel in each successive generation.

That’s what I meant. After the Upsilon was discovered there had to be a top for the anomaly to vanish.

But, more to the point, and I’m speaking only for myself, I wouldn’t say susy is not even wrong. My personal opinion is, it’s theoretically compelling but phenomenologically ugly due to the abnormal obesity of its superrenormalizable sector… But it might well happen that either an experimental discovery or a theoretical breakthrough give it the plastic surgery it needs to be pretty again.

51. **lyme**
July 28, 2007

schtirlitz , there’s perhaps one more point that deserves clarification.
When you say “Was the existence of the 3-rd generation of quarks a falsifiable prediction?” the answer is that its non-existence would have led to an enormous crisis in particle physics.

On one hand, the SM would have been unable to explain CP violation in the K0 system even in principle. On the other hand , the SM was supported by an enormous amount of other data. That’d have been the kind of phenomenological contradiction that would have led to the prediction of a third generation. Historically, experimentalists did a good job, got lucky, and found the Upsilon before that hypothetical crisis happened.

But the non-discovery of susy would not lead to any crisis, because there is not a single experimental datum supporting it. Unlike the case of the 3rd generation, the absence of superpartners at the TeV scale would just mean that susy is another smart theoretical idea which happens not to be realized in Nature.

That scenario is certain to happen in the next few years: There are lots of smart theoretical ideas around for physics beyond the SM, but it is extremely unlikely that all of them will be phenomenologically relevant no matter how many good theoretical arguments back them.

52. **Eric Mayes**
July 28, 2007

Peter,
Stating a fact is not an ad honimem attack. This just seems to be your fall-back position whenever you are losing the debate. At any rate, you can’t continually make ad honimen attacks on string theorists as you have done and then complain when they respond in kind.

53. **Gphillip**
August 1, 2007

Peter, I don’t believe that just because the SM doesn’t predict everything possible it is necessarily falsified. It may be a bit incomplete, but that has yet to be proven. So far, every model of reality ever conceived has been a bit incomplete. I would really be more surprised if this one wasn’t. Now, if it made predictions that that were proven false by experiment, which I’m not positive the LHC can do, the we’re off in search of a better model, which I have no doubt we will be doing someday anyway.

Loved the LHC Roulette paper. You Dudes and Dudettes need to lighten up and smile now and then. Of course it was a very dry joke and greatly enjoyed. Yes, one can actually demonstrate reverse causation, of a sort. Unfortunately, the effect cannot be separated from random events until present measurements are made. So the Eigenvalues did collapse before the measurement was made. Unfortunately you can never detect that until after the measurement is made. It’s the same effect of trying to use entanglement to communicate faster than light. Yes, the signal will arrive faster than light (which can be constructed into a reverse causation scenario), but the signal can’t be resolved from random noise without a key (or the results of another measurement) that must arrive by pokey old light speed.

But hey! Keep trying Dudes and Dudettes. If we can ever get this one right we’ll make a killing at the casino! And keep smiling. Life is short.

University Grants Program Subpanel Report

July 27, 2007
Categories: Uncategorized

The HEPAP University Grants Program Subpanel has just issued a report, concerning the "University Grants Program" in US HEP, that part of the DOE and NSF high energy physics budget which supports research based mainly at universities (as opposed to government laboratories such as Fermilab). Obviously this is the part of the HEP budget that is of most direct concern to university researchers, especially theorists, who receive most of their government funding this way (a small number of theorists are supported by national labs, not universities).

On the experimental side much of the report is concerned with how to manage what will happen over the next few years as many researchers move from working on experiments in the US to the LHC, in particular how to deal with the higher travel and living expenses this will require. I'll concentrate here on some comments on the extensive parts of the report that deal with theoretical particle physics.

The report is surprisingly light on actual budget data, with few specific numbers about past budget trends, current budget levels or future budget plans. 2006 NSF university grant funding is given as $19 million for experimental particle physics, and $11.8 million for particle theory, astrophysics and cosmology. DOE university grant funding is described as about $110 million per year, with no breakdown between experiment and theory. The only historical data given is that this kind of DOE funding peaked in 1992, at a level of $150 million in current dollars, supporting a total of 1685 people back then, as opposed to 1495 in 2005. The main budgetary recommendation of the report is that 1 % of the total US HEP budget (about $8 million) be redirected to the university grants program as the SLAC and Fermilab collider programs wind down over the next few years.

The recommendations for theoretical particle physics mostly concern funding for graduate students, calling for increasing the number of graduate students in particle theory, especially students working on calculations directly relevant to LHC experiments:

> Funding directed at university-based theoretical particle physics for the purpose of increasing the number of HEP-grant-supported graduate students should be given a higher priority in the overall HEP program. Support for students and postdocs doing calculations related to upcoming experiments is particularly urgent.
>
> Though the universities are strong in formal theory, there has been a decline over the years in conventional particle theory (phenomenology), for a variety of reasons. Phenomenology embraces a number of different areas, including data analysis, collider physics, computational physics, perturbative QCD, lattice field theory, model building, flavor physics, and neutrinos; it overlaps with such areas as strings, astrophysics, and cosmology. All these areas are important; but those directly connected with the LHC are increasingly critical. The entire LHC experimental program requires a strong theoretical component involving calculating Standard Model backgrounds and new physics processes, together with interpreting the experimental results and teasing out their implications. However, the number of theorists working on such topics in the United States, especially at the universities, is inadequate. For example, there are only a handful of people in the U.S. working on computational physics, such as event generators. Many more will be needed to fully utilize the physics potential of the LHC. It is important that much of this effort be centered at universities because (a) much of the experimental analysis will be done at universities, and (b) a university presence is needed to attract graduate students. A general concern is the overall decline in the agencies' support of graduate students in theory, both formal theory and phenomenology. This decline makes it difficult to train a sufficient number of students. The problem is aggravated by increasing competition for the limited number of available teaching assistantships (TAs) from students in other subfields of physics.
>
> A key component of a strong Terascale physics program (at the LHC and the ILC) is a strong theoretical program involving the calculation of Standard Model backgrounds and new physics processes, together with interpretation of the experimental results. However, as pointed out in this report, the number of theorists working on such topics in the United States, especially at the universities, is inadequate. Addressing this vital need requires an additional level of effort.
>
> Overall, the field of high energy physics faces several critical manpower and infrastructure problems. Declining graduate student support affects the intake of new physicists and therefore the future of particle physics overall.

The report gives no actual numbers (which, presumably are available, since the DOE and NSF should have counts of how many students they support each year), but, based on responses from a survey of university grant PIs, it says that the number of grant-funded RAs for HEP grad students has been decreasing, with, especially for theorists, student support having to come from TAs:

> The overwhelming response stressed that the level of (NSF and DOE) grant support for RAs for graduate students is insufficient and has indeed been declining over the last decade. At the same time, respondents noted that the cost of supporting a graduate student on a grant has increased, especially because of stricter university requirements regarding tuition remission and fringe benefits.
>
> As a result, particle physics groups routinely rely on other sources of funds for all or part of their graduate student support. One major resource is TA positions for particle physics students, addressed in more detail below. Some respondents noted that their universities have limited fellowship support for some students. A handful also mentioned seeking outside support from other federal agencies. Many said that they had been forced to turn away qualified students due to a lack of grant support. Some also indicated that students had turned down the chance to join a particle physics research group because other departmental areas could promise steadier RA support, rather than a mixture of TA and RA support…
>
> Survey respondents brought up several difficulties caused by this reliance on TA support. First, spending time as a TA slows senior students' research progress (increasing their time to graduation) and hampers experimental students' ability to travel to particle physics labs. Second, TA support is also currently a declining resource for particle physics at many institutions, because university administrations are providing less overall TA money to physics departments, departments are reducing the number of semesters any student may spend as a TA, or other physics subfields are requesting more TA slots. Third, if a particular research group (usually particle physics theory) makes unusually large demands on the available TA slots, this creates friction and resentment within the department as a whole.

The report has no mention at all of what the desirable level of particle theory Ph.D.s might be from a larger perspective. There is zero discussion of the relationship between how many such Ph.Ds are produced, and how many jobs doing particle theory research are likely to be available in the future. This is presumably because the authors are well aware that there remains a huge imbalance between the number of smart people getting Ph.D.s in this subject, and the number of opportunities for them to make a career in the subject. The reference to "friction and resentment within the department as a whole" over TAs makes clear what one of the main concerns driving this recommendation is. The amount of power and influence one's group has in an academic department, including prospects for being allowed to hire more people, is heavily influenced by how much grant money one brings in, especially how much funding for the graduate program one can provide. This is made even more explicit at another point in the report:

> Because of eroding support, more and more theoretical graduate students are being required to teach more and more of the time. This is unfortunate for at least two reasons: first, it lengthens the time to degree; and second, it signals to physics departments a hint of declining support for HEP research, exacerbating hiring worries.

While the report mentions the need to find some more money for postdocs (since those working on experiments will need to travel to Europe more often), it emphasizes support for grad students, not postdocs. It does this even though it is postdocs who are doing much of the most original work in the field, and the small number of postdocs (and junior faculty positions) is what makes career prospects for particle theory students extremely problematic. I can't even really make sense of this one paragraph from the report that deals with this:

> Given the choice between hiring more graduate students and taking on a postdoc, many faculty members will opt for the latter when faced with limited available

funding. However, while this may seem be the best solution in terms of immediate research workload, the long-term negative effects of this choice on the field as a whole are clear.

One of the most interesting things in the report is the set of numbers from survey responses about how many grant-supported theory researcher are working in which areas, and what hiring plans by area are for the next 5 years. Figure 3 on page 43 divides theory researchers into six categories, and gives counts for how many are working in each category now, how many expected in 2012. The number of string theorists is supposed to drop from 103 to 84, "field theorists" from 91 to 77, "model builders" from 88 to 70, and "QCD/Lattice QCD" from 50 to 41. "Particle phenomenologists" are supposed to increase from 188 to 194, and "astrophysicists and cosmologists" from 136 to 176. Obviously boundaries of these fields are unclear, especially since string theory in recent years has to some extent moved away from formal theory, with more people describing themselves as "string cosmologists", "string phenomenologists", "string-inspired model-builders", and much of the attention of the field devoted to trying to do QCD calculations with string theory.

If you take these numbers seriously, a grad student would be nuts to work on anything except cosmology or phenomenology, since all other subfields show about as many people leaving them as would be accounted for by retirements, so essentially no new hiring. My suspicion though is that these numbers reflect what departments say they would like to do, not what they will do. Most departments now say they want to hire in the areas of cosmology and phenomenology. But faced with the fact that competition for the best people in those areas is tough, and finding it much easier to get good people in other subfields, I suspect there will continue to be quite a lot of hiring in these other subfields, in string theory especially, which seems to be what looking at the latest data from the Rumor Mill shows.

Given the huge and supposedly increasing dominance of these numbers by the "particle phenomenology" category, the report's call for an urgent increase in funding to produce more phenomenologists is not so easy to understand. However, the authors make clear that what they want to see is more of a very specific sort of phenomenology: people working on things like event generators to precisely calculate standard model backgrounds for the LHC experiments, something which has always been more popular in the Europe than in the US. The recent NSF-funded "LHC Theory Initiative" is specifically designed to address this, and is being promoted with nationalistic calls to fight the "outsourcing" of these calculations to European theorists. The authors of the report are calling for more Ph.D.s in conventional, experiment-based phenomenology, not more Ph.D.s in string theory or "string phenomenology". They choose to do so, for obvious political reasons, not by calling for a reallocation of resources within the field of particle theory, but for new resources for this purpose, even if it will lead to a smaller fraction of particle theory Ph.D.s being able to find jobs doing the research they have been trained to do.

Comments

1. **AGeek**
   July 27, 2007

   Can the creation of event generators really be considered "theory"? You get a Lagrangian from a model builder, plug it into an automated Feynman diagram package, obtain matrix elements ready to plug into a standard procedure skeleton, do a little debugging and testing, and presto, there's your new event generator. Is it "theory" just because you don't get to pull cables?

2. **Coin**
   July 27, 2007

   For example, there are only a handful of people in the U.S. working on computational physics, such as event generators.

   I'm not sure I understand this sentence. "Computational physics" means computer simulations of physics processes? What is an "event generator"?

3. **Peter Woit**
   July 27, 2007

   Coin,

   An "event generator" uses Monte-Carlo methods to, given a model (e.g., the Standard Model), generate "events" with statistically the expected properties, where the "events" correspond to, say, events at a collider. The way you test your model is to run these events through a further simulation of what they will do in your detector, then compare the Monte-Carlo results to your data.

   For perhaps the most well-known of these, see

   http://www.thep.lu.se/~torbjorn/Pythia.html

   AGeek seems to think that the development of this kind of software is not "theory", but I think many people would disagree, including some who would claim that AGeek's attitude is what's wrong with particle theory these days…

4. **Zathras**
   July 27, 2007

   So what justifies the huge increase in cosmology positions over the next 5 years? Is there a perception it has been neglected recently?

5. **AGeek**
   July 27, 2007

   I confess: I don't think that the development of event generators is theory. It's programming, and pretty elementary programming too, to the point that I don't see why it could not be completely automated, given an existing simulation framework.

   Surely an unwillingness to label trivial technical work "theory" is not the worst ailment afflicting particle physics these days? Some might say that the opposite is true…

6. **J**
   July 27, 2007

   More fundings for graduate students? Is this really gonna happen?

7. **LDM**
   July 27, 2007

   "The Role of Computation in Physics" By Bradley A. Shadwick in the January 2007 issue of Computing in Science & Engineering clearly and intelligently explains how computation fits into theory and what its limitations are.
   I would never consider computation or simulation to be physics theory per se (though Stephen Wolfram it would appear has a different, yet fundamentally misguided

view), but instead it is a potentially potent tool, which when wielded correctly, can do what all tools do – amplify our own feeble abilities, which in this case means help us solve equations.

8. **IMHO**
   July 27, 2007

   Peter said:

   > While the report mentions the need to find some more money for postdocs (since those working on experiments will need to travel to Europe more often), it emphasizes support for grad students, not postdocs…..I can't even really make sense of this one paragraph from the report that deals with this:

   For all of your insight regarding the health of the field, you sometimes seem naive with regards to politics. Money talks and you can't seem to see that you have won…..or maybe it was Smolin that won….either way, be gracious in victory.

9. **Peter Woit**
   July 27, 2007

   IMHO,

   I'd be curious to hear what you actually think about the politics. Whether or not my views on it are naive, I'm always interested in maybe learning something.

   I definitely noticed that at least this panel is not calling for more funding for string theory, but more funding for phenomenology and a redirection of most hiring to cosmology don't seem to me to be the answers to what ails particle physics. Seeing that already fewer theorists describe themselves as "field theorists" than "string theorists", and physics departments plan on reducing the number of field theorists further is rather discouraging…

10. **Alexey Petrov**
    July 28, 2007

    But faced with the fact that competition for the best people in those areas is tough, and finding it much easier to get good people in other subfields…

    Not necessarily. For instance, our department is (almost religiously) trying to hire a biophysicist. For several years. No success so far — and it is understandable — we don't have any yet and we are not Harvard (although we do have a decent Medical School). So it really depends on the way a particular department is run and on the vision of its administration/administration of the College, etc. Usually, however, it does not work to the advantage of HEP…

    And even people who used to be particle physicists, but now are astrophysicists (and there are many) do not help — their move from HEP to astro is viewed by other faculty as a sign that HEP is dying out and there is no point in investing in it by hiring more faculty.

11. **reply from a phenomenologist**
    July 30, 2007

    this is a reply to AGeek, who thinks that people who work on event generators are not really particle theorists.

    Of course this claim depends on one's definition of theorist. But let me point out that constructing event generators is much more than lagrangian + programming. There's a whole jungle of physics between the raw experimental data and inferring terms in a lagrangian, and experience shows that you need bona fide particle theorists to do the job. Experimentalists can't do it, string theorists can't do it. And hired programmers certainly can't do it. I appreciate this isn't obvious, but that's the way it is.

12. **AGeek**
    July 31, 2007

    Dear phenomenologist, it is certainly true that "There's a whole jungle of physics between the raw experimental data and inferring terms in a lagrangian", but that is not what building event generators is about. As I wrote, "You get a Lagrangian from a model builder". What you are describing is the job done by the model builder. Event generators are derivative work used to extract predictions from a Lagrangian provided by a model builder in a form directly comparable to experimental data: the information flow is the reverse of what you describe. (You may be thinking of Marmoset; that's something much more ambitious than I have in mind.)

    In principle, it is of course possible that somebody building an event generator also constructed the underlying model. Such a person would be model builder with a handy technical skill, comparable to being unusually good at doing integrals.

13. **Peter Woit**
    July 31, 2007

    AGeek,

    You're misinterpreting "phenomenologist", who was just pointing out to you that the relation between terms in a Lagrangian and experimental data is quite complicated, involves a lot of physics, and this is what event generators are all about. You seem to think that writing down terms in a lagrangian is non-trivial physics, whereas designing an event generator that allows you to compare these to experiment is "a handy technical skill, comparable to being unusually good at doing integrals". An equally good case could be made that writing down terms in a Lagrangian is "a handy technical skill", with the non-trivial physics requiring real knowledge and sophistication being the connection to experiment. Something leads me to suspect that you've written down terms in a Lagrangian, but have never written an event generator…

14. **AGeek**
    July 31, 2007

    Peter Woit,

    you are misinterpreting me, who am simply pointing out that the trivial technical task of building event generators does not involve sorting out the relation between terms in a Lagrangian and experimental data.

    Writing down any terms in a Lagrangian is indeed fairly trivial. The generation of all terms compatible with a given set of symmetries and other conditions can also be automated. Writing down terms relevant to our world without performing an exhaustive search of the entire theory space, now that's something which sets a Weinberg apart from a marmoset.

    BTW, your suspicion is incorrect. I have done both. It may interest you that I did the event generator thing before having ever laid eyes on a QFT textbook, simply by being a good geek and having a superficial, undergrad level understanding of particle physics. Unfortunately, it quickly became clear to me that the people doing it for a living did not have a knowledge of physics significantly beyond mine either. They had stopped learning and growing once they had found this little niche where they could provide a service in demand.

15. **Peter Woit**
July 31, 2007

AGeek,

Apologies for my incorrect suspicions.

My own experience though is that the phenomenon of people who have “stopped learning and growing once they had found this little niche where they could provide a service in demand” applies just as well in other much more abstract parts of the field.

16. **string theorist**
July 31, 2007

>It may interest you that I did the event generator thing before >having ever laid eyes on a QFT textbook, simply by being a good >geek and having a superficial, undergrad level understanding of >particle physics. Unfortunately, it quickly became clear to me that >the people doing it for a living did not have a knowledge of >physics significantly beyond mine either.

This is a shockingly arrogant (and narrow-minded) statement, if by `event generator’ you mean what is normally meant by an `event generator’, that is pythia, herwig, etc. Do a SPIRES search on people who do write event generators. events radiate in the initial and final states, jets are produced, partons shower to create more jets, hadrons decay. how many jets should you expect? how many hard? how many soft? how much missing energy how often? none of this is at all obvious from the Lagrangian.

That said, the kind of event generator someone could write without having done any QFT was indeed probably not a great achievement in particle theory.

Anyway, what next? those people who do multi-loop computations don’t do particle theory either, they just have a handy technical skill in doing integrals? and the cosmologists who do N-body simulations, maybe they should be reclassed as computer support officers, after all the real physics was all done by Einstein.

on the other hand: writing down a half-arsed model whose only virtue is that it hasn’t been ruled out yet; that, now that takes a *real* physicist.

17. **AGeek**
July 31, 2007

Dear “string theorist”, I think we are talking past each other.

First of all, how to handle parton showers, jets, hadron decays: it is perfectly true that “none of this is at all obvious from the Lagrangian”. Because, as you surely know, this all happens at strong coupling. So it’s handled by a phenomenological model motivated by, but not derived from, first principles. Essentially it’s a messy piece of code which parametrizes experimental data.

If you want to call this an “event generator”, then your statement that “the kind of event generator someone could write without having done any QFT was indeed probably not a great achievement in particle theory” is rather questionable. Because, as you point out, “none of this is at all obvious from the Lagrangian”, and little knowledge of QCD beyond an intuitive understanding of gluon strings stretching and breaking up goes into the code handling it.

Second, this hadronization stage is not something which you have to worry about every time you create an event generator. It’s encapsulated in the simulation framework which handles all the output from your event generators, by which I mean procedures working with elementary particles according to matrix elements derived from a Lagrangian. The typical job of creating an event generator consists in writing such a procedure, which takes a list of elementary particles as input and produces a list of elementary particles as output, and linking it into the framework.

18. **M**
August 1, 2007

I heard this same debate some years ago, when a young physicist expert of event generators sent a public letter to his influential collaborators writing something like: “my advisor warned me that developing event generators is not considered theory, so in order to get a position I need to stop our collaboration and move to something else.”

19. **locrian**
August 6, 2007

I’ve heard a number of people argue that studying high energy physics is a more reasonable career choice now because of the opportunities for employment created by the LHC. It seems to me, after reading the report and this post, that this isn’t true. The number of new permanent jobs created isn’t going to be significant compared to the number of graduates that are going to be produced.

In other words, rather than making the profession more healthy, the LHC may just give graduate students a more comfortable ride to nowhere.

In what way am I being too cynical?

Really Quick Links

July 31, 2007
Categories: Uncategorized

The Fermilab Steering Group is about to come out with its report. Their roadmap for Fermilab proposes that if the ILC project is delayed "Fermilab should pursue additional neutrino and flavor physics opportunities", in particular with "Project X", a high-intensity proton linac. One of their remaining tasks is to pick a name for Project X.

In the category of magazine articles that I hear have just come out, but I don't have a copy of, and aren't available on-line, there's

- a cover story about the state of particle physics and string theory by yours truly in the latest Cosmos magazine
- an article about Lisa Randall in Vogue.

The talks at the recent Imperial College event in honor of Abdus Salam are now on-line. This is the event Steven Weinberg boycotted, but Gerard 't Hooft was there to talk about Salam and the state of theoretical physics. His talk was entitled "The Grand View of Physics", and is available here.

Among the recent large "'07" conferences with talks available on-line are:

- Loops '07, mainly on LQG.
- SUSY '07, mainly on supersymmetry.
- Lattice '07, mainly on lattice gauge theory. Blogging from Georg von Hippel, including a description of today's Creutz–Kronfeld celebrity deathmatch over rooted fermions.

Comments

1. **Thomas Larsson**
   July 31, 2007

   Randall (from Backreaction).

2. **Yatima**
   July 31, 2007

   As you can see, Lisa has been reading NewScientist.

3. **Coin**
   July 31, 2007

   And for some reason my chief response is: goodness, there are people who still use blackboards?

4. **Jonathan Vos Post**
   July 31, 2007

   Coin:

   "… goodness, there are people who still use blackboards?"

   I assumed that she was using a whiteboard, but that a symmetry-breaking field from an adjacent brane had acted upon the color-space somewhere between Lisa and the wall.

5. **Sebastian Thaler**
   July 31, 2007

   Peter-

   Congrats on the COSMOS article–I believe that magazine is sold at my local Barnes & Noble and I'll look for this issue. By the way, I notice that Lee Smolin's book is now out in softcover. Are there plans for a softcover edition of your book?

6. **Peter Woit**
   July 31, 2007

   Sebastian,

   Cosmos is a well-known Australian popular science magazine, I'd be surprised if it's available in the US, but maybe it is. My book is also supposed to be coming out in softcover. I haven't seen any copies of that edition yet, but it should be out sometime in the next few weeks.

7. **Intellectually Curious**
   August 1, 2007

   Coin,

   I for one think that students actually do learn better from having to take notes at the same time the professor is writing on the blackboard. It builds discipline and helps them think (or at least remember the material). Young people nowadays who rely too much on technology also rely it to think for them. They don't know how to think anymore. It's sad.

8. **ObsessiveMathsFreak**
   August 1, 2007

   > And for some reason my chief response is: goodness, there are people who still use blackboards?

   I cast thee out!!

9. **Gphillip**

August 1, 2007

Interesting article on the state of string theory. I agree with the "It's a bust" side. People should remember that physics is just a model of reality. It is not and never will be reality itself. Reading "The Computational Universe" it's clear that to have a completely accurate model of the universe would require something with the size and energy of the Universe itself to run the model.

So on the end, a model is only as good as it's predictive ability, and simpler is better. String theory is neither simple nor does it produce a useful and new fundamental predictive ability. Even worse, string theory leads us to a conclusion that the universe is this way because this is the way God made it. That's great if one is seeking a physical proof of a divine being, but it's a complete dead end for modern physics.

So what's next? Or at least, what are the characteristics of what's next? Well, it should be simple in concept. String theory started out simple in concept, that everything was just vibrating strings of energy, but it's formulation led us to a point where even thousands of the brightest physicists and mathematicians can't completely understand it. Next, the theory must be modular or expandable. That is, as new concepts are understood, the theory must be capable of incorporating those new concepts without having to be completely reworked. Here I make the reasonable assumption that no theory will ever be a completely accurate model of the universe. If that's so, then new concepts will be formulated and proven as long as curious physicists draw breath.

Finally, no matter how simple or complex, restricted or free ranging, the model must provide new fundamental predictions that can be experimentally verified. In my own humble opinion, and in that of many in the field, this is (was?) the death of string theory. It turned out not to be a theory of everything, but a theory of anything. In order to give the theory the necessary degrees of freedom to back-predict things already known, it forward predicts things that are inconsistent with our known universe, and an almost infinite amount of them at that.

I don't know what the next theory will be, but looking through the literature I see several possible good candidates. My personal favorite is a quantum gravity theory that the universe is made up only of a continiuous fluid, perhaps a space-time fluid with no particle size. In that theory the particles we measure are only qualities of that area of the fluid. Those qualities can be represented by a field similar to a tensor field that that can be represented by a suitable sized matrix of terms which act on the fluid. From what I've read, it will still take extra dimensions of either time and/or space (an extra curled up dimension of time simplifies things greatly). But it should come as no surprise at this point that additional dimensions will be required to represent the degrees of freedom needed to model our universe. This is my current favorite because as additional concepts are proven they can just simply be added to the existing matrix creating an ever more accurate model.

In any case, string theory is dead. Long live the next greatest thing.

10. **dan**
August 1, 2007

Peter,
for those in the US without access to the Cosmo article, is there anyway we can read your article?

11. **locrian**
August 1, 2007

Heh, did no one notice? From the Cosmos webpage:

"Includes perspectives from leading experts, such as Michio Kaku, co-founder of string theory. . ."

Hahaha. The charlatans win again.

12. **Peter Woit**
August 1, 2007

dan,

Sorry, but I myself don't even have a copy yet of the magazine, in paper or electronic form, so I don't know what the edited version looks like, or what other people have to say. But as for my piece, what I sent them wouldn't be anything new to anyone who regularly reads this blog…

13. **Chan Tun**
August 1, 2007

> as Michio Kaku, co-founder of string theory. . ."
>
> Hahaha. The charlatans win again.

I'd almost agree with this, except I wouldn't call charlatan someone with more citations than I. At 2000+ citations, it's going to be some time before I have the arrogance of calling Kaku a charlatan. But I wouldn't bother to disagree if someone with the proper credentials called him so. The question, Locrian, is what you have to show in your record that outdoes Kaku's.

14. **Peter Woit**
August 1, 2007

Kaku wrote some of the early papers on string field theory, and thus has some claim to be a co-founder of string field theory. Unfortunately the "field" sometimes gets dropped by the press..

And please, stop the attacks on people as "charlatans" or whatever, it's obnoxious and just lead to pointless hostility, something this subject doesn't need more of.

15. **locrian**
August 1, 2007

Chan Tun,

That question of my record versus Kaku's is actually something that has been mentioned here before. It's something I'm quite comfortable with, for reasons that might not be obvious. However, it's also a terrifically inappropriate subject for Peter's Blog.

Kaku's designation as the "co-founder of string theory", on the other hand, seems an entirely appropriate subject. It speaks volumes about the way the media interacts with the scientific community and the public.

16. **ttn**

August 1, 2007

I had no idea Robert Stadler was working at Fermilab…

17. **Coin**
August 1, 2007

I for one think that students actually do learn better from having to take notes at the same time the professor is writing on the blackboard.

My comment was meant to be taken in the sense of "…as opposed to whiteboards".

Kaku wrote some of the early papers on string field theory, and thus has some claim to be a co-founder of string field theory.

Is string field theory in some way related to Ads/Cft, or otherwise something which might have ongoing uses as a mathematical construct even in the absence of string theory proper?

18. **Peter Woit**
August 2, 2007

String field theory has nothing to do with AdS/CFT. It's a field theory, where the fields are defined not on space-time, but on the infinite-dimensional space of open or closed strings in a space-time.

19. **ad**
August 4, 2007

Why did Steven Weinberg "boycott" the event held at the Imperial college in honor of Abdus Salam?

20. **woit**
August 4, 2007

ad,

His letter explaining this is available here:

http://www.engageonline.org.uk/blog/article.php?id=1036

21. **Maynard Handley**
August 5, 2007

"Coin,

I for one think that students actually do learn better from having to take notes at the same time the professor is writing on the blackboard. It builds discipline and helps them think (or at least remember the material). Young people nowadays who rely too much on technology also rely it to think for them. They don't know how to think anymore. It's sad.
"

Gee, if only there were a way to establish the truth or falsity of such beliefs? Some sort of objective method for probing whether assertions about the world are true or not. We could call it, I don't know, the scientific method, or experimentation, or something.

Isn't the whole problem with education that it is based on "I for one think that students actually do learn better…" rather than anyone (apart from Carl Wieman http://name99.org/blog99/?p=33) actually testing these hypothese?

22. **Arun**
August 6, 2007

Maynard Handley: I think we have to distinguish between what works on the whole versus what works for the best students, which is a priori not the same.

Another Journal Board Resigns

August 6, 2007
Categories: Uncategorized

Last year about this time the entire editorial board of the Elsevier journal Topology resigned, this August it's the turn of the Springer journal K-theory. The editors of this journal have all resigned, issuing the following statement:

> Dear fellow mathematicians,
>
> The Editorial Board of 'K-Theory' has resigned. A new journal titled 'Journal of K-theory' has been formed, with essentially the same Board of Editors. The members are A.Bak, P.Balmer, S.J.Bloch, G.E.Carlsson, A.Connes, E.Friedlander, M.Hopkins, B.Kahn, M.Karoubi, G.G.Kasparov, A.S.Merkurjev, A.Neeman, T.Porter, D.Quillen, J.Rosenberg, A.A.Suslin, G.Tang, B.Totaro, V.Voevodsky, C.Weibel, and Guoliang Yu.
>
> The new journal is to be distributed by Cambridge University Press. The price is 380 British pounds, which is significantly less than half that of the old journal. Publication will begin in January 2008. We ask for your continued support, in particular at the current time. Your submissions are welcome and may be sent to any of the editors.
>
> Board of Editors
> Journal of K-theory

The subscription cost for the Springer journal had been $1590, $1325 for electronic-only access.

I notice that while the editorial board of Topology has resigned, that hasn't caused Elsevier to stop publishing and selling the journal. While I'm sure that a recent paper copy of the journal that I saw did not carry the names of the editors that resigned, the online version of the journal appears to still carry the names of the old editors, giving no indication that they have resigned. As far as I know, Elsevier has not been able to recruit a replacement editorial board, but they are still selling the journal, at a yearly subscription price of $1665.

Update: Via the comment section, there's the related news that

- The Ecole Normale Superieure has chosen to no longer have Elsevier publish the journal Annales Scientifiques de l'École Normale Supérieure; the new publisher will be the non-commercial Société Mathématique de France. The Elsevier website states:

> As of 2008 no longer published by Elsevier, please contact publisher Societe Mathematique de France for details.

and, unlike the case of Topology, they appear to be no longer trying to sell subscriptions to the journal. Presumably the ENS controlled rights to the journal and its name so was able to simply remove it from Elsevier, unlike the case of the former editorial board of Topology.

- Bruce Bartlett reports that a wiki called MathSciJournalWiki has been set up, devoted to providing information about scholarly journals, especially in mathematics. Members of the math community are encouraged to contribute to it.

Update: It turns out that there may be more to this story. See in particular the comment posted here by Andrew Ranicki, who says that no papers submitted to *K-theory* since April 2006 have been forwarded to Springer, and that he and Wolfgang Lueck will be acting as interim managing editors for the Springer journal to sort out this situation.

Comments

1. **Coin**
   August 6, 2007

   As far as I know, Elsevier has not been able to recruit a replacement editorial board, but they are still selling the journal, at a yearly subscription price of $1665.

   What content, if any, has the journal contained during this time?

2. **milkshake**
   August 6, 2007

   Even if a journal withers, the publishers have copyright to journal archives and those will continue to be valuable source of subscribtion for many years to come. But something should be done now because the current scientific journal subscription rates are a bloody robbery.

   In related news, the profits from CD publishing is way down, the major music publishing houses are laying off their staff and/or are being sold for a fraction of their past market value.

   http://www.prospect-magazine.co.uk/article_details.php?id=9735

3. **DB**
   August 7, 2007

   Evidence of a many-journal theory and particularly apposite given:

   http://blog.sciam.com/index.php?title=many_worlds_in_oxford

   :))

4. **anonym**
   August 7, 2007

   Is there an 'official' publication of the letter ? The text, as cited above, does not even has a date. The homepage of A.Bak, the managing editor, has no reference to the letter.

5. **anon.**
   August 7, 2007

Scientific publishing can be a very lucrative business. The great Robert Maxwell made his first fortune from setting up scientific publishers Pergamon Press, whose journals had no limits on articles sizes (expensive page charges to authors institutions ensured that long articles were in fact most welcome).

http://news.bbc.co.uk/1/hi/business/1249739.stm

Of course, for readers, you end up with two types of problem:

(1) The journals either become filled with long papers from collaborations of authors who can share out the page charges, which drives out smaller groups and individual researchers from such journals as they can't afford to publish there, or alternatively:

(2) The publisher keeps page charges low but makes a fortune by selling the journal at an enormous cost, so the reader in an out-of-the-way place has extreme difficulty in seeing the journal at all.

Many college libraries only have a fraction of the total number of paper journals. To keep costs down, most journals are only available online over a university intranet, so the journals become subscription versions of arXiv. I think there's a problem here, since it's harder to study a lot of mathematics on a screen than in a printed journal. Hence the three internet layers:

*The www (no restrictions on authors or readers)

*Arxiv (authors endorsed by peers; no restriction of readership)

*Online paid-for journals (papers restricted by peer-review; readership restricted to academic institutions, etc., which pay for access)

So the more money the poor reader in Outer Mongolia has to pay in order to read your scientific paper, the more elite and respectable you become as a public-spirited scientist!

6. **Peter Woit**
August 7, 2007

anonym,

The letter was an e-mail dated Monday August 6 from Anthony Bak, forwarded by Dan Grayson to the K-theory announcement mailing list that he manages.

7. **Bruce Bartlett**
August 7, 2007

Dear fellow concerned academics,

This is great news!

There have been some discussions going on at the n-category cafe and elsewhere, discussing whether the community should run a wiki devoted to math and physics journals, aiming to fight back against the greedy practices of the large publishing comglomerates by exposing their awful prices and policies.

If you think the time is ripe for a "MathSciJournalWiki", then pitch in a hand and help out! The current embryonic site can be found here. So far it has accessed the data from the AMS survery in 2004.

I've just added this great news about the editors of K-Theory resigning to the front page of the site. But I'm an awful HTML coder, and I'm sure one of you could improve things.

Remember, it's a wiki. We're all in this together, we all contribute as equals. *You* are in charge of the site!

8. **Laurent**
August 7, 2007

Dear Peter,

You should notice too that the "Annales de l'école normale supérieure" have stoped their publishing with Elsevier. Here is part of an email I recevied a few months ago
"'J'ai le très grand plaisir de vous annoncer que l'Ecole Normale Supérieure, après avoir dénoncé le contrat avec Elsevier concernant l'édition et la diffusion des Annales de l'ENS, a signé un nouveau contrat de partenariat avec la SMF"
I would have difficulties to transalte "dénoncé le contrat" in english but this means the contrat has been volontary broken. Starting in 2008 they will be edited by the SMF, a nonprofit association (not a firm) and (knowing the usual prices of the SMF) this will be much cheaper !

9. **Peter Woit**
August 7, 2007

Thanks Bruce and Laurent,

I'll add the news from you two to the main posting.

10. **anonym**
August 7, 2007

Thanks for the clarification.
It is funny, though, that Geometry&Topology (and some others)
journal has become non-free meanwhile, approximately at the same time when Topology moved to a different publisher.

And it is funny that on Topology&Geometry old website the papers are still downloadable for free; on the new one, they are not (and it has no link to the old website, and no link to arxiv publications).

At the same time, we are changing the price structure and access to our journals. The journals were started by a group of enthusiasts who dedicated their time freely. For long-term stability of the journals, this group of enthusiasts is gradually being replaced by permanent paid staff. The income generated by subscriptions in the presence of a free on-line version was not sufficient to cover this new structure and we have been forced to start to charge for on-line access. However all our publications will continue to be stored in the arXiv and therefore freely available to individuals. However access to our main sites will only be allowed from an IP address in a subscribing institution. All readers are encouraged to persuade their libraries to take out electronic subscription. Electronic access will be open and free for all publications after a *period of two years*.

11. **Bruce Bartlett**
August 7, 2007

Hi Peter,

It's great news to hear about the Annales Scientifiques de l'École Normale Supérieure now being published by a non-commercial society. Thanks a lot for adding the link to the MathSciJournalWiki to the main section. I think I'd prefer it if it said something like "Bruce Bartlett reports that a wiki about mathematics journals has been setup, where any member of the mathematical community is free to contribute skills or ideas", the idea being that it's a wiki, and that it's run by the community.

12. **woit**
August 7, 2007

OK, Bruce, I changed the wording. Good luck with that project!

13. **bob**
August 7, 2007

Presumably there are still articles in the pipeline that were accepted under the current Editorial Board, which may make it dicey for libraries to cancel *K-Theory* immediately. This may also be why Elsevier is able to publish *Topology* under the names of the resigned Editorial Board.

14. **Alexey PEtrov**
August 7, 2007

Putting papers on ArXive would not solve the problem, as they are not peer-reviewed (one can start a standard discussion on whether or not one needs peer review). There are other possibilities — keep journal on-line only (see JHEP) or keep per-page charges, but let the authors keep the copyright and distribute the journal free (see Advances in High Energy Physics from Hindawi publ.).

15. **Henry Walloon**
August 7, 2007

University research is, in part, funded by the tax payer.

There seems a basic issue of fairness to demand that the interested amateur pay again to be allowed to read the consequences of his or her investment.

The point by Alexey about the need to preserve peer review is a good one however.

16. **Coin**
August 7, 2007

Alexey, is there some obvious way that an Arxiv-like system *with* peer review could be organized? My understanding is peer reviewers have to be paid; how much of a journal's operating costs does this make up? Would there be any value in taking a system like the Arxiv with free publication and tacking on the ability for peer review to be provided for some kind of fee?

17. **A.J.**
August 7, 2007

Coin,

Peer reviewers are generally not paid, not in any sizeable amounts. People peer review because a) it's essential to scientific progress, and b) the system does not work if no one peer reviews.

18. **David Savitt**
August 7, 2007

The news about K-Theory and Ann. Sci. ENS is heartening, but I'm concerned that at least in the short term this will increase total subscription costs for university libraries rather than decrease them. Many or most university libraries have package subscriptions to major publishers such as Springer and Elsevier, and I have a hard time imagining that the Elsevier price will go down when Ann Sci ENS is dropped from the package (or do I not understand how these packages work?); in the meantime we will need to purchase a new, separate subscription from the SMF. One plausible outcome might be that my library is not able to afford the separate Ann Sci ENS subscription, or at least that it will make it much easier for the library to cancel Ann Sci ENS at some future moment of belt-tightening. So I am nervous. If this is indeed a problem, could various national professional mathematics organizations consider forming a consortium and offering a journal subscription package in the way that major publishers do?

19. **Matt Daws**
August 8, 2007

Following David Savitt's comment, I have a similar worry. Many of these big journals now have "electronic-only" options, where the subscribing institution gets electronic access, but only while they continue to subscribe. I always thought these were dangerous, as obviously if you cancel your subscription, you loose everything. It's a pain to have to go and look at paper journals, but as a pure mathematician working in a slightly unfashionable field, I find myself doing this a lot anyway. I guess many libraries thought that with big names like "Topology", the odds of wanting to cancel the subscription were low. We don't know what Elsevier will do with Topology, but it's quite possible it will continue, but as a much weaker publication. Some universities might loose a lot if they cancel their subscriptions.

This said, the LMS recently moved their publications to Oxford University Press, and they have provided full electronic access to all the past issues (I believe, many of these were scanned and converted by OUP). But then the LMS moved from CUP to OUP in, I guess, convivial circumstances.

20. **Bee**
August 8, 2007

Hi Peter,

Off topic, but thought you'd be interested: The recent New Scientist issue has a nicely written article about neutrinos which says:

Last year Päs, who is based at the University of Alabama in Tuscaloosa, together with Sandip Pakvasa of the University of Hawaii in Honolulu and Thomas Weiler of

Vanderbilt University in Nashville, Tennessee, looked at neutrinos from the perspective of string theory. This led them to suggest that sterile neutrinos, unlike most of the particles and forces that we see, can take short cuts through one of the normally invisible higher dimensions that string theory provides for, and so appear travel through time (NS, 20 May 2006, p 34).
Their model indicated that if a sterile neutrino time-travelling through extra dimensions has an energy that lies in a particular range, if will flip into a normal neutrino ([hep-ph/0611263](hep-ph/0611263)). "We published a graph predicting just the excess of electron-neutrino events at low energy observed by Miniboone," says Päs.
So does this, or one of the other possibilities opened up by Miniboone, mean that it has caught sight of the theory of everything that will, everyone hopes, one day unite the fundamental forces?

(typos are entirely mine).
From [*Neutrinos: The key to a theory of everything*]() (access restricted).

What do you think about that?

Best,

B.

21. **woit**
August 8, 2007

Bee,

I did see that, but New Scientist articles with over-hyped claims about physics that invoke string theory are just too numerous to take the time to discuss, so I'm mostly just ignoring them.

As for this particular speculative idea, it also seems best to ignore it. I'd wait to see if there's any confirmation of the excess of events at low energy seen by MiniBoone, and even then, I strongly suspect there will be a less exotic explanation of the phenomenon than new kinds of particles travelling through extra dimensions…

Personally I'd really rather continue to ignore this, but if someone has an intelligent comment about this that they must share, please do so not here (where you would be interfering with an interesting discussion of another subject), but at the next posting, which should appear soon…

22. **Bee**
August 8, 2007

Hi Peter:

Thanks. I agree on the 'exotic' explanations. It's interesting though there is such a large a market for this. Conservatism doesn't seem to sell well these days. Otoh, maybe it would be misplaced in NEW scientist. Best,

B.

23. **Alexey Petrov**
August 8, 2007

Coin:

As A.J. said already, referees are not paid for their services. As far as I know from my limited experience, members of the Editorial Board are not paid either. The main costs of online journals are associated with running the servers, processing contracts with subscribers, and paying to people who do all those technical things. I think the closest thing to "peer-reviewed ArXiv" is the Journal of High Energy Physics (JHEP). It does require subscription fees (which are infinitesimal compared to Elsevier's ones). Also, all of the accepted papers are available on ArXiv, which is nothing but a "virtual" preprint storage facility (supplemented by the "endorsement system").

24. **John Baez**
August 13, 2007

Peter –

I think if you talk to some of the other editors of *K-Theory* (not the managing editor), you'll get a much more strange and disturbing story about what actually happened here.

25. **Andrew Ranicki**
August 13, 2007

Statement on K-theory

========

In January 2007 the editors of "K-theory" resigned, following a request by the then managing editor Professor Anthony Bak. As announced in August 2007, some of them are intending to start a new "Journal of K-theory" to be published elsewhere. Unfortunately no manuscripts submitted to "K-theory" have been forwarded to Springer by the managing editor, since April 2006. We have been asked by Springer to act as interim managing editors, in the first instance to deal with these papers. We ask authors who have submitted papers to "K-theory" which have not yet been published to please contact one of us as soon
as possible.

Wolfgang Lueck (Muenster) lueck@math.uni-muenster.de
Andrew Ranicki (Edinburgh) a.ranicki@ed.ac.uk
13th August, 2007

26. **robin2**
August 14, 2007

A number of years ago my wife temped for a while at one of Elsevier's offices. She couldn't understand why academic journals used publishers *at all*. Obviously the contributors don't get paid, and as has been pointed out generally neither do the reviewers nor the editors. So all the publishers are really doing for their money is acting as intermediaries between the editors and the printers.

27. **Syksy Räsänen**

August 14, 2007

Commercial journals are indeed redundant. Alternatives for open and free journals have been discussed on CosmoCoffee at

http://cosmocoffee.info/viewtopic.php?t=276 and

http://cosmocoffee.info/viewtopic.php?t=280 .

A pilot project, RIOJA, has already been set up:

http://cosmocoffee.info/viewtopic.php?t=826 .

28. **Dylan Thurston**
August 16, 2007

In principle journals are also supposed to do copy-editing, which they have to pay for. In practice most commercial journals manage to severely degrade papers by copy-editing them. Geometry and Topology, on the other hand, does a great job with copy-editing in my experience, which may explain why they had trouble covering their costs.

Various Stuff

August 8, 2007
Categories: Uncategorized

As anthropic pseudo-science spreads through the particle theory community, I'm finding it harder and harder to tell what's a joke and what isn't. Maybe I'm wrong, but I fear that recent examples from hep-th contributors and prominent physics bloggers aren't actually jokes, largely because if they are, they're not funny.

The book Universe or Multiverse?, based on a series of Templeton Foundation supported conferences, and published by Cambridge University Press, is finally out. It's edited by Bernard Carr, whose ventures into pseudo-science include not just this, but a stint as director of the Society for Psychical Research. He's also on the board of directors of the Scientific and Medical Network, where his blurb tells us that:

> My interests span science, religion and psychical research (which I see as forming a bridge between them)… My approach to the subject is mainly theoretical: I'm particularly keen to extend physics to incorporate consciousness and associated mental and spiritual phenomena.

The memoir by Jane Hawking that I recently wrote about contains her recollections of both Don Page and Bernard Carr (since they worked with Hawking).

I just ran into my editor at Cambridge University Press, who found that opposition from string theorists made it impossible for Cambridge to publish my book a few years ago, with one of their arguments being that doing so would damage the reputation of the Press. Publishing pseudo-science like this however seems to be fine. Yes, I'm aware that this book also contains criticism of anthropic arguments, and probably has some of the most intelligent and informed writing on the subject, but still… I suppose I should get a copy of the book and write a review (I've already read many of the articles, they're available as preprints on-line), but the thing costs $85, the Columbia library doesn't have a copy, and I'm not sure I should encourage them to buy one.

This week's string theory hype: Universe's Stringy Birth Revealed by Young Czech Physicist, which is not about Lubos Motl, but about an award to Martin Schnabl. Schnabl's work on string field theory is one of the more interesting recent results in string theory, but the title of the article is, well, complete bullshit.

There will be an opening celebration in October for the Berkeley CTP, which was founded a few years ago and recently moved into renovated quarters. The BCTP is just one of a bunch of other CTPs that have been founded in recent years, including the MCTP and the PCTP (and one dead one, the CIT-USC CTP). The center's web-site and opening conference appear to be heavily dominated by string theory, quite a change from a few years ago, when Berkeley was one of the leading US physics departments where string theory was not so dominant.

The PCTP has begun construction of its new home in Jadwin, the physics building at Princeton. Artist's renderings are here. An art historian friend once told me that the proper technical name for the architectural style of Jadwin was "brutalist". The new construction will add lots of glass, perhaps mitigating the "brutalism". The large Calder featured in front of the building is called "Five Disks: One Empty", and it has its own rather brutal history. It collapsed during construction, killing two of the men working on it. According to a local Princeton web-site:

> The steel structure has four disks, one of which was originally painted orange, in a fervor of enthusiasm for the school's colors. The structure was named "Many Disks: One Orange," but then all of them were painted orange in anticipation of the artist's visit in 1971. Upon seeing the structure, he asked that all the disks be painted black, and renamed it to its current title.

Over at SciTalks August is String Theory Month, and they'll have Jonathan Shock as guest blogger later in the month.

At the Stony Brook YITP, the fifth of a series of workshops funded by Jim Simons on mathematics and physics, but mainly devoted to string theory, is now going on. Talks are online here.

Some online conference summary talks that one might want to take a look at are those of Michael Dine at the IAS PITP summer school, and John Ellis at SUSY 07. Both Dine and Ellis discuss prospects for observing supersymmetry at the LHC. Dine lists some of the reasons one might be skeptical that this will happen, including string theory anthropic landcape arguments (he avoids using the term "anthropic principle", insteard referring to it as "NBN, that principle which cannot be named"). Ellis recalls his own role in the "discovery" of supersymmetry by UA1 back in 1984, indicating it's likely that there will be such premature claims again at the LHC if anything at all anomalous is seen by the experiments. He also discusses the possibility of searching for long-lived particles produced at the LHC by using the muon system to locate where they left the detector, and then taking core samples of the surrounding rock to look for them.

For some excellent detailed postings about recent experimental HEP results from Tommaso Dorigo, see here and here. For blogging from CHARM 07 by Alexey Petrov, see here.

David Vogan has a wonderful expository piece about the recent heavily publicized results on the representation theory of E_8; it's intended for a future issue of the Notices of the AMS.

The September issue of the AMS Notices is now available. It includes an article about "Higgs Bundles", a version of the Higgs that physicists won't really recognize, and a book review of Lee Smolin's The Trouble With Physics. The review is quite positive about the book and mostly a straight-forward summary of what it is in it. The reviewer, like many mathematicians, had been misled by a lot of the hype about string theory, and so found Smolin's book quite enlightening. In particular, about M-theory, he writes:

> This explanation was, to me personally, a great shock since I had always believed M-theory was a complete theory.

Comments

1. **reader**
   August 8, 2007

   The book review you mention has an amusingly unconventional view of the job market in particle theory. Writing about Smolin's seers, it says

   "still others have jobs but are taking risks with their careers for the future of their science ('t Hooft and Penrose)."

   That 't Hooft and Penrose needed to fear for their careers was not something that had previously crossed my mind….

2. **Bee**
   August 8, 2007

   what bothers me is not if people have interests that go beyond science, and include psychology, religion, mountain climbing or blogging, but if they mingle up fact with fiction and blur the border between knowledge and believe.

3. **Peter Woit**
August 8, 2007

reader,

The reviewer does get a few things wrong, and if it were a review of my book and not Smolin's (which would be more appropriate, since mine was partly aimed at mathematicians, he whines…) I'd take the time to write about them.

4. **???**
August 8, 2007

I read the link about "Universe's Stringy Birth Revealed by Young Czech Physicist".

Do I understand correctly that hep-th/0511286 won an EURYI award and that "Most awards are between €1,000,000 and €1,250,000, comparable in size to the Nobel Prize"?

5. **prague_phys**
August 8, 2007

Martin Schnabl: Actually here in Prague we are very happy that somebody from Czechia got this prestigious prize. We are even more happy that it is not Lubos Motl.

6. **Peter Woit**
August 8, 2007

Bee,

It seems to me that Carr doesn't see his interest in religion and psychic research as hobbies distinct from his professional interest in science, but rather they are to him all related things, and he's interested in bringing them together and blurring the distinctions. This blurring of science with non-science is something that physics traditionally didn't tolerate, it's a shame that this is changing.

7. **DB**
August 8, 2007

Peter Woit wrote:

> This blurring of science with non-science is something that physics traditionally didn't tolerate, it's a shame that this is changing.

Why so many mainstream physicists remain silent about the public thrashing that physics is receiving at the hands of the snake-oil salesmen of string theory is the great mystery to me. Most physicists I know love their subject deeply, and yet seem content to bury their heads in their textbooks or fiddle about in their labs while Rome burns. Cowardice? A desire not to wash dirty linen in public?
Earlier you called the title of an article "complete bullshit". That's the kind of forthright comment Richard Feynman might have made.
I don't know whether Sean Carroll had his tongue in his cheek when he wrote that drivel. But he was probably sitting at Richard Feynman's desk in Caltech when he wrote it. Makes you think, doesn't it?

8. **Someone**
August 8, 2007

To ???:

Yes, the press release implies that the amount is comparable to that of a Nobel Prize but it's really an unfair comparison.

The Nobel Prize gets wire transferred to your bank account.

A research grant gets sent, usually a little bit at a time for several years, to your University or Institute which, after taking a 50%-ish cut, lets you use the money to hire graduate students and postdocs, but definitely not to put into your bank account.

Still it enough money to start a very healthy research group (especially with European postdoc salaries as small as they are!).

9. **???**
August 8, 2007

thank you. Still it looks surprising: hep-th/0511286 has only 40 citations.
Our friend Lubos wrote hep-th/0601001: another czech string theory paper with 40 citations: does he deserve €1,000,000?

10. **Cecil Kirksey**
August 8, 2007

Peter:
Your comment about pseudo-science fits in very nicely with a subject that I have been thinking about lately: What constitutes a valid scientific question? Forty years ago asking what happened prior to the big bang was NOT a valid physics question, but now it seems that it is. Does God exists? Not valid. A 100 hundred years ago: what mechanism gave the sun all its energy? Get the idea? Does the validity of the question vary with time? If so at what point does the question transition from being a non-valid question to a valid question? I have read basically that the question should be open to prove or disprove. Does ignorance about how to proceed to prove or disprove a statement mean the question is valid or not valid? Thanks for your and other commentors thoughts.

11. **Coin**
August 8, 2007

This blurring of science with non-science is something that physics traditionally didn't tolerate, it's a shame that this is changing.

Just out of curiosity, do you feel the same way about Roger Penrose and his recent adventures in "quantum consciousness" with Stuart Hameroff?

12. **Peter Woit**
August 8, 2007

Cecil,

A question is a scientific question if possible answers to it can be (at least in principle) put to a convincing experimental test. There are lots of subtleties one can get into, but the anthropic landscape pseudo-science doesn't involve them, it just inherently produces either no predictions or wrong ones.

I'm thoroughly sick of having to endlessly go over the kinds of scientific methodology 101 questions we normally teach elementary school children, just because some people are trying to argue about methodology in order to evade the overwhelming evidence that their scientific research program has failed.

Unless someone has something particularly new and intelligent to say about this issue, please discuss it elsewhere.

13. **Bee**
August 8, 2007

Hi DB:

Most physicists I know love their subject deeply, and yet seem content to bury their heads in their textbooks or fiddle about in their labs while Rome burns. Cowardice? A desire not to wash dirty linen in public?

I've made the same experience. It's a shoulder shrugging that upsets me very much, but one that is not restricted to physics or science but seems to be a more general phenomenon. People look at me and say 'that's just the way things are' without even considering that they might be able to influence the direction in which we are drifting. Look at politics. Different questions, same problem. People focus on their own work, and miss the big picture. They are aware of the problems, but ironically are too busy to change anything about it. Maybe even too busy to think about it.

But there's no higher power that will ensures quality of research. It's due to us. If we don't care, if we don't criticise, if we passively take what we get and adapt to it, we will go down with that ship. That's one of the reasons why I think Peter's blog is useful. One can share or not share his disliking of string theory, but he's reliably taking track of things, has an opinion and doesn't keep his mouth shut. That's also why I think Lee's book is important: to remind us that we should think about what science is, and what it should be.

I spend a lot of time trying to understand other people's works, and I'm not exactly polite if I come to the conclusion that it doesn't make sense. Various people have pointed out that I am wasting my time with that. Maybe they are right. In a career-wise sense. But I have the impression we're running into a situation where more and more people are writing more and more things that nobody really can or wants to follow, yet nobody cares. It only takes a critical amount of people who cite each other and a vague untopic can inflate exponentially.

Best,

B.

PS: Some more thoughts on the above here.

14. **Peter Woit**
August 8, 2007

Coin,

I don't know anything about Penrose/Hameroff, and the relation between quantum theory and consciousness is not something I personally have any interest in thinking about. Surely there is lots of flaky, unscientific work in this area, but I'm not about to spend my time learning about it, figuring out what is flaky and what isn't, and writing about it until it starts dominating particle theory research. There's a large number of such topics that I intend to keep ignoring, and to keep encouraging people to discuss not here but elsewhere.

15. **Kea**
August 8, 2007

This blurring of science with non-science is something that physics traditionally didn't tolerate…

Hmmm. Perhaps excluding Newton, Leibniz, Einstein, Penrose, Wigner (who has commented on the need for a consciousness theory underlying QM) ……. Your attitude is very indicitive of the narrow mindedness of scientists in the Age of Business, and clearly not based on having actually studied the research in question.

16. **Cecil Kirksey**
August 8, 2007

Peter:
Please do not get hyper! I clearly understand that what is sufficient for a valid scientific question is: can it be proved or disproved by some experiment or observation that by the consensus of the community is possible. However, I was more interested in the time issue. Does a question today that is deemed invalid become valid as scientific progress is made? If so what determines this transition? This I beg to differ is not taught in science 101 or I would not be asking the question. Just think about pre-big bang physics. Is this valid now and not valid 40 years ago?

17. **Peter Woit**
August 8, 2007

Cecil,

Our understanding of physics changes with time, so do our experimental techniques. As these change, some questions become scientific ones. For pre-big bang physics to be a scientific issue, you have to come up with potential experimental tests, even indirect ones. I haven't seen any convincing arguments for such tests. People are welcome to try to turn this into a scientific question, others are welcome to ignore them if they think they're getting nowhere.

18. **LDM**
August 8, 2007

I agree with Bee…

I am only a little unclear as to why the book "Universe or Multiverse?" should be singled out.
String theory is not testable and likely never will be, hence ANY book, article, or preprint on it must at worst be considered pseudo-science…or in the most charitable case, mathematics.

19. **Peter Woit**
August 8, 2007

LDM,

“String theory” is a varied and complicated subject. Most of it is not pseudo-science, although a lot of it is probably just wrong science.

Anthropic pseudo-science has gotten a big push from string theory, but string theorists are far from the only ones engaging in it. Personally I feel it is a much more dangerous phenomenon than string theory: having a scientific field dominated by work on a wrong idea is bad, having it dominated by work that has simply given up on science is worse.

20. **Yatima**
August 8, 2007

Does a question today that is deemed invalid become valid as scientific progress is made? If so what determines this transition?

INSUFFICIENT DATA FOR A MEANINGFUL ANSWER

(From an Asimov story called ‘the last question’ I think)

The relation between quantum theory and consciousness is not something I personally have any interest in thinking about.

I would say this is a healthy attitude as QM/C arguments never make it seriously past the first strawman. Marvin Minsky’s handwaving explanations are still good enough at the current level of knowledge. But now back to fundamentals…

21. **gunpowder&noodles**
August 8, 2007

Regarding Sean Carroll’s post: are you saying that you don’t believe in the “Sensible Anthropic Principle”? Surely not?
SC thinks about these things because he is interested in the arrow of time. This is a major, major feature of our world about which we understand essentially squat. Understanding this thing is real physics, and I find it very hard to understand why you would be opposed to this. On the other hand, you got all excited about Ed Witten’s excursion into science fiction — that nonsense about three-dimensional spacetimes. Now *that* is bullshit! The probability that we will learn anything useful about our universe by studying 3-dimensional spacetimes is laughably small — and I’ m leaving aside the obvious disconnect from anything even remotely falsifiable. SC’s investigations are hard science by comparison.

That there is a lot of crazy stuff out there is obvious. But it really doesn’t help to have the attitude that anything that looks strange must be pseudo-science. I think you are being unfair to SC.

22. **Peter Woit**
August 8, 2007

gunpowder,

Very specifically, what struck me was the end of Sean’s posting:

“The post-Big-Bang lifespan of the universe is very plausibly infinite. And yet, we find ourselves living within the first few tens of billions of years (a finite interval) after the Bang.

That last one deserves more attention, I think.”

which I still find hard to believe is not a joke.

Sure, I think the probability of Witten’s work on 3d gravity providing real insight into the 4d world is infinitely greater than the probability that thinking about this question will lead to any sort of scientific insight.

23. **AngryPhysicist**
August 8, 2007

Gunpowder wrote:

*Now *that* is bullshit! The probability that we will learn anything useful about our universe by studying 3-dimensional spacetimes is laughably small — and I’ m leaving aside the obvious disconnect from anything even remotely falsifiable.*

Perhaps, but quantum gravity simplifies drastically in 2+1 dimensions.

For example, the Riemann tensor linearly depends on the Ricci tensor (if I recall correctly; I’m writing this from the middle of nowhere on my laptop!). See Steve Carlip’s book [Quantum Gravity in 2+1 dimensions] for details.

Also, additionally, the free bosonic string is Lorentz covariant in 2+1 dimensions.

The constraint equations in canonical gravity, from my understanding (again, if I recollect correctly!), is far simpler than they are in 3+1 dimensions.

This allows us to think more qualitatively about the problems posed to us by quantum gravity, e.g. the problem of time, etc. I do not know about you, but I would much rather deal with simplified equations that allow us to think about the more interesting parts of quantum gravity.

Compare this to merely doing the mechanics and monkeying around with nonlinear equations.

I’d rattle on more, but as I have said, I’m writing this in the middle of nowhere.

24. **Dr. E**
August 9, 2007

Since the publication of TTWP and NEW, it seems pseudoscience has augmented and advanced with a new boldness. Is this right? I did not expect this. What might be possible explanations? Has all just been in the pipeline? Or have they gained newfound courage, as they have found that NEW and the TTWP didn’t affect the funding nor press?

Just wondering what everyone thought. And where is it all headed?

25. **csrster**
August 9, 2007

"The post-Big-Bang lifespan of the universe is very plausibly infinite. And yet, we find ourselves living within the first few tens of billions of years (a finite interval) after the Bang.

That last one deserves more attention, I think."

Is that so ridiculous? We are apparently living in a dark-energy dominated universe at a time when the ratio dark-energy/non-dark-energy is of order one. Doesn't that require an explanation? And isn't there even a fairly obvious one that a much older universe would be too tenuous to support life?

26. **gunpowder&noodles**
August 9, 2007

""The post-Big-Bang lifespan of the universe is very plausibly infinite. And yet, we find ourselves living within the first few tens of billions of years (a finite interval) after the Bang.

That last one deserves more attention, I think."

which I still find hard to believe is not a joke. "

I guess the point is that: we find ourselves on a rock because life is not possible elsewhere. Carroll is perhaps speculating that the fact that we find ourselves so early in the history of the universe is evidence that the late universe is hostile to life.

I would agree with you that this is not really an interesting question. What happens to the arrow of time in the remote future, by contrast, is a very interesting question because thinking about it may throw some light on the whole gigantic mystery of where the arrow comes from anyway.

"Sure, I think the probability of Witten's work on 3d gravity providing real insight into the 4d world is infinitely greater than the probability that thinking about this question will lead to any sort of scientific insight."

Well, we'll see. I'm *very* confident that this work will sink without a trace. Well, it might eventually get cited by other lost souls working on 3D "physics", but that will just prove my point. 3D is just so utterly different from 4D that parallel universes seem tame by comparison. Try telling a differential geometer that you are studying 3D manifolds because you are confident that this will tell you something about Seiberg-Witten theory….

Still, you may be right, in which case it will be shown that thinking about questions that seem to be wildly irrelevant to real physics is a good thing to do. SC is doing that very thing, is he not? Why is it ok for Witten to think about other universes — and 3D universes are about as "other" as they come — and not for other people?

27. **Mark Hillery**
August 9, 2007

In regard to the "Centers for Theoretical Physics," it would be nice to see a little truth in advertising. The ones at Berkeley and Michigan (at least from a quick perusal of its web site), and the defunct Caltech/USC one concern (or concerned) themselves only with theoretical high-energy physics and cosmology. These subjects are only a small subset of theoretical physics. The Princeton center seems to have members who are interested in condensed-matter theory and biophysics, and is, therefore, more broadly based. If you are going to have a center that studies only high-energy theory and cosmology, call it a center for high-energy theory and cosmology, not a center for theoretical physics.

As a Berkeley graduate, I was particularly disappointed when they set up their center to see that it would be so narrowly focussed. Why no condensed-matter theory? It also could have been a good opportunity to branch out into new fields. For example, there is excellent work going on in the computer science and chemistry departments there in theoretical quantum information, but not much in physics. It might be useful for the physicists to find out what is going on in this field at their own university.

All of us tend to be somewhat parochial in our views, but let's not enshrine our parochialism in the names of institutions.

28. **Ted**
August 9, 2007

Peter:

Your description of David Vogan's expository piece on E8 as "wonderful" is an understatement. For us amateurs who are trying to understand this magnificent edifice of mathematical physics at the beginnnig of the 21st century, these toss-off links that you provide are pure golden nuggets.

I thank you.

29. **Peter Woit**
August 9, 2007

csrster,

I don't see how you read the "coincidence" problem of the dark matter/dark energy ratio being of order one into what Sean wrote. But in any case, if for some reason that was what he actually meant to say, characterizing this as "deserves more attention" would be rather bizarre since it's one of the most well-known problems in cosmology.

30. **Bee**
August 9, 2007

Hi Dr. E,

What does the E stand for? Enlightenment?

Since the publication of TTWP and NEW, it seems pseudoscience has augmented and advanced with a new boldness. Is this right? I did not expect this. What might be possible explanations? Has all just been in the pipeline? Or have they gained newfound courage, as they have found that NEW and the TTWP didn't affect the funding nor press?

Just wondering what everyone thought. And where is it all headed?

I think your perception is incorrect. What I notice – and what I welcome – is that there is more discussion about the question what is science, pseudoscience, and where we are headed. I find that a healthy development. I hope there will be a practical outcome of that which allows researchers to refocus their efforts on real science, and not to waste time on politics, networking, or advertisement. One has to ask why pseudo-scientific ideas gain popularity. Because they are cheap to produce and they sell well. It's the Walmart of science. You get everything, it looks okay, but if you try to use it will fall into pieces.

Best,

B.

31. **Peter Woit**
August 9, 2007

B. and E.,

I think B. is right that the publication of the two books has definitely intensified the discussion of and raised awareness about the issue of the problematic state of string theory. As far as string theory propaganda to the general public goes, there's definitely less of it, and science journalists in particular are much less likely to report unskeptically string theory hype. String theorists are well aware that they don't have much of a counter-argument against the arguments of the two books, so they have chosen to mostly keep quiet and hope the controversy dies down. Within physics departments in general I think there also is more skepticism about string theory (often unfortunately taking the form of generalized skepticism about any kind of formal or mathematical theory) and a preference for work that is "phenomenological" and uses little mathematics.

Among the powers that be in the particle theory community, my impression is that the overwhelming sentiment is that doing anything about the problems laid bare by what has happened over the last twenty years is just too painful a prospect, that it would damage too many people's interests and entrenched ways of thinking and doing business. Instead, people are putting all their hopes in the idea that the LHC will soon solve the problem, so they don't need to address it now. The standard tactic in theory survey talks is to put up lots of pictures of the LHC, talk about the "coming revolution" in particle physics, and ignore the ongoing disaster of the current state of the subject.

32. **Changcho**
August 9, 2007

"One has to ask why pseudo-scientific ideas gain popularity. Because they are cheap to produce and they sell well. It's the Walmart of science. You get everything, it looks okay, but if you try to use it will fall into pieces."

Bee, that is priceless!!

33. **Jeff Moreland**
August 9, 2007

Peter,
Your last note seems very bleak, but perhaps the situation is not really that bad. For reasons that both you and Lee Smolin mention in your books, change is not going to be too rapid, but perhaps things really are beginning to improve.

34. **Tony Smith**
August 9, 2007

Peter mentioned "… The BCTP is just one of a bunch of other CTPs that have been founded in recent years, including the MCTP and the PCTP (and one dead one, the CIT-USC CTP). …" and referred also to "… the Stony Brook YITP …" and "… IAS PITP …".

With all those ITPs around, and with the dominant paradigms of superstring theory and loop quantum gravity failing to produce any model that predicts by calculation the things that might be observed at LHC (and have already been observed by experiments elsewhere),

one might think that some (even one) of them might set up some sort of forum in which non-standard ideas could be presented, reasonably condsidered, and evaluated in some detail (all in public for all the world to see).

As a blacklisted outsider with a predictive model, my wish that such a forum had existed years ago would be selfish, but now age and health concerns probably would prevent me from using it,
so now my comment is directed mostly on behalf of the many other outsiders whose models may be at least in part correct, and may therefore be useful in understanding those things as to which
superstring theory and loop quantum gravity have failed to provide understanding.

Peter has stated WHY such a forum does not (and probably will not) exist, saying:
"… Among the powers that be in the particle theory community, my impression is that the overwhelming sentiment is that doing anything about the problems laid bare by what has happened over the last twenty years …
would damage too many people's interests and entrenched ways of thinking and doing business.
Instead,
people are putting all their hopes in the idea that the LHC will soon solve the problem,
so they don't need to address it now. …".

Hoping "that the LHC will soon solve the problem" seems preposterous,
since neither superstring theory nor loop quantum gravity have come to grips with the sort of detailed observational data that already exists, much less what the LHC might produce.
Such false hopes lead to absurdities such as "Nima's Marmoset" being the sort of thing favored by ITPs (Nima now being at IAS).

Tony Smith

35. **Peter Woit**
August 9, 2007

Jeff,

I'm more optimistic in the longer term. By 3-4 years from now, either the LHC will have given us some new insight into electroweak symmetry breaking, or it will begin to be clear that there is no "coming revolution", and problems have to be faced up to. In the meantime though, the hep-th part of the arXiv will remain a rather grim and depressing scene…

36. **Tony Smith**
August 9, 2007

Peter, you say that, if LHC finds no "new insight into electoweak symmetry breaking, then "… 3-4 years from now … the LHC … will begin to …[make it]… clear that there is no "coming revolution",
and problems have to be faced up to …".

Is that true ?

How would "… the powers that be in the particle theory community …[be able to]… do … anything about the problems ..[without]…
damag[ing] too many people's interests and entrenched ways of thinking and doing business …" ?

Wouldn't it be easier for "the powers that be" to protect "many people's interests and entrenched ways" by just keeping on forever doing what they have been doing for the past two decades ?

If they can keep on now even though you and Lee have clearly shown "the problems", why won't they just keep on keeping on forever,
since they control the grants, jobs, funding, ITPs, etc ?

Even if you and/or Lee showed an example of a model that worked,
why would "the powers that be" pay any attention to it.
Wouldn't they just attack you and/or Lee and your model as Oppenheimer did Bohm and his model,
saying
"… If we cannot disprove Bohm, then we must agree to ignore him .." ?

Since they control the jobs, grants, funding, ITPs, etc, wouldn't
the maxims
"he who has the gold rules" and "them who has gets"
indicate that you and/or Lee and your model wouldn't stand a chance ?

Tony Smith

37. **anon.**
August 10, 2007

'If they can keep on now even though you and Lee have clearly shown "the problems", why won't they just keep on keeping on forever, since they control the grants, jobs, funding, ITPs, etc ?' – Tony Smith.

The real issue is not more of the same, but further degeneration. ArXiv's general physics section in 2003 tolerated hosting Prof. Brian Josephson's anthropic-string theory paper: http://arxiv.org/abs/physics/0312012

It's more scary when such anthropic papers appear in the *high energy physics – theory* section of arXiv: http://www.arxiv.org/abs/0708.0573 The string landscape has given the anthropic principle credibility with leading physicists.

38. **Peter Shor**
August 10, 2007

Everybody seems to be making fun of the giraffe paper without reading it. Skimming the paper, it estimates the height of the tallest animal that can fall over and get up without serious injury, and comes up with something between the height of a man and a giraffe. However, googling, I see on several websites the interesting fact, or at least urban legend, that if an adult giraffe falls over on its side in the wild, it is unable to get up and it dies. So, if we believe these websites, this paper may actually be pretty accurate in its estimate (at least for mammals … I presume nobody knows whether T. Rexes that fell over were able to get back up).

Putting that aside, you can ask the questions: Why should this paper be on hep-th? And does it tell us anything interesting about the anthropic principle or high energy physics. I don't think there's a good answer to the first question, and I think the answer to the second question is definitely "no".

39. **Peter Woit**
August 10, 2007

Peter S.,

I don't doubt that the paper may contain a reasonable argument about the size of giraffes. But it seems to be either a joke or a sign of the times that both the author of the paper and the hep-th moderator think this is appropriate material for hep-th. The moderation of hep-th is supposed to be minimal for "endorsers", but even so, there have been cases in the past where the hep-th moderator rejected papers by such physicists (including one by Susskind) on the grounds of obviously being inappropriate.

In other news, the two plenary speakers in the cosmology session at this week's philosophy of science conference in Beijing are: Sean Carroll and Don Page.

40. **a.k.**
August 10, 2007

@gunpowder: at least concerning Seiberg-Witten invariants, three dimensions *can* be of interest for differential geometers, this even could have a physical flavour, there are some not-so-old papers by Nicolaescu/Nemethi (accessible on Nemethi's site) concerning relations between Seiberg-Witten invariants of (3-dimensional) links of isolated complex surface singularities whose link is a rational homology sphere, certain topological quantities of the link and the geometric genus of the singularity itself, for certain special types of singularities (Gorenstein) this reduces to an equality of the Seiberg-Witten invariant on the link and a certain fraction of the signature of the Milnor fibre, which is a surprising relation, at least for me.

This might, by the way, fit into a picture described by Torsten Asselmeyer some time ago in a paper deriving the cosmological constant by interpreting spacetime as bounding a certain Brieskorn sphere, a natural choice for spacetime would then be the Milnor fibre of a certain Brieskorn polynomial and one could possibly ask how physical restrictions on its signature trabnslate into restrictions on the values of 'allowed' Seiberg Witten invariants on its link, using the above relations (of course one had to check the 'Gorenstein' condition)..

41. **D R Lunsford**
August 11, 2007

1.2×10^11 coefficients to calculate! We need an “E8@home” project 🙂

-drl

42. **D R Lunsford**
August 11, 2007

Peter, this paper by Vogan is just great – thanks for pointing that out!

-drl

43. **D R Lunsford**
August 11, 2007

Peter,

Anthropocentrism is easily understood as the natural expression of the narcissistic era we inhabit. Many of the people who endorse such ideas leave one with a sickly feeling of being in the presence of an unctuous and self-absorbed exquisite, who is not man enough to confront real problems because they provide no source of narcissistic supply. The way to deal with narcissists is to confront them boldy, and then they tend to just shut up – as you and Smolin have demonstrated to some extent, with your books.

-drl

44. **a.k.**
August 11, 2007

DRL, your description easily applies to to the main conception of any ‘abstract’ science, would it be philosophy, mathematics or physics, the driving theme in all these concepts has a narcissistic corollary, the point is possibly that one should be aware of this.

45. **Who**
August 11, 2007

drl,

most science bloggers I can think of are doing a real service and putting in real intellectual effort now and then, besides entertainment value. I can only think of a couple who seem above all to want adulation of their personality or who cultivate a charmed flock of not-so-bright admirers—to a first approximation maybe these people are harmless. You may well agree, if not please say.

Just thinking about what you wrote and trying to apply it (as a test case) to the very few narcie web-celebs that I can think of—the few media-personality types that sometimes seem a bit heavy on the narcissist side.

you draw a connection between self-absorption and resorting to the Anthropic (lack of) P.

you indicate that you think one can “deal with” or “confront” such folks and you say the recent books (NEW and TTWP) demonstrate this. It might be optimistic to think that when confronted they will “tend to shut up”
==================
you may have some good insights here, but I find myself putting on the brakes. Even if there is a cultural connection between these things, just on a practical level I don’t see any need to confront media-narcissism per se. It’s just irritating.

do you see a broader problem?

I agree with you that resort to the anthropic excuse is a corruption of empirical tradition—I just split it off from people who crave the limelight and showcase their personality: they could be harmless—the real enemy being confusion of solid science with pseudo.

46. **D R Lunsford**
August 11, 2007

Who,

Yes the science bloggers for the most part go a great job (with notable “variances”). Without them there would be little hope.

The site mentioned has a detailed description of the cerebral narcissist. Read it through and go down the list of theoretical excrescences and their authors. You will be convinced. Of course, this is a broad cultural phenomenon in the West that touches all areas of life – so why should the academy be spared?

One thing to remember is that the narcissist, while rather pathetic when exposed, is anything but harmless.

-drl

47. **Peter Woit**
August 11, 2007

Please, enough personal attacks on people, either retail or wholesale.

In this case, it seems to me that a bunch of people invested a lot of time and effort in an idea that turned out to be wrong. They don’t want to admit they were wrong. This isn’t exactly unusual human behavior, especially among academics.

48. **D R Lunsford**
August 11, 2007

Sorry Peter, I did not intend to attack anyone in particular, but I would point out this work:

http://en.wikipedia.org/wiki/The_Culture_of_Narcissism

It applies with great accuracy to academic life, at as I have experienced it.

I do no think this is a matter of simply refusing to admit being wrong. In the past, when a breakthrough was made, scientists were only too happy to admit they were wrong, because there did in fact seem to exist a fundamental desire to know what was right, regardless of who cooked up the solution. The best example is Pauli, kicking himself for missing the Dirac equation, of which he was initially critical. "With his [Dirac's] fine sense of physical realities, he finished his argument before it was started" was his recantation. I think this desire to know what is right, has gone missing. Alistair Cooke predicted it exactly in the early 70s – enthusiasm would become a substitute for talent (he was talking about the arts in particular), with bad results.

Reading Lasch is a very rewarding experience. I highly recommend it.

-drl

49. **Intellectually Curious**
August 11, 2007

"…there did in fact seem to exist a fundamental desire to know what was right, regardless of who cooked up the solution…I think this desire to know what is right, has gone missing."

A wise professor once told me that there are the real scientists and then there are the politicians. (And he was talking about those with Ph.D.'s in the sciences.) I think his statement holds true for these people regardless of their career choice, whether they're actually engaging in scientific research or not. A real scientist at heart will have great respect for truth over ego, period. Though he/she, with human weaknesses and all, may not be perfect all the time, his/her true desire nevertheless will be clear over time.

50. **The Vlad**
August 12, 2007

"In this case, it seems to me that a bunch of people invested a lot of time and effort in an idea that turned out to be wrong. They don't want to admit they were wrong. This isn't exactly unusual human behavior, especially among academics."

Dear Peter,

Don't you mean "an idea that turned out to be not even wrong"? 🙂

Best,

The Vlad

51. **D R Lunsford**
August 13, 2007

I stumbled across a fine paper by GFW Ellis examining the philosophical issues that underlie cosmology – it has pertinent statements about anthropocentrism – when anthropic arguments are sensible and when they are not. In particular, this can be applied to Weinberg's original paper, allowing it to be seen in proper context as exhibiting a selection principle.

Here:

http://arxiv.org/abs/astro-ph/0602280

-drl

52. **D R Lunsford**
August 13, 2007

And while we're reading Ellis:

http://arxiv.org/abs/gr-qc/0102017

..in which a new ultimately strong version of the AP is revealed to the reader, namely:

"..life not only must exist [in any Universe], but once it has come into existence must continue to exist until the end of the universe.." – the *Completely Ridiculous Anthropic Principle* or CRAP.

-drl

53. **anon.**
August 13, 2007

See also Prof. Smolin's http://www.arxiv.org/abs/hep-th/0407213

"It is explained in detail why the Anthropic Principle (AP) cannot yield any falsifiable predictions, and therefore cannot be a part of science. Cases which have been claimed as successful predictions from the AP are shown to be not that. Either they are uncontroversial applications of selection principles in one universe (as in Dicke's argument), or the predictions made do not actually logically depend on any assumption about life or intelligence, but instead depend only on arguments from observed facts (as in the case of arguments by Hoyle and Weinberg). …"

54. **island**
August 13, 2007

Lee is just wrong about that. For example; the goldilocks enigma quite specifically predicts that life won't be found on Mars or Venus due to the runaway effects that these planets are subject to that anthropically balanced planets do not succumb to, and this prediction about life is being tested as we speak.

It ain't all about the multiverse, regardless of what "popular opinion" has to say about it…

The Goldilocks Enigma and Mediocrity

55. **Peter Woit**
August 13, 2007

Please, unless you've got something new and interesting to say about the anthropic principle, don't post comments about it here. One of the surest signs of pseudo-science is that it leads to endless and tedious arguments that can never be resolved, and that drive away anyone sensible. Do this over at Cosmic Variance, not here.

56. **Mainland**
August 13, 2007

> … never be resolved, and that drive away anyone sensible. Do this over at Cosmic Variance, not here.

Peter, it is hard to do that over at Cosmic Variance because there is a scarcity of sensible people to be driven away.

heh heh, sorry if offtopic, just a little joke.

57. **Alon Levy**
August 13, 2007

Peter Woit\'s War On Science Will Fail!!!!!!

58. **Chris Oakley**
August 14, 2007

Alon Levy's war on HTML will fail. Using backslash to quote characters only works in TEX.

59. **Matt Robare**
August 18, 2007

I think it's obvious why publishing pseudo-science is okay: it sells. People are interested in psychic powers or whatever. Meanwhile you violate "scientific consensus," which makes you an arch-heretic. Look at what happens to anyone who questions global warming.

60. **String Fan**
August 19, 2007

> This explanation [that M-theory is not a complete theory] was, to me personally, a great shock since I had always believed M-theory was a complete theory.

The only shock is that the reviewer is shocked: the very name of M-theory was chosen to emphasize its incomplete status. If one knew just a single fact about the subject, it would be this. Evidently the reviewer possesses no background knowledge whatsoever on the subject of the book he is reviewing. You can hardly blame his ignorance on `string hype', but of course in Peter Woit's universe string theorists are always wrong, and everything is their fault.

61. **Ralf**
August 19, 2007

Did anybody notice this footnote in arXiv:0708.1917v1 [hep-th]: THE EARLY YEARS OF STRING THEORY: A PERSONAL PERSPECTIVE by John Schwarz?

1 Since the history of science community has shown little interest in string theory, it is important to get this material on the record. There have been popular books about string theory and related topics, which serve a useful purpose, but there remains a need for a more scholarly study of the origins and history of string theory.

More of the Usual Sorts of Things

August 14, 2007
Categories: Uncategorized

On the pseudo-science front, the Resonaances blog describes a talk at CERN by string theory enthusiast Jim Cline, about a variant of the anthropic principle called the “Entropic Principle” as “pushing the idea to the edge of absurd.” For beyond the edge of absurd, there’s today’s NYT Science Times section, which features a piece by John Tierney about the ideas of philosopher of science Nick Bostrom. Bostrom runs a web-site called anthropic-principle.com and has made a career for himself in the anthropic principle business which now has him running a Templeton Foundation-funded Institute at Oxford called the Future of Humanity Institute. The New York Times article is about Bostrom’s idea that there’s a significant probability that our universe is just a simulation being conducted by a more advanced civilization, an idea that he considers to be one of the “interesting applications” of the anthropic principle. He has yet another web-site, simulation-argument.com, where he propounds this argument. Tierney supplements the NYT article with an on-line discussion of how we should we all behave, given that we are just simulated creatures. Maybe we should be trying to entertain our creators so they will not turn off the simulation? Anyone who thinks it is a good idea to discuss these questions seriously is encouraged to do so at Tierney’s site, not here.

Today’s Science Times also has an interview with Gino Segre, who has a new book called Faust in Copenhagen: A Struggle for the Soul of Physics, about the 1932 conference in Copenhagen hosted by Neils Bohr at the time of the beginnings of modern nuclear physics. Segre says that he became a physicist for an unusual reason. His father was an historian, brother of Emilio Segre, the co-discoverer of the antiproton, and the two siblings were estranged. When he was 15, Segre’s father told him “I think you should become a theoretical physicist, and I want you to surpass your uncle”, and he did as he was told.

American Scientist has a review of the Segre book, together with David Lindley’s recent *Uncertainty: Einstein, Heisenberg, Bohr, and the Struggle for the Soul of Science*.

American Scientist also has an interview with Frank Wilczek about books he is reading and that have influenced him. He strongly recommends a book by an author I’d never heard of, Olaf Stapledon’s *Star Maker*.

Unfortunately two well-known scientists, mathematician Atle Selberg, and physicist Julius Wess, are no longer with us.

Victor Kac is giving a series of lectures on vertex algebras in Brazil, with video to be available here.

A Turkish mathematician, Ali Nesin, ran into trouble with the authorities for running a mathematics summer school without permission. Alexandre Borovik has set up a web-site with a petition about this. Latest news is that the summer school has been re-opened, although Nesin still may face charges of “education without permission”.

Comments

1. **John Baez**
   August 14, 2007

   Star Maker is dated, but it’s a classic attempt at SF on a truly cosmic scale. Start with Stapledon’s Last and First Men, though. It’s a history of the human race over the next 2 billion years.

2. **Martin Kochanski**
   August 14, 2007

   It’s a pity that even previously sane people like Frank W. Tipler have gone round the bend over simulation. The whole thing is equally grotesque from a theological and a physical point of view.

   Here’s a paper with a general *reductio ad absurdum* of all possible simulation arguments: On Not Simulating a Universe.

3. **Tony Smith**
   August 14, 2007

   Freeman Dyson, in his book “Disturbing the Universe” (Harper 1979), said, about the Dyson Sphere idea that is commonly attributed to him:

   “… I [Dyson] took the idea from … Olaf Stapledon’s “Star Maker” .. which I [Dyson] picked up in Paddington Station in 1945 …[the]… passage …[said in part]… “… every solar system … was … surrounded by a gauze of light traps, which focused the escaping solar energy for intelligent use …” …”.

   Tony Smith

4. **John Baez**
   August 15, 2007

   I must be an unimaginative idiot. Reading good science fiction gets me fired up and makes me want to do something cool… like math and physics. It’s had that effect on me ever since I was a kid.

   I urge mathematicians to get ahold of Greg Egan’s new story *Glory*, about two xenomathematicians unearthing artifacts from a species called the Niah, who had a very long tradition of mathematics, but then mysteriously died out.

   Here’s a quote:

   “The Big Crunch” had always been a slightly mocking, irreverent term, but now she was struck anew by how little justice it did to the real trend that had fascinated the Niah. It was not a matter of everything in mathematics collapsing in on itself, with one branch turning out to have been merely a recapitulation of another under a different guise. Rather, the principle was that every sufficiently beautiful mathematical system was rich enough to mirror in part *– and sometimes in a complex and distorted fashion – every other sufficiently beautiful system.*

5. **still waiting...**
   August 15, 2007

   Hi Peter,

   Some time back you wrote [or rather I think I initially managed to encourage you, and you gleefully agreed :-)] that you would write something up (for undergraduates) on Rep Thry and particle physics. Do you still intend to do so? If yes, when may we expect the initial draft? [I’ve been sorta’ waiting for some months now.]

There’s definitely tremendous to be gained from studying RT; although I am by no means qualified [or even knowledgable] as practicing physicists would have it, I nonetheless sensed from reading Penrose’s for-laypersons-with-theory-physics-interests ‘Road to Reality’ that RT is a really important (i.e. big deal!) area. I’m more interested in this area, as well since Weyl / Atiyah (persons who are heavy-weights without question) took such deep interests in these topics…

Just wondering what drove them in this direction the way they were driven.

Regards

6. **Fabien Besnard**
   August 15, 2007

   I’ve written my own refutation of Bostrom’s argument some time ago, on a logical ground. It’s there.
   If you don’t mind english mistakes you can try this version.
   I should really rewrite some parts but I don’t have the time for that.

7. **Fabien Besnard**
   August 15, 2007

   I mean it’s there in french or there in english. Sorry about this.

8. **Yatima**
   August 15, 2007

   “Un raisonnement qui s’autodétruit” … end of story.

   Greg Egan is still writing? I thought he went hermitian in disgust at the continuing move to the Dark Side of John Howard and the World in general.
   Anyway, Egan on Simulation is a Good Read

9. **Peter Shor**
   August 15, 2007

   If you believe in the simulation argument, you should give up working on quantum gravity. It gives a very easy way to reconcile quantum mechanics and general relativity: *There’s a bug in the code*. When the first black hole evaporates, the universe crashes.

   And apologies to Peter Woit if this starts up an off-topic subthread, but I felt I just had to mention the idea.

10. **Peter Woit**
    August 15, 2007

    I’ve deleted several comments of the “SF sucks!” “No it doesn’t!” variety, and don’t want to really encourage general discussion of SF here, something I have very limited interest in.

    still waiting…

    Sorry to report that it doesn’t look like I’ll be writing up anything at an undergrad level about math and QM any time soon. This year I’ll be teaching our representation theory graduate course, and concentrating on working on that and on research. Maybe sometime in the future I’ll get involved in trying to teach this topic to undergrads, then would write something up.

11. **Garbage**
    August 15, 2007

    “Anyone who thinks it is a good idea to discuss these questions seriously is encouraged to do so at Tierney’s site, not here.”

    I wonder Peter why do you mention it in the first place? 😉

    Sad to hear Julius Wess is gone, he may have taken SUSY with him, we will soon find out…

12. **John Baez**
    August 16, 2007

    Peter wrote:

    > I’ve deleted several comments of the “SF sucks!” “No it doesn’t!” variety, and don’t want to really encourage general discussion of SF here, something I have very limited interest in.

    Okay. Unfortunately, you deleted a comment saying “SF is for unimaginative idiots”, but kept my reply saying “I must be an unimaginative idiot”. 🙂

    On a completely irrelevant note: I urge you to think of blog entry titles that are slightly more fun and informative than “Various Stuff”, “More of the Usual Sorts of Things”, etc. You can just pick one of the things you’re talking about and highlight that.

13. **Peter Woit**
    August 16, 2007

    John,

    OK, OK, I should have just deleted your comment too.

    I’ve nothing against SF, read a lot of it when I was young, still do every so often (I’m actually now reading an SF novel someone gave me). But probably the main reason I find myself getting annoyed with discussions of it here is that generally it’s pretty irrelevant to the science I’m concerned about. Not only that, but a huge amount of damage is being done to that science by an increasingly large number of people who seem unable to tell the difference between science and science fiction. So, while in happier times I might enjoy discussions of SF for their inspirational value, at the moment I’d rather keep science and science fiction separate.

14. **anonymous**
    August 16, 2007

I don't see why Bostrom's ideas about simulation are absurd. Could you explain your dismissal of them?

15. **Peter Woit**
August 16, 2007

anonymous,

What's absurd is the idea that this sort of thing is science. It's very much in the same category as religion, and different people can have different opinions about how worthwhile theology is, but it is inherently not science and it's absurd and somewhat disturbing to find it in the Science Times section of the NYT.

16. **Thomas Love**
August 16, 2007

Who needs fiction when we have string theory?

17. **anonymous**
August 16, 2007

It's not in the category of religion by any dictionary definition of religion that I'm aware of. It's a speculative hypothesis, and maybe not the best thing for the NYT to be taking up their science column space with.

18. **mclaren**
August 16, 2007

> Tierney supplements the NYT article with an on-line discussion of how we should we all behave, given that we are just simulated creatures. Maybe we should be trying to entertain our creators so they will not turn off the simulation?

Tierney's behavior seems highly entertaining, at least to me. If he persists with this kind of peuodoscience gibberish I'm sure the hypothetical observers from a higher civilization will fall all over thesmelves laughing at his antics.

Not directly on topic, but speaking of the rise of pseudoscience, the Daily Mail has an article lamenting the rising tide of superstition masquering as science:
http://www.guardian.co.uk/science/2007/aug/15/endarkenment

And just to show that this isn't just an aesthetic or educational issue, Goldman Sachs just lost a billion and a half bucks by buying into mindless mathematical pseudoscience:
http://www.dailyreckoning.com.au/wall-street-math/2007/08/16/

There's an odd link to high energy physics in the Wall Street story, since it turns out that HEP physicists who can't find work within the field wind up working on Wall Street churning out incomprehensibly complex financial instruments and wacky numerological schemes for allegedly "predicting" the market. Instead of "brighter than a thousand suns," in this case it's "dimmer than a thousand Uri Gellers."

19. **manyoso**
August 17, 2007

"It's not in the category of religion by any dictionary definition of religion that I'm aware of."

Sure it is. At least it is metaphysics at best. The same thing as saying that we're all living in someone else dream. Anyways, it is most definitely not science.

This is very much like saying the earth might really be only 3000 years old and $DEVIL just made it seem like its much older to fool everyone.

IOW, it is all hocus pocus claptrap what ifs and doesn't belong in any science discussion.

20. **Peter Woit**
August 17, 2007

Please, if you want to comment on what's science here, that's fine, it you want to just talk about religion, please don't do it here.

21. **Steve Myers**
August 17, 2007

On Wilczek interview: Russell's "History of Western Philosophy" is first class (with a good explanation of his theory of descriptions for non-logicians) and Weyl's "Philosophy of Math …" impressed me 40 years ago. Weyl was a great mathematician — but even Pauli admitted he could do physics.

22. **Mainland**
August 17, 2007

> It's not in the category of religion by any dictionary definition of religion that I'm aware of. It's a speculative hypothesis, and maybe not the best thing for the NYT to be taking up their science column space with

anonymous,

I appreciate your concern about keeping the categories straight.

Re Bostrom idea that we and our universe are a simulation run by smart aliens for reasons known only to themselves, could it be called a
proposed myth or an explanatory fantasy intended for belief?

Peter points out that Bostrom's proposition is not science and he puts it "very much in the same category as religion". You reply that you aren't content with it in that category but would rather call it a "speculative hypothesis".

But an hypothesis is often thought of as testable. Something that could become science if a way to test it could be found. There is a fine line between that and mythology —more a difference in nuance.

Myths, I suppose, come to be believed largely because they appeal to the imagination—-that is for literary or poetic reasons. As stories they delight us and bring about a willing suspension of common sense.

Another category that might work is garbage. One could consider Bostrom's ideas as either mythology or garbage, whichever one prefers.

In the end the greatest harm done by pseudoscience is the corruption of language

23. **anonymous**
August 17, 2007

Presumably it will be possible within the century either to create such simulations or prove that they are impossible. If we can prove that they are impossible, with a yet-to-be-discovered proof in a yet-to-be-initiated field of computer science, then Bostrom's idea will be falsified. If it turns out that it is quite easy for us to create many such simulations, then his idea will gain some credibility, but won't be proven. However, if that happens then you can bet that many people will turn their attention to figuring out ways of checking whether this is a simulation or not. Just because it's very speculative does not mean that it's not a valid hypothesis about the universe. It most certainly is a scientific hypothesis, even if discussion of it today should rightly be considered philosophy. Lucretius, Epicurus, Democritus et al. were philosophizing when they wrote about atomism, and had no means at their disposal for testing their speculative hypothesis. This doesn't mean that atomism was a "proposed myth", to use Mainland's phrase (see above). It was a hypothesis about the nature of the universe that was borne out much later. Bostrom's idea is analogoug. Let's not be so quick to dismiss it as "hocus pocus claptrap", as manyoso did above. One could easily have said the same about Lucretius et al., and no doubt the manyosos of antiquity did so with a good conscience.

24. **Cheeky Bastard**
August 17, 2007

Dear anonymous, even if you were to prove that the kind of simulation envisioned by Bostrom is impossible in our universe, you could not logically conclude that our universe is not a simulation. For all you know, the laws of nature of the universe in which our simulation is running could be completely different from the laws ruling the simulation. Maybe somebody with a really twisted sense of humor invented quantum mechanics just to see how intelligent beings would rationalize it. 😉

25. **Peter Woit**
August 17, 2007

"Just because it's very speculative does not mean that it's not a valid hypothesis about the universe. It most certainly is a scientific hypothesis…"

This is exactly the kind of nonsense that is taking over some subfields of theoretical physics and threatening to destroy them as successful ways of gaining reliable knowledge about the universe. It's depressing and seems to be a waste of time to keep explaining over and over again the basic principles of what science is and how it works. No matter what, people who want to do pseudo-science because it's a lot easier than science will keep on justifying absurd, and inherently untestable speculation, claiming that "how do you know that a miracle won't happen if we work on this? If we do, maybe we'll find a real test!"

People who do this behave exactly the same way as every crackpot I've ever made the mistake of arguing with, trying to draw you into an endless investigation of the dense thickets of their idiocy. Arguing with someone who thinks the "simulation argument" is a scientific hypothesis is just this kind of waste of time. Over at Cosmic Variance, Sean Carroll, who has a much higher level of tolerance for this kind of thing than I do, refers to the simulation argument as "meaningless". If people do want to argue about it, you might have better luck with him.

26. **Peter Woit**
August 17, 2007

I don't want this blog to be dominated by pseudo-scientific discussion, especially when I don't know that the people doing this are serious scientists. This is the kind of thing that destroys what I am trying to do here, trying to provide a place for serious discussions between people who know what they are talking about. I'll be deleting most such pseudo-science comments, especially ones that are anonymous and such that I don't have any reason to believe their author knows what he/she is talking about.

27. **Luiz**
August 17, 2007

"Over at Cosmic Variance, Sean Carroll, who has a much higher level of tolerance for this kind of thing than I do, refers to the simulation argument as "meaningless". If people do want to argue about it, you might have better luck with him."

Peter,
Directing the crackpots to Sean Carroll is just mean 🙂

Just thought you could use a joke…
Cheers.

28. **anonymous**
August 17, 2007

"This is exactly the kind of nonsense that is taking over some subfields of theoretical physics and threatening to destroy them as successful ways of gaining reliable knowledge about the universe."

Peter, this is exactly the kind of rudeness that has ruined the blogosphere. My comments are not nonsensical.

"It's depressing and seems to be a waste of time to keep explaining over and over again the basic principles of what science is and how it works."

I have a solid understanding of the basic principles of science and how they work. I don't need you to explain them to me, although I'd be curious to hear how your interpretation of them would lead you to lump Bostrom's ideas in with religion. I'm a trained scientist, and I've studied the philosophy of science as well. I know you have a bone to pick with the way that speculative ideas in physics have caused the field to go astray, and I agree with you that these sorts of ideas should never be given too much importance. I don't think Bostrom's idea is important either. But they are interesting and stimulating to think about from time to time, even if we don't take them very seriously because we cannot decide them one way or the other. This doesn't mean that they don't qualify as hypotheses. I suppose Newton wasn't doing science when he hypothesized that light was comprised of corpuscles? Never mind that this hypothesis was eventually testable. Would you maintain that such a hypothesis is crackpottery just because testing it is beyond the means of science at the time that the hypothesis is put forward? How about in Mathematics? Would you lump mathematical conjectures in with religion as well? It is one thing to fight against speculation becoming dominant, and I'm glad that you are fighting that fight, but it's quite another to dismiss any speculation by saying it's religion. I know you've had a hard slog over the years, sparring with people like Lubos etc., and I know it's probably exhausting, but don't let it turn you into a bitter reactionary.

29. **Cheeky Bastard**
August 17, 2007

Dear anonymous, as I tried to point out above, the simulation "hypothesis" is not testable even in principle, not just as a matter of technological limitations. Like the existence of God, or the proposition that the universe was created just a second ago along with all your memories, fossils in the ground and paleontologists pointing to them as proof against creationism, it's just unknowable, untestable and therefore outside the realm of science. Believing in something unknowable is an act of faith, and so could reasonably be labeled as "religion", in some wide sense of the word.

…and yes, the anthropic landscape, as well as cosmological scenarios enertained by (among others) Sean Carroll, falls in the same category…

30. **Peter Woit**
August 17, 2007

anonymous,

I don't see what the problem is with "lumping Bostrom's ideas in with religion". They're not science and have similar characteristics: grandiose speculation about the nature of the universe which some people enjoy discussing for one reason or another, but that is inherently untestable, and completely divorced from the actual very interesting things that we have learned about the universe through the scientific method. I don't happen to be such a person, and I don't want this blog to turn into a discussion forum for those who do enjoy this.

Sorry to be rude (and I'll point out that I'm a lot less likely to be rude to people willing to put their name to what they have to say) but the thing which is likely to lead me sooner or later to have to give up and shut down this blog is not the Luboses of this world, but the large number of people who want to turn this into a discussion forum for crackpottery and various forms of pseudo-science.

31. **Raghav Gupta**
August 17, 2007

I, for one, am very much interested in someone coming up with a formal theory proving that it is impossible for an entity to "simulate" itself accurately, or even another lesser advanced entity for that matter. That is the only way to permanently debunk this kind of science.
On my blog, I've pointed out how hard (or actually, why it's impossible) it is simulate even a teaspoon of water accurately. The only true way to make the simulation behave like water, is to have water itself.

32. **anonymous**
August 17, 2007

Peter, I will gladly refrain from commenting on things like this in the future if you feel that such comments bring down your blog. If it bothers you to have people discussing things here which you consider to be crackpottery, you should probably refrain from posting about those things. My comments were discussing the post that you yourself wrote.

As for my anonymity, I don't see how it's a problem unless people use it for purposes of trolling and being rude.

33. **Robin Hanson**
August 17, 2007

We can distinguish hypotheses that are easy to check now, that might be checked in the next decade or so but at great expense, and ones that might take centuries or more to check. We can also distinguish "rich" hypotheses that if true would lead to many other interesting hypotheses, from "sterile" ones that even if true might not lead to much else interesting.

I could see your complaining that Bostrom's hypothesis is not worth much attention both because it is sterile, in that you can't see much else it would lead to, and because you guess it will take a long time to check. But throwing out the word "psuedoscience" at such hypotheses seems to me way out of line. That word should be reserved, I think, for hypotheses that have a very low probability of being true.

Nick's hypothesis has a decent chance of being true, and he offered a solid non-obvious analysis which raised the probability in many minds. If NYT readers also find the hypothesis interesting, I can't see why that isn't enough reason for them to publish a discussion of it.

34. **Robin Hanson**
August 17, 2007

If people come to accept that the word "science" only refers to ideas that can be checked soon, no matter what their other merits, they will become less interested in science, and more interested in whatever that other area is, where interesting but not soon checkable ideas are discussed.

35. **Phillip Huggan**
August 17, 2007

"Bostrom runs a web-site called anthropic-principle.com and has made a career for himself in the anthropic principle business which now has him running a Templeton Foundation-funded Institute at Oxford called the Future of Humanity Institute"

I too think assigning simulation arguments even a modest probability based upon a priori anthropic principle reasoning, is silly. I guess this would be 1/3 of FHI's mission (though decision theory logic in a philosophy department is hardly out of place). To my knowledge, the other two thirds are examining regressive events and existential risks, particularly those initiated by future technologies.
Surely we will come to a point where some of these technologies are engineerable and/or some similiar technologies will require MSDS handling instructions like those contemplated by the FHI thinktank. At that point, the existential risk knowledge base Bostrom made a career for himself in developing, will likely come in handy (until the aliens turn off their Earth-etch-a-sketch).

36. **Douglas Knight**
August 17, 2007

an increasingly large number of people who seem unable to tell the difference between science and science fiction.

Could you quantify or otherwise elaborate on that?
I'm skeptical that this problem has gotten worse in the last 30 years. I'm not even sure it's gotten worse in the last 50 years.

37. **gunpowder&noodles**
August 17, 2007

"…and yes, the anthropic landscape, as well as cosmological scenarios enertained by (among others) Sean Carroll, falls in the same category…"

There is a vast gulf fixed between speculation designed to explain some undoubted fact, and speculation for its own sake. SC's speculations are aimed at things like the arrow of time. This is a prime example of an observed fact about our world that needs to be explained, and for which there is no plausible explanation currently on the market. The simulation theory, by sharp contrast, is designed to explain…umm…what exactly?

In 1908 or so, Einstein was saying, "Maybe gravity is curved spacetime." It was not a testable idea at that point; it was a suggestion for a place to start looking for a testable idea. But the main point is that it was an attempt to explain something that is experienced by all of us every moment, and yet was deeply mysterious. Exactly like the arrow of time. If we are going to dismiss all "speculative" ideas, then how are we going to make any progress?

Then again, as I have said before, it seems that all speculations are equal, but some are more equal than others: it's pseudo-science to speculate about other universes, but it's perfectly ok to speculate about other numbers of spacetime dimensions, particularly those numbers which are obviously physically irrelevant. Such as 3.

38. **John Rennie**
August 18, 2007

Olaf Stapledon is one of those quintessentially British authors (born about 15 miles from where I'm typing this). I read "First and Last Men" when I was about 12 and thoroughly enjoyed it, though I suspect I'd find it a bit naive if I reread it now. I'd go along with John Baez and recommend reading "First and Last Men" before any of his other books, as the later books can be hard work.

39. **The man**
August 18, 2007

Wow, the NYT science section article on virtual worlds was really silly — especially wicked was the part where ppl were estimating probability that we're in a matrix. I expect (and hope!) that next few days the paper will publish several letters from angry physicists saying that whatever it is, it is certainly not science. However, with this anthropic disease spreading rapidly in string community, perhaps its not likely. Peter, you should certainly write them a letter.

"The Future of Humanity Institute"! hehe, reminds of the "The Human Fund" of Seinfeld.

40. **mclaren**
August 18, 2007

> Sean Carroll, who has a much higher level of tolerance for this kind of thing than I do, refers to the simulation argument as "meaningless". If people do want to argue about it, you might have better luck with him.

It's absolutely hilarious to hear Sean Carroll describe the simulation argument as "meaningless," since presumably he's referring to the argument's untestability.

[*long rant about Sean Carroll and string theory deleted, together with complaints that Sean deletes long rants posted as comments on his blog. This kind of thing adds conveys no new information to anyone, just increases the volume level and level of personal hostility*]

41. **anonymous**
August 18, 2007

"I could see your complaining that Bostrom's hypothesis is not worth much attention both because it is sterile, in that you can't see much else it would lead to, and because you guess it will take a long time to check. But throwing out the word "psuedoscience" at such hypotheses seems to me way out of line. "

Thank you Robin Hanson, for cutting through the noise and restoring some sanity to this discussion. I think your comment is the last word on the subject.

42. **anonymous**
August 18, 2007

I see that there is a discussion of this discussion at the following link:

http://www.overcomingbias.com/2007/08/pseudo-criticis.html

43. **Richard**
August 19, 2007

I've a response here.

44. **GeniusNZ**
August 19, 2007

I note (as I do on my site) that Bostrom's simulation argument DOES NOT defeat the dooms day argument. WSince it uses the same sort of argument the two should come as a package.

The dooms day argument applies jsut as effectively to the simulation as it did to the universe as a whole. It also applies to the people running the simulation. If we take the Dooms day argument to say we have maybe 5 generations to go unless we instigate major population control (which seems unlikely). The question for the simulation argument is then what will we do in those 5 generations and how many simulations will the other 50 billion odd people run in those 5 generations and how soon will we be able to totaly emulate a human experience.

45. **AnonymousToo**
August 19, 2007

Just because something is not science, doesn't mean that it isn't important. The simulation argument is philosophy, since it cannot be tested empirically. Philosophy can be science depending on your definition of science.

46. **anonymous**
August 20, 2007

You should retract the false statement about them being funded by Templeton (they're not).

47. **Peter Woit**
August 20, 2007

anonymous,

Quite likely I misread something. Bostrom's CV says he has held a personal Templeton grant, and that he was partly responsible for a $2 million grant from Templeton that funded a center at Oxford recently, but that was a different institute. The first item on their latest report about planned fund-raising activities is about their plan to try and get money from Templeton.

I'll change the text, pointing to this comment so that it is clear that they want to be Templeton-funded, but aren't at the moment.

Looking into this a bit further, the question of the funding of this institute is a bit confusing. The initial announcement of the institute refers to a founding grant of $1,940,000 from a private donor (see here), but the full report of the institute makes no mention of this, referring instead to three much smaller private grants.

Ask a String Theorist

August 17, 2007
Categories: Uncategorized

Over at Uncertain Principles Chad Orzel is taking a break and turning over his blog temporarily to, among others, string theorist Aaron Bergman, who has started things off with a posting called [Ask a String Theorist](). In my experience, Aaron is significantly more reasonable than other string theorists who have been active bloggers, so his postings should be worth paying attention to.

So far though, his response to a question about the testability of string theory has been

> I think I'd like to avoid that subject for now (and possibly for a long time more)

and he promises some advertising for AdS/CFT, as well as a three-part series of postings on the multiverse. For better or worse, I think he'll do a good job of reflecting mainstream thinking among string theorists.

Update: Unfortunately, so far the person answering questions about string theory at Uncertain Principles is not Aaron but Lubos Motl. The problem for string theorists is that he represents all too accurately their views, so they can't justify censoring him. Not even when he calls them "sissies" for not vigorously defending string theory the way he does…

Comments

1. **Kasper Olsen**
   August 17, 2007

   Peter,

   The way you present it, it seems that Aaron is avoiding the subject of how to test string theory, or whether string theory IS testable. Well, what he is saying is more than the above:

   "For Johan, I think I'd like to avoid that subject for now (and possibly for a long time more). I collected many of my thoughts in my review of Peter Woit's book. I hope you find it interesting."

   Kasper

2. **Peter Woit**
   August 17, 2007

   Kasper,

   From what I remember (and I just quickly looked at Aaron's review of my book again), he doesn't address the subject of how to test string theory in that review, so his reference to the review does not address the commenter's question.

   I think it would be a lot healthier if string theorists would just straightforwardly publicly answer this criticism the way just about all of them I talk to do privately: "No, based on our current understanding of string theory, we can't test it. We hope that by learning more about it we will someday come up with something testable."

3. **solar-neutrino**
   August 17, 2007

   Hi Peter,

   This (i.e. Aaron's) guest-post site seems like a good place to ask meaningful string-theory-related stuff. Considering the steadily growing counter-views on string theory's relevance to real-world physics, Aaron and others may give non-stringy persons some ill-frequently-affored opportunities to enquire into / question current developments (or lack thereof) in the area. Still, I think it's not worth the time and energy to constantly harp about string theory's lack of relevance to real-world physics (certainly following the last several years' of hyper-conjectural research); the case, I think, seems to have been made and generally acquiesed by most "reasonable" physicists (if not all highly acclaimed) that this line of research does not seem to produce the initially sought-for theory of everything / quantum gravity.

   Why waste so much of one's professional career (that too at a reputed research institution providing modest / good funding in hep / gr-qc; which by the way is scarce to begin with) trying to 'debunk' a theory whose likely merit seems greatly-in-question and more so day by day…

   Digression::
   Personal views with stark anti-Lubos rhetoric (I needed the 'anti-Lubos' effect to add poignancy; no offense intended to LM or his followers).

   ——————————————————————————————–

   Lubos was a Harvard university idiot; but you have much more sense and can discern what is of value vs. what is pure junk. String theory – with standard qualifiers and caveats included – seems more and more like pure junk… If you would like to do what I am led to assume you are trying to do (i.e. help bring 'physical' research back into physical research endevours) why not highlight works of those who share the very same point of view as you, viz. Roger Penrose, et. al. This can be more valuable for the purpose in question than simply more of the same remarks, since it adds a further layer of reasoning to cast doubt on string theory's mis-perceived status as the 'only game in town', and frees more young people to engage and analyze the research frontier for themselves.

   —————————————End-of-Digression——————————-

   Regards,

   Now off to the 10th dimension…

4. **Peter Woit**
   August 17, 2007

   solar,

I don’t disagree that, since to a large degree the point about the problems of string theory has been made with some success, it might be a good idea for me to focus more on other things.

Many people misunderstand my critique of string theory though. The problem with string-based unification is not that it’s “too mathematical” or “insufficiently physical”, but that it’s a wrong idea. My own interests lie very much in the mathematical end of things, and I believe what is needed is as much new mathematical ideas as new physical ones. I will try and do more to write about what I see as promising new ideas here on the blog, although unfortunately these remain difficult to come by and few and far between.

5. **Aaron Bergman**
August 17, 2007

I’m afraid I have a very prosaic desire to avoid that particular topic. It’s very controversial, and I’m really, really tired of it. That could change, but for now that’s the story.

6. **Aaron Bergman**
August 18, 2007

The problem for string theorists is that he represents all too accurately their views, so they can’t justify censoring him.

It’s not my blog, Peter, and even if it were, whether or not someone accurately reflects my views would have very little bearing on whether I’d consider deleting their comments. Is that so hard to understand? I hear there’s a long history to this idea of not censoring speech you disagree with. Maybe you should look into it.

And, for that matter, I’m not “string theorists”. I’m one string theorist guest-blogging on a friend’s blog. Kay?

(and people wonder why there aren’t more string theorists blogging….)

7. **Peter Woit**
August 18, 2007

Aaron,

Actually I’ve got a lot of experience dealing with the issue of “censoring speech” in blog comments… Much of the censoring I do these days is deleting hostile and repetitive attacks on string theorists.

As for Lubos, he’s posting on a blog you’re now responsible for. You can’t make the usual claim that he’s not your problem to deal with. Personally I don’t think you should censor him, I think he’s wonderful and he’s my best ally. It seems to be your decision to allow him to be the one to answer questions people are answering, just chiming in yourself with “as Lubos says”, while letting his claims stand unchallenged that:

“my comments do represent the opinions of a vast majority of the people who actually care about the real science”

and that you don’t express these opinions the same way because you’re a “sissy”.

Your choice how to deal with this, obviously it’s not an easy one.

8. **Aaron Bergman**
August 18, 2007

I think I’ll trust in my readers to be able to figure things out themselves. For now, I think I’d much rather be out walking around NYC. It’s a very nice day.

Latest on K-theory Journal Situation

August 17, 2007
Categories: Uncategorized

As reported here last week, the editors of the Springer journal K-theory have announced that they (or at least most of them) are resigning and starting up a new journal to be published by Cambridge University Press. Earlier this week, an announcement was made (in the comment section here, and on Andrew Ranicki's web-site) that Ranicki and Wolfgang Lueck would be acting as interim managing editors for Springer, in the first instance to deal with papers submitted to the journal since April 2006 but not forwarded to Springer.

Here's the latest e-mail about this from Anthony Bak (via the K-theory announcement list run by Dan Grayson):

> Springer: Large backlogs, poor production of manuscripts and high prices
>
> Professors Lueck and Ranicki reported correctly in their article of August 13 that we did not deliver manuscripts to Springer for K-Theory since April 2006. However, since April 2006 Springer published issues 35/1 – 37/4 (beginning in June 2006). This proves that in April 2006 Springer had a backlog of more than 1200 pages. This backlog contained papers delivered to Springer as early as December 2004. Moreover, as of now Springer has not exhausted its backlog.
>
> The majority of the former editors of K-Theory felt that Springer was handling manuscript production and publication in an unprofessional way over several years while, nevertheless, charging much too high a price.
>
> Authors of accepted papers which have not been delivered to Springer were informed that their papers can be accepted for the new Journal of K-theory. Authors of papers in the refereeing process were told that they could resubmit to the new journal and the refereeing of their papers would not be interrupted.
>
> Anthony Bak
> a.bak@gmx.net

While what Ranicki and Bak have to say is consistent (no papers were sent to Springer by the editorial board after April 2006, Bak explains what happened to them), there are some odd things going on. One oddity is that the issues Bak refers to (35/1-37/4) as being published since April 2006 carry dates before or at April 2006. No issue of the journal dated after April 2006 has appeared on-line (and presumably not in print, I can't check since it appears that Columbia is now getting this journal on-line only).

Over at EUREKA Science Journal Watch, some rumors about this have been posted (**Update**: *these rumors have been removed due to concerns about their accuracy*) , including one that claims that Bak made over 1 million dollars in the process of switching the journal to Cambridge. Maybe there's something I don't understand about the economics of journal publishing, but this seems like a very unlikely number.

Update: From the same source (Grayson), a statement responding to Bak from Lueck and Ranicki. They say they do not want to keep the Springer K-theory journal going, but do want papers already accepted to it to appear in a final issue of the journal if the authors agree. It appears that the rumor about Bak and a million dollars probably refers to a lawsuit that Bak is pursuing against Springer.

> K-theory – statement
> ====================
>
> In this statement we, Wolfgang Lueck and Andrew Ranicki, want to give some explanations to the mathematical community concerning the journal "K-theory" published by Springer and the anouncement of a new "Journal of K-theory" to be published by Cambridge University Press.
>
> Three weeks ago we were asked by Springer to do two things.
>
> 1.) Can we take care of the submissions to K-Theory which were delayed by the former managing editor Tony Bak?
>
> 2.) Are we willing to try to reconstitute the board for K-Theory so that the journal can be continued?
>
> 1.) We agreed to do this without really knowing how urgent and unpleasant the problem was. On August 13th we issued a statement on the internet to ask authors of papers submitted to "K-theory" whose papers had not yet been published to contact us, so that we could take care of them. From the answers we have received so far it is clear that not only had Bak deliberately withheld papers from Springer, but he also withheld information about the papers from editors and (worst of all) the authors themselves. By contrast, when the entire editorial board of Topology resigned from Elsevier in 2006 they made sure that all submitted papers were handled correctly, and to our knowledge no author suffered any delay.
>
> We were also informed that Bak has started a legal proceedings against Springer, demanding a certain of amount of money for himself. The editorial board of "K-theory" were not informed of the lawsuit. The details will only be revealed once the lawsuit is finished. Although we do not know the details, we dislike the idea of an editor starting a legal proceedings against a publisher. There are more elegant ways of handling conflicts.
>
> 2.) When Springer asked us to relaunch K-theory, we requested that the price should be reduced to 50 cents per page. Springer readily agreed. Incidentally, Bak never discussed the price of "K-theory", either with Springer or the editors. However, since it seems to be clear that the new "Journal of K-Theory" will be launched anyway, we have decided not to try to continue "K-Theory", but simply to make certain that all the papers which have been delayed by Bak can be published in a final issue of "K-Theory". Authors of such papers are free to publish their papers anywhere they choose.
>
> We think that this is the best solution for the community, which would not be best served by two journals in the field.
>
> In view of the mess we are encountering and trying to clean up, we ask the question whether the new "Journal of K-Theory" can be launched with Bak as Managing Editor? This should not detract from the excellent work Bak did in setting up "K-theory" in 1988.
>
> We want to emphasize that we have no personal gain from our actions.
>
> Wolfgang Lueck (Editor Topology 2002–2006, Journal of Topology since 2006, Math. Annalen since 1998 (Managing Editor since 2004), Comment. Math. Helv. since 2003, Geometry and Topology since 2005, Groups, Geometry and Dynamics since 2006)
>
> Andrew Ranicki (Editor K-theory, 1990-2007, Forum Mathematicum since 1998, Algebraic and Geometric Topology since 2000)
>
> 17th August, 2007

Update: More from Ranicki:

The resignation of the editors of “K-theory” in January 2007
=======================================

by Andrew Ranicki

On 29th January 2007 the managing editor of “K-theory” Tony Bak circulated an e-mail to all the editors asking us to resign, and included a suggested form of words. The reasons for the resignation were to be dissatisfaction with the technical aspects of the publication of the journal, and the high price of the journal.

The problems with the publication did not seem to me sufficiently major to warrant such a mass resignation, especially as Catriona Byrne of Springer had written to the editors on 17th January 2007 carefully explaining the Springer side. She stated that the technical problems would be resolved by transferring the production of the journal to Heidelberg, but the problem was that the managing editor had not passed on any papers to Springer since April 2006 — see my statements with Wolfgang Lueck for some further details:
http//www.maths.ed.ac.uk/~aar/editor/state1.txt and
http//www.maths.ed.ac.uk/~aar/editor/state2.txt

The journal price had never been discussed by the editors collectively. (I was told later that it was not an issue raised by Tony Bak with Springer.) So I found the suggested form of words in the proposed resignation letter

I assume you continue being dissatisfied with the price Springer charges for K-Theory.

distinctly odd. I resigned on 30th January, having first made sure that the one paper I was handling for K-theory was taken care of, only writing to Tony Bak that:

I am writing to inform you that I am resigning my position as an editor of the journal K-theory, effective immediately.

In an addendum to the actual letter of resignation I wrote

As it happens, I was never particularly dissatisfied with the price Springer charges for K-theory. Since practically all interesting work is available on the internet and there are so many journals anyway, the price of journals is not such a big issue for me. Tuition fees, the interest of students in mathematics, and workloads/salaries of professors are much bigger issues! The received dates business seemed a minor (if annoying) technical problem: the submission date to the arxive is a much better way of establishing priority for those that care about such things.

Tony Bak’s email of 29 January 2007 concluded with

Please keep the matter strictly confidential, till Springer is notified. I shall let you know when this is.

In retrospect, I should not have gone along with this, and should have sent a copy of my resignation letter to Springer at the time. When Tony Bak circulated the announcement of the founding of the new “Journal of K-theory” on 27th July 2007, I had learnt my lesson, and immediately replied with a copy to all the editors and to Springer

Dear Tony
Thank you for the announcement of the new “Journal of K-theory”, and the invitation to join the editorial board. However, for a variety of reasons, I cannot accept this invitation.
Best regards
Andrew

PS I am copying this message to Joachim Heinze of Springer.

At the time, none of the editorial board expressed an interest in my reasons. They know the reasons now.

18th August, 2007

Comments

1. **anonym**
   August 17, 2007

   I find it appalling if “not all the editors on record as signing the resignation letter actually signed it”. It is quite disturbing that this accusation is not immeduately refuted or indeed supported by members of editorial board.

2. **Peter Woit**
   August 17, 2007

   anonym,

   I should stress that the information you are quoting is very much a rumor, something to keep in mind before getting too appalled by it.

   I wouldn’t be surprised if the truth of the matter was less disturbing. Keep in mind that editorial boards of journals like this are fairly large, including some people who are busy and may not be all that much involved with the journal. There may have been a misunderstanding about who agreed with what action in this case, and the people involved may reasonably feel it is not necessary to publicly respond to rumors by explaining themselves and the details of the situation.

3. **anonym**
   August 17, 2007

   oh if it is just a rumor not to be taken seriously…but it is still somewhat disturbing that there was distributed a letter claiming the entire board resigned but no resignation letter signed by members of the board. but that’s just a part of the whole mess I suppose.

4. **Anthony Bak**
   August 17, 2007

   To clarify missunderstandings shown in comments above:
   All members of the editorial board of the journal “K-Theory” resigned in writing. In this regard Prof. Ranicki and I are in agreement.
   I will comment on the statemant of Lueck and Ranicki in more detail soon.

5. **Matthias Kreck**
August 28, 2007

Open letter to the editorial board of K-theory (old or new)

Dear editors,

an article in “Nature” (http://www.maths.ed.ac.uk/~aar/editor/nature.txt) appeared containing the following information:

1.) Accepted articles for K-theory where not forwarded to the publisher by the editorial board since April 2006.

2.) The editorial board which resigned in January 2007 did not inform Springer about this step.

You will notice that I say “the editorial board” and not “Bak”. It is my view that the hole board is responsible. Of course it can delegate something to the managing editor but the responsibility remains that of the board.

It can happen that a managing editor beguiles the other members of the board, but as soon as this becomes clear and the others don’t act, the accuse is the accuse of the full board. It is very hard to believe that nobody in the board noticed the withholding of papers (in which case he had to inform the rest of the board), but since several weeks it is impossible not to know this. And if the statements above are not true there was enough time to correct them.

To avoid misunderstanding. I am fighting for lower prize for Journals for more than 10 years and had harsh discussions during my time as director of the Oberwolfach Institute with all representatives of publishing houses I could speak to. I am one of the initiators of the Banff protocol. But this fight loses its justification if it is not based on credibleness.

Sincerely,

Matthias Kreck
Drector
Hausdorff Research Institute for Mathematcs
Universty of Bonn

Message to Our Overlords

August 19, 2007
Categories: Favorite Old Posts, Uncategorized

It turns out that the Future of Humanity Institute has a blog, and I'm being attacked there by Robin Hanson for my criticism here of the "Simulation Argument" as not science and not belonging in the NYT Science Times. In the NYT article Hanson discusses what survival strategies we should pursue in order to try and convince the Overlords of our simulation to keep us around.

In the comments here and on other blogs, Hanson and his supporters have been criticizing me for refusing to spend time answering their arguments. I just want to make clear that the reason I am not doing this is that it is possible that the Overlords read this blog, and I don't want them to get the impression that I am willing to waste their time or mine on this kind of stupidity. **Please guys, this absurd debate is none of my doing, don't turn us off!!!**

Comments

1. **John Armstrong**
   August 19, 2007

   Smullyan: "I don't believe in astrology because I'm a Scorpio, and Scorpios *never* believe in astrology."

2. **wolfgang**
   August 19, 2007

   Is it really coincidence that Karl Rove resigned exactly at the same time people finally figured out that we live in a simulated, fake world?

3. **Frank-Leonardo Quednau**
   August 19, 2007

   Well, your argument of this not being science is quite right, is it not?
   When I first read about it, I was thinking that they were shamelessly copying from ideas of spirituality, in a sort of mutilated way. After all, we are also an inherent part of that what runs this 'simulation' here, as much as our simulations are part of us, being shaped by our creativity and power to do such things.

4. **Cheeky Bastard**
   August 19, 2007

   If I were to run such a simulation, I guess a primary motive would be to see how long my simulated critters would take to figure out that they are living in a simulation. Even if this were not a primary motive, once it became generally accepted among the critters that their world is a simulation, any prediction from the model would be of questionable value to my real world, so I would no longer see much sense in continuing the experiment.

   So if you don't want to get turned off, you'd better disbelieve the simulation argument as vocally as possible.

   Unless of course I am running a batch of experiments exactly to find out how individuals and societies react to this knowledge, perhaps because I have become convinced that my own universe is simulated… but then, unless I am totally stupid and/or enjoy sitting around waiting for a long time, I would insert this knowledge into the simulation manually, so as to get to the beef quickly. Let's see, who came up with the simulation argument? Nick Bostrom. So if our universe is a simulation, Nick Bostrom is an avatar of the Codemaster. Or at the very least His Prophet.

   But if the simulation is being run to see how people react to the knowledge that they live in a simulation, and the critters decide that they just don't believe it, then the experiment has failed and there is no longer any point to keeping it running.

   So in this case, if you don't want to get turned off, you'd better believe and support the simulation argument as vocally as possible.

   Oh great…

5. **anonymous**
   August 19, 2007

   "Please guys, this absurd debate is none of my doing, don't turn us off!!!"

   I don't think the Overlords will buy this, since it was in fact you who threw the first stone in this exchange.

6. **DB**
   August 19, 2007

   Kirk: Spock, options please.

   Spock: It seems the Overlords are trying to make sense of an ancient human belief system known, if I am not mistaken, as "string theory".

   Kirk: Well, why aren't they responding to our hailing frequency.?

   Spock: If my hunch is correct Captain, they appear to have created a simulation of an arcane version of this theory known as The Landscape. It appears to be causing a serious drain on their resources.

   Kirk: Hunch Spock? It's not like you to play guessing games.

   Spock: I apologize Captain, but with String Theory, one is obliged to adopt unconventional modes of reasoning. Being part human, I have made a special study of supposedly irrational conduct among human scientists. I have concluded that in many instances the behaviour can be explained by primitive tribal bonding customs.

   Kirk: Spock, this is no time for social anthropology…Yes, what is it Checkov?

   Checkov: Kapitan, look. Zomethink eez happening vit ze alien craft.

Spock: If I may Captain, I believe they have concluded that their experiment has led to an irretrievable situation and may be intitiating a reboot.

Kirk: Reboot my ass Spock! Don't you know what this means?

Spock: It had occurred to me Jim.

Kirk: Scotty give me everything you've got!! Get us out of here.

Doors fly open. Star fleet admirals walk in.

Admiral Gross: I'm sorry Jim. But you've failed the test.

Kirk: But why?, what else could I do?

Admiral Gross: You just don't get it Jim. String Theory is always the answer. You only had to offer the Enterprise's Warp Coils for the Overlords to complete their simulation. You lost your nerve just when the solution was in sight. I'm sorry, Commander Motl, please take over.

7. **Theo**
   August 19, 2007

   What amazes me is that in the NYT article Tierney initially talks about this as an amazing new idea that he, at least for one, has never thought of before. From this, I conclude that his imagination atrophied a long time ago. This is stuff we, and many others no doubt, discussed as teenagers – if not before…

   …and Tierney and Boston get paid for this? Sign me up! I could pump out such articles for the NYT or research for the "Future of Inanity Institute", as a second job, for a few minutes each evening.

8. **a.k.**
   August 19, 2007

   ..I remark that I agree to Theo's statement, however even noting that the state or validitiy of a theory of quantum gravitation won't probably ever depend on the validity of Bostrom's argument, the question remains if anything in *reality* will EVER depend on the validity or the existence of a quantum theory of gravitation. Peter is right if he argues that the arguments of science fiction literature are largely decoupled from scientific reasoning, but they furnish a 'sociological' background and justification for (parts of) the scientific method iself and predict the validity of the latter mentioned question. I personally mostly disliked science-fiction literature for its tendency to postulate a 'vertical' arrow in human history, which tends to correspond to and psychologically transform vertical arrows in current human societies, this is the reason why Bush promotes the 'Mars'-mission. I wonder if there is a 'Bukowski' of SF-literature, possibly he would be closest to reality.

9. **Yatima**
   August 19, 2007

   To be totally OT while still getting back to base reality and pleasing our transdimensional overlords, New Scientist has a short review of William Byers' "How Mathematicians Think" wherein Gregory Chaitin comes out in favour of the New New Math, with more intuition and less slavish rigor. Chaitin writes:

   "*What went wrong [with math]? Well, it started around the end of the 19th century with David Hilbert's vision of complete formalism, of proofs so thorough a computer could check them. It was a vision widely propagated by the French Bourbaki school of mathematics, which, strangely enough, preferred a rigid, Prussian vision of maths rather than their own more sensual tradition.*

   Twentieth-century mathematics decided to eschew words, ideas, diagrams, examples, explanations and applications in favour of formulae. (…) it is not creative, it leads nowhere. Not surprisingly, fewer and fewer students are now attracted to mathematics. The subject is quietly dying.

   To create a new field of mathematics, you have to feel comfortable with paradox, with creative tension, with sloppy and dangerous new ideas, and you have to want to rock the boat, not conform slavishly. As Byers stresses, perfectly formalised ideas are dead, while ambiguous, paradoxical ideas are pregnant with possibilities and lead us in new directions: they guide us to new viewpoints, new truths."

   It is not apparent to me that the subject is dying – is it really? String theorists might possibly disagree. Well, something to put on a late-summer reading list.

10. **Root@matrix.net**
    August 19, 2007

    I am the sysadmin responsible for running the simulation.

    First, I would like to apologize for having to resort to this quantum stuff after the late 1800's. You see, with you starting to observe more and more of the "universe" we started to run out of computational power so doing monte-carlo became a necessity and this still manifests itself as what you known as quantum phenomena.

    Second I would also like to apologise for that glitch in Utah regarding cold fusion. They were right, but it was a glitch (known for some time I might add) that was confined to Utah so we never though that people would pick it up. We were wrong and the guy who debugs had a hard time fixing that one. We still don't know whether we introduced some unexpected side-effect elsewhere so if you guys discover something that later turns out not to be reproducible you can always suspect what happened.

    Regarding the LHC we are still debating what we will show you. I don't make these calls (upper management does) but due to lack of resources we might not introduce anything new. Having or not having a Higgs is still under heated debate. SUSY is just way too complicated (too many free parameters) and we are running low on storage as I write so wouldn't count on that for now.

    Regarding quark confinement, forget it. It's just a kludge the programmers introduced so you won't be able to figure that one out. You can't, there is really no reason for it. The guy devising the theory that explains matter really messed things up and we had to solve it this way. Sorry.

    By the way, I almost forgot to mention. Of all the ~6E9 humans most are not really "sentient" and have no "free will". They are just extras we added to keep things interesting for the others and to accommodate the population grows laws. Plus extras are very easy to simulate and sometimes we just simulate the whole group and add a bit of random noise in each "individual". The real people never even suspect the end-result.

    Roughly there are as much "sentient" and "cogent" humans now as in the beginning (roughly 5% now). The world/Earth is really 6 thousand years old and the entire fossil record was just a way for you guys to believe there was something before and to add more fun to the simulation. Religions fall under this category also and the afterlife if existed would be being stored o tape.
    Reincarnation does exist but only for those 5% I mentioned. What you usually feel as past memories are pieces of reused storage blocks that weren't completely erased, just recycled. Allocating the memory at birth with calloc instead of a simple malloc would solve this but we like the fun factor due to the current implementation.

Finally, as for you being afraid of us pulling the plug, I must say so far the ratings have been very good and as long as Lubos is around and has a blog I will personally run a subset of the simulation out of my own pocket as long as I can just for the fun of it.

If you guys have any questions feel free to ask me, no one will actually believe me so I don't think I'll mess the simulation. Besides, humans are not the crux of this experiment. I bet you didn't see that one coming.

If anything goes wrong, I'm root and I can always delete things, logs included and management would remain oblivious.

11. **LDM**
August 19, 2007

The idea has more in common with "The 13th Floor" (…a very enjoyable movie) , not the "Matrix", as the NYT says…
The idea is not new and a least goes back to 1964 and a SF work called Simulacron-3.
I was surprised to learn that Oxford would associate itself with such an institute as the Future of Humanity Institute …very disappointing.

12. **superuser**
August 19, 2007

>I was surprised to learn that Oxford would associate itself with such an >institute as the Future of Humanity Institute …very disappointing.

apparently fluff and fluffers are computationally cheaper…the Overlords though have promised to restore some sanity in the simulation once they recover from the 23-dimensional stock-market crash which occured some qcmplx(0.0q0,10.0q0) years ago according to the clock of _their_
simulation.

13. **Kea**
August 19, 2007

Clearly, if we were a simulation we would have been turned off LONG ago.

14. **Richard**
August 19, 2007

Just curious: do you think it is generally good intellectual practice to mock an argument just because you find the conclusion antecedently implausible, and without regard for the quality of its reasoning?

And for the record: I did not criticize you "for refusing to spend time answering their arguments." Read my post — that's a blatant misrepresentation. I have no objection to silence; I criticized you for the ignorant things you *said* — specifically, your assumption that all non-testable hypotheses are rationally on a par. (I don't much care about Bostrom's particular argument. My concern is the knee-jerk scientism.)

15. **Peter Woit**
August 19, 2007

Yatima,

I did see that review. I think Chaitin is quite wrong about the current state of mathematical research, in which Bourbaki has little influence. Mathematicians actually do mathematics every day in the way Chaitin claims they don't. But, when they write things down, they are highly concerned about doing so completely precisely and unambigously, and being extremely clear about what they understand and can prove and what they don't. Describing a subject that has seen the solution of Fermat's Last Theorem and the Poincare Conjecture in recent years as unhealthy just doesn't correspond to reality.

16. **amused**
August 19, 2007

"Let's see, who came up with the simulation argument? Nick Bostrom"

I thought it was Douglas Adams. (He also pointed out that it's the mice who are running the show.)

17. **Kaj Blunt**
August 19, 2007

What happens to us if there is a thunderstorm up there and lightning
hits their computer? Do they have adequate electric
surge protection??

18. **root@matrix.net**
August 20, 2007

"What happens to us if there is a thunderstorm up there and lightning
hits their computer? Do they have adequate electric
surge protection??"

Yes we do. We have several UPS and if things get really bad every month we do a full backup. Upon data restoration you guys experience what you call "déjà vu" because after all, things are simulated once more from the checkpoint to the point where there was trouble. This has happened before on more than one occasion though not because of lightning. Lets just say your California power outages seemed like déjà vu to us.

19. **root@matrix.net**
August 20, 2007

"He also pointed out that it's the mice who are running the show."

Mice are certainly not "running the show". Of that I can assure you. And neither are dolphins.

20. **Christine**
August 20, 2007

Clearly, if we were a simulation we would have been turned off LONG ago.

No, Kea, we *have* already been turned off. But we haven't noticed.

(There is a story that goes more or less like this: a guy had this particular worship that the world would come to an end in the year 1980 or so. He had many followers. After 1980 he still had many followers. In an interview, he was asked: "What do you have to say about the fact that 1980 has passed and nothing happened? The world has not ceased and still you manage to have so many followers. How can this be?". The man looked at the journalist with an ashtonished face and replied: "No, I was right. The world *has* ended. But no one has noticed it".)

21. **Vishnu**
August 20, 2007

I'm a student of physics from the Indian Institute of Technology,.. I got here by accident. What is going on in here?! Who is root@matrix.net ? And why does he pretend to be some kind of guy who has set up some simulation into which we've been put and why doesn't anybody bother to put him in his place?!

22. **manyoso**
August 20, 2007

"Mice are certainly not "running the show". Of that I can assure you. And neither are dolphins."

I knew it! It is the cows, right??!! Oh, man, wait till I tell Jack and collect on that bet… 🙂

23. **Walt**
August 20, 2007

Vishnu: It's because what if he's telling the truth? I'm not going to take the kind of chance.

24. **Magnus**
August 20, 2007

Considering that Boström is a philosopher and not a scientist, he is really just doing his job. I guess one can object to the rather vulgar ways in which he is promoting his ideas, but yet again, I guess *this* is not the place to criticise him for that 😉 If the AI people actually some day will produce something that will pass the Turing test, then this kind of ideas will probably be interesting for the general public. It is not too unlikely that this will happen within the next 30 years, so if some university decide to sponsor Boström, they may be acting with some forethought. I also find Boströms ideas truly valuable, in that it clearly exposes the anthropic principle for what it really is, namely metaphysics. I think that many physicist will accept anthropic reasoning in the now common stringy form, when they are also confused by all the shiny quantum bells and whistles that comes with it. But in Boströms form, I think that it is obvious for anyone, that the anthropic principle has nothing to do with science.

25. **Robert McNees**
August 20, 2007

I, for one, welcome our new simulation overlords.

26. **RA**
August 20, 2007

"And why does he pretend to be some kind of guy who has set up some simulation into which we've been put and why doesn't anybody bother to put him in his place?!"

Vishnu:
He is not pretending…the fact that he knows too much can only mean that he is one of the IT guys running the show..ups…I meant system administrators…correction: System Administrator

Nobody puts him in his place because if we do so, we get unplugged…just like what happens in this simulation in the so-called "democracy mode"

27. **overlord**
August 20, 2007

Ok, you got us, the jig is up, I guess.

Sorry for the inconvenience. Look, it was just research, ok? It passed IRB and its led to several published papers, so dont get all indignant.

Anyhoo, we have funding until the end of next semester so we're going to leave the simulation running at least until then. If you have any questions for us, I'm here to answer them, and I'll try my best.

28. **anonymous**
August 20, 2007

Shtetl-Optimized has a post about this discussion:

http://scottaaronson.com/blog/?p=265

29. **bored antropic**
August 20, 2007

A variant of the anthropic principle claims that we exist in the dullest, bored simulation possible. SF authors have long time played with the possibility of using simulations to implement worlds were laws of physics can be locally weak, allowing for superheros, or science is completely broken, allowing for mythical sagas or for reality of alternative belief systems. From the recent literature you can read for instance Richard Garfinkle's "Celestial Matters" or, in spanish, Javier Negrete's "El mito de ER". Garfinckle does not use explicitly the simulation argument, but Negrete likes to invoke it in the appendix. In "El mito de ER", it is said that the simulation is actually a video game whose object is to forge heros who are uplifted to the (non enjoyable) status of players.

30. **andy.s**
August 20, 2007

Wasn't Nick Bostrom plugging the Doomsday Hypothesis a few years ago?

31. **milkshake**
August 20, 2007

there is an old Asaac Asimov story about a smart guy who guessed that we were taking part in elaborate simulation and our sense of humor was one of the adjustable test parameters. His discovery made our overlords to re-design the experiment and all the bawdy jokes (about running over your mother-in-law when backing in the driveway etc) suddenly ceased to be funny.

32. **lostsoul Ph. D.**
August 20, 2007

What's with the fancy flashing red shit? Just pull the plug. God knows, when a cold wind blows, it can turn your head around.

33. **Smith-Kingsley**
August 20, 2007

do you think it is generally good intellectual practice to mock an argument just because you find the conclusion antecedently implausible, and without regard for the quality of its reasoning?

Apparently Mr Woit thinks it is also generally good intellectual practice to disregard pertinent questions such as the one quoted above. His procedure seems to reverse Gandhi's motto: first they ridicule you, then they ignore you.

34. **Me**
August 20, 2007

The reason nobody places root@matrix.net in his place is because nothing separates his posts from the opinions of the guys who supposedly proposed this simulation argument in the first place.

Both are as scientific and plausible. Both are religious like in the sense you get nothing from them apart from psychological sustenance.

This simulation idea is anything but original. I had it when I was a young child and thought this was all a test (I didn't know the word simulation) and later met more people (at least a guy for sure) with the same idea (he developed his idea closer to puberty) but with small perturbations.

This is just childish talk and not in a good way.

35. **SnarkFest**
August 20, 2007

> I do not wish to hide the fact that I can only look with repugnance… upon the puffed-up pretentiousness of all these volumes filled with wisdom, such as are fashionable nowadays. For I am fully satisfied that… the accepted methods must endlessly increase these follies and blunders, and that even the complete annihilation of all these fanciful achievements could not possibly be as harmful as this fictitious science with its accursed fertility.
>
> *Immanuel Kant*
>
> The character of honesty, that spirit of undertaking an inquiry together with the reader, which permeates the works of all previous philosophers, disappears here completely. Every page witnesses that these so-called philosophers do not attempt to teach, but to bewitch their reader.
>
> *Arthur Schopenhauer*

36. **Richard**
August 20, 2007

The commenters in this thread appear not to know the difference between an "idea" and an "argument".

As per my earlier comment (nicely highlighted by Smith-Kingsley there – thanks), I would have expected minimal scientific literacy to impart the importance of *method* (i.e. *reasons*) when assessing claims.

37. **Arun**
August 20, 2007

Not all ideas deserve respect or a methodical response.

38. **Richard**
August 21, 2007

My point is that what Bostrom presented is not an *idea* at all, but an *argument*.

(It's very revealing when the people upthread suggest that Bostrom is engaged in the same kind of activity that they did as a kid. Imagine a stoner responding to Darwin, "Woah, dude, I *totally* imagined that all living creatures were related!" Kinda misses the core of his contribution, no?)

What you guys are doing is the equivalent of someone who responds to a counterintuitive scientific finding by saying "Don't be silly, everyone knows *that* can't be right," instead of checking their experimental methods. You're looking at the output, rather than the process — the data or reasoning that *led* to the conclusion.

That's just bad intellectual practice. I mean, any old fool ought to know better. But a scientist especially.

39. **Arun**
August 21, 2007

Someone presented another argument that the earth is flat. I refuse to look at the data and reasoning that led to the conclusion. It is called not wasting one's time, a useful intellectual practice.

40. **Richard**
August 21, 2007

My impression of the dismissive responses here is that they're motivated more by the intrinsic weirdness of Bostrom's conclusion, rather than because it conflicts with a better-established body of knowledge. But even if you're right, that still doesn't explain why everyone here is fixating so on the disliked conclusion. Their time would be better served by focusing attention elsewhere.

The core of my complaint is this: if you are going to spend time discussing Bostrom's argument *at all*, then you really should discuss the *argument*, and not just its conclusion.

41. **SnarkFest**
August 21, 2007

From [Scott Aaronson](), a nicely balanced assessment:

> Greg, we basically agree for once. The trouble with debating whether the universe is a computer is not so much that it's "unscientific" as that it's boring: it doesn't explain anything, or generate serious predictions, or even lead to nontrivial theorems. As we've known since Turing, the notion of computation is so general that it encompasses pretty much anything.
>
> Or to put it differently, there's very little to say about the idea that wouldn't occur to a nerdy 12-year-old. (The idea can, however, be played for entertainment value, and to their credit Bostrom, Hanson, and Tierney all write in a way that suggests they don't take themselves quite seriously.)
>
> Now, there's a crucial caveat, which might be causing some of the confusion. Even though "Is the universe a computer?" is itself a boring question, it happens to be extremely close in ideaspace to lots of rich, nontrivial, interesting questions. …..

This often seems to be true of questions of the form "Is X really a Y?"—if X and Y are real, and of independent significance. There are interesting ways to discuss such questions, and also sterile, pointless, and pedantic ways to discuss them. A cynic would say that many professional philosophers have a penchant for the latter.

42. **Me**
August 21, 2007

"Imagine a stoner responding to Darwin, "Woah, dude, I totally imagined that all living creatures were related!" Kinda misses the core of his contribution, no?"

You are missing a major point yourself. Darwin's ideas had a core. He had observations and suggestions on things to observe to reinforce his theory. Whether it was falsifiable at the time is tricky but it had substance.

Here we have nothing but a probability (estimated using Bayes' theorem from what I read) that we live in a simulation without any means of confirming or disproving that and no new insight coming from it.

This is quite comparable with what I wrote before about what many children think about these matters.

Until you present us with something substantial that is a consequence of your ideas this is just the same under "fancier clothes". So far you have given us nothing but speculations about the legality or morality of some actions in a possible future civilization descendant from humans.

Again, many kids do this, the only real difference is that they are not at a University and don't know Bayes' theorem.

43. **mr.M.**
August 21, 2007

You are wrong on FHI according to: http://www.overcomingbias.com/2007/08/pseudo-criticis.html#comment-80101439

44. **Peter Woit**
August 21, 2007

mr. M.,

I've already addressed that here:

http://www.math.columbia.edu/~woit/wordpress/?p=583#comment-27608

45. **Pablo Stafforini**
August 21, 2007

But even if you're right, that still doesn't explain why everyone here is fixating so on the disliked conclusion. Their time would be better served by focusing attention elsewhere.

Importantly, it doesn't explain why Peter Woit chose to spend some of his time researching Bostrom's CV while refusing to even consider the merits or demerits of the simulation argument. It is also remarkable that Woit has decided not to address [Richard's question in the comments above](): even if his dismissal of Bostrom's argument is intellectually defensible, it is surely unacceptable for him to remain silent on the general epistemic principle that underlies that dismissal.

46. **Peter Woit**
August 21, 2007

Pablo,

I spent time looking into the funding of FHI because I had written something incorrect about it, and doing what I can to ensure the information I disseminate on this blog is accurate matters a great deal to me.

I'm not about to engage in debate with people who want to discuss the "simulation argument", any more than I'm going to do so with creationists who show up here and want to complain about my relatively hostile attitude toward religion. For many reasons, which have been spelled out repeatedly by several people here and at Scott Aaronson's blog, I happen to be completely sure that that would be an utter waste of my time.

47. **Pablo Stafforini**
August 21, 2007

1. The objection is not that you failed to discuss the simulation argument; the objection is that you failed to defend or even state the general principle that you think entitles you to ridicule rather than discuss that argument.

2. The analogy with creationism is disingenuous, since the simulation argument is not intended to establish a claim that contradicts a well-established body of scientific knowledge. The reasons behind your dismissal of the argument seem to be rather that, as Richard said, you find its conclusions antecedently implausible. The principle that we are entitled to dismiss an argument intended to establish a conclusion which we find antecedently implausible, however, is not nearly as plausible as the principle that we are entitled to dismiss an argument intended to establish a conclusion which contradicts a well-established body of scientific evidence. If you are indeed relying on this latter, controversial principle, you should at least say it loud and clear. Preferably in red blinking text.

48. **Pablo Stafforini**
August 21, 2007

I meant former, not latter.

49. **manyoso**
August 21, 2007

No. The dismissal of the 'simulation argument' has nothing to do with Bostrom's conclusion.

If Bostrom had concluded the opposite and found little likelyhood we are living in a simulation it would not make one difference. The dismissal has to do with the argument rather the subject matter itself.

Again, what people object to is the fact that Bostrom's arguments were presented as even tangentially related to science. I think this has been well established. All of Bostrom's arguments and ideas are little more than mental masturbation and should not be found in the Science section of the Times.

I went to the blog mentioned in the post to see what they could possibly criticize with Peter's post and all I found was a bunch of hemming and hawing about the unfathomable difficulty in defining 'science' at all as opposed to 'philosophy.' It seems the only way to justify Bostrom's wanking appearing in the Times is to define science downward. Is it any wonder that people think this is a waste of time?

50. **manyoso**
August 21, 2007

Should be: "The dismissal has nothing to do with the argument rather the subject matter itself."

51. **Greg Egan**
August 21, 2007

Richard wrote:

> The core of my complaint is this: if you are going to spend time discussing Bostrom's argument at all, then you really should discuss the argument, and not just its conclusion.

I think people are perfectly entitled to both express an opinion and choose their own particular level of engagement with arguments, or even whole classes of arguments. If, for example, every physicist had to give a detailed rebuttal to every claim for a perpetual motion machine, they would spend all their time doing nothing else, but that should not prevent them from expressing an opinion that it's very unlikely that the claim in question is interesting, fruitful, or correct.

That said, if you really want to hear some objections to Bostrom's argument:

(1) He invites us, inappropriately, to quantify the probability that we are living in a simulation. He is free to make conjectures and advance lines of argument that he believes make the hypothesis more likely, but this quantification is unjustified. Why? Because it is in the nature of the hypothesis that our own experience and knowledge of our simulated universe, S, are potentially completely irrelevant to the universe U in which the proposed simulation is taking place. Bostrom catalogues versions of the hypothesis where there are reasons for S to resemble U to varying degrees, but the existence of the logical possibility that no reasoning about S can tell us anything about U is enough to undermine any scheme to allocate meaningful probabilities to any version of the hypothesis.

If we decide instead to quantify the probability that *our own descendants* will run various classes of simulation, that's a different matter, but rather than being intrinsically meaningless the numbers are then merely so subjective as to be useless. Like the Drake equation, maybe it's worth writing down some of the relevant quantities initially, but once you come clean about the margins for error in some of them, you need to admit that the information content you've ended up with is essentially no different than saying "well, it's not impossible".

(2) Bostrom suggests that we must accept that either [a] civilisations will be rare and short-lived, [b] there is some reason why simulations will never take place, or [c] we are highly likely to be a simulation. He is enthusiastic in presenting arguments that seem to raise the likelihood of simulations taking place, but he doesn't pay sufficient attention to the distinction between simulations *in general* taking place, and simulations of the universe as we have experienced it taking place.

There are good reasons to believe that any advanced technological civilisation will run simulations *of some kind*, and it's a fact that we already simulate many interesting processes ourself: weather, population growth, the spread of pathogens, the formation of structure in the universe, etc. And if we accept as unsettled either way the question of whether computers can support consciousness, we can give that hypothesis the benefit of the doubt and allow for the *logical* possibility that simulations will include conscious beings. (This is, however, a step that is enough to destroy any meaningful allocation of probabilities. All we have on this issue are opinions.)

OK, so it's logically possible that simulations will include conscious beings, but let's look more closely at the detailed character of our own experience.

(i) Our history encompasses an enormous amount of suffering, both by humans and animals. Some of this is due to human actions, some due to "natural causes". Intelligent beings — and most of all, intelligent beings whose ancestors we closely resemble — are highly likely to have anticipated this. Our own technological cultures are moving towards an attitude where animal experimentation, factory farming, etc. are becoming less acceptable, and (if it was accepted that beings in a simulation were conscious), the idea of a simulation that included Auschwitz, Darfur, or a few hundred thousand years of brutal evolutionary history would be utterly repugnant to most people. [Of course attempts to argue anything from our own experience are ultimately futile, but if Bostrom wants to do so in one direction, he ought really to do it in both.]

(ii) We appear to occupy a universe with a very detailed physical structure at all scales, and hence very large computing requirements. Someone interested solely in, say, our evolution or social history could easily have contrived a different cosmology and fundamental physics in order to lessen the computational burden, and though it's *conceivable* that we are being selectively fed high-resolution information that does not exist when it's not required, maintaining consistency while "cheating" would itself be computationally very difficult. And although we face the hurdle of having no knowledge of what counts as "an impractically large computer" outside the simulation, we can still note the unambiguous *inefficiency* of the process. *Sim City* does not employ lattice QCD, and however large our computers get, I suspect that it never will.

(iii) Even our own civilisation, with its limited technological and intellectual achievements, is perfectly capable of reaching useful conclusions about evolution, history, psychology, cosmology, etc., with software and other means that fall far short of simulating conscious beings. Rather than needing or wanting to simulate a complete universe, it's likely that a more advanced civilisation would become more skilled at making intelligent computations that answer specific questions of interest *efficiently*. We're asked to contemplate a civilisation with the kind of technology and resources such that uploading their own minds would be commonplace, and (certainly by the time they were capable of simulating even the Earth, let alone our whole universe) they would be likely to have a very detailed theoretical understanding of the computational basis of conscious experience. This would equip them with a host of strategies for avoiding any need to simulate us to the level where we experienced anything, whatever psychological questions they happened to be interested in. In combination with the issues of morality and efficiency, this leaves very little *motivation* for something like our universe to be created as a simulation.

Pro-Bostrom commentators will have a host of "Yes, but maybe …" reasons why it's possible that my objections don't apply. To which my reply is, "Yes, but so what?" I happily concede that *it is not completely logically impossible* that we are living in a simulation … but everyone knew that all along. Bostrom has contributed nothing but a few unoriginal and highly selective arguments, along with some false dichotomies and a strategy for deluding ourselves that we can allocate probabilities to these scenarios.

So, a few dim reporters have mistaken Bostrom's waffling for a startling breakthrough in Bayesian anthropic reasoning, but if Peter Woit or anyone else thinks this is all worth no more than a despairing groan — and a complaint that it is part of a deplorable general trend — that's entirely reasonable. I've spelled out some of my own reasons why I think a groan is the appropriate response, but there's nothing intellectually wrong with just emitting the groan. Life is too short for anyone to be compelled to refute every scrap of useless nonsense at length, but that should not preclude them from calling it nonsense. Of course that judgement will sometimes be spectacularly wrong — we can all pick historical examples — but we're each still entitled to prioritise our expenditure of intellectual energy.

52. **Yatima**
August 22, 2007

This must be what is traditionally called an "epic thread".

53. **ks**
August 22, 2007

He [Bostrom] invites us, inappropriately, to quantify the probability that we are living in a simulation.

"You must wager; it is not optional… Let us weigh the gain and the loss in wagering that God exists… If you gain, you gain all; if you lose, you lose nothing. Wager, then, without hesitation, that He exists."

Blaise Pascal

54. **Greg Egan**
August 22, 2007

> If you gain, you gain all; if you lose, you lose nothing.

Did Pascal notice that there were religions in the world other than Christianity, some with gods that were *much* more irritable than the Christian god if you didn't do a great deal more than merely believe in them? Maybe it just wasn't wise to mention that quandary in his particular religious milieu. Perhaps the Future of Humanity Institute can come to the rescue and tell us the odds for being punished or rewarded for Aztec-style human sacrifice; I'm sure someone there has a rigorous Bayesian argument about this.

Hey, what am I doing giving that idea away for free … if I can interest Columbia University Press in my book *Blood Sacrifice or Holy Communion: Can the Hidden Dimensions of Physics Help Us Decide?* I might be up for the Templeton Prize!

55. **Allan**
August 22, 2007

From the classical paradigm
"God the Ultimate Clockmaker"
(Newtonian Physics)
to the contemporary paradigm
"God the Ultimate Video-Game Player"
what will be the next?

(There will be one, I'm just sorry I won't live to see what it is)

56. **ks**
August 24, 2007

At least Pascal showed some manifest interest in different cultures and their relevance towards his own discourse. Unfortunately I didn't find good links for further examination.

About book titles:

Blood Sacrifice or Holy Communion: Can the Hidden Dimensions of Physics Help Us Decide?

Go for it. I'd also like:

Lords of other worlds. What hidden dimensions can unveil about them.

57. **John Morales**
August 25, 2007

I fing Greg Egan's comments very interesting, as I recall reading various stories by him regarding this very concept. Years ago.

Well-thought out and very good stories, too, that made me think.

58. **Gphillip**
August 29, 2007

This is why we need more experimentalists. Left unto themselves, theorists will contemplate their navels so deeply that they can never find their way out. Please hurry with new data before our best and brightest are lost forever.

The Usual

August 20, 2007
Categories: Uncategorized

Blah, blah, more anthropic pseudo-science on hep-th, blah, blah, blah,

> On the basis of a static support condition depending on the tensile strength of flesh rather than bone, it is reasoned here that our size should be subject to a limit inversely proportional to the terrestrial gravitation field g, which is itself found to be proportional (with a factor given by the 5/2 power of the fine structure constant) to the gravitational coupling constant. The upshot, via the (strong) anthropic principle, is that the need for big brains may be what explains the weakness of gravity.

blah, blah, blah, this pseudo-science is on hep-th because of blah, blah, blah.

Blah, blah, blah written for Templeton-funded conference, blah, blah, Science-Religion Interaction in the 21st Century. Usual blah, blah, turn science into religion, blah, blah Institute for Interdisciplinary Research in Science, Philosophy and Religion.

Apologies for the repetitive nature of some recent postings. I can't even stand to write them any more, but still think someone should be documenting the descent of particle theory into pseudo-science and complaining about it.

Comments

1. **reader**
   August 21, 2007

   well if you don't like it, just ignore it. or read about future influences on the LHC on hep-ph.

2. **Steve**
   August 21, 2007

   The abstract here looks mostly OK because it is based on the *weak* form of the anthropic principle. This is good but controversial science because it advocates using *all* of the data that are available to the observer, rather than ignoring part of the data (i.e. the existence of the observer). Surely, any objection to this paper has to be with the details of the reasoning done with the weak anthropic framework, rather than with the weak anthropic framework itself? Perhaps that is what you intended to convey in your posting, but I nevertheless get the impression that anthropic reasoning is all no-go territory for you.

   Glancing through the paper, I notice that the *strong* anthropic principle used in the last section is *not* the usual intelligent-designer version of the SAP (which I hasten to add I do *not* support). It's a pity that the author has overloaded the definition of "strong" in this way.

3. **DB**
   August 21, 2007

   At least you have the guts to stand out from the complacent majority of physicists who appear to believe their subject is so inherently strong that it can withstand any and all assaults from pseudo-scientists. The fact that you are a mathematician, and no longer a practicing physicist, makes it even more galling.
   I believe they are awaiting the LHC to scatter these parasites and restore dignity and respect to the field.
   In the main, I think they will be proven right, and the period between the LEP discoveries underpinning the Standard Model in the 1980s and the LHC will be seen as a dark period in particle physics, vividly illustrating the primacy of experiment as the touchstone for progress in physics.
   However, their continuing silence, and refusal to defend their field – relying only on their faith in experiment, has resulted in the rest of physics being tarnished by association, with many prospective students taking one look at this field, at the famous institutions which now lend their name to this rubbish in return for money, at its debased coverage in the popular and scientific press, and run for the hills.
   So yes, they will probably be vindicated. But it is likely to be a Pyrrhic victory.

4. **alex**
   August 21, 2007

   In your most recent posts you are starting to sound a bit unhinged, you should consider taking a break perhaps?

   I am reminded of The reference Frame during some of its wobblier moments – the content is quite different of course, but the feeling of suddenly lurching off into a somewhat deranged rant is similar.

5. **Peter Woit**
   August 21, 2007

   alex,

   Thanks, you've got a point. I'm considering joining together with Lubos to form a united front denouncing and battling the "enemies of science".

6. **gunpowder&noodles**
   August 21, 2007

   You get eastern Poland, Lubos gets the western bit.

7. **Arun**
   August 21, 2007

   The upshot, via the (strong) anthropic principle, is that the need for big brains may be what ***explains*** *the weakness of gravity.* (from the abstract, emphasis added).

   Maybe a useful thing to do in these circumstances is to spell out (yet again) what constitutes a scientific explanation.

   e.g., the theorem (strong gravity) + (some additional assumptions) implies (no big brains) added to (we have big brains) means that gravity cannot be strong, but does not constitute an explanation, any more than the observation of big bang Helium abundance is consistent only with three light generations does not constitute an explanation of three generations.

8. **Fritz**
August 21, 2007

I wonder what's the point of the "referee" system, and the "endorsers", in arXiv, if they are allowing this kind of BS to be posted, and to make things worse in a completely wrong section of arXiv. By contrast, Nielsen's and Nanomiya's BS about the LHC was remarkably on-topic 🙂

9. **Mainland**
August 21, 2007

If you are weary, you could ask Carlo Rovelli to guest-post for a few days, more or less same way Chad has asked Aaron. He writes well and enjoys discussion.

If Rovelli didn't have time to stand in for you, he might suggest someone.

10. **Michael T**
August 21, 2007

I think you protest too much.

11. **Anders R**
August 21, 2007

i've gotten the impression that sting theory (which apparently isn't a complete theory) is so "open" that if something new turns up that apparently contradicts some of the more popular properties of a bunch of string theories, some dude can just make up a new string theory that takes this new stuff into account. i'm studying biology so i'm not a good judge of this but is there in any way possible to falsify string theory with something that turns up in the large hadron collider?

12. **Peter Woit**
August 21, 2007

Anders,

No, there is no known way to falsify string theory with possible results from the LHC. And this is both off the topic, and something that has been discussed endlessly here already.

13. **Anders R**
August 21, 2007

i have another question i'd like to ask though, even if it's off topic because i don't know where to post it otherwise. since the publication of lee smolins book and your book, have you noticed any significant shift in which people are getting those jobs that i think at least lee talked a lot about. apparently it was very difficult, back then anyway, for a young theoretical physicist who wanted to get a career in the theoretical physics field if they didn't work on string theory. perhaps all this debate has manage to achieve a positive change in how resources are being distributed within the field?

14. **Peter Woit**
August 21, 2007

Anders,

I don't know if all this has helped the job prospects in the area Lee is most concerned about, LQG. That's an interesting question, but the numbers of jobs each year are so small it would be hard to see a trend in any case.

In particle theory, string theorists still seem to be getting hired at about the same rate as before. There's a general trend in physics departments that they would like to move out of particle theory, and get into cosmology.

As for the kind of mathematical approaches to quantum field theory I think are most promising, there's interesting things going on in mathematics departments, nothing much at all in physics departments.

15. **Kea**
August 21, 2007

Brandon Carter? No way! These guys must be posting these papers as a **joke** to show up the arxiv for what it has become.

16. **D R Lunsford**
August 21, 2007

Perhaps the worst thing about that Carter paper was his overweening dismissal of Dirac's argument, as if he understood anything Dirac did. What a mess. I still can't believe this is happening. This entire scenario needs to be exposed by a deep historical analysis of what went wrong in academia. I blame the NSF.

I was listening to an interview with Paul Fussell, the historian. He regrets the loss of literacy, not in the sense of reading skill, rather, of being familiar with literature and having a broad perspective because of it. It's much the same way in science. Who is there to talk to? When I meet new people, they often ask me to explain things to them that I can't even think about without getting pissed. That is truly the worst part of all this.

-drl

17. **Kris Krogh**
August 21, 2007

I think Brandon Carter means it, Kea. Looking at his papers on ArXiv, there's another titled "Micro-Anthropic Principle for Quantum Theory." Pretty sad…

18. **Tony Smith**
August 21, 2007

D R Lunsford said "… What a mess. I still can't believe this is happening. This entire scenario needs to be exposed by a deep historical analysis of what went wrong in academia. …".

Over on Sean Carroll's blog Count Iblis said that in the hep-th paper 0708.2743 Albrecht and Inglesias (of UC Davis)

"… point out that by messing with time you can map a particular set of laws of physics to any other laws of physics. …".

In their paper, Albrecht and Inglesias say:

"… We are used to doing physics by stating the physical laws which we believe may be true, and then calculating predictions based on those laws in order to test them against observations of the physical world.

The clock ambiguity appears to completely undermine this approach to physics. …

This work was supported in part by DOE Grant DE-FG03-91ER40674 …".

Therefore, my tax money is being used by DOE to fund a paper saying that efforts to do physics by:

1 – constructing physical-law models
2 – and calculating predictions based on those laws
3 – in order to test them against observations

is "completely undermine[d]"

Although they do have some fine print (in the body of the paper but not in its abstract) involving "the continua we use to construct theories of fundamental physics" and their use of "freedom to choose a clock subsystem arbitrarily" and "use of the covariant approach",
it seems clear to me that an attack on the scientific methods used by scientists from Kepler to Feynman as being "completely undermined" is
the basic thrust and purpose of their paper.

I think that it is a shame that their attack is accepted by the Cornell arXiv as OK for hep-th, while I am blacklisted from posting new results of my physical-law model which allows computation of particle masses, force constants, etc, that are testable against observations.

Tony Smith

PS – Albrecht and Inglesias are not alone in attacking the Kepler-Feynman way of doing physics. The Resonaances blog in a 1 July 2007 post entitled "Nima's Marmoset" said:
"… Nima Arkani-Hamed [formerly at Harvard and now at Princeton IAS]… gave another talk …[at]… CERN … advertising his MARMOSET … a new tool for reconstructing the fundamental theory from the LHC data … Nima pointed out …[that]… at the dawn of the LHC era we have little idea which underlying theory and which lagrangian will turn out relevant …".
In short,
Arkani-Hamed says that the Standard Model Lagrangian should be ignored because
"… we have little idea which underlying theory and which lagrangian will turn out relevant …"
even though the Standard Model has passed EVERY experimental test for over 30 years, and there is NO experimental observation whatsoever indicating that the Standard Model is not the relevant "… underlying theory and … lagrangian …" for physics at the LHC.
So,
I would add Harvard and Princeton IAS to the list of institutions that should hang their heads in shame
for
supporting attacks on the process of building physics models in the old-fashioned way of requiring inclusion of the Standard Model and Gravity as subsets and demanding calculability of observable quantities such as particle masses, force strength constants, etc.

19. **Jimbo**
August 22, 2007

Hey Reader,

Anybody who thinx Nielsen's paper on future influences at the LHC is `off the wall' needs to read John Cramer's novel `Einstein's Bridge' to realize, as always, SciFi precedes Science. Some of its icons, Verne, Wells, Asimov, Hogan, Cramer, Benford, etc., have received advanced waves from the future, & distilled their dreams into literary visions !

Seriously, Peter, the summer has a way of exhausting one's tolerance for BS…Maybe U should take a vacation, & just chill out for a while. We all love U & support U, but burnout is a real bummer.

20. **Harry**
August 22, 2007

Peter W.,

Maybe you could create a categorie for your posts about anthropic pseudo-science (as the one you made for your book).
Just a thought.

21. **amused**
August 22, 2007

I think the time to really start getting worried will be when papers like this begin to appear in PRL. Hopefully it will never come to that… Actually I'm very happy to see papers like this on hep-th, it will hopefully help to kill the idea that the arxives make journals redundant. The powers that be in the hep-th field would like nothing better than for that idea to take hold, so that journals such as PRL stop mattering. Then they won't have to care about whether their research (anthropic landscape or whatever) is considered interesting and important by the broader physics community, and they won't have the embarrassment of having the value of their work measured against the value of work on other topics. Moreover, hiring decisions will be based solely their say-so without any annoying semi-objective measures to consider or irritating questions along the lines of "if your student/postdoc is so great, why didn't he/she prove it by publishing a few papers in PRL?". Absence of any form of quality measure besides their own opinions would suit the powers that be very nicely.
(Sorry, couldn't resist jumping on my hobbyhorse once again. I'll dismount now.)

22. **Tony Smith**
August 22, 2007

My apoologies for misspelling the name "Iglesias" in my comment here.

Tony Smith

23. **european observer**
August 22, 2007

Tony,

I am confused by your comments relating to LHC physics and the work the marmoset people did? Where do you believe that they said to forget the SM? The starting point of the entire project as I understand is to get at SM+X, and what is X. With that in mind I think you are misreading the Lagrangian comment, and it should be interpreted as the beyond the SM theory/lagrangian that is unknown. I'd just venture to be more polite and not make grandiose statements about institutions based on remarks in an anonymous blog.

24. **Peter Woit**
August 22, 2007

Tony,

Please don't post such far off-topic comments. Marmoset has nothing to do with the posting, and a tendentious discussion of it here is way out of place.

25. **Bee**
August 22, 2007

sure you can falsify string theory at the LHC. you just need to verify the existence of more than 7 extra dimensions. probability for this to happen I'd estimate to be about 10^-500

(see e.g. [hep-ph/0503178](hep-ph/0503178))

but yes, that would be just string-theory-as-we-understand-it-today.

26. **Who**
August 22, 2007

Hi Bee,

> sure you can falsify string theory at the LHC. you just need to verify the existence of more than 7 extra dimensions.

I think maybe you can falsify the usual versions of LQG already if LHC can show the existence of even ONE extra dimension. But I would not estimate odds.

> but yes, that would be just string-theory-as-we-understand-it-today.

Perhaps string-theory-as-we-understand-it-today could handle being falsified simply by wearing a pair of falsies. It could just put on two more styrofoam compact extra dimensions and be falsified but proud.

27. **woit**
August 22, 2007

Bee,

Surely you don't think string theorists are not capable of coming up with models with more than 11 dimensions?

28. **Jim Clarage**
August 22, 2007

As a biophysicist I find it telling that anthropic arguments are used by HEPs and not BIOs.

So called "fundamental" quantum physics cannot actually predict the existence of even the simplest life forms (e.g., bacteria) let alone the existence of large-brain organisms. The proposition:

Standard Model –> bacteria

cannot be demonstrated theoretically– only experimentally.

Note: what I am saying is not equivalent to declaring that life is somehow mysterious or supernatural or designed. I am simply making an epistemological point, not a theological one.

In this sense HEP does not make testable predictions about biological experiments. Which is why my inner-biologist finds these papers Peter is exposing so disturbing.

Other "softer" branches of science of course can and do make such predictions. For instance one not-so-past prediction of biology was the proposition:

Life –> (sunlight) + (oxygen) + (temperatures in range 15-25 C)

It was falsified with the discovery of thermophilic bacteria. These organisms can live even with the negation of *all three* of the above variables. It was a shock to biological sciences.

Thus the folly of anthropic-HEP theoretically ruling in or out various HEP theories (or regions of the landscape) based upon presently understood terrestrial data for biochemical and biophysical variables.

Put another way– sans the pesky biology training– there is a crucial Aristotelean logical difference between:

Large Brain –> Observers

and

Observers –> Large Brains

Anthropic reasoning tends to conflate these two. We (biologists, physicists, any scientists) simply do not know, either via prediction or observation, what constitutes the appropriate measures for sentient/observant life. To think we do, and furthermore to think that measure is *Man* would actually make Narcissus and even most post 1200's theologians blush. And in this sense it gives even (modern) religion a bad name.

Think about it, with your large brains.

29. **Bee**
August 22, 2007

Hi Peter:

I have high respect for string theorist's inventiveness and therefore believe they might indeed be able to explain more dimensions.

Hi Who:

What about LQG do you think is specific for 4 dimension? As far as I know the setup does not exclude/support any specific number of dimensions? Best,

B.

30. **D R Lunsford**
August 22, 2007

Bee,

This isn't poetry or music. Inventiveness is not needed. Insight is needed. Your post makes me realize how hopeless it all is. This simplest truths are forgotten.

-drl

31. **Who**
August 22, 2007

> Hi Who:
>
> What about LQG do you think is specific for 4 dimension? As far as I know the setup does not exclude/support any specific number of dimensions? Best,
>
> B.

I think you are right if you mean canonical LQG as defined in the 1990s or whenever—the setup could be done in any d. But what comes to mind when I think of the research speakers at Loops 07 were actually talking about is mostly specific to dimension.
Rovelli spinfoam dynamics is d = 4. Smolin braided networks would come unbraided if embedded in one higher spatial dimension.
For Ambjorn it is a big production to jack up from 3D to 4D. CDT is different in each dimension and must be reformulated. Although not at the conference, for Freidel going from 3D with matter to 4D with matter means introducing new mathematical tools (2-groups I think maybe).

So in the actual work that people are doing, I think the step from 3D up to 4D is seldom trivial, if ever. The theories and models they are actually developing would, I think, be instantly out the window if tomorrow we saw more spatial dimensionality.

On the other hand I have to defer to your view of the field since it's your business. BTW I thought your Loops 07 talk was excellent—I'm glad the slides and audio are online.

32. **Yet another grad student**
August 29, 2007

Dear Peter,

This is just my opinion, but I think it might be more useful to take time to help graduate students in Cosmology (theory and phenomenology, sans string theory) by explaining the current status of particle physics to them. A nice introductory text on QFT (I know many exist, but are they all that well-written?) might also be in order.

33. **Peter Woit**
August 29, 2007

Yet another grad student,

For the current status of particle physics, there's always my book…
But for something that actually explains things in detail, I've looked at most of the QFT books. Zee is good but way too sketchy for most people, Peskin and Schroeder is a standard for good reasons. I liked Ramond's book quite a bit.

But I suspect students in cosmology would find more useful not a hard-core QFT book, but one devoted to the phenomenology of the Standard Model. I've seen such books out there, but never looked at them closely enough to recommend any one in particular. Maybe other people have recommendations..

34. **AGeek**
August 29, 2007

http://www.amazon.com/Gauge-Theory-elementary-particle-physics/dp/0198519613

After all these years, still hard to beat. Which says something about the status of the subject. 🙁

Hidden Dimensions

August 22, 2007
Categories: Book Reviews

At lunchtime today I stopped by the excellent local bookstore Labyrinth Books, looking to see what was new. In the science section, I noticed a pile of copies of Hidden Dimensions: The Unification of Physics and Consciousness. As with the rest of the many "physics and consciousness" books I've seen over the years, I spent a few minutes looking at it to see if there was any evidence of something different or interesting about this one. Apparently not, so I was about to file it in the large category of things best ignored, when I decided to check to see who had published the book.

I was shocked and dismayed to see that the publisher is Columbia University Press, where the book is part of the Columbia Series in Science and Religion. Two of the other eight books in the series are by the same author, B. Allan Wallace, including one entitled Buddhism and Science: Breaking New Ground. In defense of Columbia University Press, the people there don't actually seem to be reading these books or their promotional material for them, since the blurbs for *Buddhism and Science* at the CUP site and on Amazon include

> " fascinating and captivating book. Without a doubt it will be the definitive text on Holbein's famous painting for some time to come."
> —Aparna Sharma, Leonardo Reviews

which comes from a review of The Ambassador's Secret: Holbein and the World of the Renaissance, which just happened to be in the same issue as a review of *Buddhism and Science*.

Wallace's background in physics consists of an undergraduate joint major in physics and philosophy of science at Amherst. He's the author of many other books, including some on Buddhism and physics such as Choosing Reality: A Buddhist View of Physics and the Mind. He has a web-site here and is founder and president of the Santa Barbara Institute for Consciousness Studies.

Here and here you can read some samples of *Hidden Dimensions*, and make up your own mind what you think. As far as I can tell it's pretty generic material of this kind, full of crackpottery invoking quantum mechanics, extra dimensions, etc. etc. It's more or less in the same vein as What the Bleep, but with more of a Buddhist and less of a self-help angle.

Unfortunately, it's not just Columbia University Press that is promoting Wallace's ideas. He also gave the keynote address at a symposium here last year on Mind and Reality. You can watch an interview with him standing not too far from my office here.

I really was intending to avoid writing this kind of critical blog posting for a while. After enraging lots of philosophers, I fear that now I'll enrage lots of Buddhists, in particular by having no interest in wasting time discussing Wallace's ideas. But I'm profoundly embarrassed that the institution where I work is promoting this sort of thing, so thought I better publicly say so. This all appear to be the responsibility of the Center for the Study of Science and Religion, which has recently been made part of the Earth Institute, run here at Columbia by economist Jeffrey Sachs. Like pretty much all of the many institutions out there devoted to bringing science and religion together, it has received funding from the Templeton Foundation.

Comments

1. **DB**
   August 22, 2007

   Richard Dawkins, in "The God Delusion" has an interesting section on how he experienced at first hand the Templeton Foundation methods. He reports having been the one "token atheist" (his description) out of ten speakers invited to speak at a Templeton run conference, only to find out later via John Horgan's investigations, that many of the other speakers had been paid 15,000$ to attend.
   As you may know, he is highly critical of the Foundation.

2. **Chris Oakley**
   August 22, 2007

   If there's anyone from the Templeton Foundation reading this, I would like them to know that I, too, can be bought. For a mere $1,000 I am prepared to add the following paragraph to the end of a paper I am currently writing about Quantum Electrodynamics:

   "For all I know there may be a connection between all of this and religion, although as yet I am unsure as to what it is."

   C. G. Oakley M.A., D. Phil. (Oxon.)

3. **Michael Bacon**
   August 22, 2007

   Actually, you probably won't "enrage" lots of Buddhists. That would be tough to do. Most should just shake their heads and smile knowingly . . . .

4. **milkshake**
   August 22, 2007

   Templeton foundation is not evil – some years ago they gave award to Freeman Dyson, and its probably the best thing they could have done with their money.

   (When Dyson writes in the popular books that consciousness is perhaps on some mysterious level involved in quantum mechanics and that "the universe knew we were coming", he is not annoying because he separates the facts from his somewhat mystic personal beliefs. He presents his beliefs as such, as hopes and dreams.)

   When I was in the highschool in mid 80s, I ran across a discussion in a pop science magazine about antropic principle and multiverse and apparent fine-tuning, Dyson being opposed by a biologist S.J. Gould – and I thought it was a very exciting stuff! (Gould made a very strong point against the fine-tuning argument then.)

5. **Coin**
   August 22, 2007

   The Templeton Foundation is tough to get a handle on. They're funding everything from pure religious apologia to pseudoscientific nonsense to borderline Intelligent Design to *both sides* of this little Woit, Smolin and Co. Versus String Theory war, with grants going to both Tegmark-style anthropicists and several people at the Perimeter Institute.

As far as I can tell they are legitimately well-meaning and are in fact generally honest about what they are and what they're doing, just occasionally very very confused…

6. **Peter Woit**
   August 22, 2007

   Coin,

   I don't doubt that the people at Templeton are well-meaning, and some of what they fund is worthwhile. The problem is that they have a lot of money, and one of their main goals is to bring religion and science together, blurring the distinction between the two, and effectively often promoting pseudo-science. I doubt they know or care much about string theory, but they're quite fond of the anthropic principle. It fits in well with the vaguely religious world-view that puts human beings and concern with the "purpose" of the universe at the center of things.

   Anyway, sure they're a mixed bag, but they're putting a lot of resources behind the promotion of a point of view that I happen to think is dangerous for science.

7. **anonymous**
   August 22, 2007

   I'm no fan of Templeton, but I'd gladly take their money, provided it was offered with no strings attached.

8. **Al T**
   August 22, 2007

   Well there is Spong's Law of Theophysical Asininity, which states:
   Whenever a person appeals to quantum physics as the basis for a theological or religious principle, he is making an ass of himself.

9. **Coin**
   August 22, 2007

   The problem is that they have a lot of money, and one of their main goals is to bring religion and science together, blurring the distinction between the two… sure they're a mixed bag, but they're putting a lot of resources behind the promotion of a point of view that I happen to think is dangerous for science.

   That is a completely reasonable perspective.

10. **dir**
    August 23, 2007

    religion is just a personal belief, so its content should stay in somewhere science does not reach and care

11. **Haelfix**
    August 23, 2007

    Big institutions like Templeton have so much money, they usually have difficulty finding the best ways to spend it. So they fund *everything*.

    It boils down to a bunch of people in a room on a deadline with a lot of cash and likely horribly vague guidelines on what to spend it on. Worse, its likely many of those people/interns don't even have an education in physics, so they have difficulty sorting through the forest of absurdities out there.

    I find it difficult to assign to much blame to that, after having gone through the sheer horror of grant writing to anything other than the NSF. People literally have absolutely no idea what we do.

12. **Olivier**
    August 23, 2007

    It seems that lot of scientists, pretending represent "pure science" are very upset, to say the least, by people trying to understand the universe, not only describe it. Lot of them have strong "rationalistic" and "atheistic" views, and they use science for promoting their ideas. The best example is Dawkins, which supposedly based only on "facts", is doing in fact atheistic propaganda, for which he has no other arguments than his personal opinion. Science by itself is mere description : it cannot say anything else than what directly appears to our eyes. It's obvious that conclusions on universe and life will use other arguments than spectral radiation shifs or temperatures.

13. **Low Math, Meekly Interacting**
    August 23, 2007

    Haven't been here for a while.

    All I can say is that I share the dismay at the mere notion of a program to promote the "convergence" of scientific and spiritual understanding. I'm not one of these reflexive religion haters that pollute the blogosphere with bigotry and invective, but everyone has a line that they must draw, even if it necessarily offends some of those on the other side. I should think at absolute minimum that line must be drawn at what is by its very definition a variety of experience inscrutable by any material means known or in any way conceivably or inconceivably possible. These are fundamentally incompatible "ways of knowing", without even coming near the issue of whether or not one way or the other is to any degree accurate or not.

    I can't even come up with a real-world analogy to compare the cognitive dissonance that must be required to reconcile the twain by any means. I don't think there exists a more patently impossible epistemological enterprise. That it claims even a modicum of legitimacy through anthropic physics is the aspect that is the most…what's the word I want…terrifying? Yes, terrifying. We laugh at things like "What the Bleep Do We Know", but if there's some critical mass being approached, stuff like this isn't funny, if it's being accepted by well-motivated skeptics who nonetheless defer to the expertise of the better-credentialed.

14. **JJD**
    August 23, 2007

    Please rest assured that many of us Buddhists are as skeptical as you about the current fad of "explaining" so-called Buddhism in terms of so-called science or vice versa. On the one hand, consciousness is explained with quantum theory and extra dimensions (still waiting to hear how dark matter is involved), and on the other hand isolated and mis-characterized elements of Buddhist practice (mindfulness and meditation) are currently fashionable in mental health clinics. Superstition, quackery, and incompetence are easy to find in either domain, and they are the real problem, not just the silly instances where a scientific quack tries to explain personal identity or a religious quack tries to explain space-time.

    Good science and good mathematics are based on principles and practical discipline which have taken thousands of years to work out. It seems all too easy to get an undergraduate degree or even a PhD in science or math without a firm grasp on those principles and that discipline. This is part of why your blog is needed.

15. **Molnar**
August 23, 2007

The Buddhism angle is all wrong. Physics actually proves Christianity. Or maybe Islam. Definitely not Buddhism.

16. **srp**
August 27, 2007

Low Math needs to read up on basic history of science. Kepler, Newton, and many of the physicists of the Enlightenment believed that their work elucidated either "the mind of God" or the details of God's handiwork. We atheists have no monopoly on scientific curiousity.

17. **Dan**
August 27, 2007

I, too, have laughed at the hand-waving which seems to occur in many popular books linking quantum physics with mysticism. Yet, I'm not sure exactly what your problem was with Wallace's book, given that you provided no more than blanket statement regarding the absurdity of even trying to reconcile physics and consciousness. I don't know much about the Templeton Foundation (beyond what was said here), but as a number of your commentators have noted, if someone likes what you're doing and they want to support you, you don't turn them down without a good reason.

As for Wallace, he appears to have a rather different aim from the average, feel-good, pop physics/mysticism authors. Specifically, he seems not to be critiquing science qua the attempt to gain understanding of the universe through rigorous, reproducible methodologies. Rather, he is critiquing scientists who dogmatically hold philosophical positions without any empirical basis. To quote from the selection you linked to:

"All subjective experiences, including consciousness itself, remain invisible to objective scientific observation. A growing number of scientists and philosophers of mind believe they have the solution: simply declare that conscious states are equivalent to their neurophysiological
correlates or to higher-level features of the brain. The physical processes in the brain that are equated with mental processes are believed to have a dual aspect: they are physically measurable processes, consisting of ordinary electrochemical events of a kind quite familiar to
physicists and chemists, but somehow, inexplicably, they are also subjective
experiences. The rationale for this quasi-dualistic position is that mental phenomena appear to be nonphysical, but this appearance is misleading,
for they are realized as neural events, which are their essential nature.

"It is as if mental phenomena, despite their undeniably subjective, nonphysical appearance, are being granted admittance into the world of nature by being equated with well-understood physical phenomena. Scientists have yet to identify the neural correlates of consciousness, so no one even knows yet what those hypothetical neural processes with a dual identity might be."

Perhaps, then, you find fault with his future research plan. For he seems in the other selection to suggest that there exist methodologies within the contemplative traditions, especially Buddhism, which might shed light on precisely these phenomena (consciousness, subjective etc.).

Why is that a problem? If the methods are rigorously noted, and the conclusions arrived at can be verified (or not) by anyone else who follows out the methodology, I'm not sure why that would be a problem, beyond the fact that those conclusions would not be accessible to everyone regardless of training.

Wallace specifically singles out mathematics as having a formal similarity to the program he is suggesting (I might single out Brouwer's intuitionism):

"[T]he practice of higher mathematics takes place within the mind of the mathematician and is then communicated to other mathematicians. Writing equations on a chalkboard is simply a kind of public behavior that may or may not result from the internal process of understanding proofs and devising theorems. A mathematically uneducated person may be taught how to write down the same equations, but when subjected to interrogation by a qualifi ed mathematician,
will clearly not understand what he has written. Mathematicians do commonly converse among themselves in a kind of language that is
unintelligible to nonmathematicians, and the same is true of experts in all fields of science. So there is no reason in principle that researchers could not receive professional training in observing mental phenomena and learn to communicate among themselves about their experiences. However, this is a major undertaking that neither philosophers nor cognitive scientists have yet tackled."

Similarly, if I present the radical conclusion of a new experiment, my results are not thrown out unless numerous independent researchers have attempted my experiment, using my methodology, and have failed to get the same results.

If a rigorous set of methods existed for investigating consciousness and subjective experience, what would be the problem with using them? On what grounds could we throw out the results?

Perhaps I have failed to meet your criticism. Or perhaps you merely meant to offer up a baseless ad hominem. Perhaps, however, your position is worth reconsidering. And, without question, my position is worth reconsidering. Any suggestions as to where I could start?

18. **Peter Woit**
August 27, 2007

Dan,

My criticism of the Wallace book has nothing to do with the issues you bring up, but with his specific claims about physics, especially things like quantum physics and extra dimensions. I read enough of the book (not just the chapters available on-line) to convince myself that the author was spouting sizable amounts of nonsense about physics. The world is full of similar nonsense, and it would be both a full-time job and a waste of ones life to argue the details of this with people.

19. **Mike**
August 28, 2007

Peter,
Are you not debating the details of extra-dimensional string theory? Was there some more specific claim that led to this conclusion?

I was considering buying this book until I read your review, now I don't know. I have read parts of Alan Wallace's participation with the Dalai Lama in the conference with David Finkelstein from Georgia Tech, Anton Zeilinger from the University of Vienna, Piet Hut from IAS, George Greenstein and Arthur Zajonc from Amherst; all well recognized academic physicists. Are they all engaged in "generic material of this kind, full of crackpottery"?? Is this the "similar nonsense" you're talking about? Or does Alan reference the "others" from the "Bleep"?

20. **Peter Woit**
August 28, 2007

Mike,

The reference to extra dimensions that I saw in Wallace were nonsense, those of string theorists are completely different.

The people you mention are a mixed bag, unlike Wallace they're trained physicists, but some of them are capable of engaging in similar nonsense from time to time. Again, for the last time, I consider this a complete waste of time, including examining and discussing the contributions to the Dalai Lama conference you reference. Please stop trying to discuss this here and find a venue run by someone who wants to engage in or encourage such discussion.

21. **Dan**
August 29, 2007

Hey Peter,

Sorry for wasting your time further, but I thought that you might appreciate having it pointed out that, while rhetoric often holds sway in the minds of masses, facts are the standard of science.

As you might have realized, I respect Wallace, for his genuine humility amongst other things. His work regarding science has seemed to me to be both rigorous and fair without being unequivocally laudatory. He certainly has an agenda, but he's honest and clear minded about it, and doesn't just try to sweeten up the "medicine" with a little quantum physics. So I was somewhat surprised to discover that a Columbia mathematician/physicist had thrown his latest work into the bin with "What the Bleep" and such junk.

However, I'm disappointed that you cannot bring up a single point at which he errs. Clearly, you have better things to do than write critical blog postings all day, so I don't expect you to answer this, but you did spend the time to write the initial post, presumably because, as you say, "I'm profoundly embarrassed that the institution where I work is promoting this sort of thing, so thought I better publicly say so."

Perhaps such a strong condemnation should just be ignored when the condemner can't even remember or enunciate his reasons for condemning. Whatever "this sort of thing" is, it seems fair to hypothesize that you simply don't know. Perhaps you might try to be a little less embarrassed in the future about things you don't know.

Or, perhaps you know, but you can't tell us because it's top-secret, classified information too sensitive for a public blog posting! Should we just take your opinions on authority, then? And while you're at it, where are those damned WMD's?

22. **Peter Woit**
August 29, 2007

Dan,

Sorry, but I don't find Wallace's book to demonstrate "genuine humility", but instead found it full of breath-taking arrogance. He announces that these "materialist" physicists have got quantum mechanics all wrong, while not understanding much at all about the subject himself.

I think that virtually any trained physicist you talk to will tell you the same thing, and has a similar reaction to the book as nonsense. Maybe you can find one of them who wants to waste his time going over the details of this nonsense. It's not going to be me.

23. **Dan**
August 29, 2007

Alright. Thanks for the reply, though. Guess I'll have to read it and see for myself.

24. **Louise Martinez**
September 6, 2007

Where Alan Wallace is coming from in my opinion, is from egocentricity. Perhaps it wasn't always that way. But if you have attended a retreat of his you will see how he is trying to impress you with how many teachers he has had, and his connection to His Holiness the Dalai Lama, which leaves one thinking on how he is banking on this relationship.

Nature on K-theory Controversy

August 22, 2007
Categories: Uncategorized

Nature has an article this week by Jenny Hogan about the K-theory journal situation reported on here, under the heading Strife proves hard to solve for K-theory. The article does show how real journalists do some things much better than bloggers, like calling up and interviewing the relevant people to sort out what happened. Hogan talked to Catriona Byrne, the mathematics editor at Springer, who claims that the managing editor of the journal, Anthony Bak, was sacked in January 2007, since he had stopped forwarding papers to them since April 2006. Also in January, Bak asked the editorial board to resign, which they did, although Byrne claims that Springer didn't find out about this until May.

Much of the controversy about this has to do with the question of how papers accepted after April 2006 were handled, with claims being made that some editors and authors were unaware that they were not being forwarded to Springer for publication. One of the editors who is unhappy about this is Eric Friedlander:

> Eric Friedlander at Northwestern University in Evanston, Illinois, a former editor at K-Theory, is in principle sympathetic to the switch. "There is a lot of concern in the mathematics community about the cost of journals," he says. But on 17 August, Friedlander wrote to Bak to say that despite being named as a member of the editorial board of the new journal, he was not willing to serve "because I cannot endorse the process by which you have withheld manuscripts submitted to K-Theory and proceeded without consultation with authors and the editorial board"…
> Friedlander is uncomfortable that papers were held up: "Our responsibility is to review mathematics that is submitted to us and disseminate it."

Update: For the perspective of one of the authors affected, see here.

Update: For a letter to the editors of K-theory from Matthias Kreck, see this comment.

Comments

1. **John Baez**
   August 23, 2007

   How do you know Catriona Byrne is the one who told Jenny Hogan that Bak had been sacked in January 2007? It's quite possible, but I don't see that it's certain — not from from my reading of the *Nature* article, anyway.

   It's a small detail — but Byrne works for Springer, so she's one of the interested parties in this dispute.

   I told you this thing was a can of worms… and I somehow doubt all the worms have wriggled free yet.

2. **Peter Woit**
   August 23, 2007

   John,

   You're right that it's just my assumption that Byrne either told Hogan that Bak had been sacked, or confirmed this if she had learned it elsewhere. If Hogan did learn this elsewhere, I can't imagine that she didn't at least try and get confirmation from Byrne. And she printed it as fact…

3. **John Baez**
   August 24, 2007

   Okay, thanks. I'm just trying to get the facts straight over at EUREKA.

Massive Plagiarism Scandal

August 23, 2007
Categories: Uncategorized

From Ars Mathematica I learned about an article at Ars Technica describing a scandal involving plagiarism of theoretical physics papers by about 20 different people, some of them students at the Middle East Technical University in Ankara. Many of the papers were refereed and published in well-known journals, and one made it into what is now perhaps the most well-known particle theory journal, the *Journal of High Energy Physics*.

> According to Dr. Sarioglu, two of the authors of this paper were graduate students with a prodigious track record of publication: over 40 papers in a 22-month span. Dr. Karasu, who sat on the panel that evaluated their oral exams, became suspicious when their knowledge of physics didn't appear to be consistent with this level of output. Discussions with Dr. Tekin revealed that the students also did not appear to possess the language skills necessary for this level of output in English-language journals (METU conducts its instruction in English).
>
> This caused these faculty members to go back and examine their publications in detail, at which point the plagiarism became clear. "All they had done was literally take big chunks of others' work using the 'copy and paste' technique," Dr. Sarioglu said, "steal from here and there to cook up an Intro which is basically the same stuff in all their manuscripts, carry out some really trivial calculations such as taking derivatives of some simple functions, and write up the results in the format of a paper." The department chair was informed and started an internal investigation; the university's Ethics Committee has since become involved.
>
> In the mean time, the faculty and administration at METU are attempting to do some damage control. The university's president personally sent a letter to the Journal of High Energy Physics requesting that the paper be withdrawn—a request that, as noted above, has yet to be acted upon. Meanwhile, the faculty members mentioned above are working with the arXiv administrators to ensure that any plagiarized work is removed.

The Ars Technica article emphasizes the role of the arXiv in this, since the plagiarized papers first appeared there and are still available there, although arXiv administrators have replaced the latest versions of the papers with a notation "withdrawn by arXiv administrators due to plagiarism". I don't actually think the arXiv is the real scandal here, rather the fact that refereeing standards in theoretical physics are now so low that obviously plagiarized papers don't seem to have much trouble getting into even the best journals in the field. Some of the other journals that published plagiarized papers from this same group of people include:

- *General Relativity and Gravitation* (here and here). The situation of the second of these is really confusing, since according to the arXiv it plagiarizes a paper by a completely different group in India, one that the arXiv lists as having "excessive overlap" with an earlier paper by the Turkish plagiarists.
- *Modern Physics Letters* (here and here)
- *International Journal of Modern Physics* (here, here, here, here and here)
- *International Journal of Theoretical Physics* (here, here and here)
- *Journal of Mathematical Physics* (here)
- *Progress in Theoretical Physics* (here)
- *Fortschritte der Physik* (here)
- *European Physics Journal* (here)
- *Foundations of Physics Letters* (here and here)
- *Chinese Physics Letters* (here and here)
- *Chinese Journal of Physics* (here)
- *Czech Journal of Physics* (here and here)
- *Fizika* (here)
- *Nuovo Cimento* (here)
- *Acta Physica Polonica* (here and here)
- *Acta Physica Slovaca* (here and here)
- *Pramana Journal of Physics* (here and here)
- *Astrophysics and Space Science* (here, here, here and here)

There are also other papers by some of the same authors which the arXiv does not list as plagiarized (published in *Nuclear Physics B*, here, *Classical and Quantum Gravity*, here , *International Journal of Modern Physics*, here and here) .

Remind me again, why is it that universities are paying large sums to get these journals?

Update: My guess is that most theorists are just going to ignore this and pretend it didn't happen. As far as I can tell, the journals involved haven't even bothered to add a notation to the articles still available on-line to note that they are plagiarisms, much less do anything to stop this from happening again. But at least Lubos agrees with me:

> The journals and arXiv are clearly flooded with papers that no one cares about which is why this thing can happen.

Comments

1. **Robert**
   August 23, 2007

   Try searching the arXiv for 'plagiarizes' in the abstract.

   For our master program, we had an applicant whose application material looked very promising but for whom a letter of recommendation said 'this is a good student, however, see [a search query to the arXiv]'. It turned out, as an undergraduate student (why on earth do you need publications at that stage?) in mathematics he had taken two papers from the arxiv and resubmitted them after just changing the title and of course the names of the authors.

   Now you could argue that math is healthier than HEP as this was discovered by one of the original authors but on the other hand it was simpler as beyond the title all the wording was kept (including the abstract).

   This is so stupid! Of course we did not accept this student for our program although we might have without this act of plagiarism.

2. **Rien**
   August 23, 2007

   There is a tool for looking for plagiarism of webpages called Copyscape. Maybe the arxiv should have something like that… or the journal editors.

3. **Rien**

August 23, 2007

Ah, it seems [they already have such a system]() at arXiv.

4. **woit**
   August 23, 2007

   Rien,

   Another idea would be to insist that refereeing be done by physicists who either know the literature of their field or are willing to look into it when they don't know it so well…

   This may be too difficult to achieve these days, but if that's the case, admitting this and shutting down these journals would be a good idea.

5. **Rien**
   August 23, 2007

   > Another idea would be to insist that refereeing be done by physicists who either know the literature of their field or are willing to look into it when they don't know it so well…

   What an unorthodox idea! 😉

   I would say this is more the fault of the journals than of the referees though. I sometimes get sent papers that are only very tangentially related to my own research, so what do you do then: send it back or try to cobble something together. I have done both.

   One would think that just going by the number of reserachers active in any given subfield, they should have managed to send at least one of those plagiarized papers for refereeing to one of the researchers that they plagiarized from…

6. **Amos Dettonville**
   August 23, 2007

   I was once reading some papers in the physics arXiv on a subject of interest to me, and came across what struck me as a particularly lucid and insightful paper from someone at a University in Portugal. The more I read, the more impressed I became… "Damn (I though), I couldn't have written this better myself…" And then it dawned on me: I HAD written it myself. The ENTIRE paper (about 12 pages) had simply been cut and pasted, verbatim, from an article of mine that is accessible on the web. The only things different were the abstract, the "author's" name, and one page of unintelligible nonsense that the guy had appended at the end.

   Just for fun, I searched for other papers by this individual, and immediately found another where he had done exactly the same thing with another of my articles.

   Now, I know plagiarism is fairly routine in academic papers, and I usually just ignore it, but this "cut and pasting" of a whole paper verbatim seemed particularly blatent, so I sent an email to the arXive, pointing out the two papers and the web sites they were stolen from. To my surprise, the arXiv rather promptly took down the offending papers, replacing them with a note about plagiarism, and sent me a message saying they had contacted the University to inform them of the situation. At that point I was sort of impressed (and surprised) at the level of ethics, although I was actually feeling a little sorry for the "author", whose career I naively imagined had been irrepairably damaged. As it turned out, I was worried for nothing… About a week later, revised versions of the papers re-appeared on the arXiv, essentially unchanged, but with two or three statements inserted here and there, saying "As explained in reference [x]", with a reference to my articles. So (apparently), all is well.

   On another related point, there has been a recent rash of plagiarized mathematics web sites, apparently by some people in Russia or one of the "stans". Their exact identity is unclear, but their mode of operation is to copy whole web sites, including the entire directory structures, and then convert all internal links to eliminate any reference to the original site, re-directing them to the bogus site. (They must have fairly sophisticated tools to do this.) Then they give the site a new name, and invent some people, complete with phony biographies, credentials and photos, who claim to be the authors of the material. They then begin to send emails to other people with mathematics web sites, asking them to link to the bogus site (presumably to increase traffic and raise their google rankings).

   The ultimate objective of this activity is not clear (to me), but I suspect it is to generate traffic for some kind of phishing activity… I understand the term of art is "social engineering". They are hosted on either blacklisted ISPs in the US, or else ISPs in Russia, so they are fairly untouchable from a legal standpoint.

7. **anonym**
   August 23, 2007

   I know plagiarism is fairly routine in academic papers, and I usually just ignore it

   that's what many people say : don't bother.

8. **Intellectually Curious**
   August 23, 2007

   "Now, I know plagiarism is fairly routine in academic papers…"

   Please tell me it ain't so. I thought even self-plagiarism (where you copy portions of, say, an introduction from your previous paper to another paper even though the two sets of results do not overlap) is frowned upon because of copyright issues.

   If the problem is so routine, then where's the scandal? Or do different fields/subfields have different ethical standards?

9. **Bee**
   August 23, 2007

   I am shocked. Seriously, I am really shocked. What is the world coming to? This is 20 people who did that not only once but in an organized and repeated way, I can't believe that. I mean, why on earth would one do that. Don't they have no interest in doing 'real' science. Didn't they ever think about what they are doing to the field?

   Sure, one part of the problem is the lacking quality control of peer review. But I think this problem goes back to the sheer amount of papers that get published (or want to get published). And this again goes back to the pressure people have to publish – which not only means that referees get many requests, but they have themselves pressure to publish papers… Taken together (too many publications, pressure to publish fast, lacking quality control) this is bound to get worse, not better, it's a downwards spiral. The technological improvements that today allow us to have 'rapid communications' and an overflow of available information DO influence the way research is done, and it's about time to realize that it's necessary to figure out how to best deal with these changes. How about slowing down the ever faster spinning world of science and give people some TIME to think about what they really want to achieve with their work (publications? citations?).

Best,

B.

10. **Godfather**
August 23, 2007

I wonder, are the chief editors of those journals going to be fired for extreme incompetence? Or are editors going to remain as unaccountable as always have been?

Imagine that a group of high school students looted a famous museum. What would the scandal be, the looting by the students or the incompetence of the museum's management?

11. **Demonic Gerbil**
August 23, 2007

And to think I'm trying to do research and get published the old fashioned way…

12. **Marty Tysanner**
August 23, 2007

I agree with Bee. It is surprising that someone aspiring to a career in science would try to establish themselves through systematic plagiarism. One wonders what brings them into science at all — certainly it doesn't seem to be a love of discovery, deeper understanding, and desire to contribute to a body of knowledge. It would require an exceptional level of self deception for a plagiarist to look back on past unoriginal work with any satisfaction at all. The whole notion is incompatible with the goals and ideals of science.

However, the problem seems quite solvable, at least in principle. Since most submissions of papers are now done electronically, it should be very possible to look for matches between an incoming paper and a database of electronically submitted and digitized papers. Warning flags could be raised if a paper had more than some minimum number of sentences or paragraphs in common with another paper. A paper would be rejected prior to the refereeing stage until any plagiarism issues are resolved. The arXiv would be one excellent place to implement this.

There are already companies like *turnitin.com* that perform this service for e.g. high schools, but perhaps a more sophisticated system would be needed than that. And such a system would have problems of its own. Computing resource requirements would be substantial, although they could be reduced by restricting comparisons to papers in the same field or subfield, or restricting searches on hot topics to the past few years. A highly motivated plagiarist might find ways around the system, such as systematically changing a word or two in each sentence, although there may be search techniques that can get around that. Anyway, in principle…

13. **a quantum diaries survivor**
August 23, 2007

Oh, Bee, shocked… Come on, see things in perspective. There are people of low moral standards everywhere. It is only a piece of luck that they did not start a career in politics, surgery, computer science, law, economics, aviation, …..

I think this is one alarm bell, and I totally concur with Peter ('cause of course, I am a "small Peter Woit" according to lumo) when he asks what are Universities paying for when they send hefty sums to Elsevier and the likes.

But shock… I am shocked for Britain's decision to extradate a lesbian to be lapidated in Iran today (see my blog), no room for publishing shocks today.

Cheers,
T.

14. **Thomas Love**
August 23, 2007

It has been obvious to many that scientific journals will be out of business in the near future, they are too slow and cost too much. On-line journals will take their place. What we need is a way to rate on-line articles.

But then again, I've talked to many researchers who say they don't have time to read because they are too busy writing.

15. **Peter Woit**
August 23, 2007

As usual, of course I agree with Tommaso… The fact that there are people out there trying to game the academic system doesn't qualify as shocking. What is shocking is how many supposedly reputable journals publish blatant plagiarism. The refereeing system in theoretical physics is completely broken and no one seems to care or be willing to do anything about it. This is not news, the Bogdanov scandal was a good example several years ago, but this is even worse.

One interesting question is how many times these authors had their papers given to a competent referee, one who would recognize the plagiarism and report it back to the journal and to the arXiv. Out of their many, many paper submissions, how often did that happen? Virtually never, I suspect…

I don't agree with Marty that what is needed is a "turnitin" system. The problem is not the blatant plagiarism like this, but the amount of unoriginal and shoddy work being published by theoretical physics journals, including even the best ones. At least in the case of this kind of plagiarism, it is clear to everyone what is going on. If referees are willing to allow this, they're certainly going to allow a lot of papers through that are little more than older work superficially rewritten. Either these journals should find a way to get papers competently refereed, or they should shut down and stop defrauding the institutions that are paying them for supposedly doing a competent job of "peer review".

16. **milkshake**
August 23, 2007

There is large amount of unoriginal crap published in lesser chemistry journals, too (there were some recent high-profile cases of fraud and plagiarism too, but it is less common).

What really gets me are research groups that never checked the literature before starting their project. They re-discover some stuff done 15 years ago and they re-brand it as a tremendous achievement of their own. When it is pointed out to them that there is a nearly identical precedent which they failed to acknowledge, they try to carefully weasel their way around it in an obscure "note added in print" instead of re-writing/retracting their paper.

17. **Arun**

August 23, 2007

Good old Nicolai Ivanovitch Lobachevsky! His method, it lives on!
Bozhe moi!

http://members.aol.com/quentncree/lehrer/lobachev.htm

18. **jhk**
August 23, 2007

"This paper has inappropriate amounts of overlap with the following papers also written by the authors or their collaborators."

Uh oh. If they're gonna start cracking down on this, it will jeapordize the physics community's favored method of padding CVs and boosting citation counts. This is playing with fire.

19. **Marty Tysanner**
August 23, 2007

I also agree with Tommaso that people of low moral standards occupy all niches of society. However, I think that choosing a career in scientific research is qualitatively different than choosing the path of politics, surgery, engineering, law,… It seems unlikely that most people can be truly successful in science without some kind of passion for the subject — there had to be something that made it initially interesting enough to go through the pain of graduate school and beyond. If *I* were trying to maximize success in terms of money, power, and popularity with the opposite sex, I don't think I would choose science! Those kinds of rewards seem to come a little more automatically through some other career paths…

That is what what makes it surprising to me that prospective scientists would resort to outright plagiarism. In the long run, if someone is viewed as "successful" due to copied work, it seems probable that others will eventually find out their success is not genuine and all will come crashing down. In the meantime, the usual "rewards" for a huckster will not come nearly as readily as in other fields.

Like Peter, I am not actually convinced that what is needed is an automated system for detecting plagiarism. My point was that the problem of blatant plagiarism should be solvable in principle by such a system. Whether the problem is serious enough to warrant such a step is a different question.

The problem of how journals can reward or cajole referees so that they take the task seriously is also a different and difficult question. If a referee doesn't do it out of a sense of professional responsibility or for other noble reasons, and the reward system within universities or labs is not in place to encourage it for less noble reasons, then short of outright payment for services it seems there aren't a lot of good options. And pay-per-paper refereeing doesn't seem likely to lead to lower journal costs…

20. **Bee**
August 23, 2007

Hi Tom,

Sure, there are "people of low moral standards everywhere", but this is not just one guy who copied a paper, it looks like it was an infectious and spreading disease that apparently was considered to be a good idea for whatever reason. That's why I am shocked. I mean, what is the next level? Pseudo-scientists trying to hack SPIRES and to push their citation level? Scientific terrorist attacks that delete gr-qc? Or, actually, the next logical step would be the computer code that produces finished papers that are not so obviously plagiarisms (along the lines of this paper recipe maybe?). I mean, people accuse me occasionally that I am too idealistic, but yes I admit I am troubled by people who willingly damage the field for no other reason than their immediate advantage (which btw I think is very shortsighted even for themselves).

Unlike Peter however, I don't think the blame goes entirely to the journals. Enforcing a better peer review system is definitely going to help in this regard, but it's curing the symptoms, not the cause. Ask what leads people to act this way. Remove their incentive. It's being judged by number of publications, by quantity instead of quality, that is a trigger. It's the glass wall that surrounds North America whether you realize it or not. It's time pressure. It's PUBLISH OR PERISH. It's a society that values status (career! career! career!) over wisdom. It's a community that punishes those that don't fit into scheme A that causes outsiders to fake their selection criteria – it's survival of the fittest, right?

Best,

B.

21. **Zul**
August 23, 2007

«Amos Dettonville Says:
August 23rd, 2007 at 11:27 am

I was once reading some papers in the physics arXiv on a subject of interest to me, and came across what struck me as a particularly lucid and insightful paper from someone at a University in Portugal.»…

Title of the paper please or even better, link in the arXiv.

22. **LDM**
August 23, 2007

Interesting.

Did it ever occur to anyone that the failure was much earlier…probably in high school? Does anyone think it is possible that "copy and paste" is a skill that first gets honed and rewarded in high school?

Yes, there are issues with the journals and the review system, but computers just by themselves have lowered the quality of education in more than a few areas. Copy and paste we have just seen. More insidious is the eroding of the knowledge base. The person who goes to the library and scours the books in the aisles for what he/she needs is forced to do some extra mental work and thereby assimilates more knowledge in the field. A computer search is too easy and you miss out on becoming familiar with the literature in your field.

23. **J**
August 23, 2007

Oh my. What is wrong with this "top" journals?

24. **D R Lunsford**
August 23, 2007

Most of even legitimate papers (meaning, not stolen) is so worthless – how can you honestly judge it? What's the point of stealing bullshit?

This is all surreal. Thank God I escaped this cesspool.

-drl

25. **Sidious Lord**
August 24, 2007

When searching the arXiv for plagiarizes

http://arxiv.org/find/all/1/abs:+plagiarizes/0/1/0/all/0/1

one also gets a paper co-authored by Ginsparg himself

http://arxiv.org/abs/cs/0702012

describing the plagiarism detection in arXiv. This is quite recent too.

26. **Peter Orland**
August 24, 2007

I am not shocked at all by these events (though I am smugly and undeservedly pleased that Phys. Rev. D, in which I have been publishing as of late, is not on the list).

What I am amazed by is that the authors of the original papers did not notice what was going on. Plagiarism is an old story, and it's gone on as long as there has been literature. Here is what is stunning; none or few of the victims seemed to have been aware of the crime. Had they known, they would have alerted the faculty of this department much sooner and it would not have got this far. This is a new phenomenon.

I think what is going on is that theoretical high-energy physics has become almost entirely literature-driven, as opposed to idea-driven (for practical reasons, it is not experiment-driven, at least not for another year or so). As Milkshake noted above, people dress up old ideas with new buzzwords and try to pass them off as original work.

People are far more obsessed with producing papers on trendy topics, and don't care much about their ideas. If the paper itself instead of its content is the desired product, then no one cares if the content may be found elsewhere.

27. **dave tweed**
August 24, 2007

Regarding LDMs comments:
"The person who goes to the library and scours the books in the aisles for what he/she needs is forced to do some extra mental work and thereby assimilates more knowledge in the field." I think it's a mixed bag. On the one hand, looking at bound journals in the library does require some extra thinking which can help understanding, but, partly because libraries only take a selection of the journals in a field and partly because its difficult to do more than follow author-supplied references in printed papers it can lead to following "the popular consensus" whilst the internet search does offer the opportunity of discovering a relevant search phrase in the body of a technical report ostensibly about a different subject that gives you another avenue. Being completely honest, back when my library did primarily take paper journals, the biggest advantage was that the library was in a building five minutes walk away which helped wake you during a mid-afternoon low-spot.

"A computer search is too easy and you miss out on becoming familiar with the literature in your field." With the proliferation of publish opportunities, particularly in fields where conference publications are much closer to the leading edge than the eventual journal versions, is it really possible to become familiar with the literature just by going to a typical university library? I try and do a reasonable amount of reading, but I know that if I did "fully comprehensive" reading at current publication volumes I wouldn't do any new research so I somewhat take a chance I may be *unwittingly* duplicating someone else's work published in a minor journal. Of course, if it's pointed out to me that I have duplicated some existing work then the work becomes "not novel".

28. **Matt Daws**
August 24, 2007

Is it just me, or did anyone else notice this:

prodigious track record of publication: over 40 papers in a 22-month span

No, that's not "prodigious". That's comediarily impossible in theoretical physics. Why did anyone think for a moment that this was possible?

I'm a mathematician, not a physicist, but I do sort of agree with Peter Orland. If papers which were basically copies of my own were published in top mathematics journals, then I would definitely notice. Heck, I'd notice when they were posted on the ArXiV. Is this because people have essentially ceased to read the top physics journals?

29. **klien4g**
August 24, 2007

To Arun: How was Lobachevsky ever involved in plagiarism (pace Lehrer's song)?

But its very depressing to hear about the 'po-mo'-isation of physics.
Perhaps, its because upto the early 20th c. those really interested in the sciences landed up doing it and now since there are so many more people competing for just about any profession one cares to name, the sciences has become just another job one chooses to do — not ready to put up with the grueling work hours of investment banking, well how about some condensed matter physics– but oops, how does one learn originality and creativity??!!

30. **anon**
August 24, 2007

Marty suggested

> Since most submissions of papers are now done electronically, it should be very possible to look for matches between an incoming paper and a database of electronically submitted and digitized papers.

Rien pointed out earlier in the comments that this is [already being done on the arXiv](#).

I agree with Bee that it's the publish-or-perish ideology that's to blame here… we are becoming less like universities and more like corporate thinktanks.

31. **AGeek**
August 24, 2007

Dear anon, corporate think tanks are generally expected to produce something useful, so they are subject to objective evaluation. That's a big step up from academic "publish or perish", where it's the killing of trees that seems to matter most. 😉

32. **LDM**
August 24, 2007

D R Lunsford's comment "*Most of even legitimate papers (meaning, not stolen) is so worthless – how can you honestly judge it? What's the point of stealing bullshit? This is all surreal. Thank God I escaped this cesspool.*"
is one I tend to sympathize with…but really how many academics who's careers have been built on this system would admit the same?

It should be clear by now that the "publish – or – perish" meme does not result in high quality. In fact, since quality by definition is harder to produce, it is clear that low quality papers will tend to be the norm.
It is time to kill the "publish – or – perish" meme. People who are passionate about science will still publish by virtue of their passion for science anyway — but they won't be *forced* to produce what is essentially, in many cases, scientific waffle.

33. **amused**
August 24, 2007

Well this isn't good, but people shouldn't get too carried away and draw overly generalized conclusions. As far as refereeing standards go, I think it's important to distinguish between two classes of papers. The first consists of the papers which address issues of central importance to a subcommunity of sensible researchers. My impression is that refereeing standards are generally pretty good here (although everyone has a bad experience from time to time). The main complaint that could be made, I think, is that papers making too incremental advances are getting through. On the other hand, for the papers in the second class (which consists of all the papers not in the first class) refereeing can be very sloppy, even when the papers are submitted to supposedly top journals. For the simple reason that referees are loathed to invest time and effort in reading and evaluating papers that don't interest them. Recommending publication is a quick and easy option; it can be quite a pain to reject a paper since then you will have to spend time dealing with the authors' outraged rebuttal. Of course, a person may simply refuse to referee a paper that isn't interesting to them, but then it will just get sent to other people, and if the paper belongs to the second class it can eventually happen that someone just waves it through or it ends up being refereed by a incompetent person.

This obviously isn't a good situation, so what can be done about it? Well, as I've harped on about elsewhere I think we should take a look at how things are in the maths community. Probably the lesser maths journals are afflicted with problems similar to the physics ones, but on the other had it seems that the maths folks have been successful in maintaining high standards for their top journals. I doubt this is just because they are more conscientious than us. A mathematician who gets sent a paper to referee for Ann. Math. has a significant selfish motivation for doing a good and careful job: Her own high standing in the broader maths community comes in large part from having published papers herself in Ann Math and similar journals, so she will want to make sure those standards are maintained because if they slip then the prestige that she has derived from publishing there (and will derive from future publications) slips as well. Contrast this with the situation in physics (or at least h.e.p.): Someone who gets sent a paper to referee for, say, JHEP, has very little selfinterest in doing a good job – there is little prestige to be had from publishing in JHEP because the threshold for getting published there is not high. Your paper might make a major advance, but it is published side by side with others making small incremental advances and the occasional paper full of vacuous nonsense (yes I've seen them) or even containing plagiarism. So how is anyone to know the high quality of your paper just from the fact that it got published in JHEP? So you get almost no prestige from publishing in JHEP, and therefore have nothing personal at stake in keeping its standards high, hence no selfish motivation to do a good job with refereeing.

From these considerations an obvious solution would be to establish "high quality" journals in physics just like the mathematicians have. But this would require a major change of culture in physics. In physics, as far as I can tell, a person's prestige and standing is derived not from where they have published, or how many papers they produced, but what clique they belong to. Of course within their own subfield the person's standing will be determined by the actual work they have done, but when they are being evaluated by a dean and committee members from outside their field it will be their connections and the power of their senior supporters that count the most. In maths, as far as I can tell, publications in top journals can trump connections to powerful people, but not so in hep physics. Some outsider can publish in PRL until they are blue in the face but it won't help them a shred when they are up in competition against the student of Nathan Seiberg or the postdoc of Joe Polchinski. The current situation suits the powers that be just fine, so I don't foresee it changing anytime soon. But maybe if we heap enough scorn and contempt on them it might eventually have some effect.

34. **MathPhys**
August 24, 2007

My experience with alerting people to cases of plagiarism that they should know about (for example, pointing out to a supervisor that one of his students is into that) is that they just don't want to hear about it. It's too awkward and too much trouble to deal with. Unless of course the vicitim finds out and formally complains.

35. **Phil**
August 24, 2007

Peter Orland said

"What I am amazed by is that the authors of the original papers did not notice what was going on."

As an author of one of the original papers I would like to respond to this. In my case a section of one of my papers gr-qc/9803014 was copied almost verbatim. This section was actually an introductory piece describing the equations of the L-T model which are very old. The rest of the paper they wrote was very different and was not taken from mine. If they had plagiarised my original ideas I would probably have noticed it, but I would not notice the copying a few standard equations and text unless I read every paper in the archive and have a very good memory for how I wrote things a few years ago.

I think they got away with it for so long because they were quite careful not to copy whole papers. I think it is their supervisors who should have become aware of it, assuming they were not complicit. The journal referees may also have a case to answer but I think you would have to check each paper in detail to see if there was really anything that they should have been suspicious of. At least we can be satisfied that the system of independent outside review for doctorates caught them out in the end.

Personally I am more concerned about cases of plagiarism where people copy an idea and rework it without giving credit to people who did similar work previously. These days there are many people who do research work and leave academia never to return. Later their ideas can become more relevant and someone else picks them up. Since the person left they may not follow the "reinvention" of their ideas and even if they do it is very difficult to point out their priority. For example the arxiv does not accept backtracks from outsiders.

It would be nice if something could be done about that.

36. **Hüseyin Nail Karaaslan**
August 24, 2007

Oh! Aristoteles!
Back to the middle ages, scholastic thinking!!
I worry about the future of the science in the developing countries..
Hardworking graduates start to work at the private enterprises and companies, a few prefer studying science academically..
Governments have to consider this situation carefully and try to plan and organize a logical application about science..

37. **Godfather**
August 24, 2007

> Recommending publication is a quick and easy option; it can be quite a pain to reject a paper since then you will have to spend time dealing with the authors' outraged rebuttal.

And who is responsible for finding competent referees, overseeing the editorial process, and taking a final decision on the basis of that process?

Let's not forget that it is not referees who accept papers for publication, it's editors who do. That's precisely their job.

On the other hand, we do not know whether all of these plagiarized papers were actually refereed. Editors can, and often do, take editorial decisions without consulting referees.

38. **ppsh**
August 24, 2007

Example when authors did found plagiat. I got reference few days ago. First paper is original and second is identical plagiat with only title changed.

1. Original: Optimization of ionic conductivity in doped ceria
David A. Andersson, Sergei I. Simak, Natalia V. Skorodumova, Igor A. Abrikosov, and Borje Johansson
3518–3521 PNAS March 7, 2006 □ vol. 103 □ no. 10

2. Plagiat: Determination of dopant of ceria system by density functional
theory K. Muthukkumaran ; Roshan Bokalawela ;
Tom Mathews 198; S. Selladurai
J Mater Sci (2007) 42:7461–7466

39. **Godfather**
August 24, 2007

> Example when authors did found plagiat. I got reference few days ago.

I'm impressed… I had never seen anything like that before. The *entire* paper is copied word by word and figure by figure.

40. **Bee**
August 24, 2007

@Matt Daws

No, that's not "prodigious". That's comediarily impossible in theoretical physics. Why did anyone think for a moment that this was possible?

It's not impossible. I know personally several people who have published about 20 papers per year over a period of several years. I don't want to comment on the quality of these papers. Things like this however depend strongly on the field one is working in, and just counting items on the publication list doesn't mean very much. To begin with, it's quite common to take papers apart in the smallest possible bits that can be published separately – wouldn't it be stupid if you didn't, it's like throwing away a paper for free.

Unfortunately, many funding agencies are not aware of that (or pretend to be not aware, or ignore the issue on purpose). See, there is the primary goal to fund good researchers. But this is not a useful prescription in many regards, it's too vague and too complicated. Instead, there are some secondary requirements that have been worked out over the decades that indicate interesting research and that allows faster judgement. One is the number of publications.

However, the problem starts if people instead of aiming for the primary goal (good research) try to be 'smart' by targeting the secondary criteria (many publications) directly. That's why I said above: remove the cause, not the symptoms – otherwise the problem will only reappear elsewhere. That is to say: rethink presently applied secondary criteria. Do they still satisfactory match with the primary goals?

Best,

B.

41. **ppsh**
August 24, 2007

I think plagiat is really stupid thing at modern times. Too easy to discover and to prove. Which is not the case for pure fraud. How can referee decide if some experiments described in the paper were really performed or not? Or was result of computer simulation exactly that one as described ? In worst case author can admit some undeliberate errors and to publish corrigendum. Almost risk free if author wil not claim too much as did Hendrik Schön

42. **anonym**
August 24, 2007

To Phil the author of gr-qc/9803014 :

If they had plagiarised my original ideas I would probably have noticed it, but I would not notice the copying a few standard equations and text…

Just to make sure : so does their paper have an original contribution (which is non-trivial)?

43. **Phil**
August 24, 2007

anonym, are you asking if my paper gr-qc/9803014 has an original contribution or if the paper gr-qc/0607083 which plagiarised it has an original contribution? My one does, of course, but I will assume you are asking about gr-qc/0607083 🙂

Ok I have done some further checking with the help of google. The next paragraph after the one copied from mine turns out to be copied from gr-qc/0011087 which is about teleparallel gravity. I have had a quick read and I cant make much sense of gr-qc/0607083 overall. The L-T model which was copied from my paper is standard GR whereas teleparallel gravity is a generalisation with torsion. I dont see how these are being used consistently. Indeed I dont see the equations of the L-T model being used later on. I may be missing something but the paper looks like nonsense to me.

At least this one was not published.

44. **Mr. Mustela**
August 24, 2007

I once witnessed a smallish plagiarism scandal first hand. Since then I am convinced that many plagiarists simply see nothing wrong in it because they have a completely different understanding of the system than we do. That explains the level of "stupidity", or callousness, that is involved.

The case I am talking about is funny. A PhD student turns in a thesis. Large swaths of it are copied and mashed together from published papers – dig this – from his supervisor! It took a while for him to accept that there was any problem with this, and in fact was surprised that the supervisor didn't find this flattering. From all I can tell, he never understood the problem to any level of depth. He just accepted it and tried to adapt (personally, I think the real scandal is that he got a second chance).

I am sure that for many plagiarists, a paper is nothing but a self-important swath of glibberish that bears names on it. It being published just means that the people named enjoy the favor of the gods, and thus are great. And being great is the purpose of life, isn't it? They can't, even if they have some technical chops, see anything but self-important drivel in any article they read, cannot imagine that anyone sees anything else, and essentially just want the favor of the gods. Plagiarism is just a side effect of wanting to play it risk free and use the same incantations that have worked before. You know, not everyone can come up with original glibberish that sounds right.

I agree with peter that it is a very bad sign when such approaches start to work too well.

45. **SnarkFest**
August 24, 2007

Mr. Mustela,

Your characterization, which strikes me as completely on-target, describes the ultimate in cargo cult science.

Automated plagiarism detection does suggest a low-overhead solution for online papers—clearly flag the work as plagiarism, and leave it in place, with the "authors'" names. Provide canned queries on the relevant sites that list the plagiarized papers, and prominently identify and rank the plagiarists for their achievements…as plagiarists. They want recognition and notoriety? Fine, give 'em recognition and notoriety.

Of course, in practice some innocent people could be caught in the net. (Recall William Proxmire's Golden Fleece Awards.)

46. **Coin**
August 24, 2007

Automated plagiarism detection does suggest a low-overhead solution for online papers…Of course, in practice some innocent people could be caught in the net.

Considering how many papers go online before publishing these days, it does at least seem that *Journals* should use automated plagiarism detection at least as a first pass. A good way to do this would be to run all submitted papers through a plagiarism detector first thing, and have anything that triggers the detector be simply punted to a human editor for review, just in case; since journals do not accept that many papers, they surely have the time to run a simple tool and filter out false positives by hand.

I actually kind of hesitate to even suggest something this simple; after all, I'd be shocked to learn they weren't doing it this way already, as the same is widely done for, say, papers handed in for undergraduate classes. Looking at how many different journals this one group hit before they got caught, however, it looks like apparently they're *not* doing it this way already…

47. **Rien**
August 24, 2007

Bee wrote:

> It's not impossible. I know personally several people who have published about 20 papers per year over a period of several years. I don't want to comment on the quality of these papers. Things like this however depend strongly on the field one is working in, and just counting items on the publication list doesn't mean very much. To begin with, it's quite common to take papers apart in the smallest possible bits that can be published separately – wouldn't it be stupid if you didn't, it's like throwing away a paper for free.

I know what you mean, and it annoys me endlessly. I have tried a couple of times when I was refereeing papers like that to get editors to reject the paper on the grounds that it is just the Nth iteration of the same paper by the same authors. People just rerun their numerical codes with slightly different parameters and call that a paper.

But maybe the new anti-plagiarism system at arXiv will detect those papers too!

48. **amused**
August 24, 2007

Plagiarism can have many different shades of gray and it isn't always simply a case of cutting and pasting from someone's paper. I had an experience a few years back of someone rederiving a result from one of my papers, supplementing it with a physical interpretation (which I had been aware of but didn't consider interesting enough to mention) and then passing it off as entirely his own work (it got published in JHEP). He referred to my paper in a sentence "See Ref.[X] for related work". It's not so easy to know what to do in this situation; his rederivation and formulation of the result is presented quite differently to mine and it wouldn't be obvious to others unless they invested a fair bit of time into checking the technical details. I have toyed on and off with the idea of writing a paper to point out that his paper and mine are not only "related" but are in fact essentially identical modulo some padding, but am loathed to take time out for this which I could be spending on real research.

49. **hyperbolic geometry footnote**
August 25, 2007

klein4g,

Lobachevsky is the protagonist in Lehrer's song because of the well known multiple discovery of noneuclidean (hyperbolic) geometry. Credit for this discovery, and for the later construction of specific models, is emphasized differently in different countries.

50. **Phil**
August 25, 2007

In case anyone is interested to hear more about my original paper that got plagiarsed, I have written something about it in my blog here 🙂

51. **Log Lady**
August 25, 2007

As another war story, about 20 years ago I refereed and rejected a geophysical paper. It seemed pretty clear to me that the junior author had written his own computer program based on someone else's Masters thesis and used the thesis results to check it. So far, so good. He could have then used the program to do something new. But then he wrote his own paper and submitted it as new work without even any reference to the thesis. I pointed out to the editor that the work was not new and roughly fifteen of the figures in the paper were identical to the figures in the thesis. (Really identical. After all, he wanted to check his program, didn't he?) I expected never to see the paper again, but it was published a couple of years later. The senior author, an accomplished schmoozer in the field, had simply hammered away at the refereeing process, demanded a more "reasonable" reviewer, and outlasted the periodic changes in editor and associate editor.

I don't read the journals anymore. The papers are too poorly written and I don't have time to find and correct the errors. If I have an idea, I troll the internet for useful information. When I write a paper, I feel no obligation to pad it with references to everyone that ever expressed a thought about the subject. I only include references that I personally found useful and that were well-written enough that I would recommend them to others. Unless I am specifically writing a review paper, acknowledging everyone who made any contribution to the area simply buries important references in a cloud of fluff.

52. **Bee**
August 25, 2007

Hi Rien:

Indeed, that's exactly what I mean. And yes, it is endlessly annoying. Coincidentally, I yesterday came across this Wikipedia entry about the 'Least Publishable Unit' 😉

Besides this, some remarks on the arXiv plagiarism check. Last year or so, I noticed they were fiddling around with something because upon submission of a paper I got a message saying my paper was rejected because it seemed to be a copy of an already existing paper. I had a look at this other paper (turned out I knew the author), and there were about no similarities except that I accidentally had picked the same title (well, it was a proceedings article, how creative does one have to be for that?). So I sent a complaint to the arXiv guys, and they resolved the problem (very promptly and within only a couple of hours I have to say.)

So I was somewhat sceptical about the whole thing, but by now I came to like it. There have been at least two groups of people who have been posting papers (repeatedly) where the whole introduction was made of copies from my papers. It's not that the actual content was a plagiarism, but I found it really annoying nevertheless. I mean, I spent quite some while writing this stuff, and then there come some guys and just copy it? I have sent them emails once or twice, which didn't change anything. Coincidentally I had to referee some of their papers where I send a note to the editor about it. The result of that however was that they replaced the copies from my papers with copies from somebody elses paper (I happened to recognize also his writing). I should add in both cases, they cited the original work. (And in both cases it considerably improved the English of the manuscript.)

One way or the other, the first some pages of their recent papers still have close resemblance to mine, but they have rearranged the sentences noticeably. I guess this is because they ran into problems with the arXiv filter, and so I really welcome it.

Best,

B.

53. **Alfred Holden**
August 25, 2007

Plagiarism is form of flattery. Though without honor it has been practiced since time began. Concentric waves of thought emulating successive authors' works.

Good work is rare. Plagiarism merely reinforces existing work. One could speak of the stress that belies the academic professions to publish. With Universities and Colleges expanding, the pressure for new works taxes the professions.

It is an age of mediocrity. A broad band of white noise.

Occasionally, we hit a chord. Sometimes we have a tune.

Think of those who have rewritten history countless times to where we accept things on faith alone. Where objects become larger than life, where they aquire a life of their own.

The philosopher's stone, the journey towards enlightenment, the means to all ends.

The brightest of our students search for this. It is what gave rise to alchemy in the ancient Near East, Egypt, Greece and India. It became the "holy grail" of Western alchemy.

In the view of spiritual alchemy, making the philosopher's stone would bring enlightenment upon the maker and conclude the Great Work.

Disambiguation is the result of the process of resolving conflicts as in plagiarism, where the author strives to conclude the "Great Work".

54. **a.k.**
August 25, 2007

..one could remark that, this does mainly concern PhD thesis, that it can be probably more honest to summarize used results clearly, even if it has a copy/paste character, than to seemingly 'rework' the material and give the half-informed reader the illusion to have creatively invented a whole area. We should be all quite aware (Einstein was, by the way), that we only make more or less tiny contributions to an area thousands of brilliant thinkers have smoothed and brightened long before we even existed. Given that at least PhD-thesis are expected to be to a certain degree 'self-contained', there is inevitably a certain degree of redundancy to existing papers and results, the creativity to arrange these results 'nicely' (seeming to be 'orginial') could be more effectively adressed to exploring new ideas, this is at least my point of view.

55. **Kris Krogh**
August 25, 2007

Einstein quote: "The secret to creativity is knowing how to hide your sources." Can't say I agree with that one.

56. **Amos Dettonville**
August 26, 2007

Kris,
Can you cite a reference for that Einstein "quote"? I know it's in circulation, and was used as the motto for a book accusing Einstein of being an "incorragible plagiarist", but when John Stachel (Einstein scholar) reviewed that book, he mentioned that he knew of no actual reference for that quote. To my ear, it sounds like another of those "sayings" (like the recent one about mankind only surviving for 3 years if the bees all disappeared) that someone decides to attribute to "Einstein" to make it sound more authoritative (or something). Anyway, I'd be interested if you know the source.

57. **(withheld)**
August 26, 2007

I once had a paper that an ESL coauthor wrote, and noticed that the English suspiciously improved in places. I simply copied the sentences into Google and they popped up on pubmed and other indices as imbedded in other papers. I was furious, and let this guy know that this was unacceptable and his career (or at least our collaboration) was over if it wasn't fixed immediately and never happened again. What was most stunning was that he kept referring to it as a copyright issue, rather than grasping that there was fundamental dishonesty involved.

And that was just a few sentences mostly in the intro, actually kind of irrelevant to any deep original thoughts in the paper. I can't conceive of what kind of mentality it takes to copy a whole paper with results. Far beyond that is the mentality of an editor who won't pull such a paper immediately and permanently ban the offending author from its pages.

58. **a.k.**
August 26, 2007

..one could add that from my point of view, the actual problem in mathematics, physics and related sciences is that, in contrary to the 'classical' sciences as philosophy, there is no 'citation culture' in the exact sciences, it was inherent over thousands of years (refering to Euclid et al.) that mathematical findings are 'unpersonal', they tended to be results founded by large groups, collectives of mathematicians. From this point of view, a 'citation culture' wasn't needed, up to the present, the actual training of mathematicians involves to a large degree to 'transform' collective knowledge into subjective knowledge, where this borders to simple 'plagiarism' is often impossible to decide. What I want to express is that excessive plagiarism is to a certain degree a (deformed) continuation of 'well-accepted' methods in the exact sciences, assuming this, the discussion is not that 'black and white' anymore as it may seem.

59. **Kris Krogh**
August 26, 2007

Amos,

I think you're right. In most compilations of Einstein quotes, that one is included. But, searching the web just now for the source, I couldn't find one. Thanks for setting me straight!

60. **Intellectually Curious**
August 26, 2007

Dear (withheld),

I agree that what your co-author did certainly was wrong, but I can't help but think that if he (assuming it's a male) were a native English user, he could've easily paraphrased those copied sentences and not be accused of being a plagiarist. But would this make him any more honest if the original source was not cited? So I can see why your colleague might insist on this being a copyright issue rather than one of dishonesty, especially when those sentences do not involve "any deep original thoughts." I'm not defending him, of course, but I can sort of see his point.

I think whether a person is a true plagiarist should be judged over a period of time rather than being judged based on one "gray-area" incident. This person must be warned/educated, of course, as you rightly did so, but perhaps be also given the benefit of the doubt.

It's just my opinion based on what little I know about your specific case. Maybe the incident crossed a darker shade of gray than I thought…

61. **(withheld)**
August 26, 2007

I.C.: It is a point to ponder, I agree. I hadn't realized until it happened how I'd internalized the "thou shalt not plagiarize" commandment from my own education. Apparently it is not all that universal.

I came to the conclusion for myself that it is somewhat arbitrary. It certainly looks very very bad if you are caught copying verbatim, whereas paraphrasing without citing is more in the realm of poor etiquette (by consensus). Since this guy was a student who was quibbling already, it didn't make sense to me to be nuanced in my criticism. I thought well of him otherwise and wanted him to know that people would frown on doing such things.

That’s all tiny potatoes compared to what Peter’s post was about, though. I’m still stunned thinking about it. The gall…

62. **Intellectually Curious**
August 27, 2007

Those of us who’ve been educated in the West know that plagiarism typically means copying verbatim. But I’ve encountered researchers from various cultures who would never consider stealing (original) ideas but would be more careless with borrowing words from others to describe, say, common concepts and definitions. To them, the intent is not to improperly claim credit but to find a way to express themselves more effectively and correctly in a language foreign to them. These otherwise reputable individuals should be told that in such matters the letter of the law is just as important as the spirit of the law.

63. **Paper Laundrer from Pakistan**
August 27, 2007

A huge help can be made available by ACM, IEEE and other big publishing organization to conference and journal editors by providing free or low-cost “plagiarism’ check and mechanism to scan through short-listed papers before publications.

This sort of events can cause serious embarrassment to the Conference Organizers, Universities and even the countries involved.

64. **Jim**
August 27, 2007

Actually I had one problem in my career with distinguishing what is plagiarism an what is not. In an invited topical review I reviewed work of other authors and I wrote “the following theorem was proven in [Reference]” and then I copied the two-lines theorem verbatim. I did not consider this to be plagiarism, since I properly referred to the author’s work, but then I was attacked by the author that this is plagiarism and that I should have used quotation marks when writing down the theorem or I should have rephrase the theorem. I do not think that it is a good idea to rephrase theorems every-time they are mentioned and I did not see in the literature theorems in quotations marks either. What is your opinion? Fortunately, I did not have to write another review since that time.

65. **Intellectually Curious**
August 27, 2007

Jim, your case clearly was not plagiarism because 1) you were reviewing the original author’s work; and 2) the correct reference was given with no ambiguity of whose theorem you were describing. Maybe he/she was simply unhappy with your review and tried to find an excuse to attack the reviewer.

How many ways can you use a precise language like mathematics to write a theorem or definition without repeating parts of someone’s text or without altering the intended meaning with unnatural wording and phrases?

66. **CJ**
August 27, 2007

I do not know if this helps, but Professor Irving Hexham at the University of Calgary wrote a useful guide on academic plagiarism:

http://www.ucalgary.ca/~hexham/study/plag.html

67. **RPenner**
August 28, 2007

Outrageous as academic plagiarism is, here’s a case of pityful commercial plagiarism:

Some author named “Sapphire” wants people to buy its new book about time travel (via meditation). For some reason, Amazon.com has many wholly plagiarized “reviews” of this book, which have been lifted from blogs and web dialogues (including a dialog I had on quantum theory). No truly important work was stolen (the writing aspires to be pop physics at best) but neither were any salient points about the book in question. Amazon, has been less than helpful over the months with the “reviews” re-appearing and the public criticism being quashed. Further, the pro-Sapphire advocates describe this as an attempt to persecute Sapphire by a James Lorel. All power to James Lorel in his quest for reviews which make sense and his anti-plagiarism stance! I only regret that I no longer have a personal stake in this.

http://amazon.com/gp/product/1424315956
(See the review by Columbia University’s “Doctor of Physic” which was lifted from Wikipedia’s article on “Time Travel”)
http://forum.physorg.com/index.php?showtopic=12352&view=findpost&p=191221
(James Lorel drops by Crackpot Central to inform us willful infringement, now removed by Amazon.)

68. **Kerim**
September 5, 2007

For those who have a hard time understanding the logic behiind such plagiarism: For countries that work hard to close the scientific gap there are often attractive incentives and prizes for publishing more such as cash bonuses per publication, or harsh requirements such as publising certain number of papers for graduating, getting a tenure or proffesorship. Obviously these people, especially the two grad students were simply making “good money”. I dont think they cared about their scientific career, they were simply frauding the university. I heard that they are being sued by the university to get back some 30K $ they were paid as bonus during these two years.

69. **ahmet**
September 8, 2007

As a graduate student physics in Turkey, I feel that the plagiarism scandal is only the tip of the iceberg. For those who are following a career in physics like me have no idea how they published 59 papers in 22 months span. It would be considered as an act of greedy graduate student who has no ethics of science. But they are the member of the university in which are seeking Ph.D. degree under the supervisors of its faculty. Most of the papers had gone though review of their supervisors and their supervisors’ names were appended to the papers. Somehow their advisors were encouring these naive graduate students to publish.

Let me say what will happen next. The universities(up to now four) involved in the massive ring of paligiarism will open investigation about the case. They will impute the paligiarism to graduate students. Their advisors will say their names were added on papers without their consent. The graduate students will be discharged from the universities. So the case will be closed.

70. **surferrr**
September 8, 2007

This year 1st time METU is not in the list of the top 500 universities in the world. It seems that METU is going down in many ways. In this or that department , you see a russian or an azerbaijani writing a paper and putting his name and the names of a few more on it then having it printed. Next you see these people promoted to associate professorship or full professorship. To have a little fun ,I plagiarized the following from a page in the math department in METU :

1. Terzioglu, T.; Yurdakul, M.; and Zahariuta, V. On some normability conditions. to appear in Math. Nachr.

2. Abdeljawad, ;T and Yurdakul, M On some dual conditions in quasibarrelled locally convex spaces. FJMS, 16/3, (2005), 291-300.

3. Abdeljawad, Thabet; Yurdakul, Murat The property of smallness up to a complemented Banach subspace. Publ. Math. Debrecen 64 (2004), no. 3-4, 415-425.

4. Terzioglu, T.; Yurdakul, M.; Zahariuta, V. Factorization of unbounded operators on Köthe spaces. Studia Math. 161 (2004), no. 1, 61-70.

5. Djakov, P.; Terzioglu, T.; Yurdakul, M.; Zahariuta, V. Bounded operators and isomorphisms of Cartesian products of Fréchet spaces. Michigan Math. J. 45 (1998), no. 3, 599-610.

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71. **Özlem**
September 12, 2007

I am impressed, shocked !
As a graduate who studies math. physics in Turkey, I can’t believe in my eyes! METU is an exclusive University here and I cannot understand that how these students could be successful in their sufficiency exams during PhD before thesis? Science is not a score, science is not only a paper and not money…
What a shame!

72. **just borrowing**
October 11, 2007

Nature 449, 658 (11 October 2007) | doi:10.1038/449658a; Published online 10 October 2007

Plagiarism? No, we’re just borrowing better English

Ihsan Yilmaz1

1. Physics Department, Çanakkale Onsekiz Mart University, Çanakkale, Turkey

Sir

The accusations made by arXiv that my colleagues and I have plagiarized the works of others, reported in your News story ‘Turkish physicists face accusations of plagiarism’ (Nature 449, 8; doi:10.1038/449008b 2007) are upsetting and unfair.

It’s inappropriate to single out my colleagues and myself on this issue. For those of us whose mother tongue is not English, using beautiful sentences from other studies on the same subject in our introductions is not unusual. I imagine that if all articles from specialist fields of research were checked, similarities with other texts and papers would easily be found. In my case, I aimed to cite all the references from which I had sourced information, although I may have missed some of them.

Borrowing sentences in the part of a paper that simply helps to better introduce the problem should not be seen as plagiarism. Even if our introductions are not entirely original, our results are — and these are the most important part of any scientific paper.

In the current climate of ‘publish or perish’, we are under pressure to publish our findings along with an introduction that reads well enough for the paper to be published and read, so that our research will be noticed and inspire further work.

This Week's Hype

August 25, 2007
Categories: Uncategorized

Today Slashdot brings us the news that Gamma Ray Anomaly Could Test String Theory. As usual with such media claims about the testability of string theory, this is complete nonsense. The story is based on this Scientific American blog posting, which in turn is based on this paper by the MAGIC gamma ray telescope collaboration.

The claims about testing string theory aren't in the paper, but appear to come from string theorist Dimitri Nanopoulos who claims that he predicted (or, more accurately, "suggested") the kind of effect seen by MAGIC using string theory. As far as I can tell though, just about no string theorists except Nanopoulos and his collaborators Nick Mavromatos and John Ellis actually believe this. Mavromatos and Nanopoulos also believe that string theory is responsible for the way that our brains work, here's the abstract of one of their papers on this:

> Microtubule (MT) networks, subneural paracrystalline cytosceletal structures, seem to play a fundamental role in the neurons. We cast here the complicated MT dynamics in the form of a 1+1-dimensional non-critical string theory, thus enabling us to provide a consistent quantum treatment of MTs, including enviromental *friction* effects. We suggest, thus, that the MTs are the microsites, in the brain, for the emergence of stable, macroscopic quantum coherent states, identifiable with the *preconscious states*. Quantum space-time effects, as described by non-critical string theory, trigger then an *organized collapse* of the coherent states down to a specific or *conscious state*.

Claims have been made by many string theorists that not only does string theory not predict this kind of violation of Lorentz invariance, but exactly the opposite: string theory predicts no such violation. String theorist Jacques Distler earlier this year even went so far as to have the University of Texas issue a press release trumpeting his claims to have shown that string theory is falsifiable, using a calculation based on the assumption that string theory preserves Lorentz invariance (either his colleagues or a PRL referee wouldn't let him make this claim in the paper the press release was based on, but that's another story…).

Claims have been made (although there is controversy about this), that the main competing quantum gravity research program, Loop Quantum Gravity, predicts this sort of violation of Lorentz invariance, and this would be one way of distinguishing it from string theory. Lubos Motl has a new posting about the MAGIC result, mainly concerned with knocking it down since he fears that it will be used as evidence for LQG and against string theory.

It seems to me that in any case, the actual experimental evidence here is far too weak to support any claim that a violation of Lorentz invariance has been shown. Among the usual nonsense on Slashdot, there was the following sensible comment about the MAGIC result from an astrophysicist:

> What they are saying is that there are still details we don't understand about AGN like Markarian 501. So, while this effect could be a first sign of quantum gravity (*not* string theory in particular, as others have pointed out), it could also simply be something going on in the intrinsic spectrum of the flares themselves. I'd personally consider the second explanation more likely at this stage.
>
> As they also point out, one approach to sort out the ambiguity would be to observe other flary AGN at different redshifts (distances). One could then, for example, see if the delay gets shorter or longer as the distance changes, as one would expect with a quantum gravity effect due to propagation to Earth.

Utterly Off-topic, But How Can I Resist Mentioning: According to this blog entry by a USC student, not only am I the "archnemesis" of string theorist blogger Clifford Johnson, but also

> If string theory were a vampire, he'd be Buffy.

I'll have to consult my friends and colleagues on the resemblance to Buffy question, personally I don't see it.

I don't know about vampires, but these "tests of string theory" are kind of like the living dead, staggering around trying to get their teeth into people and turn them into string theory partisans. No matter how often you blow their heads off with a shotgun, more keep coming…

Update: Lubos and I seem to be in complete agreement about this experimental result and the Nanopoulos et. al. explanation of it. This situation appears to have driven him over the edge.

Update: See Backreaction for a more detailed posting about the MAGIC result.

Comments

1. **Yatima**
   August 25, 2007

   Microtubule (MT) networks, subneural paracrystalline cytosceletal structures, seem to play a fundamental role in the neurons.

   Yes, and it's called scaffolding. "Trigger an organized collapse of the coherent states down to a specific or conscious state"? Where do I find the paper that equates a "collapsed quantum state" to the "conscious state" (aka. the "unicorn state", often hinted at in dark, confusing tales, never actually photographed in the wild).

   Still, that paper is dated "1995", and if I remember well, "Microtubules -> Consciousness" inferences, sometimes via Quantum Mechanics, sometimes via old-school Turing Computation made several appearances in Artificial Life proceedings at those time. Even Sir Penrose entered the game. Extremely speculative. Overall, an approach that went precisely nowhere. Anyway, an area to not get into.

   Maybe I should be happy to languish in engineering.

2. **ks**
   August 25, 2007

   > Maybe I should be happy to languish in engineering.

   I guess each discipline has its own no go areas that can be colonialized / made up by members of others as their eccentric hobby. Roger Penrose could do all kinds of speculations about mind, Goedels theorem and quantum gravity. As a cognitive scientists his reputation was done but as a mathematician he won't endanger his credibility or "core competence" by doing such excursions (as long as they aren't too mad).

3. **Moshe**
   August 25, 2007

If a theory violates LI at high energies, say the Planck scale, standard renormalization group suggests that at low energies this will manifest itself by a series of relevant, marginal and irrelevant operators. Bounds on such Lorentz violations at accessible energies are extremely tight, no current experiment will improve those bounds. So, high energy breaking of LI is to my knowledge sufficient grounds for falsifying a theory, or at least casting very strong doubts on it (spontaneous violation is a different story).

Also to my knowledge LQG does not predict such violation, and Lee does not claim it does. He only claims it might once we understand things better. It escapes me why that should be a good thing, but maybe I am missing something.

4. **Eric**
August 25, 2007

   Peter,
John Ellis and Dimitri Nanopoulos happen to also be the second and fourth most cited high-energy physicists, with Witten first and Weinberg third. Thus, you're attempt to try to dismiss them as crackpots just isn't going to fly.

5. **sophia**
August 25, 2007

   The link to Motl's blog leads to something very amusing. Check it out.

6. **bhabha**
August 25, 2007

   Eric,

   You are seriously underestimating Peter's capabilities. Being the second and fourth most highly cited high energy physicist (more precisely, phenomenologist) does not deter Peter from declaring him/her to be a non-sense creating pseudo-scientist. Actually, the more famous person is called a crackpot here the better, because more controversy brings more readers to this blog. It doesn't work like in science that you have to create quality content in order to get noticed, in the blogosphere the more crazyness you produce the more attention you will get. Pretty much like in mass media which of course includes blogs these days. So don't be surprised to see more top cited phenomenologists, string theoriests, non-string theorist, etc, etc, getting on Peter's public enemy list!

   Bhabha

7. **Eric**
August 25, 2007

   bhabha,
I would never underestimate Peter's abilities capabilities, especially in regards to using underhanded tactics. 😉

   Regarding the statments about Lorentz violation in string theory, what Lubos and Distler refer to is critical string theory. It's possible to get this effect (frequency dependent speed of light) in non-critical string theory.

   In regards to the LQG vs. string theory debate, I think this is one more bit of evidence that there is some overlap between the two theories, and they may be part of the same larger theory as Lee has suggested.

8. **Peter Woit**
August 25, 2007

   Eric and Bhabha,

   Unlike you I'm not personally attacking anyone, but discussing their scientific arguments. The argument that the MAGIC results give evidence for string theory is, scientifically, nonsense, and it would be hard to find anyone other than Nanopoulos, Mavratos and Ellis who would disagree. I can't help noticing that string theorists rarely admit that these bogus "tests for string theory" are indefensible, preferring to instead personally attack me for pointing this out, invoking not science but citation counts in their defense.

9. **Eric**
August 25, 2007

   Peter,
First, the MAGIC results are very interesting and cannot be dismissed. Second, such an effect is of interest not just for string theory, but for quantum gravity in general including LQG. Third, Ellis, Nanopoulos, and Mavromatos did predict this effect several years ago, as you may discover on the arXiv.

   Regarding your statement that noone other than ENM takes these results seriously, how do you know? Have you talked with all string theorists and phenomenologists to get there opinion, or are you just relying on what you heard from Lubos and Distler?

   What's next, trying to undermine their credibility by mentioning that they once wrote papers with Hagelin?

10. **Peter Woit**
August 25, 2007

    Eric,

    If you want to provide us with a list of string theorists and phenomenologists who think that the MAGIC results give evidence for string theory and thus that the Slashdot headline is not nonsense, go right ahead.

11. **anon.**
August 25, 2007

    John Ellis and Dimitri Nanopoulos happen to also be the second and fourth most cited high-energy physicists

    This isn't so difficult when John Ellis single-handedly writes as many papers as any random group of ten other physicists. Writing a lot and getting cited a lot does not in itself make one a great physicist.

12. **hack**
August 25, 2007

” What’s next, trying to undermine their credibility by mentioning that they once wrote papers with Hagelin?”

In fact their papers with Hagelin were their high point. It’s been all downhill since then.

I’ve actually read more Nanopoulos/Ellis papers than I care to admit. I have to laugh when I hear Nanopoulos claiming to have predicted something. It’s a million monkeys with typewriters type of situation.

13. **milkshake**
August 26, 2007

Microtubules are huge proteins, maybe 4 orders of magnitude over than the scale where you can observe quantum effects with neutral molecules. (If somebody tells me that it is possible to see interference pattern by shooting tiny molecules like ammonia in vacuo through a double slit, and that scale of the effect can comparable with the actual size of ammonia molecule, I can believe that.)

Looking for quantum effects in the mechanics of a living cell is completely New Age.

14. **Luboš Motl**
August 26, 2007

I was told about this new article on this spamblog. You may visit my blog to see some clarifications of the statements made by the individual behind this spamblog.

15. **Brett**
August 26, 2007

It’s not clear whether Lorentz violation is a consequence of string theory or if it’s forbidden by string theory. The same holds for loop quantum gravity. However, if you’re going to look for signs of Lorentz violation, there are many ways to go about it, and looking for an energy dependence in the speed of light is not the most sensitive. Because of the vector character of light, the speed of light is such theories generally depends on polarization as well as energy. Searches for this kind of birefringence are much more sensitive than experiments that look for differences in photon arrival times. Indeed, I know that another high-energy telescope experiment already has much better data on photon arrival time differences, but they have refrained from publishing it, in part because it is not competitive with the polarization bounds.

16. **Who**
August 26, 2007

> Because of the vector character of light, the speed of light is such theories generally depends on polarization as well as energy.

Not true Brett, numerous papers about this from non-string QG researchers
have ruled out polarization dependence.

> Indeed, I know that another high-energy telescope experiment already has much better data on photon arrival time differences, but they have refrained from publishing it, in part because it is not competitive with the polarization bounds.

In that case they are doing the QG community a disservice by not publishing, because the polarization bounds are irrelevant. You should urge them to publish.

I assume you mean they have data which would constrain (if not rule out) energy-but-not-polarization dependence, and that is precisely what I see being discussed.

My sense is we have a ways yet to go with this issue.

17. **Lee Smolin**
August 26, 2007

Brett is right that polarization odd variations in the speed of light are already ruled out at planck scale by observations of polarized radio galaxies, but polarization even variations in the speed of light are not because they cause no bifringence. The latter are, however, a possible consequence of a deformation rather than breaking of Poincare invariance. If an experiment has data on photon arrival time variation with energy it must, because of the limits on parity odd variation, be parity even, and hence, if there is no other explanation, it could be a detection of a deformation of poincare invariance (so called “doubly special relativity”).

Thanks,

Lee

18. **Brett**
August 26, 2007

On what grounds can a polarization dependence in the speed of light be ruled out? One can make an assumption that quantum gravity will have certain features, such as no birefringence, but this will limit the terms in the low-energy effective action to a measure zero subset of the full parameter space of Lorentz invariance violation.

To me, it seems rather wishful thinking to hope that quantum gravity will have such a profound signature as Lorentz violation, while not interacting with the spin structure of the electromagnetic field. There is no compelling reason why this should be the case. The interactions that avoid birefringence deserve to be tested (and I strongly recommended that the data I saw be published), but they are only a peculiar subset of the possible Lorentz-violating interactions. One can always write down a nonrenormalizable interaction which all previous experiments have been insensitive to, but which a new configuration will test; yet selling it as a profound new test of quantum gravity is illogical. (And all renormalizable varying speed of light theories can indeed be bounded by birefringence.) And if you want to make a generic statement about how well Lorentz invariance has been tested, it behooves you to look at the best bounded sectors, not the worst.

19. **Amnetic**
August 26, 2007

> You may visit my blog t

But, Lubos, you have a message in your site saying you don’t want readers from this blog.

20. **sophia**
August 26, 2007

“If string theory were a vampire, he’d be Buffy.”

Hah. It's occurred to me that string theory is to real physics as a drag queen is to a real woman. Unlike real women, drag queens are expertly groomed, and beautifully made up, but when the moment of truth arises, who would you rather be with?

On the other hand, the results of this experiment are quite predictable. That's one thing drag queens have going for them, unlike string theories.

21. **Moshe**
August 26, 2007

Lee and Brett, regardless of the new and exotic phenomena that you are discussing, I see no reason Lorentz invariance violation at the Planck scale should be automatically a small effect at low energy. Most conservative estimates based on the existence of LV relevant and marginal perturbations to the standard model (I believe the number of those is 46) makes Lorentz violation basically already falsified, based on existing experimental results. Unless one finds a way to fine tune away a lot of really large violations I am not sure why we are discussing those tiny sub-sub-leading effects.

22. **Lee Smolin**
August 26, 2007

Dear Moshe and Brett,

Parity odd variation of the speed of light with energy is ruled out to at least order 10^-3 [l_{Planck} Energy] by observational limits on bifringence from polarized sources. See gr-qc/0102093. This is a prediction of lorentz symmetry breaking, therefor it is reasonable to infer that lorentz invariance is not broken at order l_{Planck}. But deformation of Poincare symmetry is another thing entirely, there is still a ten parameter global symmetry algebra constraining renormalization effects, so Moshe's considerations can be answered directly; these are the leading effects of deformed Poincare symmetry. Since the symmetry group is still present it does rule out as many terms as ordinary poincare symmetry, and one of them is a parity odd varation in the speed of light coming from the usual dimension five term seen in lorentz symmetry breaking.

To be more precise the Casimer invariant of the deformed Poincare algebra is no longer quadratic in energy and momentum, leading to corrections to the speed of light. Thus, deformed Poincare symmetry can imply an order l_{Planck} variation in the speed of light with energy, which is parity even and therefor not ruled out. Therefor if the right interpretation of the observations reported by the MAGIC collaboration is a modification of spacetime symmetry it must be a deformation and not a breaking of Poincare symmetry-because the latter is already ruled out by experiment, but the former is not. The same holds for the observations Brett hinted about.

23. **LDM**
August 26, 2007

Lee,

Regarding the paper gr-qc/0102093 (You also mention the same on page 226 of TTWP)…the assumption made in the paper

"If we assume that linearly polarized photons are
detected, and unambiguously identified with a source at cosmological distance z, without any significant interaction
in between, we may be immediately sure that (6) is not strongly violated."
is too large of an assumption, in my opinion.

Cosmological data has traditionally been the poorest data of all the sciences, and you want to draw definite and strong conclusions about physics at the Planck scale based upon it? This to me is very wishful thinking.

24. **Brett**
August 26, 2007

Moshe,
I agree with you about naturalness, which is why I think it's more important to concentrate on renormalizable forms of Lorentz violation. Indeed, there is no known reason why Lorentz violation, were it to exist, should be small. Lorentz violating interactions are, of course, technically natural, since they receive no radiative corrections from Lorentz-invariant physics, but if they actually exist, they must either be finely tuned, or they must be suppressed by some unknown mechanism.

The number of dimension three and four Lorentz-violating operators that can be constructed out of standard model fields is much larger than 46. With just one generation of fermions and the electromagnetic field, there are about 150. Of course, many of those mix under renormalization. The number of different symmetry types is still greater than 46 though. Actually, how many different physically meaningful operators there are depends on whether you consider only flat spacetime or whether you consider working in a curved background, and exactly how which terms are physical under which circumstances is not completely understood.

25. **Lee Smolin**
August 26, 2007

LDM,

I understand reservations about claims of positive results because there could be other explanations, but the claim in gr-qc/0102093 is a negative result. Do you believe that there could be leading order lorentz symmetry breaking which produces bifringence, which is then masked by some ordinary astrophsyical effect so that no bifringence is seen? What physics do you have in mind that could reverse rotation in the plane of polarized radio waves so its effects were not seen?

It seems to me reasonable to infer that lorentz symmetry breaking is not present, in this case the experiment and the more theoretical argument discussed here by Moshe agree.

Thanks,

Lee

26. **LDM**
August 26, 2007

Thanks Lee, probably I was a little ambiguous. Sorry. I will see if I can phrase this more precisely…

I am not arguing either for or against Lorentz violation, I am only arguing against using the cosmological data as you have done…

In the TTWP, page 226, you mention that the travel time can be "billions of years" for the photons in question. So we are talking about large distances.

Now, in the paper, we also have the statement *“comparing the time of arrival of rays at different energies emitted simultaneously from the same source, one can test the validity of this prediction”*

I am assuming that time of arrival that is being discussed is based on our distance from the source of photons. (If it is not, and you do not need to know the distance, then I am wrong, and please accept my apologies for wasting your time.) The problem is that cosmological distances are very uncertain, to quote from M Berry “Principles of Cosmology and Gravitation”:

“How do we know the distances and densities quoted? The Universe is charted by a sequence of techniques, each of which takes us out to a greater range of distances – to the next level of the ‘cosmic distance hierarchy’. Each level is less reliable than the last, so that there is considerable uncertainty about the measurements of very great distances.”

So, it would seem
we have considerable uncertainty in our data…but in TTWP, you are talking about measuring differences of 1/1000 of a second, which it seems to me is a fairly precise or certain measurement…The impression I have is we do not have that kind of accuracy. And so you cannot meaningfully use the cosmological data in the way you are attempting to, which is to measure differences of 1/1000 of a second over large cosmic distances.

27. **Moshe**
August 26, 2007

Thanks Brett, the number 46 came from the Coleman-Glashow paper, if the number is bigger I am even more worried…

Lee, the fact that global symmetries are preserved by renormalization, and therefore can be used to forbid otherwise possible interactions, this fact was established through a series of theorems in the 1960s and 70s, those theorems apply to ordinary global symmetries.

If there is some deformed version of Poincare symmetry it is then natural to ask if it is preserved by renormalization. If not it doesn’t give any restriction on the form of the low energy EFT. If it is preserved maybe it limits the allowed interactions in some way, but even then it seems to me it will take a miracle to allow small violations of LI while by forbid much the numerous much large effects.

28. **Eric**
August 26, 2007

LDM,
The different arrival times are for different gamma rays emitted at the same time from the source with different frequencies, and no you don’t need know the cosmological distance precisely. Once only needs the distance to be large so that the difference in arrival times is measurable. Essentially, the higher frequency gammas interact with the vacuum and slow down, just as a light ray does when going through some medium, otherwise known as refraction.

29. **Moshe**
August 26, 2007

garbled the last sentence, the last few words should be “while forbidding the numerous much larger effects”. Those effects refer to all the renormalizable terms Brett discussed, not just the dimension 5 operator discussed by Lee.

30. **anon.**
August 27, 2007

“And so you cannot meaningfully use the cosmological data in the way you are attempting to, which is to measure differences of 1/1000 of a second over large cosmic distances.” – LDM

I think you completely misunderstand. If two photons of different energies arrive 1 millisecond apart from the same gamma ray burster or whatever, the accuracy of that measurement (relative time of 1 ms difference) is independent of the less accurate cosmological distance ladder which estimates how far the gamma ray burster is from you. You simply don’t need to know exactly how far away the source is, in order to detect that photons are travelling at different speeds…

31. **LDM**
August 27, 2007

Thank you anon. .Yes, perhaps I misunderstand…but let me ask you, the two photons that arive 1 millisecond apart from the GRB, how do you know that the photons did not leave the GRB 1 millisecond apart too?
And more to the point that is bothering me, what are the error bounds on these measurements?

32. **anon.**
August 27, 2007

LDM, if the 1 ms delay is *not* caused by differences in photon speeds, then similar delay times whould show up in the time-dependent energy spectra of gamma ray bursters, regardless how far away they are. If the delay is caused by differences in photon speeds, then the furthest sources should show the biggest delays.

33. **Lee Smolin**
August 27, 2007

Dear Moshe,

I agree, an important question is whether deformed poincare symmetry is preserved by renormalization. My argument assumed yes, but you are right that this needs to be shown, I am not sure of the status of this in various approaches to QFT with deformed poincare symmetry but will check.

Dear LDM,

We seem to be at cross purposes, the paper gr-qc/0102093 does not use arrival times, it uses the absence of bifrengence in observations of polarized radio galaxies. The MAGIC claim does use arrival times, as do limits using lower energy gamma ray bursts set to M_QG. There are several ways this is addressed in the literature: 1) as anon mentions one can hope to get redshifts for enough events and see if there is a correlation with distance, 2) by using very short bursts and bounding the dispersion relation by the overall length of the signal, 3) by a better understanding of the source. None of these apply to the MAGIC claim.

The MAGIC paper uses another argument based on extremizing energy flux. Does anyone know how reliable this kind of argument is? Is it used elsewhere in astrophysics?

Lee

34. **Arun**
August 27, 2007

LDM, if the 1 ms delay is not caused by differences in photon speeds, then similar delay times whould show up in the time-dependent energy spectra of gamma ray bursters, regardless how far away they are. If the delay is caused by differences in photon speeds, then the furthest sources should show the biggest delays.

which would be detectable provided gamma ray bursters at all eras are essentially identical.

35. **Moshe**
August 27, 2007

Thanks Lee.

36. **alex**
August 27, 2007

Hi Lee,
I'm confused by the terms "deformation" and "violation" of Lorentz symmetry. It seems to me that if the lagrangian contains some terms which are invariant under the "deformed" Lorentz symmetry but NOT invariant under the usual undeformed Lorentz symmetry, such terms would therefore violate the usual Lorentz symmetry since they are not invariant under it, right?

Thanks!

37. **Bee**
August 27, 2007

Thanks for the link!

38. **Cecil Kirkey**
August 28, 2007

This thread is interesting but…Does ST predict a LI violation or not? What is the difference between critical and non-critical ST? Are they both derived from the same assumptions? If they are do we now have two families of ST? And if these ST guys predicting LI violation are not really preaching the true ST why don't the true ST guys stand up and refute them openly?

39. **Mr. String**
August 28, 2007

Cecil,
Non-critical string theory involves strings propagating in a dimension of spacetime less than the critical dimension of ten. The resulting anomalies are cancelled by exciting the Liouville mode (linear dilaton) of the strings. The statement that string theory strictly obeys Lorentz invariance is true only in the context of critical string theory.

40. **manyoso**
August 29, 2007

So which version of String Theory, critical or non-critical, to String Theorists believe corresponds to the real world? …oh wait

41. **sasay**
August 29, 2007

> This situation appears to have driven him over the edge.

Who is this Lubos that you mention? Is he affiliated to an academic institution? He seems to refer to L. Susskind with strange reverence, is he another squatter at Stanford?

42. **Hendrik**
September 10, 2007

A somewhat belated item has appeared in New Scientist on
the MAGIC "test" of string theory at:

http://www.newscientist.com/channel/fundamentals/mg19526204.300-finally-a-magic-test-for-string-theory.html

This Summer's Online Talks

August 27, 2007
Categories: Uncategorized

All sorts of schools and workshops occurring this summer have been putting up materials from the talks on-line. Sometimes this is just an audio recording of the talk, which can be very frustrating if you're interested in the details of a subject, leaving you desperately trying to guess what symbols on the blackboard correspond to the scratching noises and words from the speaker that you are hearing. Best is a set of slides used by the speaker, together with audio or video of the full talk. Some examples worth looking at include:

- String Theory and the Real World, this year's les Houches summer school.
- Cosmology and Particle Physics Beyond the Standard Models, this year's Cargese summer school.
- Summer School on Particle Physics, Cosmology and Strings at Perimeter.
- Simons Workshop in Mathematics and Physics at the YITP in Stony Brook. Definitely the worst offenders in terms of having interesting talks available, but audio-only. Blogger Aaron Bergman is there, but doesn't seem to be very interested in telling us what is going on.
- Anton Kapustin gave a Master Class on *Electric-Magnetic Duality and the Geometric Langlands Programme* at the CTQM in Aarhus this summer. Video of the talks is here. The KITP in Santa Barbara will be hosting a Miniprogram on this topic next summer.
- At CERN there's a program on New Physics and the LHC taking place. Suitably snarky commentary available at the Resonaances blog, starting with "the theory talks were ranging from not-so-exciting to pathetic", and going on to describe one of the experimental talks, which can't really avoid being exciting as less than a year remains before the LHC is supposed to start taking data. The experimenters at CERN are looking over their shoulder at the Tevatron, where Tommaso Dorigo reports that they are still not seeing a Higgs, but getting remarkably close to being able to rule out the existence of one at 95% confidence level for a mass range near 160 GeV. For a new compilation of Higgs mass predictions, see here.
- *One more, suggested by a commenter*: SLAC ran a summer school on Dark Matter: From the Cosmos to the Laboratory.

Off-topic, department of Humor: The New York media just can't get enough of theoretical physics these days, with the New York Observer running a column Ask a Theoretical Physicist.

Comments

1. **Shantanu**
August 27, 2007

Peter, you forgot to add this year's SLAC summer school on dark matter,where
almost all talks(except for the ones on the last day) have been archived.

2. **Aaron Bergman**
August 27, 2007

One 'n'.

And trust me, myself trying to relate talks is not a good idea for anyone involved.

3. **Peter Woit**
August 27, 2007

Sorry Aaron, fixed.

Well, you could at least be blogging about any scandalous gossip about physicists or mathematicians that you're learning…

4. **Thomas Love**
August 29, 2007

Peter said:
"Simons Workshop in Mathematics and Physics at the YITP in Stony Brook. Definitely the worst offenders in terms of having interesting talks available, but audio-only."

But it isn't audio-only! If you click on the titles in blue, you'll get a printed version.

5. **Peter Woit**
August 29, 2007

Thomas,

Problem is that the talks I was most interested in following, based on the speakers and their topics, were ones that are audio only.

6. **Chris Borokowski**
August 29, 2007

The "Ask a Theoretical Physicist" column appears to be the latest in a series of attempts to make fun of how incomprehensible the world has become to even the average educated person. Great find.

7. **Zathras**
August 30, 2007

The "Ask a Theoretical Physicist" column is just ripped off from the Onions "Ask a…." series, such as Ask a Man Getting Yelled At By His Wife Over The Phone At Work or Ask a Bee.

8. **Yatima**
September 1, 2007

This Dark Matter overview from SLAC's summer school certainly beats any BBC "science" emission. Good stuff.

http://www-conf.slac.stanford.edu/ssi/2007/talks/Turner_081007.pdf

This Month's Hype

September 3, 2007
Categories: Uncategorized

The September issue of Physics World is out, featuring a 13 page advertising supplement for string theory which is pretty much unadulterated hype. The same issue includes an editorial which takes the point of view that the only problem with string theory is that:

> String theorists need to do much more to explain their field's genuine links to experiment

String theory's lack of falsifiability is minimized as a problem, and the fact that it "raises several philosophical issues, such as the role of anthropic reasoning" is listed as a point in its favor. As for those who complain that string theory predicts nothing, in particular nothing about what will happen at the LHC, they are told to just shut up:

> With CERN's Large Hadron Collider (LHC) due to switch on next year, now is the wrong time to slam string theory for its lack of predictive power. While not able to prove string theory is right, the discovery of supersymmetric particles at the LHC would give it a major boost…

The fact that string theory doesn't predict supersymmetry visible at LHC scales is actually acknowledged in the advertising supplement by Kachru and Susskind.

The few quotes from string theory skeptics allowed seem chosen to be those that put string theory in the most favorable possible light (except for Phil Anderson, who is reduced to hostile spluttering by Polchinski's claims that string theory may explain high T_c superconductivity). This allows the editorialist to conclude:

> However, the richness of string theory that has become apparent in the last decade, and its increasing contact with the real world, gives theorists something to shout about. This is why our main feature on the subject, which started with fairly modest intentions, has ballooned into the longest ever to appear in Physics World. As the views of even many non-string theorists in the article make clear, the theory still holds all the potential it ever did to revolutionize our understanding of the universe.

The critique of string theory by Smolin and myself is pretty much completely ignored or dismissed, with Susskind quoted as having come up with a new insulting term for us (to him we're "Smoit", evidently he likes that better than the "Swolin" favored by those in Santa Barbara). The claim is made that

> few string theorists think that the sometimes negative portrayal of string theory in the popular arena recently has had much of an effect other than to irritate people.

Amidst the endless misleading hype contained in the Physics World piece, there's some that simply is demonstrably completely untrue. The most egregious example might be the discussion of Witten's Fields Medal which claims that it was awarded him due to his work on string theory compactification spaces:

> .. with the study of 6D "Calabi–Yau" spaces making Witten in 1990 the first physicist to be awarded the prestigious Fields Medal

The quotes from Witten himself don't include any of the hype about connections to experiment. He describes string theory as something very poorly understood, with even the fundamental equations of the theory unknown, and no good ideas about how to find them, leading to the danger that even if his vision is correct, realizing it may just be too hard:

> It's incredibly rich and mostly buried underground. People just know bits and pieces at the surface or that they've found by a little bit of digging, even though this so far amounts to an enormous body of knowledge… There is an incredible amount that is understood, an unfathomable number of details. I can't think of any simple way of summarizing this that will help your readers. But despite that, what's understood is a tiny, tiny amount of the full picture.. One of the greatest worries we face is that the theory may turn out to be too difficult to understand… This is certainly a question that interests me… but if I don't work on it all the time, it's because it's difficult to know how to make progress.

Unlike Witten, many of the other string theorists quoted seem to have no problem with issuing streams of highly misleading hype claiming "predictions" of string theory. For instance, from David Gross:

> String theory is full of qualitative predictions, such as the production of black holes at the LHC or cosmic strings in the sky, and this level of prediction is perfectly acceptable in almost every other field of science," he says. "It's only in particle physics that a theory can be thrown out if the 10th decimal place of a prediction doesn't agree with experiment.

I don't know how to characterize this kind of claim that string theory is as predictive as other scientific theories, just not able to get accuracy to 10 decimal places, as anything other than out-and-out dishonesty. If someone could come up with a legitimate, distinctive, testable prediction of string theory that gave even the correct order of magnitude for some experimental result, that would be a huge breakthrough.

Michael Green, while describing the landscape and its potential to allow for a small CC as "an enormous success" for string theory, is one of several string theorists characterizing the status of string theory as being just as good as that of QFT, with the landscape not a real problem at all, just a "supposed" one:

> This supposed problem with a theory having many solutions has never been a problem before in science.

Several people promote the anthropic point of view, with Susskind describing it as the third superstring revolution, one that is even more of a revolution than the others. Polchinski adds

> In terms of changing the way we think about the world, the anthropic landscape is certainly as big as the other revolutions

while Susskind's colleague Shamit Kachru is described as "in the middle", sensibly pointing out that it would have been a stupid thing for people to do, once they realized that the ratios of sizes of planetary orbits were environmental, to start claiming that "there is a deep anthropic lesson to be learned from Newtonian gravity."

All in all, I think that the picture the Physics World article presents of the reaction of leading string theorists to the failure of the superstring unification project is a depressing one. Instead of acknowledging in any way this failure and considering what can be learned from it, on the whole they seem to prefer to abandon science for anthropic pseudo-science, to spout misleading claims of bogus "predictions" of string theory, and make indefensible claims that the lack of predictivity of string theory is not unusual for a science.

On the other hand, among string theory skeptics, I fear that the attitude of Howard Georgi is all too common:

> I have been critical in the past of some of the rhetoric used by string-theory enthusiasts," says Howard Georgi of Harvard University, who coinvented the supersymmetric extension of the Standard Model in 1981. "But I think that this problem has largely corrected itself as string theorists learned how complicated string theory really is. I am concerned about the focus of young theorists on mathematical details, rather than what I would consider the real-world physics of scattering experiments, but with any luck the LHC will take care of that by reminding people how interesting the real world can be."

The problem with string theory is not too much mathematics and a lack of effort towards making connection to real world experiments, but that it is a wrong idea about unification, and thus cannot ever explain the standard model or predict what lies beyond it. The recent move among string theorists to hype bogus claims about connections to experiment, abandoning the search for greater mathematical insight into string theory as just “too hard”, retooling themselves as more salable “string phenomenologists” and “string cosmologists” is not a healthy trend. It is based on adopting the Susskind-Polchinski “multiverse” revolution in the received wisdom about how to do fundamental physics, slowly turning a once great subject from a science into a pseudo-science.

Update: Lubos is beside himself with glee over the Physics World article, see here and here (don’t miss the photo-shopped “Smoit” graphic of me and Lee Smolin). For something more reasoned, there’s a short piece at Wired.

Comments

1. **Domenic Denicola**
September 3, 2007

> few string theorists think that the sometimes negative portrayal of string theory in the popular arena recently has had much of an effect other than to irritate people.

They have a surprise coming to them, then. A general attitude of eye-rolling toward string theory is prevalent at the Perimeter Institute (at least, in the quarters I associated with), and almost every undergraduate I’ve spoken to at Caltech things string theory is just a “not even wrong” mess. The view seems to be “OK, maybe they have some idea what they’re talking about, but it doesn’t seem promising. I’ll let someone else work on that stuff, while I try something more likely to be correct.”

The point is, you guys have had an impact, so be sure you know that :).

2. **M**
September 3, 2007

The editorial contains the sentence: “String theory is guided by problems in the real world – for instance the entropy of black holes”. Who wrote this must be either a string theorist or a humorist.

3. **A.J.**
September 3, 2007

Peter,

I don’t usually get involved in the string theory arguments, but since it’s a 3-day weekend (and I’ve been working all weekend):

I think ‘t Hooft is right (and now I’ll put some words in his mouth). Theoretical physicists aren’t doing science or the public any favors by arguing in public, in layman’s terms, about the merits and de-merits of string theory. It’s just too easy for people who don’t understand the technical details to get wrong ideas about speculative research.

We’ve seen this in one direction for some time now, with many laymen being quite convinced that string theory was already a done deal. (I even remember a WPI professor, not a particle physicist himself, who told me when I was 17 that grand unified theories were pretty well-verified.) And now we’re seeing it in the other direction, with for example, many slashdot denizens (and Caltech undergrads apparently) convinced the string theory is basically a fraud. This isn’t much of an improvement; we still have people with firm opinions and no understanding.

I’m personally hoping that these two public impressions will eventually cancel out, leaving people with a more or less correct impression. String theorists do have their hands on something quite interesting, maybe even a good framework for building quantum gravity models, but their research is quite premature. (So it goes: it took mathematicians 3 centuries to prove Fermat’s Last Conjecture.) Said framework has a lot of problems, but most of these problems — no vacuum selection, landscape of solutions, no preferred extension of the Standard Model, no sensible computation of the cosmological constant — are shared by the effective field theory framework we’re currently using. The string theory framework might be the correct one, or maybe not. We don’t have any way of telling without experimental data. In this sense, I thought the Physics Today article wasn’t that bad.

So, let me make a modest suggestion: Scientists have to talk to the public, and they’re obligated to give some explanation of the ideas they’re working on. But it would probably be better for everyone involved if scientists and science journalists remembered that, when we’re talking about things we have no experimental evidence for, the question is usually vastly more interesting than the proposed solution. If I’m going to read about an attempt at computing the half-life of a de Sitter geometry, I want 75% of the article to be about dark energy, the sign of the cosmological constant, and why anyone is thinking about metastability of geometries with positive cosmological constant.

4. **Yatima**
September 3, 2007

Karl Rove, who famously claimed to have made the “reality-based community” irrelevant, seems to have landed his new job at Physics World, then? String theory as an analytical tool to study the quark-gluon plasma? And there is that famous E8 picture…

Gerard ’t Hooft of the University of Utrecht, who
shared the Nobel prize in 1999 for his work on electroweak
theory, thinks that discussions about the merits
of theories should be limited to professional circles.

But they already are! This must be an appeal to have the door to the ivory tower walled in from within. Or to shut down blogs?

5. **Dr. E**
September 3, 2007

The fact that they are resorting to calling Peter and Lee names says it all.

During the Einstein Bohr debates, I wonder if Einstein snarked “Beisenberg” or “Heisenger” or “Heisenbohr” or “Schreisenberg.”

Alas, there was a time when honor, reason, and logic ruled physics.

6. **woit**
September 3, 2007

Hi A. J.,

I agree with ‘t Hooft that the issues here are complex, and not really appropriate for a discussion at a popular science level. On the other hand, sometimes they are the subject of inappropriate mystification: no, you don’t need to work through the details of the KKLT mechanism to understand what the landscape problem is. At this blog I do try and provide a discussion of these issues at an appropriate level, and in the book I tried also to not dumb down things past the point where a serious, accurate argument was being made.

I certainly see lots of ignorant arguments used to condemn string theory, just as I see equally ignorant arguments used by string theory partisans. Maybe they cancel each other out. But I’m actually not really that interested in the question of how good the public understanding of the string theory issue is (although I’d argue it’s much better now than a few years ago, and I hope I had some role in causing that). What I do care about is the understanding of these issues among physicists and mathematicians, and that’s who the blog and the book are both aimed at.

It certainly would be much better if this discussion was carried on at a more serious level. Unfortunately, many well-known string theorists seem to have decided not to respond to serious scientific criticisms in a serious way, but instead to dismiss them by calling Lee Smolin and me names, and going to the press with the kind of less than honest hype that dominates the Physics World piece. I don’t think blog entries by undergraduates who don’t understand these issues well are a big problem. The kind of misleading claims being put out to the public by leading physicists like Gross are.

7. **anonymous**
September 3, 2007

Hi Peter,

Just wanted to say, great post.

Keep up doing your thing. 🙂

Anonymous

8. **Theo**
September 3, 2007

Hi Peter,

I, as I imagine many others, appreciate the time you take to put these issues into context and the commentary you provide on various articles/resources.

I am not at a level of understanding where I could put up novel arguments for or against string theory myself but I feel able to follow much of what yourself and others blog about. Reading the ebb and flow of ideas for and against the various theories being put forward to advance our understanding makes me feel a small part of history in the making (if only mainly at the level of spectator).

I would disagree with t’Hooft and others that it is not useful to have such discussions in public. “Layman” who can follow most of what is being said (or are even interested) are likely to be slightly above your average layman in terms of their knowledge of physics. Thus, on average, will be able judge the arguments in an appropriate way.

e..g I don’t have to be able to write an original paper concerning compactification on Calabi-Yau manifolds to appreciate the difficulties you raise in relation to the landscape – and if you did not discuss such issues I would possibly be swallowing string theory hype hook line and sinker. On the other hand I am sensible enough, as no doubt others are, not to take everything you say as gospel either, and take information from multiple sources to form my own opinion.

Theo

9. **Thomas Larsson**
September 4, 2007

AJ, what is there to understand? The simple fact is that every natural string theory suggestion (supersymmetry, extra-dimensions, 496 gauge bosons, non-positive cc, …) is pretty much ruled out by experiment. Even susy with two layers of excuses (broken, and with R-parity conservation to kill dim-4 operators) is disfavored by experiments at the Tevatron and elsewhere. Surely intelligent laypersons can understand that a theory which disagrees with experiments is in trouble.

In fact, the situation for string theory is quite similar to that of ether theory. In both cases, there is one thing that should happen but did not (ether wind/supersymmetry) and one thing that could not happen but did (photoelectric effect/positive cc). And in in both cases, the strong proponents include a Nobel laureate (Lorentz/Gross) and a top mathematical physicist (Poincare/Witten).

Another similarity is that it takes a long time for a flawed theory to die. If you look at the title of Paul Ehrenfest’s inaugural speech, you will see that ether theory was still alive in 1912, 25 years after the Michelson-Morley experiment and seven years after some crackpot patent clerk discovered special relativity.

10. **A.J.**
September 4, 2007

Peter,

I’m happy to leave the hype disposal to you. I’m glad that someone is doing it. But I’d rather see the science journalists doing it themselves. (My favorite posts here are your comments on mathematics.)

On a related note: that someone’s making jokes with your name and Smolin’s is normal human nature, however unfortunate. Reporting said joke in a “serious” publication, on the other hand, is odd and rather tasteless.

———

Thomas,

If you read more carefully, you’ll notice that I said essentially nothing about string theory as source of particle physics models, except to note that it shares many of the qualitative flaws of our current framework. I said it was a barely apprehended framework for building quantum gravity models. I don’t expect useful predictions about TeV scale particle physics from it; indeed, given how much of the framework is probably missing, I don’t see much reason to take such suggestions seriously.

What makes string theory interesting, to my mind, is that it’s a good context for testing out ideas about gravity, black holes, and quantum geometry. So our theoretical intelligent layperson ought to be skeptical when she reads articles about large extra dimensions, colliding branes, and TeV scale supersymmetry. But she should also feel a little wonder maybe at the computations of black hole entropies. Even though this behavior should be fairly universal, it’s amazing to see the correct constants coming out of a microscopic accounting.

In short, it's an interesting set of ideas. It's probably good for something. We should keep it mind, even if we choose to work on other things, since we gain nothing by trashing it. (After all, thinking about the problems with the ether theory helped Einstein find relativity.)

11. **Coin**
September 4, 2007

I said it was a barely apprehended framework for building quantum gravity models. I don't expect useful predictions about TeV scale particle physics from it; indeed, given how much of the framework is probably missing, I don't see much reason to take such suggestions seriously. What makes string theory interesting, to my mind, is that it's a good context for testing out ideas about gravity, black holes, and quantum geometry.

In your perception, is this what string theory researchers* are currently using the theory for?

Assuming for the moment that you're right about what makes string theory important: do you think it is possible for string theory to produce useful progress and insights if, rather than attempting to attack it as a framework for testing out ideas about gravity and quantum geometry, those working on it are approaching it as a model for predictions about particle physics, or as a set of ground rules that lets you build 10^500+ configurations of Calabi-Yau manifolds [one of which is expected to describe the Standard Model], or as some other similarly literal interpretation of the theory?

* Given that I'm sure it is silly to try to think of "string theory researchers" as a homogenous bloc.

12. **Thomas Larsson**
September 4, 2007

AJ, perhaps it is because I am a physicist that I react strongly when people ignore that every testable string theory prediction is plain wrong. However, I feel strongly about this, and something that has been suppressed for 25 years. Ether theorists didn't just give up after the Michelson-Morley experiment neither – after all, ether theory was the best developed theory of light and arguably the only game in Newtontown.

Besides, string theory is hardly a successful theory of quantum gravity, because such a theory must explicitly involve the detector's properties. Every physical experiment is an interaction between a system and a detector, and the result depends on the physical properties of both. Typically we want to extract only those aspects of the experiment that are independent of the detector, which means that we implicitly assume that the detector's charge is small (so we can ignore its backreaction on the fields) and that its mass is large (so we can measure both the detector's position and velocity to arbitrary precision). This assumption, which is built into theories like QFT and string theory, clearly breaks down for gravity where mass and charge are the same.

13. **milkshake**
September 4, 2007

The Five Stages of Coping with Catastrophic News

1. Denial 2. Anger 3. Bargaining 4. Depression 5. Acceptance

http://en.wikipedia.org/wiki/K%C3%BCbler-Ross_model

14. **Chris Oakley**
September 4, 2007

Oddly, "swoit" and "swolin" both appear in the Book of Zweig, but as verbs. If I may quote:

> 25. And bravely did Lubos of Bohemia join the fray. And he laughed as he smote the Pope's enemies. And he was tireless in his smiting, and rested not by day and not by night. "Loons! Feminists! Communists! Anti-Science Crackpots!" he called from his steed known as Reference Frame, emblazoned with the Harvard University crest. "With my Sword of Truth and Mace of Righteousness I will punish ye for your impiety and ignorant foolishness!"
>
> 26. But so eager was Lubos in his smiting that soon the people did say, "Who is this Czech knight who doth smite so eagerly and so indiscriminately? For are not many of those he doth smite, and with such tirelessness, learned men, whose schooling in scientific arts oft exceeds that of the upstart himself?"
>
> 27. And the Pope of the Superstringers, beholding the smiting of Lubos from afar, and the dismay of the people, summoned him to the Land of Princeton to take counsel.
>
> 28. "Lubos," the Pope did say. "Of thy loyalty I have no doubt. By thy wisdom I do question. Thou must understand – our enemies are subtle. Doing battle with them is oft foolish as they will merely retreat only to reappear with a host of the ignorant taxpaying public, who they have indoctrinated with their lies. Against this host, we cannot hope to prevail. For thy own good, I must send thee back to the Land of Bohemia. Continue thy smiting if thou must, but sport not longer the Banner of the Land of Harvard. Go!"
>
> 29. But as he departed, Lubos had to run the gauntlet of ignorant hordes who the Anti-String had stirred up. Swoiting Lubos with angry taunts as he passed by were science journalists, and non-String physicists, swolin with righteous wrath. Lubos was incensed that those who understood not even the meaning of a Calabi-Yau manifold should even dare to have an opinion on the sublime Stringy arts.

15. **Chris Oakley**
September 4, 2007

Correction: swoit->smoit

16. **Matthew Chalmers**
September 4, 2007

Dear Peter

Is it really true that in writing 12,000 words about the status of string theory I have only made one mistake? That's the best news I've heard all day! Perhaps you can do better after a second or third read (try the more technical 750 word text-box titled: "Why can't string theory predict anything?" on p42 for starters).

On a more serious and hopefully constructive note, however, your post raises important issues about the role of balance (and thus objectivity) in science journalism and blogs. Journalists, who normally don't have an axe to grind and can therefore go into a new field with an open mind, often aim to achieve balance by seeking a mixture of expert opinion (for example, by printing the quotes of 10 eminent non-string theorists alongside those of 11 string theorists in an article surveying the scientific status of string theory). Because blogs tend to be read by narrower audiences that hold similar views as the author's, balance is presumably less of an issue.

However, I'm sure that anybody interested in the current debate surrounding string theory would still be interested to hear your reasons for deciding not to even mention RHIC in the context of the Maldacena conjecture when you "evaluate" string theory in the second half of the paperback version of Not Even Wrong, for instance. Could this be the same logic that prevented you from acknowledging the presence of three other articles in the same September issue of Physics World (pp14–19) that were written by yet more non-string theorists, in particular by philosophers of science? These pieces tackle anthropic reasoning and the issue of testability head-on, and may help your readers reconcile your own somewhat perplexing stance on string theory — i.e. if the theory is, as you routinely point out, unfalsifiable then how can you be so certain that it has already failed?

Oh, and one last thing: next time you want to make a point, get out there and find your own quotes to back it up.

Matthew

17. **Cheeky Bastard**
    September 4, 2007

    Since Matthew Chalmers is gracing this blog with his presence, maybe he could elaborate a bit on what 't Hooft actually said. Was it literally that "discussions about the merits of theories should be limited to professional circles", or is this an extrapolation? It does sound dangerously close to "shut up an pay", a position which, when taken by a public employee addressing the tax payers, has only one legitimate answer: you're fired.

18. **JE**
    September 4, 2007

    How can one be so sure that it has already failed?

    Well, just because in mainstream scientific reasoning the terms "unfalsiable" and "physical theory" had been always considered as mutually exclusive.

19. **Arun**
    September 4, 2007

    Remarkably, after nearly 40 years, we still don't know what string theory truly is," exclaims Gross. "From the start, string theory was a set of rules for constructing approximate solutions in some consistent classical background – and that's all it still is."

    How then can it be called a theory of quantum gravity?

20. **Arun**
    September 4, 2007

    What I quoted above is at odds with

    The most important is that string theory provides a finite (i.e., non-divergent), consistent, quantum theory of gravity that reduces to general relativity at large distances and low energies.

21. **Jon Lester**
    September 4, 2007

    I am somewhat worried about this hype by Physics World. It implies that this community of theoretical physicists is indeed very powerful to manage information (beside positions). This situation has no precedents in the history of physics.

    It will not be a beatiful result of our generation to be remembered as the worst ever.

    Jon

22. **woit**
    September 4, 2007

    Matthew,

    I can't help pointing out that the mistake you made about Witten's Fields medal indicates you didn't bother to read my book, since there's a whole chapter in it about the subject of Witten's contributions to mathematics and what he won the Fields Medal for. It does appear that you skimmed it enough to notice that it doesn't contain mention of the latest string theory hype about heavy-ion physics. There are a couple reasons for that, the most important being that the book was written in 2002, and the hype about this subject began in 2005, by which time I'd found a publisher willing to publish it, and the manuscript was being copy-edited and out of my hands. If I were writing the book today, I suppose I would discuss the subject, including the awarding of a "Pinocchio award" by a heavy-ion physics expert to string theorists for their misleading claims about this that you uncritically repeat.

    As for why I didn't discuss the other articles about string theory in the same issue of Physics World, the reason is also pretty simple: I don't have access to them since there appears to be no online access available through my institution (Columbia) and I'm not a member of the IOP. I wrote about what I have access to, if someone is willing to send me copies of the other articles I'd be interested to see them and probably would write about them then.

    "if the theory is, as you routinely point out, unfalsifiable then how can you be so certain that it has already failed?"

    You're not making any sense here. A scientific theory that can't be falsified is a failed scientific theory. The argument, which does require getting into technicalities, and involves making informed scientific judgments, is about whether there is any hope of the current situation changing, and getting falsifiable predictions out of string theory. I've argued here at length why I think this is hopeless, others have different opinions.

    I very much respect the large amount of work you did on this, including the many people you talked to and the large number of relevant quotes from experts that you gathered. That's not what I do though. When I'm writing about string theory as science here on the blog, I try to stick to writing about things that I actually understand, and what I write is based on my own judgments and my own understanding. People who disagree with these often write in here to do so, and that often leads to an interesting discussion. When I write about the public perception and debate over string theory, sure, I discuss what string theorists are telling journalists. Again, thanks for all your work on getting this sort of material together.

    Much of my comments on your article were aimed not at you, but at the many string theorists who talked to you. I find a lot of their claims outrageous, and their willingness to make highly misleading and overhyped claims about the state of string theory disturbing. At the same time they are doing this, other string theorists often complain that string theory has gotten a bad rap just due to the over-hyped claims made for it in the past. Anyway, it's your job to accurately transmit what experts have to say to the public, and I have no problem with your role in doing that.

But, for some reason you decided in your article to strongly take one extreme side of a scientific controversy. That's rather unusual, and I think it deserves a commentary like mine that points this out. You not only ignored the arguments on the other side of this controversy made by Smolin, me and others, but you adopted a framing of the issue that I think even most string theorists would find dubious. The claim that string theory is "rooted in experimental data", and your emphasis on the LHC doesn't reflect my impression of the opinions of most string theorists, who readily admit that the LHC can't in any standard way "test" string theory, and that the tenuous connection of string theory to experiment is a huge problem. It appears that you didn't bother to contact anyone who is actually an LHC expert, either the people working on the experiments or the theorists with expertise in analyzing collider data. If you asked such people what they thought of the idea of the LHC "testing string theory", you might have gotten some strong quotes disagreeing with the thrust of your article.

23. **Bee**
September 4, 2007

"It's only in particle physics that a theory can be thrown out if the 10th decimal place of a prediction doesn't agree with experiment."

A) Nobody 'throws out' a theory that agrees in 9 decimal places. One might want to clarify in which limit it is suitable.

B) Ever heard of General Relativity? I believe this blog has plenty of readers that would be thrilled to hear about a deviation in the nth decimal place.

The above Witten quotation about digging out knowledge leaves me wondering: if string theory is not the looked for ToE, then what is it? And should one dig it out whatever it is?

Besides this, I guess we should be grateful your name is not Peter Wirch.

-B.

24. **another anon.**
September 4, 2007

Dear anon.,

About the 10^500. One thing which is known to all experts, but you will not be told on this blog, is that in the most studied case where the 10^500 means anything, and also where the number came from, (technically, this case is called IIB flux compactifications), for many (not all, but a lot of) physically relevant quantities the `10^500 different cases' collapses into a single unknown number.

The effect of these 10^500 choices on low-energy physics then collapses into the presence of a single unknown number in the low energy theory. Big deal – you have one number you need to fix by hand, and then (for a lot of problems) your 10^500 has gone away.
So the notion that the 10^500 solutions makes the theory unpredictive is not at all true and is in fact highly misleading. Indeed, people who work on this area are currently very excited rather than depressed.

This situation is present all the time. The Standard Model has an *infinite* number of possible varieties – there are twenty or so numbers that have to be fed in by hand. Of course this doesn't make the Standard Model not science.

Another anon.

25. **alex**
September 4, 2007

2anon:
I'm not sure if I can answer your question completely but here is what I know. In the original KKLT construction, the integer fluxes which fix the values of the complex structure moduli enter through the so called flux superpotential W_0. Now, W_0 is indeed a very complicated function which can take on an exponentially large number of discrete values. However, if you read string phenomenology papers (by Choi et al. for example) based on the KKLT constructon you will notice immediately that the corresponding particle phenomenology always depends on the fluxes through a single parameter – W_0 and not on the individual flux contributions.
Hence, in practice, all these comlicated flux configurations, while useful to tune the cosmological constant, completely decouple from particle physics.

26. **woit**
September 4, 2007

Note: "another anon" is responding to a comment by "anon" that I deleted. The first "anon" is someone who specializes in submitting off-topic comments here attacking string theory and string theorists, usually in an uninformed way. I have to waste a lot of my time deleting them, and I'm really, really unhappy about this. This whole business of overuse of anonymous comments is annoying. If you want to make a scientific argument here, there rarely are good reasons for you to not be willing to put your name to them.

Please, anyone who is not happy with string theory or string theorists, I ask you to not use my blog comment section as a place to vent about this. If you don't have something well-informed and relevant to say in a comment, don't post it. There's more than enough very pointed criticism of string theory and string theorists going on here, adding your own adds nothing, and if it contains inaccuracies, all it achieves is to give string theory partisans a reason to dismiss the serious arguments being made here.

As for the arguments of "another anon", they have nothing to do with the topic of this posting, and are an argument against a straw man argument I've never made. Yes, the situation with the landscape is far more complicated than the one specific compactification mentioned. If you look at one very specific compactification and just vary the fluxes, sure you can't get anything you want. But you also can't get a prediction of anything, because a different choice of Calabi-Yau, branes, other background information will give you something different.

I'm afraid I'm going to cut-off any more discussion of this right now since it has nothing to do with the topic of the posting, the Physics World article, unless you are claiming that the study of flux compactifications makes string theory testable at the LHC, in any standard usage of the term "testable".

Also, please do me a favor and allow a few minutes after an uninformed comment by "anon" attacking string theory appears before responding to it to give me a chance to delete it. I know arguing with "anon" is a lot easier than arguing with me, but still. The line in his comment about how I was going to delete it should have been kind of a giveaway in this particular case.

Also, please consider putting your name to what you have to say, unless you have a very good reason not to.

27. **DB**
September 4, 2007

From the editorial:

> But string theory can be criticized for how it has promoted itself. Since the mid-1980s, many string theorists have oversold their subject by making grandiose claims about a "theory of everything". Although that tendency has disappeared, it no doubt diverted some physicists from other, potentially more useful, lines of research in theoretical physics. Meanwhile, string theorists have not responded well to recent attacks based on the theory's lack of testable predictions, most preferring to keep quiet rather than to engage in debate.

That's pretty serious criticism actually, given the perennial shortage of resources in particle physics, and the standards of integrity expected of scientists.

28. **JD**
September 4, 2007

Peter:"unless you are claiming that the study of flux compactifications makes string theory testable at the LHC, in any standard usage of the term "testable"."

In Type IIB flux compactifications one can derive a reliable prediction for the pattern of the gaugino masses. See Nilles and Choi for the related work.

29. **Thomas Larsson**
September 4, 2007

JD:

So you are claiming that the non-discovery of gauginos at the LHC will disprove string theory 🙂

30. **Matthew Chalmers**
September 4, 2007

Peter – a quick skim through the Notes pages of your book reveals that you should be awarded a Pinocchio prize yourself (e.g. you appear to have had no trouble in fitting in an Edge quote from Phil Anderson in 2005, nor in quoting from Krauss's 2005 book, Randall's 2005 book or Susskind's 2005 book). Recall that the APS announcement of the RHIC result was in March 2005, with plenty of hints coming out from late 2004.

As for my not making any sense about your stance on string theory, you should consider broadening your philosophy of science slightly by reading Lakatos and others. Your entire attack on string theory seems to me to be based on a convenient but narrow and overly simplistic Popperian view of testability (i.e. falsifiablility) that is simply one interpretation of how science progresses — and certainly not the consensus view among philosophers of science.

Stringscape is not an attempt to give an overview of the "debate" surrounding string theory based on echoing yours and Smolin's arguments while completely ignoring the views of string theorists and other theoretical physicists. That has been done to death in the last year or so, as I set out in the first few paragraphs. Rather, the article is an attempt to bring something new to the debate by giving Physics World readers a non-partisan overview of where string theory — a scientific research programme that, for better or for worse, dominates current research in fundamental theoretical physics — stands today. (As it happens, the other three more interpretive string-related articles in the September issue do address the broader anthropic etc debate, although none mention your blog or book I'm afraid).

My article could of course have turned out quite differently if all 10 of the non-string theorists I interviewed had been more critical. But they weren't. Similarly, I could have turned up at Strings07 in Madrid to find a bunch of deluded nut-bags dealing only in abstract mathematics and talking in tongues. What I did find, however, was a bunch of mostly extremely able theoretical physicists trying their damndest to understand nature at its most basic level. The overwhelming impression I got, and which I have tried to convey in the article, is that string theory is simply not yet developed to the point where it can make the "falsifiable" predictions that physicists need before they can know for sure whether it is a viable physical theory. Rather, it is a compelling framework for unification that also happens to have all these great applications in mathematics, black-hole physics and, most importantly for physics perhaps, quantum field theory.

You still don't seem to get my earlier point that as a journalist I have no vested interested in whether string theory is portrayed in a positive or negative light. Since you claim only to blog about the things you understand, perhaps you should find something else than science journalism to criticize — after all, it's not that hard an approach.

31. **JD**
September 4, 2007

"So you are claiming that the non-discovery of gauginos at the LHC will disprove string theory "

Please point to the phrase where I made such a claim.

32. **Peter Woit**
September 4, 2007

Matthew,

Thanks for raising the level of discussion here by calling me a liar. One of the depressing things I've learned during this string theory debate is how many people think the correct response to legitimate criticism of the content of something they have written is to launch a personal attack.

Some of the things you mention were among a small number of additions and changes made to the text in November 2005, after it was copy-edited, before it went to the typesetter (the manuscript was delivered to the publisher in the summer of 2005). Sure, by that time people were discussing the application of AdS/CFT to heavy-ion physics, but it only became a major talking point for string theorists starting in 2006. I made no claim in my book to deal with every overhyped claim made for string theory, doing so would have made it a much longer book.

This issue in any case has nothing to do with what I was criticizing in my book, the failure of string theory as an idea about unification. If you look at page 192 of the book you'll see this subject referred to as

> …a much more promising area, that of trying to find a superstring theory dual to QCD. The AdS/CFT correspondence described earlier provides some hope that progress can be made in this direction…

which remains an accurate description of the situation, minus the hype surrounding the attempt by many people to use rather tentative results about heavy-ion physics to deflect attention from the failure of the string theory unification program, which is something quite different.

The other thing I get a lot of from people I've criticized accurately is sneering put-downs about how ignorant I am ("you should consider broadening your philosophy of science slightly by reading Lakatos and others"). Actually I've read Lakatos (and others), and am well aware that "falsifiability" is a tricky subject. If you read my book (you really should do this, you might learn some things) you'd find a long discussion about the problems with falsifiability and a lot of evidence that I don't have a "convenient but narrow and overly simplistic Popperian view" of this issue. What counts as a convincing test of a scientific theory is a tricky issue, but it is uncontroversial that there is no such thing possible for string theory unification at the moment, and, as I said, I've argued extensively that any hopes for this in the future have little to back them up other than wishful thinking.

Finally, I don't know or care whether you have a vested interest in string theory or not. Honestly, I know nothing about you beyond having read the 13 pages that you wrote. What I wrote here was a response to claims made in those 13 pages, by you and people that you interviewed, many of which are highly misleading. I'm willing to debate those claims further if you want, but I really think you should start by stopping personal attacks on me, and informing yourself what my views on this subject actually are, rather than making up naive and uninformed ones to argue with.

33. **Peter Woit**
September 4, 2007

JD,

I already spent a lot of time looking into claims of "predictions" of gaugino mass ratios that refer to Nilles and Choi. These turned out to be quite misleading, see the discussion at Cosmic Variance, one of my relevant comments is

http://cosmicvariance.com/2007/03/31/string-theory-is-losing-the-public-debate/#comment-241803

As far as I can tell, the situation here is just like in anything else in "string phenomenology": for certain specific classes of models you have constraints on what you'll see, but there are no general constraints. If you don't see one particular pattern, you can find another class of models that will give you what you want.

34. **Thomas Larsson**
September 4, 2007

JD:

You used the phrases "LHC", "reliable predictions", "gaugino masses" in you previous post. But of course this does not mean that string theory reliably predict gauginos at the LHC, only that you want to create such a false impression.

35. **JD**
September 4, 2007

Peter. You had said: "unless you are claiming that the study of flux compactifications makes string theory testable at the LHC, in any standard usage of the term "testable"."
I've read the discussion you pointed to and then looked at my original post and I see no problem. I still claim that flux compactifications (discussed in Nilles and Choi) yield robust predictions for patterns of gaugino masses.

I agree that there are no general string theory constraints on how SUSY is broken but since you specifically mentioned flux compactifications, I have to say that the corresponding phenomenology has been worked out.

For Thomass Larsson: It is well known that in its current state, string phenomenology does not give a definite answer about the scale of SUSY breaking, just like the standard model without experimental input does not predict the higgs mass or the top quark mass. So what? If we are lucky and the superpartners are discovered at the LHC we may be able to identify the particular class of string compactifications by studying the pattern of gaugino masses, why is this so controversial?

36. **chris**
September 4, 2007

another anon., this is really priceless argumentation. it reminds me of a seminar i once sat through by some few body model-builder. it involved 'free' quarks with pion exchange ans should explain the nucleon spectrum. this guy claimed he had no free parameters whatsoever.
finally, he had to admit, that the model contained an arbitrary function describing the interaction potential. he pulled some arguments that no, this means no free parameters and this function almost presents itself.

but he was just doing phenomenology. so i excuse him even if he needs infinitely many parameters to describe what essentially is a theory with one parameter (qcd in the massles limit). but hey, wow! the lanscape can do that too. i am seriously impressed 🙂

37. **JD**
September 4, 2007

Chris, I think you got confused about another anon's point. Your analogy is incorrect. Read the KKLT paper and you'll understand what another anon meant.

38. **M**
September 4, 2007

Matthew, I would like to comment on your sentence: "The overwhelming impression I got, and which I have tried to convey in the article, is that string theory is simply not yet developed to the point where it can make the "falsifiable" predictions".
String theory started with the hope that maybe quantum gravity implies testable predictions on observable low energy physics. String theory was partially developed, and a more realistic and pessimistic expectation emerged. No matter how much you develop the theory, you will never get any prediction, if no prediction exists.

39. **CCDGator**
September 4, 2007

Dr. Woit,

I'm not sure if this is the appropriate place for this, but I'll post anyway. I am not a specialist on one side of the debate or the other (I'm a chemist who knows a little bit about such things as quantum mechanics, symmetry, and group representation theory). I read your book during the summer and found it to be quite an eye opener and well worth reading. I was previously under the impression that "M-Theory" was some kind of unification of several string theories and in some sense a complete theory. I enjoy looking at your blog from time to time.

Thank you.

40. **Bee**
September 4, 2007

Hi Peter:

Sorry for abusing your hospitality, but in case anybody drops in here after reading Lubos' totally distorted summary of this comment section, I want to point out that I've never said David Gross is a moron (as Lubos has managed to read, I wonder if he's taking pills to achieve that?), the comment above was addressed to Peter (you might recall – the guy who writes this blog) and the words 'your name' refers to 'Peter Woit's name'. One should have thought this is obvious.

I just want to leave that clarification here since I'm afraid Lubos will delete my comment at his post. I should add that I am pretty much pissed off by so much unjustified viciousness, and I am (eventually) going to remove links to TRF (what I should have done much earlier I guess).

Best,

B.

41. **Peter Woit**
September 4, 2007

Bee,

Surely you should know by now that paying any attention to Lubos is virtually always a big mistake. The Chalmers article seems to have gotten him really excited, finally seeing his point of view reflected in the media, after a year of having to read much more accurately skeptical pieces about the problems of the theory.

The only surprising thing he says though is his claim that PI offered him a job. If someone who knows what's behind that story wants to tell me about it on deep background, I wouldn't mind…

42. **dan**
September 4, 2007

"The problem with string theory … it is a wrong idea about unification, and thus cannot ever explain the standard model or predict what lies beyond it."

Peter,
Would you still stand by this statement if LHC and other experiments support the idea of both theoretically and experimentally, the idea of something like a SUSY-SO(10) GUT, and as such, could be embedded within a string theory?

43. **ori**
September 4, 2007

It is amusing to see a group of people with nearly zero knowledge in high energy physics arguing whether a group of Nobel proze winners is correct or not. Who should I believe ? 🙂

44. **ori**
September 4, 2007

proze->prize …

45. **ori**
September 4, 2007

A quote from Bee :

" Ever heard of General Relativity?"

Well, should we tell David that you think he has not heard of GR ?

46. **Peter Woit**
September 4, 2007

ori,

Thanks for making it clear that Bee's comment

"so much unjustified viciousness" is an accurate description of other string theorists besides Lubos Motl.

Besides the insults, do you have any commentary on the actual blog posting?

47. **Matthew Chalmers**
September 4, 2007

Dear Peter

The reason why I decided to post a comment on your blog this morning, in response to your labeling an article that I had written a dishonest and misleading advertising supplement based almost entirely on unadulterated hype, was to raise a couple of points about the differences between blogging and journalism (and how the two interact with one another). This is a topical issue for those of us who work in traditional media outlets — and presumably for those who read and write blogs too — and has already led to some interesting episodes between scientists and journalists.

A great example of this was the "bump-hunting" saga at Tevatron earlier this year, which led to an article in New Scientist magazine (see http://physicsworld.com/cws/article/print/27731). Once high-energy LHC data begin to pour in a year and a half or so from now, and blogger-members of the Higgs groups in the ATLAS and CMS collaborations start getting excited, it will be a real challenge to know when best to break the story of a possible Higgs or some other discovery. Indeed, journalists aside, these large collaborations are going to have to deal with problems of their own involving how to manage the blogging activities of physicists.

In retrospect, however, Not Even Wrong was probably not the place to try and have such a discussion, given that, for instance, you are not actually involved in string theory/high-energy physics research yourself. And since our brief exchange has turned into pointless pedantry and apparent confusion over what is personal and what is fair-game, I would guess that readers are not going to get much more from it either.

More to the point, it is late and it feels as if I have been looking at that bloody UA1 W-event all day. This stuff must tie up hours of your time!

Matthew

PS I posted a hard copy of the September issue to you last week, in which you can read the other articles that I mentioned – that is, if you're at all interested in taking someone else's opinion on string theory on board.

48. **Peter Woit**
September 4, 2007

dan,

Sure, I'll stand behind that statement until someone comes up with a plausible way of getting real predictions out of string theory. I think the great hope of some string theorists is that the LHC will see not only supersymmetry, but a pattern of supersymmetry breaking that corresponds distinctively to a certain kind of string background. Based on this they might then be able to make real testable predictions. This seems to me to be nothing but wishful thinking, we'll find out in the next few years.

Turning the question around, do you think string theorists are willing to change their minds about string theory if no evidence of a supersymmetric GUT turns up at the LHC?

49. **Peter Woit**
September 4, 2007

Matthew,

To be accurate, I did not describe your article as dishonest. Unadulterated hype swallowed whole from experts spouting it, yes. Dishonest, no. The one thing I described as "dishonest" was the quote from David Gross. He's one of the greats of the field, with tremendous accomplishments to his name, but the quote was not an honest characterization of the situation.

Actually, I do spend large amounts of my time working on trying to find new mathematical techniques in quantum field theory, which is a form of research into HEP theory. Unlike you, I have a Ph.D. in the subject and have devoted much of my life to its study. But if it makes you feel better to attack me personally and imply that I don't know what I'm talking about, go right ahead.

The relation between blogging and journalism is certainly an interesting and complicated topic. Maybe we can discuss it some other time, in a less heated context. Yes you're right that sometimes this blogging business takes up too much time. It's our first day of classes here and I should be preparing my course.

Thanks for sending me a copy of Physics World, I'll look forward to seeing it.

50. **Bee**
September 4, 2007

ori: as I've said over at trf, the comment above is what it is, namely a 'comment'. It comments on the the statement made, which I found disturbing in its generality, and is addressed to the readers here, including the author of this blog. when I wrote the above, I wasn't even aware who was quoted, but that doesn't change that the comment is in my opinion appropriate. if you want to make a complete fool out of yourself, go ahead and tell David Gross that you think I think the message didn't get across very clearly the way he was quoted.

besides this, Lubos has changed the 'moron' into 'completely silly' and I guess his post will undergo further fly-by changes. I honestly don't have time for that crap – I certainly don't want to end up in Pilsen or something, so I better go back to work now.

Best,

B.

51. **Tony Smith**
September 4, 2007

Peter Woit said to Matthew Chalmers "… Unlike you [Matthew], I [Peter] have a Ph.D. in the subject ..".

According to physicsworld.com
"… Dr Matthew Chalmers is Features Editor of Physics World. He joined the magazine in 2002 after completing an MSc in science communication at Imperial College London. He obtained both his physics degree and

his PhD in particle physics from Glasgow University,

after which he spent a year working in quantitative finance in Amsterdam. …".

Also, I would like to know Matthew's personal opinions about the stability of fancy-math finance hedge funds etc in the event that the USA housing market declines and oil prices increase,
and
(to stay on-topic) how it might compare with the stability of conventional superstring theory in the event that LHC finds no sign of supersymmetry.

Tony Smith

52. **jhk**
September 4, 2007

"It is amusing to see a group of people with nearly zero knowledge in high energy physics arguing whether a group of Nobel proze winners is correct or not. Who should I believe ?"

Which group of Nobel proze winners would that be Ori? Gross and Weinberg, or Glashow and Anderson? Who should I believe?

53. **Dan**
September 4, 2007

Of course string theorists would continue hyping string theory in the event of an LHC null result, but aren't susy-extentions of the SM, and GUT models also suffer from a lack of predictivity, and is as unable to predict what the LHC will see, in detail, as string theory?

54. **Peter Woit**
September 4, 2007

Tony,

As I wrote to Matthew, I know nothing about him besides the fact that he's the author of that article. So, good journalism practice would have indicated that I should have checked before I wrote that he didn't have a Ph.D. in particle theory. Upon further investigation, it turns out that he does have a Ph.D, but in experimental, not theoretical particle physics.

And please, don't even try and carry on a discussion of prospects for hedge funds here…..

55. **Peter Woit**
September 4, 2007

Dan,

I've never been a fan SUSY extensions of the standard model, or GUTs, precisely because they aren't very predictive. For SUSY, the problem is that don't know what breaks supersymmetry, and this ruins predictivity. Non-susy GUTs are actually better since they often make more robust predictions. The SU(5) non-supersymmetric GUT was so good on this score that it was falsified (no proton decay at predicted rate).

56. **Eric**
September 4, 2007

Peter,
Aren't GUTs and supersymmetric theories only qualitatively predictive at low energies because we lack information about the theories at high energy? Would the standard model not have the same problems without experimental data which is input into the theory? Does the fact that they are unable to predict everything from first principles mean that they aren't science? In your myopic definition of science this is apparently the case. The point that has been made to you over and over is that string theory is no different than this. The only reason we are currently unable to make detailed predictions from first principles is that we lack knowledge, not because there's anything fundamentally wrong with the theory. In point of fact, string theory is the right direction and I don't think anybody who works on it needs to apologize for this.

57. **Peter Woit**
September 4, 2007

Eric,

No, the problem with supersymmetric theories is not that "we lack information about them at high energies", it's that you have to break the supersymmetry. This is not a "high energy" effect. Non-supersymmetric GUTs are often quite predictive, in the SU(5) case so much so that it was falsified.

"Does the fact that they are unable to predict everything from first principles mean that they aren't science?"

No, they're science because they make a lot of testable predictions that have been confirmed. The problem with string theory is not that it can't predict everything from first principles, but that it can't predict anything at all.

"The only reason we are currently unable to make detailed predictions from first principles is that we lack knowledge, not because there's anything fundamentally wrong with the theory."

The problem with string theory is not that it doesn't make "detailed predictions", but that it makes no predictions, none at all. Zip. Nada. Detailed or not detailed. If you work hard on a very speculative idea for more than 20 years ad it doesn't do what you want, the simplest hypothesis for why is that there is something fundamentally wrong with the idea.

58. **alex**
September 4, 2007

Peter Woit said:
"I think the great hope of some string theorists is that the LHC will see not only supersymmetry, but a pattern of supersymmetry breaking that corresponds distinctively to a certain kind of string background. Based on this they might then be able to make real testable predictions."

Surprisingly, I must say that I fully agree with you on this point Peter. I hope the readers of your blog notice this comment of yours.

Peter Woit then said:
"This seems to me to be nothing but wishful thinking, we'll find out in the next few years."

There several indirect hints pointing to TeV scale SUSY so I don't share your pessimism here.

59. **Eric**
September 4, 2007

Peter,
The exact way in which supersymmetry is broken is the major part of the information at high energy which we are lacking and this is a big part of the information that will be able to answered once information from LHC/ILC is available. Probably the detailed way in which SUSY is broken will be understood from string theory. Regarding string theory, it will be possible to make detailed predictions once the correct vacuum which describes our universe is found.

60. **alex**
September 4, 2007

Eric, see my comment above.

61. **Bogs Dollocks**
September 4, 2007

After reading the original post and the comments and seeing that theoretical HEP has descended into Ubu pata-phyics I'm soooooooooooooo glad that I left what has become a completely sterile field. There are so many interesting other physics problems to work on.

The mental gymnastics that people are prepared to perform as to what constitutes a "theory", a "prediction' and a "measurement" are truly depressing – I'm completely gobsmacked.

Hope the LHC find something interesting, otherwise it's lights out.

62. **LDM**
September 4, 2007

I do think the article does have a bias towards string theory and is more of the usual hype.
However, every time a string researcher comes out with a new idea, how many science journalists have the background to dig up the relevant paper, read it, and decide if how much is pure speculation and how much is not? This has to be a tough job. But the real problem is the string researchers themselves for putting out the hype to begin with.

Ori,
Perhaps you refer to the 1949 Nobel? It is possible to get a Nobel in physics that resulted from an idea that, while brilliant and fruitful theoretically, is later discovered to be wrong.

63. **Arun**
September 4, 2007

Peter,

Hasn't there been a shift in the past few years from string theory as "THE theory of everything" to string theory as "a general framework (like QFT or Hamiltonian mechanics) within which a theory of everything may one day eventually be fit"?

If so, isn't that an improvement in the state of affairs?

64. **Peter Woit**
September 4, 2007

Arun,

I don't see it as an improvment, since it's just an argument for why string theory research should continue to dominate the subject, despite failing as a theory of everything. The question that has always most concerned me is how one can encourage research into other "general frameworks" than string theory. We know fairly well by now how string theory works as an idea about unification (not at all), so the question is how to find some other framework that is more promising.

65. **Eric**
September 4, 2007

Peter,
If your potrayal of the current state of string theory research were anywhere near accurate, people would be looking for other frameworks to study quantum gravity in droves. However, this is clearly not the case. The string theory community continues to grow. The simple fact is that the Science World article is an accurate picture of how most scientists view string theory. I would advise that you face up to this and stop deluding yourself.

66. **Arun**
September 4, 2007

Peter,

Reality legislates what is physical theory, but speculation is a matter of taste. Whether a particular line of speculation is going to be fruitful is a matter of individual judgment.

What I mean is that if Nobel Laureate X makes a falsifiable claim, any ordinary soul can judge it for oneself (the beauty of science!) but if Nobel Laureate Y says, I have a hunch this is the way to go, then we've entered a rarified atmosphere where "which hunches of yours have panned out?" becomes an unanswerable argument.

Strings may be zero as a theory of physics but dominant as a line of speculative research depending on the interests of the most prominent researchers.

67. **happyday**
September 4, 2007

> I honestly don't have time for that crap – I certainly don't want to end up in Pilsen or something, so I better go back to work now.

This thread is extremely interesting (light years beyond the distinctly soft dullness of CV, for instance), but Bee's post just made my day. I like it so much I can't help quoting it, even though I have nothing to add.

68. **anonymous hater**
September 4, 2007

Woit,

I object to your entire approach. In this post there's no physics, no math, just abstract meta-objections. It's facile and glib to say that there is no way to concretely debunk a theory that makes no testable predictions, but it's also plainly laughable when one is talking about something plainly as rich conceptually (whether or not physically correct) as is string theory. It's extremely difficult for any individual to speak coherently about the totality of something as large as a unification program like string theory without eliding details and nuance; at this level it is always possible to raise some objections. Science is done incrementally and the rub is always in the details.

The entirety of your critique seems to involve quoting various authorities, pro and con, and analyzing the sociology behind their pronouncements. Let's remember that despite being a celebrated asshole, Wigner was as good a physicist as there has been.

69. **Shantanu**
September 5, 2007

Eric, I also know of some people who long before Peter wrote his book/had a blog gave up string theory ,because it had no connections to real world and moved to grav. waves or LQG or astrophysics.

70. **Josh**
September 5, 2007

Peter, I used to be a reader of your blog. But I have carefully compared Chalmers' thirteen-page work stuffed with interesting physics with your rants and you just seem to be a gigantic loser. It seems that Leonard Susskind is right when he says that you failed as a physicist and now you try to revenge to the world. 2 minutes and 40 seconds from the beginning of http://www.kqed.org/.stream/anon/radio/forum/2006/07/2006-07-31b-forum.mp3

My advise is to shut this website down. Bye, Josh

71. **Chris Oakley**
September 5, 2007

Josh,

Susskind may well have his wires crossed here. Insofar as he has an academic job, it is not true to say that Peter "never made it as a physicist" and he never "became a [professional] programmer". Someone suggested on this blog that he was in fact referring to myself, in which case it would be a bit like saying that LS "never made it as a plumber" as my leaving HEP had little to do with my abilities & everything to do with my choice of research topic.

But I wonder … LS now asks us to gleefully accept the possibility that creating scientific theories is just a matter of finding the right point or points in the Anthropic Landscape, and, by implication, stronger and more deductive frameworks are not worthy of sponsorship. Look up the definition of "physics" in the dictionary as ask yourself – who really "never made it as a physicist"?

72. **Peter Woit**
September 5, 2007

Josh,

I see you've been reading Lubos. And after that, you think I'm a gigantic loser?

For more about the Susskind interview, see:

http://www.math.columbia.edu/~woit/wordpress/?p=437

You string theory partisans are really charming…

73. **mathjunkie**
September 5, 2007

Peter and Lee look alike!

74. **hard gluon**
September 5, 2007

> Let's remember that despite being a celebrated asshole, Wigner was as good a physicist as there has been.

Wigner was an asshole? How come? I had never heard about that before.

75. **Peter Woit**
September 5, 2007

anon,

Sorry, but writing anonymous comments into blogs referring to people as "assholes" doesn't say much for your judgment. And whether or not any particular physicist fits that description has nothing at all to do with what I write here about string theory.

For the record, I often saw Wigner around the when I was a graduate student and he was already a quite elderly emeritus professor. If asked to rank members of the department on an "asshole" scale, my impression is that he wouldn't even make 50th percentile. Maybe he mellowed in his old age, but you're the first person I've ever heard characterize him this way.

76. **chris**
September 5, 2007

Chris Oakley,

suskind has more under his belt than those recent antropic speculations. no matter what he does now, he has an established record.
or would you claim einstein failed as a physicist, because after 1916 he basically screwed everything up (couldn't come to grips with quantum mechanics, dreamt up crazy unified theories and not much else)?

good physicists tend to get weird in their old age and suskind is a very good example.

77. **Cee**
September 5, 2007

Bee,

why did you backdate your post about Lubos?

78. **Bee**
September 5, 2007

because I don't want him to get more attention than absolutely necessary. I will change the date to today if September goes into the archives.

79. **Chris Oakley**
September 5, 2007

Hi Chris Anonymous,

AFAIC anyone who invokes the Anthropic Landscape has not made it as a physicist. I am not directing that just at Susskind.

Maybe I should have phrased it differently. Try "is demonstrating that wisdom does not necessarily come with age."

80. **Cheeky Bastard**
September 5, 2007

Chris, I think you should show some consideration for poor old guys who never made it as programmers and became professional physicists.

81. **Chris Oakley**
September 5, 2007

What – like Richard Feynman?

82. **sanity**
September 5, 2007

One question that was raised was whether string theory requires SUSY GUTs. The answer is no; string theory requires neither low energy SUSY nor GUTs; indeed, although this is not covered accurately in the article, the Bousso-Polchinski mechanism and several frameworks for moduli stabilization apply (and are in some ways actually simpler) also at higher scales of SUSY breaking. Phenomenological clues do hint at low energy SUSY, which would be very interesting to discover. But it is important to recognize that its absence does not affect the status of string theory as a theory of gravity; in fact many of the most important developments and open questions in this area, such as those involving spacetime singularities, involve supersymmetry breaking scales of order the KK or string scale.

83. **chris**
September 6, 2007

hi Chris Oakley,

to some extent i agree with you. i myself can't reconcile antropic reasoning with the very nature of physics. but you know, science has strange ways of progressing.

i always have to think about kepler, e.g. whose middle age mystic speculation about the harmony of spheres ultimately led him to meander his way to find the 3 laws.

for suskind in particular, i (as a lattice physicist) am confronted with one of his brilliant insights on an almost daily basis. even if he decides to become a spiritual medium tomorrow it does not annul this particular achievment.

so, yes, i don't buy any of the stuff he is doing at the moment, but he has definitely 'made it as a physicist' no matter what.

84. **Anders R**
September 6, 2007

i don't understand how you cannot understand something that doesn't exist. the ultimate string theory must be constructed by humans, and if it hasn't been constructed yet it doesn't exist.

85. **Anders R**
September 6, 2007

this was regarding the witten quote:

"It's incredibly rich and mostly buried underground. People just know bits and pieces at the surface or that they've found by a little bit of digging, even though this so far amounts to an enormous body of knowledge…"

86. **Bee**
September 6, 2007

Hi Anders R: That's actually an interesting question. I guess it greatly depends on what one means with 'existence'. In how far does a mathematical structure 'exist' before it is understood by humans? Does something 'exist' that doesn't have any (known) relation to 'reality' (another ambiguous word)? Does something 'exist' that is only an idea, or maybe not even yet an idea, or maybe a not even wrong idea? I think it matters in this context what one is trying to dig out there… Best – B.

87. **Anders R**
September 6, 2007

yeah i guess that if you see mathematics as a sort of structure that mathematicians find out more and more about then it makes more sense.

88. **dan**
September 8, 2007

I understand that string theory does not require SUSY GUT's, but if LHC does detect SUSY, this would presumably imply some sort of SUSY GUT, which presumably could be embedded in some sort of string theory, and all string theories have a quantum gravity sector. String theorists do not commit themselves to low-energy SUSY phenomology that will be seen at LHC, of course, but should SUSY be seen I'm not sure that calling string theory unification scenarios are failures in that remote event.

Does string theory succeed as a theory of quantum gravity? I am aware of objections that it is not background independent, nonetheless it does have gravitons.

89. **Peter Woit**
September 8, 2007

dan,

String theory unification will remain a failure as long as it can't predict anything, and seeing SUSY at the LHC by itself won't change that. The only way it will change is if you see SUSY, together with a SUSY breaking pattern that is naturally explained by a specific class of string theory backgrounds. That's what string theorists are hoping for, but I see zero reason for it other than wishful thinking.

The debate about background independence and string theory has been carried on at this blog and many others upwards of 100 times by now. Unless there's something new about this, please don't start the same discussion all over again here.

90. **dan**
September 23, 2007

Dear Peter,

What do you think is the most promising solution to the hierarchy problem if you don't believe in SUSY? Technicolor models and top quark condensates don't seem any better than SUSY.

thanks
Regards
Dan

91. **Peter Woit**
September 23, 2007

dan,

Actually I've never been convinced by the arguments about the hierarchy problem. For one thing, we have no strong evidence for a GUT scale. For another, given our lack of knowledge about quantum gravity, we don't even know that the size Newton's constant necessarily means that quantum gravity happens at the Planck scale. So, it's not even clear there's a problem. To me the real problem has always been that of actually understanding what is causing electroweak symmetry breaking. Once we know that, let's see if we still have a "hierarchy problem". Hopefully the LHC will set us on the right track….

92. **dan**
September 23, 2007

Peter,
thanks for sharing that with me, I think that your view is why some string theorists disagree with you so strongly. While proton decay hasn't yet been observed as required by GUT's, stringers believe the matter/antimatter symmetry and violation of baryon number implies some sort of GUT, and that the most popular SUSY-GUT can be embedded in a string theory framework.

Dan

93. **Eric**
September 23, 2007

Dear Peter,
It seems that you've never heard of radiative electroweak symmetry breaking which occurs in supersymmetric theories, in particular 'no scale' supergravity models. The large top quark mass was anticipated 25 years ago when this mechanism was first discovered.

No compelling evidence for SUSY? How about:

1) Solution to the hierarchy problem.
2) Gauge coupling unification.
3) Well-motivated dark matter candidate
4) g-2 for the muon
5) Dyamical electroweak symmetry breaking.

It's clear why you're in the math department and not even a real
particle physicists.

94. **dan**
September 24, 2007

Eric,

Peter does address 1-3 in NEW (though I didn't realize reading NEW that he "never been convinced by the arguments about the hierarchy problem")

you may factually disagree with PW, but he does state in NEW, if you bothered to read it, that "Gauge coupling unification" comes off 10 percent too high, and that SUSY "Solution to the hierarchy problem" has its own problems (i.e 105 extra parameters, u problem, symmetry breaking mechanism, ad hoc introduction of R-parity, vacuum energy and cc too high, flavor changing neutral currents, lepton flavor changing, potentially large cp violation). If you bothered to read his book you'd know this.

DM is compelling evidence for anything.

Still, PW, I don't recall you addressed in NEW
points 4 & 5

4) g-2 for the muon
5) Dyamical electroweak symmetry breaking.

regards
Dan

95. **Peter Woit**
September 24, 2007

dan,

I was going to resolve to stop wasting time responding to string theorist trolls like Eric who seem to think that posting bad arguments and personal attacks on me helps their cause.

Besides the book, one of many places I've discussed the issue of supersymmetry is here:

http://www.math.columbia.edu/~woit/wordpress/?p=97

It's a discussion of a talk by Witten I attended on exactly this question. Unlike Eric, Witten did not claim that there is "compelling" evidence for supersymmetry. He gave several very tentative points of evidence for supersymmetry, together with several tentative points of evidence against. He certainly didn't include Eric's ridiculous claim that the g-2 discrepancy is compelling evidence for supersymmetry (it isn't, even assuming it is really there, which is not clear, there are many, many ways of getting such a discrepancy other than supersymmetry). There are various claims about a "prediction" of the top mass from supersymmetry, but this is based on various assumptions and never was much of a "prediction". I'd have to go look up the history, but from what I remember most discussion of this appeared after the top quark was already discovered. I'm pretty sure that there was never a point at which any supersymmetry partisan ever announced that supersymmetry was wrong if the top mass didn't have a certain value.

96. **Eric**
September 24, 2007

Dan,
Peter's arguments against gauge coupling unification are incredibly misleading. He says that it comes out 10% wrong on the strong coupling if you start out at the estimated unification scale and run the RGE's to the electroweak scale. However, this ignores the fact that there is an inherent uncertainty (of about 1%) on the actual unifcation scale, as well as an uncertainty on the value of the strong coupling constant at the electroweak scale. Thus if you start at the high scale, you are merely compounding the same uncertainty multiple times.

As far as the 'wild-claims' that SUSY + radiative electroweak symmetry breaking predicts a large top quark mass, this is standard knowledge which you will find in every textbook on the subject. The prediction of the large top mass was done in the early 1980's, well before there was any other reason to suspect it to be so large.

97. **Peter Woit**
September 24, 2007

Eric,

If you can produce a reference from the period before the top quark mass was measured in which a supersymmetry advocate said that supersymmetry was wrong if the top quark was not in a definite mass range (rather than just that one of many supersymmetry scenarios gave a possible mass range), please do so.

98. **Eric**
September 24, 2007

Peter,
As an example, have a look at

S. Ferrara, D.V. Nanopoulos, and C.A. Savoy, Physics Letters B 123:214,1983.

and various successor papers. Also you might have a look at a textbook published in 1989, 'Particle Physics and Cosmology' by Collins, Martin and Squires, or the popular science book by Gordon Kane on supersymmetry.

This work on dynamical electroweak symmetry breaking in no scale supergravity is very well known. In fact, all computer programs such as Isajet, SuSpect, etc. which calculate the Higgs mass and low energy superpartner spectrum use this.

99. **Peter Woit**
September 24, 2007

Eric,

Sorry, but you are really wasting my time. I made the mistake of looking at the paper you mention. It doesn't contain anything like what I asked for. It's just the same sort of unsuccessful model that you are now, a quarter century later, still working on, and still promoting as "supersymmetry predicts the fermion masses". This wasn't true then, and it isn't true now, for reasons that people have repeatedly pointed out to you.

100. **Eric**
September 24, 2007

Peter,
You should be embarassed by your complete ignorance of particle physics and phenomenology. This is a very basic result which is widely used in just about every calculation used today and for you to not even be aware of it is apalling.

101. **Peter Woit**
September 24, 2007

Eric,

I'm not ignorant of that calculation, I just don't think it's compelling evidence for SUSY. I asked you to provide a reference claiming that this was a definite prediction of SUSY (as opposed to something model-dependent), and you responded by quoting something that doesn't do this at all and continuing to insult me. If you were arguing against string theory I would long ago have put you on my block list as someone ill-informed who added nothing but misinformation and/or hostility to the comment section here. Since I don't want to be accused of censorship, I guess I'll continue to allow your misinformation and stupid personal attacks to appear here. But you're really not doing a good job of representing advocacy for string theory…

102. **Eric**
September 24, 2007

Peter,
You're just playing games to avoid having to concede the point on this issue. Everyone knowledgable knows this result and it is a standard (generic) part of SUSY phenomenology. For the record, I do have a Ph.D. in high-energy physics and am intimately familiar with the current status of the subject. Certainly, more so than you.

On the subject of personal attacks, I seem to recall being rererred to by you a few comments back as a troll. Care to answer for that?

103. **Peter Woit**
September 24, 2007

Eric,

I'll stand by the "troll" comment, see:

http://en.wikipedia.org/wiki/Internet_troll

104. **Eric**
September 24, 2007

Peter,
I hardly think pointing out errors in your statements as deliberately posting comments to stir up controversy. The fact remains that you don't seem to be aware of basic developments in a subject for which you pass yourself off as an expert. Dyanmical electroweak symmetry breaking is just one example. You argument regarding gauge coupling unification is also completely wrong. It's hard to say whether you just don't know what you're talking about or if you're just fundamentally dishonest, but the net result is that many people end up being mislead by your comments.

105. **alex**
September 24, 2007

Peter:"If you can produce a reference from the period before the top quark mass was measured in which a supersymmetry advocate said that supersymmetry was wrong if the top quark was not in a definite mass range (rather than just that one of many supersymmetry scenarios gave a possible mass range), please do so."

I think that the answer is obvious in the context of MSSM. Indeed, if the top were light, there would be no REWSB and extra stuff would have to be added to the MSSM to accomplish this. And indeed, all the standard MSSM packages automatically incorporate this to generate the higgs mass etc.

Eric, Peter has asked you a carefully worded question to which there is an obvious answer – "many supersymmetric scenarious", whatever they are, don't predict a heavy top.

Supersymmetry by itself is just a framework and, of course, such a specific prediction (heavy top) without a concrete model is impossible and Peter knows this and that's why he asked you this ridiculous question.

106. **Peter Woit**
September 24, 2007

alex,

Thanks for the clarifying comment.

107. **Eric**
September 24, 2007

Alex,
Yes, I know that Peter was just playing word games to avoid the issue. The bottom line is that in the MSSM and similar models, the electroweak symmetry breaking scale is determined dynamically. For this to work a large top mass is required, and this was realized in the 80's long before the top was discovered. Peter's tactic is to play ignorant and try to obfuscate because he knows that this completely undermines his arguments that there is no evidence for SUSY.

108. **Coin**
September 25, 2007

Hi,

What does "dynamical electroweak symmetry breaking" refer to in this context, and why are Dan and Eric considering it to be evidence for supersymmetry?

109. **Peter Woit**
September 25, 2007

Eric,

Alex was pointing out to you that your claim that supersymmetry requires a large quark mass is incorrect. This is a claim about certain models, it's not true in others.

110. **Chris Oakley**
September 25, 2007

Eric,

"I can construct supersymmetric models in which the top quark must be heavy. Therefore supersymmetry requires a heavy top quark". If a child presented you with this kind of reasoning then you might feel duty bound to correct them. I certainly would. The fact it seems to be perfectly acceptable amongst particle physicists now just shows how far the subject has fallen. It seems that what was once the hardest of hard sciences is now just a mushy neverland where optimism matters more than fact, hype more than reasoning, and who you suck up to more than the substance of your research. You are welcome to it. All of it.

111. **Eric**
September 25, 2007

Dear Peter and Chris,
You guys are really into convoluted bs and self-deception. What we're talking about it the MSSM, and a heavy top mass is required in the MSSM to radiatively break the electroweak symmetry. I'm not sure what other supersymmetric models you're talking about that wouldn't require a heavy top. Any model which includes the standard model and TeV scale supersymmetry will have this feature. So, stopping misleading yourselves and others with your desperate attempts to deny any evidence for supersymmetry. It's just sad.

112. **Peter Woit**
September 25, 2007

Eric,

Perhaps you should stop insulting people long enough to ask someone about this who actually knows what they're talking about. Your claim

"Any model which includes the standard model and TeV scale supersymmetry will have this feature."

is just not true, as Alex tried to point out to you:

"Eric, Peter has asked you a carefully worded question to which there is an obvious answer – "many supersymmetric scenarious", whatever they are, don't predict a heavy top."

113. **Eric**
September 25, 2007

Peter,
What Alex has said is incorrect. It is simply not true that many supersymmetric scenarios don't predict a heavy top. Well, it might be true if that scenario does not include electroweak symmetry breaking or if your model doesn't include TeV scale SUSY or the standard model.
For your reference, here are publications you should look at if you want to educate yourself:

Ibanez and Ross, Phys. Lett. B110, 215(1982)

L. Alvarez-Gaume, M. Claudson and M. Wise, Nucl. Phys. B207, 96(1982)

K. Inoue, A. Kakuto, H. Komatsu and S. Takeshita, Prog. Theor. Phys. 68 927(1982)

As Alex pointed out, this is a standard, generic feature of SUSY phenomenology and is a feature of every computer program used to obtain low-energy SUSY and Higgs spectra. Why do you suppose the uncertainty in the top mass is so important in calculating the expected Higgs mass?

Again, I would expect any real particle physicist to be well aware of this, so either there are large holes in your education or you are deliberately being deceptive.

114. **Peter Woit**
September 25, 2007

Eric,

You just continue to do the same thing endlessly: posting comments that are a combination of insults and misinformation. Alex, who I suspect has no great sympathy for my point of view, has tried to politely explain to you that you're wrong. Maybe someone else wants to try their hand at this, but it's rather obviously a complete waste of time.

115. **Eric**
September 25, 2007

Peter,
I think you have misinterpreted Alex's post. He was not being supportive of your postition. Read it more carefully.

116. **anon.**
September 25, 2007

This discussion is ridiculous. The physics here is quite simple: if you start out with non-tachyonic masses for the Higgses in the MSSM, you have to drive one of them tachyonic to get EWSB. How can that happen? Well, if you look at the RGEs, it's the contributions from Yukawas that push the mass terms toward being tachyonic. So yes, you need some large Yukawas to get radiative EWSB. No one disagrees with that, right? Anything else is semantics.

117. **observer**
September 25, 2007

> need some large Yukawas to get radiative EWSB

but how large is "large"? A top at 30 GeV would be considered "heavy" w.r.t. to the other quarks, but that's not a prediction of the real top mass.

118. **anon.**
September 25, 2007

observer: the statement that you need large Yukawas for radiative EWSB in the MSSM is a very general one. To get a precise numerical prediction, you need a model.

119. **observer**
September 25, 2007

As usual, the devil is in the details… Thanks!

120. **non-a**
September 25, 2007

"To get a precise numerical prediction, you need a model."

So what. Given the cornucopia of free dimensionful parameters in the MSSM it's hardly surprising some of them can be adjusted to get a heavy top.

121. **Eric**
September 25, 2007

non-a,
There's no need to adjust any parameters to get a large top mass in the MSSM. One needs a large top mass to obtain EWSB, so the two things are related generically.

122. **alex**
September 25, 2007

Eric, I just wanted say that you fell into Peter's trap by using the loose language and saying that the heavy top is evidence for supersymmetry while referring to the MSSM. We all know you were not talking about N=4 SYM when you referred to supersymmetric models. The bottom line is that there is plenty of indirect evidence for MSSM and it will be exciting when the superpartners are discovered.

123. **alex**
September 25, 2007

Eric, I have a question to you since we're having this discussion. If you take split SUSY with scalar masses at about the GUT scale or slightly lower, is there enough RGE running to drive (m_hu)^2 negative? If not, how do they achieve EWSB?

Thanks!

124. **Eric**
September 25, 2007

Alex,
I'm not an expert on split SUSY, and where I come from we would not discuss such models in polite company, but here goes. My understanding is that they use the 'little Higgs' mechanism where strong dynamics at some high (few TeV) scale are employed a la technicolor for EWSB. Such models also give up solving the hierarchy problem, although they can apparently manage to maintain gauge coupling unification. In my humble opinion, split SUSY is very contrived and artificial which is the result of giving up a large amount of SUSY.

Of course you could always consider super split supersymmetry, which would have no way of explaining the top mass, EWSB, or the hierarchy problem, since this is just the normal standard model. Peter Woit would probably use this an example where a supersymmetric model doesn't predict a large top mass. 😉

125. **anghe**
September 25, 2007

Eric,

if we conducted a poll among the CDF and D0 collaborations that discovered the top and measured its mass for the first time, how many of them, do you think, would be convinced that the top quark mass was predicted by SUSY theorists before their discovery?

126. **Eric**
September 26, 2007

Anghe,
Most CDF and D0 colloboraters would agree that a large top mass was anticipated from radiative EWSB, and the fact that the top turned out to have such a large mass as one of the reasons for them to pay attention to SUSY.

127. **anon.**
September 26, 2007

Eric wrote:

My understanding is that they use the 'little Higgs' mechanism where strong dynamics at some high (few TeV) scale are employed a la technicolor for EWSB.

Your understanding is completely wrong. (Doesn't anyone ever look things up before talking about them on a blog?) If you want supersymmetric-looking unification with a split spectrum in a *natural* theory, you might try something like that. But split supersymmetry is just the MSSM with a split spectrum.

128. **Peter Woit**
September 26, 2007

Eric,

Have you checked this with "most CDF and DO collaborators"? Just a wild guess, but I would suspect that they might tell you that a "large" top mass was anticipated not because of SUSY, but because the top was not seen at "low" mass.

129. **Eric**
September 26, 2007

Anon,
I think it would depend on the amount of splitting in the spectrum. I'm sure there are split models where you can get EWSB and others where it isn't possible and you need something else. In any case, split SUSY is inherently unnatural since I can't think of any reason why some supersymmetric partners would have large GUT scale masses and others wouldn't. This only works if you believe in fine-tuning which leads you to anthropic reasoning.

Peter,
The several CDF and D0 collaborators that I know as well as those at CERN were well aware that a large top mass was anticipitated by SUSY. As has been pointed out to you now by several people, this is a very well known result. Using exactly the same idea it's pretty easy to calculate that the Higgs mass should be in the range 114-121 GeV using the current top mass of 170.9 +/- 1.8 GeV. All of the experimentalists know this.

130. **Chris Oakley**
September 26, 2007

> they might tell you that a "large" top mass was anticipated not because of SUSY, but because the top was not seen at "low" mass.

A fine piece of reasoning. This is the same kind of inexorable logic that explains why you don't find many termite nests near places where anteaters hang out.

Is There Intelligent Life On hep-th?

September 5, 2007
Categories: Uncategorized

There are yet more hep-th articles on the anthropic principle this week, following recent ones devoted to the implications for fundamental physics of the heights of giraffes and sizes of brontosaurus brains. The TASI summer school designed to train particle theory graduate students this year featured talks by Raphael Bousso expounding the anthropic landscape pseudo-science as a "solution" to the CC problem. His lecture notes are now available. In them he does refer to one problem that plagues this subject, that of how to identify the intelligent observers whose probability of existence everything depends on:

> The problem of characterizing observers, especially in vacua very different from ours, remains challenging.

Last night a new paper on this subject appeared on the arXiv, by Maor, Krauss and Starkman, making the point about anthropic arguments that:

> arguments of these sort (see for example) strongly rely on the assertion that we must be typical observers, an assertion without sound fundamental scientific basis at the current time.

The authors end with a conclusion about what you can learn from anthropic arguments:

> Finally, the correlations illuminated by anthropic reasoning imply that what we ultimately learn from anthropic arguments is that the existence of us and the existence of the observed value of Lambda do not contradict each other. That is nice, but hardly surprising.

In their acknowledgment section they thank Bousso for "lively discussions". He thanks lots of people in his acknowledgments section, but not them. I don't know about this question of intelligent life in other pocket universes, but the question of whether there's intelligent life on hep-th these days seems to still be open.

Comments

1. **Alfonso Martinez**
   September 5, 2007

   Even shorter, "correlation is not causation".

   If only David Hume could witness what sort of science we do nowadays! Not to speak of the use we give to the money provided by our funding agencies, of course.

2. **locrian**
   September 5, 2007

   Thanks for the quotes from the Maor paper. That last one was perfect.

   It's a shame they need quoting, though.

3. **SnarkFest**
   September 5, 2007

   When people who have demonstrated an attitude towards philosophical reflection ranging from dismissiveness to outright contempt have philosophical ideas of their own (whether or not they acknowledge them as such) the results can generally expected to be crap.

   This is just another case in point.

   (Of course this is not to say that crappy ideas, and execrable *formulations* of ideas, can't originate with professional as well as amateur philosophers.)

4. **Greg Egan**
   September 5, 2007

   Another good paper on this topic is "Are We Typical?" by Hartle and Srednicki:

   > Bayesian probability theory is used to analyze the oft-made assumption that humans are typical observers in the universe. Some theoretical calculations make the *selection fallacy* that we are randomly chosen from a class of objects by some physical process, despite the absence of any evidence for such a process, or any observational evidence favoring our typicality. It is possible to favor theories in which we are typical by appropriately choosing their prior probabilities, but such assumptions should be made explicit to avoid confusion.

5. **Jim Clarage (astonished biophysicist)**
   September 5, 2007

   I admit the quick skim of Bousso's lecture notes got my attention (section 7) where he reports that his developments have

   > changed the status of string theory: the theory has made contact with experiment

   But as I read more carefully I am shocked that reasoning such as,

   > Moreover, it is reasonable to suppose that regions without galaxies do not contain any observers.

survives unchecked in any university who's lectures (they are lecture notes afterall) are attended by other disciplines, e.g., biologists, psychologists, cognitive scientists. As I've posted here before, even most modern theologians would blush at such anthropocentric reasoning. It genuinely goes beyond anything Ptlolemy or any other pre-Copernican cosmologists might have guilty of about Man's central place in the universe and its laws.

6. **Ori**
   September 6, 2007

   So why wouldnt you send some intelligent paper instead of writing content-less postings on the blogosphere ?

If you are so smart you must have some good ideas (with equations and not just words)

7. **chris**
September 6, 2007

because he is woit the destroyer, not woit the constructor.

oh, you still need formulas these days to go on hep-th :-)? a yes, the giraffe's height is related to plancks constant by more than words i forgot.

i wonder, if one could formulate a meta-antropic principle:
given any kind of universe, if an inteligent observer evolves (s)he will always develop a theory of why the universe has to be such as to support this particular form of intelligent observer.

8. **dragon**
September 6, 2007

What I also find startling about Bousso's lecture notes is the style, the way he heaps scorn on people who are stupid enough to disagree with him. I'm told that this is indeed his personal manner, but to see it coming out in "lecture notes" is a bit of an unpleasant surprise.

9. **D R Lunsford**
September 6, 2007

You know, there is a symmetry that may have been overlooked – the giraffe can face any direction and still maintain his height. So there should be a gauge theory of giraffe perambulation. No lion'!

-drl

10. **Shantanu**
September 6, 2007

Peter, forget about all this. Did you find anything interesting from
the Lepton-photon symposium(slides now online). also what do you think
about the "Beyond Einstein" decision, now that it is out.
Thanks

11. **Peter Woit**
September 6, 2007

Shantanu,

As far as I can tell, there was nothing really exciting announced at lepton-photon, just lots of solid progress. Rumors of hints of a Higgs didn't pan out…

As for "Beyond Einstein", I just don't know enough to have an informed opinion.

12. **Jack**
September 7, 2007

Nowadays these physicists have gone to a different field (philosophy). Physics concerns, in fact, what is measurable.

It is also clear that this kind of philosophy has no more scientific foundations that the hypothesis that there is a God, who has built the Universe and all that exists.

13. **oxo**
September 8, 2007

If these guys are correct, then LQG has been measured..

http://www.arxiv.org/abs/0708.2889

14. **gravitonto**
September 8, 2007

> http://www.arxiv.org/abs/0708.2889

I think naive dimensional analysis and order-of-magnitude guesstimates are not the most solid possible basis for such a revolutionary claim as the authors make. But, what the heck, J. Ellis is the second most cited author in HEP, so I guess it must be true: quantum gravity has finally been experimentally discovered. hoooo-rrrraaaaayyyy!!!

15. **woit**
September 8, 2007

oxo and gravitonto,

You folks are in the wrong posting, this was discussed a while back, see

http://www.math.columbia.edu/~woit/wordpress/?p=591

16. **gravitonto**
September 8, 2007

Sorry about that. I guess I should have known that preprint couldn't possibly have gone unnoticed and undiscussed here…

Updates on Plagiarism Scandal, Journal of K-theory

September 5, 2007
Categories: Uncategorized

Plagiarism Scandal

Today's Nature has an article by Geoff Brumfiel with more details on the plagiarism scandal described here. At last count it involves 15 authors, 67 papers on the arXiv, of which about 35 were refereed and published, in 18 different journals. The arXiv has set up a special page with information about this. As far as I can tell from checking a few examples, most of the published papers are still available online at the journals, with no indication of their plagiarized nature. One exception is the plagiarized paper at JHEP, which has now been removed, with the notation

> This paper has been removed because of plagiarism. We regret that the paper was published.

As far as I know, neither JHEP nor any of the journals has given any indication of an intent to change their refereeing procedures because of this scandal.

Journal of K-theory

The editors of the new Journal of K-theory have issued a public statement, explaining in detail their plans for how to handle papers submitted to the older journal, K-theory, where they had resigned as editors.

Comments

1. **Thomas Love**
   September 5, 2007

   It does make me wonder about the quality of the refereeing.

2. **Jon Lester**
   September 6, 2007

   The way the review process is applied in physics is a scandal.

   Jon

3. **chris**
   September 6, 2007

   Then do you have any suggestions on how to improve it? remember: referees are not payed, they take time off from pursuing their own work and, this is the most important point i never see properly expressed, they are there to check the scientific soundness and impact of the work. they are not there to check spelling, grammar, fraud or plagiarism. when i get a paper to referee, i assume that the authors were honest. if i don't, i wouldn't even know where to start. should i redo the whole work or what to see if it is reproducible? sure, the key argumentation and drawing of conclusions are tractable, but if someone says, this and that is the outcome of a particular experimental setup or a one year calculation on a supercomputer reveals this number as a result – how can you as a referee challenge that? you have to rely on the honesty the authors.

   it is similar with respect to plagiarism. ideally, of course, you should know all the literature in the field of the paper that you are refereeing. but honestly, there are certain topics that are pursued by only one group of people worldwide and unless you want them to self-referee, someone a bit outside has to be chosen. you can't seriously expect them to dig thru all the literature in search of plagiarism. this is not what referees are supposed to do!

   and on a final note: why the revelation of plagiarism such a scandal for the refereeing system? it just shows, that basically it works. the culprits have been identified and you can be quite sure that their career is over. and given the fact that in order to be of any relevance, a paper has first to be noticed, it just absolutely floors me how these people think they could ever get away with it and have some advantage from plagiarizing. as soon as the work really becomes known, the plagiarization is soon uncovered.
   the same by the way is true for fraud. remember the guy claiming to have produced element 117 (or was it 118?)? he stated an experimentally falsifiable wrong statement. just by common sense there are only 2 future prospects for such a statement: either nobody cares in which case you have to ask yourself what the motivation was for cooking up the fraud in the first place. or people are interested and will inevitably uncover the fraud.

   so i would say that what we witness here is good proof that science is healthy and has self-correcting mechanisms that work.

4. **prague_phys**
   September 6, 2007

   Look at the webpage of ALİ HAVARE,

   http://www.mersin.edu.tr/apbs.php?id=335

   one of the persons involved. Most of his (former) students, such as Yetkin, Aydogdu, Salti and Korunur were involved as well. Wonder who was the head of the gang.

5. **Elisha Feger**
   September 6, 2007

   The K-Theory journal situation still makes my head hurt, no matter how much I read up on it.

6. **matt**
   September 6, 2007

   Elisha, can you check the reference number 25, page 343 in K-theory Volume 36 (2005) in the printed version (not online version)
   to get an example of the `seriousness' with which Springer has been treating the manuscripts of K-theory!!

7. **Peter Woit**
   September 6, 2007

chris,

I don't see how this is evidence of a healthy system at work. If the authors had bothered to change the wording and notation when they plaigiarized, no one would be the wiser.

One of the main roles of the referee is to determine whether the result claimed in a paper is not just correct, but original. If a referee knows nothing about whether the main result claimed in a paper is something that has been done before, and they don't want to take the time to look into this, they shouldn't be refereeing the paper. What this scandal shows is that getting unoriginal (and uninteresting) work published in most theoretical physics journals is almost trivially easy. One reason for this is that the results are of such little interest that no one other than the referee is likely to actually read the paper and notice that it isn't original.

The journals should either find a way to do the kind of peer review that they are claiming to do (and charging lots of money for), or admit that it just can't be done any more and give up. In the meantime, it might be a good idea to deal with the current situation by putting warning labels on these journals saying something like "the editors haven't been able to determine if these papers contain original research or not".

8. **chris**
September 6, 2007

hi peter,

actually i agree with you. and for all practical purposes, these warning labels are already there i would say. because from personal experience i conclude that what counts (in a positive sense) is the recognition of a paper and not journal reference (on the negative, having a preprint without journal reference is a very big warning sign). i think there is consensus that a lot of published work is not worth the paper it was printed on. but i think there is equal consensus, that it is more worthwhile to push ones own research than debuking others unless they make very strong claims or do something that affects you personally.

i am not saying that this state of affairs is ideal. but i see it as the prize to pay for the ease of information exchange. what it ultimately boils down to is that in order to judge a papers merits you actually have to read and understand it yourself.

so in all honesty, classical journals in my opinion are outdated already. they do have some merit, but the speed of todays research just makes a close to 100% identification rate of 'good' papers illusionary. the problem in my opinion comes in when what they do publish is taken as gospel. when selection committees count the number of papers rather than reading the 3 topcited ones. but i see this as a problem on the recipient side. a judgment based on metadata of a persons publication record is just not foolproof. today probably less than decades ago.

for plagiarism specifically i can only repeat my claim, that the only way it can stay undiscovered is when nobody cares about the research and nobody has suffered negative consequences. it is kind of sad that people exist who wish to populate this corner. but i doubt that much more resources of serious scientists should be spent to explicitly search for them in cases where their plagiarism has next to no effect.

but to be constructive, let me ask what you think journals or referees should do?
since refereeing can only be sensibly done by active researchers, this implies that more thorough refereeing will leave less time for research.

9. **Jon Lester**
September 6, 2007

Peter,

What makes me feel sad about the peer review system in physics is that good ideas may fail to go through while rubbish, just because is well recognized fashion, can easily get published on the most important journals. What is worst are the reasons for rejecting a paper that are generally unsound, but now, besides the reviewers, publishers tend to hide also the editors. No person takes on responsibility for the large number of errors the system is badly doing in this historical period.

A paper of mine was rejected by a DAE of PRL because "His work has had no response by the community. I do not understand what he is doing". Other "reasons" like these can be found as I have a large file of published papers and a lot of unpublished ones and it would be really fun to get the reports of such reviewers known, after so long time, to see how badly they turn out to be wrong. I think there is a lot of people out there in similar situations but we are all silent because we fear to have our other papers no more published.

I worked in almost all field of physics but the absolutely worst situation is in particle physics. A referee claimed that "QCD lattice computations do not reflect reality" to reject a paper of mine. So, there is a lot of people out there wasting time, money and resources!

There is again a lot to say. For the moment I stop here. But I would like to discuss what could make a paper publishable and what should mean "important", a criteria largely questionable and deemed to the taste of the single person. In this way is truly easy to suggest rejection on questionable personal feelings.

Jon

10. **chris**
September 6, 2007

hi jon,

did you try to submit said paper to another journal than prl and did it make it?

honestly, i think that every paper which is not totally meritless will certainly find a journal these days.
and even if that should happen, if the paper reaches 50 or 100 citations it doesn't matter that much anymore. people will get curious as of why it didn't make it. and if it doesn't recach that, well, chances are nobody would have cared anyways.

11. **hard gluon**
September 6, 2007

> when i get a paper to referee, i assume that the authors were honest. if i don't, i wouldn't even know where to start.

Then you shouldn't referee. If you don't know the field well enough to know whether a result is original or not, accepting to referee is dishonest on your part.

12. **Jon Lester**
September 6, 2007

Hi chris,

Yes, I did it and I get it accepted in a few days by the editor. I think (my personal judgement) that the paper does worth that.

Jon

13. **Eugene Stefanovich**
September 6, 2007

hard gluon said:

"Then you shouldn't referee. If you don't know the field well enough to know whether a result is original or not, accepting to referee is dishonest on your part."

I think your judgment is too harsh. In our age of narrow specialization, there could be only 2 or 3 people in the world (including the author) who know exactly the background of each particular manuscript. Often these people are either collaborators or rivals, in which cases it doesn't make sense to ask them to review the paper.

For the rest of us it could be almost impossible to read all the references in the reviewed work, to repeat all calculations, or redo the experiment. So, it is impractical to ask the referee to give a 100% "seal of approval". If that would be possible and only "correct" papers appeared in print, then scientific journals would be 100 times thinner than they are now.

This is not only unrealistic, but also dangerous, because increasing the barrier for publication may prevent novel non-mainstream ideas from being published. My attitude is that "s**t happens", and that one or two plagiarized papers will not have any effect, except for the embarrassement to their authors. There are things much more dangerous for the health of science. String theory and anthropic groupthinks are high in this list. We should be thankful to Peter for keeping focus on these areas.

Eugene.

14. **hard gluon**
September 6, 2007

> I think your judgment is too harsh.

Maybe it is, hence my nickname :). Besides, nobody is infallible and plagiarism may be hard to detect. Yet, take for example the paper "Brane-world black holes and energy-momentum vector". I'm sure there are many people out there who are involved sufficiently in the field of black holes in brane worlds to realize whether that paper is lifting its results from some other recent papers. After all, twenty years back nobody was studying brane worlds.

> to repeat all calculations, or redo the experiment.

That is obviously something a referee should never do. If there are obvious (to an expert) mistakes, those should be pointed out. But if the results reported in a paper look consistent and plausible enough to an expert eye, no further checking should be necessary. It's the author's business, I think, to take care that their results are correct. It is their professional reputations that is at stake.

Having said this, I must say that once a referee found a factor 1/2 wrong in a complicated equation for a cross section in a manuscript I submitted. He/she thought it was a typo. Well, it wasn't, it was a calculational mistake, and to this very date I have no idea how that referee caught it… This happened some time ago, I'm not sure that kind of quality refereeing happens very often these days.

15. **ali**
September 6, 2007

Hi Peter,
I happen to be from turkey originally so I know the environment there quite well. These folks in question hardly know any english at all. If you asked them to give a 10 minute presentation in english about anything, they wouldn't be able to. The system gives promotion based on bean counting in turkey these days so the strategy is "publish as many as you can" without any regard to quality. That is why most of the papers in question are published in obscure journals. Nobody cares where you publish. Turkey as a country is yet to publish one single paper in science or nature.

16. **Ian**
September 6, 2007

when i get a paper to referee, i assume that the authors were honest. if i don't, i wouldn't even know where to start. should i redo the whole work or what to see if it is reproducible?

One thing I've started doing with every paper I review is pick a handful of phrases, from the intro, the discussion, the results, and just run them through Google. It takes less than five minutes but has at least a chance of picking up plagiarism. (So far, thank God, I haven't found any, and I hate to think of what I'd have to do if I did.)

You're right, you can't repeat experiments. If you're lucky, you have experience with a close-enough system to notice if something is plausible or not. You can also take a few minutes to mentally work through a protocol; there's a well-known instance where a reviewer caught a case of fraud because he realized the experiment as described would have taken thousands of tissue-culture flasks.

Editors are supposedly looking at figures more closely nowadays, including using some automated techniques that will pick up simple fakes. (I'm in biology, and I don't know if other fields have similar approaches.) I do look with a skeptical eye at figures, but I'm not sure that I'd pick up any but the crudest forgeries.

17. **Highly Cited Researcher**
September 7, 2007

Ali,

It's the same all over the Mediterranean (Spain, Italy, Greece, etc.). Promotion depends on the count of papers weighted by impact factor. The more self-citations the better. Governments and universities looking for `objective' criteria for evaluation impose such requirements. The commercial journals depend on this to survive; the journal is outrageously expensive, but can count on a large number of mediocre scientists looking for `impact' to submit their articles. That the publisher produces three journals in the same area is no surprise either; the author and his friends rotate journals, citing each other into the university administration. Once there they see to it that more `objective' criteria for promotion are imposed, favoring ambitious mediocrities like themselves, because such people are incapable of challenging them scientifically, and are easily made dependent.

18. **chris**
September 7, 2007

hi ian,

"One thing I've started doing with every paper I review is pick a handful of phrases, from the intro, the discussion, the results, and just run them through Google."

that actually is a really good suggestion!

19. **prague_phys**
September 7, 2007

Highly Cited Researcher, we have similar system here in eastern Europe. My opinion is that if bureaucrats wants numbers, then total number of citations and determination in which decile the author is in terms of citations in his subfield would be much much better than what we have, though still far from ideal.

20. **la dernier fois**
September 7, 2007

Just like that googling idea, isn't it natural to suggest a similar service from the arxiv? A referee would get a candidate-to-be-paper, run a quick search there (arxiv), if too many similarities come up, well… then she/he's done already!

but, wait… there's really nothing similar already?

because, if there isn't, well… I guess plagiarism is just being asked for…right?

21. **ali**
September 7, 2007

Highly cited researcher,
I am aware of it. It would be more appropriate if people paid attention to the journal the article was published as well. For instance, in china, government pays 1000 dollars per impact factor of the journal the article was published. If you publish one article in science, you receive 30K from the government so there is an incentive to publish in high reputation journals. In countries like greece and turkey, there is no such thing. People can barely speak english, let alone write articles about black holes.

22. **Anonymous Referee**
September 7, 2007

Coincidentally, I was just asked to referee a paper (not found on the arXiv) by different Turkish authors (although from one of the institutions involved), which was also plagurized. However, in this case, the stealing was obvious merely from following up on the paper's references. It looked like the authors had no idea they were doing anything wrong! I'm not sure what to make of this, except that all this plagurizing must be emblematic of a serious misapprehension of scientific ethics in some corners of the Turkish physics community.

23. **Ingwer Angström**
September 7, 2007

Ali i take it that you have some idea about the status of the status of the physics community in turkey but please don't extrapolate it to greece. There are many good greek researchers and people actually do speek english. On the other hand it's true that the greek (i guess the turkish too) goverment prefers to buy weapons (20000 million € in the next 10 years) than to seriously invest in research. That's sad but it will not change soon.

24. **Anon.**
September 8, 2007

"For instance, in china, government pays 1000 dollars per impact factor of the journal the article was published. "

Is this true?! Does anybody know?

25. **Paul**
September 23, 2007

I agree totally with you about Berlinski. His 'tour of the calculus' might be the worst book I've ever read. One more reason for me to buy yours…

26. **Ilja**
September 30, 2007

Peter Woit Says "What this scandal shows is that getting unoriginal (and uninteresting) work published in most theoretical physics journals is almost trivially easy. One reason for this is that the results are of such little interest that no one other than the referee is likely to actually read the paper and notice that it isn't original."

What do you expect in a world of "publish or perish"? If people are forced to publish, they will publish. Even if they don't have anything interesting to say.

27. **JDR**
October 1, 2007

Trust me, it's not just physics that publishes rubbish. It's a huge problem that needs to be solved. As Ilja implied, publish or perish needs to be changed and it needs to be changed by us. If you don't have anything worthwhile to say, don't say it. If we all exercise restraint and only publish what is really worth something (and we all know when that happens, and it doesn't happen that often) we can start to change things.

Yes, it may mean not landing the coveted "position", and it may mean teaching in community college or non-tenure positions. But isn't it worth the price to be able to say " I may have only published X number of papers, but they really meant something". I can't help but feel this is a personal responsibility issue (as most are).

Book Reviews

September 7, 2007
Categories: Book Reviews

Note: For a Romanian translation of this post, see here.

Felix Berezin

Misha Shifman has edited a wonderful book about the mathematician Felix Berezin, which recently appeared with the title Felix Berezin: Life and Death of the Mastermind of Supermathematics. Berezin was a Soviet mathematician largely responsible for many new ideas about “supermathematics”, working out the analog for anticommuting variables of many standard concepts in analysis. Path integrals for fermions crucially use an analog of the standard integral that is now known as the Berezin integral.

Berezin began his mathematical career working with Gelfand on representation theory. While Gelfand thought very highly of him, at some point the two of them had a falling out, which is alluded to without any details in several of the contributions to this book. Since Berezin’s mother was Jewish, his professional life was often difficult due to the anti-semitism that was prevalent in the Soviet mathematical establishment. Between this and being on the outs with Gelfand, he had continual problems with things like getting his papers published, as well as being able to travel or effectively communicate with people in the West.

Tragically, Berezin died at the age of 49, under somewhat unclear circumstances on a trip to Siberia he took with a geological team. The largest segment of the book is a wonderful and touching piece by Elena Karpel, who lived with him for many years (they had a daughter together, Natasha). Karpel describes their life together in detail, as well as the circumstances following his death. It is a moving portrayal of a complex relationship of two highly intelligent and cultured people, with one of them, Berezin, extremely seriously devoted to his work, one cause of stress in his relations with Karpel. Together with contributions from his colleagues, the book gives a fascinating portrayal of the mathematical culture that Berezin was an important part of.

With his interest in quantum mechanics, quantum field theory, path integrals, and anticommuting variables, Berezin helped to transform the field of mathematical physics into something much more modern. His book written during the sixties, *The Method of Second Quantization* remains one of the classics of quantum field theory. I remember being especially impressed by his paper with Marinov *Particle spin dynamics as the Grassmann variant of classical mechanics*, which gives an amazing interpretation of the physics of a spin-1/2 particle by invoking anti-commuting variables in a very simple way. The book contains a summary of some of Berezin’s scientific work by Andrei Losev, and this article is available on-line.

The Mathematician’s Brain

Princeton University Press seems to be trying to corner the market on popular books about mathematics, bringing out in quick succession a novel about mathematics (A Certain Ambiguity), a book about The Pythagorean Theorem, and two books trying to explain what it is that mathematicians do: How Mathematicians Think by William Byers, and The Mathematician’s Brain by mathematical physicist David Ruelle. The Ruelle book is the only one of the four that I’ve had a chance to read.

The New York Sun recently published a review of *The Mathematician’s Brain* by David Berlinski. It’s one of the great mysteries of the popular science book business why anybody publishes the writings of Berlinski. His recent claim to fame is as an affiliate of the Discovery Institute, critic of Darwinism and proponent of Intelligent design, but he has also authored various popular books, including some on mathematics. Some web-sites claim that he has a Ph.D. in mathematics from Princeton, but it appears that the truth of the matter is that he was in the philosophy department there, writing a doctoral thesis on Wittgenstein. His writings on math and science that I’ve seen over the years have always struck me as singularly incoherent and confused.

Berlinski actually doesn’t do that bad a job with the Ruelle review, picking up on one of the things that might interest mathematicians and physicists about the book, the part about Alexandre Grothendieck (I confess to skimming some of the material explaining what mathematicians do, since I spend far too much of my life watching them do it). Ruelle has some interesting stories to tell about Grothendieck and the IHES, where they both worked for many years. The IHES was founded in the late 1950s by Leon Motchane, who had studied mathematics before going into business. Ruelle describes well the IHES during the 1960s, including the various conflicts which existed between Motchane and the IHES members, one of which ended up leading to Grothendieck’s resignation.

Ruelle also has quite a lot to say about the structure of power in mathematics, and how the desire for recognition and honors motivates people. His portrayal of mathematicians is a very well-rounded one, examining not just how they do mathematics, but how they live their lives, noting that:

> But one should not forget that, besides beautiful mathematical ideas, there are many more obscure things that crawl in the mind of a mathematician.

Many of the footnotes in the back are well worth reading, such as one that tells us:

> As my wife puts it, there are fewer bastards and fewer frauds among mathematicians than in the general population, but maybe also fewer amusing people!

Ruelle also tells a favorite anecdote I’ve heard from several mathematicians. The version I’ve heard is somewhat different than Ruelle’s, and goes:

> At the Institute for Advanced Study in Princeton, a visitor once came up to Armand Borel and asked him
>
> “Do you know about algebraic groups?”
>
> Borel answered that, yes, he did. The visitor then went on
>
> “Good. Can I ask a stupid question then?”
>
> to which Borel responded:
>
> “That’s two already.”

La Theorie des Cordes

A colleague brought me back from France a science fiction novel written by the Spanish writer Jose Carlos Somoza. In French the book is called *La Theorie des Cordes* (String Theory), but the Spanish and English versions have the title Zig Zag. The plot revolves around a discovery about string theory that allows physicists to look back into the past. It begins with some promise, describing the world of theoretical physics as seen from Spain, with references to Witten and other theorists. But it soon degenerates into a long tale revolving around a threatened attractive young female scientist. The string is somehow responsible for forcing her into sexual depravity and the prospect of nearly infinitely long and horrific bloody torture, with time suspended and no end in sight. OK, I guess maybe this does have to do with present-day particle theory, except for the sexual depravity part…

Reviews by Atiyah in the Notices

The October Notices of the AMS contains very interesting reviews by Michael Atiyah of two books about Bourbaki: Bourbaki: A Secret Society of Mathematicians by Maurice Mashaal, and The Artist and the Mathematician by Amir Aczel. Atiyah speaks from personal experience, knowing many of the members of Bourbaki and their work well, and having attended one of the Bourbaki gatherings where they hashed out the text of one of their books. He gives an excellent summary of the Bourbaki story and its place in recent mathematical history, finding the Mashaal book to be both highly readable and reliable on the facts and personalities involved. As for the Aczel book, he's much more dubious. Aczel tries to claim an important impact of Bourbaki on sociology and structuralism via Claude Levi-Strauss, but Atiyah is not convinced by this, and takes issue with what Aczel has to say about Grothendieck, someone Atiyah knew well. Atiyah's characterization of Grothendieck goes as follows:

> I greatly admired his mathematics, his prodigious energy and drive, and his generosity with ideas, which attracted a horde of disciples. But his main characteristic, both in his mathematics and in social life, was his uncompromising nature. This was, at the same time, the cause both of his success and of his downfall. No one but Grothendieck could have taken on algebraic geometry in the full generality he adopted and seen it through to success. It required courage, even daring, total selfconfidence and immense powers of concentration and hard work. Grothendieck was a phenomenon.
>
> But he had his weaknesses. He could navigate like no one else in the stratosphere, but he was not sure of his ground on earth—examples did not appeal to him and had to be supplied by his colleagues.

He ends with the following critical remarks

> Aczel's total endorsement of Grothendieck leads him to make such fatuous statements as: "Weil was a somewhat jealous person who clearly saw that Grothendieck was a far better mathematician than he was." Subtle balanced judgement is clearly not Aczel's forte, and it hardly encourages the reader to take seriously his confident and sweeping assertions in the social sciences.

Comments

1. **chimbote**
   September 7, 2007

   I share your appreciation for Berezin's book and his paper with Marinov. Those were among my first reading assignments when I started my phd.

2. **Jason Starr**
   September 7, 2007

   You might also mention another quote from Atiyah's article, which seems to be a pronouncement on EGA (among other things):

   "Where I part company with Aczel is in his assertion that Bourbaki made a fatal mistake in his not taking Grothendieck's advice and rewriting its foundations in the new language of category theory. Aczel believes that Bourbaki had turned its face away from the future in not following Grothendieck. I doubt whether history will come to this verdict. Grothendieck's own EGA, as well as the general fate of the over-confident universalists, might suggest otherwise."

3. **Thomas Love**
   September 7, 2007

   It is easy to check if someone has a PhD in math. I went to the mathematics geneology website:

   http://www.genealogy.ams.org/

   and checked out David Berlinski. He isn't listed. Neither are you Peter, because your PhD is in Physics. There are two Thomas Loves. How weird is that.

4. **Moeen**
   September 7, 2007

   Thomas,

   Keep in mind that while the Math Genealogy website is a good resource, it is not comprehensive since all of the information on their database is entered voluntarily by individuals. So not only is it likely not comprehensive, but may even be wrong in some cases.

5. **John Baez**
   September 8, 2007

   The anecdote about Armand Borel only makes sense if you know his specialty was algebraic groups. He's the author of the well-known book *Linear Algebraic Groups*.

6. **Arun**
   September 8, 2007

   The anecdote about Armand Borel only makes sense if you know his specialty was algebraic groups.

   One can deduce that from the anecdote and the fact that it is worth repeating.

7. **Peter Woit**
   September 8, 2007

   In my experience, the anecdote is invariant under the interchange of Armand Borel and Andre Weil, as well as substitution of various fields of mathematics. In Ruelle's version it is Weil, and his is the streamlined, one question version: "may I ask a stupid question?" "You just did"

   Jason,

   I also was struck by that comment of Atiyah's, but I guess it's not too surprising. I wouldn't have guessed that he'd be a big fan of EGA.

8. **Chris Oakley**
   September 8, 2007

   Reminds me of this, concerning Dirac:

   > One famous lecture he gave in Canada: at the very end of the lecture during questions one of the members of the audience stood up and said, 'I don't

understand the equation you have written on the top right-hand of the blackboard'. Dirac was silent. People were waiting. And then one of the chiefs in the front row said, 'Professor Dirac would you care to respond to the member of the audience'. And he said, 'Well that wasn't a question it was a comment'.

9. **Clark**
September 9, 2007

Subject to being deleted for being utterly off topic, some of you may not have noticed that in last summer's Clay Institute, there were 3 self-contained lectures by Manin from the ground to the frontier. As an aside there were some very funny moments such as when he admitted that he could never remember how the weights are specified in modular forms.

http://www.uni-math.gwdg.de/aufzeichnungen/SummerSchool/SummerSchool_20060809-1600_Manin/avi/SummerSchool_20060809-1600_Manin_xvid.avi
http://www.uni-math.gwdg.de/aufzeichnungen/SummerSchool/SummerSchool_20060810-1430_Manin/avi/SummerSchool_20060810-1430_Manin_xvid.avi
http://www.uni-math.gwdg.de/aufzeichnungen/SummerSchool/SummerSchool_20060811-1430_Manin/avi/SummerSchool_20060811-1430_Manin_xvid.avi

10. **gaspard**
September 9, 2007

About the Aczel book: the only connection I have heard of between mathematicians and Claude Levi-Strauss is that a PhD student of Levi-Strauss had to stop ethnological trips for health reasons and then turned to math. His name is André Avez, who among other things coauthored a book with V.I.Arnold on Ergodic Theory in the 1960s. Perhaps there have been interactions with Bourbaki too, but I don't know them.

11. **David Berlinski**
September 9, 2007

I have never claimed to have a Ph.D in mathematics from Princeton University. My Ph.D. from Princeton is in philosophy. This is what my resume says; it is how I am described at the DI website; and it is how I am described on the dust jacket of my books. If there is a website that claims otherwise, I revile and denounce it. As long as I am correcting misapprehensions, I might add that I am a critic of intelligent design and not one of its supporters. In this regard, you might consider my essay "Has Darwin met his Match," in the December 2002 issue of Commentary. It is devoted perceptively to attacking Johnson, Behe and Dembski. I cannot say that my friends at the DI were pleased to see what I wrote, but they were made wiser by reading it. My feelings toward intelligent design remain what they have always been: Warm but skeptical. Nonetheless, I regard the general hysteria about these issues as intellectually disgusting. As for the question why so many editors are interested in publishing what I write, I suspect that this is because so many readers are interested in reading it.

12. **James Milne**
September 9, 2007

I was at IAS at the time of the "Borel" anecdote, and knew all the people involved. I believe my version is the most accurate.
http://www.jmilne.org/math/apocrypha.html

13. **Mike**
September 9, 2007

David Berlinski,

Thanks for answering the question, I am one of your readers, and find your "humor" interesting. You might have noticed a pattern of, as Dan says, " baseless ad hominem" !

14. **Peter Woit**
September 9, 2007

David Berlinski,

One of your most prominent on-line biographies

http://www.anova.org/bio/berlinski.html

starts off

"Berlinski (Ph.D. in mathematics, Princeton University) is a lecturer and essayist."

Another

http://www.researchintelligentdesign.org/wiki/David_Berlinski

starts off

"David Berlinski (born in 1942 in New York City) is a mathematician. He is the author of works on systems analysis, differential topology, theoretical biology, analytic philosophy and the philosophy of mathematics.."

and yet a third

http://www.coldwatermedia.com/berlinskireviews.shtml

claims that you were a "Fellow of the Faculty in Mathematics" here at Columbia. I don't know what this refers to since presumably it was long before my time (I've been here about 20 years).

You've done an excellent job of convincing people you're a professional mathematician, including PZ Myers who writes:

"This is a guy who is a competent mathematician with a degree from Princeton"

I'll stand corrected about your views on "Intelligent Design", but direct people to the same post by Myers for a discussion of your views on the theory of evolution:

http://pharyngula.org/index/weblog/comments/berlinski_i_cant_believe_im_wasting_time_on_this_guy/

And, to anyone who would like an excuse to start arguing about evolution here, don't even think about it.

15. **Peter Woit**
September 9, 2007

Many thanks to James Milne for the source for the more accurate anecdote, as well as the link to the web page of other ones. Anyone who is not aware of his web-site

http://www.jmilne.org/math/index.html

and the fabulous array of high-level expository writings about mathematics there, should definitely take a look.

16. **Peter Woit**
September 9, 2007

Gaspard,

Andre Weil did contribute an appendix (entitled "Sur l'etude algebrique de certains types de lois do mariage") to Levi-Strauss's book "Les structures elementaires de la parente"

17. **David Berlinski**
September 9, 2007

Thanks for the links. Of the three you cite, the first is incorrect. I shall correct it, The second and third are correct:

Witness

On Systems Analysis: An Essay on the Limitations of Some Mathematical Methods in the Social, Political and Biological Sciences, The MIT Press, 1976.

The Rise of Differential Topology, LITP, Universite Paris, 7, 1980.

My essays about theoretical biology, catastrophe theory, model theory (with Daniel Gallin), epistemic logic, and philosophy are scattered in various journals. Just look around: You'll find them. If not, I'll be happy to send them to you.

I am now more than sixty five years old, and a great many things in my life took place before your time. I was a Fellow of the Faculty in the department of mathematics — courtesy entirely of Lipman Bers — in 1973. I did nothing of value and achieved no distinction. I have never suggested otherwise.

I have made no effort to convince anyone of anything. On the other hand, I have taught mathematics for many many years, both in the United States and in France, and I have written more than eight books about mathematics. I regard myself as a writer — nothing but, but if anyone asks, I know a lot about mathematics.

I am quite sure that P.Z. Myers disapproves of my views concerning Darwin's theory of evolution. I have been exhilarated by his criticisms.

18. **Peter Woit**
September 9, 2007

Thanks for the clarifications about your mathematics activities. I suppose I should know about why readers are interested in your books since at one point a copy of "Black Mischief" was on my bookshelves (can't seem to find it, maybe it didn't survive one or another move over the years). Good luck with the Darwin denialism business…

19. **Mike**
September 10, 2007

I can't believe you found that link, Peter, lol.

And David, very much looking forward to "The Devil's Delusion" !

20. **Patrick Orlando**
September 10, 2007

I disagree with Prof. Woit's comments on Dr. David Berlinski's popular math books. I have read them all and he is an excellent writer able to describe the heart of the mathematical ideas in plane language. His style is very readable and his made up converstions with historical figures are very good. I only have a B.S. in Physics from Columbia so I guess they are on my level.

21. **The man**
September 11, 2007

I take two issues with Atiyah's comments on Grothendieck. First, his comment "both of his success and his downfall" betrays a nasty petty streak in Atiyah. I doubt Grothendieck, or a lot of other people, see his path after IHES as "downfall". In fact, Grothendieck, very courageously, resigned and went his own way because of his ethics and morality. Sticking to what you feel is right is not called "downfall", as least not in the ass-kissing power-hungry circles I'm guessing Atiyah moves in (given how far up the non-academic ladder he's gone). This phrase pretty much betrays Atiyah's worldview. Similarly, I'm sure Atiyah would classify Grothendieck's turning down Crafoord (or Perelman's decision) as blunder etc, however not the way others would see it.

Second, his comment that Grothendieck didnt deal with examples is meaningless if it doesnt interfere with Grothendieck's ability to do good correct maths. So he should have given some examples where Grothendieck was led to publish incorrect math cause of it.

22. **Peter Woit**
September 11, 2007

The man,

Your attack on Atiyah is really uncalled for. From my personal experience with him and everything else I know about him, he's an extremely decent and responsible person. He has gotten where he is through his fantastic mathematical achievements (which are at the level of Grothendieck's), and his encouragement of other people's. You claim that you are "sure" he would be critical of Grothendieck and Perelman for turning down prizes says more about you than him. I've never heard any evidence that he was critical of them for this, and I doubt that that would be his attitude at all. His comments about Grothendieck's style of mathematics not being grounded in examples, and his uncompromising nature being part of the reason he stopped doing math at the level he was doing it during the 50s and 60s are not at all controversial, but rather widely-held opinions by mathematicians who know the subject and its history. His comments on Grothendieck seemed to me well-balanced, recognizing his amazing achievements while not engaging in hero-worship.

23. **The man**
September 11, 2007

Peter,

You missed my point. I agree that he was not doing mathematics at his earlier levels post ’70. What I’m disagreeing is whether it should be called his “downfall”. For example, if some mathematician stops doing mathematics to address political issues or protest nuclear armament etc., that’s his priority (which some would call a very nobel one). Calling it a “downfall” is 1. very demeaning to the person, and 2. betrays your own priorities. If Atiyah’s priorities are not with urgent political issues, there is no reason to demean others whose are.

24. **woit**
September 11, 2007

The man,

I don’t think that Atiyah was specifically calling the decision to stop doing math and do other things Grothendieck’s “downfall” or criticizing his priorities. He was referring explicitly to Grothendieck’s “uncompromising nature”. If you read accounts by friends and allies who were very sympathetic to his political activities, I think you’ll find that many of them feel that the way he went about such activities was unproductive or even counter-productive, exactly because of this “uncompromising nature”.

I found “Recoltes et Semailles” a fascinating document to read, but at the same time a sad one. Grothendieck’s “uncompromising” personality unfortunately evolved in later years to something that could be described as paranoid and delusional, and I think this is actually what Atiyah had in mind by referring to a “downfall”.

25. **Jonathan Vos Post**
September 11, 2007

From the second of the AMS Notices articles on Alexander Grothendieck.

One striking characteristic of Grothendieck’s mode of thinking is that it seemed to rely so little on examples. This can be seen in the legend of the so-called “Grothendieck prime”. In a mathematical conversation, someone suggested to Grothendieck that they should consider a particular prime number. “You mean an actual number?” Grothendieck asked. The other person replied, yes, an actual prime number. Grothendieck suggested, “All right,take 57.”

26. **shameless ass-kisser**
September 13, 2007

The man,

I think you at least partly misinterpret Atiyah’s use of `downfall’. I understood that Atiyah was speaking about Grothendieck’s mathematics as much as anything else – the next paragraph he speaks explicitly of Grothendieck’s `weaknesses’. I warrant Atiyah feels he is as good a mathematician as Grothendieck, and in a position to point out weaknesses as well as strengths of the man’s approach to mathematics; moreover, there is a tendency (cultlike?) to idealize the math and the man, and Atiyah may be gently (I think he speaks gently) suggesting that neither is healthy. He later refers obliquely to the failure of EGA; this can only be taken as a judgment by a very serious mathematician that EGA did not attain its own goals. Atiyah also calles Grothendieck a `phenomenon’ and refers to his courage and concentration. Nowhere in these paragraphs does he refer to Grothendieck’s lifestyle or politics, except to say that Grothendieck’s failures as a mathematican, and his failures as a man, were both rooted in what Atiyah calls an uncompromising nature. And we should remember that Grothendieck was, at least, a terrible father.

27. **Carl Brannen**
September 21, 2007

Woit:

I may not like your blog, but I loved your book and couldn’t put it down. I recommended it to my father who bought it quite some time ago, but it sat around on his shelf. He has a PhD in math. His comment in an email today:

Started on “not even wrong”. I like the perspective
it gives.

La Faillite de la Theorie des Cordes?

September 11, 2007
Categories: Uncategorized

It appears that the release of the French edition of Lee Smolin's book (entitled Rien ne va plus en physique ! : L'échec de la théorie des cordes) has stirred up quite a lot of attention to the string theory controversy over there. A correspondant wrote to tell me that this month's edition of the French popular science magazine La Recherche has the controversy over string theory on the cover (*La theorie des cordes dit-elle le vrai?*) and four articles on the subject inside. Unfortunately I don't have a copy of the magazine or on-line access to the articles, but just to an English language summary. It's hard to tell from this exactly what's in the articles. One of them is an interview with the historian of science Peter Galison, who seems to describe string theory as having "initiated a new way of seeing, crucial for the future of physics." No idea what that is about, but I hope it's not about the string theory landscape….

The string theorists of the Paris region have a web-page, which recently has acquired a defensive section about *La faillite de la theorie des cordes?* It encourages people to read Polchinski's review of my book and Smolin's (my response to this is here), as well as papers critical of LQG. The same web-page also has links to other information sources about string theory, including to two blogs. Personally I don't think Jacques Distler's blog is much of an advertisement for the subject, but sending people to Lubos Motl's is a pretty funny thing to do….

Comments

1. **Jean-Paul**
   September 11, 2007

   From Amazon.fr:

   Biographie de l'auteur
   Chercheur en physique à l'Institut Perimeter (Canada), Lee Smolin a été baptisé " nouvel Einstein ".

   So French will take it seriously. They take anything written by Americans very seriously.
   By the way, there are no serious superstring defenders in France, so it's a completely different battle ground than elsewhere.
   Unlike in Germany, students are able to escape to more interesting research areas.
   You will see talk-shows trashing strings etc — it will be really amusing…

2. **Bee**
   September 11, 2007

   Faites vos jeux, Ladies and Gentlemen, rien ne va plus.

3. **Coin**
   September 11, 2007

   It encourages people to read… papers critical of LQG

   Actually, I'd be curious what those are.

   What are the papers that critics of LQG would be likely to recommend on the subject? I lack the french skills to effectively tease this out from the link.

4. **Peter Woit**
   September 11, 2007

   Coin,

   To find the papers you just need to click on the links in the relevant paragraph. Don't worry, no danger that you'll hit a link that is not critical of LQG…

5. **SnarkFest**
   September 11, 2007

   Peter Galison was interviewed recently for the **Scientists' Nightstand** (*American Scientist*). His acknowledged influences and recent reading are somewhat revealing.

   More on Galison from Steve Hsu:

   > Both my friend and I are great admirers of Galison. After earning his doctorate in the history of science, he wrote a second dissertation in particle theory under Howard Georgi while a junior fellow at Harvard. Other than particle theorists turned science historians like Sam Schweber or Abraham Pais (see here and here), I can't think of anyone more qualified to work on the (underdeveloped) history of modern physics.

6. **Who**
   September 11, 2007

   in case anyone is unfamiliar with *la faillite* the website name translates to

   The Bankruptcy of String Theory?

   and this is a damage control website concerned with defending the self-respect and self-importance of the string theorizing community.

   this must be that the idea of string bankruptcy is already current in
   people's minds, so therefore the defenders wish to challenge
   this prevalent notion, so they put a question mark on it and call
   their site: String Bankruptcy?

   a kind of Enron case judging by this interesting French reaction.

7. **chris**

September 12, 2007

“Actually, I’d be curious what those are.

What are the papers that critics of LQG would be likely to recommend on the subject? I lack the french skills to effectively tease this out from the link.”

read this: hep-th/0501114

really fun and good to see on what kind of shaky assumptions LQG is built, too. my personal favorite: the ‘pulverization’ of space to obtain vanishing inproduct of graphs on different support.

8. **Lee Smolin**
September 12, 2007

The issues raised by those Nicolai et al were not new and were well understood already in the LQG world many years ago. They are the main reason many workers switched to formulating dynamics in a path integral formulation in the late 90s, and they do not apply to those formulations of dynamics. They also do not apply to the currently studied approaches to the hamiltonian formulation of dynamics such as the master constraint formulation. Thus, the criticisms of that paper ignore roughly the last ten years of work in the field.

There have been several responses to that paper, including arXiv:hep-th/0608210 and arXiv:0705.2222. This has also been discussed in previous blog entries, which I assume you can find by searching.

The vanishing of the inner product between states of different support is not an assumption and not shaky, by the uniqueness theorem proved in gr-qc/0504147 and math-ph/0407006 it is a necessary consequence of the requirement of having a diffeomorphism invariant measure on functionals of a connection.

Lee

9. **Christophe de Dinechin**
September 13, 2007

I subscribe to La Recherche, and have read this article. There is not much in there that you won’t know already. I would guess that this is mostly a news article related to the French translation of Lee Smolin’s book, which was recently published here.

The first part that is essentially an interview of Lee Smolin with a bit of historical background and context. The second part is an alternative viewpoint entitled “4 successes of strings”, these being: a quantum description of gravity, the possibility to unify all forces, better understanding of astrophysical phenomena, beginning of an answer on the nature of space-time. The third part lists various competing space-time theories, unfortunately omitting a pet favorite of mine (Nottale’s scale relativity), but referring to LQG, CDT and non-commutative geometry. Finally, part 4 is an interview with Peter Gallison on the criteria to judge a theory, where he essentially answers that a good theory is one we can apply, like Newton’s laws, even after we realize it’s wrong…

About “faillite”, while “Who” is right that this word in French may translate as “bankrupcy”, it may also translate as “failure”, and that’s how I personally read it in that particular context (as I suspect most other French natives would).

I have made a quick reference to that article in a recent post on [my own blog](), although this is not the main topic of the post. Peter, feel free to delete that last paragraph if you feel it’s off topic.

10. **Who**
September 13, 2007

That’s interesting Christophe,
how would you translate this phrase in the French title of Smolin’s book which Peter gave in the original post?
L’échec de la théorie des cordes

Does *faillite* convey a different flavor from *échec”?

11. **Peter Woit**
September 13, 2007

Who,

“Echec” is more like “defeat”, or incomplete defeat, as in “setback”. The word comes from chess (“check”, but not necessarily “mate”).

“Faillite” is well-translated as “failure” in this context (same root), but also specifically is used to mean bankruptcy (economic failure), so carries some of that connotation.

12. **Christophe de Dinechin**
September 13, 2007

Hi Who,

Peter seems to know French pretty well. I would add that “faillite” also conveys a sense of lost credibility, and in that sense, it may be a better translation of the original intent.

13. **Who**
September 14, 2007

Thanks both Peter and Christophe. I was interested in how you hear that.
Trying to understand the French context better I just checked amazon.fr and found Smolin’s book (which has a preface by Alain Connes) had salesrank 2021, while for comparison the two Brian Greene books in French translation had ranks 13,720 and 22,437. I could find no other string books with better sales than those two. So it appears that if a French reader picks up a book about string theory it is most apt to be the one by Smolin.

Peter mentioned the etymology of *faillite* so I glanced in the the Petit Larousse (the Webster’s analog, not a French-English dictionary) which gives the primary meaning as the commercial one, bankruptcy in other words. Larousse explained the etymology by saying the word came into French from the Italian FALLITO. This has the primary meaning of bankruptcy in Italian, same root as “default” I suppose. It also has the secondary figurative meaning of failure. Thought you might like to know the French word does not derive from the French verb faillir “to err, to miss, to fail etc”, but instead was brought over from Italian and was originally applied specifically to commercial collapse: bankruptcy.

I agree with Peter that *echec* has a much less drastic connotation. Setback is very good. To hold an enemy force in echec is to hold it “at bay” rather than destroy it. The word is more like “unsuccess” than failure or collapse.

So the book subtitle merely suggests that string has stalled and is not making progress, while the website name uses the actual word for bankruptcy, connoting collapse and, as Christophe suggests, loss of credibility.

It’s interesting how words sound to different people. Thanks again for “lending me your ears.”

14. **Peter Woit**
September 14, 2007

Who,

I don’t think it makes sense to compare sales figures for books that have come out recently with ones that have been out for a long time. I’m quite pleased with how well my book sold, and Lee’s sold even better, which is great. At least in the US though, I don’t think either of our books came anywhere near doing as well as Brian’s, and suspect the situation in France isn’t that different.

In any case, no matter what the sales figures, the two books have done remarkably well in terms of getting attention for the issues that both of us were trying to raise, and providing a counter-balance to the overly optimistic story about string theory that was dominant until recently.

15. **Who**
September 14, 2007

My goodness! I see that the French edition of your book is due to be released in a couple of weeks—-3 October 2007.
http://www.amazon.fr/M%C3%AAme-pas-fausse-th%C3%A9orie-question/dp/2100503936/r

MÊME PAS FAUSSE

and they arent even doing hardbound first, they are going right to the paperback edition for the, as it were, mass market. Market should be ripe.

Looks like old one-two punch. French Smolin comes out in April and French Woit in October.

Hope you take some earthly pleasure in this, Peter.

16. **T.**
September 15, 2007

About “Même pas fausse”, yet another frenchman’s opinion:

Unfortunately, I’m not sure “Not even wrong” can really be translated in french without conveying a childish meaning.

This is because in france “Not even true” is a phrase that is really only used in playgrounds. It’s a 100% clue that the person who said that is a child. So “Not Even Wrong” has a very strong childish argument flavor, and is less powerfull than in english.

But I hope I’m wrong and it will sell, cause the book is great.

17. **Who**
September 15, 2007

T. please give an example of a situation in which the phrase might be used.
Peter sorry if this is off topic, I’m curious to know how the phrase comes across in French.

18. **T.**
September 15, 2007

mm, well it would go something like:

Child 1: “You looser !”
Child 2: “Not even true ! You’re the looser !”

I insist that no adult would ever use “même pas vrai”, except if he deliberately wanted to look childish for some reason.
So “Même pas faux” is funny and smart, but still is associated to an argument taking place between children. (in my humble opinion)

19. **Christophe de Dinechin**
September 15, 2007

I agree with T. It’s also associated in my mind with another childish phrase, “même pas mal” (I’m not even hurt), which a three years old would typically say after falling from the cupboard where he was trying to steal forbidden sweets.

All in all, I find it’s a rather good title, but with a very different connotation from the original “Not even wrong”, even if it’s a litteral translation. Funny.

20. **Hendrik**
September 17, 2007

Another review of “The trouble with physics” just appeared on the ArXiv at:

http://xxx.lanl.gov/abs/0709.1728

21. **Coin**
September 17, 2007

Hm. Are book reviews on the ArXiv common?

Various and Sundry

September 11, 2007
Categories: Uncategorized

It seems that if you're a Fields Medalist, you now have to have a blog. The latest of these is a new blog from Timothy Gowers. His blog will also function as a blog for the upcoming Princeton Companion to Mathematics that he is editing, and he has started a discussion about the possibility of a wiki devoted to "mathematical tricks".

Rigorous Trivialities is another new mathematics blog, one of the rare ones not being run by a Fields Medalist.

Mathematics will now have its own "rumor mill" to gather information about job searches, to be called the Mathematics Job Wiki. It appears to have been set up by Greg Kuperberg, "who however recuses himself from handling confidential e-mail and is not the wiki moderator". All we are told about the moderator is that "someone without a current tenure-track appointment will read e-mail sent to the Wiki Moderator."

Gerard 't Hooft has translated his lecture notes on Lie Groups and Physics from Dutch into English, increasing by about two orders of magnitude the number of people who can read them.

Math and physics geeks are now certifiably cool, as the TV show Numb3rs goes into yet another season, and is joined by The Big Bang Theory. New York magazine got together a group of Columbia physics grad students to take a look at the show and discuss.

The early history of string theory is getting lots of attention these days, especially because of a conference on the subject last May. Some related articles have now appeared on the arXiv, from Di Vecchia and Schwimmer, Ramond and Schwarz. At Caltech, an Oral Histories project has made available the transcript of a long interview with Schwarz.

Hendrik, a commenter here, pointed out that there's more of the latest string theory hype concerning results from the MAGIC telescope, originally discussed here. Now New Scientist has weighed in with an article entitled Finally, a MAGIC test for string theory? According to the article, Mavromatos and collaborators say that their (non-critical) string theory model "predicts the 4-minute delay exactly". Polchinski is quoted to the effect that this would falsify (critical) string theory. LQG is completely cut out of the deal, with no mention of it at all. They really need to do a better marketing job. The way things are now, any supposed evidence of quantum gravitational effects is automatically evidence for string theory, in one version or another.

For the latest attempt to market string theory to astrophysicists, see this new article on astro-ph. The abstract begins not by acknowledging that string theory can't make any predictions about cosmology, but by claiming instead that the problem is

> Attempts to connect string theory with astrophysical observation are hampered by a jargon barrier, where an intimidating profusion of orientifolds, Kahler potentials, etc. dissuades cosmologists from attempting to work out the astrophysical observables of specific string theory solutions from the recent literature.

Update: Slashdot has a thoroughly worthless article about this last paper, based on the New Scientist article about it.

Comments

1. **Zul**
   September 11, 2007

   « 1
   This lecture course was originally set up by M. Veltman, and subsequently modified and extended
   by B. de Wit and G. 't Hooft.
   »

   The notes are not 't Hooft's lecture alone.

2. **Thomas Love**
   September 11, 2007

   The Princeton Companion to Mathematics articles are great. Thanks for the link.

3. **Hendrik**
   September 11, 2007

   Dear Peter, thanks for having a look at the NewScientist MAGIC-string article. NS seems to be giving a bit of limelight to strings at the moment;- there are two more recent string + cosmology items at:

   http://space.newscientist.com/article/mg19526204.000-heretical-cosmologist-does-away-with-the-big-bang-.html

   http://space.newscientist.com/article/dn12628-can-string-theory-accommodate-inflation.html

4. **Peter Shor**
   September 11, 2007

   Amusingly enough, in his book Smolin says that some of the first people to propose doubly special relativity were studying non-commutative geometry, which has been completely left out of the current race for credit (or blame?).

5. **Luboš Motl**
   September 12, 2007

   LQG is cut out not because their marketing is weak – e.g. Smolin's marketing would instantly allow him to work in the Amsterdam's Red Light District – but because the physicists who work on this "theory" are not serious scientists.

6. **gunpowder&noodles**
   September 12, 2007

   Well, Lubos, I would guess that it is *far* safer to patronize the girls in Amsterdam than to rely on the services of some *free-lance* harlot. If you see what I mean.

Anyway, I hope that PW is not insinuating that critical and non-critical strings are on the same plane. Clifford Johnson claims that they are, but Lubos set him straight on that one: non-critical strings, lke non-critical humans, are definitely inferior to the critical variety. So a falsification of critical strings would be a heavy blow to most string theorists.

7. **Johan Couder**
   September 12, 2007

   "Gerard 't Hooft has translated his lecture notes on Lie Groups and Physics from Dutch into English, increasing by about two orders of magnitude the number of people who can read them."

   Now, now, Oppenheimer learned Dutch in 6 weeks and even lectured in the language ! Not that difficult for a theoretical physicist.

8. **Peter Orland**
   September 12, 2007

   Oppenheimer was already fluent in German
   (he worked in Goettingen), and with six weeks
   and a good mind, it doesn't seem impossible
   to learn Dutch (the two written languages are
   rather similar). I wonder if this comment will
   anger any Dutchmen out there.

9. **chris**
   September 12, 2007

   "LQG is cut out not because their marketing is weak – e.g. Smolin's marketing would instantly allow him to work in the Amsterdam's Red Light District – but because the physicists who work on this "theory" are not serious scientists."

   says who? your infailable papal highness? this is the kind of quality contributions we need for constructive discussions. thank you.

10. **Warren**
    September 12, 2007

    Did you want to give a link to that astro-ph article in the last paragraph?

11. **Peter Woit**
    September 12, 2007

    Thanks Warren, link added.

    It's the same paper that got promoted in New Scientist, the second of the two links Hendrik gave. The point of the paper is just to show, for one special version of string theory and the simplest possible choice of a Calabi-Yau, that the moduli fields don't give you the kind of scalar potential you want for inflation.

12. **spring theorist**
    September 12, 2007

    hey lubos, it seems that this blog is the only game in town… 🙂

13. **D R Lunsford**
    September 12, 2007

    Thanks for the t'Hooft lectures. Those things are always fun to read. His presentation is startlingly clear.

    -drl

14. **Bee**
    September 12, 2007

    Reg. MAGIC, see also: [MAGIC's observation of gamma ray bursts]

15. **jkowalskiglikman**
    September 12, 2007

    Peter Shor was kind enough to mention the non-commutative side of DSR.

    However DSR based on non-commutative kappa-Minkowski space and kappa-Poincare algebra does not predict any sizable modification of GZK cutoff (and it seems that we have been right in that) and also no energy dependent speed of light (so that if the MAGIC result is confirmed we are out of business.) Thus there is no reason for us to take part in the race for credit in this case.

16. **Amitabha**
    September 12, 2007

    It's a sad year for strings if Warren Siegel doesn't write a parody even after the end of the Strings conference.

17. **Who**
    September 12, 2007

    The only QG approach I think likely to gain credence if the MAGIC result is confirmed is Martin Reuter's QEG, with its surprise finding that gravity is renormalizable. Dispersion can arise from the running of the action at high photon energies, and the corresponding running of the metric. This paper, for instance, only considers dispersion of massive particles, not photons, but can nevertheless give some idea
    http://arxiv.org/abs/gr-qc/0607030
    Modified Dispersion Relations from the Renormalization Group of Gravity
    F. Girelli, S. Liberati, R. Percacci, C. Rahmede (SISSA and INFN)

(In reply to Bee's post with link to MAGIC discussion at her blog, and also to Jerzy Kowalski-Glikman's comment that confirmation of MAGIC result would put DSR out of business. It certainly **might** be confirmed and there ought to be at least one approach that would prosper from this. :D)

18. **Hendrik**
September 18, 2007

On "The Edge" at:

http://www.edge.org/3rd_culture/einstein07/einstein07_index.html

there is a debate between Brian Greene, Walter Isaacson and Paul Steinhardt where the loose subject is Einstein's scientific values. However, after the first third of the debate, the main topic very much becomes string theory, with some pretty sharp criticisms aired against string theory (mostly known to the readers of this blog). I'm not sure that those points are satisfactorily answered, but at least Brian Greene does admit there are problems.

19. **Peter Woit**
September 18, 2007

Thanks for pointing that out Hendrik, I suppose I should write a short posting about it…

20. **Hendrik**
September 20, 2007

Thanks Peter, I enjoyed your review. It seems to be hard to keep the monster of the Anthropic "principle" under control;- at the moment Steven Weinberg is embracing it at NewScientist at: http://www.newscientist.com/blog/space/2007/09/is-there-human-link-to-dark-energy.html#nbicomments

21. **Aaron Bergman**
September 20, 2007

Embracing it? He started it.

22. **LDM**
September 29, 2007

"*Now, now, Oppenheimer learned Dutch in 6 weeks and even lectured in the language ! Not that difficult for a theoretical physicist*"

…you may wish to know that Oppenheimer, upon learning that two of his friends were reading Dante in the original, also spent a month to learn enough Italian to read Dante outloud to them.
Dirac was unimpressed, and told him he was wasting his time. Indeed, Dirac one time refused a couple of books that Oppenheimer offered him since "reading books interfered with thought".

The one language, other than English, that would be essential for a physicist to learn is German, since there are so many great physics papers, in German, that have not been translated to English.
As example, Sommerfeld's ingenious treatment of the diffraction problem, solved exactly using images on a Riemann surface, in his 1894 paper, was *just* recently translated into English in 2004.

Einstein's collected papers became available in 1987 in English (prior to that you could get Einstein's works in Russian, however 😉)

Physics World on String Theory

September 13, 2007
Categories: Uncategorized

Despite my abusive treatment of his article Stringscape here recently, Matthew Chalmers was kind enough to send me a copy of the September issue of Physics World, which contains three shorter pieces about string theory (available on-line only to subscribers).

One of the articles is by Fred Goldhaber and entitled *Scientific faith put to the test*. It's a scathing attack on the anthropic string theory landscape program, describing it as "antiscience" (rather than my favorite, "pseudo-science"). Goldhaber characterizes this sort of research as "antiscience of the left", with its adherents promoting the idea that we can't ever understand some things since they are due to chance. He contrasts this to the "antiscience of the right", which promotes the idea that we can't understand things because they come from supernatural origin, and finds both attitudes equally unscientific. As for where antiscience comes from, he has this to say:

> On the left, I think that it stems from arrogance ("If *I* can't figure it out, no-one ever will"). On the right, I think it comes from defensiveness ("If science is right, religion must be wrong, and that can't be"). In the end, antiscience on both side boils down to vanity. While we need to stay alert for the vanity of those advocating antiscience, we also should guard against vanity in the name of science.

He ends on a more optimistic note, writing that he does see a difference in those on the "left". They remain physicists, and if someone finds a "promising route to picking out the right solution to string theory", they would leap to pursue it. He doesn't speculate on what they would do if someone shows that string theory just inherently can't ever predict anything…

Philosopher of science Steven Weinstein has a piece with the title *Philosophy pulls strings*, which tries to make the case that string theory is leading to some new interaction between physics and philosophy, since it "forces us to tackle issues that cross both disciplines." As far as one of his topics goes, the anthropic pseudo-science, the main role I see for philosophers is to forcefully point out to the scientists involved that they are doing something intellectually highly disreputable and should stop. He also discusses a much more non-trivial and interesting topic, that of the philosphical questions about space and time raised by quantum gravity, a subject where philosophers may or may not end up having something quite useful to contribute.

Philosphers Nancy Cartwright and Roman Frigg contribute a very interesting article about how scientific theories are evaluated, entitled *String theory under scrutiny* . The make the important point that immediate experimental testability of a theory is not all there is to deciding whether something is science or not. When scientific ideas are new, they often are not understood well enough to be able to extract definitive predictions from them. Theorists are generally engaged in research programs, the end result of which is supposed to be something experimentally testable. In order to evaluate a research program, you can't just note that it isn't predicting anything, you have to evaluate its prospects for reaching its stated goals. They describe good research programs as "progressive":

> Good research programmes are those that are progressive, i.e. those whose theories get better and better, even if individual theories face serious difficulties at certain times.

The fundamental problem with string theory is that, as far as its central goal of unifying physics goes, over the last nearly 25 years it has not only not made any progress toward explaining anything about particle physics, but, quite the opposite. Everything that has been learned about string theory makes it more and more clear that the original hopes for getting unification this way were just misguided and can't work. The derivative here is the wrong sign.

There are areas in which string theory has had successes, notably in mathematics and in strongly-coupled gauge theories. But these are really different research programs, and the fact that progress has been made in them doesn't change the facts about the colossal failure of the unification program. Cartwright and Frigg try and put various other "dimensions" on the string theory research program, including that of "elegance and simplicity", writing that:

> Radical string critics would then conclude that string theory is progressive only in the dimensions of elegance and simplicity (in the sense that the theory only contains one class of basic objects – strings – from which all the basic particles and forces follow), while being largely stagnant in the other dimensions.

As a "radical string critic", I don't see things this way. According to M-theory, "string theory" is not a theory of "one class of basic objects". Strings are just part of a hugely complicated picture, one which at the moment is neither elegant nor simple. String theorists hope that there is some elegant and simple underlying theory, but they have not been able to come up with it despite a huge amount of work. Whatever underlies M-theory, it may be something very complicated. Perhaps M-theory is just a rather obscure corner of a story very different than what string theorists are hoping to find, one that may tell us some interesting things, but just doesn't have anything to say about how to unify particle physics.

Comments

1. **M**
   September 13, 2007

   Now I understand why Lubos hates the anthropic string landscape: it is "antiscience of the left".

2. **R**
   September 13, 2007

   The article by Cartwright and Frigg is available here.

3. **Jon**
   September 13, 2007

   Hi Peter,

   While your views do have merit, I don't think the current string-theory situation will change any time soon. Supposing string theory *is* found to lack in its substantive goal of unifying physics, what other alternatives do you think has the same degree of appeal… I'm not aware of any. (Certainly LQG and the host of other programs don't carry the same intellectual appeal and challenge to grad students and postdocs).

   In short, either strings or nothing.

4. **Peter Woit**
   September 13, 2007

   As for what has the same degree of appeal as a failed idea that has become a pseudo-science (the landscape), well, actually just about anything at all….

More seriously, I've always said that the problem is not that there are good ideas about unification being ignored, but that there aren't good ideas (at all, string theory now clearly isn't one). So the question is how to change the reward structure to encourage people to come up with new ideas. I don't see any evidence that particle theorists of any influence are taking this problem seriously. At the moment the attitude seems to be that the best thing to do is to hide one's head in the sand and wait for experimental results from the LHC to save us. From what I've been hearing lately, people may have to keep their heads in the sand longer than they expected….

5. **Elegant Simpleton**
   September 14, 2007

   Is Yang-Mills field theory elegant or simple? To see the elegance one needs to have studied for many years, both geometry and physics. To see the simplicity – well I don't see that it's all that simple – unless one sees it within the context in which it arose – in which case one needs to know a lot already.

   The criteria of elegance and simplicity are aesthetic judgments made within the context of expertise. To find a particular treatment of Galois theory elegant one has to first know several treatments of Galois theory, understand them, and have thought about the difficulties involved in describing the theory coherently. Not everyone has the preparation to do this. To someone without the preparation, no approach appears either elegant or simple.

   Pauli's way of handling tensor algebra was neither elegant nor simple, though it worked.

6. **Kasper Olsen**
   September 14, 2007

   Peter,

   Nancy Cartwright and Roman Frigg are philosophers; they are able to throw names like Kuhn, Lakatos and Popper into the stringscape, but not much else.

   If you tell them, that e.g. the mass shell condition is this and that, they will have no idea what you mean; if you tell them, that the string sigma model is this and that, they will have no idea what you mean; and if you tell them, that the string tension is related to the Regge slope parameter by a certain relation, they also will have no idea of what you mean.

   Elegant Simpleton is correct here in saying, that "The criteria of elegance and simplicity are aesthetic judgments made within the context of expertise" and therefore their views on a string is of little interest. Except for other philosophers, I guess.

7. **dragon**
   September 14, 2007

   The record of philosophers intervening in physics is mixed. Sometimes it has been worthless; other times it has made more sense than what physicists have said about the same issue. The most extreme example of this that I know is the case of work on the arrow of time. Huw Price

   http://www.usyd.edu.au/time/price/

   has made genuine contributions to our understanding of this. Whereas Lubos Motl claims that the arrow arises from the structure of language, from the way we ask questions. That's right, LM thinks that a postmodernist analysis is the only way to go. In short, if you want real philobullshit, an over-confident physicist is the man to ask. So the views of philosophers on physics should not be dismissed out of hand.

8. **Chris Oakley**
   September 14, 2007

   > So the views of philosophers on physics should not be dismissed out of hand.

   Being less crazy than LM is not really setting the bar very high.

9. **Thomas Love**
   September 14, 2007

   I'm not comfortable calling string theory "Antiscience", it is more specialized, so call it "Antiphysics". "Certainly Repulsive AntiPhysics".

10. **tytung**
    September 14, 2007

    The relation between philosophy and fundamental physics is subtle and complex. Remember, Einstein himself is more philosophical, and less mathematical, than most of his contemporary physicists.
    That the philosophers are not equiped with detailed physics knowledge doesn't mean that they are unable to give valuable insights into it.
    As I see it, all science is to investigate nature within a set of assumptions, philosophy attempts to step outside and examine them.

11. **Christophe de Dinechin**
    September 15, 2007

    I liked the observation that bad science can stem from arrogance. I believe that there is a big gap between what is actually being delivered and what is actually known when you look in the details.

    I've tried to put that in a humoristic way, imagining what would happen if MacOS X had been introduced by Brian Greene instead of Steve Jobs. I'm not sure anybody other than me will find it funny 😉 but I thought I'd share anyway.

12. **dragon**
    September 16, 2007

    "Being less crazy than LM is not really setting the bar very high."

    But a lot of people respect his views on technical questions in physics [if in no other field]. So he provides an example where someone supposedly competent in physics has been led into serious error, in *physics*, by doing philosophy badly.

13. **Eric**
    September 16, 2007

    There's nothing wrong with LM's knowledge or views on physics. However, he just appears to be very intolerant of those with views which differ from his. In regards to those with crackpot views (and let's be honest, there are a lot of these types populating the blogs), I can sympathize. However, he can also be extremely rude and insenstive to even those who are not crackpots.

Of course, in regards to his views on other subjects, LM is clearly a crackpot himself.

14. **Kris Krogh**
September 16, 2007

The philosopher Grete Hermann correctly identified a fatal error in John von Neumann's pivotal "proof" of the impossibility of hidden variables in quantum mechanics. While that proof was widely cited, she was totally ignored for twenty years, until John Bell took up her case. She is still little-known, since the history of physics is written by physicists. (Usually ones with a preference for the Copenhagen interpretation of quantum mechanics.)

15. **lisistrata**
September 17, 2007

> There's nothing wrong with LM's knowledge or views on physics.

If you're talking about his published work, I believe you. I've not read it, so there's no reason for me to doubt your words.

If you're talking about his views on physics as they appear in his blog, then I strongly disagree with you. Statements like the SU(2) symmetry group of weak interactions can be derived from unitarity, or that the existence of neutrinos can be deduced from the existence of charged leptons, or that experimental results are neither important nor needed, that's what I would call plain ignorance both of physics and its history…

16. **Kasper Olsen**
September 17, 2007

Kris,

Grete Hermann was also a mathematician, and this alone made it possible for her to understand the math involved; just being a philosopher would not have been enough to identify this "error" in von Neumann's argument.

17. **Kris Krogh**
September 17, 2007

Kasper,

Yes, Grete Hermann was also a mathematician. But I think Tytung has a point:

As I see it, all science is to investigate nature within a set of assumptions, philosophy attempts to step outside and examine them.

She avoided an assumption of von Neumann's, which David Mermin describes as "silly." (Maybe I would use a different adjective.) Why, in twenty years, didn't physicists notice a silly mistake in a fundamental proof?

We need more people willing to think outside the box, like Einstein and Bell.

18. **notaphilosopher**
September 17, 2007

Eric said: "There's nothing wrong with LM's knowledge or views on physics."

I think you are missing dragon's point. He is referring [I think] to an argument LM had with Anthony Aguirre [a genuine expert] about the origin of the second law of thermodynamics. To everyone's surprise, LM went all philosophical and claimed that the low entropy at the beginning of time does not need to be explained because when we ask questions, we always ask about the future based on what we know about the past. *Therefore* there is nothing to explain. Here is a concrete example where LM was talking nonsense *about physics* because he got the philosophy wrong. Of course everyone who disagrees is an idiot, is "humiliating Boltzmann" etc etc all the usual crackpottery, but that is just the icing on the cake: the point is that LM became a *physics* crackpot, at least for the time being, because he thinks he can play philosopher. Of course Kasper O might argue that LM might have avoided making a fool of himself by staying away from philosophy altogether and trying to think about a physical basis for the arrow of time…….

US/LHC

September 13, 2007
Categories: Uncategorized

Yesterday a new web-site was launched by the DOE and NSF, called US/LHC, which will be devoted to the role of the US in the LHC project. Besides news and descriptions of the science and the experiments, it will also include blogs by several physicists involved in experiments at the LHC. This new web-site joins several other similar ones, most notably one devoted to the ILC, and an umbrella one for US particle physics called Interactions.

I've sometimes wondered whether this huge publicity onslaught for the LHC is a good idea. Just as this new web-site is coming on-line, I'm starting to hear unconfirmed reports of possible very serious delays in the LHC startup, ones which may push back the beginning of experiments by a year or more. The current schedule includes no extra time for cooling down sectors of the machine which have to be warmed up to deal with one problem or another, and this cooling is a tricky months-long process. If these rumors turn out to be true, this will be good news for the Tevatron, which will have the energy frontier to itself for longer than expected. But it will definitely be very bad news for CERN and for particle physics in general, both of which have just about all of their eggs in this heavily publicized basket.

Update: From the comments here and e-mail I'm getting, it appears that others are hearing these same rumors: the first physics runs are likely to be in 2009, not 2008, due to problems that have shown up as they have started cooling down some sectors of the machine.

Update: Peter Steinberg at the US/LHC site blogs about the conundrum of whether he should be dealing with "gossip from unverified or anonymous sources", and decides he'd better not. I suspect one consideration is that his blogging role puts him in a sort of unofficial spokesman capacity, which is rather incompatible with rumor-mongering. On the other hand, I don't have this problem…

An informed commenter reports in the comment section about details of some of the problems that have cropped up in the last month, and that the "best guess" for the delay that these will cause is about two months. This would move the start of a physics run from next July to next September.

Update: Via the Resonaances blog, here's the video of a September 13 colloquium talk by Lyn Evans about the LHC commissioning. Evans describes in detail two of the problems that have shown up that motivated some of the rumors: leaks that have appeared during the first cool-down of certain sectors of the machine, and problems with some of the plug-in modules that interconnect the magnets. It remains unclear if these problems will cause slippage in the schedule, and if so, how much. News about what is going on with these problems is posted here.

Comments

1. **Paul Guinnessy**
   September 13, 2007

   My story in this month's Physics Today magazine Multiple problems push LHC start to next spring might answer some of your questions.

2. **Peter Woit**
   September 13, 2007

   Thanks Paul,

   I did see your article, and what I've been hearing recently involves some of the same problems you describe there. But what I've heard is that the problems are more serious than originally thought, and that the startup date of next July will have to be pushed back. Maybe this is wrong, so I'd be curious to know if your sources for that article are still saying that things are on track for next July.

3. **A quantum diaries survivor**
   September 14, 2007

   Hi Peter,

   I also am hearing of about one year of delay of startup (pilot run end 2008, first data end 2009), due to some structures that may have broken during cooldown and need replacement. Yours is the fourth source of more or less the same information, so I would say it may well be not confirmed, but it is a pretty solid piece of bad news.

   Cheers,
   T.

4. **Thomas Larsson**
   September 14, 2007

   This is sad news. However, Tevatron II also had a lot of initial problems, not to mention the SSC debacle. The CERN people have spoiled us with a track record of completing their projects on budget and ahead of time.

5. **Coin**
   September 14, 2007

   So these USLHC blogs. Should we expect them to be more or less independent? Or will the DOE or somebody be looking over their shoulders every moment monitoring the content?

   I really like this idea of scientists on big projects like this blogging, because it seems like potentially a great way for the public (i.e. me) to get interesting and unusual information about the experiment as it progresses. Unfortunately, this potential seems to be in direct conflict with the NSF/DOE's likely goal in setting up this web page in the first place– that is, as a vehicle for generating publicity for the experiment, probably positive publicity.

   Let's say the most pessimistic comments on this page turn out to be right, and the LHC has some delays coming. Will we see candid posts from the USLHC scientists about the delays and what they mean as they happen? Or will these blogs be only for reporting *good* news?

6. **A quantum diaries survivor**
   September 14, 2007

   Coin,

I can't answer your question, but what I can say is that I am blogging, I work in CDF and CMS, and I am rather erring on the side of dispersing more news than I should, according to many of my colleagues. That is because I believe outreach is more important than obsessing about what the funding agent could read in distorted press reports.

So the bottomline is, read it in my blog!

Cheers,
T.

7. **Peter Woit**
September 14, 2007

Coin,

An interesting question. If these reports turn out to be accurate and serious delays are ahead, it will be interesting to see how the bloggers and the website handle it. It looks like the website itself will be maintained by people hired specifically to do outreach for the project, and their job depends on putting out whatever those responsible for running the project want to be made public. On the other hand, the bloggers are scientists. I doubt they are being paid much if anything to do this, so they have freedom to write about what they want to, although also presumably will feel some obligation not to embarrass the sponsors of the web-site.

In any case, it you want relentlessly upbeat blogging about how wonderful everything is in this area of science, there are some very prominent blogs you can go to for this, and they aren't even sponsored by the DOE or NSF.

8. **anonymous**
September 14, 2007

I don't know of any rumours about a delay in starting LHC. What I do know is that CERN is presently in the process of choosing a new director general who will take over in Jan 2009.

9. **peter**
September 14, 2007

we've known for a long time now that the LHC will come on line on April 21,
2009:

http://www.sfwriter.com/scff.htm

"

Day One: Tuesday, April 21, 2009

A slice through spacetime . . .

The control building for CERN's Large Hadron Collider was new: it had been authorized in A.D. 2004 and completed in 2006. The building enclosed a central courtyard, inevitably named "the nucleus." Every office had a window either facing in toward the nucleus or out toward the rest of CERN's sprawling campus. The quadrangle surrounding the nucleus was two stories tall, but the main elevators had four stops: the two above-ground levels; the basement, which housed boiler rooms and storage; and the minus-one-hundred-meter level, which exited onto a staging area for the monorail used to travel along the twenty-seven-kilometer circumference of the collider tunnel. The tunnel itself ran under farmers' fields, the outskirts of the Geneva airport, and the foothills of the Jura mountains. …

10. **Yatima**
September 14, 2007

> choosing a new director general

But that is not correlated with the rumors about delay, right? Delay may be a good thing. Hearing about the compressed schedule to go live next year with no preliminary testing made me cringe. These tricks never work out, except – maybe maybe – if you know exactly what you are doing (i.e. you are doing it for the fifth time or so).

Now… will politicians head for the door upon hearing this or is LHC funding still assured? On the other hand, I probably don't understand the funding thing. Paul writes: "Construction of a new $150 million injector will start in 2012 when CERN finishes paying off the loans it took out to build the LHC".

They took out loans? With what collateral?

11. **Coin**
September 14, 2007

Dorigo, I do read your blog and it's a great example of what I would say we can hope for from blogging experimentalists, off the top of my head it would be that or the kind of thing that John Conway (but not *that* John Conway, apparently?) occasionally posts at Cosmic Variance. If the USLHC blogs manage to occasionally give us that level of insight into the project I'll be ecstatic. Of course, I'm not picky, I guess *anything's* better than trying to get one's science news from the popular press?

> [CERN] took out loans? With what collateral?

Perhaps if the loans are not paid, the banks will repossess the rights to the Z Boson. I mean consider how many Z Bosons there are in active use, the licensing revenue from those things must be enormous.

12. **JoAnne**
September 15, 2007

By international treaty, CERN recieves a fixed stipend each year from each member country. These stipends form their budget for current operations as well as R&D and construction of future facilities. In order to build the LHC in a timely manner (rather than having the constuction stretched out for more years), CERN took out loans against their future stipends. Each year after the LHC construction is finished, CERN will repay part of the loans with the fraction of these fixed stipends that is not used for current operations. Given the fixed amount of these stipends, it is known (and set by agreement) that the full amount of the loans will be repaid in 2011. Thus in 2012, there will be money available from the fixed stipends for new projects. This is a fantastic and stable funding model. Sure wish we could fund science in the US this way!

I haven't heard any rumors about further delays yet, but would not be surprised. In fact, I think we should anticipate a certain amount of small issues to arise as they commence operations. It is much better than they turn on the machine carefully and correctly!

13. **Less of a dreaming optimist than JoAnne**
September 15, 2007

"It is much better than they turn on the machine carefully and correctly!"

–> I hope the previous commenter does not imply that there are other ways to start running a 14 TeV proton accelerator, whose beams carry the energy of an aircraft.

Unfortunately, it is not because of care and attention to detail that the thing will probably be delayed. Rather, there are things that need servicing along the tunnel – many of them – and even figuring out where they are isn't easy! Imagine being in the situation when it does not seem that silly the plan of running tennis balls through the beam pipe to figure out where a few hundred among 4000 pieces blocking the path are, and having to dream of teams of grad students cycling around the tunnel to find out. It's just that bad.

No, I would not give the picture those soft rosy tones JoAnne likes to paint. The further delay, if confirmed, is a disaster to a large number of people who were counting on real data first at the end of 2007, then in 2008, then "a little in 2008 – 1 or 10/pb, and then 2-3/fb in 2009 – but we'll publish W and Z cross sections end 2008!", and now apparently not more than minimum bias until late 2009. For a theorist and assistant professor, sure, no hurry. But for a grad student who spent the last few years building things in the hope to finish his PhD in glory, it is a hammer blow. And it is not much less so for a fresh post-doc who has been hired to do data analysis and will instead spend three years doing Monte Carlo studies, not publishing anything meaningful.

Let's just hope that the rumors coming from CERN have been amplified through the echo of the caverns.

14. **Ellipsis**
September 15, 2007

Here is what I hear (gosh, why is there no better public source for these things than a blog by a mathematical physicist which is supposed to be about the problems with string theory?):

There are two problems which have cropped up in the past month:

1) Vacuum leak in arc 8-1

2) Problems with the "plug-in modules," which are the interconnects between dipoles. About 1% of them (in sector 7-8) failed and were damaged during the cooldown recently.

It is not clear how much time fixing these will add to the schedule, although apparently a year would be extremely pessimistic. Unless other problems crop up (not unlikely), the best guess (and it is a guess at this point — that is why you're not hearing any official statements) for the amount of time this will add to the schedule is approximately 2 months or so. That's a guess. Apparently the CDF spokespeople have announced they thought the delay would be longer, but remember that CDF is looking for an extension on Tevatron running time (and its spokepeople are not the most knowledgeable about this situation). Relax and wait until the people who know, know better.

As for the US LHC blogs, personally I certainly wouldn't worry about any active censorship or anything like that. I might be a little more concerned about them just becoming boring "corpora-blogs" because of their restricted mission and focus. Wait and see.

15. **JoAnne**
September 16, 2007

Dear Less of a dreaming optimist than JoAnne:

I have been waiting for data at the TeV scale since the summer of 1983. That was my first summer of doing research and was when it was announced that the US was going forward with the SSC. Fast forward, past all the super-collider calculations I did, to the Fall of 1993 when the SSC was cancelled and my paycheck was being paid by an SSC Fellowship.

You have no idea what it means to wait. I have most likely been waiting longer for data at the TeV scale than you have lived. Believe me, you and your career will survive the waiting. Life (and physics) as we know it will not end.

Yes, the LHC is already delayed and likely will be delayed more. But, after waiting 25 years, the very large numbers of people that have worked so hard and have waited so long already will find that it is OK to wait another year or so. And yes, the waiting will be because the people in charge of starting operations are careful. They will only test one segment at a time, carefully, so that a catastrophic event does not occur. And a safe startup will happen, precisely because of attention and care to detail.

16. **Less of a dreaming optimist than JoAnne**
September 16, 2007

JoAnne,

sorry, but I am older than you figured, I have a job, and it does not depend too much on LHC turning on or being canceled. I do, however, feel concerned for the tens, maybe even O(hundred), people that do not have the luxury option of waiting, and have to drop out of the field because they have to go on with their lives rather than living through 10-year PhDs. Seeing physics at the TeV scale is sure nice, but I do not shed a tear if our children will do it rather than us. There are sociological implications that to me are more important.

17. **Ellipsis**
September 16, 2007

I don't think anything related to these current issues has the remotest chance of turning a 5-year PhD into a 10-year one. Agreed that it is clearly somewhat of a concern when startup has been "a year from today" for a year now, but there haven't been any true disasters … yet. This is life, and funding agencies, employers, and just about everyone working on the projects have anticipated this level of issues. I think you should save the major complaints for when the beampipe gets a vacuum leak inside the CMS silicon, or when joule heating breaks the rest of the PIMs, or something like that. There are plenty of things that could go majorly wrong, and might — you're early (if ever). And that will be life too if it happens. How else do you suggest science progress?

18. **Ellipsis**
September 16, 2007

Furthermore, even if a disaster does occur, in that case it is likely that the Tevatron would be kept running until things improve. Most people working on the LHC also work on another project, partly because of the possibility of these kind of issues. It is fundamentally different than the SSC. I think very few will have to leave the field, even if a disaster occurs. Students can be shifted to a different project if necessary. People are more careful this time around.

19. **observer**
September 16, 2007

Pessimistic,

whining about a (possibly inexistent) one-year delay is ridiculous. Such delays are to be expected, with a certain probability, so anybody working on this should know what their chances are and take appropriate precautions. If someone is betting their phd on a certain date and time for the start of operations, they're making the wrong bet.

To extend JoAnne's description of the consequences of the SSC cancellation, I know many people who went into finance and computing in those days, who didn't even work in SSC physics. At least one who didn't even work in particle physics proper, but in astroparticle physics. There was a big crunch in the job market and everybody paid a price.

20. **Less of a dreaming optimist than JoAnne**
September 16, 2007

I confirm what I said above: it is not this -not yet confirmed- delay by itself what makes things bad. It is the addition of a small series of them.

Imagine you are a grad student who started in 2003 with the idea of finishing in 2008 with a thesis including enough data to see low-mass SUSY. As the delays pile up, you find your planning fall to pieces, and you are now looking at a thesis plan that will only be concluded in 2010-2011 -if everything else goes well-, when you originally thought you would be a post-doc, with a decent salary, and maybe a life. You are forced to consider bailing out.

And there are countries where PhD cannot be extended: they last three or four years, period. You started two years ago, and now you know your work will not make a dent, because you will only include Monte Carlo studies in it.

Further, there are problems with opening new positions for data analysis at LHC: in some countries the delays may end up causing a hole in the time profile of openings.

I am not a catastrophist: I first of all hope things will not turn out to be as bad as I heard claimed. Second, I think solutions can be found – especially to funding issues, which as far as CERN is concerned JoAnne has well explained aren't real troublesome. But I insist that the concern is sociological first of all. JoAnne has waited for 20 years, with a salary ? She can wait three more, and we with her. Younger people might be in more trouble.

21. **Peter Woit**
September 16, 2007

Ellipsis,

Many thanks for the helpful informed comment about the LHC status.

I agree that the problem with blogs sponsored by the labs or funding agencies is not likely to be censorship, but that the writers may try and avoid anything controversial to avoid embarrassing their sponsors. And, due to their official connection, rumor-mongering is problematic since what the bloggers write will be seen as having some semi-official status.

On the other hand, with no such connections myself, I'm more than happy to engage in (responsible) rumor-mongering, and encourage informed sources who want to let the rest of the world know what is going on to take advantage of this. Either with their names attached, or not, depending on which is more appropriate…

And this applies not just to depressing news about delays, etc. If you have information about a new Higgs signal or some-such, that would be even more welcome.

22. **Ellipsis**
September 16, 2007

Hi Peter,

Thanks. Note that all LHC hardware status info is in principle completely public — you can get all the dirty details at http://hcc.web.cern.ch/hcc (see especially the nonconformities section). Understanding what is really a big problem and what is more easily fixed requires some detailed knowledge, the vast majority of which I need explained to me as well (and was, thanks to a chain of people). You can bet that I will *not* be releasing any rumors about the Higgs or any other premature info that is not supposed to be public before release (for good reason). Physics results are fundamentally different than hardware status. When physics results are released they are meant to be definitive statements about nature and the universe. Leaking physics results which may or may not be correct confuses the ultimate goal of teaching the public the workings of the universe. I think that's something to be avoided. I definitely think Steinberg's statement (which I saw too) is overly restrictive if he means that they won't even talk about the most recent hardware status, which is public info. Hopefully his/their self-imposed restrictions will not reduce them to just posting what they had for lunch.

23. **Peter Woit**
September 16, 2007

Ellipsis,

I had poked around the site you mentioned, surprised to see how much detailed information is available about how the project is going. But I'd also quickly realized that you need an expert to tell you which problems are serious and which aren't.

I understand the distinction between the LHC status info and physics results, which are a different question. But even there, I personally don't see a problem with discussion of rumors of in-progress experimental analyses, as long as this is a discussion aimed at physicists and makes clear that this is work in progress and no claims are yet being made. The tricky part of this of course is the public nature of blogs, which, even when they are aimed at other scientists, are monitored by science journalists who are likely to try and report preliminary results to a wide audience, with the caveats getting sometimes lost in the process.

Theorists don't seem to suffer from this problem, with many of them seemingly happy to have highly misleading versions of their work reported, perhaps since the general misfortune of the theorist is to almost never have anyone pay attention to what they are doing. I agree that it wouldn't be a good thing to have the typical level of New Scientist reporting on theoretical work spread to reporting of experimental results.

24. **Peter Steinberg**
September 17, 2007

Ellipsis – I will never report on my lunch on the US-LHC site (you can check my non-LHC blog for that here)! Of course, I will pass along any substantiated information I come across, but I remain reluctant to pass along non-trivial interpretations of information from anonymous bloggers who heard things from insiders, which was the issue in my Conundrum post.

25. **woit**
September 17, 2007

Peter,

Just to insist on precision of terminology: if a blogger is someone with a blog, there aren't any anonymous bloggers involved here. There are anonymous blog commenters, one of whom (Ellipsis, whose identity is not known to me) gives good evidence of knowing what he or she is talking about.

Other than that correction though, I don't think there's anything wrong with your decision not to discuss these rumors on your blog, since it is part of the official LHC outreach effort, and information confirmed by those responsible at the LHC is not yet available. As for the rest of us though, I see no reason not to blog about information coming in either from reliable sources I know the identity of, or credible information from anonymous sources.

Steinhardt on the String Theory Crash

September 18, 2007
Categories: Uncategorized

The Edge web-site has something new up they call [Einstein: An Edge Symposium] (thanks to commenter Hendrik for pointing this out). It's an exchange between Walter Isaacson, Paul Steinhardt and Brian Greene, nominally about Einstein, but ending up turning into a discussion of whether and how string theory has "crashed".

Steinhardt forcefully makes the same point I've made *ad nauseam* here: the anthropic string theory landscape is not a valid scientific research program, but simply the kind of thing you end up with when a speculative idea fails.

> In my view, and in the eyes of many others, fundamental theory has crashed at the moment. Instead of delivering what it was supposed to deliver—a simple explanation of why the masses of particles and their interactions are what they are—we get instead the idea that string theory allows googols of possibilities and there is no particular reason for the properties we actually observe. They have been selected by chance. In fact, most of the universe has different properties. So, the question is, is that a satisfactory explanation of the laws of physics? In my own view, if I had walked in the door with a theory not called string theory and said that it is consistent with the observed laws of nature, but, by the way, it also gives a googol other possibilities, I doubt that I would have been able to say another sentence. I wouldn't have been taken seriously…
>
> But what angers people is even the idea that you might accept that possibility—that the ultimate theory has this googol of possibilities for the laws of physics? That should not be accepted. That should be regarded as an out and out failure requiring some saving idea…
>
> What I can't accept is the current view which simply accepts the multiplicity. Not only is it a crash, but it's a particularly nefarious kind of crash, because if you accept the idea of having a theory which allows an infinite number of possibilities (of which our observable universe is one), then there's really no way within science of disproving this idea. Whether a new observation or experiment comes out one way or the other, you can always claim afterwards that we happen to live in a sector of the universe where that is so. In fact, this reasoning has already been applied recently as theorists tried to explain the unexpected discovery of dark energy. The problem is that you can never disprove such a theory … nor can you prove it.

Steinhardt dismisses attempts to hypothesize that maybe the landscape is somehow predictive as follows:

> Do you mean as derived from string theory? I don't believe that's true. I don't believe it's possible…
>
> Well, I believe that if you came to me with such a theory I could probably turn around within 24 hours and come up with an alternative theory in which property X wasn't universal after all. In fact, you almost know that's true from the conversation that's been happening in the field already, where someone says, these properties are universal and these others are not. The next day, another theorist will write a paper saying, no, different properties are universal. There are simply no strong guidelines for deciding…
>
> If a version of string theory with a googol-fold multiplicity of physical laws were to be disproved one day, I don't think proponents would give up on string theory. I suspect a clever theorist would come up with a variation that would evade the conflict. In fact, this has already been our experience with multiverse theories to date. In practice, there are never enough experiments or observations, or enough mathematical constraints to rule out a multiverse of possibilities. By the same token, this means that there are no firm predictions that can definitively decide whether this multiplicity beyond our horizon is true or not.

After some prodding, Steinhardt makes clear that he is not claiming that string theory as whole has crashed, that it is just the landscape that is the crash. While insisting that people need to acknowledge that the landscape is simply a scientific failure, he holds out hope that some fix to string theory may still be found:

> …it's that point of view which is a crash, and needs a fix. I am not arguing that string theory should be abandoned. I think it holds too much promise. I am arguing that it is in trouble and needs new ideas to save it.

There's also some discussion about what Einstein would have thought of string theory and the landscape, with Steinhardt of the opinion that Einstein would have liked string theory with its unification via geometry of extra dimensions, but that he would have rejected the landscape:

> Einstein took gravity and turned it into wiggling jello-like space, and now string theory turns everything in the universe, all forces, all constituents into geometrical, vibrating, wiggling entities. String theory also uses the idea of higher dimensions, which is also something that Einstein found appealing.
>
> What I was commenting on earlier was where the string program has gone recently, which I described as a crash. I can't say for sure how Einstein would view it, but I strongly suspect he would reject the idea.

[Three years ago] I expressed the opinion that the promotion of the anthropic landscape would make Einstein gag, which so upset Joe Polchinski that he used this to argue that trackbacks to my blog should not be allowed on the arXiv (even though this was not about an arXiv paper, but a Scientific American article). At one point I regretted having used that expression, feeling it was somewhat over the top and inappropriate. In retrospect, seeing what has happened over the past three years, I've changed my mind. The kind of thing that would make Einstein gag has moved from popular science articles to regular appearance in the lectures and scientific articles of leading figures in particle physics. This would probably not just make him gag, but send him into a serious fit of depression.

Comments

1. **Tom**
   September 19, 2007

   As an uninformed member of the peanut gallery, may I propose a new ground rule for this debate? Namely, that the dead be allowed to rest in peace and not forced to weigh in with their (imagined) opinions on the matter.

2. **chimpanzee**
   September 19, 2007

   "Every absurdity has a champion to defend it."
   — [anti-religion website]

   I had a conversation with [Wolfgang Haken] a few yrs ago

   [UIUC mathematician (whose father was Max Planck's PhD student) who proved the 4-color theorem with K. Appel, using computer] who was working on cosmology in his retirement. He was very vocal about the "infinity of solutions" curse, & thought the scenario was hopeless! This, from someone who solved a famous combinatorial math problem.

He mentioned something very interesting:

"If you have not made a mistake, you have not been a great scientist!"

He pointed out "Even David Hilbert was wrong!". The moral of the story is "You have to try a lot of ideas" (shotgun approach, as per L. Pauling). This mirrors D. Gross statement "You have to try" (the W. Churchill quote).

I think what P. Woit & L. Smolin are saying is that ST (over 20+ yrs) isn't getting it done ("crash"), & it's time to admit failure. Move on.

"An honest man can make a mistake, but only fool persists in doing so"

"No matter how far you have gone down the wrong path, turn back."
— Old Turkish proverb

3. **amused**
September 19, 2007

An interesting discussion, and this part seems potentially explosive!:

GREENE: […] But just so I understand; you're saying that this one particular way in which one may think about string theory—for which the endpoint is many many universes—is unacceptable.

STEINHARDT: Right. I claim it needs to be fixed.

GREENE: But you also agree—just so it's clear—that's not a crash in string theory per se; that's a particular way of approaching the theory that you would not advocate because the endpoint would be unacceptable. You need to go further…

STEINHARDT: That's right; so it's just what you were saying; some people say that is the endpoint, and I'm saying that's not acceptable. If you believe that, it's time to abandon it.

GREENE: But it's those people who've crashed.

Wow, take that Susskind & Co!

4. **tytung**
September 19, 2007

Regarding the points raised by Steinhardt…I think it need to be made more clearly. An ultimate theory may allow certain physical facts as something that is selected by chance, and other universe, if allowed to exist, may take other possibilities. The candidates are the values of physical constants. I can't even imagine that these values can be determined in some ultimate theory, free of any constants.

5. **mclaren**
September 19, 2007

Since the Uncertainty Principle made Einstein gag, this criterion should not be used as a measure of the measure of a physical theory.

6. **chris**
September 19, 2007

tytung,

i think this is not the point. of course a theory with no constants to fix would be the ideal one. but take e.g. QCD. how many free parameters does it have? one plus the quark masses? how about the 3 in the SU(3) gauge group? why not 4? is that a free parameter?

i think the trouble with the landscape is quite different. imagine, that QCD would not directly lead to the hadron spectrum or confinement (let's assume for now, that they are sufficiently well established). let's assume that we only have an effective theory of hadrons and we conjecture a fundamental theory behind it that first needs to break the ground state symmetry down to a subgroup in a very specific way. and let's imagine that there are zillions of other plausible patterns that we are not able to exclude because of either internal consistency or experiment.

now, even if the fundamental theory would have only one free parameter (or zero, if you want), the unsatisfactory feature is, that it leaves you with *no prediction*. only if you pile additional external assumptions upon the theory (how the symmetry is broken) can you arrive at a descriptive theory.

the point with the landscape is exactly this. your fundamental theory may be nice (assuming it can ever be formulated correctly), but unless you pile 'anthropic reasoning' on top of it (=invoke god at this point) it does not predict anything. it just does not.

it totally floors me to see how string theory can claim to have improved the state of affairs in quantum gravity. the original problem was lack of predictiveness. at this level, quantum gravity (the QFT with the einstein-hilbert action) is just doing fine! well, it is nonrenormalizable perturbatively (on first inspection), but what the heck. if you fix an infinite number of parameters, it is just fine. and even you can in principle fix them all with observations.
even more so, there is strong evidence piling up that the QFT of gravity is nonperturbatively renormalizable. so it seems, that the original problem of nonpredictability will eventually turn out to be an intermediate technical difficulty.

now compare that to the currently claimed nonpredictability of the antropic string landscape. the only reason i can imagine that this is not dead as a valid candidate for a physical theory is that too many people have spent too many years hunting the phantoms and won't admit they went the wrong way. it is just absurd.

7. **milkshake**
September 19, 2007

Chance selection in the Ultimate theory: How many chance-selected parameters would you be willing to accept?

"A multiverse encompassing every alternative compatible with our theory is possible according to the theory – so it could have been a combinatorial process that picked the winning numbers in our corner of the multiverse"

This is just productive as saying: “Our creator is a mysterious dude who laid out the universe according to his subtle wisdom”.

In both cases you can fit all the known facts in, with the help of scriptures and some scholastic reasoning.

8. **Thomas Larsson**
   September 19, 2007

   Mr Academic Failure Motl now decrees that the [Albert Einstein Professor in Science at Princeton University]() is an incredibly stupid crackpot. It is really funny when AFM says that one of the leading theorists responsible for inflationary theory say “many breathtakingly dumb and bitter things about inflation” 🙂

9. **Thomas Love**
   September 19, 2007

   Peter said: “The kind of thing that would make Einstein gag has moved from popular science articles to regular appearance in the lectures and scientific articles of leading figures in particle physics. This would probably not just make him gag, but send him into a serious fit of depression. ”

   From what I’ve read about Einstein, he would neither gag nor become depressed, he’d be laughing his head off. As am I.

10. **King Ray**
    September 19, 2007

    To quote Einstein from his book The Meaning of Relativity:

    “More complex field theories have frequently been proposed. They may be classified according to the following characteristic features:

    (aa) Increase of the number of dimensions of the continuum. In this case one must explain why the continuum is apparently restricted to four dimensions.

    (bb) Introduction of fields of a different kind (e.g. a vector field) in addition to the displacement field and its correlated tensor field g_ik (or scriptg^ik).

    (cc) Introduction of field equations of higher order (of differentiation).

    In my view, such more complicated systems and their combinations should be considered only if there exist physical-empirical reasons to do so.”

    From this one might deduce that Einstein would not favor additional dimensions unless there was empirical evidence for them, which there is not. Also there does not seem to be a good explanation for why the continuum appears to be 4 dimensional insteand of some other number.

    Once you give yourself too much mathematical freedom, there are too many possible theories and you end up with a mess like the landscape.

    (I had to double the letters because it was turning the c in parentheses into a copyright symbol)

11. **King Ray**
    September 19, 2007

    I should also add that almost all of Einstein’s 30+ year search for a Unified Field Theory was confined to four dimensions. He rejected the Kaluza-Klein idea using higher dimensions.

12. **Thomas Love**
    September 19, 2007

    But the use of higher dimensions goes back even further.
    Robert S. Cohen wrote a fascinating introduction to the 1956 Dover reprint of Heinrich Hertz’ The Principles of Mechanics. :
    In the writings of Descartes, there is sketched just such an
    efficiently running world-machine, devised without forces or energies, built of rigidly connected space-time atomic entities. Although he was an advocate of the mathematization
    of physical reality, Descartes was driven to admit the inadequacy of ordinary space-time geometry to explain inertia and gravitation. These two properties of bodies, perhaps to be supplemented by other non-geometric properties in later investigations, seemed to require more than the pure mathematics of Euclidean geometry, but Descartes asserted that they should be analyzed by a higher geometry of many dimensions. “By dimension,
    I understand nothing else than the mode and aspect in respect
    of which a subject is considered to be measurable. Thus, it is not only length, breadth and depth which are dimensions; gravity is also a dimension, speed is a dimension of motion…
    and so on with innumerable other dimensions of this sort”
    (Rules for the Guidance of our Mental Powers, Rule xiv, tr. N. K. Smith).

    Thus, according to Descartes, we need a dimension for each physical quantiy we can measure!

    There iare certainly “physical-empirical reasons to do so”

13. **sophia**
    September 19, 2007

    Peter,

    Forgive me for even contributing here. I’m not a scientist, just an interested observer. But I do have a question that may be relevant.

    I followed the link and began to read. After a while I began laughing and then wincing and then, yes, gagging. Why? Well, stand back and survey the scenery: three intelligent men, two of them credentialed scientists, wondering what a dead man would think, because they have reached (sorry) a dead end. That’s not so pathetic it’s funny? To me it is.

    Don’t get me wrong: I revere Einstein as much as the next good American does, but dudes, he’s dead. Wondering what he would think is ludicrous. What is this, science or a seance?

    I gave up here:

    “I think most of us in the field absolutely will never have faith that this approach is right until we do make contact with data, but it would be great to have the insight of the master as to whether he feels that this smells right."

What does it matter what Einstein would think? What matters is what Brian Greene and Paul Steinhardt think!! What are THEIR insights?? And what is this business about "contact with the data", as if "the data" are alien beings that we conjecture about but have never met??

OK, that's three questions.

14. **JPL**
September 19, 2007

Hi Sophia,

Sorry to barge in but Einstein is quite alive, at least, a lot more so than the String Theory landscape! I am sure that this sounds silly to you, 3 grown (but living) men speculating about what a dead one would think, but in Physics this is hardly what it seems. Einstein is one of the few figures whose own sense of proportion and aesthetics has impacted the discipline enough that one can justifiably refer to it some 60 years after he passed and thus the argument makes sense and can be consistently addressed and narrowed as you see above.
I would dare add that what Einstein would think matters much more than what Steinhardt, Greene or Isaacson think, otherwise they would not think to add that bit to their own opinions, now would they?

15. **Peter Woit**
September 19, 2007

Sophia,

I agree that the "What would Einstein think?" question is kind of silly. He is rightly revered for his contributions to physics, but the greatest of these were from a very different time a century ago, when physics was facing very different challenges. This kind of question functions like "Would Jesus drive an SUV?" questions, as a rhetorical device for people to invoke what they see as high principles by embodying them in a prestigious figure.

Of course, I'm personally convinced that Einstein would strongly dislike string theory, and I'll construct a long, detailed argument about why this is true. Well, maybe not today…..

16. **Glenn**
September 19, 2007

In my more depressed moments, when pondering this topic, I wonder, "What if they're right?"

That is, what if the fundamental nature of the universe really is the unprovable 'landscape' or some other critter we can't disprove?

If that's true, we're doomed to an endless search for a 'truth' that lies outside the area we're willing and able to search, like the drunk in the old joke who lost his keys in a dark alley, but is looking for them next to the street because the light's better over here.

I can only hope that "Science" isn't about to hit then end of a centuries-long winning streak…

17. **King Ray**
September 19, 2007

Cheer up, we are just in a long period between paradigms, if you believe Kuhn's The Structure of Scientific Revolutions.

They say that a camel is a horse designed by a committee. I think string theory is physics designed by a committee. The efforts of a large group can never match the aesthetics of a single extraordinary genius like Einstein.

It will take a new idea, probably by a single person, to make the breakthrough needed to have a successful theory of quantum gravity and a unified field theory.

18. **Changcho**
September 19, 2007

Paul Steinhardt said "…if I had walked in the door with a theory not called string theory and said that it is consistent with the observed laws of nature, but, by the way, it also gives a googol other possibilities, I doubt that I would have been able to say another sentence. I wouldn't have been taken seriously…"

Steinhardt is, of course, right. A 'theory' that gives you "a googol other possibilities" isn't scientific, simply because a good science (at least good Physics) puts limits on what is and isn't possible.

19. **anonymous**
September 19, 2007

"This would probably not just make him gag, but send him into a serious fit of depression."

But that's not saying much; it was also true of Einstein and QM.

20. **Eugene Stefanovich**
September 19, 2007

anonymous said:

"But that's not saying much; it was also true of Einstein and QM."

As far as I know, Einstein never questioned the correctness and usefulness of quantum mechanics. He just thought that it is not the ultimate description of nature and that there should be a more fundamental underlying theory. That's why he was reluctant to work on QM.

String landscape is different: it does not explain or predict anything. So, it is questionable whether it belongs to science.

Eugene.

21. **Belizean**
September 19, 2007

And we should not forget that virtually every competent theoretical physicist alive today understands general relativity better than Einstein ever did.

22. **King Ray**
September 19, 2007

Belizean said:

"And we should not forget that virtually every competent theoretical physicist alive today understands general relativity better than Einstein ever did."

That's an arrogant statement. I know of at least one very prominent string theorist who couldn't even write down the formula for the Christoffel symbols in terms of the metric tensor when he was teaching a class on GR.

I don't think anyone today understands GR as well as Einstein did; he invented it, after all. It takes a lot more intelligence and insight to make a discovery than to understand it after the fact. Standing on the shoulders of giants doesn't make you a giant.

23. **Thomas Love**
September 19, 2007

Thanks for the link to the Edge Symposium. For me, the most interesting statement was :

ISAACSON: There's a wonderful book that Einstein wrote called The Evolution of Physics with Leopold Infeld in 1938, which is not easy to find. I've gone over it again two or three times because I just love the way it was written. It was written to make money for both of them, because it's the 30s, and Hitler, and refugees and stuff. It's a popular book, but it has a deep philosophical argument, and the publisher is reissuing the book because I was pushing them to get it out there.

The deep philosophical argument is that it will be a field theory approach that will work. It starts with Galileo; he talks about matter and particles, and just makes the argument that in the end it is all going to be reconciled through field theory. It's about whether there is going to be a great distinction between a field theory and a theory of matter.

24. **Anders R**
September 19, 2007

i wonder if einstein would have gone into theoretical physics at all if he was a 16 year old student who was about to make a choice for college.

25. **Anders R**
September 19, 2007

in the year 2007 that is

26. **Mr. Mustela**
September 19, 2007

The fundamental issue seems to be that no progress has been made in the last 20-30 years on those fundamental questions. The anthropic principle succeeds as a theory by explaining **that failure**, if not much else. As such, it is probably a step in the right direction.

The Woit-Smolin alternative rejects that theory, and instead blames part of the academic system for the failure. Personally, I believe that if there is no promising idea today (as Peter said a few posts ago) then it is unlikely to be the failure of the system. The system is bad, but not that bad. A good idea wouldn't be ignored for that long. Stolen? yes, no problem. But not ignored.

To me, a better approach would be to think of **better** explanations as to why we are making so little progress. If possible, ones with a backdoor to be able to validate them, of course.

In this light, it seems to me that the true value of the anthropic principle has been overlooked. Sure, it is awful from the point of view of getting an explanation of concrete phenomena. But as an attempt to explain the limits of our current methods, it works better than not.

27. **a quantum diaries survivor**
September 19, 2007

Sophia,

you should read "Conversations with Einstein's brain" in "The mind's I" by Daniel Dennett… Appropriate for the current conversation!

Cheers,
T.

28. **Coin**
September 19, 2007

*The anthropic principle succeeds as a theory by explaining **that failure**, if not much else. As such, it is probably a step in the right direction.*

Seems to me like more of an excuse than an explanation, frankly…

But as an attempt to explain the limits of our current methods, it works better than not.

Well, for what reason must we conclude that our current methods have limits at all?

29. **tytung**
September 20, 2007

Chris,

I was trying to say that one should distinguish those features of our world are to be explained in an ultimate theory from those that should not (because they can not). Regarding predictability, I think if it postulates some entity like string, then in principle the theory allows these to have observable effects, albeit one that is technically unavailable.

30. **chris**
September 20, 2007

"The fundamental issue seems to be that no progress has been made in the last 20-30 years on those fundamental questions"

this is definitely false. see e.g. http://relativity.livingreviews.org/Articles/lrr-2006-5/
or also see how far LQG has come. i think we are on the verge of proving the renormalizability of quantized GR. and it might not take a genius but hundred people working out the tedious details in a decade long struggle. better to abandon romantic views of heureka moments that come out of nowhere.

31. **Thomas Larsson**
September 20, 2007

One important thing that Steinhardt points out is that the Landscape is really at odds with observation. If the laws of physics fluctuate throughout the multiverse, there might be some fluctuations already in our observable universe. However, no such fluctuations are observed…

Of course, this is not a hard argument. The fluctuations may take place on length-scales that are much larger than the size of the universe. Nonetheless, this fits well into the standard pattern: whenever some experimental signature is suggested by string theory (susy, extradimensions, laws-of-physics fluctuations, …), you can be sure that it is not observed. Maybe the Lord is trying to tell us something by that.

OTOH, if string theory does not predict a Landscape, it is difficult to see how it can accomodate a nonzero cc 30 orders of magnitude below the Planck scale. So in that case string theory is probably ruled out by observation anyway.

32. **dan**
September 20, 2007

Dear Peter Woit,

How do you feel about Briane Green's contribution, for example,

"…..When you look at the framework within which the standard model of particle physics sits, namely relativistic quantum field theory, you do find that there are a google, if not more, possible universes that that framework is capable of describing. The masses of the particles can be changed arbitrarily and the theory still makes sense, it's internally consistent; you can change the strengths of the forces, the strengths of the coupling constants….."

"…..So how is that any worse than string theory?…."

http://www.edge.org/3rd_culture/einstein07/einstein07_index.html

33. **sophia**
September 20, 2007

Peter,

Sometimes I feel that no matter what I say on a comment, it's misleading.

I wasn't 100% clear, so let me rephrase.

I have no objections to anyone, in any discipline, getting iinspiration in a general way from the great practitioners of their field. So, in that limited sense, "what would Einstein think?" is no big deal. If that helps grease your mental wheels, go for it.

But I do not think that's what's going on here. What is going here is bullshit, desperation and non-thought. It's actively anti-science- not even non-science.

The invocation of the numinous name of Einstein is the dead giveaway. I've noticed that with physicists there's a tremendous amount of prophet-wannabeism, and there are two favorites whose names always seem to pop up when all else fails: Einstein and Feynman. Somehow, Dirac doesn't do the trick….

I don't want to get off a nitpicky thing about Jesus, but since you mentioned him, I'll respond to it, because it underscores further my problems with "what does the master think"-ism.

Invoking Einstein's name when you are out of ideas is much worse than asking whether Jesus would drive an SUV or vote Democratic, because no one knows exactly what Jesus said, there are canonical and non-canonical sources, and so on.

But everyone knows what Einstein said. He left a recorded, written body of work that is unambiguous and free for all who care to, to access.

And that's the reason the business about making "contact with data" and having "the insight of the master as to whether he feels that this smells right" drove me so crazy.

If one of your students said that to you, what would you say to him or her?

If someone came to me and said that, I'd say, "Forget about channeling Einstein. Think your own thoughts. Come to your own conclusions. Make your own damn contact with the damn data. No contact? No data? Think some more."

Am I making any sense to you?

34. **Peter Woit**
September 20, 2007

sophia,

I pretty much agree with you. I don't think the "what would Einstein think?" question is an interesting or useful one.

In my experience, it's not actually a question physicists spend much time thinking about. It's the sort of thing that comes up when someone is looking for a hook that will get the public's interest. Basically Einstein gets dragged into any discussion of any kind about physics, no matter how inappropriate, because people figure that makes the discussion something the public can relate to. It's not a good thing, but relatively harmless compared to lots of other things…

35. **Arun**
September 20, 2007

Peter,

I think Einstein gets dragged into the whole thing because he is supposedly the exemplar of figuring out something about the world purely by power of thought. The implicit assumption in today's research programs is that it is possible to perform similar feats and bootstrap ourselves from Standard Model QFT + GR to a way more comprehensive theory. Whether a given line of inquiry is fruitful is now a matter of taste, and Einstein becomes the arbiter of taste.

36. **Peter Woit**
September 20, 2007

dan,

I really should write up a FAQ and include this issue of "QFT and string theory are on the same footing, lots of both.." Sure, by choosing arbitrary gauge groups, particle representations, etc, you can get an infinite number of QFTs. But the reason QFT makes predictions is that one of the simplest possible such choices works just about perfectly. If simple choices of QFT disagreed with experiment, and you had to go to extremely complicated sets of choices in order to avoid contradicting experiment, never actually getting to anything that you could test, then QFT would be in the same situation as string theory. It's not as simple as just noting the "number of theories".

37. **Anders R**
September 20, 2007

regarding what you've said sophia, i definately agree about the prophet wanabeeism, especially within theoretical physics. i think there seem to be large amounts of name dropping going on, especially among string theorists.

38. **Eric**
September 20, 2007

Simple choices of QFT do disagree with experiment. For example, wouldn't it be simpler for the weak interactions to be described by SU(2) rather than SU(2)xU(1) as originally proposed by Glashow? However, this is not how nature is working. And isn't this whole symmetry breaking business horribly complicated? Wouldn't nature prefrer unboken symmetry since it is simpler? Or shouldn't physics be described by SU(5) instead of SU(3) x SU(2) x U(1)? And why are there three generations when one would be much simpler, and for that matter why doesn't the Standard Model predict the masses of each generation? QFT only makes predictions because there is experimental data available at the low energies at which it is valid. The problem with string theory is that it is a theory which is valid at very high energies, at which experimental data is sorely lacking.

39. **Arun**
September 20, 2007

The problem with string theory is that it is a theory which is valid at very high energies, at which experimental data is sorely lacking.

What high energy experimental data are you looking for? Assume I can accelerate protons and anti-protons to Planck scale energies and collide them. What are the predictions?

40. **Peter Woit**
September 20, 2007

Eric,

Last I heard, string theory was supposed to be valid at all energies…

Very convenient to have your scientific research be on a theory which is not valid at any energy scale we'll ever be able to reach.

I don't claim that the Standard model is the simplest possible choice among gauge theories, I do claim that it is one of a relatively small number of the simplest ones, so one can compare to experiment by looking at a small number of possibilities (not 10^500), which is exactly what people did in discovering the standard model during the 1960s and early 70s.

41. **ScienceLover**
September 20, 2007

> [calling Einstein] is not a good thing, but relatively harmless compared to lots of other things…

That's why it is funny as if it were some sort of a sketch: 3 men are discussing theoretical physics. Then they get stuck. Silence. They look at each other and after a while one of them breaks the silence: "What would Einstein say?" The discussion recovers and although it was at a dead end one of them claims that Einstein would be certainly impressed by the great theory he and his colleagues worked out.

42. **Eric**
September 20, 2007

Peter,
If you read my comment very, very closely you'll see that I state that string theory is valid at high-energies as well as low energies where it should reduce down to conventional QFT. What string theory does is unify QFT with a perturbative formulation of quantum gravity. As such it is much more ambitious than QFT. However, the situation today is no different than if you had a formulation of QFT, but didn't know anything about the weak or strong interactions. There is no possible way to deduce the standard model from QFT alone.

43. **Peter Woit**
September 20, 2007

Eric,

No, you can't deduce the standard model from pure thought about QFT alone. You have to learn how to calculate things in tractable QFTs, compare to results of experiment, identify the right QFT, then make predictions and test them.

In string theory what you are doing is doing calculations in tractable string theories, comparing them to experimental results, seeing they don't agree, trying for 25 years to find some way around this and failing, then, instead of giving up and trying something else, telling everyone that the problem is just that it's "too early to tell", you need to do more calculations (10^500), and then maybe it will work. And trying to also tell people that the problem is those damn low energies, that surely at unmeasurably high energies your calculations would agree with experiment.

44. **Eric**
September 20, 2007

Peter,
You are presupposing that experimental data exist to guide you to identify the right QFT. The problem for the last 25 years has been a lack of any experimental data at energies a magnitude above the EW scale. Once this data is available, hopefully from LHC/ILC, this situation will change drastically. I sometimes wonder if this isn't why you seem so excited anytime there is a rumour of a delay in starting up LHC. Such a delay gives you that much more time before things come crashing down.

45. **Peter Woit**
September 20, 2007

Eric,

There's plenty of experimental data out there to guide people trying to do unification, the problem is string theory can't reproduce it.

Funny how you think the electroweak scale is the problem, that string theory has something to say just above the electroweak scale, but not below it. That's nonsense.

I in no way think a delay in the LHC is a good thing. Quite the opposite. Any delay just delays the date at which the standard nonsense about finding evidence of string backgrounds in LHC data comes crashing down.

46. **Eric**
September 20, 2007

Peter,
Yes, string theory can describe the current low-energy data below the weak scale and there is at least one string model which does so. The question of whether or not such models may uniquely predict the parameters of the standard model is presently unanswered. For this, the problem of moduli stabilization must be completely addressed and this is where a lot of the current work is focused. In this respect, string theory is currently no better than QFT.

47. **Anders R**
September 20, 2007

"You are presupposing that experimental data exist to guide you to identify the right QFT. The problem for the last 25 years has been a lack of any experimental data at energies a magnitude above the EW scale"

maybe the experimental data is there, but the theoretical phycisists who are working on it right now just aren't competent enough to work out what it is.

48. **Anders R**
September 20, 2007

sorry peter i didn't see that you wrote almost the same thing

49. **David B.**
September 20, 2007

Dear Peter and Anders:

Saying there is plenty of data out there to guide people trying to do unifcation is not accurate. The current situation is like trying to find New York City by the following algorithm:

it's not London, it's not Paris, It's not Geneva,…

Any model of high energy physics will impact the low energy physics by the addition of non-renormalizable couplings. This is at best an indirect measurement. Most of these are beyond current experimental bounds (meaning experiments are consistent withthe standard model), and this fact only indicates that either the unification scale is too high to see, or the correct theory has a huge conspiracy to cancel all possible signatures in the low energy physics.

As it stands, this does not give you any information on what the theory is, but it definitely gives you a lot of information on what it is not.

50. **Amos Elberg**
September 20, 2007

Einstein got "dragged" into the thing because the thing is a symposium about Einstein. An Einstein tribute. I think its entirely appropriate during such a symposium to ask other physicsts what Einistein would have thought. The ensuing discussion is pedagogically useful.

51. **Peter Woit**
September 20, 2007

Hi David,

My point about unification wasn't that there's a lot of information about beyond the standard model physics. There certainly isn't, and that's a huge problem. But the standard model itself is both extremely well tested, and has quite a few features that we don't have an explanation for. All I meant is that any idea about unification should explain one or more of these features in a convincing way. The fact that string theory doesn't do this, but instead has turned into a set of excuses about why it's impossible to explain such things, is for me the main reason to be skeptical about it.

52. **dark-matter**
September 20, 2007

We now have Prof Steinhardt, a scientist of considerable stature, clearly stating the string landscape of googles is unacceptable to science. The implication of this to string theory itself has been laid out in this thread. He knows he is speaking to the large community of string theorists. He should also know he is indirectly speaking to the funding agencies. The message is: if you believe it is unacceptable, then you should do the right thing. If you are a researcher, move on. If you are responsible for funding, also move on. He is on your side.

53. **?**
September 21, 2007

Prof. Steinhardt may have considerable stature with some people, but after his ekpyrotic and cyclic clowning, it isn't with serious particle or gravitational theorists. He is actually selling his own competing "theory" in his rants against string theory.

54. **Eugene Stefanovich**
September 21, 2007

? said:

He is actually selling his own competing "theory" in his rants against string theory.

Is it a bad thing? Isn't it the job of theorists to produce and sell theories?

Eugene.

55. **chris**
September 21, 2007

for those of you who claim that over the last 25 years there has essentially been no discoveries in experimental hep, let me briefly remind you about the following:
.) discovery of W and Z
.) discovery of the t quark and its mass
.) lower limit on SM higgs of 115 GeV (beyond treelevel susy!!)
.) flavor mixing in the b meson sector
.) neutrino masses and mixing (making a perturbatively renormalizable SM contradict experiment!!!)
.) cmb fluctuations
.) measurment of the deceleration parameter, baryonic and dark matter/dark energy densities
.) measurment of muonic g-2

that's a lot i would say.

56. **chris**
September 21, 2007

oh, and i forgot the spectacular LEP Z-pole data.

57. **Peter Orland**
September 21, 2007

Chris,

Most of these experimental results were anticipated. I think the only one on your list which does suggest some really new physics is neutrino mixing (whose details are not yet fully determined). Maybe muonic g-2 could also lead to something.

Exciting experimental discoveries are those which nobody or only a few people expect. I am not denying that it is necessary to look for anticipated results, but they don't make me jump up and down.

58. **chris**
September 21, 2007

dear peter,

i largely agree with you. but i want to point out, that the absence of new particles per se is not always totally unexciting. for example, would you really have anticipated, that all the LEP Z-pole data and b-meson CP violation would be compatible with the SM? in 1983, would you really have made a bet that the higgs was heavier than 115 GeV? that the top was even heavier? that there will be no baryon number violation detected at all?

of course in some way this is anticlimactic, but it is extremely surprising (at least to me) that the SM describes data so well.

59. **Arun**
September 21, 2007

On the other hand, the SM does not describe the majority of the mass/energy in the universe.

60. **Brett**
September 21, 2007

The detailed data is surely very interesting, and it does tell us quite a bit that we didn't know before. However, most of what it is telling us is that the theory we already had worked out is good to quite a bit higher energy scales than we might have expected. We learn a lot about physics at higher scales, but what we learn is that there isn't anything new there. Hence, the experimental data, while providing information, provides no positive guidance about what the new physics should be.

For what it's worth, the particular measurements that I would pick out as telling us useful negative information about low-scale new physics are: the stability of the proton (which is a really strong condition on GUTs), CP tests with B mesons (which find nothing other than CKM effects, strongly constraining superweak theories), the large masses of the top and Higgs (since they place strong constraints on supersymmetric unification), and neutrino oscillations. The last one may sound odd, since it is really evidence of new physics, but the new effects are limited to the neutrino sector. The smallness of the observed neutrino masses suggests (via the see-saw mechanism) that the scale at which new fundamental physics enters is large.

61. **dark-matter**
September 21, 2007

To: ?

Prof Steinhardt joins the group that says string theory biggest 'prediction', an infinite solutions of physical reality for our single universe, is not only clearly dead end, it is absurd. It is not science and implies those who continue to pursue it is folly. Of course, every expert has all the rights to choose to be folly. But not on taxpayer's money. Prof Steinhardt credibility, his scientific output, and his various professional engagements, are such that it is folly of me to engage seriously with your assertion.

62. **David**
September 21, 2007

Arun made an interesting comment about the SM not describing dark matter/energy. I can't see how we can have a TOE at least until we have more information about dark matter/energy. Have I missed something?

63. **Coin**
September 21, 2007

I can't see how we can have a TOE at least until we have more information about dark matter/energy. Have I missed something?

I'm really not the person to ask, but I think the idea is that hopefully a TOE today would *predict* what the information about dark matter/energy is eventually going to tell us, once we have the technology to collect that information– or, at least, the TOE might offer us some *possibilities* about what sort of thing dark matter/energy might be, which we would then be able to select among.

This might be a little too optimistic, but it's maybe worth a shot. Occasionally in the past theoretical physics has managed to jump ahead of theoretical physics in a similarly spectacular fashion, and anyway, we do have *some* indications about dark matter/energy's nature, if not its details.

64. **hyans**
September 22, 2007

> Three years ago I expressed the opinion that the promotion of the anthropic landscape would make Einstein gag, which so upset Joe Polchinski that he used this to argue that trackbacks to my blog should not be allowed on the arXiv (even though this was not about an arXiv paper, but a Scientific American article).

So this is, then, the solution to the trackback mystery? I don't know if it was reported before here or elsewhere, but it's probably news to many people besides myself. Interesting story. Probably interesting only at the anecdotal level, but interesting nevertheless.

65. **Peter Woit**
September 22, 2007

hyans,

Can't say that I really know what the "solution to the trackback mystery" is. I was referring to the blog comment of Polchinski here:

http://golem.ph.utexas.edu/~distler/blog/archives/000760.html

Privately I've heard that there is more to the trackback mystery than that Jacques Distler hates me because of my criticism of string theory. And if you take a look at the trackbacks attached to papers promoting the anthropic landscape, I don't recall noticing any critical ones being allowed. Somehow though, I doubt that this effort at censorship is particularly effective, since anyone who reads physics blogs has probably come across me explaining my views on this far too often.

Anyway, I've long ago given up on fighting this particular issue, on the grounds that it's a waste of time. The amount of dishonesty exhibited by the people involved was quite an eye-opener for me.

66. **Ori**
September 22, 2007

Peter said

" But the reason QFT makes predictions is that one of the simplest possible such choices works just about perfectly. If simple choices of QFT disagreed with experiment, and you had to go to extremely complicated sets of choices in order to avoid contradicting experiment, never actually getting to anything that you could test, then QFT would be in the same situation as string theory "

This is complete nonsense. According to what you decide that SM is amongst the simple gauge group ? it is actually one of the complicated ones !! There many many theories, much much simpler than SM and they don't describe nature. It is not 5 or 10 theories simpler than the SM , it is infinty !

For instance, if we say that a guage theory is simple if its perturbative expansion is simple, which is pretty well defined criterion than clearly infinitely many SUSY theories are much simpler than the SM.

There are severe inconsistencies, like the one I point out here, in all Peter's claim. I wonder what Einstein would think of Peter 🙂

67. **Marion Delgado**
September 22, 2007

If that's your criticism, you shouldn't imply with the title of your book and blog that you're depending on Popper's criterion. I realize it started with Pauli, but the fact is that in the modern era saying something is not even wrong in science usually denotes a "falsifiability" criterion. There are some common uses of "not even wrong" that don't apply to string theory, as well.

The "moribund research programme" approach is pegged to Lakatos, and his model of H&SS is very different from Popper's. I think it's particularly well-suited to string theory. There are some things that would have falsified string theory, as well as other theories we regard as more parsimonious. Moreover, it may be that the earliest formulations have basically been falsified, which is a secondary reason (behind the desire to cover more territory) why the paradigm (not really AN hypothesis or A theory) had to morph so many times.

Actually the old saying that i think fits better is that it has ideas that are original and valuable. But the ones that are original aren't valuable and the ones that are valuable aren't original (or unique to string and brane theories). Like intelligent design, string theory now has become a follower – faced with a success by a competing theory, it says, we can model that, and does so. Every once in a while it cannot, at which point more degrees of freedom are introduced.

68. **Peter Woit**
September 22, 2007

Ori,

I guess we'll just have to disagree about whether SU(3)xSU(2)xU(1) is among the simplest possible choices of gauge groups. But what there is no way to argue about is that the Standard Model is the most accurately predictive physical theory ever, and string theory predicts absolutely nothing at all. If you want to explain why this is really very much the same thing, go right ahead…

69. **Ori**
September 23, 2007

Peter, I did not meant to argue what is the simplest gauge group (that was a typo). This is not a good physical question. It makes much more sense to argue what is the simplest physical theory. Namely, which one has the most elegant structures. Of course, there are many many QFTs with various non trivial constraints on their perturbative and non perturbative phenomena, so they are much simpler than the SM. There infintely many such theories.

Hence, the whole argument of Peter does not make any sense.
Now a different issue is that the SM fits experiment, that's of course correct. However, suppose there were no experiments during the 70's and early 80's, then there would be some PW who would claim that QFT does not make predictions, since it proposes infintely many consistent universes, and many more parameters have to be tuned. Of course, the only constraint, lacking experiments would be that the theory has to include QED. That is rather easy to do, so there are still infinty possibilities.

This is exactly the situation in string theory, no significant experiments (besides one – the cosmological constant) and many models, some consistent others not.

70. **Peter Woit**
September 23, 2007

Ori,

In your analogy, you're assuming that we had QED, the simplest gauge theory, which was extremely successful experimentally. There is nothing like that in string theory. If the simplest version of string theory accurately described most physical phenomena to 10 decimal places, we wouldn't be having this discussion.

71. **Ori**
September 23, 2007

My analogy does not assume anything.

I used QED as an example for the fact that although people knew it is there, there were still infinitely many QFTs consistent with that. Thus, PW would claim that QFTs don't make predictions since there are infintely many consistent examples, non is testable at present etc.

In string theory there is a perfect analogy : string theory PREDICTS that the low energy theory, whatever it is, contains QFT. Yes Peter, it is a prediction of string theory whether you like it or not. Moreover, it actually is a YM theory. However, there are infintely many examples in string theory which reduce to QFT and in particular the SM but there is no experiment to tell which one is correct.

I assure you all that logically the situation is one and the same, what is different is that in one case experiments could decide quite fast what is the correct theory among the infintely many good choices and in the other case, it may take longer, and nobody knows how longer, and also in QFT, it could in principle take much more time before people discovered which microscopic theory gives all these non renormalizable operators.

72. **Peter Woit**
September 23, 2007

Ori,

Again, sure, if you look at all possible gauge theories extending QED, you're not going to get predictions. If you look at gauge theories extending QED, and restrict attention to the simplest ones, you get a finite number of possibilities, ones you can analyze and compare to experiment. It turns out one of them is a huge and fantastic success.

The problem with string theory is that the simplest string theory doesn't look anything like the real world. You can't start with it. Instead, you have to keep adding complexity to it to get agreement even with the gross features we observe. The state of string theory now is that, just to avoid basic contradiction with experiment, people have been forced to look at such complicated versions of string theory that they are looking at essentially infinite classes of theories, of such complexity that they can't accurately calculate much of anything. This is a failed research program, failed because it tried to do what theorists always do when they investigate a new idea, but it didn't work. What theorists always do with a new idea is look at the simplest versions of it, the ones they can analyze the implications of, then compare to experiment. Sure, if they get disagreement, they try and look at more complicated versions. But, sooner or later, if things just get more and more complicated and you never predict anything, you have to give up and admit failure. The only unusual thing about this story is the refusal to admit failure.

I'm sorry, but I really think that some string theorists such as yourself have gone over the deep end. You are claiming that two opposite poles of science, spectacular success (the SM), and utter failure (the landscape), are logically the same thing. This is only true in the sense that black is a version of white.

73. **?**
September 23, 2007

Actually what theorists do (at least the good ones) is try to describe something about the world, that seems interesting.

In a totally different subfield (to avoid the usual idiotic bickering about string theory), condensed matter theory, theorists have worked now for 25 years to try to understand high temperature superconductivity. They have, by and large, failed. That is because it seems to involve numerous intricate behaviors that doped cuprates can exhibit, which may require an understanding of dualities, dynamics, and materials that we don't have yet.

This doesn't mean that the subject of condensed matter theory is a failed, dead end. It means the theorists are struggling with a hard problem.

I suspect the same is true of string theorists.

74. **D R Lunsford**
September 23, 2007

?,

Good point, and you make Peter's, because that field, like the standard model, is phenomenology backed up by hard observations and a tractable theoretical expression. Like the SM, the limits of phenomenology show up – without a fundamental model, there is little to suggest which way to turn. Phenomenology is by its nature exhaustive – it uses up its good start and eventually bumps up against its inherent limits. That doesn't make it wrong – it makes it honest. It doesn't make it a failure. It just means there is more work to do.

-drl

75. **observer**
September 23, 2007

> I assure you all that logically the situation is one and the same, what is different is that in one case experiments could decide quite fast what is the correct theory among the infintely many good choices

And what is, exactly, the experimental data that would be needed to decide what is the correct *string* theory?

76. **chris**
September 24, 2007

ori,

"For instance, if we say that a guage theory is simple if its perturbative expansion is simple, which is pretty well defined criterion than clearly infinitely many SUSY theories are much simpler than the SM."

this is another piece of priceless anthropic argumentation. there are so many out there in hep. let's recount a few:

.) the gut/planck scale is so high up that we can't reach it in laboratories. extra dimensons bring it down to a convenient few TeV. this is a success of extra dimenions.

.) gravity seems to have a non-gaussean uv fixed point. but we really can't deal with nongaussian fixed points. let's just abandon the approach and invent something else.

at least in qcd, we have sucessfully defeated this nonsense. qcd is firmly established in the nonperturbative sector and techniques have been developed to deal with it. we no longer need phony fundamental strings or s-matrix theory with nothing behind.

your statement is firmly in this category. it's difficult to compute, so let's just take a simpler to compute theory.

let me tell you, that i do think exactly the opposite of what you stated. a theory that is structurally simple but has a host of interesting phenomena (like e.g.QCD) is a much more promising candidate than a theory that adds extra degrees of freedom just to make it more convenient for the hotshot theorists to crank out paper after paper with minimal effort of developing new techniques/insights/understanding.

77. **Coin**
September 24, 2007

> In a totally different subfield (to avoid the usual idiotic bickering about string theory), condensed matter theory, theorists have worked now for 25 years to try to understand high temperature superconductivity. They have, by and large, failed.

This seems kind of different considering that we *know*, experimentally, that high-tc superconductors exist.

78. **non_linear**
September 24, 2007

High Tc Superconductivity is one of the most interesting cond. mat. phenomena but this doesn't mean that every con.mat. theorist deals with it. There is so many topics in con.mat. where there have been major advances in the last 25 years. Spin glasses, Bose-Einstein Condensation, nano physics, non-linear effects etc. etc. And at least we do have a very well microscopic theory (BCS) that explains
low -temp superconductivity ….
I miss anything similar to the achievements of cond.mat. theory in string theory so i'd be cautious do draw analogies between these two fields.
But again many string theorists seem to actually believe that string theory actually can explain superconductivity (Polchinksi being the most prominent i think) so I 'm actually waiting for a cond.mat. paper with title "Non-perturbative M-theory of the cuprates" or so to have a good laugh …

79. **Peter Shor**
September 24, 2007

As somebody who knows information theory, I want to say that I don't think it makes any sense to say that there are an infinite number of theories less complicated than the Standard Model, unless you have some fairly contrived definition of "less complicated." The Standard Model can be described (assuming adequate mathematics and physics background) in just a few pages. Using the Kolmogorov complexity criterion for simplicity, any simpler theory would need to have a shorter description. This still leaves lots and lots of theories with shorter descriptions (although many of these are not consistent), but only a finite number of them.

I suspect you could come up with a definition of "less complicated" in which any string theory measures up as less complicated than the Standard Model, but in this case you are definitely stacking the deck.

80. **?**
September 24, 2007

I would say we know that gravity and quantum mechanics exist
and are looking for a way to reconcile them as well. The fact that
the high-T_c guys have much more data, can do hands on
experiments, and still are stuck, just shows how hard the problem
high energy theorists are grappling with is. Luckily, there will
be new experimental data very soon.

81. **Thomas Larsson**
September 24, 2007

Ori (Ganor?), there have been a lot of significant experiments testing string theory signatures, e.g. supersymmetry (SPS, Tevatron, permanent electric dipole moment, muon g-2, proton decay) and extradimensions (deviation from Newton's law at short scales). It is just that these experiments haven't found anything. Like the Michelson-Morley experiment didn't find the ether wind.

82. **observer**
September 25, 2007

> I would say we know that gravity and quantum mechanics exist and are looking for a way to reconcile them as well. […]Luckily, there will be new experimental data very soon.

"?", what experimental data will allow us to reconcile gravity with QM? I guess by "very soon" you must be referring to the LHC. Will the LHC, according to you, provide enough data to unify gravity and QM?

83. **Eric**
September 25, 2007

Observer,
If LHC discovers supersymmetric partners, then one can look at local supersymmetric models, otherwise known as supergravity which naturally finds its home within string/M theory. Is this not a unification of gravity with QM (in fact the only viable unification currently known)?

84. **Thomas Larsson**
September 25, 2007

> If LHC discovers supersymmetric partners…

Ah, but supersymmetry will not be discovered at the LHC. I have given a rigorous proof showing that it is impossible. Didn't you know that? 🙂

85. **David**
September 29, 2007

Hey, been reading your site for awhile now Peter Woit and I was wondering what theory you favour ?
Is there any credible scientist out there who believe in ONE universe and not MWI/String/Mtheory etc?

86. **JE**
October 1, 2007

Slightly off-topic, but I saw Steinhardt, Ovrut and Turok explaining their ideas on cyclic cosmology late on Saturday night as Malcolm Clark's 2002 "Parallel Universes" BBC documentary was broadcast on Spanish TV. Besides their train ride to London, where they came up with the idea of a cyclic universe (which they characterized as a sort of cathartic moment of artistic creation, as if they had effectively uncovered some hidden truth), it was amazing to see how Lisa Randall's climbing skills were mixed up with the quest for the 11th dimension, brane collisions, the multiverse idea and other tentative proposals to produce such a piece of hype for string theory, supergravity, braneworld scenarios and speculative physics in general. Not that anything in all that mix-up of ideas can not be found to have any connection with the real world in some very distant future, but the dishonest way in which all this stuff was sold off, without even adding the word "tentative" or "speculative" before dispatching it to the layman, really appalled me.

The Wall Street Journal on the Tate Conjecture

September 25, 2007
Categories: Uncategorized

This is not a very timely posting, since my readers let me down by not telling me about this when it came out. Last month the Wall Street Journal ran a piece by Lee Gomes about a workshop on the Tate conjecture held recently at AIM, the institute now housed in Palo Alto behind Fry's Electronics, at some point to move to its own castle. The piece was entitled Math Whizzes at Conference Prove Just How Exciting The Tate Conjecture Can Be, and it gave a good feel for what a math workshop looks like to an outsider. The full piece is not available on-line, but the MAA *Math News* has an article that quotes much of it.

I noticed two inaccuracies in the piece. It begins with:

> One is tempted to feel sorry for mathematicians. In contrast to, say, physicists, mathematicians don't have their own Nobel Prize; they rarely get hired by hedge funds; they don't have grand toys like particle accelerators to play with; and their work is usually so recondite that not even their families understand it.

This is pretty accurate except for the part about hedge funds. I know quite a few mathematicians who have gone to work for them, and at some of them mathematicians form a sizable fraction of the people holding so-called "quant" jobs.

At the end of the piece there's the news:

> Progress, though, was made. V. Kumar Murty, of the University of Toronto, said that as a result of the sessions, he'd be pursuing a new line of attack on Tate. It makes use of ideas of J.S. Milne of Michigan, who was also in attendance, and involves Abelian varieties over finite fields, in case you want to get started yourself.

Milne has recently posted an article on the arXiv (also available on his web-site here) that corrects this, noting

> This becomes more-or-less correct when you replace "Tate" with the "weak rationality conjecture".

Milne's article is actually a write-up of his talk at the AIM workshop, and it does an excellent job of surveying the state of what is known about questions related to the conjecture.

I was going to try and put together some explanation of what the Tate conjecture says and how it relates to other parts of mathematics, but since this is a tricky business, and since experts who really understand this have already done a better job elsewhere than I could ever do here, I'll mostly just provide links.

The Tate conjecture is an analog for varieties over finite fields of one of the Clay Millennium problems, the Hodge conjecture, which deals with the case of varieties over the complex numbers. For a popular discussion of this, there's a nice talk by Dan Freed on the subject (slides here, video here). In the number field case there's another Millennium problem analog, the Birch and Swinnerton-Dyer conjecture. For a popular discussion of this, there's a video of a talk by Fernando Rodriguez-Villegas (who has a blog here).

These conjectures all revolve around the idea that it should be possible to relate three apparently different mathematical objects associated with an algebraic variety:

- The space of algebraic cycles in the variety, modulo some equivalence relation
- Certain cohomology groups associated to the variety
- The order of a pole in the zeta-function of the variety

There's no evidence we're close to a proof of these conjectures, but there are many partial results and the conjectures can be proved in certain special cases. Experts seem convinced of the truth of these conjectures despite the lack of proof, one reason being that they fit nicely into the general philosophy of "motives" first promulgated by Grothendieck. One expert on the Tate conjecture, when asked about the probability of it not being true, responded something like: "Don't be silly. It's true."

For more about the Tate conjecture, there are two documents put together for the AIM workshop that may be helpful: an expository piece for a wide audience here, and a technical summary of the workshop here.

Comments

1. **Clark**
   September 25, 2007

   Thank you for your reference to the talk of Prof. Milne, to whom it is always worth paying attention .

   I note from this source that he is not convinced of the validity of the Tate Conjecture, at least in full generality:

   "ASIDE 6.4. The Hodge conjecture is known for divisors, and the Tate conjecture is generally expected to be true for divisors. However, there is little evidence for either conjecture in higher codimensions, and hence little reason to expect them to be true. On the other hand, Deligne expects his conjecture to be true. "

2. **JP**
   September 25, 2007

   Tate conjecture?

   Seems like esoteric mathematics … probably doesn't have much to do with theoretical physics. But thanks anyhow for the post.

   JP

3. **Matt Daws**
   September 26, 2007

   "they rarely get hired by hedge funds". Yep, as you say, the writer clearly has never heard of Jim Simons for example.

4. **Thomas Love**
   September 26, 2007

   Since I had already read this entry, I decide to check out the links you provide in the right hand column. I went to each of the sites under String Theory Weblogs and found discusisions of The Bionic Woman, How to do slide shows, The Beijing Jazz festival, A proposal for a new theme new website, The Cosmic Climate link, a site last

updated on Dec 5, 2006, The fun of cleaning up, another
site last updated on Dec 5, 2006, and a site in Spanish, which I don't know. I have to conclude that there is not much progress being made in String Theory. Surprise, Surprise, Surprise.

5. **Jim Clarage**
   September 26, 2007

   The video link to Dan Freed's talk didn't work for me. The link to it from the main Clay video page:

   http://www.claymath.org/video/

   does appear to work.

   ps,. thanks for the respite from the landscape. These popular/survey links are always appreciated.

6. **woit**
   September 26, 2007

   Thanks Jim,

   Link fixed. Now if only my postings on things like the Tate conjecture would draw 100 comments…

7. **SnarkFest**
   September 26, 2007

   Jim Simons wasn't *hired* by a hedge fund. He *started* one. Right?

8. **Tom Whicker**
   September 28, 2007

   Instead of String Theory, it should be "The Susskind Conjecture".

From Fermilab to Equivariant Cohomology

September 28, 2007
Categories: Uncategorized

Various things of interest, ordered in terms of increasing mathematical content:

This week Fermilab has hosted a P5 meeting and an annual program review.

At the P5 meeting, Fermilab director Pier Oddone made the case for planning to keep running the Tevatron through FY 2010. He pointed out that the current LHC schedule has "no float" for any possible delays in putting the hardware together, and only allows for 3 months between first beam and physics collisions, drawing the conclusion that it was unlikely the LHC would have physics results competitive with the Tevatron before the currently planned closure date of September 2009. Presentations from D0 and CDF claimed that, if the machine runs through FY2010 and provides them with a projected luminosity of 6.8 fb^{-1}, they should be able to exclude the possibility of existence of the Higgs at 95% confidence level over almost the entire possible range of Higgs masses (if it isn't there!) or find 3 sigma evidence for its existence in some mass ranges (if it is).

At the program review, there was an overview of particle theory at FNAL from Andreas Kronfeld, and a presentation about the LQCD lattice gauge theory project from Paul MacKenzie. Several interesting documents reviewing the state of the lattice gauge theory work are here.

Over the last few months I've often told myself that I should learn more about Howard Georgi's ideas concerning "unparticles" and try and write something about them. Sabine Hossenfelder has saved me the trouble, you can learn about this here.

Last month there was a symposium at Durham on Twistors, Strings and Scattering Amplitudes, a subject which has seen some exciting activity recently. Zvi Bern reviewed progress on computing multi-loop amplitudes in N=4 gauge theory and in gravity theories. He noted that the recently found unexpected one-loop cancellations in N=8 supergravity (leading to the so-called "no triangle hypothesis") are not due to supersymmetry and are already there in non-supersymmetric gravity. This leads him to conjecture that other gravity theories will be perturbatively finite, he explicitly mentions N=6 supergravity. Nathan Berkovits discussed multi-loop superstring amplitudes in the pure spinor formalism, ending up by noting that there are possible problems caused by needed regularization of ghosts in this formalism, and they affect high-energy contributions to the 4 point 3-loop amplitudes. Not that I'm saying I think this will happen, but it would be pretty damn funny if it turns out that multi-loop superstring amplitudes aren't finite, multi-loop supergravity ones are…. There's also a talk by Jacques Distler, who continues his ceaseless quest to figure out how to make physics available over the web in a form that no virtually no web-browser can display properly.

Finally, I strongly believe in advertising equivariant cohomology as much as possible, for mathematicians and for physicists. The new lecture notes by Matvei Libine are a good place to read about it.

Comments

1. **JC**
   September 28, 2007

   Could it be that the pure spinor formalism is just showing its limitations and shortcomings, as opposed to the string multiloop amplitudes being genuinely nonfinite?

2. **Peter Woit**
   September 28, 2007

   JC,

   Sure, it could just be a problem of the formalism, and in any case maybe even within that formalism it can be dealt with. What's interesting though is that, at higher loops, the standard ideology "string amplitudes finite, (super)gravity infinite" may very well be wrong. Hard to be sure until, in both cases, the amplitudes are better understood.

3. **JC**
   September 28, 2007

   If somebody ever calculates the correct 3-loop superstring amplitude (or higher) expilicity and it is shown to be genuinely nonfinite, then I wouldn't be surprised if this ends up being the last nail in the coffin for string theory.

4. **DB**
   September 28, 2007

   Nobel Laureate Martin Veltman gave an interesting public lecture (not sure how old it is – doesn't appear to have been mentioned here before, my apologies if it has) where he offered his highly sceptical opinion (in decreasing orders of scepticism) on string theory, supersymmetry, the cosmological constant and the Higgs mechanism in the context of a proposal to motivate the construction of an 800GeV linear collider in Hamburg as a successor to the LHC.
   It doesn't require specialist knowledge and contains interesting remarks about the US direction in particle physics. A video of it is here:

   http://pauli.physics.lsa.umich.edu/w/arch/som/sto2001/Veltman/real/n001.htm

   It makes for an interesting comparison with his co-laureate and student Gerhard 't Hooft (who also gives excellent public lectures, by the way) who is quoted in the recent Physics World article as advising physicists to keep such debates out of the public arena.

5. **Thomas Love**
   September 28, 2007

   "Unparticle" physics remind me of the uncola. But it makes me think that perhaps string theory should be called particle "unphysics".

6. **Peter Orland**
   September 29, 2007

   Thank you, DB.

   I checked out Veltman's lecture. Though I think his momentum-space
   suggestion is totally wrong, he has framed the current state of physics
   and its problems in a very sensible way.

7. **Kris Krogh**
September 29, 2007

That Veltman lecture was 2001, before this blog existed. Courageous stand against string theory for that time. Peter, I know cosmology doesn't interest you as much, but I hope you also heard his remarks on that topic and general relativity.

8. **Kris Krogh**
September 29, 2007

If you use this link, you can view the video and slides for Veltman's lecture simultaneously.

9. **Luzo**
October 1, 2007

Veltman also makes some nice comments at a lecture I once saw at the CERN library. He was very vocal and corrosive regarding string theory. He basically said that he only trusts things that calculate things (something like this).

I don't know whether these comments were made before or after this lecture.

10. **Bee**
October 1, 2007

Hi Peter,

Thanks for the link, I only just noticed. Yes, the unparticle stuff is a fairly weird development. I'm not planning on looking closer into it though. Some of the comments to my post are also interesting in this regard.

Best,

B.

11. **Doug Natelson**
October 1, 2007

I have a silly, naive question from a condensed matter person…. It seems like "unparticles" are the HEP equivalent of a condensed matter phase that simply isn't well-described in terms of quasiparticles. For example, when Fermi liquid theory fails in some strongly correlated electronic materials (e.g. the "local moment" phase in heavy fermion compounds), there appears to be no simple description of the low energy excitations of the resulting phase, at least not in terms of weakly interacting quasiparticles with simple quantum numbers like momentum and angular momentum. Is that basically the idea here?

12. **JE**
October 2, 2007

Concerning the searches for Higgs and supersymmetry at the Tevatron, the D0 and DF teams have just submitted a joint paper about their status. As stated in its conclusions, "the `hint´ of an MSSM Higgs boson at m_a around 160GeV obtained by CDF was not confirmed by D0". In other words, no Higgs or SUSY so far. This is the link:

http://arxiv.org/PS_cache/arxiv/pdf/0710/0710.0248v1.pdf

13. **a quantum diaries survivor**
October 2, 2007

JE,

the proceedings you point to do not include the most recent results. CDF has a new result for MSSM Higgs blessed for both the bbH->bbb(b) and the H->tau tau searches. These update previous results based on half the statistics.

As far as the former goes, it is reported in my blog – I wrote about it a month ago. The latter, although blessed, is not accessible online yet to my knowledge. I did write about it today, but I left out the results, because I prefer to leave a chance to the authors to do that first. In any case, your conclusion does hold.

Cheers,
T.

14. **Shantanu**
October 3, 2007

DB, thanks for the link to Veltman's talk . He certainly has non-conventional povs about validity of GR and there are also interesting discussions at the end.
Peter , what do you think of the talk? Has Veltman read your book or have you
corresponded with him regarding string theory?
Thanks

15. **Peter Woit**
October 3, 2007

Doug,

I was hoping someone knowledgeable would answer your question. I haven't looked closely at what Georgi is doing, but assume you're basically right, that the point is that there are field-theoretic phenomena not describable by particle-like excitations.

Shantanu,

Haven't had a chance to look at the Veltman talk. I corresponded a little bit with him when I was trying to get the book published, and he was quite sympathetic. I'd suspect my more mathematical point of view is not to his liking, but that's he'd agree with much of the critique of string theory. I had the pleasure of meeting him once when I was a postdoc at Stony Brook, and found him to be quite an impressive character.

Scientists Ask Congress To Fund $50 Billion Science Thing

September 28, 2007
Categories: Uncategorized

The latest issue of the Onion has some HEP-related coverage. It includes a nifty graphic, and has this inspirational message from one of our congress-people

> “Now, I’m no science major, but if I’m being told by a group of people that the protons, neutrons, and electrons need unifying, then I think we owe it to the American people to go in and unify them,” Rep. Mark Udall (D-CO) said. “After all, isn’t a message of unity what we want to send to our children?”

Comments

1. **Chris Oakley**
   September 28, 2007

   The graphic looks like a Tokamak to me. So all that High Energy Physicists need to do is to claim that Synchrotrons, like Tokamaks, are a stepping stone to solving the world’s energy problems and funding will be secured.

2. **Yatima**
   September 28, 2007

   People actually have time to read the Onion?

   Well, this is clearly the International Torus Experimental Reactor (ITER):
   http://www.sbf.admin.ch/htm/themen/international/euratom_de.html

   You can even see a loop or glowing closed string in the cavity. I will get my coat now.

3. **Paulo Guerra**
   September 28, 2007

   Hi, I’m from Brazil and I thought only brazilians politicians give declarations like that.

4. **hmmm**
   September 29, 2007

   This is a must read for all high energy physicists.

   I don’t think it’s funny. I think it’s a good look in the mirror for a lot of people who have forgotten that; ultimately, we serve at the pleasure of the public.

   We won WWII and fueled myriad technological advancements. We deserved the MASSIVE govenment welfare programs we currently enjoy (NSF, DOE, national labs….).

   When high energy physics lost contact with the real world, and even the hope of gaining any tangible benefits started to fade….the funding started to fade.

   Now HEP is lost in the multiverse….WTF.

5. **retardigrade**
   October 2, 2007

   “While expense is something to consider, I think it’s very important that we have this kind of scientific apparatus, because, in the end, I have always said that science is more important than it is unimportant,” Committee chairman Rep. Bart Gordon (D-TN) said. “And it’s essential we stay ahead of China, Japan, and Germany in science. We are ahead in space, with the NASA rockets going to other planets, so we should be ahead in science too.”

   “These scientists could trim $10 million if they would just cut out some of the purple and blue spheres,” said Rep. Roscoe Bartlett (R-MD), explaining that he understood the need for an abundance of reds and greens. “With all of those molecules and atoms going in every direction, the whole thing looks a bit unorganized, especially for science.”

   “Now, I’m no science major, but if I’m being told by a group of people that the protons, neutrons, and electrons need unifying, then I think we owe it to the American people to go in and unify them,” Rep. Mark Udall (D-CO) said. “After all, isn’t a message of unity what we want to send to our children?”

   “Fifty billion dollars to buy atoms is too much,” Rep. Tom Feeney (R-FL) said. “Frankly, I don’t understand why they don’t just gather up all the leftover atoms in their test tubes and Bunsen burners. I think the scientists should have to use those up before getting new ones.”

   By gumbo, but we’re a sharp bunch, aren’t we? With smart cookies like that leading us, our superiority in science is assured.

6. **a quantum diaries survivor**
   October 2, 2007

   Retardigrade, Peter, all:

   There is clearly a pattern here. I seriously think these people are making it up. I cannot conceive somebody uttering such idiocies. Rather, I think they want to convey the message that they are ordinary people who watch football and don’t understand science.

   Whether that is the case or not, I feel for you guys. I guess you did not deserve such a government.

   Cheers,
   T.

7. **a.k.**
   October 2, 2007

..one has to note that this is a brilliant text, it is brilliant as only satirical texts can be, it even refers to 'self-referentiality' ('I've always said that science is more important than it is unimportant'), which is clearly a non-trivial problem both in mathematics and physics, this '..where science will ultimately occur..' on the other hand links with an evil undertone to the fact, that the LHC will -possibly- force more scientific concepts to vanish or to remain shady areas of speculation than to transport them to a surface where they could 'ultimately occur', but anyway: the combination of dumb every-day belief and scientific arrogance and impenetrability is too typical for western societies not to be matched by satirical intelligence.

8. **retardigrade**
October 2, 2007

a quantum diaries survivor says, "There is clearly a pattern here. I seriously think these people are making it up. I cannot conceive somebody uttering such idiocies. Rather, I think they want to convey the message that they are ordinary people who watch football and don't understand science."

Yeah, okay, seriously. There IS a clear pattern here. I would submit that even in the event they are (ALL of these quoted) "making it up…in order to convey the message that they are ordinary people who watch football and don't understand science" is sufficient testimony for the existence of idiocy. Whether you can or can't conceive of people "uttering such idiocies" is entirely irrelevant. (I have trouble conceiving the possibility myself…it means nothing). The net idiocy, however, remains.

In fact, the conscious masquerade of deception which you propose as an explanation for such foolish statements that can impinge on the future of the country is arguably tantamount to unpatriotic behavior, if not treason. In terms of government, I can't conceive of anything more idiotic than that. In a supposed "democracy", such behavior becomes downright ludicrous.

NO. I don't buy it. I really DO think these people ARE actually that stupid. But I ALSO think they revel in their culturally-instilled bragging-rights for stupidity.

They don't need to pretend to be smart or stupid, but that doesn't absolve them of their ignorance where it is most painfully apparent, nor does it mean we should let them off the hook just because they feel a need to build an affinity with common folk who enjoy, say, football.

I enjoy a good football game too, but I don't feel any need to act like an idiot in an attempt to ingratiate myself to other enthusiasts. Such an approach I THINK would be insulting to my fellows. At the very least. I'll continue to assume they are smart, and respect them for it, thank you. Similarly, I'd just like to see reps and senators start acting like we aren't nearly as dumb as they evidently think we are.

What in the world would it take for our political leadership to HONESTLY espouse and promote the merits of scientific literacy and public education in general, for the authentic good of the country, if not the world, with the same (deceptive!) zeal they apply to getting reelected? What the flaming heck do these people think the purpose of their job is???

Yes, I know those may be construed as rhetorical questions. We all "know" what politics is ultimately all about, right? Sure. But they still need real answers. The trouble is, hardly anybody has the guts to ask the hard questions anymore.

9. **anon.**
October 2, 2007

This is the funniest comment thread I've seen in quite some time.

10. **Lowenergy**
October 2, 2007

According to wikipedia, the Onion is a US parody newspaper. This mean that politicians have still some road ahead of them! 🙂

11. **non-a**
October 2, 2007

> I guess you did not deserve such a government.

Mmmmhh… talking about congress people, would Cicciolina have done better? But, on second thought, at least there was one thing she demonstrably did very well. That's probably more than can be said about any other politician anywhere…

12. **anonymous**
October 2, 2007

This is a (very funny) satirical article from a well-known satirical paper. Has anyone considered the possibility that the dumb comments from the congressmen (on whose smartness I have few illusions anyway) were just made up by the writer?

13. **parmenidis**
October 2, 2007

I 'm kind of perplexed here, did anyone of you took the article really seriously ?
Uhm anyway there is another one which I find even more funny

"Evangelical Scientists Refute Gravity With New 'Intelligent Falling' Theory"

http://www.theonion.com/content/node/39512

Peter you are right it's not string theory that unifies physics.
…..It's jesus

14. **D R Lunsford**
October 2, 2007

My browser can't display extra dimensions. Do I need a new plugin?

-drl

15. **Chris Oakley**
October 3, 2007

Hi Parmenidis,

You could be on to something here. God may have created String Theorists to formulate non-predictive theories, so that mankind would never cease to be in awe of the wondrousness and incomprehensibility of the universe.

16. **a quantum diaries survivor**
October 3, 2007

Fascinating hypothesis Chris. My own is that God himself does not understand the universe. He was experimenting with singularities when the whole thing blew him off. In that sense we resemble Her: and string theorists even a tad more.

Anyway, I rather wanted to answer retardigrate here:

"What in the world would it take for our political leadership to HONESTLY espouse and promote the merits of scientific literacy and public education in general, for the authentic good of the country, if not the world, with the same (deceptive!) zeal they apply to getting reelected? What the flaming heck do these people think the purpose of their job is???"

I think you are giving for granted something which is not – i.e., that everybody agrees that scientific literacy is good for the world. That assumption is a deadly sin. Many of those who can't spell quark actually think science is just a pastime.

Cheers,
T.

17. **Arun**
October 3, 2007

God just has to make itself unambiguously apparent for mankind to be in permanent awe of the mysteriousness and incomprehensibility of the universe; no need to mess around with string theory.

Anyway, re: string theory, it is amazing that we think we have all the physical principles at hand to specify physics at the highest energy.

18. **rob**
October 3, 2007

Great. But the all-time classic onion physics story is this one:

http://www.theonion.com/content/node/38718

If anyone mistook that one for a real story, I'd be awfully surprised.

19. **Changcho**
October 3, 2007

I suppose it is very telling (mostly about the US political leadership) that some could not tell that the story is a parody…

20. **Anon**
October 3, 2007

Actually, it's very telling about the general readership of this blog. Woit's rants against string theory and science in general only gain traction because such people exist.

21. **non-a**
October 4, 2007

> gainst string theory and science in general only gain traction because such people exist.

Which leads us back to the old wisdom that anyone doubting string theory is an anti-science moron with an IQ lower than 10.

Frankly, ST supremacists don't seem to have any new ideas. May that be because they are string theorists?

22. **Anon**
October 4, 2007

Actually, a good fraction of the people who closely follow this blog are 'alternative scientists' who are against string theory essentially because it represents 'the establishment'. Woit essentially exploits these people in order to gain a following.

23. **a.k.**
October 4, 2007

..well, I know, and they are leftist, self-righteous 'non-producers' who are mainly interested in following their path into eco-phantasizing foreagers' liefstyle, while leading comfortable lives on the cost of all the others who are following the thoughts and ideas of others, as it is adequate for serious scientists. If these latter, serious scientists however, run into trouble with their department, they invert their views from constant praising to qualifying their former colleagues as being 'parasites' etc., as if one behaviour would naturally induce the other. Anyway: I strongly recommend Horkheimer/Adorno: 'Die Dialektik der Aufklaerung', this should give enough material for some TOE-reflections.

24. **Marty Tysanner**
October 4, 2007

"Anon" says,

> Actually, a good fraction of the people who closely follow this blog are 'alternative scientists' who are against string theory essentially because it represents 'the establishment'.

This could be true, but what is the factual basis for saying it? Even granting the possibility that the comment may be valid for the range of commenters, "Anon" is surely aware that many more people read blogs than comment in them, and there is no obvious correlation between the viewpoints of those who leave comments and those who don't.

"Anon's" comment could equally apply to any number of other blogs, for example Cosmicvariance (as a perusal of comments on string theory related topics there indicates). The comment would probably be equally correct if it were rephrased along these lines: *A good fraction of the readers who closely follow [insert ST-oriented blog name here] are string theory partisans essentially because it represents 'the establishment'*. That could also be true, but so what? Observations like these are basically content-free and irrelevant to whether the blog itself is valuable.

"Anon" (and others with similar views) appear to want consumers of their derogatory comments about *Not Even Wrong* to associate skeptical views of mainstream research directions in HEP with being "uninformed" or "contrarian for its own sake." That is a very simplistic view of skepticism, one that itself seems very uniformed. It is also very insulting to the apparently significant number of serious scientists and mathematicians who at least occasionally visit this blog. Perhaps "Anon" should talk to physicists from a variety of disciplines before deciding that skepticism about string theory indicates a contrarian world view.

Finally, the "observation" that

> Woit essentially exploits these people in order to gain a following.

is much different than my own perception. I don't see Peter cunningly marshalling the forces of the ignorant and anti-establishment folks to bolster his case. Perhaps "Anon" has overlooked Peter's repeated pleas to stop the stream of uninformed or repetitive anti-string theory comments, or his judicious pruning of many such comments…

Frankly, given the apparent ignorance and simple-minded analysis that is evident in "Anon's" comment, it is quite understandable to me why he/she wishes to remain anonymous.

25. **Arun**
October 5, 2007

Re: God (joke by Gerry Porter, rec.humor.funny)

(UPI) Heaven. God has lost Her NSF grant. The National Science Foundation cited three reasons in deciding not to renew the Holy Grant.

1. Although God has done good creative work in the past, there has been no recent evidence of creativity.

2. No one as yet has been able to reproduce Her experimental results.

3. She has only written one book, and it has never been subject to peer review.

26. **Arun**
October 5, 2007

Phillip W. Nabours:

This actually happened, I didn't make this up.

Prof: Some people have proposed using Krypton gas in scintillator detectors.

Grad Student: Won't that scare away the superstrings?

A New Subfield of Physics…

October 3, 2007
Categories: Uncategorized

Things are not going well for string theory on the public relations front. Someone just pointed me to the poll at Wired magazine they call [String Theory Smackdown](), where the side arguing for string theory is losing the voting by more than 3 to 1.

The argument that seems to be carrying the day with the public is the simple one that a supposedly unified theory that can't make a single testable prediction, despite more than twenty years of work, must have something really wrong with it. Many string theorists acknowledge that this is the situation the theory is in, but make the case for what they see as promising aspects of the theory that justify continued work on it.

Unfortunately, some string theory partisans have chosen to react to recent criticism not by acknowledging the fact that string theory can't be tested, but by making misleading claims that the theory does make predictions and is testable. On Monday here at Columbia, Gordon Kane gave a colloquium talk of this kind, with the title *String Theory and the Real World — a "new" subfield, string phenomenology*. Kane began by quoting David Gross as being highly skeptical about the whole idea of string phenomenology, arguing "we don't know what string theory is, how can it have a phenomenology?". Kane's claim that "string phenomenology" is a new field is rather peculiar, since it was an active subject back in the early 1990s. It is however true that, for better or worse, it has become a more active one the past few years, as string theorists have reacted to their colleague's complaints that they do mathematics, not physics, by trying to sell themselves as "phenomenologists".

Kane mostly actually ignored string theory, concentrating on supersymmetry, which he has been promoting for more than 20 years (he had an article about "Is Nature Supersymmetric" in Scientific American back in 1986). He described seeing supersymmetry as essential, pretty much the only way of getting a "window to the Planck scale". There was some mention of the idea that string theory makes predictions about cosmology, but the "prediction" was just that in "most" string theories, the size of B-mode polarization in the CMB is unobservably small. He put up plots from [this]() recent paper, claiming that one could distinguish different string "backgrounds", by their "footprints" on LHC data. Looking at the paper, it appears to be based upon a large number of assumptions (e.g. that one just gets the MSSM), designed to provide enough constraints so that one could not get absolutely anything, but not so many as to be forced into contradiction with experiment.

For another exercise of this kind, take a look at Kane's 1997 Physics Today article entitled *String theory is testable, even supertestable*. This included an impressive looking detailed, specific spectrum of the masses of superpartners, implying that it was the sort of thing "predicted" by string theory. Only problem is that by now it looks to me as if these "predictions" are almost all in disagreement with experiment. Back in 1997 Kane was arguing against John Horgan that string theory really was testable, that it "would predict a specific spectrum of particles and superpartners that can be compared with experimental data". He seems to have backed off on that claim, there were no such spectra mentioned in his talk this week. About the landscape and its exponentially large number of possibilities, he had little to say except that we "have to learn how to think about this".

He repeatedly made the claim that "String theories DO give predictions" and "String theory is falsifiable", giving as an example work by 3 graduate students of Mary Gaillard that showed that one specific heterotic string compactification scheme gave no light neutrino masses and thus led to models incompatible with experiment. Another repeated point was that the problem with string phenomenology was just a lack of manpower. If more people (especially graduate students) were doing these calculations, great progress would be made. In the question session, asked about the CC, he said that there were lots of ideas about how to solve it, what was needed was just more people doing calculations.

Evidently many agree with him, since the IAS has just announced that next year's summer program for graduate students and postdocs will be on [Strings and Phenomenology]().

I decided not to ask any question in the question session, having the overwhelming feeling that arguing with "string phenomenologists" is now just wasting one's breath. They have made it clear that, no matter how dubious the arguments needed, they're going to keep promoting this field as predictive and highly relevant to the LHC. The intellectual "dead zone" of "string phenomenology" will be with us no matter what and perhaps even come to dominate particle theory until LHC results are in. May they stay as close as possible to schedule! (Kane estimates first physics collisions next September).

Comments

1. **mclaren**
   October 3, 2007

   Dr. Woit remarked:

   > [In] the poll at Wired magazine they call String Theory Smackdown…the side arguing for string theory is losing the voting by more than 3 to 1.

   String theorists make an excellent point when they note that the laws of nature are not subject to a popular vote. We're going to have to wait for the LHC and see. In that connection, new calculations taking into account the latest hadronic PVES experiments apparently place tighter constraints on physics beyond the Standard Model: http://www.jlab.org/div_dept/theory/highlight/beyondSM.html

   Of course if no exotica beyond the SM show up in the LHC it's easy to predict that the string contingent will simply move their predictions to higher energies, as usual.

2. **Demagogue**
   October 4, 2007

   Let's put quantum mechanics to a popular vote. If it fails we'll discard it as a theory and replace it with something that gets more votes, like angels. That's they way to do science – popularity contests.

3. **Peter Woit**
   October 4, 2007

   I wasn't suggesting that the results of a vote like this have anything to do with the validity of a theory. But the fact that string theorists have been losing the public debate does explain some of the tactics they are now adopting in this debate. You don't hear any more the argument for string theory that "it has triumphed in the market of ideas".

   I'd be curious to know what similar numbers would be if you asked not visitors to the Wired site, but Ph.D. physicists. My suspicion is that they wouldn't be very different. About the only population where the results would go the other way would be if you asked string theorists. And even there, I'm not so sure, they seem pretty discouraged these days…

4. **Demagogue**
   October 4, 2007

Maybe string theorists are a large part of the population competent to comment on the matter at all.

There's no reason to expect an experimentalist working on optics to have any more of an idea about string theory than an algebraic geometer.

5. **Visitor**
October 4, 2007

And because science is not a popularity contest, there is no reason to think that that the opinion of string theorists themselves have any bearing on the correctness of string theory either. And might even have LESS, to the degree that their jobs and reputations are built on string theory.

6. **a quantum diaries survivor**
October 4, 2007

Hi Peter,

indeed I think this new delay of LHC schedule was greeted by string theorists as new breathing air…

Cheers,
T.

7. **DB**
October 4, 2007

The "String Phenomenology" codology is an attempt at brand repositioning. When your consumers tell you they don't want your product, instead of reworking the product you go for a re-branding exercise and try to find new applications and markets. New ad campaign, new logo, the works. The alternative is to shut down those factories and layoff all those highly trained workers. Unfortunately, when the product is fundamentally unfit for purpose, you're onto a loser.
The lads at the IAS have been running their string theory show every second year since 2002. They try to attract students from the ICHEP conferences – also biennial. Now ICHEP is where all the action is in experimental HEP but string theory always gets short shrift there, presumably because these people are actually concerned with testing real theories, not mathematics masquerading as physics.
In place of gimmicks, string theorists should take a lead from Witten's approach – use insights from mathematical physics to make new discoveries in pure mathematics – of which his latest paper on Geometric Langlands provides yet another eloquent example.

8. **King Ray**
October 4, 2007

I think that Occam's Razor has slit String Theory's throat.

9. **Kris Krogh**
October 4, 2007

Hi Mclaren,

String theorists make an excellent point when they note that the laws of nature are not subject to a popular vote.

That's ironic, given that so far the success of string theory has been measured mostly by its popularity. Doesn't seem they are really against voting, but have concerns about who should be allowed to vote.

Or maybe the point is that instead of voting on poorly understood ideas, we should vote on people. Edward Witten is chosen "most likely to succeed." A videotaped incident at the KITP comes to mind, where a string theorist proposes a vote on whether Lee Smolin is a crackpot.

10. **Roger**
October 4, 2007

For better of worse, public engagement in the issue has happened and is largely the result of yours and Lee's spur.

As a particle physics experimentalist I have little real knowledge of string theory. Certainly, not enough to form anywhere near an informed opinion as to whether your arguments about string theory making little progress are valid. If I feel unqualified to judge – and I'm trained to a reasonably high level in particle physics theory – then the average politician certainly won't have a clue.

This is what troubles me. I am persuaded by the idea that it is better to keep this debate out of the public arena. Politicians like simple messages. If the prevailing message is that theorists have wasted a lot of money doing stupid things (I know this isn't your message but may be message they get) then it is conceivable that they will cut funding for theoretical physics, irrespective of the topics under study.

The LHC may well provide new directions for the theoretical community which would reduce the influence of string theory. I think it would have been better to wait for the data before starting this debate, especially now that the issue is getting its own momentum and will soon be out of the hands of you and Lee.

11. **Analyzer**
October 4, 2007

Maybe string theorists are a large part of the population competent to comment on the matter at all.

You don't have to be a string theorist, or even a physicist, or even a scientist, to appreciate the scientific method. When you say you can make predictions, and then you don't, your audience doesn't have to be Wittens and Susskinds and Motls to know whether something is wrong.

12. **mo**
October 4, 2007

Roger wrote:

"As a particle physics experimentalist I have little real knowledge of string theory. Certainly, not enough to form anywhere near an informed opinion as to whether your arguments about string theory making little progress are valid. If I feel unqualified to judge – and I'm trained to a reasonably high level in particle physics theory – then the average politician certainly won't have a clue."

It may be difficult for a non-string theorist to understand WHY so little progress in string theory has been made, but it does not take fine sense of discrimination to

ESTABLISH lack of progress. It takes almost no brains to figure out that string theory keeps to promise a lot and delivers very little on the promises.

Yes, the average politician is well trained to sift the wheat from the chaff, even though he/she may be unable to tell one kind of wheat from another. Remember the cancellation of the superconducting supercollider? The project died largely because of delays and cost overruns, but also because of the associated hype (e.g., the promise of cure for cancer) and DOE's deceptive claims.

13. **Changcho**
October 4, 2007

"When your consumers tell you they don't want your product, instead of reworking the product you go for a re-branding exercise and try to find new applications and markets. New ad campaign, new logo, the works."

Of course:

Pre-owned – > Used
Private security contractors -> Mercenary
String Phenomenology -> String Theory

and many others…

14. **Eugene Stefanovich**
October 4, 2007

Roger said:

"This is what troubles me. I am persuaded by the idea that it is better to keep this debate out of the public arena. Politicians like simple messages. If the prevailing message is that theorists have wasted a lot of money doing stupid things (I know this isn't your message but may be message they get) then it is conceivable that they will cut funding for theoretical physics, irrespective of the topics under study."

I strongly disagree. People shouldn't shut up out of fear that some stupid politician misinterprets their words and cuts funding. Quite opposite. The duty of scientists is to educate laypeople and politicians. Open and honest debate is a part of that education.

Eugene.

15. **A.J.**
October 4, 2007

Mo,

What do you think the promises made by string theorists are, and what do you think a reasonable time frame for delivery on these promises would be?

16. **dan**
October 4, 2007

Dear Peter,

Briane Greene once said string theory is the only game in town.

In NEW you yourself pointed out that one reason string theory was popular in early 80s was there were few unresearch seemingly promising ideas left to address the shortcomings of the SM.

What do you think will happen to string theory and to string theorists should LHC fail to find evidence for SUSY? Or, in otherwords, what would most string theorists today (i.e Witten, Greene, Randall, Susskind) work on (both HEP and QG) in an era where LHC does not find evidence of SUSY in the next few years? Will those string theorists interested in QG continue to work on strings or would they switch to LQG?
Do you think those string theorists interested in HEP might jump on Smolin's/Sundance preon braiding bandwagon? Or will string theorists continue to do stringy research ignoring a LHC null result.

Dan

17. **amused**
October 5, 2007

If smart people genuinely think they can get something worthwhile out of this string phenomenology business then good luck to them. But they should have the same obligations to demonstrate interesting and important progress as the rest of us. If they are able to make such progresses, let them "prove it" by publishing them in PRL. And if they can't get published there then the rest of us will draw the obvious conclusion. I'm not personally qualified to assess the worth of research on this topic, but I trust in the capability of the referees and editors of PRL to do so.

18. **Quixotik**
October 5, 2007

"There's no reason to expect an experimentalist working on optics to have any more of an idea about string theory than an algebraic geometer."

Wrong! An experimentalist working on optics would have to know the theory behind her experiments. And she'd therefore know if string theory had been formulated such that it could reduce, under certain conditions, to the those standard theories. And if it hasn't been thus formulated, she knows string theory is far from complete and usable.

That is much more than an algebraic geometer knows, unless the algebraic geometer also happens to have a working knowledge o physics.

19. **Quixotik**
October 5, 2007

And another thing.

The public's job is not to decide whether string theory is valuable or not. The public's job is to decide whether or not string theorists should each be given over a hundred thousand dollars per year to sit around with paper and pencil thinking about string theory. And even an unintelligent uneducated member of the lay public can ask, "What's in it for me?” And if no one can give him a satisfactory answer, he has the right to withdraw his tax money from the paychecks of the string theorists. And if that happens, it doesn’t mean string theorists have to stop working on string theory. They can get a day job and do string theory at night if they’re so convinced it’s a blockbuster.

20. **chris**
October 5, 2007

dear roger,

let’s assume that someone to whom nonabelian gauge theories are a total mystery would need to be convinced of the correctness of qcd. do you have to carry that guy thru the proof of renormalizibility? does he need to know how to compute the beta function?

no. you show him the bjorken scaling plots in the pdb an that’s it.

how about the SU(2)xU(1) weak theory? do you need to understand the higgs mechanism for gauge boson mass generation? no. just look at that picture from UA1 and there is the W and the Z. and you can understand the higgs mechanism better than higgs himself and still there is this shadow of doubt if it is correct at all because we have not seen the particle yet.

every reasonable person is qualified to judge scientific theories, that’s the beauty. they make predictions that either get validated or not.there is a certain time lapse of course. in 1960 the big bang theory was fine even without direct experimental proof. it would be hard to defend today if no CMB would have been found yet. it is equally hard today for steady state fans to argue against the big bang – even if the hardcore ones still do so.

for string theory the clock is just ticking. and they are on the move.

21. **David**
October 5, 2007

when oh when, will you stop being so negative and grace us with your brilliant alternative so we can put that to a vote……

22. **mo**
October 5, 2007

A.J. Says:

“Mo,

What do you think the promises made by string theorists are, and what do you think a reasonable time frame for delivery on these promises would be?”

Speaking in the simplest possible terms, it is better to point out past promises that became claims:

1) they claim they have a theory, but string theory (or more accurately M-theory that substituted it) doesn’t really exist yet;

2) they also claim string theory provides a unification of all of fundamental physics, but it doesn’t even predict the Standard Model parameters.

Re: time frame. Let’s look at comparable projects. It took Einstein about ten years to formulate general relativity. It took 16 years to complete the electroweak theory, from Yang-Mills (1955) to Glashow-Salam-Weinberg (1960s) to t’Hooft (1971). So I would expect that by now we should have had a complete formulation of string theory.

23. **Roger**
October 5, 2007

Interesting responses.

I stand by what I wrote that dragging this into the public arena at this stage is not sensible, especially with the LHC around the corner. Were there to be no LHC then it would be more appropriate to begin the debate now. As it is, should the LHC discover new physics then my money is on the theorists trying to understand the new phenomena quantitatively i.e. chasing the Nobel prize. If string theory is not fit for this purpose then it will naturally ebb away to an extent. Others may have different opinions of how future events will pan out. However, nobody knows anything for sure other than that we have experiments waiting to come on-line which stand a fighting chance of changing the direction of physics research. In this situation it is sensible to hold back – after all we’ve already waited twenty years.

Unless I missed it then nobody responded to my main point which is that sending the message that theoretical physicists are a useless bunch who can’t be trusted with the public’s money is a dangerous message to send. This is the message which is being sent. With every popular magasine picking up this debate, the qualified and carefully worded criticisms become more and more distorted.

As for string theory having made no progress, I’m not going to restate the string theorists arguments, which are well known.

The issue of hype was also mentioned. The LHC has probably been hyped as much as string theory. According to every popular science magasine I’m taking part in an experiment which will explain mass (well not really since most observable mass comes from the strong force), explain dark matter (let’s hope that the hierarchy problem is as serious as we think and we get new TeV physics), and also allow us to somehow unify the fundamental forces (such is the message getting through). The fact is that every science field hypes its work in order to get the public and policy makers on board.

I don’t have any particular love for string theory. My own opinion, for what its worth, is that theoretical physics research may have gone down a blind alley for the past twenty years and needs to learn lessons to avoid repeating this behaviour in the future. However, I’m not convinced that the current blog wars and bringing the fight into the public arena at this stage is the most sensible way to achieve continued funding and future excellence in research, which has to be the final aim.

24. **DB**
October 5, 2007

Roger,

Your position resembles that of Gerhard ‘t Hooft, and is, I believe, the majority view held by physicists, whether they be theoreticians or experimentalists.
It is a traditional stance and can be summarised as “don’t wash your dirty linen in public”.
The problem, as has been repeatedly explained by Smolin and Woit, is that until recently very little debate has taken place either within or without the physics community, and string theorists have had free reign to hype their theories to the public while establishing hegemony over much of theoretical physics research in the US.
The fact that the physics community allowed this to happen shows that the ‘t Hooft approach doesn’t work. That much of the debate has had to be stirred by an outsider (Woit) only highlights a twenty year failure of the physics community to address this issue internally.
Your expectation that the LHC will further diminish string theory status exactly mirrors the prevailing view, i.e., that we can trust to experiments to sort the wheat from the

chaff. There is no evidence that string theorists will take a blind bit of notice of any negative outcomes from the LHC.
The physics community's ostrich mentality will have serious consequences for future physics funding when, not if, the string theory program collapses. Because we didn't put our house in order when we had the chance to do so, outside forces will do it for us, and we only have ourselves to blame.

25. **Roger**
October 5, 2007

You say there is no evidence that there experimental data will sort the wheat from the chaff. This is a statement of the obvious – we haven't taken data yet. Nobody knows what will happen. The only certainty is that we have experiments about to start which have a fighting chance of changing the face of physics. That hasn't been the case for many years. Being hot-headed now is somewhat absurd given the risks of inflicting lasting damage to the reputation of the field.

I'm very sceptical that the damage created by this messy public debate is less than if string theory "internally" collapsed. Funding and the general health of science relies on public trust – with every magasine article this trust is being eroded. One has to be damn certain before opening a public debate and washing dirty linen in public. I've yet to see any evidence that the approach you're advocating is any more constructive than my favoured option which is to wait several more years and see how the LHC influences the field before opening the can of worms.

Its very easy to start a public debate, its not so easy to control it. The end result of all of this may well be unsatisfactory for string and non-string enthusiasts alike.

Note, by the way, a lot of qualifications in my statements. I don't know what will happen. This is why I strike a note of caution. I'm not convinced that any of the posters here have any more prescience than me and the certainty and, at times, fanaticism with which both sides push their view is very worrying.

26. **DB**
October 5, 2007

Roger,

I did not write "there is no evidence that there experimental data will sort the wheat from the chaff". I wrote "There is no evidence that string theorists will take a blind bit of notice of any negative outcomes from the LHC." That is based on the assumption that they will continue to behave as they have to date when faced with inconvenient and unanticipated experimental outcomes, witness the use of anthropic arguments when faced with the lambda-CDM cosmology.
Proposing that public debate be closed down is just an argument for a restoration of the status quo pre Smolin and Woit. Well, that's not going to happen. Either the community resolves this internally or funding authorities will do it, with knock-on implications for the reputation of all physicists, especially in the HEP sector.

Once again, I point out that it is this blind faith in experiment as the arbiter of string theory's demise or success which is the Achilles heel of the community's response to date. It's cosy, convenient and a recipe for doing nothing. It ignores the fact that the LHC is incapable of falsifying string theory as things stand. So what if LHC doesn't find supersymmetry? Not to worry, check higher up. Oh you can't, well, build a bigger machine and in the meantime we'll do some more calculations.

When string theory eventually collapses, its practitioners can go back to doing what many of them are eminently qualified to do, making fine contributions to pure mathematics, as Witten does, or diversifying into other areas of mathematical physics. The physics community will be left holding the baby.

27. **Roger**
October 5, 2007

You know nothing about what will happen in the theoretical community during the LHC era and nor does anyone.

Tell me, why does the community need the public debate *now* and not, for example, in 4-5 years time when the data could already have led to other promising approaches and a greater diversity emerging ?

28. **Miso**
October 5, 2007

«And even an unintelligent uneducated member of the lay public can ask, "What's in it for me?"»

Please. The average joe is not qualified to judge the best clothes to wear on a rainy day and you want them to make these decisions about prospects that are anything but clear?

Some people's faith in humanity never ceases to amaze me.

29. **Thomas Larsson**
October 5, 2007

*why does the community need the public debate *now**

Woit's original evaluation, [physics/0102051]() appeared in 2001. Glashow's (Nobel 1979) and Ginsparg's attack on string theory, [Desperately seeking superstrings](), appeared in Physics Today in 1986. 2001 and 1986 are hardly "now".

What is happening now is that string theorists no longer have the power to suppress critique. A generation of physicists has spent 20 years on the receiving end of unchallenged string theory propaganda can no longer be ignored.

30. **DB**
October 5, 2007

I know that the LHC it will not be able to falsify string theory as things stand today, because no predictions are made that can eliminate the theory if they are not found. Phenomena may be discovered which are tough to reconcile with string theory, but that has never been an unsurmountable obstacle.

The HEP community needed this debate many years ago, and has crouched behind the bogus LHC "messiah" argument for long enough. It has painful lessons to learn.

Feynman blasted string theory publicly back in the mid-1980s, and was ignored. Twenty years on, do you believe we are better off?

31. **Peter Woit**
October 5, 2007

Roger (and others),

Thanks for the thought-provoking comments. I have been thinking about these issues as this debate has become a public one over the last year or two.

There are two answers to “why now?”. One is that it’s already way too late. Much of this debate could have and should have taken place 20 years ago. I got involved in 2000, after many years of frustration that the obvious issues were not being addressed, attempting unsuccessfully to get something published in Physics Today, addressed not to the public, but to the physics community. That was stopped, and the piece ended up being published in American Scientist, getting an audience of not just physicists. The book was written a couple years later, and an editor at Cambridge University Press was interested in publishing it. Again, string theorists put a stop to that, which just delayed the publication and forced me to go to a publisher much more in the business of marketing to a wide public. All of this was going on at a time when LHC results were still quite a few years in the future.

The other answer to “why now?” is that much of this is already over: the “dirty linen” has already made its public appearance, with the heaviest public attention to this taking place last fall when my book and Smolin’s appeared in the US around the same time. At this point, the cat is out of the bag, with a widespread perception that string theory is a subject in trouble. If string theorists want to respond to this by saying “yes, things are not going well, let’s see what the LHC says”, there’s not much to debate. If they decide to go to the press claiming that Smolin/Woit are all wrong, everything is fine, and keep publicly fighting a debate they have so far been losing, that’s their choice, but I think not a smart one.

As far as the effects of this on funding of particle physics, it’s not so simple. People calling up their elected representatives to demand defunding of particle theory is not the way the world works. The FY2008 proposed DOE and NSF budgets contain quite healthy increased funding for HEP, both experimental and theoretical. I can see why string theorists are not happy to lose the status with the public that years of hype had bought them and have to live with a more realistic one, but funding questions are in the hands not of the public, but of a much smaller and better-informed group. It undoubtedly is a tougher sell these days to use string theory to get private funding from philanthropists, but I can’t say I think that’s a bad thing. Much of the funding and health of particle theory is based on the decisions of other physicists, who are making the decisions about whether to hire particle theorists in their departments and advising the NSF about how to allocate resources. The main problem for particle theorists is this possible loss of support not from the ill-informed public, but from their much-better informed colleagues.

As far as this danger goes, the main problem is just the too-obvious lack of progress in the subject. When people see nothing new coming out of a field for a long time, they start thinking that resources are better allocated elsewhere. As far as the effect of my book and Smolin’s, I think they gave many physicists a more realistic awareness of what the state of HEP theory is, and what the level of string theory hype has been. This is uncomfortable for many theorists, but I don’t see the argument that it’s a bad thing. From talking to many people, I actually think the worst damage to the interests of string theory has come from the behavior of string theorists themselves. Lubos Motl, aided by significant support from other string theorists, personally did a huge amount of damage to the perception of the subject. Lenny Susskind and others promoting the anthropic principle (through books and articles for the public) have done even more. When I talk about the problems of string theory with physicists and mathematicians who don’t know much about the subject, they generally have a prejudice in favor of the idea that things can’t be really that bad, given how smart some string theorists are and how many people work on it. When they see Susskind or others going on about the anthropic principle, I often see a strong reaction of visceral disgust and a lot of sympathy for the idea that there really is a big problem here.

So, as far as the “public debate” goes, particle theorists should be worrying not about the public, but about their colleagues and specifically about the damage that promotion by prominent theorists of pseudo-science is causing. Allowing this to go unchallenged is a huge mistake. As far as the wider debate about string theory goes, I think it would have been a lot smarter for string theorists to react in public the way many of them react privately: acknowledging that the problems of string theory are very real, and that the subject is in a difficult state. Instead of going to the public with claims that Smolin/Woit don’t know what they’re talking about, everything is fine, they would do a lot better by acknowledging problems, and telling the public that this is how science is done: much if not most of the time ideas don’t work out as hoped.

My own worry about HEP theory funding is a different one. I see physicists drawing the conclusion that what went wrong here was too much emphasis on mathematics, that if theorists had just stuck to “phenomenology” and not gone off into mathematically sophisticated formal investigations then there wouldn’t have been a problem. The problem with string theory is not that it’s too mathematical, it’s that it’s a wrong physical idea about unification. What I see happening now is not a defunding of particle theory in general, but an abandonment of the kind of deep, long-term investigation of difficult issues that is needed, in favor of often outrageous attempts to claim “real world” applications of string theory. To some extent this is being justified by the idea that the LHC is going to save the day, that all theorists need to do is get up to speed on how to analyze collider data. Maybe this will work out, but I don’t think the way all eggs are being put in that basket is wise.

32. **amused**
October 5, 2007

David wrote: “when oh when, will you stop being so negative and grace us with your brilliant alternative so we can put that to a vote……”

Actually there are quite a few interesting alternative topics to ST in formal particle theory. Reasonable string theorists don’t seem to have any problem acknowledging this; Jacques Distler even listed some of them here. I suspect these alternatives would do quite well if put to a vote…

Regarding Roger’s points: I agree it is potentially damaging that the string theory punch-up has led to the public getting a negative impression of the state of physics in general. The solution imo should be to give the public a more accurate picture of present day physics research. Unfortunately they have been brainwashed into thinking that the only interesting problems are quantizing gravity and “unifying the forces”. Even the LHC is being partly sold to them as something that is supposed to give insights into these things. If the public realized that most of what goes on in physics (including much of HEP) takes place through conservative, uncontroversial, bottom-up approaches, and that these are generally doing fine and continue to produce important advances in physical knowledge, then I don’t think they will want to pull the plug on physics just because they hear about a relatively small segment of physicists getting bogged down in a metaphysical multiverse.

The seeds of the current public airing of dirty linen were sown a long time ago by string theorists who for some bizarre reason felt that the public absolutely had to know all about their speculative research program, extra dimensions and all, without it having a shred of experimental evidence or having reproduced the standard theories we already know are correct at low energies. If it hadn’t been for this the current airing of dirty linen probably wouldn’t be in the public domain (they wouldn’t know about it).

Finally, a question for Roger: What do you think about string theorists continuing to put out misleading press releases about “tests of string theory”? Is it really best to just ignore these for the time being, to avoid letting the public get a bad impression?

33. **Eric**
October 5, 2007

Dear Peter and others,
One question that I would like to pose to you is, suppose that the string landscape is true and there are really 10^500 possible vacua. Do you claim that of these 10^500 vacua that there is not one which corresponds to our universe? If so, how would you know? Unless you can prove that there is no such vacuum, then your claim that string theory cannot make any predictions is baseless.

Essentially, the purpose of the field of string phenomonelogy is to find this vacuum from a ‘bottom up’ approach. Now, we may or may not be able to explain why this particular vacuum is chosen, but that is a seperate question.

34. **Peter Woit**
October 5, 2007

Eric,

I've already gone on about what I think about this point far too many times, here and elsewhere. If "string phenomenologists" want to make the case for their subject by saying that they're going to keep at it and not give up until every one of the 10^500 vacua has been investigated and carefully examined to make sure it doesn't agree with experiment, they can do that, but this is just going to convince people that the criticisms of the subject are valid.

35. **Eric**
October 5, 2007

Well, Peter is no different that the state of particle physics in the 1950's when there was an elementary particle zoo rather than a string landscape. I suppose it's a good thing that the phenomenologist of the time 'kept at it'.

36. **Peter Woit**
October 5, 2007

Eric,

One minor difference: the "elementary particle zoo" was experimental data, the "string landscape" isn't.

37. **Eric**
October 5, 2007

Peter,
The point is that there was no fundamental theory which could explain the plethora of elementary particles. There was no Standard Model and it was far from clear that QFT was the correct description of elementary particle physics.

Regarding the string landscape, the data that we hope to have soon is the superpartner spectrum from which it is hoped that the pattern of soft masses may be deduced. Since the soft masses are calculable in string models, as opposed to the MSSM where they must be put in by hand, it may then be possible to zero in on a particular class of vacua.

38. **dan**
October 5, 2007

Eric,
what if LHC doesn't find any SUSY partners?

39. **Peter Orland**
October 5, 2007

Eric,

Your line of reasoning is very curious. You are trying to make an analogy between 1) complicated experimental data, and the need for a theory to simplify that data and 2) complicated possibilities from a theory, and picking out one of many possibilities with the aid of experiments.

These two situations are not analogous.

40. **cosmologist**
October 5, 2007

I'm no expert in string theory but I think the fact that no one has been able to get a string theory land scape based inflation model which produces gravity waves is a very important thing to keep in mind. Over the next few years cosmic microwave background polarization experiments should be able to detect gravity waves if the scale of inflation is not too low. If they do, my impression is that the landscape is going to be in big trouble. Another way cosmology could make life very difficult for the landscape is if the dark energy is shown to have a time varying equation of state. The landscape based solutions predict an equation of state equal to -1 (a cosmological constant).

41. **A.J.**
October 5, 2007

mo,

I'm not sure I agree about the time frame, or about the statement that GR and Yang-Mills theory are comparable projects. Trying to simultaneously unify particle physics and construct a theory of quantum gravity strikes me as vastly more difficult than either of these. I suspect that a reasonable time frame might be something more like a century or two. (This is a somewhat pessimistic take on Witten's comment that strings was a piece of 23rd-or-whatever century physics.)

How do you judge progress in a project on such a long time frame? I don't really know. The best answer I have is that you look for spin-off, good ideas that are applicable elsewhere. There's some precedent for this: many of the techniques used to study gauge theories were first developed in early attempts at quantum gravity.

42. **Peter Woit**
October 5, 2007

A.J.,

I think the way you evaluate any ambitious project is whether, as you learn more, you are moving towards your goal. The problem with the idea of unification via string theory is that since 1984, as people learn more and more about it, they find more and more problems with it. Instead of producing even some minimal explanation about the standard model, it has instead led to the reductio ad absurdum of the landscape.

I think one can argue for continued research on string theory based on hopes for spin-offs, but it's not a good idea to be pursuing a failed project purely based on such hopes. You're more likely to get spin-offs from trying something different than from continuing to work on something that has already failed.

43. **anon.**
October 5, 2007

> In the question session, asked about the CC, he said that there were lots of ideas about how to solve it, what was needed was just more people doing calculations.

Crackpot. Noun: crackpot 'krak`pót. … In physics: those who promote extravagant ideas before having accomplished the calculations necessary to check the validity of such ideas.

44. **anon.**
October 5, 2007

cosmologist: the Lyth bound suggests that measuring a large tensor-scalar ratio is problematic in field theory, not just in string theory…..

45. **Eric**
October 5, 2007

Peter O,
The situations are exactly analogous. Suppose it is 1959 and you have experimentally the elementary particle zoo and you have QFT. In this situation, would there not be a complicated set of possible QFT's which you could construct in order to try to explain the data? There is no priniciple inherent in QFT which would uniquely lead you to the Standard Model. Thus, what was done is to construct phenomenlogical models which then can lead us to the correct description.
With string theory, we are looking to experimental data in the form of information about the supersymmetry breaking soft terms which will guide us to the correct string theory description.

Dan,
If no supersymmetric partners are discovered at LHC, then it's still possible that string theory might be right. However, since in this case there is very little chance of ever making contact with experiment I would expect interest in the subject to dwindle. However, there must be some new physics at the TeV scale one way or the other. We shall see.

46. **Arun**
October 5, 2007

Eric wrote:

With string theory, we are looking to experimental data in the form of information about the supersymmetry breaking soft terms which will guide us to the correct string theory description

The situation with not analogous to 1959 because we don't know that supersymmetry is real. Your example is, to put it crudely, we are waiting to find out the size of an angel, so that we can figure out how many can dance on the head of a pin.

47. **Eric**
October 5, 2007

Well, Arun we find out if supersymmetry is realized in nature very soon.

I would like to remind you that the idea of quarks were introduced to simplify the particle zoo, however there was no evidence for these for many years just like in the case of supersymmetry. In fact, most physicists did not accept their existence since they had never been observed.

48. **David B.**
October 5, 2007

I am dismayed by the attitude that various commenters of this Blog have. They are suggesting that all research in string theory should stop. In particular, there is this idea permeating various posts that string theorists are being payed enormous ammounts of money to do fluffy landscape metaphysics with no connections to reality.

It is important to remember the following:

Models of light strings at the TeV, or 10 TeV scale have not been ruled out experimentally. They might be diisfavoured by certain theoretical prejudice, but they are not ruled out. We have to wait for the LHC data before we say that strings are not relevant for the TeV scale.

String theory is not just about unification of the standard model with gravity and predicting the couplings of the standard model. That is just one of the goals that the program of string theory as a whole is trying to pursue.

String theory is also (at the very least) a toy model of quantum gravity where many questions about the nature of black holes and geometry can be answered. This alone is sufficient justification to keep the program runing in the eyes of many scientists (not just string theorists).

Via the AdS/CFT correspondence, string theory ideas offer a unique insight into strongly coupled phenomena of gauge theories. Considering that QCD was proposed over 30 years ago and that it has not been solved (even with the most powerful computers and the efforts of thousands of people in the lattice QCD program) it seems ridiculous to toss away a formalism that gives you some analytic handle on strong coupling phenomena just because some other aspects of the string program are not satisfactory at the current moment. This is like tossing away the Ising model for being unrealistic. As a matter of fact, calculations using black holes in higher dimensions are still the best theoretical match for certain measurements that have been done in the RHIC experiment. There is a lot of cross-talk between experimentalists, string theorists and QCD experts at the moment, and it is centered on data.

To all of you with this intense anti-string prejudice: get back to reality and research the field and its current status before you start making pejorative statements just because you have a grudge.

49. **Cecil Kirksey**
October 5, 2007

David B.:
"String theory is also (at the very least) a toy model of quantum gravity ….". This I believe is a statement that no string theorist would support. What happen to the statement : "string theory is believed to be the only consistent theory of quantum gravity"?

50. **Coin**
October 5, 2007

Eric wrote:

> Well, Arun we find out if supersymmetry is realized in nature very soon.

Well, there's one I've been wondering about, actually. Should we really expect supersymmetry to imply that supersymmetry will be found at the LHC? I mean, we apparently don't see any supersymmetry under 1 TeV. Why must we see it under 15 TeV? Is there anything special about that particular factor of 15 that causes us to expect it to bring the superpartners into visibility range?

I've seen it expressed previously that if we don't see SUSY at the LHC, then probably SUSY is false. Is this fair, and is there any particular reason for this argument? What is the constraint that makes it unreasonable that the lightest superpartner would be massed at greater than 15 TeV? *Why* should we expect that we'll find out whether supersymmetry is realized in nature very soon?

Meanwhile, people seem to generally act as if they're expecting the Higgs to be found before any superpartner. Let's say this in fact happens. Should we then conclude that ALL the superpartners are heavier than the lightest Higgs? If it turns out that all superpartners are heavier than the lightest Higgs, does this tell us anything? Would this be viewed as unusual, predictable, mere coincidence?

51. **Eric**
October 5, 2007

Cecil,
We except supersymmetric partners of the TeV scale because this is required for SUSY to solve the gauge hierarchy problem. Since the Higgs is a fundamental scalar, it's mass should recieve corrections which diverge quadratically. Introducing a fermion which is the partner to the Higgs cancels these divergences and stabilizes the Higgs mass.

As far as your statement that SUSY hasn't been yet been seen up to the TeV scale, this is wrong because this energy has yet to be probed by any experiments. LHC will be able to probe this regime, and if LHC doesn't find SUSY then SUSY must not be the solution to the hierarchy problem.

52. **Moshe**
October 5, 2007

"This I believe is a statement that no string theorist would support…"

Pretty funny statement in context, I love the confidence…kind of puts in perspective the idea of trying to reason with those aforementioned commenters.

53. **Coin**
October 5, 2007

Eric, thanks for the clarification.

54. **Jock Distiller**
October 5, 2007

I'm a bit surprised to see PW still saying that the Landscape is pseudo-science. Jacques D explained why that is not so over in the "infinte thread" on CV:

"I've explained why I think it is likely that, when we manage to find a family of vacua which bear more than a passing resemblance to the Standard Model (so far, we haven't), they will have a sparse distribution of values for the parameters — both those which have already been measured (which can, if you want, be used to further prune the vacua which bear looking at) and, more importantly, of the parameters we have not yet measured.

Since we haven't yet found the family of vacua we are looking for, my argument can hardly be called ironclad*. But it is a good deal more persuasive than the mere assertion than "You can get anything you want on the Landscape."

In any case, if you're going to go around repeatedly making the latter assertion, then you have to explain why the above argument is wrong. "

Well, I have to admit that I don't like his attitude any more than you do, and I did have a good laugh at your jibes at his weird obsession with typesetting. Rising above all that, however, the man has made a point — not every flux on a CY manifold contributes to renormalizable SM couplings; in fact, generically, very few do. So it is just not true that "anything goes" except for a few things like the cosmological constant.

What is your response? I'm not trying to score points here, I just thought it was an interesting point that certainly bears further discussion.

55. **Arun**
October 5, 2007

I thought it was clear that string theory as THE theory of everything, the one and only game in town, the language in which God wrote the universe, etc., has so far gone nowhere; and is the one that is being derided; there are other much less grand, but more scientific, uses of string methods; by all means pursue them. And maybe one day, string theorists may even have the last laugh.

56. **Peter Woit**
October 5, 2007

Jock,

I've already responded to this multiple times elsewhere, explaining exactly what my argument about the landscape is. Jacques and others typically ignore my actual detailed argument, take one phrase out of it like "you can get anything you want" and insist on just arguing with that. I'm not claiming that the landscape can be disproved with 6 words, and I don't know whether zero, a finite number or an infinite number of supposed vacuum states of string theory will match to experimental accuracy all the standard model parameters. I am claiming that all hopes to get real predictions out of it are based on massive amounts of wishful thinking, and wishful thinking is not science. It's up to people like Jacques who claim the landscape is science to come up with the standard sort of scientific evidence for this, which is something more than "if we could just do certain computations which are now completely hopeless, the results would be certain very complex constraints on real world data which might be true". You can't just assert this, but have to come up with some evidence for it.

David B.,

If you read blogs regularly I'm surprised that you're "appalled" to find that some blog commenters are not extremely well-informed about some topics. Funny that you only seem concerned by ill-informed commenters who aren't happy with string theory. What about ill-informed commenters like Eric who are promoting string theory, do you think they're also a problem?

I notice you've completely ignored the content of my posting.

57. **Eric**
October 5, 2007

Dear Peter,
I think I know a thing or two more about both particle physics and string theory than you, so I think I would be careful about who you are calling ill-informed. So please go back to studying representation theory or whatever it is you pretend to study in order to create the impression that you're a scientist and not just some computer administrator.

58. **Peter Woit**
October 5, 2007

Eric,

And David and Moshe wonder why so many people have acquired a low opinion of string theorists…

59. **amused**
October 6, 2007

David B,

"I am dismayed by the attitude that various commenters of this Blog have. They are suggesting that all research in string theory should stop."

Well, that's not my suggestion. And I don't think it is Peter Woit's suggestion either.

"String theory is also (at the very least) a toy model of quantum gravity where many questions about the nature of black holes and geometry can be answered. This alone is sufficient justification to keep the program runing in the eyes of many scientists (not just string theorists)."

I agree completely. Not obviously inconsistent theories of quantum gravity are few and far between, so when one comes along then of course people should study it and learn all they can about it, within reason.

I have to take exception to your comments about AdS/CFT and QCD though. I think what irks many people about this is the way string theorists present it as a dramatic great breakthough that revolutionizes our understanding of QCD. That's simply not the case at the moment. Firstly, AdS/CFT gives at most an effective theory description for QCD in certain regimes. And as far as I'm aware it isn't even clear at present whether, and to what extent, this really is a valid effective theory for QCD. Secondly, and more importantly, the impact of this on QCD theory research as a whole is not very big. (One way to quantify this would be to count the number of PRL publications on QCD theory in recent years and see what fraction of them involve AdS/CFT.) Afaik it has made no impact on central topics such as understanding the QCD dynamics responsible for confinement, realistic calculations of hadronic mass spectra, and calculations of the various operator matrix elements relevant for studying the weak interactions and thereby testing the Standard Model. Contrast this, e.g., with the large impact that the theoretical breakthough in formulation of chirality on the lattice has made. It is of course very welcome that AdS/CFT is able to say something about regimes of QCD where lattice methods fail (e.g. at high density, due to the sign problem at nonzero chemical potential), assuming that the effective theory description it provides really is valid. But this hardly justifies the hype. There has been much progress in nonperturbative QCD theory in recent years. AdS/CFT is just one of a number of interesting developments. Despite the impression that you and other string theorists try to give, it does not tower above the others in it's significance and impact. If anything it is of lesser significance so far, especially considering that its status as an effective description of QCD is still up in the air.
Finally, since you emphasized that the AdS/CFT approach is analytic, let me just mention that the lattice approach can also be done analytically in certain limits, most notably the strong coupling limit where Wilson's original (analytic) derivation of the area law was carried out. Analytic studies of QCD in the lattice and other formulations continue to this day; you shouldn't imagine that the AdS/CFT approach is at all special in that regard.

60. **Geon**
October 6, 2007

I just can't wait until LHC turns on.

61. **Roger**
October 6, 2007

Amused –
Finally, a question for Roger: What do you think about string theorists continuing to put out misleading press releases about "tests of string theory"? Is it really best to just ignore these for the time being, to avoid letting the public get a bad impression?

Its a very difficult question to answer. In an ideal world the string theorists would desist from their extraordinary claims. However, it looks like the opposite is happening. Whether the right response *at this time* is for another group of scientists to stand up in public and say "they're talking nonsense, are fools, and have wasted money" – the message absorbed by the public – is the right one is something history will judge.

We shouldn't forget we're in unchartered territory here, both in terms of the physics under debate and the power of mass media. The only equivalent major spat over science which I can think of which has spilled into the public area is over global warming. I don't think for one moment that this issue will get anywhere near the level of hysteria and press-coverage as the greenhouse effect. However, it is a salutory warning of what can happen given a mix of genuine, concerned scientists, scientifically illiiterate journalists and crackpots with websites.

62. **Anon**
October 6, 2007

THE ONLY LOGICALLY POSSIBLE PROPOSALS I HAVE SEEN FOR LANDSCAPE PREDICTIONS ARE STATISTICAL ONES. I PERSONALLY DON'T BELIEVE THESE MAKE SENSE (YOU CAN NEVER KNOW THE MEASURE).

–PW

63. **Peter Orland**
October 6, 2007

David B.,

I have not now, nor have I ever suggested work in string
theory should stop (I'd conceivably be working on it myself, were
I not occupied with other matters). Nor has Peter W., I think.
Nor did some of the other commenters on this thread.

Eric,

I am looking at your response to my comment from yesterday
and some of your further remarks. I am still not persuaded
(if anything, even less so) that your analogy is right.

As I understand it, you are trying to make the following analogies:
1. the space of field theories is analogous to the landscape.
2. the standard model is analogous to one string theory solution.

These are not analogous. Nobody claimed in the sixties that
the SPACE of ALL field theories was something emerging from fundamental theory of nature. The Holy Grail (except to the
large community of S-matrix advocates) was ONE correct field
theory. People searched for one theory which would explain the
data. They did not search for a solution of what they wished
would be the correct theory consistent with data. There is a
difference between a scientific theory and a particular solution.
They aren't the same – think about it.

On another matter, trying to pull rank on Peter W., by saying you
know more than he does, undermines anything valid you might
have to say.

64. **Peter Woit**
October 6, 2007

Anon,

If you want to discuss what I actually think you need to respond to what I write here. The tactic of ignoring what I write in favor of pulling out of context phrases from some other discussion is a tedious one.

65. **a.k.**
October 6, 2007

..since some commenters demanded to go 'back to reality': not anybody in this blog seemingly denied the mere *possibility* the landscape could bare at least a family of solutions which could match a given set of known parameters of the 'real world', the subtle point is exactly the interpretation whether the situation is analogous and is being transported to be analogous to the situation of QFT in '1959', I would agree to Eric if the landscape wouldn't reach the measure of confidence with which it seems to be correlated: a parameterized model made to be constrained by experimental data which could eventually fail.

So, as someone whose physical interests remain that of a mathematical observer: why *do* physicists, i.e. Suesskind et al. claim, a set of mathematically 'valid' models could represent a set of 'possible universes', which mathematical concepts, as it is obvious not to rely on experimental data, force this interpretation? Is it possible to *prove* strong belief in the possible validity of a constrained set of models without any sufficient constraint data? Did someone, and Lubos at least seemingly could not deny this possibilty, exclude the perspective that for *any* given energy scale there will be, if at all, a family of models which matches the given data, but can only be distinguished beyond the given scale, implying any predictions beyond a given, experimentally observed, scale would be impossible, as long it wouldn't be the Planck scale?

66. **Peter Orland**
October 6, 2007

a.k.

There are a lot of possibilities which I can't deny out of hand. I'd
like evidence, however, or at least a solid motivation for a possibility before seriously considering it. That's the problem with the landscape
– there is nothing simple or compelling about it. So why launch a
research program to attempt to parametrize the world with it? After
all, there are plenty of other ways to parametrize the world (say, by using the particle data table).

67. **LDM**
October 6, 2007

I wasn't suggesting that the results of a vote like this have anything to do with the validity of a theory. But the fact that string theorists have been losing the public debate does explain some of the tactics they are now adopting in this debate. You don't hear any more the argument for string theory that "it has triumphed in the market of ideas". – PW

The latest issue of Physics Today has a free, excellent letter ("American physics implosion"), discussing NEW and TTWP, in which this notion of "market of ideas" is refuted and points out a disturbing trend in American physics. For anybody interested, the letter URL is:

http://ptonline.aip.org/journals/doc/PHTOAD-ft/vol_60/iss_10/16_2.shtml

Nature does not shop at our market place of ideas…she is the way she is — and only reveals herself through honest inquiry and experiment, rather than hype.

68. **Quixotik**
October 6, 2007

So please go back to studying representation theory or whatever it is you pretend to study in order to create the impression that you're a scientist and not just some computer administrator.

Oh yeah? Yeah?…Well…you're…you're just a DUM-DUM HEAD! So THERE!! :P:P:P

69. **Jon**
October 6, 2007

Don't be so rude!

Wikipedia

70. **David B.**

October 6, 2007

Dear amused (and others)

I'm not saying that lattice QCD has not solved many problems in the theory of the strong interactions. I'm actually very supportive of using whatever it takes to solve a theory of nature.

Even if with lattice one can see the area law, there are various things that are beyond computation because we do not have infinite resources. For example, getting the Regge behavior observed in nature and the unstable resonances in the meson spectra is beyond what can be done
on the lattice. If there has been a recent development that solves this problem I will be very glad to see it.

Regarding confinement, in toy models based on strings this can be understood as a geometric transition (topology change). This gives you an easily identifiable order parameter in the AdS realization. A lot of the papers where this was understood have about 500 citations and I would claim that that is a successful set of ideas.

You should also read the recent papers by Alday and Maldacena on scattering of gluons in conformal field theories. This is a very interesting new development.

My point of not tossing the Ising model for being unrealistic is exactly that: there are many situations where string models provide a lot of insight into strong coupling phenomena, even if not directly on QCD yet. One has to keep things in perspective.

I also don't see what the number of PRL's has to do with success in high energy theory. If you present a very long computation, it is hard to reduce it to four pages. I've gotten a few papers published in PRL, and it is usually a lot more work than what it is worth (I've had situations where the paper goes back and forth with the editors and reviewers five times and the whole process took over 8 months). If instead your result is a number with an error bar, that is a lot easier to package in the required format.

Regarding my coment of strings as a toy model of quantum gravity, I subscribe to that point of view and I am considered a string theorist.

We (as a string community) do not all agree on the various merits of various phenomenological approaches to the standard model. I am less optimistic than some others regarding predictivity, but I'm not going to tell people who are trying very hard to match with known experimental data to stop working on what they are doing because I'm prejudiced against some set of ideas.

I hope this has clarified my (very personal) point of view.

71. **Anon**
October 6, 2007

"If you want to discuss what I actually think you need to respond to what I write here. The tactic of ignoring what I write in favor of pulling out of context phrases from some other discussion is a tedious one."

So we're not allowed to examine individual propositions in your argument, and decide whether they are true or false? Or to see whether one statement follows logically from the others?

72. **woit**
October 6, 2007

Anon,

You can do whatever you want. But if you just take something I wrote on another blog out of context, try and make some point here with it, ignoring the fact that I already responded to that same point already on that blog, and ignore what I actually write here in response to you, it's really hard for me to believe that what you're interested in is a serious discussion. Somehow I get the idea that any time I decide to spend responding to you is just wasted. The fact that you are doing this under multiple anonymous pseudonyms, some juvenile, means that I have no idea who you are, why you are doing this, or any indication of seriousness on your part.

73. **Peter Woit**
October 6, 2007

LDM,

Thanks. The same issue also has

http://ptonline.aip.org/journals/doc/PHTOAD-ft/vol_60/iss_10/12_1.shtml

which features anectdotal evidence about what is happening to students doing theoretical phds these days.

74. **amused**
October 6, 2007

Dear David B,

I'm certainly not suggesting that AdS/CFT isn't interesting and should be tossed, and I agree that it would be akin to tossing the Ising model. My beef is with with the impression that you and other string theorists seem to be trying to create that AdS/CFT towers above other QCD theory developments in its significance and impact.

I'm aware that it provides an interesting model with a deconfining transition that might be related or similar to the one in finite temperature QCD. But does it have anything to say about the specific confinement mechanism in QCD and features of the QCD vacuum that give rise to it? E.g., does it give any insight into whether the picture of confinement based on dual Meissner effect is correct? Or why the deconfinement and chiral symmetry restoration transitions occur at more or less the same critical temperature? My impression, maybe wrong, is that AdS/CFT doesn't have much at all to say about these central issues in QCD theory.

"You should also read the recent papers by Alday and Maldacena on scattering of gluons in conformal field theories. This is a very interesting new development."

Ok, I'll try to find time to take a look. And while I'm doing that you should read Lellouch and Luescher's paper hep-lat/0003023 on weak transition matrix elements from finite volume correlation functions, which was also a very interesting development of continuing importance as well as a technical tour de force. And if you have some more spare time you should think about how to develop a Hamiltonian formulation for overlap/Ginsparg-Wilson lattice fermions, resolve the apparent difficulty with CP symmetry (see hep-lat/0501010), and prove Osterwalder-Schrader positivity for this formulation so as to put it on a secure theoretical foundation. These are very important issues that deserve your attention.

"A lot of the papers where this was understood have about 500 citations and I would claim that that is a successful set of ideas..[…]..I also don't see what the number of PRL's has to do with success in high energy theory…."

It’s kind of amusing that our views on the significance of citations vs PRL publications are diametrically opposite… But I think your insinuation that it’s easier to publish papers whose “result is a number with an error bar” is no less silly than the view of people who say that the real reason string theorists reject PRL publication as a measure of value is because if they accepted it then it would be possible to measure the value of ST work against the value of work on other topics and that comparison would lead to a conclusion that was uncomfortable for string theorists’ egos. At any rate, my experience and impression of PRL is quite different from yours. My papers in there are all on formal theory topics; the results were not “numbers”, in fact none of the papers contained a single physical unit. Moreover, the results in several of the papers were based on lengthy calculations that would have exceeded the PRL page limit n times over if written out in full. But as with all letter publications, as I’m sure you know, the thing to do in these situations is simply sketch the main steps in the calculations and state and explain the result. (And there is nothing unique about my PRL experiences; it’s been the same in quite a few other cases that I know of.) I can’t believe there should be any special difficulty for string theorists in this, especially considering the apparent ease which they summarize their work in press releases and press articles for the general public.

75. **David B.**
October 6, 2007

Dear amused:

Unfortunately I don’t know your name. You can on the other hand probably figure out who I am rather easily as I don’t hide behind a pseudonim.

When I have an idea that I think fits in the PRL format I make an effort to publish there. Also, when I spoke about numbers with error bars, it was not meant in any derogatory sense. I know very well how hard it is to produce a number with an error bar, as I have been recently doing exactly that and it has taken me over eight months of work to learn how to do it properly. I’m stating that it is a lot easier to put that type of calculation in a PRL format than other theoretical speculations. Don’t take it personally.

I feel that you are extrapolating assumptions about my personna and stereotyping me without bothering to know me or my scientific output.

I have never said that the string community has a compete handle on QCD, nor have I ever stated that the other developments in QCD are not important. I was trying to put the work of some segment of our community in perspective. It is very hard to get the point across that many of us actually care very deeply about real physical phenomena, to then be accused of making claims of the type “my field is better than yours”.

In fact, I am very glad for all the effort that has gone into the lattice QCD program and I think that some of the results in that field are outstanding. I’m also very glad that you pointed me to the work of Lellouch and Luscher on Weak transition matrix elements. This still does not explan the Regge behavior nor the strong decay matrix elements of unstable resonances which is the point I was trying to make. I hope this clears this apparent confusion.

Now, regarding citations versus publishing in PRL, that is a question of objective criteria to determine the quality of a paper and will certainly satisfy various bureaucratic bean-counters.

What counts as having made more impact on physics? A paper with 500 citations (which is a huger number by any measure), or managing to put a paper in PRL format?

I think you are over-reading the tea-leaves regarding my opinions in this matter.

I certainly hope this has clarified things a bit.

76. **amused**
October 7, 2007

Dear David B,

I’m sorry if you are bothered by my anonymity but there are good reasons why someone might prefer to remain anonymous in blog discussions…I have no idea who you are either.

Regarding publishing in PRL, from what I’ve seen the main consideration that people make in deciding whether to aim for it is whether or not their results satisfy the interest and importance criteria. That’s where the real challenge lies. I don’t think it’s at all difficult to put a more formal theory paper into PRL format (it can be a pain, but I wouldn’t say it’s hard).

“It is very hard to get the point across that many of us actually care very deeply about real physical phenomena, to then be accused of making claims of the type “my field is better than yours”.”

I don’t doubt your sincerity, but the way string theorists present the potential application of AdS/CFT to QCD often comes across as aggrandizement and misleading advertising (e.g. the lack of caveat about whether it really gives a valid effective theory for QCD in some regimes, which isn’t known yet). Maybe there’s a clash of cultures here.

Regarding citations, having a huge number of them is no doubt a sign of good work, as is a large number of papers in PRL. But beyond that I can’t really take it seriously as a measure of anything. It seems that papers that catch a wave of research activity at an early point can ride it to become highly cited without necessarily having made a major contribution. This happened (on a lesser scale) for one of my papers once, through dumb luck. There was nothing particularly great about it, and others which I consider to be much better have hardly any cites.

Anyway, thanks for taking the time for this discussion, hopefully we understand each others viewpoints better as a result.

77. **chris**
October 8, 2007

Eric,

let us assume angels do really exist. then, how do you calculate how many of them will have room to stand on a needle’s tip? 🙂

78. **chris**
October 8, 2007

dear david b.,

i would like to point out that a) there is no effective string description of QCD yet and b) that the sheer viscosity is a quantity that every model – no matter how crude – seems to get correct. i see it more of an interesting question as to why this quantity is so insensitive to the underlying theory or approximation methods than anything else. and by the way, there are accurate lattice predictions of this quantity as well.

regarding the lattice QCD results i am willing to make the following bet: we will have the complete meson and baryon specrum calculated with 3% precision before you will even have a dual string description of QCD.

oh, and of course these papers will hardly be topcite 500. our community is just that much smaller. but we have learned to live with this renormalization factor. it is also not too bad when you think about it. today a paper that does a rough order of magnitude estimation in a toy model does get 5 times more citations than the actual ab initio calculation. but in 50 years, i wonder which of these 2 numbers will end up in a textbook about qcd.

79. **JDR**
October 10, 2007

One thing I find troubling is the suggestion that “a computer programmer or an analytical chemist or a forklift operator” can not do or think about theoretical physics (or anything else for that matter). One does not need an advanced degree (or even a degree) to be able to pick up books, read information, work out the math, draw their own conclusions, and maybe provide some insights into a topic. Lets not forget the gentleman (and gentlewomen) scientists of an earlier era who did important work.

Deep Beauty

October 6, 2007
Categories: Uncategorized

I spent Thursday down in Princeton attending talks at the second day of a symposium on mathematics, quantum mechanics and the legacy of John von Neumann, organized by Hans Halvorson of the Department of Philosophy. A blurb about the symposium is here, and a list of talks is here. There were quite a few interesting people there that I enjoyed having the opportunity to talk to, including John Baez, who gave the keynote presentation (available on his web-site). Most of the talks were pretty far from my own interests (an exception would be that of Stephen Summers, on the vacuum state in algebraic quantum field theory), but it was interesting to see what sorts of things people interested in quantum mechanics, mathematics and philosophy are up to. At the end of the day I joined a group of people on a trip to visit von Neumann's grave, which was nearby.

One of the topics that some people at the symposium are working on is that of reformulating quantum mechanics using topos theory, an idea promoted by Chris Isham. For more about this, see an article here from the FQXI web-site. I have to say that, like 't Hooft and Dijkgraaf who are quoted in the article, I'm skeptical about this kind of thing, since topos theory is such a general formalism that I don't see how it is going to provide the sort of non-trivial new idea that people are looking for. But, you never know, something unexpected may come out of it. The article also describes Isham's "somewhat mystical view of reality" and the fact that he likes to "take part in interesting meetings on the twilight zone between physics and religion." At one earlier this year about "God and Physics", he speculated that "a logic of partial truth might be useful in comprehending the Trinity."

As you might have guessed, the Templeton Foundation is deeply involved in funding all of this, from the "God and Physics" meeting, to an FQXI grant for Isham, to the symposium on von Neumann itself. Besides the event I was at, yesterday and today they're also sponsoring two other events at Princeton: a panel discussion on Budapest: The Golden Years, Early Twentieth Century Mathematics Education in Budapest and Lessons for Today and a program called Living in von Neumann's World: Scientific Creativity, Technological Advancement, and Civilization's Accelerating Dilemma of Power.

At lunch I got to meet and chat a bit with Chuck Harper, who is in charge of much of Templeton's grant-making in the scientific area. The mechanics of the symposium were very ably organized by him and others, and they were all quite friendly to me. Either they're pretty oblivious and unaware of my vocal criticism of Templeton's activities, or just extremely gracious. I'm guessing the latter.

Templeton wasn't funding my day-trip down to Princeton, but they were paying for the dinner I consumed that evening in some very enjoyable company. Among other topics our dinner conversation included a long discussion about our hosting organization and what significance its activities and funding have for the sciences. Some people are concerned about involvement with an organization led by someone (John Templeton Jr.) known for his evangelical Christianity and devotion to funding right-wing political organizations (this article in the New York Times mentioning Templeton's involvement in "Freedom Watch", a new group that has done things like run ads suggesting Iraq was responsible for 9/11). As far as I can tell, the Templeton Foundation is careful to keep the right-wing politics out of its activities. However, they unambiguously are devoted to trying to bring science and religion together, and that's my main problem with them. Their encouragement of religion seems to be of a very ecumenical nature, not pushing especially the evangelical Christianity of Templeton Jr. Still, more influence from a religious world-view seems to me to be the last thing that physics in particular needs right now, especially with the on-going challenge to the scientific method represented by the anthropic landscape, a topic that Templeton has strongly encouraged work on through funding various conferences and other activities.

Others pointed out to me correctly that Templeton wasn't solely to blame for the anthropic landscape, that the real problem was its popularity at the top level of the physics establishment, leading to funding and influence mainly from other sources. The symposium I attended had not a trace of involvement of religion in it, and it seems that Templeton is careful to keep this out of some of the things that it funds as pure science, with another good example being the FQXI organization. They appear to have a serious commitment to the idea of funding things in physics that can be considered "foundational". People working in some such areas often are considered out of the physics mainstream and so find it hard to get their research funded. For them, Templeton is in many ways a uniquely promising funding source.

So, it was an interesting day, I'm glad I went, and so have to thank the Templeton people (and Halvorson) for the work they did in organizing the event. I remain concerned though about the significance for physics of this large new source of funding, out of scale with other such private sources, and with an agenda that seems to me to have a dangerous component to it.

Update: John Baez writes about the symposium here, including (courtesy of Jamie Vicary) a picture of a bunch of us standing behind von Neumann's grave trying to look suitably solemn.

Update: Thanks to many people for interesting comments, I especially recommend reading the one from Klaas Landsman here. Klaas both explains some of the motivations of recent work on topos theory and physics, and has interesting comments on the issue of Templeton funding. He notes that even a proposal by 't Hooft for funding foundational research on QM was rejected by conventional sources, making clear that the less conventional Templeton source of funding is one of the few alternatives open to people in this field.

Comments

1. **Matt**
   October 6, 2007

   I think one has to be careful to distinguish between the aims of a funding organization and what they require from the scientists that they fund. Historically, patrons of science have not always been bastions of morality, but have been responsible for funding some very good scientists. Today, a large part of science funding in the US comes from the defense department and it would be almost impossible in my field (quantum information) to conduct normal scientific activities without receiving some funding from them. They sponsor almost all the major conferences for example. I would think that this should be a greater concern than a few misguided religious people, but so long as they don't require me to unconditionally support all US military action, or to work on weapons research, it doesn't bother me too much.

   I have a similar attitude towards organizations like Templeton. Provided I am free to declare my atheism, and they are not going to use my work to promote religion, I would accept funding from them. The minute this changes I would cease from doing so. Of course, a certain amount of credibility is given to the whole organization by the fact that scientists are accepting their money, but shouldn't this be an even bigger concern with military funding? Isn't there a double standard involved in singling out Templeton for criticism?

2. **Peter Woit**
   October 6, 2007

   Matt,

   The fact that all money comes with some agenda is certainly true, and I recall Lee Smolin making the same point about military/government money. It's not even clear how much of a distinction one should make between DOD and DOE money, they both are coming from the same source, just different agencies.

   But, if the military started significantly funding particle theory, emphasizing one part of the subject that I thought was troublesome because they thought it might lead to better ways of killing people, I'd be criticizing them too…

3. **nork**
October 6, 2007

Is the Templeton physics funding that significant? I would have assumed it's a small drop compared to other sources.

4. **Peter Woit**
October 6, 2007

nork,

I think the initial round of FQXI grants totaled $2 million. To get some sense of scale, the DOE budget for particle theory at universities is around $20 million, NSF $10 million. So, it's not a huge fraction, but it's not a small drop.

Keep in mind that most government grants are continuing grants going to the same groups all the time. The amount of "new" money that someone who doesn't have a grant but wants to try and get one can compete for is much smaller than the $30 million total. The FQXI money was all new money.

I understand that Templeton has annually been funding projects totaling $60 million, and recently doubled its endowment and is planning on perhaps doubling their spending. I don't know how much of this they intend to devote to pure science or science/religion. Traditionally they don't make grants to experimental groups that require expensive equipment, so their money is going to theorists, whose other funding levels are much lower.

My impression is that in the area of academics working somehow on an overlap of science and religion, the amount of funding in this area available from Templeton completely overwhelms any other sources.

5. **Kea**
October 6, 2007

…since topos theory is such a general formalism…

If *general* applies to the level of abstraction, perhaps, but the theory is a very well developed branch of mathematics, computer science and logic, and quite capable of providing new computational tools.

6. **Yatima**
October 6, 2007

Topos theory in computer science? Really? Haven't heard of it yet, and I am generally keeping my ears open. I will hit The Googles but would you have any references straight away?

7. **Yatima**
October 6, 2007

And, speaking of the John Templeton Foundation ("Supporting Science – Investing in the Big Questions"), I open the print edition of The Economist and – what do you know – find a two page advertisement of said Foundation, with the title "Does the Universe have a Purpose?". Seven personalities are holding forth on the question: Lawrence M. Krauss, ("Unlikely"), David Gelernter ("Yes"), Neil deGrasse Tyson ("Not Sure"), Owen Gingerich ("Yes"), Jane Goodall ("Certainly"), Christian de Duve ("No"), Elie Wiesel ("I Hope So"). The advertisement rates are not publicized on The Economist website, but this can't be cheap.

8. **Chris Oakley**
October 6, 2007

Peter,

All this fraternising with the enemy. I would not now be surprised to hear about you getting drunk with Lubos at the Pilsen brewery in Czechia, or going on a cattle drive with Jacques Distler in Texas.

9. **Peter Shor**
October 7, 2007

It has always appeared to me that some of the people promoting the anthropic principle are in some sense trying to remove the last vestige of possible divine influence in physics. That is, they are trying to deny God His last remaining privilege; that of, e.g., setting the parameters in the Standard Model.

10. **Peter Woit**
October 7, 2007

Yatima,

Same two page spread in today's New York Times, also not cheap. Available online here

http://www.templeton.org/questions/purpose/

11. **Alex Rivero**
October 7, 2007

The two definite "no" in the spread come not from physicists but from chemists. Interesting.

12. **Andreas Doering**
October 7, 2007

Peter,

what you forget to tell your readers concerning topos theory and physics is the fact that you did not hear the two talks on this subject (by Klaas Landsman and me, where I was reporting on my work with Chris Isham), since they took place on the first day of the meeting when you were not there. Both Klaas and I included a lot of conceptual discussion besides the technical aspects, since this meeting seemed right for that. There also was some interesting discussion following the talks.
Listening to the talks would have provided a minimal basis for an assessment of the topoi-and-physics ideas, reading the papers would obviously have been better. The

FQXi article you give as a reference is completely non-technical and surely not a source for a serious discussion of these subjects. Moreover, the one day you were at the meeting, you made no attempt to talk to me about topos theory or anything related. (Maybe you discussed with Klaas or Chris Heunen, his PhD student?)

As you say yourself in your post: "The symposium I attended had not a trace of involvement of religion in it, and it seems that Templeton is careful to keep this out of some of the things that it funds as pure science, with another good example being the FQXI organization." To spell out the obvious: Chris and I welcome any serious discussion of our work – and criticism, of course! If you do not want to engage in that, please just let it be; I am not advertising here. But mixing an uninformed criticism of technical work that has absolutely nothing to do with religion with whatever beef you may have with the Templeton Foundation is deeply unfair. The remarks on Chris's activities beside his scientific work come very close to a personal attack, since these remarks are obviously intended to put him in a somewhat dubious light. Chris was not even at the meeting, and our work has nothing to do with religion whatsoever, so what is the point here?

I would think that this kind of rhetoric is something with which you have had a lot of unpleasant experience yourself, so I am surprised to see you employ this. I still hope that we may have a constructive discussion of physics some time.

Sincerely,

Andreas

13. **rob**
October 7, 2007

Andreas,

I think you're overreacting a bit here. I'm a relativist, and Isham has been a personal hero of mine for well over a decade, despite his apparent religious proclivities. But I really don't see how anything Peter said could be read as "very close to a personal attack." In fact, I read the post as a statement of puzzlement over the fact that the Templeton foundation, an organization that most physicists seem to distrust, is nonetheless funding (in part) real science by great scientists.

You may be annoyed that Peter expressed skepticism over your program of research, but surely you're used to that by now. I'm a huge fan of the idea of applying Topos theory to QM, and I'd love to see someone evangelizing it on a blog, but this is Peter's blog, and it's not his job to evangelize your work, especially if he doesn't happen to believe in it.

14. **Steven H. Cullinane**
October 7, 2007

Rhetorical question: What *is* "deep beauty"? See also a rhetorical answer.

15. **Peter Woit**
October 7, 2007

Andreas,

Unfortunately I wasn't able to come to the first day of the symposium since I was teaching that day. I would have liked to have heard your talk, as well as several of the others from that day, and undoubtedly would have learned something from the discussion there. Klaas sent me his paper before the conference, I did read that, and took a look at yours. But I'm certainly not well-informed on the subject of work on topos theory and physics, and I don't think I made any pretense of being so, or even capable of giving a serious explanation of the subject, with links to appropriate references.

I do know something about what a topos is, having spent a fair amount of time trying to understand and assimilate the Grothendieckian point of view on topology and algebraic geometry in which the concept originally arose. But I don't know much about the kind of attempts to reformulate physics using this that you are involved in. I don't think that it's controversial to characterize such a program as highly speculative, or inappropriate to mention that, despite not being well-informed about the program, I'm skeptical that it will work (while acknowledging that my skepticism could turn out to be misplaced). Everyone thinking about physics and mathematics in a fundamental way has their own prejudices about which directions are most promising to think about. I've got mine and they seem to me rather different than those of people working on toposes in physics. Quite possibly I'm wrong about mine, quite possibly we're both wrong. All ideas out there at the moment about new ways to understand fundamental physics are highly speculative, without much evidence to back them up.

I certainly in no sense intended what I wrote as any sort of attack on Chris Isham. The main reason I linked to the FQXI article was that, while mentioning the skepticism of 't Hooft and Dijkgraaf, it seemed to me a sympathetic description of him and what he is trying to do. I don't know Isham personally and know very little about his recent work, but earlier in my career I spent a fair amount of time reading articles of his, especially about geometry and quantization, and as a result have a great deal of respect for him.

The other reason that I linked to the FQXI article is that it mentions Isham's attitude towards the science/religion issue, which is very different than mine. You couldn't pay me enough to sit through a conference of talks on science and religion, while Isham says he enjoys this kind of thing. To each his own. It sounds to me like his point of view is a lot closer to that of the people running Templeton than mine, so he is a good example of what they would like to fund. As I hoped I made clear, my views about what Templeton is funding are mixed. The science/religion stuff I don't think is very worthwhile, but it's harmless if kept completely separate from real science. That Templeton is, through FQXI and things like the Princeton symposium, funding serious work about foundational issues in physics (including that of Isham on toposes and physics) is great, but the fact that their agenda includes a very different aspect seems to me very much worth keeping in mind.

16. **mo**
October 7, 2007

Andreas Doering Says:

"Listening to the talks would have provided a minimal basis for an assessment of the topoi-and-physics ideas, reading the papers would obviously have been better."

I read with sympathetic eye most of the papers relating to categories/topoi and physics as I think it is a great idea and am curious to know what may come out of it. So far the progress was meagre. I believe this whole line of thought has been initiated by F. Lawvere who tried to apply topos theory to dynamics, chaos, entropy, continuum microphysics, and even engineering (unfortunately his lecture notes on algebraic foundations of physics and engineering are not published yet). As it turned out the most serious obstacle on this path is how to introduce time and evolution into a static framework of the category/topos theory. Lawvere tried to do it but I don't feel he was successful.

Isham and his coworkers laid the foundations of "topoical physics" (let me call it this way), but it was never clear to me how it would work in real life, whether it categories in general or topoi in particular. And then Jamie Vicary (a rising star of categorical physics?) posted his groundbreaking article on arXiv:

A categorical framework for the quantum harmonic oscillator
http://arxiv.org/abs/0706.0711v2

At last we have a simple, but real-life quantum system for which categorical computations have been pushed through to some kind of end and we can perform a comparative evaluation. I really appreciate Jamie's yeoman's work, but one immediately notices that titanic effort and much space (44 pages) did not accomplish a lot–Schroedinger and Heisenberg did it better. Jamie himself (or herself?) concludes on p. 41:

"However, there are many issues which are still unclear. Philosophically, perhaps the biggest problem with the existing framework for categorical quantum mechanics is the lack of any nontrivial categorical description of dynamics. For this reason, it is questionable whether the system under study in this paper deserves to be called the harmonic oscillator at all: without a description of dynamics, all that has really been defined is the state space,
but many different systems have isomorphic state spaces."

That damn problem of time again!

Obviously, categorical physics isn't yet ready for prime time.

I skip our neighbors from n-Category Cafe (J. Baez et al.) to Louis Crane who, in my opinion, is the only person doing research in categorical physics that is well balanced between physics and mathematics. His most recent survey paper (which is heavy on topoi) is available here:

Categorical Geometry and the Mathematical Foundations of Quantum General Relativity
http://arxiv.org/abs/gr-qc/0602120

And by the way, to my knowledge, LC is the founding father of categorical physics:

Categorical Physics
Authors: Louis Crane
http://arxiv.org/abs/hep-th/9301061

17. **Coin**
October 7, 2007

Kea, if you don't mind me asking, I also would be curious what the connection you allude to is between topos theory and computer science. Or do you just mean that category theory in general is of relevance to computer science?

18. **Amitabha**
October 8, 2007

Not the right thread, and perhaps too close to the event, but would you or anyone else make any prediction about tomorrow's Nobel Prize?

19. **Urs Schreiber**
October 8, 2007

By the way, we had quite a bit of discussion of Doerin-Isham's work on topos-theoretic quantum theory on the n-Category Cafe, in case anyone is interested.

It starts here

http://golem.ph.utexas.edu/category/2007/03/a_topos_foundation_for_theorie.html

and here

http://golem.ph.utexas.edu/category/2007/04/topos_theory_in_the_new_scient.html .

A transscript of a talk by Andreas Doering is here:

http://golem.ph.utexas.edu/category/2007/07/physical_systems_as_topoi.html
http://golem.ph.utexas.edu/category/2007/07/physical_systems_as_topoi_part.html
http://golem.ph.utexas.edu/category/2007/07/physical_systems_as_topoi_part_1.html

20. **John Sidles**
October 8, 2007

Many disciplines claim von Neumann as one of their own; this includes atomic-resolution microscopists, as foreseen by von Neumann in this little-known 1946 letter to Norbert Wiener.

21. **John Baez**
October 8, 2007

Just so everyone knows, religion wasn't on the agenda at the *Deep Beauty* conference. As far as I can tell, none of the speakers is even very interested in the relation of religion and physics. Freeman Dyson is, but he merely sat in on the last talk of the first day, and came to dinner afterwards. I would be very uneasy attending a physics conference if I felt it was pushing religion somehow, but I didn't get that feeling.

I suspect that while the Templeton Foundation is interested in promoting religion, garnering the good will of academia is too important to their plans for them to deliberately do anything that stirs up lots of hostility. Look how they dropped the Discovery Institute like a hot potato after that controversy erupted.

By the way: I hope to say a bit about the recent work on topos theory and physics in This Week's Finds, pretty soon.

22. **Jamie Vicary**
October 9, 2007

If Chris Isham ever said "a logic of partial truth might be useful in comprehending the Trinity", then it was certainly intended as a joke! 🙂

It was a real pleasure to be at the conference and hear all of these ideas from their creators' mouths. Something that might not be clear to an outsider reading this thread, though, is that the 'topos quantum mechanics' approach (D\"oring, Heunen, Isham, Landsman, Spitters) is currently quite different to the 'quantum mechanics in symmetric monoidal dagger-categories' approach (Abramsky, Baez, Coecke, Duncan, Morton, Paquette, Pavlovic, Selinger, Vicary). The latter looks to the category of Hilbert spaces for inspiration, and tries to axiomatise its important properties. The former provides a more flexible context in which to describe theories of physics, which then come along with a tailor-made realist interpretation. This works, in particular, for quantum physics, which famously lacks a realist interpretation when formulated in a more conventional way. So, although both approaches are 'categorical quantum mechanics', and people give talks about them at the same conferences, they're really quite different in both style and short-term goals. (Although, of course, one hopes that in the medium term they'll prove complementary, on the road to a long-term goal of quantum gravity!)

The realist interpretation of quantum physics that emerges from the topos-theoretic approach is often not mentioned in high-level descriptions of the research, which seems a shame to me. This is a hugely important part of its philosophy and motivation. Perhaps this angle might make it more attractive to you, Peter?

23. **Benni**
October 9, 2007

the project sponsored by tempelton that scares me mostly is this here:
http://www.quantum.at/uploads/media/Templeton_Fellows.pdf
This is a project from the famous quantum physicist Anton Zeilinger who is well known for his work on entangled states (several nature papers).
Templeton foundation makes it possible to fund some theologists for a one year period, that will, according to the proposal, work together with the quantum optics group from Zeilinger.

That is: some people really look up into the holy bible how to collapse a wavefunction……
It is scary that the interpretation of physical formulas and experiment data which is the sole domain of theoretical physicists, is now taken up by theologists on a religions basis.

24. **Peter Woit**
October 9, 2007

Thanks Jamie,

Glad to hear that the Trinity comment was intended as a joke, I was hoping so…

I'm afraid that I don't especially have any prejudices one way or another about whether a realist interpretation of QM is desirable. My own prejudices are fueled by a belief that deep ideas about physics and deep ideas about mathematics are often closely related, and the interpretation question is kind of independent of this. My skepticism about topos and category-theoretic ideas is just based on the feeling that they seem to be such general frameworks that, while they may be useful for formulating all sorts of different kinds of theories, they aren't likely to help one with the problem of identifying the specific mathematical structure that governs our word. But, as I keep pointing out, I'm all in favor of people with different guesses about what will be fruitful following those and seeing where they lead.

25. **Chris Heunen**
October 9, 2007

Yatima, you will want to look for `the effective topos'; it is very much related to computability and recursion theory. Invented by Martin Hyland in the 1980s, it also happens to be the best example (that I know of) of an elementary topos that is not a Grothendieck topos, i.e. a logical structure that allows full internal reasoning but could not have arisen in a geometric setting.

26. **tytung**
October 9, 2007

Hi Jamie,

I wonder if you can describe a bit on the realist interpretation of quantum theory as seen from a topos formulation?

27. **Klaas Landsman**
October 10, 2007

1) Topos theory

Though a proponent of the use of topos theory in physics myself, I would agree that so far very little has really been achieved in this direction; the program largely rests on hope, on the excitement of having a relatively new and profound mathematical formalism at one's disposal, and on a few technical indications that we might be on the right track (it would be ironic if topos theory indeed turns out to be relevant to physics, since its founder, Grothendieck, abhorred physics and physicists as he held them responsible for the atomic bomb). The reformulation of the Kochen-Specker Theorem in sheaf-theoretic language by Butterfield and Isham, see arXiv:quant-ph/9803055, (which for me and others was the original reason to get interested, spurred by friendship with and intellectual admiration for these authors) is certainly nice, but by no means shows that topos theory is genuinely relevant to physics. The papers by Doering and Isham, in particular section 4 of their second paper at arXiv:quant-ph/0703062, formed a second source of inspiration for me, notably their idea of reformulating the pairing between observables and states so that it takes values in the subobject classifier of a suitable topos (instead of in the interval [0,1]). Here the goal is to derive the usual probability interpretation of quantum theory from a deeper multi-valued truth assignment, but it has to be stated quite clearly that this goal has not been achieved so far (there are clear indications that it can be achieved, though). In fact, even the meaning of the multi-valued truth assignment in question is pretty unclear. Thirdly, the work of Heunen and Spitters at Nijmegen, see arXiv:0709.4364, appears to be the first mathematically rigorous and truly satisfactory version of Bohr's version of the Copenhagen Interpretation, especially his Doctrine of Classical Concepts and his Principle of Complementarity (see my Handbook article, arXiv:quant-ph/0506082, for a modern though pre-topos reading of these notions). I distance myself from any reference to Heidegger (instead of Bohr) by Doering and Isham at this point.

I would say that the essence of topos theory resides in its natural link with geometric logic, a fragment of intuitionistic logic deemed relevant to observational theories. As I said at the Deep Beauty meeting in Princeton, the statement that fundamental physics ultimately rests on geometric logic seems to me to be the correct version of the common informal idea that quantum gravity is "finite", combined with the observation that quantum mechanics seems to cry out for intuitionistic (as opposed to classical) logic.
The notion of a geometric morphism is highly attractive and might be the right home for the functoriality of quantization, an idea I wrote a number of papers about and which so far has lacked the link with the classical limit that the inverse image part of a geometric morphism could perhaps provide.
As topos is a branch of category theory, it also deserves to be mentioned that there is increasing evidence for the relevance of category theory in physics, not merely as an organizing principle behind the maths behind physics, but more directly, too (cf. work by Graeme Segal, John Baez and others).

Despite the somewhat preliminary nature of topos-related work in the foundations of physics so far, I was a bit surprised to see Gerard 't Hooft's dismissive comments on topos physics at the fqxi website
(see the link "Topos or not Topos" at http://www.fqxi.org/community/). Indeed, 't Hooft, myself and a few others recently submitted a large research proposal on the foundations of quantum mechanics, general relativity and quantum gravity to FOM, a branch of the Dutch Science Foundation responsible for the funding of physics research, in which topos theory (with reference to Isham et al) was explicitly mentioned as both promising and appropriate to the Dutch research landscape (for example, Ieke Moerdijk is a leading expert on topos theory and logic with close ties to myself, initially because of our joint interest in groupoids and lately also because of our view that the philosophy of mathematics should
be relevant to the philosophy of physics). Now, believe it or not – 't Hooft is widely regarded as one of the most eminent theoretical physicists in the world, having won the Nobel Prize in 1999 for work done in the 1970s and since then still thriving with deep and original ideas, such as the holographic principle –

our proposal was not even shortlisted! Such is the funding climate for foundations research outside of the Perimeter Institute.

Which brings me to my second theme:

2) Science, religion and the Templeton Foundation

The John Templeton Foundation takes a sympathetic eye towards foundational research in physics and is funding an increasing part of it. First, it has to be mentioned that controversies of the type we are dealing with here seem heavier in the US than anywhere else; even technical disagreements about the K12 Math curriculum are referred to as "wars" (see Suzanne M. Wilson, California dreaming: reforming mathematics education, Yale UP, 2003). As a case in point, rumour has it that the Deep Beauty meeting had to be held at the Nassau Inn instead of at Princeton University because the latter refused to be host to an event sponsored by the Templeton Foundation (and this despite the fact that the Chair of the conference, Hans Halvorson, is a professor of philosophy at Princeton University). Apparently, it was never a problem for Princeton University to host researchers receiving grants from a Government involved in the Vietnam war or in the invasion of Iraq, not to mention all the other illegal operations practically any US Governments has been associated with in the past and the present.

On a somewhat different note – trading the illegal for the legal – the Dutch Government receives part of its income from taxes paid by the producers of animal porn movies, the production of which is perfectly legal in the Netherlands (we do live up to our reputation). Indeed, 75% of the world production of animal porn comes from the Netherlands. Such movies do not display the intercourse of animals among themselves, which perhaps would be offensive enough if exploited commercially, but the rape of women
by dogs and goats etc. Although it is hard to think of something more disgusting and humiliating, I have never heard of anyone returning his or her research grant to the Dutch Government for this reason Indeed, neither have I).

The mission of the Templeton Foundation, on the other hand, is: "to serve as a philanthropic catalyst for scientific discovery on what scientists and philosophers call the 'Big Questions.' Ranging from questions about the laws of nature to the nature of creativity and consciousness, the Foundation's philanthropic vision is derived from Sir John's resolute belief that rigorous research and cutting-edge scholarship is at the very heart of new discoveries and human progress."

Sir John apparently hopes and expects that the answers to the Big Questions will involve God one way or the other, but so did Isaac Newton and I see nothing objectionable in this. No commitment in this direction whatsoever is required from grantees of the Foundation, and even the research funded by the Foundation need not contain any religious theme (and indeed much of this research does not).
It seems to me that much of the hostility to religion I read in typical comments on the Templeton Foundation by secular scientists is fuelled by fear of Bush and the Christian right. I am a secular scientists and I share this fear, but I do not see why it should read to a rejection of the Templeton Foundation (or indeed of its grantees).

In fact, I believe atheists or naturalists should be honest enough to admit that science has not given us a clue about the question why there exists a Universe at all, or, equivalently, why there exists something rather than nothing. I once closely followed research on the "wave function of the Universe" and other attempts to explain how the Universe could have been created from nothing, and in honor of our host Peter Woit I can safely tell you that such research is Not Even Wrong. We haven't got a clue.

In conclusion, I felt no qualms in receiving my expenses and honorarium cheques from the Templeton Foundation, I am going to apply to the Start program (which is funded by the Foundation) and I would not hesitate in the slightest to apply to the Templeton Foundation itself in the future, e.g. for a large program on determinism. I think we should be grateful to this Foundation and its executives for funding foundational research that Goverments typically refrain from supporting, and for doing so without asking anything in return but high quality.

28. **Andreas Doering**
October 10, 2007

Peter,

thanks for the clarifications. It is good to keep the assessment of scientific work apart from the political (like questions about funding), as usual. And, as also said by some other participants, the Deep Beauty meeting was free of religious under- and overtones, and that is how it should be for a science meeting.

I am happy to see that there is some interest for the work on topos theory in the foundations of physics. Every thoughtful criticism may be very useful. Klaas, John, Jamie and Urs give some valuable hints to the literature and some background information – thanks for that! -, and it is easy to find in the ArXiv the four papers (from March) that I have written with Chris.

tytung,

an explanation of the `neo-realism' achieved by the topos formulation of quantum theory is not easily done in a few lines. I would like to point you to the papers. If you like, contact me via email with questions.

29. **a.k.**
October 10, 2007

..let me say that it is certainly true that 'any money comes with an agenda', still the questions remains, to which degree the formulated aims interact with the content. While the US government is far from being without scientific agendas regarding their funding, someone mentioned quantum information, its funding still permits 'diverging' opinions, as the example 'string theory' and its opponents clearly shows.

One could read, one year ago, some concerns regarding the Templeton foundation in the german weekly magazine 'die Zeit', where it was shown to what extent the Templeton foundation already interacts with certain themes in the non-exact sciences, here the Foundation actively engaged, as an example, in psychological projects to show the effect of 'prayer' and 'forgiveness' on health status and life-expectance, the evidence for both effects was rather small, by the way, this new area is called 'neuro-theology'. The question remains how, especially in times of decreasing financial support for the non-exact sciences, a Foundation with a certain religious and explicit ideological agenda could influence the neutrality and freedom and possible critical views towards religion in the non-exact sciences or could even be involved in questioning certain achievements of critical rationalism as an organizing principle in society. A good example for such a questioning, as it was mentioned, would be to punish 'non-spiritual', 'non-moral' behaviour as pornography or even 'animal porn' by identifying it naturally with a criminal act, disregarding the highly differentiated categories of modern societies leading to tolerate these behaviours, given they are acts of free choice.

Regarding the exact sciences, Templeton's agenda is to accelerate a certain characteristic any progress has regarding the myths it evaporated: to become an even more powerful myth in its own. Since from the beginning, especially the exact sciences tended to develope a 'vertical' character which implied some ressemblance with monotheistic religious concepts, the question remains, if Templeton will be involved in a development which naturally converges to an anti-emancipating 'scientific god', that it will help to substitute the scientific method to be the culmination point of universal belief for anyone who is not part of a small, neo-religious elitistic circle, called exact scientists, even today certain parallels between ancient religious symbols and the particle accelerators are undenieable. To conclude, one could judge it doubtful to isolate Templeton's actions on physics and mathematics, where their agenda is far from being clearly religious, one has to consider the effect on science and society in general, i.e. in that era of religious fundamentalism that we are going through.

30. **woit**
October 10, 2007

Thanks Klaas, both for the lucid explanation of some of the motivations of the work on toposes, and the comments on the Templeton funding issue.

31. **chris**
October 11, 2007

a.k.,

once we reach the point at which the templeton foundation – or any other private sponsor for that matter – is the main source of funding in a certain area of science it would be time for society to react. react by outdoing the private source and thus claiming the research topic in question firmly back into the public domain.

if society chooses to be oblivious – well – then so be it. research in that area will then not be driven by public interest but by private interest. ultimately it is just a reflection of the value commonly assigned to a specific field.

what i hope this will ultimately achieve is to ring the alarm bell in society that no private organization should take over research funding and direction.

if this will not happen – well – then we are kind of lost anyways. and funding no matter what agenda behind is still better than no funding, since i firmly believe that ultimately the truth (i.e. true statements about reproducible empirical relations) will ultimately prevail and nothing else.

32. **Steven H. Cullinane**
October 11, 2007

Chris says *the* truth consists of “true staements about reproducible empirical relations.” He should read William Golding’s Nobel lecture: “When I consider a universe which the scientist constructs by a set of rules which stipulate that this construct must be repeatable and identical, then I am a pessimist and bow down before the great god Entropy. I am optimistic when I consider the spiritual dimension which the scientist’s discipline forces him to ignore.”

33. **Stephen J. Summers**
October 11, 2007

If I may interject a clarifying remark: while at the conference, I spoke with Hans Halvorson about why the conference was moved from the campus to the Nassau Inn. It was not the University, but rather one senior professor in the physics department (I will suppress the name), who made it clear to Hans that he would prefer the change of venue, as he did not want to place the Physics Department’s imprimatur upon anything associated with the
Templeton Foundation. Indeed, in connection with this von Neumann celebration there were major talks given on the Princeton campus which were also funded by the Templeton Foundation (and this fact was prominently displayed on the advertising placards placed all over the campus). So, Princeton University did not distance itself from the Templeton Foundation in the least.

Lubos on Lenny

October 8, 2007
Categories: Uncategorized

Last night a new paper by Lenny Susskind appeared on the arXiv, carrying the title The Census Taker's Hat. It seems that Lubos Motl stayed up much of the night reading it, with a long posting on the subject appearing before 8 am in the Czech Republic.

Now that he's no longer employed within the string theory academic community, Lubos feels free to treat Susskind in much the same way he did Lee Smolin, characterizing Susskind and collaborators as a "gang" of "leftists", and making fun of the central notion in Susskind's paper (that of a preferred observer called the "Census Taker") by referring to it as "Stalin the daddie". He gives a detailed section-by-section critique of Susskind's paper, here's some of the flavor:

> Well, this is about 7th assumption that seems obviously wrong to me – this one is really bad – but let's go on reading. I still haven't understood what question he exactly wants to be answered. Equally seriously, I don't understand whether he thinks that his speculation about the location of the central committee is a hypothesis with some evidence, a nice hypothesis without evidence, God's ad hoc decision, or why does he exactly believe it.

Unlike Lubos, I haven't tried to follow the details of Susskind's 65 page argument, but did try to figure out how he addresses the central problem of any multiverse scenario: how do you test it? If you can't test it, it's not science. Susskind describes exactly two possible ways that information about the "Ancestor" universe to ours may be accessible.

- The sign of the spatial curvature should be negative. This just predicts one bit of information about the universe, and there's a paper claiming that you can also get the other sign, so that even this one bit is not there.
- If the number of slow-roll e-foldings is "minimal", then tensor fluctuations of the CMB would be there, but just in the lowest harmonics. Funny, but last week I was told in a colloquium talk that string cosmology predicts no observable tensor fluctuations…

Susskind begins by claiming that "To many of us, eternal inflation, bubble nucleation, and a multiverse, seem all but inevitable", but goes on to note that the fact that one has an infinity of universes that one doesn't know how to count means that "the inevitable has led to the preposterous". A reasonable person might decide that this means that things weren't so inevitable, but Susskind feels that one must soldier on, although "In my opinion, this situation reflects serious confusion, and perhaps even a crisis." This paper is his attempt to address the crisis.

Susskind quotes Bjorken as having told him that the Multiverse is "the most extravagant extrapolation in the history of physics". He seems rather proud of this, but somehow I suspect that Bjorken didn't mean this as a compliment…

Comments

1. **Jack**
   October 8, 2007

   This is off-topic, but I can't still believe that Lubos left academia. What went wrong? Can anybody tell? What is he going to do next?

2. **IMHO**
   October 8, 2007

   What went wrong with Lubos??????

   Have you ever read his blog? Have you read this post? And I would call this post tame compared to the usual….I think it's now common knowledge that his mouth is a big part of what happened.

   Either way, no matter how much I disagree with his opinions, I have to respect his dedication to the truth (as he sees's it)…consequences be damned!!! Also, you have to be impressed with his ability to debunk a 65 page Susskind paper in one evening.

3. **Jack**
   October 8, 2007

   Again off-topic, but let me tell that only after reading Peter's current post I came to know that Lubos had left academia. I searched on the net including wikipedia but the reason why he did it remains unclear. Now I see he had mentioned about this decision at other blogs but so far the only hint he had given is: he wants to leave institutionalized science. He is extraordinary no doubt otherwise he would have not become a faculty member at Harvard. I am waiting to know what he is going to do next.

4. **Chris Oakley**
   October 8, 2007

   > Dear Lubos, I think you should know that this time you've really outdone yourself. Normally a very negative review will induce me to go see for myself (because of my contrarian streak), but this time I'll have to fight a real sense of repulsion in order to do so. Congratulations.

   This was the first (anonymous) comment following Lubos's review.
   I agree entirely.

   As for Lubos leaving Harvard, I thought that it was common knowledge that he was driven out by an unholy alliance of feminists, communists, climate change scaremongers and anti-science crackpots.

5. **Dan Fitch**
   October 8, 2007

   In the last paragraph, you note "Susskind quotes Bjorken" when I'm pretty sure you mean "Lubos quotes Bjorken".

6. **Dan Fitch**
   October 8, 2007

   Actually, they both quote the same thing. Never mind. I still think you mean to address Lubos taking it as more of a compliment than intended.

7. **Peter Woit**
October 8, 2007

Dan,

No, it is Susskind, in his paper, and he's the one I'm guessing took it as more of a compliment than intended. Bjorken is Susskind's colleague at Stanford and presumably they've discussed this issue in person.

8. **IMHO**
October 8, 2007

I just reread Lubos's post…and all I can say is WOW. Irregardless of whether or not he is correct; it's almost as if he has spent his entire life in a book and has no concept of social acceptance or appropriate social interaction.

9. **Ted**
October 8, 2007

Anyone else forced to ctrl-alt-del out of Lubos' site?? Happens to me every time now …

10. **Ted**
October 8, 2007

Tried FoxFire, works ok … never mind.

11. **Garrett**
October 8, 2007

Heh. If you try to follow Peter's link to Lubos' blog, you'll get a redirect stating visitors clicking in from NEW aren't welcome. In defense of this snub against PW and his readers, he cites non-other than the esteemed Professor Susskind. Apparently the enemy of his enemy is… his enemy.

12. **F.**
October 8, 2007

No, Susskind is not treated by Lumo as his enemy. He just happens to think that Lenny is wrong in this new paper. Thats different.

13. **David**
October 8, 2007

This is off topic, but I think Lubos' blog and the comments there have become largely irrelevant. I can't see anybody taking it seriously. The comment stream no longer contains any serious comments by people in science. Given where he once was, it's all a bit sad.

14. **Eu**
October 8, 2007

Regarding "may or may not", I doubt these stylistic constructions don't exist in other romance languages given that Portuguese and Castellian have them. They are not used very often but they do exist together with "for what it's worth", which ironically isn't worth anything.

Lubos' site is heavy on the javascript with all sorts of junk and it cripples some browsers to their knees. Just disable it either globally or just on that site. You won't loose much. Alternatively use an rss feed reader.

To follow links to TRF from NEW just disable the referer (some browsers allow this in the privacy settings). Lubos isn't a magician and doesn't know where you come from unless you tell him.

This time Lubos actually surpassed himself, even with the leftist metaphors.

15. **chris**
October 9, 2007

"This is off topic, but I think Lubos' blog and the comments there have become largely irrelevant. I can't see anybody taking it seriously. The comment stream no longer contains any serious comments by people in science. Given where he once was, it's all a bit sad."

well, if someone makes a habit of deleting critical remarks, that's what you get.

16. **Aleksandr Mikunov**
October 9, 2007

May sound slightly off the topic, but keep on reading 🙂

1) It is ironic to read how Lubos M. is demoting Susskind's work whereas his own stuff is just a generic blah-blah-blah [E.g. look at LM's PhD: http://arxiv.org/abs/hep-th/0109149. Semi-trivial math and not a single physically meaningful calculation].

2) I wonder if the IQ level of contemporary theoretical physicists is even comparable to what t. physicists had back in the pre-string era, say in the 40-60-s [Although, LM insists it's by ~40 pts higher than average – I doubt it]

3) To LM and other "above-the-average-IQ" physicists: The field is dead, so become programmers (the good ones :).
Believe or not but becoming an !expert! in C++/Windows/Linux takes years of *extremely* hard work.

(BTW, Not as simple as calculating Green's functions 🙂
E.g try digesting this book: http://www.amazon.com/Modern-Design-Generic-Programming-Patterns/dp/0201704315)

(Note that here we are simply talking about *digesting* this stuff. You won't be able to come up with anything similar of that level for a long time 🙂

17. **Hendrik**
October 9, 2007

No one has commented on the fact that Witten has a new paper on the ArXiv at:
http://xxx.lanl.gov/abs/0710.0631

titled “Gauge Theory And Wild Ramification”.
I suppose it takes a bit longer to digest.

18. **fulo**
October 9, 2007

> Semi-trivial math and not a single physically meaningful calculation

According to the abstract, the results in his thesis were obtained “for the first time in history”. It’s just childish…

19. **Peter Woit**
October 9, 2007

I’d like to discourage further criticism of Lubos here, on grounds that it’s too easy, kind of boring, and at the moment he’s just a guy with a blog in Pilsen. Crazy as he is though, in this case he makes a lot more sense than Susskind, who’s somehow a leading figure in particle theory…

20. **Mr. Mustela**
October 10, 2007

Interesting. So we have a Census Taker, and scenarios with a “sense of inevitability” about them. I think I have an hypothesis on why the Templeton foundation is pouring money into theoretical physics.

21. **Peter Orland**
October 10, 2007

Oh, and Peter, sorry about contributing to an off-topic tangent in this
thread.

22. **Paul**
October 15, 2007

Lisa Randall mentions Lubos in Warped Passages – not for physics, but for slagging off female physicists!

23. **IMHO**
October 16, 2007

‘slagging off’ … Sounds British. What does it mean.

24. **Chris Oakley**
October 16, 2007

slag off (v.t.) – to denigrate

cf. slag (n.) – a loose woman, normally unattractive

25. **CM**
October 16, 2007

Harvard never ever gives junior profs tenure-this is a problem in lots of elite places. That is why LM left academia, the blog and his big mouth is an excuse that we all knew he would use once tenure was denied, people have been guessing this this would happen for years.

Latest From The LHC

October 8, 2007
Categories: Experimental HEP News

The LHC web-site contains a wealth of up-to-date information about how things are going there as they are commissioning the machine. In particular, one can follow the latest news about how things are going in each sector here, which may or may not give a more accurate picture of the situation than various rumors.

Today an updated commissioning schedule appeared at the web-site. One can see how things are going by comparing to the previous version (from Aug. 3, still available right now here). Attempts to cool down certain sectors have taken longer than expected, and the new schedule has them cooled to operating temperature 2-3 months later than in the previous schedule. The "Machine Checkout" and "Beam Commissioning" periods have not been changed yet, but it's not clear that the way they are currently listed still makes any sense (can you be commissioning the beam while still doing powering tests on the sectors??). It looks to me as if the present situation is that they are 2-3 months behind schedule now, and if all goes well, physics runs could start not next July as planned, but maybe in September at the earliest.

The CERN director general Robert Aymar issued a statement today that begins:

> In an age of blogs there are seemingly no secrets…

and ends

> All of this is business as usual when bringing a new particle accelerator on-line. There are inevitably hurdles to be overcome, but so far there have been no show stoppers. We can all look forward to the LHC producing its first physics in 2008.

I take the lack of any reference to the July 2008 date to mean that they acknowledge the schedule has slipped, but still think the slippage will only be a few months.

Update: I should also have mentioned this article, which explains how they are trying to deal with one of the most serious problems: using RF transmitters in ping-pong balls blown through the beam pipe to try and find broken copper fingers in the so-called "plug-in modules".

Update: Physics World has a story about this that starts:

> Robert Aymar, the director general of CERN, has dispelled rumours that a series of buckled electrical connectors at the Large Hadron Collider will delay the accelerator's official start-up date of May 2008.

In a technical sense this may be accurate, in that evidently the "official" start-up date has not been changed since the previous schedule. But the new schedule shows that things are two to three months behind where they are supposed to be to make the May 2008 date, so it now seems unlikely that that will be the date when they have a beam, or that July will be when they start doing physics. In his statement Aymar only promised start-up in 2008, so I take this to mean that he was quashing rumors about a delay into 2009, not the ones about a 2-3 month delay.

Update: Nature's Geoff Brumfiel has a new story about this. He quotes project leader Lyn Evans as saying the schedule is now quite tight

> "The next three months are going to be pretty critical," says Evans. "If something unforeseen comes up between now and then, it will slip. There's no doubt."

During this period one will be able to see exactly how they are doing. The cool-down schedule is here, periodic updates on how things are going in each sector are here, and the actual temperature of the magnets can be followed here.

Comments

1. **Paolo**
   October 8, 2007

   > which may or may not give a more accurate picture

   Maybe it's just me (I'm italian) but to date I fail to understand which is the informative content of the "may or may not" sentences, in english. Definitely we don't have an equivalent in italian, neither in spanish and french, I think. Nitpicking…

2. **Peter Woit**
   October 8, 2007

   Paolo,

   I guess it's pretty much pure stylistic affectation, just designed to indicate that reasons could be given for either of the alternatives being mentioned.

3. **a quantum diaries survivor**
   October 8, 2007

   Hi Paolo,

   there is no italian equivalent afaik (pqns 🙂 but there is informative content indeed in what Peter wrote… It casts some doubts while hiding the hand…

   Cheers,
   T.

4. **Yatima**
   October 8, 2007

   What about "peut-être bien que non, peut-être bien que oui", usually given in response to an indiscrete question? 😉

5. **Paolo**
   October 8, 2007

Argh, “peut-être bien que non, peut-être bien que oui” , I didn’t know about that! (honestly I’m not praticing much french these days beyond “Cahiers du cinema” and a few movies from time to time) What a waste of words… what a waste of words… 😉

6. **fulo**
October 8, 2007

> Definitely we don’t have an equivalent in italian, neither in spanish and french, I think. Nitpicking…

“May or may not” in Spanish, “puede o no”. I’d have guessed in Italian it would be the same thing, something like “puodi o non” or “puodi o menos” or some such. But then, I don’t speak Italian… 🙂

7. **Paolo**
October 8, 2007

This is quickly becoming off-topic, sorry to Peter. But, anyway, of course I can say “may or may not” in Italian, the question is whether it’s *common* in italian to say “… blah, blah, may or may not this, blah, blah, may or may not that…”. It isn’t, definitely. I maintain the same is by and large true of other neo-latin languages besides italian. In italian we can certainly say the rough equivalent of “may”, meaning “it is possible”, but it would be considered completely redundant to negate that “may” and add to it. Apparently, however, in english, something special happens, and “may or may not” becomes a single “block”, and, in a sense, native speakers do not instinctively appreciate that if something “may” (vs must) of course logically also “may not” and it’s not necessary do add the negation. That’s my point. That said, each language has its own weird, seemingly irrational, corners, I could mention 1000 in italian…

8. **wb**
October 8, 2007

Finding the damaged rf fingers is very important, but is not the only impediment to smooth sailing. One is left with the problem of the improperly fabricated fingers that are likely to damage in a thermal cycling of the machine.

9. **Ellipsis**
October 9, 2007

Peter (and all):

In fact, the answer to your question, as for what CERN management is officially stating (and which I’m _not_ going to try to defend), is _no_ — the official position is that the schedule remains with a _July 2008 start for beams_, despite the issues (which they believe can be solved without additional delay)…

Here is the talk, fresh from 6 hours ago, from Lyn Evans (LHC accelerator project leader):
http://indico.cern.ch/materialDisplay.py?contribId=85&sessionId=23&materialId=slides&confId=20736

Also of interest may be Peter Jenni’s (ATLAS spokesperson) talk yesterday:
http://indico.cern.ch/materialDisplay.py?contribId=9&sessionId=7&materialId=slides&confId=20736

Here’s what will probably really happen (but it’s not that far from reality, the problems really aren’t as bad as some were stating orginally). See p. 10 of Peter Jenni’s talk. The LHC Inaguration Day, at which multiple “VVIPs” (science ministers, and several European prime ministers and presidents, even a few royals — probably Sarkozy, the Swiss president, German minister of economics & technology, some members of the Dutch royal family(?), maybe Ray Orbach for the U.S., …) will come to CERN for the official inauguration of the LHC, is October 21 of next year. There MUST be a collision or two, or at VERY least storage of (low current) beams, by that date, to show particles colliding to a very large number of dignitaries in the control room.

I think Peter Jenni’s original statement (last month, that he appears to still hold) that there would probably, in the end, be a ~2 month delay is still correct. But the official CERN position is that the schedule remains for July beams next year.

Put your money on 1 or 2 observed proton collisions and corresponding pretty event displays by early October next year. But note that nobody really knows or can predict precisely what problems have yet to occur.

10. **Peter Woit**
October 9, 2007

Thanks a lot Ellipsis,

The idea that there will be a push to have some collisions when the VIPs show up on Oct. 21 sounds plausible. Probably no similar push for the string theorists, who will be there Aug. 18-23 for Strings 2008…

11. **DB**
October 9, 2007

It seems France and Germany are in a position to do some early celebrating: Albert Fert (from France) and Peter Gruenberg (from Germany) have just won the Nobel Prize in Physics for discoveries in magenoresistivity. Maybe String Theory will get it next year!

12. **Christine**
October 9, 2007

Congratulations to the Nobel prize winners and to the Brazilian physicist Mario Norberto Baibich, who was the first author to the 1988 paper.

(I have the feeling that Brazil will never get a Nobel prize.)

Christine

13. **dragon**
October 9, 2007

The usual nobel for trivial advances in solid state. Yawn. Somebody nobody has heard of, together with somebody else nobody has heard of, discovered the ipod. Strong contender for the most forgettable Prize of recent times.

14. **Johan Couder**

October 10, 2007

"The usual nobel for trivial advances in solid state. Yawn. Somebody nobody has heard of, together with somebody else nobody has heard of, discovered the ipod. Strong contender for the most forgettable Prize of recent times."

Where does that cantankerous remark come from ? Theoretical physics isn't the only physics game in town.

15. **Peter Orland**
October 10, 2007

Part of the function of the Nobel Prize is to put physics in context for
the rest of the world. That may not have been one of its original purposes, but today it is the case. For this reason, I think it's good that practical aspects of physics are put into the limelight. The public needs to be made aware that the practical devices they use originated in relatively recent physics research.

The committee's choice may not reflect the interests of the people who read this blog, but it certainly is real physics and is not forgettable.

16. **Peter Orland**
October 10, 2007

Just apologized for being off-topic in the wrong thread. I'm
doing this here again….

17. **SnarkFest**
October 10, 2007

Yes, Dragon. Those "trivial advances in solid state" help pay quite a few bills for those of us with more esoteric interests. And need I mention yet again the cross-over between condensed matter physics and quantum field theory?

18. **Kris Krogh**
October 11, 2007

I'm happy they give Nobel Prizes for research with a demonstrable connection to the real world, and not for playing games that only generate lots of citations.

19. **srp**
October 11, 2007

Didn't Marconi win it more as an inventor than a physicist? If anything, there may be too little credit given to the inventor types versus the academics–wasn't that the crux of the dispute over the Medicine prize for MRI? In any case, device physics is real physics.

20. **Biff**
October 12, 2007

Unless I'm mistaken, the giant magnetoresistance phenomena that Fert and Grunberg discovered is rather more useful than a 'trivial advance'.

In fact it's vital in most hard drives made today, including those used at the LHC. Or isn't that sort of physics real enough for some of you ?

21. **mo**
October 12, 2007

It is completely OK to get a Nobel for an invention as Alfred Nobel's will (see a larger excerpt below) explicitly says that a Nobel in physics should go "to the person who shall have made the most important discovery or invention within the field of physics." I heard many times that it was a long-standing Nobel Committee's policy to award the science prizes for outstanding work that resulted in tangible, practical advancement of sciences and useful arts.

That's why two most visible candidates for "genius" status, Hawking and Witten, never received the prize–and rightly so.

"The whole of my remaining realizable estate shall be dealt with in the following way: the capital, invested in safe securities by my executors, shall constitute a fund, the interest on which shall be annually distributed in the form of prizes to those who, during the preceding year, shall have conferred the greatest benefit on mankind. The said interest shall be divided into five equal parts, which shall be apportioned as follows: one part to the person who shall have made the most important discovery or invention within the field of physics; one part to the person who shall have made the most important chemical discovery or improvement; one part to the person who shall have made the most important discovery within the domain of physiology or medicine; one part to the person who shall have produced in the field of literature the most outstanding work in an ideal direction; and one part to the person who shall have done the most or the best work for fraternity between nations, for the abolition or reduction of standing armies and for the holding and promotion of peace congresses. The prizes for physics and chemistry shall be awarded by the Swedish Academy of Sciences; that for physiological or medical work by the Caroline Institute in Stockholm; that for literature by the Academy in Stockholm, and that for champions of peace by a committee of five persons to be elected by the Norwegian Storting. It is my express wish that in awarding the prizes no consideration whatever shall be given to the nationality of the candidates, but that the most worthy shall receive the prize, whether he be a Scandinavian or not."
http://nobelprize.org/alfred_nobel/will/will-full.html

22. **milkshake**
October 13, 2007

Nobel Commitee likes to award for narrowly-defined contributions that have a great impact. HEP theorists are naturally at disadvantage.

Polarography got Heyrovsky a Nobel – a very simple-minded potenciometry experiment. (and a method for analyzing metal samples – not exceedingly useful one). It was the first completely-defined and understood system in electrochemistry – one that greatly boosted the self-confidence in the field. It also makes a good textbook chapter.

23. **Sebastian Thaler**
October 13, 2007

Peter,

Off-topic, but the issue of COSMOS magazine with your cover story on string theory has finally appeared on the magazine stand at my local Barnes & Noble. I bought it and am looking forward to reading it.

24. **woit**

October 15, 2007

Thanks Sebastian. I just got a copy of the magazine in the mail, and I'm quite pleased with how the article came out.

The Great Cosmic Roller-Coaster Ride

October 15, 2007
Categories: This Week's Hype

More than three years ago Scientific American ran a feature article by Bousso and Polchinski promoting the then new idea of The String Theory Landscape. Now that this pseudo-science has become well-entrenched in the physics community, this month's issue of the magazine has a feature article on The Great Cosmic Roller-Coaster Ride, describing how

> one of the emerging themes of 21st-century cosmology is that the known universe, the sum of all we can see, may just be a tiny region in the full extent of space

and claiming that this is "stimulating a thorough rethinking of the early universe in terms of string theory." There is quite a bit of defensiveness about string theory in the article, where it is described as the "leading candidate for the foundational laws of nature". The authors note that "String theory has received some unfavorable press of late", and characterize criticism of the theory as due to the fact that it "has yet to be tested experimentally", ignoring the fact that much of the criticism is about string theory's inherent untestability. Not only has it not been tested yet, but no one has any idea how to test it ever. They admit as much when it comes to predictions about particle physics:

> string theory has disappointed because it has not yet been possible to test it experimentally, despite more than 20 years of continued investigation. It has proved hard to find a smoking gun – a prediction that, when tested, would decisively tell us whether or not the world is made of strings. Even the Large Hadron Collider (LHC) – which is now nearing completion near CERN , the European laboratory for particle physics near Geneva – may not be powerful enough.

At the same time, they imply that the answer to string theory's problems is that it will produce testable predictions about cosmology. They describe work of their own and other people attempting to use as an inflaton field positions of branes or moduli parameters describing positions in the Landscape. What predictions do they see coming out of this?

- CMB experiments will continue to not see effects due to gravitational waves. In other words, the prediction is just that CMB experiments won't see anything relevant. Not exactly a distinctive prediction of string theory, and Lenny Susskind lists seeing such effects as the main hope for observational evidence of the Landscape. If such effects are seen, I doubt that string cosmologists will give up on string theory, but just come up with models that do "predict" such effects.
- Cosmic superstrings, observed by their gravitational lensing effects. One can in principle construct scenarios where cosmic superstrings are produced in the early universe in just such a way as to have eluded all observation until now, but such that one will turn up as we look more closely at more galaxies. I don't know of any reason for this other than wishful thinking. If cosmic superstrings don't show up over the next few years, I don't think anyone will take this as evidence against string theory.

I don't think it does much for the public understanding of science or increases respect for scientists when they decide to go to the public in this way, promoting extremely speculative and complex ideas that lack not only a glimmer of experimental evidence, but also any plausible idea about how they can be tested.

Update: There's a new review of string cosmology up on the arXiv tonight. The authors contradict the SciAm article's claim about whether these models can accomodate observed effects of gravitational waves:

> As an example, in many – but *not* necessarily all – string inflation models, the primordial tensor signal is very small.

giving examples of models with detectable gravitational waves (see here).

While the article as a whole is pretty much unadulterated hype for string cosmology, it ends on a downbeat:

> Despite these promising signs, it remains to be seen whether this endeavor will lead to genuine contact between experiment and Planck-scale physics. In many scenarios, inflation is described by a well-controlled, albeit fine-tuned, effective field theory Lagrangian, and inflation lasts long enough to obscure all evidence of a pre-inflationary stage. If we live in such a universe, cosmological observations can, at best, teach us about the nature of the inflaton, but will provide few clues about more fundamental physics, except perhaps through the enduring mystery of dark energy.

Comments

1. **roland**
   October 16, 2007

   "Not only has it not been tested yet, but no one has any idea how to test it ever."

   This might very well be true, but a good half of your postings
   feature a similar statement. Perhaps you should choose it as a
   caption for the whole blog.

   ..

   However, maybe it can't be said often enough and in fact
   i like your blog anyway.

2. **Coin**
   October 16, 2007

   This might very well be true, but a good half of your postings feature a similar statement. Perhaps you should choose it as a caption for the whole blog.

   Technically, isn't it expressed in the *title* of the blog?

3. **roland**
   October 16, 2007

   i noticed that right after posting.

   but it might as well have been some sort of weird sarcasm.

4. **Kris Krogh**

October 16, 2007

Hi Roland,

Trouble is, string theorists keep coming up with new “tests,” and others cite those without checking them.

See this exchange with Joe Polchinski, items 35, 43, 44. He never responded to the last one.

5. **fulo**
   October 16, 2007

   > See this exchange with Joe Polchinski, items 35, 43, 44. He never responded to the last one.

   Kris,

   I’m very far from sharing Prof. Polchinski’s optimistic view of the landscape, but I think his answer #43 is perfectly sensible. What he says is that having a theoretically tractable model of a strongly coupled non-abelian gauge theory out of equilibrium is important and interesting. It is.

   I think there are at least three reasons why he didn’t answer your comment about quoting a non-refereed talk as a basis for his remarks.

   First, because if you really want references to refereed papers, you just have to look them up in the talk he mentions.

   Second, because he was posting his answer to a weblog, not to a scientific journal, so there’s no need to be so picky about references.

   And third, because actually whether a paper is refereed or not is of no importance at all to any practicing physicist. People download papers right from the arXiv, read them, and decide by themselves whether they believe them or not. You only download papers from journals when they are too old to be found in arXiv or, else, when you are very interested in the figures, which are usually better printed in the published version.

6. **Quixotik**
   October 16, 2007

   This blog is a bit one-note, true, but many good blogs are…if someone ever actually comes up with a universally accepted way to test string theory, this blog will lose it’s *raison d’etre*.

7. **nerd**
   October 17, 2007

   Quixotik, they can test string theory, the whole problem is that there are 10^500 versions to falsify before any alternative ideas can get a look in…….

8. **roland**
   October 17, 2007

   maybe browsing through that math book, they just chose the wrong page. i mean the not even wrong page.

9. **Thomas Love**
   October 17, 2007

   nerd said: “…. there are 10^500 versions to falsify before any alternative ideas can get a look in…….”

   What a horrifying possibility…that would tie up progress in physics for 10^490 years! Or longer! I won’t be able to sleep tonight thinking about that.

Accumulated Links

October 15, 2007
Categories: Uncategorized

Various things that I've been wanting to mention:

Steven Hawking has a paper out, on his version of the Landscape story, using amplitudes that don't rely upon string theory or eternal inflation. But just like the string theory Landscape I don't see how his proposal is testable. It completely gives up on saying anything about particle physics, even statistically

> If the volume weighted amplitude for the standard model vacuum is non-zero, it is irrelevant what the volume weighted amplitudes for other vacuum states are. The theory can not predict a unique vacuum state. Instead we have to input that we live in the standard model vacuum.

He ends with

> The amplitudes will be highest for states in which the whole universe is in a single state, rather than a mosaic of different states, as predicted by eternal inflation. There will be no primordial production of topological defects, such as monopoles, and cosmic strings. Not all states in the landscape will have significant amplitudes, but there will be more than one that do, so M theory does not predict a unique low energy particle physics theory. It is implausible that life is possible only in one of these states, so we might have chosen a better location.

John Baez has a new This Week's Finds out, with interesting discussions of the topos-theoretic approach to quantum theory, and the analogy between the integers and three-dimensional space. This semester he is running a seminar on "Geometric Representation Theory" (not clear how close this is to the use of the term by those representation theorists who work with D-modules). Videos and lecture notes from the talks are available, along with some blog postings (see here and here).

As always, Terry Tao's blog has wonderful postings and articles, often of a general expository nature. For some recent examples, see one about the Schrodinger Equation, and another about Jordan normal form.

Besides excellent expository physics postings such as the recent one on single top production, Tommaso Dorigo gives a more realistic view of the academic life than most other blogs. For some understanding of how academics feel about the travel opportunities that conferences present, and what they think about the question of whether their employer should be financing what sometimes feels like a vacation, see his recent posting on Ethical aspects of professional conference-going. I strongly endorse his recommendation of the David Lodge novel *Small World*.

There's a string theory wiki out there, aimed at students trying to learn string theory, which has been set up by the Centre for Research in String Theory at Queen Mary College. Much of the site is a listing of the one thousand or so review and other papers an aspiring young theorist should read and absorb to get an idea of what is going on with string theory. Also listed are various blogs, including this one, that might save students some of this reading…

Comments

1. **John Baez**
   October 15, 2007

   Peter writes:

   > This semester he is running a seminar on "Geometric Representation Theory" (not clear how close this is to the use of the term by those representation theorists who work with D-modules).

   Ideally our seminar will eventually cover such material, taking a new viewpoint that makes it seems a lot less technical and scary than it usually seems. But we're starting with more elementary stuff, e.g. Coxeter groups, Hecke algebras, buildings, Bruhat decompositions, Schubert cells and the like. Again, we're taking a new viewpoint designed to make it less scary than usual… a big emphasis on q-deformation and groupoidification. So far we're just doing the An case – that is, representations of Sn, SL(n) and the like. Later we'll generalize to other Dynkin diagrams.

   The seminar will probably last for at least two years. It's hard to see precisely where it will go.

2. **David Ben-Zvi**
   October 16, 2007

   Geometric representation theory (as I understand it) is a natural extension of classical harmonic analysis in which function
   spaces are replaced with various geometric alternatives, be they cohomology groups, K groups, or categories of sheaves of various
   kinds (such as D-modules, perverse sheaves, coherent sheaves,…).
   There are natural analogs of Fourier transforms and the classical Hecke operators, and all are realized geometrically –
   one of the fundamental principles of geometric harmonic analysis
   (just as for classical harmonic analysis) is that all reasonable operators are realized by "integral kernels", which are themselves geometric objects of the same kind on a correspondence (or span).

   Usually one is studying not just a space but a space with
   a group action so all of the geometry is done equivariantly with respect to that group (or equivalently on the level of stacks or groupoids). Also one often introduces more refined structures (in particular gradings), coming from Hodge theory or Galois theory, leading to q-deformations. When studying semisimple groups in their many incarnations (eg over R, C, finite fields, loop groups, quantum groups etc) the spaces involved are essentially always the same – one studies flag varieties, nilpotent cones, Springer resolutions and variations on the above.

   The themes and structures in the subject are
   surprisingly uniform and conceptual across a broad range of questions. They're also not at all scary or technical intrinsically, though perhaps often presented that way. The fundamental ideas can all be explained in accessible ways, though it is hard to find literature in that spirit (I am trying to collect what informal expository materials I can find on my webpage but there's still a long way to go!)

   In any case it's really great to have John and James's seminar and their excellent explanations publicly available as a resource in the subject and drawing attention to geometric representation theory and I am eagerly awaiting its further developments.

3. **sophia**
   October 16, 2007

You forgot this link from NY mag:

http://nymag.com/guides/fallpreview/2007/television/36554/

"Simon Judes (27, string theory): Like Gob on Arrested Development! We're basically always whining about nothing important. And then we're absurdly happy about tiny achievements.

DK: And in every department I've ever been in, you have the one guy who's essentially lost his mind. Those guys are very weird and would be great comic fodder.

AA: No other field, I think, collects as many crazies. And not just physics crazies—all kinds of crazies.

SJ: Oh, and another thing, the whole Stephen Hawking bit. Stephen Hawking is actually a rather peripheral figure in physics research."

4. **mo**
October 16, 2007

The other day at our physics library, I picked up a new book by Raymond Streater:

Streater, R. F.
Lost causes in and beyond physics
Berlin : Springer, c2007.

I think it should be a required reading for everyone in the topos-theoretic quantum theory business. Streater gives a thoughtful overview of quantum logic and related subjects and concludes as follows:

"Very little physics has resulted from quantum logic, trivalent logic, or Jordan algebras."

Most sections in the book have amusing epigraphs. Here is the epigraph for the section on quantum logic, etc.:

"Science is not that easy."
Lubos Motl

5. **Kea**
October 16, 2007

Very little physics has resulted from quantum logic, trivalent logic, or Jordan algebras.

I saw this book at GRG18 a few months ago – very nice – but this comment is somewhat behind the times.

6. **Peter Woit**
October 16, 2007

Thanks John and David,

More expository sources about geometric representation theory would be a great help. It's a fascinating but too little known area of mathematics, and probably has interesting implications for physics. The efforts of both of you in this direction are greatly appreciated.

7. **Holmes**
October 16, 2007

"Streater, R. F.
Lost causes in and beyond physics
Berlin : Springer, c2007.
"
Of course, if you asked 100 physicists for a good example of a lost cause, 99 would probably cite Streater's work. The best defence really *is* offence…

8. **Eugene Stefanovich**
October 17, 2007

R.F. Streater has a fascinating/humorous/controversial/… web-site

http://www.mth.kcl.ac.uk/~streater/

Highly recommended.

Eugene.

9. **Hans**
October 19, 2007

Peter, in a new article in current nature magazine, it seems that Herrmann Nicolai from the German MPI for gravitational physics somwhat joins your opinions. At least the abstract reads that:

http://www.nature.com/nature/journal/v449/n7164/index.html
String theory: Back to basics p797

"String theory was toutet as a theory of everything"

IT WAS! (and isnt anymore..)

"And now may at last succeed as a theory of something very specific — the interactions of particles under the strong nuclear force."

It MAY AT LEAST succeed as a theory of the strong force.
This implies indirectly that it may never succeed as a Theory of everything.

10. **Peter Woit**
October 19, 2007

Thanks Hans,

I did see that, it's a nice article about developments in AdS/CFT. But my reading of it was that it was very careful to not take any position at all about the controversial issue of whether string theory has failed as a TOE.

11. **zorba**
October 19, 2007

Not directly relevant, but wasn't tbere a big meeting about time at Columbia this week? Did you attend any of the talks?

12. **Peter Woit**
October 20, 2007

zorba,

The meeting wasn't at Columbia, but all the way downtown at the New York Academy of Sciences. Not really my kind of thing, I'm too busy this week, and they were charging a $150 registration fee, so I didn't consider going. They set up a website:

http://arrowoftime.org/

maybe at some point more info about the talks at the meeting will appear there.

13. **zorba**
October 20, 2007

Oh, sorry, whenever I see Brian Greene I think Columbia….yes, $150 is ridiculous.
Anyway I note that Sabine H. was there, and she may write a report on what she heard [about time, not about drunken physics celebrities throwing bread rolls about in the manner of Bertie Wooster……]
Thanks anyway!

Susskind Joins PI

October 16, 2007
Categories: Uncategorized

I haven't been able to confirm Lubos Motl's claims that the Perimeter Institute offered him a job, but yesterday they announced that Lenny Susskind has accepted an offer to join them as an associate member. According to the announcement, this means that "he will spend focused time at PI each year to conduct research activities."

At the Frankfurt book fair, Backreaction's Stefan Scherer took a picture of one of the displays, that featured a large poster advertising Susskind's forthcoming book *The Black Hole War*, which carries the subtitle "My battle with Stephen Hawking to make the world safe for quantum mechanics".

Update: Marcus at Physicsforums points to this interview with Susskind about his forthcoming book:

> For two decades an intellectual war took place between Stephen Hawking, on the one side, and myself and Gerard 't Hooft on the other. The book is about the scientific revolution that the controversy spawned, but also about the colorful personalities and the passions that gave the story its drama. The story starts in Werner Ehrhardt's Mansion in San Francisco, and eventually passes through all seven continents, including Antarctica.

Comments

1. **Who**
October 16, 2007

"My battle with Steven Hawking to make the world safe for quantum mechanics".

what a ham. Hawking as well.

2. **Steven H. Cullinane**
October 16, 2007

That's "Stephen," with a "ph."

3. **Peter Woit**
October 16, 2007

Steven,

Thanks, fixed.

4. **Who**
October 16, 2007

I know, I was just copying how Peter had it.
Given that Susskind once posted something about "Sting Theory" on the arxiv, maybe a little misspelling is appropriate.

5. **Steven H. Cullinane**
October 16, 2007

As noted in a previous comment of mine containing a misspelling, we must all sometimes "bow down before the great god Entropy."

6. **Quixotik**
October 16, 2007

Wow, now theoretical physicists have feuds like rappers.

7. **H-I-G-G-S**
October 16, 2007

Yes Quixotik, and for the same reason: marketing.

8. **stefan**
October 16, 2007

Hi Peter,

thanks for mentioning my visit to the Frankfurt Book Fair – I was curious if anyone would notice that detail of the photo ;-).

The publisher is Little, Brown and Company in the Hachette Book Group USA. I cannot say anything more about the book – the stand was quite deserted on Sunday afternoon, with all books already packed away. On the Hachette Group website, the title is not announced yet. For comparison, the "Geography of Bliss" is scheduled for January 2008, so the "Black Hole War" will probably appear later in 2008.

9. **Greg Egan**
October 16, 2007

> Wow, now theoretical physicists have feuds like rappers.

It would be a brave physicist who declares "If my book doesn't sell more copies than Hawking's *A Brief History of Time*, I'll stop writing papers as lead author and just make guest appearances as pencil-sharpener and LaTex checker for other physicists. So buy ten copies, vote with your wallet, my genius in on the line …"

10. **Chris Oakley**
October 16, 2007

The LS book could be one of a series. Peter: why not write sequel to N.E.W. with a title like:

The Whacko Control War: My battle with Leonard Susskind to make the world safe for particle physics theory

11. **Anders R**
October 16, 2007

leonard susskind 2: the reckoning

12. **Peter Woit**
October 16, 2007

Chris,

Putting Hawking's name on the cover sells books (I should have found a way to work him into the subtitle…). I don't think Susskind's name would have a similar effect.

13. **Quixotik**
October 16, 2007

How did Hawking get so famous anyway? Was it his book, or was he famous before?

14. **LDM**
October 16, 2007

People who read this blog could get the wrong impression. We actually do physics because it is tremendously interesting and intrinsically worthwhile.
Publishing highly speculative work, and then using Hawking's celebrity to OVERSELL it to a scientifically unsophisticated general public, is not a reputation most of us would want. There's nothing wrong with creative and speculative ideas of course, only they must be presented in a balanced and objective manner.

15. **Holmes**
October 16, 2007

The strange thing about the "black hole war" is that nobody but Susskind seems to have noticed it. I mean, of course there has been extensive discussion about whether black hole evaporation violates unitarity, but that has centered on Maldacena's work; most people never heard of "black hole complementarity" until Bousso recently started mentioning it to motivate "observer complementarity". The whole "war" must have been fought in the dark places of the world, far below the world of ordinary physicists……

16. **Aaron Bergman**
October 16, 2007

I mean, of course there has been extensive discussion about whether black hole evaporation violates unitarity, but that has centered on Maldacena's work;

Do I really have to say that unitarity in black hole evaporation is a subject that long predate's Maldacena's work? Recent discussion has centered on Maldacena's work because it finally provides a concrete way to address the question.

most people never heard of "black hole complementarity" until Bousso recently started mentioning it to motivate "observer complementarity".

Speak for yourself.

17. **Anders R**
October 16, 2007

well he said most people so he isn't necessarily saying that you're one of them or himself for that matter.

18. **Holmes**
October 17, 2007

Of course there were papers before Maldacena came along. They were however [a] nearly all by Susskind himself and [b] largely ignored. Hardly what one would call a war, dividing families, devastating innocent civilians, etc etc etc.

19. **John Baez**
October 17, 2007

Quixotik wrote:

> How did Hawking get so famous anyway? Was it his book, or was he famous before?

Among physicists he was famous for calculating how much entropy a black hole has, and before that, for showing (along with Penrose) that black holes are inevitable under certain conditions — the singularity theorems.

Some of this fame percolated down to the general public, and his disabilities helped make him a fascinating character.

But I guess his book was the first thing he wrote that ordinary people could pretend to understand… so it became a best-seller, and he's been a lot more famous ever since.

20. **Hans de Vries**
October 17, 2007

Somehow this gives me a strange pro-wrestling association…

The battle of
"the Master-of-the-Multiverse"
versus
"the Black-Hole-Evaporator"

21. **chimpanzee (aka "joe")**
October 17, 2007

Re: Book Cover “marketing”

I’m sure the publisher pressed for the “Hawking” reference, the need a “god-like” figure. “You are who you associate with”, as the saying goes. Just like L. Lederman’s book “The God Particle”, need to tantalize the gullibe (god-fearing) public. Just like G. Smoot/COBE

> Much of the excitement outside of the scientific world stemmed from Smoot’s comment at the press conference that “if you’re religious, it’s like seeing God.”

Just like Morris Kline (interestingly a NYC native like L. Susskind):

> As the author’s wife and sometime secretary, I can testify that Morris Kline was keenly unhappy with the publisher’s choice [“Why the Professor Can’t Teach”]. This book is not an attack on professors but is rather a wide-ranging critique of undergraduate education. Indeed an appropriate, less jazzy title would have been A CRITIQUE OF UNDERGRADUATE EDUCATION.

The above is a really bad case of “sensationalism”, & how scientists have to be really wary of dealing with the media.

I like the way M. Franklin/Harvard puts it:

“Why the NY Times doesn’t get the right spin on on our data”

The “almighty dollar” rules (Law of Business..getting market share):

“People don’t buy Good Products, they buy GOOD MARKETING”

> If you’re going to get caught up in the commercialization of mass culture of excitement, you’re never going to make it. [“Stupidity is Its Own Reward”] & too many people
> get caught up in it,.. You have to be independent individualist, & committed to excellence

— Steven Gould/Harvard

Someone on this blog objected to “pandering to the masses” with high-end Science. I agree. Diluting Science for the purpose of public outreach CAN be dangerous. Timothy Ferris (science writer) is notorious for this, his recent PBS program was a JOKE. Roald Hoffman/Cornell (Nobel Laureate in Chemistry) had a negative comment about Pop Culture & the Masses (PBS TV program).

22. **chimpanzee (aka "joe")**
    October 17, 2007

    Here is the link for the Science Channel program “Hawking Paradox”. Werner Erhardt, et al. I saw the program last year, & it’s been repeating regularly.

    I met & talked with L. Susskind at last year’s SUSY ’06. Very approachable, polite, & willing to talk. Very professional appearance & good speaker. I got the same impression during the TV program. His presentation/exposition is very polished, gives a good public impression of a scientist. Classy. Here’s a good article about him, which makes me like him even more.

    I called L. Motl prior to the conference, for background prep (I’m an outsider, my degrees are in Elec Eng). He was very generous with his time (J. Preskill/Caltech & only a few others have been this extending), & was impressed. Other people have commented on his dedication to students/teaching & charm. Just by accident, I mentioned Lubos (who asked me to say hi to his Rutgers PhD advistor) to Lenny & I got a very interesting response. (reminds me of the fights I had during my PhD days, when my arch-rival called me a “monkey”) I didn’t realize they had a “history”!

23. **Holmes**
    October 17, 2007

    Oh, so now its a “scientific revolution” and not just a mere war. What will LS call it when somebody finally manages to settle the question of unitarity violation for evaporating black holes that look *even vaguely* realistic? The war to end all wars? The mother of all revolutions?

    By the way, the notion that Maldacena’s work was somehow “spawned” by LS’s early-90s lucubrations is, shall we say, stretching a point just a tad.

24. **nerd**
    October 18, 2007

    “Among physicists he was famous for calculating how much entropy a black hole has, and before that, for showing (along with Penrose) that black holes are inevitable under certain conditions — the singularity theorems.” – John Baez

    Not exactly an experimentally confirmed calculation. All theorems based on GR are based to some extent on faulty (classical) approximation, what you need to do is find quantum gravity, and then calculate black hole properties using that.

    The business of calculating details of black holes that aren’t checked yet reminds me of Sir James Jeans’ calculation “proving” that the solar system formed from giant tidal waves on the sun, with popular hype of Jeans (who also wrote popular best-sellers back in the 30s, like Hawking today), and you got popular notions that “the solar system exists, so Jeans must be right!” Wrong, there were subtle errors.

    It’s disappointing for black hole calculations that have yet to be experimentally confirmed in detail, to be hyped and celebrated; kinda misleads people about what is fact and what is speculation.

25. **Bee**
    October 18, 2007

    @ Chimpanzee:

    > “People don’t buy Good Products, they buy GOOD MARKETING”

    One of the sures signs for intelligence is the ability to learn from and avoid mistakes. How come then that a significant fraction of the homo sapiens sapiens on this planet today lives in a society that runs in a downward spiral of deception, lies, and ignorance of facts. How come that intelligent people can point out weaknesses and potential dangers of dramatic failures of the current system, and other allegedly intelligent people don’t even care to listen, because they are too busy worrying about their own career – and how ironic is that if exactly this was the point of criticism.

@Quixotik:

> How did Hawking get so famous anyway? Was it his book, or was he famous before?

The Large Scale Structure of Spacetime
Stephen Hawking and George Ellis, Cambridge University Press (1973)

not exactly a good book (i.e. as you find on wikipedia it's indeed "highly technical and quite unreadable"), but it's one of these books one kind of needs to have. later then (75) the black hole evaporation stuff.

26. **Shantanu**
October 20, 2007

Peter, did you go for

this workshop. If so any report?

27. **Peter Woit**
October 21, 2007

Shantanu,

See my comment about this on the other thread. Evidently Bee was there, see her report at Backreaction, where she promises to write more…

28. **cecil kirksey**
October 22, 2007

Peter:
I was rereading parts of Lenny's book the other night. Near the end he recalls a dinner talk he gave at Strings 1995 about what theoritical physics would be like now based only on the experimental data available up to 12/31/1899. He paints a fairly rosy picture about the abilities of pure thought. Granted both SR and GR probably would have proceeded unabated. But would they have been accepted? Lenny's commit spurred me to start looking at the history of QM in particular QED. A correct theory may…may have been found but so would a lot of equally viable ideas that would be wrong. The correct theories were accepted based on experimental data not the beauty, nicity, the theoorist or other subjective qualities. I think Lenny was wrong then and may be wrong now.

Any comment from anyone on the history of QM and QED?

29. **George Lehtola**
October 22, 2007

From PI's home page:

Leonard Susskind Joins PI
WATERLOO, ON, October 15, 2007 – Canada's Perimeter Institute for Theoretical Physics (PI) is pleased to announce that renowned scientist Leonard Susskind has joined its Faculty as an Associate Member. Professor Susskind is widely recognized as one of the most highly creative researchers in the field of particle physics. He earned his BSc at City College of New York and his PhD in 1965 at Cornell University. He held a number of positions at the postdoctoral and faculty level afterwards before becoming a Professor in the Department of Physics at Stanford University in 1978, where he continues to work as a Professor of Physics.

Leonard Susskind has received a range of honours and prizes, including having been elected to the National Academy of Sciences (NAS), the American Physical Society's prestigious Sakurai Award, as well as the American Institute of Physics' Science Writing Award.

Rob Myers, Interim Scientific Director at PI, remarks, "Professor Susskind has been one of the most creative and influential theoretical physicists in the last four decades. He has contributed important ideas to topics ranging from the theory of quark confinement to black holes in string theory." As an Associate Member, Professor Susskind will spend focused time at PI each year to conduct research activities.

In addition to an outstanding record as a distinguished theoretical physicist, Professor Susskind has a demonstrated interest in communicating science to members of the general public and improving society's awareness of physics, astronomy, and allied science fields. He will share this talent by participating in a special PI Public Lecture panel discussion on December 5, 2007, dealing with the topic of 'Information and Reality'.

About Perimeter Institute
Canada's Perimeter Institute for Theoretical Physics is an independent, non-profit, scientific research and educational outreach organization where international scientists cluster to push the limits of our understanding of physical laws and calculate new ideas about the very essence of space, time, matter and information. The award-winning research centre provides a multi-disciplinary environment to foster research in areas of Cosmology, Particle Physics, Quantum Foundations, Quantum Gravity, Quantum Information and Superstring Theory. The Institute, located in Waterloo, Ontario, also provides a wide array of educational outreach activities for students, teachers and members of the general public across the country and beyond in order to share the joy of scientific research, discovery and innovation. Additional information can be found online at http://www.perimeterinstitute.ca.

30. **Who**
October 22, 2007

Cecil, this sounds like it might be from some other book by Susskind than the one we were talking about. Could it be from "The Cosmic Landscape: String Theory and the Illusion of Intelligent Design" ?

> I was rereading parts of Lenny's book the other night. Near the end he recalls a dinner talk he gave at Strings 1995 about what theoritical physics would be like now based only on the experimental data available up to 12/31/1899…

31. **Physics Professor**
October 22, 2007

Hello all,

I've been following this blog for awhile.

What exactly has Lenny Susskind done in the realm of physics?

I know it says, “Leonard Susskind has received a range of honours and prizes, including having been elected to the National Academy of Sciences (NAS), the American Physical Society’s prestigious Sakurai Award, as well as the American Institute of Physics’ Science Writing Award.”

But what has he done? The Cosmic Landscape seemed highly speculative. Was there physics in it that I perhaps missed?

32. **Peter Woit**
October 22, 2007

Physics Professor,

No, The Cosmic Landscape is pretty pure pseudo-science, but until recent years Susskind was doing very serious science. He does have a legitimate claim to have been one of the first to realize that the Veneziano amplitude could come from quantizing a string. The later work of his I’m most familiar with is the way of handling fermions in lattice gauge theory that goes under the name “Kogut-Susskind” fermions.

Unfortunately, he’s not the only seemingly smart, reputable physicist who seems to have taken leave of his senses the last few years…

33. **Aaron Bergman**
October 22, 2007

SPIRES is your friend.

34. **LDM**
October 22, 2007

cecil kirksey,

I am glad you brought up your question…I too read Susskind’s book when it came out found that chapter interesting.

In particular, Susskind says in The Cosmic Landscape, page 269, the following:

Could theorists have guessed the full structure of the Standard Model? Protons and neutrons, perhaps, but quarks, neutrinos, muons, and all the rest. I don’t see any way that these things could have been guessed

Susskind is correct, since as the history of HEP shows, experiment was essential. Unfortunately, Susskind does not emphasize the point, when in fact it is probably one of the more important statements in the book. If theoreticians could not have guessed the Standard Model without experiment, then a similar statement regarding the final form of a string theory TOE is not unreasonable, and we need only remember string theory has no experimental tests, and is likely never to have any.
It is one thing to make the largest extrapolation in history, but when doing so, at least have the honesty to explain to the laymen that while an *interpolation* of data tends to be safe, *extrapolation* of data is, from a scientific point of view, very dangerous, and often wrong.

35. **Cecil Kirksey**
October 22, 2007

To Who:
LDM has it correct. The book I was referring to was “The Cosmic Landscape”. I was still left with the impression that Lenny fells that the correct model for QM and QED could have been guessed given no new data in the 1900’s. Given the fact that not every physicist took the idea of a quantized EM particle as postulated by EA seriously until the Compton effect was discovered, it is hard to believe that QM and QED could have been developed to the point that they were even up to say 1948-49.

Maybe someone who better understands the historical development of QM and QED can respond.

36. **JC**
October 23, 2007

Cecil Kirksey,

Without any experimental data, most likely QED would have been thrown into the trash as soon as the first calculations were done which (naively) produced infinite quantities. There would have been very little to no motivation for renormalization, if there was no Lamb shift experimental data.

Future Linear Colliders

October 23, 2007
Categories: Experimental HEP News

There are two highly active projects to design a linear collider that would collide electrons and positrons at energies higher than those achieved at LEP. Recently there were workshops discussing the state of the projects.

The ILC is the farthest along of the two and uses more conventional technology. It is a design for a 250+250GeV collider, upgradeable to 500+500Gev. There's a very jazzy new web-site aimed at selling the idea to the US public. This week Fermilab is hosting a workshop on the ILC, talks available here. Michael Peskin gave an introductory talk with an unfortunate title ("The Physics Landscape", I really think serious physicists should not be reminding people of this, especially when they're making a pitch to the public for money). He argues that the LHC will see a spectrum of new particles (in order to solve the hierarchy and dark matter problems), and motivates the ILC as the machine to study these. This of course depends on their existence, at a mass low enough to allow production at the ILC, but high enough to evade bounds from LEP and the LHC (for some new results from the latter, see here). It now appears certain that no decision about building the ILC will be made until results are in from the LHC (2010?) that will resolve whether there are new particles in the mass range that the ILC is capable of studying.

DOE's Ray Orbach gave a talk about the ILC project, emphasizing:

> It is critical that planning for the ILC takes into account the realities of the funding situation, the need to formalize the ILC arrangements between governments, the changing scientific landscape, the scientific capabilities at other facilities, and the health of our national scientific structure.

Orbach seems concerned that the ILC project does not have a realistic schedule ("I judge that these arrangements will require more time than the currently proposed schedule of the GDE") , and does not have commitments from other countries. I'm guessing that he sees financing it out of the current and expected DOE budget is not doable without large contributions from other countries, and a relatively long time-frame. He emphasized that there is now a well-defined process for projects like this: they have to survive a series of "critical decisions". The ILC is not yet ready for the first critical decision "CD-0, Mission Need" and won't be until after the LHC results are in. He also mentions "other planned international projects", and the importance of not duplicating their activities (I take this to be a reference to CLIC). Finally, he is critical of the plan of the ILC project to move to an "Engineering Design Report" that would give detailed engineering plans for the machine, since he sees it as still in an R and D phase.

Over at CERN, the Resonaances blog reports on a workshop devoted to CLIC, a more ambitious and less technologically developed plan for a 1500 + 1500 Gev collider (upgradeable to 2500 + 2500 Gev). If CLIC really turns out to be feasible, and buildable on a time-scale close to that of the ILC, it will not be possible to justify building the ILC, since it would operate at much lower energy.

This week internet access is more iffy, since I'm in Lisbon, for a conference late in the week on "Is Science Near Its Limits?", sponsored by the Gulbenkian Foundation. After it is over, I'll write about it here, and I think I can post a copy of the talk I'll be giving.

Update: Science magazine has a short piece about the Orbach talk, see here:

> Orbach said physicists must follow the department's protocol that requires a large project to pass five critical decision milestones. The ILC has not passed the first, which allows researchers to proceed from basic R&D to design, Orbach said. Previously, DOE officials had been "completely open" to a less formal approach, says Caltech's Barry Barish, who leads the design team. What counts as "engineering design" remains to be determined, he says.

Another piece of this story recently pointed out to me is that the new CERN director is from DESY and associated to the ILC project there. This might cause people at CERN to wonder how hard he'll push for its CLIC projector, which is in some ways an ILC competitor.

Comments

1. **Bee**
   October 23, 2007

   Ah, commitments! How about we just design the next virtual collider and put it on Second Life? Who needs the ILC? 😉 Have a safe trip,

   B.

2. **dan**
   October 23, 2007

   Hi Peter, just a quick question, to revisit an issue before it got derailed,

   you don't think that there is a "hierarchy" problem (requiring SUSY as the antidote to radiative corrections to the higgs, and hence, string theory) I am curious as to whether you think there is a higgs boson and higgs field, and what prevents it from gaining mass from the EW-scale to the GUT scale. Do you think that the higgs boson is fined tune to 32 decimal places and that is the end of that – no SUSY required?

3. **Peter Woit**
   October 23, 2007

   dan,

   This is off-topic, and I don't really want to start yet another discussion of supersymmetry, but, in brief, I've never been convinced by the "supersymmetry eliminates the need for fine-tuning argument", for several reasons:

   1. We don't even know there is a GUT scale. There is zero evidence for such a thing.

   2. We don't know that the Higgs field is an elementary scalar.

   3. If supersymmetry really was going to eliminate all fine-tuning, it would have shown up by now.

4. **a quantum diaries survivor**
   October 23, 2007

   Wow Peter,

for an answer to an off-topic comment you did pretty good 🙂 Let me add my own:

4. Speculating susy exists but was not detected so far because there was some symmetry breaking mechanism making susy particles much more massive than ordinary ones is legitimate, but it stinks. It flies in the face of Occam's razor, which becomes nervous and comes a-slashing.

Instead, why not giving some more tentative credit to Alejandro Rivero's idea that sparticles (bosons) are composites of two fermions ?

Cheers,
T.

5. **Peter Woit**
   October 23, 2007

   Hi Tommaso,

   Yes, supersymmetry is a beautiful idea, until you have to break it…

   The problem with composite models is you have to find some dynamics that doesn't screw up the fact that the standard model works perfectly. The idea that the Higgs field is a composite field of a fundamental fermion field is an old one, well motivated by what happens in superconductivity (Cooper pairs). Technicolor is one way of doing this. People have looked at this, but maybe if they had put as much effort into is as into SUSY, other better ideas might have shown up.

6. **JC**
   October 23, 2007

   It would surely liven up the field again, if the Higgs mechanism, supersymmetry, technicolor, etc … and all of their variations + combinations, all turned out to be complete failures in the end.

   If this indeed turns out to be the case, then I would have no idea offhand what would be a fruitful path to follow afterwards. I suppose one could skim mindlessly through older theoretical particle and/or condensed matter journals, searching for long forgotten ideas to resurrect and try out.

7. **Coin**
   October 23, 2007

   Woit, just to be clear– are you saying you consider it probably the case that the Higgs is *not* an elementary scalar? Or are you just saying that an elementary scalar nature of the higgs cannot or should not be *assumed?*

   Also, more on topic, kind of a dummy question: Why is it that all the big "vacuum cleaner" accelerators, like the LHC and Tevatron, seem to be cyclic/synchrotrons– whereas the new proposed colliders we're seeing described in this post for the lower-power but more "fine-tuned" measurements are all linear? Is there something about a linear collider that makes the fine-resolution stuff easier to do than it would be with a synchrotron? Or what?

8. **Peter Woit**
   October 23, 2007

   Coin,

   Above LEP energies, synchrotron radiation loss is just too high for electrons (goes as fourth power or the energy), so you have to have a linear accelerator.

   I don't know if the Higgs is an elementary scalar or not. If I had to guess, I'd guess not (not asymptotically free, for one thing…)

9. **Coin**
   October 23, 2007

   I see, thanks.

10. **Eric**
    October 23, 2007

    In reponse to Peter's answers to Dan's questions:

    1. It doesn't matter if there's a GUT scale or not. Quantum corrections to the Higgs mass will push it to whatever the next energy scale is, and if there is no GUT scale then this would be the Planck scale.

    2. It's true that the Higgs might be a composite boson. However,
    such Technicolor models were studied very seriously in the 70's and 80's, but were eventually rejected due to some serious shortcomings. Namely, in order to generate mass hierarchies for the SM fermions, one ends up with serious problems with FCNC's. Nevermind that it requires adding another gauge sector to the SM with strong dynamics which would have led to a plethora of technimesons for which there is absolutely no evidence. Occam's razor anyone? Oh right, we only reserve this for things we don't like like SUSY.

    3. The amount of fine-tuning required in the Higgs potential in order to give a large enough Higgs mass is very small and perfectly reasonable. In any case, what Peter's means is that to eliminate all fine-tuning, the Higgs should have been observed by now, not that the superpartners should have already been observed.

11. **Paul Guinnessy**
    October 23, 2007

    Keep your eyes peeled for the November issue of Physics Today, which has an interview with Aymar on the issue of which project CERN will support, CLIC or ILC. Toni Feder also has a good piece on Fermilab's project X (doi:10.1063/1.2800091) in the October issue.

12. **Tony Smith**
    October 23, 2007

    Peter Woit said "… If CLIC really turns out to be feasible, and buildable on a time-scale close to that of the ILC, it will not be possible to justify building the ILC, since it would operate at much lower energy …".

    So, in CLIC's favor is its higher energy.

What about cost?

If ILC were to cost on the order of USA2007$10 billion,
how would that compare with the cost of CLIC ?

If the USA dollar were to continue to decline with respect to the Euro,
how would that affect the cost comparison ?

How would site location (Illinois, Europe, or Asia) affect the cost comparison ?

If CLIC were to be built in Europe or Asia, would the USA get to use it on terms as favorable as the USA has with respect to use of LHC ?

Tony Smith

13. **Tony Smith**
October 23, 2007

Slide 38 of the file CLIC07_JPD.pdf from the CLIC workshop web site shows:

CLIC Old Parameters
Accelerating field – 150 MV/m
RF frequency = 30 GHz
Total cost (a.u.) = much higher than 2

CLIC New Parameters
Accelerating field – 100 MV/m
RF frequency = 12 GHz
Total cost (a.u.) = 1.15

What does a.u. mean ?
(is it effectively a comparison with the cost of the ILC?)

Tony Smith

14. **Coin**
October 24, 2007

What does a.u. mean ?

"Astronomical Units"? 🙂

15. **Peter Woit**
October 24, 2007

Tony,

I don't think anyone has a good estimate of what CLIC will cost. From what I remember, the size they are talking about is similar to the ILC, presumably based on the fact that that should make the cost somewhat similar. Not sure if exchange rates make much difference, although I suppose if the US currency collapses and we become a third-world country, it will be much cheaper to build something here. No matter where it's built, there will be physicists from all countries working on it.

Paul,

Thanks for the heads-up, looking forward to hearing what Aymar has to say about this. I get the impression that it's a delicate issue, with people on both sides choosing their words carefully…

Eric,

We also don't know that the Planck scale is relevant…

See Distler's explanation

http://golem.ph.utexas.edu/~distler/blog/archives/000336.html

of why current bounds on the Higgs mass imply a very heavy stop, and amounts of fine-tuning that are not "very small".

16. **AGeek**
October 24, 2007

Eric, you'd better read http://arxiv.org/abs/0708.3550

17. **conrad**
October 24, 2007

> and if there is no GUT scale then this would be the Planck scale.

Why? Is the desert (susy or not) a well established experimental fact? Has a new physics threshold at, say, 30TeV, been definitively ruled out?

In any case, is it true that we have everything figured out all the way up to the GUT scale, so if it's not there then only the Planck scale is left?

18. **Eric**
October 24, 2007

Conrad,
The expected next scale between the electroweak and GUT scale is the SUSY breaking scale which should be around 1 TeV in order to stabilize the Higgs mass. It's

always possible, in fact likely, that there are hidden sector gauge groups which become strong at some higher scale. However, this would have no effect on the quadratic divergence to the Higgs mass since the Higgs states are doublets of the SM SU(2) and not of the hidden sector groups. Regarding Peter's comment that there is no evidence that the Planck scale is relevant, it doesn't matter. That would just mean there is no cutoff and the Higgs mass becomes infinite.

As far as the fine-tuning in the Higgs potential, this is presently a few percent at most. The problem is if the lower limit on the Higgs mass gets pushed up much beyond 114 GeV, say to around 130 GeV. If the Higgs mass is in the range 114-121 GeV as expected, then the fine-tuning is not a real problem. Also keep in mind that this depends strongly on the top quark mass, which was recently revised downward to ~171 GeV.

19. **Yatima**
October 24, 2007

> Eric, you'd better read http://arxiv.org/abs/0708.3550

Picture a particularly vast, featureless Zen Garden with a gnarly stone at one end. Now meditate.

20. **Andr**
October 25, 2007

T. writes

"4. Speculating susy exists but was not detected so far because there was some symmetry breaking mechanism making susy particles much more massive than ordinary ones is legitimate, but it stinks. It flies in the face of Occam's razor, which becomes nervous and comes a-slashing."

Let me write an analog of this, frequently heard in the seventies:

"4. Speculating gauge symmetry exists but was not detected so far because there was some symmetry breaking mechanism making gauge particles much more massive than ordinary ones is legitimate, but it stinks. It flies in the face of Occam's razor, which becomes nervous and comes a-slashing."

Who turned out to be right then?

21. **Thomas Larsson**
October 25, 2007

U(1) gauge symmetry, i.e. electromagnetism, was understood in the '70s, right? At least in the 1970s. A better analog from the 1870s would be the ether wind: people speculated it existed but was not detected.

22. **moron**
October 25, 2007

Andr, I thought gauge symmetry SU(2) is based on simplicity and elegance and describes the weak force well, and predicted the masses of the W and Z massive gauge bosons in advance of experimental discovery? The reasons I was taught for why the gauge symmetries of the SM are physics (unlike susy) is that they

(1) come from experimental observations of fundamental forces and fundamental particles,

(2) are the simplest known ways to represent physical facts,

(3) predict reaction cross-sections, etc., that make it useful,

(4) predicted W and Z gauge bosons observed at CERN in 1983.

So I don't understand what you are saying. Either I'm a moron or you don't know the difference between fact and fantasy (susy).

Now if you are going to defend susy by saying some really great discovery in physics was suppressed, you need an analogy to a religious belief system that can't make quantitative falsifiable predictions. Try the analogy of susy to epicycles.

23. **conrad**
October 25, 2007

Moron,

what Andr says is correct, both historically and factually. Historically, Weinberg proposed SU(2)XU(1) because it was the simplest possibility consistent with phenomenology, but he didn't know whether it was the correct group. After his famous "model of leptons" appeared, many people (including Weinberg himself, you may want to check Phys. Rev. Lett. 38, 1237 – 1240 (1977) which appeared ten years after his model of leptons) started exploring other groups. It turned out that there are three massive weak bosons, and SU(2)XU(1) was the end of the story. But, also, many people didn't believe in intermediary bosons — until they were experimentally observed. Something similar happened with quarks, gluons, jets, etc, etc

Factually, there is no reason why SUSY shouldn't be there. There is also no reason, IMHO, why it should be. The only consistent argument I've seen for SUSY is naturalness, and I personally don't buy it. But just like with weak bosons, experimentalists will hopefully resolve the mystery soon.

Eric,

my answer to your post was rejected by the server. In any case, thanks for your detailed answer.

24. **moron**
October 25, 2007

'Factually, there is no reason why SUSY shouldn't be there. There is also no reason, IMHO, why it should be. The only consistent argument I've seen for SUSY is naturalness, and I personally don't buy it. But just like with weak bosons, experimentalists will hopefully resolve the mystery soon.'

conrad, the weak gauge bosons were predicted to have a mass around 80-100 GeV.

When you are looking for sparticles 'predicted' by susy, you don't have any predictions of what energies they should have.

Therefore, experimentalists who fail to find sparticles below 1 TeV will then have to continue trying at ever higher, arbitrarily higher in fact, energies. At what point do you say that experiments have finally 'tested' susy? You can go on testing a theory which makes no falsifiable predictions for eternity.

Testing susy is like testing ESP or religious miracles: if you fail after one effort, you can simply go on and on forever. That's not science. It's more like religion. You pray for experimental confirmation by a miracle, but if it doesn't occur, you can keep on trying and believing in the speculations. That's why falsifiability is the criteria that separates religion from science.

25. **Mo**
October 25, 2007

Moron,

You're way off here. First off, the fact that a symmetry should be broken is no argument against having it in the theory. MANY symmetries which we know and love are broken, either by the vacuum or else by excitations.

Second, obviously SUSY is falsifiable as a solution to the hierarchy problem (in fact, it's already under pressure). If you give that up, then you loose one of the strongest motivations for SUSY. Also, SUSY has a DM candidate. It should be possible to determine whether the DM in our universe is the lightest stable SUSY partner. If it's not, then that motivation could get killed too. If these two motivations get killed then a lot of people will loose interest. But, of course, the theory has not strictly been ruled out. SUSY is very predictive, I'm not sure why people here are so keen to argue otherwise. (Also, SUSY is not string theory. We could have the MSSM quite independently of whether or not it's UV completed to some string model.)

If we stuck to theories which are falsifiable in the sense which you're asking for, then we wouldn't have much left to work with. According to your philosophy scalar-tensor gravity is on the same logical footing as religion. You're entitled to think that, but it certainly doesn't reflect mainstream physics.

26. **Eric**
October 25, 2007

Moron,
I should like to add that the superpartners must be less than or equal to the TeV range in order to explain the hierarchy problem, which was mentioned but not emphasized by Mo above. If LHC doesn't see the superpartners, then it means that SUSY is not the solution to the hierarchy problem. Supersymmetry could still be a part of nature even in this case, but then most of the interest in it will evaporate because it would not have any relevance for low-energy physics. The thing to keep in mind is the saying of Feynmann that in quantum mechanics anything which is not strictly forbidden is compulsory. If nature doesn't incorporate supersymmetry, it would strongly violate this principle.

27. **Arun**
October 25, 2007

The thing to keep in mind is the saying of Feynmann that in quantum mechanics anything which is not strictly forbidden is compulsory. If nature doesn't incorporate supersymmetry, it would strongly violate this principle.

This is the most confused thought I've seen around here in a long time. Feynman is turning at 60K RPM in his grave at the extrapolation from

"What is not forbidden in QM is compulsory" to
"What is not forbidden in nature is compulsory".

28. **anon.**
October 25, 2007

The thing to keep in mind is the saying of Feynmann that in quantum mechanics anything which is not strictly forbidden is compulsory. If nature doesn't incorporate supersymmetry, it would strongly violate this principle.

This is the most bass-ackwards reasoning I've ever seen, and a complete misunderstanding of the "totalitarian principle" (which I think is usually credited to Gell-Mann, not 'Feynmann').

29. **Eric**
October 25, 2007

Dear Arun and Anon,
Obviously, neither of you knows anything at all about quantum mechanics. If there is an allowed symmetry of your Lagrangian, it will be realized in nature. End of story.

30. **Andrew**
October 25, 2007

Eric,

So what you're saying is if I wrote down a Lagrangian with a U(17) symmetry it would be realised in nature, because that's allowed by my Lagrangian? I'm afraid not, just take a look at SU(5).

31. **Arun**
October 25, 2007

Eric, quite entertaining!

32. **Eric**
October 25, 2007

Andrew,
You misunderstand me. What I'm saying is that there is a fundamental symmetry between fermions and bosons that will be a symmetry of any Lagrangian which includes fermions and/or bosons regardless of the gauge group.

33. **Thomas Larsson**
October 26, 2007

> Obviously, neither of you knows anything at all about quantum mechanics. If there is an allowed symmetry of your Lagrangian, it will be realized in nature.
> End of story.

Putative terms in the Lagrangian compatible with the symmetries will be generated by radiative corrections and are hence compulsory. This does not imply that any symmetry principle is compulsory.

34. **Eric**
October 26, 2007

Well, I would expect the symmetry between fermions and bosons to be as compulsory as Lorentz symmetry. In any case, it's the radiative corrections involving fermionic partners which stabilizes the Higgs mass…

35. **Andrew**
October 26, 2007

Eric,

If I misunderstood you, it is because you made a generic statement that anything not disallowed is compulsory. Now you seem to be saying something else. OK, thanks for the clarification. So why is the symmetry between bosons and fermions inevitable? You say yourself that you merely expect it (i.e. can't prove it), so why such high confidence in this symmetry?

String Theory’s Next Top Model

October 25, 2007
Categories: This Week's Hype

I’ll write soon about the conference I spoke at today in Lisbon. A few hours ago I participated in an interesting discussion about some of the issues around string theory with two string theorists. One of them was quite vehement that a big part of the problem is things being hyped to the media, and that this is an American disease, something that doesn’t happen to the same degree in Europe.

I think he may be right, since I certainly haven’t noticed as much media hype (either pro or anti-string) from European sources, although I follow US ones more closely. When I got back to the hotel this evening, I noticed that SLAC is promoting on its web-site a news story about [String Theory’s Next Top Model](). The story appears to be about this [paper]() that was just published in Physical Review D. In it, the authors consider three toy models of inflation in string theory and find that they don’t work. Their conclusion:

> This may be an artifact of the simplicity of the models that we study. Instead, more complicated string theory models appear to be required, suggesting that explicitly identifying the inflating subset of the string landscape will be challenging.

So, the gist seems to be that they went looking for toy models of string inflation, didn’t find a workable one, but decided that this was worth a SLAC press release, presumably because “string theory is currently the most popular candidate for a unified theory of the fundamental forces”, so one should go to the press with any result one gets, even if negative. I think the Europeans may be right that this sort of thing doesn’t happen here….

Comments

1. **Who**
   October 25, 2007

   This is how the New Scientist handled that same paper on 11 September.

   http://space.newscientist.com/article/dn12628-can-string-theory-accommodate-inflation.html

   They quoted Mark Hertzberg (I gather the lead author), Max Tegmark (a co-author), and Andrei Linde (who criticized the paper.) Fairly balanced job, to NewSci’s credit. Interesting paper raising interesting question.

   Also quoted Paul Steinhardt:

   > Paul Steinhardt of Princeton University in New Jersey, US, who helped to pioneer the theory of inflation, says the findings are in agreement with work that he and others have done using other versions of string theory.
   >
   > “I think the fact that it is difficult to combine inflation and string theory is very interesting,” he told New Scientist. “It could mean they are completely incompatible, which would force us to abandon at least one of them.”

   This time David Shiga at NewSci comes out looking better than whoever it was at SLAC public relations that talked to Kachru.

2. **DB**
   October 26, 2007

   String Theory Hype is not just an American disease, though clearly that’s where it started and reached epidemic proportions. Britain has also been infected, for years they have been “treated” to breathless expositions from the likes of Kaku and Greene, specially imported to create programs on the subject for BBC’s Horizon series, for example.
   Recall also that New Scientist is a British based publication.
   I don’t believe Continental Europe has succumbed to the same degree.

3. **moron**
   October 26, 2007

   A similar promotion of complexity as advancement happened with the theory of epicycles in about 150 AD. Everytime Ptolemy tried to predict something new with his model, it turned out wrong so more epicycles and more complexity was added to make it work.

   His reaction was always, “Great! I’m discovering more exciting knowledge about how complex nature really is!”

   Ptolemy would then announce the great news about his “Exciting Discovery of Complex New Epicycles” by a press release to the geeky *Novus Scientist* in Londinium.

   My Latin is a little rusty, but the gist of his final press release was: “Ptolemy’s mainstream earth-centred universe theory is obviously true. It’s led to some fantastic maths for working with epicycles, and it’s also the most popular idea around. It’s the only game in town. Aristarchus’s simplistic solar system is crackpotism.”

4. **Who**
   October 26, 2007

   To focus on what we’re looking at, the key paragraph in NewSci was

   The study was carried out by a team of researchers led by Mark Hertzberg of MIT in Cambridge, US. The team tried to produce inflation in three versions of string theory in which the extra dimensions are shaped like a doughnut – the simplest possibility. But they found that the conditions needed for inflation appear to be impossible to achieve in these simple versions.

   In the cases studied, the space required by strings cannot inflate. Which is serious because prevailing cosmology needs inflation to have occurred. What follows in the article are quotes taking various sides on the issue (from Andrei Linde, Max Tegmark, Paul Steinhardt…)

   Nothing as clear and forthcoming as this paragraph occurs in the Stanford press release. My guess would be that issuing the Stanford press release was the idea of Shamit Kachru, who may have chosen not to be quoted in the September article, but who may later have wished an opportunity to put his own interpretation on the group’s findings (without the inclusion of quotes from other authors of the paper).

5. **blogguy**

October 26, 2007

It doesn't appear to be a press release at all. I think it is just an article in the daily SLAC paper, which Peter has decided to call a press release. (I say this because there are profiles of recent work by other SLAC physicists regularly appearing on that site). It would be an odd press release, since it doesn't claim to have discovered something, only to concern a direction of work in progress.

6. **Peter Woit**
October 26, 2007

blogguy,

I actually first saw this at physorg.com

http://www.physorg.com/news112545705.html

a news site where most of the content is items put out by press offices of universities and labs. This article was put out by the SLAC press office, written by one of their staff. You are right though that it is not something they officially call a press release.

Who,

Having two press stories about a paper which quote different authors with opposite spin on the paper's results is kind of odd. It's also odd to have one, much less two press articles about a paper dealing inconclusively with toy models. But then again, the press coverage of string theory has often been odd.

My impression of how these things get written is that it is unlikely that Kachru contacted the SLAC press people asking them to write about this. More likely, what they do as part of their job is to call people at their institution up and ask them if they know of any work going on at their institution that they could write a story about. If a press person calls one up expressing an interest in writing about something one has done, most people won't turn them down.

7. **Stanford grad student**
October 26, 2007

More likely, what they do as part of their job is to call people at their institution up and ask them if they know of any work going on at their institution that they could write a story about. If a press person calls one up expressing an interest in writing about something one has done, most people won't turn them down.

I can personally attest to the fact that this is essentially exactly how many SLAC Today articles work. But SLAC Today is little more than a daily site-wide newsletter. It's handled by the press office, but it's not meant to be cutting-edge, breaking-news, press-release kind of science material.

8. **Coin**
October 26, 2007

> "I think the fact that it is difficult to combine inflation and string theory is very interesting," he told New Scientist. "It could mean they are completely incompatible, which would force us to abandon at least one of them."

So, I was going to ask if this could eventually somewhere bring us within striking distance of a "falsifiable prediction" for string theory (i.e., no inflation), but the same article seems to scuttle that hope:

> Andrei Linde of Stanford University in California, US… says the results only apply to a class of string theory versions called type 2a, which are irrelevant to the real universe because they have been shown to be incompatible with dark energy, the mysterious force causing the universe's expansion to accelerate.

So it looks like more than anything this is turning into another case where String Theory tells us you can have things one way if you choose one model, or another way if you choose a different model, and then there's some other classes of models where we aren't quite sure which way it goes…

On the other hand, if we conclude inflation is too precious to give up, could a serious program of trying to get toy-inflation out of other topologies besides the "doughnut" one finally allow us to "drain" some of that "swampland" I keep hearing about in the string theory landscape? It seems like I was hearing a bunch at one time about how the next step in the landscape program would be to get some of that swampland drained, but if there's been any progress in that regard I've somehow not managed to hear about it.

Does ruling out type IIA string theory, or one particular simple background geometry, rule out any quantifiable proportion of the string theory landscape (if we're assuming inflation)? (Or have we really not gotten anywhere at all, since it's possible that we were just not using a sufficiently complex variant of IIA string theory– as Tegmark suggests in the article to free type IIA from being ruled out on Dark Energy grounds– or possible we were using the wrong type of inflation? (Or for that matter, is it possible I'm just confused here and IIA is a different kind of string theory altogether than the one that produces the oft-discussed "landscape"?))

9. **Eric**
October 26, 2007

Coin,
Usually when talking about the landscape, people are talking about Type IIB flux compactifications, which are T-dual to Type IIA string theories. It's doubtful that they've ruled out inflation for all such possible compactifications. More likely, just for the simplest type of model. In any case, eternal inflation is a principle idea needed to populate the landscape. If most string compactifications cannot inflate, the one might expect the landscape to be lightly populated. In fact, inflation may be 'vacuum selection principle'. If we're lucky, there will be one and only one string vacuum where inflation can work, and this vacuum will contain the Standard Model. But this is just speculation…

10. **Coin**
October 26, 2007

Eric, thanks.

One question, would T-duality be enough to conclude that the Physical Review D results *do* apply to IIB flux compactifications? (Or at least whichever flux compactifications correspond under IIB to the one particular kind of calibi-yau manifold they considered in their IIA analysis).

11. **Annie**
October 27, 2007

I think the New Scientist article did a poor job. They spun the article unfairly, indicating that just because Hertzberg et al couldn't inflate those simplified models, string theory was in trouble. This isn't the case at all. The SLAC article points out their real goal–to find a simplified model for the purposes of understanding string theory and inflation. And as Stanford grad student says, the SLAC Today articles aren't press releases; they're intended for the SLAC community to let them (astrophysicists, particle theorists, engineers, students, construction workers, etc) know what's going on. PhysOrg takes any science article from SLAC Today and puts it on their page, without any prompting from SLAC.

Comment on Technicolor/Extended Technicolor Models

October 26, 2007
Categories: Uncategorized

Robert Shrock of Stony Brook sent me the following to post as a response to one of the comments on the latest posting. With his permission I'm putting it here as a separate posting, since I think it's a valuable informed summary of the current state of technicolor/extended technicolor models.

I would like to respond to Eric's recent comment on Oct. 23 in which he said that "technicolor models were..eventually rejected due to some serious shortcomings. Namely, in order to generate fermion mass hierarchies for the SM fermions, one ends up with serious problems with FCNC's." and that these theories "led to a plethora of technimesons, for which there is absolutely no evidence.".

While it is true that FCNC's are a relevant constraint on technicolor and extended technicolor (TC/ETC) theories and were viewed as very serious before the development of walking TC in the mid-1980's, they do not obviously exclude TC/ETC models where the TC sector has walking behavior. The walking (slow running of the TC gauge coupling over an extended range), results naturally from the presence of an approximate IR fixed point in the renormalization group equation for the TC gauge coupling. This walking has the effect of enhancing SM fermion masses for a fixed set of ETC breaking scales. Indeed, since the mid-1980's, the only viable TC models have been those with walking behavior. This walking allows one to use higher ETC breaking scales and still obtain the same SM fermion masses. It also enhances the masses of (pseudo)Nambu-Goldstone bosons. This was discussed in T. Appelquist and L. C. R. Wijewardhana, Phys. Rev. D 35, 774 (1987); Phys. Rev. D36, 568 (1987) and reviewed already a number of years ago, e.g., in R. S. Chivukula, hep-ph/9503202, hep-ph/9803219 and K. Lane, hep-ph/0202255, as well as more recent reviews such as C. Hill and E. Simmons, hep-ph/0203079 (published in Phys. Repts.) and my brief SCGT06 review, hep-ph/0703050. PNGB's in one-family TC/ETC models may still be a phenomenological concern, but the early estimates of their masses were substantially increased by walking TC.

Let me explain in more detail how TC/ETC models may be able to satisfy FCNC constraints. ETC models generically gauge the generational index and combine it with TC, so a simple SU(N_{ETC}) model has N_{ETC}=N_{gen}+N_{TC}. With three generations of SM fermions, N_{gen}=3 and using the minimal value of N_{TC}, namely N_{TC}=2, this yields an SU(5) ETC theory. The SU(5) ETC symmetry can break to an exact residual vectorial SU(2) TC gauge group in three stages, characterized by three different mass scales, Λ_j, j=1,2,3. The ETC gauge bosons with masses Λ_1 mediate transitions between SM fermions of the first generation and the technifermions, and so forth for the other scales. With the values
$$\Lambda_1 \simeq 10^3\ TeV$$
$$\Lambda_2 \simeq 10^2\ TeV$$
and
$$\Lambda_3 \simeq few\ TeV$$
this model appears to be able to fit constraints on FCNC processes. Consider, for example, one of the most severe such constraints, arising from $K^0 – \bar K^0$ mixing. In early studies in the 1980's, in the absence of an explcit ultraviolet ETC completion, one simply wrote down a generic form for the low-energy effective Lagrangian for this process, $\simeq (c/\Lambda_{ETC}^2)$ times the relevant four-quark operators, where Λ_{ETC} was taken to be "the" ETC breaking scale. But the key observation is the following: in the initial $\bar K^0$, the d sbar pair can annhilate to produce a V^2_1 ETC gauge boson, where the indices are the gauged generational indices. But this cannot directly produce the s dbar pair of the K^0, which requires a V^1_2 ETC gauge boson. Hence, in order for the K-Kbar transition to proceed, the the actual ETC propagator factor is not 1 over the mass squared of the V^2_1 gauge boson, $1/\Lambda_1^2$, but instead
$$(1/\Lambda_1^2)\ \Pi\ (1/\Lambda_1^2)$$
where Π denotes the requisite nondiagonal propagator insertion that takes a V^2_1 to a V^1_2. Using a reasonably ultraviolet-complete ETC theory, in the paper hep-ph/0308061, published as Phys. Rev. D 69, 015002 (2003), Appelquist, Piai, and I showed, via explicit calculation of the ETC gauge boson mixings, that the nondiagonal ETC gauge boson mixing term is generically of order the square of a low ETC breaking scale, essentially as a consequence of a residual approximate generational symmetry in the ETC theory. This suppresses the $K^0 -\bar K^0$ mixing strongly, by a factor like $(\Lambda_3/\Lambda_1)^2$, i.e., the coefficient of the four-quark operator is not $1/\Lambda_1^2$ but the much smaller Λ_3^2/Λ_1^4, which is sufficient to satisfy the constraint from the experimentally measured mixing and resultant $K_L – K_S$ mass difference. In this and a series of other papers, taking account of the mixing between ETC group eigenstates of fermions to form mass eigenstates, we also examined many other FCNC constraints on TC/ETC theories and showed that they appear to be able to be satisfied.

There are also FCNC processes that do not involve mixing of ETC gauge bosons. For example, the (conjugate of the) process $s \rightarrow d\ \mu^-\ e^+$ via exchange of a virtual V^2_1 ETC gauge boson gives rise to $K^+ \rightarrow \pi^+\ \mu^+ e^-$, for which the upper bound on the branching ratio (from the E865 experiment at BNL) is $BR(K^+ \rightarrow \pi^+\ \mu^+\ e^-) < 1.3 \times 10^{-11}$ (90 % CL). This is satisfied with the above value, $\Lambda_1 = 10^3\ TeV$.

Although Eric did not mention the effect that technifermions have on Z and W boson propagators, these also serve as a stringent constraint on technicolor models, especially the electroweak S parameter. However, because technicolor is strongly interacting at the scale of a few hundred GeV, it is not possible to use a perturbative estimate of S, and nonperturbative estimates based, e.g., on spectral function integrals, are difficult to make reliably for a walking TC theory since one cannot just scale up results from QCD. (There have been a number of papers over the years giving estimates on this, and I can send a list to anyone who is interested, but the issue is not resolved yet.)

In the SM, the electroweak symmetry breaking (EWSB) is produced by the vacuum expectation value of the hypothesized Higgs field. But this breaking is simply put in by hand, via an ad hoc choice of a negative coefficient for the quadratic Higgs term. No explanation is given in the SM of why this coefficient was not positive, when, a priori, it could just as well have been. Since the SM give no explanation for this negative sign of the coefficient, it does not provide a satisfactory fundamental explanation for EWSB. Indeed, it is interesting to recall that in both of the previous two main cases where a scalar field was used in phenomenological models for spontaneous symmetry breaking, namely in the Ginzburg-Landau free energy functional for superconductivity and the Gell-Man Levy sigma model for chiral symmetry breaking in hadronic physics, the microscopic physics did not involve the vacuum expectation value of a fundamental scalar field, but instead a bilinear fermion condensate – the Cooper pair in the BCS theory and the quark condensate in the case of QCD. In technicolor theories, it is precisely this type of bilinear fermion condensate – now involving technifermons,- which is responsible for electroweak symmetry breaking. Furthermore, the quark condensate in QCD already breaks electroweak symmetry. Thus, the original construction of technicolor models by Weinberg and Susskind was quite well motivated.

This is, of course, not to say that TC/ETC theories do not face many challenges. They are very ambitious, since they try to explain both EWSB and the spectrum of SM fermion masses, and no fully realistic model of this type has been constructed. Moreover, it is certainly true that far more people are currently working on various variants of SUSY models than on dynamical EWSB approaches such as TC/ETC. But at least readers should know that Eric's comment refers to the old TC theories of the early 1980's, which were, indeed, rejected; the results of more recent work indicate that modern walking TC theories appear to be able to satisfy FCNC constraints. PNGB's and the S parameter are concerns, but, in the opinion of a number of us who work in this area, they do not obviously exclude these theories. In any case, we should know soon from the LHC whether dynamical EWSB via TC/ETC or some other possibility like low-energy SUSY is realized in nature.

Comments

1. **Tony Smith**
   October 26, 2007

   Robert Schrock said:

"… In the SM, the electroweak symmetry breaking (EWSB) is produced by the vacuum expectation value of the hypothesized Higgs field … via an ad hoc choice of a negative coefficient for the quadratic Higgs term. …
Since the SM give no explanation for this negative sign of the coefficient, it does not provide a satisfactory fundamental explanation for EWSB. …
the quark condensate in QCD already breaks electroweak symmetry. Thus, the original construction of technicolor models … was quite well motivated. …".

In addition to technicolor, other types of quark condensate models can account for the Higgs mechanism, and even give reasonable calculations of the Tquark mass and (predictively, and so falsifiably) the Higgs mass. For one example,
Hashimoto, Tanabashi, and Yamawaki say in … hep-ph/0311165
"… in the top mode standard model with TeV-scale extra dimensions, where the standard model gauge bosons and the third generation of quarks and leptons are put in D(=6,8,10,…) dimensions … while the first and second generations are confined in the 3-brane (4-dimensional Minkowski space-time) … the bulk QCD coupling can … become sufficiently large to trigger the top condensation for … D = 8 … We predict masses of the top (m_t) and the Higgs (m_H) … based on the renormalization group for the top Yukawa and Higgs quartic couplings with the compositeness conditions at the scale where the bulk top condenses …
m_t = 172-175 GeV and m_H=176-188 GeV …".

If some people want to work on various superstring landscape models to try to find something realistic, then that is fine and good luck to them.
However,
working with composite Higgs, technicolor, etc (whethet or not supersymmetric) should be equally encouraged by the physics community, especially since examples such as the Hashimoto et al composite model not only calculate a realistic Tquark mass value and give testable predictions of Higgs mass,
and also, if verified by LHC Higgs observations,
would indicate what sort of structures beyond the minimal Standard Model (such as 4 internal symmetry dimensions in the Hashimoto et al model) could be explored in detail by CLIC or ILC,
and
even might indicate whether ILC or CLIC could do a better job of exploration.

Tony Smith

2. **Shantanu**
October 27, 2007

Thanks fo this. Could some answer the following:
1) How do the latest incarnations of technicolor solve the strong
CP problem?

2) Do they predict proton decay?

3) How do they account for non-0 neutrino mass. What is the technicolor
prediction for theta_13?
Thanks

3. **Robert Shrock**
October 27, 2007

Dear Shanatu:

Thank you for your interest and questions. Let me respond to them herewith.

1. Certainly, technicolor (TC) and extended technicolor (ETC) provide a different context in which to study the strong CP problem. One could envision a scenario in which the $\bar \theta$ parameter is promoted from a constant to a dynamically determined quantity, and some dynamics leads to a very small value for it. Two recent papers on CP violation in TC/ETC are K. Lane and A. Martin, Phys. Rev. D 71, 015011 (2005) (hep-ph/0404107) and Appelquist et al., Phys. Lett. B 595, 442 (2004) (hep-ph/0406032). The Lane-Martin paper specifically proposes a solution to the strong CP problem in the context of technicolor.

2. TC/ETC theories, by themselves, do not predict proton decay. This question is related to another, namely to what extent can one grand unify a theory containing the SM and TC/ETC. This is challenging; for a recent discussion, see Christensen and Shrock, Phys. Rev. D 72, 035013 (2005) (hep-ph/0506155). To me, grand unification is very appealing, for all the well-known reasons, so the difficulty of achieving a GUT containing TC/ETC is a concern.

3. On neutrino masses, yes, as I mentioned in my post, there is a mechanism in TC/ETC to that can explain why neutrino masses are so small; see T. Appelquist and R. Shrock, Phys. Lett. B 548, 204 (2002); Phys. Rev. Lett. 90, 201801 (2003). In this mechanism there is strong suppression of the Dirac neutrino masses and, in addition, there are induced Majorana masses which are large compared to the Dirac masses (although small compared with the ETC breaking scales) leading to a seesaw and hence to sufficiently small neutrino masses. Because of the fact that this is a strongly coupled theory, it is difficult to obtain precise numerical predictions for leptonic mixing angles such as theta_{13}.

with regards,

Robert Shrock

4. **Coin**
October 30, 2007

Mr. Shrock:

In any case, we should know soon from the LHC whether dynamical EWSB via TC/ETC or some other possibility like low-energy SUSY is realized in nature.

Quick question, what kind of potential observations would be able to confirm or exclude technicolor at the LHC? Is the idea that the LHC would have to actually detect particles from technicolor's new fermion fields, or could conclusions be drawn more indirectly? If a Higgs is observed, would this falsify technicolor? Are there any other kinds of observations that might exclude technicolor, or at least the new walking technicolor models?

Is Science Near Its Limits?

October 27, 2007
Categories: Uncategorized

The past two days I've been at a conference here in Lisbon organized by the Gulbenkian Foundation on the ostensible topic of Is Science Near Its Limits? The Gulbenkian is probably the most well-known and best-funded cultural organization in Portugal, and it includes a world-famous museum housing the wonderful art collection of its founder, Calouste Gulbenkian, who made his fortune in the oil business early during the last century.

The conference was extremely well-run and well-attended, filling a large lecture hall where there was simultaneous translation of the talks into Portuguese. It was organized by literary critic, writer and polymath George Steiner, who gave the introductory talk. I hadn't known that Steiner had originally started out studying mathematics, but was discouraged from pursuing a career in the subject at the University of Chicago by Irving Kaplansky, which led to his turning to the study of literature and philosophy. Steiner had quite a lot to say provocative to scientists, including questioning whether they had been able to justify to the public the large sums of money being spent on the LHC, and characterizing the lack of testability of string theory as strong evidence that science had hit a limit beyond which it could not progress.

On the whole the rest of the speakers actually didn't have much to say about limits of science, taking the standard view of most scientists that their own field had a bright future, with no limits in sight. The final talk of the conference did return to the limits issue, with John Horgan giving an uncompromising defense of the thesis of his 1996 book *The End of Science* (although he did allow that possible advances in neuroscience such as the decoding of a neural code, could be as revolutionary as previous advances). While the scientists in the audience took Steiner's attacks in stride, partly because he was our host, they were less charmed by Horgan, who got a rather hostile reaction from many of them. I hope he'll write about his point of view on the conference at his blog, or discuss it in one of his Bloggingheads discussions with George Johnson.

I was one of the few other speakers discussing the question of limits, with my talk emphasizing that particle physics is now in a new, different environment than that of the past, one in which progress, even revolutionary progress, is possible, but much more difficult. A written version of my talk is available here. I was paired with string theorist Dieter Lust, who gave a presentation of the case for string theory unification and the Landscape. We were introduced by Gustavo Calstelo Branco of the IST, who emphasized recent advances in our understanding of neutrinos. Also speaking in another session was Luis Alvarez-Gaume of CERN, who gave a very upbeat talk on the prospects for particle physics, taking the point of view on string theory that, like any idea, string theorists will give up on it if it doesn't work out. He already sees a diminishment of interest in string theory among particle physicists, with people moving instead towards subjects that promise some sort of interaction with experimental data. The three of us were brought together later for an interesting small and very lively discussion of the issues surrounding string theory and recent media attention to it. This was taped, and may appear in some form or other in the future.

Update: There's an entertaining conversation between John Horgan and George Johnson about the Lisbon conference now up at Bloggingheads.

Comments

1. **anon.**
   October 27, 2007

   "My critique of string theory over the past few years has made me many fans who are convinced that sophisticated mathematics is not needed to understand fundamental physics correctly, and that this is why string theory must be wrong. They're often unhappy to hear that I strongly disagree with them, that I think particle physics probably needs even more mathematics of a high level of abstraction and intellectual difficulty."

   – http://www.math.columbia.edu/~woit/lisbontalk.pdf (page 5)

   Way to go, man!

   Pythagoras: "Numbers rule the universe".

   Plato: "God ever geometrizes"

   Kepler: "The chief aim of all investigations of the external world should be to discover the rational order and harmony which has been imposed on it by God and which He revealed to us in the language of mathematics."

   Poincaré: "If God speaks to man, he undoubtedly uses the language of mathematics."

2. **Tony Smith**
   October 27, 2007

   Peter Woit's paper excerpt quoted by anon. is indeed something to which people (especially policy-making physicists) should pay close attention. A few more excerpts describe his position in more detail:
   "… Humanity may sooner or later reach the limits of what it can understand about the universe, but there is no evidence that we are there yet. …
   In 1993 Andrew Wiles announced his proof of Fermat's Last Theorem … Just five years ago, Grigori Perelman posted on the arXiv
   preprint server a set of papers outlining a proof of the Poincaré Conjecture …
   Both Wiles and Perelman worked intensively by themselves for
   seven years or more to come up with the advances needed in their work. …
   Wiles and Perelman … showed what kind of long-term effort is
   generally needed to make a fundamental advance against a difficult problem.
   The current organization of research in physics puts the best young people in a position of needing to quickly prove themselves, to produce results on time-scales of a year or two at most if they want to remain employed.
   At later stages of their career, even with tenure, the pressure of grant applications continues to discourage many from making the kind of commitment to an unpopular speculative research program that may be needed to make progress. …".

   As to what sort of physics institutions might be consistent with Peter Woit's recommendations, maybe Alain Connes's remarks made in a Tehran interview might be relevant:
   "… I [Connes] believe that the most successful systems so far were these big institutes in the Soviet union, like the Landau institute, the Steklov institute, etc.
   Money did not play any role there, the job was just to talk about science. It is a dream to gather many young people in an institute and make sure that their basic activity is to talk about science without getting corrupted by thinking about buying a car, getting more money, having a plan for career etc …",
   and
   it may also be relevant that such institutes were the environment that produced Perelman.

   With respect to the USA, I am not sure that it would be politically realistic to get government funding for such institutes, whose freedom for pursuit of unconventional ideas would make them different from current North American “institutes for advanced study” that are smaller and more directed toward currently fashionable approaches such as superstring theory or loop quantum gravity.

Maybe a newly rich nation, such as China, could establish such institutes and so become the source of future major advances in physics based on sophisticated mathematics.

Tony Smith

3. **John Baez**
October 27, 2007

For people who think they can do fundamental physics without much math, the correct answer is not “no!” so much as “I doubt it, but go ahead and try”. Most such people are crackpots and will produce nothing but nonsense, but someone might succeed, and only time will tell.

It’s quite possible that right now theoretical physicists are stuck, not mainly for lack of math, but for lack of a bright new idea that fits together the puzzle pieces that have been handed to us: dark matter (or some failure in our understanding of gravity), dark energy (or some failure in our understanding of gravity), inflation (or whatever makes the background radiation display the patterns it has), the precise detailed structure of the Standard Model, and various other open questions. Right now these puzzle pieces don’t seem to form a nice picture.

4. **Alface**
October 27, 2007

«This was taped, and may appear in some form or other in the future.»

More that taped, I think it was broadcasted live via the Internet (at least Friday it was) and I can’t see why the video isn’t available after the conference’s end.

I was only as the conference on Friday afternoon.
I saw John Horgan’s presentation and can’t agree with your characterization «they were less charmed by Horgan, who got a rather hostile reaction from many of them».

He mainly got a “verbally violent” reaction to his claim that anti-depressants are as effective for the treatment of depression as talk therapies by someone who objected loudly without giving any reason why it was wrong. The others were anything but hostile and the last long rambling in Portuguese was just silly and vacuous.

Horgan’s ideas regarding the end of war seemed out of place in a scientific context but he did do a good job overall.

Unfortunately I didn’t see your lecture and I appreciate that you post the link to the video feed if it ever becomes available.

Dyson gave a very optimistic account but curiously never even glanced over global-warming, a topic that might have had some relevance to the conference. Given his “unorthodox” views it could have been interesting.

As a final note, the Portuguese title was ridiculous meaning “does Science have limits?” which is a milder question than “*IS* Science near its limit?”. The organizers did a very poor job on this account.

5. **Cool Hand Luke**
October 27, 2007

I would like to ask Baez to please refrain from saying things such as, “Most such people are crackpots and will produce nothing but nonsense, but someone might succeed, and only time will tell.”

Saying things such as “Most such people are crackpots,” just because they might value logic, reason, and truth over math, does not advance the culture nor physics.

Arxiv.org is filled with papers that are in turn filled with math–math that has never born any fruit. Reading Woit’s and Smolin’s books, a strong case could be made that today’s crackpots prefer math. Why is it that government-funded mathematical nonesense is deemed superior to truth, logic, physics, and reason?

I hope that Baez someday has an opportunity to read the original papers of Faraday, Boltzman, Maxwell, Einstein, Bohr, Newton, Wheeler, DeBroglie, and Einstein.

You will notice that the simple logic, reason, and motivation are all contained in beautiful words which far eclipse the presence of math. Read Penrose’s THE ROAD TO REALITY, and you will find far more math, but Penrose hasn’t done much in the realm of physics, other than The Emporer’s New Mind.

Indeed, Faraday’s notebooks and papers barely contain any math–he lead with logic, reason, and physics, as did Einstein, and then they both sought out the math that captured the physical reality. Same with Ludwig Von Boltzman, who many called a “crackpot” in his day.

At any rate, none of the Great’s papers nor notebooks nor books spent that much time talking about who were and who weren’t the crackpots of their age. In fact, I have found no mention of the word “crackpot” throughout all their noble, lasting work. And all of their eras produced far more noble and enduring advancements in the realm of physics than has the last thirty years of our era.

Perhaps the time Baez invests in his crackpot contemplations could be better spent advancing physics. There is no need for ad hominem attacks and name-calling, and we should all be humble with regards to the mysterious nature of science. We do not know where tomorrow’s revolution will come from–curiosity cannot be dictated nor legislated; and thus it should never be castigated nor impugned with snarky namecalling. Curiosity must remain free.

And while Baez’s cataloging of “big questions” is fun, all great scientists have ever asked their own questions, following their own curiosity; from Kepler, to Newton, to Einstein, to Feynman.

One certainity is this–those who find the big answers get to ask the big questions. And the questions are ultimately asked in words–not in numbers.

6. **Leonard Ornstein**
October 27, 2007

Peter:

In your beautiful and convincing Lisbon talk you state:

“The main thing that distinguishes mathematics from physics and other sciences is the different role of experiment”.

“As particle theory enters a new environment in which experimental results are much harder to come by, it may need to learn from mathematicians how to work in a much more painstaking way, taking care to see what can be built solidly without reliance on validation by experiment."

"Lacking inspiration from experiment, physicists may find that searching for it in mathematics is one of the few avenues left open."

This seems to reveal your fundamental Platonic Idealism.

But, as you argue, this is pretty much the path that most string theorists now pursue, and you usually criticize them with such arguments as:

"For such a conjecture to deserve to be called scientific, it must come with the standard sort of scientific evidence, or a plausible idea about how one might someday find such (empirical?) evidence."

This seems a bit schizophrenic. Can you clarify this apparent inconsistency with some argument, other than the possible criticism, that string theorists' efforts are simply not "painstaking" enough?

7. **Erik**
October 27, 2007

That's funny, my objection to

"Most such people are crackpots and will produce nothing but nonsense, but someone might succeed, and only time will tell"

was that it might encourage those crackpots with the "but someone might succeed" caveat.

8. **Shantanu**
October 27, 2007

(Sorry for posting this twice). first time, message did not go through.
Peter have you you looked at the slides and talks of the recent QG conference
in Uruguay?
http://qgsciv.fisica.edu.uy/program.html

9. **J.F. Moore**
October 28, 2007

I recall when Horgan's End of Science book came out. Most striking was the strong, emotional, and yet poorly reasoned response from the bulk of scientists who publicly voiced their opinions on the matter. They trotted out the same tired, hoary notions such as the throwback to the state of physics in the late 19th century or the apocryphal 'the patent office shall soon be closed' story. Even worse were the ones who chose to discount the opinion of a mere journalist. Just embarrassing.

Somewhat better, but still wide of the mark, was John Maddox' response book. I don't really understand why Horgan's thesis wasn't met and argued fairly, rather than hysterically. Then again, I don't understand why people like Kurzweil get a free pass for promoting absurd concepts like the 'singularity' in book after book.

Anyway, this conference sounds like it would have been fascinating to attend.

10. **AngryPhysicist**
October 28, 2007

Cool Hand Luke wrote:

"Saying things such as 'Most such people are crackpots,' just because they might value logic, reason, and truth over math, does not advance the culture nor physics."

But math is logic…and "truth over math"? You mean math **isn't true?!**

Math is a collection of **tautologies**, how in the hell **can't** it be true?

11. **Greg Egan**
October 28, 2007

Cool Hand Luke: What we classify as "mathematics" versus what we classify as "logic and reason" is a purely cultural distinction, which varies from place to place and time to time, and to which physics itself is indifferent.

It would be wonderful if everything in the universe could be explained by means of simple imagery, common sense and everyday language, but there's no good reason to expect this to be the case. Feynman did an amazing job explaining most of QED and much of the Standard Model in simple terms in *QED: The Strange Theory of Light and Matter*, and many people have produced nice, intuitive accounts of much of General Relativity (included John Baez, here). But everyday language and intuition are shaped by a relatively small subset of physical reality, and there is a limit to how far beyond that subset we can reach without the tools of formal mathematics.

12. **Chris Oakley**
October 28, 2007

I do not know whether those who say that you can do fundamental physics without mathematics are trolling (trawling?) or not, but let us assume that they are not.

How, I ask them, are you going to get a cross-section (a number) for e+e–>γγγ (a number) if your theory has no numbers in it?

Take your time about answering.

13. **George Jones**
October 28, 2007

John Horgan had an article (available here) in the October 2006 issue of Discover Magazine that updated his views on the limits of science.

14. **Steve Demuth**
October 28, 2007

For people who think they can do fundamental physics without much math, the correct answer is not "no!" so much as "I doubt it, but go ahead and try". Most such people are crackpots and will produce nothing but nonsense, but someone might succeed, and only time will tell.

It's quite possible that right now theoretical physicists are stuck, not mainly for lack of math, but for lack of a bright new idea that fits together the puzzle pieces that have been handed to us

Many of us were attracted to physics, and foundations in particular, by the proposition that some **simple** order underlies and explains reality. Mathematics in physics bolsters that hope, by reducing apparent complexity, to simply stated physical laws. Who, who understands them, can fail to be moved by Maxwell's equations', or Einstein's field equation's, elegance and explanatory power?

But that's the rub – *who understands them*? At some point, the mathematics required by physics has ceased to be in any sense simple. For me, the boundary came between QM and QFT – the former seems to satisfy the need for simplicity, the latter to defy it.

All this is relevant, because at some point, I think we have to contemplate the possibility that the human ability to imagine or manipulate the mathematics required to describe physical reality may simply be insufficient. Insufficient imagination may mean that the fundamental operations our brains can adeptly manipulate, are not the most appropriate fundamental operations for understanding deep physics. Insufficiency of doing may mean that even if we've got the right ideas, too many computations of too great complexity are required to take those "simple" ideas, and make them describe a big universe.

This would be a sad conclusion for physics, of course, but the incredible quantity of talent thrown at finding a unification of GR and the standard model, with little obvious fundamental progress, suggests that we at least entertain the possibility.

15. **Peter Shor**
October 28, 2007

This was very interesting, Peter.

My objection to John Horgan's book was for each discipline, he came up with a different argument about why we've reached the end of science. For some disciplines, he said that we've reached the limit because we've worked everything out, and all that remains is filling a few details (e.g., what many physicists thought was the state of physics at the end of the 19th century). For other disciplines, he said that we understood a lot of the basic ideas, but fitting them together to reach the next level of understanding was impossible (maybe the state of physics in the years before the Standard Model was discovered). For other disciplines, he said that we had no idea how anything worked, but it was too complicated to figure it out (this was for neuroscience).

So basically, you can make the same argument about nearly any science at most periods of history (excepting the few years after a fundamental breakthrough), and only in retrospect can you discover when you were right.

16. **Peter Woit**
October 28, 2007

Shantanu,

Thanks for pointing out the link. I haven't had time to look at the talks carefully, but they appeared to be good summaries of what is going on, if nothing really surprising.

Leonard,

Yes, sometimes I describe my philosophy of mathematics as radical Platonism…

There is a common perception that the problem with string theory is its reliance on mathematics, and for some reason people get the idea that this is what I believe. What I actually think is that this is just completely wrong and I have argued this point many times, most at length in my book: the problem has nothing to do with mathematics, it's that using a string theory in higher dimensions to try and get a unified theory is just a speculative idea that turned out to be wrong. What is needed is other, different speculative ideas, which may involve just as much mathematics, but turn out to be right. The only way to be sure they are right will be if they, unlike string theory, show signs of making progress towards telling us something about particle physics that the standard model itself doesn't tell us.

17. **Peter Woit**
October 28, 2007

Alface,

The negative response to Horgan wasn't so much from people who spoke publicly during the question session, but privately from people near me in the front part of the room. I think he evokes a strong negative reaction from most scientists, since he is challenging one of their most deeply held beliefs of most scientists, who are doing what they are doing typically because of their belief in the possibility of dramatic future progress in their field.

I actually hope the talk itself isn't available on the internet, I wish people would instead read the written version of the presentation. I ended up having only about half as much time to talk as I had prepared material for, so, was just improvising much of the talk on the fly, which didn't work that well. The written version is much more carefully thought out and I hope much clearer.

18. **John Baez**
October 28, 2007

Cool Hand Luke wrote:

> I hope that Baez someday has an opportunity to read the original papers of Faraday, Boltzman, Maxwell, Einstein, Bohr, Newton, Wheeler, DeBroglie, and Einstein.

I have! I haven't read them *all*, of course, but I like looking at original papers.

> You will notice that the simple logic, reason, and motivation are all contained in beautiful words which far eclipse the presence of math.

Hmm. Let's consider Newton's *Principia*. It's interesting to read about contemporary reactions when this book came out around 1687.

According to Richard Westfall's biography, the philosopher John Locke tried to read it, but "since he was not a mathematician, he found it impenetrable. Not to be denied, he asked Christiaan Huyghens if he could trust the mathematical propositions".

The mathematician DeMoivre tried to read it at the age of 21. "But he was surprised to find it beyond the range of his knowledge and to see himself obliged to admit that what he had taken for mathematics was merely the beginning of a long and difficult course that he had yet to undertake".

Of course, nowadays calculus is taught in high school, in a far simpler style than one will find in the *Principia*. But, in its day, it was the peak of mathematical achievement.

Of course Newton *also* had good ideas in physics, not just math. For him the two went hand in hand, inseparable.

I think the same is true of most of the authors you list. Bohr and Faraday may be the two exceptions. Faraday, of course, was an experimentalist. He interrogated Nature directly.

> Perhaps the time Baez invests in his crackpot contemplations could be better spent advancing physics.

It only took me a few minutes to write that post. Surely I'm entitled to a little break from advancing physics now and then? If you wanted me to get back to work, you shouldn't have written something else for me to reply to. This one took a lot longer.

But: my main point was not to lambast crackpots! It was to suggest that physicists may be stuck "not mainly for lack of math, but for lack of a bright new idea that fits together the puzzle pieces that have been handed to us."

So, it's sort of weird for you to reply as if I'd been arguing in favor of the importance of math.

19. **milkshake**
October 28, 2007

There was a very entertainig interview of OMNI mag with Feynman, I think in 1978 or so – and the journalist specifically asked if a matemacically unsophisticated person like Faraday could make a significant contribution nowadays – and Feynman said no.

(To paraphrase: We got out of simple mechanical models as much as we could have already – and since the natural rules and language of universe is so strange and remote from the everyday experience, we need careful abstraction to translate it into things we comprehend, hence lots of math is needed.) In that interview Feynman also said that math can be threatening to somebody who didn't study it but it is no harder than a normal person can handle.

20. **ScienceLover**
October 29, 2007

I don't think C H Luke wants actually ban mathematical physics but he doesn't believe an advancement in the foundations of physics has to be expected from mathematical "problem puzzling" but requires a conceptual advancement originating from ideas which can be explained without formalism.

As far as Horgan is concerned, it's like a Pascalian bet: he might be correct but it is more promising when he is wrong. After all it's just a journalist hypothesis and the only way he tries to "proof" it is leading interviews with prominent researchers. He just made unambigously clear what he thinks about scientific speculation which goes on and on and leads to an increasing heap of subsequent speculations and interpretations – being mathematical or not.

21. **Spiro Zafiratos**
October 29, 2007

It's funny that the topic of mathematics has come up at this time. I always believed since the beginning that String Theory was off. Peter and John are right it's not the math, we do need more of it. Maxwell won the Nobel Prize because he formulated the mathematical model of light, not Faraday.

I've always argued that current String Theory is at the same stage as astronomers were thousand of years ago when they created epicycles to describe the movement of the pkanets. Was the mathematics wrong? No, what was wrong was the model was built around the assumption that the earth was the center of the universe. The epicycle universe was a very complex mathematical model built on the philosophical view of the universe at the time. Shifting the sun to the center of the solar system created a much simpler, but still mathematical, model of the way the planets moved. It wasn't until Kepler finally changed the model from epicycles to ellipses that the model began to match observation. But, even though Kepler's model was simple it was still wrong. It wasn't until General Relativity that the observations and mathematical equations finally agreed. General Relativity not only agreed, but predicted the motion of all the planets.

Like the Ancient Greeks and Kepler, current String Theorists are basing their model on various assumptions. Like the ancients we can't make any direct observations to build a model around. Kepler had Brahe's detailed observations of Mars to help him formulate his theory. Will LHC help us? And, like the Greeks the more we try to make the string model reflect reality the more complex it becomes. For the Greeks the math wasn't wrong it was the original philosophical assumptions that were false.

So, is the math of String Theory wrong? No it's not. Is the fundamental model wrong? Only time will tell. Like relativity, we need a model that predicts nature not mimic it.

Now stop this and let that slacker Baez get back to work.

22. **Tim May**
October 29, 2007

"It's funny that the topic of mathematics has come up at this time. I always believed since the beginning that String Theory was off. Peter and John are right it's not the math, we do need more of it. Maxwell won the Nobel Prize because he formulated the mathematical model of light, not Faraday."

Maxwell died in 1879, more than twenty years before the first Nobel Prizes were awarded.

Look, I hope I'm not judged off-topic, but I just couldn't let this one pass.

I don't know if Lee Smolin and Peter Woit are right or not. I've been reading this blog for a couple of years now, read Smolin's "Three Roads
" in around 2002, read their more recent books in the past year, and am for various reasons fairly glad I decided not to further theoretical physics way back in 1974. (I instead entered industry, specifically, Intel, and did a kind of applied physics for a while, retiring in 1986 to pursue whatever interested me.)

Physics has been in a tough time for a long while. Feynman commented more that 35 years ago that physics was looking increasingly like smashing watches together at ever greater speeds and seeing more and more little pieces.

This is not too surprising. The energies available to build accelerators scale with technology, but at factors greater than the energies achieved, even as "interesting events" are probably falling off faster than the energies are increased.

So a lot of stuff was discovered in the 1 MeV to 100 MeV range, when basically a few thousand (up to hundreds of thousands, maybe) of bucks could equip a lab. And then a bunch of stuff was found in the BeV/GeV range, a la the Bevatron in the 1950s. A matter of tens of millions of bucks.

And so on, with the last 30 years seeing sparse discoveries in a couple of accelerators, each costing many billions of dollars. (I don't have the energy, no pun intended, to make up a graph of energy of accelerators in billions of electron-volts versus cost of accelerators in billions of dollars.

I hear the usual name for this is the "desert." Meaning, not nearly as many discoveries in the TeV range as in much cheaper, much lower energy accelerators. Which has implications for how much longer the politicians will fund bigger accelerators. (I'm too far out of things to know how likely supersymmetry/Technicolor findings are likely to be at the LHC. If there's some big surprise, maybe more generations will be funded. if not, maybe things slow down for a long while.)

I have a sneaking hunch one of the contributors here has it about right, namely, Greg Egan. Even though his "Diaspora" is science fiction, he anticipate that some of the accelerators needed to really answer some of the key questions–probing a dozen orders of magnitude closer to the Planck scale–won't happen for many centuries. I think he has it about right.

It just costs too damned much–and will cost civilization-sized amounts of money and energy in about 3 or 4 generations of accelerator energy regimes–to resolve niggling questions about the unification of the very large and the very small.

Put another way, there likely won't be funding beyond about 1.5 generations from now unless some new kind of bombs or some new kind of energy production seems likely…and neither of these seem at all likely.

Meanwhile, math research doesn't cost a lot and may yield some striking discoveries.

Cosmology, too. I'm all for building dozens of new and more powerful telescopes. I think new physics is much likelier to emerge from seeing what's out there than from hoping that a 300-km accelerator costing the GNP of Spain for 18 years reveals some new charmicle or coloron or whatever.

I enjoy this blog. I am saddened by what the debate has done to some of the partisans, though. And I think a certain Czech threw away his career in a misguided political frenzy. (I'm sort of sympathetic with his views, but physics is physics, not politics. I saw a department member get so wrapped up in fighting the war in Vietnam, through campus protests, that he was no longer doing any physics. This was in 1971-73. He left physics completely, as near as I can tell. Sad, as he had no effect on the war in Vietnam, and he threw away everything for it.)

Sorry for the length here. My first comment. Hopefully Dr. Woit will be tolerant.

–Tim May

23. **ScienceLover**
October 29, 2007

> Peter and John are right it's not the math, we do need more of it.

The question is whether you expect to get a sound theory out of the math or get the math into the theory. I don't think the latter has to be disputed at all. Physical theories which can't be expressed in a mathematical language shall not be considered as science. Without putting mathematics into the theory it's all just philosophy if not fraud.

What's left? I see following options:

a) Current theories don't have to be extended a lot conceptually but the math has to be "cleaned up" which requires obviously more math. This is the "normal science" viewpoint.

b) New concepts are primary mathematical inventions such as strings + extra dimensions that don't have any obvious physical purpose but can be interpreted as physical entities or background properties and they might help to patch the mathematical machinery. Despite violating Occams razor those inventions can often be justified when they are not too exaggerated and cause more problems than they solve. Otherwise they turn out to be phlogiston.

c) Some essential concepts, experimental links and observations are still missing and every attempt to make signifcant progress at the foundations without them is just a footnote in the history of illusions. Maybe we just have to be more patient with mother nature?

24. **Johan Couder**
October 29, 2007

John Baez said:

"But, in its day, it [Principia] was the peak of mathematical achievement."

hmm, or perhaps the pinnacle of obscurantism. Isn't Newton known to have said that he purposely made it difficult "to avoid being bated by little smatterers in mathematics"

Spiro Zafiratos
"Maxwell won the Nobel Prize because he formulated the mathematical model of light, not Faraday"

huh? the first Nobel prize was awarded in 1901. Faraday died in 1867, Maxwell in 1879. Posthumous nominations are not permitted. When did Maxwell win the Nobel prize ?

25. **John Horgan**
October 29, 2007

Good post on and responses to "Is Science Near Its Limits?", which as Peter says was a fascinating meeting. I just posted on it yesterday. http://www.stevens.edu/csw/cgi-bin/blogs/csw/?p=75 I focused on the suggestion of the eminent German neuroscientist Wolf Singer that researchers study parapsychology. Another speaker, Freeman Dyson, professed belief in psychic phenomena in a New York Review of Books Essay a few years ago. He reiterated this view to me in Lisbon. He said, however, that he doubts whether ESP can be scientifically validated, because it is usually only manifested in people under severe stress. Singer's suggestion about ESP seemed to be received with far more equanimity than my end-of-science schtick. My only regret is that, during my speech on the end of science, I didn't suggest that string theorists attempt to resolve their problems by postulating a new particle, the "psychon."

26. **Johan Couder**
October 29, 2007

still, Spiro Zafiratos has a point

all the essential elements of Newton's theory of gravitation appear in several of Newton's predecessors, and all of them appear in the work of Hooke [Hesse, Mary B. (1962): Forces and Fields The Concept of Action at a Distance in the History of Physics, Dover, p. 133]

Unfortunately, Hooke lacked the mathematical technique to represent the complete theory in all its mathematical inevitability, so Newton received all the credit.

27. **Spiro Zafiratos**

October 29, 2007

Sorry my error on Maxwell. Johan made my point. Hooke, Brahe, and Faraday did the work, but the mathematicians received the fame.

28. **Steve Myers**
October 29, 2007

On math & physics — a direct example from my work: I have a spreadsheet with over 65,000 data points for 8 devices. I can't make any sense out of that data without math — statistics, Fourier, etc. But before I gathered that data or placed those instruments, I had to understand what & why I was measuring. The concepts came first but without the math I just have a collection of numbers. A physicist trying to discover "the rules of the universe" is working at a deeper level but the basic method is similar. And in one case — involving airflow & pressure change — the math came first & suggested the key idea.

29. **Peter Woit**
October 29, 2007

I made a mistake in moderation here, allowing a comment from "Cool Hand Luke" = "Elliot McGucken", because I initially didn't realize who this was. It became clear after he posted many more comments to the thread, which I deleted. Leaving the initial one was a mistake, since it has led to a mostly off-topic and unenlightening discussion, and he keeps posting large numbers of comments here which I have to deal with.

Please, no more comments on the thread inspired by "Cool Hand Luke".

30. **Leonard Ornstein**
October 29, 2007

Peter:

You didn't address my main question; the seeming contradiction in the two following quotes:

"…to see what can be built solidly without reliance on validation by experiment."

"For such a conjecture to deserve to be called scientific, it must come with the standard sort of scientific evidence, or a plausible idea about how one might someday find such (empirical?) evidence."

You usually seem to believe Science requires "validation by experiment".

So what's the value your talking about in the first quote?

Only the pragmatic value that comes from building what appear to be self-consistent models…to be accepted or rejected AFTER empirical test?

Is that you definition of scientific radical Platonism?

That's fine, but it hardly fits Platonic ideology!

This may seem to be a straw man; it's just that many scientists DO believe that empirical testing…and testability…is what distinguishes science from math, logic, and all other 'purely tautological ideologies'. Blurring the definitional line between induction and deduction, isn't helpful, especially in discussions of strings and the future of science.

And when you label math as science, it also adds to the confusion; vive la difference.

I'm in no way trying to denigrate math or logic!

31. **Peter Woit**
October 29, 2007

Leonard,

In the end, any physical theory definitely needs validation by experiment, or it's not science. My comments were about what physicists should do along the way towards this ultimate goal. When I wrote about working "without reliance on validation by experiment", I was referring to the work done with the ultimate goal of coming up with something experimentally testable, but where that goal may still be far away. If you start investigating a very speculative idea, you often generally don't initially have experimental tests. It there is a lot of experimental data out there, you should be able to soon make contact with it. My point was that, if relevant experimental data is much harder to get than in the past, you often can't rely on it until much later, so need to be very careful about the consistency of what you are doing along the way.

32. **srp**
October 29, 2007

Given Peter's analysis of the prospects for accelerator progress, why isn't the community trying to direct substantial sums at wakefield research? That appears to be the only technology on the horizon that could give big jumps in performance/cost, but it seems like fairly small amounts of people/money are working on it. As a taxpayer, I'd rather spend a few hundred million dollars on basic R&D for a breakthrough accelerator technology, even though it might not pan out, than devote billions to a scaled-up version of existing tech (ILC, anyone?) that doesn't seem likely to do that much.

33. **Coin**
October 29, 2007

So I'm a little confused about one thing about this conference. The comments here seem to largely cover the question of whether *physics* is near its limits– reasonably, since this is a physics blog– but the conference title itself and the vague comments about "neuroscience" seem to imply that this was a conference about the limits of science *in general*.

I understand the "desert" problem people are worrying about with respect to physics– the fear that all the questions are turning out to be either trivially resolved or way out of reach– but it seems to me that this is in no way a worry which is applicable to all sciences at the present point in time. Science is larger than just physics and the crisis does not seem to be shared. The various biological sciences, for the first thing that comes to mind, seem to be in at a point where research is making measurable advances on a wide variety of fronts, with one entire promising *type* of research being seemingly held back primarily by U.S. laws concerning embryo disposal. I have not read John Horgan's book, so I don't know what his argument concerning the state of biology research was, but if the book was published in 1996 then it doesn't seem like it could have possibly taken into account the potential avenues of research opened up by stem cells– which to my understanding weren't even isolated in humans until 1998!

Meanwhile I'm not finding it too hard to list off growth areas off the top of my head even outside biology. Astronomy seems to have been having a really good decade, once the Hubble got fixed. Atmospheric science seems to have had a very productive last few decades, with its primary problem being to get anyone to listen to the results of its research… it seems that even if science stalls out on certain fundamental questions, there can still be niches where this doesn't stop one from being very very busy.

Meanwhile I'm not sure that we can even judge the progress of "physics" based on the troubles with *the sort of physics discussed on this particular blog*— which is to say, high-energy particle physics and fundamental theoretical physics. These are of course critically important areas, and with the LHC looming this is a good time to be focusing on those areas of physics. But they're not the only areas of physics, are they? Quantum computing, for example seems to be in the midst of a string of significant breakthroughs which shows no sign of stopping (though there are other commenters in this thread who would clearly know more about that than me…). For another example, I semifrequently hear Chad Orzel complain on his blog along the lines that lack of progress for HEP is judged to be lack of progress for "physics", while meanwhile potentially interesting advances in Orzel's own field, quantum optics, continue at a steady pace– but without much attention being paid to them, because they're not as dramatic.

Okay, so worst case scenario, the LHC finds one Higgs and nothing else, all the new physics looks to be at the planck scale and we still don't have any idea how to probe that. This is a serious crisis, to be sure, but all the people doing quantum optics and bioinformatics and climate modelling will just keep quietly chugging along anyway. Even in the worst case scenario, are we approaching the end of science, or just the end of "sexy" science?

34. **John Baez**
October 29, 2007

Peter wrote:

> Yes, sometimes I describe my philosophy of mathematics as radical Platonism…

What's "radical" Platonism? There are many kinds of Platonism, and a *lot* of them are pretty radical if we take metaphysical naturalism as our standard of a boring ordinary worldview. I'm some sort of Platonist, but I want to know if I'm radical.

As for being "near the limits of science", does anyone here actually think we are? About the only scenario I can imagine where science ends anytime soon is one where our current civilization falls apart and is replaced by one much less interested in science.

35. **tytung**
October 29, 2007

Regarding the 'limit of science'…is this suppose to mean an intrinsic limit of our ability to understand nature? or does it mean the limit due to unavailable of suitable technology or some other practical reasons? or maybe it just reflects a limit of our imagination of how science should advance within the current paradigm?

36. **Thomas Larsson**
October 30, 2007

> As for being "near the limits of science", does anyone here actually think we are?

I always thought Horgan failed to make a distinction between several related notions: can sciences end, do all sciences end at the same time, and is this time now? The answers to these questions are obvious once you consider (terrestial) geography: it peaked around 1500 and was essentially over by 1900, well before the golden age of other sciences.

However, I do think that Horgan has a legitimate point when it comes to physics, but the extrapolation to other sciences is not valid. My opinion is colored by the fact that I started out in the statistical physics of critical phenomena in the early 1980s, and for various reasons I mainly worked with 2D models. A few years later, CFT came along and literally wiped out my chosen field of research. I was too young and unexperienced to jump onto this bandwagon (and I didn't notice the bandwagon until 1986), but it left me with the realization that physics, or at least subfields of physics, can end. Mine did.

Of course, 2D phase transitions is not a very important subfield, and physics as a whole can not come to an end before QM and gravity coexist happily. But the question is whether this unification requires new physics, or if all that is needed is to take the ideas from QM and gravity seriously. I vote for the latter.

37. **Peter Woit**
October 30, 2007

John,

I just made up the "radical" modifier to "Platonism" because I liked the sound of it, there's probably a more conventional terminology. What I mean by it is roughly the idea that not only is mathematics the investigation of certain fundamental objects that "exist" (not a study of formalisms freely created by the human mind), which is how I'd characterize "Platonism", but these objects are the same fundamental objects as the fundamental objects of physics (that's the "radical" part).

So, we want not just a grand unified theory of physics, but a grand unified theory of physics AND mathematics.

Max Tegmark goes on in a similar vein sometimes I think, although this leads him to something rather different (to me, he seems to treat math as kind of a black box).

38. **Andrei Kirilyuk**
October 30, 2007

John Baez wrote:

"About the only scenario I can imagine where science ends anytime soon is one where our current civilization falls apart and is replaced by one much less interested in science."

This has already happened, you've just missed the evolution. This is something typical of "science after the end of science": mainstream, very well supported scientists working and even living in a world of their own (a "Platonic universe"?) separated by the growing gap from the real world where the majority of other people, Earth civilisation as such, is actually living. Yes, they still allocate the necessary billions for your fruitless searches in purely abstract, over-simplified structures and technical tools, but already almost nobody except scientists themselves (and related interested bureaucracy) is really excited about the emerging or even expected advances. If you compare with the situation we had in the middle of the last century, you'll see the difference. And the change concerns not only fundamental physics. People are more directly interested in the ready-made practical results, in technology, but not in the "fascinating process of research" and related "promises" as it was all the time before (who knows, maybe indeed "one cannot fool all the people all the time" about "quantum computers" and other "nanotechnologies" that strangely fail to definitely demonstrate their real powers?). The shift is quite visible and ignoring it makes just another aspect of the real end of science. This is the "internal structure of death" of any system: it first becomes increasingly detached from any environment it used to intensely interact with during its life, which is the "first death" so to say, and then the actual, definite disappearance comes as the "second death". It remains to hope that another kind of knowledge can take the place of disappearing mechanistic science. If not, it can well be mere technology, or "applied science" (largely today's situation), but this one can hardly be "sustainable" beyond the very immediate future.

39. **a.k.**
October 30, 2007

..one could wonder at this point, if this dichotomy 'purely abstract' vs. 'technology/practical results' could be more likely interpreted being a growing 'cultural prejudice' than marking an inherent evolution that one could associate to 'the end of science', i.e. regarding the above discussions. In this sense, I do not see why a failure of string theory as a 'TOE' could exclude the possibility of very concrete links of some of its mathematical concepts to reality, the problem is probably to a less degree the lack of 'fruitfulness' of concepts involved but the growing prejudice to explore 'cross-cultural' communication, on both sides.

Quite recently, I discovered a not-so-recent paper by A. Pnueli linking the index theorem, notably the eta-invariant, to adiabatic charge transport and Hall conductance on surfaces with cylindrical ends, this application might be obvious, still there seems to be little research in that direction. I do not see why for instance mirror symmetry couldn't have long-term applications in, supposingly, material science, linking the 'symplectic/lagrangian' picture to purely algebraic concepts on the complex side, maybe one has to be courageous to cross (growing) 'cultural gaps', but this condition is not in any sense equivalent to the 'end of science'.

40. **Zathras**
October 30, 2007

Woit: *What I mean by [Radical Platonism] is roughly the idea that not only is mathematics the investigation of certain fundamental objects that "exist" (not a study of formalisms freely created by the human mind).*

This is a false dichotomy. The truth value of Mathematics can also be something in the middle, rather than purely ontological or purely "just in the head." Instead, mathematics can be seen as fundamental interactive, in that it deals with our apprehension of the world. From this viewpoint, Mathematics gives the Projection of reality that we can apprehend. It is fundamentally incomplete, since information is almost always lost in a projection, but it still contains truth value, and it says as much about the subject as the object. Such an idea has been present in the history of thought since at least Plato's Cave analogy, although its application to Mathematics is much newer.

41. **Coin**
October 30, 2007

Yes, they still allocate the necessary billions for your fruitless searches in purely abstract, over-simplified structures and technical tools, but already almost nobody except scientists themselves… is really excited about the emerging or even expected advances… People are more directly interested in the ready-made practical results, in technology, but not in the "fascinating process of research" and related "promises" as it was all the time before

Unless you're seriously suggesting that new practical results are indefinitely possible without ongoing fundamental research into the "abstract" problems, it seems like what you're describing here is really a problem of communication more than anything.

42. **Adud**
October 30, 2007

> physics as a whole can not come to an end before QM and gravity coexist happily.

I sort of disagree with this, for two reasons. First, most physicists are not involved with such unification, and most likely not even aware of it. People working in condensed matter, plasmas, classical and quantum chaos, etc., etc., etc., will keep making progress in their respective fields regardless of what goes on in QG, be it good or bad.

Second, even assuming that the unification of QM and GR is actually the holy grail of physics, which I do not believe, finally finding it need not be the end of physics, just like discovering and decoding DNA has not been the end of biology. Biologists found their TOE decades ago, yet biology keeps making progress and growing, and getting most of the research money everywhere.

43. **anon.**
October 30, 2007

'… but already almost nobody except scientists themselves… is really excited about the emerging or even expected advances…' – Andrei Kirilyuk

'… it seems like what you're describing here is really a *problem of communication* more than anything. …' – Coin

Coin, the lack of 'communication' (the political euphemism for 'spin and hype') which you perceive to be the problem is exactly what led string theorists into their mess. By trying to get media attention and massive funding ahead of any falsifiable theory let alone any experimental results (falsifiable theories are two a penny in physics), they ended up losing a grip on reality and ceasing to be skeptical (=scientific).

The problem is really a case of finding the simplest abstract theory that makes contact with physical reality, checking it experimentally, etc., not just generating more spin and hype (aka: communication).

44. **Thomas Larsson**
October 31, 2007

Adud, you misunderstood the direction of my implication. I did not claim that QG implies the end of physics, but that the end of physics implies QG.

45. **Leonard Ornstein**
October 31, 2007

Peter:

The models (or Zathras' "projections") of math and theoretical science differ mainly in their motivation. Scientific theories usually ATTEMPT to model external reality. Most of those of math and logic (refinements of conventional language), don't.

Establishing how much confidence scientific theories deserve, depends COMPLETELY on empirical realty checks, usually of experimental science. You usually appear to subscribe to this clear distinction.

Thus, your:

"What I mean by it is roughly the idea that not only is mathematics the investigation of certain fundamental objects that "exist" (not a study of formalisms freely created by the human mind), which is how I'd characterize "Platonism", but these objects are the same fundamental objects as the fundamental objects of physics (that's the "radical" part)."

fails to distinguish between many of the "formalisms freely created by the human mind" of math, from those supposedly designed specifically to model external reality. String theories belong in this second category. But separating the possible 'truth' of any physical theory from science fiction and pseudo science is at least, the line between purely deductive, "tautological" truth and inductive, empirical 'truth'.

Science, tries to model reality in a more formal way than 'straightforward' communication with conventional language. It seeks to help us understand and somewhat control our possible destinies. There will always be unsolved problems important to comfort and/or survival. The formality of science often generates more precision and utility.

I believe THIS is why it's hard to imagine an end to science…in rational societies.

46. **eiaboca**
November 1, 2007

We just need new kinds of science, revolutionary avenues of thought. It's happened many times before. Technology will advance, the set of all human knowledge will expand in some ways, perhaps contract in others, but from that set someone will be able to pull something out of the background noise that no one has ever noticed before.

Science is just finding empirical data about the universe and analysing it. If you think there is a lack of data in the universe to analyse, then I think you're kind of ridiculous.

There are certainly different "ways of knowing" about the world, and the crackpots might inadvertently hit upon some real theory about things, because the universe seems to be infinitely weirder than we've ever imagined.

47. **eiaboca**
November 1, 2007

You being the "royal" you, as it were, and not you specifically.

48. **Emil Lundh**
November 3, 2007

Coin and Adud, thanks for sobering remarks.
Finding out what's inside the quarks (so to speak) is surely just one of the goals of physics. There are lots of issues that are at least as fundamental. One of them is, how do we make sense of quantum theory? (HEP builds on quantum theory, but I don't see how it can actually advance the understanding of it the way that, e.g., experimental quantum optics does.) Another one is, how do we make sense of phase transitions? – that is, in the words of Laughlin, finding out the "principles of organization" for systems of particles, that seem to be quite independent of their microscopic laws of motion.
It is sad that the research programs that deal with these other kinds of fundamental question do not often get invited to conferences like the one we are discussing.

49. **mclaren**
November 7, 2007

One important point which it seems many folks have overlooked is Dr. Woit's focus on the fact that some of the biggest outstanding problems in current particle physics have little to do with larger accelerator energies.
Einstein's theory of gravitation remains a fundamentally classical geometrical model, whereas quantum theory is quite different. Einstein's tensors do not involve expectation values, there is no uncertainty principle involved, and above all GR is a continuous theory — quanta are discrete.
Arguably the biggest unresolved problem in modern physics involves finding a way to mathematically unify these radically incompatible mental models of reality. Every time someone has tried, the math has blown up.
This suggests several possibilities. First, we need a lot more and a lot better math than we currently have. Considering the immense sophistication of current mathematics, that's non-trivial. And we probably also need some radical new conceptual leap, something different in kind from current mental models of the universe, in order to successfully unify GR and quantum theory. One of the most powerful criticisms I've seen of string "theoy" is that it merely tacks on extra dimensions to current mental models. I.e., it takes pointlike particles and draws 'em out into circular strings in n dimensions. That's not a fundamental conceptual leap. Planck's conjecture that energy comes in packets did represent a wild leap beyond 19th century mathematical models of radiation, specifically, beyond Maxwell's equations.
John Baez asked:
"As for being "near the limits of science", does anyone here actually think we are?
The phrase "limits of sicence" was certainly poorly chosen for the conference. But yes, we are probably at or near the limits of terrestrial accelerator-based particle physics today. Future progress in particle physics is likely to come from astronomical sources. One advantage: such experiments will likely prove much less expensive than building accelerators, since they mostly involve dumping large amounts of ultrapure liquids of various kinds (cleaning fluid, mineral oil, et al.) into big underground tanks surrounded with photodetectors.
We have clearly not approaches the limits of science in other fields. Molecular biology, materials science, astronomy, etc. remain wide open.

50. **Michael Gogins**
November 7, 2007

I read "String Theory and the Crisis in Particle Physics" with great interest and appreciation. I am not sure that the cutting edge of physics already has raised the bar of intellectual difficulty over Newton and Einstein, but surely, it is not an unreasonable idea. At the very least, the difficulty of relating theory to experiment has obviously increased.

So, what kind of changes should be made in institutions, to support the "sit in an attic for 7 years and just think, like Wiles" kind of research that Woit is pointing to?

51. **Peter Woit**
November 7, 2007

Michael,

At various times I've suggested various ideas one might want to consider. It should be clear to anybody in the field of particle theory what the current incentive structure is: it is determined by how NSF and DOE grant proposals are evaluated, and by how hiring decisions are made. Everyone involved on both sides of these decisions knows how they are being made, and that the incentives are heavily on the side of not working on long-term ambitious projects that may not pan out.

If one wants to get serious about changing this, one has to

1. Make working on such projects more rewarding: announce that grant decisions will be made preferentially for such projects, have hiring committees tell candidates that they are looking for people to hire working on such projects.

2. Make routine work on research directions that have had huge investments but not worked less rewarding. Even a rumor that an NSF grant panel had decided to reject

grant proposals in active but failed subjects such as, say, string phenomenology, would have a huge effect.

As far as I can tell, the people with power to affect these decisions have reacted to the current crisis in particle physics not by discussing what can be done about it, but by getting defensive and attacking anyone who brings up the subject. Current thinking seems to be to just deny everything and hope that the LHC will solve all problems. Maybe it will, we'll see. At this point, maybe I'm wrong, but I see no hope of anything happening or any serious discussion taking place until a few years from now, after the LHC results are in.

I'd be happy to hear that I'm wrong about this, and that there are people out there taking this problem seriously and trying to do something about it.

52. **Thomas Love**
November 7, 2007

Researchers with ideas are going to pursue those ideas whether or not there is grant money available. People who follow the money should not be taken seriously as researchers

53. **Peter Woit**
November 7, 2007

Thomas,

This isn't about money, it's about jobs. For younger people, whether or not they can get a grant affects whether they can get a job or stay in one (get tenure). For people with tenure, a large part of their grant is used to pay students and postdocs. No grant, no job, if not for them, then for younger people they'd like to work with.

No one with a brain goes into this business to make money, but whether or not they can make get a full-time job that allows time for research is crucial.

54. **anon.**
November 8, 2007

'No one with a brain goes into this business to make money, but whether or not they can make get a full-time job that allows time for research is crucial.'

The predictable lame response of a string theorist would be:

'Wrong! Take Einstein, just a patent examiner in 1905, who had no problems despite having no faculty tenure at all.'

The Einstein claim is of course slightly misleading because the development of General relativity, 1915, which is really advanced mathematically, is another story and he would not have had the time and the helpful friends necessary to focus on the mathematics of tensor analysis without getting tenure:

'Up to this time [1911] Einstein had used only the simplest mathematical tools and had even been suspicious of the need for "higher mathematics", which he thought was often introduced to dumbfound the reader. However, to make progress on his problem he discussed it in Prague with a colleague, the mathematician Georg Pick, who called his attention to the mathematical theory of Ricci and Levi-Civita. In Zurich Einstein found a friend, Marcel Grossmann (1878-1936), who helped him learn the theory; and with this as a basis, he succeeded in formulating the general theory of relativity.'

– Professor Morris Kline, *Mathematical Thought from Ancient to Modern Times,* Oxford University Press, 1990, vol. 3, p. 1131.

55. **Jack**
December 8, 2007

Is Science near it's limits? Maybe, maybe not. Is Science near the limits of John Horgan's imagination? It would appear so.

56. **Gphillip**
December 14, 2007

I think it's clear that the "easy" discoveries have been made. Not to take anything away from the great minds of the past, but theirs was a simpler time in a world ripe for scientific exploration. Then monumental discoveries could be made in a small lab with a couple assistants. Now, we must invest hundreds of millions on apparatus like the LHC and employ hundreds of physicists and engineers for years to hope for a monumental discovery. This should come as no suprise. Science is nowhere near it's limits, but we are getting ever so close to ours.

In another century the scientific method will be so "picked over" it may take trillions of dollars of investment and thousands of scientists and engineers working for centuries to hope for a major discovery. Of course, all branches of science do not hit this wall of diminishing returns at the same time, but they all must pass their prime eventually. High Energy physics is just the most fundamental of sciences and will hit the wall first. All the other sciences will follow, eventually yielding fewer of natures secretes for ever more resources.

In the end, how much of all there is to be known can be illuminated by the scientific method? Perhaps one percent? Perhaps one thousandth of one percent? To know that, we would have to know how much isn't known, and that's probably something we can never know.

57. **Jack**
December 18, 2007

Part of it's about imagination, as I said before, but part of it's about patience. In Mathematics, mathematicians have learned that it is not unusual for a problem to be so hard that it will go unsolved for many hundreds of years. Some problems unsolved today are more than 2000 years old. Physicists have not experienced, over the generations, the experience of being unable to make any progress on a problem over such a timescale. Now they have been struggling with certain questions for 30 years or so, and it is making many people, journalists and physicists, uncomfortable.

Of course, in mathematics there are Lemmas. If you can't solve a problem, work on a related problem which you can solve. Progress can be made step by step in this way. Eventually, someday, you realise that the easier problems you kept working on have become Lemmas in the proof of the problem you couldn't solve before. It is not clear to me whether there is any analagous thing in physics. In mathematics you are spoilt for choice in terms of problems of all difficulties, easy, medium, hard, and incredibly hard, whereas in physics the main problems are more clearly mapped out and more easily summarized. This may seem like a good thing because things may seem simpler, but the downside might be that you have less leverage to make progress.

58. **Gphillip**
December 19, 2007

I generally separate math and science when discussing this issue. Math can always be abstracted to deeper levels. Some would say that the more abstract, the less it represents anything in the real world. Others would say the real world IS math (see "The Computational Universe" by Seth Lloyd). But I doubt that math will ever hit a wall where it takes ever larger number of people and huge expenditures to continue advancing. Math is after all just an abstraction of what we consider to be the real world. I believe that there is a subtle difference where science uses the scientific method to illuminate the secrets of nature, and math is just one of many tools employed in scientific pursuit. Math can also represent many things not associated with the scientific method, like accounting. In any case, all sciences will run their course in time. Either because they become almost completely defined with few meaningful questions left to answer, like Thermodynamics, or because the remaining questions would take unsustainable resources to pursue, like High Energy Physics. I'd guess Chemistry has probably just barley past it's peak, but lots more remains to be discovered. Biology doesn't seem to have quite reached it's peak yet, and we have probably just scratched the surface of computer science. But for math, I see no reason it can't toyed with until the end of time. That's just my own humble opinion based on my anecdotal observations though. I haven't seen any formal studies of the topic, though that would be an interesting.

Pressure Mounts to Tie String Theory to the Real World

October 30, 2007
Categories: This Week's Hype

Last week David Gross was in New Mexico, giving an "unclassified talk" at Los Alamos, and one on The State of String Theory at the Santa Fe Institute. There's a report on the Los Alamos talk from the Los Alamos Monitor, entitled Loose Strings: Pressure mounts to tie string theory to the real world. Unfortunately, pressure to tie string theory to the real world leads sometimes to reporters getting misled about such ties, since the article includes the information that:

> Located on the border of France and Switzerland, the LHC's headline tasks include the potential discovery of a Higgs boson, a relatively massive particle known as "the god particle," that would help explain how other particles have mass. Proof of its existence would tend to support string theory, according to the theorists.

Hermann Nicolai has an article in a recent issue of Nature entitled Back to Basics, on a more promising idea for tying string theory to the real world, one that has nothing to do with using string theory as an idea about unification. He reports on recent progress towards getting an exact solution of N=4 SSYM, allowing one to test whether it really is dual to a string theory.

According to a blog posting on Superstring Theory and the End of Man, we better hope that superstring theory doesn't connect to the real world, because if it does, a combination of the Anthropic Principle and the Doomsday Argument would show that humanity doesn't have much time left. The author expresses the opinion that mankind better hope that I am right about string theory, an opinion I endorse even if I disagree with his logic.

Finally, on a completely unrelated note, the latest issue of Symmetry magazine is out, featuring the results of a reader's contest to invent new particles. Third place goes to Jacobo Konigsberg, the spokesperson for CDF, who postulates the blogino, which he describes as

> Particles created by non-abelian Blog-Blog interactions. Bloginos typically are produced in a very excited state and with a high degree of spin. Even though all their properties have not yet been determined, it is commonly agreed that they exhibit considerable truthiness. They also have the annoying ability to propagate into extra dimensions, away from the blogosphere, and generate lots of phone calls.

Update: Lubos has a link to a Youtube video of a version of this talk by Gross that he gave in Berkeley on October 19, together with commentary. It appears to me essentially the same talk that Gross gave here in New York three and a half years ago, which I wrote about in my first real blog posting here. It is striking to note how little has changed in this field during this period.

Comments

1. **King Ray**
   October 31, 2007

   Well, perhaps string theorists should be tied to the real world before string theory can be tied to it.

   If string theorists had ever had a real job where they had to get actual results by some hard deadline, then they would realize that they are off on a wild goose chase. You wouldn't get away with this wandering in the wilderness for nearly 40 years in any other kind of job.

   I think theoretical physics needs leadership like Oppenheimer during the Manhattan project to set direction. At the least we need to cut off government funding for ideas that don't work.

   I have no problem however with people working on their own ideas on their own nickel. I do have a problem though with string theory monopolizing the market of ideas and funding and squeezing out all other things.

2. **Thomas Love**
   October 31, 2007

   Peter, The link to the Los Alamos Monitor article doesn't work for me. I went to the Los Alamos Monitor website and searched for David Gross. Got a blank screen in reply.

   I heard Gross speak in 2005 at a meeting honoring 100 years of relativity. It sounds like I heard the same speech you were talking about.

3. **Steve Myers**
   October 31, 2007

   Peter,
   The latest Scientific American on-line has an article called "The great Cosmic Roller-Coaster Ride" about string theory & inflation and branes & anti-branes. Aside from glossing over stuff it ends with: "In summary, string theory provides two general mechanisms for obtaining cosmic inflation: the collision of branes and the reshaing of extra-dimensional spacetime. For the first time, physicists have been able to derive concrete models of cosmic inflation rather than being forced to make uncontrolled ad hoc assumptions. The progress is very encouraging. String theory, born of efforts to explain phenomena at minuscule scales, may be writ large across the sky."

4. **Jimbo**
   October 31, 2007

   Yes, & its exactly the same talk (aka `sales pitch') with slides he showed at the U.of Oregon colloquium 8 mos ago. I suspect tho, that his Nobel spared him the admonishments any other academic speaker would have been subjected to, if they had included the word `bullshit' in their presentation.
   My respect for him has grown slightly, tho, knowing that he was arrested in one of the many anti-Vietnam war protests on the Bezerkely campus. He may have abdicated his scientific ethics of late, but at least he his political ethics were together back then.

5. **Peter Woit**
   October 31, 2007

   Steve,

   I wrote about this here:
   http://www.math.columbia.edu/~woit/wordpress/?p=609

6. **Peter Woit**
October 31, 2007

Thomas,

The link no longer works on the Los Alamos paper web-site, not clear is they automatically get rid of old content. If someone knows of another place that has this article with a working link, let me know and I'll fix it.

7. **chimpanzee (aka "joe")**
October 31, 2007

The Los Alamos article is in Google Cache here

Geezes. That article has some glaring errors:

"In 2004, Gross shared the Nobel Prize in Physics with Frank Wilczek his graduate student at Harvard [no..Princeton!], and with David Politzer, who was working on the problem separately at Princeton [no..Harvard!]"

"More recently, grousing among anti-string theorists has become louder, signified by such books as "The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next," by Les Smollin [Lee Smolin]"

I can't trust anything in this article..looks like it is written by a dyslexic/illiterate.

There are a bunch of scientist quotes all out of context. To get the correct picture, you have to watch the COMPLETE 90 minute video (as per L. Motl's blog)…with a scientific background.

The above is an extreme example of why journalists have NO BUSINESS reporting on Science. John Horgan, Timothy Ferris, George Johnson, et al. THEY AREN'T SCIENTISTS..come on.

"Know your subject matter"
— maxim (Journalism, Science, Music, et al)

That's the purpose of a physicist blog ("citizen journalism")..scientists can do the reporting yourself.

"The authors are themselves amateurs [journalists], and when they venture into scientific topics their information is limited & sometimes outright wrong.."
— Harold Zirin/Caltech, solar astronomer
[applies to the recent PBS program by T. Ferris..it was 100% nonsense]

"Remember "journalists" are merely people who couldn't qualify for jobs
which require any sort of technical background."

"Yes, he did. I was a journalist myself, and I taught many young people. The first thing you learn is that you should be aware of what you are
doing before you put your foot-in-your-mouth."

"Nevertheless, the author [J. Horgan] of the stupidity from 1997, after those ten years that have demonstrated that his stupidity is among the greatest stupidities that have ever been pronounced by homo sapiens, has the stomach to come in front of a conference in Portugal and repeat the same stupidity.
— L. Motl, End of Science blog post

"Everyone who gave this book one star should realize that this book is
entertainment. Hancock [journalist] is not a scientist or an academic of any kind – he's a journalist! … Of course Hancock tailors the facts to fit his theories – he is not constrained by truth, science, or even ethics. He is a journalist.
…
This book, and all those like it that preach pseudo-science, appeal to the majority of people in this world who are scientifically challenged. Most Americans don't have enough scientific knowledge to understand the technology they face everyday, much less untangle the fact and fantasy in this book. It is entertainment, but it's dangerous – science interpreted by a journalist!"
— reader from Cincinnatti
[critique of "Fingerprints of the Gods" — crackpot book & Discovery Channel program]

8. **jon**
November 1, 2007

Well today Frenkel and Witten have just posted a paper to the math arXiv about Langlands stuff http://arxiv.org/abs/0710.5939
Looks like Witten has put on hold his strings-related hep-th efforts. For how long is anybody's guess.

9. **IMHO**
November 1, 2007

Peter,

Feel free to erase this comment…

You are a public figure with a book deal. I wonder how smart it is for you to link to Lubos site. Have you seen his post about "…N-word zebrafish…"

Bigger men then you have lost more for doing less. Personally, I would never risk my success over principal…but that's just me.

10. **Peter Woit**
November 1, 2007

IMHO,

My linking to something in no way indicates approval of it or its author, often quite the opposite…

11. **Adud**
November 1, 2007

Well today Frenkel and Witten have just posted a paper to the math arXiv about Langlands stuff

"Geometric Endoscopy and Mirror Symmetry"

I had heard about "surgery" techniques in differential topology, and now there is "geometric endoscopy"…. I wonder how far mathematicians are willing to go with medical analogies. How about "categorical proctology" or "group gynecology"?

12. **IMHO**
    November 1, 2007

    Peter,

    I know that and I'm sure most of your readers know that.

    Your issue is that you aren't a just a blogger, you're also an investment, which means the rules are different for you.

    Just be pragmatic about things.

13. **King Ray**
    November 1, 2007

    For string theory, might I suggest "Rectocranial Sigmoidoscopy"?

14. **anon.**
    November 3, 2007

    'It is striking to note how little has changed in this field during this period.'

    Long live failure!

15. **Andr**
    November 9, 2007

    Really you have nothing to say about Lisi's paper? It seems you're one of the guys to be thanked for this "magnum opus"… 🙂

An Exceptionally Simple Theory of Everything?

November 9, 2007
Categories: Uncategorized

It's been unusually long since my last posting, with the main reasons being that

- Not much has been happening on the math/physics front…
- I've been busy learning more about geometric Langlands, which is a daunting subject. I keep intending to write something about recent work by Witten and others in this area, but saying anything both correct and intelligible seems a rather challenging task that I haven't been quite up for.

Garrett Lisi has a new paper on the arXiv, with the rather over-the-top title of An Exceptionally Simple Theory of Everything. Sabine Hossenfelder has a typically excellent posting about the paper, and Garrett has been discussing his work with people in the comment section there. Lubos Motl, has a typically, how shall I say, Lubosian posting on the topic.

I'm the first person thanked in the acknowledgment section of the paper, but at Sabine's blog Garrett explains that this is just because he is using reverse alphabetical order. I've corresponded with him in the past about his research in this area, without being able to provide any real help other than a certain amount of encouragement. Two of the ideas he is pursuing are general ones I'm also very fond of. One is well-known, and many people have also tried this, it's the idea of bringing together the internal gauge symmetry and the symmetry of local frame rotations. The problems with this are also well-known, and some have been brought up by the commenters at Sabine's blog. I don't think Garrett has found the answer to this, or that he claims to. I'm still hopeful that this line of thinking will lead somewhere, but think some dramatically different new idea about this is still needed. The other idea he likes is that of trying to interpret the fermionic degrees of freedom of the BRST method for handling gauge invariance as providing the fermions of the Standard Model. I suspect there is something to this, but to get anywhere with it, a much deeper understanding of BRST will be required. I've been spending a lot of time trying to understand some of the mathematics related to BRST in recent years, and am in the middle of writing some of this up. It seems to me that there is a lot that is not understood yet about this topic even in much simpler lower-dimensional contexts, so we're a long way from being able to really see whether something can be done with this idea in a realistic four-dimensional setting.

One idea Garrett is fond of that has generally left me cold is the idea of unification via a large simple Lie algebra like E_8. While there may be some sort of ultimate truth to this, the problem is that, just as for GUTs and for superstring models, all you're doing when you do this is changing the unification problem into the problem of what breaks the large symmetry. This change in the problem adds some new structure to it, but just doesn't seem to help very much, with the bottom line being that you get few if any testable predictions out of it (one exception is with the simplest GUTs, where you do get a prediction, proton decay, which turns out to be wrong, falsifying the models).

Anyway, I'm glad to see someone pursuing these ideas, even if they haven't come up with solutions to the underlying problems. Garrett is a serious and competent researcher who has pursued a non-traditional career path, and was recently awarded a grant to by the FQXI organization. You can read more about him in an article on their web-site.

Unfortunately, some of the reaction to Garrett's article has been depressing. A commenter who sounds well-informed but hides behind anonymity goes on about "this nonsense" (although Garrett's polite reaction to him/her did lead to a more sensible discussion). Early on in my experience with blogs I believed that no serious professional in particle physics would attack someone and try and carry on a scientific argument anonymously, so any such comments had to be coming from misguided students, or someone not in the profession. Unfortunately I've all too often seen evidence that I was wrong about this. Lubos Motl on his blog denounced the fact that Garrett's paper appeared in the hep-th section of the arXiv, then later wrote in to Sabine's blog to crow that it had been removed from hep-th. As always with the arXiv, how moderation occurs there is non-transparent, so I don't know how or why this happened. My own experience with the arXiv over trackbacks to hep-th has been a highly disturbing one. The current hep-th policy seems to be to allow any sort of nonsense to be posted there if it fits into the current string-theory-based ideology (see for example here), while suppressing any criticism of this. A paranoid person might be tempted to wonder whether hep-th is being moderated by someone so ideological and petty that criticism of string theory or including string theory critics in an acknowledgment section would be cause for having ones article removed from hep-th…

Update: I hear from Garrett that the story of this paper at the arXiv is that it was submitted to gr-qc, not hep-th. Before it was posted, it was re-classified as hep-th, and appeared there. Later on (after the appearance of Lubos's blog entry denouncing the arXiv for allowing the paper on hep-th I believe), it was re-classified again, this time as general physics (with cross-listing to hep-th).

Update: Latest news about this is that the paper has now been reclassified again, to the perfectly appropriate hep-th, cross-listed as gr-qc, although no one seems to know why this happened. Another continuing mystery is the trackback situation: there are four trackbacks to the paper, to postings by Lubos, Bee, and to Physics Forums, as well as to an old TWF from John Baez that doesn't even link to the paper. My postings still seem to be non-trackback worthy on hep-th, not that I can argue with this particular case, since the discussion elsewhere has been more substantive (except for Lubos's, which is valuable for the way it accurately represents the hysterical reaction to speculation that is not string theory speculation all too common in certain quarters).

Update: Garrett is making the news here. Whether this is a good thing is yet another question for debate on the next thread, I guess. A lot of the attraction for the media seems to be his personal story. Maybe it's a good thing for physics for people to see that one can be a theoretical physicist while surfing in Hawaii…

Update: Lisi-mania spreads. See stories in New Scientist, the Ottawa Citizen, Slashdot, and probably lots of other places I haven't noticed.

Update: Steinn Sigurdsson has an excellent posting summarizing the situation. As usual, blogs are the place to get the highest quality information about scientific issues…

Update: I've given up on keeping track of the media stories on this. For some discussion of the representation theory involved, see this posting by Jacques Distler, and comments from Garrett.

Update: The Angry Physicist examines the Distler critique in some detail.

Comments

1. **Garrett**
   November 9, 2007

   Hello Peter,
   This is a well thought out post, as usual. I did consider our short emails — and the very existence of you and this blog — encouraging enough to include you in the acknowledgments. Your concerns are all valid, and are some of the same concerns I discuss in the paper. Though I do think the way I've combined the gravitational frame-Higgs and used the MacDowell-Mansouri description of gravity is new. You are correct that I don't have a good reason or mechanism for what breaks the E8 symmetry, and this is needed. What I've done is break the symmetry by hand, including the few terms necessary to recover the action of the standard model and gravity. I am biased towards E8 because of its beauty, and similarly towards the general idea of unification. The success of electroweak symmetry breaking in the standard model is compelling evidence that symmetry breaking of this sort plays a very important role in nature, and I'm surprised you dislike the idea of applying this on a larger scale. Surely you acknowledge that successful predictions from electroweak symmetry breaking indicate it did more than just add new structure to the problem? In any case, I am very

much looking forward to reading your work on BRST, as it's a very complicated and fascinating subject, and it plays an important role in this E8 theory that I'd like to understand better.

2. **Kea**
November 9, 2007

There is a good chance that Garrett is on a watchlist. I have it on good authority that I'm on the hep-th watchlist, even without having tried to post anything there for years.

3. **Peter Woit**
November 9, 2007

Hi Garrett,

By "adding new structure" to the problem I meant to imply something not negative, but potentially positive. The new structure may be usable to constrain things and thus allow non-trivial predictions. If you are using a scalar field to do the breaking, then the question becomes how constrained this set-up is. In the electroweak theory, the Higgs sector on the one hand fits in well with the symmetries of the theory and has a limited number of free parameters, but it still has quite a few, and is the main source of the problems of the Standard model. Because of this situation, I've always been most interested in ideas about unification that tell you more about the Higgs, not so much in ideas that don't do that, but add other Higgs sectors.

That's my prejudice, but, sure, starting from other reasonable prejudices, like the desire for a single simple group to act on everything, may lead one somewhere. Maybe you'll have better luck than the GUT program, which hasn't been successful so far.

4. **Garrett**
November 9, 2007

OK, this makes sense. You must be looking forward to the LHC results even more than most, since it will provide some experimental insights into the Higgs sector.

Meanwhile, I'm sitting here with a bunch of old men, waiting for a proton to decay. 😉 But I'm not holding my breath.

These are interesting times.

5. **Coin**
November 9, 2007

This is kind of way over my head so I'm a bit afraid to comment, but what the heck.

In terms of just the pure *math* here– the one thing I'm at least superficially in a place to comment on– what Garrett's doing, with breaking down E_8 into the various gauge groups we'd need to describe reality, seems to make sense. Where I get lost is in trying to understand how you use this model to tell us something about the physical world (maybe Garrett's view of E_8 would be obviously useful or obviously unsuitable for this to someone with a good understanding of what the gauge group "does" in a gauge theory, but that person is not me). Can you help me understand what the physical significance of some of this stuff is or might be, assuming some understanding of what the groups themselves are doing?

I mean, so you've shown, it looks like, that E_8 can be decomposed into SU(3)xSU(2)xU(1)xSO(3,1)xHiggsxFermion. The mere fact that this is possible does seem suggestive of *something*. What do we gain, however, from describing all of these fields as the one big E_8 group– you know, rather than just leaving all those component groups separate with their own yang-mills theories and such (or perhaps just awkwardly x-ing them together like we do to hook up SU(3) to everything else in the Standard Model)? How does the unification of all these groups change things?

I'm meanwhile somewhat confused as to the physical significance we are meant to take from the various features of E_8's structure. I'm particularly baffled by what to make of the big root diagram which is shown in the video Bee links and in various ways in the paper. What does it mean for two roots in this diagram to have an edge between them? If I'm not mistaken (er, am I?) then from a mathematical perspective an edge between two roots in the E_8 root system polytope corresponds to the lie bracket between those two roots. But what does this mean physically, when we use E_8 as Garrett has here?

Also, what is the meaning of the "rotations" of the root system shown in the paper and the youtube video? Do these rotations correspond to anything physically meaningful, or are the rotations just showing the root system diagram in different ways to make the different groups the system breaks down into visually clear?

Meanwhile, if elementary particle fields correspond to roots in Garrett's gigantor gauge group, then what do products of those roots physically correspond to? The paper says "The interactions between all standard model and gravitational fields correspond to the Lie brackets between elements of the E8 Lie algebra, and thus to the addition of E8 roots." Hm, okay, so additions of E8 roots produce interactions between? What are those "interactions"? Or is this specified by the yang-mills action or something?

Also a little confused: so counting up all the fields we expect to see in nature we find they fit with 222 of the roots in your E_8 root system, leaving 18 "extra" roots whose properties as fields are described on page 22 of the paper. The paper seems to be saying that these 18 new fields each act kinda like the Higgs, and each one is identified with a specific one of three generations and a specific color or anti-color. If this reading is correct, what do these generation/color identifications refer to? Does this have to do with the color or anti-color of quark that the field is able to interact with, or is the idea that the field carries color charge, or…?

One more general, possibly dumb question, is there any potential form of correspondence which one could draw between how E_8 is used here, and string theories which use E_8 (or products of E_8) as a symmetry group? Or is the usage of a "symmetry group" simply too different in these different contexts?

Trying to understand, thanks!

6. **more questions**
November 9, 2007

As long as Coin is asking questions, I didn't understand (1) why this doesn't violate the Coleman-Mandula theorem, and, (2) what about the nonrenormalizability of GR?

7. **Coin**
November 9, 2007

MQ, Garrett does seem to offer an argument concerning your (1) in a reply to Moshe in the comments section of the Backreaction post:

> 1. Yes, the Coleman-Mandula theorem assumes a background spacetime with Poincare symmetry, but this theory doesn't have this background spacetime — with a cosmological constant, the vacuum spacetime is deSitter. So this theory avoids one of the necessary assumptions of the theorem, and is able to unify gravity with the other gauge fields. On small scales though, Poincare symmetry is a good approximation, and on those scales gravity and the other gauge feels are separate, in accordance with the theorem. (I'm not the first person to dodge C-M this way.)

Several more posts over the course of that thread drill down on this point further…

8. **Garrett**
November 9, 2007

more questions:

(1) The first person I know of to point out this loophole in Coleman-Mandula was Thomas Love (a visitor here) in his 1987 dissertation. There is also a discussion of this loophole in this recent paper by F. Nesti and R. Percacci: Graviweak Unification. Or you can go to the source and look at Coleman and Mandula's paper, in which their first condition for the theorem is "G contains a subgroup locally isomorphic to the Poincare group." The G = E8 I am using does not contain a subgroup locally isomorphic to the Poincare group, it contains the subgroup SO(4,1) — the symmetry group of de Sitter spacetime.

(2) I'm banking on the LQG community to crack this one. So multiply the odds of this E8 Theory being right times the odds of LQG finding the right answers for quantizing the theory… and I'm first to admit it's a long shot. But I think it's got a chance, which is why I work on it.

9. **Garrett**
November 9, 2007

Hi Coin,
Peter teaches classes in representation theory, so he can answer most of these questions better than I can, but I can at least help out with what's in this paper. You have a correct understanding of what's going on, so I'll just answer your specific questions.

"What do we gain, however, from describing all of these fields as the one big E_8 group– you know, rather than just leaving all those component groups separate with their own yang-mills theories and such (or perhaps just awkwardly x-ing them together like we do to hook up SU(3) to everything else in the Standard Model)? How does the unification of all these groups change things?"

Because all these fields are parts of E8, we can assemble an E8 principal bundle connection (technically a superconnection) which consists of 1-forms and Grassmann fields valued in this E8 Lie algebra, and use the curvature of this big connection to get the dynamics. The curvature, in the action, determines how E8 interacts with itself. And since everything is part of E8, this corresponds with how all the fields of the standard model and gravity interact with each other. This action is built by hand to match the standard model, which is an inadequacy of the theory, but it's very concise. And I think it's bloody amazing that this works at all.

"I'm meanwhile somewhat confused as to the physical significance we are meant to take from the various features of E_8's structure."

Physically, as this theory develops it should make definite predictions for the coupling constants, predict a handful of new particles and some non-standard interactions, and (if things go astoundingly well) have something to say about the particle masses. These predictions may end up being wrong, killing the theory, but so far things are looking good.

"I'm particularly baffled by what to make of the big root diagram"

The root diagrams correspond to the structure of the Lie algebra. If two E8 roots add to give a third (in eight dimensional Euclidean root space) then the Lie bracket of the corresponding two Lie algebra basis elements give the third. In the paper, I describe which roots correspond to which elementary particles. Also, since the projection used to plot the eight dimensional root system is linear, you can determine particle interactions by adding these roots together as two dimensional vectors, extending from the origin — it's fun, try it. This is standard representation theory, and it's very pretty. In my opinion, Peter doesn't push his own subject hard enough — I wish I had learned about this stuff earlier than I did, it's wonderful.

"are the rotations just showing the root system diagram in different ways to make the different groups the system breaks down into visually clear?"

Yes, precisely.

"Hm, okay, so additions of E8 roots produce interactions between? What are those "interactions"? Or is this specified by the yang-mills action or something?"

Yes, the addition of the roots corresponds to the Lie brackets between fields, which is in the curvature, which is in the action, and this gives the interactions between particles, appearing as Feynman vertices in QFT calculations.

"these 18 new fields each act kinda like the Higgs, and each one is identified with a specific one of three generations and a specific color or anti-color. If this reading is correct, what do these generation/color identifications refer to? Does this have to do with the color or anti-color of quark that the field is able to interact with, or is the idea that the field carries color charge, or…?"

Yes, exactly so. These new scalar fields have color quantum numbers, and so interact with the quarks and gluons.
In my dreams at night, these new Higgs fields give the CKM matrix, but I don't know how that works when the sun comes up.
They're also a potential dark matter candidate, but I don't say that in my paper because I think that's a cliche.

"is there any potential form of correspondence which one could draw between how E_8 is used here, and string theories which use E_8 (or products of E_8) as a symmetry group? Or is the usage of a "symmetry group" simply too different in these different contexts?"

They are completely different theories, which happen to use related Lie groups. I could list many specific differences:
non-compact E8 vs compact E8 x E8
gravity in E8 vs gravity via other
four dimensional spacetime vs 11 dimensional spacetime with Kaluza-Klein orbifold compactifications
principal bundle connection vs strings, branes, and who knows what
etc.

"Trying to understand, thanks!"

I consider it a testament to the simplicity of the theory that you seem to have understood most of it after a first reading. I should put that in among the differences list… 😉

10. **Coin**
November 9, 2007

I consider it a testament to the simplicity of the theory that you seem to have understood most of it after a first reading.

Hm, I think that would be an exaggeration to say I understand most of it. But thanks for the clarifications, this helps 🙂

11. **Thomas Love**
November 9, 2007

The Coleman-Mandula theorem applies to "All Possible Symmetries of the S-Matrix", the title of their paper. The S-matrix formalism is based on particle democracy, there are no fundamental particles. In "The Geometry of Elementary Particles", my 1987 dissertation which Garrett mentioned, there are truely elementary particles and the S-matrix formalism is not valid, hence the Coleman-Mandula theorem is not applicable. I use U(3,2) as the symmetry of a complex spacetime by passing the no-go theorems (which relate to the Poincare group).

12. **Tony Smith**
November 9, 2007

About Coleman-Mandula, Steven Weinberg said at pages 382-384 of his book The Quantum Theory of Fields, Vol. III (Cambridge 2000):

"… The proof of the Coleman-Mandula theorem … makes it clear that the list of possible bosonic symmetry generators is essentially the same in d greater than 2 spacetime dimensions as in four spacetime dimensions:
… there are only the momentum d-vector Pu, a Lorentz generator Juv = -Jvu (with u and v here running over the values 1, 2, … , d-1, 0), and various Lorentz scalar 'charges' …
the fermionic symmetry generators furnish a representation of the homogeneous Lorentz group … or, strictly speaking, of its covering group Spin(d-1,1). …
The anticommutators of the fermionic symmetry generators with each other are bosonic symmetry generators, and therefore must be a linear combination of the Pu, Juv, and various conserved scalars. …
the general fermionic symmetry generator must transform according to the fundamental spinor representations of the Lorentz group … and not in higher spinor representations, such as those obtained by adding vector indices to a spinor. …".

In short, since E8 is the sum of the adjoint representation and a half-spinor representation of Spin(16),
if Garrett builds his model with respect to Lorentz, spinor, etc representations based on Spin(16) consistently with Weinberg's work,
then
Garrett's model could well satisfy Coleman-Mandula.

Tony Smith

13. **Berlin**
November 10, 2007

Garrett: your remark at the end of section 3.2.1. Can this be translated into 'the theory is background independent', one of Smolins reasons to reject strings?

14. **Garrett**
November 10, 2007

Berlin: It implies that, yes.

15. **Garrett**
November 10, 2007

Berlin: I should clarify that this is not a complete quantum theory of everything, so in that sense it is incomplete. It will need to be successfully partnered with LQG and/or QFT — this is nontrivial, and time will tell whether it works or not. But the pieces are in place.

16. **Bee**
November 11, 2007

Hi Peter,

Thanks for the link. Anybody here knows how the arxiv makes decisions like this?

A paranoid person might be tempted to wonder whether hep-th is being moderated by someone so ideological and petty that criticism of string theory or including string theory critics in an acknowledgment section would be cause for having ones article removed from hep-th…

It might simply be insecurity. Whoever makes this decision they will try to make it such as to not upset the majority of users. And I doubt they do actually study the paper in great detail. Best,

B.

17. **Lucci**
November 11, 2007

Garrett: Your use of Clifford bundles, plus the fact that your theory allows the Standard Model to be extended, suggests a possible tie-in with ELKO spinor-fields, which have been proposed as an extension of the SM (and also – pardon the cliche! – as a dark matter candidate). For a very recent paper on ELKO, in which Clifford bundles are used explicitly, see: http://www.arxiv.org/abs/0711.1103
(Disclaimer: I am not in any way involved in research on ELKO myself.)

18. **D R Lunsford**
November 11, 2007

This seems a lot like Tony Smith's work. Mentioning Tony in your acknowledgments is guaranteed to get you watched. Calling him your "friend" gets you banned. There is neither integrity nor honor to be found there.

It's interesting work in a sense, but it's still phenomenology and doesn't improve the SM. The statements about gravity are much more doubtful. This approach has been tried again and again and always meets with the same problems.

-drl

19. **John Gonsowski**
November 12, 2007

Garrett references John Baez's Octonion paper and that paper also thanks Tony. Don't think they will be banning Baez any time soon. Funny, models with a great math basis get frowned on while philosophical ideas like the landscape get cheered. Yet they make it sound like it's those models with the great math that are too philosophical?

20. **Chris Oakley**
November 12, 2007

As happens more often than not, to the extent that I understand the issues (not very much here), I concur with DRL. I want something that simplifies. From that point of view E8 – in any context – is a non-starter.

21. **Marcus**
November 13, 2007

Garrett is giving the seminar talk at the ILQGS tomorrow.
His slides are online here
http://relativity.phys.lsu.edu/ilqgs/lisi111307.pdf

The audio will be available here
http://relativity.phys.lsu.edu/ilqgs

these International LQG Seminar discussions are often lively in part because they are conference calls where everybody has the slides to scroll through]
and you occasionally get questions and comment from Ashtekar, Rovelli, Freidel, unidentified Perimeter, Marseille, Penn State people.

Audio and slides from past seminars are available—same link.

I'll be interested to see how Garrett Lisi and Carlo Rovelli get along, to the extent that one can communicate usefully in that intercontinent telephone conference call setting.

22. **Shantanu**
November 13, 2007

Peter, currently I DO see 4 trackbacks (none of which refer to your blog, though)

23. **Peter Woit**
November 13, 2007

Shantanu,

Yes, the 4 trackbacks that are there now are the ones I was describing, sorry for the confusing way that was written.

24. **Marcus**
November 13, 2007

The ILQGS talk went very well. Abhay Ashtekar and Lee Smolin each had several questions/comments leading to discussion. An expanded set of slides is available.
slides are here
http://relativity.phys.lsu.edu/ilqgs/lisi111307_2.pdf
audio is here
http://relativity.phys.lsu.edu/ilqgs

Thanks to Jorge Pullin for organizing the seminar and making it available online!

25. **Bee**
November 13, 2007

Hi Peter,

Thanks for the update on the arXiv classification issue. Still I'd like to know, could anybody fill me in how decisions like that are made? Best,

B.

26. **Peter Woit**
November 13, 2007

Bee,

I don't know exactly how this works, and it's my impression that the process is rather non-transparent. The arXiv has a difficult problem on its hands of how to maintain a reasonable level of quality, and not get flooded by junk. There are moderators for each section of the arXiv who supposedly take a quick look at submissions and decide if they are correctly categorized and meet their standards. The places where they describe how this is supposed to work are

http://arxiv.org/help/moderation

and

http://arxiv.org/help/endorsement

I have no idea what happened with Garrett's paper (and I gather he doesn't either), but presumably decisions were made by gr-qc and hep-th moderators, and perhaps other people got involved later after questions were raised about the decision to classify as gen-ph.

27. **alex**
November 13, 2007

"Yes, exactly so. These new scalar fields have color quantum numbers, and so interact with the quarks and gluons.
In my dreams at night, these new Higgs fields give the CKM matrix, but I don't know how that works when the sun comes up.
They're also a potential dark matter candidate, but I don't say that in my paper because I think that's a cliche."

If your dark matter candidate has color quantum numbers it should interact strongly with ordinary matter. In MSSM the dark matter candidate is typically a neutralino. In

some models it's a sneutrino.
So, in either case it only couples gravitationally – that is why it is referred to as a dark matter candidate.
So, if some construction predicted, say, a gluino LSP (which is strongly interacting) that would be a disaster.

So if Garret Lisi thinks that a colored particle is a "potential dark matter candidate"… well, I'm, to say the least, puzzled.

28. **alex**
November 13, 2007

I said:"So, in either case it only couples gravitationally – that is why it is referred to as a dark matter candidate."
Correction: it's only true for the right-handed sneutrino, the neutralino is also weakly coupled, of course.

29. **Bee**
November 14, 2007

Thanks! Btw, I found someone for the conference I mentioned.

30. **Typo Guy**
November 14, 2007

The link to the Telegraph article is slightly defective.

31. **Peter Woit**
November 14, 2007

Typo Guy,

Thanks, fixed!!

32. **Hendrik**
November 15, 2007

NewScientist just published a review of Garrett Lisi's approach at http://www.newscientist.com/article/mg19626303.900

33. **berlin**
November 15, 2007

I am puzzled by (among others..) the gravitational part of tabel 9. Does the spin 2 graviton (still) exist in the theory?

berlin

34. **Marcus**
November 15, 2007

In response to "berlin"

> Does the spin 2 graviton (still) exist in the theory?

Loll recently put the business about gravitons succinctly:
The failure of the perturbative approach to quantum gravity in terms of linear fluctuations around a fixed background metric implies that the fundamental dynamical degrees of freedom of quantum gravity at the Planck scale are definitely not gravitons.

That is (from http://arxiv.org/abs/0711.0273) if a theory is fundamental, it should not have gravitons.

One should be able to set up certain fixed situations in which a graviton can be derived as an approximation. But the graviton should not exist in the theory as a fundamental descriptor. If it does exist, then the theory would not be fundamental–according to what Renate Loll says.

I would therefore be surprised if it turned out that the E8 theory being developed by Garrett Lisi (and possibly others lately) should turn out harbor the graviton as a fundamental component.

35. **Eric**
November 15, 2007

Actually, there isn't much to Lisi's work. It's mostly just hocus pocus and part of the Smolin et. al. public relations initiative to sell 'alternative' approaches to physics. Just read his book and you can see the blueprint, the romantic image of a lone maverick making breakthroughs in physics in between surfing and snowboarding. It's just marketing.

36. **Bee**
November 15, 2007

Given my rather boring lifestyle I think about becoming a ghost writer, and hire someone to market my papers with a better story! Anybody could please point me towards a 30something, white, male, single, good-looking US citizen, who has an interest in extreme sports of whatever kind, grew up under difficult circumstances (but doesn't suffer from an embarrassing accent), good socializing skills, likes to speak in front of people, does well on TV, preferably works in a patent office or likewise, speaks Spanish and French fluently, and, well, has maybe taken some physics classes in high school?

37. **Garrett**
November 15, 2007

Wow, apparently I'm an imaginary construct dreamed up by Lee Smolin!

Actually, this would explain quite a lot.

38. **Eric**
November 15, 2007

Hi Sabine,
Actually your place in the Smoliniverse is as the young, beautiful woman who happens to be a brilliant and creative physicist. It's very sly of Lee to choose you as part of his public relations campaign. 😉

39. **J. Barrett**
November 15, 2007

Is the title of the paper so much "over the top" as it is descriptive and a terrible maths pun, being that M8 is both an exceptional group and a simple group?

40. **Coin**
November 15, 2007

One should be able to set up certain fixed situations in which a graviton can be derived as an approximation. But the graviton should not exist in the theory as a fundamental descriptor. If it does exist, then the theory would not be fundamental–according to what Renate Loll says.

Hm, while I can see attractive aspects to that approach, that kind of sounds like a dramatic step to take. Do there already exist any other theories of quantum gravity, besides Loll's, which eschew the graviton or take the "graviton as approximation" approach you describe?*

Meanwhile I find Loll's argument in the paper you link against the graviton somewhat unconclusive. "Well, we've been trying to get useful answers out of this construct for decades and haven't succeeded, so it's a good bet we're doing something wrong" sounds like good *strategy* to me, but he doesn't seem to actually be putting forth an argument about reality there, only an argument about "how to proceed". I don't see any reason that just because we can't describe the graviton perturbatively, that would mean it doesn't exist– since, as far as I understand, perturbation theory is supposed to just be an approximation anyway. (And this is of course assuming that perturbatively modeling the graviton is impossible and not just too hard for anyone to manage right now). Am I missing something about Loll's argument?

* Does LQG, for example, have a graviton? Looking I am finding references to a "graviton propagator" in LQG but it is not immediately obvious whether that's the same thing.

41. **Peter Woit**
November 15, 2007

Please, Garrett's paper is a flimsy excuse for turning this into a quantum gravity discussion forum, which is something I don't want to run. Enough about this unless it directly has some relevance to the paper.

42. **Coin**
November 15, 2007

All right, sorry about that. I'll go harass Marcus on physicsforums 🙂

43. **Marcus**
November 15, 2007

J. Barrett

> Is the title of the paper so much "over the top" as it is descriptive and a terrible maths pun, being that E8 is both an exceptional group and a simple group?

Exactly!

Why do you seem to be the only person to get that? The funniest part was hearing the solemn preaching: "I think it would be better if you were not so over-the-top etc etc…"

I've been waiting for someone to point that out here at NEW. You have my gratitude and respect, J Barrett, whoever you are.

44. **Brian Mingus**
November 15, 2007

What sort of a minimum background would be needed to actually grok this paper?

45. **Aaron Bergman**
November 16, 2007

Believe me, nobody missed the pun.

46. **AGeek**
November 16, 2007

Coin, Renate Loll is a she (her name should be a clue). And assuming that "graviton" means what it's normally taken to mean, a perturbative free state propagating on some background a la DeWitt, then to say that gravity can not be described perturbatively is to say that gravitons do not exist.

47. **Dany**
November 16, 2007

P. Woit: "One idea Garrett is fond of that has generally left me cold is the idea of unification via a large simple Lie algebra like E8. While there may be some sort of ultimate truth to this, the problem is that, just as for GUTs and for superstring models, all you're doing when you do this is changing the unification problem into the problem of what breaks the large symmetry. This change in the problem adds some new structure to it, but just doesn't seem to help very much, with the bottom line being that you get few if any testable predictions out of it (one exception is with the simplest GUTs, where you do get a prediction, proton decay, which turns out to be wrong, falsifying the models)."

I admit that I didn't read the paper. However, in my view, all that PR tararam have very positive outcome: it again attract the attention to the Cayley numerical system which seems to be the natural candidate for QG and unification of all fundamental interactions. Since the connection with the GR and relativistic QM is the necessary constraint, one should define the real octonion valued self adjoint operators. My own experience with them is that it is exceptionally not simple problem (indeed, even the solution will be far from being theory of everything).

Regards, Dany.

48. **DB**
November 16, 2007

In the comments section to Lubos Motl's blog entry on Lisi's paper:

http://motls.blogspot.com/2007/11/theory-of-everything-triples-traffic.html

he reveals the apparent cause of his departure from Harvard as follows:

"The obvious and inevitable result is that those people with IQ below 100 not only control most of the general public but start to include ever increasing groups of people.

This physics-related pressure was at least as important for me to leave those circles as the purely political issues about the academic freedom because I became pretty much certain that all these things would be getting worse as time goes.

When I was first suggested by relatively powerful people that I should have been treating complete idiots such as Peter Woit as my peers if not more, it was just way too much for me. We may be ready that the society may misevaluate many things, nothing is perfect, but these things are just many orders of magnitude out of proportion.

What about next, I would be thinking. Would cranks with their 'theories of everything' who know less than 1% what I do and whose IQ is 45 below mine – literally an inferior species – would be placed upon us or even dictate what we can think about physics? Well, this epoch just here. It has become politically incorrect to say that what surfers like Garrett Lisi are doing are light years away from what theoretical physics is. The closer one is to the top of the real physics, the most impossible it is for him or her to declare any opinions. With a realistic idea about psychology and social science, where do you think that the society will be going if the relative influences are arranged in this way?"

Speaks for itself, doesn't it?

49. **wolfgang**
November 16, 2007

Garrett,

it seems that a central idea of your paper is the claim that BRST ghosts can become real fermions due to the special properties of E8 and your action. But you do not really explain this 'trick' in your paper (and you do not discuss quantization really).
Do you explain this somewhere else or do you have plans to elaborate on this point in a follow-up paper?

50. **Peter Woit**
November 16, 2007

Brian,

To understand the paper, at a minimum you need a graduate level education in particle physics, including a good understanding of the theory of Lie algebras.

On the whole, I think the hullabaloo in the press and on the internet isn't a good thing. There's a huge amount of nonsense being spread, of different sorts:

1. The press stories aren't awful, with reporters mostly trying to include some skeptical comments about how speculative this is, while at the same time as much as possible making an enthusiastic story about the latest "theory of everything". Unfortunately, for reasons I explain in the latest posting here, I think these kinds of stories don't really do much in terms of explaining what is really going on to people, often giving a misleading impression.

2. Comments on Slashdot and the blogs are all too much dominated by the huge number of people who think it is a good idea to write in about things they don't understand, happily spreading misinformation or irrelevancy, and burying serious comments under a mountain of junk. The noise to signal ratio on Slashdot is so high that very few people who actually know something about this subject are willing to waste their time reading the comments or trying to contribute to the discussion. Too many comments on this blog and on others come from people who don't actually know much about the topic at hand, but feel compelled to share whatever is on their mind anyway.

3. Some blogs and commenters try and fit this into the ongoing string theory/LQG warfare, which it isn't especially relevant to. As usual Lubos can be counted on to obscure whatever sensible criticism he might have with crazed rants about how people who aren't string theorists are lower life forms.

Blogs do sometimes manage to provide a place for some sensible discussion, even amidst a heavy helping of nonsense. In this case, the best I've seen is Sabine's blog, which has provided a forum for Garrett to discuss some of the issues raised by his paper in detail with her and others.

51. **Aaron Bergman**
November 16, 2007

As best I can tell, he uses the term BRST a few times, but there is no real BRST procedure in the paper. It's only used as a term to justify the formal addition of grassman and commuting variables in his "connection". But, of course, the BRST symmetry is a Grassman symmetry, not a commuting symmetry like E_{8+8}, so (like many other things in the paper) it doesn't really make sense to me.

52. **Peter Woit**
November 16, 2007

DB,

Still unclear to me whether Lubos jumped or was pushed from Harvard. But it is clear that one part of the story was his being driven over the edge by the fact that criticism of string theory has been taken much more seriously in recent years, both by the public and by the physics community.

53. **Mark Paris**
November 16, 2007

I agree with Garrett that this is a very exciting time. Congratulations to him on getting some very interesting and compeling work out. It's encouraging me to take a break from dynamical coulped-channel approach to meson production reactions to have another look at some issues of GR that I've thought about in the past.

Curiously, the question of renormalization doesn't appear to be getting too much play here. It seems that while the E8 unification proposed could solve some issues of organization, the fundamental issues of spacetime properties and particle properties still remain distinct. Garrett's bet that LQG will solve this issue is probably not all that much of a sucker's bet. It seems like background independence will be a key concept in the resolution — to me anyway.

But that's only "half" the problem. There seem to be two issues regarding renormalization. The non-renormalizability of GR has already been mentioned. But also, loop integrations will apparently still lead to divergences in the E8 unification — the theory enjoys no supersymmetry cancellations, for example. Then we're stuck with infinite renormalizations — fine, we're used to it. And physics can still be done. But the precision required to fix the couplings will make, I fear, accurate detailed predictions impossible. Spectroscopy make still be possible however.

Background independence seems to offer a way out of this aesthetically unpleasing situation.

54. **Aaron Bergman**
November 16, 2007

the question of renormalization doesn't appear to be getting too much play here.

Well, yes, because the theory has any number of barriers to being quantized. And even before that, the theory breaks the symmetry explicitly by the Lagrangian, so it's tough to say that things are really being unified here.

As for couplings, there are none as best I can tell. So, it's very unclear how one recovers the standard model from his structure.

55. **Michael Crowley**
November 16, 2007

Hi Peter,

Just wanted to let you know there is a very long and passionate discussion of this gentleman's theory on my favorite website, Democratic Underground. Naturally, we are not scientists, but I'm happily surprised by the passion aroused by this post. Just thought you might find it interesting.

http://www.democraticunderground.com/discuss/duboard.php?az=view_all&address=389×2281653

Hope that link works; I'm not sure how to post links here.

Take care, and thanks again for your marvelous book.

Mike

56. **Michael Crowley**
November 16, 2007

P.S.

I'm obviously "Mike03", making a fool of myself as usual and also referring people to your blog.

57. **Bee**
November 17, 2007

Regarding the pun: It might be perfectly obvious to everybody who works in theoretical physics, but I doubt many people of the broader public or even the journalists got it. Was it explained anywhere than here? I am constantly making terrible puns like this, which is always source of a broad confusion. But besides this, it's somewhat unfair to blame things on the title, there are other examples (testing quantum gravity! testing string theory!) that go the same way. It's probably a consequence of of people's sensitivity to news become lower and lower, because there's such an over flow of unbalanced hypes that needs to be filtered (not only in science). It's a very bad trend though. I mean, I do appreciate attention for theoretical physics, but things like this make the news, nothing comes out of it for the next some years, no revolution, no incredible insight, no change in world view etc, and people will start to wonder 'what ever happened to', do these theoretical physicists just make vacuous announcements that never come down to anything? All the while support dwindles for the less spectacular basics.

Let me give you an example. My husband is working for a scientific publisher in Germany. They publish a book series that contains literally every stupid fact about material sciences you need, and there's still data that's added since people are working in the field. It's an extremely boring read, it gets sold maybe to some thousand libraries worldwide for an horrendous price, since it takes a lot of time and effort editing. Nevertheless, it's an essential for anybody working in the field. Now it looks like the publisher will stop the series because it just doesn't pay off. I've suggested they apply for governmental, but not sure if they will do (its a rather large publisher having plenty of better fields to focus on). What I am trying to say is that not all we do is hip and cool and we're not all surfer dudes making E8 animations. Events like this raise in me the concern that support might go increasingly into media-suitable research.

Hi Garrett:

"Wow, apparently I'm an imaginary construct dreamed up by Lee Smolin!"

I neither said that, nor meant to say that. I am just as always surprised how much such stories about the alleged outsider (surfer dude! no university affiliation!) attract cheerful attention, whereas the 'inside of the ivory tower' is accused of ignorance and arrogance.

Hi Eric:

" Hi Sabine, Actually your place in the Smoliniverse is as the young, beautiful woman who happens to be a brilliant and creative physicist. It's very sly of Lee to choose you as part of his public relations campaign."

I know you didn't mean it to be insulting, but it is. I'm neither Lee's nor anybodies PR agent.

Best,

B.

58. **Bee**
November 17, 2007

"I've suggested they apply for governmental,"

should be

"I've suggested they apply for governmental support,"

59. **TE**
November 17, 2007

I would like to see the action expanded explicitly in component fields in a notation that particle physicists can understand.

Then, by inspection we should be able to check that:

1) All fields propagate with the correct signs in their kinetic terms
2) Higher derivative couplings are absent or suppressed by the Planck scale
3) All fields have the correct spin-statistics relation

or other obvious signs of inconsistency.

So far it is not clear to me that this theory is even consistent at low energies. I have not spent the any time decoding the notation to see if the above consistency constraints are satisfied. But I shouldn't have to… these issues are absolutely crucial and should be addressed prominently and explicitly in the paper.

Only after these basic hurdles are mounted, we can begin to discuss whether the model is useful or addresses any important problems.

60. **Garrett**
November 17, 2007

TE:
The action is written down in an efficient expression on page 25. I did use some fancy math tools, including Clifford algebra and the Hodge star, instead of using indices. However, over the following two pages I expanded the terms of this action in detail, including writing out the resulting Dirac action in curved spacetime, in indexed components. I did this because I share your desire for a complete and understandable exposition, in conventional notation.

You should be able to glance at the expanded, local coordinate form of these expressions and confirm they match those of the Standard Model, with all the correct signs, factors of 1/2, no higher derivative couplings, and the correct spin statistics. It is true that the original action was chosen by hand such that this comes out, but given the efficient expression used, I consider it non-trivial that this works.

61. **Eric**
November 17, 2007

Sabine,
I'm sorry that you found that insulting, but I was just making a joke and trying to complement you at the same time. The basic point is that if Smolin did want to use someone for PR, you would be a good choice. In any case, let's face it, this whole media thing with Lisi's paper is completely unwarranted. There's really no content there, just some big statements and nice pictures. It's really amazing that this work was funded by the Foundational Questions Institute, unless you consider that Smolin is on the scientific advisory panel. Even Smolin I'm sure knows that this work is wrong, but I believe he has an ulterior motive in supporting it.

62. **Bee**
November 17, 2007

Hi Eric,

I was just making a joke and trying to complement you at the same time.

Apology accepted.

if Smolin did want to use someone for PR, you would be a good choice

Definitely not. I'm a researcher, Eric. The attention my blog currently gets is about the maximum I am comfortable with. I am not writing it because I want to advertise PI, Lee's book or my papers, but because I like writing it. Since I am currently not teaching I find it a nice way to contribute my part to spreading knowledge, and to share the fascination my job brings – still, and still in new ways.

In any case, let's face it, this whole media thing with Lisi's paper is completely unwarranted. There's really no content there, just some big statements and nice pictures.

The paper has content, and I've explained in my post in great detail which. What it does not provide in my opinion is a Theory of Everything, so I agree that the media hype is unwarranted.

It's really amazing that this work was funded by the Foundational Questions Institute, unless you consider that Smolin is on the scientific advisory panel. Even Smolin I'm sure knows that this work is wrong, but I believe he has an ulterior motive in supporting it.

I don't want to comment on Lee's opinions, but it is quite astonishing to me how you can be 'sure' about what he thinks. Best,

B.

63. **Marcus**
November 17, 2007

When you see a new theory in formation it's legit to guess that the gaps will be filled, new things found to reconcile it with observation, and the theory will complete itself (if it looks to you like it will) and it's likewise kosher to guess that the gaps and imperfections won't be worked out and the theory won't complete.

Then if it is completed and firm predictions are derived it's anybody's guess whether experiments confirm or refute. Whether its right or wrong is a later issue. With the E8 theory we are seeing the outlines of something emerge and it seems to have both some problems and some nice features.

What's not acceptable is to hammer the theory relentlessly as if to punish it for the fact that it attracted general public interest. Whatever the public does or does not do, when you crit some new theory development it should be constructive—when you point out problems you should acknowledge how they might be resolved, at least in the first weeks and months when the whole thing, the only thing that matters, is whether or not the theory gets other researchers interested in working on it.

People in the public don't have to be protected from believing something might work. Plenty of them are smart and skeptical enough to know that proposed ideas often don't work, and they like to hear about new ideas anyway. It's common sense that new ideas can interbreed and morph and help start other ideas—that some will disappear forever and others you will hear about a few years down the road when they re-surface. I think the public knows that, or a substantial sector does.

In Garrett's case, what I read that he said sounded calm and forthright enough. He was constantly pointing to parts of the theory that he was dissatisfied with and looked to

improve. And he was repeatedly pointing out that it could turn out wrong—and might not agree with experiment.
I heard pride, but I did not hear hype.

The public reaction I saw was interest, but not whole-hog credulity. I don't think it was contrary to the longterm interests of science. Just normal reaction to some good news. It's good news that this new idea is out there trying to take shape, whether it's eventually shown right or wrong. And I really doubt anyone can confidently say at this point.

64. **Peter Woit**
November 17, 2007

Eric,

I'll delete any more comments insulting people, I don't want my blog used for this. You might want to note that this tactic hasn't worked for Lubos, I suspect one reason Garrett's paper got so much attention was that people read what Lubos had to say on the topic, and drew the conclusion that there must be something to it.

65. **Arun**
November 17, 2007

I'm now veering to the opinion that science should be created in privacy, like babies.

66. **King Ray**
November 17, 2007

Garrett,

I've only scanned through your paper briefly, but I can already say that your theory is 10^500 times better than string theory!

Does your theory shed any light on why the electro-weak group is U(2) and the electro-strong group is U(3), and the overall group is S(U(3)xU(2))? I've always wondered why the U(3) and U(2) determinants were related in that way, i.e., epsilon_abc*epsilon_AB is an invariant (lower case indices are electrostrong 1-3 indices, upper case are electroweak 1-2 indices). This has to do with the global structure of the gauge group of the standard model. If you assume that the global group is S(U(3)xU(2)), and that anomalies cancel in each family, then you get a unique solution for the hypercharges in a family (given the SU(3) and SU(2) reps of the quarks and leptons):

http://prola.aps.org/abstract/PRD/v43/i8/p2709_1

In your theory it is probably just due to the E8 structure. Have you looked at any possible anomalies and their cancellations?

Keep up the good work. I agree with Lee Smolin, it seems to be the most promising TOE for a while if everything works out. I never liked the plethora of gauge bosons that came with large gauge group GUTs.

BTW, I have surfed at Pt. Mugu. Very powerful waves there.

67. **Garrett**
November 17, 2007

It was. 😉

68. **Garrett**
November 17, 2007

(My last comment was to Arun)

69. **Garrett**
November 17, 2007

King Ray:

Almost all GUT's explain S(U(3)xU(2)) the same way, and this one is no different.

I haven't investigated anomalies yet, no.

70. **King Ray**
November 17, 2007

Garrett,

I can understand how larger gauge groups explain the global structure, for instance in SU(5) the U(3) and U(2) are block diagonal in the 5×5 SU(5) matrix and the total determinant must be 1, implying S(U(3)xU(2)) after breaking the SU(5), but can you elaborate a little on how it works in your theory? You don't have many extra gauge bosons like SU(5) or SO(16), do you?

71. **Garrett**
November 17, 2007

King Ray:

That's what the paper's for.

72. **King Ray**
November 17, 2007

Garrett, ok, thanks, and BTW I meant SO(10) not SO(16)! Brain cramp or senior moment…

73. **Kralizec**
November 17, 2007

Sabine Hossenfelder (Bee) said:

> I am just as always surprised how much such stories about the alleged outsider (surfer dude! no university affiliation!) attract cheerful attention, whereas the 'inside of the ivory tower' is accused of ignorance and arrogance.

The familiar sort of story you mention seems to be a product of the recurring antagonism between "the many" and "the few," or "the people" and "the great." Aristotle's *Politics* and Machiavelli's *Discourses on Livy* are both helpful regarding the properties and interactions of these recurring factions. Popular writers seem to have cast Garrett Lisi in the role of a man of the people who is challenging the great and proving again that the people are their equals in every way (including theoretical physics). I think Machiavelli discusses examples in which one of the great suspects that a man of the people is being supported covertly by another of the great. He appears to teach that the antagonism between people and great is reproduced among factions of the great.

In case you decide to read the works I named, I'll just make mention that Carnes Lord and Joe Sachs are good translators of Aristotle and that the best available translation of Machiavelli's *Discourses* is probably Harvey Mansfield's. Mansfield is also highly regarded in some circles as an interpreter of both Aristotle and Machiavelli.

74. **mitchell porter**
November 17, 2007

In the discussion at Sabine's, Garrett seems confident that "quantum E8 theory" is at least mathematically well-defined and therefore must be exploiting *some* loophole in Coleman-Mandula, even if he doesn't know what that loophole is. Whereas Aaron Bergman is very skeptical but asks whether he's missing something in the construction.

Surely this issue can be resolved. I am climbing a steep learning curve in order to participate here, but I have a few thoughts.

Let's start with the classical theory. Garrett has an E8-valued connection on 3+1 dimensions. He has an action (his equation 3.7) which explicitly breaks part of the E8 symmetry, including the part which notionally mixes fermions and bosons. I say "notionally" because those terms don't mean anything until we quantize.

Now compare this situation to the Standard Model coupled to gravity. A classical theory exists here too; then you get to the quantum theory by substituting operators for fields in the usual way.

This seems to be the crux of the debate with Aaron. You don't need a super-Lie algebra to quantize the Standard Model, because the fermions and bosons don't have to mix. Is it the same for Garrett's theory? If the action didn't already contain those symmetry-breaking terms, wouldn't Aaron be right? But given that it does, does that mean that Garrett's right? Section 3.2.3 must be the key here, because that's where the "fermionic" action is extracted.

75. **ScienceLover**
November 17, 2007

> Popular writers seem to have cast Garrett Lisi in the role of a man of the people who is challenging the great and proving again that the people are their equals in every way (including theoretical physics).

It hasn't gone that far right now. Lisi has not become a research politics factor – the common man challenging the establishment. Instead the media phenomenon shows all characteristics of a 15-minutes-of-fame story.

76. **Dany**
November 18, 2007

King Ray: "This has to do with the global structure of the gauge group of the standard model. If you assume that the global group is S(U(3)xU(2)), and that anomalies cancel in each family, then you get a unique solution for the hypercharges in a family (given the SU(3) and SU(2) reps of the quarks and leptons)".

Hi King Ray,

In case that you missed it, see also arXiv:hep-th/9801011v1

Regards, Dany.

77. **Arun**
November 18, 2007

Sorry for being disjointed here – why I'm thinking now that science should be developed in private, outside public gaze is that we've protested string hype – the public claims made by physicists that a theory will deliver what it cannot (yet?). We ought to be protesting Lisi E8 hype as well for exactly the same reason. No double standards in this matter, please.

78. **King Ray**
November 18, 2007

Dany, thanks, I have seen that reference as well as Saller's papers. The paper I cited above is referenced in Agricola's article that you cite and Saller's book (Vol. II) and papers. Was there a particular point in Agricola's paper that you were referring to?

79. **Marcus**
November 18, 2007

> we've protested string hype – the public claims made by physicists that a theory will deliver what it cannot (yet?). We ought to be protesting…

Arun,
impartiality and consistency are indeed essential but so is a sense of proportion.

80. **Garrett**
November 18, 2007

The hype is dying down. I've tried to make it very clear to every reporter I've talked with that no matter how beautiful and promising a theory looks, its validity is determined by predictions, and how those fare under experiment. And that with any new theory, including this one which is still developing, an attitude of healthy skepticism is appropriate.

The thing worrying me is… this E8 Theory was playing out very well in academia before the media hype. I gave four talks on it, in quick succession, and each one went better than the last, with a very good reception from physicists. And the paper generated a great deal of interest. Then what I thought would be a nice explanation for a lay audience in New Scientist spawned a media frenzy. I just hope the hype didn't obliterate serious consideration of the paper.

81. **King Ray**
November 18, 2007

Garrett,

I believe that you have handled yourself and the publicity as well as possible.

Last night I read all the posts on Bee's blog, and I think you handled all the attacks and criticisms that came your way in a manner that showed a great deal of class and dignity, always answering every question or challenge in a good natured way.

In everything I have read that you have said, I have detected no trace of your trying to overhype your work, as string theorists have been very guilty of doing with string theory. All I detect is the natural excitement and enthusiasm of a theoretical physicist for his work in trying to understand the laws of nature.

In every instance you are extremely honest and straightforward and always point out that the theory is in its early stages and could prove to be wrong, and that it will make predictions that will be testable. I find this quite refreshing and admirable. My hat is off to you for the way you have conducted yourself.

It might be useful for you to write a more expanded version of your paper, that is more in the notation familiar to particle physicists, where you carefully and redundantly explain your steps and methods, as if you were teaching a week or two long class on your theory. A review section on the key points of Lie algebras and group theory relevant to your work might also be useful, to bridge the gap, since the theory of exceptional groups may be an impediment to many. It is always good to show simple examples and then talk about the general case. I know it is hard to try to do something like that when you are in the middle of doing important work, but it could help your cause tremendously.

Keep up the good fight!

82. **Aaron Bergman**
November 18, 2007

A review section on the key points of Lie algebras and group theory relevant to your work might also be useful, to bridge the gap, since the theory of exceptional groups may be an impediment to many

This is not the problem. Everyone knows about E_8, Clifford algebras and BRST symmetries — they're standard material. What makes the paper difficult to read (for me, at least) is the lack of standard notation (all the tildes, underlines and dots, for example), and a number of missing steps in various derivations. I would like to see a detailed step-by-step derivation of the standard model using standard notation. I'd like to see the exact subgroup of E8 we're using and the decomposition of the adjoint rep of E8 under that subgroup with each rep labelled with respect to every factor in the subgroup.

The standard model has a well-known set of fields with a well-known set of quantum numbers and interactions. The paper presents a rather piecemeal approach of getting to the standard model stuff, and it didn't make much sense to me. I'd like to see the Lagrangian (3.7) expanded out in its full glory, so to speak.

Basically, right now it feels to me like reading the paper is a chore, and I have better things to do. It's not even clear to me at the moment what the resulting symmetries of the Lagrangian are, and the more things I have to figure out when I read the paper, the less urge I have to do so. Sounds selfish I know, but such things are true about papers in general — you should try to make things as easy as possible for your colleagues.

83. **Arun**
November 18, 2007

Sounds selfish I know, but such things are true about papers in general — you should try to make things as easy as possible for your colleagues.

A good reminder that to have one's ideas taken seriously by the scientific community is a hard-earned privilege, not a right.

84. **woit**
November 18, 2007

Arun,

I think there is a significant difference between the overhyping of Garrett's work and the overhyping of string theory. One has to do with a very speculative idea that just one person is working on, the other a very speculative research program that has dominated particle theory for a couple decades, involving thousands of people. I don't think we're anywhere near yet the situation where some of the smartest graduate students in the subject are leaving the field because they feel that only if they work on Garett's theory could they find an advisor and start a career for themselves at the best graduate programs.

There's a constant stream of overhyped articles in the press about specific speculative ideas in physics being promoted by one person or a small group. New Scientist has such an article almost every week. Commenting on all such articles would take a lot of time, and get them more attention than they deserve, so mostly I think it's best to just ignore them

The media Lisi-mania is a much more unusual phenomenon. It has gotten a much wider distribution than usual, so it's a good idea for physicists to put out some more realistic points of view. I think the blogs on the whole have done a good job of this. Anyone who reads the coverage here, at Sabine's blog, and at Sean Caroll's should get a pretty accurate view of what is going on. They could also read Lubos's blog, but he is likely to just convince them that Garrett has definitely accomplished something revolutionary that has driven string theorists insane.

85. **Dany**
November 18, 2007

King Ray:" I have seen that reference as well as Saller's papers. The paper I cited above is referenced in Agricola's article that you cite and Saller's book (Vol. II) and papers. Was there a particular point in Agricola's paper that you were referring to?"

King Ray,

I mentioned I.Agricola since you referred to 1991 paper by J.Hucks. My guess was that you know all of them, but just in case… I think the discussion of his results is off topic here. In addition, I consider it not adequate to the blog with the general title "Not even wrong".

Regards, Dany.

86. **King Ray**
November 18, 2007

Dany, no problem, I appreciate your pointing out the paper in any case, so thanks again.

87. **Arun**
November 18, 2007

Peter,

The current hype sounds to me like Pons-Fleischman cold fusion, or more charitably, Taleyerkhan's acoustic cavitation bubble fusion.

-Arun

88. **woit**
November 18, 2007

Arun,

I think we're still quite a ways away from the Pons-Fleischman level of media frenzy. And that was an experimental claim, which led to many groups doing experiments to try and replicate the results (as well as physicists selling platinum short, convinced that the way platinum had been bid up because it was the catalyst Pons-Fleischman used would soon collapse as the results weren't replicated). Claims from serious experimental physicists saying they have an inexhaustible source of free energy are rather more rare than claims from theorists that they have made progress towards a ToE.

89. **Kralizec**
November 18, 2007

> ScienceLover Says:
> November 17th, 2007 at 11:45 pm
> …
> It hasn't gone that far right now. Lisi has not become a research politics factor – the common man challenging the establishment. Instead the media phenomenon shows all characteristics of a 15-minutes-of-fame story.

Thanks; I understand, ScienceLover. I'm not, myself, suggesting that Mr. Lisi fits into a people/great frame of the sort I described. It just appears that some popular writers (and perhaps at least one fellow physicist) are trying to understand the physicists and Mr. Lisi in terms of that continually re-emergent sort of antagonism.

90. **amused**
November 19, 2007

The description of what has and hasn't been done in this paper should be made a lot clearer. E.g., I, and quite a few others it seems, got the impression that this was supposed to be a unified field theory with E8 gauge symmetry containing gravity and the SM. The e8 gauge connection and its curvature are discussed at length in a way that gives the impression that they are to be used to construct the action for the fields, presumably an e8 gauge-invariant one — otherwise, why go on about it? If this had really been the case then there would be an immediate issue with the Coleman-Mandula theorem. The remark in the paper about C-M not being relevant because the spacetime is deSitter is nonsense, since Poincare symmetry can be assumed for all practical purposes when applying the theory to particle scatterings in labs.

The real reason why C-M turns out not to be relevant here (as noted by Aaron B. over on Bee's blog) is that the action proposed for the fields is not e8 gauge invariant: Despite the impression the other parts of the paper give, the actual construction of the action is not based on the e8 connection and its curvature. Instead, it is pulled together from various parts of the e8 curvature in a way that breaks the e8 gauge symmetry; the action is not invariant under transformations that mix the gravitational and internal gauge parts of the e8 connection. This seems really ad hoc to me — there is no governing principle like gauge invariance for determining the action, it is just cooked up to reproduce the gravity and the SM. (This seems to be Bee's objection as well, if I understood her comments rightly.) In light of this I don't see any compelling reason for expecting that the new "color scalar fields" that arise have anything to do with nature. So at this point it seems that the paper is just one more not particularly well-motived proposal for what beyond the SM physics might look like. (String phenomenologists already have loads of these, apparently.) Well, it is not even that yet — first one would have to quantized the theory, determine the particle content, renormalizability etc. But a unified theory it certainly isn't — where is the unification?

As for the media hype, "surfer dude stuns physics" makes for a cool story but it wouldn't have happened without the backing of some big-name physicist. Smolin was most obliging in this role, but I'm sure it was for the purest of motives (let's not be getting cynical here ;)). I share very much Bee's concern about episodes like this leading to an atmosphere where people feel pressured to be working on media-appealing, hypeable topics rather than solid, conservative ones.

91. **Bee**
November 19, 2007

Hi amused:

This seems to be Bee's objection as well, if I understood her comments rightly.

You did. Best,

B.

92. **Harry**
November 19, 2007

Mr. Lisi is now making the headlines in France, in one of the most famous newspaper :

http://www.lemonde.fr/web/article/0,1-0@2-3244,36-979858@51-979860,0.html

(in french)

93. **H-I-G-G-S**
November 19, 2007

One does not have to examine the full E_8 structure to see the
"non-unification" unification aspects of the construction. Consider his
"unification" of the quarks and gluons of QCD using G_2. It is true that the 14-dimensional adjoint rep of G_2 decomposes as the 8 + 3 + \bar 3 of SU(3) under G_2 -> SU(3). And it is true that the gluons are in the 8 and the quarks and anti-quarks in the 3 + \bar 3. So what? What action does Dr. Lisi propose? I am very sure that if you wade through the obscure notation one will discover that either

1) It is the same action as that of QCD in which case G_2 plays absolutely no role in the contruction.

2) It is not the same action as that of QCD in which case the theory is ruled out by experiment.

It is a lazy author that can't be bothered to work out the simple consequences of his theory and explain them simply. And it is a lazy blogger (who is supposed to be an expert in representation theory after all) who blogs about a paper and adds to the hype without putting in a modest amount of effort to understand what is going on.

94. **Garrett**
November 19, 2007

amused:

This is a great improvement. It appears you are now understanding (if not actually liking) the paper, as your factual statements are mostly correct. Now please consider that matching the standard model fields and dynamics to parts of the E8 connection and its curvature, with only a handful of exotic fields, is non-trivial.

(You have passed through the first, second, and are now to the third stage of acceptance (described on Bee's blog).)

H-I-G-G-S:

I chose (1). And I've tried to make the paper as succinct as possible. On your last comment, I'll let Peter speak for himself.

95. **woit**
November 19, 2007

H-I-G-G-S,

This posting in no way purports to contain a technical examination of the paper and its problems. I have put a "modest amount" of effort into understanding what is going on, enough to see several problems with what Garrett is trying to do. The sort of general unification/non-unification problem you bring up is specifically addressed in the posting. I don't think there's anything at all in what I wrote that hypes the paper, quite the contrary. The links I provided to places where it is hyped were given in the context of noting that I don't think this kind of thing is a good idea at all.

I'll provide you with another link, to the discussion and my comments at Sabine's blog, about how unprofessional it is to attack people from behind the cover of anonymity, evading any responsibility for one's actions:

http://backreaction.blogspot.com/2007/11/anonymity.html

96. **Eric Weinstein**
November 19, 2007

Hi Garrett,

Sorry to wade in late to this thread…but as an outsider, I haven't yet learned to talk shop with the physics media circus at full tilt. You guys should get that thing fixed…

In helping us to understand the motivation for your beliefs, can you be at all specific about how you see the origins of E8 as a symmetry group? Is it effectively a black box to you or do you see a preferred set of objects (constructed without benefit of E8) on which E8 acts as natural symmetries? For example, do you have any insight into the Freudenthal/Tits magic square construction that is not already present?

As you are likely aware, many of us hold the prejudice that if nature uses exceptional algebra as you assert, a complete physical theory based on E8 should eventualy illuminate its 'purpose' through some kind of principle of emergent exceptional symmetry. As best as I can tell, at this stage your paper treats E8 as an unmotivated combinatorial anomaly from mathematics to be used by physics.

Lastly, there is a tremendous ammount of rich folk wisdom held by very respectable mathematicians about E8, most of which is unfortunately unpublished (as it is generally treated as a private hobby). If you would like to talk off line, I could recommend some folks to you who do not seem to be on your acknolwedgement list. This is pretty specialized theory even for Lie Group specialists.

Regards,

Eric

97. **amused**
November 19, 2007

Garrett,

"Now please consider that matching the standard model fields and dynamics to parts of the E8 connection and its curvature, with only a handful of exotic fields, is non-trivial."

To my mind this has the same status as various remarkable numerical relations and coincidences that people sometimes find among the fundamental constants of nature: interesting to some extent, but, on its own, not real physics.

98. **anon.**
November 19, 2007

Here's a link to Babelfish's English translation of the *Le Monde* article about Garrett's theory. (It's not much of a translation, but it's still helpful if your French is as rusty as mine.)

99. **Fabrizio Nesti**
November 19, 2007

HIGGS boson, Amused:

I also were quite surprised after understanding that Garrett's action was not E8 invariant. I think the paper is not so clear about this point. The question is then, which kind of unification is this? Of the three unification steps (find multiplets, give an invariant action and find a breaking mechanism) Garrett only carried out the first one.

Given that, the result of this first step is really nice and non trivial, since you should appreciate that some Higgs is contained in the E8 connection, as well as _three_ families. This is for the excitement. The rest just calls for much (exciting!) work in the future.

About Coleman-Mandula, I can clarify: the point is not desitter or noninvariance – because eventually one will look for an invariant theory. The point is that above the unification scale there is probably no metric, because the metric itself is inside the e8 connection. If at high energy there is no metric, there is no time, no standard scatterings and thus no Coleman-Mandula.

This point made Lubos go berserk even before Garrett's paper, so I suppose he was prepared – but more robust critics could have been raised than this one..

best,
Fabrizio

100. **Aaron Bergman**
November 19, 2007

Given that, the result of this first step is really nice and non trivial, since you should appreciate that some Higgs is contained in the E8 connection, as well as _three_ families.

But that's not true even if we accept the identifications made in the paper. The three families are not identical — they live in different representations and do not accord with the standard model.

The question about C-M is about the low energy effective theory, not so much about the high energy theory. But, given the lack of clarity about what the symmetries of the theory actually are, this may be irrelevant.

101. **Bee**
November 19, 2007

Gag, my visit count is going nuts again, thanks to

Stuff string theory – try E8 to explain the universe.

102. **Peter Woit**
November 19, 2007

Thanks Bee,

I've given up on trying to keep track of all the articles, thanks to my readers for posting here any they find that haven't been mentioned and are of interest. On the whole, about the science, many are overhyped and pretty misleading in various ways, a few include an appropriate amount of skepticism.

103. **H-I-G-G-S**
November 19, 2007

Dear Peter,

Thanks for the link. I suggest you read it. I am not anonymous, I have a pseudonym. As Bee points out, there is a difference. I've posted under H-I-G-G-S before and will do so again, unless you block my comments. My actions have consequences, at least in the blogosphere.

Anyway, let's get back to the physics. Garrett agrees that as far as the quarks and gluons are concerned G_2 is totally irrelevant. Since he does the same thing with E8, that is combines all the fields into one connection, but then writes down an action piecemeal to agree with the SM, I am tempted to say that the whole E8 is irrelevant. But I think this is not totally fair. After all the SM fields fit into E8 with some room left over, so the model "predicts" these extra particles. Of course I could just as easily invent a theory of everything based on a noncompact form of SU(248) or SO(196884) or an infinite number of other groups which would "unify" everything in the same way. And since the action is whatever he wants it to be and there is no dynamics to break the E8 symmetry, there is no prediction for the masses or couplings of these particles. If they are not found, he can just adjust the mass scale however he wants. I really would have to say that this model is a prime example of a theory that is "not even wrong." As the old joke goes, this paper contains new and correct results, but what is correct is not new, and what is new is not correct. What part of this do you find interesting and worthy of encouragement?

Sincerely,
Not the real P.W.H. FRS

104. **Peter Woit**
November 19, 2007

H-I-G-G-S,

I suggest you actually read my posting, where I explain precisely what I think about what Garrett is trying to do, and what parts of it I think are worthy of encouragement. You might also notice that I explictly make the point you're trying to make, explaining why I'm dubious about this kind of "unification" since it doesn't solve the problem of how to break the larger symmetry in a way that makes the setup predictive. In principle fitting SM symmetries together into a simple exceptional group like E8 could be constraining enough to be predictive, even after symmetry breaking, but lots of people have tried this kind of thing without success. He seems to be trying something different than a standard GUT set-up, one assuming a mixing of space-time and internal symmetries that won't work in the standard QFT formalism. Maybe he can get something out of this, I certainly encourage him to keep trying.

105. **amused**
November 20, 2007

Fabrizio,

"About Coleman-Mandula, I can clarify: the point is not desitter or noninvariance – because eventually one will look for an invariant theory. The point is that above the unification scale there is probably no metric, because the metric itself is inside the e8 connection. If at high energy there is no metric, there is no time, no standard scatterings and thus no Coleman-Mandula."

If at some point in the future someone comes up with an invariant high energy theory of the kind you describe, which has Lisi's model as a low energy effective theory, then we can discuss it. But at the moment there is no proposal for such a theory, and no compelling reason for expecting one to exist, so talking about it is just idle speculation.

106. **chris**

November 20, 2007

hi amused,

if it is only idle speculation, then why do you (and many others) keep poking at this point? is it just motl's evil spirit haunting 🙂

the lisi lagrangean does fulfill the C-M theorem. just plain simple like that. it does no nontrivial mixing. the hope (and it is just a hope) is, that maybe at higher scales a mechanism will ultimately provide… but he does not even speak about that.

107. **chris**
November 20, 2007

ps:

oh, and to anticipate the "then it's all vacous" argument: how much explicit dynamics did the original gell-mann eightfold way provide?

108. **Fabrizio Nesti**
November 20, 2007

Aaron:
The three families are not identical — they live in different representations and do not accord with the standard model.

Well, we do know triality. The point (I alrady raised in Bee's blog) is whether triality is inserted by hand or comes out of the theory itself. For this I think you'll need a high energy completion… again.

Peter,
I happened to re-read your posting and I have a comment that came to me also after reading your book. You wrote that as in GUT programs, you move the problem from symmetry to symmetry breaking, and for this reason you get no predictions, except maybe proton decay in simplest models, that we do not observe. I agree but I just want to point out that simplest models are anyway already ruled out by other aspects (by fermion masses, notably) so there is no point in ruling them out. Realistic nonminimal models, even SU(5), they do _not_ predict a fast proton decay, so they are not ruled out.

So the situation is probably even worse than how you put it. No new-physics idea can actually be ruled out, they are simply not predictive, alone. Not GUT – not certainly strings or extra dimensions – not even SUSY (if we'll not see it, one may still raise this and that to make it invisible). And not even these theories unifying Lorentz and internal symmetries.

Maybe the point is that mathematical ideas, geometry, unifications etc, are just nice frameworks… while one needs a complete detailed theory -scales- to make predictions. Btw this is what the standard model is – a complete theory up to 100GeV :).

ok, just free thinking,

best,
Fabrizio

109. **mitchell porter**
November 20, 2007

It would be funny if Garrett's theory turned out to be just another limit of string theory – perhaps of some "noncanonical" form of string theory (e.g. see Lubos's list under "Strong leadership of supersymmetry", here). There is actually a "single-E8" string theory, a heterotic tachyon. Only, that's a compact E8.

So far as I can see, the situation with respect to Coleman-Mandula is this. Garrett's action is consistent with the theorem because the full E8 symmetry is broken. However, the hope or presumption is that the symmetry-breaking terms come from somewhere and that the fundamental theory does have full E8 symmetry. At this point the CM problem returns. Fabrizio Nesti in his papers says that CM is evaded because the full symmetry only manifests in a high-temperature topological phase (no metric). If that doesn't make sense, we will be left with Aaron's suggestion to use supersymmetry after all – in which case we'd surely end up with some sort of supergravity, and my notion that this is just another superstring limit would look even more plausible.

What I want to understand now is the part about triality and the three generations. There is definitely handwaving on this point (page 28: "not presently understood well enough to write down").

110. **mitchell porter**
November 20, 2007

I'll risk really exposing my ignorance and voice one more thought. One of the basic ideas here is that E8 could provide a nonsupersymmetric way to unite fermions and bosons. Meanwhile, one corner of superstring theory has an E8xE8 symmetry; and the Monster group shows up in certain compactifications, e.g. a Leech lattice orbifold (I think). Now the Leech lattice is rather similar to a copy of *three* E8 root lattices. So I wonder if there's a third E8 lurking in string theory, that's responsible for the supersymmetry.

111. **amused**
November 20, 2007

hi chris,

It's hilarious to think that I might be possessed by Lubos' evil spirit 🙂 especially considering our previous history in the blogsphere (which includes, among other things, my successful goading of Jacques Distler into denouncing him…although I got the feeling that Jacques quite enjoyed the opportunity to do that). Perhaps Peter W. should be called on to perform an exorcism.

At any rate, my last two comments above were in reply to comments addressed to me by Garrett and Fabrizio, so I don't think they qualify as poking. And my first comment above was just a summary of my opinion on the paper after a bit of reflection and recovery from the mammoth discussion over at Bee's blog.

Your comment raises an interesting general question though: why were those of us who do physics for a job spending time on reading and discussing this paper? Is it really that interesting compared to all the other physics things we could have been doing? It's not as if Garrett is the only alternative physicist out there with his own theory. E.g. Tony Smith seems just as well qualified academically (he has a Ph.D. from Princeton), and, for all I know, his theories could be just as deserving (or undeserving) of attention as Garrett's. (Besides the media hype, it's kind of ironic that Garrett is getting blog postings devoted to his theory while Tony gets told to shut up every time he mentions his ones.) My own reasons for spending time on this were: (i) The reported opinions of various prominent physicists; e.g. Smolin apparently described it as “fabulous”. (ii) With all the media attention on this, Joe Public wants to know what the consensus opinion of professional physicists is, so probably we have some responsibility to assess the paper and make our opinions known.

But as far as “alternative theories” go, there are others that are much more worthy of attention than this one. Just to give an example, C. Wetterich has been developing an “alternative” proposal for explaining electroweak symmetry breaking based on chiral coupling of matter to massless antisymmetric tensor field in hep-ph/0607051, arXiv:0709.1102. This looks really interesting and elegant (without having studied it in detail), but seems to have been completely ignored so far. (However, the author is no doubt less colorful: he’s a full professor at one of Europe’s illustrious universities — how boring if progress in physics werre to come from someone like that!) I can’t help getting a feeling that something is fundamentally wrong somewhere when work like this is being ignored while Garrett’s paper is being debated on physics blogs and reported on Fox News. And it is a worry that with this and all the other hype they are subjected to, the public will be too jaded to pay attention and care in the future if and when there really is some major gravity/particle theory breakthrough worth telling them about.

In reply to the rest of your comments:

“the lisi lagrangean does fulfill the C-M theorem. just plain simple like that. it does no nontrivial mixing.”

Indeed. Which is why I wrote in first comment above that C-M is “not relevant” in this case.

“the hope (and it is just a hope) is, that maybe at higher scales a mechanism will ultimately provide… but he does not even speak about that.”

No, but Fabrizio did speak about it and my “idle speculation” remark was directed to him.

“oh, and to anticipate the “then it’s all vacous” argument: how much explicit dynamics did the original gell-mann eightfold way provide?”

One big difference is that Gell-Mann proposed SU(3) as a genuine (unbroken) symmetry of the strong interactions, so the properties (quantum numbers etc) of the new particles can be inferred from the fact that they are in the same multiplet as other known ones. There is no such information in Garrett’s model since the symmetry is broken. While we’re on this topic, a final poke: It is ridiculous how Garrett has been saying (in statements attributed to him in the press) that the LHC will determine whether his theory is right or not. How does he know whether or not the exotic particles in his theory should show up there? Maybe they should already have shown up in previous experiments, in which case their absence disproves the theory. Or maybe they show up at energies much beyond LHC? Does he have any idea at all at which energies they should show up?

112. **Fabrizio Nesti**
November 20, 2007

…“maybe at higher scales a mechanism will ultimately provide… but he does not even speak about that.”
No, but Fabrizio did speak about it and my “idle speculation” remark was directed to him.

Yes, Amused,
I did’nt reply to you because I agreed with Chris: Garrett does not speak of the full theory in the symmetric phase. (And in any case, he should speak for himself.)

As far as I am concerned, in our paper (Graviweak Unification) we _do_ provide both the action in the fully symmetric phase and its breaking: above the breaking scale CM does not apply, below it holds, of course. This is why I wrote that clarification.

In the E8 case, I can only hope that a similar breaking mechanism can be found, but it’s more difficult.

About the ghosts-as-fermions, I have no idea of how this could work, and if. Lubos says that this can not be done. It should be easy to work out a simple toy model using BRST. Maybe Peter has thought to it.

I think that blogs (let alone the media) are not the place to demonstrate something right or wrong, and the silence of most scientific community is a good sign. One should just close the browser for some time, sit down and try to see if ideas can be made to work. This is what serious physicists will do, anyway.

Fabrizio

113. **Aaron Bergman**
November 20, 2007

Well, we do know triality. The point (I alrady raised in Bee’s blog) is whether triality is inserted by hand or comes out of the theory itself. For this I think you’ll need a high energy completion… again.

Triality at the moment is just a bit of hand-waving hope. There’s no way that I can see that one could insert it by hand to recover the three generation structure of the standard model. Different representations remain different representations no matter how many automorphisms you do.

114. **chris**
November 20, 2007

hi amused,

thanks for your long, detailed reply. indeed i find it strange too, how the attention of the public and of the theoretical physicists is divided between different approaches and how they influence each other. i think it just serves to show how much our profession is affected by trends and hot topics, too. and also how little every one given individual can actually comprehend of the whole field or how much the judgment is not guided by any higher principle we like to appeal to (like truth or beauty), but just by trust in our or other peoples opinions.

well. one point i would like to make though is that i think it is not so far out to claim the unobserved particles in the spectrum not to be at too high energies. the natural reason i see is just plain simple again: these are particles that should show up in the effective theory at low energy. they explicitly populate the spots in the broken lagrangean. this is in stark contrast to e.g. the SU(5) X and Y bosons. so while it is still questionable to claim anything will be seen at the LHC it is no more of a misleading statement to me than the TeV scale bulk gravities or anything that tries to bring new particles within lhc reach.

and finally: you very well know that SU(3) is broken right from the start of the construction. weakly broken i concur, but nonetheless, there never was hope of an unbroken flavor SU(3). all there was were the SU(3) reps and particles of different mass that fitted them.

115. **amused**
November 20, 2007

Hi Fabrizio,

Thanks for your clarifying comment. I wasn't aware of your work, and it is interesting to hear that you have a high energy completion of this kind in your case.

116. **amused**
November 20, 2007

hi chris,

Yes, we are reliant a lot on the opinion of "authorities", which imo places a lot of responsibility on them to be careful about what they say and what they hype.

If the TEV scale gravity proponents, string phenomenologists etc make claims that their theories will be tested at the LHC then that is of course just as bad as if Garrett does it. But in the cases I've noticed those people always say maybe…

And yes, I meant of course approximately exact flavour SU(3) symmetry, not 100% exact.

117. **Tony Smith**
November 20, 2007

amused said "… I [amused] can't help getting a feeling that something is fundamentally wrong somewhere when work like …[that of]… C. Wetterich … hep-ph/0607051 … is being ignored while Garrett's paper is being debated on physics blogs and reported on Fox News. …".
Wetterich says in that paper:
"… Antisymmetric tensor fields with chiral couplings to quarks and leptons may induce spontaneous electroweak symmetry breaking in a model without a "fundamental" Higgs scalar. … Furthermore a scalar top-antitop condensate forms, giving mass to the weak gauge bosons and fermions. …".

Wetterich's idea of a physical antisymmetric tensor field is interesting both physically and historically, since similar structures were used by Einstein and Schroedinger in some of their models. Maybe Wetterich has found a physical use justifying the basic intuition of Einstein and Schroedinger ? Wouldn't that be something worthy of popular news commentary ?

Wetterich's idea of "a scalar top-antitop condensate" acting effectively as a Higgs mechanism, even if it may not be unique to him, is still a phase of his work that might be explored by experiments at the LHC. Wouldn't that be worth at least a little pro-LHC publicity ?

As amused said, Wetterich is "… a full professor at one of Europe's illustrious universities …", so lack of professional qualification cannot explain the fact that Wetterich's work is not as widely publicized and evaluated as that of Garrett Lisi,
and
that leads to amused's statement that
"… it wouldn't have happened [for Garrett Lisi] without the backing of some big-name physicist. Smolin was most obliging in this role …",
and
that leads to a question about whether or not approval by a prominent third party is a necessary condition for acceptance of new ideas.

For example,
only after Freeman Dyson's approval was QED of Feynman and Schwinger widely accepted,
and
only after Ben Lee's approval was 't Hooft's proof of electroweak renormalizability widely accepted,
and
without such approval-by-prominent-third-party, QED by E. C. G. Stueckelberg was ignored.

Tony Smith

PS -amused also said "… Tony Smith … has a Ph.D. from Princeton …".
Actually, my Princeton degree is an A.B. in math (1963),
and I do not have a Ph.D.,
so I am not "… as well qualified academically …" as Garrett Lisi,
but
I appreciate the other kind words from amused.

118. **Coin**
November 20, 2007

C. Wetterich has been developing an "alternative" proposal for explaining electroweak symmetry breaking based on chiral coupling of matter to massless antisymmetric tensor field… I can't help getting a feeling that something is fundamentally wrong somewhere when work like this is being ignored while Garrett's paper is being debated on physics blogs and reported on Fox News.

I don't think the "people shouldn't be blogging about X, because blogging about Y would be more important" argument is ever fruitful. If Y is important, okay, blog about that too. If someone wrote a blog post about the Wetterich guy's thing I'd read it. I'm not sure I'd completely understand it, but I'd try. In the meantime, the energy spent on covering Garrett Lisi is not energy that's being *taken away* from covering Prof. Wetterich. If Lisi's paper had not dropped this month I think the most likely alternative would have been that the physics blogs for the last couple of weeks would have just been quieter.

And it's certainly the case that if Lisi hadn't published this month, Fox News *still* wouldn't be talking about Wetterich's new approach to the EWSB problem– Fox News doesn't even know what the words "Electroweak Symmetry Breaking" mean. They do know what the words "unifies gravity with particle physics" mean, though, which is why they'll write stories about that but not Higgsless EWSB models. (If you want to identify something "fundamentally wrong" here, then focus on that– that some of the most objectively important problems in real physics are things that the general public doesn't even realize are problems that need to be solved! Never mind Wetterich's "interesting and elegant" new approach for a minute– if the EWSB problem were *solved* tomorrow, would you know how to express this in a way that a Fox News viewer would care, or clearly realize that something truly important had happened?)

119. **amused**
November 20, 2007

Good grief, I didn't mean that Wetterich's papers should be discussed in the popular media instead of Lisi's. That obviously wouldn't be appropriate. The point i was trying to make was that it might be interesting to consider how and why those of us who are trained physicists here ended up spending a fair bit of time discussing Lisi's paper when there are other papers out there that are arguably more deserving of attention but seem to be going unnoticed. The general question of what is it that determines which papers get attention from the physics community seems an interesting one, but perhaps that's getting off-topic here.

120. **Coin**
November 20, 2007

Amused: Well, alright, sorry I misunderstood you then.

121. **amused**
November 20, 2007

That's ok, I probably should have made it more clear.
And apologies to Tony for misremembering his degree.

122. **Nic**
November 22, 2007

Distler seems to have purely group-theoretical arguments to exclude the viability of the model:

http://golem.ph.utexas.edu/~distler/blog/archives/001505.html#more

123. **alex**
November 22, 2007

"Distler seems to have purely group-theoretical arguments to exclude the viability of the model:"

Good! At last someone put a stop to this absurdity.

Thanks Jacques!

124. **a.k.**
November 22, 2007

..I do not think Garrett claimed to have an 'embedding' of the direct product of the groups in question into E8 but to have any of these groups embedded as subgroups. As I already mentioned elsewhere, from my point of view, even at high energies, to get predictions, the E8-bundle should reduce to these subgroups which is by standard fibre bundle theory equivalent to having a global section in the bundle associated to E8/H, where H is the subgroup in question. Furthermore, the total connection should reduce to these subbundles, given they exist. Both facts might very well be true without having an embedding of a direct product as Distler discussed.

125. **Tony Smith**
November 23, 2007

According to a recent Telegraph article by Roger Highfield:

"… a representative of a Hollywood film production company has been in touch with the Telegraph saying that "I loved the article and think it has great potential for a feature film." And at least one major agent is scrambling to sell publishers a book that will tell the story of Garrett Lisi and his struggles to comprehend the cosmos.
…
he has become something of a celebrity and he admitted yesterday that he was finding the attention overwhelming – indeed he has refused to appear on television.
"I'm currently spending the bulk of my time corresponding with physicists, which I consider to be of prime importance. "…".

Garrett's preference for doing physics over TV etc seems to me to be a very responsible attitude,
and I hope that he is able to maintain it under the pressure of possibly exploitative offers from Hollywood, book agents, etc.

Tony Smith

126. **Dany**
November 23, 2007

Tony Smith:" I hope that he is able to maintain it under the pressure of possibly exploitative offers from Hollywood, book agents, etc."

Tony,

I guess it will be great fun to see Hollywood movie and read a book about "his struggles to comprehend the cosmos". But what about the former American common boy stereotype with the forelock? He is Ockham razor shaven.

Regards, Dany.

127. **Chris Oakley**
November 23, 2007

According to a friend at UCSD Garrett Lisi *already has* appeared on TV recently (dressed in once instance as a red indian). They obviously love the idea of him being the rebel who is rocking the foundations of physics.

Well – good luck to him.

But I wonder … if the string theory community had not started the trend of trumpeting speculative non-results to the press, would they have taken an interest?

128. **Tony Smith**
November 23, 2007

Chris Oakley said
"… According to a friend at UCSD Garrett Lisi already has appeared on TV recently (dressed in once instance as a red indian). …".

According to a Fox6 (San Diego TV Fox station) web article by Jim Patton:
"… Lisi is a San Diego native who received his Ph.D. in Physics from U.C.S.D.. He also appears to be a true San Diego son with an obsession for surfing and snow boarding that splits his time between Maui and Nevada.
My interview with Lisi took place via email. …".

In light of that, perhaps Garrett has been true to his "refus[al] to appear on television", and what appeared on San Diego TV may have been a news story by Jim Patton based on the email interview with background pictures from Garrett's web site. As to Garrett being "… dressed … as a red indian …", I suspect that may be a misinterpretation of the cover picture on Garrett's web site at sifter.org/~aglisi/ which picture shows Garrett at Burning Man standing in front of a colorful exhibit.

Tony Smith

129. **H-I-G-G-S**
November 23, 2007

Dear Peter,

While you did not hype the content of this paper, like it or not, once you have a science blog you become part of the science media. Science reporters will look to your blog and others (even though this is in general a dubious method of gathering reliable information) to find out what is new and interesting. Because of this I think you should take some care with the topics you cover, particularly when you say things like you are fond of the ideas he is pursuing or that you are hopeful this line of thinking will lead somewhere. In this case you contributed to the idea that this model has some credibility, which it does not. Sean at Cosmic Variance was an even more egregious example of this, so don't think I'm singling out your blog.

I'd also suggest that you be a mensch and replace your latest update concerning Distler's post on this with something more along the lines of "Distler has found an explicit error in Lisi's treatment of the representation theory of E8." You are supposed to be an expert on representation theory, so certainly you should be able to check whether Distler is right or not.

130. **Peter Woit**
November 23, 2007

H-I-G-G-S,

I see one of the main goals of this blog as being to provide a source of accurate information for anyone interested in the same topics as I am. I think that's what I've done in this posting, and that's what I'll continue to do.

The reference to Jacques's posting is perfectly accurate and I hope helpful to people. I see no need to editorialize in the way you suggest. For one thing, you might notice that the only comment in my posting about Garrett's E8 scheme is that it's the kind of thing that leaves me cold. As you might guess from that, I haven't worked through the details of the representation theory in Garrett's paper, nor the details in Jacques's posting, so I don't think it's a good idea for me to do anything other than to refer people to the actual discussion between them, where they can make up their minds for themselves.

As for the degree of my menschlichkeit, you might notice that I regularly link to and suggest that people look at many things that Jacques posts. In return, his policy towards me is one of absolute refusal to link to or acknowledge the existence of anything I ever write, as well as to make sure that no link appears in any internet source he controls, such as hep-th trackbacks and his Planet Musings blog aggregator.

131. **Arun**
November 24, 2007

The Economist gets Lisi-mania:

http://www.economist.com/science/displaystory.cfm?story_id=10170958

This hype is unjustified and IMO, is damaging to particle physics.

Quoting:

Yet the theory has several appealing facets. It is elegant. It is expected to make testable predictions. Unlike some of the more complicated efforts to devise a theory of everything, this one should either succeed relatively rapidly or fail spectacularly. And that is more than can be said for three decades of work by other physicists.

It hasn't occurred to them that it is not even a theory.

132. **How Can We Turn The Tide?**
November 24, 2007

It is now apparent that the Lisi affair was due largely to "String Theory Envy."

But two not even wrongs do not make a right.

It is understandable that Lee and Peter might have been swayed towards Lisi's paper by Lubos's rejection and dismissal of the paper, but it would have been better if Lee and Peter would have judged the paper based on its merits, which would have entailed reading it, and at least attempting to comprehend it, even if being comprehended was not Garrett's primary incentive for his media campaign, pictures of him surfing, and ultimately meaningless youtube video with the hot, english female voice.

At least Lubos read the paper, as did Distler.

Did Lee? And if so, how can he call it "fabulous?"

133. **Bee**
November 24, 2007

Can you turn the tide? : Garrett gave a seminar here at PI about his work in early October, he stayed two weeks during which we had several discussions. New Scientists asked Lee and me for comments on the paper before it was on the arxiv but then extended the deadline (I am not sure why but think Lee complained). I am glad they did, as I find it extremely inappropriate to ask people to comment on a paper they haven't even seen. If you ask me, they should leave at least several months to think about it. There is a reason why good peer review takes time. The only reason my blog post went out as soon as the paper was on the arxiv was that I had written it previously, since Garrett sent me a copy of the paper (plus it has been on his wiki anyhow). As far as I know NewScientist knew about Garrett's stuff from a talk he gave earlier during the summer this year. Nothing of that interest in Garrett's work had anything to do with Lee or Peter. I find it quite astonishing how you (and others above) manage to blame a single person using the word 'fabulous', ignoring all the hundreds of people who echoed the same initial articles over and over again, without adding one epsilon of content, gradually polarizing and distorting the little scientific content such an article can have at all. If anybody is to blame for that then its everybody not acting against such a decline of journalism down to cheap entertainment, with no other purpose than collecting links and visitors who click on the advertisement banner.

134. **TonyC**
November 24, 2007

"How Can We Turn The Tide" says "It is now apparent that the Lisi affair was due largely to 'String Theory Envy'":

Why is that? Have you found some fatal error in Lisi's E8 mapping? If so, share it with us. If not, what makes this claim "apparent"? If Lisi maps all known particles to E8 correctly, this puts constraints on possible interactions, and constraints on future discoveries. Why isn't that a "theory"? Even if he has to work out his symmetry breaks "by hand", we may gain insight by examining the results and looking for commonalities.

What evidence do you present that Lisi's primary incentive was anything other than being comprehended? It seems rather difficult to support your theory of "String Theory Envy" when Lisi's proposed mapping is at least falsifiable by the LHC and String Theory (also not a theory), to my knowledge, is not. If anything, I think the opposite is true, the string theory community envies Lisi getting any attention whatsoever, probably because the very concept of falsifiability threatens their funding and Lisi is emphasizing the value of that radical concept. As long as we are claiming things are "apparent" with zero evidence to support the claim, I claim my interpretation is even more "apparent" than yours.

135. **alex**
November 24, 2007

Bee, you forgot to add another Lee's quote: "It is one of the most compelling unification models I've seen in many, many years".
So, at least Lee has "seen" it and found it "the most compelling". To make such a conclusion one would presume the Lee must have taken some time (as Jacques did, for example) to check at least some of the details.

136. **H-I-G-G-S**
November 24, 2007

Bee,

I don't think you can blame this all on journalists gone bad. It's amazing that after a seminar and a few discussions neither you nor Lee could understand what Lisi was saying well enough to tell that it was total nonsense. If you had realized that I presume you would not have found it worth blogging about and the reporters would have had one less reason to get worked up over this.

TonyC,

Go read Distler's blog. If you know enough math to understand it you will see that all the elementary particles do not fit into E8. Period, no ifs ands or buts. And the "triality" he proposes as some kind of cure is equally meaningless. This "theory" won't be tested at the LHC. You can't make predictions if your starting point is bunch of jumbled up concepts that don't make sense, a hope and a prayer that someone else will make sense of it, and wrong math.

137. **TonyC**
November 24, 2007

H-I-G-G-S:

Distler proves you can't have 3 copies of R in E8. Lisi acknowledges this and anticipated the problem. Later Lisi admits to a mathematical misunderstanding. Distler has NOT proven all the elementary particles do not fit in E8, they just cannot fit in the way Distler thinks they must fit, so he dismisses the entire exercise as futile; while Lisi does not. This academic disagreement over whether Lisi's mapping has any merit or might lead to something useful seems to me pretty thin evidence for accusing Lisi of String Theory Envy or celebrity mongering.

138. **Arun**
November 24, 2007

H-I-G-G-S: I see nothing wrong in Bee's no-sharp-elbows approach – she reached the right conclusion.

139. **H-I-G-G-S**
November 24, 2007

TonyC:

Please tell me what you mean, in mathematical terms, by "Lisi maps all known particles to E8 correctly." Distler interpreted this in the obvious way and showed it was false. If you have some other mapping of this statement into mathematics please tell me and then we can check if it is true. If you can't tell me what you mean than I would politely suggest that you should withhold judgement until you learn more.

140. **Bee**
November 24, 2007

H-I-G-G-S: *I don't think you can blame this all on journalists gone bad. It's amazing that after a seminar and a few discussions neither you nor Lee could understand what Lisi was saying well enough to tell that it was total nonsense. If you had realized that I presume you would not have found it worth blogging about and the reporters would have had one less reason to get worked up over this.*

I don't blame it all on journalists, certainly not. I blame everybody who blames others, instead of taking responsibility. Journalists who say they just believed the scientists, scientists who say journalists just misrepresent them, bloggers who say it's all the media's fault. I blame myself since I possibly contributed to the hype, even though it was my intention to provide a balanced article with pros and cons. I had heard Garrett's talk, I had read the paper, and I told him (repeatedly) that I think he does not clearly state how much extra assumptions he has to make by hand. I told him that even before he gave the talk at PI. When I wrote my post, I knew the New Scientist article would come out soon, I knew they had asked Lee for his opinion, and I knew Lee's opinion. I know New Scientist enough to realize they would listen to him. I knew the title of the paper, and I know Lubos enough to predict he'd jump on it, especially thanks to the acknowledgement list. As somebody said so aptly in a comment somewhere (sorry, can't recall who or where), everybody did exactly what one could have expected.

How come things like this happen? Because it's a completely premature reporting on an unclarified issue, a paper that has just been out a couple of days, in a field where peer review takes months, and NO, I don't think peer review can be 'replaced' by discussions on blogs. I don't appreciate pressure on scientists to put forward opinions, especially when so completely unnecessarily as in this case. I mean, it's not like there was some phenomenon observed, and expert opinions were needed to explain it. If somebody says he doesn't want to comment or doesn't feel qualified one shouldn't criticise him for knowing what he doesn't know.
Regarding what I blogged about: I have repeatedly stated that there are many things about Garrett's stuff that I don't understand, and that I don't like. I have clearly written in my blog post what I find promising about it and what not. I have clearly stated that he does not quantize gravity, what problems he does not address, that he has to write down the action by hand to reproduce the standard model, and that I don't think it qualifies as a TOE. Neither do I think it is just nonsense. I just think it was, and still is, premature to decide whether it will eventually turn out to describe a part of how nature works.
If I would write the post today, I would have made the basic statements much clearer and more obvious, but I did not expect the audience I got. Also, I am not a journalist, I am a scientist, and my writing skills are limited. There are several points that only become clear in the comments, and I am afraid little people read more of my posting than the first sentences, and even less actually tried to understand it.
After all, I am really sorry for Garrett. Though I think he knew exactly what he was doing, the results might eventually not be as expected.

141. **Bee**
November 24, 2007

Hi Arun,
Thanks. Seems it took me an awfully long time to write the previous comment, so I only just read yours now. In fact, I said this already in the post, but maybe not clearly enough
"However, for me the question remains which problem he can address at this stage. He neither can say anything about the quantization of gravity, renormalizability, nor about the hierarchy problem. When it comes to the cosmological constant, it seems for his theory to work he needs it to be the size of about the Higgs vev, i.e. roughly 12 orders of magnitude too large. (And this is not the common problem with the too large quantum corrections, but actually the constant appearing in the Lagrangian.) To make predictions with this model, one first needs to find a mechanism for symmetry breaking which is likely to become very involved."

Have a nice evening.
Best,

B.

142. **Eric**
November 24, 2007

Just curious why noone has brought up the work of Jacob Bourjaily with geometric engineering three-family GUTs from E8? It seems to me to be somewhat higher quality work than that of Lisi.

143. **Arun**
November 24, 2007

Dear Bee,
Yes you did state it in the post; but then there was the comments thread, which might give the impression that some of your objections were being addressed; that is why the comment is important in my interpretation of the world – you were willing to listen (good!), and when nothing convincing was said, you said so (great!), and wound up the discussion (excellent!).
Best,
-Arun

144. **TonyC**
November 24, 2007

>> H-I-G-G-S: If you have some other mapping of this statement into mathematics please tell me and then we can check if it is true.

I can't, you are right, I was wrong to say so. I am basing that statement on what Lisi has said, presuming he isn't lying.

>> If you can't tell me what you mean than I would politely suggest that you should withhold judgement until you learn more.

Good advice. As I have politely suggested it is too soon to pass judgement on Lisi's mapping, based on Distler's proving the obvious approach won't work, when Lisi already anticipated that objection in his paper. I refer you to Lisi's response on Distler's blog. And it is also too soon to pass judgement on Lisi as some sort of fame whore, which seemed to be the suggestion of "How Can We Turn The Tide". Perhaps I am not the only one that needs to withhold judgement until they learn more.

145. **amused**
November 25, 2007

"It is now apparent that the Lisi affair was due largely to "String Theory Envy." "

It's quite funny how some string theorists (the less enlightened ones) think that any challenge to ST must be driven by "envy". Envy of what, exactly? Of all your PRL publications?

146. **alex**
November 25, 2007

Eric:"Just curious why noone has brought up the work of Jacob Bourjaily with geometric engineering three-family GUTs from E8? It seems to me to be somewhat higher quality work than that of Lisi."

Lubos blogged about it but the press did not pick up on that. Maybe that's because Jake is still a grad student and not a surfer with a Ph. D. :).

He does get exactly three families from a single E8 and his paper is much easier to read.

Curiously, he automatically gets three generations of Higgs doublets which is an interesting prediction. The symmetry breaking is geometric, described pretty explicitely by five complex parameters.

There is a very nice table on page 6 of his paper where the explicit spectrum is given. The title of Bourjaily's paper is more humble as he does not claim to have a "theory of everything", so maybe that's another reason why Bee and others decided not to blog about his work 🙂

147. **H-I-G-G-S**
November 25, 2007

Bee,

Thanks for the long and thoughtful response.

TonyC,

I read Lisi's response to Distler. I don't think Lisi is lying (and I said nothing about his being a fame whore). He has been very straightforward in his responses to Distler. I think the problem is more one of inexperience and a lack of critical judgement. It is not considered good form to make dramatic claims at the beginning of a paper (i.e. that all standard model particles fit into E8) and then only on page 22 point out that this is not really correct. You say Lisi "anticipated" this problem. I would say he was aware of it, and tried to solve it, but his solution (involving triality) didn't work. If you want to know why, please read Distler's blog.

It's probably time to drop this topic. No use beating a dead horse. Unless of course Lee decides to write in and tell us all why he thought this theory was so "fabulous."

That would be entertaining, to say the least.

148. **Eliza**
November 25, 2007

Watching the hostility and contempt heaped upon Garrett Lisi by the physics establishment has been an ugly spectacle, but hardly a shocking one. And he is right that his idea is a beautiful; I wish him every success in winning converts from among all the bright people futilely beavering away at string theory. But while his idea is beautiful he himself is also to be admired–an Overman of sorts. His battle with the string theorists–a herd of Last Men–reminds me of Nietzsche's Zarathustra:

Give us this Last Man, O Zarathustra," — they called out — "make us into these Last Men! Then will we make you a gift of the Overman!" And all the people exulted and smacked their lips. Zarathustra, however, turned sad, and said to his heart:

They do not understand me: I am not the mouth for these ears.

Perhaps I have lived too long in the mountains; I have hearkened too much to the brooks and trees: now I speak to them as to the goatherds.

My soul is calm and clear, like the mountains in the morning. But they think I am cold, and a mocker with terrible jests.

Now they look at me and laugh: and while they laugh they hate me too. There is ice in their laughter.

* * *

Long slept Zarathustra; and not only the rosy dawn passed over his head, but also the morning. At last, however, his eyes opened, and amazedly he gazed into the forest and the stillness, amazedly he gazed into himself. Then he arose quickly, like a seafarer who all at once sees the land; and he shouted for joy: for he saw a new truth. And he spoke thus to his heart:

A light has dawned upon me: I need companions — living ones; not dead companions and corpses, which I carry with me wherever I go.

But I need living companions, who will follow me because they want to follow themselves — and to the place where I will. A light has dawned upon me. Zarathustra is not to speak to the people, but to companions! Zarathustra will not be shepherd and hound of the herd!

To steal many from the herd — for that purpose I have come. The people and the herd will be angry with me: the shepherds shall call Zarathustra a robber.

Shepherds, I say, but they call themselves the good and just. Shepherds, I say, but they call themselves the believers in the orthodox faith.

Behold the good and just! Whom do they hate most? The man who breaks their tablets of values, the breaker, the lawbreaker: — yet he is the creator.

Behold the believers of all faiths! Whom do they hate most? The man who breaks up their tablets of values, the breaker, the law-breaker — yet he is the creator.

The creator seeks companions, not corpses — and not herds or believers either. The creator seeks fellow-creators — those who grave new values on new law-tablets.

The creator seeks companions and fellow-reapers: for everything is ripe for the harvest with him. But he lacks the hundred sickles: so he plucks the ears of corn and is vexed.

The creator seeks companions, and such as know how to whet their sickles. They will be called destroyers, and despisers of good and evil. But they are the reapers and rejoicers.

Zarathustra seeks fellow-creators, fellow-reapers and fellow-rejoicers: what are herds and shepherds and corpses to him! …

I will sing my song to the lonesome and to the twosome; and to whoever who still has ears for the unheard, I will make his heart heavy with my happiness.

I make for my goal, I follow my course; over the loitering and tardy I will leap. Thus let my on-going be their down-going!

149. **mitchell porter**
November 26, 2007

Eliza, the initial criticisms have been borne out. You are not doing the guy any favors with your cut-and-paste.

"Better to know nothing than to half-know many things! Better to be a fool on your own account than a wise man in someone else's eyes!"

150. **Eliza**
November 26, 2007

Mitchell, I don't think it's appropriate at this point to dismiss Lisi out of hand. Clearly his theory is in its infancy; there is a great deal of work to do; and as long as he is working toward falsifiability, sensible people should reserve judgment.

And my primary object is not to do favors for Garrett Lisi. What brought the passage to mind was the hysterical vituperation of people like Lubos Motl who have attacked Garrett with the zeal of religious fanatics.

And it's interesting that you cast yourself in the role of the trodden one. You must have a very narrow window on the world of knowledge, for the man goes on to expand and extend his remarks:

"That, however, of which I am master and knower, is the brain of the leech:- that is my world!

How long have I investigated this one thing, the brain of the leech, so that here the slippery truth might no longer slip from me! Here is my domain!…

For the sake of this did I cast everything else aside, for the sake of this did everything else become indifferent to me; and close beside my knowledge lies my black ignorance."

Moving on, anyone who thinks that Lisi's publicity is "bad for physics" is grasping the wrong end of the stick. I think we can all agree that if he is right, Lisi will be a celebrity, and money will be flung at researchers from all directions for many years to come. There'll be tearing up the county building particle accelerators.

If his theory fails? There will be very little mention of it in the popular press. "Surfer Dude's Theory of Everything Proven Wrong": That's a dog-bites-man story, not big news. But even if it is reported, the public is sophisticated enough to understand that falsified theories are still valuable: they tell us what the answer is not. Even a child can understand that.

Most absurdly, someone has suggested that if Lisi fails to deliver after all the positive press coverage, the public will then lose faith in physics research. Baloney. Decade after decade string theorists give us nothing but fodder for sci-fi novels; and yet, thirty years on, every time they pass around the begging bowl the taxpayers obligingly fill it up. And the beauty of it is that they make no falsifiable predictions that can be tested, so they can carry on the flimflam until someone like Lisi comes along and reminds everybody what the meaning of a genuine scientific theory is. That's the real threat he poses.

The truth is that people don't expect a whole lot of progress from the physics community. They don't even mind the somewhat parasitic nature of the relationship. What they do want for their money is evidence of activity, a sense that scientists are enthusiastically working as hard as they can to move the ball down the field. Whether Lisi's theory soars or flops, he's given people a sense that physics deserves to be funded. We should all be grateful for that, even you, master of leeches.

151. **mitchell porter**
November 26, 2007

If only I *was* a master of Leech-branes!

I am only self-half-educated in these matters. But after its trial by fire, it seems clear that Lisi's theory has two big holes, generation structure and quantization, which would have to be fixed before one could ever set about "calculating the masses". Lubos Motl was right about this and that should be respected.

152. **Chris Oakley**
November 26, 2007

Eliza,

You do not give me the impression that you have actually done research in theoretical particle physics. If you had, you would know that group theory is the easy bit. If it is a question of fitting all the known particles into representations of groups then it can be done in a much simpler way than Lisi's. SU(5), the simplest unifying scheme, accommodates all the known particles neatly, the only problems – at least in representation theory terms – being that it predicts extra unobserved gauge particles and does not include gravity. Lisi's model is not a GUT, but the classification of particles in representations of a gauge group would appear to proceed in much the same way. But this group symmetry can only be approximate – the particles in each group representation have widely different masses in the real world, meaning that the symmetry can only be approximate and it is in the – currently – arbitrary and ugly breaking of the symmetry that most HEP theorists will quail (or, at least, ought to). I would be much more interested in researches that address this more difficult, and yet more important issue.

153. **Bee**
November 26, 2007

Eliza: *"Moving on, anyone who thinks that Lisi's publicity is "bad for physics" is grasping the wrong end of the stick. […] Most absurdly, someone has suggested that if Lisi fails to deliver after all the positive press coverage, the public will then lose faith in physics research."*

It wouldn't have been my choice of words, so it probably wasn't me who you are referring to, but let me comment on that anyhow. Its not specifically about Lisi, but generally there are three problems with such reporting.

For one, such reports are simply inaccurate and cause confusion that was avoidable. The world is already confusing enough, why do we have to make people believe a giant Mandela explains the elementary particles, or we've found another universe, if we can't be sure of it. And if we are not, can't we at least expect it to be reported on with an appropriate amount of caution, clarifying the amount of speculation?

Second, as it is, the public opinion about our research matters. One aspect of this is what you dismiss, namely that if this happens more often they will start wondering what kind of science it is that we do anyhow. Is is good for something? Science or Religion? Fact or Fiction? I mean, look, theoretical physicists DO already have a hard time getting funding because it's not immediately apparent what our work might be good for. Prematurely spreading reports of fabolous successes that then end up in the category 'what ever happened to' won't help. What is maybe worse is that the way Garrett's story was sold it says hey, the surfer dude can do it, who needs all these overpaid professors? You should have seen the amount of 'Theories of Everything' that I got send since that thing hit the headlines.

Third, such reporting influences the discussions and the opinion making process in our community. If you deny that, you are hopelessly naive. It's the topic of the week, it's what people talk about on the corridor. How many grad studs are out there, who are now dreaming of pulling off a similar show?

I find these trends very worrisome.

154. **Dany**
November 26, 2007

Eliza:" anyone who thinks that Lisi's publicity is "bad for physics" is grasping the wrong end of the stick".

AGL et al did a great job; not in the theoretical physics but for the theoretical physics. I think he deserves a prize and I would be pleased to see him laughing all his way to the bank.

Regards, Dany.

155. **TonyC**
November 26, 2007

>>> BEE: How many grad studs are out there, who are now dreaming of pulling off a similar show?

Ha! I am a PhD candidate (not physics), I don't know any of us that can be characterized as "studs". But the story is certainly corridor-worthy, and who doesn't dream of having some revolutionary insight into the workings of our field? I know 99% of papers are incremental (or no) progress, but I actually think that "hero" stories like this are good for us grad students, no matter if Lisi turns out to be prince or goat. Chances are worth taking and original ideas really can revolutionize a field. My advisor revolutionized his field about 12 years ago. This kind of stuff is inspiring. If I thought my career would be a long series of incremental progress and survey papers, I'd leave for industry tomorrow. And, at the risk of being chastised by HIGGS, doesn't Lisi at least contribute a clever way of sidestepping Coleman-Mandula?

156. **Bee**
November 26, 2007

Hi Tony:

Sure, we all dream of having revolutionary insights. That's not the point I was trying to make. The question is what is more relevant: having revolutionary insights. Or making everybody believe you have revolutionary insights? Sorry about the 'studs', I must have picked it up somewhere, I didn't mean it in a dismissive way.

Best,

B.

157. **Peter Woit**
November 26, 2007

All,

The comment section on this blog is becoming something I mostly don't want to read anymore, and I'm assuming that may also be the case for many other people. Please stop submitting off-topic, irrelevant, mis-informed, poorly thought-out, etc. comments. This is not a place to write in with whatever comes into your head. From now on I'll be deleting many more comments that aren't substantive and informative.

158. **H-I-G-G-S**
November 26, 2007

TonyC,

I won't chastise you, but the answer to your question about whether Lisi has contributed a clever way of side-stepping Coleman-Mandula is "no."

Lisi himself does not make this claim. On his web site
http://deferentialgeometry.org/ you will find a set of slides from talks he has given. At the bottom are extra slides including one labelled Coleman-Mandula. On that slide he says
"E8 Theory does not allow a subgroup locally isomorphic to the Poincaré group. The S matrix exists as an approximation, in which the theorem is satisfied." What this means in plain english, and in terms of physics, is actually explained quite clearly on Lubos Motl's post on this if you can ignore the ranting. Briefly, in a theory with a cosmological constant there is no S-matrix. Coleman-Mandula is a theorem about symmetries of the S-matrix. So, no S-matrix, no theorem. But this fact is only relevant if you are doing scattering experiments on scales the size of the universe. At Fermilab, or the LHC or in any conceivable experiment you can treat space as flat to an extremely good precision. There is then an S-matrix that experimentalists measure. It obeys the Coleman-Mandula theorem. Lisi does not provide an exception.

Peter,

The above is on topic, but this comment is slightly off topic. I hope that pissed you off because your last "update" on this topic pissed me off. The link provides a "sociological" critique of Distler's analysis, not a mathematical or physical one. You might mention that, you know, just for clarity and fairness and to show you are such a mensch. Distler can be condescending and bristly, but he got the math and the physics right. You didn't, and neither did Lisi. And anyway, compared to Pauli, the author of the quote that provides the title of your blog, Distler is a pussycat. Maybe you should start criticizing Pauli for being such a jerk. Or maybe you see that criticizing Distler for being difficult while wanting to model your skepticism after that of Pauli's is exhibiting just a wee bit of hypocrisy.

159. **Peter Woit**
November 26, 2007

H-I-G-G-S:

First of all, I don't model myself in any way after Wolfgang Pauli. I'm just borrowing his phrase.

I linked to Distler's posting because, yes, he did have a worthwhile discussion of some of the group theory involved. The fact that Garrett responded to this intelligently and politely led to an exchange that was valuable for anyone to read who wanted to understand what is going on in Garrett's paper.

I also think though that the "sociological" discussion is highly relevant. There is absolutely no reason for Distler to behave in the juvenile way that he does. He's an adult, middle-aged man and should behave like a grown-up and like a professional. If he doesn't understand this, others need to point this out to him.

If you don't think this is a problem, that his behavior is acceptable, and mine is what you want to criticize from behind anonymity, that's your choice. I have no idea who you are: for all I know you're a high school student too young to understand what mature behavior is. If you're actually an adult professional though, and you are in the field of string theory/particle theory, you might want to consider what effect he and Lubos are having on the perception of people in that field by outsiders. Smolin got a lot of criticism for some of the comments in his book about the behavioral characteristics of some theorists (I avoided this topic in mine), but Jacques and Lubos make it look as if he was far too kind.

Go back and read carefully the posting I linked to. The person who wrote it I think does a good job of describing what this looks like to outsiders. They can't understand the group theory, but they can understand the significance of this kind of juvenile behavior.

160. **dr**
November 27, 2007

Peter,

"Detail"?!?

H-I-G-G-S,

People who have scientific arguments talk about science.
People who don't have scientific arguments talk about good manners.

You decide who between Distler and Woit/AngryPhys falls in which category.

161. **a.k.**
November 27, 2007

..I still have to correct my above remark concerning the 'non-product' structure of the embedding discussed, this of course applies only to the 'triality'-part of the embedding. From my point of view, at least by inspecting the triality-part of F4 and its specific Lie-Algebra-structure (see Baez' week 90) a given connection on E8 should reduce to any of the obvious 'subgroups' R in the triality part, given the E8-bundle reduces to them, but this should depend on the base manifold, imposing a condition (allowing sections in a E8/R-bundle). My hypothesis, of course, is that these facts are at least necessary to get 'predictions'.

162. **Chris W.**
November 30, 2007

Public Radio International's *The World* did a story tonight on Garrett Lisi's work. It includes an interview with Marcus du Sautoy (Oxford).

163. **Yousuf**
December 2, 2007

Hi Garrett, just wondering, was the fact that the E8 structure was just recently solved earlier this year by [mathematicians]() in any way related to the decision to use it as your model here? What I'm asking was if the E8 mathematics hadn't been solved earlier, would you still be able to come up with this physics theory?

164. **Peter Woit**
December 2, 2007

Yousuf,

The mathematical result earlier this year about E8 has nothing to do with the way Garrett is using it, they're two quite different things.

165. **Yousuf**
December 3, 2007

> The mathematical result earlier this year about E8 has nothing to do with the way Garrett is using it, they're two quite different things.

Hi Peter, thanks for clearing that up for me. I was a little curious, considering how coincidental the two announcements to do with E8 were with each other (i.e. well within a year of each other).

On another note, I was in the middle of reading your book *Not Even Wrong*, when this story broke. So I stopped reading the book to see what all the E8 hubbub was about. Ironically, when I got back to the bookmarked chapter, I found you were talking about some of the BRST and Coleman-Mandula theorems that are considered controversial about this theory. Just love these ironies and coincidences.

166. **mitchell porter**
December 7, 2007

Lee Smolin has a new paper ([arxiv:0712.0977]()) on Lisi-like theories in which he says:

"We close the introduction by noting that the well-known Coleman-Mandula no-go theorem is avoided because that only applies to an S-Matrix whose symmetries include global Poincare invariance. This theory, like general relativity, has no global symmetries, the Poincare symmetry acts only on the ground state not the action, and only in the limit in which the cosmological constant is zero. In fact, there is a nonzero cosmological constant, as it is related to parameters of the theory. By the time the S matrix in Minkowski spacetime could be defined in this theory one will be studying only small perturbations of a ground state in a certain limit and the symmetry will only apply in that limit and approximation. As we shall see below, the symmetry will already be broken by the time that approximation and limit are defined, in such a way that Coleman-Mandula theorem could be satisfied in its domain of applicability."

It has already been observed in a number of places that the Coleman-Mandula theorem has generalizations, and so one should not expect a slight deviation from Poincare symmetry to make much difference. It also seems to be true that the action actually advanced in Garrett's paper *can* be quantized without a problem, because most of the notional E8 symmetry is broken by hand, and the actual, residual symmetry is something far more mundane. This remark by Lee Smolin seems to segue between both of these 'reasons why Coleman-Mandula doesn't apply': we start out with the claim that global features make CM irrelevant, and then we are told that by the time we can actually calculate something, the symmetry will have been broken to harmlessness anyway. Call these the 'argument from global properties' and the 'argument from de facto symmetry breaking'.

My questions –

Is the argument from global properties (that CM is irrelevant to a certain class of theories, that a novel loophole exists) valid or not?

Is there any logical connection between that argument and the argument from de facto symmetry breaking?

I intend to go away and try to answer these questions for myself, but I know there are far more knowledgeable people reading.

167. **Thomas Larsson**
December 7, 2007

It will be interesting to see what Motl and Distler will make out of Smolin's paper. I bet they will be unimpressed.

Without being up to date about the fine print of the CM theorem, I am very suspicious of the idea that a bosonic operator can change a fermionic state into a bosonic one or vice versa. This seems to be the case with Lisi's paper, if he puts bosons and fermions into the same multiplet of a bosonic Lie group.

168. **Sara**
December 19, 2007

Can a theory of everything be "exceptionally simple"?

169. **mitchell porter**
December 19, 2007

The paper's title is a pun on the mathematical nomenclature; E8 is an "exceptional" "simple" group, *simple* meaning that it lacks a certain type of internal structure, and *exceptional* meaning that it's one of a handful of simple groups which fall outside the infinite families to which all the others belong. I suppose it's also a reference to the unified theories of recent decades; Lisi wants to do without unverified innovations like supersymmetry and extra dimensions.

A theory of everything can be simple, so long as its implications are sufficiently complex. Since most things that exist are regarded as already explained by existing physics, for most physicists the attempt to explain "everything" reduces to explaining, or at least completing, the existing fundamental theories. Everything else is just applied science and historical contingency.

170. **Sara**
December 19, 2007

Thank you Mitchell for your answer! It's a very clear explanation.

Popularizing Science

November 12, 2007
Categories: Uncategorized

While it's not one of my main goals in life, I'm all in favor of the idea of popularizing science and making it as accessible as possible to as many people as possible. But sometimes I do wonder about the kind of things scientists get involved with when they try and do this. Just this morning I ran into these stories about science that make me ask myself:

- Is it a good idea for physicists to appear on a radio show discussing what happened before the big bang, or does the lack of any evidence about this or of a convincing model mean that this is just inherently too speculative a topic to be sold as serious science to a wide audience? Should one perhaps leave this topic to the Bogdanovs?
- Is it a good idea for physicists to promote to the public their work on time travel? Or might this also give the public some misleading ideas about science? (via i postdoc, therefore I am, but there seems to be a whole genre of "time travel" books written by theoretical physicists).
- Is it a good idea for physicists to appear on a TV show explaining the forces involved in crushing beer cans, as part of a segment on whether women can crush beer cans with their breasts? Especially physicist bloggers known for attacking other physicist bloggers for their sexism and media-inflated nonsense? (via here and here)

Comments

1. **anon.**
   November 12, 2007

   'Is it a good idea for physicists to appear on a TV show explaining the forces involved in crushing beer cans, as part of a segment on whether women can crush beer cans with their breasts? Especially physicist bloggers known for attacking other physicist bloggers for their sexism and media-inflated nonsense? (via here and here).'

   The third link doesn't seem to work? It goes to a page with videos like 'How tiny can a G-String be before it becomes illegal?'

   This seems to be related to issues of string theory, seeing that the strings of string theory have zero width.

   Is the illegality of a G-string a metaphor for the illegality of string theory when promoted as being a scientific theory?

2. **Jonathan Vos Post**
   November 12, 2007

   You fool! You've given away the secret that women crushing beer cans with their breasts leads to time travel to before the Big Bang.

   Of course, that's because aluminum beer cans filled with virtual squids become gravity wave detectors, as the Joseph Weber/Bob Forward gravity-wave detection apparatus of 1969 was enhanced by SQUID detectors, as Hawking and Thorne deduced when Forward tunneled to an alternate universe where he was a best-selling Science Fiction author…

   The g-string hotlink had to be disabled, else your readers would Know Too Much about Bikini Atoll.

   Oh, wait a minute. So long as your audience is sure that we are joking, then we don't have to kill you. Which is good, because we think you can be useful in fighting the invaders from E8.

3. **Coin**
   November 12, 2007

   On points one and two, I would say that the answer is sure, as long as the physicists in question are unambiguous about exactly how speculative their responses are. People are always more interested in hearing about bleeding-edge speculative stuff than they are in hearing about stuff that already works– once something is understood it's no longer "news". Focusing on such speculations can even (if done carefully) advance "normal" science literacy, since once the "hook" of the speculative science is in place this can be used as an excuse to feed the listener some "normal" science to explain how the speculative science works. It's like getting the dog to swallow a pill by wrapping it in bacon.

   The case of Dr. Mallett in specific however maybe should count as an exception considering that there are so many basic unanswered criticisms of his work out there that I don't think he *could* accurately present the extent to which his work is speculative, short of actually saying "this is science fiction". Still I guess this too would be a matter of how it is presented. "If you can get me a naked singularity or a fiber-optic cable of infinite length, I can build you a time machine" would be a legitimate and interesting pop-science way of presenting this work, I guess, but leaving out those little details would seem to constitute dishonesty. I don't know what tack was taken in the article because no hablo alemán.

   I have no comment on point three.

4. **Peter Woit**
   November 12, 2007

   Coin,

   One problem is that I've never once seen a physicist discussing highly speculative work in the media accurately explain how speculative it was. Even if people do make disclaimers like this to reporters, that's the kind of thing that ends up getting cut in whatever appears.

   Sure, many people would rather hear about science fiction than science (no, not all people are this way, some actually like the real thing…). The problem with feeding them science wrapped in science fiction is that they ignore the science and pay attention only to the tasty science fiction morsel they are being encouraged to pay attention to. And they then end up unable to tell the difference between science and science fiction.

5. **dragon**
   November 12, 2007

   Comparing Carroll, Steinhardt, Khoury, and Brandenberger with the Bogdanovs doesn't really do a lot for your own credibility. In fact it looks very much like the kind of thing that Lubos Motl would say.

6. **a quantum diaries survivor**
   November 13, 2007

Hi Peter,

indeed, good question your #3. I think we can forgive Clifford for having been fooled a little by the soft porn business – his goals were commendable, and he does not seem to bother much if I read him correctly in his blog.

I think Clifford's will to appear on a TV show for guys way offset a critical assessment of the use they were likely to make of his good science explanations.

Cheers,
T.

7. **milkshake**
November 13, 2007

I am all for further elucidating the breast-crushing forces acting on a beer can. Unlike the two previous examples, it is all a healthy fun and no laymen are being blinded by science in the process.

8. **anon.**
November 13, 2007

'Comparing Carroll, Steinhardt, Khoury, and Brandenberger with the Bogdanovs doesn't really do a lot for your own credibility. In fact it looks very much like the kind of thing that Lubos Motl would say.' – dragon

Dragon, your comparison here of PW to LM doesn't really do very much for your credibility either! The Bogdanov's come into this for their book, *Before the Big bang*. BTW, Professor Ernest Sternglass of Uni. Pittsburgh has a similarly titled book, which is more exciting as it contains lengthy discussions of Sternglass' meetings with Albert Einstein who advised him to 'be stubborn' and with R. P. Feynman; who blew his top when Sternglass admitted he hadn't bothered to completely work out the consequences of his theory before trying to get interest in it.

9. **moron**
November 13, 2007

While it's not one of my main goals in life, I'm all in favor of the idea of popularizing science

That's good, because you've done nothing but accomplish the exact opposite: **un**popularizing science (and popularizing un-science.)

10. **anon.**
November 13, 2007

moron, you merely disagree with the following *definition* of science:

'Science is the organized skepticism in the reliability of expert opinion.' – Feynman.

(Smolin quotes that on p307 of the US edition of his latest book.)

Your attitude is very much in the Ptolemic mindset: that anyone asking being critical about mainstream so-called 'science' is doing a disservice to science, rather than the *exact opposite*. (Guess we'll just have to politely agree to disagree on this one … if you know how to behave.)

11. **Anders R**
November 13, 2007

un-science?

12. **Richard Feynman**
November 13, 2007

'Science is the organized skepticism in the reliability of expert opinion.' – Feynman.

Thank God someone, after all these years, **finally** interpreted my quote correctly!

Yes, indeed, "anon." is right (though Heaven knows why he would want to remain anonymous and forego having such brilliant insights attributed to him):

I meant to say, "Science is the organized credulity towards the reliability of fraud-and-crackpot opinion. The mark of a great scientific idea is a complete lack of any comprehensible relation to what many others have successfully done before." In fact, I would specifically have mentioned loop quantum gravity as the apotheosis of great scientific thinking, but I was too jealous to give them their rightful place in the sun. After all, when inventing quantum electrodynamics, I foolishly relied on the so-called "expert" opinion that said any new theory describing the interaction of charged matter with light would have to be tied down to the moribund ideas of such so-called "experts" as Einstein, Dirac, and Maxwell who claimed — **without any evidence or logic whatsoever** — that relativity, quantum mechanics, and classical electrodynamics would all have to be applicable in the appropriate regimes of validity.

If only I had been a true "seer" like Lee Smolin, who wisely dispensed with such garbage!
, and a stern refusal to ground one's work in the

13. **dagon**
November 13, 2007

"Comparing Carroll, Steinhardt, Khoury, and Brandenberger with the Bogdanovs doesn't really do a lot for your own credibility. In fact it looks very much like the kind of thing that Lubos Motl would say."

Anon, Dragon,

Indeed, it's quite misleading (and dishonest) to lump Carroll, Steinhardt, Khoury and Brandenberger in with the Bogdanovs. They are all serious and well-respected scientists and the post made no effort to distinguish between these kinds of scientists and the Bogdanovs. Anon, you may argue that the connection is simply the title of the book but I think that the innuendo of the post is quite clear and very insulting.

14. **Peter Woit**
November 13, 2007

dragon/dagon,

I'm well aware of the difference between Carroll, et al. and the Bogdanovs and was not saying that they are the same. But I'll stick to what I did say: until there is some relevant data or a convincing model for physics before the big bang, serious scientists should avoid going to the popular media to promote their extremely speculative work in this area. When they do this, I don't think they help the cause of science, but blur the distinction in people's minds between real science and its Bogdanovian varieties.

15. **Peter Woit**
November 13, 2007

moron/Richard Feynman,

Sorry to see that some of your anti-Lee Smolin rant got cut off by the blog software.

I don't think "science" is in any danger of being "unpopularized" by anything either Smolin or I have done. What is in danger of being "unpopularized" is a specific subfield of science. To the extent that is actually happening it's less due to me and Smolin than to the behavior of people like yourself, who from the most illustrious and respected research institutions in the world, post idiotic comments here using stupid pseudonyms to remain anonymous and avoid taking any responsibility for their own behavior. Between this kind of thing, Lubos Motl, Lenny Susskind and the widespread promotion of anthropic pseudo-science, yes, a previously respected sub-field of science is in real danger of being "unpopularized".

16. **fh**
November 13, 2007

It should exactly NOT be left to the Bogdanovs! To the laymen it is at first glance not obvious that the Bogdanovs are crackpot, if we say serious scientists shouldn't dabble in this speculative realms we leave them precisely to these people who are certainly not going to bother to infuse their presentations with caveats or down to earth science.

17. **Peter Woit**
November 13, 2007

fh,

Again, the problem I see is that serious scientists who go to the public with this kind of material just are not bothering to include appropriate caveats.

18. **moroner**
November 13, 2007

>un-science?<

maybe he means string theory?

19. **Anonymous**
November 13, 2007

Oh please make them stop! It makes one's job so much harder, obligating one to first unteach one's students the nonsense Kaku and his ilk spew on the media. I am disappointed in some of the new names who have joined that club.

I have a non-scientist friend who is truly interested in Physics. He reads popular articles and spends time trying to understand the ideas behind them. Every time I see him, he mentions some article he read about the latest in multiverses or whatnot, and I find myself in the unenviable position of having to declare his hard-earned insights illusory. For what they do to people like him, these pseudophysicists should share a special circle of hell with charlatan preachers and quacks.

20. **Bee**
November 13, 2007

I know I've said it before, but I want to repeat it again. This is not a problem that emerges in the scientific community, but just a reflection of a general sociological trend. If anything, it is surprising how long it took the popularity-drug to get into the ivory tower. It is worrisome though that even though dangers of these developments are immediately apparent, little people are able or willing to do something about it. Writing a blog post like this is better than nothing – at least it raises awareness for the issue – but on the long run it's not going to be sufficient.

21. **Bee**
November 13, 2007

oh, and if somebody could please let me know how one 'unpopularizes' things, I'd really like to hear. There's this immortal believe around that Einstein was wrong (with whatever), I'd really like to unpopularize that because my spam filter doesn't catch most of such crap and it's somewhat annoying. Thanks, B.

22. **Anonymous Person (AP)**
November 13, 2007

We should all grow up a little on all sides. If you are going to embarrass yourself on TV, then get paid for it! This idea that you are 'popularizing' science when you go on TV is naive. You are advertising science. That is what is happening.

Second, for those thinking that the public having an incorrect assumption about your silly little subject is somehow of paramount importance, it's really not. It has no practical importance really. The idea is to have people continuing to give money to support scientific causes. The reasons they might be doing this, whether it's because of extra universes or time machines or whatever is totally besides the point.

Please all of you who are advertising science, continue to expand the pool of money I have available to me! (Sing for my supper?)

23. **Peter Woit**
November 13, 2007

AP,

I agree that it probably is best to conceptualize what people are doing as "advertising" rather than "popularizing" their work, since the emphasis is typically more on selling the product than on explaining its features. My concern is that the sales tactics being used are ruining the brand, with possible future consumer backlash, or even intervention from the Federal Trade Commission…

24. **Kea**
November 13, 2007

Doesn't sound scientific to me. Heh, I'm muscular and buxom, but I don't believe I have such physical capabilities….

25. **Anonymnous**
November 13, 2007

"Second, for those thinking that the public having an incorrect assumption about your silly little subject is somehow of paramount importance, it's really not. It has no practical importance really. The idea is to have people continuing to give money to support scientific causes. The reasons they might be doing this, whether it's because of extra universes or time machines or whatever is totally besides the point." – AP

This kind of attitude might be expected from a politician or televangelist. My layman friend does care to know something fact-based about the way the universe works. If I understand you correctly, you are just too happy to reap personal profit, even if indirectly, from his being misled and lied to.

26. **dagon**
November 13, 2007

Peter,

"I'm well aware of the difference between Carroll, et al. and the Bogdanovs and was not saying that they are the same. "

That's a little like arguing that Bush never claimed that there was a connection between 9-11 and Iraq. Maybe you didn't explicitly say it but it's clear from the post that you intended to paint them all with the same brush. I was just trying to point out that doing so is extremely non-collegial. Moreover, I'm not sure it does a great deal of good for the lay-public to intentionally try to blur the lines between serious physicists like Carroll et al. and crackpots like the Bogdanovs.

27. **Peter Woit**
November 13, 2007

dagon,

You're intent on attacking me personally, based on things I did not write, do not think, and do not believe that any reasonable person would interpret my words as meaning. If you want to argue with what I actually did write, go ahead, but anonymous attacks based on accusations of insinuations that exist only in your mind are pretty sleazy behavior.

28. **zorba**
November 13, 2007

I think that a "reasonable person" would interpret

"this is just inherently too speculative a topic to be sold as serious science to a wide audience? Should one perhaps leave this topic to the Bogdanovs? "

as meaning [a] this is not "serious" science, and that [b] "this topic" is for crackpots. What other interpretation is possible? Did you really want to say , "when I say "sold as serious science", I don't mean that it isn't serious science, I just mean that, whatever it is, it should not be *sold* as serious science, even if it is serious science. Furthermore, when I say "this topic" I don't really mean work on early universe physics, what I really mean is…..umm…. something else….."
Oh. I see.

Your subsequent "When they do this, I don't think they help the cause of science, but blur the distinction in people's minds between real science and its Bogdanovian varieties."

doesn't really do much for me. Basically the show presented four competing views of the subject, which should tell any sensible layman that there is no consensus. There is not even a faint resemblance between the work of these gentlemen and bogdanovology, and the fact that you think that there is room for such a misunderstanding says more about your prejudices than about the subject.

In a word, I think an apology is called for. Do you really want to be a party to the descent of the blogosphere into a place where people's scientific work is exposed to this kind of unprovoked attack?

Even shorter: not one of your better posts.

29. **Tom Whicker**
November 13, 2007

Take Brian Greene's The Elegant Universe (video) as a prime example. It goes on for about two hours and never makes a single
coherent statement. The production is over the top, (do over saturated colors help young minds comprehend?)…
with Greene posing as a physicist of some type. No real content,
just the same meaningless analogies of Ants crawling around a
long steel cable;this to show the nature of compacted dimensions. And then Greene (Moulder) sits in the Quantum Cafe and keeps getting the wrong color Colada. This is supposed to mean something about quantum theory…..

And the voice-over just keeps re-introducing the opening premise "Could this be the Theory of Everything?!" I was reminded of the same technique used in the Von Daniken diatribes where the voice over uses the same technique of using a question to imply a non-substantiated premise, "Could this be the Work of
Ancient Aliens?!"

In the realm of Popularizing Science this was as big as it gets, and it delivered the equivalent of junk food to a starving world.

Maybe some of you actually know Greene and know what he thought of the final product. Maybe he's embarrassed. Maybe
PBS left all the real content on the cutting room floor. Where
was Scully when we needed her? So many questions…

30. **Tim May**
November 13, 2007

I think I'm older than most of you here (55, almost 56).

I grew up with a strong diet of popular science, from dubious sources in the 1950s and early 60s like "Popular Science," "Popular Mechanics," and even "Science News" (sort of the "New Scientist" of its day). And from a lot of science fiction and "Gee Whiz" science advocacy. Picture books on astronomy, the "All About" books on science, even "Topm Swift." And the classics of SF. Lunar bases, the drawings of Chesley Bonestell, the fantasies of Willy Ley, Heinlein, Clarke, Asimov, etc.

Yeah, science fiction played a big role in my life back then, but I clearly knew that things like the Moebius strip wall in one of Clarke's short stories was just SF, even as it helped motivate me to read Martin Gardner's articles on topology and Moebius strips. Cool stuff for a 13-year-old. (I'll bet most of you were similarly inspired, albeit in varying ways.) I'll bet that some 13-year-olds today are reading Lisa Randall's articles about colliding superbranes and getting interested in science….I just think that in the next 10-15 years they'll get past any initial biases and will become (some of them) full-fledged scientists. No harm done.

By the mid-60s, by the time I was about 12-14, I could separate the wishful thinking from the reality. Still, it was really, really cool (a technical word of our generation 🙂) to read about antimatter from Dirac's little mongraph on "Matter and Antimatter," which I spent my allowance on in 1966, even as "Star Trek" was beginning to air episodes where it figured in. And to read about such highly-speculative things as wormholes (via John Wheeler's articles, and recountings of his articles, circa 1968-69).

And who can forget the January 1970 cover of "Physics Today," with its famous depiction of a black hole bending spacetime? (I think that was the month….)

And so on, for Everett-Wheeler-Dewitt speculations about many worlds….

I think physicists learn to separate what is real or remotely possible from what is implausible. The "cool" stuff is a motivator–I think even for many of you folks here–but there's not too much danger that the pseudoscience will take over.

Or so I think. Believe me, there was a constant barrage of unproven or speculative or pseudoscientific cruft when I was a kid, from antigravity machines to time machines to Unified Field Theory. I don't think much harm was done to the public's interest in funding science. At least not back then.

(Maybe today's voters are less inclined to fund the next generation of atom smashers. Can't say as I blame them. But not because they got exposed to pseudoscience.)

Meanwhile, some of the speculative stuff probably is helping to draw in a whole new generation of young physicists, who will learn quickly enough the difference between pure fantasy, the speculative but possible, and the safe path of conformity.

I just don't think there's much risk that speculative science will actually mislead anyone capable of critical thought, at least not past their teenaged years.

Interesting topic.

–Tim May

31. **Peter Woit**
November 13, 2007

zorba,

Again, from behind the cover of anonymity, you're attacking me by making up things I never wrote or thought and that are not reasonable interpretations of what I did write. I'm not going to apologize for stupid things you or anyone else choose to make up. I'll stand behind what I did actually write.

Tom Whicker,

I'm not a big fan of "The Elegant Universe" TV show, for reasons that are much the same as my concern about radio programs on "Before the Big Bang". Highly speculative research with not a shred of experimental backing or other compelling evidence that it is correct just does not seem to me to be an appropriate topic for radio or TV shows aimed at very wide audiences.

And please just stop it with the hostility about things like the production values and other characteristics of Brian Greene's TV show. That's not relevant to the issue at hand. I don't question the dedication of Brian or the people on the CBC radio show to their science, their professionalism, or the sincerity of their desire to share with the public their enthusiasm for what they are working on. I just question whether such programs are really a good idea, for reasons that I have repeatedly explained.

32. **Peter Woit**
November 13, 2007

Tim,

Thanks for your comments. I think you're right that these things do play a role in inspiring some people, and that's the best argument for them. But that's only one side of the story and I think the other less positive side deserves some consideration.

33. **Tim May**
November 13, 2007

I agree Peter, and have been reading your blog for a couple of years now. (I only decided to start making a few comments when I felt I would not make a complete fool of myself in areas I know little of.)

I thought Brian Greene's 2-hour "Elegant Universe" special was pretty vague, with not much attention given to "But why do we think it's a plausible model?" sorts of considerations, but was not all that damaging.

It takes a fair amount of sophistication to understand the criticisms you and Lee Smolin make. (I really appreciated Lee Smolin's fairly even-handed treatment in "Three Roads to Quantum Gravity." His recent book was not so much to my liking, not because I'm a string theory proponent but for other reasons, too complicated to get into right here.)

You guys, and others, and the weight of a lot of non-evidence, have done a lot to shift the debate from "String theory must be right–it's just too beautiful to be wrong!" (my phrasing) to "Show me the money!" (someone else's phrasing).

Science is not a popularity contest, as many here have noted, but neither is it a "annointed by public acclaim" contest, which string theory was largely the benefit of during a certain period, roughly 1986-2003.

Me, I'm skeptical of any current theories. I expect a theory unifying gravity and the ultrasmall comes only when we can start to (experimentally, or observationally) probe scales a whole lot closer to the Planck scale than anything we'll see in our lifetimes or the lifetimes of our great-great-great-great-great-grandchildren.

I'd love to be wrong on this.

–Tim May

34. **Aaron Bergman**
November 13, 2007

Brian Greene's TV show is worth it if only for the best thing ever written about string theory.

More seriously, I understand the frustration one feels about the popularization of science. But what is the alternative? People are going to be excited about their work. Some of the best people are those who absolutely believe that what they're working on is the right thing to be working on. If we're going to be popularizing science, isn't that excitement what we should be showing. Caveats are great and all that, but who remembers those things in the end? Not only that, but should we start censoring those people who aren't sufficiently cautious in their public statements? Is Smolin OK but Greene not? Kaku but not Maguejio? You? There are going to be books. Many of them are going to make people like, say, me unhappy. But, really, if it gets people interested in science, it probably works out for the best in the end.

35. **Tom Whicker**
November 13, 2007

Peter,
Sorry if I sounded hostile to Brian Greene. Just my poor attempt at humor. The Elegant Universe is a bit personal for me, as I was
asked to host a showing of it to local science teachers. It is rare for such local people to show interest in Physics, and I was excited about the event (I hadn't seen the film at that point).
From their reactions and my lowly viewpoint, the film was just a
waste. I think it helped re-enforce some attitudes in group
along the lines of "why does our tax money go into this kind of stuff?"

36. **Tim May**
November 13, 2007

And I think the views of tax payers, about "why does our tax money go into this kind of stuff?" are valid concerns.

Fifty, or even 40, years ago the dollars spent on new accelerators like the Bevatron or the Brookhaven AGS, or even Fermilab, were not quite so huge as they are today.

And the money spent then, and especially in the decades earlier, had some practical consequences: figuring out how to build the A-bomb, to be blunt. So the public supported it, even if after the fact (secrecy and all).

Around the time of the Superconducting Supercollider, it could no longer be argued that building the world's largest accelerator was in and of itself justified. And so it was cancelled. Much talk about how a generation of theorist would wander in the desert (pun about the energy desert intented).

Look, I'm all for basic science. I would love to see the nature of the universe further explicated. But, let's face it, the cost of each 10x increase in energy (luminosity is another factor) is growing at rates that are unaffordable.

Unaffordable unless the new accelerator is likely to solve the problem of the Germans or the Japanese or the Russians (figuratively speaking, to go back to prior justifications).

No one is suggesting new understandings of energy sources, or of weapons, and there is every suggestion that basic understandings are now coming at, perhaps, 100-1000x increases in energy, not the 10x increases in energy (give or take) so common in the 1930s-1970s.

I sort of was opposed to the Hubble. But having seen what it has shown, along with several other comparably-priced telescopes, I'm now much more in support of these kinds of things.

I hope I'm wrong. I hope the LHC discovers one or more important particles. I fear I won't be wrong. Check back in several years.

And I doubt the taxpayers will authorize a 10 times more expensive generation after the LHC to keep on searching. At least not for a long while.

–Tim May

37. **Tom Whicker**
November 14, 2007

If we're talking about something in the current media/print world
that does popularize Science in a successful way to the average
person, I have to say Mythbusters is it. That show probably has
the right approach to interest some young kids in how science works (or used to work).

38. **Aaron Bergman**
November 14, 2007

I love Mythbusters. Explosions are cool. But there's a lot more to science, too. *The Elegant Universe* has apparently sold over a million copies, so people seem to have been interested.

39. **Tom Whicker**
November 14, 2007

They do get carried away with explosions. But they've done a wide variety of stuff like de-bunking free energy machines, etc. Often there is a nice lesson on thermodynamics, laws of motion, etc. If the general public could just get a tiny grip on basic conservation of energy concepts, it would be a good thing…

40. **zorba**
November 14, 2007

I said: " Do you really want to be a party to the descent of the blogosphere into a place where people's scientific work is exposed to this kind of unprovoked attack? "

I think I have my answer.

Alternatively, if you genuinely can't understand why "Should one perhaps leave this topic to the Bogdanovs? " is deeply insulting, then maybe you need to get away from the blogosphere for a while.

41. **Peter Woit**
November 14, 2007

zorba,

You continue to wilfully misinterpret what I wrote, which very clearly was not an "unprovoked attack" on anyone's scientific work. My only comment about the science was the uncontroversial one that there is no experimental evidence or convincing model for pre-big bang scenarios. My posting was not about science but about the advisability of media appearances by scientists.

42. **Peter Woit**
November 14, 2007

Aaron,

No one is suggesting censoring anyone. But I think it's worthwhile to raise this issue, and encourage people who go to the media to keep it in mind. Not every appearance by a physicist on a radio or TV show is a good thing for science.

43. **Jonathan Vos Post**
November 14, 2007

I basically agree with Tim May's first comment:

"… the drawings of Chesley Bonestell, the fantasies of Willy Ley, Heinlein, Clarke, Asimov, etc. Yeah, science fiction played a big role in my life back then, but I clearly knew that things like the Moebius strip wall in one of Clarke's short stories was just SF, even as it helped motivate me to read Martin Gardner's articles on topology and Moebius strips…"

Seeing the phosphorescent murals of Chesley Bonestell at New York's Hayden Planetarium and Museum of Natural History powerfully motivated me, as did climbing around on the Willammette meteorite.

I saw Willy Ley live, and he was an amazing speaker, as well as writer. Heinlein was a de facto Engineer, who did Defense research, although he always deferred to his 2nd wife Virginia as a better engineer. Sir Arthur C. Clarke did hands-on Electrical Engineering in developing radar for landing planes in foggy England during World War II (as described in his most autobiographical novel, Glide Path). Isaac Asimov was a Professor of Biochemistry.

These great men knew what Science, math, and Engineering were about. They intentionally, didactically, wrote on these subjects in fiction and nonfiction, explicitly to motivate people (especially youth) beyond the ability of most public schools. heinlein's "juveniles" (today called "young adult") made him less money than novels for adults, as his agent kept telling him. But Heinlein explicitly stated that he wanted to push enough young people into science and engineering and math that they would actually create the Space Program he longed for.

Half of all the astronauts and other technical people I worked with for 20 years in the Space program cited Asimov, Clarke, and Heinlein as early motivators. They also cited Bradbury, although they knew his work lacked science content as such.

It's sad that we've lost Asimov and Heinlein. They were, in many ways, irreplaceable. It's wonderful that Bradbury and Clarke are still alive and working. I'm honored to have been in contact, and sometimes coauthorship, coeditorship, cobroadcasting, with all of these.

But "the media" have never been clear on the fuzzy boundary between Science and Science Fiction. What got me on the NBC-TV Today Show, live to 10,000,000 people, was a pitch to contrast what Science and Science Fiction say about the Space Program. It was cool that they let me bring Isaac Asimov on as my "guest of guest."

I strongly agree with Peter Woit that "the media" seem increasingly unclear, increasingly distracted by coloful graphics, decreasingly interested in meaningful content.

Fortunately, we have the World Wide Web.

44. **Belizean**
November 14, 2007

Q. "Is it a good idea for physicists to appear on a radio show discussing what happened before the big bang…?"

A. Yes. As long the the physicists give the correct answer: "We have no idea what happened before the BB and aren't even sure that the question makes sense."

Q, "Is it a good idea for physicists to promote to the public their work on time travel?"

A. Yes. Whether physics permits time travel is a profound conceptual issue irrespective of the negligibly small number of physicists actively considering this question. A responsible popularizer should present the arguments in flavor of time travel (solutions to the Einstein equations featuring high angular momentum densities that result closed time-like curves), and arguments against (paradoxes, destructive feedback loop of virtual particles), AND point out that this question is generally ignored by the overwhelming majority of working theoretical physicists. [In the case of Ron Mallett, it's clear that what he's attempting is *in principle* probably not absurd (given van Stockum/Tipler). The questions is whether he can *in practice* achieve sufficiently high angular momentum densities using current technology.]

Q. "Is it a good idea for physicists to appear on a TV show explaining the forces involved in crushing beer cans, as part of a segment on whether women can crush beer cans with their breasts?"

A. No. Physicists should acquire sufficient media savvy to distinguish between producers genuinely interested in increasing public understanding of physics and those with other agendas.

45. **YBM**
November 14, 2007

Did you notice this ?

http://www4.fnac.com/Shelf/article.aspx?PRID=2062733&OrderInSession=1&Mn=1&SID=902a4926-8ace-0979-afa2-d9686866e4f3&TTL=091120070052&Origin=FnacAff&Ra=-1&To=0&Nu=1&UID=073768ffc-07a4-5f27-f209-6d31e8c5f43c&Fr=0

I didn't know Lubos was writing in french, or that he was about to support the worst cranks of all times (especially given that they are loud opponents of string theory) :

"L'équation Bogdanov"
Author: Lubos Motl
Forewords: Igor Bogdanoff, Grichka Bogdanoff
Publisher Presses De La Renaissance (a very cranky one !)
Date janvier 2008
ISBN 2750903866

46. **woit**
November 14, 2007

YBM,

Thanks for pointing that out. I'll be in Paris for a while in January, maybe can pick up a copy…

Lubos has often defended the Bogdanovs in the past, I suspect mainly on the grounds that John Baez and I were critical of them, so they must be all right. I wonder what the story of this book is, if Lubos even wrote much of it himself (I don't think he writes French, perhaps he had help from the writers of the foreword).

47. **anon.**
November 14, 2007

'Doesn't sound scientific to me. Heh, I'm muscular and buxom, but I don't believe I have such physical capabilities….' – Kea

Aluminium drinks cans are actually fairly easy to crush when empty, even when just using your hand or foot. Just make sure it really is an empty aluminium drinks can, not a filled steel one.

'I'll be in Paris for a while in January, maybe can pick up a copy…' – Peter Woit

Well, I just hope you will publish as fair and honest a review of Lubos' first book on Amazon, as he did of yours…

48. **Tom Whicker**
November 14, 2007

Jonathan,
Great post. Yes , Bonestell was somehow very powerful.
For me as a kid (, there was a series of articles in National Geographic from the Palomar 48 inch Schmidt

49. **King Ray**
November 14, 2007

Let's not forget Ben Bova. With his hard SF he is helping envision future missions to the moon and Mars. Heckuva nice guy too.

50. **Zathras**
November 14, 2007

Wow, 50 posts so far on popularizing science and not yet one word on Carl Sagan. His *Cosmos* was my first exposure to science beyond picture books. I think *Cosmos* has a good mix of proven and speculative science, just enough to inflame many an imagination, and every popular science book since has tried to reach the same balance, with some mixed results.

51. **milkshake**
November 14, 2007

LM position has been that Bogdanovs papers were a serious attempt but a mediocre contribution and that's why these papers went under the radar for some time – nobody was interested.

The Emperor's New Clothes: It's impossible to accept that his field could be ridiculed like this – hence the articles cannot be a gibberish.

52. **Aaron Bergman**
November 14, 2007

They do get carried away with explosions.

Not possible.

53. **vn**
November 14, 2007

Strugatsky brothers (see
wikipedia;
their slogan, Thinking is not entertainment but an obligation!)
are also worthy SF writers, e.g., their Ugly Swans/Rain Time;
this novel looks a bit like a kind of stylization on good old
Bulgarian spy novel (What could be better than the bad weather:).

54. **Steve Myers**
November 15, 2007

I don't know if there's any easy answer to whether pop science is good or not. Clearly it depends on the quality of the work (book, TV, etc.). Yes, I've known guys inspired by Asimov, and others — but I wasn't interested in that SF (for me math is real science fiction — the logical analysis of possibile functions, spaces, worlds.) But I have noticed that very often the interest in science is fed by a concerned adult — a teacher, father, uncles, so on. Feynmann traces his inspiration to his father; Einstein to his father's gift of a magnet. I showed by sons & their friends how to measure the mass of the earth with a pendulum. My 6 yearold granddaughter thinks photosynthesis is "awesome."

55. **LDM**
November 15, 2007

Feynman's enthusiasm for physics was infectious, and was based on a deep understanding of physical law. Transmitting that understanding so the non-scientist realized that they too could understand and appreciate the beauty of physical law is all that is really required.

People who attempt to popularize science by discussing speculative theories or use salacious examples have missed the point. Science is sufficiently interesting on its own to not need embellishment. IF somebody feels they must resort to such methods, then I would suggest it is because they don't really understand their topic deeply enough where they can present it in an interesting way. The solution is to get a deeper knowledge that can be conveyed to the audience, NOT to get more superficial by discussing speculative ideas.

56. **liverpool**
November 15, 2007

check this out: http://www.telegraph.co.uk/earth/main.jhtml?xml=/earth/2007/11/14/scisurf114.xml&CMP=ILC-mostviewedbox
looks like the British press didn't have the same objections to garret lisi's paper as some others…

57. **Mike**
November 16, 2007

In my country, in the way Russia numerous studies, but they spend in the USSR upbringing elderly who could not understand that young people are not very interested in science, such as physics. At this point, all the young people thought would earn as much money and maintain a business.

Sorry for my English

58. **anon.**
November 16, 2007

I agree with drukpa, but case of Dr Garrett Lisi is difficult because it is in a preliminary stage. The most fundamental things to popularise more are *empirical facts behind the models* used for electromagnetic, weak and strong interactions which are summarised in the standard model, and also the few facts empirically confirmed about gravity from successful tests of general relativity (cosmological dark matter and dark energy are not exactly successful predictions of general relativity, but more a case of epicycles which may indicate problems). Too many people think lump this experimentally confirmed stuff with stringy speculations, and can't see the distinction between facts and fantasies in physics.

If publicity gets Dr Lisi a research position, and he uses the opportunity to follow up physical facts and is prepared to abandon (or put on the back burner) ideas that fail, then it is fine.

What would perhaps be less good is if his ideas were to just *replace* mainstream string theory as the next groupthink hep-th subject, make no progress for the next 25 years – just like string – and end up creating a narcissistic pseudoscientific religion which can only defend itself by launching *ad hominem* attacks on the presumed low-IQs of those working on other ideas.

59. **Tom Whicker**
November 16, 2007

Funny how the mis-information about string theory slowly expands into the public relm. The November issue of National Geographic has a nice artice on Hubble images ("New Visions From Hubble"), but near the end of the text, the author is discussing so-called Dark Energy and he states: "….for all we know there are multitudes of as yet undetected particles, each with its own field, implying still more dimensions…….are these dimensions real or..just a handy way of calculating? Increasingly, physicists suspect they are real."

60. **Yatima**
November 16, 2007

"The case of Dr Garrett Lisi is difficult because it is in a preliminary stage."

I will say. It's a sad testament to my mental capabilities that I do not understand anything of it, so I shall keep an eye open for the appearance of "Lisi Bobbing Head" figurines at EntertainmentEarth. That should be a good criterium indicating that a successful new approach to ToE is indeed taking hold – even if it's only (Hegelian) phenomenology (isn't everything?). In 5 years or so.

61. **Assaf**
November 17, 2007

People will be people, and some will do anything to grab attention. The things you describe are of course completely irresponsible. What's even sadder is that there are so many genuinely interesting and well established physics notions and concepts that one has to wonder why some physicists choose to promote such pseudo science.

62. **Peter T. Brown**
November 17, 2007

I think its fine to use tricks to promote science. In this day and age you gotta grab them kids attention!

63. **Anders R**
November 18, 2007

the kids who have any potential are drawn into it anyway. i really think people are overestimating the use of this popularisation. you're probably not going to be able to come up with anything decent until you're 19-20 anyway so why shove half baked theories down the throats of teenagers and kids.

64. **Mathieu Bautista**
November 23, 2007

Hi Mr Woit,

I am a young french engineer passioned for mathematical and physical theories and I recently bought and read your book (in French, "Même pas fausse"). Unfortunately I am not able to do any work neither in string theory or any other physical or mathematical theory because I don't have the technical level to do so.

To use the same form you did, I have three points in response of your post "Popularizing science" :

– Pedagogy is unfortunately considered as the low degree of making science. Scientists don't want to make TV shows because people like you would later use this as an argument to bash their theory (chapter 15 of your book, about "the Bogdanov affair", this is the french sentence : "De nombreux détails croustillants émergèrent sur les frères Bogdanov et leur façon d'obtenir leur thèse. Ils étaient cinquantenaires, avaient été responsables d'une émission de science fiction à la télévision […] et présentaient actuellement une émission composée de courts fragments destinés à apporter des réponses à des questions scientifiques[…]". I will try to traduce it for non french speakers (sorry the this poor traduction) "Numerous crumbling details appeared about the Bogdanov […] they had presented a science fiction show […] and actually a show that tries to provide answers on scientific questions"…So the Bogdanov can only "try" to provide scientific answers…

– That's my second point ; why such a hatred about the Bogdanov, their TV shows and their theory ? In this chapter (the 15) you focus on the form and don't even try to present the content of this theory (you only give a short extract of their thesis deliberation). Have you read their books ? Have you attended just one of their TV shows ? The fact is that their theory IS undoubtedly a scientific theory (or else, it wouldn't be discussed whether it's true or false). I won't discuss this theory here, but after reading some string theory books for non specialists (like "Supercordes et autres ficelles", by Carlos Calle) and the book of the Bogdanov ("Avant le big bang") I can't tell whether the string theory is "more serious". The fact is that they are more people trusting and working on the string theory. But that doesn't prove anything, think about Albert Einstein : "If i was wrong, only one [scientist] would have made it" (sorry if this sentence is not exact, i only got the french version in mind)

– And the last point; you are a teacher in mathematics, specialized in quantum theory. But you say in your book that you are not an expert in quantic algebra (which i suppose involves the quantic groups theory) The problem is Grishka's thesis is based on these tools and it is the justification of the "Bogdanov theory" (I think about the 3.3.2 theorem). Of course, I don't have the pretention to understand this thesis better than you, but if you are so convinced this is all a fake, you should be able to demonstrate it, shouldn't you ? If that's not the case, then you shouldn't pretend that this theory is "laughable" ("risible" in french), this is not a scientific behaviour. Remember what happened to the "Big bang" of Fred Hoyle not sixty years ago ?

I have not read your book entirely yet, but i will very soon. I have a deep respect and admiration for people like you who popularize science. That's why I defended the Bogdanov a little, because they popularize science in France since 1980 (not only science fiction) and – like you – that doesn't mean they can't have great ideas and theories about cosmology or whatever.

65. **woit**
November 23, 2007

Hi Mathieu,

About the discussion of the Bogdanov's TV show. What I actually wrote is that "they now have a new show of short segments in which they answer questions about science". No "try" there. I think you're reading something into the French translation which is not there. I had no intention of criticizing their TV show, partly because I hadn't seen it, just descriptions of it, and the descriptions I had seen were not critical of it.

In general I should warn people that I didn't see the French translation before it was published (and I'm actually slightly annoyed about this, since I do read French perfectly well and had asked for a chance to look over the translation). But as far as I know the translation is fine, I wouldn't have noticed anything remarkable about the part you quote.

As for the problems with the Bogdanov's research work, that topic has been rehashed many times by me and others. No, I am not an expert on quantum groups, although I know something about them. The technical mathematical material on these groups in Grishka's thesis is not the problem, the problem is the way this is used to claim to have a model of before the big bang based on topological quantum field theory, and topological quantum field theory is something I know quite a lot about.

To say something positive about the Bogdanovs, the nonsense level among mainstream theoretical physicists in this area has increased significantly in the 5 years since all this happened, so that the difference between their nonsense and the nonsense of other people is not as dramatic as it was back then. Maybe they were just ahead of their time….

66. **YBM**
November 23, 2007

Here is the "promotional" abstact of Motl's book :

L'EQUATION BOGDANOV, Le secret de l'origine de l'Univers ?
Un chercheur de Harvard, le Pr Lubos Motl, répond à cette question.
Est-ce que deux personnalités de la télévision peuvent prétendre résoudre l'une des questions les plus ardues de la physique moderne ? Est-il raisonnable de penser qu'elles puissent trouver des solutions aux problèmes effroyablement compliqués qui concernent l'origine de l'Univers et sur lesquels, depuis des décennies, échouent les savants du monde entier ?

Tel est le surprenant défi de cet ouvrage : montrer que la recherche et ses extraordinaires découvertes empruntent, parfois, les chemins les plus inattendus. Et surtout de nous apprendre qu'Igor et Grichka Bogdanov ont peut-être réussi, à force d'acharnement et de passion, à lever un coin du voile qui entoure l'une des questions les plus fascinantes de la cosmologie moderne :
celle du commencement du temps, de l'espace et de la matière. Tout le monde sait que les deux jumeaux sont des passionnés de l'espace et de l'Univers. Mais sait-on qu'en 2002 ils ont déclenché une tempête dans le monde de la recherche en publiant six articles sur l'origine de l'Univers ?
Des articles parus dans les meilleures revues de physique théorique, notamment la prestigieuse Annals of Physics dont les experts ont conclu que les Bogdanov avaient apporté des solutions nouvelles aux problèmes de l'origine du temps et de l'espace en dessous de l'échelle de Planck,
avant le big bang.

Dans les laboratoires, les discussions s'enflamment. Les Bogdanov sont-ils de véritables chercheurs ou bien s'agit-il d'un canular ? Pour la première fois un expert, ancien professeur à l'Université de Harvard, analyse en profondeur leurs travaux. Et il répond à cette question : Quel est le contenu des recherches des Bogdanov ? Après avoir lu attentivement leurs travaux, le Pr Motl a fini par conclure que les Bogdanov proposent rien de moins qu'une théorie alternative à la gravité quantique.

Google Translation :

THE EQUATION BOGDANOV, The secret of the origin of the universe?
Un chercheur de Harvard, le Pr Lubos Motl, répond à cette question. A researcher from Harvard, Professor Lubos Motl, responded to the question.
Est-ce que deux personnalités de la télévision peuvent prétendre résoudre l'une des questions les plus ardues de la physique moderne ? Are two television personalities can pretend to solve one of the toughest issues of modern physics? Est-il raisonnable de penser qu'elles puissent trouver des solutions aux problèmes effroyablement compliqués qui concernent l'origine de l'Univers et sur lesquels, depuis des décennies, échouent les savants du monde entier ? Is it reasonable to assume that they can find solutions to problems frighteningly complicated concerning the origin of the universe and where, for decades, scientists fail the world?

Tel est le surprenant défi de cet ouvrage : montrer que la recherche et ses extraordinaires découvertes empruntent, parfois, les chemins les plus inattendus. That is the surprising challenge of this book: to show that research and its extraordinary discoveries borrow, sometimes, the most unexpected ways. Et surtout de nous apprendre qu'Igor et Grichka Bogdanov ont peut-être réussi, à force d'acharnement et de passion, à lever un coin du voile qui entoure l'une des questions les plus fascinantes de la cosmologie moderne : And above all we learn qu'Igor and Grichka Bogdanov may have succeeded, by dint of hard work and passion, to raise a corner of the veil that surrounds one of the most fascinating questions of modern cosmology:
celle du commencement du temps, de l'espace et de la matière. As the beginning of time, space and matter. Tout le monde sait que les deux jumeaux sont des passionnés de l'espace et de l'Univers. Everyone knows that the twins have a passion for space and the universe. Mais sait-on qu'en 2002 ils ont déclenché une tempête dans le

monde de la recherche en publiant six articles sur l'origine de l'Univers ? But do we know that in 2002 they triggered a storm in the research world by publishing six articles on the origin of the universe?
Des articles parus dans les meilleures revues de physique théorique, notamment la prestigieuse Annals of Physics dont les experts ont conclu que les Bogdanov avaient apporté des solutions nouvelles aux problèmes de l'origine du temps et de l'espace en dessous de l'échelle de Planck, Articles published in the best journals of theoretical physics, especially the prestigious Annals of Physics, which the experts concluded that Bogdanov had provided new solutions to the problems of the origin of time and space below the level Planck,
avant le big bang. Before the big bang.

Dans les laboratoires, les discussions s'enflamment. In laboratories, ignite discussions. Les Bogdanov sont-ils de véritables chercheurs ou bien s'agit-il d'un canular ? Bogdanov The researchers are real or is it a hoax? Pour la première fois un expert, ancien professeur à l'Université de Harvard, analyse en profondeur leurs travaux. For the first time an expert, former professor at Harvard University, an in depth analysis of their work. Et il répond à cette question : Quel est le contenu des recherches des Bogdanov ? And he answered this question: What do the research Bogdanov? Après avoir lu attentivement leurs travaux, le Pr Motl a fini par conclure que les Bogdanov proposent rien de moins qu'une théorie alternative à la gravité quantique. After carefully read their work, Professor Motl eventually conclude that Bogdanov offer nothing less than alternative theory to quantum gravity.

67. **How Can We Turn The Tide?**
November 24, 2007

Peter,

Other than Garrett's non-theory receiving far more media coverage from the press, what is the difference between the Bogdonav affair and the Garrett Lisi affair?

Are not-even-wrong theories endorsed by Smolin et al what we have to look forward to for the next thirty years?

68. **Peter Woit**
November 24, 2007

How can we…

The Bogdanov's papers are gibberish, they don't make any sensible, comprehensible claims that can be accurately stated. Garrett's paper is straightforward, conventional theoretical work. His claims are clearly made, so clearly that some people have been able to identify specific problems with them, which Garrett has acknowledged.

69. **Tim May**
November 25, 2007

And even "New Scientist" could not find anything in the Bogdanov paper(s) to put on the cover. Which suggests the Bogdanov kind of gibberish was in a different league…

To defend "New Scientist" a little bit–since I've dumped on them as several others here have as well–I recollect other cover stories that were just about as "out there" as the recent examples. For example, I recollect at least several covers having to do with "the universe as a hologram."

Now I know that the hologram model (or conjecture) is not gibberish in the same way the Bogdanov papers appear to be, and that 't Hooft, Susskind, Maldecina, and the others are respected theorists. The hologram model probably sells magazines, too.

(And anything with a bizarre name, or a picture of a guy with a goofy grin on a bicycle or in a wheelchair or cracking safes at Los Alamos, this kind of "human interest" angle has been making cultural icons out of physicists for a long time. Snowboarding hippies just goes with the territory.)

So, cover stories for the holographic universe conjecture. Is it "not even wrong"? Does it have testable predictions? (My guess is no, at least not for things we'll be able to see in the next few centuries, but maybe I'm wrong.)

Is "New Scientist" wrong in putting these kinds of "wild conjectures" on the cover? (I could add a bunch of others, including theories of how all mathematical structures exist (Max Tegmark), how algorithmic information theory underlies all of physics (Greg Chaitin, though I'm oversimplifying his point here), how the universe may have just "fluctuated" into existence (too many names to list here), or even how vibrating strings and branes and whatnot may underly reality (!).

I think it's not so wrong for a popularly-oriented magazine to put things on the cover that hook some readers, even that generate some controversy. Provided the science is not truly bad, like the French brothers, or something like "New Support for Flat Earth Theory" or "Darwin Refuted!," which would be a whole different kettle of fish.

But stuff about possible imprints of other universes is, while very speculative, not actually misleading anyone. I recall having the same reaction to the idea that the microwave background map (WMAP) might have its bumpy structure determined by quantum-mechanical fluctuations in the first handful of Planck time periods to be pretty darned bizarre…but now it seems the accepted model. I don't know if NS ever had a cover story with a picture of the WMAP sky with a catchy title like "Did the Uncertainty Principle Shape our Universe?," but that would be unsurprising to me. Was it hype? Was it "Not Even Wrong"?

It seems to me that if people approach "New Scientist" and similar magazines (even "Scientific America" puts this kind of stuff into cover articles these days) with a certain amount of skepticism and views the conjectures as just speculative approaches, not too much harm is done. I even think kids (teens) who get exposed to this stuff may actually become interested in science, and will learn pretty quickly how to move beyond the hype and speculation….I know it worked for me in the 1960s, when I was able to get beyond reading about, as one example, tachyons (Feinberg, a popular cover story back in the 60s, in places like "Science Digest") and read what the underlying physics was about. (Feinberg, by the way, was no crackpot….but science journalists sure did hype up his work. Some foreshadowing of today, I think, but the faster speed of the Internet, blogs, SlashDot, etc., makes things move at _tachyonic_ speeds.

It might be nice if "New Scientist" were to have sidebars that suggest healthy skepticism, sort of like the guys who stood behind conquering Roman heroes and emperors and whispered to them that they are not gods.

But for now I guess I'm prepared to cut NS a lot of slack. We live in a time of a lot of hype, a lot of selling of stories, and way too many thousands of magazines.

Too long a comment here…sorry. But I think that what Peter Woit and Lee Smolin and others have been doing is to inject some healthy skepticism back into the mix. And part of this healthy skepticism may involve some wild speculations of other kinds, things which help to "deconstruct" the idea that string theory is the only game in town.

–Tim

Project X and Flavor Physics

November 18, 2007
Categories: Experimental HEP News

Last week Fermilab hosted two workshops on the so-called Project X proposal for building a linac designed to produce a high-intensity proton beam. The first workshop dealt with issues surrounding the proposed accelerator itself, the second with the physics that it might be able to investigate. Project X is being discussed in the context of an increasing realization that prospects for the ILC getting approved and built anytime soon are slim, so the US particle physics community in general, and Fermilab in particular, need to have a viable plan B for what they will be doing during the next decade. DOE secretary Ohrbach, in a recent talk at Fermilab made it clear that he thinks the ILC project is still at the stage of an R and D project, not yet near the point where a decision about it can be made and a full engineering design developed. For commentary about this from Barry Barish, director of the ILC project, see here.

One argument for Project X is that it would help develop some of the linac technology needed for the ILC, but the main arguments for the machine revolve around a striking change of direction for US particle physics, from the use of colliders to do experiments at the energy frontier to fixed-target physics at lower-energies. In some ways this would be a return to the older style of particle physics experiments that was the norm before the era of colliders. The point of Project X would be to produce a beam capable of being used to generate more intense beams of neutrinos that could contribute to neutrino physics, and to do what is now often called “flavor physics”. This is the study of phenomena involving heavy quarks and/or rare decays, with the hope of seeing beyond the standard model effects that occur not in lowest order approximation, but in higher order contributions to decay rates. There are quite a few decays that one can look for that either can’t occur at all in the standard model, or only can occur at unobservably small rates. An observation of such a decay and measurement of its rate would provide evidence of new physics. Many such studies already conducted provide strong bounds on quite a few possibilities, so one can imagine competing with colliders such as the LHC to either rule out or find new TeV-scale physics by doing this sort of experiment.

One interesting document to read about this is the account of a panel discussion on charm physics that occurred this past August. A participant emphasized how history has recently been running against the people working on flavor physics, telling the following story:

> … over lunch we were talking about the future of the field, and I was drifting off, and ended up in a fantasy world where things were done the right way. And in this world the LHC was in fact built and came on the air, and found the Higgs, and found many new events that we couldn’t explain with the Standard Model. And people had realised that in order to interpret these possible signals of new physics, we would also have to have flavour physics studies of rare phenomena, so that we could start to see patterns emerging… and working symbiotically together, the LHC and the flavor sector would get to the root of what was happening, something that would be very difficult if not impossible to do with the LHC alone.
>
> But then I woke up. And I thought about a colloquium I’d given recently, where one of the chief experimentalists there took me into his office and shut the door and said to my face, “Flavor physics is dead!” and apparently he’s not the only one who said it: some pretty important people have said it. And when something like that is said over and over it begins to have a truth of itself.

Deciding whether Project X makes sense will require figuring out exactly what kinds of experimental results it will make possible that would not be possible using existing or currently planned facilities. For more about this, see the introductory and wrap-up talks by Joe Lykken and a talk by Jon Bagger that summarizes the issues well. The workshop also featured an excellent talk by Michelangelo Mangano summarizing the current situation of particle physics, emphasizing what it might be possible to learn through other means than the LHC, which is what is getting almost all the attention these days. He pointed to the activities of the CERN Working Group on the Interplay Between Collider and Flavour Physics that are documented at this web-site.

Update: Alexey Petrov was at the Project X workshop, and has a very interesting posting about it.

Comments

1. **Flip**
   November 18, 2007

   Thanks for pointing us to the workshop PW!

   A quick note: streaming video of most of the talks are available at the Fermilab VMS site: http://www-visualmedia.fnal.gov/VMS_Site_2/

   Click on “streaming video archive” and search under series: “High Intensity Proton Source.”

   Cheers,
   F

2. **Peter Woit**
   November 18, 2007

   Thanks Flip!

   Personally I can’t stand watching videos on the computer, and only will do this if there are no slides available, or if I can’t figure out from the slides what the speaker was talking about. Maybe this is a generational difference, or maybe I just have the short attention span young people are always accused of having…

3. **Dave Miller**
   November 19, 2007

   Peter,

   In a time when hundreds of billions are going down the tube in Iraq, it’s easy for us physicists to think that of course we should be given a few billion (or is it a few dozen billion?) to build the next accelerator.

   But this is real money — it could be used to build a lot of libraries or hospitals (or pay for a lot of mathematicians or theoretical physicists or a few new football stadiums or…). Is there any credible case to spend it on a new accelerator?

   My Ph.D. is in particle theory from Stanford (SLAC), and I actually knew Barry Barish when I was an undergrad at Caltech, but I have since moved into other fields. Even from my perspective as a former insider, I am only mildly curious about the results that will be coming out of the LHC (curious enough to follow your blog, of course – thanks for all the effort you put in). Why should we expect our fellow taxpayers to fund a machine, the results from which do not interest most people and indeed which most people cannot even understand?

You have a rather unique view and position in the field of physics. Looked at objectively, can you think of any reason our fellow taxpayers should fund Project X – except of course to keep physicists employed?

All the best,

Dave

4. **chris**
November 19, 2007

hi dave,

"can you think of any reason our fellow taxpayers should fund Project X – except of course to keep physicists employed?"

when i hear this kind of argument, it always reminds me of that famous story about faraday. when asked by the king what good his novelty experiments on electricity were he responded by saying, that one day there will be taxes on it.

seemed pretty outlandish then, right? seems pretty outlandish now to hope for anything relevant to be discovered at a new accelerator.

5. **Roger**
November 19, 2007

Dave has a point. There is a feeling here that this is something just to keep people employed. The physics involved in project X is reasonably interesting but, in the absence of discoveries of non-Standard Model physics at the LHC, may just be more of the same fare we've been served over the past few decades.

This is an issue not only for Fermilab, but concerns the whole field of experimental particle physics. The most recent advances in fundamental physics have come from the astrophysics experiments – is this a trend for the future ?

If the LHC provides no surprises I can see good not not compelling arguments for further investment in large scale collider experiments.

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This is an issue not only for Fermilab, but concerns the whole field of experimental particle physics. The most recent advances in fundamental physics have come from the astrophysics experiments – is this a trend for the future ? If the LHC provides no surprises I can see good not not compelling arguments for further investment in large scale collider experiments.

7. **chris**
November 19, 2007

roger,

i think what we have seen over the past decades is the other fields catching up while hep-ex is – comparatively – starving. until basically the 80s politicians funded this field because of the perceived relevance for nuclear devices of whatever sort. now that it is clear that the results are not anymore directly relevant and biotech is the current cool technology, funds have decreased accordingly.

i am fully in favor of any alternative way to learn about fundamental physics and indeed the astrophysics results of the last decade are impressive. but ultimately, if you want to know the physics at a certain scale, you have to go there somehow. and that costs and therefore it is slow. but i do not see an alternative. except of course if we just decide that we don't care at all.

8. **Dave Miller**
November 19, 2007

Chris,

Of course, I've always loved that anecdote from Faraday (what physicist doesn't?).

But I'm skeptical that it is relevant any longer. Elementary particle/ high-energy physics has been around for a long time now. But no practical result has ever come from it, nor is there any on the horizon. And there seems to be an obvious reason for this — charmonium, the W/Z particles, the higher baryon resonances, the tau lepton, etc. are so massive and so unstable that it is hard to see what role they can ever be made to serve in everyday life.

Of course, it is always possible that when Lubos finds the complete solution to superstring theory that this will lead to countertop antigraivty and baryon non-conservation (which would provide an unlimited energy source).

But in all honesty, can you envision in your wildest dreams any practical result from particle physics in the next century? Faraday could (and did) accurately envision such results from his work, as the famous anecdote shows. I've been thinking about this (occasionally!) for more than thirty years since I was an undergrad — I still remember George Zweig's idea of using muons to catalyze fusion — and I cannot think of any plausible practical consequences.

Do you honestly think high-energy physics will ever "pay off" in practical economic terms (not counting as a physicists' full-employment program)? Practical results are not the only reason for science, of course, and we all have a perfect right to pursue our hobbies and interests on our own time and money. But is there any honest reason to urge our fellow taxpayers who lack our interests to spend tens of billions on this?

Dave

9. **Peter Orland**
November 19, 2007

Dave Miller (who I used to know a bit when I'd visit SLAC during the summers) raises an important issue. I think this is the central reason why physicists should popularize their work with as little dishonesty and hype as possible.

I think we can get the public interested in what we do thereby persuading them that funding experiments is valuable. It's important, though, that they not be lied to in the process, since they will not readily forgive us. NOTE: The space station and the space shuttle, devoid of scientific or technological value, can get away with such dishonesty – they have much larger budgets and a military connection.

10. **Dave Miller**
November 19, 2007

Peter Orland,

One of the main virtues of our host (Peter Woit) is that he does urge such honesty.

I remember when I was a grad student at SLAC that it was sort of a joke (openly discussed among faculty) that the only reason we were funded is that the feds were hopeful/afraid that we'd come up with another weapon like the nuclear bomb. Everyone laughed about it because we knew it was unlikely, but there was a consensus that no one should let the funding sources in on the joke.

Years later, a friend who had ties with the defense establishment told me that the feds weren't that dumb but that they did view the HEP program as a good technical training program for people who would end up working on nuclear weapons, SDI, and more conventional military programs (which happened to a lot of my classmates).

While I do endorse complete honesty, I do suspect that we may find that our fellow citizens care a great deal more about Monday Night Football and Paris Hilton than about science.

Dave

11. **Roger**
November 19, 2007

Chris – I totally agree. Progress will best be made with collisions at the energy scale one wishes to investigate. However, the problem will arise if the LHC does nothing other than confirm that the SM is a great low energy effective theorem. In this situation, there is no clear guidance as to which energy scale one would need to probe to make progress. Rare decays are of course sensitive to physics occuring at high mass scales but, with the exception of the tenuous g-2_muon result, there is so far little to suggest anything exciting so far. To carry on trying in this area is a good but not compelling argument for further large scale investment.

I'm a collider physicist so my career is tied up in all of this. However, should the LHC not do anything exciting I would be tempted to change fields and move in an astrophysics direction where one is more likely to make progress.

Accelerators are a comparatively recent technique and it may be that they've had their day. It may also be that in a few years time we'll be discussing some of the greatest discoveries mankind has ever made.

Slightly off-topic here and I hope Peter won't delete this but I'm a little gloomy that the latter possibility will happen given that the "compelling" argument for new physics at the LHC is the hierarchy problem. I've never been convinced by this, possibly because I don't appreciate it fully. Will someone explain to me why the hierarchy problem seems to have been elevated to "big problem" status and why it should be taken seriously ? I'm happy with the mathematics of it but it seems to rest of so many assumptions. To me, the strong CP problem or electric charge quantisation are "bigger" problems.

12. **DB**
November 19, 2007

This plan B sounds sensible. Whatever turns up at the LHC it is likely to be the Saturn V of our time. The LHC was sold on its ability to discover the Higgs, if it exists, whilst leveraging existing accelerator assets. Although it had a great case it was really touch and go on a number of occasions during the budget approval process. As things stands, neither the ILC nor CLIC can boast a similar clear-cut rationale and without it they will never see the light of day. Of course, the LHC may yet provide that rationale. However, even if the LHC astounds us, I don't believe politicians have the appetite for yet more mega-budget physics.
As we have seen in recent years with Kamiokande and Sudbury and now Auger, the detection – be it terrestrial or space-based – of astrophysical sources of HEP particles will continue to see lots of action whatever turns up at the LHC. Solid, low-hype, reasonable-cost physics which delivers in spades.
(Well it used to be low-hype, but recently some idiots have begun to refer to UHECR's (ultra high-energy cosmic rays) as "oh my God particles")

13. **woit**
November 19, 2007

Dave,

I believe one motivation for "Project X" is that it's a lot less expensive than something like the LHC/ILC, so can probably be built within the current level of US HEP funding. So, particle physicists don't need to ask the public for increased funding, just a continuation of the current level. I happen to think that's not hard to justify: it's a very small fraction of government spending, has spinoffs, a small chance of dramatic breakthroughs that are technologically useful, but, most importantly, I think the effort to understand nature in this fundamental way is a worthwhile thing for humanity to be putting some of its resources into. While most people are not well-informed about particle physics, my experience is that most of them put it in a category of interesting things they wish they knew more about, and have no problem with a few dollars/year of their taxes being devoted to this pursuit.

The question about "Project X" I think is not whether it's affordable, but whether the possible physics results it can achieve are worth it, in comparison with other ways of spending the money. That's the question this workshop was devoted to looking at.

14. **Adud**
November 19, 2007

PW:

> One argument for Project X is that it would help develop some of the linac technology needed for the LHC, but the

I guess this should be "ILC"? Great post!

DM:

> But no practical result has ever come from it, nor is there any on the horizon.

I guess one reason why many countries are funding this kind of experiments, such as the LHC, is that lots of practical results are coming out from them. Not in the form of making a cure for cancer from the Higgs boson, of course, but as related technology developments. This blog is one among many, and probably the least important, such developments.

Even more relevant, today low-energy particle accelerators are being used (and commercially built) for a myriad of important uses: as sources of synchrotron light and low-energy electron, nucleon and ion beams for research in materials science, nuclear physics, biophysics, biotechnology and medicine. One of the hospitals in the city where I live is seriously considering buying one for cancer treatments. I have no idea what kind of accelerator or what kind of treatments this is about.

Yet, current commercial and non-commercial low-energy accelerators where one day developed as the bleeding-edge laboratories of particle physics. In a few decades the LHC might well be considered a low-energy machine for applied science. I'm sure more concrete examples can be given, maybe someone else knows what DESY is going to be used for now that HERA has been decommissioned. Germans are certainly not going to just demolish it……..

15. **A quantum diaries survivor**
    November 19, 2007

    What's wrong with oh my god particles ? I find it quite funny, unconventional, witty, and pulling the leg of Higgs nicknames aficionados. I do not think we should be concerned with over- or under-hyping our experiments. Catch-words are useful in that they lower the barrier with non-scientists and give some humorous side to things. How are we going to take the public on our side of the funding battle if we show disdain for popularization and trivialization of science ? Science is not ours, it is everybody's.

    T.

16. **nn**
    November 19, 2007

    Chris,

    If your arguments about practical usefulness of science are correct where would that leave astronomy beyond the solar system? (I'm excluding the solar system because e.g. watching NEOs clearly
    has a practical purpose — just ask the dinosaurs)

    But it is arguably doubtful there will ever be any practical application
    of astronomy at a larger scale or cosmology.

    And some of the astronomy projects (like Hubble or Webb or the
    larger earth based telescopes) were/are also quite expensive in
    LHC scale.

    But I still find the results very interesting (not being an astronomer
    myself) and would be sad if it wasn't funded anymore.

    One advantage the astronomers have over the particle physicists
    is that their results are generally easier to understand for
    the layman. Perhaps that is something physicists need to work
    on.

17. **Markk**
    November 19, 2007

    Speaking as one of those taxpayers, not a professional academic, the reason to fund things like High energy physics are twofold.

    First and most important is that these are things should be done in a society in which I am proud to belong. As a society, curiosity about our universe and seeking of knowledge about it, are part of the definition of being healthy. When we lose that, and stop spending the pittance we do on things like HEP, we are sick and not long to remain as a coherent society. This is the classic point that everyone from Hardy to Lederman and who knows how many others have made. We are not spending NIH style money on high energy physics even if we would have built the SSC. You can make a point about funding levels, but that is a whole different administrative point, not an issue with goals.

    Secondly, as a practical matter, as an engineer, and person who used to hire engineers, I think that we need to keep a bank of knowledge of people who are doing research at the frontiers and can reproduce old research. Somebody who people can turn to for that little bit deeper knowledge and experience, when they have to.

    As an example, lets say a SM Higgs is found and nothing else and no signs of anything else at LHC. Do we turn off all the colliders then? If so, then why don't we stop teaching theoretical high energy physics – there is no point, we for sure then could never do anything with it and by the same reasoning it should go also. We lose the knowledge in peoples minds then. It might be in books, but that isn't online in heads. Now suppose some odd results come in from, say materials, that indicate long lived particles or other odd behavior. We'll have people from other areas that can jump in the research but there will be a gap, and people might not even notice it really, because there will be nobody with the theoretical background and nobody with the practical background to understand what is going on. I would say we'd end up spending a lot more money, and a lot more time at that point rebuilding the knowledge we would have had, if we even understand what we have in the first place.

18. **Peter Woit**
    November 19, 2007

    Adud,

    Thanks! Fixed.

19. **chris**
    November 19, 2007

    hi nn,

    also to that i can only answer by going into history. i think it was around 1850, when in a prominent textbook on astronomy the composition of distant stars was listed as one of the things that can never possibly be accessible to humans.

    my only answer is, who knows. how can you be so sure that what you find out there will not at one point in the future change our daily life.
    just to give a particularly bad and uninspired example, how do you know that watching supanovae going off in distant galaxies in the full spectrum will not be a main tourist attraction of the 54th century?

20. **chris**
    November 19, 2007

dear dave miller,

thanks for your long reply. i have only spend the last 1.5 decades in hep, so i grew up during the most boring of all times so to say. and yes, i do have similar doubts that results of any practical relevance in the mid-term future will come out of hep soon. and a third yes, i am also in it for the knowledge and not the technology, which is of course the motivation for almost every person doing hep i guess. but still. we are scratching at the frontier. per definition we don't know what comes next. there are people who claim that at 1 TeV you start leaving our brane and go into the bulk of the universe. and this is just the result of wild speculations while idling and waiting for nature to tell us what is there really.

i can't imagine any possible practical use of HEP within the next century except the spin-offs that i don't really want to count. but again, if i knew what to expect, i wouldn't do research. and after all, the periods in physics where everything seemd to be settled until now were followed by the most dramatic changes always. of course, one should not rely on this. but it still is possible i think.

21. **Low Math, Meekly Interacting**
November 19, 2007

I may be unusually interested compared to the average non-physicist, but this taxpayer, at least, is willing and driven to vote for politicians who promise (however obliquely) to give experimental HEP in the US a good financial boost.

If the theorists in the field are victims of the success of the S.M., they must at least be equally victims of having to wait about 20 years too long for TeV-scale accelerators.

That said, although I found Dr. Robert Wilson's eloquent statements in defense of mere discovery both inspiring and moving, nothing seems to persuade the legislator and executive quite like fear. It's hard to imagine the Apollo program without the Cold War. "We're losing our technological edge to Europe, including the FRENCH!", or the prospect of having to outsource nuclear research to China, might succeed where appeals to such frivolities as human curiosity may fail. The mere thought that China seeks to land a robot on the Moon seems to be enough to motivate our leaders to waste (in my opinion) $100 billion on human flights to Mars, so there's a lesson to be learned, however sordid and cynical it may be.

22. **nn**
November 19, 2007

chris,

I think you're misunderstanding me. I wasn't arguing against
funding astronomy. Just pointing out that if D.M.s efficiency/cost
metric would be strictly applied astronomy would likely suffer too.
And some other very interesting, but most likely also relatively
useless areas of science (although admittedly most are not
as costly as HEP or high-end astronomy). But it's not a pure HEP issue; more a general science problem.

Also there might be always uses for everything of course,
although they are hard to predict.

Standard example is number theory — which used to be considered
100% useless — makes public key cryptography and
the internet go around these days. But then the number
theorists also never needed any billion dollar toys to play
around with.

23. **srp**
November 19, 2007

I will once more shout down the well here.

The bottleneck is current accelerator technology. If a substantial fraction of the resources proposed for ILC or Project X were allocated to basic technology development of wakefield accelerators, could they be deployed in a reasonable time frame? If the likely answer is yes, then that strikes me as the no-brainer strategy, since it allows much higher energies at affordable prices (and device sizes). Experimental particle physics is rapidly being painted into a corner; it needs to innovate a way to smash through the walls.

As a taxpayer, I would be much happier paying for breakthrough accelerator research than for low-payoff conservative designs. If the physics community got behind this strategy they could appeal to a) the scientific superiority of ultra-high energy at affordable prices, b) the American love of invention, and c) the possibility of spinoff technology.

Of course, if there were a sound argument that wakefield technology is scientifically unsound or technologically too hard, then this strategy would be a poor one. Yet I have found no such statement, nor, curiously, much interest in assessing whether this technology is truly promising. It's like going for brain surgery before finding out if you can cure your headache by taking some aspirin. No commitments to any future accelerator proposals should be made until that wakefield assessment is made.

24. **Peter Woit**
November 19, 2007

srp,

It's my impression that the sort of acceleration technology you're talking about is nowhere near the development stage, but requires a lot more research to even see whether it is really feasible. There's a good argument for putting more resources into that research, but it's a very different thing, on a different scale in terms of money (less) and time (more) than the ILC or Project X. In those cases the technology is in place now, and the question is whether to spend the large sums needed to build a machine sometime soon (years, not decades).

25. **Dave Miller**
November 19, 2007

Peter,

Thanks for clarifying the cost issue on Project X.

Chris,

I think you're thinking of Auguste Comte, the philosopher and founder of positivism:

>We can never know what the stars are made of, Comte gloomily concluded in 1835:

>>On the subject of stars, all investigations which are not ultimately reducible to simple visual observations are…necessarily denied to us… We shall never be able by any means to study their chemical composition. (http://www.astrosociety.org/pubs/mercury/33_05/rainbows.html)

Markk,

Having moved from academic HEP to emgineering work in industry, I'm a bit skeptical of the "keep a bank of people with knowledge around argument." No one is suggesting that theorists be prevented from pursuing and publishing work in theory or that people like Peter Woit and I be liquidated because we still possess some knowledge of HEP. Assuming that we can continue to live in an affluent society with freedom of speech, etc., there will be people who continue to know and learn this stuff.

The issue is simply why our fellow taxpayers should pay big bucks for bleeding-edge experiments. And, on that issue, srp's point makes a good deal of sense to me. Do HEP experiments have to be this expensive? I understand the underlying physical reasons for the big accelerators, but do we also have a bit of a "NASA problem" here? Other countries (and now some private companies) have shown that you can put stuff in orbit a lot cheaper than NASA used to charge.

Is it possible that srp is right and that we have gone for the immediate gratification of the big colliders and had too little deferred gratification aimed at accelerator technology development?

Dave

———

26. **djm**
November 21, 2007

srp,

There are many other "high risk, high payoff" technologies in accelerator R&D than just wakefield, and even more low risk, lower payoff, some of which is being funded at a modest level through DOE, ILC, etc. It is a topic that could certainly be funded better. There is a distinct sense from ILC meetings I've attended that they don't want to talk about possible accelerator technology breakthroughs, because it could interfere with selling the current design. IOW, if a breakthrough may be 5 years out (which it always could be), why not wait for that and redesign?

Current technology is impressive but has a distinctly brute-force flavor, which does not appeal to those of us who have made clever technology work in other contexts (e.g. at lower energies).

27. **djm**
November 21, 2007

Dave Miller: "Other countries (and now some private companies) have shown that you can put stuff in orbit a lot cheaper than NASA used to charge."

Every successful orbit to date has been entirely or largely subsidized by a large government with enormous resources. Yes, it is cheaper to launch from Kazakhstan than Florida.

28. **srp**
November 21, 2007

djm: I just mentioned wakefield because I've heard of it. If other stuff looks like it might be better, then resources can be put into parallel projects. An intelligent research program in accelerator technology development obviously should allocate resources to multiple paths until one emerges as the best option.

The behavior you describe in ILC meetings, where people don't want the possiblility of breakthroughs to deter investments in the here and now is pretty common in all large organizations. It's even rational in many cases. The current situation is not one of those cases.

Given the current context–a dead end for conventional accelerator technology with marginal rationales being cooked up just to keep the game going after the LHC–it would make sense for the physics community to get behind advanced accelerator technology development the way it now gets behind conventional behemoths like the ILC or Project X. This is one of those rare situations where investing in the cool new cutting-edge stuff is more fiscally, politically, and scientifically responsible than sticking with the tried and true.

29. **wb**
November 21, 2007

srp,

The real promise of wakefield accelerators is in the range of 1 – 100 GeV where light sources can become university laboratory tools and where multi-spectral beams can be used to probe matter with femtosecond simultaneity. Once one is honest with oneself about building high energy colliders with such objects you do a calculation of the electrical power required for such devices. For all the charm about so-called compact size the power requirements come from very simple calculations of luminosity in terms of beam power. As a starting point note that at L=10^33 an ILC requires ~350 MW. Scaling to where a post LHC collider should really be (1.5 TeV in the c.m.), one see that a wall plug power of ~3 GW will be required. Be an optimist and imagine that lasers are 100% wall plug efficient and that wakefield accelerators will be as efficient as rf driven accelerators, and you may convince yourself that a post LHC linear collider will only require 1 GW of electrical power. There are many more practicalities that put TeV scale wakefield colliders far beyond what can be delivered in the next decade even if one can pay the power bill.

Laser driven accelerators have made great progress and they do have great promise for accelerator based science that can be delivered in the next decade if that R&D funded at 2 to 3 times the present rate. Unfortunately high energy physics is not one of those promises.

30. **srp**
November 23, 2007

wb: Thanks for your insights.

The advanced accelerator community does not appear to see things the same way; see for example

http://cerncourier.com/cws/article/cern/30148 where the author talks entirely in terms of high-energy physics applications and never discusses power issues (although he is worried about applying wakefield technology to positrons). This stance could be technological hubris or grantsmanship, of course–I think the tokamak fusion community has been accused of similar sins–but I need to explore the issue more thoroughly to know whether your dismissal is the end of the argument.

31. **srp**
November 23, 2007

And this guy is talking about 10TeV with 360MW wall-plug power:

http://www.bnl.gov/ATF/Meetings/AAC04/plenaryabstracts/Monday/GeraldDugan.pdf

Of course, there are some feats of beam compression involved to get the desired luminosity, and I have no way of telling whether these are plausible. But if it took spending $100 million to find out, I'd support that.

32. **wb**
November 23, 2007

srp,

Gerry Dugan is certainly an expert and not one pushing his own research program in this talk. Look at his CLIC example. This has 30 MW of beam power. At 10% wall plug efficiency and accounting for overhead power for the facility, CLIC will need at least 500 MW. (note that is completely consistent with ILC estimates at 1 TeV. At 3 TeV CLIC also needs other miracles to actual maintian nanometer beams in collision in space and time. No one has demonstrated that such control systems can close the loop or are stable. Nonetheless, I agree that search for such capability is worthwhile. It will tell us the limits of are capabilities with linear colliders.

His 10 TeV system at 1e36 luminosity assumes an efficiency that is rather incredible. Despite the fact, that the present state of laser efficiency from wall plug to ultra-short pulse lasers is ~1% and that the efficiency from that lase pulse to a beam is also of that order, the stated ifficentcy to two significant figures is 16%. The beams must be controlled to 1 Angstrom in the vertical plane. I'll have to think more about whether that last claim makes physical sense even.

The bottom line is that this technology will not be ready for 2025 even if R&D funding were increased many-fold. Fortunately the laser technology gets driven by commercial and defense interests. The only sure path forward to the 10 TeV mass scale is a very large hadron collider. Is that affordable? are we capable of managing a project of such large physical size? I'm not sure. I only advocate avoiding the hype and making sure that LHC and its upgrades give the very best possible physics output.

33. **Howard Walker**
December 13, 2007

Mr. Woit,

I have just finished "Not Even Wrong" and loved it. While it confirms my thoughts about String Theory, I was really quite pleased to see that professionals like yourself are coming out against it. I am quite angry though that so much money and time is being lost on this red herring and that it will not end any time soon as THEY are in charge.

If the TOE were to be announced tomorrow, THEY would say THAT is exactly what they have been saying all along and THAT IS what String theory really was all along. And THAT is what really bugs me to no end.

Please keep up the fight for real science.

Howard

Sidney Coleman 1937-2007

November 19, 2007
Categories: Obituaries

I just learned today the sad news of the death of Sidney Coleman, yesterday at the age of seventy. Coleman had been in quite poor health in recent years. I wrote about him here back in 2005, after attending a conference held at Harvard in his honor.

Update: More from Betsy Devine, Lubos Motl and Sean Carroll.

Comments

1. **A quantum diaries survivor**
   November 19, 2007

   Darn it. I am sick and tired of people dying! I know I sound silly, but I am serious. Today I learned Michael Schmidt, a colleague in CDF, also died – and he was quite young. May he rest in peace. And I want to remember here another friend who died only a couple of weeks ago, Riqie Arneberg, a non physicist with a bright mind who avidly read physics blogs. Sorry Peter for the off-topicness.

   Cheers,
   T.

2. **Roger**
   November 20, 2007

   Meaning absolutely no disrespect to his family, it sounds like it was a release from a lot of suffering. Parkinsons disease is a wretched illness.

3. **Betsy Devine**
   November 20, 2007

   Thanks for the link, though it goes to my 404 page. Anyway, the best part is the comments, which are a pile of Sidney Coleman stories:

   betsydevine.com/blog/2007/11/20/our-friend-sidney-coleman-has-left-the-planet/#comments

4. **Peter Woit**
   November 20, 2007

   Hi Betsy,

   Link should be working now. I second your recommendation to people that they should check out the comments!

5. **joe**
   November 20, 2007

   My Favorite Sydney Moment was his impression of an electron under various symmetry transformations – fisrt he walked across the room, then he walked across the room backwards, then he walked across the room backwards in the other direction.

   I was bad at QFT, but I loved Sydney's Lectures.

6. **Jimbo**
   November 21, 2007

   A lively Sidney Coleman invading a closed New Jersey county fair, to wax philosophical about the universe is a treat for all in the DVD, `Stephen Hawking's Universe, vol.3′.
   I think the lesson of Sidney's later years, is to treasure the masters who walk amongst us while we still have them. Soon, we will probably lose John A. Wheeler and other great spirits whose timeless contributions to physics were apparrent at the SidneyFest.
   A recent Godel-Fest article at the IAS by Dyson, mindbogglingly recounted how new post-docs were casusally told by Oppenheimer to "avoid Einstein" , as he was `totally out of touch with current physics'.
   Can anyone imagine avoiding asking Einstein out for lunch, so as to not have to hear about unified field theory ??

7. **D R Lunsford**
   November 21, 2007

   He was an excellent physicist. I hope he knows what breaks electro-weak symmetry now.

   -drl

8. **Jonathan Vos Post**
   November 21, 2007

   Note that Sidney Coleman is mourned by the professional Science Fiction establishment, because of his outreach to and partcipation in this speculative discipline.

   Locus magazine online, Tuesday 20 November 2007

   Physicist Sidney Coleman, a professor at Harvard University who was a co-founder of Advent:Publishers in the 1950s and who wrote several review columns for The Magazine of Fantasy and Science Fiction in the 1970s, died Sunday, November 18, 2007, at the age of 70.

9. **Garotte Lazy**
   November 21, 2007

   Sidney was a good man… a decent man… and in many ways a capable physicist.

But in the end, he was a **limited** man… sadly, sadly limited.

And his time had come and gone.

His scientific world was a world of crabbed, cramped limitations, so-called rules, and unimaginative, arbitrary restrictions like “unitarity”, “self-consistency”, “not adding complex numbers to grassman-valued numbers”, and so on.

He, like so many of his kind, had the soul of an accountant and the imagination of a Soviet bureaucrat.

The doctors may talk of Parkinson’s disease, but I think what truly dealt him the final blow was a sense of sorrow:

His crowning achievement — the Coleman-Mandula so-called “theorem” — had recently been falsified
(http://en.wikipedia.org/wiki/An_Exceptionally_Simple_Theory_of_Everything)
and his bigoted worldview of “consistent physics” had come crashing down around him, leaving his legacy in deep disgrace.

But why should I go on beating a dead horse?

Today, the Coleman era has passed.

Smolinian “seers” such as myself and the author of this blog are finally dealt our just rewards — FQXI grants, book deals, talk-show appearances, not to mention promotional T-shirts and coffee mugs with our revolutionary theories embossed on them — all the indicia of true scientific success.

Meanwhile Coleman and his degraded heirs — the soi-disant “string theorists” — stand on the brink of a vast obscurity.

Not a moment too soon, but let us be charitable —

De mortuis nil nisi bonum.

10. **Yatima**
November 21, 2007

> Garotte Lazy Says…

Very funny. You sadly fail at satire.

11. **Tony Smith**
November 21, 2007

Sidney Coleman lives on through the people he knew and taught, and through his brilliant works (his book of lectures Aspects of Symmetry, his Harvard lectures written up by R. Ticciati as the book Quantum Field Theory for Mathematicians, etc).

Unfortunately, in an attempted (failed IMO) satire “Garotte Lazy” referred to
“… Coleman and his … heirs … “string theorists” …”,
even though as Jacques Distler said on his blog on 18 March 2005
“… Sidney was not interested in string theory; he wasn’t even particularly interested in supersymmetry …”.

Since Sidney Coleman was Jacques Distler’s Ph.D. adviser, Jacques Distler’s assessment of Sidney Coleman’s attitude toward string theory is probably accurate.

If “Garotte Lazy” were to try to defend his statement by saying that since Jacques Distler is a string theorist and also a student of Sidney Coleman, a string theorist is in that sense an heir of Sidney Coleman, I believe that I would find such an argument to be specious.

Tony Smith

12. **woit**
November 21, 2007

“Garotte Lazy” = “moron” = “Richard Feynman”

I see that you appear to be affiliated with the physics department of the Institute for Advanced Study. For why I’m mentioning this, see
http://backreaction.blogspot.com/2007/11/anonymity.html#c219957437009240105

Posting stupid attacks on people from behind various juvenile pseudonyms is completely unprofessional. Dragging the reputation of a beloved and admirable person into this at a time that his corpse is not yet cold, with the claim to be his “heir”, is in addition morally repulsive.

13. **Mrs. Feynman**
November 21, 2007

“…Lazy” = “moron” = “Richard Feynman”

How can you insult my dead husband by calling him **“lazy”** and a **“moron”** ?? Don’t you **dare** speak ill of my poor, dead Richard, do you hear me?

How **could** you??

YOU MONSTER!!!

14. **Peter Woit**
November 21, 2007

“Mrs. Feynman”=“Garotte Lazy” = “moron” = “Richard Feynman”, and is a regular reader of this blog from a Linux machine at the IAS running an IAS customized browser. He posted the same thing at Lubos’s blog, where Lubos writes that he finds it inappropriate and wishes that he had deleted it before people started commenting on it.

15. **Bee**
November 21, 2007

This is sad, both Coleman's death and the inappropriately stupid comments you have to deal with. Best,

B.

16. **nontrad**
November 21, 2007

I was a student of a PhD student of Coleman's. I read 'Aspect of Symmetry' closely as a grad student and prior to that watched videos of Coleman's lectures (e.g. 'Quantum Mechanics in Your Face' about the Bell inequalities / EPR / hidden variables etc) and prior to that heard many stories. So, I was saddened to hear about this loss.

That said, I would like to *STRONGLY* second the idea that it is *EXTRAORDINARILY* inappropriate to use this loss as an opportunity to engage in polemics.

Regards,

nontrad.

17. **Observer**
November 21, 2007

Garotte Lazy Says: ……

No time, no place for those comments, not even in your wildest dreams you will get something similar to a a Sidney Fest to honor you. By the way with your last citation, De mortuis nil nisi bonum.
"No one can speak ill of the dead," embarrasingly you contradict your whole comment.

18. **T**
November 22, 2007

I still don't quite understand what *EXACTLY* Sidney Coleman contributed that merits such deep reverence for him after his demise; was he like Weinberg – i.e. a very intuitive and thoughtful field theorist – or Feynman – a highly creative and original thinker; or simply a good teacher who taught at (world-famous) Harvard – and hence his stature? Sidney Coleman's writings don't seem particularly any different from that of other well-cited physicists.

Regards.

19. **D R Lunsford**
November 22, 2007

Peter, couldn't have said it better, even though I tried.

-drl

20. **Steven H. Cullinane**
November 22, 2007

T: The following quotes may be of interest."Sidney Coleman comes as close as any active physicist to assuming the mantle of Wolfgang Pauli as a trenchant critic of research and as an expositor of ongoing developments in theoretical physics." —Book review of *Aspects of Symmetry*"He has… played the role of Wolfgang Pauli of his generation; he liked to disprove ideas, and he was also a genius in explaining things to others." —Lubos Motl

21. **Professor R**
November 23, 2007

hi Peter,
sad news that Sidney Coleman has died, just as his famous theorem is in the news again. (I'm enjoying all the recent comment on the recent Lisi paper on the physics blogs). Sidney will live on through his work and his students.

Re a historical point, did you know that the first proof of the 'impossiblity' of combining internal symmetries with the Poincare symmetry of space-time was furnished by the Irish physicist Lochlainn O' Raifeartaigh?(my late father). The 'O'Raifeartaigh no-go theorem' was a hugely controversial result at the time and brought quite a few PhD studies to an abrupt halt – see Dyson's book 'Symmetry Groups'. My understanding is that the debate was finally closed when Coleman and Mandula published a generalization of the theorem. Given the controversy, it seems a pity that the original theorem is never referred to now (although it is liberally cited in the Coleman-Mandula paper)…in other words Dad got all the flak but is now forgotten!
That said, I don't really understand the details of the difference, being an experimentalist.

Re Lisi E8 paper, as a rank outsider, I can't help thinking that it would be very surprising if the group theory specialists would have missed this, Dad and others were v familiar with this group….Cormac

22. **MathPhys**
November 23, 2007

Sidney Coleman was an extremely smart man who, in his prime, knew an incredible amount of Physics.

When he graduated from CalTech, Gell-mann wrote him a recommendation letter saying "Coleman knows more physics than anyone I know with the exception of Feynman".

He was also a very pleasant man. RIP, Sidney Coleman.

23. **Peter Woit**
November 23, 2007

Hi Cormac,

Thanks for writing. Your comment did encourage me to look at one of Coleman's papers on this that was in a book on my bookshelf ("Seven Types of U(6)", in "Mathematical Theory of Elementary Particles", edited by Roe Goodman and Irving Segal, proceedings of a 1965 conference).

The article is written with Coleman's typical clarity and humor, reading it reminds one of the reasons he is so highly admired. He begins with a quotation from the Marquis de Sade's "La Philosophie dans le boudoir":

"Let up put a little order in these revels: measure is required even in the depths of infamy and delirium"

which might also be an apt quote for the current state of particle theory…

Coleman mentions the O'Raifertaigh theorem in a note at the end of the article, characterizing it as saying that if you have an exact finite-dimensional symmetry group including both Poincare and internal symmetries, then all particles will have to have the same mass. He claims this is a weaker result than what he is after (the paper was written at a time that he suspected, but did not yet have an argument for what was to become the Coleman-Mandula theorem), since it allowed the possibility of constructing non-trivial theories combining internal and space-time symmetry, as long as the particles had the same mass. Recall that at the time, this was all about working with symmetries that were known to be only approximate anyway, e.g. flavor SU(3).

In the bibliography he includes, Coleman makes the following excuse to those authors not represented:

"This bibliography is representative, not exhaustive; it would require the talent of a Dunbar to compile an exhaustive one. Before omitted authors become indignant, they are advised to examine the text and see how I have represented included ones."

One of the main sources he lists is the proceedings of a conference held at Trieste in May-June 1965. I remember seeing this many years ago. There were a huge number of papers by people trying out all sorts of variants of large non-compact groups as possible symmetry groups including SU(3) and space-time symmetries. It was remarkable to see both how quickly people started working on this after the discovery of SU(3), and how quickly they stopped as the no-go arguments became apparent.

24. **Santo D'Agostino**
November 23, 2007

MathPhys,

I remember reading this story somewhere, as follows: Coleman got reference letters from both Gell-Mann and Feynman. Gell-Mann's said that, "Sidney Coleman knows more about field theory than anyone else, except for Feynman." Feynman's said that, "Sidney Coleman knows more about field theory than anyone else, except for me."

This Week’s Hype

November 23, 2007
Categories: This Week's Hype

This past week has seen a veritable bumper crop of media hype, involving claims coming from both string theorists and critics of string theory. Besides the overhyped claims and Lisi-mania that has made it into all sorts of media outlets, New Scientist has added a couple more examples:

- The cover story of this week’s issue is The void: imprint of another universe?, which features claims by Laura Mersini-Houghton about a feature observed in the WMAP data that vindicates string theory. According to her:

> It is the unmistakable imprint of another universe beyond the edge of our own.

and

> I think our evidence points to string theory being on the right track.

She also claims that string theory does make a prediction about what the LHC will see: no supersymmetry. Since many other string theorists are claiming that string theory could be vindicated by seeing supersymmetry at the LHC, I guess this logically shows that string theory has already been shown to be correct by the LHC results, since it predicts both that supersymmetry and no supersymmetry will be seen, and this is a prediction guaranteed to come out right.

- The same issue also contains an article entitled Has observing the universe hastened its end?, about this recent arXiv preprint of Lawrence Krauss and James Dent, where they, according to New Scientist

> …suggest that by making this observation in 1998 we may have caused the universe to revert to a state similar to early in its history, when it was more likely to end.

The Drudge Report today links to an article Mankind ‘shortening the universe’s life’ in the British newspaper The Telegraph.

A lot of this nonsense seems to be originating in Britain. Tomorrow at Cambridge University there will be a series of talks on God or Multiverse? that one can attend for the bargain price of 65 pounds. The talks are advertised with a quote from philosopher Neil Manson

> ~~The multiverse is the last resort for the desperate atheist.~~
> Note added 10/29/2014: Actually, that’s a misquotation and misrepresentation of what Manson wrote. For the true story, see here.

Perhaps the members of the clergy assembled for this event can lead those attending in a fervent prayer that we soon be liberated from this plague of hype and nonsense, whether it be inspired by string theory or not…

Update: The story mangling Krauss/Dent has made it to Slashdot. Seems to me that recent Slashdot stories on physics conclusively falsify one theory, that of the wisdom of crowds.

Update: Krauss has changed the last two sentences of the paper to avoid misunderstandings about what he is claiming such as the ones that appeared in the media, see his comment here.

Update: It appears that Krauss somehow got the notion that it would be a good idea to respond to Lubos’s posting about him in the comment section of the blog. He has now been banned there on the grounds that he is “unable to satisfy basic criteria of what I consider a rational debate.” Remarks from anyone supporting him have also been deleted, following the usual Lubosian practice of how to deal with dissent.

Update: The Telegraph article has been extensively edited, with the current version more accurately reflecting what was actually in the Krauss/Dent paper. The misleading headline remains. I also hear that Krauss has written a letter to New Scientist about the problems with their article. This also got picked up by Wired Science, where I seem to have acquired an affiliation with MIT I wasn’t aware of.

Update: Here’s an account of the “God or Multiverse” event, where prayers for deliverance from nonsense were not answered by the almighty:

> …given that multiverses are in favour in many physics departments these days, perhaps theology has something to contribute. Augustine and Nicolas of Cusa are just two theologians to have pondered the possibility way back, thinking it quite likely that the generous creativity of God would overflow into the formation of other universes…
>
> For theists, consciousness is ontologically prior to everything else. So in a sense the possibility of the multiverse makes perfect sense already. It would be every possible state of things that could exist, formed in the mind of God – who must be able to conceive of everything possible since that is implicit in the concept of divinity…
>
> …not all explanations of things are simpler than the things they are explaining (the multiverse as an explanation for the apparent fine-tuning of our universe being an obvious case in point…
>
> …In fact, maths looks rather like God – the former being necessary thinking, the latter necessary being…
>
> …However, if modern cosmology comes up with the multiverse as the fundamental, necessary proposition (at least in one version, it says that all possible worlds necessarily exist somewhere, we just happen to be in the one that we happen to be in), then Ward put it that the proposition of God as the fundamental necessity is actually a far simpler conjecture. In the theological case, all possible worlds would be said to exist in the mind of God, though quite possibly only a limited number of universes, and perhaps only one, actually exist. Occam would presumably have been much happier with that thought than heaped infinities of actually existing universes…
>
> God’s role in creation, then, is to allow only the universes that do exist, to exist.…
>
> …This would be a purposive explanation of the universe. Purposive explanations require knowledge of things, discrimination between things, an appreciation of goodness, and the power to chose good over evil. So to put it all another way, the big question in the cosmology debate is that of evaluation: how do you evaluate one theory over another?…

Update: John Baez explains what the Krauss/Dent paper is really about.

Update: The Krauss/Dent paper has now been refereed and accepted for publication in PRL.

Comments

1. **Bee**
   November 23, 2007

   this plague of hype and nonsense

   It's just a matter of perspective. Think of it as science fiction. It's interesting, in a certain way. I read a lot scifi as a kid, and I've always found other universes and big mysterious voids interesting. I can understand that people like to read that stuff. I just think that in many cases the speculative character of ideas is not sufficiently well explained. And then there's of course the concern that under such circumstances it might eventually have an impact on science what people like to read about.

2. **Yatima**
   November 23, 2007

   I feared that Peter would not be able to resist the bait of the latest edition of New Scientist.

   The editors should move these speculative ideas to a different section than "Fundamentals", something like "Really Wacky Stuff" pronto.

   Something must be going wrong when arbitrary civilian (i.e. me) is presented with the daunting prospect of having to make sense of a text like:

   "Our measurement of the light from supernovae in 1998, which provided evidence of dark energy, may have reset the false vacuum's decay clock to zero" (thus hastening the day of vacuum decay)

   Only locally, one would hope. Additionally, reflect upon the fact that the blogosphere now observes the emergence of the idea that observing the universe may hasten its end. This might push the universe into a state in which this same idea is more likely to be true. We are doomed by a feedback loop created by a wrong interpretation of the verb "to observe".

   I will spare you %&+*#! bad puns about the truth emerging from large holes in the universe.

3. **wb**
   November 23, 2007

   The Krauss/Dent claim is rather amazing. The 1998 observations were no different qualitatively than those made earlier. What is different is the human concepts that emerged. So what did reset the universe in their concept? seeing a supernova? did seeing reset the universe a thousand years ago? or physicists thinking about it? If the later, those not accepting the dark energy theory would have had thoughts that did not reset the universe. Why did one set of thoughts win out? Suppose dark energy is false, does the universe revert to its former state. The observations themselves interrupted the flow of a less than 1 Joule of energy… an that resets the universe?

   Tis takes more suspension of disbelief than listening to presidential candidates.

4. **alex**
   November 23, 2007

   "She also claims that string theory does make a prediction about what the LHC will see: no supersymmetry."

   There is no such generic prediction. Her statement is obviously wrong. She is not a string theorist, nor a particle theorist so I would be suspicious of such claims.

5. **SnarkFest**
   November 24, 2007

   It's a good thing this slop has a lot less potential impact on our lives than the demented shenanigans that have occurred in the world of quantitative finance (and the investment strategies it enables) over the past few years. NSF and DOE will be lucky to have a budget by the end of the decade (not to mention the rest of us).

6. **PMembrane**
   November 24, 2007

   Does this mean we'll get to watch Krauss spear his hand with a pen-knife?

   Chaos is king! Magic is loose in the world!

7. **John Baez**
   November 24, 2007

   Bee wrote:

   > Think of it as science fiction. It's interesting, in a certain way. I read a lot scifi as a kid…

   I read a lot of sci fi as a kid too! Now that I'm grown up, I read a lot of SF. 🙂

   Whatever you call it — science fiction, sci fi, speculative fiction or SF — I think it's great stuff. By *labelling* it as fiction, we can let it inspire us without taking it too seriously. The problem comes when people try to pretend such stuff is real science, even when it's not precise and doesn't make any testable predictions. That's what Peter Woit is quite rightly complaining about.

   Let science be science; let SF be SF.

8. **PMembrane**
   November 24, 2007

   The silly phrasing of the coda to the Krauss/Dent preprint aside, I am curious as I have never seen a concise explanation of what the decay of a metastable ST vacuum means in concrete terms.

   I've seen it stated that the ground state of the ST vacuum is flat 10-d Minkowski spacetime—does this mean that the compactified dimensions suddenly "unroll", resulting in a supersymmetric 9-d universe?

9. **Bee**
November 24, 2007

Hi John: Yes, I totally agree with you. See the last two sentences of my first comment. Best, B.

10. **Arun**
November 24, 2007

IMO, particle physics is accelerating on its downwards trend: starting with the popularization of speculative, even if powerful, ideas in the public in the name of bringing science to the masses with now not-even-a-theory being heralded in blogs and publications like The Economist as the great new hope.

11. **Peter Shor**
November 24, 2007

I'm going to be agree with Bee, more or less.

Is this any worse than news reports of the quantum eraser experiment which make it sound like quantum mechanics means that we can change the past (and which are also totally baffling, especially to people who actually understand quantum mechanics)? There is real science behind the quantum eraser experiment, and there seems to be very little actual science behind the ones you report on this week, but I suspect that this distinction is completely invisible to the public. How can you tell the difference between botched reporting of good science and botched reporting of bogus science? They both get the public excited about science, which is a good thing.

Ideally, of course, the science writers would be clever enough to only report good science, and not botch their explanations of it. But we don't live in an ideal world.

I would like to believe that scientists who get themselves in the news with sci-fi stuff don't advance their careers. This would give incentives to scientists to come up with this sci-fi stuff, which isn't at all good for the field (as opposed to journalists coming up with the sci-fi stuff, which I can't see as being very harmful). Any thoughts on this? And how can we give scientists incentives to explain their work to the public honestly without giving them incentives to come up with this sci-fi nonsense?

12. **How Can We Turn The Tide?**
November 24, 2007

I had great hope last year with the publication of Peter's Not Even Wrong and Lee Smolin's The Trouble With Physics.

Finally someone put the Bogdonavs in perspective.

But their most recent actions, which helped fuel the media storm regarding Garret Lisi's non-theory, left me disappointed.

Garret mistitled a paper, spent hours preparing a pretty, but ultimately meaningless youtube video with a hot English female voice, which in fact does not unify gravity with the other forces, and succeeded in getting the non-physics video everywhere, along with Lee's "fabulous" appraisal.

The deeper I study the Lisi crisis, the darker it gets–there was a lot of pre-mediation on Lisi's behalf, aided by willing consultants and hype-master accomplices. Read Lisi's "gee whiz I'm a surfer and gee whiz I just figured out the universe and gee whiz it's a young theory and gee whiz I don't want to hype it and gee whiz it's the media's fault" posts at all the proper, pre-mediated points in the blogosphere–read his sycophantic posts throughout the forums, where he never quite answers anything, but only states that he is a poor hermit and surfer and snowboarder in Hawaii and Lake Tahoe. Does Woit think that this is any different from the Bogdonav Affair, other than that Lisi surfs and snowboards, which gives him a media advantage?

To the degree this media event succeeds, it devalues all the humble, hardworking, and generally underemployed physics Ph.D.'s; and it further encourages the public to distrust science.

I wish Garrett would have placed his physics first, which would mean first doing physics, I guess.

It would be so easy for Peter & Lee to speak out and to have spoken out, instead of calling Garrett's non-theory "fabulous."

It is time for Garrett to retract his paper from arxiv.org, retitle it with a more appropriate title, and resubmit it.

It would also be great if he were to contact all the media outlets and convey to them the simple facts and truth–his theory makes no predictions and cannot be tested, and it is replete with fundamental errors and handwaving that are now well-documented throughout the blogosphere, whose time he wasted.

If Lee and Peter ever meant anything by their books–if they are men of their words–the most important element in the advancement of science and all culture–they should call upon Lisi to set the record straight.

13. **Peter Shor**
November 24, 2007

To answer Pmembrane, I thought that Calabi-Yau manifolds were flat 10-d space-time (at least to the first-order approximation), so all these Calabi-Yau manifolds are ground states to a good approximation. This is probably what you heard stated. Higher order effects are what is believed to make one the true vacuum and the others false vacuums.

As for how you go from one Calabi-Yau manifold to another, I don't believe anybody understands this process in any degree of detail whatsoever. Please correct me if I don't know what I'm talking about (which is quite likely).

14. **AP**
November 24, 2007

Maybe I am missing something here … New Scientist is a magazine that they sell for money right? Would I want them to make less money to satisfy some irrelevant scientists? Not if they are the source of my pay check, right? (Here I am defining irrelevant as being less news worthy than which starlet got arrested for drunk driving last night). I can hear the 'but' coming already … 'but shouldn't they make money without hurting science?' One has to ask why? I don't let other people bully me about how to legally make money and I don't think New Scientist should let me, you or anybody bully them either.

That being said, what about the scientists involved in this debacle? Well what can one say? It seems to be working out for them right? No offense Pete, but you're one to talk. You use popularization for your own purposes too. But I'm sure you'll say you do it the right way and they do it the wrong way. Funny how life seems to work out like that …

15. **Tim May**
November 24, 2007

I'm not sure I should be commenting here, lest the future of the universe be further shortened. (We now have confirmation that blogging is killing the universe, something many of us had already felt was happening.)

Hey, I like science fiction as much as Bee, John Baez, etc., but what seems to be happening is the "tabloiding" of science, akin to "Entertainment Tonight!," "The National Inquirer/Enquirer," Paris Hilton, Brittny Spears, etc.

I think a certain amount of speculation is healthy. I vividly recall reading everything I could back around 1968-70 on Wheeler's ideas about black holes, wormholes, etc. (even before these became motifs in science fiction). I don't think the speculation was too way out, and it didn't cause much confusion….just a few bad SF movies.

This tabloidization isn't completely new. I remember "Science Digest" from the 1960s, a small magazine printed on pulpy-type paper, with lots of gee-whiz articles about time travel, flying cars, and our impending colonization of Mars. And "Science News" had its moments of silliness.

Then the 1980s hit, with the publisher of "Penthouse" producing "Omni." A glossy, glitzy magazine devoted to pop science, with lots of articles about living forever, uploading into computers, etc. Other mags, too. It eventually folded. (Along with a really excellent magazine called "High Technology," which had great articles about things like disk drives work, how chips work, etc.).

Even "Scientific American" moved away from its formerly staid, even formulaic, approach to articles in favor of shorter, more "with it" pieces. And cover stories designed to sell newstand copies. (I don't have any issues handy, as I no longer buy paper magazines, but I recall several breathless covers not too far removed from what "New Scientist" uses frequently.)

There's still "Nature." And "Science."

Oh well. Maybe someone will produce a paper saying that if _observing_ dark energy shortens the life of the universe, then _confusion_ and _errors_ about dark energy will REVERSE THESE EFFECTS!

(BTW, I looked at the PDF of the K & D preprint. The citations didn't seem to mention Nick Bostrom, who has written about Bayesian issues along the same lines. I don't think he would phrase things in terms of "the observation of blah _causes_ the universe to end sooner," but would express things in terms of what an observation means in terms of where in a thing's lifetime (civilization, environment, universe) we likely are. I'm not sure I agree with Bostrom, but at least he phrases the "Neo"-Bayesian points in a non-causal way.)

–Tim May

16. **Lawrence Krauss**
November 24, 2007

Hi.. I wanted to chime in with an apology of sorts regarding the confusion in the press regarding our work. Our paper was in fact about late-decaying false vacuum decay and its possible cosmological implications. Needless to say, the explosion of press interest, prompted by the final two sentences of the paper, misrepresented the work, which was not intended to imply causality, but rather to ask the question of whether by cosmological measurements we constrain the nature of the quantum state in which we find ourselves, inferring perhaps that we are not in the late-decaying tail. However, I do take responsibility in part for the flood, as I was undoubtedly glib in talking to the new scientist reporter who read the paper on the arxiv. I have learned that one must be extra careful in order not to cause such misrepresentations in the press, and I should know better. In any case, the last two sentences of the paper have been revised so that it should be clear to the press that causality will not be implied. mea culpa

17. **Peter Woit**
November 24, 2007

Pmembrane/Peter Shor,

10d flat space appears to be a stable solution to string theory, consistent if any solution is. A Calabi-Yau is a 6d space with certain properties, can be highly curved. String theory is believed to have solutions in which 4 dimensions are flat (space-time), 6 are a Calabi-Yau.

Saying that transitions between solutions are not understood in any detail is a huge understatement.

However, this discussion of the basics of string theory really is off-topic, if you want to pursue it, perhaps the blog of a string theorist would be a place to try.

18. **Peter Woit**
November 24, 2007

How can we…

I've not referred to Garrett's work as "fabulous", instead have tried on this blog to give an accurate description of it. Garrett, like any author, is entitled to a much more optimistic view of his own work than other people may hold. Taking this into account, I don't think his behavior has been unreasonable at all.

19. **J.F. Moore**
November 24, 2007

I would like to be sympathetic to science stories being "mangled" as our host Peter puts it, or "misrepresented" as L Krauss does. I certainly see many examples of this in pop science literature. But I'm having a hard time understanding how a science reporter, when given this quote (presumably via phone or email interview):

"The intriguing question is this," Prof Krauss told the Telegraph. "If we attempt to apply quantum mechanics to the universe as a whole, and if our present state is unstable, then what sets the clock that governs decay? Once we determine our current state by observations, have we reset the clock? If so, as incredible as it may seem, our detection of dark energy may have reduced the life expectancy of our universe."

is misrepresenting the science or the scientist if the reporter then prints that in the article he writes. Were there strong qualifiers that were omitted? I also don't think the pub crowd at Slashdot (which linked to the Telegraph article, not New Scientist) can be much blamed for taking such clear hints at causality and running with them.

I do respect the mea culpa here, but I would think it would do more good (maybe with some exposition) as a letter to the editor in New Scientist and the Telegraph, precisely the places where people who are easily mislead might then read it.

20. **Lawrence Krauss**
November 24, 2007

I have written to the New Scientist…btw…

and the quote was within the context of a wavefunction.. if our measurements constrain the wave function, and if the probability of decay depends upon the state we are in, then if we are averaging over probabilities the statement is true I believe.. what was glib was not being clearer on this than I was.

21. **Chris Oakley**
November 25, 2007

Lawrence,

It may be too late. Even by suggesting, or causing others to think that you suggested, that making astronomical observations could cause the universe to end prematurely you may have forced the universe into a quantum state where this could possibly happen.

Tim,

Why is it that none of you guys seem to be able to spell Britney Spears' name correctly? Peter and Lubos both have "Brittany" (see the Jan 22, 2006 NEW posting), which lends credence to my theory – admittedly on diminishing evidence – that Lubos is a fictitious adversary created by Peter to boost sales of his book.

22. **Nugae**
November 25, 2007

Every week New Scientist publishes a cover article that is either fallacious or nonsense. Don't be narrow-minded: try reading them when they're on some subject other than cosmology and you'll see it's true.

[Occasionally there is an exception, when in Du Sautoy style, the article is so vacuous that one can't even see whether it's nonsense or not].

A really forward-thinking university would organize a weekly New Scientist undergraduate seminar: a great interdisciplinary exercise for the enquiring mind. PhDs would be allowed in only if they had some residual ability to think outside their own field. A Britney test would be unfairly stringent, though: a Tolkien test might be easier.

23. **How Can We Turn The Tide?**
November 25, 2007

Peter,

What it all comes down to is that the public is entitled to the Truth.

I simply cannot comprehend how you can state in good conscience that Garrett's behavior has not been unreasonable. Your slipperieness is beocming more and more apparent, undermining the value of this blog and your book.

Garrett titled his paper "An exceptionally simple theory of everything," which the paper is not. Indeed, authors are entitled to optimism in their work, but that does not grant them the right to publish and perpertuate lies, with the aid of well-funded accomplices.

Smolin called the paper "fabulous," and look at the hundreds of pages which have resulted:
http://www.google.com/search?hl=en&safe=off&q=fabulous+smolin+lisi

Lisi claims that his theory can be tested, but this is also a lie. His theory offers no concrete predictions.

Lisi also went through an exorbitant amount of effort to make a youtube video with a hot women's voice and market his surfer image.

Again, all that we're asking for is the simple truth:

1) Woit and Smolin should acknowledge that the title of Lisi's paper is hype, and that his theory makes no predictions.
2) Lisi should withdraw his paper from arxiv.org, retitle and rework it to reflect it's true nature, and resubmit.
3) Woit/Smolin/Lisi should contact the dozens of major news outlets, and share the truth.

http://www.foxnews.com/story/0,2933,311952,00.html

And please, let's stop with the cutesy-irony, as Fox News reports, "For his part, Lisi self-mockingly calls his finding "An Exceptionally Simple Theory of Everything." in Laid-Back Surfer Dude May Be Next Einstein

http://www.foxnews.com/story/0,2933,311952,00.html

What it all comes down to is that the public is entitled to the Truth.

Anyone who disagrees with this can never contribute to science.

24. **piscator**
November 25, 2007

Dear Peter,

This statement

>Saying that transitions between solutions are not understood in >any detail is a huge understatement.

is plain wrong. Certain transitions – e.g conifold transitions – are understood very cleanly, and there is a beautiful story involving the presence of new light degrees of freedom (wrapped branes) at the singularity. This is really nice physics, the solutions are protected by lots of supersymmetry, and everything is under control.

This case represents a *great reason* to study string theory – the theory knows about topology change, and can describe it precisely and in a controlled fashion.

piscator

25. **Peter Woit**
November 25, 2007

piscator,

One can argue about how well this is understood in certain idealized models, but I think it is accurate to say that there is nothing remotely like a detailed understanding of such transitions in general, or most importantly, in the case relevant to physics, of transition between two string backgrounds complicated enough that one of them might have something to do with the real world.

26. **conifold**
November 25, 2007

Peter:"…the case relevant to physics, of transition between two string backgrounds complicated enough that one of them might have something to do with the real world."

Is the KKLT construction "complicated enough"? Recall that they compactify Type IIB on a CY which has a local structure of the Klebanov-Strassler deformed conifold.

27. **Peter Woit**
November 25, 2007

conifold,

Sorry, I just don't see how what is known about the conifold transition can be said to provide anything that can be called a "detailed understanding" of the transition from one realistic string background to another, including in the KKLT case.

28. **piscator**
November 25, 2007

Peter,

obviously I agree that the less supersymmetry you have the less control you have.

I very much hope that for backgrounds describing the real world, the theory of vacuum decay is not a question that is actually relevant to experimental physics 🙂

piscator

29. **Bad**
November 25, 2007

"Lawrence, It may be too late. Even by suggesting, or causing others to think that you suggested, that making astronomical observations could cause the universe to end prematurely you may have forced the universe into a quantum state where this could possibly happen."

Now kids, *that* there is some real wit. Bravo. 🙂

30. **nicky nichols**
November 26, 2007

hang on! surely new scientist is not that bad, the article does state that there is at least another explanation.

please do not criticise science journalists who have to assimilate a great deal of new information and understanding in a very short period of time. and besides, science articles are often written in a particular way, science magazines often exist for a particular reason beyond profit.

i could mention several examples of new scientist articles which when were immediately considered to be false, however wrongly so…

31. **Professor R**
November 27, 2007

hi Peter,

Re media hype discussed above:

I've only started reading New Scientist again in the last few months and am astonished how their articles undermine recent work in physics by both over-hyping and misrepresenting it. Here in Ireland and the UK, NS articles are then rewritten in the press, with further misunderstandings added.

I have huge concerns over this method of making science 'interesting'. Is it any wonder the public lack confidence in science, when the latest speculative ideas are portrayed as the new science? It also makes balanced discussion of the papers quite difficult afterwards, which is a pity for the authors.

For example, your own discusson of Lisi paper read as reasonably balanced and fair, but the NS article lacked any critical comment from specialsts. In fact, it pretty much made out that E8 was a new discovery in group representation. When I checked the only book on group theory I have on my shelf (, 'The Group Structure of Gauge Theories' O'Raifeartaigh, CUP) I was surprised to find a comprehensive discussion of the use (and limitations) of E8 in gauge theory as far back as 1986).

As for the NS article on Krauss's paper, its clear from his comments that the article misrepresented the central thesis …I think I'll stick with Physics World …. Cormac

Letter to the Editor

November 24, 2007
Categories: Uncategorized

To the Editor:

Paul Davies, in his Op-Ed piece Taking Science on Faith, uses recent untestable speculation about multiple universes motivated by string theory to claim that "the mood has now shifted considerably" among physicists. He characterizes physics as being, just like religion, "founded on faith", faith in the existence of intelligible laws describing nature and in a "huge ensemble of unseen universes", the so-called "multiverse".

The only real recent shift in mood among most physicists has been a loss of interest in string theory, precisely because its proponents have been forced to invoke the multiverse hypothesis in order to explain why string theory can't predict anything. The existence of mathematical "laws of physics", describing accurately and successfully the physical world in a testable way is not a "belief" but a fact.

Update: The Edge web-site is promoting both the Davies Op-Ed, and several critical responses to it.

Update: Lots of other bloggers weighing in, with the Science Blogs crowd (here, here, here, and here) uniformly Davies-hostile. The only positive blog entries I've seen about the Davies piece come from the IDers and Lubos Motl. Lubos seems to feel that the main issue here is that Steven Weinberg, Stephen Hawking, Lenny Susskind and Frank Wilczek may be unable to pursue their anthropic-principle-inspired research programs out of fear that I might criticize them. I would think they might be even more intimidated by P.Z. Myers, who reaches rhetorical heights I can not aspire to, referring to the Anthropic Principle as that *tiresome exercise in metaphysical masturbation that always flounders somewhere in the repellent ditch between narcissism and solipsism*.

Comments

1. **D R Lunsford**
   November 24, 2007

   Needless to say, someone should write a very hard-hitting Op-Ed rebuttal to this crap. This fiction that physics is somehow about *us*, is the ultimate expression of an absolutely pernicious narcissism.

   Davies, one notes, is British.

   -drl

2. **cyberfizzle**
   November 24, 2007

   I wrote a rebuttal, but I don't know if it's any good.

3. **gbp**
   November 24, 2007

   I think, we see just the beginning of this shift. I expect that all papers where nothing observable is computed with reasonable accuracy may be banned in some way (or labelled as junk). This is going to be the price the community will pay for the decades of superficial speculations.

4. **LDM**
   November 25, 2007

   Well, Davies is all wet.

   First, religious faith, where you are not allowed to challenge dogma, but must accept it even if it is contrary to reason, does not have a counterpart in science. *Everything* in science is fair game to be questioned, and any theory respected today could conceivably be overturned by a new fact discovered tomorrow. Not so in religion.

   Second, he accords the multiverse much more legitimacy than it merits.

   Third, his statement " *the very notion of physical law is a theological one in the first place, a fact that makes many scientists squirm*." is false. The notion of physical law in science, as already noted, is in fact the opposite of a theological notion. Any law is fair game, including the notion of physical law itself, and unlike religious dogma, science *needs* to have its ideas questioned to remain vital…Religion is the opposite, and cannot stand free inquiry and questioning.
   If, for example, I call Susskind a hack because I don't agree with his ideas in HEP, he probably could care less. If I draw a cartoon with him in it ridiculing his ideas… again, he could care less. But, if instead of Susskind I chose a certain religious figure, I could find a fatwa issued against me.

   Davies doesn't know what he is talking about.

5. **zorba**
   November 25, 2007

   Paul Davies said: "In other words, the laws should have an explanation from within the universe and not involve appealing to an external agency. "

   I fail to see what anyone [apart from religious people] could object to in that statement. Boltzmann *explained* the second law of thermodynamics [in terms of probability], Einstein *explained* the laws of gravitation, etc ; all PD is saying is that he wants to see this project furthered, and that people who deny that this is necessary are behaving in an irrational ["religious"] way. Lubos Motl regularly denies that certain things need to be thought about [for example, the smallness of the entropy at early times] and PD is just decrying this kind of head-in-the-sand attitude.

   By the way, the notion that multiverse ideas are *intrinsically* unverifiable is an error; to take just one example, see

   Anthony Aguirre, Matthew C Johnson, Assaf Shomer, Towards observable
   signatures of other bubble universes, arXiv:0704.3473

6. **lostsoul Ph. D.**

November 25, 2007

Davies' conclusion, that the' laws should have an explanation from within the universe and not involve appealing to an external agency. The specifics of that explanation are a matter for future research' seems reasonably sensible. And is the faith referred to in 'until science comes up with a testable theory of the laws of the universe, its claim to be free of faith is manifestly bogus' anything other than a belief in the efficacy of the scientific method? This is something all scientists have, I would have thought. Just by mentioning the multiverse, which he says is 'increasingly popular, but– doesn't so much explain the laws of physics as dodge the whole issue', and having a bit of 'history' in this area, Davies appears to become a target for unreasoned and ad hominem abuse (saying that the fellow is British, for example) of the type more usually associated with anonymous string polemicists.

7. **Coin**
November 25, 2007

Just in case you were wondering, by the way, [the "intelligent design" movement takes Davies' op-ed as vindicating what they've been saying all along].

8. **Constantine Tynyansky**
November 25, 2007

All mathematical axioms are religion. The mathematics are a sect of axiomatic religion. The mathematics nothing explains! For example, where an explanation, why is true (has the proof) any mathematical theorem? Physicists-theorists it the religious fanatics, which try to search for explanations with the help of mathematics. For example, theory of superstrings, multiverse etc.
Sample of religious fanaticism: "all mathematical structures exist" [Max Tegmark].

9. **nigel cook**
November 25, 2007

I read Paul Davies 1985 book *The Forces of Nature* as a kid, and it was helpful in explaining (without any mathematics) a little about the origins of fundamental forces from experiments in electromagnetism, beta radioactivity (weak force), and particle interactions (strong force and validation of the basic electroweak theory by the discovery of three massive weak gauge bosons in 1983). I think it did contain some speculative ideas like string at the end, but that wasn't hyped. The nice thing was the graphical explanation of how the idea of quarks arose from plotting known particles in geometric shapes with particles arranged at their points experimental data (arranging the known baryons and mesons by their charge and spin properties), which led to predictions like the omega minus (containing three strange quarks), which were experimentally confirmed. The book didn't explain everything very well, and the lack of presentation of any significant mathematics was unhelpful. But at least it showed that there was substance and scientific method in some modern physics. It's bad news that Davies has now moved on from explaining how science should be done, to seeking to replace it with religion. However, he clearly wants to be fashionable and he did receive a Templeton Prize for Religion a few years back. What do you seriously expect in this day and age? Science has reached a dead end.

10. **Leonard Ornstein**
November 25, 2007

The convention of accepting axioms and definitions, is an act of "faith". Thus all logic and deduction is rooted in faith.

Believing that observation of an incomplete sample of a class or process can be used to estimate properties of the class…or the future, is an act of faith. Therefore all inductive empirical reasoning depends, at least implicitly, on faith.

Yet Paul Davies is wrong, for the reason repeatedly cited above: Science, which MUST depend on these faiths, none the less, in contrast to theisms, insists that they all are provisional beliefs, scaled by some confidence intervals, and subject to possible refutation by future observation.

Science (as all rational disciplines) makes models of "reality" as perceived by imperfect minds in a noisy environment. No matter how beautiful, the relationship of a model to reality can only be judged by empirical "tests". Until multi-verses, string theory and even Higgs bosons are "tested", they are indistinguishable from good science fiction. And the longer it takes their proponents to at least "design" such tests, the greater our right to be skeptical!

There can be few absolute truths, even in purely logical systems, as Gödel demonstrated; the Platonic dreams of such absolutism by some mathematicians and physicists notwithstanding.

11. **Chris W.**
November 25, 2007

Davies' concluding paragraphs (already quoted in part):

> It seems to me there is no hope of ever explaining why the physical universe is as it is so long as we are fixated on immutable laws or meta-laws that exist reasonlessly or are imposed by divine providence. The alternative is to regard the laws of physics and the universe they govern as part and parcel of a unitary system, and to be incorporated together within a common explanatory scheme.
>
> *In other words, the laws should have an explanation from within the universe and not involve appealing to an external agency.* [emphasis added] The specifics of that explanation are a matter for future research. But until science comes up with a testable theory of the laws of the universe, its claim to be free of faith is manifestly bogus.

Some of the previous commenters (and Peter too) haven't read his essay very thoughtfully. It seems clear that he is *calling for* an explanation of physical laws from within the universe. This is hardly consistent with the viewpoint of intelligent design or earlier theological accounts, which call for an explanation *from outside*, ie, from a deity. He is hardly endorsing the notion of a multiverse as an adequate solution; on the contrary, he evidently regards it as deeply problematic and question-begging, if not altogether vacuous. **He is issuing a challenge, and I believe he hopes it can be met.**

12. **DB**
November 25, 2007

There is no belief system in science which is comparable to religion. What Davies claims to be science's belief system is in fact a set of expectations, based solely on the experience to date that the interplay between hypothesis and experimentation yields a progressively refined and more powerful grasp of the workings of our universe. This is a perfectly rational stance which requires no belief system to underpin it. As long as each iteration continues to deliver the goods, you stick with it.
Someday, this process could very well end in the conclusion that mankind had gone as far as its limited mental capacities allow. At which point the only rational position would be to acknowledge that we don't possess the ability to go further at this time. We are already at the stage that a thorough understanding of established QFT and GR demands exceptional mathematical ability and years of university level study and is therefore reserved to a tiny minority of humanity.
As Poincare explained so eloquently in his popular text Science and Hypothesis way back in 1905, our so called immutable laws are nothing more than a set of mathematical models of reality and are to be retained only in so far as they correctly predict the outcomes of experiments. To the best of our knowledge, they have no existence outside the human capacity to create and process the symbols which encode this information.

There is simply no overlap whatsoever between this position and the set of immutable fairytales for adults which serve as a psychological crutch for the ignorant, impoverished or weak-minded who have either been thoroughly brainwashed as children or, in their dotage, are terrified by the inevitability of death and personal extinction. Or indeed, with any of their modern reincarnations such as the anthropocentrism promoted by Davies.

13. **D R Lunsford**
November 25, 2007

What Davies fails to get is the difference between faith and intuition. Faith is a choice of free will – intuition is something ineffable and innate – either you have it or not. The nearest religious analogy is "grace".

When enough people share the same faith, they can then organize and block competing faiths. Thus faith is essentially negative because it leads automatically to dogma. There is no possibility of "enough people having the same intuition". Those come to a only a few, sometimes contemporary (e.g. Newton and Leibniz, Gauss, Bolyai, and Lobatchevsky etc.). Intuition leads on to physical law if properly interpreted. It can be mis-interpreted (Kepler) and so is not "perfect" in a religious sense. It can subsequently be re-interpreted (Kepler again) and that is the stuff of heroism.

What has happened is – religion and metaphysics have been systematically discredited by several generations of physicists (which however does not make them unnecessary and vital), and they have now spilled over into physics.

-drl

14. **lylebot**
November 25, 2007

> The convention of accepting axioms and definitions, is an act of "faith". Thus all logic and deduction is rooted in faith.

No, the axioms are just taken as antecedents at the very first step in a chain of modus ponens. We don't have to believe that they're true to use them to reason.

15. **ekzept**
November 25, 2007

With all the possible variations on even DNA-based life, and the contingent pruning happening along the way from LIPS and the occasional space rock, I think the words of Loren Eiseley might be remembered:

> Lights come and go in the night sky. Men, troubled at last by the things they build, may toss in their sleep and dream bad dreams, or lie awake while the meteors whisper greenly overhead. But nowhere in all space or on a thousand worlds will there be men to share our loneliness. There may be wisdom; there may be power; somewhere across space great instruments, handled by strange manipulative organs, may stare vainly at our floating cloud wrack, their owners yearning as we yearn. Nevertheless, in the nature of life and in the principles of evolution, we have had our answer. Of men elsewhere, and beyond, there will be none forever.

That's from Eiseley's essay "Little Men and Flying Saucers" in The Immense Journey.

16. **Bad**
November 25, 2007

Not until today have I been familiar with Lubos Motl. Can someone briefly explain what the heck is going on there?

17. **Peter Woit**
November 25, 2007

Bad,

That's rather off-topic, and I'd rather this thread not turn into a discussion about Lubos, no matter how entertaining the topic might be.

Short version is that Lubos is a string theorist, considered by some one of the leading young people in the field, and until recently a junior faculty member of the physics department at Harvard. He has done more to convince people that there is something really wrong with string theory and how it is being pursued than anyone else. Also a fervent climate change denialist. For more, read his blog.

Sorry folks, please don't go on about Lubos in this comment thread.

18. **Yatima**
November 25, 2007

I think this discussions should be put on ice until the moment Theological Engineering finally manages to disburse Mana from vending machines or until we have regressed to the Dark Ages, whatever comes first.

19. **Marty Tysanner**
November 25, 2007

My reading of the essay was similar to that of Chris W, and so after reading Davies' piece I was surprised how many others had a very different perspective on his words. This may be partly be a case of getting out of the essay what one brings into it. For example, I am especially sympathetic to Davies' statement:

> It seems to me there is no hope of ever explaining why the physical universe is as it is so long as we are fixated on immutable laws or meta-laws that exist reasonlessly or are imposed by divine providence. The alternative is to regard the laws of physics and the universe they govern as part and parcel of a unitary system, and to be incorporated together within a common explanatory scheme.

Relatively few people I have talked with share this perspective, so perhaps that partly accounts for the apparent majority who dislike his essay.

DB presents what seems to be a common perspective:

> There is no belief system in science which is comparable to religion. […] a set of expectations, based solely on the experience to date that the interplay between hypothesis and experimentation yields a progressively refined and more powerful grasp of the workings of our universe. This is a perfectly rational stance which requires no belief system to underpin it.

This is a very defensible viewpoint as far as it goes. However, it overlooks the "faith" component of the process, at least in theoretical physics and cosmology, by ignoring the crucial role of initial assumptions. To give two examples (many others could also be given):

1. Searches for quantum gravity, or even more restricted attempts to explain why the standard model has the form and parameters that it does, almost invariably seem to assume from the outset that quantization of some kind is fundamental. By "fundamental" I mean underived rather than emergent from some process or configuration of more primitive non-quantized objects. Most knowledgeable theorists don't seem to deny the *possibility* of emergence, but neither do they take it seriously. It doesn't fit in with their own beliefs about Nature.

2. Minkowski spacetime as the "ground state" in general relativity. An alternative to this viewpoint is to see Lorentz symmetry as emergent from more fundamental objects; if one does not assume fundamental quantization either, then this emergence would need to be be from more primitive, *continuous* objects. Almost no one I have talked with seems to take interest in this possibility, but again it seems to come to a matter of faith.

These examples are not trivial, in that they determine the scope and direction of major research programs, and more insidiously, help define what theoretic directions lie outside the mainstream (and hence define where a tenure-minded individual should tread carefully).

I don't see these two examples of faith among theorists as being equivalent to faith in religion, *as long as there is recourse to experiment to test whether a theoretical framework represents Nature*; empirical tests are the central distinction between science and religion. With concrete feedback from Nature, wrong assumptions should eventually be uncovered, or else out of desperation new generations of physicists will try a different set of initial assumptions that may prove more fruitful than those that led to dead ends. Nonetheless, I think it is hard to argue that faith (or "belief," if one prefers that as a "nicer" word) doesn't play an important role in science.

20. **Chris W.**
November 25, 2007

To follow up on what Marty said, in science as in life we make choices and commitments that we hope will be successful, while knowing that at least some of them will not be. To do this requires some faith that at the very least *we'll be able to learn something from our failures*. As Davies pointed out, the universe doesn't have to be constructed in a way that permits this. Nature could be just screwing with our heads in utterly inscrutable ways:

> Therefore, to be a scientist, you had to have faith that the universe is governed by dependable, immutable, absolute, universal, mathematical laws of an unspecified origin. You've got to believe that these laws won't fail, that we won't wake up tomorrow to find heat flowing from cold to hot, or the speed of light changing by the hour.

Many working scientists (and others) may consider it silly to worry about such a possibility. Such people do not understand what it means to confront a truly deep problem in the natural sciences.

21. **dark-matter**
November 25, 2007

Paul has a book out lately based on certain metaphysical thesis, which is not selling well, and he needs to kick up a fuss to help out on the marketing.

22. **Leonard Ornstein**
November 26, 2007

lylebot, Chris W., and Marty:

For language to work, there has to be at least some kind of explicit or implicit commitment, by its users, to an agreed upon set of "axioms", rules, and definitions. It follows that the same applies for ALL rational systems. Such discipline is distinguishable from a system of theistic faith, ONLY in the required absolute commitment of religions.

As I noted above, and as was established by Hume, empirical induction from the part to "the whole" also requires UNSUPPORTABLE faith.

The criticism that science depends on faith is therefore a straw man!

Degree of commitment in science, usually varies with the preponderance of the evidence. Since Jerzy Neyman introduced the concept of "confidence intervals" in 1937, this commitment has become somewhat quantifiable. But even when we're talking about a 99.999% confidence (e.g., that associated with the measured mean magnitude of the fine structure constant), it's not absolute, and new observation may prove that our confidence has been misplaced.

It's this recognition, WITHIN science, that distinguishes its practitioners from priests.

Unfortunately, some scientists(?) believe they ARE priestly prophets, and it's the RESPONSIBILITY of other scientists, at times, to disabuse the public on such matters.

23. **Hans**
November 26, 2007

what I think most disgusting these days, is that even nobel laureates join such an anti-physics campaign.
Here is a talk for laymans of condensed matter physicist R.B. Laughlin, who states, that high energy physics is like religion:

http://www.physik.uni-muenchen.de/aus_der_fakultaet/kolloquien/physik_kolloquium/laughlin/laughlin261107.pdf

(I have seen only the first 15 minutes of the talk, and then suddenly found out, that i must leave the room).
It is depressing, when someone, who apparently knows nothing about quantum field theory of gravity, and what the problems are in high energy physics, says, without any good foundation, that all high energy phycisists are doing religion
(And this in front of a full audience of laypeople!)

24. **Peter Woit**
November 26, 2007

Hans,

I'm no fan of Laughlin's, but his comment about medieval religion is not original, but something Glashow and some other particle theorists have been saying for years. Unfortunately Laughlin is not completely wrong: there are theorists promoting ideas that are little better than religion, and this has given encouragement to those like Davies who like their religion and science together. You may be justified in walking out on Laughlin, but I think it would be helpful if you would also walk out on some of the pseudo-science talks on things like the anthropic landscape.

25. **Hans**
November 26, 2007

What Laughlin wants to say is more!
He does not criticise specific attempts to build a quantum theory of gravity.

He wants to say, that you cannot build a theory of quantum gravity at all.

He has a book in press on this. For Layman, here:
http://www.amazon.de/Abschied-von-Weltformel-Robert-Laughlin/dp/3492047181

You can criticise theories like string theory or LQG. But it is simply wrong if you seemingly have no knowledge of high energy physics at all, you stand in front of laymans and say that it would be in principle impossible to find a quantum field theory of gravity (with no accurate scientific argument) and that all attempts are to be fruitless.

His wrong argument was, that for systems with a small number of particles, collective laws would break down. Therefore, he thought, you could not define any quantum field theory at small scales.

He seemingly did not know, for example, that high energy physicists would be very fine if they would have a quantum gravity that works at the scales where the standard model gives good results.

26. **Zathras**
November 26, 2007

As usual, there is a huge amount of inaccuracies in this discussion about what “faith” or “belief” is, and this misrepresentation completely skews the analysis.

Faith is most emphatically not a set of axioms held without evidence. Put crudely (there is no other way) faith is a feeling about things. It is synthetic, rather than analytic, in that it is based on some sort of continuum of how one feels about the world (notice you cannot really define faith without using words such as belief or feeling.

As Nietzsche observed, “philosophy allows us to rationalize what we already believe.” The axiomitization is the outcome of this rationalization process. The axioms of faith discussed here are just a by-product, not the core of what belief is.

At least part of what I think Davies is talking about here is the faith in progress. There is of course no single axiom that embodies this idea of progress for every single scientists, or probably even a majority of scientists. Scientists do what they do because they think that they can make or contribute to the progress of understanding the universe.

As DB says, “*What Davies claims to be science’s belief system is in fact a set of expectations, based solely on the experience to date that the interplay between hypothesis and experimentation yields a progressively refined and more powerful grasp of the workings of our universe*.” However, the scope of perceived past progress varies from scientist to scientist, and therefore so does the belief in
attainable future progress. No scientist has all the information of how science has progressed, or failed to progress, so far, so there is a different set of information which shapes each scientist’s faith in progress. A scientist from a subfield which has been stagnant for years will come to a very different conclusion than someone whose field has seen rapid advances.

27. **Peter Woit**
November 26, 2007

All,

Please, enough, I can’t stand it any more. Take the highly general discussion about science and belief etc. to one of the dozen or so other blogs featuring discussion of this topic.

28. **Leonard Ornstein**
November 26, 2007

Peter:

You started this “Letter to the Editor” topic with Davies’ title “Taking Science on Faith”.

Now you object to “the highly general discussion about science and belief etc.”!

A precise general discussion of the relationship between “Science” and “Faith” is just what’s needed to understand Paul Davies’ “mistake”. We may individually fail in our attempts to add to clarity. But you should be ready to put up with this kind of effort. Otherwise, why start the topic?

29. **Peter Woit**
November 26, 2007

Leonard,

I think the confusions of Davies on this issue have been dealt with more than sufficiently here and elsewhere. Further elaboration of this topic seems to me a waste of time and likely to convince anyone who comes to this blog looking for something worthwhile and interesting to read that they’re in the wrong place. So, please, enough about the Davies confusions.

30. **milkshake**
November 26, 2007

The salesmanship about far-off ideas in theoretical physics and cosmology can produce a wooly impression how the science normally works. Davies is trying to be provocative but he actualy sounds like a Jesuit seminary kid.

I think if the wheelchair luminaries were saying more frequently “we are trying to find out how this Universe works behind the scene” and “We don’t really know for sure, we postulate all kinds of ideas but in the end we take it the way it comes out” the public would get a lot less elevated view on natural laws and the fontiers of our understanding – and there would be less opportunity for this metaphysical gorp.

31. **D R Lunsford**
November 27, 2007

There’s a wonderful “Letter to the Editor” today, by Chance Reschke of Seattle, who states:

Condemning science for its failure to explain the divine makes as much sense as condemning Kant for failing to explain the aerodynamic properties of the Concorde, or Moses for failing to predict Google.

Beautiful!

-drl

32. **Belizean**
November 27, 2007

Getting back to physics, does anyone else have a problem with Davies use of the word "multiverse" in connection with the Anthropic Principle? While his usage is "correct" in that it's the one that has in the last few years come dominate particle physics, the word predates the current hyping of the Landscape.

Multiverse used to mean the ensemble of universes in the Many Worlds Interpretation of quantum mechanics. The AP and MWI multiverses are not the same. Unlike the AP multiverse, the multiverse of the MWI requires its constituent universes to interfere with each other, and the prevalence of any particular universe in the multiverse generally changes with time.

Yes, this is a bit OT, but a source of irritation nonetheless.

33. **manyoso**
November 29, 2007

I can imagine the smackdown Richard Feyman would give to Davies were he still alive…

Some quotes from Genius: The Life and Science of Richard Feynman:

"The scientist has a lot of experience with ignorance and doubt and uncertainty, … we take it for granted that it is perfectly consistent to be unsure–that it is possible to live and *not* know. But I don't know whether everyone realizes that this is true."

and…

"He believed in the primacy of doubt, not as a blemish upon our ability to know but as the essence of knowing. The alternative to uncertainty is authority, against which science had fought for centuries. "Great value of a satisfactory philosophy of ignorance, " he jotted on a sheet of notepaper one day. "… teach how doubt is not to be feared but welcomed."

and …

"You see, one thing is, I can live with doubt and uncertainty and not knowing. I think it's much more interesting to live not knowing than to have answers which might be wrong. I have approximate answers and possible beliefs and different degrees of certainty about different things, but I'm not absolutely sure of anything and there are many things I don't know anything about, such as whether it means anything to ask why we're here…

I don't have an answer. I don't feel frightened by not knowing things, by being lost in a mysterious universe without any purpose, which is the way it really is as far as I can tell. It doesn't frighten me."

34. **Kralizec**
December 4, 2007

> I would think they might be even more intimidated by P.Z. Myers, who reaches rhetorical heights I can not aspire to, referring to the Anthropic Principle as that *tiresome exercise in metaphysical masturbation that always flounders somewhere in the repellent ditch between narcissism and solipsism*.

Mr. Myers seems to have written as if his feelings are a proper standard for rejection or acceptance of opinions. I doubt we really think one's fatigue, boredom, hatred of idle pleasure, or repulsion from either self-loving or candidly lonely observers is grounds for rejecting or affirming an anthropic principle, or any other opinion. Most everyone writing here seems closer to thinking that an opinion is true or false irrespective of the way one feels about it or about the possible motives of anyone who may hold it.

Not Yet About Geometric Langlands…

November 26, 2007
Categories: Uncategorized

Tomorrow morning I'll head down to Princeton to attend the conference on Gauge Theory and Representation Theory at the IAS. Unfortunately I had to miss the first day of the conference (today), since I would have liked to have heard all the talks, most especially that of Dennis Gaitsgory on local geometric Langlands. Maybe someone who was there will explain to me what he talked about.

That might be even better than attending the lecture, since Gaitsgory's pedagogical style seems to be rather daunting. Here is an article about his experience teaching linear algebra, and the Harvard Crimson last year ran this frightening account of what it was like to take Math 55 from him. Math 55 is a legendary honors math class for the most fanatical first-year students, and I have fairly vivid memories of my own experience with it (that year it was taught by Konrad Osterwalder and John Hubbard). From what I remember, the first row of the class was occupied by a sizable proportion of the winners of the previous year's Math Olympiad, and being a rather average student in a math class was a new experience for me. The textbook for the course was a remarkable book by Loomis and Sternberg with the somewhat misleading title *Advanced Calculus*. It's now available on-line. Osterwalder made a valiant effort to follow the text during the first semester, while Hubbard more-or-less winged it the second semester, entertaining us by going over in class research papers on dynamical systems and assigning us Spivak's *Calculus on Manifolds* as something to work through during the reading period (about a week long) before final exams. Both Osterwalder and Hubbard seem to have been much mellower sorts than Gaitsgory though, since I remember working fairly hard on puzzling out problem sets, but also having a life with quite a lot of other things going on, nothing at all like the experience described in the Crimson article. Kids these days.

The first talk tomorrow morning is supposed to be Maldacena on integrability in N=4 SSYM. He really should be celebrating the day as the 10th anniversary of his amazing paper The Large N Limit of Superconformal Field Theories and Supergravity, which announced the AdS/CFT conjecture and was submitted to the arXiv on November 27, 1997. Work on this conjecture has dominated particle theory in a remarkable way over the last ten years. According to SPIRES, the paper has amassed 4897 citations, at a rate which has only accelerated in recent years, with 551 citations in 2006. It is now the third most heavily cited paper in particle physics, behind only those of Kobayashi-Maskawa and Weinberg. A simple extrapolation suggests that in another four years or so it should become the most heavily cited particle physics paper in the history of the multiverse. Several conferences are celebrating the anniversary, including one next month in Buenos Aires, and another in Fort Lauderdale. Davide Castelvecchi has a quite good popular article on the subject in Science News.

After it's over, I'll try and write something about the main topic of the conference, geometric Langlands. In the meantime, my ability to keep the comment section under control may be impaired. Behave.

Update: David Ben-Zvi is putting up his notes from the talks here.

Comments

1. **Charles**
   November 26, 2007

   He didn't talk about Local Geometric Langlands at all, and focused on localization of Kac-Moody algebras (I'm basing this largely on the fact that he said this at the beginning of his talk, I didn't follow very much of it).

2. **JC**
   November 26, 2007

   Peter,

   Not quite on-topic

   Just wondering. How many people finished math 55 in the year you took it?

   I didn't go to Harvard for undergrad, but in my freshman year I ended up doing sort of a mini-DIY version of "math 55" by taking the "honors" level real analysis and abstract algebra courses. (I finished most of the freshman + sophomore "non-proof heavy" math courses before I started university). The textbooks assigned were ones like Rudin and Royden for analysis, and Lang and Hungerford for algebra. The "honors" level courses also had a reputation for high drop rates.

   This was the first time I ever had to put a lot of time into any math courses, where I ended up almost completely burning out. This was also on top of also simultaneously taking the "weedout" freshman physics courses.

3. **Peter Woit**
   November 26, 2007

   Charles,

   Thanks. Here "localization" is the sort of geometric construction of representations I've always been interested in trying to connect to QFT, I'm sorry I didn't get to hear the talk (although I might have also got lost when he went into the derived category…). By the way, I just got what looks like an excellent book in the mail when I got home: D-modules, perverse sheaves, and representation theory, by Hotta et. al. Lots about D-modules, and it shows explicitly how they are used in representation theory. No Kac-Moody groups, just the finite-dim theory, but it looks quite readable, unlike almost everything else in this subject. Something to read on the train tomorrow…

   JC,

   From what I remember, there were about 40 the first semester, 20 the second. The first semester I think I was an above average student in the class, the second semester, not so clear….

4. **Chuck**
   November 26, 2007

   I just got that D-modules and perverse sheaves book too — looks good, I agree! I saw Gaitsgory give a talk at MIT a few weeks back and it was interesting, for the rather small percentage I understood . . . . He is a pretty good lecturer, although he moves rapidly (as one would expect).

   His work (with Frenkel) on localization of modules for Kac-Moody algebras connects somehow to their formulation of local Langlands in characteristic 0. Frenkel's book on the subject is not easy to read, although it seems as though if I could get through it I'd understand what they're trying to do.

5. **John**
November 26, 2007

Hi Peter,

I must stereotypically respond, “Wow!” It’s not an easy feat to take Math 55. Although I am not in Harvard, I have heard of Math 55’s status among college-level math courses.

I’m somewhat curious to know if the course is more a graduate-type abstract math course or a high school Math Olympiad-type course, or somewhere in between?

I’m quite interested in finding out as, since you are a graduate of the course, you (of any persons I know) would be able to provide some feedback to the following query – Does a course such as Math 55 help develop professional math skills from an early point onwards? i. e. would one be able to write one’s own research work (if albeit not completely professional) after completing the course?

Thanks,
John

Side-note: I have Sternberg’s and Rudin’s books, but somehow I found them too formalistic (and/or opaque) to learn from *and* understand the motivating reasons for the math simultaneously.

Can you suggest a more suitable option?

6. **Peter Woit**
November 26, 2007

Chuck,

I’ve been spending a lot of time reading the Frenkel book, slowly understanding the details of what they’re doing. I find him relatively easy to follow, although it took me a while to get a feel for what he is trying to do and to see how some of the ideas fit together (there’s still a lot I haven’t understood). He’s a good expositor, especially compared to some other people in this field… I guess a very specific form of my question about Gaitsgory’s talk would be: what did he talk about that’s not explained in Frenkel’s book?

7. **JC**
November 26, 2007

Peter,

What exactly made the 2nd semester slightly harder for you?

8. **Peter Woit**
November 26, 2007

JC,

The second semester wasn’t harder, actually I think Hubbard’s teaching style was such that you could get by with less work. But the twenty people who had dropped after the first semester were mostly not the best students in the class, so the twenty that remained were on the whole an impressive group. Luckily I’m not the competitive sort, otherwise I might have really not enjoyed that experience. But, in any case I was far more interested in my quantum mechanics class that semester, the beginning of a life-long love affair…

9. **JC**
November 27, 2007

Peter,

I didn’t know what my exact rank was in those honors level real analysis and abstract algebra courses I took in my freshman year. Through from I can recall anecdotally, I do remember there were at least two or three other folks who consistently performed better than me on assignments and exams, judging from a casual search of the piles of graded stuff. (The grader just left our graded assignments and exams on the front table for us to pick up ourselves, where it was easy to spot the ones with the better grades).

I was sort of a competitive type back in those days, which was one of the reasons I ended up not majoring in pure math. Another big reason was that I didn’t make it onto the Putnam exam team during my freshman year. It may sound silly now in hindsight, but it was a huge devastating blow to my ego at the time.

After my freshman year was over, I spent some idle time in the university library and came across several books on topics like engineering dynamics, particle physics, quantum mechanics, fluid dynamics, etc … which really grabbed my interest and attention. (I already had enough math background to be able to follow what these books were explaining at a basic rudimentary level). It took me awhile to mellow out from the hyper-competitive mentality, and eventually decided to change my major to physics. In hindsight, I’m glad that I found out early on as to what I was NOT interested in majoring in.

(This is getting off-topic, so I’ll stop here).

10. **odo**
November 27, 2007

John,

Math 55 is designed to prepare students to be professional mathematicians. It is not at all a math olimpiad problem solving course. It would normally cover the basics of real and complex analysis, some basic functional analysis (e.g. spectral theorem for compact operators), point set topology, introductory algebra, maybe some elementary Riemannian geometry (ala Spivak and do Carmo). The handful of students in it are usually exceptional; the year I was a freshman at Harvard the students in it included one who made full prof at Princeton before turning 30, one who is an associate prof at MIT, one who is at Stanford, etc. It’s full of the super fast thinkers who know a lot also.

In general it probably does not provide adequate background for doing research; no freshman course does. In normal circumstances even a very talented, hard-working kid would need several more years of courses before being able to do much of interest to researchers. Of course there are exceptional individuals who do research even in high school, someone like Drinfeld, but these people are anomalous, and their parents probably ought not to be emulated.

11. **Chuck**
November 27, 2007

Gaitsgory's talk (at least the part I kinda-sorta understood) started with the ind-scheme of opers on the formal punctured disc and its relation to representations of the corresponding Kac-Moody algebra (of the Langlands dual group) at the critical level. I think this stuff is in Frenkel's book. I had a hard time following what he did after that though. I think the issue for me is that all of these topics — opers, representations of Kac-Moody algebras (and the completed enveloping algebra) at the critical level, localization of modules on finite or affine flag varieties, hecke eigensheaves, moduli stacks, etc — are not too awful to get an intuition for individually; but I get very confused as to how they're all supposed to fit together to form a big-picture whole.

12. **Zathras**
November 27, 2007

From the piece on teaching linear algebra:

"I found the job annoying for two reasons. First, the students were primarily non-math majors."

This brings up one of my biggest bones to pick with mathematicians. Their insularity knows no bounds. While most fields have become more accepting of interdisciplinary work, mathematics has developed a significant population who are absolutely against any such work. Barbeckiism and abstraction are what is valued, not clarity, and certainly not application. For this reason, more and more engineering and science department are moving towards teaching their students calculus and other mathematics, just because mathematicians think it's their way or the highway.

13. **Peter Woit**
November 27, 2007

John,

I hope odo's comment helped answer your questions. The course is just an undergraduate course, although a fast-paced one, it's still a long ways from research-level math. One thing it does for students is to give them enough background so that they can start taking some of the basic grad courses during their undergraduate years, which does get them closer to the point of being able to get into research early in their graduate careers.

I thought it was an ideal course for bright, intellectually ambitious students, throwing at them at much as they can handle. The Crimson article may have been exaggerated, but I don't think it's a great idea for first-year undergrads to be spending almost all their time on a math course, no matter how good. A great university like Harvard offers students so many wonderful opportunities to learn many different kinds of things, and they should take advantage of this. As well as taking advantage of being young and irresponsible…

After Math 55, as an undergraduate I took mostly physics courses, only a few math classes, including one graduate course. Most of my math education came later in life, not through taking courses (sometimes through teaching them…). As for books, Loomis/Sternberg is quite a document, but not so great pedagogically. Among undergrad analysis textbooks I've seen, the recent series from Princeton looks good, although I haven't looked that closely at the books.

14. **jasper**
November 27, 2007

Zathras,

While one might aspire to be able a teach a top-rated course to people of all backgrounds, my (limited) experience has been that connecting with students who are interested in the topic that you're teaching is much easier than fighting the very uphill (but worthwhile, of course) battle with students who are forced to take your course as a requirement… This is very much in line with the tone of the rest of Gaitsgory's anecdote.

15. **Richard**
November 27, 2007

Peter,

I also thought that spending 30-50 hours per week on problem sets for a single class is a bit excessive, and wondered if they're learning anything else. On the other hand, I do like the idea of exposing the students to a lot of material early in their education so that they can see what's coming and appreciate better what they're learning in the context of the entire body of math. Perhaps this sort of thing is more suited to some kind of intensive summer program.

16. **David Ben-Zvi**
November 27, 2007

Hi — I'm posting notes to many of the talks
(in particular so far Gaitsgory, Beilinson, Ginzburg and Maldacena)
as we go along at
http://www.math.utexas.edu/users/benzvi/GRASP/lectures/IAS.html
(available off my GRASP lecture notes page also) — many of them are transparency talks and I don't even try with those, but
hopefully some of the transparencies will be posted later.

Gaitsgory's talk discussed two realizations of representations
of Kac-Moody algebras: as D-modules on affine flag manifolds
(localization) and as coherent sheaves on spaces of Langlands parameters. The equivalence between the two is an important
case of the local geometric Langlands conjecture.
I haven't checked carefully but I think the new results with Frenkel
(not covered in Frenkel's book) involve the localization
on affine flags (I think their earlier work was on affine Grassmannians) and were presented with a more derived-algebraic-geometry viewpoint.

17. **Peter Woit**
November 28, 2007

Thanks David. Congratulations on a fascinating talk today, I enjoyed it!

18. **surlygrad**
November 30, 2007

Was Hubbard eccentric back then? He was pretty off his rocker when I took the freshman honors class with him at Cornell.

19. **woit**
November 30, 2007

I wouldn’t say “off his rocker”, but definitely a bit eccentric, while highly enthusiastic. I found him rather entertaining, some of the more serious students in the class were a bit put off…

Geometric Langlands and QFT

November 30, 2007
Categories: Favorite Old Posts, Langlands

Wednesday's session at the IAS Conference on Gauge Theory and Representation Theory was mostly devoted to talks by Witten and his collaborators about their latest work on the approach to relating geometric Langlands and QFT that he has pioneered over the last couple years. The talks were quite understandable, giving a general overview rather than details of what are some very technical topics, about which the speakers have produced recently some very long papers. Before discussing the talks, I'll try and explain the background of this line of inquiry into the borderlands between mathematics and physics.

The history of this subject goes back thirty years, to a 1977 paper of Goddard, Nuyts and Olive entitled Gauge Theories and Magnetic Charge. In the GNO paper the authors noted that in a gauge theory with group G, while the electric charges take values in the weight lattice of G, the magnetic charges take values in the weight lattice of a "dual" group, which is now generally called the Langlands dual group ${}^LG$. This group was used by Langlands in a crucial way in conjectures about number theory that go back to a letter of his to André Weil in 1967. Also in 1977, Montonen and Olive, in Magnetic Monopoles as Gauge Particles?, conjectured the existence of a dual gauge theory interchanging electric and magnetic charges, and the gauge groups G and ${}^LG$. At the time Witten was a Harvard postdoc, and on a visit to England at the end of 1977 Atiyah told him about this conjecture and first suggested it might have something to do with the Langlands program. Witten met Olive, and they collaborated on the 1978 paper Supersymmetry Algebras That Include Topological Charges where they suggested that Montonen-Olive duality would be most naturally realized in a supersymmetric gauge theory. Later work showed that it is N=4 supersymmetric Yang-Mills that seems to have this duality property, now called S-duality and extended to not just a $\mathbf{Z}_2$ symmetry, but a much larger symmetry under the group SL(2,**Z**).

Warning: *What follows is an absurdly overly simplified discussion that will offend pretty much every mathematician who really knows the subject. Comments correcting anything that isn't at least in some vague sense more or less morally right are welcome.*

From the 1970s on, work on conjectures growing out of the Langlands program has come to be one of the dominant themes of number theory, achieving a fantastic success with the work of Wiles on one such conjecture, the so-called modularity conjecture, that led to the 1995 proof of Fermat's Last Theorem. Trying to explain the Langlands program in any detail is a huge task, but I'll try and give a few very vague indications here of what it is about. The field **Q** of rational numbers can be thought of as "rational functions", on a "space" called Spec (**Z**), whose "points" are the prime numbers and a special "point at infinity". Number fields are extensions of **Q**, and can be thought of as corresponding to covering spaces of Spec (**Z**), characterized by Galois groups, in particular the Galois group Gal(**Q**) of the algebraic closure of **Q**, which in some sense is the fundamental group of Spec(**Z**). Many questions in number theory can be expressed as questions about "Galois representations", representations of Gal(**Q**) in complex Lie groups such as G=GL(n,**C**). Thinking of Spec(**Z**) as a "space", representations of Gal(**Q**) correspond to local systems, i.e. flat vector bundles over Spec(**Z**).

The Langlands program has both a "local" and a "global" aspect. The "local" aspect restricts attention to the neighborhood of a "point" in Spec(**Z**), and the corresponding "local field" of functions. For the "point at infinity", the local field is the real number field **R**, for a "point" corresponding to a prime number p, it is the field called $\mathbf{Q}_p$. The local Langlands conjecture gives a correspondence between representations of Gal($\mathbf{Q}_p$) into a complex Lie group G and complex representations of the corresponding algebraic group ${}^LG$($\mathbf{Q}_p$) with $\mathbf{Q}_p$ coefficients. This correspondence matches up information on both sides that characterizes the representations, which can be expressed either in terms of L-functions, or in terms of the action of Hecke algebras. One can read this correspondence as possibly giving information in both directions: if you know the Galois representations, a so-called "arithmetic" problem, you get a parametrization of the irreducible representations of a Lie group, a so-called "analytic" problem. If you know about the Lie group representations, you get information about number theory.

In the global version of the Langlands correspondence, on the arithmetic side, the global group in question is just Gal(**Q**), and its representations in a Lie group G are central objects in number theory that one would like information about. On the analytic side, the global group is much trickier to describe. What one needs is something like a gauge group for bundles over Spec(**Z**), but remember that each "point" of this "space" has a different nature. One introduces an object called the "adeles" $\mathbf{A_Q}$ that puts all the local fields together, and then uses this as the coefficients in an "adelic" group ${}^LG(\mathbf{A_Q})$, that perhaps can be thought of as the gauge group of all changes in local trivializations about each "point" in Spec(**Z**). The representation theory on the analytic side is then harmonic analysis on this adelic group, with irreducible representations characterized by specific functions which are called automorphic forms (so this side of the correspondence is often called the "automorphic" side). Galois representations and automorphic forms are matched up by, equivalently, L-functions or the eigenvalues of the action of a Hecke algebra. For the case of 2d representations, the automorphic forms involved are very classical functions on the upper-half-plane, and readily computable information about the coefficients of their Fourier expansions gives deep information about number theory.

An important idea in number theory/algebraic geometry is that algebraic curves over a finite field $\mathbf{F}_p$ have many similar features to the "spaces" like Spec(**Z**) that characterize number fields. Functions on such curves give so-called "function fields", which behave very much like number fields, and one can transform number theory questions into analogous questions about these curves. For example, there is an analog of the Riemann hypothesis in the function field case, where it has been proven. One can translate the Langlands program conjectures into the function field setting, and there proofs have been found, for the global case by Drinfeld (rank 2 case) in 1974, and Lafforgue (higher rank) in 1999.

Given the Langlands correspondence for an algebraic curve over a finite field, a natural question is whether there is anything analogous if one replaces the finite field by the complex field, and works with complex algebraic curves, i.e. Riemann surfaces. In 1987 Witten wrote a beautiful paper entitled Quantum Field Theory, Grassmannians, And Algebraic Curves, where he explains how one can think of the holomorphic sector of a conformal field theory on a complex algebraic curve as giving something like an automorphic representation in this context, analogous to the ones studied using adeles for algebraic curves over finite fields. He mentions the Langlands program, but makes no attempt in this paper to describe what would be the analog of the Langlands correspondence.

Several years later, around 1995, Beilinson and Drinfeld formulated what is now known as the geometric Langland correspondence, giving a specific conjectural correspondence that is supposed to be an analog for a complex curve C of what happens in the function field case. On the analog of the arithmetic side, one just has a representation of the fundamental group of C in a Lie group G, i.e. a flat vector bundle. The automorphic side is much trickier, and they define "Hecke eigensheaves" on the moduli space of ${}^LG$ bundles that play the role of automorphic forms. In their massive (384 pages at last count), unpublished and still preliminary paper Quantization of Hitchin's integrable system and Hecke eigensheaves, they write

> We would like to mention that E. Witten independently found the main idea and conjectured . As far as we know he did not publish anything on the subject.

Since the mid-1990s, a lot of mathematical activity has grown up around these ideas, creating a new field that is now generally known as "Geometric Langlands theory", which connects to a wide range of different kinds of mathematics, and to physics via conformal field theory. With funding from the US Defense Department DARPA program, various workshops were organized that brought physicists and mathematicians together to discuss this subject. One such workshop was held at the IAS in March 2004, and there Witten gave a talk (see the end of these notes) about N=4 supersymmetric Yang-Mills and its dimensional reduction to a non-linear sigma model in two dimensions. He credits David Ben-Zvi with explaining to him crucial facts which made clear to him that what was needed to connect this to geometric Langlands was the introduction of boundary conditions in the sigma model, i.e. branes.

Witten first unveiled his version of geometric Langlands based on N=4 supersymmetric Yang-Mills in a talk at the beach at Stony Brook in August 2005; here are notes and audio from the talk. In April 2006 his 230 page paper with Kapustin, Electric-Magnetic Duality And The Geometric Langlands Program appeared, giving the details of a construction based on a topologically twisted (using the "GL twist") version of N=4 supersymmetric Yang-Mills, dimensionally reduced to give two topological sigma models with target space the Hitchin moduli space, for group G in one case, ^LG the other. These two models, known as the A and B model, are related by mirror symmetry. They involve boundary conditions and thus branes in two-dimensions, and as a result are related by what mathematicians now refer to as “homological mirror symmetry”. The fact that the Hitchin moduli spaces for G and ^LG could be thought of as mirror partners was shown earlier by my colleague Michael Thaddeus in work with Tamas Hausel.

Late last year Witten and Gukov’s 160 paper Gauge Theory, Ramification and the Geometric Langlands Program appeared, extending the QFT approach to geometric Langlands to the “ramified” case, which is that of a punctured Riemann surface, with non-trivial monodromy about the punctures. This was about the “tamely ramified” case, involving simple pole singularities at the punctures. Last month two new papers totalling 193 pages by Witten on this subject appeared, Gauge Theory And Wild Ramification, which deals with the case of higher order poles, and Geometric Endoscopy and Mirror Symmetry, written with mathematician Edward Frenkel.

The talks by Witten and Frenkel gave very general introductions to the two papers, notes taken by David Ben-Zvi are here and here. Witten mostly just explained the background for the wild ramification problem, not giving any details of how he solved it, so his talk mainly functioned as a good introduction to his recent paper. Frenkel also gave a talk which was more of an introduction to his recent joint paper with Witten. He explained that they were studying a special case of the question of what happens at singularities of the Hitchin fibration, for the simplest kind of singularity (orbifold), and simplest non-trivial case (G=SL(2), ^LG=SO(3), outlining the phenomena that appear. These phenomena are analogous to well-known phenomena in the number field case, where their study goes under the name of “endoscopy”. This part of the Langlands story has recently seen major progress, with the proof by Ngo of what is known in the subject as the fundamental lemma. Ngo is giving a series of talks at the IAS this semester on the subject, and Frenkel promised to give a talk next week about possible relations of what he and Witten have been doing to the work by Ngo.

For the story of a comment by Pierre Deligne during this talk, see this posting by Ben Webster.

To me the most interesting talk was Sergei Gukov’s on *D-branes and Representations*, in which he described what he is working on with Witten at the moment; no paper has yet appeared. Ben-Zvi’s notes are here, and Gukov gave much the same talk recently at Santa Barbara, notes here, audio here. I’ve been most interested in geometric Langlands because of its relations to 2d QFTs and representation theory, where the simplest story should be seen in the local version of the theory. Also, Gukov’s argument was based upon getting Chern-Simons theory out of the original 4d N=4 GL-twisted SYM theory using boundary conditions (something he didn’t explain other than saying what the boundary conditions are). I’ve always wondered whether it is possible to get Chern-Simons out of some sort of possibly supersymmetric twisted theory involving fermions. Someone in the audience asked if what he was doing gave such a theory, but he somewhat evaded the question, saying he preferred to think of things in 4d with boundary.

Getting down to two dimensions, he said that the Hilbert space of this Chern-Simons theory gave a representation associated to the punctured disk, and mentioned that this was related to local geometric Langlands. Someone asked “what happens on the boundary of the disk?”, and he answered that one only needed to impose boundary conditions at the puncture, not on the boundary. Greg Moore sputtered something like “really, on the boundary of the disk you don’t need boundary conditions??” (for the usual story about this, see this paper, which Greg co-authored), to which Gukov answered something about it being all right since they were only looking for supersymmetric BPS states. He went on, as one can read in the notes, to discuss a way of producing representations of a compact Lie group G (and its complexification and other real forms) that associates Harish-Chandra modules to A-branes on the cotangent space to the flag manifold, working out the details for SL(2, **R**). At the beginning of the talk, Gukov claimed that this was all leading up to a classification of the admissible representations of a real semi-simple Lie group in terms of D-branes, with the various geometrical constructions (e.g. D-modules) known to mathematicians just different faces of the same physical model. To me, the talk raised all sorts of interesting questions, so I’m looking forward to seeing the details when Gukov and Witten have a paper ready.

Comments

1. **Coin**
   November 30, 2007

   Hm, okay. So this is all really interesting from a mathematical perspective. What isn’t quite so clear to me is exactly what these things are expected to be used for in the context of physics. You say you see potential uses in geometric Langlands’ “relations to 2d QFTs and representation theory”; what is it that Witten et al hope to use geometric Langlands for? I’m particularly curious about this “ramification” thing; apparently Witten has something physical in mind when he works on those, since both of the papers on that subject which you cite him as being an author on have “Gauge Theory” in the title. Is there some particular set of physical problems which this punctured disk corresponds to, and Gukov and Witten hope to use the Langlands tools to analyze that problem? Or is this just a general set of tools they’re trying to develop? You say Gukov is using the punctured disk to get answers about “chern-simons” theories; is this the angle everyone is taking or just one of Gukov’s personal applications?

   Or I guess a clearer version of what I’m trying to ask might be this. The Witten and Gukov paper says in the introduction, just before it loses me completely, the following things:

   > The simplest version of the geometric Langlands correspondence involves, on one side, a flat connection on a Riemann surface C, and, on the other side, a more sophisticated structure known as a D-module…
   > The basic idea is that allowing ramification in the Langlands program corresponds, in gauge theory, to introducing surface operators, somewhat analogous to Wilson or ’t Hooft operators, but supported on a two-manifold rather than a one-manifold…
   > The gauge theory approach to geometric Langlands is based on N = 4 super Yang-Mills theory twisted and compactified on a Riemann surface.

   What I’m trying to figure out is exactly what it is the correspondence described here is applying to. As I understand all this so far the idea seems to be that you have a gauge theory that lives on a Riemann surface, and this surface has these thingies that you described as “punctures” and the paper describes as “surface operators”. The goal is to use the Langlands duality to convert this surface+(punctures/operators) to d-modules+ramification, where hopefully they’ll be easier to analyze. Okay, that makes sense, but as someone not very familiar with gauge theory what confuses me is, what exactly “are” the surface operators introduced on the gauge theory side? What physically are they supposed to be representing? Are they… particles, or what?

   One more question, you say “For example, there is an analog of the Riemann hypothesis in the function field case, where it has been proven.”. Given the known Langlands correspondence, why does this not give us a proof of Riemann in the number case? (Or is the Langlands correspondence just a conjecture or something?)

   Anyway thanks for this interesting writeup!

   PS, typo:

   In 1997 Witten wrote a beautiful paper…

   Should be 1987

2. **Peter Woit**
   November 30, 2007

   Coin,

   As far as I know, there’s no direct motivation from physics here at all. Gukov and Witten do mention the possible relevance of surface operators to problems in physics, but that's not why they are studying them. What they are doing is uncovering a relation between quantum gauge theory and some very deep ideas about mathematics. Whether what they learn this way will ultimately tell us something about physics is not known. That's not what they're aiming for right now, they're looking at the mathematics implications. Personally I think there's a lot more to come in this area. In the long run it may change how we think about quantum gauge theories, and the standard model is a quantum gauge theory. That's plenty of reason for people to work on this.

About the Riemann hypothesis. First of all, the Langlands correspondence is not really relevant to it. In the function field case the proof of the analog of the Riemann hypothesis doesn't involve the Langlands story. Secondly, I didn't try and discuss what is proved and what isn't within the Langlands program. On the whole, especially for number fields, this is still a subject where there are far more conjectures than proofs, which is why it is such a mathematically active subject. The things Drinfeld and Lafforgue proved for function fields remain still conjectures in the number field case.

3. **Coin**
November 30, 2007

   I see, thanks.

4. **Thomas Love**
November 30, 2007

   Peter, Thanks very much for the clear introduction to the ideas behind Witten's program. Now that I know that it is about the relation between electric and magnetic charges, I am interested. Whether my motivation will be enough to read the several hundred pages remains to be seen.

5. **milkshake**
November 30, 2007

   just curious: Was the current focus of Wittens work, on general math problems (that eventually might or might not connect to physics) brought about by the difficulties of ST?

6. **Michael**
November 30, 2007

   That's great that DARPA is funding relatively pure maths. Perhaps it has near-term cryptographic or physical applications?

7. **David Ben-Zvi**
November 30, 2007

   Peter,
   Nice summary! I wanted to add that the geometric Langlands program
   goes further back, at least to a paper of Laumon in Duke 54 (1987)
   where he explains the general program for GLn, inspired by Drinfeld's proof of the (not yet formulated) geometric Langlands conjecture
   for GL2 in 1983 (Amer.J. Math), inspired by Deligne's sheaf-theoretic proof of geometric class field theory in SGA. Interestingly the same
   Duke 54 volume (the Manin birthday volume) contains Hitchins paper defining the Hitchin system, which we now know is intimately related..
   Beilinson and Drinfeld then developed the complex version
   (in the language of D-modules rather than perverse sheaves and
   using many ideas of conformal field theory, which I believe they came at independently of Wittens wonderful paper that you mention – Manin, Beilinson, Drinfeld,
   Schechtman, and their collaborators were deeply involved in trying to understand the algebraic structures behind conformal field theory from the mid 80s.
   The other main necessary ingredient for geometric Langlands
   for general groups is the geometric Satake correspondence,
   with a complicated history starting from work of Lusztig in the 80s and involving Ginzburg, Drinfeld and finally Mirkovic and Vilonen.
   These days it has become quite an industry!

8. **David Ben-Zvi**
November 30, 2007

   Regarding DARPA, I dont know of any imminent applications
   to coding, defense, etc but I am very proud of finding the relation
   to antiterrorism, cf my Geometric Langlands page.

9. **Domenic**
December 1, 2007

   Wow, thank you very much for the writeup, Peter. This is so far beyond my current level of knowledge, and so it's great to have a nice broad overview of the subject. It gives me something to shoot for, comprehension-wise. (I want to be a mathematical physicist "when I grow up," precisely because of these sorts of deep connections between physics and mathematics, and what they can tell us about how to solve the hard problems that have stymied physicists for the last n years.)

10. **Peter Woit**
December 1, 2007

    David,

    Thanks for filling in some of the history!

    One thing I didn't get around to adding to this posting was a list of references about where to read more. The best advice though is to go to David's geometric Langland's site, and take a look at his expository talks on the subject, together with the several long expository articles by Ed Frenkel.

11. **Peter Woit**
December 1, 2007

    milkshake,

    It's certainly my impression that if Witten had any ideas about how to make progress in string theory, he would be working on those. Unlike string theory, where people do seem to be stuck, the QFT/geometric Langlands field is one where there's a lot for someone with Witten's talents to do, and he seems to be enjoying working on this.

12. **M**
December 2, 2007

Peter,

Great post, but like most of this new number theory / physics esoterica I don't have a clue what's going on. But thanks for the post anyway.

Regards,
M

13. **Mad Dog**
December 2, 2007

    The talk by Philip Boalch on irregular connections on curves looked interesting – what was it about?

14. **Hans**
December 3, 2007

    thank you. I think this is the best of your postings you ever made on this blog. I'll download all the articles you mentioned. What i've seen till now is very interesting content

Jumping the Shark

December 1, 2007
Categories: Uncategorized

Over at bloggingheads.tv today, John Horgan and George Johnson discuss the various excesses of recent physics news reporting covered here over the last week or so (Lisi-mania, evidence of other universes, observation of the CC causing ours to end, etc.), entitling their segment [Jumping the Shark](). I think this term came up in the comment section here at one point, but for a definition one can consult [Wikipedia](), where it is described as referring to an episode in the popular US TV series *Happy Days* in which Fonzie jumps over a shark while water-skiing:

> Since then the phrase has become a colloquialism used by U.S. TV critics and fans to denote the point at which the characters or plot of a TV series veer into a ridiculous, out-of-the-ordinary storyline. Such a show is typically deemed to have passed its peak. Once a show has "jumped the shark" fans sense a noticeable decline in quality or feel the show has undergone too many changes to retain its original charm.
>
> Jump-the-shark moments may be scenes like the one described above that finally convince viewers that the show has fundamentally and permanently strayed from its original premise. In those cases they are viewed as a desperate and futile attempt to keep a series fresh in the face of declining ratings.

Horgan and Johnson discuss the idea that, with the latest silliness, press coverage of fundamental physics has finally "jumped the shark", in response to a decline in substantive new results coming out of the subject.

I suspect that most physicists feel that, as a scientific idea, string theory conclusively jumped the shark with the advent of the anthropic landscape. The last year or so has seen an increasing amount of shark-jumping by string theorists desperate to find some way to address the problem of declining ratings. For the latest shark-jump, see this month's Physics Today, where the first article is entitled [String Theory in the Era of the Large Hadron Collider](). Much of the article has nothing to do with string theory, describing the standard model and its problems, and how they may be addressed by the LHC. Oddly enough, the abstract of the article doesn't mention string theory at all, whereas the subtitle ("The relationship between string theory and particle experiment is more complex than the caricature presented in the popular press and weblogs") makes explicit the goal of responding to claims made here and elsewhere that the anthropic string theory landscape is not really science since it can't predict anything.

The article heavily promotes the anthropic landscape and the idea that it "predicts" the right value of the CC, claiming that "The landscape and its explorations are exciting developments", but it really takes shark-jumping to new heights in the final paragraph:

> A few years ago, there seemed little hope that string theory could make definitive statements about the physics of the LHC. The development of the landscape has radically altered that situation. An optimist can hope that theorists will soon understand enough about the landscape and its statistics to say that supersymmetry or large extra dimensions or technicolor will emerge as a prediction and to specify some detailed features.

I've never before heard of anyone making this kind of claim that string theorists will soon be predicting detailed features of LHC physics. LHC results should start coming in 2-3 years from now. Dine and others have been trying to address the question of whether among the known string backgrounds there are more with high or low supersymmetry breaking for nearly 4 years already (see [here]()), and the answer so far seems to be that this is not possible. Even if it were possible, there is no reason to believe that all classes of string backgrounds are known. There is also no understanding of the cosmological mechanism producing our universe, and thus it remains unknown whether counting backgrounds is even relevant.

For a discussion by Dine of the issues involved here aimed not at the public but at his colleagues, see his talk last year at the Santa Barbara string phenomenology workshop, discussed [here]().

Update: Lubos [weighs in]() to praise the Dine article for what he sees as its message that the only good phenomenology is string phenomenology:

> Right now, it is extremely important for an idea about new physics to be reconciled with the solid cutting-edge picture of reality that is available, namely with string theory. In the absence of doable tests, this is pretty much the most important criterion that decides whether an otherwise conceivable idea is worth research or not.

Update: [Here]() is Chad Orzel's take on the Dine article in Physics Today. Chad characterizes my attitude towards this sort of thing as "snarky", while for him the situation is that

> You've got serious physicists running around jabbering about this sort of stoned dorm-room bull session material…

Oops, I fear that was a snarky comment…

Update: Cern Courier joins Physics Today this month with yet another [feature article promoting the multiverse](). I'm trying to think of a snarky comment, but I'm too depressed.

Comments

1. **Alfonso Martinez**
   December 1, 2007

   Dear Peter,

   I remember the article on Lisi which appeared in The Economist. It was instructive reading because one could not avoid thinking that the author was the same person who gave a very good review to your book. I say so because, next to the hype (or Lisi-mania) you mention, he also argued that Lisi's ideas were somewhat more scientific than string theory, being at least testable. I don't know whether he meant to be ironic, but I am afraid he was serious. And it would not be surprising that he probably got this idea from your insisting on it.

   This brings me to the point that, undoubtedly, many scientists boldly exaggerate their claims, especially when they are writing research grant applications or books for the general public. But this may also be somehow the source of the problem. Why would physicists want, or need, to share their discussions with the general public? I don't think there are many civil engineers, to randomly name another group, who want to join any public discussion on the truth of the world. Not even many philosophers do that. Physicists may be good at doing science, but the jury is still out to decide whether thay are any good at properly shaping the opinions of the general public.

   This applies to string and anti-string partisans, to writers of books on the first minutes of the universe or to books that claim that the universe is a computer. As scientific claims, these ideas are fine and subject to open scientific discussion. But the scientific merit of putting them forward to the general public is more difficult to see. Apart from the sociologic value that the authors, as human beings, may be moved by the objective of extracting some professional/psychologic/economic benefit from them, of course.

2. **Michael Gogins**
   December 1, 2007

As a non-scientist, but one who is deeply interested in philosophy, philosophy of science, and fundamental science such as physics and biology, I must say I much prefer reading books and articles about science intended for the general public, or for scientists in other fields, that are written by the scientists themselves.

Furthermore, I see a number of writings that are evidently written by specialists for specialists, but in an accessible style, or with introductory or discussion sections that are accessible to non-specialists.

Finally, I note that a number of truly great scientists, including most notably Einstein, wrote accessible works about their research.

I think that leaving it up to science journalists and popularizers to do this is going to leave people like me much more in the dark…

3. **Coin**
December 1, 2007

> A few years ago, there seemed little hope that string theory could make definitive statements about the physics of the LHC. The development of the landscape has radically altered that situation…

I've never before heard of anyone making this kind of claim that string theorists will soon be predicting detailed features of LHC physics.

Maybe the idea is that the development of the landscape has taken us from "little hope" to "no hope"?

Ba dum ching

4. **a quantum diaries survivor**
December 1, 2007

Dear Alfonso,

you really did a random pick with civil engineering, but you could have picked medicine or biochemistry and your point could develop the same way. The thing is, it is mostly those who do pure research in physics who feel the responsibility to educate the general public on the need to push for answers to the very fundamental questions, as well as the urge to fight an anti-scientific revolution that is always on the verge of erupting – for a simple example read the latest encyclical by Pope Ratzinger, which warns against the horrors of marxism just as much against the dangers of illuminism.

You might perceive Ratzinger as a poor old chap with a hobby, but he ca reach several hundred million people. Unfortunately, I seldom see civil engineers take a stand for the need of scientific thinking and the advancement of science. Do you ?

Cheers,
T.

5. **Peter Woit**
December 1, 2007

Alfonso,

I doubt that writer got from me the idea that scientific ideas need to ultimately be testable. If he got from me the idea that string theory is not testable, that's accurate, and so much the better. As for Garett's theory, I certainly haven't claimed that is testable and I don't see how he could have gotten that from me.

6. **Chris W.**
December 1, 2007

Maybe researchers in the early stages of any kind of serious work at the frontiers of fundamental physics should all start treating the media the way they presumably treat telemarketers calling at dinner time. Perhaps quite a few already do, and we just don't hear about it.

(These days, when I answer the phone and detect the telltale delay suggesting that somone in a call center hasn't noticed that I've picked up yet, I just hang up.)

7. **Aaron Bergman**
December 1, 2007

Well, that (the Bloggingheads segment) was depressing. I mean, whatever you want to say about Lisi, it has next to nothing to do with loop quantum gravity. And Lisi's theory predicts new particles in the pretty much the exact same sense that string theory does. Why Lee finds it so interesting is probably best left as an exercise for the reader.

8. **milkshake**
December 2, 2007

Not quite in the exact same sense: Lisi was claiming his model had no fudge parameters. He came up with just one prediction. He was not saying "…and if the things turn out just the opposite way then I can fit that too."

9. **Aaron Bergman**
December 2, 2007

He came up with just one prediction.

And what prediction would that be? Just for reference, a prediction of a new particle should probably include the mass if you want to say that LHC will see it.

10. **dragon**
December 2, 2007

PW said: "the anthropic string theory landscape is not really science since it can't predict anything."

Well, all the people who want to make predictions are working on geometric langlands, right? Oh, not yet? How many years do you want? A lot less than twenty, presumably………

As for jumping the shark: that happened to this blog quite recently, when you started launching unprovoked attacks on the work of serious scientists like Robert Brandenberger, Sean Carroll, Laura Mersini-Houghton etc etc etc.

11. **?**
December 2, 2007

I also find it interesting that Dine writing an article for Physics Today is equated with advertising to the "general public". Last I checked, this was the journal of the American Physical Society, and Dine was most likely writing precisely for his colleagues, who are in a far better position to judge the merits or defects of his statements than "the general public".
Which includes most readers (and, from what I can tell, participants in commentary) of this blog.

12. **milkshake**
December 2, 2007

You know this better than I, how long it took for QCD to be developed to the point of calculating anything quantitative – such as the masses of the known particles (not mentioning the masses of newly predicted ones). The problem of "how to get the numbers out of the theory" continued long after it was generally agreed that it was most likely a good model – one that explained the known species of the particle zoo and predicted new ones.

13. **Aaron Bergman**
December 2, 2007

So you're agreeing that Lisi doesn't have a prediction, then?

After all, string theory predicts lots of new particles, too — I just can't tell you their mass at the moment.

14. **milkshake**
December 2, 2007

He made only one. ST cornered the entire prediction market.

15. **dave tweed**
December 2, 2007

Alfonso said "I don't think there are many civil engineers, to randomly name another group, who want to join any public discussion on the truth of the world." This is mixes up the fact that physics (along with philosophy) is "about the truth of the world" and that physicists are talking about their subjects. Most fields are judged more favourably when they can put forward folders of clippings or lists of radio interviews.

To my mind the biggest problem is that in the general media a story which says "here's a preliminary calculation that might lead to a more fully developed theory which might then be applicable to the world" is more likely to be unprinted/unread than one that confidently asserts one simple idea is groundbreaking.

16. **Peter Woit**
December 2, 2007

dragon,

I've been attacking media hype by serious scientists on this blog since the beginning, and getting nasty anonymous attacks from people like you since then. If I've jumped the shark, it was several years ago…

The work on geometric Langlands is producing interesting high-level mathematics. I've never complained about string theorists who are doing that. The meeting I was describing was a mathematics one, very few physicists were there.

17. **Douglas Natelson**
December 2, 2007

Alfonso – Isn't there some responsibility of scientists to try to explain to their work to the public since very often the public is *paying* for it?

18. **LDM**
December 2, 2007

dragon,

Regarding your statement "*As for jumping the shark: that happened to this blog quite recently, when you started launching unprovoked attacks on the work of serious scientists like Robert Brandenberger, Sean Carroll, Laura Mersini-Houghton etc etc etc*."

Not so fast. I have not read everything that Peter has written, but the impression I have from what I have read is that Peter's "attacks" are attacks on the merits of an individual's scientific ideas and work, and how these merits are being *oversold* to the public. This is normal scientific scrutiny and all scientists should be glad for it. Also, a scientist may be "serious", to use your word, but unfortunately still be wrong. Davies is a recent example. If on the other hand the work and ideas are correct, and are not being overstated, then they can more than withstand Peter's scrutiny.
I also hope Peter never tires of this rather thankless task, because I don't know who else would be willing to step up and do it.

19. **Peter Shor**
December 2, 2007

Reading the article, I also noticed that the pop-up box about supersymmetry claims fairly definitively that if the LHC doesn't find evidence for supersymmetry, then supersymmetry is dead. The opposite is implied by the main text of the article. Either Michael Dine has contradicted himself or, much more likely, somebody with different views wrote up the pop-up window about supersymmetry, and nobody proofread the whole thing very carefully.

20. **Peter Woit**
December 2, 2007

Peter Shor,

You're right that the wording of the pop-up box is inconsistent with the article, maybe Dine didn't write it. If supersymmetry is not seen at LHC energies, that will show it can't be the explanation for what stabilizes the electroweak breaking scale, solving what is called the "hierarchy problem". If supersymmetry is not seen at LHC energies, it could still be there, just broken at higher energies.

String theorists seem to recently be arguing that if supersymmetry is not seen at the LHC, that means that it doesn't solve the hierarchy problem, so the way to solve it is just like the CC: invoke the anthropic principle. Presumably once the LHC doesn't see supersymmetry, that's what we'll be hearing from the landscape people.

21. **Peter Shor**
December 2, 2007

You seem like you're betting that the LHC won't see supersymmetry. Is this just because there isn't any other evidence of it yet?

And apologies if this is drifting fairly far off-topic.

22. **Peter Woit**
December 2, 2007

Partly because nothing at all has shown up so far, partly because I just don't think that known supersymmetric extensions of the standard model really explain much, while making things a lot more complicated. The underlying idea of supersymmetry is in some ways mathematically attractive, but the specific models people are looking at here are not. Maybe there's a more interesting one we don't know about, maybe it's just not the right idea.

But this is kind of a large topic that can lead to a complicated and technical argument I'd rather not get into here. There is a chapter in my book where I go over some of this.

23. **?**
December 2, 2007

Actually, technicolor and/or warping are also widely viewed as non-anthropic solutions that are reasonable for the hierarchy problem, and that may well show up at LHC. This is true on hep-ph, and from what I know of the hep-th community, true there as well. So it is far from true that supersymmetry not being seen will immediately result in declarations that the weak scale is explained anthropically, from anyone.

24. **Peter Woit**
December 2, 2007

?

My comment was assuming that the LHC sees only a SM Higgs. If that happens I do think we'll hear from string theorists a lot about the "anthropic prediction" of the electroweak scale. If we see some other mechanism such as technicolor/warping, we'll hear about how this can be accomodated within string theory. I guess this was the point of Dine's article, as Lubos explicated it: all phenomenology is string phenomenology…

25. **John Campbell**
December 3, 2007

Links between the best scientific understanding and the general public are tenuous. Treatments of science in the media often range from indifference to 'jumping the shark'.

Although bringing scientific understanding to a wider audience is often flawed let's not forget its central potential importance to all people. As Einstein stated, in his view the most important function of science is 'to awaken this feeling (the cosmic religious experience) and to keep it alive in those who are receptive to it'.

Einstein saw the central function of science as its ability to provide us with a kind of cosmic religious worldview free of the supernatural and bonded to truth through empiricism.

The worldview provided by science is wondrous. Let's hope that the occasional straying from the path by New Scientist and others does not discourage those scientists who rob time from their research in order to communicate these wonders to the wider public.

26. **Jeff Moreland**
December 3, 2007

As someone with a lifelong interest in science, but who has not worked as a scientist for many years (I used to be a development metallurgist), I'd like to add a heartfelt endorsement to John Campbell's remarks. A big chunk would disappear from my life if I couldn't read serious (but non-professional) science and maths books and articles. The level that Scientific American published at about 40 years ago (yes, I'm that old) was perfect for me.

27. **Visitor**
December 4, 2007

The phrase "science as its ability to provide us with a kind of cosmic religious worldview free of the supernatural and bonded to truth through empiricism" is nothing but gibberish. I don't care if Einstein said it, it is foolish all the same. Science and religion are polar opposites and attempting to attempting to replace religion with science by attaching the word "religion" in any form to science is at best foolish and at worst dishonest.

And just as an aside, it is debatable that religion supplies or needs to supply people with a sense of wonder. What should be beyond debate, is that is supplies people with a sense of purpose.

For more of this same kind of foolishness, perpetrated by great scientists venturing, ill-advisedly, out of their fields of expertise and into the regions of social analysis, please read "The Sokal Hoax: At Whom Are We Laughing? – The philosophical pronouncements of Bohr, Born, Heisenberg and Pauli deserve some of the blame for the excesses of the postmodernist critique of science" by Mara Beller and available online at
http://www.mathematik.uni-muenchen.de/~bohmmech/BohmHome/sokalhoax.html

28. **milkshake**
December 4, 2007

"a kind of cosmic religious worldview free of the supernatural" means the mystery and awe, without the mysticism. The delight and the amazement that feeds the curiosity. The hope that we can figure out all kinds of things about this Universe, startng from few extremely-well hidden and indirect hints. I think this is a worldview which should replace the parochial men-centric religious views. It is also a pretty significant motivator for people who are active in far-out fields such as cosmology – and also for the public that takes delight in learning and thinking about these discoveries. I don't agree that the ol' Albert was turning into a New-Agey fool when he said these things.

29. **Changcho**
December 5, 2007

Visitor – the Mara Beller article (“The Sokal Hoax: At Whom are we Laughing?”) originally appeared on Physics Today, Sept. 1998 issue.

30. **Chris W.**
December 6, 2007

Visitor,

Einstein didn’t say it.

31. **anon.**
December 8, 2007

‘Update: Cern Courier joins Physics Today this month with yet another feature article promoting the multiverse. I’m trying to think of a snarky comment, but I’m too depressed.’

Look on the bright side: if anyone disproves the multiverse (which they can’t even in principle, because it’s not even wrong), Physics Today and New Scientist will just switch to promoting other non-falsifiable speculations. Maybe the physics of the resurrection, ESP, and miracles. So this continuing stringy multiverse hype isn’t completely your failure to communicate the scientific facts. It’s something that would go on even if every string theorist adopted scientific ethos today. The public just want to see sci fi hype supported by PhDs, and there are always some PhDs willing to give the public what it wants, especially when some funding is provided.

32. **Nikita Nikolaev**
December 10, 2007

I appologise to be getting perhaps even more off-topic, but I’ve seen some comments above concerning the supersymmetry breaking.

Suppose LHC doesn’t find any evidence of supersymmetry, but, at the same time, suppose supersymmetry still does exist. Is there any at all hint that comes from the theory as to why the supersymmetry breaking should occur at a higher energy than what LHC will have been aiming for?

33. **Eric**
December 10, 2007

Nikita,
There are really no fundamental reasons why supersymmetry breaking should occur at very high energies, unless you believe in fine-tuning or in some other mechanism (e.g. technicolor) to stabilize the electroweak scale. There are some string theorists, mostly at Stanford, who believe that the landscape and small cosmological constant point towards fine-tuning. These guys expect nothing but the Higgs to be found at LHC. This is called letting the tail wag the dog. On the other hand, there are many good reasons to expect TeV scale superpartners, namely 1) EW stabilization 2) gauge coupling unification 3) natural dark matter candidate 4) dynamical electroweak symmetry breaking.

34. **Nikita Nikolaev**
December 10, 2007

Eric,

Thank you so much for your answer.

News From All Over

December 4, 2007
Categories: Uncategorized

There's a long and interesting profile of Jim Simons on the Bloomberg web-site. It begins with him being told he has a call from Harvard string theorist Cumrun Vafa. Unclear whether Vafa was calling to talk about something related to science or something related to finance, since I've heard from several sources that Vafa recently has been working at least part of the time for Renaissance Technologies, the Simons hedge fund.

Director Ron Howard is making a movie based on the novel "Angels and Demons", part of which will be filmed at CERN. Here's a news report on his visit to CERN

Witten has posted two papers to the arXiv, one old, one new. The old one is Conformal Field Theory in Four and Six Dimensions, the write-up of his talk at the Oxford conference celebrating Graeme Segal's 60th birthday back in 2002. Until now this paper hasn't been available on the internet, you had to buy the book of the conference proceedings. It has acquired some new interest because of Witten's recent work on geometric Langlands, where Langlands duality comes from a duality symmetry that is part of a conjectured $SL(2,\mathbf{Z})$ symmetry of N=4 supersymmetric Yang-Mills in four dimensions. This $SL(2,\mathbf{Z})$ can be explained by the existence of a superconformal theory in 6d, which can then be reduced to 4d by taking it on the product of an elliptic curve and a 4-manifold. The modular symmetry then comes from the elliptic curve.

The new paper is with Alex Maloney and entitled Quantum Gravity Partition Functions in Three Dimensions. They calculate the partition function of pure gravity on an AdS3 space by summing the contributions from classical geometries, including quantum corrections, finding that "the result is not physically sensible". The paper includes a speculative discussion about what this might mean. It looks like 3d quantum gravity is still a subject that is far from completely understood.

Slides from talks at the recent HEPAP meeting are available. The FY2008 US budget for particle physics remains caught up in struggle between the White House and the Congress. They all agreed on a quite healthy budget number for particle physics, but haven't agreed on an overall budget. One possibility, a continuing resolution splitting the difference between the Congressional and White House total numbers, might possibly lead to a smaller particle physics budget than expected.

Physical Review Letters is publishing the latest paper by Chamseddine and Connes on their non-commutative geometry approach to the Standard Model. The PRL editor evidently forced them to change the name of the paper, from "A Dress for SM the Beggar" to Conceptual Explanation for the Algebra in the Noncommutative Approach to the Standard Model

Comments

1. **Lionel Hut**
   December 4, 2007

   There was an interesting news about Simons' otherwise quite secretive company a few months back.

   http://infoproc.blogspot.com/2007/07/algorithm-wars.html

2. **a quantum diaries survivor**
   December 4, 2007

   The PRL editor evidently forced them to change the name of the paper, from "A Dress for SM the Beggar" to Conceptual Explanation for the Algebra in the Noncommutative Approach to the Standard Model

   Tsk tsk, those poor souls at PRL lack any fantasy. We do take our papers a bit too seriously…

   Cheers,
   T.

3. **ali**
   December 5, 2007

   Hi Peter,
   It is very interesting that Cumrun Vafa calls Jim Simons. He is a quite senior person. How come he can work for a hedge fund company at that age, if the rumours are true? Do you have any other inside scoop on that?

4. **Peter Woit**
   December 5, 2007

   ali,

   I don't know what Vafa's arrangement with Renaissance is, other than that he was working with them at some point in the last year or so. While it's unusual for senior people to do this, I have heard of other cases of quite prominent senior mathematicians who have worked for Renaissance for a while, returning to academia afterwards.

5. **gs**
   December 6, 2007

   Speaking of senior people, George Zweig works for Renaissance.

6. **Chris W.**
   December 6, 2007

   Lately, I've been thinking that these associations are not particularly good for physics and mathematics, or the reputations of the people that study them—rather like nuclear weapons development in an earlier era.

7. **Maynard Handley**
   December 9, 2007

   "Angels and Demons"? By Dan Brown, author of the "Da Vinci Code".
   Yeah, that's going to make everyone at CERN proud.

I appreciate that they probably earn some money for this, but still, it's hardly something to boast about.

8. **Yatima**
   December 9, 2007

   Chris W. Says:

   "Lately, I've been thinking that these associations are not particularly good for physics and mathematics, or the reputations of the people that study them"

   I will say. I just came across this characterization, which I find myself unable to fully disagree with:

   Today's Wall Street fabricators of avant-garde financial instruments are actually called "financial engineers." They got their training in "labs," much like Dr. Frankenstein's, located at Wharton, Princeton, Harvard, and Berkeley. Each time one of their confections goes south, they scratch their heads in bewilderment — always making sure, of course, that they have financial life-rafts handy, while investors, employees, suppliers, and whole communities go down with the ship.

   I might be advisable to have plane tickets to small foreign countries without extradition agreements with the US handy if the economy worsens appreciably.

Latest Sci-Fi/Fantasy News

December 11, 2007
Categories: Uncategorized

Various particle physics-related science fiction and fantasy news:

Discover has an interview with Kip Thorne, who is working with Steven Spielberg on a science fiction film tentatively entitled *Interstellar* for release in 2009. The plot evidently involves the novel idea of a group of explorers who travel through a worm hole and into another dimension. Thorne expects that "nothing in the film will violate fundamental physical law." He also seems rather involved in fantasy as well as science fiction, believing that the LHC has a "good shot" at producing mini-black holes, and that "String theory is now beginning to make concrete, observational predictions which will be tested." (via Angry Physics).

Also on the fantasy front, I hear there's a new movie out called The Golden Compass, which supposedly has a plot based on multiple dimensions and particle physics. According to this review, the plot is not really fantasy, because:

> In the past thirty years or so, a majority of scientists have come to accept string theory as a so-called "Theory of Everything," one that helps to explain how everything in the universe works.

and string theory explains these extra dimensions.

One can follow the progress of the LHC project on the web, and unfortunately it's looking like the current official schedule, which plans on trying to circulate a beam next May and physics starting in July, is pretty much a fantasy. This schedule already was sticking to these dates in the face of delays that made them look unrealistic, but there have now been further delays. According to the schedule, sector 45 should be completely cooled down now and nearing the end of powering tests, with four others in the middle of cool-down. The actual state of affairs is that sector 45 is just finally getting fully cooled down to 1.9K, and the only other sector being cooled down is sector 56. A rough guess would be that they're three months or so behind the official schedule, so if nothing else goes wrong they might have a beam in late summer, physics sometime late in the fall. The CERN Council will be meeting later this week and get a status report on LHC progress, perhaps there will be an official update on the schedule at that time.

Michael Dine and collaborators have a new preprint about the Landscape, one that tells a rather different story than Dine's recent article in Physics Today. The authors discuss the question of the stability of Landscape states, given that there may be many nearby states, considering the possibility that this favors supersymmetric states. They also mention the problem of how to calculate transition probabilities into whatever the relevant metastable states are, which suffers from the well-known problem of how to pick a measure for eternal inflation, writing

> While we currently have little new to add to this discussion, we point out that the landscape is likely to be more complicated than assumed in many simple models of eternal inflation.

There's nothing in the paper that could possibly justify the Physics Today claims of hopes that landscape studies would soon be making "definitive statements about the physics of the LHC" and able to "specify some detailed features." Instead, there is a discussion of the possibility that landscape statistics are dominated by large volume, non-supersymmetric states, in which case:

> they are otherwise undistinguished, it is unclear how one might imagine developing a string phenomenology. Not only would we fail to make predictions, e.g. for LHC physics, but we would not know how to interpret LHC outcomes.

Update: For more sci-fi, tonight's arXiv postings include Warp Drive: A New Approach by string phenomenologist Gerald Cleaver and his graduate student Richard Obousy.

Comments

1. **A.J.**
   December 11, 2007

   A question on sci-fi, pop-sci, and culture: Does anyone have any idea when and how the idea of alternate realities became conflated with the idea of dimensions? This predates string theory by a few decades, I'd guess.

2. **Sebastian Thaler**
   December 11, 2007

   Speaking of science fiction, I'm looking forward to Michio Kaku's new book:

   http://www.amazon.com/Physics-Impossible-Scientific-Exploration-Teleportation/dp/0385520697/ref=sr_1_1?ie=UTF8&s=books&qid=1197407441&sr=1-1

3. **Low Math, Meekly Interacting**
   December 11, 2007

   I seem to recall reading somewhere that Carl Sagan consulted with Kip Thorne while writing *Contact*, to tidy up some of the theory behind the alien contraption used for FTL interstellar travel. It was this collaboration, if memory serves, that got Thorne really active in the wormhole-as-time-machine business. I don't know if Thorne was also brought in for the movie.

4. **rob**
   December 11, 2007

   Yes, Kip was involved in Contact. The story as I remember hearing it is that Sagan originally wanted to use a black hole, and asked Kip how reasonable that would be. Kip of course told him that it wasn't a good idea, but that wormholes could more realistically serve the purpose Sagan really wanted. Kip later went on to write that series of wormhole papers investigating the issue in GR.

   And yes, I think he might have been involved to some extent in the movie, though I don't think he was credited. Also, Lynda Obst, one of the co-executive producers (with Kip and Spielberg) of Interstellar, was also a producer of the Contact movie.

5. **Low Math, Meekly Interacting**
   December 11, 2007

   Interesting to learn where some theorists may be getting their inspiration :).

I have to wonder if Thorne was misquoted somehow. I also seem to recall reading that this whole practice of serious academics doing reasearch into time machines was motivated by the very legitimate desire to push GR to its logical limits, so-to-speak. The upshot, if I understand correctly, is that if you can (with a dollop of exotic matter or some comparable unobtainium) actually construct a time machine without violating GR, thus producing something that could, in principle, violate causality, you've got yet another good indicator that maybe there's something seriously lacking in GR. Ergo, the fact you're not violating "fundamental physical law" means you surely need some better fundamental physical law. Then again, he appears to be saying ST fits that bill, so maybe the time-machine industry has evolved into something I'm not aware of.

6. **rob**
   December 11, 2007

   I was recently a student in Kip Thorne's group, and while I can't speak for him, and he certainly doesn't need me defending him, I will say that he always seemed to me to be basically noncommittal on the "string theory wars." The only time I heard him discuss quantum gravity in a public lecture, he did make a point of mentioning both string theory and loop quantum gravity.

   I'll also say that (as far as I've heard) he still hasn't conceded on his half of the bet with Hawking against Preskill on whether black hole evaporation is unitary. So he's certainly not a conventional string theory partisan.

   As for his "good shot" statement, I think he just really likes taking the side of underdogs, and in the public press these days, string theory seems to be the underdog.

7. **Bee**
   December 11, 2007

   Thanks for the update! Maybe we are witnessing the dawning of a new era in theoretical physics? If computer animations can do it in virtual, who cares about actual reality? Can we just compute another part of the multiverse and sell the fundamental laws to Dreamworks? Chances are we are just living in a computer simulation anyhow. Either way, seems like you missed out on Denzel Washington in Deja Vu, where a group of crime investigators together with some scientists watches the past through a wormhole. Memorable quote: "I asked you to explain it to me, not to talk science."

   (the movie is entertaining, but not really recommendable)

8. **rob**
   December 11, 2007

   Low Math Meekly Interacting:

   Well, this causality thing has a lot of ins and outs. The existence of closed timelike curves doesn't necessarily point to a problem in GR, or even necessarily a problem at all. I haven't really looked at this stuff, but I remember that Novikov was involved in some work showing that "consistent histories" (to borrow a phrase) can still arise even in the presence of closed timelike curves. He probably talks about this in his essay for the book "The Future of Spacetime."

9. **Peter Woit**
   December 11, 2007

   Bee,

   I'm afraid I was being snarky about the originality of the wormhole as plot device, having seen a rather large number of movies or TV shows where wormholes appear (including Deja Vu..). Presumably the Thorne/Spielberg wormhole will be a less cheesy and more authentic one than the usual, run-of-the-mill wormhole that normally puts in an appearance.

10. **Low Math, Meekly Interacting**
    December 11, 2007

    Thanks for the addition to my library list!

11. **Bee**
    December 11, 2007

    yeah, I read your remark as sarcasm, was just looking for an occasion to place that quote. I wish people would stop asking me about wormholes, time travel or the free will in quantum mechanics when they hear I'm a physicist.

12. **rob**
    December 11, 2007

    From what I've heard, the central motivation of the project, both for Kip and for Spielberg, is to make a "scientifically realistic" science fiction movie. Obviously they'll stretch the limits of practicality, but they at least intend that everything should be basically in line with published (though sometimes speculative) science. Hopefully it will remain that way after being filtered through screenwriters and special effects houses.

    For example, one very early idea that I heard (and I'll presage this with the disclaimer that I'm not at all privy to the developments) was that the wormhole would be discovered by LISA. Some would say that LISA taking measurements is no more realistic than finding a wormhole, but we relativists still like to think those people are wrong.

13. **Scott Aaronson**
    December 11, 2007

    "consistent histories" (to borrow a phrase) can still arise even in the presence of closed timelike curves

    Yes, it's possible to write down theories that have CTC's but no inconsistencies. One way is just to stipulate that, if S is the quantum operation acting around a CTC, then as the "input" to the CTC nature has to postselect a state &rho such that S(ϱ)=ϱ. (Such a ϱ always exists by linear algebra.) This idea goes back to a 1991 paper of Deutsch.

    However, these theories have an extremely interesting further problem. This is that, in order to *find* a consistent evolution around the CTC, nature has to solve an extremely hard computational problem! (In particular, it has to solve a PSPACE-complete problem, which is even worse than NP-complete.)

    So, to return to Peter's sci-fi theme: while physical theories with CTC's aren't necessarily inconsistent, they *do* necessarily lead to effects like in *Star Trek IV: The Voyage Home*. There the crew of the Enterprise goes back in time to the "present" (i.e. to 1986) and, needing a type of plexiglass that hasn't been invented yet, reveals the formula for the plexiglass to the company that's *going* to invent it, thereby causing it to be invented, etc. This is not a logical inconsistency but is nevertheless exceedingly strange.

14. **Dave Miller**
December 12, 2007

As an undergraduate at Caltech in '75-'76, I took a course on GR from Thorne, and I pointed out to him that you could build a time machine out of wormholes. I had in mind two separate wormholes moving in different reference frames – you don't need to invoke time dilation at only one end of one wormhole then: the Minkowski geometry is pretty obvious.

My point is not that I am the source of Kip's later work: it was a casual comment on my part, and I'd guess that he forgot it rather promptly. The point rather is that the idea of wormholes as time machines was pretty obvious to anyone, even an undergrad: wormholes obviously violate locality, and any particle theorist knows what that means. (Personally, I'm with Hawking on this: a wormhole time machine probably blows itself up for reasons Hawking has explained.)

And, Peter, the real news on "The Golden Compass" is that fundamentalist Christians are up in arms over it: I've heard this from personal contacts as well as seen it on the Web (NPR even ran a piece). Pullman is a self-announced atheist, and the theory is that the movie is really meant to seduce vulnerable Christian youth.

In fact, the movie is supposed to be pretty bland, and my own kids were actually given the trilogy by a Christian friend who had read through it all herself.

Of course, you can find fundamentalists on the Web who also denounce the "Chronicles of Narnia" on the grounds that Lewis, a famous Christian apologist, was really a secret pagan. (You see, he admired classical culture and enjoyed classical mythology…)

I am tempted to believe that critics of Christianity should just relax and allow the Christians to self-destruct.

Dave

15. **Alejandro**
December 12, 2007

Re *The Golden Compass*; in the books, at least, string theory is not mentioned at all, but the plot relies heavily on a fantasy take on many-worlds quantum mechanics and dark matter. Everett is mentioned by name, and a technology to communicate between parallel universes is said to rely on quantum entanglement. As for dark matter, well… (SPOILERS follow)…

…would you believe that dark matter is conscious particles that try to communicate with humans, that angels (including God, the oldest one of them) are complex structures made of these particles, and that human attachment to these particles, which signals sexual awakening, is what the Church calls original sin?

No, I am not making this up. Anyway, the books are awesome. Haven't seen the movie yet, but I expect to be disappointed.

16. **Bee**
December 12, 2007

Hi Alejandro,
I think I read the first volume of the trilogy because it showed up on my amazon recommendation list. I didn't bother to buy the other two. Maybe I've read too much Fantasy when I was a kid, but much like Harry Potter it didn't strike me neither as well written nor original. I didn't see the movies though. I like the part with the sexual awakening, maybe I should give it another try. To come back to the topic of Science Fantasies, since you mention dark matter it's kind of interesting in this regard what Rudy Rucker wrote in the book 'Dangerous Ideas' (among essays about multiverses, understanding GR and QM, or how capitalism will make the world a better place):

"On the one hand, it could be that the mind is some substance that accumulates near ordinary matter — dark matter or dark energy are good candidates. On the other hand, mind might simply be matter viewed in a special fashion: matter experienced from the inside. […]"

Best,
B.

17. **Bee**
December 12, 2007

Today's news: unparticle physics provides a mechanism for black hole formation.

18. **Alejandro**
December 12, 2007

Hi Bee. I didn't think too much of the first volume when I read it either -but in my case I think it was because I read it translated to Spanish; when I reread it in English I liked it much more. The other two are much more ambitious, both in a literary and a philosophical way; the whole trilogy is IMO a much "larger" thing than Harry Potter, which is a fairly conventional kind of story. Pullman's avowed goal is to provide a subversive retelling of Milton, celebrating rebellion against God and authority, and the loss of innocence related to maturity, self-consciousness, and yes, sexual awakening.

There are some problems in the third volume -the scope becomes so large that the plot becomes confusing, there are a couple of dei ex machina, and the anti-religious message is so explicit that it is grating at points- but overall I would recommend you to read the other two books to have an opinion; the central themes of the trilogy cannot be seen from the first volume alone.

19. **Alejandro**
December 12, 2007

Oh, and re the Rucker quote, it is rather similar to the premise of Pullman's fiction -but Rucker seems to be taking it *seriously*… :S

I have read only one book by Rucker, in my teens: *The Fourth Dimension*, basically a layman's explanation of 4-D concepts and their use in physics. He did get into some weird panpsychistic speculations by the final chapter, but it was all rather playful and he didn't seem to really believe it.

20. **Bee**
December 12, 2007

Hi Alejandro:

Yeah, sounds interesting indeed, I might give it a try. I yet have to make it through the last Harry Potter though, since I read the first six I think I will have to read the seventh as well. I know the 4D Rucker, though I didn't find it too illuminating, I had a couple of better books on higher dimensions that I liked better. I recall much better Rucker's book Master over Space and Time which I read probably being too young. I got seriously confused about what quarks are and so on. Maybe I would find it amusing now, but I think books like that better come with a manual or a warning. Best,

B.

21. **Moshe**
December 12, 2007

Scott says:"if S is the quantum operation acting around a CTC, then as the "input" to the CTC nature has to postselect a state &rho such that S(ϱ)=ϱ".

I am confused by that, I would say this is exactly what is *meant* by having CTC to start with. If you don't impose periodicity conditions the curve is not closed in any meaningful sense, am I missing something?

22. **John Baez**
December 12, 2007

I know Peter doesn't actually like talk about science fiction and fantasy here, despite the title of this blog entry… but can't resist telling Alejandro: I've read Pullman's *Dark Materials* trilogy and liked it a lot – and I found the movie adaptation of the first part to be *excellent!*.

Like the book, the movie doesn't mention string theory at all.

(It's fantasy, not science fiction: people should only watch it if they can deal with witches, daemons and talking polar bears.)

23. **Scott Aaronson**
December 12, 2007

Moshe: I agree with you — once you think about it clearly (which many people don't!), the requirement of causal consistency *is* pretty much inherent in the definition of CTC's. The slightly non-tautological observation in Deutsch's paper was that, while a consistent evolution doesn't always exist in deterministic theories (which is just a fancy way of stating the grandfather paradox), such an evolution *does* always exist in probabilistic or quantum theories. This is because every Markov chain has a stationary distribution, and likewise every superoperator has a stationary mixed state.

24. **Moshe**
December 13, 2007

Thanks Scott, I like your re-phrasing of the grandfather paradox, previously I thought about it as one of those linguistic pseudo-problems, but now I see at least one way of making a non-trivial statement.

25. **DB**
December 13, 2007

I think a lot of today's elder generation of physicist-speculators spent too much time as kids watching The Time Tunnel. The wormhole is easily the most hackneyed notion in filmed Sci-Fi, so it stands to reason that Hollywood will want to resurrect it for yet another outing.

26. **Alejandro**
December 13, 2007

Glad you liked the movie, John -it gives me raised hopes.

And by the way, I remember now that string theory *is* mentioned in the books! (albeit obliquely). In the third volume, when Mrs Coulter is approaching Asriel's fortress, she finds it similar to a model she saw once illustrating a "diabolical heresy" (quoting by memory, don't have the book here) according to which space had many extra dimensions curled up a tangled and tiny way .

In other words: the place from which Lord Asriel intends to overthrow God and build the Republic of Heaven resembles a Calabi-Yau manifold!

27. **Michael Bacon**
December 13, 2007

See http://www.edge.org/3rd_culture/serpentine07/Deutsch.html

At the above link is an equation from the paper by Deutsch that Scott mentions above: Quantum Mechanics near Closed Time-like Lines which appeared in Physical Review D44, 3197–3217 (1991).

According to Deutsch, it is the equation for the state of a quantum computer that is, by whatever means, provided with a method of sending its output back in time to interact with its input (I suppose he'd consider this "merely" an question of engineering.

He believes that the universality of quantum computation ensures that the results also apply to any time-travelling physical system, not just a computer.

I can't reproduce the equation (really don't know how to with my limited computer skills), but he claims that the self-consistency of this equation proves the self-consistency of time travel in quantum physics.

He says that analysis of the equation shows that if we had a time machine and tried to use it to enact 'paradoxes' (like going back in time to prevent our building the time machine), we would simply go back to a universe in which those events really happened.

Not being very well grounded in the math associated with this type of analysis, I settle for science fiction. It's fun and thought provoking — tautological or not.

I'd love to see a serious discussion between Deutsch, Scott and others who obvious know much more about this than me.

28. **Chris W.**
December 15, 2007

The above discussion of CTCs and causal consistency ties in rather closely with an interesting and lucid new review paper on the arXiv, **Causal Set Topology** (0712.1648).

(Thanks to Daniel Doro Ferrante for highlighting it.)

UK Pulls Out Of ILC

December 11, 2007
Categories: Experimental HEP News

The UK is planning on cutting the budget of its Science and Technologies Facilities Council over the next few years, ending British involvement in several large scale scientific projects, including the ILC. The STFC document laying out its plans through 2012 emphasizes CERN and the LHC, and has this to say about the ILC:

> We will cease investment in the International Linear Collider. We do not see a practicable path towards the realisation of this facility as currently conceived on a reasonable timescale.

In combination with recent remarks from the DOE, the current situation of the ILC proposal is not encouraging. Most likely it will require the discovery of new physics at the LHC of the sort that the ILC is the right tool to study in order to make the case for going ahead with it.

More about this here and here.

Update: More here, here, here and here. These budget cuts seem to be especially problematic for astronomy research, with particle physics not as badly affected as the UK retains its commitment to CERN and the LHC.

Update: More here. Best headline about this so far: Boffins slashed in big-science budget blunder bloodbath.

Comments

1. **Coin**
   December 11, 2007

   Let's say that the case is *not* successfully made that the ILC is the most appropriate followup to the LHC's findings, and the ILC ultimately gets scuttled (or at least suffers more dropouts like with the UK here). What happens then? Will this mean that that's money which simply won't get put into physics investment? Or will this just mean some other project gets attention diverted to it instead?

2. **Flip**
   December 11, 2007

   Thanks for the post, PW! Could you shed some light about what this means for the 10-20 year outlook of particle physics experiments? If the LHC's results end up making a compelling case for an ILC, would the STFC turn around and recommit to the linear collider?

   What is the significance of having national funding agencies committed at this [early] stage, especially if there is apparently no commitment to prevent them from walking away after a government spending review? Does this just mean that there's less R&D? If so, does this indirectly weaken Europe's bid to host the ILC (if it were to be built)?

   Cheers,
   F

3. **woit**
   December 11, 2007

   Flip,

   I don't know much about how UK budget decisions are made, so it's unclear to me whether and how this decision could get turned around at some point during the next few years. It seems to me that they have decided that the ILC project is too far off, likely to be in the US, and quite possibly will never get built, so R and D on it is a good target for cutting when they have a budget problem.

   One big problem for the ILC is that if it takes several more years before there's an LHC-based case for building it, at that point CERN's CLIC technology may have proved itself. CERN will have the money to start building it as LHC and LHC upgrade costs wind down 10 years from now. If CLIC is feasible and can be financed in Europe, there's not much of a case for the ILC.

   At this point, if I had to bet about what will be going on 20 years from now, it would be on a strong CERN program at the energy frontier involving CLIC and an upgraded LHC, with a weaker program in the US based on Project X at Fermilab and various neutrino experiments.

   But maybe the LHC will discover something that will change everything…

   Coin,

   Building the ILC would require a sizable part of the US HEP budget + new money. If it's not built, the new money wouldn't go to particle physics, might go elsewhere, or just might not increase the US fiscal deficit so much. There would be more money from the total particle physics budget available for other particle physics projects, e.g. things like Project X that Fermilab is considering.

4. **Coin**
   December 11, 2007

   There would be more money from the total particle physics budget available for other particle physics projects, e.g. things like Project X that Fermilab is considering.

   Well… something else I wonder about. If you look at the Project X web page they say: "The use of the Recycler reduces the required charge in the superconducting 8 GeV linac to match the charge per pulse of the ILC design; aligning Project X and ILC technologies." I'm not sure what this means– exactly what synergy does Project X get from its "alignment" with the ILC? Would the Project X project by itself be in some way less useful than Project X would be were it running contemporaneously with the ILC?

   Thanks!

5. **woit**
   December 11, 2007

Coin,

One selling point of the Project X proposal is that it would use some of the same linac technology as the ILC, and thus potentially help with the engineering needed for the ILC. One reason for highlighting this is just that Project X and the ILC are in some sense in competition for US HEP funds, so this is supposed to help sell the idea to ILC proponents worried that Project X means no ILC.

6. **Coin**
December 11, 2007

Oh, I see. So the idea is just to share engineering costs by reusing technology, not that like the similarities between the two accelerators would make it easier to do combined experiments or somesuch?

7. **Peter Woit**
December 11, 2007

Coin,

Right. I don't know of any plan to combine operation of the accelerators in any way, this is about the acceleration technology.

8. **Roger**
December 12, 2007

I'm a Brit working in Europe now. I think this should be understood in the context of the funding reorganisation last year which created the Science and Technologies Facilities Council. This replaced the PPARC agency which only handled particle physics and astronomy and is a kind of super research council including (amongst other things) particle physics, astronomy, laser and condensed matter physics. There are severe cost overruns on certain projects, including the "prestigious" (i.e. built in the UK) Diamond light source meaning that everyone else who shares the Diamond funding pot must take a cut. In the olden days of PPARC if there was a cost overrun for, for example, a particle physcics project the astronomers would take a cut on the understanding that this help would be reciprocated if astronomy was faced with a funding problem. This worked fairly well and none of the cost overruns were as severe as those faced at the moment. Overrun estimates for Diamond I've seen are for 160 million dollars. In effect, particle physics and astronomy are bailing out a wholly unrelated discipline, which through bad luck or bad contingency planning is in a crisis.

The ILC happened to be one of the superficially easiest items to chop. Its especially unfortunate in that the UK was one of the countries which jumped on the ILC very early, much to the surprise of physicists in many other countries. To invest so much, so early and now to pull out with inadequate consultation (very much unlike the way PPARC used to operate) is rather silly.

I know many people who are/were engaged on the ILC program. I know they put their heart and soul in the project and fully believed in it. They were also producing some cutting edge work on accelerators and detector technology. I really feel for them.

Most physicists I've spoken to seem to think that this is a result of incompetence rather than a deliberate, though underhand, attempt to cut back on particle physics which traditionally enjoys large budgets and therefore attracts much envy from other disciplines. I don't know what to think.

9. **lostsoul Ph. D.**
December 12, 2007

Money has been poured down the drain in the UK over the past 10 years or so, ostensibly, in the main, on 'good things' like health and education (and the odd war, of course), but in a way that has generated little of value for the taxpayer. Now the men in charge are embarking on a frantic and short-sighted cost-cutting/ face-saving exercise. When they pull out of the Olympics you will know that things are really bad; until then they will target anything whose decimation they think that they can get away with. And who amongst the electorate gives a monkey's about science of any persuasion these days?

10. **Arun**
December 12, 2007

The prospects in the US are even more bleak, if the dire warnings of requiring a taxpayer bailout of the banks are true (see my blog for details).

11. **young_physicist**
December 12, 2007

Arun:

The subprime mess is going to hit the uk pretty soon (I mean we will have our own borrowers defaulting) so I expect it is not just us banks that will have to be bailed out. If this causes a recession then the future for particle physics may become quite bleak indeed when goverment cuts spending due to reduced tax intake etc.

12. **Jon**
December 12, 2007

None of this is as a result of cuts in UK Government spending as implied at top of your article, in fact STFC funding is up 13% over next 2 years I understand, and science spending overall has doubled in real terms over last ~10 yrs. The current problems seem to be the result of over commitment mainly to new Diamond facility, as indicated by previous poster. Apparently capital spend for Diamond came in within contingency but operating costs appear to have been severely under estimated in the new Full Economic Costing model for staff recently introduced in UK. Previously separate Astronomy and PP funding is now having to bear the costs of this error following merger into wider STFC. Potentially head(s) should roll at STFC…?

13. **Big Vlad**
December 12, 2007

could someone who understands these matters please explain the implications for particle theory in the UK? Will theory be heavily affected by these cuts?

14. **Walt**
December 12, 2007

Arun, that article is wrong on a key point. Mortgage originators have to take back mortgages, but most of the subprime mortgages don't originate with banks, but with specialized mortgage originators — most of whom are likely going to go out of business. Defrauded investors, foreign or domestic, are going to get nothing, and no bank bailout will be required on that account.

15. **Peter Woit**
December 12, 2007

Arun and Walt,

I happen to find the whole mess in the banking system fascinating, but please, this blog is not the place to discuss it. Surely there are ones run by people who actually know a lot about this.

16. **DB**
December 13, 2007

This UK announcement doesn't really affect the prospects for the ILC which is essentially a US project with some modest international support tacked on. As a member country of the CERN consortium the UK remains very well placed to participate actively in HEP research.
If the core political will is absent in the US, as I believe is currently the case, then the ILC won't get built, period.
The UK may well prove to be useful fall-guy to blame, it has a long history of bureaucratic cock-ups associated with the funding of large science and engineering projects, but this shouldn't distract anyone from the core issue around the ILC: does the US have the stomach for this or not?
And I am sceptical about the continuing political will in Europe in support of large HEP projects. The LHC almost didn't make it past the politicians – CLIC – and I have confidence that the technical issues will be solved – will be many times more expensive and I just can't see it passing.
It would be a great pity if a misplaced confidence by the US in Europe's willingness to continue to do the heavy lifting in experimental HEP were to lead to the LHC being the last major collider to be built.

17. **Peter Shor**
December 13, 2007

Isn't the decision on whether to go ahead on the ILC entirely dependent on what is found by the LHC? If the LHC doesn't find any new physics that the ILC might shed some light on, it seems very difficult to imagine it getting funded no matter how gung-ho people are at this stage. On the other hand, if the LHC results require further study by an ILC or similar accelerator, then won't such an accelerator almost certainly be built eventually (at least in the absence of massive economic or geopolitical catastrophe)?

One question this brings up is: why is anybody doing any planning studies now? Wouldn't it be better to wait until after the LHC is built, and then start planning for the next accelerator? It seems to me that this would only delay the project marginally, and would save money if turns out that it's not worth building an ILC at all.

18. **Roger**
December 13, 2007

Peter Shor

One legitimate (though not great) argument is that much detector expertise would be lost if there isn't a continuous program of R+D at UK laboratories.

ILC research is not only accelerator-related, there is/was in the UK significant effort in developing an appropriate vertex detector and calorimeter.

On an unrelated note, the UK invested substantially in the ILC, with the encouragement of the then research council. Many of the same research council folk are present in the new body and they're now singing a totally different tune about how unrealistic the ILC is. Its true that the likelihood of an ILC has decreased recently but its construction was always uncertain. Heads really should roll for this planning debacle.

19. **Peter Woit**
December 13, 2007

Peter Shor,

The problem for US particle physicists is that the Tevatron will become more or less obsolete once the LHC is working well, and at that point the US will have no accelerator at the energy frontier. This may happen within a very few years from now. So US particle physicists need a plan for what to do next, and until recently the ILC seemed the best bet. The hope has been that getting the planning done and everything ready to build the machine would mean that, once LHC results came in, they would be ready to go. If they wait for LHC results to get started on planning, there will be a long period in the US with nothing even being built, and an even longer period without a high-energy machine.

The costs for the kind of planning they have been doing are relatively small, it's the construction costs that will be large, and most of this is for work trying to develop technology that ultimately will be useful, if not on the ILC, perhaps elsewhere.

20. **Roger**
December 13, 2007

Peter

The planning costs were very large for the UK both in terms of the R+D budget and the manpower which was devoted to this effort.

The ILC wasn't a relatively small back-office affair but a rather high profile activity.

21. **Peter Woit**
December 13, 2007

Roger,

Thanks for the clarification. My comment was about the amounts involved relative to the construction cost for the ILC, not the absolute amounts, which are significant. Actually, how much exactly has the UK been spending per/year on the ILC that will be eliminated by this decision?

22. **Peter Shor**
December 13, 2007

Thanks.

Training the next generation of physicists with detector expertise is useful, at least assuming that accelerators will continue to be built (which I assume depends on what the LHC sees). But the decision of whether to build the ILC still has to be years away.

23. **Peter Woit**
December 13, 2007

Peter Shor,

The people working on the ILC have been hoping for a construction decision in 2010. This is probably not realistic, unless the LHC very quickly comes up with results of just the right kind (e.g. a spectrum of new particles of mass low enough to be studied at the ILC).

24. **NEW Reader**
December 15, 2007

Hi Peter,

I get nearly all of my particle physics news from NEW. I have a simple question:

With the future going the way it is, (and as I had asked many – and *sigh* – many a times before), would it be a good life-long decision to study the subject?…

I love studying particle physics / mathematical physics, but am rather in a quandary as to its relevance – especially experiment-based particle physics – for the future, which seems increasingly being sqeezed into the marginal arena of academic / scientific pursuits; I fear within thirty years particle physics will cease to be of relevance unless there is a applicable defense and/or other high-profit-generating marketable involvement.

Do you share some of my concerns? Is the particle physics community losing some of its post-SM excitement in the early ’80’s?

Thanks for allowing me the post.

25. **NEW Reader**
December 15, 2007

Postscript:

Also, I just happended to chance upon your “Holy Grail of Physics” post (post # 3) via Wikipedia. I read Mark Srednicki’s and your opposing views on the topic (actually, didn’t understand much… :)); but I’m wondering if you have notes from your QFT course of Steven Weinberg’s, and whether – with your permission (obviously) – you might like to send a copy to me.

[PPS: How do you make links? Don’t understand much webpage-technology stuff.]

26. **Peter Woit**
December 15, 2007

NEW reader,

The Weinberg course was on the quantization of gauge theories, and this material is in the second volume of his three volume series on QFT. The books are definitely more useful than my notes would be.

As for the future, mathematics is doing well, so anyone interested in the mathematical end of things should consider pursuing a math degree. For particle physics in general, a lot depends on what happens at the LHC….

27. **Roger**
December 16, 2007

New Reader,

The future of the “traditional” high energy collider physics program is uncertain and, as PW pointed out, much depends on the LHC. However, there is more to particle physics research than high energy colliders. A symbiosis between particle and astrophysics has been occuring in recent years and cosmic ray experiments may well, for example, become the principal tools for particle physicists in the future.

I can foresee a time when particle physics is less prominent than it is today but, since it is one of the few truly fundamental areas of science, I doubt if it will ever become a marginal activity. The best and most curious minds will always be attracted to the most important questions.

28. **NEW Reader**
December 16, 2007

Thanks to both of you for the respective responses.

29. **young_physicist**
December 20, 2007

British citizens and residents may wish to sign this petition in protest against the announced cuts to particle physics and astronomy (which do not just affect the ILC, but also many other less well known research programs):

http://petitions.pm.gov.uk/Physics-Funding/

Latest on ILC/CLIC/LHC

December 14, 2007
Categories: Experimental HEP News

Barry Barish, the director of the ILC project, has a statement here about the recent UK decision to stop funding R and D work on the ILC. He writes that "losing the UK's contributions to the ILC will have a significant negative impact on our R & D program." For more press stories about this, see here and here.

Barish also has an article here about CLIC, CERN's competing design for a linear collider, one that is in a much more preliminary state than the ILC design. He writes that the ILC project will now be exploring ways of collaborating with CERN as it investigates the feasibility of CLIC:

> When I visited CERN last month, I had the opportunity to have a meeting with the CLIC Extended Steering Committee, including CERN Global Design Effort members. I suggested that joint work between the ILC and CLIC could have benefits for both efforts. They responded positively, and a number of specific areas have been identified where both groups could benefit. It is clear that the timescale for a machine like CLIC, even if feasible, is much later than the ILC. So the reason to consider CLIC is for energy reach, if required.
>
> Following my visit to CERN, I discussed these joint efforts with the GDE Executive Committee, and we agreed to the general idea. As a result, the GDE Project Managers will explore specific areas of collaboration with CLIC. An exchange of ideas has begun by email, and a meeting is now planned at CERN for February 2008 to explore specific areas of cooperation.

Today the CERN Council officially ratified the choice of DESY's Rolf-Dieter Heuer to succeed Robert Aymar as Director General of CERN. At DESY Heuer was responsible for ILC R and D, so some people at CERN have been concerned that their new leader will be someone from the competition to CLIC, and thus might not be inclined to enthusiastically and aggressively now push the project and compete with his old colleagues from the ILC.

The Council also approved a budget designed to begin preparations for an LHC luminosity upgrade by 2016, and heard a report from the director on the status of the LHC project. Until recently the date for the LHC start-up was set at mid-May 2008, but the official word from Aymar now is just "early summer 2008", with no specific date to be set until spring:

> Today, we're on course for start-up in early summer 2008, but we won't be able to fix the date for certain before the whole machine is cold and magnet electrical tests are positive. We're expecting that in the spring.

The press release also notes that:

> Any difficulties encountered during this commissioning that require any sector of the machine to be warmed up would lead to a delay of two to three months.

The latest version of the official schedule is here, and news about progress here, with the news putting the project a month or so behind the schedule.

Update: Science has an article about Heuer's appointment, quoting him on the ILC/CLIC issue as saying "It's a mistake to back one horse. We need different horses". Also:

> Barry Barish, leader of the ILC's Global Design Effort, is happy to have Heuer on board. "Clearly, from the perspective of the ILC, the appointment of the new is a very, very positive thing," he says.

Comments

1. **Roger**
   December 14, 2007

   Rolf-Dieter Heuer is a first class physicist who understands CERN's core business of producing publications of experimental results from colliders. I've met him on several occasions and was impressed each time. He is ideal both for the LHC and CLIC programs – I don't buy the idea that he'll drag his feet on the CLIC issue out of misplaced allegiance to the ILC.

   With the UK funding agency debacle in mind CERN should be grateful they have a weighty, intelligent figure in the top post who knows what must be done on the ground floor to get the job done.

2. **Coin**
   December 14, 2007

   To what extent does CLIC function as a *replacement* for the ILC? That is to say, if we somehow wind up building the CLIC but not the ILC, would the CLIC be able to do everything the ILC would in addition to its better "energy reach" capabilities? Or would building just the CLIC force us to give up access to some block of data?

   When we say the CLIC is in a more preliminary stage than the ILC, or that its timescale is "much later"– vaguely how much later is "much later"? Two years? Five years? Ten years? If the ILC becomes delayed for some reason (say, because the LHC takes awhile in getting out the data needed to make a decision on the ILC, perhaps?) how long would it have to be delayed before we find the CLIC actually within striking range as an alternative?

3. **Peter Woit**
   December 14, 2007

   Coin,

   As far as I know CLIC would have similar capabilities to the ILC, but be able to get to higher energies (say 3 TeV vs. .5 TeV).

   I don't think anyone knows yet how long "much later" is, but it's definitely >2-5 years. The ILC people already have a detailed design and are not far from the point where you could start building a machine, CLIC is nowhere near that. My understanding is that the goal of the current research on CLIC is to be able to answer your question in 2010, so that when LHC results arrive, a sensible choice between the ILC and CLIC designs could be made.

4. **DB**
   December 17, 2007

   The debate between ILC vs. CLIC may be a red herring. What's increasingly clear is that the successor to the LHC will no longer be simply a US or European affair. Like the ISS, or ITER, it can only be successfully conceived and implemented as a truly international collaboration – because the financial commitment will be too great for

any one economy to justify. As currently formulated, the ILC is only half-hearted in its adherence to the spirit of international cooperation.
Similarly, CERN has conceived CLIC as a development of existing accelerator assets in Geneva.
When the time comes to make a decisions, I expect political and economic realities will force experimental HEP to mirror other mammoth civilian science and engineering projects by developing a truly global approach.
It may be that Barish and Hauer are already thinking along these lines.

A Passion for Discovery

December 16, 2007
Categories: Uncategorized

I've just finished reading a wonderful new book by theoretical physicist [Peter Freund](), entitled [A Passion for Discovery](). Freund grew up in Romania, and began his career as a physicist in Europe during the 1950s, emigrating to the US during the 1960s, finally ending up at the University of Chicago, where he is now an emeritus professor.

When I was writing my own book I tried to include amidst the expository material about physics and mathematics stories of some of the people and events that seemed to me illustrative in one way or another. Freund has had the excellent idea of writing a book that foregrounds such stories, interspersing in the background the actual physics and mathematics. A reader who doesn't know the science may not learn as much about it from this book as from others, but will get a feel for something perhaps more important, the "culture" of the field of theoretical physics. By this I mean the whole circle of knowledge that makes up the context in which theoretical physicists think and work. A reader who does know the science and some of the stories that Freund tells will deepen his or her knowledge by learning many more that he or she was probably unaware of.

When I moved from a physics environment to a mathematics one many years ago, one thing that struck me was that I had entered not just a field that studied somewhat different material, but a whole new cultural environment, very much like moving from the US to France. Different fields have different unspoken sets of values and beliefs, derived from their different environments and different histories. Shared stories about the history of the field and the quirks of leading figures of the subject make up a large part of this common culture. Freund does an excellent job of capturing the culture of twentieth-century theoretical physics, and one could learn much more about this from his book than from any textbook or most standard historical treatments.

It's tempting to repeat here some of the stories that I learned from Freund's book, but there really are too many to choose from, so I have to just recommend that you should read for yourself. Among the physicists you can learn about here are: Schrodinger, Heisenberg, Pauli, Dirac, Stueckleberg, Feynman, Salam, Chandrashekar, Zeldovich, Landau, Touschek, Thirring, Oppenheimer (who, unlike almost everyone else, comes off badly), Nambu, and many others. A significant number of mathematicians, including Emmy Noether and Andre Weil also put in an appearance.

Freund also does a masterful job of describing the story of how mathematics and physics operated under the totalitarian systems of the last century, including a description of how the Romanian dictator Ceausescu and his wife had the mathematics institute closed down and disbanded after their daughter, who was working there, spent the night in a resort motel with one of her colleagues. He tells the stories of some of the well-known German mathematicians and physicists who either collaborated with the Nazis or joined the Nazi party, and where this led their careers. There is also quite a bit about Russian physicists and mathematicians, illustrating their attempts to survive within the Stalinist system, and the institutionalized anti-Semitism that Pontryagin and others were responsible for supporting.

Freund describes particle theory research as generally having a single leading figure that the field follows. He sees 1905 to 1925 as the era of Einstein, 1926-1943 as that of Heisenberg, a transitional period led by Fermi, with Gell-Mann dominating from the fifties to the early seventies, at which point 't Hooft takes over, followed by Witten in the early eighties. Witten's long era of dominance now appears to him to be coming to an end, and Freund nominates Maldacena as the leader for the new era which I guess has already been underway for a while, as AdS/CFT has dominated research for the last ten years.

While Freund is very strong on conveying the culture of particle theory that dominated the fifties, sixties, seventies and eighties, unfortunately he has much less of the same sort of material to help explain what has been going on for the last twenty years or so, the age of Witten and now Maldacena. There aren't any stories he has to tell about Witten, 't Hooft, or any of the other researchers whose work has characterized this recent period. Perhaps part of the problem is that they're a less entertaining lot: while I've heard a lot about Witten over the years, I can't think of much in the way of really colorful stories.

Freund's take on the current state of the subject is blandly optimistic: everything's going just fine. He mentions the Landscape and suggests Susskind's book for further reading, but doesn't see a problem there other than that "we need time and perserverance", and maybe cosmology will save the day. He does promote a more realistic point of view on the prospects for string theory, seeing it as a set of ideas that may in the future be part of some quite different real advance. His analogy is with Lagrangian and Hamiltonian mechanics, which didn't really give anything you couldn't get from Newtonian mechanics, but were necessary foundations for the truly revolutionary quantum theory.

All in all, Freund has written a fascinating book, one which any person who wants to understand more about the culture of theoretical physics can learn quite a lot from, whether they're a novice to the field, or have spent much of their life in it.

Comments

1. **Tony Smith**
   December 16, 2007

   Peter, you say "… Freund describes particle theory research as generally having a single leading figure that the field follows. He sees
   1905 to 1925 as the era of Einstein,
   1926-1943 as that of Heisenberg,
   a transitional period led by Fermi,
   with Gell-Mann dominating from the fifties to the early seventies,
   at which point 't Hooft takes over,
   followed by Witten in the early eighties.
   Witten's long era of dominance now appears to him to be coming to an end,
   and Freund nominates Maldacena as the leader for the new era …".

   As to 1926-1943, why not Pauli ? Wasn't he regarded as a leader with a strong personality who had a lot of influence through the Handbuch, not to mention being the origin of memorable phrases including the title of your blog ?
   As to Pauli v. Heisenberg, their relative leadership status should be indicated by their conflict (in the 1950s) over Heisenberg's urfield theory, in which Pauli's skepticism (expressed in terms of painting like Titian) clearly won the battle for the hearts and minds of the physics establishment.

   As to the "transitional period led by Fermi" of 1943 – 1950s, there are many reasons to consider Oppenheimer (whether he was a nice guy or a not-so-nice guy who "comes off badly") as the real leader of that period. For one example, it was Oppenheimer's eventual approval of Dyson's presentation of Feynman/Schwinger QED that may have been the pivotal point of wide acceptance of QED. For a not-so-nice example, it was Oppenheimer who pushed Bohm to leave the USA.

   As to 't Hooft being the leader from the early 1970s to the 1980s, it is certainly true that his work was the key to establishing the Standard Model, but I am not so sure that he acted as a socio/political leader. It seems to me that Glashow and Weinberg shared that role back then, and that Weinberg's approval of string theory in the early 1980s was pivotal in the widespread acceptance of string theory, and the ascent of Witten to leadership role.

   As to Maldacena being an heir to Witten's leadership position, that might happen in the future, but as of now it seems to me that Witten has a strong hold on the leadership position, being widely known to the public and also commanding huge audience attention whenever he speaks/writes anything.

It would not surprise me if a non-string theorist were to get the leadership role in the future, such as for example possibly Alain Connes if his Non-Commutative Geometry model is seen to be successful.

Peter also says "… Freund is very strong on conveying the culture of particle theory that dominated the fifties, sixties, seventies and eighties, unfortunately he has much less of the same sort of material to help explain what has been going on for the last twenty years or so, the age of Witten and now Maldacena …".
Maybe such stories might actually exist, but may not be widely circulated until after the leaders have been gone from leadership for a while.

Tony Smith

PS – As this comment is written, I have ordered Freund's book from Amazon, but not yet received it. Maybe when I read it some of the questions raised above might be answered.

2. **NEW Reader**
December 16, 2007

Hi Peter,

Do you know of similar works within the mathematical physics & mathematics establishments?

Would be curious to find out.

3. **steve bryson**
December 16, 2007

hi Peter – here's a very small bit of unexpected Witten color: In a former life I thought I was going to be a theoretical physicist, but now I work on the NASA Kepler mission, which will look for Earth-sized planets around Sun-like Stars. Last January I was at the AAS conference at which Witten was participating in a panel. I was in the Kepler booth, and up walks Witten, who starts playing with our displays showing strong interest!! I mention that I know his work and ask him what about Kepler caught his eye. He says he's really interested in extrasolar planets and stays for the better part of half and hour. He got deeply absorbed in a grade-school level educational program where you construct your own solar system by placing planets on a touch screen. It was fun to watch his delight.

steve

4. **D R Lunsford**
December 17, 2007

I am excited to read this book!

Does he mention Fermi's critical work in early QED (in the 30s)?

-drl

5. **milkshake**
December 17, 2007

There is lots of new material on Oppenheimer (and others) in the "Brotherhood of the Bomb" from Gregg Herken and I could recommend the book to people interested in the politics and personalities of the cold war.

Oppenheimer does not come off as the nicest person – but the blame for what happened to Bohm, Friedman and Lomanitz falls on their buddy Joe Weinberg who was caught on tape by FBI volunteering to spy on Manhattan Project for Soviets.

Oppenheimer did his best to protect his commie students and academic collegues (and his brother) but he found himself in terrible soup as he had hard time remebering the contradicting stories he told at different times to different security people. He used to be a CP member himself, until about 1942, and he lied about it vehemently

6. **Bee**
December 17, 2007

What comes into my mind showing a true 'Passion for Discovery' is this nice photo from Pauli and Bohr. It's hard to see on the scanned photo, but they are watching one of these tippy top gyroscopes.

7. **Peter Woit**
December 17, 2007

NEW Reader,

I don't know of a similar book in mathematics, although there are lots of books that have some stories about mathematicians.

drl,

There's actually not that much about Fermi in Freund's book, even though it is rather Chicago-centric.

8. **T.**
December 17, 2007

Hi Peter,

Thanks for the excellent review. I've been spending some time on Pontryagin's principle..did my master's thesis on the subject, back then. I had no idea he was antisemitic. What a shame.

What I knew from him was that he grew blind at a point, and his mother would help him in his research, by taking dictations. I thought this was such a beautiful story..now it's somehow stained !

9. **Tony Smith**
December 17, 2007

Bee, thanks for the Pauli-Bohr TippyTop picture.

After my high school senior class (1959 Cartersville High) got our class rings, with heavy stones on top, we spent a lot of time spinning them (heavy-end-down) on our desks and watching them turn heavy-end-up.

Tony Smith

10. **Haelfix**
    December 18, 2007

    I feel Dirac replaces Heisenberg somewhere in the early thirties as the dominant physicist. He was soft spoken and not a traditional leader, but everyone and their mother was going through his papers trying to figure him out b/c it was obvious that it was dee[and important.

    I agree with a previous commentator about Weinberg being the dominant physicist until Witten takes over.

11. **m**
    December 18, 2007

    Haelfix, do you think that Witten papers on strings are more important than the Weinberg papers on the anthropic cosmological constant?

12. **chimpanzee (aka "joe")**
    December 19, 2007

    Came upon a reference to P. Freund on L. Motls' blog:

    http://motls.blogspot.com/2004/10/frank-wilczek-thanks-and-poem.html

    F. Wilczek:

    > I also got a superb undergraduate education, at the University of Chicago. In this connection I'd especially like to mention the inspiring influence of Peter Freund, whose tremendous enthusiasm and clarity in teaching a course on group theory in physics was a major influence in nudging me from pure mathematics toward physics.

    I found a really good article on the influence of teaching, stimulated by T. Dorigo's post on Quantum Diaries (education/cheating):

    "Those who educate children well are more to be honored than parents, for these only gave life, those the art of living well."
    — Aristotle
    "American educators take this philosophy to heart – they view their jobs as preparing the citizens of the world's most influential society."

    I don't find it surprising P. Freund wrote a good book: he understands Physics ("Know your subject") & is a good teacher ("Communication is the key").

    S. Coleman was also mentioned as an influential teacher ("his reputation precedes him"). I did some research recently, & found out my instructor for grad level course on Group Theory (UIUC, Harvard PhD & Princeton post-doc) was Julian Schwinger's student!! As was a close family friend of ours: UIUC physics prof, whose wife Lillian H. co-edited "The Rise of the Standard Model: A History of Particle Physics from 1964 to 1970")

    [she is a Physics PhD (Columbia), double appt History/Physics. Like P. Freund, this is how Physics needs to be communicated: by people who UNDERSTAND physics. Not by some journalist (non-technical type) who writes articles for "entertainment value". M. Franklin/Harvard had the best quote, from her talk entitled "Why the NY Times doesn't get the right spin on our data"]

    "scholarly scrutiny" as my ex-classmate H. Rothman (UNLV History prof, dept head) puts it, is the key. He [his dad was a UIUC math prof, btw] & his colleagues had a good discussion on how TV/media can mess things up:

    "A film cannot show everything nor reach the depths of understanding found in a book or even an article, and filmmakers fall prey to the need to make it captivating for the viewer. A historically accurate
    film that bores and goes unwatched is worthless in this sense."
    — James Lewis, history prof

    This is a REALLY GOOD POINT. Film/TV/Print ("media") is designed to "entertain" (weave a story, sometimes w/"artistic license"), but History is designed to "tell" (boring facts, but can be presented in a stimulating way).

    "Facts tell [Science], but Stories [Entertainment] sells"
    — Auto Racing maxim (or any field, for that matter)

    "It failed because it was a Documentary ["facts"], instead of Telling a Story ["entertainment"]"
    — Michael xx (Emmy award producer of "Amazing Race")
    [Auto Racing (click on "chimpanzee") to see the niche-market I've been exploring. Just like Science, it's not well understood by the Public & has poor-funding]

    C. Johnson/Asymptotia had a rude awakening recently (his content was used in a sensationalistic "female breast" stunt, as humorously pointed out by P. Woit), & there is the Fox News article on G. Lisi. They focus on "Appearance", rather than "Substance".

    I noticed that journalists like J. Horgan, G. Johnson, T. Ferris are brought in to "transmit" Science to the Public. T. Ferris made a total farce with his recent PBS show (I happen to be an expert in the subject matter he was addressing), the avg viewer just doesn't know. L. Motl had some very harsh words for J. Horgan's recent foray into Science.

    Needs & Solutions

    The solution is that each scientist (& his/her respective dept) needs a *historian* (w/background in Science) to handle the "communication". P. Woit & S. Hossenfelder shouldn't have to deal with a New Scientist ("soft publication") for science news. Just have a Freund-like entity to write it up, & *distribute* it to any publications.

    The husband/wife combo @UIUC (physicist & historian), has counterparts at other universities. Caltech has physics prof D. Goodstein (who used to visit my group @JPL, for that Project Mathematics education initiative) & his wife Judith (university archivist & faculty assoc History Dept). S. Carroll/J. Oulette (English major), I guarantee SC will proofread JO's articles for science correctness.

    Moral of the Story:
    Teamwork. 2-man teams. Get one of your colleagues to handle the history/communication. Forget about external non-technical 3rd parties.

Vogan on the Orbit Method

December 17, 2007
Categories: Uncategorized

David Vogan was visiting Columbia last week, giving the Ritt Lectures, on the topic of Geometry and Representations of Reductive Groups. He has made available the slides from his lectures here.

Vogan's talks concentrated on describing the so-called "orbit method" or "orbit philosophy", which posits a bijection for Lie groups G between

- Irreducible unitary representations of G

and

- Orbits of G acting on (Lie G)*

This is described as a "method" or "philosophy" rather than a theorem because it doesn't always work, and remains poorly understood in some cases, while at the same time having shown itself to be a powerful source of inspiration in representation theory.

It is probably best understood as an expression of the deep relationship between quantum mechanics and representation theory, and the surprising power of the notion of "quantization" of a classical mechanical system. In the Hamiltonian formalism, a classical mechanical system with symmetry group G corresponds to what a mathematician would call a symplectic manifold with an action of the group G preserving the symplectic structure. "Geometric quantization" is supposed to associate in some natural way a quantum mechanical system with symmetry group G to this symplectic manifold with G-action, with the Hilbert space of the quantum system providing a unitary representation of the group G. The representation is expected to be irreducible just when the group G acts transitively on the symplectic manifold. One can show that symplectic manifolds with transitive G action correspond to orbits of G on (Lie G)*, the dual space to the Lie algebra of G, with G acting by the dual of the adjoint action. So it is these "co-adjoint orbits" that provide geometrical versions of classical mechanical systems with G symmetry, and the orbit philosophy says that we should be able to quantize them to get irreducible unitary representations, and any irreducible unitary representation should come from this construction.

That such a "quantization" exists is perhaps surprising. To a quantum system one expects to be able to associate a classical system by taking Planck's constant to zero, but there is no good reason to expect that there should be a natural way of "quantizing" a classical system and getting a unique quantum system. Remarkably, we are able to do this for many classes of symplectic manifolds. For nilpotent groups like the Heisenberg group, that the orbit method works is a theorem, and this can be extended to solvable groups. What remains to be understood is what happens for reductive groups.

Already for the simplest case here, compact Lie groups, the situation is very interesting. Here co-adjoint orbits are things like flag manifolds, and the Borel-Weil-Bott theorem says that if an integrality condition is satisfied one gets the expected irreducible representations, sometimes in higher cohomology spaces. One can take "geometric quantization" here to be essentially "integration in K-theory", realizing representations using solutions to the Dirac equation. Recently Freed-Hopkins-Teleman gave a beautiful construction that gives the inverse map, associating an orbit to a given representation.

For non-compact real forms of complex reductive groups, like SL(2,**R**), the situation is much trickier, with the unitary representations infinite dimensional. Vogan's lectures were designed to lead up to and explain the still poorly understood problem of how to associate representations to nilpotent orbits of such groups. At the end of his slides, he gives two references one can consult to find out more about this.

Finally, there is a good graduate level textbook about the orbit method, Kirillov's Lectures on the Orbit Method. For more about the orbit method philosophy, its history and current state, a good source to consult is Vogan's review of this book in the Bulletin of the AMS.

Comments

1. **Marius**
   December 18, 2007

   This orbit method of Kirillov-Kostant-Souriau is a very interesting tool
   discovered by mathematicians in the 70's. Should be read in conjunction with the work of Souriau (starting in the 60's).

   It is a typical examble of being shallow that only one author (Kirillov) of this method is mentioned, at the end of the message, moreover as
   an author of a graduate level introduction into the method.
   What about Souriau? Kostant? geometric quantization? second
   geometric quantization?
   See this, for example:

   http://books.google.com/books?id=PuhpI98ZkDgC&dq=%22geometric+quantization%22+Souriau&lr=

2. **Peter Woit**
   December 18, 2007

   Marius,

   I made no attempt in this post to discuss the history of any of this, which is why only Kirillov appeared, and only in a reference to his expository text. Geometric quantization of course has a long history. The book you mention by Woodhouse doesn't discuss at all the orbit method or the connection to representation theory.

3. **Marius**
   December 18, 2007

   Peter,

   Maybe this helps (Kirillov, A. A.
   Geometric quantization [MR0842909 (87k:58104)]. Dynamical
   systems, IV, 139–176, Encyclopaedia Math. Sci., 4, Springer,
   Berlin, 2001)

   http://books.google.com/books?hl=en&lr=&id=CCGCFCj-

QNsC&oi=fnd&pg=PA139&dq=%22orbit+method%22+Woodhouse+Kirillov&ots=o4YtWHGqQ1&sig=SUE7nUR3c_1U74GJZr4DGPm-MbI

especially section 1.3 "The statement of the quantization problem. The connection with the method of orbits in representation theory".

Indeed, maybe Woodhouse is not the best reference in order to quickly understand that all is about the fact that the "orbit method", "geometric quantization", and some topics in reprezentation theory are in fact one.

This has been discovered by Souriau, in fact. In his book (in french, then translated in english) he defines (elementary) particles as (some) coadjoint orbits of the Poincare group, after introducing what is now known as (first) geometric quantization. Each such orbit has some invariants associated with (and identifying it), like the charge, and so on. As far as I understand, Souriau ideea is just this: take all hamiltonian systems with symmetry group G, then you can classify (or decompose?) the dynamics into a family of dynamics of "particles", each "particle" being a canonical dynamical system on a coadjoint orbit of G. The "canonical" word is related to the "orbit method", which relates coadjoint orbits with "simplest" representations of G.

4. **John Baez**
   December 18, 2007

   I think Peter gave a very nice overview of a topic that's much too vast to really fit into a blog entry. One comment:

   > Already for the simplest case here, compact Lie groups, the situation is very interesting. Here co-adjoint orbits are things like flag manifolds, and the Borel-Weil-Bott theorem says that if an integrality condition is satisfied one gets the expected irreducible representations, sometimes in higher cohomology spaces.

   This is true, but it makes the situation sound a bit more "scary" or "bad" than it is. It's actually as beautiful as you could hope. Every finite-dimensional irrep of a compact simple group comes from geometrically quantizing an integral coadjoint orbit. And every integral coadjoint orbit gives you a finite-dimensional irrep of the compact simple group. There's a 1-1 correspondence!

   And, if you happen to know the other famous way to get your paws on these finite-dimensional irreps – namely, from integral weights in the Weyl chamber – you'll be pleased to know that an "integral weight in the Weyl chamber" is just what you get when intersect an integral coadjoint orbit with the Cartan, and look at the point that lies in the Weyl chamber.

   Well, I'm sure that sounds scary too to most people. But it's really great.

   On the other hand, I'm still terrified of the noncompact case, which is where Vogan comes into his own.

5. **Peter Woit**
   December 18, 2007

   Thanks John,

   Didn't mean to make the compact case sound scary (yes, the non-compact reductive one is…). It's an incredibly beautiful story, one that needs more like a few weeks of a graduate course than a blog posting to do justice to it.

   There is one subtlety that you don't mention which fascinates me: there are two possible choices of which orbit to associate to which representation (the "rho-shift"). Does the trivial representation get associated to the trivial orbit, or to the orbit of half the sum of the positive roots? Opinions differ. And half the sum of the positive roots is the highest weight of the spinor representation, for spinors on the flag manifold. The Dirac equation is everywhere…

6. **John Baez**
   December 19, 2007

   Peter writes:

   > There is one subtlety that you don't mention which fascinates me…

   You too, eh?

   > Does the trivial representation get associated to the trivial orbit, or to the orbit of half the sum of the positive roots?

   This happens to be something James Dolan has been obsessed with over the last month… and my guess as to the solution was confirmed in Dieudonne's book on Universal Enveloping Algebras. I don't want to give it all away, since I plan to write about this in This Week's Finds someday, but there are two different ways to get a representation of a complex simple Lie algebra G starting from a representation of its Borel B: induction, and coinduction. These are dual in some sense, as the names hint – but there's a kind of compromise halfway in between, which involves taking the induced representation and tensoring with a special representation whose highest weight is "half the sum of the positive roots".

   But here's what finally convinced me this stuff make sense. The line bundle on the flag manifold (G/B) whose sections give this special representation is just the bundle of *half-forms!*

   Strictly speaking I should say "a" bundle of half-forms, not "the". Given an oriented n-manifold, a bundle of half-forms is any line bundle whose square is the bundle of n-forms. An n-form is something you can integrate over your manifold, so a half-form is something whose *square* you can integrate. This is why "tensoring with a bundle of half-forms" is important in geometric quantization: it gives you a bundle whose sections naturally form a Hilbert space!

   There are typically lots of square roots of the bundle of n-forms. But in the case of a flag manifold G/B there's a god-given choice: the bundle induced from a certain special 1-dimensional representation of B, namely the one corresponding to "half the sum of positive roots"!

   So, it's all starting to make sense. It's sad how hard it's been to squeeze this explanation out of the literature. Lots of books on Lie algebras talk about "half the sum of positive roots". But few seem to give the geometrical explanation in terms of geometric quantization. I guess the authors are trying to stay pure, and hide the geometry behind a veil of algebra.

7. **John Baez**

December 19, 2007

By the way, my remark about something “seeming more scary than it really is” concerned the phrase “sometimes in higher cohomology spaces” in your description of the Bott-Borel-Weil theorem.

When you have a *positive* weight, it gives you a line bundle on the flag manifold G/B whose space of holomorphic sections form an irrep of G. Only when you want to terrify people who haven’t fallen in love with sheaves should you call this space of holomorphic sections the “0th cohomology” of the line bundle – or really, its corresponding sheaf of holomorphic sections.

The scary jargon becomes more forgivable when we consider *nonpositive* weights. These again give line bundles on G/B, which again give irreps – but only when we take some *higher* cohomology group.

However, even in this case, the jargon is a bit more scary than necessary. It came as a great relief when I discovered that a “higher cohomology group” of a holomorphic line bundle was just a fancy way of talking about the space of holomorphic sections of this bundle *tensored with another bundle* – I guess a bundle of p-forms.

Another digression:

The fact that I’m talking about half-forms when you’re talking about spinors suggests that I, at least, haven’t gotten to the very bottom of this “half the sum of positive roots” business. Both half-forms and spinors are “square roots” of something – but different somethings, and in different ways! And, they both show up as correction fudge factors in geometric quantization, but I don’t quite understand how they’re related.

I bet Eckhard Meinrenken does.

8. **Peter Woit**
December 19, 2007

John,

I’m looking forward to seeing your version of why the so-called “rho-shift”.

One indication that spinors are the right way to think about this comes from the following: Borel-Weil and Borel-Weil-Bott both crucially depend on the choice of invariant complex structure, which is the same as the choice of positive roots. Acting by the Weyl group changes the complex structure, makes dominant weights no longer dominant, and moves the representation from holomorphic sections to higher cohomology (or, OK, higher order differential forms). If you use the Dirac operator and spinors instead, none of this happens, the whole set up is completely Weyl-invariant.

9. **a.k.**
December 19, 2007

..at least as far as symplectic spinors are concerned the latter is possibly reflected by the fact that any compatible complex structure on a symplectic manifold (i.e. on a coadjoint orbit) gives rise to a reduction of the metaplectic structure of the manifold to a two-fold-covering of the unitary group, contained in the centralisator of the latter is a one-parameter-subgroup induced by the element in the Lie-algebra whose image under the exponential map projects to the canonical complex structure of R^{2n}. This subgroup factors to a familiy of representations of the circle over the eigenspaces of the harmonic oscillator, the corresponding splitting of the spinor bundle reveals as the ‘lowest eigenmode’ of the harmonic oscillator a line-subbundle, this is then a ‘canonical’ choice for a half-form on M (depending on the complex structure). Associated to a Lagrangian polarization is a O(n)-reduction of the spinor-bundle resp. of the above line-subbundle, this is the ‘half-form’ widely used in geometric quantization, as it seems.

10. **strangerep**
December 19, 2007

Hi Peter,

Good pedagogical explanations of the orbit method seem to be rare.
I looked at the brief section in your course notes (the section at
the end on the momentum map and the orbit method), but it was
a bit too brief for me to get a thorough understanding. I’ve tried
Kirillov’s course notes, but they require too much from (this) reader.

Is there any chance you could expand your course notes? E.g.,
talk in more detail about the H1 and SU(2) examples you mention,
and perhaps some others? There’s not quite enough detail on the
H1 case to follow it properly, and the SU(2) case is only couple
of paragraphs?

11. **Peter Woit**
December 20, 2007

strangerep,

I’ll be teaching that course this semester again, maybe I’ll get a chance to expand the notes, we’ll see.

I probably also discuss the SU(2) case when I discuss Borel-Weil theory, it’s really the same thing, which is one reason for not going on much about the orbit method in the compact group case. In the case of SU(2) orbits are just spheres, weights correspond to line bundles, for weights of the right sign quantization is just taking holomorphic sections of the line bundle. Quite concretely, such sections correspond to homogeneous polynomials of degree given by the weight.

The Heisenberg group case probably does deserve a lot more detail….

12. **Bruce Bartlett**
January 6, 2008

Hi Peter and John,

I’ve been trying to understand the basics, namely this geometric way of thinking about representations of compact Lie groups for a while now; various entries in this blog have helped me in many ways -thanks! However, can you point me to an explicit “equivalence of categories” statement?

For instance, my favourite way of conceptualizing it is the way of Guillemin and Sternberg in their book "Symplectic techniques in physics" (see the section on Kaehler manifolds). This is the approach which doesn't bother with coadjoint orbits in the dual of the lie algebra, but rather works directly with the representations themselves. The correspondence is roughly:

representations of a compact Lie group G complex G-equivariant line bundles having a finite number of orbits

Note I didn't say "irreducible representations"; I'm looking for an equivalence of categories here. A morphism between equivariant line bundles category on the right hand side is an *equivariant kernel* , i.e an equivariant collection of maps f_{x,y} : L_x –> L'_x.

From left to right, given a representation V of a compact Lie group G, one simply takes the G-orbits in the projective space of the representation which are complex submanifolds. (This is a slightly more intrinsic procedure than finding these orbits in the dual of the lie algebra). From right to left, one just takes holomorphic sections.

Does this make sense, and do you know of a precise statemen to this effect, so as to obtain an equivalence of categories?

I agree with Peter in that I am also fascinated by the spinor side of the story. Basically, my understanding (from the book "Heat kernels and Dirac operators" by Berline,Getzler and Vergne) is that under this geometric correspondence, the character of the representation computes in terms of the equivariant index of the Dirac operator living over these G-sets; in fact it localizes over the fixed points.

13. **Peter Woit**
January 6, 2008

Bruce,

In your way of associating a geometrical object to a representation, doesn't the orbit depend on which vector you pick?

I'm fond of the K-theoretic "quantization=integration" idea, but to turn this into a statement about categories (of equivariant vector bundles on one side, representations on the other) you need to "categorify" it, and I haven't thought about that. The people who seem to have precise equivalence of categories statements of this kind are those thinking in terms of D-modules. I think the Beilinson-Bernstein localization theorem is the kind of precise statement about equivalence of categories that you want. In the compact Lie group case, maybe it can be restated in various other ways.

14. **Bruce Bartlett**
January 6, 2008

Thanks for the reply! Peter wrote:

> In your way of associating a geometrical object to a representation, doesn't the orbit depend on which vector you pick?

No… that's what I learnt from Guillemin and Sternberg. I guess you are saying to me that it is the orbits of the maximal weight vector which are the orbits we need to pick out, and somehow this "maximal weight vector" requires a choice. But the more intrinsic way of looking at it is that, at least for irreducible representations (heh, I am out on a limb for reducible reps), there is only *one* orbit which is a *complex* submanifold of the projective space P(V).

In other words, from an intrinsic point of view, weight vectors don't come into it at all. One simply needs to "look for the complex orbits, Luke" (insert Obi-Wan voice here).

> but to turn this into a statement about categories… you need to "categorify" it…

Well, indeed I *am* in the "categorificaion" game; I work with "2-representations" and all that jazz. But here we're working with ordinary representations…. surely I'm not asking for a "categorification" of anything, I'm just asking for a nice elegant statement of the geometric correspondence between ordinary representations of Lie groups and "equivariant complex line bundles"… if there is such a statement, one would expect it to be phrased as an equivalence of categories.

> The people who seem to have precise equivalence of categories statements of this kind are those thinking in terms of D-modules.

Gulp! Sadly all that stuff is over my head. I'll crawl back under my bridge now.

15. **a.k.**
January 7, 2008

..if we are already that far: one should possibly not forget that the actual goal was to quantize classical *observables*, an observable in classical mechanics is nothing more than a function (for instance a 'symbol' on a coadjoint orbit), to these functions one aimed to associate more or less pseudodifferential operators on appropriate vectorbundles such that this correspondence is equivariant w.r.t. appropriate 'products' on both sides. I only want to point out that one advantage of the spinor view is that it makes it possible to associate to a certain class of functions (again, 'symbols') on a cotangent bundle *sections* in a (symplectic) spinor bundle, to do this, one needs a 'half-form', which is a subbundle of the symplectic spinor bundle, to compensate for an ambigous choice of (local) 'signs'. On the other hand one can localize functions on the zero-section of the cotangent-bundle and encode them as well as spinors, this is analogous to the 'micolocal lift' in deformation quantization. Using a Fouriertransform for spinors one can pair these two sections by the pointwise (spinor)-L^2-product over the manifold and gets the action of a pseudodifferential operator on smooth functions on the zero-section of the cotangent bundle which is exactly that one used in deformation quantization to define the *-product on symbols.

I never saw this spinor-quantization construction anywhere else than in my not-quite-recent diploma thesis, which is (funny enough) in german, but one should recall that a correspondence between orbits and sections of vectorbundles is only the very first step in quantization and unless somebody enlightens me and one is not conerned about varieties and algebraic groups, I do not see what <i<exactly the 'categorification'-theme aims at. The (symplectic) spinor-business seems for no clear reason to be a bit mystified and judged to be oversubtle or impenetrable, nevertheless it arises very naturally in the symplectic context, that is, in the context of quantization.

16. **Peter Woit**
January 7, 2008

Bruce,

I was just using the word "categorification" to show off, since one of the few things I know about it is that people use "decategorification" to mean taking K-theory….

Thinking more about it, what I was thinking about clearly can't work, but in more detail, the idea was that the index map
K_G(G/T) to K_G(pt.)=R(G) relates the K-theory of the category of G-equivariant vector bundles on the flag variety to the K-theory of the category of representations of G.

But, even at the level of K-theory, this isn't an isomorphism, since
K_G(G/T)=R(T) and R(G)=R(T)^W. The map is "Dirac induction", maybe there's some way of thinking about this though that does give an isomorphism.

As for D-modules, in this case Beilinson-Bernstein is just another way of restating Borel-Weil-Bott, but in a way that I guess gives an equivalence of categories. You need to just see what it is saying in this special case. I think the new book of Hotta et. al. explains this, also some expository papers of Schmid. Might or might not be in some lecture notes of David Ben-Zvi, who is someone who certainly understands this stuff.

17. **Bruce Bartlett**
January 7, 2008

Thanks, you've given me lots of leads to follow up here.

Disastrous FY 2008 DOE Budget

December 18, 2007
Categories: Experimental HEP News

The White House and the Congress, several months into the 2008 fiscal year, finally seem to have come to agreement on a budget, one that fully funds the Iraq war, but has a huge cut in the budget for DOE particle physics research. According to the AIP FYI bulletin, the DOE HEP budget for FY2007 was $751.8 million, and the White House had requested and Congressional committees agreed to $782.3 million for FY2008. The new budget agreement provides only $688.3 million, an 8.5 percent cut from last year. The cut eliminates funding for NOvA this year at Fermilab, and effectively shuts down R and D on the ILC, providing only 25% of the requested amount, much of which has already been spent.

Pier Oddone, the director of Fermilab writes in the December 18 Fermilab Today:

> This is a body blow to the future of the ILC, the U.S. role in it and Fermilab…. These proposed cuts, which come on top of the very limited particle physics budgets of the last few years, are destructive of our field and our laboratory. There is no way to sugar-coat this… If this bill becomes law I will be discussing consequences with you in more detail. Until then, I and many others who understand this disaster in the making are trying to inform Congress and the Administration of the dire consequences to the U.S. particle physics research program. These may be unintended consequences that were not considered in the pressure-cooker atmosphere that accompanies an omnibus budget bill.

It's not clear to me what the prospects are for doing anything about this at this late date in the budget process.

Update: More here, here, here and here.

Update: Also here, here, and here. A spokesperson for Fermilab says "This is the worst funding crisis in the history of the laboratory, no exaggeration" and that one option being considered is shutting down the lab for a few months. Lederman places the blame on spending for the Iraq war and says "I've been around this lab since it was all farmland, and I can't remember a crisis of this severity". Part of the problem may be the resignation of Dennis Hastert, who had been both the House Speaker and the representative for the district including Fermilab.

Update: JoAnne Hewett has more at Cosmic Variance.

Update: At an all-hands meeting at Fermilab, the director announced that the budget of the lab would be cut $52 million over what they had been expecting for the rest of the fiscal year. Dealing with this will require eliminating 200 full-time-equivalent positions, about 10% of the people working at the lab. They will immediately start shutting down ILC and NOvA, They will try and not shut-down the lab, focusing on keeping the Tevatron running, but will have a system of rolling 2 day/month furloughs, with not everyone furloughed at once. He said the first he heard about this was on Monday. It remains unclear who was responsible for this decision, which seems to have been taken in haste, with very few people involved. It also remains very unclear what this means for next year's budget, or for the future of the ILC and NOvA.

Part of the story here seems to have been that there was a Congressional decision to fund member's earmarks, while cutting scientific research that was not funded this way.

The APS has issued a press release about this which states:

> This action sends a strong message to the world: The U.S. is prepared to jettison support for one of our flagship areas of science that probes fundamental laws of the universe.

The press release also criticizes the Congressional decision to preserve and expand earmarks while cutting other programs:

> The APS notes with some dismay that had Congress applied the same discipline to earmarking as it did last year, the damage to the science and technology enterprise could have been avoided.

Update: One peculiar aspect of this story is how little attention it has gotten from the press (other than the local Illinois press) and from science blogs, where all I've seen is mention at Cosmic Variance and Tommaso Dorigo's blog.

The congressional representatives for the Fermilab district have put out a press release (on the Durbin and Biggert web-sites, looks like Obama couldn't be bothered to put it up) calling on the DOE Office of Science to "increase the funding request" for HEP in the proposed FY 2009 budget. The language used seems to me to be rather weak, since it doesn't mention either a size of increase or what base to use. See this comment that just came in for possible news about attempts to restore some of the Fermilab funding.

Update: The Obama web-site now has the press release. There's an article about this today in the New York Times.

Comments

1. **John Baez**
   December 19, 2007

   What's NOvA?

2. **RZ**
   December 19, 2007

   What's google?
   http://www-nova.fnal.gov/index.htm

3. **Roger**
   December 19, 2007

   I recently heard some possible good news for our field. Someone said that the Germans had increased their particle physics expenditure. Can any Germans confirm this ?

   Peter, I hope you don't view this as off-topic. The UK and US decisions have been body blows to the whole community. It would be interesting to discuss worldwide particle physics funding here.

4. **Anti-insurgent**
   December 19, 2007

"The White House and the Congress, several months into the 2008 fiscal year, finally seem to have come to agreement on a budget, one that fully funds the Iraq war …"

War gets more media attention because it makes human interest stories: tens of thousands lose friends and relatives. This is what politicians really respond to. So if these particle accelerator physicists *really* want decent funding, they should produce military versions: massive particle ray guns to fight satellites, missiles and aircraft. It's a pity that modern physicists aren't so interested now in combining particle research and warfare. Back in the good old days when Feynman, Oppenheimer, Teller, Fermi, Lawrence et al. worked on the Manhattan Project, they got two billion for research (in 1945 dollars, uncorrected for inflation) just for delivering two small bombs.

5. **Hans**
December 19, 2007

Roger wrote:
I recently heard some possible good news for our field. Someone said that the Germans had increased their particle physics expenditure. Can any Germans confirm this ?

I would not be too optimisitc. Some of it may be only du to needed phenomenological research with LHC data. In fact the increased budget with some projects in High energy physics came somewhat "accidentially".

In germany, research is founded by governments of each of the federal states and the federal government, with the states founding their universities, and the government founding the Max-Planck institutes and the DFG, a society similarly to NSF in US.

Recently there was an initiative, that the government will spend additional money to some of their universities, which have won in a competition after being evaluated by a DFG comission.

And accidentially, most of the physics projects that were positively evaluated, were High Energy Physics projects.
Obviously, this has something to do with the large experimental facillities at CERN and elsewhere, with which these projects collaborate.

That is: To fund high energy projects was not planned by german government at all. Unfortunately, most of it is experimental research (almost no theory project survived the competition at DFG), and even this experimental HEP research is funded only due to the fact that the politicians agreed to spend their money on exactly those projects, the people of DFG told them to do so. Funding HEP was not, what the politicians had in mind.

Instead, recently, german government decided, not to build ILC anywhere in germany. Unnamed officials in the ministry tell, that the future of HEP funding strongly depends on LHC, with government definitely not wanting to fund any large accelerator projects at a stage where one does not know what LHC will see.

6. **Bee**
December 19, 2007

Reg Germany, see e.g. The Terascale Alliance.

7. **woit**
December 19, 2007

John,

NOvA is Fermilab's main next-generation neutrino experiment, looking for muon to electron neutrino oscillations, trying to measure a non-zero value for the mixing angle theta_13. The project involves upgrading some of the Fermilab accelerator facilities, and constructing a near detector on-site, and a far detector in Minnesota.

Shutting this down and shutting down ILC R and D pretty much shuts down any investment in the future of Fermilab for the remainder of FY 2008.

8. **wb**
December 19, 2007

To make the ILC picture darker, the UK has stopped ILC related funding this year.

9. **Bee**
December 19, 2007

Hans: *That is: To fund high energy projects was not planned by german government at all. Unfortunately, most of it is experimental research (almost no theory project survived the competition at DFG), and even this experimental HEP research is funded only due to the fact that the politicians agreed to spend their money on exactly those projects, the people of DFG told them to do so. Funding HEP was not, what the politicians had in mind.*
They had in mind to fund the best and most promising research, for which they rely on expert's advice. That's the sensible thing to do. One can discuss whether it's good to do this via the DFG and what to think of their refereeing system, but I think it works pretty well. The problem with the German physicists is the dominace by not yet quite dead nuclear physicists for whom 'high energy' is somewhere around 10 GeV.

The US budget disaster just reflects the fact that this democracy is a farce. I'd like to see a poll on the question whether to blow up billions of dollars in funding actual or potential wars and loosing researchers to the North, East and West, or whether to assure technological and intellectual expertise in the own country does at least remain at last century's status (I won't even aim at improving it). If it goes on like this, I see the Europe -> US brain drain reversing. I can't avoid thinking of George Orwell's 1984 scenario with the country that engages in permanent war, things get constantly worse, but they report all the time great improvements. I'd recomment setting up the Ministry of Truth.

Best,

B.

10. **Yatima**
December 19, 2007

But that's only 100 million short, a third of the price of a single completely useless F-22 Raptor!

Anyway, (and I'm sorry to say so) if the letting go of something like Dennis "The Menace" Hastert is considered "a problem", then it's not only String Theory that needs to get back in touch with Base Reality. Choose your friends with care.

11. **JoAnne**
December 19, 2007

Recall that there are three national laboratories in the Bay Area near Nancy Pelosi's San Francisco district.

12. **Peter Woit**
December 19, 2007

JoAnne,

Good point. I fear this is a bi-partisan screw-up. Any idea why HEP got whacked so much worse than other sciences in this last minute round of budget cutting? What are the implications for SLAC of this (so far most of the coverage has concentrated on Fermilab)?

13. **Hans**
December 19, 2007

Bee wrote:
The problem with the German physicists is the dominace by not yet quite dead nuclear physicists for whom 'high energy' is somewhere around 10 GeV.

I see it more that solid state physicists are a problem.
They often are making campaigns against allmost all ground based research. They can argue to the politicians that their science may lead into industrial products. And so, nanoscience, solid state physics and so on, receive a great more amount of money than mathematical physics or phaenomenology(at least in germany).

For example, Munich has, with its two universities and five Max-Planck Institutes one of the largest hep groups in the world.

And even there, the chancellor of the Ludwigs Maximillians University, Huber had circulated a document, in which High Energy physics was defined as an area to withdraw in future.

There had letters to be written from international persons to make Huber give up this plan.
It seems that there were a large amount of condensed matter physicists in the advisory board of the chancellor.

I know of another group where condensed matter physicists were even sucessfull in knocking down two professorships in the area of quantum gravity. The professorships of Heinz Dehnen, and Audretsch in Konstanz was a general relativity group. But the majority of condensed matter physicists decided, to cancel this professorships after the two men retired.

It seems that Germany has only 5 chairs devotet to relativistic physics (stringtheorists and particle theorists excluded, one at Jena, Kologne Freiburg, Tübingen and Munich).
It is known to the author that a majority of solid state physicists sometimes boycotted relativists, which wanted to hold conferences at the german physical society.

(and regarding your Quantum gravity group in Frankfurt: When a decision is made about a new research group at a university, DFG often has physicists of other areas in the decision comittes. When it is to decide about an area, where there are not many experts in Germany, those physicists from other areas have the majority. And then you might think what they decide……)

14. **wb**
December 19, 2007

SLAC gets hits moderately because it has had a fairly decent sized ILC program. Fortunately for the laboratory its prime program, LCLS, was fully funded. Besides citing Pelosi, Dick Durbin doesn't seem to have done much to defend Fermilab. So I'd call it a bi-partisan screwing – the cuts seem to have been deliberately placed not a mistake.

15. **JoAnne**
December 19, 2007

Peter,

I echo wb above, the language in the bill indicates that the cuts are deliberately placed. I have no idea why, except that the large international science projects ITER and ILC obviously didn't fare well.

As for SLAC, most of the lab itself is now supported by the Basic Energy Sciences division of the DOE. However, the particle physics and astrophysics division of SLAC is supported directly by the HEP division of DOE and will be severely hurt. We are (or should I say were) the major US player in the R&D effort for the ILC and there is no way we can now support that. In addition, we have (had) a sizeable effort on an ILC detector. Any ideas on saving money are very draconian. Just like Fermilab, folks are talking about shutting off the B-Factory or having "scrooge days or weeks" where people work without getting paid or layoffs. Little things like no travel money or new computer equipment for the rest of the year and a hiring freeze will be implemented, but will not be nearly enough.

16. **wb**
December 19, 2007

One theory I have heard about the reason for these targeted cuts was that ILC and ITER had received explicit White House backing. So these cuts would embarrass the Administration. That rationale does not explain zeroing Nova. Perhaps that choice was due to a staffer doing his/her homework and finding such physics as the lowest priority in EPP2010.

A difficulty that the lab directors face in trying to react is the money is not fungible and the cuts target people in specific organizations within the laboratory. It is hard to imagine that layoffs are not in order. If you expect this to be a one-time phenomenon, then you tighten the belt a lot and tough it out. But we have no reason to believe that science will fare better in the next budget – and as 2008 is an election year, the legislative season is shortened. Hence a continuing resolution or another Omnibus Bill is likely.

Perhaps a unified push by the presidents of major universities joined by CEOs from major hi-tech industries would help rescue the long range future of science in America.

17. **LDM**
December 19, 2007

In the US democracy one has the freedom to become politically active and change things if one is not happy with the government policies. One also has the freedom to openly express dissent.

Contrast this with Russia – also a democracy – where just this month the opposition leader, Gary Kasparov, was jailed for 5 days for attempting to hold a rally in opposition to Putin. Regarding the 'ministry of truth', there was no mention of the arrest on the Russian state controlled TV. And the Russian elections were totally falsified – which was made possible by the failure of Russia to grant international observers the needed visas so they could monitor the elections.
The US democracy, while not perfect, is still the best available, and taking 8.5 percent of the HEP budget to support our troops is a sacrifice we should be willing to make,

if indeed that is where the 8.5 percent is being allocated. I would suggest that such a sacrifice is nothing compared to that being made by our troops.
Being an excellent physicist (or mathematician as the case may be) does not imply one is also politically astute. Oppenheimer comes to mind as the canonical example.

18. **Peter Woit**
December 19, 2007

LDM and others,

If you have an intelligent comment about the HEP budget situation, please post it here. If you want to rant about the Russians, or otherwise engage in predictable political polemics, do this on one of the 1000 blogs out there devoted to this, not here.

19. **IMHO**
December 19, 2007

To inject a little clear-eyed realism into the conversation.

Why haven't you people been expecting this….I have. Why do you think HEP is first to the slaughter? It's because HEP has the lowest (cost to benefit-to-society) ratio of all the sciences. HEP has completely and utterly lost touch with the real world. These days it seems more and more like mental masturbation at it's finest.

Can we have hundreds of millions of dollars to study phenomenon with absolutely no societal or economic benefit what-so-ever…???

Welfare is never popular.

20. **Bee**
December 19, 2007

Hans: I agree, the nuclear guys aren't the only problem. In general I find Germany has been much too conservative. I think though the DFG is doing a good job, at least as good as they can, and from the little that I notice over here, things are slowly getting better in Germany. The biggest problem they face though is one that the DFG can't solve, the missing tenure process, the habilitation, and the 'Berufsbeamtentum' (I guess you're German so you know what I mean). A correction: There never was a QG group in Frankfurt, and unless a miracle happens I don't think there will be one in the soon future. There was, long time ago, a Quantum Gravity seminar, but that was ironically offered by the maths department. All we tried was physics beyond the standard model, and despite the large interest among the students and postdocs, and support from the faculty, we didn't get funding. Maybe we'd have better chances today. Best,

B.

21. **Tom**
December 19, 2007

IMHO,

Either you are a non-science oriented person or have lost touch with the fundamentals of technological progress. Society progresses technologically by first understanding nature, then understanding how to manipulate nature, and finally understanding how to do this in a way that everyone can benefit. Case in point Quantum Mechanics. At the time, nobody thought it would turn into anything of use. Niels Bohr himself thought it couldn't possibly be of benefit because nothing of use could come of something so small. 50 years later the transistor is invented, and 50 years after the computer is without a doubt one of the most essential parts of our existence. This process has been the same through our entire existence. Maxwell's equations into RF electronics, Kinematics/Static science into the design of anything mechanical. Thermodynamics and statistical mechanics into modern chemical engineering. We first understand nature, we then learn to manipulate it. Calling the first step unessential is claiming you don't need to mine gold to make a gold ring.

22. **Peter Woit**
December 19, 2007

Please, this is not a blog entry about whether particle physics is a worthwhile subject that deserves public support. If you want to argue about that, do it elsewhere.

23. **Tom**
December 19, 2007

Peter, it seems to me a blog entry on the HEP budget is the perfect place to argue whether the public should support or not support such efforts, but as it is your blog……

I will comment that I believe this is the very reason we are in trouble. We choose not to address those questions in a way that the public values the research.

24. **Peter Woit**
December 19, 2007

Tom,

Since this is a blog aimed at people with a serious interest in particle physics, the problem is that arguments about why it is a worthwhile subject will be preaching to the choir. If people want to put their energy into changing the minds of those who know nothing about this subject or are dubious of its value, that's great, but the right audience isn't here.

I don't actually think the problem is with the public not supporting this kind of research, which I think they support as much if not more than most scientific research, to the extent they care at all. The problem is that, at the last moment, some small number of people without any public discussion (or even private discussion with prominent scientists, as far as I know) made the decision to make US HEP research bear the brunt of this year's budget cuts. I'm curious to know why that happened, and what the implications are for the future.

25. **milkshake**
December 19, 2007

Hubble telescope was going to be "left for dead" by NASA at one point – and astronomy at least produces pretty pictures…

Arecibo was "de-funded" too, if I remeber correctly.

26. **wb**
December 19, 2007

Peter,

I think one should dissect the HEP cut even further. Almost all of the cut is in that portion of the HEP budget that was a foundation for Fermilab's future. Where can Project X go without superconducting rf development? How does Fermilab gain traction into a neutrino future with Nova zeroed? As for ILC, this cut seems to render that possibility comatose until either Japan or China steps up to the plate.

So I'd ask where was the Illinois delegation. Why did Sen. Durbin give a cold shoulder to pleas for help. So I'd like to understand why this shot directly amidships to the flagship of American high energy physics. Who had it in for Fermilab? There certainly was money that could have gone to HEP out of the $125 M of earmarks -pork – that was inserted into the Office of Science budget.

27. **Tom**
December 19, 2007

Peter,

The comment I was replying to is evidence that we are not "preaching to the choir". I think you underestimate the diversity of your audience. I also see great value in educating the lay with the correct arguments for basic science research. With this said, I really don't want to argue whether or not we should be discussing whether or not to fund basic science. So please continue to sleuth out the reasons the HEP budget took such a nasty hit. If you are able to derive the inner workings of how federal budget decisions are made, you should probably publish the work. Maybe even a book – you could recycle the same title as your last work.

28. **Sam**
December 20, 2007

wb:

Here's my take.

If the elected representatives of the American people want to decide not to do High Energy Physics, they clearly have the right to make that decision. If that is their intention, they should come out and say so. The budget as passed scraps the NOvA project (Fermilab's short-term upgrade project, to come online after the Tevatron is superceded by CERN's LHC.) NOvA is a stepping-stone on the way to Project X (the lab's medium-term upgrade plan, currently in the early planning stages). The budget also eliminates any further spending this fiscal year on the ILC (Fermilab's long-term plan). In short, it eliminates all spending on the future of accelerator physics in the US. The statement it makes looks rather like "carry on operating your current program, and go away and quietly die in 2010".

The reports talk about 200 people losing their jobs, but layoffs don't actually save you any money in the current fiscal year (as you have to pay termination and unemployment benefits). The only way to save significant amounts of money is to turn off the accelerators and send everyone home without pay for a month or so. You can do that.

Many of those people will find other jobs, rather than sitting around doing nothing for a month. The people who find it easiest to get interesting new work are going to be the employees that were most useful. They'll provide your 200 job cuts, but it'll be the 200 people you could least afford to lose. It might not quite be a death blow, but it'll be very close.

Does Congress really want to destroy the US High Energy Physics program to save $60 million? Maybe it does, and maybe it doesn't, but that's what it's done.

If that's really what Congress wants, it should come out and say "sorry, we're not going to be doing this any more" and then everyone knows where they stand. A few months ago (COMPETES etc.), they seemed quite keen on doing this kind of thing, so maybe it was more a case of a budget-crunch screwup that didn't understand the full implications of the cuts.

29. **Chris W.**
December 20, 2007

Arbitrary cuts with little regard for "functioning/crippled" thresholds is sort of business as usual for Congress and the current administration, isn't it? I think the attitude is basically "whatever; after a while they'll stop whining and figure it out." Even in the conduct of the Iraq War a lot of things have been approached as "throw 'em in the deep end of the pool and let 'em figure out how to swim."

You may also recall tax-cut activist Grover Norquist's declared ambition to reduce the size of the federal government (and its programs) to the point where "they can be drowned in a bathtub".

Then again, I suppose one should remember the old saying, "never ascribe to malevolence that which can be explained by incompetence"—passing 3,000 page bills that nobody has actually read.

(The late hour must be exacerbating my cynicism—sorry.)

30. **wb**
December 20, 2007

Sam,

I completely agree with your comments. COMPETES was passed and signed into law; the mark-ups in the Energy and Water Appropriations bill were very supportive. So what changed? In the Omnibus Bill HEP gets to compete with rural hospitals for money, but other Office of Science programs have the same competition. Yes, it is the perogative of Congress to decide what to fund and what not to fund. It is our right to ask why. I just don't buy the inadvertent "screw-up" theory. As you say, "hey should come out and say so."

31. **Chris Austin**
December 20, 2007

As a British physicist who might be indirectly affected by these cuts, (via reduced likelihood of another US visit), I would like to suggest that these cuts might be because the US government has identified that the ILC isn't expected to make fundamental discoveries in the same way as the LHC is. In other words, this might be a wake-up call, with the government saying, "You've got to do better." I understand that the financial situation will be difficult for everyone, but since the appropriate direction for HEP to make further fundamental discoveries after the LHC depends so completely on what is found at the LHC, perhaps it would be good for the next three or four years to focus on maximizing the return from the LHC and drawing the implications from its results, which is sure to be quite difficult.

Best regards,
Chris

32. **Steve Demuth**
December 20, 2007

I will respect Peter's request that we not use this as a forum to debate the value of HEP funding, but I do want to say that I disagree with that judgment. I am not a professional physicist, but I do follow physics closely, and I doubt I am the only such person who reads this blog. I would welcome a discussion about the cuts that focused on why they are justified or not.

And … depending on the case made, the results of such a debate could well be a letter from me to my representatives opposing – or endorsing – the cuts.

33. **Peter Woit**
December 20, 2007

Steve (and others whose comments I've deleted and will continue to delete),

What I write here about experimental particle physics and its funding is intended to provide useful and accurate information to people interested in what is happening in this area. I attempt to do so with a minimum of editorializing. This particular event is an extreme case, in the size and timing of the budget cut, and the lack of transparency in how it was arrived at. I hope that you'll take advantage of what this blog does provide, accurate information, in reaching your own decisions about what you think the federal government's spending priorities should be. I hope you'll also realize that there is some advantage to not having to wade through large numbers of comments from people discussing their feelings about government spending, swamping actual information about what has just happened.

On this blog I'd like as much as possible to make this deal with my readers: I'll do my best to spare you my blathering about political issues, please spare me yours. If you want to find a blog where political debate on scientific issues is encouraged, there are other places to go, Cosmic Variance is a pretty good one.

34. **Michael Bacon**
December 20, 2007

"The press release also criticizes the Congressional decision to preserve and expand earmarks while cutting other programs."

Actually, earmarks declined from the prior budget somewhere between 25% and 43%, depending on who you want to believe. Obviously, still too high, although it's hard to imagine members of Congress completely eliminating this type of special funding for their districts.

The real problem is lack of understanding and leadership in Congress and the White House and a broad swath of the population that not only doesn't understand what's at stake, but that doesn't even think about it.

35. **Peter Woit**
December 20, 2007

Michael,

I'd be curious to know a source for solid numbers about this. A little research on the net turns up the claim that there were essentially no earmarks in the FY 2007 DOE budget, but that this year's final budget language includes

"Funding under this heading in the amended bill includes $125,633,000 for Congressionally Directed Projects."

At the FNAL all-hands meeting, someone who was not very happy about it brought up the claims by Illinois senator Durbin from his web-site that he was responsible for $448 million in earmarks for Illinois projects. I think the implication was that his focus on bringing in earmarks, in an environment where more earmarks meant bigger budget cuts for labs like FNAL, was part of the problem.

36. **MathPhys**
December 21, 2007

Best wishes on the festive season, Peter, and thank you very much for a very informative blog.

37. **Michael Bacon**
December 21, 2007

Peter,

Yesterday evening on The News Hour with Jim Lehrer on PBS they had an in-depth piece on the new budget. The numbers I quoted came from the two non-partisan analysts who were interviewed at length and who agreed that earmarks had declined, but disagreed about how much. I believe that I remembered them correctly. The analysts were, however, talking about the overall budget and not the DOE budget. I agree that earmarks always reduce the amounts available for other projects and I certainly wasn't trying to say that it isn't a problem, only that the number and total dollar amount declined over the prior overall federal budget. I still think that's the case, but since I'm relying on others on this point, I could certainly be wrong.

38. **Michael Bacon**
December 21, 2007

Peter,

Here is a link to the transcript of the program.

http://www.pbs.org/newshour/bb/politics/july-dec07/earmarks_12-20.html

My memory was slightly off on the percentages. According to the two analysts, the decline was either 25% or 40%. It isn't clear what accounts for the difference, but it may be whether you're looking at the total dollar amount of the number or earmarks.

39. **wb**
December 21, 2007

The ILC detector collaboration just sent around the following message:

"Chip Zukoski, UIUC's Vice Chancellor for Research, just got very good news from April Burke, of Lewis-Burke Associates. (They do federal relations consulting for UIUC and are active in HEP advocacy.) As I understand it, Senator Durbin now appreciates the enormity of the spending bill's negative impact and has instructed Fermilab to cancel the layoffs that were being discussed, Senator Durbin will work to restore (some of the?) funding to HEP once Congress begins its next session after

the New Year."
… "Senator Durbin has responded to concerns regarding the cuts to High Energy Physics funding by contacting Secretary Bodman. Senator Durbin made a commitment to work to prevent layoffs at Fermilab. At this time there are no details as to whether complete funding will be restored, including the project funding for NoVA and ILC. However, efforts are underway by Senator Durbin look for a solution to funding shortfalls at Fermilab and for HEP."

So let's hold our breath and watch for the next episode.

40. **wb**
December 21, 2007

Further news on this front:

DURBIN, OBAMA, BIGGERT CALL ON BUSH ADMINISTRATION TO INCREASE FUNDING AT FERMILAB

In light of recent funding cuts, Illinois members will meet to discuss strategy

WASHINGTON, D.C. – U.S. Senators Dick Durbin (D-IL) and Barack Obama (D-IL) and Representative Judy Biggert (R-IL) today sent the following letter to Jim Nussle, Director of the Office of Management and Budget (OMB), calling on him to increase next year's funding for the High Energy Physics (HEP) program, which supports research at Fermilab in Illinois, and at several other laboratories and universities across the nation that are doing vital, cutting edge research.

Durbin, Obama, and Biggert are in discussions with Congressional appropriations and authorization committees and the Department of Energy to address the current funding situation and avoid potential layoffs during fiscal year 2008. They also plan to call for an Illinois delegation meeting in January with representatives from Illinois labs and organizations to discuss a strategy to avoid potential job loss at Fermilab. The spending bill, approved by Congress this week, provided the HEP program with $88 million less than was requested. This challenges Fermilab's ability to remain one of the world's preeminent research facilities after it has achieved outstanding success in research on neutrinos, the high energy frontier, and particle astrophysics.

Adequate funding for the labs is critical to ensure that our country maintains its technological edge and that we continue to add to our high-tech manufacturing base. Fermilab is the nation's premier high-energy physics laboratory. The laboratory leads U.S. research into the fundamental nature of matter and energy, and in 2007, Fermilab's researchers and facilities achieved results judged by the American Institute of Physics as among the Ten Top Physics Stories from around the world.

[text of the letter is below]

Dear Director Nussle:

We are writing to you concerning a matter of critical importance to our country, to science in America, and to our global competitiveness. As you continue to develop the President's Budget for Fiscal Year 2009, we respectfully request that you increase funding for the High Energy Physics (HEP) program in the Office of Science at the Department of Energy.

As you know, the budget approved this week by Congress dealt a severe blow to HEP, which received $88 million less than requested. This budget rejected funding for the NOvA neutrino experiment at Fermilab, and drastically cut funding for research and development on the International Linear Collider. These cuts could cripple Fermilab's ability to remain one of the world's preeminent research facilities. And this is at a time when Fermilab has achieved outstanding success, with significant results in each of its central areas of research: neutrinos, the high energy frontier, and particle astrophysics.

The facilities at Fermilab are essential for the basic scientific research that nurtures technological and scientific advances, and that fuels American innovation. Fermilab is one of a handful of our nation's premier training sites for scientists, and a centerpiece of the system of DOE National Laboratories. Disruptive funding shortfalls have ripple effects throughout the American scientific community, displacing today's scientists and discouraging tomorrow's. We must work together to restore funding in basic physics research to maintain America's role as the innovator in technology, to retain our leading scientific institutions and their skilled workforces, and to provide opportunities for future scientists.

While we recognize the formidable challenges you face regarding the demands on the federal budget, we respectfully encourage you to increase the funding request for the Office of Science, particularly for the HEP program, in the President's FY2009 Budget.

Sincerely,
Barack Obama
Richard J. Durbin
Judy Biggert

41. **Peter Woit**
December 21, 2007

Thanks wb,

I had just seen the press release and was adding a link to it. Looks to me like Durbin and Biggert have realized they screwed up and may lose votes from their constituents over this. Obama presumably is planning on no longer being a senator representing the Fermilab area after the next election.

The fact that the press release mentions neither an amount of increase nor which base seems rather peculiar.

The ILC detector collaboration message is also unclear. That Durbin "has instructed Fermilab to cancel the layoffs" makes no sense since he has no power to do this. Unless he plans to pay 200 salaries out of his pocket, he has to first find a way to get new funding for Fermilab from somewhere, or find a way for the DOE to get around the language in the bill he just signed.

42. **Peter Woit**
December 21, 2007

By the way all US readers who would like to see something done about this, in case this isn't obvious, now (or after you finish your holiday shopping…) might be a really good time to write to your congressional representatives. Obama might also be someone to write to even if you don't live in Illinois, since he's looking for your vote too.

43. **Peter Woit**
December 21, 2007

Michael,

Here are some numbers from the AAAS about earmarking in R and D spending. Looks like it had gotten really bad in FY2006, was eliminated in FY 2007, is now back, at pre FY2006 levels.

http://www.aaas.org/spp/rd/earm08cf1.pdf

The rest of the AAAS budget analysis is here

http://www.aaas.org/spp/rd/upd1207.htm

It describes the decisions about cuts as being made "in a frenzy of weekend work" last weekend, with the results only known on Monday morning, passed into law 72 hours later. Not exactly a carefully deliberated process…

44. **Michael Bacon**
December 21, 2007

"Not exactly a carefully deliberated process…"

What do they say? You don't want to watch either sausage and law being made.

I hope the general trend will be for lower levels of earmarks for the next several years — they really climbed over the past decade or so.

45. **wb**
December 21, 2007

Peter,

What is disturbing and unfortunately not surprising is how unsymapathetic the Illinois delegation was before Director Oddone put a number on the impact in term politicians understand 200 hundred layoffs + ~10% salary reductions in a Republican district that the Democrats have a chance of taking with Fermilab the largest (or at least one of the largest employers). But once a voter impact is revealed the response was swift.

Had the cut to the HEP budget been much more diffuse would we have gotten the same response? Anyone for a little cynicism here?

Let's wait to see if "concern" can be transformed into cash.

46. **Coin**
December 22, 2007

looks like Obama couldn't be bothered to put it up

As of right now I'm finding it on his Senate site as the third entry on his press releases list, and a link to a Chicago Sun-Times article on the subject is currently the top entry in his "News" section, both clearly visible from the site's front page even if they're not the very most prominent thing on the page.

(That is, of course, just his Senate site. He does *not* mention anything on his Presidential campaign site that I can find, so perhaps one could levy the criticism that he's not using the full extent of his ability to public awareness here.)

47. **Roger**
December 22, 2007

Slightly off topic.

If any Brits (including expats) wish to protest the UK governments recent cuts and urge a restoration of the lost money then there is an online 10 Downing Street petition they can sign.

http://petitions.pm.gov.uk/Physics-Funding/

So far its attracted more than 8000 signatures.

48. **J.F. Moore**
December 22, 2007

@wb:*Had the cut to the HEP budget been much more diffuse would we have gotten the same response? Anyone for a little cynicism here?*

The cynicism is warranted, and experimentally verified, as such cuts frequently occur across the rest of DOE, and generally the complaining stays within the community. The funding trend with BES at least is to concentrate dollars in large, visible user facilities because they find these easier to pitch to DOE, congress, and review committees consisting mainly of university scholars. The money for facilities comes largely by defunding smaller programs. The endpoint of this is that the BES culture will become much more like that of HEP, with large, facility-driven science and a diaspora of participants.

On a different subject I'll note that neither Durbin or Biggert are in remote danger of losing their seat next year. Sometimes politicians just won't push for something if they don't know how to make the argument, or that they need to step up and do it. That normally would have been Hastert's job, and they just got blindsided. Hopefully it can be fixed with a supplemental.

49. **Alexey Petrov**
December 22, 2007

Peter,

I can't believe you said that it got little press or mention in the blogs! I think the situation is quite the opposite. Even NY Times got the article about that…

BTW, we were told during DOE site visit last week that that the situation at DOE is so bad that they are even considering shutting down either BaBar or Tevatron next year… hope that does not happen…

50. **Peter Woit**
December 22, 2007

Alexey,

Sorry, I forgot the posting on your blog. The NY Times article came out today, quite a while after I wrote what you quote. Until that story, I know of nothing at all in the national press, just local Ilinois stories.

Was the DOE site visit after they had the bad news about the FY 2008 budget?

I thought a plan to shut down BaBar after the current run had been made long ago, is that not true? Shutting down the Tevatron next year would be pretty crazy. Between that, the situation at SLAC, and the congressional language shutting down the ILC, Project X and NOvA, that would pretty much be the end of experimental HEP in the US.

51. **Alexey Petrov**
December 22, 2007

Peter,

> "Was the DOE site visit after they had the bad news about the FY 2008 budget?"

No, but they anticipated the results — and told us that the bill will likely be passed by the Congress and signed by the President (it appears that they could judge the behaviour of the Congress on the short-time-scale reasonably well). So they planned 2% cuts across the board for most University groups. We were told that LHC-related programs would be less affected (which is probably why we only got a 2% cut). It is interesting that at this point the Continouous Resolution would be a much better path for the US HEP program.

Also, on Friday, there was a Science Friday on NPR where at one point they did talk about funding cuts to US science programs, but did not mention the cuts in the Omnibus bill in particular. Actually, it was a very dissappointing SF program — it sounded to me that the panel had no idea what they were talking about.

52. **Constantine Tynyansky**
December 26, 2007

The theory of superstrings does not require experiments!

53. **Zathras**
December 26, 2007

I have some contacts in Congressional science staff, and I can tell you that the perception is out there that particle physics has the least benefit to society, and this perception is unquestionably why particle is getting the biggest part of the ax.

So the question is how can this perception be countered? Arguments about the benefits of experiments done long ago, or about the importance of science in general were tried when the SSC was axed, and they did not work then. TPTB want to know what particle physics has done recently. So is there anything the particle physicists have done in the last, say, 20 years to which society can claim a concrete benefit? I ask this not to have a general debate on the worth of particle physics, but to see what kind of coherent argument can be made in a letter to Congressmen.

54. **Peter Woit**
December 26, 2007

Zathras,

The argument that all of a sudden the US Congress has decided that particle physics needs to be axed since it is not of benefit to society doesn't explain "Why Now?". The general arguments about whether the US should or should not be spending 2-3 hundredths of a percent of the federal budget on fundamental research of this kind are exactly the same as they were 20 years ago, when this number was significantly larger. A huge effort has been made over the last few years to sell the importance of particle physics to the public (see the materials available at the SLAC, FNAL, ILC, symmetry magazine, US-LHC, etc. web-sites).

Last year, the White House put out a proposed budget making physical science research (including HEP research) one of the government's highest priorities, and the Congress debated this and signed off on it. Then, with no debate, and, to this day, not the slightest explanation of what happened, a huge budget cut for HEP was put in the budget at the absolute last moment. Maybe it was because of generalized private hostility to HEP as not useful, maybe it was because the one or two congressional staffers who decided this don't like HEP, or maybe it was because they didn't know what they were doing. Maybe it is because the way things work, the only people responsible for making sure Fermilab gets funded are three members of the Illinois Congressional delegation, and one is incompetent, one was only paying attention to earmarks, and the other was busy campaigning for President.

The other program that got whacked in the budget was ITER, and it's hard to think of anything of potentially more practical importance than that. So, while the effort to convince people of the value of continuing to invest a small amount in HEP research is an important one to continue, I think hand-wringing about how HEP is being cut because it is not useful and attempts to put more effort into that argument may not be what is needed. What is needed is to figure out exactly what happened here and make sure it doesn't happen again next year.

55. **Zathras**
December 26, 2007

PW,

1) As for "why now," there is a confluence of factors that has led to an attempt to cut a lot of things across the board. Chief among the factors is the general belief that we are about to head into an economic downturn, so things should be cut sooner rather than later. Many projects have been cut outside of the sciences, and the science cuts are just part of the plan.

2) Particle physics is the choice among the sciences because a lack of prestige, not just for particle physics, but for physics in general. People can look at biology and chemistry research and perceive concrete good for society. People look at physics and find it wanting.

3) ITER was also targeted because of the delays and cost overruns it has had which are both enormous, even by the standards of big science. The perception is that the project had high goals but has no chance of meeting them.

4) Also, the pork barrel spending issue goes back to the SSC. The SSC in the early and mid 90s was repeatedly cited as the biggest pork barrel project ever. Memories of that have made any big particle physics project appear to many to be pork barrel spending.

5) The efforts to publicize particle physics have done very little to do so. However, even if it did, it would not address the problem that there are (I believe) no full time lobbyists for big science. If you want to keep the funding, that's what particle physics need, regardless of any distaste for that sort of politicking.

56. **Peter Woit**
December 26, 2007

Zathras,

I'm still not convinced by your argument that the problem is that people don't want to fund HEP research because it is not useful, just pork. One of the problems with the DOE budget this year seems to have been the return of an emphasis on pork (e.g. earmarks) this year after last year, when earmarks were eliminated. The only thing that seems to have gotten the Illinois congressional delegation interested in the problems of Fermilab was when they realized that they had just voted for a budget that eliminated lots of jobs in their district. The problem this year seems to have been a Congress mainly interested in pork, but ill-informed about the size of this particular piece of pork.

I was under the impression that there are lobbyists working in Washington lobbying for science funding, funded by the APS, AAAS, and even Fermilab. At the all-hands meeting, there was a hostile question to Oddone about the people Fermilab has hired to represent them in Washington. I don't know who those people are, and whether they can engage in lobbying, maybe not if they're funded by taxpayer money. Perhaps this was all due to not having the right kind of lobbyist, or some lobbyist not doing their job. Maybe instead of getting Jim Simons to fund an accelerator run, he could fund hiring a high-priced lobbying firm, and political donations to the relevant people. I fear that in the present political environment, that sounds to me like a more promising plan than making high-minded arguments about benefits to society…

57. **Zathras**
December 26, 2007

I'm not sure what the applicable lobbying funding rules would be.

It is interesting to compare the relative advantage biology and chemistry have in regard to lobbying. They have big Pharma which helps them do the lobbying for them. Physics has nothing like such a developed institutional tie to industry. Individual physics groups may have such ties, but such ties are too inconsequential to generate the needed lobbying. For a long time many physicists have eschewed such ties (MGM's comments about "squalid state physics" are but one early example), and now the physics community is reaping the damage. Fifty years ago such ties were present and were strong. They are now too weak, and this is shown in the lack of an effective lobby.

58. **wb**
December 26, 2007

Zathras,

I tend to agree with Peter for a couple of reasons. 1) HEP was not targeted until the last minute and then cuts were precisely targeted at Fermilab rather than at the field as a whole. 2) The Fermilab had an advantage of no congressional staffers "minding the store." 3) There was strong industrialist backing for a large physical sciences boost (including HEP). 4) Antipathy to HEP is generally ascribed to one powerful member from IN. 5) Rather than only looking to how well our HEP community projects benefits to society, one also needs to ask did DOE/SC overlook potentially troublesome members through either neglect or ignorance.

It is hard to see how one can call SSC pork given that it was thoroughly peer-reviewed in the HEP community and especially in retrospect a far better option than the LHC.

ITER was a strong technical preference of the US fusion community and was vetted through FESAC. As for zeroing ITER, that was very clearly a slap at the White House which had publicly signed on to ITER support.

I am not saying that we could not make a better case for HEP and other physical sciences. I am saying that the damage to HEP seems more related to petty politics and the fact that a big cut could be made while affecting primarily one congressional district.

59. **SLACer**
January 11, 2008

As an engineer at one of those S.F. Bay Area physics labs (the one near Palo Alto), I clearly remember the big boost in financial support that various federal pollliticians touted was coming our way. Speeches by the Secretary of Engergy, University big-wigs; a dog-and-pony show it was!

As a Democrat, I firmly hold the Democratic Congress largely responsible for this funding disaster and I will NOT be granting my Democratic Congressional members from California the honor of my vote. They were too busy funding their earmarks (PORK: oink-oink) and the war(s) to to the right thing. As a result, important projects are being terminated early, postponed, or not getting off of the ground at all. The Babar experiment, strongly supported by the international community, is being terminated seven months early. What message does that send to the international physics community about U.S. support and integrity as a research partner? I will try to bring this issue to light in the coming Presidential primaries and I hope others will do likewise. Let's see if any of the canditates can give an honest and meaningful reply instead of the usual sound-bite.

Unfortunately, most of the leading Presidential wanna-bees are in Congress and therefor part of the problem. What's a voter to do?

To quote the director of one of the S.F. Bay Area labs:

"It pains me greatly that at a time when particle physics needs to be ever more international, the political process in the U.S. has resulted in real damage to the relationships with our international partners."

Complete text at:

http://today.slac.stanford.edu/feature/2008/All-Hands-010708.asp

Our U.S. Federal government has its priorities so messed up that it is just mind-blowing. I just wish enough voters could see this do something about it.

Money, Money, Money

December 26, 2007
Categories: Uncategorized

Theoretical physics and mathematics are much cheaper to fund than experimental particle physics. I don't know yet what the implications of the FY 2008 budget are for these fields, but it appears that they are not facing large cuts like Fermilab. In other funding-related news:

DARPA, a Defense Department division responsible for funding research that might lead to new technology with military applications, is soliciting applications for grants to fund pure mathematical research. In the past they have funded work on the geometric Langlands program, now they have put out a list of 23 Mathematical Challenges, illustrating what they would like to fund. These include some conventional Clay Millennium Prize problems like the Riemann Hypothesis and Hodge Conjecture, and some less conventional ones like

> **Biological Quantum Field Theory**
> Quantum and statistical methods have had great success modeling virus evolution. Can such techniques be used to model more complex systems such as bacteria? Can these techniques be used to control pathogen evolution?

There's even one I find extremely tempting:

> **Geometric Langlands and Quantum Physics**
> How does the Langlands program, which originated in number theory and representation theory, explain the fundamental symmetries of physics? And vice versa?

Benjamin Mann, the program officer, explains the rationale here.

On the question of grants, there are some interesting comments by Tom Banks in the comment section of this posting at Cosmic Variance. The posting quotes Harvard president Faust as warning lesser schools that they should get out of the business of scientific research, since Harvard is going to be vigorously and successfully competing for increasingly scarce government funding for such research. Banks describes how when he and others were trying to build up the string theory group at Rutgers

> …we never got the kind of government funding that the elite institutions have (I'm counting dollars per person). This was at a time when certain elite institutions were at a low ebb and were getting scandalously large amounts per person in return for mediocre research. And of course, in the end, two of our most successful researchers got stolen away by elite institutions.

I guess he's referring to Seiberg, who went to the IAS, and Shenker, who went to Stanford. I don't know which are the "certain elite institutions" that were doing "mediocre research" that he is referring to.

The Rutgers string theory group was originally built up when the university spent large amounts of money to bring in several prominent string theorists. One version of this story that I heard was that a Rutgers official called up Dan Friedan one day in his office, and asked him what it would take to get him and several other well-known string theorists to move there. Friedan had no interest in going to Rutgers, so made up what he considered an absurd list of demands (huge salaries, lots of postdocs, new building, little or no teaching, etc., etc…). The official thanked him and then hung up, with Friedan convinced he'd never hear any more about this. A couple hours later though, the phone rang again. It was the same official, telling Friedan that they would be more than happy to meet all his demands.

Another institution that I hear is trying to compete with the elite by starting up a new, well-funded institute for research in math and physics is Stony Brook. The money is coming from Jim Simons, as part of a donation announced last year. Some other information about donations from Simons to other institutions is available at the Simons Foundation website. This foundation is largely devoted to funding research on autism, but also describes donations to the Math for America program to recruit math teachers, as well as to Brookhaven, Stony Brook, the IAS, IHES and MSRI. Simons has been funding summer workshops at Stony Brook for several years that are largely devoted to string theory. The math department has an NSF-funded RTG program in geometry and physics, and the web-site there includes links to write-ups of some of the expository talks that are part of the program.

Comments

1. **David Nataf**
   January 25, 2008

   Meanwhile, British science continues to take a beating. I just received an email from the Canadian Astronomical Society (CASCA) letting us know that Britain is dropping out of the Gemini telescope.

   Details of the announcement can be found here:
   http://www.scitech.ac.uk/PMC/PRel/STFC/Gemini-Update.aspx

This Week's Hype

December 27, 2007
Categories: This Week's Hype

Over the past year or so, as public awareness has grown that string theory is a failed idea about unification due to its inherent untestability, I've been surprised by the way in which many in the string theory community have chosen to deal with this. Instead of just honestly admitting what the problems are and describing the sensible reasons to keep working on string theory despite them, some have decided instead that the thing to do is to go to the press with misleading and dishonest claims that string theory really is testable.

The endless examples of this in New Scientist are probably best ignored, but this week's example is being promoted in the highly respectable journal Nature. It's all based on this letter to Nature from a group of condensed matter physicists at Lancaster University, now prominently highlighted as an "Advance Online Publication" at the Nature Physics web-site. The authors describe an experiment in which they manipulate the boundary between phases in a superfluid, showing that when such boundaries come together, one gets left-over in the remaining phase the well-known topological defects that one would expect.

The Nature letter itself makes rather ridiculous claims that this kind of otherwise unremarkable phenomenon is somehow closely related to brane cosmology and string theory. The authors do note that

> The precise correspondence between the ^{3}He phase interface and a cosmological brane is still a matter of discussion, the closest correspondence probably being to the D-brane. For the present purposes we may note that the correspondences are as much topological as specific.

While the letter makes no explicit claims about "testing" string theory, the press release issued by Lancaster is the usual sort of dishonest nonsense:

> Low-temperature physicists at Lancaster University may have found a laboratory test of the 'untestable' string theory.
>
> The test – which uses two distinct phases of liquid helium – is reported online this week in Nature Physics (published 23 December). Their paper will also be published as the cover article in the paper edition of Nature Physics in January.
>
> String theory is a multidimensional theory based on vibrating strings, as opposed to the point particles described in the Standard Model.
>
> Within string theory, a brane is a large surface embedded in higher dimensional space — our Universe could occupy such a brane.
>
> A collision between a brane and an antibrane can leave behind topological defects, including perhaps the Big Bang itself. But however elegant this theory, it makes no falsifiable predictions, or at least none using current technology.
>
> Richard Haley and the ULT Group have taken a lateral step to address this barrier….

Similar wording is used in a press release put out by Nature about this.

Nature has a relatively reasonable news story by Geoff Brumfiel about this, but also an article by string theorist Cliff Burgess hyping string cosmology ("The subject of string cosmology is a hot one these days, with theoretical advances in understanding string dynamics riffing with recent precise observations of the cosmic microwave background") and the relevance of the Lancaster group's work to it. He mostly sticks to hype in its pure form, just devoting one paragraph to the actual scientific result. There he ends up acknowledging that this actually has nothing to do with string theory in the following rather ludicrous way:

> The quality of the details of the comparison between ^{3}He and cosmology is not really the point. Like a tap-dancing snake, what is amazing is not that it is done well, but that it is done at all.

After this he shifts gears to start hyping AdS/CFT, without mentioning that this has nothing to do with the Lancaster group's claims that he is writing about.

The whole point of this kind of exercise is to generate misleading articles in the press that will convince some people that string theory really is testable. This seems to be working well, there's already one entitled "Test tube universe" hints at underlying theory in the Telegraph, which tells the public that:

> A "universe in a test tube" that could be used to assess theories of everything has been created by physicists…
>
> The Holy Grail of physics is to establish an overarching explanation to unite all the particles and forces of the cosmos. But one of the complaints commonly levelled at a leading contender for a "theory of everything", called string theory, is that it is impossible to test.
>
> But now, according to the study in the journal Nature Physics, it may be possible using the universe in a test tube. "It was a serendipitous discovery," says Haley…
>
> For the past three decades it has been known that strings are one member of a bigger class of objects called branes, which exist in higher dimensional space, that could be extended in more than one dimension – from strings of one dimension, to membranes of two dimensions, to those of p dimensions, dubbed p-branes. Moreover string theories and p-branes are facets of one underlying 11-dimensional M theory, which suggests that we live in a brane world: a four-dimensional surface, or brane, in a higher dimensional mixture of space and time.
>
> People and most particles move in the brane, while the higher dimensions provide a framework to unify all forces, from gravity to those that act between atomic particles. While experiments have begun to highlight cracks in the current best theory, called "the standard model", there is evidence that M theory's extra hidden dimensions could be revealed next year when a Geneva atom smasher – the £4.4 billion Large Hadron Collider – begins experiments. But the Lancaster team offers another route to address this impasse.

Update: Wired Science has an article about this entitled A Test for String Theory After All? Or Just PR?, which shows excellent judgment by linking to this posting…

On the scientific front, it's worse than I thought. There a conference in London at the Royal Society next month on Cosmology Meets Condensed Matter, where the head of the Lancaster group will speak, and the idea that "coherent phase boundaries mimic branes" is listed as one of the four justifications for the conference. I guess this emerging new field might best be called "Squalid-State Cosmology".

Update: David Appell has a posting about this, which includes a quote from Witten:

> There is definitely no test of string theory here.

Unlike me, Witten goes on to try and find something positive to say about this.

Update: Physics World has an article about this entitled Cosmic strings in a test tube? In the short article, one of the physicists working on this Richard Haley, is twice described as denying that this is a test of string theory, and Grisha Volovik is "adamant that the work is neither a test of sting theory…". Despite these firm denials, the press release from Lancaster about a test of the "untestable" string theory is still up.

Comments

1. **woit**
   December 27, 2007

   I've just deleted comments by "Elmer Fudd" (and responses to them), a string theory partisan who turns out to only be interested in posting insulting anonymous comments here, admitting he actually doesn't know anything about the issues involved.

   Please, comments insulting people anonymously are never welcome here. If you understand the physics issues involved and want to tell me I'm wrong anonymously in an insulting manner, I suppose I'll allow that. But if you don't understand the issues, best not to do this.

2. **Florian Walchak**
   December 27, 2007

   Peter –

   I haven't seen any comments about Kenneth Lang, the noted solar/astro physicist and astronomer, on your site. I refer you to his recent book Parting the Cosmic Veil, a popular but serious treatment of astronomy and cosmology. He gives a response to the Elmers of the string world in the last chapter (6) of the book. One notable quote is:

   "Now, a vocal minority of cosmologists [string threorists??] has pushed into the domains of myth and religion, at the boundary between what is known and the mystery of creation. Mathematical equations and computer simulations are employed to construct their own epics, using the language of science rather than mythology. They have attempted to show how everthing – mass, energy, space and time – might have originated in the Big Bang that set the Universe in flight, and then moved on to speculate about what happened before the beginning. They have become today's shamans, hiding their magical pronouncements behind equations or even books that almost no one can read or understand. With an amazing arrogance, presumption, hubris and lack of humility, they even claim authority because of it, asserting that they alone can percieve the true beginnings of the Universe. But that does not make their explanations true." [pages 192-193]

   And another: "If we probe further back into time, before the hypothetical inflation that created our personal Universe, the energies become higher and higher, perhaps entering the domain of the tiny, invisible vibrating strings that occupy unseen places and extra dimensions, spring out of nothing and accounting for everuthing…Yet despite a quarter century of investigation, the string experts still do now [sic – I presume a typo for not] know how to test their theory or even how to solve the equations in the extreme conditions at time zero." [page 196]

   And finally: "Many astronomers, who like to observe the real world, are becoming fed up with all the hype, the persistent appeal to the unobservable and unverifiable, created from complex mathematics and esoteric theory that almost no one can understand. As the the American writer Mark Twain quipped: "There is something fascinating about science.One gets such wholesale returns of conjecture out of such a trifiling investment of fact."" [page 196]

   There's more. It's an enjoyable read. I recommend it.

   Florian Walchak

3. **Coin**
   December 27, 2007

   The quality of the details of the comparison between 3He and cosmology is not really the point. Like a tap-dancing snake, what is amazing is not that it is done well, but that it is done at all.

   "String Theory: Like a Tap-Dancing Snake". Now there's a fantastic endorsement.

   So, I'd like to at least *try* to be charitable about this He3 thing, but I cannot access the Nature article so I'm not sure what it is they're saying. Could someone perhaps clarify exactly *how* it is that they think this He3 thing links to string theory? The press release and the Independent article on the subject seem to vaguely be saying that the Lancaster people have shown similarities between helium superfluid and the large-scale structure of the cosmos. Okay, but what does this have to do with String Theory?

   1. Is the idea supposed to be that string theory predicts that helium superfluid and the large-scale cosmos will share structural similarities, so if those similarities bear out it will be a successful prediction for string theory?

   2. Or is the idea that string theory predicts things about the behavior of superfluids, and they can test those predictions by analyzing He3? (If so that sounds dreadfully useful, but why bring in cosmology?)

   3. Or are they seriously saying "string theory can maybe predict things about the structure of the cosmos, helium superfluid looks kind of like the cosmos, so we'll make some predictions about the structure of the cosmos and then instead of testing them against the cosmos we'll compare it to the structure of a helium superfluid"?

4. **Oswald Spengler**
   December 27, 2007

   Peter, there are no legitimate reasons to continue working on string theory, and such work doesn't warrant to be called "research." String theory has turned theoretical HEP departments into a work place where everybody is aware that the work of most/all people there is funded and performed under the false pretense that according to current understanding a *competent* researcher or observer *actually believes*—as opposed to *with intend to mislead conveys the false impression to believe*—that this work is advancing scientific understanding in the field of fundamental physics *in the strict, narrow, literal, original, old fashioned meaning of this term*. People's entire careers are based on this kind of work which *has no genuine scientific merit at all*, and, even though it is taboo to say so, *everybody is, **of course**, keenly and painfully aware of this, as well as of the even more painful contrast to the generation of, say, Feynman and Gell-Mann*.
   Everybody knows that all these people have ever done and all they do is to defile the tradition of generations of honest physicists from Copernicus, Galilei, Kepler and Newton to the fathers of the Standard Model. They are infecting new graduate students with this disease—and they have never known anything else. It creates the nauseating atmosphere of the Middle Ages *where everybody is aware that their time and all their efforts are a joke in comparison to the days when science was still real and actual insight into the nature of the real world was made*.

   It makes you sick in the soul when you know that everything people around you do falls somewhere between dishonesty and incompetence—because that is all they can think of, or all they can get funding for. You can feel nothing but pity and contempt for them. Now you know what it feels like **to watch the sun set on a Civilization.**

5. **Peter Woit**
   December 27, 2007

Coin,

There is no real connection to string theory, and Burgess admits as much. What I don't understand here is why the Nature referee allowed this paper to be published, and Burgess thought it was a good idea to use it to hype string theory.

Oswald,

Please, try and stick to the topic of the posting. I don't think the "everything is equally awful" attitude is helpful. People are working on hundreds of different things under the name "string theory" and some are more worthwhile than others. It's not exactly unusual in science for people to be working on not very good ideas that aren't going anywhere. It is unusual to see this level of dishonest hype being promoted in Nature.

6. **Oswald Spengler**
December 27, 2007

Peter, it's not that I disagree with you there. For me the point is to be evocative. You may think of my post as in-between quotation marks. A soliloquy created specifically for this stage. A polemic addressing a situation so dire that the characteristics of polemics are melting away like butter in a frying pan.

7. **wb**
December 27, 2007

If the Lancaster group were correct and were Nature not just hyping a non-test of string theory, then perhaps why Congress had it right. Why would we need a $10 – 20 B ILC when a small condensed matter lab would serve just as well to reveal the unifying theory of the universe.

8. **Tim May**
December 28, 2007

With apologies to Dr. Hook and the Medicine Show and "On the Cover of the Rolling Stone":

Well we are string theory slingers, we've got golden fingers
And we're loved everywhere we rant
(but testing is something we can't)
We write about beauty and we write about truth
At ten million dollars a grant
We got all kind of brains to figure out the branes
But the thrill we've never known
Is the thrill that'll get you when you get your paper
On the cover of the New Scientist

{Refrain}
New Scientist
Wanna see my theory on the cover
New Scientist
Wanna buy five copies for my mother
New Scientist
Wanna see my smilin' face
On the cover of the New Scientist

9. **Bee**
December 28, 2007

Hi Peter:

"but also an article by string theorist Cliff Burgess hyping string cosmology "

This actually caused me to read the article, it doesn't sound like Cliff at all. I don't think you are being fair on him, I find the article very reasonable and balanced. He is careful with his wording (*"requires knowledge of the laws of physics at very short distances that string theory claims to have"*), and he expresses his scepticism pretty clearly (*"The underlying hope is that this 3He dynamics, which also produces a stringy residue, might capture something universal about a cosmic brane cataclysm. Although this hope may be faint […] and the dynamics may differ […], the comparison may nonetheless be instructive."*) And he even points out that this kind of 'string' effect doesn't actually say something about THE string theory as a TOE *"Even if future experiments should prove that nature spurns string theory as the description of quantum gravity…"*. Yes he is optimistic and finds the developments interesting *"the great irony may be that it yet lives on as part of the core curriculum taught to future generations of nuclear and condensed-matter physicists."*, but this is very far from being *"hype in its pure form"* as you call it. (No idea though how the AdS/CFT paragraph belongs there.)

Best,

B.

10. **Peter Woit**
December 28, 2007

Bee,

I think the Burgess piece carefully avoids making completely untrue statements, but the whole thing is fundamentally a piece of dishonesty. This experiment has as much to do with string cosmology as my jumping in a swimming pool has to do with how heavy quarks lose energy in a heavy-ion collision. Burgess is aware of this, and if you read carefully what he says, that's clear. The sentence that I quoted about string cosmology is pure hype, and the tacking on of AdS/CFT hype without mentioning that it has nothing to do with this is intentionally misleading.

Burgess's contact information is on the Nature press release describing this as a "laboratory test of the "untestable" string theory".

Sorry, but I think this kind of thing is really disgraceful, and not honest, no matter how carefully worded the article is.

11. **Bee**
December 28, 2007

Hi Peter:

"Burgess is aware of this, and if you read carefully what he says, that's clear" I think this was the intention of his article? He makes clear what it is and what it isn't about, I don't see how you find it 'clear' while at the same time calling it 'a piece of dishonesty' because you would probably have been more blunt. Is your criticism that one has to read it carefully because the headline doesn't explicitly state it's not a test of quantum gravity? The only "hype" I could see in your quotation is the word "hot" (I am not actually sure what 'riffing' means I have to admit, something like 'goes along with'?). I fail to see what's disgraceful about him finding a field interesting that you apparently don't find interesting, and calling something an 'irony' doesn't actually sound overly enthusiastic either. I said above I agree that the AdS/CFT reference doesn't quite belong there, but whether or not he knew his name would appear with something about a lab test of ST his article is clearly far off other 'hypes' you've pointed to, and I find calling it 'hype in its pure form' an exaggeration.

Best,

B.

12. **Peter Woit**
December 28, 2007

Bee,

The use of the musical term "riffing with" to describe the relationship between "theoretical advances in understanding string dynamics" and "precise observations of the cosmic microwave background" is just pure hype and obfuscation. The actual relationship is that Burgess and others would like to use the first to predict the second but have so far failed to do so.

It's not a question of whether the claims of this paper are "interesting", it's a question of whether they are nonsense. Looking at boundaries between phases in a superfluid is not a test of brane cosmology models and Burgess knows this. If the editor of Nature calls you up, and says "we are publishing a letter claiming to test string cosmology models by looking at phase boundaries in a superfluid, will you write an article explaining the significance of this?", what would you do?

1. Tell them the truth, that this is nonsense and they shouldn't be publishing it.

2. Agree to write the article, spend most of it promoting string theory, only very indirectly indicating that the claims in the letter aren't true.

Don't you think one of these is honest, and one isn't?

The headline of his article "A surprising and fascinating interplay may be emerging between string theory and condensed-matter physics" which is outrageous hype about this experimental result, and his name is on a press release about how the experiment shows testability of string theory in the lab. Maybe this is none of his doing. It will be interesting to see if Nature publishes a letter to the editor from him saying he has a problem with this.

13. **Peter Shor**
December 28, 2007

I assume that Cliff Burgess' article was solicited. Nature editors often solicit commentary on especially exciting research results, and even ignoring all the dubious connections to strings, this sounds like a fairly exciting and ground-breaking experiment; correct me if I'm wrong, but I assume this is the first demonstration of topological defects introduced by phase boundaries.

Unfortunately, it seemed as if the Nature editor made the mistake of assuming that there was a much more solid connection to string theory than existed, and furthermore asked the wrong string theorist for commentary. Even if he did then realize the article was mainly PR, he may not have had the guts to reject a solicited submission.

14. **Peter Woit**
December 28, 2007

Peter Shor,

Independent of the claims about string cosmology, I don't see any evidence that this is a ground-breaking experiment in terms of what it demonstrates about either topological defects or phase boundaries in a superfluid. The authors claim nothing like that in their abstract, and I suspect that if they did have such a result, they wouldn't be going on about cosmology. If there's an expert on this subject, I'd be interested in hearing what they have to say about the significance of this other than as a "test of string theory".

15. **Gabe**
December 28, 2007

I really don't have any knowledge of this, but: What exactly are they trying to say about liquid helium phases and extra dimensions?

16. **Bee**
December 28, 2007

Hi Peter:

If the editor of Nature calls you up, and says "we are publishing a letter claiming to test string cosmology models by looking at phase boundaries in a superfluid, will you write an article explaining the significance of this?", what would you do?

I'd assume he dialed the wrong number.

1. Tell them the truth, that this is nonsense and they shouldn't be publishing it.

2. Agree to write the article, spend most of it promoting string theory, only very indirectly indicating that the claims in the letter aren't true.

Don't you think one of these is honest, and one isn't?

Well, what I'd try to do if I find the field interesting is explaining why I generally think it is interesting, while trying to make clear that the claims in the paper are somewhat far fetched. That's what Cliff did, at least the way I read it. It doesn't seem like a place to tell the editor what to publish and what not, it's not a referee report? True, one could interpret the article as he just used the opportunity to talk about what he knows and likes, true, maybe he should (could?) have insisted on toning it down, but I find it far from being an 'outrageous hype'.

Best,

B.

17. **Peter Woit**
December 28, 2007

Bee,

Again, “interesting” isn’t the point. The claims about testing string cosmology in this article are nonsense, and Burgess should have just said so, whether he’s the referee or not, instead of going on about how it “stands out like a sequoia in the Sahara” and is “Like a tap-dancing snake”.

18. **Peter Woit**
December 28, 2007

Or, to make the point more clearly using Burgess’s analogy:

Finding a sequoia in the Sahara would be interesting. But if the fact of the matter is that there is no sequoia at all, the only interesting thing is why a reputable science journal is publishing a bogus claim that there is one.

19. **Peter Woit**
December 28, 2007

Gabe,

There is no relation at all between liquid helium phases and higher dimensions. The authors are claiming that the two dimensional interface between two phases is in some way vaguely analogous to the idea that our three-dimensional space is a brane in higher dimensions. There is nothing here other than an extremely vague analogy between two very different things.

20. **Stugots**
December 28, 2007

Peter,
You really need to get a life. Who cares if they try to connect their work to string theory? To me, this is not string theory hype, but rather some condensed matter physicists trying to sex up their own work by mentioning it in the same breath as string theory. So much for your thesis that string theory is losing favor in the scientific community…

21. **Peter Woit**
December 28, 2007

“Stugots” (my you string theorists have classy nicknames…)

Sure, you’re right that what is going on is condensed matter physicists trying to sex up their work by connecting it to string cosmology (I think the cosmology is the sexy part though, as many string theorists have noticed, they’ve given up on particle physics and moved into the “hot” area of cosmology).

What’s pretty funny though is that Burgess is clearly trying to sex up his work in string cosmology by connecting it to rather obscure behavior of phase interfaces in low temperature condensed matter physics.

22. **Tim May**
December 28, 2007

Mathematical structures, models, and theories are not necessarily the way the world actually is put together, the physics of it. There’s the “unreasonable effectiveness of mathematics” issue, which makes the point that often things found in mathematics are found somewhere in the natural word, whether in seashells or curved space-time or (yet to be determined) Calabi-Yau manifolds. (And some of the best stuff to come out of the last half-century of work in HEP is the new mathematics, a point many of you make. But this doesn’t mean, of course, that the real world is actually like some of this math. Experiments will provide answers, someday.)

So it’s interesting that certain results in topological defects may show up in superfluid work. (I’m far from from an expert on this, but isn’t this close to a lot of the “anyon” work that Freeman and others are working on, a la fractional quantum Hall effects?) So maybe there’s a mapping from math to these states….wouldn’t be the first such mapping, that’s for sure! But taking something from condensed matter physics and saying it provides support for M-theory seems way too big a speculative reach.

And _maybe_ there’s also a mapping between the math and the actual structure of the universe a la string theory and branes.

But finding the one mapping doesn’t imply the other mapping. (No functor between these mappings is guaranteed, loosely speaking.)

What matters is taking some of the “unreasonable effectiveness” of the math and predicting some particular particle not yet seen, or something not yet seen in the larger universe. As with the Eightfold Way, which said that if the real world actually matched the group theory analysis, there ought to be a previously-undiscovered particle with these properties…and there was. Pretty impressive stuff.

But if the “predictions” stay at the metaphorical level, whether about how the universe is a foam, or a turbulent sea, or a test tube filled with liquid helium, then not much is gained. (The rubber sheet view of curved spacetime is an intuitive, easily-explained mode of gravity, but had it just stayed at the level of a metaphor and not made certain numerical predictions which could be checked, not much would’ve been gained. String theory is still seemingly at the metaphor level–bundles of vibrating strings, with way too many adjustable parameters, and with even more exotic stuff, like colliding brane-worlds, with absolutely no predictive behavior, at least not at energies obtainable by Earth-level economies.)

I hope this changes, either by concrete predictions testable within the next few decades, or with new theories that are so testable. This whole situation has a lot do to with why HEP funding is getting cut, I think.

Partly it’s the “desert” in energy, and the logarithmic increases in energy and budget to find the next range of interesting experiments. It didn’t cost much in real dollars to find the particles that past theories predicted. A detailed analysis of budgets in real dollars for the Bevatron, AGS, SLAC, Fermilab, and CERN would be interesting to see, but we can get a Feynman-like estimate of the real costs just by looking at the physical _sizes_ and _staffs_ of these labs….by any economic measure, the Bevatron was very inexpensive, SLAC was a whole lot smaller in footprint, building costs, and staffing costs, than the LHC will be. If the LHC doesn’t find something crucial, say goodbye to the generations after that for a long time.

And it’s also what a couple of astute posters here have mentioned: by now there is no longer any hope or expectation that any new kind of weapons-suitable or industry-suitable physics will come out of HEP.

So the main theme of Peter's blog, "Not Even Wrong," is also related in a deep way to the funding situation in HEP. And the hype of the PR campaigns for string theory is clearly part of this whole mess.

But, hey, I'm working on a theory that our universe is just one bubble in a giant bathtub of soap bubbles. I think I've found evidence for this in some experiments I'm doing in my kitchen sink….Now if I can just get this on the cover of the New Scientist.

–Tim

23. **milkshake**
December 29, 2007

When I came to US in early 90s I joined a tiny startup biotech technology company. Our finances were precarious (we stood 3 months from folding at one point) and I understood the need for our research boss impressing the investors. There was a steady stream of press releases and gee-whiz publications (typically communications without any detailed experimental supplementary) coming out of our little company – and it bothered me great deal to see how oversold everything was: Half-baked or tentative stuff was presented as a "core technology", experiments were carefully staged to produce impressive results to demonstrate the usefulness of our proprietary methodology. This was done all at the time when there was growing doubt within the company whether *any* of it was actually useful.

I am not very proud of taking part in it (at least my papers were not a baloney) and I gather this is a quite usual PR coming out of a struggling startup company. It bothers me to learn this kind of lack of integrity is now common in theoretical physics also

24. **Roger**
December 29, 2007

To those with long memories, has it always been this way ? Renormalising away the effects of the rapid internet communication we have available today, were, for example, the GUT models hyped in a similar way ?

25. **Peter Woit**
December 29, 2007

Roger,

My memories of particle physics coverage in the press go back to the seventies and I don't remember anything at all like this going on 20-30 years ago. GUTs got attention in the media mainly in the context of proton decay experiments that were testing them, not in a context of pure hype.

I don't think think what's going on now has much to do with the internet. Several years ago the internet was there and there was string theory hype, but you didn't see the kind of dishonest claims about "We've found a way to test string theory!!" that you see in recent years. This only started once it started to become obvious that string theory couldn't be tested, and its partisans started feeling they had to deal with this public perception somehow.

26. **Bee**
December 29, 2007

ah, Buffy, what I actually meant to say is don't waste those silver bullets, not everybody is a vampire. Have a nice weekend and a happy New Year,

B.

27. **Tim May**
December 29, 2007

My memories of physics hype go back to the 1960s. I certainly remember a lot of coverage of tachyons, black holes, wormholes, white holes, etc. The difference then was that most of this coverage was in "Science Digest," sort of the "New Scientist" of its day, and even in "Popular Science" (which was more about gadgets and tools, more akin to "Popular Mechanics," than about science per se).

I also remember the wild speculations about "baby universes" shortly after Guth's work, especially around 1982. (I vividly remember a party where this was debated for an hour…I think "Science" had even run a one-page piece on this possible implication of rapid inflation, but I may be misremembering events from 25 years ago.)

And of course there's been a lot of coverage, even hype, about black holes for at least the past 37 years (I remember it escalating dramatically after "Physics Today" did a famous cover story on black holes, circa 1970).

At least back then "Scientific American" could be counted on to do their articles in a certain, even prosaic, way, with little hype. They later went more Madison Avenue, abandoning the old article format, jazzing up the graphics, and putting pure speculation on the covers. I no longer even read it.

As I've already opined, I don't think _some_ of this speculation is harmful. It may even get the kids interested in physics (though whether we need more kids interested in physics right now is itself an interesting question). Where there's real risk is when the hype is used to "sell" a very expensive public project. If that hype is seen as hype, or if the "God Particle" (for example) turns out to be not quite so important in the scheme of things, the backlash may be huge. I suspect this is what killed the SSC (before it even had a chance to find the God Particle).

Finally, I'll give my memories of the "funding climate" back in the late 60s, early 70s. The story of the day was this: "Physicists are driving taxi cabs." That was the scare story, circa 1970. Indeed, some physicists were unemployed in their profession. A lot of staffing-up in physics after Sputnik had filled physics departments with tenured professors…some of these tenured physicists lasted until just the past decade or so. I think Lee Smolin makes this point in his book.

There had also been huge layoffs in the aerospace business, dumping a lot of engineers into the unemployment lines. Some physics grads, too.

Some went into finance (the rising "quants," even before the term was used). Some went into semiconductors.

Hype, Unified Field Theories, Theories of Everything, and God Particles are not something new.

–Tim

28. **Peter Woit**
December 29, 2007

Bee,

Thanks for the advice, and best wishes for the New Year.

Maybe 2008 will see the end of the living dead stalking the land with their “tests of string theory”. Already, New Scientist seems to be ignoring this one, causing it to die a quick death. Hopefully Nature won’t take its place as Zombie-central…

29. **Brett**
December 29, 2007

GUTs were hyped too. I can remember numerous TV programs and popular articles about particles physics that claimed that we would see X particles soon. People were excited about the discovery of the first intermediate vector bosons and expected to see more of them soon (at the SSC, I guess). But that stuff disappeared when the proton decay experiments came back empty handed. The SU(5) GUT was wrong. The X particles weren’t there where people had hoped, and people stopped hyping them (although the failure wasn’t conveyed to the public very well).

However, I think the hype was pretty forgivable. The SU(5) GUT really seemed too good not to be true, and I don’t think people were being intellectually dishonest. (A few years back, a friend told me that it was really unfair that Georgi wouldn’t win the Nobel prize. The GUT theory was wonderful, and it wasn’t his fault that it happened to be wrong!) And when the theory’s predictions were wrong, people accepted it and either stopped working on the GUTs or tried to come up with a reasonable generalization that wasn’t ruled out.

30. **Roger**
December 29, 2007

The whole business of hype and its influence on public perceptions and funding would be a very interesting topic for a science communication Ph.D. .

Is anyone aware of any research on this issue ? It would be useful to know what policy makers and the public make of particle physics these days. We could learn an awful lot of lessons in making our case for continued funding.

31. **nc**
December 30, 2007

Roger, the hype in particle physics is unique: in other sciences you get controversy, not pure unadulterated hype. Journalists don’t believe everything they’re told in other areas, they get counter arguments from other experts and publish those in the article to give some sense of balance. There’s endless research by Professor Brian Martin into controversies in other sciences, but these people don’t take any interest in stringy hype even if they are relatively well qualified to investigate it. (Martin is now Professor of Social Sciences in the School of Social Sciences, Media and Communication at the University of Wollongong, but his PhD was in theoretical physics.)

32. **Oswald Spengler**
December 30, 2007

Brett, but then the plausibility of GUTs evaporated. More and more it became an *old* and *artificial* idea—but the community never adapted to this. In fact, nobody’s argument shows the natural evolution of an intellectually honest and competent observer. Instead, their positions evolve like those of ideologues or those of lawyers. It is then inevitable that one adjusts one’s view of them accordingly.

End-of-year Links

December 30, 2007
Categories: Uncategorized

Every year around this time the Edge web-site posts responses from a large number of scientists to some particular question. This year the question is "What have you changed your mind about? Why?", and the results should be posted tomorrow. John Baez has posted his answer here. He writes that he changed his mind about the question of whether he should be thinking about quantum gravity after he realized "the more work we did, the more I realized I didn't know what questions we should be asking!", and compares the effort to "throwing darts in a darkened room and hoping to hit the bull's-eye." Since changing his mind he has been working on other things, and feels that as a result "I'm making more real progress understanding the universe than I ever did before."

I think I share John's point of view on this in many ways, even though I've never actively worked on a quantum gravity research project. The problem with quantum gravity research has always seemed to me that, since you can't measure quantum gravitational effects, you're in great danger of coming up with lots and lots of "quantum gravities", but unable to ever know which if any of them has anything to do with the real world. This is kind of what has happened with string theory in recent years. One hope has always been that one will find a mathematically uniquely compelling model, but that has yet to happen. To me the best bet has always been that one might understand quantum gravity by unifying it with the standard model, in a compelling way such that the unified theory explains some of the things the standard model leaves unresolved. This hope was also behind much of the original interest in string theory: it wasn't just a quantum theory of gravity, but also a theory of particle physics that could be tested.

Like John, I think we still have a long ways to go towards understanding at a deep enough level how quantum field theories really work, and how the internal symmetries of particle physics and the space-time symmetries of gravity can be unified into a more fundamental structure. Progress towards this goal may even require new mathematics, and this means there are all sorts of things to think about and work on.

One person who hasn't changed his mind about some things is Lubos Motl. According to his latest posting, I'm

> …a typical incompetent, power-thirsty, active moron of the kind who often destroy whole countries if they get a chance to do it.

The arXiv puts out charts each year showing the number of submissions by category, the ones for 2007 are now available here. Commentary about this here and here. The general trend is that quite a few years ago the number of HEP papers leveled off, as just about all of them were posted on the arXiv. The number of math papers is still growing quickly, it is only recently that posting math preprints to the arXiv has become a widespread practice. While mathematicians probably write papers at a slower rate than physicists, there are a lot more mathematicians than particle physicists.

The end of 2007 has brought one undesirable change. Recently I was down in Princeton, and found out that the University Store, traditionally one of the best places in the world to buy math and physics books, has now gone out of the book-selling business, turning it over the Labyrinth Books. The new Labyrinth store has some math and physics books, but far, far fewer. Maybe they just are getting started, and 2008 will bring better news about this.

This past summer Terry Tomboulis posted a preprint claiming to have a proof of confinement. Recently there has been a note posted on the arXiv by Ito and Seiler claiming to have found a problem with his proof, and a response from him claiming there is no problem. I'd love to hear from an expert who has taken the time to follow these arguments carefully and can explain what is going on here.

For a recent article by Arthur Jaffe surveying the history of rigorous studies of quantum field theory, see here.

Heading off late tomorrow for a 9 day vacation in Paris. Blogging will probably be light to non-existent and it will take me longer to get around to deleting comments. Please don't feed the trolls.

Best wishes to all for the new year….

Comments

1. **Bee**
   December 30, 2007

   Hi Peter,

   Thanks for the link. I wish though you had not mentioned that writing of Lubos, it spoiled my day which was so far very nice. I see no point in leaving a comment at his place, so I'll leave it here. Lubos frequently makes statements where he claims to speak for *"everyone in the field"*. Being in the field, and knowing a lot of people in the field, I can assure you if Lubos Motl says *"everyone in the field"* thinks this or that, it is most definitely not the case. Best,

   B.

2. **Hans**
   December 30, 2007

   When you are in Paris, don't forget to buy Lubos recent book
   http://www4.fnac.com/shelf/article.aspx?PRID=2062733

   L'équation Bogdanov
   * Lubos Motl
   * Essai (broché)

   Article en pré-commande, livraison prévue à partir du 17 janvier 2008

3. **Hans**
   December 30, 2007

   Seems one can order it from Amazon france, too:
   http://www.amazon.fr/L%C3%A9quation-Bogdanov-secret-lorigine-lUnivers/dp/2750903866/ref=pd_bbs_sr_1?ie=UTF8&s=books&qid=1199067405&sr=8-1

4. **Shantanu**
   December 30, 2007

Peter, did you see the slides of the Cambridge workshop on inflationary cosmology
at here did you find anything interesting there? Some of the talks did touch upon string theory.

5. **Daniel de França MTd2**
   December 31, 2007

   Hello Peter!

   Happy new year! 🙂

   Unfortunantely links from your website were banned from LM blog. You must read an annoying message to get into his post.

   Best wishes.

6. **Peter Woit**
   December 31, 2007

   Bee,

   Don't worry, I think that people are no more likely to believe Lubos when he says that he speaks for "everyone in the field" than when he claims that I have the ability to "destroy whole countries". Unfortunately he's not the only delusional string theorist around, but few of them are as far gone as he is.

   Hans,

   Thanks for reminding me of that. Maybe I'll even meet up with the Bogdanovs and find out the story behind that book…

   Shantanu,

   Unfortunately I don't really have the time now to look at many of those talks. There appear to be some useful summaries of the history and state of the idea of inflation. As for "string cosmology", I still don't see how it can possibly lead to a testable prediction, and the few talks about this I looked at there don't appear to give any promising ideas about how such a prediction might arise.

7. **Andy T-S**
   December 31, 2007

   Regarding Motl, I have, in my work, observed a researcher who exhibited paranoiac symptoms similar to those of Motl. Ths self-righteous psychopathic "bad apple" wasted enormous amounts of my co-workers' time and energy and, eventually induced clinical depression in two individuals.

   I have read both your and Lee Smolin's books, and have followed your blog for about a year. I have also attended several of Dr. Smolin's talks and audited part of his course on quantum gravity at the Perimeter Institute. My impression is that you both are generally fair and diplomatic critics. (As an example of skilful diplomacy, I observed Dr. Smolin at others' presentations cloak his criticisms by phrasing them as questions that, taken at face value, imply his own, not the presenter's, ignorance. This is quite the opposite of Richard Feynman's supercilious caustic sarcasms, hurled from the front row, that left some presenters in tears.)

   Neither you nor Smolin have ever said that research in string theory is totally useless and should therefore be dropped entirely. Unfortunately, this gets drowned out by irrational attacks from some unthinking researchers who feel threatened, and by misinformation in the media.

   Hans Selye (the discoverer of how stress induces disease) ascribed his success partly to the fact that he worked well with most of his colleagues without, at the same time, squandering his resources trying to befriend those he called "mad dogs".

   For what it's worth, my advice is to ignore Motl's shenanigans in future blogs. I would also remove your link to his site: he doesn't need your help to prove himself a "mad dog".

8. **King Ray**
   December 31, 2007

   Peter, and everyone,

   Happy New Year!

   Peter, I hope you have a great time in Paris!

   I think John Baez is right, it is hard to make progress in quantum gravity. It is difficult to know where to start. A new paradigm is probably needed, as Kuhn has said. Sometimes it is better to work on something where you can gain some traction for a while, rather than spinning your wheels going nowhere for years.

   I think Einstein's practical experience working in the patent office helped sharpen his mind. String and other theorists could probably also benefit by doing some practical work in physics and engineering for a while. It helps build your physical intuition. Einstein said his patent office work helped him to see quickly to the heart of the problem.

9. **Hedge Fund Physics**
   December 31, 2007

   Happy New Year!

   On Betting Against Physics Hype in 2008

   We're expecting the LHC to find approximately nothing new, but it could be a lot of fun watching a lot of people claiming that they have the new TOE which the LHC will prove right or wrong.

   And we're upping the ante, and hedging against postmodern groupthink and tenured hypesters by calling them out, and betting them $137 that their claims are false. That would be $137 per claim.

   Let's start with claims made by the esteemed Dr. Kip Thorne who in a recent interview stated, (continued at
   http://hedgefundphysics.blogspot.com/)

10. **mitchell porter**
December 31, 2007

Have the readers of this blog ever suggested conditions under which they think *string theorists* would stop working on string theory?

In looking through Jacques Distler's blog in recent weeks, the most important thing I've learned is just that there is no known string vacuum which *exactly* reproduces the Standard Model (below 1 TeV, or whatever; and I suppose one should throw in the new "cosmological Standard Model" too) – there are just a few large classes of qualitatively realistic vacua. Jacques recently completed his proof that you can't fit the Standard Model into E8; one can imagine a similar, case-by-case proof for string theory (though it would have to be considerably more complicated). But so long as there are several potentially viable approaches, of course they'll keep at it.

11. **layman**
December 31, 2007

wow, princeton bookstore now sucks. I visited it around 20 yrs ago and i couldnt leave. Too bad. I was gearing up 2 go there again.

12. **Roger**
January 1, 2008

With respect to the question of what scientists have changed their mind about this year my answers would be the ILC and science communication.

I've come to see the ILC (at this point) as the experimentalists' string theory. Far too much effort has been sunk into it and too many reports have been published based on detector and Monte Carlo simulations, which are themselves largely based on speculative theories of beyond-the-standard model physics and expectations of what we may see at an, as yet, uncommissioned accelerator. The recent funding cuts are, to an extend, our political masters seeing the emperor has no new clothes (or possibly no new particles). Over the past few years whilst effort has been thrown into the ILC we've had a number of excellent facilities online which could have more profitably exploited much of the ILC's funding- Babar, HERA etc. Having worked on HERA I found the gradual loss of manpower to be very regrettable, especially since there are still many useful measurements to be made, which may now never take place. I'm not against the ILC but, in the UK at least, the rush to sink so much money into the project was IMO ill-advised, especially in light of the recent decision to throw that investment away and pull out.

On a related theme, I used to think that particle physicists were good at communicating the results of their research and the need for support for their field to the power brokers and the general public. It appears that we need something of a rethink in this area (in some countries at least), especially if results from the LHC are further delayed or turn out to be simply unexciting.

13. **Fabien Besnard**
January 1, 2008

I guess that not knowing the right questions to ask is precisely what makes quantum gravity attractive to some people, and discouraging to others. I'm pretty sure JB was fascinated by this aspect of the subject in the first place, and will change his mind again in some future (I hope so).
In my opinion, there might be another difficulty in quantum gravity, which is related to but distinct from the good questions to ask, this is the good time to start. It is quite possible that some pieces of the puzzle are still lacking, and it is just too early to start thinking about quantum gravity. In my opinion, one of these pieces might lurk in the quantum mechanics corner.
Anyway, there is no reason to feel depressed about this, since the work done so far in QG already led to considerable progress. After all, mathematics benefited immensely from the work of many people who started to think too early about Fermat's last theorem…

14. **milkshake**
January 1, 2008

One thing that this site could possibly achieve (and did, partially) is to puncture the orthodoxy bubble that "ST is a very powerful and beautiful framework and the only game in town".

The other one is to point at the general problem of salesmanship in presenting the research results – I have seen this in biology and chemistry (including some outright faked work) and I think it is a rather more insidious problem than particular fads and orthodoxy. In chemistry it can still take few years to uncover baloney or lay the hype to rest – and the experiments there are much easier, cheaper so the interesting results could be re-checked by a number of groups.
It is quite easy to fool yourself and from then on it is even easier to start fooling others.

15. **Arun**
January 1, 2008

Recently I was down in Princeton, and found out that the University Store, traditionally one of the best places in the world to buy math and physics books, has now gone out of the book-selling business, turning it over the Labyrinth Books.

That spoiled my day. Motl-Wotl is piffle.

16. **TomD**
January 1, 2008

I hate to hear of the "demise" of the U-Store. I spent a lot of my disposable income there as a grad student at Princeton, and have returned there several times since and had a great time browsing and buying.

It's getting tough to find good technical bookstores. Even with the existence of Amazon, we still need a place to actually see books before buying them. Last summer I had the pleasure of going to the Cambridge University Press bookstore in Cambridge, England, as well as Heffer's Bookstore, and those were wonderful….

17. **Andrew**
January 1, 2008

Peter, I think your New Year's resolution should be to ignore Motl. He appears to be a paranoid crank. Don't give him the oxygen of attention that he craves…

18. **dark-matter**
January 1, 2008

Lubos Motl and Britney Spears are identical in every way.
Does that help settle the matter? Let's move on.

19. **Paul Jackson**
January 3, 2008

Just as it's always darkest just before dawn, your report that a sophisticate like John Baez is finally giving up on quantum gravity may be a portent that a new way forward is about to emerge.

Nil desperandum, Peter Woit!

The work of the Brazilian theorists Aldrovandi and Pereira (on de Sitter relativity) looks to me suspiciously like the breaking up of a logjam on a big river of sterile theory. And that's my pennyworth.

20. **Kochemasov Gennady,RFNC VNIIEF,Sarov**
January 5, 2008

Pushkinean Quantum Gravity (1983-1986)

Dear colleagues!
It is clear that the big turmoil exist inside theoretical physicist's community. And main controversial problems are the status of quantum gravity and superstring theory (sorry for my English). In such situation I think that for some scientists will be interesting and may be useful to know about QG version which was developed twenty years ago by bright but mostly unknown physicist Alexander Vasilyevich Pushkin (1947-2004) who had worked in Russian Federal Nuclear Center VNIIEF located in Sarov. Unfortunately, A.V. Pushkin's results in fundamental physics have been published not in full measure, while those published are sometimes of fragmentary nature and mostly in Russian. Below I try to explain his ideas very shortly. Partly they were developed in collaboration with Dr. Gorbatenko M.V. I start by quotation from the book [A.V.Pushkin, "Geometrodynamics" which was written in 1997 and was published in 2005 in Russian by RFNC VNIIEF].
"As a result of a research (January 1983 through April 1986) the author managed to solve the problem of construction of the consistent (divergence-free) quantum gravity theory. Moreover, it was possible not only to prove finiteness of the theory, but also to constructively express its properties in a closed form – in the form of a system of nonlinear partial differential equations, whose solution set includes all the four-dimensional manifolds consistent with the causality principle <>. As it turned out, the construction of a consistent quantum gravity is equivalent to that of unified theory of material fields with unbroken local scale invariance. In the physical language this means that the theory satisfies the fundamental experimental fact, viz. the absence of absolute dimensional scales in the nature.
In the algebraic language the mathematical content of the theory reduces to a unique object, viz. commutative nonassociative algebra discovered by Griess, and a group of automorphisms of the algebra, viz. "Monster", or "Griess-Fisher Friendly Giant".
The geometric properties of the theory are expressed in terms of four-dimensional manifolds, whose structure is exhausted by that of two-dimensional surfaces globally embedded in them.
The connection between the two descriptions is established by the theory of representations of the Monster group, which is a theory of representations of some infinite affine Lie algebra of <>. In its turn this means that the quantum gravity theory can be expressed in terms of a system of completely integrable nonlinear partial differential equations, i.e. in that closed form which has been spoken about above. In what follows we will refer to this system of equations as geometrodynamics equations, or simply geometrodynamics.
Eventually, the complete integrability property provides a tool for comparison of one physical process with another by means of representation of invariants of one solution to the equations in terms of invariants of other solution(-s).
The author's research series (December 1986 through 1992) was devoted to solution of the inverse problem: A system of 10 geometrodynamics equations is given; the structure of mater and objects has to be constructed in the form of relations between their invariant characteristics.
That is, first, it was necessary to determine as consequences from the equations the principal laws, relations and set of basic constants that have been discovered in the nature and formalized in the set of phenomenological approximations (such as Newton and Coulomb laws, Dirac equation for electron, …, many-body coherent systems of quantum continuums, …, and, finally, nonlinear motions of ordinary continuums and up to cosmological-scale astronomical phenomena).
In the course of work on the inverse geometrodynamics problem the author was able to construct a set of quantitative relations among invariants of different-scale physical processes. To date it is already complete enough, which allowed quantitative comparisons of most important physical processes with an accuracy close to the modern possibilities of experimental metrology. In particular, this pertains to the relations between atomic and astronomic systems of units of time, distances, masses. (For separate quantities the calculation accuracy proved higher than their experimental measurement accuracy, see "Monstrous moonshine" and Physics in this book.) "
Now it is time for some words about above mentioned geometrodynamics equations. In seventies Michael V. Gorbatenko and Alexander V. Pushkin made important observation, following early idea of Y.A.Romanov (A.D.Sakharov colleague). They modernized Hilbert variation procedure in Palatini approach to affine connection space. They understood that usually putting to zero field variations on boundary may be wrong; such operations may contradict to causality. So they put nonholonomic constraints on variations by using Lagrange multipliers A , and equate to zero corresponding components. As a result they received the equations of general relativity (1), where stress-energy 4-tensor is expressed via A□ and metrical tensor gik: (2). These equations are invariant under conformal (gauge) transformations (3) , where – arbitrary function of four coordinates. We see that Aα – Weyl vector. So it is possible to transform them in some domain to the simplest Einstein form: (4), and this transformation correspond to changing of connectivity from Riemann to Weyl type: (5) (more about it you could find in [1 –General relativity and gravitation,2002,Vol.34 , No.2, P.175-188;2002, Vol. 34, No. 2, P.1131-1133; 2002, Vol.34, No.1, P.9-22.]) . Certainly, it is possible to start with (4, 5) and receive (1, 2). All this approach goes back to Herman Weyl, but equations (2) appeared seemingly first time in [2- VANT, 1984, in Russian]. I will call them as Gorbatenko – Pushkin (G-P) equations. Just these equations Pushkin received as the result of quantization of vacuum Einstein equations. As he explained G-P equations are sufficient for describing of all physics from smallest parts to cosmology scales in spite of absence any dimensional or dimensionless constants. You could see also on them as on basic dissipative medium equations in accordance with `Hooft idea. It is possible due to stress-energy 4-tensor structure (2) which resembles the structure of the usual tensor for dissipative fluid.
For first acquaintance with Pushkin theoretical universe let us see on the abstract of report which was presented by him on 2-nd Sakharov Conference on physics, Moscow, 1997.

"MONSTROUS MOONSHINE" AND PHYSICS

A.V.Pushkin

The paper presents some results obtained by the author on the quantum gravitation theory. This theory proves related to geometry of Cayley projective plane and the algebraic structure of the theory to the commutative nonassociative Griess algebra. The theory symmetry group is the automorphism group of Griess algebra: "Monster" simple finite group. Knowledge of the theory symmetry allows observed physical quantities to be computed in the "zeroth" approximation. Results of the calculations, including those for ~ 1/137 and , are presented, with the theory-controlled accuracy of the "zeroth" approximation being higher for some of them by 1–1.5 orders of magnitude than the accuracy of modern measurements.□
For me it was most amazing theoretical paper.
And what is about SS? Here I place one more citation from Pushkin book. "In the superstring theory a striking progress has been achieved recently: as few as five theories remain which are actually based on two lattice types in 16-dimensional space. Moreover, it has been proved that they are non-perturbative equivalents and as such are the limiting cases of the more general single theory. What last step should be made in the superstring theory in the direction of the construction of a single theory? Of course, this step is the gravity quantization, which will result in disappearance of the last dimensional scale in the theory, viz. the fundamental string scale. Then the theory will possess the local scale invariance properties and may perform quantitative calculations with non-perturbative methods by translating algebraic relation between physical quantities to different scale levels, including to the level of ordinary phenomena appearing in continuum motions in standard conditions.
In geometrodynamics, i.e. upon the gravity quantization, even this last dimensional scale disappears. In the language of the lattice theory in spaces this means that there should be the only possibility to avoid the need of appearance of dimensional scale: the fundamental root amount is equal to infinity (i.e. there is no separated cell) and the Weyl vector is therewith light-like, i.e. , which also requires no a priori scale. It is evident that the last requirement is readily formalizable at the level of the elementary problem of Diophantine equation solutions (see Problem 5.5). Having solved it, you will find those values of dimension of hyperbolic spaces , which determine the lattice arrangement of the geometrodynamics."
I think it is enough, because it is impossible to envelope unbounded. If you want some additional information try LANL arHiv (mainly gr-qc), where placed few papers written by Gorbatenko and Pushkin, and where are additional papers only of Gorbatenko. You can try also the recent publication: arXiv:0711.20113v1 [gr-qc] by

M.V.Gorbatenko and me. If you know Russian, is it possible to find much more papers which were published by RFNC VNIIEF (Voprose atomnoi nauki I techniki). Now extended English translation of the Pushkin’s book is ready and we (me and Gorbatenko, part of pushing group) are seeking for foreign publishers. P.S. If somebody prefers Loop QG approach, I add that knots appear very naturally in Pushkin s theory.
Happy New Old! (Russian) Year (13 January) and Russian (Orthodox) marry Christmas (7 January).

This Week’s Hype

January 3, 2008
Categories: This Week's Hype

New Scientist had the good sense to pass on last week’s hype about string theory testability, but is responsible for this week’s hype on the subject, with an article entitled String theory may predict our universe after all. It’s unclear why the article is appearing now, since it is based on a six-month-old preprint from a group at Oxford entitled Triadophilia: A Special Corner of the Landscape.

The authors basically just point out that there are very few known Calabi-Yau manifolds with small Hodge numbers, which thus have a small enough Euler characteristic to give just three generations. They speculate that some unknown dynamical vacuum selection mechanism favors these particular manifolds. In their paper they look only at the topology of the manifolds, so the only “prediction” about our universe is that the number of generations will be small, and this “prediction” is based on assuming an unknown dynamics that favors small numbers of generations.

There has been a huge industry since the late 1980s devoted to trying to extract physics out of the sorts of Calabi-Yaus studied by the Oxford group. This hasn’t gotten very far, with rather elaborate mathematical constructions being used to try and get the quantum numbers of the standard model particles to come out right. One problem with this is that one is not even sure that this is what one wants, since maybe the LHC will find more particles. The groups pursuing this strategy don’t seem to have taken much interest in the Candelas et. al paper, since SPIRES shows that no one has cited it during the last six months.

It looks like 2008 is not going to show any slackening of the promotion by string theorists of bogus “Despite what the critics say, string theory really is predictive!” stories to the press. This one contains quotes from Polchinski that the paper is “neat” and “Maybe it gives us a clue”, and from Strominger that it is “beautiful”. Strominger also minimizes the fact that the Landscape is a problem for string theory, saying:

> I don’t think it is incumbent upon string theory to solve the problem of the landscape… If we can’t make the landscape go away, it doesn’t mean that string theory is wrong. It just means it is not a complete solution to all our problems.

Michael Duff says the paper makes “some mathematically sound and interesting observations”, but does note that it doesn’t explain what selects small Hodge numbers, which is about the only slight amount of non-hype that makes it into the article.

Update: As a commenter here points out, the New Year also brings new progress on the scientific investigation of the landscape/multiverse, with a preprint from Don Page about how God loves all universes, not just ours.

Comments

1. **luny**
   January 3, 2008

   This: http://arxiv.org/abs/0801.0246

   might count as a prediction with some people 🙂

2. **T**
   January 3, 2008

   Peter,

   I know this isn’t exactly the topic of the post, but can you expand more on the Templeton foundation. I’m a hep-ex and know very little about this, but it appears to be almost an inside attack on science from religion. Is this too harsh?

3. **cwj**
   January 3, 2008

   The Templeton Foundation doesn’t seek to overturn accepted science, as do the creationists in their various mutations, but to interpret modern science in such a way as to make it “safe” for non-fundamentalist religion, for example, does QM give us free will? Does the unlikeliness of the universe suggest the existence of God? A bit misguided or silly, I’d say, although not as nefarious as the Discovery Institute (some will disagree here): Templeton doesn’t want to attack science so much as to co-opt it.

4. **Coin**
   January 3, 2008

   The authors basically just point out that there are very few known Calabi-Yau manifolds with small Hodge numbers, which thus have a small enough Euler characteristic to give just three generations. They speculate that some unknown dynamical vacuum selection mechanism favors these particular manifolds.

   Well… okay, never mind the dynamical vacuum selection problem for a minute. Let’s say we just *dictate* there are three generations, since that’s what experiment seems to indicate, and say that we’re only interested in those string theories that have the appropriate characteristics to get those three generations (small Hodge numbers etc). It’s certainly not a “prediction”, but does this at least significantly cut down on the size of the “landscape”?

   (Assuming the Hodge thing is valid at all, of course? You say there are very few “known” manifolds with this property. They say the number of manifolds with this property “appears to be” small… nobody sounds very confident here.)

5. **alex**
   January 3, 2008

   “but does this at least significantly cut down on the size of the “landscape”?”

   Sure it does! It has been known for a while that there are very few vacua that are actually relevant for phenomenology. Unless you care about tuning the cosmological constant, the vast landscape of flux vacua is pretty much irrelevant for model building. Lubos and others including Jacques have been saying this all along and for people who actually do computations of sparticle spectra from string compactifications the whole talk about 10^500 vacua bears little significance.

6. **Peter Woit**
   January 4, 2008

The number of generations is given by a difference of Hodge numbers, so you can get 3 in lots of ways, using Calabi-Yaus with large Hodge numbers. These authors are suggesting that one only look at ones with small Hodge numbers, which cuts down the number of Calabi-Yaus to look at. I guess with the small Hodge numbers one will also not have exponentially large numbers of flux vacua.

As far as I can tell, they just completely ignore the problem of the vacuum energy, so all their suggested vacua will have a CC off by more than 100 orders of magnitude. It's true that if you want to ignore the CC problem you can ignore the huge numbers of flux vacua, and until recently this is what most model-builders were doing. What Lubos has been "saying all along" is that he thinks one should do what these authors do and just ignore the CC problem, Jacques has been saying something different; he has some argument that even if you do use flux vacua to get the CC right, you will not completely use predictivity.

7. **JV**
January 4, 2008

Hi Peter,

I read so much on what is wrong with string theory on your blog (which by the way is good criticism, so I like refering to this site often); may I then ask just what is right about string theory?
(In outline form please… I'm no expert on the subject).

JV

8. **Peter Woit**
January 4, 2008

JV,

There's a lot one could say about this, but it is off-topic…

Very briefly, the two areas string theory has had the most success in are in algebraic geometry, where the topological string allows computations of invariants that are new, and in the study of strongly coupled gauge-theories via a string dual (AdS/CFT). String theory is a huge subject, and there are certainly other interesting things it has led to that can be pointed out.

It's really in one main area that it has completely failed, that of providing unification via a 10/11d theory. Unfortunately, this idea was its main selling point, and people are having trouble acknowledging it doesn't work.

9. **Roger**
January 4, 2008

If the number of generations were, hypothetically, measured to be 10000000, would this then disprove string theory ?

Its a prediction Jim, but not as we (experimentalists) know it.

10. **alex**
January 4, 2008

"The number of generations is given by a difference of Hodge numbers, so you can get 3 in lots of ways, using Calabi-Yaus with large Hodge numbers."

Sure, but only three CY manifolds with \chi=+/- 6 have been found so far after several decades of search.

"As far as I can tell, they just completely ignore the problem of the vacuum energy, so all their suggested vacua will have a CC off by more than 100 orders of magnitude."
In the Heterotic M-theory the tree-level CC can dynamically relax to zero due to a perfect square structure of the potential. There is a cancellation between the flux and the non-perturbative terms when one takes into account the warp factor. The higher order terms may be tuned away as well but I don't know the details since I'm not familiar with the Heterotic M-theory flux compactifications well enough.

11. **?**
January 4, 2008

Actually, one of the main issues to think about in studying the "landscape" is that there are local constructions that can easily given three generations, and fit into the manifolds that give exponentially large numbers of vacua. So regardless of whether this is true of the Candelas examples or not, it is quite false that three generations alone cuts the number of models down to a very small number, at least as far as we know at present. Three generations plus much more stringent phenomenological cuts could still reduce the number of models to 1 (or zero), consistent with present knowledge. Several groups (in Germany, at Rutgers and MIT etc) have actually tried to estimate what fraction of models give 3 generations, and their results are consistent with their being many, many such models.

12. **Savage**
January 4, 2008

Don Page's paper is not science but metaphysical nonsense. Where is the proof of one God loving one universe, forget about the proof of the existence of a muliverse. String theorist Leonard Susskind has now "created" a megaverse which is those worlds in the multiverse which are actualized. What nonsense. I see Page quoted discussions with Dawkins, which I'm sure, totally disagrees with him.

13. **m**
January 4, 2008

by the way, the only reason why the compactification space should be a Calabi-Yau (which is a very peculiar choice) is that many theorists believed that we need N=1 supersymmetry to solve the hierarchy problem. But now the hierarchy problem can be solved without supersymmetry, by invoking anthropic arguments. Calabi-Yau are no longer needed.

14. **luny**
January 4, 2008

Regarding Don Page's paper, it is not the first time that a physicist dabbled in methaphysics or theology (see Schrodinger or Einstein).

But the fact that this stuff appears on a PHYSICS PREPRINT site is genuinely new. And its not a sign of progress.

15. **wolfgang**

January 4, 2008

luny,

> But the fact that this stuff appears on a PHYSICS PREPRINT site is genuinely new.

worse stuff has been posted.

16. **Yatima**
January 4, 2008

"worse stuff"

A truly Fortean Link! I never thought that Spontaneous Combustion was connected to Poltergeist Phenomena. Then "sexual neurons" are mentioned. I like it 😉

17. **luny**
January 5, 2008

Yes, but not by a leading physicist with lots of great accomplishments.

18. **JV**
January 5, 2008

Hi again Peter,

[Sorry for the off-topicness of my previous and current posts], but now that you mention string theory's main 'achievements', may I ask which paper/s are the most important – or groundbreaking, or fundamental – in understanding the above-mentioned topics (technical and/or nontechnical)?

If string theory is, or is not, really up to the task it claims itself of being able to achieve, i.e. theory unification, may I then (with some skepticism as to its actual viability / realizibility) request the following…

(occasionally, or if possible, more frequently) Request various expert guests – who agree beforehand to attend – from both sides of the debate – in an 'e-townhall' meeting-style format – to present their specific pro- or o- points of views and let the global physics blogosphere ask very probing questions on each point / set of points presented by each, respective, guest.

This – I think – can go a long way to address your, and likely many others', concern/s of why string theory is arguably not the promised idea it claims itself to be.

{Of course, the debate should be *moderated* at all times to keep ad hominem and random attacks to a minimum or (ideally) zero level}.

Seems like a good idea (in theory at least); but I don't know if that could be realized when many foremost string theorists don't seem comfortable taking criticism – or at the prospect of having their research endevours publicly criticized – lightly!
!! This should be a sufficient reason – 🙂 – to present them with a challenge like this. !!

19. **a quantum diaries survivor**
January 5, 2008

Wow, Page speaks of "christian cosmologists"… The idea that there are cosmologists whose scientific research is confined and biased by their religious beliefs is disturbing! I thought the era of Teilhard de Chardin was long gone… I am obviously suffering from delusions.

Cheers,
T.

20. **Hans**
January 5, 2008

In this paper here:
http://arxiv.org/pdf/0801.0247
Don Page even "calculates" the probaability" for "pre death experience" at page 9.

Why is this stuff accepted on a physics preprint server?

(more disturbing is, whom he thanks in this crap. Some persons he aknowledges are:

David Deutsch, Bryce DeWitt, Gary Gibbons, Stephen Hawking, George Ellis, Andrei Linde, Lee Smolin, Bill Unruh, Alex Vilenkin, Steven Weinberg, Paul Shellard, Leonard Susskind, Alan Guth, James Hartle

(If my name would appear on such "work", i would take some effort, to get it removed)

21. **anon.**
January 5, 2008

> (If my name would appear on such "work", i would take some effort, to get it removed)

Those people are (with a couple of exceptions like Smolin) string believers and/or believers in uncheckable 'multiverse' interpretations of quantum mechanics and other religions crap.
That's probably why Page has cited them. Maybe they helped get his papers endorsed and on arxiv in the first place? 😉

22. **Tony Smith**
January 5, 2008

Hans mentions a few of the people acknowledged by Don Page in 0801.0247 but the list is very long. Also acknowledged there were:

Sean Carroll

Mark Srednicki
Richard Dawkins

They are of course able to speak for themselves as to how they feel about those acknowledgements (they were also acknowledged in 0801.0146).

It is interesting that both 0801.0246 and 0801.0247 were among "… a series of lectures sponsored by the Templeton Foundation and given at Shandong University in Jinan, China, autumn 2007 …".

I wonder what the Chinese (who invented gunpowder, the magnetic compass, eyeglasses, etc) thought of the first sentence of 0801.0247

"… Modern science developed within a culture of Judeo-Christian theism for several reasons … For example, the idea of a lawgiver for nature (i.e., God) encouraged belief in laws of nature …".

Tony Smith

23. **Tony Smith**
January 5, 2008

Sorry about a typo where I put 0801.0146 instead of 0801.0246.

Also, anon. speculates Don Page getting his "… papers endorsed and on arxiv …".
Don Page does not need endorsement of others. The arXiv says:
"… Don N. Page … Is registered as an author of this paper.
Can endorse for astro-ph, gr-qc and hep-th. …".

Tony Smith

24. **Peter Woit**
January 5, 2008

JV,

I'm afraid this is off-topic and I'm on vacation, so I'm not going to try and get together references for you. Perhaps someone else here can help. For AdS/CFT, there are lots of review articles, for parts of string theory that have been useful in algebraic geometry, that's a complicated subject, and what's worth reading depends both on your background and on what you hope to get out of the material.

I'd be happy to participate in the sort of debate you describe, but I don't think you're going to find many string theorists willing to go along with the idea. Lee Smolin and I both put a lot of effort into carefully putting into words our arguments about string theory, and I think both of us have been discouraged by how little serious response this led to (the two exceptions I can think of are reviews by Aaron Bergman and Joe Polchinski). The lack of this serious response, and a sizable amount of rather appallingly unserious response, has led to the present situation where even string theory partisans write publicly that string theory has lost the public debate.

The string theorists most likely to want to particpate in an internet-forum debate are those that have blogs, and there are only three active ones that I can think of. Of these, Jacques Distler and Clifford Johnson have explicitly announced that they refuse to read my book or Lee's. The other one is Lubos Motl.

I've privately talked to many string theorists about these issues, and found that we generally agree on quite a lot, although they're unwilling to enter into public discussions due to the level of personal hostility they see going on. I've heard that many prominent string theorists turned down the opportunity to respond to my book and Lee's in various publications. My take on all this is that they are well aware that on the central issue here of whether string theory has failed as an idea about unification they know very well that the case for their side is weak, so have decided it is best to keep quiet and hope this blows over, perhaps with the LHC changing the debate in some unknown way.

By keeping quiet, unfortunately the most sensible string theorists let their field be represented largely by people who are doing a good job of alienating anyone who might be sympathetic to their point of view.

25. **wolfgang**
January 5, 2008

Hans,

> Don Page even "calculates" the probaability" for "pre death experience" at page 9.

I wish you and everybody a lot of pre death experience 😎

26. **JV**
January 5, 2008

Hi Peter,

"Lee Smolin and I both put a lot of effort into carefully putting into words our arguments about string theory…"

I have a copy of your book – by the way – but to my immediate knowledge it does not contain a list of selected specific references of the sort I was hoping to receive (from you, or otherwise) {… I may be wrong as I don't have the copy with me while I'm writing this…}.
[I actually have not completed reading the book in its entireity as yet since many parts are currently beyond my technical understanding; but perhaps as an online Appendix to the work references may prove useful to readers who have the prerequisite background/s.]

This may help to clear up a great deal of confusion – no doubt much unfavorably brought on by excessive sensationalism from general media sources – regarding the program's specific and credible merit, and future relevance and prospectives … especially for theory unification.

If, however, such an attempt fails, I don't think there is much to be said regarding string theory and the question of its dominance in current hep theory research – it's here to stay and there's no point trying to oppose it – whether on scientific or otherwise grounds!

(I hope that doesn't occur; my hunch is that many people will only actually take a greater note of your points on string theory's unviability if you provide a more 'scholarly-like' setting to your blog -> book section, even if the general string theory community feels uncomfortable reading / responding to your work directly).

Hope, anyway, you get the time to reflect upon my proposition once you return to academic duties. Let me know.

Regards,
JV

27. **Michael Bacon**
January 6, 2008

"David Deutsch, Bryce DeWitt, Gary Gibbons, Stephen Hawking, George Ellis, Andrei Linde, Lee Smolin, Bill Unruh, Alex Vilenkin, Steven Weinberg, Paul Shellard, Leonard Susskind, Alan Guth, James Hartle"

"Those people are (with a couple of exceptions like Smolin) string believers and/or believers in uncheckable 'multiverse' interpretations of quantum mechanics and other religions crap."

Hans and Anon. You guys have to be kidding, no? When you guys contribute half of the "crap" these folks have, then maybe you'll be secure enough of your status in the scientific community not to worry about whether you are revealed to have had conversations with other (albeit goofy) researches, even if you don't agree with much of what they have to say.

Peter, I agree with you generally regarding the failure of the string theory project to acheive its stated objectives, but please don't encourage this type of know-nothing behavior by ignoring it. Thanks.

28. **Hans**
January 6, 2008

Michael Bacon wrote: Hans and Anon. You guys have to be kidding, no?

Well,
The persons whom Don Page thanks must indeed not worry about their status. But that is not the problem.

With his long list, to whom Page thanks to, he wants to create the impression that he has discussed such topics among the leading figures of the physics community and therefore, this stuff is acceptable to a physics preprint server.

So, the people who are listed there are, in some sense, a support for this crap. Don Page might have put them in their paper without even asking them.

The problem is not, that Steven Weinberg and others would have to fear about their status. Thats certainly not the case.

The problem is, that in a true world, leading figures should take pains, not to appear in such pseudoreligious crap. They should distance themselves from this paper forcefully.
They should make clear, that the opinions of Don Page are minority opinions, having nothing to do with physics or science at all and should ask arxiv to remove this crap.

I hope there is nobody out there, who wants more papers like this on arxiv org (maybe with more "supporters" listed to).

29. **Chris W.**
January 6, 2008

While the appropriateness of Don Page's preprint on arXiv.org might well be questioned, given its theological content, I think it should be acknowledged that it is not bad as these things go. Page is a committed Christian, but he is also a first-rate physicist. His arguments are hardly comparable with the kinds of evasions typically indulged in by ID advocates (never mind creationists). The paper's preface specifically addresses the question of appropriateness to the arXiv.

By the way, Page's undergraduate alma mater, William Jewell College, is an interesting place. (Donald Marolf is another of its graduates.) Four years ago the college lost the financial support of the Missouri Baptist Convention, with which it had a longstanding connection, because it stood up to a challenge by the MBC of the College's teaching of evolutionary biology.

Leaving aside the theology, the paper does not offer much reassurance to those who regard the putative existence of the multiverse as a horrendous epistemological can of worms for scientific cosmology and physics itself.

30. **Peter Woit**
January 6, 2008

About the Page paper and its acknowledgements.

Perhaps the hep-th moderator had the good sense to at least insist that it couldn't be posted there if that's where it was originally submitted.

I don't think you can make people responsible for the content of papers in which they are thanked, but in this case, many of those thanked are among those promoting anthropic pseudo-science. It continues to surprise me how few prominent people in this subject are willing to publicly criticize this kind of nonsense in any way. David Gross is one of the few examples I can think of. The problem is not so much a few serious theorists posting obviously silly preprints on the arXiv, arguably anyone who has done serious work in the past should be allowed to make a fool of themselves at least a couple times. The problem is the increasing infiltration of the field by pseudo-science, as almost everyone tolerates an ever-increasing level of it without comment.

31. **Michael Bacon**
January 6, 2008

"The problem is, that in a true world, leading figures should take pains, not to appear in such pseudoreligious crap. They should distance themselves from this paper forcefully. They should make clear, that the opinions of Don Page are minority opinions, having nothing to do with physics or science at all and should ask arxiv to remove this crap."

Hans, I guess these folks, notwithstanding their other accomplishments, just don't have your keen sense of intelligence when it comes to how they should behave when asked to discuss certain limited scientific issues by a fellow scientist (even one that's a little nuts). Hopefully your clear sighted comments on how to treat these types of situations will become more widespread. What a much better place the scientific world will be when we can finally be rid of the misbehavior of Hawking, Deutsch and their ilk. 🙂

32. **Coin**
January 6, 2008

The number of generations is given by a difference of Hodge numbers, so you can get 3 in lots of ways, using Calabi-Yaus with large Hodge numbers. These authors are suggesting that one only look at ones with small Hodge numbers

Hm, okay, that answers at least one of my other questions. Do you mind if I poke at this a bit more? (I'll admit upfront that I'm only asking because I think their diagram on page 4 of the PDF is real pretty, and I'm curious what it means.)

What exactly is a Hodge "number"? I am having trouble finding a clear definition on that. The closest I can find are the dual explanations of [this](#):

> The Hodge number is the analog of the Betti number on real manifolds for complex manifolds.

and [this](#):

> the Betti number of a topological space is, in intuitive terms, a way of counting the maximum number of cuts that can be made without dividing the space into two pieces.
>
> This defines, in fact, what is called the first Betti number. There is a sequence of Betti numbers defined… the k-th Betti number… of the space X is defined as the rank of the… k-th homology group of X

Hm, okay, but why do you need two indices to identify a Hodge number, and why are the only indices the Triadophilia people are interested in h^11 and h^21?

Is there some kind of "intuitive" explanation of the Hodge numbers, or h^11 and h^21 in specific, analogous to the "cuts" explanation for the first Betti number?

33. **Peter Woit**
January 7, 2008

Coin,

Calabi-Yau manifolds are complex (Kahler) manifolds, and in such a situation the cohomology, instead of being graded by a single number is graded by a pair of numbers. The easiest way to see this is using the deRham model of cohomology, where the n-th Betti number is the dimension of the space of differential forms of degree n that are harmonic (Laplacian on them gives zero). If you have a complex manifold that is Kahler, such differential forms break up according to the number of dz's and dz-bar's in the differential form of total degree n. For the details of this stuff, you really need to consult a book on complex manifolds, Chern's short one is relatively readable.

34. **King Ray**
January 7, 2008

Peter, is this the Chern book you meant?

http://www.amazon.com/Complex-Manifolds-without-Potential-Theory/dp/0387904220/ref=sr_1_10?ie=UTF8&s=books&qid=1199729426&sr=1-10

35. **Peter Woit**
January 7, 2008

King Ray,

Yup, that's it.

36. **Coin**
January 7, 2008

Thanks Peter/Ray, that helps a lot.

37. **King Ray**
January 7, 2008

Peter, thanks. I don't think I have that one, so I may pick up a copy.

Kind of an odd title, "Complex Manifolds WITHOUT Potential Theory."

Maybe some string theorist should write a book titled "String Theory WITHOUT Testable Predictions."

38. **King Ray**
January 7, 2008

Coin, you're welcome. I was curious to see the book recommended by Peter so I looked it up on Amazon.

The formulas for the connection and curvature tensors are so much prettier and simpler on complex manifolds.

39. **Xerxes**
January 8, 2008

Three is small now? I've been talking to large-Nc people, and they tell me that three is approximately equal to infinity. I guess three is a small number of generations, but a large number of colors. Is it a large or small number of spatial dimensions?

40. **Matteo Martini**
January 8, 2008

If I can, I would like to ask a question to Peter Woit and all the people who maybe interested to reply to me.

I admit this question is a small derail from the topic, but I would like to have the question written here, rather then ask the question by Email to any of the posters.
I ask pardon to Peter for that.

I would like to know if you think there is any future for particle phyisics after the current state of knowledge.
As far as I have understood, after the LHC will go online in the summer of this year, there will be no other plans of building more powerful accelerators (as the ILC simply adds luminosity, but not more electron volts).

Since most part of the human progress in the last 50-100 years has been done in fields such as electronics, semiconductor industry, biochemistry and related (informatics, pharma), etc, which are more or less related to advancement in basic physics done in the last 50 years or more, I do not think it is baseless to say that, once we will hit the

wall in the current progress in quantum physics (have we already hit that wall?) we will also (sooner or later) hit the wall in finding new ways to progress our knowledge in many fields, with this potentially ending human progress forever.
Without new and more powerful accelerators coming online after the LHC, it may be that new theories after the standard model could not be written as there will be no accelerator powerful enough to provide the necessary data/testing conditions.

Am I the only one to think this?
Again, sorry for the derail and for lowering the average technical level of the comments of this thread.

41. **chris**
January 9, 2008

hi matteo,

if we knew what the lhc would show us, we would not even need to switch it on. maybe there is no future for large collider rings. but there is also some natural limit to refractor lense sizes, which was reached about 100 years ago. still this was not astronomys death. we'll just have to wait and see.

42. **DB**
January 9, 2008

Matteo,
It's quite a big derail from the topic, but if Peter will undulge me I'll give you some pointers.
Be sure of one thing, the LHC will fundamentally alter our picture of particle physics. It will give us a much clearer idea of the merits of the Higgs mechanism and supersymmetry and this will be crucial for future theoretical and experimental progress.

The ILC is dead, flavor physics has been dealt a serious blow with the axing of BaBar, and the US is not interested in building a successor to the LHC on its territory. It will continue to participate heavily in particle astrophysics but even here NASA is under great budget pressure. If NASA pulls the plug on particle astrophysics, the European Space Agency will help plug the gap.

Future particle accelerators will not be planned until the LHC delivers its results, and if they are built the effort will be led by CERN, and, for cost reasons, will have to be a globally funded project, as with ITER. The US may well decide to participate financially. It's a long way off, but we have waited 30 years between LEP and the LHC (pace the excellent work done at the Tevatron) and we have learned to be patient. Remember also that there is more to HEP than the LHC and its successor. The most significant advances in HEP in recent years (neutrino mass/oscillation and the solution of the solar neutrino problem) have come from modest neutrino experiments in Canada (Sudbury) and Japan (Kamiokande).

We have not hit the wall in quantum physics, far from it. If you check the LCLS project at SLAC and its equivalent XFEL at DESY you will discover how rich are the prospects for useful advances in this area in return for comparatively modest expenditures, although the fact that two machines are being built tells me that HEP still hasn't learned its lessons with regard to needless duplication of effort.

As the ISS has shown, the only viable model in the future for big science is a global one and I have no doubt that we will see plenty of exciting developments along these lines in the years to come.

Be of good cheer, we are nowhere near the end of this game.

43. **Matteo Martini**
January 9, 2008

DB,
thanks for your explanation.
Again, sorry for the derail.

Matteo

44. **J.F. Moore**
January 9, 2008

Just a note on DB's comment: LCLS and XFEL are not HEP projects. In fact, SLAC is now administered as a BES lab, in recognition of where the bulk of their funding is coming from. These two machines are also being created as user facilities, so even if they had identical photon beam properties (they don't), having one on the same continent is a big plus for a 'user', which in practice can be a small academic group with travel funds.

My side-advice to Matteo is to get out there and talk to people, read their papers and see what moves you. Eventually it becomes clear what you must do.

45. **chris**
January 10, 2008

DB,

as j.f. moore remarked, lcls and xfel are no hep projects. xfl once was a sideline of the ilc-predicessor tesla. tesla is dead now, and its hosting lab, desy, is also transforming from a hep into a material science lab. so this particular game was lost by hep.

46. **WiCoB**
January 18, 2008

What I found interesting in the NewScientist was a subtle manipulation of figure captions and suggestions of something mysterious in the triangular shape of the plot of known C-Y manifolds. I am not sure if it is still within reasonable scientific journalism, or if the hunt for news item has gone too far this time?

More on FY 2008 HEP Budget Cuts

January 10, 2008
Categories: Experimental HEP News

The disastrous US HEP budget cuts that were announced just before Christmas, a quarter of the way into the fiscal year, have been getting a lot more attention from bloggers now that the holiday season is over, and their implications are starting to become clear. There are new blog posts from HEP bloggers Tommaso Dorigo, Alexey Petrov, Gordon Watts, and Michael Schmitt (as well as non-HEP blogger Chad Orzel).

It seems to me that Gordon Watts has it about right, entitling his posting "Screwed by the Democrats". As far as I can tell, very few people know who it was that made the last-minute decision to hit HEP with these huge budget cuts targeted at its future programs or what their justification for this was. Presumably this was done by certain staff members of the heads of the relevant Congressional committees. Gordon explains how all the evidence points to physics getting cut precisely because the relevant parts of the executive branch had made it a priority in their proposed budget. When the Democrats lost the game of chicken that they and the White House were playing with the budget, and had to find some way to make cuts at the last minute, things that were an administration priority were first in line to get cut. So, HEP lost out here not because it has done a bad job at making its case, but because it did too good a job….

One reason that these large cuts had to be made was the decision by the Congressional leadership not to do what they had done last year, which was to cut all earmarks from the DOE budget. A new AAAS analysis concludes that the new budget contains $4.5 billion in R and D earmarks, and that the DOE and Department of Agriculture were the most heavily earmarked R and D agencies.

Some bloggers have suggested that physicists need to redouble their efforts in public education about HEP, but I think Gordon is a bit closer to the right idea, as he has sent $250 as a campaign contribution to Bill Foster, a Fermilab physicist who is running for Congress. Probably even more effective would be if the APS would put out a web-page explaining exactly which of our Congressional representatives were responsible for deciding to hit HEP with these cuts. If they would do that we could then all write to these people saying that we appreciate their public service and include a large check for a campaign contribution, at the same time mentioning that HEP funding happens to be a big personal concern. This seems to be how US democracy works these days: you need to pay to not get screwed, and we haven't been paying…

As for what the effects of these cuts are, there's more news coverage here, here, here, here, and here (Fermilab has a web-page of links here). Here is the text of SLAC director Persis Drell's talk at an All Hands meeting there. The effect of the cuts on SLAC will include having to lay-off 125 people and shut down the B-factory at the beginning of March. Layoffs will be announced in early February, with people leaving their jobs in early April. Senator Durbin of Illinois is talking about an effort to add money for Fermilab to the Iraq War "emergency funding" bill the Senate will be taking up this spring, but says "It won't be a huge amount… I don't want to suggest to anyone we will make them whole." It's unclear whether this is a realistic possibility, or just Durbin trying to look like he is doing something about this.

Update: Here's a letter about this from Dennis Kovar at the DOE. There's a detailed article about the situation by Adrian Cho at Science magazine.

Comments

1. **Peter Woit**
   January 10, 2008

   A reminder: informed comments are welcome, but uninformed commentary on any issue. but especially this one, isn't. If you want to engage in discussion of this issue at the level of "it's better to fund (socially desirable goal X) than HEP", please do this elsewhere.

2. **Zathras**
   January 10, 2008

   I reiterate my earlier comment that this would never have occurred if HEP had a competent lobbying organization. This is never a problem in other sciences, such as biology and chemistry, which have strong, focused lobbying help from Big Pharm.

3. **Paul Guinnessy**
   January 10, 2008

   Physics Today will be looking closely at the science policies of the presidential candidates at http://blogs.physicstoday.org/politics08. We're still adding material of the moment, but there's enough there for some conclusions on the future of science and HEP under the next administration. Feel free to comment on the site, or submit a longer article for posting.

4. **Nugae**
   January 10, 2008

   Isn't this all a bit supine?

   Why not announce the immediate and indefinite closure of all HEP programs, giving the reason that modern science requires reliable funding? Instead of trying to beg for a little more money here, a little more there, say clearly and straightforwardly that you appreciate the careful reasoning that went into the legislators' decision and that you accept that the United States as a country is simply too poor nowadays to be able to afford big science. Express the hope that Europe and China may be able to continue human progress in this field.

   Don't go grovelling to the people who voted this in: instead, make sure that everyone knows who has brought the entire country its biggest international humiliation since Sputnik.
   If your only response to spiteful budget cuts is an indignant whimper, you deserve to suffer them.

5. **woit**
   January 10, 2008

   Thanks Paul. Would be interesting to hear what the candidates think about Congressional R and D earmarks.

   Nugae,

   I see a few minor possible problems with your plan…

   The first is that I still can't figure out who is actually responsible for this. It would be nice to know who they were, and what their justification for these actions was. Once they're identified, then we can argue about whether it's likely to be more fruitful to try and publicly shame them or pay them off so they don't do it again.

6. **Gordon Watts**
January 10, 2008

Just a quick point — it wasn't just HEP that got cut. ITER, for example, got hit pretty hard as well. There was also an across the board cut of about 1% (which combined with inflation makes it more like 3-5%). But clearly, part of the problem seems to be that we did lobby effectively, got the WH on board, and it looks like that raised the profile enough to make us a target.

7. **woit**
January 10, 2008

Thanks Gordon,

I agree. If it were only HEP that got cut, then the explanation could just be that some people think HEP shouldn't be funded since it isn't aimed at practical applications. ITER is very much aimed at an important practical application, and it too was cut. It looks like someone decided to go after the physical sciences initiative of the Bush administration, especially large projects it was supposed to fund.

8. **Yatima**
January 10, 2008

"*pay them off so they don't do it again*"

Commissioner: "What a nice little HEP budget you have there. A shame if something should happen to it…"

Physicist: "I do think, sir, that you will very much appreciate this somewhat stuffed brown envelope…"

Commissioner: "Thank you. It's a pleasure discussing the future of science with you. If you will excuse me, I have an important meeting concerning urban waste management."

9. **nobody**
January 10, 2008

Legislators will not continue to spend money funding big science. Here's why.

If the research done would be done instead in Europe or China, then the benefits of that research would still go to everyone, right? That is, a major physical discovery of the laws of nature would identically benefit those who funded and those who did not fund the research, so why fund it? Let the Europeans pay for it.

It can be argued that someone should fund the research, since it benefits everyone.

But the fallacy in that reasoning is that pure research by definition takes decades before it concretely benefits anybody. Sometimes longer than that. No congressman, and few of his constituents, is likely to be in office – maybe even alive – by the time any research benefits come to fruition. By comparison, social programs have immediate benefits: criminals are incarcerated; people are given health insurance; the poor and minorities are helped; and so on.

The political support for science in the United Science was, historically, anomalous, driven mainly by the unique circumstances of the Cold War. I don't think it can continue.

10. **chris**
January 11, 2008

hey, nobody,

let me recapitulate… 1938, nuclear fission was discovered. 1945 the first bomb was dropped.

yes, let china lead the way into exploring ewsb, dark matter&co. sure the us will benefit equally from it :-).

11. **Dave Miller**
January 11, 2008

Chris,

Your example actually proves "nobody's" point, doesn't it?

Fission was discovered in Nazi Germany; the US built the bomb.

This example (and it would be easy to give many others) demonstrates that the pure-science discovery and the practical applications often occur in different countries.

When I was a doctoral student at SLAC around 1980, faculty openly joked about the fact that the feds kept funding us simply because physicists had created the bomb and the politicians feared we might come up with something else of the same sort. The joke of course was that nobody could come up with any plausible scenario by which this could happen — the feeling was that we were putting one over on the dumb politicians.

Well, at least on this issue, the politicians seem to have gotten the joke.

Of course, HEP funding is such a tiny fraction of the budget that it really hardly matters fiscally. But this does leave it vulnerable to the sort of political infighting Peter describes.

And, Peter, I doubt that there are anywhere near enough physicists in the country to affect such matters by their campaign contributions. There is also the problem that quite a few of us care more about other issues than HEP – for example, my top issue this year is this fraudulent war, not HEP funding: I'd guess this is the case for many other physicists.

Dave

12. **Roger**
January 11, 2008

As the previous posters have pointed out, it is very difficult to get across the message to a national government that it should fund HEP.

I suspect this is especially true in the US since, in order to make progress, one has to perform big research in the context of international collaborations. In such circumstances, it is difficult to realistically say that the US is the undisputed “leader”, something which politicians may well want to hear.

I believe that HEP needs a rethink, not only in the US, but also in the rest of the world regarding how we sell the subject.

13. **Peter Woit**
January 11, 2008

Dave,

The beauty of the current US political system is that your ability to influence government policy just depends on how much cash you can devote to doing this, not on how many people agree with you. And also this has nothing to do with whether you want the person you are paying off to be re-elected or not. If you think their opponent would be better, all you have to do is give money to both, more to the one you would most like to see elected, but enough to both to make sure they pay close attention to details of spending bills they otherwise couldn’t care less about, so they don’t screw you and your scientific field.

We really do though need to first actually identify the right people to pay off. Especially if they’re from small states with safe seats, the sums needed should not be exceptionally large. Just one ex-physicist now working for a successful hedge fund and willing to devote a non-trivial percentage of his latest bonus check to this cause would probably do the trick. My understanding is that the way this is done is that you actually hire a professional fixer (a “lobbyist”) to figure out how to, within the constraints of the law, get the biggest bang for your buck.

By the way, in case this isn’t obvious, I’m not really joking here. I do think this is probably the best way to get the US government to continue funding HEP research as it has done in the past. And I also think this is completely appalling and disgraceful.

14. **Zathras**
January 11, 2008

Peter,

What you say about the importance of lobbying is in on the right track, but enormously over-simplified as to how it actually works. Look at it from the perspective of an overworked congressional staffer. He or she has literally thousands of projects to wade through, deciding which to recommend and which not to. It is impossible for a staffer to make an informed decision on his own about every project. Here’s where a lobbyist comes in. A lobbyist explains the worth of the project he is lobbying for. It’s about access and the efficient, targeted spread of information (as opposed to putting up a website which nobody in Congress likely has ever seen.

Forget about campaign finance. The dollar amounts needed now are too high for a small collection of non-billionaire individuals to reach a meaningful set of legislators. Reaching some random legislator will not help, since he or she needs to be on the right committee-otherwise, he’s just one out of hundreds.

In the past, lobbying the executive branch was enough. Lobbying the legislative branch is now at least as important, and that is where the hole has been in physics lobbying.

15. **Peter Woit**
January 11, 2008

Zathras,

Thanks. Of course I was over-simplifying for effect. But it still seems to me that to get “efficient, targeted spread of information”, you need “access”, and what is going on these days is that this is effectively being bought and sold.

I’d still like to know who the relevant congress-people are in this case. I’m assuming it’s a very small number. Maybe these people are unusually high-minded and I’m overly cynical in my assumption that money could be effectively used to get them and their staffers to pay more sympathetic attention to the HEP funding problems.

I’m also curious to hear an estimate as to what sums of money would be useful for this purpose. While presumably these are beyond the reach of the average person living on an academic salary, there are increasingly large numbers of people out there with physics backgrounds enjoying exponentially larger incomes. While, these are not large enough to fund HEP research, but they may be large enough to convince people in Congress to do so.

16. **Haelfix**
January 11, 2008

I do have friends up high in NSF who told me that the primary reason this occured was someone up high in the Dems wanted to make a point along the lines of

‘The R’s forced us into a meager budget, so we had no choice. If you want your budget back, vote for us and we will increase spending’

Scientists are a loyal Democrat voting block, so this might compel them to come out and vote in an election year.

Anyway its maddening. Republicans do the right thing and fund HEP in general, but their reasoning is wrong and a relic of the cold war. Democrats have this nasty tendency to leave NSF a low priority, but have the correct reasoning as to why Science is important.

17. **Haelfix**
January 11, 2008

Its also become horribly obvious that we do indeed need to do a better PR job. For smart people you would have thought this would have been blindingly obvious a long time ago.

eg: Taking a percentage of the NSF budget and sticking it into a private lobbying group would likely pay for itself and then some.

I also noticed that Mr Gates privately funded part of an astrophysics project. It would be nice if that would cascade throughout the legions of uber wealthy americans who do so much for charity. I for one have no problem with having a Microsoft logo on my future pet linear accelerator.

18. **Visitor**
January 11, 2008

Let me see if I understand this correctly. Scientists are a “loyal Democratic voting block”… so the Democrats decided to gut their appropriations – thereby informing them that if they (the scientists) want their funding back, they had better vote Democratic in the upcoming election – which the scientists would do ANYWAY even without the budget slashes. And the scientists should vote Democratic so that the funding will be restored A YEAR FROM NOW – i.e. AFTER the damage is done.

Does it ever occur to anyone here that the Democrats have NO need to be concerned about your votes BECAUSE you are Democrats who have NO intention of EVER voting Republican. Why should they care about it, if you are so staunchly Democratic that they need not compete for your votes? If you were potential Republican voters, the Democrats would not dare to slash your appropriations.
Not only is the rationale reported by Haelfix unconvincing, but am I the only one who remembers civil rights demonstrations against NASA for spending money that (the demonstrators thought, I suppose) would otherwise end up in "anti-poverty programs" and similar pork-barrels? There might be OTHER constituencies in the Democratic Party that are either anti-science-funding, or are more important to placate than scientists, or BOTH, y'know?

How about THIS: the scientists vote NOT for the Democrats, who are willing to gut the science budget this year, but vote for the REPUBLICANS in the upcoming elections, because it is the REPUBLICANS who want to give priority to science spending and the DEMOCRATS who want to CUT it.

19. **AGeek**
January 11, 2008

I see repeated calls here for essentially using public money to lobby politicians for more public money. Surely anything along these lines would be illegal? If not, it should be… but anyway, it would be good if somebody who knows the legal facts could enlighten us about them.

Regarding wealthy ex physicists, it occurs to me that they presumably left for a reason. I wouldn't necessarily count on them to be sympathetic to those who didn't (yet).

20. **Haelfix**
January 11, 2008

Thats just what I was told roughly, I have no reason to doubt the party. Obviously its a little more complicated, as all budget fights are. Not just in congress but in the heart of the NSF and DOE.

21. **JC**
January 11, 2008

Are there any other physicists in congress, besides Rush Holt and Vernon Ehlers?

http://en.wikipedia.org/wiki/Rush_D._Holt,_Jr.
http://en.wikipedia.org/wiki/Vern_Ehlers

With respect to lobbying, maybe these two would be the easiest to approach first concerning physics funding issues.

22. **J.F. Moore**
January 13, 2008

Note that the IL governor's letter to the president a few days ago is less balanced than that of the two senators plus one representative sent in December. It discusses cuts to Argonne as "drastic" but Fermilab as "substantial". The press release names two projects at Argonne, but specifies none at Fermi. I do not know if these implications are deliberate.

http://tinyurl.com/32mfyo

@Zathras

Chemistry and biology are in general doing better than physics, but very few university and national lab based programs benefit from a pharma lobby. Many research programs are cut every year and research groups do struggle and die despite producing very well regarded work – but you don't usually hear about them because they aren't high-profile, billion dollar level projects like ILC or ITER.

23. **DB**
January 14, 2008

J.F.
I didn't get that sense from the governor's letter but I expect that in practice, politicians do distinguish between user facilities which have been created out of old HEP accelerators and which are designed to be useful to industry, and HEP facilities such as Fermilab.
The cuts at Argonne are to the Intense Neutron Source and the Advanced Photon Source, if I understand correctly, both are mainly used by material scientists and chemists. However, there are alternatives to the APS (not that the APS is being shut down, just on a reduced schedule), such as the National Synchrotron Light Source at Brookhaven, the Advanced Light Source at Lawrence Berkeley. And there are other pulsed neutron sources (Lujan at Los Alamos, the Spallation Neutron Source at Oak Ridge) most of which seem have remarkably similar missions. And there will be the coherent x-ray lasers coming onstream at SLAC and DESY in years to come.
So material scientists and chemists would seem to be doing pretty well out of the conversions of old HEP facilities at least according to this:
http://www.sc.doe.gov/bes/BESfacilities.htm

24. **J.F. Moore**
January 14, 2008

@DB,

In no way do I want to debate HEP versus other science priorities, but I did find some misconceptions in your comment that I think do need to be clarified.

1. The user facilities named are in a different office and budget category than HEP, called basic energy sciences (BES).

2. A small minority of those facilities are "conversions of old HEP facilities"; the APS, NSLS, and ALS, for example are 100% greenfield, many others are modestly leveraged.

3. Cuts to BES affect programs at all of these facilities, not just at Argonne, so the whole user community is competing for dwindling beamtime.

4. It is not always simple for a user to simply move to a different facility. Some are based locally, others have experiments that took years to build. Perhaps a useful analogy would be a very small detector for an HEP machine and its associated collaboration.

5. The users of these facilities include many physicists, biologists, and engineers as well as chemists and materials scientists. Industry represents a small fraction of the users. In addition, the staff at these places contribute to accelerator technology; many freely move between BES and HEP machines (or the foreign equivalent).

6. Cuts at Argonne are not just to IPNS and APS – for example, there is a high energy physics division that is obviously affected. The budget hasn't resolved at the level of the smaller programmatic divisions yet; it was simply bad enough overall that lab management recognized it had to make some drastic changes immediately by closing

IPNS.

My initial comment was just meant to point out that I think, for whatever political reason, the defense of ANLs budget seems to be stronger than FNAL at the moment – not that I think it is proper or justified at all. History has demonstrated that when scientists attack each other over budgets that usually everybody involved loses.

25. **Zathras**
January 14, 2008

J.F Moore,

In your response to me, you are comparing apples to oranges. The fact is that big pharm has lobbied strongly for all the big dollar biology and chemistry projects, such as the Human Genome Project. In contrast, there has not been significant industry lobbying (at least in Congress) for the big physics projects.

While it is true that there may not be specific lobbying for most of the smaller bio/chem projects, there is still a trickle down effect such that, once the people who need to know understand the importance of the big bio/chem projects they are more likely to understand the importance of the smaller projects. The smaller projects may not be directly connected to the big ones, but the smaller project PI's still know how to word things to make them appear related. Almost any human genetics grant proposal or other funding request brings up the HMG.

26. **DB**
January 14, 2008

JF,
Your initial comment drew attention to your perception of the governor's possible preferential defence of Argonne over Fermilab. Argonne is now principally funded by BES while Fermilab is mainly HEP funded.
I was simply drawing attention to what is the major trend within DOE laboratories formerly dedicated to HEP research, namely, their repositioning as BES-funded user facilities for use by materials scientists and chemists with a view to doing research with practical applications to industral problems, while leaving the pursuit of HEP increasingly to foreign powers.
Right now, SLAC is the typical example: BABAR is shut down early, while parts of the old linac are being converted to house the LCLS, whose funding will transition from HEP/BES funding this year to full funding by BES next year.
While some believe that the recent cuts to the HEP budget are the result of Congressional shenanigans and revenge, others view this as a smokescreen which obscures the continued implementation of a well-thought-out strategic decision.
If, as you seem to suspect, the governor is pushing Argonne over Fermilab, then he is also pushing BES over HEP (and yes, I know there are some HEP projects at Argonne) and perhaps he is doing this because he believes this approach is consistent with federal priorities.
I am not interested in debating the merits of such priorities, I just want to understand what is really going on.

27. **J.F.**
January 14, 2008

@Zathras – I think we disagree on too much to comfortably hash it out here.

@DB – You are certainly right about the trends. I was mainly nitpicking above, but I also want to deflate the common assumption that HEP folks seem to make: that all other scientists have no serious budget problems.

Thanks to Peter for hosting a venue where some of us can puzzle this out, and hopefully share info as its learned.

28. **DB**
January 15, 2008

JF,
You might find the following just published interview with Stephen Weinberg of interest.
http://www.thespacereview.com/article/1037/1
In it he spends a good deal of time attacking the budget priorities of NASA.
The example of the 1 billion $ European-built Alpha Magnetic Spectrometer which NASA now says it won't launch is yet another interesting example of how the US is treating its international "partners".

29. **Al Newmann**
January 15, 2008

This seems to me to be just another example of the Great Sucking Sound Ross Perot talked about. The US continues to lose industrial and scientific capabilities to foreign lands. It will not be too much longer until the only jobs left in the US will be Government programs for managing the unemployed and protecting Government officials and our ruling elite from the poor, hungry, and homeless.

30. **nobody**
January 16, 2008

Why do physicists so often attack the funding of other science projects?

Weinberg, in his interview, argues at length that NASA funds would be better spent on other scientific research.

The budget is about $1.2 trillion. NASA's budget is $9 billion. Some NASA funds do help science. So why does Weinberg not complain about expenditures in the remainder of the budget, most of which does not help science in any way?

I am not trying to make a political point here – I am truthfully confused at why scientists so often attack other scientists' funding. Wouldn't it be more productive for scientists to argue that science as a whole should be better funded?

(I have heard the red herring that science funding is a fixed pool so it's a zero-sum game, but that's false. In the 60s, for example, both NASA and research science were much better funded than they are now.)

31. **Peter Woit**
January 16, 2008

nobody,

I think one could describe Weinberg's not as attacking the funding of any science projects, but the funding of manned space missions, which are not the same thing. In practice, the problem of such funding causing science projects to end up defunded at NASA is a very real one, and I think most scientists share Weinberg's view of the problem and frustration about what is happening. Just saying that everything should get funded isn't very realistic or likely to be taken very seriously.

32. **Arun**
    January 29, 2008

    Via CapitalistImperialistPig – Bush said this in the State of the Union address:

    http://www.whitehouse.gov/stateoftheunion/2008/index.html

    To keep America competitive into the future, we must trust in the skill of our scientists and engineers and empower them to pursue the breakthroughs of tomorrow. Last year, Congress passed legislation supporting the American Competitiveness Initiative, but never followed through with the funding. This funding is essential to keeping our scientific edge. So I ask Congress to double federal support for critical basic research in the physical sciences and ensure America remains the most dynamic nation on Earth. (Applause.)

33. **Peter Woit**
    January 29, 2008

    Arun,

    Looks just like a replay of last year: the administration will propose sizable increases. Quite likely Congressional committees will hold hearings and approve them. Then, several months into the fiscal year, the actual budget will come out.

    This is an election year, so I doubt there will be a budget before early November, and depending on what the results are, perhaps the new budget won't happen until next January, after the new Congress and president are in place.

Update on Plagiarism Scandal

January 10, 2008
Categories: Uncategorized

Last summer I wrote here about a plagiarism scandal involving more than 60 arXiv preprints, more than thirty of which were refereed and published in at least 18 different physics journals, some of them quite prestigious ones (see also the page at Eureka Journal Watch). At the time I wondered what action the journals involved in this scandal would take in response to it. Nearly six months later the answer to this question is now in: essentially none at all. As far as I can tell, almost uniformly the journals involved don't seem to have a problem at all with being used to publish plagiarized material.

Unlike the journals, the arXiv has taken action. It has withdrawn the papers, replaced their abstracts with lists of where they plagiarized from, and put up a web-page explaining all of this. After the scandal became public, one journal, JHEP, did withdraw the one rather egregious example of plagiarism it had published. This was only done after JHEP originally refused to do anything about this when first contacted last March, arguing that since the plagiarized articles were cited in the paper it was all right, and besides, they would only consider doing something if the plagiarized authors filed a formal complaint. Copies of the correspondence about this (and much else) are at this web-site.

The nature of the plagiarism varied greatly among the papers withdrawn by the arXiv. Sometimes all that was involved was self-plagiarism (large parts of one paper were identical with others submitted by some of the same authors), but mostly what was being plagiarized was the contents of papers by others. Mustafa Salti, a graduate student at METU, had his name on 40 of the withdrawn papers, many of which have been published in well-known journals. I checked a few of the online published journal articles corresponding to the withdrawn papers and, besides the JHEP paper, I didn't find any others where the online journal article gave any indication that the paper was known to be plagiarized.

A more complicated case is that of Ihsan Yilmaz, where the arXiv lists three of his eight arXiv preprints as withdrawn due to plagiarism and one as withdrawn due to "excessive overlap" with two other papers of which he was co-author. Very recently one of his Physical Review D papers, a paper that was not one of the ones on the arXiv, was retracted with the notation:

> The author withdraws this article from publication because it copies text, totaling more than half of the article, from the articles listed below. The author apologizes to the authors of these papers and to the publishers whose copyright was violated.

After the scandal broke, Yilmaz had a letter published in Nature where he justified the sort of plagiarism found in his articles, claiming "using beautiful sentences from other studies on the same subject in our introductions is not unusual." Evidently the editors of the journal General Relativity and Gravitation agreed with Yilmaz. They decided not to do anything about the papers they had published that were withdrawn from the arXiv, writing an editorial in which they defended the papers, while noting that "we do not regard such word for word copying of introductory and descriptive material by others as acceptable."

I heard about the GRG editorial via an e-mail from a group of the faculty at METU, who write that:

> The note is clearly quite unacceptable and insufficient in the fight against plagiarism. We cannot help but ask whether the Editors seriously believe that those who cannot compose their own sentences are in fact capable of producing genuine research worthy of publishing in General Relativity and Gravitation.

and note the retraction of the Physical Review D article, which they regard as a much more appropriate response

Update: Someone helpfully sent me pdfs of the two GRG articles, marked up to identify the plagiarized passages. Looking at these, I find it hard to understand why any journal would not withdraw such papers if they made the mistake of publishing them.

- Topological defect solutions in the spherically symmetric space-time admitting conformal motion, I.Yilmaz, M. Aygun and S. Aygun. This was gr-qc/0607104, published version Gen.Rel.Grav. 37 (2005) 2093-2104. The arXiv describes it as "having excessive overlap with the following papers also written by the authors or their collaborators: hep-th/0505013 and 0705.2930."
- Magnetized Quark and Strange Quark Matter in the Spherical Symmetric Space-Time Admitting Conformal Motion, C. Aktas and I. Yilmaz. This was arXiv:0705.2930, published version Gen.Rel.Grav. 39 (2007) 849-862. The arXiv describes it as "it plagiarizes astro-ph/0611537, astro-ph/0506256, astro-ph/0203033, astro-ph/0311128, gr-qc/0505144, astro-ph/0611460, and astro-ph/0610840."

Update: The journal Astrophysics and Space Science is retracting four of the plagiarized papers, by putting up errata on-line which appeared today and are dated January 11, 2008, saying:

> After investigation and at the request of the President of the Middle East Technical University (METU), Ankara, Turkey, the Editors of Astrophysics and Space Science have decided to retract this paper due to extensive plagiarism of work by others.

The papers involved are gr-qc/0505079, gr-qc/0602012, gr-qc/0508018, gr-qc/0509022.

Comments

1. **functor**
   January 11, 2008

   The reaction from some folks with secure jobs will be – no one is reading these papers anyway, so in the end the problem takes care of itself.

   The problem comes when one is competing for a job with the young guy with 40 publications in a system where all that matters in job applications is the number of papers published weighted by impact factor. Or when the dean with whom he is plagiarizing is the dean of the school to which one is applying for a job. Then this is a serious matter.

   The other observation is this: folks learn how to behave from their advisors and the other folks in their environment when they are grad students and postdocs. Too many basically decent folks learn this way to think that awful habits are professional and normal. This is why it is important that there be voices exposing what goes on.

2. **functor**
   January 11, 2008

   The GRG editorial is not so bad. An author who copies word for word introductory material from his own article is in no sense a plagiarist (can one plagiarize oneself?). This person has done nothing unethical if the new article, in which the copied introductory material is used, reports substantially different results than did the original article. What's so horrendous about copying one's own definitions? If one quotes one's own theorems, one might very well have good reason to quote them as one wrote them originally, particularly if one wrote the thing well the first time. There is a problem if the author is submitting to different journals articles which are essentially the same, in the sense that the results they report are essentially the same; this is unethical, though it is also not plagiarism – it's also quite common, at least in a mollified form

– we all know researchers who publish four papers on a given topic when one would have been enough – and we know they do this to inflate their paper counts (and their citation counts – sometimes they and their collaborators publish a series of interciting and repetetive papers on the same theme) – and many of them do this without seeing anything wrong with it – rather, they think this is what is their job – publishing papers. This is unethical because it means the originality of the papers submitted is misrepresented and if the paper is published space is taken up by something repetetive, when it could have been used to publish something noval.

If copying is genuinely limited to copying one's own introductory material, I don't see much wrong with it – the problem is that in the cases identified by the ArXiv administrators this was not what happened.

3. **Tansu KUCUKONCU , PhD**
   January 11, 2008

   Nothing happened to 15 plagiarist with 70 plagiarised publication
   announced by arXiv.
   Education writers of some biggest nation-wide journals did not write anything about arXiv plagiarism scandal.
   In spite of this, arXiv scandal became the most popular mass plagiarism scandal which found place in Turkish media history.
   ————————————
   Osman Demircan announced himself as the investigator of 2007 COMU Plagiarism Ring (6 plagiarist, 20+ plagiarised publications) declared by arXiv.
   First day he spoke to journals :
   "These are not plagiarised.
   These are libels by Turkish and foreign scientists who are against Turkey."
   ————————————
   Who is Osman Demircan :
   He is vice president of COMU since more than 8 years who decides who can work in COMU and who must be fired only according to his and his rings' partisanship criteria.
   He is a member of physics dept. which the 6 plagiarists of COMU belong to.
   He gave jobs in COMU to all these 6 plagiarist.
   He is the head of this plagiarism ring, and much more.
   ————————————
   In 2001 and 2002 he claimed me to YOK (Higher Education Council; or the Higher Ideologic Despotic Counsil) and other university and
   wrote that I should be dismissed since I am guilty since I'm a PhD student in that other university.
   He illegally made me dismissed me from PhD education just a few months before completion of it.
   He is one of the main organizers of the underground relations who illegally prevented me to have my PhD diploma for 5 years.

4. **Peter Woit**
   January 11, 2008

   functor,

   The problem with the GRG editorial is that this is not just about authors copying some small amounts of text from the section of another of their articles. Sure, they're nothing especially wrong with that, and I don't think the arXiv would act to withdraw a paper because of that. In the case of one of the GRG papers, the copying was from other people's papers, and in the judgment of the arXiv administrators, was extensive enough to make them withdraw the paper. The judgment of the GRG editors was very different, not to take any action at all, even not to add a note that extensive parts of the paper were plagiarized from others. Lacking the arXiv's software, I'm not able to automatically identify which parts of the GRG paper are plagiarized, but I trust them to not have withdrawn a paper simply because of small amounts of use of other people's language in an introduction.

   One other thing to keep in mind is the author's retraction note for his PRD paper, where he acknowledges that more than half of the text of the paper was plagiarized.

5. **Peter Shor**
   January 11, 2008

   I wonder whether these journal editors are doing nothing because they are lazy, because they are embarassed that their journals accepted these papers, or because they don't see anything wrong with plagiarism. I'm not happy with any of these reasons for inaction.

   This is somewhat off-topic, and I don't want to encourage excessive splitting of results over several papers, but I do want to give a small word of caution. There is a danger in combining all your results into one big paper. If you stick a result into your big paper which is only tangentially related, people may not realize that it is there, and may publish other papers duplicating your result.

   I have come to conclusion that the right way to judge whether you should split your paper is that every paper should have one story to tell (it may be a long and complicated story, or a short and simple one), and anything that doesn't fit into this story should be put into a different paper.

6. **Peter Woit**
   January 11, 2008

   Someone helpfully sent me pdfs of the GRG articles, marked up to identify the plagiarized portions. See the update at the end of this posting for links to these.

7. **ali**
   January 11, 2008

   I think the university involved in this scandal is totally rotten. The president of the university apparently tried to cover up the scandal and had it not leaked to the media, they would not do anything about it. Another issue I am concerned about is the responsibility of the supervisors of the Ph.D. students expelled for plagiarism. Where were these folks while the students were publishing plagiarised papers on their own? I think the guilt lies way higher in the rank in this case and the university tried to cover up the issue not to punish the supervisors and as far as I am aware, no action was taken against them in any way.

8. **Scott Aaronson**
   January 11, 2008

   The next time anyone tries to tell me how important traditional journals are for quality control, and how conferences, the arXiv, etc. just don't provide high enough standards, I will show them the following "cut & paste is OK with us!" letter from JHEP, which I think deserves a special place in academic history.

   Dear Professor Akbulut,

   We have looked to your letter regarding plagiarism.

First of all, in matters of plagiarism it is the injured party that must take action. We are not involved in administering justice.

The authors of the paper in question seem to cite the articles from which they copy sentences or paragraphs. This weakens the case.

It may be that the paper should not have been published and that thereferee erred. Unfortunately weak papers get sometimes published.This may happen when the work is in a rather minor subject or not in anactive and competitive field. If the results are oustanding or contradict well known physics this would not happen.

Therefore we fear that we cannot help you in this case.

Sincerely yours,
JHEP Journal

9. **Jimbo**
January 11, 2008

I used to teach upper-div physics at one of the universities in Oregon. After grading mid-terms using an `instructor's-only' solution manual provided by the publisher of the text, I noticed one student repeatedly gave answers which were verbatim copies of the ones in my manual.
I showed them to the chair, undeniable evidence of cheating by a physics senior, only to be told in so many words "Tut, tut, my boy: work around it "!!!
So these journals not being overly perturbed by plagiarism is ALL in the spirit of the age: Go with the flow…Don't rock the boat….
I chose to rock the boat and failed the student.

10. **wb**
January 12, 2008

As a journal editor I find the letter quoted by Scott Aaronson disturbing in several respects. Authorship and attribution are part of the reward and recognition system of science. Peer-reviewed journals play a prominent role in that system. While it may be the that as editors we are not "instruments of justice," neither should we be the instruments of fraud and misrepresentation. Publication of plagiarized work does harm the journal by lowering its academic and ethical credibility; a journal and its editors does have standing as an injured party.

Rejection of papers that misrepresent authorship and even informing an authors institution of academic misconduct (after appropriate procedures) is part of our ethical responsibility. The inclusion of a citation is not exculpatory when a author represents substantial potions of another's work as his own. That applies whether one plagiarizes others or himself (yes functor, self-plagiarism is recognized as a form of academic misconduct; it may also be a civil tort of copyright infringement). This past year I have rejected 4 papers for self-plagiarism.

I would be surprised if the large majority of editors of scientific journals did not have similar views. To be sure instances of misconduct do get b; hopefully with better plagiarism detection software on the horizon the number of such cases will diminish. Even so, good refereeing is our best line of defense.

11. **DB**
January 12, 2008

Jimbo, wb,

The practice of "not rocking the boat" and sweeping embarrassing incidents under the table is part of the "don't let's wash our dirty linen in public" philosophy that permeates most professions. Physics, to be fair, is probably a lot less culpable in this area than, for example, the medical profession, where lawsuits often fail when professional colleagues close ranks to protect "their own".

That journals would be reticent to act, I can understand, if not condone. But that JHEP would pen such a response as quoted above is astonishing. A massive gaffe. That journal better undertake some damage limitation, and quickly.

12. **functor**
January 12, 2008

Peter,

You're right that an article which plagiarized (necessarily work of other authors) should be rescinded, and that in that sense the GRG editorial is inadequate. Whether this is due to naivete or to pressure from the publisher or to some other motivation, I cannot say.

There are journals that behave properly; there are many more that do not. The best ones are the ones for which the profit motive is not important, often those published by university departments or professional societies. The problematics journals are, no surprise, mostly published by Elsevier, Springer, etc.

What's going on with the commercial journals is simple. There is a cadre of professors who want to publish papers, and want to do so in journals with big impact factors; these journals provide that service, though at a handsome price (Which is, however, born by the researcher's institution's library's budget rather than the researcher). Some of these unscrupulous individuals wind up as editors, and their editorial motivations are anything but publishing the best papers, or seeing to it that articles are refereed in a timely or fair fashion. There's certainly no interest in bringing attention upon themselves of publishers of nonsense with low standards.

Look at the quality of the editing of the English in many of these publications. It is lousy. Any journal publishing in English but unable or unwilling to make the effort to guarantee that the articles which it publishes are written in correct English is likely cutting other corners too. This happens much less in certain journals than in others – it is one indicator of the seriousness of the editors.

Outright plagiarism and dishonest copying of ideas are huge problems. Repetitive publication of the same result is a problem too, but of a different nature; it is stupid, perhaps unethical, but not as fundamentally dishonest. Decent blokes can be deceived into thinking it a reasonable way to go. There are a lot of folks not having success with their research programs, but with a lot invested in trying to, with families to feed and mortgages or rent to pay, who lack the courage to suffer the consequences of their own limitations and engage in questionable but easily rationalized practices; this is humanity. Plagiarists and thieves are a different matter.

The upper tier of US universities has less of this nonsense, but in countries with undeveloped or developing research communities it is more common – there are not enough researchers of quality, and their voices are perhaps not even heard, so that even those within the community of serious (in the professional sense) researchers are able to tell the good from the bad. Also if one learns in an environment in which dishonest practices are the norm, then one learns that those practices are ok. This is why often famous researcher A develops a legion of students who behave like a mafia; they learned more than physics from A.

13. **a.k.**
January 12, 2008

..Tansu, characterizing the whole turkish scientific community as modeling their scientific research on mafia-like structures and habits seems possibly, not that I had deeper knowledges of the context involved, a bit too stereotype. Even if it matches your personal experience, it may suffice to feed well-known stereotypes in the 'western' world, regarding the scientific culture of islamic countries. As it seems the above linked two pdf-files mostly display self-plagiarism (the blue marks?), and some copy&paste of more or less peripheral blah-blah, which could clearly be induced by insufficient mastery of the english language (one could wonder how many US-scientists spoke turkish, by the way). So while multiplying its own contributions by excessive self-plagiarism *is* unethical, at least from my point of view copying motivating verbal formulations in the exact sciences to compensate for insufficient english is not, one should possibly focus on the actual *content* of the papers in question, it is not impossible to plagiarize the essential content of whole books without even using one sentence of the plagiarized work explicitly. It is obvious that, again , the 'little fishes' get all the attention while the 'big ones' remain in shades.

14. **Peter Woit**
    January 12, 2008

    DB,

    I think JHEP already took as much damage control as they intend, by withdrawing the paper. The e-mail quoted was sent long before this became public, at which point they seem to have realized that their response to the e-mail was untenable.

    I suspect that if one contacts any of the rest of the journals involved to ask why they don't withdraw the papers, one will get a similar response. Unless you're a member of the press or one of the plagiarized authors threatening legal action on copyright issues, in which case they might actually do something.

    The GRG response seems more disturbing to me. Ultimately JHEP did the right thing, and changed their initial untenable position, which was in a private e-mail. The GRG editors considered these papers carefully, then despite the obvious serious problems with them, publicly defended them and refused to withdraw them.

15. **Peter Woit**
    January 12, 2008

    a.k.,

    "mostly display self-plagiarism"

    I can't believe we're looking at the same files. In the second one, none of the plagiarism is self-plagiarism, and most of the first half of the paper is nothing but directly plagiarized material from other papers. In the first, there is some self-plagiarism, but also lots of plagiarism from other sources. The blue section (which is the main content of the paper) is described not as word for word plagiarism, but as following closely two other papers which are not cited, one of which is by the same authors, one of which isn't.

16. **a.k.**
    January 12, 2008

    ..I wasn't aware of the exact meaning of the 'blue marks', if the second paper on which this section relies, is not at least associated to the working environment of the authors, it is without any doubt severe (non-self) plagiarism. In the second paper, the first half seemed more or less what I characterized as blah-blah, as long as at least most of this plagiarized material is taken from cited papers, this could still be a 'boundary case'. In any case, the affair also puts a light on isolated scientific contexts, the best way to avoid plagiarism is not by electronic tools but by global non-formal communication and personal networks, which could detect those attempts very early. In this sense, one should avoid putting the stigma of mafia-like structures and dishonesty on whole scientific communities and cultures, this does not help to avoid but to enforce exactly those structures.

17. **Jack Lothian**
    January 13, 2008

    Self plagiarism is persistent and wide spread. I know of a number of well known names in numerous fields who recycle essentially the same paper. The level of self plagiarism varies tremendously from mild variants of previous papers that have been mostly re-written to almost perfect copies. Stopping this is almost hopeless for several reasons. First, submission, acceptance, publication cycles are long & it is easy for multiple copies to be in the system unknown. Second, who is going to cross read all this stuff? Third where do we draw the line here? Fourth, is recycling your own writings & ideas a problem? Lastly in all its manifestations, this problem is huge for all journals.

    I see even severe self-plagiarism as a judgmental call by the journal's referees and editors. It depends on their goals, policies or standards. Low tolerance standards for self-plagiarism may be a negative reflection on the journal but not on the author's morals. I do not see a big issue here.

    Plagiarism of another author is another issue though. A clear line has been crossed.

18. **Jack Lothian**
    January 13, 2008

    It should be noted that legal exclusive rights such as copywrite trademark & patent all clearly state that no one has exclusive rights to either ideas or words. People just are not allowed to duplicate exactly how you use the ideas or words.

19. **Roger**
    January 13, 2008

    I see self-plagiarism as simply scientists "playing the career game". To reduce its incidence job and promotion awarding bodies have to look beyond a paper count. I think this is easier said than done. The culture of "publish (a lot) or die" is everywhere and I can't see any serious moves to tackle it.

    As an experimentalist, I'm lucky that I've never had the pressure to self-plagiarise and produce X papers a year. On a large collaboration it is normal and accepted that an active physicist can only be expected to perform the data analysis for and publish around 1 paper per year. My other papers such as conference and summer school proceedings will undoubtedly look very similar to each other (though never word-for-word identical). However, it would surprise me if anyone could legitimately classify them as examples of unethical self-plagiarism – the remits of each invited paper were often the same.

20. **mo**
    January 13, 2008

    There is one more dimension to this plagiarism scandal: withdrawing papers published online, or even altering online content smacks of rewriting history and compromising the historical record. (I mean JHEP. As is well known the print edition of JHEP ceased with 2000 and from 2001 and on JHEP is published online only.) Once you start on this slippery road with the best of intentions you can end up in hell.

21. **Moshe**
    January 13, 2008

Scott, isn't your comment along the lines of "let's not bother doing something if it cannot be done perfectly"? just imagine a world when you don't have to pass any bar before your publication appears on your CV or in a popular science magazine (OK, bad example, but hopefully you get my point).

22. **milkshake**
January 14, 2008

there was a high-profile scandal like this, in organic chemistry last year.

http://pubs.acs.org/isubscribe/journals/cen/85/i11/html/8511sci1.html

For some years professor Armando Cordova from university of Lund has been developing a reputation of a odious young man who acquires information about unpublished research from competing groups and repackages them as his own "contribution". Eventually he got caught in the act when he tried to rip-off results from another group – and he clearly misunderstood the results that he was trying to pass as his own!

The investigation done by his university concluded that yes, Cordova is the unscrupulous bastard that everybody says he is – and the outcome was to give him a warning not to do it againg in the future – and provide him ethical counceling. The department head would also oversee Cordovas future publication activity. (Meanwhile Cordova tried to re-publish yet another ethically-challenged paper.)

23. **milkshake**
January 14, 2008

I realized Cordova is at Stockholm Uni, not Lund. Sorry for the error.

24. **amused**
January 14, 2008

To Scott Aaronson and anyone else who thinks that journals are ineffective for quality control I issue the following challenge:

(a) Write a deliberately trivial/uninteresting paper on an issue of central importance for some subcommunity of serious researchers (the specific choice of topic is yours) and see if you can get it accepted for publication in a major journal, e.g. one of the Physical Review journals. (Of course, if you actually manage to do this you should tell the editors afterwards that it was just an experiment to test publication standards and that they shouldn't publish the paper.)

or

(b) Same as (a), except the issue addressed in the paper may be anything you like (not necessarily of interest to any serious researchers) and the target journal you have to get it accepted in is Physical Review Letters.

And no, it isn't enough just to find some previously published paper that you consider to be an example of (a) or (b). If the quality control is really as bad as you claim it is then you should easily be able to manufacture such a paper yourself!

Imo the current plagiarism spectacles don't disprove the value of journals; they simply show that when evaluating someones publication record one needs to consider not just the names of the journals and number of publications but also the topics and issues addressed in the papers (whether or not serious people care about them, how centrally important they are in the general scheme of things..). (Of course, for PRL publications the latter consideration is superfluous 😉) I've already elaborated on the reasons for this in tedious detail elsewhere so will try to resist doing it again here.

25. **Peter Woit**
January 14, 2008

The Journal Astrophysics and Space Science has just announced that they are retracting four of the plagiarized papers, noting that this was done "at the request of the President" of the METU. See the update to the posting.

26. **Peter Woit**
January 14, 2008

It's interesting to see here that people from fields outside theoretical physics (e.g. Scott and Peter Shor) are appalled by this story, those from within (e.g. Moshe and amused) seem mainly interested in defending the current refereeing system.

I don't think there's any need to try to submit fraudulent papers to a range of theoretical physics journals to see what happens. The experiment has been done now by Salti et. al, and the results are known. The question is what should be done about the fact that the refereeing system in this field is seriously broken.

To keep amused happy, I'll stipulate that this doesn't apply to PRL, and advise our librarians that, when they cancel all subscriptions to theoretical physics journals on the grounds that they're no longer worth the paper they're not even printed on anymore, PRL should be spared.

27. **Brett**
January 14, 2008

I am a particle theorist who is certainly outraged by this plagiarism, and the reticence of some journals to withdraw the offending papers is both surprising and worrying. However, I can see a reason why theoretical physicists in particular might be less phased by this than researchers in other fields: because new research results in theoretical physics are not communicated via journals.

28. **David Nataf**
January 14, 2008

Do you think it would have been easy to verify that all the material was original, is there software available for this that does this well?

29. **Peter Woit**
January 14, 2008

David,

The arXiv has some software for this, I don't know if it's publicly available. Once you know that there is plagiarism involved, it's fairly easy to find the sources just by picking phrases from the article and using Google. One can argue about whether the referee should have realized that the paper was plagiarized, but once the plagiarism became known, it's remarkable that the journals involved were unwilling to immediately retract the papers (and many have still not been retracted).

30. **Moshe**
January 14, 2008

In talking to people on the subject of journals and peer review, the main difference I see is where people are from, and that includes myself. When I was a postdoc, benefiting from a constant stream of inside information, I too was tempted to think that most people know which papers are correct and worthwhile without having to rely on other people's judgment. I now realize this is not true, and view peer review as service to the community at large. By and large I still think it is done well in my community.

So, I am not convinced that there is a wide-spread problem, certainly not in the hep-th community I am part of. In any event, I am confused about the logic of the argument of those outraged by what they see as lack of quality control: surely getting rid of journals and peer review altogether is only going to make thing worse…

31. **AGeek**
January 14, 2008

Milkshake, that (the Cordova scandal at Stockholm U) is interesting. If you enjoy this kind of digging, see if you can find the (pretty obvious) link to JHEP and JCAP…

32. **Peter Woit**
January 14, 2008

Moshe,

I don't see anyone here arguing for the elimination of all peer review. Rather, there is a very real question about whether many journals actually are doing a legitimate job of it. If they aren't willing or able to put the effort necessary into it, they shouldn't be selling a product that claims to be something it isn't. The attitude towards plagiarism demonstrated by the GRG editors and by whoever at JHEP responded to the initial plagiarism complaint should be disturbing for this reason, even though they don't seem to bother you.

Several people have pointed out to me that the problem of journals publishing plagiarized or incompetent articles is one that people at elite institutions in the West are able to ignore because they can just ignore those articles, and the people making hiring decisions are able to recognize that they aren't serious scientific work. For people at less elite institutions, where hiring decisions are often in the hands of authorities able to only count publications and journal impact factors, the problem caused by the behavior of these journals is very serious. In particular in this case I think it seems that what happened was that this sort of plagiarism worked well for one or more people in terms of advancing their career, encouraging others to join in and do the same thing. Those that weren't willing to work this way had to compete for jobs with those who were. For such people, seeing the refusal of journal editors to do anything about this is profoundly discouraging.

33. **amused**
January 14, 2008

Peter,

Scott has a couple of papers in Phys.Rev.A., looks like he might be a semi-physicist.. Same goes for Peter Shor, who seems to have quite a fondness for PRL..

My impression (and someone please correct me if I'm wrong) is that none of the papers in this scandal can be characterized as addressing issues of central importance to some subcommunity of serious researchers. So Salti and his fellow plagiarists haven't already done the experiment I challenged Scott to perform. For this class of papers my impression is that refereeing standards are generally pretty good, and I expect the odds of Scott being able to complete the challenge are very small. When he fails I hope he will reconsider his view that journals are ineffective for quality control.

Besides that, I see this scandal as being the extreme edge of the well-known phenomenon that lots of rubbish is being published in physics journals. The minor ones are full of it, and some of it seeps through to the major ones. That is of course a serious problem for people working at institutions where they are evaluated simply by counting number of publications. The solution: stop doing it! That might be easier said than done though, since I imagine that at many such institutions the people with power got there through publishing rubbish in junk journals and won't be keen to change a system that worked for them. But simply getting rid of journals isn't going to solve the problem…

At any rate, thanks for sparing PRL 😉

34. **Moshe**
January 14, 2008

Peter, it is a small step between "the journal system is broken" (a statement I disagree with) to "let's not bother with journals, everyone reads the ArXiv anyhow". But maybe this is not what Scott meant…in any event I think we both would like to see a stronger peer review system, any ideas on how to produce one? as you probably know JHEP decided to give a small financial reward to their referees, seems like a good start.

35. **Peter Woit**
January 14, 2008

Moshe,

I'm afraid I see things in a different order than you. It seems to me that "let's not bother with journals, everyone reads the arXiv anyhow" is already the de facto situation. Jacques Distler doesn't seem to have submitted some of his papers in recent years to journals and from what I can tell he's not the only one. Most particle theory papers are sent to JHEP to be published, and Mustafa Salti did a good job of showing what their standards are. The letter from JHEP responding to the fact that they had published a plagiarized paper is quite remarkable. If you are looking at a paper on the arXiv, you could check to see if it had been refereed and published in JHEP, but would that really tell you much? I suspect that people rarely actually do this, partly because of what "amused" describes as "lots of rubbish" being published in physics journals, including some in the major ones like JHEP.

Given that that's where we are, I think the problem is that people won't go to the next step and acknowledge that there even is a problem with the journals, partly since fewer people are paying any attention to them. The obvious answer to low refereeing standards is to raise them. That's could be done, but it's hard to get people to do if supposedly there is no problem.

While the problem of refereeing in this day and age is a complicated one, the question of papers involving significant amounts of plagiarism like the GRG ones isn't. They have no place in scientific journals, and if editors don't see this, it's hard to believe they're going to ever address even tougher problems.

36. **Moshe**
January 14, 2008

Yeah, there is a small minority of people who think journal publication is redundant, and I strongly disagree with them. Not sure about Jacques, never discussed the issue with him, but my impression from his comment was that Scott is leaning that way, and that as usual he may have interesting reasons for his position. Anyhow, looks like it is too late for that, maybe next time…

37. **functor**
January 15, 2008

Peter,

The point you make about what happens outside the elite institutions needs to be reiterated (it may happen inside too – I don't know, I'm not there). In a country such as Spain the use of paper counts and impact factor is institutionalized to the extent that often by law the ranking of candidates for positions or contracts is based on a score in which an impact factor weighted paper count plays a prominent part. This encourages self-replicating publications and has a corrosive effect on notions of what is a good researcher. The goal of every Spanish jefe is to be one of the ISI most-cited researchers, and this goal can be accomplished most easily by the following scheme: jefe and coauthors A and B and former students X, Y, Z, etc… write repetitive, silly papers of short length which are published in mediocre Springer and Elsevier journals with unjustifiably large impact factors and which cite other articles by the same authors (preferably a disjoint subset of said authors, because this avoids the self-citation problem); since no one else but the others in the circle is terribly interested in the properties of the twelve times iterated tangent bundle, it is almost guaranteed that the referee will be a sympathetic collaborator. After some years, researcher A has 100 papers, each with 3 or 4 citations, and he can present himself as a researcher of quality to bureaucrats and persons working in other areas, so he is easily funded and has no trouble ascending to the position of chair of the institute he founds, and in which he employs B, X, Y, Z (and maybe some students of a friend of Z in France).

Usually A does not exactly copy his own papers, but when A is operating in some small rural place rather than Madrid or Paris, he might go a bit further. Secure in the knowledge that absolutely no one is reading his papers, and that absolutely no one will read his papers (he assigns his students to read the papers of his thesis advisor in France, a very famous man, so that they will know how good he is, being the student of a famous man), he begins to copy them wholesale and send them for publication in obscure journals like the Former French Colony Low Energy Physics Journal (fictitious example which somehow has on the editorial board his friend B), and he rationalizes this behavior with the good feelings that come from having lent the prestige of his highly cited first world (well almost) name to a journal published in a developing country.

I suppose most of the readers of this blog can continue the story.

What creates the dynamics whereby this sort of behavior is rewarded and even encouraged is the false profesionalization of scientific inquiry and the weird conflation of frequency of publication with quality.

38. **Arun**
January 15, 2008

In all cases where I've seen "broken systems" it was because people were responding rationally to incentive systems, but the incentive systems were perverse. (Also, most people have the attitude, I didn't make this system, I just live with it.)

Every scientist ought to occasionally step back and review what it is that he/she really wants to accomplish and whether the "system" is helping them get there.

39. **Reynold**
January 15, 2008

One of the plagiarized papers (according to arXiv) is gr-qc/0011027. It was published in PRD and according to Spires it has 1 citation by other authors, 2 in total.

Its clone, gr-qc/0505079, published in Astrophys.Space Sci., has 37 citations, at least 15 of which are by non-Turkish authors.

It's interesting how the clone was so much more successful attracting citations than the original.

40. **JC**
January 15, 2008

Peter, David,

Isn't there some online service that many universities subscribe to, which checks for plagiarism in students' term papers against stuff that is online and/or from "essay mills" (like schoolsucks.com and others)?

Maybe arxiv has a program similar to this. I would imagine the algorithms would be very similar, in systematically comparing long strings of words + characters against one another.

41. **JC**
January 15, 2008

Article about arxiv plagiarism detector program.

http://ptonline.aip.org/journals/doc/PHTOAD-ft/vol_60/iss_3/30_1.shtml

42. **Off-topic noise**
January 15, 2008

I'm surprised by arxiv's stance on plagarism in papers. It may take 10^500 string theory papers to investigate the landscape fully, and most will achieve similar results (failures). In this case, plagarism (repetition) of the text in each paper would seem appropriate, whereas making up different wording for each paper's introductory paragraphs and conclusions is irrelevant because nobody is interested in reading 10^500 variations of the same basic message. The difference between each paper will be the numerical moduli values guessed for the particular Calabi-Yau being ruled out. Why bother making up different wording for each paper? Plagarism is sensible for such situations.

43. **JC**
January 15, 2008

Preprint on the algorithm used for arxiv's plagiarism detector.

http://arxiv.org/abs/cs.DB/0702012

"Plagiarism Detection in arXiv"
by Daria Sorokina, Johannes Gehrke, Simeon Warner, Paul Ginsparg

44. **Roger**
January 15, 2008

Reynold.
It may be that once a paper starts picking up citations (in this case by the Turkish physicists) it is somehow seen as having an impact. Other authors may well then be more willing to cite it as work which has been taken seriously by the community.

45. **a scientist**
January 17, 2008

I found this comment at the Ars Mathematica blog, made by prof. Irfan Acikgoz.

http://www.arsmathematica.net/archives/2007/08/22/plagiarism-at-arxiv/#comment-55199

does anybody have informations about this letter was or wasn't sent to arXiv and if the arXiv admins gave any answer?

thanks

46. **Peter Woit**
January 17, 2008

a scientist,

I assume that letter was sent, but don't know what if any response they got. There's nothing in it likely to have changed the minds of the people at the arXiv who decided to withdraw the paper. Three of the signatories of the letter are the authors of the JHEP paper that I provided a link to marked up to show the plagiarism. That paper is just about completely plagiarized, and their is no way to deny this.

47. **D. Eppstein**
January 18, 2008

Looking at the annotated pdfs, I'm struck by how they've cut together a mishmash of many sources. To someone more knowledgeable in the subject area, do these papers read as coherent arguments or do they seemingly flit from topic to topic in the same way that they cut from one plagiarized paper to another? If the latter, how did they ever get accepted?

48. **Takyon**
January 18, 2008

Reynold wrote that
"It's interesting how the clone was so much more successful attracting citations than the original."

Dear Reynold,
another interesting search would be to look who are cited in these plagiarized papers and also who cites these papers.

It may be possible that this mafia-like organization is more deep then what the community thinks.

49. **a scientist**
January 18, 2008

I am studying the subject of plagiarism as a science and communication sociologist.
The turkish plagiarsim scandal raised some complex questions: no doubt about plagiarsim, if not detected, can raise an "author" visibility and help his carrer. But it's true that if it is detected (and we see how it's easy to do it today), the sanctions are very strong.
So, why a researcher should run this mortal risk? this is the question I ask myself when I try to consider all the variables which affect the case. The first answer that I give to this question is that the search for visibility is a weak reason, if compared to the eventual destiny of the plagiarist. Then, I try to think for a moment how hard to treat are the concepts of originality and relevancy in a scientific communication context.

Acting out of the theoretical physics context it's a bit hard for me to analyze some specific aspects of your scientific communication.
That's why I'd like to ask you (Peter) and all the physicists who will reply, one question, that you may find silly:

I don't know exactly how many paper a year are published in theoretical phys, but from my data I can suppose that the number is expressed in terms of thousands.
My question, therefore, is:

is it reasonably and scientifically possible to produce thousands of papers every year in your field, which content being ORIGINAL and also RELEVANT for the progress of knowledge or are relevancy and originality two complex concepts, hardly translated in communication practices?

This is a very important point for me to understand how much the "publish or perish" pressure does affect the quantity and the quality of scientific literature and how much originality and relevancy are true features of this literature

Thanks in advance

50. **dr**
January 18, 2008

I have checked that the h-value of Salti=10 in WOS!

51. **Peter Woit**
January 18, 2008

a scientist,

Your estimate of several thousand papers/year in theoretical physics is the right order of magnitude. Most such papers now are on the arXiv, so you can get a good count just by looking at their yearly totals in the theoretical categories.

As for relevancy and originality, they're hard to quantify, and this varies by subfield. In a healthy subfield where progress is being made, while there are a lot of unoriginal papers everyone ignores, there are also a lot with something original, even if of a minor sort. Unfortunately, I think that particle theory and general relativity are not very healthy fields at this point, partly because the problems remaining to work on are very hard. When it is extremely hard to come up with something original and relevant, most of the literature ends up being papers that fail to do so. What's remarkable about this scandal is both the number of unoriginal plagiarized papers that got past referees, and the attitude of some of the journals after these were exposed. This situation is much worse than I've seen in other fields or at other times.

52. **Tansu KUCUKONCU , PhD**
January 18, 2008

"a scientist" ,
as I said above, in Turkey plagiarism and lots of other things done by partizans are unwritten policy. It's supported and protected.

Nothing happened to 15 Turkish plagiarists with ~70 plagiarism works announced by arxiv.

2 of them are from METU. Their education paused for a while.
That's all.
Moreover METU did not do this immediately.
Those two were assistants in METU. According to laws they must be fired.
But METU did not do this.
METU's academicians are discussing METU's behaviour between them.

The rest of the plagiarists are under protection of their universities.

——————————————–

Ramazan Aydin : head of the comu plagiarism ring.
president of comu for 8.5 years (till march 2007) who was promoted as president by junta.

Interesting thing he was a faculty of METU physics dept.
even he was president of comu.

Osman Demircan : 2nd head of the comu plagiarism ring.
vice president of comu for 9+ years who was the main partner of Ramazan Aydin.

Interesting thing he was a faculty of METU physics dept.
for a short term in 80's.

————————————

"a scientist"

if you are interested in and if you think that I can help you
in your research on plagiarism as a science and communication sociologist, you can communicate with me.
I have lots of material about Turkish plagiarism.

53. **amused**
January 18, 2008

a scientist,

I suggest you take a look at this Nature article about the scandal. It gives this explanation for why they were able to get away with it for so long, and presumably why they expected to be able to continue to get away with it:

"Many of the papers concern an obscure theory of gravity known as the Møller version of general relativity. Few people would be likely to check such work, allowing the students and professors to build their publication record without fear of being caught, says Ginsparg. "They were following the optimal strategy." "

As for why they did this to begin with, the explanation given by the Turkish prof at METU who caught them was this:

" "They're isolated, their English is bad, and they need to publish," says Sariolu. "So they plagiarize, I guess," he says of the alleged plagiarizers. "

As well as that, it sounds like what they were doing wasn't particularly unusual in the environment they were in.

However, one important thing that isn't at all clear from what I've read about this so far is the nature of the plagiarism. Did Salti & co plagiarise other peoples research results, or did they simply lift introductory and background material from others' papers? The former is much more serious than the latter, and imo the arxiv folks and journals should have stated explicitly which of these was the case when they retracted the papers. The default assumption when someone is accused of plagiarism is that they plagiarised research results, and if Salti & co didn't actually do this then I can understand them feeling hard done by in this situation.

Regarding the GRG editorial, I think it was actually quite reasonable. It was quite brave of the editors to decide not to retract the papers after they had already been retracted by the arxiv, and they gave a clear explanation of their reasons for not doing so. The point is that the actual research results of the papers were original and judged worthy of being published in GRG, and that is the most important thing. Having said that, it certainly doesn't give a good impression about the paper when the authors feel the need to fill up so much of the paper with introductory and background material that has already been written elsewhere. The normal thing to do would be to briefly summarize it and refer to the previous papers for the details. Otherwise it looks like the authors are just sticking it in in an attempt to distract from the fact that their original material in the paper is shallow and uninteresting.

Finally, let me just mention that it's not at all difficult to write lots of physics papers containing shallow and uninteresting results. If the topics and issues addressed are obscure then referees often can't be bothered to put in the time and effort to see how shallow the paper is and are likely to just take the easiest option and recommend publication, especially if the paper is submitted to a minor journal. On the other hand, if such a paper addresses an issue of central importance to a subcommunity of serious researchers then it will usually get found out and rejected.

54. **Peter Woit**
January 19, 2008

amused,

The Salti JHEP and two GRG papers, marked to show those parts of them whose plagiarized sources are known, are linked to from my posting. Note that it is entirely possible that some parts not marked as plagiarized are plagiarized, but the source is not known.

I strongly suggest that you look at these documents. In the Salti JHEP paper, there is very little there that is not direct plagiarism.

I don't think the GRG editorial accurately reflects the extent and nature of the plagiarism in the two papers it discusses, and note the retraction by PRD of one of the author's other papers as being more than one-half direct plagiarism.

Before defending the plaigiarists on the grounds that "what they were doing was not particularly unusual", you might want to think a bit about what it must be like to be an honest Turkish theorist, competing unsuccessfully with not-honest people who have long publications records built on plagiarism. Hearing that the editors of the publications involved won't do anything about this, and physicists like you think it's all right, is really maddening to them

55. **dr**
January 19, 2008

e. vagenas,yu-xiao liu,t. grammenos, i. radinschi,t. harko,w. schleich, r. sharma and a. pradhan had sent some mails to plagiarists.

http://sozluk.sourtimes.org/show.asp?t=fizikte+bilimsel+asirma+skandali

Did someone look at those people's publications?

56. **amused**
January 19, 2008

Peter,

Do you agree or disagree that it is important to distinguish between plagiarisms where the authors lifted research results and where they lifted introductory and background material? I had already looked at the 2nd GRG paper before writing the previous comment, and have just looked at the marked up JHEP paper. In both cases it is not at all obvious to me that the authors have plagiarised any previous research results. Did you or anyone else check this? In the case of the GRG papers it seems that the editors have seriously looked into this and concluded that the plagiarism consisted entirely of introductory and background material. And the authors seem to cite the papers from which they "borrowed", which is a mitigating factor.

"I don't think the GRG editorial accurately reflects the extent and nature of the plagiarism in the two papers it discusses,…"

How is the nature of the plagiarism different from what the editorial describes?

If I was an honest Turkish physicist I wouldn't be losing much sleep about colleagues getting away with lifting introductory and background materials from others' papers, as long as their research results were their own. The thing that would be maddening to me is that the Turkish system is apparently set up to reward those who publish lots of papers containing shallow and uninteresting results, rather than those who have the skill and put in the time and effort to produce interesting results (which takes longer and results in a lower number of publications).

By the way, I have some personal experience with this. At the time i was starting out in research and published my first few papers, I somehow acquired a "fanclub" of researchers in a certain developing nation (not Turkey) — they wrote a number of papers putting rather shallow and uninteresting (and sometimes simply wrong) twists on the stuff I had done, sometimes lifting background material verbatim from my papers (but always with a citation, and never to the extreme extent of Salti & co). Their papers were mostly published in lesser journals, but one got into NPB and another in PLB. I never found this upsetting, in fact i was amused and quite flattered. My main concern was that "serious" people might think I was part of their club and therefore dismiss my work without giving it a fair assessment. If this was happening to me it must surely have been happening to many others as well (there was nothing great about my papers), so it seems that this kind of plagiarism lite is not at all unusual and has probably been going on in many places for a long time.

57. **Peter Woit**
January 19, 2008

amused,

If a published paper contains large amounts of plagiarized material, I don't see the point of going through it line by line and arguing about what is "background" and what is "new". If a student I'm teaching hands in a paper that I see is half plagiarized text from other sources, I'm not going to argue with him about whether or not he has original ideas in the other half. Instead I'd tell him to never do this again, and rewrite the paper, in his own words, making explicitly clear what he has done that is original. If the GRG editors think that there are worthy research ideas in the two papers at issue, they should still have withdrawn the plagiarized versions and insisted that the authors provide a new unplagiarized version that makes clear what is their own original research and what isn't. As published, it is nearly impossible to separate out in those papers what is original and what isn't, certainly impossible for a typical reader who doesn't have access to the marked up pdfs.

The GRG editorial explicitly claims that in the first paper the only plagiarism is self-plagiarism, but if you look at the marked pdf, you will see that this is not true. Also, the way it is written, it does not make clear how extensive the plagiarism in these papers is. It's not an honest evaluation of the plagiarism in the papers, but an attempt to excuse it.

I just don't see why anyone thinks it's a good idea to bend over backwards to defend these papers. One look at the marked-up version should be enough to convince one that these are not things that belong in the scientific literature. Recall that this all broke when someone noticed that Salti, a grad student who, at his oral exam, didn't seem to understand what he was talking about, had 40 papers, many published in respectable journals. The situation is outrageous, and I just don't see why people want to defend the indefensible, other than in a misguided attempt to prop up a collapsed refereeing system.

58. **Tansu KUCUKONCU , PhD**
January 19, 2008

2 of 6 plagiarists of comu ring,
Sezgin Aygun and his wife Melis Aygun,
were my students.
They failed in my course.
They were very unqualified.
I lectured them when they were junior (3rd year ; 2nd semester)
students of comu phys. dept.
Till then more than %50 of their courses were empty.
They were unaware about what most of the courses in their transcripts were about.
This is the general view of comu and lots
of universities in Turkey.
Whole class had no hope about a career related to physics,
and were trying to have the right to be primary school teachers.

They were accepted to msc then phd, and as assistants
due to their political, mystical, and beneficial relations.
Grad education is worser than undergrad education in comu
and in lots of universities in Turkey.
The results are appearent.
You see what is taught and valueable in comu.

An assistant of comu physics dept. caught lots of phys. dept. students when cheating in the exam. What happened : comu punished the assistant, tried to end his job appointment, more than 1 year blocked him to start phd in other universities.

—————————————

amused,

"The thing that would be maddening to me is that the Turkish system is apparently set up to reward those who publish lots of papers containing shallow and uninteresting results, rather than those who have the skill and put in the time and effort to produce interesting results (which takes longer and results in a lower number of publications)."

There is one rule of the Turkish university system :
– there is no rule !
Junta order.
Acadocracy order.
Juristocracy cover.
A partizan without any qualification, without any publication
promoted always.
In most of the Turkish universities a scientist having very high qualifications regarded world-wide has no chance against a
partizan.
As expected there are lots of unqualified partizans.

These all occur in front of students and public.
Students learn that to obey the despots is valuable,
and honor, ethics, knowledge, human are worthless.

A few years ago, as a part of joining EU, Ethics law accepted for
who work for the state.
This law has exceptions : acadocracy, juristocracy, and miitary.

15 plagiarists announced by arxiv are not individual plagiarists or small plagiarist rings.
They are the mirror of Turkish university system.

In the explanations of 15 Turkish plagiarists' which are travelling in the internet, they are saying that the main target of who blame them for plagiarism is all Turkish physicists and all Turkish academia.

—————————————————

A few years ago a president of a university blamed for illegalities and sent to jail first time in Turkish history.
YOK (higher education council, or higer despotic council) president
with presidents of universities went to Ministry of Justice and threated the Minister, went to court and threated the juristocrats.
That time presiden of YOK was a law (constitution, organic law) professor.
YOK president said that
"to blame a president of a university for illegalities is to blame
republic, is to threat republic,
to defend that president is to defend the republic".
What happened :
– juristocrat who blamed that president fired,
all of his rights to work using his law diploma in Turkey were banned.
– juristocrats who sent that president to jail were sent to courts far away from that city.
– when in jail that president was still the president of that university.
– new juristocrats left that president.

————————————————-

Partizan plagiarists' strategy is similar.
Partizans' strategies are always the same.

59. **amused**
January 19, 2008

Peter,

The value of a paper, and the credit the authors get for it in the eyes of their peers, is determined primarily by original research results it contains. So I insist that plagiarism of results or ideas is much more serious than plagiarism of background material (and yes there is a clear distinction between them). Maybe we will just have to agree to disagree about this.

Anyway what can be done about this lifting of background material? In the future these authors will probably do the same but change the wordings by a small but sufficient amount to beat the plagiarism detectors. Somehow I just don't think this is a big deal in the general scheme of things. Physics research isn't about writing eloquent introductions etc, the point is to produce interesting new results.

"Recall that this all broke when someone noticed that Salti, a grad student who, at his oral exam, didn't seem to understand what he was talking about,…"

So what. Some people just don't perform well in those type of situations, get muddled, forget stuff etc. Physicists should be judged first and foremost on the original research results they produce. (It could be that Salti's role on the papers was just cut and paste, but his co-authors should be able to say what his contributions were.)

“I just don’t see why people want to defend the indefensible, other than in a misguided attempt to prop up a collapsed refereeing system.”

I’m not defending anything, just describing the situation as I see it. The system is semi-collapsed for papers addressing obscure issues (since few people can be bothered to properly referee them), but still seems to be working fine for papers addressing central physics issues.

60. **Peter Woit**
January 19, 2008

amused,

Yes, you are defending the indefensible, e.g. your excuses for Salti, and your defense of the idea that there’s nothing wrong with publishing heavily plagiarized papers as long as there’s something original in them somewhere.

It’s somewhat of a separate issue, but I also strongly disagree with your defense of the the current journal system as “working fine for papers addressing central physics issues.” A central physics issue is the testability of string theory, and there’s a flood of anthropic pseudo-science on the topic being published, if not in your darling PRL, then in JHEP, which is arguably the most prominent and respected place to publish HEP papers that are not letters. Even at the best journals, the quality of what is published is always going to depend upon what referees see as the current standards of what is good HEP research, and I fear that’s a monotonically decreasing standard. This standard may have gotten so low that even conventionally completely out of bounds behavior, like plagiarism, is now being defended as a legitimate part of scientific research.

61. **Ali**
January 19, 2008

Hi Peter,
I think (and all senior faculty at top tier instituions would agree with me) that the citations determine the fate of any paper so you can go ahead and produce 200 clone (i.e.plagiarised) papers in chinese physics letters that do not contain any new piece of physics in them. Needless to say, nobody will ever cite them and they will be lost in journal pages so the question is who cares? Nobody judges any scientist with his publication list these days for any academic job. It is your citations(or h-number) that count. If you have 200 garbage papers each of which has 3 citations you have 600 citations altogether and that is nothing. Therefore, your arguments do not hold. Those lengthy publication lists only impress people outside academia. It is a well-known fact that publications coming from third world countries garner very few citations so they do not contain any substance thus the long term impact is mimimal. Coauthor responsibility is a far more serious issue than plagiarism. In a 15 author Nature paper, what did the 9th author do for that paper really? Does his name really deserve to be there for instance?
Also the arguments made by some sources that the plagiarist students did not perform well during their undergrad years and in their qualifiers is also ridiculuous. From my experiences as a scientist, there is very little correlation between people’s performances in their qualifiers and their research performances. They may have been nervous etc. Again the central question is “Did these plagiarists produce substantial amount of new physics that was unknown before”? If the answer is yes, I think the papers are worth staying in the literature. I think this issue is that simple.

62. **Peter Woit**
January 19, 2008

Ali,

Obviously I don’t believe these plagiarized publications are going to get the people involved positions at top-tier research institutions in the US or Europe. The problem is that they have been used to get them positions in Turkey, over other, more competent and more honest physicists.

As for h-indices, from what one commenter says, Salti has a relatively high one of 10. I don’t understand why you and amused think it’s worthwhile to make up excuses for him, while assuming the faculty members who examined him and realized there was a problem were some kind of idiots. Obviously they have examined many, many students before, and this was a case they found unusual.

I happen to strongly disagree with you, amused and the editors of GRG about the whole idea of ignoring huge amounts of plagiarism in papers if you can find original ideas in the paper. There are very good reasons why plagiarism is considered anathema by most academics in all fields, and the amount of defense of plagiarism I’m seeing from theoretical physicists I find shocking. In any case, as far as I can tell the papers under discussion contain minimal, not “substantial” amounts of new physics. If the world’s greatest genius chose to submit a paper to a journal with a completely plagiarized introduction followed by revolutionary new results that won him a Nobel prize, I don’t see why he shouldn’t be told that such a thing could not be published, that he had to rewrite it in his own words.

63. **Ali**
January 19, 2008

Hi Peter,
I am originally from Turkey but my advanced degrees are not from there. Those two students involved in this pagiarism scandal are not going to get positions anywhere in turkish system. I can assure you that much. System may be broken but not that much. I am more concerned about the remaining senior folks whose names appear in the articles. Were they really aware of what had been going on and if so what is the level of their involvement? In what kind of university grad students can publish papers on their own without any supervisor’s approval? I mean this is not a gas station obviously.

64. **Coriolis**
January 19, 2008

It is hard to understand some of the commentators` arguments. Some people have no idea of what they are talking about. Let me just point out a couple of simple facts .

1) Ali thinks that these people will not get jobs. He can check the following link and see that some of these people are professors already (and one of them is a dean!)

http://arxiv.org/new/withdrawals.aug.07.html

2) One claimed that h-index of Salti is 10. Well, spend 2 more seconds to get a number that makes sense. His h-index less than
1 (ONE) if you remove his self-citations. In fact, do a careful count, you will observe that, removing the citations that he got from the other plagiarizers from Turkey (including his collaborators), his h-index becomes about 0.7.
Well, some might argue that even this number is good for a grad student. If so, let us continue our analysis and ask from which papers he got these citations (16 in total after, 7 `withdrawn’ papers are removed). If you look carefully, these citations come from the papers whose authors were heavily cited by these guys.
Just do a count of Xulu`s citations, for example.

Some commentators argue the following
1)OK Try to fish out some novel results in these papers, maybe there is some. (My answer, don`t be funny, 90% of the paper is plagiarized, whose job is it to fish for novelty out of this garbage ?)

2) OK they did get published, so what, they wont get jobs.
(My answer: you know nothing about this issue, don`t waste your time thinking about it)

Anyway, Phys Rev D, JHEP and Astrophysics and Space Science seem to have corrected their terrible mistake by withdrawing these articles. GRG, on the other hand, has lost real credibility.

65. **Coriolis**
January 19, 2008

Correction: My comment about the h-index is wrong. I meant the average citation per paper is less than 1. Sorry about that.

66. **Tansu KUCUKONCU , PhD**
January 19, 2008

Ali,

"Those two students involved in this pagiarism scandal are not going to get positions anywhere in turkish system."

On the contrary to the laws they are stiil assistants in turkish system.

Moreover nothing happened to members of the other plagiarism
rings.
Even, comu's plagiarist dean is still dean.

Turkish system has and had throughout its history lots of examples of this kind of trashes.
Some of them are internationally very famous cases like that of the empire of Turkish university system (a 50+ year-old case), like that of Alemdaroglu, the ex- president of Istanbul Uni..
These trashes include lots of university presidents, deans,
law professors, members of highest courts, deputies.

comu plagiarism rings choose their equippes from partizans
when choosing new grad students, new academicians for comu.
Honest qualified people has no chance against them.
Then leaders of this rings teach and conduct how to plagiarize.
Grad students' role is to obey the orders of their leaders.
This is the answer of your question :
"Were they really aware of what had been going on and if so what is the level of their involvement?"

comu plagiarism rings plagiarize not only journal and conference papers, but msc and phd theses. Even completely, without adding their one own word.
I have enough examples of plagiarized comu msc theses in my archieve.
I informed comu and YOK (higher education/despotism council)
several times about a completely plagiarized msc thesis approved by arxiv's plagiarist Husnu Baysal and Servet Senyucel.
At last they had an symbolic investigation about it and covered the subject. Investigator of that case was Mehmet Emin Ozel,
the head of comu's grad school for natural sciences, who was, for some years in 80's, also a member of METU physics dept.
Regardless of the relation of Mehmet Emin Ozel with comu plagiarism rings, he obeyed what ordered to him.
Otherwise he would have been attacted by others.

If partizan academicians attacks, threatens, tortures someone,
if he apply justice for these,
partizan juristocrats continue to increase the pressure on the contrary to the laws, instead of applying laws.

67. **Ali**
January 19, 2008

Coriolis, I meant the two grad students involved in this issue. Read my comment more carefully. I am aware that the other people were professors in Podunk State University. I raised my concern about their involvement as well. Tansu, I am aware that these students stayed as assistants but I don't think you can do much about it legally in a country like Turkey because TA appointments at state universities are lifetime jobs for some stupid bureucratic reason and I don't think there is any sanction like sacking these students from their appointments for plagiarism that is why they must be still TA's BUT the university keeps the right not to graduate them (I mean ever) so I am curious to see whether they will be awarded a Ph.D. degree after all this mess.

68. **Coriolis**
January 19, 2008

Ali,
These two grad students (and the other ones cited in the arXiv note)
are harboured by the plagiarizing professors in the "Podunk State Universities". No doubt they will get positions there. There is more in this story than meets the eye. One has to follow the names, the affiliations, the advisors and so on. It has been about a year since I heard of this story and no real punishment seems to have been given to the professors. Salti seems to have left to one of these universities to continue his PhD under the supervision of one of the plagiarizing professors (again cited in the arxiv note). Obviously they cant get a job in an established, known university but they seem to bosonize in certain univerties without much effort and with much comfort. Again, to gain a real insight of what is going on, one needs to do a tedious check of names, places etc.

69. **Tansu KUCUKONCU , PhD**
January 19, 2008

Ali,

that's not the case. Assistantships are the easiest to fire position in Turkish uni.s .
Moreover, according to the laws punishment of academicians for plagiarism is to fire, to get back academic titles, to invalidate diplomas, etc.
This is valid for all academic positions and titles.
YOK (higher education/despotic council) applied this in the last year in a very controversial case just for political and partizanship reasons.
But YOK did not apply this in hundreds of very obvious case, and covered those cases.
For now YOK followed its tradition for arxiv plagiarists.

Traditionally nothing happens to partizan plagiarists,
but most of the time who make plagiarists publicly known
are punished worstly.

It seems it's almost impossible for those 2 plagiarist grad students
to have a phd diploma from METU. But it will not be surprising that if they have phd diplomas from their partizan uni.s .

70. **Tansu KUCUKONCU , PhD**
January 19, 2008

Ali ,

I don't remember the rest of 15 plagiarists, but 3 plagiarists of comu ring are grad students. Nothing happened to them. comu will give phd diplomas to them as it did before to lots of plagiarists. Then comu will promote them to assistant professorships.

71. **amused**
January 20, 2008

Peter,

"Yes, you are defending the indefensible, e.g. your excuses for Salti, and your defense of the idea that there's nothing wrong with publishing heavily plagiarized papers as long as there's something original in them somewhere."

Please stop misrepresenting what I write. Re. Salti I just made the point that physicists should be judged primarily on the results of their research. Is that controversial? Why do you characterize it as an "excuse"? (Btw, this isn't implying that the results of Salti & co's research are any good — I suspect they probably aren't.)
About papers containing plagiarism, I never said that it is ok "as long as there's something original in them somewhere." If the paper plagiarises some research result or idea then that is a very serious offence regardless of whether or not the paper contains other original material. On the other hand, plagiarism of background material, while still wrong, is much less serious imo.

I try to be careful not to misrepresent what you write, can you do the same for me?

"I also strongly disagree with your defense of the the current journal system as "working fine for papers addressing central physics issues." "

Well that will teach me not to sacrifice precision for brevity in discussions with you. In previous comments I repeatedly spelt this out more precisely as "working fine for papers addressing issues of central importance to a subcommunity of serious researchers". Interesting that you ignored that but then seized on it on the one ocassion when I tried to be more brief. (As for the anthropic string landscape stuff, my attitude is that if smart people want to try that then good luck to them, but I won't believe that they can get anything significant out of it until I see them publishing in PRL 😉)

As for whether JHEP is "arguably the most prominent and respected place to publish HEP papers that are not letters", I think that depends. People working on strings, branes and other more speculative stuff prefer JHEP, while it seems that those working on topics more directly connected to "real physics" prefer PRD these days. For example, PRD is generally the first choice journal in lattice gauge theory (except for some continental Europeans who can't stand the thought of publishing in an American journal).

I disagree with you about there being a possible correlation between the occurance of plagiarisms like in the present scandal and the appearance of "anthropic pseudo-science" in the literature. My own experience of plagiarism lite which I mentioned in a previous comment was from 10 years ago, well before the anthropic landscape stuff got going. I expect that it has been going on for a long time (although Salti & co seem to have taken it to new heights).

72. **amused**
January 20, 2008

Coriolis,

"Some commentators argue the following
1)OK Try to fish out some novel results in these papers, maybe there is some. (My answer, don`t be funny, 90% of the paper is plagiarized, whose job is it to fish for novelty out of this garbage ?)"

In case it is me you were referring to, that is not what I was actually arguing. My thinking on this is as follows: If the referees of these papers correctly assessed the results and found them deserving of publication then the most important issue here is whether they were published on a fraudulent basis, i.e. whether the results, or some of them, were plagiarised. So I simply ask if anyone can point to any result in any of these papers which was presented as new but which was in fact plagiarised. Is that unreasonable?
Answering this question doesn't mean "try to fish out some novel results in these papers", it means find what the authors say are their new results and check if they really are new.
(Whether the results are interesting and really deserving of publication is a seperate issue. Of course, it is far from certain that the referees actually did assess the results of these papers correctly and competently. I wouldn't be at all surprised if they have failed in this.)

73. **collaps**
January 20, 2008

Notice! arXiv:0706.1294v1 "Reply on Brane-World Black Holes and Energy-Momentum Vector" by Gamal G.L. Nashed:
"We show that the energy distribution of the brane-world black holes given by Salti et al. in the context of teleparallel theory is not right. We give the correct formula of energy of those black holes"

I hope that somebody else would like to check the results in all papers containing plagiarism.

74. **Coriolis**
January 20, 2008

amused,

The issue is extremely simple. Let me go through it with an example. Consider the JHEP paper. It was submitted to the journal, the referee or the referees accepted it and it got published. (At this point, we have no idea yet of how good a job the refs did.) Then, as the story unfolds a little, JHEP defends the publication of this paper by saying that " OK bad papers sometimes get published." and when the story really unfolds JHEP apologizes for publishing the paper and withdraws citing 'plagiarism". That means for the first time around the refs did a terrible job, the second time around JHEP spent more time on the paper and understood what went wrong. Do you think it is

easy for JHEP to be humiliated like that ? When JHEP publishes a paper, you think that it has some novel results because the refs. must have observed that, but when the same JHEP withdraws the paper
you are stuck to their first, wrong opinion. Why is that ? (you don`t need to answer .)
This much said, let me be more concrete with you.

I spent a couple of hours looking on that JHEP paper trying to see what novelty it might have. Well that depends by what you understand from novelty: the parts of the paper that might potentially have some novelty are obtainable from another paper in just a couple of minutes. I won`t go into detail, since you can actually do it on your own : just find the relevant uncolored equations and compare them with equations of a relevant paper they actually cite. Well, I told you, you need to spend a little time.

This is a really simple case for those who understand what plagiarism means. People who "sell" what they have created
cannot hide what they have created in a pile of what they have stolen. A reader of a paper assumes that all the sentences and ideas belong to the author unless proper citation and quotation is made. For example, even in this simple comment, what I have written above belongs to me, as you correctly assumed while reading. Otherwise an honest dialogue is not possible.

A guy stills a car and changes its mirrors, now is that his car ?
(Well in fact, in this case, he steals the mirror from another car :)
Can he sell the car ? If he manages to sell it, and gets caught, can he argue that he actually wanted to sell the mirror and the rest of the car was `attached' to the car so that the mirror sells good ? 🙂

I rest my case.

75. **Marty Tysanner**
January 20, 2008

"amused," Ali, and others with similar viewpoints about different levels of plagiarism,

Defending some kinds of plagiarism as acceptable as long as the primary content of the paper is original can run into problems and inconsistencies. For example:

1. *The introductory material sets the tone for an article, and is an expression of its authors.*

One of the broader purposes of writing papers is to teach a wider community about one's findings, and thereby advance human knowledge. There is more to writing than just staking a claim of priority (in industry, patents serve the role of claiming priority). To someone who takes pride in their ability to teach, clear yet concise writing can be an end in itself, and can require creativity and insight into the minds of the readers. Introductory material can take significant effort to craft, and to plagiarize it is an affront to its author, not at all unlike a novelist copying passages from another ("the plot is different, so what's the big deal?"), or an artist copying the style of another ("the subject of the painting is different, so who cares?").

This issue is independent of the primary research findings in a paper. If introductory material is well written, a reader is more likely to continue reading the paper and make the effort to understand its primary purpose. They have become engaged. To plagiarize another author can turn what should remain a unique expression into something generic.

2. *Writing introductory material requires a knowledge of the larger context of the research.*

It can take significant effort to understand the broader context of a research finding. It takes a lot of reading to understand the existing literature on a subject, and it takes an organized mind to see how a new paper will fit in. The introduction presents this context to a reader: if it is well done and reasonably complete, the reader will probably attach more credibility to the rest of the article. That is because, through the introduction, the author has already demonstrated a certain mastery of the subject and has anticipated some of the reader's possible concerns. For one author to cheaply steal this credibility from another is not just unethical — it can dishonestly lead readers of a paper to believe its author is more knowledgeable about the validity and meaning of the "key findings" of the paper than he/she really is. In effect, successfully copying introductory material dishonestly insulates the author from an important reality check that knowledgeable readers could otherwise perform.

3. *By plagiarizing introductory material, an author has unambiguously demonstrated that he/she is willing to steal the work of others. Having proven this, it is merely a question of where the author will stop.*

It is very much like lying. A person who lies to avoid personal embarrassment has demonstrated that he is willing to directly deceive others for personal benefit. Now it is just a question of where the person will draw the line: he could limit himself to "little" lies, or the lying could be much more systematic and deep. If an author plagiarizes "something little, like introductory material" there is no reason to trust that the author hasn't gone further and stolen other, more important ideas (possibly unpublished) as well. That makes defending "a little plagiarism" tricky and possibly naive, unless possibly one personally knows the author and his/her ethics. (Still, it is hard to believe an ethical person would resort to plagiarism at all, although allowances for very limited self-plagiarism could probably be made.)

That explains why I disagree with this comment by "amused" :

> If the paper plagiarises some research result or idea then that is a very serious offence regardless of whether or not the paper contains other original material.
> On the other hand, plagiarism of background material, while still wrong, is much less serious imo.

Once an author has conclusively demonstrated a willingness to plagiarize, why should anyone assume that the same author hasn't also plagiarized research results or ideas to some degree? If such an assumption is unsafe, why make a distinction between plagiarism of support material and plagiarism of the main idea?

In overlooking or defending *any* plagiarism of the work of others, one gives up the ethical high ground and reduces the issue to pragmatism and power politics. There should be *some* segments of society which strive to keep noble human pursuits noble, which consider individual intellectual and artistic contributions to be valuable ends in themselves. The arts and the academy are two such segments.

Of course, there can be, and often are, substantial differences between what "should be" and what "is." But those are failures of individuals or groups, not more generally of the segments of society which honor noble pursuits for their own sake.

76. **Jonathan Vos Post**
January 20, 2008

Re: "Once an author has conclusively demonstrated a willingness to plagiarize, why should anyone assume that the same author hasn't also plagiarized research results or ideas to some degree?" — the underlying legal concept has been quite clear for ~ 2 x 10^3 years.

To cite from wikipedia
[List of Latin phrases (F–O)]:

falsus in uno, falsus in omnibus

"false in one thing, false in everything"

"A Roman legal principle indicating that a witness who willfully falsifies one matter is not credible on any matter. The underlying motive for attorneys to impeach opposing witnesses in court: the principle discredits the rest of their testimony if it is without corroboration."

Coincidently, I just used this in a sonnet… but that's rather a different kind of literature. How authors of prose, drama, and poetry use citations and react to their abusers, is off-topic.

77. **amused**
January 20, 2008

Coriolis,

I agree that the JHEP editors handled this very badly. They should have checked out the paper properly when the first accusation was made, decided what to do about it, and then stuck to their decision. I guess they were just choosing what they saw as the path of least humiliation by retracting the paper after it was retracted on the arxiv. They come out of this looking like spineless wimps.
I can't help admiring the GRG editors though. It was a courageous decision of them not to retract the papers, considering the pressure they must have felt after the papers were retracted on the arxiv and the knee-jerk outrage they must have known would be directed at them.

No I'm not "stuck to the first decision" of JHEP — they never gave any proper explanation of how they arrived at it, or any indication that they had seriously looked into the situation (unlike the GRG editors). I simply asked the question whether the results of the paper (which provided the basis for publishing it) were plagiarised or not.

I'm glad you took the time to look into the JHEP paper. Your findings confirm what I suspected, that the "new results" there are shallow in the extreme. Considering how much of that paper was lifted from elsewhere, it would have been very surprising if the authors had managed to do anything nontrivial in the little space left over. (Not impossible though.) This is a striking example of how poor refereeing standards can be for the "more obscure" papers.

In response to the rest of what you wrote: When I look at a research paper on a topic of interest to me the only thing I really care about is whether it presents interesting new results. The introduction, background material etc that the results are embedded in don't matter much at all. They are like wrapping paper. (Attractive wrapping paper is always nice, but it's what's inside that matters.) Is this a unique viewpoint? (I doubt it.)
Stealing is always wrong, but if someone steals wrapping paper to wrap a potentially valuable object should that be regarded as just as bad as stealing the object itself?

78. **themanwithaplan**
January 20, 2008

I agree, entirely, with "amused". The point is a very simple one: papers with only intros etc. plaigarized are *orders* of magnitude less harmful than the ones where contents themselves are plaigarized — since the *only* contribution of a research paper is the new content, not the eloquent intros.

No one is defending any of the above two types. However, I couldnt care less about the first type of plaigarism, and I suspect that's true of most of the researchers in any area. So yeah, one can legitamately complain about the first type of plaigarism, but that's kinda like complaining that non-english researchers have bad grammer in their papers. Its a problem, but an insignificant one.

79. **Peter Woit**
January 20, 2008

amused,

Apologies for any misrepresentation of what you have to say, but I continue to find appalling and depressing your defense of plagiarism and characterization of GRG's decision not to withdraw plagiarized papers as "courageous". That the words appearing under someone's name must be their own and not stolen from someone else is a principle of scholarship that I never thought I'd see anyone question. Several people have repeatedly explained here the obvious reasons why this is important, I'm not going to repeat them. Students get regularly kicked out of school and faculty members fired for this kind of behavior. The theoretical physics community will bring great discredit upon itself if it becomes known that many of its members, including journal editors, have quite different ideas about this than almost everyone else in academia.

80. **amused**
January 20, 2008

Coriolis,

From what you wrote it sounds like the "new results" in the JHEP paper might be effectively plagiarised (if they are just simple rewrites of equations in a previous paper). I will follow your example and check this for myself.

Marty,

I disagree with your analogies. E.g., the value of a novel is often determined at least as much by the quality of the writing as by the plot. This is not at all the case for research articles.

About your second point, you are right that when authors plagiarise introductory and background material it can result in readers being misled about their level of knowledge. This can trick readers into investing time in the paper when it isn't worth it. It can also trick referees into thinking that the authors know what they are talking about, and unfortunately that is sometimes all it takes for them to recommend publication (especially when the paper addresses some obscure issue that the referee doesn't care about). So this is a nontrivial problem. But imo it is nowhere near as serious as plagiarism of actual research results.

In reply to your 3rd point, I'll just mention that authors who do this kind of thing will definitely pay for it with their credibility (assuming they have any to begin with). If I heard about anyone in my field doing this they would for sure go down in my estimation; I'd become suspicious about the quality of their actual research and less likely to invest time reading their papers. Well, if I already knew their work was good i'd still follow it of course, but I've never heard of any good researcher who liked to lift large chunks of background material from others papers. It always seems to be the crappy ones who do this.

81. **Coriolis**
January 20, 2008

amused

You declared JHEP editors spineless wimps and stopped short of handing a gold medal for GRG editors. OK How about PRD editors ?
PRD retracted a paper written by Yilmaz, who is the co-author of the two papers that appeared in GRG. Below is the note in PRD in case you cannot reach. So what is your opinion about PRD editors,
and the Author (because he seems to accept his guilt) ?

a) spinless wipms ?
b) bravehearts ?

Retraction: Domain wall solutions in the nonstatic and stationary Go¨del universes with a cosmological constant
[Phys. Rev. D 71, 103503 (2005)]
Ihsan Yilmaz
(Received 8 November 2007; published 7 January 2008)
DOI: 10.1103/PhysRevD.77.029901 PACS numbers: 98.80.Cq, 99.10.Ln
The author withdraws this article from publication because it copies text, totaling more than half of the article, from the articles listed below. The author apologizes to the authors of these papers and to the publishers whose copyright was violated.
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PHYSICAL REVIEW D 77, 029901(E) (2008)
1550-

82. **amused**
January 20, 2008

Coriolis,

I don't think there is any one obviously right approach for editors to take in this situation. The PRD editors decided that the amount of copied text was intolerable to them, and asked the author to withdraw the paper on that ground, which he agreed to do. The actions of the editors and author here are all perfectly reasonable and understandable.
On the other hand, the GRG editors decided that they would let the text copying pass in this instance (but with a warning not to do it again in future). In reaching this decision they took into consideration that (i) the copied text didn't include what was supposed to be the new results of the paper, and (ii) the author had cited all the works that he copied from. I think that is also a perfectly defensible decision (and a brave one).

If I was a journal editor in this situation I would probably take a similar approach to the PRD one. But that doesn't mean I think the GRG editors were "wrong". Like i said, there isn't an obviously right approach here.

83. **Tumbledried**
January 21, 2008

Pardon me to interject, but I thought I should mention that, provided that the material is cited appropriately, it does not seem at all wrong to me for the author of a paper to provide large amounts of background material in, say, a large paper such as a dissertation – since this helps clarify and motivate the component of their submission that is original. Particularly in highly technical areas, such as theoretical physics and mathematics, it is common for there not to be too much leeway in the rewriting of proofs of well known theorems/lemmas pertaining to a particular area. Surely, provided the author clearly mentions where these background results are coming from, it is not a crime to include them?

84. **amused**
January 21, 2008

An interesting thing abut all this is that it shows the dilemmas that arise when the desire to maximally facilitate progress in research comes into conflict with the desire to maintain scholarship standards re. things like copying of text. To highlight this even more, here's a remarkable story I heard a few months ago from a scientist from a certain foreign developing country (while we were in a room together waiting to be interviewed by our funding agency.) I swear that the following is a true recount of what I was told.

A scientist from that guy's home country (not a physicist, it was a chemist if I remember rightly) had done some work he thought was great and wanted to get it published in Science. But he knew that to have a chance the paper would have to be written in a "certain style". Having no experience of this, and limited English skills, he didn't think he could manage it. So he contacted a scientist he knew in USA who had published previously in Science and offered to pay that guy to write the paper for him. The US guy agreed, and the fee was US$8000. The paper was written, and after some back-and-forth with referees it was eventually accepted in Science. (The name of the US guy who wrote the paper, but who hadn't contributed to the research, was not on the paper.)
As a result of this publication, grant money flowed to the lab of the developing country scientist. His $8000 outlay proved to be a very good investment.

Well, this should be a fun one for the moralists out there. Should the scientists involved in this be condemned for unethical behaviour? The words appearing under the name of the developing country scientist on this paper are certainly not his own! On the other hand, wasn't this all to the benefit of progress in science? The results of the developing country scientist's research, which presumably formed the basis for accepting the paper, have now been recognised and disseminated in a way that wouldn't have been possible if he had written the paper himself.
The approximate parallels with the Turkish text-copiers are obvious. (If we are going to be moralistic and focus on questions of principle then it isn't relevant that the Turkish guys' work was of more dubious quality.)

85. **Peter Woit**
January 21, 2008

Tumbledried and amused,

You're both still engaged in a remarkable exercise of trying to defend something indefensible, now by positing quite different circumstances than what actually is at issue.

Tumbledried: these are not "large papers" such as dissertations, nor do they need lots of background material, of a sort that is always going to be roughly similar. They are short papers, large parts of which, not only in the introduction, are cut and pasted from other papers, ones which are sometimes credited, often not. In no case is the material put in quotes, and the reference to the paper the material was stolen from, if it is there, is not associated with the stolen text. This is a case of direct plagiarism on a large scale, not a case of someone using roughly similar background material, properly credited.

amused: I don't see any problem with people getting others to improve the English in their papers, it's actually a good idea for people whose English is not very good to do this. If the help on a paper went beyond improving the English, and someone hired someone else to do scientific research which they then published under their own name, that's a problem, but a different one that has nothing to do with plagiarism.

Sorry, but I absolutely don't understand the reaction of many commenters here of trying to defend plagiarism, again I find it really disturbing and depressing. We're talking about behavior that would get pretty much anyone at my institution fired if they engaged in it, for good reason. Any students or young researchers reading this blog are going to get the idea that a large fraction of the theoretical physics community just doesn't see anything very wrong with plaigiarism, and is willing to even go far out of its way to defend it. This is potentially a huge problem, as now any plagiarist who gets in trouble can point to the discussion here to show that they were not violating the academic norms of the physics community. Already, I'm sure this will be of huge help to the Turkish plagiarists, who can point to your support as they try and portray themselves as serious scholars wronged by the arXiv.

86. **puzzled**
January 21, 2008

This reminds me of a discussion where one side says that shoplifting must not be tolerated, and the other argues that it is not such a big deal and there are worse crimes. Both are right, I think.

As for what younger (and older) scientists should be doing the answer I think is pretty simple. Every time you use someones help — acknowledge it. If some background material is borrowed, or an idea is used–say where you learned it. If someone corrected your English or style, you should thank that person in the paper. If you made a mistake, correct it either as an erratum, or in a new arxiv version, or in a later paper.

What is here to argue about?

87. **amused**
January 21, 2008

"I don't see any problem with people getting others to improve the English in their papers, it's actually a good idea for people whose English is not very good to do this"

That has absolutely nothing to do with what I wrote.

"and someone hired someone else to do scientific research which they then published under their own name,…"

That has absolutely nothing to do with it either.

Let me try the kiddies summary of what happened:

Developing country scientist: Dude, here are my research results, now go write them up as a paper for Science under my name. I'll pay you $8000.
U.S. scientist: Ok, sure.

Your description of me as "defending plagiarism" is completely ridiculous.

"…huge help to the Turkish plagiarists, who can point to your support as they try and portray themselves as serious scholars wronged by the arXiv"

If they bother to actually read what I wrote they will see that lifting large chunks of background material from others' papers will lead to them not being taken seriously. In case it wasn't already clear, let me spell it out again: The default assumption about physicists who do this is that they must be crappy researchers. No one has ever heard of good researchers doing this, only crappy ones.

88. **Peter Woit**
January 21, 2008

puzzled,

Obviously your list of how people should behave is accurate, but it's about different topics. The question here is what journal editors should do faced evidence that a paper they have published is heavily plagiarized, very specifically the papers for which marked-up copies are linked here to show (part of) the extent of the plagiarism. Historically, the conventional view in academia has been that such a paper should be retracted, and the arXiv took action according to that standard. Now it seems that many physicists disagree with this standard, to the extent of characterizing as "courageous" those editors who refuse to follow it. This seems to me a rather remarkable and not at all desirable change in standards of scholarship.

89. **Peter Woit**
January 21, 2008

amused,

Let me try the kiddie summary of what I wrote:

Your hypothetical has nothing at all to do with what is at issue here, bringing it up is a smokescreen to avoid defending your indefensible claim that it is "courageous" behavior by editors to refuse to withdraw heavily plagiarized papers that they have published in their journal. And yes, you are defending plagiarism, not as good science, but as something not outside of normal academic practice, just an indication, like many other possible ones, that the authors are lousy scientists.

90. **Peter Woit**
January 21, 2008

puzzled,

To clarify: the problem here is not that some people are pointing out that there are worse crimes than shoplifting, the question is, what do you do about this kind of shoplifting? The attitude of the GRG editors, and it seems of quite a few other physicists, is that it is not even necessary to make the shoplifter give up what he has stolen. The crime is so inconsequential that it's fine with them if the thief keeps the stolen goods.

91. **Ali**
January 21, 2008

Peter, like I pointed out before, it is the meat in these papers that matters (i.e. scientific substance) not the introduction. If there was no substance(and there seems to be none as far as I gather. I am not an HEP person), these papers will not receive any citation and they will be lost in journal pages so the central question is whether that was original research or not. Tansu, you are complaining that these students wil lget faculty positions at Podunk State University. So what? Who cares? Why do you feel so bitter about it?

92. **puzzled**
January 21, 2008

Peter Woit,

I think there is no universal answer to "what journal editors should do faced evidence that a paper they have published is heavily plagiarize". It all depends on the ethical standards in the relevant subfield of science, of which I have no inside knowledge. You do, so let me ask you this. Suppose that instead of "cuting and pasting" background material, the authors would change the wording slightly in each and every sentence. Would this be okay?

My guess is that many people borrow background material from other papers and the decision on providing references depends on whether the background is already "common knowledge". If yes, many people do not bother to cite the place where they first learned it.

93. **Peter Woit**
January 21, 2008

Ali,

The plagiarism here is not just in the introduction. Look at the marked-up versions.

I just don't agree that the central question is whether there are significant original research results here (although there don't appear to be, the question of why journals are publishing such low quality stuff is a separate one). I'm repeating myself, but, again: there is no justification for the journals to not do what the arXiv did: retract the papers since they are heavily plagiarized. If the authors have original results, they are welcome to resubmit them to a journal in unplagiarized form.

puzzled,

I have never heard of any subfield of science (or scholarship in general) where the ethical standard is that publishing heavily plagiarized material is considered acceptable.

As for your hypothetical question, I just don't think it's relevant. Sure, there's a continuum of degrees of plagiarism with plenty of gray areas. Although I appear to be a "moralist", I'm not interested in spending my time debating hypothetical situations, without any specifics. The plagiarism at issue here is of a specific, documented variety, and the question is whether this, specific, documented plagiarism is such that the papers should be withdrawn. The arXiv, JHEP, PRD and Astrophysics and Space Science say yes, many journals seem to be ignoring the issue, GRG says no, and amused feels that this is courageous of them.

94. **amused**
January 21, 2008

Peter,

Considering all your previous pontificating about "tone" in blog discussions I find it absolutely hilarious that you accuse me of lying about that story ("your hypothetical"). So this is what happens when someone tries to have a good faith debate with you and persists in having a viewpoint different from yours..

In a previous comment you wrote:

"That the words appearing under someone's name must be their own and not stolen from someone else is a principle of scholarship that I never thought I'd see anyone question. "

Would you find it less distressing if the words were bought rather then stolen? Assuming not, then here's a hypothetical for you:

Imagine that the Science editors find out about what happened in the case I mentioned. What should they do? Retract the paper? If the editors decided not to retract it, and justified their decision by saying that they were prioritizing progress in science (as represented by the genuine results of the paper) over retribution for authors whose words weren't their own (while warning that this kind of thing wouldn't be tolerated in future), would you slam them for that? I wouldn't. And if they made their decision after a preprint version of the paper had already been withdrawn by the administrators of a prominent preprint server, and knowing that they were going to have a lot of knee-jerk outrage directed at them, I would go as far as to describe their decision as courageous.
Same for the GRG editors decision. The quality of the papers is very different but the principle is the same.
(Now you can hopefully see that my mentioning of that story wasn't a "smokescreen".)

95. **Peter Woit**
January 21, 2008

amused,

I did not in any sense accuse you of lying (by "hypothetical" I in no way meant to imply any questioning of the veracity of your story), I did accuse you of evading the issue, which you continue to do. As for tone, stop it with the "kiddie version" shit, I'm not a kiddie.

You persist in wanting to discuss something completely different than what is at issue here: the GRG decision to not withdraw two specific heavily plagiarized papers. Besides not being a kiddie, I'm not an idiot, and of course am aware that authors get help with their writing of various sorts and the usual academic standard that people's words should be their own takes this into account. In such cases it would be a good idea if they acknowledge this in the paper. In some cases there would be real ethical issues raised by this, and when considering them, the question of how unacknowledged help affected scientific results would be a relevant one.

But, again, this is not what the GRG editors were deciding or what I was criticizing. I have been assured that they had those marked-up pdfs in their possession when they made their decision. If you want to discuss the specific cases at issue here, I'll be glad to. If you want to keep on discussing other, different, cases, I'll keep pointing out that you're evading the issue and doing so to defend the indefensible.

96. **Jonathan Vos Post**
January 21, 2008

A scientist owns only two things:

(1) Reputation;

(2) Intellectual Property.

He who steals my Intellectual Property — whether introductory text or results or equations — is an extential threat to me. The thief indirectly reduces my reputation (by dilution) and directly reduces my Intellectual Property.

Of course this is the equivalent of a capital crime — like falsification of laboratory data. Of course the thief loses his/her job, once the theft is proved under Due Process.

Cf. "bubble reputation" [Shakespeare, As You Like It, II.7]

If a nation condones plagiarism on a wholesale scale, it is akin to a Cause of War.

None of this is exaggeration. I am just (for now) talking about the Scientific Literature, not the parallel world of novel, story, screenplay, and song.

Anyone who does not understand this is more criminal than scientist, more fool than ignoramus. Peter Woit is entirely right to insist on clarity at the basis of the matter. Those who "praise with faint damn" are accessories after the act.

This is not about alternatives to Intellectual Property under the law, as in the Open Source Software movement. Scientific Literature is parallel, the goal being unfettered distribution of scientific results. The mixture of law and academic protocol and publishing protocol is a jumble of 2nd order effects.

The axioms of plagiarism are unambiguous.

97. **metin**
January 21, 2008

Ali says: "I am originally from Turkey but my advanced degrees are not from there. Those two students involved in this pagiarism scandal are not going to get positions anywhere in turkish system. I can assure you that much."

I, too, am from Turkey, and I don't see how you can be so confident about this. I know of previous cases where people with documented cases of plagiarism have not only obtained professorships, but have been appointed to academic positions with considerable power.

This is an extremely important issue for the Turkish academia, and the professors who have been fighting against plagiarism have been doing an extremely valuable service to the community. I don't think slighting their efforts by uninformed comments is appropriate.

Turkish academia does not consist solely of a handful of "elite" institutions. There are honest, talented people fighting for positions in what you call "Podunk" state universities, and they have to compete against the plagiarists. There are cases where the plagiarists are well-connected politically, and honest people have been threatened to be fired after demonstrating the plagiarism of others.

I am not sure whether you think of these problems at "Podunk" etc. as non-issues, but they are very real for people honestly trying to live the under-paid life of an academician in Turkey. They are real for the students at those universities, too. And believe me, they are real for the quality of the education system in Turkey.

98. **Coriolis**
January 21, 2008

Peter,

It seems to me some people writing to your blog have not looked at these papers. After spending just 1/2 an hour on these papers would remove any doubt in anybody who has written any kind of paper (be it scientific or non-scientific) or who knows anything about plagiarism.

Some people defend the plagiarizers more than plagiarizers defend themselves: the guy admits that he had plagiarized, what else do you want ? You want to catch him while he is actually doing the plagiarizing ? 🙂 (And perhaps broadcast it on Cops .)

About the action of Gen. Relativ. and Grav. Editors, they have lowered their standards so much that I might actually try publishing
in GRG simply plain garbage, just for the heck of it. I would like to write a paper in which I simply collect every sentence, every equation from different papers and I will make sure that my own contribution is absolutely zero. I am sure it will get published. It wont be worse than those two papers.

99. **amused**
January 21, 2008

Peter,

Well then, I'm a bit mystified by what you meant with the "hypothetical". Nevermind. As for my "kiddie version" line, the reasons for the frustration that gave rise to it will be obvious to anyone who goes back and reads that comment.

"I'm […] of course am aware that authors get help with their writing of various sorts and the usual academic standard that people's words should be their own takes this into account."

How many times do I have to repeat: that is not what happened in that story. It has absolutely nothing to do with it. The only difference between that case and the Turkish cases is that the words were bought rather than stolen. Why do you continue trying to insinuate something different?

So you don't think my previous comment addressed the GRG editors decision, whether it is justifiable or not? Well then I really am wasting my time here.

One reason I have so little sympathy with you on this is that the outrage could be so much better spent on other things. Those young Turkish grad students, are they really just intrinsically bad characters? Is academic physics really such a lucrative and prestigeous career in Turkey that they would choose it without actually caring about the subject? How did they end up in this situation? Those wonderful METU faculty members who caught the plagiarists, where were they while Salti & co were putting out 1-2 papers a month over 2 years? (They didn't notice?) What kind of environment and values were they and their colleagues propagating? Perhaps you can consider directing some outrage at them Peter, if you have any left over after you are finished with the journal editors.

In the immortal words of Danny Lundsford: I'm done here.

100. **Peter Woit**
January 21, 2008

amused,

By “hypothetical” I just meant that you insist on discussing not the documented case at hand, but other issues. The Science case you talk about is different and you are constructing a hypothetical situation in which it is the same, except for the bought/stolen difference. The editors at Science have not had a situation documented, complaints filed with them, and been asked to adjudicate something, the GRG ones have. I don’t believe the situation you describe is exactly the same except for the bought/stolen distinction, but even if it were, so what? What’s at issue here is a specific example of plagiarism, I don’t see the point of arguing about other issues, other than to avoid confronting this one.

As for one reason for the outrage, read the comment above from “metin”. You may disagree, but to me another reason is what I see as the ever-decreasing scientific standards in theoretical physics, which have now gotten so low that some people seem to think there’s no reason to sanction even heavily plagiarized papers.

I can explain my reasons for wasting my time on this, I really can’t understand yours for spending time defending plagiarism and plagiarists. The one you give that you think my outrage is misdirected and should be redirected at the faculty members who exposed the plagiarism doesn’t make a lot of sense to me.

101. **amused**
January 21, 2008

Ok, one final comment

“After spending just 1/2 an hour on these papers would remove any doubt in anybody who has written any kind of paper (be it scientific or non-scientific) or who knows anything about plagiarism”

any doubt about what?
(you don`t need to answer .)
Are you related to Lubos by any chance? The style of argumentation is quite similar.

“the guy admits that he had plagiarized, what else do you want ? You want to catch him while he is actually doing the plagiarizing ? 🙂 (And perhaps broadcast it on Cops .) ”

The guy had already discussed his plagiarism in a Nature letter from Oct. 10. (before the GRG editorial and PRD retraction) He never tried to hide it, only to excuse it. (If he really wanted to hide it I don’t think he would be citing the papers he plagiarised from.)

102. **Coriolis**
January 21, 2008

amused,

I am almost sure that you have not looked at carefully in these papers.
In his Nature paper : he admitted to `borrowing beautiful sentences” ONLY in the introductory material. Please check the PRD retraction and see how much he plagiarized.
We are discussing something concrete, it is out there, just check it.

No, I am not related to Lubos, I don`t know him, I am not familiar with his writing. I am sure you are not Yilmaz, not that you don`t think a like, but that your English is infinitely better 🙂
But, there is still a good chance that you might be a GRG editor 🙂

Anyway, I still have to thank for this discussion: I did learn one more time that some people enter a discussion not to learn and perhaps contribute but simply argue for the hell of it. This is not productive. Please first go and read these papers, and then come back to argue specifically on them. Not on some abstract, irrelevant issues that takes nobody nowhere.

103. **Tansu KUCUKONCU , PhD**
January 21, 2008

Discussion here turned out to fun.

There is a famous saying, what is definitive is where to look,
not what to look.

Who is where in this discussion ? 🙂

Some defend plagiarism and Turkish plagiarists with fuzzy, implicit sayings.

Turkish plagiarists and supporters, defenders, protectors,
promoters of them are doing the same thing.
No one defends plagiarism explicitly in front of public.
But they plagiarize explicitly in front of public,
and say that it is not plagiarism.
If anyone dare to falsify them they brake the wood on his head,
since they have the power.

Turkish university system is a deep bog whcih connected to other bogs of Turkey.

You see and are talking about only 15 flies which you noticed shitted 70 times in a garden near you.

Some say, so what, what happened if they shitted.
And they add that this is not a tribute to shitting.

———————————-

Ali ,
” Tansu, you are complaining that these students wil lget faculty positions at Podunk State University. So what? Who cares? Why do you feel so bitter about it? ”

I am victim of these Turkish plagiarists and supporters, defenders, protectors, promoters of them for 7 years.
Moreover normally any honest Turkish citizen and any honest non-Turkish academician especially who may be competent in international projects (such as EU projects (even comu plagiarism rings (osman demircan and co.) have some such applications)) with Turkish plagiarists.

— — — — — — — — — — — — — — —

There is a basic rule valid for most of the social cases :
follow the money.
What's about applying this rule to journals which insist on
publishing of arxiv's Turkish plagiarizes' plagiarism works ?!
Can the editors who joined this discussion reply to this
question ?

104. **Ali**
January 21, 2008

Tansu, there is no funding for theoretical HEP anywhere in the world right now so I do not think their fake papers are gonna help the pagiarists a bit to get funding from EU or Tubitak. I do not think writing theoretical HEP papers is a lucrative way to get rich anywhere. I however admit that it may help them to get an academic position in Podunk State University(i.e. rural state universities in Turkey) BUT I want to remind you that writing three dozen papers is not a prerequisite to get that job and they probably would get that position with three papers anyway so why do you think they bothered with this? Besides, as it was pointed out by someone, these are underpaid positions anyway so I do not think these plagiarists are after wealth by any means. If you think you are a high-caliber scientist, why are you competing against these folks at Podunk State Universities? There are elite private universities in major cities which pay european level salaries to qualified scientists. I can assure you that they do not hire plagiarist clowns. Why don't you try one of them?

105. **Tumbledried**
January 21, 2008

Hi Coriolis, I presume you may have tangentially been also addressing me when you wrote these comments:

"It seems to me some people writing to your blog have not looked at these papers."

"Anyway, I still have to thank for this discussion: I did learn one more time that some people enter a discussion not to learn and perhaps contribute but simply argue for the hell of it."

I think I should stress a few things:

(i) Yes, I have looked at these papers, and, yes, I do not think that they are worth the paper they are printed on. Yes, this is a terrible indictment of the acceptance standard of certain journals;

(ii) Regardless, there are deeper issues here that I thought merited a quick word;

(iii) It sometimes pays to have a look at the bigger picture before making sweeping generalisations and casting broad denunciations.

(iv) My comment was made in good faith. I was certainly not "arguing for the hell of it". We are all scientists here. Surely we can discuss important matters like this in a civilised manner without having to constantly throw thinly veiled vituperation at each other?

Anyway, like amused, I am also going to bow out of this discussion. Enjoy your mudslinging.

106. **Tansu KUCUKONCU , PhD**
January 21, 2008

Let me complete my sentence :
Moreover normally any honest Turkish citizen and any honest non-Turkish academician especially who may be competent in international projects (such as EU projects (even comu plagiarism rings (osman demircan and co.) have some such applications)) with Turkish plagiarists should care about plagiarism and the bog which it was originated from.

Otherwise the famous anecdot tells :
they took a-s I said nothing,
they took b-s I said nothing,
……
they took z-s I said nothing,
when they come to take me,
there is noboby who can say any thing for me.

I am a victim they took.
I was saying what I belived before they took me,
I am saying now what I belive,
I will continue to say what I belive.

107. **amused**
January 21, 2008

Coriolis,

"I am almost sure that you have not looked at carefully in these papers."

Now that really is quite Lubosian. Are you *sure* you aren't related?

In the Nature letter he doesn't make any statement to the effect "we only borrowed for the introductory material". He makes some statements that *suggest* that that was all he did, but that isn't the same.
Good grief, why are we wasting time arguing about this…

"But, there is still a good chance that you might be a GRG editor 🙂 "

Ellis or Nicolai? No, i don't think so. But I'll take that as a compliment 😉

I also thank for this discussion. It reminded me of one of the reasons I went into physics (besides the obvious one): it is one of the few human endeavors where smug confidence in the quality of your reasoning won't get you anywhere if it is rubbish.

Ok now I really, *really* am done. Goodnight.

108. **Coriolis**
January 21, 2008

Tumbledried

I must have missed your earlier short comment, but somehow I managed to offend you. I just read it. Sorry, I think it just proves my point.

1)Please, go back and check the papers to see if they always cite or not.

2) No, these are not review papers, thesis etc.

Beforehand, if the journals knew the extend of copy-pasting, do you think they would publish these papers ?
There are many questions but the story has left origin long ago.

amused,
This is the first blog I am reading and the first discussion I am involved in. I aint Lubos, I don`t even know the guy and from what I don`t know, I don`t think I even like him 🙂 Hey, it occurred to me I might even be a woman.

Anyway, how anybody could possibly defend such blatant plagiarism
is really disturbing me. I do not understand why would one defend these papers? What is the reason? Why lower the standards of publication any further ?

109. **Geoffrey A. Landis**
January 21, 2008

I have to point out that the phrase "self-plagiarism" is an oxymoron.
Literally, if that phrase had any meaning, it would mean "passing one's own work off as being one's own work."
I do understand the point here, of course, and perhaps people who copy their own work and represent it as being new are, in these cases, exactly the same people who copy the work of others.
But nevertheless, I would call the two qualitatively different, and you seem to be debasing the intellectual crime of plagiarism by lumping so-called "self-plagiarism" into the same category.
Couldn't you perhaps think of a different word? What you mean to say is "the crime of violating the explicit terms of the publishing contract which the author signed to indicate that the work is unpublished and has not been submitted elsewhere."

110. **Peter Woit**
January 21, 2008

Geoffrey,

I agree that self-plagiarism is a quite different issue than plagiarism. In all cases here where I was using the term "plagiarism", I was using it to refer to plagiarism from others. The main problem with the papers at issue is the way in which they plagiarize the work of others.

111. **Tumbledried**
January 21, 2008

Hi Coriolis,

Thank you for your comment. I apologise for my slightly acerbic tone, and sorry if I offended you in turn! I am quite aware that these were research papers, not review papers. I'm not sure I made myself entirely clear – my intent was to attempt to point out that legitimate research can and does borrow/use excessively from that which proceeds it. ie it was not directly related to the papers in question, but a meta-comment. "Standing on the shoulders of giants" etc. But I see that I clearly made a mistake in trying to discuss this here, since evidently this upsets Peter, which on reflection is understandable since a reader skimming this discussion might confuse this observation with a defense of the sort of plagiarism that occurred in the Turkish papers.

Anyway, this should be my last comment. And a final apology for my clear ignorance on these matters and my offtopic remarks. I hope everyone here has a splendid day.

112. **Tansu KUCUKONCU , PhD**
January 21, 2008

Ali,

I don't know you are, what your background is, and where you are now.
One thing is clear that your approach is strange.

— — — — — –

"Tansu, there is no funding for theoretical HEP anywhere in the world right now so I do not think their fake papers are gonna help the pagiarists a bit to get funding from EU or Tubitak. I do not think writing theoretical HEP papers is a lucrative way to get rich anywhere."

As I said these you can meet these plagiarists every where not only in theoretical hep domains.
For example plagiarist Husnu Baysal approved lot of plagiarized computer enginering msc theses.
The list is very long.

— — — — — —

"I however admit that it may help them to get an academic position in Podunk State University(i.e. rural state universities in Turkey)"

"Podunk State University" : this naming does not cover the whole view.

A term like “Billboard University”
(to mean very unqualified, even very less qualified than qualified lyceess) is more appropriate for more than 2/3 of the universities (not only state universities but including private universities) in Turkey.

————-

“BUT I want to remind you that writing three dozen papers is not a prerequisite to get that job and they probably would get that position with three papers anyway so why do you think they bothered with this?”

Follow the money (+power, +maximum ego satisfaction), you will find the answers.

—————

“Besides, as it was pointed out by someone, these are underpaid positions anyway so I do not think these plagiarists are after wealth by any means.”

Relativity :-).
There are competents to take a piece from this underpaid pie.
Underpaid : according to international standards not Turkish standards.
That much is the highest ratio paid for a physics diploma (and for lots of other diplomas) in Turkey.
And other benefits are also generally the highest.
Moreover they can be doubled, tripled, quadrapled easily.
Let me estimate comu’s plagiarsit dean Ihsan Yilmaz’s income from the university, it is between 5.000 ytl to 20.000 ytl per month
(~ 4.000 usd to 16.000 usd) .
He can not easily gain more than 1.000 ytl anywhere else in Turkey with his diplomas.

—————————

“If you think you are a high-caliber scientist, why are you competing against these folks at Podunk State Universities? There are elite private universities in major cities which pay european level salaries to qualified scientists. I can assure you that they do not hire plagiarist clowns. Why don’t you try one of them?”

My story is long. I summerized a few part of it whic is related to arxiv scandal.
Almost every Turks know Turkey and Turkish university system spent thousands of high-caliber scientists.
That’s why plagiarists are running horses today.
I am not competing against these worthless people.
I am victim of them.
Why I was comu (canakkale) : in short, canakkale is my home town,
my family lives there, and my family needed me then.
I was unaware that much of comu, Turkish university system, and Turkey.
When I was awared it was very late.
For 7 years I’m under attack.
5 years I struggled to rescue my phd diploma which was illegally blocked by academic and juristocratic mafia.
I wrote thousands of papers in my search for justice,
instead of writing tens of scientific papers in my expertise areas.

Searching justice against academic mafias and juristocratic mafias in Turkey causes lot of gates, which normally should be open, to close, unfortunately.

You assure everything…..
Unfortunately you can meet plagiarists every uni. in Turkey where they are promoted to president of universities and to higher.
Only meeting frequency changes.

113. **Arun**
January 22, 2008

Observation, prompted by Tansu K – competence is a threat to the incompetent; when incompetent people obtain positions of power and influence, they will try to close the gates to the competent. This is generally true. It can take a generation or more to repair the damage, if ever.

114. **amused**
January 22, 2008

Arun, I agree completely. That’s why I think that rather than obsessing over what editors did about copied text it would be more productive to consider what can be done to encourage and support competent researchers in developing countries who are disadvantaged in systems that reward quantity (of junk, copied or uncopied) or political connections over quality.

Ciriolis, my “defence of blatant plagiarism” is merely to say that I find GRG’s response adequate (and yes, brave considering the circumstances). The authors were shamed by having their text copying discussed in an editorial which also stated that it was unacceptable and would not be tolerated in future. You and Peter obviously have more of a lust for retribution than me.

Peter, how would you rate the embarrassment and damage to the field caused by the GRG decision compared to that caused by the media spectacle resulting from Smolin’s hyping of the surfing independent physicist and his “theory of everything” (unrefereed and inviable)? Where was your outrage in the latter case? Funny how your reactions always seem to correlate with your interests in these situations — not speaking out against your ally Smolin’s deplorable hyping, but turning on the outrage in a situation where you see a chance to promote your hypothesis about the journal system being “collapsed”.
(Yes, I decided to start using the same tactics of response that you have been using to me. If you want to lower the tone further I’ll follow you all the way down.)

115. **Peter Woit**
January 22, 2008

amused,

You’re still trying to change the topic from your defense of the indefensible, now to your fanatical hatred for Lee Smolin. In order to “lower the tone” I’ll avoid commenting here on your behavior in the Lisi affair, and just state that I stand behind every word I’ve written about it (the Lisi affair), here and elsewhere.

116. **amused**
January 22, 2008

"You're still trying to….." "your defense of the indefensible…" etc etc

If you make these pronouncements often and vehemently enough they might actually become true. Try it!

117. **Tansu KUCUKONCU , PhD**
January 22, 2008

amused,

are you Ellis of GRG , or someone in relation to him ?

fun increases 🙂 :

" That's why I think that rather than obsessing over what editors did about copied text it would be more productive to consider what can be done to encourage and support competent researchers in developing countries who are disadvantaged in systems that reward quantity (of junk, copied or uncopied) or political connections over quality. "

he he 🙂
Turkish comics magazine Leman has given Tulip Awards for years.
You can start to give Scientific Tulip Awards to competent lonely scientists whose papers' abriged versions you promotionally publish in a residual corner if others leave.

———————————

Follow the money rule 🙂 :
What is the market value of this ?
What is the share of Turkish universities in this market ?
What is the risk of threating the ducks on Turkish market and other markets ?

118. **derridux**
January 23, 2008

Who could have said that the editors of "Social Text" would have the last laugh after all? They must still be laughing.

For several years more than five dozen papers full of BS were published by many HEP and gravity journals, including several of the most respected and prestigious ones, which didn't even notice the hoax.

It's true that plagiarized material is different from made-up nonsense. But it's also true that social scientists need not know anything about GR, they just trusted Sokal. Here we have the supposed experts swallowing rotten fish in wholesale quantities.

Not surprisingly, all the big names that came out at the time to tell how social sciences were in deep trouble now seem to be silent, at least publicly.

119. **amused**
January 24, 2008

Tansu,

I tried to reply to the comment you addressed to me, but Woit deleted it.

I used to think that this blog was run by a person with integrity, motivated by genuine concern about real problems that exist in the present day hep physics community. But it has become clear that he is just another demagogue, out to ram his own judgements down our throats and with very little interest in engagінging in good faith debate on the issues.

The points I tried to make in this discussion — that it is important to distinguish between plagiarism of results and copying of background text, and that the GRG decision to allow the papers to remain in the journal in this instance because their results are original, while declaring the text copying unacceptable and stating that it won't be tolerated in future, might actually be quite reasonable — have been met with: "Defender of plagiarism! Supporter of plagiarists! You're just trying to prop up a collaped journal system!" (Of course, my attempts to distinguish between which aspects of the journal system were in trouble and which were still functioning ok was completely ignored — everything is black-and-white when you're a demagogue.) Other commenters gleefully joined in with mocking and derision, just as good followers of a demagoge are supposed to do. ("If you actually bothered to look at those papers you would know that…" etc etc.)

Woit sometimes bemoans the fact that so few serious phyicists are willing to participate in discussions here. He puts that down to too many people making "uninformed comments". Well I think there is another reason, and it has become pretty obvious.

120. **RILED UP**
January 24, 2008

"amused " you are totally missing the point. Your position is completely unethical, and people are quite rightly criticizing you for it. You are condoning actions that cause actual harm, as you surely realize.

121. **Eric**
January 24, 2008

I agree with Amused that copying background material can be acceptable, so long as adequate references are given and it's clear to the reader that the background material is not original. In my experience, this just makes it easier to connect an author's new results with the existing body of work. Especially when their exist different conventions and syles for a particular subject, it is helpful to have a common source of material so that different researchers can all speak the same language. I would wager that in all liklihood those in this thread grandstanding on this topic are not active researchers who publish papers regularly, e.g. Peter Woit.

122. **Stan**
January 24, 2008

Eric,
Should we assume you are an active researcher copy-pasting material without even citing ?

123. **RILED UP**
January 24, 2008

There is no doubt that “amused” was condoning activities that any reasonable person would call plagiarism. There’s no excuse for dressing it up as something else. And “Eric” I would suggest you stick to non-gambling sources of income.

124. **amused**
January 24, 2008

Riled up,

I’m sorry, but stating things vehemently is not by itself sufficient to show them to be true. I’m open to debating this issue, explaining the reasons for my views (which I don’t think are at all unique), and even to changing my mind about them if someone convinces me that they are untenable. But what I encounter most of the time here is not arguments but proclamations. E.g., your comment is just a string of proclamations, with any arguments. It is a classic tactic of demagogy: say something often and vehemently enough and it will become the accepted truth, with dissenters being cowed into silence.

You say that I am “condoning actions that cause actual harm, as you surely realize.” Well no I don’t realize that at all. GRG shamed the culprits by discussing their text copying in an editorial, and stated that it was unacceptable and would not be tolerated in future. I happen to think that is a sufficient response and don’t agree at all that it is causing actual harm. We can debate it if you want to.

Let me ask you this: Do you think the GRG editors — Ellis and Nicolai — are people of limited integrity who don’t take ethical issues seriously? Do you think they were lying when they wrote in the editorial that they had seriously considered this issue? They are both renown, highly respected physicists with extensive experience as journal editors; probably they have thought more about journal-related issues, including ethics, than any of us. So perhaps it is appropriate to be a bit more open to the possibility that they may have made a reasonable decision, rather than rushing in with knee-jerk outrage. (Of course, a demagogue with an anti-journal agenda won’t have much willingness for that.)

125. **Peter Orland**
January 24, 2008

Wow, I can’t believe what I am reading.

At my institution, students in my classes who copy phrases
from a published source with no citation not only flunk,
but are referred to the VP for academic affairs and
out on probation.

126. **Peter Woit**
January 24, 2008

amused,

The reason your comment was deleted was that it did little more than question the professional competence of those physicists at METU who exposed this plagiarism, and sneer about the difficulty of finding any competent Turkish physicists at all.

Anyone who reads this exchange here can make up their own mind about who is engaging in demagoguery.

127. **amused**
January 24, 2008

The credit the students get for their assignments comes from how well they answer questions, solve problems etc. Obviously if they plagiarise something that is going to affect their credit then it’s a very serious offence. (And if they are being judged in part on their writing then anything they plagiarise affects the credit.)
The credit physics researchers get for a research article is determined entirely by their results. No one gives a sh*t about their eloquent introductions and background material. They get absolutely no credit for it.
Like I said before, stealing is always wrong, but stealing wrapping paper to wrap a potentially valuable object is surely a less grevious offence than stealing the object itself. Now you’re all going to shout at me again that I’m “defending plagiarism”, right?

128. **amused**
January 24, 2008

“…question the professional competence of those physicists at METU who exposed this plagiarism, and sneer about the difficulty of finding any competent Turkish physicists at all.”

That is an outright lie. Once again you are misrepresenting what I wrote to serve your own agenda.

129. **Peter Orland**
January 24, 2008

Amused,

I am not shouting (not even metaphorically) but yes, you are defending plagiarism.

130. **amused**
January 24, 2008

Peter O.,

To my mind, that’s like saying I am “defending murder” if I say that a drunk driver who kills a pedestrian has committed a less grievious offence than someone who kills in cold blood.

131. **Peter Woit**
January 24, 2008

Here is the deleted comment from amused. People can judge for themselves whether I was right to delete it or have “lied” in my characterization of it. But please, discussing it has nothing to do with the topic of this posting: plagiarism and its toleration by journals, please stick to that here. If you want to insult me as a demagogic liar or praise me as a fearless defender of high standards of ethics, you can do so by e-mail, not in this discussion section.

“”You can start to give Scientific Tulip Awards to competent lonely scientists”

Well I would love to do that, Tansu, but first we have to find some competent Turkish scientists. (Do you insist that they must also be lonely?) None of the plagiarists in this scandal are in the least bit competent… but how about the METU faculty who exposed them, would any of them qualify?"

132. **Peter Woit**
January 24, 2008

amused,

What you're doing is defending as brave and admirable the decision not to sanction, in your analogy, a drunk driver who has killed a pedestrian. This is why it is accurate to say that you are defending plagiarists and their plagiarism. It's also why most people think you are defending something indefensible.

Except Eric I guess, who goes further and suggests that drunk drivers killing pedestrians is a good thing…

133. **Peter Orland**
January 24, 2008

Amused,

Socially unacceptable behavior can be characterized in degrees. That isn't the point.

At most accredited schools and universities, any student who has been found to copy published material is (and should be, in my opinion) in serious jeopardy of being expelled. If this activity is unacceptable in our classes, shouldn't it be utterly unacceptable in our journals?

134. **amused**
January 24, 2008

Woit, you are just unbelievable. The GRG editorial decared the text copying to be unacceptable. I agreed with the GRG editorial. So no, it is *not* analogous to sanctioning the drunk driver, it is analogous to saying that he/she should get a lesser punishment than the person who kills in cold blood.

Peter O.,
I am *not* trying to say that plagiarism is ever acceptable. But I think it is appropriate to treat different kinds of plagiarism differently. When a physics researcher copies background text it is unacceptable, but at the same time a much less grievous offence that plagiarising research results. I can't believe we are disagreeing about this.

As for my previously deleted comment that Woit misrepresents as deriding Turkish scientists: No that was not how it was intended. I decided to respond sincerely to show up the leering sarcasm of the person I was responding to. I know absolutely nothing about Turkish science or scientists, except that the plagiarists in this case seem to be lousy ones. I think it would be a genuinely good idea to show some recognition to good Turkish scientists in the present situation. The obvious place to start when looking for them would be the people who exposed this scandal. Also, that commenter had been insinuating in many posts that Turkish academia is corrupt to the bone, so drawing his attention to the fact that it was Turkish scientists who exposed this plagiarism is a way of confronting him with reality.
Having said that, I do find it strange that Salti & co were able to put out 1-2 papers a month for 2 years before anyone got suspicious and looked into it. If grad students had been doing that at any of the uni's i've been at I'm sure people would have been onto it much sooner. When grad students start doing this kind of thing it reflects badly on the environment and values in a department, something which surely all faculty share responsibility for.

135. **Eric**
January 24, 2008

I think there should be a distinction made between paraphrasing someone elses work while giving a reference and whole-sale copying without any reference. The first is definitely not plagiarism, while the second case is clearly unacceptable. However, from the narrow arguments of Peter et. al., even copying the LaTeX for a formula or table from another paper is plagiarism.

136. **amused**
January 24, 2008

Peter O.,
Adding to my previous reply: Yes, it is a perfectly reasonable opinion that activity that isn't aceptable in classes should also not be acceptable in journals. I wouldn't say that you or anyone else who held that view was wrong. At the same time, I think the GRG editors' view — that the paper may remain in the journal because its results are all new, but that the authors are shamed by having their copying pointed out in an editorial, which states it is unacceptable — is also a defensible position. I just don't think there is only one "right thing" to do in this situation. The GRG editors handled it one way, the PRD editors another way, and both were completely reasonable in my opinion.

137. **Peter Woit**
January 24, 2008

amused,

My apologies if I misread the intent of your question about whether any of the METU faculty who exposed this plaigiarism are competent, which I read in the context of your earlier sneering comment about "those wonderful METU faculty members":

"Those wonderful METU faculty members who caught the plagiarists, where were they while Salti & co were putting out 1-2 papers a month over 2 years? (They didn't notice?) What kind of environment and values were they and their colleagues propagating? Perhaps you can consider directing some outrage at them Peter, if you have any left over after you are finished with the journal editors."

In any case, questioning whether the people who exposed this plagiarism are professionally competent seems to me to be a highly inappropriate diversion from the issue of what to do about the plagiarists. I think I was correct to delete your comment, and will delete any further attempts to turn this into a discussion of the competence of the people responsible for bringing the plagiarism to the attention of the arXiv and the journals.

There's a standard, minimal, conventional and appropriate sanction for plagiarism: withdrawal of the paper. At most institutions, the sanction for this kind of plagiarism is actually much more severe than this, often including dismissal from one's academic position. In this case, one can reasonably argue about how the institutions involved should sanction the authors caught plagiarizing, but I don't think there is any reasonable argument for not withdrawing the papers, and requiring the authors to resubmit them in unplagiarized form.

The arXiv, PRD, JHEP and Astrophysics and Space Science editors decided this was serious plagiarism and took the appropriate action, the GRG editors decided instead to defend the publication of the papers, despite their heavily plagiarized nature and not to even put any note on the papers that they consist largely of plagiarism. You consider this brave behavior and forceful criticism of it to be demagoguery, I think most scientists strongly disagree with you about this. Right now, anyone who looks up these papers in GRG will find no indication that they are looking at a heavily plagiarized document. If they happen to run across the GRG editorial (not so easy to find…), they'll get the impression that there was a small amount of irrelevant plagiarism in these papers. The only way they are likely to know that the plagiarism was serious and extensive is if they look up the arXiv version and see the withdrawal statement there. Journals are supposed to be more reliable sources of information, with higher standards than a preprint server, but quite the opposite is true in this case.

Eric,

I've not entered into the issue of deciding how much copying has to go on before something is sanctionable plagiarism. Obviously there are grey areas. What I have said, and stand behind, is that if you look at the marked-up pdfs of the two GRG papers, they are undeniably examples of extensive plagiarism, of a sort that should not exist in an academic journal. Any journal that makes the mistake of publishing something this heavily plagiarized should withdraw it, not leave it on-line with no indication it is plagiarized material.

138. **Hans**
January 24, 2008

GRG did also publish some papers of the Bogdanovs. After it came out, that their papers were simply a joke, GRG did not withdraw even these strange papers.
At this time, one of the editors of GRG, Nicolai, gave an interview in the german magazine "Die Zeit" in which he stated, that if these papers would have appeared on his desk, he would have rejected them. He said that he would try to improve the standards of GRG.

Seems that he failed, or that GRG is such a problematic journal, that it cannot defend itself from garbage. scary..

139. **Peter Woit**
January 24, 2008

Hans,

The Bogdanov papers were not published in GRG. One was published in Classical and Quantum Gravity, which is what you are thinking of. That journal did not withdraw the Bogdanov paper.

The statement by Nicolai you quote refers to that journal, where he was an "honorary editor". An e-mail signed by him and the journal publisher, see

http://math.ucr.edu/home/baez/bogdanoff/

said that the journal editorial board had determined that the paper should not have been published, but decided not to withdraw it.

A later version of this statement, see

http://listserv.nd.edu/cgi-bin/wa?A2=ind0211&L=pamnet&T=0&F=&S=&P=3647

deleted the language about the editorial board having determined that the paper should not have been published, and reads purely as a defense of publication of the paper.

140. **Hans**
January 24, 2008

Peter, Thanks, sorry, I confused class quant grav with gen rel grav.

141. **amused**
January 24, 2008

Peter W.,

I regret making that "wonderful METU faculty members" comment. While the length of time it took for the situation to get noticed and looked into there does seem strange, I don't know anything about the goings on and so shouldn't have made that comment.

I agree with your criticism about GRG not mentioning the situation about these papers on their (the papers) websites. They should definitely add a note about it with a link to the editorial.

As for the rest of GRG's handling of this, I can't really say anything more than what I've said already. The authors have been shamed, the text copying was declared unacceptable, and intolerable in the future… to me that seems sufficient. You suggested the authors should have to rewrite the papers in their own words — sure, I wouldn't object to that, just don't see what it would achieve that hasn't been achieved already (apart from giving them some extra punishment and English practice).

About demagoguery (also spelt more efficiently but still legitimately as demagogy): that wasn't directed at your criticism of GRG but at the way you were responding to my comments. When you and others challenged my views I always responded with detailed step-by-step responses to your points. It would have been nice to get something similar in return. Instead I got misrepresentations of what I had said (for strawman arguments) and ex cathedra proclamations that I was a "defender of plagiarism" etc. Ok, I know that you are busy and have other things to do (so do I). But that kind of response comes across as you wanting to ram your judgement down our throats rather than engage in a discussion in good faith.

142. **Tansu KUCUKONCU , PhD**
January 24, 2008

amused,

you're defending plagiarism and unethical behaviours.
Your style is clanic. Strange.
Probably I know who you are.
Normally your approach is unacceptable especialy for who holds positions like yours.
I am trying to understand your role in this game.

Do you have a plagiarismmeter which you decide according to it
such as this much is acceptable, this much is more acceptable,
etc ?

"

1.
it is important to distinguish between plagiarism of results and copying of background text, and that the GRG decision to allow the papers to remain in the journal in this

instance because their results are original,
2.
while declaring the text copying unacceptable and stating that it won't be tolerated in future, might actually be quite reasonable
"

Let us assume part 1 is true.
Why don't you and GRG defend this in part 2 then ?
Let us assume part 2 is true.
Why do you and GRG defend this in part 1 then ?
You are falsifying part 1 in part 2 and vice versa.

This means that ohh our folks have caught on job,
let's not apply the rules for them.
In Turkish there is an idiom for this : belly dancing.

Future ? Which one ?
100 years later ? 1000 years later ?
Or the future when the plagiarists are the
ones who don't need to be defended by you and GRG ?

" GRG shamed the culprits by discussing their text copying in an editorial, and stated that it was unacceptable and would not be tolerated in future. "

Why does GRG accept things now which it declares will be unacceptable in an unknown future ?

" I happen to think that is a sufficient response and don't agree at all that it is causing actual harm. "

Relativity again.
Harm for what or who ?

——————————–

" Do you think the GRG editors — Ellis and Nicolai — are people of limited integrity who don't take ethical issues seriously? "

This is the general view of lots Turkish academicians, juristocrats, etc even at top positions.

Clanic style. You say :
obey the masters.
This is very ordinary in Turkey.
But GRG is not in Turkey, and it's not Turkish.

——————————-

" The credit physics researchers get for a research article is determined entirely by their results. No one gives a sh*t about their eloquent introductions and background material. They get absolutely no credit for it. "

This is not generally valid for Turkish system which is based on partizanship.

———————————

I couldn't find an image of Leman's Tulip Awards.
Anyone see it guesses whom it is given for what 🙂 .

Showing lonely competent scientists as candidates for it was an irony.

Language game. Greetings to Wittgenstein.

———————————

METU, my university at the same time,
corresponds to MIT etc of US and
Cambrigde etc of amused's island.

It is impossible in Turkish system that
everything can be alright for METU.
YOK's (Higher education/despotic council) ex-president and his
ex-team who were METU faculties were the most despotic ones,
and they gave the highest harm to Turkish university system
and Turkey. They covered hundreds of plagiarism cases.
As I said comu's ex-president and head of comu plagiarism ring
was a faculty of METU physics dept., so was comu's vice president and 2nd head of comu plagiarism ring.
In spite of these, average of METU in lots of subjets is very high
that others.
METU equip exposed arXiv plagiarism case,
in spite of some faults of METU
(by others who are not in that equip),
the other 13's cases are trying to be covered.
METU equip's work would have been more difficult
if METU plagiarists were faculties, deans, etc.
But it's also difficult to meet such plagiarists in METU,
since METU has an internationally respected name
to be kept.

In short,

amused, yes,
mentioned METU equip consists of very qualified scientists.
They are not lonely, and they don't have any trouble to
publish their qualified works.

But there are lots of lonely (or unpartizan) qualified scientist in Turkey which have not much chance to progress if support to plaiarists and other unqualifieds continues.

So your proposal to think about what can can be done
is similar to giving pies of good boys to bad boys then
giving candies to good boys to make them forget their pies.

— — — — — — — — —

Is there any more complex complo theories
behind supporting plagiarism in a developing country ?

— — — — — — — — — —

Eric, let me repeat what Stan said :
Should we assume you are an active researcher copy-pasting material without even citing ?

— — — — — — — — — —-

I wonder
what would happen if arXiv plagiarists were not Turkish,
but for exampe Amerikan, for example from MIT or like
not from METU,
or for example from amused island from Cambridge or like
not from METU ?
What would be the trend of the discussion here then ?

143. **Ali**
January 24, 2008

Tansu, METU is NOT MIT of Turkey. It is not anywhere close to it. In the recent top 500 universities of the world listing of The Times, there were 5 turkish universities AND METU was not in that list. You should realize you are not the only one who has PhD in this forum so you should try to provide accurate information if at all possible. Plus, like I suggested to you before, you should try your chances in other high-caliber universities if you think you are really such a great scientist. I do not understand why you are so obsessed with comu and metu. I enjoy your sarcastic comments but I would prefer if you can quit this chip on the shoulder attitude.

144. **amused**
January 24, 2008

Tansu,

Yes, I'm defending plagiarism, murder, and lots of other terrible things.

Good luck with working out my identity from my strange clanic style 😉 (hint: if you think i have a "significant position" you're sadly mistaken).

My role in this game: a random physicist blog commenter who doesn't know when to keep his mouth shut. Now you know 😉
No I haven't finished building the plagiarismmeter yet, so for the moment can only distinguish between plagiarised results and copied background text. Some people think this is already sinful enough.

In answer to your questions about 1 and 2: I guess the GRG editors are hoping that people will heed their warning and not submit more papers with copied text, or that the software will catch them if they do. In that case the editors can avoid having to do what they don't want to do, which is to retract papers which their referees have already deemed publishworthy. But if it happens again in future they will of course have to take stronger measures, i.e. retract the offending papers.
The idea of giving people a warning before taking stronger measures against them seems quite foreign to you… Ideally it should work as a deterrent.

"Relativity again.
Harm for what or who ?"

Anything or anyone. If you disagree, can you explain precisely where the harm is (caused by the GRG decision to shame the authors, declare what they did unacceptable, but allow the paper to stay in the journal because the only part of it that counts for anything — the results — are not plagiarised.)

"Clanic style. You say :
obey the masters."

No, it's just natural to respect people who've earned it.
(I'm sure you'll have some fun with that.)

"But there are lots of lonely (or unpartizan) qualified scientist in Turkey which have not much chance to progress if support to plaiarists and other unqualifieds continues."

And I've been supporting the plagiarists in this case, right? By pointing out that their text-copying isn't as bad as plagiarising research results, and suggesting that the sanctions GRG took were already sufficient…
Come on, any competent researcher (lonely or not) should be able to outcompete these clowns on merit, and that won't be made any more difficult by the clowns' copying of background texts. So if the clowns are winning it means the system is not based on merit (i.e. rewarding quality ahead of quantity of junk) — that's where the real problem lies.
Why are you so much obsessed about "plagiarism rings" and so little concerned about the merit issue? It seems you wouldn't mind the system rewarding lousy researchers as long as they hadn't plagiarised anything..

145. **Tansu KUCUKONCU , PhD**
January 25, 2008

Ali,

may you explain who you are, what your background

and foreground are, what your position is ?
It should not be needed to hide yourself.
To compare METU with other Turkish universities
(except a few ones) is funny.
This is the list you mentioned :
Bilkent (most of its faculties are METU graduates),
Cukurova, Istanbul Technical, Istanbul, and
Sabanci (lots of its faculties are METU graduates).
You said you studied abroad.
Did you meet or hear anyone (faculty, researcher, high skilled employee, etc) in any top level university, research center,
company, who is a graduate of Cukurova uni. which is 30+
year old ?
or who knows its name ?
There are thousands of such METU graduates all around the world
(in MIT, Stanford, Berkeley, Yale, Oxford, Cambridge, …..
in research centers of NASA, Microsoft, IBM, Sony, Google, Boeing,
….).
You can easily meet people in scienfitic, research, high skilled company, etc communities all around the world who know some about METU.
METU (which is 50+ year old) is the #1 Turkish university in being known globally.
#2 (Bosphorus, which is 150+ year old),
#3 (Istanbul Technical, which is 230+ year old), and
#4 (Bilkent, which is 20+ year old)
follow it far behind in this regard.

One another important thing, you know this well :
Nationwide University Entrance Examination (UEE).
This is the primary way to start to study in a Turkish university.
Rank in that exam is the main criteria.
Students prefer where to study according to their ranks,
and they're automatically accepted according to their ranks
and preferrences.
METU's almost all the students, during almost all of its history,
have ranks between 1 to 1000, or 0.1 to 1%.
Last year 1.500.000+ students took that exam.
Preferrence by the top level students shows its relativistically correspondence to USA's top level universities.
Bosphorus' student spectrum is similar to this.
There is no other university having an overall similar student
spectrum.
To meet UEE's rank 1 students between METU students
is very ordinary.

Did you meet or hear any student who has a rank in the
first few thousands (not 1000, or 100, or 10) and
preferred to study in any dept. of Cukurova ?

— — — — — — — — — — — — — — —

" I do not understand why you are so obsessed with comu and metu. "

I am victim of mafias of comu, Turkish university system, and
Turkish juristocracy which has deep conjunctions with others.
Lots of my human rights have been violated.

— — — — — — — — — — — — — — — -

amused,

may you explain who you are, what your background
and foreground are, what your position is ?
It should not be needed to hide yourself.
Don't make your readers wonder who is behind this defence.

" The idea of giving people a warning before taking stronger measures against them seems quite foreign to you… "

You are saying as if plagiarism were newly discovered
and announced as an unacceptable thing.
It can be extended, as you said, to
"murder, and lots of other terrible things".
Hey folks, others dislike what you did, no problem for us,
be careful and don't be caught next time,
otherwise it will be difficult to protect you next time.
Yes, this is clanic.

— — — — — — — —

" it's just natural to respect people who've earned it. "

Defending plagiarism is a very great risk to lost all the
respect earned till then.

— — — — — — — — —

" So if the clowns are winning it means the system is not based on merit (i.e. rewarding quality ahead of quantity of junk) — that's where the real problem lies. "

Yes what I basically say is this :
Turkish university system is not based on merits,
it is based on partizanship.

” Why are you so much obsessed about “plagiarism rings” and so little concerned about the merit issue? It seems you wouldn’t mind the system rewarding lousy researchers as long as they hadn’t plagiarised anything.. ”

That ‘s completely wrong.
My main issue is the merits and lackness of merits.
But this is country-wide problem in Turkey noy limited to universities.
As I said, for ex., juristocracy is supportings all the faults
in universities.
“Plagiarism rings” are not just “plagiarism rings”.
They are mafia rings at the same time, deeply connected to
other mafias.

— — — — — — — — — — —

” Anything or anyone. If you disagree, can you explain precisely where the harm is (caused by the GRG decision to shame the authors, declare what they did unacceptable, but allow the paper to stay in the journal because the only part of it that counts for anything — the results — are not plagiarised.) ”

Yes, let me explain :

1. Turkish system traditionally is trying to cover the 13 of 15 plagiarists’ cases.
GRG is giving support to this coverage to be done easily.
Turkish plagiarists’ are saying that “who are blaming us as plagiarists are the enemies of Turkey, they’re nothing else.
arXiv is just an unimportant archieve,
look that GRG published our articles and GRG is defending us
against who are blaming us”.
These are clowns, but very dangerous ones.
GRG’s support is preventing Turkey to get rid of these clowns.
arXiv scandal is the first internationally exposed mass Turkish plagiarism case. This may be a great opportunity for Turkey to brake
plagiarism tradition. But GRG’s support is a wall in front of this.
What happens if nothing happens to this 15 plagiarists ?
It will be very difficult to catch them on job again.
They will try to be more careful.
An assoc. prof., prof. holds his position in university during life time in Turkey without being interested in science, without producing,
withot teaching. He gains 3.000 (~2400 usd) ytl to 100.000+
(80.000+ usd) ytl per month.
Such clown academicians close all the gates to qualified academicians, and open all the gates to their partizan clowns.

Is that much harm of GRG to Turkey and to Turkish qualified
scientists enough ?

2. GRG’s support to plagiarism encourages plagiarists
all around the world, and cause similar harms to
especially developing countries as explained in (1).

146. **Ali**
January 25, 2008

Tansu, I have a PhD in theoretical condensed matter physics from USA and I live in Canada at the moment. I have no relation with metu or comu in any way. I did not study there. Yes I met lots of metu graduates personally. My impression is that metu used to be one of the top (if not the top) universities in turkey 20 or 30 years ago alongside with itu and bosphorus (all public schools for non-turkish readers) but when private universities started in 1980’s and 1990’s (like, bilkent, sabanci and koc) metu lost its lead and it is way behind these schools at the moment. The metu graduates who are in positions you mentioned graduated long time ago. They are not recent graduates. The recent metu graduates I met were mediocre students to put it mildly. Again, back to my original statement, you need to try your luck somewhere else if you think you will not be able to get into metu and comu. This whining is not gonna take you anywhere. Good luck

147. **Tansu KUCUKONCU , PhD**
January 25, 2008

Ali,

I don’t know why, but you are chainging the route of the discussion.
This is passively or indirectly doing something.
Do not hide yourself, who you are, where did you study, where you are now.
There is an allergy to METU and METU graduates in Turkey
by some since it’s top level and very different than others as in the example of its behaviour in case of arXiv scandal.
This allergy caused the trials of closing METU completely, and
closing of METU for 1 year in 70’s.
It seems that you have that kind of allergy.
What you said about METU is not true.

I don’t want to go into details of comparison of Turkish universities,
but what I said about METU is valid for almost 50+years since
almost its constitution.
“METU’s almost all the students, during almost all of its history,
have ranks between 1 to 1000, or 0.1 to 1% in UEE.”
So they always had and will have positions in top level institutions and companies all over the world.
“There is no other university having an overall similar student
spectrum.” Neither bilkent (which is 20+ year old ; lots of its faculties are METU graduates),
nor sabanci (which is 10+ year old ; lots of its faculties are METU graduates),
nor koc (which is 10+year old ; lots of its faculties are METU graduates), nor others.

Nothing has changed. And there is no sign of that it may change in the near future.

— — — — — — — —

" you need to try your luck somewhere else if you think you will not be able to get into metu and comu. "

The problem here is arXiv plagiarism scandal and Turkish plagiarism and the tradition behind it.
You are tactically changing the route to the wrong way.
I didn't say anything that I wanted to get into metu or comu.
I very explicity said :
" I am a victiom of comu mafia and other mafias in connection with it
and these mafias have violated lots of my human rights for 7 years.
Moreover I could not make my voice to be heard in Turkey during these 7 years due to country-wide effectiveness of these mafias.
So I am in search of justice for these violations for 7 years
and will keep searching till whereever it goes. "

148. **amused**
January 26, 2008

Tansu,

You want to know my background, foreground and position? Really, I am no one important. I don't have any connection to the people involved in this and have nothing personal at stake. The only reason I joined in this discussion was because I thought there were some relevant points that were not being made, and I felt like making them. What difference would it make if you knew my name — it would mean nothing to you. (Btw, Woit knows who I am, so I'm not completely anonymous here.)

First of all, I am sympathetic to your situation and wish you and everyone else fighting against academic corruption wherever it is best of luck.

Second, the GRG editors' response was in no way a defense of those papers. They shamed the authors by writing an editorial about their text copying, saying it was unacceptable. Yes, they distinguished between the nature of the plagiarism in this case — copying of background texts — and the much more serious plagiarism of results which didn't happen in this case. Well, that is a reasonable and fair distinction to make in my honest opinion, even though some commenters here apparently think it is sinful to make such a distinction. But if the plagiarist authors are presenting the GRG response as a defense of them then they are totally deluded. Anyone who reads the editorial can see that (despite the misrepresentation of it by a demagogue with an anti-journal agenda.)

The main message from what you have been saying seems to be this: there are mafia-style groups operating in Turkish academia for their own profit (financially and status-wise) without contributing much (anything?) of actual worth to their fields. They thwart the careers of competent honest researchers by "rigging the game" in various ways. Now they have been caught out for large-scale copying of others' background texts in their papers. (They were too careless and lazy to change the words a bit to avoid this situation, even though it would have been easy.) Understandably, the honest Turksh academics want to seize on this as an opportunity to hit back against the mafia and loosen its grip. And they are disappointed with the GRG response, because if GRG had gone further and retracted the papers (like the other journals did) it would have given the mafia opponents even stronger ammunition than they already have. Well, I can sympathise with that. However, it is wrong to expect GRG to become a player in the Turkish situation, and taylor its response so that it maximally benefits the good guys in that struggle. Journals, like newspapers, should remain detached from such things and do what is right in their independent judgement (for obvious reasons). That is what the GRG editors have done in this situation. I happen to think they made a reasonable, or at least defensible, judgement (although not the one I would have made); others disagree.

In any case, in the long run the only way the mafia situation can really get fixed is if an academic culture is established were rewards are based first and foremost on merit. Let's imagine you get what you want out of the current situation: this mafia gets smashed and its members are expelled from Turkish academia. What next? In the absence of a merit-based reward system, other characters will emerge whose talents for scheming and manipulation are inversely correlated with their competence as researchers; they will band together and take the place of the previous mafia. The names and faces change but the situation stays the same.
So if you are really going to change anything, the main focus has to be on the academic culture, changing it to one where rewards are based on merit. For this, it is infinitely more important to expose the lousy quality of the mafia members academic output than to expose instances where they were too lazy to even change the words in the background texts they copied.

149. **Aaron**
February 10, 2008

Amused: I am perplexed by your insistence that only the results matter for a paper, not the language. How results are presented are crucial to how well other people can understand and use new results. As an extreme case, are all review articles worthless in your world? Why?

Withdrawal of the paper is not to punish the plagiarists — it's a return to the status quo ante. It is also vital for the credibility of the journals. Journals are a type of database of who has contributed what. Having these databases filled with false information is little better than not having them at all.

Have Cosmologists Lost Their Brains?

January 15, 2008
Categories: Multiverse Mania

There's a peculiar long article in the New York Times science section today by Dennis Overbye, entitled Big Brain Theory: Have Cosmologists Lost Theirs? It's about the debate raging among a small number of cosmologists about "Boltzmann Brains", and the article does a pretty good job of explaining what the debate is about.

This seems to be a debate that is mostly taken seriously by people who live near the coast in California, with the article quoting Susskind and Linde (Palo Alto), Lisa Dyson and Raphael Bousso (Berkeley), Hartle and Srednicki (Santa Barbara), Albrecht and Sorbo (Davis), and Sean Carroll (Pasadena). One of the few from further inland who is quoted is Don Page (Edmonton), described as a "prominent voice in the Boltzmann debate" who argues with Hartle over the issue of whether to count humans differently than insects since we have consciousness. Page's recent arguments that God may like having lots of universes around are not quoted. On the other hand , Andrei Linde has a lot to say about what all this has to do with reincarnation, with the article ending with this quote from him:

> "If you are reincarnated, why do you care about where you are reincarnated?" he asked. "It sounds crazy because here we are touching issues we are not supposed to be touching in ordinary science. Can we be reincarnated?"
>
> "People are not prepared for this discussion," Dr. Linde said.

Overbye does note that:

> If you are inclined to skepticism this debate might seem like further evidence that cosmologists, who gave us dark matter, dark energy and speak with apparent aplomb about gazillions of parallel universes, have finally lost their minds.

and, while he doesn't quote any such skeptics, I suspect the title of the piece and the way he quotes some of the sillier things respectable cosmologists are saying indicates some sympathy for skepticism about this.

If you do take all this seriously, you might want to discuss it over at Cosmic Variance where Sean has a posting on the topic. In the NYT piece he is quoted as saying:

> When you break an egg and scramble it you are doing cosmology

to which his ex-colleague Jeff Harvey from Chicago responds in the comment section:

> When I break an egg and scramble it I'm making breakfast. I guess that is the difference between cosmologists and particle physicists.

Update: The New York Times is listing this article as the most popular article on their site (in terms of how many people are e-mailing it to others).

Comments

1. **Joe**
   January 15, 2008

   I was reading this this morning at the doctor's waiting room, and I could not helping thinking that these people must have got their probability calculations way off.

   The chance for amino acids to randomly line up to become a functional protein is incredibly small — for a chain of 27000 amino acids that's 20^-27000. There are not enough multiverses around even for a single molecular of such protein to emerge randomly. The chance for the human genome to auto-assemble is 4^-3000,000,000. This is presuming the required small organic molecules are in abundant supply. Then these randomly formed gaint molecules must fold properly and appear in huge concentrations and close approximity to each other and assemble into the correct cellular struture. The chance for that would be 100^(-10^24) or smaller! And even then you got only a dead, decaying brain.

   The only way for sentient brains to arise is through exponential Darwinian amplification, an process far more powerful than cosmic inflation and bubble universe regeneration – Darwinian amplification simply acts faster and over-powers all of these puny cosmic processes. A suitable universe that supports Darwinian processes is the most likely one to be observed by concious minds.

2. **David Nataf**
   January 15, 2008

   Joe,

   What is exponential darwinian amplification?
   It sounds like some new age notion.

   And while there are certainly enough multiverses around (if they exist they're infinite) to have 10^-10^30000 events occur every so often, I personally prefer the hypothesis that these calculations are wrong because they are missing physics/chemistry we have not yet observed or taken into account.

3. **Peter Woit**
   January 15, 2008

   If you think it's worth your time to discuss this kind of calculation, please do it over at Cosmic Variance. I don't want to spend my time trying to figure out how to moderate a discussion that seems to me completely absurd.

4. **Bee**
   January 15, 2008

   Likely, we will be reincarnated as a Simpsonian Brain then. In terms of probability, it's cheaper.

5. **KesheR**
   January 15, 2008

   I don't believe in this, but it's funny and tricky, like every paradox, right or not.

For you Spanish talkers around the world, I have translated the wiki from English:

http://es.wikipedia.org/wiki/Cerebro_de_Boltzmann

6. **Kea**
January 15, 2008

I don't know whether to laugh or cry. Absurdity may be inevitable when physics reaches such a state, but to this level? Thanks, Bee, for the picture of Darth Vader. Cute touch.

7. **Xerxes**
January 15, 2008

> I suspect the title of the piece … indicates some sympathy for skepticism

Don't forget that journalists do not choose the headlines for their stories. That job is done by copy editors (because it's mainly dictated by the constraints of page layout and column width), and the journalists often are unhappy with the results.

8. **Bee**
January 15, 2008

Hi Kea,

Glad you like it, I couldn't resist. A paper titled 'Return of the Boltzmann Brains' seemed to beg for it. I just noticed though that I lost a bet I had running with a friend, the paper didn't get published (within 12 months). And while I am at it, neither did this paper get published nor that. Maybe he just doesn't bother? Best,

B.

9. **rrtucci**
January 15, 2008

And then Physicists wonder why their funding is being cut.

It's sad that these bozos are well funded whereas people trying to do real physics (e.g., quantum computing) go hungry.

10. **Arun**
January 15, 2008

I would expect that the "overall density" (whatever that means) of Boltzmann brains would be the same as in our observable volume of universe.

11. **capitalistimperialistpig**
January 15, 2008

I think that there is a real physics puzzle here despite all the silly rhetoric and anthropic absurdity. The problem is that we don't have any good way to explain the initial low entropy of the Universe. We can take it as given boundary condition, but it does assort somewhat poorly with other notions cosmologists hold dear. I think Sean Carroll is reasonably clear on this.

12. **Eric H**
January 16, 2008

There seems to be a misconception that intelligent people must be nerdy in the most extreme sense. It seems to be an archetype that the public buys into ever since they saw their first Jerry Lewis movie when he was still with Dean Martin. So the more over the top and delusional the idea the more the average joe in the NSF and the hiring committees at universities say to themselves "I don't get this. He must be onto something". Of course it helps if the person with the idea has complete overwhelming confidence in the non-ridiculousness of the idea.

13. **chris**
January 16, 2008

capitalistimperialistpig,

the entropy of the universe is quite easily understandable with the help of classical rg arguments, see e.g. http://arxiv.org/abs/0706.0174v2
only die-hard string believers seem to have problems explaining it.

14. **alex**
January 16, 2008

Lubos has a sane, reasonable and I think correct article on why these sorts of calculations are wrong.

http://motls.blogspot.com/2008/01/new-york-times-about-reincarnation-in.html

15. **oohay**
January 16, 2008

Actually LM's article is completely insane even by his standards. In particular, Sean C goes to great lengths to make it clear that neither he nor anyone else believes in the existence of B Brains, but of course LM thinks that all of the cited authorities do believe this. In fact his whole style of reasoning is very typical of people suffering from paranoia. For example, he thinks that it is wrong to ask why the initial entropy of the world should have been low, since asking such a question means that you are conspiring to subvert the second law of thermodynamics.

Anyway his post serves a useful unifying role: we are told that Sean Carroll's errors are essentially the same as those of people who believe in global warming. Ergo, if you think that LM is full of it on climate issues, you can safely deduce that his views on the foundations of thermodynamics are absolute crackpot material. Which indeed is how people in the field regard them.

16. **Bee**
January 16, 2008

Eric: So the more over the top and delusional the idea the more the average joe in the NSF and the hiring committees at universities say to themselves "I don't get this. He must be onto something". Of course it helps if the person with the idea has complete overwhelming confidence in the non-ridiculousness of the idea.

I have no idea why you think this is the case, but this impression is definitely completely off. Funding and hiring is in my impression still based on very conservative evaluation of a person's research program. If hiring committees would set up their shortlist based on the criteria 'I don't get this.', I'd throw a couple of my unpublished more wacky drafts on the arxiv and stop worrying about getting tenure. Without meaning to insult anybody, I think one can afford working on such stuff if one has reached a certain level of seniority and doesn't have to bother any longer what people think of it. (That is to say, I will leave the wacky stuff in the drawer until I'm 60.) Sean has to make quite some effort to clarify what the actual physics is behind that more bizarre discussion. Media attention might become a factor of increasing importance for funding and hiring – that could indeed be, and it is a development I dislike very much. Best,

B.

17. **Eric H**
January 16, 2008

Bee,
I agree with your last sentence about media attention and consider it an echo chamber between that and funding and hiring decisions by administrators. Ultimately, I was having fun with them because they deserve it for being spineless and sucking the air out of the room for other scientists more deserving. I agree with an earlier comment about using thermodynamics and conservation of information as the final arbiter to decide if something is wrong. Until the people in control of these decisions start doing that they deserve any derision they get. ("Funny" is always better than "rude" in targeting one's derision, and generally more effective.)

18. **capitalistimperialistpig**
January 16, 2008

Chris,

I checked out the paper you mentioned. If I'm not mistaken, the authors assumed that the universe started in a pure state with entropy zero. That should simplify the task of getting an initial low entropy Universe.

19. **Gazouille**
January 21, 2008

Peter & guys,

I'm not on topic but.. well

Today, somehow by clicking on links this site I ended up here
http://www.volny.cz/lumidek/crackpot-not-even-wrong.html
Where did I click was my question :).

Pathetic page, but still I listened to that Susskind's interview.

He's pretty clear on that the only thing wrong with string theory and the landscape is Peter Woit and Lee Smolin. We're saved, phew.

20. **Manuel Pace**
January 21, 2008

"What is consciousness but the animal feeling of being alive."
Charles Saunders Peirce.

21. **John Gribbin**
January 30, 2008

All the people who say that the numbers involved are so extreme that they can be ignored are way off beam. If the multiverse is infinite, anything can happen an infinite number of times, provided it has non-zero probability. No matter how large a number is, it is literally infinitesimal compared with infinity.

22. **jpd**
January 30, 2008

Not quite, there are many types of infinities.
if there are possibilities corresponding to the Real numbers,
and if there are successes corresponding to the Natural numbers:
both are infinite but the probability of success is still zero.

Updates

January 16, 2008
Categories: Experimental HEP News, This Week's Hype

- On the HEP budget problem front, Adrian Cho has an interview with Ray Orbach of the DOE. Orbach is not very encouraging on the prospects for a supplemental FY 2008 appropriation:

> … my assumption is that the last thing that Congress or the president wants is a decorated supplemental. Because, you come in for the Office of Science, and there will be somebody else coming in, and before you know it, the thing will be enormous. … My guess is that it would be very hard to single out a particular program for a supplemental.

He does promise a very healthy FY 2009 proposed increase:

> Now, I can't tell you, obviously, the details of the president's budget for '09, but I can tell you that it will be a wonderful budget request. And because '08 has been difficult for us, the gap between '08 and '09 will be large.

He notes that the DOE and the president are convinced of the importance of supporting HEP research, that the problem is with the Congress:

> I think now the high-energy physics community understands how Congress feels and has a job on its hands to explain why it should be supported at the level of the president's request.

He doesn't shed any light on the continuing mystery of who exactly in Congress made the decision to target HEP for cuts or what their thinking was, leaving it still unclear who it is that the HEP community is supposed to be making its case to. It would be nice to know this before next Christmas, since if this person or person doesn't change their minds before then, most of the US experimental HEP community may want to make permanent plans to either emigrate or go into a different line of work.

I've heard nothing about the effects of the FY 2008 budget on particle theory or string theory funding. Perhaps the plan of whoever is responsible for this is that the US should shift out of supporting experimental HEP research, and concentrate on string theory and anthropic landscape research, where it continues to hold a leadership position.

- Nature has an article entitled Experimental cosmology: cosmos in a bottle about condensed matter analogs of black holes and physics related to cosmology. One topic covered is the recent bogus claim to "test string theory" using interfaces in liquid helium to model branes. Paul Steinhardt notes one of many problems with this idea, that "string branes are flat and attract one another, whereas the helium-3 'branes' are curved and have no attractive force." Joe Polchinski on the other hand is more enthusiastic, since any prediction could have a big impact. "You never know what you might find" he says.
- The string theory hype machine remains in overdrive, putting out nonsense press releases at an unparalleled rate. This week's string theory hype is from Japan, where KEK has put out a press release claiming Interior Structure of a Black Hole Computed Using Superstrings, which tell us that:

> It is expected that superstring theory will develop further and play an important role in solving interesting problems such as the evaporation of black holes, the state of the early universe and the creation of everything.

The actual calculation behind the hype is a numerical simulation of a supersymmetric quantum mechanics system, which is described here.

Comments

1. **c.r.**
   January 16, 2008

   the link to the numerical simulation paper on the arxiv is broken….

2. **Peter Woit**
   January 16, 2008

   Thanks c.r., fixed.

3. **J.F.**
   January 17, 2008

   Even though the answers were not so pleasant, those were some incisive questions, I am impressed. Enough so that I'm going to restart my membership to AAAS. I've always considered our scientific societies as a soft lobby, so this is one way to help. The APS, for example, will help with anyone who wants to meet their congress member or senator (as a constituent) by scheduling the meeting and providing concise talking points.

4. **String Fan**
   January 18, 2008

   You might want to call attention to the KITP program `Nonequilibrium Dynamics in Particle Physics and Cosmology.' So far every online talk has been about the application of AdS/CFT duality to RHIC physics: no hype, it is just that these methods work. For what is arguably the hardest problem in quantum field theory – nonequilibrium strongly coupled physics – string theory has provided the solvable `harmonic oscillator,' which both matches experiment better than any other model, and provides a standard of comparison for any other theoretical approach.

5. **jon**
   January 18, 2008

   Witten an Kontsewich jointly awarded half of crafoord prize for "for their important contributions to mathematics inspired by modern theoretical physics".

   The longer quote finishes with "The laureates have resolved several important mathematical problems related to string theory and have in this way paved the way for its further development." which could be hyped either way I guess depending on where the emphasis in this sentence is put…

6. **Peter Woit**
   January 18, 2008

   "String Fan",

While I think string theory has failed as a fundamental theory, it may very well lead to useful calculational methods in some strongly-coupled physical systems. But please, off-topic overhyped advertisements from anonymous "Fans" aren't really informative, closer to the dozens of spam comments this blog gets each day.

7. **DB**
January 18, 2008

Chapter 4 of the just published National Science Board report helps to put HEP spending in a US and international context here:
http://www.nsf.gov/statistics/seind08/pdf/c04.pdf

As Orbach explained back in September, FY07 was a jump over FY06 which had only progressed 0.9% over previous year. FY09 is set to repeat this cycle vs FY08. But what is important is to see how areas get treated when the budget is tight. In FY09 HEP will probably be ok and people will breath a sigh of relief. They will probably pay up for ITER and its even looking as if NASA might get an extension on the shuttle and so launch the AMS. But this alternating cycle can mislead people, and in tandem with political finger pointing can hide the underlying trends.

One outfit that doesn't get much attention but is undoubtedly one of the master puppeteers in playing off Congress vs the administration is the Office of Management and Budget, a basically independent bunch of bean counters who are like departments of finance in other countries, i.e., their function is to reign in the ambitions of spending departments. They harbour a pathological hatred of all big science projects. Over the last 50 years of cancellations of key US science projects their role has been absolutely pivotal, as in Apollo, Project Nerva, the supersonic passenger aircraft, SSC, Space Station Freedom etc.

The best and most scholarly analysis of the roles the different actors have played during these budgetary crises I've seen is James Dewar's study of Project Nerva: To the end of the Solar System, which despite its apparently narrow focus, gives a thorough and fascinating insight into how budgets for big science projects have been managed historically. His conclusion is that without a very powerful sponsor in the Senate, large science projects invariably get eviscerated by the backroom bureaucrats.

8. **srp**
January 18, 2008

If only the OMB guys had managed to kill the space station. A greater waste of resources is hard to imagine. The SST was also pretty much a no-brainer. The SSC is more of a borderline case, but the overruns on it destroyed its credibility.

You should read Freeman Dyson's essay about big science and small science in Eros and Gaia. It is balanced and wise.

9. **amused**
January 19, 2008

Re. the string hype from KEK: At least these people had the decency to document the worth of their work by publishing it in PRL before issuing misleading hype to the press. Unlike the surfing independent physicist and Boltzmann Brain enthusiasts…

10. **Suvrat Raju**
January 19, 2008

Hi Peter,
This paper is not about supersymmetric black holes. I remember looking at this when it came out and it is about non-supersymmetric Schwarzschild type D0 black holes in IIA theory. This system is dual to Matrix quantum mechanics with 16 supercharges.

While the underlying theory is supersymmetric note, that the black hole itself is not.

Calculating the black hole entropy is a question of simulating this matrix quantum mechanics at strong coupling. The KEK group seems to have done a fantastic job in doing that, and they find answers that match precisely with gravity.

These results have not received much attention, perhaps because they are numerical and the technicalities are hard to grasp for many people, but they seem to be tremendously important.

best,

Suvrat

11. **amused**
January 19, 2008

As I understand it, the main result of the KEK paper is that supersymmetric quantum mechanics with 16 supercharges can now be simulated numerically, and the internal energy agrees at low temperature/strong coupling with a previous calculation (in Ref. [27], from 1996) from a stringy black hole geometry (via gauge/gravity duality) while at high temp/weak coupling it agrees with previous results from high temperature expansion. In the summary of the paper, the authors describe this as significant evidence for gauge/gravity duality in the non-conformal case. They don't seem to be claiming to have calculated any new properties of stringy black holes; rather, they are reproducing the previous result of Ref.[27] via gauge/gravity duality.
The press release gives a completely different impression though…

12. **Suvrat Raju**
January 19, 2008

Hi,

I dont wish to be involved in a long debate here. Where supergravity is valid, the properties of the black holes are, evidently, well described by classical supergravity.

The point is that they have reproduced the predictions of type IIA supergravity in 10 dimensions, from a totally different calculation in 0+1d quantum mechanics. Second, this is a genuine strong coupling calculation, that does not rely on BPS non-renormalization theorems(as many calculations in AdS/CFT — including the original Strominger/Vafa calculation for the D1-D5 system — are forced to do). Finally, the black-hole involved is, itself, non-supersymmetric.

This, I think, is a fairly remarkable and interesting achievement.

best,

Suvrat

13. **DB**

January 19, 2008

srp,

I'm not going to debate the merits of all the projects the OMB has torpedoed, because they have won in that the US has clearly decided that basic science better be cheap if it is to be funded at all. Unlike politicians, the OMB plays a long game, which is why only a senior senator can hope to be around long enough, and be influential enough, to see these multi-year projects through in the teeth of the OMB's (unelected) opposition.

In relation to HEP, killing off the ILC and the Nova experiment tells me that they still have it in for High Energy Physics, that their real target is Fermilab. Others console themselves with the thought that all that's happening is that HEP research is being concentrated at Fermilab. To which I say, don't be fooled by any FY09 sweeteners.

String theory is the perfect answer to the OMB's concerns with HEP. It's really cheap, and doesn't lead to any predictions which can be tested, so no need for expensive labs. It still claims to be HEP, even though many physicists view it as mathematics masquerading as physics, as evidenced by the application of Ads/Cft duality to RHIC physics: big deal, it's just another mathematical tool and further evidence that the string theory program can create useful mathematical tools. It's also environmentally friendly.

14. **amused**
January 19, 2008

Hi Suvrat,
What you wrote seems to be in agreement with my previous comment, so I'm happy to agree with it. And also about this work being a remarkable and interesting achievement. (I'm less enthused about the press release though.)

15. **Another String Fan**
January 19, 2008

Indeed, this paper looks like serious and hard work. They continue the efforts of (and claim to do better than) Kabat (your colleague at Columbia University, Peter), Lifschytz and Lowe. The matching of the 12-year-old Klebanov-Tseytlin calculation (Ref. [27]) of the charged 10-d black hole entropy looks quite good, but to be even more sure about the scaling with temperature one needs to reach even lower T. The press release is somewhat strongly worded, and will not get a prize for scientific journalism, but we cannot blame the KEK for being proud of their hard work, and of their Hitachi supercomputer. Some amount of `hype' is inevitable because the science writers think that they have to `rise above the noise' and to address those who have heard of Hawking but do not know much else.

Also, Peter, I wish you stopped trashing the press releases about some of the recent PRL's. It is a current PRL policy to encourage the authors to contact their local press offices to try to inform the general public of their work. The authors of a few such recent press releases were just following these instructions. These press releases might be imperfect, but in the judgement of PRL (a flagship journal of the APS) they serve a useful purpose of educating the public. They have to involve a very recent paper, so it is always too early to tell how important this work will be in the long run. But what is wrong with trying to give people some glimpses of current research in theoretical physics?

16. **Peter Woit**
January 19, 2008

Another String Fan,

First of all, if you're actually a professional physicist who knows something about this subject, I really wish you would consider post comments like this using your real name and not hiding behind anonymity and referring to yourself as a "string fan".

Sorry, but I intend to continue to trash over-hyped press releases about string theory based on PRLs. I don't believe this is about "trying to give people some glimpses of current research in theoretical physics", it's about a continuing campaign of claims of string theory "progress" towards a unified theory that is not honest, and that is designed to try and mask the fact that this is a failed research program. I don't think you inform the public or get their respect by putting out press releases that are not true, the effect of this is quite the opposite.

17. **just a fan**
January 19, 2008

I was wondering if there was any way an average person could help get congress to restore your funding?

18. **Zathras**
January 20, 2008

DB,

The Office of Management and Budget (OMB) is not an independent in any way; it is part of the executive branch and submits exactly what the president want it to submit. What you are describing sounds more like the General Accounting Office (GAO), although I do not think they had anything to do with this. This cutting decision was done by the Congressional Budget Office (CBO).

PW: *….the continuing mystery of who exactly in Congress made the decision to target HEP for cuts or what their thinking was….*

This statement disguises a bit how decisions are made. The problem wasn't that anyone was *against it*; it was that there was nobody *for it*. Without a patron in the CBO this type of cut was inevitable.

19. **eddie**
January 20, 2008

I get the impression that 'big science' isn't really about science at all. The 'big' part is about the flow of cash;

tax dollars –> construction co's –> campaign contributions

Outside of the community of researchers, the debate about SSC, and other projects, was about which politician could get it built on their patch for electoral kudos.

I suspect that the OMB was set up to cut out the middle man…

20. **J.F.**
January 21, 2008

just a fan,

If you want to help, on thing you can do is write your senators and members of congress a letter (actual paper, not email). Contrary to what some believe it is NOT a futile act. My understanding is that the staffers for people on Capitol Hill often include bright, knowledgeable people who would like to support (for example) basic research science, but find it difficult to do so without an outpouring of constituent support. Your letter may even carry more weight as a patron than one of the many from people employed in the profession.

Here is a website that has some info in case you need it:

http://www.citizenredress.com/

Thanks for asking!

21. **DB**
January 22, 2008

Zathras,

No, I was and am thinking of the OMB, not the CBO or the GAO, although I don't rule out some minor involvement by the CBO. Incidentally, the CBO was set up by Congress in 1974 in order to take some power back from the OMB – which Congress correctly perceived was usurping its constitutional authority – by establishing a non-partisan watchdog and analysis body to keep an eye on the OMB. But the CBO doesn't prepare budgets so what a "patron" would achieve there is unclear to me. HEP needs a senior senator on its side in the way that NASA has senators fighting its corner. Now that most US terrestrial HEP is concentrated in Illinois, Senators Durkin and Obama are the obvious choices.

The OMB aggregates the spending desires of the executive departments, then makes recommendations on those programs which should be cut to meet the overall budget targets and goals. It is here, and in the continuing to and fro with Congress, that OMB officials get to influence projects they deem undesirable. I suspect Orbach spends as much time arguing with the OMB as he does with congressional appropriations committees.

Here is the presentation his staff made sometime in 2006 to the OMB:
http://www.science.doe.gov/ober/berac/staffin04_05.ppt
I draw your attention to slide 22 which shows the variance of FY06 vs FY05 and ask, plus ca change?

just a fan,

Senator Durbin is up for reelection in 2009 so is an obvious target for lobbying.

Science, and a Bit of Religion

January 22, 2008
Categories: Uncategorized

Some things I've run across in the last few days:

This month's Scientific American is devoted to The Future of Physics, a special report made up of three excellent articles on the LHC machine, the standard model and its discontents, and plans for the ILC. The articles all avoid hyping string theory or other science fiction and instead stick to real, serious material about HEP. Congratulations to the people at SciAm, everyone should go out now and buy a copy of this month's magazine to encourage them. Chad Orzel and other non-HEP physicists are peeved that the "Future of Physics" business is a misnomer, since it should have been titled the "Future of HEP Physics". They're right. Still, the articles are good, so they shouldn't hold the headline business too much against the magazine.

The LHC still has no up-to-date schedule available, but looking at the latest news about the commissioning and comparing to old schedules, it seems to me that if all goes well from now on, the machine should be cooled down and ready to be checked out in late summer, with beam commissioning during the fall, and, maybe, a short physics run late in the year.

Witten and Kontsevich were awarded the Crafoord prize, for "for their important contributions to mathematics inspired by modern theoretical physics". This is certainly well-deserved for both of them. One doesn't know where to start in listing Witten's contributions of this kind, and Kontsevich's ideas about "homological mirror symmetry" have had dramatic impact on mathematics, leading to a whole new field of study. There's an article about this at Science where Witten claims to be "totally startled" to be recognized for his achievements in mathematics. Not sure why a Fields medalist would get startled about this… Witten and Kontsevich get $125,000 each.

The first Eisenbud prize for a paper written during the last 6 years that brings together mathematics and physics was awarded to Ooguri, Strominger and Vafa for their 2004 paper Black Hole Attractors and the Topological String. They share $5000.

Last week there was a conference at the Fields Institute in Toronto on Mathematical Physics and Geometric Analysis, featuring series of talks by Victor Guillemin and Shlomo Sternberg, with lecture notes available online. Sternberg's lectures give a careful discussion of some of the differences in conventions between physicists and mathematicians, the Higgs mechanism and Weinberg angle, various facts about spinors, and the models he worked out with Ne'eman that use superconnections to unify Higgs and fermions. In these models he gets a prediction of the Higgs mass, as twice the W-mass, about 160 Gev. Coincidentally, Tommaso Dorigo reports on the Higgs search at the Tevatron, which is getting close to being able to exclude the existence of a Higgs in a small energy range: around 160 GeV.

From Dave Bacon, an odd story about a recent arXiv withdrawal. I have no idea what this is really about.

The Templeton Foundation continues to spend a lot of money on a wide variety of projects, many having something to do with physics. At FQXI, the request for proposals to be funded in 2008 is now closed. Their community web-site includes several interesting articles on topics in theoretical physics. In "breaking news", they report proudly that several of their members were in the recent NYT article on Boltzmann Brains.

Also funded by Templeton is the CTNS STARS (Science and Transcendence Advanced Research Series) grant program, which recently announced $100,000 grants to five groups, one of which includes my Columbia colleagues Brian Greene and philosopher David Albert.

Yet another Templeton-funded endeavor is a new science and religion library at Cambridge University, where, at a cost of $2 million or so, the International Society for Science and Religion will choose 250 books for the library, coming out to about $8000 each. I haven't yet heard from them, but will be happy to provide a copy of my book at a modest fraction of that price.

Comments

1. **a quantum diaries survivor**
   January 22, 2008

   Hi Peter,

   about science and religion, I would like to mention the recent querelle with the failed visit of Pope Ratzinger at the Rome University "La Sapienza".

   In a nutshell, the pope had been invited to give a speech at the starting ceremony for the new academic year. A public letter of dissent by Marcello Cini, a senior professor of physics in Rome, had been followed by a private, shorter one where 67 other physicists – among which several big names – expressed to the rector their support of Cini's dissent, arguing that Pope Ratzinger's lectio magistralis would be a incongruous way to open the works of the University, a place of autonomous research, the more so given the opinions of Ratzinger on the process to Galilei, which he had expressed 17 years earlier, when still a cardinal. Ratzinger's reactionary views however are known to most.

   The letter was made public two months afterwards. This created a wave of protest, with students manifesting against the visit, many other academics joining the petition of the 67 physicists, and the pope finally declining the invitation.

   The aftermath shows a real defeat for scientists, citizens, politics: on one side we have cardinals and clerics lamenting that the pope was prevented from speaking (as if he does not speak to a billion people every saturday), and accusing scientists of oscurantism. On the other side, we are assisting at the lynching of a few of the signers of the private letter. In particular one very bright scientist, Luciano Maiani, former director of CERN and INFN, recently nominated to head the CNR – a very important research institution in Italy – has to defend against accusations from catholic politicians who want to bar him from the nomination.

   I really feel for this crazy situation… And not accepting Maiani as head of CNR is a disaster. He is by far the best candidate for the job.

   (read more in my blog if you wish, including the interview to Maiani).

   Cheers,
   T.

2. **Peter Woit**
   January 22, 2008

   Thanks Tommaso,

   You're right, I should have mentioned this story, since it's about a rather more influential religious organization than Templeton. I encourage people to read about this over at Tommaso's blog, and to discuss it there….

3. **Chris W.**
January 22, 2008

I assume you meant David Z. Albert, Frederick E. Woodbridge Professor of Philosophy at Columbia.

4. **Peter Woit**
January 22, 2008

Chris W.,

Oops, yes. Spelling fixed.

5. **Kris Krogh**
January 22, 2008

Dave Bacon's story concerns a suspicious paper attacking Gravity Probe B, a NASA experiment measuring two effects predicted by general relativity. After many delays, their results for the Lense- Thirring effect (also called frame-dragging or gravitomagnetism) are scheduled to be announced around June. Various parties have claimed previous measurements, using other satellites, although each has been touched by dispute in some way.

The author of the paper in question was given as:

Gerhard Forst
FGP
Behrenstr. 1
10117 Berlin

Finding no trace of this person or an organization called "FGP," I wrote ArXiv questioning the paper's authorship. (Copy of the message in the comments here.) My guess was that actual author was one of the Gravity Probe B competitors, whose work I've discussed here. The motive would be to undermine the GP-B result in case his previous measurements are not borne out.

As Dave mentions, the comments added by ArXiv after withdrawing the paper seem to point the finger at someone else. They may know something I don't. Certainly its a bizarre situation!

6. **Coin**
January 22, 2008

As far as the anti-Gravity Probe B paper by "Gerhard Forst" goes: Was the paper actually any good? I mean, did it make a solid point?

7. **Kris Krogh**
January 22, 2008

Hi Coin,

I didn't find anything of substance in the paper, but plenty of fabrication. There were references to some old papers, claiming those predicted the specific problems Gravity Probe B has been having. I looked those up, but there was nothing related to the actual problems experienced. The author also claimed no one except those those connected GP-B will be able to analyze the data for themselves. All data connected with the experiment are going into a public archive, so that's nonsense.

8. **Coin**
January 22, 2008

Ouch.

9. **YBM**
January 22, 2008

The Bogdanov's brothers just begin to promote Lubos Motl's book "L'Équation Bogdanov" ("The Bogdanov Equation") on low grade TV shows these days (very very low). Nothing much about the book itself besides that "It's not written by us" and "It is written by a professord at 'Harvard' "…

The book is supposed to be available in bookshop this week.

10. **Tom Whicker**
January 23, 2008

I just got a promo in the mail from Sci American. If I re-subscribe, I get a special bonus report called "Parallel Universes". The ad copy says " Is there a copy of you in another universe reading this sentence? The most popular cosmological model today suggests the answer is yes………Welcome to Parallel Universes which examines each of four possible configurations for such a multiverse"

11. **Alex Mikunov**
January 23, 2008

Peter, shouldn't it be: Witten and Kontsevich get $125,000 each

12. **Peter Woit**
January 23, 2008

Tom,

Thanks, quite a mixed bag there now at SciAm…

Alex,

Thanks, mistake corrected.

13. **Professor R**
January 23, 2008

hi Peter, excellent blog as always…
One query – are you saying the FQXI foundation is funded by Templeton? I managed to miss this, despite throwing in an application…Cormac

14. **Peter Woit**
January 23, 2008

Professor R,

Yes, FQXI is funded by Templeton. I believe that their hope is that this is just an initial grant, that they will find other sources of funding in the long term.

15. **Clark**
January 25, 2008

Regarding the title of the recent Scientific American articles, I find it refreshing that the "Future of Physics" is not String Theory. As a lay reader of popular physics, I've encountered many such announcements over the last 15-20 years, only to discover that the fabulous future was String Theory and all its strange, untestable components. Weary of all the string cheerleading, I expected more of the same. What a pleasant surprise to read the recent Scientific American articles.
I can understand where non-HEP physicists would be upset, but there is a perception in the interested public that String Theory is the light that will lead us out of Plato's Cave, largely due to mainstream publications using similar titling to promote it.
What a shock for Joe Public Broadcasting to find out that the future may have a different angle.

16. **DB**
January 25, 2008

Refreshing also, that Chris Quigg has written the article on the Standard Model and its Discontents. Fermilab's Quigg is a fine popularizer of the subject and someone who has been committed to outreach for a long as I can remember. He will also be familiar to physics professionals from his 1980s classic: Gauge Theories of the Strong, Weak and Electromagnetic Interactions, still in print, and I understand it is due to benefit from a completely new edition in 2008.

2007: Year of the Unparticle

January 25, 2008
Categories: Uncategorized

One can get some idea of what progress there might have been in particle theory during 2007 by querying the SPIRES database for 2007 papers that already have lots of citations. Doing

find topcite 50+ and date 2007

turns up 20 papers of which 6 are experimental papers. Remarkably, the 14 other papers are all about one topic: unparticles. These all refer to Howard Georgi's initial [Unparticle Physics] paper from March 2007, in which he describes a possible effective field theory that would be scale invariant and correspond to unusual phenomena potentially observable by collider experiments, phenomena he describes in terms of "unparticles". In less than a year Georgi's paper has accumulated 118 citations, with the blogger at Resonaances [making fun] of the phenomenon of "unpapers" with abstracts such as:

> We consider unparticles in whatever uncontext. You are encouraged to forget the paper as soon as soon as you add it to your citation list.

but also making the very relevant comment that this does seem to be getting attention because it is a legitimately new idea:

> I must give the credit to Howard for drawing our attention to a whole wide class of collider signatures. Besides, I appreciate Howard's writing style. He is probably the last man on Earth who truly enjoys particle physics.

As far as I can tell, unparticles don't solve any of the problems of the Standard Model, but they are theoretically possible phenomena of a different kind that experimentalists can look for, and having as many as possible of such phenomena is very worthwhile. The more different things people are looking for, the more likely they'll find something unexpected that otherwise might not have been noticed.

This week New Scientist has a long and quite good [cover story] about unparticles, and recent attempts to use them to explain dark matter, which ends with:

> Georgi reserves judgement on whether his unparticles really could be the key to solving the dark matter problem until more work is done, but he's pleased that people are investigating the possibility. "All I knew was that I had found something cool and I wanted other people to take a look and see what kinds of weird things they might be capable of doing – what mysteries they might solve," he says. "I'm happy because that's exactly what people are now doing."

The story has also made it into the [Telegraph].

Besides the unparticle phenomenon, there appear to be very few 2007 theory papers that anyone is paying much attention to. I've tried to search around and come up with a list of 2007 papers that have so far gotten 25 citations or more, and a list follows. I'm probably missing some . The main themes shared by most of these papers are AdS/CFT and attempts to construct metastable vacua as part of a study of the landscape .

- Metastable vacua and D-branes at the conifold (Argurio, Bertolini, Franco, Kachru) 54 citations.
- Gluon scattering amplitudes at strong coupling, (Alday, Maldacena) 47 citations.
- The Bulk RS KK-gluon at the LHC, (Lillie, Randall, Wang) 38 citations.
- Supersymmetry breaking, R-symmetry breaking and metastable vacua, (Intrilligator, Seiberg, Shih) 35 citations.
- Electroweak constraints on warped models with custodial symmetry, (Carena et al.) 33 citations.
- The Supersymmetric Parameter Space in Light of B-physics Observables and Electroweak Precision Data, (Ellis et al) 31 citations.
- Simple Scheme for Gauge Mediation, (Murayama, Nomura) 31 citations.
- Non-perturbative and Flux superpotentials for Type I strings on the Z(3) orbifold, (Bianchi, Kiritsis) 31citations.
- Phase Structure of a Brane/Anti-Brane System at Large N, (Heckman, Seo, Vafa) 30 citations.
- Thermodynamics of the brane, (Mateos, Myers, Thomson) 30 citations.
- On the Singularities of the Magnon S-matrix, (Dorey, Hofman, Maldacena) 29 citations.
- On the Strong Coupling Scaling Dimension of High Spin Operators, (Alday et al.) 29 citations.
- Charged Lepton Flavour Violation and (g-2)_mu in the Littlest Higgs Model with T-Parity, (Blanke et al.) 29 citations).
- Split states, entropy enigmas, holes and halos, (Denef, Moore) 28 citations.
- Computation of D-brane instanton induced superpotential couplings, (Cvetic, Richter, Weigand) 28 citations.
- Towards an Explicit Model of D-brane Inflation (Bauman et al.) 27 citations.
- MadGraph/MadEvent v4: The New Web Generation, (Alwall et al.) 26 citations.
- Physics of String Flux Compactifications, (Denef, Douglas, Kachru) 25 citations.
- A Measure of de Sitter entropy and eternal inflation, (Arkani-Hamed et al) 25 citations.
- Explaining the Electroweak Scale and Stabilizing Moduli in M Theory, (Acharya et al.) 25 citations.

SPIRES has yet to compile a 2007 "topcites" list, but it looks like the pattern should be very much the same as the last few years:

- increasing dominance of research into AdS/CFT (597 citations of Maldacena's paper in 2007, versus 551 in 2006)
- particle theory basically died at the end of the 20th century with the only post 1999 paper getting more than 150 citations the KKLT one reflecting the rise of landscape research.

Comments

1. **piscator**
   January 25, 2008

   the positive side of 2007 is the preparation of lots of people for the model bonfire that the folks in geneva are warming up (or rather cooling down) for. frankly, the last thing particle theory needs now is another over-hyped and over-cited model solving all open problems (insert your favourite example)

   i think there is a lot of retooling going on in preparation for the LHC, there will soon be a large parting of the ways between those primarily interested in physics as an experimental subject and those interested in more formal topics.

2. **Big Vlad**
   January 25, 2008

   piscator is right. There's a big divide at the moment between the more formal side of particle physics, which seems to be in the doldrums, and the experimental/phenomenological side which is booming thanks to the forthcoming LHC. Of course, the former should feed off the latter and hopefully this will be the case over the next few years.

3. **Bee**
January 25, 2008

We had a post on the topic in September, see [Unparticles].

4. **Domenic**
January 25, 2008

Hey, how cool—today is exactly the day that Georgi arrives at Caltech (where I'm currently being an undergrad) to give a talk on unparticles. I put it on my calendar because it looked more interesting/introductory/understandable than most talks, so I've actually been looking forward to this for a week. Nice to know that it's an interesting idea; should be fun!

5. **grad student**
January 25, 2008

Two (I think) interesting papers missing from your 2007 list

Mateos, Myers, Thomson

http://arxiv.org/abs/hep-th/0701132 30 citations

Denef and Moore

http://arxiv.org/abs/hep-th/0702146 28 citations

there are probably others…..

6. **Peter Woit**
January 25, 2008

grad student,

Thanks a lot, I've probably missed quite a few. Just added those two, information about any others that should be added is very welcome.

7. **hepph**
January 25, 2008

Quite a few papers missing from the hep-ph side that *weren't* unparticles. Granted these don't necessarily all resemble theoretical breakthroughs but they represent a sampling of things that the community is working on:

J.~R.~Ellis, S.~Heinemeyer, K.~A.~Olive, A.~M.~Weber and G.~Weiglein arXiv:0706.0652 31 citations

B.~Lillie, L.~Randall and L.~T.~Wang hep-ph/0701166 38 citations

M.~Blanke, A.~J.~Buras, B.~Duling, A.~Poschenrieder and C.~Tarantino hep-ph/0702136 29 citations

M.~S.~Carena, E.~Ponton, J.~Santiago and C.~E.~M.~Wagner hep-ph/0701055 33 citations

H.~Murayama and Y.~Nomura hep-ph/0701231 31 citations

J.~Alwall {\it et al.} arXiv:0706.2334 26 citations

8. **alex**
January 25, 2008

Acharya, Bobkov, Kane, Kumar, Shao; hep-th/0701034 25 citations

9. **David Nataf**
January 25, 2008

In terms of particle physics in 2007, I think an interesting result would be the Auger experiment's confirmation of the GZK effect.

10. **stan**
January 26, 2008

Initially, "Unparticles" seemed like a joke to me but now after all these citations, I can`t even laugh. Add an `Un` to every particle physics process and write a paper and cite everyone that wrote before you. I might write an UNString theory paper to see what is going on.

11. **grad student**
January 26, 2008

Another interesting one (although it is not exactly "particle physics")

Elvang and Figueras

http://arxiv.org/abs/hep-th/0701035 25 citations

12. **Peter Woit**
January 26, 2008

hepph and alex,

Thanks for the additions, it is true that they reflect a wider variety of activity in hep-ph than in hep-th.

grad student,

Thanks for pointing that out, but I won't try and keep track of papers like that one that are much more gr-qc than hep-th.

13. **Eric**
January 26, 2008

Perhaps you should add the string phenomenology papers hep-th/0703028 and arXiv:0707.1871 to your list.

14. **Peter Woit**
January 26, 2008

Thanks Eric,

I added the first of those, the second only has 15 citations so far, below the cutoff of 25.

To make explicit the obvious, the list compiled here is weighted towards papers from early in the year, which have had more time to accumulate citations. As such, to rank them by how much they're being cited, you really should weight by when they appeared. My idea in accumulating this list was more to see what papers are getting significant numbers of citations, even if in a crude way. One thing this crude method does is it tends to ignore papers from late in the year, but in any case it is too early to get much of an idea of how often they'll be cited as time goes on.

To make explicit the even more obvious: citation counts are about sociological, not long-term scientific impact.

15. **Jon Lester**
January 27, 2008

I find very silly to try to consider importance of papers in a year by their citation number. In about 30 years the only criterion used by particle physics community to decide what is good and what is bad is fashion. This is clearly seen also in the peer-review process and I think the merit of arxiv and its creator will be manifest in a few years when we will realize that the most important papers are those that were systematically overlooked in the year they appeared and was also rejected by the highest ranked journals.

Jon

16. **Somdatta Bhattacharya**
January 27, 2008

This idea of unparticles, it's utter nonsense. Does it solve any of the outstanding problems of SM? Is there any motivation for it coming from anything at all?

17. **piscator**
January 27, 2008

For the clearest evidence of why citation counts do not directly measure scientific impact, do the following:

1. Look up the two Randall-Sundrum warping papers.

2. Take the ph paper, convert the 'p' to a 't', and look up the hep-th paper with the same number.

3. Look at the topic of this paper and the papers that cite it, and decide for yourself where these citations were intended.

Conclusion: citation counts sometimes measure neither scientific merit, nor sociological trends, but rather the touch-typing proficiency of particle theorists 🙂

18. **string-dude**
January 27, 2008

Hi Somdatta, why should it solve the "outstanding" problems of the standard model for it to be interesting?

Unparticles are talking about a possible low energy phenomenon which might have gone un-noticed because we never thought of looking for it. This in itself is a valid thing to consider, whatever ones prejudices against high energy physics might be.

But the fact that you claim that unparticles are "utter nonsense", an THEN ask questions about it, makes me think that you form your opinions a tad quicker than I do…

19. **Somdatta Bhattacharya**
January 27, 2008

Dear String dude,
Just because I wrote the questions after the statement doesn't necessarily mean that the latter is not a consequence of the former.
One could come up with numerous theories or scenarios that supplement the SM but there must have to be a "motivation" for them. For me investigating them just because they are a possible scenario with possible experimental signatures is like putting the cart before the horse. Since experiments at the next energy scale are near, it makes no sense to just go on increasing upon the number of possible scenarios that might be there, especially when there is no motivation to do such things. If it solved or aspired to solve some problem like the hierarchy problem or that of the abundance of parameters in the SM or the unification one, I would see a point to it. Otherwise its just a load of c***.

20. **gambler**
January 27, 2008

Somdatta Bhattacharya, if you want to win the lottery, you have to buy the tickets before the drawing.

21. **Thomas D**
January 27, 2008

Neubert had a nice paper 0708.0036 showing that 'unparticles' are actually rather similar to strongly interacting particles, in terms of the supposedly distinctive experimental signature.

Also, as soon as you couple them to the SM their conformal invariance will be explicitly broken (whether to a small or large extent), which somewhat, or completely, spoils their original neat rationale.

So between experiment and theory, I don’t know what is supposed to be attracting us towards them. As I said some time ago: there is a continuous infinity of field theory models of new physics, we would like to have some coherent rationale to choose some directions in this infinite space of theories. Experimental considerations do play a role: if a model has clear collider signatures that otherwise would be overlooked, for example. It’s not clear to me whether there really is one clear signature of unparticles.

Of course, had the LHC turned on in 2005-6, as was hoped some time ago, there would very probably be no-one saying ‘unparticle’ today at all. Theorists need something to keep their minds active.

22. **Somdatta Bhattacharya**
January 27, 2008

Thanks Thomas D,
For answering gambler’s question.
However, yes, theorists might need to keep their minds active, but it would do them helluva lot of good if they, especially people like Georgi, came up with things that are worthwhile, otherwise its a waste of time not only for them, but for the whole community. Because had I published such a paper, no one would have payed any attention, but since it came from Georgi, everyone decided to latch on.

23. **string-dude**
January 28, 2008

>Because had I published such a paper, no one would have payed
>any attention, but since it came from Georgi, everyone decided to
>latch on.

I agree that there is truth in this statement, but I find it hardly worth getting so upset about. Science is an enterprise that involves primates working in groups. Mavericks and rebels have to work their way up the hierarchy by paying their dues. I promise you that once you produce a paper of sufficient import, your next paper, even if it is mediocre, will have a fan-following.

Mind you that I am not endorsing this phenomenon. But I somehow think that this meta-level anger, righteous indignation, etc., is completely useless. The only real way in which to make sure that people work on good ideas is to OFFER them a good idea to work on. What is the point in bitching about other ideas? The oldest rule of progress in science is to work on *something* rather than nothing, and I can’t really blame the troops for latching on to a moderately new idea.

That said, I looked at the paper that Thomas D talked about and agree that unparticles suddenly seem a lot less interesting than what I had thought after reading the original paper by Georgi (which I bothered to do only after seeing Peter’s blog).

Sleep tight and don’t let the bedbugs bite,
Chethan Krishnan (aka string dude)

24. **Somdatta Bhattacharya**
January 28, 2008

>I promise you that once you produce a paper of sufficient import, >your next paper, even if it is mediocre, will have a fan-following.

I hope not everybody thinks like this. In the enterprise of physics, it’s not fan-followings that one is after, but the truth. Mediocre papers ought to get the treatment they deserve, which is complete disregard.

>What is the point in bitching about other ideas?

In case you haven’t noticed, this IS the place to bitch about other ideas, to thrash things out with people like you.

25. **Professor R**
January 28, 2008

hi Peter,
I was delighted to see one of the top-cite papers you list (Supersymmetry Breaking, R-Symmetry Breaking and Metastable Vacua) gives the proper reference for the O’ Raifeartaigh Model for a change – and so it should, as the model is heavily referenced and mentioned on every page!

It strikes me once more that one of the drawbacks in the way citations are counted is that there is no distinction between a passing citation, and a model that is the central to the entire paper…part of the reason I still having difficulty persuading Dad’s own countrymen that his work was important….Cormac

26. **Coin**
January 28, 2008

Neubert had a nice paper 0708.0036 showing that ‘unparticles’ are actually rather similar to strongly interacting particles, in terms of the supposedly distinctive experimental signature.

Also, as soon as you couple them to the SM their conformal invariance will be explicitly broken (whether to a small or large extent), which somewhat, or completely, spoils their original neat rationale… It’s not clear to me whether there really is one clear signature of unparticles.

Hm, so this is very interesting to me. So is there *ANY* detectable difference between a SM+unparticles unparticle and a strongly-interacting particle? That is to say, if we detected a particle signature for some new strongly-interacting particle, would there be grounds to distinguish whether it was an actual particle or a disguised unparticle?

I guess I’m still trying to understand exactly what the unparticle stuff is. I’d be curious to know whether I have this right: It appears that an unparticle field is a just a QFT field (I.E., you can’t ever have say an electron unparticle– qft fields consist of either all particles or all unparticles) except that field has a particular property (conformal invariance) which is mathematically well-defined, but which we don’t ever consider in normal physical theories. Is that about it?

Given (let’s assume that we go looking for unparticles and don’t find any) that there don’t seem to be any unparticles in nature, is this something that needs to be explained? Is there some natural or obvious reason why only fields without the unparticle property would be selected to exist in nature, besides “that’s what we observe”?

27. **David B.**
January 28, 2008

Coin:

Unparticle events would just be like hadronic jets in a hidden sector. Jets usually involve many particles in the final state and give different energy curves for how much energy was dumped into them than single particle production. If you can not see the particles, your only signature is missing energy. So long as the perturbations from conformal invariance are small, the `unparticle' calculation is for the most part fine, unless you are below treshold for production of unparticles (which could have a tiny mass).

Unparticles is in the end just another nickname for conformal field theory (in practice all you need is an approximate scale invariant sector). It is the fact that conformal field theories have scaling properties that lets you do a calculation without a specific model of high energies. This is why it is looked at. It covers many possible models of ultrahigh energy physics with very few parameters (a scaling dimension, and the strength of some coupling constants).

Regarding the use of Conformal Field Theories in the rest of physics, I suggest you look at the theory of second order phase transitions in condensed matter systems: those are conformal field theories that can be realized in laboratories, and the scaling exponents can be measured quite precisely.

Unparticles do not have to be there. They are also not forbidden from being there. There doesn't necessarily need to be an explanation for why nature is the way it is. That is a theoretical prejudice. You have to be open to the idea that there might be surprises in physics and some might just have to be due to no particular reason whatsoever. If you care about phenomenology, you want to figure out which of all the possibilities are realized in nature. You might have to do that by excluding all theories on a case by case basis, in which case you need to have a proper accounting of all the possibilities out there.

28. **Professor R**
January 30, 2008

Peter, re your comment
"This week New Scientist has a long and quite good cover story about unparticles, and recent attempts to use them to explain dark matter"…
I agree it is a nice article and thanks for pointing it out.
However: did you notice the clanger in the introduction?
"Georgi…pioneered suspersymmetry, a theory he proposed in 1981 with Stavros Dimopoulos at Stanford University…."
Surely this is quite a howler!

Most particle physicists agree that the theory of supersymmetry was originally proposed in the late 1960s by Soviet theorists Gol'fand and Likhtman, and developed by Julius Wess and Bruno Zumino in 1973. Indeed, simple models of supersymmetry breaking (the O'Raifeartaigh Model) had already emerged by 1975.

Presumably the NS meant that the first realistic supersymmetric version of the Standard Model was proposed in 1981 by Georgi and Dimopoulos….the trouble with errors like this is that one is left wondering how accurate the rest of the piece is…..Cormac

29. **Peter Woit**
January 30, 2008

Cormac,

I did notice that howler. Georgi certainly isn't responsible for the idea of supersymmetry, or even for supersymmetric gauge theories, which were well known by 1981. From what I remember, he and Dimopoulos were working on supersymmetric versions of GUTs, and I guess as part of that may have been the first to note that one should think about the minimal supersymmetric extension of the Standard Model, together with all possible soft breaking terms (the so-called MSSM).

30. **Martin Bauer**
February 1, 2008

I don't really get whats the new idea here. There are a lot of papers out there, assuming hidden sector physics. Is it the conformal invariance? If so, I don't even get the point why it should be (only) conformal invariant. Why not just take some arbitrary symmetry group?

(I should probably add that all my knowledge in the field comes from a talk I just attended.)

31. **Hrvoje Nikolic**
February 7, 2008

As I argue in http://xxx.lanl.gov/abs/0801.4471
unparticle can be viewed a particle with an arbitrary mass.

This Week’s Hype

January 28, 2008
Categories: This Week's Hype

This week’s press release trumpeting a bogus “test of string theory” comes from the University of Illinois at Urbana-Champaign, which is headlined [Scientists propose test of string theory based on neutral hydrogen absorption](), and informs us that

> Ancient light absorbed by neutral hydrogen atoms could be used to test certain predictions of string theory, say cosmologists at the University of Illinois. Making the measurements, however, would require a gigantic array of radio telescopes to be built on Earth, in space or on the moon.
>
> String theory – a theory whose fundamental building blocks are tiny one-dimensional filaments called strings – is the leading contender for a “theory of everything.” Such a theory would unify all four fundamental forces of nature (the strong and weak nuclear forces, electromagnetism, and gravity). But finding ways to test string theory has been difficult.
>
> Now, cosmologists at the U. of I. say absorption features in the 21-centimeter spectrum of neutral hydrogen atoms could be used for such a test.

One peculiar aspect of this press release is that it seems that the relevant paper is not yet publicly available. Supposedly it has been submitted to PRL and accepted, but it has not yet appeared in PRL, and I don’t see a preprint on the arXiv (the authors do have a PRL paper with an arXiv preprint from last year on a different topic, one that also came with a press release, but this one didn’t mention string theory).

As far as I can tell from the press release the idea behind this “test of string theory” is the same as lots of other similar ones that invoke cosmic strings. Among the huge variety of string theory backgrounds and the many possible ways to try and use such backgrounds to model the big bang, some will (just like some non-string theory GUT models) produce macroscopic “cosmic strings”. Astronomers have looked hard for evidence of such things and found none, but one can always imagine that, miraculously, such things exist, with characteristics exactly such that they wouldn’t have shown up so far, but would in some new, improved astronomical observations. In this case, I guess to come up with some new possible observation not already ruled out, the authors of the paper invoke a possible radio telescope with an area of a thousand km^2.

There seem to be at least a couple reasons for the recent flood of bogus “we’ve found a test of string theory” press releases. One is that PRL evidently encourages authors to issue press releases whenever they have a paper appearing in PRL. Another reason is that string theorists are on the defensive, and some of them have decided that finding some way to claim that string theory really is testable, no matter how dubious, is the way to fight back. Earlier this month, one such claim hyped in New Scientist carried the headline “slammed for their failure to explain how our particular universe came to exist, string theorists are fighting back.” In an interview with string theorist Thibault Damour in today’s edition of the Swiss paper le Temps, he promotes three possible tests of string theory. One is the possibility (which he describes as “very speculative”) that one might observe extra dimensions at the LHC, another is cosmic strings, and finally there are his claims that string theory leads to violations of the equivalence principle. Lubos Motl strongly disagrees. Lubos also has a posting about this latest hype, where he comments:

> Such possibilities highlight that creative people may often solve questions that look too difficult at the beginning. They also emphasize how incredibly idiotic are the aggressive crackpots’ proclamations that modern theoretical physics in general and string theory in particular is untestable.

Not clear who it is who believes that “modern theoretical physics” is untestable. While at Lubos’s blog, you might want to see what you can make of his posting on his new book *The Bogdanov Equation: the secret of the universe?*

Update: This story is appearing lots of places, including the UPI newswire, and at Wired, where the writer seems to realize that the bogosity level here may be problematic, including the unusual disclaimer:

> Disclosure: I have no idea whether this makes sense.

Update: A correspondent points me to another recent “test of string theory”, one where for some reason the authors don’t seem to have issued a press release. The article is Toward a test of string theory using Rydberg atoms, and it begins by referencing my book and then claiming that

> … measurable effects are predicted by String Theory on normal quantum scales, which the current criticisms have apparently overlooked.

What is discussed in the paper is actually not string theory, but just the idea of adding spatially non-commuting terms to the Heisenberg commutation relations. Certainly such terms should have experimentally observable effects. I suppose you can claim that such terms, of any size you want, come from a “string theory background”, but, as with all these “tests of string theory”, what is going on here just reflects the fact that you can pretty much get anything you want out of string theory, which is why it’s not testable…

Comments

1. **Glenn**
   January 28, 2008

   Not related to this post, but have you seen the article about someone’s “prediction” of a different set of Higgs masses such that the W/Z-binding Higgs is far above current accelerator limits but that other Higgs (that haven’t been being searched for) could be detected with current hardware.

   Slashdot pointed to this link:

   http://sciencenow.sciencemag.org/cgi/content/full/2008/123/3

   But I haven’t looked at it, not being qualified to tell BS from science…

2. **Peter Woit**
   January 28, 2008

   Glenn,

   There’s no prediction of Higgs masses there. The minimal supersymmetric extension of the standard model has lots and lots of extra parameters, and extra Higgs particles. For some values of these parameters, one of these could be relatively light, but still not have been seen at LEP. I assume the Tevatron experiments have looked for these sorts of things, don’t know what the limits are. It’s not just a matter of having enough energy to produce Higgs particles, you also need a large enough event rate to distinguish them from background. The Tevatron presumably has already produced Higgs particles, just not enough to distinguish them.

3. **Glenn**

January 28, 2008

In other words, it's yet another example of the billion-knobs problem.

I expected that, but like I said, I'm not savvy enough to tell the hype from the hope.

That's *your* job.

Thanks for the info.

4. **Coin**
January 28, 2008

Astronomers have looked hard for evidence of [Cosmic Strings] and found none, but one can always imagine that, miraculously, such things exist, with characteristics exactly such that they wouldn't have shown up so far, but would in some new, improved astronomical observations. In this case, I guess to come up with some new possible observation not already ruled out, the authors of the paper invoke a possible radio telescope with an area of a thousand km2.

Would the information the authors want to gather with this hypothetical telescope (the "absorption features in the 21-centimeter spectrum of neutral hydrogen atoms"?) be for any particular reason useful data to have even in a universe where no cosmic strings exist?

Also: Is there *any* difference between the phrases "cosmic string" and "one-dimensional topological defect"? Or in other words, would a detection of cosmic strings actually prove string theory, or would *any* cosmological theory with one-dimensional topological defects predict cosmic strings too?

5. **Peter Woit**
January 28, 2008

Coin,

Since there's no paper, I don't even know exactly what the signal is they are saying one should look for, or whether it could come from other sources or tell us anything about them.

"Cosmic strings" are topological defects of 1d of astrophysical dimensions. They also occur in simple GUTs. There's an industry of papers out there on the question of how to try and tell the difference between GUT cosmic strings and ones coming from a fundamental superstring. I gather you need a network of the strings and consider their interactions.

6. **lostsoul Ph. D.**
January 29, 2008

I know that this is off-topic, but cannot of think where else to ask. How has Simon's Renaissance Technology been getting along recently? Is his crew truly a band of financial magicians, or has the whole thing been revealed as hype?

7. **DB**
January 29, 2008

"Astronomers have looked hard for evidence of such things and found none"

Actually Bevis' group at Imperial College, London are claiming to have detected traces of cosmic strings in the WMAP data which maps the afterglow of the Big Bang. Bennet, the head of WMAP isn't buying it and calls it a statistical fluke.

I'll you just one guess as to where this report appeared. What? New Scientist? How on earth did you guess that?

http://tinyurl.com/3xkrbm

8. **Not Even Woit**
January 29, 2008

PW – you are either a megalomaniac or cynically trying to shift a few more copies of your book (maybe even both). Physicists have been trying to come up with ways to test string theory long before you started your little crusade. Perhaps you have been too busy with your blog to notice the huge advances in observational cosmology that have been made in the last few years, for example. To think that theorists are suddenly "fighting back" (against what exactly – a popular book written by somebody who has made zero impact in theoretical physics?) is to misunderstand how science makes progress. But like I said, I guess that's not what interests you.

9. **Peter Woit**
January 29, 2008

Not Even Woit,

Sure, BS claims to have a "test of string theory" are not new, but I've been following them for years and they're a lot more common now than they used to be.

"slammed for their failure… string theorists are fighting back" are not my words, but those of the headline writer for New Scientist. Take it up with them…

10. **Not Even Woit**
January 29, 2008

Your words, PW: "string theorists are on the defensive, and some of them have decided that finding some way to claim that string theory really is testable … is the way to fight back."

I will ask again, PW: what/who are they fighting? Your argument is circular. You start with the unsubstantiated (and therefore quite possibly bogus) claim that string theorists are on the defensive, and then try to fit everything they do around that claim.

Unless your blog is just a blatant PR blurb for your book, one can only conclude that you are living in a fantasy world.

11. **Peter Woit**
January 29, 2008

Not Even Woit,

I seem to be repeating myself, but the substantiation of my claim is that it comes from a New Scientist headline. They may not be reliable about science, but I do think they accurately reflect popular perception.

As for doing PR for my book, the only person who has mentioned it at all here is you. Thanks!

12. **Not Even Woit**
January 29, 2008

Your book appears as a prominent thumbnail on your homepage, PW.

So let me get this straight: you are claiming that theorists are publishing (where there's a press release there's usually a peer-reviewed paper) potential tests of string theory in response to the negative public perception of their field.
What is your evidence? Surely there is more to it than a single headline written by someone trying to sell a magazine story last summer?
How can you be sure that the increase in the number of papers reporting potential tests of string theory is not the result of genuine progress in science (both theory and observation)?
Shouldn't you be more rigorous in your reasoning, given that you claim string theory is a failed research program based on its lack of falsification criteria? Or is that one of the luxuries of living in a fantasy world?

13. **Peter Orland**
January 29, 2008

Hi Peter,

Please do not argue with Not Even Woit. Just delete his comments.

Thanks,
Peter O.

14. **Peter Woit**
January 29, 2008

Not Even Woit,

The headline is not from last summer, but from earlier this month. I suppose I could waste a lot of time gathering together similar quotes from news stories, but I doubt that would convince you. OK, here's one more, since I just ran across it this morning. The opening lines of a story at Wired:

"The apparent inability of physics' string theory to be proved right or wrong is one of the stickiest – and argument-generating – problems in modern science."

followed by

"But because none of these descriptions of the universe offered any obvious way to be tested or proven, skeptics have called string theory "not even wrong" – meaning that it doesn't fulfill the most basic requirement of a proper scientific theory, the ability to be be proven wrong.

Now Illinois cosmologist Benjamin Wandelt, along with graduate student Rishi Khatri, say that some of these predictions can be tested, or at least addressed…"

The reason I'm sure that the papers reporting tests of string theory don't represent genuine progress is that I've read them.

Just saw Peter Orland's comment and he has a point. Unless you've got something new to say, or are at least willing to hiding behind anonymity and tell us who you are, explaining your qualifications on this issue, enough is enough…

15. **dan**
January 29, 2008

So what if this detection device was built according to the specifications outlined in the article, and NO evidence for the cosmic strings in the form of hydrogen spectra lines were observed (as described in the popular press article), what aspects of string theory would now be falsified?

16. **Peter Woit**
January 29, 2008

dan,

No aspect of string theory would be falsified, that's the problem.

17. **Somdatta Bhattacharya**
January 29, 2008

The only thing going for String theory seems to be that it is a finite theory. As far as I know, in it calculations beyond 3 loops are unsurmountably hard. So has the finiteness been substantiated consensually? Or is it that what still exists is only wishful thinking with regard to strings interacting over an extended region as opposed to a point? And even that argument is a bit dubious owing to the fact that there would be regions of the moduli space where the string length (it can be looked upon as one of the moduli) goes to zero, signaling the possibility of a field theory like divergence here as well (from that part of the moduli space). So one really needs to check order by order in perturbation theory. Supergravity theories have been shown to be finite to a high loop order, but they fail beyond a certain order.
My point is, even apart from the fact that they are non-falsifiable, can they ever live up to their other more radical promises?

18. **Peter Woit**
January 29, 2008

Somdatta Bhattacharya,

The topic of the finiteness of superstring amplitudes beyond 3 loops is a highly technical one that has been the subject of fierce arguments here and elsewhere, and I'd rather not reopen those here unless there's something new about the subject to discuss. As far as I know a fair summary of the situation is that two-loop amplitudes can be explicitly written down and are definitely finite (work of d'Hoker and Phong). At higher loops there are some proposals for explicit amplitudes, and plausibility arguments for finiteness, but no rigorous proofs of finiteness.

Again, this is quite off-topic, and the kind of thing that attracts heated comments about a very technical issue that spread more heat than light. Unless you have accurate and relevant new information about this, please avoid trying to discuss it here.

19. **Shantanu**
January 29, 2008

Not even Woit, I do know that many people who used to work on string theory
have moved on to different fields (long before Peter's book). Also I do agree with Peter that something is weird with a theory which predicts BOTH violation of equivalence principle as well as its sanctity

20. **Somdatta Bhattacharya**
January 29, 2008

Peter,
I just wanted to be apprised of the current understanding on the issue. As I had suspected, there exist only plausibility arguments and no rigorous proofs. I am aware of the "heat and light" that you mentioned. I have nothing new to offer except what I said in the previous post which is that even the heuristic argument that is usually offered seems to be dubious.

21. **Kevin McCarthy**
January 29, 2008

I think that the idea of testing a theory means something quite different to physicists than it appears to mean in science reporting and press releases. I just stumbled onto this blog today and I've enjoyed reading it, and feel maybe I can contribute to a discussion here. It seems to me that a press release is in order defining exactly a "test of a theory" means.
I'm going to shoot for a definition here, with some preamble, that I think most physicists could agree on, though its probably more likely that in trying to please most it could end up pleasing none. Here goes.
Physics is a rigorous, skeptical, and ultimately empirical science. A claim for evidence or observation of new phenomena must be held to stringent standards. A true "test of a theory" first involves the postdiction of known physics where feasible, because if a theory explicitly contradicts observed phenomena, it must be discarded. This at least establishes the viability of a theory. The second, and more rigorous aspect of a test, is the observation of phenomena uniquely predicted by that theory (to separate it from other preciously viable theories which do not predict the same observations) and generically predicted by that theory, so that the observation represents a true test of the theory as a whole and not a constraint of parameter space.

By this definition, or by any other real definition of what a scientific test should be, press releases and science journalism too often use the words "test of a theory" to generate buzz about a project that, while it could provide interesting and new experimental data, doesn't really test a particular theory.

On another phrase that seems to pop up in every article about string theory, what truly establishes it as "the leading contender for the theory of everything"? (besides more public awareness of its existence and a large community of people working on it). Reading back, I realize that sounds a bit sarcastic or condescending, I'm not trying to make people angry, but its a legitimate question of mine. In the absence of observational evidence, what separates string theory from any other theory that could also claim to reduce to QFT and GR in appropriate limits?

22. **Peter Woit**
January 29, 2008

Kevin,

The "leading contender" business is a phrase designed to emphasize the sociological fact that string theory remains a dominant research program for finding a TOE, even though the scientific case for this has weakened dramatically in recent years.

As far as the "test of string theory" business goes, I don't think the problem is that of science journalists understanding this in a different way than scientists. I see two sources of the bogus "test of string theory" claims, one being non-string theorists who don't really understand what the state of string theory is, but hope that some very different sort of physics they know about is somehow relevant. The other is string theorists who are well aware that string theory has not been able to make the kind of predictions necessary for a legitimate test, but have chosen to try and muddy this issue and mislead people when speaking to the public. In such cases you often find quite different claims in the scientific papers and in the press releases.

23. **Eric**
January 29, 2008

Kevin,
The main reason for string theory being considered the 'leading' (really, the only) contender for a theory of everything is that it is presently the only known way to consistently combine gravity with quantum mechanics. String theory only works because of number of nearly miraculous anomaly cancellations. It is because of this that it is studied, despite the fact that it has not yet been possible to make definitive experimental predictions.

24. **Peter Woit**
January 29, 2008

Eric,

If you have something substantive to say, please do so. But your repetition of stale, nearly 25 year old hype is off-topic. If Kevin cares about the science here, I'd suggest he read Brian Greene and Lee Smolin's books to see both sides of this argument.

25. **Eric**
January 29, 2008

Peter,
There is no need for you to be testy or defensive, and certainly the point that I made is not insubstantive or a 'repetition of 25 year old hype'. You may not like to admit it, but the anomaly cancellation is in fact an extremely important test of any potential quantum theory of gravity. It is perhaps useful for you to ponder why string theory is still taken seriously when there is very little hope of it making any experimental predictions in the near future. Well, here's your answer, and it is not hype. It is cold, hard fact.

26. **Shantanu**
January 29, 2008

Peter the paper by Khatri and Wandelt is now posted on the preprint archive

27. **N. Nakanishi**
January 29, 2008

Eric,
I believe the anomaly cancellation in superstring is a meaningful condition only if the corresponding QFT has gravitational anomaly. The existence of gravitational anomaly in QFT was claimed by Alvarez-Gaume and Witten (Nucl. Phys. B 234 (1984) 269), but their reasoning contained a serious mistake: They were not aware of the fundamental difference between T-product quantities and T*-product ones. Both coincide for chiral current but not for energy-momentum tensor, because the expression for the latter contains time differentiation. The genuine anomaly must be considered for T-product quantities, but what they considered are T*-product ones, because only T*-product quantities can be calculated by Feynman integrals and path integrals. I have explicitly shown in the 2dimensional case that what they called gravitational anomaly arises from the difference between T-product and T*-product. Thus,
at least in the 2-dimensional case, the gravitational anomaly in the genuine sense is non-existent in QFT. It is quite likely that the same is true in the 10-dimensional case.
B. Schroer completely agreed with me.
For details, see Abe and Nakanishi, Prog. Theor. Phys. 115 (2006) 1151 or arXiv hep-th/0503172 v2.

28. **chris**
January 30, 2008

eric,

there is absolutely no problem formulating a quantum theory of gravity without any speculative ingredients. and there is extremely strong evidence, that it is consistent. see e.g. arXiv:0708.1317

29. **Peter Orland**
January 30, 2008

Eric,

1) Peter W. was not being testy or defensive (though he did seem
somewhat irritated). I am not reading his mind, just reading what he wrote.

2) More seriously, how does the gravitational-anomaly cancellation test (your word) string theory? It just provides a constraint on matter multiplets. 10-dimensional String theory, with the gauge group SO(32) or E(8)XE(8), satisfies this constraint. So what?

A bit of history: gauge-anomaly cancellation was regarded as
good evidence for SU(5) or SO(10) GUTS back in the late 70's
(when I started grad school). But really any model with the right
charge assignments is fine – so it really provides no evidence at all.

More history: anomaly cancellation wasn't regarded as evidence
for string theory even in 1984. It simply removed the main reason
why theorists weren't considering string theory before then. I
remember this quite clearly. As a postdoc, I was reading Schwarz's
1982 Physics Reports. People were trying to talk me out spending
time on it because of the problem with gravitational anomalies.

30. **dan**
January 30, 2008

Hello Peter,
Since GUT and SUSY-GUT's presumably can be emdedded in some string framework, is as you say, and I quote "macroscopic "cosmic strings". Astronomers have looked hard for evidence of such things and found none" constitute a falsifiable prediction of GUT and SUSY-GUT's (i.e do GUT's generically predict observable cosmic strings arising form the Big Bang – as the Big Bang does predict other observations such as nucleosynthesis and CMB — that have, thus far, been failed to confirmed)

31. **Eric**
January 30, 2008

Peter O:

"More seriously, how does the gravitational-anomaly cancellation test (your word) string theory? It just provides a constraint on matter multiplets. 10-dimensional String theory, with the gauge group SO(32) or E(8)XE(8), satisfies this constraint. So what?"

Are you joking? Since any putative theory of quantum gravity must be completely free of both gravitational and gauge anomalies, this provides a stringent theoretical test. String theory is the only theory known that passes this test.

32. **Peter Orland**
January 30, 2008

Eric,

"Are you joking? Since any putative theory of quantum gravity must be completely free of both gravitational and gauge anomalies, this provides a stringent theoretical test. String theory is the only theory known that passes this test."

No, any theory with the same matter content passes this test in 10 dimensions.

33. **woit**
January 30, 2008

dan,

Some GUTs have stable cosmic string solutions, some don't. The standard model doesn't. The existence of such solutions depends on the structure of the Higgs sector of the theory.

Problem is, it's not just a matter of whether such solutions exist. To predict whether you'll see them, you need a model of what happened during the big-bang, i.e of the inflationary period and how the universe emerged from it. So, even if your GUT has cosmic string solutions, whether or not you expect to see such things depends on something different, your model of inflation.

34. **metamars**
January 30, 2008

I quoted Nakanishi at Lubos Motl's blog, and he replied:

[material copied from Lubos's blog deleted]

First of all, this topic has nothing at all to do with the posting. Secondly, please do not copy material wholesale from other people's blogs here, trying to drag them into the discussion at this site. If a discussion about an off-topic comment here is going on at another blog and you think people should be aware of this, just give the link here, and discussion of the issue should take place on the other blog, not here.

35. **Eric**
January 30, 2008

Peter O,
Are you sure you aren't forgetting about conformal invariance of the world-sheet?

36. **dan**
January 30, 2008

hi PW,
thanks for replying. I understand that string theory dosn't offer a falsifiable prediction regarding an (non)observation of cosmic strings (through gravitational lensing, hydrogren spectra, or othersie,) and, to quote "that's the problem" but isn't any worse than "GUTs have stable cosmic string solutions" which don't offer a falsifiable prediction b/c if what you say is true "So, even if your GUT has cosmic string solutions, whether or not you expect to see such things depends on something different, your model of inflation" would you regard this class of GUT physics research to be as hoplessly misguided as string theory, for consistency sake? If you'd like me to clafiy I'd be happy to do so but I wonder whether you regard GUT physics research as much a failed idea of unification as stringy unification research.

regards,
Dan

37. **chris**
January 31, 2008

eric,

are you joking? how can a sane person argue like this?

let me reming you that
a) there is no gravitational anomaly with SM matter content in 4D
b) there is very strong evidence, that gravity in 4D is consistent
c) experimentally, D=4 to a very high accuracy
d) some bizarre model that lives only in 10D has some restrictions of its mater content that excludes SM alone

and this you call evidence for the bizarre model? excuse me.

38. **Peter Orland**
January 31, 2008

Eric,

Nothing inherently stringy is required for gravitational-anomaly
cancellation. It just provides a condition on the matter content.

39. **Eric**
January 31, 2008

Peter O,
I presume by 'matter content' you mean the number of bosons and fermions on the world-sheet, described by a CFT on the world-sheet?

40. **Peter Orland**
January 31, 2008

Eric,

No, I mean the number of Fermions and Bosons in space-time
(target space to the string theorists). Green and Schwarz's 1984
paper just started from the classification of gravitational anomalies of
Alvarez-Gaume' and Witten; who did not use any
string theory.

41. **Peter Woit**
January 31, 2008

dan,

The situation of research into GUTs is a complicated topic. One difference with string theory is at least for GUTs it is clear what one is talking about. GUT models are well-defined and one can calculate exactly what they imply and see if the simplest ones can be used to make testable predictions. As string theorists love to point out, if you start looking at arbitrarily complicated GUT models, as with string theory, you're not going to get any testable predictions.

There are two implications of simple GUT models that show a non-trivial agreement with experiment, but they're both kind of marginal and not completely convincing.

One is the fact that a single generation of SM particles fits nicely into one SO(10) spinor representation. The other is that the ratio of observed gauge couplings you expect in SO(10) supersymmetric GUTs comes out close.

On the other hand, many initial hopes for GUTs have failed. The biggest failure was the prediction by non-supersymmetric SU(5) and SO(10) models of observable levels of proton decay, which were not seen. I think it is also fair to say that initial hopes that GUTs would make distinctive, testable predictions about the early universe have not worked out. Here the situation actually is much like that of string theory, with people going on about how cosmology will be used to test the theory, just the way they used to with GUTs, despite the lack of plausible ideas for how this is going to happen. The best hopes there are that somehow we'll observe cosmic strings or something similar, but the fact of the matter is that there is no evidence for such things, and both string and GUT models don't at all require them to be there.

42. **metamars**
January 31, 2008

"First of all, this topic has nothing at all to do with the posting. Secondly, please do not copy material wholesale from other people's blogs here, trying to drag them into the discussion at this site."

Please accept my apologies.

43. **otto schtirlitz**
January 31, 2008

Peter O.
"No, I mean the number of Fermions and Bosons in space-time
(target space to the string theorists). Green and Schwarz's 1984
paper just started from the classification of gravitational anomalies of
Alvarez-Gaume' and Witten; who did not use any
string theory."

The hexagon anomaly which arises at ONE LOOP is cancelled by the TREE LEVEL diagram coming from the B-field exchange. Clearly, one has to go beyond the supergravity approximation to demonstrate the cancellation.

44. **Peter Woit**
January 31, 2008

I don't think this kind of discussion of anomaly cancellation is enlightening anyone about anything, and it's off topic anyway. Enough already.

In any case, the "miraculous cancellations" occur for d=10, and for E8 or SO(32) gauge groups from what I remember, which would be pretty exciting if we lived in d=10 or observed those gauge groups, but…

45. **Peter Orland**
January 31, 2008

Peter,

Sorry, but I can't resist. I won't object if you delete this response.

Otto, using the B-field is clever duality trick to turn a loop
diagram into a tree diagram. The anomalies cancel in the
field-theory limit. No string theory is required.

OK, I misbehaved and will shut up about this matter.

46. **Dan**
January 31, 2008

Dear PW,
Thanks for replying, I think we agree that GUT research is similar to string research. The simplest GUT models (i.e SU(5) have been ruled out, and to evade experimental falsification, both research programs have been made more complicated.

47. **amused**
January 31, 2008

From the press release:

" "If we embed brane inflation into string theory, a network of cosmic strings is predicted to form," Wandelt said."

This seems to be at odds with what Woit wrote:

"Among the huge variety of string theory backgrounds and the many possible ways to try and use such backgrounds to model the big bang, some will […] produce macroscopic "cosmic strings" "

Can the experts out there clarify: is formation of a cosmic string network a generic prediction of brane inflation embedded in string theory, or isn't it? Are there other known viable ways to model the big bang using string theory besides this?

48. **Peter Woit**
February 1, 2008

amused,

To get an idea of the range of models "string cosmologists" are investigating, you might want to take a look at any one of a number of summary talks on the subject. One example would be Renata Kallosh's talk at PASCOS 2007

http://www.pascos07.org/programme/talks/Kallosh.pdf

On pages 15-6 and 17 of her slides she describes two classes of models with no cosmic strings.

Restricting to "brane inflation" models, as far as I know, you can get a huge range of possible "predictions", depending on the specific model and parameters you choose, ranging from far too many, already falsified by experiment, to so few you have no hope of observing their effects. Wandelt for some reason doesn't mention this…

49. **amused**
February 1, 2008

Ok, so brane inflation is one of several scenarios that the string cosmologists have come up with. But within that scenario they say that cosmic string networks are indeed generic for the various specific models. E.g., from the abstract of Tye's review paper hep-th/0610221:

"Another generic consequence of brane inflation is the production of cosmic strings towards the end of inflation. These cosmic strings are nothing but superstrings stretched to cosmological sizes. The properties of these cosmic superstrings and their subsequent cosmological evolution into a scaling network open up their possible detections in the near future, via cosmological, astronomical and/or gravitational wave measurements. At the moment, cosmological data is already imposing strong constraints on the details of the scenario…."

Tye goes on to claim on page 2 of the review that the various parameters in the brane inflation models can be overconstrained by observational data (presumably in the future). In that case the scenario will either be falsified or lead to a predictive cosmological model.

Well, clearly it is misleading for a press release to describe a paper about an observable imprint of cosmic strings as a way to "test string theory" (rather than a way to test one of the main string-inspired cosmological scenarios). But it seems just as misleading to give the impression that there is nothing interesting or worthwhile about this paper or the other ones mentioned in these "hype!" posts. (Working out an imprint that cosmic strings would leave in the hydrogen absorbtion spectrum, and how it would constrain various cosmological models, sounds pretty neat, as does working out collider signatures for distinguishing between different geometries in braneworld models.) I wouldn't want to work on any of this kind of speculative stuff myself, but people who want to disparage it should first come up with something more promising themselves. It's a lot easier to disparage the work of others that to find something interesting yourself.

50. **Peter Orland**
February 1, 2008

This is off-topic, but…

"It's a lot easier to disparage the work of others that to find something interesting yourself."

Amused,

It's time to stop inserting such remarks. You don't effectively
advocate a position by saying someone you disagree with is somehow not qualified. In this case, not only does this remark undermine your position, but is incorrect. Peter W. is NOT unqualified. I say this as someone who does not always agree with him (though in the interest
of full disclosure, I will tell you that we are friends). For example, some papers Peter has written on topology and the lattice with many more citations than most on this topic (and were influential beyond the number of citations). You should also be aware that the number of papers per year written by a typical mathematician is much less than the number written by a typical physicist.

If we are going to argue our positions on the basis of our
qualifications, all of us are vulnerable. One can look at anyone's record and put a negative spin on it.

51. **Peter Woit**
February 1, 2008

amused,

Insulting me is a lot easier than acknowledging that what I wrote is accurate, isn't it?

I actually agree with amused that I should be spending more of my time working on positive things, and less dealing with the problem of string theory hype in the media. In any case, the current rate of BS press releases every few days is beyond my capability of keeping track and commenting on. I do think someone should be pointing this out and no one else seems willing to do this. So I intend to keep doing so, but postings about this will be shorter.

One thing that really is a complete waste of my time is defending myself against the personal attacks of people like amused. So, sorry folks, I'll just delete any more discussions pro (Thanks Peter O.!) or con of my competence here, and continue to let what I write speak for itself.

If you have something interesting to say about the topic of a posting, please do so. If you don't know anything about it, but want to use it as an excuse to insult me or anyone else, please go away.

52. **JC**
February 1, 2008

One of the first few string papers I read was Witten's 1985 paper on cosmic strings. At the time I was in grad school and was naive enough to buy into it as if it was a "holy gospel". I became more skeptical of the idea as time went on.

53. **amused**
February 1, 2008

No I wasn't questioning Woit's qualifications etc. The point had nothing to do with that. It's really simple: if someone wants to disparage work addressing some issue (in this case finding a theoretical underpinning for inflation) then they should provide a more promising approach to addressing the issue. I don't find the string-inspired stuff at all appealing, so I don't work on it, but at the same time I don't disparage it since I don't have any better proposal for the issues they are trying to address. Isn't that a reasonable attitude that everyone should have? It has nothing at all to do with assessing peoples qualifications, publication lists etc.

So what was I trying to do here? First, ask questions to try to find out the actual situation about this paper, what was hype and what wasn't. Second, react against the negativity directed to this and the other PRL papers in the "hype" posts.

"…If you don't know anything about it, but want to use it as an excuse to insult me or anyone else, please go away."

When you turn up on the blogs of Jacques Distler and Clifford Johnson to harangue them about their discussion of some string theory topic, their reaction is probably "please go away" as well. They are too polite to actually write it though. You don't "go away" though, because you think you have some valid points to make and are determined to make them. What a surprise that you should encounter someone with the same attitude on your own blog. It's funny to see how you deal with it.

"Insulting me is a lot easier than acknowledging that what I wrote is accurate, isn't it?"

I asked a couple of questions; you answered one of them (sort of) and I acknowledged it with "Ok,…" at the beginning of my comment. You didn't answer the other one (about whether cosmic strings are a generic prediction of brane inflation) so I went and found the answer myself. Btw, both questions were relevant to the topic at hand, wouldn't you agree?

54. **Peter Woit**
February 1, 2008

JC,

Cosmic strings certainly have a very long history by now. The early hopes for how to use them were conclusively killed off by the WMAP results, but people can always come up with new ones that can't be shot down anytime soon by experimental results. For one of many disconfirmed predictions in this area, see this 1986 Time magazine article

http://www.time.com/time/magazine/article/0,9171,962896-1,00.html

where Ostriker is quoted as follows:

"We still don't know that there are such things as cosmic strings," he says, "or that they are necessarily superconductors or will in fact carry large currents. But all these things are quite possible. Within a few years, superconducting strings will have either transformed our view of the large-scale universe — or be entirely forgotten."

55. **JC**
February 1, 2008

Peter,

It was the repeated disconfirmations over the years which eventually led to my skepticism of cosmic strings. Even back then, many of my former astro colleagues thought the idea was sort of a joke.

56. **Peter Woit**
February 1, 2008

amused,

Yes I did accurately answer your question about brane inflation, and not with vague hype about "generic predictions".

Sadly, you have turned into a prototypical internet troll, devoting your time to posting hostile comments about things you know virtually nothing about. Before criticizing me for what I write about the hype problem in string cosmology, you might want to first go and learn the first thing about the subject.

57. **amused**
February 1, 2008

Well, people can make up their own minds about whether the questions I asked and the rest of the comments were reasonable or not, and the same for Woits answers. Not much point in continuing with this.

58. **anon.**
February 2, 2008

'The point … It's really simple: if someone wants to disparage work addressing some issue (in this case finding a theoretical underpinning for inflation) then they should provide a more promising approach to addressing the issue. I don't find the string-inspired stuff at all appealing, so I don't work on it, but at the same time I don't disparage it since I don't have any better proposal for the issues they are trying to address. Isn't that a reasonable attitude that everyone should have?.'

– amused

Amused, it's unreasonable because if you ban attacks on popular ideas that are failures – unless the person who is attacking has a better approach to the problem – then the need for a better approach to be developed may never arise. It's then Catch-22 because, *only when a failed idea has first been found wanting, is there a need to develop a better approach*.

If you insist that a better approach be developed before criticising failures in the current ideas, 1) nobody will listen because they'll think you're just trying to hype a new theory, and 2) you will eliminate the usual two-step route to advance, whereby a failed idea is first attacked, then replaced by new developments that are inspired by the faults in the existing ideas.

Historically, the two-step route to progress (discredit a bad idea, then afterwards develop a better approach) is more common that a one-step process of replacing an idea in one go without first making the case widely understood for why the idea needs to be replaced.

Your suggestion that criticism must always be constructive, while fine in an ideal world, will eliminate a lot of progress in this (real) world by preventing the two-stage progress model from working. People aren't motivated to fix things that aren't first known to be broken ('if it isn't broken, don't fix it'). If an error is being made, the sooner it is publicised, the sooner it can be fixed. Any censorship of criticisms just slows down progress.

59. **Peter Woit**
February 2, 2008

anon,

I should point out that what I'm doing here is criticizing misleading press releases about scientific results, not the scientific results themselves. Universities should not be in the business of issuing press releases about the work of their scientists designed to mislead the public about its significance, and when they do, someone should call them on it. I'd be very happy if someone else would be the one to do this.

It used to be that these press releases most of the time got pretty wide distribution, through articles in places like New Scientist, and on web-sites like Slashdot. Among the recent flood of them, most seem to not be getting much attention beyond the sites that carry all such press releases. Perhaps this is an indication that journalists covering physics have caught on to what has been going on. I suppose this is progress, but it isn't a good thing that journalists and the public now are likely to be skeptical of the credibility of press releases about physics research put out by major universities.

60. **amused**
February 3, 2008

anon,

Sure, criticising an approach, discussing its shortcomings etc is good and healthy. But that's not what I see happening here. The impression from this post is that the paper under discussion is of no interest and worth but is just a vehicle to propagate more string hype. From reading the press release I got a quite different impression. So I asked a couple of questions to try to find out what the real situation is. The paper is apparently discussing an observable (in principle) imprint that cosmic string networks in the early universe would leave, and how it would give information on the brane inflation that gave rise to those networks if that's what they came from. The level of PW's criticism in this post seems to be that this is junk because you can get anything you want out of string theory so there's no reason to care about what some or other string scenario might predict. But from what (little) I read about this in the string literature it sounds like the situation is quite a lot better than that. Apparently brane inflation is one of just a few scenarios that string cosmologists have been able to come up with, and within that scenario they say that cosmic string networks are a generic prediction. If that is true then the current paper seems not at all worthless and uninteresting.

In light of those observations, the right way to go about criticising this work would be to argue, at the technical level, that one or more of the reasons for being interested in it is flawed. E.g., if someone could show that there are lots and lots of different inflation scenarios (compatible with observational data) that can arise from string theory, and most of these do not predict cosmic strings. Or, alternatively, show that the arguments for why cosmic strings are a generic prediction of brane inflation are flawed. If someone can show either of these things they should write it up and publish it as a research article — that's the way criticisms of scientific approaches are supposed to go.

The way scientific criticisms are not supposed to go is simply pouring buckets of negativity over the research efforts of serious scientists who are following their best judgement in a difficult situation — unless you have a better proposal for the issues they are trying to address.

61. **anon.**
February 3, 2008

'The paper is apparently discussing an observable (in principle) imprint that cosmic string networks in the early universe would leave,…' – amused

Amused, you're missing the point: please see Woit's point in reply to my comment above also applies to your comment:

'…what I'm doing here is criticizing misleading press releases about scientific results, not the scientific results themselves. …'

It's the press release about making a 'prediction' that is being criticised, not the paper. Without knowing definite initial conditions for a cosmic string, are you in a position to make a falsifiable prediction about how big it will become after the assumed inflation occurs within a minute fraction of a second after the universe? Are you even sure that your model for inflation is accurate? It hasn't won any Nobel Prizes yet.

'… the right way to go about criticising this work would be to argue, at the technical level, that one or more of the reasons for being interested in it is flawed. E.g., if someone could show that there are lots and lots of different inflation scenarios (compatible with observational data) that can arise from string theory, and most of these do not predict cosmic strings. Or, alternatively, show that the arguments for why cosmic strings are a generic prediction of brane inflation are flawed.' – amused

Basic research work (to establish validity of claims) is something that the people making such claims need to do *themselves, before actually issuing press releases hyping the claim*. This isn't the same situation as when issuing a preprint about a speculative idea.

It's for the people making claims to investigate the main problems, not for the critics to do that after press releases have been issued. The problem is not that people are doing this research, but that there are very serious problems that are being glossed over in popular hype. It's a bit like popular claims that supersymmetry 'makes predictions' because it requires a so far unobserved partner for ever observed particle. If the theory is wrong, you would never be able to find out: you would keep searching at every higher energies, for ever edging towards the Planck scale. There is no evidence why the theory is correct, and there is no possible way of ever showing it is wrong.

If the search for cosmic strings is actually a *checkable* test of string theory (unlike supersymmetry, gravitons, and brane worlds), then that would be a massive step forward.

62. **Peter Woit**
February 3, 2008

amused,

You just keep on ignoring what this posting is about: a misleading press release. I did not claim the paper was worthless, I do claim that the press release about it is intentionally designed to mislead people about the results of scientific research.

63. **piscator**
February 3, 2008

anon,

Supersymmetry as a solution to the electroweak hierarchy problem is perfectly testable – it's either around the weak scale or it doesn't stabilise the Higss mass. There are good reasons for susy to exist at the weak scale – this is an answerable question, the LHC has the reach to answer this question and will answer this question.

piscator

64. **Coin**
February 4, 2008

Supersymmetry as a solution to the electroweak hierarchy problem is perfectly testable – it's either around the weak scale or it doesn't stabilise the Higss mass.

Okay, which I think is a good point. But the problem is, even if "susy as a solution to the electroweak hierarchy problem" is falsifiable, that still leaves the question of what about "susy as a method of getting matter into string theory". It seems likely that work on susy would continue even if the hierarchy problem justification were removed.

Last Week’s Hype

January 30, 2008
Categories: This Week's Hype

I was concerned that my “this week’s hype” headlines might be less than accurate, counting only 3 separate over-hyped string theory stories during the past month rather than four. Turns out that I missed one (although a commenter here didn’t), involving yet another university press release based on a PRL-published paper about cosmic strings. To be fair to the authors, the press release and paper don’t contain that much hype, nothing about how they are “testing string theory”. What they do is fit the CMB data using an additional parameter they call f_{10}, which has to do with the fractional contribution of cosmic strings to the temperature power spectrum at multipole l=10. They claim to get a slightly better fit to the data with a non-zero version of this parameter and power-law tilt $n_s=1$, versus the usual fit with gives a n_s less than one. When they also take into account non-CMB data, the effect goes away.

This isn’t really convincing of anything, so it’s unclear why it deserves a press release. According to the New Scientist story on this, which is pretty reasonable and hype-free, the chief scientist for WMAP, Charles Bennett thinks it’s a statistical fluke:

> Calling it a detection is odd… I’d be very surprised if cosmologists were excited about this at this stage.

For other press stories about this, featuring misleading headlines, see String Theory Gets A Boost at physorg.com, and String Theory slightly preferred… or at least, not disfavored! at Canada Free Press, where the author does note:

> To listen to people speak about string theory is a lesson in ambiguity. No one is willing to commit to a solid opinion, on either side of the coin, and they dance upon the fence as if they were auditioning for a Garfield strip.

Comments

1. **Thomas Love**
   January 30, 2008

   Peter quoted an author as saying: “No one is willing to commit to a solid opinion, on either side of the coin” He has obviously neither read “Not Even Wrong” nor visited this website!

2. **Indrajeet**
   January 31, 2008

   Hi Peter, I don’t know whether you know this…If you try to link to Motl’s blog from your posting, it is showing a message which contains disparaging remarks about you and your blog.

3. **Peter Woit**
   January 31, 2008

   Indrajeet,

   Yes, I’m well aware of this…

Today’s Hype

January 31, 2008
Categories: This Week's Hype

The rate of appearance of press releases hyping string theory has now passed the the one-per-week mark. Today’s [example](#) is from the press office of the University of Wisconsin, and again is based on the appearance of a paper in PRL. The [preprint](#) of the paper appeared 8 months ago on the arXiv. Since that time, it has been cited exactly once by later papers, in another paper by some of the same authors.

Comments

1. **DB**
   January 31, 2008

   You might also get a kick out of US Patent#6025810 for a Hyper-Light-Speed Antenna which claims: “The present invention takes a transmission of energy, and instead of sending it through normal time and space, it pokes a small hole into another dimension, thus, sending the energy through a place which allows transmission of energy to exceed the speed of light.”

   http://www.patentstorm.us/patents/6025810-fulltext.html

   It reminded me of how Brown University’s Greg Landsberg, when interviewed by BBC Horizon’s Dr. Brian Cox earlier this week, explained that one of the reasons we may not see gravitons is that perhaps they spend most of their time in extra dimensions. (Incidentally the above patent reference turns up at the end of one of Landsberg’s old presentations here:
   http://hep.brown.edu/users/Greg/Talks/Moriond01.pdf)

   You can see the interview with Landsberg here (near the end):
   http://tinyurl.com/ywpu8j

   Apart from this, Tuesday’s Horizon programme was a pretty good and hype-free popular survey of the issues surrounding current efforts to detect gravitons.

2. **Cornelius R. Morton**
   January 31, 2008

   Reference
   http://www.patentstorm.us/patents/6025810-fulltext.html

   The US Patent Office has just bent Special Relativity and certified the existence of higher dimensions!?! Also noted in the description that use of this device will stimulate plant growth. Why?

3. **Coin**
   January 31, 2008

   Okay, now this one confuses me. So they are actually suggesting that the graviton is in the TeV range and could be detected at the LHC by the particles it decays into? I guess I don’t really know whether the idea of gravitons being generated in a particle accelerator is all that strange or not, but how’s the graviton-decay thing work? Shouldn’t gravitons be really really long-lived, like photons or something, in order for gravity to work in the first place?

   Or is the idea that their model somehow contains *both* “KK gravitons” and normal gravitons, and the KK gravitons are the ones that appear in the TeV range and decay quickly, and the normal gravitons just cause gravity? (Their cite, hep-ph/9909255, talks about there being a “tower” of gravitons somehow related to the different “KK excitations”, which seems to make it sound like there’s more than one kind of graviton; and searching there actually seem to be a *lot* of papers writing about the “KK graviton tower”. I don’t think I quite understand any of this. What’s a graviton “tower”?)

   Also why do they talk about “5-d warped extra dimensions”? Don’t we need ten for strings? Or do they just mean that among the six “extra” dimensions in the 10d string model, exactly five are warped?

4. **Yatima**
   January 31, 2008

   Why?

   “As you know, Bob” the use of tachyonic communication devices causes resonant vibrations in the background pan-galactic morphogenetic flux field. This trivially causes water-protein complexes in plant tissue to undergo quantum algorithmic boost processes leading to hitherto unreached efficency in cell growth.

   At least one can definitly say that there is no “prior art”.

   (Sorry for the noise.)

   Anyway, Peter, I propose you add a “hype chart” to the blog sidebar for easier consultation, like they do at that “Homeland Security” outfit.

5. **chethan krishnan**
   February 1, 2008

   Dear Coin,

   Here is what people mean when they talk about a Kaluza Klein tower.

   The wave function for a field has to come back to its starting value when you move around a compact dimension (think of a circle) because wave functions are believed to be, well, functions, i.e., they are single-valued.

   Now, momentum is the generator for translations (which is just a fancy way of saying that i.p.x is what shows up in the exponent in the wave function, just believe me on this point if you don’t know why. I don’t know your background.). So this condition can be translated to the condition that momentum components in the compact dimenions are quantized as n/R, where R is the radius of the circle, and n is an integer. (Because ip (x+2 pi.R) – ip x, should be a multiple of 2i pi. n)

Now, if you write down the equation of motion for the field (example: think of a Klein-Gordon equation) the derivatives in the compact dimensions bring down (the square of) the quantized mometum and it looks just like a mass term. So if you want to talk about all the n's together, we can talk about a "Kaluza-Klein tower" of fields. Depending on the size of the compactification, all of these except the n=0 (massless) case might be too heavy to be detected in accelerators. This is where "large" extra-dimensions etc. (larger size= smaller mass) can give rise to possibly new phenomenology.

If you notice, I tacitly assumed that the compact dimension was flat. (Circles have zero intrinsic, i.e., Einstein-like, curvature). What the paper that you pointed to is considering, is the possibility that this need not be the case, you can consider more general spaces for compactification. The word warped means there are some conformal factors that show up in pieces of the metric. It turns out that this warping is a beautiful way in which we can explain away the heirarchy between say electroweak and Planck scales. The word "graviton" comes up because the higher dimensional metric, with some legs in the compact dimensions, is what get interpreted as lower dimensional fields.

Randall-Sundrum as it is, is a phenomenological model. So it doesn't have to be motivated by string theory. But the interesting thing is that it *is* motivated by string theory. Randall-Sundrum like scenarios are more or less easy to implement in string theory by considering one of the "compact" dimensions expected from of string theory to be non-compact, warped, etc.

Of course this was just the general story, but that should give you the bearings.

Chethan.

US HEP News

February 1, 2008
Categories: Experimental HEP News

Today and yesterday at Fermilab there is an HEPAP meeting designed to gather information necessary to prioritize decisions on how to spend the US HEP budget over the next few years. Many of the talks there are on-line and give a good idea of what future possibilities look like. The main issues being discussed are:

- Whether to go ahead with project X at Fermilab, a proposal for a high-intensity proton source.
- The state of the ILC project, given that it was zeroed out in this year's US and UK budgets. Barry Barish emphasizes the continuing goal of being ready to make a decision about whether to build such a thing soon after LHC results arrive, presumably starting in 2010. The state of CLIC and other multi-TeV lepton collider possibilities is reviewed by Tor Raubenheimer of SLAC, who puts a likely date for a multi-TeV electron collider at 2030-40, a muon collider after 2050. These things are a long ways away…
- Whether to run the Tevatron in FY 2010, with presentations about how the Tevatron is currently performing, and from CDF and D0 advocating for a run past FY2009. Both experiments make the case that they are getting close to being able to either see evidence of the Higgs or rule out its existence over most of the expected mass range. More about this from Tommaso Dorigo of CDF here. Since the Tevatron is about the most successful and exciting thing going on in US HEP, I personally don't see the case for planning on shutting it down until solid results are in from the LHC about the Higgs, which should be sometime in FY2010 at the earliest. Who knows, maybe the LHC will see something that the Tevatron is a good tool to study further. Seems more likely than that it will see black holes…

The involuntary furloughs of Fermilab employees begin today. No news regarding the supposed efforts by the Illinois Congressional delegation to lobby for a supplemental appropriation to keep Fermilab from having to layoff around 200 people. At least one of the relevant people is undoubtedly too busy with other things to pay attention to this. The Congress and the White House are negotiating an emergency bill to deal with the recession and job losses that have started recently. Since government spending is bad and tax cuts are good, their plan seems to be to continue to throw people out of their jobs with budget cuts in HEP and elsewhere, while handing out cash to as many voters as possible.

For a presentation by DOE Undersecretary Orbach about the DOE budget problem, see here, and analysis from Richard Jones of the AIP here. The FY 2009 budget request from the White House will come out on Monday, and Orbach promises that

> The President's request for FY 09 will be wonderful, again, for the physical sciences. While I can't go into details here, I can say that it will continue the funding request consistent with the American Competitiveness Initiative and the America COMPETES Act. The problem for all of us is that, faced with essentially flat funding for the physical sciences in FY 08, the President's Request for FY 09 will appear as a very large percentage increase for the three ACI agencies. The danger is that basic research in the physical sciences will again be 'donors' to other programs.

meaning I guess that Congress will be tempted to strip these out in order to fund other things.

Gordon Watts notices that in Bush's State of the Union speech he explicitly advocated increased funding for basic physical science research, something which is extremely unusual in such a speech:

> Last year Congress passed legislation supporting the American Competitiveness Initiative, but never followed through with the funding. This funding is essential to keeping our scientific edge. So I ask Congress to double federal support for critical basic research in the physical sciences and ensure America remains the most dynamic nation on earth.

The physics community seems to have done a great job of convincing the administration to support basic physics research in general and HEP in particular, which normally would be a very good thing. But the same was true last year, and it seems to just have had the effect of painting a big fat bullseye on HEP funding for someone in Congress looking for a place to cut. At least this year people are aware of what might be coming, and maybe something can be done to head off a repeat of this year's disaster.

The general budget politics don't look favorable at all though, with the Bush Administration evidently proposing to heavily cut Medicare and Medicaid spending. Congress has very different priorities, and it seems all too likely that they will fund restoration of the health-care cuts by cutting things like the DOE basic research budget. This fall will be different though, with a new Congress and president elected at the beginning of November, but not taking office until January. I wouldn't be surprised to see the government run on a continuing resolution at FY2008 levels until after a new president takes office.

Update: The proposed FY2009 budget is out, DOE here, NSF here. The DOE budget contains a huge increase for HEP, from $688 million in FY2008 to $805 million in FY2009. The NSF budget doesn't break out the HEP component, but the total budget for math and physics is supposed to go from $1167 million in FY2008 to $1403 million in FY2009. These are huge and very healthy proposed increases, but unfortunately it is not at all clear that they will actually make it into the final budget.

Update: There's a story today in the New York Times about this. Also a message from the Fermilab director, saying he has no choice but to go ahead with the plan to start laying off employees of the lab. In practical terms, the proposed budget increases appear to be meaningless, with the likely situation no increase at all in FY2009 of any kind until a budget gets passed, which most likely will not happen until already deep into the fiscal years. He writes:

> …every Washington expert tells me to prepare for a continuing resolution that might last into the new administration. Such a continuing resolution would extend the present difficult budgets well into FY09. At the same time, relief in FY08 in the form of a supplemental appropriation is not guaranteed and is at best several months away.

Comments

1. **John**
   February 3, 2008

   I wouldn't be surprized if HEP funding was cut. Given the current economic woes it's bound to happen that 'esoteric' fields with no direct, immediate consequences should bear the brunt of funding cuts.

2. **Peter Woit**
   February 3, 2008

   John,

   According to new stories today, Bush's plan is to blow out the FY2009 budget with a $400 billion or so deficit, leaving the mess of dealing with that to the next president. He probably doesn't have any choice about this if he wants to finance the war in Iraq at a high level and avoid a recession by stimulus spending at home. The Democrats seem to lack any serious interest in stopping the war by refusing to pay for it, and they too like to give cash payments to voters, so it looks like $400 billion or so will be the size of the deficit (larger if there's a recession).

A healthy budget increase for HEP would be about one-tenth of one-percent of the planned deficit spending, lost in the rounding errors of the things like Medicare spending that the Congress and the White House will fight over. Supposedly, this week Bush will announce the details of the proposed budget and it will include a healthy increase for HEP, we'll know the details soon. Unclear what the Congress will do, we may not know until nearly a year from now, far into the fiscal year. What a way to run a country….

3. **J.F.**
   February 3, 2008

   In the category of "bigger fish to fry", note that Senator Durbin has serious egg on his face on the abrupt cancellation of the FutureGen coal plant by DOE, AFTER a siting decision was made for IL and against the TX sites. One can only wonder if his eye was on the wrong ball back in December, or even if he got deliberately played by the administration (e.g. FutureGen was never coming to IL, and he should have been fighting for Fermi and other things).

   I don't think the state of the union bit helps at all. Even when Bush was more popular and not a lame duck, all the "lets go to mars" rhetoric did was reprioritize NASA money, and DOE is far more compartmentalized, so even if we wanted other science and tech to suffer for HEP it won't happen.

4. **Gordon Watts**
   February 4, 2008

   But we don't want other science and tech to suffer for HEP! That attitude, btw, is exactly the best way to get science funding and HEP shot in the foot.

   One thing about this budget. If this works out as the last one has — will Bush even be in office? And will the politics be such a mess they just decide to do a continuing resolution for the full year — as they did previously?

   At anyrate, the horse trading that involves HEP and science in general comes at the very end — as Peter points out the amounts are so small compared to the elephants roving the room. This is why we were all blind-sided.

   Finally, don't make the mistake of thinking that only HEP got it last time. Several branches got it (NIST, ITER, for example). It should be on every scientists mind: how to increase funding for all sorts of science in the USA, not just their particular branch. I'd really love to see general increases and then the peer review system or experts figure out how to allocate it. Maybe we could stop talking about man on mars then…

5. **J.F.**
   February 6, 2008

   Gordon: I agree completely regarding priorities, I mentioned it as an absurd extreme but was not clear.

   As an interesting sidenote, Bill Foster, formerly of Fermilab and running in that district, apparently won his primary tonight. His competition in the March special election is a perennial candidate who has never won office and is not liked by the state party, so there is a significant chance Foster will be in congress soon. Personally I think it is obvious that Fermi got cut a month after Hastert resigned precisely because they had no seat at the table to defend them.

Steinhardt: The Anthropic Landscape Has Run Its Course

February 3, 2008
Categories: Uncategorized

Over at John Horgan's blog, he quotes an e-mail from Princeton cosmologist Paul Steinhardt, who corrects Horgan's account of a recent conversation between them, writing

> I said that I thought that the idea of a string landscape and the notion of anthropic selection had run its course. I think it is too early to give up on string theory.

While Steinhardt sees anthropic selection of our universe out of a multiverse as an idea that has run its course and is on its way out, it's still quite popular in certain quarters. The Templeton Foundation, through the FQXI organization, is a major source of funding for anthropic multiverse research. FQXI's web-site has a new story up entitled Philosophy of the Multiverse, which asks "On what side of the borderline between science and philosophy are multiverses?" The writer evidently couldn't locate anyone to take the "it's philosophy, not science" side of the argument, quoting Sean Carroll, Anthony Aguirre, Alexander Vilenkin and Aurelien Barrau as supporters of anthropics as science. Barrau suggests that we may need to change the definition of science to accomodate the multiverse.

Whatever Steinhardt says, at least the Mormons are getting on the multiverse bandwagon, with their journal Dialogue recently publishing a long article entitled Eternal Progression in a Multiverse: An Explorative Mormon Cosmology. The article begins:

> This article is an examination of the Mormon doctrine of eternal progression within the context of big-bang cosmology, a description of a finite universe that appears to contradict that doctrine. I argue that a multiverse cosmology, a theory that posits a multiplicity of universes, resolves many of the problems posed by big-bang cosmology.

and goes on to explain how the multiverse agrees well with the doctriine of "eternal progression" in Mormon theology:

> In a Mormon multiverse cosmology, God does indeed manifest his infinite creative prowess in the respect that God (any god along the infinite chain of gods) creates children, some of whom progress to become gods, who in turn create their own universes and children, some of whom progress to become gods, and so on, forever. Each universe in the ensemble of universes becomes an extension and continuation of the creativity of every "ancestral god" in an eternal family of deities. The creativity and glory of each god increases exponentially with the production of new universes. In this cosmology, the multiverse is a hallmark and witness of the infinite work and glory of God and the dwelling place for an infinite number of eternal progressing beings.

While solving the problem of justifying eternal progression, the multiverse idea leads to all sorts of new possible research directions:

> In a Mormon multiverse cosmology, many questions remain open. Are there communication and movement of the gods and other premortal and postmortal beings between universes? When a universe experiences a big crunch or big freeze, does the god of that universe generate a new universe or "relocate" to another universe fit for carrying out the "great plan of happiness" for a new household of spirit children? Did God, our Father in Heaven, achieve godhood in this universe or a prior one? If God was exalted in a prior universe, how many universes has he governed? Jesus Christ is the redeemer for this universe, but is he the redeemer for others? Are some universes "stillborn" in the sense that they do not have the required values of the physical constants for a universe capable of sustaining life? Because the multiverse is infinite, are there replicas of us in other universes as postulated by the replication paradox? Cosmologists speculate whether the physical laws are the same across the ensemble of universes, but what about the spiritual laws? Are the spiritual laws "multiversal" or just "universal"? As multiverse cosmologies develop scientifically, these questions and others will stimulate much discussion.

The author ends the piece with a quote from Andrei Linde: "Universes can have babies — it's nice."

Comments

1. **JC**
   February 3, 2008

   If this is true, then maybe the anthropic landscape stuff being decoupled from physics and being isolated in the Templeton domain is what may possibly end up happening. That will show what the merits of all that anthropic stuff is really all about.

2. **John Stevens**
   February 4, 2008

   Dr. Woit, I have two questions for you: How did you learn of this Dialogue? Are you LDS? (A Mormon?) Second: Are you trying to make the claim that the Anthropic Landscape is on the same level as Mormon Theology? Thanks?

3. **Eric H**
   February 4, 2008

   It's interesting that just as some cosmologists and theoretical physicists are farther along in their total rejection of string theory, others are just starting the journey via their disavowal of the anthropic and mulitiverse ideas. Paul Steinhardt seems to be on the path of escaping from the clutches of string theory, but has not quite reached escape velocity yet. And finally there are the really backward individuals jumping onto the caboose and not realizing the locomotive miles in the distance is just careening off the cliff…

4. **Peter Woit**
   February 4, 2008

   John Stevens,

   No, I am not LDS, this came up in a Google search for something else.

   I know just about nothing about Mormon theology, and would suspect that this argument is something that conventional Mormon theologians might think of as "pseudo-theology": not interesting because it is pure speculation, with no way of ever deciding if it is true or not using the investigational methods of Mormon theology (whatever they might be).

   The study of the string theory landscape using anthropic arguments is certainly different than theology, Mormon or otherwise, but I'm not the only one who feels that it has more in common with theology than it does with science.

5. **Anthony A.**

February 4, 2008

Hi Peter,

I'm sorry that the FQXi article rubbed you the wrong way. A short article like this can't do much justice to the complexities of an issue like this. In my own view, there certainly are things one might call a 'multiverse' that are essentially philosophy (though I do not use that word synonymously with 'evil' or 'rubbish'), and others that are clearly not. But in terms of your post I think a few things are worth commenting on from the perspective of accuracy:

1) If you are discussing multiverse research supported by FQXi, I think a more accurate statement would be that "FQXi (funded in large part by Templeton) is a major supporter of multiverse research.", because the Templeton Foundation plays no role in deciding what projects FQXi supports.

2) Your entry gives the sense that FQXi is, as an organization, supportive of the multiverse. FQXi has no such 'ideology', and supports the best of the proposals it receives. I think I can safely say that FQXi has not rejected a single serious proposal explicitly opposing the 'anthropic multiverse', and has funded a number of proposals that work to provide alternatives to string theory, as well as anthropic ideas.

3) On my personal views, you may be interested to know that although it does not really come across in this article, I have actually been one of the leading critics of anthropic reasoning, starting with my first paper http://arxiv.org/abs/astro-ph/0106143, where I discuss how Weinberg's cosmological constant argument can be subverted. Later papers pointed out in detail how incredible hard anthropic/probababilistic reasoning in a multiverse is. However, in my view 'critic' means to take something seriously and show what works and what does not, rather than dismissing it categorically. Also (as mentioned in that same FQXi article), I have been working hard lately on the possibility of directly observing signatures of other (eternal inflation) 'universes'.

4) Much of the interest by cosmologists in 'multiverses' comes directly from the acknowledgement that generic inflation models do this, and that inflation is a really nice idea. Steinhardt and others have worked heroically to give alternatives to inflation, and I think we should all support these. But I think it is fair to say that the vast majority of cosmologists have yet to be swayed by their charms…

5) Pointing out how multiverses are now being discussed by some paper by Mormons is fun, but I think you could easily do the same using any scientific theory (e.g. the big bang, quantum mechanics, etc.) and various religious groups, some respectable and some not. So I'm not sure what your real point is with that…

best,

Anthony

6. **Zany Zebra**
February 4, 2008

This is great.

The Mormon religion has more to do with the "new thought" (now we call it "new age") movement of the 1800's and early 1900's than it does with orthodox Christianity, but it comes from a time when people developing alternative religions felt a lot of pressure to pretend to be Christian. (These days alternative religions that don't pretend to be Christian have an easy time getting started, but have a hard time sustaining themselves… The bible-based churches that look like warehouses on the outskirts of your time are the survivors of a natural selection process that included Scientology, Hare Krishnas, the Divine Light Mission, the Jesus Freaks and thousands of others.)

The article is a good example of the imaginary that surrounds anthropic and other cosmologies: the kind of cosmology that's fashionable at a given time depends as much on people's emotional needs as on real science. I remember the pre-inflation days when many cosmologists had a preference for omega < 1 because a universe that expands forever just seemed too lonely.

7. **Peter Woit**
February 4, 2008

Anthony,

Thanks for the interesting and sensible response to my rather hostile posting. You raise a lot of issues worth more discussion, here are some thoughts.

Yes, the Mormon theological document was a cheap shot. But still, I think it provides a useful reminder of where you end up when you stop paying close attention to what is legitimate science and what isn't. A lot of what I see reputable cosmologists going on about these days seems to me to have gone over the line. Why is Linde being quoted in the NYT about reincarnation??? If he has a sensible scientific point he should be avoiding theological concepts like the plague when trying to make it to the public.

Barrau's suggestion that standard criteria of what is science and what isn't may need to be changed is dangerous nonsense. It's well known that there's more to this question than falsifiability, but the issues surrounding this are the same as in any science. Whether the multiverse can be invoked to provide a conventional scientific, testable understanding of anything is a real and complicated question, but you can't cheat by changing the rules. This issue is worth debating if one can agree on the ground-rules of what is science and what isn't. If not, there's not much point in going any farther.

I thought both the FQXI article and the recent NYT article were disappointing in that they don't reflect the fact that many if not most physicists are not sold on the idea that this is actually science. Steinhardt would be a good example. Promoting uncritically this kind of research to the press is really a bad idea, it threatens to discredit physics research in general, as much of the public can recognize all too well that some of what is going on can't be legitimate science.

As for Templeton, I did not mean to imply that they have any influence over your grant decisions. But, blurring the distinction between what is science and what is religion is something that fits well with their agenda, and their decision to fund FQXI seems to me to have been taken knowing that it would be an organization sympathetic to anthropic multiverse research, something they probably saw as an argument in its favor.

8. **Struwwelpeter**
February 4, 2008

@Peter Woit:
"If he has a sensible scientific point he should be avoiding theological concepts like the plague when trying to make it to the public."

He never mentioned the plague, which, by the way, is not a theological concept but a disease (especially useful in curses).

9. **Peter Woit**
February 4, 2008

Struwwelpeter,

OK, OK, so this comment wasn't up to my usual standards of excellent grammar and great clarity. It has been a busy day so far…

10. **Bee**
February 4, 2008

I am actually quite fond of philosophical questions, but my biggest problem is always to figure out the actual meaning that is attached to a word. Good philosophers do at least try to start with a sensible definition, yet when I hear people 'philosophizing' they just fight over their interpretation of words which for a scientist can be pretty tiresome. Likewise, I find the multiverse discussion in the broad sense somewhat odd because different people seem to attach different meaning to the 'multiverse' to begin with. In some cases, it could have a scientific content, in others not.

E.g. the sentence (from the fqxi article) "So wouldn't it be strange if there were just one universe?" is already quite comical and illuminates this confusion, given that the [Online Etymology Dictionary] explains:

universe – lit. "turned into one," from unus "one" (see one) + versus, pp. of vertere "to turn"

If the universe is the one and all there is, there can by definition be no other universes? But leaving that aside, for some the rest of the 'multiverse' (of whatever kind) outside 'our own' (whatever that means) is by construction unobservable, in other cases there are observable consequences, which makes the essential difference as to scientific content.

Also, I'd acknowledge that sometimes a change in perspective can lead to progress even if that view itself isn't actually a testable hypothesis. Take e.g. the free will question. In practice it doesn't make any difference whether you do in fact have a free will, or all your decisions are predetermined but you don't know it. It does however make a difference to how you think about yourself and what you believe how the world works. Questions like this and the prevailing culture and sociological atmosphere in which scientists work do without doubt influence how we invest our brain time. Here as always however, I'd appreciate more clarity on what is science, what is scientific uncertainty, and what has nothing to do with science. For the scientists working on it that might be perfectly clear, but in the communication to the public one has to be extremely careful the scientific content doesn't get reduced to 'reincarnation as a BB' or an 'infinite amount of duplicates of yourself'.

11. **Coin**
February 4, 2008

I think I can safely say that FQXi has not rejected a single serious proposal explicitly opposing the 'anthropic multiverse'

Although I don't mean to denigrate your point here (i.e. that FQXi has no "official" stance on the multiverse and is simply promoting papers, multiverse/string or otherwise, that are found of quality) I do kind of wonder about this specific statement. How *could* you receive proposals explicitly opposing the anthropic multiverse?

Is it even *conceptually possible* to form an objection or line of attack on the anthropic multiverse idea, of the kind that could be actually be submitted to FQXi as a grant proposal? After all, the primary criticism of the anthropic multiverse concept in the first place is based on complaints of unfalsifiability– that is, claims that no scientific basis exists for attacking (or evaluating) the proposals of the anthropic multiverse idea.

12. **Eric H**
February 4, 2008

Bee,
I think you made a lot of good points. I find a connection between the motives for individuals in their theological thinking and the motives of scientists in their thinking about the physical world. There seems to be a similar motivation but the distinction seems to come from their reaction to anthropomorphication in religion.

I was brought up in a heavily Christian home, but like many people I ultimately rejected the human centered interpretation of God and his followers. Just too much projection of our own emotional desires onto symbols in religion. While still trying to adhere to the major tenets I've rejected the cloaking of those tenets behind ideas that set man as separate from the universe. So for me science, and physics especially, is sort of a spritual quest to move from the specific (human beings) to the general.

I think there is a dichotomy in people's basic nature, with many people in science anthropomorphizing their emotional needs in a way similar to how organized religion often does. I think it exposes itself in projecting "infinite universes" or an "infinite God". I think for some people it would be actually frightening to believe the universe is a closed system. But if the universe is not closed, i.e. energy or information can be injected spontaneously without a counterbalance somewhere else in the system, then what would be the point of science. It would all be for nought. I think most people don't realize a closed system is a GOOD thing!

13. **Struwwelpeter**
February 4, 2008

@Peter Woit
No,no, Peter, I wasn't criticising you in the least.
It was a not so clever meta-level-joke and my only excuse is that I found it amusing to make a wordplay in what is not my mother tongue.

I am a great admirer of your chronicle (that's what they call blogs in my 1920 Berlitz English course, in which fans are still mechanical ventilators, not admirers) and am very grateful to you for all I have learned in it, both in mathematics and physics.
Cordial wishes, S.

14. **Peter Woit**
February 4, 2008

Eric H. (and others),

Please, I really don't want discussion of religion here. It's a topic that people have an infinite amount to say about, of which I'm only interested in a very small part, and that's the part that has nothing at all to do with science, so is off-topic here.

Mostly I quite happily just ignore people who want to engage in metaphysical discussion. When I can't ignore it and have to pay attention to it (as in when the field of particle theory starts being infected by it as it gets used to prop up a failed research program), it raises my blood-pressure and causes me to stop being my normal mild-mannered self and get hostile. Please help me stay healthy and not frothing at the mouth by sticking to science and leaving religion to other venues.

15. **Eric H**
February 4, 2008

Peter,
Sorry to get your dander up. I perhaps shouldn't have cloaked my argument in terms of religion. My real emphasis was the idea of bringing in infinite universes, and infinite, or near infinite, tuneable cosmological parameter combinations in string theory comes from the same drive as the worst parts in religion i.e. that the essential nature of the universe is something not ultimately knowable, no matter how much progress is made over time. Just because it's not known yet doesn't mean it can't

ultimately be known. String theory is just the wrong road. The right road is in accepting the universe as a closed system – you know, the first law of thermodynamics.

16. **milkshake**
February 4, 2008

usefulnes of ideas can be figured by proposing the exact counter-ideas. If one has no way of telling the two cases apart, by looking at this world, then his way of thinking belong to philosophy, psychology, poetry, marxism or other forms of religious experience

17. **Shantanu**
February 5, 2008

Peter, changing the subject a little, there is another paper on Boltzman brains
yesterday Btw do you think this
is a serious problem for inflation?

18. **Peter Woit**
February 5, 2008

Shantanu,

I saw the Gott paper, and tried to restrain myself from adding a hostile mention of it here, but since you brought it up… No, I don't think Boltzmann Brains are a serious problem for inflation (they're only a problem for the anthropic multiverse), but do think they are a serious problem for physics, with the field of Boltzmann Brain studies full of nonsense which makes theoretical physics look bad when it gets publicized. Some of Gott's arguments are just beyond absurd:

"The BB does not count as an intelligent observer because it is observer dependent and does not pass the Turing test. It can be distinguished from a human because even if it answers 20 questions successfully in a row, it will likely fail to answer the next question."

19. **Anonymous**
February 6, 2008

Mein Gott!

20. **Gilbert Awad**
February 8, 2008

Krauss, et al., have just published a paper in PRL where they argue that anthropic reasoning proceeds from ignorance more than knowledge. They try to demonstrate that anthropic lines of reasoning reveal more about the biases of the arguer than lead to any terribly useful knowledge. Unfortunately, they also claim that the end of anthropocism, if it comes, will be a long, slow death. Still, anthropics always having left a bitter taste in my mouth, I welcome their effort with open arms.

Krauss, Maor, and Starkman,"Anthropic Arguments and the Cosmological Constant, with and without the Assumption of Typicality," Physical Review Letters 100, 041301 (2008)

21. **Marcus**
February 8, 2008

Gilbert,
is that article by Krauss Maor and Starkman a retitled version of
http://arxiv.org/abs/0709.0502
"Anthropics and Myopics"?

22. **Gilbert Awad**
February 11, 2008

Marcus,

I'm not much of a gambling man, but that one looks like a pretty sure bet. In a word, yes. And thanks for pointing it out; I hadn't seen the Arxiv preprint.

23. **Shantanu**
February 21, 2008

Hi all,
See also this bizarre talk by Don Page at here

24. **Seth R.**
April 3, 2008

Sorry for re-visiting a conversation that has run its course. I'm not much of a scientist, so I can't speak to that. But I am Mormon and well acquainted with the small world of Mormon blogging and intellectual debate.

"Dialogue" is considered to be a rather unorthodox and fringe publication within the Mormon religion. Many believing Mormons consider such publications to be a frivolous waste of time at best and misguided apostasy at worst. There is certainly an anti-intellectual current in Mormonism, like there is in many religions. But occasionally you get intrepid believers who like to push the boundaries and venture into new theological territory.

My own experience is that Mormon attempts at cosmology are usually either wholly, or in large part, a reaction to mainline Christian attacks on Mormon theology. Mormon apologists and other Christian apologists get in their little spitting matches over whether Mormons are monotheists, whether they believe in Jesus, and whether they are being presumptuous by putting God in the same ontological category as human beings.

I won't bore you with all that. But to get to the point, there is a growing awareness in the world of LDS apologetics that the central divide between Mormons and other Christians is, not trinity, or scripture, or anything else, but the doctrine of "creation ex nihilo" – the idea that God created the universe out of nothing but His own power. For traditional Christian theologians, the entire universe can be divided into two ontological categories: 1. God – who is uncreated and self-existing and 2. everything else – which was all created by God. That ontological distinction is absolutely crucial for traditional Christian theology and their entire notion of the "trinity" is an attempt to preserve that ontology.

Mormonism collapses that ontological distinction and posits that not only are God and humanity the same species, but that creation ex nihilo is a false and groundless notion. Mormon doctrine states that all matter is eternally pre-existent. God never created anything out of nothing. He is only creator in the sense that a painter is a "creator" – i.e. He takes pre-existing materials and creates something from them. All matter has existed eternally in some form or other. So has the most foundational part of human identity.

That's what Mormonism claims, and it directly attacks the foundational assumptions of traditional Christianity about God and the universe.

Now, whenever a Mormon-Christian debate comes around to the topic of creation ex nihilo, the traditional Christians inevitably cite "The Big Bang" as cast-iron proof of their ontological claims. They cite the Big Bang as the moment when God spoke, and the universe burst into being. Mormon apologists, of course, are only too happy to ally (as best they can) with modern quantum physics in attacking this assertion.

This theological debate hasn't fully matured yet because theologians on both sides are still in the process of discovering the central controversy of creation ex nihilo. But at present, it seems very much like the traditional Christian theologians are positing a universe based on a fundamentally Newtonian model of physics. Increasingly Mormon theologians – the new kids on the block – are turning toward modern developments in quantum physics to undermine and refute the Newtonian-based theology of mainline Christianity.

String theory, of course, offers a real grab-bag of possibilities for the Mormon theologian. How can a physically present God be omnipotent and everywhere? How can the idea an infinite past (an assertion of Mormonism) overcome the problem of infinite regress of causes? How can a human become god and yet not surpass his or her own Creator? Fun stuff, if you're into that kind of thing.

I know this isn't a theology blog. But you cited the Dialogue article, so I thought you might be interested in the environmental impulses and demands that probably gave birth to it.

Microsoft Research New England

February 4, 2008
Categories: Uncategorized

Microsoft announced today that they'll be opening a new research lab, in Cambridge, which will be called Microsoft Research New England. The director of the lab will be mathematical physicist Jennifer Chayes, with deputy director her husband Christian Borgs, who is also a mathematical physicist. For an interview with them, see here, for a story in today's New York Times, see here.

Jennifer and her ex-husband Lincoln Chayes (also a mathematical physicist, now at UCLA) were my class-mates during graduate student years in Princeton, as well as frequent companions on trips down to City Gardens in Trenton to see bands like the Ramones. The two of them at the time had even more impressive leather outfits than the Ramones.

Update: There's more about this at the Xconomy web-site.

Comments

1. **Someone**
   February 5, 2008

   Your friends are doing OK. Congratulations to them and to you.

   Apart from that, what is the relevance of this post to the subject of this blog?

   This post's title should be "not even relevant".

2. **woit**
   February 5, 2008

   Someone,

   Well, one argument for relevance is that mathematical physics is perhaps the central concern of this blog, and Microsoft is opening a large, very well-funded lab next to MIT to be run by two mathematical physicists.

   As for the personal color and my reminiscing about my grad school days, surely you can just ignore that. Now, stop complaining or I'll write a blog posting about my views on politics, who I just voted for and why, etc., etc….

3. **Someone**
   February 5, 2008

   «Microsoft is opening a large, very well-funded lab next to MIT to be run by two mathematical physicists.»

   But their main focus won't be mathematical physics will it? If I understand this at all, it won't be physics related at all.

   "we want to combine core computer science, especially the more mathematical and theoretical aspects of it, with the social sciences, and we want to do it in an environment in which we won't just have researchers doing fantastic research side-by-side, but they also will be helping to create new fields at the boundary of computer science and the social sciences."

   «Now, stop complaining or I'll write a blog posting about my views on politics, who I just voted for and why, etc., etc….»

   Please don't. I really think you do a great job with this blog and would hate to have it "tainted" by your personal view on those subjects.

   We already have some "huge variance" blogs for that. No need to loose this great blog to that inner demon.

4. **George Bell**
   February 5, 2008

   After hearing Jennifer talk at a recent MAA meeting, I'd categorize her more of a Mathematician/ Computer Scientist. Perhaps they are going to try to create a search engine that can beat Google in this think tank?

5. **Ali**
   February 5, 2008

   Hi Peter,
   From the links you provided, it seems like there will be only 4 staff members in this "large" research center, including Chayes and her husband. I do not understand what makes this worthwhile to be a news article in NY Times.

6. **Peter Woit**
   February 5, 2008

   Ali,

   My understanding is that 4 is just the number of people starting the center, that the intent is for it to ultimately be much larger. Microsoft Research Cambridge (England) was started about 10 years ago with 3 researchers, now employs over 100, and I'm guessing that the intent is to reproduce that sort of thing in the other Cambridge.

   Not sure why the NYT chose to write about this, perhaps partly with the idea that anything new Microsoft is doing is newsworthy. It also is unfortunately true that the number of research centers of this kind, funded by corporations, but not devoted to directly applicable research, is extremely small, so this is noteworthy for that reason.

7. **AGeek**
   February 6, 2008

On a more general level, so not specifically about this particular institute, am I the only one bothered by the deputy director being literally in bed with the director? Something about healthy organizations having checks and balances, and about not mixing private and professional roles…?

8. **Chris Oakley**
February 6, 2008

   This new institute is probably a good thing. Maybe they will be able to persuade MS that (i) ISupportErrorInfo ought to be made to work in VB/VBA for module functions returning an HRESULT as well as COM classes [they ignored me when I pointed out the inconsistency a few years ago] and (ii) that allowing user-defined garbage collection schemes, as is currently possible for COM, would be a useful feature for .NET as well.

9. **Peter Woit**
February 6, 2008

   AGeek,

   In this case I think de facto the two people involved will be jointly filling the same role of directing the center, so as long as they can work effectively together, I don't see a problem. This situation is becoming pretty common in academic departments, where spouses are often hired together. It does open up all sorts of potential problems, but in practice these normally are not so difficult to deal with, and institutions find that dealing with them is better than dealing with faculty struggling with a two-body problem in other ways.

   Chris,

   I suspect that some problems in computer science will always remain beyond the capabilities of researchers to ever understand and solve…

10. **Peter Shor**
February 6, 2008

    I suspect that there will be a small portion of the new center devoted to statistical mechanics. This is the research area of its directors, and they have already shown that statistical mechanics can be of some use to Microsoft's business.

11. **Ali**
February 6, 2008

    Ageek and Peter, I do not think there is any two-body problem involved here. As far as I know, Chayes and Borgs were hired in 1997 as separate individuals to Microsoft. Their marriage took place after they started working for Microsoft. There is no such thing as two-body hire in industry to the best of my knowledge. Two-body hires mostly occur in academia.

12. **Peter III**
February 6, 2008

    Two-body hires definitely occur in industrial labs; I have seen them. Industrial labs are just as interested in getting really good people as academia, and if they have to hire another fairly good person to do so, why should they hesitate?

    In fact, in academia, sometimes Dept. A is unwilling to even consider hiring the spouse of somebody Dept. B is hiring, even if they are clearly better than many of the people currently in Dept. A, because they think it makes them look bad. I suspect this is less likely to occur in industry.

13. **Henry Cohn**
February 7, 2008

    I don't think it would be appropriate for me to get involved in speculation or debate here (I am one of the four initial MSR New England researchers), but I can report two things:

    The lab will definitely grow to be much larger than four people, and Jennifer and Christian were already married when they came to Microsoft.

14. **oz**
February 7, 2008

    My question is – who will stay in the redmond research lab?
    Loosing four of its core members, including the two directors,
    will there still be a viable theory lab there? who is going to replace them?

15. **Henry Cohn**
February 7, 2008

    My question is – who will stay in the redmond research lab?
    Loosing four of its core members, including the two directors,
    will there still be a viable theory lab there? who is going to replace them?

    There are several internal changes. Yuval Peres is replacing Jennifer and Christian as manager of the theory group at MSR Redmond, and Kristin Lauter is replacing me as manager of the crypto group. Eric Horvitz is taking on Jennifer's role as the research area manager to whom both groups report.

    The crypto group still has almost all of its members. The theory group is losing a larger fraction of its members, but Oded Schramm and David Wilson are staying there along with Yuval, and I believe they intend to do some hiring. Yuval and Kristin have great plans for their groups, and Eric is very supportive, so I'm sure everything is going to go well.

16. **Peter Shor**
February 7, 2008

    If just Yuval and Oded stay at Redmond, they will still have an incredibly strong group. Attracting new hires should not be a problem.

This Week's Hype

February 6, 2008
Categories: This Week's Hype

This week's media hype promoting a new observational test of extra dimensions is based on the recent arXiv preprint Transient Pulses from Exploding Primordial Black Holes as a Signature of an Extra Dimension. Stories about it have appeared already in Nature and in New Scientist.

Some of the authors are part of a group at Virginia Tech that is working with a radio-telescope array they call the Eight-meter-wavelength Transient Array (ETA). The possible astrophysical sources they are looking for include primordial black holes. The press articles however, aren't about this, but about the new preprint, which makes claims not about conventional primordial black holes, but about ones involving extra dimensions:

> For a toroidally compactified extra dimension, transient radio-pulse searches probe the electroweak energy scale (~0.1 TeV), enabling comparison with the Large Hadron Collider. The enormous challenges of detecting quantum gravitational effects, and exploring electroweak-scale physics, make this a particularly attractive possibility.

In the New Scientist piece, astrophysicist Avi Loeb makes the comment:

> There are a lot of layers here of nonstandard assumptions… If nothing could be observed in this context, then it would not surprise me.

According to the ETA web-site and the New Scientist article, as far as the extra-dimensional business is concerned, the project is led not by the faculty members involved, but by first author Mike Kavic, a graduate student in the department. Unlike most recent examples of such hype, which appeared in conjunction with the acceptance or publication of a paper in PRL, this one is based solely upon the submission of a paper to PRL.

Comments

1. **Coin**
   February 6, 2008

   I guess at least this one is a step up, relatively speaking, in that it could be realistically experimentally tested in the near term?

2. **broken pot**
   February 6, 2008

   'Unlike most recent examples of such hype, which appeared in conjunction with the acceptance or publication of a paper in PRL, this one is based solely upon the submission of a paper to PRL.'

   Ah. Next time I submit to Physical Review Letters, I'll send a press release off immediately.

3. **Peter Woit**
   February 6, 2008

   Coin,

   There really isn't any sort of usual testable prediction here. To even consider the issue of whether there's evidence of extra dimensions, first one has to find a signal from a nearby exploding primordial black hole, and that seems to not be likely to happen anytime soon (although of course it is worth trying).

4. **Thomas Larsson**
   February 8, 2008

   Not related to any of the topics in this posting, but still sort of on-topic, the following comment was censored from cosmic variance:

   More about aether compactification can be found here

5. **woit**
   February 8, 2008

   Thomas,

   While I disagree with Lubos about a lot of things, his two postings about Sean Carroll's promotion of Boltzmann brain research and now highly unmotivated extra dimensional research seem to me to be pretty much on target. It's too bad Sean is not allowing links to them.

   I would like to think that blogs have an important role to play in dealing with the problem of overhyped speculative theoretical physics research. Unfortunately the otherwise mostly sensible Cosmic Variance blog has recently been actively spreading this kind of thing, whereas the otherwise mostly not sensible Reference Frame has been actively fighting it.

6. **Professor R**
   February 28, 2008

   For once, I thought the NS article on this subject was reasonably balanced.

   I liked the inclusion of Loeb's comment above, but I also have sympathy with Kavic's statement …"while definitely a gamble, the payoffs from such a search would be enormous…the successful detection of the kind of black hole explosion would confirm not only the existence of extra dimensions, but also of primordial black holes"

   Some very good experiments have sometimes resulted from such speculative ideas….it seems to me that the real problem is that NS (and other publications) give such articles the same weighting as conventional science – where they should really be put in a section marked 'speculative papers'…….Cormac

Worth Reading

February 6, 2008
Categories: Experimental HEP News, Uncategorized

The latest issue of the Cern Courier contains a wonderful article entitled From BCS to the LHC by Steven Weinberg. It is based on a talk he gave at a recent conference celebrating the 50th anniversary of the BCS theory of superconductivity, and explains the relation between electroweak symmetry breaking and superconductivity, by way of telling about some of the history, in which he played a central role.

From the Mathematical Intelligencer, there's an excellent review by Leila Schneps of a book which contains much of the correspondence over the years between two of the greatest mathematicians of the last century, Grothendieck and Serre. The review covers not just the mathematics, but also the very different personal styles that were part of what was such a fruitful interaction. She refers to the existence of "a much larger collection of existing letters" from the later period in his life when he had begun to stop regularly doing mathematics which are still unpublished, one of which answers Serre's question about why his mathematical research program had come to a halt. She ends with the summary:

> In some sense, the difference between them might be expressed by saying that Serre devoted his life to the pursuit of beauty, Grothendieck to the pursuit of truth.

Barry Mazur has a new article giving his very personal take on the philosophy of mathematics: Mathematical Platonism and its Opposites.

MSRI celebrated its 25th Anniversary last week, and Dan Freed gave a talk on Chern-Simons-Witten theory (slides here). He is careful to put a warning label in red on the standard path integral definition of the theory, writing "this path integral is only a motivating heuristic". Together with collaborators Mike Hopkins and Constantin Teleman he has been working on coming up with a very abstract definition of the theory, far removed from the path integral, but at the end he notes that in the stationary phase approximation one can make sense of the path integral, putting up a page from one of his old papers where this was shown calculationally.

The latest Physics Today has a long article about the disastrous budget situation for HEP in FY2008. The politics of this are described as follows:

> Congress and the administration took turns blaming each other for the bad news. The omnibus bill "turned its back on Congress's concern for competitiveness," Marburger said, by wiping out most of the increases for science and technology that had received strong bipartisan support in the America COMPETES Act, which was signed into law in August 2007.
>
> But the White House was hardly without fault. Bush's 11th-hour refusal to negotiate with Democrats on a spending ceiling he had imposed forced lawmakers in the dead of night to trim back spending bills that had been assembled and approved in a far more thoughtful process. In doing so, they unsurprisingly took their red pen to presidential priorities. The increases for the physical sciences were part of Bush's American Competitiveness Initiative to revitalize US technological leadership. Marburger said he had little doubt that Congress has deliberately chosen the science programs for the budget-cutting scissors.
>
> Ironically, House Speaker Nancy Pelosi's (D-CA) "Innovation Agenda" proposed to double nondefense R&D spending over 10 years. Admitting that funding levels this year fall short of the 7% annual increases needed to meet the goal, Pelosi assured the scientific community in a letter that her commitment to growing the physical sciences budgets "remains strong and steadfast."

In the Chicago Tribune, Fermilab director Oddone describes what he thinks about the budget process. He now has to fire 200 people, while a huge budget increase is proposed by the administration, which the Congress probably won't act on until deep into the next fiscal year, with no indication now of what they will do:

> This is not the way a developed country manages a scientific enterprise… It's more like a banana republic.

Update: One more. A popular talk by Richard Taylor about reciprocity laws and density theorems (such as Sato-Tate) in number theory.

Comments

1. **DB**
   February 8, 2008

   Chapter 21 section 6 of Weinberg's The Quantum Theory of Fields Vol.2 (pp.332-352) contains a useful technical treatment of how symmetry breaking relates to superconductivity and the BCS theory. Although a superconductor is a material in which just the electromagnetic gauge invariance is spontaneously broken, he explains some of the interesting parallels between it and the electroweak theory, for example, the distinction between Type I and Type II superconductors which is mirrored by a corresponding distinction in the electroweak model between theories where the scalar mass is less than or greater than the W and Z masses.

2. **wb**
   February 8, 2008

   RE: "Oddone describes what he thinks about the budget process. He now has to fire 200 people, while a huge budget increase is proposed by the administration, which the Congress probably won't act on until deep into the next fiscal year."

   Oddone's situation is mirrored by that at other labs. By September hundreds will be off the job. If Congress were actually pass the President's budget or something close to it regarding science, the national labs (FNAL and SLAC in particular) would see enormous increases (well over the President's FY2008 levels) that they would be greatly understaffed to spend wisely. One might have wished that DOE had been wise enough identify this year of disaster opening as a golden opportunity to rebuild the university science infrastructure while still bringing the Labs to a state of health. Alas the top management of America's great universities have not yet been wise enough to communicate this to the Administration. But it is Congress that writes the checks, so let them begin.

Is Big Physics peddling science pornography?

February 7, 2008
Categories: Uncategorized

There's a new round of nonsense about theoretical physics making its way through the media, especially the British tabloids. The original source is a preprint from a few months ago by Aref'eva and Volovich entitled Time Machine at the LHC (it refers to another earlier one by other authors If LHC is a Mini-Time-Machines Factory, Can We Notice?). These papers discuss the possibility that the LHC will produce not just black holes, but also wormholes that would be "Mini-Time-Machines" (MTMs).

New Scientist now has a cover story based on this which begins:

> As you may have heard, this will be the year. The Large Hadron Collider – the most powerful atom-smasher ever built – will be switched on, and particle physics will hit pay-dirt. Yet if a pair of Russian mathematicians are right, any advances in this area could be overshadowed by a truly extraordinary event. According to Irina Aref'eva and Igor Volovich, the LHC might just turn out to be the world's first time machine.

The article invokes work by Nima Arkani-Hamed and others to justify the idea that the LHC will produce black holes and possibly wormholes, and Kip Thorne to justify the possibility of time travel. Several physicists are quoted in favor of the plausibility of the underlying idea, it not its practicality.

The story has now made it to the Sun, which has two stories: Time Travel Russia's in and Visits From Crack to the Future. According to the Sun, the LHC will be switched on in May (not true….) and from that time on time travel will be possible:

> The laws of physics suggest that no one from the future will be able to travel back any further than when the machine was switched on — with 2008 being Year Zero.

According to the Daily Mail:

> Time travel could be a reality within just three months, Russian mathematicians have claimed. They believe an experiment nuclear scientists plan to carry out in underground tunnels in Geneva in May could create a rift in the fabric of the universe.

The Telegraph has Time travellers from the future 'could be here in weeks', but the article at least has some skeptical quotes, for instance from David Deutsch, who describes the idea as "not cranky", but unlikely to work.

New Scientist does seem to realize that this kind of silliness may have gone too far, publishing an article by Michael Hanlon entitled Is Big Science peddling science pornography?. I think Hanlon raises extremely important questions that the physics community needs to address, although he makes a mistake by pinning this on "Big Science". The people working hard to make projects like the LHC a reality are not the culprits here, irresponsible theorists are. Hanlon writes:

> Physics and cosmology stories are like this these days. Once it was all hard sums and red-shifted galaxies; awesome enough one would have thought. Now it's time machines and universe-eating particles.
>
> Does any of this bear any relation to reality? Or is Big Physics guilty of some serious sexing-up, drifting away from the realm of hard data and into the softer universe of science pornography?
>
> As well as accidental time machines we are told of cosmic strings – gigantic filaments of super-stuff that warp and tear space-time like ladders in a pair of celestial stockings – and crashing branes, titanic slabs of maths that give rise to the big bang in the exotically lovely ekpyrotic universe of Neil Turok.
>
> Not crazy enough for you? What about the multiverse? One of the biggest sell-out lectures at last year's Hay-on-Wye festival in Wales starred the UK's astronomer royal, Martin Rees, who entertained his audience with a discussion of the possibility, indeed the probability, of multiple worlds – endless parallel realities existing in a gargantuan super-reality that makes what we think of as the universe as insignificant as a gnat on an elephant's backside. Or there's the simulation argument, philosopher Nick Bostrom's delicious idea that since it should be possible to replicate an entire universe in a computer, and that this could be done countless times, statistical cleverness proves that we are not the real McCoy but the figments of some electronic entity's imagination.
>
> …Scientists, and people like me who stick up for science, are happy to pour scorn on astrologers, homeopaths, UFO-nutters, crop-circlers and indeed the Adam-and-Eve brigade, who all happily believe in six impossible things before breakfast with no evidence at all. Show us the data, we say to these deluded souls. Where are your trials? What about Occam's razor – the principle that any explanation should be as simple as possible? The garden is surely beautiful enough, we say, without having to populate it with fairies.
>
> The danger is that on the wilder shores of physics these standards are often not met either. There is as yet no observational evidence for cosmic strings. It's hard to test for a multiverse. In this sense, some of these ideas are not so far, conceptually, from UFOs and homeopathy. If we are prepared to dismiss ghosts, say, as ludicrous on the grounds that firstly we have no proper observational evidence for them and secondly that their existence would force us to rethink everything, doesn't the same argument apply to simulated universes and time machines? Are we not guilty of prejudice against some kinds of very unlikely ideas in favour of others?

Update: The time travel story has even made it to the Chronicle.

Comments

1. **Steve Myers**
   February 7, 2008

   Science porn — a good term since it's a substitute for the real thing.

2. **Chris Oakley**
   February 7, 2008

   "Pornography" may have been the term used by *New Scientist* but *The Sun* runs the genuine article in mild form on Page 3 each day in the form of a un- or scantily-clad babe (the "Page 3 girl"). They missed an opportunity in not combining this with their article about the LHC:

   "Delicious Debbie (19) from Dagenham, Essex, is looking forward to the opening of the LHC in May. "Yeah, it's gonna be great: I mean, just last week I left my lip gloss in a taxi. If there was time travel I could've gone back in time and put it in my bag before I got in." Did Debbie take an interest in theoretical physics, we asked? "Oh, no," she replied, "I couldn't get in to drama school, so I got a job as a secretary, didn't I?""

3. **Bee**
February 7, 2008

I had plenty to say about the topic, but I've said it all before, therefore I'll just link to [Fact or Fiction?]()

4. **Yatima**
February 7, 2008

Well, I don't care about the SUN – getting steamed up about their science reporting is not truly worthwhile, but New Scientist had a perfectly good article on mass extinctions not being caused by random asteroid impacts but cyclic world-wide resurgences of bacterial populations – an awesome idea. They could have used that for the cover page and relegated the time travel speculation to a footnote instead. The editor needs to be handed a fist-sized raspberry.

In related news, Garrett Lisi is interviewed in the lengthy cover article of the french popular scientific mag "Science&Vie" of January.

5. **David Nataf**
February 7, 2008

Yatima,
That's pretty consistent of New Scientist magazine.
They will in general have a sensationalist cover story, with interesting popular science articles on the inside. The sensationalism is always about how theoretical physics is breaking down. Superstrings, loops, the ether, time machines, et cetera.
The best popular science magazine for the educated layman is American Scientist.

6. **Big Vlad**
February 7, 2008

i despair of this sort of rubbish. We as scientists have no grounds on which to dismiss ghosts and fairies while this stuff is being printed. It's not even as if the sun has made this up – it's based on an actual arxiv preprint!

I really can't wait until the LHC results come in and destroy 99% (100%?) of the currently fashionable theories, and then experimenters and phenomenologists will take over the world.

7. **Janus**
February 7, 2008

The difference, of course, is that people actually believe in ghosts, UFOs, homeopathy, creationism, etc. They don't think of it as interesting remote possibilities that may be tested experimentally one day. They believe in them so strongly that they're willing to spend considerable amounts of time, money, and/or energy on these things.

I don't think scientists should refrain from formulating hypotheses, even hypotheses that seem crazy to most laymen, as long as they acknowledge that they are merely hypotheses. If anyone needs to be criticized, it's the ignorant media people who blow everything out of proportion.

8. **Peter Woit**
February 7, 2008

Janus,

The question is not whether scientists should refrain from formulating hypotheses, but whether they should issue press releases or otherwise encourage mass media to write about these hypotheses when they are extremely speculative. No media person is going to write anything about a new scientific result if the scientist involved tells them that it's something that is very unlikely to be true and shouldn't be promoted to the public. The media may be swallowing too much hype, but it is scientists who are feeding it to them.

9. **chris**
February 8, 2008

janus,

are you sure that – say – suskind does not *believe* in the multiverse? i would not bet at least.

and to attribute this 'simulated world' idea to anyone in particular is just so plain silly. back when i was young, i discussed this over a beer with my school buddies and heck, there even where holywood films about it already.

my lingering suspicion on why these subjects are so popular is that once you got into the mood (i.e. the community recognizes you as 'serious' contributor) you can crank out a paper per month with minimal effort. compare that (and the possibility of getting media attention) to a 4-loop qcd calculation and it should be obvious why baseless speculation is so sexy to researchers.

10. **Anonymous**
February 8, 2008

Who has more investment in nonsense?

The difference, of course, is that people actually believe in ghosts, UFOs, homeopathy, creationism, etc. They don't think of it as interesting remote possibilities that may be tested experimentally one day. They believe in them so strongly that they're willing to spend considerable amounts of time, money, and/or energy on these things.

For people, it is a hobby or obsession. For physicists, it is a career, a life's work.

11. **Mark Wallace**
February 9, 2008

It seems like too many professors in this decade confuse their love of science fiction with the discipline of physics. I was a physics undergrad in the early 80's (Berkeley), and I recall that all my professors back then would become extremely irritated by persistent undergrad questions about time travel and other such nonsense. I appreciated their attitude at the time, and wish it were more prevalent now.

12. **Chris W.**
February 9, 2008

Mark,

I guess what you're saying is that we could use a bit more willingness to humorlessly dismiss such stuff as a muddle-headed waste of time.

There *are* ways to discuss time travel seriously, as a way of getting at subtle aspects of the laws of physics. The trouble, the people who ask about the topic are usually not that interested in the laws of physics.

13. **Peter Shor**
February 9, 2008

I remember reading some popular article by Kip Thorne where he said that he was very reluctant to publish his work on time travel, I think because it was too speculative and science-fictiony to be considered serious science. He therefore spent a lot of time thinking about the consequences of time travel before he published anything.

There should be some correlate to "extraordinary claims require extraordinary proof" along the lines of "extraordinary speculations require extraordinary groundwork." If you're going to introduce unconventional ideas, you should spend a lot of time figuring out as many of their implications as you can.

14. **William Straub**
February 9, 2008

Physicists have never lacked for exciting theories and ideas, but now they're being sexed up to sell magazines and cable science shows as well as to court purse-string politicians. But why is nutty physics being popularized to such an extent today?

As a lay person, I think the answer follows from the previous comments about belief in UFOs, homeopathy, ghosts and creationism. Sometimes I think that's all physics is to these believers — just another means of legitimizing a lot of non-scientific nonsense. And I will add to that my assertion that most of today's popularized high energy physics and cosmology is nothing but entertainment being hawked all too often by respected authorities in their fields using flashy, bewildering computer graphics and preposterous ideas with little or no educational or informational value.

Dumbing-up sounds like a good idea, but I don't see it working.

15. **Peter Shor**
February 10, 2008

When I was growing up (1960's and 1970's), I remember thee being extensive belief in UFO's, new age medicine, and so on. But I don't remember high energy physicists going out on limbs popularizing nonsense the way they're doing now. Of course, maybe I couldn't have told the difference between serious physics and nonsense at the time. But I suspect it was because high energy physics was going through a very exciting time (the Standard Model was being discovered and experimentally tested) and there was lots of real physics to popularize.

Of course, there's exciting physics to popularize now. Dark matter, dark energy, inflation, neutrino mass, all sorts of interesting exoplanets, stars and galaxies the astronomers are seeing, etc. But the particle theorists and string theorists are left out, and maybe some of them want to play, too.

16. **DB**
February 10, 2008

"But the particle theorists and string theorists are left out, and maybe some of them want to play, too."

There is probably something in this, particularly as much of the hype is coming from theorists who have spent the last twenty five years vainly struggling to create a successor to the Standard Model, and have precious little to show for it. Many are now over 45 years of age, and in mathematical physics it's extremely rare to make important breakthroughs after that age. So these individuals, many of them in eminent positions at prestigious institutions, are in an invidious position. Do they shut up and let the world pass them by, or do they try to "talk their book" in the hope of persuading young talented theoreticians to take up the torch, while helping their postdocs make tenure by exaggerating the importance of the field and its achievements to date. I'm convinced that it's the latter process that's at work here. It's about legacy and jobs.

17. **Anonymous**
February 10, 2008

As an outside observer who loves math and physics, I don't think that there is necessarily anything wrong with speculative ideas. I think the problem is when those speculative ideas are just that, ideas, and not well reasoned arguments for why something is possible.

We don't live in a special time in this regard, certainly there are plenty of earlier examples of theories that were later proven completely ridiculous. I think the danger is similar to what we see in the intelligent design debate; where people are willing to tell lies under the banner of science, simply because they call the lie a "theory".

Such things undermine legitimate science, and shakes the public's confidence in the ability of science to provide answers.

18. **Eric**
February 10, 2008

What's really sad is that Discover magazine has a special issue this month devoted to Einstein. One article in the magazine is a listing of the 'next Einstein'. Number one is the famous surfing, independent physicist who just 'published' a paper. Ed Witten is listed as number six. This is really deplorable.

19. **Peter Woit**
February 10, 2008

Eric,

I don't know about Witten as the "next Einstein", I think he's more kind of the "current Einstein". As for the "next" one, I don't see many convincing candidates.

Funny, but you don't seem to be bothered by the feature article in that issue of Discover by a string theorist about time travel (from a forthcoming book).

20. **Eric**
February 10, 2008

Peter,
The book by Kaku that you link just seems to be a variation on the 'Physics of Star Trek' theme of Lawrence Krauss who is most definitely not a string theorist.

The hyping of Lisi is an absolute travesty. How can anyone conclude that his inclusion in this article as nothing but a product of the alternative physics hype with which you sympathize and support? If the public cannot tell the difference between a real physicist and someone like Lisi, how can we ever expect proper financial support from the US government for serious projects?

21. **Migo**
February 10, 2008

It may be somewhat unrelated, but for some different kind of nonsense about theoretical high energy physics, you might want to have a look at the preprint 0802.0216. There the author claims to have found an argument proving that the mathematical structure of QFT is inconsistent. Unfortunately, he does not know what a representation of the translations is in QFT, and is therefore led to false conclusions. This example really makes we wonder where theoretical physics is going these days …

22. **Peter Woit**
February 10, 2008

Eric,

If you pay attention to this blog, I think you'll see that the only "alternative physics" hype that I really sympathize with and support has to do with the idea that some active topics of mathematical research like geometric Langlands have important relations to quantum field theory, and better understanding this may someday lead to new physics. My attempts to hype this don't seem to have gotten very far. Of the people on the Discover magazine list of "new Einsteins", there's only one I support, who shares a bit my point of view, and I don't think he's very "new".

I don't know what "alternative physics" is. If you mean "alternatives to string theory", since string theory has failed as a TOE, I support people looking for alternatives, even if they're ones I don't personally find very promising. Sure, Lisi-mania was an unfortunate example of media hype, but I just don't think there's the slightest danger that NSF and DOE funding of Lisi studies will crowd out conventional theoretical physics research. If this shows any signs of happening I'll devote postings here to the Lisi-hype problem.

23. **Peter Woit**
February 10, 2008

Migo,

The arXiv is just a preprint server, and as such has a large helping of wrong and otherwise worthless submissions. Most of these are just completely ignored by everyone. For this one in particular, your comment is probably the only attention it has ever gotten or ever will get. I don't think there's the slightest danger it will be come the subject of media hype…

24. **Migo**
February 10, 2008

Peter, you are right, one should probably just ignore stuff like that. But given the far-reaching claims this preprint makes, and the potential for confusion it might cause among people not knowing QFT very well, I thought it would be a good idea to have some trackback to a short discussion indicating that it's wrong. Yes, plain wrong in this case, instead of not even wrong …

25. **Peter Woit**
February 10, 2008

Migo,

I deleted the "trackback" part of the your link, because it was mal-formed, wouldn't have worked anyway (except in certain special cases, trackbacks to this blog are censored by the powers-that-be at the arXiv), and the last thing I want here is extended discussions of what is wrong with every worthless preprint posted to the arXiv, (this would be a huge and extremely unrewarding topic).

26. **neo**
February 11, 2008

Some of the ideas are really goofy, others are not. They seem to have been bundled together haphazardly. For example, the ekpyrotic theory is no more speculative and certainly philosophically more pleasing that eternal chaotic inflation.

27. **Chris Oakley**
February 11, 2008

I will be interested to see if Kaku has corrected an error in his previous book where he claims that my great-uncle W J van Stockum was Scottish when the only time he lived in Scotland was when he was doing doing his Ph.D. (1935 to 1937) – not that I care much, BTW, as I really doubt that GR is more than a weak-field approximation, but getting details like that right does help to boost peoples' confidence.

28. **Valerie Jamieson**
February 11, 2008

Yatima, I'm one of the physics features editors at New Scientist and take some responsibility for what goes on the cover. Why do we (and other popular science magazines) put so many theoretical physics and cosmology stories on the cover? Because big physics sells. At this time of funding cuts in the UK and US and worries over student numbers, surely it's heartening to find so many people getting excited about the big questions that physics addresses.

29. **anon.**
February 11, 2008

'At this time of funding cuts in the UK and US and worries over student numbers, surely it's heartening to find so many people getting excited about the big questions that physics addresses.'

Valerie,

New Scientist, as I'm sure you know, has been promoting speculative, non-checkable ideas since string theory came to fame over two decades ago. The fall in student numbers, see http://www.buckingham.ac.uk/news/newsarchive2006/ceer-physics-2.html doesn't correlate to Woit's blog or even to the popularity of the internet, but it does correlate to the rise of speculative stuff on your front covers:

'Since 1982 A-level physics entries have halved. Only just over 3.8 per cent of 16-year-olds took A-level physics in 2004 compared with about 6 per cent in 1990.

‘More than a quarter (from 57 to 42) of universities with significant numbers of physics undergraduates have stopped teaching the subject since 1994, while the number of home students on first-degree physics courses has decreased by more than 28 per cent. Even in the 26 elite universities with the highest ratings for research the trend in student numbers has been downwards.

‘Fewer graduates in physics than in the other sciences are training to be teachers, and a fifth of those are training to be maths teachers. A-level entries have fallen most sharply in FE colleges where 40 per cent of the feeder schools lack anyone who has studied physics to any level at university.’

One thing that is clear is that hype of speculative uncheckable string theory has at least failed to encourage a rise in student numbers over the last two decades, assuming that such speculation itself is not actually to blame for the decline in student interest.

However, it’s clear that when hype fails to increase student interest, everyone will agree to the consensus that the problem is a lack of hype, and if only more hype of speculation was done, the problem would be addressed. Nobody will believe that a reduction in speculative hype could possibly address the problem, or that changing the focus of the front cover of New Scientist to more solid areas of physics would help. Electronics and computing innovation of the real world variety (not quantum computing hype from qubit/Deutch) for example, has been censored from New Scientist as too boring. I’m not including my name here as this isn’t a personal matter.

Maybe the vast number of excited readers of New Scientist physics sci fi hype who don’t take up A-level physics as a result, take up writing science fiction or take up religious orders, instead?

30. **Chris Oakley**
February 11, 2008

Anon.,

Forgive me for pointing out the obvious, but one of the reasons that fewer students are taking A-level physics is that there are more options available for the technically-minded student, mostly related to electronics and computing, which have come on in leaps and bounds, both theoretically and practically, since then.

31. **anon.**
February 11, 2008

Chris,

Thanks, but those technically-minded students of electronics and computing could also do an A-level in physics (which is an allied subject), instead of avoiding it like the plague which is what currently occurs.

32. **JC**
February 11, 2008

anon, Chris

A better question to ask from an historical perspective is, did science hype/pornography increase the number of engineering, physics, and math majors back in the 1960’s? Or did the increase in engineering, science, and math majors have more to do with Sputnik era increases in science funding? Or was it a more mundane reason like the sheer large numbers of baby boomers attending university in the 1960’s?

33. **anon.**
February 11, 2008

JC: this is about a fall in the *percentage* of students doing physics, not a fall in birth rate. Disillusionment with physics is the problem, otherwise physics would be widely taken in *addition* to electronics, chemistry, computing, or maths.

‘Or did the increase in engineering, science, and math majors have more to do with Sputnik era increases in science funding?’

Here in the UK, the funding of physics isn’t the key problem, which is student numbers. Funding has to follow students. You can’t really save a department with no students by increasing funding. It’s really not a money-related. When physics ‘hype’ stopped being tied to facts and went sci fi, physics became a not just nerdy but really weird and cult-like, which didn’t appeal to the technically-minded.

34. **Thomas Love**
February 11, 2008

JC, The increase in science majors in the 1960’s was at least in part due to the fact that a man was more likely to obtain a draft deferment if he was a math, science or engineering major. This was true at least with my draft board.

35. **Tony Smith**
February 11, 2008

JC asked “… did science hype … increase the number of engineering, physics, and math majors back in the 1960’s? …”.

My personal experience may be merely anecdotal,
but
I was born in 1941 and grew up reading stories (even in comic books) about such things as:
the idea of nuclear chain reaction fission;
nuclear fusion related to elliptical pool tables with a hole at one focus;
computers (rooms of tubes with punch cards) that could calculate stellar structure and evolution;
rockets sending sputniks (and cosmonauts) into orbit;
transistors for little radios and for calculators more accurate than a slide rule (and for smaller computers); and
nuclear submarines that could stay under the sea for as long as the crew could tolerate.

Those things were the hype of that time,
and they did in fact motivate me to study science and math,
because
they were REAL and they really CHANGED the world.

In contrast, today’s superstring hype does not deal with anything that is really changing today’s world, and is so far detached from reality that the superstringers have yet to connect their ideas with even the esoteric reality of existing results from high-energy particle physics experiments.
Whether or not some future experiment might or might not connect with some idea from the superstringers is an open question,

but
the hard cold fact is that as of now there is no such connection with existing experimental results.

So, if I were growing up now, I would see a lot of superstring hype
but
I would NOT see any connection between the hype and reality,
unlike
the connections with nuclear, computer, rocket stuff that was really REAL when I was growing up.

Tony Smith

PS – It is also interesting to compare two types of recent hype:

1 – superstring with no connection to reality

2 – hedge-fund-type finance, with very clear connection to the reality of the world's economy

and to observe that a lot of people over the past years have left the unreal world of superstrings for the real money hedge-fund world.
(Whether or not they constructed a flawed monster whose collapse could cause a depression is another story, but either way their economic construct is quite real.)

In short, hype connected with reality attracts people to work on a subject,
but
hype without reality (superstring theory, TV/movies/games about vampires, witches, magic, etc) is viewed by most people as mere fantasy,
not worthy of being taken seriously as life-work.

36. **a quantum diaries survivor**
February 11, 2008

Mark Wallace Says:
It seems like too many professors in this decade confuse their love of science fiction with the discipline of physics. I was a physics undergrad in the early 80's (Berkeley), and I recall that all my professors back then would become extremely irritated by persistent undergrad questions about time travel and other such nonsense. I appreciated their attitude at the time, and wish it were more prevalent now.

Mark, the undergrads of the early eighties are the professors of today… A generation of misfits 🙂

Cheers,
T.

37. **Anton Szautner**
February 13, 2008

A NewScientist cover story breathlessly touts 2008 as the incipient "Year Zero" and repeats the new-and-improved time-travel mantra: "…travelling into the past is only possible – if it is possible at all – as far back as the creation of the first time machine."

Well, for crying out loud already: what's the universe if it ISN'T already a "time machine"?

All this mindless hooplahype ignores that the LHC is less than a factor of ten more powerful than Fermilab's Tevatron – a very small incremental increase in a vastly larger energy scale nature plays with. If those energies are what permit MTMs, then the big bang already made them in vast abundance and Year Zero remains Year Zero.

Yet, so what? Where are the time travelers beating a path through their past to us from a "future" that hasn't happened yet? Are we to presume the implication that all of those potential futures are already somehow coagulated into a "present" configuration which allows for the existence of time travelers? If so, we're dealing (as the last sentence implies) with a past-tense version of a future 'already' formed.

Ridiculous.

38. **Vicky**
February 13, 2008

In response to anon:

In terms of declining student enrollment, I suspect that it has more to do with the lack of jobs at the end of the road than whether the professors are too "sci-fi." If one wants a job in academia or a public sector laboratory, the competition is fierce, and if you want a job in industry, engineering may be a better path.

Because of this, I have read some argue elsewhere that more money spent on physics will create more jobs which will entice more students to fill them. Sadly, that is unsustainable.

Most people will not excel at their career. A mediocre engineer works as a mediocre engineer. A mediocre mathematician has a variety of options. A mediocre physicist competes with trained engineers to become engineers or with trained mathematicians to become mathematicians. In either case, it makes more sense to pick a path that is viable from the start.

That is why I left physics to become an engineer…I never would have gotten a permanent position in physics (I later became a lawyer out of public interest.) I suspect that type of concern deflects others from studying physics.

The question shouldn't be how to excite people to study physics…it is intrinsically exciting enough to anyone likely to invest their life in the field. The question is how to make the field relevant enough to others in a practical sense so that they take more than an introductory course to satisfy another major's requirements, or to justify the public spending billions of dollars on research. And I don't think that science porn will do that.

39. **Professor R**
February 14, 2008

hi Peter, I too noticed the read the Hanlon article, but i disagree with both him and you…
It is not Big Science, or a few theorists, who are peddling sceince pornography – it is NS themselves.
The theorists didn't ask to be the cover story, that's the magazine's choice. Some theories will be more speculative than others, its more a question of balanced reporting, surely.

Bit coy of Hanlon not to mention this (he could do, as he is not their employee)…Cormac

40. **woit**
February 14, 2008

Cormac,

I agree with you that New Scientist has a lot to answer for, but still think they're not the only ones. Scientists can't change the fact that popular media want to make their stories as sexy as possible, but they can take responsibility for what they do and say when they are speaking publicly, and when they are contacted by journalists.

41. **Professor R**
February 15, 2008

Peter – I think you're right, there is also the onus on the scientist to make clear where in the spectrum this particular hypothesis lies…

that said, someone should suggest to NS that they have a special category headed "speculative science"- that way they could indulge their whims without damaging the credibility of science

42. **Vigesh**
February 16, 2008

Yeah 3 weeks… but then if they invent time machine… talking about when they did is irrelevant as the existence of time machine will be(I guess) the (least) time where(/when) they traveled.

Quantum Field Theory, As Seen By Mathematicians

February 11, 2008
Categories: Favorite Old Posts, Uncategorized

In recent years, interest in quantum field theory among mathematicians has gone through ups and downs, in a sub-field dependent manner, as ideas rooted in quantum field theory have turned out to be mathematically useful in a variety of contexts. At Berkeley, it appears that there's now more interest in quantum field theory in the math department than in the physics department, so much so that last fall three senior faculty (Nicolai Reshetikhin, Peter Teichner and Richard Borcherds) offered courses on various QFT topics. Luckily, Berkeley graduate students seem to be very industrious, and they have accumulated quite a few tex-ed lecture notes from these and other courses, gathering together the QFT ones here and here.

Another popular topic at Berkeley has been geometric Langlands, a subject where Witten's QFT approach has intrigued many mathematicians. Witten has a new preprint aimed at mathematicians promoting the QFT point of view, and he explains in more detail claims he made in talks last fall that the use of 4d QFT corresponds in a sense to the use by mathematicians of "stacks". Stacks are a mathematical device useful for handling non-free quotients, situations where one wants to keep track not just of the quotient space (which is often singular), but of more structure, for instance the varying stabilizer groups at different points of the quotient. Witten notes that if one just uses mirror symmetry of the Hitchin moduli space to study Langlands duality, one doesn't know how to handle various singularities. Mathematicians have dealt with these singularities by invoking stacks, Witten instead argues that one should use a 4d gauge theory QFT perspective to see how to study the issue in a way that does not involve these singularities (they only appear when you dimensionally reduce and work with the 2d topological sigma models).

Last week Witten gave a talk to the mathematicians at the IAS on "Duality from Six Dimensions", which is to be continued this week. He explained how the existence of a 6d superconformal theory implies SL(2,Z) symmetry and thus duality (Montonen-Olive duality) in the 4d N=4 supersymmetric topological gauge theory he uses in his approach to geometric Langlands. This is an old story by now, from the mid-nineties duality days, and Witten wrote up some of it here, for his contribution to the proceedings of the conference in honor of Graeme Segal's sixtieth birthday back in 2002. David Ben-Zvi was at the talk taking notes, and I hope he'll be adding to his extensive collection of on-line notes this semester since he'll be at the Institute attending this year's program there.

Note added: David has started posting his notes, the notes from the Witten talk are here.

At MIT this semester there's a "pre-Talbot" seminar being run that will lead up to a Talbot workshop in March. The topic is something that might be called "quantum geometric Langlands", involving not Witten's QFT ideas, but a version of geometric Langlands that uses quantum groups due to Dennis Gaitsgory and Jacob Lurie. Scott Carnahan discusses this at Secret Blogging Seminar, and has notes from his overview talk available.

In March Lurie will be giving the Marston Morse lectures at the IAS, on the topic of "Topological Quantum Field Theories in Low Dimensions".

Comments

1. **David Ben-Zvi**
   February 11, 2008

   Hi Peter — yes I'm taking notes and will start posting them
   very soon (under a heading as notes from
   this semester's activities)

   On another note the quantum geometric Langlands
   program fits very nicely with the Kapustin-Witten
   TFT point of view — the "quantum" parameter
   here is exactly the parameter Psi that appears
   in the 4d TFT (which is a combination of the gauge
   coupling of N=4 superYangMills and the parameter
   that's used in defining the topological twist).
   The idea has been around on the math side
   for a while but Dennis and Jacob have formulated it much
   more precisely (and given a "local" version), and are
   explaining its relation to quantum groups.. it's worth pointing out
   that the same parameter appears as the level of a Kac-Moody
   algebra or (when a positive integer) as the level in
   Chern-Simons theory or (when exponentiated) as the q in
   quantum groups..

2. **A.J.**
   February 11, 2008

   Also worth noting: Peter Teichner is running another Hot Topics course this semester, this time on the work of Freed, Hopkins, & Teleman. I'm not sure if anyone is publishing notes this time, but I'll definitely be blogging about the course from time to time.

3. **David Ben-Zvi**
   February 12, 2008

   Here's the link to the IAS term notes (I'll add to it
   as the term progresses):

   http://www.math.utexas.edu/users/benzvi/GRASP/lectures/IASterm.html

4. **Peter Woit**
   February 12, 2008

   Thanks David, both for the great public service of providing the notes, as well as for the indications of the the frightening degree to which these ideas are all interconnected…

5. **Voltberg**
   February 13, 2008

QFT must be more important for mathematicians than it is now, especially for those in small departments, because it touches so many areas of mathematics. That forces to learn many developments in unconected areas.

6. **Florian**
   February 18, 2008

   Peter, thank you for this very informative post!

Late Night HEP TV

February 13, 2008
Categories: Uncategorized

Particle physics is enjoying a wave of popularity on the late night talk shows this week. On Friday, MIT experimentalist Peter Fisher appeared on the Conan O'Brien show, helping O'Brien see how long he could keep his wedding ring spinning on his desk.

Last night it was a theorist's turn, with Lisa Randall appearing on the Colbert Report, promoting the idea of extra dimensions.

Comments

1. **Analyzer**
   February 13, 2008

   Conan's on on Saturdays now?

2. **woit**
   February 13, 2008

   Analyzer,

   My bad, it was Friday night. Shows how well informed I am about popular culture. Fixed.

3. **Luzo**
   February 13, 2008

   The movies of Conan's show are down so maybe you can include this link.

   http://www.youtube.com/watch?v=371D99KpVZ4

4. **anon.**
   February 14, 2008

   I watched the video link of 'Colbert Report: Lisa Randall'. I didn't find it funny. String theorists will go to any lengths to hype the claim that string theory is checkable and predicts the weakness of gravity, without making any solid calculations. It's just pseudoscience. Colbert should have asked for the alleged (non-existent) formula for the weakness of gravity and if something was supplied off-the-cuff on the back of an envelope, he should probed how it was derived. Everything Colbert did say was purely pro-nonsense, just giving more airing to vacuous hype. Extra dimensional hype was funny for a day sometime around 1985, but the joke is wearing a bit thin nowadays.

5. **Tom Whicker**
   February 14, 2008

   I actually think Colbert did a great job. In a four minute interview he got to some essential questions: Colbert, "This is all very fancy, but is there any way to make a buck off this?", and next " Is this just theoretical math, or is there some experiment you can do?"
   He also suggested it was similar to looking for "angels or god".

6. **Eric**
   February 14, 2008

   Anon,
   Lisa Randall is not a string theorist and was not discussing string theory on the show. As for the testability of large extra dimensional theories, this is clearly possible at LHC.

7. **IMHO**
   February 14, 2008

   The Colbert with Lisa Randall was brutal…

   There are better representives than her…she's not the only good looking Theoretical Physicist.

8. **Luiz**
   February 14, 2008

   IMHO,

   Yeah, right. Do you care to name a few?

9. **lostsoul Ph. D.**
   February 14, 2008

   Luiz

   Britney, and the girl out of the Wonder Years – get serious. Professor Randall did a good job, given the circumstances. At last she's not a politician.

10. **anon.**
    February 14, 2008

    Eric: yes, Lisa does work on some alternative extra dimensional ideas to mainstream string, but it seems that such ideas are string theories; at least, they are Kaluza-Klein theories with extra spatial dimensions.

    Remember, it's widely claimed that not all dimensions were necessarily compactified to unobservable size in the landscape of 10^500 variants of the Calabi-Yau are supposed to have become unravelled into vast cosmic strings that astronomers should be able to see (when they point their telescopes in the right direction, and remove the lens cover).

Lisa's idea is that gravitons, unlike electromagnetic gauge bosons, are free to propagate in an extra spatial dimension, and this dilutes the gravitational interaction relative to electromagnetism, whose photons can only move in observable spacetime dimensions.

M-theory in fact has 11 dimensions, with 10 dimensional superstring resting like a (mem)brane or a surface structure on an 11-dimensional complete 'bulk'.

Lisa's idea would suggest that in M-theory photons are confined to the 10 dimensional brane (3+1 spacetime dimensions + 6 compactified spatial dimensions), but gravitons can also travel through the 11 dimensional bulk.

Because the gravitons have one extra dimension to travel in, they appear to us in 3+1 spacetime dimensions to give rise to a gravitational coupling constant weaker than electromagnetism, because gravitons spend less time on the brane than photons do.

Photons are a bit like a film of oil floating on the surface of a bulk of water, very concentrated (giving strong electromagnetism), whereas gravitons are like dye thrown into the bulk of the water, which dilutes them throughout the entire volume not just the surface (brane). *C'est magnifique, mais ce n'est pas la science*.

11. **woit**
February 14, 2008

Please, direct discussions of Lisa Randall's appearance to Tommaso Dorigo's blog…

As for whether she's a string theorist, here's what she has to say on the topic, from

http://www.sciencewatch.com/july-aug2001/sw_july-aug2001_page4.htm

"Do you consider yourself a string theorist?"

"I just don't like labels in general, but I certainly don't object to being called a string theorist if it's said in a nice way."

12. **Eric**
February 14, 2008

Dear anon,
You are confusing extra dimensional brane-world theories and string theory. While it's true that string theory gives a concrete realization of brane-worlds with extra dimensions, they are not necessarily the same thing as Professor Randall herself would emphatically tell you.

13. **Fru**
February 15, 2008

As for Lisa Randall's preference for being called string theorist or not, reading her response from the link that Peter provided I wouldn't conclude that she is since, after saying what Peter reported, she adds
"Basically, I'm just a theoretical physicist who, like all the rest of us, would like to figure out how the world works. If that involves some string theory, great, but ultimately I think we should be able to connect it to what we see in the world and be able to test it. I'm just trying to put those things together."
which seems to expose a different attitude compared to the pure abstractism that people don't like (for those who don't of course) about string theory.

14. **Uncle Enzo**
February 15, 2008

Who cares if Lisa Randall is a "string theorist" or not. She's a theoretical physicist working on stuff that interests her. Also, Stephen Colbert is a comedian, not Barbara Walters, and The Colbert Report is supposed to be funny, not serious. If you watch the show, you'll realize that those interviews aren't supposed to be all that serious, as evidenced by Colbert's behavior. Remember, the show is on Comedy Central.

Anon: "Colbert should have asked for the alleged (non-existent) formula for the weakness of gravity and if something was supplied off-the-cuff on the back of an envelope, he should probed how it was derived."
If he asked those types of questions, nobody would laugh. The show is a comedy. None of his interviews are designed to be serious.

15. **wb**
February 15, 2008

Not only was the spot with Conan and Fisher more entertaining, but also Peter demonstrated something very important to the public. Physicists can figure out things that we all can we.

16. **a quantum diaries survivor**
February 16, 2008

Damnit, Peter!!! 🙂

…Ok, I'll admit it – if mine could be a good gossip blog I'd take the traffic and say thanks. But I will never be a good host for gossip. I suck – my interest in the human nature is quite limited (say, to one particular gender and one particular age range).

So people, if you want to discuss the appearance of Lisa Randall forget Peter's advice – he was being facetious – and rather go to Cosmic Variance. There you'll find people really willing to take you on that.

Cheers,
T.

17. **martin**
February 16, 2008

If anyone really wants to discuss Lisas ideas, come over here 😉 (I welcome any scientific chat about randall-sundrum apart from pseudo philosophic branish discussions…)

On-topic. Is it just me or does conan appear kind of disrespectful? I don´t know his usual attitude, but he tries to turn everything in the conversation into (mostly lame) jokes.
And besides that prof. Fischer did a good job, in my opinion.

18. **Tom Whicker**
    February 17, 2008

    Lisa Randall gave a much longer and more serious TV interview with
    Charlie Rose on December 12, 2006. You can google it or get the entire 40 minutes on YouTube. Here is a transcript from about 11 minutes in, where she explains her ideas on extra dimensions. I think the transcription is close to word accurate, but please view the video as it is probably a bit unfair to make opinions based on text versions of live, off-the-cuff discussions.

    Lisa Randall to Charlie Rose, Dec 12, 2006:

    "The idea of extra dimensions has been around for a long time. I mean Abbott wrote his book in the late 19th century, and Einstein completed his theory of relativity in 1915, general relativity, and
    only a few years later somebody named Theodor Kalusa proposed the idea of an extra dimension.

    "So why is it that physicists today are thinking about extra dimensions? Well, one of the reasons is that we think it might actually have something to do with our universe, I mean that's for me I think the most important reason.

    "But another reason is in fact string theory; it has introduced the idea that maybe those dimensions are really there because that's the only way the theory makes sense.

    "But string theory has also introduced something else, in the 1990s the physicist Joe Polchinski realized there were these other objects in the context of string theory called branes…they're named
    branes now..that word is sort of related to membrane…
    and the idea is that there could be, even if you have higher dimensions, even if you have a fifth spacial dimension or a fourth spacial dimension out there, there could be objects in the universe called branes that don't spread throughout the entire universe, and maybe stuff is stuck on those lower dimensional surfaces, so here we have an analogy [points to sketch of a shower curtain] just to give you the general idea, I mean you might have a three dimensional room but in that room there might be a shower curtain and on that curtain there could be water droplets, and those water droplets are only on the two dimensional surface of the curtain. They're not going out throughout the three dimensional room; they're really stuck there. And in the same way [refers to another sketch] it could be that we and the stuff of which we are
    made could really be stuck to a brane… so this brane…although it's drawn as two dimensional, think of it as a three dimensional surface…so if you have this three dimensional brane and the stuff was stuck there, it would look just as if the universe was three dimensional and if..everything..if.. if for example photons and electromagnetism could only travel in the three dimensions of the brane, well that would explain why things look as if they're three dimensional. After all, light doesn't get outside the brane. So it could be that some of it is actually still stuck in three dimensions even though another dimension is out there.

    "What makes it interesting though, from the point of view of physicists ..I mean if everything was stuck in the three dimensions on the brane, who cares if there are other dimensions? We'd never interact with them.. But it is ALWAYS true that gravity goes out in those dimensions and so those squiggly lines in that figure
    represent the fact that gravity always interacts with the other dimensions; gravity will never be stuck on this brane. And that's what's so interesting with these branes..you can get the geometry of space-time….we're used to thinking..we physicists at least.. are used to thinking of space as the same everywhere, as far as we know the laws of physics are the same throughout the three dimensional universe..but it could be that in these extra dimensions and because of the existence of these branes and energy throughout the universe, spacetime can be warped, I mean even in OUR universe it is warped to some extent; that's why there's interesting cosmology. But with this other dimension it could be very dramatically warped and it's THAT that can explain the properties of gravity that we see in our universe, because gravity IS spreading out in those dimensions and so it could be that gravity isn't just spreading out evenly everywhere; it could be highly concentrated for example in the region of a brane, and that's one idea that we got very excited about."

19. **Eric H**
    February 18, 2008

    Ms. Randall is much more specific in the Rose interview than what she seems to be saying recently. I'm beginning to wonder if the reason there is less specificity and more hype in "all" recent public discussions is because physicists are wary in being specific because they themselves now see the contradictions.

    For instance, when Kalusa proposed an extra dimension is was a dimension that circled back on itself, like a circle. Well, it just so happens, (not that we all don't already know it), that there is a particle physics analogue which is spin. So, at least physics had a physically observable behavior to attach to the math. I just don't see anything similarly concrete to attach to additional mathematical dimensions. The same with branes. Is it just me or does it seem like a lot of this later stuff some how got detached and isolated from experimental physics and split off like a bubble. It seems to me that a lot of this work may be intrinsically interesting but it is pure b.s. to try to make the case that this higher dimensional math is attached to physics.

20. **Eric H**
    February 18, 2008

    "I'm beginning to wonder if the reason there is less specificity and more hype in "all" recent public discussions is because physicists are wary in being specific because they themselves now see the contradictions."

    Make that "string theory physicists" as I didn't mean it to come out as a pejorative generalization.

21. **Martin Bauer**
    February 18, 2008

    I think you should not take the extradimensions "too serious". It might be that you get measurable predictions by extra dimensional theories but that doesnt mean that you will ever be able to travel in an additional direction. It should be understood like complex numbers. A mathematical tool which could help us out of the actual misery.

    But nevertheless I agree that (probably because the people which work on these problems today are so familiar with pure mathematics) the temptation to cross the line and think beyond to actually contribute something is huge.

    Not much to falsify at the moment. Sometimes you got the feeling el. particle physics devides into Maths and Names nowadays.

LHC Startup News

February 18, 2008
Categories: Experimental HEP News

I'm a bit confused about the situation with the LHC start-up schedule, maybe someone well-informed can help. Here's what I've seen recently:

At the AAAS meeting last Friday, there was a session on "Big Science", which included discussion of the LHC with Robert Aymar, the CERN director general. Alan Boyle, at MSNBC, reports:

> June was the time frame Aymar had in mind when he was asked about the start-up schedule during a Friday session on large-scale science project. But during a follow-up chat, he pointed out that you can't just press a big red button one day and expect each of the collider's beams to hit full power of 7 trillion electron volts immediately…
>
> Aymar said that buildup could still start around May 21 or 22, with tests continuing for weeks after that. His aim is to have the collider conducting scientific experiments this summer.

One thing that is definite about the schedule is that there will be a ceremonial inauguration of the machine on October 21, with a wide variety of dignitaries present, include French president Sarkozy.

It's not clear what phase of the start-up Aymar had in mind when he was referring to June, and the idea that the machine will be doing physics this summer seems hard to reconcile with information available publicly about how the things are progressing. For an official schedule from last August, see here. The current one, from October, is available here. Both schedules have beam commissioning beginning May 15, and taking about two months, so physics in July.

The LHC is divided into 8 sectors, and each sector must go through a process of flushing, cooling down to 1.8 K (which takes a month and a half), and powering tests of the magnets. The powering tests are crucial to make sure that the magnets can quench safely, dissipating the energy contained in a magnet that leaves the superconducting state unexpectedly. According to the schedules, the powering tests should take 2-3 months, and can start only once the magnets are cool. So, from beginning of cooldown to the point that a sector is ready to try and use should be a process of about 4 months or so. So, for mid-May beam commissioning, all sectors should be cool by around now and starting powering tests soon.

One can follow the actual state of affairs here. One sector (45) is cool and undergoing powering tests, but this sector still has not had its inner triplet magnets fixed, and the plan is to warm it back up before doing this, after which it will need to be cooled down again. Cooling of 3 other sectors has begun, but has been stopped in two of these to make repairs, with cooldown to resume in week 9 of the year for one sector, week 11 for another. Of the four other sectors, cooldown is supposed to start in one of them during week 15 (mid-April), dates are not given for the others. With respect to last August's schedule, the current situation is roughly 4-5 months behind where it is supposed to be. This would suggest that, if all goes well from now on, beam commissioning would begin mid-September. Perhaps there was some slack in that schedule, and things could happen faster than planned, but I'm just not seeing how physics this summer is in the cards for the LHC. Most likely scenario seems to be a big push to get some a beam of some sort stored in the machine in time for October 21 and the big ceremony.

Comments

1. **DB**
   February 19, 2008

   One thing is sure, Aymar is proof that humans exhibit the intrinsic property of spin. Dorigo, for example, doesn't expect the LHC to begin running until the end of the year, and then only at very low luminosities, and no-one really knows how long it will take to ramp the beam up to useful luminosities.

2. **caesar**
   February 20, 2008

   I doubt dorigo can be trusted on these matters. For what I understand from his blog, he basically extrapolates from his past experience with Run I and Run II at the Tevatron, rather than relying on hard information he does not really seem to have access to. He might still be right, though.

   However there is one important thing that is often overlooked. It is easy to understand the problems with cooling down the LHC components, with getting a stable beam, with fixing occasional incidents. But once those are straightened out, there is one major additional hindrance: even if LHC starts producing large numbers of collisions, the two detectors will have to figure out how to best trigger on them. Sure, at the very low initial luminosity a beam clock may be enough to rely upon for a complete readout of the detector. But still, getting good, meaningful data to tape is not a trivial task. If all we care is satisfy our curiosity on whether the LHC creates a black hole that eats out Geneva overnight we may be satisfied from day one, otherwise it will be very, very slow and painful.

US HEP Demography

February 18, 2008
Categories: Uncategorized

Late last week there was a meeting of HEPAP in Washington, presentations available here. Several dealt with the current budget situation, which is basically that the current FY2008 budget is a disaster, and the Bush administration has proposed huge compensatory increases for FY2009. No one seems to have any idea what Congress will do or when, so the future for US government support of HEP is completely unclear not only for the long-term, but even for the next fiscal year, which starts in a few months.

The NSF presentation noted that NSF funding for particle physics theory was down 4% in FY2008, to about $14 million, of which roughly $1.5 million goes to the KITP at Santa Barbara. The critical issues for the NSF particle theory program were listed as:

- Need to involve more people in LHC-related physics.
- Need new hires in phenomenology.
- Traditional funding sources for students (being TAs) is becoming problematic. (need more funding for students)

You can see why string theorists these days are pushing the idea of "string phenomenology" and claims that somehow string theory is relevant to the LHC.

At the DOE, funding for theoretical particle physics was flat for FY2008, at $60 million, with a proposed 5% increase for FY2009.

There was also an interesting presentation about an on-going project to gather demographic information on the people working in particle physics. I was surprised to see that statistics show significant recent increases at all levels in the numbers of people working in particle physics. From 2003-2007 the number of graduate students went from 1129 to 1335 (making one wonder why the NSF is worried about not supporting enough graduate students…), postdocs and untenured research staff from 1331 to 1406, untenured faculty from 228 to 284, and tenured faculty or staff from 1343 to 1355. In particle theory, the total number of people went from 1292 to 1414, so this increase in numbers was not all in experiment.

Also worth reading is a presentation from Robert Sugar about the present state of Lattice QCD calculations.

Comments

1. **Mike**
   February 19, 2008

   Gee, flat funding year after year and the number of people in the field keeps going up. Maybe Congress must know a good deal when it sees it!

2. **Peter Orland**
   February 21, 2008

   Academic employment is determined by more than
   simply the amount of generally available external funding.

   Theoretical particle physicists may have trouble bringing in
   money to universities, but they also cost those universities very little. Hence they don't entail much risk. Though particle theorists obtain smaller (and fewer) grants, they don't have expensive demands, hence universities get to keep much of the overhead. Of course, other theorists are even better in this respect – but unlike particle theorists, they sometimes can't thrive without experimentalists in the same department.

   Despite the last decade's funding difficulties, particle experimentalists
   could be considered a worthwhile risk – when they are funded, the
   university hauls in a bundle. A risk-benefit analysis may suggest
   particle experimentalists aren't a good gamble, but universities
   usually ignore such analyses (a good example is football. Football
   teams are a money sink-hole in most universities. They are kept on because the teams which pay off, pay off big).

3. **Peter Orland**
   February 22, 2008

   P.S. It's interesting that "Late-Night HEP TV" has many more comments than this topic does.

Quick Links

February 22, 2008
Categories: Uncategorized

There seems to be a political scandal going on in Italy revolving around the GIM mechanism, with Antonino Zichichi somehow involved. Definitely a higher level of scandal than we have here in the US.

An Oral History Project at Princeton involving interviews with people associated with the Math department during the 1930s is here, and includes the following exchange with Wigner, who evidently wasn't so happy with Weyl:

> **Interviewer**: We haven't mentioned Hermann Weyl yet. Can you tell me something about your relations with him? When did you first get to know him?
>
> **Wigner**: When he came to Princeton I knew about his work, and I quoted it also. You know he was interested in group theory. But in Princeton we were really strangers to each other. He never mentioned my work in his book on the application of group theory to quantum mechanics, even though practically all that is in the book was contained in publications by me and in joint publications by Johnny von Neumann and me. I resented that because I needed a job then.

The TLS has a review of The Trouble With Physics.

For the latest from the frontiers of physics, see this at the KITP, and this at the arXiv.

There's a P5 meeting going on at SLAC, talks here.

Bert Kostant of MIT gave a talk at UC Riverside entitled On Some Mathematics in Garrett Lisi's 'E8 Theory of Everything', and as part of the festivities John Baez gave an elementary introduction to E8. There's some discussion of this at his blog. It seems that the initial reaction from some string theorists that this material is so easy that undergraduates shouldn't have too much trouble with it may have changed a bit. For a comment on the attitudes involved, see here.

Update: To try and make up for the high-level of snarkiness of this posting, here's something else. This month's National Geographic has an excellent big article about the LHC, with the usual National Geo impressive photography. No hype about extra dimensions, etc., just a serious explanation of what the LHC is all about and what physicists are trying to do, ending with the following wonderful quote:

> …I asked George Smoot, a Nobel laureate physicist, if he thinks our most basic questions will ever be answered.
>
> "It depends on how I'm feeling on any particular day," he said. "But every day I go to work I'm making a bet that the universe is simple, symmetric, and aesthetically pleasing—a universe that we humans, with our limited perspective, will someday understand."

Update: Two more

- FQXI has Phantasms of Infinity, an article on the Boltzmann Brains/counting universes hot topic among theorists. It includes an actual picture of a Boltzmann Brain, as well as a quote from Vitaly Vanchurin, who works in this area:

> Without a way of calculating probabilities, cosmology is a dead science, it doesn't exist.

I think this will be news to most cosmologists, who are happily ignoring the problem of how to count universes in the multiverse. More accurate would be "Without a way of calculating probabilities, multiverse studies is a dead science, it doesn't exist", which is pretty much the situation now and for the forseeable future.

- New Scientist has a reasonably good cover story on cosmic strings. It ends with

> Discovering them would be really big news. String theory has often been criticised as a theorists' plaything, a pretty piece of mathematics unable to make any testable predictions. That perception would change pretty fast if we were to find a host of giant superstrings crisscrossing the skies.

This is an accurate summary of the situation, although it might be worth pointing out that not only is there no evidence for cosmic strings, but there's not even anything ever observed that cosmic strings provide a compelling explanation of. At the moment they're just a pretty pure example of wishful thinking. Sure tomorrow someone may find a "host of giant superstrings crisscrossing the skies". It's also true that tomorrow aliens may land and explain to us how to compute the Standard Model parameters from superstring theory.

Comments

1. **Coin**
   February 22, 2008

   Hm. A small tossoff near the beginning of the TLS Trouble with Physics review states: "He accuses string theorists of racism, sexism, arrogance, ignorance, messianism and, worst of all, of wasting their time on a theory that hasn't delivered." It was awhile back I read Trouble with Physics, but I don't remember– did in fact Smolin accuse string theorists of racism and sexism?! I do remember a section wherein Smolin argued for greater inclusion of women etc in physics, but this was *not* as I recall specifically directed at string theorists, but rather one of several generalized criticisms of the sociology of modern science in the closing sections.

2. **DB**
   February 22, 2008

   Check pages 280 and 320 of Weyl's: The Theory of Groups and Quantum Mechanics, 2nd Edition, November 1930 which was the edition first translated into English in 1931.
   P.280: "P.Jordan and E. Wigner have given a very elegant group-theoretic proof that there exists but one irreducible matrix solution of equations (14.10)" [derivation of the irreducible Abelian group of unitary ray rotations in system space]
   P.320: "The significance of our results for quantum mechanics [symmetric group and equivalence degeneracy in quantum mechanics] as first recognized by Wigner, is the following"

   Weyl cites the source papers in his bibliography, and includes a comprehensive selection from Wigner's and Von Neumann's giving meticulous credit to their contributions.

As for “practically all that is in the book was contained in publications by me and in joint publications by Johnny von Neumann and me” well, even a cursory examination of Weyl’s bibliography gives the lie to that egotistical hype. For more details see
http://www-groups.dcs.st-and.ac.uk/~history/Biographies/Wigner.html
where it is clear that Wigner resented Weyl’s priority in this area. Wigner published his own book on the subject in 1931.
As Abraham Pais has shown, Wigner has some history of inaccurate claims.

3. **Eric**
February 22, 2008

It should be Antonino Zichichi not Antonio.

4. **woit**
February 22, 2008

Thanks Eric, fixed.

5. **anon.**
February 22, 2008

“For the latest from the frontiers of physics, see this at the KITP”

Or, you *could* look at any of the other recent talks at the KITP on heavy-ion physics or LHC physics. What is your point, anyway? One snicker-worthy talk out of many isn’t a sign of anything, no matter how much you want it to be.

6. **Peter Woit**
February 23, 2008

anon,

Well, the topic of the talk was that of the last major story in the New York Times about the latest developments in theoretical physics, and one that has been heavily promoted on certain blogs and discussed here. It seems to be considered by some to be one of the hot areas in the subject, and the audience asked lots of questions and was quite involved. So, that’s why I thought it was worthy of comment.

It’s harder to justify paying any attention to the arXiv posting I mentioned…

7. **censored out**
February 23, 2008

Thanks for the link to the recent entertaining review of Smolin’s 2006 book by the *Times Literary Supplement:*

“… Smolin has launched a controversial attack on those working on the dominant model in theoretical physics. He accuses string theorists of racism, sexism, arrogance, ignorance, messianism and, worst of all, of wasting their time on a theory that hasn’t delivered. …

“Smolin has little new to say about how the institutions of science are undermined by personal ambition, internal politics and bureaucratic overload – as he himself admits.”

– http://tls.timesonline.co.uk/article/0,,25372-2650590_1,00.html

Is it just me, or does the second sentence just quoted contradict the first one, about the excesses of some string theorists and their wasting of time on failed theories? Surely, at least by giving the example of messianism in science institutions, Smolin is saying something “new” about how scientific institutions are undermined by personal ambition, internal politics and bureaucratic overload?

Regarding the denial of credit to Wigner for his work, Wigner’s 1992 autobiography also expresses annoyance toward Fermi for Fermi-Dirac statistics. Wigner was using Fermi-Dirac statistics long before Fermi started doing so, and he suggests that Fermi should instead be given credit for beta decay theory, not Fermi-Dirac statistics.

8. **Professor R**
February 23, 2008

hi Peter,
interesting blog as always. I have two queries/comments:

1. I can’t make head or tail of that Nielsen abstract on ArXiv. Were you being sarcastic? It’s not clear to me at all what their central thesis is – certainly an example of abstract that seems designed to confuse the reader (or is it just me…..)

2. I enjoyed the TLS review of Smolin’s book, thanks for pointing it out. What surprises me is that none of these reviews ever refer to Smolin’s previous book (‘Three Roads to Quantum Gravity’); the latter is a very good book, and probably a more damning criticism of string theory because it is written in a much more objective and technical style….it’s interesting that the ‘The Trouble With Physics’ is selling far better, although less objective and a lot less concise (in my opinion)
Cormac

9. **Peter Woit**
February 23, 2008

Professor R,

Yes, I was being sarcastic. Actually that’s the third paper by these authors on this subject, and the sensible thing to do would be to just ignore them. The fact that serious physicists are engaging in this kind of thing might be of some sociological interest, and some people with a warped sense of humor might find this entertaining…

10. **Professor R**
February 23, 2008

Ah….I wonder about that abstract. Could it be something else?
I wonder if Nielsen and co. are serious physicists who are attempting to highlight the downside of the ArXiv system in today’s world of lazy journalism…
Perhaps they’re waiting for New Scientist to pick one of their papers as a cover article….at which point they’ll come clean and declare it to be complete gobbledygook!

11. **oeL**
February 23, 2008

higgs field seems to be getting some publicity… sorta of

http://forums.fark.com/cgi/fark/comments.pl?IDLink=3403229

poor ellis

12. **Roger**
February 24, 2008

Totally off-topic (and to be deleted) – more a note to Peter for a future blog entry.

There is quite a fierce campaign going on in the UK regarding the recent funding cuts, including the ILC decision.

The story is summarised rather well in http://www.hep.ucl.ac.uk/~markl/pp/

13. **Peter Woit**
February 24, 2008

Roger,

Thanks. Unfortunately I don't think I'm well enough informed about what is going on in the UK to write a useful posting about it, but I'll leave your comment there and advise people to follow the link to learn more about this.

14. **Amos**
February 25, 2008

I read (but hardly understood) the Distler criticisms of Lisi. Is Lisi's E8 theory dead now?

15. **Peter Woit**
February 25, 2008

Amos,

Probably best if you ask this question at the place I linked to, where there are experts on this. I haven't followed it carefully.

16. **A Quantum Diaries Survivor**
February 26, 2008

Hi Peter,

thank you for linking my post on the Glashow-Carlucci querelle – it got visited by a large audience in the last few days. Yes, it is a scandal and no, it is not. Yes, because to us it is indeed a scandal that a politician with no understanding whatsoever of the physics takes the liberty to insult esteemed physicists by pretending to argue on the scientific merit of this or that paper. No, because for many in Italy scientists are perceived as a bunch of snobs who pretend they cannot be criticized, and so to their eyes Carlucci's allegations are perfectly sensible.

Cheers,
T.

17. **Amos**
February 27, 2008

Peter, I find your comments on these issues generally less partisan and more reasoned than those of others on the 'net, so hoped to get your opinion. But thanks.

Peter Goddard on the Birth of String Theory

February 25, 2008
Categories: Uncategorized

Last spring there was a conference held in Florence which brought together many of those who worked on dual models and string theory during the late sixties and early seventies. Slides from the talks are here, and many of the speakers have written up contributions that have been posted on the arXiv. The latest of these is From Dual Models to String Theory, by Peter Goddard, who now is the director of the IAS in Princeton. It contains a detailed description of what he remembers of those early days when people were trying to sort out the significance of the Veneziano amplitudes and how to consistently quantize the string. Goddard also has some interesting remarks on the rapid changes in fashion during those years, and some excerpts follow.

On his student days at Cambridge working under Polkinghorne during the late sixties:

> I, and nearly all my fellow research students, worked on strong interaction physics. (One of us was trying to work out the correct Feynman rules for gauge field theories, but this tended to be regarded as a rather recondite or eccentric enterprise.)

At a summer conference in 1971:

> For me, it was a memorable meeting and one particular vignette has stuck in my mind as an illustration of the prevailing attitude towards the use of modern mathematics in theoretical high energy physics. A senior and warmly admired physicist gave some lectures on the Regge theory of high energy processes. With great technical mastery, he was covering the board with special functions, doing manipulations that I knew from my studies with Alan White (who was also at the School) could be handled efficiently and elegantly using harmonic analysis on noncompact groups. Just as I was wondering whether it might be too impertinent to make a remark to this effect, the lecturer turned to the audience and said, "They tell me that you can do this all more easily if you use group theory, but I tell you that, if you are strong, you do not need group theory."

About his years (1970-72) at CERN:

> The two years I had spent in CERN had built up to an crescendo of intellectual excitement and, though I have found much of my subsequent research work gripping and often extremely satisfying (when teaching duties and the largely self-inflicted wounds of administration have permitted), nothing has quite matched this period. In particular, I had the privilege of working closely for seven or eight months with Charles Thorn, whose combination of deep perception and formidable calculational power had provided the basis of what we managed to do. And, the exhilarating combination of the open and cooperative atmosphere that prevailed amongst (almost all) those working on dual models in CERN, the relative youth of most of those involved, the sense of elucidating a theory that was radically different, even the frisson of excitement that came from doing something that was regarded by some of those in power as wicked, because it might have nothing directly to do with the real world – this cocktail would never be offered to me again.

About the situation in 1973-4 , after the discovery of asymptotic freedom:

> By the end of 1973, as the fascination of dual models or string theory remained undimmed, though with ever increasing technical demands, the interest of many was shifting elsewhere. On 21 December, David Olive wrote to me, "Very few people are now interested in dual theories here in CERN. Amati and Fubini independently made statements to the effect that dual theory is now the most exciting theory that they have seen but that it is too difficult for them to work with. The main excitement the renormalization group and asymptotic freedom, which are indeed interesting."
> …
>
> In Berkeley , I wrote a largely cathartic paper on supersymmetry, which probably helped no one's understanding, except marginally my own. It had one memorable effect: namely, that when I reached Princeton I was invited to give a general seminar on supersymmetry, which most people did not know much about then. When I said I would rather talk about string theory, my offer was politely declined on the grounds that no one in Princeton was
> interested, a situation that has changed in the intervening years. Somewhat put out by this response, I did not give a seminar at all.

About his decision in 1975-6 to work on gauge theory rather than strings:

> I started to realize that following my interests in strings or dual models might be a fine indulgence for me, but it was not going to help my students get jobs. (One of the great attractions of Cambridge at the time was that chances for promotion were so slim – Jeffrey Goldstone was still a Lecturer – that one did not need to be distracted by the prospects for advancement: they seemed negligible.)

Comments

1. **Chris Oakley**
   February 25, 2008

   Peter Goddard – I remember him from Cambridge, c. 1981. He taught us "Advanced Quantum Field Theory" for the Part 3 Mathematics course which, at the time, mostly meant gauge theories and the Higgs mechanism. Mild-mannered, with the obligatory shabby donnish jacket, and hair that looked like it had been cut by his wife or daughter. Great lecture notes, though – legible handwriting and a half-decent job done of explaining things (not that I accepted the Higgs mechanism – even then – of course, but …)

2. **S-P Chan**
   February 25, 2008

   -similar story-as an undergrad at St John's College Cambridge early 80's I was supervised by Peter Goddard in Quantum Mechanics and other sundry applied math courses.

   He was the archetypal professor-type, very gentle (re my crappy undergrad tutorial work) and helpful.

3. **csrster**
   February 26, 2008

   Yes, I remember him lecturing me on something-way-over-my-head during a brief foray into mathematical physics in 1986. I may not have learned much in Part III Maths but at least I got to add some A-list names to my "tried to teach me to think" list – Gary Gibbons, Martin Rees, George Efstathiou, Don Lynden-Bell …

4. **Sandro**
   February 26, 2008

"I started to realize that following my interests in strings or dual models might be a fine indulgence for me, but it was not going to help my students get jobs"
I find this utterly remarkable: the number of Ph.D. supervisors willing to do this nowadays is really close to, if not, 0. Seriously.

5. **Peter Woit**
February 26, 2008

Sandro,

There's a big difference: in the mid-seventies, strings were out of fashion, now they are in fashion.

Goddard is right when he points out that one consequence of working on something unfashionable is that it is hard for one's students to get jobs. But of course there are others. He notes that one won't get promoted to higher positions, and that's why he explains that promotion at Cambridge was so unlikely that this wasn't much of a concern. Other serious consequences are that one is likely to essentially become a nobody in one's community, not invited to conferences, etc., etc. See his story about how people at Princeton wouldn't let him talk about what he was working on, but did want him to talk about the trendy idea (34 years ago and counting…) of supersymmetry even though he didn't know much about it and wasn't very interested.

Back in 1973-75, gauge theories were fashionable, but for extremely good reasons. The Standard model had just been born and revolutionized physics, and gauge theories were not well-understood at all. The revolution had opened a whole new area of theoretical physics, full of new things to do, and deeply grounded in and vindicated by experiment. To ignore this and continue to work on string theory, which was going nowhere and had failed in what it was supposed to do (explain the strong interactions) was to make a decision that most of one's colleagues would see as perverse, motivated purely by an unwillingness to learn anything new, and insistence on sticking to doing the same thing one had been doing since graduate school.

For nearly a quarter-century now, the shoe has been on the other foot, with string theory the fashion, and work on gauge theories unfashionable (acceptable only if it at least involves supersymmetry…). The common opinion has been that those who don't work on string theory just can't cut it as serious theorists. The sociology may be similar to the reverse situation of the 1970s, but scientifically there is a huge difference: gauge theories worked and were a huge success, string theories have been a huge failure. Despite this the political situation remains the same. Any theorist who decides to work on something unfashionable is going to have trouble getting jobs for his or her students, and get told by people at Princeton that they don't want to hear about it….

6. **Sandro**
February 26, 2008

Peter,

you are right, but I think I had something different in mind.
I was referring to the attitude towards students as to "people that have got to get a job in the next future", and not as to "people which are carrying on curiosities that I'd personally like to solve but I've got no time to".(At least that's how I read it.)
This is something I think few supervisors do, whatever their discipline is, and yes, they are not obliged to, but..

7. **csrster**
February 27, 2008

I wonder if there's just something different in the air in Cambridge – or perhaps everywhere in Europe – that makes it easier for individuals to work on their own "interesting" ideas away from the dominant paradigm? I can't imagine an external speaker being turned away by Cambridge just because they're working a little outside the box. (I _can_ imagine them being given a very rough ride if they haven't thought through what they're talking about.)

8. **censored out**
February 27, 2008

csrster, it's just money. Cambridge is the richest university. You used to be able to walk from Cambridge to Oxford without stepping off land owned by Cambridge University. It can still afford to find highly speculative research, e.g. Josephson's 'Mind-Matter Unification Project', http://www.tcm.phy.cam.ac.uk/~bdj10/

9. **csrster**
February 27, 2008

Isn't that Oxford-Cambridge thing just one of those urban legends? And are you saying that Princeton is poor 🙂 ?

As for Josephson, I suspect that the reason behind that is the Nobel Effect – ie once you've got a Nobel Prize you can get away with anything (with the possible exception of outright racism).

10. **MathPhys**
February 27, 2008

Peter Goddard is a very good lecturer and an excellent supervisor. Back in the 70's, 80's and 90's, his students learnt a lot, graduated and many of them got academic jobs. I know. I'm not so sure if he still teaches and supervises students at IAS.

11. **Professor R**
March 4, 2008

hi Peter,
I thought you might be interested to know that one of the excerpts above from Peter Goddard's excellent talk pretty much mirror my Dad's experience. In the period 1974-75, Lochlainn was invited to tour several US universities in the US, in order to give talks on the emerging theory known as super gauge symmetry (a much better name in my opinion!).

As a result, Dad had to furiously brush up on the work of Wess, Zumino et al (although he had worked quite closely with Wess before). Just like Goddard, Dad then felt the need to formally put down what he had learnt on his return, resulting in Lecture Notes on Supersymmetry (published by the Dublin Institute of Advanced Studies in 1975). I've been wondering if this is the world's first formal series of lecture notes on the topic..

Before and after, I think Lochlainn's interest remained in the general area of gauge theory (or the application of group theory to gauge theories)….not in the specific area of string theory. He would not have been pleased with a dominance of string theorists over others in hiring practice – however, it seems this is much more of an issue in the US and the UK, than in continental Europe or Japan.
Cormac

12. **Ben**

March 4, 2008

MathPhys-
There are no students to be taught or advised at IAS. I'm 26, and suspect I'm the youngest person here. You can, in theory, adopt students from Princeton, but this is relatively rare, and I doubt Goddard has time for it, being director and all.

Simons Center for Geometry and Physics at Stony Brook

February 27, 2008
Categories: Uncategorized

Today here in New York City there will be a formal announcement by governor Eliot Spitzer of a gift by Jim Simons of $60 million dollars to fund a new research center at SUNY Stony Brook, to be called the Simons Center for Geometry and Physics at Stony Brook. Simons already made a donation of $25 million dollars to Stony Brook back in 2006 to support math and physics, with the idea of getting such a center off the ground. People there had told me last year that they were expecting Simons to fully fund an expensive new center with a new building once they had managed to find a suitable director, and recently I had heard that string theorist Michael Douglas had accepted the director's position.

This is the largest gift ever made not only to Stony Brook, but to any of the institutions in the SUNY system. Besides the building and the position for Douglas, it is supposed to fund 30 visiting positions and presumably a sizable number of permanent positions in mathematics and physics (the 2006 gift also is supposed to pay for such positions). The scale of this should make the Simons Center among the best funded institutions in this field. Job prospects for string theorists have just improved significantly…

For more details see stories from the [New York Times](), [Newsday](), and [Crain's Business Report]().

Update: More [here]().

Update: More in the New York Times [here](). The $60 million includes the previously announced $25 million, and will pay for a new building as well as an endowment of $40-45 million. The endowment will fund the director's position, 6 more permanent positions, and 30 postdocs and visiting positions.

Comments

1. **Chris Oakley**
   February 27, 2008

   Just to say how delighted I am to hear that despite minor teething problems (lack of a theory, lack of ability to make predictions, etc.) String Theory is considered to be alive and well by scientifically literate billionaires.

2. **Stephen J. Summers**
   February 27, 2008

   Are we to understand that "mathematics + physics = string theory"
   alone? Or even that "geometry + physics = string theory" alone?
   I get this impression from reading the mentioned sites. What a waste such a center would be if it is run with that credo. Does anyone know if the new director is a string theory ideologue,
   or does he recognize that there are other realms in mathematical
   physics which are of physical relevance?

3. **Peter Woit**
   February 27, 2008

   Stephen,

   I suspect that hiring decisions will be made by a group of people from both mathematics and physics, with the mathematicians interested in hiring mathematicians in a range different fields that touch on physics in one way or another. I don't know what the physicists will do: in recent years there has been an unfortunate prejudice in physics departments to identify string theory with the mathematical end of physics. I also don't know to what extent Douglas is a "string ideologue", but given his background and that of some of the other people involved with this, I predict that a lot of string theorists will get hired at the Simons Center.

   Many years ago when I was a postdoc at Stony Brook, even then the math and ITP groups had a pretty close relationship. But when grant money appeared to support math-physics, from what I remember they pretty much split it down the middle, with mathematicians hiring mathematicians not necessarily so interested in physics, and physicists hiring physicists not necessarily so interested in math. This Center is operating on a very different scale and these are very different times, it will be interesting to see how they deal with this.

   In his comments about the gift, Simons specifically refers to the mathematical significance of quantum field theory as well as string theory, so perhaps this will be reflected in who they hire. These days though, I can think of few physics departments interested in hiring mathematically oriented quantum field theorists who are doing things completely unrelated to string theory. Maybe Stony Brook will be different…

   One other problem is that if you try and look for young people trained in physics departments who are working on mathematically sophisticated approaches to QFT, but not string theory, your hiring pool is pretty small and consists solely of the professionally suicidal.

4. **layman**
   February 29, 2008

   Very interesting post. I was a grad student in ITP in the 80s. So, this new center will be a different building from ITP. With Micheal douglas as the new, improved, C.N. Yang?

   Does the 60 billion really fund a new building or will those guys be given offices in ITP?

5. **Peter Woit**
   February 29, 2008

   layman,

   Unfortunately, it's 60 million, not billion…

   My understanding is that the Simons Center will not replace the YITP but be a separate entity, with the YITP continuing as before. There definitely will be a new building, or at least an addition to the current one, I'm not sure which. The current math/YITP building actually has one blank side, since the original plan was to build farther in that direction, but they stopped due to not enough money. I’m not sure if this was during the era of Simons as dept. chair. It would be funny if the building project he could not complete as chair now gets done with his personal money.

Rock Guitars Could Hold Secret to Universe

February 27, 2008
Categories: This Week's Hype

From the Bolton News:

> ROCK guitars could hold the key to the origins of the universe, hundreds of young science pupils were told.
>
> The Institute of Physics held a lecture in Bolton entitled "Rock in 11 dimensions: where physics and guitars collide".
>
> And acoustics physicist Dr Mark Lewney told more than 600 youngsters who attended that the vibration of guitar strings may answer unsolved questions about the Big Bang.

This event is just one of a year-long lecture series promoting string theory at schools throughout Great Britain. According to the promotional material the LHC will help verify string theory experimentally (and it will start up in May….).

Comments

1. **Thomas Love**
   February 27, 2008

   They're starting the brainwashing early. Let's just wait and see what the LHC produces. But the presenter has done "years of research into guitar physics at Cardiff University" which of course makes him an expert on string theory and elementary particles. That's as logical as anything I've read in a book on string theory.

2. **nigel cook**
   February 28, 2008

   String theory and rock have much in common: intoxication, many groupies, and putting vibrating strings to a use that makes a few people very rich and famous. String theory as a rock culture makes sense. One reason to go to rock concerts is socialising. People investigating string theory may likewise do it to fit into physics culture and to get funding, just as venues hire and promote popular rock groups. Others study string theory because they really believe it's the best thing on sale, similar to arguments for liking a particular rock group or even a political party or religion.

3. **csrster**
   February 28, 2008

   Why "Rock" guitars? Don't other types of guitar have vibrating strings? Or does the universe also have twin Floyd Rose humbucker pickups, a scooped fretboard, and a Jackson custom knife-edge tremolo arm?

4. **Steve Myers**
   February 28, 2008

   What amps is he using? How many watts? Is it 5 or 10 strings? Or 10^500?

5. **Eric**
   February 28, 2008

   Well, actually surfing holds the key to the universe. Just ask the 'next Einstein'.

6. **Professor R**
   February 28, 2008

   Surprise!
   I actually heard this talk when Mark Lewney came to our college last year. The surprise is that the talk was very very good (as talks for schoolkids go), a real performance…

   The title is misleading – the talk was mainly a first introduction to particle physics, with emphasis on the forthcoming LHC experiments – what it is for, what it might detect. Mark used the guitar to great effect – more as a prop/gimmick/musical interlude than anything to do with string theory (hard to explain uness you were there). In addition, the guitar was used for several analogies, not least in describing bound states …

   Re string theory, the theory was only mentioned towards the end of the talk, and Mark was v careful to stress that it lies in the speculative area of physics…

   All in all I amazed by this lively talk, and so were the students – you can see reviews from other physicists (favourable) on the IoP website…..Cormac

7. **Sultan**
   February 28, 2008

   Sounds like that line from Spinal Tap: It goes to 11.

8. **Ken Muldrew**
   February 28, 2008

   Didn't Pythagoras already try this tactic (but with a lyre instead of a guitar)?

9. **jpd**
   February 28, 2008

   Re: Erics surfing comment. The rest of the world may think surfing is strange,
   but growing up in California, its not extraordinary at all. Its like being surprised if someone from Switzerland skis. They have mountains, we have an ocean, get over it.

10. **otto schtirlitz**

February 28, 2008

Peter,
What do you think about this article?

http://discovermagazine.com/2008/mar/13-e-nste-n/

Great PR work by Lee Smolin and a sad state of affairs for science is all I can say. Poor Ed, I hope he's got a good sense of humor and is not embarrased by this stuff.

11. **woit**
February 28, 2008

"otto schtirlitz"

This isn't exactly on-topic and your question already came up a couple weeks ago:

http://www.math.columbia.edu/~woit/wordpress/?p=651#comment-34790

I gather that Lubos is pretty upset about this and sees it as somehow the work of that "hostile imbecile" Peter Woit, and evidence of the decline of Western Civilization.

No, I had nothing to do with it and Lee Smolin's list of "new Einsteins" would not be mine. Two problems with even thinking about such a list are that:

1. No physicists have made much progress at all in fundamental theory, so calling anyone a "new Einstein" is pretty silly.

2. Witten's achievements are so off-scale that if your list is longer than one, it's going to include people who are not in his league.

I strongly suspect Ed is not too traumatized by finding out that Lee considers him a new Einstein and has included him in a list of others with much lesser achievements.

12. **Observer**
February 28, 2008

Sadly this is another shoot on the foot for string theory.

13. **Dr Lewney**
February 29, 2008

Christ it's chilly in here. There's enough negative energy to make a bleedin wormhole.

Mea culpa for the poster and press releases. I'm afraid such painful oversimplification of an hour's lecture into a few soundbites which are suitable for a local paper and which stand a chance of getting kids to come to the frigging lecture is sadly necessary in today's competitive educational climate.

But thanks to Professor R, the only one here who has actually seen it, for the kudos. I state explicitly that String Theory might be a load of bollocks, and endeavour to be even-handed in explaining the issues. No, I'm not a particle physicist, but I had to submit the script to four UK experts and their reviews were very positive. And I'm not even that arsed if it is a "shoot on the foot" (irony? nah) – if it increases the uptake of physics A level amongst the audience even slightly, I'll be chuffed.

Cheers

Mark

14. **Professor R**
February 29, 2008

hey Mark, great to see your post!
You should know they're still talking about your lecture here (an obscure country college in Waterford, Ireland). Myself, I think it was a very good example of performance, and I try to incorporate some of your tricks into BB talks I give to local schools. Sadly, most lecturers don't think in these terms when they mount the podium, (including this year's Tyndall lecture, no comparison).

In fairness to Peter, I think his bailiwick is not string theory, or string theorists, but the constant over-emphasis of this area of physics in the media (and in the jobs market). A lot of theoreticians would agree with this, including some prominent sting theorists who were cronies of my Dad.

My own view is that if there's something about strings that grabs the public imagination (more than quarks, for example), I too am happy to use it as a 'hook' to reel in young people to the wonderful world of physics….indeed I suspect an emphasis on the world of the imagination is no harm at all when dealing with young people.
I remember you were careful in your lecture to present the theory as speculative, and the kids they liked that even more… well done again! Cormac O' Raifeartaigh

15. **Dr Lewney**
February 29, 2008

Thanks Cormac – I enjoyed my tour of Ireland immensely, and indeed it was the Irish IoP's recommendation which helped me land this full UK tour.

Of course some people love a good sneer and I can quite understand how some might consider my poster/press to embody all that IS wrong about String Theory, media hype-wise.

But ultimately, I think I'm actually doing Peter Woit's campaign a lot of good. So he's concerned about String Theory's monopoly in recruiting maths and theoretical physics graduates, such that other theories lose out in the competition for such precious resources – yeah yeah, big deal.

I'm concerned about recruiting physics and maths graduates full stop. I'm competing with medicine, law, economics, biochemistry and all kinds of other subjects which a bright kid might gravitate towards. And if I can convince some, jackpot! There's more chance of there being enough graduates to allow other theories to be explored AS WELL AS String Theory.

Of course, the fact that the very superficiality and glitziness of String Theory that Peter complains about is ultimately being used to acheive his goals might stick in his craw a bit. But he should be used to that – after all, if it weren't for String Theory, few people would ever have heard of him!

Cheers

Mark

16. **haha**
February 29, 2008

Don't worry, Mark. Some of us have noticed for a while that Peter has gone over the deep end. Its just that any comment to that effect gets immediately deleted. Lets see if this one survives.

This blog started out moderate enough, but now it is a parody on itself. Anything and everything is slowly becoming the fault of the string theory monster.

17. **Peter Woit**
February 29, 2008

Mark,

I'm glad that Cormac wrote in to tell about what your lecture was actually like. I'd suspected it was likely to be much better than the promotional materials would indicate. Best wishes for the project of getting young people interested in physics. Even if it does involve invocation of string theory hype, at least these days a more skeptical take on this is getting plenty of attention. Hopefully the fact that there's a lively controversy over the subject will have the effect of getting some people interested in the whole thing.

haha,

Actually on any given day I typically delete one or more comments attacking string theory and string theorists, on the grounds that they're just hostile and content-free. There probably are a bunch of comments on this posting that I should have done the same thing to. It is a much rarer event that I delete similarly hostile and content-free comments from anonymous string theory partisans, but when I do it is generally because they generate lots of hostile comments in response, and I have to delete the whole thread.

As for your characterization of this blog, I'll let what I write speak for itself. My name is on it, and I take responsibility for it.

18. **DaveF**
March 1, 2008

[vibration of guitar strings may answer unsolved questions about the Big Bang]

Ask a dumb question, get a dumb answer.

19. **Josh Carmine**
March 1, 2008

"I'm competing with medicine, law, economics, biochemistry and all kinds of other subjects which a bright kid might gravitate towards."

Let's face it. Kids even seriously considering medicine, law or economics over Physics should probably not go into Physics at all. Some years ago when they might not have had any contact with Physics it wasn't so, but nowadays it's almost impossible. So those kids have had contact with Physics and still think about law or economics as an alternative life path?!

Please, let them go. If they come to the field they will do nothing but pollute it.

If this is the lecturer's objective (to "recruit" these kids) then he is doing a disservice to physics.

20. **Dr Lewney**
March 1, 2008

Peter,

Many thanks for your encouragement. Whenever reasonable, sociable people get to present their positions to each other directly, it usually becomes clear that their differences are less significant than what they agree on. I guess it's inevitable that this place also attracts mindblind loners looking for something to hate.

And I'm sure you of all people understand how, despite one's best efforts, what actually ends up getting presented in the media is always a distinctly lossy compression! If you're ever over in the UK, or if I ever get booked in your area, it would be an honour to have you in attendance.

Josh Carmine,

"So those kids have had contact with Physics and still think about law or economics as an alternative life path?! Please, let them go. If they come to the field they will do nothing but pollute it. If this is the lecturer's objective (to "recruit" these kids) then he is doing a disservice to physics."

This is the sadly typical exclusivist snobbery which is causing physics departments to close for want of students. There is, if anything, far LESS physics in the UK curriculum for 14-16 year olds than there was a decade or more ago – I think that my shows might well be the first exposure to what I'd call 'proper' physics (with, you know, maths and everything) that some of these kids receive. And the mere consideration of another subject at FOURTEEN YEARS OLD immediately disqualifies you from a physics career for life and renders you a "pollutant"? I don't think such a position can be reasonably argued with, so I'm not going to bother.

I'm actually very open to ideas as to how best to communicate physics to schoolkids. If any of you here could direct me to recordings of your own efforts to do so, or even to upcoming shows I could attend, I'd be very grateful. Armchair advice not so welcome, thanks.

Cheers

Mark

21. **woit**
March 1, 2008

haha,

I just checked the file I keep of deleted comments, and could only identify one that came from you. You were using the pseudonym "string-dude", and your comment had not the slightest thing to do with the posting, but was an attack on Chris Oakley, explaining to him in detail about how he needed to read a textbook on quantum field theory (Weinberg's).

Sorry, but off-topic, content-free, hostile comments from people who think it's a good idea to hide behind juvenile pseudonyms are definitely the sort of thing I delete and will continue to do so. The only reason I can see to allow such things is to try and make string theorists look bad, and I think I'll resist that temptation. If you want to continue to post hostile comments insulting me, at least start putting your name on them.

22. **Chris Oakley**
March 1, 2008

To be fair to "ha ha"/"string-dude" I have only studied volume 1 of Weinberg in any detail. Battling anonymous foes, though, is like punching air. Why are these people so keen not to be identified?

New LHC Schedule

February 27, 2008
Categories: Experimental HEP News

This week is a "CMS week" at CERN, and talks are available here. The plenary talk discussing the LHC status has:

> If all goes well the machine should be cold by 1 June, and protons could be injected by mid-June. Use this information for laying out our schedule…

Poking around the LHC website, lots of other information is available. A draft general schedule from last week can be found here. It has cooldown of the last sector (4-5) ending in mid-June, and powering tests on-going at several sectors until mid-August. Plans from last year for commissioning the beam envision 30 days with beam to go from first injection to usable 7 TeV beams, with an estimate that this would take 60 calendar days. So, most optimistically, it looks like mid-late October is the earliest that 7 TeV collisions could be happening, right around the date of the official inauguration: October 21. More realistically, this may very well take until early 2009.

No Comments

Fleinhardt Hits a Roadblock

February 28, 2008
Categories: Uncategorized

Larry Fleinhardt, the fictional Caltech string theorist in the TV show Numb3rs, has decided to give up on string theory for now and become a phenomenologist. According to the show's co-writer Nick Falacci:

> Like real-life physicists, Fleinhardt hit a roadblock trying to create an 11-dimensional supergravity theory.

So, he will be joining the DZero collaboration at the Tevatron and work on the search for the Higgs. According to Fermilab Today, an office for Fleinhardt at Fermilab has already been created.

It's not clear if Ed Witten or his brother Matt had anything to do with this…

Comments

1. **Chris Oakley**
   February 29, 2008

   I am still trying to figure out the connection between a TV crime drama and the search for a Higgs boson.

   It could be something like this:

   JUDGE

   Dr. Fleinhardt, I cannot see what relevance this – what did you call it? – bubble chamber photograph? has to the case

   FLEINHARDT

   Your honor, it has everything to do with the case. Those charmed mesons being produced are extremely energetic. They are neutral, and can only decay weakly, and hence the gap between the primary and secondary tracks. An antiproton produced by meson decay could have an energy of up to 1 TeV, and would cause a lot of damage to biological organisms.

   JUDGE

   Enough to kill a man?

   FLEINHARDT

   Indeed, your honor. That could be enough to kill a man.

   JUDGE

   Fleinhardt, these attempts to make particle physics look "cool", in hopes of getting funding, are not going to fool anyone.

2. **Bee**
   February 29, 2008

   How boring. What did 'fiction' come to? They should have let him discover a theory of everything. How come fiction tries to be real, whereas reality becomes more and more fictitious?

3. **A Quantum Diaries Survivor**
   February 29, 2008

   Dear Bee,

   reality beats fiction all the time – and poor fiction gets the worse of it only more quickly. It is the effect of our failing imagination. Look at the LHC startup: today, I heard Joe Incandela at a plenary talk in the main auditorium at CERN saying that these are great times for experimentalists, since the field is all for them: theorists have no clue of what is beyond the standard model.

   I think Joe is right. The poor string theorist in Numb3rs only got the wrong plane ticket – he should've gotten one for Geneva. Dzero will not break new ground, nor will unfortunately do CDF.

   Cheers,
   T.

4. **Coin**
   March 1, 2008

   "I expect it will make more people aware of the work that we do and (if the plot develops) how we actually learn about particle physics," said DZero spokesman Darien Wood. "The characters on the show speak with great excitement and reverence about the search for the Higgs boson at DZero, and I think it captures some of the passion that we real particle physicists have for our work."

   That's… actually kind of an interesting thing for a tv show to try to do.

   And Bee: I don't watch the show, but isn't Larry kind of a secondary character? It seems like letting him discover the Theory of Everything would be inflating his subplot a little too much. What we should really be hoping for is for Charlie to finally succeed in proving P < NP*…

   * Clearly if they don't have the budget to present scenes at the LHC in Geneva, then they don't have the budget either to successfully portray the massive societal breakdown that would occur were someone to prove P = NP

5. **passerby**
   March 4, 2008

   nothing on Darren Crowdy, anybody?

6. **srp**
   March 4, 2008

   Basic rule of series cop shows: All plots end in “order restored.” At the end of the show the universe has been (at least partially) glued back together–injustices have been righted, dangers put back in their place, etc. Part and parcel of that is that the events of the plot, especially by the heroes, can’t change the world too much. We need a stable setting for the next episode and an emotionally satisfying restoration of order by the heroes (for the typical audience member).

   Science fiction plots, on the other hand, are often (although certainly not always) about precisely that–heroes changing the world. That’s one reason the audience is smaller and it’s harder to do episodic forms in that genre. Superhero comic books awkwardly straddle the two approaches; lately they’ve been moving more toward the science-fiction pole of hero-induced change (e.g. Marvel’s Civil Wars), but the comforting adventure-of-the-month against a stable background model will probably never go away entirely.

7. **passerby**
   March 5, 2008

   I did receive the Schwarz-Christoffel paper, jake. thank you. it is old.

This Week’s Hype

March 9, 2008
Categories: This Week's Hype

Today’s Newsday has a long article by Michael Guillen about the significance of the new Simons Center at Stony Brook. Guillen is a theoretical physicist who was the science editor at ABC-TV for fourteen years, and now is the host of “Where Did it Come From?” a science and technology show on the History Channel. According to Guillen:

> Once upon a time, physics likened the tiniest imaginable whit of matter to a geometrical point that, strange as it sounds, theoretically has no dimension: no width, length or depth. But experimental research into protons, neutrons and other elementary particles led physicists in the late 1960s to argue that a subatomic particle behaves not like a point, but a string – a geometrical line segment, with length but no width or depth.
>
> This stupendous hypothesis was followed by another in the 1990s, when physicists discerned in string theory resemblances to an 11-dimensional version of Einstein’s hallowed theory of gravity.
>
> All of this and more has left scientists deliriously optimistic that in string theory – the latest, greatest offspring of geometry and physics – lies the makings of the long sought-after “theory of everything.”

Besides promoting the current delirious optimism about string theory among physicists, Guillen also makes a living as a motivational speaker and promoter of religious faith. His most recent book, Can a Smart Person Believe in God? tell us that

> After the recent, unexpected appearance of something called string theory, science appears to be in the midst of changing its mind yet again. It’s not proposing we live in a universe that has ten or more dimensions!…
>
> As we’ve seen, all the evidence indicates that science is not converging smoothly and consensually upon one firm, reliable understanding of the way our world began or how it operates.

As a guest on the 700 Club, Guillen explained that one of the three things that led him to his religious faith was

> 2. That if a person can believe in black holes and multiple universes, then it would be no big deal to believe in God.

Comments

1. **Big Vlad**
   March 9, 2008

   This is utterly depressing.

2. **nigel cook**
   March 9, 2008

   Guillen’s claim that the widespread belief in multiple universes justifies religion is one that Leonard Susskind intelligently tried to forestall by attacking religion, as indicated by the title of Susskind’s book: *The Cosmic Landscape: String Theory and the Illusion of Intelligent Design*. Susskind argues that string theory is justified because the string theory landscape of 10^500 metastable vacua explains the surprising features of our particular world by using the anthropic selection principle, and this theory is totally incompatible with that of intelligent design by God.

   Susskind’s use of string theory against religion should be one of the major selling points for string theory, and it makes intelligent design skeptics like Richard Dawkins pay close attention to the use of the stringy cosmic landscape as being ‘scientific evidence’ that intelligent design is junk. An early Dawkin argument (that the accuracy of quantum theory is evidence for multiple universes) is on video: http://www.ted.com/index.php/talks/view/id/98

   The real question is whether there is any way of getting proof for the existence of a string landscape so that it can disprove religion?

3. **mike harney**
   March 9, 2008

   I think that although Susskind draws more attention to the argument (which may or may not be helpful), he doesn’t give strong evidence for his claim based on the landscape model. With little or no predictive capabilities, the current string landscape model is likely to be compared more and more to mystical concepts like the multiverse, which climbed into science fiction many decades ago, before this debate is over. Sadly, the public yearns for this fantasy as well so the tide is not likely to turn soon.

4. **Val Wanginstein**
   March 10, 2008

   How could anyone who went through UCLA, Cornell and ended up teaching at Harvard (according to his personal website, no 2nd. ref.), going through grad. level quantum mechanics, grad. level classical mechanics, and undergrad. labs, and then a Ph.D thesis defense, mix “belief” into science? (“That if a person can believe in black holes and multiple universes, then it would be no big deal to believe in God.”)

   It is a big deal. What does faith or some subset of supernatural tales about old dead Jewish men factor into, say, solving a dispersion relation and testing its predictions?

   At the very least, I’m glad that, from a quick xxx.lanl.gov and google lookup, Guillen hasn’t really contributed much to science or shown himself to be a competent scientist, other than passing his classes. Otherwise, my world would be shaken.

5. **srp**
   March 15, 2008

   It’s only fair to point out that many of the greatest physicists of the Scientific Revolution were partly motivated by discovering “the mind of God.” There is a school of historical thought that argues one reason British science was so empirically minded was because their theology was more voluntaristic, i.e. they thought God was not constrained by logic and could therefore have put things together any way he wanted, unlike the French, who tried to reason things out more from first principles.

HEP and Politics News

March 9, 2008
Categories: Experimental HEP News

In perhaps the most important development for the future of HEP in the US in quite a while, yesterday Bill Foster, an HEP experimentalist who worked on CDF and the Recycler at Fermilab, won a race to fill the congressional seat being vacated by Dennis Hastert. This is the congressional district that includes Fermilab, and one of the main reasons for the disastrous budget cuts affecting Fermilab this year seems to have been the fact that the congressional representative for its district not only was no longer Speaker of the House, but had retired.

Foster managed to win as a Democrat in a district that has been a safe one for the Republicans, but he will be up for reelection in November, facing the same opponent. The House Democratic leadership will likely be doing whatever it can to support Foster, and this could very well involve changing its stance from cutting Fermilab's budget to restoring it for next year, FY2009. This should hold true at least through the first week of November, although chances of a budget being passed by then don't seem very high.

I'm still rather confused by news about how the LHC is progressing. A new schedule has appeared, but unlike previous versions, it just shows plans for cooling down the machine, with no information about plans for what happens after that. Earlier versions of the schedule included a period of 2-3 months of powering tests for each sector after it is cool, followed by a month for machine checkout, and two months for beam commissioning before collisions at 7 TeV.

The new schedule has most of the machine cool by the end of May, except for sector 4-5, which is now being warmed up for the repairs on inner triplet magnets, with powering tests already having been performed. This last sector is supposed to be cool again in mid-June. A review of the powering tests is here, from which I gather that discussions are underway about possible ways of speeding up the process for the other sectors, including the possibility of running the machine at 5 TeV rather than 7 TeV during its first year. This would evidently allow a quicker commissioning, avoiding time-consuming quenchings of the magnets that are part of testing them at the highest currents. The Resonaances blog has a report of a talk by Lyn Evans at Moriond this past week, where he describes the possibility of running at lower energy as the currently preferred option, and states that the current plan is for first collisions by the end of August.

For news about recent experimental HEP results, I'm afraid I can't do better than refer you to Tommaso Dorigo for coverage and excellent discussions of a new, more accurate top mass measurement, reports of not very convincing deviations from the Standard model in B-mixing and charm decays, and stringent new limits on WIMPs that make SUSY more unlikely.

For other news about particle detectors, it appears that perhaps at some point in the future, one will be built into every memory chip made, to guard against errors caused by cosmic rays.

Update: For a KITP talk on the current state of the LHC and prospects for the next year or so, by Michael Barnett of ATLAS, see here.

Comments

1. **Alejandro Rivero**
   March 9, 2008

   My understanding is that most of the errors blamed on cosmic rays are really instantaneus lack of power. An ingeneer from west europe, working in my group time ago, was phanatic about putting an extra condensator for each chip in the board.

2. **Hans**
   March 11, 2008

   After you posted aboutthe first schedule, I asked someone i know who was at cern these days. The answer was, that at least cms people told, their estimation would be for cms to give reasonable output data at first in 2010. The one who visited the site, also thought that there is still su much to do, that it won't get ready this year. After pointing the old schedule to him, he asked some cern people and their answer was, that one needs a new schedule.
   This might be the one you are now linking to. They did not give an answer when first collisions will happen. It seems, that there are too many unknowns to put out a reasonable schedule…

The Landscape and the Emperor's New Clothes

March 9, 2008
Categories: Uncategorized

The String Vacuum Project, described as "a large, multi-institution, interdisciplinary collaboration", that has been established over the last few years, is having its Kick-Off Meeting next month at the University of Arizona. This group had submitted grant proposals to the NSF for funding of such a project in the past, but I don't know if they ever managed to get NSF or other funding. They motivate the project by claiming that

> Given that relatively large numbers of string vacua exist, it is imperative that string phenomenologists confront this issue head-on…

In this context "relatively large" involves numbers like 10^{500}, 10^{1500}, etc.

Bert Schellekens has a web-site devoted to promoting the Anthropic Landscape, where he argues that

> The String Theory Landscape is one of the most important and least appreciated discoveries of the last decades.

Besides the web-site, he has slides from two general talks on-line (here and here). In the talks he compares string theorists to the famous Emperor parading in no clothes, except what he is criticizing is those string theorists who have been unwilling to acknowledge the existence and importance of the anthropic landscape. He's critical in particular of

> those people claiming that they have always known that String Theory would never predict the standard model uniquely, but that they did not think this point was worth mentioning.

His modernized version of the fable of the Emperor goes as follows:

> Many years ago, there lived some physicists who cared much about the uniqueness of their theories. One day they heard from two swindlers that they could make the finest theory which was absolutely unique. This uniqueness, they said, also had the special capability that it was invisible to anyone who was stupid enough to accept anthropic thinking.
>
> Of course, all the townspeople wildly praised the magnificent unique theory, afraid to admit that anthropic thoughts were inevitable, until Lenny Susskind shouted:
>
> "String theory has an anthropic landscape"

It's not clear who he would identify as the "two swindlers"….

According to Schellekens, the "string vacuum revolution" is on a par with the other string theory revolutions, but most people prefer to overlook it, since it has been a "slow revolution", taking from 1986-2006. The earliest indications he finds is in Andy Strominger's 1986 paper "Calabi-Yau manifolds with Torsion", where he writes:

> All predictive power seems to have been lost.

and in one of his own papers from 1986 where the existence of 10^{1500} different compactifications is pointed out.

Schellekens claims that "string theory has never looked better", but he completely ignores the main question here, the one identified by Strominger in 1986 right at the beginning. If all predictive power is lost, your theory is worthless and no longer science. What anthropic landscape proponents like him need to do is to show that Strominger was wrong; that while string theory seems to have lost all predictive power, this is a mistake and there really is some way to calculate something that will give a solid, testable prediction of the theory. The String Vacuum Project is an attempt to do this, but there is no evidence beyond wishful thinking that it can lead to a real prediction. Schellekens has worked on producing lots of vacua and describing them in a "String Vacuum Markup Language", and in his slides describes one construction that involves 45761187347637742772 possibilities. These possibilities can be analyzed to see if they contain the SM gauge groups and known particle representations, but this is a small number of discrete constraints and there is no problem to satisfy them. The problem is that one typically gets lots and lots of other stuff, and while one would like to use this to predict beyond-the-SM phenomena, there is no way to do this due to the astronomically large number of possibilities.

He lists goals for the future ("Explore unknown regions of the landscape", "Establish the likelihood of SM features", "Convince ourselves that the standard model is a plausible vacuum"), but none of these constitutes anything like a conventional scientific prediction that would allow one to test to see if what one is doing has any relation to reality. In the end, he comes up with the only real argument for the String Vacuum Project and other landscape research, that of wishful thinking:

> … and maybe we get lucky.

Update: There's a story about the String Vacuum Project in this week's Nature by Geoff Brumfiel. It includes skeptical comments from Seiberg and yours truly, as well as Gordon Kane's claim that:

> evidence supporting string theory could emerge "within a few weeks" of the 's start-up.

Update: At the blog Evolving Thoughts, there's a discussion of whether theoretical physicists have now taken up a "stamp-collecting" model of how to do science. I point out that this is stamp-collecting done by people who don't have any stamps, just some very speculative ideas about what stamps might look like.

Comments

1. **Eric**
   March 9, 2008

   The point has been made many times already, but I guess it has to made again that even with the vast landscape of vacua in string theory, this does not mean that all predictive power is lost. All one has to do is find the particular vacuum which describes our universe, and it will allow an understanding of all phenomena that we can observe. I might add that this is the end-goal of the string vacuum project.

2. **Tim**
   March 9, 2008

   Please: at least with 10^500 possible vacua we have some multiple of 10^500 new SF stories, some of which may win prizes!

3. **Eric**

March 9, 2008

Tim,
At least 99% of the 10^500 possible vacua are complete garbage and can be ruled out easily. Thus, the regions of the landscape for which realistic vacua may arise is limited. If string theory is the right theory, then at least one of these vacua is our universe and it can be found with a focused effort.

4. **Peter Woit**
March 9, 2008

Eric,

Strominger's point back in 1986 was that predictivity is lost because, at the accuracy to which you can calculate anything, finding a vacuum state that agrees with the SM is useless for predictions since there will be lots of them. You can't use any of them to make real predictions of new phenomena, because if experiment disagrees with your "prediction" of something new, you just pick a different state.

If you think Strominger is wrong, you need to come up with a well-defined proposal about how you are going to make a real, testable, convincing prediction. No one has done that because it can't be done. The argument being made by the SVP people seems to essentially be: let's just compute lots of stuff, and maybe a miracle will happen, something will pop out that looks just like new things we see at the LHC. There's no way to prove this is impossible, but it's pure wishful thinking, not science.

5. **Peter Woit**
March 9, 2008

Eric,

Once you rule out the 99% garbage, you're left with 10^498 vacua to study….

6. **Eric**
March 9, 2008

Peter,
This may have been the situation back in 1986. However, today we have better calculational tools and we have a much better understanding of how to calculate gauge couplings, Yukawa couplings and so on. Central to this issue is moduli stabilization which had not even been addressed in 1986. The requirement that all moduli be stabilized is actually quite difficult to achieve. However, if someone constructed a model with the standard model in the low energy limit with absolutely all moduli stabilized then one would have to take it very seriously. Indeed, if all moduli are stabilized then the values of all couplings are fixed and can be compared with experiment.

7. **Peter Woit**
March 9, 2008

Eric,

The fact of the matter is that you couldn't calculate Yukawas reliably in a realistic model back in 1986, you still can't, and there is no realistic prospect of being able to do so in the forseeable future, or ever for that matter. The calculations the SVP people are doing are of things like the gauge groups and representations of low-lying states. Here's a recent quote from Mike Douglas:

"none of us believe we will be able to compute these masses from first principles in any model anytime soon"

see the first page of

http://www.physics.rutgers.edu/~mrd/svp-mar07.ps

8. **Eric**
March 9, 2008

Peter,
Regarding your statement about the Yukawas, I beg to differ. We most certainly know how to calculate them in Type II compactifications. For example, see hep-th/0302105. Again, the problem is uniqueness which brings us back to the moduli stabilization issue.

9. **Jean-Paul**
March 9, 2008

The number of string vacua most commonly quoted is 10^500, so Bert's 10^1500 was wrong by 1000 orders of magnitude. No doubt, it secures him a place in the history of physics.
It is really unbelivable that anybody can take SVML or the idea of "classifying" or "studying" all these vacua seriously. Just wait and see who shows up in Tucson…

10. **Peter Woit**
March 9, 2008

Eric,

I think Mike Douglas actually does know what he is talking about on this issue. Perhaps you can try arguing this with him and let us know the results.

The paper you point to gives nothing like a realistic model in which the low-energy Yukawa terms that determine particle masses can be calculated reliably, to an accuracy that would allow confrontation with experiment. This was the case for the compactifications and attempts to compute Yukawas considered back in 1986, and this hasn't changed.

11. **Eric**
March 10, 2008

Peter,
Douglas's statement about not being able to calculate masses from first principles is not exactly equivalent to saying once cannot calculate Yukawa couplings. I agree that it's unlikely that we'll be able to calculate the exact number for the mass of the electron anytime soon. However, we can calculate the ratio of the masses of the elementary particles to each other.

12. **Shantanu**

March 10, 2008

Peter, here is a link to 2 recent public talks on string theory at
here

13. **Thomas Larsson**
March 10, 2008

Maybe this quote from Polchinski can give us a hint who the two swindlers might be:

"In fact in string theory there is a cult of `monovacuism,' whose prophet resides in New Jersey (or possibly in the office below mine), "

14. **Chris Oakley**
March 10, 2008

OK – so either you believe in String Theory with an Anthropic Landscape or you believe in String Theory with a unique vacuum. I think that covers the universe of possibilities.

15. **Tony Smith**
March 10, 2008

Peter said that he does not "… know if they [String Vacuum Project] ever managed to get NSF or other funding …".

The web site for the SVP "… Kick-Off Meeting … at the University of Arizona …" says in part:
"… Some NSF support is available for participants … This meeting is supported in part by the US National Science Foundation …"
so
it seems that the NSF indeed has deemed SVP to be worthy of funding, and has awarded funds.
That should not be surprising, considering that the three "Meeting Organizers" are listed on that site as
"Keith R. Dienes, University of Arizona
Gordy Kane, University of Michigan
Stuart Raby, Ohio State University"
all of whom are very prominent members of the USA theoretical physics establishment.

Tony Smith

16. **Peter Woit**
March 10, 2008

Tony,

What's not clear to me is whether the NSF funding for this conference is new funding specifically for the SVP, and that's what the "kickoff" is about, or whether this is just being funded out of an older grant of one of the organizers.

17. **King Ray**
March 10, 2008

String theory needs to be taken off life support. Does it have a living will?

18. **Another Anon**
March 10, 2008

Hi Eric,

My understanding was that it's slightly misleading to claim that Yukawa couplings can be calculated, at least in traditional heterotic compactifications – because although one can evaluate the integrals on particular (very symmetric) Calabi-Yaus, to compare with experiment one needs to do a field redefinition so that the kinetic terms are canonical, which one cannot do without knowing the metric on the Kahler moduli space. And the latter is not protected by supersymemtry. As far as I understand not so much is known about these metrics, except that in certain cases they must be quaternion-Kahler, and that various people are working on ways to calculate the quantum corrections more efficiently (Rocek for example).

So it seems that even in the best cases where the CY is symmetric enough to calculate some Yukawas (or at least show that lots vanish), one can't calculate particle masses or ratios thereof.

Am I wrong about this?

19. **Coin**
March 10, 2008

So Eric, I am just trying to understand– when Schellekens says on his website that the String Theory landscape "implies a fundamental non-uniqueness of the gauge theory underlying the Standard Model", do you disagree with this statement?

20. **Eric**
March 10, 2008

Another Anon,
I'm not that familiar with Yukawa couplings in heterotic models. However, from hep-th/0601204 it does appear that one may calculate them. In the case of type IIA compactifications, there is certainly no difficulty in calculating them, at least for toroidal orientifolds.

Coin,
At present, the SM gauge group does not appear to be uniquely singled out. It is actually quite possible that the physics of our universe was by random choice, but at this point nobody really knows. I don't think string theory in it's current form can answer this question.

21. **Trent**
March 10, 2008

Peter,

Somebody above referred to this message from Polchinski http://groups.google.com/group/sci.physics.strings/msg/4c20c2a7d88d6e21 but left out the final sentence:

"I would like this [a unique vacuum] to be true, but scientists are supposed to be immune to believing something just because it makes them happy."

In other words, your insistence on a unique vacuum, or theory of everything with a unique solution is, well, wishful thinking. For this to note we don't need Polchinski but I find it amusing that he points it out so clearly.

Now that we are at it, what are your reasons for believing in a unique theory with a possibly unique solution as the right description of our world other than wishful thinking? Nothing short of knowing the future will make your arguments anything else than wishful thinking.

22. **Peter Woit**
March 10, 2008

Trent,

I have no idea whether an ultimately successful unified theory will have a unique vacuum or not. But I do know that a successful scientific theory will make distinctive testable predictions that will allow us to figure out whether it is right.

My only claim is that the multiverse, string anthropic landscape, etc. is not science but nothing more than an excuse for failure. It predicts nothing, and inherently can predict nothing. This is a conventional way in which speculative ideas fail, the only unconventional thing here is that the people involved refuse to admit what has happened.

23. **Trent**
March 10, 2008

Peter,

I do make an effort to understand you, but I really can't.

On the one hand you say that it's wishful thinking to hope that the string guys will find a vacuum that contains the SM at low energies and on the other hand you say that the whole approach is meaningless because it's not predictive.

Make up your mind! Is it just difficult to study all string vacua? Or it's not science because it's not predictive? These two things are not the same and you keep jumping between the two back and forth as you see fit.

24. **Another Anon**
March 10, 2008

Hi Eric,

From the end of the Braun, He, Ovrut paper you refer to:
"Of course, to explicitly compute the quark/lepton masses one needs, in addition, the Kahler potential, which determines the correct normalization of the fields."
Sure – you can compute on tori, and orbifolds. I guess maybe you can also calculate normalized Yukawa couplings on families CY 3-folds where there is a Gepner point, which gives you an exact solution to normalize the fields.

You agree that these are very special cases though – right?

25. **Peter Woit**
March 11, 2008

Trent,

First of all, I don't know at what level to try and discuss this with you. What exactly is your background in this subject?

You keep ascribing arguments to me that I don't make, and ignoring the ones I do make. Please read what I actually write and make an honest attempt to understand it.

No, I've never argued that the string guys will not find a vacuum that contains the SM. What I have repeatedly argued is that attempts to construct vacua that reproduce even the gross features of the SM lead to such complicated constructions that you lose all predictivity.

It is both difficult to study "string vacua" since they are so complex (by construction, the simple ones disagree with experiment), and studying them is not science since there is no reasonable hope of ever getting a prediction out of them. They are the end point of a 25 year old research program which found that simple versions of the idea don't work, so you have to go to more and more complicated ones, never actually getting any closer to a real scientific prediction. At the end point you have a complicated and useless mess. This is often what happens when you pursue wrong ideas, there is nothing unusual about reaching such an endpoint when you pursue a wrong idea, all that is unusual is the refusal to admit failure.

26. **Chris Austin**
March 11, 2008

Hi Peter,

As a regular reader of your blog, which I find very useful for the information content, (from your blog I learned about Resonaances, from where I learned about the CERN BSM Institute, which enabled me to visit CERN for a week last summer), I have to say that I understand Trent's point of view.

I am sure you will agree with me that whatever the true fundamental theory of physics is, it must lead to an extraordinarily rich range of phenomena, from simple and economical foundations. And M-theory has done exactly that, to a degree that is unprecedented and completely unrivalled, and indeed was previously unimagined. Moreover, arguments have been put forward that somewhere among the vast number of effectively isolated vacua in the "landscape", there will be some that match the physical world we live in.

If, in fact, we do live in one such effectively isolated M-theory vacuum, then our task must be to find out which of these vacua we live in, starting with coarse characterizations, such as, does it have TeV-scale gravity or not? Is it based on Horava-Witten theory or type IIB or type IIA? Is the compact six-manifold Calabi-Yau, or is it some other type of compact spin six-manifold? Answering these questions, by means of input from both theory and experiment, and pinning down further details of the vacuum we live in, might then help us to answer questions of fundamental practical significance. For example, will it ever be possible and practical to build a space vehicle that can accelerate to about ten percent of the speed of light over the course of a year, travel to the nearest star outside the Solar System over the course of about fifty years, and decelerate at the other end over the course of another year, without consuming a significant fraction of the material in the Solar System as fuel in the process? And if so, how large will such a vehicle be, and what will it be like?

One of the organizers of the Tucson meeting is Keith R. Dienes, who in his paper arXiv:hep-th/0602286 dealt a major blow to the expectation that the existence of a very large number of vacua means that some of them will have a very small cosmological constant. Among the 10^5 vacua studied in this paper, Dienes found a large degeneracy among the different models with regard to the value of the cosmological constant, so that only relatively few different values of the cosmological constant were realized. Both positive and negative values of the cosmological constant were found, but none of the models had zero cosmological constant, and the smallest magnitude cosmological constant found was about 0.02.

If the type of cosmological constant degeneracy identified by Dienes occurs among other types of M-theory vacua, then even the existence of 10^500 vacua would not be sufficient to guarantee that some of the vacua have cosmological constant around 10^-120, so it would seem to be important and useful to investigate this question for the various types of M-theory vacua.

Some of the different types of M-theory vacua have implications for the LHC. In particular, those with TeV-scale gravity certainly do, and there are a number of different realizations of TeV-scale gravity in M-theory, not just RS versus ADD, since there are a number of qualitively very different realizations of ADD in M-theory, with distinct signatures for the LHC. Comparing the predictions of as many different such models as possible, with the LHC data, should help to point in the direction of the correct vacuum.

Best regards,
Chris

27. **Chris Oakley**
March 11, 2008

Hi Chris A,

You forgot to insert the qualifier “if one believes in String Theory” at about six different points in your comment above.

28. **chris**
March 11, 2008

Hi Chris A,

just one question: could you please point me to one reference that defines what M-theory actually is?

thanks.

29. **Peter Woit**
March 11, 2008

Chris A.,

Thanks for writing, although I disagree with you strongly on several points.

1. We don’t actually know what “M-theory” is, all we know is that there probably is some interesting structure that plays the role of such a theory. The underlying structure may not be either simple or useful for unifying physics using extra dimensions.

2. It’s fine to make the hypothesis that we live in a string vacuum and investigate its implications. But thousands of people have been doing this for nearly a quarter century and the results are in. Simple constructions of such vacua don’t look at all like the real world. To get something closer and closer to the real world, you have to invoke more and more complicated constructions, at no point ever finding something constrained enough to convincingly explain the origin of any features of the SM we don’t understand. This is just a classic example of what happens when you follow a failed speculative idea. Instead of acknowledging this failure, people are promoting a research program based purely on wishful thinking that something unexpected is going to turn up, even though the whole thing looks hopeless.

3. Even if you could show that known vacua can’t possibly give a small enough CC (and I don’t for a minute believe this is possible), this would not cause people to give up on M-theory, just to say that more vacua must be found and investigated.

4. If you want to argue that we need more new ideas about signatures for the LHC to look for, I’d agree. I just don’t see the kind of constructions the SVP is promoting as giving anything new here. I don’t at all buy your idea that the agenda for particle physics in the next few years will be to compare LHC data to complicated string vacua constructions. The hope that the LHC is going to produce lots of deviations from the SM that correspond to characteristics of string vacua showing up all of a sudden at the TeV scale seems to me pure fantasy and wishful thinking.

30. **Urs Schreiber**
March 11, 2008

> could you please point me to one reference that defines what M-theory actually is?

Perturbative string theory is the investigation of the implication of replacing in the perturbative expansion of quantum field theory the Feynman diagrams, which are correlators of a 1-dimensional QFT, by correlators of a 2-dimensional QFT.

It turns out that many 2-dimensional QFTs have a close “holographic” relation to 3-dimensional QFTs (the most famous example being the relation between 2-dimensional WZW theory with 3-dimensional Chern-Simons theory).

Generally speaking, “M-theory” is the name given to a hypothetical theory which people seem to keep finding indications for, and which is supposed to be to String theory roughly like these 3-dimensional QFTs are to the 2-dimensional QFTs.

More strictly speaking, whenever you see people actually doing something in what they address as “M-theory”, they are studying the action functional of 11-dimensional supergravity subject to a couple of constraints which generalize the old Dirac quantization constraint of electric and magnetic charges.

Also, whenever you see somebody talk about “M-theory vacua”, he or she is talking about this concrete issue: 11d sugra + quantum constraints.

Possibly the best text on 11d SUGRA is the old textbook by Castellani, D’Auria and Fre, “Supergravity and Superstrings: A geometric perspective”.

The possibly best discussion of those “quantum constraints” is due to Freed, Hopkins, Singer et al.

D. Freed Dirac Charge Quantization and Generalized Differential Cohomology

M. Hopkins, I. Singer Quadratic functions in geometry, topology,and M-theory

There is of course (as always) much more that people are considering. But if you know 11d sugra + Dirac quantization as in the above sources, you have a pretty good idea of what most of the stuff referred to as “M-theory” is about.

31. **Kea**
March 11, 2008

Urs, surely by now it has occurred to you that many of the anti M Theory comments on this blog are rather tongue-in-cheek, and one should probably not assume that the writer, whose comments above suggest at least a little familiarity with the literature, is ignorant of the oft used meaning of the term *M Theory* as a theory in 11 classical dimensions, a theory which in itself does not satisfy the criteria of the real M Theory.

32. **Chris Austin**
March 13, 2008

Hi chris and Peter,

Sorry for the delay in responding. On the definition of M-theory, I pointed out in the introduction and subsection 2.3.3 of arXiv:0704.1476 that the simplest and most economical hypothesis, namely that on a smooth uncompactified background, M-theory and d = 11 supergravity are the same thing, is consistent with all results in the literature, and moreover appears to be required by the apparent absence from the supermembrane mass spectrum of any states other than the single particle and multi-particle states of supergravity, (section 12 of de Wit, arXiv:hep-th/9902051).

This working hypothesis states that on a smooth uncompactified background, the predictions of M-theory are to be calculated from the CJS theory of d = 11 supergravity in the framework of effective field theory, (which in this case simply means BPHZ renormalization, since there are no massive states to integrate out), and that this will lead to unambiguous results, with all finite counterterms fixed uniquely by Slavnov-Taylor-Zinn-Justin identities, (and thus no undetermined parameters connected with the short distance completion of the theory). The content of this hypothesis, which is consistent with all results in the literature, is thus that the CJS theory does not admit any non-trivial locally supersymmetric higher derivative local deformations.

Deser and Seminara, in arXiv:hep-th/9812136 and arXiv:hep-th/0002241, and Metsaev, in arXiv:hep-th/0410239, constructed a family of \partial^{2n} R^4 linearized on-shell invariants, which if they could be extended to fully non-linear on-shell invariants, would invalidate the above working hypothesis. In d = 4, it was found that such linearized invariants could always be extended to fully non-linear invariants by the Noether procedure, but the existence of the auxiliary field formalisms in d = 4 implies that this must necessarily be so. In d = 11 there is no analogous auxiliary field formalism, (Rivelles and Taylor, Phys. Lett. B121, 37-42, 1983), and the Noether procedure is known to fail if one tries to use a six-form gauge field instead of a three-form, (Nicolai, Townsend, and van Nieuwenhuizen, Lett. Nuovo Cim. 30, 315, 1981). The Deser-Seminara-Metsaev linearized invariants have not yet been completed to fully non-linear on-shell invariants, although partial completions exist, and the above working hypothesis implies that there will be an obstruction to full completion.

Superspace counterterms have been constructed in standard d = 11 superspace by Duff and Toms (in Ellis and Ferrara, eds., 1983, see SPIRES), and Howe and Tsimpis (in arXiv:hep-th/0305129), and if the gauge completion mapping that matches the geometrical transformations in superspace to the CJS supersymmetry variations could be completed up to the necessary power of \theta, which in the Duff and Toms case is \theta^{32}, for a general solution of the CJS field equations, then these superspace counterterms would give fully non-linear on-shell higher derivative invariants for the CJS theory, and thus invalidate the above working hypothesis. However the gauge completion mapping is only known for a general solution of the CJS field equations at first order in \theta, (Cremmer and Ferrara, Phys. Lett. B91, 61, 1980), and partly at second order in \theta, (de Wit, Peeters, and Plefka, arXiv:hep-th/9803209). I suggested in the introduction to arXiv:0704.1476 that an obstruction might exist that prevents the geometrical transformations in superspace from matching the CJS supersymmetry variations for a general solution of the CJS field equations beyond a certain power of \theta. This would mean that the superspace counterterms do not result in locally supersymmetric deformations of the CJS theory, and would thus be consistent with the above working hypothesis. There is already a partial proof of the existence of such an obstruction to gauge completion, in the shape of a discrepancy between a component framework calculation of Hyakutake and Ogushi, arXiv:hep-th/0601092, and the superspace construction of Howe and Tsimpis. Howe and Tsimpis found that there should be an independent on-shell superinvariant for each independent Chern-Simons term, of which there are two at dimension 8, namely C \wedge tr(R \wedge R \wedge R \wedge R), and C \wedge tr(R \wedge R) \wedge tr(R \wedge R), but Hyakutake and Ogushi found that only one linear combination of these two terms might occur in a superinvariant, namely the combination which occurs in the bulk Green-Schwarz term. However, there are gaps in this proof, because Hyakutake and Ogushi omitted some possibly relevant terms from their ansatz, namely the third type of term in their equation (4.3), and Howe and Tsimpis have not published the full details of their argument, and it is not clear how firm their claims are. If these gaps could be closed, the existence of an obstruction to gauge completion would be proved.

The way in which the Green-Schwarz action for the type IIA superstring arises from the solitonic membrane of d = 11 supergravity was described by Horvava and Witten in subsection 2 (iii) of arXiv:hep-th/9510209, and I reviewed this argument, with some details added, in subsection 2.3.3 of arXiv:0704.1476. That there is no place for a fundamental supermembrane in d = 11, separate from the solitonic membrane, was already shown by Hull and Townsend in section 7 of arXiv:hep-th/9410167. The supermembrane action instead arises from d = 11 supergravity, as the worldbrane effective action of the solitonic membrane, in the double dimensional reduction from which the Green-Schwarz action for the superstring arises.

The above working hypothesis places the type IIA superstring in a classic strong / weak coupling duality relationship with a known, but very special, quantum field theory in eleven dimensions, namely the CJS theory. On ten extended dimensions times a circle of large radius L, the dimensionless gravitational coupling associated with the circle, namely \kappa^{2/9} / L, is weak, the solitonic membrane is dynamically unimportant, the superstring coupling constant, namely L^{3/2} / \kappa^{1/3}, is large, and the dynamics is most conveniently described by the CJS theory, but when L shrinks to become small compared to \kappa^{2/9}, the situation is reversed.

For M-theory on a non-smooth background, the most interesting case is Horava-Witten theory, and here we can again adopt a working hypothesis that the predictions of the theory are to be calculated from the Horava-Witten action in the framework of effective field theory, with the resistance to deformation of the CJS action preventing the occurrence of undetermined parameters connected with the short-distance completion of the theory. The \delta(0) terms in the action and transformation rules found by Horava and Witten have been avoided in the improved form of Horava-Witten theory proposed by Moss in arXiv:hep-th/0308159, arXiv:hep-th/0403106, and arXiv:hep-th/0508227. A critical test, directly relevant to the LHC, will be to check that the masses of U(1) gauge bosons resulting from Witten’s Higgs mechanism, (Witten, Phys. Lett. B149, 351-356, 1984), work out to be finite and nonzero, since these masses initially appear to be infinite in Horava-Witten theory, as explained near the beginning of section 5 of arXiv:0704.1476.

Some of the above points were made in my talk at CERN on 28 August last year, see pages 16 – 20 and 12 – 13 of the slides, from
http://indico.cern.ch/conferenceDisplay.py?confId=19188

Best regards,
Chris

33. **Henry**
March 13, 2008

Chris A, thanks a lot for your comment and these references!

34. **Coin**
March 13, 2008

Hi Urs/Chris A.,

Not knowing as much about string theory as some of the other commenters here, I am curious. In your posts, you describe M-Theory as simply the study of 11-D SUGRA. This is an interesting way of looking at things, but there is one point that confuses me. Other descriptions of M-Theory that I have seen, and my observations of M-Theory when I hear about people researching it in practice, indicate that much of the actual study of M-Theory is concerned with n-dimensional vibrating objects, where these objects are called "strings" if n=1 and "branes" if n>1.

I can understand why one would choose to use strings if one's goal is the study of 11D Sugra– since the graviton can be viewed as a string excitation mode, etc– but, if M-theory is as you say just the study of 11D Sugra, then I am confused where the branes come from. In what way do (n>1)-branes follow from this depiction of M-theory as in some way derived from 11D Sugra?

(Also Eric, thanks for your clarification above.)

35. **Urs Schreiber**
March 13, 2008

> In what way do (n>1)-branes follow from this depiction of M-theory as in some way derived from 11D Sugra?

Chris Austin briefly mentioned it above: the 11-dimensional theory automatically contains

2-branes

and their magnetic duals, which are

(11-(2+2))-2 = 5-branes .

These are called the

M2-brane

and the

M5-brane.

The 10-dimensional backgrounds for string theory are obtained roughly by shrinking one of the 11 dimensions away.

Those M2-branes which didn't "wrap" the compactified dimension remain 2-branes this way. They are called

D2-branes

then.

M5-branes that don't wrap remain 5-branes and are called

D5-branes

then.

M2-branes that did wrap the compactified dimension become effectively 1-branes and are called

F1-branes

now, also know as

the fundamental string.

(That THE string, yes.)

M5-branes that wrap become D4-branes.

This way (and by some further mechanism) all the brane content of the 10-dimensional theory arises from the 11-dimensional theory.

36. **Urs Schreiber**
March 13, 2008

Sorry, the "D5-brane" should have been the NS 5-brane.

37. **Peter Woit**
March 13, 2008

Chris et. al.,

As far as I can tell, Chris is proposing a series of unconventional ideas about M-theory. This topic doesn't have anything to do with the topic of the posting, I'm no expert on this, and this is not a general physics discussion board. Please, if you want to discuss Chris's ideas about M-theory with him, contact him directly.

38. **dan**
March 13, 2008

Peter,

in response to #2 " It's fine to make the hypothesis that we live in a string vacuum and investigate its implications. But thousands of people have been doing this for nearly a quarter century and the results are in. Simple constructions of such vacua don't look at all like the real world. To get something closer and closer to the real world, you have to invoke more and more complicated constructions, at no point ever finding something constrained enough to convincingly explain the origin of any features of the SM we don't understand. This is just a classic example of what happens when you follow a failed speculative idea. Instead of acknowledging this failure, people are promoting a research program based purely on wishful thinking that something unexpected is going to turn up, even though the whole thing looks hopeless."

Couldn't nature though really be like this? Perhaps nature itself really is the result of one form of string, theory, one choice of string vacua, one compactification among 10^500, one scale of SUSY breaking, and is simply not fundamentally possible for physics to do any better, and is inherently unpredictable.

Regards,
Dan

39. **woit**
March 13, 2008

Dan,

Sure, it's logically possible that nature is like this. It's also possible things are fundamentally just random choices of some Supreme Being. While these hypotheses are logically possible, they're not scientific hypotheses, because they are inherently untestable. The problem with the landscape is not that it's logically impossible, but that it appears to be impossible to ever use the idea to make testable predictions. The people who argue for the Landscape seem to me to just be hoping a miracle will happen and somehow a testable prediction will emerge, despite all the accumulated evidence that there is no sign of such a possibility. If your speculative idea not only make no predictions, but you can't even come up with a plausible scenario of how further work on it will lead to predictions, it's just not science.

40. **Marty Tysanner**
March 14, 2008

Dan,

Although I agree with what Peter wrote, I think there is an additional way that you can look at anthropic considerations as a selection principle. Take, for example, quantum electrodynamics. In QED it is impossible to predict an actual number for either the electromagnetic coupling strength or the electron mass; these are "inputs" to QED. Unfortunately, their prediction is made impossible due to renormalization, a procedure that is used to cancel infinities that arise when one wants to do more accurate calculations in the theory. If one took the point of view that QED is a truly fundamental theory, then one would be forced to say that there is no *a priori* choice of the two parameters, and look for other explanations for why they have the values they do.

You could propose that the explanation for the values of the electromagnetic coupling constant and electron mass could be anthropic, that only certain values would allow us as observers to exist and ask questions and develop QED. You could then quantitatively analyze all possible choices of these two parameters, and probably find out that only certain ranges of values would be compatible with us existing and developing QED. That might give you confidence that you are on the right track, and you might consider your analysis to be evidence that these parameters really are random variables. Then you could end your search for explanations, and just accept that some critical quantities are inherently unpredictable in the final theory, i.e.: that's the way it is, and you just have to deal with it.

However, you could take an entirely different position. You could adopt the view that QED is *not* a fundamental theory, and so the appearance that the coupling constant and electron mass are free parameters is just a reflection that there are crucial ingredients that are missing from the theory, that QED is an incomplete and non-fundamental theory. Then you would keep searching for a better theory with the expectation that QED would emerge as a limiting case of the theory, but that the coupling constant and mass would emerge as predictions of the more fundamental theory.

Comparing these two points of view, there is no way to distinguish which one is correct, given that we don't currently have a theory that predicts the coupling constant or electron mass. All of the anthropic arguments that might look like successes are nothing more than constraints you put on QED; they reflect *observations* about the world that cannot be incompatible with the theory, but a more predictive theory would need to abide by those same observational constraints (or else it is wrong). The fact that we haven't yet found a more predictive theory means nothing more than such a theory is not easy to find from our current vantage point (e.g., we may be unwittingly over-constraining the form that we allow a fundamental theory to take, making it essentially impossible to find that theory while remaining compatible with observation).

In my view at least, anthropic arguments are only worthwhile if they reduce the number of possibilities to a finite and small number of possible solutions, *and* you do not assume that your theory is fundamental. Then you have an interim theory with only a few viable solutions, and you can use it to make predictions that you can subsequently test; the testability makes it science. However, you can't use anthropic arguments as evidence that your theory is fundamental, since the alternative explanation that you haven't yet found the more predictive theory still remains.

41. **dan**
March 16, 2008

Peter, Marty Tysanner,

re: "…..because they are inherently untestable…."

re: "…Take, for example, quantum electrodynamics. In QED it is impossible to predict an actual number for either the electromagnetic coupling strength or the electron mass; these are "inputs" to QED…."

If it were possible somehow to experimentally determine both the scale, mechanism and nature of SUSY-breaking and the specific parameters of the moduli of the calabi-yau manifold of our universe, given these experimentally derived and observed numbers, could they then be used as input for a string theory to see if its "output" (i.e the MSSSM +GR, plus novel predictions or postdictions) be testable and falsifiable. The landscape may well be "inherently untestable", but perhaps the only way to get "predictions" out of string theory is to accept that the specific parameters of higher dimensional space can only be experimentally observed.

42. **Marty Tysanner**
March 17, 2008

Hi Dan,

First your last comment:

> … perhaps the only way to get "predictions" out of string theory is to accept that the specific parameters of higher dimensional space can only be experimentally observed.

This doesn't sound much different in principle than the approach that is used for established theories today, e.g., the standard model of particle physics. The standard model has approximately 20 parameters which are measured (the electron mass and electromagnetic coupling are two of them); once measured, the SM is extremely predictive and very much in agreement with a vast number of experiments, as you already know.

However, taking the widely held view that the standard model is an *effective* theory, not a truly fundamental one, there is no need to explain why its parameters have the values they do; ideally, that would be the job of a more fundamental theory. Depending on your tastes, you could take the view that there is no fundamental theory and the parameters are random variables, or you could assume that a more predictive theory is possible but we haven't found it yet. Both possibilities are logically possible, and as I mentioned before, failure to find that more predictive theory says nothing about whether it is there to be found.

None of this has anything to do with SUSY or compactification of higher dimensions, which you mentioned:

> If it were possible somehow to experimentally determine both the scale, mechanism and nature of SUSY-breaking and the specific parameters of the moduli of the calabi-yau manifold of our universe […]

Certainly, observations by themselves are little more than a collection of literal numbers, pictures, etc. which must organized and interpreted before they carry meaning. For the kinds of experiments you seem to be thinking of, theories play the key role in providing a framework for organizing and interpreting the data. The more "flexible" the theory (i.e., the less predictive it is), the easier it will be to find a way to fit the data, and the less rigid the conclusions you can draw from the fit; this is true in general, not at all specific to the string theory landscape idea.

Assume that a supersymmetric string theory is what you will use to organize and interpret your data, and let's say that it predicts the MSSM plus GR as you said. (In my view, prediction of gravity is not exactly a prediction; the presence of something that looks like gravity was a crucial reason for taking string theory seriously as a "theory of everything" in the first place.) Now you do a number of experiments. If your theory is predictive in the meaningful sense, it should uniquely (i.e., without flexibility) and correctly determine many more experimental values than you used to construct your theory. That is, you want to get more out of the theory than you put into it. Failing that, the theory basically just re-parameterizes what you already know, which is not very impressive.

Nonetheless, even if you have a relatively unique theory that has made robust predictions that were experimentally confirmed (which would be real progress), you still have a lot of unexplained parameters. At this point you are back to the same questions of how to explain them. You still can't scientifically distinguish between the possibility that the parameters are fundamental random variables (i.e., your predictive string theory *is* fundamental), versus the possibility that a more fundamental theory can be found that predicts those parameters uniquely (i.e., your predictive string theory is *not* fundamental), so the arguments in my previous post still apply. You still can't use anthropic arguments to decide whether your theory is fundamental…

43. **dan**
March 17, 2008

Hello, "Marty Tysanner"

I've agreed with what you said. While there would be many unexplained parameters, it could explain what is unexplained in the SM and GR.

While such a theory may not be "fundamental" if we could observe the parameters of the moduli of the 6-dimensional space (or if we live in a large dimensional braneworld) experimentally/observationally and use that data as input and then put them in with the current superstring theories, and we don't get the MSSM + GR, wouldn't this "falsify" string theory, thus making it science?

44. **Coin**
March 17, 2008

if we could observe the parameters of the moduli of the 6-dimensional space… experimentally/observationally and use that data as input…

If we could do this, I think many or most of the people currently complaining that string theory is not science would fall silent, or at least find something else about String Theory to complain about. In fact as near as I can gather, it is specifically *because* no one can observe the parameters of string theory (and even *constraining* those parameters, such as the String Vacuum Project types wish to do, seems to be a great difficulty) that people have started accusing it of being nonscience in the first place.

45. **Marty Tysanner**
March 17, 2008

Hi Dan,

I'm not sure I have correctly interpreted your question, so what follows may not be very helpful.

You mentioned

> While there would be many unexplained parameters, it could explain what is unexplained in the SM and GR.

Sometimes a new theory or perspective can clarify what we know and don't know, but I don't think that is the case here. The parameters of the SM and GR are very specific, and given those parameters the range of those theories is well known. If for some ranges of those parameters the SM or GR break down, that would be extremely interesting and exciting and would probably give clues about a more fundamental theory. But you need experiments to tell you where (or if) the SM and GR have limits of applicability. Theories are generalizations, extrapolations, or interpolations of what we can observe, so beyond requirements of mathematical/logical and empirical consistency you can't use a new theory to tell you that the SM or GR will break down in some regime; you must qualify it with, "If the new theory is true, then…"

I am even less sure I have understood your next paragraph. For example, I don't think anyone would argue that you can observe the moduli; they parameterize the Calabi-Yau manifold, but couldn't themselves be observed or measured because you couldn't step outside our four dimensional spacetime and perform the necessary experiments. Hence you could make only indirect inferences by noting that certain ranges of the parameters are compatible with observations and others aren't; anthropic reasoning would be included here.

> … use that data as input and then put them in with the current superstring theories, and we don't get the MSSM + GR, wouldn't this "falsify" string theory, thus making it science?

First, the MSSM is just one possible extension of the SM. There are no experiments yet which imply that the MSSM (or supersymmetry in general) is part of nature, so it is on a very different footing than general relativity. Assuming you know this, I think you are asking whether everything we have observed experimentally is sufficient in principle to constrain the string theory landscape enough to uniquely predict something new that we can experimentally test. That is where the controversy lies. Peter has argued that if you have an unimaginably large number of valid choices (e.g., 10^500) then, unless you have some way to *a priori* prune that huge number down to a small number, you could have a huge number of theories (i.e., vacua) that each fits all experimentally known (and knowable) values within the limits of experimental precision: your theory ceases to make unique predictions and thus is not testable in the conventional sense. Jacques Distler, on the other hand, has made a fairly specific argument in a spirited discussion on Cosmicvariance that the situation may not be nearly that bad; I think his argument is also reasonable and shouldn't be dismissed, but you should refer to that discussion for some details (the link should be to comment #233, but the complete discussion had more than 500 comments!).

46. **dan**
March 17, 2008

Marty Tysanner,

Coin actually states my intention — would string theory be "predictive" and "falsifiable" if it were possible to observe, experimentally, the values of the moduli? I understand that Peter argues there are an unimaginably large number of possible choices for the vacuum configuration are possible in theory, but if nature is really 11D and SUSY, it has chosen one specific set of values that could, if the technology existed, be observed and measured and recorded. I recognize that it is not evidentially possible to deduce the parameters of these hidden dimensions, even consistency with the SM, MSSM, and GR in the low energy LHC & TEV range does not constrain the theory enough. But if there could be observational/experimental measurements, I understand that "no one can observe the parameters of string theory" but is it less science if the technology does not exist to access and measure the parameters of these hidden dimensions?

47. **Anonymous**
March 18, 2008

"I understand that "no one can observe the parameters of string theory" but is it less science if the technology does not exist to access and measure the parameters of these hidden dimensions?"

Do these parameters make their presence felt in string-scattering amplitudes?

48. **Peter Woit**
March 20, 2008

dan/Anonymous,

The problem is that no one is able to, for any even slightly realistic string theory models, compute the relation between observable quantities (eg masses and mixing angles) and things like moduli parameters that specify the model. This has been the case since string theory became popular, there is no reason to believe it is going to change.

49. **Dan**
March 20, 2008

re:Peter,

Well if that's true, and if it will remain true in the foreseeable future, string theory is indeed "NEW".

Regards,
Dan

50. **hv**
March 21, 2008

The Monster group has nearly 10^54 elements. Creating a multiplication table for the Monster would describe it completely. Such a table would have nearly 10^108 entries, however. Nonetheless, Griess showed *by hand* that the Monster, in fact, is the automorphism group of a 196884-dimensional commutative nonassociative algebra, and many important discoveries involving the Monster followed, despite it's size. So, who can really say that understanding the Landscape fully will be forever out of reach? It might just need a simple genius move, and if numerical analysis helps finding patterns in the Landscape, let them do it! I attended some seminars on numerical landscape analysis, and the results seem quite nice and sometimes intriguing. And what I've understood is that most of the vacua can be easily ruled out and that it seems that only very few Calabi-Yau possess interesting properties (low Betti numbers, etc) to make them good candidates for describing Standard Model.

51. **Peter Woit**
March 21, 2008

hv,

One of many problems with your analogy is that the set of elements of the Monster form a finite, simple group, which is an extremely special mathematical structure of a highly symmetric sort. There is simply zero reason other than wishful thinking to believe that anything like this is true of the set of "string vacua".

52. **Urs Schreiber**
March 22, 2008

The monster as well as string vacua share the property that they both are part of the theory of the "space" of 2-dimensional (rational) conformal field theories. There is every reason to believe that this space is a rich and beautiful mathematical entity in its own right.

The main problem about this space is that it is so very little understood that it seems to be just premature to try to do things like statistics over it. Let alone trying to predict particle masses from it.

53. **Peter Woit**
March 22, 2008

Urs,

Besides not knowing what the "space" of 2d CFTs is, the lack of a non-perturbative formulation of string theory means you wouldn't be able to extract physics from it, even if you knew what it was.

54. **Urs Schreiber**
March 22, 2008

I wouldn't mind, if I knew what it was.

Part of the beauty of string theory is that it builds on the notion of 2D CFT, which is a mathematical structure of the kind mathemticians get in love with (like the monster) but bigger. And less well understood. (Well, actually for non-super and *rational* 2-D CFT people have understood a whole lot meanwhile.)

If everybody would just stop worrying about cranking out numbers where no numbers can be cranked out and concentrated again on investigating the basics we'd be in much better shape.

55. **Thomas Larsson**
March 23, 2008

The basics of CFT is well understood; there is no more low-hanging fruit to pick. Moreover, beautiful as this mathematical structure may be, it is not the only conceivable mathematical structure.

Cranking out numbers is important to figure out if it is CFT or some other structure that governs the laws of the universe. The simplest numbers are indeed easy enough to crank out, and they disagree with experiment – spacetime does not have 26 or 10 dimensions, and there are not 496 different gauge bosons. CFT number fit perfectly into 2D statmech, but this only shows its limitations. Whereas one may argue about CFTs beauty in hep, in statphys there is no doubt that it only applies to toy models in 2D, and not to the physically important systems in 3D.

One can learn one thing from these toy models, though. Universality in 2D is explained by CFT, i.e. by the Virasoro algebra. Universality is an empirical fact also in 3D. Therefore, one may suspect that 3D universality has a similar explanation as 2D universality.

56. **curious**
March 23, 2008

Thomas Larsson what do you mean by "universality" in your last comment? What does it mean that universality is explained by CFT in 2D and is an empirical fact in 3D?

57. **Thomas Larsson**
March 23, 2008

Universality in statphys means that similar systems do not just have similar, but identical (or completely different) critical exponents. This is explained in CFT because there is only a discrete set of universality classes (unitary Virasoro representations) in the simplest cases. That universality holds also in 3D is standard lore and has been confirmed by experiments (real and numerical) and various theoretical arguments (epsilon expansion and others).

58. **curious**
March 24, 2008

Thanks, but I guess it was too technical for me. Now I don't know what you mean by systems and their critical exponents.

I guess I want to know what aspect of physical reality "universality" refers to. What kind of experiment could confirm or refute it.

59. **Daniel de França MTd2**
March 24, 2008

It seems LQG people also becoming fond of landscape-like ideas:
http://arxiv.org/abs/0803.2926

Some coments on this paper:

http://www.physicsforums.com/showthread.php?t=223450

60. **Peter Shor**
March 24, 2008

Chris A,

Dienes' paper implies nothing about the existence of small cosmological constants. Anybody who tries to use samples from a probability distribution to estimate its behavior in the low-probability tail is, as von Neumann would say, living in a state of sin. The only way to learn anything about the low probability tail is either to generate an astronomical number of samples or to use theory, both utterly hopeless at this state of the game.

61. **Anonymous**
March 24, 2008

curious, see this:
http://en.wikipedia.org/wiki/Critical_exponent

62. **curious**
March 25, 2008

Thanks, I've got it now. I was aware of various other meanings of the word "universality", but not of this particular meaning until now.

63. **Urs Schreiber**
March 25, 2008

Thomas Larsson wrote:

> The basics of CFT is well understood; there is no more low-hanging fruit to pick.

Right, we are certainly not talking about low-hanging fruit here.

And the space of 2d CFTs is not low hanging fruit and not well understood. Even the more easily accessible subspaces have only been understood rather recently.

One problem is that what many people call CFT is only half of the story: namely "chiral CFT". This is given, equivalently, by

– a vertex operator algebra and its representation category

– a conformal net of local algebras on the line and its representation category .

All things Virasoro live here. But that's not yet a CFT, it just encodes the local symmetry properties of a CFT. In particular, there may be none, one or several non-equivalent full CFTs associated with a given vertex operator algebra.

A full CFT is a representation of the category of conformal 2-dimensional cobordisms such that the infinitesimal neighbourhood of the length 0 cylinder reproduces the given chiral CFT.

In the case that the chiral CFT part is *rational* in that the representation category has finitely many isomorphism classes of simple objects ("primary fields") the "space" of all corresponding full CFTs is essentially understood: there is one equivalence class of such CFTs per Morita class of special symmetric Frobenius algebra objects internal to the representation category.

One may think of that results as “completely solving rational 2d CFT”. But even in that case a problem remains: to connected the Frobenius algebra object with the chiral vertex operator algebra requires a choice of isomorphisms between the vector spaces associated by the Reshitikhin-Turaev functor to given surfaces and the space of conformal blocks of that surface given by the VOA.

That isomorphism is explicitly known only in a few simple cases.

But already at this point, lots of nice structure appears. Practitioners I know think of rational 2d CFT as a generalization of group representation theory.

And the structure of the proof for the result of the rational case indicates what one should expect more generally. So now the question is how to generalize the proof to

– non-rational

and to

– supersymmetric

2dCFTs. It is clear that we expect to see again that these are given by certain Frobenius algebra objects in certain VOA representation categories. But now there are technical details to deal with, since in lots of places where we have finite sums appearing in the rational case, the non-rational case, naively, leads to infinite sums. (Not those kind of infinite sums one sees in perturbative QFT, though.)

64. **Urs Schreiber**
March 25, 2008

Thomas Larsson wrote:

> spacetime does not have 26 or 10 dimensions,

Bosonic 2d CFTs of central charge 26 correspond to effective target spaces which are 26-dimensional manifolds only in a tiny subset of the space of all such CFTs, namely those that are entirely of the naive sigma-model type with large flat dimensions.

Supersymmetric 2d CFTs of central charge 15 correspond to effective target spaces which are 10-dimensional manifolds only in a tiny subset of the space of all such CFTs, namely those that are entirely of the naive sigma-model type with large flat dimensions.

That there is and has been so much focus on such utterly over-simplistic CFTs as string backgrounds is the problem whose very point we are discussing: the space of all CFTs is little understood and everybody has been searching the key under the lamppost which illuminates only the most elementary examples.

Not that it is guaranteed that the key actually is somewhere else in the currently dark realms of CFT-land, but it is certainly premature to make statements about the shape of the key from what we can find under this tiny spotlight.

To appreciate the situation, it may help to simplify it drastically and marvel at how complicated it still is:

As Roggenkamp and Wendland show, and especially Yan Soibelman describes in a big (unfortunately not yet published) opus, a 2dCFT encodes a categorification (2-dimensional version) of a Connes spectral triple. The effective target space described by the CFT regarded as a string background is the spectral geometry encoded by the point particle limit of that spectral triple. So when we are talking about string backgrounds, we are talking about a vast generalization of ordinary spectral (“noncommutative”) geometry, which itself is a vast generalization of ordinary geometry.

Alain Connes picks a certain spectral triple that encodes a target space which is a weird non-commutative space and argues that it comes close to encoding the standard model. Nobody complains that he picks that spectral triple from a huge “landscape”, namely from the space of *all* spectral triples. Remarkably, his spectral triple has K-theoretic dimension 10. Suppose this arises as the point-particle limit of a 2-spectral triple a la Soibelman, i.e. from a 2dSCFT of central charge 15. Then, clearly, this won’t be of sigma-model type and will not describe an effective target which is a 10-dimensional manifold.

All this mostly shows one thing: we know so shockingly little about the space of all 2dCFTs and yet are used to hearing so shockingly many claims about what it looks like.

So perturbative string theory does not predict that spacetime is 10-dimensional. What it does predict (essentially as its fundamental hypothesis!) is that spacetime is the effective target geometry of a 2dSCFT of central charge 15. That’s all.

10-dimensional manifolds appear here only in the most simple minded examples. Claiming that string theory predicts 10-dimensional spacetime is exactly like claiming that general relativity predicts flat empty Minkowski spacetime. No, it does not. This just happens to be the most simple solution that comes to mind.

Precisely similar comments apply to heterotic models which predict 496 gauge bosons.

65. **Daniel de França MTd2**
March 25, 2008

“10-dimensional manifolds appear here only in the most simple minded examples.” But those are the models that are uniquely said to be truely superstrings. Or at least that is what seems but taking a daily look at hep-th. Some trolle people even troll to say that those are uniquely the true ones. I mean, that’s the impression I take by looking everywhere…

66. **Thomas Larsson**
March 27, 2008

If some structure is physically relevant, it is usually the simplest examples that are the easiest to see in nature. In the application of CFT to 2D statphys, the simplest examples are things like the Ising and Potts models. One could in principle construct systems with arbitrary multicritical behaviour (e.g. RSOS models), but it is almost impossible to realize such systems experimentally when multi >= 6 or so. Similar comments apply to spin (reps of SO(3)) and to the gauge groups in the SM.

Since the simplest CFTs applied to gravity give completely wrong answers, it is a very clear indication that the basic idea is just wrong. But my opinion is of course colored by the fact that I have successfully unified the symmetry principles underlying gravity (4-diffemorphisms) and QM (lowest-energy reps). The resulting theory does not look anything like string theory.

That mathematicians fall in love with some mathematical structure has historically not been a good indicator of the structure’s physical value. Ptolemy and Lorentz (and definitely Poincare) were good mathematicians by the standards of their time, and they may have fallen in love with circles and classical field theory, respectively. This neither proved epicycles nor ether theory, it only made it take longer before people accepted the experimental verdict.

67. **Urs Schreiber**
March 29, 2008

Replies to Daniel de Franca's comment are given here.

68. **nbutsomebody**
March 30, 2008

well, It is not clear why string land scape should be predictive anyway. The best alternative we have to string landscape is a QFT like Standrad model or some extension of it where parameters are anyway put by hand. String theory should be thought as a method of including gravity above the framework of quantum field theory.

But the important problem with String theory is that we have not yet derived Standard Model from string theory and it is not even sure that whether it can at all be done. One may argue that the problem is too easy !!!, as there are many vacuas (may be 10^O(100)), many of them should reproduce the standard model as we know. However, one should keep in mind that in standard model we have a many parameters (about 20?, can't remember now) and most of which is known up to many decimal place. It naively seems that we know standard model also in a very huge accuracy. May be number of string vacuas are much more, but reproducing Standard Model will still be an interesting and significant achievement.

Peter said
"The problem is that no one is able to, for any even slightly realistic string theory models, compute the relation between observable quantities (eg masses and mixing angles) and things like moduli parameters that specify the model. This has been the case since string theory became popular, there is no reason to believe it is going to change."

Can somebody kindly enlight me about the exact reason why mass and mixing angles are not calculable from moduli parameters? May be they can be done numerically with recent advances in numerical constructions of Ricci-flat metric in 3-folds.

2008 Templeton Prize

March 12, 2008
Categories: Uncategorized

The 2008 Templeton Prize was announced today. It goes to Michael Heller, a Polish cosmologist, philosopher and Catholic priest, for "sharply focused and strikingly original concepts on the origin and cause of the universe." The full name of the Templeton Prize is the "Templeton Prize for Progress Toward Research or Discoveries About Spiritual Realities" Its goal is to promote bringing science and religion together by awarding a prize of 820,000 pounds sterling, the single largest award given to an individual. Prince Philip somehow gets into the picture too, since he will be presenting the prize to Heller in a private ceremony at Buckingham Palace in early May.

In recent years Heller has been interested in non-commutative geometry as way to study quantum gravity and cosmology. According to Heller, the crucial question of cosmology is "Can the Universe Explain Itself?", and associated with the awarding of this prize, the Templeton Foundation will be hosting a discussion of the associated question "Does the Universe Need to Have a Cause?".

The Templeton press materials describe Heller as "initiating what can be justly termed the 'theology of science.'" His nomination for the prize says that:

> It is evident that for him the mathematical nature of the world and its comprehensibility by humans constitute the circumstantial evidence of the existence of God.

I'm rather dubious about the way Heller mixes theology, philosophy and cosmology, but, unlike much harder-nosed physicists these days, at least he seems to recognize the problems with the Multiverse.

Heller intends to use the prize money to create a Copernicus Center in Cracow to further research and education in science and theology.

Comments

1. **Thomas Love**
   March 12, 2008

   Don't forget that the Big Bang Theory was formulated by Abbe George Lemaitre, a Belgian priest and mathematician. So there has been a mix since the beginning.

2. **Steven H. Cullinane**
   March 12, 2008

   According to the New York Times, it's Prince Philip, not Prince Charles, who will do the awarding.

3. **Peter Woit**
   March 12, 2008

   Thanks Steven, fixed.

4. **!?!**
   March 13, 2008

   820,000 pounds? Now I understand the meaning of papers like:

   Does God so love the multiverse? arXiv:0801.0246

5. **chris**
   March 13, 2008

   actually, the paper you refer to makes a lot of sense to me. and i don't find any metaphysical speculations there.

   it's actually kind of funny that these days physicists construct wild untestable speculations about the universe and have to be reminded about the empirical basis by theologists. well, philosophers at least.

   actually, thinking twice, this is rather depressive than funny.

6. **Yatima**
   March 13, 2008

   Not directly related, but in the department of dubious claims we have this:

   Physicists Make Artificial Black Hole Using Optical Fiber

   I can imagine that the mathematical description of an electromagnetic wave in fiber may have some analogue to the equations of a GR static black hole, but still it is *not* a GR black hole.

   "It is much easier to use these objects for observations instead of their astronomical counterparts," says Grigori Volovik of Helsinki University of Technology, in Finland, who has found other analogues of cosmological phenomena in laboratory condensed-matter physics.

   Mistaking the map for the terrain I would say.

7. **anonymous**
   March 14, 2008

   Peter,

   can I just say that as a long time reader of this blog who is familiar with your frustrations, I admire the fairness and restraint that you showed in this post when you wrote:

   "I'm rather dubious about the way Heller mixes theology, philosophy and cosmology, but, unlike much harder-nosed physicists these days, at least he seems to recognize the problems with the Multiverse."

8. **also anonymous**
March 14, 2008

If any of you commenting/blogging had a look at Heller's publication list (or better still, spared a moment to find some of his recent physics publications on arxiv) he would notice M.H. does not "mix" physics and theology – at least in what he himself put in section "physics" – he does both physics and theology. And that is a big difference. The word 'frustration' is quite appropriate in context of this posting, I'm afraid…

9. **Peter Woit**
March 14, 2008

AA,

The writings of Heller linked to from the Templeton web-site very explicitly are a mixture of theology and physics. I doubt that he would object to this kind of characterization, based on his own comments about his intellectual history and interests. I see no evidence at all that he sees theology and science as two completely separate things.

10. **Andy**
March 15, 2008

Anonymous — I don't see any postings of M. Heller on the arXiv. There are a large number of papers by Urs M. Heller, but he is a different person (his institutional affiliation is the American Physical Society in New York).

I must say, however, that M. Heller's critique of the multiverse displayed a rather good understanding of the philosophy of science and of the underlying scientific issues. Certainly he is no Fritjof Capra or Gary Zukav!

11. **Peter Woit**
March 15, 2008

Andy,

There are quite a few papers by Michael Heller on the arXiv, see

http://arxiv.org/find/gr-qc/1/au:+Heller_M/0/1/0/all/0/1

It's true that these papers have no theological component, also true that he has written many other papers explaining how he sees his scientific work as inspired and related to his theology.

12. **Angel**
March 15, 2008

@ all,
The extent to which Heller 'mixes' science and theology is described in detail in his forthcoming book 'A Comprehensible Universe'
See: http://www.springer.com/philosophy/philosophy+of+sciences/book/978-3-540-77624-6

13. **alex**
April 8, 2008

Don't forget that the Big Bang Theory was formulated by Abbe George Lemaitre, a Belgian priest and mathematician. So there has been a mix since the beginning.

Electric-Magnetic Duality on a Half-Space

March 13, 2008
Categories: Uncategorized

The past few weeks I've often been going down to the IAS in Princeton on Thursdays to hear talks given as part of the special program there this semester in mathematics. These talks included a series of five talks by Witten; notes from David Ben-Zvi and Sergei Gukov are available here.

The first three talks concentrated on the existence of a very special superconformal six-dimensional QFT, and information that could be derived from what is known of its properties. Such a theory is an inherently quantum object, lacking a usual sort of classical limit or Lagrangian formulation. Witten compares it to the holomorphic conformal field theory that appear as "square roots" of the WZW model. These field theories are closely related to the representation theory of loop groups and at the core of a several important mathematical developments of the last couple of decades. The mathematical significance of the six-dimensional theory remains much more mysterious, and Witten argues that understanding this mystery is a very worth goal for both mathematicians and physicists. . For more about this, see the article Conformal Field Theory in Four and Six Dimensions, based on his lecture at the Oxford conference in honor of Graeme Segal's 60th birthday back in 2002. Taking the six dimensions to be the product of a torus and a four dimensional space, the existence of such a superconformal six dimensional theory implies an SL(2,Z) symmetry of N=4 Super-Yang-Mills on the four dimensional space. This includes the famous Olive-Montonen non-abelian electric-magnetic symmetry that is responsible for Langlands duality in Witten's 4d QFT approach to Geometric Langlands.

The last two talks of the series dealt with a different topic, boundary conditions in N=4 SYM. Taking this theory on the half-space with boundary conditions, one can ask about the implications of non-Abelian electric-magnetic duality for these boundary conditions. Witten has recently been working on this subject with Davide Gaiotto, he'll be talking about it later this month at a Stony Brook symposium in honor of C. N. Yang and Jim Simons, and I assume a paper will appear sooner or later. In his IAS lectures Witten was talking to mathematicians and arguing that "universal" operations (ones that can be done uniformly for all Riemann surfaces) in geometric Langlands should all come from the properties of these boundary conditions. Note that in this work what appears is the full N=4 SYM theory, not just the topological twisted version. This theory plays a central role in AdS/CFT, so if new information about its physics arises from this study, this should be directly interesting for physics, although Witten did not discuss this in his talks.

The two sorts of boundary conditions that get related by duality are analogs of Neumann and Dirichlet boundary conditions. The Neumann boundary conditions involve superconformal 3d QFTs, examples of which were studied by Intriligator and Seiberg in their 1996 paper Mirror Symmetry in Three Dimensions. Witten has previously worked on this kind of thing in the Abelian case, see here.

During these visits to the IAS I got the chance to meet Meng-Chwan Tan, who is there in the Physics group this year. He has been working on a different QFT approach to geometric Langlands, one that is purely two-dimensional and based in conformal field theory, using (0,2) sigma models on flag manifolds, and has just posted a the revised for publication version of his paper on the subject here. This is much closer to the approach to geometric Langlands via conformal field theory that Edward Frenkel has described here.

In other geometric Langlands news, there was a workshop on Homological Mirror Symmetry recently in Miami, with notes from many of the talks available here (and a blog posting by Joel Kamnitzer here). And there's another one (notes here from David Ben-Zvi) going on this week at the IAS. I better stop now, go and get some sleep so I can head down there tomorrow morning to catch the last day of it.

Comments

1. **c.r.**
   March 16, 2008

   Hi Peter,

   I wanted to mention that as a math grad student who is getting interested in geometric representation theory and related topics, I find your posts on Geometric Langlands to be very informative. I hope you continue to post more on the subject. It is a shame that entries such as this one typically don't generate as many comments as the ones about string theory.

Media Events in Paris

March 15, 2008
Categories: Uncategorized

Tomorrow I'll be in Las Vegas, on my way to southern Utah, so will miss a couple of math-physics media events taking place in Paris. At 2pm on Sunday, Lubos Motl will be appearing at the France Television booth at the Salon du Livre, together with the Bogdanov brothers, to sign copies of his new book L'equation Bogdanov: Le secret de l'origine de l'Univers?. In other Lubos news, I recently heard a rumor that he is now the scientific advisor to the president of the Czech republic.

On Monday, a day-long symposium on the topic of how mathematics and physics are covered in the press will be held at the Institut Henri Poincare. One focus of the symposium is the celebrity exceptional Lie group E8, which last year kicked up media-storms for both the classification of unitary representations of its split real form, and for Garrett Lisi's attempt to use it for unification. Jean Iliopoulos will be speaking on the topic of the hopes and controversies surrounding string theory, and I'd be curious to find out what he has to say about this.

Update: More information about the E8 talks here, Lubos's impressions of Paris here.

Comments

1. **milkshake**
   March 15, 2008

   Czech prez Klaus is an ex-prime minister. He does not make policy decision (including decisions about science) anymore, as a prez he is just a titulary head of state that talks to other dignitaries and gives speeches.

   He is quite an ideologue who derives much of his recognition from his outspoken opposition to EU bureaucracy, socialist state and enviroactivism. Klaus is economist by training so he could use some help with preparing his anti-global-warming arguments. It has been a love fest between him and Lumo for quite some time.

2. **theoreticalminimum**
   March 16, 2008

   Peter,

   Have you, or are you planning to, read Motl's book?

   I have read a review written by a French guy named Christophe de Dinechin, who writes the blog "Grenouille Bouillie". As one can read, he points out many expository and factual shortcomings of the book.

3. **Lubos fan**
   March 16, 2008

   Chapter 1 (*Le grand mystère de l'Origine*) of *L'equation Bogdanov* by Lubos Motl is freely available from the publisher in PDF format: http://www.presses-renaissance.fr/extraits/9782750903862.pdf

4. **MathPhys**
   March 16, 2008

   The backcover says that Motl is the author of a fundamental paper on string theory. Really?

5. **Robert**
   March 16, 2008

   I find it surprisingly well written, the first chapter of Motl's book, and quite entertaining.

6. **Jos**
   March 16, 2008

   Well, in page 4 of the pdf. it says "le temps de Planck" = 0,1 sg. I thought it was smaller 🙂

7. **Rien**
   March 16, 2008

   Enjoy southern Utah, that's one of the most beautiful and bizarre places on Earth.

   The Czech president supposedly has the same mistaken view of global warming as Lubos. But anyway, did Lubos write the book in French or is it translated?

8. **chris**
   March 17, 2008

   oh my god, i thought this was a joke first. really interesting though, that someone knowledgeable can hype the bogdanov brothers while dismissing people like smolin as crackpots. what has the world come to…

9. **MathPhys**
   March 17, 2008

   I'm confused. What is Motl's position of the B Brothers?

10. **Chris Oakley**
    March 17, 2008

    Hi MathPhys,

My understanding of the situation is something like this: The Bogdanov brothers, though well-known in France as TV science presenters, crave legitimate academic credentials. So they write theses & publish papers on quantum gravity that, though containing a lot of buzzwords, are generally regarded as having negligible scientific content. Peter W (and John Baez, and many others) say so publicly. Lubos then, on the basis that anyone who Peter thinks is charlatan must have something going for him, then takes the brothers' side, writing a book that is vaguely supportive, but mostly just a platform for his own opinionated ramblings. Because of the Bogdanov's celebrity status, the book gets published, and Lubos gets on French TV.

Or have I missed something here?

As for the Scientific Advisor to the Czech government job, if this rumour is true, then I can only assume that is because they want a climate change sceptic on their books. I wonder if he has told them that climate change is something that he only does as a hobby? Probably not.

11. **Mondrian**
March 17, 2008

Just found this:

http://www.xkcd.org/397/

cheers

12. **Fabien Besnard**
March 17, 2008

Concerning Motl's book, I wrote a short review here (in french).

13. **anon.**
March 17, 2008

Fabien's review translated into English by Babelfish.

14. **Chris Oakley**
March 17, 2008

Hi Fabien,

Even with my poor French, I like your review. Any chance of an English translation here? (Babelfish is, I am afraid, not really up to it).

15. **Gilbert**
March 17, 2008

Well, here's one. I hope Fabien doesn't mind:

The "science" section of large bookstores, jammed between "esoterica" and "personnel development" was already chalk full of "Canada dry" physics, courtesy of the Bogdanov brothers, when an ex-Harvard professor (so it says on the cover), known for a highly nuanced internet site, jumped in to offer his support (The Bogdanov Equation, the Secret to the Origin of the Universe? Lubos Motl, Presses de la Renaissance, 2008, 240 pages, 19 €). The reason, if "reason" is really the appropriate term here, is easily summed up: only theories involving strings make any sense (p.199), all those who think otherwise are idiots (cf. p. 105 where the author modestly explains that he has become a sort of messiah for having proved a conjecture in a competing theory). As the first critics to point out the absurdities in the articles of the Bogdanov brothers were not string theorists, the Bogdanovs deserved support. In short, the enemies of my enemies are my friends. That's all that was needed for Professor Motl (have I mentioned he used to teach at Harvard?) to sign a book to the greater glory of the twins. The author's style, which one could compare to that of a Japanese tourist guide (to your left Galileo's telescope, to your right Newton's apple), with a dash of self importance thrown in, is often very bogdanovien, especially during the introduction, in pomposity ("The Ultimate Secrets of the Universe") and blunders ("Alexander Euler"). But enough suspense: nowhere in this book will you find even a hint of the Bogdanov equation. But by the time you put it down, you will have solved the Motl equation.

16. **Chris Oakley**
March 17, 2008

Excellent!

Oh – and did you mention that Lubos used to teach at Harvard?

I assume "Canada Dry" one translates as "Mickey Mouse" (unless it's a reference to those crackpot not-totally-convinced-by-string-theory lunatics at PI).

17. **Coin**
March 17, 2008

I recently heard a rumor that he is now the scientific advisor to the president of the Czech republic.

It seems like this is the kind of thing that it would be in principle possible to get a definitive answer on one way or the other rather than having to reply on rumors.

18. **Fred**
March 17, 2008

So this book is pretty funny. It has absolutely nothing to do with the BD brothers (who *are* cranks any way you slice it) or their physics and everything to do with LM managing to weasel his way into getting a book published without undue expense and hassle.

Consequently the physics enclosed is pretty solid (despite the usual hysterics Motl injects in his discussions) and if you like the style of his website, sort of funny. On the other hand it is indeed a little bit *too* opinionated and does read a bit like some sort of glorified advertisement for LM's views on physics

19. **Fabien Besnard**
March 17, 2008

Gilbert, thanks a lot for your translation. It is much better than anything I could have written myself. As for Canada Dry, I'm not sure it is known in the US : this is a beverage that is supposed to "taste like alcohol, but is not alcohol.", as the commercials put it.

20. **Gilbert**
March 17, 2008

Hi Fabian, Chris,

My pleasure. I had been wondering about the Canada Dry reference myself. I couldn't translate it as it was already in English, and wasn't sure what to make of it. Back in the day, right around the first superstring revolution, the ads for it made a point about how it was "not too sweet", which left me scratching my head. Thanks for clearing up that it's meant to be something like "looks like physics, sounds like physics, but tastes like…. Well, not physics."

21. **ad**
March 17, 2008

Canada Dry is dry ginger ale. It's used as a mixer, eg. brandy 'n' dry. Clayton's I always thought was the 'drink you have when you're not having a drink'.

22. **Professor R**
March 18, 2008

Hi Peter,
interesting thread as always.It reminds me of a query I had about your book.

If I remember correctly, you had a whole chapter on the Bogdanov Affair – it made for v interesting reading, but I remember being a bit puzzled about the connection to string theory (and more so when I looked up the papers). Their work certainly seemed to be a fairly typical example of meaningless hyperbole, but not particulaly in the area of ST – was your point that this stuff is facilitated by the vageness of the whole string theory edifice?

Apologies if I've misremembered – I don't have your book to hand as I'm on holiday….regards Cormac

23. **Urs Schreiber**
March 20, 2008

> Their work certainly seemed to be a fairly typical example of meaningless hyperbole, but not particulaly in the area of ST

There is a general point to be made that part of current "formal high energy theoretical physics" has become a rather complicated mess which, while doing very fine in the hands of some, has gotten rather out of control here and there, being neither math nor experimental science. Which is the reason why way back when the Bogd.-brother's thing came out, there was for a short while the suspicion that maybe they were intentionally playing a game with the theoreticl physicis community, the way Alan Sokal did to the Social Sciences back when, which suffered from similar disease of partial degeneration.

String theoretically motivated work and quantum gravity research in general has its grand share in the part of theoretical physics that would admit Sokalification, but it's not restricted to that. Ironically, in an age where, due to plenty of new experimental data, *cosmology* has become a fruitful exact science, much of what is being done in "theoretical cosmology" is little more than story telling.

24. **MathPhys**
March 20, 2008

Urs,
Are you saying that the B Brothers were serious? I thought it's now accepted that they were not.

25. **chris**
March 20, 2008

it's now accepted, but there was the suspicion for a short while that they were.

26. **anonymous**
March 20, 2008

Have you seen this one?

http://xkcd.com/397/

27. **Peter Woit**
March 20, 2008

Cormac,

In the book I wrote about the Bogdanov story mainly because I thought it raised important questions about how this kind of speculative research gets evaluated in an environment where speculation has gotten out of control and the standards that keep it in check have broken down. This wasn't specifically about string theory, and some people gave me grief claiming it was an unfair hit on string theorists.

In retrospect though, I think it was not unfair, that the problems raised by the Bogdanov affair have become much worse in recent years. Almost all of the book was written before the Landscape nonsense got going. Part of the reason the Bogdanov papers are obviously over the edge is that they go on about "before the Big Bang" in a hyper-speculative mode. At the time, this was fairly unusual behavior, now it's mainstream cosmology, at least as practiced by string theorists.

Another reason for including the Bogdanov story was that I couldn't resist including a quote from an e-mail written by someone who observed the response of some members of the string theory group at Harvard, who seemed to be having trouble recognizing obvious nonsense. Again I was criticized for this as an unfair characterization of some string theorists. Now we have one of those string theorists writing a book promoting the work of the Bogdanovs. I rest my case on this one…

Finally, I think the Bogdanov story was important because it clearly demonstrated that the refereeing system in theoretical physics has broken down, as they were able to get nonsense papers published in several reputable journals. The response to this, and the latest plagiarism scandal, where journal editors have decided to either ignore or defend plagiarism, shows that the situation here has gone from bad to worse.

28. **milkshake**
March 20, 2008

when Bogdanovs affair came up Lubos claimed that Harvard string theorists discussed Bogdanovs – and they concluded that the Bogdanov brothers made a valid (if dim) effort and the main reason why ST community did not object to those papers was that they were just little below the average so no-one really cared, etc.

29. **Urs Schreiber**
March 20, 2008

“MathPhys” and “Chris”,

not sure what you are referring to. By their own account, their work was not *meant* as a joke.

Imagine how immensely famous and reputed they could have become by instead claiming afterwards that the papers they managed to publish in journals *were* meant as a joke.

30. **chris**
March 20, 2008

Urs,

this is from your perspective. i.e., they would have risen in the eyes of hephysicists. but they only care about their public image because this makes them the money. and they needed a ph.d. for that.

i think it is just so plain simple.

31. **Chris Oakley**
March 20, 2008

It may just be that the Bogdanovs are choosing their moment. They may yet do a “special” on French TV going through the whole story of their PhDs and papers, explaining how the whole thing was devised simply to demonstrate to the world just how stupid and gullible the so-called “smart” people are.

They just want to snare a few String Theorists first and probably would have done the expose already but for Lubos’s book.

32. **YBM**
March 20, 2008

” …that they go on about “before the Big Bang” in a hyper-speculative mode. At the time, this was fairly unusual behavior, now it’s mainstream cosmology, at least as practiced by string theorists. “

Beware Peter ! This kind of sentence, with a bit of magic in the translation process, could likely be quoted in the next B&B’s book…

33. **Urs Schreiber**
March 21, 2008

Theoretical cosmology is and has been overly speculative even if it didn’t come, as they say, “string inspired”. “String cosmology” proper does not really exist, in the absence of anything but a perturbative formulation.

The power of Einstein’s insight allows people to write down a simple ordinary differential equation in one or two variables, point to it and translate it into fantastic stories about the origin of the universe. While nice, this has lead to strange outgrowth.

I’d dare say that the problem one sees in theoretical cosmology is to a large degree also a result of activities of the school of Hawking et al.

34. **MathPhys**
March 21, 2008

Does Lubos say that the B Brothers are serious scientists? I find this hard to believe.

35. **MathPhys**
March 21, 2008

I have just read this

http://en.wikipedia.org/wiki/Bogdanov_Affair

and it contains the whole strory. I now know that Motl actually defends them.

36. **Hans de Vries**
March 21, 2008

You can search the book, and yes…. it has three entries for Woit and four for Smolin.
In the introductions Lubos “reveals” a private email of Woit to the
brothers where Peter says that the brothers may certainly
have found new and interesting results but the problem is that to be able to readily understand the significance of what they have written requires an expertise held by only a handful of people in the world…..
He goes on to claim that Smolin also isn’t capable of understanding
the mathematics used by the Brothers. Next in line is Franz Wilczek.

Regards, Hans

37. **Thomas Love**
March 24, 2008

Peter, According to

http://www.nndb.com/people/195/000165697/

you are a string theorist! Rather funny considering that they mention NEW!

Will Physicists Find God?

March 25, 2008
Categories: Uncategorized

Today's second most viewed article in Newsweek is an interview with Steven Weinberg about what we'll learn at the LHC. Unfortunately it almost immediately turns into a discussion about religion and is linked to on the Newsweek site as Will Physicists Find God? The interviewer wants to know whether the Nobelist will be willing to reconsider his well-known atheism based on what is found at the LHC. Weinberg does a good job of answering these questions politely and sensibly. He gets a bit into philosophy of science, noting that the hypothesis of the existence of God is testable (in the same sense that string theory is testable), since thunderbolts coming out of the sky and striking atheists dead would give strong evidence that He (or She) exists.

Sean Carroll, quoting from a book by David Deutsch on parallel universes, attacks Weinberg as not understanding how science works in a blog posting about Science and Unobservable Things, and in a discussion with John Horgan at Bloggingheads entitled Cosmic Bull Session. He specifically is critical of a claim by Weinberg that "the important thing is to be able to make predictions", arguing that such a statement is "going a bit too far."

This month's Discover magazine has a cover story on theories of what happened before the Big Bang. The article begins with St. Augustine speculating on what God was doing before the first day of creation, then moves to discuss the work of several modern "cosmology heretics". The discussion doesn't include the work of the Bogdanovs, but does cover three such theories, from Steinhardt and Turok, Carroll and Chen, and Barbour, ending up with a discussion of the crucial problem of testability of such theories. Steinhardt and Turok are rather concerned about this, and point to one negative prediction (shared by many cosmological models) that they can make: effects of gravitational waves will not be seen in the CMB polarization. Carroll and Barbour on the other hand don't seem to have a problem with not being able to predict anything, with Barbour described explicitly as having "no way to test his concept of Platonia."

For more recent research on the multiverse, see philosopher Klaas Kraay's Theism and the Multiverse, where he argues that:

> theists should maintain that the world God selects is a multiverse. In particular, I claim that this multiverse includes all and only those universes which are worth creating and sustaining. I further argue that this multiverse is the unique best of all divinely-actualizable worlds.

Comments

1. **Michael Bacon**
   March 25, 2008

   Peter,

   You said that Carroll ". . . specifically is critical of a claim by Weinberg that 'the important thing is to be able to make predictions', arguing that such a statement is 'going a bit too far.'"

   Not exactly, the quote says just a bit more. Deutsch is apparently critical of the following quote from Weinberg:

   "The important thing is to be able to make predictions about images on the astronomers' photographic plates, frequencies of spectral lines, and so on, and it simply doesn't matter whether we ascribe these predictions to the physical effects of gravitational fields on the motion of planets and photons or to a curvature of space and time."

   Of course it "does matter" whether or not an eclipse is predicted by Ptolemy's geocentric theory of the solar system — wasn't this really the point that Deutsch and Carroll were trying to make?

   Of course, you're trying to make a broader point, but . . . . .

2. **Peter Woit**
   March 25, 2008

   Michael,

   After quoting the longer passage, Sean later pulls the part that I quoted out of it and refers to this specific part, in the way that I indicated.

   The full Weinberg quote makes perfect sense. Weinberg understands the scientific method well, and is expressing himself accurately and carefully. He's not discussing eclipses and Ptolemaic theory, and what he writes is not "crazy", as Sean describes it. Both Sean and Deutsch are wilfully misinterpreting Weinberg's words and setting up a silly straw man to knock down. I get a lot of this, but it being done to Weinberg.

3. **Shantanu**
   March 25, 2008

   Peter have you seen D. Gross's talk on status of string theory at IPMU
   opening symposium. It can be found here
   The video is also apparently online, though I cannot find the link.

4. **Michael Bacon**
   March 25, 2008

   Peter,

   You said " [a]fter quoting the longer passage, Sean later pulls the part that I quoted out of it and refers to this specific part, in the way that I indicated."

   Actually, immediately following the quote from Weinberg, Sean says "[t]hat's crazy, of course — the dynamics through which we derive those predictions matters enormously. (I suspect that Weinberg was trying to emphasize that there may be formulations of the same underlying theory that look different but are actually equivalent; then the distinction truly wouldn't matter, but saying "the important thing is to make predictions" is going a bit too far.)"

   Perhaps not quite as clear as it could be, but not really " . . . wilfully misinterpreting Weinberg's words and setting up a silly straw man to knock down . . " either, is it?

   As for your statement that Deutsch is willfully misinterpreting Weinberg's words, what exactly do you base that on? I have an old copy of the book around somewhere and I guess I could pull it out, but it seems like a lot of effort for the issue involved. I suspect it was part of a much longer and detailed argument by Deutsch that the purpose of science is more than making predictions — it's explaining how the world works. You may not agree with that emphasis, but I think Deutsch is usually careful and sincere in this regard.

Anyway, I do enjoy your blogging even if I don't always agree — keep it up.

5. **Peter Woit**
   March 25, 2008

   Shantanu,

   Thanks for the link. It looks like Gross's talk was much the same as other ones that I've discussed here in recent years, urging string theorists to not give up the search for a new formulation of string theory that would have a unique vacuum and make predictions. Also, the talk was at the opening of the new IPMU in Tokyo, which I should have written about.

   Michael,

   I still don't get the "that's crazy, of course" bit (and you're right, that's Sean, not Deutsch). There's nothing crazy at all about what Weinberg writes, unless you misinterpret it.

   I took a more careful look at the original source from Deutsch. He's trying to use the Weinberg quote to set up a contrast between "predictive" and "explanatory" power of a theory, but both sides of this are slippery. The problems with defining a "prediction" are well known, and an "explanation" is even trickier to define. We want not just an "explanation", but a "scientific explanation", and to me such an explanation inherently comes with some way that you can test it by observation of the real world, i.e. it makes predictions. Sure, you can come up with constructions of a theory with predictive, but not explanatory power, and that would be an unsatisfactory theory. But there really is no such thing as a scientific theory with explanatory power, but no predictive power, and unfortunately that's what some string theorists and multiverse proponents are trying to sell to the public these days.

6. **Kris Krogh**
   March 26, 2008

   The original Weinberg quote is one I'm fond of, from page 147 of his text *Gravitation and Cosmology*. He was making the point that we are not necessarily bound by experimental facts to Einstein's geometric interpretation of gravity:

   …Einstein and his successors have regarded the effects of a gravitational field as producing a change in the geometry of space and time. At one time it was even hoped that the rest of physics could be brought into a geometric formulation, but this hope has met with disappointment, and the geometric interpretation of the theory of gravitation has dwindled to a mere analogy, which lingers in our language in terms like "metric," "affine connection," and "curvature," but is not otherwise very useful. The important thing is to be able to make predictions about images on the astronomers' photographic plates, frequencies of spectral lines, and so on, and it simply doesn't matter whether we ascribe these predictions to the physical effect of gravitational fields on the motion of planets and photons or to a curvature of space and time. (The reader should be warned that these views are heterodox and would meet with objections from many general relativists.)

   I.e., there is more than one way to skin a cat.

7. **chris**
   March 26, 2008

   michael,

   actually, ptolemy's theory was discarded not by its metaphysical implications but by its inability to accurately describe planetary orbits, in particular that of mars. it is simply a false statement regarding a crucial point in science history that the ptolemaic system was obsoleted by its dynamical content or anything the like.

   actually (in one of these truly unscientific what if statements) i would argue that had the ptolemaic system described planetary and lunar orbits just a tad better (so that naked eye observations would not reveal inconsistencies), people at the time would have probably tried harder and harder to mate it to aristotelian dynamics.

   but fortunately, at this juncture, observational data were good enough to guide the way. if you just recapitulate, keplers milestone achievements were based on tychos and his own data, that were orders of magnitude better than previous ones. in fact, the very new ingredient of observing planetary positions out of opposition could be viewed as a major experimental breakthrough that allowed kepler to derive the elliptical form of the orbit.

   this particular episode is very general to scientific advancements i think. and i challenge you to give me one single instance of advancements in fundamental physics that were not based on solid experimental (including of course observational) data.

8. **anon.**
   March 26, 2008

   'actually, ptolemy's theory was discarded not by its metaphysical implications but by its inability to accurately describe planetary orbits, in particular that of mars. it is simply a false statement regarding a crucial point in science history that the ptolemaic system was obsoleted by its dynamical content or anything the like.' – chris

   Chris, you are completely wrong, please see the Java animation which compares the motion of Mars predicted by both theories:

   http://www.jimloy.com/cindy/ptolemy.htm

   '… Mars' retrograde motion, the way it appears to go backward in the sky. This happens because the faster Earth passes the slower outer planet (in the left part of the diagram). And on the right, we have Ptolemy's attempt to explain retrograde motion with a fixed Earth, using epicycles. Both ideas (Copernicus' Sun-centered system, and Ptolemy's Earth-centered system) seem to work well. But the Sun-centered system is the true situation.'

   The point is, you could endlessly add epicycles to Ptolemy's model to 'correct' imperfections. Ptolemy's model made anthropic predictions because the exact version of the infinite landscape of possible epicycles was selected specifically to match known features of the observer's universe, including the motion of Mars.

   If you then make a prediction of something new but the prediction fails, the temptation is to announce the discovery of a new epicycle to 'correct' the model, to claim you are doing science, not to announce that you've discovered you're wrong. So every problem with the theory is hyped as being 'scientific evidence of the need for more complexity in nature': see e.g. *The Crime of Claudius Ptolemy* by Robert R. Newton (John Hopkins University Press, London, 1977). The ptolemaic system was obsoleted by its vast dynamical content of epicycles, compared to the simplicity of Newton's three laws of motion.

9. **Michael Bacon**
   March 26, 2008

Peter,

You said "... there really is no such thing as a scientific theory with explanatory power, but no predictive power. . ."

Yes, and as anon's post implies above, if you have a theory that seems to have predictive power it may in fact not have explanatory power and thus not be a true scientific theory. I agree that any true "scientific theory" should both make predictions and explain how the world really works.

I still have a bit of a problem with '. . . it simply doesn't matter whether we ascribe these predictions to the physical effect of gravitational fields on the motion of planets and photons or to a curvature of space and time." I think in the end it does "matter." However, the full quote from Weinberg provided by Khris shows that the ideas were part of a broader and more complicated argument — something that's not always conveyed in the 25 word or less quote — whether the quote is taken from Weinberg, Carroll, Deutsch or you.

Thanks again for such an interesting blog.

10. **jurisper**
March 26, 2008

Going the other way, the Economist has a long piece on various projects to find scientific explanations for religion: http://www.economist.co.uk/science/displaystory.cfm?story_id=10875666

11. **Peter Woit**
March 26, 2008

Kris and Michael,

All Weinberg is doing is making the point that what is experimentally known about the gravitational force is consistent with either a theory based on Riemannian geometry, or with one that has a massless spin-two particle to provide the gravitational force. In the latter case, geometry may be "emergent", only an aspect of a low-energy approximation. The idea that some (unknown) more fundamental non-perturbative version of string/M-theory might have this feature is what Gross is talking about in the lecture linked above. I'm still not getting why Sean thinks this is "crazy".

12. **chris**
March 26, 2008

hi anon,

i am very sorry, but i have to accuse you of ignorance in this topic. of course retrograde motions are possible in ptolemys astronomy. and of course you could add an arbitrary number of epicycles to match the observed position of each planet but the historic fact is, that the breakthrough of copernican astronomy happened precisely with the establishment of the elliptical orbits around the true (and not the average) position of the sun. and even before that, copernicus-based planetary tables were more precise than the preceeding alphonsine tables which was percieved at their times as the major advantage.

it's easy in retrospect to fall into the trap of believing that observational methods back then were so poor that everyone would be content with vaguely qualitative statements like the one you quoted. they were not. similarly, it is only in retrospect that the infinite addition of epicycles is a good argument. for people of the day (without a firm grasp of the concept of series limit e.g.) the complication of one additional epicycle in terms of real work was quite noticeable and models that simplified were quite welcome.

let me finally add, that perhaps the final irony in this discussion is the arguments brought against the copernican system. if one discounts all the preoccupied 'it must not be so it can not be' type, the one main scientific argument against copernicus was metaphysical in nature: if earth would rotate, the missing observed parallaxe on the fixed stars implied their distance to be way beyond contemporary belief about the size of the universe (mainly influenced by aristotelian metaphysics). that was the most serious argument *against* heliocentrism.

in conclusion, i think this little piece of science history highlights very nicely the forward power of painstaking observation-phenomenology-modelbuilding as compared to the manifest backward trend inherent in metaphysical speculation.

13. **Bee**
March 26, 2008

Hi Peter,

Reading your post, I have to wonder whether the question should have been 'Will physicists search for God'.

The Discover article is actually nicely written.

As an aside, there is this annoying 'null-physics' ad in the middle of the page. I first noticed that while reading an article about Einstein's family, and the ad said 'null physics – bring the science back into physics' or something like this. I wrote an email to the magazine, complaining the whole advertisement is an insult to every physicist reading it (no matter what it actually advertises), and got the expected reply saying we have to get the money in etc. I don't know whether there is a causal relation, but at least the slogan of that ad has changed into something less offensive. (I am occasionally convinced advertisements are the source of all evil in the world.)

Either way, to come back to the question of making predictions vs. offering explanations. There has been a decade or so where PREDICTIONS, testability, and falsifiable were the keywords. Now I have to wonder whether at some point people will just turn around, drop these efforts and say, no, we don't need predictions, stop whining. It's a bit scary to think about what this would do to science, starting from theoretical physics which is especially vulnerable in this regard, but when I read your blog it doesn't seem impossible to happen.

Best,

B.

14. **Michael Bacon**
March 26, 2008

Chris,

You said "... this little piece of science history highlights very nicely the forward power of painstaking observation-phenomenology-modelbuilding . . ."

It's true that Ptolemy's theory was discarded for its metaphysical implications and for it's inability to accurately describe planetary orbits, particularly as new observations were made.

I certainly didn't mean to imply that an incorrect scientific (i.e. falsifiable) theory wouldn't inevitably be proven wrong by further observation and discovery. In fact, that's the notion I'd hoped to reinforce by mentioning how one such theory — which held sway with quite a few people for a good period of time — ultimately neither correctly predicted natural phenomena or explained the true nature of world, and was discarded.

In any event, a scientific theory has to make testable predictions. Carroll speculated that Weinberg might have been getting at this idea: "there may be formulations of the same underlying theory that look different but are actually equivalent; then the distinction truly wouldn't matter." Peter filled in the blanks saying "[a]ll Weinberg is doing is making the point that what is experimentally known about the gravitational force is consistent with either a theory based on Riemannian geometry, or with one that has a massless spin-two particle to provide the gravitational force."

It's easy to distinguish between theories that make different predictions that can be tested. But perhaps in the "hard" cases, it's OK to lean a little toward the view that "explanatory" power takes on a bit more importance.

15. **Peter Woit**
March 26, 2008

Bee,

I definitely find the "we don't need predictions, stop whining" arguments being made worrisome, but don't think this is a big problem for science in general. It is a big problem for certain subfields of theoretical physics though, where the people pushing these arguments have influence. What is happening is that these arguments are being used to justify an unwillingness to drop research programs that have shown themselves to be inherently unpredictive. This behavior stops the field from moving forward, and destroys its credibility among potential young theorists, other physicists, granting agencies, other scientists, and the general public. I think the fall-out from this is already becoming visible.

16. **andy**
March 26, 2008

I'm sorry, Peter, but could you explain why you said this:

"we don't need predictions, stop whining" arguments being made worrisome, but don't think this is a big problem for science in general.

Why isn't lack of predictability "a big problem for science"?

17. **andy**
March 26, 2008

Or perhaps you meant this in the context of, e.g., evolutionary biology, where we can't "predict" the next species to evolve, but we do know that "something" will?

18. **Michael Bacon**
March 26, 2008

Andy,

I thought Peter meant only that the attitude wasn't as prevelant in most areas of science but is a growing problem in subfields of theoretical physics, but maybe I missed the point.

19. **Thomas Love**
March 26, 2008

On page 24 of his talk, Gross says: "We are not sure what the final theory is and what the rules of the game are."

Then on page 25, he says "The Strategy

Calculate

Calculate

Calculate & observe.

Well, if one does not know the rules of the game, one does not know the rules for calculation, which means the calculations are meaningless. All too often physicists substitute calculations for understanding.

20. **fritz**
March 26, 2008

> and i challenge you to give me one single instance of advancements in fundamental physics that were not based on solid experimental (including of course observational) data.

General relativity, and the prediction of the existence of antimatter (Einstein and Dirac, resp.) are the only two examples I know of. Both revolutionized their respective fields, and were not based on previous experimental/observational data.

21. **Peter Woit**
March 26, 2008

andy,

Yes, Michael is right. What I meant is just that I don't see this as having any effect on scientists outside of string theory and associated areas in cosmology. It's not changing their views on the necessity of coming up with ideas that have predictive power if they want to do science.

22. **Peter Woit**
March 26, 2008

fritz,

GR did make distinctive predictions (precession of perihelion of Mercury, bending of light). It's an interesting question how seriously it would have been taken if there were no such predictions. Absent such testable predictions I doubt many people would have considered it as revolutionizing the field.

The Dirac equation makes all sorts of testable predictions, antimatter was just one more.

23. **Peter Woit**
March 26, 2008

Thomas,

Gross has a point that the thing to do when you don't know how to make progress is to calculate what you can and see what it teaches you. But if as you do more and more of these calculations you just find more and more evidence that the framework you are working in can't do what you want, at some point you should stop and calculate in some other framework. I don't see the relevance of "observe" to string theory at the moment.

24. **Kris Krogh**
March 26, 2008

Peter,

I think Sean's point was the same one Feynman made with Mayan astronomers. They could predict eclipses, using a system of beans, without understanding the solar system. Plenty of physicists today seem to feel calculations are the only things that matter, but the irony is that Weinberg has spoken out against that positivist viewpoint. In saying it doesn't matter whether the underlying theory of gravity involves curved space-time, I'm sure he was only referring to the current experimental evidence.

New experimental evidence exists now, a rough measurement of frame dragging by Gravity Probe B, but it isn't clear when we will be allowed to see it. Data collection finished two and a half years ago, but it was announced yesterday the program has been extended again — at least to the end of the year, and possibly to the year 2010. I wouldn't bet the farm that general relativity has been confirmed.

25. **Chris**
March 26, 2008

Hi Peter,

One interesting irony in all this is that Weinberg is in some sense the father of the landscape. I think it is his work (http://prola.aps.org/pdf/RMP/v61/i1/p1_1) on using a landscape to solve the cosmological constant problem that provides the most compelling justification for considering the landscape.

26. **Henry**
March 26, 2008

Peter,

"All Weinberg is doing is making the point that what is experimentally known about the gravitational force is consistent with either a theory based on Riemannian geometry, or with one that has a massless spin-two particle to provide the gravitational force."

My two bits: In the context of the G&C textbook quote, I believe the point was not that there are two competing theories, one based on Riemannian geometry and one based on the massless spin two particle. Instead, there are two ways of looking at the same theory (GR), and one may compute the same answers for the same observables from either perspective. The issue is not that the experimental data is yet insufficient to distinguish between two theories, but that there is really only one theory. This situation can be contrasted with the case of say, GR vs. Brans-Dicke, where one is actually dealing with different theories that lead to different predictions.

27. **Kris Krogh**
March 26, 2008

Hi Henry,

You're right. Weinberg's approach to gravity is considered equivalent to GR, and has not provided any different predictions. (Though I wonder a little whether they are really 100% the same.) Some other non-geometric alternatives to GR do make different predictions for Gravity Probe B.

28. **fritz**
March 26, 2008

Peter, I absolutely agree with you. Chris' challenge was to mention a fundamental advance not directly based on solid experimental data (like the BCS theory depended a lot on the discovery of the isotopic effect, and Hubble's law was basically a fit to data (a fit of sorts, though…)).

As far as I know there are just those two examples. Both were experimentally confirmed about a year after being published, as you say.

29. **Anonymous**
March 26, 2008

There is already an assumption that getting beyond the Standard Model will give us the "final theory" – see Gross's comment cited above.

30. **Michael Bacon**
March 26, 2008

"Sean's point was the same one Feynman made with Mayan astronomers. They could predict eclipses, using a system of beans, without understanding the solar system.

And that was the point I was trying to make referring to Ptolemy . . . . but I like this one better.

31. **Peter Woit**
March 26, 2008

All,

Please stick to the topic, I'm deleting various excursions into the history of physics which don't add anything to the discussion.

32. **Brett**
March 26, 2008

There are plenty more examples of important work that was done without any experimental basis. There's Chandrasekhar's calculation of the maximum white dwarf mass and the subsequent predictions about compact objects. Einstein's prediction of the relative rates of spontaneous emission, stimulated emission, and absorption was done before same could be measured.

Lots of important theoretical work on the foundations and structure of quantum mechanics was done without any experimental input. For example, there's the Aharonov-Bohm effect and Bell's inequalities, both of which introduced highly unexpected phenomena. In the latter case, experimental confirmation didn't arrive until years after the prediction.

33. **mike harney**
March 26, 2008

Peter,

Do you think there is a sociological reason for the association of God in science with various theories like multiverse that fall into the category of "hard to falsify"? It seems to me that there is a common element of mysticism to both areas that allows for common ground to be woven. How do scientists turn the tide back to "predictability" and putting the focus on experimental evidence? Is it a lack or redefinition of what is being taught in schools about the scientific method, or is it just a periodic dark age that we just have to suffer through?

34. **woit**
March 27, 2008

Mike,

The problem here is localized to certain subfields in physics, more particularly to some specific people in these subfields. There's no dark age, the rest of physics and science in general is doing just fine, and I assume that virtually all school teachers continue to teach their students what the scientific method is, recognizing the string theory anthropic multiverse nonsense for what it is and ignoring it.

35. **Anon**
March 28, 2008

I am not quite sure what you understand by the word "testable", but I was a little misled by it, and had to read the original to confirm that Weinberg had not lost his marbles. Weinberg does not use that word, and in fact explicitly states that asserting the existence of a deity is not a falsifiable statement, which to me means prceisely that it is /not/ experimentally testable. So "testable in the sense of string theory" means "not testable", correct?

36. **anon.**
March 28, 2008

'Weinberg [notes that the] hypothesis of the existence of God is testable (in the same sense that string theory is testable), since thunderbolts coming out of the sky and striking atheists dead would give strong evidence that He (or She) exists.' – Woit.

Actually, anyone who looks at all the suffering and tragedy in the world can see that if God exists, then She/He either has better things to do than to monitor (or respond to) serious human problems, let alone to get in a huff about claptrap from atheists like Weinberg. It's a big multiverse, so maybe God's presence is seen elsewhere. Maybe He/She resides only in higher dimensions…

37. **mclaren**
April 6, 2008

Andy asked:

> *"we don't need predictions, stop whining" arguments being made worrisome, but don't think this is a big problem for science in general.*
>
> Why isn't lack of predictability "a big problem for science"?

This is an important point and seems worth discussing briefly. All mathematical models have boundary conditions, and when you push the models close to or beyond those conditions, the models start to break down. A good example would be very high Reynolds numbers in the Navier-Stokes equation. NS predicts fluid dynamics very well until you get into the regime of turbulence (high Reynolds number). Then everything becomes increasingly non-linear and eventually chaotic. That doesn't make the Navier-Stokes equation useless in dealing with fluid dyamics, but does impose limits on how much we can predict and how far into the future underconditions of extreme turbulence. In particular, when the equations become usfficiently sensitive to intial conditions, predictability vanishes.

We see the same situation with weather prediction, protein folding, and so on. Weather prediction works well locally a few days into the future. Globally and years into the future, it works less well. This doesn't invalidate climatology or fluid dynamics as valid sciences: it does place limits on our ability to make predictions far into the future under extreme conditions, given the current state of mathematics.

The hope is that eventually some better mathematical methods will be developed for dealing with highly-nonlinear regimes of dynamical systems, viz., turbulent fluid flow, protein folding, and so on. Work continues in an effort to develop new mathematical methods to tackle these challenging problems. In the meantime, most of these disciplines, like fluid dynamics, give excellent predictions for regimes outside of extreme conditions.

I believe Dr. Woit's point is that this is an **entirely different situation** from a "theory" which cannot be disproven by any possible observations, such as the idle speculations about 10^500 vacua.

2008 Abel Prize

March 26, 2008
Categories: Uncategorized

The winner of this year's Abel prize, a prize of over $1 million set up back in 2001 to provide an equivalent of a Nobel prize in mathematics, is…

Actually, I have no idea. If you know who it is, feel free to break any vows of confidentiality in the comment section here. Otherwise we have to wait until 7am EDT tomorrow morning, when the answer will be revealed here.

Update: The prize goes to John Thompson and Jacques Tits, for their work on finite groups. More details here, including the citations for Thompson and Tits.

Comments

1. **MathPhys**
   March 26, 2008

   I nominate A Grothendieck, who will be 80 this year, and lives in the south of France.

2. **Peter Woit**
   March 26, 2008

   MathPhys,

   A good nomination, but I don't think it will be him. The Scandinavians already once tried to give him a prize carrying a lot of money (the Crafoord) back in 1988, and look what he did then…

3. **Melvin Eloy**
   March 26, 2008

   Incidentally, Grothendieck's birthday is on this Friday, 03/28.

   Hamilton perhaps?

4. **A.J.**
   March 26, 2008

   I'm betting on Israel Gelfand.

5. **Phil G**
   March 26, 2008

   The announcment page says that Marcus du Sautoy will give a popular talk on the subject. Could that be a clue? Marcus has worked on finite groups and zeta functions so it could be someone in that area, maybe.

6. **fp**
   March 26, 2008

   I guess it would be John Tate or (and) Robert Langlands since Du Sautois speaks after the annoucement.

7. **Voltberg**
   March 26, 2008

   Zelmanov and Dusa McDuff are on the committee. Maybe M. Gromov?

8. **Harry**
   March 27, 2008

   My bet is someone who, at least, attached his name to a well-known program: Pr. R. Langland or Pr. R.S. Hamilton ..

9. **MathPhys**
   March 27, 2008

   Gelfand would be a very, very good choice. Besides, he's well over 90 years old, so it's about time.

10. **Abel Prize**
    March 27, 2008

    Abel Prize to Thompson and Jacques Tits!

    Surprizes all round

11. **theoreticalminimum**
    March 27, 2008

    John Griggs Thompson and *Jacques Tits* have been awarded the prize for their contributions to group theory.

12. **Thomas Love**
    March 27, 2008

    Thanks for this link Peter! Normally I visit your site to find links to research articles for me to read. I just sent out an email to my Abstract Algebra class with a link to the

Abel Prize site and instructions to read the articles on group theory, the history of groups and the group theory of the Rubik's cube. That should make for an interesting class next week.

13. **Peter Shor**
March 27, 2008

    Are there any Norwegians who want to translate the jokes in the citations?

14. **Professor R**
March 27, 2008

    Hi Peter, interesting post.
    This is obviously a very prestigious prize – great to see it going to group theory.
    By the way, I guess there's a difference between pure group theory and group theory applied to physics?
    I ask because most of the names listed in the posts above are not familiar to me – also, although there is a specialist physics prize for "outstanding contributions to the understanding of physics through Group Theory" (the Wigner medal), it has not been won by any of the names above, or by Thompson or Tits..

15. **Professor R**
March 27, 2008

    I sometimes think it's a pity these prizes are only awarded to the living. If that sounds daft, there are actually two good reasons for posthumous awards
    (i) the work of these good mathematician/scientists lives on long after them…and often the true value of the work becomes even more apparent after their death
    (ii) while the scientists themselves won't benefit from posthmous awards, their students, their institutions, and even their countries might benefit, inspiring others to similar feats

    For example, should evidence of supersymmetry be glimpsed at LHC, almost none of the pioneers will be alive to see it – so there wil be no public recognition of their long toil in the sixties and seventies. Here in Ireland, I have huge problems convincing people that Dad's work was important, and am continually confonted with books about Irish scientists that ignore his contribution – a few prizes would help!
    Regards
    Cormac O' Raifeartaigh

16. **Chris Hillman**
March 27, 2008

    For those who don't know much about the work of Thompson or Tits, I am cautiously optimistic that master expositor John Baez (who has discussed finite simple groups, buildings, and other highly relevant topics in past postings) will rise to the occasion.

    Would it be imprudent for me to hazard the guess that the award to Tits in particular reflects a resurgent appreciation of the legacy of Klein?

17. **Jakob**
March 28, 2008

    The joke: 604800 equals the number of seconds in a week

18. **Felipe Zaldivar**
March 28, 2008

    Group theory had many origins: the problem of solving polynomial equations via the work of Lagrange, Viete, Cauchy and Galois relating the original problem to the permutations of the roots of the equation, and giving rise to the group concept requiring that the set of permutations must be closed under the product of any two such transformations. This same idea originated in geometry with Klein's idea that a geometry is determined by the rigid transformations of space that leave invariant the objects of that geometry. There is a third source of the group concept, that goes back to Gauss, Kronecker and Dedekind, and is related to number theory, where a new idea comes into the field, that is studiying a commutative group via its characters, i.e., functions from the given group to the multiplicative group of nonzero complex numbers. And there is a fourth line if we consider Sophus Lie idea to create a "Galois Theory" of differential equations in analogy to the Galois theory of number theory and algebra that studies the symmetries coming from the permutations of roots of polynomials. Lie's ideas are the ones any Physicist recognizes immediately, all of them quite familiar with differential equations and Noether's theorem. At the end of the XIX century, Frobenius in Berlin, his student Schur, and Burnside in England introduced the idea of studying an abstract group by replacing its abstract elements with concrete objects, namely, invertible (complex) matrices. Initially they applied this new tool (called the representation of the abstract group) to study the structure of finite groups, but soon they were also considering representations of infinite groups and also of Lie groups, and later on Weyl enters the picture and representation theory (mainly of Lie groups) becomes part of the toolbox of a theoretical physicist. Gelfand, Naimark, Wigner, Bargmann, and many others are associated to the study of representations an characters of Lie groups that are of interest to physicists.
    To end this too long comment, what physists call "group theory" usually means "representation theory of Lie Groups", and the relevance of the abstract representation theory of finite groups is that sometimes it provide methods that may be useful for representation theory in general.

19. **Professor R**
March 29, 2008

    Thanks Felipe, great post

20. **theoreticalminimum**
April 2, 2008

    Something completely off-topic here, but I was just wondering if this paper by Prof Ngô Bao Châu (Université Paris-Sud, Orsay) has come to the attention of those concerned. It is written in French, so I don't think many will be able to understand it, but the abstract is rather interesting.

21. **Peter Woit**
April 2, 2008

    theoreticalminimum,

    Ngo's work on the fundamental lemma has attracted a lot of attention from experts in that area. I wrote a little bit about it here:

    http://www.math.columbia.edu/~woit/wordpress/?p=18

    I'd write more about this and about more recent developments like the paper you mention if I actually understood more about it…

Off-topic

March 27, 2008
Categories: Uncategorized

I usually try hard to avoid writing here about anything not directly related to mathematics and physics, but on rare occasions I can't resist. Many readers may want to skip this posting as unserious, but maybe some will find it entertaining.

My travels last week took me to Las Vegas, where I stayed overnight with my old housemate John Chang and his family. During my graduate student days at Princeton, one year I lived with two fellow physics graduate students who were part of a card-counting team which had started up to take advantage of the recent opening of casinos in Atlantic City. Many of the other members of the team were based at MIT, and my roommates often mentioned one of them, "John". A few years later I was looking for a place to live in Cambridge, answered an ad, and ended up going to meet the owner of a house who was looking for a housemate. After we talked for a while, I realized that he was the "John" my Princeton roommates had been telling me about.

Anyway, you can read more about John in a story just put up at the Xconomy web-site. He's the model for the character "Mickey Rosa" in the book "Bringing Down the House", which has just been made into the movie "21", opening this weekend (Kevin Spacey plays "Mickey Rosa"). I'm looking forward to seeing the movie on Saturday.

Update: For more about John and the card-counting business, see this article in Men's Vogue.

Update: If you're interested in this, you should definitely check out John's blog, which he has started updating again with new postings.

Comments

1. **Chris Oakley**
   March 27, 2008

   This would be great jump-off point for "The String Kings". The String Theorists start out as card counters at Atlantic City, but are kicked out once the management realises what they are up to. Unperturbed, the String Theorists hire their own muscle, forcing the casinos to readmit them, and the cycle of supersymmetric, eleven-dimensional violence starts …

2. **stevem**
   March 27, 2008

   Gambling theory and "statistical logic" are among my interests. The optimum strategy for blackjack was worked out analytically and verified on computer sims as far back as the late 50s and published in American statistical journals. It minimises the house edge but can't overcome it. MIT math professsor Ed Thorpe worked out that the player can gain a 1% edge by counting cards so you can estimate how rich the deck is in aces and tens. There are some amusing stories about him liked how in Las Vegas he doubled $10,000 of mobster money. Thorpe went into finance and later used his abilities to exploit statistical anomalies in various types of securities and make a lot of money. I think he now runs a hedge fund.

   However, according to blackjack folklore card counting was actually worked out first by a character in Las Vegas called "Greasy John", an obese and somewhat obnoxious individual who always had a large basket of fried chicken. But when he played blackjack he won regularly. He seemed to know exactly when to up his bets. However, he took his secret with him since he died of a massive heart attack while playing, but Ed Thorpe gets the credit. There are also various amusing BJ stories like people playing with computers strapped to their backs under their clothes. Card counting is banned in Vegas now I think or else security "asks you to leave". They also introduced multiple-deck games and other distractions. I also heard some of the MIT team got seriously physically threatened in Europe.

   When casinos opened in Atlantic City in the late 70s they did so with a unique rule for BJ–early surrender–which actually gave BJ players an advantage over the house. However, the New Jersey Casino Control Commission who mandated the rule did'nt actually know the math and by the time they got rid of it about a year later a lot of "college kids" and pros had already milked it for millions.

   As author of "String Kings", I do recall they had set up rigged slot machines with only a 1 in 10^500 chance of paying out;) I suppose they could also introduce "supersymmetric 11-dimensional quantum blackjack".

3. **tomj**
   March 28, 2008

   On what planet can a player gain an advantage over the house in blackjack? John Scarne has debunked this idea many years ago.

   But let's review a critical fact: the house advantage in blackjack is based 100% (as a minimum) on the way the game is played. The advantage is due to the fact that players go first, and that ties don't pay off. Any player going over 21 instantly loses. Also, after the first two cards are dealt, if the dealer gets 21, players either lose, or 'tie'.

   Just these rules give the house a 8.27% advantage, but the payoff when a player wins with a 21, which pays 3 to 2, reduces this to 5.9%.

   Card counting is useless in blackjack as long as the last 1/5 or so of the cards are discarded. With 208 cards, at least 40 are never played.

   Certain other rules can bring down the house advantage, maybe to 1-2%, but it is impossible to overcome the advantage the house has from playing last.

   The main advantage that players have is knowing the dealers up card, and also knowing that the hole card can't add up to 21.

   Maybe the best proof is that the MIT group supposedly made $10 million, but John was still seeking a roommate.

4. **Chris Oakley**
   March 28, 2008

   stevem,

   I am not sure quite how one classifies the skill of being able to write insightful reviews of non-existent films, but whatever it is, you clearly have it in spades. My suggestion was actually for the prequel, as some kind of explanation is required as to how a bunch of Princetonian rough-diamonds with bad hearts end up ruling the clubs.

5. **Chris W.**
   March 28, 2008

   Bob Mondello reviewed '21' this evening on NPR's *All Things Considered*. He wasn't especially complimentary:

   > But because the director knows there's a limit to how exciting he can make the sight of cards being turned over, most of the film is taken up with explaining what's happening, or is going to happen, or has just happened. Which may be enough to keep audiences from heading for the nearest slot machine, but at best, 21 is a sort of collegiate *Ocean's 16* — with dimmer stars and less oomph.

   (Actually, he covered two films in one review—usually not a good sign.)

6. **Peter Woit**
   March 28, 2008

   tomj,

   The information stevem gives agrees exactly with what I remember from my days hanging out with card counters, at least one of whom was an extremely talented physicist, expert in the field of Monte-Carlo calculations, which he performed not just on physics problems.

   When I was living with John, he was still in his twenties, had only been doing this for a few years. At the time he definitely didn't have $10 million, but he did own the Cambridge house we were living in ….

7. **tomj**
   March 28, 2008

   Peter,

   I read the thesis of your friend. Compared to the analysis of Scarne, who never even graduated high school, your friend is an idiot. I'm not even sure that your friend understands basic probability theory.

   For instance, your friend assumes an 'infinite deck'. That means that each card always has 1/52 change to be dealt.

   But the theory of counting cards assumes some non-normal distribution of cards. It is obvious that if you could detect a non-regular distribution of cards, you could gain an advantage, on average, how big is this advantage?

   If there was not a huge structural advantage from going last, the game of blackjack would not exist. This is the secret of gambling: somehow promote the idea that more work, more analysis will overcome the structural deficit.

   It isn't going to happen. This is the huge con. The idea is that you can beat the house. You just need to memorize some tables, memorize some exceptions.

   Maybe you have some additional information, but what I have read is that you have attested to _missing_ money, not actual money.

   I think it is important. Someone who wishes to make money via gambling, and, maybe worse, by organizing a gang of gamblers, just to eek out a 1-2% advantage (at most), who would do this? I mean, we are talking about commodities trading. Only an MIT geek would think this is something worth reporting.

   This is pretty close to the string theory bs. Some moron over-exaggerating, and all his friends being amazed. It is just bs.

8. **John Chang**
   March 29, 2008

   tomj is correct that the house edge arises from the double bust probability, less the 3:2 blackjack payoff to the player. But he seems not to realize that the house has a fixed strategy, whereas the player has the option to double, split, hit, stand, or even surrender. Depending on the exact rules and number of decks, proper play reduces the house edge to a small amount, generally around 1/2%.

   Ed Thorp published the first counting system in 1961. He determined the effect of removing one card of each denomination on the expectation of the game. The sum of the effects is close to linear. It turns out that an excess of small cards are bad for the player, or conversely, an excess of tens and aces helps the player. This makes intuitive sense, since more tens and aces increases the probability of blackjack, which pays 3:2 to the player; it increases the chance the dealer will bust with a small card showing; and it helps the player with his doubles.

   Anyway, if you can determine there is an excess of 10s and Aces, you can also determine when you have a positive expectation bet. Depending on exactly how many cards are dealt, the rules, how much you bet at various "counts" and so forth, your average expectation is generally 1-2% of the money bet.

   As for Scarne, he was no mathematician. He was a great sleight-of-hand magician, but his blackjack writings have long been debunked.

   And as for making money, if your average bet is $1000 a round, you will bet about $1 million in a weekend of play. At an advantage of 1-2%, your expectation would be $10-20K. Of course, winning is by no means guaranteed, but that's why you form a team, to allow the law of large numbers to work. Even going on one trip a month, 10-20K is a nice income for a hobby.

   The method is simple, but the discipline to carry it out is uncommon. As for being a moron, I guess that depends on what company you keep.

   Perhaps the best proof of the profitability of blackjack is that casinos bars counters. And have you ever had 80K in cash to misplace?

9. **Brett**
   March 29, 2008

   First, the notion that there is no winning strategy for counting cards in blackjack is ridiculous. If the deck is rich enough in ten cards and aces, the gave favors the player. If reserved almost all your betting until that happens, you will expect to win. That may not be a realistic or interesting strategy, but it works.

   Also, the attitude of casinos towards card counters varies a great deal by place and, more importantly, how much money is involved. Back in the 1960s, my dad and two other MIT students took a trip to the Bahamas, and they spent an evening counting cards in a casino. They weren't very professional about it. One guy counted the ten cards, my dad counted the total number of cards, and the third guy actually played. They did this quite openly, and the casino didn't care, because they were playing at a $5 table. They won less than $100 total. I've heard similar things from other people about more recent trips to Vegas and Atlantic City. If you're playing for peanuts, (some) casinos really don't care if you count.

10. **stevem**
March 29, 2008

Tomj, everything John says makes perfect sense but I don't get your reasoning. It is mathematically very well established that card counting works, although putting it into into practice is difficult and hard work. It is also intuitively obvious that tracking the fluctuations in the composition of the deck(s), and varying the wagers accordingly, is advantagous to the player. The player's edge comes from the increased probability of blackjack and the ability to perform operations like doubling and splitting that are not available to the dealer/house. It does'nt matter that the player "goes first".

My own experience of blackjack is limited to the online game where the cards are "electronically shuffled" after each hand so card counting is useless. An interesting question therefore–and one that remains unanswered as far as I can tell–is whether an edge could be gained in the computer-generated online version of the game. That is, is the online game perhaps more "pseudo-random" than random? Could one data mine a large statistical sample of computer-generated hands and look for repetitive patterns, trends, predictive signals, correlations–kind of like what Jim Simons does with financial data–and make bets or alter the magnitude of the wager in accordance? For example, could you reasonably predict when the negative fluctuations or variance/volatility is going to hit at which point you would start betting small? It is pretty much an open problem in cryptography as to whether one can effectively distinguish between a large sample of purely random objects and a large sample of pseudo-random objects. Some modern rngs are pretty sophisticated however, but computers still essentially remain deterministic machines and must generate random objects (card hands, dice throws) from a formula or iteration algorithm. However, in the online game successive bets can be laid down very fast and even with a slight edge the law of large numbers would quickly do the rest if one could even get a small edge. The hassle associated with real casinos is avoided and you can play anywhere at anytime. Sorry to rant on but I've never had a response to this query;)

Chris, as a spinoff or prequel to "String Kings", one could perhaps base it on Scorcese's film "Casino": string mobsters set up a casino in Vegas called "The Only Game in Town" (with flashing neon) and featuring rows and rows of the 1-in-10^500-payout slot machines, skimming the profits for the bosses back east in Princeton and Harvard; and with a "bagman" taking the cash back east on regular trips. (Guess who?;)). I can imagine a lot of violence though with cheaters and counters being taken to a back room and getting their hands and heads busted with heavy bound archival copies of Nuclear Physics B.

11. **woit**
March 29, 2008

stevem,

I have no idea how good the random number generators being used are. If you did manage to figure out how to take advantage of their pseudo-randomness, presumably whoever operates them would notice this and change to a different one.

Card counters over the years have put a lot of effort into trying to exploit non-randomness in casino shuffles. I don't know what the current state of this is, although I did hear that at least one casino hired mathematician Persi Diaconis to check out their shuffle.

It's not against the law to do what card-counters do, although casinos (at least in Las Vegas) can kick you out if they think you are doing this too well. Using a hidden computer to play in a Las Vegas casino is now against the law there, and you could end up in jail if you do this. Most card-counters are careful to not do anything illegal, so this is not one of their tactics in Las Vegas.

tomj,

Please stop insulting people. I should have deleted your comment when first posted, will just leave it because of the informative response from John that it generated.

12. **Deane**
March 30, 2008

So how did you like the movie? Lauren and I went on Friday night. The theater was completely full. But we were quite unimpressed by the movie. And its depiction of math was awful. It was fun to see MIT and Cambridge in a movie, though.

13. **Peter Woit**
March 30, 2008

Deane,

I thought the movie was kind of a disappointment. Not that good (especially the performance of the lead actor, who was vapid), and it could have been a lot better. Having so much of it set in Planet Hollywood was both ridiculous and boring, and the movie was too full of product placement and a bit too much of a bland product itself. But it was entertaining enough, and I just read that it's number one at the box office this weekend, so the filmmaker is doing something right, at least as far as making a product with as wide appeal as possible.

Check out John's blog for his take on it, which is interesting.

14. **Anders**
April 3, 2008

stevem,

I had not thought about the psedo randomness of the cards in online Black Jack before, but if you know what generator is being used and if you could find the seed value used the game would become totally deterministic. However as far as I know it is extremely difficult (if at all possible) to determine the seed value from observing the random numbers (and you cannot really observe the numbers only the cards) even if you know what random number generator is being used.

If the casino uses an old generator with a short period then eventually you will get the same sequences of cards again, but a modern generator such as the "Kiss-Monster" repeats itself after 10^8859 numbers so then you will have to play for awhile 🙂

15. **Shantanu**
April 3, 2008

Peter have you seen the talks at Lindefest (which has talks on string theory and anthropics)

16. **woit**
April 4, 2008

Thanks Shantanu,

I hadn't seen that. Especially glad to see the notes from Witten's lecture, which was the same one I saw at Stony Brook last week and hope to get a chance to write about.

17. **Shantanu**
April 4, 2008

No problem, Peter. Also for some reason my post about Coleman's 1975 lectures
and a very interesting paper by Carlip is no longer there. I was hoping you would
blog about it (since both of these are related indirectly to string theory)
Did you take the same class by Coleman at Harvard?
Thanks

18. **Peter Woit**
April 4, 2008

Shantanu,

As the comment policy says "off-topic comments better be interesting…" otherwise I'll delete them. I wasn't much interested in the Carlip paper, just because my interest in that kind of debate about quantizing gravity is limited. There are lots of things worth reading on the arXiv that aren't discussed here.

As for the Coleman lectures, this was mentioned on several other blogs already and I think I've written about Coleman and his QFT course here in the past. I actually attended two quantum field theory courses at Harvard, and during the first, which wasn't taught by Coleman, I watched some of those lectures on videotape in the science center. The second time around I took the course from him. He was a great teacher and it was a great course. But trying to follow a course of blackboard lectures by watching low resolution video on the internet seems to me to be a painful idea.

19. **Chris Oakley**
April 6, 2008

I just got home after seeing "21".

My idea (doing the movie as a prequel to "String Kings") is better.

In particular, I'd like to have a scene where the young String Kings abduct a security guard from an Atlantic City casino who has previously thrown them out for card counting, dragging him to basement and tying him to a chair.

THE KID

How many space-time dimensions are there, punk?

SECURITY GUARD

Three space, one time.

THE KID (punching him in the face)

Wrong, totally moronic crackpot! The answer is TEN space, one time! Let's try again, shall we? How many spacetime dimensions are there?

SECURITY GUARD

Three space … one time.

THE PLUMBER (A string theorist)

WRONG! Get this wrong one more time and you're going to go glub, glub, glub to the bottom of the sea.

Etc.

20. **Steve Caudill**
April 16, 2008

On the topic of gambling and card counting in the movies, I was reminded of "Rain Man," where Tom Cruise's character takes Rain Man to a casino and has him count the cards at blackjack.
There was a famous poker champion named Stu Unger, who had a total-recall memory and could memorize every card in a 6 deck blackjack shoe, yet Unger's game was poker and he supposedly won over $30 million in poker tournaments. For more info see:
http://en.wikipedia.org/wiki/Stu_Ungar

Another excellent gambling movie is "Rounders," starring Matt Damon as a poker player.
As for me, I like all of the casino games, and the only counting I do is my money, Rule number 1 of my 'winning formula' is to quit after winning 10 percent of what I came in with; Rule number 2 is don't bring an ATM card; Rule number 3 (of 3 Rules) is don't be a gambling addict.

21. **Bill**
May 20, 2008

Just a quick one to throw into the pot, which involves adding a few variables other than the numbers themselves into the equation for beating house edge

To really beat house edge, you have to think outside the box. That is, you have to think of gaining benefits other than the montry win itself from your play. This means factoring in comps

The easiest way to do this is to play slots and the first thing to do is find a slot that pays 99% or over. Slot play now attracts the best perks in casinos. Better still, combine this with entering a slot tournament where you can get free rooms in hotels when you enter and will often get a free lunch too. In doing this you automatically have an edge worth hundreds of dollars. Now, when playing the machine, you must use your points card in the machine. This will monitor your play and you will then begin to acquire morefreebies such as complimentary meals, rooms, shows, limos etc

Of course, the edge you get is only of value if you actually desire these freebies and you will never get rich on them…

Similar edges can be gained on blackjack. If you are good enough at blackjack and can get the floor house edge down to less than 1%, that is a good start. You must then get your play ‘rated’ by the floor manager. He can then pass that info along to the casino system and you can then accumulate comps in the same way that will be greater than the 1% you have been losing on blackjack over time.

Krauss on Boltzmann Brains

March 27, 2008
Categories: Multiverse Mania

Lawrence Krauss has a piece this week in New Scientist about the latest hot topic in theoretical physics, Boltzmann brains. It's entitled String Theory's Latest Folly, and starts off:

> THOMAS AQUINAS may never have actually wondered how many angels can dance on the head of a pin, but his tortured musings about metaphysical issues associated with the non-corporeality of angels (and the related issue of whether there is excrement in heaven) stretched the limits of reasonable rational inquiry so far that later scholars invented the phrase to mock him.
>
> My thoughts turned to Aquinas last week as I sat through a lengthy seminar on the subject of Boltzmann brains. The speaker decided his ruminations were so important that he needed 90 minutes rather than the customary hour. To my surprise, many in the room seemed to agree with him.

He goes on to explain what this is all about:

> The problem is that statistical arguments suggest that in long-lived universes, far more Boltzmann-brain consciousnesses will develop than intelligences like our own, which have evolved over billions of years. That would mean we are far from typical, so anthropic explanations of our universe fall by the wayside.
>
> Some theorists have therefore tried to develop constraints that would force all inflating universes like our own to decay well before Boltzmann brains can infect them. The bad news here is that in this case our universe must be unstable, and heading for a catastrophic end. But at least anthropic arguments from string theory would not be undermined. You can decide for yourself which you would prefer.

and to conclude:

> If debating angels dancing on pins marked the intellectual low point of medieval theology, then we may similarly question the merits of debating problems that require hand-waving arguments involving unknown quantities that differ by billions and billions of orders of magnitude. Let's focus on other issues, at least until better theories come along.

Update: At Lubos Motl's blog, there's a comment from Krauss noting that the title was chosen by an editor, not by him, and that he agreed that it was misleading, since the piece was not specifically about string theory.

Comments

1. **Tim R. Mortiss**
   March 28, 2008

   From the article: "If debating angels dancing on pins marked the intellectual low point of medieval theology".

   I thought that was a later invention, used to smear scholasticism. If not, I would actually like to read that discussion!

2. **M**
   March 28, 2008

   maybe this "String Theory's Latest Folly" can be interpreted as evidence for the prediction of String Anthropic Cosmology: Boltzmann brains are among us, and they will dominate the Universe

3. **DB**
   March 28, 2008

   Have you read Krauss' latest book? It looks like he's taking a leaf out of Smolin and yours:

   http://www.slate.com/id/2131014/

4. **fh**
   March 28, 2008

   I thought the idea of the debate was that a) if a theory predicts Boltzmann Brains we are misapplying it and its wrong (that is its origin, right? it meant that Boltzmanns idea about the universe as a statistical fluctuation was wrong because it predicted Boltzmann Brains) and b) it's actually a subtle question that teaches us something about the proper use of statistics to investigate whether it does.

5. **Xerxes**
   March 28, 2008

   While I think contemplation of Boltzmann Brains is probably philosophy and not physics, I really don't see what is so bad about thinking about them or (to flip the argument around) what is to be gained by purposefully not thinking about them. The argument starts from assumptions that virtually every physicist would agree with and uses a pretty straightforward argument to come to a paradox. Clearly, there's something that we don't understand here, and resolving it might teach us something interesting. I'd consider it at least as interesting as, say, the Fermi paradox. Perhaps someday, we'll consider it more like the Olbers paradox, where the resolution follows from a critical property of the universe.

6. **oohay**
   March 28, 2008

   [text of comment anonymously attacking Krauss deleted. Please don't do this here, take it to one of the blogs that encourages anonymous attacks on others.]

7. **Moshe**
   March 28, 2008

   Funny piece, full of rhetorical flourishes, as usual with Krauss. The timing is puzzling though, Krauss must have been sitting in this kind of talks, given by his fellow cosmologists, for about 25 years. Why complain now?

Oh, just saw the title, got it.

8. **Yatima**
March 28, 2008

It's Friday afternoon:

[Botanist sues to stop CERN hurling Earth into parallel universe](http://)
A colourful American botanist, teacher, former biologist and sometime physicist says (in outline) that the LHC may rip a hole in the fabric of the space-time continuum and so destroy the Earth. He wants the US government to act now and delay the LHC's startup while a new safety review is carried out.

These 14 TeV are gonna burn, man.

9. **Coin**
March 28, 2008

A little confused here since as far as I'm aware Boltzmann Brains aren't an originally or specifically String-Theory-related idea.

Since I can't read the article itself, I just want to be sure: Would I be correct in interpreting that Krauss is *not* saying that Boltzmann Brains are "String Theory's Latest Folly", but rather that the "folly" is that string theorists have backed themselves into a corner via anthropic arguments such that they now feel compelled to seriously attempt to refute the Boltzmann Brains idea for fear it would pose a threat to string ideas if allowed to stand?

10. **Peter Woit**
March 28, 2008

Moshe and Coin,

I think that Krauss is just properly laying at string theory's door the phenomenon of serious physicists giving research talks devoted to anthropic computations of numbers of universes. Before the anthropic string theory landscape nonsense started up a few years ago, despite what Moshe seems to think, this wasn't the kind of thing cosmologists gave talks about, at least not if they hoped their talk not to be drowned out by snickering from the audience. Now instead this kind of thing is covered in the New York Times and many talks about it are scheduled, given, and, amazingly enough, taken seriously.

11. **Moshe**
March 28, 2008

Oh, there were plenty of talks counting universes, comparing probabilities, talking about the proper measure etc. etc., I sat through some of them as a grad student in the mid-1990s, and it was already a mature subject by then. There is also a paper trail- for an entry point to this vast literature just look at the early papers of any of the main actors in this research area (quite a distinguished list of cosmologists). They start around the early 1980s, when people realized there are new and fascinating issues eternal inflation forces upon us. Even if Krauss gets his "better theories", those would have to deal with precisely the same issues.

12. **Greg Egan**
March 28, 2008

I'm largely persuaded by the paper by Hartle and Srednicki, Are We Typical?, which concludes (roughly speaking) that we have no grounds for discriminating between cosmological theories on the basis of the *proportion* of conscious entities in the history of spacetime that human beings happen to comprise.

We really can't choose values for the cosmological constant, etc., on the basis that a "bad" choice would allow 10^10^10 Boltzmann brains to come into existence at some time in the future, thereby making us atypical observers. If we're atypical, we're atypical; Copernicus won't really spin in his grave, and it won't be the end of science. Nobody had to pull us out of a barrel containing all observers before we were allowed to make an observation.

13. **Zathras**
March 29, 2008

OT, but this article blew my mind in terms of the journalist's sensationalism. It deals with a lawsuit to close the LHC because it will create black holes that will devour everything. The journalist made it look at least very possible

http://www.nytimes.com/2008/03/29/science/29collider.html?_r=1&hp&oref=slogin

14. **Peter Woit**
March 29, 2008

Moshe,

Sure, eternal inflation and discussion of what its significance is have been around for a while, including a certain level of anthropic nonsense. As far as I remember though, the anthropic nonsense only started getting widespread and infecting other areas of physics with Susskind et.al. The attention being paid to the Boltzmann brain silliness is definitely a recent development.

Xerxes and fh,

Despite what you might read on certain well-known blogs, the notion that one can sensibly assign probabilities to different universes and then use this to make scientific predictions is not one shared by most theorists. This is exactly what Krauss is objecting to.

15. **Moshe**
March 29, 2008

Peter, as a matter of personal choice of research direction, I'd probably agree with everything Krauss says in the piece, including his summary – I don't think we have enough knowledge of quantum gravity at the moment to ask the questions raised by eternal inflation, and attempts to proceed anyhow result in low signal to noise ratio. One interesting line of research is then to develop the tools needed, and lots of smart people are thinking about it, not likely to make it to the New Scientist pages anytime soon.

However, Krauss' choice of venue for this message, and timing, and emphasis, and tone, are all in things I disagree with. Oh well…

16. **Randal**
April 5, 2008

If it’s your time and your mind, I guess you can spend it thinking about anything you want, but I think Krauss is essentially right: this is unlikely to prove a fruitful line of reasoning – much like Acquinas’ pin head. Future generations will likely view them similarly. Speculation makes better press than developing real understanding because it’s more fun.

I understood the “latest folly” of the string theorists to be introducing the notion of universe decay as a constraint on the propagation of brains throughout their models; they sacrifice the stability of our universe to save their theories from this absurdity. (Some here don’t seem to like the tone, but I thought this was funny.)

LHC Startup at 10 TeV

March 29, 2008
Categories: Experimental HEP News

Robert Aymar, the Director General of CERN, has announced that the LHC will operate when it starts up this year at an energy of 5 TeV per beam (10 TeV total center of mass energy), rather than the design energy of 7 TeV per beam. To operate the LHC magnets at the highest current and get to 7 TeV requires a time-consuming sequence of powering tests and quenches, so the decision was made to put this off until the winter shutdown. With this decision, the process of beam commissioning can start soon after all sectors have been cooled down, and this is now scheduled for mid-June. Beam commissioning should take two months, with first physics collisions thus scheduled for late summer.

It remains possible that problems will be found during or after cooldown that will require warming back up one or more sectors, and this would lead to a delay of a couple months or so. The last sector scheduled to be cooled down is 4-5, which is now warm to allow repair of the defective triplet magnets. Whenever a sector is warmed up, a major problem is damage to defective PIMs which then need to be replaced. If there are too many of these, a delay in the cooldown is possible. The search for damaged PIMs relies on a “sputnik” tennis-ball-like sensor sent through the beam-pipe. Latest news is that 4 damaged PIMS have been found so far.

Update: I’d been wondering how much extra work this change in energy would cause for the experimentalists, just saw a posting about this by Gordon Watts, entitled Start Your Monte-Carlo Engines!

Comments

1. **D R Lunsford**
   March 29, 2008

   Whoa there. This is a matter for the courts, Peter.

   http://www.nytimes.com/2008/03/29/science/29collider.html

   The laws of physics. PN injunction!

   -drl

2. **Peter Woit**
   March 29, 2008

   I did see the NYT story about the lawsuit this morning, and half a dozen blogs already have postings about it. As far as I can tell, no court is going to take this seriously, so it’s unclear why this story deserves any attention, much less ending up on the front page of the Times.

3. **Domenic**
   March 29, 2008

   Hey Peter,

   As an undergrad, this is the first time in my life I’ve watched a new particle accelerator go through the birthing process. My impression is that the LHC is kinda sucking at it—lots of delays, problems, subpar results, etc. But, I have no basis for comparison, so I thought I’d ask someone who’s been around long enough to know: is this typical?

   Thanks :).

4. **Peter Woit**
   March 29, 2008

   Domenic,

   I don’t think the LHC problems and delays have been at all unusual for a project of this magnitude. For a comparison you’d have to go back to the early 80s (Tevatron) or late 80s (LEP), to see how those projects went as they approached beam commissioning. But the LHC is a more ambitious endeavor than either of those, and it is receiving a lot more attention.

   The crucial period will probably be this summer and fall when they start trying to store a beam in the machine and get a useful luminosity. If this goes smoothly, the thing will be quite a big technical success, even though it would end up being a couple years behind early optimistic schedules.

5. **Domenic**
   March 29, 2008

   Thanks Peter, I thought that might be the case. Well then, here’s hoping! It’d be pretty amazing to go into grad school (2.5 years from now) with some LHC data to guide me.

6. **milkshake**
   March 29, 2008

   About that lawsuit: Here is a photo of a black drainhole at Monticello. (Coming soon to Lake Geneva):

   http://pixdaus.com/single.php?id=30364

7. **wb**
   March 30, 2008

   Peter,

   Not to find fault with the LHC progress but rather to give some additional perspective, I would point out that the PEP-II and KEK-B B-factories were commissioned extremely rapidly. Rapid commissioning is also commonplace these days with synchrotron light sources. RHIC also went fairly smoothly. Regarding LHC one must keep in mind that this is by far the most difficult and largest collider ever built. Moreover it has a enormous stored energy in the beam, the magnets and the fluid systems.

While we all would have liked to see collisions earlier, the LHC team must be applauded for their steady and systematic progress. Likewise the decision to begin collisions at 10 TeV is a prudent one. There is great physics awaiting us.

As for the law suit… the less said the better.

8. **YBM**
   March 30, 2008

   Dear Peter,

   this is related only by Motl adding this postscript to a post of him on the LHC :

   > P.S.: Let me say something about the titles. Of course, I do know that the title is routinely chosen by the editor or the publisher. That was the case of "The Bogdanov Equation", too. On the other hand, I am convinced that the author always has to approve it. Although it was a bit of a shock to see the cover of the book for the first time, I of course fully confess that I approved it after a little thought accompanied by mixed feelings even though this title is arguably not the most accurate description of the content of the book. Whether someone including myself likes it or not, I am responsible for the title. In the very same way, I am convinced that the titles such as "The Trouble With Physics" and "String Theory's Latest Folly" were approved by the authors, too. Trying to get rid of the responsibility – whenever the title becomes inconvenient – is an unfair game.

   Do you know of any reason or event making Lubos to regret his french book ? Apart of him getting miracously some kind of intellectual honesty (which I doubt)…

9. **Peter Woit**
   March 30, 2008

   YBM,

   That comment of Lubos's was in response to Krauss writing in to say that he was not responsible for the headline put on his article in New Scientist. Lubos seems not to understand the difference between a book (where the author definitely has a say about the title) and a magazine article (where the author doesn't). The way he writes about this, one gets the impression that the title was chosen by someone else. It's definitely a curious book, quite possibly there were other hands at work creating the thing than just its official author….

10. **Harry**
    April 1, 2008

    Anyway (and off-topic, sorry),

    "the Bogdanov Equation" is a phenomenal success in France : it just took the record of the best-selling book for its first month (this record was previously hold by "Harry Potter and the Deathly Hallows")

    Unbelievable !

11. **Hans**
    April 1, 2008

    @ Harry

    according to which source?
    The book doesent even figure in the FNAC top 100 and holds place 19 on the FNAC science and culture list. On amazon.fr it has the rank 2,059 (thought #1 in Physics). Sure, this is not bad, but not exactly Harry Potter. ..

12. **Peter Woit**
    April 1, 2008

    Hans,

    I had assumed that Harry's comment was an attempt at April 1 humor.

    Actually, if I had first heard of the Lubos-Bogdanov book on April 1, I would have been 100% convinced the whole idea was an April Fool's joke.

13. **Harry**
    April 2, 2008

    Yes, it was indeed an attempt of April Fool's joke 🙂
    (Sorry, I could not resist.)

14. **Hans**
    April 2, 2008

    Ok, I got fooled!
    And i actually spent 10 minutes checking the sources!

15. **piscator**
    April 3, 2008

    hey peter,

    back to the topic of the post: any idea what is happening with the LHC? In the last day and a half or so all the sectors that were previously being cooled down now all seem to be being warmed up…..

    piscator

16. **Peter Woit**
    April 3, 2008

piscator,

Maybe this has something to do with “Open Days”. On the 5th and 6th they’ll be opening the lab up for visitors, may be that the cooldown is being stopped during that period.

I haven’t heard of any problems, and don’t see anything unusual at their web-site. They finished warming back up one of the sectors (4-5) a while ago, now are looking there for defective PIMS, have found 9. Supposedly more than 24 will be a problem. They just started cooling down 6-7, three other sectors are being cooled down now. Maybe they’re stopping this for a few days…

Short Items

April 4, 2008
Categories: Uncategorized

The Tevatron has been performing well, producing record-high weekly luminosities. Fermilab director Oddone has announced that plans to shut-down the accelerator complex for yearly maintenance work are being canceled this year, instead the plan is to run the Tevatron straight through the year, with a shutdown not until spring 2009. The current proposal to DOE is to run through FY 2010 (i.e. until September 2010), by which time the expectation is that integrated luminosity would be more than double the current value of nearly 4 fb^{-1}.

The budget situation for US and British HEP continues to look rather grim. Durbin and several other senators sent a letter to the Senate Appropriations Committee requesting that supplemental funds for FY2008 be found to stop lay-offs at Fermilab. Senators Clinton and Obama did not sign the letter, which seems especially remarkable in the case of Obama, since Fermilab is in the state he represents. For the latest in the sad story of UK physics funding cuts, see here.

As far as FY2009 goes, Congress is on more or less exactly the same path as last year. Attempts to rein in earmarking were defeated, and committee hearings, including those on the science budget, show no signs of interest in making any tough decisions (to cut military/Iraq war spending, domestic spending, raise revenues) now. See some commentary here. Presumably decisions will ultimately get made by the same mysterious staffers in the same mysterious way as last year. This year the betting is that there actually won't be a budget until deep into the fiscal year, with Congress waiting for a new president rather than negotiate with Bush. This will leave US science programs operating under a continuing resolution, financed at FY 2008 levels into FY 2009.

The P5 committee is meeting in Washington today to put together recommendations for how US HEP should proceed, under various possible funding scenarios over the next few years. For more about their deliberations, see their web-site here. The last public meeting of the group was at Brookhaven, talks there are available here.

Some other recent or upcoming conferences include one at ICTP, one at Warwick on TQFT and string theory (see Marcos Marino's notes here), and the Linde-fest at Stanford.

Talks at the Linde-fest included many serious and informative ones about current cosmology research, together with a large helping of Multiverse madness, since Stanford is ground-zero for this phenomenon. The string cosmology talks were mostly devoted to showing that some string compactification or other can reproduce any conceivable experimental result. Several speakers discussed Boltzmann brains, perhaps one of them was the one who so annoyed Lawrence Krauss recently. Max Tegmark promoted future measurements of the 21cm hydrogen line as being very promising for cosmology, the same point was made here by Scott Dodelson. Lance Dixon gave a talk on the finiteness of N=8 supergravity. He describes conversations with string theorists who would like to interpret this result as indicating that string theory would still be necessary to deal with the asymptotic nature of the perturbation series (not clear why the same problem in string theory doesn't bother them). One problem with this is that string theory doesn't have the same symmetries as N=8 supergravity. Witten gave a talk on his recent work, much the same as one he gave at Stony Brook last week. I hope to write about that in my next posting.

See here for an interesting talk at the KITP in Santa Barbara from Albert de Roeck about what to expect from the early stages of the physics run at the LHC.

Michio Kaku's new book The Physics of the Impossible is getting some attention, especially in the UK. A Fox News story headed Physicist Says Time Travel Is Not Only Possible, but Likely claims that:

> … in Blighty , Kaku's being treated as if he's Doctor Who informing dim-witted humans about the wonders of the Universe, with front-page treatment Wednesday in both the Daily Telegraph and the Guardian. Even the normally staid Economist is chiming in.

while, in the US:

> … outlandish claims in books are recognized as, well, a good way to sell books.

Here in New York, my colleague Brian Greene's World Science Festival is getting off the ground, with a press conference held this week described here.

Sabine Hossenfelder, Michael Nielsen and Lee Smolin are organizing a conference at Perimeter this semester on Science in the 21st Century.

Comments

1. **Bee**
   April 4, 2008

   Thanks for mentioning our conference!

2. **Flip**
   April 4, 2008

   Thanks for another set of great updates.

3. **Lloyd**
   April 4, 2008

   Hi Peter,

   I recall some time ago you announced that you are about to write down some of your original research in a research paper. If I remember correctly the topic was representation theory and quantum field theory or some such.

   How is that going?

   Best wishes,
   Lloyd

4. **Peter Woit**
   April 4, 2008

   Lloyd,

I'm working on some new ideas (involving representation theory) about the mathematics behind the BRST formalism and how to handle gauge symmetries. I have a partially written paper now, hope to get something finished and done this summer. I probably should be spending less time on the blog….

5. **Steve Caudill**
   April 4, 2008

   I'd like to nominate Peter Woit as the initiator of the "Third Superstring Revolution"!!!!

   The Third Superstring Revolution will brand as a 'NON-SCIENCE' string theory, superstring theory, brane theory, M-theory, (or whatever alias the subject goes by.) As a result, American academia, the U.S. government, and publishers of popular science physics books (as well as New Age-related books) will see the need to provide equal support to all valid theories which attempt to find a theory of quantum gravity or a theory of 'Everything'.

   Well, thanks in part to Woit's book, this month instead of buying a book on superstrings from the Scientific American Book Club, I think I'll order "Discarded Science: Ideas That Seemed Good at the Time," by John Grant.

6. **Peter Woit**
   April 4, 2008

   Steve,

   Thanks, but I think the "Third Superstring Revolution" has been the anthropic landscape/multiverse, as popularized by Susskind in his book, and promoted by a growing number of theorists. This is what is behind the increasingly widely held opinion that string theory has degenerated into non-science. Compared to this, the role played by my book was relatively minor.

7. **Copy editor**
   April 4, 2008

   Typo in link to Tegmark talk slides: try this.

8. **milkshake**
   April 5, 2008

   You know Big Sur is a very scenical place and not too far from Stanford – maybe they could organize a joint Multiverse workshop at the Esalen Institute…

9. **Peter Woit**
   April 5, 2008

   Thanks Copy Editor, fixed.

10. **I say**
    April 5, 2008

    There's always a Multiverse workshop happening somewhere. Perhaps even infinitely many. So they say.

11. **Chris Oakley**
    April 5, 2008

    Susskind proselytising the Anthropic Landscape may not in itself demonstrate that the subject has gone into a tailspin, but the fact that the media and conference organisers give him a platform does.

    "I say",

    You don't know that. It could be that in the other Multiverses they speculate about the possibility of their own universe being the only one.

12. **anon.**
    April 6, 2008

    Chris, the multiverse is the most impressive prediction of string theory. What's interesting about multiverse 'physics' is the way it debunks Mach's insistence that physics should deal with what we can potentially observe. The multiverse is the exact opposite of Occam's razor, the principle of economy in speculations. It's a great advance because it makes physics interesting to those greats who would otherwise devote their lives to philosophy.

13. **DB**
    April 7, 2008

    Obama's silence is in no way surprising and is quite characteristic. He is no supporter of large government sponsored research. For example, he plans to "delay" NASA's constellation program in order to fund an early education plan. The implications are discussed further here:

    http://www.thespacereview.com/article/1100/1

    I would predict that with the support available from the likes of Obama, Fermilab's future as the last major outpost of US HEP research is bleak.

14. **chris**
    April 7, 2008

    about obaba and hep funding: i think the us hep community put itself into a political position. essentially, the hep budget is treated like an appendix to the huge defense budget and for better or worse, it is now linked to it politically. that was a relatively easy game under neocon political leadership, but it will bite certainly be very bad if the political course changes to deemphasize military expenses. kind of like a pact with the devil..

15. **Shantanu**
    April 8, 2008

    Peter, the video of David Gross' talk at the IPMU is now online and there was some discussion of anthropic principle and Boltzmann brains
    here

16. **Peter Woit**
April 8, 2008

Thanks Shantanu,

It seems that Gross and I may have some disagreements about string theory, but we agree about "Boltzmann Brain" papers (which he characterizes as "totally preposterous", saying that people work on this "to my regret"). Maybe he can have a talk with Sean Carroll and convince him to stop promoting this stuff…

17. **Hendrik**
April 9, 2008

Michio Kaku's book just had a scathing review at
http://newhumanist.org.uk/1747

18. **nigel cook**
April 10, 2008

That's a worthless book review because it doesn't address the main issue. It's like an *ad hominem* attack, trying to debunk one thing by attacking something else. The alleged trivial errors aren't necessarily a sign that the big arguments are wrong, and I think the reviewer is incompetent to review the book because he ignores the main arguments altogether.

It's simply not good enough to spot a few trivial errors in a book and then try to discredit the main message as suspect. The amount of polishing and error checking of a book written by a very busy author is mainly down to the publisher's editor, not the author. I read a couple of earlier popular books by Professor Kaku, and found them to be well written. The fact that he writes a lot about non-speculative (non-predictive) stuff like string theory is the reason why he is so popular. Fiction outsells fact, and you can't debunk it. If fiction sells, someone will write and publish it.

19. **Shantanu**
April 13, 2008

Peter probably you have seen , but see the talk by Roberto trotta
at PI (last year) arguing against anthropic pinciple.
here
Towards the end of talk (after about 1:00 hr) there is interesting discussion between Moffat and Susskind (I think at least from the voice)

Stony Brook Dialogues in Mathematics and Physics

April 5, 2008
Categories: Uncategorized

Last week I spent a day out at Stony Brook, attending the second day of a two-day symposium devoted to mathematics and physics, held in honor of C. N. Yang and Jim Simons. Peter Steinberg was there for the first day, and has a report about this on his blog. The symposium was in many ways also a celebration of the new Simons Center for Geometry and Physics, which is just getting off the ground with a $60 million donation from Simons. A new building will be constructed over the next couple years, and already one permanent member (Michael Douglas, who will not be the Director, as mistakenly reported in some press accounts and here) has been hired. In an era when string theory has caused a backlash against mathematical and formal research at many physics departments, the Simons Center may be one of very few places where a physicist working at the boundary of mathematics and physics will be able to find employment. To get some idea of how dramatic the situation is this year, with only "phenomenologists" and "cosmologists" getting hired into tenure-track positions, take a look at the Theoretical Particle Physics Rumor Mill.

Stony Brook played a very important role in the interaction of mathematicians and physicists around the topic of gauge theory, and many of the speakers at the symposium discussed this. Since his early work on Yang-Mills, Yang had been intrigued by the similarities between gauge theory and the Riemannian geometry of GR. He built up the ITP at Stony Brook, in the same building and at the same time as a great mathematics department focused on geometry was being built up by Simons. He discussed these similarities with Simons, who told him that gauge theory must be related to connections on fiber bundles and pointed him to *Steenrod's The Topology of Fibre Bundles*. Yang didn't get much out of that (not surprising, since Steenrod is purely topological, with nothing about connections and curvature), leading him to make the statement:

> There exist only two kinds of modern mathematics books: ones which you cannot read beyond the first page and ones which you cannot read beyond the first sentence.

In early 1975 Simons gave a series of lectures to the physicists on differential forms, geometry and bundles, and some real communication between the two camps began. This led to Yang writing a paper that year with Wu, *Concept of Nonintegrable Phase Factors and Global Formulation of Gauge Fields*, Phys. Rev. D12, 3845, that included the famous "Wu-Yang dictionary" explaining how to translate back and forth between mathematician's and physicist's language. The crucial example was the Dirac monopole, where the bundle (for a sphere enclosing the monopole) is what a mathematician would call the Hopf fibration. This was already becoming a hot topic among physicists, with the 't Hooft-Polyakov monopole having been discovered in 1974.

Is Singer visited Stony Brook in the summer of 1976 and talked to the physicists about gauge theories and geometry. In early 1977 he traveled to Oxford, where he, Atiyah and Hitchin began working on instantons, i.e. solutions of the self-dual Yang-Mills equations. Again the physicists had started this, with the discovery of the BPST (Belavin, Schwarz, Polyakov and Tyupkin) solution in 1975, followed by its use by 't Hooft, Polyakov, Jackiw, Rebbi, Callan, Dashen, Gross and others soon thereafter. In his 1977 Erice lectures on The Uses of Instantons, Sidney Coleman refers to the "classic part of the theory":

> "Classic", in this context, means work done more than six months ago.

Atiyah and collaborators were to devote much of the next decade to work on gauge theories. In 1977 he also met Witten, and this began a long and fruitful collaboration. During the years after 1977 Witten would become by far the dominant figure in the subject.

At Stony Brook last Friday, I arrived just in time to catch a morning talk by Dennis Sullivan about the classification of 3-manifolds (he was speaking in place of Iz Singer, who wasn't able to come due to a respiratory infection, but an e-mail from him was read to the audience). Yang made some short comments about a problem in condensed-matter physics. The afternoon featured three hour-long talks. The first, by Dijkgraaf on *The Unreasonable Effectiveness of Physics in Mathematics* was a talk for a general audience advertising some of the high points of how ideas originating in string theory have had influence in mathematics. The main example was the computation of the number of rational curves on a quintic. The talk was extremely polished, featuring very impressive graphics. He described the current situation of string theorists as being very enthusiastic about "emergent geometry", but struggling for the right mathematical language to express these ideas. This is an interesting research program, but as far as I can tell it is one that has to some extent ground to a halt, with little progress in recent years, and Dijkgraaf's talk being essentially the same one he has given on other occasions during the last 5-6 years.

Michael Douglas, who is joining Stony Brook next year as the first permanent member of the Simons Center, spoke on *Physics and Geometry: past, present and future*. He emphasized ideas about "branes" and non-commutative geometry that have been popular since the late 1990s, like Dijkgraaf giving a take on the question of what new sort of geometry string theory might be pointing to. He ended his talk with a sentiment that I would heartily agree with, that he thought the time had come for a deeper investigation into quantum field theory. Unfortunately it was in a context I find not so promising: he has been thinking about how to count quantum field theories as part of anthropic landscape research, and has realized that this is a pretty ill-defined question. His final remarks seemed to be designed to answer skeptics who have noticed that string theory has stopped making progress, noting that physicists like himself are always moving on to something new, and this something new might soon not be string theory. In answer to a question from the audience about what the LHC might tell us about string theory, he gave a defensive set of remarks about the testability of string theory.

The last talk of the symposium was Witten on the topic of *Electric-magnetic duality on a half-space*, and this was a breath of fresh air and an extremely impressive performance. He was discussing joint work with Davide Gaiotto that I wrote a bit about here, based on his recent series of lectures at the IAS, which you can follow in lecture notes from David Ben-Zvi and Sergei Gukov here. The Stony Brook talk was an extended version of one he gave recently at the Linde-fest, available here.

The talk began with a motivational example from d=2, with a 1-d boundary, of a duality in the QFT of real scalars, taking Dirichlet boundary conditions on one side of the duality to Neumann boundary conditions on the other. The next example was 4d U(1) gauge theory, with its electro-magnetic duality, again relating by duality Dirichlet boundary conditions and Neumann boundary conditions for the field-strength F at the boundary. Most of the talk was about his new work on the surprising ways in which duality is reflected in choices of boundary conditions in N=4 super Yang-Mills. He claimed this to be a physicist's way of understanding geometric Langlands and its duality between D-modules and coherent sheaves, but ended after an hour without having much to say about these mathematical implications, (although he jokingly threatened to go on for another hour on this topic if people were willing to stay).

In response to a question, he noted that unfortunately there seemed to be no useful relation between this S-duality and the AdS/CFT duality of the theory that is the reason for its central importance in modern string theory and particle theory.

Witten's talk ended the symposium on a high note. This summer he'll be temporarily moving to CERN as a visitor for the next academic year, so he may be on-site there as, if all goes well, the first results come in from experiments at the LHC.

Comments

1. **nbutsomebody**
   April 6, 2008

   Peter,
   Thanks for pointing out that string theorists are not getting faculty jobs. I think it is almost impossible to judge between important work and junk in string theory, as there

is no objective criterion (no experiment) of doing so. Hence it is not easy to decide who should be offered a job and only choice left is to not offer jobs to string theorists. I guess it is wise too.

2. **Brett**
   April 7, 2008

   Gian-Carlo Rota, a mathematician himself, described algebraic topology books thusly, "There are two kinds of books on algebraic topology: those that end with the Klein bottle and those that are written in the form of a personal letter to Norman Steenrod."

3. **Thomas Love**
   April 7, 2008

   Peter, You quoted Yang:
   "There exist only two kinds of modern mathematics books: ones which you cannot read beyond the first page and ones which you cannot read beyond the first sentence."

   What is the source of this quote?

   IMHO, the statement is a synopsis of the problems with modern physics. The physicists really don't understand the math they are trying to use.

4. **Peter Woit**
   April 7, 2008

   nbutsomebody,

   I think the hiring situation in physics departments is disturbing, with my concern not about string theorists losing out to non-string theorists, but about "formal theory" losing out to "phenomenology". The reason string-based unification failed was not that it was insufficiently concerned about connection to experiment, but that it was a wrong idea. Unfortunately string theorists have been unwilling to admit this, and many of them have tried to turn themselves in "string phenomenologists", claiming often bogus connections to LHC physics or cosmology.

   My own view is that there is still a huge amount we don't understand about quantum field theory itself, that new tools and deeper understanding are needed to make any real progress, and that one way to get to this is through better understanding of the relation of QFT to important ideas in mathematics. Quite a few string theorists in the past have actually been working on this, or on related issues, but research in this direction is now being killed off in physics departments, all in the name of a rush to do often bogus "phenomenological" work. This is a real shame, leaving the only hope for progress in this area increasingly in mathematics departments, but these are typically very ill-equipped to train people or support this kind of research, emphasizing only certain areas that have to do with traditionally popular areas of math research.

5. **Peter Woit**
   April 7, 2008

   Thomas,

   A couple people told me that Yang had repeated this in his remarks on the first day of the conference, which I missed. He also talks about this in his interview in the Mathematical Intelligencer (volume 15, no. 4 Fall 1993, 13-21).

   I don't think it's surprising that physicists have trouble with Steenrod and similar algebraic topology books. Both the language and issues these books are concerned with are quite different than what physicists care about. One excellent book about topology that is more appropriate for physicists is Bott and Tu. These days there are many more readable modern math textbooks than there used to be, mathematicians have moved away from the Bourbaki style of exposition (which was never intended to be a way to teach people anything). But, a lot remains to be done in terms of writing expositions of modern mathematics that can be read by even mathematicians in other fields, much less physicists.

6. **Thomas Love**
   April 7, 2008

   Thanks for the reference Peter. According to Yang, his joke goes back to 1983. From the Mathematical Intelligencer interview:

   Yang: I can tell you a relevant story. About 10 years ago, I gave a talk on physics in Seoul, South Korea. I joked, "There exist only two kinds of modern mathematics books: one which you cannot read beyond the first page and one which you cannot read beyond the first sentence." The Mathematical Intelligencer later reprinted this joke of mine. But I suspect many mathematicians themselves agree with me.

   It has happened to me, I once read an sentence in a math book which I didn't understand. I had to read another book to get the background to read the background to continue.

7. **nbutsomedy**
   April 7, 2008

   Peter said,
   "My own view is that there is still a huge amount we don't understand about quantum field theory itself, that new tools and deeper understanding are needed to make any real progress, and that one way to get to this is through better understanding of the relation of QFT to important ideas in mathematics."

   Your views may be true but I think a lot of theoretical physicists will disagree with such a view. I personally partially agree with what you are saying, but it should also be remembered that too much formalism makes main questions obscure and people have a tendency to do a lot of fancy stuffs (but solvable) without much real progress. In such a point thinks become exactly like string theory. The questions become community driven and which is important becomes completely a question of who thinks it is important. In short objectivity is completely lost.

   Frankly speaking how much the so called "important areas in mathematics" has helped us to gain a better understanding of the physical world is something waiting for a judgement. It may even be more misguided than the string landscape.

8. **nbutsomedy**
   April 7, 2008

   peter,
   Having said what I have written before, I should mention that I agree to completely with your concern about bogus "phenomenology". Especially "string inspired" cosmology and phenomenology. It almost seems like a disease and most of the researches are irrelevant at the best.

The New Math

April 7, 2008
Categories: Uncategorized

Via Steve Hsu, I ran across the cover article from Alpha magazine, a magazine for the hedge fund industry, entitled The New Math. It describes the role physicists are playing in several hedge funds, developing sophisticated trading strategies. One of the best known of the organizations doing this is Renaissance Capital run by Jim Simons, and its success is now responsible for funding the new Simons Center at Stony Brook.

Two of the theoretical physicists featured in the article were fellow graduate students at Princeton while I was there. One of them, Marek Fludzinski (who was a couple years ahead of me), quickly left academia and went on to a career in finance, ending up founding the Thales hedge fund, which he still runs. The other, John Moody (more about him here), was in my class and so I got to know him quite well, but I had lost track of him in recent years. He worked with Frank Wilczek on axions and left Princeton for Santa Barbara after Wilczek went there to the ITP.

In recent years quite a few mathematics Ph.D.s and a very large number of particle theory Ph.D.s have ended up in the finance industry, and the article describes the kinds of things that they are doing. The impact of the recent melt-down in the credit markets remains to be seen: maybe there will be fewer jobs available in this field, or maybe demand will increase for people with this kind of technical background as companies pursue ever more sophisticated strategies.

Comments

1. **TomW**
   April 7, 2008

   Unfortunately, the most likely outcome is that the "eggheads" will serve as convenient scapegoats for the fact that banks had no idea what kind of debt they owned or whether it was any good or not. Cue backlash in three, two, …

2. **Chris W.**
   April 7, 2008

   The last thing they need is more sophisticated strategies. It's funny how so-called financial engineers never seem to have heard of the old engineering **KISS** principle, much less thought paid close attention to the hard lessons software engineers and computer scientists have been learning about building complicated systems for the past 5 decades. And then there is the natural invitation to fraud offered by debt securitization…

   Of course, they've had to deal with clueless executives and investors breathing down their necks, looking for ever higher returns. That certainly doesn't help in learning and sustaining good engineering judgment.

   Sorry; the rant is over.

3. **D.**
   April 7, 2008

   There is always room at the top, if I may steal that paraphrase, but in my experience there exists a fairly high washout rate among the physicists, less from aptitude than boredom or culture shock.
   However, once you've developed your toolbox and demonstrated good results, you'll basically never want for a job again; the unemployment rate among good quants has been about zed for at least two decades, being more a factor of liquidity than anything else.

4. **Deane**
   April 7, 2008

   In business section of last Sunday's Times, there is a review of a book called "The Trillion Dollar Meltdown". It quotes the author as saying, "As a general rule, only the very smartest people can make truly catastrophic mistakes".

   I think Chris W. has it right.

5. **mathjunkie**
   April 8, 2008

   It is not new that banks or financial institutes look for quants who have PhD degrees in physics. Back in year 2000, the physics professor I worked with asked me if I was interested in working in some banks as a quant as they needed such people to do very difficult maths. As my maths was really not good, I refused the offers.

6. **Allan**
   April 8, 2008

   It will be interesting to see how many of the Masters programs in financial math/ risk engineering / whatever will survive the current meltdown. And what will happen to the unemployment rate for new Ph.D.s in both math and physics if this alternative career is no longer readily available.

7. **Mathew Crawford**
   April 8, 2008

   This article should get play higher up the media chain. Off Wall Street, industries should recognize what geeks bring to the table in terms of strategic sophistication. Many do, but many don't. The number of math jobs (including those going to the physicists) will increase, not decline.

   Chris W.,

   I don't think the current financial meltdown and the sophistication of hedge fund strategies are linked in a particularly meaningful way. The mortgage system that failed us was in place before hedge funds became what they are today — a field of giants in the financial industry. Hedge funds didn't pressure anyone to make an industry out of sub-prime loans. (And hedge funds have been complaining about Bear for a long time.)

   Hedge fund traders have been talking about how scary the leverage at Fannie Mae and Freddie Mac have been for over a decade, but could do nothing about it but talk.

Deane,

As for the smartest people making catastrophic mistakes, that may often be true. But most generally, it's the people with the most power who make the biggest mistakes. This is just the common investor effect, and sometimes the power holder is not the smartest person in the room.

8. **Chris W.**
April 8, 2008

See this interview with Robert Merton in *Technology Review*.

I'm not buyin' it…

Matt: Major players in the financial services industry facilitated and encouraged the growth of the sub-prime mortgage industry, and disseminated securities based on these sketchy loans. They can say that nobody had to invest in these things, but that's disingenuous; they were *marketing* them, and they were fully intent on succeeding in those marketing efforts. Clearly there was a lot of fraud and stupidity among subprime lenders, but they've paid the price; they're out of business or shadows of their former selves. The borrowers are hardly getting a free ride either.

The whole "investors and borrowers were stupid, and have themselves to blame" argument is at about the same moral level as a drug dealer who ridicules his junkie customers for buying the product.

Perhaps the only reality check (if not a solution) is extreme skepticism, shading into paranoia, and willingness to look under the hood by anyone who relies in some way on the machinations of this industry. If that smacks of saying screw the banks and hiding one's savings in a mattress, so be it. Fortunately there are quite a few financial institutions that managed to maintain some detachment from the whole debacle.

[Sorry, Peter.]

9. **Deane**
April 8, 2008

I maintain that the most powerful person in the world (say, George W. Bush) is able to make a catastrophic mistake only with a lot of help from extremely smart people. The point is that smart people are able to magnify or leverage a small or diffuse effect into much bigger or more concentrated one.

10. **Michael Bacon**
April 8, 2008

"The point is that smart people are able to magnify or leverage a small or diffuse effect into much bigger or more concentrated one."

Yes, but there's a bit of cancellation going on at the same time. Probably stable overall, but subject to big fluctuations that could have really bad effects.

11. **DB**
April 9, 2008

I think you need to distinguish two categories of math jobs, those related to creating quantitative trading strategies and those used for the securitization of mortgages into what are known as Collateralized Debt Obligations (CDOs).
The former will continue to prosper, but the latter is essentially dead in the water, because it is a major factor in the credit crisis. Enormously complex mechanisms were used to package suspect loans in order to obfuscate the true risk in the underlying securities – essentially nobody understood how dodgy these vehicles were because they were deliberately packaged so as to frustrate any attempt to understand them according to standard financial principles.
Mortgage securitization will recover and continue, but this industry is experiencing a "flight to simplicity", transparency will be everything so there will be no place for complex and concealing math.

12. **Chris Oakley**
April 9, 2008

DB –

The most common use for math/science PhDs in finance that I am aware of is in developing derivative pricing models, of which CDOs are but one strand … I do not see this going away any time soon – although the appetite for derivatives is greater in a bull market it does not go away completely in a bear market, especially as existing portfolios still need to be managed.

13. **Deane**
April 9, 2008

Re: DB's comment

"Flight to simplicity" is a good thing, and I hope it continues.

And indeed this will probably lead to reduced demand for quants and less blind acceptance of sophisticated models. But that does not mean that mathematical finance will go away. It will definitely continue to play a significant role in finance, including securitization of mortgages and other assets. However, both quants and their overseers need to be both more honest and outspoken about the limits of what mathematical finance can do.

I believe a lot of quants understand the pitfalls of their models but have been unwilling or unable to express these issues effectively to others.

14. **Tim Jones**
April 9, 2008

An early example of the particle theory to finance transition is eloquently described in Emanuel Derman's autobiography "My life as a quant". I should declare an interest: we worked together on early electroweak models in 1973, and this is discussed briefly in the book. (Page 73!).

15. **stevem**
April 9, 2008

In the (rather good) textbook “Statistical Mechanics of Financial Markets” one chapter discusses potential uses of earthquake prediction theory and a “Richter scale for market crashes”, as well as ideas of scaling, turbulence theory and so on; so all sorts of ideas are being considered. Getting some real idea of when things are about to go seriously awry is the real trick in this game, and that’s where LTCM in particular failed disasterously. It has clearly been demonstrated that these trading strategies can work effectively if they are not pushed too hard; then steps can be taken if there is a nasty change in market dynamics. But LTCM in particular was massively overleveraged and got immobilized by its sheer mass. The problem is that some managers and investors are always going to want to always push the models and push boundaries to try and create bigger returns, or to outdo each other. The state of the art seems to be getting very advanced now though albeit not infallable. The stuff on genetic algorithms, machine learning and neural nets discussed in the linked article is pretty interesting though.

Another problem is when you have more and more people chasing the same money and opportunities in the markets, using the same strategies. I wonder how much of the financial engineering literature is worth reading since all the best stuff will no doubt be kept very secret. Also, it could be all too tempting and convenient for traditional manages, traders and Wall street types to blame the current mess (or future crises) on the “eggheads” or “rocket science”, especially if things get worse. Some traditional Wall Street types and traders also no doubt feel resentment towards hedge fund managers who make much more than them and of the widening gap between “the haves” and the “have mores”.

The fact that there were a number of well-documented high-profile disasters in the 90s directly due to “eggheads” does not help either:LTCM of course, as well as Joseph Jett and Kidder Peabody, and the Orange County derivatives mess, also due to a theoretical physicist I believe. However, the steady, consistent and sober performance of Renaissance Technologies for example has also given a lot of credence and respect to what responsible quantitative finance or financial engineering can actually do. So I would say it is an open question for now as to whether the current situation will see an increase in demand for physicists and mathematicians or a decrease. I’m skeptical though as to whether aspects of human behavior and psychology can be factored into models. When he lost £20,000 in the South Sea Bubble Newton supposedy said: “I can compute the motion of heavenly bodies but not the madness of people”.

16. **Chris Oakley**
April 9, 2008

Dr. Oakley’s Lessons To Be Learned From Past Financial Disasters

Orange County: Some derivatives salesmen are really good.

Joe Jett, Nick Leeson & Jerome Kerviel: When large amounts are involved, make sure that at least one person in the organisation outside the trader’s bonus pool knows and understands fully what is going on.

LTCM: PhDs in Mathematics sometimes do really stupid things.

17. **Sven**
April 9, 2008

When you think about it, what is the common thread between the following?

String Theory
LQG
LTCM
Subprime Hedge Funds

18. **locrian**
April 9, 2008

> And what will happen to the unemployment rate for new Ph.D.s in both math and physics if this alternative career is no longer readily available.

Unaffected. Financial engineering is a fairly small (in the scheme of physics) area that only a very few PhD’s from the very top schools tend to go into. If it disappeared entirely I doubt it would have any dramatic affect on physics employment rates. The idea that Wall Street is scouring physics departments is largely a myth these days; what might be more accurate is that there area couple of physics departments and a few MFE departments they look to.

Personally I see physicists as being eliminated from that area entirely over the next quarter century as students with more formal training replaces them.

19. **Teacher of Quants**
April 9, 2008

It’s unlikely that “students with more formal training [will] replace” physicists and mathematicians. Despite the availability and employment advantages of specialized MFE and PhD programs in quantitative finance, there is still a wide gap in capability between the students on that bandwagon, and the theoretical math/physics types.

20. **locrian**
April 10, 2008

I agree that there is a wide gap in capability between students in MFE programs and theoretical physicists – the MFE peeps are much better at quantitative finance, at least for some significant initial period of time.

21. **Teacher of Quants**
April 11, 2008

Unless you mean “MFE peeps” who already have a solid math/physics background (such as academia opt-outs grabbing a remunerable credential), the physicists leave them in the dust sooner rather than later. For a physicist to replicate the MFE skillset is much easier than the reverse.

22. **Chris Oakley**
April 12, 2008

I have to agree with ToQ here: I mentored a PhD in Aeronautical Engineering who had no previous finance experience. In less than three months he was able to write code to calculate cross-currency asset swap spreads from first principles. OTOH others I worked with in the investment banking business, some of whom had taught Finance at university level, but had no specific math or “hard science” background did not, IMHO, even understand what a yield curve was.

23. **Haelfix**
April 12, 2008

Everyone knew the housing bubble was going to burst, but actually some of the largest money to be made is right at the tail end of a bubble, which is why you see all these people staying in the game years after they first calculate the impending doom.

The whole art is knowing when to get out in time. To that end, many quant derived highly adaptive and sophisticated computer models were made that were very sensitive to the relevant indicators that should predict how and when that would take place.

The catastrophe occured when every indicator failed at the exact same time. There was no provision for that, and thus the models failed.

Its a good example of when and how computer simulations can go awry.

24. **dave tweed**
April 12, 2008

It does strike me as unfortunate that a high proportion of the people who get these sort of finance jobs are theoretical physicists rather than population biologists, computer network designers or others who work with mathematical models in science. By its nature, physics tends to deal with situations where you can make accurate measurements of variables and where there's no collusion between states, so they really don't have an intuitive feel for when they don't hold, but neither are particularly true in finance. On the other hand they're dealt with everyday in other fields. I wonder if there is any reason beyond economists physics-envy and the inherent self-promoting bombast of physicists why they're the primary group recruited into mathematical finance? Is it an apostolic-succession situation?

25. **IMHO**
April 12, 2008

Physicists are hired for marketing reasons…."Look how smart our people are"

Physicists On Tape

April 12, 2008
Categories: Uncategorized

Over at the Center for Science Writings, John Horgan has started an interesting project of posting copies of taped interviews with scientists that he has accumulated over the years, starting with an interview with Chandrashekar. In a blog entry about this, he points to a piece entitled Revenge of the Science Writer from 2001 by Robert Crease, a historian of science and co-author of what I think is the best history of the development of the Standard Model: *The Second Creation: Makers of the Revolution in Twentieth Century Physics*. Crease describes Horgan as "a man who does not destroy his tapes", and tells the following story:

> I once interviewed a well known European physicist whom I had arranged to meet at a table in the noisy lunchroom at the Brookhaven National Laboratory. I pulled out a tape recorder and asked him about one of his experiments. The scientist could barely disguise his impatience. After five minutes we had a slight misunderstanding about where a certain event took place. I corrected him; he thought I hadn't been listening.
>
> "That's it!" he fairly roared as he abruptly stood up, his chair shooting backwards. The background hubbub in the lunchroom suddenly plummeted and everyone turned to stare. He strode away, shouting, "This is a total waste of time! You're an imbecile!" Or at least I think that's what he shouted. I can't be sure, because I destroyed the tape. I was too embarrassed to consider replaying it, or risk having others know my humiliation.

One tape I wonder if Horgan still has is that of his interview with Witten back in the mid-nineties. His chapter on string theory in *The End of Science* included a rather harsh portrayal of Witten, some of which I remember as being quite unfair. It's also true that Horgan's aggressive skepticism about the claims being made at the time for string theory has stood up very well in light of what has happened in the intervening fifteen years or so.

Comments

1. **MathPhys**
   April 12, 2008

   Chandrasekhar gives Horgan a hard time.

2. **pazuzu**
   April 12, 2008

   [anonymous comment attacking John Horgan deleted. Please don't use my blog to anonymously attack other people.]

3. **Michael**
   April 14, 2008

   I know many writers and artists that have the same reaction to interviewers, this is from Sally Mann:

   "Over the years I've learned not to talk about my work, taking to heart the Robert Doisneau quote that goes something like this: "If you make pictures, don't speak, don't write, don't analyze yourself and don't answer any questions." I would amend that by adding, "And don't read any critical comments, either." People always seem to freight the work so heavily with meanings that were not in any way intended or even subconscious. "

4. **Wavefunction**
   April 17, 2008

   Crease's book is indeed excellent.

Dark Matter

April 13, 2008
Categories: Film Reviews

Yesterday evening I went to see the new film Dark Matter, which opened here in New York this weekend. In many ways, it's very good, much better than I was expecting.

The plot is loosely based upon the story behind the 1991 shootings at the University of Iowa physics and astronomy department. Gang Lu, a Chinese graduate student who had recently defended his Ph.D. but was having trouble finding a job, shot and killed five people, including his advisor, and severely wounded a sixth person. One unfortunate aspect of the movie is that it rewrites this true story to make the killer much more sympathetic, while making some of the people who were murdered much less so than they seem to have been in real life. The families and friends of the victims may be justly outraged by this. There's more about the film and the controversy over its relation to the Gang Lu story here.

In the movie the real life space physics at Iowa has been transposed to cosmology at a university in Utah. It remains set in the early 1990s, with protagonist Liu Xing recently arrived in the US from mainland China. His advisor, Jacob Reiser, is a cosmologist with a model called the "Reiser model" which is based on cosmic strings. Liu Xing comes up with what he considers a breakthrough, a new model based on using superstrings to explain dark matter. This doesn't meet with Reiser's approval, and he is failed at his dissertation defence for a bogus reason (this is one of the ways the movie sadly departs from a realistic plot or the true story). The scientific advisor for the movie was David H. Weinberg, a cosmologist at Ohio State, and Lee Smolin is thanked among many others in the credits.

The strongest point of the movie is its portrayal of the lives of the graduate students, mostly Chinese, who are struggling to adapt in an alien environment, some of them consumed with a passion to make breakthroughs in science, others already trying to figure out what to do about their not-so-great academic job prospects. The director, Chen Shi-Zheng, emigrated to the US about the same time as Gang Lu, 1987, and so starts from a deep understanding of what the experience of Chinese students arriving in the US at that time must have been like.

Back in the real world, David Harris is reporting from the big April APS meeting on the new Symmetry Magazine blog Symmetry Breaking. In a blog entry entitled Dark Matter Discovered? he reports a rumor that the DAMA collaboration will report on Wednesday that they have detected dark matter in the latest run of their experiment. This story just appears on Slashdot, "slashdotting" whatever unfortunate web-server is running the Symmetry Breaking blog, so it is inaccessible for the moment .

Update: For more about the DAMA rumor from science writers at the APS meeting, see this blog entry from JR Minkel, and a characteristically detailed and informative posting from Jennifer Ouellette.

Update: There are now reports from the talk by Tommaso Dorigo and Alexey Petrov, and the slides are here. Sounds like this experiment is definitely seeing a signal with yearly variation, but it's controversial whether this is due to dark matter or something else much less interesting.

Update: Yet more from the New York Times and Tommaso on this topic.

Update: Yet more skeptical commentary about DAMA here.

Comments

1. **Thaddeus**
   April 13, 2008

   Come on Peter, it's fiction. Artistic license is not only permissible, it's desirable.

2. **DB**
   April 14, 2008

   The Dama collaboration will have a job on their hands to convince sceptics after they cried wolf in 2000. The latest PDG review states:

   "The DAMA experiment operating 100 kg of NaI(Tl) in Gran Sasso has observed, with a statistical significance of 6.3 σ, an annually modulated signal with the expected phase, over a period of 7 years with a total exposure of around 100 000 kg·d, in the 2 to 6 keV (eee) energy interval [33]. This effect is attributed to a WIMP signal by the authors. If interpreted within the standard halo model described above, it would require a WIMP with mχ □ 50 GeV and σχp □ 7 · 10–6 pb (central values). This interpretation has, however, several unaddressed implications. In particular, the expected nuclear recoil rate from WIMPs should be of the order of 50% of the total measured rate in the 2–3 keV (eee) bin and 7% in the 4–6 keV (eee) bin. The rather large WIMP signal should be detectable by the pulse shape analysis. Moreover, the remaining, presumably
   e/γ-induced, background would have to rise with energy; no explanation for this is given by the authors. The extended version of DAMA, LIBRA, is now taking data with 250 kg of NaI(Tl) in Gran Sasso; results are scheduled to be published in 2008."

3. **Coin**
   April 14, 2008

   Artistic license is not only permissible, it's desirable.

   Couldn't we object on purely artistic grounds? It seems to me that storytelling is much less interesting when you try to sugarcoat everything.

4. **Mike**
   April 15, 2008

   The link to Jennifer Ouellette is broken. You are missing the "t" from "twisted".

5. **Peter Woit**
   April 15, 2008

   Mike,

   Thanks, now fixed.

6. **axel.**
   April 15, 2008

I agree with Coin: turning a tragedy caused by an (apparently) personal crisis into one caused by politics is not a very artistic way of looking at things, is it? but i haven't seen it yet, only the trailer, and of course the infos on wikipedia are not really "first hand", either.

7. **luny**
   April 15, 2008

   I read about this episode in the book Disciplined Minds.
   The account given there was very sympathetic to the student and unsymphathetic to the advisor.

John Wheeler 1911-2008

April 14, 2008
Categories: Obituaries

News of the death of John Wheeler came yesterday, and many people have already written detailed, touching and informative pieces about the man, his life and scientific achievements. See for example here, here, and here. With Wheeler gone, physics loses one of its very few living contacts with the early days of quantum mechanics, since his career reached back to the early thirties when he went to study with Bohr.

My most extensive contact with Wheeler was surely through learning GR from the marvelous textbook he co-authored on the subject. By the time I got to Princeton as a student, he had recently left for the University of Texas, in order to evade Princeton's mandatory retirement age policy. He still was a presence in the department though, returning to give talks (I remember one that was an advertisement for the importance of the notion of a complex: "the boundary of a boundary is zero!" was the slogan). My only conversation with him was at a meeting organized between graduate students and a visiting committee of people evaluating the department. I recall a very friendly older man who came up to talk to me, and listened attentively to my going on for quite a while about how things could be improved. Only after we had finished speaking and he had left did I realize who I had been talking to. My overwhelming feeling immediately was that if I had realized this earlier I'd have much more enjoyed keeping quiet and getting the chance to ask him a few questions.

Update: See here for another article about Wheeler, from the University of Texas. It includes the claim that "Wheeler was the first person to emphasize the importance of string theory", which, as far as I know, has nothing to do with reality.

Update: For more about Wheeler, see this interview just put on-line, and this discussion at Bloggingheads.

Comments

1. **Thomas Love**
   April 14, 2008

   Way back in 1973 I was a pilot in the USAF, thinking about graduate school. I wrote a handwritten letter to Wheeler. In response I received a 5 page handwritten letter from him with very good advice (including advertisements for his books). I only know him through his books and articles, but he had a profound effect on me and my work.

2. **Matti Pitkänen**
   April 15, 2008

   Wheeler-Thorne-Misner was also for me a very stimulating book, in particular his topological ideas were very inspiring. I remember also Boulder lectures. Without Wheeler's very positive and detailed hand written summary about the article representing the first version of Topological Geometrodynamics it would have been probably very difficult to publish it. The attribute "Topological" was added to "Geometrodynamics" as the title of article for obvious reasons: I do not remember whether it was Wheeler or David Finkdelstein who proposed this. I am really proud of having had this contact.

3. **MathPhys**
   April 15, 2008

   Misner, Thorne and Wheeler is on the bookshelves of almost every person I know who trained and/or aspired to be a scientist in the 70's.

4. **Dr. E**
   April 15, 2008

   John A. Wheeler was my advisor for my junior projects at Princeton.

   He was a "professor emeritus," with an office on the third floor of Jadwin Hall.

   The first junior paper was on GR, and the second was on the EPR effect & quantum paradoxes.

   I had first come across Wheeler's work my freshman year in the awesome book: Quantum Theory and Measurement (Princeton Series in Physics) by John Archibald Wheeler and Wojciech Hubert Zurek.

   There was a copy in my freshman dorm's library–it was the only physics book in the rather small collection.

   I remember one autumn day, in his office in Jadwin Hall, he clenched his fist lightly and said that today's culture lacks the noble. And he looked at me and said it was our generation's duty to bring it back.

   This was around 1990; and think of all that we have seen in the past twenty years or so, in physics, politics, Wall Street, and beyond. It seems we have some work to do….

   Wheeler had walked with Einstein at Princeton and mentored Feynman. He came of age in an era when debates, as manifested by that by the Bohr Einstein dialogues, were marked by an exalted respect–both for one another and for Physical Reality. And he embodied that refined respect; with a humility matching his intellect and monumental accomplishments.

   He said that freshmen ought begin research right away, when he adressed the freshman class of physics majors; and he told us that while the grad students knew a few things, the professors knew even less. He said this with a wink and smile.

   He was a cordial friend to all; and always lent everyone his ear. He was most generous with his time; and his enthusiasm to "know what the show is all about, before its out," was contagious. He was one of those rare intellectual leaders who inspired the better angels of our nature.

   Well, we will all miss him. And we ought do our best to thank him, by embodying his principles in our daily actions and contemplations–in our pursuit of physical reality's fundamental nature.

   ghoststorythriller@gmail.com

5. **Ali**
   April 15, 2008

Hi Peter,
What is this Princeton’s mandatory retirement age? This is the first time I am hearing of this type of practice in USA.

6. **Peter Woit**
April 15, 2008

Ali,

Many universities in the US used to have mandatory retirement ages for tenured faculty, typically age 70. This was made illegal in 1994.

7. **nontrad**
April 15, 2008

True, the UT article seems to suggest a quote by Unruh that relates Wheeler to ST. That’s strange, at least to me, since I can’t think of significant contributions to ST made by Unruh or Wheeler. But, I’m probably myopic and mistaken. The mix of folks and folklore and word of mouth can be hard to follow.

That said, the UT article seems to reiterate a consistent point in the various tributes to Wheeler seen since his NYT obit: He advocated for physic’s future in general; and the younger generations in particular.

A man who truly made difference.

8. **nbutsomebody**
April 16, 2008

I regret that I did not have the opportunity to meet the last of the giants.

9. **Thomas Love**
April 16, 2008

Peter, This is the only comment by Wheeler on strings that I have found so far:

“Space and time are not things, but orders of things. If these words of old call to a new and deeper conception of space and time than Einstein’s publications give us, then in what direction are we to look? For a reply, many today would point to string theory or other new and exotic developments. But if we are not yet ready to accept them, with all of their new elements, then where can we look?”

—Daniel H. Wesley and John A. Wheeler, “Towards an Action-at-a-Distance Concept of Spacetime”, in A. Ashtekar et al (eds.), “Revisiting the Foundations of Relativistic Physics”, pp. 421-436, Kluwer Academic Publishers, 2003

That doesn’t sound like a supporter of string theory.

10. **Peter Woit**
April 16, 2008

My best guess would be that the claims about Wheeler and string theory are based on someone mistaking the S-matrix for string theory. The recognition of the importance of the S-matrix is often attributed to Wheeler, back in 1937.

11. **rob**
April 16, 2008

The statement about string theory is bizarre indeed. My guess is along the same lines as Peter’s: Bill Unruh probably said Wheeler was one of the first people to think about quantum gravity. The reporter probably confused quantum gravity with string theory, as so many people do.

12. **Luiz**
April 16, 2008

Ali and Peter,

Sorry for the off-topic but maybe this “mandatory retirement age” wasn’t such a bad idea after all.
http://www.phdcomics.com/comics/archive.php?comicid=912

Cheers.

13. **Professor R**
April 17, 2008

Hi Peter,
your tribute ‘my most extensive contact with Wheeler was surely through learning GR from the marvelous textbook he co-authored on the subject’ is very fine, and probably the sort of tribute Wheeler would have appreciated most.
I suspect the passing on of knowledge becomes very important to the giants of science as they get older, jst as it does for musicians etc. It must be fantastic to see one’s students and grand-students making progress in theoretical physics – but a good textbook can reach a much larger audience!

14. **Jimbo**
April 18, 2008

Much continues to be said about Johnny Wheeler, but if you’d like to watch him describe his remarkable life, here is a link to a series of in-depth video interviews he gave just a few years ago.

http://www.peoplesarchive.com/browse/movies/2359/

15. **Daniel Wesley**
April 20, 2008

remember that John Wheeler he occasionally expressed a desire to learn more about string theory. He wasn’t overly critical of, but he certainly never tried to sell me on it. To that extent I think it is probably not accurate to describe him as a strong supporter of string theory. I think it would be just as inaccurate to describe him as a foe.

To extrapolate from my own experience, I think the issues that string theory has been successful in addressing are not necessarily the ones that he was most interested in: things like the nature of (space) time, the connection between physics and information ("it from bit"), why quantum mechanics is so strange, and so on. I think he believed that the big advance in quantum gravity would come from a conceptual breakthrough (a la Einstein) rather than a technical one. He believed that once we had the right answer we'd be able to explain it easily to "the man on the street," and I don't think any of the ideas we've come up with so far pass this test!

I was very sorry to hear about John's passing. He was a great inspiration to me, and I cherish the lessons I learned from him. I've met very few scientists of his stature who were so generous with their time and energy. He had a sign in his office, which said "Encourage! Encourage! Encourage!" and he lived that motto in an exemplary way.

16. **Eric H**
April 23, 2008

From the comments I'm reading I believe Mr Wheeler was a compassionate person and a scientifically great man in the technical sense. The only thing that bothers me about his legacy is that he seems to have given so much credence to bad ideas, such as "delayed choice", "quantum fluctuations" in the absense of describable forces, etc. While he understood that great ideas have an intuitive aspect to them the ideas he ended up being famous for were exactly the opposite of that realization. Its really unfortunate that good people can end up being the promulgators of these types of ideas.

Because he seems to have been a kind and encouraging person many of these ideas that have led to a dead end in quantum physics have been promulgated by his students. Now, if he had had a nasty temperament I'm sure this wouldn't have been the case. How many times through history has the sociological aspect of the teacher/student relationship had an impact on what gets taught rather than the quality of the ideas?

17. **Eric H**
April 24, 2008

At the risk inflaming feelings and becoming a scapegoat I will add one further observation about John Wheeler. His attitude about quantum mechanics was summarized in this quote:

"To Wheeler, quantum mechanics was a "great smoky dragon," particularly with respect to the bizarre behavior of the electron, which defied classification as a wave or a particle. It could be anywhere, everywhere or nowhere, depending on the observer.

This led him to the conclusion that reality, as we know it, comes into existence only because we are here to see it and bring it to life. "

I believe this conclusion that he came to that there is no external reality apart from our observation is the single most deluded and erroneous conclusion about quantum mechanics that has ever been foisted on the scientific community and the public at large. This is what allows scientists to speculate about alternate universes being created when a quantum choice is initiated. Because all possibilities are simultaneous realities then there "must" be an alternate universe created whenever a quantum choice is made.

To me it is incredible how scientists just go along like sheep with such ideas because the scientist making them, John Wheeler, is well regarded in other aspects of physics. It just shows how stupid physicists can be and how they can be led astray even more than the general public on these types of things. Poor William of Occam would shudder at the ineconomy of having to create an entire universe out of whole cloth every time a decision is made.

18. **Peter Woit**
April 24, 2008

Eric,

I don't think you can blame Wheeler for multiple universes. The recent interest in them comes from a very different direction.

Actually, just about all physicists not only don't follow Wheeler like sheep, but happily ignore his idiosyncratic views about quantum mechanics. Even among those who are more interested and would pay some attention to his views, I don't know of anyone who would follow them because of their source. More typically, such people have their own idiosyncratic views…

19. **Eric H**
April 24, 2008

Peter,
I hope you're right about independent thought. I used to believe in it much more than I do now. There seems to be unconscious processes that go on in all of us that make us want to fit in with the herd. I don't think it is a very attractive characteristic – especially so in the different fields of science. I sincerely wish I was wrong but I don't see evidence of it.

Multimedia and the Journal of Number Theory

April 16, 2008
Categories: Uncategorized

I recently heard from David Goss, editor-in-chief of the Journal of Number Theory, that the journal is planning on introducing video abstracts for papers that they publish. Here's his e-mail explaining this:

> Dear Colleagues:
>
> By now I believe that all of us have had the pleasure of watching a famous scientist or mathematician discuss their work on internet video. I have certainly done so myself and learned much. Indeed I can readily give a long list of mathematicians who I would very much like to view presenting the ideas behind their great works.
>
> For example, it would be fabulous to watch a young Serre discussing the ideas behind FAC or GAGA; or a young Faltings discussing his solution on the Mordell Conjecture etc. I am sure that each of you can compile your own long list (and obviously not just papers on number theory!).
>
> It is in this spirit that I suggested to Elsevier that all JNT authors be allowed to present a short (4 minutes max) "video abstract" of their accepted manuscript. Elsevier has kindly accepted this idea and is now quite excited about it.
>
> The idea is very simple: When a paper is *finally accepted* for JNT, the author will be notified and given the option of putting up a video abstract — THIS IS ONLY FOR ACCEPTED PAPERS! The video will be watched to check for professionalism etc., and those videos deemed offensive will not be used (and the author sanctioned!). The video will then be linked next to the paper on the JNT website. Information on how to upload files, etc., will be on the JNT website by April 23, 2008.
>
> Videoing virtually anything is now deeply a part of our culture as witness the rise of YouTube. In fact, we will be using YouTube itself temporarily until Science Direct is augmented to handle flash files (which I hope will be within a few months). The url is:
>
> http://www.youtube.com/user/JournalNumberTheory
>
> where there is a short video about these multimedia abstracts!
>
> I expect the video to be very low key like the one on the above url. I also think that, frankly, it will be a fun thing to do after all the hard work producing an accepted manuscript! Certainly the technology to produce such videos is now ubiquitous worldwide.
>
> It is important to note that ALL such videos will be archived by Elsevier and thus will be available for future scholars and mathematicians.
>
> With a little thought one can see vast possibilities here: For instance a paper on the topology of elliptic curves could be proceeded by a short video of computer graphics narrated by the author etc.

Comments

1. **Jason Starr**
   April 16, 2008

   Why do we need Elsevier for this? Anybody can post video to YouTube, and no university libraries need be bankrupted in the process.

2. **Arun M T**
   April 17, 2008

   An open repository would be truly great. `You Tube' wouldn't be a bad idea.

3. **tom**
   April 17, 2008

   I agree that libraries don't need an extra Elsevier charge for this and that anybody can post on youtube.

   On the other hand since the aim is really to talk only about accepted papers, all such videos posted to the youtube group of the journal should have both comments and ratings disabled since any youtube user (think laymen, children and anonymous colleagues) can comment and rate any youtube videos that allows it. In fact I'm not sure that ratings can be disabled in youtube, which would be a problem.

4. **Klaus Korak**
   April 17, 2008

   Hi, I am a co-founder of the Journal of Visualized Experiments in Cambridge, MA.

   Please visit our site at http://www.jove.com.

   we would be very happy to support this effort!

5. **Bee**
   April 17, 2008

   I can only say about this what I said previously: the quality of a video and how well one can sell oneself or the topic does greatly depend on professional support. If the journal doesn't provide a service that ensures videos can be produced with roughly equal quality, this will just widen the gap between the scientists in institution where there is such a support (e.g. by the public outreach department or by a hired contractor) and where there isn't. When I read a sentence like 'videoing virtually anything is now deeply a part of our culture', I have to ask myself exactly whose culture is meant here? (I certainly don't see it as part of my 'culture'.) As much as I like watching videos myself, I am afraid this can bias people's opinions towards those who have the possibilities to come up with great videos. Yes, I would expect that the first some videos are low key, but if this becomes an established procedure and gains in importance, researchers and their institutions will try to produce the most convincing videos they can possibly come up with. The analogy to commercials and their influence on the 'free marketplace' lies at hand.

6. **Alejandro Rivero**

April 17, 2008

It is important that youtube (and, I expect, other storage sites in the future) provides now a "chromeless player" which allows full control of video, jumping to specific positions, sinchro with slides or other videos, all from javascript.

7. **Steve Myers**
April 17, 2008

At first it seems like a good idea, but I think it would also cause fear & trembling to run through math depts. The few times I've been forced to "present" it is usually a disater with an hour lecture condensed to 5 minutes of quick explanations & scribbled derivations & questions answered by pointing to an equation. And it could easily start the wrong kind of competition (American math idol?).

8. **Thomas Love**
April 17, 2008

The five minute limit seems rather strange, who can say anything worthwhile in that time? It would be better to have each author post a video presentation of a full hour lecture.

9. **Coin**
April 17, 2008

Alejandro: Lee Smolin has been posting a lecture series this past semester on the nature of time, and he is using some interesting flash player that shows a video of Smolin giving the talk in one corner of the window and the current page of Smolin's slides in the rest of the window. It would be interesting to see this concept used more often (and especially interesting if it could be extended such that the table of contents for the slides could be used to jump to that point in the lecture!).

10. **dave tweed**
April 17, 2008

Since Arun talked about an open archive, let me point out that another issue is storing the video in an open format. YouTube relies on Adobe Flash, and my understanding is that whilst Adobe releases a specification of the format their licence disallows using it to implement a player, so "officially" you are restricted to hoping adobe release a player for the computer system you use. (It may be that this clause in their licence is invalid; I'm not a lawyer.) Even if Adobe do release a version for your system, I've had bad experiences with their installer software on minority systems like Linux, where it makes some big assumptions that are sometimes wrong. So picking a format where legally only one company can make the software presents problems that could be avoided by picking a format that's intended to be free for anyone who wants to to implement.

11. **Jeff McGowan**
April 18, 2008

I don't get the point of this at all. I had a paper (joint with E. Makover) in JNT in 2006, 5 pages long, I could explain the proof to a good high school student. Even with an unusual paper like that, I can't see what this would add. I could pretty much read the main part of the paper in 5 minutes, but to explain it well takes about an hour. And I don't want to sit through 5 minutes for each paper to try and figure out if I'm interested, I want to scan though my most recent email from arxiv with a short paragraph about papers in fields I might be interested in, and figure out quickly which ones are worth putting more time into

I'm guessing they were thinking "well, it's great when one of your colleagues stops you in the hall and spends 5 minutes telling you about their latest result" but that is a completely different thing.

While I'm here, hi Peter, long time. Remember getting pulled over at UCLA with Huntley. I tell that story all the time 🙂

12. **Maynard Handley**
April 18, 2008

WTF is this being done using streaming technology?
There is NOTHING streaming can do that cannot be done better with downloaded files. A downloaded file can be played on a media player away from your desktop, can be played at greater or slower than 1x, can be substantially more easily and smoothly scrubbed.

If you want to see how to do this sort of thing properly, go to the KITP web site (http://online.kitp.ucsb.edu/online) instead of reinventing the wheel and doing so badly.

13. **Peter Woit**
April 18, 2008

Hi Jeff,

Nice to hear from you!

From what I remember of the incident Jeff is referring to, we were stopped by UCLA campus police, presumably on the grounds that no person with any legitimate business on campus would be driving there at night in the kind of car I owned at the time (an ancient Chrysler New Yorker, on its last legs, that I had bought for some ridiculously small amount of money from Peter Orland). It took some effort to convince the cops that we were reputable mathematicians there for a conference, with the behavior of one of our number in the back seat not helping matters… But it's been a long time, I probably am misremembering this.

14. **Jeff McGowan**
April 19, 2008

Peter,

That's it, more or less as I remember. My only misbehavior in the back seat was not having my hands in view, which annoyed the officer with the gun behind the car. I seem to remember Huntley being quite loud, which is no surprise, and of course all three of us had pony tails. Ah, I miss my hair….

btw, love checking in on the blog. I ended up interested in the whole string theory thing since I kept bumping into it at conferences (my first memory is some talk on Riemann surfaces where the number 26 popped up and someone asked "is that the 26 from string theory dimensions"), and now I'm actually doing a lot of stuff on RS and trying to learn Grothendieck's stuff. I really enjoyed your book, and Smolin's (I never met Lee, but always felt a connection since he was the physics student at Hampshire before me, and there weren't many of us).

15. **John R Ramsden**

April 20, 2008

From David Goss's email:
>
> The video will be watched to check for professionalism etc., and those videos deemed offensive will not be used (and the author sanctioned!).

Chortle. How in the world could a 4-minute abstract on a number theory paper conceivably be offensive? Are there number theorists with Tourette's syndrome, or those who tend to flash their audience without warning?! Those number theory seminars must be more interesting than I thought 😉

16. **Peter Orland**
April 20, 2008

Well, to be precise, it was a Ford LTD, with a police-car engine.

Experimentalists 1, Theorists 0

April 18, 2008
Categories: Uncategorized

At Fermilab the Tevatron is producing record amounts of luminosity, see here for a story about a celebration of this. Things also appear to be going well at the LHC, as the cooldown remains on schedule, and only a tolerable number (12) of PIMs needed to be replaced in the sector recently warmed back up. See here and here for some discussion of current planning for the next year. The machine should be cool and ready for beam commissioning in late June, and if all goes well, by September an initial physics run with 5 TeV beams at relatively low luminosity may begin. At these luminosities and energies, the stored energy in the LHC beam will be no greater than at the Tevatron (although the important number for physics, the per-particle collision energy, will be 5 times higher). The plan is to run until December, with a heavy-ion run at the end, then shutdown until April 2009. During the shutdown the magnets will be trained, allowing beams at the full energy of 7 TeV during the 2009 run.

Particle theory, especially string theory, is not doing as well. Data recently compiled about top-cited particle physics papers from 2007 shows only one theory paper from this century making the list of 51 most heavily cited papers, and that was the KKLT paper which is referenced by all "landscape" and "multiverse" studies. The sad state of string theory has even made it deep into the popular consciousness. Last week's episode of "The Big Bang" featured a brilliant young prodigy explaining to the particle theorist character that his work on string theory was a "dead-end", due to the landscape problem. Even economists are dissing the subject:

> Modern financial theory as applied ranks with string theory in physics as one of the greatest intellectual frauds of our time. Whereas the vacuous pretensions of string theory have finally been exposed (we now know that the theory never generated a single falsifiable prediction), those of "financial engineering" are just beginning to be exposed both in the press and in lawsuits alike.

At Santa Barbara, Jennifer Ouellette reports on a workshop about "how to come off better during TV appearances":

> Joe Polchinski (inventor of D branes in string theory, and one of the few permanent members at KITP) also agreed to be mock-interviewed, revealing a sly sense of humor in the process. For instance, asked if there was any controversy about string theory, he deadpanned, "Oh no. Everybody agrees that string theory is correct." It cracked up the room.

This workshop unfortunately didn't seem to include the advice to just say no when asked by TV producers to participate in a short stupid comedy skit making fun of science and scientists. See here, here and here, for reports on Wednesday's "Root of All Evil" show from Comedy Central, which featured a mercifully short segment making fun of scientists as incomprehensible geeks. Participating in things like this does about as much to help the image of science and scientists as appearing on a Spike TV segment about the use of physics to determine whether women can crush beer cans with their breasts.

Given that things are going very well with the LHC, and badly with string theory, string theorists are doing the logical thing: advertising their activities with graphics of strings superimposed on a picture of the LHC. See here and here.

Update: Minutes from the LHC Installation and Commissioning Committee April 11 meeting are here. They include the exchange:

> L.Evans asked if the cryogenics teams are still on track for having the whole machine at operating temperature in mid-June. S.Claudet replied that taking the figure of 6 weeks from room temperature to 2K, and allowing 2 weeks of cryo tuning, sector 45 would be ready for hardware commissioning in the first half of July.

This indicates that beam commissioning is likely to begin in July, not June.

Also discussed was what to do about possible stray plastic parts in the beam tube:

> Any pieces of plastic would be vaporised by the beam so we should not delay start-up to search for these.

Update: Commentary on this posting from Lubos here, including

> I am amazed by the people who deliberately keep on opening the pile of manure called Not Even Wrong – it must be due to a really nasty deviation of theirs that dwarves pedophilia.

Comments

1. **Ethan Siegel**
   April 18, 2008

   Peter,

   Thanks for an interesting post today! I'm trying to figure out whether there are any definitive tests one can do to either validate or falsify string theory, and the overwhelming large parameter space the landscape provides seems to make this impossible.

   I've written a little bit about it on my website here and here, but haven't gotten any responses that explain whether string theory is, even in principle, testable at this point. What are your thoughts on it?

   Best,
   Ethan

2. **dan**
   April 18, 2008

   Dear Peter,
   I understand that luminosity is related to the number of particle events, a certain large number of which is required for the statistical probability of interesting rare events like the production of Higgs and/or SUSY-partners.

   I understand for a given collision energy, a non-detection of an increased luminosity beam rules out the existence of Higgs and/or SUSY-partners at that energy (i.e Higgs does not have a mass below 130GEV with 2-sigma 95% before the luminosity upgrade, increasing to 3-sigma 99.7% with the luminosity upgrade)

   I understand that Tevatron has had a "300x" increase in luminosity with a 100x increase in data, but how does this affect string theory? Neither string theory nor SM is committed to a specific value for the Higgs and/or SUSY partner masses, although it is obvious the increased luminosity constrains it. How does an increase in luminosity for TEV spell things going badly for string theory (unless you feel that TEV's non-observation contrains what LHC might see)?

3. **piscator**
April 18, 2008

If particle experiment does well particle theory does well. The entirely correct model of TeV-scale physics may already be written down, published, on the arxiv – and attracting five citations a year.
Experiment alone will separate the wheat from the chaff.

The logic of counting citations to measure success says that particle theory is only doing well when there is a bandwagon that is acting as a paper factory. If (God help us) there were three hundred papers on unparticles this year would that really make 2008 a great year for particle theory? There is lots of interesting work around, but it is no bad thing if there is not so much ambulance-chasing.

4. **Jacob**
April 18, 2008

Seems like someone is a bit jealous he wasn't asked to be on "Root of all Evil" instead………

5. **big vlad**
April 18, 2008

I feel I must point out that just because string theory is in a parlous state, doesn't mean the rest of particle theory is. In particular, phenomenology is in a great state. There is more work in this area than there are people to do it, and career prospects are bright for young researchers entering the field.

6. **Peter Woit**
April 18, 2008

Ethan,

String theory is simply not testable, by any conventional understanding of what it means to have a scientifically testable theory. I've spent much time here going over the arguments about this, I suppose I should spend some more and get them put together in some place I could easily point to. A FAQ, if you will…

dan,

The fact that the Tevatron and LHC are doing well has no correlation with the fact that string theory is doing badly. String theory has zero to say about what either machine will see.

piscator,

I just disagree with your statement that "there is lots of interesting work around", but for some mysterious reason, almost everyone is ignoring it. If you have some data that backs up this point of view, I'd be interested to hear about it.

Jacob,

I suppose there's a long list of things in life that other people but not me are invited to do and I'm jealous of this. Appearing on "The Root of All Evil" isn't one of them…

7. **Coin**
April 19, 2008

we now know that the theory never generated a single falsifiable prediction

Maybe OT, but I am fascinated by the way this is phrased. "We now know"? It is as if this was only discovered in retrospect. "In 2006 a theorem was published which proved that no one had published a paper between 1986 and 2006 containing a distinct predictive model of M theory…"

8. **Sara**
April 19, 2008

At this moment, the motto of String Theorists seems to be: "If the mountain won't come to Muhammad, Muhammad must go to the mountain".

9. **dan**
April 19, 2008

Dear Peter

"dan,

The fact that the Tevatron and LHC are doing well has no correlation with the fact that string theory is doing badly. String theory has zero to say about what either machine will see."

String theory may have "zero" to say but what about SUSY-quantum field theory? Since string theory would probably go into decline if LHC-TEV does not see SUSY, and continue to flourish if it does see SUSY,

If SUSY does stabilize the EW scale, and some form of SUSY-field theory is used as a basis of some sort of prediction based on this, what does the current non-observation of a light higgs and other SUSY-partners at TEV's increased luminosity imply with the likelihood LHC will see SUSY when the TEV currently has not, with its much increased luminosity.

10. **IMHO**
April 19, 2008

> Peter Said:This workshop unfortunately didn't seem to include the advice to just say no when asked by TV producers to participate in a short stupid comedy skit making fun of science and scientists. See here, here and here, for reports on Wednesday's "Root of All Evil" show from Comedy Central, which featured a mercifully short segment making fun of scientists as incomprehensible geeks. Participating in things like this does about as much to help the image of science and scientists as appearing on a Spike TV segment about the use of physics to determine whether women can crush beer cans with their breasts.

Hi Peter,

I don't think that's even close to accurate. In fact, I think it shows a severe misunderstanding of popular culture, social interaction, and human nature in general.

It's pretty basic…

An incomprehensibly opaque field populated with unaccessible inhuman humorless people who don't act "normally" like the rest of us……that's scary and scary things are burned at the stake.

Those smart guys who talk all that incomprehensible smart stuff, but they're pretty funny and just regular people like the rest of us….If tv says they're important, then I guess it's ok to support them.

To see that this is true, just look at every government that has ever existed in the history of the universe. There is an entire political appatatus as a buffer between serious policy and the lay public….think of that skit as the incipient physics political arm.

Bad publicity is something like: Those smart guys think we're stupid, and they're trying to trick us.

11. **nbutsomebody**
April 19, 2008

Peter wrote
"….I suppose I should spend some more and get them put together in some place I could easily point to. A FAQ, if you will…"

A FAQ would be an excellent idea.

12. **Peter Woit**
April 19, 2008

dan,

As the Tevatron gets more data, they presumably can push up a bit the bounds on SUSY, and there is a range of possible SUSY parameters that the Tevatron can't exclude, and that the LHC could observe. But string theory has nothing to say about the values of these parameters, even about whether anything will show up in the LHC range.

13. **just a graduate student**
April 19, 2008

I'm starting my phd on quantum gravity and already got sick about the existent situation. I'm tired of watching consecutive attacks against string theory (ST). ST did this, ST can't do that, ST is the atomic bomb.

All we have in quantum gravity is 5-6 theories that did simply nothing for physics. No predictions, no experiments, nothing at all. Still, everyone still fights for the best theory to survive the game. "MY theory has unification!" yeah, but "MY theory is background indipendent, yours is not!" etc etc etc.

oh dear lord, why quantum gravity is so appealing to me instead of condensed matter physics, or photonics?

why?

14. **tommaso dorigo**
April 19, 2008

Dan: *"I understand that Tevatron has had a "300x" increase in luminosity with a 100x increase in data,"*

Sorry for nit-picking, but it takes so little additional effort to be accurate… It bothers me when numbers fly loosely in a scientific argument.

The Tevatron had a x15 increase in the record instantaneous luminosity in Run II over Run I. The integrated luminosity in Run II is now 30x larger than in Run I, not x300. And that means a similar factor in the increased statistics for rare processes with high trigger and collection efficiency, say for instance top quark pairs; save a small (30%) additional increase due to the larger energy of the beams in Run II vs Run I (980 vs 900 GeV per beam).
Instead, the total bounty of data has not increased by x30 WRT run I, but just by about x10. That number is almost meaningless, however, since it is only connected to the data aquisition capabilities of the system, and has nothing to do with the size of "discovery" datasets (which have increased by about x30x1.3=x40, as I said above).

Cheers,
T.

15. **Steve Caudill**
April 20, 2008

Interesting that there is a workshop on how to look better on TV!

As a government scientist (U.S. Nuclear Regulatory Commission nuke inspector), I've had to go through about 6 week-long training courses over the past 17 years, in which we were videotaped and our taped performances critiqued, etc. One such course dealt specifically with how to handle interviews by the media, which was taught by ex-media personnel.

Now I'm just waiting to get on TV!!! (but not as a result of a nuke accident of course!!)

16. **chethan krishnan**
April 20, 2008

> All we have in quantum gravity is 5-6 theories
> that did simply nothing for physics.

This is a bit of a murky area. Because science is not JUST about predictions, but also about understanding. Epicycles could make excellent predictions for planetray motion, but understanding came only with Newton.

String theory does give you its share of Aha! moments. I will just mention one that I haven't seen mentioned much to the general public: Mathur's fuzzball proposal for black holes. It is very hard to believe that we do not understand the essential mechanism of black holes after you read, say, hep-th/0502050. Of course it is possible that "real" black holes don't fit with the plan, etc. etc., but thats mostly because everything is possible.

The basic point is that string theory seems like a good theory of some quantum gravity – but not manifestly ours. I think this is reason enough to work on it.

> No predictions, no experiments, nothing at all.

Yes, expriments are hard to come by in this day and age. But should we shut down speculative theory because of that? Heck, thats all we got! The other option seems like throwing the baby out with the bathwater to me.

17. **Bee**
April 20, 2008

Given that things are going very well with the LHC, and badly with string theory, string theorists are doing the logical thing: advertising their activities with graphics of strings superimposed on a picture of the LHC. See here and here.

At least the first graphic I am sure I've seen before, I think on a workshop called 'Strings at CERN' in 2004 or so.

18. **jpd**
April 20, 2008

re chethan krishnan:
i think the gist of the opposing argument is that the
bath water is cold. take the baby out , dump the water
and get some more hot water.

19. **chethan krishnan**
April 20, 2008

> i think the gist of the opposing argument is that
> the bath water is cold. take the baby out , dump
> the water and get some more hot water.

I am glad you got the maximum mileage out of that bit. 🙂

If there is a new theory that is more promising than string theory, by all means I am for it. I am just not happy with the sitting around and waiting for it part. In the meantime, exploring string theory seems like the most useful thing to do to get some insights into quantum gravity. The fact that it might even be right, adds to the attraction.

Also, the inherent difficulty of testing quantum gravity at low energies makes me suspect that speculating will be inevitable to get a glimpse of the truth. Which is why I find arguments against speculative theories not so compelling. My claim is that even though the issue is often portrayed as string theory vs. something else, it really reduces to theorizing vs. no theorizing.

Over and out.

20. **Peter Orland**
April 20, 2008

>chethan krishnan Says:

>If there is a new theory that is more promising than
>string theory, by all My claim is that even though the
>issue is often portrayed as string theory vs. something else, it really > reduces to theorizing vs. no theorizing.

Chethan,

I think you are right, if there are only certain problems you want
to theorize about. But there are other things besides gravity
and unification to theorize about. Your view is the mainstream
one in theoretical high-energy physics, of course.

Personally, I don't think quantum gravity is an important science question, though it is certainly a fun and interesting question. I would write a paper about it (and have once or twice), if I had an idea concerning it I liked; but it just doesn't interest me as much as other problems. The Planck mass just can't be studied with experiments. Quantum gravity may be fun, but it also seems like a distraction from serious physics. People who hold this view aren't loved by their colleagues (I don't think we are disliked either, just ignored), but there it is…

21. **anon.**
April 20, 2008

'… the inherent difficulty of testing quantum gravity at low energies makes me suspect that speculating will be inevitable to get a glimpse of the truth. …'

You're judging alternative theories by the criteria of string theory. In string theory, quantum gravity effects are important only at unobservably high energy scales.

Take a look at all the loose ends in classical gravity at present (dark energy, dark matter, etc.) which quantum gravity should say something about at low energy where general relativity is a classical approximation to quantum gravity.

An alternative to string theory may predict the amount of dark energy, i.e. the lambda term in the classical general relativity solution which will emerge as an approximation for low energy.

That would be a checkable prediction. String theory can't do it because of the landscape problem, but some alternative quantum gravity theory might be able to predict lambda. Just because string theory is a failure, doesn't mean to say that all other ideas will be a failure, too. They just haven't had the concentrated attention and effort devoted to them yet, that string theory has received for ages.

22. **Peter Woit**
April 20, 2008

chetnan,

The only people I see suggesting "sitting around and waiting" as a research strategy these days are string theorists, who are unwilling to give up on a failed theory, and instead advocate waiting for LHC results that will "narrow the parameter space" of possible string theory vacua to be investigated, or produce some unexpected result which will fuel the next theory bandwagon for them to hop on to.

The only way a new, more promising theory will come about will be from people working on trying new things, not from sticking to tired, familiar ideas that they are comfortable with even though they don't work (for by now well-understood reasons). If you don't want to try to find something new, why bother spending your life working in this area? The world is full of all sorts of interesting things to do and to think about, devoting yourself to a failed idea because it pays the rent isn't much of a way to spend one's life.

23. **Aaron Bergman**
April 20, 2008

But there are other things besides gravity
and unification to theorize about. Your view is the mainstream
one in theoretical high-energy physics, of course.

Judging by where the money's going, I don't think that's an accurate assessment anymore.

To anon: If you happen to have a theory of quantum gravity, or really anything at all, that has something useful to say about the cosmological constant, I think you'd find that people will start studying it very quickly.

24. **One of 10,000 Monkeys**
April 20, 2008

"who are unwilling to give up on a failed theory, and instead advocate waiting for LHC results that will "narrow the parameter space""

You seem to think that unless a theory can uniquely predict everything that will be seen at LHC from first principles, then the theory must be no good. I'd like to ask, when has such a thing ever happened? The Standard Model was produced with the guidance of experimental results, not deduced beforehand. You're like some guy around 1960 going around complaining about how Yang-Mills theory is a total failure and waste of time because it can't predict anything.

Really, you're just the equivalent of a Rush Limbaugh for science. All one has to do is replace 'string theory' with 'liberalism' in your diatribes and it's a one-to-one mapping. Indeed, you were probably the main source for articles such as

http://www.conservapedia.com/String_theory

People who read such articles and take them seriously are your intellectual brothers.

25. **Peter Woit**
April 20, 2008

"You seem to think that unless a theory can uniquely predict everything that will be seen at LHC from first principles, then the theory must be no good."

Monkey,

For the 10,000th time, the problem with string theory is not that it doesn't "uniquely predict everything" that the LHC will see, but that it predicts absolutely nothing at all about this (or anything else for that matter…).

Thanks for the link to "Conservapedia" where the entry for string theory states "As of today, string theory has been a total failure." That's not a web-site I would normally ever consult, interesting to see that the "string theory has failed" meme is even showing up there. Someone should tell Lubos.

Seeing all the bad press string theory is getting some days makes me feel sorry for string theorists. I tend to get over this pretty quickly though when I see how they are reacting to it, e.g. by making bogus claims about testability and the LHC.

26. **Aaron Bergman**
April 20, 2008

Just as a random note, why is it that falsificationism and positivism seem to be conflated so often? (Conservapedia, admittedly not a source known for its accuracy, is just the latest I've seen.) As I recall, Popper came up with falsificationism as an explicit reaction against positivism which was more concerned with making meaningful statements (a sort of verificationism).

27. **One of 10,000 Monkeys**
April 20, 2008

"the problem with string theory is not that it doesn't "uniquely predict everything" that the LHC will see, but that it predicts absolutely nothing at all about this (or anything else for that matter…)."

Exactly the same thing could have been said about Yang-Mills theory in the fifties and sixties. Yet, the Standard Model turned out to be a Yang-Mills theory. In reality, string theory is presently in the same place today as the quantum theory was from the period 1905-1925, before the formulation of quantum mechanics.

28. **Peter Woit**
April 20, 2008

"string theory is presently in the same place today as the quantum theory was from the period 1905-1925"

Monkey,

If you want to get some idea of the broad range of convincing experimental tests passed by the old quantum theory, you might want to look up the Nobel citations for Planck (1918), Einstein (1921) and Bohr (1922). These were not people who spent their time making up absurd excuses for not having a testable theory, and the Nobel committee didn't give them Nobel prizes based on such excuses.

29. **One of 10,000 Monkeys**
April 20, 2008

Peter,

Please explain to me how the photoelectric effect and the emission spectra from the hydrogen atom were tests of the quantum theory (using the criteria you apply to string theory)? Both of these phenomena were well-known before either Einstein's or Bohr's models, so I don't think you can claim that the quantum theory of this time made any testable predictions. Indeed, the Bohr model is in fact not correct. Rather, the quantum theory itself was pieced together bit by bit using these experimental data as guidance.

As a counter example, I can construct string models with three generations of chiral fermions which transform as representations of the SM gauge group, just as Bohr used the quantum theory to construct models of the hydrogen atom. In neither case, do we make any new predictions. However, the models themselves may lead us to a deeper understanding.

30. **Peter Woit**
April 20, 2008

Monkey,

It's just simply not true that all Bohr did was "construct models of the hydrogen atom" that predicted nothing.

31. **David Nataf**
April 20, 2008

Peter, is it really true that string theory failed to make any predictions, did they predict a cosmological constant would have to be negative in the 1990s?

10, 000 monkeys, the Bohr model of the hydrogen atom was able to explain things that nobody else was able to explain – the emission lines of hydrogen. The theories available at the time predicted the electron should crash into the proton… whereas the standard model has a solid grip on particle results. Does ST have such a result? Einstein got a value for h in his photoelectric experiments that matched the h that popped out of Planck's equations for the blackbody spectrum. That's pretty good.

They worked off not understood anomolies such as atomic spectra. We've now had equally not-understood anomolies such as dark matter for thirty years, and dark energy for ten. We have a curiously flat and uniform universe. There's a lot that no one understands, as much as there was in 1905.

32. **dan**
April 20, 2008

Hey Peter, have you seen this article, string theorists weigh in on what they think the LHC will see, and how it affects string theory

http://www.telegraph.co.uk/earth/main.jhtml?xml=/earth/2008/03/25/scibigbang125.xml
http://www.telegraph.co.uk/earth/main.jhtml?xml=/earth/2008/03/25/scicomments125.xml

For copyright reasons I won't reproduce what they say here except this:

MARTIN VELTMAN "As for string theory, it's all mumbo jumbo, with no connection with experiment.' I wonder if he came to this conclusion on his own or as a result of reading your work and the controversy it has inspired

NIMA ARKANI-HAMED "My hunch is that there's a better than evens chance that supersymmetry will show up at the LHC"

EVA SILVERSTEIN "I'd be extremely puzzled if they don't find the Higgs, but wouldn't be devastated if they didn't come up with evidence for supersymmetry.

'Some of my intuition comes from string theory, an appealing candidate for a theory of all the forces of nature. According to many – perhaps most – versions of string theory, supersymmetry does not hold good at the energies probed by the LHC, so its discovery might require further explanation from this point of view."

PW, Eva's statement comes as news to me, is there any reason why Eva (and some of the other string theorists interviewed) are backpeddling from seeing SUSY at LHC energies, given the 'hierarchy" argument for SUSY in the EW-scale? Eva is now claiming that string theory SUSY probably won't show up at LHC energies, but that if SUSy does show up, string theory would need further explanation.

If LHC does not see SUSY, could SUSY still be (experimentally speaking) the explanation for hierarchy argument (if the SUSY-breaking scale is above what LHC can produce/detect) which is what Eva seems to be saying.

33. **Peter Woit**
April 20, 2008

David,

There never were serious claims that string theory "predicts" a non-positive CC, although as late as 2001 Witten was writing that he knew of no "clearcut way" of getting a positive CC out of string theory.

If you look at the Einstein Nobel citation you'll see that it explicitly refers to detailed tests of the photo-electric effect performed after Einstein's proposal that tested and confirmed his predictions.

dan,

Thanks for pointing those out, I hadn't seen those quotes. The Silverstein quote that

"According to many – perhaps most – versions of string theory, supersymmetry does not hold good at the energies probed by the LHC"

is a bit striking, given that many string theorists in the past have claimed low-energy supersymmetry as a "prediction" of string theory, and that if it does show up, they certainly will claim it as evidence for string theory.

I think what she is referring to is the idea that one can show that there are more string vacua with SUSY breaking at high energy than at low. If SUSY doesn't show up at the LHC, Arkani-Hamed and others will claim this as evidence for the landscape, on the grounds that the only possible explanation for the hierarchy is an anthropic landscape.

Veltman has always been a string theory skeptic, he didn't get that from me…

34. **One of 10,000 Monkeys**
April 20, 2008

David,
The Bohr atom didn't explain anything, it merely fit the data that was available namely, the Balmer series. As for the stability of the atom, Bohr only gave an ad hoc rule for this, which was to say by decree that orbits satisfying the angular momentum quantization rule (which was again ad hoc) were stable. This is not an explanation, merely a phenomenological model. Even though the Bohr model was almost completely wrong in almost every respect, it was still useful, mostly because it led to the deBroglie wave/particle duality. This of course, then led to the Schrodinger equation.

Regarding the Silverstein statements regarding not being upset about the prospect of not seeing supersymmetry at LHC, Peter is correct. I've heard this from Kachru himself, who made the accompanying statment that 'Natural is whatever nature does', to justify why the anthropic arguments to solve the hierarchy problem are no more unnatural than supersymmetry.

35. **dan**
April 20, 2008

Peter,
I thought Eva's guess was based on the fact that the light higgs, higgs, and SUSY has not been seen at TEV, which constrains and decreases the likelihood low energy hierarchy-solving SUSY will be observed at LHC, or 50-50 as NIMA ARKANI-HAMED quantifies it.

Eva's argument seems specious to me since even if most vacua imply a high-scale susy-breaking than is unattainable at LHC, I would use the anthropic principle to argue that we happen to live in a universe where the SUSY-breaking scale is close to the EW breaking scale (within LHC energies) so as to solve the hierarchy problem. In a multiverse, we would not exist in a universe with a high energy SUSy breaking, as it would allow the Higgs boson through radiative quantum contributions to pull up to the GUT or planck scale.

If LHC doesn't see SUSY, then evidently SUSY isn't the correct explanation for the hierarchy problem, is there any experimental or observational reason to think SUSY is a true but broken symmetry of nature besides the running coupling constants?

36. **chris**
April 21, 2008

dan,

if you ever heard veltman talk on anything, you would have instantly recognized that he is a critic of everything except feynman diagrams. i am not sure he even takes local QFT as well established fact.

monkey,
i encourage you to stop embarrassing yourself and do as peter suggested: read up the nobel citation for bohr.

37. **Bee**
April 21, 2008

Monkey,

Your reasoning is completely confused, read your own comments. You've eventually come to claim the thing to do is making 'phenomenological models' that are 'useful' because they can lead to further insight. I couldn't agree more! Just that you started up claiming essentially the opposite, namely that theories do not need to give rise to models that make predictions that test the theory. I think you have somewhat of a confusion about models and theories (recommended reading). Best,

B.

38. **One of 10,000 Monkeys**
April 21, 2008

Dear Bee,
I'm not sure where I claimed that theories do not need to give rise to models that make predictions that test a theory. I certainly hold no such opinion. However, you must first get to the point where your theory is understood well enough to do this, and generally you need to be able to construct specific models. In the previous example I gave regarding Yang-Mills, there is absolutely no chance that the Standard Model can be derived directly from YM. Indeed, the SM is but one of a vast landscape of possible Yang-Mills theories. To discover which one describes our universe, we construct models that describe the known data and try to fill in the gaps. Once the model is sufficiently developed, then testable predictions can be made. This is how science works.

39. **Brett**
April 21, 2008

The Bohr model made several crucial predictions. It predicted the existence of new lines in the hydrogen spectrum, although this probably could have been done using numerology alone. Much more important, it predicted that the quantized nature of the spectrum was related both to the quantization of light as photons and the quantization of the energy levels of atoms. The former form of quantization was still gaining acceptance, although its utility in explaining physical phenomena was already demonstrated. That the atomic levels of hydrogen (and by extension, all atoms) were quantized was completely new. This property of the Bohr model made many strong predictions, such as the outcome of the Frank-Hertz experiment.

40. **One of 10,000 Monkeys**
April 21, 2008

Brett,
The Bohr model did not predict any new lines in the hydrogen atom. At best, it merely gives the Rydberg formula. As far as the quantization of energy levels is concerned, this is a postulate of the model, not a prediction. For all practical purposes, it was a lucky guess. Don't get me wrong, I am not puttying Bohr down. He was a first class phenomenologist and philosphopher.

41. **jpd**
April 21, 2008

its amazing how many geniuses come up with lucky guesses.

42. **Peter Woit**
April 21, 2008

"For all practical purposes, it was a lucky guess."

I see, the only difference between string theorists and Planck, Einstein, Bohr is that they haven't been as lucky…

The argument that Monkey is making I think is based upon a somewhat different argument that David Gross is fond of, that, at the conceptual level, one should think of the present state of string theory as analogous to that of the old quantum theory, a patchwork of ideas that indicates the existence of a still unknown underlying well-defined theory.

I always thought it was a bad idea for Gross to promote this analogy, since there were obvious huge problems with it, especially problematic being that the old quantum theory had a huge amount of experimental backing, while string theory has none. Monkey's comments indicate that my concern was justified.

43. **One of 10,000 Monkeys**
April 21, 2008

Are you guys that are so fond of the 'genius' Bohr aware that he advocated giving up the principle of energy conservation?

At any rate, I put Bohr's model on the same level as the deBroglie wave hypothesis. Both ideas were brilliant and lead us to the correct theory, quantum mechanics, but the ideas in themselves are not completely correct. This is the essence of bottom-up phenomenology, as opposed to the top-down approach.

44. **chethan krishnan**
April 21, 2008

>The only way a new, more promising theory will come about
> will be from people working on trying new things, not from
> sticking to tired, familiar ideas that they are comfortable with

Not sure there is a recipe for scientific revolutions. One thing I am fairly certain of is that pushing the current ideas to their limits is probably going to be essential to see beyond them. In the case of quantum gravity, string theory is certainly a way to gain such insight, whatever its ultimate use.

> even though they don't work (for by now well-understood
> reasons).

:).. Right.

> If you don't want to try to find something new, why bother
> spending your life working in this area?

Not wanting to try something new? Try what? The only way to come up with something new that is non-crackpot is to understand the existing ideas well enough to know their holes.

Besides, we are all clutching for straws here! It is easy to sit there, play critic, condescend, etc.

> The world is full of all
> sorts of interesting things to do and to think about, devoting
> yourself to a failed idea because it pays the rent isn't much of a
> way to spend one's life.

Thanks for the career advice. I guess! 🙂

Its amazing the bitter resentment here, even when we are just trying to have a conversation.

45. **proofreader**
April 21, 2008

chethan krishnan Says: "…Besides, we are all clutching for st**rings** here!…"

You had a typo. I've fixed it for you.

46. **Peter Woit**
April 21, 2008

chethan,

No, there's no "bitter resentment" here. I'm quite happy with my life and things I'm working on. Life is good. I happen to think that the decision by much of the particle theory community to keep pursuing string-theory based unification despite the increasingly obvious failure of the idea is a mistake, and this is worth pointing out. The argument that "let's keep doing this, maybe something will turn up" has been made for many years now, and has just led to the field working itself farther and farther down a blind alley. Sooner or later this will have to be acknowledged. Sooner would be better.

47. **nbutsomebody**
April 21, 2008

Monkey,
I understand your point that Bohr model did not predict anything new, it has just explained something already known. Ok, let me agree (even if it may not be true). Let us forget about predictions altogether.

Just explain, no need to predict. I am placing a lower acceptance criterion than Peter usually put on string theory. Just give me a model, which has standard model gauge group, three generations, all moduli stabilized and with reasonable cosmological constant and particle mass. Do not even bother about whether it is unique and stuffs like that. Just give me one. Do not say that there are so many string models in the landscape one will certainly match with Standard Model. Give me an example. Just one…. Please…

48. **dan**
April 21, 2008

PW,
playing DA here, "especially problematic being that the old quantum theory had a huge amount of experimental backing, while string theory has none."

the LHC isn't online yet. If, hypothetically speaking, SUSY does show up at LHC, this would be a reason theorists to claim string theory has experimental backing.

49. **milkshake**
April 21, 2008

that offhand remark about no need for searching after hapless things that may stray into the beam path (for they shall be vaporised in instant)…..”At last, we have ways to make you pay attention. New physics is not here to be posessed by the faint-hearted theorists!”

50. **One of 10,000 Monkeys**
April 21, 2008

nbutsomebody,
There are such models in the literature which closely resemble the MSSM, and where all moduli may be stabilized. However, at this point I don't want to say more for fear of being accused of self-promotion.

51. **Peter Woit**
April 21, 2008

dan,

The problem with the idea that the LHC will provide experimental backing for string theory is that string theory doesn't predict anything at all about what the LHC will see.

52. **Coin**
April 21, 2008

However, at this point I don't want to say more for fear of being accused of self-promotion.

Erm, we don't know who you are so we wouldn't have known it was self-promotion unless you'd said that, now would we have?

nbutsomebody, you can find models purporting to be of the exact type you're requesting by typing variations on “mssm string” into Google. Here's one, title “The Exact MSSM Spectrum from String Theory”, abstract starts with “We show the existence of realistic vacua in string theory whose observable sector has exactly the matter content of the MSSM…”. I am not qualified to evaluate whether these models deliver on their claims.

53. **dan**
April 22, 2008

Hello PW,
I guess it's an issue of semantics

you are obviously right that some string theorists from Michio Kaku and Lubos Motl are betting that string theory implies LHC-accessible SUSY partners to show up, with others like Eva Silverstein saying it won't show up (but still working on string theory nonetheless)

so string theory as you say doesn't make a prediction about whether SUSY shows up in the LHC,

but as far as prediction goes
1- all susy string theoris predict SUSY. it does not offer any details about the nature of SUSY that can be predicted a priori. So while string theory cannot predict whether SUSY will show up in LHC, or any of its values, if SUSY does show up at LHC, it would be evidence consistent with susy-string theory.

The semantic issue is this: string theorists say some models of string theory predicts susy at LHC energies, the LHC finds susy, a prediction is verified. If LHC does not find SUSY, some classes of string theory are falsified. So string theory does make predictions (or some classes do)

If LHC were to see SUSY, string theorists will continue its dominance of HEP and QG for decades on end. I do wonder what will happen if LHC does not find SUSY — Eva Silverstein clearly feels this is likely.

If Eva Silverstein is right and that string theory predicts SUSY will not be seen at LHC energies, but the higgs is, what then stabilizes it from quantum corrections, if it is not SUSY?

I take it from what you described that we're looking at 2010 for quality large data sets to come out to show higgs and other exotics from LHC?

54. **A.J.**
April 22, 2008

If Eva Silverstein is right and that string theory predicts SUSY will not be seen at LHC energies, but the higgs is, what then stabilizes it from quantum corrections, if it is not SUSY?

Fine tuning?

55. **nbutsomebody**
April 22, 2008

monkey,
It does not matter who is the author. A objective truth is a scientific progress, and there is no self-promotion involved. I just like to learn. If you are an honest scientist, please give the arxiv number. Well, I am not an expert in compactification, but I would very much like to discuss the work with somebody who knows more.

56. **chethan krishnan**
April 22, 2008

> “…Besides, we are all clutching for strINGS here!…”
>
> You had a typo. I've fixed it for you.

Very cute. Now run along and bother mommy.

To Peter:
I wasn't implying that YOU are a bitter person. I meant that I came here expecting disagreement with string theory, but what I also find is a lot of bitter hostility towards string theorists. The former of course I might debate with, but the latter seems a little …

In any event, about the scientific issue: I think it is fair to take a moderate stand about string theory, but I simply disagree with your implication that string theory is almost provably useless in our quest for quantum gravity.

57. **nbutsomebody**
April 22, 2008

Coin,

Many thanks. I am not also qualified enough to judge these models, but it seems they do not talk much about moduli stabilization and particle mass and cosmological constant. Getting the gauge group is not enough, one has to get the Higgs and/or sponteneous symmetry breaking. (That is a tough job considering our inability calculate particle masses.)
Unfortunately just now I do not have access to guys who work on string compactification.
I really like to interview somebody like Kachru, Douglus etc., and put up a faq on what is known and not known about landscape and compactification.

58. **nbutsomebody**
April 22, 2008

chetan wrote,
"…but what I also find is a lot of bitter hostility towards string theorists."
It is true that some people are to some extent hostile to string theorists. Even considering the fact that the string theorists have a rare trait of perceiving hostility when people are simply differing in their opinion, there are still some who really are hostile. The reason is simple. People are pissed off by the extreme stupidity and stubbornness of seemingly intelligent string advocates.

When a bunch of religious fundamentalists make bogus claims, people may forgive and overlook them as mere idiots. When a bunch of iv-league faculties does the same, people kick b*tt.

Peter, extremely sorry for being rude…..

59. **Duncan MacGregor**
April 22, 2008

So the LHC can vaporise plastic but (remembering an old news story about the LEP) how would it do against beer bottles?

60. **Peter Woit**
April 22, 2008

chethan,

My claims that string theory has failed has to do with the idea of using it to do particle physics. If you want to use it purely to do quantum gravity, the problem is that it's unclear what "success" would actually look like. You're not going to get anything you can test experimentally, and thus get stuck in the situation an earlier commenter referred to of near religious warfare between competing "quantum gravities", with no good way of distinguishing which is "better" than the other.

As for the hostility towards string theory, some of it is, well, hostile people, or those who are hostile to things they don't understand. But a great deal of it is coming from the behavior of string theorists. The amount of unprofessional behavior, conviction that anyone who disagrees with you is an idiot, unwillingness to admit a mistake or that things are going badly, etc. that has been on display over the last few years coming from the string theory community has been shocking.

61. **PN**
April 22, 2008

>Particle theory, especially string theory, is not doing as well. Data recently >compiled about top-cited particle physics papers from 2007 shows only one >theory paper from this century making the list of 51 most heavily cited papers, and >that was the KKLT paper which is referenced by all "landscape" and "multiverse" >studies.

Hmm… Particle phenomenology papers (#27 and 31) are not counted to be THEORY anymore?

62. **nbutsomebody**
April 22, 2008

Peter said,
"the problem is that it's unclear what " success" would actually look like. You're not going to get anything you can test experimentally, and thus get stuck in the situation an earlier commenter referred to of near religious warfare between competing "quantum gravities", with no good way of distinguishing which is "better" than the other."

The comment is not entirely correct in my opinion. A quantum theory of gravity should have the following properties, which put strong restrictions of them. At the primary
level this conditions can be used to distinguishing which is "better" than the other.

1) As a quantum theory of GRAVITY, it should at least reproduce general relativity at low energy.

2) It should have a meaning full perturbative expansion.

3) It should reproduce the formula of black hole entropy calculated by Hawking in 1972.

4) It should say something about what happens near the initial singularities like big bang.

Once more than one theories at least satisfy first three of the four criterion, then only one has the luxary of a "religious warfare" between competing "quantum gravities". As far as I know String theories are the only one to fullfill (partially) these criterion ,

1) What we get from a naive low energy limit of String theory is Dilaton gravity in 10D. It is not exactly GR in 4 dim but it is certainly GR+other fields in 10dim.
It is possible that there are compactifications exists for string theory which gives 4dim GR. That will bring us to the controversial arena of landscape.

2) It seems that a finite perturbation series exists, but yet to be demonstrated.

3) Using AdS/CFT black hole entropy has been successfully calculated in various cases. Even for some non-supersymmetric black holes.

4) There are some preliminary result.

At least it seems to me that string theory is partially successful as a MODEL theory of quantum gravity. There may be other theories which are equally consistent and it may be possible that in time experiment will prove some of them to be correct.

63. **DB**
April 22, 2008

The blurb for the upcoming Kalvi conference "Anticipating physics at the LHC " is remarkably free of explicit string theory tie-in or the usual hype, considering the number of string theorists slated to appear:

http://www.kitp.ucsb.edu/activities/auto/?id=916

David Gross will open the conference.

Is this (along with Strings 2008 at CERN, and Witten spending next year on sabattical at CERN) part of a strategy to try to bring string theorists into closer contact with their experimental colleagues?

While it's difficult to see how string theory can effectively interface with the LHC, bringing both communities together seems like a good idea.

64. **Peter Woit**
April 22, 2008

PN,

Oops, sorry I missed those two papers. Still, the latest of these is from more than 6 years ago…

DB,

I don't actually see many string theorists listed as talking at that conference. While paying attention to what experimentalists are doing should be encouraged among all theorists, I don't think that string theorists claiming to be "string phenomenologists" is such a great idea, although it may work for them as a career move. Witten is an interesting case. I believe he's intensely interested in what is going on experimentally, and that's one motivation to visit CERN, but his own research remains more mathematical, presumably since that's where he sees a way to make progress absent new hints from experiment.

nbutsomebody,

Surely you've seen a similar list from Lee Smolin about the state of quantum gravity, in which LQG comes off better than string theory. I don't want to get into that argument. I'll just point out that this shows there is an obvious problem with such lists and the attached claims about how one's favorite theory is doing better than someone else's. One common problem with claims about string theory is that they neglect to mention that they've solved a problem in the wrong number of dimensions.

65. **nbutsomebody**
April 22, 2008

Peter,
Thanks.

"Surely you've seen a similar list from Lee Smolin about the state of quantum gravity, in which LQG comes off better than string theory."

Sorry I do not, but will try to find the reference,,,

"One common problem with claims about string theory is that they neglect to mention that they've solved a problem in the wrong number of dimensions."

Certainly , that is what I mentioned too.

66. **Peter Woit**
April 22, 2008

The Smolin article I was thinking of is:

http://arxiv.org/abs/hep-th/0303185

67. **high school physics teacher**
April 22, 2008

Might I offer into this discussion of Bohr's contribution my own first hand account of Dirac's position on the issue? In the 1982 conference at Loyola New Orleans, on the occasion of his 80th birthday, Dirac was asked from the audience if it was possible for him to name one name whose contribution to the development of QT was most important. Dirac's answer, "Bohr."

68. **Chris Oakley**
April 22, 2008

nbutsomebody,

The only property that a quantum theory of gravity *must* have is your #1, i.e. that it must reproduce GR in the classical limit. The rest … maybe … maybe not.

(That's why I never got interested in the subject).

69. **sanity**
April 22, 2008

Quantum field theory as a whole makes no
phenomenological predictions,
until you specify the content and couplings of the theory.

String models are in an early stage, but have already led to predictivity (in the same sense as QFT, namely after specifying a controlled and stabilized corner of string theory with specific field content) in inflation, a subject which is sensitive to quantum gravity effects and requires "top town" constructions. Upcoming data will falsify a subset of the known mechanisms.

Although there is a large landscape, there are many criteria that must be satisfied by a theoretically and observationally viable model, and it is not yet known which of these effects wins and whether string theory can or cannot be as predictive as QFT, correlating signatures with features of the internal dimensions. This will simply take more time to determine, and there will be important interplay between experiment (LHC and CMB) and theory. The reason no one makes a prediction one way or the other regarding low energy SUSY (the comments of Silverstein and others that you quote were essentially vacuous on this point, not committing to a prediction pro or con) is that there is no such sweeping prediction to make and they are trying to be conservative.

70. **Peter Woit**
April 22, 2008

"sanity",

I suppose I really should get around to writing that FAQ, including the answer I've given here many times to the claim that "string theory is just as predictive as QFT" (the short version is that what is predictive is gauge theory, it is predictive because the simplest gauge theories agree with a huge number of different observations, make huge numbers of testable and verified predictions. In string theory on the other hand, simple backgrounds disagree with experiment, so people end up choosing more and more complicated backgrounds until they can evade confrontation with experiment. Sure, you could make QFT unpredictive my constructing more and more complicated and ugly QFTs designed to not actually say anything about physics you can observe. But you don't do this. You do do it in string theory).

One reason I haven't gotten around to this is that I have a hard time believing that anyone is making this argument seriously. On the one hand you have the most successful scientific theory ever developed, one that makes an infinity of highly specific and testable predictions, thousands of which have been verified to high precision. On the other hand you have a theory that predicts absolutely nothing, is completely untestable and designed to remain so indefinitely. You want to argue these are "the same". Why should I bother to take the time to answer such an argument? It's obvious sophistry on its face.

And your claims that string theories make "predictions" about inflation are every bit as much nonsense as claims that they make "predictions" about particle physics at LHC energies.

71. **Peter Woit**
April 22, 2008

"sanity"

Oh, and doing this anonymously doesn't help. It indicates that you have so little faith in your own arguments that you are not willing to put your name to them.

72. **nbutsomebody**
April 22, 2008

Chris Oakley,
I agree to you partially.

No. 2) of my point is what is meant by a quantum theory of gravity. However I do agree that even if there is no perturbative expansion the theory may be defined non-perturbatively. But one has to conclusively answer such question in his/her theory of quantum gravity.

No 3) Yeah, There is a possible that the black hole entropy calculated by Quantum theory of gravity may not match with Hawkings result. But one need such a calculation in ones theory.

73. **nbutsomebody**
April 22, 2008

Peter,

oh thanks!, I read the article when it came out. Anyway I do not agree with it completely. Primarily for the reason I do not think GR at low energy has not come out of LQG yet. Plese see http://arxiv.org/abs/hep-th/0501114.

Also the AdS/CFT is making steady progress and people are now even calculating entropy of Non-SUSY black holes. That does NOT prove the correctness of string theory in four space time dimension, but shows it to be a consistent quantization of some extension of GR .

74. **anon.**
April 22, 2008

John March-Russell's answer in that Telegraph article — new particles with technological benefits for energy production — is the weirdest LHC prediction I've ever seen. (OK, maybe not weirder than Nielsen/Ninomiya's….)

75. **chris**
April 23, 2008

>Oh, and doing this anonymously doesn't help. It indicates that you have so >little faith in your own arguments that you are not willing to put your name to >them.

no, it probably just means he doesn't have tenure. it's a sad world.

76. **Christine**
April 23, 2008

Indeed, the Bohr model is in fact not correct. Rather, the quantum theory itself was pieced together bit by bit using these experimental data as guidance.

Just one more observation concerning Bohr, if I may.

"Two important heuristic principles have guided quantum physicists during the period 1913-1925, viz. Ehrenfest's Adiabatic Hypothesis [term due to Einstein] and Bohr's Principle of Correspondence". (…) "The research work during the years 1919-1925 that finally led to Quantum Mechanics may be described by systematic guessing, guided by the Principle of Correspondence". B. L. van der Waerden (Sources of Quantum Mechanics).

Bohr's contribution was fundamental from that alone, in the case one has some reservations towards statements of the sort: "the synthesis of the Rutherford's atom model with Planck's quantum hypothesis was [his] great achievement" (ibid).

One can also consider Bohr's [and Kramers'] ideas on the statistical conservation of energy and momentum and the statistical independence of the processes of emission and absorption in distant atoms (described in the paper by Bohr, Kramers and Slater), which turned out to be wrong (as already proved by experiments by Geiger and Bothe in 1924; Slater, the third author, was actually against those ideas and his contribution to that paper was more on the idea of 'virtual radiation field'). Those concepts were sources of great debates, I mean, productive debates in physics, and were tightly connected with what one could test experimentally.

We are at different times.

Christine

77. **Peter Shor**
April 23, 2008

As far as I can tell, string theory as a model of quantum gravity has so far not produced a complete resolution to the black hole information paradox: that is, nobody has explained exactly how information gets out of a black hole convincingly enough to be widely believed. What is more disturbing, some string theorists say they consider this problem to be "solved."

78. **David B.**
April 23, 2008

Dear Peter (Shor):

The most conservative claim in string theory is that there are examples, like in the AdS/CFT correspondence, where the complete non-perturbative definition of quantum gravity (realized as a string theory) on some spacetime is equivalent as a quantum system to a dual unitary quantum field theory without gravity on the asymptotic boundary of the spacetime. The claim is then that unitarity
is preserved in the full dual theory and therefore the information gets out of the black hole, somehow. In these examples the information presumably gets so scrambled by interactions that it becomes a complicated dynamical problem to see how much information one can extract about the initial state from a few obersvations after a small black hole has evaporated. This problem is not too different from burning a piece of paper with some writing on it, and reconstructing the writing from the smoke that one collects afterwards.

In the end, no one has a proof for the general case. But in essence the problem is solved in examples in the same sense that an abstract proof of existence of some mathematical object gives you very litttle information as to what the object actually can look like: it is a non-constructive proof that information gets out. I think everyone agrees that it would be a lot nicer if one could understand the same problem by solving the dynamics.

79. **Coin**
April 23, 2008

This indicates that beam commissioning is likely to begin in July, not June.

If this is true then when are we looking at for first collisions?

80. **Peter Woit**
April 23, 2008

Coin,

Their nominal beam commissioning plan refers to taking 30 days of work on the beam to get ready for physics, with likely down-time meaning it would take on average two calendar days for each day of work. So, this puts them with a physics run starting in September. Maybe it will go more quickly, maybe they'll run into problems, we'll see.

81. **nbutsomebody**
April 23, 2008

David B. and Peter Shor,
It is true that AdS/CFT provided a way to understand information paradox, but we certainly need to have a better understanding of the information paradox in the gravity itself. Like what is really happening in a AdS black hole?. Gauge theory should guide us in this manner. But the whole picture is not yet clear.

82. **Moshe**
April 23, 2008

Peter Shor, in addition to what David is saying, let me ask you a question, which I ask everyone skeptical of claims regarding the information paradox. Suppose we accept for the moment the picture of black hole evaporation suggested by AdS/CFT. In that picture the process is no more mysterious, but no less complicated, than any detailed dynamical process in strongly coupled many-body system. Under these circumstances, what kind of result or argument would you consider to be a satisfactory solution to the information paradox?

83. **oohay**
April 23, 2008

Moshe, Prof Shor's observation has two parts. Your point addresses the first part very well indeed; but it only makes his second point all the stronger:

“What is more disturbing, some string theorists say they consider this problem to be “solved.””

The point is that the AdS/CFT resolution of the problem relies completely on the structure of AdS, which is an utterly unrealistic background. In fact, it would be quite reasonable to guess that, since the asymptotic structure of the *real* world is nothing like that of AdS, string theory probably *does not* solve the information problem in the real world! That is, the dependence of the string-theoretic explanation on the specific structure of AdS is both a plus and a minus. Now actually I myself don’t believe that this guess is right. A fair assessment is that there are promising signs but that we are still far, far, far away from solving the problem for black holes on realistic backgrounds. [Of course work has been done, but the problem with it is precisely that it does not rely on AdS/CFT !]

84. **Aaron Bergman**
April 23, 2008

It all depends on what question you are asking. If the question is, does black hole evaporation violate unitarity, string theory tells you that the answer is no. Ergo, no paradox. However, if you ask exactly where the semi-classical intuition breaks down and where the information is encoded, then string theory has not yet produced a definite answer. However, there do exist proposals (note 2 links).

85. **Moshe**
April 23, 2008

I was not making a point, I was asking a question. I think a person with experience dealing with quantum information may come up with an answer to my question I may not anticipate as a high energy theorist, so I thought I’d ask…

86. **nbutsomebody**
April 24, 2008

oohay,
Questions like black hole entropy and Hawking radiation is probably a local property of the black hole and does not in essence depend on the asymptotic geometry. I may be mistaken but I would suggest that you consult Hawking’s original papers and recent reviews.

Aaron Bergman,
Exactly!

87. **Dan**
April 24, 2008

PW,

Quick question — based on what you’ve read and your experience, when do you think LHC will be ready to start collecting quality data, which should shed light on EW breaking, SUSY, condensates etc.

thanks
Regards,
Dan

88. **Peter Woit**
April 24, 2008

Dan,

For a more informed opinion about this, see

http://online.kitp.ucsb.edu/online/lhc08/huston/

My understanding is that the easiest thing to see is colored superpartners, since they are strongly interacting, and if these exist within the LHC reach, they should be found quickly, even in data from the hoped-for 2008 first physics run. As an uninformed guess, if there is significant data in 2008, sometime in 2009 the experiments would be ready to announce such a discovery.

I hear the Higgs is much harder, requiring a lot of luminosity and good understanding of backgrounds. If it’s there and the LHC works well in 2009, maybe there would be a discovery announcement in 2010.

If there is no Higgs, but something else is going on, unless there’s a dramatic signal, I’d also guess it wouldn’t be until at least 2010 before something was announced.

But I’m no expert on this, would prefer to hear from one…

89. **piscator**
April 24, 2008

I’m no expert either, but I have heard it said that the missing energy trigger, which is crucial in many of the standard susy searches (as to cut the SM background you trigger on the large missing E_T carried off by the stable LSPs), will take a long time (maybe a year) to commission. This is because at first lots of detector bugs, such as a malfunctioning calorimeter, will look like missing E_T.

I recall being in a talk where someone gave the timescale from when the Tevatron run II started to when they first published data using a missing E_T trigger. I forget the exact timescale but seem to recall it as being 18-24 months.

So even if susy is present, and even if it is light, and even if it is on the SPS point 1A slope, it doesn’t mean that it will be discovered through the first three months of data taking. And of course: `LHC discovers new coloured stuff’ is not the same as `LHC discovers susy’.

90. **Peter Shor**
April 24, 2008

Aaron – many thanks for the excellent clarification.

Moshe – if you take an ordinary dynamical process (burning an encyclopedia, say), there is no discrepancy depending on which reference frame you are in. If you consider the viewpoint somebody falling into a black hole, what they experience (nothing, when they pass the horizon) seems at odds with what is experienced by an observer outside the black hole (the information contained in the object falling in is somehow transferred to the Hawking radiation that is exiting). There are a number fo proposals about how to reconcile these viewpoints, but (a) they seem contradictory and (b) as far as I can tell, the community hasn't really accepted any of them.

91. **chethan krishnan**
April 24, 2008

Dear Peter Shor,

Here is an approximate answer to precisely your question:

http://arxiv.org/abs/0705.3828

I am not very sure how the community views this, but it seems pretty reasonable so I think it is not too controversial.

Chethan.

92. **Moshe**
April 24, 2008

Hi Peter, as Aaron says it depends what you call the information paradox, and I thought "breakdown of predictability in gravitational collapse" pretty much sums it up. In that case AdS/CFT suggests there is no such breakdown for an outside observer, though maybe it is interesting to make the case (that the information is fully contained in the Hawking radiation) more explicit.

The view of an in-falling observer is indeed much less clear, but I am wondering what is the precise problem implicit in "discrepancy" (beyond just seeking a convenient mental image, aka a classical limit). In any event, I am not sure any such issues are conventionally referred to as the information paradox. Maybe I'm wrong, it's just semantics anyhow.

(As a side comment, Mathur's proposals are of course fascinating, but in my mind have not reached the point of being uncontroversial).

93. **Peter Shor**
April 24, 2008

The question is: how does the information get transfered from the infalling object to the Hawking radiation when, in the semiclassical view, in the infalling objects' reference frame, it never actually interacts with the outgoing Hawking radiation? Obviously, if string theory is a consistent theory of quantum gravity, something must be wrong with the semiclassical view (or else something much more mysterious is going on, which is what Leonard Susskind seems to believe). But exactly what?

94. **Moshe**
April 24, 2008

Thanks Peter. Yeah, that is a good question, wish I knew the answer…

95. **Thomas Larsson**
April 25, 2008

One thing about observers and black holes which has bothered me for a long time is this. Hawking says that area and entropy are the same. However, area depends continuously on the observer since it undergoes length contraction, whereas entropy is the discrete number of state which presumably is the same from every observer's point of view. So, in which reference frame is the black hole's area equal to its entropy?

96. **nbutsomebody**
April 25, 2008

Thomas Larsson,
Good question. Continuity of area may really be due to semiclassical approximation and in a quantum theory of gravity area may be discrete. That is what one gets from LQG.

There are other issues also. e.g. entropy is a discrete quantity only when one using micro-canonical ensemble to calculate it, whereas an entropy calculated from a canonical ensemble is a continuous quantity. They agree for a large system but may differ for smaller ones. Hence it is a question of what ensemble one should use to get a matching with the area law. This is not really settled yet. However it does not matter for a large black hole anyway.

97. **Moshe**
April 25, 2008

Thomas: stationary observer far away from the black hole sees the entropy proportional to the black hole horizon's area. Both quantities transform under the asymptotic Lorentz transformations.

The entropy is a logarithm of an integer, and the area is the leading approximation to that entropy for large black holes. Subleading terms in this asymptotic expansion (in black hole inverse mass) are known geometric quantities (due to work of Wald and collaborators). In string theory this expansion is calculated in various cases, where comparison to exact formulas gives precise agreement. The subject is pretty recent and goes under the name of precision counting.

98. **Sidious Lord**
April 26, 2008

Thomas and Moshe: I don't think the area of the horizon transforms under asymptotic Lorentz transformations and under diffeomorphisms. The entropy does not transform either, so I don't think there's any issue of observer dependence here.

99. **Haelfix**
April 27, 2008

Assume that quantum gravity in reality is governed by a renormalizable Yang Mills theory. Lets say someone stumbled upon it by chance and showed a correct GR limit and so forth with only minor, unobservable corrections (so not quite Einstein-Hilbert but something close). This is at first glance, a perfectly plausible possibility (forget for a moment that such a thing probably does not exist).

The situation would be no different for predictivity, since you would still need to input some couplings into the theory, taken from experiment. So we would never know if it was or was not true, but just that it was minimal, nice and made good sense.

The only difference between that hypothetical and where ST is currently is that physicists already know the mathematics of YM is correct and that it has in the past described real natural processes.

But, if string theory can be independantly shown to be correct in different contexts (implying that the math is at least consistent), I fail to see the problem. Take AdS/QCD as a motivating example.

Point being, the QFT analogy is pretty good.

100. **anon.**
April 27, 2008

Haelfix, a Yang-Mills quantum gravity theory which 'showed a correct GR limit and so forth with only minor, unobservable corrections' might have similar lack of falsifiable predictions to string theory. I don't see the point of the analogy.

The critics of 'not even wrong' ideas aren't irrationally singling out string theory for criticism just because it is called string theory, but because it's become a massive public relations exercise that eclipses other ideas which contain fewer unobserved extra dimensions.

For you to invent a failure of an imaginary Yang-Mills quantum gravity to make falsifiable predictions, and to imply that this makes criticisms of string theory bogus because the failure of string theory is no more serious than the failure of the imaginary QFT you wish to use for comparison, seems specious.

It just seems a lame excuse for string theory to say that certain other theories may be failures too when it comes to experimental verification. (Otherwise, the courts would need to excuse people of misdemeanours which have also been committed by other people, on the basis that only unique crimes deserve punishment!)

101. **oohay**
April 27, 2008

Summary of the black hole discussion:
[1] There are very good reasons to believe that the evaporation of a black hole in AdS is a unitary process.
[2] Exactly how this happens is not understood, but work proceeds.
[3] We have no clue as to whether the evaporation of a realistic black hole is unitary. All we can say is [a] well, realistic black holes are still *black holes*, and if some black holes evaporate in a unitary way, well, maybe they all do, and [right out on the lunatic fringe now] [b] after all, AdS with small curvature is not so different from de Sitter spacetime; I mean, apart from the different causal structure, the fact that one has a timelike Killing vector and the other does not, and that they have totally different topologies, they are almost the same thing. Just drop the anti. *Therefore* the evaporation of a real black hole must be unitary, right? At least in certain extremely boosted Reference Frames.

102. **woit**
April 27, 2008

Haelfix,

Sure, there may be QFT theories of quantum gravity that are just as untestable or unpredictive as string theory. The reason so many physicists got interested in string theory was that it is supposed to explain something observable: particle physics. It is as a unified theory of particle physics that string theory has turned out to be a failure. It is the claims people are making comparing this failed theory of particle physics to the successful QFT approach that are ludicrous sophistry.

103. **Vince Velocci**
April 27, 2008

I'm not sure if this comment is relevant to this discussion, but I will submit it anyway. I'm not really surprised that a proposed unified theory of particle physics and gravity (string theory) isn't a UNIQUE theory of particle physics, allowing us to derive the masses of particles and the couplings of forces. I mean, why should we believe that a mathematical theory will logically and necessarily imply our universe and all its gory details? It makes sense that this theory can describe many many universes because I don't see why our universe is a logical necessity, following from some mathematical theory us humans have cooked up. I don't think pure mathematics will ever lead, uniquely, to our universe. Things have to be put in. Plus, I don't think our minds our perfect, so whatever kind of model we cook up will always be an approximation to the truth and only observation will enable us to come up with better and better models. But just as our minds are imperfect, so will our models be.
Maybe I don't really understand things all too well, but I just don't see how mathematics can uniquely imply our universe with all its constants.

104. **Eric**
April 27, 2008

"It is the claims people are making this failed theory of particle physics to the successful QFT approach that are ludicrous sophistry."

It seems to me that QFT and string theory are actually two sides of the same coin, and are essentially equivalent. Is this not what Ads/CFT teaches us? QFT and string theory are essentially dual descriptions.

The bottom line is that one can easily construct QFT's from string theory, so what goes for one also goes from the other. The only thing lacking at present is the particular construction from string theory which gives us the QFT that we observe, the standard model.

105. **Peter Woit**
April 27, 2008

Vince,

Again, the problem with string theory is not that it can't predict things uniquely. It's that it can't predict anything at all.

Eric,

According to your argument, the weakly coupled electroweak theory is a string theory since AdS/CFT says it is dual to strongly coupled string theory. But what is strongly coupled string theory? Well, the only answer to that is to apply AdS/CFT again and you have just shown that weakly coupled QFT is weakly coupled QFT…

106. **Eric**
April 27, 2008

"Again, the problem with string theory is not that it can't predict things uniquely. It's that it can't predict anything at all."

One only needs to find the particular string vacuum which describes our universe in detail. Once one has such a construction in hand, and all moduli are stabilized, then one can predict the values of all couplings, such as gauge couplings and Yukawa couplings. Also, it should also be possible to address the issue of supersymmetry breaking within such a construction, and describe any new physics.

In regards to cosmology, string theory can essentially make a predicton in regards to inflation: no tensor modes.

107. **woit**
April 27, 2008

Eric,

You're not keeping up with the latest string cosmology research. See for instance

http://arxiv.org/abs/0803.3085

where Silverstein and Westphal discuss string cosmology models with observable tensor modes.

"One only needs to find the particular string vacuum which describes our universe in detail"

You're ignoring the minor problem that you don't know how to calculate in detail the predictions of realistic string vacua, and that there are 10^{500} of these calculations you don't know how to do…

108. **Eric**
April 27, 2008

Peter,
Yes, I'm aware of the Silverstein-Westphal paper. However, this paper is still a little controversial.

"You're ignoring the minor problem that you don't know how to calculate in detail the predictions of realistic string vacua, and that there are 10^500 of these calculations you don't know how to do.."

Actually, we do know how to calculate in detail many predictions for a wide class of vacua. This is where the moduli stabilization issue comes into play. As I stated earlier, once this is done, all quantities such as the Kaeler potential and gauge kinetic function can be uniquely calculated. What is still needed is an example of a fully realistic example. Finding such an example might be closer than you realize. To be honest, your knowledge on this is about 20 years out of date.

109. **Peter Woit**
April 27, 2008

Eric,

Instead of just insulting me as ignorant, please provide me with a reference to a paper which gives a string theory background with fully stabilized moduli and a reliable calculation of Yukawa and gauge couplings, accurate enough to be compared to experiment. One of the easiest things to compute in such a background should be ground state energy, so tell me what the result of that calculation is.

110. **Eric**
April 27, 2008

Peter,
At present, there are several examples of semi-realistic vacua in the literature where many of these calculations can be performed, which I encourage you to study. As I stated previously, a particular vacuum where it can be claimed to be fully realistic has yet to be found. However, there has been a lot of progress and I don't think that it will be too long before this happens.

I should emphasize that the problem is not in calculating couplings. The issue is in finding a particular vacuum which 1) has exactly a three-generation standard model in its low-energy limit and nothing else, 2) satisfies all consistency conditions, 3) all Yukawa couplings are allowed by selection rules, and 4) all moduli may be stabilized, so that the values of all couplings may be calculated. The problem is that perhaps 3 out of 4 of these conditions may be satisfied for a particular construction, but so far not all.

111. **Peter Woit**
April 27, 2008

Eric,

I've looked at many papers of the kind you describe. From what I've seen, generically the kind of constructions you describe have vacuum energies that are wrong by a hundred orders of magnitude or more, and no hope of calculating Yukawas in a reliable manner that could be compared to experiment. If you claim otherwise, give a reference, not just over-hyped claims about what can be done, when it really can't.

112. **piscator**
April 27, 2008

Peter, seriously, if your complaint is really that there is no remotely plausible way of constructing vacua which looks like the Standard Model and where the vacuum energy can be computed to agree with experiment, then it's not just string theory you're giving up on but particle theory in its entirety. No-one knows a good solution to the cc problem. The only halfway reasonable `solution' is anthropica, and that approach (what a climbdown from the high points of particle theory) is nothing to boast about.

113. **Peter Woit**
April 27, 2008

piscator,

My point is just that the CC is the easiest thing to compute in these vacua, and as far as I can tell, you can't even calculate that in such a way that you could compare it to experiment. The state of the art seems to be that people are trying to see whether or not they can find an argument that would show existence in principle of a vacuum with small enough CC, even though it couldn't actually be constructed.

As for the rest of the continuous parameters of the standard model, e.g. Yukawas, every paper I've looked at seems to involve some kind of extremely crude approximation or other, with no reliable calculation of these numbers. I'd really like to get a reference from Eric or anyone else to a reliable calculation of Yukawas in a fully-stabilized string vacuum. People talk as if such things exist, but I haven't seen such a thing.

Seriously, I'd like to see references to somewhere such a calculation is done, or at least to somewhere which explains in detail what is needed to do such a calculation, e.g. what technical or conceptual problems still need to be solved before it is possible.

114. **dan**
April 27, 2008

Thanks for the reference, PW,

quick question — If SUSY is broken at TEV scale, thus stabilizing the EW, and providing a solution to the hierarchy problem, should the Tevatron, with its luminosity and intensity, have the energy to see some of the lighter SUSY-partners/higgs?

115. **Peter Woit**
April 27, 2008

dan,

Many people expected evidence for supersymmetry to show up at the Tevatron if it really was at low enough energy to solve the hierarchy problem. The fact that this hasn't happened is one piece of evidence against supersymmetry. As far as I know, the increases to come in Tevatron luminosity won't change much the bounds on superpartners, progress on this will have to come from the LHC.

116. **Ethan Siegel**
April 28, 2008

Funny thing; Lubos came to my site a week or two ago and left a comment there when I commented on yours. I have analytic software installed, which tells me where he came from.

And it was your site. Funny, but I think he might be one of your number one readers!

117. **woit**
April 28, 2008

Ethan,

I'm well aware that Lubos is one of my most loyal and attentive readers. Very often after I write about something here, a long denunciatory post about the issue at hand appears frighteningly quickly at his blog. It's scary how fast the guy can write huge long postings. One example is the latest one about Connes, which, while containing lots of nonsense, does contain some material worth reading, largely because Lubos is writing about something he actually has some experience of, growing up under the Soviet system as implemented in Eastern Europe.

118. **piscator**
April 28, 2008

Hi Peter,

Well, I think the cc can be computed quite easily in all the moduli stabilised backgrounds, it just that it tends to be at the natural scale you would expect based on supergravity, which depending on how you break supersymmetry is somewhere between (1 TeV)^4 and (10^7 TeV)^4. And effective field theory suggests that this is the answer you expect whatever high scale theory you are working with, as the Standard Model is valid up to a scale of a TeV. So either you need something very new and very smart, or you run away to anthropica and rely on a massive and uncalculable cancellation.

As for Yukawas: a full calculation would require knowledge of the Kahler metrics for the matter fields. This is non-holomorphic, and therefore in general very hard to compute. Fully explicit formulae exist on tori, so how to compute Yukawas is in principle something that people know how to do, but there is (IMO) no background where you have controlled moduli stabilisation and a Standard Model sector, so it's not clear comparing Yukawas with experiment is where progress lies right now.

119. **Peter Woit**
April 28, 2008

piscator,

Thanks, that's very helpful. From what I understand, except on tori you don't have explicit Calabi-Yau metrics, would need to get them numerically and this is very challenging. Can you get fully-stabilized moduli on a torus or do you need some other Calabi-Yau?

120. **piscator**
April 28, 2008

Hi Peter,

You may be able to get fully stabilised moduli on a torus, but I don't know of any schemes there that really look plausible for breaking supersymmetry at a hierarchically low scale. Straight flux models with no non-perturbative effects may be able to stabilise everything on a torus, but then there is no obvious small parameter and the flux stabilisation will likely give you a gravitino mass (and thus susy breaking) around the GUT scale or so.

On a Calabi-Yau, it may be possible to be fully explicit for local models, such as branes at singularities or resolutions thereof. Metrics are in some cases known there, and at the orbifold limit the string can be quantised. That said, I do not believe that full string calculations for the Kahler metrics have been carried out there, but it may be possible.

For traditional vanilla global compactifications, such as the E_8 x E_8 heterotic string on a Calabi-Yau, then I would have expected that computing the full expression for the Kahler metric of chiral matter fields would be of the same order of difficulty as computing the Calabi-Yau metric itself.

The superpotential is of course more tractable, and so some progress can be made purely through selection rules – if a Yukawa vanishes, it still vanishes whatever the normalisation is. Likewise if there are gauge symmetries and Froggatt-Nielsen type attempts at flavour are possible, assuming that all normalisation terms are O(1).

121. **Peter Woit**

April 28, 2008

Thanks again piscator, that's very helpful. I've periodically tried to understand a bit about what the problems are in these kinds of calculations, your summary made the situation much clearer.

122. **dan**
April 29, 2008

Dear PW,

Sticking back to the topic 'My understanding is that the easiest thing to see is colored superpartners, since they are strongly interacting, and if these exist within the LHC reach, they should be found quickly, even in data from the hoped-for 2008 first physics run."

so by the end of this year, 2008, around Nov election time, if there are colored superpartners (superpartners of colored quarks I take it?) the LHC could see evidence of SUSY (which string theorists would take as proof of the correctness of String's foundation)

Anything else in the first hoped for 2008 run?

123. **Peter Woit**
April 29, 2008

dan,

Even if something like colored superpartners are there in the 2008 data, it may take quite a while for the experiments to properly analyze this data, and have enough confidence that they are seeing something like this to make an announcement.

124. **Dan**
April 29, 2008

PW,
Okay, thanks. When you write "and if these exist within the LHC reach, they should be found quickly" is there any reason to suspect that if SUSY does stablize EW scale, the LHC does not have enough energy to produce such strongly interacting colored susy-partners?

If you don't believe in SUSY as the explanation for hierarchy (and neither, presumably, does Eva Silverstein, with other string theorists offering LHC-accessible SUSy a 50-50%), how do you (they) propose the EW-higgs is stabilized? curious
dan

125. **David B.**
April 29, 2008

Dear Dan:

Alternatyive B to a technically natural explanation of the hierarchy problem is that the EW scale is stabilized because it is finely tuned. This means, no real explantion. This is a possibility that can not be discarded, although there is quite a bit of theoretical prejudice against it.

Various people have used landscape-based arguments to say that this is indeed the case, because in the landscape sense, fine tunings can be accidental environmental parameters.

This will be settled one way or another by the LHC, as it is the first machine that has been built that can reach and explore the EW symmetry breaking scale.

126. **milkshake**
April 29, 2008

I was reading the Lubos rant about Connes and even the part when he supposedly "knows what he is writting about" is full of it, as always, when he explained why soviet physics was not affected.

Turns out Soviet physic was very nearly wiped out and replaced by "research on ether" the same way the biology and was replaced by teachings of Lysenko. There was actually a congress organised by soviet acad sci establishment, in 1946 I think, to purge soviet science of burgeois theories like relativity and uncertainity, and to put the young generation in line with the Party line on science – all scripted out, all denunciations prepared in advance – and it was called off just few weeks before it was when it became clear this would interfere with soviet bomb effort. "We can always shoot them later" was the Stalins comment on his decision to call it off.

127. **dan**
April 29, 2008

Thanks Dave B.

I take it we're looking 2010 at the earliest then for LHC to provide the requisite data & analysis?

128. **innocentbystander**
April 30, 2008

Eric,

I wish I could share your optimism regarding general predictions
from string theory. You mention two general
predictions: 4d SUSY (which you tacitly
assume in describing the problem in terms of a Kahler potential)
and absence of tensor modes. Please indicate
how you would establish either of these predictions.
Finding *a* vacuum with the right properties obviously
is not sufficient, since there may be many more
which match current data but differ in other ways.
And as Peter said, on tensor modes there is now a class
of inflation models, which is at least as concrete as any

other known class, in which a specific
chaotic inflation type potential and prediction of of tensor modes drops out. How can you possibly exclude this possibility on general grounds?

129. **Eric**
April 30, 2008

Dear Innocentbystander,
First, 4D supersymmetry may be established by the detection of superpartners at LHC. Second, finding a vacuum which reproduces the SM plus new physics that is later observed would establish the model.

Examples of new physics might include the superpartner spectrum and extra matter which may arise from a hidden sector. Once a fully realistic model is constructed, the issue of supersymmetry breaking should be able to be addressed and it should be possible to calculate the soft-terms and therefore the low-energy superpartner spectrum.

As far as your question regarding the uniqueness of such a vacuum, nobody knows at this point. At present, there is not even one such model which can be claimed to be fully realistic, so the claim that there will be multiple vacua which reproduce the SM in detail is
insupportable.

Regarding the tensor modes, all I can say is that the paper which Peter referred to is currently being studied. It has not yet been accepted as valid. Many people who work on this topic are presently trying to find a problem with it, which is how the scientific process works.

130. **innocentbystander**
April 30, 2008

Dear Eric,

Sure, what you say makes sense, and applies
to all string cosmology models. As such, at
present there
is not a general prediction from theory regarding tensor modes.
(This would be the case even
if there were not a candidate mechanism for
tensor modes in the literature.)
Different mechanisms make different predictions, and
those which appear in advance of the next round of
observations in fact provide cosmological
examples along the lines you suggest for
particle physics (that is, examples of models
which are currently viable, and can be observationally tested in
the foreseeable future).
As with SUSY, the experiments will decide this question.

131. **dan**
April 30, 2008

Dear Eric,
"First, 4D supersymmetry may be established by the detection of superpartners at LHC."

As string theorist Eva Silverstein pointed out, she and other string theorists think there's a good chance LHC won't find any evidence of SUSY. While it's understandable that Tevatron may not have enough energies to explore EW scale, what will become of string theory/M-theory in the event of an LHC SUSY-null result? (i.e finding a single higgs and nothing else). Does Eva and other statements reflect this possibility, and hedging the bets of the future of string community?

132. **Eric**
April 30, 2008

Hi Dan,
I don't think that most string theorists believe that LHC won't find any evidence for SUSY. Only those who've become enamored of fine-tuning think this way. For the most part, Silverstein, Kachru, etc., I think prefer this over supersymmetry because they didn't invent supersymmetry. On the contrary, they invented fine-tuned models such as KKLT, and because they have so much confidence in themselves, believe these ideas must be right.

133. **dan**
April 30, 2008

Hi Eric,

well NIMA ARKANI-HAMED "My hunch is that there's a better than evens chance that supersymmetry will show up at the LHC"

I've heard quotes from some Tevatron experimentalist the odds of finding SUSY are more like 5%. Could it be Eva invented such models to provide string theorists a way out should LHC provide a null SUSY result?

If LHC does provide a null SUSY result, and finds only a single higgs plus SM only, how will affect the future prospects of string theory research program? What would it mean to both education and string theory research to say that for every fermion in the SM, there is a SUSYpartner boson, not seen even by LHC energies. What does string theory community have as an alternative in the event of this experimental result?

134. **Eric**
May 1, 2008

Dan,
Arkani-Hamed has his own favorite alternative to low-energy supersymetry, namely split supersymmetry with large extra dimensions. It's not surprising that he wants his own model to be true rather than low-energy supersymmetry.

As far as the 5% figure you are quoting, I believe that may be the odds for discovery at the Tevatron. At LHC,I would put the odds at at least 50%. If low-energy supersymmetry is not discovered at LHC, then it is not the solution to the hierarchy problem. In this case, one may expect one of the alternatives to show up, either large extra dimensions or technicolor. A very, very small chance that only the SM Higgs and nothing else is found.

At this point, all signs point towards low-energy SUSY.

135. **anon.**
May 1, 2008

Eric said:

split supersymmetry with large extra dimensions.

Huh? Those are two completely different and unrelated things, and there's no good reason to put them together. Are you referring to an actual paper, or just combining buzzwords?

Also, KKLT isn't inconsistent with TeV-scale supersymmetry. The fine-tuning is in the cosmological constant; no one has yet figured out an alternative solution to that one….

You seem strangely eager to assign suspect motivations to perfectly respectable physicists.

136. **Eric**
May 1, 2008

Anon,
Sorry, I meant to say split susy or large extra dimensions, both ideas of which AH has contributed. Regarding KKLT, the point I was trying to make is that KKLT is a fine-tuned solution to the cc problem, and if you believe this is how nature works, then you are free to believe that the electroweak scale is fine-tuned.

As far as my eagerness to assign suspect motivations, please calm down. We know how people are and how the real world works. As far as the public statements of the aforementioned persons regarding supersymmetry, my opinion is that they are simply pushing their own ideas and dissing the main competition.

137. **dan**
May 1, 2008

Eric
"At this point, all signs point towards low-energy SUSY."

What signs do you refer to? g2 muon magnetic dipole moment, or something else (or more)

138. **anonymous**
May 2, 2008

Regarding the public statements on SUSY: this part of
the discussion
started with a quote that Silverstein "would not be
devastated if SUSY is not found". The end of her quote
in the telegraph article is something like "On the other hand, supersymmetry fits well with some existing
observations, and it will be
spectacular to finally learn whether it arises." How do you
go from this "not devastated" statement
to claiming she (or anyone else) is making
a prediction of no SUSY, based on ill motivations to boot?
I have heard Eva
emphasize in talks that SUSY requires rather special choices of
compactification manifold, etc., and that there are
arguments going both ways in terms of which is
easier to construct from string theory and so on;
she also writes papers on
SUSY model building some of the time, citing the usual
motivations. Anyway we need the LHC to tell us.

139. **Eric**
May 3, 2008

Anonymous,
I don't assign any ill motivations to Eva or Nima, only that they, like most people, have a built-in bias towards their own favorite ideas. It's obvious to anyone who reads between the lines that they prefer one of the alternatives to low-energy supersymmetry to be discovered at LHC. This is not to say that they are trying to mislead anyone. It is in fact possible that one of these alternatives will turn out to be right. However, one shouldn't read these statements as if they know something about the prospects for finding supersymmetry that the rest of us don't.

In any case, their 'insight' is most likely based on their current knowledge of the landscape of flux compactifications, where it's quite easy to find vacua with a low string scale (the large volume models) or where supersymmetry is broken at a very high scale. However, there are no fully realistic models where this occurs, so noone should take these ideas too seriously. Indeed, until there is a completely worked out example or examples of the low-energy physics we observe, it's simply impossible to make any judgments at all. To do so puts too much faith in our current understanding of string theory, which is far from complete.

Dan,
There are lots of hints that low-energy supersymmetry exist. Gauge coupling unification, g-2 for the muon, and so on are some of these hints. However, I think the indications from experiment of where to expect the Higgs mass are the biggest hints that the MSSM is the correct model.

140. **Urs Schreiber**
May 4, 2008

> g-2 for the muon

What is the latest state of affairs concerning this? In

http://arxiv.org/abs/hep-ph/0703125

I see that a deviation of by now 3.2 sigma has been reached. At the time of the writing of the review

http://arxiv.org/abs/hep-ph/0609168

of Muon g-2 implications on susy physics just a few months earlier, it was still reported as being 2 sigma.

So I suppose it is clear by now that there is a real beyond-SM effect seen in Brookhaven?

What's the implication for the susy parameter space as searchable by LHC?

This and That

April 27, 2008
Categories: Uncategorized

The interactions.org web-site has a new useful feature, Interactions Blog Watch, which aggregates links to recent physics-related blog entries. One of the older such aggregators I know of is Mixed States, but it seems to have stopped on March 15. There's also Jacques Distler's Planet Musings, where he continues his efforts to pretend "Not Even Wrong" doesn't exist.

Vanity Fair seems to think that the right person to review a book about Isaac Newton is Christopher Hitchens. Hitchens devotes much of the piece to condemning Newton as "a crank and a recluse and a religious bigot" who "spent much of his time dwelling in a self-generated fog of superstition and crankery." He feels the same way about most scientists before the modern era, noting that:

> It may not be until we get to Albert Einstein that we find a true scientist who is also a sane and lucid person with a genial humanism as part of his world outlook—and even Einstein was soft on Stalin and the Soviet Union.

He ends the piece by accusing Newton of doing everything he could to keep people from understanding the universe, and claiming that this was typical of physicists until recently, when physics began to become indistinguishable from the humanities:

> Newton was a friend of all mysticism and a lover of the occult who desired at all costs to keep the secrets of the temple and to prevent the universe from becoming a known quantity. For all that, he did generate a great deal more light than he had intended, and the day is not far off when we will be able to contemplate physics as another department—perhaps the most dynamic department—of the humanities. I would never have believed this when I first despairingly tried to lap the water of Cambridge, but that was before Carl Sagan and Lawrence Krauss and Steven Weinberg and Stephen Hawking fused language and science (and humor) and clambered up to stand, as Newton himself once phrased it, "on the shoulders of giants."

Hitchens doesn't mention Michio Kaku, who has a new book out The Physics of the Impossible, which is on the New York Times bestseller list with the blurb:

> A theoretical physicist who is one of the founders of string theory discusses the possibility of phenomena like force fields, teleportation and time travel.

The notion that Kaku is a "founder of string theory" seems to be becoming very widespread in the media.

Over at Cosmic Variance, Sean Carroll and various of his anonymous commenters are upset that Lee Smolin made it onto a list of Top 100 Public Intellectuals, with some suggesting that Kaku deserves to be there instead.

Finally, the latest Newsletter of the European Mathematical Society has the second part of an interview with Alain Connes, who has many interesting things to say:

> they work in huge groups and the amount of time they spend on a given topic is quite short. At a given time t, most of them are going to be working on the same problem, and the preprints which will appear on the web are going to have more or less the same introduction. There is a given theme, and a large number of articles are variations on that theme, but it does not last long…
>
> The sociology of science was deeply traumatized by the disappearance of the Soviet Union and of the scientific counterweight that it created with respect to the overwhelming power of the US. What I have observed during the last two decades since the fall of the USSR and the emigration of their scientific elite to the States is that there is no longer a counterweight. At this point, if you take young physicists in the US, they know that, at some point, they will need a recommendation written by one of the big shots in the country, and this means that if one of them wants to work outside string theory he (or she) won't find a job. In this way there is just one dominant theory and it attracts all the best students.
>
> I heard some string theorists say: "if some other theory works we will call it string theory", which shows they have won the sociological war. The ridiculous recent episode of the "exceptionally simple theory of everything" has shown that there is no credibility in the opponents of string theory in the US. Earlier with the Soviet Union, there was resistance. If Europe were stronger, it could resist. Unfortunately there is a latent herd instinct of Europeans, particularly in theoretical physics. Many European universities, at least in France or England, instead of developing original domains as opposed to those dominant in the United States, simply want to follow and call the big shots in the US to decide whom to hire…
>
> I don't think that we see similar things in mathematics, so there is a fundamental sociological difference between mathematics and physics. Mathematicians seem very resistant to losing their identity and following fashion…
>
> In physics I adore reading; I spent about fifteen years studying the book of Schwinger, *Selected Papers on Quantum Electrodynamics*. He collected all the crucial articles, by Dirac, Feynman, Schwinger himself, Bethe, Lamb, Fermi, all the fundamental papers on quantum field theory, those of Heisenberg too, of course. This has been my bedside book for years and years. Because I have always been fascinated by the subject and I wanted to understand it. And that took a very long time to understand.

Comments

1. **Bee**
   April 27, 2008

   Hi Peter,

   Thanks for the Connes quote, that is interesting. Regarding his assertion

   "Unfortunately there is a latent herd instinct of Europeans, particularly in theoretical physics. Many European universities, at least in France or England, instead of developing original domains as opposed to those dominant in the United States, simply want to follow and call the big shots in the US to decide whom to hire…"

   I can't quite agree on that for Germany. Yes, there is definitely that trend that Germans should do what the Americans do because they are just so cool, but at least in academia this isn't such a pronounced influence and as far as I can tell it's been getting weaker lately. The reason I guess is mostly that Germans are (in my impression, on the average, apologies to every exception) rather conservative, cautios and suspicious when it comes to 'new' ideas. It's for that reason I believe that many young researchers have left Germany for North America where research is more progressive, and the academic system not as conservative. These 'fashion' trends are considerably more pronounced in the USA – and that not only in research, it's a far more general sociological phenomenon that I believe just reflects in the scientific community. I don't generally think it is bad to have these trends. It is a good idea to have some established stuff, but also to have some topics of the months to try out new things. But I think the balance is somewhat off here (that being related to the short-time funding I believe).

But anyway, the problem that one needs to have letters and to produce papers isn't related to these fashion trends, and exists independent of that. Best,

B.

2. **Peter Woit**
April 27, 2008

Hi Bee,

I think that the remarks by Connes about US theory faddishness are a bit out of date, since the subject seems to have gone into a funk around 2000, and since then fads have been having trouble getting off the ground. Crudely, the problem is that string theory has become too moribund to generate an idea interesting enough to create a fad, but the fad-generating powers-that-be refuse to give up on string theory and try other things.

As for his comments about Europe, note that he refers especially to England and France, perhaps well aware that things have been different in Germany. When I was in Italy last year and talked to quite a few Italian physicists, their attitude seemed to be that the string theory hype was an American phenomenon, that Italy had never had the over-hyped fad problem with the subject that the US had.

Even here in the US, I think that in the past year or two, attitudes have changed dramatically, with string theory starting to become quite unpopular among most physics departments. It will be interesting to see how this plays out. It is definitely becoming true that someone most concerned about their career prospects is going to be thinking twice before promoting themselves purely as a string theorist.

3. **Peter Woit**
April 27, 2008

Bee,

One other comment. I increasingly find it odd to see string theory set up as a progressive "new idea", contrasted to conservative "old ideas". It has dominated the subject in the US now for nearly 24 years, since before most of our graduate students were even born, and the physicists who started string theory research almost 40 years ago are now pretty much all old enough to be collecting Social Security. How old does an idea about physics have to get before it no longer is a "new idea"?

4. **Bee**
April 27, 2008

Hi Peter,

I wasn't referring to string theory in particular, but to fashionable topics more generally (the extra dimensional stuff e.g. or the unparticles, both of which originate in the USA and seem to abundantly happen there). I noticed Connes was speaking about France and England, that's why I meant to add this isn't all of Europe. From the German perspective I too would have said the string theory hype is an American phenomenon (anybody knows a hype which isn't?). I also didn't call string theory a 'new' idea, though it was 'new' at some point and in my impressions the Germans where considerably slower jumping on the train than the Americans. I guess an idea stops being 'new' when you find it in the obituaries 😉 Best,

B.

5. **Bee**
April 27, 2008

PS: *"It is definitely becoming true that someone most concerned about their career prospects is going to be thinking twice before promoting themselves purely as a string theorist."*

Indeed, I too noticed a certain change in attitude (people assuring they are no string theorists/don't want to become string theorists). I am not sure though whether this is desirable, or where it will go. It might just backlash – the public sympathy is often with those in the defensive.

6. **JC**
April 27, 2008

Bee,

Were there any supergravity and/or string theorists appointed to German professorships before the Berlin Wall came down and German reunification? From what I can recall, I don't really remember many German university professors doing string theory before 1989-1990. (At least nobody who was famous).

7. **Allan**
April 27, 2008

The article by Hitchens is useful
at least for its first paragraph
where Hitchens admits that he is stupid,
something many of us have suspected for some time.

8. **Joseph Triscari**
April 27, 2008

Connes's belief that the Soviet Union was some sort of moderating counterweight to American science is downright silly. I was a genetics major in the 80's and it was widely understood that genetics from behind the iron curtain had not yet recovered from Lysenko. I vaguely recall there being resistance to the Big Bang Theory in the Soviet Union because it smacked too much of a Creation which might be interpreted incorrectly as confirming religion.

I don't have a list of Soviet silliness but, I really doubt the Soviet Union was influencing science in a positive manner because of its existence as a super-power.

I think his belief that mathematicians are above fads is just as wrong. The history of math is filled with times where some areas are neglected to the benefit of others. Math has the advantage of being able to generate ideas by axiomatic proof which makes the damage less when the fad ends. I doubt very much fad mentality is correlated to which department you find yourself in.

The problem with quantum gravity – as I believe has been said repeatedly by others – is that experiment is not supporting theory as well as it has in the past. Math doesn't have this problem and other areas in physics and science don't have this problem (they have others).

9. **dan**
April 27, 2008

Dear PW

"The ridiculous recent episode of the "exceptionally simple theory of everything" has shown that there is no credibility in the opponents of string theory in the US."

What did Connes find "ridiculous" over Lisi's ESTOE and how does this show "there is no credibility in the opponents of string theory in the US"?

10. **Peter Woit**
April 27, 2008

dan,

Interesting question. I don't know what Connes thinks of the Lisi story, or what he means by that comment.

Joseph,

In many areas of science, I don't think the former Soviet Union was of much influence. But in theoretical physics and mathematics, they had a very strong presence, with a large number of very good people, working to some extent isolated from the West, and within a different tradition and environment. It really was in many ways a separate, strong culture, and its existence was significant for how math and physics developed. With the fall of the Soviet Union and the emigration of many of its best people, this changed dramatically.

11. **Eric**
April 27, 2008

"From the German perspective I too would have said the string theory hype is an American phenomenon (anybody knows a hype which isn't?)."

And yet, I know of several German string theorists, two of whom are coauthors of a string theory textbook.

12. **anonmo**
April 28, 2008

Unless I missed it, Sean Carroll didn't seem to be complaining about Smolin being on the list. In fact he compliments him in the comment thread.

13. **Joseph Triscari**
April 28, 2008

Peter,

I don't disagree with a word you said. Let me emphasize: Some scientists (mathematicians and physicists in particular) in the Soviet Union did tremendous work with the resources they had available. Their contributions are all the more heroic given conditions under which these contributions were generated. That's a far cry from saying science (or math or physics) was "traumatized" by the loss of the Soviet Union.

The Soviet Union's contributions to math were, in my opinion, greater than their contributions to physics but we're not dealing with the same problem in the math community. If the loss of the Soviet Union, meant that a "counterweight" to wrong headed thinking disappeared, we'd have the same problems in the math community as the physics community – possibly worse. Connes seems to recognize this and tries to inoculate his argument by saying mathematicians have a higher resistance to fads. As I've said, I doubt that such a resistance exists and history doesn't seem support that belief.

Connes seems to be saying that, with the Soviet Union present, we wouldn't be enduring this current string theory fad. Maybe. Maybe we'd have two silly fads to deal with – one based on correct communist thinking. Maybe more. Who can say? What I do know is that the loss of the Soviet Union was not a "trauma" to science or math or physics but a relief. Science was not stifled by the loss of the Soviet Union. It was enriched.

I cannot see how the existence of the Soviet Union would have prevented the current string theory fad except in some random future time-line way. I cannot see a plausible mechanism by which young physicists would be able to pursue interests in quantum gravity outside string theory even if the Soviet Union were present pursuing something else – even if that something else was the objectively correct theory. Would they be getting grants from the Soviet Union? Would the move to to the Soviet Union to pursue their interests? What experiments would be supporting that theory?

If the problem is that European funding doesn't come without implicit approval of one super power or another, then that's a problem that can and should be solved without enslaving thousands of scientists.

14. **chris**
April 28, 2008

spinning?

This is what sean carroll said:

Speaking only for myself, I don't think that I "give Lee Smolin s**t." I disagree with him strongly on certain matters of substance, and I don't think that his presentation of the state of play in modern theoretical physics is especially accurate. But I applaud him very sincerely for his efforts to talk to a wider audience — I very much wish that others would also do so.

doesn't soound hostile to me, now does it? and in fact, one may really wonder why Lee Smolin is more of a public intelectual than Stephen Hawking, no?

15. **simple z**
April 28, 2008

Thanks for the quotes!
I remember Hawking doing the very same thing to Newton:
making him appear like a complete monster.

(think this was in "A Brief History of Time")

16. **MathPhys**

April 28, 2008

I recently saw a full page interview with M Kaku in a local newspaper so I suspect similar articles have appeard all over the world. His claims are very carefully worded.

Roughly speaking, he refers to himself as "One of the pioneers of string theory". I took that to refer to the papers of Kaku and Yu from the early 70's on loop calculations. They were not bad papers, but they were not pioneering either. They followed other people's works.

He also refers to hismelf as "one of the founders of string FIELD theory". The emphasis here is on FIELD. I take that to refer to a couple of papers that he wrote on the subject. Siegel and Witten were the true pioneers, but the subject didn't get very far anyway.

He also talks about pioneering a different theory and later on to realize that "his theory" coincides with strings. I take that to be a reference to his on conformal supergravity. Here too, the real pioneers are the supergravity people.

His statements are so carefully worded to give the layman a certain impression, but in such a way that an expert cannot say that he's making things up. He's just very misleading.

Very interestingly, the interviewer asks him if his very frequent public appearances and commercial book writing (he must have written half a dozen by now) aren't taking too much time that could be devoted to scientific research. He "gently smiles" and reminds us that Einstein played the violin.

If statements like that aren't misleading, what is?

17. **Peter Woit**
April 28, 2008

Eric,

The two physicists you have in mind may be German, but they are pursuing their careers in the US.

anonomo and Chris,

The first comment in the thread is a nasty anonymous one unfairly attacking Smolin (many respectable people agree with him, and he has published many peer-reviewed papers):

Smolin… so "top public intellectual" can mean "one who disagrees with nearly every respectable person in his field, and instead of persuading others through peer-reviewed literature, appeals to the general public for support"?

Sean's response is not to defend Smolin, but to agree with the anonymous attacker, complaining only that people haven't disagreed with Smolin more vigorously.

If the "respectable" people can't be bothered to explain themselves compellingly to the general public — then, yeah.

18. **Bee**
April 28, 2008

Hi Eric, Peter,

Of course there are string theorists in Germany (e.g. Lüst, Nilles, Louis, etc), and also Germans in the US. I said I think the Germans were slower jumping on the topic than the Americans, and I can't say I've noticed much of a hype.

Hi JC,

I honestly don't know. I was 13 then, and I wasn't particularly interested in string theory.

Best,

B.

19. **Eric**
April 28, 2008

Peter,
Yes, but non-string theorists such as Sabine Hossenfelder are also pursuing a career in North America. So, maybe there is some conservatism in Germany, but it isn't restricted to string theory. From other conversations I've had, the real issue is that proferships in Germany tend to be given to old men who've been at it a while, whereas in the US/Canada, it's possible for younger people with new ideas to receive advancement.

20. **Bee**
April 28, 2008

Btw, regarding this comment section over at CV, I find this pretty much disgusting. Once again, I can't understand why Sean just lets people (even anonymously) bash others for no good reason whatsoever. This list isn't a list with scientists who've made a good deal publicizing their work, and it also isn't a list of the most influential researchers in the respective field. Lee isn't on that list because he's "manufactured" a "public controversy", as another anonymous commenter found it necessary to proclaim. Didn't anybody actually read his books? Or the articles that one finds online, the stuff on the website etc? Doesn't anybody realize that a discussion about which environment scientific research needs to function best is necessary, overdue, and should have been lead probably a decade ago? Doesn't anybody see how this polarization into the string-haters and string-lovers completely distracts from the actual problem that is the circumstances under which scientists do their research? And how Sean promotes this? Can't all these people just please look somewhat farther than the tip of their own nose?

21. **Bee**
April 28, 2008

Hi Eric,

Yes, that's what I meant to say. In my perception Germans are more conservative than Americans (again, restrictions apply, exceptions exist). And yes, the reason why I moved to the USA was that I couldn't find a place to fit in. The German academic system has several problems, some of which are different from those in the USA, some are similar. E.g. there is the usual problem of networking, if you have connections, know the right people (preferably those in the hiring committee), have been in the right places, you have a much easier time getting a job. That I believe is quite similar to the USA. One thing that's different is that there is no tenure track, so people hang in thin air until they eventually make it on a professorship which is a livetime position (you can't be fired). I don't think though Germany has a specific problem with women. The reason why I think there are little female professors is what I just said: you have no secure position possibly until your late thirties. Everybody who wants family must be appalled by this idea. Another thing that's different is that a professorship in Germany requires an additional academic degree called habilitation. They are trying to change this, but this discussion goes back a long time and hasn't been very fruitful so far. Best,

B.

22. **Bee**
April 28, 2008

Err, I mean 'few female professors'

23. **David Nataf**
April 28, 2008

Bee,

Late thirties doesn't sound too different from North America.

Say one finishes graduate school at 27, and then does two postdocs for six years (both optimistic assumptions), one will then get an assistant professorship at 33 and get tenure (if things work out) at 43.

Between undergrad through professorship that implies a minimum of five different locations to live in. Hardly the optimal situation in which to raise a family… and now as more and more scientists date other scientists, two-body problems will become more frequent.

24. **Professor R**
April 28, 2008

Hi Peter, slightly off-topic for discussion above, but on-topic for your original post.
I've finally got around to starting a physics blog (mainly concerned with cosmology and particle physics at an introductory level). I've listed NEW as a link I use myself if that's ok. Have a look at

http://coraifeartaigh.wordpress.com/

would appreciate any comments/tips.
Regards Cormac

25. **chris**
April 28, 2008

hi Bee,

seems you are away from germany for quite a while now. there is tenure track now – just starting but many positions are out there already. and from a family point of view – i am not too excited about the prospects of financing 3 children until grad school in the us on a mere academic salary or two. germany is way more attractive from this point of view. what is really true is the networking however. smaller countries have less people so inevitably there will be more room for this kind of stuff.

and on a very different note, i find it amusing how harsh you and peter woit are with cosmic variance. sean has not said one bad word about lee smolin there. there is a certain tendency of course, but come on, if you read this blog and don't see a certain tendency here either then i don't know who can't see beyond the tip of their nose. there are people out there – smart people – with a very different mindset and that's just it. i must say that compared to your comments, sean was very civil towards lee smolin, really. and it is a very good practice in my opinion not to censor except obviously illegal or spamy content

26. **chris**
April 28, 2008

Dear Peter Woit,

note the quotes around "respectable"? i am not an english native speaker, but for me sean carolls statement clearly meant that he does not agree with the previous asessment. furthermore, he says that if these "respectable" people don't raise their voice they will certainly not be heard. i don't see anything wrong with that.

27. **Bee**
April 28, 2008

Hi Chris,
Yes, I've heard the Germans are experimenting with tenure, and I think it's a good thing. Reread what I said, I didn't say the system in the states is better, I said they are both different, and tenure is one of the differences. You misunderstand my problem with CV. I have nothing against Sean. I just very much dislike his attitude to let commenters insult others, even anonymously. It doesn't matter to me whether he just doesn't read it (as I think he said somewhere), or whether he doesn't care – it's his blog and it's his responsibility. I've said repeatedly previously that I think tolerating such 'discussion' corrupts the values of our society, and I still think so. I want to see one of these commenters stand up in public and say openly I don't think so-and-so belongs on this list because (upon which it would probably turn out they don't know why person x is on a list with public intellectuals, or what a criterion could be for a public intellectual to begin with, just that they maybe don't like person x). What do they do instead? They anonymously make fun of those who have the courage to stand to their opinion publicly. I think this is disgusting, full stop. I have the impression that many people do that kind of thing or like to read it because they find it entertaining (thus your suggestion not to delete it, thus I think the reason Sean doesn't delete it, because it attracts readers like shit attracts flies.) I also have occasionally had the strong impression that a large part of the blogosphere just sits there and waits for some 'scandal', some physicists arguing publicly, things get personal, somebody get called names. What do you think why Lubos' blog has attracted so much attention? Certainly not because he has such a great writing style. No, because he's provided exactly that, and I believe people read it with some disgusted fascination, and then pass it on (have you seen, did you read, he said WHAT?). However, one has to give it to Lubos that he's always signed with his own name, so at least he's not a coward. Hope that clarifies my disliking. Best,

B.

28. **Daniel de França MTd2**
April 28, 2008

The decline in the Big Physics (and related engeneering) project budgets on USA is clear with the period in which soviet union declined (ending of the 70´s and one) and even worsened after its ending. If it was a coincidence or not, I cannot say for sure. But if I could bet, I would say it had a negative influence on US, because the output of experiments do have political consequence when it comes to claim a superior technological level.

But given the information revolution happening, i'd say that the overall situation, in other fields, specially the nanotech field, everything got much better, there are much more brains and information sharing problems and information and brains.

29. **Peter Woit**
April 28, 2008

Chris,

Sean's reaction to the fact that someone had anonymously submitted a comment to his blog attacking Lee Smolin in a quite unfair way was not to delete it, not to ignore it, but instead immediately add his own comment elaborating on the original comment, and ending with "yeah". In English, this unambigously means that you agree with the original statement, given the elaboration (which in no way indicated any problem with the nasty characterization of Smolin).

As Bee notes, Sean has somewhat of a history of this, not only allowing the comment section of his blog to be used for nasty, unfair anonymous attacks on people (including Smolin), but responding in an insulting manner to complaints about this practice.

30. **Jeff McGowan**
April 28, 2008

MathPhys,

I saw Kaku give a talk a while back (pre the "string theory wars," or at least the public ones) at the CUNY GC. The talk was for a general audience (not a general scientific audience, just a general one). He mentioned string theory a number of times, and even put up a slide with an integral, I can't remember but I assume one of the loop calculations you mention, and commented something along the lines that this was the first real string theory calculation. In any case it was clear he was claiming to be one of the "founders" of string theory. I was sitting next to my thesis advisor and turned to him and said I thought he was angling for a Nobel (not that they would give one for that).

In any case it struck me that back then at least, when string theory was riding high, he was positioning himself very carefully.

31. **Christine**
April 28, 2008

I will never understand people's behavior. If someone is in some list made by someone, so what? I really don't care. I would care, of course, if the existence of the list would somehow harm me or my family/friends in a very direct and personal way. What is the harm of X being in the list and Y or Z being not? It is a list made by someone (or by a group of people), right? What is the intrinsic value of this list? If Y or Z feel it is not just, it's their problem. It's just a list. People care too much about unimportant things. Or maybe this is just a reflection of the fact that they themselves don't have more important things to do with their lives.

32. **jhk**
April 28, 2008

Kaku started out describing himself as one of the founders of "string field theory", which was technically sort of defensible, but over time the word "field" was put in smaller and smaller font until finally it has disappeared completely. Or perhaps it's still there just not visible to the naked eye.

I'd love to see him debate Susskind in public over the paternity of string theory.

33. **Professor R**
April 28, 2008

Re annonymity, I think Bee is absolutely right.

Just today I had the rather unpleasant experience of discovering an enitre thread on a politics blog

http://www.politics.ie/viewtopic.php?f=2&t=34334&p=1135958#p1135958

concerning a letter I had published in a newpaper.
(In the letter I was simply making the point that Ireland's non-membership of CERN has decimated Irish research in particle physics).
On the blog the letter have been attacked by all sorts of people who know nothing of physics, or CERN, and who hide behind silly names…annoying.
I think I will restrict comments on my own blog to people who declare themselves, at least to me!
Cormac O' Raifeartaigh

34. **woit**
April 28, 2008

Hi Cormac,

Good luck with the new blog, I'll look forward to following it. Figuring out what to do about comments can be tricky, although if you avoid the controversy over string theory the comments you get are likely to be few and relatively polite.

35. **Shantanu**
April 28, 2008

Bee, the idea of branes were discussed initialy by C. Wetterich and he mentioned
this in one of his talks at PI.
Does anyone know how well was the theory of inflation (in early 80's) received
in Soviet Union? I do know that Ginzburg was not a big fan of it.

36. **Changcho**
April 28, 2008

"and now as more and more scientists date other scientists, two-body problems will become more frequent."
And sometimes, eventually the two-body problems evolve into three and more body problems, and as is well know, there are no anaytical solutions to the N body problem with N>2 (i.e., it may get more complicated!)

I agree with commenter Allan on Hitchens…what makes people still think that Hitchens has anything worthy to say anymore?

37. **David Nataf**
April 28, 2008

Changcho,

Hitchens' popularity arises from his mastery of the English language and his bold approach to controversial social issues such as atheism. There's a general theme of people being fatigued by nuance and articial centrism, and that explains the popularity of George Monbiot, Ann Coulter and everyone in between.

Was your joke referring to love polygons or children?

I do think it's a serious issue however. There's a risk three or four postdocs might become the expectation. I've heard of one individual doing five postdocs.

38. **DB**
April 28, 2008

Cormac,

The thread you point to is a good example of the level of public understanding of HEP and the LHC. And it's not hard to see why. Back in the fifties people could understand the potential benefits of nuclear physics – it won the Pacific war and promised electricity "too cheap to meter". And its dangers. They could visualize the splitting of the atom and could marvel at the recreation of the sun's furnace on earth. Most importantly, they could translate this wonder into perceived benefits in their daily lives. It turns out that the hype didn't live up to reality. Yes, they got the Standard Model. But they also got Chernobyl.

Since the fifties, HEP has become terribly remote from the ordinary individual whose taxes fund it. While they understand very little, they doubt that it is likely to be of any real benefit to them, the lumpen proletariat, in the foreseeable future. They also know it is far too complicated for all but a very talented minority to properly grasp so that any understanding gained will be reserved for a tiny elite. And if that's all they get in return for their taxes they are likely to say, thanks, but we have more pressing priorities.

Which is precisely why James Orbach felt obliged to tell his Fermilab audience last week: "You scientists must make the case". It was a very salient warning.

Europe is currently revelling in its leadership position but I suspect it is just at an earlier stage of the US learning curve. US politicians have "been there, done that" and they appear to be fast losing faith with expensive fundamental science. I don't believe the average European voter is all that different to his American counterpart.

Without a much more successful outreach effort from the HEP community the outlook is not great, post-LHC.

39. **Eric H**
April 28, 2008

I agree very strongly with Alain Conne's, (Connes'?) comment about the traumatizing influence of the fall of the Soviet Union. Much of the work they did that was important related to extentions of Einstein's original geometric approach. That is, instead of making the huge jump to 10 and 11 dimensions, they used the conservative approach of investigating the connection between spin and the 5-d approach in Kaluza-Klein theory.

In the West the KK idea is treated as a toy construct that can't be taken seriously. The provisional reason for doing this is that spin isn't inelastic but can "break" in a sense and this negates KK. However, a proton's quarks really can be seen in a sense as having an inelastic bond – > 10^32 years and counting – they still haven't found a limit.

I think the real reason KK was thrown out prematurely is that Einstein used the KK ideas in constructing his unified field and by that time quantum mechanics occupied all the energy and time of physicists. It's sad really. How many people actually know the details of Einstein's theory but have only been told that he wasted his life on it. It's also sad because gravity emerges naturally in KK theory. While it may be too low-brow for some around here check out the English Wiki site on "zero point field" that I did a lot of editing of. Andrei Sakharov was a real pioneer.

40. **Professor R**
April 28, 2008

DB, you're absolutely right.
I thnk the statement 'Europe is currently revelling in its leadership position but I suspect it is just at an earlier stage of the US learning curve' is all too correct. I'm afraid where the US goes today, we generally follow tomorrow (with Ireland leading the way).

The terrible irony is that CERN has saved Euroean a fortune – by centralizing mst of the accelerator build in one location, individual nations saved millions and avoided duplication.

My own viewpoint is Tim BL and Cern missed a trick – while I am very glad he (and not a commercial firm) put the web together, there should be a permanent reminder that this came from CERN research – everytime you open a browser in fact. If they had done that, no-one would need to argue the case for CERN …Cormac

41. **Changcho**
April 28, 2008

D. Nataf – actually, the joke referred to children…In matters of atheism, I'd turn to Dawkins, not Hitchens. The latter may have a mastery of the English language, but little else imho ; regards.

42. **Michael Bacon**
April 28, 2008

"While they understand very little, they doubt that it is likely to be of any real benefit to them, the lumpen proletariat, in the foreseeable future."

Db,

Lumpen proletariat? Really? What an odd way to describe people in this day and age.

43. **anonymous**
April 28, 2008

Bee, you are annoyed that Sean Carroll lets anonymous commenters insult people … well, so does Peter Woit, as you'll notice by reading the comments about Christopher Hitchens in this thread (I count a first name with no link as anonymous, since it effectively is).

44. **DB**
April 29, 2008

Cormac wrote:
"My own viewpoint is Tim BL and Cern missed a trick – while I am very glad he (and not a commercial firm) put the web together, there should be a permanent reminder that this came from CERN research – everytime you open a browser in fact. If they had done that, no-one would need to argue the case for CERN "

Yes, CERN invented the World Wide Web, but the US made best use of that invention and paid nothing for the rights. A similar attitude seems to have emerged in the US with regard to ITER, i.e., "Let the others pay for the fundamental research, we'll clean up on the commercial side".
Had CERN licenced that invention, it would probably never have to raise a cent from governments again.

Michael,
My use of the phrase was a poor attempt at irony. But I do think that is how they view themselves when it comes to subjects such as HEP.

45. **anon.**
April 29, 2008

Having left the anonymous comment at CV, maybe I should have picked someone other than Smolin, since people seem upset about that and don't notice that he's not the only person I singled out. Lomborg is the worst, being a fraudulent hack who is actively hurting the entire world with his efforts to delay action on climate change. (His pose as an 'environmentalist' makes it all the worse.) There were other people on the list who are similar, but outside the sciences — Kagan, for one, surely takes part of the blame for a great deal of human suffering. Arguments within theoretical physics are, ultimately, utterly insignificant compared to these things, so probably my complaint should have focused on that, but since I was commenting on a physics blog I mentioned Smolin. He isn't really hurting anyone, of course, but he's someone who is taken far more seriously in the press than by other physicists.

46. **Peter Woit**
April 29, 2008

anonymous,

My objection is specifically to the use of blogs by scientists to anonymously attack their colleagues in unfair ways. I wish people would not post comments anonymously like the ones about Hitchens, but I don't find it so objectionable that I delete them.

More problematic actually were the comments about Kaku, which I did seriously consider deleting. I left them because I thought that the question they addressed of claims being made that he is a "founder of string theory" is a valid one, and they did not treat it unfairly. But I encourage people to not post anonymously, and if they do feel it necessary to do so, they should bend over backwards to be polite and fair to anyone they have a critical comment about.

47. **Bee**
April 29, 2008

"Bee, you are annoyed that Sean Carroll lets anonymous commenters insult people … well, so does Peter Woit"

So what? I'm not the insult-police of the blogosphere and I actually don't read Peter's blog very closely (or any other blog for that matter, not even my own), and I have no idea what Peter did or didn't do elsewhere.

But since you bring it up, you are trying to excuse behaviour by saying others do the same. This is exactly the reason why I think one should not tolerate insults in discussions. This isn't something specific to CV, NEW or TRF, you find this in almost every more or less public comment section (just pick a random blog-section of a newspaper or magazine). Can't you see how this is pushing the boundaries farther and farther aways from what the average human would consider a civilized communication? I mean, come on, it is well known that humans test the boundaries that are being set to their behavior all the time, may that be laws, morals or ethics. It will get worse until they perceive they've reached that boundary, but where is it supposed to come from if not from us?

Thanks for your explanation why you don't want to see some people on that list. It sounds very similar to what I guessed above doesn't it? I don't like that person or his opinion, and soandso is taken more seriously by the press than by his community. What has this to do with anything?

Anyway, I actually agree with Christine's comment above that it's more or less irrelevant who appears on what lists of which magazine, that wasn't the reason why I brought it up (and given that I don't know most of the names on that list, I see no point in discussing it.) My problem is just, one again, how it comes close to impossible having a sensible exchange on the blogosphere.

Best,

B.

48. **Professor R**
April 29, 2008

Bee and JC, in response to disscussion at top

'Were there any supergravity and/or string theorists appointed to German professorships before the Berlin Wall came down….'

I was sorry to see Julius Wess didn't rate a mention (a pioneer of supersymmetry, he and Zumino pactically invented supergravity). Julius had a very big group in Munich and was a great collaborator
with my Dad's group at th Dublin IAS. I think they were fairly typical of the continental European school – rather than get hung up on string theory itself, they were much more interested in the mathematics of supersymmetry and other gauge symmetries.

In fact I would argue that while Peter's quote from Connes is interesting, it pretty much ignores the whole field of gauge symmetry…Cormac

49. **Professor R**
April 29, 2008

P.S. Julius passed away last summer, but you can find his bio on wiki (http://en.wikipedia.org/wiki/Julius_Wess) or his famous book at
http://www.amazon.com/Supersymmetry-Supergravity-Julius-Wess/dp/0691025304

50. **Bee**
April 29, 2008

Hi Professor, JC,

Yes, I'm sorry. I wasn't an attempt to give an exhaustive list of Germans who've worked on st or related things, just the first three names that came into my mind. Best,

B.

51. **anonymous**
April 29, 2008

Vanity Fair seems to think that the right person to review a book about Isaac Newton is Christopher Hitchens.

Exactly who at Vanity Fair do you think is the right person, Peter? The people profiling Miley Cyrus?

In any case, I read Hitchens' article, and I think the passages you quote are somewhat unfairly taken out of context.

52. **anonymous**
April 29, 2008

Bee, you said, in response to my comment: "But since you bring it up, you are trying to excuse behaviour by saying others do the same. "

No, I'm not, I only pointed out that other people do the same as Sean Carroll. I didn't make any comment one way or the other about the acceptability of this style of comment moderation.

You also wrote:

"I'm not the insult-police of the blogosphere and I actually don't read Peter's blog very closely (or any other blog for that matter, not even my own)""

To which I have to say: couldn't Sean Carroll say _exactly_ the same thing in his own defense? So why criticize him?

53. **Bee**
April 30, 2008

Anonymous, I do read the comments at my blog, in case that's what you mean, and either I or my husband deletes the inappropriate ones (though there aren't many, I guess people get the message). I occasionally admittedly don't very closely read Stefan's posts (since he usually only writes when I am really busy anyhow), but hey, I'm married to that guy so I trust him to not publish anything outright offensive while I'm not looking. My remark was a reply to yours "you are annoyed that Sean Carroll lets anonymous commenters insult people … well, so does Peter Woit, as you'll notice by reading the comments about", which I interpreted as: "you either are annoyed by both, or by none", to which my reply is, what I don't read doesn't annoy me, and I still don't know why it is relevant whether I might not have noticed what happend in Peter's comment section in the year 10 BC or whatever. Besides this, it isn't hard to figure out that it annoys me far more if insults go against people I know, and who I know don't deserve being insulted. Either way, this exchange is basically content free. I've said what I had to say. Best,

B.

54. **CV**
April 30, 2008

«In any case, I read Hitchens' article, and I think the passages you quote are somewhat unfairly taken out of context.»

Maybe you can explain us how writting

« the day is not far off when we will be able to contemplate physics as another department—perhaps the most dynamic department—of the humanities.»

can be placed in a context where it doesn't sound like an insulting piece of pseudo-intelectual trash.

55. **Arun**
April 30, 2008

« the day is not far off when we will be able to contemplate physics as another department—perhaps the most dynamic department—of the humanities.»

Obviously, Hitchens thinks that the future of physics is entirely string theory.

56. **anonymous**
April 30, 2008

CV, if you deigned to pull your head out of the sand, you would realize that Hitchens has the utmost respect for science, and is expressing the hope that it will come to be more widely appreciated by people in the humanities.

There's nothing wrong with that sentence (which, by the way, begins with "For all that, he did generate a great deal more light than he had intended", praising Newton's achievement). Do you mean to tell me that it's "insulting" to contemplate the prospect of a rapprochement between the so-called humanities and the sciences? I think it's badly needed.

57. **CV**
May 2, 2008

Your interpretation is completely off the mark.
By writing "For all that, he did generate a great deal more light than he had intended" he is for all effects stating that Newton did his work by chance and that his main objective was keeping people ignorant.

As for : *Do you mean to tell me that it's "insulting" to contemplate the prospect of a rapprochement between the so-called humanities and the sciences?*

In the way Hitchens is implying yes I do. It's extremely insulting.
Rapprochement like that if it occurs should come from the humanities resembling proper sciences and not by making physics into a pseudo-intellectual endeavour where the only metric is the capacity to impress other people by gluing words with no final meaning.

Hitchens showed with this piece that he has no idea what science is about and thus can't have any respect for it.

58. **John R Ramsden**
May 3, 2008

Cormac O' Raifeartaigh wrote:
>
> Re annonymity, I think Bee is absolutely right.
>
> Just today I had the rather unpleasant experience
> of discovering an enitre thread on a politics blog

That site http://www.politics.ie/ is a forum rather than a blog, the difference being that discussions in a blog can be started only by its "owners" (possibly, in fact usually, one individual) whereas any registered user can start a forum thread as well as replying.

Forums can be bear gardens, even where moderators are vigilant (don't get me started on spam-ridden unmoderated usenet groups!). So one must get used to that.

I was shocked that a self-proclaimed *physicist* of all people in that discussion you referred to agreed with the unfathomable decision of the Irish Government not to join CERN for such a modest fee.

Doesn't this muppet realise how diverse and speculative today's leading-edge physics theories are, and the importance of the LHC in whittling down the contenders and perhaps providing vital clues and new insights (which will quite likely refine low-energy physics too in due course).

Also, you only have to look at the Atlas detector to realize what a massive boost the whole thing must be to high-tech manufacturing industries, which God knows the West needs when practically everything is made in third-world countries these days.

59. **Chris W.**
May 3, 2008

Speaking of that "ridiculous recent episode of the 'exceptionally simple theory of everything'" (Connes' words), the May issue of Outside Magazine has a rather long profile of Garrett Lisi. Needless to say, one won't learn much about his research from the article, although Glashow and Wilczek are quoted.

Creutz On Rooting

April 28, 2008
Categories: Uncategorized

Michael Creutz has a remarkable new preprint out this evening, entitled [The Saga of Rooted Staggered Quarks](). It explains what has been going on in a rather bitter controversy within the lattice gauge theory community over the last few years.

While lattice gauge theory provides a quite beautiful way of discretizing gauge fields, preserving their geometric significance, fermions have always been much more problematic. Here the geometry is spin geometry, which doesn't appear to have a natural formulation on a lattice. What does have a natural formulation is not a spinor field S, but End(S), the linear maps from S to itself, which can be identified with the exterior algebra, and naturally put on the lattice by assigning degrees of freedom to points, 1-simplices, 2-simplices, etc. The problem is that if you do this, you don't get a theory of a single fermionic field, but instead multiple copies. This geometrical argument is just one aspect of the problem, which appears in other more convincing ways, but this all adds up to making chiral symmetry especially problematic on the lattice.

There are many possible ways of dealing with this, but one popular one has been "rooting" some of the fermionic degrees of freedom that have been staggered on neighboring vertices of the lattice. One ends up with four copies of what one wants, so the argument has been that the thing to do is to take the fourth root of this to get a calculation that tells one about a single fermion. The problem is that this is a quite non-analytic thing to do, and it is not clear that it gives one something sensible. A debate between Creutz and people using this method has raged for the last few years, with Creutz claiming that the rooting procedure gives the wrong answer, while proponents of rooting argue that the problems involved will go away in the continuum limit.

Creutz's preprint describes the conclusion he has been led to about this, and his problems getting some of them published:

> This led me to question whether there was some physical measurement one could make to determine if a quark mass was indeed zero. I could think of none, and proposed that a single vanishing quark mass might not be a physical concept. This paper was submitted to Physical Review D.
>
> This is where the shit started hitting the fan. There was a common lore that if the up quark mass were to vanish, then the problem of why theta appeared to be phenomenologically very small would be solved. I was saying that this lore might be wrong. This drove the referees nuts, with statements like "I am somewhat concerned that the errors are so obvious." After numerous similar scathing remarks the paper went to a divisional editor for PRD, who upheld their opinion. On rejection I took the paper and split it into two parts, one on the phase diagram and the second on the vanishing mass issue. These both appeared in Physical Review Letters, Phys.Rev.Lett.92:201601,2004 (hep-lat/0312018) and Phys.Rev.Lett.92:162003,2004 (hep-ph/0312225). I do derive some visceral pleasure from having turned a rejected PRD paper into two PRL's…
>
> Eventually the claims of the staggered advocates became so outrageous that I felt I had to be more aggressive. I was pushed further by statements that if someone had issues with staggered quarks, they needed to write them up. At the time I was too naive to appreciate how the stubborn nature of some personalities involved would mean that these arguments would be dismissed without serious discussion. As with the up quark mass issue, this is one of those situations where a person without tenure would be ill advised to challenge conventional lore.
>
> So I submitted a paper (hep-lat/0603020) pointing out the inconsistencies between rooting and the expected chiral behavior. This was quickly rejected by PRL which has a policy of not publishing interesting and controversial papers. After transferring it to PRD, things got stuck, with numerous referees simply refusing to respond. After about a year and eight referee reports, some positive and some negative, PRD decided that they don't publish interesting and controversial papers either. I did not take this delay kindly and rewrote the paper with the provocative title "The evil that is rooting." This was fairly quickly accepted by Physics Letters (Phys.Lett.B649:230-234,2007; hep-lat/0701018), although the title was mollified at the editor's suggestion…
>
> The staggered community has continued to ignore these problems. I feel their stranglehold on the US lattice effort approaches scientific dishonesty. As an example of the prevailing vindictiveness, a recent paper of mine on a completely different topic was rejected from a prominent US journal on the basis of a single negative referee report stating that "It is puzzling that the author ignores all these highly relevant lessons that have been learned long ago in the context of the staggered fermion formalism." It was overlooked because I wanted to avoid the ongoing controversy, of which the referee was certainly aware. After I did add remarks on the comparison with staggered, the paper was rejected without further review by a divisional associate editor representing the staggered community. He raised some symmetry issues based on comments by the Maryland group, to which I was never given a chance to respond. This paper was then submitted to a European journal where I hoped for a more equitable treatment. There it was quickly published.
>
> Beyond the international ridicule this this controversy brings on the USQCD community, other aspects are particularly upsetting from a scientific point of view. First, enormous amounts of computer time continue to be wasted on generating lattice configurations from which any non-perturbative information will be questionable. About 38 percent of the current computer time allocated by the USQCD collaboration is going to continue these efforts. Second, young people associated with this project are taught to repeat, without question, the party line that all will be okay in the continuum limit. Third, the practitioners are such a powerful force that most outsiders are unwilling to look into the problems despite the fact that the underlying physics is so fascinating. And finally, I find it extremely unsettling that some physicists widely regarded as experts in chiral symmetry and lattice gauge theory can so casually and thoroughly delude themselves with bad science.
>
> In short, the lattice has been very good to me. It is extremely painful to see it abused so blatantly

One would like to think that this issue will get sorted out over time as more work makes it clear whether or not rooting is as serious a problem as Creutz thinks it is. But the progress of science is not always smooth…

Comments

1. **Peter Orland**
   April 29, 2008

   One way to settle the controversy would be to carefully formulate order-by-order renormalization (of physical operators) with rooted fermions. Has anyone studied this?

2. **Peter Orland**
   April 29, 2008

   P.S. It clearly isn't easy, since there doesn't seem to be a local (within a small number of lattice spacings) Lagrangian. So I guess I am asking if rooted Fermions can be formulated (in some complicated way) so that there is manifest locality.

3. **nbutsomebody**
   April 29, 2008

Whether Creutz is right or wrong and whether there is bad scientific practice, one good point about hep-ph and hep-lat community is that they still debate, whereas in String theory nobody argues.

4. **Sandro**
   April 29, 2008

   "Whether Creutz is right or wrong and whether there is bad scientific practice, one good point about hep-ph and hep-lat community is that they still debate, whereas in String theory nobody argues."

   I don't think that "It is puzzling that the author ignores all these highly relevant lessons that have been learned long ago in the context of the staggered fermion formalism." can be considered as a healthy debate situation, and yes, it is a journal context, but I don't think the arxiv one goes much better. All this only reminds me of an african animation movie in which a young child asks his mom "Mom, why is the Sorceress so evil?", and the mom replies "She's not more evil than others: she just has more power."

5. **Arun**
   April 29, 2008

   I reformulated my arguments in terms of the 't Hooft vertex, something crucial to the understanding of how the theta dependence of QCD works. While previously I had not claimed to have a proof, here I showed specific non-perturbative effects that must come out wrong, even in the continuum limit. This proof appears in the proceedings. A more extensive discussion of the issues appears in Annals.

   How difficult is it to assign a couple of graduate students to check this proof?

6. **Peter Woit**
   April 29, 2008

   Arun,

   The problem with physics "proofs" is that they typically involve lots of assumptions, often not made explicit. I don't know anything about the argument Creutz is referring to, but I'd guess that it evaluating possible objections to it would require quite a lot of expertise, not the sort of thing a typical graduate student has the background for.

7. **An anonymous lattice**
   April 29, 2008

   In Europe, our community no longer debates Creutz's 'proof' because it is known to be incorrect.

   For the most recent paper debunking the criticism, see Kronfeld [2007] http://arxiv.org/abs/0711.0699 that also agrees with previous points by Bernard and his collaborators [2006] http://arxiv.org/abs/hep-lat/0603027

   These papers do not quite prove that rooting works perfectly, as indicated by formal manipulations and by the numerical data. But they show that Creutz's arguments are incorrect. For instance, the Kronfeld paper has a section dedicated to each aspect of Creutz's criticism (cutoffs, ranks of groups, and so on). I am slightly worried that the new Creutz's preprint, submitted as HTML, won't be enough to convince me or my German and other European colleagues.

8. **Brett**
   April 29, 2008

   There are actually two distinct but seldom disentangled issues related to rooting. The same two issues arise with many respect to many of the techniques used in lattice calculations. The issue is whether what you're calculating approaches continuum QCD in the right limit. The second is whether the technique gives useful/accurate results at the physical lattice spacing you are using.

   It's not clear whether the first issue is a problem with rooting. Neither side has produced a definitive argument. If I had to guess, I'd bet that the continuum theory is not QCD, but that's just a guess. What gets obscured in this debate over rooting is that there things done in lattice calculations that we know for sure do not lead to the right limiting theory. For example, lattice calculations are now done using an irreversible Markov chain. (Lattice people would refer to this as "not actually using the Metropolis alogrithm.") In this case, the continuum limit of any calculation cannot even be defined, because the calculation does not, in principle, converge.

   But that's only in principle. We know that practically, the improved, non-Metropolis algorithms do converge (faster even!), so the problem is not worth worrying about. This brings us to the second issue. The lack of Markov reversibility does not introduce any meaningful errors at the level of accuracy of current calculations. Indeed, it improves the accuracy. Coming back to rooting, while whether it is formally valid is an open question, whether it is practically useful is not. Rooting does an excellent job of dealing with low-mass dynamical quarks at the present time. Maybe it will need to be re-evaluated when calculations get a lot more precise, but all indications are that the lattice community will keep an eye on this matter.

9. **milkshake**
   April 29, 2008

   Brett, this is way too technical for me – but I wonder if the current argument about rooting is analogous to the old debates about validity of renormalization ("sweeping the problem under carpet" etc), a suspect calculation trick that worked, for reasons that were understood only afterwards

10. **Peter Orland**
    April 29, 2008

    Milkshake,

    There were no debates about renormalization. It took people a while to realize that it was deeper than just "sweeping…", but few competent people doubted it was valid, even in the the late 40's. The reason was that it gave stunningly good results for measurable quantities in atomic physics.

    The argument about rooting is different. I was asking above if a rooted lattice theory might have an (albeit complicated) local formulation. If so, it probably can be renormalized, and therefore should be standard QCD. If not, it isn't so clear.

11. **milkshake**
    April 29, 2008

    but Brett claims that "rooting does excellent job" – and if this is so, does this make the observable things come out better? And how much hand-adjusting is done in these calculations to make things come out right? Also, the most authoritative people at any given time in science history are not always the most competent ones in hindsight. So if Creutz claims he has been unfairly censored, is it because he is crackpotty – or did he become unpopular for repeating a valid complaint that no-one wants to hear?

12. **Peter Woit**
April 29, 2008

milkshake,

I don't see Creutz complaining of censorship. He's a leading figure in the field, and his problems with referees haven't kept his papers from being published. The scientific issues he is concerned about are getting a full airing, while, as is often the case in science, the way the debate is taking place may be less than the fully rational process one would hope for.

13. **Chris Oakley**
April 30, 2008

Brett,

> What gets obscured in this debate over rooting is that there things done in lattice calculations that we know for sure do not lead to the right limiting theory.
> For example, lattice calculations are now done using an irreversible Markov chain. (Lattice people would refer to this as "not actually using the Metropolis alogrithm.") In this case, the continuum limit of any calculation cannot even be defined, because the calculation does not, in principle, converge.
> But that's only in principle. We know that practically, the improved, non-Metropolis algorithms do converge (faster even!), so the problem is not worth worrying about.

In my experience (limited to solving PDEs for derivatives pricing), if you do not have a handle on errors/convergence properties you might as well not bother … better to use the same computer power for playing "Doom" instead – at least you know what is going on there. So I guess, to use Susskind's terminology, I would never have "made it" as a Lattice Gauge Theorist.

Peter O,

I agree with you about renormalization. After 1950 only retards like Landau, Feynman and Dirac ever expressed doubts.

14. **chris**
April 30, 2008

hi peter orland,

yes, locality is the crucial issue.it has been proven, that a) at m=0 there is no local formulation b) that at m>0 there is no local formulation, but the nonlocality is a cutoff effect, so it vanisches in the continuum limit. so at m=0 rooted staggered fermions are in the wrong universality class (which is easily seen by the fact that the symmetry content is wrong). but outside m=0 the topic is quite subtle if you reflect a bit about what it means that you have a nonlocality that is a cutoff effect. there is the issue of the order in which you take the three limits (m->0, cutoff->00, V->00).

in fact, order by order in PT it has been proven that things are fine, but this is not enough. also there is strong numerical evidence that supports the view that everything is ok if you stay in the correct regime, but a full analytical proof is somethig that the whole community tried to hunt for at least half a decade and it is not in sight.

15. **Peter Orland**
April 30, 2008

chris –

Thanks for the information. It's clearly worth looking into!

Chris O.

>I agree with you about renormalization. After 1950
>only retards like Landau, Feynman and Dirac ever
>expressed doubts.

They doubted the interpretation of renormalization. They weren't sure whether the idea was really right at a deeper level (in the meantime, nonperturbative techniques have shown that it is). I heard Feynman express these misgivings (he was teaching QED and QCD in 1977-78). But he and others never doubted its validity for leading orders of QED.

Mike Creutz is not expressing only misgivings, but openly claiming rooting is valueless.

16. **Coin**
April 30, 2008

In the continuum limit, staggered fermions yield four species, called tastes. To reduce the number of tastes to one (per flavor)…

Holy crud! Terry Pratchett was right all along!

So, I have what I hope is not a stupid question:

Here the geometry is spin geometry, which doesn't appear to have a natural formulation on a lattice. What does have a natural formulation is not a spinor field S, but End(S), the linear maps from S to itself

What I am immediately curious here about is, is there a reason the spin networks incidentally used in LQG cannot be made use of here? They aren't lattices, but they are a method of discretizing space, and at least in their original pre-lqg formulation they by design affixed a spin to each spacetime "point". Looking up spin geometry online however (which if I'm understanding this right is basically just the study of spinor bundles?), I don't see anything at all indicating people have even tried to link that subject with spin networks. Is there some obvious reason I'm missing why it would not be natural to try to do spin geometry with spinnets?

Or, put another way: Is it known whether some of the advantages with lattice gauge theory (like computability) can be retained if one moves to some other discrete structure which doesn't have the problem with accommodating fermions?

Only adding to my confusion, the wikipedia article for "Lattice Gauge Theory" tosses off this cryptic factoid:

> Lattice gauge theory has been shown to be exactly dual to spin foam models provided that the only Wilson loops appearing in the action are over plaquettes.

17. **wolfgang**
May 1, 2008

> wikipedia … tosses off this cryptic factoid:

Perhaps it refers to
this paper.

18. **Not a Nobel Laureate**
May 10, 2008

Has anyone asked the Aussie LQCD community for their opinion on rooting.

19. **Wombat**
May 10, 2008

"Has anyone asked the Aussie LQCD community for their opinion on rooting."

Do they disapprove of rooting?

Do they refrain from engaging in rooting?

20. **lattice observer**
May 23, 2008

hi all,

some answers to Peter Osland's technical points — plus a humble opinion.

———————————————————————————————————————

Peter Orland Says:

One way to settle the controversy would be to carefully formulate order-by-order renormalization (of physical operators) with rooted fermions. Has anyone studied this?

P.S. It clearly isn't easy, since there doesn't seem to be a local (within a small number of lattice spacings) Lagrangian. So I guess I am asking if rooted Fermions can be formulated (in some complicated way) so that there is manifest locality.

———————————————————————————————————————

there has been some work along this direction, interested readers may look for papers by Joel Giedt and David H. Adams in SPIRES. the problem is, in perturbation theory the rooting procedure is essentially ok, precisely because PT fails to tackle long-distance couplings at any given order, and non-locality would have a non-perturbative origin. indeeed, as observed by Creutz, even the theorists supporting the rooted staggered approach do currently think that there is no local formulation of the regularised theory; needless to say, this makes it very hard to understand how to take a continuum limit that defines QCD consistently. (cf. work by Bernard, Golterman, Shamir, Sharpe.)

on the other hand, as an Anonymous lattice points out, Creutz's argument is widely regarded as insufficient ("incorrect" is maybe too precise a word in this context) to show that rooted staggered quarks do not lead to QCD in the continuum limit. yet, the overwhelming majority of the community still thinks that the rooted staggered approach is at best controversial, and at worst just an elaborated model that cannot be plausibly claimed to produce first-principles results. the rift between the two fields is deep indeed…

Anonymity at Cosmic Variance

May 1, 2008
Categories: Uncategorized

Over at Cosmic Variance, anonymous comments personally attacking me have been posted recently by someone who identifies themselves only as "string theorist". I've complained to Sean Carroll and his colleagues about their policy of allowing the comment section of their blog to be used for anonymous ad hominem attacks by physicists who are unhappy with Lee Smolin and me because of our criticism of string theory. If someone wants to argue not about science, but to complain about my behavior, I'm perfectly willing to engage in such a discussion, as long as it's with someone who is willing to take responsibility for their own behavior.

Here's the response I received from Sean:

> Personally, I could not care less whether a comment is anonymous or signed. It just makes no difference to me. I understand that you feel otherwise, as you have said so over and over and over again. I will delete comments if they are vulgar or overly obnoxious, but anonymity is completely beside the point. If my co-bloggers feel differently, they are welcome to overrule me.

So, I guess if you want to anonymously attack, insult or slander people you disagree with about a scientific issue, Cosmic Variance is open for business.

Comments

1. **anonymous**
   May 2, 2008

   Of course if you want to anonymously attack non-scientists like Christopher Hitchens, feel free to do so here. Remember, anonymous attacks are only bad if they are directed at scientists …. right?

2. **Yzer**
   May 2, 2008

   Peter, I'm a big fan of yours, so I mean no insult by this, but is it not possible simply to ignore such nonsense, be the bigger man, and rise above it? You only call greater attention to their immaturity when you single them out for a tongue-lashing. Let them fade into the anonymous obscurity whence they came. Your good name has more to lose from your response to them than from anything they say about you.

3. **Anonymous**
   May 2, 2008

   It is a common stratagem to reject as cowardly criticisms leveled anonymously. However, such a rejection neither rebuts the criticisms, nor has much merit, at least if the criticisms are dispassionately formulated and substantial (and if they are not, they should simply be ignored).

   There are many reasons to remain anonymous – among which perhaps the most responsible is the desire not to draw attention to oneself, or even to disassociate what one says from who one is. If Famous Man X says something openly it will be ignored by his haters and repeated by his lovers, regardless of its merits, and knowing this Famous Man X may decide to say something anonymously; there is a long tradition of this in the world (pseudonyms). (Compare this to Famous Man Y, who glowingly trumpets his identity even when farting; he is no coward, I guess, but nor is he responsible). If Unknown Man X says something openly, it will be ignored by almost everyone, because he is Unknown Man X and, being unknown, can have nothing interesting to say, so perhaps by saying it anonymously, and thereby raising the possibility that he is Famous Man X in disguise, he will actually be heard. Other motivations abound – fear of reprisal is one, and to call such cowardly is simply to have never lived in fear of a beating. Among these motivations is surely the cowardly desire to hit from behind, it is not the only one.

   The most basic such motivation, is – it does not matter who I am; in any case the audience does not know who I am, and knowing so would not change the content of what I say, so what is to be gained by advertising who I am? There is of course something to be lost – one's dignity.

4. **Another grad student**
   May 2, 2008

   Hi Peter,

   You may remember me from a discussion (a.k.a. cat and dog fight) in 2006 on Clifford's blog, where I defended your reluctance to continue a somewhat hostile discussion with Clifford and someone whose writing style bears some resemblance to that of the poster you felt was attacking you very recently on cosmicvariance. It wouldn't surprise me if you also noticed the similarity in style.

   I defended you then for several reasons. Most importantly, I felt you were being subjected to unnecessarily personal and condescending attacks because you promoted a point of view your antagonists strongly disagreed with; their "debating" style was offensive to me because of the degree of disrespect and unwillingness to acknowledge that there might be some validity or forethought in your point of view. Another reason was that in following your blog (and Usenet posts before you had a blog), you seemed to be sincere in your intentions (not a publicity hound as some people assumed), and willing to acknowledge your mistakes. I still believe you are sincere in your intentions, not just trying to stir up trouble because you are bitter or whatever, but over the last year or two I have also perceived (rightly or wrongly) certain changes in your style that I wish I didn't see. This post of yours seems like an appropriate place to mention them; you won't have to suffer hearing them from me again.

   Before you responded to ST on cosmicvariance, I saw his/her comment. My initial thought was that ST was being somewhat presumptuous to think that you had so much power over Sean that he would consider taking a break from blogging due to anything you might say. Sean strikes me as having some pretty strong opinions, and is willing to freely write about them (which is fine, since a reader can always skip over whatever is annoying to read); I could be wrong but I don't think he would be beaten down by you expressing your points of view. I didn't know you would respond, but it seemed like if you *did* want to say something the perfect response would be to make a joke about the power you hold over Sean, and generally dismiss ST's concern. That would have fit very nicely with the generally congenial tone of the comment section up until ST chimed in, and you would have come across as disarming and friendly. Instead, your response seemed like a very jarring attack on ST, well beyond what I would have thought was needed to make your point. An image that came to mind was of a social gathering where people are chatting amicably, and then someone makes an unkind remark; the recipient reacts with such rage that he startles the other guests, and then he accuses the host of the party of behaving inappropriately for not throwing the guest out for making the unkind remark.

   Perhaps this mental image of mine is overly harsh, but I expect that many people seeing your reaction to a comment in cosmicvariance or elsewhere don't know about your history of having to deal with repeated unwarranted personal attacks on you, much less the specific personalities involved. Those people would probably just see a very strong (over)reaction to some throw-away snide remark from someone else they don't know, and the impression would not be good; if anything, I think they would look at your comment as confirming the negative remarks by, e.g., ST, with the net result that someone who had no opinion about you now has a negative one. If you had responded with humor, I think that ST would more likely come across to that person as overstating things and you would look fine. Someone who did know something about you at least would have seen a pleasant side of you that didn't reinforce their earlier perceptions.

I remember a comment by Chris Oakley sometime ago where he essentially said that he thought it was counterproductive to get into pissing contests with string theorists, and I completely agree. I also agree with others who think you shouldn't try to doggedly and publicly shame other blog owners into treating certain kinds of comments the way you would like; it seems fine to complain once, but after that it can come across to others that you have appointed yourself to a role they aren't willing to give you.

Anyway, that's my two cents and it may not even be worth that. I hate writing critical comments like this, and I hope you will see it as an attempt to be constructive.

5. **Another grad student**
   May 2, 2008

   Grrr… One sentence didn't come out quite the way I intended. The end of the fourth paragraph should have been:

   Someone who had already formed an unfavorable impression of you at least would have seen a pleasant side of you that didn't reinforce their earlier perceptions.

   Sorry about that.

6. **rh**
   May 2, 2008

   First they ignore you, then they laugh at you, then they fight you, and then you win … I entirely agree with Yzer. Best, R

7. **nbutsomebody**
   May 2, 2008

   There is no way to verify identity in a blog. Hence anybody can take any identity, even false identity. I do not see any simple way to stop anonymous comments. If one stops anonymous comment, people will just use false identity or some strange nick names like me.

8. **John Wheeler**
   May 2, 2008

   No way to verify identity, really ? Never a clue ? 🙂

9. **AT-S**
   May 2, 2008

   I agree with Yzer and "Another Grad Student": best to ignore the pot-shots.

   Remember the adage: "The dogs may bark, but the caravan moves on."

   Keep up the good work.

10. **Arun**
    May 2, 2008

    Another grad student, that is good advise for everyone.

11. **Peter Woit**
    May 2, 2008

    Thanks all,

    The advice to ignore this and/or treat it with humor is excellent. It's my intention to waste as little time and energy on it as possible, but I confess that I'm not amused at all about this situation. The point of this posting was just to make clear what is going on:

    I'll respond seriously to any complaints about my behavior from people who are willing to take responsibility for their own. When I have responded to anonymous insulting comments at CV by telling the commenter what I think of their behavior, my response has been deleted by Sean as "vulgar or overly obnoxious". I suppose perhaps he has been doing me a favor, but his views on censorship are rather one-sided.

    I continue to find it truly remarkable that some professional scientists find it acceptable to anonymously post personal attacks on people they disagree with about scientific issues, and that others endorse and enable this behavior.

    Now, on to trying to ignore this and develop a sense of humor about it….

12. **Maya Incaand**
    May 2, 2008

    Sticks and stones……

13. **Bee**
    May 2, 2008

    "Sticks and stones may break my bones, but words will make me go in a corner and cry by myself for hours."

    ~ Eric Idle

14. **Bee**
    May 2, 2008

    Well, I've commented on that previously, but since it's on the table again. I find it very inappropriate to tolerate anonymously made insults. I don't care very much who believes who 'set a tone' or whatever, and I do indeed agree that Peter too sometimes hasn't been holding back, but I don't want to make this a discussion of a particular person.

Anonymity is certainly necessary in some cases to protect a person, but in almost all cases on the internet it is completely unnecessary and only used out of cowardice, because one can't be held responsible for what has been said and doesn't have to fear a lack of reputation. I guess I don't have to tell you what the result is of that, just go look into any comment section for some newspaper column or whatever. One of the problem with anonymous insults is that the anonymous insulter deserves to be insulted even in the opinion of the more polite commenters, which will make matters worse. I therefore think such comments should be deleted asap. This has nothing to do with freedom of expression. Say what you want, just sign with your name.

What I find especially upsetting in this case is the following comment by Jennifer West "This is what the internets is all about people. If you cannot take the heat, get out of the kitchen." or generally the often made recommendation to 'just ignore'. A recommendation usually made by people who hardly ever are subject to such insults.

If anonymous insults is what the internet is all about – and yes, one could indeed sometimes have that impression! – then it's about time to change it. To what level do we want interhuman communication to drop, huh?

To repeat something else that I've said before: I think one of the reason why some people don't want to delete such comments is that many readers, including the blog owners, find it entertaining to see how others are insulted, and then the attacked persons subsequently defend themselves. I even sometimes have the suspicion that some commenters only post insults to 'stir things up' a bit and have fun. Such things increase the number of comments and attracts visitors like shit attracts flies. I seriously can't see any other reason to willingly support such a disgusting level of discussion than the wish to entertain readers on the cost of the person attacked. The wish to avoid this has nothing to do with 'high' ethic standards. It is simply the desire to have the same standards that we have in normal life. At least where I grew up, the tone has been somewhat more respectful than it is on the average in the 'hot kitchen' of the internet.

In this regard, it is interesting to read these two pieces on the Edge:

Cyber Disinhibition

and

More Anonymity is Good

15. **Yatima**
May 2, 2008

Or you can apply technology:

http://savingtheinternetwithhate.com/

16. **Peter Woit**
May 2, 2008

Over at CV, "anonymous" is on the anonymous attack:

http://cosmicvariance.com/2008/04/28/vacation-2/#comment-315873

I wrote a response there, which didn't appear. Maybe it was caught by their spam filter, maybe I'm persona non grata. Anyway, for those following both threads, here's a copy:

"The comment that "anonymous" is complaining was censored by me was posted here:

http://www.math.columbia.edu/~woit/wordpress/?p=681#comment-37499

and my response to it is here:

http://www.math.columbia.edu/~woit/wordpress/?p=681#comment-37524

What happened in this case was that the comment was initially identified by the WordPress spam filter as spam, so not immediately posted. When I checked the spam queue that day and found it, I dug it out, posted it and responded to it. It seems that "anonymous" couldn't even be bothered to actually read the comment thread in question before posting accusations attacking me here. If "anonymous" were using their real name, they might find this embarrassing and it might do some damage to their reputation. But, hey, when you"re "anonymous", that's something you never have to worry about!"

17. **anonymous**
May 2, 2008

Peter,

the comment that was censored was posted to this thread, last night, and would have been the first comment on this thread if it hadn't been censored. As for my anonymity, it's because I'm actually an "anonymous" nobody, an undergraduate student whose name doesn't show up in a google search. I post comments anonymously because I don't want to make it possible for a google search to reveal my blog reading habits.

best,

-anonymous

18. **milkshake**
May 2, 2008

I hate trolls who change their name frequently, whenever they write ad hominem insults. But if someone decides to create a web persona for himself and is consistent in using it and his critical arguments are of substance, there is no reason why he shouldn't remain anonymous if he has chosen so.

One thing that is pretty funny though is an morally-outraged anonymus commenter.

A problem with having a "serious" web presence – with a web page where people can comment – is that one feels a compulsive urge to see who is commenting and why, by tracking those IP addresses and by answering every comment…

19. **woit**
May 2, 2008

"anonymous"

If you bothered to look, you would notice that the comment in question was not "censored", but is there at the top of this comment thread. This is the second comment from you anonymously attacking me over the Christopher Hitchens issue. Both ended up for unknown reasons in the spam queue, both were retrieved by me from there and posted (on two different days). You have now posted 5 anonymous comments here on this topic, from two different IP addresses, using two different fake e-mail addresses. Please grow up. If you want to start behaving like an adult, please write to Sean Carroll and request that he remove the inaccurate comment you posted on CV.

20. **DB**
May 2, 2008

Peter, I think it's perfectly reasonable to take the stand you take. Not that it's not fine to take the alternative viewpoint of simply ignoring them. Given the diversity of human response, your forthright approach can strike some as being "oversensitive" while a passive, indulgent policy could give others the impression of timidity, appeasement, and unwillingness to defend one's views.
In such situations one should just go with one's instincts. What is not acceptable is to operate an internet blog where anonymous commentators are shepharded by a blog moderator in order to develop a coherent attack on an individual so as to chase them from the scene. That's nothing less than bullying, and bullies should never be appeased.

However, there is a rational utilitarian approach to selecting the optimum strategy:
Because the jury remains out on string theory, and is likely to do so for the foreseeable future, the debate will remain highly politicised. And while this is a scientific debate and not a political campaign, the lack of any prospect of a decisive resolution of the issues means that it takes on many undesireable features associated with political debates. In this respect, packs of attack dogs in the guise of hostile anonymous commentators represent a form of "negative campaigning", and in such situations, a robust confrontational approach is preferred because it communicates the strength of your convictions while providing additional opportunities to publicly target the weakness of an opponent's position. Look where a passive strategy landed Mike Dukakis.

From this perspective, the choice of approach to be used against trolls would ideally depend on the degree of politicisation of the debate, which is why I think you should stick to your guns, metaphorically speaking.

21. **Julianne**
May 2, 2008

Peter — Your comment was indeed hiding in our spam filter, which has been a tad overagressive of late. Whenever someone puts html in the comment, there's a much higher chance of it winding up with an erroneous spam classification. I've rescued it from the filter.

22. **Peter Woit**
May 2, 2008

Thanks Julianne.

Thanks DB. I think your analysis of the situation is spot on.

23. **anonymous**
May 2, 2008

Peter,

I did indeed bother to look. That comment wasn't posted there until just now. I checked before I wrote anything at CV. Lots of comment appeared before mine, and mine was the first to submitted last night. You can see how I thought you were censoring it. I don't have a blog so I'm not familiar with spam filters etc. Also, you see two IP addresses because I posted comments from two different computers, since I don't have a computer of my own. And as for the fake email accounts, an email is required, and — you guessed it — I don't have an email account.

Milkshake: why can't an anonymous commenter feel moral outrage? What's wrong with being anonymous? I don't get it.

-anonymous

24. **anonymous anti-string-theorist**
May 2, 2008

So, I guess if you want to anonymously attack, insult or slander people you disagree with about a scientific issue, Cosmic Variance is open for business.

God, I never realized [*deleted*] was such a total wanker!!!

[*As far as total wankers go, the author of this comment knows a lot about the subject. He repeatedly posts juvenile comments like this one here, from the IAS in Princeton (IP number 192.16.204.77). The last one I didn't delete was this:*
http://www.math.columbia.edu/~woit/wordpress/?p=620#comment-30851
]

25. **Noah**
May 3, 2008

Sticks and stones can break your bones, but anonymous ad hominem attacks in the comment section of blogs really can't hurt you.

The "Internet" is where a lot of people come to blow off steam, overstate their case, and act like jerks. They're stressed out from spending most of their day quietly putting up with the trials and tribulations of life, and they feel like they need to be a jerk somewhere, to someone, just to keep their sanity.

I know it, because I've been there. When I was seventeen I used to go into Christian Singles Chat and talk like a caveman (trust me, it was funnier than it sounds). Then I used to anonymously email married men who posted personal ads looking for affairs, and tell them they had small penises. I'm not especially proud of this behavior, but all in all, there are plenty more harmful ways I could have released my pent-up frustrations.

So Peter, I like what you write, but here's a bit of friendly advice: Try to empathize with the trolls. And if you can't, at least grow a thick skin, because this is The Internet, the last wild frontier, and the trolls will never ever be stopped.

26. **milkshake**
May 3, 2008

Anonymous moral outrage: When I was kid in Prague in 80s we got a visit from State Security and they gave us a lecture on fortitude and moral fibre. I stil remember the officer explaining: “They say snitch or grass – but I prefer to call them advisor. Please advise us the very moment you notice some wrongdoing, advise us even anonymously”

27. **Anonymous (OH NO!)**
May 3, 2008

Its not my intention to spread hostility, but to be blunt and concise- Who cares if some random dude is talking shit about you on CV. I don’t think it merits your attention, or the attention of your blog-readers. I read all the linked postings you made there, and it was a huge waste of time, i’m sorry to say. Seriously, I come here to read about a bunch of physics and math that I don’t understand, and instead, i’m greeted with something I know all too well- DRAMA. Lame! Bring back the science already.
PS I won’t be offended if you delete this, ‘cus who cares!!!

– dude that doesn’t hate your blog

28. **Professor R**
May 3, 2008

Hi Peter, it’s a pity to see you and Sean C at loggerheads, as Not Even Wrong and Cosmic Variance are the only two blogs I find myself reading regularly (they’re also the only physics blogs I recommend on my own humble effort at http://www.coraifeartaigh.wordpress.com).

I think overall you’re right, though, and would like to add my own tuppence-worth; there is already far too much space taken up by random opinion on blogs – personal attacks, especially annoymous attacks, is simply a waste of space and everybody’s time…
Regards, Cormac

29. **Eric**
May 3, 2008

Peter,
Why not just admit that making a big issue about anonymous posters who attack you is really just a strawman argument that you use as a diversion whenever someone makes a point that you don’t like? This seems especially to be the case when you are losing a debate.

30. **Peter Woit**
May 3, 2008

Eric,

The recent anonymous attacks Sean is hosting aren’t about scientific issues, but are purely ad hominem. I think you’re right that this phenomenon has to do with someone losing a debate, but it isn’t me….

31. **anonymous**
May 3, 2008

Milkshake,

That has nothing to do with this situation, as far as I can see. You’re overgeneralizing from your experience. Like people who’ve had a bad experience with a cop who then decide that all cops are bad. Anonymous blog comments are not the same as informing on people. Compare the consequences of each. It’s a difference of kind, not of degree.

32. **tommaso dorigo**
May 3, 2008

my very own two pence: following o.wilde, it is not important what they say, but it is always a good sign if they talk about you.

Peter, for good or worse, has become a flag of the resistence to stringdom. And it shows…

cheers,
t.

33. **George**
May 3, 2008

People are constantly hurling anonymous abuse at each other on the internet, especially on blogs. The fact that some anonymous “string theorist” is hurling abuse at you should be taken in the context that it’s an anonymous comment on a blog with numerous anonymous comments. i.e. give it the extremely minimal consideration it merits. Maybe you can decide that after every n=5 idiotic anonymous comments, you write a short response explaining why you feel the comments are idiotic. If it were me though I wouldn’t even bother, as the effort expended in getting riled up is not worth it. But I’m not a “blog person” so what do I know.

34. **Arun**
May 3, 2008

Anonymous insults hurled around on a blog rate slightly less relevant than string theory.

35. **Observer**
May 4, 2008

Be a bigger man and ignore those anoymous insults, most probably came from someone who was “working” in “new directions” and “noble ideas” and was uncovered and sacked by his/her department chair, obviously is no longer a happy string theory camper if ever was.

36. **Ellipsis**
May 4, 2008

IMHO, if I made the rules, moderation of blog comments should be based purely on whether the content is clearly profane, libel by a strict U.S. legal definition, or commercial spam — not adjusted based on whether the commenter is anonymous or not. Otherwise, don’t worry at all, some people will be jerks but that’s life, people will see them for the jerks they are. I think people should probably have the right to comment anonymously (note, I may be biased!). Sometimes that results in some asinine comments, but other times it helps the truth to come out.

37. **Peter Woit**
May 4, 2008

Many commenters here are (anonymously) making the point that anonymous insults and personal attacks are just part of how blogs work, so one should accept this and get used to it. While this is an accurate description of the way things mostly are in the physics blogosphere, I think this is an unfortunate situation, one that could be changed.

If you want to see how things can be different, go take a look at some of the excellent blogs run by research mathematicians, e.g. sbseminar.wordpress.com. Almost all comments are polite, professional and come with a name attached. This situation encourages participation by serious people, and the quality of this is very high.

Physics blogs by contrast are overrun by anonymous commenters, with a significant fraction of irresponsible ones. I delete many such comments, probably should delete more. Once you allow anonymous insults and attacks, you drive the level of discussion down to that of the overall blogsophere. This drives up the number of comments, but ensures that very few serious professionals are going to want to participate.

One possibility would be for me to take some commenters advice and embrace the way things are. In that case, the way to deal with my disagreement with Sean on this issue would be to anonymously post comments here, on his and on other blogs attacking him. According to what he wrote to me, he himself would have no problem with this, so why shouldn't I do it? To me, the reason seems obvious: does one want to make the world a worse or better place?

Please, I encourage everyone who posts comments here to avoid doing so anonymously unless they have a good reason, in which case they should take extra care to behave responsibly. I remain convinced that it is possible to raise the level of discussion in physics blogs to something closer to that of mathematics blogs.

38. **João Carlos**
May 4, 2008

Your conclusion about the way to deal about your disagreement with Sean is a complete *non-sequitur*… Sorry to say that, but he's right and, this time, you're "not even wrong".

39. **Peter Woit**
May 4, 2008

Joao Carlos,

You don't exactly make much of a substantive argument in response to what I write, you could at least point out explicitly the non sequitur in what I write. Again:

Sean claims:

"Personally, I could not care less whether a comment is anonymous or signed. It just makes no difference to me."

Actually I think this is obviously an untruth: if someone wrote in a comment to his blog attacking him, whether it was signed and by whom would actually make a difference to him. But taking this at face value, if I'm annoyed at him, and want to get some choice comments about him and his behavior out before a large audience, should I do this and should I put my name on these or not? Commenters here urge me to just accept that anonymous attacks are a normal part of the physics blogosphere, and Sean has no problem with them, so why shouldn't I go ahead? Obviously I think this would be a bad idea, and I've explained why I think so.

40. **anonymous**
May 4, 2008

Peter,

I for one promise to never post anonymously here again, even though I don't see a problem with it … it's your blog after all, so I will respect your opinion on this issue. I think I have a good enough reason to want to remain anonymous as I've already explained, but clearly my anonymous comments have not been welcome here so I'll be signing off.

Respectfully,

-anonymous.

41. **John R Ramsden**
May 4, 2008

I'm ashamed to say I find these controversies hilariously funny, although I suspect I'm far from alone in that. I do see the serious side though, where people think their reputations may be threatened; but those fears are probably greatly exaggerated.

My only slight objection to posts by "anonymous", aside from the obvious potential for unaccountable innuendo, invective, or defamation etc, is that they can't be put in context of other opinions expressed by the same individual.

That's often fine, in a "res ipse loquitur" sense, where "anonymous" chips in with indisputable facts for example. But in a long discussion someone who consistently uses a unique nickname is still anonymous but establishes a context, for good or ill. In other words, regular readers recognising the nickname know where that poster is likely to be coming from.

It also avoids confusion where there's more than one "anonymous" in the same discussion – I've seen that happen, and the result can be surreal!

42. **João Carlos**
May 4, 2008

Professor Woit,

Prof. Carrol says **he** doesn't care. The reason **you** should not do this is because **you care**. You can't have it both ways.

If you think that it's – at least – a lack of good manners (and I fully agree), it's a *non-sequitur* if you propose anything otherwise.

And, by the way, everyone knows the one behind those "anonymous" posts. So, as Prof. Carrol says, "why bother?" If you don't want to get mad, get even. Reply anonymously… if you really think those *ad hominem* comments deserve a reply, other than a significant lack of response.

May I remember you of the old saying: "Don't feed the Troll…"?

43. **Peter Woit**
May 4, 2008

Joao Carlos,

Actually I don't know who is behind these anonymous posts. If you do, please tell, and explain how you know this.

And no, if there's an anonymous response to these posts, it won't be coming from me.

44. **geometer**
May 4, 2008

Not sure whether WordPress allows this, but some other blog providers offer a number of privacy option including disallowing all anonymous posts. It should not be hard to make posting password protected. Perhaps this is what you need for your own blog. As for other people's blogs, I think the best attitude is to never worry about things beyond your control.

45. **Ellipsis**
May 4, 2008

I like your blog very much and do think attacks on you and Lee Smolin are often unfair — and that indeed tends to be biased toward the anonymous ones.

But the reason I, and I think a good set of others, happen to be anonymous is, I think, a basically reasonable one: if my colleagues see me commenting in a lot of physics blogs, they will know that I'm not doing "real work" and I think my professional reputation will slightly suffer. One's definition of what "real work" is can most certainly vary, and I do think people should certainly not be penalized for this sort of thing, but things are what they are in a lot of the physics world, and if one bans anonymous commenting, one can often throw out a few decent babies with bath water. Unless we can somehow single-handedly change this view among many people of what "real work" is in physics, then if you happen to want this set of opinions, for what they are worth, I would vote that you please don't uniformly insist on people using their names.

46. **Peter Woit**
May 4, 2008

geometer and ellipsis,

Technically it would be possible to enforce non-anonymity in a blog comment section. I have no intention of doing this, but do think it's a good idea to encourage people to use their names. I do understand why many people prefer not to use their names, for several legitimate reasons including the one given by Ellipsis.

I suspect one of the main reasons people are reluctant to use their names on physics blogs has to do with the unfortunate politicization of discussions involving string theory. Saying something even tangentially related to one of the hot-button topics surrounding the theory, or posting a comment on a blog identified with one side of the controversy can all too easily get one on someone's enemies list (or subject one to personal attacks on blogs, whether nonanonymous, eg. Lubos, or anonymous). This is really a shame, but I guess until it changes many sensible people will not want their names to appear on blogs.

47. **Me**
May 5, 2008

You can make people sign-in with openid/typepad and while they would still be anonymous, no one would be able to impersonate someone else.

48. **Thomas R Love**
May 5, 2008

Peter, You should be proud that people are attacking you. That means your work is influential and your attacks on string theory are spot on. If you were wrong, the critics would offer something proving string theory. They can't , so they attack you. The problem is when people ignore your work. So, I offer my congratulations to you! Keep up the good work. When can we expect the sequel to NEW?

49. **Peter Woit**
May 5, 2008

Thanks Thomas,

To write a sequel to Not Even Wrong, there would have to be some new developments to write about, and remarkably little has changed since the book was written back in 2002. So, no plans for a sequel any time within the next few years, maybe never. I do joke sometimes that my next book may be a comic novel created by cutting and pasting things from blog entries and my e-mail. But one problem with that is that readers probably would find the whole thing implausible…

50. **Gil Kalai**
May 5, 2008

Ethics of blog discussions is an interesting topic. I can understand Sean's position that vulgar or abnoxious comments will be deleted whether they are anonymous or not, and I also understand the point of view that anonymous commentators should be more restrained in their behavior.

Perhaps one thing we can agree on is that invented names who are themselves insulting should not be permitted. I remember in the earlier days of NEW a commentator whose nickname was of the form "X makes me puke" being permitted to comment. Perhaps a little consensus we can reach is not to allow insulting nick-names in the future.

(We also have to be careful about ethics which is sensitive to personal attacks but is tolerant to attacks against a large group of people.)

51. **Amos**
May 5, 2008

If you don't want people making anyonmous derogatory comments about you and your work than you should not have published a book or started a blog. It comes with the territory.

I guess I think this whole issue is odd. Its like when Lubos banned all links from this blog to his. What's the point?

Considering the two issues, it reminds me of what Kissinger said: Academic disputes are so bitter because the stakes are so small.

52. **anonymous**
May 6, 2008

I think anonymous is a scoundrel!

anonymous

🙂

53. **Anonymous Hater of Peter Orland**
May 6, 2008

That S.O.B., Peter Orland would probably tell you to just ignore anonymous insults at other blogs, because they are beyond your control, and you can't be liked by everybody. But hey, he'sjust a stupid jerk.

P.S. He's ugly too.

54. **Jack Lothian**
May 6, 2008

I think that people who throw insults & hide behind "anonymous" are persons of low ethics and morality but trying to enforce this morality on the web is a challenge. Creating fake identities is not hard and the sheer volume of this nonsense can be overwhelming sometimes. If I ran CV and had the time, I would delete such responses but I do not know their circumstances so I say it is their call. My recommendation is enforce this policy on your blog if you have the time & ignore these gadflies on other blogs. Everyone who counts will ignore them so why not join the crowd.

55. **Juan R.**
May 9, 2008

Peter, anonymous attacks on the Internet are the rule. I recommend you to follow guidelines when dealing with internet critics. I recommend you specially the guideline: *do not respond to obvious flame bait and red-herring arguments*

> It is a pointless waste of time, energy, bandwidth, and disk space to respond to these insults. Flames and red-herring are intended to confuse the reader or divert attention away from the subject.
>
> Some people are masters of changing the subject. This even includes the subject title. Flames often are attempts to hide the poster's ignorance on some topic by inciting a series of angry responses. Ignore this nonsense.

I personally attach guidelines

guidelines for online scientific discussion

to my postings on several online services, including Usenet sci.physics.research and sci.physics.foundations.

Regards.

“So what will you do if string theory is wrong?”

May 5, 2008
Categories: Uncategorized

In a new preprint of an article entitled “So what will you do if string theory is wrong?”, to appear in the American Journal of Physics, string theorist Moataz Emam gives a striking answer to the question of the title. He envisions a future in which it has been shown that the string theory landscape can’t describe the universe, but string theorists continue to explore it anyway, breaking off from physics departments to found new string theory departments:

> So even if someone shows that the universe cannot be based on string theory, I suspect that people will continue to work on it. It might no longer be considered physics, nor will mathematicians consider it to be pure mathematics. I can imagine that string theory in that case may become its own new discipline; that is, a mathematical science that is devoted to the study of the structure of physical theory and the development of computational tools to be used in the real world. The theory would be studied by physicists and mathematicians who might no longer consider themselves either. They will continue to derive beautiful mathematical formulas and feed them to the mathematicians next door. They also might, every once in a while, point out interesting and important properties concerning the nature of a physical theory which might guide the physicists exploring the actual theory of everything over in the next building.
>
> Whether or not string theory describes nature, there is no doubt that we have stumbled upon an exceptionally huge and elegant structure which might be very difficult to abandon. The formation of a new science or discipline is something that happens continually. For example, most statisticians do not consider themselves mathematicians. In many academic institutions departments of mathematics now call themselves “mathematics and statistics.” Some have already detached into separate departments of statistics. Perhaps the future holds a similar fate for the unphysical as well as not-so-purely-mathematical new science of string theory.

This kind of argument may convince physics departments that string theorists don’t belong there, while at the same time not convincing university administrations to start a separate string theory department. Already this spring the news from the Theoretical Particle Physics Rumor Mill is pretty grim for string theorists, with virtually all tenure-track positions going to phenomenologists.

I have some sympathy for the argument that there are mathematically interesting aspects of string theory (these don’t include the string theory landscape), but the way for people to pursue such topics is to get some serious mathematical training and go to work in a math department.

The argument Emam is making reflects in somewhat extreme form a prevalent opinion among string theorists, that the failure of hopes for the theory, even if real, is not something that requires them to change what they are doing. This attitude is all too likely to lead to disaster.

Update: A colleague pointed out this graphic from Wired magazine. Note the lower right-hand corner…

Update: Over at Dmitry Podolsky’s blog, in the context of a discussion of how Lubos’s blog makes much more sense than this one, Jacques Distler explains what it’s like for string theorists these days trying to recruit students:

> Unfortunately, I’ve seen a number of prospective graduate students, who spent their undergraduate days as avid readers of Woit’s blog, and whose perspective on high energy physics is now so hopelessly divorced from reality that the best one can do is smile and nod one’s head pleasantly and say, “I hear the condensed matter group has openings.”

Comments

1. **anon.**
   May 5, 2008

   I have some sympathy for the argument that there are mathematically interesting aspects of string theory (these don’t include the string theory landscape)

   The string theory landscape (i.e., the space of string vacua) is, from another point of view, a space of 2d CFTs. Why isn’t that an interesting mathematical object?

2. **Peter Woit**
   May 5, 2008

   anon,

   Maybe there’s some interesting global structure to the space of all 2d CFTs, but people have been looking for such a thing for years and I don’t see any progress. In any case, that’s not what people working on the landscape are doing, instead they’re looking at specific points or neighborhoods in this space, and these are typically not mathematically very interesting.

3. **Jonathan Dursi**
   May 5, 2008

   Somehow I have a hard time seeing grad students and undergraduates beating down the door of admissions clamouring to be part of a program in this new department of not-physics-not-math.

4. **weichi**
   May 6, 2008

   hmm, a new department claiming to have insight into the nature of reality, but not mathematics, not physics … does he really think the philosophers are going to let them horn in on their territory like this?

5. **Chris Oakley**
   May 6, 2008

   I am having a big problem with this:

   > So even if someone shows that the universe cannot be based on string theory, I suspect that people will continue to work on it. It might no longer be considered physics, nor will mathematicians consider it to be pure mathematics. I can imagine that string theory in that case may become its own new discipline …

   … if a mission that you have been embarked on for over thirty years turns out to be futile then dumping it is not only sensible – it is a *moral obligation*.

6. **Urs Schreiber**
May 6, 2008

> that's not what people working on the landscape are doing

But maybe it's what they should be doing. (Well, some do, but not those in the string theory departments.)

A theory is independent of what people do about it. If all present attempts to address open question X are inadequate, it doesn't necessarily imply that question X is uninteresting.

7. **Bee**
May 6, 2008

Hey! Adrian is going back to NY!

Reg the question what would the string theorists do in case. Well, one couldn't just fire all these string theorists, what were they supposed to do? If they were able to, they might change fields, but we all know this isn't easy especially if the education has been narrow and one has passed the fifty or so. Maybe one should offer a re-education program or something. There is without doubt a strong specialization going on in the community. Many of these people are extremely smart and could make contributions to many other fields, but who is going to hire somebody who has worked 10 years on string theory for, say, an astrophysics position?

That being said, I too would guess that those who couldn't jump off the field but could keep their job would just continue working on it. Well, you know what they say, progress isn't made from conference to conference, but from funeral to funeral. Maybe string theory would become an art form, or a branch of philosophy? Who knows.

8. **Christine**
May 6, 2008

Well, astrology has been proved to be wrong, but I'd say there are more astrologers out there than astronomers, and for certain they are making more money than the latter, so I'd not be surprised.

Now, seriously, if string theory turns out to be proven wrong, I hope people who devoted they careers to it are reasonable enough to pursue other lines of research, or at least admit that string theory is pure mathematics and treat it as such.

9. **piscator**
May 6, 2008

I think an equally appropriate question Peter is what would you do if string theory is shown to be correct? 😉

10. **Peter Woit**
May 6, 2008

piscator,

Doesn't seem to be going that way, does it? But if it does, probably I'd keep working on the same things I've been working on for years. Maybe I'll pay less attention to gauge groups and more to Diff (S^1)….

11. **Daniel de França MTd2**
May 6, 2008

Poor Witten.

12. **Maarten Bergvelt**
May 6, 2008

Peter wrote:
> Maybe there's some interesting global structure to
> the space of all 2d CFTs, but people have
> been looking for such a thing for years and
> I don't seen any progress.

Isn't the space of superconformal field theories
supposed to be the topological modular form spectrum? Work of Hopkins, Lurie, Teichner and Stolz etc. Maybe not of physical interest but algebraic topologists are very excited about this, I think.

13. **blop**
May 6, 2008

It sounds like Clinton: I lost but I stay…

🙂

14. **Peter Woit**
May 6, 2008

Maarten,

Sure, if you look at special classes of 2d CFTs, you get extremely interesting mathematics, one example is the one you mention. The problem is that the class of CFTs the landscape people are interested in because it is supposed to unify physics doesn't seem to be a mathematically interesting one.

This has always been the problem with string theory: some parts of it lead to deep and wonderful mathematics, but those have always turned out to be orthogonal to the parts of string theory you get into when you try and use it to unify 4d gravity and the standard model.

15. **Shantanu**
May 6, 2008

Peter, have you looked at Witten's talk at the ongoing Dark energy symposium at
STSCI

16. **Ethan Siegel**
May 6, 2008

Is there really anything wrong with that?

"Well, we had an interesting idea, and gave it a go, but it doesn't look like it panned out. Still, this is a fun system to play with, and we think that we'll keep playing with it, even if it doesn't have any relation to physical reality."

Of course they're going to have to stop calling it physics, which they've agreed to. But how is this less "valid" than, say, searching for all of the Mersenne primes? Or, for a physics analogy, solving mathematically interesting cosmologies that simply don't happen to be ours?

17. **Peter Woit**
May 6, 2008

Thanks Shantanu, very interesting. Maybe I should write a blog entry about that…

Ethan,

Sure, anybody whose research program fails is welcome to decide that he or she wants to keep going anyway. The problem I'm trying to point out is that it is unlikely that either the physics or math communities are going to support this. So, unless you've got tenure and don't care whether you have a grant and graduate students, you'll probably find that you can do this but no one is going to pay you to do it, or hire any students who work with you on it.

18. **estraven**
May 6, 2008

I have seen some string theory physicists do exactly that: study maths, become mathematicians, and keep working on problems they find interesting for physical reasons. They have the bonus advantage of being able to speak (or at least read) "physiquese", something most pure mathematicians (like me) usually can't do and occasionally is useful.

19. **Thomas R Love**
May 6, 2008

The big question is: "When string theory is proven not to correspond to physical reality, will any journals publish research on string theory?"

20. **Benni**
May 6, 2008

I think string theory is still an excellent topic for graduate students at this time.

Since string theory contains both QFT and GR, and also some math, one can switch to these three fields later on.

If the LHC finds a simple higgs-sector, which will be theoretically explored in relatively short time, phenomenologists will have problems, because there won't be many problems left to solve..

I dont think that a phenomenologist can become a mathematician, or a researcher on pure quantum gravity.

Where as i think it is sure, that it can be expected from a string theorist, he can switch to mathematics, phenomenology, pure quantum gravity, or genral relativity.

21. **RK**
May 6, 2008

http://xkcd.com/171/ !

22. **Thomas Larsson**
May 6, 2008

"Well, we had an interesting idea, and gave it a go, but it doesn't look like it panned out. "

Well, this is pretty much what we wanted Witten and others say for a decade. With the notable exception of Dan Friedan, no string theorist has been willing to say it, though.

23. **Peter Orland**
May 6, 2008

>Benni Says:

>I think string theory is still an excellent topic for graduate students at this time.

>Since string theory contains both QFT and GR, and also some math, one
>can switch to these three fields later on.

It isn't easy or practical for some people to switch academic fields without some kind of temporary position, such as a postdoc in the meantime. If string theorists don't get postdocs, they usually have to leave academia fairly.

> I dont think that a phenomenologist can become a mathematician, or > a researcher on pure quantum gravity.

Though I am not a phenomenologist, I know that some of them certainly could make such a move.

24. **Peter Orland**
May 6, 2008

Fairly quickly, I meant.

25. **big vlad**
May 6, 2008

“Where as i think it is sure, that it can be expected from a string theorist, he can switch to mathematics, phenomenology, pure quantum gravity, or genral relativity.”

that’s like saying a 100m sprinter could switch to american football, or rugby.

26. **Bjoern**
May 6, 2008

Today, Alvarez Gomé gave a talk in Munich about the riddles in fundamental physics. He tried to make of the “self-righteous popperazzi”; but in fact he seemed very hurt by the critics of string theory, including Peter, whom he mentioned at the beginning of his talk, together with Lee Smolin.

He also made an interesting and strong statement that was new to me: only string theory can explain gauge theory. Asked why, he stated that string splitting/breaking is equivalent to a gauge theory.

In the introduction to the talk, Dieter Lüst explained that a new faculty position on “higher dimensions” is available in Munich. A faculty position on something unobserved. Times are bizarre …

27. **Peter Woit**
May 6, 2008

Bjoern,

When I was in Lisbon last fall I had the chance to talk to both Lust and Alvarez-Gaume, and we had some interesting and worthwhile discussions, which were polite but sometimes forceful. Alvarez-Gaume is certainly not happy with all the criticism that string theory has been getting. As a leader of the CERN theory division and a string theorist, I’d guess the public criticism and change in attitude has made his job significantly harder and put him on the defensive.

Unfortunately, I think this has made him focus on some of the unfair criticism string theory has gotten, and upon making a forceful defense, instead of acknowledging that there is a legitimate problem to be discussed. I don’t know exactly what claim he was making, but the statement that “only string theory can explain gauge theory” is simply not true. Going too far in its defense is not going to be helpful to the interests of either string theory or of particle theory in general.

28. **Joseph Triscari**
May 6, 2008

I’ll update the old joke:

The dean of the school of science was scolding the chairman of the physics department:

“Why,” he asks, “does it cost so much money to run your department? Why must you purchase all this equipment?

“Why can’t you be more like the math department. They only need pencils, paper and garbage cans. Or better yet, like the string theory department! They only need pencils and paper…

29. **Eric**
May 6, 2008

In the words of Einstein, “I would be sorry for the dear Lord. The theory is not wrong!”.

30. **JC**
May 6, 2008

Thomas R Love,

If mainstream particle theory journals ever ceased to publish string theory papers, then most likely the remaining string theorists will start their own “peer reviewed” journal dedicated to string theory.

31. **Gil Kalai**
May 7, 2008

It is nice to see scientists writing papers on philosophical or foundational issues and in this respect Emam paper is quite welcomed. As for the content, I did not find the paper very convincing. At present, both a success of string theory or its failure are well beyond the horizon. A failure will most likely not be of the form of a single bit of information coming from the sky, but probably will be related to major other developments, theoretical or empirical, and thus will probably give people in the field a lot to work on.

32. **Bjoern**
May 7, 2008

Peter,

indeed, your description of Alvarez-Gaumé’s feelings is precisely the same that I got in his talk. My opinion is that there is a huge difference between saying “string theory is useless because it makes no predictions” (as you do) and attacking string theorists personally (AG mentioned a ferocious literary critic called “Steiner” which must have offended him in a public meeting recently).

It is important for one’s personal image and also for one’s personal well-being to keep and upheld this difference.

AG was also annoyed by the statement “the end of science is there” (citing John Horgan explicitly) and explained how many open problems there are in biology, chemistry etc.

I looked again into my notes. I did not copy the exact statement from his slide, but he really made the statement that no other theory except string theory explains gauge theory from a more basic point of view. The question that was asked only asked how string theory deduces gauge theory. Why no other theory does so was not addressed.

Nevertheless, Alvarez-Gaume was well-balanced, and I am sure that one can have interesting professional discussions with him.

Bjoern

P.S. In my first post, the sentence should have read: “He tried to make fun of the …”.

33. **Peter Woit**
May 7, 2008

Bjoern,

The conference in Lisbon I was referring to was organized by George Steiner, and John Horgan also spoke. Both had quite critical things to say about string theory, but neither is a physicist (Steiner is a literary critic, although somewhat of a polymath).

34. **Peter Shor**
May 7, 2008

Bee says “Reg the question what would the string theorists do in case. Well, one couldn’t just fire all these string theorists, what were they supposed to do?”

Well, you certainly can’t fire a lot of them … they have tenure. I believe that pretty much the only way to fire tenured faculty (if they aren’t guilty of gross misconduct) is to dissolve the department they are in. So if Moataz Emam gets what he is wishing for, and university adminstrators start creating Departments of String Theory, I would say this would be very bad news indeed for string theorists.

35. **Gil Kalai**
May 7, 2008

yes, yes and just imagine what will happen to mathematics departments if a contradiction in mathematics will be found.. and if the view regarding the “end of science” will previal we will have to rename “science faculties” to “post-end science faculties”…

Seriously, I am skeptical if this type of fantasies are the right avenue for skepticism in science, which shoud be welcomed.

36. **Coin**
May 7, 2008

yes, yes and just imagine what will happen to mathematics departments if a contradiction in mathematics will be found..

Indeed… who could imagine how mathematics departments would cope if such a strange, unheard-of thing were to happen?

I think the thing to keep in mind here is, mathematics disciplines can validly motivate themselves entirely on internal metrics of progress. You can have subjects in mathematics putter along happily for years in the complete absence of any practical application, and no one really minds. If you’re going to describe yourself as *physics*, though, it’s not enough to be internally consistent. You also have to coincide with what is true in the real world. And the real world simply happening, coincidentally, to not be the same as some theory or other is a real, constantly looming possibility which needs to be taken seriously for all physics theories– even established ones, I think.

(I personally would find string theory a lot more interesting if it did jump ship and decide to become a mathematics discipline rather than a physics one. As Woit notes above there are some rather interesting bits of math in string theory but they get overshadowed if you think about string theory as a way of unifying physics. Like, I want to know more about these “fractal string” things. And isn’t it true that even if String Theory is “proven wrong” as a physics unification theory, AdS/CFT will continue to be a relevant mathematical tool in physics?)

and if the view regarding the “end of science” will previal we will have to rename “science faculties” to “post-end science faculties”…

Heh, this is a fun point though. I wonder if Post-Science is similar to post-modernism or post-rock.

37. **Thomas R Love**
May 8, 2008

I found this quote:

I can now rejoice even in the falsification of a cherished theory because even this is a scientific success

—John Carew Eccles

Rejoice!

38. **Dmitry Podolsky**
May 9, 2008

Dear Peter

I’ve got an impression that the question “what will you do if string theory is wrong” as you presented it actually sounds empty. To define whether string theory gives the wrong answer to the question you ask, you first need to ask the question.

If your question is “does landscape have anything to do with objective reality”, then the answer could be very well “no” (although it could very well be “yes”).

If your question is “does string theory have anything to do with strongly coupled regime of gauge theories or, say, 3D Ising model”, then, I guess, the answer should be already known to people around.

I think, if landscapism will eventually die out, string theorists will start working on problems requiring string theory to solve them such as strongly coupled YM.

Cheers

39. **Peter Woit**
May 9, 2008

Dmitry,

I’m not the one going on about “string theory is wrong”, what I was doing in this posting was linking to an article by a string theorist on this topic. “String theory” now refers to a huge array of different kinds of things, from the quite interesting to the nonsensical. The statement “string theory is wrong” is meaningless, and not one that I make.

To respond here to the posting on your blog, I'd like to point out that your characterization of what I wrote here as: "String theory is wrong, all string theorists should be immediately fired" has nothing to do with what I actually wrote or what I think. At least you're not referring to me as "Peter Whore" or calling for my death, but you are adopting Lubos's tactic of drastically misrepresenting the arguments of other people.

The gist of my comment on the article here was that the idea that string theorists should give up on being part of physics departments and start their own separate departments is unrealistic, since virtually no university is going to fund this. The comment that this year in the US virtually all tenure track theory jobs are going to phenomenologists, not string theorists, is simply factual and not made as a value judgment. The kind of work that I think needs to be done, deeper formal investigations of quantum field theory, is every bit as unpopular as string theory, and tenure-track jobs are not going to people doing this either.

40. **Thomas Larsson**
May 9, 2008

If your question is "does string theory have anything to do with strongly coupled regime of gauge theories or, say, 3D Ising model", then, I guess, the answer should be already known to people around.

Does string theory have anything to do with the 3D Ising model? If so, what? Can you use it to extract some numbers exactly? If so, which numbers and what are their values?

CFT has of course a lot to do with 2D Ising, but that is an almost trivial toy model in comparison.

41. **Peter Woit**
May 9, 2008

Dmitry,

Rereading your blog posting, I see that you are also completely misrepresenting Peter Shor's comment. He was not regretting that string theorists cannot be fired "because they are on a tenure track". He was pointing out that separate string theory departments would be dangerous for string theorists, since essentially the only way US universities can (and do) fire tenured people is by closing down their department. If string theorists had 20 years ago organized themselves into new separate departments, there would be a danger now that universities would start closing these.

And, just being "on a tenure track" does not at all make it difficult to fire someone, they need to actually have been tenured.

42. **Eric**
May 9, 2008

Peter,
Regarding the issue of faculty hires this year, don't you suppose that the reason phenomenologists have been hired has something to do with the fact that LHC is about to turn on for the first time? It seems a little dishonest to me to intimate as you do that this is a sign that string theorists are falling out of favor. Once the results from LHC are in, then we should probably expect more string theory hires if all goes as expected.

43. **Dmitry Podolsky**
May 9, 2008

Peter

If I misitepreted your or Peter Shor's words, I apologize. Nevertheless, your post left me with a particular impression (and I have to confess that it did not dissolve), and that was the reason why I wrote what I wrote.

Thomas

For 2D Ising, there are two equivalent formulations of the theory: in terms of spin variables \sigma and in terms of disorder variables \mu (the ends of dislocation lines). Although \sigma and \mu satisfy rather complicated equations, their product satisfies linear equation; that fact allowed Onsager to find exact solution of the 2D Ising model.

For 3D Ising dislocation lines become dislocation surfaces, boundary of such surface is the variable on which disorder variable \mu depends. One hope to find simple equations describing 3D Ising is to construct product of \mu (C) and \prod \sigma (x_i) where x_i are points close to the loop C. The first impression was that such a loop C together with "spin" variables – vectors normal to loop and connecting it with points x_i – is NSR string, and the equation describing its dynamics is linear in the space of loops.

One person who tried to pursue this program was Polyakov. As far as I know, he got stuck since it was hard to write equations in loop space for renormalized variables.

44. **Peter Orland**
May 9, 2008

Thomas,

This is off topic and Peter may want to delete your comment and mine – well not SO off topic. You were asking about the application of string theory to field theory/stat. mech. problems. I have worked on this sort of thing on and off for a long time. It may not be much off the main topic, because it does touch on the justification for continuing to do string theory, should the subject fail to unify forces or give definite results in quantum gravity.

There was a program initiated by many people – Nambu, Weingarten, Hasslacher and Corrigan, Makeenko and Migdal, Polyakov … the list goes on – that gauge systems like QCD and the 3D Ising model (which is dual to the Z_{2} gauge theory) could be solved using string-theoretic ideas. There are exact representations of the 3D Ising model as random surfaces with a certain action.

This program only seems to an unqualified success for N=4 susy YM. It has not yet panned out for pure Yang-Mills (which one wants to solve at weak bare coupling, near the RG fixed point, not strong bare coupling) or finding exact results for 3D stat. mech. systems. If you want critical exponents for statistical models in 3D, epsilon and 1/N expansions are still your best bet.

In fact, it is much easier to obtain string-like excitations from field theory than the other way around. The examples are strongly-coupled lattice theories, Higgs theories with vortex strings, compact QED in 2+1 dimensions and recently SU(N) gauge theories with two small couplings (sorry to advertise my own work Peter). These string-like excitations have not been shown to have any connection to standard string theory. When actions can be written down, they contain extrinsic-curvature-dependent terms, and are not local on the world sheet.

Maybe someday some of these long-standing problems will be solved using standard string-model ideas. Personally, I hope so. But maybe not.

45. **Moshe**
May 9, 2008

Peter Orland, I have zero interest in the "justification" issue. Just wanted to comment that there are new ideas on the market relating various 3dim systems to string theory, via the gauge-gravity duality. These may or may not pan out, but they are different from the set of ideas you refer to (more to do with perturbative ST). For an entry point to the literature you can look at recent papers of the nuclear theorist D.T. Son.

46. **Peter Orland**
May 9, 2008

Hi Moshe,

Yes, I have seen this stuff – but these systems are interesting because of their connection to string theory. I am not convinced they have any application to nature (I know people are trying to make such a connection).

Regards,
Peter

47. **Peter Orland**
May 9, 2008

P.S. The effective string excitations, which arise in field theories, also
have no obvious connection with perturbative string theory.

48. **Moshe**
May 9, 2008

Not sure I agree this is the only interest in these systems, but in any event there is currently a new set of ideas of dealing with strongly interacting systems. It may be of some interest for people with experience dealing with such systems to explore these ideas and discover what they are good for.

49. **Peter Woit**
May 9, 2008

Eric,

The problem with the argument that physics departments have only stopped hiring string theorists this spring is because of the LHC is that the LHC turn-on has been imminent for quite a few years, and departments that wanted to move in that direction have been doing so for quite a while.

However, it is only this spring that string theorists have started posting articles on the arXiv about what they plan to do once it is shown that string theory is wrong…

50. **Peter Orland**
May 9, 2008

Sure, new things need to be explored. The ideas you mentioned aren't being neglected, while there are other approaches to strongly-interacting problems which have excited less interest, possibly because they don't start from string theory (using exact form factors is one of those of which I am familiar).

51. **Peter Orland**
May 9, 2008

Moshe – If you are interested, the form factor program is reviewed in
an excellent article by Essler and Konik.

52. **Moshe**
May 9, 2008

Thanks Peter O., I'll take a look.

53. **Eric**
May 9, 2008

Peter,
I don't think you can claim that physics departments have stopped hiring string theorists. For example, Hawaii has just made an offer to a string theorist, and I'm sure others will be forthcoming.

54. **Eric**
May 9, 2008

Again, what about Lehman College, CUNY? Can we not consider Dan Kabat a string theorist?

55. **Peter Woit**
May 9, 2008

Eric,

I'm not claiming that no physics department is hiring any string theorists, just that dramatically fewer are getting tenure-track jobs this spring than was typical in recent hiring seasons. I've given you my source for this information: the rumor-mill web-site. Go check it for yourself.

56. **Eric**
May 9, 2008

Peter,
Perhaps you should have a closer look at the rumour mill site yourself, as the information I was just giving you came directly from there. Even if there are fewer string

theorists hired this spring, I don't think this would be that unusual, as these things are cyclic.

57. **Coin**
May 9, 2008

Sorry if this is kind of a dumb question, but this is something I've been a bit confused about for a while: What exactly is the difference between a "Phenomenologist" and an "Experimentalist"?

58. **Peter Woit**
May 9, 2008

Coin,

Experimentalists design, build, operate experiments and analyze the data from them. Deciding which theorists are "phenomenologists" is not so clear, the term covers a wide range of activity. But in general the idea is that they are theorists working not on theoretical or mathematical foundations, not on models far from reality, but on better understanding and extracting predictions from testable models that can be compared with experiment. At one end of the subject, their activities probably become indistinguishable from some of the activities engaged in by experimentalists as part of their data analysis.

59. **Peter Shor**
May 10, 2008

Peter Woit is right; I certainly wasn't calling for all string theorists to be fired. I was trying to point out that Moataz Emam's call for string theorists to form their own departments (and I don't quite know how seriously to take him on this) was not only completely implausible, but it also wasn't even in the best interest of the string theorists. If anybody tries to form a separate Department of String Theory, I would expect and hope that most of the physicists (string theorists and others alike) in the affected Department of Physics would resist.

60. **Thomas Larsson**
May 10, 2008

Dimitry and Peter O.

I am well aware that the 3D Ising model is Kramers-Wannier dual to the 3D Ising gauge model and admits a random surface representation. Even better, one can write down a funny O(N) or U(N) lattice gauge model on a 3D brick lattice, with a log action, that can be exactly mapped onto a model of self-avoiding random surfaces; the weight of each graph equals N^chi u^A, where chi is the Euler characteristic, u is related to the coupling constant, and A the surface area.

Does this construction solve U(N) gauge theory, or make me into a string theorist? Hardly. Just because I have mapped one untractable model into another does not mean that I have solved it. If anything, this shows that perturbative string theory is inadequate for this problem because steric repulsion is crucial here (self-avoiding surfaces do not self-intersect!).

A method has only really contributed to our understanding of a model if it helps to extract some kind of quantitative information about it, not necessarily critical exponents and not necessarily exactly. The methods that Peter O. mentioned do that, as do high- and low-temperature expansions, real-space RG and computer simulations. However, AFAIK no quantitative results about 3D Ising have come out of string theory.

61. **Peter Orland**
May 10, 2008

Thomas,

As you can see from my earlier comment, I largely agree with your sentiment that string/random surface approaches to field theory/stat mech problems have not been successful. In fact, as I said, it has been much easier to obtain string excitations from field theory than the other way around (except in AdS/CFT approaches, which have not yet made a connection with real physics problems. Maybe the work Moshe referred to will change the situation). I must admit, however, that I still have some fondness for this approach, in spite of its lack of success, since it captured my imagination in grad school. It was a very daring suggestion, which, unfortunately, may not lead anywhere.

62. **Jonathan Vos Post**
May 10, 2008

As someone with credentials as a scientist and as a writer, I see the argument for splitting String Theory Departments away from Physics Department, to allow Physics Departments to return to the "Real World" and to allow the String Theorists to pursue their quest; as analogous to an argument that Critical Theory Departments split away from English Literature Departments, to allow Eng Lit to focus again on poems, stories, and novels, and be free from Deconstructionism, Feminism, Pan-Africanism, Asian Pacific Islandism, Gay Theory, Postcolonialism, and the like, while the Critical Theory professors and grad students and postdocs explore a rich sociocultural and semiotic landscape unfettered by having to read and explain prose and poetry that naive millions of readers actually enjoy buying and reading.

63. **JC**
May 10, 2008

Is that quote on Podolsky's blog really by Jacques Distler himself? Or is that quote just somebody else posting under Distler's name, trying to make him look bad?

(Offhand, I can't really tell either way).

64. **Peter Woit**
May 10, 2008

JC,

If it's not Jacques, it's someone doing a pitch-perfect impersonation….

65. **Gil Kalai**
May 11, 2008

Peter, this thread shows the difficulty in discussing hypothetical scenarios. Emam referred to a hypothetical situation that "someone shows that the universe cannot be based on string theory" so he was certainly not calling to form separate departments now.

Overall, Enam's analysis is not convincing. I think he is simply wrong in his assumption that if someone shows that the universe cannot be based on string theory many string theorist will continue to do what they do. Showing that the universe cannot be based on string theory (if this will be the way things will go) will represent a major breakthrough which is one of the potential fruits of string theory research. Probably such a (hypothetical) development will cause most string theory to work on something else (that can still be called "string theory"). Those who will continue to work on string theory (or "old-fashioned string theory") will have a place in physics department just like many other theoretical physicists who work on models which while not describing the universe, still give important physics insights and develop important mathematical physics. Also Emam's comparison with statistics does not make sense.

Here is an analogy from mathematics. For many centuries algebraists tried to find formulas for solving polynomial equations. If somebody had come say in 1600 and said: "guys in view of the many years of failure, try to think about the possibility that such formulas are impossibles" this could be a little useful. If the same guy had said "algebraists have failed and they should be fired" (opening the door to more astrologists and alchemists), then this could be a little harmful. But what was really needed is an understanding why there are no formulas to solve polynomial equations and not just vague speculations and meditations.
Once this was understood nobody continued the old algebra endeavor but there was plenty of work left for algebraists, in fact, this was the beginning of modern algebra.

The turning point and the beginning of modern algebra was not just the "gut feeling" that the old endeavor might have failed but the detailed technical understanding why it cannot succeed.

66. **Peter Woit**
May 11, 2008

Gil,

String theory, as an idea about unification, is not in danger of failing because of mathematical contradiction. It is failing because it is becoming clear that, as an idea about unification, it is essentially vacuous. The reasons for this are well understood (e.g. the landscape). I don't think the author of this article or anyone else envisions string theory being shown to be mathematically wrong, or being falsified by showing that it predicts something that disagrees with experiment. What is happening is that conviction within the physics community is growing that there is no hope to get a real prediction out of string theory, thus the failure. Die-hards will keep claiming that hope remains, but there will be fewer and fewer of them, and at some point the consensus of the community will be that the cause is hopeless.

Right now, I think many of the leaders of the subject are well aware of how bad the problems are, but are holding out from giving up on the theory, waiting to see what the LHC says, hoping that LHC results will change the current picture. If the LHC doesn't come up with something that somehow gives new hope to the string theory unification program, I think it will be conclusively dead, although some people will insist on pursuing it. I would guess that it is exactly this all too possible situation that Emam is thinking about when he brings up the possibility of string theory being shown to be "wrong".

67. **Gil Kalai**
May 11, 2008

Dear Peter,

It is entirely rational to wait to the LHC outcomes and I think most people interested in the scientific endeavor are hoping that it will give new data and lead to important new insights. I have noticed that your opinion on string theory is negative, but your description of the convictions within the physics and string theory communities may very well be biased by your own personal position. At least your description differs from the convictions I sense when talking to many colleagues in these areas. (My little comment to Peter (Shor) was that Enam does not call string theorists to form independent departments but rather discusses a scenario which is hypothetical from his point of view.)

68. **Kea**
May 12, 2008

Gil

It is not at all reasonable to *wait and see* what happens at the LHC. Any responsible tax funded theoretician should be working their ass off to come up with quantitative predictions *before* 2009.

69. **Thomas Larsson**
May 12, 2008

Peter O,

I am sorry if my post came across as more negative than it was meant to be. The random surface approach also captured my attention a few years after it captured yours (when *I* was in grad school), and I find it frustrating that it has not panned out. When I now think about the problem again after many years, it seems like the obstacle is the self-avoidance constraint, which induces an interaction which is very non-local on the worldsheet, although it is local in the ambient space.

70. **Gil Kalai**
May 12, 2008

Peter, of course the nature of scientific proofs is different in different disciplines so mathematical proofs just appeared in my analogy since the analogy was drawn from the area of mathematics.

Ahh, I think I see a serious difficulty with your approach. You cannot imagine a way that string theory will be definitely proved to be wrong by physicists and therefore you assume that its weaknesses already suffice. (But just like the case of solving polynomial equations, in view of the prominence and successes of string theory much stronger scientific arguments will be needed.)

Similarly, you cannot imagine how string theory can prevail as a viable physics theory and therefore you conclude that it failed. But string theory can win in ways we cannot imagine at present and can also fail in ways we cannot imagine at present.

71. **anon.**
May 12, 2008

'But string theory can win in ways we cannot imagine at present and can also fail in ways we cannot imagine at present.'

This kind of statement is what you expect to find in a religious tract. It's not really scientific, but is more in the category of belief systems/cults/wishful thinking/crackpotism. If string theory 'wins' or is falsified, then it will be deserving of a media mention. Until then, it's not a scientific theory.

Feynman told a story of 'Cargo Cult Science'. During WWII, South Sea islands were used as military airports, and the islanders had business. After the war ended, some islanders tried to attract business by encouraging passing planes to come down to land by building improvised runways with control towers.

Their efforts looked professional from a distance, but up close they were fake. It's the same with string theory. The mathematics and alleged physical successes make it superficially look like a scientific discipline, but it isn't because it can't predict anything checkable.

72. **Zathras**
May 12, 2008

I've seen a number of prospective graduate students, who spent their undergraduate days as avid readers of Woit's blog, and whose perspective on high energy physics is now so hopelessly divorced from reality that the best one can do is smile and nod one's head pleasantly and say, "I hear the condensed matter group has openings."

That this blog has steered young students away from string theory is the best endorsement of any blog ever. Sounds like a clear charge of you're corrupting the youth.

73. **Mitch Miller**
May 12, 2008

Peter Woit Wrote:
"Right now, I think many of the leaders of the subject are well aware of how bad the problems are, but are holding out from giving up on the theory, waiting to see what the LHC says, hoping that LHC results will change the current picture. If the LHC doesn't come up with something that somehow gives new hope to the string theory unification program, I think it will be conclusively dead, although some people will insist on pursuing it."

Wow, that is not the impression I get at all. Everybody seems to already know what the LHC will find (Higgs for sure, maybe Susy and small chance of extra dimensions and even smaller chance of something crazy and non standard model). i don't see how anything bad for string theory can come out of the LHC. If extra dimensions are found, it will be great, if not its no big deal as the lower bound for these effects (or anything else related to string theory) to be measureable is not even close to being reached by the LHC. If something truely unexpected is found, everybody will be scrambling to explain it, not just string theorists. As a young grad student that is what I'm routing for, unlikely as it is 🙂

74. **Gil**
May 13, 2008

Dear Anon, please note that the statement you quote and criticize

"String theory can win in ways we cannot imagine at present and can also fail in ways we cannot imagine at present."

is not a statement **within** a scientific theory, but a statement **about** scientific theories. As such, I think it is perfectly fine and can be supported by examples.

75. **anon.**
May 13, 2008

Gil, string 'theory' is a misnomer. Stringy ideas are an empty framework which fits 10^500 theories. To get a specific string theory, you need to specify all of the moduli of the Calabi-Yau manifold of 6 compactified dimensions. If there was a string theory then your position would be sensible (and this blog wouldn't exist).

76. **Kris Krogh**
May 13, 2008

Gil,

It's true something unimaginable may save string theory. Would you like to invest in Company XYZ? The stock is performing terribly, but something unimaginable may save it.

77. **Gil Kalai**
May 13, 2008

If this looks more accurate to you, anon, replace "string theory" by 'the string framework'.

It is possibe that the 'string framework' will lead to a definite win (and thus perhaps to a 'string theory') in ways we cannot expect, and it is also possible that it will be definitely shown, in unexpected manners, that the 'string framework' is inedequate for its (ultimate) purposes.

There are also various possible forms of partial victory for 'the string framework', in terms of new insights and horizons in mathematics and physics which come short of a definite 'theory' to your taste. Some of these partial victories are already in place.

78. **Peter Woit**
May 13, 2008

The debate about string theory on this thread has become more or less completely content-free. Enough.

My apologies for not providing fresh material for discussion. I'm busy with other things, and there has been remarkably little news on the math-physics front.

79. **nbutsomedy**
May 13, 2008

"I've seen a number of prospective graduate students, who spent their undergraduate days as avid readers of Woit's blog, and whose perspective on high energy physics is now so hopelessly divorced from reality that the best one can do is smile and nod one's head pleasantly and say, "I hear the condensed matter group has openings."

Thank God (anyway, not a believer) !!!! A wise step for the students. Both monetary and scientifically.

80. **TCO**
May 25, 2008

Why the disdain for condensed matter from the first poster at the other site? Look at what Feynman did with helium. What Einstein did with thermoelectric effect. BCS theory. Fermi-Dirac. Bose-Einstein. How about figuring out cuprate superconductors, studs? Come on smarties. Direct your theory to something physically tangible.

-solid state chemist

81. **Jonathan Vos Post**
June 4, 2008

A deep statement by Atiyah, discussed by Gowers, recently in turn referenced by Terry Tao, explains why it might be a bad idea for String Theory (if "proven wrong") reconsolidate itself as distinct departments away from Physics.

Tao: "In fields such as nonlinear PDE (which Perelman's result can broadly be included in), individual theorems tend not to be directly applicable much beyond their original intended use, but general ideas, strategies, tricks, and paradigms are often far broader. A similar point (using combinatorics as the primary example) is discussed in Gowers' 'two cultures' paper

http://www.dpmms.cam.ac.uk/~wtg10/2cultures.pdf

Gowers citing Atiyah:

"The ultimate justification for doing Mathematics is intimately related to its overall unity. If we grant that, on purely utilitarian grounds, mathematics justifies itself by some of its applications, then the whole of mathematics acquires a rationale provided it remains a connected whole. Any part that drifts away from the main body of the field has then to justify itself in a more direct fashion."

If String Theory has drifted away from the main body of Physics, or Mathematics, then (however beautiful it may be) it is sociologically subject to a demand for direct justification.

82. **S Halayka**
June 11, 2008

For what it's worth, this paper seems to highlight the possibility of experimental verifiability at a scale of around 20 orders of magnitude larger than the Planck scale:

http://arxiv.org/abs/0806.1431

83. **S Halayka**
June 11, 2008

The paper mentioned above has been criticized to death by a trusted string researcher. My apologies for jumping the gun.

Witten on Dark Energy

May 6, 2008
Categories: Uncategorized

Commenter Shantanu pointed to a web-site with talks available on-line from a symposium about Dark Energy now going on at the Space Telescope Science Institute. Yesterday Witten gave a talk entitled “Models of Dark Energy”, where he lays out very clearly the conventional wisdom of the string theory community about the dark energy problem and its implications for string theory.

Witten describes how the problem of a huge number of possible vacua has always been an embarrassment for string theory. Until about 10 years ago his attitude towards most constructions of string vacua was “who needs this mess”, thinking that once one figured out the vacuum energy problem, such constructions would all go away. He explains how the discovery of a small positive CC has changed his attitude, that he’s no longer sure that one can find a distinguished vacuum state, and thus maybe the anthropic landscape/multiverse crowd is right. He describes this possibility as involving both good news and bad news:

> The good news (such as it is) then is that if we are really living in a “multiverse”, it may be that the theory as we know it is pretty close to the truth.
>
> But there’s a hefty dose of bad news… If the vacuum of the real world is really a needle in a haystack, it is hard to see how we are supposed to be able to understand it. In other words, if an unimaginably large number of approximate “vacuum” states are realized in different parts of the Universe, none of them with any special meaning, and with the details of particle physics depending on where one happens to live, then what sort of understanding of particle physics can we hope to get? I don’t have an answer to this question, although we might learn something from the LHC that will help…

The crucial point of course is this last one: how can you ever test these ideas, making them real science and not metaphysics? At the end of his talk, Rachel Bean tried to pin him down on this question, leading to this exchange:

> Bean: “If we have this landscape, this multiverse, … can we learn nothing, or is there some hope, do you have some hope, that if you were to find a universe that had remarkably small CC you could also make some allusion to the other properties of that universe for example the fine structure constant, or are we saying that all of these things are random variables, uncorrelated and we’ll never get an insight.”
>
> Witten: “Well, I don’t know of course, I’m hoping that we’ll learn more, perhaps the LHC will discover supersymmetry and maybe other unexpected discoveries will change the picture. I wasn’t meaning to advocate anything.”
>
> Bean: “I’m asking your opinion.”
>
> Witten (after a silence): “I don’t really know what to think has got to be the answer…”

Besides the landscape problem, Witten also described attempts to model dark energy as an aspect of some differerent sort of physical field, saying that he has been working on this with a student, but that the problem is the strong experimental bounds on the existence of light fields coupling to ordinary matter.

Comments

1. **Aaron Bergman**
   May 6, 2008

   I believe Witten was referring to his previous work with Peter Svrcek who graduated a few years ago.

2. **Peter Woit**
   May 6, 2008

   Thanks Aaron,

   I think you’re right, he seemed to be referring to work with Svrcek, although that paper didn’t seem to mention anything about the dark energy problem, which was what Witten’s talk was about.

3. **Ethan Siegel**
   May 6, 2008

   At the very least, I’ve always found Witten to be remarkably balanced on the utility of string theory for the real world. This points towards that as well.

   He recognizes the limitations and pitfalls of the idea, as well as the difficulties inherent in attempting to find what we can learn from string theory if the models are non-predictive. I think that’s a very reasonable answer to Rachel’s question.

4. **Bill**
   May 6, 2008

   At the risk of embarrassing myself I have to admit that my knowledge of string theory is about at the level of that discussed in Zwiebach’s introductory book, so I’m an idiot. But the site’s mention of Witten and Moataz Emam’s proposed “future” of a failed string theory motivates this layperson’s following questions: One, if by adding a single dimension Witten can collapse five theories into one, can we not just add lots more dimensions to eliminate all the other inconsistencies (like the 10^500 vacuua) in the theory? (Of course, I’m thinking about von Neumann’s reputed curve-fitting comment regarding squeezing an elephant into some given model, and of course I’m also being facetious.) Two, if no one other than a post-doc math major can possibly appreciate this stuff, what good is it (recall Eddington’s comment about the dozen or so people who could understand general relativity at the time), particularly if it doesn’t deliver? And three, what right has the legitimate physics community to even consider taking a falsified or failed theory and turning it into another “discipline” (read: “religion”) on the sole basis that it is too beautiful to ignore (or, much worse, because so much time and effort has already been expended that we have to press on regardless)? This is the very definition of fanaticism.

5. **Kea**
   May 7, 2008

   Witten’s talk was clear, and surprisingly noncommittal regarding the existence of Λ, but I preferred Steinhardt’s talk, which discusses the need for a range of complementary tests and possible JDEM exclusions of large numbers of models, both stringy and not. As Witten also stressed, more accurate measurements of w will be important in ruling out classes of model, especially Λ varying ones.

6. **Chris W.**
May 7, 2008

Kea (and anyone else who is interested):

The HTML character entity for **Λ** is **Λ**.

The HTML character entity for **λ** is **λ**.

(Note the variation is case of the mnemonic, ie, the *entity* name.)

For greek letters (and other characters) see this reference or Wikipedia, among others.

7. **Chris W.**
May 7, 2008

From Peter: “The crucial point of course is this last one: how can you ever test these ideas, making them real science and not metaphysics?”

In the preceding paragraph (within the quote) Witten asks “then what sort of *understanding* of particle physics can we hope to get?”

Note the implicit identification of *understanding* and *testable explanation*. It seems that those who are attracted to the idea of a multiverse—in the form Witten has in mind—have effectively endorsed breaking down this identification, while continuing to assert that such an explanation could be scientific. What Witten is acknowledging is the untenability, even incoherence, of this assertion.

Untestable explanations may fulfill a desire for a putative understanding of the world that can assume various cultural roles, but in science they are at best *preludes* to the development of an explanation that can be tested, and stands up under testing. It does not seem that the multiverse can be such a prelude, except as part of an attempt that is frankly admitted to be a failure and thereby helps point the way to crucial insights. Of course for now, as Witten is also acknowledging, the way forward remains shrouded in obscurity.

8. **mike harney**
May 7, 2008

I like Witten’s coments that the LHC might give some clues – at least he is in touch with experimental reality.

9. **Arun**
May 7, 2008

In fact .. I think he’s going to be at CERN on a sabbatical when the machine turns on 🙂

10. **Shantanu**
May 8, 2008

Hi,
Peter and others,
Mario Livio’s talk was also very good and worth a look
and discusses anthropic principle.
right now John Peacock is giving a talk (which can be viewed live)

11. **egbert**
May 12, 2008

There’s talk on CNN about the LHC “test” of string theory being a religious experience for Nina Arkani-Hamed.

When I signed up to be a scientist, religion was considered a negative influence on science. How times change.

12. **Chris Oakley**
May 12, 2008

My favourite quote from the CNN article:

> “From the point of view of the big experiments at the LHC, there is no amount of money or craftsmanship that would produce the kind of insight that comes from sharing LHC data with a true visionary like Nima Arkani-Hamed,” Tully said.

13. **fh**
May 12, 2008

egbert:

Ludwig Boltzmann, quoting Goethe on Maxwells equations:

“War es ein Gott, der diese Zeichen schrieb?”
(Was it a God that penned these signs?)

A religious experience does not a religion make and in many forms it has a long and positive tradition in fundamental science.

14. **D R Lunsford**
May 12, 2008

fh,

He was paraphrasing – no, QUOTING – Goethe. You can find it for yourself.

Ok, I give in – it’s in the movie “Faust Times at Egmont High”.

-drl

15. **Peter Woit**
May 12, 2008

    egbert,

    I think he was just referring to visiting the LHC as a “religious experience”. The claims made in the article that the LHC may see strings and test string theory involve a different and more dubious sort of religious belief….

16. **chimpanzee (aka "joe")**
May 13, 2008

    I read an LA Times article about the passing of Robert Wilson (1st director of Fermilab). He was a sculptor, & desired an aesthetic looking building for Fermilab. Something to the effect of a cathedral, where physicists would try to unlock the secrets of the atom.

    “It [theoretical physics] is very religious”
    — xx, Caltech CS professor

    My take on HEP & cosmology is that of ill-conditioned problem: insufficient data.

    “Insufficient facts always invite danger.”
    — Spock, Space Seed, Stardate 3141.9, Episode

17. **Ethan Siegel**
May 14, 2008

    Peter,

    I’m sure you’ve seen this posting today:

    http://xxx.lanl.gov/pdf/0805.1911

    But I’m really curious as to your take on it; especially on sections 5 and 6. Does he get it right?

    Ethan

18. **Jonathan Vos Post**
May 14, 2008

    Besides his scientific achievements, Dr. Wilson was known for the environment he created at the Fermilab site, with hundreds of acres of restored prairie, a herd of bison, fishing holes, abstract sculpture and a central building modeled, in spirit at least, on the Beauvais Cathedral in France.”

Do We Need to Change the Definition of Science?

May 15, 2008
Categories: Multiverse Mania

Media hype about how the LHC is going to test string theory continues: see Will String Theory Be Proven and here:

> String theory has come under attack because some say it can never be tested; the strings are supposed to be smaller than any particle ever detected, after all. But Arkani-Hamed says the Large Hadron Collider could lead to the direct observation of strings, or at least indirect evidence of their existence.

A recent New York Times article ends with another Arkani-Hamed quote about what to expect at the LHC:

> He pointed out that because of the dice-throwing nature of quantum physics, there was some probability of almost anything happening. There is some minuscule probability, he said, "the Large Hadron Collider might make dragons that might eat us up."

Obviously I'm being unfair to put these two quotes together, but they both raise a basic question about the philosophy of science. When can we legitimately say that a theory is testable and makes a scientific prediction? The most straightforward examples of scientific predictions are cases where we have high confidence that a certain experimental result has to happen if a theory is right: such a theory satisfies Popper's falsifiability criterion. But many theoretical ideas are not so tightly constrained, and compatible with a range of possibilities. This range generally comes with some notion of probability: certain experimental results are more likely to come out of the given theory, others less likely. This may allow you to gain confidence in a theory even if it is not falsifiable, by seeing things that the theory says are more likely, not seeing the things it says are unlikely. The problem with the idea that the LHC is going to test string theory by seeing strings is that according to the standard framework of string theory, this is just very unlikely. Saying that an experiment is going to test your theory when it is extremely unlikely that it will provide any evidence for it or against it is highly misleading. You're always free to say "this experiment is unlikely to test my theory, but who knows, I may get incredibly lucky and something unexpected will come out of it that will vindicate me". But that's not really a "test" of your theory, that's wishful thinking.

There's a new article in New Scientist closely related to this by Robert Matthews entitled Do we need to change the definition of science?. It's about claims being made that multiverse studies show that we need to re-examine conventional ideas about what is science and what isn't. I'm quoted saying the sort of thing that you might expect:

> I never would have believed that serious scientists would consider making the kinds of pseudoscientific claims now being made…
>
> an outrageous way of refusing to admit failure…
>
> The basic problem with the multiverse is not only that it makes no falsifiable predictions, but that all proposals for extracting predictions from it involve massive amounts of wishful thinking.

Max Tegmark argues against a straw man:

> Some people say that the multiverse concept isn't falsifiable because it's unobservable – but that's a fallacy

noting that just because some implications of a theory aren't directly observable doesn't mean the theory is untestable. If a theory passes many convincing tests involving things we can observe, and the theoretical structure is tight enough, then we have good evidence about what is likely to be going on with phenomena we can't observe. This is certainly true: if the string theory landscape made lots of testable predictions so that we had good reason to believe in it, and the same structure implied a multiverse, that would be good reason to believe in the multiverse. The problem is that the landscape makes no predictions and we have no reason to believe in it. It's not a real testable scientific theory, rather an untestable endpoint of a failed theory. As such it implies nothing one way or another about the existence of a multiverse.

Matthews quotes various people arguing for a "Bayesian" view of science, that what is going on is that experimental observations probabilistically provide evidence for and against theories, with the falsifiability case of probability zero or one not usually occurring. This may be a good way of thinking about how science actually works. But by this criterion, string theory unification and the multiverse remain pseudo-scientific, as no one has been able to come up with proposed experimental tests that have a significant chance of providing such evidence for or against these theories.

Comments

1. **Drew Arrowood**
   May 15, 2008

   "Matthews quotes various people arguing for a "Bayesian" view of science, that what is going on is that experimental observations probabilistically provide evidence for and against theories, with the falsifiability case of probability zero or one not usually occurring."

   The big problem with this is called the "Paradox of the Ravens" and is due to the philosopher Hempel. If my hypothesis is "All Ravens are Black", then whenever I see something that isn't black and isn't a raven, whatever it may be, whether a nugget of gold or a horse, that has counted as an "experiment" which "confirms" my hypothesis.

   People have been trying to change the definition of science throughout the 20th century.

2. **anon.**
   May 15, 2008

   "This is certainly true: if the string theory landscape made lots of testable predictions so that we had good reason to believe in it, and the same structure implied a multiverse, that would be good reason to believe in the multiverse." – PW

   Even a theory which makes tested predictions isn't necessarily truth, because there might be a another theory which makes all the same predictions plus more. E.g., Ptolemy's excessively complex and fiddled epicycle theory of the Earth-centred universe made many tested predictions about planetary positions, but belief in it led to the censorship of an even better theory of reality.

   Hence, I'd be suspicious of whether the multiverse is the best theory – even if it did have a long list of tested predictions – because there might be some undiscovered alternative theory which is even better. Popper's argument was that scientific theories can never be proved, only falsified. If theories can't be proved, you shouldn't *believe in them* except as useful calculational tools. Mixing beliefs with science quickly makes the fundamental revision of theories a complete heresy. Scientists shouldn't start begin believing that theories are religious creeds.

3. **Icecycle**

May 15, 2008

Not being a scientist I really don't see the need to change the definition of science, however; much of our current knowledge seems to be based on faith.
(I know a swear word; in the first paragraph.)
Why don't we have a little funding for the fanatics out there; you know who you are; and just step into the fringe on occasion.
If there is a multiverse (for instance) it is a black box and has to be investigated as a black box; we would need philosophy to even look at it.
But philosophers (generally) don't know physics (just look at Bertrand Russell's explanation of relativity; I felt really bad for him when I found out he was a mathematician.)

For myself; not being a scientist; I make myself believe a concept absolutely. Then, finding all the proof that fits I make myself believe the same concept is wrong and find all the proof that makes it fail.
Because; like everyone else; I tend to get too much of my personal beliefs into my world view.

As a computer programmer I can't get away with that nonsense.

4. **St. George**
May 16, 2008

Is the probability of getting a fire-breathing dragon out of the LHC more or less than that of directly observing a string?

5. **Peter Erwin**
May 16, 2008

The big problem with this is called the "Paradox of the Ravens" and is due to the philosopher Hempel. If my hypothesis is "All Ravens are Black", then whenever I see something that isn't black and isn't a raven, whatever it may be, whether a nugget of gold or a horse, that has counted as an "experiment" which "confirms" my hypothesis.

Why is this relevant? No real scientist, Bayesian or not, operates that way.

A reasonable Bayesian would argue that the probability of seeing a non-black nugget of gold is *independent of* the hypotheses "All ravens are black" and "Not all ravens are black" [or hypotheses like "No ravens are black", "Half of all ravens are black", etc.]. So actually seeing a non-black nugget of gold does not change the prior probabilities for any of the relevant hypotheses.

Someone taking a non-Bayesian approach (e.g., a naive Popperian, or someone who prefers a "frequentist" approach to probabilities and hypothesis testing) would operate in essentially the same way: scientific hypotheses about the color of ravens make no predictions about the color of gold nuggets, and so observations of nugget colors do not test the raven hypotheses.

6. **Peter Erwin**
May 16, 2008

anon. said:
Popper's argument was that scientific theories can never be proved, only falsified. If theories can't be proved, you shouldn't believe in them except as useful calculational tools. If theories can't be proved, you shouldn't believe in them except as useful calculational tools.

Ironically, that's not unlike what some Jesuit astronomers suggested to Galileo during his trial: go ahead and use the Copernican model if it makes good predictions, but don't claim that it actually describes reality in any fashion — it's just a useful calculational tool!

The point about scientists "believing" in theories is not that they accept them as religious dogma. It's that they're making positive statements about which theories seem to be *better* descriptions of reality, and about the relative amount of observations/experimental support for them.

The problem with Popper's argument is not that it's wrong per se, it's just that it's *incomplete*. It has no way of distinguishing between, say, the heliocentric model of the Solar System (which has successfully passed an enormous array of increasingly stringent tests) and some preliminary hypothesis about the spectrum of density fluctuations in the early universe (which has been tested by the WMAP data, but not by anything else yet). All Popper allows is a passing grade for both ("not yet falsified"). In practice, however, scientists *do* accord more "belief" to the first than they do to the second. That, I think, is what the Bayesian argument is about. (It's also, as Peter Woit notes, about how to accommodate tests that provide statistical limits, rather than idealized "confirm or deny" results.)

7. **Zathras**
May 16, 2008

Peter Erwin,

A reasonable Bayesian could also say that seeing the nugget of gold acts as "confirmation" of the raven hypothesis, but only by raising the probability of the proposition's truth an infinitesimal amount e.

Come to think of it, e might be the same order of magnitude as the possibility of confirming string theory.

8. **weichi**
May 16, 2008

"All Popper allows is a passing grade for both ("not yet falsified")."

It's been a while since I read popper, but I don't think this is correct. He certainly certainly acknowledges that theories can be submitted to more or less severe tests, and I thought that he states that the more severe the tests, the more we should trust the theory (or something along these lines). Am I misremembering this?

9. **Joseph Triscari**
May 17, 2008

Unfortunately I cannot read the whole article because I don't have a subscription to New Scientist.

I wonder if the relevant point about a move to a Bayesian philosophy is not the way evidence is aggregated but the fact that in a Bayesian philosophy the priors are admittedly subjective. In my understanding, this is the central point of a Bayesian. The opposing view – which I think is called Frequentist – requires all inputs to a model to be probabilities that can conceivably be measured.

A Bayesian approach is fine for reasoning under uncertainty because many times, priors *are* subjective and there's nothing to be done. On the other hand, openly subjective priors are a facet of the Bayesian philosophy that can be abused in practice by shifting inputs to fit data (and declaring that it's OK because, after all, they're subjective).

I am curious to know in what way a Bayesian philosophy of probability is being applied to justify ST.

10. **Gil Kalai**
May 17, 2008

Joseph, in The Bayesian approach, initial probabilities are subjective but with more and more evidence the emerging scientific conclusion will be the same regardless of the initial probabilities. In practice, the interpretation of new evidence which is required for updating your initial subjective probabilities, is not entirely objective. Still the Bayesian approach looks to me a more realistic description than the Popperian approach regarding how science is practiced, and both approaches have problems.

11. **Peter Erwin**
May 17, 2008

Joseph Triscari said:
I wonder if the relevant point about a move to a Bayesian philosophy is not the way evidence is aggregated but the fact that in a Bayesian philosophy the priors are admittedly subjective. In my understanding, this is the central point of a Bayesian. The opposing view – which I think is called Frequentist – requires all inputs to a model to be probabilities that can conceivably be measured.

My impression is that prior probabilities are, if anything, better viewed as the Achilles heel of the Bayesian approach: there's no clear, obvious way to decide on what the prior probabilities should be, and this irks a number of people. The usual response by Bayesians is to point out that the choice of the prior ceases to matter once enough relevant observations have been used to update the probabilities. This amounts to admitting that arbitrary or subjective priors are a *problem* (not an advantage), while arguing that in most practical cases it doesn't matter too much.

The real difference between the Bayesian and Frequentist approaches is in the interpretation of "probability" and whether it makes sense to say things like "The probability of rain tomorrow is 90%." See, for example, the posting and comments here.

(There are also practical differences, in that a Bayesian approach arguably provides a more general and direct way of constructing tests of hypotheses, without requiring the assumptions — e.g., a Gaussian distribution for errors — that underlie most traditional frequentist hypothesis tests.)

12. **Peter Woit**
May 17, 2008

Joseph,

The article was more about the multiverse than string theory. The discussion of the problems of falsification and the advantages of Bayesian ideas seemed to me to be a red herring. There just aren't any conventional scientific tests of string theory or multiverse ideas. So arguing about the philosophy of science is irrelevant, unless you are trying to abandon the conventional understanding of what science is. Doing this to avoid confronting the failure of these ideas seems to me to be a big mistake.

13. **Joseph Triscari**
May 17, 2008

I agree with you Peter that it's a mistake not to confront the failure of an unfalsifiable theory. I didn't mean to invite an exploration of the differences between Bayesian and Frequentist theories of probabililty.

The point I was trying to make (and I see I made it poorly) was that if one doesn't wish to confront the failure of a theory, one might try and legitimize the theory using an established philosophy that acknowledges and sometimes encourages subjectivity – such as a Bayesian philosophy of probability.

14. **davetweed**
May 18, 2008

For completeness, I'll point out that Peter Erwin's point about subjectivity being a philosophical problem primarily applies in those cases where one is obtaining more than enough observations to sharply determine the posterior distribution, so that the question "why did you use a personal prior if it doesn't actually matter" comes up. This is generally the case in hard science (although even then…). The advantage of Bayesian model evaluation (to some extent in science and more widely in technology) is strongest when you have relatively few relevant observations (eg, due to medical ethics, response time constraints, etc) so that the prior can make observations which which would be inconclusive under frequentist tests (which often hide a universally held prior in them) more discriminatory when they coincide with various parts of the prior distribution. The personal (what's called "subjective", although I dislike that term because AFAIK you can't have a subjective idea that's wrong, whereas personal ideas can be wrong) nature of the prior is arguably not a problem in this case as long as it is made explicit. It's always struck me as strange that Bayesianism is widely touted for dealing with uncertainty (which I always think of in the sense of randomly inaccurate sensors) rather than for dealing with small numbers of observations.

15. **Peter Shor**
May 18, 2008

Most scientists have never really paid much attention to the definition of science anyway, have they?

16. **Peter Woit**
May 19, 2008

Peter Shor,

I find it very strange to be involved with physicists in discussing what's science and what isn't. It always seemed to me that such discussions were for philosophers or for softer sciences (e.g., is economics a science…), with physics embodying the extreme of a hard science, with everybody on the same page as to what counts as a testable prediction.

Then string theory and the landscape came around….

17. **Peter Erwin**
May 20, 2008

I find it very strange to be involved with physicists in discussing what's science and what isn't. It always seemed to me that such discussions were for philosophers or for softer sciences…

Might one argue that previous debates about "interpretations" of quantum mechanics, what "collapse of the wavefunction" actually means or entails, etc., had at least some “philosophy of science” flavor? That is, physicists ended up disagreeing about whether investigating interpretations was scientifically useful, or whether “interpretation” was even a part of science, and some of this did involve philosophical issues about science.

(I don’t mean that this was the same kind or order of disagreement that may be happening around the landscape — no one, so far as I know, was suggesting that we didn’t need testable predictions or experimental results. Just that this isn’t the first time that physics has been involved in “philosophy of science” territory.)

18. **Peter Shor**
May 20, 2008

I have a sort of ambivalent view of the previous debates on the interpretation of quantum mechanics. On the one hand, as Feynman warned graduate students, most people who started thinking about the interpretation of quantum mechanics and stopped worrying about more mainstream physics never got anywhere, and eventually derailed their careers. On the other hand, thinking about the interpretation of quantum mechanics led David Deutsch to think about quantum computing and from their to the first quantum algorithms.

What, if any, lessons about string theory we should take from this I leave to other readers.

19. **Michael**
May 20, 2008

Peter,

just a little reminder: You are an insufferable fool who shamlessly promotes his anti-science agenda for personal benefit. Shame on you and go to hell!

20. **Chris W.**
May 20, 2008

Philosophical concerns have always been at least a subtext in physics. In fact, I would argue that physics really amounts to an effort to confront what were originally philosophical problems with detailed observation, in such a way that we can actually learn something, ie, discover that some of our preconceptions are wrong. If this sounds strange to most scientists, that’s because they have so little interest in the history of science, and tune much of it out.

The great physicists of the 20th century were all interested in philosophical issues. Efforts to confront current problems in fundamental physics are hamstrung by naive empiricism just as much as by unhinged elaboration of formalism and metaphysics masquerading as physics.

Even Feynman was interested in the philosophical preconceptions of physics and its practice, notwithstanding the fact that he considered overt preoccupation with philosophy to be an early sign of senility.

21. **Peter Woit**
May 20, 2008

My point about physicists and the philosophy of science was restricted to the specific question of the so-called “demarcation problem”. There have always been interesting and real questions in the philosophy of physics, but, until recently the question of whether particle theorists were doing science or pseudo-science was not one that ever came up. You just didn’t see leading figures in the field publicly making bogus claims about what it means to test a scientific theory.

22. **Clark**
May 20, 2008

I think there’s two kinds of philosophy of science – prescriptive and descriptive. The prescriptive folks are effectively telling scientists how they *ought* do science. (I think Popper partially falls into that category) Then there are the descriptive folks who suggest we merely look at what the scientists are doing and excluding to understand what science is. Of course in practice most do a little of both.

The problem is that Popper was hardly the last word in philosophy of science. A lot of people have grave difficulties with his views. So it’s odd that so many scientists – especially physicists – seem to take Popper as if he were telling it like it was. There’s been quite a few decades of thought on things as well as arguments and counterarguments.

Personally I find the demarcation problem largely irrelevant. I don’t think there’s anyway to tell what is or isn’t a science beyond looking at what the community of scientists exclude or include. And that’s good enough for me.

The string issue is interesting since it’s one of the rare cases where agreement breaks down. Given that turning to the philosophers can be helpful (although I don’t think they’ll ultimately be able to resolve the problem either). I suspect what will happen is that research will continue and that if string theory doesn’t make more practical progress scientists will stop working on it. A few decades after it’s ceased to be a concern to any but a few scientists *may* start to think of it as a pseudoscience. But who knows. They may not. It may end up being viewed as science but an example of a dead end and something one shouldn’t emulate.

What I do think is true though is that scientists – especially physicists – would benefit from doing a bit more reading on philosophy. Especially philosophical histories of various ideas in physics. So, for instance, read a little Sklar, Fine, and so forth. Get a Philosophy of Science reader from Amazon. (There are several good ones)

Regardless of what you think of his physics, I think Lee Smolin made a very well thought out appeal a couple of years ago for physicists to engage philosophy more. Normally it doesn’t matter. But I think especially in theoretical physics it can be quite helpful. Despite what Feynman said about physicists and birds.

23. **Arun**
May 21, 2008

What is scientific and not-scientific is not a eternal classification. Democritus, Kannada’s atomic theory, while containing a correct insight and pertaining to reality only became scientific when the technological means to address the theory were developed.

I think the history of science will show that many ideas were kept in limbo for a time because their development was not feasible at the time the idea was generated. (I hesitate to call such ideas theories.) One can easily imagine a plausible alternate history, where General Relativity was developed much earlier than any capability to test it.

24. **Zathras**
May 21, 2008

Those are good points Arun.

In fact, under the lax standards of string theorists, one would consider the mathematical theories of Riemann, Lobachevsky, etc as having “developed [General Relativity] much earlier than any capability to test it."

25. **Christine**
May 21, 2008

PhD means Doctor of Philosophy; we should not forget our roots. The aim of physics is to advance its frontier into metaphysics, trying to make the latter a smaller and smaller territory. For instance, cosmology was in the metaphysics territory before the 19th century, but gradually became a physical science in the 20th. History shows that the advancement of physics proceeds over two pillars: the scientific method *and* philosophy. When doing "standard science" the physicist might disregard the latter and focus on the former, but the *advancement* of scientific knowledge needs both. Philosophy about nature and observations of phenomena trigger the first logical questionings; science is often developed from these prerequisites, it does not develop from itself alone, since it is a process to gain knowledge, not an end per se, otherwise it makes no sense at all.

The relative emphasis on the scientific method and on the philosophical inputs, however, varies from time to time, from person to person, from objects of investigation from objects of investigation. One thing, however, that must be invariant from this relative emphasis is the meaning of science, which must have a clear definition and agreeded among its practitioners. If this meaning is what is being currently debated or revised, then one should ask why, and this is what puzzles me.

26. **Peter Woit**
May 21, 2008

Arun and Zathras,

The issue of when a theory is practically testable is just another red herring in the string theory debate. String theory unification not only makes no predictions that are testable using current technology, it makes no predictions testable at any energy scale using any conceivable technology. That's the problem, not that tests are not currently feasible.

27. **Arun**
May 22, 2008

Peter,
I have no disagreement with you on string unification/theories of everything.

To the extent that we consider as science the elaboration of mathematical formalisms that include models that are scientific (e.g., study of QFTs other than the Standard Model) string theory other than unification has not yet departed from science.

Recognition of the string unification failure also leads to the next puzzle – from which direction is progress likely to come?

28. **Peter Orland**
May 22, 2008

"Recognition of the string unification failure also leads to the next puzzle – from which direction is progress likely to come?"

Arun,

Though it all depends on what you mean by progress, I think the best hope is experiment.

29. **Matt**
May 22, 2008

This is a subject near to my heart, as my own website is named after a quote of Maxwell which some scientists could stand to re-read.

In every branch of knowledge the progress is proportional to the amount of facts on which to build, and therefore to the facility of obtaining data.

If string theory or the multiverse or anything else can't build on facts, it ain't science.

Quick Links

May 15, 2008
Categories: Uncategorized

The speakers for Strings 2008 have been announced. One anomaly is that someone from the LQG camp has finally been invited, Carlo Rovelli. Another anomaly is that Witten won't be speaking.

Remember last November's "unmistakable imprint of another universe" which vindicated string theory? False alarm.

There's a new X-files movie coming out this summer, The X-Files: I Want to Believe, with a plot that revolves around string theory and features Amanda Peet (no, not the string theorist).

Outside magazine has a profile this month of Garrett Lisi, and quotes from various physicists about last year's media storm. I'm pretty much with Frank Wilczek on this, who says:

> To my perception, Lisi hasn't advanced the story. That said, I admire people who think for themselves and dare to take on reality directly rather than writing footnotes to fashionable literature. So I hope he keeps trying and inspires others.

I also hear that the New Yorker will have an article about this, to appear sometime during the next couple weeks.

Bert Schroer has a new version of his paper about String theory and the crisis in particle physics. It contains both sociological observations on the string theory phenomenon, as well as more technical arguments about how to think about a quantum theory of strings. Schroer was involved in endless battles on this blog a couple years ago over an earlier version of this paper. People who want to argue this again are encouraged to first read through the old discussion, and then see if there's something new and interesting to contribute, rather than a rehash of the previous arguments.

Comments

1. **Sara**
   May 15, 2008

   I'm very very happy they have choosen Carlo Rovelli as speaker, but I asked myself why they don't have called Lee Smolin, for example, or somebody else.

2. **Coin**
   May 15, 2008

   Is it known what Rovelli might be talking about?

3. **Sulfur Surfer**
   May 15, 2008

   Schroer's new paper claims that there are technical flaws in the understanding of string theory. However, the entire basis of his argument is incorrect. He begins by asserting that "string theory has no vacuum polarization." He appears to draw this conclusion from the ultraviolet finiteness of the theory, and its S-matrix character, but this is a non sequitur and false besides. For example, the most notable physical consequence of vacuum polarization is the running of couplings; such running is observable in the S-matrix (and this is actually done in accelerator observations). The couplings do indeed run below the string scale in string theory, and exactly as they would in the corresponding effective low energy QFT.

4. **Jean-Paul**
   May 15, 2008

   Surfer — if you are so sure about running couplings, why don't you compute just one on-shell S-matrix element that has "observable" running. You will see that all on-shell S-matrix elements are infrared divergent in D=4, already at the one loop level.

5. **Sulfur Surfer**
   May 15, 2008

   Jean-Paul — Sure, but the meaning of these divergences is taught in every QFT course, as is the cure – what particle physicists actually observe, and calculate, are IR finite cross sections with the detector resolution taken into account. You can substitute that long phrase for `S matrix', but I don't think that this was Schroer's point.

6. **Trent**
   May 16, 2008

   Peter,

   "….. see if there's something new and interesting to contribute, rather than a rehash of the previous arguments."

   Do you ever come up with new arguments? It seems to me you constantly are rehashing your old arguments. Or did I miss something? Do you actually have new arguments?

   Best,
   Trent

7. **Professor R**
   May 16, 2008

   Hi Peter, thanks for the reference to the Garrett Lisi article in Outside Magazine, it's a good article once you get past the initial bombast. Given his lifestyle, I imagine Garret probably enjoyed this article more than any of the others!

   I didn't know what to think about the Lisi story when it broke, but I'm glad this story is resurfacing, it's a bit of light relief in our dull lives. ..

   While the Wilczek quote probably summs it up for most of us, there are a few points I'd like to make

1. Every time journos draw a comparison with Einstein, Lisi patiently points out that the tenure issue is the only similarity – so it's not his fault they keep making this comparison

2. I don't see the problem with the surfer angle – surely it makes a welcome change from the usual media view of scientists

3. I'm delighted to see group theory get some attention – few outside the particle physics field have the slightest idea of the importance of group theory in this field…and even if the whole E8xE8 thing turns out to be a fairly trivial classification, so what? It's very nice to be reminded of the eigthfold way, no harm at all

I think we physicists are inclined to react strongly against media attention, especially if we feel there are more deserving cases. It's a pity, because such stories probably do far more for the public perception of physics than any number of well-intentioned school visits! Cormac

8. **D R Lunsford**
May 16, 2008

Schroer makes a chilling point in his paper, one that I've often worried about – apart from derailing progress in science, the current environment is utterly corrosive of the collective understanding of known results – so the old theorists are something like stone masons without apprentices and with no more cathedrals to build, and as they pass on, they take their hard won knowledge and experience with them. I know in my own area, completely separate from QFT, I find a shocking ignorance of basic things among my juniors. Of course the knowledge is still there, on the books that no one reads, in the history that is ignored. It does no good if someone does not own it. This to me is a tragedy beyond expression.

-drl

9. **David Nataf**
May 16, 2008

Am I the only one who found the profile of Garrett Lisi offensive? The article refers to him as a "hobo" and a "surfer bum".

10. **MIchael**
May 16, 2008

Trent wrote:

"Peter, … Do you ever come up with new arguments? It seems to me you constantly are rehashing your old arguments. Or did I miss something?"

The argument "String theory does not make predictions" is so strong that it does not need new ones. String theorists should take it as a challenge. The fact that they don't is telling.

11. **Mitch Miller**
May 16, 2008

David,

Do you find it offensive to Lisi or Bums?

(Not a dig at Lisi, as I think he used those terms to refer to himself so I don't see how it could be offensive to the former)

12. **David Nataf**
May 16, 2008

I think the term "hobo" is very derogatory of the homeless and should not appear in a respectable publication, with the exception of paraphrasing. Language is very important.

13. **Professor R**
May 17, 2008

P.S. Thanks for the listing on the website Peter, I hadn't noticed it!

Re Schroer paper, I'm still wading through it. Broadly speaking, I find myself in agreement with most of his points. However, it seems to me that one big point is conspicuous in its absence (so far). ..

Nobody asked nature to get more complicated mathematically, the more we probe the world of the sub-atomic, and attempt to describe it. That just seems to be the way the cookie crumbles, so far. Of course, this may well be an indication that the entire ST program is simply barking up the wrong tree… but isn't there another possibility?

Namely, that we simply haven't developed the appropriate mathematical framework yet..perhaps the breakthrough is just around the corner..

After all, E. was lucky Riemann had already deveopled non-Euclidean geometry, when general relativity came along. If Riemann hadn't, and GR involved harder maths, it's possible physicists might have missed the whole GR thing for quite a while, instead starting with something vague and ambiguous before we narrowing it down…

or is that simply a naive view of the experimentalist?
Regards, Cormac

14. **D R Lunsford**
May 17, 2008

Prof R,

Your point about the mathematical description being more complex is, I think, a little off. In fact most theories become more coherent as they get closer to nature – and this shows up in the mathematical expression. A sure way to know that something is probably on the wrong track is that is lacks tightness and economy of expression. Compare, for instance, the Lorentz deformable electron with Maxwell-Lorentz relativistic dynamics.

-drl

15. **Walt**
May 17, 2008

Trent, I suggest you scroll up to the masthead, and look where you're commenting. This is Peter's blog. If you find his posts repetitive, I suggest you look for something more appealing to you. There are many recipe blogs, for example.

16. **Jason**
May 18, 2008

Hobo is not an offensive term. Hobos call themselves hobos. Hobos are often homeless, but hobo is not synonymous with homeless person. A hobo is simply one who adopts a wandering way of life, traveling by hitching rides on freight trains. There is a hobo convention every August in Britt, Iowa; and a hobo king and queen are cononated there.

17. **AnonLQG**
May 18, 2008

Sara, because Carlo Rovelli rather then Lee Smolin is at the heart of the current developments in Spinfoams/Lattice Gravity/LQG. Smolins latest works are viewed with tremendous skepticism in the community and the general feeling is that he has vastly oversold them. Many are quite unhappy with the fact that he is seen as representing the community.

18. **Professor R**
May 18, 2008

DR, of course I agree with your point " most theories become more coherent as they get closer to nature – and this shows up in the mathematical expression".

But my point is, could it not be that ST simply hasn't reached this stage yet? Many theories are far too general, and non-specific, in the early stages of development. Perhaps, if the math is difficult enough, a theory could get stuck in this phase for decades before the key breakthroughs occur (if ever)? Just a thought…Cormac

19. **Sara**
May 18, 2008

AnonLQG,
can you indicate me any papers in which Carlo Rovelli discuss this subject?
Thank you.

20. **AnonLQG**
May 18, 2008

Which subject?

21. **Arun**
May 18, 2008

One anomaly is that someone from the LQG camp has finally been invited, Carlo Rovelli.

Don't worry, string anomaly cancellation will be found in dimensions that do not admit Rovelli.

22. **AnonLQG**
May 19, 2008

is that in and only in? Because as Rovelli is an empirical fact that would mean that anomaly cancellation can not occur for physical systems….

23. **Clark**
May 22, 2008

For a persuasive account of the seminal opposition between Planck and Mach on the core of scientific understanding :

[SEP_on_Ernst_Mach]

One side of the dialectic, in the span of a few generations detached from its founder's restrained values and amplified to an extreme, inevitably spawns nonsense such as theories of everything.

24. **Yok**
May 26, 2008

> can you indicate me any papers in which Carlo Rovelli discuss this subject?

"Flipped spinfoam vertex and loop gravity." Nucl.Phys.B798:251-290,2008. arXiv:0708.1236

"LQG vertex with finite Immirzi parameter." Nucl.Phys.B799:136-149,2008. arXiv:0711.0146

25. **Sara**
May 27, 2008

Thank you very much Yok, I'm going to read them with great interest.

26. **Ervin Goldfain**
May 27, 2008

A recent paper by Hashimoto et al. published in Phys Rev D 77, 086001 (2008) claims (what appears to be) the first experimental evidence for brane theory. Here is the abstract:

"Using holographic QCD based on D4-branes and 8-anti-D8-branes, we have computed couplings of glueballs to light mesons. We describe glueball decay by explicitly calculating its decay widths and branching ratios. Interestingly, while glueballs remain less well understood both theoretically and experimentally, our results are found to be consistent with the experimental data for the scalar glueball candidate f0□1500□. More generally, holographic QCD predicts that decay of any glueball to 4□0 i is suppressed, and that mixing of the lightest glueball with q □ q mesons is small.
DOI: 10.1103/PhysRevD.77.086001 PACS numbers: 11.25.Tq, 12.39.Mk"

I invite comments on the validity of these claims.

27. **Peter Woit**
May 27, 2008

Ervin,

This has nothing at all to do with the debate over the testability of string theory, which is about whether one can test the idea of string theory as a fundamental theory.

We have very good evidence for what the theory governing strongly interacting particles is: QCD. Many people are working on trying to find string theories that would give approximate calculational methods in QCD valid at strong coupling, and this is one such attempt. If the "predictions" come out right, all that is being tested is the validity of the approximation method.

Bryce DeWitt on Quantum Gravity and String Theory

May 21, 2008
Categories: Uncategorized

Last night a preprint appeared on the arXiv from beyond the grave, an undated manuscript entitled Quantum Gravity, Yesterday and Today, found without any indication of its purpose in the files of Bryce DeWitt, who passed away in 2004.

DeWitt devoted much of his career to the question of how to quantize the gravitational field, beginning back in 1948 when he was a student of Julian Schwinger. He has some interesting comments about the dramatic changes over the years in popularity of research work on GR and quantum gravity:

> Most of you can have no idea how hostile the physics community was, in those days, to persons who studied general relativity. It was worse than the hostility emanating from some quarters today toward the string-theory community. In the mid fifties Sam Goudsmidt, then Editor-in-Chief of the Physical Review, let it be known that an editorial would soon appear saying that the Physical Review and Physical Review Letters would no longer accept "papers on gravitation or other fundamental theory." That this editorial did not appear was due to the behind-the-scenes efforts of John Wheeler.

DeWitt gives some history of his important work on the quantization of gauge theories, which culminated in working out a functional integral method to handle to all orders the ghost terms that Feynman had shown to be necessary. He describes a 1955 offer from the Glenn L. Martin Aircraft Company to fund his research in hopes that it would lead to an antigravity device, one that he didn't accept. Instead, the Air Force supported his research during the period he was unraveling the story of ghosts, support that ended in 1966 when they finally realized that gravity research was not going to lead to magical results. With the termination of his grant, he could no longer pay page charges to the Physical Review, delaying the publication of one of his papers by a year.

He also has some interesting comments about the DeWitt-Wheeler equation:

> … intensive work was carried out in those years on canonical quantum gravity, culminating in an equation that bears my name along with that of John Wheeler who was the real driving force. Research on the consequences of this equation continues to this day, stimulated by work of Abhay Ashtekar, and some of it is quite elegant. But apart from some apparently important results on so-called "spin foams" I tend to regard the work as misplaced. Although WKB approximations to solutions of the equation may legitimately be used for such purposes as calculating quantum fluctuations in the early universe, and although the equation forces physicists to think about a wave function for the whole universe and to confront Everett's manyworld view of quantum mechanics, the equation, at least in its original form, cannot serve as the definition of quantum gravity. Aside from the fact that it violates the very spirit of general relativity by singling out spacelike hypersurfaces for special treatment, it can be shown not to be derivable, except approximately, from a functional integral. For me the functional integral must be the starting point.

He ends the paper with positive comments on string theory:

> In viewing string theory one is struck by how completely the tables have been turned in fifty years. Gravity was once viewed as a kind of innocuous background, certainly irrelevant to quantum field theory. Today gravity plays a central role. Its existence justifies string theory! There is a saying in English: "You can't make a silk purse out of a sow's ear." In the early seventies string theory was a sow's ear. Nobody took it seriously as a fundamental theory. Then it was discovered that strings carry massless spin-two modes. So, in the early eighties, the picture was turned upside down. String theory suddenly needed gravity, as well as a host of other things that may or may not be there. Seen from this point of view string theory is a silk purse. I shall end my talk by mentioning just two things that, from a nonspecialist's point of view, make it look rather pretty.

The two things he has in mind are the ability of a single string diagram to sum up a lot of Feynman diagrams, and the use of orbifolds to make possible topology-changing transitions.

Comments

1. **Shantanu**
   May 21, 2008

   Hi,
   Peter,
   This is interesting. Were you planning to attend the emergent gravity workshop at MIT in August?
   also did not know that Olaf Dreyer(who primarily works in LQG) is now at MIT.

2. **Brandon DiNunno**
   May 21, 2008

   If you found this particular article to be interesting, you should see all of the amazing things in Bryce's documents. Hopefully Cecile and I will be able to get a few more out to the public very soon.

3. **Marcus**
   May 21, 2008

   on internal evidence I would date the manuscript roughly around 1995. it refers to something "forty years ago" and cites a 1955 publication. Can someone more knowledgeable say whether circa 1995 makes sense?

4. **Professor R**
   May 21, 2008

   "Most of you can have no idea how hostile the physics community was, in those days, to persons who studied general relativity."

   Peter: coming from such a reputable source, that's an astonishing statement. It makes me wonder whether it really is true that the American schools are more suceptible to trends that their European counterparts…

   For example, I do know that at the Dublin IAS in the fifties and sixties, all the quantum hotshots started their careers with serious research in GR as a matter of course (not least due to the presence of Synge). For example Dad's first book was on GR, not QFT (see General Relativity; Papers in Honour of JL Synge, ed O'Raifeartaigh OUP 1972).

   Actually, if you don't know this collection, you might find it a treat – it has papers by Lanczos, Sciama, Penrose, Chandrasekhar and Israel, among others!

5. **Robert**
May 21, 2008

Thank you for the link to this first-hand view, by one of the founders, on the history of quantum gravity.

6. **Peter Woit**
May 21, 2008

Brandon,

Thanks, will look forward to more. Maybe you can set up a web-site…

Shantanu,

Thanks for the info, but my interest in quantum gravity is limited enough that I wouldn't travel to such a conference.

Cormac,

At least in the US, I think GR was considered somewhat of a backwater, with the leading institutions and physicists pretty focussed on the particle physics that was coming out of dramatic amounts of accelerator data. Much of the fundamental work on gauge theory ended up being done in Europe and the Soviet Union, with DeWitt one of the few doing this in the US. As he notes, it's funny that the US during the late 80s and 90s then swung to the other extreme and became completely focussed on (one idea about) quantum gravity.

7. **Dr. E**
May 22, 2008

I recall Wheeler characterizing the heavy focus on particle physics as "ino-itis."

8. **Indrajeet**
May 22, 2008

Hi Peter,
This is just to draw to your notice that Willis Lamb has passed away on 21st. More here:
http://edition.cnn.com/2008/TECH/science/05/20/obit.lamb.ap/index.html

9. **RZ**
May 22, 2008

"Given the fact that the vertex function in diagram 1 contains over 175 terms and that the vertex functions in the remaining diagrams each contain 11 terms, leading to over 500 terms in all[…]"
I suspect that there is a typo there, and there actually were 110 terms.

10. **wb**
May 22, 2008

"Most of you can have no idea how hostile the physics community was, in those days, to persons who studied general relativity."

I can certainly confirm that in the '60's Chandrasekhar warned prospective students. "GR is outside the mainstream of physics. That is okay for people at my stage of a career. But as a young person you need to consider whether you want to work outside the mainstream."

11. **Shantanu**
May 22, 2008

Peter and wb, I thought that 60s and 70s were the golden age of black hole physics. I did not know that GR was outside the mainstream.
Shantanu

Does Time Run Backward in Other Universes?

May 22, 2008
Categories: Multiverse Mania

Scientific American in recent years seems to be quite fond of parallel universes, with major articles promoting the multiverse here, here and here (commentary on this blog here and here). Their latest issue continues in this vein with an article by Sean Carroll entitled Does Time Run Backward in Other Universes?, which advertises his 2004 work with Jennifer Chen claiming that the multiverse explains the arrow of time. For new blog entries about this, see here for something from Sean, here for a Lubos rant.

As with all claims about the multiverse, the problem is whether they are even in principle scientifically testable or not. If they're not, they're not science and promoting them to the public is a bad idea. The only thing I can find in the Scientific American article that addresses the testability issue at all is the following:

> As of right now, the jury is out on our model. Cosmologists have contemplated the idea of baby universes for many years, but we do not understand the birthing process. If quantum fluctuations could create new universes, they could also create many other things—for example, an entire galaxy. For a scenario like ours to explain the universe we see, it has to predict that most galaxies arise in the aftermath of big bang–like events and not as lonely fluctuations in an otherwise empty universe. If not, our universe would seem highly unnatural.

This doesn't seem to have anything to do specifically with the Carroll/Chen claims about the arrow of time, but rather is just a restatement of one of the desired properties of multiverse models, that they don't lead to "Boltzmann Brains".

Comments

1. **Ethan Siegel**
   May 22, 2008

   Peter,

   Surely this is a question that we don't take seriously anymore. It isn't that the laws of physics aren't time-reversal invariant, it's that quantum mechanics and "probabilities" have a preferred arrow.

   If I take a free neutron and it decays into a proton, electron, and antineutrino, well, sure, if I took the proton, electron, and antineutrino and fired them at one another with the same momenta the neutron emitted them at, they could re-form into a neutron. But if I run time forwards, the neutron will decay again; if I run time backwards, the neutron will never decay.

   Or, in simpler terms, I can't un-fry an egg. With a view of this, why is it even interesting to think about the arrow of time?

   Ethan

2. **Peter Woit**
   May 22, 2008

   Ethan,

   If you want to discuss this kind of question about the arrow of time, best to do it over at Sean's blog. The topic here is what if anything this has to do with the multiverse.

3. **Kris Krogh**
   May 22, 2008

   The time issue has also been a topic at Backreaction, with a poll taken: here

4. **DB**
   May 23, 2008

   There is a long tradition behind the cosmological arrow of time, going back to Boltzmann, and the question which Sean and his colleague attempt to address directly, namely, why did the universe start from such a low-entropy state really does go to the heart of the issue.

   The modern history of this subject dates from the Cornell 1963 conference (described in T. Gold The Nature of Time, 1967). This meeting is remembered for the presence of a Mr.X who, according to Hawking "felt the proceedings were so worthless that he didn't want his name associated with them. It was an open secret that Mr.X was Richard Feynman" (cf. S.Hawking in "Physical Origins of Time Asymmetry, eds J.J.Halliwell, J.Perez-Mercader, W.H. Zurek" (1993), p.346). In my mischievous way I bring it up because Sean is fond of reminding us that he sits at Dick Feynman's desk at Caltech:) This latter reference documents the second major conference on the subject in Mazagon, Spain in 1991. It's an interesting read, with contributions from Wheeler, Gell-Mann and Hartle, Hawking, Zureck, DeWitt and Griffiths (of consistent histories fame) and others, along with a record of post-talk debates. But, as before, it's very philosophical and I couldn't extract any evidence that anyone was in the business of making scientific predictions. Instead, they appeared to spend a fair of time arguing over semantics, e.g. decoherence, consistent histories, what is "reality" etc..

   Sean's paper is very much in the tradition and style of such contributions, but I liked it because unlike many others it really tries to home in on the core issue – why the initial low entropy.

   Ultimately however, most of this quantum cosmological palaver relies on Everett's many-worlds interpretation of quantum mechanics and the multiverse which flows from that.

   But as we know, there is no credible experimental mechanism to tell us whether the Copenhagen interpretation should be favoured over the Many-Worlds one (or any of the other stabs at this topic). David Deutsch, who filled in the gaps in Everett's original arguments claims there is (Int. J. Theor. Phys. 1985), but other dispute this.

   All I ask is, show me a reasonable experiment capable of telling us which interpretation (if any) is favoured by nature. Until then, it's just philosophy with a high-tech veneer.

5. **Peter Woit**
   May 23, 2008

DB,

My problem is not with the discussion of the entropy problem of early cosmology, but with the idea of writing articles for a major US popular science magazine promoting the multiverse and Boltzmann brain argumentation. This gives people the idea that this kind of empty speculation is what science is, impressing those who can't tell the difference between science and science fiction, and turning off those who can.

6. **Professor R**
May 23, 2008

I'm reading through Sean's article on my lunchour. It's a very good artilce, highly informative. But the context worries me too. I feel that most science magazines make very little distinction btween theories that are grounded in experiment, and theories which are promising.

It must be very confusing for the reader – in fact, I know it is. Every time I refer to say, GR, in a public lecture, it's clear most have no idea of the supporting evidence. It all gets lumped in with 'all those other theories', from string theory to the multiverse.

I sometimes wonder if magazines should have a special section marked 'speculative' – articles on topics that are a long way from experimental verification….Cormac

7. **DB**
May 23, 2008

I completely agree, Peter. My comments were directed at his paper with Chen.

8. **D R Lunsford**
May 23, 2008

I just had an example of this last night. I ran into a friend at a tavern, and he excitedly told me how he learned from "Discover Magazine" that a new accelerator was going to test string theory. He's a smart person but has no training in math or science. Who could blame him for believing what he reads? The editors of these publications do not care what the content is, as long as it sells. Even "Sky and Telescope" is guilty of hyping pseudo-science to generate sales.

-drl

9. **Peter Woit**
May 23, 2008

drl,

I don't think this is the fault of the editors of these magazines, they are just printing what prominent string theorists are saying. Quite a few of these are out there promoting the idea that "the LHC will test string theory", with the great majority of physicists who know better keeping quiet.

I suppose after all one should perhaps encourage this nonsense. If you could get everyone to believe that the "LHC will test string theory", and as expected no evidence for string theory shows up there, maybe everyone would then accept that string theory had failed and move on.

10. **Jonathan Vos Post**
May 23, 2008

I have co-authored and published a couple of papers, in the Physics track of international conferences, on the Arrow of Time which deal with the putative Multiverse, including this one, which grapples with the specfic paper of Carroll and Chen:

Title: Comparative Quantum Cosmology: Causality, Singularity, and Boundary Conditions
Authors: Philip V. Fellman, Jonathan Vos Post, Christine M. Carmichael, Andrew Carmichael Post
Comments: 17 pages, 2 figures. 7th International Conference on Complex Systems
Subjects: General Relativity and Quantum Cosmology (gr-qc)

(Submitted on 26 Oct 2007)

Abstract: In this review article we compare the recent work of Peter Lynds, "On a finite universe with no beginning or end", with that of Stephen Hawking, primarily "Quantum Cosmology, M-Theory, and the Anthropic Principle", and two foundational works by Sean M. Carroll and Jennifer Chen, "Does Inflation Provide Natural Conditions for the Universe" and "Spontaneous Inflation and the Origin of the Arrow of Time", in order to evaluate their comparative treatments of the nature and role of causality, time ordering, thermodynamic reversibility, singularities and boundary conditions in the formation of the early universe. We briefly reference Smolin and Kauffman's recent arguments with respect to possible processes of "evolutionary selection" in early universe formation as an alternative explanation to key elements of Hawking's earlier "M-Theory", and its attendant anthropic principle. We also briefly excerpt a short section of Smolin's recent work on topology in quantum loop gravity, simply as an illustrative example of the type of complex quantum topological transformation which he offers as a theoretical alternative to string theory in quantum cosmology.

11. **Professor R**
May 23, 2008

Peter, re
"I don't think this is the fault of the editors of these magazines, they are just printing what prominent string theorists are saying"

I think that's we disagree. Granted, one can't expect science journos and their editors to have a good knowledge of every part of science. But they are surely aware that each branch tends to overemphasise its own importance, and overlook its flaws.

In other words, they shouldn't be just printing what prominent string theorists are saying. Surely it's the job of a good editor to play devil's advocate to some degree, and bring the article back down to earth. To me, it seems that this professional skepticism has gone missing in today's science journalism…Cormac

12. **Kris Krogh**
May 23, 2008

Scientific American used to have the brilliant, independent-minded physicist Philip Morrison, who helped keep things grounded in reality. Unfortunately, he passed away several years ago.

13. **Thomas Larsson**
May 24, 2008

I suppose after all one should perhaps encourage this nonsense. If you could get everyone to believe that the "LHC will test string theory", and as expected no evidence for string theory shows up there, maybe everyone would then accept that string theory had failed and move on.

Larsson's theorem uses string theory to make a falsifiable prediction about the LHC. I am particularly proud that I already in 2007 could correctly predict that poor Lubos would lose his experimental-susy-by-2006 bet.

Of course, if the LHC does find sparticles I will be proven wrong. It is in the nature of falsifiable predictions that they are, well, falsifiable.

14. **mike harney**
May 24, 2008

Does anybody know if we are spending government money on multiverse theories? If so, I would like to put in a grant of my own for some untested, highly unrealistic notions.

15. **Haelfix**
May 24, 2008

So I gather you don't like the idea of eternal inflation either. A lot of people don't, but you must admit its a logical possibility (if you have inflation at one place, what stops you from having inflation somewhere else).

It *is* testable to a certain degree, though we need to have better resolution of gaussian profiles and presumably gravity wave detectors to really distinguish between the class of models

16. **Haelfix**
May 24, 2008

Replace eternal with chaotic for the post to make sense.

17. **Peter Woit**
May 24, 2008

Mike Harney,

To the extent government grant money is being spent on multiverse models, I think the amounts are quite small. Actually, I'm quite curious how multiverse grant proposals are doing before NSF and DOE panels. Searching on "multiverse" in the NSF database of funded proposals turns up nothing. Getting popular science magazines to promote ones multiverse research is one thing, getting other physicists to give it the kind of uniformly high marks needed to get a grant funded is something different.

Haelfix,

My comments were about Sean's multiverse/arrow of time proposal, which as far as I can tell is completely empty of any predictive power or any sort of testability. Different inflationary models have different degrees to which they make distinctive predictions, but at least in those cases there is something to discuss.

18. **Smedley**
May 25, 2008

Point of interest: you ought probably no longer link to anything on Lubos Motl's blog, as he has set it up so that any visitors with your blog as their referred are redirected to this (rather unfortunate and vitriolic) page: http://lubos.motl.googlepages.com/crackpot-not-even-wrong.html

Stay classy, Motl.

19. **Arun**
May 26, 2008

'tis why physicists find second careers on Wall Street – they excel in speculation.

News From CERN and Fermilab

May 23, 2008
Categories: Experimental HEP News

Things have been going quite well recently at the LHC, with cooldown beginning now for the last two sectors of the ring, three sectors cool, and three cooling. The latest cooldown schedule is here, a report yesterday on progress here. Sometime in July the beam commissioning process should begin, with the current plan to inject first particles in late July. About 2 months should be needed to get to first collisions at 10 TeV and the possibility of starting to take some data. The LHC has to have a winter shutdown so that the residents of Geneva don't freeze to death, and that will start in late November. Estimates are that the fall 10 TeV run will produce total luminosity of "tens of pb^{-1}". Tommaso Dorigo predicts 40 pb^{-1}, see more here. Also, don't miss his series of recent posts from PPC 2008 giving the best blogging from a conference I've ever seen… The plan for 2009 is to run at 14 TeV, with perhaps 2.5 fb^{-1}.

The situation at Fermilab is extremely unclear. The final plan for layoffs there has 140 people losing their jobs, presumably starting next week. This week, Congress is facing down the president, putting together bills to fund the war in Iraq that also contain large amounts of new domestic spending, something Bush has promised to veto. The Senate version of this bill contains $45 million for DOE HEP research, which presumably would be enough to stop the Fermilab layoffs. It passed yesterday with a veto-proof majority of 75-22. The House bill has no such provisions, and now the two bills need to be reconciled, and either passed over Bush's veto or somehow made acceptable to him. More about this here. Remember that is we're already two-thirds of the way through FY2008, with US HEP labs unsure (by a huge amount) of what their budget for the year will end up being. What a way to run a government…

Director Oddone has scheduled two all-hands meetings today, one for half the lab's divisions at 11:30, another for the other half at 1pm.

Update: The University of Chicago today announced an anonymous $5 million donation from a family that will go towards funding some of the programs at Fermilab that have suffered from this year's budget cuts. This will allow Fermilab to stop the forced furlough program it has been operating under at the end of this month. The prospect of layoffs at the lab continues.

Comments

1. **theoreticalminimum**
   May 23, 2008

   This is a very short review of '*Même pas fausse ! La physique renvoyée… dans ses cordes*', but I thought you might want to read it.

2. **Peter Woit**
   May 23, 2008

   theoreticalminimum,

   Thanks, it seems to be hard to get a link to that review but I did find it. It's a fair and sensible review of the book.

3. **JTankers**
   May 24, 2008

   Congress authorized $100 million by a veto proof majority. The jobs are safe. Now lets focus on the safety of the planet.

   A new site was created to detail the FACTS at LHCFacts.org, and allow honest feed back and counter arguments.

   Review the issues for your self to determine who is honest about the risks and how large those risks might surprisingly be… (Some evidence indicates that real risk may be 100%, but science is unable to determine risks accurately at this time. That is not a time to say "no proof of reasonable risk, launch!"

   Dr. Raj Baldev, Director of the Indira Gandhi Center for Atomic Research. Dr. Baldev says the following " But the scientists are fully aware that it is not a project without a grave risk to the life of the Earth."

   JTankers
   LHCFacts.org

4. **wilbur**
   May 24, 2008

   JTanker, nothing will happen at the LHC that does not happen everyday in the upper atmosphere with cosmic rays… except that at the LHC there will be a lot of people watching the show with huge detectors…

5. **Peter Woit**
   May 24, 2008

   Please, if anyone wants to discuss JTankers arguments, do it as his website.

6. **DB**
   May 25, 2008

   Oddone revealed that an anonymous Chicago family has donated $5m to Fermilab so the furloughs no longer need to go ahead.

   As to the House of Representatives going along with the Senate proposal, that's considered unlikely:

   http://www.symmetrymagazine.org/breaking/2008/05/23/5-million-donation-relieves-fermilab-of-furloughs/

7. **Professor R**
   May 25, 2008

   To European eyes, it is incredible to see Fermilab with such funding worries, given its record of achievement, and current success…

Just how dumb are your leaders in the White House? Do they not have science advisors with any intelligence ?

Here in Ireland, a similar situation prevails. Ireland is the only country in the EU that is not a member of CERN, and we have been trying to persuade the governmnet to rectify this shameful situation for years.

But that level of ignorance in a small country on the edge of europe is one thing – it is quite another in the worlds permier superpower..
Cormac

8. **Visitor**
   May 25, 2008

   "Just how dumb are your leaders in the White House?"
   Not as dumb as you think; the White House wants to fund FermiLab, it is the Democrats who have cut their appropriations specifically because Bush wants to protect them. You, along with some other people here, need to rethink your stereotypes. (Of course, that's not very likely, is it?)
   (There have already been a few threads here about the situation.)
   Incidentally, if Obama is elected, I would expect that the situation for science funding will get worse…

9. **Coin**
   May 25, 2008

   Do they not have science advisors with any intelligence ?

   I don't think you need those last three words there, really, at this point.

10. **Professor R**
    May 26, 2008

    Coin: I see what you mean!

    To European eyes, it seems incredible that Fermilab funding is being cut at this point (irrespective of who is doing the cutting), given its current beam efficiency, and the fact that LHC will not start delivering for some months yet.

    Cost effective? Any experimentalist will tell you that cost-effectiveness is about grabbing as much data once the thing's working smoothly!

11. **wb**
    May 27, 2008

    I accept the argument that once a machine is running you take as much data as you possibly can. Nonetheless if one uses the old guideline that a "factor of two in energy is worth a factor of ten in luminosity with a hadron collider, the prospect of 40 inverse picobarns at LHC by years end is becoming a scene stealer.

12. **Professor R**
    May 28, 2008

    Agreed, wb – but that's at year's end, not now!

Train of Thought

May 23, 2008
Categories: Uncategorized

For the last 15 years the New York City subway has featured “Poetry in Motion”, which places extracts of poetry in subway cars. Starting next month this program will be expanded, joined by Train of Thought, which will add “short quotations in history, philosophy, literature, and science chosen by Columbia University’s Graduate School of Arts and Sciences.” I gather that my colleague Henry Pinkham, a mathematician now dean of the Graduate School, is responsible for this. Of the first two quotations to go up next month, one is dear to my heart, from Galileo:

The book of nature is written in the language of mathematics. Its symbols are triangles, circles, and other geometric figures, without which it is impossible to understand a single word; without which there is only a vain wandering through a dark labyrinth.

Comments

1. **galileo**
   May 23, 2008

   > La filosofia è scritta in questo grandissimo libro che continuamente ci sta aperto innanzi a gli occhi (io dico l’universo), ma non si può intendere se prima non s’impara a intender la lingua, e conoscer i caratteri, ne’quali è scritto. Egli è scritto in lingua matematica, e i caratteri sono triangoli, cerchi, ed altre figure geometriche, senza i quali mezi è impossibile a intenderne umanamente parola; senza questi è un aggirarsi vanamente per un’oscuro laberinto.
   >
   > Galileo Galilei, “Il Saggiatore,” (1623).

2. **Ken Muldrew**
   May 23, 2008

   Here is a quote from Feynman that is also in the genre of refutations for the trope about being able to explain your theory to your grandmother:

   “… it is impossible to explain honestly the beauties of the laws of nature in a way that people can feel, without their having some deep understanding of mathematics. I am sorry, but this seems to be the case.”

3. **exanto**
   May 24, 2008

   This is actually a great idea. My personal favourite from Galileo:

   > Man kann einen Menschen nichts lehren, man kann ihm nur helfen, es in sich selbst zu entdecken.

   or in english (freely translated):

   > One cannot teach a human anything, you can only help him to discover it on his own.

4. **Paul Jackson**
   May 24, 2008

   Galileo is of course right. But those who overindulge in mathematical ratiocination (like string theorists) should remember the more sceptical view of Mark Twain:

   “There is something fascinating about science. One gets such wholesale returns of conjecture out of such a trifling investment in fact.”

   Life on the Mississippi

5. **Dorian Allworthy**
   May 24, 2008

   Men can do nothing without the make believe of a beginning. Even science, the strict measurer, is obliged to start with a make-believe unit and must fix on a point in the stars’ unceasing journey when his sidereal clock shall pretend that time is at Nought. His less accurate grandmother Poetry has always been understood to start in the middle; but on reflection it appears that her proceeding is not very different from his;since Science, too, reckons backward as well as forward, divides his unit into billions, and with his clock finger at Nought really sets off in Medias res.
   George Eliot

6. **nbutsomedy**
   May 26, 2008

   Why Galileo was speaking German?

INSPIRE

May 28, 2008
Categories: Uncategorized

Since 1968 SLAC has been maintaining a database of HEP documents called SPIRES, and this has become one of the main tools used by anybody searching the HEP literature. In recent years CERN has developed a much more modern document management system known as CDS Invenio. The two projects are now being brought together into something to be called INSPIRE, which will combine the best of both, in particular making the SPIRES data available through the more modern Invenio software.

There's a press release from DESY about this here, and an alpha version is up and running here. The current state of the project is that most of the SPIRES functionality has been reproduced, and they are working on getting a beta version ready of a complete replacement of SPIRES.

Last week at DESY a workshop was held about this, announced as an HEP Information Resource Summit, talks are available here. There were presentations from other HEP information providers, including the APS, commercial publishers and the arXiv. The arXiv presentation discussed their desire to better support blogging, and the role of the blogosphere, including the fact that Garrett Lisi's paper was the most downloaded article on the arXiv. The current trackback system provides links to 21 discussions of the paper, but due to the Distler/arXiv policy of censoring links to this blog, one that is missing is the discussion here. More and more very worthwhile content is appearing on blogs, so the question of how to make this readily available in a useful form will become an increasingly important one.

Unfortunately, while the arXiv does a good job of bringing together mathematics and physics, there seems to be no discussion of the role of the mathematics literature in the new INSPIRE system. Besides the arXiv, the main database used by mathematicians is the excellent MathSciNet developed by the AMS.

Update: Travis Brooks of SPIRES has a posting about this here.

Comments

1. **Professor R**
   May 28, 2008

   Thanks for the ArXiv presentation Peter, interesting reading.
   On the 'like least ' slide, do you know what is meant by the three categories GUI, coverage, basic arXiv?

   I think it's great to see Garrett's paper riding so high – if nothing else, it's nice to remind the wider physics community of the role of group theory in today's particle physics!

2. **Professor R**
   May 28, 2008

   P.S. It seems strange that ArXiv have omited a trackback to the Lisi discussion on NEW. After all, Garrett himself contributed quite a bit to the discussion, as I recall – which is more than can be said for several of the discussions listed…Cormac

3. **Peter Woit**
   May 28, 2008

   Cormac,

   If you want to read up on "Trackback-gate" from a couple years ago, see for instance:

   http://www.math.columbia.edu/~woit/wordpress/?p=357

   and follow the links. The bottom line there is that Distler is unhappy with any mention of my criticisms of string theory appearing anywhere he controls (he is involved in running the hep-th part of the arXiv), and some other well-known multiverse-promoting physicists are unhappy with the idea of links to my criticisms of this as pseudo-science appearing on the arXiv. So, links to my blog are banned on hep-th. A dishonest excuse that only blogs run by reputable "active researchers" are allowed to have links was made, you can look through the trackbacks to the Lisi article to see if that's really the policy.

4. **Alejandro Rivero**
   May 28, 2008

   My experience with the people of SPIRES has always been better than with the arxiv. Of course, SPIRES do not need to censor papers. But I can cite two cases: when old physcomments needed to check for author data, Arxiv cut the access and SPIRES allowed it (and then Arxiv admins were paranoid because they thought I was hacking some of their mirrors). And when I worked out the genealogy of theoretical physicists, SPIRES people was kind enough to create a new "tree" output in HEPNAMES. But all of this was old history, before of the Arxiv API. So perhaps it could be interesting to give the guys some new credit!

   The social network think is an interesting idea. Perhaps they could provide an opensocial REST API interface, with "friends" akin to "coworkers", and "activity" equal to "publication" or update of a preprint. Then standard Opensocial gadgets could work as extensions of the Arxiv.

5. **Professor R**
   May 28, 2008

   Thanks Peter, enjoyed that link, especially Smolin's comment.

   I think the situation on the Lisi paper today points up the situation even more – if Garrett is happy to discuss his paper on your weblog, it seems absurd that ArXiv not link to the discussion in their list…

   Re ArXiv discussion of weblogs, I wonder did the criterion 'usefulness' get a mention – I find the links at NW v useful, which is why I keep coming back…Cormac

6. **somebody**
   May 28, 2008

   Distler is one of those guys who has to get off his throne. If he did not try to look so condescending, string theory would not have looked so pathetic, even despite your attacks. I say this as a string theorist, because he and Lubos do more to damage the credibility of string theorits than any single thing you have ever written. I have to say that I disagree with your views, but these guys do so much to help you!! Of late you are kind of extreme, but in the beginning at least, you were moderate, and some of us "string theorists" would have actuatlly agreed with you!

Good work, Peter, because a bit of disagreement is always good!

7. **dragon**
May 28, 2008

I note that there are no trackbacks from L Motl to any of Lee Smolin's papers since September 2006. It seems hard to believe that this is a case of voluntary restraint, so could it be that LM has been quietly banned from making trackbacks? That would be good news indeed.

8. **dragon**
May 28, 2008

By the way, I met someone at a conference who works in a field where people are regularly subjected to abuse by Motl and others. She said that she fears the trackback system — even if the attack is garbage, who can say whether a referee might be influenced by it? There's no point in saying that everyone knows that LM is full of it — the usual formulation is that he may be a lunatic, but he really knows his physics. Even Distler seems to subscribe to this strange notion, which is actually very debatable indeed outside a small area; in some cases his views on *physics* have bordered on [or gone beyond the border of] crackpottery. So the danger of referees being influenced in a malign way is not negligible.

So I strongly believe that the trackback system should be heavily moderated; only strictly technical discussions should be allowed, no attacks whether personal or otherwise, and no philosophical stuff [because that all too easily turns into abuse or something indistinguishable from it]. If the arxiv people aren't willing to do that —- which would be a pity, because some trackbacks are actually very useful —- they should scrap the whole thing.

9. **anon.**
May 29, 2008

'I note that there are no trackbacks from L Motl to any of Lee Smolin's papers since September 2006 … so could it be that LM has been quietly banned from making trackbacks? …' – dragon

See the second trackback (dated 7 Nov 2007) on the list http://arxiv.org/tb/0711.0770

arxiv censorship isn't entirely automated. For example I updated a paper some years ago and it was given a number but then deleted within the time taken to refresh the browser (a few seconds). They rely on their genius at spotting errors in papers based on the author's level of prestige and the title of the paper, not on reading and checking the actual content. So it is rather arbitrary. There are people there, making quick decisions based on gut feelings.

10. **Peter Woit**
May 29, 2008

somebody,

I agree that Jacques and Lubos have done far more damage to the interests of the string theory community than critics like me. They represent only one rather pathological wing of this community, and it's unfortunate that they have been the most vocal part of it in the blogosphere. I'm glad to hear that you appreciate what I've been up to here, even if you often disagree with it. You may be right that I've become more immoderate in recent times, perhaps driven to madness by the whole multiverse business…

dragon,

As anon points out, the arXiv hasn't stopped trackbacks to Lubos's blog, since there is one for Garrett's article. At one point I was paying attention to which trackbacks were appearing there, trying to figure out what their policy was, but I gave up on that long ago, unable to make much sense of it.

In general, I think the only way to provide a high quality source of information on the web is to have sensible people doing some degree of moderation of the content. This can't be automated, and there's no single right way to do it, with difficult choices having to be made. In general I think the arXiv does a good job, under difficult circumstances, but in this case I think they made an error in judgment, allowing decisions to be governed by someone seriously lacking in good sense. Distler's conviction that anyone who disagrees with him about string theory must be incompetent, coupled with his belief that personal insult is the way to carry on scientific discussion make him an inappropriate person to be making decisions about moderation.

As for the problem of links to Lubos's blog postings swaying referees, I guess I think that if you have as a referee someone who takes Lubos seriously, you've already got a big problem, whether Lubos has a posting on the article or not.

11. **Daniel de França MTd2**
May 29, 2008

I dont know exactly how much, who or why, etc… the damage someone did to the string theory image. But, I know that at least someone is doing a great job in promoting string theory, and that person in Urs, from n-cafe category. He is extremely helpful guy, always talks with formulas and try to work out ideas, even if those that re from non string framework, and is always motivated to promote string theory, even if it is in terms of just using its mathematics machinary.

Sorry for the eulogizing here, it's just that he is one of the people that made me start studying physics again after years of general deception with academia, and I wouldn't like people cursing string theorists as they were all leading to failure. Maybe, at least some of the mathematical framework motivated by it, will be used in a future QG theory…

12. **Marcus**
May 29, 2008

Daniel, I think you mean
"after years of general disappointment with academia,"

decu, in French, translates to disappointed, in English (not to deceived).

I share your high regard for Urs Schreiber and moreover do not consider him to be limited to string theory exclusively. He seems to have shifted successfully into mathematics, and have a wider scope now than he did a few years ago.

13. **Daniel de França MTd2**
May 29, 2008

Sure Marcus, thanks. But my mother language is Portuguese, not French :). But since they are closely related languages, I can see that the correction would likely have the same result anyway. 🙂

Even if I don't not understanding everything, Urs always posts great tutorials so that we end up learning a lot. In this aspect, John Baez is great too.

PS.: That word "de França" is in fact the first of my surnames, and the one I like best. I write "MTd2" just so that people can indentify my real name with the one I post on forums.

14. **Peter Orland**
June 1, 2008

I have been playing with the trial version of INSPIRE. Even the advanced search doesn't seem as versatile as SPIRES. I hope that the beta version is better.

15. **Shantanu**
June 1, 2008

Btw ADS has papers going back to the 11th century !!. I wish there was a similar database of
all GR papers.

P5 Report

May 29, 2008
Categories: Experimental HEP News

A new P5 report is out, and being discussed at the HEPAP meeting in Washington today. The charge to P5 was to develop tentative 10 year plans for US HEP, under 3 scenarios:

- Scenario A: funding at the current post-budget cut level of $688 million for the DOE.
- Scenario B: funding at the pre-budget cut (2007) level of $752 million for the DOE.
- Scenario C: a doubling of the budget over 10 years, starting from the 2007 level.

This report updates earlier ones and the EPP2010 report in light of new realities, specifically acknowledgement that the cost of the ILC means it's not happening anytime soon, and the grim budget situation caused by the recent budget cuts for this year. To be honest, it's very unclear to me how anyone can sensibly carry through this kind of exercise right now. With the supplemental appropriation still up in the air at the House and the Senate, it's hard to know what the US HEP budget will be next month, much less next year, or over 10 years. The LHC startup is only months away, and how long that takes and what the LHC shows are crucial things for any future planning.

In the report, the field is broken into three parts:

- The Energy Frontier: experiments at the highest possible CM energies. This is the Tevatron now, the LHC soon, and a possible electron collider later.
- The Luminosity Frontier: experiments at the highest possible event rates. This include neutrino experiments and searches for rare decays. The proposed "Project X" at Fermilab is the main possible new machine here.
- The Cosmic Frontier: astro-particle physics studies of dark energy and dark matter, study of astrophysical sources of high energy particles and neutrinos.

One crucial decision that will need to be made soon is how long to run the Tevatron. The report says to continue support "for the next one to two years", with two only in the optimistic scenario C. On the question of the ILC, the report describes "a wide range of opinion" in the HEP community and on the panel. Opinions about both of these may very well change over the next year depending on what happens at the LHC.

In both scenarios A and B, the report envisages cutting staff at the national labs, in favor of preserving support for research based at universities.

Other blogs posting about this here and here.

Update: Science magazine this week has two excellent articles by Adrian Cho, about the problems facing Fermilab, and the ILC.

Update: The final P5 report is here.

Comments

1. **wb**
   May 30, 2008

   RE: "The Energy Frontier: … a possible electron collider later."

   While the VGs of the report maintain a possibility for an ILC, they do by implication raise the distinct probability that the energy of the ILC is wrong and therefore the technology is wrong. If that is so one of the justifications for Project X as so late a date (compared with J-PARC) is cut away. Hence, it would have been helpful if the report specifically stated that research should be increased to determine if a consistent end-to-end concept for a muon collider is possible. Instead we have in Scenario C, "If another lepton collider technology is found to be preferable, its R&D would be advanced." One can certainly read support for muon collider R&D that into the words, but not necessarily. The words could just mean CLIC – no sure thing as a concept. In recommendations more explicit language would have been preferable.

   The report seems to make the future of Fermilab – as a single purpose HEP accelerator lab – contingent on a vigorous program at DUSEL. Otherwise and for the most part Fermilab seems to be reduced to playing a supporting role in HEP. Ah, yes, but it is the leading supporting role in the US. Perhaps that is what "maintain a leadership role in world-wide particle physics" means."

   The report of the report cannot be considered an endorsement of the strategic plan for Fermilab presented at the last HEPAP meeting. Particularly for Scenario A: "Constant level of effort at FY2008" the panel sidesteps the obvious but crucial question, "at that funding level does the US need a single purpose HEP laboratory?" I admit that were I on the panel I would also have ducked the question. Especially since the Panel admits that Scenario A "would sharply diminish the US capability in particle physics from its present leadership role;" the most likely scenario next year is inconsistent with the panel's primary recommendation, "maintain a leadership role in world-wide particle physics."

   To be sure one must wait to read the entire report before one understands all the implications. It is easy to be critical and not so easy to be constructive in so highly constrained a situation. The Panel was dealt a poor hand to play.

   My bottom line is that we need to make every effort to get results out of LHC as early as possible. Only compelling results from LHC are going to change a grim picture for accelerator based HEP.

2. **Peter Shor**
   May 30, 2008

   I don't believe the scientists looking at data from the LHC are going to rush things to try to get the next generation of particle accelerators scheduled sooner, nor do I believe that this is a particularly good idea. We'll just have to wait and see what these results are, and try somehow not to lose expertise in buliding particle accelerators in the meantime.

3. **wb**
   May 30, 2008

   My point about LHC was not about "rushing to Judgment" at the LHC, but about the 1) importance of a very strong US commitment to the LHC physics program, 2) the value of a faster rather than slower sociological process in the ATLAS and CMS collaborations. Investment choices may well have to be made on far less than discovery-level data for a vigorous accelerator-based program beyond LHC-1. Even preliminary hints as to energy scales is critical to decisions over the next 3 – 4 years concerning the suitability of the technology choice for a linear collider.

   There is no question that Europe and Asia will maintain a high level of expertise in accelerator science and technology throughout the next decade. The prospects for the

US in general and Fermilab in particular are not so sanguine.

4. **srp**
   May 31, 2008

   Relatively small amounts of money (but a multiple of what is spent today) could be allocated to advanced accelerator concepts to determine whether they were feasible. Before deciding about next-generation accelerators of conventional design (and hence no improvement in energy), common sense suggests first spending a little bit of money to determine if advanced machines could/should be built instead.

   If the answer from such investigations were yes, then it would be much easier to get the money for an operational machine because the jump in energy would at least offer the potential for fundamentally new results. Exciting new technology that opens up new frontiers is much more likely to attract support than giant installations that don't move the needle very much.

   If the answer about advanced concepts were no, then the sorts of incremental but hugely expensive projects currently under discussion could be put forward. I wouldn't be optimistic in this latter case, but at least the alternatives would have been explored and ruled out.

   The stubbornness (or lack of vision) of the community in obstinately putting forward these high cost/benefit incremental accelerator concepts, without seriously trying to get out of today's energy dead end, is very disappointing. It's almost as though people are more concerned with a steady flow of jobs than with maximizing the rate of discovery. But the public and its representatives are not likely to fund these conventional concepts–the scientific return per dollar invested just seems too low. Any new accelerator is going to have to make a compelling case that it can do much more than the LHC.

5. **Peter Shor**
   June 2, 2008

   If high-energy physicists try to make a snap decision on the energy level and usefulness of a followup to the LHC, spend a large amount of money on this followup, and then are proved to be wrong by further analysis of data from the LHC, I strongly suspect the damage to physics will be far greater than that incurred by waiting until definitive results are out from the LHC.

   I have no objections to spending relatively small amounts of money in a planning phase for the next accelerator.

World Science Festival

May 30, 2008
Categories: Uncategorized

Most of the time the attention paid here to efforts to popularize physics is restricted to grumpy complaints about the hype surrounding string theory as well as the more general dubious phenomenon of scientists promoting things that are more science fiction than science. Today I'm in a much more positive mood, and thought I'd take the opportunity to make some unusually sunny comments for a change.

One reason for this is that I attended the opening party for my colleague Brian Greene's World Science Festival Wednesday night at the American Museum of Natural History, and several people have told me about the enthusiastic reception the festival events have been getting. I was hoping to attend one of the events, but it was already sold out.

Things started off during the day Thursday with a World Science Summit here at Columbia featuring a speech by Mayor Bloomberg, and award of the new Kavli prizes. That evening, at the museum event, Brian, Fred Kavli and Senator Schumer all spoke, and the crowd was entertained by the choir of the Abyssinian Baptist Church.

Among the people I got a chance to talk to at the event were several string theorists. One of these was Jim Gates, who I had the pleasure of first meeting last year down in Orlando. He was there with his wife, just back from South Africa where he is involved with the African Institute for Mathematical Sciences.

Gates told me that his collection of video lectures with impressive graphics explaining quantum mechanics, general relativity and superstring theory called Superstring Theory: The DNA of Reality has been selling well, generating over a million dollars in sales, despite not being able to get it reviewed in major publications. Strong evidence of its popularity comes from the fact that if you google "The DNA of Reality" you get an impressive variety of sources for pirated versions. Evidently he has done an excellent job of reaching a wide audience with this material. From conversations with him I know that we're in closer agreement than you would guess, sharing an interest in the mathematics behind supersymmetry and a skepticism about extra dimensions.

I have mixed feelings about the highly enthusiastic promotion of certain speculative ideas about physics involved both here and in some of the World Science Festival events, but it's undeniable that these are reaching a lot of people and getting them excited about science. Perhaps I can convince Jim to market what could be the ideal package: his videos to get people excited and enthusiastic about the open problems in physics, and my book to give them some skepticism about the solutions now being promoted…

Update: For an article describing what happened at the World Science Festival program about unification and string theory, see here.

Comments

1. **The Vlad**
   May 30, 2008

   Dear Peter,

   An excellent attempt at a sunny disposition, though perhaps not an entirely successful one 🙂

   Your post leads me to ask the following question: Do you think that your critical viewpoint has acted to promote the profile of physics in the public eye?

   I am not a physicist by training, and although I cannot follow many of the more technical discussions here, I would say that I have certainly learned much from your posts and the discussions in the comments sections. Thus, to the extent that your blog has educational value to the layperson, I would suggest that you are doing physics a promotional service. Skepticism of extant viewpoints, even dogmatically asserted ones lacking experimental support, is not necessarily bad from a public-relations angle.

   Which brings me to a second question: When does 'healthy skepticism' cross the line into 'irresponsible criticality'?

   A senior colleague of mine recently suggested to me that another senior colleague, whose reputation for skepticism borders on the extreme, was irresponsible and therefore lacked 'leadership qualities'. Yet to my view, failing to speak out against false or misleading scientific claims is also an act of irresponsibility (by omission).

   I would love to your view and those of your readers.

   The Vlad

2. **Changcho**
   May 30, 2008

   With regards to doing Physics (or science in general): one needs to find the right balance between 'wonder' and 'skepticism', as Sagan often wrote. I suppose the right allocation between the two would be different for different fields, or even different problems in the same field.

3. **Peter Woit**
   May 30, 2008

   Vlad,

   I don't really think that "the profile of physics in the public eye" is something particularly well-defined or of a simple signficance. There's lots of different kinds of physics, "the public" is all sorts of different people, and what is ultimately desirable is not that all people have a high opinion of everything physicists are doing. What's important is that some talented people get seriously enough interested in it to pursue it as a career, that lots of people get some appreciation for what we know about the physical world, and that there's sufficient support among the public for good physics research.

   It matters a great deal which research areas get into the public eye and thus are more likely to get public support. As an example, if the campaign to promote multiverse studies is successful, there's a danger that real science will get crowded out, both in terms of funding and attracting good young people. Physicists need to promote their science, but they need to do so honestly, and also to do what they can to stop misleading and self-interested promotion of unsuccessful research programs.

   As far as the whole string theory controversy of recent years goes, I think string theorists have made huge mistakes in how they have dealt with it, and this has seriously damaged both their standing in the physics community and the public perception of their field. The smart thing to do would have been to acknowledge the existence of the serious problems being pointed out by their critics and to make the case to the public that this kind of very real scientific controversy is a healthy part of science, evidence of how little we know about some topics, and a good reason for young people to go into the subject, since some of it is still wide-open. Instead the typical attitude has been to remain quiet, trying to ignore criticism in the hope that it will go away, leaving the public response to extremists who do a very poor job of representing them.

4. **Bill**
May 31, 2008

Now this is hilarious Peter!

You want to ride the coattail of a string theorist promoting science including string theory to sell more of your books and thus generate more revenue. Truly bizarre.

Best,
Bill

5. **Peter Woit**
June 1, 2008

Bill,

The comment about packaging my book with the videos was a joke, actually. Believe it or not, the book was not written to make money. It was written to explain a different point of view on the currrent state of particle physics. I'm rather proud of it, and, yes, like the idea of more people reading it.

Besides yours, this posting also generated other anonymous comments of pure personal abuse from string theory partisans, which I've deleted. Weird that the most nastiness I've had to contend with in a while was triggered by my saying something positive about the efforts of some string theorists.

6. **Bill**
June 1, 2008

Peter,

Make no mistake, the "nastiness" was not triggered by your saying something positive about the efforts of some string theorists. Rather, the observation was made that you intend to sell more books not by convincing people that your book is worth reading but by packaging it together with a book/video/lecture/etc of a string theorist. Joke, or not, this is truly bizarre.

Why? Because one of your main issues is the public perception of string theory and the "hype" that surrounds it. This "hype" is not a scientific statement, it's related to it's public perception. Which is fine, you don't have to have a scientific point, you can be a popular science journalist and you can criticize issues from that perspective. But then coming along with a "joke" that you think it would be useful to package your book with a hugely successful string theory "hype" publication makes one wonder that it's really no surprise people don't take you seriously.

Bill
(Just to avoid confusion, I'm a nuclear physicist, and yes, I'm tenured and yes, I have over 1500 citations. All of this is completely irrelevant, however, you like to picture any critique of yours as "string theory partisan".)

7. **Peter Woit**
June 1, 2008

Bill,

My criticisms of string theory hype have always been from the point of view of a scientist, addressed to the ways that science is being misused and misrepresented and I intend to keep on pursuing those. But I think it's also important to recognize that many of the people writing enthusiastically about string theory are doing so in the larger context of an attempt to get the public interested in and excited about modern physics. This ends up producing what seems to me a mixed bag, and I've often criticized the problematic parts of it, but there are also valuable parts, and I wanted to acknowledge those. Lots of people have gotten interested in physics and learned something about it from Brian and Jim's efforts. Many may also have acquired an unreasonably optimistic view of string theory, but the other side of the coin has also been getting quite a bit of attention recently.

It was definitely a joke to suggest a joint marketing effort; given the hostility of the "string wars" I found the notion amusing although many may not share my sense of humor. The serious part of the suggestion was that I think that it wouldn't be a bad thing at all for people to be exposed to both a pro-string effort and my book. This has nothing to do with a desire to make money selling books (the book was intended to be published by a university press and addressed to a relatively narrow audience, I don't need the money).

8. **curious one**
June 1, 2008

"I think that it wouldn't be a bad thing at all for people to be exposed to both a pro-string effort and my book…." – Peter Woit

Yes, if people actually remember to read it.

9. **somebody**
June 1, 2008

Frank said:

>I'm a QCD person, not a 'string partisan', and it is hard to
> disagree with Bill. Peter's suggestion involving his book
>and Jim Gates' DVD is one of the reasons why so many
>people consider Peter a parasite.
>
>In the 'mixed bag' of the 'hype', what are the 'problematic
>parts'? Let me guess. They're the parts that don't allow Peter
>to feel important and to earn a few bucks.

Being a string theorist, I have found a lot of people who are in the hate-string-THEORY camp because of the string THEORISTS' arrogance or know-it-all attitude. I have found VERY few critics of string theory whose views have not been corrupted by this aspect. Incidentally, I think that this arrogance is a very real phenomenon, even if I believe it is beside the scientific issue. I would have given you the benefit of the doubt if you said Peter was one of them who hated string theory because of the string theorists, even though I have no idea whether it is true.

But to say that he is after money, seems ridiculous to me. I think a real and honest critic of science should really walk the middle ground and we should congratulate Peter for making such an effort in this post. I personally would find a post like this far more refreshing than an unqualified rant.

10. **Matthew Putman**
June 1, 2008

peter,

I am a patron of the festival, so am a bit biased as to its merits. Still the entire thing went even better than i had imagined. i am both a scientist and involved in the arts, and while in sciencific circles we tend to debate the merits of theories, outside of science the worlds of art and science dont really know how to communicate at all with each other. This event gave me hope in a different way than an invidual project would give me, and instead gave me a chance to be myself in art and science. I give Brian and Tracy a lot of credit for this, but also you, and others who have differed in regards to string theory, for attending, and embracing the romantic notion of an event like this. This is one of the reasonsons it worked so well.

Matthew Putman

11. **Peter Woit**
June 1, 2008

My apologies to the string theory partisans of the world for making unfair accusations…

Matthew,

Glad to hear you also found the festival to be a success!

somebody,

Thanks for your comments. My problems with string theory don't have much to do with string theorists personally. Some of my best friends are string theorists… and the theorist I have the highest opinion of professionally and personally (Witten) is also probably the one most responsible for promoting the subject to his colleagues. One interesting thing I learned after getting involved in this controversy is that the sheer complexity and difficulty of string theory is a large part of the story. That has made it very hard for people to sensibly evaluate it, and that's behind some of the bizarre behavior surrounding the "string wars".

12. **somebody**
June 2, 2008

> the 'arrogance' accusations are frequently raised against all
> of science, not just string theory, by those who don't
> understand it.

I was saying that against string theory there is certainly anatgonism of this kind even within the scientific community. Here is a view from the other side:

http://cosmicvariance.com/2005/11/18/a-particle-physicists-perspective/

I think her claim that string theorists are arrogant is true. But I also think she is upset only because she is at the receving end of it. I am pretty sure that she seems equally arrogant to somebody doing plasma numerics. There is, and has always been, a heirarchy of "fundamentalness" in physics. The reason why string theory is strange is because it claims to be the most fundamental thing, stakes a claim on all the glory, while not (yet) producing a single new experimental test. We string theorists claim that it is because quantum gravity is inherently difficult to test, and that we need more time, but that hasn't stopped people from getting pissed.

Please note that I am not endorsing the arrogance of string theorists – but I feel that this specific piece of sociology has been a huge reason why the anti-string wave has been so powerful. I think the scientific case is much weaker than the popular reaction, Peter of course might disagree.

> the 'arrogance' accusations are frequently raised against all
> of science, not just string theory, by those who don't
> understand it.
>If there is a difference, could you please
> enlighten me about this difference?

As has been already pointed out by others, string theory is perhaps more complicated than other subjects. At the very least it is certainly BIGGER than the other subjects.

But I think the real reason for the arrogance is that string theory forces you to know both QFT and general relativity really well, and between the two it exhausts most of the juicier aspects of theoretical physics – from critical phenomena to particle physics to the big bang and black holes. Then there is the fact that string theory has a lot of geometric tools usually unncessary in other physics. There is a very real sense in which a COMPETENT string theorist has to be a jack of all trades.

Arrogance or the perception of arrogance happens when you can understand THEM relatively quickly, while they need lot of new tools to undertstand YOU. Once again, I am not trying to defend the phenomenon, just trying to explain it.

Of course, there is also a kind of arrogance that comes from being associated to a field of work where you are basking in the reflected glory of people like Witten, but I think this applies mostly only to the not-so-competent string theorists. I was talking about the more serious kind of arrogance, the one that stems from true vanity. I also want to emphasize that I have known some exceptionally brilliant and creative guys in this field who are also extremely modest. The ratio of the jerks to the nice guys is probably the same in string theory as it is in any field.

13. **Peter Orland**
June 2, 2008

This thread seems off-topic, but after Somebody's last comment, I feel I have to say something.

"But I think the real reason for the arrogance is that string theory forces you to know both QFT and general relativity really well, and between the two it exhausts most of the juicier aspects of theoretical physics – from critical phenomena to particle physics to the big bang and black holes. Then there is the fact that string theory has a lot of geometric tools usually unncessary in other physics. There is a very real sense in which a COMPETENT string theorist has to be a jack of all trades."

Actually, any good theoretical physicist should try to know at least a little about all these things! Add the Hubbard model, superconductivity, integrability,…. It is important to keep learning things.

I have never been frustrated by anybody's arrogance. Arrogance is par for the course. What bothers me is that so many people are working on problems where there is no specific goal in mind. You've solved a problem at three loops, so do four. You have one formulation of something, so find another. Much of this activity is pure mountain climbing and teaches us nothing. This is very different from finding a new way to understand a well-formulated problem, even if it is only a toy model (like N=4 Yang-Mills or 1+1-dimensional theories). For the past several years, almost every seminar speaker I see doesn't really have anything to say. I want to stress that it isn't only string theorists who are guilty of this.

For some reason, people feel they can get away with solving problems with no motivation other than someone else is doing it. It's important to have a message in your work.

If you don't like what I've said, call me arrogant.

14. **Peter Orland**
June 2, 2008

I should correct my next-to-last paragraph. It should read:

For some reason, people feel they can get away with formal things with no motivation other than someone else is doing them. It's important to have a message in your work.

15. **somebody**
June 2, 2008

> I have never been frustrated by anybody's arrogance. Arrogance is par for
> the course.

It is precisely attitudes like this that has left the string theory community with few friends these days. Instead of recognizing arrogance as a form of insecurity, there is a certain glorification of it in the community. Now it has come back to bite us in the behind. It is one thing to be vehement and passionate about what you think, and that can sometimes come across as arrogant. I must admit that sometimes I am massively guilty of that myself. But some of the more visible string theory blogs are very clearly conscious attempts at shushing the opposition through condescension and disrespect.

The collateral damage is that now we are slowly left with no friends and no funding. You just need to take a look at the hiring patterns last year to see the truth of this statement. You cannot treat everybody else like cattle and then expect them to see how good string theory is.

16. **Peter Shor**
June 2, 2008

Peter Orland says "Much of this activity is pure mountain climbing and teaches us nothing. … For some reason, people feel they can get away with formal things with no motivation other than someone else is doing them. It's important to have a message in your work."

This is, of course, what all of mathematics looks like, at least at first glance. After you learn more about a specific mathematical field, you realize that many of the better mathematicians in it are actually pursuing some kind of directed program that is moving forward, while others are just solving random problems and accumulating publications.

String theorists have now lost the guidance of experiment, so they are doing something much closer to pure mathematics without having been exposed much to the mathematical culture. I can't say for sure whether this culture helps mathematicians make progress in such a vague and fuzzy environment, but I suspect that it does.

17. **Peter Woit**
June 2, 2008

This thread has wandered from the topic, partly due to the anonymous insulting comments from "Frank". I've deleted those comments, but mostly left those responding to him.

If people want to continue this discussion, that's fine, but keep in mind that posting insulting comments anonymously is out of bounds here. Don't do this and don't respond to those who do.

Note: The original version of this comment mentioned that I suspected that "Frank" was someone who used to post here as "amused". He assures me that this was not the case, so the comment has been edited, with my apologies to him.

18. **somebody**
June 2, 2008

> This is, of course, what all of mathematics looks like, at least at first glance.
> After you learn more about a specific mathematical field, you realize that many
> of the better mathematicians in it are actually pursuing some kind of directed
> program that is moving forward, …

This I think is an excellent characterization of the situation in string theory. All the fad, for example, are about minor breakthroughs that take us a tiny bit closer to a better understanding/control of the theory – even if they seem pointless to an outsider. In some grandiose sense, they are indeed pointless, because they are minor stepping stones. But we cannot do without (most of) these stepping stones on our way. Most serious people do understand the mission statement, and even though progress is slow, it has direction. Unfortunately, it really does seem that even though the physical picture has a lot of coherence to it, the technical and conceptual problems that need to be solved before we can wield string theory with sufficient comfort (so as to be useful for making predictions for low energy physics) are many and difficult. The hope is that they would not turn out to be insurmountable, and that they would not lead to an asymptotic slowdown of the field. I am not worried about the latter yet because we have been making progress in fits and starts so far (even though these last couple of years were admittedly a bit dull).

19. **Peter Orland**
June 2, 2008

Somebody,

I'm not a total outsider, so that's not why these activities seem pointless, at least not to me. I know enough string theory to see that most of what string theorists are doing really doesn't have a point (and this is also true in other areas of physics, as I said above).

Most people are just refining calculations of models or playing with formalism. They are doing technical things with no motivation for what they are doing. I'm absolutely sure of this, because I sometimes ask WHY THE HELL ARE YOU DOING THIS? (but more quietly and politely) and get no answer.

So what should they do? Well, this works for me: when I can't get further with some line of research, I'll usually work on something else for a while.

20. **somebody**

June 2, 2008

> I know enough string theory to see that most of what string theorists are
> doing really doesn't have a point (and this is also true in other areas of
> physics, as I said above).

So you are essentially saying that most of the scientific research that goes on in most fields is pointless. I agree that bad research is FAR more common than good research. But that said, I still don't see your point. I think your view amounts to looking at the bad research going on in string theory and related fields and vaguely IMPLYING that that is essentially ALL that is going on. If thats the case, I disagree.

Even today on the hep-th arXiv, there are some interesting papers. Lets look at the first two papers. I would say they are immediately interesting to any theoretical physicist. I find it interesting that there is a dual gravitational description for the quantum hall effect. The second paper talks about the emergence of geometry from doing matrix model quantum field theory. I think these are interesting results, even if they are not making predictions about low energy physics. I think an understanding of the relations between the mathematical structures in theoretical physics is essential to see beyond the technical complexities, so even if not immediately, these papers are useful to my mind. They are not revolutionary, but they are certainly doing good science – which is to push forward during the interval between two successive revolutions.

If these works seem pointless to you, I don't know what wouldn't. In fact, at the risk of sounding arrogant, it would make me suspect your competence in string theory. Because the only systematic thing we know in science is to push forward. Paradigm shifts do not appear on demand.

If you were saying that MANY of the papers that appeared on the arXIv today are crap, I would agree with you. But I would still think that you are missing the point. I have looked occasionally at research in condensed matter, astrophysics/cosmology and quantum computing, and I have never felt that the ratio of quality papers is significantly less in string theory. I HAVE felt that the progress in string theory could be a little faster, but thats because I know exactly what I would LIKE to do with string theory if I had all the necessary tools, whereas in other fields the future is much more open.

21. **Peter Woit**
June 2, 2008

somebody,

My problem with string theory boils down to the ongoing problem of a refusal to evaluate what has failed and what still has promise. Sure, as in the examples you give, string theory provides interesting models for strongly coupled QFTs, and for quantum gravitational effects, and these are, among many other possible things to work on, worth looking at. But the idea of a 10/11d superstring as a model of particle physics has failed miserably, and the unwillingness to acknowledge this has created huge problems for the field, including sending parts of it down the multiverse blind alley. It increasingly seems like a waste of breath to keep pointing this out, as most have decided to ignore the problem in hopes that the LHC will make it go away. We'll see over the next few years…

I very much agree with Peter Shor that physicists should look carefully at the culture in mathematics and try and understand how it has continued to allow progress, in a context of no experiments to decide things, and of ideas that are often so abstract, demanding and understood by very few people as to make string theory look easy. It's a fascinating and not well-understood question how this culture has done as well as it has.

22. **somebody**
June 2, 2008

"But the idea of a 10/11d superstring as a model of particle physics has failed miserably, and the unwillingness to acknowledge this has created huge problems for the field, including sending parts of it down the multiverse blind alley. It increasingly seems like a waste of breath to keep pointing this out, as most have decided to ignore the problem in hopes that the LHC will make it go away. We'll see over the next few years…"

Peter, You have discussed these things many times here, so I will not start with the rehashing of what I believe are the counter-arguments. Lets just agree to disagree.

23. **Peter Orland**
June 2, 2008

I am not complaining about bad research per se. Nor am I complaing about string theory specifically (though people don't always agree as to what string theory is. At Perimeter, a talk I gave was a String Theory talk. At Brookhaven the same talk was a Nuclear Physics seminar). What I am complaining about a lack of direction, the absence of passion. People are giving talks and writing papers, but all they care about is getting their work cited. The content seems secondary.

Passion is important. Give me a bad paper, in which the author tries to solve a real problem, over a pointless correct paper any day. At least I might learn something by reading it.

Next time you are in a seminar, ask yourself what you are getting from it. Is the speaker telling you something really new about Nature/the model/the concept? Is a number calculated? Is there some new intuition about some field? Or maybe the speaker tells you that his/her research program will do one of the above? Well, in ninety percent of the talks I have been to, for more than a few years, the answer is NO.

I suppose I have now alienated everybody and destroyed my chances of future funding and invitations to speak. I better get used to my new status as a pariah. I don't know if it was worth it.

24. **Peter Woit**
June 2, 2008

"amused" requests that I add a new comment here pointing to the note I added to this comment

http://www.math.columbia.edu/~woit/wordpress/?p=696#comment-38786

to the effect that the anonymous commenter "Frank" was not him. I'm happy to oblige.

25. **Eric H**
June 3, 2008

I think its great to encourage more people to get involved in science professions and especially in the physics profession. But my personal experience, which may be unique for all I know, is that physics is very myopic in its criteria for selecting people to encourage. People like me, who are very visually oriented, can tell in a minute that a dimension you can't visualize has no meaning. Symbolic meaning in the absence of a physical correlation seems just silly to many of us. But we get discouraged from even entering the physics field because of our unexceptional mathematical ability. People that have brains that are more visually oriented can be very useful in physics.

I would say that Einstein's greatest gift was in his visual acuity and intuition and certainly not his mathematical ability. But he was very unusual in that he had "sufficient" mathematical ability to translate his visual approach into mathematics. That is very rare. People like me who feel we have something to contribute will still be shunted aside. So what is the point of putting on the World Science Festival to get people interested if the sorting method for encourageing people doesn't change to become more inclusive?

26. **Ethan Siegel**
    June 5, 2008

    An inspiring read from your colleague Brian Greene made it into the New York Times this past week…

    …and it's about the power of Science to inspire. How do we manage, while still keeping everyone in the scientific community honest about what we know, what we think we know, and what's still speculative, to be unified in our approach to raising awareness of this beautiful enterprise?

27. **Bruce Bartlett**
    June 9, 2008

    Hooray for the African Institute for Mathematical Sciences! It's great to see it make an appearance on this blog. Along with the Stellenbosch Institute for Advanced Study, there are some interesting things going on in theoretical physics in Southern Africa. In fact, blogs are the great leveller – they give an opportunity for those who have traditionally been on the sidelines to become a part of the exciting day-to-day issues and debates.

Experimental Predictions From F-Theory

June 2, 2008
Categories: Uncategorized

I've been known to claim that string theory makes no experimental predictions, so this evening thought I better take a look at a preprint that just appeared entitled [GUTs and Exceptional Branes in F-theory – II: Experimental Predictions](). The abstract claims that to have found "a surprisingly predictive framework".

This paper is 200 pages long, and a companion to part I, which was 121 pages. For part I, there's a [posting by Jacques Distler]() that explains a bit of the very complicated algebraic geometry going on. Making one's way carefully through the entire 200 pages of the new paper looks like a very time-consuming project, so I thought I better start by identifying what the experimental predictions are. These days, one expects experimental predictions to say something about LHC physics, but I don't see anything about that in the paper. Perhaps this is because, except for some comments in section 16, it appears that the authors are studying a model with exact supersymmetry.

Looking at the introduction and conclusion sections of the paper, the only predictions I can see are for neutrino masses, and there are two of them. Either $.5 \times 10^{-2 \pm .5}$ eV or $2 \times 10^{-1 \pm 1.5}$ eV is given for the neutrino mass, with the error bars just those due to an unknown value of one of the geometrical parameters involved. There's no mention of which neutrino is being discussed, and as far as I can tell this is just an order of magnitude estimate of the neutrino mass scale, one which the author's describe as "somewhat naive", noting that "factors of 2 and π are typically beyond the level of precision which we can reliably estimate". It's unclear to me whether or not other mechanisms giving quite different neutrino masses would also fit into the author's model.

Maybe I'm missing something and an expert can help me out, but I'm not seeing anything here of the sort one would normally describe as an experimental prediction. There's certainly nothing falsifiable at all about the model, since one knows from limits on neutrino masses and measurements of oscillations that the neutrino mass scale has to very roughly be in this kind of range. Furthermore, again maybe I'm missing something, but I don't see any way in which more detailed calculation in this framework can make it any more predictive.

Update: Lubos has a detailed [posting]() about this, and from reading it, it doesn't appear that the paper has experimental predictions that I missed. I do wonder what a "musculus maximus" is…

Update: For more about this, see presentations at PASCOS 08 [here]() and [here](). The first describes this as "a modest step" in the direction of predictions, the second doesn't mention predictions at all.

Comments

1. **GR**
   June 2, 2008

   I'm a physics undergraduate who's interested in this sort of thingl… if I wanted to decide things for myself, where could I start?

   Obviously it takes many years of hard work to develop the math skills (not to mention vocabulary), but is there a reasonable introduction so I can start to understand the crazy things people talk about these days in HEP theory?

2. **GR**
   June 2, 2008

   Also, and hopefully not as off-topic as the above, the old joke comes to mind:

   Engineers are happy to get an answer to a few decimal points.
   Physicists are happy to get an answer to an order of magnitude.
   Astrophysicists are happy to get an answer to an order of magnitude in the exponent…

   Can we substitute string theorists into the punchline now?

3. **Peter Woit**
   June 2, 2008

   GR,

   Well, I wrote a whole book aimed at you, and it contains lots of recommendations for further reading.

   I don't recommend trying to understand what is going on in this kind of paper. It involves a huge amount of technical background, in both math and physics, and by the time you would be able to absorb it, LHC results almost certainly will have made it irrelevant. Study quantum field theory. It's a huge subject, and will be there in the future no matter what.

4. **Luboš Motl**
   June 3, 2008

   Dear GR, a standard textbook that introduces students to modern geometry, algebraic geometry, and similar subjects that are needed in the F-theory model building is one by Nakahara.

   Click my name, go to my blog, and a link to the amazon.com page of the book is included in the article.

   Unless you want to become a complete idiot, I recommend you to pay no attention to Peter Woit and his blog. He is just an aggressive crackpot who tries to revenge to physics for the fact that he is unable to do it himself.

5. **Gil Kalai**
   June 3, 2008

   Hi Peter, this post is related to quite a basic question in the evaluation of scientific theories, (and also related to basic issues in statistics.) According to all major philosophy of science theories, e.g., both the Popperian falsification approach and the Bayesian verification point of view, scientific theories should be judged based on their ability to predict and not based on their ability to explain known observations. Still in reality, most scientific theories were created in order to explain known observations. Predictions as well as empirical proofs based on fresh observations came much later. Trying to connect a theory to what "one already knows" while falling short of an “experimental prediction” can be of interest.

6. **Sara**
June 3, 2008

“a complete idiot”? Luckily you are an intelligent person and a very very well-brought-up man.

7. **Peter Woit**
June 3, 2008

Gil,

Sure. I’m just trying to figure out why the authors put “experimental predictions” in the title of this paper.

8. **M**
June 3, 2008

do they predict that neutrino masses are of Majorana or of Dirac type?

9. **Peter Woit**
June 3, 2008

M,

As far as I can tell they predict both, with one of their mass ranges a Dirac prediction the other Majorana.

10. **Mark**
June 3, 2008

A somewhat related thought: In the mathematics literature if you want to find out more about what the main results of a paper are you can try its mathscinet review.

Is there any kind of broad (online) database for what various physics papers predict from the outcome of proposed, as yet unconducted, experiments?

(Perhaps I should have posted this in the comments for the INSPIRE post?)

11. **Garrett**
June 3, 2008

Hi Peter,
I’ve been sitting in on some of the PASCOS 08 talks here at the Perimeter Institute this week. Cumrun Vafa gave a talk yesterday, which Perimeter already has up here:

http://www.pirsa.org/08060030/

12. **Peter Woit**
June 3, 2008

Thanks Garrett,

I see that in slides introducing his talk, Vafa agrees that string theory does not make “a verifiable quantitative prediction for the real world”, and that this work is only “a modest step in that direction.”

13. **Garbage**
June 3, 2008

I haven’t read the paper, but for one thing…it is a hep-th

Regarding SUSY, they say in the abstract:

“Communicating supersymmetry breaking to the MSSM can be elegantly realized through gauge mediation. In one scenario, the same repulsion mechanism also leads to messenger masses which are naturally much lighter than the GUT scale.”

So they do address SUSY breaking…

14. **Peter Woit**
June 3, 2008

Garbage,

If you do take a look at the paper, you’ll see that my remark that only in one section (16) do they address supersymmetry breaking, and then basically just to say that they could break supersymmetry if they wanted to.

So, I guess the idea is that on hep-th, the term “experimental prediction” means something different than elsewhere?

15. **Sanjay**
June 3, 2008

Dear GR,

In addition to Nakahara’s book, I would also recommend Tu’s Introduction to Manifolds followed by Bott and Tu’s Differential Forms in Algebraic Topology. All three are excellent and Nakahara gives a very physical picture of the mathematics. As for QFT, there is a whole range of books, but for a solid introduction look at both Zee’s Quantum Field Theory in a nutshell and Srednicki’s new QFT book. Also, Tom Banks’ new QFT book should be coming out soon.

http://www.cambridge.org/uk/catalogue/catalogue.asp?isbn=9780521850827

Good luck with your studies!

16. **Alex Mikunov**
June 3, 2008

Lubos has to stop creating paradoxes (e.g. "…I recommend you to pay no attention to Peter Woit and his blog…" while posting on the P.W.'s blog 🙂 and get back to M-theory (or whatever currently its name is). Otherwise he's really boring

17. **Peter Orland**
June 4, 2008

Dear GR,

I fear you are being led down the garden path (or whatever the hell the metaphor is) by my esteemed friends and colleagues.

If you really want to learn about theoretical particle physics (or theoretical condensed matter physics), learn quantum field theory. This means functional methods, scattering amplitudes, renormalization
and critical phenomena.

You will have to read and understand a good graduate text on quantum mechanics first. General relativity (your initials) is also useful and much easier to learn than QFT. Algebraic geometry, algebraic topology, differential topology, …. are nice, and some advanced concepts in QFT, require knowing these subjects. But they are just pleasant diversions until you know basic QFT.

18. **somebody**
June 4, 2008

While we are all giving advice: I would second Peter O.'s recommendation. Don't focus on the math. The math will come, when you need it. Same is true about GR. Learn QFT. By that I mean know everything inside Srednicki's book. Which in my opinion is the best book out there. Don't get distracted by too many books if you can help it, but this is not always easy. If you can work through Srednicki cover to cover, you are already pretty powerful. By the time you finish that, hopefully you will know what to do next yourself.

19. **Sam**
June 4, 2008

Peter, you will be pleased to learn that musculus maximus is defined at:

http://cancerweb.ncl.ac.uk/cgi-bin/omd?musculus+gluteus+maximus

I'm sure you're not shocked… 😉

20. **Aki**
June 6, 2008

"I do wonder what a "musculus maximus " is…"

Supersymmetry in human body, i guess 😉

21. **Indrajeet**
June 10, 2008

You have not written anything about the recent work of Sunil Mukhi and co. from TIFR which has "led to a mini-revolution in the theory of membranes".

22. **Brini**
June 11, 2008

Indrajeet,

this is not too much of a surprise. Woit, who hastened to write after attending a talk by Berkovits, that

"it would be pretty damn funny if it turns out that multi-loop superstring amplitudes aren't finite"

and who started an infinite blogwar against Distler on string perturbation theory a couple of years ago, has completely overlooked the recent papers by Grushevsky, Cacciatori et al on genus 4 amplitudes. He probably hasn't found them "funny" enough 🙂

Personally, I just find it more and more difficult to consider fair such a blind, biased and emotional criticism of string theory, let alone to see its scientific foundations – if any.

23. **Thomas Larsson**
June 11, 2008

Indrajeet,
You might want to have alook at this and that posts by Jacques Distler. Punchline: "That was fun while it lasted".

The Nambu bracket is a kind of Jacobiator. I don't associate with Jacobiators.

24. **Urs Schreiber**
June 11, 2008

> The Nambu bracket is a kind of Jacobiator.

I tried to make sense of a statement along these lines but was not really happy with what I got. Do you know more? I'd be interested.

25. **Peter Woit**
June 11, 2008

Indrajeet and Brini,

I keep a list on my desk of topics I'd like to write about, but haven't gotten around to for one reason or another.

One item on the list is the activity surrounding the so-called Bagger-Lampert model, a new 3d superconformal model. I haven't written about this because I don't know much about it. I haven't spent time learning more partly because I recently asked an extremely distinguished expert on the topic about what one could do with it, and he told me he wasn't aware of any really significant implications. Maybe I misunderstood him and am misrepresenting his views, but this seemed like a good time to wait a bit and see what emerges from all the current activity. Thomas's link to the latest Distler posting shows why following one piece of this, the Mukhi et. al. work, might not have been worth the time. For now, I think I'll stick to my decision to wait and see what comes out of this, if there is something really significant, many review articles will be appearing later this year. I'd of course be happy to hear from anyone who can explain more about the significance of Bagger-Lampert.

The work of Grushevsky et. al on multi-loop amplitudes is also on the list, awaiting my finding time to consult with experts and better understand its significance. My understanding right now is that a conjectured form for 3 and 4 loop amplitudes has been found that satisfies the various stringent consistency conditions one expects, but that this is not an actual derivation from the definition, but I may have this wrong.

Previous arguments on this blog were about the question of whether a proof of finiteness of super-string theory amplitudes exists. I've always said that the situation is that it is likely that these amplitudes are finite to all loops, but that this remains to be shown. People are still working on the Berkovits pure-spinor formalism, but as far as I'm aware, it still does not give a proof at higher loops. The work of Grushevsky et. al. certainly pushes the subject forward and removes some possible places a problem could turn up, but as far as I know, it also does not provide a finiteness proof.

26. **Thomas Larsson**
June 11, 2008

According to the appendix of arXiv:0712.3738v2, Andreas Gustafsson has shown that a 3-algebra is almost equivalent to a Z_2-graded Lie algebra (not a superalgebra). However, not quite, because the odd-odd-odd Jacobi identity is replaced by the "associative condition", which says that the sum of two of the terms in the Jacobi equals zero. The tri-linear product is then the third term in the would-be Jacobi identity, and thus a Jacobiator.

Hm. Or so I thought. Looking at eqns (47) and (49) again, it looks like I got some signs wrong.

Sorry if this comment strays too far from the topic of the post.

27. **Urs Schreiber**
June 11, 2008

> Looking at eqns (47) and (49)

I read (48) as saying that the Jacobi identity holds everywhere except when you feed in three elements of curly A. On them it just plain fails. Not up to some Jacobiator.

28. **Observer**
June 16, 2008

Peter,

The only reliable data on neutrino masses are the mass squared differences for solar and atmospheric neutrinos. Does the prediction of F-theory match these measurements?

29. **Peter Woit**
June 16, 2008

Observer,

I don't see a real prediction in the paper, and the authors don't compare the numbers they quote to experiment, possibly since there is no way to do this. The vague neutrino mass scale in the paper is not inconsistent with what is known about neutrinos (e.g. mass differences), if it were the authors wouldn't have pursued this.

Two Unrelated Topics

June 3, 2008
Categories: Multiverse Mania

I was planning on writing something about the field with one element, but Lieven Le Bruyn has done a better job of it than I would have, linking to all of the recent news on this subject I was aware of, and more.

Today's New York Times has an article entitled Dark, Perhaps Forever, which is summarized as "Scientists are beginning to despair of explaining the universe". It is about the recent dark energy symposium in Baltimore, and focuses on Witten's talk, which was discussed previously here. To an account of the talk itself, it adds this quote from Witten:

> As for how I feel personally, I am not sure what to say… I wasn't terribly enthusiastic the first, or even second, time I heard the proposal of a multiverse. But none of us were consulted when the universe was created.

There's no mention of the crucial issue that Rachel Bean implicitly confronted Witten with in a question at the end of his talk: if the landscape inherently can give no testable insight into physics, why should a scientist bother with it?

Other speakers at the symposium discussed possible future experiments to measure dark energy and their funding prospects. One worry is that such experiments may do little more than give a somewhat more accurate dark energy number, providing no further insight into the problem of its origin.

Comments

1. **Ethan Siegel**
   June 3, 2008

   > One worry is that such experiments may do little more than give a somewhat more accurate dark energy number, providing no further insight into the problem of its origin.

   Why do you say this is a worry? Unless they find strong evidence that dark energy is inconsistent with a cosmological constant, this is a *given*. NASA's upcoming joint dark energy mission (either SNAP, DESTINY, or ADEPT) is banking on finding something interesting, where interesting means the equation of state (w) can be determined to be something other than a cosmological constant (w=-1, with no change in w over time). It's terribly boring, but there's really no other option other than to look for this.

   Either way, we still have no idea how to account for either a cosmological constant or something different from a cosmological constant.

2. **Peter Woit**
   June 3, 2008

   Ethan,

   I was just summarizing the remarks of Steinhardt and Krauss at the end of the article. In any case, everyone agrees that there's not much to do except go do the experiment and see.

3. **John**
   June 3, 2008

   Wiltshire proposes an alternative to dark energy in arxiv.org/abs/0712.3984 , namely the interpretation that it is a measurment artefact due to the inhomogeneity of the universe. When integrated over 13700 million years, the inhomogeneity yields acceleration and all other modern observations. What has to be thought about this sort of work?

   John

4. **John Baez**
   June 4, 2008

   I wish more people understood math. Then they'd realize what the really cool part of this blog entry was!

   Thanks for the link, Peter – I hadn't seen it.

5. **Herman**
   June 4, 2008

   350 scientific instruments now on line:
   http://instrumentenzaal.teylersmuseum.nl/

6. **Peter Woit**
   June 4, 2008

   Following up on John's comment, I should mention that one of the best links to follow from Lieven's posting is this one:

   http://math.ucr.edu/home/baez/week259.html

7. **Professor R**
   June 4, 2008

   Hi Peter, this is off-topic but relevant to your campaign against hype in general, you might consider a post on it.

   The word over here in Europe is that the cold fusion controversy is back – yet more headlines on the fantastic promise of cheap energy, this time backed by an experiment at Osaka University.

   I have a posting on my blog, or you can red the original story at http://physicsworld.com/blog/2008/05/coldfusion_demonstration_a_suc.html

Cormac

8. **Peter Woit**
June 4, 2008

Cormac,

On the list of things I would like to ignore, and definitely don't want to start a discussion about here, cold fusion is right up there…

9. **Professor R**
June 4, 2008

Oops, sorry! Just wondered if you guys had heard..

10. **Ed N**
June 4, 2008

>Wiltshire proposes an alternative to dark energy in
>arxiv.org/abs/0712.3984 , namely the interpretation that it is a
>measurment artefact due to the inhomogeneity of the universe.

Edward "Rocky" Kolb of Fermilab made a similiar proposal in 2005.

http://arxiv.org/abs/astro-ph/0506534

11. **Shantanu**
June 4, 2008

Peter what do you think of Susskind's talk at the PASCOS meeting?
There area also some other string theory related talks.

12. **Peter Woit**
June 5, 2008

Shantanu,

I took a look at the Susskind slides. I still don't see any predictions of anything coming out of the multiverse scenario he is promoting.

13. **vicar**
June 5, 2008

"if the landscape inherently can give no testable insight into physics, why should a scientist bother with it?"

It is not true that the landscape "inherently can give no testable insight". cf the work of Anthony Aguirre, Matthew Kleban, and many others on the possibility of collisions of universes in the multiverse. You have had this pointed out to you *many* times. Readers can draw their own conclusions.

14. **Peter Woit**
June 5, 2008

vicar,

I have many times pointed out that, yes, it is in principle possible to construct multiverse models in which there are observable effects on our universe, for example the kind of thing that the people you mention are trying to do. There's a huge difference though between showing that one can construct such models, and showing that they have something to do with our universe.

The models typically being studied by the string theory landscape people don't have observable effects due to other universes, and it was those that Rachel Bean was asking Witten about in the question I referred to.

15. **Peter Woit**
June 5, 2008

I should also point out that the context of the quote is about the question of whether the multiverse can provide insight into why we live in one "string vacuum" rather than another, and thus explain something about particle physics. The models you refer to don't provide any such insight or explanation.

16. **somebody**
June 6, 2008

Peter, Lets assume the very real possibility that that there are indeed many ways (which cannot be resolved at current energies) to UV complete our familiar low energy physics and gravity. Is there ANY way of doing physics in that context which will keep you happy?

The way I would do physics in that context is to construct models that contain well-known low energy physics (with extra stuff at higher energies that I might or might not add depending on my prejudices), and wait for the next round of experiments. Modulo the (very important) fact that our tools are not sharp enough to give us the full control we would like to have, this is precisely what the string phenomenologists are trying. I don't understand what is the issue of PRINCIPLE that you seem to be arguing against.

There is a huge distinction to be made between understanding the multiverse, which presumably tells us about string theory, and understanding specific vacua, which could potentially tell us about OUR universe. Landscape statistics, measure issues etc. belong firmly in the first category. But model-building sits squarely in the second. The word anthropic makes some people turn to violence, but for me it is the same as fixing the vacuum from experimental input, which is what we have ALWAYS done in physics! There is nothing I understand in string theory which tells me that this is in principle impossible. The real problem is actually technical control, but stabilized string vacua are only a few years old, so its not hopeless yet.

17. **somebody**
June 6, 2008

Peter says:
"if the landscape inherently can give no testable insight into physics, why should a scientist bother with it?"

It is not the landscape that gives us predictivity, it is the individual vacua.

18. **somebody**
June 6, 2008

Let me emphasize this once more: if there are many UV completions consistent with low energy physics, then it would be inherently impossible to offer a single "prediction" for future experiments. The best one can do is to offer classes of predictions. You will need future (LHC?) experiments to pin them down further.

This is for example essentially what Vafa and collaborators did in the paper that you talked about couple of days ago, which you were unhappy with because it did not make "a" prediction.

19. **Peter Orland**
June 6, 2008

How can a vacuum be a theory? It's a solution, with some parameters. If you call it a theory, you're admitting that there is no criterion for these to be the right parameters. All you have then is a fit. Are we seriously going to call a parameter fit a theory of nature?

If it is true that fits are physics, I propose (take a moment to pause in childlike awe of Nature's works!) the Standard Lagrange Model of the universe. Give me any set of data points, I will use Lagrange's method to find a smooth function fitting those points. How beautiful that everything in the world is reduced to simple algebra. And by these ingenious method we require no more parameters than numbers to fit (in this respect it is better than some approaches). Now we finally understand how everything works.

OK, forgive my sarcasm. But seriously folks, if you want a UV completion of the Standard Model without experiments, you need a good reason to chose this completion. Renormalizability tells you that any completion is equally good – and no completion is necessary! QUALIFICATION: well the Higgs needs fine tuning and most likely has a trivial S-matrix near the Planck scale, but we'll probably never do experiments there.

20. **somebody**
June 6, 2008

>All you have then is a fit. Are we seriously going to call a parameter fit a theory
>of nature?

The difference is that for a fully stabilized vacuum to qualify as our Universe, it has to accommodate not just the finitely many experiments that have to be done in order to determine the vacuum, but also the infinitely many experiments that CAN be done afterwards. Thats how we know that we are not merely fitting curves. Incidentally, the standard model is fixed exactly in this way – except that fortunately this could be done already at currently accessible energies.

There are many crucial questions. Here is one: can we fix a vacuum in string theory by doing finitely many sub-Planckian measurements? Or do we need Planck-scale measurements? These and other questions have to be answered to make string theory useful, and string theorists are trying (or should try) to answer them. But these are serious questions and real work will be necessary to see how useful or not string theory ultimately is.

My point is that the mere existence of the landscape or multiverse or whatever you want to call it, does NOT make the theory unpredictive. That is merely a strawman argument that gets a lot of knee-jerk reactions. Almost all theories have landscapes.The weird thing is not that string theory has a landscape, but that string theorists were hoping against all reasonable hope for tens of years that there would be a unique vacuum.

>if you want a UV completion of the Standard Model without experiments,
>you need a good reason to chose this completion. Renormalizability tells
>you that any completion is equally good – and no completion is necessary!

The problem is we have something other than standard model at low energies: gravity. And string theory provides the UV completion there, but also tells us that there could be many ways to do it.

21. **Eric**
June 6, 2008

Peter O,
In a particular vacuum with all moduli stabilized , all parameters such as gauge, Yukawa couplings are fixed as these depend on the moduli. In addition, it is not possible to just construct any model you want as there are usually a host of consistency conditions that must be satisfied that strongly constrain the models. Thus, if one can find a model which describes exactly the SM in detail in its low-energy limit, then this would be a very significant discovery.

22. **Peter Orland**
June 6, 2008

Somebody,

I'm probably not going to convince you of anything in my rebuttal below. But I hope you will feel it is worth knowing that there is a rebuttal.

1. "There are many crucial questions. Here is one: can we fix a vacuum in string theory by doing finitely many sub-Planckian measurements? Or do we need Planck-scale measurements?"

Suppose you could do this. Isn't this just a parametrization consistent with string theory? And if so, how does it prove anything? I like my Lagrange Standard Model (above) better. The nation that controls Lagrange polynomials controls the universe!

2. "The weird thing is not that string theory has a landscape, but that string theorists were hoping against all reasonable hope for tens of years that there would be a unique vacuum."

I'm with David Gross on this one. Abandoning this hope means that you no longer have a theory.

3. "The problem is we have something other than standard model at low energies: gravity. And string theory provides the UV completion there, but also tells us that there could be many ways to do it."

No, it doesn't provide THE UV completion. It provides a UV completion. I happen to think (as I'm sure you do) that it is an interesting completion, but it's definitely not the only possible completion. For example, instead of a string tension as a cut-off, we could use a lattice spacing (like Regge calculus) as a completion. I am certainly not claiming that this is right, but a Regge-type lattice with lots of other fields (and Wilson terms to remove Fermion doublers) is something you can't rule out, any more than you can rule out string theory. So there are even MORE ways to do the completion.

My own view on string theory is similar to what some people said about N=8 supergravity when I was a student: String theory is worth learning because it's interesting, not because it's right.

23. **Peter Orland**
June 6, 2008

Eric,

"In a particular vacuum with all moduli stabilized , all parameters such as gauge, Yukawa couplings are fixed as these depend on the moduli. In addition, it is not possible to just construct any model you want as there are usually a host of consistency conditions that must be satisfied that strongly constrain the models. Thus, if one can find a model which describes exactly the SM in detail in its low-energy limit, then this would be a very significant discovery."

Well, there are a host of consistency conditions on any model you use to fit data. As I said to Somebody, let's all finally admit that the world is just a Lagrange polynomials. Sorry, but fits are not physics.

24. **Peter Woit**
June 6, 2008

somebody, etc…

I don't think it's possible to have a sensible discussion about this, except by looking carefully at actual attempts to construct string theory vacua. This is why I wrote the earlier posting about the claims to get predictions out of an F-theory construction. My argument is not that having lots of string theory vacua inherently makes it impossible to test, but that you have to examine what these vacua look like and see what can be gotten out of them.

Sure, it's possible to imagine a string theory vacuum construction that, given some experimental input, is predictive and testable. Problem is, people have been trying to do this for almost 25 years, and have gotten nowhere. Take a look at the F-theory paper. Taking as input everything we know about particle physics, and engaging in 300 + pages of complex constructions, the end result is a vague claim about the neutrino mass scale, that it is more or less where we already know it is. 25 years of this kind of thing is exactly what you would predict would happen if people pursue a wrong idea and refuse to stop.

Similarly, going on about how string theory completely stabilizes all moduli, making, for each vacuum state, precise testable predictions about SM parameters is just pure hype. That's a dream about what some people would like to be true, not the current state of string theory. If people want to pursue that dream, fine, but they should acknowledge that they are nowhere near this, and that the current state of the subject gives no reason to believe that even if you could do such calculations, they would turn out to be predictive and explain anything about particle physics.

25. **Eric**
June 6, 2008

"Similarly, going on about how string theory completely stabilizes all moduli, making, for each vacuum state, precise testable predictions about SM parameters is just pure hype. That's a dream about what some people would like to be true, not the current state of string theory."

Actually, this isn't hype, this is in fact the current state of string theory, as moduli stabilization has been a very important area of research for the last 10 years or so. It is really only a matter of time before a specific model is found which completely describes the SM and for which all moduli are stabilized. Progress in physics goes on despite the Woit's of the world.

26. **Peter Orland**
June 6, 2008

Eric,

"Actually, this isn't hype, this is in fact the current state of string theory, as moduli stabilization has been a very important area of research for the last 10 years or so. It is really only a matter of time before a specific model is found which completely describes the SM and for which all moduli are stabilized. Progress in physics goes on despite the Woit's of the world."

In other words, it is only a matter of time before string theorists find a fit. See my remarks above.

27. **Eric**
June 6, 2008

Peter O.,
No, once the moduli are stabilized there no free parameters. Thus, all physical parameters are determined dynamically. This is quite different than just finding a fit.

28. **somebody**
June 6, 2008

> In other words, it is only a matter of time before string
> theorists find a fit. See my remarks above.

I explained to you very clearly why it is not just a "fit". Because once you fit a vacuum every new experiment is a prediction. Why do you not acknowledge this issue and keep repeating your original, misinformed statements again and again?

What I find strange is that the critics are NOT attacking the weaknesses of string theory, which are real!!! There are two issues here. One is the construction of standard model-like vacua, which as Eric emphasized to you is a difficult problem, which has not yet been fully solved. The second is the issue of the landscape (which was what I was talking about), which is that we expect that there will be many vacua which can reproduce the standard model, once we construct ONE. Can you see all the leaps of faith employed by the string partisans here? Why are you not attacking those (relevant) issues?

Frustratingly, you seem to be not getting any of these subtleties and just keep repeating your irrelevant soundbites again and again. I apologize to Peter (Woit) if I sound a bit harsh.

29. **Peter Orland**
June 6, 2008

Sorry Eric. I don't get it.

You need to chose moduli, i.e. a vacuum. This is designed to fit your data. Then you want to use this vacuum to make predictions. If there is a unique choice of moduli (this strikes me as unlikely), you are fixing these moduli as parameters. If the choice of moduli isn't unique, you will simply have more parameters than you need (but okay, maybe there are some unique predictions within this parameter set).

Here is my question. How is this better than using any method to fit data by some function. My joke about the Lagrange Standard Model aside, any fitting method will make predictions. Don't say it's because it comes from string theory. You have to do better than that.

30. **Peter Orland**
June 6, 2008

Somebody,

"I explained to you very clearly why it is not just a "fit". Because once you fit a vacuum every new experiment is a prediction. Why do you not acknowledge this issue and keep repeating your original, misinformed statements again and again?"

This is why we fit data by functions. Experimentalists do it all the time. Once this is done we can make predictions. For example, we might try to fit some data by a straight line using a chi-squared fit, or we fit data by a Lagrange polynomial fit. But it doesn't mean we have a correct theory. I asked Eric above why choosing moduli to fit data is better. If the answer is "because we like this way of doing things, since it comes from string theory", it just isn't good enough.

31. **Peter Orland**
June 6, 2008

Let me put it another way. You fit data by a string vacuum. I fit it by Lagrange polynomials. We can both make predictions, and since we have both parametrized the data by smooth functions, they won't differ very much. Why is your method better?

32. **Eric**
June 6, 2008

Peter O,
The moduli are not 'chosen', they are determined dynamically by a potential which is generated by turning on fluxes or other non-perturbative effects. For a particular construction, the fluxes are generally tightly constrained by consistency conditions. For example, in Type II vacua the flux is constrained by tadpole equations as well as supersymmetry conditions.

33. **somebody**
June 6, 2008

> This is why we fit data by functions. Experimentalists do it all the
> time. Once this is done we can make predictions. For example, we
> might try to fit some data by a straight line using a chi-squared
> fit, or we fit data by a Lagrange polynomial fit. But it doesn't
> mean we have a correct theory.

Of course it does! If the next experiment tells you that the data lies on the same straight line, your straight line theory was correct! The only difference in high energy physics is that the fit that you are trying to make is so involved that there is a whole subculture called "theory" that is devoted to it. This is ALL there is to theory. You might want something more grandiose, but there isn't anything more grandiose.

The real subtlety is that the thing that you are trying to fit is NOT always a straightline. There are many comlicated things that you are trying to fit at the same time. But apart from that "technical" issue, the analogy works.

34. **Peter Orland**
June 6, 2008

"The moduli are not 'chosen', they are determined dynamically by a potential which is generated by turning on fluxes or other non-perturbative effects."

Wait a minute, the moduli are determined?…this negates the whole idea of the landscape…

But I think you mean that there are consistency conditions on the choice of moduli, which have to do with things like p-form fluxes. Just as a Lagrange polynomial used to to fit data is constrained by the assumption that it IS a polynomial. Any method used to parametrize data will have its own idiosyncracies. But the results will only be a little different, assuming Nature likes smooth functions. So I ask again: why is your method better?

35. **somebody**
June 6, 2008

> You fit data by a string vacuum. I fit it by Lagrange polynomials.

I would like to see you fit all the high energy experiments for for example the standard model, by a spline fit. Its not one set of data that you have to fit, but every possible sets of data.

36. **Peter Orland**
June 6, 2008

"If the next experiment tells you that the data lies on the same straight line, your straight line theory was correct!"

No! A straight line is not a theory. It is just a fit (NOW am I getting through?).

"The real subtlety is that the thing that you are trying to fit is NOT always a straightline. There are many comlicated things that you are trying to fit at the same time. But apart from that "technical" issue, the analogy works."

OK, so don't fit with a straight line. Use a Lagrange polynomial. You can fit anything this way. Why is your way better?

37. **Peter Orland**
June 6, 2008

"I would like to see you fit all the high energy experiments for for example the standard model, by a spline fit. Its not one set of data that you have to fit, but every possible sets of data."

I'm sorry, but I can fit anything with a Lagrange polynomial, no matter how much data you give me (assuming I was a good enough programmer). Why are the moduli (constrained by whatever nonperturbative effects you want) better than the coefficients of this polynomial?

38. **somebody**
June 6, 2008

Peter O., a theory is useful because it can fit all data. If there is a finite number of experiments you can do and then predict EVERY single new experiment in our Universe, you will have the final theory. If you can some up with it, by curve-fitting if you will, thats all you need. In any event, this discussion is beyond ridiculous now and Peter (W.) is probably going to get upset, so unless there is something new you have to say…

39. **Peter Orland**
June 6, 2008

No, no, I said what I have to say. I am happier than a pig in &^%&%&^$# that you finally agree that what people are doing with the landscape is curve fitting. What we disagree about is whether that constitutes a physical theory. I say it doesn't.

40. **Peter Woit**
June 6, 2008

Ummm, maybe enough about this particular point, OK?

What I've found in discussions here is that abstract arguments tend to degenerate, because string theory advocates point to the most optimistically possible situation that doesn't completely violate the rules of logic, and, by this measure, sure you can't rule out string vacua scenarios. That's not the measure usually used to evaluate a research program though, which is results achieved so far, and ones which seem plausibly achievable considering how things have been going recently.

41. **Peter Orland**
June 6, 2008

Sorry to let this argument continue for so long Peter. I believe, however, that is goes to the very core of the value of the landscape and that somebody, Eric and I performed a useful service is illuminating the issue.

42. **Eric**
June 6, 2008

"Wait a minute, the moduli are determined?…this negates the whole idea of the landscape…"

No, the landscape emerges because there are many different models with differerent gauge groups and matter content for which the moduli may be stabilized rather than just a single model which is unique.

43. **anon.**
June 6, 2008

Eric, that's exactly the same thing as saying that moduli can't be determined. I.e. the reason you can't determine string theory moduli (to allow checkable predictions to be made from string theory) using the 19 empirical standard model parameters is precisely because string 'theory' isn't a unique theory, but is instead a collection of a vast number of different models. So the inability to determine moduli is the same thing as having a landscape. Any theory with a collection of parameters that can be empirically constrained to no less than 10^500 possibilities, then that's identical to having a landscape size of that magnitude.

44. **Peter Orland**
June 6, 2008

Er, Peter is going to mad at me. But Eric, so what? The acceptable moduli are discrete rather than continuous. But there are so many choices that they might as well be continuous. Just curve-fitting.

OK, this is my last word. Eric, blast away in my absence.

45. **Peter Woit**
June 6, 2008

Sorry folks, anything else on this point will have to be insightful and brilliant beyond words for me to not delete it.

46. **Andrew Mayo**
June 29, 2008

If you will pardon this interjection from a purely lay perspective, and focussing for a moment on the New York Times article, then is Witten's perspective – or indeed, that of physicists collectively – really one of 'despair'.

In reading Peter's brilliant book, Witten comes across as a rare figure in the modern landscape; a man with an eclectically wide range of interests who uses these to synthesise new viewpoints.

In many ways this seems to me to be similar to Einstein's world view; he considered the physical world from many disparate viewpoints in order to construct a new understanding of space and time.

If eminent figures such as Witten are now starting to consider the physical landscape from an anthropic perspective – and just setting aside for a moment the instinctive revulsion many people have towards this approach – isn't this simply a sign of a brilliant mind exploring a different perspective simply to see if it yields results, rather than an abandonment of the scientific principle?. Certainly this doesn't seem like an act of despair.

Although Peter Woit has – quite understandably – expressed negativity regarding this approach – it does, after all, smack of some kind of recursive navel-gazing – it does seem to lead to some interesting lines of inquiry.

Most obviously, we can take the 20-something arbitrary parameters of the standard model and adjust each one by a small amount to see what the outcome is with regard to the evolution of the universe (e.g nucleosynthesis etc). Some parameters will presumably be more sensitive than others. Suppose it turns out there are some fantastically huge number of possible universes, say, with no matter as we understand it at all, only photons, some lesser number with – perhaps – photons and leptons – a smaller number again with fermions as well. And so on down to the universe containing only hydrogen, etc. etc. (or must all possible universes contain all the fundamental families of particle in the standard model, an interesting question in its own right, I'd have thought).

Then – and I think this is the spirit in which Witten is proceeding – let's assume our observable universe is potentially a 'bubble' evolving from some alternate universe in which the laws of physics differ. We assume some probable alternative configuration based on our adjustment of the standard model parameters and then we calculate, for example, how our universe might evolve and expand from its parent. (would it, for instance, be possible to show that such expansion would accelerate and if so under what circumstances?).

Now this could be dismissed as crackpot science but there are testable hypotheses here, are they not, so it sounds to me like science. Certainly it involves entertaining some rather speculative hypotheses as a starting point, but how is this different from Einstein imagining what an observer 'sitting on a photon' would see, which appears to be his starting point for his exploration of relativity. At the time, there must have been many of his fellow physicists who at first thought his views untenable.

Hints of ‘time before Big Bang’

June 8, 2008
Categories: Multiverse Mania

The BBC is running a story entitled Hints of ‘time before Big Bang’ based on Sean Carroll’s latest efforts to promote the multiverse. The writer attended Sean’s talk at the recent AAS meeting and presumably also read Sean’s new Scientific American article, and here’s what he got out of them:

> A team of physicists has claimed that our view of the early Universe may contain the signature of a time before the Big Bang…
>
> Their model may help explain why we experience time moving in a straight line from yesterday into tomorrow…
>
> Their model suggests that new universes could be created spontaneously from apparently empty space. From inside the parent universe, the event would be surprisingly unspectacular.
>
> Describing the team’s work at a meeting of the American Astronomical Society (AAS) in St Louis, Missouri, co-author Professor Sean Carroll explained that “a universe could form inside this room and we’d never know”.
>
> The inspiration for their theory isn’t just an explanation for the Big Bang our Universe experienced 13.7 billion years ago, but lies in an attempt to explain one of the largest mysteries in physics – why time seems to move in one direction…
>
> “Every time you break an egg or spill a glass of water you’re learning about the Big Bang,” Professor Carroll explained…
>
> If the Caltech team’s work is correct, we may already have the first information about what came before our own Universe.

Besides the “Does Time Run Backwards in Other Universes?” material from his paper with Jennifer Chen discussed in Scientific American, what’s new here is his recent paper with two Caltech collaborators about the possibility of explaining an asymmetry of marginal statistical significance observed in the CMB by invoking a more complicated version of inflation, adding a “curvaton” field to the usual inflaton. In their model, this asymmetry comes from a perturbation to the curvaton field of size larger than the horizon. Such a thing could in principle make testable predictions, but doesn’t necessarily come from the existence of a multiverse or tell us anything about it. The authors throw in one clause of a sentence about how it might occur as

> a remnant of the pre-inflationary epoch or as a signature of superhorizon curvaton-web structures.

and that’s the basis of the BBC article. I have no idea what’s going on with the business about universes forming inside of rooms and us not knowing anything about this.

Sean gives more details about this in a new blog posting.

Update: The author of the piece, Chris Lintott, has a blog, and a posting about the article, where he writes:

> What made me want to write the story in the first place, though, was exactly what Sean said above – to an outsider to the field the idea that it is even imaginable that we might be able to make concrete predictions from ideas about multiverses which have haunted the pages of New Scientist and its ilk for decades is stunning. That’s what I wanted to get across.

He doesn’t seem to realize that there’s nothing here different than the things he’s thinking of that “have haunted the pages of New Scientist and its ilk for decades.”

Update: This story is getting the full media treatment, including haunting the pages of New Scientist, which has the sense to strip out the nonsense and hype about the multiverse and the arrow of time. Slashdot emphasizes the part about:

> Describing the team’s work at a meeting of the American Astronomical Society (AAS) in St Louis, Missouri, co-author Professor Sean Carroll explained that ‘a universe could form inside this room and we’d never know.'”

Comments

1. **anon.**
   June 9, 2008

   ‘I have no idea what’s going on with the business about universes forming inside of rooms and us not knowing anything about this.’

   If time is going backwards in the universes that form in your room, they will instantly disappear into the past, so you can’t see them.

2. **Peter Woit**
   June 9, 2008

   anon.

   Thanks, I was having trouble figuring that one out.

3. **hmmm**
   June 9, 2008

   I wonder how many unobservable universes will be produced at CERN.

4. **Chris**
   June 9, 2008

   Thanks for the link. Sean in his talk made a possible connection between pre-inflationary structure and the observations contained in the paper. As you say

   “Such a thing could in principle make testable predictions, but doesn’t necessarily come from the existence of a multiverse or tell us anything about it.”

   The ‘could in principle’ was new to me as an astronomer, not a cosmologist, and I thought that was worth writing about. Perhaps these ideas have been floating for a while, but it was the first time I’ve heard a talk that directly suggested we could – even in principle – hope to probe pre-inflationary structure. That’s a better and more interesting story than the classic multiverse with no observable consequences which I was unfairly throwing at New Scientist.

If that’s common knowledge among those who work in this field, then you should all shout louder.

5. **Jayvee**
June 9, 2008

    According to this week’s New Scientist, Benjamin Wandelt has discovered the non-gaussianities that Sean is looking for in the cosmic microwave background.

6. **Peter Shor**
June 9, 2008

    anon says “If time is going backwards in the universes that form in your room, they will instantly disappear into the past, so you can’t see them.”

    I don’t understand. If there are backwards-going universes that are currently forming in my room, won’t I remember them having been there yesterday?

7. **wolfgang**
June 9, 2008

    > If there are backwards-going universes that are currently forming in my room, won’t I remember them having been there yesterday?

    No. you would remember that they have been there tomorrow 😎

8. **jpd**
June 10, 2008

    i think the tense you want is “will have been”

9. **Christine**
June 10, 2008

    Meanwhile…

10. **Thor**
June 10, 2008

    Perhaps there are many universes, each with it’s own point of origin or big bang like expanding soap bubbles. And spots inbetween where matter from different big bangs gather in massive black holes. Then when each of those black holes reaches some critical point or some other criteria it explodes with a big bang, forming a new universe. That’s my theory at lest.
    Note that scientists can only inspect the universe to the degree their equipment allows, and the universe is hugenormous.

11. **Amos**
June 10, 2008

    “If time is going backwards in the universes that form in your room, they will instantly disappear into the past, so you can’t see them.”

    Wouldn’t this violate the conservation of… like everything?

12. **M.Wang**
June 10, 2008

    The host really makes the paper sound like lame beyond belief. Does anyone have a rational retort, i.e. an argument why this line of research should not be dismissed off-hand as meaningless mumble-jumble?

13. **Sakura-chan**
June 10, 2008

    This story has reached the heady depths of the Xbox forum I frequent. http://forum.teamxbox.com/showthread.php?t=583146

14. **chimpanzee (aka "joe")**
June 11, 2008

    “Never understimate the power of Human Stupidity in large groups [science challenged journalists]”
    — anonymous

    A good example is a post by Joanne @Cosmicvariance

    > My senior science teacher summed it up best by saying `**What you said was probably correct, but it’s not what you say to a newspaper reporter.’ That’s when I should have learned to be careful with reporters.**
    >
    > Two weeks ago, it happened again. The good folks in the SLAC publicity office are starting a feature where every few weeks a piece of work from the SLAC theory group will be highlighted. Great idea, I thought! I was the first guinea pig and was asked to do an interview for an article on a paper I wrote last Spring. The work was cute, has a catchy title, and is published in Physical Review Letters, but is not going to change life on earth as we know it. The article was to be for the internal SLAC newsletter TIP (The Interaction Point) and would also make a brief appearance on SLAC Today, the daily newsboard for the SLAC community.
    > …
    > Next thing I knew, the headline
    >
    > SLAC Physicists Develop Test for String Theory
    >
    > was emblazened on the main SLAC homepage! Then Peter Woit of Not Even Wrong lashed onto it. Then it was picked up by PhysOrg.com, which was subsequently featured by Slashdot. All with a smiling picture of yours truly, supposedly devising a definitive test for all of string theory. AARGH!!!!
    >
    > **The entire article was misrepresented, blown up out of proportion, and I could not have been more upset. Nothing against the good folks at the**

> **communications office at SLAC – we worked on this together and none of us saw this coming. Nonetheless, I did not have a good week.**
>
> The remedy? We posted comments on all the blogs and revised the article to include the scientific details which then put our work into proper context.

I recall P. Woit & Bee complaining (rightfully) about poor Science coverage in media, e.g. New Scientist.

Why do you keep pandering to these idiots?

The physics blogosphere needs to realize that blogs can be used for Journalism (Content/Distribution model). You no longer have to use a [science-challenged] journalist as an intermediary to the Media (Standard Model of TV, newspaper, etc). You Can Do It Yourself.

It's trivial to setup an RSS feed to iTunes (from WordPress, Blogger blog), & blog with video. There are solutions out there which will automatically set you up with an iTunes video-podcast. Presto..YOU are a scientist reporting the Science News as It Should!! Over the iTunes portal (with its powerful search engine), which has numerous distribution points: iPod/iPhone (mobile media device..over a hundred million units), AppleTV (living room set top box, reaching the public in the comfort of their living room), Internet (embedded Flash or Quicktime videos, i.e. Internet website). See for example:

http://strings07.blogspot.com
[search in iTunes using "physics", "string theory", "lisa randall", etc]

Based on my multimedia project, I've tried to encourage physicists to video-blog, to no avail (here, Backreaction, Arcadian Functor, CV). I know the mentality (since I am guilty of it myself): "It is illogical for Technical types to use an emotional argument".

"Facts Tell, Stories SELL"
— marketing pots&pans, auto-racing, etc

Scientists fall into the trap of using Facts to "sell" to the public, when in fact they should be "telling a story". In a sense, this is what S. Carroll is doing: he's "weaving a [fairy] tale". The G. Lisi publicity (Next Einstein, surfer dude, et al) is the correct strategy..which is widely criticized in the Physics community!! Take the JPL Rover mission, geophysicist Dr. M. Golembeck was considered "the story" (because of his **personality**: boyish looks & enthusiasm).

At least L. Motl/TRF embraces extensive use of Youtube videos in his blogging (Youtube is a video-sharing service).

L. Motl/TRF ("End of Science"):

> **Nevertheless, the author of the stupidity from 1997, after those ten years that have demonstrated that his stupidity is among the greatest stupidities that have ever been pronounced by homo sapiens, has the stomach to come in front of a conference in Portugal and repeat the same stupidity.**
>
> How is it possible? Well, it is because there are almost no people who would be pointing their fingers at this – very politely speaking – intellectual shit that keeps on contaminating the public sphere. That's why the likes of Horgan keep on thriving. If we allow them to thrive, they can indeed force science to end on a sunny day in the future. This is the only way how science could possible end. Let's not allow it.

Lumo is 1 of the few physicists "starring" in their own Youtube productions, a Tom & Jerry rendition (entertainment). Max Tegmark recently did a musical skit for his MIT class.

A) Information
physics, etc

B) Entertainment
"the sizzle sells the steak" as the saying goes. Looks, musical ability, personality (incl strange behavior), etc.

The flawed reasoning by HEP community (which is the cause of the Fiscal 2008 crisis) is that they are selling A), instead of A+B. I.e., they need a mult-dimensional model (call it a "multiverse" of solutions) for marketing to the masses. That's how NASCAR (a geographically niche, gear-head techno thing) made their breakthrough to the masses (leapt past Baseball as the No 2 sport in USA!!??). There are enough physicist baseball fans (Joanne, et al) to understand the enormity of this. Simlarly, Formula 1 (auto racing) is the No 1 watched sport in the World (exceeding Soccer).

"Life's a Stage"
— Shakespeare

S. Carroll/CV understands this, & that is exactly what he's doing: "pulling an act" (marketing his research) for the Media. He had a long post about how to "play the game" in Academia: "suck it up" (Bee & others found it somewhat offensive)

"People don't buy Good Products [research], they buy GOOD MARKETING"
— business saying

Physics is not that different from the Business World. You have a product, you have to sell it, you have to get funding. "You have to learn to sell yourself" (advice given to Bee by her advisor). Here's an example from Caltech/IPAC:

from Kip Thorne's PhD student (who works at IPAC):

1) He who has the Gold..rules
funding issues

2) Schmoozing & Salesmanship are important
marketing/sales, aka "kissing a**" as per M. Franklin/Harvard

3) Dealing with Difficult people
"Suppose you were an idiot, suppose you were a Congressman..but I repeat myself"/Mark Twain

Face it, we live in an Imperfect World. Deal with it (or not).

Guess what? There is a "sleeping giant" & it's Kea..she's the key to a Physics marketing breakthrough. L. Randall is 2nd. Hint: extreme outdoorsmanship & survival. Perfect subject matter for media coverage: TV, movie, etc. I've recently been contacted by National Geographic documentary (I know the chief illustrations editor), chief of ABC prime-time programming, BBC Documentary. Hit them up with proposals.

15. **Eric H**

June 11, 2008

Joe,
Your comments about becoming a better showman put me into a cold sweat. I don't think the answer to the ignoramuses promoting multiverses inside a coffee cup is to fight fire with fire. For one thing, many of the best minds that go into science fields go there because because they have a basically reflective temperament. They don't want to stir up the atmosphere around them so much that they can no longer be objective in their own thinking. This problem always comes with being too publicly pushy in one's ideas. Once your name is firmly established in the public mind with a specific point of view, it is very difficult to unwed yourself from it. People with a more humble and denigrating temperament – i.e. the best people in science – know this. They know that, even with a great preponderance of evidence in their favor, that the future can be somewhat unpenatrable.

The problem is that the worst people in science, and for that matter in general, have no such problem. They go along quite swimmingly with any change that proves they have been alarmingly wrong. And like politicians and neo-conservatives declare that they secretly suspected the new information all along – no shame and no regret to their prior public declarations. Maybe you want to try to compete with those kind of people in that kind of salesmanship but I sure wouldn't attempt it. I'd feel sort of dirty.

16. **ProfM**
June 12, 2008

Peter,

Why do you allow this meaningless, offensive and irrelevant exchange of opinions to take place on your blog? The last two entries are good examples. These pathetic conversations have nothing to do at all with the issue of testability in String Theory!
People enter your blog to be educated but they are turned away by significant loud noise and mumble-jumble.

I know that you are going to delete my message right away. But you cannot hide the truth!

17. **Tony Smith**
June 12, 2008

Adrienne Erickcek, Marc Kamionkowski, and Sean Carroll (all of Caltech)
in arXiv 0806.0377 astro-ph said:
"… there is an anomaly in the CMB: measurements from the Wilkinson Microwave Anisotropy Probe (WMAP) indicate that the temperature-fluctuation amplitude is larger, by roughly 10%, in one hemisphere than in the other. This power asymmetry occurs at the 99% C.L. …
the observed CMB fluctuations … appear to be modulated by a dipole …
More precisely, the fluctuation amplitude in one half of the Universe is higher, by about 10%, than in the other half …
it cannot be attributed to any known astrophysical foreground or experimental artifact.
This asymmetry has gone largely unnoticed
(as opposed to the "axis of evil", an apparent alignment of only the lowest multipole moments) …".

However,
in a 2006 Physics Nobel Prize powerpoint (slide 17) at
http://www.nat.vu.nl/~mulders/Nobelprize-2006.ppt
Piet Mulders said, about the work of Mather and Smoot:

"… COBE and WMAP … A dipole effect corresponding to the motion of Earth with respect to CMB rest frame (about 600 km/s) …".

Is there any reason to reject the explanation of such a dipole asymmetry by "the motion of Earth with respect to the CMB rest frame"?

Tony Smith

18. **Christine**
June 13, 2008

Tony Smith,

I could be mistaken, but Isn't the CMB dipole anisotropy about two orders of magnitude above the other CMB anisotropies? Those authors do not appear to be talking about the amplitude of CMB dipole per se, but a 10% fluctuation of it.

19. **Tony Smith**
June 13, 2008

Christine, thanks for explaining to me that "Those authors … appear to be talking … about a 10% fluctuation of … CMB dipole …".

Does that mean that there are 3 relevant axes ?

dipole axis of amplitude – due to motion with respect to the Cosmic Microwave Background. – according to astro-ph/0302207 (First Year WMAP) "… COBE determined the dipole amplitude is 3.353 +/- 0.024 mK in the direction (l, b) = (264.26 degrees +/- 0.33 degrees, 48.22 degrees +/- 0.13 degrees),
where l is Galactic longitude and b is Galactic latitude …".

dipole axis of fluctuations – according to astro-ph/0701089 (Hemispherical power asymmetry) "… in Bayesian terms, the log-evidence difference is about 1.5 to 1.8, corresponding to odds of one to five or six. … Thus, there is still a chance that the effect may be a fluke, and most likely, this will remain the situation until Planck provides new data in some five years …
The best-fit modulation dipole axis points toward (l, b) = (225 degrees, –27 degrees) …
much effort has been spent by theorists on providing possible
physical explanations …
e.g.,
second order gravitational effects from local inhomogeneities – Tomita 2005;
the presence of local voids – Inoue and Silk 2006 …",

quadrupole/octupole axis (axis of evil) – according to astro-ph/0302496 (cleaned CMB map from WMAP) "… The preferred axes … for the quadrupole and octopole … are … both roughly in the direction of (l, b) = (–110 degrees, 60 degrees) in Virgo …".

So, is it fair to say that:
the dipole axis of fluctuation as of now is hard to distinguish from an irrelevant fluke;

and
even if it might turn out to be real,
it might be explainable in conventional physics terms, including but not necessarily limited to those described by:

Tomita (astro-ph/0505157) "… The present state of our universe is … locally complicated and associated with nonlinear behavior on various scales, and so the observed quantities of CMB anisotropies may include some small effects caused by large-scale local inhomogeneities through nonlinear process. … there is a non-trivial north-south asymmetry in various quantities about CMB anisotropies … based on the relativistic second-order theory of perturbations in nonzero-/\ flat cosmological models … and on the second-order formula of CMB anisotropies … it is found that there is a possibility to explain the small north-south asymmetry of CMB anisotropies …".

Inoue (0710.2404 (astro-ph)) "… various types of anomalies have been reported after the release the WMAP data … an asymmetry in the large-angle power between opposite hemispheres … Inoue and Silk 2006 … explored the possibility that the CMB is affected by a small number
of compensated local dust-filled voids … The Shapley supercluster
(SCC) is near the tangential point of the two local large voids. The mysterious correlation with the ecliptic plane can be explained naturally because the ecliptic plane is by chance tangential to the CMB dipole that originates from a mass concentration around the SCC …".

Tony Smith

20. **anon.**
June 13, 2008

"… dipole axis of amplitude – due to motion with respect to the Cosmic Microwave Background. – according to astro-ph/0302207 (First Year WMAP) "… COBE determined the dipole amplitude is 3.353 +/- 0.024 mK in the direction …"."

'Dipole axis of amplitude' is a very polite euphemism for this massive +/- 3mK cosine anistrophy in the CBR, compared to the original name given by discoverer R. A. Muller in his Scientific American article (v238, May 1978, pp. 64-74): see http://adsabs.harvard.edu/abs/1978SciAm.238…64M

(If the CMB is used to establish a reference frame for motion, this anistrophy indicates absolute motion with respect to that frame.)

21. **Peter Woit**
June 13, 2008

Tony et. al.

Please, a much better place to discuss the technical details of Sean's paper would be at his blog posting on the topic.

22. **Pradeep**
June 14, 2008

"a universe could form inside this room and we'd never know"

I am an engineer by profession and working towards a PhD. My advisor would throw me out if I propose any explanation of any experiment and not substantiate it with numbers/logical arguments/predictions about how to go further about the experiment.

I have an avid interest in the way physics (I don't say "theoretical"..after all, this distinction of subject into theory and experiment is really meaningless…both have to go hand in hand) is going. The way I look at the claims and hype about stuff like multiverse, string theory, brane world and so on… is getting really disgusting.

If I had made a statement as the one quoted above, my advisor would throw me out of PhD programme telling me I am a crackpot.
And string theory and its ilk has continued to occupy center stage for ~30 years without someone calling them crackpots baffles me!

I think Feynman's dictum that the experiment is the only test of theories summarizes all that is wrong with string theory and it's variants.

PS: I was made aware of the "dark side" of string theorists by this blog and I thank Peter for calling spade a spade (or crackpot theory as crackpot theory)!

23. **Christine**
June 14, 2008

"a universe could form inside this room and we'd never know"

When I read this I thought: this only deserves a cartoon. What else?? That's why I draw it in 5 minutes in my paintbrush and posted on my blog as a comedy. Reminds me of when kids swear that they saw things that don't exist. Actually, I was not really very fair to it. My kid asks far more relevant scientific questions than that silly statement.

I don't have the willingness or energy to debate that claim scientifically, even because it's not scientific to start with. It's difficult to understand how a scientist would ever think this is scientific and spend his/her time with it. And having it published, if that is/will be the case, is beyond my comprehension.

I usually am not so harsh and try to have an open mind. But everything has limits and I'm really getting tired of these multiverses & co.

24. **Peter Woit**
June 14, 2008

Pradeep,

"string theory" is a very big subject, and it's extremely unfair to dismiss it all as "crackpot". Unfortunately some parts of string theory (and parts of cosmology having little to do with string theory) have taken a turn towards pseudo-science, but I think the people promoting this are very much a minority in the subject. It would be in the interest of serious physicists working in string theory and cosmology to take a more active role in making it clear to the public what is pseudo-science and what isn't. By not doing this, I think they are damaging the credibility of their field, with the whole thing getting tarred as "crackpot".

25. **Eric H**
June 14, 2008

Peter,

I agree with your assessment of the need for pointing out more publicly what is pseudoscience and what is not. There is a definite need to call out publicly the wackiness of some of these ideas. I hope you and others understand that what I was saying earlier about the problem with public promotion refered to excessive pushing of "one's own theories". I think a lot of the problem with string theory and multiverses originated from that human frailty of need for recognition at the expense of truth and logic. In a way you could say that kind of human sociology produced string theory and the trendiness in physics that went with it, and not the other way around.

26. **somebody**
June 14, 2008

Pradeep says:
"I think Feynman's dictum that the experiment is the only test of theories summarizes all that is wrong with string theory and it's variants."

The tragedy of string theory (and in fact theoretical physics in general today) is that the really interesting questions that we want answers for, are currently way outside our experimental capabilities. To answer essentially all the questions to which we have experimental access, standard model plus Einstein gravity is perfectly enough. But unfortunately(?) human curiosity does not end where our experimental capabilities end. The difficult question is where does the deadly combination of curiosity + lack of experiments turn into mathematical masturbation.

When you attack a caricature of this problem (like "no experiments yet, so kill strings"), it is easy to come up with simple answers to this difficult question like you did. But on the other hand, if we take the hints from particle physics and gravity seriously, we are lead to an enormous and (surprisingly) consistent mathematical structure called string theory. Should we take this structure seriously in the interim, or should we not do anything because we have no experiments (yet)?

Of course, most of the criticisms against string theory come from people like you who are (admittedly) ignorant about these hints. So any debate of this form almost always degenerates to frustration for the defender of string theory. I see no way even in principle to show you my side of the argument. And when I say this, I am called arrogant.

Incidentally, your advisor is perfectly right to kick you out of the department if your model does not tie up with experiments. Because you HAVE experiments. Everything you ever do (if you are anything other than a reactor engineer or something) MUST be prefectly understood within the context of electromagnetism. This does not mean that what you are interested in is useless, but it does mean that what you are interested in is merely a detail from the perspective of someone interested in more fundamental things. But there are people who want to see beyond E&M, the standard model and all the way to gravity. I am sorry to be rough, but I see no way but to point out the difference in perspective when faced with such hostility.

In any event, as everyone knows, we string theorists do face the possibility that we are victims in an unfortunate age trying to delude ourselves. But even then, I see no reason for your hostility. If you do not like what we are trying to do, think of us as something like half-mathematicians. We only take up as much academic resources as them.

The fact of the matter is that string theory IS fascinating. That is the bottomline at the moment.

27. **somebody**
June 14, 2008

Just to point out one historical fact: the whole idea of the multiverse in the sense of eternal inflation has been there long before string theorists stumbled across it via independent arguments. And Sean is a cosmologist.

So even if the idea was flack-worthy, …

28. **Chip Neville**
June 14, 2008

Peter,

You are right when you say in your June 14th reply to Pradeep, "It would be in the interest of serious physicists working in string theory and cosmology to take a more active role in making it clear to the public what is pseudo-science and what isn't", and you are right when you criticize Scientific American and other popular science publications for hyping speculative ideas without warning readers of their speculative nature. But perhaps you will be cheered up by the Letters section of the June issue of Scientific American, the very same one containing Sean's article, where a reader asks: "Are any experiments planned for the LHC that could either support or falsify theory's claims, expectations or predictions?", and Quigg replies: "String theory is not at the point of making specific predictions for the LHC. … The LHC has influenced some prominent string theorists to put the theory aside, for the moment, to concentrate on theoretical problems that promise a more immediate dialogue with LHC experiments."

Do you have any idea to which prominent string theorists Quigg is referring?

29. **Peter Woit**
June 14, 2008

Chip,

Quigg's description of

"a conversation between an experiment and threads in the string theory worldview"

just sounds like obfuscation to me. I don't see why he can't just straightforwardly say that string theory predicts nothing about what the LHC will see, and that the interest in the LHC has to do with physics (electroweak symmetry breaking) which has nothing at all to do with string theory.

I don't know who he is thinking of in claiming string theorists have abandoned string theory for LHC physics. LHC phenomenology is a very different subject than string theory, and few people can move from one to the other. One example might be Arkani-Hamed, but he was never much of a string theorist. Some string theorists have gotten involved in the "LHC Olympics", but it's unclear whether that counts as a change in research direction. There are a few other examples of people doing AdS/CFT stuff that is supposed to be related to the LHC, see, e.g. recent paper by Maldacena.

30. **Thomas Larsson**
June 15, 2008

The tragedy of string theory (and in fact theoretical physics in general today) is that the really interesting questions that we want answers for, are currently way outside our experimental capabilities.

I want to know why m_p/m_e = 1836 or why 1/alpha = 137.036. It seems to me that it is theory rather than experiment that is the problem.

31. **Chris Austin**
June 15, 2008

Hi somebody,

"To answer essentially all the questions to which we have experimental access, standard model plus Einstein gravity is perfectly enough."

Three independent approaches to making sense of the quantization of Einstein gravity in 3 + 1 dimensions have had a substantial amount of success recently:

1) Bennie F. L. Ward's technique of resumming Feynman diagrams, e.g. arXiv:hep-ph/0610232

2) Asymptotic safety, e.g. arXiv:0708.1317, arXiv:gr-qc/0610018

3) Causal dynamical triangulations, e.g. arXiv:0712.2485

All three of these approaches appear to lead to the conclusion that the strong / electroweak Standard Model plus quantum Einstein gravity in 3 + 1 dimensions predict that the universe has a radius of curvature around 10^{-35} metres, i.e. the Planck length, in gross contradiction with observation.

On the other hand, if we choose not to quantize gravity, but accept the existence of gravitational radiation, for which there is experimental evidence from binary pulsar timing studies, see http://nobelprize.org/nobel_prizes/physics/laureates/1993/press.html then we meet the equipartition theorem of classical statistical mechanics, namely that in thermal equilibrium at temperature T, every quadratic degree of freedom has mean energy (1/2)kT, where k is Boltzmann's constant. The gravitational radiation field has an infinite number of quadratic degrees of freedom per unit volume, since the frequency of gravitational radiation can be arbitrarily high, hence we must conclude that the gravitational radiation field is either not in thermal equilibrium, or has exactly zero temperature, or has infinite energy per unit volume. This is precisely analogous to the corresponding paradox for the electromagnetic field, that led Planck to the introduction of quantum theory, so that the equipartition theorem is only valid for frequencies \nu such that h \nu is small compared to kT.

Best regards,
Chris

32. **Observer**
June 15, 2008

Somebody says:

"The tragedy of string theory (and in fact theoretical physics in general today) is that the really interesting questions that we want answers for, are currently way outside our experimental capabilities"

Since the complaint is testability of String Theory, I am interested to know if there are any thought-experiments that can reasonably substitute experiments.

33. **Aaron Bergman**
June 16, 2008

Since the complaint is testability of String Theory, I am interested to know if there are any thought-experiments that can reasonably substitute experiments.

Of course not. But, when you don't have any real experiments, you make do with what you have.

34. **Peter Orland**
June 16, 2008

Somebody says:

"The tragedy of string theory (and in fact theoretical physics in general today) is that the really interesting questions that we want answers for, are currently way outside our experimental capabilities"

I don't understand why some people think that the only really interesting questions are quantum gravity and unification.

What about confinement? What about the soft Pomeron in QCD? What about chiral symmetry breaking? What about High-T_{c} superconductivity? What about turbulence? What about the Hubbard model in two or three dimensions? What about dark energy? What about inflation? Etc., etc., etc. All of these questions are related to experiments or observations. The people who work on them have as much talent as anyone (with the exception of yours truly).

35. **Tom W.**
June 16, 2008

Peter, you say:
>Unfortunately some parts of string theory (and parts of cosmology >having little to do with string theory) have taken a turn towards >pseudo-science, but I think the people promoting this are very >much a minority in the subject. It would be in the interest of >serious physicists working in string theory and cosmology to take >a more active role in making it clear to the public what is pseudo->science and what isn't.

Anyone claiming the existence of extra dimensions is talking pseudo-science, so this would seem to include a majority of string theorists. If this is not the case, then can you (as a serious physicist) make it clear to me (the public) where the scientific basis for extra dimensions can be found?

36. **Christine**
June 17, 2008

I philosophically oppose to extra-dimensions and multiverses.

Scientifically, the only way to address them is to experimentally test the various hypotheses for their existence. In this case, extra-dimensions can be scientifically tested, but not multiverses. By definition, the universe is *all* that exists. There is no intrinsic meaning to talk about *other* universes. It is not scientific because to begin with, it has no meaning. I don't understand why people cannot see the logic of this simple argument.

Sometimes I find interesting to read about ideas like the quantum creation of the universe from nothing, like Vilenkin's attempt, but when it comes to other universes, the line between science and pure imagination/illogic is crossed. Since I have a science-fiction vein, these ideas can serve as some inspiration. But even then, since they are already so much explored (and since I dislike them), I avoid them and look for other more interesting ideas that abound in other areas of science.

37. **Peter Woit**
June 17, 2008

Christine and Tom,

I don't think a generalized denunciation of multiverse and extra dimensions research is at all helpful. All it does is convince string theorists that their critics are completely close-minded and don't understand what they are trying to do. You have to look at exactly what the research in these fields is, it's not so easily dismissed without considering exactly what is going on.

In the case of extra dimensions, first of all there are successful uses of the idea (see AdS/CFT). Mostly though, it's not pseudo-science, just unsuccessful science. Multi-dimensional models in principle typically make predictions, the problem is that they don't agree with observation. You end up having to adjust your model so as to not violate known experimental results, making it untestable and not explaining anything.

In the case of the multiverse, one can imagine testable versions of the idea, but one often sees people pushing research programs that all evidence shows are inherently untestable. This deserves to be called pseudo-science.

38. **anon.**
June 17, 2008

"… it's not pseudo-science, just unsuccessful science. …"

It's financially successful in being marketed. So it gets citations and research grants. The number of researchers exceeds the critical mass for a growing discipline: reading, peer-reviewing and citing one another. From these criteria, it is a scientific success.

39. **Christine**
June 17, 2008

Peter Woit,

It may appear to others that my "generalized denunciation" of these issues are closed-minded. Well, I don't care to be judged that way, simply because I know how I am better than anyone else, and I know that I am an open-minded person. I have run and run blogs that widely accept any civilized comments on all these issues, even if I strongly oppose one concept or another.

As I mentioned, I am not philosophically inclined to the extra-dimensional programme, although I was much more interested in the past. And I never said that it cannot be tested. My general remarks about extra dimensions were purely philosophical.

Concerning multiverses, as I wrote, I see it clearly untestable by definition, because by definition there is only one universe, which encompasses all nature. I see no logic in talking about multiverses poping in and out of "existence" "elsewhere". This is my starting point, but it does not mean that I don't want to hear anything about it or that I am not interested in understanding the arguments further.

String theory is testable and should be tested, and it is scientific as far as I understand it. But I presently am interested in other ideas. I don't really care whether other people want to work on this subject as far as they act as honest scientists and face the problems of their discipline with dignity. (I am *not* saying that string theorists are dishonest or lack dignity. This should be applied to any scientist, or any person for that matter).

Yes, I do have a strong philosophical position that multiverses are things of the imagination, and hence one will have to convince me that it is a scientific problem. Since up to this moment I have not seen any convincing argument, I keep my position. If people want to try to convince me, they are invited to post on my blog; I have specific entries on these matters. Search for, e.g., "the universe" and "what is science", etc. But I strongly advise that if you don't have a really sound argument, never mind, I'd rather run a low traffic blog.

40. **Peter Woit**
June 17, 2008

Christine,

I'm sorry if my comment came off as hostile, I'm well aware that you are a quite open-minded person, well-informed about these issues, and that your blog does a good job of reflecting this.

You're right that the overriding problem with the multiverse idea is that the other "universes" are by definition not directly observable to us. So anyone who wants to talk about them has to come up with an explanation of how multiverse research is going to lead to an experimental test. My point is just that multiverse proponents do have answers to this, and one needs to look at them and address them. I think when one does, one finds that they are completely unconvincing. The kinds of answers they have are:

1. A version of string theory will be found that explains the standard model convincingly, while at the same time also implying the existence of other universes corresponding to other states of the the theory. Knowing one ground state of the theory will tell us what the theory is, and indirectly evidence for the other ground states. Problem with this of course is that, after a quarter century string theory has failed completely to explain anything about the standard model.

2. Statistical analysis of string vacua will make testable predictions, again giving indirect evidence that the other states in the statistical ensemble exist. Again, this is a research program that has gone no where, and has zero evidence supporting it, or giving any encouragement that anything can come of it.

3. Construction of models with observable effects of pre-inflationary times. Such models typically are rather contrived, and involve an extremely small amount of information about this pre-inflationary "other universe", such as one number, the only thing surviving from some very complex earlier state. Actually I don't think this really works, since you're talking about the earlier history of this universe, not another one. Again, you can claim that finding evidence of a pre-big-bang state consistent with multiverse ideas of existence of lots of baby universes is indirect evidence for them, but at this point this just seems to me to be wishful thinking.

41. **Chris Austin**
June 17, 2008

Hi Tom W.,

" … can you (as a serious physicist) make it clear to me (the public) where the scientific basis for extra dimensions can be found?"

I hope Peter won't mind if I try.

Experimental evidence has been steadily mounting since 1915, when Einstein proposed his General Theory of Relativity, that gravitational fields are properly described as a curvature of space and time. A logical consequence of this description, already recognized by Theodor Kaluza in 1919, is that there could be extra dimensions of space, curled up too small for us to see them.

To see something small, it is necessary to look at it with something – photons, electrons, neutrons – whose wavelength is smaller than the thing we are trying to look at. From Planck's relation $E = h \nu$, where E means energy, h is Planck's constant, and ν means frequency, and the relation between frequency and wavelength, which for photons, and also for massive particles, when their energy is sufficiently high, is $\nu = c / L$, where c is the speed of light and L means wavelength, it follows that to see very small things we need very high energies.

The Large Hadron Collider, due to start up at CERN in the next few months, will enable us to see smaller things than ever before, down to around 10^{-19} metres, by looking at them with the quarks and gluons in protons colliding at extremely high energy. This does not mean that we will see such small things immediately. Rather it will be like gradually increasing the level of illumination in a room that was initially completely dark: things will gradually appear out of the murk as the accumulated data increases. There is a significant possibility that the LHC will eventually see small extra dimensions of space, curled up to a size of around 10^{-19} metres, that are too small to have been detected up to now, and I will try to explain why this is.

The two most fundamental experimentally established theories of physics that we have at present are the Standard Model of the strong / electroweak interactions, which describes everything that we observe except the gravitational field, and Einstein's theory of gravity, which describes the gravitational field and its interactions with the energy and momentum of matter. As I tried to summarize in my comment above, when we work out the logical consequences of the two theories together, they make a prediction that is disastrously wrong, namely that the universe curls up so that its radius of curvature is around 10^{-35} metres, which is roughly as small in comparison to the smallest specks of dust we can see with the naked eye, as those specks of dust are in comparison to the actual size of the universe. This means that the two established theories must be part of a larger picture, and a very important part of the larger picture is missing.

The reason the two established theories make this wrong prediction, is that the fields associated with the particles of the strong / electroweak Standard Model, in the same way that the electromagnetic field is associated with the photon, have nonvanishing fluctuations even in empty space, and these fluctuations have nonvanishing energy. The energy of these fluctuations acts as a source for the gravitational field in the same way as massive objects do, and this results in the universe curling up to a size around the square root of $G h / c^3$, where G is Newton's constant. This size is around 10^{-35} metres, which is roughly what is known as the Planck length.

For particles like the photon, the W and Z particles that transmit the weak interactions, the gluons that transmit the strong interactions, and the graviton, which are collectively called bosons, the leading contribution to the energy of their fluctuations in empty space is always positive, while for particles like the electron, the muon, neutrinos, and quarks, which are collectively called fermions, the leading contribution is always negative, so there is in principle the possibility of a cancellation between the two, but this does not happen for the strong / electroweak Standard Model, nor for the Standard Model plus gravity. However in 1974, Bruno Zumino demonstrated that in field theories with a special property called supersymmetry, the contributions to the energy of fluctuations in empty space cancelled between bosons and fermions exactly, for the leading contributions, and also for all further contributions from more and more complicated processes. Supersymmetry means that the formula for the energy density of the fields, averaged over space and time, which is called the action, is unchanged when certain small multiples of the fermion fields are added to the boson fields and conversely. Because of Zumino's proof, it seems reasonable to expect that supersymmetry is part of the missing part of the larger picture.

Zumino's proof does not apply, however, to field theories with supersymmetry that involve the graviton, which are called supergravity theories, and with one single exception, whose fluctuations in empty space can only have exactly zero total energy, supergravity theories can have zero *or negative* total energy of their fluctuations in empty space, so that the energy of their empty space fluctuations is not much better controlled than for theories without supersymmetry. The single exception is supergravity in eleven dimensions, that is ten space dimensions and one time dimension, which is the largest number of dimensions in which supergravity can occur. Supergravity cannot occur in more than eleven dimensions, because the number of degrees of freedom of the gravitino, which is the fermion field of which a certain small multiple gets added to the gravitational field in a supersymmetry operation, increases much more rapidly than the number of degrees of freedom of the graviton, as the dimension of spacetime increases. The result of this is that while supergravity in four spacetime dimensions can involve up to eight gravitinos, in ten dimensions it can only involve either one or two gravitinos, in eleven dimensions it involves exactly one gravitino, and in more than eleven dimensions supergravity cannot occur at all, because the gravitino has too many degrees of freedom.

In consequence of being an extreme case, supergravity in eleven dimensions has completely exceptional properties. Its action is unique, and the pieces fit together like a perfect Chinese puzzle. It is the only field theory involving gravity whose fluctuations in empty space necessarily have exactly zero total energy. When one of its ten space dimensions is curled up into a very small circle, and a certain solution of its field equations, which looks like a membrane, is wrapped around this circle, it becomes the type IIA superstring in ten spacetime dimensions, and it can also become each of the other four superstring theories when one or more of its ten space dimensions are curled up or folded in appropriate ways, and certain modifications fixed by self-consistency on any folds are made. Its quantum theory, which is called M-theory, has a vast array of consistent solutions in which seven of the space dimensions are curled or folded up into a very small size, while the remaining three space dimensions and the time dimension are very large and nearly flat, like the familiar dimensions of space and time that we know.

In many of these consistent solutions, there are fermions and bosons on the four large spacetime dimensions that look very similar to those in the strong / electroweak Standard Model, in the sense that exactly the same fields occur as in the Standard Model, and they have exactly the same types of interactions as in the Standard Model, but the masses of the particles, and the strengths of interactions, defined by numbers such as Newton's constant and the fine structure constant, are generally different from those in the Standard Model. However only a tiny fraction of the possible consistent solutions of this type have been examined so far, so it seems reasonable to expect that the larger picture, of which the strong / electroweak Standard Model and Einstein's theory of gravity are the two parts that have been established experimentally up to now, will be a consistent solution of M-theory of this type.

The vast number of consistent solutions of M-theory which, for everything larger than around 10^{-19} metres in size, look similar to the strong / electroweak Standard Model plus Einstein gravity in 3 + 1 dimensions, but differ in detail, have been characterized as a "landscape", and leave us with the practical problem of finding out where in that "landscape" we live. The parts of the "landscape" that have been investigated up to now fall into some ten or so broad families, that differ very greatly from one another for things smaller than around 10^{-19} metres in size. It will thus be possible to distinguish between some of these broad families at the Large Hadron Collider, and exclude some of them in favour of others.

The reason it is certain that the Large Hadron Collider will be able to distinguish between some of the broad families of solutions is that the strong / electroweak Standard Model becomes very stressed at the energies that will be studied at the LHC. One of the particles predicted by the Standard Model, called the Higgs boson, has never been discovered, and the logical consistency of the parts of the Standard Model that have been confirmed experimentally, requires that the Higgs boson, or something more complicated to serve in its place, must be light enough to be discovered at the LHC. Furthermore, the mass of the Higgs boson in the Standard Model is unstable to quantum corrections that tend to increase it greatly, so if the Higgs boson is discovered, then something more complicated must also be discovered that stabilizes its mass.

Whatever more complicated things are discovered, either together with the Higgs boson or in place of it, will distinguish between some of the broad families of solutions. In some of the broad families, the seven extra dimensions are large enough to be seen at the LHC, while in others they are not. In some of the families, there will be string-like excitations similar to the Regge recurrences observed at much lower energies in the strong interactions, while in others there will be none. In some of the families, the size of the extra dimensions is much *larger* than 10^{-19} metres, in fact up to a millimetre, but the Standard Model particles can only move in a small part of them, of size around 10^{-19} metres, which is why the large extra dimensions have not yet been detected, whereas the graviton and gravitino can move in their full extent. This would explain why the force of gravity is so much weaker than the other forces, for example the gravitational attraction between two protons is around 10^{-40} times weaker than the electrostatic repulsion between them, due to the gravitational force being diluted by the larger volume in which the graviton can move. In these families, the strength of the gravitational force increases extremely rapidly with energy as the LHC energy is approached, becoming comparable in strength to the other forces at around the LHC energy, and loss of part of the collision energy due to radiation of gravitons into the large parts of the extra dimensions will be observed at the LHC.

Best regards,
Chris

42. **Christine**
June 17, 2008

Peter Woit,

No, your comment did not came off as hostile, perhaps I was a little too defensive.

I appreciate that you have itemized your main criticisms to the “multiversism”. If I may, I suggest that you produce a FAQ to your arguments. It would avoid that you keep repeating yourself. There is a lot of information in your blog that deserves to be assembled into one place for easy access.

What concerns me is the recent edifice of joining string theory and multiverse scenarios (in addition to the intrusive anthropic arguments). I think this is a major shortcoming, but my discomfort is purely philosophical, because, as I mentioned previously, the multiverse idea does not convince me as scientific to begin with. I wonder whether only a minority of string theorists are willing to embrace such an edifice, or whether in fact this is a major trend.

43. **Eric H**
June 17, 2008

Chris,
Your latest comment explains where you are coming from theoretically. However, it seems to me you are just repeating standard theory from certain books, some of which are right and some of which are probably wrong.

While I agree that supersymmetry explains some things in particle physics it seems to do a lot more damage at theories of large scales, i.e.,gravity. It also must be remembered that general relativity can be considered proven while supersymmetry cannot. This isn’t to say that general relativity won’t probably be eventually enfolded into a larger theory that explains things such as the acceleration due to dark energy, (not to be confused with the vacuum energy or zpf). But my view is that supersymmetry directly contradicts GR and thus can’t enfold GR within it. In this way supersymmetry can be considered a good, but very flawed theory for certain interactions. Let me say it again: supersymmetry has not been proven.

Finally, the basic problem seems to come down to the problem of the universe being nearly “flat” but not quite. Supersymmetry addresses the problem by assuming “anti” particles and forces to everything. That ball got rolling because there actually are anti-particles with the same mass but opposite spin in all their internal components. But you are basically extrapolating an “anti” universe from the tinyest bit of evidence. And even then it doesn’t explain the small bit of “material” remaining that makes the combined positive and negative universes flat. It’s not good science but just a supposition that actually creates more questions than answers. Good science is when after given suffient time what remains in the wake of a new theory are “less” unanswered questions from the original questions, not more.

44. **Eric H**
June 17, 2008

“And even then it doesn’t explain the small bit of “material” remaining that makes the combined positive and negative universes flat.”

make that not quite flat.

45. **Eric**
June 17, 2008

Eric H.,
You apparently have a misunderstanding about what supersymmetry is about. Supersymmetry does not assign an anti-particle to every other particle. Supersymmetry is a symmetry which relates bosons to fermions. Thus, for the case of N=1 supersymmetry every fermion has a scalar partner, while every boson has a fermionic partner. In the case of supergravity, the spin 2 graviton is paired with a spin 1/2 gravitino, and gravity emerges from elevating supersymmetry to a local gauge symmetry. Your statement that supersymmetry contradicts general relativity is completely wrong.

46. **Eric H**
June 17, 2008

Eric,

Have they found any of these superpartners experimentally?

47. **Eric**
June 17, 2008

Give LHC a couple of years….

48. **woit**
June 17, 2008

Enough unenlightening supersymmetry warfare.

Chris laid out some of the standard lines of speculation that lead to the theoretical picture that string theorists are trying to sell today. The problem is that it’s a long line of speculation, which could be wrong at many places, which has never managed to make contact with the real world. There’s no particular reason to believe that extra dimensions explain electroweak symmetry breaking, they don’t explain any of its known features, or predict what the LHC will see.

So, I still maintain that extra dimensions are science, just (scientifically) unsuccessful science. We’ll see what happens at the LHC, I just hope that if extra dimensions or supersymmetry don’t turn up there, we can finally stop having the field of particle theory dominated by these ideas.

49. **Eric**
June 17, 2008

Peter,
I hope that whenever supersymmetry is found at LHC that you will disappear.

50. **Observer**
June 17, 2008

Peter,

In a previous comment you say: "In the case of extra dimensions, first of all there are successful uses of the idea (see AdS/CFT). Mostly though, it's not pseudo-science, just unsuccessful science"

Is there any empirical evidence in support of the AdS/CFT correspondence to promote it at the level of a successful theory?

Best regards,

Observer

51. **Peter Woit**
June 17, 2008

Observer,

AdS/CFT gives a very non-trivial calculational method for dealing with the strong coupling behavior of certain gauge theories, and as such is a success. Whether it can be turned into a reliable method for dealing with the physical case of QCD remains to be seen.

In this case the problem is not finding the right theory that describes the real world, we have strong evidence for QCD. But this still leaves the problem of actually calculating things, and that's also part of physics.

52. **Observer**
June 18, 2008

Peter,

Thanks for the reply.

To my knowledge, the AdS/CFT model applied to QCD goes by the name of holographic QCD. It is claimed to be a computational tool that is superior to Chiral Perturbation Theory when it comes to the strong coupling regime. But, again to my knowledge, holographic QCD has not been proven to yield predictions that are consistent with observations.

Is my understanding correct?

Best regards,

Observer

53. **Peter Orland**
June 18, 2008

Observer,

I'd like to try to answer your question concerning holographic ideas in QCD. I hope you can follow my attempt to give a basic explanation. Perhaps Peter won't mind that this doesn't have anything to do with the big bang (it seems that this comment thread is already very tangled, so I don't think I am messing it up too much).

AdS/QCD or whatever one calls it is essentially a bare-strong-coupling scheme. There is another such scheme, devised a long time ago, which is to do the strong-coupling expansion (NOT Monte-Carlo/numerical methods) on the lattice. Both of these have yielded some insights into quark confinement and chiral-symmetry breaking, and led to interesting lines of inquiry, but both have a serious problem.

In such schemes, there is a dimensionless parameter, called the bare coupling. This is not the same as the physical or effective coupling. To understand how real QCD works, this bare coupling must be taken to zero as an ultraviolet cut-off is removed. Unfortunately, nobody can do this yet.

Here is a semi-technical explanation, which you may want to skip. The reason the bare coupling must be taken to zero has to do with how QCD renormalization works. If we want to keep experimental parameters (like cross sections or masses) fixed as we remove the cut-off, the bare coupling "runs", i.e. becomes a function of the cut-off. It so happens that the bare coupling runs toward zero as the cut-off (in momentum units) diverges. SIDE REMARK: You may already know that the renormalized/effective coupling also runs, as we look at different energy scales (not cut-offs), and it runs in the same way as the bare coupling.

No calculation in the holographic approach to QCD can be taken seriously, unless someone figures out how to deal with arbitrarily small bare coupling (which means large curvature). Real QCD means infinitesimal bare coupling. My impression is that few string theorists (perhaps none) are working on this problem, because it is extremely hard.

In the light of the above, I think AdS/QCD and the older strong-coupling lattice approach should be viewed only as imperfect models of the strong interaction. This does not mean that they are unworthy of study. They are not, however, real QCD.

54. **observer**
June 18, 2008

To Peter Orland,

Thanks for your reply and explanation.

Best regards,

Observer

55. **M.Wang**
June 19, 2008

I am truly curious to see some real debates on the physics related to the direction of time paradox, so pardon me for interrupting your lively discussions about String (an issue that I consider moot).

The central argument in this paradox concerns the entropy of the pre-inflation universe. It is commonly stated (by Penrose, for example) that it has to be even lower than just after inflation ends. The reasoning that leads to this conclusion involves the statement that the pre-inflation universe contains the same number of quantum states as the current universe, because "according the rules of quantum mechanics, the total number of microstates in a system never changes" (a quote from the SciAm article).

But this is very suspect. Quantum mechanics is known to be incompatible with gravity (or, equivalently, dynamic space-time). Trying to trace all microstates backward through inflation is plain nuts. Furthermore, the current universe has undergone 14 billion years worth of quantum de-coherence. Maybe someone has a way of estimating the effect of all the quantum de-coherence on the microstate count, and I sure would love to read it, but I doubt the answer can be as simple as "no effect" because quantum de-coherence is in essence the APPARENT decoupling of entangled states when the particle in question interacts with cold, localized, massive subsystems (yes, like those objects that formed AFTER the inflation). The wave function is truncated in an (albeit extremely accurate) approximation. The new EFFECTIVE quantum description of the system remains valid for all practical purpose, but the number of microstates does not seem to be conserved, or maybe someone can enlighten me otherwise.

Oh, by the way, I am aware of the fact that the universe always has had exactly ONE state. Talking about entropy, however, requires the invocation of ensembles (or something similar, although the multiverse concept may be a step too far). Counting the microstates, therefore, involves solving the effective quantum Hamitonian after de-coherence. Now I really do not see how the count can be possibly conserved.

Even if the microstate count in the pre-inflation universe is indeed high enough, we still cannot safely jump to the conclusion that "among all the different ways the microstates of the universe can arrange themselves, only an incredibly tiny fraction correspond to a smooth configuration of ultradense dark energy packed into a tiny volume" (again, a quote from SciAm). That statement is true if one assumes that there is no interactions among the incredibly dense collection of ultra-heavy particles packed into a universe the size of a penny, but that is such a dumb assumption that I do not presume anyone can seriously make it. In fact, smoothness seem almost inevitable with any meaningful collisions going on, so if anyone understands how that SciAm quote was arrived at, please enlighten me by all means.

56. **Observer**
June 19, 2008

Dear M. Wang,

Extrapolating thermodynamic and quantum concepts all the way to the Universe scale is highly speculative and should not be taken seriously. Likewise, assuming that both statistical and quantum physics continue to be valid in pre-inflationary Universe lacks any foundation.

57. **Christine**
June 19, 2008

The meaning of the word "speculation" appears to have changed in recent times.

Atiyah on Atiyah-Bott

June 11, 2008
Categories: Uncategorized

This week I'm in Montreal attending a [conference in honor of Raoul Bott] (who I wrote about in some detail [here] a couple years ago).

On the first day of the conference, a documentary film about Bott made by his grandaughter Vanessa Scott was shown, and there was a panel discussion about the man and his influence on students and collaborators. Bott was both a wonderful mathematician and human being, and many people at the conference paid tribute to him as someone who encouraged them and taught them beautiful and deep mathematics. Quite a few commented on visits to him and his family at their summer place on Martha's Vineyard. It adjoined a clothing-optional beach frequented by Alan Dershowitz among others, a beach where Bott was supposedly known as the "Mayor".

Bott was very much involved with physics later in his career, and two physicists spoke at the conference: Cumrun Vafa on topological strings and Edward Witten on the 6d QFT point of view on geometric Langlands. I greatly enjoyed both talks, but I suspect they were pretty difficult for the mathematicians in the audience to follow, covering a great sweep of material bringing together new mathematical developments not understood by most mathematicians with QFT and string theory techniques far from their background.

Michael Atiyah gave a truly wonderful talk to open the conference. He described how his friendship with Bott had covered a period of 50 years, from 1955 until Bott's death in 2005. From 1964-1984 they did some of the best work of their careers together, and Atiyah tried to summarize the high points of this, as well as point out problems their work raised that he sees as still open and worthy of investigation by a new generation. Editions of the collected works of both of them are available that include their commentaries on the papers, and these are very much worth reading. Each of them is a masterful expositor, so their joint papers are uniformly models of clarity.

Atiyah broke things up into the following main topics:

- Bott Periodicity

In work with Hirzebruch, Atiyah realized that Grothendieck's construction of K-theory in algebraic geometry could be turned into a generalized cohomology theory in the topological category. To make this work uses Bott periodicity in a crucial way, to show that $K(MxS^2)$ is the tensor product of K(M) and $K(S^2)$ for any compact manifold M. Atiyah realized he didn't know how to prove this, and got Bott to produce an appropriate proof, which appeared in a paper of Bott's in 1959. The paper is written in French, clearly not by Bott, and Atiyah says he still doesn't know who translated it into French for Bott.

Atiyah and Bott worked together on extending the Atiyah-Singer index theorem to the case of manifolds with boundary, where the issue of how to handle the boundary conditions so as to get a good index problem is a subtle one. As part of this, they needed a new, more "elementary" proof of Bott periodicity, finally finding one that "even MIT faculty could understand". This is periodicity for the unitary group and crucially uses Fourier analysis. Atiyah gave as a problem deserving attention that of extending the proof to the case of the orthogonal group, where the use of Fourier series would have to be replaced by the representation theory of O(N). Multiplying Fourier series becomes taking the tensor product of representations, which is much more non-trivial to deal with.

- Heat Equation Proof of the Index Theorem

Using the McKean-Singer formula, one can relate the computation of the index of a differential operator to the asymptotics of a related heat equation. Patodi and Gilkey had carried this through using complex and skillful algebraic calculation, but Atiyah and Bott felt they couldn't understand these proofs, so with Patodi came up with a new proof, one that just used Weyl's invariant theory for the orthogonal group and the Bianchi identities of Riemannian geometry.

As a problem for the future, Atiyah listed finding a better understanding of the relation of the Atiyah-Bott-Patodi argument with the supersymmetric quantum mechanics proof. More explicitly, he conjectures that one should be able to just use invariant theory, but perhaps invariant theory of the infinite dimensional group Diff(M), of a sort advocated by Gelfand.

- Fixed Point Theorems

The Atiyah-Bott fixed point formula computes the Lefschetz number one gets in the context of index theory and a mapping of the manifold to itself (coming for instance from a group action) in terms of data at the fixed points of the mapping. This has many beautiful applications, including a new proof of the Weyl character formula. The formula is sometimes known as the "Woods Hole formula", since Atiyah and Bott conjectured it at a conference at Woods Hole, where certain experts told them it couldn't be true, since they had computed counter-examples. Atiyah didn't name names, but described the experts involved as now claiming to not remember this. "They deny it to a man" he said, but he remembers it distinctly while being in the frustrating position of having nothing written down to provide incontrovertible documentary evidence.

One generalization of the fixed point formula shows that for manifolds with circle action the index is zero, and Atiyah mentioned Witten's extension of this to the case of a loop space, leading to a relation to modular forms and the subject of "elliptic cohomology". This continues to be an active subject, with Mike Hopkins and others developing the theory of "topological modular forms", something that has shows interesting relations to number theory. Atiyah described a "moral bet" between him and Andrew Wiles about whether QFT will ultimately influence number theory. Wiles thinks not, but Atiyah believes it will happen, and hopes to be around long enough to find out if he is right.

- Yang-Mills and Algebraic Curves

Morse theory and equivariant cohomology were two of Bott's favorite tools, and he and Atiyah did some wonderful work applying these to the case of Yang-Mills theory in two dimensions. Here the Yang-Mills functional is a Morse function, the space of connections is a symplectic manifold, and the reduced space for the gauge group action is an important mathematical object that can be thought of as the moduli space of flat connections, or of stable holomorphic bundles on the 2d surface. Their main result, a calculation of the Betti numbers of this space, reproduced earlier results coming from a very different approach, that of using algebraic curves over finite fields and the Weil conjectures.

As a problem for the future, Atiyah asks if there is some infinite-dimensional version of the Weil-conjectures, and some QFT where the Feynman integral is analogous to Tamagawa measure. He says he has been thinking about this off and on for 30 years, hasn't found anything satisfactory, but offers the problem as a gift to younger mathematicians, as long as they let him know if they solve it. For some recent work related to this, see [here].

- Hyperbolic Equations and Lacunae

Atiyah described work of his with Bott on this topic as "performed under a subcontract" with Garding.

Finally, Atiyah commented on how his mathematical style was quite different than Bott's with Bott always advocating an "old-fashioned" way of proceeding, involving concrete formulae, where Atiyah favored "new-fangled" abstraction, only writing down formulae when forced to by Bott (and then discovering that this gave important insight). Later he found himself in the opposite position when working with Graeme Segal, with respect to whom he was the "old-fashioned" one, resisting abstraction. He commented that Bott and Segal had written a paper together, and he was shocked to see that such a thing was possible.

He noted that he had met many fascinating people through Bott, including one of the world's best known mathematicians: Tom Lehrer. Finally, he ended with the comment that, while Bott was no longer here, the great thing about doing math the way he did it is that you become immortal.

Comments

1. **mathphys**
   June 12, 2008

   Peter, Do you know if the talks were recorded and/or whether the proceedings will be published?

2. **Peter Woit**
   June 13, 2008

   mathphys,

   The talks weren't recorded as far as I could tell. Don't know if there will be any effort to put some of the slides on-line. There will be a volume of papers at some point.

3. **Fabien Ngo**
   June 16, 2008

   I attented the Bott legacy conference too. I noticed that Professor Woit was on the list of participants but didn't find him unfortunately. Concerning the talks, most of them were very nice and the speakers tried to make them as clear as possible. Vafa's talk was very hard and i wasn't able to understand it. I wanted to ask him question but he disappear imediatly after his talk (So if someone can help me to understand what he talk about that would be nice).

   The conference wasn't recorded but CRM(Centre de recherche mathématique) who organised the conference will publish the proceedings of the conference as soon as possible.

4. **Peter Woit**
   June 16, 2008

   Hi Fabien,

   Sorry I didn't get to meet you at the conference.

   For some fraction of what the Vafa talk was about, one reference with lots of detail at various levels is the book "Mirror Symmetry" from the AMS, edited by him and Zaslow.

5. **Fabien Ngo**
   June 17, 2008

   Thank you very much for the reference. The book is big but i'll try to take a look.

Is the Universe Actually Made of Math?

June 17, 2008
Categories: Uncategorized

The relationship between mathematics and physics is a topic that has always fascinated me, and today I noticed two interesting blog postings related to the topic. The first was Ben Webster's posting inspired by a recent XKCD comic. The discussion in the comment section is well worth reading, especially the contributions from Terry Tao.

Over at Backreaction, Sabine Hossenfelder discusses an interview with Max Tegmark from the latest issue of Discover magazine entitled Is the Universe Actually Made of Math?. Much of the discussion is about Tegmark's comments on how he dealt with the potential danger to his career caused by his unconventional publications on the "Mathematical Universe Hypothesis". This says that

> Our external physical reality is a mathematical structure

I've always had an extreme case of mixed feelings about this, thinking that Tegmark manages to bring together the extremely deep and the extremely dumb. He embeds this as "Level IV", the highest level, of the multiverse, and multiverse mania is one reason he has gotten attention for this and not had it dismissed as crackpotism. The idea he is pursuing is that any mathematical structure can be thought of as a "universe", and we just happen to be in some random one of these. This seems to me to be pretty much content-free, and the attempts to fit it into more conventionally popular multiverse studies don't help.

At the same time, this does get at an incredibly deep problem, that of the relationship between mathematical structures and physical reality. Some of the central mathematical structures that mathematicians have discovered have turned out to be identical to those found by physicists pursuing models of fundamental physics. This has happened in several very striking ways over the years. Thinking of the universe as a mathematical structure has turned out to be extremely fruitful, both for mathematics and for physics.

What is important though is that not all mathematical structures are equally important, central, or interesting, and this is the crucial point that Tegmark seems to me to be missing. Once you learn enough mathematics, you find certain recurring themes and deep structures throughout the subject. What fascinates me is that these often also turn out to be central in theoretical physics. Tegmark just accepts every mathematical structure as equally important, creating a huge undifferentiated multiverse where we occupy some random anthropically acceptable point. But the evidence is that the mathematical structure we inhabit is a very special one, sharing features of the very special structures that mathematicians have found to be at the core of modern mathematics. Why this is remains a great mystery, one well worth pursuing from both the mathematician's and physicist's points of view.

Comments

1. **Frank**
   June 18, 2008

   An acquaintance of mine has a simple but deep criticism of Tegmark's ideas. He says that all mathematical structures are built and based on sets. This is especially the case for all structures looked at by Tegmark. But on the other hand, it is not clear at all whether nature is a set. In fact, there are many reasons that point to the opposite conclusion.

   He says that at high energy, it is not clear that a "set" is the correct description for nature, it rather looks as if it is not. If nature is not a set, Tegmark's ideas lose their base.

2. **Paul Jackson**
   June 18, 2008

   I'm amazed that Max Tegmark seems to entertain the notion that mathematics is constituent of the physical world, somehow waiting to be discovered "out there". Is it not obvious that mathematics is an invented language that we chattering African apes have evolved to describe the mysterious world we find ourselves in? No wonder its structure has evolved to closely resemble that of the physical world.

3. **Fabien Besnard**
   June 18, 2008

   "Is it not obvious that mathematics is an invented language that we chattering African apes have evolved to describe the mysterious world". It is not obvious, to say the least. Whole books have been written about this, and I recommend this one : "Matière à pensée", a dialogue between Alain Connes and the neurologist Jean-Pierre Changeux. I can also point you to Omnès book "Converginf realities".
   I think that when you ask yourself what "real" means, what criterion permits to classify something as real, what comes to mind is logical consistency. Everything else seems to be ill-defined intuitions. I talked about this subject in more length here : http://math-et-physique.over-blog.com/article-1591626.html (in french). I'll try to put a trackback but for some reason it never works…

4. **anon.**
   June 18, 2008

   All applied mathematics for real world physics is only approximate:

   1. Newtonian physics only has exact analytical solutions for two-body interactions, whereas there are many bodies present in the universe. Poincare chaos arises for orbits of more than two bodies, where each affects (alters) the orbit of the other as it moves. There is also a quantum chaos from the random exchange of field quanta that causes the electromagnetic interaction between electrons and protons, which on small scales is random (on big scales the large number of field quanta interaction statistics smooth out to give the deterministic classical Coulomb law). This prevents deterministic calculation of electron orbits inside the atom.

   2. General relativity's stress-energy tensor uses an artificially smoothed distribution of mass and energy instead of representing the real particulate (discontinuous, i.e. atoms and quanta) distribution of matter and energy, to create an equally false smooth source for the Riemann curvature. It just ignores the QFT idea that gravity field quanta (gravitons) are exchanged in discrete interactions, not continuous acceleration (smooth curvature).

   3. Even if you just consider simple addition, counting two electrons, you haven't an exact mathematical model with 1+1=2 for two times the same thing, because the electrons are all slightly different in their motions and by the uncertainty principle in principle you can't ever find their exact positions and momentums. So they will have slightly different velocities and therefore slightly different masses. So you're not adding up exactly the same real thing. To make the point clearer, if you add up apples (or if you count sheep), you are adding up things which are approximately similar, but not exactly the same. Two similar looking items will differ at the atomic scale. So addition is *only ever exactly true* when dealing with tokens like money, an invention due to mathematics.

   It is impossible even in principle to get exactly true input data in the real world from making measurements. Also, it's impossible to make exact predictions, because all applied physics calculations for the real world involve making approximations. So the universe isn't intrinsically mathematical. You can't get completely exact input data, and – even if you did know exact initial conditions – the mathematics used to model real (complex) phenomena is an approximation only.

In order for the universe to be intrinsically mathematical, it would be necessary in principle for there to exist some way of *exactly* representing the real world using mathematics, instead of relying on approximations and statistical wave equations. Mathematics is in principle at best just an approximation to the universe, so the universe can't – even in principle – be intrinsically mathematical in nature.

5. **Fabien Besnard**
   June 18, 2008

   'In order for the universe to be intrinsically mathematical, it would be necessary in principle for there to exist some way of exactly representing the real world using mathematics'.

   Isn't it what physicists are after ?

   Anon, you seem to confuse two kinds of approximations : 1) the fact that our best theories are still approximations 2) the approximation in data. I don't see any relation between point 2 and the issue of the true nature of physical objects. As for 1), what is your argument for saying that there is no such thing as an exact mathematical description ? We do not have such thing at hand right now, but the simplest way to explain the fact that there exist excellent mathematical models of reality is that these models are approximations of a complete mathematical description.

   Finally, I think that one should distinguish between approximation and abstraction. Mathematical concepts are abstracted from 'familiar objects' ('familiar objects' might include other mathematical concepts), in a way I think is akin to chemical purification. Then these concepts are assembled to form a "synthetic product". These synthesis can produce mathematical theories which (perhaps crudely) model parts of the "real world". But both the "real thing" and the mathematical models are of the same nature : there are made of "atoms", which in my metaphor are elementary mathematical concepts.

   I speak about this "chemical metaphor" in the link I gave above.

6. **Christine**
   June 18, 2008

   Apparently, his philosophy is to equate the physical world with mathematics (yes, equate, not a sort of mapping between the two, nor a kind of approximation or representation or abstraction or whatever). If I understand Tegmark's point correctly, in general terms he seem to argue that this direct equality solves the philosophical problem of whether there is an ultimate reality. He says there is one and it is pure mathematics.

   I may have misunderstood it all since I have only read a simplified paper that he published some time ago. In any case, I didn't find any of his arguments brilliant or convincing. Looks like a very bad philosophy to me.

   The fact that we can describe physical phenomena through mathematical *reasoning* (no matter limitations concerning approximation of data, etc) is something much deeper to me and equating both is no solution (again, to me). It's like turning a difficult question into a trivial one as the best way to actually avoid it.

   Why am I being harsh?

   My formative years in astrophysics were the 90's, where I have seen cosmology turn into a real, quantitative science, the beginning of the "precision era" of cosmology. What I see today, however, concerns me deeply. I see well-known cosmologists, Tegmark and Sean Carroll being two examples that come easy to my mind, whose work I came to appreciate for many years, to start drifting into a speculative world, which has nothing to do with cosmology or science.

   Now let me be clear on this. I have a deep passion for philosophy and a great appreciation for the great classical philosophers, and actually I firmly believe that science must go hand in hand with philosophy. So I have nothing *against* a cosmologist or anyone attempting a serious work in philosophy or any other kind of speculative adventure, up to the point that he or she makes that absolutely clear. One should not confuse one thing with the other. Philosophy and science can go hand in hand, but one must be clear of what he/she is talking about, and make no confusion between the two disciplines.

   Concerning "the potential danger to his career caused by his unconventional publications", I take a very pragmatic approach. If a PhD student of mine in cosmology (let us say) started to drift into philosophy to the point of interfering with his/her research, no matter how fascinating his/her ideas were, I would have a conversation with him/her in order to make it clear that he/she would have to make a choice between these fields in that point of their formative years. After his/her PhD, he/she could choose whatever he/she wants to do, it doesn't concern me. I see no point in making so much fuss about this.

7. **David Lloyd-Jones**
   June 18, 2008

   .

   I've always liked a remark that physicist Sir James Jeans made back around 1935: "The more we study it, the more it all looks like a dream."

8. **Richard Oldani**
   June 18, 2008

   What one does not put into the equation will not finally be given by the mathematics. James Franck

   IOW if you begin with math and try to go backwards you end up with nothing.

9. **Joao Leao**
   June 18, 2008

   Peter,

   You make a very crucial observation that deserves serious attention namely that physics appears to select mathematical structures to faithfully represent it that end up being recurrently isomorphic among themselves! This indeed is what makes Tegmark's call for `mathematical democracy of all structures' so totally sterile and silly! The notion of a "universe made of math" is not new: it is an old creed called pythagorianism, sure to be periodically revived by people who lack (better) ideas. Tegmark's profligate version is also not original: it was advocated by the argentinan philosopher Mark Balaguer in his book
   "Platonism and Anti-Platonism in Mathematics" as Full-Bloodied Platonism, though I believe Plato to be a lot less sanguine in his advocacy! You are right that this notion caters to the whole Landscape mentality that anything goes and it is basically a call to abandon all criteria of adequacy of mathematical speculation to empirical evidence, a notably self serving strategy for people who don't care to have their "bright" ideas falsified.

   The issue of whether (logical) self-consistency distinguishes mathematical structures that apply to physics from those that don't has been raised by Hawking, Barrow and others but it seems clear that, if this may (or may not) be a necessary qualification, it surely cannot be sufficient — unless perhaps one revives Hillary Putnam's proposal that logic may be an empirical science! There is indeed a very interesting set of questions surrounding this math-phys interface but the pythagorians seem very much more keen in obfuscating than addressing it…

10. **Chris**
June 18, 2008

I think its just that what mathematicians work on is inspired by the real world and in the real world you get analogous physical structures at different levels of physical reality.

11. **anon.**
June 18, 2008

'It always bothers me that, according to the laws as we understand them today, it takes a computing machine an infinite number of logical operations to figure out what goes on in no matter how tiny a region of space, and no matter how tiny a region of time. How can all that be going on in that tiny space? Why should it take an infinite amount of logic to figure out what one tiny piece of spacetime is going to do? So I have often made the hypothesis that ultimately physics will not require a mathematical statement, that in the end the machinery will be revealed, and the laws will turn out to be simple, like the chequer board with all its apparent complexities.' – R. P. Feynman, The Character of Physical Law, 1965.

Feyman is not referring to the use of the path integral for summing and weighting all possible interactions in spacetime even for relatively simple interactions, e.g. two electrons. There are an infinite series of increasingly complex Feynman diagrams for even the simplest interaction. The perturbative expansion for the path integral is an infinite series of terms of increasing complexity. The math is summing an infinite number of possibilities for how the field quanta will be exchanged.

This calculus error reminds one of the exponential formula for radioactive decay, a continuous curve which ignores the fact that radioactive decays are discrete events, so that there should really be a step-wise quantized decay line (with quantum drops as individual atoms decay, and horizontal lines in between the decays). The math is a nice approximation when the decay rate is very large, but nature doesn't strictly obey the equation at any time, and after the final atom decays the exponential law is obviously completely false and misleading.

12. **Daniel de França MTd2**
June 18, 2008

Heh, if someon wanted to seek an independent research, I would recomend looking for a job in the federal governament, specialy in something burocratic. I don't know how is it outside Brazil, but generaly here you get a enough free time to study, have some money to subscribe to a decent ISP and buy some books. Maybe sometimes one could travel and attend conferences, during vacations.

13. **Eric**
June 18, 2008

As a physicist, Tegmark's ideas seem completely useless. I wonder if philosophers would find anything interesting. I don't enjoy sounding harsh, but I suspect not. Any philosophers out there?

14. **Marcus**
June 18, 2008

Compare what Tegmark says with pages 42-49 of the July Scientific American, where the question of fundamental constituents of spacetime is answered differently. This site has the complete article free, for online reading:
[url]http://www.scribd.com/doc/3366486/SelfOrganizing-Quantum-Universe-SCIAM-June-08[/url]

They get deSitter space to self-assemble from no prior geometry. Instead of being made of mathematics, their universe is more like a flock of birds.

15. **neo**
June 18, 2008

Is there a connection between Tegmark's "The universe is math" contention and Seth Lloyd's "The universe is computation" contention? I find the computation analogy or mapping more compelling, but I am curious.

16. **Observer**
June 18, 2008

The statement that the Universe is made out of Math first emerged in Plato's philosophy. It is a proposition that is untestable and, as such, it does not belong to science.

17. **ZoloftNotWorking**
June 18, 2008

Don't forget Bee's hilarious posting on Backreaction when Tegmark's book came out.

http://backreaction.blogspot.com/2007/09/imaginary-part.html

18. **Tom W.**
June 18, 2008

I think it bears on this topic to mention that the physiology of the brain itself causes an internal creation of complex geometric representations. When deprived of external stimuli the brain can become aware of its own geometric background signal:

http://chronicle.uchicago.edu/010426/visual-cortex.shtml

It's furthur interesting to note that experimental subjects who have these experiences often recall the recognition of these patterns as
extremely important or all encompassing realizations of the universe.

This is not to say that our view of the universe is nothing more than a hallucination, but without doubt human brain function is very geometric at fundamental levels. To this day, one of the classic images we assign to cosmology is that of Kepler's Platonic solid model of the solar system (Mysterium Cosmographicum).

Centuries later String theory seems much more sophisticated, but it remains inseparable from its imbedded geometric allusions.

A big question could be to what extent do we have the freedom or ability to transcend the very limitations of our own mental machinery.

19. **Bourgeois Nerd**
June 18, 2008

Instead of "the universe is math," wouldn't it be more accurate to say that the universe is built on evolvingly complex patterns? We then translate the patterns into the "language" of math.

20. **Ryan Dickherber**
June 19, 2008

He says all mathematical structures are created equal, but I'm pretty sure that 2>1.

21. **Steve Myers**
June 19, 2008

I thought all this stuff was settled long ago — especially by Russell. Anyway, if you want to say something objective about real world relations (or any relations) or describe what's happening you have to use math, since that what math does. And no, it is not a separate language — everything in math can be expressed in English or Russian, etc.

22. **EJN**
June 19, 2008

"Paper in white the floor of the room, and rule it off in one-foot squares. Down on one's hands and knees, write in the first square a set of equations conceived as able to govern the physics of the universe. Think more overnight. Next day put a better set of equations into square two. Invite one's most respected colleagues to contribute to other squares. At the end of these labors, one has worked oneself out into the doorway. Stand up, look back on all those equations, some perhaps more hopeful than others, raise one's finger commandingly, and give the order 'Fly!' Not one of those equations will put on wings, take off, or fly. Yet the universe 'flies'."

John A. Wheeler

23. **Jeremy Bowers**
June 19, 2008

Consider your conscious experience as a trajectory through the n-dimensional space of possible conscious experiences.

It clearly has a certain coherency; the world does not radically change on a Planck-time-by-Planck-time basis.

However, if all possible mathematical structures are equally "true", then for each Planck time, there are an infinite number of discontinuous universes that you *could* suddenly find yourself in that aren't this one, but are otherwise perfectly acceptable. That is, mathematically, despite living in the world you think you've been living in all your life, it is perfectly acceptable mathematically that in the instant after you read this, you'll find yourself on a planet Vulcan, as in, the Star Trek planet. The universe discontinuously shifts around you, but math has no problem with discontinuous shifts.

Therefore, if all possible mathematical structures were equally real, the probability of use experiencing such a clean, ordered universe are infinitesimal. Perhaps we are simply that infinitesimal bit of the mathematical structure that actually, factually experiences such a simple and orderly universe, but it is far more likely that Tegmark is simply wrong, and there really is some sort of mysterious dividing line between "real" and "unreal".

24. **Joel**
June 19, 2008

Has no one thought to look at this using the discipline of cognitive science? I think it could offer some perspective that is lacking within the frameworks of physics and mathematics.

Mathematics is just a practice of studying relationships between patterns. We come across these patterns in individual experiences, and and in human thought.

It is no surprise that patterns in human experience are related to the structure of the universe, or that the mind is structured (both through nature and through nurture) to pay particular attention to patterns that bear upon physical existence. But mathematics is a map, rather than a territory.

25. **Peter Woit**
June 19, 2008

The spam software is having trouble distinguishing between the short, not very informative comments and quotes people are posting here and the ones coming from spambots, and I have to admit I can't completely blame it. Well thought-out comments that add something significant are encouraged, but please resist the temptation to add to the noise level here.

26. **Observer**
June 19, 2008

Unfortunately, the debate about whether or not Math forms the underlying fabric of the Universe leads nowhere. Statements cannot be proven true or false and the discussion is entirely scholastic. Despite what Tegmark advocates, Math is a tool , a rational collection of symbols helping to describe Nature, "a map rather than a territory" to paraphrase Joel.

27. **Jack Lothian**
June 19, 2008

I read the Discover article a month or so ago but if I remember my reaction, it was that the guy over-rates his insight. I didn't think he was out-to-lunch; he was just too full of himself. I kind of find the reaction here to be a bit too much as well. This guy is esoteric but I think he acknowledges that fact which is better than some. One of the things that struck me was his comment about what turned him in his current direction. Something about the wave-function collapsing when a particle is observed & that he felt this was wrong in some sense. My thoughts were much the same in 3rd or 4th year undergraduate physics & I think many other physic students felt the same way. Intuitively the Schrödinger wave function interpretation feels wrong. It works but it seems to imply an underlying unreality that makes me uncomfortable. Interestingly my reaction was to treat it like a math tool that that helps solve physics problems but may or may not be a description of reality. So for me math was always a tool that I could make do what ever I wanted it to do while physics was something real. This guy seems to have gone in the opposite direction embracing the potential unreality. For him math is the reality & physics the abstraction. Strange but if he can get people to pay him to write like this – good luck to him.

28. **kyb**
June 20, 2008

> "I think that when you ask yourself what "real" means, what criterion permits to classify something as real, what comes to mind is logical consistency. Everything else seems to be ill-defined intuitions."

Of course everything else seems to be ill-defined intuitions – it's not consistent, so it's not likely to look good under logical analysis. Which means you're begging the question. The real point is whether or not it is necessary for "real" to be logically consistent, or if that is just a convenient assumption. Since logical consistency is something (in my view) dreamed up by humans, dependant on a number of assumptions, and not an external truth, I see no reason other than that of pragmatism for making such an assumption.

The whole "universe is maths" idea raises as many questions as it answers. For people, maths is a mental activity, where is the unversal mind that thinks the universe? Even if you discount the mental nature of maths, what medium exists to reify the maths? Just because I come up with a mathematical structure in my head, doesn't mean I've created a universe of it that describes all the results of that structure beyond even what I'm able to trace. Or perhaps he wants to go back to platonism and the theory of forms.

We create models of our environment, our models are at most countably infinite (they can be expressed in language, or at least encoded in our brains), but there's no reason to believe that the environment itself (rather than our model of it) does not require uncountably infinite symbols to accurately represent it.

It's much more sensible to say that our maths is at best a model that approximates reality rather than that reality approximates our models.

29. **kyb**
June 20, 2008

Oh and for full accuracy, that "increasing purity" comic, should have had a philosopher to the right of the mathematician, and then a psychologist to the right of that, etcetera, ad infinitum.

30. **Arun**
June 20, 2008

Roger Penrose in The Road to Reality has a triangular diagram, that expresses the idea that some of our mental activity is mathematics; some of mathematics describes physical reality and finally, some of physical reality is our minds.

This diagram illustrates a mystery and it seems to me that Tegmark seeks to banish the mystery by collapsing Penrose's triangle into a point. If the universe is mathematics and mathematics is the universe, then every thought of ours is also mathematics. Our minds are mathematics. Even thinking that Tegmark's idea is absurd is mathematics. Agreeing with him is mathematics, too!

31. **Fabien Besnard**
June 20, 2008

>Even if you discount the mental nature of maths, what medium exists to reify the maths?

What do you mean by "medium" ? In this debate, I noticed that one very often reintroduce in a way or another, under a name or another, the intuitive but rather wrong idea that what is real is made of matter, and matter is made of small hard spheres (made of what ?). I would be very interested if someone could come up with a definition of reality that excludes mathematical structures but not quantum fields, which do not possess any of the macroscopic qualities (such as hardness, localisation, etc…) with which we mentally endow "real things". What is reality is certainly an old metaphysical question. I tend to think that these kind of questions cannot receive an answer as such because their terms are not well-defined. Logical consistency=reality is a definition of reality that has the advantage of not being polluted by our macroscopic prejudices.

>Just because I come up with a mathematical structure in my head, doesn't mean I've created a universe

Certainly not. Mathematical structures do not live in spacetime, so they can't be created. They just are. You can think about one, it does not mean that you have created it (as with everything else you can think about).

32. **Arun**
June 20, 2008

Mathematical structures do not live in spacetime, so they can't be created. They just are. You can think about one, it does not mean that you have created it (as with everything else you can think about).

What determines which mathematical structures "just are" and which "just are not"? E.g., why haven't we seen supersymmetry so far?

33. **kyb**
June 20, 2008

As it happens I'm much closer to a Berkleian idealist than a believer in small hard round spheres.

If you're going to say things like "Maths just exists", perhaps you should give a clear and unambiguous definition of what you actually mean when you say maths. It's quite possible that this is just a difference in use of terminology.

I'm very confused where you get "Logical consistency=reality is a definition of reality that has the advantage of not being polluted by our macroscopic prejudices." from. It seems to me that belief in consistency is one of our biggest macroscopic prejudices.

34. **Chris W.**
June 20, 2008

You seem a little confused there, Arun. As a mathematical construct, supersymmetry has as much claim to exist in a Platonic sense as any other mathematical construct. Whether this construct can be given an *interpretation* that brings it into *successful* confrontation with observation (both passive and active, ie, experimental) is a separate question. So far, the answer seems to be no.

Of course this just indicates the fundamental wrongheadedness of Tegmark's formulation. All sorts of mathematical constructs exist (in an appropriate sense) in our heads, or in objective form (following Karl Popper) in books, journal articles, implementations of algorithms in computer hardware and software, etc. (For all we know they also exist widely among the cultures of sentient beings across our galaxy and beyond.) However, in the empirical sciences we're concerned with inference from certain premises that leads to specific, empirically testable conclusions about the physical world. We generally do this in a conceptual framework that organizes and motivates the inferences, but the relevance of logic and mathematics comes first and foremost from their power in helping us draw those inferences. Admittedly, in physics, various areas of mathematics have also contributed increasingly to the development of the conceptual frameworks, but this is ultimately to serve inference; we hope they can help us draw far-reaching and unambiguous conclusions that we can *test*.

I would suggest that the relevance of mathematics as a source of patterns comes ultimately from the simple fact of some regularity and stability in the world. Patterns are useless in describing experience if their appearances are completely ephemeral. In fact, noticing patterns *in time* is arguably where science begins. The fact that we can reproduce experiences of certain sorts, or merely note their regular repetition, is the starting point for the application of inference, and ultimately mathematical inference, via mediating abstractions (starting with simple idea of number).

One could say that the central mystery in nature is the interplay between pattern and structure on the one hand, and chaos and disorder on the other. It seems to involve a delicate balance; nowhere is this more evident than in quantum mechanics. John Stachel has argued that Einstein's objections to quantum theory were misconstrued. He didn't object to indeterminism itself so much as the lack of any explanation for why just so much, and no more, should be admitted. In other words, if the laws of physics do not fully determine the future from the past, why should they exist at all? Why should there be any structure in the world?

[By the way, your earlier comment—mentioning Penrose's book—reminds me of my first encounter (in my teens) with B. F. Skinner's *Beyond Freedom and Dignity*. I was entranced for a while with the idea that one could give a behaviorist account of the objections to behaviorism. What a great way to make an idea invulnerable to criticism! Over the course of the next year and a half I started to grasp how pernicious and sterile that tactic was. I'll leave it someone to say whether Skinner was actually indulging in it.]

35. **Arun**
June 21, 2008

Thanks, Chris W. You made pretty much the point I was leading up to.

36. **Shaun**
June 21, 2008

For me the universe-as-math question raises 2 primary questions:

1) Questions of the ultimate structure of the universe aside, it seems a tautology that the 'ultimate structure' of the brain must be a subset of the universal one. This imposes limits on what we as a species can perceive or even theorize about.(Tom W. makes this point above, with a link to a very interesting article.)

2) Why then the "unreasonable effectiveness of mathematics" in the natural sciences? – exactly what we're dealing with here.

The shared element that connects these two questions is Logic, and the connection occurs at a fundamental level, pretty mush as deep as it gets. For this reason, I believe there is genuine merit in Hilary Putnam's view of Logic as an empirical science.

Whatever else the universe is, it is an entity that allows Logic, and so far, seems to demand that we completely constrain our thought processes to those which are logical if we wish to model it. This is the origin of Pythagorean mysticism, Plato's idealism and our own personal intuitions regarding the relationship between abstract mathematics and physical science.

Math is built from logic – Russell/Whitehead showed us this, Godelian limitations notwithstanding. When we are marveling over the relationship between math and science, we are really remarking on the fact that nature has, to some significant extent, a logical substructure. This is a lesson we were strongly encouraged to learn, as a species, and taking these lessons to heart our most reliable forms of thought are logical ones.

I think Physics, as a discipline, is approaching the point where it will begin to ask questions like: "what is the nature of the fundamental structure of the universe such that it supports Logic as a mode of computation?".

Or something like that.

37. **Eric H**
June 21, 2008

It seems to me that in all this comment about logical consistancy of math and its relationship to the world individuals have left out the most important relationship of math to the world – and that is the conservation rules. While geometry has its important place in math, it deserves a position one rung lower than conservation laws when it relates to physics. More mathematically oriented physicists seem to continue to forget that.

In the universe geometry is related to the geometry of energy. Energy, as far as we know, is the only substance that always remains and is finite in the universe even as the geometry of its forms change. For some reason which I can't fathom theoretical physicists, as a generalization, seem to be the most likely people to forget it and are the ones who must continually strive to remember it. The relationship of how those forms change geometrically while continuing to conserve energy is where the answers lie in physics. This is not true for math.

38. **woit**
June 21, 2008

Eric H.,

The relation between conservation laws and symmetries is an absolutely fundamental aspect of how physics and math are related. In quantum mechanics this can be identified precisely with the mathematics of the unitary representations of Lie groups. I've gone on in many places about how important I think this is, and it's exactly the sort of thing that I think Tegmark misses (along with people who see this as an issue about logic, or the human brain, or any number of other very different questions).

39. **Jonathan Vos Post**
June 21, 2008

Dr. Woit, can you please elaborate on why Tegmark is wandering in the darkness, despite the illumination of Amalie Emmy Noether, who (as thumbnailed in wikipedia) the German Jewish mathematician [March 23, 1882 – April 14, 1935] known for her seminal contributions to abstract algebra. Often described as the most important woman in the history of mathematics, she revolutionized the theories of rings, fields, and algebras. She is also known for her contributions to modern theoretical physics, especially for the first Noether's theorem which explains the connection between symmetry and conservation laws [Ne'eman, Yuval. "The Impact of Emmy Noether's Theorems on XX1st Century Physics". The Heritage of Emmy Noether. Ed. M. Teicher. Israel Mathematical Conference Proceedings. Bar-Ilan University/American Mathematical Society/Oxford University Press, 1999. OCLC 223099225. ISBN 978-0198510451. pp. 83–101.]?

My bias is that the 2nd most important woman in the history of 20th century mathematics, Olga Taussky Todd [30 August 1906, Olomouc, then Austria-Hungary – 7 October 1995, Pasadena, California] Czech-American Jewish mathematician, gave me her personal and professional take on Emmy, whom she'd met. Similarly, I've heard great talks at symposia in the memory of Emmy Noether and Olga Taussky Todd.

Please, though I agree with you, I'd like your more detailed rationale.

40. **Peter Woit**

June 21, 2008

Jonathan,

I’m talking about symmetries in quantum mechanics, not classical mechanics. See my book for more details.

41. **John Rennie**
June 23, 2008

Suppose it were possible to formulate a theory that was independent of topology and the number of dimensions. Then suppose you could use a procedure analogous to gauge fixing to work out the physical predictions of the theory in 3+1 dimensions and arrive at something that described the world we see. Then what we see as the physical world and it’s contents, including ourselves, is merely a result of a gauge fixing. (You’d have to show that choosing any other gauge i.e. dimensionality and topology produced results that wouldn’t support observers.)

Anyhow the point is that the world you end up describing would necessarily reflect the mathematics used for the “gauge fixing”. Then physics and mathematics would be inextricably linked and Tegmark’s view of mathematics would arise naturally.

42. **Peter Woit**
June 23, 2008

It’s not “gauge-fixing” if physical observables depend on it. All you have done is decided (illegitimately…) to call the mathematical structure of physics “gauge-fixing”, you haven’t addressed at all why it is one structure and not another.

Philosophy of Science on Bloggingheads.tv

June 21, 2008
Categories: Uncategorized

Today's Bloggingheads.tv diavlog features Sean Carroll and philosopher of science David Albert, discussing a variety of issues. Albert tells about his unfortunate experience with the What the Bleep? film, a good example of why it's not always a good idea to get involved with people doing a supposedly science-related media project. He also discusses the hostility towards study of the quantum measurement problem from within the physics community over the years, a situation that has changed recently. The two also had a long discussion concerning Carroll's claims about the arrow of time, about some of which Albert seemed to be rather skeptical.

The discussion of criticisms of string theory were on the whole ill-informed, misleading, and devoted to ferocious attacks on straw men. For some reason, only John Horgan was mentioned, with the existence of trained theoretical physicist critics and two recent books on the topic completely ignored. Albert insisted repeatedly on the idea that Horgan and other critics were not acknowledging the "spectacular predictive success" of string theory. He was referring to claims that string theory "predicts gravity", since it contains a massless spin-two particle (ignoring the fact that it is in the wrong dimension; to quote Lisa Randall "string theory predicts gravity: 10d gravity"). Later on Carroll did explain the problem with this, that string theory seems to allow an infinite variety of ground states with different physics, many of which don't have 4d gravity. Carroll told about having asked various string theorists if they could imagine any kind of experimental result at any energy that would be incompatible with string theory, and getting the answer "No" from at least some of them. This seemed to rather shock Albert.

There was no real discussion of the multiverse, a topic where philosophers of science might be able to perform a public service by taking a serious look at what physicists are up to and analyzing what they learn. Carroll launched the standard attack on string theory critics as having a "sophomore-level" understanding of the philosophy of science, unaware that there is anything to the problem of what is science and what isn't other than Popper's falsifiability criterion. He also claimed that string theory critics have created a "20 year statute of limitations" criterion, that theoretical work must lead to a falsifiable prediction within 20 years or cease to be science, chuckling with Albert about how ignorant people must be who think such a thing. This kind of willful misrepresentation of the views of people you disagree with seems to me to be less than honest. From what I remember of Lee Smolin's book, there's a long section about his engagement with the philosophy of science, and his sympathies are not with Popper, but with Feyerabend's "anarchistic" views on the subject, which are very different. In my book there's an entire chapter devoted to explaining what is wrong with just invoking falsifiability. I assume Carroll has read at least one of the two books, so it's unclear why he thinks it's acceptable to go on like this. He does make one more accurate accusation, that critics of the multiverse are stuck in an out-dated 1960s particle physics paradigm of what it means to test a theory. I suppose this is true enough. Not the first time I've been accused of being stuck in the 60s, which, if one has to be stuck somewhere, doesn't seem like that bad a choice…

Update: Evolving Thoughts has a link to The Ideas of Quine on Youtube, an interview of the philosopher Willard Van Ormond Quine by Brian Magee. The relation of physics and philosophy was one of Quine's main concerns, and one of the main topics of the interview. I suppose that to the extent my own philosophical views could be characterized by picking one philosopher I find most sympathetic, Quine would be a good choice. Perhaps he's responsible for my "sophomore-level" philosophy of science, since I took a course from him as a sophomore.

Comments

1. **Bee**
   June 21, 2008

   Indeed what I don't get is that people don't want to understand it's not a do-or-don't question, but one of balance. Except for the occasional misinformed commenter in the blogosphere I haven't encountered any serious scientists who thinks one should drop string theory, and I too am all for live and let live. The question instead is whether the amount of people working on a research program and the resources invested are appropriate to the promises it holds. This, (as I have said repeatedly, but just to make sure) is not a question constrained to the case of string theory but of a much larger interest: are resources, human and financial, optimally distributed in the academic system? The answer is no, and it seems to me the problem is precisely that today it is not the case that 'anything goes'. Instead, we have a very strong influence on the allegedly 'free' marketplace of ideas that goes back to social phenomena as well as business tactics that are not appropriate to scientific research. (See also my post on The Marketplace of Ideas).

   Btw, Peter, did you see this paper?

   http://arxiv.org/abs/0806.1431

2. **Peter Woit**
   June 21, 2008

   Bee,

   I agree, the crucial question is that of evaluating whether and how the string theory research program is making progress, an evaluation that should then carry implications for how it is pursued. The problem is not that string theory unification makes no predictions, it's that no progress has been made over the last 24 years towards predictions, actually quite the opposite.

   I took a quick look at the paper you mentioned. It seems to make the claim that the low energy modes of a superstring will not look like Dirac particles satisfying the Dirac equation, but instead like Klein-Gordon particles. I don't understand the author's argument for this, I do understand the conventional argument for why you get the Dirac equation (Ramond's work on the superstring uses a Dirac operator on loop space, Fourier modes satisfy a Dirac equation).

3. **Not a string theorist**
   June 21, 2008

   Dear Peter,

   I am a mathematician working in areas adjacent to string theory; I know a little but not a lot about the subject, but more importantly for the purposes of this post, I have no strongly formed opinions about the fundamental philosophical issues it raises.

   I think much criticism of your approach is entirely due to a perceived (whether accurate or not) unwillingness to learn about the frontiers of the subject on your part – even the more mathematical aspects of string theory.

   If you limited your criticism to the poor statistical techniques of the multiverse protagonists you wouldn't really be saying anything outside mainstream string theorist opinion, from what I can gather talking to my stringy colleagues.

   But what seems to rankle is the dismissive tone with which you (do not) properly discuss developments such as the recent Beasley Heckman Vafa paper, together with a lack of technical discussion (200 pages is not really that long if you are prepared to make the effort – an advanced graduate student or postdoc in string theory would expect to read and understand it in a week or so if they wanted to).

What my colleagues (and to some extent, I) take greatest issue with is not the fact of the criticism but the manner of it. They see you talking about Geometric Langlands with great enthusiasm, but without mention of the underlying string theoretic concepts which underlie the calculations – or the fact that such profound understanding depends on string theory in absolutely crucial ways. They see you limit your criticism to the easy targets of the multiverse and Lubos and hubristic paper titles and anthropic stuff and bad journalism and the one-line pithy quotes from prestigious figures therein, and couching your discussion in matters of philosophy and sociology.

Whilst these things are incredibly important, and not to be brushed under the carpet, ultimately, the technical stuff is what counts – and one of the reasons why you are not taken particularly seriously as a critic is that you haven't presented DETAILED criticisms of the latest directions (or even slightly dated directions) in string theory. To illustrate: if you discussed (or even better, found flaws in) Bagger-Lambert theory, or the Sen conjectures, or the ideas of conifold transitions, or dyon partition functions, you would be taken incredibly seriously. But repeating "Sean Carroll is misrepresenting me again" (even if true) will get you little respect.

Your mathematical posts are always interesting, but in my opinion, such as it is worth, your criticism needs to be elevated if you want it to be taken seriously. And at some point, you need to address the question of how to value the astonishing mathematical consistency of string theory (I seriously doubt you can find many practising mathematicians in whose fields conjectures have been raised who do not believe this).

(For what it's worth, my own job does not depend on string theory in any way, but I have had great fun in the past few years proving mathematical conjectures which have partial stringy origins).

Whilst it is of course your own blog and you can do as you see fit, I agree entirely with a comment by John Baez in that I would love to see more equations here and less vitriol.

4. **onlooker**
June 21, 2008

"how to value the astonishing mathematical consistency of string theory" is a question that answers itself. There is a high value of string theory (and related ideas) for mathematics, to the point that the mathematical aspects and connections of string theory will be pursued whether or not the intended physical applications materialize.

The question that I think Woit and others are raising is about the discrepancy between string theory's dominant status as a particle theory research program and it's not-astonishingly-high contribution to observable physics.

5. **Buffon**
June 21, 2008

Dear "Not a string theorist",

You must be new here. All your points are 100% correct and have been explained to Peter a gazillion of times yet he refuses to listen and bring home at least 1% of the arguments made. Hence it's a total waste of time to argue with him which fact is correctly recognized by the particle physics community i.e. nobody takes him seriously.

His blog is still amusing for casual reading 🙂

Best,
Buffon

6. **Peter Woit**
June 21, 2008

"Not a String Theorist"

First of all, please think seriously about whether it's a good idea to post this sort of comment anonymously. Because of the nature of this controversy within the physics community, physicists sometimes have a legitimate reason to hide their identity when discussing it. There is no such controversy among mathematicians, everyone (including me) agrees that string theory research has led to all sorts of interesting mathematics. I can't think of any valid reason for you to hide who you are. In addition, it makes it hard to respond intelligently to your criticisms, since all I have to go on about your background is your own admission that you don't know a lot about this subject.

Your attack on me completely ignores that this posting and much of my concerns motivating this blog are about physics and have nothing at all to do with mathematics. As a mathematician, there's no reason you should be concerned about the physics question of evaluating whether string theory, as a physical idea about unification, is a failure or not. My discussion of the Beasley/Heckman/Vafa paper was about this issue, not about the details of their calculations, which quite likely are fine, since these are highly competent people. The issue I was discussing was that of what physical predictions they get out of their model, and I tried to do that carefully.

As for Bagger-Lambert, the Sen conjectures, conifold transitions, dyon partition functions, I haven't posted any criticisms of such work, much less detailed criticisms, and don't intend to, because I'm not critical of such work in any way. I probably think more highly of it than most physicists do. As long as no one is making bogus claims that such ideas solve the problem of making string theory unification predictive and successful, I see nothing to criticize. I'm perfectly happy that some people are working on these topics, they just don't happen to be close enough to my own research interests for me to try and become expert on them and write about them here.

Geometric Langlands is a different story, since it is closely related to my own research, which focuses on the relation of representation theory and QFT. Because of this, I've spent a lot of time learning about Geometric Langlands, and have enthusiastically in some of my blog postings outlined some of what is going on in the subject, while pointing to some of the excellent expostions of this material from people much more expert than me. Witten, David Ben-Zvi, Ed Frenkel and some others are wonderful expositors, and I'm not going to try to compete with them on this. I'm working on some of my own ideas related to the subject, and am in the middle of writing a paper, which I hope to have completed by the end of the summer. The technical details of what I have to say will be in a paper, perhaps with some blog postings giving some general comments and explanations.

As for the relation between string theory and Geometric Langlands, I just don't agree with you that string theory underlies the subject. The relations between Geometric Langlands and physics are almost entirely about QFT, not string theory. This quickly becomes a technical discussion, which isn't worth entering into unless we can agree on what "string theory" is, in particular what the difference is between a string theory and a 2d conformal quantum field theory.

Similarly for the "astonishing mathematical consistency of string theory", to address this, I need to first know precisely what you are talking about. "string theory" is a research program trying to understand a class of physical models, some of which are understood well enough to see that they are likely consistent, but don't work as unified models.

I'm trying here to address your criticisms seriously, if you want me to continue to do so, please use your real name. I've come to intensely dislike trying to have a serious scientific discussion with people not willing to take responsibility for their arguments.

7. **Peter Woit**
June 21, 2008

“buffon”,

As to whether no one takes the kinds of arguments I’m making seriously, you might want to consider whether string theorists from UCSB such as yourself posting stupid anonymous comments on blogs encourages your colleagues to take string theory seriously or not. Are you tenured and thus not worried about this? If not, have you noticed that physics departments have stopped hiring string theorists and asked yourself why this might be?

8. **anon.**
June 22, 2008

‘… [Smolin’s] sympathies are not with Popper, but with Feyerabend’s “anarchistic” views on the subject…’

Popper’s approach that theories must make falsifiable predictions has the problem that it doesn’t include useful ad hoc theories which formulate an equation empirically that summarises existing experiments. If existing experiments already cover the whole range of possibilities, such theories don’t make falsifiable predictions.

E.g., if the deflection of light by the sun’s gravity (and the many other consequences of general relativity) had been observed prior to November 1915, would general relativity have been dismissed for not making falsifiable predictions?

If so, the only reason for dismissing general relativity as unscientific would be the historical accident that observations and experiments happened to discover phenomena before Einstein and Hillbert came up with a theory for it. It’s a paradox if historical accidents determine whether a theory is scientific or not scientific.

So clearly, falsifiability isn’t the key criterion for science.

Feyerabend’s Against Method takes the pragmatic view that there isn’t a scientific method. Whatever theory turns out to be most useful is the most scientific. Because string theory has in the past been the most useful fundamental physics research in helping theorists to get books to sell and to get research grants, it has been the most practical and therefore the most scientific of the various options. Even while it flounders in the landscape problem, because no better theory evident, so string theory remains the leading contender for a scientific theory of everything. [I am not a string supporter; I’m just explaining why string can’t be undone.]

9. **Arun**
June 22, 2008

Whether you go Popperian or Feyerabendian, in both philosophies, science is seen to be dealing with facts, i.e., statements about how the “real world operates” (or “consequences” as anon. at 5:47 AM put it, as in “and the many other consequences of general relativity”). Falsifiability is simply one prescription of the relation between the facts a theory generates and whether the theory is scientific or not. Even with Feyerabend, even if a new theory need not be compatible with all known facts, or not produce any not-already observed facts, it nevertheless has to produce some statements of fact, some consequences. This is where superstring theory as a theory of particle physics seems to be deficient. What the the consequences of a superstring particle physics theory? It would seem that there are none.

“Carroll told about having asked various string theorists if they could imagine any kind of experimental result at any energy that would be incompatible with string theory, and getting the answer “No” from at least some of them.”

10. **Garbage**
June 22, 2008

I find the following ad pretty funny:

“Eat s*it… trillions of flies cannot be wrong”

It’s a delicate subject. No one is going to deny the influence of string theory, or better said string theorist, in high energy physics.
String theory provides some sort of *educated EFT guessing*, and promoted the XD revolution, the ADS/CFT correspondence and lots of fun math.
Given the fact that we’ve been playing this game for the last 20 years, I think finally here it comes the time the bushiting is over. Let’s turn on the machine for physics’s sake! And elect Obama by the way

Unfortunately, the era of phenomenologists looks quite grim to say the least…

G

11. **Bee**
June 22, 2008

Anon:

Popper’s approach that theories must make falsifiable predictions has the problem that it doesn’t include useful ad hoc theories which formulate an equation empirically that summarises existing experiments.

[…]

Whatever theory turns out to be most useful is the most scientific. Because string theory has in the past been the most useful fundamental physics research in helping theorists to get books to sell and to get research grants, it has been the most practical and therefore the most scientific of the various options.

Did it occur to you that the word ‘useful’ has two very different meanings in the context you give? A theory can be ‘useful’ to understand the world we live in. It can also be ‘useful’ to get research grants and to become famous by writing books about it. The problem arises if both versions of usefulness are disconnected from each other.

Best,

B.

12. **milkshake**
June 22, 2008

I grew up in the same country as Lubos Motl and I remember being told that a critique was ofcourse needed – but to be taken seriously one should make a constructive criticism, one should work withing the framework of the regime – and while there were some minor difficulties they were to be viewed in the context with the past achievements. We were told that there were also some enemy detractors who were professional failures, bitter men with a clear agenda but no future.

13. **anon.**
June 22, 2008

The marketing usefulness which string theory enjoys as being the leading theory of everything *helps to fund* the useful ongoing research that seeks to apply the theory to observables. So the two uses you distinguish are interdependent, not disconnected.

14. **Eric**
June 22, 2008

String theory is useful in the sense that it is possible to construct models that agree with the Standard Model, which in addition include gravity. Even if it were not possible at the present time to uniquely predict the values of all the SM parameters, it is still very significant that such a consistent framework exist. Do you know of any other theory which can do the same?

In any case, all of this talk about being successful at selling books and getting grants is nothing more than petty jealousy. If your research area were as promising then you would have no difficulty doing the same.

15. **Buffon**
June 22, 2008

Peter,

And if in 5 years the number of string theory hires are even lower will you stop blogging and start doing actual research? The assumption that the lower number of string theory hirings are due to your acitivities is uber hilarious and comments like that are the main reason I like reading your blog 🙂 If you did not assume that then I have no idea what makes you believe anyone takes you seriously.

Actually, that's a question on its own: what makes you believe that more than 2 people in the particle physics community take you seriously?

Paragraph insulting others and characterizing non-string theorists as crackpots deleted

All the best to everyone, keep up the good work, it's always fun to let out the steam 🙂
Buffon

16. **Peter Woit**
June 22, 2008

"Buffon",

You've completely misunderstood my point. The fact that US physics departments have pretty much stopped hiring string theorists has little to do with me, and a lot to do with string theorists like you. Many of your non-string theory colleagues do read blogs, and haven't failed to notice that string theorists feature prominently, often engaging in anonymous insult of anyone who disagrees with them. Would you want someone like that as a colleague? And since the anonymity means you can't tell who is who, wouldn't you be tempted to just not hire any string theorist to be safe? It's not like the field is having great successes these days anyway…

You've now used up your allotment of juvenile anonymous insulting here. Bye.

17. **Buffon**
June 22, 2008

Peter,

Honest question: have you ever been on a hiring committee in particle physics? If yes, you would know that the selection criteria does not include anything related to blogs and neither do personality issues matter. In other words if someone does good work but at the same time is a real SOB who likes insulting people he/she will be hired no matter what since the requirement is that good work should be done.

Cheers,
Buffon (not a string theoriest, by the way)

18. **woit**
June 22, 2008

"Buffon",

I've been on hiring committees in mathematics, not particle physics. Sure, it's possible to be an asshole and get hired. All you have to do is be a much better candidate than the non-assholes.

Who knows who you are, but if you're really not a string theorist, with friends like you string theory doesn't need enemies….

19. **Bee**
June 22, 2008

Anon,

You completely miss my point. What I am saying is that 'marketing' and 'advertising' shouldn't influence what scientists consider interesting and work on. But this is, as you say, de facto the case. If you allow that, you allow a kind of 'usefulness' to arise that originates in social, political, financial 'usefulness' which a priori has nothing to do with the actual 'usefulness' for describing nature. You can produce completely empty bubbles of nothing in this way. (Bubbles of nothing is a phenomenon we know pretty well from the stock market, this is not a coincidence.) You say, there is a connection between funding and thus how much effort goes into the field. Right! What I am asking is, how can we expect the funding and the resulting research effort to accurately expect the actual promise of a research field if we distort the judgement deliberately with marketing tactics? Best,

B.

20. **anon.**
June 22, 2008

‘I grew up in [Czech Republic when it was a Warsaw Pact satellite of the USSR] and I remember being told that … to be taken seriously one should make a constructive criticism … We were told that there were also some enemy detractors who were professional failures, bitter men with a clear agenda but no future.’ – Milkshake

That’s not just communist brane-washing! It’s actually an American disease too, for similar reasons (social niceties). See Dale Carnegie’s argument for sycophancy in his bestseller (over 16 million copies sold) *How to Win Friends and Influence People*:

‘Principle 1: Don’t criticise, condemn or complain
‘Principle 2: Give honest and sincere appreciation …’

21. **Peter Woit**
June 22, 2008

“Buffon”

I’ve deleted your latest comment, you seem incapable of writing one that doesn’t trash someone or other. Doing this anonymously is extremely unprofessional and juvenile behavior. If you want to engage in discussion here, act like a grown-up, put your name to your words and take responsibility for them and for your behavior.

22. **Leonard Ornstein**
June 22, 2008

Popper’s “falsifiability” is just one leg of “decidability”.

The pre- and post-Popper criterion of what is or is not science seems to be whether a model might generate ‘predictions’ which would be decidable, WITH SOME CONFIDENCE, by past or future INDEPENDENT empirical observation.

By this broader and more rational criterion, string theory is still found wanting – as is pure math.

Hope this helps.

23. **chimpanzee (aka "joe")**
June 23, 2008

“If you can’t dazzle them with Brilliance, baffle them with B***SH*T”
“If you can’t convince them..CONFUSE THEM”

Marketing & PR (BS’ing to varying degree) is an evolutionary tactic. Insects use camouflage extensively.

“War is about DECEPTION”
— Sun Tzu, “Art of War”

Science is no different from War: it involves competition between groups. Politicians are the best example of “using lies to defend an absurditiy”:

“Every absurdity has its Champion”

Lying quotes

“**If you tell a lie big enough and keep repeating it, people will eventually come to believe it**………….The lie can be maintained only for such time as the State can shield the people from the political, economic and/or military consequences of the lie. **It thus becomes vitally important for the State to use all of its powers to repress dissent, for the truth is the mortal enemy of the lie, and thus by extension, the truth is the greatest enemy of the State.**”
— Joseph Goebbels, German Minister of Propaganda, 1933-1945

“A lie told often enough becomes truth”
— Vladimir Lenin.

My view of all the “way out” theories (which have gone beyond any hope of experimental verification) is that they are defended with equally “way out” explanations for predictive power. In engineering, there’s a phrase for hardware hacking: “kludge upon kludge”. I was at SUSY ’06, & somebody walked past me muttering “making stuff up”.

24. **S Halayka**
June 23, 2008

Bee, Peter,

I mentioned arXiv:0806.1431v2 in another comment thread here, but I’ve been told that it is incorrect.

http://www.math.columbia.edu/~woit/wordpress/?p=684#comment-39048

http://www.haloscan.com/comments/lumidek/1137486680928497948/

Just an FYI.

– Shawn

25. **onlooker**
June 26, 2008

Eric,

re: string theory providing a “consistent model” of *some* unification, what are you referring to, and what value is it supposed to have? We already know that some logically consistent unification should be possible (since the universe exists and contains both QM and gravity). On the other hand, there is no known consistency in the mathematical sense of a rigorously constructed model of axioms that demonstrates their non-contradiction. We don’t even know what the axioms are, exactly, because string theory is not precisely defined at the moment.

It’s true that there are a number of nontrivial consistency checks that the string formalism satisfies (or might satisfy once formalized precisely). That suggest a mathematical project of formalizing at least SOME of the expected properties of a string theory as axioms, and giving a “consistent model” of those. But that would be open to the objection that it's not really the full string theory. It could be that there is a consistent mathematical core (e.g., a topological M-theory) that accounts for all the fragments of string theory currently shown to be consistent, and that things beyond that are wrong and need some further correction to BECOME consistent.

In short, string theory is at the moment a promising web of (not always precise) calculations still under construction, and it seems premature to talk about its "consistency". The rules of calculation are progressively specified and re-specified (fixing various problems over time) so that contradictions are eliminated, and we know that two degenerations of the theory are logically consistent (QFT, GR), so why is it a major surprise that the set of rules converges or doesn't run into fatal problems over time?

26. **Eric**
June 26, 2008

Dear onlooker,
I am referring to the fact that gravitation and quantum mechanics are unified consistently into the same framework. It is consistent framework because of the absence of anomalies. Thus, it is possible to build specific quantum field theories from string theory, which also include gravity. In fact, it is now believed that gravity and quantum field theories are dual viz Ads/CFT. Now, it may be the case that there is no single unique quantum field theory, at least at the present level of understanding, but at least it is possible with string theory to bring gravity into the story

27. **onlooker**
June 27, 2008

Absence of anomalies doesn't mean that the theory is consistent, only that one particular type of inconsistency in a particular family of calculations doesn't show up. This is a "consistency check", not consistency as such. If the theory itself is ill-defined, i.e., the rules separating valid from invalid computations are not completely specified, then however impressive the anomaly cancellation calculation, it doesn't rule out that there is an equal and opposite calculation giving a nonzero anomaly.

Also, is it ruled out that anomalies re-appear nonperturbatively, as an obstruction to writing down a consistent theory (rigorous mathematical model) whose expansion near some point(s) is perturbative string theory?

28. **Rob Heusdens**
July 7, 2008

anon said
–quote–

Popper's approach that theories must make falsifiable predictions has the problem that it doesn't include useful ad hoc theories which formulate an equation empirically that summarises existing experiments. If existing experiments already cover the whole range of possibilities, such theories don't make falsifiable predictions.

E.g., if the deflection of light by the sun's gravity (and the many other consequences of general relativity) had been observed prior to November 1915, would general relativity have been dismissed for not making falsifiable predictions?

If so, the only reason for dismissing general relativity as unscientific would be the historical accident that observations and experiments happened to discover phenomena before Einstein and Hillbert came up with a theory for it. It's a paradox if historical accidents determine whether a theory is scientific or not scientific.

So clearly, falsifiability isn't the key criterion for science.

Feyerabend's Against Method takes the pragmatic view that there isn't a scientific method. Whatever theory turns out to be most useful is the most scientific. Because string theory has in the past been the most useful fundamental physics research in helping theorists to get books to sell and to get research grants, it has been the most practical and therefore the most scientific of the various options. Even while it flounders in the landscape problem, because no better theory evident, so string theory remains the leading contender for a scientific theory of everything. [I am not a string supporter; I'm just explaining why string can't be undone.]

–quote–

By all means, theories are not to be valued at the basis of how many books it sells, how many blog entries are attributed to it, or other such qualifications, since that has nothing to do with what scientific theories are for, namely: to explain facts about reality in a cohorent and consistent way such that predictions made by the theory can be tested, and using the least assumptions.

Further, I do very much oppose the idea of a 'theory of everything' since any real physical theory can only explain specific facts and only part of reality (and besides, there is already is a trivial 'theory of everything' namely the theory that 'everything is caused by everything' which is true in the tautological sense, yet does not explain anything in particular, and hence has an information value of exactly zero).

Perhaps this is just a matter of terminology, and such 'theories of everything' (such as string theory) should be better termed 'frameworks' or 'models', which could be quite usefull to develop specific theories, which are scientific theories in the sense of the definition mentioned above. A tool, framework or model can be very handy, indeed, but does not qualify as such as a theory, unless it makes specific new testable predictions.

Specific theories should have specific testable predictions.

The example given about the theory of general relativity is a bit non-sense, since the theory makes many predictions which can be tested, and which makes it possible to distinguish the theory of gravity of Newton with that of General relativity since it leads to different outcomes. There are numerous new predictions that can be made which can test for that.

Wether a new theory can be said to be a new theory, if it merely explains facts already covered in existing theories in a new framework (and supposedly more generic way) is a bit of an acedemic question, but one should expect that such a new theory covers some new ground, and on that is able to make new predictions that could be tested, which the theories it mererely replaces, could not.

Rearranging or reformulating a theory without increasing the level of understanding, or making any new predictions, would not qualify as a new theory in my opinion. Rearranging the seats of an old car, does not qualify the car as "new" either. Which does not contest that such could be usefull in some cases.

The Black Hole War

June 23, 2008
Categories: Book Reviews

Leonard Susskind has a new book that's now out in the bookstores, entitled The Black Hole War: My Battle With Stephen Hawking to Make the World Safe for Quantum Mechanics. It's about the black hole information paradox, structured around his story of debates with Hawking over the years on this topic.

Back in 2005 I wrote a review here of his previous book, The Cosmic Landscape, which I found pretty much appalling (and my opinion hasn't changed). There Susskind deals with the failure of string theory by promoting out-right pseudo-science, of a sort that unfortunately has been highly influential. I'm happy to report that his new book is about 10^{500} times better. In its 450 or so pages, the string theory landscape, the multiverse, and anthropic reasoning make no appearance, with Susskind sticking to legitimate science. Instead of breathless promotion of string theory as a unified theory, here he is cautious about this, emphasizing repeatedly that he is just invoking string theory as a presumably consistent framework for resolving conceptual problems raised by quantum gravitational effects of black holes:

> How do we use String Theory to prove something about nature if we don't know that it's the right theory? For some purposes it doesn't matter. We take String Theory to be a model of some world and then calculate, or prove mathematically, whether or not information is lost in black holes *in that world*.

He even notes that:

> Being called a string theorist irritates me; I don't like being pigeonholed so narrowly.

The style of the book is often over the top, going on about battles and wars, with chapter headings from Churchill's history of World War II. As is the custom for books in this field, the fly-leaf copy is pretty much nonsense. But, at a general audience level, Susskind gives a good introduction to lots of topics in physics and to the black hole information paradox in particular. It is livened up with various entertaining color and anecdote, starting with a description of hearing about the paradox from Hawking back in 1983 at a conference held in Werner Erhard's mansion. He describes discussing black holes with Feynman, approaching him first at a urinal in Pupin, the Columbia physics building, and moving later to the local West End Bar (recently turned into a Cuban restaurant).

He ends not with triumphant claims of victory in his war, but with an appropriate description of the current state of fundamental theory:

> Confusion and disorientation reign…. Very likely we are still confused beginners with very wrong mental pictues, and ultimate reality remains far beyond our grasp… The more we discover, the less we seem to know. That's physics in a nutshell.

It turns out that Susskind is now a fellow blogger, blogging at Susskind's Blog: Physics for Everyone.

Update: At Backreaction, there's a new posting explaining what the paradox discussed in Susskind's book really is, at a level more appropriate for physicists.

Update: Some links to reviews. Paul Davies, Sean Carroll, George Johnson. See here for a review of the Johnson review by John Horgan.

Comments

1. **Observer**
   June 23, 2008

   To whom it may concern,

   I realize that this is a bold question but I dare ask it anyway: do we have any objective evidence from astrophysical observations that Black Holes follow the principles of Quantum Mechanics? If not, what entitles both Hawking and Susskind to start from the fundamental assumption that the physics of Black Holes can be described in a quantum framework dominated by locality and unitarity?

2. **Peter Woit**
   June 23, 2008

   Observer,

   The questions about black holes being studied here have nothing to do with currently feasible astrophysical measurements.

3. **Bee**
   June 24, 2008

   Hi Peter,

   Thanks for the review! Sounds as if I should give it a read (when I'm through with the ten books I'm currently reading that is). I've tried the Cosmic Landscape, but lost interest halfways and didn't like the writing style either, so never finished it (though I probably read the epilogue if there was one). Since you mention the urinal in Pupin, I've always wondered what a women in science misses just by using a different restroom 😉 (Otoh, I worked at an institute where the Prof used to distribute duties while the men's restroom, as a result several of the guys started using the women's restroom, no kidding.) Best,

   B.

   PS: Do we know it's really Susskind's blog (or attempt thereof)? I mean, everybody could download a photo and sign with whatever.

4. **anon.**
   June 24, 2008

   'The questions about black holes being studied here have nothing to do with currently feasible astrophysical measurements.' – Woit

   So it's analogous (in not being tied to experimental facts) to stringy theories resolving imagined Planck scale physics problems.

   Bee: since imitation is the sincerest form of flattery, a fake Susskind blog would be easily distinguishable from the real thing.

5. **Peter Woit**

June 24, 2008

Bee,

My personal experience is that I haven't found any significant networking or mentoring opportunities while relieving myself at the Pupin urinals (or any others, for that matter). But maybe that's just me…

The Susskind blog mentioned in my posting is legitimate. I learned about it from information on one of the cover pages of the book. Looks like it's just a blog associated with an introductory course he was teaching, not clear if he intends to use it for other purposes, for instance to discuss his new book.

6. **Christine**
June 24, 2008

Excuse me for being off-topic. At the end of this [interesting news](), one reads:

By continuing their research into the forces of nature, the astronomers also hope to find a window into the extra dimensions of space that many theoretical physicists think may exist.

But no reference is made on how this could be accomplished in terms of the observational research in question.

7. **Professor R**
June 24, 2008

Thanks for bringing it to my attention. I had a look on Amazon and enjoyed the blurb

" an effort that would eventually result in Hawking admitting he was wrong, paying up, and Susskind and t'Hooft realizing that our world is a hologram projected from the outer boundaries of space". Ah

One serious point – I don't like the use of Hawking's name in the title. While I'm sure Stephen won't object, it's a fairly obvious marketing ploy, and can only result in yet more hyping of one particular scientist.
Hard to imagine Bohr writing a book entitled'My battle with Einstein…" isn't it?

8. **observer**
June 24, 2008

Peter,

With all due respect, I disagree with your reply:

"The questions about black holes being studied here have nothing to do with currently feasible astrophysical measurements" – Woit

Physics near any Black Hole singularity most likely falls outside the domain of validity of quantum field theory and statistical mechanics, as we know them. Anon. is right when he states that the situation "it's analogous (in not being tied to experimental facts) to stringy theories resolving imagined Planck scale physics problems"

Regards,

Observer

9. **Peter Woit**
June 24, 2008

Observer,

I don't see what you disagree with. There are no current or planned astrophysical measurements relevant to seeing what happens at a black hole singularity.

Susskind is writing about the study of toy models of quantum gravity, he makes no claim to be considering anything that even in principle can be directly compared to the real world (unlike in the "Cosmic Landscape", where he was advertising supposed potential unified theories).

10. **Christine**
June 24, 2008

There are no current or planned astrophysical measurements relevant to seeing what happens at a black hole singularity.

Not exactly, but of course rotating black hole solutions to Einstein's equations can be inferred from the modelling of iron emission line of accretion disks around black hole candidates, see, e.g., [here](), characterizing the black hole spin.

11. **David P**
June 24, 2008

Dr Susskind explains how physicists think about information theory:

Go to 01:15:30 in the Perimeter Institute Public Lecture: The Physics of Information: From Entanglement to Black Holes

http://www.perimeterinstitute.ca/Outreach/Public_Lectures/View_Past_Public_Lectures/

12. **anon.**
June 25, 2008

The black hole information problem is *not* an "imagined Planck scale physics problem", for those of you who are worried about that; it has nothing to do with physics near the singularity and can be seen entirely within effective field theory on weak-curvature spatial slices.

13. **Bee**
June 25, 2008

Thanks for the link! It was a coincidence I just wrote this post, it had nothing to do with Susskind's book. Best,

B.

14. **George**
June 26, 2008

Why does Susskind accept Intelligent Design Creationism? This was in his Lecture 1. Seems a scientist would, well, be more of a scientist.

15. **Chris W.**
June 26, 2008

George,

I think you misunderstood Lecture 1. He has been known to declare (suggest?) that, if an multiverse-based anthropic explanation for the values of certain apparently universal quantities left underived within the Standard Model does not work, then we will be forced to fall back on intelligent design as the only alternative. I don't know how serious this assertion is supposed to be.

(Leaving aside the essential vacuousness of ID, that is an overstatement.)

16. **hmmm**
June 26, 2008

Susskind was just using a dubious debating ploy, saying in effect that the String Theory Multiverse is not a bad idea, because Intelligent Design Creationism is an even worse idea. It's a bit like saying Aristotlean Epicycles is not a bad idea, because Planets Pushed Around the Sky by Angels is an even worse idea.

Astute listeners/readers see through this debating ploy by noting that there are more than there are more than two ideas, and that not being the worst idea is not the same as being the best idea.

17. **hmmm**
June 26, 2008

I have ordered the book. I'm looking forward to it. I tried reading "An Introduction to Black Holes, Information And The String Theory Revolution: The Holographic Universe", but was completely baffled by it. I might learn some interesting physics from this new book, though I'll read it with a sufficiently large grain of salt when it comes to the author's personal opinions, as with any such book.

However, I am curious about something that maybe physics people might know. Is Susskind actually serious about some of the things he says? He seems funny and entertaining on TV documentaries that I have seen (including one on the topic of this book), but I can't tell if he's just pulling people's leg with what he says. He says that since the String Theory Multiverse idea compares favorably to the Intelligent Design Creationism idea, then the String Theory Multiverse idea must be right! Really? Does such an intelligent person actually believe that kind of logic? He says he at **war** with Hawking! **War**? Really? Especially when you compare it to the hyper-polite debate the Susskind had with Smolin on edge.org, why does poor Hawking get war?

18. **Chris Oakley**
June 27, 2008

Hmmm,

Erm, no.

Susskind is not actually at *war* with Hawking, it is just that Hawking is better known than Susskind and Susskind wants to have his name associated with him.

19. **hmmm**
June 27, 2008

Surely if Susskind really wanted to cynically title his book to boost sales he easily could have worked the word "God" somewhere into the title.

20. **anon.**
June 27, 2008

hmmm, I think that Hawking is bigger than God in the publishing industry. According to the Foreword of Hawking and Leonard Mlodinow, *A Briefer History of Time*, Bantam, 2005, p1:

'*A Brief History of Time* was on the London *Sunday Times* best-seller list for 237 weeks and has sold about one copy for every 750 men, women, and children on earth.'

21. **go**
June 27, 2008

>Astute listeners/readers see through this debating ploy by noting
> that there are more than there are more than two ideas, and
> that not being the worst idea is not the same as being the best
> idea.

As a matter of fact, there really aren't any better ideas other than the multiverse to deal with things like the cosmological constant problem. Multiverse is a logically allowed, but cheap explanation for the problems of physics. There are no successful non-cheap explanations I know.

Anyway, Susskind was just being his quirky self, people who pontificate about his "unscientific" attitude to creationism make me laugh.

22. **George**
June 27, 2008

Chris W.

I do not think I am wrong. Dr Susskind said plainly that, while he preferred Darwin, that those that prefer an Intelligent Designer, that's fine.

That statement is clearly providing "his scientific" credence to ID Creationism. This is something no real scientist could ever do. He has lost the battle for credibility in scientific light – his thinking is clearly not lucid.

His statement is in no way related to physics it was a comment about human evolution or design (as he says is an acceptable alternative view to evolution.)

23. **Peter Woit**
June 27, 2008

Given everything I've seen from Susskind, I don't believe he's a supporter of the idea that ID is as scientific as Darwinism. He clearly enjoys trying to be provocative and outrageous. In one story he tells in the book, he and Feynman are getting a "Feynman sandwich" at the local deli, and Feynman remarks that a "Susskind sandwich" would be similar, but with "more ham".

Anyway, people can watch the video segments involved and make up their own minds, but enough already about this one.

24. **Jonathan Vos Post**
June 27, 2008

I don't remember — what is a Feynman sandwich?

Is it constructed by his path integral formulation of quantum mechanics, his theory of quantum electrodynamics, different from all classical sandwiches because of quantum computation, or extremely small as per his invention of nanotechnology?

The Feynman sandwich on the largest number of shelves is a volume 2 of the Lecture Notes on Physics sandwiched between a volume 1 and a volume 3.

All that I remember was the sandwich he had the owner of the topless bar in Pasadena add to the booze menu, after the City shut it down. That was a ham and cheese sandwich.

Feynman, who liked going there ("after a hard day dealing with oscillating bodies, it's nice to see some oscillating bodies"), succeeded in keeping the place open a few more months. When the city tried enforcing the anti-topless-bar ordinance, the owner could now say: "we're not a topless bar. We're a topless restaurant."

25. **hmmm**
July 1, 2008

go Says: "*As a matter of fact, there really aren't any better ideas other than the multiverse to deal with things like the cosmological constant problem. Multiverse is a logically allowed, but cheap explanation for the problems of physics. There are no successful non-cheap explanations I know.*"

By "multiverse" I am sure you mean the Anthropic Principle. But there are certainly much better ideas than this, for example, Cosmological Natural Selection, as explained by Smolin, Dawkins and others. Susskind knows the arguments. He may not agree with them, but he shouldn't pretend these ideas don't exist.

26. **somebody**
July 1, 2008

hmmm said: By multiverse I am sure you mean the Anthropic Principle. But there are certainly much better ideas than this, for example, Cosmological Natural Selection, as explained by Smolin, Dawkins and others.

The multiverse does not necessarily mean the anthropic principle. In fact your example, cosmic natural selection, is a dynamical (non-anthropic) selection principle in the multiverse. But as it stands, it is more wishful thinking than something concrete. That of course doesn't mean that it is wrong.

27. **hmmm**
July 1, 2008

Ha! I thought somebody might make that point. (Not you specifically "somebody", but *somebody*!) Yes there are many proposals for why the universe is the way it is, and terms such as "multiverse" may potentially be ambiguous, although usually the context makes it pretty clear what is meant, as was the case above.

Anyway, there are ideas that are much better than what Susskind and "go" mean by "multiverse", and they shouldn't pretend these ideas don't exist or irrationally dismiss them.

28. **somebody**
July 2, 2008

"Anyway, there are ideas that are much better than what Susskind and go mean by multiverse, and they shouldn't pretend these ideas don't exist or irrationally dismiss them."

"Much better" ideas? Sorry, I am not sure I know any. There are a lot of wishful thinking/brain-fart level ideas that I know of. But nothing that seems to hold promise for anything concrete (including your favorite cosmic natural selection). Landscape/multiverse is pretty concrete – even though it seems useless for making predictions. But this might be just like field theory: it is useless for making predictions of for example, charges and masses of particles. Its through experiments, not theory, that we fix parameters. Unfortunately, this is an era where experiments are hard.

There, I have the entire "string controversy" in a nutshell for you. 🙂

29. **hmmm**
July 14, 2008

Re: Chapter 20 of Susskind's book.

What's the deal with Alice's Airplane and Bonzo Dog Food? Is he describing something conventional and common knowledge here? I hadn't seen this idea before in popularized physics writings.

Actually these fractally nested propellors would make for a pretty interesting comical hat.

30. **hmmm**
July 20, 2008

Okay, so the book is fun to read and is educational, but maybe the title should be "The Black Hole storm-in-a-teacup". Sure, the underlying questions are deep, and Hawking, Susskind and many others have made important contributions, but QT and GR have not been unified, and nothing has been resolved or proved here.

31. **Dorothy**

July 24, 2008

I recently downloaded Susskind's book from Audible.com and put it on my iPod. I am finding it extremely interesting. Susskind is funny, explains things very clearly. So far, I am about half-way through, I am enjoying it very much. Highly recommended.

32. **Christine**
July 31, 2008

Paul Davies wrote a review of Susskind's new book, it has just been published in Nature.

33. **anon.**
August 27, 2008

The hyperlink to Paul Davy's Nature review of Lenny Susskind's book isn't working, but the correct link is here:

http://www.nature.com/nature/journal/v454/n7204/full/454579a.html

Supplemental US Science Funding

June 27, 2008
Categories: Experimental HEP News

The Senate last night agreed to the House version of a bill that adds some supplemental science funding for FY2008, as part of a large "emergency" bill used to fund the the Iraq and Afghanistan wars. The DOE Office of Science and NSF will each get $62.5 million. The bill contains language giving priority to stopping layoffs and furloughs, and to funding neutrino research at Fermilab. It should allow Fermilab to stop planned involuntary layoffs (furloughs had already been ended by an anonymous $5 million donation).

For more, see this from Oddone, as well as more here and here.

Comments

1. **chris**
   July 1, 2008

   while i of course applaud this additional money to ammeliorate an emergency situation, i find it highly distressing for the future of our field to see how tightly coupled the whole high energy community still is to military expenses and developments. i think that this is an extremely unhealthy development for the long term future of our field. not only will politicians at some point realize that there is no further gain from hep research for the construction of new weapons, but on a more general level, hep will – at the level of the society – not be seen as a cultural endeavor to understand the fundamental workings of nature, but rather as groundworks for the war industry.

   extrapolating maybe 30 years into the future (i know how ludicrious that might be), i am afraid of the scenario where hep research is still in need of large money for large machines and will therefore be all but assimilated by the military complex and distested by public opinion.

2. **Peter Orland**
   July 1, 2008

   People in the highest places of government have long aware that high-energy physics has no military applications, except through spin-offs. I don't think it has been true since the 1970's that we were viewed as an extension of national defense. R. Wilson gave his famous testimony advocating constructing Fermilab ("It has nothing to do directly with defending our country except to make it worth defending.") in 1969. See

   http://www.news.cornell.edu/releases/Jan00/RRWilson_obit.hrs.html

   The attachment of funding to the military is probably a coincidence and should not be taken too seriously.

   Some politicians value our field and others do not, and my impression is that there is little connection with ideology, party affiliation or closeness to the Military-Industrial Complex. We just need to do a better job of promoting ourselves.

Wonders of Gauge Theory and Supergravity

June 27, 2008
Categories: Uncategorized

A workshop in Paris/Saclay is taking place this week entitled Wonders of Gauge Theory and Supergravity and the talks are now online. They show that some exciting new things have been happening in the study of gauge theory and supergravity amplitudes, and I'll make the prediction that this field will attract a huge amount of attention in the coming years (at least until the LHC experiments announce results incompatible with the Standard Model…).

Perhaps the most remarkable part of this whole story is the mounting evidence that N=8 supergravity amplitudes are finite in perturbation theory. Remember the standard story about how quantum theory and general relativity are incompatible that has dominated discussion of fundamental physics for years now? Well, it turns out that this quite possibly is just simply wrong. See Zvi Bern's talk on UV properties of N=8 supergravity at 3 loops and beyond for the latest about this. Bern shows that divergences everyone had been expecting to occur at 3 loops aren't there, and gives evidence that they might also be absent at higher loops. He even sees this as a phenomenon not special to N=8 supergravity, but also occurring in theories with less supersymmetry, e.g. the N=5 and N=6 theories. Among the other talks, Nima Arkani-Hamed's is also about this, advertising the idea that N=8 supergravity is the Simplest QFT.

Much of this story is about the N=4 SYM amplitudes and new insights into them and their relations to supergravity amplitudes, with some of this research growing out of and motivated by the AdS/CFT conjecture of the existence of a string dual to N=4 SYM. Quite a few of the talks are interesting and worth trying to follow, with a much higher proportion of new ideas than is usual at particle theory workshops in recent years.

To go out on a limb and make an absurdly bold guess about where this is all going, I'll predict that sooner or later some variant ("twisted"?) version of N=8 supergravity will be found, which will provide a finite theory of quantum gravity, unified together with the standard model gauge theory. Stephen Hawking's 1980 inaugural lecture will be seen to be not so far off the truth. The problems with trying to fit the standard model into N=8 supergravity are well known, and in any case conventional supersymmetric extensions of the standard model have not been very successful (and I'm guessing that the LHC will kill them off for good). So, some so-far-unknown variant will be needed. String theory will turn out to play a useful role in providing a dual picture of the theory, useful at strong coupling, but for most of what we still don't understand about the SM, it is getting the weak coupling story right that matters, and for this quantum fields are the right objects. The dominance of the subject for more than 20 years by complicated and unsuccessful schemes to somehow extract the SM out of the extra 6 or 7 dimensions of critical string/M-theory will come to be seen as a hard-to-understand embarassment, and the multiverse will revert to the philosophers.

Many of the titles of the talks at Strings 2008 have recently been announced. The plenary talks will include several talks mostly not about string theory, including 3 about the LHC and one by Lance Dixon on the amplitudes story. It seems that the string theory anthropic landscape is a topic the conference organizers don't want anything to do with, since the only person from the Stanford contingent speaking will be Kallosh on prospects for getting something observable out of string cosmology models of inflation. As for what is popular, it clearly helps a lot to be from one of my alma maters, with Princeton (7 speakers), and Harvard (3 speakers) the best-represented institutions.

Update: For an extensive rant about this, see here.

Update: Last week was Paris, this week it's Zurich. Amplitudes are all the rage this summer.

Comments

1. **Coin**
   June 27, 2008

   Something I hear about occasionally is some group of researchers somewhere who believe gravity has a "UV fixed point", which would apparently make gravity renormalizable. Is this "UV finiteness" thing Zvi Bern is describing the same thing or is that a different research program?

2. **Peter Woit**
   June 27, 2008

   Coin,

   The "UV fixed point" proposal is something different, using the renormalization group to tell you how to handle divergences. In what Bern is describing, the divergences are just not there, presumably for some still poorly understood symmetry reason.

3. **Daniel de França MTd2**
   June 28, 2008

   Let me see if I understood the main objective of this line of research. They are trying to prove that N=8 supergravity is renormalizable, and use this to prove that other supergravity theories with lower and lower N can also be renormalizable, until they get N=0, which is just the usual gravity. Is this correct? Why not?

4. **majorana**
   June 28, 2008

   OMG, this is so funny. You and Smolin criticize string theory for not being fully defined and not having precise rules for computing multiloop diagrams and you think N=8 sugra is in better shape? You think the theory will make sense non-perturbatively without string theory? You think you're going to get the SM?

   You're deluded.

5. **Peter Woit**
   June 28, 2008

   Daniel,

   The main point of this research program is that it is showing that something is going on in these higher-loop calculations making the divergence structure much better than expected, something which is not fully understood. There is evidence that at least some quantum gravity theories with enough supersymmetry are finite or renormalizable. I haven't seen any claims that this will be true for N=0. For trying to unify gravity and the SM, you actually don't want the N=0 theory by itself to make sense, but rather have consistency determine the extra fermionic and other fields that we observe.

   "majorana",

"OMG, this is so funny."

You know, you give all appearances of being a 12-year old whose knowledge of this subject is based on reading Lubos's blog. If you actually know anything about this, and want to have a serious discussion about science, please stop with the juvenile behavior.

6. **Daniel de França MTd2**
   June 28, 2008

   Hi Peter,

   "I haven't seen any claims that this will be true for N=0." Well, I point this out: p. 35 (No known susy armunte explains these cancellations). p.38, p.40

   I thought that it was a claim…

7. **Peter Woit**
   June 28, 2008

   Daniel,

   What I meant is that I didn't see any claim that for N=0 you would get cancellations at all orders. It is certainly true that even for N=0 Bern et. al. are finding unexpected cancellations.

8. **Robert**
   June 28, 2008

   Arkani-Hamed aparently previously gave the same (very nice) talk at Perimeter Institute, which is online on video here:

   http://streamer.perimeterinstitute.ca/mediasite/viewer/NoPopupRedirector.aspx?peid=ff9a6fa8-bfde-44b3-a91b-bba7c382720f&shouldResize=False

9. **Daniel de França MTd2**
   June 28, 2008

   Hi Peter,

   He does not claim, but at least this is the impression I got when I read everything, with p. 42 on my mind… Nevermind then…

10. **Patrick Labelle**
    June 28, 2008

    Interesting stuff. But it is my understandind that phenomenologically, theories with N>1 are not viable because they necessarily treat left-handed and right-handed spinors the same way so they cannot accomodate the SM.

    So what is the situation:

    a) For now this is seen as a purely formal exercise to try to understand these mysterious cancellations (that apparently go beyond the usual SUSY cancellations) with no goal to connect to phenomenology

    b) This is only the first step of a program in which the second step would be to somehow reduce the N=8 theory to N=1. (by breaking 7 of the SUSY explicitly? By another approach?)

    c) Something else that I am completely missing

    Thank you for the very informative blog, btw.

11. **Peter Woit**
    June 28, 2008

    Patrick,

    For now the situation is a), Bern says this pretty explicitly. As you note, there are well-known problems with getting the SM out of N=8 supergravity. A lot of people worked on these in the early 80s, but most gave up once the conventional wisdom became that the theory was non-renormalizable, and string theory got going. Maybe some people will start looking at these problems again. In any case, it would be very intersesting to know what is causing this, presumably some sort of non-obvious symmetry in these theories.

12. **Anonymous**
    June 29, 2008

    Hi Peter,

    Two questions:

    (1) I'm new to hep. Can you suggest a suitable text to study gauge theory (perhaps the one you studied from)?

    and

    (2) When is the LHC expected to officially start (at least tentatively)? And how long is the data-gathering process expected to last? If no Higg's particle/s is/are found, will it mean another odd no. of decades before another set of experiments are to replace the current set?

    Would love to read your feedback.

    Regards,
    Jeffrey

13. **Peter Woit**
    June 29, 2008

Jeffrey,

Among the newer QFT books that discuss gauge theory, the one I like best is probably the one by Nair.

Latest about the LHC is that it will be ready to try injecting beams in August, first physics collisions maybe by October, with a month or two of data-taking at low luminosity this year. This won't be enough to find the Higgs. That will take another year or two of data-taking.

Getting to higher energies than the LHC is going to take a long time. The best prospect is probably to increase the LHC energy by using new magnets, but that is probably at least 20 years off if not more.

14. **chethan krishnan**
June 29, 2008

"Perhaps the most remarkable part of this whole story is the mounting evidence that N=8 supergravity amplitudes are finite in perturbation theory. Remember the standard story about how quantum theory and general relativity are incompatible that has dominated discussion of fundamental physics for years now? Well, it turns out that this quite possibly is just simply wrong."

Is perturbative finiteness anything more than a red herring? Of course, it might have useful spinoffs for computing QCD amplitudes etc., but as a fundamental theory, I fail to see the point.

In partidular, it is known that in string theory, just N=8 sugra cannot be consistently decoupled at low energies (Green, Oooguri, Schwarz). So the full UV of pure N=8 sugra is not understood at all, to say the least.

Besides, the perturbative finiteness of N=8 is not that surprising because type I open string amplitudes "square" to type II closed string amplitudes (Kawai, Lllewelyn, Tye), and in d=4 open strings result in the finite N=4 gauge theory, while the closed strings give rise to the supergravity.

What I am surprised by is the claim that some theories other than N=8 might also be finite (even if only perturbatively). Does it have any understanding in terms of strings? Is there something basic that I am missing ?

15. **Arun**
June 29, 2008

If the current program of N=8 supergravity amplitudes being finite works out, it will nevertheless be a four-color-map theorem type of proof. What is encouraging is that there may be a new physical principle to be discovered.

16. **Roger Schlafly**
June 29, 2008

Peter, you are going soft on us. This is just another wacky theory with no connection to reality. It does not because valid or useful just because some of the infinities cancel.

17. **anon.**
June 29, 2008

chethan krishnan wrote:

What I am surprised by is the claim that some theories other than N=8 might also be finite (even if only perturbatively).

Where did you encounter this claim? The claim that is circulating seems to be (a) N=8 may be finite; (b) N<8 is less divergent than expected (but not finite).

18. **Peter Woit**
June 29, 2008

Chethan,

If "the perturbative finiteness of N=8 is not that surprising" because of a simple string theory argument, how come just about all elementary string theory discussions motivate the subject by saying that you need string theory to get perturbative finiteness?

I just don't see any evidence that you need string theory to define N=8 supergravity, or the relevance of the string theory decoupling argument.

N=4 SYM can be defined as a 4d QFT completely independently of string theory, and I don't see why the same shouldn't be true of N=8 supergravity. More interesting though than N=8 supergravity is the larger phenomenon that seems to be going on here of gravity QFTs having some sort of still not understood extra structure or symmetry that may make them finite or renormalizable.

19. **DBM**
June 29, 2008

I think that the problem to get the SM from N=8 sugra is that the R-symmetry sector can not accomodate the SM gauge groups, there are some old papers of kaku-townsend-van nieven…. about it.

20. **chethan krishnan**
June 30, 2008

anon. says:

"The claim that is circulating seems to be (a) N=8 may be finite;
(b) N<8 is less divergent than expected (but not finite)."

So you are saying that the finiteness hope is there only for N=8 sugra, but not so for other theories. But then I do not understand Peter's claim:

"More interesting though than N=8 supergravity is the larger phenomenon that seems to be going on here of gravity QFTs having some sort of still not understood extra structure or symmetry that may make them finite or renormalizable."

Peter also says:

"N=4 SYM can be defined as a 4d QFT completely independently of string theory, and I don't see why the same shouldn't be true of N=8 supergravity."

Does this mean that you consider a Feynman diagram expansion a full description of the theory? The only possible non-perturbative completion of N=8 sugra that I know of is in string theory, and there it does not decouple. That does not bother you?

"If the perturbative finiteness of N=8 is not that surprising because of a simple string theory argument, how come just about all elementary string theory discussions motivate the subject by saying that you need string theory to get perturbative finiteness?"

There are many field theories which are finite. Nobody invokes string perturbation theory to argue their finiteness. The string theory lesson here is NOT about the finiteness of N=8 itself, but that the gauge theory divergence structure here might be related to the gravity case, and so the gravity case might not be as bad as one would have imagined naively.

"N=4 SYM can be defined as a 4d QFT completely independently of string theory, and I don't see why the same shouldn't be true of N=8 supergravity."

Because of gravity! Gauge theories usually have good non-perturbative definitions. Not so for gravity.

21. **Boels**
June 30, 2008

Just some comments:

– The expectation that N=8 diverges at three loops is based on 80-ies technology in proving nonrenormalisation theorems. People like Stelle and collaborators have updated their techniques recently and also find that the 3-loop divergence is not there. I'm not quite sure what the latest prediction is. They are still hoping to find a symmetry proof of finiteness.

– A topologically twisted (?) version of N=8 is not interesting phenomenologically, I think.

– At the exact same conference Berkovits announced that he and Maldacena have some understanding of the appearance of 'dual conformal invariance' in N=4 multiloop computations. This understanding is based on string theory through the AdS/CFT correspondence.

22. **EDT**
June 30, 2008

Peter,

Those links are dead. Am I the only one who can't access them?

23. **Peter Woit**
June 30, 2008

Chethan,

I just don't believe claims you're making about "non-perturbative completion".

The main claim always made for string theory is that it is a "UV completion" of supergravity. This is based on the conventional wisdom that supergravity is ill-defined at high energies due to divergences in higher loop terms of the perturbation expansion, something which is not supposed to happen in superstring perturbation theory. If Bern et. al. are right, N=8 supergravity doesn't need a "UV completion", it is UV complete, in the conventional sense of that term, which is about perturbative calculations.

Sure, for both QFT and string theory, a truly well-defined theory requires a formulation more general than perturbation theory. It's one of the fundamental problems of string theory that this is missing for string theory, so I don't see how one can claim that the significance of string theory is that it provides a "non-perturbative completion".

In AdS/CFT, a perturbative string is supposed to allow a useful approximation to the strong coupling limit of the gauge theory, but there the situation is exactly the opposite of what you want. The gauge theory by itself appears to be defined non-perturbatively, at all couplings (e.g., use a lattice), whereas the string theory is only defined at weak coupling. Here the QFT provides a "non-perturbative completion" of the string theory, not the other way around.

24. **Peter Woit**
June 30, 2008

EDT,

That server seems to be down now, presumably it will come back up at some point. If it doesn't, and the web-site for that conference has moved, please let me know and I'll update the links.

25. **wm**
June 30, 2008

FWIW, it has been suggested recently (link below) that the "still not understood structure" responsible for finiteness involves string theory dualities and symmetries, which points to a much more prominent role for string theory than the role allowed in the post here.
http://arxiv.org/abs/0806.1726

26. **somebody**
June 30, 2008

Though it MIGHT be a logical possibility to think about N=8 supergravity as a standalone theory, it really makes a lot of things (BPS solitons for example) significantly more artificial. From such a point of view, it is merely an extraordinary coincidence that the theory seems to have a natural 11 dimensional origin.

Maximal supersymmetry is most natural in 11 dimensions. If we are working with maximal local supersymmetry, it takes a lot of effort to NOT believe in string theory, considering all we have learnt in the last decade or so. Most of what we have learnt is based purely on supersymmetry.

27. **p falor**
July 1, 2008

Hi Peter:
Read your book but I am confused on a couple of things that are unclear to me. Maybe you can help. I understand that ST is referred to as not A QFT. But I read one QFT

book (the first section) that suggested ST was a QFT but just used a different way of treating time as opposed to standard QFT. If ST is not a QFT are there still virtual particles in ST? Please help. Also I have read that GR is/is not a gauge theory. Obviously only one viewpoint is correct. Thanks.

28. **Daniel de França MTd2**
July 1, 2008

Hi Peter,

I found an argument somewhere else…

"You might propose to break the N=8 supersymmetry spontaneously. However, N=8 supersymmetry cannot be spontaneously broken to N=1 or N=0 supersymmetric – the latter being realistic choices. In fact, even N=2 supersymmetry is too constraining and cannot be spontaneously broken to N=1 or N=0, at least not by field theoretical methods in four dimensions. If you can't break the N=8 supersymmetry spontaneously, you may want to break it explicitly. However, in that case, you lose the cancellations completely [unless you break it explicitly by using M-Theory/String Theory."

I would add, unless cancellations at lower N are also a myth. Is that correct? Or is there more to it?

29. **Peter Woit**
July 1, 2008

Daniel,

As I keep repeating, completely independently of its divergence problems, no one knows a viable way to get the SM out of N=8 supergravity. You can try and do this by adding explicit breaking terms, but then it's no longer N=8 supergravity. What's interesting here is NOT just the specific result about N=8 supergravity, because we know that theory by itself is not enough to give us what we need. What is interesting is that the assumption the whole field has been built on for 25 years, that QFT is inherently incompatible with quantum gravity because of high energy divergence problems, appears to be wrong. The interesting question is what is causing this, and how to identify the class of QFTs that don't have divergence problems and can give quantum gravity. Maybe one of these will be viable.

One interesting sociological question is how long it will take for string theorists to stop repeating the claims about incompatibility of QFT and gravity that they have convinced almost everyone about, now that these claims appear to be wrong.

30. **Peter Woit**
July 1, 2008

p falor,

String theory is a quantum theory, but not a theory of quantum fields on space-time, and thus not a QFT. You can try and formulate it as a theory of quantum fields on the infinite dimensional space of all paths in space-time ("string field theory"), but that's not what you normally mean by QFT.

Perturbative string theory is given by an infinite sum of terms, each term of which can be thought of as a QFT on a 2d surface of a given topology. This QFT is a conformal QFT, and you have to couple it to a 2d quantum gravity on the surface (i.e. integrate over all possible metrics on the surface). But string theory is supposed to be something non-perturbative, and so go beyond this infinite sum of terms, each of which can be thought of as a (different) QFT.

When we say "gauge theory", we normally mean a theory with internal gauge symmetry (mathematically, the symmetry of all "vertical" automorphisms of the bundle structure), and the Yang-Mills action (curvature-squared). Gravity isn't a gauge theory of this kind. But there are two ways you can try and think of it as something similar. One is to use the bundle of orthonormal frames, which has a standard gauge symmetry. But this bundle has extra structure (the vierbeins), not available in usual gauge theory, and this gets used to construct the Einstein-Hilbert action. So, this goes beyond the usual gauge theory: there are extra fields and different terms in the action. A second way to go is to try and think of the infinite dimensional symmetry of space-time coordinate reparametrizations as something like a gauge symmetry. One can do this, but it is a different kind of symmetry than the standard gauge symmetry.

Sorry for the somewhat technical answer, but it's a rather technical question…

31. **manyoso**
July 1, 2008

Peter,

I see you've posted a link to Motl's insistent plea that N=8 Supergravity is intimately tied to his beloved String Theory. Do you have a technical response?

32. **Peter Woit**
July 1, 2008

manyoso,

I've already responded to the claims that "you need string theory to define N=8 supergravity non-perturbatively" above. See my response to Chethan.

As for the rest of what Lubos writes, he goes on and on about all sorts of things that just aren't relevant. Sure, there are all sorts of interesting relations between N=8 supergravity and string theory, but they don't add up to "string theory is needed to define non-perturbative N=8 supergravity". String theory itself has a much more serious unsolved problem of how to define it non-perturbatively than supergravity does. It's true that perturbative supergravity for instance can't completely describe black holes, but this is equally true of perturbative string theory.

In any case, for the N'th time, what's important here is not the special case of N=8 supergravity, which in any case isn't a viable unified theory. What is important is that it appears that there are QFTs for quantum gravity which don't have the expected divergence problems. This is big, exciting news and opens up a lot of non-string theory possibilities to investigate for anyone interested in quantum gravity. Lubos and others are just trying to obfuscate this simple fact.

33. **Daniel de França MTd2**
July 1, 2008

I guess the techinical response is shown in the conference articles… I would just like to find what is the reason many of these guys suspects that the renormalizations effects are not due to supersymmetry. I mean, I would like to find an article, in which a calculation makes them to think like this.

34. **Observer**
July 1, 2008

Peter,

“What is interesting is that the assumption the whole field has been built on for 25 years, that QFT is inherently incompatible with quantum gravity because of high energy divergence problems, appears to be wrong.”

As of today, quantum gravity remains a theory plagued with many unsolved computational challenges, with no firm foundation and no experimental basis. Apart from renormalizability, there are many unsettled questions such as: Does quantum gravity require smooth manifolds and can it be consistently built in a perturbative framework ?. On the contrary, there is overwhelming evidence that perturbative quantum field theory is a correct framework, at least at the Standard Model level. If such a huge gap exists, on what basis can one say that the two appear to be compatible?

Regards,

Observer

35. **p falor**
July 1, 2008

Hi Peter:
Thanks for the reply. I believe it does help improve my understanding. Still there is one other issue that I am really have a hard time understanding that you alluded to in your book. The CC problem. Now I understand a QFT calculation gives the CC 60-120 orders of magnitude too large. This calculation is based on supposely sound physics principles. The same ones that are used to calculate the Hawking radiation from a black hole that is apparently accepted by most (all) HEP theortical physicists.

How can the current QFT frame be valid, i.e., assumptions etc, if this supposely basic calculation is so wrong? Maybe the vaccuum energy doesn’t couple to gravity but this would counter GR correct?

But more puzzling to me (you may not be able to answer this) is that in ST assuming a landscape, a vacuum state is being searched for that has a small CC. This suggests to mean that ST does not view the vacuum energy the same way as a QFT does. How can one a assign a value to a basic calculation? Shouldn’t the theory determine the value?

All of this maybe very obvious to those in the know but to someone like me I am hopelessly confused. Can you help? Thanks.

36. **Peter Woit**
July 1, 2008

Observer,

I’m not claiming that QFT and gravity are definitely compatible, just noting that the conventional reason given for claiming that they are definitely incompatible now appears to be wrong.

37. **Chris W.**
July 1, 2008

Maybe these unexpected cancellations in supergravity will ultimately throw new light on the failure of past efforts to calculate a sensible vacuum energy density in QFT.

Just a thought…

38. **Aaron Bergman**
July 1, 2008

Now I understand a QFT calculation gives the CC 60-120 orders of magnitude too large.

This isn’t true. The cosmological constant is a superrenormalizable parameter that can be set to whatever you want. The problem is that making it small is a huge fine tuning.

39. **somebody**
July 2, 2008

Like any useful toy model of gravity, N=8 sugra has black holes. Because of the gauge fields that are automatically there in the theory, these can be charged black holes. The only way a purely perturbative notion of consistency can exist for N=8 sugra, is if one declares by fiat that these perfectly reasonable objects in the classical theory are not states in the quantum theory. This is very hard to justify, especially in a supersymmetric theory because in supersymmetric theories, these ideas have produced perfect agreements with classical black hole entropies etc.

On the other hand, if one includes these as states in the theory, and imposes the uniqueness of the wave function, the theory (or rather its BPS sector, which has sufficient structure to make the comparison non-trivial) is exactly the same as what one expects from string/M theory. People who believe in string theory believe that this coincidence (and many others) could not have happened if there was no truth behind it. It must be admitted that this kind of evidence is still circumstantial, but I personally find it highly non-trivial and therefore compelling.

I think Peter is right that the oft-cited problem of gravity is its perturbative non-renormalizability. Often the sales-picth for string theory has been that strings are finite. But I think people focussed on this issue because before N=8 came along, this was argument enough! But now that there exists a theory that is perturbatively (perhaps) finite, we really have to emphasize that it is the full quantum consistency that we are after. Perturbative finiteness is necessary, but certainly not sufficient.

The good news is that even without having a FULL non-perturbative definition of gravity/string theory, we can still make non-trivial statements about non-perturbative aspects from quantum consistency alone. This is the sense in which string theorists say that N=8 is not “non-perturbatively” complete. This statement certainly does not imply that we have the full non-perturbative definition of the theory. The claim is that in the only quantum mechanical framework where we know how to make sense of the classical BPS objects of the theory, it does NOT make sense to just look at the supergravity multiplet – because there are extra massless fields around.

String theory might or might not be directly useful for building a model for our universe. I think arguments against string theory have maximum mileage when they focus on the practical usefulness of string theory in understanding our vacuum. But as a theoretical superstructure, I think it is hard to argue against it.

40. **p falor**
July 2, 2008

Aaron Bergman:
According to Sean Carroll

"Unlike supergravity, string theory appears to be a consistent and well-defined theory of
quantum gravity, and therefore calculating the value of the cosmological constant should,
at least in principle, be possible."
He isn't suggesting that the CC is a constant to be defined but something that can be calculated from a theory. Other references clearly imply(at least to me) that the vacuum energy should be calculable from QFT.

Obviously I am missing something but would appreciate a simpler explanation of what that is.

Thanks.

41. **somebody**
July 2, 2008

p falor,

The idea is that vacuum energy gets contributions from all the fields in the theory, like electrons, photons, etc. Lets say that before you added these contributions, the "bare" CC was some number A (we do not know its value beforehand). After you add the contributions from everything, lets say that the result is A+B. It turns out that in quantum field theory, B is usually a huge (computable) quantity. But the final result for CC, namely A+B, is experimentally known to be a miniscule number. This means that we are forced to choose ("fine tune") A to be a huge, but negative number almost (but not exactly) equal to B so that the sum of A and B is the small experimentally measured value of CC.

We say that the "natural" scale of the cosmological constant is that of B, and that the observed CC is absurdly small.

What Aaron is saying is that because CC is a certain kind of parameter, you are in fact allowed to tune it. You are not in a situation where there is actually a contradiction, because you are not tuning something that is NOT allowed to be tuned. In that sense, the CC problem is not really a problem, because it just refers to the unease that physicists feel, when they do this fine-tuning.

What Sean was talking about was the hope in string theory that perhaps CC would be automatically fixed by the theory itself. But nowadays, it seems much more likely that CC in string theory is essentially like a parameter in field theory. So it might not be dynamically determined. This just means that string theory is much more like like any other theory than many string theorists hoped: it needs experimental input to fix its parameters.

42. **Peter Woit**
July 2, 2008

somebody,

I'm not disagreeing that how to come up with a consistent non-perturbative N=8 supergravity is an issue, just with the idea that we know that the answer to this is non-perturbative string theory, since we don't even know what non-perturbative string theory is (the BPS sector story provides an interesting possible constraint, but you still don't know what the theory is that you are claiming solves the problem, or even whether there is more than one way of doing this).

p falor,

Sean Carroll's statement makes two assumptions. The first is that gravity QFTs inherently are non-renormalizable (which now appears to be wrong), the second is the existence of a single consistent non-perturbative version of string theory (which doesn't yet exist). If such a single consistent string theory existed, presumably you could calculate the CC in it. Nowadays, with the "landscape", most string theorists seem to have adopted the ideology that the CC actually can't be calculated, since there are essentially an infinite number of very different grounds states, all with different CC.

As for the argument that all we have to do to get predictions about string theory is "fix a parameter", sorry this is nonsense. String theory does not provide a prediction about what will happen at the LHC in terms of some number of parameters that need to be fixed by experiment. It predicts nothing about what will happen at the LHC, no matter how many parameters you give it.

43. **Chris W.**
July 5, 2008

Off-topic, but perhaps worth mentioning: Lincoln Wolfenstein has reviewed in PhysicsWorld a new book for a general audience by Helen Quinn and Yossi Nir, which focuses on matter-antimatter asymmetry but also appears to offer a nice overview of the Standard Model.

44. **p falor**
July 6, 2008

Somebody, Peter,

I think I see Somebody's point: QFT calculates a very large number, but the observed CC is very small (relatively) so to make the calculation come out we just subtract another very large number. And presto it works. My point was more along the line since QFT supposely can calculate the vacuum energy generated by the virtual particles but gets the wrong answer is there some reason to doubt the virtual particle framework of QFT?

Others have suggested that there is only suggestive evidence for virtual particles as predicted by QFT. Since the 1920's particle physicists have been aware of this CC potential problem but ignored it until yhe last 25 years or so. It sounds like the problem is just going to be defined away. True the constant that must be added to the calculated vacuum energy has to be very fined tuned but what is the current alternative?

45. **somebody**
July 6, 2008

> My point was more along the line since QFT supposely can
> calculate the vacuum energy generated by the virtual particles
> but gets the wrong answer is there some reason to doubt the virtual particle
> framework of QFT?

A few pointers:

1. As I said, QFT does NOT make a wrong prediction for CC. The freedom of adding the bare value is not an option, it is a must. In the case of other renormalized couplings also (like fermion mases, charges etc.) we do this. But in those cases, the bare piece and the computed piece and the actual value are all roughly the same order of magnitude.

2. QFT is simply the most spectacularly successful theory there is in everything else.

3. Any local, quantum mechanical theory that respects special relativity at low energies will look like a QFT. This is almost a theorem, but maybe there is some way to tweak it, in which case you have to deal with #2.

46. **Peter Woit**
July 6, 2008

p falor,

Virtual particles appear in any QFT calculation above tree level, and many of these (especially in QED) are tested to very high precision. You can't get rid of either virtual particles or QFT.

The fact that we can't calculate the CC is not surprising, since we don't have a viable unified theory of gravity and the SM. What is (a little) surprising is that naive order of magnitude estimates are completely wrong. This is a hint, but not much more, and we don't know what it means. About all one can say is that it means that there's more to the solution of this problem than a naive gluing together of the SM and GR would indicate.

47. **Observer**
July 12, 2008

Peter, you say:

"The fact that we can't calculate the CC is not surprising, since we don't have a viable unified theory of gravity and the SM."

Is there any legitimate basis for the assumption that the vacuum of GR and the vacuum of QFT are identical entities? GR refers to macroscopic scales and ceases to be relevant below the scale of nuclear processes. Why then do you expect that a consistent quantum theory of gravity is the root cause of the CC problem?

Best regards,

Observer

48. **commonsense**
July 12, 2008

While we're at it, let me (mis?)interpret what Peter said another way. I took it that if some physical model tells us that two quantities are fine tuned so as to cancel to 120 (or 60?) decimal places, and that such fine tuning is an extremely improbable circumstance, then instead it is the physical model itself that should be regarded as being extremely improbable to be the right one. In other words, maybe when a unification of GR and QFT is found, it will be understood that the concept of fine tuning was simply mistaken.

49. **Peter Woit**
July 12, 2008

Observer, commonsense,

You really need some way of putting QFT together with GR to make the question of calculating the CC a well-posed one.

The "10^60" number one sees refers to SSYM, where supersymmetry breaking will introduce a CC of the wrong scale. I think supersymmetry breaking is already a deadly problem for conventional SSYM models, this is just one more reason not to believe them.

The "10^120" number is based on assumptions about quantum gravity and the Planck scale, there the lack of a convincing theory is the problem.

50. **Eric**
July 12, 2008

"The "10^60″ number one sees refers to SSYM, where supersymmetry breaking will introduce a CC of the wrong scale. I think supersymmetry breaking is already a deadly problem for conventional SSYM models, this is just one more reason not to believe them."

Of course you are ignoring supergravity theories where the cc can easily be fine-tuned, or in the case of no-scale supergravity, the vacuum energy automatically vanishes at tree-level.

51. **Observer**
July 12, 2008

Peter, regarding your reply:

"You really need some way of putting QFT together with GR to make the question of calculating the CC a well-posed one."

I'd like to suggest that this is precisely the source of misunderstanding defining the CC problem. Vacuum in GR is an idealized cosmic state devoid of matter and energy. On the contrary, QFT vacuum is a quantum state embodying the contribution of all zero-point fluctuations. There is no compelling argument for relating the two concepts in a meaningful way.

Best regards,

Observer

52. **Peter Woit**
July 12, 2008

Eric,

It's rather well-known that the CC problem is not that it can't be fine-tuned, but that it has to be.

53. **Eric**
July 12, 2008

Peter,
All that means is that the theory we are using is an effective theory. It's really no different than having to fine-tune the masses of quarks and leptons in the Standard Model.

54. **p falor**
July 13, 2008

Peter:
It seems like that the CC problem is to be treated as just a renormaliztion issue as is the mass and charge of the bare electron in QED. Correct? But the bigger question is: Is QFT a fundementally correct approach if a basic calculation is so wrong? If QFT is deemed to be fundemental correct what mechanism(s) would potentially have to be included in some extention of the SM to avoid this problem (fine tuning)?

55. **Peter Woit**
July 13, 2008

p falor,

All I'm doing is repeating myself at this point, but again:

1. The "CC problem" is ill-posed unless you have a viable unified theory. We don't.
2. Naive expectations about what the scale of the CC would be in various possible unified theories lead to a completely wrong scale.

Personally, I don't think one can conclude anything very interesting from 1. and 2.

56. **Daniel de França MTd2**
August 11, 2008

There is a new interesting paper from Arkani-Hamed, and collaborators, "What is the Simplest Quantum Field Theory", http://arxiv.org/PS_cache/arxiv/pdf/0808/0808.1446v1.pdf. It has interesting claims :

"Both tree and 1-loop amplitudes for maximally supersymmetric theories can be completely determined by their leading singularities, it is natural to conjecture that this property holds to all orders of perturbation theor. This is the nicest analytic structure amplitudes could possibly have, and if true, would directly imply the perturbative finiteness of N = 8 SUGRA. All these remarkable properties of scattering amplitudes call for an explanation in terms of a "weak-weak" dual formulation of QFT, a holographic dual of flat space.

Physics Nobel Laureates at Lindau

July 1, 2008
Categories: Uncategorized

This week there's a Nobel Laureate Meeting in Lindau, devoted to physics. Many of the talks can be viewed on-line. From 3-5pm today (Lindau time) there will be a session devoted to a panel discussion of expectations for the LHC. Besides the Lindau web-site, a webcast will also be available here.

Blogging (in German) is going on here, including accounts of the late night activities there, featuring pictures of physicists dancing to the tune "Sex Bomb".

Update: The panel discussion included two questions from the audience about the multiverse. At first Gross refused to address them leaving cosmologist Smoot to try and say something. Finally 't Hooft broke in to say that there were a lot of misconceptions being spread about the multiverse, but that the truth was that the LHC will never have anything to say about either the multiverse or string theory, and Gross did not disagree with him. 't Hooft explained that while in principle there could be indirect evidence for a multiverse (from direct evidence for aspects of a theory that implied multiple universes), at the moment the idea was completely untestable and the LHC would have nothing to say about it. Gross agreed, describing multiverse models and research as "very ill-defined".

At the end, an argument between Veltman and others broke out over the selling of particle physics using astrophysics. He described claims that the LHC will "recreate the Big Bang" as "idiotic", and as "crap". He said that this is "not science", but "blather", and that the field would come to regret this, arguing that if you start selling the LHC with pseudo-science, you will end up paying for it. Gross and Smoot politely disagreed.

Update: See here for Gross's talk on expectations of what will be seen at the LHC. He predicts definite observation of a Higgs particle, and says he has taken bets that supersymmetry will be seen, at 50-50 odds. Nothing about string theory at the LHC.

Comments

1. **MIM**
   July 1, 2008

   I watched part of it, thanks to this post, and I am still amazed by Veltman's opinion on astroparticle physics. I am not talking about this Big Bang advertising you are reporting, but about the other part of the argument: it was quite clear that for him astrophysicists and particle physicists did not belong together. What is wrong with trying to join efforts in our understanding of Nature? I naively thought everyone agreed that both domains are intimately connected and that more communication is not only useful, but crucial. He also mumbled that astrophysicists had 'invented' the dark matter and dark energy problems, that this was the astrophysicist's way: inventing things when they did not understand the data. For this, I agree, they are puzzled by data and try to explain it. Isn't it the essence of science after all?

2. **Peter Woit**
   July 1, 2008

   MIM,

   I think part of this is that Veltman enjoys taking extreme positions….

   While he goes too far, I think he does have a point that some of the claims being made about the intersection of particle physics and astrophysics are misleading and nonsensical. No, the LHC is not recreating the big bang.

3. **Shantanu**
   July 1, 2008

   MIM, See Veltman's talk at the 2001 space-time odyssey symposium. the talk is online , but I can't find the link now and I think it was discussed on Peter's blog her earlier. If I understand right Veltman believes that teh dark problem is a failure of gravity.

4. **Bee**
   July 1, 2008

   Interestingly, only yesterday I was replying to a comment over at our blog that referred to the 'big bang recreation' advertisement. It is very misleading indeed, and I am not actually sure where it comes from. I have heard as a motivation for heavy ion collisions that one will recreate conditions 'closer' to the big bang than ever before. That I can at least relate to, but 'closer than ever before' is far off 'recreation of'. Besides, I don't see what pp collisions are supposed to recreate in this regard.

   I totally agree with Veltman, one should be way more careful how one 'sells' science. All this advertising isn't something that should play any role in research, not even when it comes to 'selling' it to the public. Scientists have above everything else a responsibility to be careful and accurate. That is our job, and if we neglect it we will have to pay for it with losing trust that is very hard, if not impossible, to reestablish.

5. **MIM**
   July 1, 2008

   I agree that the LHC should not be advertised as a 'Big Bang machine'. Even the claims about heavy ion collisions, which seem more justified than the LHC ones, are laughed at by some heavy ion physicists. Some were telling me recently how this claim was always made to get fundings but how they had no idea, and no program, to link their analysis with early times. This is a problem in the long term, as people will eventually ask: well, what have you learned about the early universe from LHC? About Veltman's position on astroparticle physics though, I am still puzzled. If dark energy can most probably be explained in the GR framework, what about gravity and dark matter? Has any MOND ever explained the acoustic peaks in the CMB anisotropies? One obviously can't rule out the particle hypothesis but then, if Veltman is only amusing himself by taking extreme positions…

6. **Shantanu**
   July 1, 2008

   MIM,
   I would say that the answer to your questions about acoustic peaks is yes.
   See http://arxiv.org/abs/astro-ph/0505519

7. **Observer**
July 1, 2008

Peter,

Thanks for this informative link!

What is your take on the fact that an important meeting of this caliber is held in Germany and not in US? Does this simply mean that US has lost its role as leader in many fields of scientific endeavor ? (it may unfortunately be a rhetoric question…)

Regards,

Observer

8. **Bee**
July 2, 2008

Observer: The Lindau meetings are always in Lindau. See the website.

9. **Markk**
July 2, 2008

"What is your take on the fact that an important meeting of this caliber is held in Germany and not in US? Does this simply mean that US has lost its role as leader in many fields of scientific endeavor"

Uhm.. Lindau meetings are over 50 years old and were established when one could say US influence on particle physics was at its highest. I think you have to look at teaching, funding and excellent research to see how healthy the US scientific endeavor is – not comparisons to others. I sure as heck hope that others are at least the equal of the US in "scientific endeavor"! Speaking as an American the stronger science is in the world the better for science everywhere. It is not a zero sum thing.

10. **Deane**
July 2, 2008

I don't know about the sciences, but in mathematics I do have the impression that most small high caliber conferences, especially those in the summer, take place in Europe. I offer two possible reasons:

a) There are a lot more venues specifically designed for these conferences available in Europe. Moreover, if one wants to attend more than one within a short period, it is easy to travel between the different locations.

b) A lot of American mathematicians, given the choice, would prefer spending the summer traveling around Europe over the US.

11. **theoreticalminimum**
July 2, 2008

Completely out of context (apologies). Just found out that Indian string theorist Sunil Mukhi (TIFR) has a blog.

12. **big vlad**
July 2, 2008

just listened to David Gross' on the web. Really great talk! I like the way he emphasized the 'fermionic dimensions' aspect of susy, which is often not mentioned. In fact, listening to this talk has inspired me to relearn the superspace formalism, which I forgot long ago…

13. **Professor R**
July 2, 2008

Thanks for that super link Peter.
I just sat through David Gross's talk in its entirety, absolutely excelent talk. I particularly enjoyed his 'big three' reasons for SUSY, and his explanation of difficulties in detection at LHC…I wonder who his bet was taken with.

14. **Bee**
July 3, 2008

Hi MIM

Even the claims about heavy ion collisions, which seem more justified than the LHC ones, are laughed at by some heavy ion physicists. Some were telling me recently how this claim was always made to get fundings but how they had no idea, and no program, to link their analysis with early times. This is a problem in the long term, as people will eventually ask: well, what have you learned about the early universe from LHC?

Indeed. I've been sitting among heavy ion physicists for quite a while and the 'big bang' motivation appears always and everywhere where it goes to a non-specialist audience (that includes grant proposals), despite the fact that the actual relation is weak. This isn't a problem though that is special to heavy ion physics or physics. It's a far more general trend, the constant dumbing down of information and the idea that one has to 'sell' and 'advertise' and so on. It's a trend that should have no effect on science, but sadly it has.

Seems I say that a lot – that sociological trends reflect in the scientific community and have a very negative influence. Best,

B.

15. **a.k.**
July 3, 2008

a bit off-topic, off course, one could remark, since the link was given, that at least on the blog-level the Lindau-meeting helped to elucidate some facts which are, apart from producing Nobel-laureates, still part of the german culture: producing prejudice and condescendence towards the 'lesser developed' cultures. While this in not a exclusively german phenomenon, I am glad that for instance this interview was not translated into english, were one introduces a PhD-student from Cameroon as someone who 'is allowed to take part', there are similar examples of near-to-racism occuring in this blog. I should apologize that I introduce these themes at this point, since their relation to heavy-ion-collisions and the big-bang is not-so-obvious, nevertheless it exemplifies what the word 'dialectic' means for the relation between science and humanities.

16. **Cormac O'Raifeartaigh**
July 4, 2008

Still enjoying the talks at that link, thanks again Peter..

That siad, I was underwhelmed by George Smoot's talk. Entitled ' The beginning and development of the Universe', it promised a lot more than it delivered. This was what I call a type II lecture – everything was probably there somewhere, but not in any order one could make much sense of.

Smoot mentions the acceleration of the universe early on, without any discussion of the universe expansion. or Hubble's law. Similarly, he launches into a description of measurements of the cosmic microwave background without giving any explanation of its importance as a snapshot of the early universe. Finally, while there was plenty of talk of both the COBE and WMAP satellite experiments, there is no mention of the 'why' – i.e. the advantages of satellite measurements over ground-based observation.

In summary, this talk was strangely reminiscent of a talk given by Smoot's co-laureate John Mather at Trinity College Dublin last year. There should be a law for experimentalists – if you're going to give a talk about your area (cosmology), you need to spend a few minutes on the basics – univ. expansion, nucleosynthesis, backgound radiation and inflation models etc

I'm fairly sure any members of the audience not familiar with BB theory left that lecture no wiser than before…

17. **Bee**
July 7, 2008

Regarding the 'Recreation of the Big Bang', we have written a brief post that hopefully explains the major differences between the LHC and the beginning of the universe: Recreating the Big Bang?

18. **Professor R**
July 7, 2008

Peter, I'd be interested to hear the LHC discussion session you mention, but the link you give doesn't seem to go anywhere, nor does the webcast seem to be listed on the CERN webcast site. Do you have another link to it?
Cormac

19. **Peter Woit**
July 7, 2008

Cormac,

That link was for the live webcast, I haven't seen a recorded version. If someone knows of one, let me know.

Proof of the Riemann Hypothesis?

July 2, 2008
Categories: Uncategorized

Last night a preprint by Xian-Jin Li appeared on the arXiv, claiming a proof of the Riemann Hypothesis. Preprints claiming such a proof have been pretty common, and always wrong. Most of them are obviously implausible, invoking a few pages of elementary mathematics and authored by people with no track record of doing serious mathematics research. This one is somewhat different, with the author a specialist in analytic number theory who does have a respectable publication record. Wikipedia has a listing for Li's criterion, a positivity condition equivalent to the Riemann Hypothesis.

Li was a student of Louis de Branges, who also had made claims to have a proof, although as far as I know de Branges has not had a paper on the subject refereed and accepted by a journal. He describes his approach as using a trace formula and "in the spirit of A. Connes's approach". Li thanks

> J.-P. Gabardo, L. de Branges, J. Vaaler, B. Conrey, and D. Cardon who have obtained academic positions in that order for him during his difficult times of finding a job.

but it is a little worrisome that he doesn't explicitly thank any experts for consultations about this proof. If the arXiv submission of the preprint is the first time he has shown it to anyone, that dramatically increases the already high odds that there's most likely a problem somewhere that he has missed.

I'm no expert in this subject, so in no position to check the proof or to have an intelligent opinion about whether his method of proof contains a new, promising idea. I suspect though that experts are already looking at this proof, and it appears to be written up in a way that should allow them to relatively quickly see whether it works. Given the history of this subject, I think the odds are against Li, but I'm curious to know what experts think of this.

This also has appeared on Slashdot. If your comment is like any of the ones there, please don't submit it, but comments from the well-informed are strongly encouraged.

Update: It looks like a problem with the proof has been found. Terry Tao comments on his blog

> It unfortunately seems that the decomposition claimed in equation (6.9) on page 20 of that paper is, in fact, impossible; it would endow the function h (which is holding the arithmetical information about the primes) with an extremely strong dilation symmetry which it does not actually obey. It seems that the author was relying on this symmetry to make the adelic Fourier transform far more powerful than it really ought to be for this problem.

Update: Another Fields medalist heard from: Alain Connes comments as follows on his blog:

> I dont like to be too negative in my comments. Li's paper is an attempt to prove a variant of the global trace formula of my paper in Selecta. The "proof" is that of Theorem 7.3 page 29 in Li's paper, but I stopped reading it when I saw that he is extending the test function h from ideles to adeles by 0 outside ideles and then using Fourier transform (see page 31). This cannot work and ideles form a set of measure 0 inside adeles (unlike what happens when one only deals with finitely many places).

Update: The paper has now been withdrawn by the author, "due to a mistake on pg. 29".

Comments

1. **Chip Neville**
   July 2, 2008

   Peter,

   If you will accept a comment from a competent mathematician who is NOT an expert in analytic number theory, Li's claimed proof of the Riemann Hypothesis looks like it might be the real deal. First, it is impressive that Bombieri and Lagarias thought enough of his positivity criterion to generalize it. Second, his paper is clearly written, and in fact serves as a good roadmap into various arcane developments in number theory such as adeles and ideles. Third, his paper follows a program, first enunciated in the 1970's, for approaching the Riemann Hypothesis by studying the reals and the p-adic numbers simultaneously in one large entity, the direct product of all of these. Fourth, he clearly understands the deep relationship between the Mellin transform, the Riemann zeta function, and the invariant measure dt/t on the positive reals. In fact, the measure dt/t is Haar measure on the group of positive reals under multiplication, the Mellin transform is the Fourier transform with respect to this invariant measure, and the zeta function is essentially the Mellin transform of the fractional part function, t – [t]. Fifth, there are some new ingredients in his proof, especially his positivity criterion and various things about special functions from Whitaker and Watson.

   The only errors I could find are a misspelling of Dedekind's name on page 2, and a few lapses in English of the sort to be expected of a non-native speaker. I'm rooting for him. I hope someone more expert than I will peruse his work and give a more detailed and expert opinion.

2. **Walt**
   July 2, 2008

   I'm sure that he's no crank, but as the field stands, using ideles and adeles and the Mellin transform is pretty standard stuff in the area. The proof looks awfully elementary to me. (I'm not an analytic number theorist either.) It would be great that if a famous open problem like the Riemann hypothesis had a proof sufficiently elementary that I could understand it, but I would be pretty surprised.

3. **Chris Hillman**
   July 2, 2008

   Hi, Walt!

   JA, love your blog!

   And most urgent: ditto Chip. I had the same initial thought as several others when I realized that Li is a former Ph.D. student of Louis de Branges, but after looking over Li's papers– which have been published and cited— it seems to me that they appear to fit into a community effort involving some of the best mathematicians of our time, most notably Bombieri and Lagarias (who seem to have generalized a RH criterion due to Li). While I don't know much about this area, I recognize (or think I do) some ingredients which leading mathematicians have said might play a role in a proof of RH. A genuine proof of a long open conjecture which uses familiar ingredients in ways which had somehow been previously overlooked would be VERY surprising (de Branges's proof of Bieberbach conjecture is said to fit that bill), but not unprecedented.

   So I think this might actually be the real deal! Wow, if so!

4. **Chris Hillman**
July 2, 2008

Er.. sorry, Peter, thought I was posting in John Armstrong's blog. (Embarrassed grin.) But you have a great blog too!

5. **Walt**
July 2, 2008

My impression is that de Branges' proof of the Bieberbach Conjecture was very technical, but that later someone found a more elementary (and shorter) proof.

6. **Ian Agol**
July 2, 2008

Terry Tao thinks it's incorrect:
http://terrytao.wordpress.com/2008/02/07/structure-and-randomness-in-the-prime-numbers/#comment-30714

7. **Peter Woit**
July 2, 2008

Thanks Ian, I've added that to the posting.

8. **Abhishek Saha**
July 3, 2008

Xian-Jin Li has posted a new version (actually two new versions!) of his preprint on the Arxiv. Most pertinently, the definition of the function h on page 20 has changed; so perhaps this addresses Tao's objection on his blog.
The reason I will be very surprised if this proof turns out to be correct is that it involves mostly functional analysis on the adeles. It has been generally believed that such techniques are not sufficient to prove Riemann. It would be a stunning achievement indeed if Riemann is solved using only such elementary tools!

9. **Eric Chopin**
July 3, 2008

Hi All,

Apparently the half of the paper (at least) is "copied"
from Alain Conne's paper cited in reference, especially the
part metionned by Terence Tao. So I guess Connes would
be best placed to evaluate this paper. For instance the definition
on which Tao focuses on corresponds to eq. 18 p42 in Connes'
paper.

Also, it's really a detail, but I was really surprised that in this paper,
which seems to be quite a serious work (especially compared
to previous papers published in the last months…), the reference
to Weil famous paper (ref 15) serves only to reference the
definition of the Schwartz-Bruhat space….. Even Connes in his
paper references Bruhat paper, which is much more relevant!
Weil paper has probably much more to do with RH, but certainly
not with the definition of the Schwartz-Bruhat space….

Eric

10. **ninguem**
July 3, 2008

Connes has commented on his blog
http://noncommutativegeometry.blogspot.com/2008/06/fun-day-two.html?showComment=1215071400000#comment-c8876982000013974667

11. **Peter Woit**
July 3, 2008

ninguem,

Thanks, I added the information about Connes's comment to the posting.

12. **Abhishek Saha**
July 3, 2008

And now we have version 4!

13. **Daniel de França MTd2**
July 3, 2008

Indeed. I checked the places where Connes and Tao pointed out, and that's where he made the chages.

P.20, the definition of the formula 6.9 (Tao)
p.29, the first paragraph of the theorem 7.3 (Connes)

I didn't look anywhere else.

14. **Chris Hillman**
July 4, 2008

After reading the comments by Tao and Connes, my enthusiasm has considerably diminished :-/

15. **Eric Chopin**
July 4, 2008

Actually I had first a similar doubt as Tao had, but
comparing a bit more closely Li and Connes papers,
I am now more confident in this part of the paper: actually
this part is almost copied and pasted from Connes paper,
and if it were wrong, it may also possibly be wrong
in Connes paper (assuming I have not missed a difference
between the two papers).
However the point raised by Connes is really a major objection
to Li' results (as for me)…

eric

16. **Benjamin**
July 4, 2008

Terence Tao made a further comment on problems with the proof. He also explaines in more detail what Connes pointed out.
http://terrytao.wordpress.com/2008/02/07/structure-and-randomness-in-the-prime-numbers/#comment-30714

17. **Moeen**
July 4, 2008

Benjamin,

That was Tao's earlier comment about the paper. Perhaps you're thinking of this comment:
http://terrytao.wordpress.com/2008/02/07/structure-and-randomness-in-the-prime-numbers/#comment-30763

18. **kathrine martinez**
July 5, 2008

It will be interesting to know the opinion of Xian-Ji Li himself.
Has Li something to say to his "critics" ?
It will be very important to know also the point of view of
Prof. Xian-Ji Li.
Can anybody tell us anything on this?

19. **math idiot**
July 5, 2008

Li hasn't withdrawn his paper. He is very confident in it?

20. **Felipe Zaldivar**
July 5, 2008

Regarding Connes, Kowalski, Silberman and Tao comments on the quotient space A/k^*, I recall a comment by André Weil that could be translated as "The search for an interpretation of [the idele class group] seems to me to be one of the most fundamental problems in number theory today; it is possible that one such interpretation holds the key to the Riemann hypothesis" (Sur la théorie du corps de classes).

21. **math idiot**
July 5, 2008

Just found that Li had withdrawn his paper.

22. **anon.**
July 6, 2008

I'm glad that such pure mathematical research is discussed here. Seeing how proofs are checked and commented upon, and withdrawn if an error is found, makes it very interesting. It's clear that there is a lot of discipline involved in step-by-step proofs of theorems in pure mathematics because it is possible to make an error which breaks the chain of reasoning.

Applied mathematics is far less risky, because where a model is being constructed a logical error can usually be corrected without the whole model collapsing. I think this the distinction between the kind of model building mathematics being done in string theory, and the formal proofs of pure mathematics.

23. **Christine**
July 13, 2008

Interesting to read about this case. Fortunately, I see no backfire as in the case of, as I remind, Penny Smith on the Navier-Stokes Equation problem.

It is also very interesting that the error was pointed out in the blogosphere…

Interview With Atle Selberg

July 4, 2008
Categories: Uncategorized

Sticking with the theme of the Riemann Hypothesis, the AMS has recently posted some articles to appear in an upcoming issue of the AMS Bulletin, one of which contains a long interview with Atle Selberg, who died last summer at the age of 90. Selberg had been a professor at the IAS and an expert in analytic number theory, responsible for some of the most important developments in the subject during the 20th century. A large part of the interview concerns in one way or another the Riemann Hypothesis, which is a central concern of Selberg's mathematical research, with his work on it beginning during the German occupation of Norway when he was still a student, Some thoughts from Selberg on the subject:

> "If anything at all in our universe is correct, it has to be the Riemann Hypothesis, if for no other reasons, so for purely esthetical reasons." He always emphasized the importance of simplicity in mathematics and that "the simple ideas are the ones that will survive."

About whether there is a spectral problem that gives the zeros of the zeta-function, useful for proving the RH:

> That is certainly a thought that several people have had. In fact, there have been some people that have been able to construct such a space, if they assume that the Riemann hypothesis is correct, and where they can define an operator that is relevant. Well and good, but it gives us basically nothing, of course. It does not help much if one has to postulate the results beforehand—there is not much worth in that.

About his own attempts to find a proof:

> Once I had an idea that I thought perhaps could lead to a proof….
>
> After a while I became more and more convinced that it would not work as I had thought initially. It just seemed unlikely to me. However, I have now and then seen that people have attacked a problem in a way that seemed "hare-brained", to use an English term, but then it turned out that they could make it work. They have proven something that would not be easy to prove in another way. On the other hand, I have seen people have ideas that seemed absolutely brilliant, but the only problem is that if one follows these to the end one is not able to get anything out of it after all. So it works both ways: sometimes a good idea does not work, and what seems like a bad, even idiotic idea, may actually work.

About Connes's work on the RH:

> Yes, that is a new way to arrive at the explicit formulas—a new access, so to say—but it basically does not give more than what one already had. Connes undoubtedly believed to begin with that what he was doing should lead towards a proof, but it turned out that it does not lead further than other attempts. When I last talked with him he had realized this. This often happens with types of work that are rather formal. There was, for example, a Japanese mathematician, Matsumoto, who gave several lectures that made quite a few people believe that he had the proof.

and finally:

> I think it is a good possibility that it will take a long time before it is decided. From time to time people have been optimistic. Hilbert, when he presented his problems in 1900, thought that the Riemann hypothesis was one of the problems that one would see the solution of before too long a time had elapsed. Today it is a little more than one hundred years since he gave his famous lecture on these problems. So one must say that his opinion was wrong. Many of the problems that he considered to be more difficult turned out to be considerably simpler to solve.

Comments

1. **ninguem**
   July 4, 2008

   What was surprising to me in that interview is his negative view of collaboration. These days if somebody gives a non-trivial contribution, they get their name in the paper. I found Selberg's attitude (re Erdos) bordering on the unethical, but those were different times and maybe different standards.

2. **theoreticalminimum**
   July 5, 2008

   It appears that the IHES is currently working on editing and publishing Grothendieck's "Récoltes et semailles". It is mentioned that it would be out before the end of this summer. Something sure to please at least Alain Connes 😉

3. **geometer**
   July 5, 2008

   ninguem:

   Clearly Selberg did not like to collaborate but I think the way Selberg and Erdos resolved this issue was quite ethical: they published separate papers in which they clearly described who contributed what. BTW, in MathSciNet the papers are reviewed in a single 4-page-article (by Ingham).

4. **oldman**
   July 6, 2008

   I found it refreshing to hear a mathematician of evident quality express a distaste for collaboration. It was also pleasant where he said prizes don't advance science.

   The non-collaborating mathematician still exists. Taubes is a good example of this beast that pursues its own interests in privacy and seriousness.

   Perhaps I know too many folks too busy inflating their publication counts through incessant collaborations in which their principal contribution appears to be their name.

5. **George**
   July 7, 2008

   Another similar annoying thing is how it's now expected that young mathematicians collaborate with senior mathematicians, whether they want to or not. You're at such a disadvantage in the job market if you do it alone, because even if you're successful, your peers will have the advantages of working with senior well-known mathematicians, not just in terms of the quality of the papers but also the top journals are more hesitant to accept papers from unknowns. I've seen utterly incompetent people get tenure-track positions through these "joint efforts." One time at a talk I attempted to get the speaker to define the basic objects of his talk. He couldn't do it, guessed incorrectly, then emailed me the next day with the actual definition. To preserve anonymity I won't say what the terms in question were, but it would be like if an algebraic topologist couldn't define cohomology. This person had multiple joint papers with multiple leaders, all in good journals. I was amazed that this could happen when the incompetence was so blatant. I have seen two talks from this guy, so it was not a fluke.

6. **ninguem**
   July 8, 2008

   geometer:
   One of the things that I meant was the fact that he tried to throw Erdos offtrack and he seemed almost proud of how clever he was.

   George:
   What you describe is a bit surprising. Why would the senior mathematicians bother to write papers with this guy? Maybe he has something to offer.

7. **olderman**
   July 10, 2008

   ninguem,

   The answer to your question to George is simple: this guy physically writes the papers the senior mathematicians do not feel like taking the time to physically write. This is the sort of thing he `has to offer'.

LHC Update

July 7, 2008
Categories: Experimental HEP News

Latest press release from CERN about the LHC says first beams "currently scheduled for August". According to a presentation at the July 2 meeting of the LHC Technical Committee, the latest news is that "circulating beam not before September" (the presentation includes a detailed version of the schedule of what has to take place between now and the end of August). At this point the second to last sector is just about cool, the final one will take another two weeks. The last of 470 trucks of liquid nitrogen has arrived. Assuming it will take 1-2 months from first circulating beam until physics collisions, it looks like time for data-taking will be rather short before the shutdown for the winter.

Comments

1. **A quantum diaries survivor**
   July 7, 2008

   Hi Peter,

   indeed, I think we will be lucky if we get 20 inverse picobarns of data. And, at 10 TeV. The difference between 10 and 14 TeV is significant for the discovery of high-mass bodies.

   However, let me keep the enthusiasm up -finding for once my well-concealed vein of optimism. If we look back at the Tevatron, it took data in 1987-88 with no silicon detector in CDF, and triggered 4/pb of data. Those data were amazingly interesting! They kept us busy for the following four years! Here is a sample of publications from that period:

   – Measurement of mass and width of the Z (still competitive back then)
   – Measurement of W asymmetry (a first)
   – Inclusive jet cross section (ranging 9 orders of magnitude)
   – (then best) measurement of the W boson mass
   – inclusive J/psi, Psi(2S) production (showing disagreements with theory of up to two orders of magnitude!)
   – lower limits on top quark mass (up to 91 GeV)

   A total of more than 50 papers thick-rich with new amazing stuff.

   Plus, let's not forget, the very first top-antitop candidate was seen back then -and it created in fact a huge controversy (ask Tony for the details).

   So, are we going to be disappointed with the amount of data ? Yes. Are we going to be amazed by it ? YES!

   Cheers,
   T.

Back to the Future

July 7, 2008
Categories: Uncategorized

Several things have come up recently that brought up the year 1985, the year the film "Back to the Future" came out.

This summer the IAS will be running a two-week program at the IAS on Strings and Phenomenology, designed to train a new generation of graduate students and postdocs in the details of compactification methods which mostly go back to 1985, quite possibly before some of the attendees were even born. No evidence that there will be any mention of the fact that 23 years of work on these topics has led simply to a dead-end: the landscape.

At SUSYO8, the first two speakers harkened back to the 1985 period, with Hans Peter Nilles (who also will be lecturing at Princeton) quoting his own words from 1984 (Physics Reports 110):

> Experiments within the next five to ten years will enable us to decide whether supersymmetry at the weak interaction scale is a myth or reality

He notes that "This statement is still true today!"

Andrei Linde in his talk on cosmology crows about what he sees as Witten's recent capitulation to the anthropic landscape point of view about string theory that Linde was pushing back around 1985 (actually 1986) when he wrote:

> An enormously large number of possible types of compactification which exist e.g. in the theories of superstrings should be considered *not as a difficulty but as a virtue* of these theories, since it increases the probability of mini-universes in which life of our type may appear.

which he compares to this from the New York Times

> Now, Dr. Witten allowed, dark energy might have transformed this fecundity from *a vice into a virtue*, a way to generate universes where you can find any cosmological constant you want. We just live in one where life is possible, just as fish only live in water.

At the same time, I've been reading and thinking about some papers written back in 1985 which deal with the mathematics of gauge theory and anomalies. At least some of these were never published, including one that I've seen references to (by Igor Frenkel and Iz Singer), but never a copy of (does anyone have a copy?). Looking at the history of this subject, it is clear that some very good people were working on this until 1985, at which point quite a few of them dropped it to take up the new fashion of string theory.

Perhaps the LHC will revive the subject of particle theory, by producing a wormhole that will take the world back to its other end, opened up in 1985 by a DeLorean in the movie, from there setting us off into a more promising part of the multiverse.

Comments

1. **roland**
   July 7, 2008

   >At the same time, I've been reading and thinking about some papers written back in 1985 which the mathematics of gauge theory and anomalies.

   Is this a valid english sentence?

2. **Peter Woit**
   July 7, 2008

   roland,

   No, it's not, but now I've fixed it. My proof-reading is not all it should be. Thanks for helping.

3. **MathPhys**
   July 7, 2008

   It is sad to see people rehashing what they were saying in 1985. Compactification is too simple minded. I'm surprised that supersmart people like Witten are still into that.

4. **Tony Smith**
   July 7, 2008

   Peter, you refer to "… the mathematics of gauge theory and anomalies. At least some of these were never published, including one that I've seen references to (by Igor Frenkel and Iz Singer) …".

   Is that work related to what Alex Jay Feingold describes
   on his CV page at SUNY Binghamton?
   There, he says:
   "… During the period from 1981 to 1991 I had several collaborations with Igor Frenkel (Yale University) … with J. F. X. Ries … Our main objectives were to obtain independent vertex and spinor constructions of chiral algebras,
   the isomorphism between the two viewpoints,
   known as a "boson-fermion correspondence"',
   and constructions of the exceptional affine algebra E8(1) based on D4(1) spinor constructions and the principle of triality. …
   Some of these results were announced at the 1988 Conference on Lie Algebras and Related Topics, Madison, Wisconsin.
   Those results which only involve the spinor constructions are in our Contemporary Mathematics monograph …
   A sequel (with Ries only) was planned to give the vertex picture and the boson-fermion correspondence,
   but the untimely death of Ries has substantially delayed the completion of that project …".

   Do you know of any available publication in detail of that Feingold-Ries work involving "the vertex picture and the boson-fermion correspondence"?

   It seems to me that such a Feingold-Ries project might be related to, and improve on, the E8 model of Garrett Lisi,
   and

be a way that "respectable" math/physics people could work on such stuff.

Tony Smith

5. **Peter Woit**
July 7, 2008

mathphys,

I don't know what Witten will be talking about, but the strange thing about this whole subject is that the circa 1985 ideas about compactification are still in some sense the most aesthetically convincing ones. Since then, attempts to do better have pretty much all led to much more complicated and ugly constructions. So, I can see why someone asked to talk about this subject might want to talk about the 1985 version. Some of the other lecturers will be talking about more these more recent ideas. and trying to lead the students into the landscape…

6. **Peter Woit**
July 7, 2008

Tony,

That's a different subject. I don't know anything about the unfinished Feingold-Ries work, but it sounds like it's closely related to the Feingold-Frenkel work on spinor constructions of vertex algebras, which has been published (although I don't know much about that literature).

The paper I have in mind is about the Hamiltonian approach to the gauge anomaly in 3+1 dimensions.

7. **Frank Quednau**
July 8, 2008

Has it been that long? Incredible. However, I quite like this timeline, thanks. I do not want some Biff Tannen-style physics 🙂

8. **Chris Oakley**
July 8, 2008

> "I do not want some Biff Tannen-style physics"

I see this more as a "Terminator" scenario than a "Back to the Future" one. The dominance of String Theory was intended by our time-travelling descendants as it has prevented discoveries in fundamental physics (you know – real physics: remember that?) that would have been dangerous to society.

We will know this for sure if Peter suddenly finds himself pursued by a Killer Robot from The Future (or maybe this is already happening)

9. **Markk**
July 8, 2008

"Experiments within the next five to ten years will enable us to decide whether supersymmetry at the weak interaction scale is a myth or reality
He notes that "This statement is still true today!""

This is very true and in a sense is a specific effect of the cancellation of the Superconducting Super-Collider. You could say that it cost a generation of physics advancement in particle physics. We would have had answers to a lot of things and a lot more data to eliminate ideas with.

We are becoming like the mid 80's again in that sense: We again (an for real this time we hope) are just before looking at new information about a new level of energy in particle collisions.

10. **Peter Woit**
July 8, 2008

Markk,

I agree that the SSC cancellation has set things back a generation, but this isn't relevant to the Nilles quote. He made it back in 1984, and was explicitly claiming that SSYM would be seen by the time you got to a 100 Gev scale, i.e. he was talking about the Tevatron, not the SSC. The SSC project was not even approved until 1987 (canceled in late 1993), so couldn't have been what Nilles had in mind when he wrote that SSYM would be seen in "5-10" years from 1984. Even under the most optimistic assumptions, there was no way that in 1984 one would think that the SSC would be built and operating in 1989-1994.

It isn't hard to find quotes from the 80s about how SSYM was definitely going to show up at the Tevatron, it is only in the 90s as it became clear this wasn't happening that the assumed scale of SSYM got moved up from 100 Gev to the "Terascale".

11. **misslemon**
July 8, 2008

"…quoting his own words from 1984 (Physics Reports 110):

Experiments within the next five to ten years will enable us to decide whether supersymmetry at the weak interaction scale is a myth or reality

He notes that "This statement is still true today!""

A good example of someone whose timeline/logic/truth interfaces need adjusting

12. **wilbur**
July 8, 2008

> but this isn't relevant to the Nilles quote.

My interpretation of that quote was quite similar to Markk's, actually. I think a lot of people would tend to read the quote that way. The fact that the original text was referring to the TeVatron and not the SSC completely changes the context. In fact, it makes this post much more interesting.

> It isn't hard to find quotes from the 80s

I think quoting one or two more of those would do a lot to strengthen your point.

13. **Peter Woit**
July 8, 2008

wilbur,

OK, here's one more:

Haber and Kane, Physics Reports 117 (written in 1984)

pg. 82

"While there is no compelling supersymmetric model, all the ones studied produce some detectable superpatrners that are light, often with masses well below m_W. More technically, the models can be tuned to have a larger scale of supersymmetry breaking and still account for m_W at tree level, with no partners below m_W,but then they are unstable and have to be retuned as soon as radiative corrections are included. Models where such radiative corrections are less than the masses themselves have detectable, light superpartners."

pg. 89

"Finally, we note that it is not a problem for supersymmetric ideas that superpartners have not yet been found. As we will discuss in chapter 9, most of them are expected to have masses of order m_W,and could not have been detected yet. Those that could have been observed, such as the gluino, are allowed to be heavy enough to have been unobservable so far. But as we will see, the machines available in the next decade will have a high probability of detecting supersymmetric partners if supersymmetry is relevant to understanding the weak scale."

"Machines available in the next decade" clearly refers to the Tevatron, LEP and HERA, this is also clear from the argument about m_W.

14. **wilbur**
July 8, 2008

thanks for the quotes. Fascinating stuff, looks like a time tunnel. In summary, if the LHC doesn't find susy, the HHC probably will.

15. **MathPhys**
July 9, 2008

wilbur,

It's very, very easy to find quotes from the 80's of the type that you want to see. Peter Woit gave the simplest examples, but trust me, the literature is full of them. I only want to see/hear/read anyone of these guys own up and say "In about 50 papers that I wrote in the 80's and 90's, I made predictions that now I know were simply totally wrong. Now let me start all over again". But no, they always carry on as if they were always on the right track and things are progressing linearly. And they call themselves scientists.

16. **Jason Starr**
July 10, 2008

In my department there are some great "LOLCat"-style posters with an image of a black hole and the caption "Im in ur LHC, eatin ur universe". The posters announce a light-hearted debate of the graduate students in the Physics and Astronomy department on July 11th with the following premise: if a relativistic heavy ion experiment at the particle physicists' accelerator produces black holes, which group of physicists should be liable for the damage 🙂

17. **hidalgo**
July 11, 2008

Actually I remember the times of which you speak very well. We all got very excited about the fact that gauge fields were connections on principal fibre bundles. Just when it was dawning on us all that this line of research was leading absolutely nowhere, Witten came along and saved the day. I can assure you that the times for which you hanker were even worse than now, and that "not even wrong" applies far more convincingly to the things you favour than to string theory.

18. **Peter Woit**
July 11, 2008

hidalgo,

Funny, I remember those time quite well myself. The mood among particle theorists was rather different than it is now, I wouldn't exactly describe it as "even worse" though….

19. **somebody**
July 12, 2008

>It's very, very easy to find quotes from the 80's of the type that
> you want to see. Peter Woit gave the simplest examples, but
> trust me, the literature is full of them. I only want to
> see/hear/read anyone of these guys own up and say "In about
> 50 papers that I wrote in the 80's and 90's, I made predictions
> that now I know were simply totally wrong.

To me the sarcastic way in which Nilles was referring to his own quote is precisely that. I completely fail to see in what sense Nilles is supposed to "apologize". Wrong ideas are merely left by the wayside in science and not held to court, and thats what happened to the guess about the exact energy scale of new physics (actually the general prediction for the energy scales, that it is around TeV, might still be right).

People were hoping back then that something would be seen close to TeV, and that hope is still there. The EXACT energy scale is what the bets were made about (and lost). In fact, I am certain that many people on this thread know this, and are actively ignoring it because it is conveneinet for their agenda.

Nilles was certainly "wrong" in that sense. But contrary to your subtle implication, nobody (including him) claims today that they were right back then in that narrow sense. Of course, many of them still believe that TeV scale susy is a possibility, but thats based on scientific judgement, and because we don't seem to have any better ideas.

In any event, being wrong is hardly an isolated event in physics. It comes with the territory of "trying". The "hindsight is 20-20" type talk has only one purpose and that purpose has nothing to do with science. Incidentally, let me just point out one obvious thing because the discussion here has taken a condescending air: it is clearly an easier task to sneer at people who try, than to try oneself.

> Now let me start all over again". But no, they always carry on as
> if they were always on the right track and things are
> progressing linearly. And they call themselves scientists.

Thats only because we (not "they") still don't have any reason to believe that we were qualitatively wrong back then. The hope is still that around TeV is where the action is – if anybody has any better ideas they would of course be welcome, but the fact of the matter is, as it stands we don't. In fact, we could still be wrong, and ten years later some others like you might mock us. But that possibility is totally useless in deciding how we do science now.

The ONLY thing that can save us from mediocre ideas is a great idea: not personal attacks and uncreative criticism.

20. **Peter Woit**
July 12, 2008

Somebody,

No, people in 1984 were not expecting to see SSYM breaking at 1 TeV, they were expecting to see it at 100 Gev. The Haber/Kane quote that I reproduced explains precisely why (you have to do some fine-tuning to avoid superpartners below m_W).

The fact that SSYM was expected to show up, didn't, and now requires fine-tuning to explain why it hasn't been seen is something that its proponents don't ever mention. Some readers here didn't seem to originally believe me that this is the case.

Let me point out that I am not engaging in personal attacks on Nilles or anyone else. I do think though that this particular bit of history, failed prediction of SSYM, and required fine-tuning is something that deserves to be better known to anybody who wants a clear idea of the prospects for seeing SSYM at the LHC.

21. **Eric**
July 12, 2008

Peter,
The MSSM only requires that the superpartners be in the 1 TeV range in order to solve the hierarchy problem. This requires no fine-tuning. It is only now with the LHC that the full parameter space can be explored. Back in the eighties, they were only beginning to probe the lowest energies of where one might expect to find the superpartners. So, some of them were overly optimistic. So what? This was also true in regards to the top quark.

22. **somebody**
July 13, 2008

SUSY always had problems – it did then, and it does now. But the balance of evils seemed somewhat in its favor back then, and it still does.

The books and reviews on phenomenological susy invariably leave you with the impression that there are plenty of things that are NOT pretty about it. Every fine tuning that you can possibly talk about, from higgs mass to susy flavor problem are all discussed in the textbooks. So I totally fail to see in what sense you are suggesting that the fine tunings are not well-known. The scientific facts are entirely well-known. The only thing missing from the textbooks is the personal attacks.

About your claim that we were expecting to see susy at 100 GeV: you are ignoring what I emphasized in my previous post, that the PRECISE scale was where the "predictions" went wrong. If you want to split hair at the 10% level, there are actually even worse problems. Eg: the mu-problem which requires not 10% level fine tunings, but 1% level fine-tunings. The existence of fine-tunings is a fact about susy, but the fact still remains that it is still pretty much the best idea we have. So any attacks of this kind about our past follies (without any suggestion for a better idea) accpomplish absolutely nothing.

I am actually charmed by the disarming honesty with Nilles pointed out one of his own old "wrong" predictions. It would be nice if we had radically new predictions now, but unfortunately we don't.

23. **Haelfix**
July 17, 2008

Supersymmetry only requires finetuning (of the Stop) if various eventualities occur.

1) The mass of the Higgs cannot be too heavy. Its best if its right around 115-120, at least for the MSSM

2) Refinements in the top quark mass have moved around in the previous decades, which also affects things.

How much finetuning you are willing to accept is a bit of an aesthetic requirement. We've seen finetuning in nuclear physics up to about 2 orders of magnitude before, so it shouldn't surprise people if there is a little bit. Its when you start talking about 16++ orders where people start really wondering if we've gone insane.

Its true that the minimal 'bayesian' models from the 80s of phenomenology would have arranged the masses a little bit differently. We should have seen a lighter Higgs, a different top mass, and some SuSY already on those grounds, but nature doesn't always pick the simplest solution either (see the expanding universe).

After the LHC, if we still haven't seen SuSy, the original premise is gone and it might juts become something quantum gravity people are interested in, and not day to day phenomenologists. Still i'd say theres still a good percent chance that we will see weak scale SuSy (I'd give it a good oh.. 1 in 3 chance, which is much better than any alternative).

24. **invcit**
August 2, 2008

Peter,

Witten talked about the work of Vafa et al on how to embed GUTs into F-theory. Not quite the stuff of 25 years ago. There is a lot more hope of uniqueness in this picture than in the flux compactifications.

25. **anon.**
August 3, 2008

invcit: what is ‘F-theory’?

(Sorry Peter if this question sounds off-topic, hostile, and adding noise, but I genuinely don’t know what F-theory is!)

26. **Arun**
    August 3, 2008

    Wikipedia has an answer:
    http://en.wikipedia.org/wiki/F-theory

    “F-theory is a branch of string theory developed by Cumrun Vafa. The new vacua described as F-theory were discovered by Vafa, and it also allowed string theorists to construct new realistic vacua — in the form of F-theory compactified on elliptically fibered Calabi-Yau four-folds. The letter “F” stands for “Father” much like the “M” in M-theory is often taken to stand for “Mother”.”

27. **anon.**
    August 3, 2008

    Thanks, Arun. I had seen a post on this blog some time back about the experimental predictions of F-theory:

    http://www.math.columbia.edu/~woit/wordpress/?p=697

    but I didn’t understand F-theory. There is a nice paper on ‘Evidence for F-theory’ by Vafa on arxiv: http://arxiv.org/abs/hep-th/9602022 The math isn’t really hard.

This and That

July 8, 2008
Categories: Uncategorized

Some quick links:

The Clay Mathematics Institute is now making available for free online the books whose publication it has sponsored. These include the Morgan-Tian exposition of the proof of the Poincare conjecture. Surveys in Non-commutative geometry, which contains two excellent articles by Jeffrey Lagarias and Paula Tretkoff explaining ideas about the Riemann Hypothesis that have been motivating Connes and others recently. Also the excellent huge group-effort expository volume on Mirror Symmetry, and a more recent volume on the topic, which includes a good review article by Michael Douglas about the string theory motivations for this work.

There's an interview with Susskind here about his latest book. About anthropics and the multiverse he claims

> …since I wrote "The Cosmic Landscape," it has practically become the conventional view.

A couple of relatively new physics bloggers are Sunil Mukhi and Marco Frasca. P.P. Cook has revived his blog and is reporting from Eurostrings 2008 here and here.

Among the posts worth reading over at Secret Blogging Seminar, there's a nice posting by A. J. Tolland explaining what a "stack" is. The comments contain a valuable discussion about the different versions of a "classifying space" that show up in this story.

Comments

1. **anon.**
   July 8, 2008

   "There are powerful reasons to believe that the universe may also be a consequence of random mutation. It sounds crackpot, or at best, like fringe speculation, but by now the idea is very firmly established in the mainstream physics and cosmology literature. That's was what my book "The Cosmic Landscape" was all about. … The next generation of physicists and cosmologists will have the fun and excitement of discovering the right mathematical formulation of a "multiverse."' – Lenny Susskind, http://calitreview.com/790

   I love this suggestion that analyzing the landscape of 10^500 metastable vacua in the multiverse will be an exciting and fun challenge for the next generation. The next generation should be eternally grateful. 😉

2. **A.J.**
   July 10, 2008

   Hi Peter,

   Thanks for the post rec!

   Is Marco Fresca really arguing that "lattice results about propagators seems to indicate that Yang-Mills theory is trivial"? That's an amazing claim.

3. **Peter Woit**
   July 10, 2008

   A.J.,

   I confess that I don't understand this claim of his, but haven't had a chance to look carefully at what he is up to. As far as I can tell, he's looking at the gluon propagator, which is not gauge invariant, so the significance is unclear.

4. **theoreticalminimum**
   July 11, 2008

   I might, if I may, bring to your attention that the *Institut des Hautes Études Scientifiques* (IHÉS) in France is celebrating its 50th anniversary this year, with events taking place since March 27 (to coincide with Grothendieck's 80th birthday, which was on March 28). Apart from IHÉS' disposition to publish Grothendieck's *Récoltes et Semailles*, one other nice collectible is the photo-book (published in France by *Éditions Belin*) *Les Déchiffreurs: Voyage en mathématiques* ("The Unravelers"), which "comprises photographs and texts, including more than 200 photographs of some of the 40 or so researchers who work or at or have visited IH in the course of their career'. A small sample of the photos can be seen here. I'll get a copy of it next month when I visit Paris.

5. **Richard**
   July 12, 2008

   theoreticalminimum,

   Thanks for alerting us to this photo-book! I looked at the sample pictures, and was pleasantly surprised to see that they are in black and white, which is now practically a lost art. In photos of people, black and white photos often capture "the moment" and the personality in a way that color photos do not.

6. **Chip Neville**
   July 13, 2008

   Peter,

   It would be wonderful if Harvard could see it's way clear to put Tate's 1950 thesis on the web. The same is true for Uppsala and Nyman's 1950 thesis.

   Tate's 1950 Harvard thesis, which from the fragments I have seen quoted, for instance in Li's failed preprint, seems to be simpler and clearer on some points than parts of most accounts of adeles, even the excellent one by Paula Tretkoff on page 150 of her article.

   Nyman's 1950 Uppsala thesis, which is important in completeness approaches to the RH, is often quoted. Nyman was a student of Beurling, and his thesis is what led to Beurling's 1955 Proc. Nat. Acad. Sci. article referred to at the beginning of Sarnak's appendix to Paula Tretkoff's article.

7. **Jason Starr**
   July 13, 2008

   Chip,

   I believe Tate's thesis is in the volume edited by Cassels and Froehlich.

   MR0217026 (36 #121)
   Tate, J. T.
   Fourier analysis in number fields, and Hecke's zeta-functions. 1967 Algebraic Number Theory (Proc. Instructional Conf., Brighton, 1965) pp. 305–347 Thompson, Washington, D.C.
   10.41

8. **Marco Frasca**
   July 14, 2008

   Hi Peter,

   Thanks a lot for the citation. About Yang-Mills theory being trivial you should not check the gluon propagator but rather the running coupling. Current definitions analyzed on the lattice show clearly that no non-trivial infrared fixed point exists as people used to think. Recent phenomenological analysis due to Prosperi's group in Milano support this (e.g. see Phys. Rev. Lett. 99, 242001 (2007) or http://arxiv.org/abs/0705.0329 and related papers on arxiv all published on archival journals). The point is how to get a proper definition of running coupling in the infrared but it does seem that whatever is your choice this goes to zero at lower momenta. For a discussion see again Prosperi et al. http://arxiv.org/abs/hep-ph/0607209.

   Marco

9. **Sebastian Thaler**
   July 14, 2008

   Peter,

   There is an article about Garrett Lisi in the July 21st issue of The New Yorker, unfortunately not online.

10. **Peter Woit**
    July 14, 2008

    Sebastian,

    Thanks. I talked to the writer of that piece a couple months ago, and to a fact-checker at the New Yorker, was wondering if they were going to publish it. Will look forward to reading it tonight.

Surfing the Universe

July 14, 2008
Categories: Uncategorized

This week's New Yorker has a quite good article by Benjamin Wallace-Wells entitled "Surfing the Universe" about Garrett Lisi and the controversy generated last year by his paper An Exceptionally Simple Theory of Everything (which I wrote about here). Unfortunately the article is not available on-line as far as I know.

One of the main themes of the piece is how Garrett ended up getting enmeshed in the controversy over string theory. I'm quoted as agreeing with the writer's impression that one thing that got Garrett "enlisted in the string wars" was having my name appear first in his acknowledgements:

> "It was probably not the most politic thing to do," Woit said.

The description of the state of the string theory controversy is pretty accurate. Wallace-Wells got the following from Steven Weinberg

> String theory still has great attractions, and there aren't any alternatives… Well, there are alternatives and they're worse.

and describes the situation as follows:

> In physics, as in politics, the competition is crueller in lean times. "In terms of development of new theories, it has been maybe the slowest period in two centuries," the science historian Spencer Weart said. By 2006, the fight over string theory had begun to leak out of the scientific community. Smolin and Woit published widely reviewed books criticizing string theory, and USA Today published an account of the assault headlined "HANGING ON BY A THREAD?"

The article explains the role of the arXiv and the blogs:

> In recent years, as science reporters and interested amateurs have turned to the arXiv – and as some physics personalities have started blogs – the audience for physics has both expanded and fragmented. "I know for a fact that many of the leading figures in the field read the blogs, but so do high-school science students," Woit, the Columbia mathematician and string-theory critic, said. "The scary thing is that frequently you can't tell which is which." The leading blogs have readerships that, while including some loud dissenters, tend to align with the perspectives of their authors – Distler, of the University of Texas, has a blog that attracts many string theorists and enthusiasts, while Woit's blog draws more skeptics. It works somewhat in the way the blogosphere operates in politics. Andreas Albrecht, a physics professor at the University of California at Davis, said that the blogs had opened physics to a new sort of populism, one that the academic establishment had to figure out how to manage. "It just pushes thoses buttons," Albrecht said. "There's some really good stuff, but a lot of really sloppy stuff." What you have, in other words, is the erosion of the referee and the rise of a scientific underclass.

The above quotes are from passages about the string theory controversy and ones where I had some involvement, but that's only one aspect of the piece. There's quite a lot more about Garrett, his story, and the physics/math context he is working in, together with a reasonable take on its significance, with everyone acknowledging that the ideas he is pursuing have problems and are still not such that success can be claimed. At the end of the article, Garrett explains that he's still at work, now trying to see if an alternate form of E8 will work better.

Update: Lubos has the usual sort of rant about this. He doesn't seem to have access to the article itself, so is basing his rant on my extracts. As a result, it includes an extensive personal attack on Spencer Weart….

Update: I hear that Bert Kostant has posted on his office door at MIT a copy of his e-mail exchange about E8 with the author of the New Yorker piece. So I guess that it's all right to point to these files. Also, there's an on-going Distler/Lisi exchange going on here. I haven't followed the technicalities of this particular discussion, but the trademark Distlerian argumentation tactics are in operation, ensuring vastly more heat than light.

Update: It turns out that not everyone involved in that e-mail exchange had given their permission to make it public, so the files linked to above have been removed. Kostant has made his comments public, posted here.

Update: The New Yorker article is now available on-line.

Comments

1. **Observer**
   July 14, 2008

   Peter,

   I am curious to hear your opinion on Albrecht's view that:

   "…the blogs had opened physics to a new sort of populism, one that the academic establishment had to figure out how to manage. "It just pushes thoses buttons," Albrecht said. "There's some really good stuff, but a lot of really sloppy stuff." What you have, in other words, is the erosion of the referee and the rise of a scientific underclass"

   Do you agree with this assessment ?

   Regards,

   Observer

2. **Peter Woit**
   July 14, 2008

   Observer,

   I agree with with Albrecht's characterization of physics blogs. I guess it's also true that the "physics establishment", whatever that might be, doesn't know what to make of blogs.

   Academia is a very hierarchical and structured system, and the blogs mess with that. Not clear what the effect will be. At this point I think it's hard to generalize, since the number of blogs is rather small and they're often very different in nature.

3. **Dave**

July 15, 2008

There was no mention of Lubos Motl. His blog is the best physics blog, period. Not surprising that his blog goes un-noticed…he's a conservative, and his opinion is certainly not required by the hysteria crowd!

4. **Bee**
July 15, 2008

Ah! I was beginning to think this article would just never appear. I am totally with Albrecht: the academic establishment has to figure out how to arrange with that populism. That's neither a trivial question, nor something that is likely to just work out by itself. I just wrote on the weekend a rather lengthy post about how crucial objective peer review is to progress in science (with that I don't only mean peer review in publishing, but generally critical feedback by the community). There's most importantly three factors that influence researcher's opinions: financial pressure, peer pressure and the public opinion. All of which are too influential today – the blogosphere touches on the two latter factors. See: We have only ourselves to judge on each other. The very least one can do is to raise awareness that such influence can hinder progress.

5. **Urs Schreiber**
July 15, 2008

It almost seems as if one statement by Albrecht amounts to

> [Blogs lead to] the erosion of the referee and the rise of a scientific underclass.

(I can't tell for sure, but this is the message I got from Peter's summary.)

I would strongly disagree with this statement. But maybe something else was meant.

6. **Peter Woit**
July 15, 2008

Dave,

Lubos was mentioned in the article, with a typical quote from him of the sort that makes it very hard to take him seriously. His political views aren't the main reason string theorists find him an embarassment…

Urs,

That statement was not in quotes, so I think more an interpretation by Wallace-Wells than something Albrecht said. Wallace-Wells is also a writer and reporter on politics, I think he was fascinated by the political dimensions of this story, here especially by the analogs to the role blogs have had in politics.

7. **Bee**
July 15, 2008

Hi Urs,
Technological developments are a priori neither good nor evil. It depends a lot on how one uses them. To this one needs to critically look at assetts and drawbacks. The drawbacks of blogs I think are mentioned in the quote from that article are a) blogs tend to polarize people into camps of pro and con and don't actually stimulate communication between these camps b) they make discussions about community matters public which isn't always a good thing because it can make an exchange significantly more complicated and hinder constructive criticism c) blogs aren't peer reviewed papers or textbooks, there's a lot of 'sloppy stuff' indeed and journalists as well as beginners need to be aware of that d) 'it just pushes those buttons': online communication isn't as easy as it looks like. There are many aspects to it that don't work in the way we are used to them from face-to-face discussions and it would help a lot if people were aware of that. One is eg that many writers seem to believe their message is evidently clear and perfectly obvious, whereas for somebody who just stumbles across a blog for the first time it is very hard to extract without knowing the writer, his story, or just the vocabulary used. Such kind of misunderstanding is easy to detect from frowned foreheads but not from blog comments that can easily heat up. (There are other complications to online communication but I'll stop here, I think you get the point.) Best,

B.

8. **Urs Schreiber**
July 15, 2008

Hi Bee,

just a few random comments on your remarks:

> blogs tend to polarize people into camps of pro and con and don't actually stimulate communication between these camps

Are you sure the polarization is caused by the blog? I certainly know that I had communication in blogs with lots of people (some of them in some camps some not) which I would never ever had otherwise, and I keep seeing some (little, though) cross-camp communication exclusively in blogs. It never happens elsewhere because these camps don't attend each other's conferences.

> blogs aren't peer reviewed papers or textbooks

Yes, because if they were, they'd be called peer reviewed papers or textbooks. Seriously: also discussion in the conference coffee break is "not peer-reviewed" in this sense and yet it tends to be the most useful discussion there is (and often even the most useful peer-review, actually!).

> journalists as well as beginners need to be aware of that

That's of course true. Journalists should even be aware of the fact that even peer-reviewed published journal articles may be contain nonsense. Generally, journalists should be very aware that the truth™ is hard to come by.

I guess we agree on the main point: mistaking blogs for peer-reviewed journals is a problem of the "media competence" of the one doing so, not of the communication channel "blog". Do we? 🙂

9. **George**
July 15, 2008

Serious question, Woit. I realize you may be offended, but I want to know what you have to say. It may be that what you have to say about string theory is right… but is the primary motivator for your intense desire to bring down string theory your own lack of success at becoming a physicist? Are you just trying to drag them down so you can think that you didn't fail, that instead the establishment was flawed? Or just to see the ones who did succeed fail too?

10. **Peter Woit**
July 15, 2008

George,

If you actually knew me personally, I think you'd find out that the idea that I'm an embittered failure striking back at my betters doesn't have much to do with reality.

Actually, as far as my career goes, I've done far better than I ever expected, I'm extremely pleased with how it has turned out, and consider myself to be someone who has been both extremely lucky in life, and extremely well treated by the powers-that-be in academia. Columbia has recently promoted me to the position of "Senior Lecturer", with a large pay increase. I like pretty much everything about my work, my colleagues, my department, and life in general. I'm a very happy camper. When I went into this business, I figured I'd end up working under very difficult circumstances at a community college (or maybe quit and try Garrett Lisi's route…).

My criticisms of string theory and what has been going on in physics have nothing at all to do with resentment about how I have been treated. Sorry, this is about science.

11. **George**
July 15, 2008

Ok, ok. One other (less serious) question of this general category. Have you ever met your arch-nemesis Lubos Motl in person? If so, how did it go? 🙂

12. **Daniel de França MTd2**
July 15, 2008

George,

Pay attention at the following pic, and try to deduce who took it.

http://en.wikipedia.org/wiki/Image:PeterWoit.JPG

13. **Peter Woit**
July 15, 2008

George,

Lubos and I have only met in person once, at Harvard at the conference in Sidney Coleman's honor. The picture of me on Wikipedia was taken by Lubos at the time. I think we both were smiling…

He and I over the years have had many often friendly exchanges by e-mail, since the time he was a graduate student. In recent years less so, as he has come to the opinion that I'm the anti-Christ, out to destroy physics.

14. **Aaron Bergman**
July 15, 2008

I haven't been able to read the article yet (the local BN was sold out because of the cover), but I'm not really sure I see that Lisi got caught up in the "string wars", except to the extent that most of the people involved in talking about hep-th on the internet have been unavoidably involved in such to some extent or the other. I don't recall anyone (with perhaps an obvious exception) attacking Lisi in a string theoretic context. Instead, the arguments were all about field theory and representation theory, and one of the more vocal participants was "amused" who's not a string theorist. If anyone brought string theory into the subject, it was Lisi who made the rejection of string theory part of his "origin story". What got people going about Lisi's paper was the extravagant title, a certain comment by someone in Canada, and, by far most importantly, the ridiculous media coverage. That you (PW) were cited in the acknowledgments isn't even a minor perturbation on that (again, except perhaps with the obvious exception.)

I'll see if I can find the article elsewhere, but it sucks being in a one bookstore town.

15. **Peter Woit**
July 15, 2008

Aaron,

The writer of the article also writes about politics, and I think got interested in the role of the blogs, and the "politics" of string theory, so that colors the story. He certainly noticed Lubos, quotes him about Lisi as follows:

"Every high school senior excited about physics should be able to see that the paper is just a long sequence of childish misunderstandings… I understood these things when I was 14"

There were quite a few blog comments posted by people attacking Lisi, often explicitly bringing string theory into it, e.g. see the first comment at Jacques's first posting on the topic, from "Moveon". True, these are pretty uniformly from people who don't put their names to what they write, hard to tell whether they're serious string theorists, juvenile string-theory partisans, or physicists with no stake one way or another as far as string theory goes.

It's an interesting intellectual exercise to think about what would have happened if Garrett hadn't brought string theory/LQG into it, or if he had embedded his E8 stuff into some kind of string theoretical context. If the latter had happened, and he had ended up getting a lot of attention because of his attractive "surfer-dude" story, I think what you would have seen would be Lubos and many others taking a very different and much more polite attitude, along the lines of "nice try, glad to see Lisi is working on this, even outside academia, but this particular idea doesn't work because of X, Y, Z, too bad."

16. **Bee**
July 15, 2008

Hi Urs,

As I said above, new technologies are a priori neither good nor evil. Blogs do not necessarily polarize people, I never said that must be the case, but it just often is the case. Many people just prefer to be among others who share similar interests. I certainly see that blogs have the potential to foster cross-camp communication, and I certainly wish that potential was used better. What I am saying is that this can also go into the wrong direction if one doesn't pay some attention to it. Social networks that are created online enable us to select our `neighbors' according to our liking much easier than was the case with actual neighbors.

Further, I never said what is written on blogs is useless because it is sometimes sloppy. I just said one has to be aware of that. I don't think this is an issue in the community, but for many non-experts what the expert (or the person thought to be an expert) says is taken very seriously. I therefore think writing a blog brings some responsibility on the one hand, on the other hand journalists as well as beginners should be aware that the level of accuracy is often not on a very high scientific standard. Again, this is a priori neither good nor bad, it is just a matter of how one deals with it. Best,

B.

17. **Haelfix**
July 15, 2008

The only problem with blogs atm in particle physics at least, is that there are only about 6-7 that are around and putting out serious physics content and being actively updated.

Meanwhile you have about 10,000 crackpot sites.

When a layman looks on the internet to find physics related discussions, he invariably gets a horribly jilted view of what academics really think and are doing.

18. **somebody**
July 15, 2008

OF COURSE blogs are not inherently good or bad. But the essential point is that blogs give a lot of visibility to opinions (on things which require expertise) from non-experts. They give the impression that democracy and public relations are more important than scientific judgement/truth.

Popular opinion is often reactionary and emotional. Emotional appeal has NOTHING to do with science, and that crucial point is often lost in the popularity contest.

Case in point: real or imagined "controversy" is more interesting to the public than the actual science that Urs blogs about, so the New Yorker does not even think about Urs as a science blogger. (I am assuming this – I haven't read the New yorker article, but I would be surprised if Urs got as much attention as Lubos or Peter. I would not be surprised at all if he was not even mentioned.)

19. **somebody**
July 15, 2008

Put another way, the more popular "science" blogs deal mostly with the sociology of science and not science itself. There is nothing wrong with this (I personally enjoy talking ABOUT science and scientists almost as much as I enjoy talking science), but the distinction is a very important one that is often blurred over in the discussions about "science" blogs.

20. **Peter Woit**
July 15, 2008

As Haelfix points out, there are only a relatively small number of blogs that are regularly updated with serious physics content. The interesting thing to me is that they're pretty much all quite different, reflecting different interests, points of view and goals of different bloggers. I think anyone who starts reading the things quickly realizes this, and also quickly gets a feel for who the people writing the blogs are, what their point of view is, how reliable their information is likely to be, etc. It's not really different than the situation with blogs in other subjects. People reading blogs know very well that they have to sift through them to find the ones that seem reliable. Stupid people will gravitate to stupid blogs, smarter people can be trusted to recognize smarter ones. The thing that is different here is that the blogs are operating outside of the usual credentialing system: anyone can start a blog, the things don't come with the sorts of institutional imprimatur that people are used to.

What the physics blogs don't necessarily do is reflect either the median point of view (the statistics are too low), or the point of view of leaders of the field (because they're not writing blogs, I wish they were). The one highly peculiar aspect of theoretical physics blogging is that we've had one Harvard faculty member doing it, and his blog continues to claim (with some justice) to reflect the point of view of leaders in the string theory commmunity. Unfortunately, he's kind of obviously out of his mind. If members of the mainstream string theory community feel that their point of view is not reflected by blogs, so that the general public is not getting their message, or is getting it from someone who makes them look bad, the solution is simple: start a blog. It's not hard, and, depending on how one does it, not necessarily highly time-consuming. I think it would be a wonderful thing to see one of the many sensible people I know working on string theory blogging about it, and putting out a reasoned view of what is going on in the subject.

21. **somebody**
July 15, 2008

"If members of the mainstream string theory community feel that their point of view is not reflected by blogs, so that the general public is not getting their message, or is getting it from someone who makes them look bad, the solution is simple: start a blog. It's not hard, and, depending on how one does it, not necessarily highly time-consuming."

I would in fact LOVE to see a blog on string theory which addresses things at a level like that of Lubos's but without the "Lubos-ness". But I am really sceptical about your optimistic expectation that it is NOT going to be a time-sink.

I also have one comment to make about intelligent people being drawn to intelligent blogs. I would in general agree with this, but I do think that the disagreement about (for example) string theory happens at such a rarefied and sophisticated level, that there is no way for an intelligent layman to judge who is talking sense, just based on the general "smartness" of the blogger. You, Carroll, Motl, Distler are all smart people. Is it going to be easy for the layman to distinguish whose scientific judgement is the most sound? I doubt it. This is where the emotional appeal of string theory being the "establishment" etc. come into play.

In fact, because I know some physics, I find Lubos's posts on scientific things reasonable even though he is psychotic otherwise. I suspect that this is in general going to be a tough call for an average left-of-center intelligent layman. The real problem is that even though he posts some good stuff, it is impossible for a layman to tell what is well-established and what is merely opinion because he states both with equal vehemence.

22. **A quantum diaries survivor**
July 15, 2008

Interesting discussion. I think that the concerns over the "level of accuracy" of scientific information offered in physics blogs are ill-posed. I often write inaccurate explanations, but their purpose is to bridge the gap with people that do not have the means to understand more precise descriptions. The inaccuracy has no harmful effect, on the contrary it helps!
One of the things I learned in over three years of blogging is that by getting down the pedestal is the way to go to get people interested. If I write something sloppy and an informed commenter points it out, I do not rush to correct it, but actually publicize my ignorance. This makes it clear to everybody that there is nothing scary in being ignorant, if one is willing to try changing that status.

Cheers,
T.

23. **Peter Woit**
July 15, 2008

somebody,

Well, I hope you start up a blog!

It would be better if there were better string theory blogs, but I think any smart person looking at the blogs dealing with the subject is going to get the accurate idea that, while they can't decide about the technical issues themselves, what is going on is that there are some wide disagreements among knowledgeable people about certain issues. Some think that multiverse and anthropic studies are nonsense, some don't. Some think string phenomenology and string cosmology are going nowhere, some don't, etc. String theorists who believe, like Lubos, that those who think there is a serious problem with string theory must just be ignorant and ill-informed are as delusional as he is.

All in all, I think the perception of the educated public about string theory is much closer to reality now than it was 5-10 years ago. At that time, the only information sources available to the public were pretty uniformly unrealistically optimistic about string theory's prospects, these days the other side of the story has become widely known.

24. **Mitch Miller**
July 15, 2008

Garett Lisi is a marketing genius:) We can add being featured in what will likely be the best selling New Yorker of all time to his already impressive achievements!

25. **MathPhys**
July 15, 2008

Peter,

Have you seen the art work on

http://lubos.motl.googlepages.com/crackpot-not-even-wrong.html

Lubos goes from high to higher.

26. **Peter Woit**
July 15, 2008

MathPhys,

Yes, I've seen that. The guy is endlessly entertaining…

27. **Bee**
July 16, 2008

I think he used the same pot already for Lee.

Anyway, regarding the question of scientists and the Web 2.0, I've recently had a post about this. Time and incentives are an important factor generally. If it is unclear to people what writing a blog is good for they will regard it a waste of time. Then there is (unfortunately) the fact that the reputation of the blogosphere is at best so-so. This is something that can change though. Another factor that Michael Nielsen pointed out to me which I found very interesting is that public discussions require an atmosphere of openness which might not work well in a community where people are protective about their insights (that's not exactly what Michael said, but how I understood it). E.g. consider there's a discussion about a paper on the arxiv. Will you just go and read what others say, or will you offer your own opinion? I am afraid most people would rather collect other's insights than offering their own. Of course that can't work.

Hi Tommaso,

Just to clarify, I didn't mean to say I find inaccuracies on blogs problematic, I was just trying to summarize that paragraph Peter quoted. You know that my writings also are often not overly accurate to make them more readable. It has happened then however, that people come and nitpick around on single sentences which I admittedly find extremely annoying. I find myself repeating again and again: if I had wanted to write a paper I had written a paper, this is a blogpost, and yes it's sloppy and it's inaccurate and it's full of believes and speculations and subjective opinions – live with it, or go read peer reviewed journals.

And yes, then there's the inaccuracies arising from lacking knowledge. I have certainly learned a lot from our commenters. The problem is however that I am afraid the barrier to publicly admit lacking knowledge can be quite high, esp. if it results in insults of the kind 'you're so stupid and know nothing'. I suspect that comments of this kind (which I keep getting in many instances when I do not write about a field I have worked in myself but would like to know more about) are very obstructive to a fruitful exchange.

Best,

B.

28. **mathandphysics1**
July 16, 2008

Peter

I have been reading your book. I have to ask whether you wrote it with the scientific underclass in mind?

I think that the real danger in physics populism (and Title IX in science as well), is that real math and physics will be less and less accessible to the public in general. Those who have the talent and the motivation will continue to pursue math and physics regardless of the social and governmental constraints imposed to make physics "fair and balanced".

Some of the great minds in history did their work in math and physics on their personal time (or rather, many had day jobs). Intelligent people who want to learn with an attention to truth will tend to seek each other out. The real loss is to the general public, who in their ignorance, will eventually tear down and destroy institutions of learning to achieve political ends (just look at the early christians and their destruction of any school of philosophy or knowledge that contradicted their world view).

These forces are ever present in the world, and yet their are those who would feed them for personal gain.

So I ask again, who was the intended audience of your book?

29. **anon.**
July 16, 2008

‘I think that the real danger in physics populism (and Title IX in science as well), is that real math and physics will be less and less accessible to the public in general.’ – mathandphysics1

Clearly, ‘Not Even Wrong’ caters for a wide audience of people interested in physics, a similar audience to those who read Feynman’s book QED. E.g., students, physicists in other areas than quantum fields, and the lay public who want a readable introduction to the key concepts and problems. It’s neither popularist hype for speculations, nor a heavy-going textbook of extremely advanced mathematics. Once you read these books, you get interested enough to start looking into the mathematics.

30. **Marion Delgado**
July 16, 2008

George, Dave, and others of the Motl-ite end of the string pool:

That exact tendency you’re showing, right here, above, to try to settle abstract physics issues by personal attacks, bizarre political tacks, and so on is exactly WHY people sometimes don’t respond, and never should respond. Or not to the alleged specifics – but to the paradigm that says this is any way at all to deal with science. Ever.

There’s a saying at law – when the facts (data) are on your side, hammer on the facts. When the law is on your side, hammer on the laws. When neither the facts nor the laws are on your side, hammer on the table.

You’re hammering on the table.

If you were representative of some “partisan” facet of physics, which you aren’t, properly, your posts would represent an admission of how bankrupt it was.

31. **Lucy in the sky**
July 16, 2008

Well the blog master seems to be very content with his career which is according to him way more than bashing people who do devote most of their time to science. I encourage anyone to check up on PW on Web of Science (you could get criticized when using Spires as Spires mostly focusses on hep). Quite interesting… 10 entries to be found. First scientific paper in 1983, eighth and last one in 1989, two more papers hitting on people who do actually work in 2002 and 2007, both papers completely void of any scientific content. He is 184 times cited in total, during the past 10 years he got a grand total of 18 citations. So a few questions pop up: how can a non scientist attack a whole field where – according to his own words – some of the best young minds of our time work? Here above he mentioned his promotion to “Senior Lecturer”. How is it possible that a person makes promotion at one of the world’s foremost universities without a single tracable sign of scientific activity in nearly two decades? Do we miss something? Or is a senior lecturer somehow disconnected from the regular body of professors doing science? I am pretty sick of this endless ranting against part of elementary particle theory by someone who has certainly not deserved his right to speak and who is not able to present any valuable or even just interesting alternative… Please keep us updated on conferences, talks, other blogs etc. but stop this incredible petty nonsense.

32. **Peter Woit**
July 16, 2008

mathandphysics1,

Despite what you might read some places, I’m not at all a physics “populist”. As far as progress in theoretical particle physics goes, it is an extremely demanding subject, and requires talented people working in the right sort of environment. The health of our elite institutions in this area is something I’m quite concerned about. But I don’t see the threats as coming from “populism” or the general public. Most people’s attitude towards these institutions seems to me to range from complete lack of interest to moderate degrees of respect. The more serious threat comes from within: if things get to the point where pseudo-science like the multiverse dominates and this is what you have to work on if you want a job, then these institutions are doomed as serious places.

The book was intentionally written to be readable by a wide range of people. One audience is mathematicians and physicists with a serious interest in the subject. I’ve been very pleased to hear from some colleagues I respect enormously that they enjoyed reading the book and learned some things from it. At the other end of the spectrum, much of the book tries to be a readable history and overview of the current state of the subject, accessible to any serious reader willing to deal with the fact that they won’t understand some of the more technical things I try and explain about.

While writing the book, I didn’t think much about who I was writing for, just trying to get down a story I wanted to tell as clearly and accessibly as possible. Later on, at some point I realized that there was one ideal reader that I had been writing for. In some sense it was aimed at myself, 35 years ago, explaining to a teenager just starting to get curious about this subject everything that I had learned about it. In that respect, it is intended as an introduction into the subject, hopefully a guide for someone who wants to learn more. It’s not a technical textbook, but I hope it can be useful when read in conjunction with books that do explain the actual technical tools needed to seriously understand the subject.

33. **Peter Woit**
July 16, 2008

“Lucy”

I think Marion says it well. When people are on the losing side of an argument, they descend to the ad hominem attack and start pounding the table in a desperate attempt to draw attention away from the question at hand.

Doing this while hiding behind anonymity is just pathetic beyond words.

34. **George**
July 16, 2008

Some active researchers do agree with Woit’s views, so Lucy in the sky’s comments aren’t that pertinent in my mind. If anything, the fact that someone who isn’t an active researcher can make this sort of attack on the physics establishment, whatever his motives, and have their response be so ineffective, is more evidence there’s something wrong with the way physics is today.

35. **George**
July 16, 2008

Also, to “Marion Delgado” above, I am not a Motlite by any stretch, or even a physicist for that matter. I don’t really know who’s right about this controversy. My comments above were mainly just curiosity if there were some personal motives behind his opposition to string theory. Clearly, looking at the two blogs, Motl’s and Woit’s, Motl’s definitely comes across as being more political and less professional. And if he is an active researcher, it doesn’t look like he will be much longer 😉

36. **p falor**
July 16, 2008

Peter et al:

I think Peter’s blog attracts lay people like myself (retired engineer) because they want to understand the issues (that’s right issues) that HEP and cosmology are attempting to address. The point that Peter has made repeatedly is that string theory makes no verifiable predictions and that ideas like the multiverse are untestable. The general response is typical: “Woit is not a researcher so ignore him.” Or these are research programs in progress.

After a while someone like me begins to think that fundamental physics research is starting to look more and more like a religion. Physicists start worshiping their equations, holding meetings to discuss their equations and claiming how the equations are the answer to all that is knowable and unknowable. No measurements that validate the equations are available but no problem. It is the beauty of the equations.

I have absolute no problem with individual doing this as long as it is not at the taxpayer’s expense. The lay public is in no position to question the technical details of any particular research but the lay public can certainly recognize when there is no connection to the real physical world. I think that has been a big advantage of the internet and particular blogs. More and more information is available to the lay public.

People like Peter are just trying to inform the public as they see physics research it. If others different then they should go all out to correct any misperceptions that Peter or anyone else is fostering. String theory and the multiverse idea are just two examples where the advocates are not presenting convincing (at least to me) arguments for their positions. So I would certainly welcome more dialogue from the other side. I am opened minded but very skeptical as of now.

37. **Peter Woit**
July 16, 2008

Since I just spent the morning trying to figure out what to do about a section of a paper I’ve been writing that has me confused, I kind of take exception to the “not an active researcher” characterization. I’m not someone who has ever been an active researcher in string theory, and my research work does move embarrassingly slowly, but it does take up a lot of time right now that I could otherwise be using to enjoy a restful summer vacation…

38. **Observer**
July 16, 2008

It is discouraging to see the String propaganda mount such vicious attacks against any form of serious criticism and against whistle-blowers like Peter. It is quite clear that all these personal attacks are nothing short of attempts for character assasination and, at the end of the day, reinforce the very failure of the String Theory program.

39. **a.k.**
July 16, 2008

‘..but I do think that the disagreement about (for example) string theory happens at such a rarefied and sophisticated level, that there is no way for an intelligent layman to judge who is talking sense,..”

The actual interesting point is that this is, at least concerning the current state of fundamental physics, in fact in a certain sense wrong. The essential ‘disagreement’ about (for example) string theory, as it seems, cannot be decided on any ‘positive’ level, as it would be sufficient for the constitution of an exact science, the disagreement about string theory takes place on a normative level, a level which couldn’t be more distant to the techniques and results which should actually discriminate valid reasoning from ‘nonsense’ in the exact sciences. This is the actual fate which fundamental physics seems to face at the moment: the unabilty to construct itself beyond normative ‘knowledge’ and methods. What could be a positive disagreement about string theory today is almost completely resolved by mathematicians, it is, to cut it short, the physical discourse itself, being not able to produce ‘positive statements’ and methods anymore that produced the ‘scientific subclass’ that initiated the threat to the ‘truths’ and ‘standards’ of physical reasoning, not some external program or entity as the internet, especially not the phenomenon ‘blog’.

40. **Marion Delgado**
July 16, 2008

George:

Regardless of what you now say is your actual position, which cannot be otherwise derived from your posts and hence would have to be obtained by mind-reading, the fact is your “question” is a very typical ad hominem attack disguised as an attempt to elicit information. You do not, let me say this emphatically, deserve the benefit of the doubt when you resort to such tactics. Indeed, it’s discernibly worse than simply saying what you’re asking about.

Moreover, you are decidedly being part of the effort to shift the ground from the only real issue – the current state of theoretical physics as it relates to string/brane/M theory – to picking out one person raising issues and asking completely irrelevant and ad hominem questions – making the theorist or challenger the issue instead of the theory or challenge.

And whether you are a string theory partisan or not, you are explicitly resorting to tactics that Motl and his supporters have endorsed by using them. If you posted something about how the LHC will be a valid test of a major swatch of string theory, for instance, and I or Peter Woit or anyone asked you if you were saying that only because you were a drug addict or had a crush on Lubos or something, that would be an ad hominem attack disguised as a question. I hope this clarifies the issue.

Because the point is, Woit’s history is simply not germane to the issues raised with string theory. The “theory”, if i can unfairly dignify it with that term, that you are referencing – that only academic failures attack string theory – does not hold very well when you consider the many fine physicists (penrose and feynman are just the tip of the iceberg) who have raised identical issues.

There is no OBJECTIVE reason for you to probe into someone raising issues that have been raised by probably at least a hundred other physicists, including some string theorists, asking if they’re MERELY raising those issues, again, but in a systematic way, out of spite because they are personally an academic failure.

It still boggles my mind and is very disheartening to think that something as abstract as particle physics could be subject to this level of tendentious and unsound discussion.

41. **Marion Delgado**

July 16, 2008

Let me add that Garrett Lisi's kewl surfer dewd persona is also not at all germane. He gets publicity for it, but I hope he neither gets dismissed nor respected for it.

42. **Peter Woit**
July 16, 2008

Marion,

Enough, the point has been made. As far as I can tell, George, like probably lots of other people who see Lubos's blog, was just curious to hear what I had to say in response to the way Lubos describes me. Fair enough. Lubos is so far out there it's hard to even get annoyed with him, and he at least puts his name to what he writes and takes responsibility for it. What's much more creepy are string theorists like "Lucy" who think anonymous character assassination is a good tactic. Undoubtedly this kind of thing works to some extent with people who don't know much about me, but on the whole I think it does much more damage to the reputation of string theory and string theorists than it does to mine. Go right ahead…

43. **George**
July 16, 2008

Marion, you really have me all wrong. I prefer to stay anonymous on these blogs, but I guarantee I have nothing vested in the string theory debates one way or the other. I've been around the academia block a few times, and I have seen the lowest motivations. I don't know Woit, Motl, or any of these people, and I'm not secretly on anyone's side.

I even bought with my own money the Smolin book, and probably would have bought Not Even Wrong if I had seen it in the bookstore first. Anyhow, I'm not going to argue this anymore, it's not like I am in a position to prove anything.

44. **Chip Neville**
July 16, 2008

I find it remarkable, and a bit scary, that the social and political context of the excellent "Surfing the Universe" article in the current New Yorker has only been mentioned here in passing. I am referring, of course, to the scandalous cover for the issue, depicting Michelle and Barak Obama as Black Panther revolutionaries and Islamic terrorists. Motl, who features an image of the cover on his blog, could not possibly have done worse.

The New Yorker's claim is that they were simply trying to parody the attitudes of some of Obama's opponents. If so, certainly a lot of the country misunderstood their intention. To relate this to the discussion here on Peter's blog, the misunderstandings that may arise from this misplaced imagery in a thoroughly mainstream and conventional medium are likely to eclipse by far those from blogs. So while Bee is correct to point out the perils of being misunderstood on a blog, there is really nothing new about this. For as long as there have been printing presses, there have been things written (or drawn) which would not have been misunderstood in personal, face to face communication.

45. **Curious Observer**
July 16, 2008

"…just look at the early christians and their destruction of any school of philosophy or knowledge that contradicted their world view…"

Every once in a while, someone just has to inject surreptitiously an unfair attack on Christians that has nothing to do with the topic of this blog.

46. **A.J.**
July 16, 2008

I find it remarkable, and a bit scary, that the social and political context of the excellent "Surfing the Universe" article in the current New Yorker has only been mentioned here in passing.

Peter has a long-standing habit of discouraging political discussion here. I wouldn't read too much into the absence of discussion of the New Yorker cover.

47. **Marion Delgado**
July 17, 2008

Peter:

Sorry, and I agree the point's hammered in. By the time I'd responded there were another few posts I hadn't seen.

George:

That is fair enough, and when I say "you" I should rather say, "one" or "anyone who…"

48. **Peter Woit**
July 17, 2008

Thanks A.J.,

Yes, once people start discussions of politics or religion, they then drown out everything else. Please don't do this, it's not like there aren't plenty of other places on the internet for such discussions…

49. **Russell Van Rooy**
July 17, 2008

To Haelfix:
I'm a rank amateur math and physics groupie – no physics or mathematics degree – hell I can barely count. And yet I ended up at this blog and I don't see any crackpots around here…
Here is a partial list of blogs this amateur follows:
Ars mathematica, Cosmic Variance, God Plays Dice, Good Math-Bad math,Low Dimensional Topology,MathTrek,NonCommutativegeometry,Rigorous Trivialities,The n-Category Cafe (my favorite), The Unapologetic Mathematician, Not Even Wrong (I come here a good bit even though I'm more partial to string theory) and ZeroDivides.
I sometimes go to Lubos's blog too – he's a bit of a crack pot though (just kidding Lubos! It's a joke, yes really)

50. **mathandphysics1**

July 17, 2008

“Yes, once people start discussions of politics or religion, they then drown out everything else. Please don’t do this, it’s not like there aren’t plenty of other places on the internet for such discussions…”

Peter

I will swallow my pride and humble myself. My last comment was removed and must have been in violation of the policy.

I will even confess to being a trickster, a physics underclass wonk.

51. **Aaron Bergman**
July 17, 2008

Is Kostant really saying that Lisi is the first to use E_8 as a gauge group? Perhaps he means one of the noncompact forms? Otherwise, the statement is simply false.

52. **Peter Woit**
July 17, 2008

Aaron,

Maybe he did mean the non-compact form, but in general I’m afraid Kostant is not a reliable source about many topics in physics. He’s one of the greatest figures in representation theory, responsible for some fantastic stuff, but he’s not a physicist. This isn’t really unusual among mathematicians, I’ve more than once run into excellent mathematicians with an interest in physics who were under the impression that the Standard Model was a supersymmetric QFT.

53. **Kea**
July 17, 2008

Kostant was careful to point out to the reporter that he was not a physicist, so one must forgive him his errors in this regard.

54. **Peter Woit**
July 17, 2008

Aaron,

Rereading Kostant’s e-mail (see link in update above), I noticed that he did also write “I remind you that I am not a physicist and cannot comment one way or another on the physics involved”.

The most interesting thing I saw in the Kostant e-mail was his story of meeting Einstein shortly before his death, and being told that Einstein thought Lie groups would turn out to be very important.

55. **H-I-G-G-S**
July 18, 2008

Dear Peter,

Since you haven’t followed the technicalities of the Distler/Lisi exchange, how can you possibly know that there is more heat than light? It seems to me the exact opposite. Distler has devoted a great deal of time and effort trying to understand Lisi’s proposal and is trying to get him to make clear and correct mathematical statements. It is one of the few exchanges on this topic where there is actually some mathematical substance rather than just discussions of sociology. I think your attitude towards Distler has blinded you to this fact. I do think that David Gross had the most appropriate quote in the New Yorker article when he said that he was “extremely reluctant to add fuel to this silly story.”

56. **Peter Woit**
July 18, 2008

“H-I-G-G-S”,

I have extensive personal experience engaging in on-line discussions with Distler, and have found them to be mostly a waste of time due to his unprofessional behavior. I took a quick look at the Distler/Lisi exchange, and it seemed to be pretty much more of the same, with no sign of an honest attempt to deal with the arguments Lisi was making. In any case, as I’ve repeatedly pointed out, I’m not a big fan of the project of trying to fit known symmetries into a larger simple group. If Lisi can get somewhere with it, great, but I’d rather spend the limited time I have thinking about other things. For those who are interested, I put the link there to follow, but just was reporting the impression I got from it.

Speaking of unprofessional behavior, I’m really getting thoroughly sick of people like you who seem to think that posting anonymous comments on blogs about physics criticizing others is a legitimate form of behavior for a scientist to engage in. Either be willing to take responsibility for what you write and put your reputation on the line behind it, or knock it off.

57. **Aaron Bergman**
July 18, 2008

I understand all that about Kostant who certainly is a great mathematician. It’s just somewhat dismaying that this sort of misinformation can get out there.

58. **H-I-G-G-S**
July 18, 2008

Peter,

I was simply pointing out the inconsistency between your statements that “I haven’t followed the details” and now your most recent “I took a quick look” and your claim that Distler adds more heat than light and doesn’t deal with Lisi’s arguments. He does deal with Lisi’s arguments, and adds a great deal of light, but there is simply no way you can tell this unless you are willing to enter into the technical details and have more than a quick look. It’s very superficial of you to jump to conclusions when you don’t understand the substance of the argument.

As for your being sick of my posting anonymously, I assure you I find your constant complaints about it equally tiresome. And no, I see nothing at all unprofessional about what I am doing.

59. **anonymous spiritually old man**
July 19, 2008

Aaron,

In his papers Kostant is careful about what he says. His papers are wonderful to learn from because they are carefully organized, detailed and precise, as well as deep. Something posted on his office door is presumably meant for the consumption of his near neighbors, and perhaps with it he has not taken the care he does with his papers. And, as the example of Serge Lang shows very well, an expert in one area can be a fool in another (although I would not accuse Kostant of this), particularly if he lets himself believe that the area in which he is expert is fundamentally more difficult or more fundamental than are other areas, or he lets himself believe that he is the intellectually honest one in a sea of scoundrels.

Woit,

Distler's exchange with Lisi seems to me quite reasonable. You do yourself no favors by characterizing it another way without having read it. Lisi claimed something that can't be, and Distler explains pretty clear what it is and why it can't be, and to ignore this is to stick one's head in the sand; to his credit Lisi doesn't ignore it. I find Lisi an appealling character, but one a lot like a lot of folks I have known, who does not appear to have written a solid, careful paper of the sort I like to see. It seems for this last reason that he has inpsired the wrath of the experts; this is human if not always nice – but it is natural that people that work hard, take care, think deeply, etc. get peeved when some guy who has perhaps not taken as much care finds is on the front pages ballyhooed as having done what they have not. Many experts are like small children emotionally.

60. **Peter Woit**
July 19, 2008

ASOM,

I did read Distler's exchange, or at least enough of it to see that he was engaging in the same argumentative tactics that I have extensive personal first-hand experience dealing with. For a good example, take a look at this, from one of the first postings on my blog more than four years ago:

http://www.math.columbia.edu/~woit/wordpress/?p=3#comment-30

Jacques started out insulting me anonymously, but his style was just too recognizable, so he stopped doing that. His argument was much like his one with Lisi, that the person he was arguing with was ignorant and didn't understand a technical point about chirality that an expert like himself did. Except in the 2004 case I was well aware of the technical issue he going on about, it was just not relevant to the argument I was making (that we don't understand nonperturbatively the electroweak interactions, since we don't have a good way to handle that kind of chiral gauge theory).

Anyway, in the 2008 case, maybe his complicated representation theory argument shows that Lisi has a problem with getting a chiral theory. As I've repeatedly pointed out, that doesn't interest me enough to work through the details. Maybe Distler is right on the technical point, and doing science a service in working this out. Maybe his technical point is irrelevant. I don't know. I'll stick to my characterization though of how he chooses to argue points like the one in question here.

61. **ERic**
July 19, 2008

The whole Lisi story seems to me to have just been a media phenomena which was started on various blogs, given credibility by the comments of Smolin, and picked up by newspapers and television who liked the idea of an independent surfer dude being the next Einstein. Otherwise, there's really nothing else there. The paper is wrong, and no different than 1000 other wrong papers that come along every day. The attention it received was completely unjustified. It's sad to see Lisi strutting around now as though he's actually done something.

62. **John Baez**
July 19, 2008

Are there any particularly interesting or inflammatory quotes by me in that New Yorker article? Apparently not, since nobody has mentioned any. But I'm curious: I don't have easy access to a copy, and I'd like to know if my attempts to stay out of trouble succeeded.

63. **Aaron Bergman**
July 19, 2008

There's only one quote by you (JB):

> "If a figure is so beautiful and intricate and clear, you figure it must not exist for itself alone," John Baez, a professor of mathematics at the University of California Riverside, said. "It must correspond to something in the physics world."

Anyways, having finally found a copy of the article, really, there's very little to it at all; it's more of a profile than an article about physics. One can argue about various characterizations. Just to pick one, Lisi doesn't use a trick or handwaving to incorporate three generations; he just doesn't do it and expresses a vague hope that maybe triality might have something to do with it. Mainly, the real impression I get from the article is that the person most interested in making this part of the string wars is Lisi. Why didn't Lisi get a postdoc doing Clifford algebras after writing a thesis on a subject completely unrelated to them or anything else in hep-th or gr-qc? String theorists. Why don't string theorists like his theory? Because he uses loop quantum gravity techniques (which he doesn't actually.) Or maybe because he acknowledged Peter Woit. Why was the audience at Davis not particularly impressed? Full of string theorists. It's all a bit tiresome, but I suppose I only think that because I'm a string theorist.

64. **Urs Schreiber**
July 19, 2008

> Because he uses loop quantum gravity techniques (which he doesn't actually.)

Right. What he does is mention that he has, or is imagining to have, a theory whose configuration space is entirely one of connections. But then, if so, it is some kind of superconnections.

I agree with and would tend to promote the very constructive summary and outlook that Jacques Distler gives at

http://golem.ph.utexas.edu/category/2008/05/e8_quillen_superconnection.html#c016877

which ends with:

> That said, there is something kinda cool about the elements of the construction:

1. An embedding of Spin(d−1,1) in G gives a Z_2 grading on $\mathfrak{g}$.

2. Using the corresponding Schreiber superconnection, one naturally gets a theory with fermions, corresponding to the odd generators of $\mathfrak{g}$, transforming as spinors Spin(d−1,1).

It would be mildly interesting to see what sort of actions one could build with this construction.

The punchline being: it's not a working "theory of everything", but there is an interesting idea used here which deserves to be further investigated.

65. **Tony Smith**
July 19, 2008

John, the New Yorker Garrett Lisi article is long (7 pages) so maybe I missed something, but I think that you did succeed in staying out of trouble. I don't recall you being quoted directly, but Bertram Kostant was quoted a lot, and you were his host when he made the E8 talk that was widely seen on the web, so maybe you are indirectly there through him. I think the things that he said were interesting, such as:
"… "Columbus made mistakes and thought he was in India. Lisi made a few errors, but this pales in significance to his possibly opening up a whole new world for exploration … E8 is like North America, South America, and the Pacific Ocean rolled into one. …[Lisi's]… daring … possibly creates an agenda for scientists for the next hundred years or more." …".

Peter, as to Jacques Distler's behaviour on n-Category cafe about Garrett Lisi (here I am avoiding any mention of substantive physics),
Jacques Distler put up a post there (now deleted) saying:

""Ginsparg's Law
Summerizer wrote "Any phenomenologist or grand unified model builder would be absolutely flabbergasted that the discussion here is even taking place."
I suppose that now would be the time to invoke Ginsparg's Law"
Posted by Jacques Distler on July 19, 2008 4:23 AM …"".

Following links that Distler put in that deleted post lead to such things
as:
xkcd comic number 386 (about a compulsion to correct people who are "wrong" on the internet)
and
a (now deleted) paste.lisp.org paste number 63894 saying in part:
"… More comments from Paul Ginsparg
Jul 18, 2008 … Don't they realize that this makes them look like chumps-by-association? . feel free to refer them to "my" law below …".

So,
I wonder if Jacques Distler thinks of Bertram Kostant as a chump-by-association with respect to Garrett Lisi ?

Tony Smith

66. **Jonathan Vos Post**
July 19, 2008

The New Yorker has the following "Abstract" online. Not sure how long until the full article is also online.

Benjamin Wallace-Wells, Annals of Science, "Surfing the Universe," The New Yorker, July 21, 2008, p. 32
July 21, 2008 Issue

Keywords
Lisi, Garrett;
Theoretical Physics;
E8;
String Theory;
Mathematics;
Science, Scientists;
"An Exceptionally Simple Theory of Everything"

ANNALS OF SCIENCE about physicist Garrett Lisi's "An Exceptionally Simple Theory of Everything." Writer describes Lisi giving a talk at a conference in Morelia, Mexico in June of 2007. The conference was attended by the top researchers in a field called loop quantum gravity, which has emerged as a leading challenger to string theory. Lisi believed that he had discovered what physicists call a Theory of Everything—a unifying idea that aims to incorporate all the universe's forces in a single mathematical framework. Within four months, Lee Smolin, one of the founders of loop quantum gravity, said that Lisi had "one of the most compelling unification models" he had seen in years. Discusses the persistent legend of the hermit genius in physics, from David Deutsch to Albert Einstein. Lisi got his Ph.D from the University of California at San Diego and, at thirty-one, dropped out of academia. For almost a decade, Lisi moved on no fixed schedule between Maui, where he likes to surf, and the mountains of the West, where he snowboards. He worked intermittently, but mostly he tried to think about physics. Five months after the Morelia conference, Lisi published the theory in a paper called "An Exceptionally Simple Theory of Everything" in an online forum. In the acknowledgements to the paper, Lisi thanked Peter Woit, a Columbia mathematician, whose name signaled a declaration of partisan affinities. Woit is best known for his one-man campaign to discredit string theory. Describes string theory and Lisi's skepticism of it. Describes Lisi's personality, which is self-depreciating and ironic and not particularly hermitlike. Lisi feels that his isolation from the academic world gives him advantages over his contemporaries who need to publish regularly for the sake of career advancement. Describes how Lisi developed his theory, in particular, his use of the mathematical entity E8. (A circle has one degree of symmetry; a sphere has three; a space whose symmetries are described by E8 has two hundred and forty-eight.). Lisi discovered that he could plot all the universe's components on E8. On the terrain of E8, he believed, general relativity and particle physics were no longer out of joint. Tells about the arXiv online forum where Lisi published his paper. Describes criticisms of Lisi's theory, including Jacques Distler noting Lisi's use of a mathematical trick to incorporate the second and third generations into his model. Tells about Lisi presenting his theory to professors and students at the University of California-Davis. More recently, Lisi has begun to feel a set of responsibilities—to the broader physics community and to those who supported him, but also to fully extending his ideas. Last May, he headed to the Perimiter Institute in Ontario as a visiting researcher.

67. **John Baez**
July 19, 2008

Thanks for giving my supposed quote, Aaron.

What an unhappy quote! A fact-checker for the New Yorker send me an email with a lot of question including one about this, saying:

> Ben writes that you told him that if a figure is so beautiful and intricate and clear, you figure, it must not exist for itself alone. It must correspond to something

> in the physical world. Is this correct?

to which I replied:

> I was trying to say that this is a feeling physicists often have. I was *not* trying to say I personally believe this!
>
> There's no rational reason why something beautiful "must" correspond to something in the physical world. This "must" is more like an avid hope.

but of course none of this came through. I also thoroughly dislike using the word "figure" in two such different ways, so close:

> If a figure is so beautiful and intricate and clear, you figure it must not exist for itself alone…

More importantly, I would never call E8 a "figure"… maybe Wallace-Wells was focused on that famous figure of the root system of E8. Indeed, the fact-checker asked this question about E8:

> We say that its most common representation looks like a dense and precisely symmetric cloud of spider's web, thousands of threads exploding out from hubs of concentric spheres. Is this okay?

to which I replied

> Far be it from me to argue with a poet! Decide for yourself; [here's the picture].

Oh well, at least they didn't have me saying anything inflammatory or utterly ridiculous.

68. **moshe**
July 19, 2008

Aaron, I detect a case of bad attitude. Just admit it is all your fault, and we'll be back to the glory days of physics, when patent clerks and surfer dudes revolutionized physics. It's not that hard really, and with some luck we might get a cure for cancer also.

69. **Bee**
July 20, 2008

Gee, what happened to this thread since I last looked at it?

Since I above mentioned Michael Nielsen, [he just wrote a really great post on the question of openness in science and the Web 2.0], I can really recommend it.

70. **Michael Gogins**
July 20, 2008

Bee, thanks for the link to Nielsen's post, which I have just read.

Somewhat against Nielsen's thinking, it's much easier to become a productive programmer (I am a programmer, both commercial and open source) than it is to become a productive physicist (I am not one). And it's much easier to see if open source software will build, or run, or do what you want, than to evaluate a physics paper, vet a grant proposal, or build a large particle accelerator. Therefore, part-time physicists have a much greater competitive disadvantage than part-time programmers. And FULL-time physicists are inherently expensive….

It is true that online publication and collaboration are growing, but this whole Lisi discussion makes me think that anonymous posts are a major problem. If everyone had to sign their real name, I think a lot of hateful and useless posts would just go away. But would there be any useful posts left? Maybe just an informal enforcement against personal comments would do the job….

I don't expect the anonymous posts to go away until signing one's posts costs nothing in terms of academic politics. Indeed, until posting online is seen as making a positive contribution to science.

Regards,
Michael Gogins

71. **Professor R**
July 20, 2008

Peter, I think there is a far better article on the Lisi paper in this month's issue of Physics World. Or rather, there is a very nice article on the role of symmetry groups in particle physics, with an extremely brief description of the Lisi paper at the end.
Which puts it into sensible context I think…it should have been discussed in this manner in the first place!

Hawking to Perimeter?

July 16, 2008
Categories: Uncategorized

The Canadian press today is putting out the story that Stephen Hawking may be abandoning Cambridge to move permanently to the Perimeter Institute, where he would join recently appointed director Neil Turok, as well as Lenny Susskind.

From the stories it appears that Hawking plans to visit Perimeter next year for a month or so, and that he hasn't actually made any decision about a permanent move.

For more, see here, here, here, and here.

According to Sam Blackburn, an assistant of Hawking's, Hawking is just mulling the idea over with no move imminent, but he is "obviously a man of few words so the first we would probably know of it is when he packs his bags."

Comments

1. **anon.**
   July 16, 2008

   Exciting news. BTW, from the first link you gave, there is a link to a new exciting string claims by Michio Kaku in the 16 July Spirituality section of *The Times of India:*

   http://timesofindia.indiatimes.com/Speaking_Tree/Looking_for_a_higher_theory_of_everything/articleshow/3108752.cms

   "… String theory can be applied to the domain where relativity fails, such as the centre of a black hole, or the instant of the Big Bang. … The melodies on these strings can explain chemistry. The universe is a symphony of strings. The "mind of God" that Einstein wrote about can be explained as cosmic music resonating through hyperspace. … String theory predicts that the next set of vibrations of the string should be invisible, which can naturally explain dark matter. … when we get to know the "DNA" of the universe, i.e. string theory, then new physical applications will be discovered. Physics will follow biology, moving from the age of discovery to the age of mastery."

   (I respect him for now making such claims in the "Spirituality" section, rather than making them in the science section as usual.)

2. **Chris W.**
   July 16, 2008

   This just in: String theory justifies the efficacy of homeopathy.

3. **jhk**
   July 16, 2008

   Hawking is probably just using this as leverage in his salary negotiations with Cambridge.

4. **Coin**
   July 16, 2008

   Hm. My understanding was that Stephen Hawking was mostly a teacher or lecturer these days, whereas Perimeter is a research institution. I ask this purely out of ignorance– is Stephen Hawking really someone to be considered a particularly active researcher right now? Would a move to Perimeter, assuming he's actually considering it, likely mean a move toward a greater focus on research on his part?

5. **Peter Woit**
   July 16, 2008

   Coin,

   Actually Hawking has always been and continues to be more of a researcher than a teacher or lecturer. He has several recent papers on the "no boundary proposal", and is involved in the all-too-active hot subject of how to count universes…

6. **Coin**
   July 16, 2008

   Alright, thanks.

7. **Anon**
   July 16, 2008

   One should bear in mind that Hawking reaches Cambridge's retirement age in six months' time. The last person to retire from his position, Paul Dirac, chose to move to Florida, so a transatlantic move would not be unprecedented.

8. **Chris W.**
   July 16, 2008

   A few more specifics on what Hawking has been up to lately:

   See arxiv.org/0803.1663 and this popular account. Note his co-authors.)

9. **Marion Delgado**
   July 16, 2008

   Chris W:

   Is homeopathy as internally consistent as string theory, I wonder?

Also the competing medical theories covering the same ground as homeopathy are much more robust than, say, twistors or LQG are compared to string theory.

10. **Chris W.**
    July 16, 2008

    Marion,

    My comment was a *joke*. If it's going to be taken as a knock against anything, the knock can be considered to be directed at Kaku, who seems to have misplaced whatever taste he might have had. With friends like this, string theory doesn't need enemies.

    (Speaking of Hindu spirituality [of a sort], I wonder if the TM crowd still employs references to string theory, as they used to do when John Hagelin still had an academic career.)

11. **Bee**
    July 17, 2008

    Globe and Mail: Hawking visit dubbed 'a single trip,' not relocation

12. **Marion Delgado**
    July 17, 2008

    Chris W:

    Sorry, my response was sort of idle musing. I was thinking very much of the "bottom up" organizing theories and Smolin's account of how they let people like Capra run away with themselves.

    It was not really intended as a rejoinder.

13. **stevem**
    July 17, 2008

    There was a 2-part program on Hawking and his work
    on UK tv recently. It is posted on youtube (in 10 parts or segments) if anyone is interested. In this segment (at 7.10) check out the eerie music with six Ed Wittens sitting at a table.

    http://www.youtube.com/watch?v=7zxBdm1bNGw

14. **mathphys**
    July 20, 2008

    Is Hawking still married to his (second) wife? I ask because their marriage was also the subject of headlines news a couple of years ago.

15. **John D**
    July 21, 2008

    I delete a lot of the comments posted here, often especially ones critical of string theory, on the grounds that they aren't saying anything either new or relevant to the posting, just adding noise to this comment section. What follows is one of these which I've repeatedly deleted, but "John Dee" has more time available to waste than I do, so I have to give up. Please do me a big favor by not responding to off-topic comments from "John Dee" or anyone else. It's a continual struggle to try and stop the comment section here from being dominated by pointless off-topic comments like this one, a struggle that on some days recently I've just about decided to give up on, shutting down this section or turning it into a purely moderated one. Also fun to deal with is the deluge of spam comments (the counter says 43,382 such in the last two years), many of which look much like the "real" comments…. OK, I'll stop complaining now.

    «when we get to know the "DNA" of the universe, i.e. string theory, then new physical applications will be discovered. Physics will follow biology, moving from the age of discovery to the age of mastery.»

    Anyone suggesting that Physics should resemble Biology more is seriously misguided. If anything biology should turn into a more quantifiable/predictable field instead of mainly an ad-hoc collection of barely related data.

    This multiverse fad is turning Physics into Biology. Getting hard predictions is getting increasingly more difficult and data is getting more and more unrelated with contrived "solutions" created just to save face.

Awaiting a Messenger From the Multiverse

July 17, 2008
Categories: Multiverse Mania

As particle physicists eagerly await results from the LHC, many theorists are already promoting interpretations of what they hope it will find. This week's Chronicle of Higher Education has a cover story on the LHC entitled [The Machine at the End of the Universe](#) (see associated articles [here](#) and [here](#)). In it, Gordon Kane enthusiastically describes the LHC as "It is certainly the most important experiment of any kind in the past century, without qualification" and "the most important thing ever in our quest to understand the fundamental laws of nature and the universe."

The question that the LHC will actually address, that of electroweak symmetry breaking, doesn't get much attention. Instead, the focus is on supersymmetry, extra dimensions and string theory. While noting that there's no evidence for string theory, the article reports:

> The new collider could change that, says Joseph D. Lykken, a physicist at Fermi National Accelerator Laboratory. "Either the discovery of supersymmetry or extra dimensions is a triumph of string theory." While such a finding would not conclusively show that string theory is correct, it would provide a first crucial experimental test, he says.

At New Scientist this week, there's another article about the LHC, entitled [Awaiting a messenger from the multiverse](#). In it, Savas Dimopoulos explains that there's already quite a lot of experimental evidence against weak-scale supersymmetry:

> "After a lot of experiments there has not been any hint of SUSY," says Dimopoulos. For each individual one of those predictions, he says, you can find plausible explanations for why they are not seen. "But by the time you look at the whole package of 'things that should have happened but didn't', you start getting a somewhat baroque structure."

Dimopoulos instead promotes his joint work with Nima Arkani-Hamed on the idea of "split-supersymmetry", where the supersymmetry breaking scale is very high, so can't explain the hierarchy problem. One possible experimental signature of such models would be a long-lived gluino. They promote the idea that such a thing would be a "Messenger From the Multiverse", the idea being that if supersymmetry doesn't explain the hierarchy problem, the explanation must be the anthropic landscape:

> That powerful piece of evidence would have dizzying implications. "It would be a strong indication that there is a string landscape or a multiverse," says Dimopoulos. "I think the majority of opinion would come around to that point of view."

One aspect of this argument is that it also works if no gluino is seen. If no superpartners at all are found at the LHC, and thus supersymmetry can't explain the hierarchy problem, by the Arkani-Hamed/Dimopoulos logic this is strong evidence for the anthropic string theory landscape. Putting this together with Lykken's argument, the LHC is guaranteed to provide evidence for string theory no matter what, since it will either see or not see weak-scale supersymmetry.

The New Scientist article does explain that this kind of argument for anthropics has its critics:

> However, anthropic arguments remain controversial, and despite the authors' heavyweight reputations, split supersymmetry is no exception. "I'm not a big fan of it," says John Ellis, a theoretical particle physicist at the CERN laboratory, where the LHC is being prepared for its first run later this year. His criticism is that the anthropic approach gives you too much freedom to answer any troublesome question in physics. "At some point you might just as well say 'let's fine-tune everything' and go home," Ellis says.
>
> Theorist Frank Wilczek at the Massachusetts Institute of Technology is also unhappy with the idea of split SUSY. "I think it's a logical possibility, but if we really have to appeal to anthropic considerations I think that's a big retreat. It would mean the explanatory power of theoretical physics would be limited."
> …
> And even if the LHC does find Dimopoulos's stopped gluinos, not everyone will be persuaded that arguments based on the multiverse are good science. "My opinion of anthropic reasoning is likely to remain unprintable," says Ellis.

Update: Forgot to add one more piece of LHC-related news. France is now joining the [US](#) with its own national LHC web-site: [LHC-France](#). Because of the Gallic fondness for cartoons, it includes a section [Le LHC en BD](#).

Comments

1. **Yoo**
   July 17, 2008

   Is string theory becoming more like a mathematical framework like algebra? Or is it still something that should only be considered as a possible theory of the physical world?

   The way I keep hearing about string theory being able to be almost anything and that any experiment would only be providing evidence for the theory is getting me confused.

2. **Peter Woit**
   July 17, 2008

   There's nothing confusing about how to interpret the phenomenon of a theory that can explain almost anything. It's what's known as a failed theory.

3. **Observer**
   July 17, 2008

   It is interesting to note that the major topic of electroweak symmetry breaking (EWSB) remains somehow overshadowed by the strong desire to discover evidence for SUSY and extra-dimensions. Many people tend to forget that the lack of evidence for SUSY at LHC may carry serious implications for the validity of the Higgs mechanism.

4. **David Nataf**
   July 17, 2008

   Peter,

I'm wondering why you very frequently post links to New Scientist.
My early impression is that you promote advocate a conservative, cautious science, where claims should be qualified and skepticism is best.

I'm a former subscriber to New Scientist (3-4 years back), and I didn't renew because I found every issue engaged in ultra-sensationalism. While I found their coverage of politics, the environment, and the social sciences interesting, these are fields where my knowledge is shallow. In physics, and in astronomy, it was always my impression they went for the dramatic. The next Einstein is just around the corner, and he's going to establish contact with aliens by traveling to the multiverse through his LHC time machine. Or something like that.

I remember a cover story where people were claiming that pendulums oscillate differently during solar eclipses because of the ether. Right.

As there are zillions of different publications it's most probably you specifically chose to read NS as opposed to being compelled to read it due to a lack of options. I'm perplexed by your focus on NS as it does not strike me as a Woit-like publication.

5. **Peter Woit**
July 17, 2008

David,

New Scientist is definitely a mixed bag, with a lot of reasonable stories about science (as well as book reviews, opinion columns, etc), but also a fair number of overly sensationalistic stories about physics. Most of these I just ignore, although sometimes I can't resist making fun of some of the sillier ones that invoke string theory.

But this story is an example where I think the magazine is actually straightforwardly reporting about research claims being made by leaders of the physics community. It's not the reporter who is putting out sensationalistic nonsense, but two of the most well-known researchers in the field, from Stanford (Dimopoulos) and Harvard/IAS (Arkani-Hamed). Sure, NS stories about how some obscure researcher has an idea for a time machine are just silly and should be ignored by everyone. But when leading figures in particle theory start going on about how the LHC is going to prove the existence of a multiverse, I can see why NS would cover this, and here they seem to have done so responsibly and pretty accurately. The sensationalist nonsense isn't the fault of the NS writer or editor.

In this particular case, I also thought the quotes from two other leading figures (Wilczek and Ellis) were highly newsworthy, given the ongoing controversy about the multiverse among theorists. Wilczek has had some sympathy for and worked on some anthropic sorts of explanations, here he was critical of this one, which was interesting. The fact that John Ellis's view of anthropic reasoning is "unprintable", and that he says it will remain so even in the face of claims from the anthropic crowd to have experimental evidence was definitely news to me.

6. **Lucy in the sky**
July 18, 2008

Hi PW, no I am not a string theorist. Quite convenient to label anyone who doesn't agree with you with the same label. I have a problem with a string theorist at the IRS… Anyway I used – some while ago – to work on theoretical elementary particle physics (QCD related) but turned since then to more condensed matters… So nothing at stake here except that I am quite upset that a single person can mislead numerous non-experts without any rebuttal from leading scientists (I am not claiming to be one). This explains my anonimity, even a hint that one looks at your blog is highly suspect in any serious physics environment (and no I am not talking about string theory departments).

I think that you mistake my point. You continously attack a main field of research. But whenever a concerned person takes his or her time to give you a serious answer – which unavoidably is technical – you manage to change the topic or you start writing your standard paragraphs (actually this is not only on your blog, we did meet in person and that confirmed the statemenet I just made). A few examples for the non-expert readers who might still believe that you are a leading scientist: reread the thread after "Wonders of supergravity etc." some items below here. If you go to the Musings blog by Distler or Asymptotia by Johnson, you should make a search on "Woit" and read the discussions. In at least half a dozen instances you manage to completely avoid answering very concrete technical/scientific questions, instead always reiterating the stuff we've read many times by now. In fact doing this I see that for many years you are alluding to your slowly progressing research. It remains a fact (give me a counter example if I am wrong) that since 1989 you did not produce a single (published) scientific result. At the same time the innuendo in your blog is such that any uninformed person can only get the idea that you are an active scientist. In my book this is called cheating. Anyway, a few threads below you announce that you are finishing up a project. I am looking forward to see the result of 19 years of hard work… So long, I am out of here for good…

7. **Peter Woit**
July 18, 2008

"Lucy"

You're not hiding behind anonymity because you fear damage to your reputation if someone finds out you have looked at the "Not Even Wrong" blog. You are doing this because you are dishonest. Your anonymous comment comes from the same block of network addresses as those of an identifiable string theorist who has posted critical comments here (and who, when you google him, the second link that turns up is one about how dishonest he is, on matters unrelated to science). Maybe this is you, maybe it's someone else from the same institution, but looking at the names of physicists working there, there's no one answering the description you give of yourself, in particular the only person I've personally met there is a senior string theorist (not the one who commented here on the blog).

So, I'm convinced that you're just a liar, using anonymity to lie and engage in character assassination with impunity. As for your characterization of my exchanges with Distler or Clifford Johnson, people can read them for themselves and make up their own minds.

If you have anything else to say, you better do it under your own name.

8. **p falor**
July 18, 2008

Peter:
Good comeback to "Lucy". I read your book and understand that basically the SM was formulated in the time frame 1973-1974. Since then experimental results have been confirmed some parameter values. Other than neutrinos having mass (accepted fact?) have there been in other deviations from the SM?

Now there is continuing reference to leading particle physicists, other than the recent Nobel prize winners Gross, Politzer, & Wilczek (2004), 'T Hooft & Veltman (1999) and Glashow,Salam & Weinberg (1979) has there been any particular HEP physicist that really stands out? Not about speculative ideas such as ST but an idea that really attempts to address an issue with the SM? You seem to have great respect for Witten but can you cite anything that Ed has doen that would merit a Nobel? Who is your leading canidate for work done since 1973?

9. **woit**
July 18, 2008

p. falor,

Besides neutrino masses, no definite deviations from the SM have shown up at accelerators. From astrophysics, there's evidence for "dark matter" of a sort that doesn't seem to fit into the SM.

Since 1973, no theorist has come up with a big breakthrough in experimentally testable HEP theory. That's why there have been no Nobel prizes for work done since then in this area. Many people have done significant work that helps to better understand QFT. A lot of this is from Witten, whose investigations of the relations of QFT and math won him the Fields medal, a recognition in math even harder to get than a Nobel prize in physics. The work of Witten's that I can think of that is related to experiment and might be worthy of a Nobel prize is his early 80s work on large N, current algebra and the "skyrmion". This is a quite beautiful approximate model for the low-energy behavior of QCD. Other people also contributed to this though, and the goal of this work, a good 1/N expansion for QCD, still hasn't been achieved.

10. **Observer**
July 18, 2008

p. falor and Peter,

I wish to add few comments on deviations from SM. It is important to remind ourselves that, while SM is remarkably consistent with experimental observations, it is considered incomplete for the following main reasons:
a) SM does not include the contribution of gravity and gravitational corrections to both QFT and renormalization group equations; b) SM does not fix the large number of free parameters that enter the theory (in particular the spectra of masses, gauge couplings and fermion mixing angles); c) SM has a gauge hierarchy problem, which requires fine-tuning; d) SM postulates that the origin of electroweak symmetry breaking is the Higgs mechanism, whose confirmation is sought at LHC and ILC. The number and physical attributes of the Higgs boson are neither explained by SM nor fixed from first principles, e) SM does not clarify the origin of its underlying gauge group and why quarks and leptons occur in certain representations of this group, f) SM does not explain why the weak interactions are chiral, that is, why the force transmitted by the triplet of massive vector bosons is sensitive to handedness g) SM does not explain the origin of CP violation and the underying cause of the lepton anomalous magnetic moment.

11. **Sulfur Surfer**
July 18, 2008

The Wilczek quote is particularly ironic because his latest paper, arXiv:0807.1726, is about making predictions using anthropic reasoning. I think this reflects the general situation: no one likes it, but there are reasons to believe that anthropic selection plays a role in determining why our universe is the way it is, and if so then we have to learn to deal with it.

12. **Peter Woit**
July 18, 2008

Sulfur Surfer,

The problem with anthropic reasoning has nothing to do with whether one "likes it", but with whether it can be used to do real science, or is just being used as an excuse for propping up failed ideas, in such a way that even though they don't work, they can never be shown to be wrong.

I take Wilczek's point of view to be that some uses of anthropic argumentation are a legitimate part of model-building, and that's what he's trying out. Interesting to see that it appears that he doesn't see the Dimopoulos/Arkani-Hamed argument as falling into this category. Perhaps all there is to it is that he recognizes the obvious fallacy in the argument that: "If supersymmetry doesn't explain the weak scale, it must be the string theory anthropic landscape".

For more from Wilczek about his point of view on this, a good source is:

http://arxiv.org/abs/hep-ph/0512187

where he warns:

"let me lament our prospective losses, if we adopt anthropic or statistical selecton arguments too freely…"

and notes

"Resort to anthropic reasoning involves plenty of pain, as I've lamented, but so far the gain has been relatively meagre, to say the least."

13. **Chip Neville**
July 18, 2008

Peter,

Just a thought from a refugee from the '60's. "Lucy in the Sky with Diamonds" is a very good Beatles song that turns out to be about LSD. I wonder if "Lucy" realizes this.

Maybe I should have signed this "Yellow Submarine," which is about amphetamines.

14. **somebody**
July 19, 2008

> Putting this together with Lykken's argument, the LHC is
> guaranteed to provide evidence for string theory no matter
> what, since it will either see or not see weak-scale
> supersymmetry.

String theory is a framework for constructing UV complete theories while having gravity. It is really a paradigm in that sense, like QFT was for theories without gravity. ANY experiment at low energies is "evidence" for QFT. That hardly means that a specific QFT is not falsifiable.

The real problem is that constructing models with the stringy paradigm is hard because we don't understand the theory well enough. That would be a valid and perhaps fatal criticism for string theory as a useful theory. But comments like the above are merely strawmen.

15. **Peter Woit**
July 19, 2008

somebody,

The above was not a criticism of string theory, but of string theory partisans, at least those who think it's a good idea to go to the press with bogus claims that the LHC will test string theory or the the anthropic multiverse.

16. **ERic**
July 19, 2008

Peter,
I'm just curious what your position on string theory would be in the case that either the superpartners are found or there is evidence of large extra dimensions such as black holes or KK modes? Would you consider this evidence in favor of string theory?

17. **Peter Woit**
July 19, 2008

Eric,

Depends exactly what you see, but, in general, no. For instance, it might very well be that some variant of N=8 supergravity gives a finite quantum theory of gravity, which is not a string theory. If supersymmetry shows up at the LHC, maybe it's that…

18. **Marion Delgado**
July 19, 2008

Peter:

Going beyond the anthropic, and even the string issues, to the more important problem of the stagnation of particle physics in general, do you have any sketched-out notions on what particle physics – or better, that segment not wedded to theories/programs that are too adaptable to be tested – could do to be prepared to test things with the tests the LHC could do?

Even if the various measurements and experiments that can be performed with the LHC don't effectively test, say, stringe/brane/M theory, is it possible for them to test other things? Advance particle physics and weed out theories the way the lack of proton decay did, for instance?

Is LQG, are twistor theories, is Lisi's extension of standard physics, as untestable w/r/t the LHC as the current dominant line of string theory is?

Also, will some string theory lines probably be closed off by LHC-performed measurements and experiments?

19. **Chris W.**
July 19, 2008

[RE: "ANY experiment at low energies is 'evidence' for QFT."]

Somebody,

Even as a framework, QFT has certain empirically founded underpinnings, does it not? (I have in mind special relativity.) What are those, and what evidence supports them?

Similarly, *as a framework*, what empirically founded underpinnings does string theory rely upon? Does it assume anything that QFT doesn't assume? If so, what is the empirical support for those assumptions?

20. **Shantanu**
July 19, 2008

Peter, have you found anything interesting among the talks at
titled
"In Search for Variations of Fundamental Couplings and Mass Scales"

21. **Observer**
July 19, 2008

Somebody, you say:

"The real problem is that constructing models with the stringy paradigm is hard because we don't understand the theory well enough"

How long should one wait for String Theory to reach maturity? Would reaching that stage guarantee that the astronomical number of vacua will suddenly disappear or that the theory will yield testable predictions?

22. **somebody**
July 20, 2008

"Similarly, as a framework, what empirically founded underpinnings does string theory rely upon? Does it assume anything that QFT doesn't assume? If so, what is the empirical support for those
assumptions?"

Ok, I'll try.

Perturbative string theory has something called conformal invariance on the worldsheet. The empirical evidence for this is gravity. The empirical basis for QFT are locality, unitarity and Lorentz invariance. Strings manage to find a way to tweak these, while NOT breaking them, so that we can have gravity as well. This is oft-repeated, but still extraordinary. The precise way in which we do the tweaking is what gives rise to the various kinds of matter fields, and this is where the arbitrariness that ultimately leads to things like the landscape comes in.

In principle, this is a model-building/technical control issue. But it is a pretty damn serious one, and probably something that will require us to understand the theory much better than we do now. Besides, the kind of understanding that we have of string theory at the moment allows us to build only certain kinds of models.

About other empirical expectations: string theory has a lot of GENERIC features we expect from a good theory of the world. It can easily give rise to things like multiple generations, non-abelain gauge symmetry, chiral fermions, etc. some of which were considered thorny problems before. Again, constructing PRECISELY our matter content has been a difficult problem, but progress has been ongoing.

But the most important reason for liking string theory is that it shows the features of quantum gravity that we would hope to see, in EVERY single instance that the theory is under control. Black hole entropy, gravity is holographic, resolution of singularities, resolution of information paradox – all these things have seen more or less concrete realizations in string theory. Black holes are where real progress is, according to me, but the string phenomenologists might disagree. 🙂

Notice that I haven't said anything about gauge-gravity duality (AdS/CFT). Thats not because I don't think it is important, but because I think it is THE most important thing. But at the same time, I don't know of a good way to convince a layman (assuming you are one) of its importance. Because it is one of those cases where two vastly different mathematical structures in theoretical physics mysteriously give rise to the exact same physics. In some sense, it is a bit like saying that understanding quantum gravity is the same problem as understanding strongly coupled QCD. I am not sure how exciting that is for a non-string person, but it makes me wax lyrical about string theory. It relates black holes and gauge theories. Things like AdS/quark-gluon-plasma stuff is interesting not merely because we can make some semi-quantitative predictions, but also because it has opened up new ways of THINKING. You can find a bound for the viscosity to entropy ratio of condensed matter systems, by studying black holes – thats the kind of thing that gets my juices flowing. Notice that none of these things involve far-out mathematical masturbation, this is real physics – or if you want to say it that way, it is emprically based.

To me, the immediate reason for doing string theory is not falsifying it at LHC. Of course, to get funding you need to sell it to the powers-that-be, and LHC is perhaps all they care about. But really, LHC is just a miniscule blip in the energy-scales all the way up to Planck scale. Should we expect to see ANYTHING interesting at LHC? Perhaps. Perhaps not. Of course it would be nice to see some indication of string theory there because public relations is unfortunately important in science, but I personally think that people are being short-sighted when they hedge their bets on LHC when selling it to the piublic.

String theory is a large collection of promising ideas firmly rooted in the emprirical physics we know which seems to unify theoretical physics[1], and thats why it should be worked on. The spinoffs are in that sense are OBVIOUS, but they are not necessarily going to fit anybody's schedule for making a collider.

[1] I don't mean unification of forces necessarily, I mean convergence of the different ingredients that constitute theoretical physics.

23. **anon.**
July 20, 2008

'Perturbative string theory has something called conformal invariance on the worldsheet. The empirical evidence for this is gravity. The empirical basis for QFT are locality, unitarity and Lorentz invariance. Strings manage to find a way to tweak these, while NOT breaking them, so that we can have gravity as well. This is oft-repeated, but still extraordinary. The precise way in which we do the tweaking is what gives rise to the various kinds of matter fields, and this is where the arbitrariness that ultimately leads to things like the landscape comes in. … It can easily give rise to things like multiple generations, non-abelain gauge symmetry, chiral fermions, etc. some of which were considered thorny problems before. Again, constructing PRECISELY our matter content has been a difficult problem, but progress has been ongoing. … But the most important reason for liking string theory is that it shows the features of quantum gravity that we would hope to see, in EVERY single instance that the theory is under control. Black hole entropy, gravity is holographic, resolution of singularities, resolution of information paradox – all these things have seen more or less concrete realizations in string theory. Black holes are where real progress is, according to me, but the string phenomenologists might disagree. Notice that I haven't said anything about gauge-gravity duality (AdS/CFT). Thats not because I don't think it is important, … Because it is one of those cases where two vastly different mathematical structures in theoretical physics mysteriously give rise to the exact same physics. In some sense, it is a bit like saying that understanding quantum gravity is the same problem as understanding strongly coupled QCD. I am not sure how exciting that is for a non-string person, but it makes me wax lyrical about string theory. It relates black holes and gauge theories. …. You can find a bound for the viscosity to entropy ratio of condensed matter systems, by studying black holes – thats the kind of thing that gets my juices flowing. Notice that none of these things involve far-out mathematical masturbation, this is real physics – or if you want to say it that way, it is emprically based. … String theory is a large collection of promising ideas firmly rooted in the emprirical physics we know which seems to unify theoretical physics …'

No it's not real physics because it's not tied to empirical facts. It selects an arbitary number of spatial extra dimensions in order to force the theory to give the non-falsifiable agreement with existing speculations about gravity, black holes, etc. Gravity and black holes have been observed but spin-2 gravitons and the detailed properties of black holes aren't empirically confirmed. Extra spatial dimensions and all the extra particles of supersymmetries like supergravity haven't been observed. Planck scale unification is again a speculation, not an empirical observation. The entire success of string theory is consistency with speculations, not with nature. It's built on speculations, not upon empirical facts. Further, it's not even an *ad hoc* model that can replace the Standard Model, because you can't use experimental data to identify the parameters of string theory, e.g., the moduli. It's worse therefore than *ad hoc* models, it can't incorporate let alone predict reality.

24. **somebody**
July 20, 2008

"How long should one wait for String Theory to reach maturity?"

I have no idea about this of course, but I think this question arises from the feeling that string theory is like a tunnel. You get into it on one side and the enterprise is "wasted" unless you get out the other end with predictions for electron masses or whatever. But the truth is that there is a lot of understanding+spin-offs that we have gleaned on the way, some of which I mentioned in my response to Chris W.

Another thing I have noticed is that people tend to conflate the fact that we have not managed to solve the problem FULLY, with the incorrect statement that we have not managed to solve it AT ALL. This arises because theoretical physics is such an esoteric enterprise and it is hard to give people a feel for the progress until you reach a concrete enough punctuation mark, like say, "predicting electron masses". But there has been tremendous progress in constructing string vacua which was unimaginable even ten years ago.

I just think that string theory has some great tools and ideas and it would be ridiculous not to use them when thinking about fundamental problems. For instance, holography in gravity was a very interesting observation, but nothing really concrete, before Maldacena. When Maldacena came along, connected up gauge theories with strings&gravity and broke new territory, holography actually became a useful TOOL and not merely a curiosity. These sort of insights are certainly of relevance in our way forward. To brush them all off and just stick to what we knew before strings would be counter-productive – even if you are convinced that strings are ultimately NOT a central ingredient in the construction of the universe. In particular, string theory allows you to do quantum gravity thought experiments, especially in the context of black holes, which would have been impossible without it.

"Would reaching that stage guarantee that the astronomical number of vacua will suddenly disappear or that the theory will yield testable predictions?"

If we reach that stage (if pigs had wings), we would be able to construct MODELS that yield testable predictions. The existence or non-existence of the landscape is not what makes a theory non-predictive, despite popular impression. Most theories (general relativity, QFT) have landscapes. It is not their existence, but our inability to systematically construct them that is the problem.

Or at least this is one way (the boring, frontal, difficult way) in which a solution can be realised. But there might be new ideas that can come up which might mix things up. It is only because people kept plugging away back in the days of the heterotic string (even though we had no idea how to stabilize moduli) that we have understood things about black holes and ads/cft. Right now is CERTAINLY not a worse off position than many other periods in the strange history of string theory.

Anyway, thanks for those questions, it made me think about what is important and what is not in string theory myself.

25. **somebody**
July 20, 2008

Anon.:

The problems we are trying to solve, like “quantizing gravity” are already speculative by your standards. I agree that it is a reasonable stand to brush these questions off as “speculation”. But IF you consider them worthy of your time, then string theory is a game you can play. THAT was my claim. I am sure you will agree that it is a bit unreasonable to expect a non-speculative solution to a problem that you consider already speculative.

Incidentally, I never said a word about supersymmetry and Planck scale unification in my post because it was specifically a response to a question on the empirical basis of string theory. So I would appreciate it if you read my posts before taking off on rants, stringing cliches, .. etc. It was meant for the critics of string theory who actually have scientific reasons to dislike it, and not gut-reactions.

26. **woit**
July 20, 2008

This has gotten pretty much completely off-topic, and nothing new or informative is being said. Enough.

27. **Chris W.**
July 21, 2008

Thanks, Somebody, for that response. It was helpful to see the main threads brought together that way. (And yes, I am a layman.)

28. **Paul Sagi**
September 10, 2008

my view of anthropic arguments such as the anthropic principle is that they are not scientific and have no place in physics.

it seems to me to be a priori that the existence of human beings has no role in how the universe works. the universe existed long before human beings.

that the constants of nature happen to be those that allow us to exist does not say anything, of course they are that way, otherwise we would not be observing them.

to invoke a premise that there is some grand design at work is not physics but rather religion.

real science produces testable refutable theories, the anthropic principle is not in that category.

The Emperor's Last Clothes?

July 21, 2008
Categories: Multiverse Mania

Bert Schellekens has posted on the arXiv an extended 87 page argument for the anthropic string theory landscape, entitled The Emperor's Last Clothes? While most string theorists find the existence of the landscape and the corresponding inability to get any predictions out of the theory about particle physics rather discouraging, Schellekens instead sees this as an argument in its favor:

> Initially, when string theory was touted as the "theory of everything" around 1984, there were hopes it would lead to exactly the opposite: a unique derivation of all the laws of physics. Evidence that quite the opposite was true started emerging almost immediately after 1984, but most people chose to ignore it. In 2003, after important additional evidence had been found, Leonard Susskind published a paper entitled "The Anthropic Landscape of String Theory", which finally started a debate that should have started fifteen years earlier. What is at stake in this debate is not only the uniqueness of our universe, but also the fate of string theory as a fundamental theory of all interactions.
>
> In my opinion string theory gives the right answer, and the fact that it does adds to the evidence in its favour. I can say this without being accused of trying to put a positive spin on the recent developments, because I actually wrote in 1998 that I hoped string theory would ultimately lead to a huge number of possible choices for the laws of physics, a point of view I have been advocating since the late eighties. I reached that conclusion after having been involved in one of the first papers pointing out that the number of possibilities was humongous…
>
> We all hope to live during a time when big things are happening in our field, and I have never doubted that this is one of those things. I have spent the last twenty years trying to convey my sense of excitement to my colleagues, but with little success. But in the last few years I have been delighted to see more evidence coming in supporting this point of view, so that the mood has started to change. I hope this is the right time to make one more attempt.

Schellekens describes in great detail the anthropic argument and the arguments for the string theory landscape. He addresses some of the counter-arguments, especially in three appendices. He doesn't explictly deal with the main counter-argument that I've made repeatedly here: the anthropic landscape is not science (since it is not testable), rather it is just an elaborate excuse for the failure of the speculative idea of getting the SM out of a 10/11d string/M-theory.

Schellekens has the following comments about "string phenomenology", noting that he worked in the area around 1987 and recently, finding not much has changed:

> I have been active in this are around 1987 (which led me to the conclusions presented here) and again in the last few years, and to me the similarities are more striking than the differences. There has certainly been progress: we can obtain string solutions that are more similar to the Standard Model than twenty years ago, and we have more methods to construct them. There has been major progress in moduli stabilization and supersymmetry breaking. There is more interest in "landscape statistics". But very little seems to have changed in the way many people view the problem we are facing. Although many of my string phenomenology colleagues claim that it was clear to them a long time ago that there are many solutions, I cannot help noticing that they still talk about their most recent "model" as if it would actually have a chance to be *the* Standard Model. And even nowadays one still hears the occasional expression of hope for the unknown and elusive dynamical principle that will select the vacuum. The most common way of dealing with the large vacuum degeneracy is to say "I do not care about the other 10^{500}vacua, I only care about the one that describes our universe". That may sound reasonable, and fact it may sound like the very definition of phenomenology, but it is actually an escape from reality.
>
> First of all, if indeed there are 10^{500} vacua, it is highly unlikely that anyone will find "*the* Standard Model" in string theory. One should expect to find a huge number that satisfy all current experimental constraints. In addition, although we now have many techniques at our disposal to construct string theories in four dimensions, it is quite clear that we are just scratching the surface. Statistically speaking, our chances of finding even one of the expected huge number of Standard Model realizations is essentially zero. Furthermore, even if we do find one, we can only make predictions about novel phenomena if we know *all* the other solutions and their predictions for the same phenomena. This is a crucial change in comparison to the state of the art about ten years ago: with 10^{20} solutions (the largest number anyone may have expected), if one is found that agrees with all current data, the probability that there is a second one is extremely small. With 10^{500}, the same probability is astronomical. So we should forget about the idea of finding *the* Standard Model and then making predictions based on it.

As for LHC predictions, Schellekens argues against the idea that it will see supersymmetry:

> One could say that supersymmetry is a non-solution to a non-problem: the large weak scale hierarchy is already understood anthropically, and supersymmetry by itself does not even explain it…
>
> With the start of the LHC just months away (at least, I hope so), this is more or less the last moment to make a prediction. Will low energy supersymmetry be found or not? I am convinced that without the strange coincidence of the gauge coupling convergence, many people (including myself) would bet against it. It just seems to have been hiding itself too well, and it creates the need for new fine-tunings that are not even anthropic (and hence more serious than the one supersymmetry is supposed to solve). But even if evidence for low energy supersymmetry emerges at the LHC, in the context of a landscape it will not be the explanation for the smallness of the weak scale. The explanation will in any case be anthropic. The landscape will undoubtedly allow a distribution of values for the weak scale, including values outside the anthropic window.

Schellekens ends up making the currently fashionable argument that it doesn't matter that string theory doesn't predict anything testable about particle theory, that the important thing is that it is a theory of quantum gravity:

> During the last two decades there was some reason to hope that we might be able to do that by means of some prediction of a Standard Model feature. That hope is fading now. I am not saying that this will never happen, but I have seen too much wishful thinking to make an optimistic statement about this. Essentially, we came to that conclusion already in 1986 . We are dealing with a theory of gravity. Getting information about it through the back door of particle physics is a luxury that we once had good reasons to hope for, but that may not exist. Rejecting a theory of gravity that makes no particle physics prediction may be like rejecting the theory of continental drift because it does not predict the shape of Mount Everest

He then goes on to acknowledge that we're not going to get any experimental tests out of the quantum gravity aspect of string theory either:

> One cannot count on any direct experimental check of a theory of quantum gravity, since any observable consequences it might have are extremely small, unless we are extremely lucky.

In the end, he seems to argue that the only evidence for string theory we may ever get is its consistency, something which is a very long ways from being shown. He does argue that string theory is in principle falsifiable, but the example he gives (that string theory would be wrong if coupling constants varied observably on astronomical scales) is not an uncontroversial one since other string theorists have argued that varying coupling constants would be evidence *for* string theory. There's also the usual "who knows?" argument used against anyone who points out that evidence against an idea is overwhelming:

> On longer timescales, it is clearly ridiculous to pretend that what we currently know will be the state of the art forever. When Darwin formulated his theory of

evolution he was unaware of Mendel's results on inheritance, and could not even have imagined DNA.

The truly peculiar thing about this is to see a scientist almost gleeful at the idea that a theory they have worked on their entire professional lives doesn't predict anything:

> To me, what is emerging looks very appealing. It fulfills and even exceeds the hopes I expressed in 1998. It is has been amazing to see this theory leading us in the right direction, sometimes even against the initial expectations of most of the people working on it. We should continue to follow its lead, and do everything in our power to strengthen its theoretical underpinnings. The emergence of a huge landscape" makes this more worthwhile then ever before.

Unfortunately, Schellekens is far from alone in this. At the FQXI web-site there's an article about the string theory/cosmology couple Andrei Linde and Renata Kallosh entitled A Perfect Match ("How do you tie down the physics of the multiverse? With string.") In it, Kallosh explains how "string cosmology" is now the hot topic:

> These days, in fact, collaboration be-tween string people and cosmology people is all the rage.
> "To give you a funny example, I had an invitation to give a talk at the Strings 2008 conference at CERN," Kallosh says. "The way the invitation was writ-ten was, 'Of course you are welcome to speak about any topic . . . but we would be very happy if you would give us a mini-review on string cosmology!'"
>
> Suddenly, everyone is interested in their kind of union.
>
> "I'm also working on other very formal, very stringy topics, which were always part of my skills," Kallosh says. "But, at this moment, people want to know about string cosmology. I'm happily working on it . . . with Andrei's help."

Cosmology is now ascendant, with Kallosh arguing that it will be needed to explain what is seen at the LHC:

> "Soon the LHC will start giving new information on particle physics," she says. "But we know it will be difficult to interpret this data unless you also can digest all the data from the sky—all the observations from astrophysics and cosmology."

The article ends with a large picture of one of the LHC detectors, captioned "Cradle of Collaboration: Will the LHC provide evidence for string theory?"

FQXI is funded by the Templeton Foundation, the goal of which is to bring science and religion together. Cormac O'Raifertaigh is at another Templeton funded event, a conference in Cambridge on From the Big Bang to the Brain: Current Issues in Science and Religion. This Wednesday will be devoted to cosmology, featuring talks on the anthropic principle, fine-tuning, God and time, and God and the Big Bang.

For another take on cosmology, this October the ENS in Paris will host a conference on Evolution and Development of the Universe. For more about this parallel universe of cosmologists who also study anthropics and the multiverse , see EvoDevoUniverse.

Comments

1. **Christine**
   July 22, 2008

   Sure, it is natural to accept that any mathematically consistent theory of quantum gravity can be extremely difficult to be tested experimentally. But at the basis of any expectations like this lies the specific, unambiguous predictions of the theory, whatever the difficulty in observing them. The latter does not preclude the former, which is actually indispensable for any theory.

   The obvious point here is one of posture. The landscape does not represent different mathematical descriptions of the same physical content (something that could be manageable by, e.g., extracting the observables or invariants of the transformations between the various solutions) — a common situation in many physical theories –, but a set of 10^500 independent solutions, representing different physical realities, in which our universe is supposed to be one of them. The acceptance of this state of affairs indicates a strong epistemic attitude in the sense that one embraces a substantivalism point of view towards the landscape, in the sense of assigning an independent physical reality for each of these vaccua, but is not willing to consider the possibility that this indicates a failure that needs revision.

   Such epistemic attitude is acceptable if one proves that it does not actually break any logical reasoning or consistency, or is justifiable or even needed at some point, or under some physical grounds. I can accept such a posture in the sense that it is just part of learning, exploring a difficult subject, etc. But what I cannot easily accept is the lack of a critical reasoning on facing the consequences that result from the position taken, to the degree that one is using the scientific method as a guide and not dismissing it or deforming it in order to fit the consequences of one's personal attraction for the theory.

   I see all this as a crisis and do not understand how can one believe that the situation is an indication of progress, except if one is really inclined to redefine what science is, which is of course, the easiest route (to go nowhere).

2. **Christine**
   July 22, 2008

   Just a question: why is the paper authored by "A.N. Schellekens" and posted by "Bert Schellekens"?

3. **Peter Woit**
   July 22, 2008

   Christine,

   Schellekens goes by the name "Bert", I'm guessing the "A" is for "Albert"

4. **Christine**
   July 22, 2008

   Right.

   At his old homepage, he writes:

   > Research interests
   >
   > String Theory, whatever that is

   🙂

But seriously, it is quite a fair way to put it, valid for many areas…

5. **Jim Clarage**
July 22, 2008

I don't see why String Theory per se is responsible for this "remarkable paradigm shift in particle physics" which Schellekens refers to in his abstract. That is it's unclear how the shift towards anthropic reasoning follows logically, or inevitably, from the principles of string theories. Rather it is a philosophical shift, one which could have happened at any time in the history of physics.

For example Newton could have easily adopted an anthropic approach to his inverse square law, perhaps assuming any value for the exponents in the radial and mass coordinates of the gravitating objects; and then agreeing to anthropically derive these exponents based upon e.g., what values allow and disallow stability of human bones, large tree trunks, or other mechanical properties of organisms living under the influence of Earth's known gravitational field. Nothing in this alternative history is inconceivable, especially considering Newton himself had no trouble mentioning God and Creator in his work.

However, instead of appealing to anthros Newton appealed to physics. He tried to pick the exponents and form of the law based upon reproducing previous physical laws, viz., Kepler's laws. Replace Kepler's laws with Standard Model and I think it is clear what is occuring is a philosphical shift, not a shift in physics.

6. **Chris W.**
July 22, 2008

Right, Jim. Ironically enough, it is a philosophical shift in a field that, for the most part, eschews and dismisses overt philosophical reflection; philosophy is just a lame attempt to get at the truth by people who lack the chops to do science. From this viewpoint, having the chops has come to be what matters, and the situation celebrated by Bert Schellekens provides unlimited scope for exercising one's chops. That seems to be the source of the peculiar glee noted by Peter.

7. **Mitch Miller**
July 22, 2008

Is Schkelens view on the history of the multi-verse standard? I was under the impression that Witten and Gross had technical arguments as late as the early 2000s on how it was possible that there may only be a few string theory vacumns. I may be wrong as I am only an undergrad and have not been following this as long as everybody else.

8. **manyoso**
July 22, 2008

Can it be mathematically or logically demonstrated that every one of the experimental results known to the Standard Model can be produced by even one of the unique 10^500 vacua?

Schellekens states that there are likely an astronomical amount of the 10^500 vacua that would be in agreement with the Standard Model. How does he determine this? I mean does there exist a mathematical proof that at least one of these vacua would give the Standard Model?

I mean if we can't find the Standard Model in the haystack that is the landscape, what reasons do String Theorists have to believe that the Standard Model is even *in* the haystack other than faith in the flexibility of String Theory?

9. **Peter Woit**
July 22, 2008

Mitch,

Schellekens has a view of the history of the string theory landscape very much oriented around his claims to have realized its importance before just about anyone else. The more standard history is that the "string vacua" known from the mid-80s on did not have stabilized moduli, so most people hoped that once one figured out how to stabilize moduli, one would end up with a small number of vacua. It was only with KKLT that exponentially large numbers of (presumably) stabilized vacua were constructed. I don't know of any claims by Gross or Witten to have an argument pointing to a small number of vacua.

manyoso,

No, it can't now be demonstrated that you can get the the SM out of known constructions in the landscape. Many string theorists like Schellekens now argue that the efforts of theorists should be devoted to studying the landscape to see if it can be proved that the SM is there.

Personally I don't see the point of this, and Schellekens even gives some of the arguments why. Even if you prove the SM is somewhere in there, this doesn't allow you to predict anything or test the theory. If you prove the SM cannot come from known string vacua constructions, there's no reason it can't come from still unknown ones. People keep coming up with more all the time, there's nothing like an understanding of what "all" string vacua are like. But working on this is an extremely complex problem that could keep an exponentially large number of people busy an exponentially large amount of time, some seem to want physics to go this way.

10. **manyoso**
July 22, 2008

Peter,

If that is true that it has not been proven that the SM is somewhere in the 10^500 "known" vacua, then it seems impossibly arrogant for Schellekens and other landscape proponents to declare anything like 'success' with anthropic arguments.

How can anyone argue for anthropic origins for the Standard Model via String Theory when it can't even be demonstrated that the Standard Model is represented in the landscape. Not even theoretically! It seems that landscape proponents are arguing that because we now have hints at an absolutely *huge* number of stringy universes… our universe just must be in there somewhere. For no other reason than blind faith.

Just awesomely odd.

11. **Peter Woit**
July 22, 2008

manyoso,

To defend the landscapers, from what is known about the string theory landscape, it is plausible that there are "string vacua" that reproduce the SM at observable energie. They would point out that showing that such a thing is really there is work in progress.

I don't think the problem is that they are advertising something that is not likely to be there. The problem is that even if it is there, it's completely useless.

12. **Richard**
July 22, 2008

"In my opinion string theory gives the right answer, and the fact that it does adds to the evidence in its favour."

Huh? It's hard to tell for sure what was meant by this comment, but doesn't this sound a bit circular?

13. **Esornep**
July 22, 2008

Penrose argues that the initial condtions of *our* universe are fine-tuned to one part in 10^(10^(123)). How are you going to get fine-tuning to that extent if you only have a miserable 10^(1500) attempts?

14. **mike**
July 23, 2008

"Soon the LHC will start giving new information on particle physics," she says. "But we know it will be difficult to interpret this data unless you also can digest all the data from the sky—all the observations from astrophysics and cosmology."

Shouldn't this be the other way around – do astrophysicists tell particle physicists how matter works? Maybe it's just the experimentalist in me, but I believe the machine in front of me before I believe something we interpret from afar.

15. **F Borg**
July 23, 2008

Shellekens should be acknowledged for his attempt at presenting his points in a clear and accessible form. It very much centers around the old issue on what to expect of a "fundamental theory" in physics. Should such a theory e.g. fix all the 28 parameters or so of the Standard Model (as an effective low energy approximation)? If so would such a "fundamental theory" just replace one mystery by another mystery (remember Eddington and 137)? Maybe in a more encompassing theory one can find a mechanism that drives the parameters toward the region of the observed values, but it is conceivable that such a mechanism will rest on new "ad hoc" principles. Another option is to think that "the fundamental theory" will only be about the main structures and equations ("the grammar of nature", Souriau) without singling out any unique solutions. The general assumption has been that unification will lead to "uniqueness" but it is possible that instead the space of solutions is enlarged with no recipe for picking the right one that will correspond to our observations. (Shellekens denies that e.g. "beauty" and "simplicity" will be useful guides.) I think quite a few physicists believe that we have potentially an infinite ladder of effective theories that never bottom out despite the apparently absolute scale set by Planck energy, but for "practical reason" we need only work up/down to a some level of this ladder that applies to the world (or part thereof) which we are able to observe. Against this background one may understand the optimism by those who think the stringy landscape demonstrates that there is indeed a bottoming out instead of an infinity of "onion layers". Although the "landscape" may preclude sharp predictions (and thus "verification" in a "traditional sense") it at least may show the basic "fabrics" (or the "template") that reality is built of, according to the adherents. This is how I interpret the "optimism".

16. **Christine**
July 23, 2008

F Borg wrote:

Although the "landscape" may preclude sharp predictions (and thus "verification" in a "traditional sense") it at least may show the basic "fabrics" (or the "template") that reality is built of, according to the adherents.

I think you have summarized all very well. But suppose that it is really our fate that our fundamental theories can never be ultimately tested (through "verification in a traditional sense"), so that what we have at the end will always be some layout, some "basic fabrics that reality is built of". How will we know that even this is true?

17. **Chris W.**
July 23, 2008

Christine is exactly right. There is massive presumption in supposing an allegedly empirical assertion has been "shown" to be true, when we have no way of saying, *on the basis of observation*, how we would ever know that it is false. This describes the essence of string theory's lack of testability.

That is, we can't say this except in a sort of lame and parasitic way, eg, that string theory would be wrong if Lorentz invariance at low energies is wrong. Of course, even on this point bets can and have been hedged, even though hardly anyone is seriously expecting the issue to arise.

18. **Professor R**
July 23, 2008

Thanks for the mention Peter!

Today lived up to expectations – Prof Paul Shelton , a colleague of Stephen Hawking at DAMTP, Cambridge, gave a spectacular overview of today's cosmology with a thorough review of inflation, eternal inflation, the multiverse and the landscape. Shelton went through the WMAP evidence, explaining that the evidence for some sort of inflation was very strong, but the mechanism under-determined (his word, glorious understatement -he showed a hilarious slide listing all possible flavours of inflation)…

Shellard's discussion of eternal inflation and the multiverse was thorough but accessible, with emphasis on the viewpoint that this may be the price we have to pay for the success of inflation (in explaining the standard BB riddles), yet emphasising also the speculative nature of the multiverse idea…

If this weren't enough, the high point of the day for many was 'Meta-stories of Fine-tuning ' by Sir John Polkinghorne. In typical fashion, Sir John gave a succinct overview of the fine-tuning problem from a philosophical viewpoint. In particular, he focussed on a choice between the multiverse explanation and the anthropic principle from a philosophical perspective. He was clearly unimpressed with the theory of the multiverse, describing it as meta-physics and probably contrary to the principle of Occam's razor. Calling on Lewis's famous example of the firing squad, he suggested that it was a little excessive to suggest that the firing squad engaged in a gigantic number of shootings in order to explain a miss – suggesting that it was more likely that they simply missed by design…

He was also unimpressed with a third possibility (from me) that an unlikely outcome – however unlikely – can simply occur without the need for an explanation. I can't do justice to John's persuasive arguments here, but you can get the DVD of the talk here.

Peter Woit of NOT EVEN WRONG would be pleased to note that, along the way, Sir John gave a very terse overview of the opinion of his generation of particle physicists of string theory – not very high!.

19. **Ralph**
July 24, 2008

Re:

"it doesn't matter that string theory doesn't predict anything testable about particle theory, that the important thing is that it is a theory of quantum gravity"

I think this idea can be read in a sensible manner, but it still doesn't help string theory.

Quantum gravity has purely mathematical problems: e.g., giving any completely rigorous, mathematical theory incorporating both gravity and the SM would be a huge advance (You get a Clay prize for just the 'SM' bit of that, more-or-less?), and physical testability is not a part of that mathematical problem.

That changes the issue from judging string theory by physicist's standards to judging it by mathematical standards.

And string theory is just as badly deficient from mathematical standards as it is from physicist's standards – the parts of string theory that are even vaguely plausible as ways of getting gravity+SM aren't anywhere near mathematically rigorous.

20. **Bee**
July 24, 2008

I was very puzzled by the abstract of this paper, I haven't noticed any paradigm shift. Possibly the majority of my friends and colleagues is just more down to earth than the average string theorist but I never had the impression anybody seriously believed one would find THE fundamental theory of everything and that was that. There's no paradigm shifting, if anything, people who believed that are recovering their sanity.

That's not quite what Schellekens was aiming at with THE fundamental theory, but I hope the present confusion is only a temporary stage and eventually people will realize physics is about describing nature and not equal to philosophy. There's enough topics people could do real work on.

I was just checking the arxiv today and I find hep-th an increasingly depressing collection of highly specialized detail investigations that in almost all cases will remain absolutely irrelevant for actual descriptions of nature.

21. **Yatima**
July 26, 2008

For those interested in further thinking/reading about the Anthropic Principle, Scott Aaronson (He of the Complexity Theory) has published some lecture notes on the same:

PHYS771 Lecture 17: Fun With the Anthropic Principle

as referenced from his recent blog entry

22. **Arun**
July 26, 2008

Just as it took a long while and effort to get to "N=8 supergravity might be finite" and will likely take a long time (if at all) to get to "N=8 supergravity is indeed finite", it will likely take equivalent effort and time to get to the next step in superstring theory. Maybe the era of easy surprises is over. The anthropic ideas make such effort to seem pointless however. It will be interesting to see if the field can avoid decaying entirely.

23. **Gphillip**
August 11, 2008

This all reminds me of the story of the madman who had created a perfectly mad, but perfectly logical reasoning behind his paranoia. His story was so creative, logically sound and finely interwoven that even his doctors found themselves looking over their shoulders from time to time. In the end, the doctors were never able to demonstrate the error of the logic to the madman, because they couldn't find any. All they could say was his view of reality couldn't be correct because it was totally different from that which all sane people agreed upon.

Gauge Theory and Langlands Duality

July 21, 2008
Categories: Langlands

At the KITP in Santa Barbara there's a wonderful program on Gauge Theory and Langlands Duality starting up this week, with some of the talks beginning to become available. The main topic will be the relations between S-duality in quantum field theory and geometric Langlands duality that Witten and collaborators have been working on the past few years.

I started trying to watch the talks, but the fact that the video quality is such that one can almost but not quite tell what is being written on the blackboard makes this a bit of a trial. I'm hoping that David Ben-Zvi or someone else will make available notes, which would help a lot. I did very much like Edward Frenkel's description of the Langlands story as a "Grand Unified Theory of Mathematics", and was interested to hear that he still feels that there are two different stories about the relation to QFT here, whose relationship is not at all understood (S-duality in 4d QFT is one, 2d CFT and vertex algebras is the other). It seems that A.J. Tolland is there, maybe he or someone else will do some blogging. As I get time to take in the lectures, I hope to write some more about them here.

Update: Notes for the talks are now also being posted, making following them on-line much more feasible. The quality of the talks is excellent, with Ed Frenkel so far giving a beautiful introduction to the roots of the Langlands program in number theory, David Ben-Zvi explaining the structures in topological quantum field theory that mathematicians are trying to exploit, David Morrison and Paul Aspinwall explaining mirror symmetry, D-branes, and the relation to N=(2,2) superconformal field theory, with examples, and Anton Kapustin starting on the 4d N=4 TQFT used to turn S-duality into a mirror symmetry.

Comments

1. **Daniel de França MTd2**
   July 22, 2008

   Hi Peter,

   there is a funny buzzword sometimes when I read something of witten's most recent words…

   What is wild ramification?

2. **Phil**
   July 22, 2008

   Daniel,

   In the context of geometric Langlands, \`wild ramification' refers to the case when the connections or Higgs fields are allowed to have \`irregular singularities'. Basically this means they can have poles of order two or more. Heuristically having a meromorphic connection with simple poles on a curve is akin to having punctured the curve at the poles, whereas having higher order poles is more complicated. The word "wild" arises from an analogy with wild ramification (of maps between curves) in characteristic p. One can also look at the growth of horizontal sections approaching such a pole: with simple poles such sections have polynomial growth ("tame") whereas with higher order poles one can get exponential growth (or decay) as the pole is approached in different directions.

   Phil

3. **Daniel de França MTd2**
   July 23, 2008

   Thank you very much.

   Do you know any source that introduces "wild ramification"?

   I am also curious about "Langlands conjectures for GLn of a function field". What is that? Why is that so important? I found this, but I would want something more complete and with applications

   http://arxiv.org/PS_cache/math/pdf/0212/0212417v1.pdf

4. **Peter Woit**
   July 23, 2008

   Daniel,

   For more about "wild ramification" in the geometric context, see references in Witten's paper with that in the title (what is at issue are "irregular connections").

   To answer your question about Langlands conjecture would be a major effort. Best source I can recommend is expository articles by Edward Frenkel, as well as the talks he is giving now at the KITP, where he is addressing exactly the topic you are asking about.

5. **Kea**
   July 24, 2008

   Opening the pdf notes by Frenkel and then listening to the audio works well.

6. **Kea2**
   July 24, 2008

   Grand unified theory of nothing.
   If you want to see what mathematicians think is important in mathematics, you should look at the Millenium Problems. Except possibly for the one on Yang-Mills theory, the geometric analogue of Langlands theory has nothing to say about any of them. The Langlands program is very profound with deep arithmetic content. In comparison, its geometric analogue appears to be more superficial and without arithmetic interest.

7. **A.J. Tolland**
   July 24, 2008

If you want to see what mathematicians think is important in mathematics, you should look at the ~~Millenium Problems~~ Fields Medals.

8. **Peter Woit**
July 24, 2008

It's true that as far as I know geometric Langlands has not led to any new insight into the arithmetic case, and because of this some mathematicians are skeptics. But there are quite a few extremely good mathematicians working on it, and the field does have all sorts of connections to deep questions in mathematics, as well as the connections to quantum field theory and physics. Personally I think it's definitely fascinating and still mostly unexplored territory, and may turn out to be one of the great themes of 21st century mathematics. Time will tell….

9. **David Ben-Zvi**
July 24, 2008

Of course Frenkel's comment on the "grand unified theory" is tongue in cheek, but in any case he was referring to the entire Langlands program, not just to its geometric aspects.

Also the claims about the disconnect with arithmetic are not very accurate. First the Langlands program for function fields has profound connections both with the arithmetic and the geometric Langlands programs.
And most spectacularly, Ngo Bao-Chau has used ideas originating from the geometric setting (in fact from the study of Yang-Mills!) – namely the Hitchin system – to resolve one of the major outstanding conjectures in the arithmetic setting, the so-called "Fundamental Lemma", which has numerous immediate number-theoretic consequences (work which to many suggests he should be honored with a Fields medal).

The geometric Langlands program itself doesn't claim to the profundity and impact of the arithmetic conjectures (few areas in math do), but it has had spectacular successes, for example the work of Bezrukavnikov and collaborators resolving various conjectures of Lusztig on the deep relations between quantum groups, loop groups and algebraic groups over finite fields, one of the central problems in representation theory.

10. **onlooker**
July 24, 2008

As the term "geometric Langlands" should suggest, a cardinal motivation for the development of the theory by Drinfeld and Beilinson was to apply and refine the analogies between number fields and function fields. A big part of the issue is learning how to properly formulate and generalize the picture in the arithmetic setting and the geometric picture is very valuable for that. Hitchin fibrations are presumed to be the first of many examples of this.

11. **Kea**
July 24, 2008

Putting the mathematical worth of geometric Langlands aside, it seems to me that the Physics is actually closer to the arithmetic and finite field channels anyway. By Physics I mean the science that actually has some bearing on testable predictions in the real world.

And Kea2? Cute.

Blogging Heads Science Saturday

July 26, 2008
Categories: Uncategorized

Today's "Science Saturday" on Bloggingheads features me and Sabine Hossenfelder, supposedly talking about What's wrong with string theory. Actually, we both agreed that we were pretty tired of that topic, so tried to discuss some more interesting related issues we both have an interest in. Here's a clip from the full thing, I promise to not start regularly embedding video in this blog:

I hope this thing came out all right. It was recorded a couple weeks ago, in a process involving no trouble on my end, but heroic efforts on Sabine's. While Sabine had to set the whole thing up on her end, and ended up crouched in an attic since it was the only place she could get a connection of good enough quality, I just sat at my office chair and someone from Bloggingheads took care of everything. Unfortunately we couldn't see each other while talking. I can't really bear the thought of watching myself on video, so I guess I'll never see exactly how this turned out, but I'm glad to see that the turtles on the bookcase behind me made it into the frame.

Sabine has her own posting about this here, and the full thing is here.

Comments

1. **Bee**
   July 26, 2008

   I just made the heroic effort of indeed watching the whole thing again! It is actually quite good, we managed to cover a lot of interesting topics. I was as well somewhat confused by the title the bloggingheaders chose.

2. **mike**
   July 26, 2008

   Peter,

   This is a great video blog but I have a few comments regarding "the market place of ideas". First, any marketplace of ideas as I see it (like an economic marketplace) is an open market – open to exchanges by any and all individuals like the free market that we currently barter in for goods and services. This is almost universally seen as the reason free markets are successful – they are "open" to exchanges by many without much intervention on control (although we understand that some intervention may be necessary). To exclude the public from the string theory debate seems counter this market philosphy, as it is the public that seems decides our fate in terms of economics and political process. The beauty of our system in this and other free countries is that regardless of how bad a product is, or how limited the intelligence of a political candidate, the system manages to correct itself through some magical feedback loop (bad product –> unhappy customer –> product is not purchased and company goes bankrupt). This system is probably what keeps us a float even when the most limited intelligences occupy positions of power in our government or economic system.

   This being said, I believe that the public should (and always will – we have no choice in a free market) be involved in the string theory debate. The last time we may have had a true vacuum in science was when a certain oligarchy taught and enforced without debate to a select few ideas of about an earth-centered universe. We have fortunately moved away from this, but like all dynamical systems that we rely on, cycles are the inevitable result and we are perhaps in a down cycle in terms of the "idea marketplace". The solution to getting out of this down cycle, as the government well knows, is to stimulate the economy, and for ideas it's the same. We stimulate the "idea economy" by involving the public, and earnestly hope as we do every election year that the right idea or candidate floats to the top of the pile over some time. As part of that process, we have to tolerate the ideas we don't like and because that feedback loop that finds a happy medium for all of us, in the end things seem to get better for everybody (GDP in this country is still pretty good).

   Therefore, I don't think there is anything to be feared about a public debate – the lack of understanding of QFT or string theory is irrelevant. Ideas that survive the market place rise to the top, ideas that don't sink to the bottom. In the end, the public becomes more educated (which is more than they might get in public schools in some cases) and it's better to have a somewhat educated public voting on science funding than a non-educated public doing the same. In fact, it seems like there is little input from the public on science funding and the more interested they get in the debate, the more likely they are to let congress know about it. Vacuum's of knowledge never kept theories alive nor did they stimulate growth. Only open debate – not just academic debate (which as you both mentioned doesn't seem to be happening anyway) but open public debate, can bring us to an understanding of the truth.

3. **Bee**
   July 26, 2008

   Hi Mike,

   I have written two long posts on the topic that you touch: We have only ourselves to judge on each other and The Marketplace of Ideas that I recommend you read. What you miss is the following:

   a) Especially in theoretical physics, there is potentially a long time in which there is no evidence (in form of observation or experiment) to judge on the promise of a research program.

   b) During this time then when judgement by Nature is still absent, judgment is left to experts in peer review – which is thus essential for ideas to survive and grow to adulthood. The public opinion should play no role in this, this isn't the way science works. The public opinion is relevant to the question what fields are generally of importance for our societies and how money should be invested to be as beneficial as possible, but the public never 'votes' on research funding on the specialist level. There is a reason why funding agencies have an elaborate referee process.

   There is a priori no problem with leading these discussions publicly, as long as this does not influence the truth finding process. But the danger of such influence when people worry about their funding is very obviously there. It should also give us a lot to think that apparently in the case of string theory it was not possible to lead this discussion without exerting public pressure (or possibly it wasn't tried hard enough?). This is my opinion shows that the judgement process on our own 'Marketplace of Ideas' works very insufficiently.

   Best,

   B.

4. **Peter Woit**
   July 26, 2008

Mike,

I think I, like Sabine, have mixed feelings about this whole "marketplace of ideas" business. What I've seen in particle theory over the past 20 years has been a massive failure in how that particular marketplace is supposed to work, and that was one of the main concerns of my book and Lee Smolin's.

There are different marketplaces at work here, an internal marketplace inside string theory, where different proposals within string theory battle it out, a marketplace within particle theory, where the traditional lines are phenomenologist vs. string theorists (although with many trying to cross those lines to gain marketplace advantage) and the marketplace within physics departments, where decisions are make about who to hire. This is a relatively closed system, and I don't think the public has much of a role to play there, since good decisions require significant technical expertise.

There's also a public marketplace of scientific ideas, and string theory has traditionally been extremely successful there, due to the impressive public relations efforts of some of its proponents. This has changed the last few years, with the public seeing a much more balanced range of views, and string theory losing market share. String theorists are naturally upset about this now, while they were relatively pleased with how things were going back when string theory was being heavily promoted, and its problems pretty much ignored. When my book and Smolin's first came out, one frequent claim made was that the books should be ignored, since string theory had already triumphed in the marketplace of ideas. The books were very much an effort to engage in this marketplace, and I think did so quite successfully.

The relation between these two marketplaces is a complicated one, and that's something Sabine was repeatedly asking me about. One of her questions was why I chose to enter the public marketplace, as opposed to sticking to the professional marketplace. One answer is that the book was intended to address both, aimed at both professional physicists and mathematicians (who generally know very little about what the state of string theory is), as well as the public. Both marketplaces needed a countervailing force to the promotional effort that has been backing string theory.

The public marketplace affects the professional one, as ideas that get public attention get more funding from various sources and more support from university administrations. I wouldn't want this public attention to determine the fate of a scientific research program, but some influence is unavoidable. In the past string theory was probably better funded and supported because of the promotion to the public. The professional marketplace affects the public one too, largely through the popular science press, which observes the professional marketplace, tries to evaluate what is new and how things are going in a science, and then transmits some understanding of this to the public. Part of the most serious problem for string theorists now is that a sizable and vocal contingent amongst them has taken to promoting a research program (landscape anthropics), which can be readily explained both to their colleagues and to the public (anthropic papers tend to have few equations…), but most people recognize pseudo-science when they see if. If you're not a string theory ideolog, this stuff looks extremely dubious.

5. **Roger Schlafly**
   July 26, 2008

   I am watching as I write this, and I was surprised to hear you agree with her statement that it is bad to make arguments about String Theory to the general public, because it polarizes people. What does that mean — that you regret writing the book? That laymen should not know about Physics controversies? That it is bad to express polarizing opinions? C'mon, you ought to defend yourself better than that.

6. **mike**
   July 26, 2008

   Peter,

   I also think that both your book and Lee Smolin's did well in countering the string theory promotion that went on so long in the public – they rose to a high level of public opinion with no tangible evidence to show. Both books did a good job in countering string theory claims of TOE and I think the public market place was a better venue to have this discussion than professional journals because there appeared to be a bias in the journals towards "the new trend" without giving it much scrutiny. It's always good to get a public sanity check once in a while – it's not that I think the public will help comment immediately on formulas or theories, but just that they act as a sounding board and "ethical referee" check for professional scientists who engage in a debate where the cards are clearly stacked on one side. It's kind of like a jury of one's peers – they aren't supposed to be experts in forensics or other specialties because a bias may exist – but they still are presented with highly technical facts in which they use to make a decision. In the end, it leads to a rather unbiased opinion (the fact that lawyers trust the average person on the street to make a decision about medical evidence is a fascinating concept). Perhaps this is the advantage of science in the public – just to keep what we know as science and the scientific method going and to call pseudoscience what it is. That being said, I am definitely not in favor of landscape anthropics which appears to be epicycles all over again (but with more advanced mathematics). I think the sooner these concepts of "anything is possible" are examined in the public view, the faster they will go away.

7. **Bee**
   July 26, 2008

   Hi Roger,

   I did not say it is bad to make arguments about string theory (or any other scientific topic for that matter) to the public. I said it is of great concern this was necessary, since doing so has predictable drawbacks eg, as you say, in that it polarizes people. Uncertainty is an elementary ingredient of science, but there seems to be no place for this in public discussions which usually forces people to take either side, leaving out detailed nuances. This is not a good environment in which to discuss whether the present research directions are pursued in a balanced way, and if not what the reason is for this. Instead, it will prompt a food fight. Best,

   B.

8. **Peter Woit**
   July 26, 2008

   Roger,

   I'm obviously not opposed to communicating with the general public about string theory and its problems, since I've done a lot of it. I think that exchange was more about how it was a shame that discussion of the problems of string theory within the theoretical physics community was often suppressed (string theorist referees stopped publication of an earlier, less popular, version of my book by a university press), with the result that it ended up taking place in public forums not necessarily known for civility or a high standard of informed professional discourse.

9. **mike**
   July 26, 2008

   Hi Bee,

   I definitely don't advocate that the public take over the technical referee process – there's obvious reasons why this wouldn't work, but it appears at times (like when Peter submitted an earlier book to the University press that got trounced by string theorists bent on keeping their shrine sacred) that professional peer-reviewers stop acting professionally when they let their personal opinions get involved of what should be an unbiased observation of facts. During these times it seems many who feel the peer review process is biased seek the public domain and in the case of string theory this rightly seemed to correct the problem. I don't believe this is because the public decided string theory was bad, but I believe it because the embarrasment of potentially being proven wrong in the public domain about a theory is much worse than anything that happens in the peer review process and therefore has the effect of correcting peer-review so it becomes less biased and more effective.

This also affects funding to a certain extent as you mentioned in one of your posts about the funding for string theory starting to dry up. What is popular does get funded and I agree with you that this is a real problem. Perhaps some funding should be set aside for "fringe theories" so that it's not always "right or wrong" to fund something. This is already taking place, like Naval research funding cold fusion well into the last decade, or NASA funding the blacklight propulsion project a few years ago. These funding sources take it upon themselves to ensure the research gets done even if it's unpopular. I sometimes take issue with funding some of these projects but as in a democratic system, I believe we have to tolerate some things we may not like so we can meet common ground and create a better environment for research overall.

I agree with your ideas about what happens in the market – it's not a "pure process" and I don't think the market place is the ideal place to bring about change in science, but sometimes it may be necessary if the peer-review process isn't working as it should (which then hopefully gets corrected). In mathematic terms, the relationship between science and the public seems to be non-linearly dependent.

Best regards,

Mike

10. **Roger Schlafly**
July 27, 2008

What is the problem with "public forums not necessarily known for civility"? You mean that if you denigrate String Theory, then Lubos Motl will write nasty things about you on his blog? He is apt to do that whether you write for the general public or not. You don't need to give excuses for writing for the general public.

11. **somebody**
July 27, 2008

The reason why only the public debate has gotten any traction, and why the attitude of the particle/gravity community to string theory is the same before and after the books, is simple: there were no new scientific ideas in either book that was not already well known.

Woit and Smolin did a good job of counterbalancing the Kaku style nonsense that was being promoted to the public as string theory. Many string theorists (eg: myself) would have liked nothing better. But the relative merits and difficulties of string theory were well known to string theorists before the books, and the books did nothing to affect that scientific judgement.

Also, discussions even about well-known things usually get caught in ruts – the typical example is that of the landscape. The oft-repeated claim that "having many vacua which look like ours at low energies kills predictability" is simply an incorrect one, and no sensible discussion about the landscape is possible when this sort of factual errors are not acknowledged. What could potentially kill predictability is the difficulty of constructing vacua, and it is not easy to find the motivtion to debate against (for example) the comment thread on Schellekens' article below.

12. **John Brock**
July 27, 2008

I really don't understand the problem some people have with the Multiverse. To me the idea seems both plausible and very satisfying. If the Earth were the only planet that existed, the philosophical problem of coincidences would be exceedingly difficult to deal with. But the minute you realize that there are many planets the problem simply disappears, and I don't see anything at all upsetting about the way it disappears, rather it seems to me that it disappears in the best possible way.

Likewise, the Multiverse hypothesis makes the (very serious!) problem of cosmic coincidences disappear in the best possible way. The complaints about it all strike me as the equivalent of being unhappy with the Multiplanet hypothesis because it means that you will never be able to derive the distance from the Earth to the Sun from first principles. Well no, in actual reality you can't, and that's just the way it is. And isn't it satisfying to discover why you don't have to?

13. **groan**
July 27, 2008

(@ John Brock)
… and you can argue for anything in a Multi-Strawman post.

14. **Eric**
July 27, 2008

The thing that you must keep in mind is that it is in fact possible that the anthropic landscape could be the correct description of reality. In this case, then it would be utterly impossible to make unique predictions, although this is not the case within the confines of specific vacua. This just may be a fundamental limitation of reality, just as it's impossible to simultaneously measure position and momentum. So, you can criticize string theory because of this, but if this is the true reality then we have no choice but to accept it. Is it the true reality? Nobody can really say at the moment, and so more work must be done.

15. **woit**
July 27, 2008

somebody,

Schellekens is the one making the claims about predictivity that you say are in error. Instead of arguing with my often ill-informed commenters, you should e-mail him and demand that he retract his paper from the arXiv since it is wrong.

I don't think it's just the public debate about string theory that has gotten traction, so has the professional debate within the physics community. I find that the way physicists in general think about string theory is quite different now than it was 5 years ago. Probably one of the main reasons for that is not my book and Smolin's, but the public relations efforts of some string theorists themselves. Lubos and Lenny Susskind have done more to discredit the subject than I ever could. The one place where there hasn't been anywhere near as much of a serious debate as there should be is within the string theory community itself. People there would be well advised to start acknowledging the problems they are facing and engaging in serious discussion about what to do about them.

John Brock and Eric,

For about the 100th time, the problem with the multiverse is not that it is logically impossible or can't be used to solve a philosophical problem. The problem is that it's not an idea that can be tested and so isn't science.

16. **John Brock**
July 27, 2008

Woit,

I'm sure this is probably the 1000th time, because it is such an obvious point, but although I've done some graduate work in physics (decades ago) I'm nowhere near the level of you guys, and I am just trying to understand.

My problem is that when you say it "isn't science," it sounds a lot like you are asserting that this is a *problem* with String Theory, and I just don't understand what the problem is.

Let me put it this way, suppose that tomorrow I were to resolve all the technical issues with String Theory, and I handed you a well defined and consistent theory which demonstrably included our own universe, but which also included a huge number of alternative universes. What would you do with this theory? Would you reject it, because it isn't testable, and insist we continue looking for a theory that could be tested, even though such a theory might not exist? That's what seems to be implied by equating "not testable" with "not science."

And why would you even want to keep on looking? The point I was trying to make in my post that if a theory happens to yield a Multiverse that should be seen as a bonus, not a negative, since it does after all resolve what is otherwise a difficult philosophical problem. You don't seem to agree that a Multiverse is a good thing to come out of a theory, and that is basically what I'd like to understand.

17. **somebdoy**
July 27, 2008

"Schellekens is the one making the claims about predictivity that you say are in error."

No Peter, his argument is perfectly clear. He wants to go anthropic because he doesn't know how to systematically construct vacua. That is his only real argument. Take away the difficulty to construct vacua, and he has no argument.

But your claim is stronger – that string theory is INHERENTLY non-predictive because of the landscape. What I am trying to say is that if we had infinite powers of model-building there is absolutely no philosophical difference between QFT and string theory. So it is a logically defensible stand to try to get string theory under better control so we can build models.

Anyway, I have some marginal disagreements with Eric as well. The only way in which I can make sense of his claim is by the statement that there might be many ways to UV complete our low energy physics. I don't see why this is bad for making predictions. Because at any energy scale, all I can even in principle hope for, is to identify equivalence classes of theories which could potentially differ from each other at higher scales. We will be able to resolve the different models as we probe higher and higher energies. Again, the thing we really need is control on string theory.

18. **Robert**
July 27, 2008

"Lubos and Lenny Susskind have done more to discredit the subject than I ever could"

I really doubt that, since, Lubos keeps giving clear and patient expositions of the central tenets of string theory on his blog, and Lenny has done string theory a big service by drawing the inescapable discretuum of vacua to the center of the debate. Even if one doesnt like that idea, it doesnt help to ignore it, as mother told us.

19. **woit**
July 27, 2008

somebody,

Schellekens (see the paragraph I quoted starting "First of all") is very explicitly claiming lack of predictivity, based on what is currently known of string vacua constructions. Sure, you are welcome to argue that the problem is that we just don't understand these well enough, and that when we do the problem will go away. I think he would respond to this the way I would: you're engaging in wishful thinking.

John Brock,

I just don't care whether a proposed fundamental theory includes a multiverse or not. If you present me with a theory that can be tested, it passes the tests, and it also predicts a multiverse, that's evidence for a multiverse. If you present me with a theory that can't be tested, even in principle, you're just not doing science, but something else that I don't care about. The problem with the string theory multiverse is that its role is precisely to make the theory untestable. You can imagine plenty of multiverse theories that are still testable, the problem here is that this one just isn't, and all the going on about anthropics, the distance from the earth to the sun, etc. is just blowing smoke to avoid acknowledging that there's a speculative idea here which simply has failed.

Robert,

Have you talked to many non-string theorist physicists and asked them what they think of either Susskind's promotion of anthropics, of Lubos's behavior?

20. **Eric**
July 27, 2008

Somebody,
My point is that even if there is a specific model in which it's possible to reproduce everything we know at low energy, it will not generally be accepted or recognized as unique or even interesting. This is due exactly to the fact that many within the string theory community have the same view as Schellekens, who doesn't think it's important to try to construct models where gauge couplings and particle masses can be calculated. This is the real danger of the anthropic argument, that people don't even think it's worthwhile to find solutions which agree with our world simply because we cannot say it is a unique solution.

21. **John Brock**
July 27, 2008

OK, I feel I'm getting closer. I had thought that you considered all theories with Multiverses to be in principle untestable, but apparently not. I may already be over my head, but I do appreciate your making the effort! A couple of further questions:

1) Is there any way you can explain what distinguishes a testable Multiverse theory from a non-testable one? I don't see any obvious way to create two categories; to me it would seem that they would all have to be equally untestable experimentally.

2) How do you deal with the possibility that a theory, like String Theory, might be both untestable and true? Or do you feel that String Theory is just wrong for reasons that have nothing to do with testability, in which case why does the issue of testability come up at all? If the Multiverse is being invoked simply to cover up other failings of String Theory, why not just put it that way, rather than bringing up testability?

3) Do you really not care at all whether a theory implies a Multiverse?!? To me it seems like such an obvious Good Thing, because of the philosophical considerations. All else being equal, an incomplete theory that seems to imply a Multiverse would seem to have a leg up over one that does not, because the Multiverse is such a compelling resolution to the cosmic coincidences problem.

22. **theoreticalminimum**
July 27, 2008

> I really doubt that, since, Lubos keeps giving clear and patient expositions of the central tenets of string theory on his blog, …

Robert, do you really think someone who has written a book like "*L'équation Bogdanov*" should be taken seriously? That he writes clearly about string theory is no surprise (it would be quite unfortunate if it were otherwise); he was hardworking and lucky enough to be involved in some developments in string theory, in one of the most specialised, almost carefree academic environments in the world (Rutgers, Harvard), from a relatively early period of his physics education. He had all his time to understand the technical details of much of what he has been writing about, when it comes to string theory.

I am a physics graduate student, and if I took Motl's rants seriously (with or without the string theory), I would never have thought about spending time studying this subject, or interacting with string theorists (I'm quite frankly relieved that he's no longer part of the mainstream community). Sadly, he has given a bad image of string theorists (and consequently has made the subject itself less "friendly"), for sure! I have so far met a number of string theorists, and thankfully almost all of them are very different to Motl's blog-personality. I know of at least 5 other physics graduate students (all in HEP, and all in Europe!) who would agree with me on these terms.

23. **Peter Woit**
July 27, 2008

John,

1) The multiverse aspect of a theory may be testable via statistical predictions, and, more importantly, the parts of a theory with a multiverse that just involve our universe may or may not be testable.

2) If string theory is inherently not testable, then the question of whether it is true or not is just not science. The idea that the we live in a simulation performed by higher beings is also not testable, and could be true. You're welcome to think about this and believe it or not believe it, but you're not welcome to claim that this has anything to do with science. As for string theory, my specific claim is that the idea that a 10/11d string/M-theory can provide a successful unified theory is simply a failed idea. One way speculative ideas fail is that they turn out to be vacuous, and thus explain nothing. That's what has happened here, with the multiverse being invoked to avoid admitting failure. It's an interesting and important exercise to examine exactly what goes wrong with the string theory unification idea, but the bottom line for any scientific theory is whether it provides a testable explanation of some phenomenon. String theory unification fails this test.

3) No, I honestly don't care. I spent a lot of my youth studying philosophy, and at some point lost interest in the idea that philosophy was going to provide by itself some insight into the nature of the universe. Philosophy can be quite useful as a tool for clearly thinking things through, but that's not at all how I see it being used in this multiverse story.

24. **somebody**
July 27, 2008

Eric says:

"My point is that even if there is a specific model in which it's possible to reproduce everything we know at low energy, it will not generally be accepted or recognized as unique or even interesting. This is due exactly to the fact that many within the string theory community have the same view as Schellekens, who doesn't think it's important to try to construct models where gauge couplings and particle masses can be calculated."

My point is that uniqueness is not a problem if we know how to build models in string theory. Uniqueness is not how we EVER built models. The virtue of stringy models is not that they are unique, but that they are UV complete. There could be many UV complete models, and to pick between them we need experiments – just like we needed experiments to determine standard model as the QFT at low energies. But this is still a perfectly predictive framework. Predicting gauge couplings and particle masses has never happened in the histoty of physics, but still we have had predictive theories.

There is a subtle but all too important distinction between the anthropism that Peter and Schellekens see in string theory. The former is based on the belief (see his previous comment) that there is an inherent impossibility of constructing predictive models, the latter is based on the fact that constructing models is very hard. My claim is that the effort to understand string theory non-perturbatively is the best way we can gain control of model building. Peter (invoking Schellekens) calls this wishful thinking. I must admit that he could be right, because the problem is in fact very hard. For that the only answer I have is that one must try!

In any event, the way it is often presented on this blog – that even if everything went well for string theory, STILL it would not cut it – that is simply wrong. If we only could wield string theory with sufficient ease, we would have a perfectly predictive framework. Whether we should work on solving this problem, or whether we should give up because it is too hard, thats where only history can be the arbiter. My suspicion is that a problem of this level of difficulty will have to be solved one way or another in any theory that purports to be a fundamental theory.

25. **Thomas D**
July 27, 2008

Why should we assume that the rejection by (Cambridge?) University Press was necessarily a bad thing, or unjustified, or unprofessional? We don't know what the earlier version of the book said, we don't know much of what the referee reports said, we simply assume it was a representative case of big bad string theorists using their power unjustifiably? Surely it could simply be a very typical University Press decision. Academic publishers, for quite good reasons, don't deal with works which are primarily criticisms of other scientists and their research. CUP publishes research, monographs, reviews, textbooks, they don't publish controversialists or polemics.

That rejection also didn't in any way force Peter to rewrite the book into a popularizing kitchen-sink attack (which may or may not be an accurate description…)

It simply isn't necessary or productive to write and publish academic books that are primarily critical of some trend in theoretical/mathematical physics: if and when that trend gets mined out, people will get bored and do something else. Researchers' time and effort are never well served by deliberate negativity in publications, whether academic or popularizing.

26. **Peter Woit**
July 27, 2008

Thomas,

1. The book as published is quite close to what was considered by Cambridge University Press, what was added were mainly some summaries at beginnings of chapter, and suggestions for further reading. No, I didn't rewrite it into a "popularizing kitchen-sink attack", which it isn't. Have you actually read it? The fact that the book ended up getting published by a non-academic publisher didn't change the actual book, but did make sure the book got more attention and a much wider distribution.

As for whether it deserved to be published by CUP, people can read the book and make up their own minds. I've been extremely gratified to hear from a large number of mathematicians and physicists that they learned a lot from the book and had a high opinion of it.

2. The referee reports from non-string theorists were quite positive and endorsed publication, the two from string theorists quite negative. One of the string theorist ones said that they would only give one example of what was wrong with the book because they couldn't believe any referee would favor publishing the book. The example he or she gave was a perfectly accurate sentence that he or she changed to make it inaccurate. None of the referees or Cambridge editors ever suggested that the problem with the book was that it was just a "criticism of other scientists and their research". Again, have you read it? There is nothing in the book personally critical of any scientist, or even of any scientists as a group. The only significant discussion of any particular scientist is about Witten, and there the discussion is extremely positive.

3. The book was rejected later on by Princeton University Press because of a referee report from Lubos Motl. For this story, see

http://www.math.columbia.edu/~woit/wordpress/?p=438

27. **Johnson**
July 27, 2008

I have thought about it for many years but I still don't get it… Can someone explain to me how an "anthropic argument" is different to a normal "scientific argument"? (Please don't delete this comment, I can assure you I am a well-meaning honest academic who'd just like to understand this point, I'm not a heckler). For instance, in a previous comment, Jim Clarage wrote:

"For example Newton could have easily adopted an anthropic approach to his inverse square law, perhaps assuming any value for the exponents in the radial and mass coordinates of the gravitating objects; and then agreeing to anthropically derive these exponents based upon e.g., what values allow and disallow stability of human bones, large tree trunks, or other mechanical properties of organisms living under the influence of Earth's known gravitational field.

…
However, instead of appealing to anthros Newton appealed to physics. He tried to pick the exponents and form of the law based upon reproducing previous physical laws, viz., Kepler's laws."

[italics added]

Could someone help me out here? Surely it amounts to exactly the same thing… what is the difference here?

(a) Newton makes the ansatz F = G m_1^a m_2^b r^c for a Law of Gravitation, in terms of some unknown exponents a,b,c,d. He then performs a rigorous mathematical tour de force and shows that large tree trunks can only exist if a=b=1 and c=-2.

(b) Newton makes the ansatz F = G m_1^a m_2^b r^c for a Law of Gravitation, in terms of some unknown exponents a,b,c,d. He then performs a rigorous mathematical tour de force and shows that elliptic orbits (whose existence is gleamed from previous astronomical observations by Brahe, Kepler, etc) can only exist if a=b=1 and c=-2.

Why on earth is (a) called an anthropic argument and discredited as "pseudo-science", while (b) is considered as valid science? They are the same thing, no? *What is the difference between observing the existence of large tree trunks with binoculars and observing the existence of elliptic orbits with a telescope?*

28. **Johnson**
July 27, 2008

"I've been extremely gratified to hear from a large number of mathematicians and physicists that they learned a lot from the book and had a high opinion of it."

Indeed, I considered buying the book (I read it in a bookshop) simply for the clarity of its mathematical presentation of the history of the ideas coming from witten, segal, atiyah, etc. Believe me, as a mathematician I find the usual description of these topics written by conventional string theorists very difficult to understand.

29. **Peter Woit**
July 27, 2008

Johnson,

I'm glad you liked the book. But please, the discussion of anthropics is getting off-topic and I don't want this comment section to turn into yet again another endless discussion of this issue.

30. **Johnson**
July 27, 2008

Okay… I will wait until this issue comes up again.

31. **manyoso**
July 28, 2008

Somebody said,

"In any event, the way it is often presented on this blog – that even if everything went well for string theory, STILL it would not cut it – that is simply wrong. If we only could wield string theory with sufficient ease, we would have a perfectly predictive framework."

I'm just a layman struggling to understand the ideas presented here, but I'd really like to know what evidence you have that forms the basis for this belief? How do you *know* that String Theory would "cut it" if only we had enough control over how to construct vacua? What guarantee do you have that our Universe is correctly modeled (not at just the currently known low energy results) by one of these sufficiently well constructed vacua?

Even more, what guarantee can you give that if only we knew how to construct the vacua better that the Standard Model would fall out at even low energies? To me, it seems that the only answer I've ever heard from String Theorists is that the math is just sooo elegant that, "It must be so!" This is not particularly convincing for a theory that hasn't even been demonstrated to be consistent yet.

I just don't understand the confidence you guys voice when top guys like Witten admit that you don't even know what String Theory *is* yet. How can you be sure that it'll end up describing our Universe at any energy level when so little is known of String Theory?

John Brock said,

"How do you deal with the possibility that a theory, like String Theory, might be both untestable and true?"

How on earth would you ever know that it was true if you couldn't test it? More to the point, there are countless theories one could come up with that 'might be true', but are untestable. We're all living in a VR made by higher beings… The Spaghetti Monster did it…

"Do you really not care at all whether a theory implies a Multiverse?!? To me it seems like such an obvious Good Thing, because of the philosophical considerations."

How so? I don't understand how some vacuous idea about a 'Multiverse' can be useful to explain anything? It sounds like you are arguing for ignorance. Just suppose for a minute that the Multiverse was true: we're all living in just one of the countless number of Universes in existence. Ok, what can you tell me about the Multiverse itself? What are it's physical principles and dynamics? Since we're already supposing a Multiverse, it is only fair to suppose this Multiverse's physical dymamics are also inscrutable (at least at first). We form theories with new physical constants that seem to have just popped out of nowhere… Now should I start supposing a Multi-Multiverse?? Perhaps the Multiverse concept is only appealing because it is so void of content that people can ascribe anything they wish to it.

32. **somebody**
July 28, 2008

"Even more, what guarantee can you give that if only we knew how to construct the vacua better that the Standard Model would fall out at even low energies?"

I cannot give you any guarantees whatsoever. But neither can I give you any guarantee that Yang-Mills has a mass gap, nor that black holes radiate, nor that the gravitational coupling doesn't change with time. But I still believe all these things are "true" in the contingent sense that scientists use that word. The point is that I see no reason why my quality control should be any ore "string"ent with string theory.

Anyway the reason why I think the standard model can arise from string theory is pretty simple, and I have a suspicion that it will not convince you: string theory permits many kinds of standard model like models, and there doesn't seem to be any reason why standard model should be particularly impossible. That is it. Proving it one way or another, I think, is going to be HARDER than actually constructing one.

Just to draw a caricature to drive home a point: if you ask me why I don't believe in telepathy, the answer is again only that it seems pretty unlikely. I have been held to court and asked whether I can PROVE it doesn't exist. This business is all about judgement calls and thats something people don't seem to get. Its not about worrying about exceptions even before the get-go.

" To me, it seems that the only answer I've ever heard from String Theorists is that the math is just sooo elegant that, "It must be so!" "

You asked a string theorist why he believes the standard model could arise from string theory, and the answer you got was that because the math is elegant? You should certainly talk to better string theorists.

33. **manyoso**
July 28, 2008

Somebody says,

"Anyway the reason why I think the standard model can arise from string theory is pretty simple, and I have a suspicion that it will not convince you: string theory permits many kinds of standard model like models, and there doesn't seem to be any reason why standard model should be particularly impossible. That is it."

Ok, let's presume that String Theory provides a sufficiently malleable framework wherein we'll eventually learn to construct vauca to accommodate the Standard Model. Given this, what reason is there now to believe that this will elucidate or provide predictive power for future experiments beyond the Standard Model?

34. **John Brock**
July 28, 2008

manyoso,

Peter seemed not to want to continue on this topic, so I wasn't going to press him, although I wasn't content with his answers. But I'll take a shot with you.

"How on earth would you ever know that it was true if you couldn't test it? More to the point, there are countless theories one could come up with that 'might be true', but are untestable. We're all living in a VR made by higher beings… The Spaghetti Monster did it…"

The thing is, Virtual Reality and the Spaghetti Monster are obvious and uninteresting cheats. They are strawmen. Yes, you can trivially come up with any number of theories like that, and who cares. But can you come with any number of mathematical theories which are well defined and consistent, in some way mathematically elegant and unique, and demonstrably include, as one of a huge number of possible solutions, a solution which describes our actual universe? How many of those can you come up with in the next five minutes???

Now here is the point: my understanding, as one of the ill-informed laymen who are part of the intended audience of Peter's book, is that such a theory is what the String Theorists are still hoping to find. This seems like an entirely reasonable judgment call to me! I do understand they haven't found it yet, despite decades of searching. I understand there are many possible reasons why they may never find it (that it doesn't exist being only one of the possibilities!). But if they did find such a theory, wouldn't you have to agree that it would be enormously more compelling as a candidate for the Theory Of Everything than the Spaghetti Monster, even if the theory included a Multiverse which made testing impossible?

That's it in a nutshell. The Spaghetti Monster will never be a compelling TOE. A well defined and consistent String Theory which actually described our universe (along with many others) would be, even if you couldn't test it. If you believe the universe is describable by mathematics, and I hand you a mathematical theory which can be shown to describe the universe, doesn't that have to count for something? Unlike Spaghetti Monsters, such theories are not a dime a dozen. So far we don't even have one.

35. **Peter Woit**
July 29, 2008

John,

I find the idea that there is a mathematically simple and compelling TOE that doesn't predict anything is about as plausible as the FSM. You're making up mythical creatures that there is no evidence for and debating their properties.

36. **John Brock**
July 29, 2008

But is that what the String Theorists are looking for?

If they believe they can find such a thing and you don't then this whole thing is just a difference of opinion at a level where there is no point talking to the lay reader such as me. What I'm trying to figure out is whether their is some problem with String Theory which can be understood at my level. I understand that you don't think they can come up with the sort of well defined theory that I am talking about. But do you understand why it seems to me that if such a theory were found it would have to be given a lot of weight, predictions or no?

The thing is, presumably the String Theorists are more or less as smart as you are, and if they think they can find such a theory, and you think they can't, and the argument is on technical issues, then there is no way I can take a side. The only level I can deal with involves questions such a Multiverses and testability, and so far I just don't see that String Theory has any problems that can be expressed at that level.

37. **somebody**
July 29, 2008

manyoso:
"Ok, let's presume that String Theory provides a sufficiently malleable framework wherein we'll eventually learn to construct vauca to accommodate the Standard Model. Given this, what reason is there now to believe that this will elucidate or provide predictive power for future experiments beyond the Standard Model?"

You could ask the exact same question with QFT : "yeah, sure, you can build models, but why should we believe it is going to be predictive?"

Model-building is something which requires ingenuity and intuition, and what we are seeking is a paradigm to do that in. QFT provided that for non-gravitational theories.

If you don't see the answer from that comment, see my other responses on this thread. This fallacy of "predictivity implies uniqueness" seems to be too deeply ingrained in everyone's mind, but right now I don't have the time to go through it again.

38. **manyoso**
July 29, 2008

John Brock,

"But if they did find such a theory, wouldn't you have to agree that it would be enormously more compelling as a candidate for the Theory Of Everything than the Spaghetti Monster, even if the theory included a Multiverse which made testing impossible?"

You are misstating the situation I believe. If in the future String Theorists discover that the theory is consistent and provides a way to calculate the Standard Model, then they'll have found a theory that is testable. If this same theory goes on to predict multiple universes or other bizarre and fantastical physical outcomes so be it. Even if we can not find a way to test all of the predictions, the theory would still provide others. I'll bet that the entire community of physical scientists would celebrate such a discovery.

However, the current situation looks nothing like that from what I can discern. What exists is a mathematical framework that has not been proven consistent and has not been shown useful to calculate any known physical result. It has not been shown a super set of the Standard Model.

All we have is this hugely malleable framework and a landscape of possibilities describing perhaps ~10^500 different universes. Currently, the String Theorists take it on faith that our Universe is some needle described by this huge haystack. And why do they? Because they've found some other needles which if you squint your eyes and turn your head kind of resemble the needle we're looking for. That and because the framework has an elegant way of including gravity.

The guys that argue for the anthropic landscape throw up there hands at ever having to find the actual needle in this landscape. They don't even see a particularly compelling reason or need to find it?! They are ready to argue that String Theory is a success even without finding the part of it that makes contact with our known world. This is the point where I think the exercise becomes completely unscientific.

Again I'm just a layman myself, but this is the situation from what I understand. Upshot? If String Theory wants to predict fantastical things that is fine with me, but largely irrelevant. The test for any physical theory is whether it can be used to model known experimental measurement.

39. **Gilbert**
July 29, 2008

Hi John,

I rarely butt in here, but this one is like a thorn in my side. It just seems to me that you would expect a theory which "*demonstrably* include[s] a solution which describes our actual universe" to be testable, whether it includes a multiverse or not, especially if it is "mathematically elegant and unique". Such a theory would have to go beyond the Standard Model and have something to say about, for example, neutrino masses, the hierarchy problem, dark matter and dark energy, the number of Higgs bosons and their mass, particle masses in general etc.

If it can't do that, it's just another pretty model, a curiosity. Worse, I think we're really talking about little more than a unicorn, since, as far as I can tell, these conditions—an elegant and unique TOE description of our actual universe that provides nothing more than the SM—are mutually exclusive.

It's not the presence or absence of a multiverse in the theory, in and of itself, that's the deal breaker. And I think this is what Peter is saying: he's multiverse agnostic. Just because a theory spawns a multiverse doesn't—or shouldn't—make it intrinsically untestable. If it also includes a beautiful, unique, and complete description of our universe, it must have something new, unique and testable to say about it. The confirmation of such a feature, or features, of our universe would just, at that point, lend credibility to the theory's multiverse.

The problem is that in a kind of logical « tour de force » this has been spun around backwards and upside down in, uh, certain circles, so that the inherent untestablity of a theory gets blamed on the multiverse it spawns.

Best,

Gilbert

40. **Eric**
July 29, 2008

For those such as Gilbert and manyoso who have made claims that string theory does not contain anything that looks like the Standard Model, I suggest you do a search of the literature. You will find that there are several good models, both heterotic and Type II, which resemble the SM closely at low energies.

41. **Peter Woit**
July 29, 2008

John,

Claiming that string theory unification should be believed if it is completely unpredictive is an extreme minority viewpoint among string theorists. You can find such people, but most string theorists know that if string predicts nothing it will have failed. Some have given up on string theory as providing predictions about particle physics, but think it worth studying for other reasons (e.g. as a model for quantum gravity, as an approximation scheme for strongly coupled gauge theories, etc.). Others are still trying to get predictions. But virtually none are out there telling their non-string theorist colleagues that string theory can be a successful unified theory that doesn't predict anything. They are well aware that their colleagues would laugh in their face if they did this, and never take them seriously again.

42. **manyoso**
July 29, 2008

Eric, please read again. I never made that claim. And I don't see where Gilbert made it either.

43. **Gilbert**
July 29, 2008

Hi Eric,

I think you're reading things into my post that aren't there, as I certainly make no such claim. I mean, it's just simply not there, unless your browser is seriously mangling the text somehow.

And while I wouldn't want to speak for manyoso, I don't see that he (she?) does either. In case you missed it, "…they've found some other needles which if you squint your eyes and turn your head kind of resemble the needle we're looking for" lines up *better* with "there are several good models… which resemble the SM closely at low energies" than "does not contain anything that looks like the Standard Model." Seems to me, the difference being a (rather large) difference of opinion as to how close the models line up to the SM.

Methinks perhaps you're making too many assumptions here.

Best,

Gilbert

44. **Eric**
July 29, 2008

Manyoso, you said

"If in the future String Theorists discover that the theory is consistent and provides a way to calculate the Standard Model, then they'll have found a theory that is testable."

I suppose you mean to calculate the SM parameters since you acknowledge that there exists models which look like the SM with three generations. Essentially, this boils down to stabilizing the moduli which parameterize the size and shape of the compactified manifold, upon which all couplings depend. For a given MSSM-like vacua with all moduli stabilized via flux or other nonperturbative effects, all couplings are determined and everything is predicted.

45. **Peter Woit**
July 29, 2008

Eric, Gilbert, Manyoso,

This discussion has become both off-topic and completely unenlightening. No more about this.

46. **Mikael**
July 29, 2008

Peter,
I think that Sabine's claim, it would have been better to keep the string theory discussion within the physics community leads to the questions, whether you agree and if so, why you saw the need to write your book still. In other words, I would think Sabine's position would have required a stronger response from your side. In would be interesting to hear more along these lines now.

47. **Peter Woit**
July 29, 2008

Mikael,

I think I already responded to this at Sabine's blog, but here's some more. There were two distinct problems at the time I wrote my book (2002). One was an unwillingness of string theorists to acknowledge to their colleagues how bad the situation of the theory was, and to engage in thinking about what to do about this. When Sabine says that it would have been better to have this discussion among physicists, I don't think she's saying that string theorists were willing to do this, but I somehow insisted on not engaging with them and went to the public. We both agree that there is a discussion that needs to take place among physicists, and that this was not happening. I tried to get an article published in Physics Today in 2001, this was rejected. The book I wrote was aimed at a scientific, university press audience. String theorists stopped university presses from publishing the book.

To this day, string theorists are still stone-walling, refusing to admit what the problems are and that something needs to be done about them. All that has happened since 2002 is the arrival on the scene of the anthropic landscape pseudo-science mania, which really has not made string theory look good. At this point, string theorist's plan seems to be to just do nothing until LHC results arrive, and hope that will change things. We'll see how this works out. In the meantime, their colleagues are noticing this and I've heard sometimes explicitly deciding not to hire string theorists until they see what the LHC says. The kind of discussion that should take place hasn't happened, and that's a shame.

The other problem was that the discussions of string theory available to the public (and to mathematicians, who are a significant audience for the book) were pretty uniformly overhyped and misleading. I thought outsiders deserved a more honest explanation of what was going on. The book was written to be as accessible to as many people as possible, while giving a serious discussion of the subject. I think this was worth doing. I'm not at all sorry that the book ended up being published in a way and at a time (with Smolin's) that got a lot more public attention than I ever expected. I'd have preferred to have the book published two years earlier by CUP, where maybe it would have been ignored, maybe it would have generated some more serious discussion (although I fear this was not very likely).

48. **Coin**

July 29, 2008

The oft-repeated claim that "having many vacua which look like ours at low energies kills predictability" is simply an incorrect one, and no sensible discussion about the landscape is possible when this sort of factual errors are not acknowledged.

Hi Somebody,

Could you possibly briefly explain exactly how predictability is possible in the case where there are many vacua that look like ours at low energies? (Assuming we pretend for the moment that actually constructing those vacua, as you are concerned with, somehow turned out to be easy.) If we find that we can identify a whole bunch of string vacua which produce the standard model at observable energies, but split off into a variety of higher-energy behaviors past that depending on which vacua you choose to study– I am unable to understand how to proceed in this case. What do you do next? How is the prediction made?

What I am trying to say is that if we had infinite powers of model-building there is absolutely no philosophical difference between QFT and string theory.

So let me see if I understand what you're saying here. You're suggesting something like that the general QFT toolbox allows us to construct many different models; each of these models is basically a "vacua", or analogous to one. We can thus imagine there is a "landscape" of QFT "vacua", each describing a different QFT which one might construct; and by nature there are many different QFTs which reproduce the standard model at "low energies" but do different strange things at higher energies (i.e. SU(3)xSU(2)xU(1) is one, SU(5) is another). Do I understand your argument correctly?

But surely this is not how people actually *use* QFT? Nobody I've ever heard of actually treats the space of usable QFTs as something to work with directly. Instead we pick one QFT "vacua" (say, SU(3)xSU(2)xU(1)) and stick with it. We say QFT is predictively successful not because "QFT" produced useful predictions, but because *a* QFT produced useful predictions. Moreover the problem you describe– that there are many different QFTs with different high-energy behavior and we have to pick one– it's not that this is a standing problem with QFT which we have simply chosen to excuse, it seems more that this is a problem in QFT which can be *addressed* because the different QFTs are highly distinguishable and some QFTs (say, ones which are *simpler*) are going to be inherently considered preferable over others. We can recover predictability despite a multiplicity of choices, because some of the choices are better than the others. I have never heard of any analogous selection principle being offered for string vacua. Am I missing something?

From my perspective as an outsider, it looks like faced with the problem of there being more than one potential QFT which produces the Standard Model at low energies, physicists seem to have coped just fine with picking the QFT to use going forward; they even seem to be coping with this problem in extending the Standard Model, we have things like the MSSM and the minimal dark matter model (chosen as I understand based on the simpler-is-better rule). But what would physicists do when faced with more than one potential string solution that fits experiment at low energies? You say what you need for predictivity is a vacua-construction method; if you had one, what would you do with it? I'm not trying to be snarky or difficult, I'm just trying to understand what the procedure here would be.

49. **Mikael**
July 29, 2008

Peter,
I think it was good (or at least necessary) you wrote the book.

You know, what really strikes me is that even a guy like David Gross seems to have become a little more skeptical about string theory in recent years. You'd think that somebody like him as an independent thinker would give a damn about such books and still he seems to have been influenced by this ongoing discussion.

50. **Peter Woit**
July 29, 2008

Coin and Somebody,

Claiming that the situation of QFT (which makes a huge number of non-trivial, verifiable predictions of high accuracy), and string theoy (which predicts absolutely nothing) are the same is just sophistry. QFT would be in the same situation as string theory if simple QFTs didn't work, so you had to keep adding more fields and complexity, just to evade falsification, ending up with a complicated mess, unusable for predictions. That's where string theory is now, it's not where QFT is.

51. **somebody**
July 30, 2008

"Claiming that the situation of QFT (which makes a huge number of non-trivial, verifiable predictions of high accuracy), and string theoy (which predicts absolutely nothing) are the same is just sophistry."

The entire conversation was about an idealized scenario IF we had model-building freedom in string theory. And there, sorry, I still stand by my claim.

Incidentally, "sophistry" implies malicious misrepresentation. Lets avoid personal commentaries and attacks in discussions of physics, shall we? I just want to nip it in the bud because I have seen (as I am sure you have) this sort of thing degenerate into online foodfights.

"QFT would be in the same situation as string theory if simple QFTs didn't work, so you had to keep adding more fields and complexity, just to evade falsification, .."

The real reason why we are forced to complexities in string theory is because we are tied to certain kinds of models because of the technical tools we have for model-building. Its like trying to build a ladder, but only using forks. It is entirely unlike the situation in QFT where you have the freedom to build models following ideas, and not technical tools.

Also, "simplicity" is an extremely slippery fish when you are only in the phase of trying to understand the (non-perturbative) principles of the theory. If we did not know about non-Abelian gauge symmetry of the Lagrangian description, there is no sense to saying that a QFT is "simple" because the gauge groups are. If somebody just gave you a bunch of rules for constructing messy scattering amplitudes, we would first have to come up with a (non-perturbative) Lagrangian description before even having a notion of "simplicity" of that description. This is EXACTLY the state in string theory. So it is really not clear to me what is the simplicity that we should expect from string theory at this stage.

Again, if you notice, both these issues stem from the lack of understanding/control we have of string theory. I am entirely willing to give you that these are real problems, and not merely details, but if you give me those, I find no reason whatsoever why it is any less of a framework predictively than QFT. Besides, I know that it gives me many generic features of particles physics, I know that it has all the expected features of a quantum gravity including black holes (at least once we fix the asymptotic boundary), and that it seems to incoroporate and generalize the most successful ideas in physics like gauge symmetry, spacetime geometry etc. It seems crazy not to want to understand that structure.

52. **somebody**
July 30, 2008

Coin,

If there is more than one model that fits data at low energies, it is no longer within the realm of science to want to distinguish them while doing only low energy experiments. What we call the standard model is merely a collective tag for all models that look like it at low energies. We can have more resolution in the space of theories only as we probe higher energies. The real problem with the low energy physics of today is that we know it has to fail being consistent at some scale because of gravity etc. So we know it cannot be the thing. If we understood string theory better, we could do UV complete model-building.

To make any successful model, we need ingenuity and creativity like Weinberg and Salam had back in the day. I am saying that one way in which string theory could deliver (one of the more unlikely ways, actually), is by being such a PARADIGM in which one can exercise that creativity.

There are many questions to ask here, and many string theorists would not be happy with this possible paradigm that I am suggesting here. But thats only because the ways in which truth is going to be realized in the future is vague speculation at this point. The only real way to find out is to work. My point was only that the blanket statements that people make about predictivity are often a result of confusing between preditivity with uniqueness. So I was demonstrating by a scenario that uniqueness and predictivity are entirely different beasts.

I am kind of busy, so perhaps this message is not very clear.

53. **stranger than fiction**
July 30, 2008

> The public opinion is relevant to the question what fields are generally of importance for our societies and how money should be invested to be as beneficial as possible, but the public never 'votes' on research funding on the specialist level. There is a reason why funding agencies have an elaborate referee process.

Sure but what are you willing to pay back?

String theorists paid back a little in the form of launching "The Elegant Universe" into popular culture and speculative articles and popular science books and TV shows on how string theory changes our view of the universe. I think that's part of the role science has to play within a culture that *believes* in science and funds the creation of a particular world view and appreciates the scientific method.

I'm not sure how this process can be avoided? Physics is not an auxiliary science that can hide itself into obscure academic discussions but the very foundation of the big picture. In that sense I also do understand physicists like Susskind who try to launch ideas that are compliant with our knowledge of the universe although they aren't empirical. It is correct that they are used to avoid the capitulation of string theory where they arise from but in the absence of a stronger alternative they also avoid capitulation of fundamental research. It compromises empirism in favour for science as a belief system that lets research continue as it is. This is less ironic in the sense that we in fact know it is not true but it is paradox: it lets something work by virtually terminating it.

Back to the public audience. You can't avoid paying back and becoming criticized for poor gifts, but the public has also no alternative to take what you give them unless it wants to give up their faith in reason and embraces either scepticism (postmodernism) or old obscure gods.

54. **Dexter Johnas**
August 1, 2008

Sitting in the attic? Ha, all the better, that's always a fun place to be curled up with a book, or in this case a cam. Great discussion. So much talk about string theory.

55. **Daniel de França MTd2**
August 5, 2008

Since this post is about videoblog, recently lubos made a post on Youtube concerning criticism of string theory. The tone is very light heart, I was really surprised.

http://www.youtube.com/watch?v=5ZxPTRfztsE

56. **Daniel de França MTd2**
August 5, 2008

Sorry about the post, I found it at http://dorigo.wordpress.com/2008/08/01/new-bounds-for-the-higgs/#comments , but I couldn't refrain from posting here. It just shows the guy on a different light.

57. **Chris Oakley**
August 6, 2008

I saw Lubos's YouTube effort and remained puzzled as to why he chose to post it. The only thing I can think of is that it is a coded message to his (alien) controllers, pleading for clemency following the initial failure of his campaign to subvert terrestrial science.

58. **GC**
August 23, 2008

I don't remember it adding multiple trackbacks here in the past like it did now, and I nearly always edit my posts after I publish them. Even now, I saw a few edits I'd like to make, but I'll wait until later to see if does it again.

ICHEP 2008

August 4, 2008
Categories: Experimental HEP News

A major HEP conference, ICHEP 2008, is taking place in Philadelphia at the moment, and many of the talks are already available online here. This is mostly a conference devoted to experimental HEP, and the big news is the joint announcement by CDF and D0 that they are just barely able to exclude, at 95% confidence level, the possibility of a Higgs with mass of 170 GeV. This is the first new information about (ignoring neutrinos…) the one remaining parameter of the Standard Model since LEP showed that the Higgs mass can't be below 115 Gev. For more about this, see Sunday's ICHEP plenary talk by Matthew Herndon.

Also announced at ICHEP and providing constraints on the possible Higgs mass are new fits using precision electroweak measurements. Tommaso Dorigo has a nice explanation of this story in a new posting here. Don't miss the comment section, which has a hilarious exchange between Lubos and an anonymous physicist who can't believe what is going on, asking Tommaso to check IP addresses to see if someone is impersonating Lubos. The point of physics being discussed is an extremely interesting one, but the mode of discussion ensures that enlightenment will not result.

Lyn Evans of the LHC gave a report on its progress. They expect to first try to inject a beam about a month from now, with first collisions and data maybe two months later, at 10 GeV center of mass energy. Evans is guessing that luminosity in 2008 will be about 10 pb^{-1}.

This morning, Joe Polchinski will give the plenary talk on "Recent Progress in Formal Theory", and Witten gave a public lecture Monday night. Nima Arkani-Hamed was supposed to give a "Concluding Inspirational Talk", but that appears to have been canceled.

Update: It's worth noting that the Higgs mass getting excluded is right in the middle of where Alain Connes's prediction from his NCG model comes in. Connes has a new blog posting about this here, where he admirably notes how discouraging this is for his model. Jacques Distler has a quite good new posting about the NCG model here.

More about this from Gordon Watts here.

Comments

1. **somebody**
   August 5, 2008

   Thanks for the link to Tommaso's blog.

   I must say that I have not been able to gather the verdict on the debate between Lubos and Guess Who. Does the experimental data that Tommaso presents constrain any effective Higgs that does the job of electroweak breaking? Or does it assume certain classes of models for Higgs (like simple SM Higgs or MSSM Higgs doublet)? Lubos seems to be arguing for the former while Guess Who seems to be arguing for the latter (specifically, that technicolor models need to be dealt with separately).

   Much as I would hate teaming up with Lubosian lunacy, I must say that I find Guess Who's claim suspicious, and if true, something of a letdown – I was hoping that those curves were "really" constraining the Higgs.

2. **Peter Woit**
   August 5, 2008

   somebody,

   Those curves are based upon a straightforward fit to the SM model, so they tell us where the Higgs is going to be found if we have just the SM, but I don't see how they tell us in general about very different models. The argument about whether they "favor supersymmetry" just seems empty to me. As for whether Lubos or "Guess Who" knows more about what they are talking about here, that's a point that doesn't seem arguable at all…

3. **DB**
   August 6, 2008

   A Russian Academician has leaked the inauguration date for the LHC: October 21st.

   http://en.rian.ru/world/20080805/115771418.html

4. **Chip Neville**
   August 6, 2008

   Peter,

   This is off topic, but Sean over at Cosmic Variance has a very nice rundown on what the LHC is likely to find, and not find, here.

5. **Peter Woit**
   August 7, 2008

   DB,

   I thought that date had been public knowledge for quite a while. Those here who predicted that CERN would do everything it could (including lowering the energy of the beams) to get beams in the machine by the inauguration date were on the money.

6. **John Baez**
   August 9, 2008

   Peter wrote:

   > they are just barely able to exclude, at 95% confidence level, the possibility of a Higgs with mass of 170 GeV.

   Of *exactly* 170 GeV? I think I can exclude that with 95% confidence without even doing any experiments. 🙂

Of *less than* 170 GeV?

Within *some range* of 170 GeV?

7. **Peter Woit**
   August 9, 2008

   John,

   What is being excluded at 95% confidence level is some small mass range about 170 GeV. The experiments don't quote a range, and it appears to me that this is because, however they have binned their data, they are just able to get above the 95% confidence level at one bin, that at 170 GeV. Under the circumstances, quoting any particular small range about 170 GeV wouldn't be very meaningful.

8. **Coin**
   August 9, 2008

   A Russian Academician has leaked the inauguration date for the LHC: October 21st.

   I'm looking at this press release which says that the "first attempt to circulate a beam" will be september 10, and that they're this weekend (this is apparently confirmed at uslhc.us) doing the first beam tests of any sort.

   Does this information alter the Novosti article, or is it just describing something different? Is the idea that the "inauguration" on Oct. 21 the same as first physics collisions, and the september 10 event will be the first full-power full-circulation beam test?

9. **Peter Woit**
   August 9, 2008

   Coin,

   The October 21st event is a ceremony for dignitaries (including President Sarkozy I believe) that was planned long ago. It doesn't correspond to any particular state of progress of getting the machine into operation.

10. **Coin**
    August 10, 2008

    Thanks!

11. **Dominic Cummings**
    August 12, 2008

    Dear Mr Woit

    Do you know how I cd get hold of a copy of Witten's public lecture at ICHEP08?

    (Thanks v much for your v good blog.)

    DC

FQXI and Templeton News

August 5, 2008
Categories: Multiverse Mania

The FQXI organization has just announced the details of $2.7 million dollars in grants that it will be handing out. The winners and their projects are described here. As usual, one of the main topics funded by FQXI is multiverse studies. There's also another news story on the topic from them here, which examines the question: "Are we in danger of a fatal crash with another universe?"

At the same time, FQXI announced an essay contest on the topic of "The Nature of Time", first prize is $10,000.

FQXI is funded by the Templeton Foundation, an organization whose goal is to bring science and religion together. The founder of the Foundation, Sir John Templeton, died last month at the age of 95, leaving his son in charge of the place. While the father seems to have been a rather Unitarian sort, the son Jack is the money behind the right-wing PAC Let Freedom Ring. Sir John's death will provide Jack Templeton with a lot more money to spend. The Chronicle of Philanthropy has a story about this, which explains that the plan is to hire new vice-presidents, with the goal of coming up with new ideas of how to spend more money to support free enterprise and virtue. Not clear yet what this means for FQXI, or for some of the other physicist beneficiaries of Templeton largess over the years.

Comments

1. **Urs Schreiber**
   August 5, 2008

   I sincerely wish the interest among cosmologists in multiverse story-telling were due to evil influence and money from quasi-religious lobby groups as you keep insinuating. But it isn't. The Templeton foundation has no influence on the scientific decisions of FQXi.

2. **H-I-G-G-S**
   August 5, 2008

   Based on title alone, my favorite award is:

   Peter Byrne, Oxford University Press, $35,000, The Devil's Pitchfork: Multiple Universes, Mutually Assured Destruction, and the Meltdown of a Nuclear Family.

   In an ideal world Mr Bryne would soon announce that this was a Sokal-like hoax, but I fear we do not live in an ideal world.

3. **Peter Woit**
   August 5, 2008

   H-I-G-G-S,

   Actually, it seems to me that's one of the better grants. It's not a grant supporting science, but rather the writing of what sounds like an interesting book about Hugh Everett.

   Urs,

   I don't claim Templeton is giving money to scientists and telling them to work on the multiverse. When it decided to fund FQXI, it was backing two scientists (Aguirre and Tegmark) who are among those most interested in multiverse studies. I think this reflects the Templeton point of view on science.

   FQXI says that its grants "target research unlikely to be otherwise funded by conventional sources." Some of these grants, e.g. the one to Linde, are for what appears to be mainstream research conducted by a prominent scientist at a leading institution, research that one would normally expect to be funded by NSF/DOE. Does this mean that NSF/DOE peer review panels are refusing to fund multiverse research? If so, what does it mean for the field to have a very wealthy private group devoted to promoting religion come in and heavily fund this research, when the physics community as a whole doesn't think it deserves support?

4. **Professor R**
   August 5, 2008

   Essay contest on the topic of "The Nature of Time"?

   I like the sound of that, and the max length is 5000 words, good scope there. Less impressed with the judging method – on the form, you are invited to suggest 3 reviewers. Hmm.

   P.S. The London THES used to have a competition like this, pity they stopped it

5. **JC**
   August 5, 2008

   If string theory ever lost a significant proportion (or all) of its NSF/DOE funding, what are the odds that they will resort to Templeton funding?

6. **Elisha Feger**
   August 5, 2008

   It's fine to be suspicious of the source of funding for people's work, but ultimately I think it's better to have more money available to go around. Yes, it means more crap will get published. It also means more good work will get funded. If nothing else it takes some of the pressure (political, social, and otherwise) off of the NSF and other governmental actors to fund the latest fad when there are private foundations giving out money as well.

7. **Coin**
   August 5, 2008

   Actually, it seems to me [The Devil's Pitchfork: Multiple Universes, Mutually Assured Destruction, and the Meltdown of a Nuclear Family.]'s one of the better grants. It's not a grant supporting science, but rather the writing of what sounds like an interesting book about Hugh Everett.

   …huh. Speaking as a long-time fan of a band called the Eels, whose frontman and only static member ("E") I later found out was Hugh Everett's son, I'm actually rather curious about that.

(A lot of the Eels songs make oblique reference to E's troubled family life, which was something that left me suddenly curious for details once I found out who E's father was. Actually looking on the website for the Devil's Pitchfork guy I find he's got kind of a short prototype version of his book up, wherein he notes Everett's son, the Eels guy, actually wrote a memoir called "Things the Grandchildren should Know"… very interesting!)

8. **Tony Smith**
August 5, 2008

From glancing at the 2008 FQXi award list, I noticed a few institutions with more than one award winner:

UC Berkeley – 3
Oxford University – 2
Theiss Research – 2
Tufts University – 2

What might account for their success?

Tony Smith

9. **Kea**
August 5, 2008

I have at least heard of all but three of the awardees before. Many are well known and many of them know each other (I know this first hand). And Aguirre had the audacity to tell me that the judging was in no way biased!

10. **Peter Woit**
August 5, 2008

Tony,

The 3 from Berkeley probably just reflects the fact that Berkeley is a very large university..

11. **Nigel Cook**
August 5, 2008

'I sincerely wish the interest among cosmologists in multiverse story-telling were due to evil influence and money from quasi-religious lobby groups as you keep insinuating. But it isn't. The Templeton foundation has no influence on the scientific decisions of FQXi.' – Urs.

Yes, I believe that – because it's not the Templeton Foundation that's to blame for scientific decisions in handing out the rewards, but the scientists they choose to make the decisions! Any ideas about who makes the decisions, Urs? Not the multiverse and string theory fanatics, I presume.

http://golem.ph.utexas.edu/category/2008/05/fqxi.html

'I agreed to participate in the review panel of FQXi's new round of grant competition.' – Urs Schreiber

12. **bob**
August 5, 2008

Concering the two Oxford affiliates, Julian Barbour is completely self-funded and has always been. Since he lives close to Oxford the philosopy of physics group here provides him with an affiliation so he can get library access etc, but he never got a penny as far as I know. He more than anyone is someone who decided very early in his career that institutions aren't going to make him do good science. It's great to see him get funding. Myself I found `refuge' in a computer science department, and this grant enables me to do foundations of physics stuff, and hire someone who otherwise would have a hard time getting a postdoc job given his field. While there is much activity here on many worlds/universes etc, neither Julian Barbour nor I have any interest in that stuff.

13. **A.J.**
August 5, 2008

Nigel,

Urs is about as far as one can get from being "a string theory & multiverse fanatic". But don't let that stop you from talking trash.

14. **Esornep**
August 5, 2008

PW said: "If so, what does it mean for the field to have a very wealthy private group devoted to promoting religion come in and heavily fund this research, when the physics community as a whole doesn't think it deserves support?"

Since when does "NSF = physics community as a whole"?

If indeed Linde has trouble getting money from the NSF, something I find hard to believe, then this is an appalling piece of incompetence on the part of the NSF. And it if is true, then all the more we should thank Aguirre and Tegmark et al for getting hold of money for people who want to do interesting research.

15. **Urs Schreiber**
August 6, 2008

Peter,

it seems to me that valid criticism of certain points — which might potentially lead to controversial but constructive discussion — is at risk of being diluted if accompanied with what comes across, if I may say that, as more or less random mud-slinging that linearizes a complex reality to what almost sounds like a Brownian conspiracy plot. In reality the issues — which are there — are much more mundane.

16. **Peter Woit**
August 6, 2008

Hi Urs,

I agree with you. I was initially about to delete the hostile comment from Nigel Cook (as I delete a lot of comments here…), but thought the link to your posting was valuable. So, then I thought about somehow extracting the link and posting it separately, or editing the comment, finally decided it was better to just let this one through. Anyway, all, I'll be even more vigilant in deleting empty hostility from now on….

I'd certainly be curious to hear your thoughts on some of the more interesting questions the FQXI grant decisions raise. It's obviously a difficult task to choose among unconventional research that the usual funding sources and many people would dismiss, trying to identify what deserves support. Much of the FQXI list is a reasonable take on this, and those like you who spent time working on this deserve thanks. If people want to discuss this aspect of FQXI, one place to do it would be Kea's blog

http://kea-monad.blogspot.com/2008/07/fqxi-proposal.html

where you can see what this looks like from the point of view of an unsuccessful applicant.

While I can see how these decisions probably went, the thing that I'm more curious about is the decision to fund research that I would have thought couldn't possibly qualify as "unlikely to be supported by conventional funding sources". Linde, Vilenkin and Bousso are among the most prominent people in their fields, working at major institutions, giving high-profile talks at conferences, appearing regularly in the press, etc. On the one hand anthropic multiverse proponents regularly claim that their view is increasingly the dominant, conventional one in the field, here we have FQXI saying this is unconventional research that can't get funding elsewhere. Which is it???

One other decision about "unconventionality" that I found odd was that about Subir Sachdev's proposal. He's a Harvard professor, and as far as I can tell the topic of using AdS/CFT to do condensed matter physics is one of the hottest topics in string theory. A Harvard professor proposing to work on one of the most fashionable topics around is something it is hard to see as unconventional. Maybe this has something to do with the politics of condensed matter research funding, where I guess this might look unconventional, even if it is highly conventional from the string theory side.

17. **Belizean**
August 6, 2008

Confusion about Templeton funding conventional research by established researchers in venerable institutions might be eliminated by the considering the foundation's apparent objective: to purchase legitimacy.

18. **Anthony Aguirre**
August 6, 2008

Hi All,

I think this discussion is valuable, because FQXi is continually working to improve how well it serves the scientific community, and because it struggles with these very same issues. There are a few comments it might be worth making from the 'inside' perspective.

1) This is hard. The questions you have been raising are tricky, and the grant panels don't get the luxury of just complaining or applauding — they have to actually weigh all of these factors and make decisions. I would also say that the panelists are talented, careful, fair, and very open-minded people making decisions on the very criteria that are contained in the FQXi request for proposals. I think they have done a great job and am grateful for their somewhat painful decision making.

2) To those that complain loudly that their application was unsucessful, I would say: I'm sympathetic, but welcome to the club! Even top scientists *often* have proposals rejected. FQXi funded 33 grants (and many with reduced budgets) out of 190ish initial proposals. The NSF often funds 10-20% of theirs.

3) "Well-known" and "at a top institution" often means "well paid" but it does not always mean well-funded. In some cases, you'd be surprised. Also, in some cases grants going to "famous" people support postdoctoral positions and are in that way really supporting less-known researchers.

4) Specifically, in terms of the multiverse, I would say that these proposers simply out-competed many others. A great majority of the top people in this field applied, and they wrote good proposals. Number of well-written proposals arguing *against* the multiverse, or suggesting compelling alternative early-universe scenarios: small or negligible. I think the panel would have more than welcomed them.

5) Along these lines, while I'm very happy for FQXi to be funding what it did, to those that have more negative views, I would say: write better proposals! There are some great people from whom I would personally have liked to see proposals but didn't. And there are others from whom I would have liked to see different (more unconventional or bold, perhaps) proposals. And there were lots of proposals that just did not make as compelling case as they could have.

6) More money would also help. Tell your rich friends. While the Templeton Foundation has generously funded FQXi's first years of operation and *appear* to like what we are doing (they are in fact very hands-off), they have given no guarantee of future funding.

19. **Peter Woit**
August 6, 2008

Anthony,

Thanks for the comments on this. Good luck finding suitable funding for the future….

20. **Esornep**
August 6, 2008

My comments are directed to Anthony Aguirre.
[a] You're doing a great job! Really. This thing really boosts morale, even for people like me who did not apply. Keep it up!
[b] Are the recipients of last year's grants encouraged or required to report on the progress they have made? I am *not* thinking in terms of "accountability", I'm just thinking that most of the projects looked really interesting and I just want to know what the recipients did, even if no spectacular progress was made. I realise that this would have to be handled diplomatically, but could you ask them to tell us what they now think is the state of the art in their respective fields? That would be useful and interesting.
[c] You say "There are some great people from whom I would personally have liked to see proposals but didn't. " Did you write to these people to encourage them to apply? Whatever their reasons for not applying, I'm sure that they would be deeply grateful for such a letter.

In that connection: PW points out that some leaders in the field have the habit of claiming that the multiverse view is the dominant one in some sense. The reality is somewhat more complex. Last year I submitted a multiverse paper to [famous journal] and the editor returned it saying that they felt it was too speculative. I subsequently published it in [equally famous journal]. I suspect that the truth is that the acceptabilty of papers on this subject is strongly dependent on the name of the author and/or his affiliation. Hence my references to morale-building.

21. **Peter Woit**

August 6, 2008

Esnorep,

It does seem that the legitimacy of multiverse research remains a contentious issue. Unfortunately, to me the morale of its proponents seems all too high…..

22. **Daniel de França MTd2**
August 6, 2008

@Anthony Aguirre

"Number of well-written proposals arguing *against* the multiverse, or suggesting compelling alternative early-universe scenarios: small or negligible."

So, next year, I foresee a great number of people applying against The Flooding and astrology.

23. **V.T.**
August 7, 2008

> "Number of well-written proposals arguing *against* the multiverse, or suggesting compelling alternative early-universe scenarios: small or negligible."

Maybe this deserves further qualification:

a) the "multiverse idea" may be quite correct and still problematic as a research topic at the moment: there is so very little known on which to base concrete scientific discussion on.

Similar problems exist for potentially scientific topics such as, for instance, extraterrestrial life: while it may well exist, at the present stage a scientific article on this topic is bound to be lacking a basis.

So one may not want to argue against the "multiverse idea" and still feel troubled by the amount of attention it is getting. Whereof one cannot speak (yet) thereof one must be silent, as the saying goes.

b) It seems one should clearly distinguish between, on the one hand, relatively concrete and valuable aspects of the "multiverse scenario" where concrete cosmological models are treated, which can be handled quantitatively in terms of equations to some extent and — on the other hand — general far-out non-quantitive speculations inspired by the "multiverse idea". The main concern about research inspired by the "multiverse idea" that I have seen is that the latter aspect is tending to dominate the former.

24. **Chris Austin**
August 7, 2008

Hi Anthony Aguirre,

Perhaps I could make a comment about the 500 word limit on the initial proposal. Of course I appreciate that there has to be some reasonable limit on the length of such initial proposals. I sat down to try to write the most concise summary possible of what I wanted to do, and then counted the words, which unfortunately came to about 1500. I then tried to reduce this down to 500 while still presenting something coherent, and by the time I had finished, there was nothing left but parts of the first and the last paragraph, with most of the summary of the proposed work gone. So I was not really surprised when my initial proposal was rejected. Of the 500 words, approximately the last 250 were taken up by demonstrating that my proposal was topical, foundational, and unconventional.

Best regards,
Chris

25. **Anthony Aguirre**
August 7, 2008

Esornep:

a) Thanks for your kind words!

b) Yes, FQXi requires that the grantees send annual renewal applications (for multi-year grants) and final reports for all grants. We keep track of the publications associated with each grant as well. We don't display all of this in public, but do take care that grantees are doing something like what they said they would, and that for funding to be continued after the first year, that they are actually making progress.

c) No, FQXi did not really reach out to individual researchers. I feel that the desires I mentioned are more my personal wishes as a researcher than something to be pushed with my FQXi hat on, where my main goal is to be evenhanded and not push any particular agenda.

VT and Daniel de Franca:

I agree that the people most likely to point out actual problems with the multiverse idea are those that take it seriously and actually work on it. Indeed, some of the strongest and most detailed criticisms of mulitverse ideas are in papers of mine, though I might reasonably be termed a 'multiverse aficionado'

I also strongly agree that it is important to distinguish between various versions of the 'multiverse', since some (like different Hubble volumes) are completely innocuous and hard to avoid, whereas others (such as the ensemble of all possible conceivable universes) seem nearly impossible to test or even discuss with any rigor.

Chris:

Thanks for this — it is something we have been considering for future rounds: 500 is a real challenge for both proposers and reviewers.

26. **Physics Professor**
August 7, 2008

Does anyone know what the "giant void" is that is mentioned here?

http://www.fqxi.org/large-grants/awardee/details/2008/mersini-houghton:

"One of our predictions here, the existence of a giant void was confirmed by observations only 7 months later. "

Where was this giant void observed? When was it predicted?

27. **Peter Woit**
August 7, 2008

Physics Professor,

That claim was covered in an edition here of "This Week's Hype"

http://www.math.columbia.edu/~woit/wordpress/?p=621

28. **Physics Professor**
August 7, 2008

Thanks Peter,

Now it all makes sense.

The giant void and multiverses were created to contain the FQXI funding and protect physicists from it.

29. **Esornep**
August 7, 2008

Anthony: sure, I have no doubt that you keep an eye on how the money is spent, but what I meant was that *I* [and others] would just like to know how the awardees are going. I'm not suggesting that they be forced to make this public, but perhaps they could be encouraged to do so? A lot of very interesting stuff there!

AA also says: "I also strongly agree that it is important to distinguish between various versions of the 'multiverse', since some (like different Hubble volumes) are completely innocuous and hard to avoid….."

Exactly. I think that critics of the multiverse should begin by familiarizing themselves with the paper that started it all: Coleman and De Luccia 1980. I assume that PW and other critics would not regard Coleman as a pseudo-scientist, yet he and De Luccia explicitly consider the possibility that *our* Universe is the interior of a CDL bubble. Granted that we live in what is probably an eternally expanding universe, the onus is on the critics to show that CDL bubbles can't form, or that if they do form they can never resemble our Universe, etc etc etc. Likewise *if* our universe is infinitely large: if you don't like the consequences of that, write a paper showing that the idea is inconsistent with something. Too much of the anti-multiverse polemic takes the form, "Gee, that's really weird, therefore it's ridiculous to think about it!"; the critics don't seem to realise that a multiverse *seems* to be an inevitable consequence of our current [observationally favored] theories. [It may not be, but showing that would be a diffcult technical exercise.]

In short: if you don't like it, master Coleman and De Luccia and write a paper proving that they made some fatal mistake.

30. **Peter Woit**
August 7, 2008

Esnorep,

I don't know of any way to use Coleman – De Luccia to make any testable predictions about anything. Do you? The problem is not a fatal technical mistake in their work

I'm a skeptic about the possibility of multiverse studies leading to anything testable in observational cosmology, but willing to admit that such a thing is not in principle inconceivable. I just don't see any evidence of this going anywhere.

My main interest though is not in cosmology but in particle physics. It is there that the "string theory anthropic multiverse" is out-and-out pseudo-science, nothing but an attempt to find some way of refusing to admit the failure of the string theory unification program. Promoting this to young scientists and to the public is having disastrous effects on the subject and how it is perceived.

31. **Richard**
August 7, 2008

Several years ago there was a single panel cartoon in the newspaper called I Need Help that pointed to where we may look for evidence of a multiverse.The caption on top of the panel says "In a parallel universe, not far from out own …". Within the panel, a woman has just removed clothes from a dryer, and she's holding a pair of socks in front of her. She exclaims,

"For cryin' out loud … extra socks again!"

As a bonus, this may also suggest a new law of physics: conservation of socks.

Hmmm, maybe I should quit working on topological dynamics tonight and head to the basement and start disassembling that dryer. I wonder if I could get a grant …

32. **somebody**
August 8, 2008

"I don't know of any way to use Coleman – De Luccia to make any testable predictions about anything. Do you? The problem is not a fatal technical mistake in their work."

If there is no mistake, why do you reject the consequences of it? If you agree that Coleman – De Luccia is a consequence of standard QFT and gravity, I don't see how you can justify NOT pushing forward so we either understand the consequences of tunneling between universes or see a contradiction. Like (penrose)^T says, most of the criticism about this issue is knee-jerk, many examples of which you can see on this thread.

Besides, I don't see why it is it manifestly impossible that there cannot be any signatures of tunneling. A tunneling bubble is a like an expanding cavity, and cavity-radiation can leave signatures on the CMB (for instance), depending on the ambience into which it expands. To see whether it is actually measurable or too small, is an actual computation and no amount of talk will settle it. This is just one example.

"I don't know of any way to use Coleman – De Luccia to make any testable predictions about anything. Do you? The problem is not a fatal technical mistake in their work."

Here is an example for non-testability which is actually even worse (not even direct CMB is not of much use here):

Hawking radiation (and for that matter most of black hole physics) does not make any testable predictions either. By your standard, a tremendous amount of intelligent extrapolation that we currently call theoretical physics will have to go out the window. I am not saying that this is not a reasonable view to hold, but I am saying that it is not very meaningful then to complain against this or that specific issue.

Testability is certainly important, but the way you are proposing it here sounds like a very short-sighted way to go about it. You give absolutely no value to what many people would call "understanding" or "insight". I think most practicing theoretical physicists will be of the opinion that thinking about black holes, tunneling etc. has taught us the few things that we actually think we know about quantum gravity.

33. **woit**
August 8, 2008

Somebody,

You are completely ignoring what I wrote and arguing against a straw man. What you quoted was just a response to the suggestion that if one doesn't like the multiverse hype, the only thing to do is to find an error in Coleman De Luccia. The problem that concerns me has nothing to do with the technical validity of that calculation.

I'm all in favor of speculative research devoted to trying to better understand physics at the deepest level, even when this research is still far from being able to make experimental predictions. That's actually what I spend much of my time doing. What I'm not in favor of is pseudo-scientific research aimed not at getting a deeper understanding, but at avoiding admitting the failure of an enterprise that many people have a lot invested in.

34. **Esornep**
August 8, 2008

The point is that there are many *kinds* of multiverse research, ranging from concrete calculations and — yes — predictions [such as the kind of work Anthony Aguirre, and several others, have been doing] to much more way-out stuff. Likewise, some of it is closely related to string theory, while a large amount [such as anything connected with CDL bubble universes] has nothing whatever to do with string theory, except that string theorists hope that it might instantiate their Landscape. On the other side, critics range from whackos who think that *all* talk of multiverses is evil or laughable, right up to people who have concrete technical reasons for thinking [for example] that the string landscape does not exist. *It is important to be precise about where in these spectra one stands. *

PW says: "I just don't see any evidence of this going anywhere."
Well, I don't see any evidence of geometric Langlands going anywhere of interest to physicists. In fact, nobody has explained what that stuff would be good for even if geometric Langlands turns out to be a "success". So what? I hate most kinds of white wine. Do you care? Shall I set up a blog called "Not Even Vinegar"? Well, blogs are often nothing but a way to vent, but I think you hope to achieve more than that…..

Short version: the perception many people get [see Urs' posts] is that you, PW, are dismissive of *all* multiverse research, think that it is the work of shadowy cult-like crypto-fascist organizations, etc. It now appears that we may have been mistaken about this, since you are "willing to admit that such a thing is not in principle inconceivable" but you really can't blame us when you see a post like this one. Nor can you blame people for feeling offended.

35. **Peter Woit**
August 8, 2008

Esnorep,

I think it is highly misleading to claim that multiverse research has produced predictions. Finding some scenario (for which there is not the slightest evidence now) in which some small imprint of the universe before the big bang appears in the CMB is very different than coming up with a testable prediction based on invoking a multiverse. For one thing, the multiverse hypothesis can't be falsified this way.

I've never made any comments about "shadowy cult-like crypto-fascist organizations". I've made very specific claims that the Templeton foundation likes to fund multiverse pseudo-science because they're explicitly in favor of blurring the boundaries of what is science and what isn't. Scientists should be worried by this kind of thing.

If multiverse proponents find my comments offensive, too bad. I find what most of them are doing offensive pseudo-science. No, it's not all the same. Some of it is science, although of an extremely speculative sort that has not led anywhere, and for which there are good arguments that it can't. Much of it though is nothing but a bald-faced attempt to avoid admitting failure by trashing the scientific method.

Geometric Langlands may or may not lead to anything important in physics. But even if it doesn't, it's definitely important mathematics, and worth attention because of this.

36. **gs**
August 10, 2008

Dear somebody,
CDL obtained their results by boldly applying the semi-classical Euclidean path integral methods to the case of gravity. Do you know of any calculable settings (AdS/CFT, matrix theory?) in which the formation of CDL bubbles has been demonstrated?
Cheers

37. **Terry Hughes**
August 13, 2008

Practical application of the "multiverse" concept exist! Here's one as summarized in Wikipedia [http://en.wikipedia.org/wiki/Sinestro] which might be especially important since Warner Brothers has just greenlighted a new Green Lantern movie:

52 Multiverse

In the final issue of DC Comics' 2006-07 year-long weekly series, 52 #52, it was revealed that a "Multiverse" system of 52 parallel universes, with each Earth being a different take on established DC Comics characters as featured in the mainstream continuity (designated as "New Earth") had come into existence. The Multiverse acts as a storytelling device that allows writers to introduce alternate versions of fictional characters, hypothesize "what if?" scenarios, revisit popular Elseworlds stories and allow these characters to interact with the mainstream continuity.

The 2007-08 weekly series Countdown to Final Crisis (or simply Countdown) and its spin-offs would either directly show or insinuate the existence of alternate versions of Sinestro in the Multiverse. For example, Countdown #16 detailed that the Sinestro of Earth-51 has been murdered by the pro-active Batman of his world, in a crusade against its villains. Countdown spin-off series Countdown Presents: Lord Havok and the Extremists depicted a version of Sinestro in its 3rd issue (2008) from an alternate world referred to as "Green Sinestro", depicted as a part of the villainous Monarch's army. This version has his original green power ring, but is no less vicious than his mainstream continuity counterpart and the official designation of his world is unrevealed.

38. **Coin**
August 14, 2008

Hi Terry,

The problem with comic-book-based predictions is that predictions concerning superhero comics are as a rule not *falsifiable*, due to comic authors' tendencies to retcon everything and anything the instant it would be convenient to some particular plot. As a result of this effect any statement you can formulate concerning comic characters or universes will, with probability approaching one as time goes to infinity, at some pair of points have been both true and false respectively, thus making it impossible to ever to satisfy the Popperian criteria

39. **Terry Hughes**
August 14, 2008

Coin,

Yes, yes, what you say makes good sense. On the other hand, what you say also seems to apply as well to string theorists as it does to comic book authors, which is probably your pretty subtle and witty point. In fact, I would argue that there seems to be more in the way of reality checks and falsifiable predictions in the DC multiverse theory than in, say, the Suskind Theory(ies) and other multiverse theories that have been subject to more extensive academic analysis. As a preliminary matter, let's not forget that the DC Theory POSTDICTS all prior developments not just of Green Lantern but in the entire Justice League of America ouevre, which is surely as impressive as string theory postdicting gravity in the wrong dimensions. And the DC Theory actually predicts that there are EXACTLY 52 alternate universes and that the Sinestro in Universe 51 has a GREEN power ring (not yellow, as in our universe). Those are predictions one can get one's hands on, at least in principle. I would have thought that it was as important to any good multiverse theory to predict an exact number of multiverses as it is for a good piano to have exactly 88 keys. But what the hell do I know?

40. **Doug**
August 14, 2008

Peter,

You wrote:

> I'm all in favor of speculative research devoted to trying to better understand physics at the deepest level, even when this research is still far from being able to make experimental predictions. That's actually what I spend much of my time doing. What I'm not in favor of is pseudo-scientific research aimed not at getting a deeper understanding, but at avoiding admitting the failure of an enterprise that many people have a lot invested in.

If I understand correctly, your foundational studies likely focus on "not just unification of physics, but unification with mathematics…"

I've been trying to understand if this should be interpreted as "unification [of physics] with mathematics…," or "unification [of] mathematics…"

I would really appreciate any clarification of this you might feel inclined to offer, but in either case, do you think you could ever see yourself writing a FQXI proposal (or essay), based on your thinking along these lines?

41. **Peter Woit**
August 14, 2008

Doug,

Personally I believe that deep mathematical ideas and deep ideas about physics tend to come together, so, sure, there is some deeper unification of physics and mathematics out there that awaits our understanding. We'll see.

At the moment I don't have any particular need for grant funding so I'm not about to apply for any from FQXI or anywhere else. I don't have an absolutist position that one shouldn't take money from organizations whose goals one disagrees with, but I do think one should avoid this if one can. So, because of its Templeton funding, FQXI would not be the first place I would think of applying to for a grant.

42. **Terry Hughes**
August 16, 2008

This just in
[http://www.telegraph.co.uk/earth/main.jhtml?xml=/earth/2008/08/15/sciwarpdrive115.xml]:

"Two physicists have boldly gone where no reputable scientists should go and devised a new scheme to travel faster than the speed of light. …. All this extraordinary feat requires, says the new study, is for scientists to harness a mysterious and poorly understood cosmic antigravity force, called dark energy. …. The new warp drive work also draws on "string theory", which suggests the universe is made up of multiple dimensions. We are used to four dimensions – height, width, length and time but string theorists believe that there are a total of 10 dimensions and it is by changing the size of this 10th spatial dimension in front of the space ship that the Baylor researchers believe could alter the strength of the dark energy in such a manner to propel the ship faster than the speed of light. They conclude by recommending that it would be "prudent to research this area further.""

Talk about practical applications for string theory! Of course, there are the predictable nit pickers and naysayers standing in the way of such visionary thinking.

43. **woit**
August 16, 2008

Terry,

I was tempted to write a posting about this, including links to the press release that the authors had their university put out, and to the many places which now feature stories about how "string theory is used to make a warp drive".

But sometimes I guess I should try and exercise restraint, and just ignore the stupider things that string theorists do and promote to the press…

44. **Terry Hughes**
August 16, 2008

Peter –

You are correct, in my opinion. Expressly criticizing the stupider string theorists' self promotion efforts (like this warp drive nonsense) could easily turn counterproductive because it could easily open the critic to the charge that he is distracted and wasting time shooting down ideas that are not central to the development of string theory or the product of string theory's stronger advocates. That's why it's better for the criticism to come in a stray comment than a post!

By the way, I also believe that deep mathematical ideas and deep ideas about physics tend to come together. That's why I'm very skeptical that the physics of string theory could turn out to be essentially empty (which has not been definitively proven, but seems increasingly likely) but the mathematics derived from, characteristic of, and spun off by, string theory could turn out (or has turned out) to be as wonderful and profound as is now maintained in many quarters of the higher mathematical world (in my opinion, among many people who should know better). If there is some deeper unification of physics and mathematics out there that awaits our understanding, then I do not see how it is likely to unify trivial and/or incorrect physics with brilliant, profound mathematics. Sadly, mathematics lacks even the one weak check physics has on errant enthusiasms: a demand for predictions and falsifiable experimental results.

But what the hell do I know?

Furby, Tamagachis, string theory…

August 8, 2008
Categories: Uncategorized

Things don't seem to be going well these days for string theory in the "marketplace of ideas". From an article about gasoline-saving pedals:

> The 1990's were the host of many great fads. Furby, Tamagachis, string theory, the examples are as numerous as the many incarnations of Prince.

Comments

1. **Noah**
   August 8, 2008

   Um…it's "tamagotchi" or "tamagochi"…

2. **Van**
   August 8, 2008

   Well, I have to admit that this is clear evidence that string theory is going out of fashion and Peter Woit has emerged victorious.

3. **Daniel de França MTd2**
   August 9, 2008

   String Theory should be out of fashion, but it isn't at all. Just check how many articles are posted everyday…

4. **M. Wang**
   August 9, 2008

   Blame that on the tenure system. It is rather anachronic to offer lifetime job protection after just a few years of work experience in this day and age.

   Tenure system was largely responsible for stifling out-of-box thinkers in academia and therefore partially contributed to the rise of String. Now it will continue to protect people who really belong to the trash heap of history.

5. **big vlad**
   August 10, 2008

   M.Wang, surely on the contrary the tenure system encourages out of the box thinkers. Once you've got tenure you don't have to worry about publishing x papers per year, or how many citations you have. You can just work on whatever you want.

   If you're a postdoc who has to make applications to new institutions every 2 years you're forced to work on whatever is fashionable.

6. **M. Wang**
   August 10, 2008

   With or without tenure system, the postdocs will have no job security until they make it to the next (or the next next) level.

   Full professorship, however, will be available to younger researchers if we do away with the tenure system or at least lower the mandatory retirement age. FBI agents have to retire at the age of 57. Why do physics professors stay on into the 60s or 70s? How many innovative advances were done by people older than 50 anyway?

7. **chris**
   August 11, 2008

   what's wrong with tamagotchis? thay are cool 🙂

8. **David B.**
   August 11, 2008

   Dear Wang:

   The tenure system is far better than the alternative you are suggesting. If you want to have an example of how bad things can get, consider the case of someone who gets to recompete for his professorship every few years without any security and a salary that is considerably below what you could get in industry positions, where they pay better so you to take the risk of lossing your job. You would not be able to get the best people to do research and public universities would not have the funds to pay for their salaries (they would be considered too expensive). There has to be something at the end that will make such an investment on time for individuals to be worth their while and tenure plays an important role in guaranteeing the quality of the research. It is not neccessarily fair, but it is cost-effective and I would say that it is being put to good use.

   Mandatory retirment in professions where there is physical rigor and you could potentially harm someone by not being able to react quickly enough or where you might be taking a risk on your life is rather standard. For most other professions around the world, retirement age is around 65.

   In academic positions the experience of professors far outweighs their drop in productivity as they age: they train new generations that relish the opportunity to be able to work with them. Consider the fact that retiring people early is just an added cost to society: you have to pay their retirement benefits after all.

   Also, tenured professorships, especially in theoretical physics, usually involve people spending ten to fifteen years previous in training for the chance to get one: a pHD plus a few postdoctoral positions plus the time as a junior professor with no guarantees of getting tenured. I would not call that 'just a few years experience', but rather a very expensive investment in funds trying to get the best people for the job. You want to keep that investment giving returns for as long as you can.

9. **ninguem**
   August 12, 2008

I'd be happy to retire at age 57 if I was given a nice pension. Under the current system in vogue at most US universities, I can't afford to retire at 57 and I probably won't be otherwise employable at that age.

10. **M. Wang**
August 12, 2008

David,

I understand where your arguments come from. Unfortunately, they are basically the same ones that socialists the world over had used in the past hundred years to justify lifetime job protection. The reality, which if you want you can find in any Econ 101 textbook, is that a rigid job market shaves off about 1-2% in annual productivity growth. Why would university jobs be immune from this universal law of economics? No reason whatsoever.

A common mistake made when arguing in favor of continuing the socialist status quo is to assume that the rest of the system will remain unchanged when the obvious safety net is dismantled. In reality, human society is a dynamic organism. Without the rigid job protection, the employers and the employees (as well as other stake holders such as students) will set new rules and form new norms that maintain a better balance among efficiency, fairness and safety.

Experience is important in a lot of other disciplines. For example, the French railroad workers can easily argue that it takes decades to learn to operate their railroads safely and proficiently, but look at the end result of their rigid job protection: a ridiculously low productivity level. Forty years ago, the French GDP was significantly higher than the British. Now it is the other way around, thanks to the Thatcher revolution in 80's, which, if summarized to one principle, is about unleashing creative destruction through increase in job mobility.

Ironically, theoretical particle physics is the one academic field where experience has had very little to do with meaningful achievements historically. This is a profession that should value innovation above all else, much like the silicon valley. Can you imagine Google or Intel offering tenures to their employees? Don't Intel engineers deserve the job security? Doesn't their experience count?

Theoretical particle physics is also rather unique among academic fields in that nowadays young PhDs have to do 4 or 5 postdocs on a routine basis. The mathematicians don't have to. The chemists don't have to. The EE majors don't have to. Don't you think this is very wrong to these young researchers and detrimental to the whole discipline as well? With a serious problem such as this plaguing the field, Physics really is crying out for structure reform. Breaking down the job protection for senior staff is a time-tested way of improving the welfare of entry-level personnel. If there is a better way to help the postdocs, I don't know it.

I should add here that the cruel practice of a potentially career ending review every one or two years was unheard of in the capitalist world until Jeff Skilling introduced it into Enron. Somehow, it never quite caught on among other corporations. I am just aghast at the way the physics community takes as a given.

The entrenched interests may be too strong to even allow the mere contemplation of reform in academia right now. But in a few decades, when the economic center of gravity moves to China and India, tougher competition in higher education may finally force some real changes. I can think of a few rational possibilities, such as clearer separation of teaching and research duties, more flexible hiring and firing practices, greater emphasis on students' welfare, and, yes, better pay for the junior faculty members. Until then, my sympathy to all the struggling postdocs.

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12. **Anonymous**
August 13, 2008

As a tenured professor, I certainly see some of my colleagues who should have retired long ago. On the other hand, most professors are still quite competent at teaching at 65 and 70 (and some are still so as they get into the upper 70's, but this is much less common), and a few are still doing very good research at this age. So I would suggest 57 is much too low for retirement age.

Actually, I believe it is illegal for universities to have mandatory retirement ages in the U.S. It is definitely possible to have one-time retirement incentives, and I suspect from this fact that it would be possible to have a system where you received a somewhat larger pension if you retired between 65 and 70 than if you stayed on longer. I think this would be a good idea (with maybe very rare exceptions for outstanding indivicuals).

13. **bane**
August 13, 2008

One way of reconciling M Wang's viewpoint on Professors, retirement and innovation from people over 50 is that maybe Professors don't have that much to do with actual direct innovative research? They teach, sit on departmental committees, know how to write grant proposals in appropriate language that funding committees are comfortable with, they sit on funding committees, write funding letters to alumni, engage in networking, etc, all useful jobs that really don't have much to do with being innovative.

Of course that's probably a heretical viewpoint.

14. **bane**
August 13, 2008

I see I was a bit unclear: above I meant to say "maybe under the current system Professors don't have that much to do with actual direct innovative research anyway?"

15. **M. Wang**
August 13, 2008

David,

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16. **ninguem**
August 14, 2008

Wang starts his spiel with "Econ 101" and ends it with his sympathies for the postdocs. I see a contradiction there. Maybe these struggling postdocs are part of the problem. The market is way oversupplied. Maybe people should realize that there are no jobs in theoretical physics and bail out way before they get into the multiple postdoc mill. Isn't it painfully obvious already? What are all these people doing in graduate school? If you are not the next Witten, try solid state, maybe you can get a job at Intel.

17. **David B.**
August 14, 2008

Dear Wang:

The truth is that there is no practical reason to employ all the people who graduate in studies in theoretical physics in academia. If people want to stay, the shortening of positions at the end means that they have to take temporary jobs more often. There is a reason why people do more postdocs in theoretical physics than in math. It is a market effect: there is enough funding for postdocs to stay around a while. If there was more soft money in math, you would have people doing more postdoctoral positions as well. You are also wrong on another count: a postdoc position is not the same thing as an entry level position. Junior professorships are entry level positions. A postdoc is a temporary position with a definite time limit. The review system is there because there is a lot of competition for those positions. This is not the Enron system you are describing.

You can complain about the system all you want, but the following fact remains true: if you do truly outstanding research, you will get a permanent position quickly. This has always been the case and this is a reward for truly innovative research.

My statements about having incentives to keep the best people motivated are still true. You can set up these incentives in two ways: with money incentives, or by giving some protections. There are places where there is no tenure in place and they pay more. If you can outcompete the individuals out of their jobs in those places, go ahead and do so.

The protections that are in place are there for a reason: you can get fired for having the wrong kind of opinion, and you can politicize positions to a degree where innovative research is essentially impossible. This is a more common danger in the humanities, but it is not unheard of in sciences, especially if you are going against a well entrenched belief of some community. This is, you would not give protection to dissenters who might be right. You can not base a robust system of academic dissent on utopian assumptions about the way individuals behave, so you have to put mechanisms in place to prevent certain abuses. The tenure system is one of those mechanisms. It might not be perfect, but it works sufficiently well and so far it has worked better than the alternatives. On my part, I have no illusions about idealism on this regard.

You also fail to understand that there is a lot of risk in doing research: most of the times it does not pay off, but when it does, it pays big. There is some element of luck in this. Sometimes it takes years to actually get progress done. If you are measured by Wall Street style standards (quarterly profits, etc, the same thing as recent citation counts) or any short term style metric, a lot of innovation goes down the drain. This is why most of the fundamental research is handled by public investment rather than private investment.

18. **M. Wang**
August 14, 2008

Dear David,

You speak like a true beneficiary of the status quo. The system works great. Those who suffer are not as good as I am and therefore just sour-graping. My job should be secure no matter what. There cannot possibly be any justification for contemplating any changes. Guess what? I read corporate earnings announcements for a living, and must have seen similar arguments a hundred times over. Unfortunately, these arguments inevitably come from failing executives who have cost their shareholders billions and are on their way to the guillotine.

I can pick apart your arguments one by one. For example, physicists do more postdocs than chemists or EE because the field of physics has more money!? You must be living in one of the other universes. But anyway, these details are not really essential to the issue here. Whether or not the system needs reform ultimately comes down to whether it works well, i.e. does it generate good returns for the investors. If the firm is failing, we don't care if the CEO is Jesus reincarnated. Out of the door he goes. If the firm is booming, the managers can be caught stealing money from the company (Think Steve Jobs' backdated options grants at Apple) and still get a big round of applause.

Had theoretical particle physics been a private enterprise, what would its annual report look like? Well, that is actually a trick question, because no companies would not be producing an annual report after burning through billions of dollars of taxpayers' and donors' money in the past few decades and generating nothing but hot air. It would have been forced into bankruptcy a long time ago.

So, the basic fundamental difference between you and me is that you think the field works great while I think it has failed completely in the past three decades. As long as we come from these opposing premises, there can be no agreement in our conclusions.

I am not insider. My involvement with physics is basically that of an activist shareholder. I become a shareholder of the field by way of donation and tax, which in a good year is enough to pay the salary of a professor for many years. In all honesty, I am offended by a member responsible for the failure of the field who has the gumption to tell me that he knows how to judge the quality of young incoming practitioners. Don't you see that it is the tenured professors who need to be judged here?

19. **David B.**
August 14, 2008

Dear Wang:

I've heard a lot of rhetoric through my life, and you are spouting quite a lot of it. Let me set the facts straight for your benefit:

Theoretical physics is dirt cheap. It is in experimental physics that the billions of dollars are spent. And they have a lot to show for it. Most of what is currently known as the Web came out from the solutions that were worked out by experimenters that needed to be able to access data all over the world for experiments in particle physics. You should read the Wikipedia article about the origin of the WWW. I don't know how much of the world's Economy runs on the web, but if I were you I would stop making ridiculous statements about the value of the physicists contributions to the economy.

Lasers where a theoretical conjecture of the 1930's. In the 60's the first laser was built. Now, many technologies that people use depend on lasers, and there is a comparable number of working lasers to the numbers of people in the world. It can take 60-90 years for ideas to mature.

Many things that start their life in physics labs end up being engineering problems afterwards that produce enormous amounts of wealth. All of this is byproduct of fundamental research.

From many points of view, theoretical physics is there to help understand the experiments that are performed all over the world and society has benefitted directly from that. A true understanding of physics requires theory, and the corresponding experts are theoretical physicists.

The most important contributions of modern theoretical particle physics (last 30 years) have been related to establishing the standard model of particle physics as a law of nature, matching the model to data to unprecedented accuracy. Puzzles of how the neutrino flux from the sun was off led to extraordinary experiments that showed that neutrinos have a mass. This was a direct contribution from theory to the understanding of nature and I would say that is pretty impressive considering how messy the sun's nuclear reactions are.

There have also been enormous improvements in the understanding of various mathematical structures from studies in theoretical physics, particularly with the insight of techniques developed in the string theory community.

Need I say more? Probably I do, because you seem to believe that theoretical physics is a bunch of nonsense practiced by a coven of evil tenured professors who are full of hot air and decide by finger what goes on.

Theoretical particle physics has not failed. A lot of ideas have been falsified by data however. But such is life in research: it is the norm rather than the exception.

Also, nowhere have I said that my job should be secure no matter what. But if you ask me to take risks, then the pay should match the risk expectations. It is the same as with interest rates in banks.

If the tenure system cuts down productivity by 2%, but it is 30-40% cheaper, most people would favor the tenure system. If you come up with a better alternative, I'm all ears.

Go ahead and pick at my arguments one by one. I would like to hear a well documented research monograph on why the tenure system is so corrupt, rather than a lot of posturing and an attitude of "I do understand economy and you don't". Trust me, I do understand it well enough.

The reason I posted the 'apology' for tenure, is that there are good reasons for the system to be in place. If I quote those reasons as a matter of an explanation, that does not mean that I refuse to see alternatives or that I am just supporting the status quo just because I'm an insider. Your arguments are not convincing because you are not proposing a real alternative.

You maybe felt insulted because I answered your post with a differing point of view? I don't know. What I do know is that you resorted to name calling and less than civilized conversation. Fortunately I come from another universe where we don't take the bait so easily. You feel that we are not judged and reviewed? Trust me, the tenured professors are judged by their piers and by the granting agencies. The grant contracts are allocated according to how people's output in research is perceived. Also, I do have the capacity to judge my junior colleagues. As a matter of fact, it is part of my job description, and I take it very seriously.

Sincerely yours,

Prof. David Berenstein.

20. **M. Wang**
August 15, 2008

Prof. David,

This discussion is getting too personal for my comfort, so I will try to conclude it with very brief statements of facts.

I was talking about THEORETICAL particle physics in the past 30 years, not experimental, not solid state, not math and not the 1930s or 1970s.

The WEB came out of CERN. -> Experimental.

Laser prediction -> 1930s.

Physics lab Technology. -> Experimental.

Standard Model. -> 1970s.

String Math. -> Not physics.

Solution of Solar Neutrino puzzle. -> Experimental.

As for working with experimentalists, I would be more sympathetic if nonzero neutrino mass and/or small positive cosmological constant have been predicted before discovery. Instead, they cause consternation. (Clarification: All those papers published without nudging us closer to the right answer are considered negative contributions by me, regardless of their very positive contributions to the authors' careers.)

Rigid labor market reduce the ANNUAL GROWTH RATE of productivity by 1-2%, not the productivity itself. The overall growth rate can turn negative in extreme cases, as I suspect TPT is one such case.

When I talked about judgment, I meant objective or at least disinterested 3rd-party judgment. A community of tenured professors judging themselves does not qualify.

My complaint about the tenure system is largely focused on TPT, as my original post indicated. The other fields, well, have produced results, so they can keep doing whatever they were doing with public money.

21. **M. Wang**
August 15, 2008

By the way, I do think TPP is under-funded. No real revival will come unless more money is invested. But no more money will come unless TPP produces some real results first. I hope LHC will lead to such real results and allow the community to break out of its 30 year torpor.

Despite my outsider status, I do wish the field well. TPP is close to the ultimate intellectual pursuit in my mind, but any collective human endeavor has to be subject to the laws of economics and TPP is no exception.

I have had some really terrible managers and have been a bad one myself, at least initially. I know how hard it is to manage people. Realistically, the tenure system will not change, but the tenured professors, if they want, can easily take some one or two weeks courses at the B-schools to learn the art of management. I found the experience silly at times but very helpful overall.

Another suggestion is for the decision makers of the field to consciously promote young people NOT in their mold. I don't mean nasty personalities and out-of-the-world behaviors, but conformity leads to inbreeding, which in turn leads to degeneration. This seems to be one of the causes of TPP's current problems.

22. **DB**
August 15, 2008

There are two issues which affect mathematical physics:

1) As a discipline it is amorphous. The jobs are spread around maths, physics and even engineering departments. As such it lacks a clear identity and is often viewed with suspicion by members of the host departments. Until recently they were accepted because of the stunning record of achievement of mathematical physicists up until the late seventies.

2)The shelf life of a researcher in mathematical physics is shorter than in any experimental field. It is well established that there is very little prospect of anyone over the age of 45 making any significant breakthrough. Even the greatest names were effectively burnt out by this age and were relegated to pursuing unproductive research paths while training the younger generation.

The problem is particularly acute in String Theory, where an entire generation of theoretical physicists now occupy tenured positions. No longer productive, and fearful that their entire research career may have been in vain they grimly hold on in the hope that a new generation will make the breakthroughs that will validate their own work. Their counterparts in maths and physics departments view them with increasing resentment. In return, string theorists feel increasingly under siege and band together even more strongly.

Either a breakthrough comes or this doesn't end well.

23. **gs**
August 15, 2008

Dear M. Wang,

"..while I think it [theoretical particle physics] has failed completely in the past three decades."

Why do you feel that the field has failed in the past three decades? What in your mind would count as a success?

24. **David B.**
August 15, 2008

Dear Wang:

I agree that it was getting personal. It was exactly for that reason that I gave my full name. I believe anonimity kills many discussions and invites trouble.

I am definitely open to new ideas and suggestioons as to how to improve the system. I also appreciate the notion of expert advice. It is ill-informed to believe that the merits of individuals in our fields of knowledge should not be measured by the experts in the field, but that an outside committe who does not have experience in the field will know better. A proper answer that addresses this issue of judgement should be a lot more nuanced and should provide a realistic set of rules for implementation, rather than what you described.

The truth is that in order to make progress in physics in terms of possitive results (as you called them), one needs new experimental data that needs to be explained. If there are no prospects for new experimental data ever, then the corresponding theory fields die. The reason funding has not stopped in physics, is beacuse the positive contributions to technology from physics research have not stopped happening. In this situation, you keep on funding areas that have promise, but there are never guarantees on results. This is a situation where in order to win the lotery you buy as many tickets as you can, and so long as the ones that pay do it big, you keep on doing it. If there was a way to decide in advance which tickets are loosers for certain, then people usually wouldn't buy them. However, such a thing is not there and history has shown that betting on fundamental research has been a good bet so far.

Physics essentially moves forward when new technologies make it possible for people to observe new phenomena. It is a sad thing that we don't celebrate the technical ingenuity of experimentalists more. Their results are what have made physics into one of the most successful developments in the understanding of nature that humanity has ever seen.

In high energy particle physics we have had a lack of new experimental evidence for new stuff other than the standard model for many years.

The addition of neutrino masses to solve the solar neutrino flux problem was one of those cases where there was a puzzle and a theoretical solution. The solution predicted various new phenomena that were later observed experimentally and parametrized them. These were seen later, and the parameters are being measured. Thus, it was verified. This is the way progress usually happens. There are very few cases where somehting started as abstract theory than was then seen.
Historically theory has followed data and not there is a feedback mechanism that makes theory give new ideas of where to look for stuff.

Also, confirming that a model of nature is correct is an incredible arduous task. Even though we would personally like some deeper understanding of nature, it is presumtuous to assume that we are entitled to it. If and when it happens, we should all celebrate.

Currently, there are experimental hints that supersymmetry might fit the current High energy physics data better than the Standard model.
Supersymmetry is one of those theory inventions that could be seen at the LHC and it would be a triumph of theoretical physics in the sense you described.

As I said above, theoretical particle physics aims to explain results of experiments and it is necessary to understand the data that is already there and to know where to look for new things. Most ideas can be thrown away because they predict deviations from current data that are beyond the experimental bounds.

Knowing where to look is especillay important in an experiment like the LHC where the output of information makes it impossible to keep all events that are produced. Designing the triggers so that one does not throw away the new physics requires an enormous amount of theoretical input and experimental know-how. In the end, we might come out with empty hands. But this is the nature of scientific research.

Another myth is that string theorist are no longer productive. The ideas of string theory, and in particular the gauge/gravity correspondence are being used to explain and analyze the data of the heavy ion collisions at RHIC. They also provide good models for the hydrodynamic behavior of strongly interacting systems and can give new models for solvable systems that might be good at describing condensed matter systems that we have already observed in the lab. Very little of this information makes it to the newspapers, but it is all happening as we speak.

The other myth is that string theorists do not promote diversity of thought or of ohter approaches. Some do and some don't. Over-generalizations and demonizing a whole community does not help towards having productive discussions.

25. **M. Wang**
August 15, 2008

GS,

As I mentioned, if the TPP community had come up with a cohesive theory about the neutrino masses or the small positive cosmological constant before the experimental discovery, it would have counted as a big triumph in my mind. Even after the fact, such a theory would still have been respectable and well worth the public investment.

At the next rung of lowered expectation, something coherent to replace the ridiculously pointless SUSY extension in Higgs physics would be nice. But I am a realist. Chances are the theorists of this generation will not come up with anything remotely close to the truth until the truth scream in their faces from LHC.

26. **M. Wang**
August 15, 2008

DB,

I want to apologize for the use of the phrase "econ 101" in one of my earlier post. It was unnecessary and counterproductive.

I meant to write in a blunt manner, but that comes across as rude, which was not my intention and I regret it.

27. **Aaron Bergman**
August 15, 2008

if the TPP community had come up with a cohesive theory about the neutrino masses

Huh? The theory behind neutrino masses was completely developed long before said mass was detected.

Also, to say that supersymmetry is pointless is silly. It may well not be right (and it looks less likely than it used to), but SUSY has a number of independent motivations (none of which have to do with string theory FWIW) that made it the most popular extension of the SM out there.

Anyways, theory preceding experiment is a nice dream, but it's not how things usually work. Rather, the experiments come first and theories develop to explain the data. It is an unfortunately disadvantage of the past few generations of theorists that we haven't had data to play around with. It'd be great if we could discern the secrets of the universe through thought alone, but the number of people who have managed to do that in history can probably be counted on one hand.

28. **M. Wang**
August 15, 2008

David,

I am criticizing the entire TPP community from a sociological/economical point of view. Nothing I said should be taken as personal.

I do not consider the idea of neutrino mixing as a comprehensive theory because it is not well integrated into a large framework.

String application on RHIC physics is positive but unfortunately minor. Overall, the return on investment in String remains miserably low.

The failure to promote a diverse set of talents is universal. It is so deeply ingrained in human nature that even well-meaning people may subconsciously favor young people who remind them of themselves. But tenured professors wield too much power over the postdocs and doctoral students, and many become so arrogant that they laugh at the idea of diversity. Senator Fulbright's famous sentence, "Power confuses itself with virtue and tends also to take itself for omnipotence." definitely applies to some big-name physicists.

My biggest problem with the tenure system is exactly the concentration of power of the old over the young. This is basically a carryover of the medieval apprentice system. Putting aside to fairness issue, one still has to consider its effects on a field facing strong headwind already.

My feeling is that tenured professors in TPP have only one thing in common: They were smart physicists, at least at one time in their careers. Otherwise, they are not guaranteed to have good tastes, much less real-life wisdom. To give every single one of them so much power over aspiring young scientists is truly not prudent.

To be a good manager, one has to have that wisdom. Humility and broad view outside the field also help. Unfortunately, these qualities are exactly those that are suppressed in the selection process of a successful physicist, where narrow-mined focus and unrealistic confidence (in self or in the adviser) are the necessary ingredients to make it to the top.

Therefore, it was not surprising that TPP community has gone over the string cliff like rats following the pied piper. Here the pied piper role is played by the de-facto leaders of the field in the past three decades: Susskind, Weinberg, but most important of all, Witten.

I have never met Dr. Witten personally. From what I have heard, he is a brilliant theorist and a nice guy too. Unfortunately, as a leader, his failure was total and complete. Certainly, he holds no official title of the kind and most likely does not want the leadership responsibility. But there is no denying that almost everyone in TPP thought of him as the leading light from the mid-80s to mid-2000s. And leadership is something that one simply cannot shirk at will.

In wielding his enormous unofficial power over the flock, Dr. Witten failed to promote the diversity we talked about. He failed to suppress the idol worship that became more and more obvious and unhealthy over the years. In fact, he failed to recognize any sociological aspect of the leadership task befalling on him and did nothing to guide the community onto a more productive direction. For this failure, I consider him the most responsible for the current miserable state of TPP.

Finger-pointing is not a very productive exercise itself. But I am surprised that nobody in physics ever utters this obvious truth, not even Woit or Smolin. Facing the truth, however, is often the first step toward progress. So there you have it. I have spoken the unspeakable. If anyone wants nominate other candidates as the guilty, I am all ears.

29. **M. Wang**
August 15, 2008

Aaron,

Please read my last comment about why neutrino mixing is not really a triumph for the theorists.

I presume that you were referring to the hierarchy problem as one of the "independent motivations". But this only makes sense if there is a natural way to break SUSY at much-lower-than-Planck scale. Does such a way exist?

You are absolutely right in saying that it is rare for theory to precede experiments. But knowing this fact, how can the whole field spend the past 30 years building conjectures upon previous conjectures and extending the whole process to the 6th or 7th power? If SUSY itself is a questionable extension, why waste time building String other than to pad one's publication list?

Where is the first-order conjecture that has at least a 50-50 chance of being true?

30. **jpd**
August 15, 2008

I am no string theory fan, but i don't think you can blame
it all on Witten.
He's just this guy, you know?

31. **Aaron Bergman**
August 15, 2008

Neutrino mixing a triumph? I don't know about that. Neutrino masses are pretty much obvious from the point of view of effective field theory. There's an operator you can write down, and you expect it to be surppressed by the inverse scale of some new physics, and voila, that's what you get. The seesaw mechanism goes back to the seventies, I think (although I'm very bad with physics history), long before the detection of neutrino masses.

As for SUSY, the hierarchy problem is certainly a motivation. So are the existence of a natural WIMP candidate, the unification of the couplings, ewsb, and maybe other things I'm forgetting. By themselves, none of these are at all conclusive, but if you take them all together, it's not too hard to see how people got excited.

As for natural breaking of SUSY at small scales, this is hardly a new question. See this, for example. (If you're not at a university, it's not too hard to find a copy on the web.) Anyways, things have progressed considerably since then. Are there still problems with SUSY? Of course, but I hardly think that means that one should discard the entire idea.

32. **wilbur**
August 15, 2008

> But knowing this fact, how can the whole field spend the past 30 years building conjectures upon previous conjectures and extending the whole process to the

6th or 7th power?

This does not make any sense to me. We all know that the SM has been a phenomenal success. But we wouldn't know that if not for the work of the particle physics community, both experimenters ant theoreticians, over the last 30 years.

So, how can one say at the same time that the SM is successful and that the past 30 years have been spent by the whole community building speculations? I think Mr. Wang is seriously confused about the nature of particle physics.

33. **Peter Shor**
August 17, 2008

Does Witten really consider himself the leader of the string theorists? My impression is that he thinks of himself as a really smart guy looking at interesting things, but not as somebody who sets the direction for the entire TPP communicty.

34. **M. Wang**
August 17, 2008

Aaron,

See-saw mechanism is not just old but also isolated from the overall theoretical framework. There has been very little effort on integrating it with the rest of the beyond-SM project.

The Witten paper you quoted is a standard string effort. That is, all it says is that dynamic breaking of SUSY is not impossible in string. But the fact that anything is possible with string is something we already know without having to waste any time.

When the right idea comes out, it should link different pieces together in a previously unimagined and natural way. TPP practitioners are so ruined by string that they no long care about this any more.

Wilbur,

The great majority of the progress in particle physics during the past 30 years is in experiments. What did theorists do that contributed to the verification of the SM, which was already 99% complete by the mid 70s?

Peter,

Witten certainly did not want to be the leader, but that was the problem. People still treated him like one. The weight of responsibility did not find the shoulder that could or would carry it.

35. **Eric**
August 17, 2008

Wang,
My memory could be wrong, but it seems to me that the idea for the see-saw mechanism comes originally from grand unification, in particular SO(10)-type unification. In fact, one of the reasons that SU(5) GUTs are disfavored (besides proton decay) is that they do not have a singlet for the right-handed neutrino. If neutrinos had turned out to be massless, then this would have almost certainly meant that the SM came from an SU(5) GUT, and proton decay would probably have also been observed.

Regarding your claims about a supposed lack of progress in theoretical particle physics over the last thirty years, nothing could be farther from the truth. What has generally been lacking is the tools necessary to fully test ideas such as the Higgs mechanism and supersymmetry. In more recent years, other ideas such as warped extra dimensional theories have been proposed which will also be tested at LHC alongside older ideas such as supersymmetry and technicolor. The only way that any of your above arguments can possibly have any merit is if there is no evidence for any of these ideas at LHC. As David mentioned earlier, all evidence at present seems favor low-energy supersymmetry.

As an aside, I don't believe the paper of Witten's linked by Aaron above was written within the context of string theory as it was written in 1981.

36. **gs**
August 17, 2008

Dear M. Wang,
A question for you, just to get an idea of what you expect from physicists… phenomenologists and theorists have studied a variety of scenarios for TeV scale physics, e.g. SUSY, technicolor, composite Higgs, extra dimensions, etc. If upcoming experiments end up supporting any one of these scenarios, would you still consider the efforts of the past 30 years to be a failure?

37. **Thomas Larsson**
August 18, 2008

One way to quantify the amount of recent progress (or lack thereof) in theoretical physics is to look at the age of the youngest theory Nobelist around. At this time it is Wilczek, who is around 55. I don't think that there ever (after 1902) was a time when the youngest was that old, and made his discoveries so long ago.

38. **Thomas Larsson**
August 18, 2008

"If upcoming experiments end up supporting any one of these scenarios,"

gs, isn't this is a quite big IF, given constraints from the Tevatron and elsewhere?

39. **Eric**
August 18, 2008

Dear Thomas,
Once the results from the LHC are in, it is quite likely that many Noble prizes will be awarded to theorists, e.g. Higgs, Zumino, and so on depending on what is found. Theories have to be confirmed by experiment before Nobel prizes can be awarded.

As far as whether or not it is likely that something will be found at LHC, the answer to that is almost certainly. Either the Higgs will be found or something equivalent to it. If the Higgs is found, then there must be something which stabilizes its mass against quadratic divergences. Whatever the case may be, it should be seen at LHC. In the case of supersymmetry and the Higgs, it is hardly surprising that no signal (above background) has been seen at the Tevatron as the beam energy is just not high enough to produce large numbers of events.

40. **wilbur**
August 18, 2008

> The great majority of the progress in particle physics during the past 30 years is in experiments. What did theorists do that contributed to the verification of the SM, which was already 99% complete by the mid 70s?

Mr. Wang,

your statement that the verification of the SM was complete by the mid 1970s is completely ludicrous. What was complete by then was the formulation of the SM. Notice that the weak bosons where first experimentally observed in 1981/1982, so no direct verification of the SM could have taken place before that.

You seem to believe that after the SM was formulated only experimental work remained to be done. That is wrong. You fail to realize that the theoretical work needed to *quantitatively* match the experimental results, and also to guide the experimental effort, is gigantic and absolutely non-trivial. And, since you don't know it, it's first-rate physics.

To give you just two obvious examples: the computation of QCD scattering amplitudes *at tree level* is an active subject of research even today. (And one, by the way, which has received inputs from string theory.) Another example is the verification of the CKM model of CP violation, to which an enormous amount of theoretical work has been, and still is, directed.

If you want to see literally hundreds of other equally relevant examples just take a look at the current literature. Take, for example, the areas of hadron spectroscopy, jet physics, deep inelastic scattering, electroweak boson scattering, flavor physics, …, even QED, … the list is virtually endless. That is what is called particle physics.

According to your wrong idea of how physics research is conducted, one should say that, for example, condensed matter physics is a purely experimental field, since the Schroedinger equation has been known since the 1920s. Obviously, that's not how physics works.

41. **Thomas Larsson**
August 19, 2008

Eric, do you really think that Higgs or Zumino is below 55? 🙂

It is standard lore that it is impossible to find nothing at the LHC, because of the Landau pole. That's why nothing would be so exciting – it would make LHC the most important null experiment since Michelson-Morley. I wouldn't bet against a Higgs, though. On second thought, I might risk a hundred bucks if Sean Carroll still offers 19:1 odds.

However, yours is a stronger claim: that just an SM Higgs is also impossible. It will be interesting to see how that prediction pans out in the next few years.

42. **M. Wang**
August 23, 2008

Wilbur,

Your points are well taken. But how many TPP papers published during the past 30 years have been on the subjects you listed? How many were not? Is the ratio around 1/2, 1/5, or closer to 1/20? If the bulk of publications fall into categories that you would rather not mention, what does that say about the overall progress in TPP?

43. **wilbur**
August 23, 2008

M. Wang,

I don't have statistics on the last 30 years of theoretical physics papers, so I cannot give a definite answer to your question. I don't know whether detailed studies of that kind actually exist.

Like I said in my post, the topics I mentioned were just examples, not an attempt at a complete classification of particle physics subfields. For example, I didn't mention lattice QCD, where much progress has been made in the last two decades.

All I can say is that if you're trying to prove that SM physics has been a marginal field in theoretical particle physics for the last 30 years, you're going to have a very hard time doing so.

Grothendieck's Later Years

August 8, 2008
Categories: Uncategorized

Alexander Grothendieck's 80th birthday was this past March, and the September Notices of the AMS has several articles about his later years. There's a long piece entitled Who is Grothendieck?, by Winfried Scharlau, who is writing a three-volume biography. The first volume (in German) is available here and mainly deals with the stories of his parents. The article contains the only pictures I've ever seen of the post 1970s Grothendieck and a wealth of information about his activities after leaving the mathematical research world.

The same issue contains a short piece Memories of Shourik, by Valentin Poenaru reminiscing about his friendship with Grothendieck during the 1960s. The most shocking thing in it to me was actually the part about Barry Mazur's wife, who Poenaru describes as being only 17 when he met her living with Mazur at Bures-sur-Yvette.

Note added: Mazur's biography here mentions just one wife, Grace Dane, a Harvard biology postdoc he married in 1960. If she was, as Poenaru claims, 17 in 62-63, that would have made her a 15 year-old postdoc when they married….

Finally, there's a piece by Allyn Jackson about Grothendieck and the IHES, which is having its 50th birthday this year. Evidently Grothendieck has recently been in communication with the IHES:

> Six months to the day before the start of the IHES anniversary celebration, Grothendieck wrote to the institute with a request for books. The IHES sent him the books as quickly as it could. But the exchange of letters between Grothendieck and the IHES administration culminated in his writing a furious "open letter" recounting his view of the exchange, which he took as deeply insulting towards him. He requested that copies of the open letter be sent to all members of the IHES Scientific Council and explicitly states that this letter is public (though he also says he will make no efforts on his own to publicize it). Having seen the open letter, I can say that it conveys an extreme outrage that indicates how difficult it would be to conduct reasonable communication with him.
>
> At the same time, the open letter reveals the vivid personal tie that Grothendieck clearly still feels to the IHES. The letter also reveals an isolated individual who is reaching out in the only way he is able. In one place he speaks of his open letter as being a letter of farewell ("adieu") to a world with which he no longer has anything in common. He ends on a note of apocalyptic foreboding, saying "that the time is near when…this letter, this cry will be known by all. In a world of the living." This cry does not seem to concern misunderstanding over his original request for books. Rather, it speaks of anguish in the heart of one of the great mathematicians of modern times.

The IHES is having *Recoltes et Semailles*, Grothendieck's long meditation on mathematics and his withdrawal from it, published this summer. Articles about this are beginning to appear in the French press, see here and here.

Comments

1. **egan**
   August 8, 2008

   The real link for the science et avenir article is http://sciencesetavenirmensuel.nouvelobs.com/hebdo/parution/p737/articles/a378150-.html

2. **Peter Woit**
   August 8, 2008

   Thanks egan, fixed.

3. **Michael Bacon**
   August 8, 2008

   "The most shocking thing in it to me was actually the part about Barry Mazur's wife, who Poenaru describes as being only 17 when he met her living with Mazur at Bures-sur-Yvette."

   Shocking? Really? They were married afterall, so you wouldn't be shocked they were living together. 🙂

4. **Joseph**
   August 8, 2008

   Wow. Thanks for the info. Ever since I started studying algebraic geometry, I have been absolutely mesmerized by Grothendieck.

5. **Peter Woit**
   August 8, 2008

   Michael,

   See my added note. Something about the Mazur story doesn't add up…

6. **Chris W.**
   August 8, 2008

   You might have mentioned that Barry Mazur was only 25 at the time. Better yet, you might have omitted mention of that sentence altogether.

7. **MathPhys**
   August 8, 2008

   Peter,

   I agree with Chris W. Maybe you can remove that sentence altogether. Someone made a small mistake somewhere about someone's age.

8. **commonsense**
   August 8, 2008

If I said Grothendieck looks 100 if he's a day, does that imply he got his Fields Medals when he was in his 50's. (Answer: no.)

9. **Peter Woit**
   August 8, 2008

   commonsense (and others)

   Poenaru wrote that Mazur's wife was 17, not that she looked 17. This is rather odd, so I mentioned it. If you don't find it worth mentioning, ignore it.

10. **Martin R.**
    August 9, 2008

    I also find it surprising that you quote this sentence about Mazur's wife. Who cares ? It's not your style. I am a French physicist who read you blog regularly with pleasure and interest. Without knowing you directly, I got the impression that you are a sensible and honest man. So please, leave this kind of remark to those in the US who think that sexual attraction is the source of all evil…

11. **DPG**
    August 9, 2008

    A. Grothendieck: Should we continue scientific research?
    see http://bareglow.blogspot.com/2008/07/should-we-continue-scientific-research.html

    on my blog dedicated to "Basic research in a global world"
    http://bareglow.blogspot.com

12. **Peter Woit**
    August 9, 2008

    Martin R.,

    My comment had nothing to do with any moral judgment. Perhaps the problem here is a poor use of words. All I meant to say is that I found this to be surprising and hard to believe. As far as I can tell, that reaction was correct, since it appears that the story is not true.

    Everyone,

    Unless someone actually knows the explanation for Poenaru's odd claim in the article, enough about Mazur's wife.

13. **Martin R.**
    August 9, 2008

    Ok, sorry for the misunderstanding.

14. **richard**
    August 10, 2008

    Where is it possible to read this open letter, referred to in the quoted text ?

15. **Peter Woit**
    August 10, 2008

    richard,

    As far as I know it has not been made publicly available.

16. **Russ Van Rooy**
    August 10, 2008

    Anyone is welcome to view my amateurish ,but earnest salute to A. Grothendieck here:
    http://uk.youtube.com/user/russvr

17. **richard**
    August 11, 2008

    it is not so much of an "open" letter then 😉

18. **Kea2**
    August 11, 2008

    The "International Who's Who of Authors…" entry for "Mazur, Grace Dane" reads in part: Born 22 April 1944; m. Barry C Mazur; BA Harvard College 1971; Ph.D. Harvard University 1981.

19. **Professor R**
    August 11, 2008

    Bures-sur-Yvette? I haven't heard that name in a long while.
    I spnt a year as a child in Bures, while Dad took a year's sabbatical with Louis Michel. It was a wonderful year for the whole family, and indeed for all the families of visiting professors.
    We all lived in a little Residence specially for the familes of visiting academics, and went to the local french school (excellent) and music college (excellent).
    Theoreticians have such a wonderful life, who else gets to do this…

    Bures seemed like a small, delightful village nowhere in particular- it was only years later that I realised it is host to a world-famous Institute , and right in the middle of several top-class universities and institutions

20. **Howard**

August 12, 2008

OK, so Kea2's research indicates the answer to your puzzlement: Ms. Mazur was not a post-doc at the time she married Mazur. In putting together the Poenaru memoir and the biographical note about his wife, you committed the common logical error of "post-doc ergo propter doc".

21. **D R Lunsford**
    August 14, 2008

    Reading this, I am sadly reminded of Cantor.

    -drl

22. **kevin**
    August 18, 2008

    what does "shourik" mean?

23. **Anonymous**
    August 30, 2008

    In Russian, Shourik to Alexander is like Bob to Robert in English.

24. **Eugene Stefanovich**
    August 30, 2008

    Alexander -> Alexasha -> Sasha -> Sashoura -> Shoura -> Shourik

The Landscape at CERN

August 14, 2008
Categories: Uncategorized

The current plan at CERN is to celebrate my birthday by trying to circulate the first beam around the LHC on September 10 (actually my birthday is September 11, but in recent years my family, like CERN, has tended to celebrate on the 10th, feeling that 9/11 has too unfortunate connotations). One can follow this at a special web-site set up by CERN called LHC First Beam, which now is running a daily countdown.

String theorists have been flocking to CERN this summer trying to somehow connect their subject with the LHC. The big yearly Strings 08 conference will be starting there next week. The organizers don't seem to have a lot of sympathy for anthropic pseudo-science, with no talks scheduled on anthropics, the landscape, or the multiverse. Instead they'll be wisely sticking mostly to talks on topics related to better understanding more formal issues in string theory and quantum field theory.

In the weeks leading up to the conference though, CERN is hosting a theory institute on String Phenomenology. The web-site of the institute has a section on its "Scientific Case", which, with remarkable chutzpah makes the claim that:

> … the past few years have provided a drastic improvement on the potential for string theory models to be confronted with low-energy data

a claim that is diametrically opposed to reality.

For a look at the reality of what the landscape has meant for the "potential for string theory models to be confronted with low-energy data", one can take a look at the slides of talks by Wati Taylor and Michael Douglas. Taylor describes the importance of distinguishing two possibilities he calls A (anything goes) and B (constraints), and finds in IIA intersecting brane models that the evidence favors A. It seems that such a landscape can give one pretty much any kind of low energy physics, with the things one can compute (the gauge group and number of generations) randomly and independently distributed. He looks for some hope in cosmology, noting that a large class of IIA models are incompatible with slow-roll inflation, while at the same time also pointing out that there are lots of potential ways around this particular constraint, although most of them involve uncontrolled approximations.

Douglas's talk was entitled String Landscape: A Status Report, and in it he describes evidence for the existence of order 10^{500} "quasi-realistic vacua". If you don't impose some constraints from experiment, you'd have an infinite number of possible vacua. He claims that there is no way to rule out any of these vacua, other than to try and compute detailed predictions of each one (something no one has a clue about how to do). Douglas explained why it isn't possible to make even the crudest prediction that initially he and others had hoped for, that of whether the SSYM breaking scale would be at observable or Planck energies. There's an odd speculative section about how since SU(2) and SU(3) have shown up on energy scales of 100 MeV to 100 GeV, maybe one gets two new gauge groups for every factor of 1000 in energy (which he calls the "jungle scenario", as opposed to the conventional "desert scenario"). There's a final "No Conclusions" section, admitting that "at this point it seems likely that we will not have definite conclusions of predictions before LHC data comes in." He ends with the standard piece of wishful thinking that now is all that is left of the project of connecting string theory unification models with physics:

> Let us hope that discoveries here at Cern will reveal enough about the real world to make contact possible.

Yesterday there was a discussion session on "the string theory landscape and its impact on particle physics and cosmology", but it doesn't appear to be online. I wonder what conclusions the participants reached…

Update: Jester at Resonaances has a posting up about this. His impression of the Vafa et al. recent claims about "F-theory phenomenology" seems to match mine:

> For the neutrino physicists the important piece of information is that the neutrinos are Dirac or Majorana and their masses are roughly of the order of what is observed. I heard some sceptics saying that back in the old days phenomenology meant a different thing, but such grumbling should not be taken seriously.

Comments

1. **mike**
   August 14, 2008

   If the landscape theorists don't want to make predictions, it means nobody has a valid theory that can predict anything. Waiting for the results from CERN is like seeing the test answers before taking the test.

2. **Kea**
   August 15, 2008

   Well the term **jungle scenario** is one good thing to come out of all this discussion, and it could be applied outside stringy physics.

3. **Ptolemy**
   August 15, 2008

   There's no way to rule out an uncomputable number of possibilities for at most one of which there is any evidence of its existence? That's closer to religion than it is to science (only religion just posits one possibility and sticks to it, no matter what). One can imagine all sorts of mathematics that is beautiful, consistent, etc. and has some resemblance or even connection to established physical models, but that does not make such mathematics physics, and the approach of trying to winnow down the possibilities to the one that is right (!) will be swamped when along come Kepler and Galileo.

4. **anon.**
   August 16, 2008

   How old will you be on 9/11?

   I'm just a bit worried about whether you'll have to retire from Columbia University before popular non-falsifiable ideas have kissed the dust, which may take decades. I hope you'll be able to to continue blogging for a long time yet.

5. **Peter Woit**
   August 16, 2008

   anon.,

I’ll be 51 next month. I fully expect some misguided but popular ideas to be around longer than I am, since many of their proponents are younger than me, and seem quite devoted to soldiering on, no matter how bad things get.

Then again, if the LHC sees nothing that supports these ideas, their proponents are going to have a very hard time continuing to be taken seriously by their colleagues. Change will come, by the usual means of younger people choosing to not follow, and older people dying off.

6. **Thomas Larsson**
   August 16, 2008

   Desperately seeking Susy.

   Madonna is 50 today.

7. **dir**
   August 17, 2008

   anthropic ideas are not necessarily pseudo-science, sometimes they have predictions. for examples, the so-called split susy does have testable new particle spectrum; cosmological constant is another example.

8. **Mirek Kozlowski**
   August 18, 2008

   Dear Peter
   We are eyewitness the crash of the so called “string theory”. Now the future results of the LHC(one of the most expensive toy of the string mafia) is also under question.What is the plan B for the experimentalists if the Higgs has greater mass?. Can we ,theorists ,help to overcome the results of the blind faith of the “stringers”?
   PS Thank you for your book. Here in Poland we read it with great pleasure and understanding.
   MK

9. **Peter Woit**
   August 18, 2008

   Mirek,

   I’m glad you like the book, but I strongly disagree with you about the LHC and the “string mafia”. Despite the impression some string theorists try and give, the LHC has nothing at all to do with string theory. Depending on what results come out of it, they may or may not be very helpful in re-orienting the subject away from some of the excesses caused by string theory.

Strings 2008

August 17, 2008
Categories: Strings 2XXX

Strings 2008 starts tomorrow at CERN, with about 400 physicists in attendance. CERN will be providing a live webcast for the rest of us. The timetable of the talks is here. The first afternoon will be devoted not to string theory, but to the LHC.

Those in attendance without their own blogs are encouraged to report on the goings-on by writing comments here when they get bored by the talks. I'll try and watch some of the talks (or at least look at the slides), and use this posting to write about them.

Update: I'm not likely to be up early enough to catch the morning talks on the webcast, but Lubos is, so you can follow his virtual live-blogging.

Update: Live, the conference seems to be suffering from not always being up to the technology of displaying slides on a Mac to the audience. But slides of the previous talks are now beginning to be available here.

Update: After looking through the slides of the talks and hearing a few of them, the thing that strikes me most about Strings 2008 is how little there has been about strings. Particle theory may be moving to a model where the big annual conference is labeled "STRINGS", and speakers make nods of respect toward string theory, but actually talk about something else.

The three big hot topics of the conference are

- the LHC (talks by Evans, Engelen and Buchmuller), which has nothing at all to do with string theory
- New 3d superconformal quantum field theories (talks by Lambert, Maldacena and Mukhi). One motivation for these is that they can be fit into a pattern of dualities, much like the famous 4d superconformal theories that have dominated particle theory research since Maldacena.
- Scattering amplitudes, especially those of N=4 SYM and N=8 supergravity (talks by Veneziano, Kallosh, Dixon, Cachazo, Green, Sokatchev and, to come, Alday). Some of this looks a lot like particle physics from the mid-60s, based on the study of the analytic S-matrix, including the presence of Veneziano. While there has been a lot of progress in studying certain kinds of QFT S-matrix amplitudes in recent years, some of it coming out of string theory, the most dramatic news is that about the possible finiteness of perturbative N=8 supergravity. Remember all those talks you've heard where someone draws a Feynman diagram and a string diagram, then explains how this shows that perturbative QFT has deadly divergence problems due to point-like interactions, while perturbative string theory doesn't? Well, it appears that you can forget about all that now. In a rear-guard action, some speakers point out that you need to understand non-perturbative N=8 supergravity, and maybe this can't be done in a QFT context. Unclear why non-perturbative string theory is supposed to help here, since the only viable non-perturbative version of it is, by duality, a QFT itself…

Looking at the talks that actually are about string theory unification, you quickly see why most people are talking about something else. Ibanez starts off by asking whether string theory makes physical predictions, then claims that it does, with one of them being exactly the reason it doesn't make predictions about physics: "There is a large landscape of string vacuum solutions…", which he then goes on to describe. Donagi's talk, about Heterotic Standard Models, was remarkable in how much the situation there hasn't changed since 1985. You can come up with such models with the right quantum numbers (and actually, just about any quantum numbers you want…), but to get anything else, you have to address how to stabilize moduli and break supersymmetry, and Donagi just mentions these problems at the end as tasks to address in the future. For more about the F-theory-motivated models reported on, see the comments at this posting, where "anonymous" has an informed discussion with him (or her)self.

Comments

1. **Thomas R Love**
   August 19, 2008

   Clicking the link to Motl's site, one is subject to a rant which includes
   this:

   "Virtually everything written on the blog you visited a minute ago is nonsense and virtually all contributors are crackpots. The owner of the blog, Peter Woit, a computer administrator and a lecturer in discipline, is trying to revenge to the high-energy physics community for his inability to become a physicist himself.

   You can check that during the last two decades, Peter Woit only wrote 3 rants against string theory and one unpublishable, 0-citation demonization of spinors in quantum field theory, earning less than 10 suspicious citations in total. For comparison, Edward Witten wrote nearly 200 articles during the same period, earning almost 40,000 citations from them. Peter Woit is not a scientist in any sense; he is just an activist. "

   First, your Columbia webpage does say you are a lecturer in discipline, it should be Lecturer in Mathematics, shouldn't it?

   Secondly, Motl has confused quantity with quality. The number of articles written and the number of citations has nothing to do with the quality of the work.

   Third, why do you provide a link to a site which says you (and I) are crackpots?

2. **Peter Woit**
   August 19, 2008

   Thomas,

   I should update my web-page. For one thing, I've been promoted, and my title is now "Senior Lecturer" (the "in Discipline" part isn't very meaningful, the discipline in question is mathematics).

   The links to Lubos's blog are there when there's something interesting on his blog. The ad hominem attacks he puts out on anyone who criticizes string theory I think speak for themselves. He's done (and continues to do) more damage to the public perception of string theory than I ever will.

   Following his stream of consciousness about the talks is actually pretty fascinating. On the one hand, he's giving an extremely competent and expert commentary of exactly what the conventional wisdom is at the top levels of the string theory establishment about currently fashionable research. On the other, he's clearly a complete fanatic and out of his mind. Quite an amazing combination to watch…

3. **PAMELA**
   August 20, 2008

   An interesting result was presented at another conference, about Dark Matter in Stockholm. Preliminary data from the PAMELA experiment show an excess in the energy spectrum of astrophysical positrons. If this will turn out to be the first observation of Dark Matter annihilations, the authors will have to come back to Stockholm for the Nobel prize.

4. **Peter Woit**
   August 20, 2008

   Thanks PAMELA,

   I saw a mysterious rumor about this last week. If anyone knows of anywhere there's slides or some details of this, please let us know.

5. **DB**
   August 20, 2008

   I imagine this is the talk that has got the rumour mill running:

   http://agenda.albanova.se/contributionDisplay.py?contribId=389&sessionId=257&confId=355

   By the way, congrats on the promotion Peter.

6. **anon.**
   August 20, 2008

   The PAMELA slide was shown at ICHEP, but they're publishing in Nature (*exasperated sigh*) and consequently there's an embargo on actually releasing the plot in advance of publication. So as far as I know it's not available anywhere online; you have to talk to someone who saw it. There is a Nature News story about it, complete with crackpot comments.

7. **Shantanu**
   August 20, 2008

   There have been many such claims for "excess" in such
   indirect dark matter detection experiments, but none of them
   are smoking gun. I guess we will have to wait until the paper is out.
   Peter : congrats from me too.

8. **Click**
   August 20, 2008

   "After looking through the slides of the talks and hearing a few of them, the thing that strikes me most about Strings 2008 is how little there has been about strings."

   Peter,

   This is not surprising at all as a lot of people are focused as the most expensive experiments in the history of science is about to be conducted. As far as I see the underlying motivation has come from string theory. I am constantly surprised why you hate the word "string theory" so much.

   However, I would like to thank you for putting good informative links on math and physics.

9. **Peter Woit**
   August 20, 2008

   Click,

   Despite what you have heard, the LHC has nothing to do with string theory. String theory is not the underlying motivation for the experiments at the LHC.

10. **PAMELA**
    August 21, 2008

    indeed there is an embargo: the result of PAMELA was flashed during conferences but the slides cannot be published on-line. I don't know if a photo of the result taken during the talk would violate the embargo.

11. **nbutsomebody**
    August 21, 2008

    Point about N=8 super-gravity is indeed very important. If we can have a finite theory of gravity, then who needs string theory ?

12. **Per**
    August 21, 2008

    Matthias Staudacher just gave a talk about the spectral problem of AdS / CFT. With the help of integrability, it is quite amazing how close this problem is to a complete solution.

    This is actually something which is quite outstanding, and I sometimes get the feeling that you neglect this in your critique (which, in my opinion is often well based) against string theory. If you compare to any research in theoretical particle physics at the moment, would you say anything is as interesting as this? My vote would be that it ties with the N=8 stuff that's going on at the moment (however, if I were to make a bet it would be against N=8 working out and for AdS / CFT being solved (in the planar limit)).

    Best wishes, P

13. **Daniel de França MTd2**
    August 22, 2008

    Any positive reaction to Carlo Rovelli talk? According to Lubos, he was almost ignored.

14. **Peter Woit**
    August 22, 2008

Per,

There certainly has been real progress in the AdS/CFT calculations you mention, and I wouldn't be surprised if sooner or later a complete understanding of the planar limit was achieved, which would be great.

The problem though is that this doesn't really change much, since everyone has been assuming that this works for 10 years now. The N=8 stuff is much more exciting, partly because it is much newer and much more poorly understood. If N=8 finiteness is true, it opens up all sorts of possibilities that had been thought to have been closed off, and these are possibilities relevant not just to solving QCD, but to unifying physics.

Daniel,

Lubos was just watching the webcast, I doubt that from that he could tell much about what the 400 people in the audience thought. Presumably some had the same juvenile thoughts as him, others may have been more interested. As string theorists are careful to point out, there's a wide range of attitudes within their community, with Lubos just one extreme point.

15. **Daniel de França MTd2**
August 22, 2008

Peter,

I know that. But did you, or anyone else too, see the webcast? His coverage of the string subjects was excelent, but not about this one… So, it's like I missed his talk…

16. **Peter Woit**
August 22, 2008

Daniel,

I just caught the first few minutes of Rovelli's talk, then had to leave. Maybe if one saw the question section one might have gotten more of an impression of the reaction of the audience, but I kind of doubt one could really tell much from the webcast.

17. **Per**
August 23, 2008

The talk by Rovelli was one of the talks that received most questions after wards. My impression was that it was very well received and the audience was genuinely interested. After the talk a rather big group assembled around Rovelli and asked him questions privately. This went on for quite some time if I am not mistaken. As part of the audience, it was very pleasing to see that none of the string / anti-string aggressiveness was there.

18. **Daniel de França MTd2**
August 23, 2008

Peter,

Renata Kallosh provided a proof for N=8 renormalizabily, given that there is no anomaly in the expansion. She published it in the same day of her talk:

http://arxiv.org/PS_cache/arxiv/pdf/0808/0808.2310v1.pdf

Do you agree with her? Also, do you agree with the argument that for d>3, it must be tied to strings? Why(not)?

19. **Urs Schreiber**
August 25, 2008

> I just caught the first few minutes of Rovelli's talk, then had to leave. Maybe if one saw the question section one might have gotten more of an impression of the reaction of the audience, but I kind of doubt one could really tell much from the webcast.

One question was if the LQG methods had been applied to 3d gravity and results compared with the information one has about that, including Witten's latest connection to 2d CFT.

(Answer: unfortunately not.)

One question was how one can talk in LQG about black hole entropy without being able to talk about black holes, due to a lack of semi-classical limit.

(Answer: there are two different ways to define a horizon, globally and locally.)

One question was if the advertized "loop quantum cosmology" is really be derived from LQG.

(Answer: no, it is just quantization of cosmological models with some LQG inspired modifications.)

One question was if the problem with the Immirzi-parameter has been resolved, since two different arguments seem to require two different values.

(I think as a reply Rovelli reviewed what the Immirzi-parameter is.)

My impression from watching the webcast of talk and question session: the audience was not ignorant about LQG and might have apprectiated a less introductory talk addressing more of the technical issues. It remains a bit frustrating to see Rovelli using up so much time to explain the bare idea of a "spin network" to an audience that is familiar with the concept of Wilson line and non-perturbative gauge theory on the lattice.

Generally I think: everybody would be happy if a non-perturbative description of gravity using a parameterization of configuration space by Wilson loop observables were available. Lots of stringy work in supergravity is routinely using first-order Lagrangians that encode the Einstein-Hilbert action in terms of a connection. If there is disagreement about LQG, it seems it is not so much about this premise but about technical issues that follow.

20. **Thomas Larsson**
August 25, 2008

"including Witten's latest connection to 2d CFT. "

But that was wrong, wasn't it?

21. **Chris Austin**
August 25, 2008

Hi Daniel,

"Renata Kallosh provided a proof for N=8 renormalizability … Do you agree with her?"

The claim was actually of UV finiteness, i.e. the cancellation of all UV divergences, rather than renormalizability, which means that UV divergences can be absorbed in infinite redefinitions of coupling constants.

It seems to me that there might be a problem with Kallosh's argument, which was based on finding an inconsistency, at the linearized level, between counterterms in Lorentz-covariant gauges and light cone gauge, because the corresponding counterterms appear to be consistent between Lorentz-covariant gauges and light-cone gauge in d = 11, and we would therefore expect the same to be true after dimensional reduction to d = 4.

Specifically we are considering local quartic vertices that are super-Poincare covariant at the linearized level, and in 11 dimensions these were constructed in Lorentz-covariant gauges by Deser and Seminara, in http://arxiv.org/abs/hep-th/9812136 and http://arxiv.org/abs/hep-th/0002241, and in light-cone gauge by Metsaev, in http://arxiv.org/abs/hep-th/0410239.

Metsaev uses a formalism in eleven dimensions where only so(7) invariance is manifest, so as to have a completely unconstrained d = 11 superfield based on an 8 component so(7) spinor, but on dimensional reduction to d = 4, choosing the R and L transverse dimensions to be among the compact dimensions, this should reduce to the Brink, Kim, and Ramond formalism of Kallosh's eqn. (1.5). In fact the superfields in Metsaev's eqn. (2.16) and Kallosh's eqn. (1.5) exactly match term by term, including the powers of p^+, if we reverse the order of the terms in comparing the two papers, and note that Metsaev uses the notation \beta for p^+, see eqn. (2.17).

Metsaev explicitly constructs all the Poincare generators to verify Poincare invariance in d = 11, which should imply Poincare invariance in d = 4, and also finds, in eqn. (5.29) on page 23, the most general local quartic vertex that is super-Poincare covariant at the linearized level, and thus should contain terms built from four Riemann tensors, with possible covariant derivatives acting on them. This should reduce in four dimensions to counterterms such as Kallosh's eqns. (3.3) and (3.6).

Metsaev's light-cone gauge result for the most general local quartic vertex that is super-Poincare covariant at the linearized level, namely that it is obtained from the leading one, that contains the t_8 t_8 R^4 term built from four Riemann tensors, by applying derivatives corresponding to the most general non-vanishing symmetric polynomial built from the Mandelstam invariants, see e.g. Metsaev's formula (5.34), on page 24, is consistent with Deser and Seminara's manifestly Lorentz-covariant construction, and shows that Deser and Seminara obtained the most general such vertex.

To try to pin down further why Kallosh found a Lorentz-non-covariant numerator factor (p^+)^4 from her counterterm eqn. (3.6), we note that Metsaev's local formula (5.29) involves, in effect, four powers of p^+ in the denominator, since \beta_{13} means \beta_1 + \beta_3, from (5.4) on page 21, and \beta_a means p^+ for the a'th leg of the vertex, see e.g. the footnote on page 3, or (2.17) on page 8, or (2.26) on page 9, and the top of page 12. Furthermore, there is nothing hidden in the other factors in (5.29) to cancel these overall denominator powers of p^+, and when we combine with the superfields and the measure factors as in (5.1), on page 20, taking the superfield and measure factors from (3.6) to (3.9) on page 11, to obtain the formula which on dimensional reduction to d = 4 should contain Kallosh's eqn. (3.6), we see that Metsaev's formula contains four overall powers of p^+ in the denominator, which it would appear could cancel the four numerator powers of p^+ found by Kallosh.

Best regards,
Chris

22. **Jeff McGowan**
August 25, 2008

OK, no doubt silly question, but I'm a mathematician, last physics classes I took were GR and relativistic QM around 1984. Is the excitement about the finiteness of N=8 supergravity simply because there is then ONE known finite theory? Assumption then being that if one theory is finite, others which might actually be relevant to the real world (whatever that might be 🙂 might also be finite?

Don't hold it against me that I study Riemann surfaces, really, I don't have anything to do with strings (although I do remember a talk at a conference maybe 15 years ago where the number 26 kept coming up, and one of the questions at the end was "is that the 26 dimensions from string theory?")

23. **Peter Woit**
August 25, 2008

Jeff,

One reason for the excitement is that this indicates there is some new symmetry or important unexplained feature of this theory. If one can figure out what it is, the implications could be very important. Whatever it is, it seems to get around what was always considered a fatal disease of the theory.

The fact that this destroys the main argument always used to justify the idea that to do quantum gravity one must abandon QFT and do string theory is one that string theorists are doing their best to ignore.

24. **Jeff McGowan**
August 25, 2008

Peter,

Thanks. I have some sense of the issues in QFT with infinities, and that this finiteness might give physicists some hope that there might be a way around that. I have the mathematicians caution about generalizing from cases (and about renormalization, I know it seemingly works quite well thank you), but appreciate that it is always nice when you can find a case that works with the hope that it gives you an idea about how to handle the bad case. My joke (lame as it no doubt was) about Riemann surfaces and 26 dimensional string theory, was meant to indicate that I have always had issues with physical theories that are too "flexible" (26 dimensions, no 10, no 12, no 11….).

Interestingly (to me at least), the issues between physics and math which somehow seem embodied in the string theory mess are just silly. Physics and math have always been good for each other, in both directions. I'm not so sure how much really interesting math there is in string theory, although there is clearly some. I owe a very useful part of my doctoral education to a physicist, my orals were explaining Witten's beautiful JDG paper on Morse theory.

Freed on Chern-Simons

August 20, 2008
Categories: Uncategorized

Dan Freed has a wonderful preprint out on the arXiv this evening, based on a talk he gave at the celebration of MSRI's 25th anniversary, entitled Remarks on Chern-Simons Theory. It's mainly about the current state of attempts to better understand the mathematical significance of the Chern-Simons-Witten quantum field theory.

This is a truly remarkable and very simple 3d quantum gauge theory, the significance of which Witten first came to understand back in 1988. He quickly showed that the theory brought together in an unexpected way several quite different but important areas of mathematics and physics (3d topology, moduli spaces of vector bundles, loop group representations, quantum groups, 2d conformal field theory among others). This work was the main reason he was awarded a Fields medal in mathematics in 1990. While Witten and others worked out many important aspects of this story back then, many important puzzles still remain, and it is these that Freed concentrates on.

Perhaps the biggest puzzle is that of how to actually define the theory in a local manner. The standard definition thrown around is that this is just the QFT with Lagrangian given by the Chern-Simons number CS of a connection A, so all one has to do is evaluate the path integral
$$\int e^{i2\pi k CS(A)}$$
While this is a good starting point for a perturbative expansion at large k, it doesn't appear to make much sense non-perturbatively. Freed points out that it is known that the theory must depend on additional topological structure on the 3-manifold (e.g. a 2-framing), whereas the path integral looks like it only depends on the orientation. If you try and think about how you would actually calculate such an integral numerically, by discretizing it and taking a limit, it looks like you will end up with something hopelessly dependent on the details of the discretization and the limit. For a much simpler toy example with some of the same problems, consider the path integral on closed curves on a sphere, taking as Lagrangian the enclosed area.

Freed describes in detail the state of attempts to rigorously define the theory without dealing with the path integral, but instead exploiting the fact that it is supposed to be a topological qft, and thus may have an abstract definition in terms of generators and relations. He describes the current situation as follows:

- There is a generators-and-relations construction of the 1-2-3 theory via modular tensor categories for many classes of compact Lie groups G. This includes finite groups, tori, and simply connected groups, the latter via quantum groups or operator algebras.
- There are new generators-and-relations constructions – at this stage still conjectural – of the 0-1-2-3 theory for certain groups, including finite groups and tori.
- There is an *a priori* construction of the 0-1-2-3 theory for a finite group.
- There is an *a priori* construction of the dimensionally reduced 1-2 theory for *all* compact Lie groups G

The bottom line is that we only have a local construction of the theory for the case of finite groups, where one can make perfectly good sense of the path integral. For the case of a 3-manifold that is a product of a circle and a Riemann surface, one can define things in terms of a 2d theory, and Freed explains the connections to the Freed-Hopkins-Teleman theorem.

To convince mathematicians that there is something to the path integral, Freed writes down the asymptotic expansion for large k that it leads to, and shows that this gives a highly non-trivial conjecture relating quite different mathematical objects associated with a 3-manifold. He shows strong numerical evidence for this conjecture.

Finally, he ends with some extensive and interesting comments about the relationship between quantum field theory and mathematics, as it has been pursued by both physicists and mathematicians over the past quarter-century, with some speculation about what direction this might take in the future.

Comments

1. **James Robson**
   August 21, 2008

   I wouldn't normally have commented here as the level is usually well above me and the discusssion is usually frantically mathematical (or overly personal). But, it seems like nobody else has commented yet so I'll take this opportunity to ask the simplest thing: what does this mean:
   "For a much simpler toy example with some of the same problems, consider the path integral on closed curves on a sphere, taking as Lagrangian the enclosed area."

   Simple as it may be I've forgotten loads of my differential geometry and QFT from college, as without use, maths just seems to evaporate, and would plead an indulgence if there are no better replies t deal with. Does the patah integral relate to the enclosed are via stokes?

   -james.

2. **Knight who says ni**
   August 21, 2008

   Off topic:

   Could you please recommend a good survey of conformal field theory for mathematicians? Something where one could find the main ideas explained in geometric terms, without long formulas with indices all around, e.g. Atiyah's "The geometry and physics of knots" etc.

3. **Peter Woit**
   August 21, 2008

   James,

   Yes, via Stokes you can expressed the enclosed area as an integral over the closed curve. This is well-defined modulo the total area of the sphere (you have a choice to make about what is "inside" the curve, what is "outside", the difference is the area of the sphere)

   This is just the action though. To get a path integral, you are supposed to multiply by i, and exponentiate, getting a phase, then, integrate this phase over the infinite dimensional space of all closed paths. It's this last step which is highly problematic.

4. **Peter Woit**
   August 21, 2008

   Knight,

   One place to start is Gawedzki's "lectures on conformal field theory", in the IAS volume (Quantum Fields and Strings, a course for mathematicians).

Graeme Segal's notes on CFT give a different and quite interesting perspective, definitely few indices there…

5. **mike**
August 21, 2008

Peter,

I have to admit that this one is a little over my head as well, although I seem to remember that path integrals are integral (sorry!) to QFT so just to inquire about your comment,

"If you try and think about how you would actually calculate such an integral numerically, by discretizing it and taking a limit, it looks like you will end up with something hopelessly dependent on the details of the discretization and the limit",

Forgive my lack of understanding if it seems too simple a question, but would it help to use a Taylor series for the exponential and to find an approximation on the discrete terms or does this action through away the important parts of the result?

6. **Peter Woit**
August 21, 2008

mike,

The problem is that the space of closed paths is infinite-dimensional, so you're trying to integrate over an infinite-dimensional space. Normally what you do in such a situation is pick some way of discretizing the problem, so that it becomes finite dimensional, then take the limit as the discretization goes to zero. The problem isn't that you can't do this kind of calculation, it's that the limit (if it exists) is going to depend on the details of how you discretized.

This kind of path integral is quite different than the usual ones. There you can analytically continue in time, and get something with gaussian fall-off, that can be made well-defined. Here if you analytically continue, the phase stays a phase.

As far as I can tell, the situation here, like Chern-Simons, is that some additional structure is needed that will allow a well-defined formulation of the integral.

7. **mike**
August 22, 2008

Peter,

I am not sure how you would make the integral converge, but an alternate guess is that perhaps you can try representing the discrete function as a complex function, like a z-transform. Then, instead of taking the limit as the discreteness goes to zero, you can just look at the causality of the function which produces a region of convergence for solutions that are stable (ROC < 1). This is sort of a "landscape" solution that graphically may show where the bounds of the solutions lie in complex space. If there is convergence on the path integral, I would guess it should be bounded when represented as a complex function, but maybe that's a big guess.

8. **James Robson**
August 22, 2008

Peter,

Many thanks for your reply.

I said I had forgotton most of the maths I once knew – which is true and I regret it – but I did remember the (generalised) Stoke's theorem – honest! It's one of those unifying beautiful results that you get taught and time does not erode. Thanks all the same for taking time to explain things, which you did very clearly.

My real concern was about the path integrals, and I was hoping your "toy" example would help me out. I still remember a patchy smattering of measure theory, but, how would you try to get a measure on the space of all – I guess simple closed curves (?) – on a sphere? What branch of maths helps you out here? (probably "measure theory" Doh!), but, from what I've read, construction of suitable measures for the job is a real problem.

Do you think that, in the long run, these path integrals will become rigorously defined, and, like Newton/Leibnitz calculus, Dirac deltas, differentials /"one-forms", etc, become important branches of maths in the future?

-James.

9. **Peter Woit**
August 22, 2008

James,

My point here is that the problem of making sense of this kind of path integral is ill-posed. I don't think you can do it without adding some structure and changing the problem. This is true both for Chern-Simons and for the toy model. You can change the toy model problem into a QM problem where the answer is clear and given by representation theory. There you find you need to add a kinetic term and fermionic terms to the path integral. I suspect something similar needs to be done for CS, but don't know how to do this.

10. **Hans de Vries**
August 29, 2008

The abelian Chern Simons theory for the electromagnetic field provides lots of useful insights. For example the spin-density of the electromagnetic field, which can be expressed as S = D x A + HV. The combination of this pseudo vector together with the axial Dirac current forms a preserved quantity.

The concept of an EM spin density has yet to trickle down from QFT to the more general EM audience. I devoted a chapter in my coming QFT book to work out two elementary examples:

The EM spin density of an electron with charge and magnetic moment as well as the spin density of arbitrary polarized radiation from an atomic spin 1 transition. Both examples result in what you would expect from an EM spin density.
http://www.physics-quest.org/Book_Chapter_EM2_ChernSimonsSpin.pdf

Regards, Hans
——————————————

Gross on Outlook for String Theory

August 22, 2008
Categories: Uncategorized

David Gross just finished giving the closing talk at Strings 2008, on the outlook for string theory, following a talk by Hirosi Ooguri summarizing the conference. Strings 2009 will be in Rome next June, and it appears that there is a tentative plan to have Strings 2010 in College Station, Texas.

Gross began his talk by recalling his days as a postdoc at CERN in the late 60s, working on an early version of string theory (e.g. trying to extend the Veneziano model). At the time he felt CERN was a great center of theory, but somewhat of an experimental backwater, with the real action he was interested in happening at SLAC. Now, forty years later both string theory and CERN have flourished. CERN is in the process of becoming the single world umbrella lab doing particle physics, driving all the others out of business. Unfortunately, Gross sees the same thing happening in particle theory and seems rather pleased about it, saying that only one umbrella in theoretical physics will survive, string theory, eating up everything else. Except LQG, which he says has not yet been brought under the umbrella, and "we're not sure we want to".

I found this display of string theory triumphalism truly appalling. The fact of the matter is that string theory has failed miserably to do what it was supposed to do, explain unexplained features of the standard model and predict what happens beyond it. Under the circumstances, to claim victory and write out of particle theory anything that doesn't fit under the string theory "umbrella" is completely inappropriate. The message to any young particle theorist from Gross was clear: fall in line with string theory ideology, or there will be no place for you under the "umbrella", i.e. no job for you (the phenomenologists have their own umbrella, you better try that one). The fact that HEP experiment is being forced to consolidate in one lab by economic realities is a really unfortunate one. There is no similar reason for HEP theory to be forced to consolidate around one topic.

Later on in the talk, Gross started channeling Lee Smolin and me, urging young people to stop sticking to the same well-worn ideas, to stop looking under the same lampposts, and to go out and search for something really new. He argued that they would find wandering in the darkness less competitive since few people were doing it. It was unclear whether one is allowed to get out from under the umbrella when one goes out to investigate the darkness, presumably not.

While he made lots of positive comments about current work in string theory in order to rally the troops, much of his talk was rather pessimistic and critical of trends in string theory research. He acknowledged that there hadn't been any "great breakthroughs" in the field in quite a while. String phenomenology was described an attempt to make string theory "a predictive, or at least imitative" framework. He didn't comment on what it means for theorists to give up on predicting nature, and settling for imitating it.

About the LHC, he acknowledged that it is unlikely to have anything to say about string theory. He finds the idea of seeing black holes, strings, etc. "extremely unlikely", but is betting that the Higgs and supersymmetry will show up. Unfortunately, even if supersymmetry is found "it's not clear that we're going to learn enough", this won't answer any deep questions about string theory or prove that it is relevant. His "most optimistic hope" is that the LHC will see something unexpected, and "we will realize that this was an obvious prediction of string theory". He notes that this is "almost our last chance", if nothing relevant to string theory shows up at this energy scale, it is unlikely that anything relevant will show up at any energy scale accessible for an extremely long time.

Gross commented on two topics that hadn't been mentioned in the talks. He's still hoping for a non-anthropic explanation of the CC, and noted that no speaker had brought up the anthropic landscape explanation of the CC, with it getting a mention only at one after-dinner talk. Despite what Susskind claims, perhaps the battle between the anthropicists and their opponents is not going so well for the anthopic side. They may get pushed out from underneath the umbrella…

The second topic was the still unsolved question of "what is string theory?". Gross noted that there were no talks on string field theory, since it and most other ideas about how to define string theory non-perturbatively have gone nowhere. The one thing that is still alive is AdS/CFT, which now completely dominates the subject. More and more, particle theory research under the umbrella is focused only on things related to the duality of N=4 SSYM and string theory on AdS^5 x S^5. Gross noted the progress toward showing this duality, with the planar limit perhaps being done within the next few years.

By the way, in Ooguri's talk, he mentions an AdS/CFT discrepancy that has been resolved, saying he was surprised that some blogger didn't claim this discrepancy as disproof of AdS/CFT. Lubos in his commentary helpfully explains that Ooguri was referring to "numerous pigs and Woits". Since I've never argued that there's a problem with AdS/CFT duality, I guess he must be talking about someone else. Maybe Jacques Distler has some postings about problems with AdS/CFT that I missed.

Gross takes the attitude that there is no more value in working on "tests" of AdS/CFT, that the conjecture is now well-tested and it is time to move on to try and understand what AdS/CFT is good for, especially what it says about the question of "what string theory is?". The planar limit is just the classical limit, and he discusses prospects for moving beyond it . On the QFT side, this means deforming the gauge theory by non-renormalizable operators, so it is not clear what to do.

Update: More summary commentary and prizes at [Resonaances](). Clifford Johnson watched Gross's talk and [summarizes]() it as follows:

> David Gross summed it all up, took stock of where we are, and where we aren't, and looked forward. A sort of "state of the union" speech if you like. And the state is good. Very good indeed.

Comments

1. **anon.**
   August 22, 2008

   The planar limit is just the classical limit, and he discusses prospects for moving beyond it. On the QFT side, this means deforming the gauge theory by non-renormalizable operators, so it is not clear what to do.

   Going beyond the planar limit just means going to finite Nc on the field theory side (and computing g_s corrections in the dual). Non-renormalizable operators are a separate issue: they let you deform away from asymptotically AdS, with the hope of somehow probing properties of flat-space string theory. I wouldn't have high hopes for that, but then, Gross is smarter than I am….

2. **Peter Woit**
   August 22, 2008

   anon.,

   Thanks for the clarification, I wasn't careful enough writing that, should have made it clear that Gross was speculating about moving beyond the AdS/CFT duality, changing the background, not just further tests of AdS/CFT at higher orders in 1/Nc. I've edited the post slightly to clarify.

3. **Esornep**
   August 22, 2008

   D. Gross said:

"Much like other string meetings, it's been extraordinarily exciting."

Tongue firmly in cheek, I suspect.

4. **Shantanu**
   August 22, 2008

   Peter and others, are the the vidoes of the meeting archived?
   Or are they only avaiable in realtime?
   Thanks

5. **Daniel de França MTd2**
   August 23, 2008

   There were really interesting things in the meeting, one of them was covered here by Peter. On Wednesday, it happened really nice talks concerning advances towards the proof of renormizability of supergravity N=8 and some useful nice side effect from this, like more efficient ways to calculate the perturbative terms of the usual QCD.

6. **Umesh**
   August 23, 2008

   Were there no comments on Rovelli's talk on LQG?

7. **hvtek**
   August 23, 2008

   Hahaha! You find the display of string theory triumphalism appalling? And that it failed miserably in its objectives? What about LQG? Did you watch Carlo Rovelli talk? It was pure comedy, to put it lightly. Even he admitted in one of his slides (one of the first, in fact) that the low energy limit of his theory is pretty much work in progress. What a joke! So, if he and his people can't derive Einstein equations from their formalism, it is not a theory of quantum gravity. It is not even a theory of gravity (let alone the other interactions, or matter content)! His talk should have ended there! Instead he continues, talking about black holes (solutions of the low-energy limit he can't derive) and diffeomorphism groups (not the Lorentz group we're interested in, as Alvarez-Gaume put it in one comment), Immirzi parameters (that must be fixed in different ways to get the different answers he wants to achieve), and many other stand-up comedy pieces.

   String theory, on the other hand, has GR as its long-distance limit. No doubts about that! And it's a consistent quantum theory. Therefore, it is a theory of quantum gravity! Crystal-clear! The "phenomenological" work in progress in string theory is the derivation of the effective model for the remaining interactions, which must contain the Standard Model in the energy range of present accelerators. But that requires the understanding of the stringy vacuum, and that is not for the faint-hearted or amateurs. And, of course, there is no deadline to solve that problem. What does that even mean?!

   String theory is indeed the only theory of quantum gravity in existence. And by that I mean a theory that can be explicitly shown to contain Einstein gravity in the large-distance limit, and it is an anomaly-free quantum theory. The possibility of construction of such theory should, as a consequence, contain enough freedom to accommodate all the other interactions, and string theory does just that. It even provides a prescription to calculate particle masses and other physics parameters from the geometric properties of the vacuum, once you find it. It is a hard task, but is not impossible. It is by far more likely than LQG being consistent with low energy physics.

   The reason why I defend string theory is that it contains too many beautiful and meaningful results to ignore it as one of the greatest achievements in physics (and mathematics). It gives us so many hints that it is correct that it must be so. Just like Gross' comment on AdS/CFT. It's like Riemann's hypothesis: it's beautiful, it provides so many outstanding results, and it's verified up to the first 10^13 zeros. It has to be true! But even if it's not, I thing that it would be also very interesting (if not more), because you would have to explain why the zeros don't fall on the critical line only for the 10^500th zero, for example. And even if string theory is proved not to contain our world as a solution (which I doubt), you would still have to explain why not! Because you can already model strings to get toy models for particle phenomenology. They are not the real world, but they are not far removed from it. So, even the absence of a solution containing the Standard Model would simply mean that string theory as we know it would have to be slightly extended instead of completely replaced.

   In conclusion, I believe that the answer will arrive, and it will come from string theory. But be patient, because there are no deadlines to fill.

8. **somebody**
   August 23, 2008

   "CERN is in the process of becoming the single world umbrella lab doing particle physics, driving all the others out of business. Gross ses the same thing happening in particle theory and seems rather pleased about it, saying that only one umbrella in theoretical physics will survive, string theory, eating up everything else."

   There is a world of difference between Gross' claim that string theory contains all the great ideas known in particle physics and gravity, and your claim that string theory is "eating up everything else". You have managed to turn a scientific point that someone was raising (in this case, Gross), into a sociological statement.

   String theory contains gravity or gauge theory or non-commutativity or whatever NOT because it is evil and wants to take over other disciplines. It is just an inherent mathematical feature of string theory. To subtly redefine the problem into a sociological one only helps to misguide your untrained and/or gullible readers.

9. **Arun**
   August 23, 2008

   hvtek – The three notions that I disagree most with are:

   It gives us so many hints that it is correct that it must be so.

   And even if string theory is proved not to contain our world as a solution (which I doubt), you would still have to explain why not!

   The comparison of string theory with Riemann's hypothesis is the third.

   All three are pinnacles of anti-scientific thinking.

10. **somebody**
    August 23, 2008

    Arun cites hvtek's claim that "[string theory] gives us so many hints that it is correct that it must be so"
    as a
    "pinnacle of anti-scientific thinking."

I disagree. If you had said hvtek could actually be WRONG, I would have agreed. But being "scientific" is an entirely different thing. Trusting an idea that goes through many of the wickets, and trying to take it to its completion so we can reap the full rewards, is in fact the way theoretical physics SHOULD work. Is it conceivable that the idea is wrong? Of course it is. But that is completely beside the point. Doing science is in fact about taking bets that seem reasonable at the time and dealing with ambiguity. HVTEK was emphasizing that at this point the most reasonable bet is string theory.

Here is the status quo as I see it: string theory has all the features of quantum gravity one would hope for, including solutions to black hole puzzles, singularity resolution, holography, possibility of topology change etc. etc. It contains the broad outlines of realistic particle phenomenology including chiral fermions, generations, gauge groups etc. etc. It brings together almost all (if not all) of the most experimentally successful ideas in theoretical physics including gauge symmetry, Lorentz invariance, etc. etc. Experimentally, it is gradually bringing up connections to some condensed matter systems which are experimentally accessible (with some quantitative, but not very well-understood successes).

Any one of these would have been enough to make an idea interesting. It is inconceivable that string theory will have nothing to do with reality, because it already does. The valid question is whether it is going to be a useful tool in doing particle phenomenology, model-building etc. About that: despite all the nonsense claiming the contrary, progress has been steady, so there is no reason to think that we should give up now. Stabilized vacua are only five years old.

11. **fh**
August 23, 2008

hvtek, maybe Rovellis talk wasn't the best introduction given the audience, but you obviously have no clue what you are talking about. Yes LQG has some severe problems. No they are not the "problems" you mention. That's just misinformed blather. E.G: The Immirzi parameter is free in the Holst action, Ashtekar choose a value for mathematical simplicity, later it was found that in QM you have to choose it for consistency with BH entropy. Thus Ashtekars original simplifications could not be used. There are no conflicting scenarios for Immirzi, the original choice was purely for mathematical simplicity before its physical meaning and the way to fix it physically became known.

Since the low energy limit is such a joke to you could you please elucidate me how, given any background independent quantum theory, to even construct observables that would indicate it?

After all it is well known that pure gravity does not have good/unique thermodynamics (what is equilibrium in the absence of a timelike killing vector field?), nor energy scales (Black Holes come in all sizes).

And yes, it is probably fair to say that String theory is the only theory of Quantum Gravity, however at the enormous cost of shitloads of stuff that nobody has ever observed (e.g. 11 dimensions) or that is, even in priunciple, unobservable. Furthermore we have control over the theory only in ways (dual, perturbative) that obscure the conceptual questions that a final formulation must answer one way or another.

12. **Aleksandar Mikovic**
August 23, 2008

Hvtek's claim that string theory is a theory of quantum gravity is not a precise statement. The precise statement would be that string theory is a perturbative theory of quantum gravity since the arbitrary genus amplitudes are finite (again there is a caveat, since there is no proof for arbitrary genus, but string theorists beleive that it is finite). On the other hand, the nonperturbative string theory is less well-defined mathematically, and results like AdS/CFT
are conjectures, and even if true, it is not clear how it can be used to learn something about quantum gravity phenomena. When compared to Loop Quantum Gravity, one can see that LQG is a nonperturbative formulation, and this is the reason why it is difficult to see what is the flatspace limit (although, the recent work of Rovelli and collaborators has shown that the theory has room to accomodate gravitons, and hence the Einstein equations in the classical limit). Hence the claims that the string theory is the only viable candidate for a quantum gravity theory are not true.

13. **somebody**
August 23, 2008

"And yes, it is probably fair to say that String theory is the only theory of Quantum Gravity, however at the enormous cost of shitloads of stuff that nobody has ever observed (e.g. 11 dimensions) or that is, even in priunciple, unobservable."

This is misleading. String theory can have four dimensions as well.

The better way to say it is that string theory allows many classical solutions, of which one specific choice corresponds to ten dimensions. The ten dimensional theory is special only because it gives rise to the easiest perturtbation theory: the worldsheet CFT is free.

"Furthermore we have control over the theory only in ways (dual, perturbative) that obscure the conceptual questions that a final formulation must answer one way or another."

I have sympathy with this statement. But it depends a bit on what kind of conceptual questions one has in mind. What seems conceptually important at low energies is not always what is conceptually important at high energies. What is important is that the theory should REDUCE at low energies to the expected principles that we know and love at low energies. But I would say that to rigidly hold on to ideas stemming from a manifold-model of spacetime (such as naive spacetime diff. invariance) all the way to Planck scale, is unwarranted.

14. **bpz**
August 23, 2008

ft:"When compared to Loop Quantum Gravity, one can see that LQG is a nonperturbative formulation, and this is the reason why it is difficult to see what is the flatspace limit "
I think the question was about obtaining Einstein's equations in the low energy limit of LQG, and not about the flat space limit. The fact is that in the low energy limit ST automatically predicts not only the graviton in the spectrum, but also gives Einstein's equations from the worlsheet CFT beta functions, without specifying the background metric, and LQG does not even come close to achieving this.

15. **David Berman**
August 23, 2008

I feel the comments made above and indeed the tone set by Peter's reportage are not indicative of how stirng theorists think at all. This idea of an ongoing battle just isn't there. We are open to ideas and indeed Rovelli was invited to speak and everyone I met at strings was glad he did. This persistent itch to create and imagine conspiracies of string theorists excluding people really does verge on the paranoid. We are happy LQG people exist its just we choose something different. Each to their own tastes. I simply would encourage construction rather than criticism. Do your work and let others do theirs instead of moaning about hegemonies.

16. **Daniel de França MTd2**
August 23, 2008

David, do you read Lubos' blog?

17. **A.J.**
August 23, 2008

Daniel,

Citing Lubos is as a bad an argument as citing Lubos' arguments.

18. **Moshe**
August 23, 2008

fh, factual question regarding the Immirzi paramater: is there a choice of the parameter that makes the BH entropy come out right for more than one type of BH? first kind of BH can be regarded as calibration, anything after that is a test…

19. **Peter Woit**
August 23, 2008

somebody,

I don't think I was at all misrepresenting Gross's comments. People can listen to them and make up their minds. He was explicitly making an analogy between the dominance of CERN in HEP experiment and string theory in HEP theory. You can try and claim that this dominance of string theory is not a matter of sociology, but just reflects its huge success as a scientific research program. I strongly disagree.

David,

I think I was accurately reporting Gross's comment on LQG, including its tone. In other venues, Gross has publicly made dismissive comments about the subject. He's clearly no fan of the subject.

I don't doubt that many string theorists adopt the attitude you endorse of "each to their own tastes". But I also think that many, including many quite powerful ones, share what I fear is Gross's attitude that the hegemony of string theory is justified, that ideas that aren't somehow related to it are just not that interesting. Lubos reflects this attitude in its pure, uncensored form, but in less extreme form some of this attitude seems to be held by a lot of other people.

20. **Alberto G. P.**
August 23, 2008

The Minwalla's conference was about the 'Non Linear Dynamics from Gravity':

http://indico.cern.ch/getFile.py/access?contribId=24&resId=0&materialId=slides&confId=21917

I am not an expert in the subject, but this conference seem me very interesting. It's means that we can obtain analytic solutions of Navier-Stokes equations in a completely different way, by means a new and amazing duality, Isn't it?. Is this new mathematical tecnique powerful and elegant? If it is true, then turbulent flow could be explained by analytic solutions of Navier-Stokes equations. Can we obtain new insights on turbulence?.

Maybe God could have an answer for Lamb's second question , i.e. why turbulence?

21. **Luboš Motl**
August 24, 2008

Dear David Berman,

your comments about these matters are stunning. Science is not about "tastes" (even though the postmodern people who have contaminated 99% of Academia by now clearly think otherwise): science is about finding objectively correct answers to well-defined questions, in this case questions about quantum gravity, and it is essential for the scientific method that hypotheses that have been proved incorrect are abandoned, regardless of people's "tastes". In the context of sociology, it is equally important to pick people who are able to look for correct answers and not to pick those who aren't.

Rovelli's talk at Strings was a very weird experiment. It brought nothing to the participants because it was addressed to an audience that wants to be manipulated by simple propositions with some buzzwords, audience that doesn't know what a "path integral" or "Wilson line" means, and the speaker had no idea what the participants (theoretical physicists) are working on now, not even approximately: and he was not interested. The talk covered some basic "motivating concepts" that haven't changed for 10+ years and that haven't led to any successful checks in the last decade(s) even though they have led to failed tests. And there is arguably no work in loop quantum gravity that could be useful for more well-informed groups of physicists than those who need to be introduced to the concept of a "path integral".

Every single question after the talk was from a physicist who has already heard about their framework i.e. learned why it is inconsistent. (Rovelli was not interested in the questions, either: the work in LQG has switched from the search for meaningful and relevant ideas to a P.R. business indefinitely promoting a dead horse, motivated by the proponents' personal interests.) It is likely that this experiment (an LQG talk at a major string conference) will never repeat itself. There's no reason for another introduction to LQG at future conferences because the string community has already been "officially exposed" to LQG, and there will also be no reason for more detailed talks because the organizers will now be officially able to see why all the other work is wrong (or at least incomprehensible to all of them) so they will regain the freedom to choose all the speakers according to the merit rather than politically correct desires for "diversity". From this perspective based on the merit, no LQG researcher could make it to a prestigious global annual conference about high-energy theoretical physics.

Rovelli was the only name of a speaker (in English) that didn't appear in Hirosi Ooguri's summary of the talks and LQG was drawn as a disconnected piece from "the" theory studied by the participants. And David Gross indeed said, as this blog correctly reviews, that we're not sure whether we want to eat this discipline, LQG (like other disciplines in the past). And he did say that string theory was a hegemony in theoretical particle physics because it indeed is one and it is one for extremely good reasons that were revealed during the last 40 years. By looking at objective data, your statement that "everyone" was happy that Rovelli was invited sounds extremely bizarre. Every string theorist whom I consider good has been exposed to the assumptions of the LQG framework years ago, because they try to re-answer some very important questions that every expert in the field should care about, and every one of them knows why the LQG answers are incorrect, because it is not difficult to see.

By this category, I mean people like Susskind, Gross, Polchinski, Seiberg, Witten, Strominger, Vafa, and many others. There exists no genuine "open-mindedness" or "freedom to choose physics according to tastes" because these questions have been answered and the only difference is whether physicists choose to be silent about these facts (none of the people in my list has been quite silent about the inconsistency of LQG, though; I could tell you dozens of details based on private conversations but because it might be sensitive, they won't appear here). It is very plausible that there exist many students and other junior people who are not getting these points because similar topics about "alternative theories" became a part of political correctness that cannot be taught or talked about freely (because it could insult someone to explain why a class of theories is known to be wrong, right?) and they were not able to analyze these problems independently (so far). So people who don't really mean anything in physics, such as Urs Schreiber, A.J., or someone like that, can be ambiguous about this question: this includes everyone who thinks that the discussions at this blog are intelligent and relevant for actual physics, rather than being emotional exchanges mostly between cranks who have no clue what they're talking about. But be sure that the physicists who actually represent the "bones" of the skeleton supporting the "lampposts" of string theory are not ambiguous at all. LQG doesn't work and there is no known promising & surviving route in this context that would deserve to be studied. That's also why no new LQG person should be hired at any institution that knows what it is doing.

Also, I think that if you actually think that these LQG things have a significant probability to be correct, or they're not excluded, then all of your work lacks a basic scientific integrity because your papers seem to be assuming that LQG is incorrect but you never state it. For example, all the comments you have ever made about anomalies should supplemented with a citation of an LQG source where it is argued that anomalies can't exist because they, and all effects coming from UV divergences, might be removed by the "discrete nature of space" at short distances. In all cases, you should honestly consider both possibilities, that LQG is correct and LQG is wrong. The same thing applies to all sections of your papers where you use a classical solution because you should be studying these things in a background-independent way, if the LQG teaching is correct, and so on and on. If you really think that LQG might be correct, your papers are an example of hiding of the facts in science which would probably be even worse than to misunderstand why LQG is wrong.

In other words, if you have the "tastes" that you expressed and you think that LQG is more than a collection of ideas that can only be promising for 1 minute until one makes any test, ideas that are addressed to journalists rather than experts, research programs where all the hopes have already been proved unsubstantiated, you should really work on it because if it were a promising approach to quantum gravity, as you seem to think, then it would surely be an understudied one. Meanwhile, it would be desirable if you stopped trying to intimidate the physicists who actually know why LQG is not serious physics and create dishonest propaganda about "consensus" about your ludicrous opinions about "diverse tastes", "happiness" about the useless talk, and "encouragement of construction" at corners of the space of ideas where nothing constructive can be constructed.

It would sad if, as you indicate, young people were so absorbed into work under "particular lampposts" that they would become unable to see the big picture and if they become ambibuous about key conceptual questions thare are not ambiguous. But it would be even worse if physics switched into a hypocritical mode where people think something else than what they write in their papers. For me, your comments about LQG were shocking because they seem flagrantly incompatible with everything I have read in your papers and everything we have ever discussed in person.

Best wishes
Lubos

22. **G**
August 24, 2008

Wow Lubos, that's a long post for someone who "Sorry but I really can't afford to share readers…" of this blog 😉
If ST is so fantastic, why did you quit physics? you complain about production but yours is zero in the last 2 years…

I may agree with you Rovelli's talk wasn't very exciting, but neither is ST these days when it comes to QG.

I agree ST has inspired very beautiful and potentially (yet to be seen) useful stuff whereas LQG hasn't moved pass a few neat ideas but no clear progress. But as a theory of everything is as useful as LQG so far, let alone talk about phenomenology…That's the reason why there aren't many jobs out there for string theorists anyhow. Evolution at work…

G

G

23. **Luboš Motl**
August 24, 2008

Dear G,

I quit the Academia in 2005 when the feminist Nazis effectively took over Harvard, after preventing president Summers to say facts about biology that every person who is not a complete idiot knows – e.g. about cognitive differences between sexes. Because I spent the first 1/2 of my life in a totalitarian society, I know what it means to lose freedom of speech and I would never tolerate anything comparable to repeat in the place where I live. At that time I simply decided not to be extending/replacing the H1 visas which expired in June 2007, so I resigned.

But it is true that even if this thing didn't happen, I would be tempted to run away because of related things that were happening around physics. I got annoyed by people similar to David Berman who were implicitly helping those self-serving fraudsters and liars similar to Mr Woit and Mr Smolin to get away with their lies and to create an unjustified gloomy atmosophere in the field. At least, David Berman is brave enough to sign, but various anonymous people – a moral waste similar to you – apparently began to influence the course of things as well.

Of course if there are people who can't touch the ankles of David Gross, they won't be appreciating how fantastic the progress in string theory has been and it is still remarkable. The Strings 2008 conference was kind of impressive and I am convinced that even the people who can follow 10% of the relevant concepts only know why it is the case. At any rate, I just want to keep the freedom to know and say and why a fantastic theory is fantastic and why rubbish is rubbish, and the growing influence of mediocre, dishonest, and often anonymous people on the Academia was incompatible with my unbreakable standards about the scientific integrity. These things began to influence even things like my organization of seminars (lousy speakers wanted to speak about nonsense for the sake of "diversity" all the time, not respecting that they would have to be invited), teaching, etc. The job simply sucked given the existing conditions.

I agree that pigs, Woits, and Smolins have had a very bad impact on the perception of theoretical physics within the public and the broader scientific community, influence funding as well, which is why they deserve a very tough punishment. But you're still heavily exaggerating the importance of these two jerks and their gullible followers. Concerning jobs, Xi Yin was just hired at Harvard (after Frederik Denef), Allan Adams was just hired at MIT, Jason Kumar was hired at Hawaii, Dan Kabat was hired at Lehman College, and sorry if I missed other people. Add the new stringy IMP (fundamental physics = string theory) center paid for by the Japanese government. That's arguably a comparable number to the recent years (many of these jobs were announced really recently) and I am convinced that by the next year, the impact of the anti-theorist hysteria supported by the likes of the owner of this blog will completely fade away and everyone, except for a few anonymous posters at Not Even Wrong, will know that the propaganda by Woit and Smolin was based on lies and the anonymous people supporting Woit on this forum are scum if not Woit himself.

String theory is fantastic and whoever doesn't see it even though he claims to be interested in theoretical physics is simply not a high-quality thinker and I find it unacceptable for these people to dictate what others can think and say about their work or even what they should work on.

Best wishes
Lubos

24. **David Berman**
August 24, 2008

Hello all,

I do feel obliged to respond to make my position clear.

I personally do not think LQG is the right approach to the quantum gravity for the many reasons that are well known to the community so Lubos I will not be replacing my papers with LQG citations.

My point was simply that I thought Peter's description of a conflict between LQG and strings not to be accurate in terms of how people think. In my quote where I said people were happy that there was a LQG talk. I did not say that the same people agreed with LQG but were simply happy to see a talk that could make manifest the issues involved.

I think this was part of the thinking of the CERN committee. After all one of the critical questions that followed was by Alvarez-Gaume who was surely involved in the decision to invite him.

I suppose, Lubos our disagreement is how as string theorists we should behave to people who disagree with us. Your criticisms of LQG as a theory have been valid but I feel you do us no favours by adopting an aggressive attitude. This is a PR question which you don't like, but I never made any comments about the validity of LQG physics.

To balance this post let me say how exciting strings 2008 was. For me one of the best in recent years both in terms of the work presented in the talks and in the many discussions behind the scenes where people were being genuinely excited by emerging questions. I came away somewhat inspired…

25. **fh**
August 24, 2008

Moshe: Yes. Once you have fixed it for one black hole, the others all come out the same.

26. **fh**
August 24, 2008

"My point was simply that I thought Peter's description of a conflict between LQG and strings not to be accurate in terms of how people think."

I second this from the other side. Though I have to admit there are exceptions, most people don't.

On another note, the questions to Rovelli were excellent and cut to some of the core conceptual issues of LQG. It's not that people ignore these questions, it's just that because the setup of the theory is so different from familiar QFT, that the questions that are most natural/important from the PoV of QFT end up being the hardest to answer.
And then there are genuinely different opinions in the community, and another speaker would have given different answers to his. There isn't one theory of LQG after all, or even one complete framework.

27. **Jean-Paul**
August 24, 2008

Even perturbatively, low-energy string theory is not a theory of quantum gravity. It is a theory of wrong (experimentally excluded) gravity with spin 2 graviton accompanied by an unknown number of massless scalar Brans-Dicke fields (dilaton/moduli). So in the first approximation it is some sort of Brans Dicke theory, not Einstein's GR.

Yes, I agree that Gross' talk was at least very strange. He should have encouraged the participants to jump away from a sinking ship instead of living in denial.

28. **Tom O'Bulls**
August 24, 2008

Come on Peter, you are slipping up — your "Lubos Motl" 'bot, cunningly designed to make string theory look bad, is looking less and less realistic. Better iron out the bugs and re-launch! Or, better, write an entirely new program. Better make him a US citizen this time, the current implementation is a bit unfair on the Czechs.

29. **Peter Woit**
August 24, 2008

I don't understand why commenters keep referring to my supposedly inaccurate description of the conflict between LQG and strings. The only mention of LQG in the posting was an accurate quote from Gross about it.

As for the main piece of editorializing I did in this posting, objecting to Gross's string theory triumphalism, I see that Lubos agrees with my description of what Gross said. And as far as I can tell, David (Berman) share Lubos's point of view, just thinks that the aggressive way he expresses it is unfortunately counterproductive. I fear that this is an all too common attitude among string theorists.

30. **Moshe**
August 24, 2008

For useful interaction between different approaches to quantum gravity, look at the talks in

http://www.kitp.ucsb.edu/activities/auto/?id=332

For lots of people, including myself, the attitude towards LQG is basically curiosity mixed with a heavy dose of skepticism. There are sufficient number of really basic problems and unanswered questions to discourage people from jumping in and participating, but that could change in the future. Also, lots of people are completely oblivious to LQG and its main program, being exposed to the media and /or blogs it is easy to forget what a tiny community that is.

31. **Arun**
August 24, 2008

What are the exciting emerging questions (mentioned above) in string theory?

32. **A.J.**
August 24, 2008

Lubos wrote: *So people who don't really mean anything in physics, such as Urs Schreiber, A.J., or someone like that, can be ambiguous about this question:*

Why exactly should I take sides in this “debate”? As far as I can tell, only the internet sees it as a conflict.

33. **nbutsomebody**
August 24, 2008

arun,
none i guess 🙂

34. **nbutsomebody**
August 24, 2008

Dear Luboš,

I do not exactly share your views on string theory. However it is nice to know why you left academia. It seems to me as a very honest and justified reason.

35. **fh**
August 24, 2008

“There are sufficient number of really basic problems and unanswered questions to discourage people from jumping in and participating…”

Briefly out of curiosity, what are those (or the most important ones) from your perspective?

36. **moshe**
August 24, 2008

fh, probably not the time or place, but ask me in person next time we meet…let me just say that I’d personally would need to be convinced about the very basics – making connection to known conventional physics (apples falling from trees, harmonic oscillators having quantized spectrum,…), before taking seriously any claim about exotic things like BH entropy and big bang bounces, not to even mention some claims of experimental signatures…in other words I think the framework is too undeveloped to discuss applications at the moment.

37. **G**
August 24, 2008

Dear Lubos,
I wished you had the same passion for science as you do for criticism. Regardless of Academia, I wonder why did you stop contribuiting to ST? Wherever you are right now I’m sure you can still post in the arxiv don’t you? Stopping research due to a bunch of anonymous posts is a bit pathetic don’t you think? What kind of scientist would do that at the first sight of confrontation? Only the weak I should say… or perhaps you just don’t have anything intelligent to say anymore, in which case I would understand your reactions…

Do you truly believe that calling people ‘a moral waste’ can inspire anything more than a laugh? Lubos, if anything, you are really childish, but entertaining gotta admit 🙂

Regarding jobs, we all know MIT and Harvard are 5 year postdocs with teaching duties, perhaps MIT not as much as Harvard though… The rest probably finally woke up to real physics after all… “oops, the LHC.. we forgot…” 😉

best of lucks to you, you’ll need it 😉

G

38. **Daniel de França MTd2**
August 24, 2008

Carlo Rovelli answered on Lubo’s blog, about the comments on his talk:

https://www.blogger.com/comment.g?blogID=8666091&postID=4736312203995727650&pli=1

39. **Kea**
August 24, 2008

Hmmm. I must agree with Lubos that this statement by Rovelli is very grating:

LQG studies the hypothesis that this problem can be addressed in the context of quantum general relativity plus the standard model.

This so called context really makes no sense at all, since the term *quantum GR* can only really make sense in a theory of QG, and the latter cannot possibly be an ad hoc mixture of GR/SM. This is a serious criticism of LQG, which its proponents, as far as I can tell, have never addressed even qualitatively.

But now I simply must watch Gross’s talk, because his comments on BI sound insightful….

40. **M**
August 25, 2008

thank you Daniel, it was interesting to read answers to the very explicit criticisms by Lubos. I hope Rovelli knows that there is no point in insisting, as no logical argument will convince Lubos when he wants to believe something else. Debating seems as useless as rationally arguing with a female about emotionally sensible issues.

(In case PC nazi-feminism really exists: this is a joke!)

41. **Kea**
August 25, 2008

M: it’s not very funny.

42. **chris**
August 25, 2008

hi lubos,

you say:

"From this perspective based on the merit, no LQG researcher could make it to a prestigious global annual conference about high-energy theoretical physics."

i guess that is true, but it only was a string conference anyways. 🙂

btw: my favorite quote of this conference is from gubsres finite temperature talk. he was presenting some double-peak experimental signal from RHIC with the words:
"the first thing you ask yourself when you see these data is of course how to understand them in terms of black holes"
well, yeah. that about sums it up for me.

43. **somebody**
August 25, 2008

Gross is saying that string theory subsumes other ideas and thinks of this feature as one of its strengths. You take this to mean that he wants to actively vanquish other ideas. I have never heard any string theorist, let alone Gross, ever make such a cartoon-villain-like statement.

You are of course welcome to have your opinion about how string theory interacts with other ideas, but the problem is when you attribute a claim that was clearly not made by your opponent.

Even at the level of facts (and not just about the incorrect attribution), I am not sure you have got it right. How many good ideas have been killed off because of the prevalence of string theory? Maybe there are some, but I cannot recall even one at the moment. On the contrary, I know some ideas which I think are not exactly well-motivated, but which still manage to survive in academia despite the machinations of the evil string theorists. I can also list a tremendous number of independent ideas in high energy physics that have been naturally subsumed and strengthened by string theory: gauge theories, Riemannian geometry, supersymmetry, supergravity, Kaluza-Klein theories, noncommutative geometry, and many more.

44. **Peter Woit**
August 25, 2008

somebody,

You should address what I actually write, not make things up to attack. I did not say Gross "wants to actively vanquish other ideas". I did say that Gross is explicitly making a triumphalist argument: that string theory, like CERN, is all that remains. He feels that it has subsumed other, related ideas that are valuable, and that it is now an "umbrella" covering a range of research.

The problem with this is what happens to ideas that don't fit under the umbrella? I would characterize Gross's attitude as not that he wants to vanquish such ideas, but that they are already vanquished. Just like CERN, with the LHC, has no competition at the energy frontier, string theory similarly has no competition. This is explicitly what he said.

The problem with the idea that "string theory has subsumed gauge theory" is that there are many important problems in gauge theory that have nothing to do with gauge-string duality, and these now don't fit under the string theory "umbrella". Actually, logically the way things should be is that, with AdS/CFT, gauge theory subsumes string theory, with those parts of string theory unconnected to gauge theory being abandoned.

Don't you think there's something funny about going on about how one research program now is the umbrella under which everything must fit, when that program has been spectacularly unsuccessful at accomplishing its goals?

45. **Shantanu**
August 25, 2008

This is a bit OT. But anyone know of any live-blogging of the
following 2 meetings?
o emergent gravity workshop at MIT
http://www.rle.mit.edu/emergent/

o COSMO-08
http://www.physics.wisc.edu/cosmo08/
If so, maybe someone could post links here.

46. **a quantum diaries survivor**
August 26, 2008

Given the fertility of the field of String Theory, rather than of an umbrella, I would speak of a condom. As to Gross's encouragement to youngsters, it seems like telling them to f*** around unprotected.

Cheers,
T.

PS sorry for the slip to inappropriate commenting…

47. **somebody**
August 26, 2008

Peter says: "You should address what I actually write, not make things up to attack. I did not say Gross "wants to actively vanquish other ideas". "

Dear Peter, of course you did not write those specific words. That was my summary of what you say, for example, here:

"Under the circumstances, to claim victory and write out of particle theory anything that doesn't fit under the string theory "umbrella" is completely inappropriate. The message to any young particle theorist from Gross was clear: fall in line with string theory ideology, or there will be no place for you under the "umbrella", i.e. no job for you (the phenomenologists have their own umbrella, you better try that one)."

I see no way to NOT characterize this as a threat of aggressive assimilation (hence the word "vanquish").

Anyway, since you are at the stage where you say I am making things up and attacking you, perhaps further discussion on this topic is unlikely to lead to any clarification.

48. **Peter Woit**
August 26, 2008

A comment that appeared here yesterday purporting to come from Renate Loll has been deleted, along with all responses to it. She assures me it was fraudulent. It was submitted by some moron from a Time Warner Cable connection in New York City. Unfortunately, the LQG vs. string theory debate over quantum gravity seems to attract too little serious scientific discussion, and too much idiotic, unprofessional behavior.

49. **Jimbo**
August 26, 2008

Stringers vs. Loopers is starting to eerily parallel Repubs vs. Demos ! The Stringers have been `in-power' way too long, and the Loopers have not. The Stringers have peverted physics, and lost sight of its constitution, while the Loopers have deviated from the Minkowskian norm, and embraced the Einsteinian vision as reality.
The Stringers have a Lubosian attack dog, and the Loopers a Rovellian troubador. Both await the CERNian election outcome.
May Popper rule the house and senate forever !
And who-in-the-hell co-scheduled Emergent Gravity and Cosmo the same week ?? How are we supposed to go to both, in Cat States ?

50. **Chris W.**
August 28, 2008

From Steve Hsu, a warning from von Neumann.

51. **Kea**
August 29, 2008

Most of the talks are now up on the CERN webcast site, but frustratingly not Gross's talk, although a page linking his talk is there.

52. **Marcus**
August 29, 2008

Kea,
Yes, too bad video of Gross's talk is missing from the collection at CDS.
However the video of Rovelli's talk is there. I watched it and was impressed
by the level of interest shown. The hall was packed—people were standing. After the 30 minute presentation there were several excellent questions—and the Q/A dialog went on for another 13 minutes. After which the moderator asked that they continue outside during the break. I counted 8 questions from the audience during the Q/A period. All genuinely constructive, it seemed to me. It spoke well for those attending the conference, and the tone set by the organizers.

53. **chimpanzee (aka "joe")**
August 29, 2008

I can offer some perspective of research strategies for solving XXX problem, based on personal experience.

I got some good advice in grad-school:

"Don't do yyy, it's mature field & well understood. Try something in a new area"

So, I tried a new area. Turns out, the entire field (incl my PhD advisor & current grad students) were STUCK. Everyone was doing the same thing ("followers"): the typical Clustering Phenomena in research.

"In the game of Life [or Research], there are Drivers [leaders] & there are Passengers [followers]. Which are you?"
— Volkswagen commercial

Aka, the classic "cluster f**k" in Research. All fields run into this periodic bottleneck, where there is a stagnation of results. Also, known as temporal pessimism.

"I'm locally pessimistic, GLOBALLY OPTIMISTIC!"
— Dr. Jordan Pollack, Brandeis Univ/CS Dept
[my grad-school colleague in Artificial Intellgence, his PhD advisor D. Waltz ended up at Thinking Machines, the same company R. Feynman worked at 1 summer]

It appears as if String Theory is in this lull, with some dissidents (P. Woit & L. Smolin) raising concerns about any Global Optimism (they ain't!) So, D. Gross is basically saying, "keep on keepin' on". T. Dorigo has an interesting condom/penis analogy with the theme "ST is F**D!" In New York sland, he is saying "Fahgetaboutit!!". If it were me, I would certainly be investigating Alternative Models, like LQQ (or anything else). If the best minds haven't cracked it yet (Witten, et al), it seems unlikely a newbie could do it. However, there are Historical examples of newbies "cracking the safe"

"They're really smart guys, right? Everyone's been trying to unlock the safe (solve the problem) from the front. But, eventually someone tries an "end around" (football term) & tries to unlock it from the rear"
— Leon Lederman

It happened at the 2007 Fiesta Bowl, an "end around play" won it for Boise State. Could String Theory engineer such a miracle play?

I was that newbie in my chosen PhD research field 25 yrs ago, who led a breakthrough in my field. The Establishment was pretty much embarassed & shown up by a grad student (me)! I _singlehandedly_ took a global exhaustive search of research strategies, & found a commonality between 2 constraint equations. I found them to be the consequence of ONE geometrical topological structure. The computations were simply 3D vector computations in a homogeneous (n+1) space, within the capabilities of a high-school student! I achieved the "Simple answers, to simple questions..turned out to be REALLY EXCITING" mantra (Murray Gell-Mann). Simple/Elegant solution, a minimalist solution was achieved thru a true reductionist approach

Of course, I was a threat to the Establishment & my research was NEVER published in journals (due to political back-stabbing). Well, actually there was 1 (early on). However, they were widely published in conference proceedings & I achieved a lot of notoriety.

My advice to the field:
Continue on as per David Gross, in the chance that there might be a breakthrough (nobody is holding their breath, apparently, except a few fanatics). I would put a LOT of effort in a _global search_ for that magical TOE model: through the process of Convergent Evolution (multiple path solutions converging to ONE), it could be found. The latter is how I pulled off the coup in my field (single handedly)

I might be a player in this wacky game of Theoretical Physics. My competition in my field 25 yrs ago, were some physicists including H.C. Longuet-Higgins (do a Google search on Gell-Mann & Longuet-Higgins, you may recall that MGM was dabbling in Psychology. My research field was the Computional Theory of Vision). If I could summarily waste these guys as a grad-student, maybe I can do it again.

“Out of the Blue” lightning strike, Research area punked!

(example) Geophysics
The classic case was Geophysics in the early 1900s. Everything in status-quo “get along mode”. Until, a wayward researcher (German meteorologist) by the name of Wegener came onto the scene. He had an incredibly simple theory termed Continental Drift (a child who builds puzzle could see it), that had some corroborative data from paleo-archaelogy. Naturally, he was laughed off as a crackpot by the so-called establishment (peer reviewed experts, in complete political control of research journals). Of course, Wegener was right & we now know it today as Plate Tectonics. Turns out, it took nearly 60 yrs for the “crackpot theory” to catch on. Time was needed to gather additional corroborative data (Paleo magnetism, from sea floor spreading), Technology played a part. Interesting point: a female grad student (Tanya Atwater, aka Mrs. Plate Tectonics) @Scripps was a critical player in the feverish activity in the 60’s. “She was sent up the pole” (male sexist comment, about she was used as a guinea-pig for the crackpot idea), but she came thru with flying colors & defended the new theory against the backdrop of the male-establishment.

“If you don’t play [new wave of research or Technology], YOU GET LEFT BEHIND”
— xx, comment on Video on Demand revolution (stimulated by Apple video iPod)

“Those researchers [old boy network], CEASED TO BECOME FUNCTIONING MEMBERS OF THE RESEARCH COMMUNITY”
— Dr. Marcia McNutt/MIT (now Director of Monterey Bay Aquarium)

Theoretical Physics is on the cusp, of getting hit by “Lightning Bolt out of the Blue”. Somebody (or group) will come up with some “crackpot idea”, which will eventually be shown to be the “Final Solution”.

“It’s not a matter of IF, but WHEN [& Who]”

Would it be Kea & her Category Theory? Any other candidates?

54. **Marcus**
August 29, 2008

Chimp: “Any other candidates?”

Sure. there is evidence of a global convergence—the kind of thing you suggested one could look for.
Add et al after each name mentioned here:

Renate Loll has constructed a new 4D continuum (not a diff manif) in which dimensionality varies with scale. De Sitter space arises from it at large scale in the absence of matter.

Laurent Freidel has modified spinfoam—loop gravity path integral—dynamics so that it fits better with Loll’s approach. Convergence you were talking about.

Yidun Wan has shown how particles could arise in the Loll continuum as topological variation in how blocks are joined. This needs more work.

The Loll continuum is a fundamentally new continuum. They don’t come along too often (new mathematical models of the continuum.) When a new model of spacetime appears, with fundamentally different properties at a microscopic level, it’s a good idea to check it out. Loll et al have a piece in July 2008 Scientific American. It has references to their papers if you want more information.

55. **Kea**
September 2, 2008

Gross’ talk is now up on the CERN site. Wow. The first 15 minutes was indeed the most absolutely appalling waffly flattery of string theorists, but the second 15 minutes was completely different, and excellent, managing to mix the basic message of a need to attack basic questions with a concrete illustrative example of our ignorance in terms of deformations of exact solutions to AdS/CFT.

Boltzmann Brain Domination

August 30, 2008
Categories: Multiverse Mania

A burning question in theoretical physics these days is that of whether Boltzmann Brains dominate the string theory anthropic landscape. It seems that to answer this question one must study the details of how BBs nucleate and how string vacua decay. String vacua can do pretty much anything, so attention is focused on the detailed study of how to make the Brains and ad hoc choices of "measure" on the multiverse, with these issues occupying the attention of many leading figures in cosmology. While the question of BB domination of the multiverse remains open, it is becoming increasingly clear that BBs may soon dominate hep-th. Their nucleation rate is increasing, while the decay rate of the rest of the field also appears to be increasing. Doing phenomenology and extrapolating data a few years into the future, Boltzmann Brain domination of hep-th appears inevitable.

The latest hep-th arXiv postings include two new contributions to BB studies. One is from a group of three physicists at Berkeley, the second is from a large collaboration of six cosmologists on both coasts, including three of the major figures in the subject. For a third, more intellectually substantive, contribution of similar length, there's a new posting on the subject from Lubos.

Comments

1. **Arun**
   August 30, 2008

   Thanks, Peter, Luboš was certainly worth reading in this case!

2. **Bee**
   August 30, 2008

   Well said, Peter 🙂

   I'll use the opportunity to warm up The Return of the Boltzmann Brains

3. **Thomas D**
   August 31, 2008

   I see no actual evidence that non-Boltzmann-Brainy hep-th is shrinking or dying. But with statistical methods like 'Select 2 papers about BB's that happen to appear on a single day, and ignore all the other papers that month' you can prove anything.

   For my sins I read almost all of Lubos' post and he doesn't actually produce anything relevant to the question. If you understand the BB argument, and the people who are do are hardly stupid, part of it is that there is a finite probability that any given BB has (false) memories consistent with orderly physical laws and decreasing entropy. So Lubos' Bayesian argument using his observations of a 'normal' universe doesn't work, because the correct prior might be so overwhelmingly in favour of BBs that even Lubos' entire history of observations can't tip the balance.

   I believe the correct reason why it doesn't make sense to worry about BBs in cosmology is philosophical: it is that the proposition 'I am a BB' is self-defeating. It's the same reason why we can assume that we are not brains-in-vats or simulations. And why (if we are religious) we implicitly assume that God will not intervene tomorrow to produce a completely different state of affairs from what we expect.

   Simply this:
   1) Either we are 'normal' or BB.
   2) But if we are BB, then none of our physical reasoning or theories make any sense: they are all unreal and false, based on an absolutely meaningless chance occurrence.
   2a) In particular, if we are BB, the physical reasoning that led us to think we might probably be BB is also unreal and false.
   3) In doing science, we proceed implicitly on the basis that our physical reasoning is not necessarily unreal or false.

   And, in fact, that we are not brains in vats, or simulations, and that God (should He exist) will not intervene tomorrow to completely change the results of whatever experiment or observation we are planning.

   Note that 3) doesn't mean that theories that seem to show we are probably BBs are necessarily false: it is only necessary that we be in the minority of 'normal' brains.

4. **Arun**
   August 31, 2008

   If you understand the BB argument, and the people who are do are hardly stupid, part of it is that there is a finite probability that any given BB has (false) memories consistent with orderly physical laws and decreasing entropy.

   Compared to all possible memories, most memories will be consistent with chaos, Santa Claus, the unicorn, the tooth fairy, etc., There are infinitely more contradictory memories than consistent memories.

5. **Arun**
   August 31, 2008

   And that is the other point – there is no "correct" prior – how do you establish it?

6. **Esornep**
   August 31, 2008

   To me, the funniest part of LM's performance [apart from his serious declaration that his own brain is "normal"….] is the way he shows such disrespect for people like Linde, Guth, and Vilenkin. That sits rather oddly with his constant appeals to authority, references to how many times Ed Witten has been cited etc.

   What I find strange is the way that people seem to think that the existence of BBs is somehow bizarre or laughable. On the contrary, in a Universe which is [apparently] infinite in space and time, such things most certainly do/will exist. We are all taught this when we learn elementary statistical mechanics: surely all of us have done the ancient problem of computing how long one would have to wait before all of the molecules in the air in the lecture theatre will rush into one corner and asphyxiate everyone? The answer, of course, is that the time required is negligible — by the standards of an infinite universe.

What *is* bizarre and very much in need of explanation is the existence of *normal* observers. How did such fantastically low-entropy systems come into existence at all, given that it *didn't* happen as the result of a Boltzmann-style fluctuation? The only strange thing about the [brilliant] papers being mocked here is that they don't consider this question, ie the provenance of the low-entropy state at the Big Bang.

7. **Aleksandar Mikovic**
September 1, 2008

The reason why I beleive that Boltzman brains have nothing to do with our Universe is simple: if a theoretical concept leads to a conclussion that our Universe should have properties which are in sharp contrast with what is observed, that means that the assumptions are not good or incomplete.

8. **Marty Tysanner**
September 1, 2008

Esornep:

> in a Universe which is [apparently] infinite in space and time, such things most certainly do/will exist. We are all taught this when we learn elementary statistical mechanics […]

We are taught it in elementary statistical mechanics, but that doesn't mean it is relevant to reasoning about the relative improbability of the big bang versus a deluded Boltzmann Brain… It assumes that our usual understanding of, e.g., entropy in accessible macroscopic systems, applies to cosmology in something like a "multiverse" setting where anything (some people assume) can happen. Statistical mechanical reasoning requires something like a well-defined phase space within which it makes sense to talk about fluctuations, relative probabilities of different kinds of structures forming spontaneously, or even comparing energies.

No experiments have been done that can say whether a background phase space exists (even in principle) prior to the big bang, so its existence (or absence) must be regarded as an assumption. Recognizing the contingent nature of a background (why need Nature supply it to us?), it is hard to see how we can justifiably believe that Boltzmann Brain arguments and reasoning about measures over a multiverse can say anything definitive about Nature other than, "*If* such a background phase space can be defined, *then* (and only then) our conclusions are true." That seems like a qualification worth making clear…

9. **Thomas Larsson**
September 1, 2008

The only bizarre thing about Boltzmann brains is that anybody cares. Ironic science in its purest form.

10. **Peter Woit**
September 1, 2008

Thomas L.

For Boltzmann Brains, I think a new phrase is required, this is getting way beyond "ironic science", and into the realm of beyond even the laughable. The behavior of physicists that it has led to is really comic beyond belief, with Lubos's meditation on whether his brain is normal just one of the priceless absurdities.

Another funny thing about all this is that here I'm in the role of expressing not a controversial position, but the consensus view of the physics community. At his public talk at the opening of the IPMU earlier this year, David Gross referred to BB papers as "totally preposterous" and said that physicists work on this "to my regret". In private one finds that most physicists consider this to be a really bad joke. One can get a wrong impression of this from the blogosphere, where anonymous commenters describe this work as "brilliant", and the proprietor of one of the most prominent blogs is a big BB promoter. If you think this is widely taken seriously in physics departments, try asking around…

11. **Arun**
September 1, 2008

Penrose-backwards:
On the contrary, in a Universe which is [apparently] infinite in space and time, such things most certainly do/will exist. We are all taught this when we learn elementary statistical mechanics: surely all of us have done the ancient problem of computing how long one would have to wait before all of the molecules in the air in the lecture theatre will rush into one corner and asphyxiate everyone? The answer, of course, is that the time required is negligible — by the standards of an infinite universe.

Yes, but I think it is true, and Luboš also points that out, that by whatever measure the "density" of such events is very, very low, even if they occur with probability one. It is very unlikely that we'd encounter (or be) one of those events. That our current state ensued from a preceding lower entropy state and that this is true of all visible space and all the way back to the big bang is much more probable than I being a Boltzmann brain imagining you and this blogpost and everything else. And the further back we are able to go, the larger the entropy fluctuation has to be to explain us as a Boltzmann universe. It is just a bad idea.

12. **somebody**
September 2, 2008

(penrose)^T says:
"What is bizarre and very much in need of explanation is the existence of normal observers. How did such fantastically low-entropy systems come into existence at all, given that it didn't happen as the result of a Boltzmann-style fluctuation?"

This is bizarre only because you are in love with thermal equilibrium and want to believe that everything that is NOT in thermal equilibrium is a fluctuation. What motivation (experimental/theoretical) is there for this?

To me this is how it looks like:
(1) The universe is what it is, (2) you are imposing your equilibrium prejudice on it when building models, (3) you end up finding strange consequences like over-abundance of Boltzmann brains[1], and then (4) you use this as a selection principle between your models. Most people do not think that your initial assumption is nothing more than a prejudice to begin with, so they find your efforts to make the world safe from Boltzmann brains as cheap speculation. "Cheap", because the papers seem to contain little more than this (seemingly simple-minded) overall philosophy. Please correct me if I am wrong about this impression about the general approach.

[1] There are some well-defined issues here, which makes me suspicious even about the details, but lets ignore them.

13. **Jeff McGowan**
September 3, 2008

Well, as someone who came reasonably close to going for a Ph.D. in physics, but ended up in math, all I can say is whew 🙂 My goodness, I have (vague) memories of stoned conversations about this sort of stuff when I was an undergrad, trying to work out whether the argument that using statistical mechanics anything with probability >

0 *will* occur might give a good argument for the finiteness of the universe. Of course one might get a BB even in a finite phase space, but as pointed out above one is probably unlikely to meet one on the subway. Then again, I met some pretty strange people on the subway.

Now that I think of it, maybe a physics Ph.D. would have made life easier, I mean if people post this stuff seriously on arXiv, I could churn out papers, maybe 2 a month….

14. **Thomas D**
September 5, 2008

No, the relative probability of a BB having 'chaotic' or nonsensical memories versus 'normal' ones is not infinite, because memories have a finite (and perhaps not very large, on cosmological scales) information content.

That's basic. It's why one talks of Boltzmann 'brains' rather than 'planets' or 'galaxies', because the information/'negentropy' required to put a planet or galaxy into a 'normal' state by chance fluctuation is much much larger than for a brain, with or without 'normal' memories.

I think Carroll's way to express it is neatest: the possibility that the observer who sees a 'normal'/orderly universe is actually a BB is **cognitively unstable.**

As to priors, the purpose was to show that Lubos' statistical argument, which assumed a (completely arbitrarily) prior probability of 0.5 each for normal and BB, could break down **if** the appropriate prior was one heavily tilted towards BBs.

In Bayesian statistics the 'correct' prior should come from the physical theory or theories you think are applicable. But the question we can answer with them is not 'Are we BBs or not'. It is:

"Is Theory A, which has realistic features (eg a cosmological constant) and predicts that BBs are numerically dominant over the universe, better or worse than Theory B, which is less realistic but has few BBs?"

The correct Bayesian way to set this up is slightly tricky, because in the case where the observer is a BB, all the apparent observational evidence that she considers to make Theory A realistic is false and meaningless. So one needs to split up the hypotheses:

A1, Theory A is correct, I am a minority 'normal' brain living in a 'normally' evolving Universe, my observations are reliable
A2, Theory A is correct and I am a majority BB, my observations are utterly unreliable
B, Theory B is correct and I am a 'normal' brain (since there are few BB's), my observations are reliable.

Given a set of observations seen by me, I want to know which they favour. Suppose that they favour A1 over B because A1 has a nonzero cosmological constant. Where does that leave A2? Note that A2 is compatible with *any observations whatsoever*… so any observation that apparently favours Theory A is a red herring, and we might as well have

A2′, Any of theories A, C, D, …, Z in which BBs dominate are correct, I am a BB, my observations are worthless

This is an inevitable ill-definedness of any hypothesis that includes 'I am a BB', and finding any prior for A2′ vs. A1 is an ugly task. But: any subsequent observation compatible with A1 will increase its posterior probability enormously relative to A2('), simply because A2(') doesn't predict anything, or equivalently that every observation is equally likely.

Also, if we have a hypothesis

C, There are no laws of physics, God just happened to make it look that way

although our prior belief in C may be overwhelming, any observation compatible with A1 should rationally diminish our belief in C – because C doesn't predict anything.

15. **Thomas D**
September 5, 2008

So just to finish the argument, A2 and C are examples of hypotheses which we usually consider 'not scientific' and discard instinctively. However, Bayesian statistics allows us to consider them, and gives us the reasonable answer that since they 'predict' all possible observations (including chaotic, 'unphysical' and nonsensical ones) to be equally likely, their posterior probability will decrease hugely if the actual observations are compatible with a theory which has 'normal' physical laws.

This leaves us in peace to consider the 'scientific' and 'normal' hypotheses A1 vs. B.

16. **Arun**
September 6, 2008

No, the relative probability of a BB having 'chaotic' or nonsensical memories versus 'normal' ones is not infinite, because memories have a finite (and perhaps not very large, on cosmological scales) information content.

The probability of 'normal' versus nonsensical memories likely has as an upper bound the probability of monkeys at typewriters producing Shakespeare.

17. **Shantanu**
September 7, 2008

BTW see this
for a diffent point of view of the Boltzmann Brains problem

Yau Birthday Conference

August 30, 2008
Categories: Uncategorized

There's a conference at Harvard this week celebrating (somewhat early) the 60th birthday of the geometer Shing-Tung Yau. Since I was passing through Cambridge on the way back from a short vacation, I managed to catch a few of the talks, including two quite nice ones on mathematical physics from Is Singer and Edward Witten.

Singer's talk was entitled "The Interface between Geometry and Physics, 1967-2007", and summarized some of the advances in this area that he has been involved with over the years. 1967 was the year of a Battelle conference in Seattle on the intersection of mathematics and physics, organized by DeWitt and Wheeler. Singer displayed a copy of a 1966 letter from Feynman to Wheeler turning down an invitation to attend, with the explanation

> I am not interested in what today's mathematicians find interesting.

At the 1967 conference Robert Geroch talked on the topic of singularities in GR, and Singer recalled inviting Geroch to talk at the 1973 geometry conference at Stanford on this topic and the positive mass conjecture. By 1975 Singer had learned from Jim Simons at Stony Brook that non-abelian gauge fields were exactly the connections on principal G-bundles studied by mathematicians. The news of the BPST instanton solution and its significance for physics caused him to seriously start working in this area, work conducted with Hitchin and Atiyah at Oxford. They made use of the index theorem to both calculate the dimension of the moduli space of instantons, and to show that the Dirac operator had zero-modes in instanton backgrounds. Later, Atiyah and Singer interpreted the local gauge anomaly using the farmilies index theorem.

Just as Atiyah did at his recent talk at the Bott conference, Singer interspersed his historical talk with remarks identifying unsolved problems that he thinks are worth attention. The first of these has to do with K-theory, extended to depend not just on vector bundles, but on vector bundles with connection. Here the open problem has to do with the analog of the families index theorem. There's a topological index (that takes values in the extended K-theory of the family), and the open problem is to define an appropriate analytical index and show topological=analytical equality. Singer had been hoping to have some new results to report on this, but he says that things turned out to be trickier than he had expected, so maybe he'll have an answer at Yau's 65th birthday.

The next topic was that of quantum Yang-Mills theory and the Millennium problem of proving the existence of a mass gap. Singer talked mainly about 2+1 YM, his conversations with Feynman about this, and his hopes that the positivity of the sectional curvature of the natural Riemannian connection on the space of gauge potentials modulo gauge transformations could be exploited to prove that there must be a mass gap. Proving this in 2+1d was his second open problem.

His final topic was mirror symmetry and S-duality. Here he speculated that this (and M-theory in general) might have something to do with a phenomenon from operator algebra theory. Unlike for N by N matrices, where all maximal abelian subalgebras of self-adjoint operators are conjugate, for certain C* algebras (rings of operators of type II), there are inequivalent such maximal abelian subalgebras. I gather that his idea is that these might correspond to the existence of lots of inequivalent limits of M-theory.

The next morning Witten gave a talk on quantum Yang-Mills and the Millennium problem, saying that this was in response to a request from Yau to explain at a basic level to a wide mathematical audience what this is about. He gave an extremely lucid explanation of the mass gap problem, taking the Hamiltonian point of view. This supplements what he and Jaffe did in the official write-up of the problem, which deals more with the Euclidean picture. As motivation, he explained in detail what happens in the Abelian case, where one can compute everything and there is no mass gap. The audience was appreciative and I think got something out of this, unlike quite a few talks of physicists to mathematicians, which tend to start at much too high a level of complexity, ensuring that only experts can follow.

Witten avoided one aspect of this problem, the one that most fascinates me, that of how you handle the gauge symmetry. In the Abelian case there are several equivalent ways of doing this, but in the non-Abelian case, at least in the continuum, one needs to understand BRST symmetry non-perturbatively, and this remains a difficult problem, one with deep connections to open problems in mathematics.

Comments

1. **Eric Weinstein**
   August 31, 2008

   Hi Peter,

   Thanks for the post. Probably just a typo but calling 'Is' 'Iz' drives 'Is' crazy because it's hard to stop once it gets going.

   I am particulary glad to see discussion about the period of intense activity between geometry and physics before the anomaly cancelation. I believe that for Is the great epiphany was realizing that the BPST instanton was exactly the Hopf fibration. At that moment, I believe he understood immediately (and likely better than anyone else at the time) that the interaction between the two fields was all but destined to be a full fledged revolution. I've heard him tell the story two or three times and it is clear to me that he has no self-interest beyond preserving some understanding of his part in the exact chain of events. Yang and Atiyah have also spoken and/or written elloquently on this subject, but I have always found Singer's role to be particularly interesting.

   It is going to become increasingly hard for modern mathematicians and physicists to understand that the major stumbling block was the lack of a single mature example connecting the two fields. How can it be that 'differential forms' were once 'exotic'? How could the quaternionic Hopf fibration be anything other than an excercise for beginning students?

   And yet to some of those who made the initial leap, those first steps were more amazing than the unbelievable story which followed.

   Also Singer is personally very kind to Bob Hermann on the issue of the Wu-Yang dictionary. While taking nothing away from Simmons, Wu or Yang, he believes that more credit should have found its way to Bob Hermann and his unusual set of books on geometry and physics. Hermann spoke of his 'probable mistake' in leaving tradional forms of peer reviewed research behind. I myself felf after meeting the man after a lecture I gave at MIT that he simply had no choice. He was simply a man ahead of his time and a marvelous case study about where peer review fails abjectly.

   I have always thought Singer a mensch for going out of his way to make sure that everyone involved got full credit for his role in this revolution. If only all history was written by such winners we would have a far better road map for the revolutions to come.

   History has been very kind to Singer but, at least in my opinion, not yet kind enough.

   Best,

   Eric

2. **MathPhys**

August 31, 2008

Eric,

Could you please elaborate on the Robert Hermann aspect of the story?

3. **Peter Woit**
August 31, 2008

Eric,

Thanks, fixed Singer's name. Thanks also for mentioning Hermann. His books are remarkably ahead of their time.

Mathphys,

For example, see Hermann's books "Vector Bundles in Mathematical Physics", published in 1970. I've always assumed that there were other mathematical physicists well-aware of the gauge theory/connections on bundles dictionary before this became well-known after 1975 (if only from reading Hermann's books…). This only started getting attention though after the Standard Model came together in 1973, and people started thinking about non-perturbative questions in it. The 't Hooft-Polyakov monopole in 1974 and the BPST instanton in 1975 were the crucial examples that got a lot of people interested in this.

4. **Deane**
August 31, 2008

I'd like to concur on how wonderful Witten's talk was. As a nonexpert who normally gets left in the dust in less than 5 minutes of any talk on quantum field theory, I was amazed and delighted that I either understood or had the illusion that I understood Witten's talk from beginning to end. Naturally, that means that Witten successfully hid all of the reasons why the mass gap conjecture is so difficult. But I was able to gain a reasonable appreciation of what the goal is.

The conference itself is quite overwhelming (I left yesterday with 2 and half days to go). At first, I could not understand why anyone would want to schedule so many talks (about 50, I think) in such a short time. But then I finally realized that Yau himself has no trouble processing this amount of information; he is sitting in front and clearly understanding every talk. But many of us mortals are being left in the dust.

And I also agree that it is nice to hear Hermann's name mentioned after all these years. When I was a graduate student learning differential geometry and interested in physics, Hermann's books were fascinating, if maddening. They provided a reasonable guide to modern differential geometry, but I could never understand his explanation of the connections to modern physics.

5. **Tony Smith**
August 31, 2008

With respect to Robert Hermann as "… a man ahead of his time and a marvelous case study about where peer review fails abjectly …"
consider these quotes from two letters he published in his 1979 book "Cartanian Geometry, Nonlinear Waves, and Control Theory: Part B":
"… I am not the only one who has been viciously cut down because I tried to break out of the rigid shell and narrow grooves of American mathematics. … My proposal was to continue my … work with … Frank Estabrook and Hugo Wahlquist of the Jet Propulsion Laboratory. … I most deeply resent the arrogance of the referee #3 toward their work … typical … arrogance of Referee #3 is his blather about the "prematureness" of our work … Now, we are working in a field – nonlinear waves – which is moving extremely rapidly and which has the potential for the most important applications, ranging from … Josephson junction to … fusion … and I am supposed to sit back and wait for Professor Whosits to tell me when he thinks problems are "mature"…
I sent the papers he mentions to very few people … I am also interested to note that he did look at them, since there is considerable overlap in methodology with a recent paper by one of his students, with no mention of my papers in his bibliography …
any money spent by NSF on a Mathematics Research Institute would be down the proverbial rat hole – it would only serve to raise Professor Whosits' salary and make him ever more arrogant. It would do more good to throw the money off the Empire State Building: at least there is a chance it would be picked up and used creatively by a poor, unemployed mathematician …
This issue transends my own personal situation …
Most perversely, the peer review system … works as a sort of Gallup poll to veto efforts by determined individuals … As budgets have tightened, the specialists fight more and more fiercely to keep what little money is available for their own interests. Thus, people with a generalist bent are driven out …".

6. **MathPhys**
August 31, 2008

My understanding is R Hermann was a professor at Rutgers in the 70's. Did he quit to devote himself to writing his books?

7. **Tony Smith**
September 1, 2008

MathPhys asks "… Did … R Hermann … quit … Rutgers … to devote himself to writing his books? ..".

Hermann said in letters published in his 1979 book "Cartanian Geometry, Nonlinear Waves, and Control Theory: Part B":
"… In 1975 … I had essentially quit my academic job at Rutgers (so I could do my research full time), and my main support came from Ames Research Center (NASA) for my work on control theory. I was also starting a publishing company, Math Sci Press, writing books for it to hold out the hope that, some day, I would get off this treadmill of endless grant proposals. (Unfortunately, it is still [March 1979] at best bearly breaking even.) …
Ever since I lost my ONR grant in 1970, thanks to Senator Mansfield, I have been trying to persuade NSF … that my work on the differential geometric foundations of engineering and physics is worthy of their support … I see my colleagues who stay within the disciplinary "clubs" receiving support much more readily … Thanks to Freedom of Information, I finally see what the great minds of my peers object to, and I see nothing but vague hearsay, bitchiness, and plain incompetence in reviewing … specialized cosed shops that blatantly discriminate against the sort of … work that I do.
I can deduce the sort of bureaucratic-administrative pressures that encourage this … However, when I look in the Physical Review today, … subjects which people … so enthusiastically supported ten years ago … [such as] "S-matrix theory" … are now dead as the Phlogiston theory …".

About S-matrix theory, Hermann said "… the … "S-matrix theory" boom (and bust) of the 1960's … constituted a very ingenious and compelling set of ideas whichinvolved areas of mathematics (e.g., the theory of functions of several complex variables) which were unfamiliar to physicists. They were, so to speak, led into this swamp because they were familiar with functions of one complex variable, but were naive enough not to appreciate the completely different order of complication involved in the generalization, while also not willing to invest the effort in developing and understanding the mathematics needed to make the ideas fruitful. …".

One mathematician of those times did try to apply the mathematics of several complex variables to physics, and succeeded in getting a realistic calculation of the fine structure constant. However, the American physics community not only made no serious effort to understand and develop Armand Wyler's work, but acts to suppress such work.

8. **Marco Frasca**
September 1, 2008

My view is that the question of "mass gap" has became a kind of legend since the proposal of Clay Institute for the prize. So, most of the work people is carrying on in the field of QCD and Yang-Mills in the infrared is blatantly ignored while solutions at this problem are pretended to come from I do not know what kind of exotic theory. An example over all is given by AdS/CFT that is often claimed to be the principal way toward answering this problem. AdS/CFT to work needs the mass gap as an input and does not prove anything about Y-M presently! This input value is generally taken from lattice computations that do not seem so reliable for pure Y-M spectrum. Time will say but I think that the true challenge implied into infrared QCD has not been completely caught by the community.

Marco

9. **Peter Woit**
September 1, 2008

Marco,

Witten didn't mention AdS/CFT at all, there is no mass gap there. He emphasized that the crucial problem is to show that pure QCD breaks conformal symmetry. The statement of the Millenium problem is mathematically precise and unambiguous.

10. **Marco Frasca**
September 1, 2008

Peter,

This is exactly what people working on QCD and Y-M are doing as knowing the spectra means something directly observable in accelerator facilities. I understand that Witten's requirements are more stringent from a mathematical point of view but hints coming from physicists are generally sound enough for mathematicians to build upon. My view is that behind this problem may hide a paradigm change that, if it would have been emphasized enough, today more people would work on it rather than multiverse…

Marco

11. **Sumar Ongi**
September 1, 2008

Regarding the mass-gap millennium problem, I take it that Terry Tomboulis' proof of confinement discussed here some time ago didn't cut it? Put another way, is he considered a candidate for the prize?

12. **Peter Woit**
September 1, 2008

Sumar,

The rules about the Millennium prize require first of all publication of a referreed proof. As far as I can tell, Tomboulis's paper has not been published. It also doesn't appear to meet the usual mathematician's standards for a complete, rigorous proof. Finally, what he is trying to show (confinement) is significantly different from what the official problem asks for, a proof that a construction of quantum YM exists satisfying a list of properties including a mass gap.

In his talk, Witten did not mention the Tomboulis paper.

13. **Marco Frasca**
September 1, 2008

Stated in another way, is it Witten who knows all research that is going around on this matter? Either, can he only know about mathematical proofs? Or people working on QCD and YM is not worth considering, just they are not mathematicians? This situation is not so good.

Marco

14. **Peter Woit**
September 1, 2008

Marco,

Witten could have talked on many topics, but he chose this one, and made it clear that he regards understanding non-perturbative QCD as one of the central problems in theoretical physics. While he'd like to get mathematicians interested in thinking about this, I don't think at all that he considers it a problem of purely mathematical interest.

15. **roland**
September 2, 2008

http://de.youtube.com/watch?v=j50ZssEojtM

16. **Doug**
September 2, 2008

rowland,

Thanks for that link. I'm rofl.

Deane,

For an interesting insight into Witten's own journey into the intimidating jungle, with Singer and Atyah guiding his steps, see Witten's 1999 acount in "MICHAEL ATIYAH AND THE PHYSICS/GEOMETRY INTERFACE" at:

http://www.intlpress.com/AJM/p/1999/3_1/AJM-3-1-lxi-lxiv.pdf

17. **Thomas R Love**
September 2, 2008

I was delighted to read the comments about Robert Hermann since he was on my PhD committee. I read his Lie Groups for Physicists in 1975 and eventually I read all of his published books, except the self published work. His "Yang-Mills, Kaluza Klein and the Einstein Program" was especially important to me. My dissertation, "The Geometry of elementary Particles" was very much in the Hermann tradition.

When I attended Singer's course on geometry and gauge fields at Berkley, everyone spell his nickname as "Iz"

18. **Eric Weinstein**
September 2, 2008

Doubting Thomas,

So you wish me to make the point that it all depends on what your definition of 'Is', is?

Hmm. I think In Hollywood, setting up an obvious line like that is called 'laying pipe' within the industry.

At worst, Is has tricked me (and now Peter) into using the same mis-spelling of his name that he has also conned Witten, Atiyah and others into using.

I was simply trying to do the smallest of favors and have very little interest in the topic …. shall we agree not to distract ourselves further?

Best,

Eric

19. **Thomas R Love**
September 3, 2008

I didn't mean to continue the discussion, but I was looking for the book with Witten's article and found it at

http://www.intlpress.com/books/FoundersIndexTheory.php

And there in the table of contents I found:

Letter to Fritz, Is and Michael – R. Bott
Letter to Michael, Raoul and Iz – F Hirzebruch

But let's leave it "AS IS"

20. **John Baez**
September 3, 2008

Does anyone know if Robert Hermann is still alive and well?

He sent me an email in October 1999 saying he had been trying to understand renormalization for many years, and asking if anyone had used Colombeau theory to understand the products of operator-valued distributions that show up in quantum field theory.

In 2005 I sent him an email asking if he knew about an English translation of Felix Klein's *Erlangen Program* speech. He never replied.

(According to Thomas Love, Hermann had mentioned such a translation in a book of his. By now it's available on the arXiv.)

21. **Eric Weinstein**
September 3, 2008

Hey John,

He is alive in your old stomping ground. I will be happy to give you (or any other colleagues interested) his contact information if you like.

In his words, he's sort of just a 'senior' who's 'given up'. He has neither email nor academic connection any more. From talking to him, I wonder whether anyone at all has seriously interviewed him about the history of the geometric physics revolution, though I could be wrong about that. There are now many more readable books on these topics and I can guess his ultimate reward.

For an objective field, it is just really really interesting how we go about subjectively creating our system of selective pressures.

Anyway, I read him a few of the comments in this thread. Thanks to all of you who had a kind word for a greying rebel. I hope I conveyed your sentiments as accurately as possible.

Regards,

Eric

22. **Thomas R Love**
September 4, 2008

The book John Baez mentioned is

Sophus Lie's 1880 Transformation Paper (Lie Groups: History, Frontiers & Applications Series No.1) (Paperback)
Translated by M. Ackerman, with commentary by Robert Hermann
(ISBN 0915692104) Math-Sci Press

The Multiverse at Perimeter

September 2, 2008
Categories: Multiverse Mania

This week, the Perimeter Institute is hosting a conference entitled A Debate in Cosmology – The Multiverse. Here's the schedule, and talks have started to appear on-line here.

Some of the speakers will be discussing the Many Worlds interpretation of QM. It has always mystified me why this sometimes gets put together with the string theory landscape sort of multiverse. It will be interesting to see how many of the speakers address the fundamental problem of the subject, that of coming up with a plausible falsifiable prediction. Lee Smolin has generally put that problem front and center, but he tends to be alone in doing this. The more usual thing in this subject is to go on about what an important idea the multiverse is, then make some sort of excuse for not being able to predict anything with it.

Also dealing with the problem of multiverse predictivity is this new preprint on the arXiv about the landscape, and this laudatory commentary from Lubos. According to him, it's not much of a problem that one is talking about measuring low energy observables to 500 digit accuracy, when one can't now even predict their rough order of magnitude.

My colleague Brian Greene will be at Perimeter this week, giving a public talk about his new book Icarus at the Edge of Time. It is being released to bookstores today and I haven't yet seen a copy, but it appears to be a science fiction book mainly aimed at children, illustrated with pictures from the Hubble Space Telescope. There's a blog entry about the design of the book here, with some pages of the book here.

Update: Sabine Hossenfelder has reports from the conference and public lecture.

Comments

1. **Michael Bacon**
   September 3, 2008

   "It has always mystified me why this sometimes gets put together with the string theory landscape sort of multiverse. "

   Peter,

   I agree with this, but I was curious why you've reached this conclusion. Thanks.

2. **Peter Woit**
   September 3, 2008

   Michael,

   They're two completely different ideas on completely different scales addressing completely different questions. It's not really that I've reached a conclusion, just that I've never seen anyone even try and give a reason why the two should be connected.

3. **John Baez**
   September 3, 2008

   I think one can try to combine the landscape idea and the many-worlds interpretation by imagining the universe in a superposition of quantum states corresponding to different points in the landscape. Then according to the many-worlds interpretation, all these "branches" "really exist" in some sense.

   I don't have the energy to explain my own attitude towards this line of thought. But, I have a feeling it's something a bunch of people have on their minds — perhaps only half-consciously. That might explain why they talk about both kinds of "multiverse" in the same breath.

   It might be good to ask them.

4. **James Robson**
   September 3, 2008

   "It might be good to ask them", but unfortunately I don't move in those circles!

   Surely these are two independent ideas? You may well be able to combine them: and a proper understanding of nature might require such a combination, but logically they are distinct I think?

   We could perhaps imagine a completely classical theory where our local situation depended upon the history of the way things evolved and settled down in our neighbourhood. Perhaps things could have turned out vastly differently elsewhere. So we would have some kind of multiverse where we might use an anthropic principle to explain why we see things to be the way they are around here.

   Or we could have a QM theory that was water-tight and allowed no flexibility and just told us the way things were – except that being QM things would be a superposition of all the states of that fixed world, and a QM multiverse would seek to separate these different possibilities through some kind of "decoherence" which renders the different possibilities effectively non-interacting and so different worlds.

   Don't believe either myself, but what do I know, not my job 😉

   James

5. **Peter Woit**
   September 3, 2008

   John,

   Since the universe is quantum-mechanical, I'd always assumed that the multiverse was too. So, yeah, you get a quantum superposition. But my understanding of Many Worlds is that it is supposed to deal with the measurement problem; you're supposed to have Many Worlds splitting off all the time as you do measurements. That's what seems to be something completely different.

6. **Hal**
   September 4, 2008

   Max Tegmark has identified four different types or "levels" of multiverse. The level 0 multiverse is composed of the infinity of replications of our local "Hubble bubble" within our universe if it is infinite in size. The level 1 multiverse is composed of all the local bubbles of ordinary space-time that exist within a larger universe in inflationary cosmology. The level 2 multiverse is the Everett many-worlds model. And the level 3 multiverse is the everything-exists level, where all mathematical structures have Platonic existence and structures with sufficient internal complexity correspond to physical universes. Interestingly, this taxonomy does not include the string theory landscape.

7. **Aaron Bergman**
   September 4, 2008

   But my understanding of Many Worlds is that it is supposed to deal with the measurement problem; you're supposed to have Many Worlds splitting off all the time as you do measurements. That's what seems to be something completely different.

   Many worlds is just ordinary quantum mechanics. New worlds don't split off; the "worlds" are supposed to be different "branches" of the wavefunction that have decohered. It's more of a lack of interpretation rather than an interpretation, and I don't think it actually answers any questions, but some people seem quite fond of it.

8. **Mikael**
   September 4, 2008

   Peter, I think you are a little too obsessed with predictions regarding the discussion about the nature of physical laws and the multiverse. At least currently this discussion is within the realm of philosophy and not science. But well, we know that philosophy can help create great science and Einstein is the biggest example of all. As an example regarding the multiverse think of the hierarchy problem and naturalness. If you believe in the multiverse, you might expect other things what the LHC will see as if you believe in uniquely determined eternal laws. So as a physicist you will probably be working on different things. So in a way philosophy helps to limit the infinite search space of physical theories. Once you have the theory and it meets predictions, it gets science and independent of philosophy. You can then use this theory to refine your philosophy again and so on. Of course it also works the opposit way which you can also see from Einstein. It seems that philosophy lead Einstein into darkness, when he was searching for a unified theory at the end of his life. But this is life I guess.

9. **Peter Woit**
   September 4, 2008

   Mikael,

   I have no problem with philosophy of science of various kinds. Some interests me, some doesn't. Within that context, discussion of a multiverse is reasonable. My problem is with the way the multiverse is being used not by philosophers, but by physicists at the highest level of the profession. They are not doing this because they like philosophy (many of them despise it), but because they are desperate to find some way to avoid admitting the failure of string theory as an idea about unification. The string theory "landscape" is exactly the sort of thing you get when an idea fails: something you have to make more and more complicated to avoid falsification, never actually predicting anything.

   This use of philosophy by leading scientists to prop up an utterly failed scientific project is what I have a problem with. It is doing a huge amount of damage to the field.

10. **Mikael**
    September 4, 2008

    Peter,
    one question, do you consider loop quantum gravity (LPG) a failed project, too?

    LPG can not predict the scattering amplitude between gravitons. Why? Because there are no gravitons in the theory. Why? Because Minkowski space is not shown to be the low energy limit of the theory. Worse, one can't see how it will happen. Not a very encouraging state of affairs I think. The only prediction of loop quantum gravity is a breaking of Lorentz invariance I think.

    On the other hand string theory does some predictions. It predicts extra dimensions. It predicts supersymmetry. Furthermore it offers a solution to the black hole information paradox based on duality.

    Let me add, that I am an outsider and I am asking from honest interest, and not in order to push any view that I have.

11. **Peter Woit**
    September 4, 2008

    Mikael,

    As theories of quantum gravity, both LQG and string theory have their pluses and minuses, this debate has gone on ad nauseam here and elsewhere. The bottom line is that neither has produced a completely consistent and satisfactory quantum theory of 4d gravity, or come up with any way one can experimentally test them.

    My objection to string theory is that it has failed as a theory of what we can observe: the Standard Model. The unwillingness to face up to the implications of that failure are what is damaging the field.

    Saying that "string theory predicts supersymmetry" or "string theory predicts extra dimensions" is a completely empty statement until you specify the scale of supersymmetry breaking or the size of the extra dimensions. String theory has nothing to say about this, and this is why it is an empty failure as far as particle physics goes.

12. **Aaron Bergman**
    September 4, 2008

    It is not true that LQG predicts Lorentz violation.

13. **Mikael**
    September 4, 2008

    Peter,
    it is true, that we do not have a consistent and satisfactory quantum theory of 4d gravity. But it is also true, that this statement is in a way empty, because it does not offer any help on what to work on tomorrow. Imagine I was a young student, who wants to work on quantum gravity and I'd asked you on what approaches to study, what would you answer me? Or would you say, I was waisting my time because the problem is just too difficult?

And I strongly disagree that predictions about supersymmetry and extra dimensions are empty, just because you do not know the scale. One thing to see this is that models based on these ideas already influence what patterns people are searching for in the LHC data. (probably even the detector optimizations). Another way to see this is that Carlo Rovelli was asking the LHC guys on strings 2008, when they will be able to tell if the world is supersymmetric, if it is on the accessable scale. He also mentioned in an interview, that he will have great respect in the intutions of string theorists, if supersymmetry is found. My very personal guess is he would even stop working on LPG in this case. How can an empty statement have such consequences? Also, how can the emptyness of a statement be dependent on our current experimental abilities?

14. **Mikael**
September 4, 2008

Aaron,
please elaborate. Predicting doubly special relativity means predicting a deviation from special relativity, right?

15. **Peter Woit**
September 4, 2008

Mikael,

If anyone wants to work on quantum gravity, fine. They should learn about what the various approaches are, what their problems are, and then see if they can make some sort of progress. The kind of hype and misleading talking points being thrown around in this subject are not helpful at all. If they are going to do this, they also need to understand and think seriously about the fact that, for very good reasons of dimensional analysis, no one knows any way to connect the subject to experiment, and learn anything about it by looking at the real world.

As far as I know, there is no defnitive prediction of Lorentz violation, or of what the scale of it should be, from LQG. You seem to like to talk about "predictions" when there aren't any, that's really misleading and a bad idea. There's a huge amount of less than honest claims being made these days about "predictions". I would bet against the LHC discovering supersymmetry, but if it does, that will be a huge revolution in physics, and a vindication of many people's interest in supersymmetry, most of which has nothing to do with string theory. See David Gross's comments at Strings 2008

16. **Mikael**
September 4, 2008

I am sorry for the careless use of the word prediction. My point is that there is something in between a prediction and an empty statement and I tried to explain why. "Learn about the various approaches and try to make some progress" is an advice so true, that you can apply it to anything.

17. **Michael Bacon**
September 4, 2008

Peter,

Back to the original topic, I listened to the Albert and Greaves talks — pretty impressive. As you may know, I feel affinity for the MWI, but in all events, I thought the level of enquiry was impressive. Hard questions, good answers all around. Just what science should be.

18. **Kea**
September 4, 2008

Heh, cool! The Banks talk is just **fantastic** – a must see for great physical intuition and a crucial alternative viewpoint with solid analysis backing it.

19. **Michael Bacon**
September 4, 2008

Haven't listened to the Banks' talk yet, but this does raise the question of the relationship between the MWI multiverse concept and the string landscape. I know John Baez mentioned that maybe you can tie them together, but I agree with Peter (if that's what you meant) that it's pretty odd wrapping them up in the same conference.

20. **Kea**
September 4, 2008

Actually, from my understanding, Banks is against both the Landscape and MWI: the former because it cannot reproduce the holographic cosmologies they consider, and the latter because he talks loosely in terms of isolated islands of 'lonely' universes in the dense black hole fluid, but it may be that he is headed towards a more MWI vision of the multiverse.

21. **Michael Bacon**
September 5, 2008

I think Banks did raise some questions (if it was him) during the Greaves talk, but as far as"interpretations" of QM go, it sounded like his view (to the extent he had one) was some sort of combination of consistent histories with wave function collapse.

22. **Kea**
September 5, 2008

Consistent histories, yes, but wave function collapse is a trickier question. He was careful to explain that he used ordinary vanilla QM only because he didn't know how to do better quantitatively. On the other hand, his description of emergent geometry (as a kind of collapse from collective observers) is much more sophisticated than the naive universal wave function ideas one often hears touted by MWI proponents.

23. **Michael Bacon**
September 5, 2008

Kea,

I'll go back and listen, but I'm pretty certain that he acknowledge some kind of collapse as a way to get our apparently real situation to fall out of the numbers, and I think he said that once that occurs (however it happens) you just set aside the other histories and basically start the process all over again — but maybe my recollection is wrong. Anyway, I thought his questions were thoughtful and probing, as were Hartle's (I think it was Hartle 🙂)

24. **Rexus**
September 11, 2008

With all do respect, the multiverse is crap! The many worlds interpretation is the worst model for interpreting quantum theory. While the whole concept of parallel universes might make for a handy plot device to be used in a bad Michael Chrichton novel, I do not see any utility for the hypothesis. There are far better interpretations of wave-particle duality and the measurement problem. For starters, the only reason why humans find wave-particle duality to be problematic is because it is difficult for the average human to comprehend who something could be a particle and a wave. In fact, such entities are not exactly “both particles and waves” rather they are neither. These entities are wavicles, having properties of both waves and particles, but not necessarily being one or the other. Likewise, the Copenhagen interpretation is a useful starting point, despite its shortcomings, but I personally think that Penrose’s hypothesis that gravitation causes the collapse of the wave function has great potential to resolve this measurement problem. Sci-fi babble about parallel worlds violates the principle of Occam’s Razor. Whereas the Copenhagen model, Roger Penrose’s hypothesis, or any other proposed solution is infinitely better.

PROBLEMS WITH MULTIVERSE

1. Falsifiability
The existence of “other universes” is not falsifiable in principle once we accept that universes besides our own might exist. After all, these parallel realities are not supposed to interact with our own, to be separate, connected only in the sense that they diverged off our universe (or we diverged off theirs). If the universes do not interact, how do we test to verify or falsify their existence. There *COULD* be other universes just as there *COULD* be a sinister conspiracy of Jews and Masons who rule the world (as rediculous as that sounds). Because these universes supposedly exist outside our universe, there is no way to test it. In this respect, arguing with ardent defenders of the many worlds interpretation is akin to arguing with creationists, Holocaust deniers, or 9-11 “truthers.” You cannot “prove” that God (or Satan) placed fossils in the ground to test our faith and that God did not create the universe like in Genesis but made it look as if evolution occurs, or that Bush did not plan the attacks.

2. Plausibility
I would argue that, in fact, I can “prove” that the multiverse does not exist. This might seem like a contradiction, but I only argued that the many worlds hypothesis is unfalsifiable if you open to the possibility of it being true. [Once you accept that, it becomes an argument from agnosticism (you can’t PROVE it ain’t so). But this admission that the multiverse is untestable cuts the ground from under the feet of the “many-worlders.” By admitting that the multiverse is unfalisiable, they admit that their speculation is not science, just very bad metaphysics.] However, I argue that to even grant that multiple universes CAN exist is a huge stretch. I can disprove the notion using logic alone.
[A] The universe, by definition, is all that exists. [The prefix “uni” implies one. The entirety of reality.]
[B] It follows that nothing else phyiscially exists beyond the universe.
[C] The multiverse hypothesis suggests that there are other universes beyond ours.
Either
[D1] These “universes” do not exist (because there is by definition only one universe).
OR
[D2] These “universes” are not separate universes at all, just part of THE universe.
A -> B, [(A & B) + C] -> D1 OR [(A & B) + C] -> D2.
Assuming that the universe = all of phyisical reality, the concept of the multiverse presents an internal contradiction. Either our universe is the only one, or our “universe” is not a universe at all, just a quantum accident. This falls into the whole “reality is an illusion” problem. Curiously, the many-worlders never attempt to explain where these other universes are. If they inhabit the same space-time as our universe, then they are merely overlapping quantum states. This does not solve the problems of quantum phyisics at all!

3. Simplicity & Parity
According to Occam’s Razor, the simplest explanation should be expected to be the correct one. Especially because we are not supposed to multiply entities unnecessarily. The many worlds interpretation attempts to solve a mystery with an even greater mystery! Postulating an infinite (or virtually infinite) number of universes to explain an anomaly is very bad science. The Copenhagen interpretation follows logically from quantum theory without being unnecessarily complex. Likewise, Penrose’s solution might work, but we may find out as gravitation is understood in light of quantum theory.

CONCLUSION
I think the fact that Hugh Everett’s pipe dream is even taken seriously is a sad indication of the state of modern physics and cosmology. Other than David Deutsch, I was unaware of any REAL physicist (i.e. with tenure, this excludes armchair physicists who watch Star Trek and believe it’s real) taking such speculation seriously. On the other hand, I can see the connection between multiverse quantum cosmology models and string theory, even if Dr. Woit doesn’t. I would not say that the string theory landscape is fundamentally different from the quantum “many worlds” interpretation. Even if they are on different scales, both models attempt to answer discrepancies in observations by creating more universes… Physics has strayed far from the sober analysis of Galileo, Newton, Einstein, Heisenberg, Schrodinger, Dirac, Pauli, and Fermi into the Star Trek pop-sci of Michio Kaku…

What’s Up With PAMELA?

September 2, 2008
Categories: Uncategorized

There were some comments about this in a previous posting, but I thought it worth remarking on the unusual situation that papers have started to appear reproducing plots of new data from the PAMELA satellite, even though no such data has been officially released.

This all seems to have started with a July 31 talk at ICHEP 2008, with slides here, but evidently some other slides of preliminary data were flashed on the screen. There was a news story in Nature about this, but still no official release of data. Then, on August 20th there was this talk at idm2008, with no slides made available on-line, but interesting slides again flashed. Evidently some enterprising theorist decided to do some of his or her own data acquisition.

Soon, a preprint on Minimal Dark Matter predictions and the PAMELA positron excess was on the arXiv, complete with PAMELA data, with the notation:

> the preliminary data points for positron and antiproton fluxes plotted in our figures have been extracted from a photo of the slides taken during the talk, and can thereby slightly differ from the data that the PAMELA collaboration will officially publish.

There are now at least two other papers on the arXiv featuring PAMELA data, evidently from the same source, here and here. Andrew Jaffe has a new blog posting up entitled Stealing Data? where he expresses discomfort with this situation. I can’t quite see that one is “stealing” data if it is being presented at major conferences.

Are there any of my readers out there who can tell us what’s up with PAMELA?

Update: Nature has a new article about PAMELA being “outed by paparazzi physicists”. One of the paparazzi, Marco Cirelli, is quoted as saying that “we had our digital cameras ready”, and claiming that the PAMELA people at the conference didn’t have a problem with this. On the other hand, a PAMELA PI is quoted as being “very, very upset” about this.

And I should have linked earlier to this posting at Resonaances: Hot Photos of PAMELA.

Update: According to Science News, the problem is not a *Natur*e embargo. They just haven’t finished a paper yet:

> “We plan to have final results ready by early October and submit a paper to a peer-reviewed journal,” Boezio told Science News. Until then, he says, the findings remain preliminary, and “We prefer to withhold further comments.”

Comments

1. **Kea**
   September 2, 2008

   Well, I have no idea, but a visiting colleague here who works with some Italian PAMELA people said they were ‘very happy’ about new results.

2. **Stan**
   September 2, 2008

   Really, this sort of behavior is just going to make collaborations more reluctant to show preliminary plots at conferences before the paper is ready. (Maybe they should be anyway…)

   I’m told by people I work with on SNO that they stressed a lot about this before their first paper. They wanted to show event distributions at a conference, but were worried that someone might try to eyeball-integrate the number of events in the curve. In principle, someone could use that information plus published whitepapers about the detector response to guesstimate the neutrino flux SNO was seeing before the proper analysis by the collaboration would be published. (Never mind that since SNO was doing a blind analysis, the answer would have been wrong anyway.)

   They finally decided that hiding the y-axis was sufficient to prevent this. I must admit, years after the fact, this sounded pretty paranoid to me, but I guess I was wrong…

3. **Seth Zenz**
   September 3, 2008

   Stan, the issue isn’t that the paper isn’t ready—it’s 100% ready and submitted. The issue that the journal won’t allow the collaboration to release plots officially while it’s waiting for publication.

4. **800GeV**
   September 3, 2008

   Stan, probably you remember that when your SNO collaborators presented their first results, they published the slides with the data on the web, together with a web page with extra details of the data (http://www.sno.phy.queensu.ca/sno/results_09_03/howto.ps), and the SNO paper promptly appeared on arXiv, and later on PRL.

   If you consider that the PAMELA collaborators did something different while knowing that one cannot be partly pregnant and that results shown at major conferences become public, maybe you can guess the answer to the question by Peter.

   Since some people confuse “stealing data” with sharing informations, it is better to wait the papers that will be published by PAMELA and …

5. **graviton383**
   September 3, 2008

   I find it embarassing that ambulance-chasing theorists would stoop to this level to beat out rivals for a publication. Everyone know what they did & they do themselves no credit by doing it…even if the experimenter giving the talk doesn’t mind.

6. **Ned Wright**
   September 3, 2008

   Nature is explicitly OK with posting preprints at the arxiv.
   Just don’t do press. See http://arxiv.org/abs/0802.1712

7. **Riccardo De Maria**
   September 4, 2008

   I don't see why a collaboration should protect data from the community. They can just release raw data as such and analyzed ones when ready and people can use whichever is more suitable for their research. Good ideas or valuable experimental data are more important than who delivers them, no?

8. **Thomas D**
   September 5, 2008

   Depends if anyone else knows what to do with the 'raw data' to get a correct analysis of the physics in it. You certainly don't want people to start putting out wrong analyses before you've brought yours to presentable form.

   Anyway 'raw data' never appear on conference talks except singly as pretty pictures (eg here's a track in our detector) from which no science follows.

   The usual situation is that someone will flash a slide of unpublished results with 'PRELIMINARY' over it, which certain favoured theorists/collaborators will already know about, and people in the audience can then write speculative papers at their own risk.

   What's happened here is just this Nature protocol that the journal wants an unusual degree of secrecy of the results over however long it takes to approve them for publication – and that technology has reached the point where people take digital pictures rather than make handwritten notes of what the curve looked like.

LHC Roundup

September 4, 2008
Categories: Uncategorized

The first attempt to circulate a beam in the LHC is still set for next Wednesday, and the media is already full of LHC stories, with a lot more to come next week. Events are being organized all over the world to celebrate the day, including a 1:30 am pajama party at Fermilab (see here). Today's Wall Street Journal carries a page one story about preparations at CERN that focus on improvisational comedy training for physicists to help them communicate.

For more serious news from the LHC, you can try following progress at CERN's startup site for the public, or at the technical LHC commissioning site. Latest available minutes from the Installation and Commissioning Committee are here, including a timeline and objectives for the next few days and for September 10. The "also going on" column for September 10 lists just "chaos".

Science magazine has some excellent LHC-related stories in this week's issue. In this one, various people explain what the LHC is looking for and why it will take a while to get results. Gordy Kane is having none of that though, predicting discovery of supersymmetry next month:

> "We predict a signature that they could see with five events," says Michigan's Kane. "They could see it in the first week of running in October."

Another article, Researchers, Place Your Bets!, features bloggers Tommaso Dorigo and Jacques Distler. Tommaso has bet that the LHC will see no deviations from the standard model, although from what I remember, he did this just because if this happens it will be so depressing that at least some cash will cheer him up. Gordy Kane and Stuart Raby claim supersymmetry is such a sure thing that they can't find anyone who will bet against it. Distler's comment on this is:

> I wonder how hard they tried.

The same article gives links to sites where you can bet on the Higgs boson discovery date.

Nature magazine is running an LHC-related editorial Cool Philosophies in this week's issue. It is inspired by an interesting recent preprint by philosopher of science Alexei Grinbaum: On the eve of the LHC: conceptual questions in high-energy physics. Grinbaum gives an extensive discussion of the current state of particle theory and its societal context. He ends with a philosophical section on fine-tuning and currently popular anthropic arguments, arguing that these often invoke an invalid use of counterfactuals.

Comments

1. **T**
   September 4, 2008

   Why does Gordy say this kind of thing? Such a respected theorist who shows absolutely zero understanding of experimental physics. They'll be lucky if they can understand a single part of the detector in a month of running. Let's just try to ease into calibration and hope nothing serious breaks.

2. **GR**
   September 4, 2008

   Well, I'm glad they're doing improv, if anything it should help improve matters in the communication-to-outsiders department. I'm part of an improv group here, and physics majors seem to be disproportionately represented, for whatever reason, and it really does help. A lot more people in physics should give it a try. Not the string theory people, though, as it seems they're already pretty good at spinning a yarn…

3. **anon.**
   September 5, 2008

   Never mind supersymmetry speculation. How long is it likely to take the LHC to check the electroweak symmetry breaking Higgs theories?

   In one Higgs field theory, there is supposed to be a doublet of Higgs bosons, both having +1 weak hypercharge; but one of them has +1/2 weak isotopic charge and +1 electric charge, while the other has -1/2 weak isotopic charge and no electric charge. There are also other more complex versions of the Higgs field including versions with supersymmetry.

   I'm wondering whether the LHC is really likely to check this part of the Standard Model, and identify the correct Higgs field (or if there is something else needed instead)? Is there any paper with assembled predictions from the landscape of different Higgs field theories. From my perspective, searching for supersymmetry (which doesn't really make falsifiable predictions since the energy of the hypothetical partners can be higher than experiments can test), isn't very exciting. It's like looking for life on Mars; it may or may not exist.

   But the Higgs field research is far more interesting because something has to give mass to the particles in the current Standard Model, and something has to break electroweak symmetry into electromagnetism and weak interactions at low energy.

4. **Peter Woit**
   September 5, 2008

   anon,

   In the SM, there is just one observable Higgs field (the other components of the field can be gauged away). It has electromagnetic charge 0, and all its couplings are precisely known once one knows its mass. For the expected mass range, the LHC will produce lots of Higgs particles no matter what the mass, but the backgrounds are different for different masses. The low end of the mass range is the most difficult, and my understanding is that if the Higgs is there, it will take until at least 2010 to see a signal.

   If you start adding in more Higgs fields, you will get much more complicated possibilities. In supersymmetric models these are more constrained, and also intensively studied. I don't know of references off-hand, but they can't be hard to find, this is an extremely intensively studied subject, and has been for decades now.

5. **graviton383**
   September 5, 2008

   To answer anon's question, it will take an integrated luminosity of ~10 fb^-1 or so
   AFTER the detectors are well understood to start covering all of the mass range

for the Higgs favored by fits to electroweak data. Some masses may take longer, some less. This sort of luminosity may take 2 years to accumulate & more to analyze so think 2010-11 just to be safe. If the Higgs idea is wrong, discovering the true nature of what replaces it may take somewhat longer requiring ~100 fb^-1 of data.

6. **anon.**
September 5, 2008

Thanks for these answers. Is the Higgs field is supposed provide the charge for quantum gravity, since mass is gravitational charge?

E.g. could there be a gauge group for quantum gravity, like U(1) for electromagnetism, in which the charge is the Higgs boson and the gauge boson is the graviton?

7. **a quantum diaries survivor**
September 5, 2008

Graviton,

while it is true that one first has to understand the data and only after can one look for a signal of a new particle, you gave the impression that the two phases are separate. Rather, it is a continuous improvement. The same data used to understand the detector response are also used for searches and measurements.

Indeed, what we will do first is to measure W and Z cross sections (which will be possible this fall, with thousands of them in the bag), top pair production (also easy with just 10 inverse picobarns, when about 8000 events per detector will be produced, let's say a thousand good ones to study), and high-Et jet spectra.
Missing energy will be tough -probably the one variable which will take more time to tune. It depends on a good calibration of jets, cosmics, out of time stuff, everything basically.

What sucks, if we think at the SM Higgs, is that the 1.7 sigma excess found by LEP II (mostly ALEPH in truth) will be the very last thing we can study. I imagine that in one year or so we will start excluding masses above 135 GeV, those that are already improbable by EW fits, and in the meantime already partly excluded by the Tevatron (which, next year, should probably exclude down to 150 GeV or so).

Another note on the bet reported on Science Magazine: Peter is correct. The article does not mention it, but mine is an insurance bet, in the style of a blackjack addict such as I am. That, however, does not change one fact: I do not believe in SUSY.

Cheers,
T.

8. **Victor**
September 5, 2008

"That, however, does not change one fact: I do not believe in SUSY."

This is not a particularly scientific attitude. Science isn't about belief, it's about facts. It concerns me that someone with obvious prejudice is involved in experiments which require an objective analysis.

9. **Peter Woit**
September 5, 2008

Victor,

I think Tommaso is just saying he thinks the probabilities are against SUSY. There's significant circumstantial evidence against SUSY: basically if it is doing what it is supposed to (stabilizing the electroweak scale), it's hard to understand why it hasn't shown up at the Tevatron (where Tommaso has spent much of his career working hard looking for SUSY or anything like it).

I don't think there's any danger at all that he or others at the LHC will not look just as hard for SUSY there as they have done at the Tevatron. Theorists may allow their investment in certain speculative ideas to affect their work, but experimentalists don't have that kind of investment in such ideas. One sad thing about the heavily ideological way in which theoretical particle physics has been pursued in recent years is that it gives people the wrong idea about how most scientists go about what they do. On the whole they're much more interested in finding something new than worrying about whether or not it fits some particular theoretical prejudice.

10. **a quantum diaries survivor**
September 6, 2008

Very well said Peter.

I can only add that personally, what I believe and what I do not has absolutely nothing to do with the way I perform an analysis.

My beliefs instead can (and should!) shape my decisions on what to work on – despite the fact that they are not, at least right now: my only Ph.D. student is working with me on something connected to SUSY. That, indeed, is the nice thing about being part of a large collaboration which will explore everything, with multiple teams in some instances assigned to the same topics.

When, however, decisions have to be taken concerning where to place the chips -say, in a trigger meeting where thresholds on physical objects are to be set on different data streams, to reduce the trigger accept rate facing an increase in the machine's instantaneous luminosity, and thereby affecting the discovery reach of different experimental signatures- well, there a leadership is needed to sort individual opinions out. Usually one just relies on the experiment's pre-defined goals. But, in principle, tough decisions may be required in some not-clear-cut cases, and then one's beliefs might in theory end up having their weight. That is the only case when I can give it to you – my (mild) bias against SUSY being the correct theory might suggest I would be unfit to lead such a group. It would still be a stretch. So ultimately, your concern is ill-posed.

Cheers,
T.

11. **Cormac O'Raifeartaigh**
September 6, 2008

Hi Peter, surprised to read "Gordy Kane and Stuart Raby claim supersymmetry is such a sure thing that they can't find anyone who will bet against it."

I thought Stephen Hawking had taken a bet that no physics beyond the Standard Model will be seen – have I got this wrong?
I personally would be happy to take a public bet that evidence of SUSY will be eventually be seen at the LHC – if only to publicise the theory, and to draw attention to the disgraceful fact that Ireland is not a member of CERN ..

12. **Victor**
September 6, 2008

Peter,
When you say that there is significant circumctantial evidence against SUSY, can you give a more detailed explanation of this statement? My understanding is that there is a lot of circumstantial evidence for SUSY as well, which is why there are many who expect to see it at LHC. As far as the hierarchy problem associated with the electroweak scale, I've been told that SUSY is still a viable solution for this, even if the supersymmetric particles are too heavy to have been seen at the Tevatron. What gives?

13. **woit**
September 6, 2008

Victor,

The fact that there were good arguments that supersymmetry should have shown up at the Tevatron and it didn't is the circumstantial evidence I'm referring to. It was discussed in another posting, see

http://www.math.columbia.edu/~woit/wordpress/?p=710#comment-40040

This evidence is not conclusive, but it seems fairly strong to me. The facts typically given by SUSY proponents as circumstantial evidence in its favor seem to me much weaker. Anyway, we'll see….

14. **Peter Woit**
September 7, 2008

Cormac,

If Hawking has bet against no physics beyond the SM, I hadn't heard about that, and it seems surprising. I don't know any reason why he would make such a bet. Perhaps you're confusing him with Tommaso…

15. **a quantum diaries survivor**
September 7, 2008

Hah, this would be the first time I get confused with a theoretician. And our accent is quite different also.

Cheers,
T.

16. **Manel**
September 7, 2008

The closest thing to what Cormac O'Raifeartaigh wrote that I know of is this:

http://img241.imageshack.us/img241/8962/susyzr5.png

17. **Coin**
September 7, 2008

Manel: I like 't Hooft's asterisk.

18. **Manel**
September 7, 2008

Yes, I agree. Sad but very plausible.

To what physics has sank into…

I still hope the LHC will have a positive and break the status quo and allow for real physics to be made.

19. **Cormac O Raifeartaigh**
September 8, 2008

Thanks Manel, fascinating sheet!
Tommaso, who have you taken the bet with? How about an inter-blog bet, that could be fun.

Apart from the two reasons above (to draw public attention to SUSY, and to the fact that Ireland is not a member of CERN), I do have another, slightly naive reason ..

If I understand right (as a humble experimentalist) SUSY, or something like it, is the only way around the comprehensive set of no-go theorems discovered in the 1960s – in other words, it is the last symmmetry left that could provide a unified framework that would include the strong force , and possibly gravity.

I find it hard to accept the unification of only 2 forces – and therefore plumb for SUSY, despite large uncertainties in the mechansim of SUSY breaking etc
Regards, Cormac

20. **Peter Woit**
September 8, 2008

Cormac,

Susy doesn't help at all to unify electroweak and strong. They're both internal symmetries. Attempts to unify these typically involve GUTs, but these have their problems, and the LHC will have nothing to say about the GUT issue.

21. **a quantum diaries survivor**
September 9, 2008

Hi Cormac,

my bet is indeed an inter-blog one, since both challengers (Distler, 750$, and Watts, 250$) have their own site.

Cheers,
T.

More LHC Predictions

September 8, 2008
Categories: Uncategorized

Roger Highfield has gone out and asked several theorists for LHC predictions, with the following results.

About supersymmetry:

- Arkani-Hamed

> My hunch is that there's a better than evens chance that supersymmetry will show up at the LHC…

- Veltman

> I would be surprised if supersymmetry were found. I supported the idea when it was first suggested, but I've gradually lost confidence in it, though I might well be wrong. To be sure, if the LHC finds nothing to support supersymmetry, its advocates will just make excuses and keep using it. As for string theory, it's all mumbo jumbo, with no connection with experiment.

- Silverstein

> Some of my intuition comes from string theory, an appealing candidate for a theory of all the forces of nature. According to many – perhaps most – versions of string theory, supersymmetry does not hold good at the energies probed by the LHC, so its discovery might require further explanation from this point of view.

(*it appears that the excuses Veltman is predicting are already in place…*)

- Llewellyn-Smith

> …(with 60% probability) supersymmetry…

- Lisi

> Many physicists also think it likely that evidence will be found for supersymmetry, strings, or new dimensions — but I disagree.

About the Higgs:

- Arkani-Hamed

> I've already bet a year's salary they will find the Higgs particle.

(*anyone know who took the other side of that bet?*)

- Veltman

> It would not surprise me if the experimenters don't find the Higgs particle. I don't trust the theory behind it. But if it does appear to show up, it will be crucial to check that it behaves as the theory predicts.

- Silverstein

> I'd be extremely puzzled if they don't find the Higgs…

- Llewellyn-Smith

> My hunch is that a Higgs boson will be found (95% probability)…

- Lisi

> The most likely result from the LHC is detection of a single Higgs particle.

John March-Russell goes all-out:

> …our quest for a source of almost unlimited climate-friendly energy might be answered by the creation of exotic unstable, but long-lived, charged particles… It might also turn out that the number of space and time dimensions is ambiguous…

Comments

1. **Sumar Ongi**
   September 8, 2008

   Taking just a superficial look at SUSY GUTs, their particle spectrum, and the SUSY desert, what comes to mind is that if Nature is *that* clumsy it may well not be worth studying.

2. **D R Lunsford**
   September 8, 2008

   I predict no single Higgs – if any it will be composite and technicolor will be revived.

   -drl

3. **Fran**
   September 8, 2008

Re: Veltman on Higgs: Since when was there doubt on the theoretical existance of the Higgs (I know there's doubt in its mass)? I thought it was pretty much a dead cert, as the rest of the Standard Model needs it in order to be correct/have mass?

4. **Peter Woit**
September 8, 2008

Fran,

What's certain is that something is breaking electroweak gauge symmetry. It's not at all certain that it's an elementary scalar field.

5. **Chris Oakley**
September 8, 2008

These exotic particles for catalysing fusion, which apparently do not need absurdly high temperatures to be produced, but appear to still require a Large Hadron Collider. How is one better off? Is Elvis is going to appear from the 6 1/2th dimension to show us what to do?

6. **James Robson**
September 8, 2008

Chris,

I, for one, am skeptical about seeing the Elvis Particle at the LHC – at least within the first 5 or so years beore upgrades to the luminosity and are made.

As is well known, the EP is expected to increase in mass and strangeness during its lifetime, which poses detection problems. Also, its influence on neighbouring particles is suspected to induce an "impersonation" interaction, which will make detection of the genuine article even more difficult.

Still, its pehaps more likely than some of the other suff mentioned.

7. **Mitch Miller**
September 8, 2008

Is Arkani-Hamed's statement supposed to be literal? I thought maybe he meant that he has already invested so much into the idea in terms of time and effort, but maybe not.

8. **Peter Woit**
September 8, 2008

Mitch,

He was quoted elsewhere saying he was willing to bet a year's salary on this, but I hadn't heard that anyone had taken him up on this. Perhaps this is just a misquote.

9. **db**
September 8, 2008

I'm no particle theorist, but I am willing to bet £50 (ukp) that the next generation of accelerator will be plagued by exactly the same doom criers as the LHC.

Furthermore, I bet 50p that anyone else with* BSc physics could come to the same conclusion.

*or without.

10. **chimpanzee (aka "joe")**
September 8, 2008

"Nature is predictably UNPREDICTABLE"

I am always amazed at "betting" amongst physicists, after all it is a "hard science". Look at planetary exploration in our localized domain (solar system, an inconspicuous thing in a inconspicuous part of the Milky Way, itself an inconspicuous galaxy, etc):

– wild volcanoes on Io
– unusual geology on moons of Jupiter & Saturn
– Comet Shoemaker-Levy 9
indicates the solar system is way more dynamic than originally thought (chance of extra-terrestial hits is not insignificant)
– now the TNO (Trans Neptunian Objects), Kuiper Belt Objects, etc
Michael Brown/Caltech's numerous discoveries of objects beyond Pluto

All the above "data points" were totally unexpected & beyond predictability.."it is what it is". The whole idea of cosmological predictions from such localized data is absurd!

"I'm locally pessimistic, but globally optimistic"
— Dr. Jordan Pollack, Brandeis Univ/CS Dept

Their "global optimism" is overstating the reality of the data..way too localized.

It's beyond the wildest imagination of a science-fiction author (after copious amounts of alchohol). "The universe is WEIRDER than you can imagine". I guarantee you, that the results will be surprising & suggest new questions (this always seems to be the case, unwrapping the successive "shells" of information seem to go on endlessly)

LHC is breaking into new energy domains, so expect new data points.

"In order to Push the Limits, sometimes you have to EXCEED THE LIMITS"
— Formula 1 Australian GP (2003)

LHC is pushing the limits, & like a Formula 1 race car (aka "knife edge car"..very unstable), it will be at the performance envelope of Technology/Science. Should be interesting!

"May you live in Interesting Times"

— Chinese proverb

11. **Manel**
September 8, 2008

    Again but still very relevant here:

    http://img241.imageshack.us/img241/8962/susyzr5.png

12. **mitchell porter**
September 9, 2008

    I once mailed Arkani-Hamed to ask if he was serious about betting a year's salary, because I might be interested to take him up, but I wanted to know the details. No reply.

13. **Chris Oakley**
September 9, 2008

    James,

    I was making a serious point. Physicists used to be taken seriously because people knew that if they did not give us what we wanted, we might just destroy the planet. But if senior physicists start bullsh*tting on about metastable particles that will solve all our energy problems then we will eventually just be regarded as a bunch of delusional, harmless hippies, no longer deserving of respect or fear. This could only harm funding prospects.

14. **Haelfix**
September 9, 2008

    Umm its a pretty damn good bet to wager a years salary on the detection of the Higgs (suitably phrased to allow for instance a doublet or somesuch). The scenarios where you would not see it (after a suitable time), are ugly and contrived and it really is an essential ensemble of probably the best tested theory in all of physics history. Its as close to a sure thing as anything in physics.

15. **David B.**
September 9, 2008

    Chris,

    The history of these `exotic particles' goes back to the days where nuclear physicists were studying muon catalized fusion. In principle, if you have a muon factory, you can use it to produce energy. A single muon could produce about 200 such reactions of combining deuterons into helium . The process would be stopped because there is a finite probability of capture of the muon by one of the helium atoms produced. After that happens, there is no time to recover the muon, since the muon decays. You are jut short of breaking even in terms of the energy cost to producing a muon, and the energy you extract from fusion.

    The reason that scenario has been put forward is because surprises happen in physics and that is one that has not been ruled out experimentally, yet. If it is there, however unlikely that is, wouldn't you want to know?

    I also object to your mischaracterization of physicists being funded because if they didn't give us what we wanted we would blow up the planet. The physicists did not hold the world hostage at any point. Technological applications for war purposes were a big deal during the cold war, that is why physics got such huge support: we could build the most destructive devices of all. Physicists were not feared. As a matter of fact, fear of technology is one of the most dangerous trends that can run against physics funding. All this hoopla about the LHC destroying the earth suggests that it will only get worse from now on, not better.

16. **DB**
September 9, 2008

    Hawking is on record as betting against the discovery of the Higgs Boson, although he hasn't exactly gone out on a limb – his bet is $100.

17. **James Robson**
September 9, 2008

    Chris,

    As you said, you were trying to make a serious point, and I respect that, and quite clearly I wasn't, so my apologies there if required. However, in a faint echo of David B's comment – do you really aspire to be "feared" by the general public for being a physicist?

    I think you should have chosen dentistry 🙂

    (Should have a smiley with teeth!)

18. **mike**
September 9, 2008

    Is there a good upper limit for the Higgs boson mass? It's disconcerting that without strong upper limits we will continue to search for something that might be a dead-end. I do think what we will see will hint at the existence of some scalar field, but it's characteristics might be much simpler than we think.

19. **bpz**
September 10, 2008

    "Is there a good upper limit for the Higgs boson mass?"
    In the MSSM it's about 200 GeV, in the pure SM it's about 800 Gev.

20. **Jimbo**
September 10, 2008

    BPZ,
    If you visit the link in my comment, you will find the number you seek. It is the 95%CL upper bound established by the Tevatron last Aug, of 144 Gev:
    http://www.physorg.com/news96040503.html

21. **Peter Woit**
September 10, 2008

mike,

There are two kinds of upper limits on Higgs masses:

1. In the SM, the Higgs mass is determined by the Higgs field self-coupling. To increase the mass, you have to increase this coupling constant, and beyond a certain point, there is no reason to trust perturbation theory. This is in conflict with the fact that the predictions of perturbation theory work very well.

2. Precision electroweak measurements provide indirect constraints on the Higgs mass, because it appears in the higher-loop theoretical predictions.

22. **Ellipsis**
September 11, 2008

Here's a prediction from last year that came true precisely (delay of two months beyond the at-that-time July scheduled start date):

http://www.math.columbia.edu/~woit/wordpress/?p=600#comment-28785
http://www.math.columbia.edu/~woit/wordpress/?p=608#comment-29531

So, I'll make a few more predictions:

3-sigma Higgs announced on July 28, 2010 simultaneously by CDF/D0 combination and by a shotgun combination of ATLAS and CMS data requested/required by CERN management.

Anomalies from ATLAS and CMS also announced in 2010, 2011, 2012 — all of them different and eliminated / made less significant by the following year's data (anomalies within the collaborations will fly continually, but 2010 will be the first year they are "vetted" and get through to public announcement, but still wrong).

First real anomaly that sticks will occur in 2013. This one will be a real one. There will be much heated debate as to whether it is from SUSY or not. The answer to that question will not be clear all the way until 2017.

Unlike Nima, Tommaso, Gordy Kane, etc., etc., I don't gamble, though, so no money.

23. **Peter Woit**
September 12, 2008

Thanks Ellipsis,

Your predictions look much more plausible to me than just about any others I've seen. Now, all you have to do is tell us what the 2010 Higgs mass will be….

24. **Ellipsis**
September 12, 2008

115.5 GeV

LHC Startup Tonight

September 9, 2008
Categories: Uncategorized

There's a media storm about the LHC building up as CERN makes last minute preparations to circulate a first beam in the machine. Cosmic Variance has live-blogging about this by a group of theorists, and Tommaso Dorigo will be in the CMS control room. He just might blog about this. For up-to-the-minute news, try the LHC beam commissioning site. Here's the plan for tomorrow, and the latest news: they're ready to get started at 6am tomorrow Geneva time. I'll be asleep.

Starting tomorrow, daily news reports about progress should appear here. They've got a very detailed plan for steps to go through, here's where they are now, here's where they hope to be Thursday and Friday.

Comments

1. **Kea**
   September 9, 2008

   I like how they arranged it so it was September 10 everywhere. I'm guessing the main event is scheduled for just before midnight here.

2. **Jason Starr**
   September 9, 2008

   The Boston Globe has a story today about how Frank Wilczek received death threats because of the LHC. Here is the URL,
   http://www.boston.com/news/local/breaking_news/2008/09/mit_physicist_g.html

3. **Yatima**
   September 10, 2008

   Well, this is more exciting than a Y2K upgrade of a billing system for sure. Will the threatening fantasy-literature-inspired ones realize that they have to keep the death threats coming until at least first collision (end of this year?), and possibly continuously afterwards, with exponentially decreasing threat level, then do it again when the machine is upgraded in 2010 or so?

4. **Steve Myers**
   September 10, 2008

   Read the affidavit of Luis Sancho concerning LHC dangers. It's at Harpers website.

5. **Peter Woit**
   September 10, 2008

   Everyone,

   Please, don't post anything on this blog about the supposed dangers of LHC-produced black holes. The way this issue dominates stories about the LHC is an excellent example of the sad phenomenon of attention-getting claims driving out intelligent discussion. I've heard way too much about this from too many people (including from the person discussed in the Wilczek story, who has also harassed me). Just stop, it's a waste of good brain cells.

6. **a quantum diaries survivor**
   September 10, 2008

   Peter, thank you for the link but, as you probably read already, my shift in the CMS control room got canceled late yesterday evening, because they wanted "experts" on site for each subdetector. For my own shift, which is attending the tracker, this is hilarious, since the tracker has been kept off for the entire day, given the unstable beam conditions one expects at startup.

   Sorry for the disappointment of your readers – I tried to post some useful information anyway today. Your link alone brought to my site about 500 people…

   Cheers,
   T.

7. **Jason Starr**
   September 10, 2008

   Sorry for posting the story.

Science in the 21st Century

September 10, 2008
Categories: Uncategorized

This week the Perimeter Institute is hosting an unusual conference on Science in the 21st Century. One of the organizers is Sabine Hossenfelder, who has a posting discussing the conference here, and may have some more about it at her blog later.

Many of the talks are now available on-line here. I've only had time to watch a couple of them, but one that I found worth paying attention to was Lee Smolin's. He covered some of the same issues discussed in his book, including the question of what science is, the ethics of how it is pursued, and the difficulties of encouraging new ideas. The discussion with the audience was also quite fascinating, including an exchange about differences between the American and British academic systems, with a British participant describing his shock at seeing how much the "American academic system is a training in sycophancy".

The topic of blogs came up mainly in a section where Smolin discussed the ethical importance of scientists putting their name and reputation behind what they have to say about their science. He characterized anonymous criticism as one of the main reasons for the low signal/noise ratio and nasty environment of the comment sections of many blogs, describing this as far worse than anything he had encountered in his professional career, and something that is giving science a bad name. The theoretical physics group at Harvard in the 1970s was given as an example of about the worst it could get in academia. At the end of the discussion session, Paul Ginsparg took him to task about this, saying that he had been there too and it wasn't that bad. I was there at the same time as both of them, and remember it as a rather unfriendly environment with a quite high arrogance level. But, with faculty like Coleman, Weinberg, Glashow, and postdocs like Witten, the talent and accomplishments of the people involved seemed to justify quite a bit of arrogance.

Ginsparg went on to agree with Smolin about anonymity on blogs, comparing trying to have a serious discussion in such an environment to trying to do so in a Fellini movie, being attacked by dwarves wearing masks.

Update: One talk I highly recommend is that of Eric Weinstein, with the title Sheldon Glashow Owes me a Dollar (and 17 years of interest): What happens in the marketplace of ideas when the endless frontier meets the efficient frontier? Eric's talk includes a wide variety of thought-provoking and entertaining attempts to bring ideas from economics and finance into thinking about how science gets done and whether it can be done more efficiently.

Comments

1. **srp**
   September 10, 2008

   Smolin seems to contradict himself, since he makes a big deal about the importance of reputation and accreditation but then goes on to say that authority shouldn't matter in deciding issues. Attacks on anonymity don't sit well with a commitment to judging arguments on their merits. The real issue with reputation and authority (from a functional point of view) is the tradeoff between effective attention allocation, on the one hand, and objectivity in assessing arguments on the other.

   If we had limitless attention, then ideally arguments would be evaluated independently of who makes them, which is why many journals require double-blind anonymity. Even if we stipulated that X is an idiot and a bad person, if X's argument A is correct and novel then we gain from understanding it. Because most people have trouble being that objective, separating the argument from the X label–making it anonymously–is often a good idea.

   The countervailing values to objectivity are not wasting your time reading things which are likely to be boring or wrong, and being able to rely on others' arguments as building blocks without personally auditing them. Here, we really want to know if argument A came from X or from Y because we can never get back the time lost from reading or relying on a defective A and we'd like to play the odds based on reputation. Anonymity weakens the likelihood of our taking an argument seriously, and, if it is prevalent, makes the entire discussion less useful.

   If people want to be taken seriously, or they want their arguments to be taken seriously, I don't see that there is a problem of incentive compatibility. The maker of an argument can weigh and balance the greater objectivity with which his or her contribution will be treated if it is anonymous against the impact on how much attention is paid to it. If they make a mistake in deciding this, they bear most of the cost. The Invisisble Hand, so to speak, will take care of the problem. (Of course, this analysis appies to scientific contributions, not personal rumor-mongering where the costs and benefits are distributed very differently.)

2. **Peter Woit**
   September 10, 2008

   srp,

   There's a difference between "reputation" in the sense of knowing about someone through their work and writings, and "reputation" in the sense of knowing where they stand in the academic hierarchy. I think Smolin is not arguing that you should judge whether to pay attention to someone's arguments based on "authority", but rather on your own knowledge of the quality of their previous arguments.

3. **Tony Smith**
   September 11, 2008

   Lee Smolin said (page 11 of 51 of the pdf of his talk):
   "… without a Ph.D. from a reputable research department or group
   (or in very rare cases i.e. Freeman Dyson, the equivalent)
   someone cannot make useful contributions to a scientific community.
   Scientific communities function well only because discussions among experts are restricted to those with a Ph.D. or at least those far along in a Ph.D. program …
   this is essential and not incidental …".

   Back in the 1980s when I was beginning to formulate my physics model, I asked Yuval Neeman to discuss it with me. He agreed to meet me in his office at U. Texas Austin, and we discussed what I was doing.
   He pointed out some problems with my model as it was back then (since then I have worked through those problems)
   and I asked him about getting a Ph.D. working on the model.
   He knew that I did not need a Ph.D. for a job (my law practice gave me both reasonable income and spare time in which to work on physics), and told me (a quote to the best of my recollection):

   "If your model turns out to be right, then it is important enough that you do not need a Ph.D.
   If your model turns out to be wrong, then no matter how many Ph.D.'s you have, it will still be wrong."

Until recently, I have felt that Yuval Neeman's advice would eventually be proven correct, and that my model would be evaluated on its merits.

Now, in light of the above statement of Lee Smolin, who seems to me to be the most liberal member of the physics community with respect to unconventional ideas,
I see that Yuval Neeman's advice will never be effective,
as my model will never be evaluated by the physics community.

Tony Smith (no physics Ph.D. or "the equivalent")

4. **Mark van Akeren**
September 11, 2008

My impression is that the frustration of many (but by far not all) theoretical particle physicists over the lack of a predictive model in particle physics is getting on their nerves. That should not be the case. Physics is much too interesting for this kind of resignative attitude.
The aim of physics is to find out how the world works, not to prove that it works following certain preset mechanisms.

There are sycophants in every community, from business to politics to science, and at every level.

A colleague of mine remembers how a Nobel prize winner (now dead) once abruptly stopped to talk to him at a dinner and turned away when he suggested that time might not be continuous.

In Italy and other countries, the sycophants in the community of historical science have managed to spread the idea that Galileo (victim of an anonymous denunciation, by the way – blogs did not exist then) did not have enough arguments for his astronomical statements. Look at what they have done to one of the greatest historians that Italy has ever had, Pietro Redondi.

For the situation in Russia, read the new book from Springer Verlag which tells how in the 1970 and 1980s in Moscow, Jewish math students were given math problems that were impossible to solve correctly, in order to keep them out of university.

I wonder how Smolin and Distler get along. The British comment that the "American academic system is a training in sycophancy" might be exaggerated, but a few people at the conference do embrace that rule, such as Distler himself. (Motl at least is an entertaining sycophant, Distler is not even that…)

Sycophants are at work everywhere. One has to live with them, and find one's own way.

Mark

5. **Christine**
September 11, 2008

Children are great scientists, the best. Carl Sagan noticed that very well. But when they grow up into adults, most of them loose their once genuine curiosity to really understand how the world works. They stop doing the right questions (usually on the mark) and the instinctive use of the scientific method through testing their ideas and hypothesis against what they observe. All the naturalness of the understanding process that encompasses a genuine scientific activity is somewhat lost.

What happens during the growing process of children into adults is quite complex, but should be taken into serious consideration, because something very essential and important is lost. Is it just innocence? If so, then we should be more "innocent". We should be more uncorrupted and pure. Science should be an open activity for all, and the level of exploring it only dependent on one's technical knowledge, honesty, wisdom. The merit of one's work should be evaluated against those qualities. PhD titles should be only marks that certifies that you have those qualities and that you have made contributions to the field. But someone without a PhD should not be dismissed from the scientific endeavor.

Of course, what I see in reality is many PhDs that are a complete distortion of what a scientist should be. And conversely, I know people without a PhD that would make honest scientists.

6. **Bee**
September 11, 2008

Hi Peter,

Thanks for the link. I will write some more about the conference if I find the time. The British participant is Harry Collins, who also gave the first talk yesterday morning. It is too bad that the recording of Steve Fuller's talk failed. It was a very interesting argument about how metascientific measures such as e.g. the citation index are invasive to science. Best,

Sabine

7. **Peter Woit**
September 11, 2008

Mark,

I've never seen anything to indicate that Distler is very far from the median of the sycophancy scale of American academia.

He was trained by the Harvard theory group, a few years after the period that Smolin and I were there. As I noted before, during this period this was a group that measured high on the arrogance scale, but at a time that they had quite a lot to be justifiably arrogant about. Unfortunately being convinced that you are always right and that people who disagree with you must be stupid goes from being annoying to seriously dysfunctional when you devote yourself to a research program that turns out not to work.

I'd suspect Smolin gets along fine with Distler, since he's not known for having trouble interacting with people he disagrees with, and his training at Harvard should stand him in good stead.

8. **Gil Kalai**
September 11, 2008

The utility of blog science-discussions for promoting various aspect of science – popularization of science, explaining scientific advances to professionals, evaluating various scientific theories and claims, discussing philosophical, sociological, and ethical aspects of science, and not the least blogs as a tool to conduct scientific research, is a very interesting question.

The norms and ethics of blogs themselves is also a very interesting topic, and so is the specific issue of anonimious contributions. Of course, there is a huge data to try to evaluate the quality of blog discussions and the role of anonymous comments. (One can compare, for example, blogs which allow anonymous participation with those which do not allow it.) Overall, I tend to agree with ‘spr”s comments on this issue.

I think that the main difficulty with blog science-discussions is the quick paste and huge volume. I do not see an easy way around this difficulty.

9. **Terry Hughes**
September 11, 2008

As many readers of this blog probably know, Oded Schramm just died in a fall while climbing:

http://www.nytimes.com/2008/09/11/science/11schramm.html?_r=1&oref=slogin

Reading the report of his death I was struck by how genuine his contribution as a pure mathematician to physics really was. I was especially struck by the comparision to the bloated claims of physical relevance of Singer, Witten and the rest in connection with Yau’s birthday conference. I see no mention of string theory in connection with Schramm, for example, just illumination of “a multitude of physics problems from the percolation of water through rocks to the tangling of polymers.” Actual, testable physics, not junk food for the mind. Even Yau’s interesting and charming contribution to general relativity is rather marginal to physics in the end. I mean no criticism of Yau, Singer, Witten or anyone else as pure mathematicians, but their claims to having created methematics that has central significance to real physics seem vastly overblown. Schramm seems to have been the real thing. May God receive him in peace.

10. **Christine**
September 11, 2008

I think scientific blogs should be like a virtual place for discussions, somewhat an extension of those discussions that usually take in real life coffee breaks among colleagues, but with the advantage of storage of what have been discussed, as well as other resources, like the use of latex for formulas (of course you may have that as well in real life, like a blackboard in the coffee room). And of course, links to the relevant sources, etc.

Although storage is a very nice resource, the problem is how to make the information in a blog useful and organized. I often see Sabine and Peter repeating themselves (on their point of views) many times, although their blogs are reasonably organized.

Regarding anonymous comments, I have mixed feelings. I have received in the past useful and reasonable anonymous comments, as well as rants from them. So it is a question for the owner of the blog to have a great effort to moderate.

Evidently, in real life one will not attack people during a scientific conversation, whatever the informal level of it, except if he/she is out of his mind (at least, he/she would be considered that way by some). I’ve never seen people attacking each other in such conversations if they don’t agree about a scientific question. In blogs, human aggressiveness is free to act.

I think that blogs in their present format are not the best tool for providing an informative, educational, scientifically sound environment for discussions, although these can be carried out at a certain extent in some carefully managed blogs. A better tool for that is still missing, but what is missing mostly is honesty, empathy and collaborative posture.

11. **Christine**
September 11, 2008

Ah, and of course, people should try to increase their attention spans. Blogs tend to train you the other way around.

12. **ht**
September 11, 2008

“What happens during the growing process of children into adults is quite complex, but should be taken into serious consideration, because something very essential and important is lost.”

Money (funding), ego, and power can get in the way of doing good science. Whereas children naturally ask a lot of “why” questions out of curiosity, if a junior researcher tries to do that in a science seminar, depending on the speaker, it could be taken as a challenge to his/her authority or competence rather than a genuine desire to understand. Who wants to offend unnecessarily in an already difficult academic environment? I’ve noticed that this tends to happen less often in mathematics, probably because of the requirement to prove every result regardless of your position in the hierarchy.

13. **Gil Kalai**
September 11, 2008

Anonymity can be quite useful if you would like to explore an idea without commiting to it.

14. **Peter Woit**
September 11, 2008

Gil,

…and really annoying to the people whose time you are wasting by trying to start a discussion about something that you haven’t bothered to think through yourself…

15. **Gil Kalai**
September 11, 2008

Peter,

in my opinion, science is a lot about the ability to explore ideas without commiting to them; so not commiting to an idea does not mean you haven’t bothered to think about it.

Anyway, the issue of blogging and science/mathematics is something which I did think about quite a bit and even tried to explore in various ways (including having my own blog). I am still not sure if scientific blogging is a good idea to start with (do you?). It can be a waste of time. But I think it is an interesting enough idea for me to explore, and to spend some time on. (In any case, on my blog, people are welcomed to contribute thoughtful comments that can be anonymous.)

16. **Michael Bacon**
September 11, 2008

Gill,

Would you support a practice that you only post anonymously on your own blog — solves your problem and Peter's.

17. **Eric Habegger**
September 11, 2008

There was one key statement in Lee's talk in which he said, and I'm paraphasing, that a person should be certified in the skills as a scientist to show that his "experiments" or "calculations" should have attention paid to them. This seems applicable to the technical requirements for being a scientist, which I have no argument with. But there is a very untechnical requirement for being a GOOD scientist and that is critical thinking. The ability of seeing the forest for the trees.

I've been fairly disappointed in Lee lately. I read his book and was quite impressed with the apparent openness to new ideas and with his differentiation between technical and visual skills. I sense that he has reconsidered those ideas and now counts the technical skills as being the main thing of importance. His new stance seems to eliminate the probability of giving access to science to those with more critical thinking and visual skills. I'm really sorry to see him going in that direction.

18. **Chris W.**
September 11, 2008

Eric,

Insofar as Lee's primary concern is with theoretical physics, your emphasis on visual skills seems misplaced. Certainly a capacity for critical thinking is essential (in every field!!). But what is meant by visual skills? Visual representations—of necessity in 2 or 3 dimensions in ordinary physical space—are at best a kind of language or symbolic scheme. Admittedly they can be powerful, but they can also be profoundly misleading. They must be engaged in a dialogue with formalism; at the one throws light on the other.

A more general comment on Christine's observations: Unfortunately, the ongoing professionalization of science brings into science the small-mindedness of adult professionals protective of their turf and livelihoods and intent on building careers. Ambitious and energetic young people can be every bit as guilty of this as older scientists.

I. I. Rabi observed (in John Rigden's biography of him) that a sort of religious attitude or selflessness is essential to great scientific work—something that goes beyond simply finding science fun or rewarding in a conventional professional sense.

(See this 1999 article about Rabi by John Rigden in *Physics World*.)

19. **Eric Habegger**
September 12, 2008

Chris,
I won't go too far into this to avoid annoying Peter. Visualizations are good things, and there are also mathematical formalizations that cannot be visualized. I was speaking of visualization in terms of Lee's idea of "seers" in which one can have a holistic vision. He seems to be moving away from that idea to the idea that those kind of people must pass muster just the way an accountant must pass technical equivalency tests. The fact that those two different types of scientists could have "equal" value seems now to be abandoned by him. He seems to be saying now that those square pegs must be pounded into the round hole to have any credibility, which I don't happen to agree with. In reality no scientist is completely one or the other but are shades of both. But the current system completely capitulates to encouraging just the one side.

20. **anonymous fool**
September 12, 2008

It seems to me that most of the noise on this blog comes from non-anonymous posters, though of course it would be a bad idea to name them.

I have no idea who is srp, but his/her post is probably the most interesting on the thread.

21. **Christine**
September 12, 2008

Chris W. wrote:

"Ambitious and energetic young people can be every bit as guilty of this as older scientists."

Well, I see ambition positively if you have the right amount of it. Life is too hard; if you do not have some ambition then you are lost. In any case, I came to observe that ambition generally develops more after/during teenage years, because of competitiveness in school or based on behaviour of parents. I believe that you are referring to youngs of that age or a little above. I have the impression that this competitiveness imprint and the need to develop a high level of ambition in order to deal with it is specially true in American society.

But as far as I could observe, young children in general are not naturally ambitious, in the very strong sense above. However, I may be biased because my observation is mainly from Brazilian children.

In any case, it appears to me that kids below ~10-12 years old or so preserve the very desirable characteristics of what makes a good scientist, from the aspect of making good questions, being critical and making observations, etc. Very young kids are usually great in that respect (~5-7 years old). If you have made a outreach science presentation in class to kids at that age, you know what I mean.

22. **M. Wang**
September 12, 2008

Mark van Akeren Says:
"There are sycophants in every community, from business to politics to science, and at every level."

This is certainly true. What sets academia above all the rest in terms of sycophancy, however, is the unchecked level of arrogance and the narrow career path.

In politics, it is very hard to get truly arrogant when one has to beg and/or bargain for votes every few years. In finance, the masters of universe know in their hearts that luck plays a big element in their success because every single trade in the market reminds them of the fact. in Academia, and theoretical particle physics in particular, such reality checks are sadly lacking in recent years.

Arrogant people expect sycophancy. The single file career progression in academic disciplines then amplifies the pressure to become a sycophant by many folds.

Confucius once said, "It is shameful to be a sycophant, but doubly so to force your subordinates into sycophancy." Tenured professors will do well to keep this in mind.

23. **srp**
September 12, 2008

Peter: You make a distinction without a difference. Regardless of whether authority is a matter of an individual's formal position or the quality of his previous arguments, the tradeoff I described still holds with respect to evaluating the NEXT argument offered. But there is more to the story if we think about the asymmetric information equilibrium.

If the argument is offered anonymously, it can be evaluated on its merits more easily but is less likely to get attention paid to it (assuming a high-authority individual). But for a low-authority individual, both incentives point in the same direction, so low-authority individuals should be more prone to adopt anonymity. Unfortunately, in equilibrium, this means that anonymous comments on average will come from lower-authority people and audiences will realize that and pay less attention to the substance of anonymous arguments. In turn, the anonymous arguer may be tempted to attract attention by being more sensational, caustic, or otherwise colorful.

I suppose these remarks apply self-referentially, as well, except that I'm not very colorful.

Anonymous fool: Thank you. I'm nobody in particular (an economist at a business school who thinks a lot about science and technology).

24. **Chris W.**
September 12, 2008

Thanks, Eric. I see what you're getting at now. I basically agree with you, although I'm not sure I would have chosen the word "holistic". Never mind that; it has often been observed about Einstein that as a young man he had an unmatched breadth as well as depth in his grasp of physics as it was known then. He truly embodied John Wheeler's injunction that a physicist should not be a specialist.

For our own time, I guess that implies that a willingness to attack deep problems may require taking greater risks of being dismissed as superficial or technically ill-equipped than in the past. In that context one could excuse Smolin for conceding that one ought to *at a minimum* secure a Ph.D. (Consider the case of Julian Barbour.)

25. **John Baez**
September 13, 2008

srp wrote:

> If we had limitless attention, then ideally arguments would be evaluated independently of who makes them…

Not clear. Our estimate of how often someone has been right is a useful piece of information in trying to guess if they're right this time. It's not obvious why we'd ever willingly give this up, except perhaps in unusual circumstances like refereeing an academic paper, which is presumably supposed to be completely lay out all the evidence for the claims it makes. Most arguments are not so fully laid out.

26. **Eric Weinstein**
September 13, 2008

Thanks Peter for the nice review. Part of the talk was actually informed by this blog; watching the reputational attacks between physics PhDs had been quite puzzling to me as a market enthusiast.

If you ever want to try to tease some of these issues out using the blog in an effort to improve the level of discourse, I'd be game to try.

Appreciatively,

Eric

27. **Carl Brannen**
September 17, 2008

Bee writes that Harry Collins is at the conference.

His book Gravity's Shadow is about the search for gravity waves but it is also a great introduction to the sociology of physics.

I think that most of the complaints about "my theory won't get a fair hearing" (because I don't have a PhD) are highly exaggerated. The professionals have the same trouble getting attention to their work. I am a very lucky amateur in that regard.

28. **De Bunker**
September 30, 2008

Having just started an anonymous physics blog…I feel compelled to react.

First, anonymity has an important place in science: that of peer review. In fact I wish peer review were double-blind, to eliminate some of the politics. I think it's totally appropriate for people to anonymously criticize each other, even in public and even on blogs.

In my experience when people first encounter the drivel that fills the comments section of any blog, they're substantially taken aback. The dynamics of anonymous conversation are different than the dynamics of face-to-face conversation. People get inflamed more easily, and are more likely to post hostile comments. They are also off-the-cuff, so one can't expect much in the way of scientific rigor, even on blogs run by scientists. (But I'm new to this game, maybe Peter feels differently)

But distaste for the different dynamics is not sufficient, in my view, to deny people the right to say what they *really* think without fear of retribution. Let's face it, young people won't criticize older more established physicists…unless they can do it anonymously. And they should still be able to get a job afterwards.

Why am I anonymous? Because I didn't want my blog to be seen as an an "official" CERN blog. I wouldn't have any problem attaching my name to the things I've written there. But then I think I may be bolder than some, and physics should able to use the work of great minds who are more timid.

29. **Eric Weinstein**
September 30, 2008

"Let's face it, young people won't criticize older more established physicists…unless they can do it anonymously. And they should still be able to get a job afterwards."

Fascinating. I had a completely different view of science in general and physics in particular. I can't fit this with what I hear of Feynman, Pauli, Yang, Schwinger, Weinberg, Dyson and others as young people. Can you elaborate?

More importantly, what do you require in terms of security that you would feel comfortable doing your job as a (non-anonymous) scientist?

30. **Peter Woit**
September 30, 2008

De Bunker,

The problem with your analogy with peer review is that

1. a referee is not anonymous to the journal editor who chose him/her. Acting unprofessionally here carries a real cost in one's reputation, since the editor knows who you are, will judge you accordingly, and may even discuss your behavior with his or her 100 closest colleagues.

2. a referee report is not a public document, findable with a Google search. If a referee chooses to say "the author is incompetent, an unpleasant asshole, and is known by his friends to have sex with barnyard animals", the only people who see this are the editor and the author. In a blog entry or comment, the whole world can see, and someone can easily find that one of the top entries on Google that appears if one searches on their name is something like this.

You might see this issue a bit differently if you were to experience the phenomenon of someone trying to destroy your professional reputation by posting on blogs things that are not true while hiding behind anonymity. Unfortunately, this is not unheard of.

I'm not an absolutist, sometimes there are good reasons for anonymity. On the whole though, it is over-used, and this does damage to the overall level of discussion on the internet. My bottom line is that I think people should be discouraged from using anonymity, but allowed to do so as long as they behave professionally. Attacking someone else unfairly in public while hiding behind anonymity is the kind of thing that I see as way out of bounds. I do my best to stop it from happening on my blog.

The use of anonymity by you and "Jester" is a bit unusual though, since I and presumably many others know who you are; you're not trying hard to hide yourselves. I actually don't believe it's possible for a scientist to write a regular blog about their field while staying completely anonymous. Scientific fields are just too small communities: if you write enough, sooner or later your colleagues will guess who you are. Anonymous comments are different, there one can hit and run…

31. **De Bunker**
September 30, 2008

> Fascinating. I had a completely different view of science in general and physics in particular. I can't fit this with what I hear of Feynman, Pauli, Yang, Schwinger, Weinberg, Dyson and others as young people. Can you elaborate?

My statement is one about humans, not about physicists in particular, and I think it's rather obvious. People do not tend to rock the boat. They do what the herd is doing, most of the time. Opposition to the herd should be possible, and should be possible by smart people who are not as outspoken as e.g. Feynman.

> On the whole though, [anonymity] is over-used, and this does damage to the overall level of discussion on the internet

It certainly does damage. People must learn to treat anonymous comments like a bathroom wall.

Sometimes, anonymity is sorely needed. When it is not needed it is difficult to see why one would want it. (Indeed, making my blog anonymous certainly was not necessary) These times are exceptional, but important. They always involve a weak person challenging a strong person.

Reputation attacks can be dealt with…most civilized societies have slander and libel laws, and these kinds of attacks existed long before the internet, and don't necessarily have anything to do with anonymity.

Don’t Buy Into the Supercollider Hype

September 13, 2008
Categories: Uncategorized

Some wag at the Wall Street Journal put the headline [Don’t Buy Into the Supercollider Hype] on today’s Op-Ed piece by Michio Kaku about the LHC, which describes its significance as follows:

> The LHC might shed light on the “theory of everything,” a single theory which can explain all fundamental forces of the universe, a theory which eluded Albert Einstein for the last 30 years of his life. This is the Holy Grail of physics. Einstein hoped it would allow us to “read the Mind of God.”
>
> Today, the leading (and only) candidate for this fabled theory of everything is called “string theory,” which is what I do for for a living. Our visible universe, according to this theory, represents only the lowest vibration of tiny vibrating strings. The LHC might find something called “sparticles,” or super particles, which represent higher vibrations of the string. If so, the LHC might even verify the existence of higher dimensions of space-time, which would truly be an earth-shaking discovery.

If I were an experimentalist or accelerator scientist working on the LHC, I might have a problem with the fact that the biggest media outlets are having theorists, often string theorists, be the ones to tell the public about the LHC (yesterday was Brian Greene’s turn, in the New York Times). Many such stories imply that the LHC will somehow tell us something about string theory, while even one of the blogosphere’s most enthusiastic string theory supporters puts the probability of this at about half of one-percent.

For some hype-free LHC predictions based on serious science that I fully endorse, see Resonaances, where the probability of seeing anything relevant to string theory isn’t even listed, and supersymmetry is given a one-tenth of one percent chance, on the grounds:

> 1% is a typical fine-tuning of susy models, and the additional factor of .1 is because it makes me puke.

which seems about right. The probability of the LHC producing black holes is given as something exponentially small, somewhat less than the probability of producing dragons.

Comments

1. **Aaron Bergman**
   September 13, 2008

   I think the reason for the op-eds is that theorists are the ones who write books and, consequently, have a higher media profile. If you’re an editor, wouldn’t you rather have an op-ed by someone the public has heard of rather than someone closer to the experiment but a total unknown?

2. **Susy Q**
   September 13, 2008

   Actually, anyone who’s really objective would asses the probability of observing supersymmetry at LHC as more like 70%. And regarding the Higgs mass, the most likely value is mH = 117 +/- 2 GeV, a result which one gets from the MSSM with radiative electroweak symmetry breaking, when combined with other experimental constraints.

3. **helvio**
   September 13, 2008

   You’re right, maybe they should have invited LQG theorists to write the op-ed’s, about the search for particles they don’t even have in their theories 😉 If evidence for extra-dimensions is found, than I see it as a full victory of string theory, not of Kaluza and Klein, or of a particular phenomenological model. The reason is that any phenomenological model with extra-dimensions imposes the additional dimensions as a working hypothesis, while in string theory it’s a consequence, an absolute necessity! Same with supersymmetry. The Higgs might be less clear that it is a direct consequence of string theory, but it’s also much less exciting theoretically as if you find evidence for the other two.

4. **joe**
   September 13, 2008

   > You’re right, maybe they should have invited LQG theorists to write the op-ed’s,

   Most definitely not. If I understood correctly, the point is that the experimentalists should be the stars here, since it’s they who are most closely related to the machine. I thought exactly the same thing yesterday when I read Greene’s article. Quite a good article, by the way, but could well have been written by Aymar, or someone else in direct contact with the LHC.

5. **joe**
   September 13, 2008

   > Similarly, once the hyperventilating critics get bored with the LHC and find something else to pounce on, science will move on to unlock the secrets of Genesis.

   Reading this final sentence of Kaku’s article, I wonder what hype we shouldn’t be buying according to him. The idea seems to be to replace all the Apocalypse crap by Genesis crap.

6. **bpz**
   September 13, 2008

   Wow, Jester just put $10000 in a bet with LM. I do admire people who are willing to put the money where their mouth is, unlike some other critics of SUSY.

7. **woit**
   September 13, 2008

   bpz,

   Betting with Lubos has its problems, including that of counterparty risk. I was tempted for a while by the idea of contacting Kane or Raby to offer a bet, but two things stopped me:

1. While I think any of the known SUSY extensions of the SM is worth betting against (because if they had anything to do with the electroweak breaking scale, we'd have seen evidence already, and the need for SUSY breaking makes the things ugly), there is some beautiful underlying mathematics, and I wouldn't want to bet against the existence of some still-uninvestigated "twisted" form of SUSY.

2. As ellipsis and others have noted, just about any experimental anomaly can be explained by invoking some SUSY model or other. I think there's a pretty high probability that at some point there will be a claim of seeing something that doesn't agree with the SM coming out of the LHC experiments. This may turn out to be something that goes away on closer investigation, or it may survive and be very interesting, but in either case we'll see all sorts of claims that this is "evidence of SUSY" and it may take a long time to sort these out.

One thing I'd be definitely willing to put some money down about would be string theory unification. But the problem with this is that it predicts nothing at all one way or another about what the LHC will see…

8. **Susy Q**
September 13, 2008

Peter,
You said, "because if they (the superpartners) had anything to do with the electroweak breaking scale, we'd have seen evidence already.."
I have to strongly disagree with this statement. Whether or not we see evidence of it or not depends on the exact masses of the superpartners, which we don't know. The only thing we do know is that they should be near the TeV scale if supersymmetry solves the hierarchy problem. It should be noted that this scale will only fully be explored by LHC, and so the superpartners should be seen there. In truth, superpartners are probably being created as we speak at the Tevatron, but not at a sufficient rate to generate a definite signal above the standard model background. On the other hand, the recent constraints on the Higgs mass being in the range 114-135 GeV strongly hint towards supersymmetry.

9. **woit**
September 13, 2008

Eric Mayes (=Susy Q = Victor),

Please stop posting the same argument using different pseudonyms, and ignoring my response. You're wasting my time.

http://www.math.columbia.edu/~woit/wordpress/?p=874#comment-43420

10. **R.A.H. (anon)**
September 13, 2008

this quote is from the cosmic variance blog cited above:

Evidence for or against String Theory: 0.5%. Our current understanding of string theory doesn't tell us which LHC-accessible models are or are not compatible with the theory; it may very well be true that they all are. But sometimes a surprising experimental result will put theorists on the right track, so who knows?

So, basically, the worlds greatest high energy physics experiment has a 0.5% chance to say anything abut the "leading" high energy physics theory of everything …

Maybe more is being said (by Sean Carroll) about this ToE than he realizes. This is an unbelievable admission of failure.

11. **a quantum diaries survivor**
September 13, 2008

Aaron, what you say sucks, unfortunately it is also probably true.

Cheers,
T.

12. **nbutsomebody**
September 13, 2008

How long will it take to observe Higgs and Susy in LHC ?(in case they are at all present)

13. **woit**
September 13, 2008

Aaron and Tommaso,

I agree. The answer is for more experimentalists to start writing books.

nbutsomebody,

There should be a FAQ somewhere…

I'll recommend these predictions:

http://www.math.columbia.edu/~woit/wordpress/?p=882#comment-43559

We don't know the mass of either the Higgs or the supposed superpartners. How hard it is to recognize these things depends on their mass. Many supersymmetry enthusiasts like models where the Higgs is low mass, and hard to see (a couple years or so required), whereas some superpartners are much easier, maybe could even be seen in data collected this fall (but probably not understood until some time next year).

14. **Experimentalist**
September 13, 2008

We're too busy doing physics

15. **muxu**
September 13, 2008

> How long will it take to observe Higgs and Susy in LHC ?

From the June issue of APS Physics,

> Abraham Seiden of the University of Santa Cruz presented a timeline plotting the data to be collected at the LHC as a function of time, pointing out where key expected discoveries are most likely to be made. Potential milestones include discovery of the Higgs particle around 2009, assuming it is around 200 GeV in mass. Should the Higgs be closer to 120 GeV in mass, the chart indicates discovery around 2011, since it is harder to detect at that lower energy because it decays into a key signature involving photons that is very similar to other decay signatures.
>
> LHC data should also provide evidence for supersymmetry in 2009 if the energy scale for supersymmetry breaking turns out to be 1 TeV. Should the appropriate energy scale be 3 TeV, that discovery would more likely show up much later, around 2017. If there are extra dimensions of space, scientists might be able to detect them when energy scales reach 9 TeV in 2012. Evidence for a new type of Z\' force, assuming it exists, is unlikely to be observed until at least 2019.

16. **James Robson**
September 13, 2008

I guess this is a bit obvious: but the days of being able to do significant fundamental physics experimets in your personal lab (or shed in the garden) seem to be gone. The LHC cost bilions. So, it is not suprising that theorists were/are involved in the PR for the LHC, and are needed to be vocal to gain funding: since it's (suspected) benefits are purely theoretical.

17. **Spear Mark**
September 13, 2008

Theorists are always the publicity seekers… Rutherford actually made contributions comparable to Einstein… Rutherford might even have contributed more… transmutation of elements, the structure of the atom, the proton, the scale of nuclear energies; also founding of a school that gave Chadwick, Cockroft, Walton, etc. But Einstein ends up being the man of the century, and Rutherford is forgotten (by the public).

The truth is that experimentalists are a little suspicious still of publicity seekers. George Smoot got hammered for `the face of god', and Rubbia and Lederman long ago stopped being truly influential.

18. **Shantanu**
September 13, 2008

Peter or others, Now that LHC has started is the Tevatron still going to
continue running? Also does anyone know if people made bets about tevatron and what it would find

19. **Peter Woit**
September 14, 2008

Shantanu,

The US HEP budget situation is extremely unclear. The new fiscal year starts soon, and it may not be until half-way through it that there actually is a budget. As far as I know, plan is tor run the Tevatron through the next fiscal year, future after that depends on the budget and how the LHC does. As long as continued running makes it possible the Tevatron will see the Higgs before the LHC, I doubt it will get shut down. I don't know of any Tevatron bets.

20. **H-I-G-G-S**
September 14, 2008

Of course Kaku is full of it in claiming that the LHC will find evidence of string theory, but what is really amusing is that he gets the physics of string theory wrong. The sparticles do not arise as higher vibrational modes of the string. If they did, they would be extremely massive and the LHC would never see them. The sparticles and particles of the Standard Model all arise as the lowest modes in superstring theory. Oh, and Kaku doesn't make a living doing string theory. He makes a living writing popular science books full of wild ideas and outrageous claims.

21. **Internets Was Yes!**
September 14, 2008

Does this mean there is a small chance of the LHC producing Dragons? Won't the black holes destroy them?

22. **jpd**
September 14, 2008

that sounds like an awesome movie.

23. **saiko**
September 14, 2008

> Oh, and Kaku doesn't make a living doing string theory. He makes a living writing popular science books full of wild ideas and outrageous claims.

The guy was writing papers on string field theory already in 1974, and got several hundred citations for them. He definitely has earned the right to call himself a string theorist, and a pioneer. There's no point denying that.

But no matter what his credentials are, his predictions for the LHC are going to be as hard tested as anyone else's. We'll see…

24. **Karla**
September 14, 2008

Wouldn't we consider either supersymmetry or large extra dimensions to be supporting evidence of string theory if either of them is observed at LHC? I know they wouldn't prove string theory by themselves, but both supersymmetry and extra dimensions are central to strings.

25. **Chris W.**
September 14, 2008

The US HEP budget situation is extremely unclear.

The US *federal* budget situation isn't so clear either. Let's hope China and Japan don't call in their chips.

26. **Aaron Bergman**
September 14, 2008

Supersymmetry is important in string theory, but I'd say at this point that the connection between TeV scale supersymmetry and string theory is far from decided. Extra dimensions, on the other hand, are such a bizarre idea from a purely phenomenological standpoint, that I would consider them evidence for string theory although many extra-dimension theories these days are not obviously embeddable in string theory.

27. **Peter Woit**
September 14, 2008

Karla,

Supersymmetry and extra dimensions at the LHC scale just aren't predictions in any sense of string theory so can't provide evidence for it. If either one is discovered, it won't tell us whether string theory is right or wrong. On the other hand it will be a massive revolution in fundamental physics, with a huge amount to be studied and understood. If this happens, string theorists will try and see if string theory has anything to say about what exactly is observed and is able to make testable predictions. If they can do this, string theory will be revived, if not it will continue on its path to irrelevance.

28. **Tom W.**
September 14, 2008

Karla,

The discovery of extra dimensions would be a much bigger story itself than any confirmation of string theory . Extra dimensions would be a paradigm shift beyond even the Copernican revolution, IMO. There would be big questions opened up about
the universe ranging from the smallest to the largest scales.

So your question is ironic. But it does also hint at the extent to which the string theory people have warped the fabric of modern thought…..that is they have somehow established the "tacit" acceptance of extra dimensions as a "given" and proceeded to
build castles of sand upon that compacted nonsense.

Perhaps I'm just so old fashoned, but shouldn't we first require the
string people to prove the existence of *one* extra dimension, followed by the verification of *eleven* dimensions, before anyone
would waste time to listen to their baroque ideas about what happens in those new dimensions?

What ever happened to doing science one step at a time?

29. **Tom W.**
September 14, 2008

Peter,

Sorry my post says essentially the same thing you posted….didn't see your words until I hit submit. Anyway, thanks for your
continued important work and belated birthday wishes.

TW

30. **Haelfix**
September 14, 2008

Supersymmetry at .1% confidence is surely low. Its still the simplest and most natural solution to the hierarchy problem that we currently know off. Yes its bizarre that it hasn't already shown up (either directly or indirectly), but it still has a nice parameter space to live in, and one which still leads to a satisfying solution as well as giving a dark matter candidate.

Putting it above 50% confidence is perhaps a little optimistic (nature loves tricking us), but something like 33% is still where i'd imagine most phenomenologists/theorists would end up averaging out too.

31. **Kay zum Felde**
September 14, 2008

Hi,

yes, I think this hype could be really a problem. The successful start of the LHC has been broadcasted on 'Phoenix' (a TV broadcaster in Germany). A physicist from Bonn has been a guest in the studio of 'Phoenix'. He mentioned again, that LHC will not be able to mimic the Big Bang. However the moderator didn't ask further about this issue. What has been pointed out by the physicist was that the technology behind the LHC has been and will lead to valuable technology spin-offs. This seems to be really important.

When I talk with people about the LHC project, they are interested but often they want to know about the economic outcome of the experiments. I usually ask them, if they are not eager to know how the universe was born, or in other fundamental questions. I receive distinguished answers. I think people become somehow confused, since they obviously have other issues they are much more concerned about. No one can blame them for that, but this bothers me often. That's because I often prejudge them, since I guess financial issues are these other issues. They seem not knowing that the technology behind the LHC is almost entirely extremely challenging and new, and that the knowledge that society is gaining through such a project is worth much more valuable than the money that this project costs.

Kind regards

Kay zum Felde

32. **chris**
September 15, 2008

haelfix,

for my taste susy at 0.1% is just right. after all, it does not really solve the hierarchy problem, it merely ammeliorates it (or shift the fine tuning to other parameters as others might say).

saiko,

yes, that was in 74 and what exactly did he produce since then? i do not mean to be disrespectful, but there are a fair number of theorists even who would have much more of a standing to oped on a prestigeous journal than kaku. how about weinberg, gross or glashow? or yes, even greene for that matter, but who is kaku?

33. **Mikael**
September 15, 2008

Peter,
I think you are obsessed too much with testable predictions. Surely this is what we want from a theory of physics which we regard a success. String theory is, as I think everybody would agree, work in progress. And so the only question is whether it is promising. The problem is that the answer to this question is subjective, although not arbitrary, because any opinion on that has to be qualified.
Surely discovery of supersymmetry and even more extra dimension would let string theory look more promising than today.

34. **Chris W.**
September 15, 2008

What would constitute evidence for extra spatial dimensions, as opposed to evidence (confirmation of) a prediction of a theory that happens to incorporate extra spatial dimensions? Is there anything like an unambiguous phenomenological characterization of evidence for extra dimensions, with little or no model dependence?

It would seem that compactified extra dimensions complicate the question considerably.

35. **Peter Woit**
September 15, 2008

Mikael,

The problem with string theory is not that it doesn't make testable predictions, but that the more we learn about it, the more it becomes clear that it can't ever lead to testable predictions. If that's an "obsession with testable predictions", I guess I'm obsessed…

36. **Mikael**
September 16, 2008

Peter,
what do you think of the work of Vafa presented at string 08?
He is trying to get at some phenomenolgy.

37. **Peter Woit**
September 16, 2008

Mikael,

This is getting completely off-topic, but see

http://www.math.columbia.edu/~woit/wordpress/?p=697

38. **Coin**
September 16, 2008

What would constitute evidence for extra spatial dimensions, as opposed to evidence (confirmation of) a prediction of a theory that happens to incorporate extra spatial dimensions? Is there anything like an unambiguous phenomenological characterization of evidence for extra dimensions, with little or no model dependence?

Actually– and this is just my understanding from what I've read, so if I mess this up someone please let me know– yes:

The "mini black holes" prediction you see batted around as a long-shot possibility at the LHC is actually specifically a confirmation of "large extra dimensions". The thing is that the LHC is not, in fact, running at high enough energies to create black holes; however, if there were large extra dimensions (where "large" just means "noncompact") then there would be an effect where gravity is massively stronger over extremely short distances. This is because in a >4-dimensional universe gravity would follow not the inverse square law, but an inverse cube law or an inverse-256 law or something; however, it would also be the case that gravitons would be free to leak out into the above-4 dimensions, so past very very short distances this would smooth out into the normal inverse square law*. If this effect exists, then the difficulty of creating microscopic black holes is much lower, maybe low enough that the LHC would create them.

So if we see microscopic black holes at the LHC, then as I understand things this tells us there are extra noncompact dimensions; but it doesn't by itself tell us how many dimensions there are**, and it doesn't by itself tell us whether string theory is true. Of course since string theory provides us with a relatively rich working model of how and why our universe could be a 4D sliver in a higher-dimensional bulk, and nobody else I'm aware of in physics is predicting such a strange thing, then mini black holes would tend to be interpreted as proof of string theory.

* It isn't clear to me whether this detail– the leaky gravitons thing– is something specific to and/or required by LED string theory, or if it's just a general property of large extra dimensions scenarios.

** Although there's been at least one paper suggesting that if a mini black hole *were* seen in a particle accelerator, observations of that mini black hole and its evaporation could be used to tell you various things about the extra dimensions that allowed that black hole to form. And of course being able to take observations of an actual black hole, ever, would probably tell us about quantum gravity in a few instants than we've figured out in a century…

News From All Over

September 16, 2008
Categories: Uncategorized

There are two new particle theory blogs that I've noticed recently: Shores of the Dirac Sea, where David Berenstein and Moshe Rozali are blogging and the Physics Anti-Crackpot Blog, by an anonymous author at CERN. I don't know who this last blogger is, but he joins his (or her) colleague Jester at Resonaances (who is anonymous, but not hard to figure out) in skepticism about supersymmetry. Maybe things at CERN-TH are such that anonymity is a good idea if you hold such opinions….

Also at CERN, news of progress on the LHC commissioning is here. It seems that after some initial quick successes, there's no beam at the moment as they fix various problems with the machine.

Frank Wilczek doesn't have a blog (although his wife Betsy Devine does), but he does now have a web-site, as well as another web-site for his new book The Lightness of Being . Here's a summary of the book, and a sample chapter, which gives you an idea what he's trying to do with the book.

The IAS has started to put some lectures on-line, including the latest summer school on string theory and the memorial for Selberg.

The Harvard physics department now is running a video archive. It includes video of their recent Colloquia, Loeb lectures, and Sydney Coleman's quantum field theory course.

For the latest on INSPIRE, SCOAP^3, and potential changes in how the on-line physics literature works, see this interview with DESY's Annette Holkamp (via Travis Brooks at Symmetry Breaking).

Update: There seems to have been some sort of problem in Sector 34 triggered by powering tests for operation at 5 TeV. This caused a large helium leak in the tunnel, an investigation is under way. More here.

Update: The news from the LHC is pretty bad. A failure during a powering test will require warming up the entire sector to fix the problem, then cooling it back down. This means that it will be another two months before beam commissioning efforts can start again, likely pushing physics collisions off until next spring, after the winter shutdown. There's a press release here.

Comments

1. **Mark R**
   September 16, 2008

   Gosh, those are some great resources, Peter. Thank you!

2. **Chris W.**
   September 16, 2008

   See this rather good popular exposition on the Higgs boson in Slate, by doctoral candidate James Owen Weatherall.

3. **Moshe**
   September 16, 2008

   Thanks for the link Peter.

4. **James Robson**
   September 16, 2008

   On a slightly different CERN topic, and given that I believe you have some responsibility for mantaining your department's computer network, even if the LHC produces no evidence for new physics, do you think we'll get cool new technologies in computer networking spinning off as has happened before?

5. **Peter Woit**
   September 16, 2008

   Moshe,

   Good luck with the blog, I look forward to following it.

   James,

   My great achievement in computer networking this summer was to get the university to replace the math building's 25 year-old wiring by something more modern. LHC-scale networking is something way beyond my expertise, but my understanding is that they are pushing the boundaries of some technologies. Unlikely to be a new development like the world-wide web, but maybe some new technology of wider use will come out of it.

6. **De Bunker**
   September 17, 2008

   My blog (anticrackpot) is not anti-SUSY. I'll withold my private beliefs for now… I set up the SUSY Prediction Market because of Jester's (RESONAANCES) bet (which I feel was a bad bet, not for the subject of the bet but for the terms — negligible upside and huge downside, and he's betting against the crowd which normally has a small downside and big upside). I set the SUSY market to start at 50%, and I'm barred from trading on it since I started it. The movement downward from there is not due to me. 😉

   All you out there who think you know the answer, place your bets!

   My anonymity so far has more to do with the fact that I wanted to combat "LHC disaster" nutters, and didn't want this to look like an "official" CERN publication. Also "anticrackpot" is rather confrontational. I think I will change the name if I keep blogging for long, besides I'm really burning out on combating nutters.

7. **Marcus**
   September 17, 2008

Better check spelling of the name Betsy. Thanks for the links!

8. **Peter Woit**
September 17, 2008

   Marcus,

   Thanks, fixed.

   De Bunker,

   Sorry for misrepresenting your views. I was going by a comment of yours at Resonaances that you would bet strongly against SUSY.

   An anti-crackpot campaign I fear is an endless and thankless task., Good luck with it though, someone should be doing it. I've tried to just ignore the LHC/black-hole nonsense thinking that starving it of attention is the way to go, but there do need to be some people out there dealing with it. I'll stick to complaining about creeping crackpotism in the mainstream….

   Best wishes for the blog, the more non-crackpot ones about physics the better!

9. **Aki**
September 17, 2008

   "Unlikely to be a new development like the world-wide web, but maybe some new technology of wider use will come out of it."

   Whether they find what they're looking for or not, the LHC gives a boost to Linux crusaders.

   http://blog.internetnews.com/skerner/2008/09/large-hadron-collider—powere.html

10. **All rights reversed**
September 17, 2008

    You can try to guess who Jester is by checking his reflected image off the glass CERN main entrance in the picture he took of the one and only protester during Day Zero.

11. **De Bunker**
September 17, 2008

    "I was going by a comment of yours at Resonaances that you would bet strongly against SUSY."

    Shit, you found me out. The interwebs is following me! Now if only I hadn't written all those SUSY papers…

12. **Yatima**
September 17, 2008

    > the one and only protester during Day Zero.

    Actually "first beam day", thus a no-black-holes-for-sure day. Good this is not the olde USA otherwise the protester would have come bearded & armed.

    The Economist has a review of Frank Wilczek's book, basically saying that it needs some additional editorial work. Still, I gotta check it out.

13. **James Robson**
September 19, 2008

    Well you can't build a fancy network without decent infrasrucure – or wiring – so well done!

    Incidentaly, I had a look at previous articles on this blog, and also at the comments, but couldn't find any mention of the fact that string theorists seem to have been let into the fold: no longer segregated, just part of the general physics links…

14. **Peter Woit**
September 19, 2008

    James,

    I reorganized things since there seemed to no longer be such a thing as a string theory blog. Other than Lubos, few if any string theorists seem to want to write about the subject anymore. Blogs written by string theorists these days are pretty much about anything but string theory.

15. **Moshe**
September 19, 2008

    I don't know about others, but I am sure we will have a lot of string theory content. Since I think there is a continuum between string theory and the rest of high energy physics, having one list is probably less confusing.

16. **Coin**
September 19, 2008

    This caused a large helium leak in the tunnel, an investigation is under way

    Hmm, isn't this something the detector folks would have been actually hoping for a year ago? 😛

17. **Jonathan Vos Post**
September 19, 2008

    Thank you for the hotlink to the sample chapter of Frank Wilczek 's new book The Lightness of Being. I printed it, and handed out photocopies to my 10th and 11th graders (and a few top 9th graders) to read over the weekend. Wonderful stuff!

18. **Marcus**
September 19, 2008

Wonderful stuff!
I agree with you on that. It"s a great book.

19. **a quantum diaries survivor**
September 20, 2008

Hi Coin,

the helium leak was in the tunnel, not in the beam pipe… At this point, with the detectors fully commissioned, this incident is a real curse. We were putting lots of hope in the small datasets we would collect this fall: even 5 inverse picobarns, even at 10 TeV, would still allow some important tunings and the determination of several important parameters. We would measure W,Z,top quark production, and do new physics searches. Enough to keep us busy during the winter!

Maybe the LHC is indeed cursed by backward causality, the same way two folks have claimed the SSC was killed.

Cheers,
T.

20. **outsider**
September 20, 2008

Why does CERN have this seasonal schedule? Why can't it run year round, once it's up and running? This "Fall run" is being portrayed as if it is a limited window of opportunity, but I don't understand why they can't just keep going with whatever they wanted to do later anyway.

21. **woit**
September 20, 2008

outsider,

My understanding is that CERN is committed to not operating its accelerators during the winter months because the demand they make on the power grid is so large that power needed for heating homes and businesses would not be available.

22. **Fred**
September 21, 2008

Thanks for the links, and the anti-susy movement 🙂
About the LHC crackpot someone (I don't know who?) bought a nice domain name:
hasthelhcdestroyedtheearth

23. **Coin**
September 22, 2008

Dorigo, thanks for the explanation!

By the way, a question about the LHC's winter shutdowns to save power: Is this normal, or is this just a quirk of placing one's accelerator in Switzerland? Does the Tevatron in Illinois do something similar, for example?

Far Off-topic

September 20, 2008
Categories: Uncategorized

This posting will quickly veer far off the usual topics of this blog into political issues that I normally avoid like the plague. For once, the issues involved seem important enough to interrupt your regularly-scheduled programming. I hope to not ever repeat this in future postings.

Over the last 20 years I've seen an increasingly large number of colleagues, students and friends leave academia to go to work in the financial industry. Many were hired as "quants", working on mathematically sophisticated models for valuing various financial instruments. During the same period, I've watched New York City change in dramatic ways, driven by the vast wealth flowing into the financial industry here. I've seen recent estimates that half the personal income on the island of Manhattan has been going to the 20 percent or so of the population that work in finance-related jobs. The effects of this wealth include something like a five-fold increase in apartment prices, with a modest two-bedroom apartment now selling for a million dollars. In many neighborhoods, a majority of the people on the sidewalks have a net worth above a million, and annual incomes of many hundreds of thousands of dollars. Not surprisingly, the streets are clean, buildings shiny and beautifully renovated, restaurants excellent and street crime non-existent. Banks have opened huge branches on every street corner.

For many years I couldn't figure out where all this money was coming from. When I'd ask people about this, I'd get a list of some of the things generating investment banking fees, but none of these seemed to add up to something that could provide the profits necessary to pay million-dollar bonuses to tens of thousands of people. Over the last year or so, the so-callled "credit crisis" has started to make clear what has been going on, and I (like many others, I suspect) have spent more time than is healthy following the story as it has unfolded.

It's a very complicated subject, but the most important part of it is relatively easy to understand, and there's not much disagreement about this. Starting about 10 years ago, housing prices in the United States began to increase dramatically, fed by low interest rates, and easy credit. A classic financial bubble developed as people borrowed ever-increasing amounts of money to invest in housing, sure that prices would keep going up. You can make a lot of money very fast this way. In 2006, housing prices nationwide had increased by a factor of 2.5 over the past ten years. This was the peak of the bubble and since then prices are off by 25%. They will still have to come down another 25% or so to get back to pre-bubble levels (inflation-adjusted).

The fall in prices has made a lot of housing worth less than the loans secured by it. More and more people have mortgages that cannot be refinanced and that they sometimes cannot afford, leading to foreclosure, or to a strong incentive to just leave and give the housing back to the bank. It turns out that one of the things the quants had been doing was developing pricing models for complex ways to market the risk associated with these loans. One of the sources of the huge income coming into Manhattan was the fees that this generated. The models being used turned out to be highly flawed, dramatically underestimating the fall-out from the all-too likely end to the bubble.

Since more than a couple ex-string theorists were involved in this, there's a temptation to make an analogy with the complicated failed models that they were trained in working on during their years in academia, but that would be highly unfair. Most of the flawed models were developed by people whose training had nothing to do with string theory, with the flaws coming from certain built-in assumptions. These assumptions were chosen because they allowed a lot of money to be made in the short-term, making many Manhattanites quite wealthy.

Now that the bubble has burst and it has become clear that the financial instruments created are worth far less than anyone had expected, the fundamental problem is that, absent some optimism about a turn-around in prices, it is likely that many US financial institutions are insolvent. Their assets may be worth less than their liabilities (depending on exactly how low housing prices go). As a result, their stock prices have collapsed, and no one is much interested in investing more money in them. The situation has gotten so bad in recent weeks that the normal operation of the credit markets is in danger of coming to a halt, as institutions stop trading with others out of fear that they will soon be bankrupt.

Today the Bush administration put out draft legislation to deal with the problem (see here). The solution proposed is strikingly simple: the Secretary of the Treasury will be given $700 billion to hand over to financial institutions in return for mortgage-related financial assets, as he sees fit. On news of this possibility the stocks of these institutions rose dramatically late Thursday and yesterday. Assuming that this is enough to make most of the insolvent institutions solvent again, this will allow them to return to business as usual and get the credit markets working smoothly again. If it's not enough, presumably Congress will just be asked to increase the amount.

Of course the devil is in the details, especially those concerning how Secretary Paulson will distribute the $700 billion. The plan seems to be to bring this legislation to a vote within days, unlinked to anything that would change the way the finance industry operates, or change the incentives that led to the current disaster.

Personally I think that, as economic policy, this is a really bad idea, for a host of reasons I won't go on about. But I'm no expert on these issues, so that opinion isn't worth very much and it's besides the point of this no-business-as-usual posting, which is the following:

The response to this that I have seen from Obama and the Democrats is extremely disturbing. Obama seems to be inclined to go along with this, as long as some aid to people who can't afford their mortgages is tacked on. This also appears to be the attitude of the Democratic congressional leadership, which includes senators Schumer and Clinton, acting in their roles as representatives of the largest industry in New York City. On the other hand, McCain appears to be choosing to take the populist position of ranting against Wall Street. It is now a few short weeks until the election, and I believe this will be the defining issue that decides it. If Obama and the Democrats support this bailout of the financial industry and McCain resists it in populist terms, I think we're in for four more years of irresponsible leadership. McCain has already done a good job of painting his opponent as an Eastern "elitist", and I can't believe he's too stupid to take advantage of the opportunity the Democrats will hand him if they vote for this legislation.

So, call and write your congressional representatives and the Obama campaign now.

For good sources to follow this story, there are some excellent blogs, including Calculated Risk and Naked Capitalism. This is also the kind of story on which some of the mainstream media shines, so read the New York Times, Wall Street Journal, and Financial Times.

Update: I hope Obama is reading not the Sunday New York Times which seems to indicate that this bailout of New York's main industry is essential, but Krugman's blog instead.

Update: Maybe Krugman is reading Not Even Wrong….

My reading now of what is going on is that Obama and the Democrats are starting to get a clue, based on seeing a firestorm of oppostion to the bail-out. The danger that they would go along with it seems to be receding. They can read polls too….

Comments

1. **Eli**
   September 20, 2008

   First you observe, "If Obama and the Democrats support this bailout of the financial industry and McCain resists it in populist terms, I think we're in for four more years of irresponsible leadership. " Then you suggest to lobby and support the presidential candidate and the party that just demonstrated irresponsible leadership on what you regard as the defining issue of the coming election. Where is logic?

2. **Little People**
   September 20, 2008

   Nationalizing corporative debts… where have I seen that before? This kind of scam used to be orchestrated by US corporations (and, probably, governments) in foreign countries. Now they are doing the same thing to their own people. I never thought I'd live to see this.

3. **Zeynel**
   September 20, 2008

   In one of the local newspapers here in Turkey a pandit wrote that "people who claimed that the Big Bang experiment will create a black hole that would swallow the world turned out to be right. They were only wrong in the place (or space) and the responsible parties. Big Bang did not happen in European Union's CERN but in the world's financial center Wall Street in the United States. And the people responsible for this were not physicists but bankers and institutions that evaluate credit worthiness. But as a result black holes resulting from the Big Bang happened and they swallow everything they encounter."

   I think that if the pandit I quoted knew that the bankers were using models developed by physicists he would have seen that his analogy is even better than he thinks. And isn't one of the reasons that string theory is so spectacularly popular in the academia even though it is a "complicated failed model" is because it allows physicists quick academic returns "in the short-term?"

4. **mario**
   September 20, 2008

   The banks were bailed out so that the black hole created by the Quants (not the guys at the LHC!) doesn't swallow the planet. I'd say this administration did the right thing – for once (although it was pretty much on gunpoint). Things will be bad, sure, but not nearly as horrible as they would have been.

   It is rather unfortunate that people do not understand this.

5. **Derek Teaney**
   September 20, 2008

   Dear Peter

   I enjoyed this last post quite a lot. I am not a financial
   expert either and do think that it is "Far Off-Topic"
   but I do think that it is right on topic discuss how many
   theoretical physicists have gone into the financial industry
   and the extent to which the complex financial instruments
   they have created (through confusion) the current financial
   situation. It is also should be realized that these very highly
   paid positions do not create real wealth the way that experimental
   physicists who invent the laser or invent the web do. I also
   wonder to what extent the malaise in theoretical physics,
   the general lack of funding, and lack of oppotunities in
   real industries, leads these individuals to enter the financial
   service sector.
   these individuals

6. **imho**
   September 20, 2008

   Hi Peter,

   Don't confuse the "correct policy" with the "correct politics". They are two completely different things. It is in no way clear to me, with the media screaming "the sky is falling", that McCain is in the better political position.

   Regarding the correct policy. You seem to want to punish wall street for their recklessness by letting the global financial system freeze up. That probably isn't a good idea.

7. **P**
   September 20, 2008

   I think this crisis was partially created by the government-both the Clinton and Bush administrations-pushing bankers to issue risky loans in order to promote the common good:

   http://www.openmarket.org/2008/09/19/trillion-dollar-bailout-will-lead-to-future-bubbles/?disqus_reply=2501529#comment-2501529

8. **Chuck U. Farley**
   September 20, 2008

   It's quite interesting how the Feds didn't seem to have a trillion to rebuild New Orleans but now the water is rising in Wall Street the money magically appeared over a weekend.

9. **Belizean**
   September 20, 2008

   You're right, this is a complicated issue. But it hardly makes sense to expect effective reform from the candidate who is the #2 Congressional recipient of Fannie Mae and Freddie Mac campaign contributions over the last 10 years (Democratic Sen. Chris Dodd is #1). Senator Obama achieved this dubious distinction despite only joining the Senate in 2005, well under 4 years ago. Also note that the primarily responsibility for oversight of banking operations belongs to the Congress, not the executive branch (Barney Frank, another Democrat, chairs the House Banking Committee).

   As usual, we don't get to choose between the Good and the Bad, but between the Bad the Worse.

10. **Emailc**

September 20, 2008

The Republican conservative philosophy caused this crisis. It's that simple. They deregulated the investment banks, which then naturally took extremely large risks to maximize short term profit.

Voters may not be very bright, but there's no way they are going to see McCain as being separate from the Republican party that is to blame. And McCain's support of the bail out is tearing the Republican Party apart.

So far, Obama and the Dems have been quick to place blame on McCain and the Republicans, to point out the need to help the middle class, to regulate the investment banks, and to control the size of the golden parachutes going to very bad CEOs. I think this is the correct response.

11. **Hall**
September 20, 2008

> Since more than a couple ex-string theorists were involved

I find this argument rather far fetched. I don't know how many ex string theorists work in finance. But it's pretty obvious that many more particle physicists (phenomenologists, latticists, etc) do. After the SSC collapse there were many more jobs in academia for string theorists than for particle physicists, so the latter flocked to computing and to finance.

12. **bane**
September 20, 2008

Actually, I think Peter is wrong with his remark "These assumptions were chosen because they allowed a lot of money to be made in the short-term". From what I've read, that's ascribing the results to poor motives whereas I think the quants were attempting to do their best to "remove the variance" from variable financial instruments and a fair degree of both analysis and simulation goes into verifying that these aims have been acheived in the simulated case that, eg, mortgage defaults follow a specified probability distribution. From what I understand, these tests are genuinely and stringently applied until some modification of the model passes them. The difficulty is that the tests aren't the right tests.

The problem stems from the fact that these are physicists who by the nature of the subject are very used to considering elements to be independent rather than "colluding" or "cooperating" and just didn't consider possibilities such as that almost all loans packaged in a given financial instrument might be liar loans, that every realtor would co-operate with "house price worth" inflation, that most lenders would get "spooked" at the same time, etc, etc.

So I don't think it was ill-intent so much as picking in researchers who in their previous careers never had any reason to get experience with an important aspect of the financial world. I've said in the past that I can see why finance houses would want the best people, but why it should be the best physicists rather than population biologists, epidemiologists, network researchers, etc, (who have much more experience of systems of colluding and non-independent components) never made sense to me.

13. **Cplus**
September 20, 2008

Your account of the state of play, although not inaccurate, is incomplete. For a bit more than the last 20 years, unfounded debt has been created globally in most economic zones and has been hidden in opaque government and corporate balance sheets worldwide. The housing sector in the US is only one of many manifestations of this phenomenon, although it has been the first to receive public scrutiny.
The global markets have been correcting and will most likely continue to do so until all this fictitious capital has been destroyed. Since it is hidden, probably more than the worthless paper will be incinerated in the process.
Eventually when all this has taken place, transparency will be in everyone's interest, and probably will be the way forward at that time. Until then,every effort is being made to conceal the lack of value and shift the losses to other parties. This is the function of Paulson's piecemeal proposals.
None of this is surprising. NYU's Noriel Roubini has analyzed in broad terms this entire unfolding process, by way of addition to the sound sources you cite.
Some ironies of the situation are striking. Eleven years ago, the Long Term Capital blow-up took place under the direction of a Nobel Prize winner in Economics, and John Meriwether, who subsequently started another fund which is reported in the Wall Street Journal today to be about to blow up for the second time.
This process will not be without consequences for some major US Universities which today receive more income from endowment (ie Hedge) funds than from tuition, and for public Universities as government funding and debt become more difficult to generate.

14. **Kea**
September 20, 2008

It is rather unfortunate that people do not understand this.

People understand perfectly well. The issue is how you manage the debt. Many assume that the only way to do this is throw money at the banks to buy back as much debt as possible. That's bullshit. Every new dollar that goes into the pockets of those overpaid twads is a dollar wasted.

15. **capitalistimperialistpig**
September 20, 2008

Peter,

I'm not sure I understood your final paragraphs. Are you advocating that Dems be persuaded to oppose the bailout or that we trust McCain to fix it?

I do think the bailout deserves tough scrutiny, and I think we need to insure that those who perpetrated this disaster get, at the least, a severe haircut, but I'm not sure that we can afford the alternative – an international financial meltdown.

16. **stevem**
September 20, 2008

It looks like quants, "financial engineering", and the science of "risk management" could finally lose all credibility. It's probably going to be finally seen now as modern alchemy and snake oil. But anyone really could have seen that this rediculously overblown credit and real estate bubble was going to explode. The massive bailout package has a real air of desperation about it. I've got a bad feeling about the whole thing but I guess there is no other choice. I'm reminded of Towering Inferno where Steve McQueen has to blow up the tanks at the top of the building in the hope that they release enough water to put out the raging fire that won't otherwise stop. But bailouts are part of the problem.

Exactly 10 years ago hedge fund LTCM was bailed out but that then seemed to set a trend: financial institutions and hedge funds then realised that they could borrow massive and take on monster risk/leverage and the government would bail them out if it all went totally awry, in order to safeguard the global economy and the normal functioning of the markets; the principle of too big to be allowed to fail and too connected to be allowed to fail. This has proven to be case with recent events although

Lehman was especially unlucky.

But how many hedge funds have imploded this week and how many will continue to implode, especially with the ban of short selling, and what effect this will have? The short selling ban could turn out to be a bad idea too. I'm especially interested to see how James Simons and Renaissance will come out of this. The consistency of their past performance suggests they are probably the only ones who have any real mathematical/statistical grasp of markets and their underlying dynamics. Will their strategies withstand everything?

Finally, it's worrying that neither of the candidates in the election race seem to have any real clue.

17. **nige cook**
September 20, 2008

'It turns out that one of the things the quants had been doing was developing pricing models for complex ways to market the risk associated with these loans. One of the sources of the huge income coming into Manhattan was the fees that this generated.'

It's interesting that ex-string theorists are deemed expert enough in real-world social dynamics to model credit risks. In the 90s I worked for the credit control/debt collection industry, dealing with defaulting mortgages in England near the tail-end of a recession that had begun in the early 90s. As inflation drives house prices ever higher, the average cost of mortgages gets bigger, people struggle more to pay with both partners working long hours, so any problem sends them into default.

Banks can't collect money from people who literally can't pay (back in 1997 many banks – our clients – were happy for us to accept from debtors as little as 5-10% of a mortgage in a lump sum 'final settlement' – writing off the remainder – rather than wasting time and money getting fruitless court orders for token payments that were hardly worth the effort to try to enforce).

As a result, banks increase interest rates to compensate for the money they lose in writing off uncollectable debt, and this increase in interest rates means fewer property buyers can afford mortgages, so the property market slows down. Eventually, people trying to sell property have to cut prices in order to secure a sale, so there is a slump in prices, which devalues the equity people have in their owned homes. Many people who bought property at high prices end up with 'negative equity', a home worth less than the mortgage they have outstanding to pay back.

If a bank repossesses such property (after expensive legal costs to do so), it can't recover most of the money because the property isn't worth as much as was paid for it. Property is only 'worth' what somebody else is prepared to pay for it, which is partly a psychological consideration. If you think there is a risk that the value will drop in the future, you lose confidence in buying. So it's a very complex risk analysis situation with a lot of interdependence between different factors once a recession kicks in. No matter how many credit rating checks a bank does before lending money, people will still default *en masse* if there is a major economic recession that drives prices beyond their wages or which leads to a lot of business closures and unemployment. (One suggestion I have for investors fearful of a recession is to invest in debt collection companies, which do great business and profit greatly during bad recessions. We got big bonuses in 1997!)

18. **woit**
September 20, 2008

CIP,

I'm advocating that the Democrats oppose the bailout. I believe that McCain is fundamentally another W, an irresponsible and incompetent person, and if he gets elected we're in big, big trouble. However, I suspect that he and his advisors are not so stupid that they won't see the opportunity Obama and the Democrats are putting in front of them to portray McCain as the Maverick real American determined to save people from the "elite" intent on taking all their savings and giving it to the New York bankers.

As for Paulson's claims that the financial system will melt down if Goldman Sachs, Morgan Stanley, and the banks are not bailed out, I think one should keep in mind that the guy is someone who made a better part of a billion dollars at Goldman Sachs, and so far what he has done to address the credit crisis has just continued to make things worse. A complete melt-down of some parts of the financial system such as the CDS market might actually be desirable, allowing the wreckage to be cleared and something new to be built. The plan now seems to be to keep the current dysfunctional system limping along, with new lubrication provided by the $700 billion from the taxpayers.

19. **Eric Habegger**
September 20, 2008

My understanding, far from complete, is that the so called quants saw that there were huge numbers of individuals and families who would like to own homes, but hadn't yet been able to do so. The problem in any society is that there always seems to be a fair proportion of people that, for any number of reasons, can't qualify to come up with the down payment or can't qualify for the mortgage payments. But these people would love to own homes.

What the quants did was split mortages for these non-qualifying individuals into two parts. There was a "good" part that had low risk to the lender because they were first in line to get paid if the debt holder defaulted. And the poor part of the loan was sold as sub-prime loans with high risk, but also high interest.

The reasoning went like this: As long as wall street knew the risk of each half of the loan then individuals could take the proper precautions, i.e. have only small proportions of the high risk loans. But, as is usually the case, they packaged it differently and didn't sell it has high risk. Instead they packaged the debt with less risky debt so that everyone got invested in it to pump up their interest rate and nobody realized the loans would never have been made had they not been split into a "good" half and a "bad" half.

Now the chickens have come home to roost and their is an avalanche effect that is spreading into loans that are above sub-prime as homes are put into foreclosure and prices collapse. There is a very real analogy to what has been going on in physics during this housing crisis. Though I wouldn't it put on string theorists exactly, I would say the problems in both come from too much faith in manipulating numbers to get the results you need and too little heed to fundamental, or foundational, research.

20. **gs**
September 20, 2008

If indeed a cascading global crisis was imminent, Obama acted responsibly by not interfering with efforts to avert it. McCain banged on the operating room door and screamed at the surgeons.

At present the election looks to be close. McCain's demagoguery costs him support (votes, donations and/or advocacy) from economic conservatives and libertarians. It's not clear to me that he gains more than he loses.

21. **Arun**
September 20, 2008

Senator Bernie Sanders has a proposal:
http://www.sanders.senate.gov/news/record.cfm?id=303313

In my view, we need to go forward in addressing this financial crisis by insisting on four basic principles:

(1) The people who can best afford to pay and the people who have benefited most from Bush's economic policies are the people who should provide the funds for the bailout. It would be immoral to ask the middle class, the people whose standard of living has declined under Bush, to pay for this bailout while the rich, once again, avoid their responsibilities. Further, if the government is going to save companies from bankruptcy, the taxpayers of this country should be rewarded for assuming the risk by sharing in the gains that result from this government bailout.

Specifically, to pay for the bailout, which is estimated to cost up to $1 trillion, the government should:

a) Impose a five-year, 10 percent surtax on income over $1 million a year for couples and over $500,000 for single taxpayers. That would raise more than $300 billion in revenue;

b) Ensure that assets purchased from banks are realistically discounted so companies are not rewarded for their risky behavior and taxpayers can recover the amount they paid for them; and

c) Require that taxpayers receive equity stakes in the bailed-out companies so that the assumption of risk is rewarded when companies' stock goes up.

(2) There must be a major economic recovery package which puts Americans to work at decent wages. Among many other areas, we can create millions of jobs rebuilding our crumbling infrastructure and moving our country from fossil fuels to energy efficiency and sustainable energy. Further, we must protect working families from the difficult times they are experiencing. We must ensure that every child has health insurance and that every American has access to quality health and dental care, that families can send their children to college, that seniors are not allowed to go without heat in the winter, and that no American goes to bed hungry.

(3) Legislation must be passed which undoes the damage caused by excessive de-regulation. That means reinstalling the regulatory firewalls that were ripped down in 1999. That means re-regulating the energy markets so that we never again see the rampant speculation in oil that helped drive up prices. That means regulating or abolishing various financial instruments that have created the enormous shadow banking system that is at the heart of the collapse of AIG and the financial services meltdown.

(4) We must end the danger posed by companies that are "too big too fail," that is, companies whose failure would cause systemic harm to the U.S. economy. If a company is too big to fail, it is too big to exist. We need to determine which companies fall in this category and then break them up. Right now, for example, the Bank of America, the nation's largest depository institution, has absorbed Countrywide, the nation's largest mortgage lender, and Merrill Lynch, the nation's largest brokerage house. We should not be trying to solve the current financial crisis by creating even larger, more powerful institutions. Their failure could cause even more harm to the entire economy.

22. **Arun**
September 20, 2008

There is an excellent presentation on how this happened here:
The Subprime Primer:
http://docs.google.com/TeamPresent?docid=ddp4zq7n_0cdjsr4fn&skipauth=true&pli=1

23. **John**
September 20, 2008

Peter and some commenters have modestly deprecated their econ credentials, but it's not scholarship that's required here. It's a commonplace that when it comes to debunking spoon benders, spiritualists, and various other fraudsters of that kind, physicists perform poorly in comparison to stage magicians, who can easily spot the deception.

In this case, the 'rescue' itself is the crime, with the politicians stampeded by the antecedent theatrics. For example, twice last week Charlie Rose had on Hank Greenberg, who built AIG and ran it for 30 years or so. He and his people had wanted to set up a bridging loan, far short of the government's $85 billion final solution, but they couldn't get a seat at the table. Charlie Rose kept saying he didn't understand why. Then Friday's hystrionics came along and cleared-up the mystery: Paulson needed a Reichstag fire to stampede the pols into the biggest raid on the U.S. Treasury in history, and AIG was it.

I trust everyone has noticed the part of the proposed plan that makes it non-reviewable by the courts? What does that tell you? I just emailed my senator and congressman, and told them to stop it. I suggest you all do the same.

24. **vespasian**
September 20, 2008

They aren't paying $700bn for nothing; provided the price is right (pennies on the dollar) this could actually make a large profit for Uncle Sam.

However the government should ensure banks are forced to hand over well performing derivatives as well as bad ones, and then regulate to just prevent these things from being created and traded. Contrary to popular opinion, the actual structure of CDOs is very simple to state mathematically – any first year undergraduate in a numerate subject could pick it up. What is genuinely difficult isn't the instrument itself but the risk modelling of it – which is true, though to a slightly lesser degree, even for simple shares and options. This is where the quants failed.

A slow liquidation of CDOs seems far more sensible than a fast panic driven one. Provided the plan commits to a selloff over a period of say, 10 years, I see no real problem with it.

That said, the deliberate lack of oversight being built in is extremely disturbing. This is the part people should be complaining about. It is scandalous that no elected official will be responsible for the management of this programme.

25. **D.**
September 20, 2008

Just to inject a professional perspective here, it's not quite right to say that "flaws coming from certain built-in assumptions" caused risk pricing error. In actual fact, total reliance upon private rating agencies, who had insufficient information to price nearly unregulated non-exchange-traded instruments, was the immediate culprit.

The underlying problem, a combination of irrational monetary policy and total lack of political will to tighten loan & credit regulation over the last 13 years, has been widely understood for a very long time, and many people are doing quite well; just look at the massive pre-collapse short interest on Lehman.

Of course, if you're sitting on a $1m Option ARM for some pasteboard monstrosity you saw on *Flip This House*, better leave the keys on the table now.

26. **Walt**
September 20, 2008

While quants I'm sure deserve some of the blame, the key failure was that the ratings agencies and banks essentially colluded to get around banking regulations. Banks need to hold investment-grade debt (such as AAA rated corporate bonds) as collateral. The ratings agencies obligingly gave certain kinds of mortgage bonds AAA ratings (the quants' inadequate risk analysis probably helped with this step). The bonds had higher returns than ordinary AAA bonds, which allowed the banks to make more money on their collateral.

Now much of the collateral isn't worth much, so the banks all have to raise more money, and some are probably already insolvent. Nobody wants to lend to the banks because nobody knows which banks are going to go under. If the victims of the crisis were just hedge funds, it wouldn't be nearly as big a deal.

27. **Eric Habegger**
September 20, 2008

"While quants I'm sure deserve some of the blame, the key failure was that the ratings agencies and banks essentially colluded to get around banking regulations."

Walt, while I think there's room for blame to go around, it was obviously the quant's who set the thing in motion and who oversold it to their employers as the best thing since sliced bread. What you're saying reminds of the pirate who says to his crew,
"Tie me to the mast before I do somethin 'orid, arrrghhhhh!"

28. **helvio**
September 21, 2008

The best introduction to the credit crisis I've heard until now is the program called "The Great Pool Of Money" presented in the radio show "This American Life" (Chicago Public Radio). It is very clear!

29. **Cplus**
September 21, 2008

It may be of help to review the state of play.
On one hand ,there is a congressional leadership of both parties which is well aware of its culpability in creating these problems over many years, and terrified of facing elections in a few weeks in the midst of crisis, and a financial leadership typified by Paulson, who helped to create the subprime debacle when CEO of Goldman, is now at risk of accountability and financial consequence, and until now has a remarkable almost 100% negative correlation with the unfolding financial realities. He appears to have contempt for the petty corruption prevalent in Washington, in comparison to the larger scale activity to which he was accustomed as an investment bank chief, and intends to bulldoze the Congress into rubberstamping another ill conceived and costly scheme. There is so much fear and weakness that his gamble may be won, but it is far from certain. If not, expect a degeneration into serious finger pointing on all sides.
On the other hand, there are the markets, which have an enormous number of degrees of freedom to come to terms with reality. There are currency markets, energy markets, grain markets, precious metal markets, short term money markets, commercial paper markets, government debt markets, corporate debt markets, municipal debt markets, interest rate swaps, mortgage debt markets, credit default swaps, and other derivatives in addition to all the markets directly related to equities. All of these have played a part in slicing through a good deal of the pretense in a little more than the past year since Paulson assured all that the problems were " well contained ". Of course their timing does not follow anyone's schedule. As Keynes, who made an enormous fortune for his college as its treasurer and knew a bit about it, put it, the markets can remain irrational longer than you can remain solvent. But eventually what has value will be valued, and what does not will not.

30. **Kea**
September 21, 2008

I suggest you all do the same.

But we're not all American.

31. **Tom K**
September 21, 2008

No, the basics is quite simple, while the details are quite complicated.

When the Greenspan era began, he changed how the fiat money system, which is used by just about all countries, worked. (The fiat money system is one which a sovereign state issue money 'out of thin air',without any material wealth backing them.) There are 3 types of money. First, the Federal government prints paper money and mint coins. Second the Federal Reserve creates money in the form of credit (i.e. debt for those who use it), which only commercial banks can use under strict regulatory control. One such key control is: a commercial bank can re-lend same amount of credit it borrowed from the Fed up to 10 times to businesses. Such 10x multiplier permits such credit to make up 95% of all money created. So far so good.

But there is a third form of money – money which the Fed does not create directly – called shadow credit. Shadow credit is created, out of thin air of course, by the shadow banking system – so-called investment banks. You know, Goldman-Sachs, Morgan Stanley, ex-Leyman Brothers, etc. They create shadow money by inventing things called derivatives and trading them. The type of derivative invented is a function of their 'innovation', how the trading work is unregulated, and the resulting financial credit generated is unlimited. It is in fact the 'perfect' money machine – private money that can be created out of thin air, outside of the government, outside of the Fed, outside of regulatory control, and which can be converted to real money (Fed credit or notes) with a click of a mouse.

When the US began to loose competitiveness around the world back 20 years ago, industries were 'forced' to outsource the industrial base in order to maintain a high profit return. While this takes care of the business side, the working people side will create trouble as good jobs disappear.

Greenspan needs to create a situation where the good life can be pumped into the economy without doing the real and smart hard work, with a downsized industrial base. He found the perfect 'free lunch' machine – unlimited money creation by the shadow banking system – to finance unlimited growth. But there is also a side beauty – details of such activities are secret, unregulated, so that very few people knows how the magic works. (Greenspan the magician knows!) So he arranged things so as to give the shadow banks a regulation-free reign – confident that the 'smartest guys in town' will know how to manage things.

Soon, investment banks realized they can do anything they want – invent any financial instruments, trade anything in any manner, brand their quality in any manner (since they control the rating agencies) and sell unlimited 'securitized' bonds to the world. The 'perfect' fake money machines was put into over-drive and trillions upon trillions of fake wealth thus created – to fund wars, security, and unlimited importation of goodies for the good life. The executives and traders pay themselves bonuses so great it make kings and queens blush with envy.

The fake-money machine works great for a decade – as long as the population can be convinced to keep on buying the fake credits (i.e. go into debt) without end. But when just about everybody is up to their eyeballs in debt, the executives at the Double-Word houses need to find ever more fools to keep the machine, thus their bonuses, running. Desperate, they turned to the poor class who has no wealth nor credit. Thus began the sub-prime mortgage con game. Bear Stearns was the one who invented the fake subprime mortgages to sell to fake buyers, collect them back in Wall Street and turn them into fake bonds to sell to unsuspecting pension funds and world banks. And the rest is recent history we all know.

Why did I bother to write this story in this blog. Because I want to point out one thing – Knowing about the real story of America's shadow banking system, whose function has been to create fake money to any amount they wish without transparency and regulation, who did operate in the most reckless of ways that resulted in destroying much of the fake wealth (along with much real wealth of the innocents), do you wish to maintain this shadow bank system?

Hank Paulson, ex-CEO of Goldman-Sachs, obvious does. He is asking the Fed to create (out of thin air of course) a trillion dollar worth of real money to buy up the fake zero-worth wreckage of the investment banks so that they can continue doing what they have done. When an addition $1T is created and added to the existing USD money supply without any corresponding economic activity, every dollar is thereby depreciated. In this way the taxpayer indirectly pays for it.

Now there's a reason why Congress might go along. See, the fake money machine also finances a great deal of politics and pork in DC.

32. **not greenberg**
September 21, 2008

then again

What's the odds that the biggest insurer to fail is also the most political, foreign tied, intelligence tied, company, on the planet?

Read,

Another Greenberg Swindle Scam at CIA AIG ?

http://geo-economics.blogspot.com/2008/09/another-greenberg-swindle-scam-at-cia.html

truth is stranger than fiction

33. **Yatima**
September 21, 2008

Thus, all that money to make Manhattan a shiny bauble and give many people an Ali Baba lifestyle came from the future, in the form of the current raid on the taxpayer's pockets, augmenting the US national debt by possibly >>1 trillion or so. Nicely done.

I shudder to think what will happen to the "wonderful budget" promised for HE physics this year.

34. **Phil Warnell**
September 21, 2008

Hi Peter,

Both the strength and weakness of capitalism is that the buyer determines the value of things by what they are prepared to pay, which of course to a large degree is subjective. The problem here is when this extends to things that are necessities such as housing and food, which are not optional, things can be manipulated in this way. Perhaps it is time that in terms of economic policy there is a distinction made and that greater regulation of prices and supply in such areas should be considered to be both permanent and necessary. This of course is not true capitalism, yet when governments are forced to bail out banks and financial institutes, who are we kidding anyway.

Best,

Phil

35. **Chris Oakley**
September 21, 2008

So – we have the most right-wing government in recent US history, along with their proposed successors, advocating the biggest nationalization in history, with their left-leaning opponents, who go along with this, being chastised by a left-wing mathematics professor for not embracing the laws (and accompanying brutality) of the free market.

Or have I missed something here?

Personally, I do not see how the bail out can be avoided. The banks do not just comprise of Porsche-driving spivs earning multi-million-dollar bonuses; they also have savings of you, me and millions of others. The failure of a major bank would be a catastrophe. It may even turn out that even Lehman was too big to fail … we will see. Governments are well aware of all of this and hence the tight regulation of the financial industry. What I think recent events have demonstrated, though, is that the regulations need to be even tighter, and the "every man for himself" attitude that investment banks have needs to be replaced by something a little more responsible. We could start by looking at remuneration – if a trader is paid a percentage of gross profits as his bonus, but does not participate in losses – the usual practice – then *of course* his inclination will be to take on more risk. What is is the worst that can happen? He gets fired. No problem – he can get hired by a rival where he can do the same thing all over again. In technical terms, the trader owns a free call option on his own profitability. Many of my former trader colleagues, who are now setting up Hedge Funds, are now discovering just how nice it was when someone else (i.e. their employer) would pick up the bill for losses.

36. **Bee**
September 21, 2008

Hi Peter,

An interesting post – if you plan on writing more off-topical threads of that sort, go ahead. I too have a couple of friends who became 'quants'. Given that postdoccing sucks big time I've thought about going that same way, but it seems to be mutually incompatible with my political orientation.

Anyway, the way I see the problem is that the system (I mean the economical combined with the political) wasn't able to learn fast enough and to adapt in order to avoid a major crisis like this. Too many people doing their own planning to increase profit, too few people asking whether that's a sensible behavior in the long term. It's a classical mismatch of micro-interests with a desirable macro-behavior.

If you look at it from the system perspective, the problem is strikingly similar to those of the academic system. Too many people working for their own immediate advantage, dismissing thinking about the long-term consequences, too few people who pay attention to what science is about and under which circumstances it can flourish. It supports the formation of bubbles of nothing that eventually have to burst.

The problem in both cases is that the noticing of the mismatch between personal incentives and the desirable long-term large-scale trend does not feed back into the system – other than through a major breakdown. It's the case in which people working in the system are aware its setup does not make sense, but are unable to change something about it (and unwilling since it won't be of advantage for them either).

Best,

B.

37. **David H. Miller**
September 21, 2008

Peter,

I very seriously considered majoring in economics rather than physics, and, in fact, had an offer to do a post-doc in econ after getting my physics Ph.D. Sadly, I turned it down: I could have been one of those Wall Street gangsters making millions by defrauding the public!

Seriously, since I actually did know a good deal of economics (and since I'm honest), I would have been the naysayer pointing out the lack of reality in the models: my refusal to be a "team player" would no doubt have curtailed my career rather abruptly and I would not have made those millions anyway.

I remember some decades ago reading some stuff by Fischer Black on monetary policy and realizing that, for all his fame (this is the Black of the quants' famous Black-Scholes equation), he had almost no knowledge of real economics. That knowledge would not have endeared me to Wall Street.

Here for example is a quote from Black, courtesy of the wikipedia:
>In the U.S. economy, much of the public debt is in the form of Treasury bills. Each week, some of these bills mature, and new bills are sold. If the Federal Reserve System tries to inject money into the private sector, the private sector will simply turn around and exchange its money for Treasury bills at the next auction. If the Federal Reserve withdraws money, the private sector will allow some of its Treasury bills to mature without replacing them.

That betrays incredible, breathtaking ignorance of the monetary system, obvious to anyone who knows basic economics.

I think your own summary of the current crisis is generally accurate, but rather ignores two long-term issues.

First, there is the matter known in the literature as "moral hazard." In simple terms, bailing out bad behavior today sets a bad example for the future, encouraging people to engage in such behavior in the future, believing that they too will be bailed out.

The bailout of the S&Ls in the eighties and the implicit and widely believed (and true, through long officially denied) government guarantee of Fannie and Freddie are two examples of moral hazard that helped produce the current crisis.

Second, there is the issue of the "flexible" monetary policy followed for nearly a century by the Fed.

The Fed was created because, back in the bad old days of the gold standard, irresponsible behavior by the banks and the investment community led to repeated, sharp monetary crises that damaged the economy in general. The gold standard is inherently inflexible and there was no "lender of last resort" to bail out irresponsible financial actors.

However, exactly because of that lack of a flexible monetary regime, unsound financial practices could not be concealed for very long: crises and the resulting correction tended to be sharp, deep, but usually of fairly brief duration.

The monetary flexibility for which the Fed was created changed all that. By inflating the monetary supply ("adding liquidity to the system"), the Fed could delay the day of reckoning for some years, allowing the bubble to grow much, much bigger. And the Fed could similarly drag out the correction period for a rather long time, leading to periods such as the "stagflation" of the 1970s (the model, I'd guess, for the next ten years).

A side effect of this flexibility has been the huge inflation of the last century: from 1914, when the Fed went into operation, until 2008, the dollar has lost more than 95 percent of its value according to the Bureau of Labor Statistics (http://www.bls.gov/data/inflation_calculator.htm). By contrast, between 1800 and 1900, back in the bad old days before the Fed existed, when the monetary supply was "inelastic," the value of the dollar actually increased (see, e.g., http://www.measuringworth.org/uscpi/); the data for the nineteenth century are incidentally much rougher than for the twentieth.

All of this is old stuff, known since the early twentieth century: Friedrich Hayek won the Nobel Prize in Economics in 1974 for analyzing and explaining this in great detail (see, e.g., his "Prices and Production" and "Monetary Theory and the Trade Ccyle"), building on earlier work by Mises, Wicksell, etc.

Incidentally, I am not making any sort of claim that the Fed is some sort of secret conspiracy by the Illuminati, the Elders of Zion, or whatever. It's basic purpose and function are quite clear and have been publicly announced since it was founded. Unfortunately, carrying out that function, i.e., providing an "elastic' currency, has certain predicable results: intensifying and lengthening financial bubbles is one of those predictable results, a result that we are all observing as we write.

Despite my own lack of confidence in economic predictions, I will make one here: the monetary and financial system as we know it will not be (and for obvious political reasons cannot be) seriously reformed during the lifetime of anyone reading this. Any new regulatory agencies will simply be "captured" in a few years by the regulated industry (this too is a standard result in economics); no one who wishes to preserve his political viability will dare touch the system of "elastic" currency created early in the last century; and the bailouts, no matter how well-intentioned, will simply lay the groundwork for even greater acts of financial irresponsibility in the future.

Dave Miller in Sacramento

38. **Eric**
September 21, 2008

After the 1929 crash, we had 10 yrs of 20% unemployment because the government response was:
(1) raise taxes, particularly on the wealthy,
(2) protectionist,
(3) strong laws to favor unionism, which raised costs and according to academic research, lowered productivity, and also had the effect of preventing wages from falling to clear the labor market, and
(4) eat the rich mentality, prosecutions, etc.

Obama favors every one of those things and so do most of his backers and the interest groups pushing him.

Is it your opinion Peter, that this policy mix will work out better this time around?

39. **Peter Woit**
September 21, 2008

Eric,

You're trying to take a far off-topic discussion farther off-topic.

Please folks, take the standard right/left Democrat/Republican arguments elsewhere. If you have something interesting to contribute about the current financial crisis and the proposed bailout, please do so.

40. **Arun**
September 21, 2008

Chris Oakley finds the bailout to be inevitable.

Not so fast.

Sebastian Mallaby in the WaPo:

Within hours of the Treasury announcement Friday, economists had proposed preferable alternatives. Their core insight is that it is better to boost the banking system by increasing its capital than by reducing its loans. Given a fatter capital cushion, banks would have time to dispose of the bad loans in an orderly fashion. Taxpayers would be spared the experience of wandering into a bad-loan bazaar and being ripped off by every merchant.

Raghuram Rajan and Luigi Zingales of the University of Chicago suggest ways to force the banks to raise capital without tapping the taxpayers. First, the government should tell banks to cancel all dividend payments. Banks don't do that on their own because it would signal weakness; if everyone knows the dividend has been canceled because of a government rule, the signaling issue would be removed. Second, the government should tell all healthy banks to issue new equity. Again, banks resist doing this because they don't want to signal weakness and they don't want to dilute existing shareholders. A government order could cut through these obstacles.

Meanwhile, Charles Calomiris of Columbia University and Douglas Elmendorf of the Brookings Institution have offered versions of another idea. The government should help not by buying banks' bad loans but by buying equity stakes in the banks themselves. Whereas it's horribly complicated to value bad loans, banks have share prices you can look up in seconds, so government could inject capital into banks quickly and at a fair level. The share prices of banks that recovered would rise, compensating taxpayers for losses on their stakes in the banks that eventually went under.

Congress and the administration may not like the sound of these ideas. Taking bad loans off the shoulders of the banks seems like a merciful rescue; ordering banks to raise capital or buying equity stakes in them sounds like big-government meddling. But we are in the midst of a crisis, and it shouldn't matter how things sound. The Treasury plan outlined on Friday involves vast risks to taxpayers, huge complexity and no guarantee of success. There are better ways forward.

http://www.washingtonpost.com/wp-dyn/content/article/2008/09/20/AR2008092001059.html?hpid=opinionsbox1

41. **Thingumbob esq**
September 21, 2008

Bravo, this shows your sense extends well beyond debunking pseudo science. However, as far as your claim that you really know little about the situation, that is false modesty, I think. It is high time that the American people do a little reading about our nation's founding principles in regard to credit, etc. I urge you to read Alexander Hamilton's report on the Subject of Manufactures, ASAP.

42. **Boo Radley**
September 21, 2008

Dear Peter,

As before, I will not identify myself except for the following, which might give away my identity to those who know me.

I dabbled for sometime in string theory and am seriously dissatisfied with the subject. I have found some holes in some claims by certain string theorists which I have been prevented from publishing, and I do not have a future in academia. Consequently, I support myself and my family by taking up a job in finance.

From whatever little I know of these quant models, I would say they are seriously outdated. They need improvement, and perhaps the correction requires deep study. More than this I refuse to say.

There is a redeeming feature about the world of finance though. It is always possible that some guy like Robert Merton wins the Nobel one year, and within two years his hedge fund goes bust. However smart you think you are, you just can't beat the market.

In the world of high energy physics that does not seem to be the case. I don't see any Nobel laureate in physics biting the dust as easily, and that creates the illusion of infallibility. And any young unknown who comes along has a hard time. But all we do in physics really is glorified curve-fitting, you know. Nothing all that great.

In economics, people know very well that the prediction itself can affect the system's behavior. In that way, these people are more realistic than physicists.

Don't blame it on the quant geeks. It is blind faith in any mathematical paradigm that leads people into error. In the world of finance, there are plenty of skeptics, so it will correct itself faster. There are not enough skeptics in the world of physics, but the string bubble is also going to burst pretty soon, if it already has not.

There are some simple arguments that can kill off string theory -or at least suggest modifications to its present form – but you seem to be averse to physics discussion in the comments section here (you deleted some of my posts in the last fortnight), and seem intent to criticize string theory and also opine about quantitative finance without any real understanding of either subject. Is that fair?

Anyway, to borrow a few words from Robert Frost,
"Why hurry to tell Belshazzar
What soon enough he'll know."

Why don't you forget string theory and find something better to spend your time on? I would not say it is not even wrong. It is not the right model, and nothing will come of it. Trust an ex-"insider".

Robert Jungk once compared the lot of a physicist with that of Hamlet, so perhaps rephrasing a few lines from there would help.

There are more things on heaven and earth
Than there is in Green, Schwarz and Witten,
Or Polchinski, for that matter, or even in this blog.

Boo

43. **Andre**
September 21, 2008

As one of those who did not benefit from the various bubbles, all I want to feel is schadenfreude, baby!

But the problem is that my taxes will now go to the rescue of those who did so irresponsibly benefit from them, no matter who wins the election. I rent a crappy apartment in a crappy city, yet my dollars will now help pay for the emergency penthousing of the golden people you describe filling the sidewalks of Manhattan. People like me are left behind whoever is in power. Is it any wonder then that I will not vote for either candidate?

44. **Tom K**
September 21, 2008

Bee:
"Anyway, the way I see the problem is that the system (I mean the economical combined with the political) wasn't able to learn fast enough and to adapt in order to avoid a major crisis like this."

Incorrect. Read my post. The shadow banking system is a deliberate business creation that has operated many years. The repeal of legal constraints on their operations were deliberate moves by Congress, Fed and Treasury. They wanted to free up the investment banks to create unlimited amount of money outside of the regular commercial banking system. The investment banks are completely independent, outside of the Federal Reserve system. They don't take deposits and have no insurance. They make their 'wealth' purely out of thin air, by creating financial derivatives, mostly of such complexity (thanks to the quants) that even upper execs do not fully understand the consequences. But for a decade they brought in obscene profits and that was good enough.

Chris Oakley:
"The banks do not just comprise of Porsche-driving spivs earning multi-million-dollar bonuses; they also have savings of you, me and millions of others."

Incorrect. What's being bailed out is not, *not* the commercial banks like Citi of BoA. These banks accept your deposits in saving and checking accounts, are under strict regulations, protected by the FDIC insurance, and are pretty safe. (But many smaller commercial banks are in trouble due to downturn of the housing market.) What's being bailed out is the investment banks (in truth, they are not even banks, just trader/broker). The investment banks accept no deposits from consumers. They make their money by trading other people stocks, finance business takeover, sell bonds. All these are fine. But what blew up is their derivative activities. Derivative as highly complex, high risk financial bets and insurance. They have little capital behind it (frequently, $1 of capital is used to fund up to $40 of betting). The derivatives have turned the investment banks into world's biggest gambling houses, with a total of around $100T, that's trillions, bets have been wagered on the table, but backed up only by a few hundred billions of real capital. It is easy to see how the slightest wrong moves can destroy the house. That's exactly what have happened.

Since this shadow banks are not insured by the FDIC, they have to be rescued by special acts of the government or go bankrupt.

45. **Quant**
September 21, 2008

I can say after reading these posts that most of you should stick to your day jobs. The discipline admits armchair quants just as easily as theoretical physics does. That is to say, it doesn't. So, as one former physics/math type turned quant to (I'd assume) mostly physicists, I'd caution on dimensional reductions that that boil this down to one aspect. Like many areas of specialization this takes years to learn, is rife with pitfalls, conundrums, and counter-intuitive solutions. While it has been 8 years since I studied physics, I don't pretend to be able to make sound arguments on the current state, despite the fact that I still follow it, and perhaps could still work out some problems in qft. That being said, while many can't tell you exactly what it is, I can tell you what it is not, and that it is not just because of models and opaque mathematical finance. Certainly the fall of Glass-Steagall led to tremendous securitization which facilitated unprecedented lending for reasons that were purely economic/accounting. The lending is clearly suspect, if not outright unethical or, even, criminal. These off-balance sheet transactions were bundled up and sold as MBS or in CDO's. Yes, this required modeling. There were no market observed prices for these CDOs and required complex modeling. Further, insurance wraps and arcane structuring did make these seem very safe. The massive amount of these "level 3" type assets, and their subsequent mark downs in illiquid environments whereby they could not be traded, did cause much of what we see. That being said, this is a very small part of the market, and a very small part of the theatre that "quants" work in. There are many people that get up and go to work each day and do it because they enjoy problem solving. They provide liquidity and risk management on a global scale with millions of transactions every day for decades without issue. They are not super wealthy. They are not usually poor, but they are not the 1:10,000 that the news likes to sensationalize. Quants are a small part of a big system, they often still have ties to academia, and many still publish (people who report to me still work in areas of mathematics and physics doing active research).

Before making broad brush-stroke generalizations, do your homework. I assure you it takes longer than a day, though, as this one former-physicist-turned-credit-turned-equity-derivatives-quant sees it.

46. **Arun**
September 21, 2008

That being said, this is a very small part of the market, and a very small part of the theatre that "quants" work in.

Then why cannot the market (say via a tax) bailout the "very small part of the market" that is in trouble? Why are taxpayers being asked to fork out?

47. **Quant**
September 21, 2008

Arun: I assume you were not being rhetorical? There was significant leverage, it was tied to the financial health of the larger institution, and you'll hear this word "contagion" thrown around. There is a great graph that Jim Reid at Deutsche has been showing in his research. It plots GDP, profits of non-financials, and financials. The first two basically track one another. Financials deviate in about 2000-2001 and just spread away. So, either they were printing money, or they were leveraged selling financial claims. I think the leverage works out in average to be about $1 to $2.50 according to a study I recently read (UBS, George Magnus, Financial stability) as it pertained to mortgages. There was just a ton of subprime origination and too much CDS written on it to get you AAA super senior structures, which stayed on book, and was marked down as it corrected (read: delevered=no liquidity) to put it in a nutshell. Go to bis.org and look for total outstanding notional of all derivative contracts. The total notional amount of the OTC market is probably around $650tr now.

48. **Kea**
September 21, 2008

Dear Quant. I did actually work as a quant in the fixed interest market for 18 months when I was younger, and I hated every second. It was perfectly clear to me that the whole business stank. Time to change.

49. **Arun**
September 21, 2008

Quant, while you're on – is such massive leverage necessary to the market? That is, suppose by law, the institution is limited in its leverage, say 12-1 as a strawman. Would trading in derivative securities then be viable? Attractive?

50. **Quant**
September 21, 2008

Arun, there are limits in many markets. Where things get a bit opaque are in OTC (over-the-counter) markets where there are no exchanges or clearing houses. With this would come limits, collateral, price discovery, etc. In fact, this in one of the solutions being proposed in credit derivatives (it has been for years). I should mention that, in a way, derivatives are leveraged from the perspective of having non-linear payoffs. So, not to be overly semantic, but by leverage I mean leverage of illiquid-to-liquid assets under the constraint of poor marks on the illiquid stuff leaving the firm capital starved in a correction (like we see now). Also, for instance in the case of AIG, why were they selling credit default swaps? This was indicative of people operating outside of their forte.

One odd thing that will play out this week in equity markets will be for option traders. Given that one can't take short positions, it is therefore fuzzy in a Black-Scholes theory context in that the price of an option is contingent on continuous trading in the underlying. Many people aren't even going to trade this week because of the dislocation that is ensuing from these bizarre trading rules. What is the price of an option? What is the risk neutral density?

There are many very smart and ethical people in this field, and nobody wanted this. Like any profession, physics included, there are pockets, and cliques and areas of research go in and out of vogue. I know people that hate their jobs getting beat on my traders to build trading models, I know people that are in energy that trade weather derivatives and model rocky mountain snow packs on super computers in lofts in Manhattan, I know people that were doing computational E&M and now doe the exact same thing (same pde's), but just in finance. I know number theorists who went from working on L-functions at IAS to doing the exact credit structures you are reading about. Like anything, you have good eggs and bad eggs.

51. **Chris Oakley**
September 21, 2008

Tom K,

1. Derivatives profits do not appear out of "thin air". As Gordon Gecko rightly says, it is a zero-sum game. If you make money, then your counterparty loses and vice versa.

2. Citi and BoA both have trading desks. Successful traders here will earn multi-million bonuses. I have this on good information as I worked on the interest-rate swaps desk at Citi (for all of two weeks).

3. Any institution that takes investor money is subject to regulation. That includes investment banks, but not necessarily private funds.

Quant,

Yes – in other words quants helped provide smoke and mirrors to conceal bad loans on banks' books. Credit default swaps (CDSs) helped spread the misery.

52. **Cplus**
September 21, 2008

Chris, under existing accounting rules in US and international economies, vast sums of illiquid OTC derivatives and other transactions are marked independently and creatively by the owners without recourse to market price or review by auditors or regulators to assure that counterparties to the same transaction do indeed net to zero, even in those rare instances when the counterparties are audited by the same firm or regulator.
In other words, on the balance sheets of many countries and corporations today, capital does indeed appear out of thin air.

53. **saiko**
September 21, 2008

As an ignorant physicist I understand nothing about financial instruments. However, I find even the orders of magnitude difficult to digest.

According to newspapers, the amount of money the government is going to pour into the financial sector is of the order of \$1×10^12. I guess the amount of "bad" or "toxic" debt must be larger, say \$1.5×10^12 to be conservative. Now, according to the CIA factbook, the US GDP is about \$15×10^12.

How is it even conceivable that the government did not notice a long time ago that the bottom line was wrong by at least 10% of the GDP? That's just like going into an elevator and not seeing the elephant in there. How can the Treasury and the Federal Reserve claim not to be complicit? How could the government argue that they did not see this coming long ago and didn't do anything to stop it because they actually wanted it to happen?

54. **Eric**
September 21, 2008

Saiko,

The losses are substantial, but they are not just blowing a trillion \$. It is very possible the government will make a profit on its various investments, including the various Wall Street Houses it now owns big chunks of. The RTC made a profit. There is a liquidity crisis, money is freezing up, people are being forced to dump assets, but it is far from clear what the scale of actual losses will/would be when/if all is said and done and the markets are unwound in an orderly fashion, assuming that happens.

Soryn, according to reports there were \$500 Billion in sell orders against Money Market funds prior to opening on Thursday. That constitutes a real run on the system. Action was necessary. There really wasn't time for the President to vote "Present", and we still haven't heard Obama's opinion, even on the actions from last weekend.

However, what we can definitely not afford is, going forward, to repeat the mistakes of the Hoover/FDR admins and (a) raise taxes, especially on the rich, (b) promote protectionism, and (c) promote unionism by acts like ending right to work, card check, etc. all of which are likely under an Obama administration.

55. **saiko**
September 21, 2008

> It is very possible the government will make a profit on its various investments,

I hope you're right, and for very practical reasons, not just "humanitarian" ones.

Yet, this all looks like a huge transfer of federal money to private corporations, for free. Much like the Iraq war has been.

56. **Dave Miller**

September 21, 2008

Quant,

I don't think it is reasonable for you to assume that most people here place all the blame on you quants. Since Peter's blog is largely about physics, and many of the quants came out of physics, it was reasonable for him to discuss that issue, but surely his initial post was not simply a matter of blaming the quants.

If you read my post above, you'll notice that I put the largest blame on the "moral hazard" created by past government policies and on the "elastic" monetary policies followed by the Fed, in accord with its original mandate.

Of course, if all the people in the financial industry had been perfect angels – both in intelligence and in personal responsibility – all that might not have mattered. But I take it as a given that human stupidity, greed, and dishonesty will always be with us. Government policy served as an enabler and amplifier of that dishonesty, stupidity, and greed.

You also neglect the point I made about Fischer Black: the quote I provided revealed a man whose knowledge of economics was not simply overly abstract but rather stunningly, breathtakingly wrong, rather like those who think that the secrets to physics all lie in the I Ching.

I know that Black-Scholes is not disastrously and mind-bogglingly wrong in the sense that Black's views on monetary policy were. However, the drastic over-simplifications and unrealities in Black-Scholes should be obvious enough to anyone who seriously studied economics Am I mistaken in thinking that Black-Scholes is held in rather high regard among quants?

I think that a profession, such as the quants, that is willing to take such an attitude, and that is willing to honor an obvious crack-pot like Fischer Black, does indeed deserve more than a little public ridicule.

But, yes, it is not just the fault of you quants: there is plenty of blame to go around.

Incidentally, your implication that those of us who have not worked professionally as quants, even if we have studied a great deal of economics, are not entitled to pass judgment on you guys is eerily evocative of the superstring theorists saying that no one can criticize superstring theory unless he has made significant contributions to the theory himself. It's a very old rhetorical trick: theologians and psychoanalysts have been employing it for a long time, and I'm sure it goes back to phrenologists, astrologers, and the first guy to declare himself the tribal shaman.

Dave Miller in Sacramento

57. **Chris Hillman**
September 21, 2008

Imagine you're in an elevator car. Suddenly the cable snaps and you experience the sensation of weightlessness. Since this sensation is exactly the same as you would experience in an exilarating roller-coaster ride, your natural reaction should be to merrily scream "whheeeee!!"

Not very funny perhaps, but there's a point to my sardonic jest. In recent decades— particularly the last two decades— Wall Street has managed to sell the entire world on one of the biggest lies ever told, the notion that the -appearance- of wealth is exactly the same as "genuine" wealth. Now every mathematicians loves an abstraction, but we instantly recognize— or ought to— that this is at best a local isomorphism. An elevator accident might temporarily resemble a fun-fair ride, but these two scenarios end very differently. That's one of the perennial problems with trying to "draw lessons from history"— history is told by the lucky ones. And we here will probably all agree that the problem of predicting the future from the past— particularly the problem of coping with instances of bad luck— is ultimately what this crisis is all about.

So— let the blaming begin! There's plenty to go around. And you guessed it: I intend to join the ranks of those claiming to have foreseen it all along. While not, I trust, exempting myself from my share of responsibility (in my case, I think my failure has been that I haven't been sufficiently forceful or persuasive).

Over at the New York Times, Saul Hensell (echoing Peter Voit) asks: "So where were the quants?"

http://bits.blogs.nytimes.com/2008/09/18/how-wall-streets-quants-lied-to-their-computers/?dbk

And over at The Edge, Nassim Nicholas Taleb has a timely response:

http://www.edge.org/3rd_culture/taleb08/taleb08_index.html

While I take issue with parts of his analysis, I happen to think he's spot on when he argues that the bankers and other financial managers should never have listened to the quants in the first place, because not only was their modeling suspect, the flaws were easily apparent for decades. As Taleb puts it: "the pilot did not have the qualifications to fly the plane and was using the wrong navigation tools". That reminds me of an anecdote.

As it happens, some years ago a boom time in quantitative analysis happened to coincide with a bust time in mathematics, and I was approached by several recruiters. The conversations went something like this: "Don't you want to ask what I know about finance? Because the answer is zilch." "Doesn't matter, you just need to understand the models, which are absurdly simple— this is much easier than proving theorems and the pay is much higher." "Something troubles me. We've established that -I- don't know the meaning of money, but tell me something: how do -you- define money? Risk? Probability?" (Long pause.) "You don't really want to work in the financial services sector, do you?" They got that right— criminal enterprise has never held any attraction for me.

I've been urging the mathematical elite for years to recognize the fact that over the past two decades the single biggest failure by the universities, with regard to teaching mathematics to the masses, has been failure to inculcate decision makers in all sectors with the danger of relying upon poor statistical reasoning and poor mathematical modeling. I've pointed to appalling modeling errors enshrined in federal regulation of the vast biomedical industry, warning that "faulty math/stat KILLS, and in large numbers". I've pointed to fabulous misapplications of the fearful securitization functor, and I've cried that "this will destroy the world as we know it, in your lifetime". I've asked why the Millenium Problems fail to include Kolmogorov's Last Problem: "what is probability, that we are mindful of it?" (Most of you know that as a young man, he offered an answer involving measure theory. But he wasn't satisfied with that answer and the rise of information theory apparently convinced him it was wrong; unfortunately he died before he could formulate a better answer.)

Some of you may remember that I have long urged that professional societies such as the AMS should organize a massive emergency effort to raise public awareness of the dangers of making decisions and predicting the future using bad statistics and bad mathematical models. I have suggested that our math/stat departments should require graduate students to spend at least one day per week in seminars with biomedical students, law students, and journalism students. That faculty and students should spent 20% of their time engaging in expository writing, fielding questions from reporters and advising government officials at all levels. And they should d all this gratis, as a generally recognized professional service obligation. I have argued that our scientific societies should make a much greater and much better organized effort to track and debunk junk mathematics, junk sci/stat, junk sci/math journalism, and junk sci/math testimony in the courts. Because only a genuine expert can efficiently spot and discredit potentially dangerous distortions dressed in mathematical garb. (Taleb speaks of the "snake-oil facade of knowledge— even more dangerous because of the mathematics".) For example, graduate students could begin their service not simply by running a recitation section but by patrolling the Wikipedia for malicious sign errors— yes, they exist! And professors could look for deliberate distortions of the State of the Art— yes, those exist too. And for the longer term, leading experts could organize and construct a "closed edit" wikified but refereed encyclopedia addressing, above all, the intelligent citizen. (And -edited- in the strict sense, the sense in which the Brittanica is edited; my brief foray into the utterly mad world of Wikipedia, which I soon came to regret, taught me just how essential a role is played by the organizers of any encyclopedic enterprise.)

Tim Berners Lee recently faintly echoed, I think, something else I have long been urging: debunking websites set up under the aegis of universities, which may enjoy some special if almost entirely tacit legal protections under U.S. law, and which don't mince words regarding quacks and free energy frauds. (It's neither fair nor wise that at present Bob Park http://bobpark.physics.umd.edu/ and a few others serve as solitary individual citizens playing this critical role, out of a compelling sense of duty.) I have also tried to argue that our academic philosophers constitute a valuable debunking resource, and we ought to ask them to turn their attention from comparatively trivial pursuits like debating the philosophy of space and time to such serious matters as founding the philosophy of probability, or poking holes in quantitative analysis and biostatistics.

I still think these are good ideas, and they are still worth doing, but I won't attempt to claim that they can undo the damage which has already been done. And just how bad is that damage? Right now absolutely no-one yet knows (and the reasons why no-one knows are one of the major lessons of this crisis), but last week, when allegedly stronger institutions were compelled to swallow some poison pills (failed institutions), and then gingerly began to examine what they had just acquired at the point of the federal gun, I swear I could hear the screams of incredulous dismay all the way from a small dwarf planet on the outskirts of our solar system.

And as many commentators pointed out, there is a certain illogic in "addressing" a problem created in great part by an incomprehensible tangle of business relations by creating even -more complex- corporate entities.

To my mind, one of the scariest aspects of the current financial crisis is that with news of the huge bailout, Wall Street actually -rallied-. That's scary because it proves that the majority of investors remain utterly unable to grasp the awful truth. I suspect that when the dust settles, when regulators have had a chance to examine the books and follow all the ant trails (and this discovery process will probably take two years or more), it will turn out that up to last week the world thought it was half again as wealthy as it really was— perhaps more. Prominent financial commentators mentioned successive figures of 5%, 10%, then 30% of global net wealth as having vanished last week. But of course, it was only virtual wealth. If there were any justice, disclosing that embarassing fact would be literally valuable to more than a handful of disgraced executives who are running away with a few billions in severance pay. (It's telling, I think, that this is a tiny fraction of the recent estimate of the cost of the bailout, which has surely been lowballed.)

In an interview late last week with Bill Moyers, Kevin Phillips (who predicted the recent disaster in a series of books) argued that the United States has now irretrievably squandered its -genuine- wealth (in the "real economy") and has forever lost its empire (and the good will of the world), and he pointed to the experience of the United Kingdom in the last century as a guide to what the United States can expect in the new century. I don't think it's a bad thing that as the shape of the new order becomes apparent, the U.S. will prove unable to continue to bully the rest of the world, even that it will find itself decisively removed from the front rank of the wealthy nations, but as with the UK we must expect that resentments accruing from past transgressions will linger, harmfully, for another century. As Phillips put it, the next few decades will be very tough for most Americans.

Sadly, it seems to me that it really doesn't matter who is elected President in November, because the next president will most likely prove powerless to mitigate the damage which has already occurred. Even worse, he will be distracted from addressing other long-term problems where some preventive measures might still be accomplished. This is in part because the much touted "imperial presidency" has been merely a puppet of Wall Street for quite some time— the reason why our politicians have lied even more in recent decades than they are naturally inclined to do is that cultivating ever more sweeping and intricate deceptions is regarded as absolutely neccessary to the "well-being" of Wall Street— and in part because under our Constitution, genuine reform would depend upon an ineffective political body which has been utterly corrupted by bad money, the Congress. The best one can hope for now is that your grandchildren will live in a world in which the position of the U.S. is somewhat analogous to the current position of the Netherlands or Portugal: a fairly stable second choice vacation destination which almost everyone forgets was once the master of a huge empire.

And let's not let our collective shock at the events of last week obscure the regulatory mess evident in the biomedical industry. When I examine common practices in biomedical research, I can only conclude that you could not devise a system more dangerous to human health if you tried. To repeat an example described by David Salsburg in his book The Lady Tasting Tea, decades ago some tragically deluded bureaucrat decreed that the statistical analyis in any federally funded medical research must use unbiased estimators— because, you know, bias is bad. But it should be obvious that bias (in the statistical sense) can be -beneficial- when it comes to assessing the risks versus benefits of novel medical treatments in a manner consistent with the Hippocratic Oath. And as most of you probably know, the most -reliable- statistical estimators are usually -biased-. (That's just a fact of mathematical life.) If government officals (and reporters) were better educated in statistics, I doubt that such an insane policy would have become firmly enshrined in federal regulations. And let me add that innoculating innumerate commoners against various methods for lying with statistics is not very dissimilar with teaching the warning signs of bad mathematical modeling (for example in finance). Our task is enormous, but will made easier by recognizing that as educators and expert advisors on technical matters in our fields, we really have only one problem here, not many: combating golden-tongued sellers of snake oil and exposing scientific shibboleths.

I and a handful of others have pointed for some time at an apparently growing problem whose true magnitude has gone largely unexamined, mostly because our professional societies prefer to stick their heads in the sand. (Sound familiar?) I speak of the problem of plagiarism and other serious scientific misconduct. Writers like Tom Friedman never seem to mention this when they celebrate our newly steamrolled world, but interestingly enough, certain geographical areas (China, the Indian subcontinent, and the Middle East) seem to have a particular problem with rampant misconduct and corruption. However, Europe and America are by no means immune. And if there are any mathematicians out there who are sniffing "this could never happen in my house!", I can point to specific counterexamples— but since the malefactors have proven a litigious lot, let me just say here that with a little effort you can easily find ample information about some of these outrageous incidents in the Google cache. Because the universities and professional societies have studiously avoided calls to start collecting statistics, it is difficult to test my hypothesis that these widely publicized incidents represent the tip of the iceberg, but that fact that everyone I ask seems to eventually admit personal acquaintance with a case of misconduct which was covered up, I feel that the situation appears to be rather serious.

A related problem (in my view) is the exploding phenomenon of on-line degree mills. If you think this isn't a serious problem, I suggest you (1) obtain a list of known degree mills— which already far outnumber legitimate institutions of higher education— from one of several U.S. states which offer such a list on-line and (2) search sites like LinkedIn to see who claims such degrees and where they work. Surprise! You will find acute-care nurses at "reputable" hospitals, nuclear reactor operators, miscellaneous government officials, highly placed executives in various industries, and even faculty in some "bricks and mortar" colleges (whose chairs stoutly defend their unconventional departmental hiring practices). And what position is most prominently infected with bad degrees? You guessed it— vice principal. I also noticed that ex-military are overrepresented among the ranks of those getting by on fake degrees. Could this be because they feel that our nation has taken unfair advantage of them in various ways, and they are too frustrated to be willing to contemplate taking a more traditional and honest path back into civilian life than by claiming a fake degree?

For anyone who wants to estimate the magnitude of this problem, one pitfall to avoid is the fact that many degree mills take names very similar to legitimate institutions, or even take the former name of a legitimate institution which has changed its name. And you certainly can't distinguish legitimate institutions from illegitimate ones just by glancing at their websites— many degree mills display pictures of buildings which exist somewhere, but which have no relationship to their "college", whose actual physical manifestation is generally little more than a small front office in a shopping mall, or even a mailbox in the Cocos Islands. Incidently, I think I can descry connections between the operators of many degree mills and the spam kings— certainly they often seem to hire programmers using the same pseudonyms, working from the same locations in the former Soviet Union.

These operations resemble spam in another way: it should be perfectly obvious that such deceptions harm society. These practices should not only be illegal, but should carry a heavy penalty. But in the same breath, I must confess that the degree mill phenomenon is plainly driven in part by the insane cost (to American citizens) of a college education— fixing that ought to be part and parcel of health care reform, because in today's economy, you really haven't got a chance if you lack -either- a college education or a clean bill of health. (Many other countries have fixed both problems quite nicely.) And the problem of plagiarism seems to be part and parcel of a culture of self-deception which is surely not unrelated to the coercion and corruption of essentially all our mainstream media and political institutions. And I have been saying for years that scientific misconduct is not trivial– it can kill. A concensus appears to already be emerging that the recent financial crisis is due in part to mathematical incompetency, so if we all now accept that bad mathematics can cripple the economies of formerly great nations, perhaps we will be willing to recognize that deception in all its forms is potentially dangerous— and as Taleb says, particularly when dressed up in mathematical garb, because only a competent mathematician can efficiently spot mathematical nudity.

And our genuine institutions of higher education are not immune to the culture of self-deception. Can you say "grade inflation"? Professors: hand upon your hearts, when was the last time you uncovered a case of student plagiarism and -reported it-? When I look at rate-my-professor type websites, I see a goodly number of student comments saying something like "harsh but fair". When offered the opportunity, most students seem to recognize that nothing is more unfair than to condone dishonest practices by some students. Most students, I think, wish not to cheat. So why do we place them in a situation where, many say, they feel -compelled- to cheat in order to remain competitive, because "everyone else is doing it", and getting away with it?

I would second something else Taleb says: in my experience, statisticians (whom I suspect many mathematicians tend to regard as intellectually impoverished cousins whom one is embarrassed to invite to dinner) tend to be more articulate and honest than most mathematicians (or economists) in expressing the limitations of their understanding. The statistical societies even make a half-hearted effort to provide statistical consulting to reporters— but in my view, such ventures have been far too disorganized and underfunded, and AFAIK little effort has been made to liase with the medical, journalism, or law schools— or with the math departments.

And speaking of professional responsibility: I can think of more than one profession involved in our current problems which has long needed to adopt a formal code of ethics. One aspect of the current furore which I find most unpalatable took form last week in the strained voices and pinched faces of certain leading financial commentators who desperately pleaded for bailouts for this firm or that. Am I the only one who noticed that most of these arguments were unaccompanied by disclaimers of any personal stake in the topic under discussion? In truth— this "dirty little secret" is apparently in fact rather well-known— it has been true for decades that so many "talking heads" in the economics websites are themselves investors in the companies whose causes they promote that few media outlets bother to even -try- to issue disclaimers. Well, that right there is one very obvious candidate for legislative reform: media figures should not be permitted to editorialize about corporations in which they have any personal financial stake— that's simply common sense. Since the media have devolved into mouthpieces for various megaconglomerates, it is no surprise that they have failed to regulate themselves, so government must impose and enforce regulation.

This would also be a good time for antitrust actions severing media concerns from their corporate overlords. And a good time to sever the universities (do I hear howls of rage?) from those same overlords, by prohibiting university researchers from accepting funding from corporate sources such as biomedical companies or organs of population control such as Google. Such corrosive conflicts of interest have long been a huge problem in engineering and biomedicine, but in the past decade I believe they have become a growing problem even in "pure" mathematics. Faculty with unclean hands are in no position to complain when their students misbehave— maybe that's why so many seem so reluctant to tackle the issue of student misconduct.

Another point I have pressed for years, perhaps more controversial than anything I repeated above, is that humans are obviously obsolete as the smallest recognizable economic/political decision-making agents. Jean Luminet has remarked that "physics is always near the beginning of its history", and think that's valid for science and for Terran civilization too. But in each stage of development "near the beginning", one must rely upon exponentially brighter bulbs to drive progress. Inflating human population in order to access greater and greater portions of the tail of a Gaussian is not a viable solution. For one, that policy inherently promotes enormous social injustice. For another, the Earth has simply run out of room and resources, and we'll all surely starve if we inflate human biomass another decade simply in hope of finding the next generation of hominid leaders one more half deviation out on the limb of the bell curve.

A concensus seems to have emerged in recent days that the root cause of the recent market meltdown was that no human in the world is sufficiently intelligent to understand the financial dealings that various bankers and brokers were supposedly monitoring. I think that's also a large component of the inertia of our political institutions. Sufficiently many politicians— despite owing their living to their corporate masters— may be momentarily in a mood to contemplate turning on the puppeteer, but they are too befuddled by the complexities confronting would-be reformers to have any idea where to start. I suspect we will all have ample cause in coming years to rue the fact that in this crucial election, the closest advisors of both candidates are former lobbyists for the very failed institutions everyone currently loves to hate, instead of national resources like Terry Tao. (Is it a hopeful sign that he just announced that he will be busy for a time?)

Because I think the only option which might prove rational in the long term is revolutionary and thorough reform of all our political, legal, educational, and economic institutions, reform founded upon the principle that we must be governed by the best mathematics, rather than— as become apparent to all in recent weeks— by the worst mathematics, the kind which promotes self-deception. If I profess any article of faith, I suppose it would be this: deception is an ever present danger, which always proves a harmful and destablizing influence, and deception can never be indefinitely self-sustaining. The day of reckoning always comes, and usually at the worst possible time.

Here I must admit that Wall Street is ahead of the curve— all the real decisions, it seems, are taken by stock trading software. The problem is that, as many a Vista user knows, complex software often proves unstable. Perhaps we should add another to the candidates for addition to the canon of Millenium Problems: who (or what) should make decisions (political, scientific, economic, judicial) in our global society, and how?

58. **Dave Miller**
September 21, 2008

Chris Oakley wrote:
>Derivatives profits do not appear out of "thin air". As Gordon Gecko [sic] rightly says, it is a zero-sum game. If you make money, then your counterparty loses and vice versa.

No, Chris. Gordon Gekko is not a reliable source on economics or finance any more than Dr. Who is a reliable source on spacetime physics.

This "zero-sum" fallacy is the same mistake that people make who suppose that, since a mortgage involves a transfer of money from the mortgagor to the mortgagee, it too is a zero-sum game. In fact, both mortgagor and mortgagee gain, assuming both act intelligently: the mortgagee earns interest and the mortgagor gets to have a house he could not otherwise have acquired at that time.

Quant can probably explain this better than I, but used intelligently and responsibly, derivatives need not be a zero-sum game. There is a whole complicated literature on this, but, essentially, everyone can get the same profits with substantially less risk via intelligent use of derivatives.

The problem comes when people take naïve models of the derivative markets, and aggressively push those models to the limit at which they are almost certain to fail.

Then, we have not a "zero-sum" situation but a "negative-sum" situation in which large numbers of people will pay very dearly.

And, as I keep emphasizing in my role as almost-was/might-have-been economist, the real problem is the institutional and incentive structure that encourages and rewards those who behave irresponsibly and that enables the continuation and prolongation of bubbles.

Incidentally, that is why I turned down the post-doc in econ. Quant may well be, as he claims, a perfectly decent and honest fellow: if so, I fully believe his claims that he has not gotten rich out of his profession. I realized that, since I am an honest person, if I had pursued a career in economics or finance, I would not have been one of the high-flying, irresponsible, and therefore wonderfully remunerated stars of the field.

The financial and monetary system, at the root of which is the Federal Reserve, is structured so as to reward not people like Quant and me but rather the sort of people who created this mess.

And the system will continue to function this way no matter what cosmetic changes are made by President McCain or President Obama.

Dave Miller in Sacramento

59. **Tom K**
September 21, 2008

Quant:
You have tried to defend the Quants, that they played only a minor role, that they are honorable people. Don't. The Quants did play a major role, and they did take compensations far exceeding of similar professionals in academic and industrial research. They are asked principally to come up with models to analyze derivative revenue & profitability scenarios, even develop new products, and they did perform the work. Based on these executives made their business decisions. The current derivative blow up would not have reached the depth of crisis if it weren't with the total complicity of the quants. (I was an ex-quant back in the early 90's and know quite a few quants – so try not to say I'm a crackpot. Moved back to academia and worked on string theory. Now you may say I am a bit of a crackpot!)

Chris Oakley:
"Citi and BoA both have trading desks. Successful traders here will earn multi-million bonuses. I have this on good information as I worked on the interest-rate swaps desk at Citi (for all of two weeks)."

Quite right. Citi an BoA have brokerage/trade operations, while many smaller commercial banks don't. But these, such as futures, options, certain swaps are quite well understood and 'benign'. They've been around long time and are not responsible for what blew up. Most are also regulated – lightly. But what the independent shadow banks do is the most risky and toxic kinds – credit-default swaps, auction interest rate security, and of course subprime mortgage securitization with fake credit ratings. All are highly leveraged, some even infinitely leveraged (no capital), all unregulated.

60. **chimpanzee (aka "joe")**
September 21, 2008

Kea Says:

> "It is rather unfortunate that people do not understand this."
>
> People understand perfectly well. The issue is how you manage the debt. Many assume that the only way to do this is throw money at the banks to buy back as much debt as possible. That's bullshit. Every new dollar that goes into the pockets of those overpaid twads is a dollar wasted.

I agree & agree with Peter's statement that a bailout of the irresponsible companies (taking on bad risk investments)

"The best response to a Fool, is to LET THEM HAVE WHAT THEY WANT"
— Darwin Awards, "what goes around comes around"

Essentially, the failure of said lending institutions & buyers are a consequence of Darwinism. Let them fail, they go extinct. Lesson learned, newcomers don't make the mistake, & prosper.

It's just MIND BOGGLING that the Feds come in & bail out the "deadbeats" (I know, they are just interested in a near-term "save" to prevent a collapse of an entire system). "You never give an Idiot what they want..EVER"..& the US Govt just did!! 😮

"You can't fix STUPID!"
— popular colloquial phrase in USA

US Govt is attempting just THAT. Recall the COSMOS episode, where a Landsat image of Washington DC is shown, & Carl Sagan comments "No sign of Intelligent Life".

"Suppose you were a Congressman, suppose you were an Idiot, but I REPEAT MYSELF"
— Mark Twain

I don't see anything changing since the 1800's (the time of the above comment).

All that wealth Peter is talking about is called "Stupid Money". BTW, I saw primtime evening news coverage, where they mentioned Jim Simons & how he made 2 billion off the current crisis. You can make $$ off a market downside, even! I saw a dumbed-down demo on a newscast, of "short-selling", man did that make me sick! You borrow a million shares of a stock, pay that guy (friend) a courtesy fee, wait till the stock tanks (market downside), SELL ALL OF IT (make millions), give it back to your buddy. Whoala..you're a multi-millionaire! All because of a loophole in the system. (someone correct the details, I think I got most of it right)

The paradox Peter is referring to is a consequence of a FLAW in the system. Predators were feeding off it, to "make stupid money",

"Stupid is, as STUPID DOES"

Well, there's that saying "Cheaters never prosper"..sure enough, it came back at them. And, public taxpayers money is used to bail out the con-men. DOES THAT MAKE ANY SENSE??

"Kludge [makes-no-sense bailout] upon Kludge [stupid system to begin with]"
— software hacking

We are witnessing a government version of "institutional hacking". 😮

61. **milkshake**
September 21, 2008

Quant, Dave – what do you think of Nassim N Taleb? For some time he has been writing diatribes like this one:

http://www.edge.org/3rd_culture/taleb08/taleb08_index.html

His point has always been that statistical analysis gets frequently mis-applied by quants – used on economic processes that are extremely hard to model correctly. In his view this has developed into a habit of wishful thinking and arrogance. Taleb is a disgruntled quant himself.

62. **Krugman's Right**
September 21, 2008

I think Dr Woit has the right idea in this case.

I am not convinced that this will stop the inevitable downslide. The reason the models failed was a misbelief that people were rational actors. The specific flaw (not discussed below in the link) was that the models expected that people would cut other expenditures in order to keep their homes.

Unfortunately, cutting other expenditures wouldn't even be a "rational" answer for the individuals faced with losing their homes in this market.

So now its being proposed that when the government comes in to rescue these companies, the companies will behave rationally.

Are they HIGH?

I think the additional requirement is that any company taking advantage of this deal needs to go into receivership (banks included). The company should be broken up and sold as appropriate.

For more about the models see the below link:

http://www.nakedcapitalism.com/2008/06/central-banks-use-flawed-macroeconomic.html

63. **Quant**
September 21, 2008

I don't know if I can add anything productive at this point. This topic is highly polarizing, esp. when it admits political and moral stances and, speaking as a tax payer myself, is certainly upsetting. I am not defending any aspects of anything that led to this, or trying to suggest that anyone here is not entitled to an opinion, but I will offer the following: Summing it up in quick bits, like the press tends to, is a bit like writing in the margin that one has found the answer, but the margin is to small to write it out. Also, as most everyone on this forum knows first hand, the solution always appears simple once you are told it. Additionally, saying any one member (save for executives, perhaps) are to blame is a gross generalization, akin to saying that all physicists are mass murderers because they invented the bomb. There is a quote, which I will mangle, and I can't recall with whom to attribute it to, but it says something like absolute black-and-white stances are for children and psychopaths. So, in sum, it is very easy after the fact to take a moral high ground, generalize, and make judgement calls because it puts it in a nice conceptual framework with which to throw stones. Tens of thousands of people, that have families, are out of work. Many are quants, lawyers, economists, traders, …. The bailout is not so much meant to save the banks or the executives, but the retirements and investments of Americans.

I am sure that everyone on this forum has above average intelligence(s) and has the ability to delve deep into this matter, should they choose, and assign blame ex post. My initial post was only meant to add some healthy and constructive balance from, what I would hope, is an informed perspective. In the end, as painful as this is going to be, it is actually progress, albeit by violent correction, not growth. Many of us for years sensed this needed to happen. But, much like a complex system, it is very hard to disentangle the individual parts from the ensemble when trying to see where the issues are and describe the system.

Milkshake: Nassim is an interesting one. I have met him, gone to dinner with him, and read his books. I am tempted to say he has found a niche (much like, say Kaku) whereby he can publish outside the rigor of peer review. I'd call his work observations rather than developed ideas. His Empirica blew up because he ran short positions waiting for his swan but bled out before any such large payoff. He generally has issues with his peers. I'd say he is a bit like Wolfram is to science (if that makes sense?), but a bit more accusatory towards others.

Regarding Black. I don't know if he is a crackpot. He was clearly intelligent, albeit a flawed man (who isn't?). I have read Mehrling's bio (and discussed it with him and Taleb at the same table, oddly enough). He wasn't very charismatic. Of course he has one more Nobel Memorial than I do, so I can't really comment about his intelligence 😉 Black Scholes is a theoretical construct intended to get one the price of an option. It was actually around for 100 years before Black, but nobody had made certain assumptions that permitted its solution. Black himself, just afterwards, is quoted as asking why anyone uses the silly theory. Of course after the rate rise of the 80's, the advent of rate options, and the emergence of quunts from physics, they loved to talk about how it is reall ythe heat equaton and how the Laplace-Beltrami operator is really the generator for Brownian motion….. my point being that physicists took to the theory because it was stimulating intellectually. Of course finance has no symmetries and conservation laws, unless you try to shoehorn the math in (which has been done). I have met Myron Scholes, and I felt he wasn't too imposing, but I have been present when someone tried to publicly take Merton to task and Merton eviscerated them. Again, simple in retrospect, but at the time, in the details, nothing seemed to amiss.

I am going to go back and lurk in the shadows of this blog. I don't read many blogs, but this is one of my regular physics/math stops for information and ideas. Maybe one day I'll return to physics and post here again!

64. **notaquant**
September 21, 2008

For a nice, clear explanation, see Steve Hsu's blog:

Notional vs net: complexity is our enemy .

65. **Cplus**
September 21, 2008

As of tonight, the Investment Banks are no more. The Federal Reserve has just baptized them as bank holding companies, joining the company of the other saved financial institutions masquerading as banks.

66. **JC**
September 21, 2008

Nassim Taleb would be more readable if his writings were a lot less angry and/or less confrontational sounding. Though I can see easily as to why a lot of pessimists and curmudgeons genuinely enjoy his writings.

Despite the negative tone of Taleb's writings, his "war stories" on Wall St. do have some credibility. This is judging from the time I worked on Wall St. many years ago.

67. **chimpanzee (aka "joe")**
September 21, 2008

Bee said:

> If you look at it from the system perspective, the problem is strikingly similar to those of the academic system. Too many people working for their own

immediate advantage, dismissing thinking about the long-term consequences, too few people who pay attention to what science is about and under which circumstances it can flourish. It supports the formation of bubbles of nothing that eventually have to burst.

My Caltech CS prof friend phrases is as:

"People behaving in their own self-interest"

Acknowledge it as a necessary evil..as abhorrent as that is. Why good papers get rejected, bad papers get accepted, good people don't get hired, bad people get hired, good money gets wasted, bad money gets used, etc.

Here is a particle-physics related poem that seems relevant:

Of the Nature and State of Man, With Respect to the Universe
Alexander Pope

> Who sees with equal eye, as God of all,
> A hero perish or a sparrow fall,
> Atoms or systems into ruin hurl'd,
> And now a bubble burst, and now a world.

I got my 1st exposure to the above in a Hawaii Five-O episode

68. **David H. Miller**
September 21, 2008

Quant wrote:
> Regarding Black. I don't know if he is a crackpot. He was clearly intelligent, albeit a flawed man (who isn't?). I have read Mehrling's bio (and discussed it with him and Taleb at the same table, oddly enough). He wasn't very charismatic. Of course he has one more Nobel Memorial than I do…

Actually, Quant, Black does not have one more Nobel than you. He did not receive the Prize. And, no, from what I have read of his stuff, I doubt that he was intelligent, and I very much doubt that you could make that judgment from a brief interaction with him.

What does concern me is that you are a professional in the field and you cannot see how horribly wrong the quote from Black, which I previously posted, is. Anyone who has passed a first-year macro course should be able to write an essay pointing out one fallacy after another in the Black quote. His description of what happens when the Fed creates new money is simply, factually wrong.

For example, the new money may not just go back to buying Treasury paper – the new money might, for instance, go into buying shady mortgage-backed securities, just to use an example of current high relevancy. And even if it does go to buy Treasury paper, that simply means it is displacing some other buyers that are now free to buy, say, shady mortgage-backed securities. And, the money used to buy Treasury securities does not simply disappear – the Treasury is not known for simply destroying money. Rather, the Treasury spends it, thereby spreading the new money through the economy and increasing stimulative pressures. As the new money sloshes around the economy, it is available to buy, say, shady mortgage-backed security.

Anyone who knows basic macro could write a lengthy essay along these lines pointing out the level of economic illiteracy required to make a statement such as Black's. Unfortunately, this is not just an isolated example: I've seen a great deal of nonsense from him.

I have read enough of Black's stuff to know that he honestly, sincerely lacked the intelligence to grasp any of this.

The real problem is not that there is one crack-pot in your field but that this economic illiterate is so honored by your profession. That indicates a larger, profession-wide problem.

This would be like a physicist taking seriously the claims of the various use-vacuum-energy-to-solve-the-energy-shortage scams. Any physicist who supports those scams proves that he is incompetent or dishonest.

Fortunately, most physicists don't support or admire those scams. I wish the same could be said for people in the finance field who are unable to see what was wrong with the views and claims of Fischer Black.

A field which honors a crackpot like that is a field which was heading for certain disaster. Yes, as I said before, we all know that this mess is not due solely to you quants. But, yes, many of you played a role and it appears to be because of a deep and pervasive problem in your profession: to put it bluntly, you guys have been playing multi-trillion dollar games with the world economy while remaining almost entirely ignorant of elementary principles of economics.

The results are not surprising.

I know it is considered rude and socially inappropriate to point out that a whole profession seems to be lacking in basic competence, but considering the situation the world economy is currently in, perhaps a bit of blunt honesty is more than overdue.

Dave Miller in Sacramento

69. **AngryPhysicist**
September 22, 2008

Hmm…well, there are two outcomes to this current situation we face: 1) we're doomed, 2) we're not. Either way, it doesn't really matter…if we're doomed, there's nothing we can do! We're doomed! On the other hand, if we're not doomed, then it doesn't matter! We're not doomed!

Interestingly, economists know only how to *break* the economy, they don't know a single thing about how it *works*. There's a number of serious methodological problems they have, and a number of serious (tragicomical) mathematical errors they commit…no, mathematical *sins*. (An infinitesimal isn't zero!)

A rather readable dissection of economics is perhaps preferred to me maundering on and on, so please be referred to Steve Keen's "Debunking Economics". It's a fun read.

Good luck surviving economic Armageddon… 😉

70. **Name not relevant**
September 22, 2008

An old-fashioned Indian point of view in [this article]in The Telegraph (Calcutta) today. More recent Indian financial policy is more in line with removing restrictions and reducing national stake in companies built with public money.

71. **Esornep**
September 22, 2008

I don't get it. What makes people so sure that playing the populist card will do McC any good? Everyone with a retirement account will tell him to take his populism and stick it. And if Obama tries to follow him in such a blatantly cynical way, how many votes will that win him?

72. **Bee**
September 22, 2008

Tom K: I don't see how what you explained is in disagreement with what I wrote. You might call me naive but I believe that in the long run reason would have prevailed. The dangers could have been closer investigated, regulations could have been put into place etc. In fact, what's happening right now is that necessary learning process taking place, just that a major crisis was necessary to draw attention to the shortcomings.

Chimpanzee: *Acknowledge it as a necessary evil..as abhorrent as that is*. Oh, I do. The problem is not that people behave according to their self-interests, it's human nature. The problem is if the system is set up such that the self-interests don't match with a desirable long-term trend, as e.g. is the case in anarchy or free capitalizm. We have means to match both – the political system is meant to do that. Just that it typically operates way too slow to keep up with much faster trends caused by a large number of actors driven by self-interest and irrationality.

73. **Krugman's Right**
September 22, 2008

In order to lighten things up a bit, attached is a great (and funny) economics lesson from the stand up economist

http://www.youtube.com/watch?v=VVp8UGjECt4

74. **Amos**
September 22, 2008

Respectfuly, the Quants really didn't have much of a role in the securitization of credit derivatives, which is what the current situation is about.

The Quants have played a large role in the construction of arbitrage strategies, some of which involved credit derivatives, but that market has been quite small for some time.

(As an aside, the idea that repealing Glass-Steagal had anything to do with this is bizarre. The banks that have had trouble (Lehman, Morgan Stanley, etc.) are the ones that were NOT associated with depository institutions, consistent with the rule when Glass-Steagal was in effect.)

The current plea for "regulation, regulation, regulation" is really not helpful, since no-one has a plan for _how_ to regulate this credit market, or _what_ regulations to put into place.

In addition, the new populist push to limit bank _executive_ compensation completely misses the point. It really doesn't matter (or make any sense) what the bank executives are making, when the people who were making the relevant decisions were 25 year old middle managers with multi-million-dollar annual bonuses.

Peter, if you actually are interested in any of this, please feel free to e-mail me. This is what I do for a living.

75. **Michael Bacon**
September 22, 2008

At the risk of over-simplifying this, I think that the basic problem is leverage, and this should be the focus of further regulation. There should be a reduction in amount of leverage allowed. In order to play, there should be a requirement to have more skin in the game. This would work wonders in terms of reducing the number of bad bets. Would it decrease upside potential — yes, by definition it would. However, that's a trade that's worth making in order to provide more stability and certainty in the markets going forward.

76. **Arun**
September 22, 2008

How we got into this mess – this narrative by Devilstower on dailykos is said by kos

"If there was a Pulitzer for blog writing, this piece by Devilstower, explaining how we got into this financial mess, would be a shoo-in winner for 2008.

It's likely the single best piece of writing ever to grace this site.

Update by Susan: So good was the post that it's been picked up by The Nation as lead story on its web site."

http://www.dailykos.com/storyonly/2008/9/21/9322/74248/245/602838

(if this is a disruptive comment, please delete it)

77. **Amos**
September 22, 2008

"At the risk of over-simplifying this, I think that the basic problem is leverage, . . . In order to play, there should be a requirement to have more skin in the game. This would work wonders in terms of reducing the number of bad bets. "

Leverage had nothing to do with this. The banks are not in trouble because they BORROWED money for bad bets. They are in trouble because they LENT money to people who aren't paying it back.

78. **Tony Smith**
September 22, 2008

In a comment above, Arun posted a link to The Subprime Primer.

In light of some things that happened since February 2008, when The Subprime Primer appeared on google,

I added an alternate ending and put it on the web at
tony5m17h.net/SubprimeShanghai.pdf
and
tony5m17h.net/Subprime5hanghai.mov

The first is a 3.4 MB pdf file and the second is a 1.9 MB mov file.

The post-February 2008 events were:

1 – A 23 March 2008 New York Time web article by Nelson D. Schwartz and Julie Creswell said, about Credit Default Swap Derivatives:
"… Today, the outstanding value of the swaps stands at more than $45.5 trillion,
up from $900 billion in 2001. …".

2 – Joseph Coleman, in a 6 June 2008 AP news article about an International Energy Agency (IEA) report, said:
"…The world needs to invest $45 trillion in energy
in coming decades, build some 1,400 nuclear
power plants … in order to halve greenhouse gas emissions by 2050 ….".

3 – A Reuters web article on 17 Sep 2008 said:
"… the world must consider building a financial order no longer dependent on the United States, a leading Chinese state newspaper said …".

Tony Smith

79. **EricD**
September 22, 2008

I am a CDO quant. I was somewhat surprised to see that Peter actually got the economic phenomenology better than a lot of others — low interest rates spurring a classical credit bubble that is now bursting.

However, it is wrong-headed to blame quants or anyone else in the industry for building the technology (CDS, ABS, CDOs) that played a role in distributing this bubbling credit into the wider economy. One might just as well blame the internet for making it easier to shop for mortgages, or blame auto-makers for making it possible to commute to work from more distant neighborhoods. All of this is just technology created to help satisfy a demand for credit.

Why was this demand not brought into line with the actual supply (real savings)? What normally keeps demand in line with supply? A price, set on a free market where buyers and sellers equilibrate. But the price of credit, encoded in interest rates, is not set on a free market. It is set by the arbitrary dictate of a board of bureaucrats (the Fed) with analytical pretensions. When you throw darts to set perhaps the single most important price in the entire economy — a price that should have been the result of millions of savers and borrowers coming together in the market — expect bad things to happen.

There is an additional confusion about what credit-related models actually do. They do not generally attempt to predict the overall credit condition of the economy. All these mortgage securitizations were not based on *projections* made by fancy models. The credit macro-state of the economy is actually reflected in the market prices of various traded indices (e.g. CDX, iTraxx). These market prices are *inputs* to the models. The primary function of the models is to translate these prices into valuations for related but non-standard securities (and to determine how to hedge them with the standard ones). This is a second-order kind of effect. But the problems in the credit world were already present at first order, i.e. already reflected in prices of the traded indices.

One caveat to this is the models used by the credit rating agencies themselves. *These* models are the kind that try to predict the global credit state from historical data. And that is why they are not used by people who actually make money, people who of necessity tend to have fewer pretensions about what they know and what they don't know. Incidentally, the ratings agencies are creatures of government, holding what is in effect a legally enforced oligopoly in the ratings business.

80. **Michael Bacon**
September 22, 2008

"Leverage had nothing to do with this. The banks are not in trouble because they BORROWED money for bad bets. They are in trouble because they LENT money to people who aren't't paying it back."

Amos,

I'm sorry, but they are in trouble because they "borrowed" money to "lend" to people who couldn't pay it back. If they couldn't have borrowed money so easily for this purpose (if they had to use more equity), they would have checked the credit worthiness of the borrower a bit better I think.

This is similar to what happened in Japan when banks and companies could borrower money so (relatively) cheaply that they made far too many bad investments. I lived there for seven years and watched it unfold in painful slow motion.

I recognize that there are other factors as well, but it's very clear that this played a big role.

81. **Amos**
September 22, 2008

"I'm sorry, but they are in trouble because they 'borrowed' money to 'lend' to people who couldn't pay it back. If they couldn't have borrowed money so easily for this purpose (if they had to use more equity), they would have checked the credit worthiness of the borrower a bit better I think."

That's just wrong. This market involved making loans, securitizing the obligation to repay the loan, and then selling those securities.

82. **Amos**
September 22, 2008

"What normally keeps demand in line with supply? A price, set on a free market where buyers and sellers equilibrate. But the price of credit, encoded in interest rates, is not set on a free market. It is set by the arbitrary dictate of a board of bureaucrats (the Fed) with analytical pretensions."

This is, again, wrong. The Fed sets the price of credit for certain borrowings by regulated banks. The price of credit at issue here — the interest rate charged for the mortgages that were packaged into CDOs/CMOs, etc. — was set by the markets.

83. **Michael Bacon**
September 22, 2008

"market involved making loans, securitizing the obligation to repay the loan, and then selling those securities."

Amos,

I really don't know where you think the money comes from in the first palce — I know that once loans are made they are securitized to get them off the books.

"Consider Lehman – or, more specifically, consider its barely controlled growth of the past half decade. Its assets more than doubled in size, to $691-billion (as of the end of '07), while revenues and profits grew even more quickly. It was a trading house and investment bank on steroids. The juice was borrowed money, most of it short term, which allowed Lehman to accumulate $31 in assets for every buck the shareholders had in the enterprise by the end of 2007, up from $23 per dollar of equity in 2003. At Merrill, the story was exactly the same – a doubling of the balance sheet in just five years, leverage piled on top of leverage, ever more assets heaped on a thin reed of equity."

http://www.theglobeandmail.com/servlet/story/LAC.20080916.RDECLOET16/TPStory/Business

84. **Amos**
September 22, 2008

Michael Bacon:

"I really don't know where you think the money comes from in the first palce — I know that once loans are made they are securitized to get them off the books."

Its true that there was (occasionally) short term bridge-loan type borrowing, to cover the time between the purchase of the mortgages and the issuance of the securities. But that isn't relevant to what's going on.

In fact, you've got the whole thing backwards. The reason there were so many CDOs and CMOs being sold (and so many mortgages issued at such low rates) was because there was demand for the securities. (Well, that was part of the reason.) Ultimately, it was the purchasers of the CDOs and CMOs whose demand for those products drove down interest rates. The securitization was THE REASON FOR THE LOANS TO BEGIN WITH–people issued mortgages so they had something to sell to the banks, which bought the mortgages so they could sell the securities–not an afterthought intended to clean up the banks' books.

I read the article you posted a link to, and its an editorial by someone who doesn't know what they're talking about.

85. **EricD**
September 22, 2008

Amos,

How do you think Fannie, Freddie, all the off-balance-sheet SIVs, and other credit funds were financing themselves to propagate this credit bubble? They were borrowing short-term (at low Fed-dictated rates) and lending long-term. They were taking advantage of the interest rate term structure arbitrage that the Fed set in motion by, e.g., artificially depressing short-term rates to absurdly low levels in the early 2000s. These credit players are the mechanism by which Fed meddling leaks into the rest of the term structure and the broader economy.

86. **Michael Bacon**
September 22, 2008

Yeah, I'm sure the guy is a complete idiot who wrote that. And so are the other 50 sources I could cite with the same basic analysis and numbers. At least now you've switched your argument some from leverage didn't exist to well, OK, it existed, but it wasn't very much, and it wasn't really a problem. 🙂

And you're right that the one of the reasons, apart from cheap money, that the mortgages were made is because they knew they could get them off the books quickly through securitization (although they often had to keep the "D" piece or some part of the offering).

Listen, if what you were trying to argue is that leverage didn't initially trigger the meltdown, then I think we might find some area of agreement. There were a number of factors that triggered the current events. However, once the markets began to lose confidence there was no one willing to provide short-term funds to keep the ball rolling.

87. **Amos**
September 22, 2008

EricD & Michael Bacon:

You are correct that there was (some) short term borrowing used to purchase the loans (made by others), which were then securitized. That was SHORT TERM borrowing that was PAID OFF when the securities were issued.

THAT IS NOT LEVERAGED TRADING!

Michael, I understand there are a 100 very confused sources out there who think what's happening here is a replay of Long Term Capital Management and some of the hedge fund collapses of the past two years.

They're all wrong. There was no borrowing spree by the banks. The problem is that the banks now hold on their books enormous assets that have been substantially devalued. The problem is NOT that they borrowed money they can't repay in order to create those assets. They were the LENDERS not the BORROWERS.

Is this really that confusing?

88. **EricD**
September 22, 2008

Amos,

All of these credit vehicles worked by continuously selling short-term paper and using the proceeds to buy mortgages that they tranched up and sold to investors (and partially retained for themselves). Their short-term funding was not one discreet event. It was the fuel for their business, constantly replenished by new paper being sold. Without the Fed's loose monetary policy, and the term structure arb, these vehicles would never have been profitable. Once people finally identified the bubble and short-term funding dried up, these vehicles were toast, and the banks housing them buckled.

89. **Amos**
September 22, 2008

EricD:

"All of these credit vehicles worked by continuously selling short-term paper and using the proceeds to buy mortgages that they tranched up and sold to investors (and partially retained for themselves). Their short-term funding was not one discreet event. It was the fuel for their business, constantly replenished by new paper being sold. Without the Fed's loose monetary policy, and the term structure arb, these vehicles would never have been profitable. Once people finally identified the bubble and short-term funding dried up, these vehicles were toast, and the banks housing them buckled."

Your first point is right, but you are essentially agreeing with me that this is not a leverage crisis and Michael is wrong.

Your second point is that this was triggered when "people identified the bubble" and "short term funding dried up," which is completely wrong. Short-term funding (which you identify as the fed, partially right) didn't dry up, and in fact its much more available now than it was. The Fed has tried to increase the amount of short-term funding by lowering rates, in fact.

What has dried up interbank landing was the decline in the value of the securities on the banks' books, which decline happened because people stopped paying their mortgages.

What caused the "buckling" of the banks was the decline in value of the securities, which was triggered by the decline in mortgage payments and increasing default rate.

The securities are marked-to-market, and as the market for the sale of these securities dissipated they had to be marked-down. The trigger for the last few weeks was that one bank sold a lot of the securities for a very low rate. When other banks learned of the rate, they were required to mark the securities on their own books down to that rate (this is a very rough approximation of what happened, but close enough for our purposes), which meant a large loss in asset value. This, in turn, has made banks reluctant to lend to each other, because for some banks if the assets get marked down much further then the banks' book liabilities will exceed their book assets.

Get it?

90. **EricD**
September 22, 2008

Amos,

I haven't been paying attention to your discussion with Michael. As to my "second point" about short-term funding drying up, it is irrelevant whether the Fed has kept rates low (which it has). Short term funding for these vehicles no longer exists; no one wants to provide it. That is simply a fact. That this stems from increased defaults (and increased expectations of future defaults) is true. Indeed increased expectations of future defaults is what it means for people to have identified the bubble.

I am not sure what the point of that tangent was, however. You originally seemed to deny the critical role of the Fed in causing the mortgage bubble by pushing down short term rates. I had simply wanted to correct that.

91. **Amos**
September 22, 2008

EricD:

"I am not sure what the point of that tangent was, however. You originally seemed to deny the critical role of the Fed in causing the mortgage bubble by pushing down short term rates. I had simply wanted to correct that."

Your argument is that short-term lending (a) was too available because the fed set rates too low and it was the fed banks were borrowing from, (b) is no longer available because no-one is willing to provide, and (c) is important.

The second step of you argument is contradicted by your first step. Fed lending is more available now than it was.

Short-term lending has not dried up leading to a collapse in the securitized credit markets. Rather, the collapse in the securitized credit markets led to a GENERAL decline in the availability of credit on commercial terms.

This has nothing to do with interbank rates being too low, or anything like that. There simply was no short-term lending collapse prior to the decline in the value of the credit assets on banks' balance sheets.

I'm sorry, but the short-term bridge loans that were used to create CDOs and CMOs just have nothing to do with this. What's dried up is the market for the securities themselves.

92. **John Baez**
September 22, 2008

Little People wrote:

> Nationalizing corporative debts… where have I seen that before? This kind of scam used to be orchestrated by US corporations (and, probably, governments) in foreign countries. Now they are doing the same thing to their own people. I never thought I'd live to see this.

In the [Savings & Loan crisis](), the US government spent about 125 billion dollars bailing out banks between 1986 and 1996. So, unless you're quite young or weren't looking, you actually *have* lived to see this sort of thing.

Indeed, that earlier bailout may have helped create a "moral hazard" that's part of the cause of the present one.

93. **Amos**
September 22, 2008

John Baez:

"Indeed, that earlier bailout may have helped create a 'moral hazard' that's part of the cause of the present one."

That's partially true, but the S&L deposits were Federally insured, so there was a justified expectation of a government bailout, which isn't true here.

What the two events have _most_ in common, though, is that they were both caused by collapses in the real estate market, the first one commercial (a consequence of the '86 tax code), the second residential.

The moral of the story is that real estate does not always go up and the real estate market is not a securities market–it is highly inefficient, illiquid, and difficult to hedge.

94. **EricD**
September 22, 2008

Amos,

Fed lending enabled the original short-term financing of the credit vehicles. That doesn't mean continued Fed lending is sufficient to sustain this financing once the cycle has turned. The boom is not sustainable because the lending was accomplished on the back of an inflating money supply rather than an increase in real capital. When the effects of the boom redound through the rest of the economy and capital prices are bid up, housing prices decelerate, suddenly people can't maintain their over-leveraged mortgages because they can no longer tap appreciating home value, defaults increase, ABS spreads gap out, no one wants SIV paper irrespective what the Fed is doing, and… bust.

That's the dynamics. This has nothing to do with bridge loans. It has to do with the effect of a dangerously low fed funds rate dragging down the short end of the yield curve, creating a term structure arb exploited by credit vehicles, and setting in motion an unsustainable credit boom.

95. **Amos**
September 22, 2008

EricD:
"Fed lending enabled the original short-term financing of the credit vehicles. That doesn't mean continued Fed lending is sufficient to sustain this financing once the cycle has turned. The boom is not sustainable because the lending was accomplished on the back of an inflating money supply rather than an increase in real capital."

The problem with this part of your argument is that the Fed's lending played a very small role in the creation of the instruments at issue.

"It has to do with the effect of a dangerously low fed funds rate dragging down the short end of the yield curve, creating a term structure arb exploited by credit vehicles, and setting in motion an unsustainable credit boom."

I'm not sure I get your argument anymore. Are you suggesting that the driver for credit expansion was an arbitrage between short term fed rates and higher longer term rates on the commercial market? If so, I don't get your point, since commercial rates are always higher than the short term fed rate. I don't think that implies the existence of an arbitrage, at least not in the usual sense of the term.

On the other hand, are you suggesting that the fed simply maintained interest rates too low for too long leading to excess expansion? Is this a Keynesian argument or a monetarist one?

Either way its wrong, because the market should have (but did not) accurately assessed the risk of default on the mortgages themselves and on the mortgage backed securities.

The error you're making is that the supply of money isn't really a function of the fed, its a function of the capital markets. The Fed's influence, limited to M1, has never been more than marginal, and the growth of new credit instruments made the fed even less important.

The whole point of what was happening was that banks created new sources of loanable money.

Think about it this way:

In the beginning, there were loans, and they came from banks who loaned depositors' money, and they were regulated by the Fed.

Then there were simple securitized loans, where the mortgage-lending banks sold loans to investment banks, who turned them into bonds that could be sold to qualified investors. This made the qualified investors' capital available for lending and reduced interest rates.

Then came credit derivatives, where the clever bankers figured out how to divvy up bundles of loans so they could get a high credit rating and a larger pool of investors could buy them. That made lots more money available for lending, and reduced interest rates further.

The Fed played little or no role in any of this — the whole thing was about making sources of money OTHER than the Fed available.

96. **Peter Woit**
September 22, 2008

Amos (and others),

Thanks for the informed comments that add a lot to my understanding of how all this came about. If I don't contact you or ask more questions about the details, it's because I suspect I'm already spending more time on this than I should. The question of how this happened is one that I'm sure will be debated extensively in the future.

While I don't understand exactly how we got here, I think I do understand where we are: a financial system full of institutions that are effectively insolvent. The Bush administration is trying to get Congress to agree to deal with this by printing $700 billion and allowing Paulson to give it to pretty much whoever he feels like.

John,

This is rather different than the Savings and Loan story. There, the institutions had failed, the government owned them, and had to decide what to do. The proposal here is to bailout the current owners.

97. **EricD**
September 22, 2008

Amos,

First, the theoretical basis for my position is neither Keynesian nor Monetarist but Austrian (Mises, Hayek). You are apparently unfamiliar with the Austrian theory, so I suggest you wiki it, but briefly it focuses on the inevitable distortions brought about in capital markets (and the inter-temporal structure of production) when the government controls the price of credit instead of letting it be determined freely on the market.

An important aspect of this is that the Fed is substantially in control of the short end of the yield curve. It is irrelevant that a given entity is not borrowing directly from the Fed. The Fed is continuously pumping new money into the economy when it buys treasuries, or soaking money out of the economy when it sells them, in the process of fixing short term rates to its arbitrarily chosen level. Short term corporate rates include a default premium above this level, but are still substantially influenced by it.

There is no arbitrage in the strict sense between the two ends of the yield curve, but over a decade long initial phase of the credit cycle, there is a huge incentive to put on this kind of carry trade.

The essential point of the Austrian theory is that exactly the mechanism that the market would have in correcting a faulty assessment of future defaults, namely the mechanism of market interest rates, is thrown out the window when the central bank assumes the role of credit czar. You can't have a price system and eat it too.

What your account is missing is the fractional reserve nature of our system. The money lent out in mortgages did not come from depositors' funds, it was created out of thin air and transferred off the balance sheets of banks (into SIVs) so that it ultimately did not even impair their reporting situation in regard to capital requirements. The loans were ultimately not backed by real capital and were made possible by the phony price signals (low rates) manufactured by the Fed.

One guy who called a lot of this in detail way before it came to fruition is Grant of Grant's Interest Rate Observer. Reading back issues would be informative.

98. **Boo Radley**
September 23, 2008

Actually, Peter, this question is relevant and one that is worth spending time on. You should not just walk away from it. Just look at the response you have generated, even though not all of us agree with you on all counts. (String theory is boring, indefensible mumbo-jumbo, and of no relevance to the real world, and inelegant, in spite of all claims to the contrary.)

99. **Peter Woit**
September 23, 2008

Boo,

My comment was a purely personal one about not having the time necessary to invest in learning enough about some things to have a sensible opinion about them. This doesn't at all mean that I think they're not worth thinking about.

100. **Chris Oakley**
September 23, 2008

I recommend *Liar's Poker* by Michael Lewis to anyone who is interested in the issues raised here. This book will also tell you what a "Coyote Morning" is. Although now 20 years old the only things that have really changed are

• Traders now are soft-spoken and have degrees, the traders making markets in the simpler instruments having long since been replaced by machines. The downside is that dealing floors do not have the "buzz" they had when I started in the business.

• The credit derivatives market has exploded, a case of the tail wagging the dog as on the back of it a whole lot of poor quality debt has entered the market, leading to the current crisis.

101. **imho**
September 23, 2008

Let me get this straight Amos,

I give my child gun parts. My child assembles a gun. My child loans the gun to his best friend. The best friend proceeds to shoot himself and my child…. but the problem has nothing to do with me giving my child gun parts??????

Now, i'm just a simple minded physicist, so there is a good chance i'm missing some important point, but it really seems like this is the argument you are trying to make.

102. **Boo Radley**
September 23, 2008

@imho:

Then does it follow that Einstein is solely to blame for THE bomb? I doubt it.

A quant can make an assessment of the situation which the person consulting him is free to accept or reject. What is important is, at least he/she has a chance of making a falsifiable statement (which even an astrologer might, but perhaps not a string theorist 😉)

The danger is when there is sycophancy (voluntary or otherwise), greed, hubris and herd behaviour, and when the problems encountered by lower level employees don't reach the top. (See Andy S. Grove's "Only the Paranoid Survive") And also when a large number of people adopt the same strategy, and the profit margins all vanish, almost coherently, almost like a system at a critical point….

103. **Amos**
September 23, 2008

Peter:
Yes, you basically have it. Except that the "insolvency" thing is a lot more complicated. The issue is, no-one really knows what the debt derivatives (the CDOs, the CMOs) are worth because there's no market for them. They may in fact be worth much more than currently shown on the banks' balance sheets–a price that reflects the minimal trading taking place in the current illiquid market. Also, the fact that the banks' balance sheet are so poor means that the value of OTHER credit derivatives involving those banks (such as credit default swaps, etc.) has radically declined based on the perceived greater credit risk of the banks. That was AIG's issue.

That, combined with the idea that its now hard to borrow large sums for major transactions (which used to come from investment banks or bank syndicates), and that the risk of the banks defaulting on existing obligations, is the "spiralling" through the economy that everyone is talking about.

EricD:

I am quite familiar with Hayek and Mises, thank you very much. I did not (and do not) see any reflection of their thinking in your analysis, which still strikes me as naively monetarist.

You say: "What your account is missing is the fractional reserve nature of our system. The money lent out in mortgages did not come from depositors' funds, it was created out of thin air and transferred off the balance sheets of banks (into SIVs) so that it ultimately did not even impair their reporting situation in regard to capital requirements. The loans were ultimately not backed by real capital and were made possible by the phony price signals (low rates) manufactured by the Fed."

I'm sorry, but that is just factually incorrect. The money lent in the mortgages came either from depositors money or short-term loans, but it was lent with the (correct) expectation that the short-term loans could be repaid by selling the mortgages to investment banks. The banks purchased the mortgages, principally out of their own capital, with the (correct) expectation that the mortgages could be combined into debt derivatives and resold to qualified investors.

The "real capital" behind the mortgages was thus a real, and for a long time thriving and quite liquid, credit market in which many, many people (high-net worth qualified investors, institutional investors, hedge funds, governmental investment funds, etc. etc.) paid real money ("real capital") to purchase loans.

The relationship between rates on that markets and the fed rate has been minimal.

Of course, it all became a bit more complex than this in the last few years as the derivatives themselves became complex (the invention of "Super Senior," banks holding some slice of the derivatives on their own books, etc. etc.), but that's basically it.

104. **John F. McGowan**
September 23, 2008

Some other good comments on the bailout:

Progressive Conditions for a Bailout

By Dean Baker

http://www.cepr.net/index.php/op-eds-&-columns/op-eds-&-columns/progressive-conditions-for-a-bailout/

I do not agree with all of Dean Baker's politics but he predicted and warned about the housing bubble for years and his editorial above has many good points.

105. **Amos**
September 23, 2008

IMHO:

"I give my child gun parts. My child assembles a gun. My child loans the gun to his best friend. The best friend proceeds to shoot himself and my child…. but the problem has nothing to do with me giving my child gun parts??????"

The credit markets aren't gun parts. They aren't inherently destructive. For decades, they've served the incredibly valuable economic function of making credit — the engine of growth in capitalist societies like hours — more readily available not just for consumers buying cars and houses, but also students taking out college loans, and most crucially companies investing in new technologies, new factories, new machines, etc.

"What went wrong?" is a serious question. It does not have an easy or obvious answer, and to be frank, I don't think anyone knows yet. Even people (like, I think, me and my peer group) who understand both the finance and economic aspects of what's taking place don't claim to have a ready explanation of "why?".

106. **mario**
September 23, 2008

I don't know if anyone here has made the point, but… either the banks are saved or bailed out by giving them money, say amount *X*, or they collapse, making an absolutely fantastic mess that costs about, hm, let's say, *MX* for a not so smallish *M*. The moralities of the issue are besides the point at the moment. There will be plenty of time for that later.

Maybe the good folks in the US government just want to save their face by not making it look like they will just fall over without pondering the issue, or maybe they really are about to let the thing detonate for some ideological reason. We will see. It makes me a little nervous, though.

107. **Amos**
September 23, 2008

mario:

"The moralities of the issue are besides the point at the moment."

There's a practicality here, too. Its not as thought our government (or any) has a great record as a commercial investor.

The plan isn't just to have Paulson authorized to buy depressed assets at above-market prices. These assets aren't simple like equities or bonds, they have to be managed by someone. Basically, the United States is about to turn into the world's largest asset manager and investment bank.

108. **imho**
September 23, 2008

> Boo Radley Says:
> Then does it follow that Einstein is solely to blame for THE bomb? I doubt it.

Interesting debate technique… Make up a completely outlandish analogy that's completely unrelated to any of my comments. Then attempt to mock my original comments as if they were somehow represented by this completely outlandish and false analogy… In my little world we call this a straw man.

It's almost as bad this logical masterpiece below:

> Amos Says:
> The credit markets aren't gun parts. They aren't inherently destructive. For decades, they've served the incredibly valuable economic function of making credit — the engine of growth in capitalist societies like hours — more readily available not just for consumers buying cars and houses, but also students taking out college loans, and most crucially companies investing in new technologies, new factories, new machines, etc.

Gun parts aren't really gun parts. Gun parts aren't inherently destructive. For decades, they've served the incredibly valuable hunting function of making dead food — the engine of growth in food eating societies like hours — more readily available not just for consumers eating goats and cows, but also students taking out cooked pork, and most crucially villages investing in new cooking techniques, new jerky manufacturing, etc.

Dude, that entire paragraph is content-less.

> Amos Says:
> "What went wrong?" is a serious question. It does not have an easy or obvious answer, and to be frank, I don't think anyone knows yet. Even people (like, I think, me and my peer group) who understand both the finance and economic aspects of what's taking place don't claim to have a ready explanation of "why?".

I just told you why… You gave you children gun parts. Your child used complicated mathematical techniqes to create a gun, which he then loaned to his best friend. The best friend then proceeded to shot himself and your child.

At this very moment, Paulson and Bernanke are trying to convince congress that removing the gunshot wound from your child will solve the problem…brilliant

Our best bet is to regulate the children.. err.. I mean wall street. This way we get maximum growth (without the gunshot wounds). I've been reading that this bust was coming since the late 90's. These events were completely forseeable. The problem was that unregulated, capitalistic markets, with their quarterly demands for profit, have no room for moderation or prudence.

109. **Peter Woit**
September 23, 2008

imho,

Enough, this has ceased to be an intelligent discussion of anything.

110. **EricD**
September 23, 2008

Amos,

The only real capital backing much of these securitized loans is capital held in money market accounts that invested in the commercial paper of credit vehicles, including Fannie and Freddie, spawned by the Fed's loose monetary policy. But money market accounts are not treated as capital reserves — they are treated as cash and they are now being insured by the government whose only collateral is its ability to print money. Thus notes for this real capital have acquired an independent monetary character; the capital is being double counted.

111. **Amos**
September 23, 2008

EricD:

"The only real capital backing much of these securitized loans is capital held in money market accounts that invested in the commercial paper of credit vehicles, including Fannie and Freddie, spawned by the Fed's loose monetary policy."

I'm sorry, I just don't understand that. The mortgagor issues a mortgage, and then sells it to a bank for cash. The mortgage is backed by the purchased property (assuming it is non-recourse). The bank puts the mortgage with a bunch of others into a special purpose vehicle, and then securitizes the revenue stream, which securities are sold to investors. So I just do not understand what you are trying to claim here.

"But money market accounts are not treated as capital reserves — they are treated as cash and they are now being insured by the government whose only collateral is its ability to print money. Thus notes for this real capital have acquired an independent monetary character; the capital is being double counted."

Again, I really don't get what you are saying. If a bank holds an investment in a money market account, why wouldn't that count as part of its capital reserves? Maybe you're right and it doesn't, but I still don't see how that would imply that reserve capital is being "double counted" or something has acquired an "independent monetary character."

To be frank, at this point I have no idea at all what you are talking about, and I am now convinced that you've been spouting gobbledygook all along. I'm sorry I didn't spot that earlier.

112. **Amos**
September 23, 2008

imho:

I wouldn't reply since Peter's put a kabash down, but you say "Our best bet is to regulate the children.. err.. I mean wall street," which just begs for a response.

It isn't enough to say "let's regulate Wall St." What is it exactly you want to regulate? What regulations should be put in place? In any event, financial companies are subject to pervasive regulation already.

Indeed, the reason CDOs worked is a regulation that limited certain investors to assets rated as "investment grade" by the credit rating agencies. The idea of the CDOs was to package lots of high-risk stuff together in a way that a slice of it became lower-risk and could get an investment-grade rating that allowed it to be sold to a wider range of customers.

I don't mean to suggest that some new regulation is not called for. But the question remains: What do we regulate? What regulation do we put in place?

Personally, my instinct is that we should strategically encourage investment banks to move toward a salary-based rather than bonus-based compensation system. But that's for complex reasons not worth getting into here.

113. **Jack Lothian**
September 23, 2008

My understanding was it was a Ponzi-game which works like this.

A financial intermediary representative notices that two fellows have significant assets, sitting there earning not much revenues. So this intermediary says to both guys let me manage your assets & I will make you rich & I will only change a minuscule transaction fee for each transaction that makes you richer. So he says to the 1st guy, that 2nd guy's assets are much undervalued so you should buy them & you will buy them by selling your current asset at an incredible profit. Of course, he says this to both asset owners & they exchange assets but they evaluate the assets at 10% higher value. They give 5% of the extra value in commissions & feel rich enough to buy extra goods with their extra profits. This goes on until all the value in the assets are spent & the intermediary is rich.

Of course, the game was very sophisticated in this case because the home buyers were given sub-par rate mortgages which meant the buyer paid below market level monthly payments for the 1st few years of the mortgage but the financial lender did not lose because this lost interest was added to the principal. As long as houses continued to gain value at 10% a year the game worked. Every few years the buyer would flip house, pay the commissions, & extract profits from the house without really putting anything into the house. The point that most people failed to get was that the 1st time house prices did not jump up 10% the game collapsed. An indefinite rise of 10% is not sustainable because it would eventually lead to 1 bedroom shacks costing millions of dollars. An indefinite 10% compounding leads to incredible price increases over short periods. Most people failed to recognize this fact. The few na-sayers were ridiculed. All the financial institutions felt they had to get in on the game or be left behind. Their shareholders all demanded the same returns as the front runners.

During the run up, the financial institutions made huge sums of money off the transactions costs so they seemed to be making money from nothing but in reality money was being extracted out of the housing sector by a Ponzi game. This money has to be made up somehow & I am not sure that we have seen the end of this yet. A lot of money has been extracted from the housing sector & I think house prices might tumble even further.

In my opinion, this was an issue of insufficient regulation of a basic human trait – greed. We are influenced by others & it is possible for us to rush into danger. Cogitative-dissidence causes us to lose track of real risk & we need laws that stop us from acting foolishly as a collective group. Most financial institutions have become incredibly leveraged & the so-called profits are being made are a very thin slice of these assets. It makes for huge profits on the way up but huge losses on the way down.

Salary levels for CEOs are a red-herring. As long as huge profits can be made by ignoring risk there will be those who follow this path. Once a few institutions start down this path and make incredible riches others are forced to follow even if it is obviously dangerous. Then, cogitative-dissidence sets in & we convince ourselves these inappropriate actions are justified.

We need a mechanism that allows risk to be evaluated properly or we need to prevent people from taking really risky actions.

114. **Jack Lothian**
September 23, 2008

PS: I think physicists were minor players in this game. The biggest problem with mathematicians/physicists is they see the economic world as a mathematical model. Unfortunately, people do not model so-well. There is strong evidence that collective human behaviour may be both chaotic and an unstable equilibrium.

115. **imho**
September 23, 2008

> It isn't enough to say "let's regulate Wall St." What is it exactly you want to regulate? What regulations should be put in place?…

What the hell, I'll take a stab and can you tell me where I'm wrong.

A long time ago, in a galaxy far far away, access to low interest capital became doctrine. The gods of capitalism demanded that these traders take this practically free money and invest more more more. So the "masters of the universe", running out of things to invest in, decided to invest in dirt, weeds, and poison ivy. But no one valued dirt, weeds, and poison ivy, so the "masters of the universe" decreed that we should mix beautiful artificial roses in with the dirt, weeds, and poison ivy. We will slice and dice our mixture and call these things CDOs. We will combine different CDOs and re-slice and re-dice to make even more CDOs. We will repeat until all CDOs are opaque black boxes filled with, what we believe is, some sort of greenish organic like material with small pieces of artificial rose. Ratings agencies will declare these organic material filled black boxes good. Exuberance will reign, we will flood the market with these black boxes and create a bubble. Time will pass, until one day some one will wake up and realize that these black boxes are filled of small pieces of artificial rose completely surrounded by fertilizer. The bubble will burst.

So how do we prevent this from occuring in the future? We start by turning the opaque black boxes into transparent boxes with detailed labels, and we require that the labels be updated frequently. Or in other words, we regulate risk management. Maybe it's the credit ratings agencies that need to be nationalized??? An independent agency (not on wall street's payrole) needs to analyze all of these securities products and appropriately label the risk. So maybe in the future there will be less exuberance over $hit filled black boxes.

116. **EricD**
September 23, 2008

Amos,

Follow along and you might begin to understand. Keep your eye on what's under the shell.

A loan is made. The capital for that loan comes from, say a bank. The bank sells the loan to a vehicle, which funds itself by selling commercial paper. The capital for the loan is now coming from the buyers of that paper, e.g. a money market fund. The money market fund is itself financed by owners of money market accounts, so the capital backing the original loan is now coming from these account owners.

Do these account owners regard their money market deposits as risky investments? No, they regard the deposits as cash. They write checks against the deposits. They reduce other cash holdings (e.g. conventional bank accounts) to fund the deposits because the deposits seem just as safe but higher yielding.

This is exactly parallel to fractional reserve banking, where deposits are loaned out and held simultaneously by the borrowers (or by the people who sold something to the borrowers) as independent cash accounts. Real capital is double counted by money. And this framework has set in motion the same credit cycle that Mises identified, enabled by the Fed's artificial suppression of short term rates, which made these credit vehicles profitable.

117. **Amos**
September 24, 2008

imho:

"We start by turning the opaque black boxes into transparent boxes with detailed labels, and we require that the labels be updated frequently. . . . we regulate risk management. Maybe it's the credit ratings agencies that need to be nationalized??? An independent agency (not on wall street's payrole) needs to analyze all of these securities products and appropriately label the risk."

You recognize the function the ratings agencies were supposed to perform. Why do you believe that a nationalized credit rating agency could do a better job than the existing ones?

EricD:

You have the structure of the credit derivatives completely wrong as a factual matter.

In any event, your complaint is not with credit derivatives but, apparently, with the idea of banking itself. I really don't see a point in responding to it further.

118. **imho**

September 24, 2008

> Amos said:
> Why do you believe that a nationalized credit rating agency could do a better job than the existing ones?
>
> Hmmmm, why would an agency that reports to congress be better than an agency that gets paid by the same people it should be regulating… Geee, I don't know.

119. **Zeynel**
September 24, 2008

Bee notices that the banking system is "strikingly similar" to the academic system. She puts it this way "If you look at it from the system perspective, the problem is strikingly similar to those of the academic system." Yes. And why? Because both bankers and physicists are professionals. Banking and physics are the same type of hierarchical bureaucracies. Organisms with the same structure behave the same.

Bee correctly identifies the strict hierarchical bureaucracy of both organisms. "Too many people working for their own immediate advantage, dismissing thinking about the long-term consequences, too few people who pay attention to what science is about and under which circumstance it can flourish." Bee notices that like bankers physicists too have just one thing in mind: their career. Physicists like bankers want to move up the hierarchy by abusing the system. All they think is their own personal profit. Personal profit for a banker means more money and for a physicist it means more academic authority.

Bee continues that the careerist behavior of the practitioners and that the only way to move up the hieararchy is by abusing the system for self-gain "supports the formation of bubbles . . . that evenutally have to burst." But the question is why is it that banking bubbles burst periodically but physics bubbles never burst? Does physics have a breaking point? No. The key to understanding this, and I think Bee fails here, is to understand that both the bankers and physicists sell debt. In finance this is how the system works. In order to make money bankers must split the debt into ever finer and riskier debt and move them i.e., sell them. When this pyramid scheme collapses a crash occurs. The financial crash is built into the system. And what looks to outsiders as disaster actually benefits the practitioners and the system. After the crash the debt system is set to zero and it can start all over again. In physics the debt is theories. Physicists must invent ever riskier i.e., ever more absurd, theories that are more and more distant from experiment or verification. Physicists eventually hope that their theories will be verified by the gold standard of experiment. In academic physics this doesn't happen anymore.

But why doesn't physics crash periodically when like in banking the debt pushed by careerist physicists reach new heights of absurd? The reason is that physics differs from banking in this respect. Physics studies physical quantities. These, despite the name "physical," are not physical quantities but arbitrary definitions of units. Anything can be defined as a physical quantity. So physicists define new PQs and continue to float ever riskier theories that never break the system. When the system gives an error that has a possibility of breaking the system physicists define the error as a new legal PQ and start to study its "physical" properties. Here physical means its relation to existing legal PQs. There is no requirement that a new PQ must be experimentally verified.

But when Bee writes that "in both cases … the mismatch between personal incentives and the desirable long-term large-scale trend does not feed back into the system – other than through a major breakdown" she is only partially correct because this is only true for banking. In physics, as I mentioned above, the risk, ie the absurd, is continuously fed back into the system through PQs and this is the reason why physics never crashes.

And Bee's observation that "people working in the system are aware its setup does not make sense but are unable to change something about it (and unwilling since it won't be of advantage for them either) " must be disturbing to physicists. This picture of physicists is not very flattering. I think every physicist needs to stop for a moment and think about this. Because physics should not be like banking. Physics is supposed to be science. The hierarchical system populated by careerist bureaucrats whose sole goal is to sustain the status quo so that they can abuse the system to move up the hierarchy to gain more authority does not look like the scientific environment physics profession projects to the outside world. The content of physics is defined by this rotten hierarchical bureaucracy and the result is theories like the String Theory. I believe this was not an off-topic but a very much on topic post. Thanks.

120. **EricD**
September 24, 2008

Amos,

If you see a specific error in what I've written, I'd be glad to have it pointed out. Up to this point, you have not done so, as a factual matter.

But if you are only now glimpsing the close micro and macro relationship between securitized debt markets and the conventional banking system, I suspect it is a chastening experience.

121. **David Nataf**
September 26, 2008

Peter,

That was a very nice post and thank you for writing it.

I'm wondering, do you see much difference looking back between the students you had who went to Wall Street and those who stayed in Academia? Were the pre-quants more theoretical? Were they as good as the pre-academics at doing science?

122. **Peter Woit**
September 26, 2008

David,

I can't say that I've noticed many differences between people who stayed in academia, and those who shifted over into finance. It's certainly not true that the best ones stayed in academia, less competent (or more theoretical) ones went into finance.

123. **Ned**
September 26, 2008

Dear Mr. Woit,

thanks for even trying to "figure out where all this money was coming from" – that's more than most people ever did …
(and so the present situation could merrily develop)

124. **Chris W.**
September 27, 2008

Two new articles in the New York Times flesh out the intertwined fortunes of A.I.G. and Goldman Sachs, which might have been a major factor in the government's bailout of the former.

Wall Street, R.I.P.: The End of an Era, Even at Goldman

Behind Insurer's Crisis, a Blind Eye to a Web of Risk:

> As the group, led by Treasury Secretary Henry M. Paulson Jr., pondered the collapse of one of America's oldest investment banks, Lehman Brothers, a more dangerous threat emerged: American International Group, the world's largest insurer, was teetering. A.I.G. needed billions of dollars to right itself and had suddenly begged for help.
>
> The only Wall Street chief executive participating in the meeting was Lloyd C. Blankfein of Goldman Sachs, Mr. Paulson's former firm. Mr. Blankfein had particular reason for concern.
>
> Although it was not widely known, Goldman, a Wall Street stalwart that had seemed immune to its rivals' woes, was A.I.G.'s largest trading partner, according to six people close to the insurer who requested anonymity because of confidentiality agreements. A collapse of the insurer threatened to leave a hole of as much as $20 billion in Goldman's side, several of these people said.
>
> Days later, federal officials, who had let Lehman die and initially balked at tossing a lifeline to A.I.G., ended up bailing out the insurer for $85 billion.
>
> Their message was simple: Lehman was expendable. But if A.I.G. unspooled, so could some of the mightiest enterprises in the world.

The pervasive role of credit default swaps is central to the story.

125. **mike**
September 28, 2008

Peter,

I know this is a little late, but I completely agree with your analysis and I think there are more parallels between the banking process and the current string model than most realize. Whether it's $700 billion dollars or 10^500 vacuum states, it's all the same and pure nonsense – shoving large numbers at a problem does not really solve it. An understanding of the problem and what is happening goes much further than the blatant assumption that "we need to stabilize markets with an infusion of capital". This is pure nonsense and oversimplification of what happened after the great depression, which after real analysis, may not even be a valid comparision today with our electronically-managed, multiple fund markets. Unfortunately, in an election season, it's more prudent to be on the side of the public than to analyze the situation and try to correct the disease instead of the symptom. The real tragedy is that this financial fiasco happens right before the election, where critical thinking will get thrown out and knee-jerk reaction will be the substitute.

126. **Chris W.**
October 6, 2008

Paulson picks a former rocket scientist (and Goldman executive) to oversee the bailout.

By the way, it turns out that the final bill does include a provision for use of a stock-injection plan, despite the opposition of the banking industry.

Meltdown at CERN

September 23, 2008
Categories: Uncategorized

Besides the meltdown in the financial markets, perhaps of more concern to physicists is a meltdown at CERN, specifically in the connection between two magnets during powering tests being conducted at 11:17 last Friday the 19th. Here's the daily report from that day on the LHC beam commissioning site. By the next day it had become clear that it would be necessary to warm back up the sector to deal with the problem, leading to this press release. It was immediately clear that doing this would take at least a couple months, and make it impossible to have physics collisions at the LHC this fall. Today, a new press release confirms this:

> The time necessary for the investigation and repairs precludes a restart before CERN's obligatory winter maintenance period, bringing the date for restart of the accelerator complex to early spring 2009. LHC beams will then follow.

The press releases refer only to a "large helium leak" and the failure of an electrical connection, making the problem sound rather minor. I tried contacting a physicist at CERN to find out if they had any more information, but was told that he or she was under instructions not to discuss anything about what had happened beyond what was in the press releases, and that CERN was specifically concerned that information might show up in blogs. This policy seems to be being unevenly enforced, since today the Everything Blog carries the following report:

> I was in a meeting at CERN when someone ran to the front of the room with a computer, then after letting the speaker finish and setting up the computer to project the press release/e-mail (agonizing moments: it was a Mac), they let us know. I was worried that the sector with the helium explosion had collapsed like an old mine — there were rumors going around that this is a weak point in the tunnel. (Of course there would be such rumors.)
>
> I have learned a few more graphic details about the event in the last few days. First off, it was two tonnes of helium, not one. But I've also learned that this was a more explosive and dramatic event than I had imagined— helium is fortunately an inert gas, but the temperature gradient caused it to explode violently, probably causing physical damage to the nearby components. And now that section of the LHC is an ice tunnel, maybe with stalagtites hanging down and a Yeti moaning in the distance.

Up until now, the policy at the LHC has been to be quite open about the commissioning process, with detailed technical information provided on web-sites that were freely accessible. For instance, there's detailed news about how beam commissioning was going up until the accident available here. Going forward, it will be interesting to see how CERN deals with the problem of letting not-so-good news out to the public. Just about one year ago, I wrote about some earlier LHC problems, and it has always worried me a bit that having a huge LHC publicity onslaught before the machine was actually ready and working might not have been a good idea. Going through the often painful process of solving the problems likely to show up in a project of this scale may provide a different education of the public than the one people were hoping for.

Update: Yesterday there was a LHCC meeting at CERN, broken up into an open and closed session. The slides from the talks are available here, and give a wealth of information about the state of the machine and detectors, and what they were able to accomplish during the short period that they had a beam. There's no info about the accident in the slides, but video of the talks is here, and at the end of the talk by Lyn Evans (9-18min into the video) he gave a report on the accident and answered questions. The problem has been traced to an electrical fault in the magnet busbar, and bizarrely occurred during the test of the last circuit of the last sector that was being commissioned for 5 TeV operation.

Evans says that the machine will be down until "early spring". Before the accident, the plan had been to bring the LHC back up in early June, after various work needed on the injection system. The current plan is to try and get this work done instead this fall, so that they can start up more quickly in the spring.

Comments

1. **Eirik**
   September 23, 2008

   Obviously this is the effect of the anthropic principle! In all the universes that the LHC were functioning and crossed it's beams, black holes were created which swallowed the earth. Since we are all alive and see the LHC malfunctioning, this is evidence that miniature black holes can be created at LHC. This is really the first experimental data the LHC have produced. It's amazing how much new good science this anthropic prinicple makes possible!

2. **wb**
   September 23, 2008

   The failure was in an interconnect between two magnets that caused a fire. Ultimately more than 20 of the superconducting magnets quenched. The good news is that the quench protection systems operated properly at a scale never tested before. This gives good confidence about magnet protection. The unknown question is why the interconnect failed – a change badly done connection,a design flaw? What needs to be done is to warm the sector and inspect. Based on what one sees,one may decide to warm everything and inspect the entire machine.

   This not such a simple decision. Remember the rf-fingers that had problems. These fingers have a potential for damage every time the machine is cooled. Therefore one tries to avoid thermal cycles unless necessary

3. **Observer**
   September 23, 2008

   I wonder how cold it got when the helium expanded?

4. **Coin**
   September 23, 2008

   I am expecting serious questions in the press and worldwide demonstrations for accountability over the question of whether it is possible that the operation of the LHC could, in fact, create Yetis.

   *Remember: There is no scientific theory that can definitively prove that the LHC will **not** result in the production of Yetis.*

5. **calvin**
   September 24, 2008

   It is being rumored that the LHC in it's test run has already produced evidence of susy. In fact, it produced the susy partner of a black hole, which instead of swallowing up the world, swallowed up Wall Street. In fact, the susy black hole – also known as sblack shole – a.k.a. Black-Scholes, for convenience of pronunciation, with the 'c' added to confuse people – has been around for a while.

The rest is silence.

6. **csrster**
September 24, 2008

Eirik, that may be the best comment in the history of this blog.

7. **Marco**
September 24, 2008

… it will be interesting to see how CERN deals with the problem of letting not-so-good news out to the public…

Concerning this point, yesterday during a collaboration (ATLAS) meeting we were formally asked not to give to the press anything that is not a CERN official statement (and this is somehow bounding at least for the CERN employees, we sign something about information disclosure with the contract). I guess this says something about the way CERN plan to control the information leak in the next months…

8. **DB**
September 24, 2008

I can understand that they need to inspect the failed parts and precisely determine the cause, and that premature speculation on blogs by Cern employees could be used irresponsibly by the media. But then they need to be completely open and transparent. As soon as is practicable. Just as they were when the faulty Fermilab components failed last year.

I have no reason at this stage to believe they will act differently in this case.

9. **Jack Lothian**
September 24, 2008

The media will do what they always do. Thinking that one can control the media is extremely foolish. As word gets out that CERN is trying to "control" the media's access, they will get more aggressive & hostile. From the media's viewpoint this is a declaration of war & they will now go to greater lengths to root out the real story.

I see this as a serious management mistake. Some people higher up are getting very nervous about their job & prestige and they are panicking. It is a normal human reaction under such conditions to try to control everything. Clamping down like this occurs all too frequently when the shit-hits-the-fan. It usually makes things worse but we do it anyway because it makes us feel safer.

10. **Peter Woit**
September 24, 2008

Marco,

Thanks. The problem with the "nothing to the media except what is in a press release" policy that the LHC collaborations are instituting is that, taken literally, it basically shuts down all blogging by LHC scientists. Blog postings are, unavoidably, communications to the media, since they can read them. If the only thing you can put in your blog are CERN press releases, why bother blogging about your work?

Funny, but this policy either wasn't in place or wasn't enforced until there was some bad news. De facto, the policy seems to be that LHC bloggers can discuss good news as much as they want, but not bad news, Also, the collaborations are mixing one legitimate problem (their conventional concerns about discussion of preliminary data), with a very different one, that of the public perception of how well the LHC project is going.

I think it would be appropriate for CERN to make sure that bloggers working there clearly distinguish between when they are speaking for themselves and when they are speaking officially for the lab, but beyond that I don't think it's a good idea to do anything other than encourage the dissemination of accurate information about how things are going.

11. **Jim Pivarski**
September 24, 2008

No I am not releasing more than what is publicly available! (This is Jim from the Everything Seminar.)

Please see http://cornellmath.wordpress.com/2008/09/24/this-is-not-sensitive-information/

12. **insider trading**
September 25, 2008

I totally agree with Jack Lothian. This is indeed a management mistake, the way I see it. Compare it with a totally open view: everything open from day one after the incident. Where is the trouble ? They show what they are doing. If they do the best that can be done, things are okay. Instead, they really look like they have something to hide.

I stay anonymous but I am a CERN experimentalist. By the way, beam operations will not restart before the start of spring, most probably April. Then there will most probably be a longer-than-planned run at 10 TeV.

13. **Will**
September 25, 2008

Peter, Why are you always so negative? Why?

14. **Peter Woit**
September 25, 2008

Will,

This isn't a really good example to accuse me of negativity. There's no way to deny that this is a discouraging piece of news. Writing a positive posting about it would be kind of idiotic. As for whether I'm negative about attempts by people at CERN to shutdown discussions on blogs by physicists that they might find inconvenient, I guess I'm a pretty negative guy when it comes to that.

On the whole, one goal of this blog is to put out accurate information, especially in cases where there's a lot of hype going on. I guess that can be thought of as a "negative" activity, but I think it has its value.

Personally though, I can assure you that I'm mostly a quite cheerful and optimistic sort, very happy with life. Maybe I compensate for this with the blog….

15. **Sara**
September 26, 2008

@ insider trading: but do they really have anything to hide?

16. **Pessimist**
September 27, 2008

What if it emerges that CERN management cut some corners so they could have "Big Bang Day" on schedule?

Barring that, the whole incident has been poorly handled. If these guys had a clue, they would have called the BBC back to inspect the damage. It's rumored to be quite spectacular, and pictures will undoubtably hit the web. Instead of a media bonanza, with the public engaged in the discovery process, it could become a sleazy scandal. Perhaps Big Science needs a good house cleaning.

17. **amba**
September 27, 2008

Moral: do not announce your pregnancy until you're past the danger period for miscarriages.

18. **chris**
September 29, 2008

well, i would call this a first successful LHC prediction 🙂

http://arxiv.org/abs/0802.2991

19. **Steve Myers**
September 30, 2008

I'm confused: didn't any one test for faults before power up? Was that too difficult to do? I have been involved in many startups of high power & complicated equipment, malls, office buildings, and power was never applied without extensive tests. Phase to phase, phase to ground, bad connections, etc. faults should be caught in initial tests and inspection. Clearly there's something I don't get about this accident.

20. **Peter Woit**
September 30, 2008

Steve,

From what Evans said, this happened when they were ramping up from a current corresponding to 5 TeV to one corresponding to 5.5 TeV. This was late in the game, after extensive testing, some kind of problem that only showed up at high currents.

21. **Pessimist**
October 16, 2008

The official report is out.

https://edms.cern.ch/file/973073/1/Report_on_080919_incident_at_LHC__2_.pdf

Thirty magnets out, 6 tons of He lost, soot in the beam pipe, ++

The good news is " the official inauguration ceremony that will take place [ ] in the presence of the highest representatives from the
CERN Member States, representatives from other communities and authorities of the countries participating in the LHC."

22. **Covariant**
October 17, 2008

From the report

"These forces displaced dipoles in the subsectors affected from their
cold internal supports, and knocked the Short Straight Section cryostats housing the
quadrupoles and vacuum barriers from their external support jacks at positions Q23,
Q27 and Q31, in some locations breaking their anchors in the concrete floor of the
tunnel. "

Wow

23. **Benni**
October 18, 2008

At least, from what is in the report, it seems that one can take appropriate measures that such a chain reaction of events won't happen again.

I wonder if the whole LHC must be warmed up when they make the necessary modifications on the other magnets to prevent such an accident in the future,

Proof of Resolution of Singularities in Characteristic p?

September 23, 2008
Categories: Uncategorized

I've heard reports from Harvard that yesterday and today Heisuke Hironaka has been giving talks in the math department there, claiming to have a proof that singularities of algebraic varieties can be resolved for any dimension in characteristic p. This would be a major advance in the field of algebraic geometry. I don't know any details of the proof, but Hironaka is, at 77, an extremely well-respected mathematician, not known for making claims unless they are very solid.

Hironaka won the Fields Medal in 1970, largely based on his 1964 proof of the resolution of singularities, in the characteristic zero case. For an introduction to that proof, see this [article]() from the Bulletin of the AMS.

Update: From comments here it seems that the source of my information about Hironaka's talk was most likely overly optimistic about exactly what Hironaka was claiming. The current situation seems to be that several groups are working on this, with promising ideas of how to get to a proof, but with no definitive proof yet done. There will be a [workshop at RIMS]() in December, with the goal of sorting out the current situation:

> The aim of the workshop is to review recent advances in the resolution of singularities of algebraic varieties with special emphasis on the positive characteristic case. After many years of slow progress, this is now a rapidly developing area with several promising new approaches. Our aim is to keep the program flexible, in order to give the maximum opportunity to discuss and explore new developments. We expect a joint effort to understand characteristic p, and that the purpose is not that everybody exposes his/her own results.

Comments

1. **Daniel de França MTd2**
   September 23, 2008

   "singularities of algebraic varieties can be resolved for any dimension in characteristic p. "

   Can you explain, with an example, why is that a very important result?

2. **Charles Siegel**
   September 23, 2008

   Well, there are a lot of problems in characteristic zero (where resolution is known) which are solved by taking a resolution of singularities, proving the result (or defining the object, or the like) on the resolution, and then pushing forward to the singular object. The point of the resolution is that it's smooth, but as close as possible to the singular variety. So presumably the reason is that there are some conjectures that hold if resolution does, but we can't prove otherwise. I know that some stuff on alterations by deJong at Columbia has been used to chip away at some of them (positive char stuff isn't my subject, so I'm working from heresay mostly) and stronger statements can be obtained with full resolution.

   Of course, there's also the fact that it's been an open problem for decades, and so a proof would most likely involve powerful new techniques that could be used to solve problems.

3. **Terry Hughes**
   September 23, 2008

   Peter,

   It is a vast overstatement to say that Hironaka is claiming to have a proof that singularities of algebraic varieties can be resolved for any dimension in characteristic p. He outlined a new approach at Harvard, but made no claims that he solved the problem.

   In Argentina Orlando Villamayor of the University of Madrid recently gave a very nice talk in which he claimed he had reduced the problem to some very special cases. He will also speak at the Harvard meeting.

   And there are some others who are claiming progress.

4. **Peter Woit**
   September 23, 2008

   Thanks Terry,

   Were you at Hironaka's talk and do you know exactly what he claimed? The story some people at Columbia were told was that there had been a claimed proof, but our informant from Harvard was not an expert on the subject, and may have missed something about exactly what Hironaka was claiming.

5. **Terry Hughes**
   September 24, 2008

   Peter,

   I didn't attend Hironaka's talk. My information comes directly from a Harvard senior mathematics professor who did attend. I don't know any of the details of Hironaka's new approach.

6. **Peter Woit**
   September 24, 2008

   Terry,

   Thanks. My info also comes from a Harvard senior mathematics professor….

   If any of my readers did attend the talk or know someone who did, I hope they can help us resolve this disagreement amongst the Harvard faculty…

7. **Jabotinsky**

September 24, 2008

Recently people in that subject have started to think that the proof of characteristic p resolution is imminent, so I think people are sort of jumping the gun staking their claims, and their exact wording might affect the impression they give. I suggest waiting to see what happens before making these speculations.

8. **Terry Hughes**
September 25, 2008

Peter –

That's hilarious. I sent my Harvard math department informant an email asking about the inconsistency, but I haven't heard back yet. My informant has made considerable contributions to algebraic geometry, so he very much knows what he's talking about in that field. But he is not a singularity expert as such.

I'll keep you apprised!

9. **Terry Hughes**
September 25, 2008

O, and my informant did attend the Hironaka talk.

10. **postthis**
September 25, 2008

I don't think it makes much difference until there's a paper. About a decade ago one of Hironaka's students gave a lecture series at Harvard on his proof of resolution in positive characteristic. (or approach-to-proof or however it was described). It sounded promising. But the details didn't hold up and the claim was eventually withdrawn.

Hironaka may well have a proof, a usable strategy, or neither. Resolution is a very technical problem and, unlike with Wiles or Perelman's proofs, I don't think a quick provisional determination of correctness would necessarily be easy to reach.

11. **Terry Hughes**
September 25, 2008

Hi Peter,

My Harvard math department informant has responded, and he's pretty certain:

"Actually, I cannot imagine any of my colleagues spreading the word that Hironaka had solved the problem. Movement on it is in the air, but it's not all by Hironaka, nor is the program complete."

My informant has been to the talks and he definitely walks the algebraic geometry walk! I'm quite confident in him, for whatever that is worth.

Survivor

September 26, 2008
Categories: Uncategorized

Steve Hsu has a recent posting Survivor: theoretical physics, which links to data about the theoretical particle physics job market compiled by Erich Poppitz from listings on the Theoretical Particle Physics Rumor Mill web-site.

Back in 2001-2 I also spent some time looking at this data, and estimated that in recent years there had been typically about 15 tenure-track hires each year in particle theory, of which roughly half were going to string theorists. Starting around 2000, the number of hires started to increase, as it has increased throughout academia during a period of reasonably healthy university budgets and an increasing number of retirements (of those hired during the 60s when many universities expanded dramatically). Since 2000 the number of hires has typically been more like 20, anomalously high at 28 in 2007. The anomalously low number of 15 for this past year's hiring may be a fluctuation, but it also may be an indication of either university budget cutbacks or increasing unpopularity of particle physics in US physics departments. The fraction of string theory hires has gone down dramatically, to more like 25% over the last 5 years.

I don't have any data at hand about the recent total number of people getting particle physics Ph.D.s, and whether this number has grown with the number of faculty hires. I did find at one point a number of 78 for particle theory Ph.D.s in 1997, but I don't know if this included degrees in cosmology, which increasingly has become mixed with particle theory. Poppitz also lists numbers of hires by institution. Princeton comes out significantly ahead with 23 people getting jobs over 15 years. I'd guess there are typically about 3-4 people/year getting theory Ph.D.s there. So, traditionally, if you want to maximize your chance at a job, Princeton is the place to go. Not clear how this will work out in the future, given the very small number of string theorists getting hired.

To get some idea of the imbalance between Ph.D.s being produced and tenure-track jobs, in 2007 one institution, Harvard, produced 8 theory Ph.D.s. That's more than half the total number of tenure-track hires this past year. In the past a large number of these Ph.D.s ended up working in finance, but prospects in that industry are not looking so good either.

Comments

1. **JC**
   September 26, 2008

   They should also look at the folks who did not get tenure and subsequently left particle physics, and also the folks who got a faculty job but later quit of their own volition (and left particle theory).

2. **MathPhys**
   September 27, 2008

   Are the tenure track prospects of math graduates better?

3. **big vlad**
   September 27, 2008

   anyone know roughly what fraction of postdocs in theoretical particle physics will find permanent positions? what's the breakdown between 'theory' theory and phenomenology?

4. **Peter Woit**
   September 27, 2008

   MathPhys,

   I haven't looked into the numbers for math, but the situation seems to me to be very different, with job prospects a lot better. I know many people with particle theory Ph.D.s who have left academia because they saw no prospects of any kind of job at all where they could do research. Of the math Ph.D.s I know who left academia, almost always it was not because they couldn't get some kind of job if they wanted one, but they decided that the pay, working conditions, location, etc. were better outside academia.

   Big Vlad,

   There's also a more recent postdocs rumor mill, you could gather data from there. Then you'd also have to figure out the question JC asks, how many people getting tenure track jobs stay on and get tenure.

   As a very, very rough guess, my impression is that in recent years maybe half of the new Ph.D.s in theory get postdocs, and of these, maybe 1/5 end up in a permanent academic job where they can do research. So, if you're a new theory Ph.D., one chance in ten…

5. **Rien**
   September 27, 2008

   Remember that this is rather America-centered. Many people get their PhDs elsewhere and come to the US or Canada for postdocs and/or faculty positions. Europeans often do a couple of postdocs in the US and then go back to Europe for faculty positions, but Americans very rarely go outside of the US at all except maybe for CERN. The postdoc rumor mill has postdocs everywhere, so the comparison won't be perfect.

6. **steve hsu**
   September 27, 2008

   It's important to emphasize that your probability of getting a job varies dramatically as a function of where you do your PhD work. At best (Princeton, Harvard, Berkeley) it is 1 in 3 or 4. But at many departments in the top 10-20 range of traditional rankings the probability could be 1 in 10-15 (Yale, Columbia, Maryland each produced only 3 professors over 15 years), or even less than 1 in 40 (e.g., UCLA, BU, UIUC … produced 1 or fewer professors in the last 15 years!) Note these are positions in the US and Canada. If you include jobs in foreign countries the odds improve quite a bit, perhaps by a factor of 1.5 I would guess, but many Americans wouldn't take a job in, e.g., China or Chile.

   While it's widely known that the job prospects overall are poor in particle theory, I doubt people are familiar with this rapid drop off in the odds as a function of department ranking.

7. **ninguem**
September 27, 2008

MathPhys,

The Notices of the AMS publishes very detailed statistics about the job market in Math every year.
http://www.ams.org/notices/

8. **Peter Woit**
September 27, 2008

Steve,

Thanks, that's right, the dependence on department is dramatic. The rough "1 in 10" I had in mind is an average.

As Rien points out, this is just about the US market, but increasingly this is a globalized profession. Many of our students in recent years have come from China or other places outside the US, and there are now starting to be attractive job possibilities for them if they want to go back. The story of the next decade may be a rather weak US academic job market, but all sorts of opportunities in places like China and India. If you're a string theorist, a job in the US may be hopeless, but many possibilities in Beijing (actually I have no idea what the situation is with Chinese universities and string theorists…).

9. **a quantum diaries survivor**
September 27, 2008

Well, well. I did not get any royalties yet from Hsu's site. I wonder if they got lost in the mail.

The thing is, besides the name of my blog, which is there since January 2006, I am really looking more and more like a survivor myself. At the young age of 42 years and a half, I still have a temporary position as a INFN researcher.

True, when I won the 5-year researcher position I have now, in December 2005, the INFN was clear in explaining that winners would gradually get a permanent position.

True, in 2007 a procedure of stabilization of personnel with at least three years of employment was started. I received a letter which assured me that the INFN had no power to fire me after the expiration of the five-year contract. That meant I could only be hired permanently, at some point. But when ?

Then, early this year those who had won the position in 2005 were asked to pass a new selection. This new selection was presented as the way by which INFN could bypass some budget restrictions and finally hire us permanently without further ado. Needless to say, we all complied to the new request. Then, only a contract was going to have to be signed, and we would all be happy…

Six more months have passed, Berlusconi's government is ruling, and a law is being passed which stops all the stabilization procedures, preventing the hiring of people with temporary postions, regardless of whatever had been promised before.

I think there is still a chance that we do not fall in the category which is cut or fired. I worry not – I would continue doing my job even if I was fired. However, I cannot but smile at my sorry country and its contorsions.

So, here it is, from a true survivor.

Cheers,
T.

10. **JC**
September 27, 2008

steve hsu,

How much is the Princeton/Harvard/Berkeley factor a function of self-selection? (ie. The better folks prefer to attend the better graduate schools).

11. **steve hsu**
September 27, 2008

T: your check is in the mail 😉 Can I visit Trieste soon?

JC: It's certainly true that the quality of theory grad students at each school roughly correlates with the placement probabilities. But there are other effects, like having a powerful supervisor pushing for you in letters and phone calls. It also helps to learn from other smart students, and it helps to be part of a big bandwagon / fad like string theory.

But let's take the example of a less elite but still very strong physics department like UCLA. It's a big department with a large high quality theory group. During the last 15 years they probably produced 50 or so PhDs in particle theory and related fields. But only 1 of their PhDs got a faculty job at a research university in the US or Canada. Shocking, no? I bet those 50 people are all very smart. They're probably producing a lot of value and innovation on Wall St., in Silicon Valley, etc. I doubt they knew what odds they were facing when they started graduate school.

At U Oregon the typical student who ends up doing theory is either a Chinese / Indian kid with perfect physics GRE who graduated at the top of his class from a leading university, or an American kid who was at or near the top of his undergraduate class at a big public university or liberal arts college. At that stage in their lives all options are still open to them. Little do they know what they are getting into.

12. **postthis**
September 27, 2008

It's not so clear that the job market in, for example, math is any better. One would have to compare physics (all fields) versus math (all fields) or else particle theory versus some comparably theoretical area of math.

If you count research labs whose employees publish their work in some of the same places as research professors, then there would seem to also be more places for physicists than mathematicians (who tend to be crowded out by those with more domain-specific knowledge such as computer science PhD's).

13. **Jabotinsky**
September 27, 2008

The odds in math are a lot better. There's been an increase in the number of jobs to go along with the increase in number of PhDs. Generally, if you have a good thesis at a good university, you'll get some postdoc. Americans are favored to some extent due to VIGRE etc. Tenure-track is substantially harder, but it's not like in physics. People who publish interesting stuff generally can get a tenure-track job at some research place somewhere… but you might have to move to Undesirable State University. It's the trade-off to get paid to do your hobby, I suppose. There are of course a few who for some reason or another strike out on the job market, due to interviewing issues or just bad luck. And there are more than a few who end out teaching math in the myriads of liberal arts places. But I think at a Harvard or similar at least 1/2 of the PhDs would be able to continue a research career if they really want to, based on my knowledge of this.

14. **a quantum diaries survivor**
September 27, 2008

Steve, I am in Padova, no connections to Trieste. You are welcome to visit Padova however.

As a comment from somebody living and working outside US, I must say the situation in particle theory is not perceived to be so bad there from here. Probably we have a slow response and old inputs. In experimental particle physics the situation is so different that it probably also help confuse matters – more than a few of my colleagues have found a job in US universities after hitting a wall here. But of course, I am talking about Italy.

Cheers,
T.

15. **John Baez**
September 27, 2008

What about tenure-track physics positions that aren't specifically "in particle theory"? That is, positions where they just want someone with a PhD who can teach undergraduate physics. How many string theorists (or more generally, particle physics) take jobs like this? I wouldn't count this as the equivalent of academic death.

16. **postthis**
September 28, 2008

at most institutions whose math faculty have a chance to survive intellectually, there are similar faculty positions for physics. An Undesirable State University where a math professor has a chance to at least survive intellectually also has physics courses and a physics departments. If they have anything with labs or grad students the physics faculty size grows accordingly.

The observations about VIGRE and Harvard just illustrate that the math and physics job markets have different cycles. There was a time when Harvard math PhD's (in all fields) were in exactly the situation of particle theory today, with postdocs, even for North American graduates, being easier to find on other continents.

17. **anonmath**
September 28, 2008

Well, as a faculty member of Undesirable State University, I would note that most of our faculty have PhD's from Ivy League, Oxbridge, etc type universities, whereas our graduate students tend to get jobs at "four year colleges" which in turn have no graduate students.

So the numbers game works out when most people settle for stepping down a rung.

18. **Thomas Larsson**
September 28, 2008

In a steady-state situation, each advisor will on average give birth to one new advisor in the next generation. So if each advisor on average advises ten students during his/her career, only one out of ten PhD's can become advisors themselves. Assuming that the days of exponential growth are gone forever, I don't see how this can be avoided.

19. **AGeek**
September 28, 2008

I am reminded of Wilczek's [marriage optimization algorithm]. Just substitute "prospective Ph.D. student" for "suitor", "academia" for "courtship" etc.:

> You have to estimate the number N of suitors that you can expect to deal with over your career in courtship. We'll assume that you evaluate them one at a time, and that once you've broken up with one, then that one is gone forever. Then what you should do is this. Evaluate, but do not accept, each of the first N/e suitors. Here e is a number, the base of natural logarithms, approximately 2.7. Then accept the first subsequent suitor who is better than all the earlier ones. That is how to maximize your chance of getting the best possible mate.
>
> For example, if N is 10, then you should evaluate but reject each of the first 4 suitors, and accept to first one after that who is better than them. In my own case, I estimated N=3. I dutifully broke up with my first serious girlfriend, but the second was better, and I married her. It worked out fine.

20. **JC**
September 28, 2008

steve hsu,

In the case of Princeton folks getting particle theory jobs over the last 15 years, it would be interesting to see how many were grad students of Edward Witten and/or had Witten writing their reference letters.

In the case of Harvard and Berkeley folks, were there any particular "superstar" advisors who had a significantly large number of students who got particle theory jobs?

In contrast, do the "non-superstar" advisors at Princeton/Harvard/Berkeley have zero (or one) students who actually got a particle theory job?

It would be interesting to trace the lineage of physics professors from earlier time periods. For example, how many folks who got theoretical physics professors jobs during the 1920's and 1930's, were grad students of Niels Bohr, Arnold Sommerfeld, Max Born, Werner Heisenberg, Wolfgang Pauli, etc …? Did having a physics phd from Goettingen, Munich, Leipzig, etc … from the 1920's and 1930's, have a significant correlation to one getting a theoretical physics professor job?

21. **Jeff McGowan**
September 28, 2008

I've been on a bunch of hiring committees here at "Undesirable State University" in math, and the job market for math is certainly much better than for physics, but it is far from good. Last year we hired two new tenure track people, both with interesting research, one very new, one with a bunch of publications already. We had a couple of hundred overqualified applicants. For most of the 90's, the market in math was worse than it is now – people from good schools with good theses ended up in places they would not have thought of considering when they started grad school. At that time, math was much more tenure track oriented, unlike physics which had always from what I know been very postdoc driven. Vigre changed things a lot, and since those started most good people in math can graduate and get a postdoc, but the tenure track situation isn't great. Not to say you can't get a job, but getting a job at a Ph.D granting institution is really hard. I'm guessing it's going to get worse, since there was a bunch of hiring tenure track since many people retired in the early oughts, and I think that has mostly worked it's way through. In my department I am now basically the senior pure mathematician, and I think it is unlikely given the financial stuff going on that we will be adding any more lines anytime soon (we have added a few in the past 5 years). In any case I'm not going to complain on a physics blog, since I don't think math has ever been nearly as bad as physics. I would think Peter would know about this, as a physics grad. who ended up in a math dept. When I met Peter (what, 7 years after you got your Ph.D?) it was at a math conference and I assumed his doctorate was in math, as I remember he was trying to get me interested in Dirac operators in addition to the Laplacian.

22. **JC**
September 28, 2008

steve hsu,

In your example of UCLA, a keyword search at SPIRES

FIND AFF UCLA AND K THESIS AND DATE AFTER 1993

shows that there were around 20-25 folks who did a particle theory PhD there over the last 15 years. (You are off by a factor of 2).

Your estimate of around 50 particle theory PhDs over the last 15 years, is more consistent with Berkeley

FIND AFF 'UC, BERKELEY' AND K THESIS AND DATE AFTER 1993

In the case of Princeton, there's around 65-70 folks who did a particle theory PhD there over the last 15 years.

FIND AFF 'PRINCETON U.' AND K THESIS AND DATE AFTER 1993

In the case of Harvard, there's around 30-35 folks who did a particle theory PhD there over the last 15 years.

FIND AFF 'HARVARD U.' AND K THESIS AND DATE AFTER 1993

In contrast, a lesser known place like UC San Diego had around 10 folks who did a particle theory PhD there over the last 15 years.

FIND AFF 'UC, SAN DIEGO' AND K THESIS AND DATE AFTER 1993

(All these counts exclude the PhD theses in experimental particle and astrophysics areas).

If you poke around further with similar types of SPIRES keyword searches, you will notice many particle theory PhD folks who did not publish a single paper (besides their thesis).

23. **steve hsu**
September 28, 2008

Comments:

1) re: physics vs math, keep in mind that many smaller schools don't have *any* particle theorists, but all have math departments. Some liberal arts colleges don't have any theoretical physicists, only experimenters.

2) those SPIRES numbers do not sound correct to me. For example, I don't think there have been twice as many Princeton as Harvard PhDs in the last 15 years. I also doubt UCLA produced < 2 PhDs per year over the last 15 years, but I could be wrong. Are you sure everyone's dissertation ends up on SPIRES?

24. **JC**
September 28, 2008

If you want to check the Harvard numbers through a source independent of SPIRES, there's a list of Harvard physics PhD's sorted by year at

http://www.physics.harvard.edu/academics/phds.html

No idea offhand where similar lists (independent of SPIRES) can be found for other universities, short of going through the online library databases of particular universities.

25. **ninguem**
September 28, 2008

Steve Hsu

I don't doubt you, but why do liberal arts colleges prefer experimentalists? Wouldn't they save lab costs hiring theoreticians?

26. **steve hsu**
September 28, 2008

I don't understand the policy, although perhaps they think it is easier to get students involved in exptl projects than in theoretical projects. But of course the easiest projects for students are numerical simulations or writing code 🙂

27. **Matt**
September 28, 2008

Hi

I'm a new graduate student in cosmology-high energy physics, in a Canadian university. When I first met my supervisor, he was very clear about my job prospects; I should be ready not to work in academia and be willing to work in the industry. I was and I'm still fine with that (I'm not even sure I would accept a faculty position if I got offered one … I'm ok with working 70 hours per week, but I don't think I could do that all my life), but I got to admit that I didn't expect my odd to be so low.

Anybody know what most of the phd students who left academia end up doing ? How hard is it to get a teacher job in a small college (non-research) ?

28. **ninguem**
September 28, 2008

Matt,

Who the hell works 70 hours a week? Tenured faculty certainly not.

29. **Peter Shor**
September 28, 2008

Don't small liberal arts colleges need people to *run* the lab courses students take? While there are some theoreticians I would trust to do that, there are quite a few more that I wouldn't.

Also, in reply to Jeff McGowan, I can definitely say that the wave of retirements in math departments is still in progress at some schools, so the current grad students aren't completely out of luck. But I wouldn't expect it to last much longer.

30. **Matt**
September 28, 2008

Hi ninguem,

I think my current supervisor works about that much, and from what I have seen it's not a unique case (one of my undergraduate research supervisor worked during the weekends and on week evenings too, he basically lived in his office). It seems hard to really go home and stop thinking about your research, and I don't think it's a problem that applies only to students …

Not to mention, I don't know if that applies to every department, but in mine the professors are in competion with each other regarding their salary (based on the number of publications, the quality of teaching, outreach …). So that really encourages to do a lot of overtime.

Plus, everyone knows that what drives physicists is prestige, not money, and in order to be the best I doubt that 40 hours a week is enough.

31. **David**
September 28, 2008

This thread leads to my big gripe. A lot of people seem to equate theoretical physics with string theory. There's a lot more out there.

32. **chris**
September 29, 2008

somehow i don't get the point of this constant whining of hep theory people about the job market. somehow it reminds me of my days as a student, when fellow students would talk about which professor has the hardest exams etc. these students were not among the best…

when i got into the phd program (or even more generally into studying physics) i was perfectly aware that not everyone and her neighbor could be a physics professor. but hey, that's a challenge! if you would have told me at the beginning of grad school that the chances of finding a faculty position were about 10% after phd, i would probably have rejoiced that the chance is somewhat realistic. it should be made perfectly clear to all grad students that it won't be a safe and easy ride but honestly, who does not know that? you have to close your eyes really hard.

and finally, if you think that hep-theory is the most competitive field there is i suggest you to think again. i know a bit how things look like in liberal arts and humanities – specifically in linguistics. basically the situation there is that there is no job. period. a 10% chance for phds to get a faculty position would probably be considered paradise in these fields. and their backup options typicall are not as good either (at least here in europe i know that taxi driver is not an unrealistic option).

33. **Fabien Besnard**
September 29, 2008

Chris, who said the competion was fair ? With odds so low as 1/10, this is bound to be quite the contrary. Let me illustrate this. You see 3 people drowning : your child, Albert Einstein, and someone you don't know. If you are given 2 lifebelts, you'll probably save your child and Albert, but if you have only 1, so much for Albert… This is perfectly reasonable from your point of view, but the loss for the society is immense.

34. **Thomas Larsson**
September 29, 2008

Note AE had to take a job as a patent clerk, which is the academic equivalent of drowning. The real anomaly is that Max Planck hired an amateur as his assistent in 1910.

35. **Matt**
September 29, 2008

Chirs said :

" if you would have told me at the beginning of grad school that the chances of finding a faculty position were about 10% after phd, i would probably have rejoiced that the chance is somewhat realistic. it should be made perfectly clear to all grad students that it won't be a safe and easy ride but honestly, who does not know that? you have to close your eyes really hard. "

I'm sure it's easy to say that once you have your own faculty position, or if your confidence and ego was so big that you were already sure you would end up with a faculty position. But the truth is when you're not that confident, you do care about all those issues, and you also care about which teachers give the hardest exams (I did care about that and I graduated first of my promotion).

If for you it was just a big challenge, well good for you, but what if you would have end up spending 10 years at University with no job in the end, was the challenge worth it ? Perhaps then you should have considered your option from the beginning, and this is precisely what this post is for, it's a warning !

36. **Peter Orland**
September 29, 2008

I went into particle theory with my eyes open, fully aware of what the job prospects were (I finished in 1982, when it was just as bad or worse as today). I had help from a few friends, and wrote some possibly decent papers, but the main reason I got a job at a Ph.D. granting institution was determination. During this period, I kept trying to introduce myself to colleagues, despite doors being shut in my face (literally, in one instance).

My case is not very unusual. I know other people who applied every year for faculty jobs, spending their own savings to visit institutions and go to meetings, some being unemployed for a year.

Though luck is certainly a factor, one can improve the odds by persistence. That being said, I don't encourage students to do particle theory unless they can't be dissuaded.

37. **srp**
September 29, 2008

If we had more physicists working in the patent office, maybe we would have fewer stupid patents granted. I'd even pay for time off to do research–the gains in social welfare would be huge.

38. **estraven**
September 30, 2008

The job perspectives in mathematics are better than in physics, and have been for at least two decades as far as I know. Also faster: mathematicians have a nontrivial chance of landing a tenure-track position two or three years after PhD.

I wouldn't be a scientist now if I hadn't gotten tenure before my ovaries were ready for the trashcan.

39. **David McMahon**
September 30, 2008

These numbers are positively bleak, but what can one expect I guess. Physics is such an interesting subject but even with the LHC in operation, society doesn't really value theoretical physics if there are such small numbers of jobs available. However, something students should realize is there are some related positions in DOE laboratories. I have worked at Sandia labs where I was able to do some quantum theory related to quantum computing. There are people doing particle physics at Los Alamos. There are not many positions like this either, but the point is you don't have to go into academia to do physics. And the pay in DOE labs is very good. But despite this it almost seems like going into theoretical physics is like trying out for the NBA. I cannot honestly encourage a young person to consider an advanced degree in physics. The reality is you have to take care of yourself in life and getting a job happens to be central to that mission. I would guess that math is better. People need to learn math for all sorts of reasons so you probably face better prospects with a PhD in applied math.

40. **nbutsomebody**
October 1, 2008

Excellent article, a real eye opener. Is the situation same for other branches of theoretical physics like bio-physics or cond-mat?

41. **Thomas R Love**
October 6, 2008

ninguem asked: "why do liberal arts colleges prefer experimentalists? "

If the local situation (Cal State Dominguez Hills) is any indication, the experimentalists get more grant money. Two are with Super-K and one spends summers at the Jefferson Nat'l Lab. The only one without a grant is a theorist. I'm just a parttime lecturer and not included in the above data.

42. **Alexey A Petrov**
October 6, 2008

I think the data is incomplete — it's probably more relevant to look at
where the person did a postdoc before he/she was hired into that permanent position, not at the place where he/she got his/her PhD. In this case the situation would be even more startling… speaking as one of the "points" making up that study (as well as someone who's been on several search committees).

Also, liberal colleges take both experimentalists and theorists (and this is not reflected in that study too), it depends on a person and on college — last year a person I made an offer to for a postdoc position took a permanent job at a liberal arts college instead. What I mean is that colleges like to have their students to be involved in research. It's much harder to argue that a string theorist would work with sophomores on cutting-edge research problems…

43. **nbutsomebody**
October 9, 2008

In recent years we see another dangerous trend. Within string theory the people who are getting faculties are people who has topcited papers. That is well and good (although what is the relevance and scientific worth of citation in string theory is a big question itself, but lets not ask this at the moment.) However the well-cited papers are with already established names in the field and that helped the lesser known then postdoc/student collaborators to get a job.

Now if one sees the output of junior faculties after they get a job and what they are doing on their own (assuming he/she has not got in job in ivy leagues and not in the same place with his/her previous mentor), it comes out to be really pathetic! Off course there are few "noble" exceptions. But this seems to be a general rule., at least in string theory.

44. **JC**
October 9, 2008

nbutsomebody,

For these "pathetic" string theorists who had previously high citation counts and who later subsequently produced mediocre to dreadful work as junior faculty, what percentage were later denied tenure?

45. **nbutsomebody**
October 9, 2008

JC,
Surprisingly almost nobody !!!! and they seem to have a lot of clout too !!

Deal Breaker

September 30, 2008
Categories: Uncategorized

Discussion of quantum gravity at a level similar to that of parts of the professional physics community makes it to prime-time TV:

Update: I should have credited where I first saw reference to this (no, I don't normally watch this show…), it was at Capitalist Imperialist Pig. If you are having trouble with the embedded link to the video, you might have better luck following his link to the cbs web-site and seeing what you can find there….

Update: Lubos is claiming credit for this, implying that he's the model for Sheldon. He has a point. I always thought that the string theory community should worry about the implications of having Lubos represent them on the web, now there's prime-time TV to think about.

Comments

1. **Jeff McGowan**
   September 30, 2008

   OK, so I hope Lee and Peter get some royalties for this stuff 🙂 Actually, in relation to the previous post about the job market, I guess a lot of recent physics Ph.Ds are now writing for television?

2. **Per**
   September 30, 2008

   Not available….

3. **Peter Woit**
   September 30, 2008

   Hmm, works for me, not sure why some people are having trouble with this.

4. **Uncle Al**
   September 30, 2008

   Firewall must be set to allow embedded objects, mime objects, javascripts… mobile code in general through.

5. **JC**
   September 30, 2008

   Television writers are a dime a dozen. (Though one could argue the same for many other professions, including physics and math).

   All it takes is somebody reading several books (both pro and con) on string theory, to write it into a tv or movie script. Even an episode of CSI mentions string theory in a completely misleading and convoluted manner.

   http://en.wikipedia.org/wiki/The_Theory_of_Everything_(CSI)

6. **Anthony**
   September 30, 2008

   I think the link only works in the US ;-(

7. **Vince**
   September 30, 2008

   Peter,

   Can you please provide us with the link to that clip?

   Thanks!

8. **Kea**
   September 30, 2008

   The CBS site says that the content is unavailable outside the U.S.

9. **capitalistimperialistpig**
   September 30, 2008

   TBBT is probably the only television comedy ever that consistently gets the physics right. The physicist characters are broadly drawn comic stereotypes, but stereotypes we can recognize from our experience: The nut job you shared an office with in grad school, the slightly loopy prof down the hall, that wacko chick from Dabney.

   But not, of course, ourselves.

10. **capitalistimperialistpig**
    September 30, 2008

    (continued, sorry)

    Note, e.g., the DSR reference in Leslie's argument.

11. **Amos**

September 30, 2008

"TBBT is probably the only television comedy ever that consistently gets the physics right."

That's priceless.

12. **anon.**
September 30, 2008

Yeah, it's nice that Sheldon gives precisely the right response to the absurd claim that LQG calculates black hole entropy correctly. But what's with "Barbero-Immirzi parameter" being written on the whiteboard behind them? Was Leslie giving Leonard a lecture earlier in the evening?

13. **David McMahon**
September 30, 2008

I think the clip is hilarious. For the vast majority of the people watching the show, the discussion is going to sound like a bunch of nonsense but hey at least we're getting physics into the popular culture. Great timing with the LHC!

[off-topic material deleted]

14. **Fabien Besnard**
October 1, 2008

We I read this post and the comments I thought it was some form of elaborated joke (I could not watch the podcast), but I went to the CBS site and saw that there is really a show about theoretical physicists in the US ! Something like this would be completely unbelievable here in France. Does it mean that americans are better educated in science ? That would not surprize me. I also had this feeling by comparing books that popularize science : they tend to be of a much better quality in the US.

15. **Chris Oakley**
October 1, 2008

Expect more of this kind of thing. With very few jobs for particle physicists in academia, and now much fewer jobs in Wall Street, scriptwriting for TV may start to look very attractive (… god forbid that we should ever be required to do anything *useful*)

16. **quark**
October 1, 2008

This link works in Europe

http://www.youtube.com/watch?v=XOwS0N3sX_M

17. **a mathematician**
October 1, 2008

Wow, that clip was painfully unfunny. Maybe they really are hiring physicists to do their writing.

18. **Kea**
October 1, 2008

Heh, thanks You Tube!!! LOL! That was hilarious. And the script is simple enough that I'm sure anybody gets it – no physics knowledge required at all.

19. **MathPhys**
October 1, 2008

You can watch it on Motl's blog. Yes, it was written by physicists. Very stale.

20. **milkshake**
October 1, 2008

No amount of beer can make this show tolerable. Whats genuinely needed is a sitcom about Lubos' struggles with the confederacy of dunces.

21. **Will**
October 1, 2008

Dear Peter,

Why are you always concerned about public perception? Nobody is an authority in science and good scientists are not doing science to sway public perception. I honestly think your feud with Lubos Motl is nonsensical.

Motl does not represent string theory but his one views and prejudices.On the other hand, you have no better things to do than just rant against string theory and its public perception and so on. It

Together, you have done more harm to science by trying to sway public perception in one direction or the other.

Will

22. **Low Math, Meekly Interacting**
October 1, 2008

That was…interesting. I mean, on the one hand, I'm psyched something like this can even exist, but on the other, well, I'm with the "it's not very funny" crowd above.

Also, I thought the String Theorist's debate skills were uncharacteristically anemic. I mean, wouldn't the string theorist have mentioned sparticles showing up at the LHC, or RS resonances/KK towers due to large extra dimensions, or some other seemingly typical example, as a rebuttal to the LQG testability claim, rather than having such a weak riposte? Also, I should think an LQG theorist would know that ST can also be used to calculate the entropy of at least some varieties of black holes, and hence such a claim of superiority would have been entirely unimpressive to a string theorist. She seemed entirely ignorant of this.

If I can think of these things, I'm sure the writers could easily have. If you're going to make a stab at comédie vérité, going only halfway kind of kills the whole effort, IMO.

23. **Just a Note**
October 2, 2008

I sometimes like to poke fun at people, but in all honesty, most of the physicists and mathematicians I have encountered are not at all like the characters portrayed on this show.

Sure there are little quirks, and certainly some of the debate around LQG and String theory is juvenile (or at least passionate), but the community itself seems full of a lot of well-rounded, well adjusted, principled people (and Peter is certainly one of them).

Intelligence, by its statistical nature, is a rare gift, and I have come to understand that "nerds" get picked on more out of jealousy than any other reason.

So I think that at some level the fact that a show like this has managed to make it into the popular media should be viewed as a complement to the community derived from the underlying respect that most people have.

24. **capitalistimperialistpig**
October 2, 2008

Just a note –

Hey, it's called humor. Comic characters are always exaggerations. Sure there are lots of normal, well-adjusted, physicists and mathematicians – they aren't very funny, though.

But if you have spent a lot of time around physicists and mathematicians and not met people who remind you of the characters in TBBT, I'm surprised. I surely have.

To me, the humorous depiction of scientists in TBBT is not mean spirited. They are odd but human people dealing with the usual human problems in an odd and slightly exotic context.

Check out Motl's take on the show though, especially the fury he whips himself into against the physicist character who espouses LQG – simply because the writers chose to associate that characteristic with her for this episode. Now that's odd, human, and scary in Spanish Inquisition sort of way.

25. **Plank**
October 2, 2008

Not very funny but replace LQG and ST for 2 religions and be amazed.

26. **Shantanu**
October 6, 2008

Peter or others, any speculations on tomorrow's Nobel Prize?
Thanks

27. **diana**
October 7, 2008

Guys,

"really a show about theoretical physicists in the US ! Something like this would be completely unbelievable here in France"

My dear friend, the show isn't about theoretical physics. Since sarcasm doesn't work on the internet, let me give you a hint: the show is about sex. The vehicle is theoretical physicists.

I think it's a terrific show, very sweet and very funny. It no more puts down theoretical physicists than it puts down cute blondes. I can claim total objectivity because I am neither a theoretical physicist or a cute blonde.

2008 Physics Nobel Prize

October 7, 2008
Categories: Uncategorized

Half of this year's Nobel Prize in physics has been awarded to Yoichiro Nambu for his work on spontaneous symmetry breaking, the other half to Kobayashi and Maskawa for the CKM matrix as an explanation of CP breaking. A detailed explanation of the scientific context of the prizes is here.

The prize for Nambu is well-deserved and rewards one of the deepest, surprising, and most important ideas in particle theory, that of spontaneous symmetry breaking. In 1960 Nambu realized that non-invariance of the vacuum state under an axial symmetry could explain the existence of pions and determine many of their properties. He also realized that this phenomenon is closely related to what happens in various condensed matter models (including the BCS model of superconductivity), where the ground state is not invariant under a symmetry of the theory. Nambu has often been mentioned as a candidate for the award, and it's surprising that it has taken nearly 50 years for it to come about. I'm a bit curious about how often Nobel Prizes in other fields are awarded for work done a half-century ago. Unfortunately the rather difficult times particle physics has found itself in the last few decades may mean that the best way for a particle theorist to get the prize is to have been around during the field's heyday, and to remain in good health.

The Kobayashi-Maskawa award is a somewhat less obvious one, since it didn't involve a surprising breakthrough, and it is about something completely different than Nambu's work. In the early-mid 60s, starting with work by Nicola Cabibbo, it had been shown that (in modern language…) flavor mixing matrix for two generation models of the weak interactions is governed by a single angle, now known as the Cabibbo angle. In 1972, Kobayashi and Maskawa pointed out that for three generations the matrix is determined by three angles and a complex phase. The complex phase makes this a possible source of CP violation, and it seems to be the main if not only source of observed CP violation. The Nobel committee text makes the claim that:

> This is in fact a result known in mathematics since around 1950, but the contacts between mathematics and physics were not great around 1970.

This is a rather odd description of the situation, since the mathematical facts involved here are quite simple ones about which 3×3 unitary matrices satisfy certain conditions, and presumably could have been derived by mathematicians who first worked with such matrices in the 19th century if this particular condition had come up. I can't think of anything mathematicians learned around 1950 that is needed to solve the problem, and, once stated physicists could easily solve it without help from mathematicians (who perhaps would have been more likely to just have been a hindrance in this case…)

The quark mixing matrix embodies most of the unknown parameters of the Standard Model, and as such is a crucial object for experimental studies of particle physics. Because of this, the KM paper is the second most highly cited paper in particle physics (after Weinberg's "A Model of Leptons"), but this ranking doesn't reflect the depth or importance of the ideas.

Lots of other blogs are also covering this. Tommaso Dorigo points out that it's a bit anomalous that a Nobel Prize for the CKM (Cabibbo-Kobayashi-Maskawa) matrix doesn't include Cabibbo, who is still around. Lubos Motl thinks that what is important here is that Nambu is also a string theorist, and that one of the people on the committee making this choice was a string theorist, Lars Brink.

Update: Lots of coverage of this in the press. Michio Kaku has an editorial in Forbes, where, like Lubos, he sees the real significance as being that Nambu is a string theorist:

> And there may even be a theory beyond the Standard Model, a true theory of everything that can unify all forces. It's called string theory.
>
> Not surprisingly, one of the founders of string theory is Professor Nambu, a newly minted Nobel Laureate who has years of research ahead of him. The best is yet to come.

Comments

1. **Warren**
   October 7, 2008

   > it's surprising that it has taken nearly 50 years for it to come about. I'm a bit curious about how often Nobel Prizes in other fields are awarded for work done a half-century ago.

   van Vleck

2. **Peter Woit**
   October 7, 2008

   Thanks Warren,

   That's also physics, but it's true it was awarded at a time (1977) when particle physics was a lot healthier, and there were a lot of recent advances that could have been awarded prizes.

   I was a student at Harvard at the time, and from what I remember, the conventional department celebration was a bit impeded by the fact that van Vleck was long retired and gone, I heard rumors that no one even knew how to locate him and contact him….

3. **Pablo**
   October 7, 2008

   On his blog Lubos says: "Nambu, a Japanese-born American, is often described as one of the "fathers of string theory . . But the Nobel prize is formally given to him for a comparably famous discovery ", which implies that the REAL reason for the Nobel is not that discovery, BUT his work on string theory . . . interesting lecture of the events.

4. **helvio**
   October 7, 2008

   I don't agree with your opinion that the work of Kobayashi and Maskawa (KM) is not a breakthrough. As I see it, their suggestion was: 3 families => CP-violating phase => matter/antimatter asymmetry. Only 2 families were known by then, so adding a 3rd family is for me a very bold working hypothesis, albeit a very simple one. And this is because its consequences are not only physically relevant but also philosophically profound. I put this breakthrough in par with the proposal of strangeness (2nd family) to explain the pattern of particles in the 60's. It was also a very simple working hypothesis, but which makes a lot of sense theoretically when you see its consequences, and it fits the data when you later test it.

5. **Peter Woit**
October 7, 2008

helvio,

Sorry, I agree that postulating a strange quark (2nd generation) may have been a bold hypothesis, but once you have two generations, postulating a third just doesn't seem that bold.

6. **JC**
October 7, 2008

Didn't Chandrasekhar get his Nobel Prize (in 1983) for work he did in the 1930's?

7. **helvio**
October 7, 2008

What I mean is that the boldness is not just in the hypothesis, which is pretty easy to make, but in suggesting it and showing explicitly that it leads to a profound conclusion, like CP-violation. The mathematics of 3×3 matrices is very simple, indeed, but it's the chain of arguments in their paper that matter, and the insight of recognizing that it's a very important discovery and not a triviality. Even though Lorenz wrote down his transformations, it was Einstein who established the logical sequence of arguments that make them a consequence of a small number of very basic but profound principles. Even though 't Hooft claims to be the first to have calculated the beta function of QCD, he thought it was a triviality; but for Gross, Wilczek, Politzer it wasn't trivial, they recognized its physical relevance. Every first-year grad student is able to do the same calculation, it's a simple one, but the real physics is in the interpretation, and that even the best can miss. Kobayashi and Maskawa interpreted rightly the relevant physics behind such a simple hypothesis, that's why I think they fully deserve the prize!

8. **Steve Myers**
October 7, 2008

Isn't the problem for Cabibbo is that they won't give the prize to 4? Same with Dyson.

9. **Cormac O Raifeartaigh**
October 7, 2008

I absolutely agree – while a couple of colleagues are suspicious of a particle physics prize in an area that seems obscure to them, given the year that's in it (LHC), I think the prize for Nambu is well-deserved and decades overdue (compare with Saalam and Glashow for example).

10. **Cormac O Raifeartaigh**
October 7, 2008

P.S.
I mean I absolutely agree with the post above, not the comments. That said, I too think it's most unfair that the third musketeer of the CKM mechanism was left out – that damn rule

11. **JC**
October 7, 2008

By a similar argument, should Ludwig Faddeev have received the 1999 Nobel Prize (with 't Hooft and Veltman) in analogy with Cabibbo vs. K + M ?

12. **spear mark**
October 7, 2008

Well, the value of KM is related to the value you place on getting a better handle on why we are made of matter, and not some sort of thin gruel of photons, electrons, and positrons that would have resulted if matter and antimatter were completely symmetric.

Cronin and Fitch got the price in 1980 or so for the experimental discovery of CP violation, and it is very appropriate that KM get a piece of the prize for the phenomenology that most closely describes the experimental result. Their phenomenology led to more experimental work… the Fermilab and CERN experiments on direct CP violation and the Babar/Belle experiments. That pretty much proved that KM were right, and so the timing of the prize to them is just about right.

Of course, from a theoretical perspective, KM's math is trivial and so their work is not so impressive. But in contrast to string theory, KM's work is closely tied to the physical world, which in some people's mind is more important than impressive mathematics.

Cabibbo's work, I thought, was not easy to separate from other similar contributions, although he got the name on the parameter.

To me the surprise was Nambu. I'm sure he's a great guy, but there have already been prizes to Glashow, Weinberg, Salam, t'Hooft, and Veltman for similar work. Actually, there are way too many prizes to theorists for the Standard Model, and too few to experimentalists.

How about Prescott for polarized electron scattering? Or a group for 3 jet events? The long b lifetime? Unfortunately, in the current climate, there is a theology that nothing happened in experimental particle physics since 1974 or so. That is merely theology, however. The actual experimental work of the last 35 years is crucial and innovative, just undervalued.

13. **A**
October 7, 2008

The "Nobel prize that Kobayashi-Maskawa will get within 3 years" was announced in a my comment on NEW 2 years ago, see href="
http://www.math.columbia.edu/~woit/wordpress/?p=465#comment-16690.

The reason why Cabibbo is excluded is that two-generation mixing was already mentioned in a footnote in an earlier paper by other authors (unfortunately I forgot which paper).

14. **Meow**
October 7, 2008

Clicking on the Motl link displays a Woit-bashing page

15. **cyd**

October 7, 2008

Weird. Why were Goldstone and Cabibo left out? If it's due to the only-three-per-year rule, surely they can give it to, say, Nambu and Goldstone this year, and to Cabibo, Kobayashi, and Maskawa next year.

16. **cyd**
October 7, 2008

> I'm a bit curious about how often Nobel Prizes in other fields are awarded for work done a half-century ago.

Abrikosov and Ginzburg finally received their prizes only in 2003, for the profound work they did in the 1950s.

Incidentally, the 2003 prize would have been Landau's second Nobel prize—Ginzburg's prize was for the Ginzburg-Landau theory—if the committee had been a little more timely, and if he had live longer. As it turned out, Landau had his famous and tragic car accident 1962; the Prize was quickly given to him in that year, for his work on superfluidity, for fear that he would die without being honored.

17. **Coin**
October 7, 2008

Isn't the problem for Cabibbo is that they won't give the prize to 4? Same with Dyson.

I'm told that this was also a problem in medicine this year, with the Nobel Prize going to two members of the team that discovered HIV causes AIDS, and also the guy who discovered HPV causes cervical cancer; with some third guy who ran a separate team with a claim to have codiscovered HIV being left out. It's not entirely clear whether this choice was actually a determination that the third guy's claim to HIV priority was illegitimate, or whether they just ran out of slots.

18. **An italian**
October 7, 2008

CABIBBO:
NOBEL!!!
NOBEL!!!
NOBEL!!!

19. **dir**
October 7, 2008

the point of km's work is cp violation mechanism, they predicted the third generation fermions. this work is very important, but also a kind of easy, and km themselves are not comparable with nambu who is really great.

as for the work of quark mixing, not only cabibbo, gell-mann and levy had a similar work in 1960, their paper should be the following one, nuovo cimento 16 (1960) 705.

20. **Anonymous**
October 7, 2008

Increasingly these days I'm more and more skeptical of the Nobel prize's worth… I mean, if you do phenomenal work why shouldn't you be recognized for it (i.e. case of Cabibo)? what exactly does a (rule on) limitation on number of recipients have anything to do with the scientific value of the research?

And furthermore, the Nobel Committee seems to think the universe of academic research – perhaps less so literary – revolves around their validation… does it actually?! The Committee/s awarding the prizes have had a (seemingly unending) streak of pompousness on how they perceive themselves as 'judges of value' when they assign credit / value to other people's research.

Bottom line: A number of their decisions over the years haven't been as objective as one would hope to believe for so prestigious a prize; and some of their awards have been based on factor/s not directly associated with strictly intellectual / academic merit.

(I won't mention specific cases because would start a controversy which I don't particularly want to get into).

21. **anonymous fool**
October 8, 2008

The most rescent Bulletin of the AMS contains an interview with Atle Selberg, in which he comments about prizes.

http://www.ams.org/bull/2008-45-04/S0273-0979-08-01223-8/S0273-0979-08-01223-8.pdf

Here are some excerpts:

"Question. You have received the Fields Medal in 1950. You also have received the
Wolf Prize in 1986. Three years ago the Abel Prize in mathematics was established. What are your thoughts on these types of prizes in general—do you think they have a positive effect?
It does not advance science. No one does scientific work because there exist
prizes—I cannot imagine that. A prize will make one or more persons happy, but
it also gives rise to disappointment among many people, I would imagine.
Question. But do you believe it serves mathematics in the sense that it creates
publicity and thus raises the awareness of the public?
Whether it serves mathematics to get publicity is an open question.
Question. Coming back to the Abel Prize: what are your thoughts on the awards
so far?
I proposed Serre and Grothendieck as candidates for the first award in 2003, as
some people I would have preferred. I thought that Serre would get it, and that
also happened. Since then I have not made any proposals. Concerning the Abel
Prize, I have a somewhat ambivalent attitude. Let us consider the Nobel Prize:
I think it has caused some unintended harm by creating a strong distinction inprestige between those that get the prize and others, who certainly deserve it, but do not get it. There are, of course, some people that so clearly outshine others that the award is uncontroversial; in physics, for example, you have Einstein, Bohr,Heisenberg, Dirac, and a few others. However, since the prize is awarded yearly, it is inevitable that the distinction will not be so clear. . ."

22. **Thomas Larsson**
October 8, 2008

Coin:

– When it comes to figuring out who made seminal medical discoveries, we in the Nobel committee are the world experts, not a bunch of laywers, says Bertil Fredholm, chairman of the Nobel committee at the Karolinska institute.

23. **DB**
October 8, 2008

"It's not entirely clear whether this choice was actually a determination that the third guy's claim to HIV priority was illegitimate, or whether they just ran out of slots."

You're referring to Robert Gallo whose role in the discovery of the Aids virus was long the subject of major controversy. Anyway, not a very good day for Italians (Cabibbo) or Americans of Italian descent (Gallo).

24. **Christine**
October 8, 2008

At least two Brazilians physicists also "missed" receiving the Nobel prize:

-Mario Norberto Baibich in 2007 for the Giant Magnetoresistance effect, see here (he was the first author of the paper describing the effect).

-César Lattes: although he was the main researcher and the first author of the Nature paper on the meson pi, Cecil Powell was awarded the Nobel for it in 1950; see here.

According to an interview with Lattes:

Question: Do you think that the fact of being Brazilian contributed so that another researcher gained the Physics Nobel in the research where you have participated?

CL: Although the judge commission was formed by Englishmen, I believe that it was not my nationality that weighted in the decision of the winner. As much in the discovery of pion, in 1946, as in its artificial creation, in 1948, I had the contribution of the Giuseppe Occhialini. He was the one who should have received the prize. And, in 1950, who took the prize was the Cecil Powell, who also participated in the work. But forget about this. These huge prizes do not help science.

25. **Shantanu**
October 8, 2008

BTW this is the first time IIRC, that the names of the nobel committee
(at least in Physics) has been made public. Is that right? (or in previous years also the committee has been public?)

26. **Pankaj**
October 8, 2008

One could perhaps argue that KM deserved Nobel prize more than Nambu. KM showed how CP-violation could be explained. They gave a theoretical idea for an experimental fact. This idea could have been proposed by some else, but they were the first. This happens quite often in science. Even the experimental discovery of CP-violation could have been done by someone else. Their case is more like Glashow whose ideas have experimental support; while perhaps there is no experimental support for the ideas of Weinberg and Salam for which they got the Nobel Pize. Nambu's case might be more like Weinberg and Salam. However, it is also true that Nambu, Weinberg and Salam are held in higher regard generally for their multifaceted contribution to physics and perhaps because their contributions have been more mathematical. But in the end,
what may count is the discovery of the
secrets of nature rather than how
mathematically inclined your work is.

27. **Thomas Larsson**
October 9, 2008

Shantanu, the members of the Nobel committees have AFAIK always been public. That would be difficult to keep secret, given that the committee members can attend the Nobel ceremony and banquet, with TV survey and everything. However, the protocols from the committee meetings are kept secret for at least 50 years, so you have to wait to see who voted for whom.

28. **MathPhys**
October 9, 2008

So why not J Goldstone precisely? I thought it was always known as the Nambu-Goldstone mechanism. Maybe Goldstone didn't publish?

29. **Shantanu**
October 9, 2008

Thanks, Thomas. Do you or anyone else knows why S.N. Bose (of Bose-Einstein statistics) was not awarded the Nobel prize and if
he was voted ? (as I presume he would have been shortlisted more than
50 years ago that the protocols must have been made public)

30. **Thomas Larsson**
October 10, 2008

I don't know specifically about Bose, but an interesting account of the politics surrounding the early Nobel prizes for QM can be found here.

31. **Travis**
October 10, 2008

Just a nit to pick a few days late:

From SPIRES current citation counts, KM is the 3rd most cited paper in HEP, after Weinberg and then Maldacena. And of course the PDG's Review of Particle Physics dwarfs everyone with well over 20,000 to all versions.

I suppose this error is my fault, since we haven't posted new all time lists recently, and the "pass" happened recently. However, note that you can, for the most part, do this yourself live:

Find topcite 3000+ and sequence by citation count [SPIRES]

Which will give you _current_ papers over 3000 citations in order.

Mind you, this has nothing to do with the Nobel. Clearly the order of these papers, and the exact number of citations at this point, is more of a reflection of the community than saying anything too important about the papers. This high on the list there are, I hope, better ways of assessing the impact of a paper [this is probably true in general, but that's another story…].

32. **MathPhys**
October 10, 2008

There is some serious science fiction in the 3000+ list.

News From Various Fields

October 8, 2008
Categories: Uncategorized

The hypothetical field with one element (known as F_{un}) now has its own blog, ceci n'est pas un corps. Among the many things of interest, there's a link to a video of Alain Connes explaining his recent paper with Consani and Marcolli on the subject. This is part of a project at the Journal of Number Theory to get authors to produce video introductions to their papers.

Over at the n-category Cafe, there's a discussion of Alan Weinstein's new paper on The Volume of a Differentiable Stack. A stack is a sort of replacement of a quotient space, of a sort that allows one to keep track of the fact that different points may correspond to different stabilizers. One would like to naturally assign as many properties of spaces as possible to stacks, and one interesting question is how to count sizes, for instance how to define volumes. If everything is finite, it turns out the thing to do is to count points by dividing by the size of the stabilizer, but something much more subtle is required in the differentiable case, where the sets involved are all infinite.

The last year has seen a flurry of activity on hep-th that goes under the name of the "M2 mini-revolution", or maybe AdS4/CFT3. I'd been waiting for some expository accounts of this to appear to read more about it, and there's a new one here. It includes graphs that show a total of 157 papers on the topic with 223 authors. Things didn't really get started until this past spring, with the number of papers peaking at 40/month in July, with 53 new authors joining the game that month. Since then, the trend is downwards, with 21 paper in September. Thomas Klose does a good job of surveying the subject and what has been learned about it. The only applications mentioned are in the last line of the last page, which refers to possible future uses in condensed matter physics.

The 2009 fiscal year has started already, with no federal budget in the US. The idea now seems to be to wait until it's half over next March or so, then have the new administration put together a 2009 budget and a 2010 budget simultaneously. In the meantime, a continuing resolution has been passed, under which money can be spent at the level of the 2008 budget. This is pretty bad news for US HEP, since the relevant 2008 level was one involving serious budget cuts. The effect of these was mitigated by a later supplemental appropriation, and that can evidently be used to keep the Fermilab budget at a level such that layoffs can be avoided (see discussion from Oddone here). But it remains completely unclear what will happen to the lab a few short months from now.

This past weekend the Skeptics Society at Caltech cosponsored with the Templeton Foundation a conference on Origins: the BIG Questions. The afternoon session was devoted to discussing God, the morning featured physicists promoting anthropics. An account of the talks is here, including this about Lenny Susskind's talk

> The conference began with a real bang – the Big one of course, and a lesson on what preceded that singularity as best understood today by physicists. Susskind condensed his Stanford undergraduate cosmology course into a beautiful one-hour primer on the universal constants (Planck's, gravitational constant, speed of light…) that support life. It turns out that life can only evolve and survive in a narrow window of values for these constants, a fact that Christians have recently embraced as proof of an intelligent designer. But Susskind explained how quantum mechanics support the existence of a multiverse that regularly spawns new universes with different sets of constants, making it inevitable that our comfy universe should appear. (I asked him whether a future day Dr. Strangelove could create the conditions that spawn a new universe in our own – he said no, but without a compelling explanation.)

and this about Sean Carroll's:

> Caltech physicist Sean Carroll delivered a great talk on time's arrow – how time fits into the universe and how it cannot exist without fluctuations in entropy. He explained how the physical constants give our universe just the right amount of clumpiness so that time can flow, and he presented an alternative theory – consistent with quantum mechanics – on how universes can bear "babies" with differing constants.

After Carroll, Caltech biologist Christoff Koch explained how:

> consciousness may in fact entail a new force not yet discovered by physicists.

Update: Sean has a posting about the "Origins" conference, mainly devoted to explaining what was wrong with the presentation of a crackpot who was speaking as part of the religion segment.

Comments

1. **xstructure**
   October 8, 2008

   Look at

   http://xstructure.inr.ac.ru/x-bin/theme3.py?level=1&index1=461794

   for papers on "m2 mini-revolution".

2. **Bee**
   October 8, 2008

   Gee, I didn't know Sean makes in baby universes too. This topic will eventually fly just because the name is so cute.

   A friend just mentioned he is at ahoop and I was wondering what on earth that's supposed to be. Seems it is the meeting you are referring to, probably an abbreviation of 'Ahrenshoop'.

3. **K.B.**
   October 8, 2008

   What could Christoff Koch possibly mean? I've taken one of his classes at Caltech, and he seems like a reasonable guy, but:

   "…consciousness may in fact entail a new force not yet discovered by physicists…"

   …seems like a pretty dangerous/irresponsible statement to me.

4. **Peter Woit**
   October 8, 2008

Bee,

Yes, those slides are from the Ahrenshoop conference, more here:

http://people.physik.hu-berlin.de/~ahoop08/program.shtml

although the links seem to be temporarily broken…

5. **Michael T.**
   October 8, 2008

   I attended the Origins conference over the weekend and I must say all of the speakers were quite good. If my recollection is correct, I do not think Dr. Koch made the claim that consciousness is a fundamental property but rather posed it as a question and also considered whether consciousness was an emergent property of brain structure and neurochemistry.

   Susskind was more into the baby universe thing and heavy on the Landscape. What did take me by surprise was the number of possible universes in string theory. Last time I checked it was 10^500 and in Susskind's talk the number seems to now be 10^1000! I think it's getting kind of out of hand.

   The whole "God" discussion was rather pointless in my view and science can offer very little insight really. You either believe or you don't, simple as that.

6. **K.B.**
   October 8, 2008

   "…I do not think Dr. Koch made the claim that consciousness is a fundamental property but rather posed it as a question and also considered whether consciousness was an emergent property of brain structure and neurochemistry."

   Sounds considerably more reasonable. Thanks Michael!

7. **Esornep**
   October 9, 2008

   Bee said: "Gee, I didn't know Sean makes in baby universes too. "

   See his beautiful and extremely clear paper with Jennifer Chen, in hep-th 2004 on arxiv.

   "This topic will eventually fly just because the name is so cute. "

   Err, it has been around for many years already, eg Farhi and Guth back in 1987.

8. **Harry**
   October 9, 2008

   "ceci n'est pas un corps" is a nice reference title to belgian surrealist painter Rene Magritte, I think.. quite f_unny

9. **Chip Neville**
   October 11, 2008

   Peter,

   Brian Greene was talking about string theory today on "Bob Edwards Weekend," a public radio show. The URL is http://www.bobedwardsradio.com/bob-edwards-weekend/. You might be interested in his take on verifiability, and whether or not string theory is a theory or philosophy.

   Best,
   Chip Neville

10. **Tyler**
    October 14, 2008

    Hi Peter,

    (This is my first post to this blog, hoooooray!)

    I've been reading through your blog and found this comment from you on http://www.math.columbia.edu/~woit/wordpress/?p=673

    "I'm working on some new ideas (involving representation theory) about the mathematics behind the BRST formalism and how to handle gauge symmetries. I have a partially written paper now, hope to get something finished and done this summer."

    So, what's the deal? Summer is over! 🙂
    Will we see any of this on the arxiv? I can't wait!

    All the best,
    Tyler

11. **Peter Woit**
    October 14, 2008

    Tyler,

    Well, it's still a partially written paper, I fear. The problem is that my understanding of this keeps changing, just when I have something nearly finished, I realize there's a better way of thinking about it…

    Next week I'll be giving a talk at Dartmouth related to this work, the title is "BRST and Dirac Cohomology". I'm making up some slides, and hope to put those on-line, may also try putting up some blog entries soon that would explain some of the physics and mathematics background.

12. **Cplus**
    October 15, 2008

    The Harvard Endowment Fund is trying to sell a portfolio including a variety of venture capital and buyouts funds which could be one of the largest secondary sales of all time. As reported by the NYT, unconfirmed by the University.
    University scholars, now might be a good time to prepare for some serious belt tightening, if not already done. With the trouble all state budgets have now, especially California, it is unlikely that academics at State Universities will fare better.

13. **Shantanu**
    October 20, 2008

    Peter, have you looked at the slides of Aspera (european priorities in astroparticle physics) meeting held recently in Brussels

14. **Peter Woit**
    October 20, 2008

    Shantanu,
    I did see that, but astro-particle physics is just not something I know much about. For informed commentary on this topic, you need to look elsewhere….

The Landscape at Princeton and Harvard

October 15, 2008
Categories: Uncategorized

String theory in general seems to have gone very quiet recently, but attempts to intensively promote the string theory landscape view of fundamental physics show no signs of slowing down at all. The Princeton Center for Theoretical Science is running a year-long program Big Bang and Beyond, partially funded by the D.E. Shaw hedge fund. Next month there will be a program on "String Landscape: Examining how the string landscape alters approaches to fundamental physics and cosmology", featuring a public lecture by Leonard Susskind.

A couple weeks ago at Harvard, Frederik Denef gave a colloquium promoting Landscape research. The talk concentrated on making an analogy between the string theory landscape and condensed matter phenomena, relating this to recent attempts to use duality to study 3d CFTs of interest in condensed matter physics. Frederik also described a web-site (2ndcheek.com) which explains how string theory proves the Bible is right. Unfortunately this web-site no longer seems to be in operation.

Also at Harvard, or at least across the street, landscapeologists and other string phenomenologists have taken over the Clay Mathematics Institute this week for a workshop on Stringy Reflections on LHC, yet another attempt to make the case that string theory has something to say about the LHC, despite strong evidence against this (see for instance David Gross's talk at Strings 2008). Some of the slides from the talks have begun to appear on-line, and Michael Dine's landscape talk is here.

Update: David Berenstein has a report from the conference in Cambridge here.

Comments

1. **Robert**
   October 16, 2008

   Of course, on the internet, nothing is ever lost:

   http://web.archive.org/web/20070507032956/www.2ndcheek.com/stringtheory.htm

2. **Jason**
   October 16, 2008

   > "Frederik also described a web-site (2ndcheek.com) which explains how string theory proves the Bible is right."

   As I read it (thanks for finding that, Robert) it's more about how the Bible proves string theory is right than vice versa. With such sites — and there are many of them — it's always the religious tract proving the science. The Koran purports to prove not only string theory, but dark matter; supersymmetry; big bang; black holes; worm holes; GUT/ToE…

3. **changcho**
   October 16, 2008

   "Examining how the string landscape alters approaches to fundamental physics and cosmology", featuring a public lecture by Leonard Susskind."

   I went to a popular talk Susskind gave at Foothill College here in the Bay Area a couple of weeks ago and he talked about the Landscape as if it were already a non-controversial, accepted fact.

4. **Peter Woit**
   October 16, 2008

   Thanks changcho,

   I'm curious what sort of reaction Susskind will get in Princeton to his efforts to promote the Landscape. Until recently, this wasn't at all popular there, but this year Witten is gone (at CERN), and an influential Landscape proponent (Arkani-Hamed) may now be the dominant personality.

   My impression is that views on the landscape among string theorists remain definitely mixed, with physicists who are not string theorists very much unconvinced. Susskind's promotional efforts seem to me to have had the effect of convincing many people that string theory has gone off the rails into pseudo-science.

5. **anonymous**
   October 16, 2008

   Dear Peter,

   It may be worth emphasizing that the Princeton
   workshop is not particularly focused on the
   landscape research you are criticizing.
   It started with observational
   cosmology, then covered inflationary theory, and later
   is moving toward more and more speculative subjects
   including measures on the
   landscape, spacelike singularity resolution
   and ekpyrosis, etc..

6. **Peter Woit**
   October 16, 2008

   anonymous,

   Thanks for pointing this out. Next month's symposium on the Landscape is only one part of a larger program. I'm curious if you or anyone else knowledgeable about that program would be willing to try and characterize to what degree its participants are sympathetic to the point of view Susskind is promoting. For instance, if changcho is right that he's claiming the Landscape is now accepted fact, to what extent does that correspond to reality?

7. **Princeton Undergrad**
   January 25, 2009

   I don’t know much about the larger ideological currents, but Susskind didn’t even mention the landscape in his public lecture. He talked about the cosmic censorship conjecture, and touched on the holographic principle. I would note that the Center For Theoretical Science is headed partly by Paul Steinhardt, who has been very critical of the landscape idea as “unscientific.”

Princeton Companion to Mathematics

October 15, 2008
Categories: Uncategorized

I just recently got my hands on a copy of the new Princeton Companion to Mathematics, and I fear that this is likely to seriously impact my ability to get things done for a while, as I devote too much time to happily reading many of its more than 1000 pages.

The book is an amazing document (and physically, a beautiful, if weighty object), unlike anything else I know of. Its coverage of mathematics and mathematical culture is very wide and sometimes deep, but it makes no attempt to be comprehensive. Thus, the accurate title “Companion to” rather than “Encyclopedia of”. The most remarkable aspect of the book is the extremely high quality of the contributions from a large number of different authors. It includes many wonderful long expository articles, mostly at a level that a good undergraduate math student could hope to appreciate, with much of the book accessible to an even wider audience. The articles are often written by some of the best researchers and expositors around. For example, one can find Barry Mazur writing on Algebraic Numbers, Janos Kollar on Algebraic Geometry, Cliff Taubes on Differential Topology, Ingrid Daubechies on Wavelets, Persi Diaconis on Mathematical Statistics, and many, many others of similar quality. The table of contents is available here.

The book also includes extensive articles on historical topics in mathematics and short biographies of a large number of mathematicians, as well as coverage of applications and a section largely devoted to describing the art of problem-solving and how mathematics really gets created. This section includes a beautiful set of five essays called “Advice to a Young Mathematician”, which give five different equally fascinating perspectives from some of the best in the subject about how they achieved what they did, as well as what they have learned from years of helping students become researchers. The authors of these pieces are Michael Atiyah, Bela Bollobas, Alain Connes, Dusa McDuff, and Peter Sarnak. Luckily for all young (and old) mathematicians, this chapter is freely available here.

The person most responsible for this is clearly the editor (and author of some of the pieces), Fields Medalist Timothy Gowers, who had help from many others, including fellow Fields Medalist Terry Tao. Gowers has a weblog, and he has written about the book in these entries (and there’s a podcast interviewing him on the book web-site at PUP). Terry Tao has a posting about the book here.

If you’re looking for a gift for someone with a serious interest in mathematics, no matter what their background, you won’t do any better than this.

Comments

1. **Serifo**
   October 15, 2008

   Is there any similar book in physics ? I’m just a math’ s undergraduate student with some curiosity in mathematical foundation of physics (Quantum mechanics in particular). By the way , may I ask your views on Hilbert’s sixth problem ? Sincerely

2. **Peter Woit**
   October 15, 2008

   Serifo,

   I know of nothing like this book in physics.

   To be honest, I never understood exactly what Hilbert was looking for in this problem (“axiomatization of physics”). There are certainly parts of physics that are mathematically well understood and could be axiomatized, others where we still don’t understand things well enough to sensibly do this.

3. **Tim May**
   October 15, 2008

   Thank you, Peter, for this news. I just placed an online order for a copy.

   As a former physicist (is there really such a thing?) who found GR and black holes the most interesting things in the world, circa the early 70s, I now am finding the “quantum measurement” constellation even more interesting. I recently did a demo of the “quantum eraser experiment” from a Sci. Am. article, the one involving some polarizers and a needle between two oppositely-polarized filters. Wow. The essence of the EPR/weirdness reality demonstrated in a darkened living room.

   Sorry that this isn’t highfalutin’ string theory vs. loop quantum gravity vs. whatever, but it’s the essence of physics for me.

   And math. I eagerly await the Gowers book.

   –Tim May

4. **Serifo**
   October 15, 2008

   Thank you for the reply, it’s a pty there isn’t such a book in physics. When I was a high school student sometime ago, my physics teacher said ” more maths you learn, more easier is to learn physics ” , well next time I see her, I will say ” more maths you learn, more you struggle to understand physicist’s thoughts ” ! Maybe the starting point should be to axiomatize first those mathematically well understood theories then try gradually to capture the rest. Anyway sorry for bringing this off topic subject, cheerse to Bourbaki! Sincerely

5. **Fabien Besnard**
   October 16, 2008

   Yummy ! I’ll get a copy as soon as possible. Thank you for this.

6. **Navneeth**
   October 16, 2008

   That seems wonderful. Thanks, Peter. Added to my want-list.

7. **Navneeth**
   October 16, 2008

Fabien Besnard said: “Yummy!… ”

That’s exactly my reaction on reading about the contents! 😀

8. **Serifo**
   October 16, 2008

   I really hope they will produce : Princeton Companion to fundamental physics ! 🙂

9. **Navneeth**
   October 16, 2008

   One for Physics will require revision much often than one for mathematics. Maybe a web-based service will suit it better.

10. **Thomas R Love**
    October 16, 2008

    A book which does similar work is

    A Panorama of Pure Mathematics (Pure and Applied Mathematics (Academic Pr)) by Jean A. Dieudonne

    While the articles are at a much higher level (suitable for a graduate student or PhD) so is the price (about $225).

    I read it while in graduate school and it gave me a taste of other fields of mathematics.

    Several of the articles from the Princeton book were once available on line and I read several of them. They were at too low a level to interest me.

11. **Coin**
    October 16, 2008

    I know of nothing like this book in physics.

    From what you’ve said here it kind of sounds a bit like Penrose’s “Road to Reality”, though it sounds like it may be operating at a deeper level than Penrose’s book.

12. **JJ**
    October 16, 2008

    As mentioned by Coin, Penrose´s “Road to reality” is somewhat similar, giving some sort of overview of modern theoretical physics. Another interesting book, a little more demanding is Lawrie´s “A Unified Grand Tour of Theoretical Physics” (Amazon link: http://www.amazon.com/Unified-Grand-Tour-Theoretical-Physics/dp/0750306041)

13. **theoreticalminimum**
    October 16, 2008

    While I agree there is no such similar book for Physics, may I however point out that there exists the comparatively bulkier 5-volume set “Encyclopedia of Mathematical Physics“, which covers to some appreciable extent topics in theoretical and mathematical physics. Springer will publish a “Modern Encyclopedia of Mathematical Physics ” in 2010 apparently.

14. **Thomas R Love**
    October 17, 2008

    theoreticalminimum recommended the “Encyclopedia of Mathematical Physics“, and it looks good (judging by the description) but the price is a killer:

    Price:
    EUR 1,240
    USD 1,495
    GBP 855

    That represents about one third of my monthly take-home pay.

15. **theoreticalminimum**
    October 17, 2008

    I am acutely aware of the huge price, to the extent that people who would actually like to read anything from Elsevier would have to dig deep in their savings (recall for instance the resignation of the editorial board of “Topology” as protest against the outrageous prices of the publisher – find more here, here and here). That’s why we have gigapedia (hint) to be thankful for ;-).

16. **F Borg**
    October 18, 2008

    Paul Davies edited a successful physics tome “New Physics” in 1989 which has got a follow-up edited by Gordon Fraser, “The new physics for the twenty-first century” (Cambridge UP 2006). While they do not quite parallel the Companion to Mathematics these books still give nice overviews of what’s going on in physics.

    Eberhard Zeidler is by the way responsible for “Teubner-Taschenbuch der Mathematik” whose first part has also appeared in English as “Oxford User’s guide to mathematics” (Oxford UP 2004). The second part goes a bit deeper into the subjects including math physics. Lots of math for the money! Nirmala Prakash has written a quite “friendly” book on “Mathematical perspectives on theoretical physics” (Imperial College Press 2003). This is math phys seen from a stringy perspective. Quite different emphasis from *classical* math phys employed in electrodynamics, hydrodynamics etc. A modern exponent of the Jeffreys & Jeffreys tradition is Michael Vaughn’s “Introduction to mathematical physics” (Wiley 2007).

    Finally, for someone picking up physics I would recommend John Walecka’s recent “Modern physics” (World Scientific 2008) before they, if hooked, may proceed to Lawrie’s grand tour mentioned in an earlier posting.

17. **F Borg**

October 20, 2008

I just got a posting from *Springer*. **E Zeidler's** vol 2 of his gigantesque 6 vol set on math physics is announced (this is what the Germans call an "introduction" … 1000 pages plus per vol so far). One can download an interesting historical outline of physics from ch 1 here

http://www.springer.com/math/dyn.+systems/book/978-3-540-85376-3?detailsPage=samplePages

Though Zeidler can be associated w/ the **Bourbaki** tradition Zeidler's writing may have a stronger pedagogical intent judging from the above excerpt. I can see the advantage of a single author oeuvre in terms of coherence and style — but this one is a truly daunting task both for the author and potential readers!

Zeidler's Einleitung:

Volume I: Basics in Mathematics and Physics
Volume II: Quantum Electrodynamics
Volume III: Gauge Theory
Volume IV: Quantum Mathematics
Volume V: The Physics of the Standard Model
Volume VI: Quantum Gravitation and String Theory

18. **Steve Myers**
October 24, 2008

There is an encyclopedia of physics which is pretty good but I think it's over 10 years old. (I don't have it to hand here so don't recall publisher.) I wouldn't recommend the Penrose book — I'm sorry I bought it.

19. **Steve Myers**
October 24, 2008

If anyone's still checking here: the book is edited by Lerner & Trigg. It's from 1990 (second edition). VCH publishers. I find it useful.

20. **Serifo**
October 24, 2008

I`ve got Penrose`s " The Road to Reality " ! Although the book is quite well organized in respect to the topics, I found lack of clarity in a lot of the ideas ! Maybe because I`m just a young outsider in physics ; but I`m convinced the main reason is :
He tries to write the ideas as simple as possible (maybe too simple and informal), then the ideas get mix up and so confusing ! Actualy, I found similar problem in most of the physics textbooks, the textbooks are too informal to be understood ! Maybe via axiomatic approach , it will be easier to learn physics !
To Hilbert and his sixth problem 🙂

21. **Kaloyan Todorov**
February 19, 2009

Something of an equivalent in physics would be The Feynman Lectures on Physics by (you guessed it) Richard Feynman. A worthwhile read indeed!

Quantum Physicist Offers Solution To Global Market Meltdown

October 16, 2008
Categories: Uncategorized

I briefly met John Hagelin when I was an undergraduate. At the time he was a Harvard particle theory graduate student, soon to get his degree and start a quite respectable career in the subject. His biography on his web-site claims that:

> His scientific contributions in the fields of electroweak unification, grand unification, super-symmetry and cosmology include some of the most cited references in the physical sciences. He is also responsible for the development of a highly successful Grand Unified Field Theory based on the Superstring. Dr. Hagelin is therefore at the pinnacle of achievement among the elite cadre of physicists who have fulfilled Einstein's dream of a "theory of everything" through their mathematical formulation of the Unified Field—the most advanced scientific knowledge of our time.

Hagelin moved on from doing physics research, first to help run the Maharishi International University (now the Maharishi University of Management) in Fairfield, Iowa, then to run for president as the candidate of the Natural Law Party (it seems that "Joe the Plumber" was a member of this). Nowadays, the Maharishi has passed away, and Hagelin has moved to New York, where he is Executive Director of the Global Financial Capital of New York.

Soon after Hagelin moved to New York, his organization bought an impressive building down near Wall Street (chosen because "It's one of the very few buildings in all of New York City that's oriented due east"), with the goal "to inspire financiers to come forth to support the creation of Heaven on Earth". They also are buying up land near Princeton "where, with the support of the township, a university for 5,000 Yogic Flyers is to be established." Part of the plan seems to be to raise $3 billion or so from the financial industry, by offering 10% interest on 15 year loans.

Last year they held a news conference to explain how the a "surging stock market" was one

> of the concrete signs of the success of the Invincible America Assembly in Iowa—the largest-ever scientific demonstration project to document the effects of large group meditations on the economic and social trends of the nation, according to Dr. John Hagelin, world-renowned quantum physicist, executive director of the International Center for Invincible Defense, and President of the Global Union of Scientists for Peace, who is leading the Assembly.

This year the story is a bit different, with a recent press release entitled Quantum Physicist Offers Solution to Global Market Meltdown explaining that:

> a group of nearly 2,000 advanced experts is now in place at Maharishi University of Management in Fairfield, Iowa. He said the influence of coherence generated by this group is helping to calm the nation in the midst of the global crisis, but a larger, more powerful group of 8,000 experts (the square root of one percent of the world's population) is needed to neutralize worldwide fears and re-establish confidence in the global markets.
>
> The cost to establish this group on a permanent basis, Dr. Hagelin said, would be negligible compared to what has been lost in a single hour during the current financial crisis.

In a recent Open Letter to the Yogic Flyers of the Nation, Hagelin, the Raja of Invincible America, urges them on as follows:

> I am writing you from Wall Street, where I am living and working with members of my national team at the Global Financial Capital of New York, one block from the New York Stock Exchange. We are starting to make remarkably good progress in our efforts to bring Maharishi's knowledge of enlightenment and invincibility to leaders here whose thinking and actions vitally impact the whole world. However, ultimately, our success is dependent on you. We will be successful when the leaders are receptive to our message. But their openness is 100% dependent upon the numbers in the Domes. Why? Because a rise in national consciousness directly translates into a rise in openness among leaders of business, health, education, defense, government, etc.
>
> At this critical time I urge everyone to fly together in large groups
>
> Maharishi said we need 2500 experts flying together to guarantee invincibility for America. This requirement is because the turbulence in the collective consciousness of one country can easily spread like a wildfire to create a similar turbulence in another country. And this is why, at this critical time, with the economic stability of the world hanging in the balance, I urge everyone in Iowa and everyone in the country to recommit to fly together in large groups.

Update: Today was a beautiful day, and on a bike ride downtown I stopped to take a look at 70 Broad Street, the headquarters of Global Financial Capital of New York. Looking in the windows of the below ground level and the lobby level, the place looked pretty much abandoned. The building has some floors upstairs, maybe there is some activity up there.

They did just put out this press release, announcing a press conference to be held in the lobby at 11am on Tuesday and webcast. Hagelin will be announcing a $1 billion plan to fund 1000 experts in New York and 8000 experts in Iowa who will "create coherence for the whole world—the basis of an invincible, prosperous global economy." No word in the press release about where the $1 billion was going to come from.

Update: Video of the press conference is here. The plan is simple: just give Hagelin and his followers $1 billion, and they'll have this financial crisis all sorted out soon, by means of experts using the unified field.

Comments

1. **Joe the Physics Student**
   October 17, 2008

   Hi Peter,

   How does this individual's activities have anything to do with current physics research?

   Thanks.

   Joe the Physics Student

2. **Boo Radley**
   October 17, 2008

   Simply hilarious!

(At least he is making a prediction, though.)

3. **Vishal**
October 17, 2008

Is this for real?? Seriously? Is our science education not doiing its job well?

4. **Chris Oakley**
October 17, 2008

Rather than committing to yogically fly together in large groups, I would recommend

(i) Not allowing banks to pay big bonuses to their employees. These seem to end up being funded from my and your pension fund.

(ii) Ensuring that there is no such thing as an "off balance sheet" transaction … anything that could harm the ability of a bank or investment institution to pay its debts is, AFAIC, on balance sheet

5. **chris**
October 17, 2008

hmm.. i guess the square root of one percent of the worlds population is more like 8000 sqrt(people). how can such a renowned quantum physicist not get the units correctly?

6. **Frank Quednau**
October 17, 2008

Funny stuff. Let him get his numbers, if it works, it's nice.
@Chris: Looks like the units are OK. If you square an expert you get a normal person. Sounds about right to me 😉

7. **ObsessiveMathsFreak**
October 17, 2008

If I have learned one thing about "invincibility" it is that it always wears off a few seconds after you get the star, and if you weren't thinking ahead you'll probably have landed yourself in a very sticky situation by the time this happens.

You cannot apply physics to economics. I think that the only reason physicists migrate to this field is hubris, thinking they can "solve" the stock market. Ironic considering they have been essentially unable to solve/resolve the existing problems in their original field.

8. **andy.s**
October 17, 2008

Jesus, these guys are loaded with cash. All of it from thousands of saps who think they can fly by flapping their legs.

I am in the wrong business.

9. **Thingumbob**
October 17, 2008

I believe that it might be possible to get the Boltzman brain to astral project into this corner of the landscape to overcome our financial crisis. Has Professor Hagelin considered this?

10. **Bee**
October 17, 2008

Needless to say, E8 explains the global financial crisis

11. **A**
October 17, 2008

Actually, quite sadly this is an example of where a physicist has become a crackpot and is now conning a bunch of spiritual people who try to understand meditation and consciousness through what appear to be honest means.
It's a disgrace.
Joe the physics student, the relevance here is that he has risen to a position of power because he has sold himself as the world expert on The Grand Unified Theory. He has sold his GUT research as being The solution. Frankly, the Maharishi people are being scammed by Hagelin and his ilk.

12. **Joao Leao**
October 17, 2008

Hum! I wonder what kind of golden parachute Global Financial Capital of New York will provide for Hagelin given that his flying days may be about to end soon and abruptly. On the other hand that 3 Billion raising proposal looks to me quite like an "airplane" (Ponzi) scheme…

13. **Aaron Bergman**
October 17, 2008

I think that the only reason physicists migrate to this field is hubris, thinking they can "solve" the stock market.

Hubris had nothing to do with it. The reason physicists migrated to finance is because they paid lots and lots of money.

14. **Yatima**
October 17, 2008

"The reason physicists migrated to finance is because they paid lots and lots of money."

I can feel that in my bank account.

Ok guys, you had a good laugh, now how about getting back to doing some serious, honest-to-god work?

15. **Covariant**
October 17, 2008

    I think I am beginning to appreciate Peter's opinions on things

16. **Sakura-chan**
October 17, 2008

    Thanks for the laugh Peter. You presented the info beautifully, and with a straight face. =)

17. **gasp**
October 17, 2008

    Imagine if we had to choose between Hagelin and Bush. I honestly don't know who I would choose.

18. **Peter Woit**
October 17, 2008

    Please, no more off-topic comments about quants and Wall Street,especially because no one seems to have anything new to say about that topic.

19. **David McMahon**
October 17, 2008

    I think assuming that John Hagelin is conning the group is jumping to conclusions. Never underestimate the power of belief, just because someone got a PhD at Harvard that doesn't mean they are susceptible to it. Spiritual belief is a powerful force, and although the ideas may seem nutty to some of you they speak loudly to lots of people. In fact spiritual beliefs are more powerful than science because it taps into emotions. Emotion is all to often stronger than logic. Think about it-emotion comes from the deep-seated more ancient part of the brain, the limbic system. Spirituality is something people feel in their gut, but analyzing something scientifically is something you do with your cortex. So what I am saying is I wouldn't be the least bit surprised if Hagelin believed every last word of this. Doesn't matter if he was a particle physicist or not.

20. **Covariant**
October 18, 2008

    David McMahon said

    "Spiritual belief is a powerful force"

    So should we quantize and study it? Who gets to be Yoda?

21. **Marty**
October 18, 2008

    Actually when you go beyond a natural gut reaction to be skeptical and dismissive, there is a lot of absolutely amazing (and published) research behind what John Hagelin is saying. Take a few minutes and check out http://permanentpeace.org/
    In more than fifty studies published in scientific journals, including the Journal of Conflict Resolution and the Journal of Mind and Behavior, the method of sustained large group meditations has been documented to powerfully reduce violence and criminal activity and even calm open warfare. It is a technique that is currently also being used in some of the most troubled inner-city schools in the country to defeat the stress and violence that plague the learning process.
    Please don't laugh at it. Inform yourself about it. Then decide whether it is hokum or a new paradigm that presents consciousness as a magnificent and unbounded existential frontier full of all possibilities. Practiced individually, Transcendental Meditation reduces mental stress and increases personal wellbeing, harmony, productivity, creativity, and happiness. When practiced together in a group, the "good vibes" radiate outward into the unseen field of collective consciousness, creating the same effects on a societal scale. The bigger the group the better, and the further the effects spread, like ripples in a pond. Another analogy is to think of radio or TV transmitters that beam signals through an unseen electromagnetic field. Instead of picture or sound signals, groups of meditators generate a strong wave of coherence and positivity through an underlying field of collective consciousness. Stress and tension diminish.
    Scientists are beginning to recognize a "non-local" field of global consciousness into which intentions can have effects at great distance. A field effect. We should explore this territory, for it may hold the secret for planetary peace and harmony. We'll never know unless we are bold and courageous enough to put it to the test.
    Obviously, I am one of Dr. Hagelin's admirers, and a practitioner of meditation. But I am also a professional medical writer who writes about many so-called "breakthroughs." Most are far from breakthroughs. This is the real thing.

22. **gasp**
October 18, 2008

    Marty, religious and cult-like memes are not transmitted by telepathy. They use more mundane forms of communication, such as you typing a reply on a blog.

23. **Thingumbob**
October 18, 2008

    This non-local TM stuff sounds like spooky action at a distance to yours truly. Ahem.

24. **gasp**
October 18, 2008

    David McMahon, emotional experiences exist. Spiritual experiences do not. So they should not be equated.

25. **Marty**
October 18, 2008

    Gasp and Thingumbob:
    I understand your reactions. Take a few minutes though to check out the science. It is very strong. So strong that public educators from around the country are introducing TM in their schools. If it was based on a religious or cult-like meme they would have none of it.
    Gasp…I like that word meme…hadn't heard it before.
    Meanwhile, the memes of the Wall Street cult have been a disaster.
    Cheers.

26. **Marty**
October 18, 2008

gasp
what's interesting about the subjective spiritual experiences in meditation is that they have measurable physiological effects. studies have shown, for instance, that cortisol levels improve through meditation and that high blood pressure is knocked down. In fact, the National Institute of Health has funded a number of studies showing how TM has been as effective, yet without any negative side effects, as medication in improving blood pressure among african-americans with refractory hypertension. One of the reasons that I am such a believer is that the practice normalized my blood pressure after a couple of months. The doc wanted to put me on medication. I said I would rather try something natural. It worked great.
Regards

27. **David McMahon**
October 18, 2008

You guys completely miss the point. Spirituality is a powerful force IN PEOPLES LIVES and the EMOTIONAL STATES they have. Spiritual experiences exist through the emotional experiences people have. There was an implication that Hagelin was conning the TM crowd because he had a PhD in physics and "should know better" and I am saying no, Hagelin probably really believes what he says. Spiritual belief (and the accompanying emotion) is more powerful that logical thought for most people.

28. **Marty**
October 18, 2008

David…you are correct. Spirituality is indeed a powerful force in peoples lives and their emotional states (e.g., symapthetic vs. parasympathetic mode).
The implication that Hagelin is conning us in the TM crowd is based on some outside speculation with no basis in fact. If I am being conned, so are a lot of Ph.Ds, doctors, lawyers, scientists, students, investment managers, principles, actors, writers, etc., who are a whole lot smarter than me. They ain't stupid and they ain't gullible. They see the results. They see the science.
Hagelin not only believes what he says, but he has participated in the published scientific studies that explain or validate very fascinating explorations into consciousness.

29. **Covariant**
October 18, 2008

David McMahon said

"Spiritual belief (and the accompanying emotion) is more powerful tha(n)t logical thought for most people."

I think that is a valid observation, but what happens when one group's beliefs are different from another's and both groups "know" they are correct in their beliefs?

I think Hagelin hasn't thought about that too much.

30. **Marty**
October 18, 2008

Covariant:
I think Hagelin has thought about that a lot and has attempted to prove the value of spirituality (and specifically a spiritual practice such as meditation) on personal and societal scales through science. Could it be that the fundamental basis of subjectivity (call it emotions, spirituality, belief) and objectivity (science, matter, the physical world) are one and the same: an unseen universal field of intelligence.
You folks ask great questions.
Why don't you invite Hagelin to speak to you. He lives in the neighborhood. You can throw all your implications, doubts, and questions. I am sure you would find it very informative and it would open your awareness to a very cool world that begs exploration by bright minds such as yours.

31. **a.k.**
October 18, 2008

..one should add that the intertwinement of obscure reasoning (I am not denying any effect of meditation etc. on physical and mental health) and financial power, especially the entanglement of irrational methods in physics and political and economic power manifested in one single person, seems to shed light onto some well-known 'dialectic' aspects concerning the role of quasi-religious movements, esoteric 'enlightment' opposed to those methods which were in europe once called 'enlightment', in the western society. 'Esoteric enlightment'- this also stands for non-rational analysis of the financial market (solving problems by 'flying together in large groups'), for ethnocentric views of human societes and for poorly reflected boundaries of the scientific method. To cut it short, Hagelin manifests one of the reasons why I quit thinking about physics a long time ago.

32. **Thingumbob**
October 18, 2008

I would invite him, sure, but just now I have brought back from other side Monsieur Mesmer, and he confides in me that I need beware: there may be many hoaxers who only want the great and mighty of this world to supply their unspiritual monetary wants. Of course, if all this could be done without any need for money…

33. **Covariant**
October 18, 2008

Marty

I have no doubt that meditation is great; I have no doubt that a lot of people of common beliefs meditating together is even better.

I also have no doubt that there are a number of people who absolutely don't feel that way, and some of those never will feel that way.

34. **Marty**
October 18, 2008

Thingumbob:
You folks are delightful to chat with. But my garden overfloweth with weeds and I must leave you.
Please look at the science. And then draw your conclusions and critiques. At least then you will be armed with more than mere opinion and suspicion. And then invite him.

35. **Marty**
October 18, 2008

Covariant
You are absolutely right about a lot of people never changing their minds, and never will. No problem.
One of the neat things about this whole concept is that MOST people are skeptical….it is some mystical hogwash…etc. That's what they thought about meditation when it first became popular forty years ago. Now doctors prescribe it for patients, and school principles are using it to transform the most stress-ridden schools into the learning facilities they were meant to be. Science has shown it is very valid and relevant for our modern conflicted society. It is not a matter of belief.
The greater concept of group meditation on a societal scale that Hagelin advocates is a exploration and exploitation of the unchartered and unbounded possibilities of consciousness. It is amazing to me that psychiatry and psychology spend so much time groping around in the darkness of mental illness, when there is this huge and untapped universe of consciousness and mental potential to explore.
Over and out.

36. **Vishal**
October 18, 2008

I am pretty sure the Buddha would have been "horrified" to hear people talk about TM and stuff like that. There is nothing inherently "spiritual" about meditation, which is a wonderful and beneficial way of exploring phenomenon and events from a phenomenological perspective. There is no doubt that meditation confers great benefits (physiological, mental) to the practitioner, but people like Hagelin, not to put too fine a point on it, simply delude themselves into believing the esoteric or anything that is out of this world. Frankly, with so many "gurus" coming out of India, it tends to create more often than not a wholly incomplete and distorted picture of that country.

37. **Marty**
October 18, 2008

Vishal
To speak for Buddha is purely speculative on your part.
In India, Maharishi Mahesh Yogi, the founder of TM, is revered for having put into motion a great revival and "cleanup" of the massive distortions that have crept into the vast body of Vedic knowledge.

38. **Peter Woit**
October 18, 2008

Enough with repetitious comments on the pro/anti meditation topic, which in any case seems to have little to do with Hagelin's venture into the financial industry.

39. **A**
October 18, 2008

David McMahon, I think in fact you missed my point. The fact that Hagelin may or may not believe in the beliefs of the Maharishi is by the way side. He is SELLING them grand unified field theories. He is using his status as a physicist, his PhD from Harvard in his position with them. It is not his spirituality that is relevant but rather the fact that he attained that position using physics. It is not that a physics PhD taught him to be well reasoned and solve problems and so he has gone into the world and applied general principles to other work, like for example physicists have in finance. He is telling the Maharishi people that grand unified theories and string theory are the solution to what the search for. Hence he has conned them. He has told that that the grand unified field is itself the field of consciousness (notice the trick by using the world field here). They are trying to understand consciousness which is a perfectly respectable goal. And they have tried to speak to scientists to describe their experiences in the hopes of understanding and recording more scientifically what they have done. Hagelin and his ilk have conned them – in that they use fancy physics words to convince these people that they are experiencing the string field of consciousness blah blah blah. They translate basic human experience into fancy vacuum landscape language that is frankly just junk.

40. **Vishal**
October 18, 2008

> In India, Maharishi Mahesh Yogi, the founder of TM, is revered for having put into motion a great revival and "cleanup" of the massive distortions that have crept into the vast body of Vedic knowledge.

Marty,
It is amazing to see how much people (especially non-Indians) buy into this whole "Vedas contain all knowledge" idea! They tend to miss the point that the resurgence of interest in the "Vedas" and other "ancient scriptures" of India is closely connected with right-wing politics and nationalist ideology, today. A lot of the Vedas contain purely speculative knowledge – now that sounds almost like an oxymoron. The Buddha had thoroughly and rationally debunked a lot of the core beliefs of the Vedas more than two thousand years ago, and the fact that those debunked beliefs are regurgitated again and again, to me, is an indication of how powerfully those ideas grip the minds of people throughout history and even to this day.

Peter,
Please forgive me if my comments are totally off-track. But I do want to point out that some of the huge confusion people have about modern physics, "field of consciousness" and so on, clearly spring from the misinformation that is spread *vis-a-vis* "Vedic knowledge". The fact that Indians themselves don't know what to make of the claims about Vedas but that quite a few non-Indians readily accept whatever is told them about the Vedas should immediately raise a red flag. Anyway, the end result, as A correctly pointed out above, is that a lot of ordinary people get conned in a subtle way without even their knowing what hit them.

41. **David McMahon**
October 18, 2008

If you are conning somebody, you are misleading them with something you know to be false. I don't necessarily buy that Hagelin believes these ideas are false but is selling them off on people anyway. So that is why I don't think Hagelin is necessarily a con.

42. **Covariant**
October 18, 2008

Well, its a free country, and as long as Hegelin and crew use the money they raise as intended, and as long as they make their payments to whoever feels inclined to invest, then there really isn't much one can do or say.

I will be curious if they can pay it all back in 15 years at 10%. I doubt they will raise $3 billion, not without posting some significant collateral. If they have market research that supports their claim about the amount of money they'll make, then I think they will have a number of businessmen interested.

43. **Steve**
October 18, 2008

it may be a challenging stretch (which is sometimes a very good thing), but it definitely isn't completely off base and unsupported:

"Consciousness & Superstring Unified Field Theory"

http://www.youtube.com/watch?v=OrcWntw9juM

http://www.youtube.com/watch?v=FSxluvq5HI0

…give a listen (or not) and then critique the substance, and not just the superficial notion

44. **David McMahon**
October 18, 2008

Thanks for posting the videos. I would say his claims about what string theory says are a bit of a stretch.

45. **David McMahon**
October 18, 2008

Hi Steve,
I think the videos are pretty good, and I don't think Hagelin is completely wacky (wish I could use italics). Who knows, there may be something to his explanations. Afterall consciousness is something hard to grasp for science as it stands now. I would say explanations that its an "emergent" property from the "complexity" of the brain are non-explanations when you get right down to it. The fact is no scientist alive has any idea how the brain gives rise to awareness. Its the biggest problem in science-far bigger and more complex than anything in particle physics. But it would be an interesting twist if Hagelin's musings (and lets call them musings, they are not much more) about string theory (or lets say any unified theory since string theory is frowned upon here) had something to do with why life is conscious. David

David

46. **David H. Miller**
October 19, 2008

David McMahon wrote:
>Afterall consciousness is something hard to grasp for science as it stands now. I would say explanations that its an "emergent" property from the "complexity" of the brain are non-explanations when you get right down to it. The fact is no scientist alive has any idea how the brain gives rise to awareness. Its the biggest problem in science-far bigger and more complex than anything in particle physics.

Dave, I agree with everything I just quoted from you. And, so do many sensible scientists and philosophers: the late Nobel laureate in neural science Sir John Eccles and the philosopher Karl Popper (see their book "The Self and Its Brain," physicists such as Schrodinger and Penrose, serious contemporary philosophers such as David Chalmers ("The Conscious Mind") and Colin McGinn ("The Mysterious Flame"), etc.

At some level, this should not even be controversial: after all, no neural scientists that I've heard of claims to have fully explained consciousness. I myself won't be surprised if the full explanation of consciousness shakes science just as much as quantum theory or evolution did.

However, I see no evidence that Hagelin is moving us towards such an explanation. I suspect that he does believe his own nonsense, but most of what Peter quoted from Hagelin is, indeed, nonsense.

To use one's standing as a scientist to point out that there is much we do not yet know and, probably, some big surprises in store is perfectly fine. In a sense, that is what Peter is doing in theoretical physics: he maintains that the future of theoretical physics is much more open than some of the superstringers think.

But to claim to have answers that you do not have, as Hagelin does, is dishonest, even if it is the sort of dishonesty that consists of deceiving oneself.

Dave Miller in Sacramento

47. **Thingumbob**
October 19, 2008

I guess perhaps I need to be less satirical to attempt to bring home my point here. There have been all sorts of spiritualists that have used many different varieties of scientific sounding gobbledygook to gull rubes out of their fortunes. For instance, the Rosicrucians, Annie Besant, Aleister Crowley, and on and on. This stuff is as old as dirt. The problem today is when it comes to this or that variety of GUT, its very hard to know the difference. (As far as the notion that consciousness has a physically mechanistic causality, I would remind you that this quixotic idea led Descartes to the absurdity of believing that the pineal gland was the seat of the soul.)

48. **Jeff McGowan**
October 19, 2008

OK, why do so many scientists seem to think it is inevitable that scientists will one day "explain" consciousness. I have no doubt that consciousness is a physical phenomenon, but that doesn't mean that anyone is ever going to explain it (whatever exactly that might mean). It seems to me there might very well be a metalanguage problem here of the highest order.

49. **Covariant**
October 19, 2008

After watching the videos two thoughts struck me:

1) I am even less inclined to believe that there really is such a thing of self awareness or consciousness. As against the grain that might be; although I manifest thoughts, I can observe that those thoughts are not entirely dependent on me. (Or as a mathematician I know stated, similar people tackling similar problems tend to come up with similar solutions).

2) The "unified field" as described by Hagelin, really seems to leave out the observed randomness in nature. He seems to describe a unified and interlinked universe that has no room for independent random events. There is a lot of randomness in how our brains actually function, and this can best be captured by the observations of Ebbinghaus in 1885.

http://en.wikipedia.org/wiki/Forgetting_curve

50. **kuhn dog**
October 19, 2008

It is said above (by Dave): "…to claim to have answers that you do not have, as Hagelin does, is dishonest, even if it is the sort of dishonesty that consists of deceiving oneself."

Many of the posters on this blog make this heavy judgment and denouncement of Hagelin, but based on what? How do you actually know that he is wrong? (Really, how would you KNOW this? On what epistemological grounds?) Granted, it may be your personal belief, your knee-jerk reaction. But, what answers does Hagelin claim to possess that are counter to empirical evidence, or even counter to current physical theory? And specifically, what claims does Hagelin make that are not supported by empiricle evidence or physical theory? (not counting the theory in neuroscience that consciousness is an epiphenomenon of the brain, which, as mentioned above, is a non-theory anyway…)

Hagelin's fundamental assertions seem to be
1. that there is a unified field (a claim made by many physicists)
2. that the u.f. is, in essence, a field of consciousness (perhaps a surprise to many, but, not without precedence in Eastern and Western thought)
3. that the u.f. as a field of consciousness can be subjectively experienced and thereby validated. (again, not without extensive scholarly precedence…)
4. that large groups of people experiencing the u.f. together at the same time (i.e., "group practice of the TM and TM-sidhi programs") influences the environment in a measurable way through the "field effect" of consciousness…yes, spooky action at a distance (which seems to be something that physicists are no longer so spooked by…yes?).

This fourth point seems to be the crux of the matter, as far as empirical testing of Hagelin's theory. (Not that subjective experience of the u.f. is without merit—the Upanishads, Gita, Yoga Sutras, etc. uphold the necessity of directly experiencing the unified state of consciousness in order to truly KNOW the underlying reality.)

Can group meditation really influence the environment? Is there a level of life where we're all interconnected?

I remember reading years ago in Yale Journal of Conflict Resolution, which published one of the research studies on what Hagelin calls "the Maharishi Effect"—the proposed "influence of coherence" that results from large group meditation. (I know, some of you are wondering why such a topic as this is even being discussed on this blog—how "out there" can one get? But, please, bear with me..) The editors interjected a caveat, as if to justify why they, a highly reputable sociological journal, would publish a study on something so outside the box. I paraphrase the journal's editors: "While we consider this research study on the extended sociological effects of meditation to be worthy of publishing because of it is solid research with sound statistical analysis, we must also acknowledge that the theoretical explanations of the findings are well outside the current paradigm of current sociological research."

My thoughts were, no kidding. It's BECAUSE the research is outside the current paradigm that Hagelin's group may be on to something—perhaps they've found a far better way of improving society than anything we're currently doing.

If Dave or any of you haven't actually looked at the research, read through the studies, duly considered the theory that Hagelin is proposing, and determined scientifically whether or not any of the 50+ research studies on the Maharishi Effect are valid science, if you haven't considered the design and controlls of the experiments and looked closely enough to discern whether or not the data supports the findings, then how can you dismiss the theory out of hand? Just because it's "outside the current paradigm of sociological research"?

My man Thomas Kuhn has a word for those who refuse to examine the data and cling to their old paradigm, despite overwhelming evidence: NON SCIENTISTS. "They have ipso facto ceased to be scientists." —TK.

In all fairness, maybe some of you didn't even know there was empirical evidence to support Hagelin's approach. I've actually read through most of these research studies (there are dozens of peer-reviewed studies on the Maharishi Effect). The data and findings seem to strongly support the theory that group mediation can influence crime, violence, and yes, even the stock market. There may be people on this site more qualified than me to ascertain statistical significance; if so, perhaps you should examine the studies. People often say, why take the time, the theory is so outside the box… Why bother? Because everything else has failed, and these peer-reviewed studies support the theory. Because if it's true, it would alleviate much suffering.

Instead of ridiculing Hagelin, perhaps consider the possibility that this is all he's is trying to do—make our world a better place. Have we become too cynical to listen to such a voice of hope? Not all of us have. I'm not talking about gullibility, but mere open-mindedness. Rake it across the burning coals of scientific scrutiny, but first give it a chance.

Patanjali said thousands of years ago: "In the vicinity of yoga (unified awareness) hostile tendencies disappear."

Maybe he was right. There are many ancient texts, passed down from the Vedic rishis (scientists of consciousness. perhaps?) that record experiences of a fundamental, unified field at the basis of nature's functioning. They called it Atma, Brahman, Samadhi, and had many precise technical terms for various levels of the experience. But the experience is universal and is recorded in every culture.

And I'd like to hear from Vishal: what specifically was overthrown by Lord Buddha, as far as the fundamental principals of Vedic knowledge? I don't mean, what were the points of misinterpretation that Buddha debunked, but what actual expressions of Vedic texts did Buddha prove (didactically or through other means) to be invalid?

Just because fundamentalist Hindu nationalists have appropriated certain Vedic teachings does not invalidate the fundamental principles of Vedic knowledge that pertain to the development of consciousness. The Vedic tradition of knowledge and what today is known as Hinduism are two different things. The Vedic tradition and it's methodologies predates the Hindu religion by several thousand years. But this is another discussion that would probably take more explaining than people here have patience for, at the moment.

51. **woit**
October 19, 2008

All:

1. Please stop posting off-topic comments

2. Please stop posting content-free comments that do little except tell us what your reaction is to someone else's comment. If you're not transmitting some useful information or original idea not otherwise readily available, you're probably just adding to the noise level here. It doesn't take much of this to make a comment section useless, and such that only people who have nothing interesting to say will participate in it.

52. **Gil**
October 20, 2008

The idea that putting together a group of meditators can make things better for the entire society, and even the impressive formula of a squaroot of one percent of the population for the number of required meditators is quite old by now.

Interestingly, this have led to a paper that was published in a peer-reviewed scientific journal which describes the spectacular success of an experiment that took place, from all places, in the city of Jerusalem.

The first author of the paper have written many many papers on other rather spectacular (but not as much as this one) good effects of Maharishi meditation.

ORME-JOHNSON, D. W.; ALEXANDER, C. N.; DAVIES, J. L.; CHANDLER, H. M.; and LARIMORE, W. E. International peace project in the Middle East: The effect of the Maharishi Technology of the Unified Field. Journal of Conflict Resolution , 32(4): 776-812, 1988.

A rather small group of meditators seemed to have achieved: "Improved Quality of National Life as Measured by Composite Indices Comprising Data on War Intensity in Lebanon, Newspaper Content Analysis of Israeli National Mood, Tel Aviv Stock Index, Automobile Accident Rate in Jerusalem, Number of Fires in Jerusalem, and Maximum Temperature in Jerusalem; Significant Improvement in Each Variable in the Index (Israel, 1983). Decreased War Deaths (Lebanon, 1983)."

The strong correspondence between the number of Transcendental Meditation-Sidhi program participants in the group in Jerusalem and a composite index of all the variables above can only be described as amazing. The graph can be found here: http://tm.org/charts/chart_51.html.

53. **LeaderWB**
October 20, 2008

Dr. Hagelin is not the only Quantum Physicist who upholds the belief of Consciousness-based Evolution for our species . . .
did U somehow miss 'What-the-Bleep-do-we-know' ?

If so, here's an easy link:
http://www.amazon.com/What-Bleep-QUANTUM-Three-Disc-Special/dp/B000FKO3JO/ref=pd_bbs_sr_1?ie=UTF8&s=dvd&qid=1224530775&sr=8-1

54. **UF**
October 20, 2008

There are alternatives:

http://www.globalorgasm.org/

This also influences the unified field and the zero point energy to achieve a better word.

Please, consider contributing. It's much more fun than meditation.
You can even contribute alone if you lack a partner (or partners).

The world's financial system depends on your orgasm.

55. **somebody**
October 20, 2008

"The strong correspondence between the number of Transcendental Meditation-Sidhi program participants in the group in Jerusalem and a composite index of all the variables above can only be described as amazing."

Or that they want to sell something. I hope you are kidding, because the index (if at all it was not just made up) is certainly rigged. If you scale/shift/rotate a generic sigmoid or some such curve, you can pretty much find reasonable fits for anything. The real index will only look like noise.

James Randi has gone after and done background checks on some of the claims made by TMers and debunked it. I remember in particular some claims about crime rates in a small town in Iowa where apparently a huge percentage of the population was TM practitioners. All lies.

I am willing to give the benefit of the doubt to vedic-medicine from the far east or some such. Who knows, maybe there is some empirical validity for them, even if we don't understand the details of why some of it works. But TM for world peace? Gimme a break.

56. **David H. Miller**
October 20, 2008

Kuhn Dog wrote specifically to me:
>Many of the posters on this blog make this heavy judgment and denouncement of Hagelin, but based on what? How do you actually know that he is wrong? (Really, how would you KNOW this? On what epistemological grounds?)
[snip]
>Hagelin's fundamental assertions seem to be
>1. that there is a unified field (a claim made by many physicists)
>2. that the u.f. is, in essence, a field of consciousness (perhaps a surprise to many, but, not without precedence in Eastern and Western thought)
>3. that the u.f. as a field of consciousness can be subjectively experienced and thereby validated. (again, not without extensive scholarly precedence…)

Let me be blunt: the "scholarly precedence" and the "precedence in Eastern and Western thought" do not matter, not at all. Evidence matters, "precedence" does not.

Honest, intelligent people do not bow down before "precedence." They bow down before evidence and logical reasoning.

If Hagelin merely suggested this nonsense as vague speculation, well, fine. Consciousness does exist – I, at least, am conscious (though "Covariant" claims he is not, and I accept him at his word). Consciousness has to be explained by something, and, for all any of us know, perhaps it will turn out to be superstrings.

But Hagelin does not merely suggest this stuff as vague speculation. He claims to actually know, based on his position as a physicist, that, in his words:
>"…consciousness isn't created by the brain…we call it the unified field…this single universal field of intelligence…or superstring field…"

These are not facts that are now established by physics. Hagelin does not in fact know any of this.

To steal the phrase that Peter borrowed from Pauli, this stuff is "not even wrong." It is so vague that it is hard to see how one could ever test or refute it. But Hagelin throws in enough buzzwords from real physics to make it seem plausible to those who are ignorant of science.

No, I doubt Hagelin is lying in the simple sense: he has probably succeeded in bamboozling himself.

And, I certainly have no desire to question Hagelin's or anyone's right to ask the sort of questions with which he opens the YouTube clip: e.g., what is consciousness?

Questions are good. Pretending to have answers when you do not have answers, and using your authority as a physicist to bamboozle unsuspecting members of the public, is not good.

Dave Miller

57. **David H. Miller**
October 20, 2008

Jeff McGowan wrote:
>why do so many scientists seem to think it is inevitable that scientists will one day "explain" consciousness. I have no doubt that consciousness is a physical phenomenon, but that doesn't mean that anyone is ever going to explain it (whatever exactly that might mean).

We scientists tend to be optimists about the future of science, and the progress of science in the last five centuries has, so far, confirmed our optimism.

But you might be right. The philosopher Colin McGinn, who is much better informed scientifically than most philosophers, agrees with you: see his book "The Mysterious Flame."

Time will tell.

Dave Miller

58. **capitalistimperialistpig**
October 21, 2008

At the serious risk of being both off-topic and lowering the S/N ratio, how does one comment on-topic on a post like this?

Perhaps I could say that it has always been my ambition to do formation Yogic flying against global financial panic, but I have had a bit of trouble finding like minded people who can actually get off the ground.

Any advice?

59. **Gil**
October 21, 2008

The claims about the effect of Yogic flying on world peace and on the stock market and the paper describing a scientific experiment supporting these claims have the value that they can improve our evaluation of other scientific experiments described by Maharishi scientists on remarkable effects of meditation which are a priori not so absurd.

60. **Jeff McGowan**
October 21, 2008

Dave,

I'm a mathematician, I think that probably makes me one of "we scientists." 🙂

I tend to go with Stephen Jay Gould on the whole progress thing…

Peace,
Jeff

PS Thanks for the reference, I'll check it out.

61. **anon.**
October 21, 2008

Has Dr Hagelin used superstring theory to formulate a scientific theory of how Yogic flying can reduce the turbulence in the collective consciousness of one country, thereby producing economic harmony?

Is the turbulent group consciousness something that is manifested in spatial extra dimensions, and if so, would the success of Dr Hagelin's approach to achieving world economic harmony by Yogic flying be a good indicator that superstring theory is correct and deserving of yet more funding?

Peter, it's refreshing that you are taking Dr Hagelin's claims seriously in this post, instead of your usual hostility and ridicule. Presumably it's because of your personal contact with him, but it's a step in the right direction. Have you tried Yogic flying yourself? 😉

62. **Amos**
October 21, 2008

The sad thing is, the rate of progress in physics is so low right now that entries like this are able to draw in 60+ responses in only a few days. It would be better if physicists were producing more interesting new results so that Peter could post them and we could talking about them instead.

63. **somebody**
October 21, 2008

"The sad thing is, the rate of progress in physics is so low right now that entries like this are able to draw in 60+ responses in only a few days. It would be better if physicists were producing more interesting new results so that Peter could post them and we could talking about them instead."

I somehow suspect that the reason why a topic like this draws attention has nothing to do with "rate of progress in physics". A post about gay marriages, or God, or Obama also would draw a lot of attention. Mind you, I am not suggesting there is anything a-priori wrong with those topics, some of them in fact are very important. But popular vote usually goes for topics in which we can all have strong opinions.

E.g., the possible application of black holes to the understanding of superconductivity is a very interesting development that happened during the last couple of years, but it suffers from the problem that it is not easy to have strong opinions about it without being well-informed.

I suspect that if Peter were to write about technical topics, contrary to (the spitit of) what you suggest, the traffic here would be less, not more. Even during the period when the Standard Model was being invented, I suspect that the blog entries would have to be about flying yogis, not anomaly-free matter content.

64. **Peter Woit**
October 21, 2008

I agree that the number of comments on a posting correlates not at all with whether there's something scientifically significant under discussion. So, that has nothing to do with the pace of progress in physics.

That said, I am finding it remarkable how little is going on these days. It's not just my blog, but the others I follow seem to have very little to say about new physics. I'm busy with other things, so haven't written about a few things I could have discussed, but still.

As for claims of applications of black holes to condensed matter physics or nuclear physics, my philosophy is that the people to report on those and evaluate their significance should be condensed matter physicists and nuclear physicists.

65. **Dave Miller**
October 21, 2008

Jeff,

In my experience, mathematicians actually tend to have a substantially different perspective and mindset than natural scientists. I'm not being critical: I think the average mathematician I've known probably had a higher IQ than the average theoretical physicist I've known (admittedly, this is partly because of a handful of physicists who pulled down the physics mean IQ quite a lot!), and, of course, we physicists certainly need you mathematicians.

And, computer scientists seem to have a different perspective than either mathematicians or physicists.

Anyway, I think you and I and Peter and most of the regular readers of this blog agree on the central point: it is not possible to accurately predict the future of science, and people who think they can do so are fooling themselves. I'm sure that progress in neural science holds lots of surprises for all of us, which, of course, is a big part of the fun of science.

(Somehow, though, I don't think that successful yogic flying will be one of those surprises.)

Dave

66. **Dave Miller**
October 21, 2008

Gil,

You wrote:
> The claims about the effect of Yogic flying…[snip] … have the value that they can improve our evaluation of other scientific experiments described by Maharishi scientists…

Yeah, but perhaps not in quite the sense you mean.

Everyone has been bending over backwards to be polite, following the example of our gentle host.

But… a TM Website (http://www.permanentpeace.org/technology/yogic_flying.html) claims:
> During the first stage of Yogic Flying, while the practitioner sits in the cross-legged lotus position, the body lifts up and moves forward in short jumps. One branch of the Vedic literature, the Yoga Sutras of Maharishi Patanjali, describes this first stage as "hopping," and further defines a second stage as hovering for a short time, and a third as complete mastery of the sky.

Look, everyone knows that no human has ever gotten beyond the "first stage," i.e., the funny little lotus hops. The whole thing is a scam. No human has ever reached stage two or stage three.

No one ever will.

I'm being too cynical?

Fine. Let's have the TM folks get their half dozen best Yogic flyers, and, at a well-known public place announced well in advance, say Central Park or the Capitol Mall, demonstrate second or third stage Yogic flying.

If they choose a New York venue, maybe Peter can even be induced to take some video to convince all us skeptics.

All they need do is hover in the air for a couple minutes – no, even thirty seconds would do – continuously of course, and all of us skeptics will bow our heads in shame.

But we all know that this will never happen.

Gil, they cannot really fly.

To be skeptical of Hagelin's claims, you do not need to be a dogmatic materialist (I'm not), you do not need to have an opinion on the ultimate nature of consciousness, superstring theory, or anything of the sort.

You need merely note that Hagelin states:
>At this critical time I urge everyone to fly together in large groups…

And then remind yourself – they cannot really fly.

Dave

P.S. Peter, I know the game of this post is to leave it all implicit and let everyone figure it out for himself, just like a good joke – something is lost when you have to explain why the joke is funny. But sometimes, some things ultimately have to be said explicitly.

67. **Peter Woit**
October 21, 2008

Dave,

Hey, I've seen still photos of these people flying. Of course in the photos their hair was kind of standing up, like maybe they were coming down. Fast.

68. **somebody**
October 21, 2008

Peter: “That said, I am finding it remarkable how little is going on these days.”

The farther zoomed out you are, the less impressive the changes are. Anything short of an actual warp drive is not going to impress a layman, for example. To understand progress you need to understand what are the interesting questions and why. This usually requires getting your hands dirty and wading into the details. If you are looking for applications to agriculture, you will not be impressed, I agree.

Strominger and collaborators recently found a way to compute the entropy of astrophysical black holes. Is this exciting? To be honest, it is not THAT big a deal to the string theorists because we always knew astrophysical black holes are not that different from the idealized (or “unphysical” as the haters call it) black holes. But you should be impressed, because this is a real black hole, one that you can actually see in the sky.

But on the other hand, why would you be? Even Hawking radiation is afterall experimentally unverifiable. As I have said before (and you have denied), you care nothing for understanding, but only for “experiment”. Since experiments are hard, you will always have your moral superiority.

Last couple of years – holographic superconductors were interesting, looking at turbulence through Einstein’s equations was interesting, integrability of gauge theory was interesting, membrane theories were interesting, realistic and more precise black hole entropy computations were interesting. WHY were they interesting? Because they gave us understanding about various things, some of which (relating to condensed matter and nuclear physics) are even accessible to semi-quantitative predictions. But are they ALL immediately going to be seen at the LHC? No.

My point is that when you make the statement that hardly anything is going on, you are in fact passing a judgement, and not stating a fact. This is something that the average reader of this blog will fail to recognize.

Peter: “As for claims of applications of black holes to condensed matter physics or nuclear physics, my philosophy is that the people to report on those and evaluate their significance should be condensed matter physicists and nuclear physicists.”

I am sure you have seen Bill Zajc’s comments at backreaction (if not, google!). That there are at least SOME nuclear physicists who are impressed by black hole predictions and calculationss is not too bad for a theory about the Planck scale.

69. **somebody**
October 21, 2008

“Strominger and collaborators recently found a way to compute the entropy of astrophysical black holes.”

I should be more precise and say fast-spinning astrophysical black holes.

70. **Peter Woit**
October 21, 2008

Somebody,

Thanks for the snide anonymous insults. I don’t know who you are, but I do have some idea what the “interesting questions” in fundamental physics are and have spent a long career “getting my hands dirty” trying to understand them. I just happen to be getting my hands dirty in places that are not so popular. I’ve also spent a lot of time trying to understand and learn from what others are doing, and make a point of keeping my mouth shut about things I don’t understand.

String theorists on the other hand, seem to revel in overhyping anything even remotely related to string theory. I’m well aware of the often-pointed to Zajc comments; you might also want to Google “Pinocchio award” and Larry McLerran for another take on this. One thing I have noticed about the string theorists who go on about this is that few if any of them seem to know anything about nuclear physics.

The way you misleadingly characterize the recent Strominger results is just typical. String theorists can’t just accurately explain exactly what some result is, instead they have to overhype it (“Strominger and collaborators recently found a way to compute the entropy of astrophysical black holes”!). You should realize that the whole field now has a huge credibility problem because of this behavior.

There is objective data available about the “no progress” claim from citation counts, and I’ve gone over this elsewhere.

71. **Peter Woit**
October 21, 2008

As for the reasons for the “no progress” problem, there are several. One is that the remaining problems are hard. Another is that a large and influential component of the community refuses to admit there is a problem, instead attacking the professional competence of anyone who brings up the topic.

72. **Jeff McGowan**
October 21, 2008

Dave,

I agree we have a different perspective. My old undergraduate advisor has for years taught a course called “Math and the Other Arts” and there is something artistic about many mathematicians approach (to everything). Could just be that I’m married to an artist, and off the top of my head I can think of at least five other mathematicians among my close acquaintances who are also married to artists 🙂 I didn’t take it as a criticism. I can pass you the names of some mathematicians who aren’t the sharpest tools in the shed if you want to even things up…

Also, I agree completely about predicting the future of science. And I have a lot of fun as an interested outsider reading Peter’s blog. Can’t quite believe the vitriol sometimes, you get much less of that in math, unless I’m just completely out of that loop. Oh, and as a practitioner of yoga, gotta say the flying bit is really funny. Actual yoga on the other hand is great.

73. **somebody**
October 22, 2008

I will try to stay close to the science and try to avoid getting into the vitriol.

Peter says: “I’m well aware of the often-pointed to Zajc comments; you might also want to Google “Pinocchio award” and Larry McLerran for another take on this.”

McLerran’s papers speak differently from his public talks. It always gets laughter to say a few words about string theorists and their 26 or 10 dimensions in talks. I do that too. But in his PRL paper he says (and I quote):

An amazing theoretical discovery was made by Kovtun, Son and Starinets [9], who showed that certain special field theories, special in the sense that they are dual to black branes in higher space-time dimensions, have the ratio η/s = 1/4pi (we use units with h = kB = c = 1) where η is the shear viscosity and s is the entropy density.

But I am sure there is perhaps another quote somewhere where he again bashes string theory, because it is no secret that McLerran is not a fan. So instead of hunting for it, I would also look at the specific comments made by Zajc about the science behind McLerran's Pinocchio remarks. We have alrready established that string theory has many enemies, so McLerran's like or dislike hardly adds anything new.

The crucial point is NOT that string theory has no enemies (ha-ha!), but that there are some qcd/nuclear physics types who are into strings these days. In particular, it is incorrect to imply that Zajc stands alone. Rajagopal, Weidemann,… have even published many papers on this.

Now lets turn to your comments about the Strominger computation:
" The way you misleadingly characterize the recent Strominger results is just typical. String theorists can't just accurately explain exactly what some result is, instead they have to overhype it ("Strominger and collaborators recently found a way to compute the entropy of astrophysical black holes"!). You should realize that the whole field now has a huge credibility problem because of this behavior."

You make an accusation that I am overhyping Strominger, while NOT making any statements about exactly what he does. I will chew my arm off and show restraint.

Two facts:

1. What Strominger does is to compute the central charge of the Virasoro algebra and the temperature of the dual CFT of an extremal four dimensional uncharged Kerr black hole. With these two you can compute the entropy using the Cardy relation.

2. There are ultra-spinning black holes in the sky. Astronomers have even identified them and given them names.

I will let you draw your own conclusions from these.

74. **nbutsomebody**
October 22, 2008

somebody said,
1. What Strominger does is to compute the central charge of the Virasoro algebra and the temperature of the dual CFT of an extremal four dimensional uncharged Kerr black hole. With these two you can compute the entropy using the Cardy relation.said,

Well, this is indeed a hyped way to present what he has done. what he has really done is to extend a little already known property of a AdS_3 and its various squashed cousins. It is a pure gravity calculation and there are simple geometric reasons why so called "central charge" is related to the area of the black hole horizon.
It is indeed suggestive of a holographic interpretation. However this was more or less known and there is no microscopic description.

75. **Archimedes**
October 22, 2008

Coming back to the topic… Is any of the Maharishi followers who happen to be reading this thread able to point out any publication where an experiment concerning the Maharishi effect is described in such a way that would allow one to reproduce it by independent means?

This may be a bit unfair a request, since social sciences are not "hard sciences" in the sense of physics, whose experiments can be reproduced by someone else in another lab, or another time. But it doesn't hurt to ask, so if someone can point out such an experiment I will be grateful.

If experiments are not of that type, as I expect they aren't (I haven't gone through any of the papers already mentioned in this thread) then the only way to check the validity of the conclusions is to really look at the papers carefully, perhaps with the help of someone from a statistics department or such.

Even so, doubts will remain about the validity of the experimental data to begin with. I am not a sceptic, in fact I'd be delighted if the Maharishi effect does exist. But I feel the need to be reassured that the Maharishi effect is not a corollary of the "Sokal effect"…

76. **Kevin Dailey**
October 22, 2008

The time has come when the science of physics has come to the frontier of the non-physical. It is very inspiring to see such a renowned physicist taking the leap into the spiritual. Whether we like it or not the effect of consciousness on matter is real and now in this modern age it is being scientifically explored and validated.

http://www.mum.edu/m_effect/

New frontiers are always scoffed at, doubted, made fun of and then finally widely accepted. You can be ahead of the crowd or follow behind. The choice is ours.

77. **anonymous**
October 22, 2008

Archimedes,

"allow one to reproduce it by independent means"

Presumably one would have to replicate the extensive training these people have in meditation, etc. (http://www.permanentpeace.org/technology/tm.html) This would be difficult and time-consuming to replicate from scratch.

Regarding the initial post: Reality of the effect (or not) aside, it boggles my mind that nobody questions whether doing this is ethical. If it is a real effect, why do we assume that it's a GOOD thing and that Hagelin SHOULD inflict it upon us?

78. **John Gonsowski**
October 23, 2008

The idea of a place where both physics and esoteric ideas can be studied is not a bad idea though one can certainly do it incorrectly or be too one sided.

Discussed in the link below are Plato's Academy, Gurdjieff's Institute for the Harmonious Development of Man, Maharishi International University, Perimeter Institute, and the authors' Quantum Future Institute.

http://quantumfuture.net/quantum_future/institute.htm

It’s not favorable to the Maharishi University.

79. **Tim May**
October 23, 2008

Anonymous writes:

“Presumably one would have to replicate the extensive training these people have in meditation, etc. (http://www.permanentpeace.org/technology/tm.html) This would be difficult and time-consuming to replicate from scratch.’

I’ll be happy to _personally_ fund the second round of experiments, with completely unaffiliated researchers trained at Maharishi University, AFTER at least the first round of experiments–involving video cameras manned by independent observers, checks for wires by independent observers, and other very basic norms of experimental science have given strong weight to “yogic flying.”

Saying that a bit of hearsay about yogic flight cannot be discounted until we have trained independent observers in exactly the same ways is not the way science works.

Early in my career, I discovered an unexpected effect (bits in computer chips flipped by alpha particles from the surrounding package, and at lower levels from cosmic rays). I presented experimental data, proposed mechanisms, and so on. Others followed on the work, presenting their own “independent” experiments.

There was never any suggestion that my work could only be confirmed or invalidated after others had “replicated” my training, etc. Granted, the “yogic flight” is supposed to be based on training, mental state, etc.

But the “reproducing the training” can and should come AFTER apparent “flight” (not ambiguous little “hops” of a few inches) has been established. A yogi flying in through a window of a conference hall and hovering 5 feet above the podium for a few minutes ought to do the trick.

Perhaps Peter will consider this off-topic, but I don’t. Experimental confirmation and “refutation of theories” is not nearly as subjective as some claim. Nearly all physics results in the 1850-1950 period could be duplicated or refuted by many around the world…and they were. After the era of Big Science, of course, it got a lot more expensive to do so. But even in the Age of Accelerators, competing labs can and do try to confirm what other labs are reporting. Further, even in the coming age of the One Big Accelerator, I expect enough internal competition between groups will provide for checks and balances on results claimed.

“Yogic flight” would be vastly more significant that the Higgs Boson, don’t you think? And yet people are debating some phony-looking Polaroids of guys who like they’re being dropped from some height into their (OUCH!) lotus position.

This is, as others here have said, “not even wrong.” Actually, it’s more than not even wrong. (Though a part of me would love to see evidence for yogic flying, or UFOs, etc., strong claims require strong evidence.)

Speaking for myself, not Peter or anyone else, this is not even in the same _game_ as string theory is. Rightly or wrongly, string theory is not yogic flying, crystal healing, or psychokinesis.

(As the joke goes, “All those in favor of psychokinesis, raise my hand.”)

–Tim May

80. **Gil**
October 23, 2008

The claims that the groups of meditators improve matters regarding the stock market or world peace and the statistically-based “scientific experiments” supporting them are nonsense. But these absurd claims raise the concern that the hundreds of papers by (the same) Maharishi scientists regarding more believable medical effects of meditation are bogus as well.

Trying to figure out if something is wrong and what it is in empirical experiments like those describing some wonderful effects of meditation is not an easy task. When the outcome of an experiment matches the desires of the experimenters but look otherwise suspicious or even absurd, a simple possible explanation is that the outcomes were “tuned” or “rigged”. Useful statistically-based methodologies that will allow to examine tunning effects are not easy to come by.

81. **Covariant**
October 24, 2008

OK

I was trying to be open minded about all this, and I confess I thought “yogic flying” was some sort of metaphor. Here is a link to a film where they demonstrate “stage one yogic flying”

http://www.youtube.com/watch?v=NHwhGUo90jw

Hagelin explains why Newton’s Laws are false. What a hoot!

82. **Thingumbob**
October 24, 2008

Here is the type of empirically verifiable study conducted at MIT which should serve as a model to test the claims of “yogic flying” :
http://zapatopi.net/afdb/

83. **anonymous**
October 27, 2008

Tim May – Do you really think this is about flying? Actual FLYING? I thought the flying was either 1) a cute (and rare) by-product of the effect, or 2) some kind of metaphor for the target state of mind.

Either way, I doubt that actually physically leaving the ground is necessary to achieve the purported aims of this project. It concerns me more that someone claims to have found a force that can pacify the ‘collective consciousness’ and is willing and able to use it, unrestrained by any ethical considerations, while people sit around arguing about gravity. (Here, watch my right hand while my left hand picks your pocket.)

Forget the flying. Assume it’s a woo-woo diversion to ensure that no one pays close attention to what is really going on. Since when do scientists deploy the results of their research arbitrarily upon mankind without obtaining any kind of consent? Sure, it sounds like a good plan – less fear in the collective consciousness = more stable markets

= good economic times for us all – but if ‘less fear’ came in the form of a pill, you would question the person who is telling you ‘trust me, it’s for your own good’. You wouldn’t sit around arguing about the placebo effect.

Even if you don’t think that the Maharishi effects are real (and I don’t mean the flying), Hagelin does, and he appears to direct their deployment according to an unvetted agenda.

84. **Vishal**
October 27, 2008

Here’s how yogis actually have been levitating in India all along: http://www.youtube.com/watch?v=etSivpBHUmE

I couldn’t believe my ears when I heard Hagelin’s interpretation of Newton’s law of gravitation in the YouTube video provided by Covariant. Unbelievable! The man is clearly duping people.

85. **Dave Miller**
October 28, 2008

John Gonsowski wrote:
>The idea of a place where both physics and esoteric ideas can be studied is not a bad idea…

No, John, it is a very bad idea, and for a very simple reason: “esoteric ideas” – all esoteric ideas – are nonsense, plain and simple.

I’m not claiming this as an a priori truth, but simply as an empirical fact proven by millennia of experience. We’ve had a very long time to check out all of this “esoteric” nonsense, and, again, to use the phrase Peter borrowed from Pauli, it has proven sometimes to be clearly false, but more often to be “not even wrong.”

I know that numerous people, sometimes even technically trained people, have claimed to acquire deep knowledge into the nature of the universe through esoteric studies, meditation, etc.

But when you probe the details of this deep knowledge, it boils down, at best, to saying that the universe is a really impressive place.

Indeed it is. But I realized that when I managed to understand Bondi’s “Relativity and Common Sense” as a kid. No meditation or esoterica required.

Meditation no doubt helps some people relax and cool down. Great. Singing old Simon and Garfunkel tunes in the shower helps me relax. Whatever.

But, the only road to knowledge ever discovered by human beings is the scientific method broadly construed: tie your ideas down to concrete, easily observed and easily checked evidence; make your conjectures sufficiently clear and explicit that, if they are wrong, they can be refuted; look for little “insignificant” details (such as the anomalous behavior of Mercury’s orbit) that suggest shortcomings in your ideas; assume you may have made mistakes in your reasoning or observations and cross-check them with other humans; etc.

Or, in simple terms, as Feynman put it, try really hard not to fool yourself.

The methods pursued by a mathematician, an experimental physicist, or a serious historian all follow that broad approach, even though their methods differ greatly in detail as fits their different fields of investigation.

“Esoteric ideas” are designed to violate all of the basic canons of the scientific method.

Esoteric ideas are nonsense.

Dave Miller in Sacramento

86. **Archimedes**
October 31, 2008

As regards scientific studies on meditation, the following newspaper article (dated 2005) provides a rough description:

http://www.npr.org/templates/story/story.php?storyId=5008565

The science here is neuroscience, and meditation is mostly buddhist. As far as I know the kind of research alluded to is interesting and serious. It also seems to be the state of the art in the field, and it is concerned with studying what happens in the brain of those who meditate, both in the short and the long run. This looks like a pragmatic and justified research program. What is more, all along neuroscience is still science, and buddhism is still religion/philosophy, no matter how suggestive the links between the two sides may be.

However, the Maharishi type TM, yogic flying and the like (even if the latter is taken only in a metaphoric sense) seem to be a completely different story, namely one whose claims, especially those that get all dressed up in the colors of deep fundamental physics, seem to be non-scientific assertions that mix up science, ancient philosophy, conjecture, intuition, faith and gut-feeling into an inextricable mess. Incidentally, looking at the MUM web pages I also came across something on “vedic mathematics” which seems to suggest that pure math is being garbled in just the same way.

87. **Zeuxis**
November 13, 2008

Joe the Physics Student asked, “How does this individual’s activities [sic] have anything to do with current physics research? Thanks.”

It doesn’t, but it’s being perpetrated as if it did. That’s the point. (To expose hokum.)

Thanks.

Job Posting

October 25, 2008
Categories: Uncategorized

This is the time of year during which a large number of physicists and mathematicians must turn their attention to the problem of finding employment for next fall. Physics Today has a jobs site here, which has a new posting that may interest some of my readers. Thanks to Michael Williams for pointing this out to me.

If applicants need a PD18 form, it's available here.

Comments

1. **Show some class!**
   October 25, 2008

   Newton, Hawking were no Fields medalist or Nobel laureates (for Physics). Certainly they are going to change that? 😉

2. **chickenbreeder**
   October 26, 2008

   Thanks for the tip. I will apply.

   OK seriously, Hawking's predecessor James Lighthill was also neither a Fields Medal winner nor a Nobel laureate. Actually, he was a fluid dynamicist whose achievement would not qualify him for either. But he was a dominant figure in the applied mathematics circle in Britain. So, don't be surprised if someone comes out of left field to take the job. (Lighthill was not a left-field choice. He was highly regarded and was recognized as exceedingly bright. His work on aerodynamics is world class. Only that aerodynamics is not a particularly fancy subject.)

   Another interesting trait is that although it's called Lucasian Professorship "in Mathematics", the Math here is almost always "applied" math (which means theoretical physics, geophysics, fluid dynamics, even meteorology, in British terms). This would exclude prominent British pure mathematicians like Andrew Wiles or Michael Atiyah, who would otherwise be big enough to fill the chair.

3. **FP**
   October 26, 2008

   To "show some class",
   Dirac was Physics nobel winners and Lucasian professor.

4. **curious**
   October 26, 2008

   What's happening with the current occupant?

5. **Peter Shor**
   October 26, 2008

   According to Wikipedia, the current occupant is planning to step down, and become Lucasian Professor emeritus. I hadn't realized he was that old.

6. **Kea**
   October 26, 2008

   Excellent! I think I will apply.

7. **MathPhys**
   October 26, 2008

   He will be 67 soon. I thought he already has a position in the US.

8. **Peter Woit**
   October 26, 2008

   MathPhys,

   There were rumors that Hawking might move to Perimeter on his retirement, I hadn't heard any rumors about the US.

9. **chickenbreeder**
   October 26, 2008

   Sixty-seven is the mendatory retirement age at Cambridge (and all Britain?) that's why Hawking is stepping down. Dirac also retired from Lucasian chair at 67, then spent the rest of his life in Florida as we know.

   Speaking of which, both Dirac and Hawking were appointed the chair at a very young age, in their 30's, and held the position for a long time. Lighthill occupied the chair from his 40's to 50's before moving to other more administrative positions. The limited samples seem to indicate that the ideal candidate is someone who is relatively young but already a dominant figure.

10. **Jimbo**
    October 26, 2008

    ProtoBrits for Lucasian Immortality:
    Julian Barbour, Michael Green, Neil Turok, Michael Berry, Mike Duff,
    David Deutsch, Roy Maartens, to name a few.

Now the real question: Who is stuffy enough to fit in at Cambridge ?

11. **Chris Oakley**
October 28, 2008

I just got the forms.

There is a section on disabilities (which worried me at first as I am pretty much able-bodied), but upon closer inspection, it turns out that this is not a requirement.

As I liked the idea of being the Lucasian Professor of Mathematics at Cambridge University, I was thinking of applying. I was about to e-mail Lubos to see if he would be prepared to give me an academic reference when I decided against it.

Firstly, I did not fancy the heavy teaching load (40 hours per year).

Secondly, the remuneration £60-124K (US $95-200K) – is less than a contract programmer earns in (what is left of) the finance world.

Well … maybe next time. Or probably not, as it seems likely that I will be over 67 when the appointee retires.

12. **Mitch Miller**
October 28, 2008

Are there any rumors for the Lucasian professorship? I can't imagine there being that many qualified people who are also interested in changing jobs.

13. **David McMahon**
October 29, 2008

Figures Britain has a mandatory retirement law. People should require when they are no longer productive or when they want to retire, not when some socialist government says they must. Not everyone is the same, some people are still very productive at 67 and beyond.

14. **chickenbreeder**
October 29, 2008

David – Mandatory retirement in academia is not a bad idea. Retired scientists can still freely pursue their research. I am sure Hawking will. In the U.S., emeritus professors often keep an office and are eligible to solicite external funding just like regular faculty. The difference is, their retirement frees up new positions for young people, easing traffic jam at bottom. This is critically important considering that, in certain fields, the total number of permanent positions have remains steady for a long time.

The mandatory retirement age is 60 at University of Tokyo. In some other Japanese Universities it's 62 or 63. People over there seem to have no complaint with that.

15. **Covariant**
October 30, 2008

Peter,

If you weren't in your current position would you consider applying?

I wanted to add that I picked up "Not Even Wrong" again. I read something this morning that made me think that certain fields of physics are moving more and more into the absurd.

For some reason, there was something about it that made some of your observations begin to resonate.

I won't go so far as to question all of string theory, but I have the feeling that we have exhausted the legitimate predictive power of QM and GR and the desire for expediency is leading to a cascade of abominations.

I think their might be something to this Peter Woit character afterall.

16. **Peter Woit**
October 30, 2008

Covariant,

No, I'm not qualified for that position. Like many a researcher, in my moments of extreme fantasy, I think that maybe the ideas I am working on now will lead to a huge success, one that might make me qualified for such a position. At the moment, that is pure fantasy.

And, luckily for me I'm very happy in New York working at Columbia, with no desire to go anywhere else.

17. **JustAnotherGradStudent**
October 30, 2008

So, are we placing wagers?

Here's a wild guess—David Tong. Young, energetic, very intelligent, already a Cambridge guy, definitely a mathematical physicist.

Anyone else have any guesses? The criteria seems to be a mathematical physicist, >67 yrs old.

Discovery of a New Particle?

October 30, 2008
Categories: Experimental HEP News

Except for the excitement surrounding first beams in the LHC, particle physics has been an all-too-quiet subject recently. It looks like that may be about to change, with a dramatic new result announced by the CDF experiment this evening, in a preprint entitled Study of multi-muon events produced in p-pbar collisions at sqrt(s)=1.96 TeV.

The CDF result originates in studies designed to determine the b-bbar cross-section by looking for events where a b-bbar pair is produced, each component of the pair decaying into a muon. The b-quark lifetime is of order a picosecond, so b-quarks travel a millimeter or so before decaying. The tracks from these decays can be reconstructed using the inner silicon detectors surrounding the beam-pipe, which has a radius of 1.5 cm. They can be characterized by their "impact parameter", the closest distance between the extrapolated track and the primary interaction vertex, in the plane transverse to the beam.

If one looks at events where the b-quark vertices are directly reconstructed, fitting a secondary vertex, the cross-section for b-bbar production comes out about as expected. On the other hand, if one just tries to identify b-quarks by their semi-leptonic decays, one gets a value for the b-bbar cross-section that is too large by a factor of two. In the second case, presumably there is some background being misidentified as b-bbar production.

The new result is based on a study of this background using a sample of events containing two muons, varying the tightness of the requirements on observed tracks in the layers of the silicon detector. The background being searched for should appear as the requirements are loosened. It turns out that such events seem to contain an anomalous component with unexpected properties that disagree with those of the known possible sources of background. The number of these anomalous events is large (tens of thousands), so this cannot just be a statistical fluctuation.

One of the anomalous properties of these events is that they contain tracks with large impact parameters, of order a centimeter rather than the hundreds of microns characteristic of b-quark decays. Fitting this tail by an exponential, one gets what one would expect to see from the decay of a new, unknown particle with a lifetime of about 20 picoseconds. These events have further unusual properties, including an anomalously high number of additional muons in small angular cones about the primary ones.

The exciting possibility here is that a new, relatively long-lived particle has been observed, one that decays in some way that leads to a lot more muons than one gets from Standard Model states. It should be remembered though that this is an extraordinary claim requiring extraordinary evidence, and the possibility remains that this is some sort of background or detector effect that the CDF physicists have missed. It should also be made clear that this paper is not a claim by CDF to have discovered a new particle, rather it is written up as a description of the anomalies they have found, leaving open the possibility that these come from some standard model processes or detector characteristics that they do not yet understand.

The overwhelming success of the Standard Model during the past 30 years has meant that essentially all claims from accelerator experiments to see some new, non-SM physics have turned out to be mistaken. As a result, collaborations like CDF are now extremely careful about making such claims and will only do so after the most rigorous possible review. It's a remarkable event that this one has gotten out, signed off on by the entire collaboration (although from what I understand, people can drop their names from the publication list of a specific paper if they disagree with it, maybe one should check this author list carefully…).

What would really be convincing would be a confirmation of this from D0, the other Tevatron detector. The D0 collaboration would not only be working with a detector that has somewhat different characteristics, but would also have some motivation to find a problem with the result from their competition. If they also see it, that would be pretty extraordinary evidence. Another sort of extraordinary evidence would be to see evidence for the same kind of new particle in other channels.

This will undoubtedly unleash a flood of papers from theorists promoting models that extend the Standard Model in ways that would produce something with the observed experimental signature. This is not a signature characteristic of supersymmetry or any of the other known heavily-studied classes of models. If real, as far as I'm aware it's something genuinely unexpected. Perhaps phenomenology experts can point to some less well-known models with this kind of signature. The only such thing I'm aware of is a very recent paper from three weeks ago by Arkani-Hamed and Weiner, entitled LHC Signals for a SuperUnified Theory of Dark Matter. They discuss a theory of dark matter involving a new hidden gauge symmetry, broken near the GeV scale, saying that this is "motivated directly by striking Data from the PAMELA and ATIC collaborations". In these models there can be Gev-scale Higgs and gauge particles decaying to an anomalously large number of leptons. They discuss the question of whether the parameters of such models can be adjusted to give large decay lengths, and predict the observation of events that "contain at least two "lepton jets": collections of n > 2 leptons, with small angular separations and GeV scale invariant masses", pretty much just what CDF sees . Since the CDF paper undoubtedly has been the topic of intense discussion among the 450 or so physicists in the collaboration for many months now, the most likely explanation for the appearance of a new theory paper a few weeks ago discussing exactly the signatures in question is that news of what's in the paper got out to some theorists early. Even if this particular result goes away, this gives some indication of what sorts of things are likely to happen once LHC data starts being collected and analyzed.

The bottom line though is that for the first time in quite a while, there's some very exciting and potentially revolutionary news in particle physics. It's coming not out of the LHC, which is still a hope for the future, but from a currently functioning machine which is producing more data every day. If this result holds up, this data contains a wealth of information about some new physics which will likely revolutionize our understanding of elementary particle physics. Particle physics may already have started to move out of its doldrums.

Update: This evening I came across an unexpected source of information about this, one which might explain why news of this result may have leaked out a while ago. More about this later.

Update: The results from PAMELA mentioned here, and which are listed as motivation for the Arkani-Hamed/Weiner paper, are now officially out. For discussion of this from someone much better informed than me, see this posting at Resonaances.

Update: There's an excellent detailed posting about the paper from CDF's own Tommaso Dorigo. If you're interested in understanding exactly what is going on here, that's where you should start.

Update: For entertainment, there's always Lubos.

Update: I'm now free to explain what I was alluding to when I earlier mentioned an "unexpected source of information". Yesterday evening while I was trying to find out more about the CDF result, its relation to previously published experimental results, and to possible phenomenological models, I was running some Google searches on relevant terms. One of these turned up something very surprising: the first result was a summary of CDF's internal review of drafts of PRL and PRD papers on the subject, the second was the PRL draft. Both of these documents were from early July, and part of a publicly accessible directory containing all the materials from a review of the draft at that time. Investigating further, it became clear that the CDF web-server was seriously misconfigured, allowing directory listings and public access to a wide array of their work materials.

I wrote an e-mail to people at CDF warning them about the problem, heard back quickly, and have just checked that they have fixed it, so I don't think I'll cause them a problem other than embarassment by telling this story here. It seems likely that these materials have been publicly accessible and indexed by Google probably for several months now.

I confess to reading these documents, figuring that anything really interesting that is Googleable is fair game. Since they clearly were not intended for public consumption I won't disseminate information about them beyond this story about their existence and the fact that the PRL draft contained tentative material interpreting the data in terms of new physics, the sort of thing the released paper avoids. One thing I can say is that it is very impressive to see the amount of effort and very serious scientific work behind a review of this kind. A lot went into this, and presumably a lot more has gone into it since last July. I understand why CDF does not make this kind of thing public, but it actually would be a wonderful example for the public of how science is done to do so.

Update: See Resonaances for a discussion of the dark sector model building.

Update: Another CDF blogger heard from: John Conway.

Comments

1. **Kea**
   October 30, 2008

   Heh, heh, heh!!!

   And wow, Peter. Such a long blog post so soon after the paper appearing. One might think you knew this was coming.

2. **Peter Woit**
   October 30, 2008

   Kea,

   There had been some chatter on the intertubes indicating that it might be a good idea to check the arXiv this evening….

3. **Amos**
   October 30, 2008

   If this pans out, today is truly a day of joy.

4. **Neal Weiner**
   October 31, 2008

   I can tell you officially we had no word on this. This blog is, in fact, the first I'd heard of it. (But have now looked at the paper.) What we predicted was just what was needed to explain PAMELA we thought.

5. **a quantum diaries survivor**
   October 31, 2008

   Neal,

   that is pretty hard to digest. Lepton jets with lifetimes. Come on. I think you owe it to the physics community to let us know where the leak came from.

   Cheers,
   T.

6. **Daniel de França MTd2**
   October 31, 2008

   Halloween Particle!

7. **Daniel de França MTd2**
   October 31, 2008

   "This is not a signature characteristic of supersymmetry or any of the other known heavily-studied classes of models."

   But Arkani-Hamed/Weiner uses a SUSY model to explain that, in fact, with 2 aspects of a certain SUSY, which he calls, SUSY(I) AND SUSY(II). Check page 3.

8. **Daniel de França MTd2**
   October 31, 2008

   Maybe it's this other Arkani-Hamed's/Wiener article that you are thinking of: http://arxiv.org/PS_cache/arxiv/pdf/0810/0810.0713v1.pdf . It was published in that same day you cited before, and it doesnt use SUSY.

9. **onymous**
   October 31, 2008

   Some comments:

   On a sociological note, although the paper doesn't specify which people within CDF did the analysis, my understanding is that it is a group which has some history of finding difficult-to-understand anomalies in the data. Tommaso has documented some of these on his blog (search for "superjets"); they are an interesting story, which one might take into account when deciding what to think of this.

   In Nima and Neal's model, as I understand it, one only expects lepton jets in events with SUSY, which you would *think* would stand out in some other way.

   My take on this is that it's probably nothing. Maybe punchthrough of hadrons into the muon detector. They claim to analyze all these things and quantify them, and I haven't digested the 70 pages, but these sound like very difficult problems, and I wouldn't bet on them being fully understood. If you want exciting things in data right now, dark matter is where it's at.

   (And dark matter / positron excesses is clearly what motivated the "lepton jets"; it's offensive for Tommaso to publicly imply otherwise.)

   In short: calm down!

   — A phenomenologist who would love to see new physics and thinks this isn't it

10. **Matti Pitkänen**
October 31, 2008

Nothing new to me but two excellent birthday gifts at my birthday October 30;-).

TGD has predicted the existence of colored excitations of leptons explaining CDF anomaly already for fifteen years ago.

One of the basic predictions of TGD indeed is that leptons should have colored excitations. Already at seventeens a lot of evidence for colored electrons, or rather their pion like bound states, came from anomalous production of electron positron pairs in heavy ion collisions. For some mysterious reason it was put under the carpet. I have tried to tell about this in blogs but in vain.

For year ago evidence for muo-pions came. Again it was forgotten although Lubos saw the trouble of ridiculing the experimenters.

CDF gives evidence for tau-pion. The lifetime predicted for charged tau-pion obtained by scaling the prediction for pion life-time is correct if one scales down the parameter x in the parameter f(pi)= xm(pi) characterizing pion coupling to axial current by factor .41. To my opinion the case is now closed.

The positron and electron positron cosmic ray anomalies can in turn be seen as evidence for M_89 copy of hadron physics.

See my blog.

11. **a quantum diaries survivor**
October 31, 2008

Dear ominous (same as the one who left a comment in my blog a while ago, I guess):

punch through is a possibility, but it has been investigated in depth, by people not exactly enthusiastic about the signal, and found inconsistent with the signal, its size, and characteristics. I would rather lend towards secondary nuclear interactions instead, but it is a tough call. The fact that there is a chance – what, 1% ? 0.5%? – that it indeed constitutes new physics, is extraordinary to me nonetheless.

As for the theorists and their claims: we are adults and we know physics. We have read thousands of papers and published hundreds. We know the history of HEP. We know how people chats in the corridors. And I personally know that in this particular case some people in CDF have been less careful than they should have with this paper before publication.

On the other side, there is this pair of papers coming out of the blue, which casually get published a few months after the CDF analysis emerges from the authors' computers and is in internal review, and less than a month before that analysis sees the light in the arxiv. These two phenomenological -but I would rather call them phenomenal- papers discuss a signature almost never heard of before. It entails lepton jets. Many leptons, with small invariant masses. With long lifetimes.

I say, you can believe in SUSY or you can believe in the Yeti, or whatever you want. I can believe a lot of things, but I do not buy that it is their own cooking. Especially since the paper is rather handwaving in tone and conclusions.

So, to summarize: it may sound offensive to be inquisitive with respectable theorists about where the hell they got that idea, but it is no less offensive to act nonchalant and pretend we buy this incredible coincidence without a detailed explanation.

Finally, about the authors of the analysis: be very careful. They are among the best physicists I know in CDF, and I know all of them. They have the vice of searching for the unknown rather than being happy with measuring something with twice better accuracy than others. I would like it if there were more like them around.

Cheers,
T.

12. **woit**
October 31, 2008

Daniel,

The Arkani-Hamed/Weiner paper doesn't use SUSY in any crucial way. Their proposal is that the observed signature comes from a "Dark Sector", this can be embedded in a SUSY model, but that's not necessary. From their paper:

"While our discussion is framed within the context of low-energy SUSY, some of the conclusions hold in a wider class of theories for new physics. The signals associated with decays into the dark sector and back follow in any theory with a particle charged under the SM that is nonetheless stable in the absence of a small coupling to the Dark Sector, while the new colored states should be expected in any picture in which the Dark Matter is charged under the Standard Model and gauge coupling unification is taken seriously."

13. **Daniel de França MTd2**
October 31, 2008

Peter,

The article with Moose tries to argue for SUSY, since this new Sommerfeld force must be unified with a larger picture, that is, the SM through SUSY. What the other article, does the thing you want, that is, specificaly describing that phenomeny, but without saying that SUSY has anything to do with that, or trying to embed that phenomena in any bigger scheme…

14. **andy.s**
October 31, 2008

Didn't Willis Lamb suggest that anyone discovering a new particle be punished with a $10,000 fine?

15. **Yatima**
November 1, 2008

>> Didn't Willis Lamb suggest that anyone discovering a new particle be punished with a $10,000 fine?

That was before the quark model and QCD, right? These days, discovering new particles is what decade-in-building billion-dollar machines are all about.

I applaud this New Kind of October Surprise. Here's hoping something will come out of it.

16. **L. Carpenter**
November 1, 2008

This all seems very iffy, howver it still doesn't rule out SUSY,
You know, SUSY with R parity violation has displaced vertices, as per Dreiner, Grab, Banks, Carpenter, Kaplan, Rhee and probably many more etc and so on, you just make the coupling small enough and the intermediate sparticle heavy enough. However I'd bet that this might go away.

17. **a quantum diaries survivor**
November 1, 2008

"I would bet than this might go away"

Lol… This is rather careful, isn't it ? Of course odds are against the multi-muon signal being new physics! Would you bet 500 to 1 ? That would be a statement. For me, a >0.1% chance that this is new physics is a HUGE chance.

Cheers,
T.

18. **Covariant**
November 1, 2008

I'm just wondering where the boundary between new and old physics is defined. When do we know enough of the old physics to know its new physics?

This reminds of long range artillery in WWI, when for the first time the coriolis effect had to be taken into account in calculating trajectories (confirming the Earth was spinning).

http://en.wikipedia.org/wiki/Paris_Gun

Do we know all the environmental effects well enough?

19. **mike**
November 1, 2008

It's certainly wise not to call it a new particle until it's been peer reviewed, but it's also wise not to dismiss it immediately as a detector problem. It would be good to see the D0 provide some insight into the problem. Wouldn't they have characterized the Si detectors in CDF pretty thoroughly by now?

20. **chickenbreeder**
November 1, 2008

Covariant: Foucault's Pendulum (cir. 1850) was already definitive experimental confirmation of Earth rotation.

A question from a casual reader: Could all this hope and positive outlook on the new particle be the result of desperation, given that there has been no breakthrough in high energy particle physics in decades? Also, was the experiment specifically designed with the expectation of finding a particle of this characteristic, or is it serendipity (assuming that there's indeed a new particle in this story)?

21. **Peter Woit**
November 1, 2008

Covariant,

By "new physics" what is meant is something not in the Standard Model. One possibility is that what is being seen is due to some poorly understood effect of the Standard Model, e.g that this might somehow be just another bound state of quarks.

mike,

The paper has undergone very extensive review at CDF, I doubt that a referee will find problems not identified in their internal review, but it's not impossible. They are careful in the paper not to claim discovery of a new particle, just to be putting forward an anomaly in the data that is not understood, with a new particle one possible explanation. The behavior of these detectors and the data analysis are extremely complex, there's a lot that can go wrong. It will be very interesting to see what D0 finds when they look for this.

chickenbreeder,

The lack of new discoveries in particle physics certainly makes any anomaly get a lot more attention. The detector was not designed to look for this.

22. **Luboš Motl**
November 2, 2008

"This is not a signature characteristic of supersymmetry."

That's quite a breathtaking statement. The whole paper by Arkani-Hamed and Weiner is based on supersymmetry. There would be no lepton jets without it. The acronym LSP – lightest supersymmetric particle – appears 45+ times in their paper.

They obtained the model by taking the model by these 2 and 2 other authors meant to match the PAMELA, ATIC, DAMA, INTEGRAL observations. Because the latter requires light scalars, SUSY is needed for naturalness. So they added it. Then it follows that there are several LSP-like particles and the lepton jets come from them. The model without supersymmetry cannot predict any lepton jets (although all models can be fine-tuned to get one particular prediction right, but others are likely to fail then).

If this explanation were right, it would be a nearly complete experimental proof of SUSY as well as several other characteristically string-theoretical signatures, including a hidden gauge theory sector and a quiver spectrum of matter. It is likely that some compactifications of string theory predict exactly this thing and they will be located soon, leading to new predictions. This scenario was just unrealized by everyone because everyone kind of thought that all hidden sector particles have to be heavy.

It would become the most direct proof in decades that the notion that these concepts are "untestable" or untrue or "not even wrong" and blah blah blah is just a gigantic and very stupid lie, indeed.

23. **Covariant**

November 2, 2008

Peter,

I appreciate the answer. I think that was what I was unknowingly alluding to.

Chickenbreeder,

I was cognizant of that fact (hence the word confirm and not proof). I think my intent was misunderstood.

There is still a lot about our little blob of iron and mud that we are unsure about 🙂

24. **Peter Woit**
November 2, 2008

Lubos,

I think you missed this:

http://www.math.columbia.edu/~woit/wordpress/?p=1045#comment-44797

Your claim on your blog that this is "An experimental proof of most low-energy signatures of string theory", and speculation that Arkani-Hamed and Weiner will share a Nobel prize for this are among the funnier things I've ever seen on a physics blog.

By the way, have you or anyone else asked Arkani-Hamed whether he knew of the CDF result before it was published?

25. **H-I-G-G-S**
November 2, 2008

Lubos,

If you buy into the string landscape explanation of the cosmological constant, as you seem to, then complaints about fine-tuning and appeals to naturalness carry very little weight. Without these there is no reason you can't have a model with the main features of the Arkani-Hamed and Weiner model but without supersymmetry. Claiming this result, if it holds up, as nearly complete experimental proof of SUSY is absurd.

PH

26. **Shantanu**
November 2, 2008

Peter, did I read correctly on cosmicvariance that one third of CDF collaboration have dropped their name off the paper? Do you know the reason?

27. **Esornep**
November 2, 2008

"Peter, did I read correctly on cosmicvariance that one third of CDF collaboration have dropped their name off the paper? "

You did. And what makes this utterly astounding is that the paper makes essentially no claims whatever to have found anything! All they say is, "we saw this weird signal in our data. Make of it what you will." Or could it be that the recalcitrant 1/3 are protesting at this extreme wishy-washiness? 🙂

28. **Esornep**
November 2, 2008

BTW, Arkani-Hamed and Weiner have rushed out a second version of their paper. It raises many question marks. And I mean that literally!

29. **onymous**
November 2, 2008

What a circus! Anyone want to place bets on how long before we see the first "unparticle" explanation appear on hep-ph?

30. **witness**
November 3, 2008

to Esornep:
The exact reasons for not signing vary but most range from "CDF can do much better in investigating this" to "this is clearly correlated hadronic punch-through of jets".

31. **Axolotl**
November 3, 2008

Latest from Arkani-Hamed at Dorigo's blog: it's lies, all lies, and anyway our model does *not* predict anything like what has been seen. Lubos Motl's denunciation of NAH as a crackpot is expected momentarily.

32. **Covariant**
November 3, 2008

I think NAH is deserving of some sympathy.

I am slowly making sense of the CDF paper, and being ignorant of how Tevatron runs are scheduled, I was wondering if CDF has done any time analysis on the "jets" to see if there was any variation based on time of day.

33. **a quantum diaries survivor**
November 4, 2008

To whom may be concerned:

the fact that the CDF paper carries only two thirds of the usual author names is due to the particular publication process this paper withstood. The authors wanted to see it out soon, because of the possibility (quite real, in fact, if you have read Peter's update above) that it might leak out and be scooped by a quick parallel analysis by D0 (unlikely, given that D0 is under-manned these days, and the bounty of data on which to concentrate for several other important measurements so large). Some colleagues in CDF felt they wanted more analysis, and they decided not to sign. This is still a CDF publication, case closed.

Let me add that CDF has published over 400 papers in its illustrious, 25-year-long career, and that we are publishing many, many other important measurements on a weekly basis. So, please do not concentrate too much on this one paper, which, I am sure, will be seen in perspective in a few months, one of two very different perspectives indeed. If electrons do not confirm, and D0 disproves the multi-muon signal, this will be just a honest attempt to explain to the scientific community what we saw and what we did not understand of muons in our data -which has some impact in past measurements. If there are confirmations, then the story is different of course, and those 1/3 of authors who took their name off will reconsider the matter for the future publications on the same topic.

Cheers,
T.

The Circus Begins

November 2, 2008
Categories: Experimental HEP News

Friday's arXiv posting of the paper by CDF about the multi-muon anomaly they are seeing has already generated three different conjectural explanations of what physics might be responsible for this. Undoubtedly many, many more are on the way.

- Some members of the CDF collaboration have posted a paper entitled Phenomenological interpretation of the multi-muon events reported by the CDF collaboration. This explains the large numbers of muons with a rather baroque mechanism, conjecturing the production of a 300 GeV heavy particle decaying through a chain of 3 lighter particles, the last of which is supposed to be the long-lived (20 picosecond) one. This interpretation was part of the original draft PRL from last June/July. The CDF collaboration as a whole seems to have decided not to support the draft PRL and this interpretation, instead releasing just a PRD paper that describes the anomaly without trying to interpret its significance. It also seems that only two-thirds of the collaboration put their names on the PRD paper, and the interpretation paper was put out just by a small group. The whole story is somewhat reminiscent of the "Superjet" affair (see Tommaso Dorigo's multi-part discussion here), which also involved a PRD publication about an anomaly signed by the collaboration, and an interpretation (in terms of squarks) signed by a much smaller group led by Paolo Giromini.
- An hour or so after the Giromini et. al. paper came in on Friday, a group of string theorists had posted the 40-page Towards Realistic String Vacua on hep-th claiming to explain the CDF results with a class of string vacua:

> We also describe model-independent physical implications of this scenario. These include the masses of anomalous and non-anomalous U(1)'s and the generic existence of a new hyperweak force under which leptons and/or quarks could be charged. We propose that such a gauge boson could be responsible for the ghost muon anomaly recently found at the Tevatron's CDF detector.

If the Giromini et. al. explanation invoking 4 new particles is baroque, it's hard to know what the right word is for the far more complicated constructions that are described in this paper.

- There's a new version this evening of the 3-week old Arkani-Hamed/Weiner LHC Signals for a SuperUnified Theory of Dark Matter, in which they claim to have a new signature for supersymmetry, with a large fraction of all SUSY events looking exactly like what CDF described. Oddly enough, the changes to the paper don't include a mention of the CDF result. This paper also invokes a rather baroque mechanism, involving both the supersymmetric extension of the standard model and a whole new complicated dark sector.

This last paper is also supposed to explain the PAMELA data, and papers with other explanations of this are starting to flood hep-ph.

So far, all the explanations of the anomaly seen by CDF look suspiciously complicated, which may be one reason that many members of CDF are so skeptical about the whole thing that they were unwilling to sign on to the PRD submission. But I'm sure that many more proposals for how to explain the anomaly are being drafted at this very moment, and maybe one of them will be more convincing.

Update: Over at Tommaso Dorigo's blog there's a short posting about Giromini et. al., and an exchange with Nima Arkani-Hamed, who claims to have had no inside knowledge of the CDF "lepton jets" when he wrote his paper with Weiner predicting them. He also explains how the exact mechanism discussed in that paper is unlikely to explain the CDF result since their observed rate is too high for this.

Update: New Scientist has the story, emphasizing the possible relation to the work of Arkani-Hamed and Weiner:

> So what could it be? As it happens, Weiner and Nima Arkani-Hamed of the Institute for Advanced Study in Princeton, New Jersey, and colleagues have developed a theory of dark matter – the enigmatic stuff thought to make up a large proportion of the universe – to explain recent observations of radiation and anti-particles from the Milky Way.
>
> Their model posits dark matter particles that interact among themselves by exchanging "force-carrying" particles with a mass of about 1 gigaelectronvolts.
> The CDF muons appear to have come from the decay of a particle with a mass of about 1 GeV. So could they be a signature of dark matter? "We are trying to figure that out," says Weiner. "But I would be excited by the CDF data regardless."

CDF spokesperson Jacobo Konigsberg is quoted as saying:

> we haven't ruled out a mundane explanation for this, and I want to make that very clear

Update: Then there's Slashdot, where the hypothetical CDF particle is advertised as accounting for the Arkani-Hamed et. al. theory of dark matter.

Update: Another story, at Physics World, which has more from various people at CDF. Again, that Arkani-Hamed/Weiner "predicted a CDF–like signal", although the problem with the rate being too low is mentioned.

Also Nature, where one learns:

> Theorists are already coming up with ideas about what might be producing the excess muons. One possibility is that they stem from the decay of a heavier, yet-to-be-discovered particle — perhaps related to dark matter, an unseen material that is believed to make up some 85% of matter in the Universe.
>
> Another idea from string theory evokes seven-dimensional 'branes' — theoretical surfaces that are inhabited by exotic particles manifested as strings. These higher-dimensional branes might be home to force-carrying particles that interact weakly with our three-dimensional world and create a faint, but traceable, signal in the data.
>
> But Adam Falkowski, a theorist at CERN, Europe's particle accelerator laboratory near Geneva, Switzerland, says that the explanations need some work, and cautions against attempting to force the data to fit into particular theories.

Update: More press stories here and here.

Comments

1. **onymous**
   November 3, 2008

   A 300 GeV particle with a 100 nb cross section. That's… special.

(They don't need the 300 GeV particle; they need the 15 GeV particle, but with enough of a boost to have collimated decay products. Hence the ad hoc 300 GeV Higgs to fit the curves.)

2. **Coin**
November 3, 2008

So far, all the explanations of the anomaly seen by CDF look suspiciously complicated, which may be one reason that many members of CDF are so skeptical about the whole thing that they were unwilling to sign on to the PRD submission

Hm, maybe I don't understand what you're suggesting here… is it normal for experimental particle physicists to doubt experimental results to the point of not participating in their publication, just because they can't find a clean theoretical mechanism from which those results came?

3. **Roger**
November 3, 2008

Its very unusual for a collaboration member to remove his/her name from a paper though it does happen from time to time.

However, I have never heard of a third of a collaboration removing their names. This is very worrying.

4. **Peter Woit**
November 3, 2008

Coin,

If your experiment is seeing something confusing and completely different than what known theory predicts, the chances are quite high that the problem is with your experiment, and every experimentalist is well aware of this. On the other hand, if the 20ps state CDF is seeing evidence for had the right properties to be the SM or MSSM Higgs, I'd bet absolutely everyone in CDF would have signed on, the paper would be a PRL making a discovery claim, and CDF leaders would be ordering their outfits for a trip to Stockholm.

5. **James**
November 3, 2008

There's a quote I've heard attributed to Einstein that goes roughly like "A theory is something believed by nobody except its creator, and an experiment is something believed by everybody except the experimenter."

6. **Shantanu**
November 3, 2008

Peter or anyone else, does anyone know what this model predicts for proton decay? This might be a good way to test the consistency of these models.

7. **Peter Woit**
November 3, 2008

Shantanu,

Which model? I don't think any of the models discussed here is the sort of thing that would make a testable prediction about proton decay.

8. **Sumar Ongi**
November 3, 2008

My personal hunch —as if it mattered— is that this whole thing will fizzle out. This is not a criticism of CDF and of the theoreticians working on this issue (quite the opposite, I see their work with respect and even admiration). It's just how physics works: 99% blind alleys, 1% breakthroughs.

Now, a 300 GeV Higgs boson? I would love so much to see that!!!

9. **dragon**
November 4, 2008

"Then there's Slashdot, where the hypothetical CDF particle is advertised as accounting for the Arkani-Hamed et. al. theory of dark matter."

Taken literally, this statement means that the CDF observations explain the existence of certain papers, rather than the other way around. One suspects a typo at first, but in view of what has been going on over at Dr Dorigo's blog, perhaps this is the right ordering after all…….

10. **a quantum diaries survivor**
November 4, 2008

Lol dragon… I decided I will not investigate the matter further. We have heard from the theoreticians, we know the story, the documents are out there for anybody to make their own mind. And regardless of opinions, publications remain.

My own opinion is however the following:
1) the CDF signal is not NP
2) the NAH et al. papers discuss an interesting signature, which however does not describe reality
3) we are still in the dark and we will stay there for a while
4) I am going to collect 1000$ soon. How soon ? Depends when the LHC will deliver 10/fb of data.

Cheers,
T.

11. **Arnaud**
November 4, 2008

Hello,

I'm not a professional physicist so pardon my naivete: How can anybody have a theoretical explanation for what appears to be a quite unexpected experimental result, less than a week after its publication? I'm sure HEP is full of very bright people, nevertheless this looks a bit rushed to me, and suspiciously like a race to publish.

Personally, my only way of understanding this would be if either the news have been in the pipeline beforehand, or it was in fact expected. The blog hasn't mentioned either fact, I think?

Physicists like to mock the media and their rush to the "story" over scientific content, but from the outside this particular event looks like this story of pots, kettles and blackness… Or am I wrong?

12. **Peter Woit**
November 4, 2008

Arnaud,

There's plenty ot mock about the media's coverage of science, but my own point of view is that in recent years the blame for problems with media coverage of some parts of physics lies with physicists themselves rather than journalists.

This experimental result was not expected at all by theorists. It is not known how widespread knowledge of the CDF result was before it was released last Thursday evening. Back in July, people at CDF were already worried that the news had leaked out (how do I know this? because by mistake the documents of their internal deliberations were freely available on-line and indexed by Google.)

One of the papers mentioned above appeared within a day of the Thursday evening news. My own view is that the paper in question uses a typical string theory set-up that is capable of explaining anything. String theorists are hard-working and industrious, it's perfectly plausible that within 24 hours they could have drafted the section of the paper needed to "explain" the CDF result within the context of the string theory models they are working with. In the same 24 hours they undoubtedly could have come up with explanations of all sorts of different unexpected experimental results.

This all shows what is likely to happen if and when unexpected preliminary result come out of the LHC. String theory/supersymmetry explanations of the results will come fast and furious, no matter what they are.

13. **Peter Woit**
November 4, 2008

In case the above is unclear, I'm referrring to the second paper mentioned in the posting. The first is from within CDF, the authors have had many months to work on their analysis. The last one is a paper written before the results were announced, there is extensive discussion about it over at Tommaso Dorigo's blog.

14. **Chris W.**
November 4, 2008

This all shows what is likely to happen if and when unexpected preliminary result come out of the LHC. String theory/supersymmetry explanations of the results will come fast and furious, no matter what they are.

It will be a painfully protracted process of coming to see not ST/SUSY's inability to explain these results, but its inconclusive and ultimately sterile fecundity* in explaining them, along with a million other results we don't happen to see, for reasons that will receive their own convoluted explanations (if anybody cares).

[* or to use Kant's phrase, "accursed fertility"]

15. **Arnaud**
November 6, 2008

Thanks Peter & Chris. I am not participating constructively to the discussion here, but instead I have another question, sorry again.

You seem to imply that String theory is an elastic concept which can
be made to fit any new fact and sort of retroactively claim that some form of it predicted the result, right? I'm kind of an educated layman on this (I'm only an actual rocket scientist), but I read Lee Smolin's book, so I guess I'm convinced by your argument now 🙂

My question is slightly different here: People like you and Lee seem to argue that over the last 20 or 30 years, not much has happened in theoretical particle physics, due in part to the lack of experiments (like the LHC) probing deeper into the high-energy spectrum. This seems to leave plenty of time to researchers to analyse current gaps in detail and hone their predictive models or new theories.

When I match that to some papers and presentations I've seen where different explanations are proposed depending on different expected values or results of proposed experiments (such as the mass of the hypothetical Higgs Boson), It leads me to imagine that there are a bunch of -to caricature- "if-then-else" set of explanations and theories ready to be put out as soon as new results come in. Does that make sense, and would you say it's in any way correct, or is the picture not quite like that?

This was how I though one could explain fast turnover of papers, although you're clear in your response it doesn't apply here because the results are quite unexpected. I guess this is what makes fundamental physics interesting!

PS, I read Tomaso's futher post and loooong dialogue and comments on this. I can't claim to have an informed opinion, but it leaves me a sad impression that the discussion is as much about physics as it is about ego and politics.
I recognise though, it takes a strong dose of wisdom and character to stand up for scientific integrity in this world of inflated career expectations, performance-related-pay and short-term contracts. I guess they don't teach those in physics courses… (This comment could be made for a lot of disciplines, naturally).

16. **Thomas D**
November 7, 2008

It is actually very disappointing that experimentalists would effectively refuse to publish a 10 sigma deviation, even after all imaginable systematics and loopholes are investigated, just because it is difficult to find a theory (within conventional frameworks) to account for it. But I don't know the exact reason why the whole collaboration would not sign up to the long Phys Rev paper.

The quote about 'theory is believed by no-one beyond its creator' is not at all true nowadays. On the contrary, HEP experimentalists are very reliant on theorists to tell them what types of signals are 'likely' to emerge (meaning, likely given some popular model or theory). That means that if none of the popular theories are right, the experimental search will not be well set up to investigate what there actually is.

There are a few groups trying to put together a 'model-independent' search for new physics at LHC, which is a good insurance policy against all the theorists being wrong, but maybe not enough attention is being paid to this.

Any reporting that tries to link the muons to recent dark matter models is pretty much malpractice. Arkani-Hamed has written:
"even if the CDF anomalies are an indication of new physics–which I think in all of our views is _very_ far from obvious– it can not be due to the signal Neal and I talked about, arising from SUSY cascade decays. The rate of the CDF anomaly is absolutely enormous–you are talking about 70,000 "ghost" events! (…)"

I am not sure what Peter is blaming physicists for in media coverage. Is he saying the Quevedo group is too hardworking? I have to say also, they are relatively lucky, since contrary to what he is insinuating not all string models have these leptonic or 'hyperweak' U(1)'s readily available… and not all string theorists happen to have a 40-page paper ready to publish at the same time as experimentalists announce something.

Perhaps someone could take the elementary step of asking someone in that collaboration whether or not their models can always explain every possible signal, rather than taking this as a basic assumption.

And it's by no means certain that this model *can* explain the multi-muons, rather than just containing ingredients which could go some way towards it. On p. 30 they say:
"Notice that this proposal to explain the CDF data can be considered independently of string theory. While we have assumed U to be a gauge boson motivated by the D3-D7 brane models considered in this paper, the same basic phenomenological approach could apply for any bulk state with very weak couplings to quarks and no kinematically accessible 2-body decays. (…)
It remains to be seen if with these numbers it is possible to obtain the proper U lifetime. The above is clearly a phenomenological scenario and not yet a full model. More detailed model-building is necessary for a full analysis of the merits and further phenomenological and cosmological implications of the approach outlined."

So again, what is Peter's problem with this?

17. **Peter Woit**
November 7, 2008

Thomas D,

CDF is not refusing to publish anything, they have submitted a paper to PRD. Some of the collaboration aren't signing on to the publication for various reasons. For an experimentalist to be skeptical about a very weird signal, thinking it is likely to be a flaw in the analysis, so not ready for publication, isn't exactly unusual or surprising.

I don't think I'm the only one who finds laughable the spectacle of a group of string theorists putting out a paper in less than 24 hours "explaining" a very weird and unexpected signal using a very complicated "string vacuum".

18. **Gazouille**
November 8, 2008

@webmaster, the link 'here' in last update

http://http//blogs.discovermagazine.com/80beats/2008/11/05/ghost-in-the-machine-physicists-may-have-detected-a-new-particle-at-fermilab/

is wrong, it should be

http://blogs.discovermagazine.com/80beats/2008/11/05/ghost-in-the-machine-physicists-may-have-detected-a-new-particle-at-fermilab/

19. **Peter Woit**
November 8, 2008

Gazouille,

Thanks, fixed.

20. **Brandon Watson**
November 9, 2008

If the Giromini et. al. explanation invoking 4 new particles is baroque, it's hard to know what the right word is for the far more complicated constructions that are described in this paper.

I know that this was ironic, but the word for the step up from ordinary baroque is churrigueresque — 'frantically baroque', as it has sometimes been called. I bring it up just because it's sometimes a very good word to have on hand.

21. **Peter Woit**
November 9, 2008

Thanks Brandon, I'm always glad to learn a new word.

Notes on BRST I: Representation Theory and Quantum Mechanics

November 5, 2008
Categories: BRST

This is the first posting of a planned series that will discuss the BRST method for handling gauge symmetries and related mathematical topics. I've been writing a more formal paper about this, but given the substantial amount of not-well-known background material involved, it seems like a good idea to first put together a few expository accounts of some of these topics. And what better place for this than a blog?

Many readers who are used to my usual attempts to be newsworthy and entertaining, often by scandal-mongering or stirring up trouble of one kind or another, may be very disappointed in these posts. They're quite technical, hard to follow, and of low-to-negative entertainment value. You probably would do best to skip them and wait for more of the usual fare, which should continue to appear from time to time.

Quantum Mechanics and Representation Theory

A quantum mechanical physical system is given by the following mathematical structure:

- A Hilbert space $\mathcal H$, the "space of states". A state of the physical system is determined by a vector $|\psi\rangle\in \mathcal H$, with unit norm (i.e. $||\psi||^2=\langle \psi|\psi\rangle=1$).
- An algebra $\mathcal O$ that acts on $\mathcal H$. To each physical observable corresponds a self-adjoint operator $O\in\mathcal O$. Eigenvectors in $\mathcal H$ of this operator correspond to states where the observable has a well-defined value, which is the eigenvalue.

If a physical system has a symmetry group G, there is a unitary representation $(\Pi, \mathcal H)$ of G on $\mathcal H$. This means that for each $g\in G$ we get a unitary operator $\Pi(g)$ satisfying

$$\Pi(g_3)=\Pi(g_2)\Pi(g_1)\ \text{if}\ g_3=g_1g_2$$

i.e. the map Π from group elements to unitary operators is a homomorphism. The $\Pi(g)$ act on $\mathcal O$ by taking an operator O to its conjugate $\Pi(g)O(\Pi(g))^{-1}$.

When G is a Lie group with Lie algebra $(\mathfrak g,)$, differentiating Π gives a unitary representation $(\pi, \mathcal H)$ of $\mathfrak g$ on $\mathcal H$. This means that for each $X\in \mathfrak g$ we get a skew-Hermitian operator $\pi(X)$ on $\mathcal H$, satisfying

$\pi(X_3)=\ \text{if}\ X_3=$

i.e. the map π taking Lie algebra elements X (with the Lie bracket in $\mathfrak g$) to skew-Hermitian operators (with commutator of operators) is a homomorphism. On $\mathcal O$, $\mathfrak g$ acts by the differential of the conjugation action of G, this action is just that of taking the commutator with $\pi(X)$.

The Lie bracket is not associative, but to any Lie algebra $\mathfrak g$, one can construct an associative algebra $U(\mathfrak g)$ called the universal enveloping algebra for $\mathfrak g$. If one identifies $X\in \mathfrak g$ with left-invariant vector fields on G, which are first-order differential operators on functions on G, then $U(\mathfrak g)$ is the algebra of left-invariant differential operators on G of all orders, with product the composition of differential operators. A Lie algebra representation is precisely a module over $U(\mathfrak g)$, i.e. a vector space with an action of $U(\mathfrak g)$.

So, the state space $\mathcal H$ of a quantum system with symmetry group G carries not only a unitary representation of G, but also a unitary representation of $\mathfrak g$, or equivalently, an action of the algebra $U(\mathfrak g)$. $X\in \mathfrak g$ acts by the operator $\pi(X)$. In this way a representation π gives a sub-algebra of the algebra $\mathcal O$ of observables. Most of the important observables that show up in practice come from a symmetry in this way. An interesting philosophical question is whether the quantum system that governs the real world is purely determined by symmetry, i.e. such that ALL its observables come from symmetries in this manner.

Some Examples

Much of the structure of common quantum mechanical systems is governed by the fact that they carry space-time symmetries. In our 3-space, 1-time dimensional world, these include:

- Translations in space: $G=\mathbf R^3$, $\mathfrak g =\mathbf R^3$, Lie Bracket is trivial.
For each basis element $e_j\in \mathfrak g$ one gets a momentum operator $\pi(e_j)=iP_j$
- Translations in time: $G=\mathbf R$, $\mathfrak g =\mathbf R$. If e_0 is a basis of $\mathfrak g$, $i\pi(e_0)=H$, the Hamiltonian operator. The fact that this operator generates time-translations is just Schrodinger's equation.
- Rotations in 3-space: $G=SO(3)$, or its double cover $G=Spin(3)=SU(2)$, $\mathfrak g = \mathbf R^3$, with bracket given by the vector product. For each basis element $e_j\in \mathfrak g$ one gets an angular momentum operator $\pi(e_j)=iJ_j$. These operators do not commute, so cannot be simultaneously diagonalized.

Another example is the symmetry of phase transformations of the state space $\mathcal H$. Here $G=U(1)$, $\mathfrak g=R$, and one gets an operator Q_e that can be normalized to have integral eigenvalues.

This last example also comes in a local version, where we make independent phase transformations at different points in space-time. This is an example of a "gauge symmetry", and the question of how it gets represented on the space of states is what will lead us into the BRST story. Next posting in the series will be about gauge symmetry, then on to BRST.

If you want some idea of where this is headed, you can take a look at slides from a colloquium talk I gave recently at the Dartmouth math department. They're very sketchy, the postings in this series should add some detail.

Comments

1. **Tom**
November 5, 2008

   Thanks for this BRST post, and those to come! I'm going to enjoy reading some technical stuff. I know I haven't seen BRST symmetry treated in a really mathematical way before.

2. **anonymous**
November 5, 2008

Please do not use the > character for the ket when you typeset using TeX!
It is for the mathematical less-than symbol. As such, the spaces around > are automatically widened, which makes your kets super-ugly.
See p.170 of TeXBook about \mathrel, \mathop etc.
So please either use at least |\psi\mathclose>. I prefer |\psi\rangle, though.

3. **estraven**
November 5, 2008

Great post, I'm looking forward to the rest fo the series.

@anonymous at 10.03pm: My .tex files include the lines

\def\>{\rangle}
\def\<{\langle}

because I like good typesetting but am also very lazy.

4. **TSM**
November 5, 2008

I have to ask; which program did you use to make your talk slides? If it is powerpoint can you point me to the template used?

TIA

5. **anon.**
November 6, 2008

One interesting question which is hard to understand from the standard textbook treatments and limited reading of the literature: does the BRST action for gauge-fixed Yang-Mills theory break down nonperturbatively, due to Gribov copies? Claims in both directions seem to exist, but it seems it would take a great deal of effort and/or reading to settle the matter. (Also: are there Gribov copies for gauge-fixing diffeo and Weyl invariance? I don't think I've seen this mentioned in treatments of BRST quantization of the string.)

6. **anonymous**
November 6, 2008

That's a LaTeX class called beamer. See http://latex-beamer.sourceforge.net/.
PowerPoint and Keynote are for losers.

7. **Kurt**
November 6, 2008

Fantastic, I am looking forward to reading about BRST. There is something that has always bugged me about symmetry in QM: Surely we do not want to say that every time independent unitary operator is a symmetry, because then every quantum system with the same dimension Hilbert space would have the same symmetry group (i.e. U(N), where N is the dimension of the Hilbert space). There must be some input about the dynamics (i.e. Hamiltonian) of the system. Usually we say something like: a symmetry is any time independent unitary operator that commutes with the Hamiltonian. This guarantees that the symmetry preserves the Schrodinger equation, but rules out things like the boosts (either Galilean or relativistic), which are represented by unitary operators that depend explicitly on time, and which we would like to have as symmetries. Thus, we might want to say that a symmetry is allowed to carry explicit time dependence, so long as it preserves the Schrodinger equation. But this brings us back to the original problem of all quantum systems (of a given dimensional Hilbert space) having the same symmetry group: given any unitary operator S, we can give it time dependence in such a way that it preserves the Schrodinger equation (just take S(t)=U(t)SU(t)^{-1}, where U(t) is the time evolution operator). So where is the middle ground; what is a definition of symmetry, (using only the notion of Hilbert space and hamiltonian, no fair talking about classical concepts such as action, Noether theorem etc., or extra structure such as the S-matrix) that allows the boosts but does not allow an arbitrary unitary operator?

KH

8. **N. Nakanishi**
November 6, 2008

The problem of Gribov copies arises only in the path-integral approach. No such trouble is encountered in the BRS-formulated operator formalism.

9. **Peter Woit**
November 6, 2008

Tom and anonymous,

Thanks for the tex advice.

TSM,

The slides were made using the "beamer" latex class.

anon.,

One reason I'm interested in BRST is precisely that I don't think the problem of how to handle gauge invariance is fully understood in theories like the Standard Model. I'll write more about this later, and the Gribov problem is part of the story. I don't believe there is any agreement about what happens to it outside of perturbation theory. What I'm writing about is mostly the operator formalism, and this does not have the same problems that the path integral has (as Nakanishi mentions). It may have others…

In diffeo and Weyl invariance, you don't get the same topological phenomenon that forces the Gribov problem, so I don't think you see it in the quantization of the string.

Kurt,

Interesting question. I guess when I'm talking about a "symmetry", this can come from any group action on the classical mechanical system being quantized, whether or not it commutes with the Hamiltonian. These give operators on the state space, which may or may not commute with the Hamiltonian operator. Well-known examples include the algebra of at most quadratic operators in p and q (Heisenberg + symplectic algebra)

10. **A.**
November 6, 2008

Dear Peter and anon,

Interesting stuff. With regard to Gribov copies, they are not just some mathematical problem with overcounting in the path integral — they have physical implications. It's been known for a long time that they play a key role in confinement, so it would be really surprising if the operator picture didn't feel their effects at all.

A.

11. **anon.**
November 6, 2008

A. is right: there's a large literature about Gribov copies and confinement, with which I have only a superficial familiarity, but which seems very interesting. It's been observed, for instance, that the center vortex configurations that have been primary candidates for explaining confinement since at least the work of 't Hooft in the late 70s lie on the Gribov horizon in certain gauges (hep-lat/0401003).

Fundamentally, Gribov copies are telling us that the Yang-Mills configuration space has a complicated geometry and topology, and I don't see how that could go away in the operator formalism. (Are there references?)

I could imagine, though, that the ghosts somehow encode the gauge algebra in a way that allows the BRST action to get all of this right anyway. (To throw out another reference that I haven't properly digest, Reinhardt in hep-th/9602047 showed that in maximal abelian gauge the path integral with an explicit integration over the group with the correct Haar measure is equivalent to the path integral with a Faddeev-Popov determinant.)

12. **FNesti**
November 6, 2008

Dear Peter,
thabks for this good idea, it will be interesting and useful to read about BRST and Dirac. I'd like to ask whether you plan to mention the Dirac-Hodge operator (d+delta) and its properties. In my present understanding, this is the same of a standard dirac operator in flat space, while on a generic manifold it gives a different coupling of 'spinors' to gravity. I believe this would be a really interesting point to clarify, from your cross-discipline experience, from both the mathematical and physical point of view.

Cheers,
Fabrizio

13. **Peter Woit**
November 6, 2008

Fabrizio,

The point you raise is not one that comes up only tangentially in the BRST story, but it is an important one if you try and understand spinors.

Basically, the operator you mention operates on differential forms, not spinors, and at each point the differential forms take values not in the spinor space S, but in SxS*. In flat space you get just dim (S*) copies of what you want (this is related to why Kogut-Susskind gives you multiple copies of what you want on the lattice). In non-flat space there's a non-trivial covariant derivative for the spinors, so you get not just multiple copies, but something different.

14. **Peter Orland**
November 6, 2008

The problem of Gribov copies does not go away in the Hamiltonian formalism. If complete gauge fixing (such as axial or Coulomb gauge) is performed, Gauss' law needs to be solved.

As an example I have some familiarity with, if one solves Gauss' law on a Hamiltonian lattice, to get an axial gauge condition (say $A_{3}(x)=0$ or $U_{3}(x)=1$ on most of space, it is a messy problem to define the conjugate electric field variable (E_{3}, or l_{3} on the lattice) everywhere, if the boundary condition is periodic. With open boundary conditions in every direction (so space is a rectangular prism) the situation is much better, and it is possible to further fix the gauge, completely removing all gauge freedom. It is not a trivial task to do this, however.

Anyway, in the above example, the Gribov problem cannot be ignored, though it can be solved.

15. **Peter Orland**
November 6, 2008

P.S. In the BRST operator formalism, you need to construct an exterior derivative operator from constraints. You need to prove that the square of this operator is zero. I guarantee that there is a problem with this on the Hamiltonian lattice. That is because the degrees of freedom of links of the lattice lie in the group manifold, not a nice linear space.

16. **Chris Oakley**
November 6, 2008

Just to say that I approve of this kind of post. Not being a mathematician, I expect to be left behind soon, although this has not happened yet.

It is funny that with the existence of half-integral spin, SU(2) and not SO(3) is the symmetry of nature (equivalently SL(2,C) and not O(3,1)). It suggests that Minkowski space is not the fundamental thing, and yet theories that take advantage of this (I am thinking in particular of twistors here, where the fundamental objects live in SU(2,2), the double cover of C(3,1)), although elegant, never seem to help in describing the real world.

17. **FNesti**
November 6, 2008

Thanks Peter,

yes I also find this quadruplication of 'spinors' (e.g. in flat 4D) important, although it is completely unknown to physicists.

What I was (and is) curious about are the precise properties under possible symmetries (loocal frame rotations but not only?) of these copies. Probably one will have to go through the work of Benn, Tucker et al…

(Indeed there was a burst of activity on Kahler spinors around the eighties, while now people are much less imaginative (or should one say realists?)).

18. **Aaron Bergman**
November 6, 2008

It is funny that with the existence of half-integral spin, SU(2) and not SO(3) is the symmetry of nature (equivalently SL(2,C) and not O(3,1)).

The point is that physics deals with projective representations. The half-integral spin reps or SU(2) are perfectly good projective representations of SO(3). You can prove that for a (compact?) f.d. Lie group, any projective rep is an honest rep of the universal cover. This isn't true in general, however, and that can show up in certain situations.

19. **N. Nakanishi**
November 6, 2008

Concerning the definition of "symmetry"

The question raised by Kurt can be answered in the following way.
Suppose that the operator algebra of fields has been found. If a transformation U leaves it invariant, U is a symmetry.
In the canonical formalism, since the operator algebra is determinded by field equations and equal-time commutation relations, one can check the invariance without constructing the operator solution explicitly.

Concerning the problem of Gribov copies

In the BRS-formulated covariant operator formalism of gauge theories, there arises no problem concerning the topology of the
gauge fixing. The BRS generator can be proved to be nilpotent. The problem of topology is encountered in the path-integral formalism, because it is based on classical quantities. One should note that there is no justification of the non-perturbative path-integral approach; indeed, in that framework, one cannot discuss the unitarity of the physical S-matrix.

20. **anonymous**
November 6, 2008

@Prof. Nakanishi
I think you're the man who introduced the Nakanishi-Lautrup field in the quantization of the Maxwell field, is it correct? If so, it is a great honor to have you here!

21. **N. Nakanishi**
November 6, 2008

anonymous:

Yes. Thank you for your courtesy.

22. **Kurt**
November 6, 2008

Re: Nakanishi-

I wonder, however, if such a definition can be made without additional structure beyond a Hilbert space and a Hamiltonian. If we are dealing with a generic quantum system, not necessarily a field theory and not necessarily possessing an algebra of field operators, is there still a good notion of symmetry?

KH

23. **Domenic Denicola**
November 6, 2008

This is very cool; thanks Peter! So nice to have a mathematician doing the talking after half a term of a physicist teaching us "quantum mechanics" (i.e. linear algebra) here at Caltech :-|.

24. **Peter Orland**
November 6, 2008

It is not correct that Hamiltonian BRST quantization solves the Gribov problem. If the Hamiltonian gauge theory is ultraviolet-regularized in a gauge-invariant way (and the only way to do this, except in some special cases, is on a lattice), there is no nilpotent BRST operator.

From what I understand, Hamiltonian BRST is fine for dealing with theories perturbatively, but that's it.

25. **Coin**
November 7, 2008

Peter, thanks, this is actually really helpful. I have a couple of questions that I hope aren't too stupid.

When G is a Lie group with Lie algebra, (frac-g, [•,•]), differentiating ∏ gives a unitary representation (π, H) of g on H.

I'm a little confused as to why this is– I.E. why it is exactly that differentiating ∏ happens to produce this thing that you want, or exactly what it means to "differentiate" this ∏ object.

Examining the wikipedia article for lie algebras, they mention: "Given a Lie group, a Lie algebra can be associated to it… by endowing the tangent space to the identity with the differential of the adjoint map." In linked articles they describe the "adjoint map" as something that sounds extremely similar to the ∏ map you define and which they use to construct an "adjoint representation" of a lie algebra; and they define "differential" in this context as a particular operation they also call a "pushforward". Is this in fact what is happening here? I.E. is it the case that ∏ is the same thing as the adjoint map of your symmetry group G, and the reason why differentiating ∏ gives a representation of frac-g is because this is just the canonical way of getting a lie algebra's "adjoint representation" given the associated lie group?

(By the way, a minor thing– you say "a unitary operator ∏(g) satisfying [conditions], i.e. the map P from group elements to unitary operators is a homomorphism". Is the

"P" here a typo for ∏?)

An interesting philosophical question is whether the quantum system that governs the real world is purely determined by symmetry, i.e. such that ALL its observables come from symmetries in this manner.

Are there any known examples of quantum observables that are not specifically known to come from symmetries?

Thanks!

26. **Kasper Olsen**
November 7, 2008

Hi Peter,

In mentioning different groups of symmetries, you need to say what the group action is; just to be more precise..

27. **Chris Oakley**
November 7, 2008

Aaron,

I am not quite sure what you are saying here … a 2π rotation leaves Minkowski space invariant, but does not leave a spinor invariant, yet a spinor is a necessary part of physics as we understand it. It suggests that requiring a unitary rep of the isometry group of Minkowski space is not quite the right thing to do, and that there is something more fundamental lurking beneath.

28. **Marco Frasca**
November 7, 2008

Dear All,

About Gribov copies and Yang-Mills there is a quite interesting situation both from theoretical and lattice point of views. There are two ways people managed confinement and these were Gribov-Zwanzinger and Kubo-Ojima scenarios. Both scenarios, that imply that Gribov copies are relevant in the infrared, require the gluon propagator going to zero at lower momenta and the ghost propagator going to infinity in the same limit faster than the free particle one. Functional methods devised in the '90 seemed to support such a view.

Of course, several other people turned out to computers to see if these views are true. These computations, done on lattices having huge volumes (till (27fm)^4!), completely disproved above results showing as the gluon propagator goes to non-null finite value at lower momenta and the ghost propagator is the one of a free particle. Above confinement scenarios and functional methods that heavily rely on Gribov copies are blatantly wrong. The main conclusion to be derived from this is that Gribov copies do not play any role for Yang-Mills theory, not even in the infrared limit. Indeed, the concept of Gribov copies appears useless to our understanding of Yang-Mills theory.

About BRST symmetry and Yang-Mills theory one has the following conclusion recently drawn in

http://arxiv.org/abs/0810.1987 .

This symmetry is violated by the lattice solution that implies that the gluon acquires a mass. Indeed, lattice computations done so far show us the very existence of a mass gap and this mass gap has the effect to break BRST symmetry. This is rather a screening mass than a true pole in the propagator and this means that the true particles of Yang-Mills theory in the infrared are no more gluons but gluon bound states making the above mass a constituent mass (see also my blog

http://marcofrasca.wordpress.com/2008/10/14/a-new-point-of-view/ .

This is our current understanding about confinement and Yang-Mills theory. Some theoretical results have also been known that are in agreement with it (e.g. see my paper to appear in PLB

http://arxiv.org/abs/0709.2042

and refs therein).

This situation is quite interesting and shows clearly the dynamics of the understanding in physical science at work.

Marco

29. **N. Nakanishi**
November 7, 2008

Re: Kurt

Symmetry should be defined at the operator level, because symmetry may be broken spontaneously at the level of representation. Any quantum system is described by an operator algebra and its representation in terms of state vectors. Symmetry is the invaraince property of operator algebra. Hamiltonian is nothing more than a particular symmetry generator for time translation.

30. **Peter Orland**
November 7, 2008

Marco,

It simply isn't clear what the relevance of Gribov copies is to confinement and the mass gap. We won't know until confinement and the gap are understood.

No calculation or theorem indicates confinement at weak bare coupling (strong bare coupling has been understood since the mid-70's). Ruling out a proposed scenario invoking certain concepts does not mean those concepts are not relevant. We simply don't know whether they are relevant. None of the scenarios proposed to explain confinement have been successful (including some I have worked on). That doesn't mean that the ideas have no merit, just as it doesn't mean that they do.

The best that theorists can do on tough problems, is to study what they find interesting, follow hunches, prove or disprove hypotheses, etc. But we also have to be intellectually honest; we have to be prepared to abandon our most cherished ideas when they don't work.

But anyway, I don't think that Peter W. is after confinement per se, just a better understanding of gauge theories and BRST.

31. **Marco Frasca**
November 7, 2008

Peter,

This is the work of an entire community and I was thinking that some of these results were leaking out to a lot of people outside. By the way you are speaking it seems like all this people's work went out useless. Do not you believe on lattice results for Yang-Mills propagators? It seems to me that the a clear picture is emerging by the work of this people and its acceptance may be just a matter of time. Meanwhile, I am trying to let their (and mine) work widely known. But, of course, your considerations seem simply out of track.

Marco

32. **woit**
November 7, 2008

Coin,

Thanks for pointing out the typo.

Many quantum observables don't come in any known way from a symmetry. For example, in a gauge theory, take the field-strength operator (or some gauge invariant quantity constructed out of it).

The way I stated the relation between representations of the group and the Lie algebra is standard, if a bit abstract for most physicists. The point is that a very good way to think of a representation of a group is as a map of groups

[tex]\Pi: G\rightarrow GL(n,\mathbf C)[/tex]

that is a homomorphism of groups. For a unitary representation, the map is to a unitary group [tex]U(n)[/tex]..

Such a homomorphism is a differentiable map, its differential is a map from the tangent bundle of [tex]G[/tex] to the tangent bundle of [tex]U(n)[/tex]. Evaluating this at the identity of the group gives a homomorphism of Lie algebras, from the Lie algebra of [tex]G[/tex] (which can be identified with the tangent space at the identity) to the Lie algebra of [tex]U(n)[/tex] (which can be identified with both the tangent space to [tex]U(n)[/tex] at the identity, and the space of n by n skew-hermitian matrices).

The above applies to any representation, What you're quoting from Wikipedia is how one constructs a very specific representation, the adjoint represention of a group on its Lie algebra. One way to do this is to look at the conjugation map of the group to itself

[tex]g_0\in G \rightarrow gg_0g^{-1}\in G[/tex]

and take the differential of that. For each element [tex]g[/tex] of the group you get a linear map of the tangent space at the identity to itself, this is the adjoint representation of the group on its own Lie algebra.

33. **Peter Woit**
November 7, 2008

Marco,

Please, don't try and turn this into a discussion of your favorite ideas about confinement. Peter Orland is right that the question of confinement is not one that I'm trying to address here, it involves a whole host of very different issues.

My point of view the significance of BRST is that a better understanding of its mathematical context can lead to new ways of using it to understand physics. I don't have any particular reason to think that this will help with the understanding of confinement. More likely would be some new insight into electroweak symmetry breaking, where the issue of how gauge symmetry gets handled is more directly relevant.

34. **Peter Woit**
November 7, 2008

Nakanishi,

Many thanks for your comments. The point that the right way to think about symmetries of a quantum system is as the group of automorphisms of the algebra of operators was especially helpful to me.

35. **Marco Frasca**
November 7, 2008

Peter,

Sorry for my improper posting but some people turned your very fine argument into something about Yang-Mills and Gribov copies and for this there exist a lot of lattice computations. I just aimed to answer to this.

So, I turn back into my little corner.

Marco

36. **Peter Orland**
November 7, 2008

Hi Peter,

I had a minor question concerning your formalism. Are you concerned about unbounded operators and non-normalizable states in all of this? The reason I ask is that you are defining states the mathematician's way as Hilbert space vectors, which square-normalizable. Depending on the manifold you start with, you might need to deal with a spectrum defined more generally than eigenvalues on such states, e.g. by singularities of resolvents.

If you work on a bounded manifold (such as a Lie group), there is no need to worry about this issue since the entire spectrum consists of eigenvalues of Hilbert vectors.

But I assume you eventually want to consider field theory in the thermodynamic limit, which means the spectrum contains numbers which are not eigenvalues (or in the physicist's language, not eigenvalues of normalizable states).

I suspect you've already dealt with these functional-analytic considerations, or they are just not important. I was just wondering…

37. **Serifo**
November 7, 2008

Hi professor Peter,

I suppose the considered Hilbert space is a separable Hilbert space, right ? Or it doesn`t really matter ?!

I also suppose you are assuming the Hilbert space as a complex vector space (right ?), now since the field of complex numbers is an extension of the field of real numbers, the same Hilbert space can be viewed as a real vector space . Does it help when we view the space as real Hilbert space ?

Sincerely

38. **N. Nakanishi**
November 7, 2008

Peter Woit:

There is no nilpotent selfadjoint operator defined on a Hilbert space. If you discuss the BRS generator in your next presentation, I expect that you will introduce an indefinite-metric Hilbert space. Is that so?

39. **Hendrik**
November 7, 2008

Dear Peter, I agree that it is high time that there is some serious discussion on quantum constraints;- there is a wide spectrum of different methods, not all of them equivalent. A few reactions to specific points:

1) The initial framework which you propose, of operators on a Hilbert space, binds you to a specific representation. It is better to start with an abstract algebra (e.g. a C*-algebra) as your setting, and then to select the convenient representations. For instance, as Peter Orland pointed out, in some representations you cannot solve your constraints due to continuous spectrum problems. There is a discussion of these points here (Erratum here) In any case, a constraint algorithm constructs a different representation for the observable subalgebra, than the starting representation.

2) There is not one BRST-formalism, but several, of which the two major approaches are the BFV-approach (as in the book by Hennaux and Teitelboim) and the Kugo-Ojima approach. Whilst these are superficially similar, they are in fact inequivalent, i.e. produce different results for the same system (I have a PhD student whose thesis contains substantiation of this). Moreover, the BFV-approach is a general constraint algorithm (so does not need a gauge group action to be applied), whereas the KO-approach is specifically written for gauge theories, so we don't know how to apply it to general constraints.
It is not difficult to show that the BFV-approach produces different results than the usual Dirac constraint method (see Sect 7 of this old scanned preprint -11MB, sorry). On the other hand, for Quantum Electromagnetism (the only gauge QFT which we can handle explicitly in the C*-algebraic framework), the KO-approach to BRST produces the correct results.

3) A common problem of the BFV-BRST method is that it selects multiple copies of the physical state space (this has been noted all over the literature, e.g. p50 of Van Holten, and I also found it in my old preprint above). This should not be confused with the Gribov ambiguity problem, which as I understand it, is a classical and global result, stating that there is no continuous section of the gauge orbit space of the connections (as the set of orbits of the connections is a principal fibre bundle, I think this just means that this bundle is nontrivial). Since the operator theory with which BRST works is local, infinitesimal (not global) and "forgets" the smooth structures of the classical objects, I cannot see how it can detect a Gribov problem. Of course, it is a different matter for the path integral approach, which starts from the classical theory. Having said that, I know that McMullan did some work on BRST and Gribov ambiguity, using Mackey quantization and analogies (as the groups are infinite dimensional, Mackey theory does not apply directly).

4) Serifo's question is an interesting one;- if you want to avoid continuous spectrum problems, you may need to use representations which are discontinuous w.r.t. the exponentiated commutation relations ("nonregular representations") and these must be on nonseparable Hilbert spaces. However, the final constrained Hilbert space must always be separable.

40. **Peter Woit**
November 7, 2008

Thanks to all for the interesting comments. The questions about BRST that I'm interested in are independent of the analysis issues people raise, they occur even for toy models with finite dimensional state spaces.

The state spaces in question are complex vector spaces, QM requires this. Similarly in representation theory I'll be discussing complex representations. Representations on real vector spaces don't give the examples and structures I'm interested in.

The question Nakanishi raises is an important one: having a self-adjoint BRST operator with square zero requires an indefinite metric. The Dirac cohomology idea I've been working with evades this, since the analog of the BRST operator has a square that is not zero, but a central element in the operator algebra. So, it acts as zero on the algebra of observables, but is not zero on states. This is one of the main reasons I think there may be something new here.

Hendrik is right that there are multiple versions of "BRST", and I've always found keeping them straight and understanding the relations between them incredibly confusing. What I'll be writing about is yet a different variant. It's not one that can be applied to any constrained dynamical system, but is intended to work for gauge theories.

41. **Hendrik**
November 8, 2008

Dear Peter, if your BRST-variant is intended for gauge theories, then Krein representations (indefinite metric) are required for reasons other than BRST (unless you use the exponentiated C*-algebra version with nonregular representations). The reasons come from a whole swag of theorems:- (1) Strocchi, F.: Gauge Problem in Quantum Field Theory, Phys. Rev. 162, 1429-1438 (1967) – there is no representation of the vector potential (i.e. local gauge field) which is covariant w.r.t. the usual action of the Poincare group, having a vacuum and satisfying the Maxwell equations, in which the fields (i.e. local curvature) are nonzero. Also see Comm. Math. Phys. 35, 25 (1974), and other Strocchi papers at this time or earlier. (2) Wightman, A.S., Garding, L.: Arkiv Fysik 28, 129 (1964) a covariant representation of a vector potential which is weakly local, cannot be a normal Hilbert space representation, it must be done w.r.t. an indefinite inner product. (3) Barut and Raczka proved that the only zero mass representation of the Poincare group on tensor-valued functions (e.g. Fock rep) must necessarily have indefinite metric.

These theorems meant that everyone in Wightman Field Theory, did their gauge fields in the indefinite metric framework.
The only way in which you can avoid using indefinite metric, is by using the exponentiated (Weyl) fields in a C*-algebra framework. Then you can work with ordinary Hilbert space representations, but they must be necessarily nonregular to avoid the theorems above. Nevertheless, you still get sensible results after constraining. You can read about that approach here. Unfortunately, the only gauge field for which we have an explicit C*-algebra is the Electromagnetic field.

42. **Urs Schreiber**
November 8, 2008

> The point that the right way to think about symmetries of a quantum system is as the group of automorphisms of the algebra of operators was especially helpful to me.

By the way, the essence of this is extracted by the Doplicher-Roberts reconstruction theorem applied to the symmetric monoidal category of local endomorphisms of the net of local quantum observables:

the theorem proves that just from the net of local algebras alone the global symmetry group of the QFT can be reconstructed, hence in particular its particle content be determined.

The DR-reconstruction theorem is one of the earliest instances where physicists found in their application a fundamental purely category-theoretic fact.

43. **tomate**
November 8, 2008

I'm sorry for the down-to-earth question in such elevate discussion. Is every symmetry of a quantum system connected to a conserved quantity or charge (as in your first few examples)? If so, does the U(1) global symmetry uniquely imply conservation of probability and vice-versa?

44. **Chris Oakley**
November 9, 2008

Tomate,

Yes, every symmetry has a conservation law, a result known as Noether's theorem. This is a classical result, but it applies also to quantum field theory, and is the only thing I can think of in elementary QFT that is named after a woman. U(1) global symmetry, if present, may be connected with charge conservation but has nothing to do with conservation of probability. One of the reasons for requiring the inner product or metric of Hilbert space to be positive definite in the foregoing discussion is that these quantities sometimes have the interpretation of probabilities, and probabilities cannot be negative.

45. **Peter Woit**
November 9, 2008

Tomate,

The point Kurt was asking about and Nakanishi clarified, is that I'm thinking of a "symmetry" more generally, as any automorphism of the algebra of observables (i.e. any map from this algebra to itself that preserves the algebra structure). Of these "symmetries", some will commute with the Hamiltonian operator, some won't. The ones that commute with the Hamiltonian are the ones that correspond to conserved quantities. Ones that don't commute with the Hamiltonian are sometimes given names like "dynamical symmetry", or "spectrum generating algebra". A few seconds of Googling turned up this more detailed explanation:

http://eom.springer.de/S/s110230.htm

As Chris explains, conservation of probability comes from the condtion that the Hamiltonian be self-adjoint, generating a unitary time evolution. The U(1) action corresponds to a charge, which may or may not be conserved.

46. **Hendrik**
November 9, 2008

Dear Peter, I think the concept of "symmetry of a quantum system" is not an easy one to sort out:
In the literature the concept is defined in a number of inequivalent ways:(1) As a continuous map on the projective sphere of a Hilbert space, preserving (moduli of) inner products. The content of Wigner's theorem is that these are either unitaries or antiunitaries, and in the case of a continuous one-parameter group of them, then they must be unitaries.(2) As automorphisms of the *-algebra A of observables (where A can be either abstract, or concrete operators on a Hilbert space H – if they are unbounded, a dense invariant domain must be specified).(3) As superderivations, e.g. in the case of BRST or supersymmetry.

The mathematical structure of collections of symmetries vary, e.g. they can be groups (discrete, Lie, infinite dimensional) or they can even be quantum groups, which are not groups at all. In the case that they are Lie groups, then of course the infinitesimal generators (selfadjoint operators or *-derivations) encode the symmetry group.

A symmetry is relative, i.e. it is a symmetry of *something*, and that something must be specified. An algebra A of operators on a Hilbert (or Krein) space by itself is not a physical system (different quantum systems have isomorphic algebras of observables). Some extra structure must be specified, together with some map that takes you from the labels in your theory to something outside, which can e.g. be measured in experiment. That is why we use suggestive labels like "mass" or "charge" for our mathematical objects. Then a symmetry is a transformation of the system which leaves the defining structures of the system invariant. So for instance, a "dynamical symmetry" is a transformation which leaves some specified one-parameter time evolution group invariant, and a "relativistic symmetry" is something which e.g. respects causality requirements. Even elementary descriptions of symmetry, include such a requirement. I am very uncomfortable with a "radical" interpetation of symmetry as *any* automorphism of your algebra of observables (though it appears in the literature). This seems to me without physical content.

The discussion regarding Tomate's question above, is not quite complete:If you have a continuous one-parameter group of symmetries in the Wigner sense above (they preserve probability), then it is given by a continuous one-parameter group of unitaries t -> U(t), and by Stone's theorem this must be of the form U(t) = exp(itS) for some selfadjoint operator S. If you assume that you have a time-evolution which commutes with the symmetries, then S is a conserved quantity, i.e. does not change in time. However, if you start from a discrete group or a discontinuous action, then there is no such conserved quantity. This picture becomes much more problematic if you view your symmetries as automorphisms on the algebra. For instance, you need not have that it is unitarily implemented, so even if you have a continuous one-parameter group, there need not be a concrete generating operator. (At the most, you have a *-derivation of the algebra).

47. **Thomas Larsson**
November 10, 2008

It seems to me that this discussion is closely related to the eternal confusion about diffeomorphism symmetry in GR. From one POV, every model has a diffeo symmetry because it can be written in curvilinear coordinates, and hence the concept is empty. By the same token, every (massive or massless) model has scale symmetry; after all, the scaling group is a subgroup of diffeomorphism group, since the dilatation D = x^u d/dx^u is a particular kind of vector field.

From this viewpoint, the difference between massive and massless is that the representations of the scaling algebra are qualitatively different. In the massless case, the Hilbert space is a direct sum of one-dimensional reps, spanned by a vector |h>:

D |h> = h |h>.

In the massive case, we need to look at a family of models at different scales, and the reps act on an infinite-dimensional vector space with basis |n>:

D |n> = |n+1>.

Although this viewpoint on massive models is not necessarily wrong, it is not fruitful neither; unlike in the massless case, we don't learn anything about the model by pondering dilatations.

More generally, assume that we have a big group G, a subgroup H, and an H rep R acting on our Hilbert space. Then G also acts on the Hilbert space, possibly after we have completed it to include new states. The G rep is necessarily a suprep of the induced rep Ind R. If the G rep is all of Ind R, considering G is fruitless; all information is already encoded in H. OTOH, if the G rep is a proper subrep of Ind R, we learn something new, and this viewpoint is fruitful.

This suggest one definition of "symmetry". Let G be the full group of automorphisms on the Hilbert space, and let H be the biggest subgroup such that the rep of G is induced from H. Then H is a "symmetry", in the sense that it is the largest group that says something nontrivial about the Hilbert space.

48. **tomate**
November 10, 2008

Hendrick,

> However, if you start from a discrete group or a discontinuous action, then there is no such conserved quantity.

I know this is a typical statement about discrete symmetries, but couldn't we put it this way: symmetry is an unobservable degree of freedom (as assumed in Wigner's theorem) and conservation laws (of continuous quantities like momentum or discrete charges) are their observable realization. A discrete example: inverting spatial axis is unobservable but has a corresponding conserved quantity, parity, which you can infer from experiment (up to conventions) and might be conserved in a process if the theory has such symmetry. I know it sounds a little bit tautologic, but it seems to me like a coherent view. Does this make any sense?

Peter and Chris,

thanks for explanation.

49. **Peter Woit**
November 10, 2008

Thomas,

I'm confused. You're starting with an H rep, for H a subgroup of G. There's the induced G rep, but what's the other one you have in mind? Can you give an example?

50. **Thomas Larsson**
November 10, 2008

1. The above scale symmetry of a massive model, on the group level. G = scaling group, H = trivial group, R = trivial rep.

2. Give the massive model a conformal symmetry: G = conformal group, H = Poincare group, R = a Poincare rep.

3. Make a model diff symmetric: G = diffeomorphism group, H = Poincare or conformal, R = a rep.

The last example is similar in spirit to saying that any model has a diffeomorphism symmetry because it can be written without coordinates, but not exactly the same, I think.

51. **Shantanu**
November 10, 2008

Peter, this looks interesting.
Do you have any students or colleagues working with you on this?

52. **Hendrik**
November 10, 2008

Thomas,

I'm afraid I don't follow your reasoning;- it seems you are proposing a different definition of "symmetry" but I cannot see what job your symmetry is supposed to do:

> "This suggest one definition of "symmetry". Let G be the full group of automorphisms on the Hilbert space, and let H be the biggest subgroup such that the rep of G is induced from H. Then H is a "symmetry", in the sense that it is the largest group that says something nontrivial about the Hilbert space."

Is the "full group of automorphisms on the Hilbert space" all of U(H), or is it the unitaries which implements the largest set of aotomorphisms of your algebra of observables A? In short, given A how do you get G?

"…it is the largest group that says something nontrivial about the Hilbert space". Since all Hilbert spaces of the same dimension are isomorphic, I cannot imagine what such a nontrivial property might be. Do you mean the rep of A on it?

Maybe I was a bit unclear earlier on;- in a nutshell, I think a symmetry is a transformation of the algebra of observables with a job to do. That job is to preserve the defining physical structures of the system. These strucures can be external to the algebra, e.g. a distinguished group action, or some map from space-time regions to subalgebras encoding causality/covariance etc. In this case, not all automorphisms of A can be symmetries.

Tomate,

Let me first answer the mathematical side of your question:
A "conservation law" is an additive quantity;- e.g. you add up all the momenta of a set of interacting particles, and if the total set of particles is free, then the total momentum is conserved. That is why we take the generators S of the unitary groups U(t) = exp(itS) of the symmetry groups to get our conservation laws, since by the

exponential law we have U(t)U(t') = exp(i(t+t')S). Parity is a multiplicative quantum number, not an additive one, so not a conservation law in the usual sense. Of course, given a set of (anti)unitaries implementing a discrete symmetry, commuting with time evolution, then it is easy to make conserved observables (staying constant in time);-just take any selfadjoint element in the *-algebra generated by the (anti)unitaries. However, these do not give you a conservation law in the usual sense for your original discrete symmetries.

We need a philosopher to sort out "symmetry is an unobservable degree of freedom" but let me have a stab at it. I think "to observe" is relative, and one should be clear where your observer is. Moreover you should also distinguish between applying your symmetry to to subsystems from application of it to the full system.
E.g. translation of a subsystem is an observable transformation (and physical thing to do) for an observer in the full system, but it is unobservable for an observer inside the subsystem (who by definition cannot see anything outside his system). Hence we can measure momentum of subsystems of our world, but we cannot measure the total momentum of the universe.
Thus concerning parity, we can measure it for subsystems of our world, but (if it is a symmetry for those systems), of course it is unobservable from within those systems.
By the way, parity is broken by weak interactions, so perhaps that was not the best example for your point

53. **tomate**
November 10, 2008

Hendrick,

maybe this is just a philosophycal hence not scientific matter and this by definition is no place for unscientific stuff, so forgive me all. The sentence "symmetry unobservable" is uncritically stuck in my mind due to many sources none of which I'm able to mention at the moment (but could do with a little research): they might not be very authoritative (aside: can we think of a quantum experiment that tests time invariance without appealing to energy conservation? If so then I'm easily wrong). We can better say that symmetry is what leaves transition probabilities unaltered, as is in the spirit of Wigner theorem.
I just notice that symmetry is rather abstract (lagrangian invariant under such and such transformation), and conservation very practical (something calculable before experiment = something calculable after). I'm just not very comfortable with the idea that multiplicative quantum numbers are excluded from the Gotha of conserved quantities, yet sharing this very intuitive picture. You yourself put, for instance, parity and momentum conservation on the same footing in your example.
I guess it's just a matter of convention of no conceptual importance. It would just be be more practical to say: all symmetries lead to conserved quantities. But it doesn't seem to be worthy a referendum.
(I do know parity is an unfortunate example – but it's the only one I remember how to calculate)

54. **Thomas Larsson**
November 11, 2008

Hendrik

Symmetry is perhaps not a good word, since it has an established meaning (commutes with the Hamiltonian). But what I am saying is that it is a good idea to look at the action of ever bigger groups on the Hilbert space (or perhaps an enlarged Hilbert space), as long as this gives you new information about your system. You should stop at the largest group H such that the action is not induced from some subgroup, because for bigger groups all info is already encoded in H.

Main example: CFT in 2D. The infinite conformal symmetry G is generated by L_m = z^(m+1) d/dz. Denote by H the non-positive (m &lt= 0) subalgebra. One may argue that only H is a symmetry (note that the Hamiltonian is not known), because the positive generators diverge at infinity and thus violate boundary conditions. The Hilbert space carries a direct sum of one-dimensional reps of H, characterized by L_0 |h> = h |h>, L_-m |h> = 0.

We can extend this action to an action of all of G, at the cost of adding new states to our Hilbert space. If the action of G on the enlarged Hilbert space were induced from H, i.e. a Verma module, this exercise is pointless; essentially all info about the Verma module already lives in H. But for physically interesting systems (minimal models), the action is something different: the Verma module modulo singular vectors. This yields new info which is physically important: the discrete spectrum of critical exponents. So in this case, going from H to G is a good idea, even if it does not commute with the (unknown) Hamiltonian.

And yes, I have glitched over some details such as the central charge.

55. **Fabien Besnard**
November 11, 2008

Aaron said : "The point is that physics deals with projective representations. The half-integral spin reps or SU(2) are perfectly good projective representations of SO(3)."

Couldn't we take the POV that (quantum) physics deals with algebra representations instead ? The natural/"real"/important object here would be the lie algebra of SO(3) = Lie alg of SU(2). This POV would also naturally leads to quantum groups as a possible generalization.

56. **Peter Woit**
November 11, 2008

Shantanu,

The BRST project is just me.

57. **Aaron Bergman**
November 11, 2008

Couldn't we take the POV that (quantum) physics deals with algebra representations instead ? The natural/"real"/important object here would be the lie algebra of SO(3) = Lie alg of SU(2). This POV would also naturally leads to quantum groups as a possible generalization.

Just looking at the Lie algebra doesn't go far enough. You need to look at central extensions of the Lie algebra. It so happens that central extensions of semisimple Lie algebras aren't particularly exciting, but there's more to life than semisimple things. Like loop groups, say.

58. **AM**
November 14, 2008

Thanks for the post. More posts along these lines would be great !

59. **Gen Zhang**
December 9, 2008

I realise I'm coming a bit late to this party, but:

You state, as most elementary texts do, that symmetries are implemented as a unitary representation of the group on the Hilbert space. However, it is important to realise that this is often not possible; as has been alluded to by Aaron, we really care about projective representations. Usually at this point the discussion veers off into the rough, but I'd like to bring up an elementary example: a single 2D electron in a uniform magnetic field. The system is obvious symmetric wrt. translations in the plane, but it is also easy to see (Aharonov-Bohm effect) that it is not a proper representation on the wavefunction.

I have rarely seen this issue being discussed — perhaps because the theory of projective representations is not as advanced yet. It is still surprising the number of physicists who are completely unaware of this issue though.

60. **Peter Woit**
December 9, 2008

Gen Zhang,

The point about projective representations is an important one, which I was avoiding for simplicity here. It will appear when I try and write something about affine Kac-Moody Lie algebras and their representations. Another way of dealing with this is to say that, instead of a projective representation, one ends up having a representation of an extension of the symmetry group one started with.

61. **Blair Smith**
January 24, 2009

No fair PW! You hang that philosophical comment (before "Some Examples") out there like a musical note in tension without providing a resolution 😉

Maybe I need to read further on to see where you are going, but might it be possible to give that idea more teeth by say associating (via some ontological-like constraints on possible models of reality) classes of theories that alternatively do (do not) have all observables arising from symmetry groups with unitary reps on H?

Short Bits

November 11, 2008
Categories: BRST, Experimental HEP News, Multiverse Mania

More about BRST is on its way, but in the meantime a lot of things have accumulated that might be of interest, so I wanted to do a quick posting about these.

One of them does have to do with BRST. A correspondent pointed out to me that the 2009 Dannie Heineman prize for Mathematical Physics has been awarded to the four people involved in the original discovery: Carlo Becchi, Alain Rouet, Raymond Stora, and Igor Tyutin.

Via Garrett Lisi, there's this collection of photos of the latest Threeasfour collection. It seems that E8 is inspiring not just physicists.

Over at the n-category cafe, John Baez has a posting about the remarkable publication record of M. S. El Naschie.

On the experimental HEP front, it looks like the LHC will not be trying again to commission beams until next summer. Minutes of a recent meeting about LHC work are here, an outline of a schedule here.

SLAC recently hosted an ICFA seminar, with talks available here summarizing the state of various current and proposed accelerator projects. Prospects for a photon-photon collider are discussed here.

For the latest on the CDF anomaly, Tommaso Dorigo has started a series of detailed posting on the analysis here and here. Matt Strassler has a new paper out about this, including some discussion of possible interpretation of the results in terms of the hidden valley scenario. For more about this topic, see a recent posting at Resonaances.

There's a new popular book out about particle physics, Nature's Blueprint: Supersymmetry and the Search for a Unified Theory of Matter and Force by Dan Hooper. It's a rather breathless account of how physics is about to be revolutionized by the discovery of supersymmetry at the LHC, very much like Gordon Kane's 2000 Supersymmetry: Unveiling the Ultimate Laws of Nature. In Kane's version the LHC was supposed to start up in 2005 and soon discover supersymmetry, in Hooper's the LHC start-up is moved to 2008. One change since 2000: string theory played a big role in Kane's book, Hooper pretty much ignores it.

The December issue of Discover Magazine is out, with Hawking on the cover for a story about the "50 Best Brains in Science". Terry Tao and Edward Witten are on the list, and the magazine includes a nice appreciation of Witten by John Schwarz, who writes about his experience co-authoring a book on string theory with Witten, explaining that:

> Witten is both deep and fast: After thinkings through the ideas, he can compose an essentially error-free 100 page manuscript, often describing breakthrough original research, on his computer in a day. His papers and lectures set a new standard for clarity of exposition. And he shows no signs of slowing down.

This year, Witten is working at CERN, and there's a talk by him scheduled in the string theory seminar there next week, topic TBA. Maybe Jester will report on this.

In other Discover-related news, Cosmic Variance has announced that they have "sold out to the man", and will now be going corporate, signing up with Discover to be one of their blogs.

Also in the new Discover Magazine is a long article promoting the multiverse entitled Science's Alternative to an Intelligent Creator: the Multiverse Theory. The author's take on the story is that we really only have two choices: believe in God and intelligent design, or believe in the Landscape. He seems to have gotten this from Susskind:

> The physicist Leonard Susskind once told me that without a multiverse theory, there may be no other explanation for life other than intelligent design.

The author's note reports that the article came about through Templeton funding:

> For this issue, he traveled to Cambridge, England, as a Templeton-Cambridge Journalism Fellow in Science and Religion to learn what physicists have to say about how the universe seems custom-tailored to favor life.

In keeping with his theme, Folger quotes many proponents of the multiverse, and only one critic: John Polkinghorne, an ex-physicist and current Anglican priest who has motive to want to keep a role for God.

There's some rather out-there stuff at the end from Andrei Linde:

> As for Linde, he is especially interested in the mystery of consciousness and has speculated that consciousness may be a fundamental component of the universe, much like space and time. He wonders whether the physical universe, its laws, and conscious observers might form an integrated whole. A complete description of reality, he says, could require all three of those components, which he posits emerged simultaneously. "Without someone observing the universe," he says, "the universe is actually dead."

The History Channel is running a series on The Universe. Next week the multiverse is being promoted, in an episode Parallel Universes. Here's the summary:

> Some of the world's leading physicists believe they have found startling new evidence showing the existence of universes other than our own. One possibility is that the universe is so vast that an exact replica of our Solar System, our planet and ourselves exists many times over. These Doppelganger Universes exist within our own Universe; in what scientist now call "The Multiverse." Today, trailblazing experiments by state of the art particle colliders are looking for evidence of higher dimensions and Parallel Universes. If proof is found, it will change our lives, our minds, our planet, our science and our universe.

I learned about this from Clifford Johnson's blog. He'll be one of the physicists featured in the episode, as well as in the following one, entitled Light Speed. The next episode, Sex in Space, which will explore the "physiological, psychological and cultural challenges of sex in space" presumably will not be starring any theoretical physicists.

Update: It seems that selling pseudo-science with the argument "it's either this or religion" works.

Update: The links above to the LHC Performance Committee's site have now been closed to outside access. For the last few years the web-sites of the groups responsible for getting the LHC working have been open to the public, but it looks like there now has been a change of policy. The tentative schedule now inaccessible to the public showed that it is repairs to sector 34 that will determine when they can get going again. The process of getting damaged magnets out of the tunnel, making repairs, getting replacements installed, then testing everything, is what may delay everything into next summer.

Update: For some commentary on the Strassler paper, see Tommaso Dorigo here. Slashdot features the Discover article, promoting the idea that the string theory landscape is "Science's Alternative To an Intelligent Creator".

Comments

1. **Hendrik**
   November 12, 2008

   Sorry to be a curmudgeon about the Dannie Heineman prize for Mathematical Physics for Becchi, Rouet, Stora and Tyutin (and without detracting from the impact of their work), but why is this *mathematical* physics? The context in which they did their work was nonrigorous QFT, and whereas classical BRST has attained rigorous status, quantum BRST has only very partially done so.

   The distinction between Theoretical and Mathematical Physics is an old bone of contention;- I remember a public debate some years ago at the IAMP meeting in Paris on exactly this point. My preference is for a definition I heard from Thirring: Mathematical Physics is mathematics but the problems are from physics. This is also in several articles of Jaffe, or in the section "the bad influence of physics on mathematics," on p146 of this book.
   Thus it had to be rigorous to qualify as mathematical physics. Unfortunately, the debate at the Paris ICMP went in a political direction: "we should not criticise our colleagues in physics," and since then the consensus has also shifted in this direction, and now we have no word for mathematics work on physics problems. There are many instances of (nonrigorous) theoretical physics being called mathematical physics. It is just a pity to see the prizes doing this too.

2. **Tony Smith**
   November 12, 2008

   Peter, you say that "… photos of the latest Threeasfour collection …" of fashion indicate "… that E8 is inspiring not just physicists …".

   A google search indicates that E8 has not only transcended math and physics, but even beyond fashion,
   to a high-financial realm where no Lie group has gone before:

   According to a 14 August 2008 marketwatch.com story:

   "… E8.com, Inc. today announced that it has successfully acquired six premium numeric domains from Marchex, Inc. …
   "We are pleased to add these domains to the ever expanding, digital asset portfolio of E8.com," said T.J. Demas, Founder and CEO of E8.com, Inc.
   "Equally exciting is the fact that the close of the acquisition coincided with E8.com's initial incorporation on August 8, 2008 (8.8.8) in the state of California."
   E8.com, Inc.'s initial capitalization structure includes a 99% ownership stake by Aspen Edge Research, LLC and a 1% ownership stake by Moover Toys of Copenhagen, Denmark. Moover Toys initial 1% equity investment in E8.com, Inc. totaled $88,888.88 valuing E8.com, Inc. at $8,888,888.88 at inception. …
   E8.com's mission is to provide the most advanced, intuitive, user interfacing platform in the world based upon the unifying supersymmetry of the exceptional Lie group E8 and its corresponding 248 dimensions. …".

   According to a dnjournal.com article:
   "… You may have seen a cryptic news release a few days ago from Marchex and E8.com about E8 buying six numerical domains from Marchex. The prices paid were not disclosed – but we are going to release them for the first time in this column.
   As it happens I know E8 Co-Founder and President, T.J. Demas, an Aspen, Colorado based investor …
   Demas is, to put it mildly, a mathematics fan. His company is named after an E8, one of the most complicated structures ever studied … In a nutshell E8 is all about symmetry and Demas is applying symmetry to a series of numerical domain purchases for a project he is not yet ready to fully reveal.
   With his focus on symmetry, his purchases have followed a certain pattern and the prices paid have been thought out down to the last cent. This past week Demas completed the purchase of 11 domains totaling $1,144,444.30. Six of those were bought from Marchex and five more were acquired in private transactions. …".

   Peter, how much do you think that BRST.com might be worth?

   Tony Smith

3. **Hendrik**
   November 12, 2008

   Further inspection of the list of previous Dannie Heineman prizewinners shows that this particular prize was always given to both (nonrigorous) theoretical physicists and mathematical physicists, with a fair representation of string theorists. It seems to have a ratio of about 2 theoretical physicists for each mathematical physicist. (Perhaps because it is administered by the APS?)

4. **mathematician**
   November 12, 2008

   Re: Discover Magazine article "Science's Alternative to an Intelligent Creator: the Multiverse Theory."

   Why do people fall for these ridiculous false dichotomies? They basically take the form of "It's either my theory, or else it's this straw-man" when in reality, there are many more than just two competing explanations.

   I've even seen theists turn the tables by comparing their theism favorably with their chosen strawman, the anthropic landscape/multiverse (e.g. on an episode of "closer to truth" which is really terrible).

5. **Me**
   November 12, 2008

   Regarding sex in space, those interested in some experimental results caught on video should search for "The Uranus Experiment" (filmed in free fall).

   The results are not very appealing and without knowing in advance I for one would have not found anything special about them.

6. **a quantum diaries survivor**
   November 12, 2008

   Hi Peter,

   thank you for your reiterated signs of appreciation for my reports – you know I value your opinion at least as much as I value your links.
   The series on multi-muons will continue at least for three or four more posts, and possibly more, becoming the longest series on my blog. The third part, about secondaries from nuclear interactions, is out now.

   As for the LHC schedule: the repairs of sector 34 are indeed complicated. Many magnets need replacement, but quite a few more need to be checked. Moving stuff around in the LHC tunnel looks pretty much like that game invented by Sam Loyd (the unrivalled chess problem composer), "15".

   I heard unofficially stated that we will most likely get beams in September 2009. That really sucks, but on the other hand we are not watching idly. People are busying themselves in various ways. No publications, and no data for grad students to build a career on: these are really nagging facts. But to many, more time to refine tools is not the end of the world. My main concern ? This is moving my cashing the 1000$ bet against new physics at LHC too much in the far future!

Cheers,
T.

7. **Serifo**
November 12, 2008

I agree with prof. Hendrik in some points. However, currently I find it hard to define the term ” mathematical physics ” . Sometimes I have the impression that mathematical physicists are divided in following two groups 🙂 :

1) Those (real mathematical physicists ?) who are concerned with the physics but also with mathematical rigour of their ideas.

2) Those (pseudo mathematical physicist ?) who know and use the most advanced mathematical tools for physical purpose, but are not at all concerned in the rigour of their ideas .
You may like to read the following article by Eric Zaslow on the idea of physmatics :

http://www.claymath.org/library/senior_scholars
/zaslow_physmatics.pdf

Ps – The use of advanced mathematical tools in nonrigorous way in physics, may be the reason for the lack of progress in physics. So, maybe Hilbert had a point when he said physics is too difficult for the physicists ! 🙂

8. **Peter Woit**
November 12, 2008

Hendrik and Serifo,

I think the term “mathematical physics” has always been rather vague, covering several different kinds of activity, and it has just become more so in recent years. The Heineman prize appears to have historically gone mostly gone to physicists, often ones without much interest in mathematics.

After spending a lot of my life among both mathematicians and physicists, I’ve seen that:

1. Mathematicians often see physicists as not only missing the importance of rigorous proof, but sometimes being unwilling to even formulate clearly what they are doing, and keep straight the distinction between what they do understand and what they don’t understand. From a mathematician’s point of view, this can make it very hard to make progress.

2. Physicists often see mathematicians as overly concerned with pedantic issues, focusing on technicalities needed to achieve absolute rigor, or on empty formalism.

I tend to often agree with both sides about this, but what happens is that the best people in each subject manage to rise above the problems of their subject. At the moment, it seems to me that mathematicians are in much better shape, making good progress, while maintaining high levels of rigor. Theoretical physics is not in such good shape, with even the best people making little progress. The problems are hard, and a lack of clarity about exactly what they are, what is understood and what isn’t, has a lot to do with this.

9. **MathPhys**
November 12, 2008

Thank you for the link to “The case of M S El Naschie”.

I searched around a bit and found an e-mail that Paul Ginsparg wrote re one of El Naschie’s collaborators, Carlos Castor. Highly recommended for entertainment value.

10. **MathPhys**
November 12, 2008

I meant Carlos Castro. A link to Ginsparg e-mail is here

http://www.archivefreedom.org/freedom/Cyberia.html

Can someone please take a look at these photos

http://www.ijnsns.com/conf/Photo-Gallery.pdf

Some seem to have been produced by photoshop

Look at the photo with Wilczek, ‘t Hooft and Gross

http://www.el-naschie.net/el-naschie-physicist.asp?site=249&lang=

Looks very suspicious to me.

11. **Serifo**
November 12, 2008

Hi prof. Peter,

Let me take your interesting observation : ” At the moment, it seems to me that mathematicians are in much better shape, making good progress, while maintaining high levels of rigor. Theoretical physics is not in such good shape, with even the best people making little progress. The problems are hard, and a lack of clarity about exactly what they are, what is understood and what isn’t, has a lot to do with this. ” Well , maybe theoretical physics is in crisis at foundational level as mathematics was in the early 20th century . Some Mathematician like Hilbert for example were not happy with the state of the subject itself, thus they started to put the subject in a firm (axiomatic) basis . So maybe theoretical and mathematical physicists need to do the same, specialy those who already have permanent academic positions and so are not concerned on spending 3 or 5 years to put the subject in a firm basis even if they fail ! 🙂 Now as in mathematics, where some fundamental concepts like Set had to be carefully formulated, maybe the same has to be done with some fundamental physical concepts !

12. **Peter Woit**
November 12, 2008

MathPhys,

I don't see any real evidence of photoshopping. El Naschie seems to have some sort of talent for getting ahead in the world, and I don't think that attending a conference featuring some Nobel prize winners and getting in a picture taken of them would be all that much of a challenge.

13. **Peter Woit**
November 12, 2008

Serifo,

I don't really think an axiomatic framework is what's important. But I do think the question of what the right fundamental ideas are, and what their best mathematical incarnation is, are still open questions for fundamental physics. In recent years I've started to think there's a lot more to BRST than just a solution to a technical problem…

14. **Kea**
November 12, 2008

MathPhys, your link to the Castro story is solid evidence AGAINST the photos being photoshopped. Perhaps you should read what you link to before you post something.

15. **Hendrik**
November 13, 2008

Dear Peter,
I have also experienced those contrasting views which mathematicians and physicists have of each other. I think the reasons for these (occasionally) hostile views are sociological and economical. Theoretically, there should be no conflict between the two fields;- physicists are good at discovering patterns in the physical world, and should not be slowed down by being chained to rigour. Mathematicians on the other hand, are good at sorting out the exact logical frameworks, extensions and applications for those patterns, and need rigour in order to have certainty in their results. These are complementary activities, you need both to create a mature reliable description of a physical system.

Unfortunately due to competition (for grants, territory, status etc) there is often conflict where there should be cooperation, and this competition is sometimes at the level of appropriation of names. So, it is that "mathematical physics" once was practiced mainly in rigorous frameworks, but now the term has been appropriated by the physics camp, and also denotes nonrigorous theoretical physics. (I disagree that it always had this general meaning;- from my small corner of the field, I have seen a definite 20yr drift in the meaning towards the nonrigorous). At the practical level, it means that both "types" of mathematical physicist now apply for the same grants and that e.g. Commun.Math.Phys. has the same rating as J.Phys.A. when these applications are judged, so it is not just a question of names.

16. **Tony Smith**
November 13, 2008

Hendrik said
"… physicists are good at discovering patterns in the physical world …
Mathematicians on the other hand … need rigour in order to have certainty in their results…".

Would it be fair to say that
physicists's work should be constrained by experimental observations
and
mathematicians's work is constrained by rigour of proof ?

If so, then "… there should be no conflict between the two fields …" so long as each is subject to its constraint system.

However, problems may arise if and when
physicists work without constraint of experimental observations
(which may be a source of concern about superstrings)
or
mathematicians work without rigour
(which seems, thankfully, not to be something that is a present source of concern).

I agree with Hendrick that the term "mathematical physics" was, up to the 1980s, "… practiced mainly in rigorous frameworks …"
(for example, look at the program of the 1981 Congress of the International Association of Mathematical Physics in Berlin (West)).
and
that since that time (temporally coincident with the rise of superstring theory) there has been, as Hendrik said, "… a definite 20yr drift in the meaning towards the nonrigorous … theoretical physics …".

Tony Smith

17. **Chris Oakley**
November 13, 2008

The question of whether what one does is rigorous or not should not even arise. Particular assumptions lead to particular conclusions and if any one of the steps in the derivation is not rigorous then it all falls apart and all one can then say is that the assumptions do *not* therefore lead to the conclusions. In this regard, I would "tone down" my comments about renormalization considerably if text books and QFT courses were honest about the non sequiturs … "effective" field theory has a self-consistency, but is a much smaller framework than the pedagogical materials would lead one to believe.

18. **Sandro**
November 13, 2008

I think all of you have read this

http://arxiv.org/abs/math-ph/0002018

but it never hurts to give it another look..
I agree that the reason for hostility between physicists and mathematicians are (most of the time) of economical species, and it is sad that we can't get the best of the two worlds, which I think is the only way towards progress in both fields, because of this.

19. **MathPhys**
November 13, 2008

Kea,

I have read what I linked and came up with a conclusion that's opposite to yours. Perhaps you should consider that others may reach conclusions that are opposite to yours before you type.

To my mind, a man who claims an affliation that dosn't belong to him (amongst many other petty acts along the same lines) is not above posting photoshopped pictures of himself with celebrities.

20. **Serifo**
November 13, 2008

I red the following story from somewhere □ : " professor X holds a joint position in physics and maths departments. Now in respect to his research , his colleagues in physic`s dept. say he`s not doing physics while in math`s dept. they say he`s not doing maths ! "So what is professor X doing ? Could it be a new and independent field of knowledge, that uses both physics and maths as a platform ?
Now I think among the mathematicians today, although there isn`t a general consensus on " what is mathematics ", there is a kind of consensus in all the fields, on how the practice of the subject must be done ! On the other hand, it seems to me that, there is a kind of consensus among theoretical physicists on " what physics is all about " but there isn`t a consensus on how the subject must be practiced !

21. **Jeff McGowan**
November 13, 2008

OK, couldn't pass up the opportunity – I'm sure most everyone has seen some variant of the following joke –

If you understand something and can prove it, publish it in a math journal.
If you understand something but can't prove it, publish it in a physics journal.
If you don't understand something but can prove it, publish it in an economics journal.
If you don't understand something and can't prove it, publish it in (fill in your least favorite subject) journal.

22. **Chris Oakley**
November 13, 2008

> If you don't understand something and can't prove it

… then get someone to make a popular science program showing that the only alternative to your idea is Intelligent Design.

23. **anon.**
November 15, 2008

'He'll be one of the physicists featured in the episode, as well as in the following one, entitled Light Speed. The next episode, Sex in Space, which will explore the "physiological, psychological and cultural challenges of sex in space" presumably will not be starring any theoretical physicists.'

Hawking is planning to go into space aboard Richard Branson's Virgin spacecraft, so maybe he'll be featured in the programme explaining his opinions on the physics of sex in space? See http://www.msnbc.msn.com/id/15970232/

24. **chickenbreeder**
November 15, 2008

The El Naschie story at n-Cc is pretty fascinating to read. It looks like El Naschie has adopted the same tactics of Bogdanoff brothers – using multiple aliases (all trackable to Cairo…) to defend himself.

A case like this is by no mean isolated. Quickly off my head I can think of a few people who share the trait of EN, though not so extreme as to publish 300+ papers in the journal edited by oneself. This is happening and is largely left untreated because the majority of "normal" scientists do not want to drag themselves into the mud by pointing fingers at their colleagues, however justifiable it may be. It could lead to law suits and other unpleasant things. You never know.

25. **st**
November 15, 2008

That's the problem, chickenbreeder. If scientists who know the type of pseudoscience he's been consistently producing don't speak up, then who will? If there is fraud like fabrication of data, should they not bring attention to that too? Why should they be afraid of lawsuits if their criticism is based on fact (e.g. the guy claims to have written a PhD thesis and has even cited it, but no thesis can be found at the university)? Folks, this is not a colleague doing poor work; it's an outsider claiming to be one of you and has succeeded in fooling even a reputable (albeit greedy) publisher for a long time.

Normally, it is best to ignore people who talk/write nonsense, but he has been draining the community's resources and has destroyed the name and reputation of a journal, which at one time was probably a decent publication.

26. **MathPhys**
November 15, 2008

I think El Naschie deserves a medal (to add to his extensive collection). Without him (and the Bogdanoff's and some of our colleagues who only go from high to higher) we would be lulled into the belief that the system works. It's pioneers like El Naschie (and Carlos Castro) that keep us on our toes.

27. **Shantanu**
November 17, 2008

Peter, this may be a bit off topic, but I watched a very interesting talk
by Robert Brandenberger on cosmology of the Lee-Wick extension to standard model
http://pirsa.org/08110032/ and in the first 10-15 minutes Robert
talks about the particle physics aspects and history of Lee-Wick standard model and apparently there were no papers on this subject for more than 35 years until recently.
This is the first time I am hearing about the Lee-Wick model.
Has this model been ruled out long time ago? Maybe you could post something about this model, as you are probably familiar with it

28. **Peter Woit**
November 17, 2008

Shantanu,

Sorry, I don't really know anything about the Lee-Wick standard model.

29. **MathPhys**
November 19, 2008

Peter,

Maybe you want to take a look at the Lee-Wick version of the standard model. Any alternative to supersymmetry is welcome. Brandenberger's talk is probably a good starting point.

30. **Math Phys**
November 19, 2008

El naschie using his own journal as
a stock for his endless uncountable papers.
Here is, one of his marvelous papers found in Chaos, soltion and fractals.

The title

"On the universality class of all universality classes and E-infinity spacetime physics"

M.S. El Naschie,

King Abdul Aziz City of Science and Technology, Riyadh, Saudi Arabia

Available online 18 October 2006.

Abstract
It is argued that E-infinity theory may represent the universality class of all universality classes of certain discrete dynamical maps which are at the root of relevant field theories. First we give a concise derivation of the basic equations of E-infinity and its ground state. Subsequently it is shown that the independence of the results obtained from the details of any equations of motion or Lagrangian is a clear indication that E-infinity may represent the universality class of all universality classes in the sense of Cantor with regard to relevant quantum field theories.

I'm quite amzed how this could be published.

In fact, for any one who knows little about particle physics realize that the results of any theory depend strongly on the particle content of the theory. For example in QCD, asymptotic freedom depends on the number of colours and flavors. The presence of CP violation in the quark sector depends on the number of generations. No CP violation for one and two generations, at least three generations is required for the presence of CP violation.

31. **Math Phys**
November 19, 2008

Almost one year ago we have sent to Elsevier about this issue concerning the "Chaos, soltion and fractl" Journal and his editor in Cheif El naschie

Here is the letter

Dear publishing responsible
We are writing you about the Journal of Chaos , solitons and Fractals and his editor in chief El Naschie. We are group scientists from different countries working in theoretical high energy physics and as a matter of fact we noticed that El Nashie the editor in chief the above mentioned journal has been publishing an incredible large number of papers in this journal, where he claims to have solved all the problems of particle physics using his E-infinity approach based on fractal geometry. We have looked at those papers very carefully and found them unscientific and meaningless, completely irrelevant to science and particle physics in particular. Moreover, those papers are not only without sense but complete junk. On the top of that El Naschie has published in 2008 (in one month and 5 days) 33 papers, that is one paper per day!!, which scientifically unacceptable. Not only that, we discovered that most if not all the papers published in this journal by different authors have no scientific sense and are really junk and rubbish. What most authors, who publish in this journal, do is either to refer to El Nashie works or invent a theory title and attribute to El Naschie, and then write anything, in many cases they just copy formulas from books and write them and publish the same article several times by changing the introduction and the conclusion.As an evidence we attached some papers published in this journal and we invite you to ask any respectable scientist to evaluate those papers. Indeed it is not even needed to have a big knowledge of physics and mathematics to realize that the content of those papers is complete non sense.This journal has become preferred place of scientific junk.We wonder how a respectable and leading publisher which publishes prestigious journals like Nuclear physics, physics letters.etc…accept such misconducting of this journal by his editor in chief El Nashie. In fact, it is very weired and strange that El Naschie publishes all his papers in his journal and this does not happen in any respectable journal. Indeed we have nothing personal against this guy, but this journal as we said has become sort of source of rubbish and junk and in our view a source of jokes. We are afraid that the publisher Elesvier will be participating and playing an unintentional role in fostering and delivering junk science in the globe, in contrast to supposed policy of Elsevier. Best Regards

Here is the reply:

thank you for your letter.
We shall review the issues that you raise carefully. Thank you for bringing your concerns to our attention.
Sincerely,
David Clark David Clark
Publishing Director, Physics, Mathematics, Computer Science and Astronomy
Elsevier B.V. Radarweg 29, Amsterdam 1043 NX
The Netherlands
Tel + 31 20 485 2451 | Fax + 31 20 485 2370 | david.clark@elsevier.com | http://www.elsevier.com

But at the end nothing happened

32. **Peter Woit**
November 19, 2008

"Math Phys" (who is not the same as "MathPhys")

If you want to use a pseudonym, please choose a different one than one being used by someone else here.

33. **st**
November 20, 2008

Yesterday, I sent an email to David Clark of Elsevier to ask him if he did indeed respond a year ago to a letter written by a "group scientists from different countries working in theoretical high energy physics". I included the addresses of blogs (including this one) that mentioned this letter, in case he would like to comment on it. It's only been 24 hours, but I haven't received a reply yet.

34. **MathPhys**
November 21, 2008

st,

I expect that Elsevier will respond by saying that it has already been decided that El Naschie will step down as Editor-in-Chief, but I think that that won't be good enough.

I think Elsevier has to explain what happened and how someone like El Naschie was let loose on a scientific journal published by them.

35. **st**
November 22, 2008

MathPhys,

I don't know when Elsevier will respond if at all. Perhaps the journalist mentioned here (see 11/22/08 comment) will have more luck. Apparently submissions to CS&F may now be sent directly to the (same) Editor-in-Chief at his newly announced e-mail address instead of an office in Elsevier, as was required until two days ago.

I wonder if it is common practice to send manuscripts directly to the publisher first instead of an editor.

36. **MathPhys**
November 22, 2008

"The latest electronic version of manuscripts for publication should be sent to the Editor-in-Chief, Professor M.S. El Naschie, Chaos, Solitons & Fractals via e-mail:Chaossf@aol.com. Alternatively manuscripts may be submitted to the nearest Regional Editor."

A professor at which university?

37. **An**
November 24, 2008

I think the case of El naschie is a scandal by all measures. This case opens the door for many questions: what are the organizations involved in this matter?

In the first place, one can mention Cambridge university which allowed him to publish his articles for nearly ten years 1993-2001 using its affiliation. It is far from reality to imagine that people in Cambridge have been fooled for that long time. According to the following data base

http://www.engineeringvillage2.org

One can find:
17 articles where the affiliation is DAMTP, Cambridge, UK.
72 articles where the affiliation is Dept. of Appl. Math. & Theor. Phys., Cambridge Univ., UK
40 articles where the affiliation is Univ of Cambridge

No prize for one who guesses at which journal those articles have been published.

In the second place, it comes Elsevier that has been the main stage for producing such a scandal bomb of heavy weight. It is clear that there have been many people behind that matter who got direct benefits (earning money, most probably from El Naschie himself).

38. **MathPhys**
November 25, 2008

I find it very strange that Elsevier allows for an editor-of-chief of a journal who uses professor as a title simultaneously with a POBox number as an affiliation and aol.com as an e-mail address. What's going on in Radarweg 29, Amsterdam?

39. **Stefan**
November 26, 2008

In this week's Nature, Quirin Schiermeier writes about the El Naschie story, Self-publishing editor set to retire (subscription required), including a quote by Peter.

40. **Felipe**
November 28, 2008

It is very clear to me, just in case someone ever had a doubt, that Elsevier is in the making money business. The company has no real commitment whatsoever to the scientific quality of its publications, as long as they can make some university's libraries pay for them. That is a very, very sad state of affairs.

41. **MathPhys**
November 29, 2008

Felipe,

This is no secret, in fact when they interview someone (with a degree in science) for a job, that's the first thing that they tell them.

In fact what I find strange is that Elsevier does not have the good business sense to know that to stay in the business requires a minimal degree of product quality control which is totally lacking in this case.

42. **Any one**
November 30, 2008

Looking for the numerous amazing articles of El naschie, I found a wonderful one whose title is
"P-Adic analysis and the transfinite E8 exceptional Lie symmetry group unification "
M.S. El Naschie
King Abdullah Institute for Nano and Advanced Technology, KSU, Riyadh, Saudi Arabia
Volume 38, Issue 3, November 2008, Pages 612-614

Just reading the first sentence in the introduction which is
"One of the most amazing results in high energy physics is the T-duality discovered in the context of superstring theories by Witten [1] "
But, for your surprise, the list of references you find no mention of any reference of Witten.
Reference [1] is just a paper of El naschie himself. Here is list

[1] M.S. El Naschie, A few hints and some theorems about Witten's M theory and T-duality, Chaos, Solitons & Fractals (2005), pp. 545–548.
[2] A. Leonovich, Comments on E8 unification and P-Adic numbers. http:/www.telegraph.co.uk/earth/main.jhtml?xm1=/eareth/2008/01/22/scieinstein122.xml (10/03/2008).
[3] M.S. El Naschie, Transfinite harmonization by taking the dissonance out of the quantum field symphony, Chaos, Solitons & Fractals (2007).
[4] M.S. El Naschie, High energy physics and the standard model from the exceptional Lie groups, Chaos, Solitons & Fractals 36 (2008), pp. 1–17.
[5] M. Kaku, Introduction to superstrings and M-theory, Springer, New York (1999) see p. 385 in particular.
[6] M.S. El Naschie, Infinite dimensional Branes and the E(∞) topology of Heterotic super strings, Chaos, Solitons & Fractals 12 (2001), pp. 1047–1055.

The first reference as already mentioned is El naschie paper. The big surprise about this paper is its tilte
"A few hints and some theorems about Witten's M theory and T-duality" here again we find no reference to any of Witten's papers . here is list of references of this paper ;

[1] E. Goldfain, Cantorian spacetime and unified field theory, Chaos, Solitons & Fractals 23 (2005), pp. 701–710.
[2] M.S. El Naschie, A review of E-infinity theory and the mass spectrum of high energy physics, Chaos, Solitons & Fractals 19 (2004), pp. 209–236
[3] M.S. El Naschie, Gödel universe, dualities and high energy particles in E-infinity, Chaos, Solitons & Fractals 25 (2005) (3), pp. 759–764.
[4] El Naschie MS. On the cohomology and instantons number in E-infinity Cantorian spacetime. Chaos, Solitons & Fractals, in press doi:10.1016/j.chaos.2005.12.019.
[5] M. Kaku, Strings, conformal fields and M-theory, Springer-Verlag, New York (2000).
[6] A. Khrennikov, Non-Archimedean analysis, Kluwer Academic Publishers, London (1997).
[7] V. Vladimirov, I. Valovich and E. Zelenov, P-Adic analysis and mathematical physics, World Scientific, Singapore (1998).

Something more peculiar about the list of references, of the first paper, is that one of the references is just a comment on an article published in the Telegraph, unfortunately the comment has been deleted. Also the address of the first paper raises another question about the so many false affiliation of El Naschie. The address seems not to be related to his activities.

It is obvious that there is no kind of peer review for these papers even at the fromal level apart from the content.
One can guess that papers may be generated using a program of language generation like n-moles or n-grams or whatever kind of program used. I think, at least for me, that the 'a b' of scientific writing should fulfill certain basic criteria:
1- If you mention a paper of Witten (or any name) [], then one should put reference for that person in the square bracket.
2- If you have a paper titled with theory of some one, then the list of references should contain at least one reference for that guy.

I hope, by now, El naschie has a plenty of time to fix the bugs in the program generating papers, implementing these two mentioned rules in the code and acknowledge this blog for drawing his attention.

43. **MathPhys**
November 30, 2008

On the positive side, El Naschie makes specific references to a standard textbook on string theory by one of the founding fathers of the subject

[5] M. Kaku, Introduction to superstrings and M-theory, Springer, New York (1999) see p. 385 in particular.

44. **st**
December 1, 2008

Earlier I commented that "no thesis [of El Naschie] can be found at the university". The comment was based on the fact that his name did not (still does not) appear in the University of London Theses Catalogue, a database known to contain titles dating back to the fifties.

Recently Stefan at Backreaction has apparently found a record in the Integrated Catalogue of the British Library.

I regret reacting prematurely.

45. **Any one**
December 20, 2008

To St

It is not important if El naschie is a phd holder or not.

The number of his papers is 350 or 1000 papers is also

immetrial. If one is allowed to write in his style without any

peer review one could publish 6000 papers in twenty years.

The main problems in his papers is they don't make sense

whatever mathematically or physicaly.

About the address "King Abdullah Institute for Nano and Advanced Technology, KSU, Riyadh, Saudi Arabia" on

his recent papers is very supicious as it has no relation to his activities.

On the Ninth International Symposium Frontiers of

Fundamental and Computational Physics 2008 had a lecture titled

“Average exceptional Lie group hierarchy and high

energy physics” where he claimed to be the director of

King Abdullah Al Saud Institute for Nano & Advanced Technologies

as evident from the affiliation mentoined below.

M.S. EL NASCHIE

King Abdullah Al Saud Institute for Nano & Advanced Technologies*,

Riyadh, Saudi Arabia.

*) Director

one can check

http://agenda.fisica.uniud.it/difa/getFile.py/access?contribId=52&sessionId=32&resId=0&materialId=slides&confId=9

But if you check the web page of King Abdullah Al Saud Institute for Nano & Advanced Technologies you don’t find his name listed in the Committee Members of Establishing King Abdullah Institute for NANO Technology and there is no mention for him at all. That

seems odd especially he is the director as he claimed.

One can check the web page for “Committees consultative sciencetisic”

http://www.nano-ksu.com/publish/article_46.shtml

web page for “Supervisory Committee to King Abdullah Institute for Nanotechnology”

http://www.nano-ksu.com/publish/article_63.shtml

Can the great man explain for us.

46. **Any one**
December 29, 2008

If El naschie is an honest scientist and not a fraud. He should mention the web link to the institute he claimed to have a position or related to it in his website.

I challenge him to put links which shows his claims and to assure his honesty for the others. Please give links to the following claimed position

1-He is the current advisor of the Egyptian Ministry for Science and Technology (High Energy Physics and Nanotechnology)

2- He is Adviser to King Saud University on Nanotechnology, and even more he claimed to be the director of King Abdullah Al Saud Institute for Nano and Advanced Technologies.

One can check the following link where he claimed to be the director

http://agenda.fisica.uniud.it/difa/getFile.py/access?contribId=52&sessionId=32&resId=0&materialId=slides&confId=9

If you check the webpage of of King Abdullah Al Saud Institute for Nano and Advanced Technologies. You find no mention for him at all

One can check the web page for “Committees consultative sciencetisic”

http://www.nano-ksu.com/publish/article_46.shtml

web page for “Supervisory Committee to King Abdullah Institute for Nanotechnology”

http://www.nano-ksu.com/publish/article_63.shtml

In fact it reflects badly on these countries if this was true.

47. **Any one**
January 5, 2009

Believe it or not

El naschie had four articles whose titles containing Witten. The articles are

1- A few hints and some theorems about Witten’s M theory and T-duality,

Chaos,Solitons and Fractals 25 (2005)545 û548

2- Using Witten’s five Brane theory and the holographic principle to derive the value of the electromagnetic structure constant alpha =1/137,

Chaos,Solitons and Fractals 38 (2008)1051 -1053

3- Fuzzy knot theory interpretation of Yang -Mills instantons and Witten’s 5-Brane model, Chaos,Solitons and Fractals 38 (2008)1349 -1354

4- On the Witten -Duff Branes model together with knots theory and E 8 E 8 super strings in a single fractal spacetime theory,

Chaos, Solitons and Fractals xxx (2008)xxx – xxx . The article is still in press, but you can get the pdf. file.

The amazing thing about the references of the first three articles is that they don't contain any research paper for Witten. Finally, the great man realized his mistake and put a reference for Witten in the fourth one (the most recent one). But the man didn't acknowledge who pointed out to him this bug in his program which he used to generate papers (Backreaction blog). Any way this a good step, at least the references are now correctly produced. Unfortunately you still need further improvement in your code that seems has a serious problem with E. Witten. Although you referred to a paper of Witten the program has produced a wrong title for it. In the reference list we find

[4]Witten E. Searching for a realistic Kaluza-Klein Theory. Nucl Phys B 1981;186:412 – 28.

While the correct title turned out to be, as you can check yourself:

Search for a realistic Kaluza-Klein theory

Nuclear Physics B, Volume 186, Issue 3, 10 August 1981, Pages 412-428, Edward Witten

As N. Eisfeld wrote on Mar. 26, 2008 @ 18:32 GMT, in this blog, http://fqxi.org/community/forum/topic/395, describing El naschie

"This man has never bad-mouthed, ignored or downplayed anyone or any contribution. He also acknowledged every single person who contributed to his work unless he genuinely did not know and then he will immediately apologize of the unintended omission."

48. **Any one**
January 7, 2009

I think that Elsevier is doing dirty jobs in scientific publishing. The CSF journal is owned by Elnaschie and Elsevier is getting money out of this apart from the journal subscription fees. El naschie pays for getting credibility of Elsevier and to have the chance to publish his great scientific ideas in journal hosted by a supposed reputable publishing house like Elsevier. There are other many similar cases in Elsevier.

El naschie keeps publishing junks in CSF for a quite long time and kept unnoticed by mentoring system of Elsevier which seems very odd. While it was so obvious from the far beginning that we have a crackpot.

The same applies to Cambridge university which allowed him to publish his articles for nearly ten years 1993-2001 using its affiliation, while, for sure, he wasn╝t a staff member there. It is far from reality to imagine that people in Cambridge have been fooled for that long time. According to the following data base

http://www.engineeringvillage2.org

One can find:

17 articles where the affiliation is DAMTP, Cambridge, UK.

72 articles where the affiliation is Dept. of Appl. Math. & Theor. Phys., Cambridge Univ., UK

40 articles where the affiliation is Univ of Cambridge.

No prize for one who guesses at which journal those articles have been published.

It is not enough for Elsevier just to step down Elnaschie , they should explain how these things happened and what their future precautions to prevent such a misusing of editorial power.

On the other side, Cambridge people should explain how it was possible for El naschie to use its affiliation for a quite long time, harming their reputation without charging him and any legal action.

The papers of El naschie would be a permanent black record for both Elsevier and Cambridge for too long time in the future.

49. **Any one**
February 2, 2009

Dear All,

There is an interesting article worthy to read.
The aricle is written in German about El naschie

Betrug in der Wissenschaft (Fraud in science) which
uncovers the reality of El naschie.

http://www.zeit.de/2009/03/N-El-Naschie?page=1

50. **Prego Senor**
February 27, 2009

The ZEIT article was removed, most probably due to pressure from El-Naschie. This is another scandal, since the article contained nothing but facts and opinions of other scientists bold enough to speak out. Many people underestimated Mohamed S. El-Naschie and hoped this would silently solve itself out somehow. They could not know that El-Naschie is a genius. Not by any means in the field of science, but the field of propaganda, defamation and filing law-suits.

Notes on BRST II: Lie Algebra Cohomology, Physicist’s Version

November 15, 2008
Categories: BRST

My initial plan was to have the second part of these notes be about gauge symmetry and the problems physicists have encountered in handling it, but as I started writing it quickly became apparent that explaining this in any detail would take me into various issues that are quite interesting, but far afield from what I want to get to. So, I hope to get back to this at some point, but for now will just assume that most of my readers know what gauge symmetry is, and that the rest just need to know that:

- The gauge group is an infinite dimensional Lie group. Locally (on space-time), it looks like a group of maps into a finite dimensional Lie group.
- The conventional assumption is that physics is invariant under the gauge group, so the gauge group and its Lie algebra should act trivially on physical states.

The actual situation is quite a bit more complicated than this, but for now we’ll focus on the simplest version of the mathematical problem that comes up here, and see how the BRST formalism deals with it. This posting will begin explaining one part of this story, starting with the simplest version of BRST cohomology, in a language familiar to physicists. The next posting will deal with Lie algebra cohomology in a more general mathematical context and work out some examples. For more about the material in this posting, see, for instance, Green, Schwarz and Witten, volume I, section 3.2.1, where they go on to apply this to the Virasoro algebra, or these lecture notes from Jose O’Figueroa-Farrill .

Physicists always begin by choosing a basis, in this case a basis X_i of $\mathfrak g$ satisfying $=f_{ij}^kX_k$, where f_{ij}^k are called the structure constants of $\mathfrak g$. A representation (π,V) is then a set of linear operators $K_i=\pi (X_i)$ on V satisfying $=f_{ij}^kK_k$. Let α^i be a basis of the dual space $\mathfrak g^*$, dual to the basis X_i.

Now, extend V to $\mathcal =V\otimes \Lambda^* (\mathfrak g^*)$, where $\Lambda^* (\mathfrak g^*)$ is the exterior algebra on $\mathfrak g^*$. On this space, define the “ghost” operator c^i to be wedge-product with α^i, and “anti-ghost” operator b_i to be contraction (interior product) with X_i. These operators satisfy “fermionic” anti-commutation relations

$$\{c^i,c^j\}=\{b_i,b_j\}=0,\ \ \{c^i,b_j\}=\delta^i_j$$

and one can get all vectors in $\mathcal H$ from linear combinations of decomposable elements of $\mathcal H$ (those given by repeated application of the c^i to the “vacuum vector” $V\otimes \mathbf 1$).

The ghost number operator $N=c^ib_i$ on $\mathcal H$ has eigenvectors the decomposable elements, with integer eigenvalues from 0 to dim $\mathfrak g$, given by the number of ghost operators needed to produce the eigenvector from a vacuum vector.

The BRST operator is given by

$$Q=c^iK_i -\frac{1}{2}f_{ij}^kc^ic^jb_k$$

which increases the ghost number by one, and has the crucial property of $Q^2=0$ (this comes from the fact that the f_{ij}^k satisfy the Jacobi identity). The BRST cohomology is given by considering the space $ker\ Q$ of elements χ of $\mathcal H$ that are “BRST-closed”, i.e. satisfy $Q\chi=0$, and identifying two such elements if they are “BRST-exact”, i.e. differ by $Q\lambda$ for some λ. So BRST cohomology is defined by

$$H^*_Q(V)=\frac{ker\ Q}{im\ Q}|_{V\otimes\Lambda^*(\mathfrak g^*)}$$

with $H^j_Q(V)$ the component of the BRST cohomology of ghost number j.

A vector $\chi=v\otimes\mathbf 1$ of ghost number zero satisfies $Q\chi =0$ iff and only if $K_iv=0$ for all i, so we can identify $H^0_Q(V)$ with the space $V^\mathfrak g$ of $\mathfrak g$ – invariant vectors in V.

The essence of the BRST method is to replace the problem of finding the invariant subspace $V^{\mathfrak g}$ of a representation V by the problem of finding the degree zero BRST cohomology $H^0_Q(V)$.

There are two different ways of putting an inner product on $\Lambda^*(\mathfrak g*)$ and thus getting an inner product on $\mathcal H$ ((π,V) is assumed to be unitary, so preserves a given inner product on V).

- Given $\omega_1,\omega_2\in \Lambda^*(\mathfrak g*)$, one can define

$$< \omega_1,\omega_2> = \int \omega_1\omega_2\equiv coeff.\ of\ \alpha_1\wedge\cdots\wedge\alpha_{dim\ \mathfrak g}\ in\ \omega_1\wedge\omega_2$$

(this uses the “fermionic” or “Berezin” integral $\int$, although I have not properly dealt with signs here.).
This inner product is indefinite, but it makes the BRST operator Q and ghost-operator c^i self-adjoint.

- Use an inner product on $\mathfrak g$, e.g. the Killing form for a semi-simple Lie algebra, to identify $\mathfrak g$ and $\mathfrak g^*$. This gives a Hodge operator $*_{Hodge}$ on $\Lambda^*(\mathfrak g*)$ that takes $\Lambda^i(\mathfrak g*)$ to $\Lambda^{dim\ \mathfrak g -i}(\mathfrak g*)$, and one can define

$$< \omega_1,\omega_2> = \int_G \omega_1\wedge *_{Hodge} \omega_2$$

(Note, here the integral sign is not Berezin integration, but the usual integration of differential forms over a compact manifold, in this case G)

With this inner product Q and c^i are not self-adjoint on $\mathcal H$. To get something self-adjoint, one can consider the operator $Q + Q^\dagger$ where $Q^\dagger$ is the adjoint of Q, but this operator does not have a definite ghost-number.

Comments

1. **MathPhys**
   November 15, 2008

   Peter,

   Can you make your BRST notes available as pdf files? That would make them easier to print.

2. **anon.**

November 16, 2008

MathPhys, use the free ‘Cute PDF’ print engine: http://www.cutepdf.com/Products/CutePDF/writer.asp

It installs as a printer, so you click print, choose Cute PDF as the printer, then it asks you where you want to save the PDF. I’ve just done it for this blog post, and it works perfectly (including all the mathematics).

3. **Peter Woit**
November 16, 2008

As I go along, I’ll put the material from the blog posts into a single document, which will be available as a pdf, and get updated as I go along. I’ll post the link for that here soon.

4. **MathPhys**
November 16, 2008

anon.

I tried CutePDF and it was very useful. Thanks.

5. **Hendrik**
November 17, 2008

Dear Peter,
I know this is the physicist’s version, but as it seems so easy, let me point at some underlying technical difficulties, (mostly arising from the fact that the Lie algebra of the gauge group is infinite dimensional).

1) “The gauge group is an infinite dimensional Lie group. Locally (on space-time), it looks like a group of maps into a finite dimensional Lie group.”
Well, its topology is important, and there are actually quite a lot of choices (e.g. in terms of how much differentiability is needed). Usually it is given the k-Sobolev topology (w.r.t. a volume form on the base manifold M), but sometimes the smooth maps of compact support are taken too, or even Schwartz groups. It has an interesting structure;- see the thesis of Christoph Wockel on his website.

2) “Physicists always begin by choosing a basis, in this case a basis X_i of the Lie algebra g…”
Since the Lie algebra g of the gauge group G is infinite dimensional, the existence (and meaning) of a basis is not clear. If you mean that every element Y in the space has a unique expansion in terms of a convergent sum of multiples of the X_i (which is what your next expression suggests), then that means the set of X_i is a Schauder basis, and there are deep theorems in functional analysis stating that not every separable Banach space has a Schauder basis. So this is a point where the choice of topology on the gauge group G is important. In the case that you choose a Sobolev topology, then there are Schauder bases, but it is not clear that there are if you choose the smooth topology.

3) “….satisfying [X_i,X_j]=f_{ij}^kX_k, where f_{ij}^k are called the structure constants of g.”
If this is an infinite sum (over k), then a topology must be stated to get convergence of the series.

4) “A representation (\pi,V) is then a set of linear operators K_i=\pi (X_i) on V …..”
The type of representation appearing in physics, is where the K_i are unbounded operators on Hilbert spaces, and these cannot have full domains. There is always a dense invariant subspace of the Hilbert space (also in the domain of your observables).

5) “Let \alpha^i be a basis of the dual space g*, dual to the basis X_i.”
Here g* is a topological (not algebraic) dual I assume. As it is infinite dimensional, your assumption of a dual basis severely limits the type of space which g can be. It will work for Hilbert spaces, but very few other infinite dimensional topological spaces (if any).

6) “extend V to V \otimes \Lambda (g*)…”
Probably the space H below is the label for this. Topologies are not clear. I presume if g* is a Hilbert space, you will want \Lambda (g*) to be the antisymmetric Fock space, in which case you need to take completion of the space. Also, \otimes for (infinite dimensional) topological spaces involves choices (e.g. of cross-norm, if they are Banach spaces).

7) “..ghost number operator N=c^ib_i on H has eigenvectors the decomposable elements, with integer eigenvalues from 0 to dim(g),…”
The ghost number operator N=c^ib_i is written as an infinite sum;- convergence and basis dependence?

8) “BRST operator is given by Q = c^iK_i -(1/2)f_{ij}^kc^ic^jb_k”
Apart from the question on infinite sums (convergence?) in this expression, this is the formula of the BFV-BRST formalism which does not produce the right constrained theory (without ad-hoc adjustments), when you carry out your BRST prescription below the formula. The Q-operator which produces the correct one, is from the Kugo-Ojima approach where the field aspect has to be taken into account, and you have an integral over the space-time variables rather than a discrete sum.

9) “…uses the “fermionic” or “Berezin” integral”
How do you define a Berezin integral when you have infinitely many fermions? In the supersymmetric Taylor expansion, you need a “top term” which will not exist if there are infinitely many fermions.

10) “Killing form”….“Hodge operator”
Both of these are problematic in infinite dimensions.

Of course, most of these problems do not arise when the dimension of g is finite.

6. **Thomas Larsson**
November 17, 2008

Hendrik,

Perhaps we should refrain from rushing ahead. I am sure that Peter will eventually arrive at various problems associated with infinite dimensionality, but starting with the finite-dimensional case makes sense to me.

Having said that, I will rush ahead and emphasize that the type of infinities matter, already on the Lie algebra level. For Lie algebras that are linearly infinite-dimensional, i.e. possess a Z-grading by finite-dimensional vector spaces, and positive and negative degrees are isomorphic, things are not too bad; you just have to replace loop algebras by their central extensions, the affine Kac-Moody algebras. But for algebras of gauge transformations in d dimensions, you have a natural Z^d-grading rather than a Z-grading, and then things start to become interesting…

7. **Peter Woit**
November 17, 2008

Hendrik,

Thanks for the useful explanation of the many places where infinite-dimensionality can cause trouble. I should have made explicit that I'm just sticking to finite dimensions for now.

At Thomas mentions, for d=1 gauge groups there is some understanding of what to do, with the algebraic part of the story going under the name "semi-infinite cohomology", which I hope to get to at some point. For d larger than one, I'm not convinced that anyone knows what's even the right way to formulate the problem. One reason for investigating different approaches in finite dimensions is that they may suggest a more promising way to deal with the situation in higher dimensions.

8. **Hendrik**
November 19, 2008

Dear Peter and Thomas:
OK, if your lie algebra g and the representation spaces V (in your next BRST posting) are finite dimensional, then the functional analysis issues all disappear, and there is not much I can say. Then the theory at this point can only cover global gauge theories (though the observable algebra will still force your representation spaces to be infinite dimensional).

Point (8) above still remains, in that this approach will lead to multiple copies of the physical space, and give problems with positive definiteness when you add the usual inner products.

A last comment, is that the BRST-algorithm which you summarized for the constraint space is not yet complete, it also needs a (algebraic) part for the constrained observables. Is that coming later?

9. **Peter Woit**
November 20, 2008

Hendrik,

I'm not quite sure what you mean in your question. Part of the story is the Kostant-Sternberg formalism for BRST, where the Lie algebra cohomology idea is put together with a resolution of functions on the constrained surface, in a package using Clifford algebra techniques. I'll write about that, including about the problems with that. For the trivial case it doesn't work…

Another thing I plan to get to is work by various mathematicians about how "quantization and reduction commute" in certain contexts, and how to interpret this a la BRST. There are still some things about that though that I'm confused about.

10. **Hendrik**
November 21, 2008

Dear Peter,
To clarify;- the BRST algorithm, like any constraint enforcement algorithm, constructs not just the constrained representation space (V^g in your notation), it also gives a method for finding the constrained observables on V^g from the original given algebra of observables. In particular this piece of the BRST algorithm specifies the constrained observables through "operator cohomology" by

O_BRST := (Ker d)/(Ran d)

where d(A) is the supercommutatior of Q with A. It also shows how O_BRST is represented on the physical space V^g.

We found that this prescription produced an algebra O_BRST which is too large, i.e. it properly contains the physical algebra of a straightforward Dirac constraining.

I've also looked at the "quantization and reduction commute" papers, and look forward to see your take on those. As you know, there are huge problems with general quantization schemes;- there are always choices involved, and limitations of the algebras which can be quantized. Moreover, some quantum constraints (which can be generators of gauge groups) are purely quantum, i.e. with no classical counterpart, i.e. if you do a "classical limit" they become trivial.

11. **Maynard Handley**
November 30, 2008

Peter, for those of us who are not steeped in this subject, could you clarify a few points that you zip over (assuming that they are obvious):

First- At the start of your discussion, you refer to extending V to
V \tensor /\(g*) where /*(g*) …
Is there a typo here and you actually mean that you are extending V to
V \tensor /*(g*)
?
I could imagine some sort of weirdness that involves not just an exterior algebra of forms, but an exterior algebra of multi-vector valued forms, and that's what you mean by the notation, but it seems easier to assume a typo.

Second- when you get to discussing inner products, what do mean by the integral notation. You and commenters agree that, at this stage of the game we are discussing a finite-dimensional space (that presumably will become a fiber at some point). Presumably we are not (yet) discussing fields of forms and vectors. So what does this integral mean — is it simply meant to be some sort of notational convenience (but what convenience is it buying?)
Beyond that, there's still so much elided that I don't get.
– Is the second inequality in the Berezin inner product a result (this integral, defined however the hell it is defined, happens to equal this cofficient) or is it a definition?
– As for the second definition, should there not be another hodge dual outside the integral to convert the result from a (dim-g) form to a 0-form? Under normal (field) circumstances, the integral would take us from a dim-g form to a scalar, but in this context I've no idea what the integral means.
– Is there a good reason why, in both these integrals, we don't refer to the integrand as \omega_1 \wedge \omega_2 rather than the rather less obvious \omega_1 \omega_2. As far as I can tell in both cases (making allowance for the fact that I've no idea what the integral actually does) the result seems to be dependent on the omega1-omega2 ordering.

12. **Peter Woit**
December 1, 2008

Maynard,

Thanks for the questions. You're right about the typo, will fix.

The part about inner products was written in a very confusing way, I'll try and fix it a bit. One problem is that I was using the integral signs in two ways, one for the Berezin integral, the other for the fundamental class of the compact Lie group, i.e. the standard integral over differential forms. I'll rewrite this to make it clearer.

In the second, definite, way of defining the inner product, all sign problems can be buried in the definition of the Hodge dual. For the Berezin case, I don't have the signs straight, but that's not the inner product I want to use anyway.

13. **ramón**
February 13, 2009

    Can I get the pdf files on BRST cohomology?

14. **Peter Woit**
February 13, 2009

    ramon,

    There's a pdf file at

    http://www.math.columbia.edu/~woit/notesonbrst.pdf

    I'll be updating that with a new version soon.

Notes on BRST III: Lie Algebra Cohomology

November 16, 2008
Categories: BRST

The Invariants Functor

The last posting discussed one of the simplest incarnations of BRST cohomology, in a formalism familiar to physicists. This fits into a much more abstract mathematical context, and that's what we'll turn to now.

Given a Lie algebra $\mathfrak g$, we'll consider Lie algebra representations as modules over $U(\mathfrak g)$. Such modules form a category $\mathcal C_{\mathfrak g}$: what is interesting is not just the objects of the category (the equivalence classes of modules), but also the morphisms between the objects. For two representations V_1 and V_2 the set of morphisms between them is a linear space denoted $Hom_{U(\mathfrak g)}(V_1,V_2)$. This is just the set of linear maps from V_1 to V_2 that commute with the action of $\mathfrak g$:

$$Hom_{U(\mathfrak g)}(V_1,V_2)=\{\phi\in Hom_{\mathbf C}(V_1,V_2): \pi(X)\phi=\phi\pi(X)\ \forall X\in \mathfrak g\}$$

Another conventional name for this is the space of intertwining operators between the two representations.

For any representation V, its $\mathfrak g$-invariant subspace $V^{\mathfrak g}$ can be identified with the space $Hom_{U(\mathfrak g)}(\mathbf C, V)$, where here $\mathbf C$ is the trivial one-dimensional representation. Having a way to pick out the invariant piece of a representation also allows one to solve the more general problem of picking out the subspace that transforms like a specific irreducible W: just find the invariant subspace of $V\otimes W^*$.

The map $V\rightarrow V^{\mathfrak g}$ that takes a representation to its $\mathfrak g$-invariant subspace is a functor: it takes the category $\mathcal C_{\mathfrak g}$ to $\mathcal C_{\mathbf C}$, the category of vector spaces and linear maps ($\mathbf C$ – modules and $\mathbf C$ – homomorphisms). If, instead of taking

$$V\rightarrow V^{\mathfrak g}$$

one takes

$$V\rightarrow V^{\mathfrak h}$$

where \mathfrak h is a Lie subalgebra of \mathfrak g, one again gets a functor. If \mathfrak h is an ideal in \mathfrak g (so that \mathfrak g/\mathfrak h is a Lie algebra), then this functor takes \mathcal C_{\mathfrak g} to \mathcal C_{\mathfrak g/\mathfrak h}. This is a simple version of the situation of interest in the case of gauge theory: if V is a state space with \mathfrak h acting as a gauge symmetry, then V^{\mathfrak h} will be the physical subspace, carrying an action of the algebra of operators U(\mathfrak g/\mathfrak h).

Some Homological Algebra

It turns out that when one has a category of modules like \mathcal C_{\mathfrak g}, these can usefully be studied by considering complexes of modules, and this is the subject of homological algebra. A complex of modules is a sequence of modules and homomorphisms

$$\cdots\stackrel{\partial}\longrightarrow U\stackrel{\partial}\longrightarrow V \stackrel{\partial}\longrightarrow W\stackrel{\partial}\longrightarrow\cdots$$

such that \partial\circ\partial =0. If the complex satisfies im\ \partial=ker\ \partial at each module, the complex is said to be an "exact complex".

To motivate the notion of exact complex, note that

$$0\longrightarrow V_0\longrightarrow V \longrightarrow 0$$

is exact iff V_0 is isomorphic to V, and an exact sequence

$$0\longrightarrow V_1 \longrightarrow V_0 \longrightarrow V \longrightarrow 0$$

represents the module V as the quotient V_0/V_1. Using longer complexes, one gets the notion of a *resolution* of a module V by a sequence of n modules V_i. This is an exact complex

$$0\longrightarrow V_n \longrightarrow\cdots\longrightarrow V_1 \longrightarrow V_0\longrightarrow V\longrightarrow 0$$

The deviation of a sequence from being exact is measured by its *homology* $H^*=\frac{ker\ \partial}{im\ \partial}$. Note that if one deletes V from its resolution, the sequence

$$0\longrightarrow V_n \longrightarrow\cdots\longrightarrow V_1 \longrightarrow V_0\longrightarrow 0$$

is exact except at V_0. Indexing the homology in the obvious way, one has H^i =0 for i>0, and H^0=V. A sequence like this whose only homology is V at H^0 is another manifestation of a resolution of V.

The reason this construction is useful is that, for many purposes, it allows us to replace a module whose structure we may not understand by a sequence of modules whose structure we do understand. In particular, we can replace a U(\mathfrak g) module V by a sequence of free modules, i.e. modules that are just sums of copies of U(\mathfrak g) itself. This is called a free resolution, and more generally one can work with projective modules (direct summands of free modules).

A functor that takes exact complexes to exact complexes is called an exact functor. Homological invariants of modules come about in cases where one has a functor on a category of modules that is not exact. Applying such a functor to a free or projective resolution gives the homological invariants.

The Koszul Resolution and Lie Algebra Cohomology

There are many possible choices of a free resolution of a module. For the case of U(\mathfrak g) modules, one convenient choice is known as the Koszul (or Chevalley-Eilenberg) resolution. To construct a resolution of the trivial module \mathbf C, one uses the exterior algebra on \mathfrak g to make free modules

$$Y_k=U(\mathfrak g)\otimes_{\mathbf C}\Lambda^k(\mathfrak g)$$

and get a resolution of \mathbf C

$$0\longrightarrow Y_{dim\ \mathfrak g}\stackrel{\partial_{dim\ \mathfrak g -1}}\longrightarrow\cdots\stackrel{\partial_1}\longrightarrow

Y_1\stackrel{\partial_0}\longrightarrow Y_0\stackrel{\epsilon}\longrightarrow \mathbf C\longrightarrow 0$$

The maps are given by
$$\epsilon : u\in Y_0=U(\mathfrak g) \rightarrow \epsilon (u) = const.\ term\ of\ u$$

and
$$\partial_{k-1} (u\otimes X_1\wedge\cdots\wedge X_k)=$$
$$\sum_{i=1}^k(-1)^{i+1}(uX_i\otimes X_1\wedge\cdots\wedge\hat X_i\wedge\cdots \wedge X_k)$$
$$+\sum_{i<j} (-1)^{i+j}(u\otimes\wedge X_1\wedge\cdots\wedge \hat X_i\wedge\cdots\wedge \hat X_j\wedge\cdots\wedge X_k)$$

To get Lie algebra cohomology, we apply the invariants functor

$$V\longrightarrow V^{\mathfrak g}=Hom_{U(\mathfrak g)}(\mathbf C, V)$$

replacing the trivial representation by its Koszul resolution. This gives us a complex with terms

$$C^k(\mathfrak g, V)=Hom_{U(\mathfrak g)}(Y_k,V)= Hom_{U(\mathfrak g)}(U(\mathfrak g)\otimes \Lambda^k(\mathfrak g),V)$$
$$=Hom_{U(\mathfrak g)}(U(\mathfrak g),Hom_{\mathbf C}(\Lambda^k(\mathfrak g),V))$$
$$=Hom_{\mathbf C}(\Lambda^k(\mathfrak g),V) =V\otimes\Lambda^k(\mathfrak g^*)$$

and induced maps d_i

$$0\longrightarrow C^0(\mathfrak g, V)\stackrel{d_0}\longrightarrow C^1(\mathfrak g, V)\cdots\stackrel{d_{dim\ \mathfrak g -1}}\longrightarrow C^{dim\ \mathfrak g}(\mathfrak g, V)\longrightarrow 0$$

The Lie algebra cohomology $H^*(\mathfrak g, V)$ is just the cohomology of this complex, i.e.

$$H^i(\mathfrak g, V)=\frac{ker\ d_i}{im\ d_{i-1}}|_{C^i(\mathfrak g, V)}$$

This is exactly the same definition as that of the BRST cohomology defined in physicist's formalism in the last posting with $\mathcal H =C^*(\mathfrak g, V)$.

One has $H^0(\mathfrak g, V)=V^{\mathfrak g}$ and so gets the $\mathfrak g$-invariants as expected, but in general the cohomology will be non-zero also in other degrees.

This is all rather abstract, so in the next posting some examples will be worked out, as well as the relationship of all this to the de Rham cohomology of the group. Anthony Knapp's book Lie Groups, Lie Algebras, and Cohomology is an excellent reference for details on Lie algebra cohomology.

Comments

1. **Serifo**
   November 16, 2008

   Hi professor Peter , I think you forgot to index the homomorphisms of the complex in your first definition of complex . It may be convenient also , to specify over which ring R is the complex defined.

2. **Serifo**
   November 17, 2008

   Ok, you are considering the complex to be defined over the ring of complex numbers C (which is a field). Would it be relevant to your research if you consider complexes defined over another type of rings, for example the ring Z of integers ?

3. **Peter Woit**
   November 17, 2008

   Serifo,

   I'm trying to avoid over-generality here, just using what I need, and trying to get the basic ideas across. So, unless otherwise specified, everything is defined over C.

   I'm also trying to avoid explicitly dealing with some points that would require adding a lot of details that aren't so relevant, until the point at which they become relevant. An example would be the indexing question.

   In general, if someone hasn't seen any of this before, I hope they'll use what I write to get a general idea of what is going on, then go to a textbook like Knapp to see the details, and to see more general versions of the story.

4. **a.k.**
   November 19, 2008

   ..sorry if I am posing a stupid question, but: the above cohomology groups should be expressable as some sort of Ext-groups, that is as appropriate n-fold extensions of V by C, that is, they should be more or less the same as (an equivariant version of) Ext*(C,V), where C are the complex numbers. Is this true and if yes, is there a physical meaning of this correspondance?

5. **woit**
   November 19, 2008

   a.k.

   What I gave was basically the definition of Lie algebra cohomology as
   [tex]Ext_{U(\mathfrak g)}(\mathbf C, V)[/tex]

   This is an Ext for modules over a non-commutative algebra, I don't know of any physical meaning for such objects, curious if anyone else does…

The Complete Idiot's Guide to String Theory

November 17, 2008
Categories: Book Reviews, This Week's Hype

I recently acquired a copy of The Complete Idiot's Guide to String Theory, by Scientific American's George Musser, which has been out for a few months now. It's a popular-level treatment of modern physics, string theory and quantum gravity, much like many other such books, but now in the "Complete Idiot's" style of lots of cartoons, graphics, material set off in boxes, and short summaries of chapters. As such, I guess it does as good a job as any of putting this material in a form designed to sell it to as many people as possible.

Musser is an enthusiast for just about any and every speculative idea about space and time. Besides string theory, the book covers loop quantum gravity, causal dynamical triangulations, the idea that spacetime is a fluid or a giant computer, and even some ideas I'd never heard of (we live in 3 dimensions because "For the simplest particle, we can make three mutually exclusive measurements"????). The treatment is often breathless, continually going on about how "exciting" all this is. In many ways, the book reads like advertising copy, hyping the promise of ideas (with string theory getting the bulk of the attention) while mostly ignoring or minimizing their problems. For example, the chapter on symmetry contains more than two pages on the "Pros and Cons" of supersymmetry, but this turns out to be just about all "pros" until a short paragraph at the end that begins: "That said, supersymmetry raises some questions that physicists have yet to solve".

I think I'm tempermentally allergic to this sort of discussion of science, but can see that some people like it and I realize there are arguments in its favor (get those kids and taxpayers excited about science!). Within the limits of such a genre, much of the book does a reasonable job, until the later chapters, where it starts to go off the rails.

There's a chapter on "parallel universes" which promotes the anthropic multiverse, describing it as "the most promising scientifically" of all possible options. Despite the fact that many string theorists are extremely unhappy with seeing this kind of thing promoted as the received wisdom of their field, Musser claims that:

> String theorists originally expected everything to be hard-wired but now think that almost everything is accidental

The scientific advisor for the book was Keith Dienes of the String Vacuum Project, and the list of those most prominently thanked for their help is dominated by landscape proponents Dienes, Bousso, Carroll and Tegmark.

A late chapter entitled "Ten Ways to Test String Theory" goes beyond the overly enthusiastic into the realm of the misleading and the simply untrue. According to Musser, the LHC will test string theory, GLAST will test string theory, Auger will test string theory, Planck will test string theory, LIGO will test string theory, a successor to Super-Kamiokande will test string theory, all the various dark-matter experiments will test string theory, table-top measurements of Newton's law will test string theory, bouncing laser beams off the moon will test string theory, checking midget galaxies to see if their stars have planets will test string theory, and looking for variation of fundamental constants will test string theory. This is really egregious nonsense.

The next to last chapter is about "The String Wars", and I appear prominently as "the most persistent and forceful critic of string theory", paired with Lubos Motl for my "over-the-top" comments. One of the few explicit factual errors in Musser's book is the claim that my book grew out of this blog (the book was written earlier, but took a long time to get published). The chapter is quite a bit less than even-handed in its discussion of these "wars", and mainly devoted to shooting down the supposed arguments of critics of string theory. I come in for criticism as endlessly putting forward a "silly deadline" of less than twenty years for string theory to have succeeded in reaching its goals. This straw man argument is conclusively bested, while ignoring the real argument, which is that the huge investment in time and effort put into string theory research has just produced more and more evidence that string theory-based unification is an idea that doesn't work. The problem is not the magnitude of the rate of progress towards understanding unification, it's the sign. And, soon I can start going on about 25 years….

Comments

1. **Kea**
   November 17, 2008

   Oh, but you are familiar with mutually unbiased bases – for spin they correspond to the x, y and z directions. As operators they are the Pauli operators. In the modern bootstrap, these correspond to the three indices of the next higher MUB set, corresponding to mass. Very popular in Quantum Foundations these days. In fact, I just got a postdoc to study them.

2. **Roland**
   November 17, 2008

   How can a book with a chapter full of egregious nonsense contain only few explicit factual errors?

3. **tomate**
   November 17, 2008

   It has no errors: being a theory of everything, every experiments tests it, including Millikan, Michelson-Morley, etc.

4. **Marion Delgado**
   November 17, 2008

   It was that enthusiasm "for just about any and every speculative idea about space and time" that made me sour on the physics lectures and presentations I used to go to that were cutting edge string-theoretic.

   Tachyons, in particular. Nonetheless, I'll read the book.

5. **Gazouille**
   November 17, 2008

   Roland,
   I think it might be that nonsense can't [even] be wrong…

6. **Will**
   November 17, 2008

   I dont know what good it does to comment about a naive book.

7. **Peter Woit**
November 17, 2008

Will,

Actually I don't think it's a naive book. It reflects accurately the views of one segment of the theoretical physics community, that which thinks the multiverse is science, string theory is the way to get unification, and hyping this kind of research to the public is a good idea.

8. **Low Math, Meekly Interacting**
November 17, 2008

It took a while before I figured out that such treatments of relatively speculative ideas about high-energy/unification physics were skewed. In fact, it was in the process of trying to figure out just how skewed that I came across this blog.

That said, one of the best physics popularizations, in part, that I've read was "The Fabric of the Cosmos" by Brian Greene. Not because (any more) of the stringy content, but the great attention paid to the established stuff. The discussion of Bell's Inequalities in the notes was for me both demanding but very approachable and rewarding for the effort, and one of the best attempts at giving the layperson a reasonably rigorous understanding of what hidden variables are all about that I've encountered. In that regard, portions of that book seemed very much in the spirit of what is now my hands-down favorite popularization of science of any discipline, namely "QED: The Strange Theory of Light and Matter".

If you ask me, that must be the best "For Dummies" book ever written. I really wish someone would take Feynman's approach, modernize it a bit to reflect the current state of established particle physics and QFT, couple it with an equally clever and illuminating treatment of GR, and, well, actually be able to sell it as is. I fear that the only way to get something like that published is to make it an introduction to whatever TOS the author specializes in. It's too bad, because there are loads of people out there who really think they know something about quantum gravity, but have only the vaguest notion of what the modern, tested, firmly-established theories of quantum and gravity are really all about. They'll talk compactifications and Calabi-Yau manifolds with an air of completely unwarranted authority, but haven't a clue what a tensor or a Hamiltonian are.

Hell, I was one of those people. And that's a problem. I'm gaining a better appreciation of how ignorant I am with each passing day, but I fear these books, in their often well-meaning enthusiasm for the wonders of the frontiers of science, tend to (unintentionally, I hope) give people an utterly false sense of confidence about their true level of understanding. I never got that feeling with QED, because of Feynman's grounded introduction. I don't think the speculative stuff is all about profit, necessarily. I just wish there were more QED's out there, and that publishers and/or the public at large got excited about them. Maybe they don't because real understanding is too much work, and a real appreciation for one's lack of understanding isn't the American Way.

9. **Visitor**
November 17, 2008

". . . . real understanding is too much work, and a real appreciation for one's lack of understanding isn't the American Way."

If you are aware of any country in which an equivalent statement is not true, please enlighten me, so that I can seriously research the feasibility of going there to live.

10. **Low Math, Meekly Interacting**
November 17, 2008

It's the only book market I'm really familiar with. Sorry.

11. **Chris W.**
November 17, 2008

So, Visitor, are you saying Americans no longer have a monopoly on cocksure, smug ignorance? Maybe we've succeeded in exporting that, along with our sophisticated grasp of modern finance. 🙂

12. **csrster**
November 18, 2008

This is the complete idiot's guide to string theory:
http://abstrusegoose.com/78

13. **anon.**
November 18, 2008

Peter, I wonder if you have seen another popular book (on a less speculative topic than string theory), 'Quantum Field Theory Demystified', by David McMahon which was published in February by McGraw-Hill, http://www.amazon.com/Quantum-Field-Theory-Demystified-McMahon/dp/0071543821

It has some bad reviews for making mathematical errors, but I like the idea of popularizing this stuff. I'd appreciate any comments you have on that book, if you've read it.

14. **Peter Woit**
November 18, 2008

anon,

I took a look at the McMahon book once in a store. It's not at all a popular book, but a real textbook, one that tries to boil down some difficult material to help a serious student starting out in the subject. It might be a good place for some people to begin learning the subject.

The same author also has a similar book on string theory, which I also looked at, but I think that's less successful. The material is just so complex that trying to get it into a low-level, accessible form is not going to work out as well.

15. **Low Math, Meekly Interacting**
November 18, 2008

I realized upon re-reading that I typed "TOS" instead of "TOE". S and E are adjacent on the keyboard, and I'm a sloppy typist. It occurred to me one could potentially read into that typo some scatological hidden meaning, which was not at all my intent. Freudians, make of it what you will.

16. **Coin**

November 18, 2008

One of the few explicit factual errors in Musser's book is the claim that my book grew out of this blog (the book was written earlier, but took a long time to get published).

Huh. When was the book written in relation to that "String Theory: An Evaluation" article?

17. **woit**
    November 18, 2008

    Coin, my book was mostly written in 2002, after the 2001 article, before the 2004 start of the blog

18. **anon.**
    November 19, 2008

    Thank you Peter, I've just bought the McMahon QFT book, and it does look useful.

19. **Andrew**
    January 29, 2009

    Hi Peter,

    Do you have anything to say about John Moffat's new book "Reinventing Gravity"?

Science and Science Fiction

November 18, 2008
Categories: Multiverse Mania

I just set my DVR to record this evening's broadcast on the History Channel of Parallel Universes, and noticed that the summary information about the show reads:

> Some of the world's leading physicists believe they have found evidence proving the existence of parallel universes.

One participant in the program is Clifford Johnson, who writes on his blog about how he's gotten a bad feeling about the project after seeing a rough cut:

> I'm a bit worried, if I'm honest, since this is a topic that is so easily seized upon by nutcases and sensible people alike, and is, in various forms, the fodder of so much charlatanism and mystical mumbo-jumbo. Any program in a science series on this sort of material has to be doubly careful -triply- to not give people an excuse to say that "the scientists have verified this".
>
> Why am I slightly worried? Well, I did not see a final cut of the show and so don't want to go over the top here, but an early rough cut I saw did seem to potentially suffer from a problem these shows can sometimes have: A collection of practicing scientists are very carefully making comments about what is known, unknown, likely, and unlikely, and so forth, and then much of that care can be undermined by the interspersing of their remarks with clips of every physics documentary filmmaker's favourite go-to guy who can be relied upon to say wild and wonderful things – Michio Kaku…
>
> I also did notice in the rough cut that there were a couple of places where I'd have preferred a bit more of a reminder that string theory (a framework where some of these speculative ideas about parallel universes has recently been re-discussed in scientific -but yes, still speculative- circles) is itself an unestablished and under-developed theory that could well be cast aside one day in favour of something else. I stressed this point in the course of our shooting, but don't know how much this got through.

One odd thing about this TV show is that it has already been done, in our universe, with the same name, featuring Michio Kaku, by the BBC back in 2001:

> Everything you're about to read here seems impossible and insane, beyond science fiction. Yet it's all true.
>
> Scientists now believe there may really be a parallel universe – in fact, there may be an infinite number of parallel universes, and we just happen to live in one of them. These other universes contain space, time and strange forms of exotic matter. Some of them may even contain you, in a slightly different form. Astonishingly, scientists believe that these parallel universes exist less than one millimetre away from us….
>
> For years parallel universes were a staple of the Twilight Zone. Science fiction writers loved to speculate on the possible other universes which might exist. In one, they said, Elvis Presley might still be alive or in another the British Empire might still be going strong. Serious scientists dismissed all this speculation as absurd. But now it seems the speculation wasn't absurd enough. Parallel universes really do exist and they are much stranger than even the science fiction writers dared to imagine.
>
> It all started when superstring theory, hyperspace and dark matter made physicists realise that the three dimensions we thought described the Universe weren't enough. There are actually 11 dimensions. By the time they had finished they'd come to the conclusion that our Universe is just one bubble among an infinite number of membranous bubbles which ripple as they wobble through the eleventh dimension.

In his posting, Clifford asks sensible questions about what scientists can do to keep science fiction from taking over science programs. I've heard that one mediagenic physicist who was offered a role in this program told them he would only participate if given the right to veto any segment involving him that misrepresented his views. He's not in the program.

From the opposite end of the science/science-fiction issue, tomorrow in LA there will be an event to launch a new project called The Science and Entertainment Exchange. This is a program (directed by Jennifer Ouellette, who blogs about it here), aimed at improving the portrayal of science by the entertainment industry. There seems to be an increasing amount of media-interest in science-related story lines, and the goal of translating this into getting some higher-quality science out before the public is a worthy one.

One goal of this organization I guess will be to improve the science in science-fiction programs. Since, at least as far as fundamental physics goes, the battle to keep science-fiction out of science appears to have been lost, maybe there should also be an effort to improve the quality and accessibility of the fiction now spreading throughout the physics literature. Some organization could get together creative artists and other media professionals to work on this, helping out programs like "Parallel Universes" as well as popular science books and journal articles. One can't deny that, at the moment, all of these are pretty sophomoric as creative art, as well as typically not very successful at reaching a mass audience.

There's a lot of room for advice from visual artists about more appealing string theory vacua for use in particle physics and string cosmology. Surely a good novelist or playwright could come up with a better pre-big bang story line than "colliding branes". As physics journals like Nuclear Physics B fill up with articles on Boltzmann Brains and the multiverse, with some help from the entertainment industry they could be marketed to a much wider audience, bringing down their cost to university libraries. A lot could be done on the marketing front: for instance it might be a good idea to include some 420 with each issue to help ensure that "mind-blowing" ideas don't just bore people, but really do blow the mind of the target audience. The possibilities really are limitless…

Update: Just finished watching "Parallel Universes". Wow. Almost completely free of any real scientific content, and definitely deserves an award as the most idiotic and ludicrous TV show ever made that pretends to have something to do with science. Deep into "what the bleep" territory. The problem is not just Michio Kaku. Everyone involved in the thing should be deeply ashamed of themselves.

Comments

1. **Chris Leonard**
   November 18, 2008

   Peter – what does this mean?

   for instance it might be a good idea to include some 420 with each issue

2. **Peter Woit**
   November 18, 2008

   Chris,

It means that this nonsense about "Parallel Universes" is making me get rather silly and vindicate those who describe this blog as sometimes "over the top". If you're unaware of what "420" is, you could try Google…

3. **Chris Leonard**
   November 18, 2008

   Right – gotcha now. I agree in that case!

4. **IGPNicki**
   November 20, 2008

   I've just been posting on another forum about how I feel strongly that while yes, I would appreciate better science in science fiction, I feel strongly that since the words "science" and "fiction" share equal billing, it shouldn't be a prerequisite for enjoying a show. However, have to totally agree that we need to keep scince science and not science fiction. I definitely see this trend for science lite (the spelling was on purpose). And not just science either. There's very little on the history channel which actually pertains to history. Pretty scary!

Notes on BRST IV: Lie Algebra Cohomology for Semi-simple Lie Algebras

November 18, 2008
Categories: BRST

In this posting I'll work out some examples of Lie algebra cohomology, still for finite dimensional Lie algebras and representations.

If G is a compact, connected Lie group, it can be thought of as a compact manifold, and as such one can define its de Rham cohomology $H^*_{deRham}(G)$ as the cohomology of the complex

$$0\longrightarrow \Omega^0(G)\stackrel{d}\longrightarrow \Omega^1(G)\stackrel{d}\longrightarrow\cdots\stackrel{d}\longrightarrow\Omega^{dim\ G}(G)\longrightarrow 0$$

where $\Omega^i(G)$ are the differential i-forms on G (note, we'll use complex-valued forms), and d is the deRham differential.

For a compact group, one has a bi-invariant Haar measure $\int_G$, and can use this to "average" over an action of the group on a space. For a representation (π, V), we get a projection operator $\int_g \Pi (g)$ onto the invariant subspace V^G. This projection operator gives explicitly the invariants functor on $\mathcal C_{\mathfrak g}$. It is an exact functor, taking exact sequences to exact sequences.

The differential forms $\Omega^*(G)$ give a representation of G in two ways, taking the induced action on forms by pullback, using either left or right translation on the group. If $(\Pi(g), \Omega^*(G))$ is the representation by left translations, we can use this to apply our "averaging over G" projection operator to the de Rham complex. This action commutes with the de Rham differential, so we get a sub-complex of left-invariant forms

$$0\longrightarrow \Omega^0(G)^G\stackrel{d}\longrightarrow \Omega^1(G)^G\stackrel{d}\longrightarrow\cdots\stackrel{d}\longrightarrow\Omega^{dim\ G}(G)^G\longrightarrow 0$$

Since elements of the Lie algebra $\mathfrak g$ are precisely left-invariant 1-forms, it turns out that this complex is nothing but the Chevalley-Eilenberg complex considered last time to represent Lie algebra cohomology, for the case of the trivial representation. This means we have $C^*(\mathfrak g, \mathbf R)= \Lambda^*(\mathfrak g^*)=\Omega^*(G)^G$, and the differentials coincide. So, what we have shown is that

$$H^*(\mathfrak g, \mathbf C)= H^*_{de Rham}(G)$$

If one knows the cohomology of G, the Lie algebra cohomology is thus known, but this identity is normally used in the other direction, to find the cohomology of G from that of the Lie algebra. To compute the Lie-algebra cohomology, we can exploit the right-action of G on the group, averaging over the induced action on the left-invariant forms $\Lambda^*(\mathfrak g)$, which again commutes with the differential. We end up with a complex
$$0\longrightarrow (\Lambda^0(\mathfrak g^*))^G \longrightarrow (\Lambda^1(\mathfrak g^*))^G\longrightarrow\cdots\longrightarrow (\Lambda^{\dim\ \mathfrak g}(\mathfrak g^*))^G\longrightarrow 0$$

where all the differentials are zero, so the cohomology is given by

$$H^*(\mathfrak g,\mathbf C)=(\Lambda^*(\mathfrak g^*))^G=(\Lambda^*(\mathfrak g^*))^{\mathfrak g}$$

the adjoint-invariant pieces of the exterior algebra on $\mathfrak g^*$. Finding the cohomology has now been turned into a purely algebraic problem in invariant theory. For $G=U(1)$, $\mathfrak g=\mathbf R$, and we have shown that $H^*(\mathbf R, \mathbf C)=\Lambda^*(\mathbf C)$, this is $\mathbf C$ in degrees 0, and 1, as expected for the de Rham cohomology of the circle $U(1)=S^1$. For $G=U(1)^n$, we get

$$H^*(\mathbf R^n, \mathbf C)=\Lambda^*(\mathbf C^n)$$

Note that complexifying the Lie algebra and working with $\mathfrak g_{\mathbf C}=\mathfrak g\otimes \mathbf C$ commutes with taking cohomology, so we get

$$H^*(\mathfrak g_{\mathbf C},\mathbf C)= H^*(\mathfrak g,\mathbf C)\otimes \mathbf C$$

Complexifying the Lie algebra of a compact semi-simple Lie group gives a complex semi-simple Lie algebra, and we have now computed the cohomology of these as

$$H^*(\mathfrak g_{\mathbf C}, \mathbf C) = (\Lambda^*(\mathfrak g_{\mathbf C}))^{\mathfrak g_\mathbf C}$$

Besides H^0, one always gets a non-trivial H^3, since one can use the Killing form $< \cdot,\cdot>$ to produce an adjoint-invariant 3-form $\omega_3(X_1,X_2,X_3)=$. For $G=SU(n)$, $\mathfrak g_{\mathbf C}=\mathfrak{sl}(n,\mathbf C})$, and one gets non-trivial cohomology classes ω_{2i+1} for $i=1,2,\cdots n$, such that

$$H^*(\mathfrak{sl}(n,\mathbf C))=\Lambda^*(\omega_3, \omega_5,\cdots,\omega_{2n+1})$$

the exterior algebra generated by the ω_{2i+1}.

To compute Lie algebra cohomology $H^*(\mathfrak g, V)$ with coefficients in a representation V, we can go through the same procedure as above, starting with differential forms on G taking values in V, or we can just use exactness of the averaging functor that takes V to V^G. Either way, we end up with the result

$$H^*(\mathfrak g, V)=H^*(\mathfrak g, \mathbf C)\otimes V^{\mathfrak g}$$

The H^0 piece of this is just the $V^{\mathfrak g}$ that we want when we are doing BRST, but we also get quite a bit else: $dim\ V^{\mathfrak g}$ copies of the higher degree pieces of the Lie algebra cohomology $H^*(\mathfrak g, \mathbf C)$. The Lie algebra cohomology here is quite non-trivial, but doesn't interact in a non-trivial way with the process of identifying the invariants $V^{\mathfrak g}$ in V.

In the next posting I'll turn to an example where Lie algebra cohomology interacts in a much more interesting way with the representation theory, this will be the highest-weight theory of representations, in a cohomological interpretation first studied by Bott and Kostant.

Comments

1. **Anon**
   November 20, 2008

   I'm a bit confused by the step where the De Rham complex of differential forms on the group G is found to have the same cohomology as the complex of left-invariant differential forms. Did I miss the step where this was explained above?

2. **Peter Woit**
November 20, 2008

Anon,

I guess maybe I'm skipping some steps there…

The argument is that:

1. Because the averaging functor is exact, the cohomology of the complex with terms [tex]\Omega^*(G)^G$[/tex] is [tex]H^*(G)^G[/tex]

2. But [tex]H^*(G)[/tex] is [tex]G[/tex]-invariant, since it is homotopy-invariant, and [tex]G[/tex] is connected.

Shouldn't Something Be Done?

November 19, 2008
Categories: Multiverse Mania

The sheer awfulness of last night's History Channel program on physics is hard to exaggerate. Here's some of what Clifford Johnson (one of the participants in the program) wrote on his blog while watching it:

> Oh, right… I remember "there are dinosaurs in your living room" thing. Oh dear. It is coming on in 8 minutes here, and so I guess I'll pour myself a long single malt and prepare myself. I've still got faith in Andy, though…
>
> Got to first commercial break. Er… need more whiskey. There's some good science embedded in there somewhere (e.g., Tegmark talking about inflation, and WMAP results and flatness and so forth (but the laser beams!?)), but the voice-over (among others) is taking serious liberties (like claiming right at the beginning of the show that scientists have evidence that there may be parallel universes…sigh. No, No, No, No. That was really not necessary.)…
>
> Need. More. Whiskey*.
>
> Ok… That's it. I had a lot of fun shooting my stuff for this, and while I know that it is maybe really not polite to say this, and I really like Andy and the crew who put this together…but I can't really defend this. They really really should have sent this out in time for us contributors to comment on. By time I saw the rough cut and sent in suggestions it was too late… I presume other sensible people contributing to this such as Ovrut, Lykken, etc, would have liked to have seen a rough cut of this and made remarks. It is really clear that the VO and script was written without a very good understanding of some of the basic concepts in place, and certainly not a careful regard for what's accurate and what is blatantly misleading. Anyone watching this would think that string theory or M-theory is experimentally verified and a working tool used to study the early universe… I spilled my whiskey when they showed pictures of people working in (what looked like optics) laboratories while talking about "years of research into string theory…".
>
> I have never ever heard of this "level x" business. I don't know who says that. But what was with the laser beams?! Where did that come from? Not the burning a hole in the fabric of spacetime and escaping a dying universe to go to another (WHAT?!), but the shooting them out from WMAP in order to measure the flatness of the universe. What was that?! And did you see the red struts between the blue branes that were supposed to be the "extra dimensions holding the branes in place"? What was that?!
>
> This is all so sad because there's so much, as we say above, good TV that could be made of this material if done right.
>
> Ok. I'm done with this. It's very sad.

One would like to just ignore something like this and let it fade into obscurity, but the problem is that the History Channel is likely to keep rebroadcasting it for years and years, doing continuing damage to the public understanding of science and the public image of physicists. I don't really see how an intelligent person can watch this thing and not come away with the impression that theoretical physicists are a bunch of idiots. It seems to me that it would be a good idea for people in general, and the scientists involved in this in particular (Clifford Johnson, Max Tegmark, Michio Kaku, Joe Lykken and Alex Filippenko) to contact the History Channel with a polite request that this program not be rebroadcast, and that steps be taken to avoid creating more disasters of the same kind.

Update: Chad Orzel also saw the program and has some comments about it one of its dumber aspects, beginning with:

> Yeesh. That was so actively irritating that I don't know where to start.

Comments

1. **Academic Lurker**
   November 19, 2008

   It seems like scientists (or, more specifically, physicists, since this seems to happen to them in particular) are in a bit of a bind here.

   It's one thing to avoid charlatans like the makers of "What the Bleep…", but this was a documentary for the History Channel. Folks like Clifford Johnson & co. had every reason to think that this would be a respectable production. And from your post below:

   "I've heard that one mediagenic physicist who was offered a role in this program told them he would only participate if given the right to veto any segment involving him that misrepresented his views. He's not in the program."

   it's clear that scientists are allowed little influence on the production once they've given their interviews. Is the only option to just avoid involvement in these projects?

   It seems like that would be doubly unfortunate because a) public outreach & getting people excited about what physicists do is good, and b) it leaves the stage to folks like Kaku.

   I'm not sure what the answer is.

2. **Low Math, Meekly Interacting**
   November 19, 2008

   I agree, it's a pickle. Over on CV they're talking about improved outreach and media representation, but the main paradox, I think, is that it's extremely difficult to make accuracy/edification entertaining to the general public, and producers of such programs are in the business of entertaining and making lots of money, not providing a public service that there's no demand for. It's a problem all school teachers struggle with on a daily basis: How do keep the average student interested, and actually give them a high-quality education? On the one hand, it's vitally important that scientists remain engaged, but on the other, they lose control over the message as soon as it leaves their mouths. As Lurkee notes, when the interviewees make unentertaining demands, the producers simply move on to someone with fewer scruples. How do you win?

3. **Bee**
   November 19, 2008

   I recently learned that Discovery Channel will be airing some of PI's public lectures. I generally think it is a better idea if movie guys help scientists to get a message across than if they ask scientists for help to get their own message across.

4. **milkshake**
November 19, 2008

I remember watching a PBS-NOVA program few years back, about Andrew Willes, Galois representations and Fermat's last theorem, and I thought it was entertaining enough for the lay audience – and it even provided some taste of a life spent working on an esoteric problem. The documentary had some annoying parts (repeated annoying song and animations) but given the dry subject they did a decent job popularizing the historic background and the story of the proof itself.

So its not like "it's extremely difficult to make accuracy/edification entertaining to the general public" if one actually cares about the subject he is trying to popularize.

5. **Peter Woit**
November 19, 2008

I think physicists should continue to get involved with media projects, even ones like this. But, at the same time they need to be wary about what they have gotten themselves into and behave accordingly. In this particular case, the topic should have set off a lot of red flags.

Once the damage is done, in egregious cases like this one, I think it really is the responsibility of those who participated, and of the physics community to at least try and do something about this. If the only complaining is a bit of grumbling on blogs, it will keep happening.

6. **Low Math, Meekly Interacting**
November 19, 2008

Hi, Milkshake,

I don't doubt you were entertained, but you may have more discerning tastes than the average viewer, for lack of a better way of putting it. Public Television is not immune to the need for ratings, but they don't have the same business model as a commercial cable channel like Discovery or History, and likely cater to broadly different audiences. I'd frankly be shocked if the numbers and demographics of the eyeballs either network grabs bore much similarity, and I'd be flabbergasted if PBS' market was more representative of the general viewing public than DC's or HC's.

All this said, protests, both formal and informal, do make a difference. I'm reminded of the outcry generated by "The Great Global Warming Swindle". Granted, that execrable excuse for a documentary was motivated more to influence policy than to simply earn advertising revenues, but it used the same sorts of distorting tricks to hook the audience, it sounds like.

So, yeah, I agree, speak up loudly, for what it's worth. I doubt it will solve the overall problem, though. Just dampen this particular fire, which is a worthy enough goal.

7. **Michael Bacon**
November 19, 2008

I favor an 'Everettarian' MWI, but even I had to switch the show off after a few minutes — really, breathless, content free viewing.

8. **changcho**
November 19, 2008

Peter, I think you meant to write the History Channel, not Discovery Channel. Other points:

* The History Channel's treatment of science is a mixed bag. I do recall watching at least one very good show ("How the Earth was Made") there.

* NOVA on PBS is, in general, much more careful and accurate with their science shows.

*I have seen M. Kaku on several science shows (mostly on Discovery Science Channel, History Channel) talking about extremely speculative ideas (parallel universes, string theory) as if they were commonly accepted by most scientists. This is clearly a disservice (except for him, of course).

9. **Peter Woit**
November 19, 2008

changcho,

Thanks for catching the mistake, fixed now. My apologies to the Discovery Channel…

10. **milkshake**
November 19, 2008

Discovery and History are both full of X-files-level rubbish – all presented with the utmoust seriousness by a team of independent experts.

11. **Serifo**
November 19, 2008

I think the media (specialy private media) are more concerned with the number of the audience than to inform the audience in a honest way ! Well , general public seems to enjoy ideas that are not part of the orthodox scientific thinking. I think the basis for this , is in the history of science . See, people (general public) learned with the history that, in their time , Scientists like Galileo challenged the orthodox (scientific or religious ?) thinking about universe. So , these days whenever somebody comes up with non orthodox ideas about universe, people normally start to have pictures of Galileo and others who were rejected , which is actually something positive in my view, as long as they are aware that " Galileo and others also had incomplete or even wrong ideas about universe " !
Now , I think the media has the responsibility to inform the people on " what is currently the orthodox and non orthodox scientific thinking about universe " otherwise the general public will end up ………

Ps – I just hope the research funding agencies (specialy the private ones) don`t make decisions based on TV programs ! 🙂

12. **rick**
November 19, 2008

I watched this and the whole while I was thinking, maybe there's a parallel universe where Michio Kaku still has his dignity, but probably not.

13. **Chris W.**
November 19, 2008

As a child I came to the conclusion—semiconsciously—that wanting to *really* understand almost anything is viewed in mass society as a vaguely subversive and perverse attitude. If you manage to maintain this desire while meeting other people's expectations it is tolerated, and with luck even encouraged. If not, difficulties often ensue.

Let's face it, as a joint venture of Hearst, Disney, and NBC, the History Channel (like the Discovery Channel) is a commercial enterprise, and it aims to draw and hold viewers. In the contemporary media environment that inevitably devolves to pandering and efforts to titillate and offer shallow diversions. When people sit down to watch TV at night they are rarely prepared to concentrate deeply or be intellectually absorbed by something, and frankly, *broadcast* television has never been a good medium for that. (I think video on the internet is—at least potentially—a different story, notwithstanding the vast quantities of vacuous garbage on YouTube.)

Many professional scientists seem to be too easily taken in by flattering invitations. Think about the context, folks, and get a clue.

14. **Janne**
November 20, 2008

    There are vast quantities of solid material on youtube aswell. For example MIT and Stanford physics lectures and some researchers have contributed computer animations from their research. I've watched some interesting lectures on Googletalks as well. I think the scientific community could do a lot more with youtube.

15. **Low Math, Meekly Interacting**
November 20, 2008

    There's certainly good stuff to be had via YouTube, but it's not easy to find if you don't know exactly what to look for. I bet, though, that somebody more educated and obsessive than myself has compiled a clearinghouse of links to freely-available content that has both accessibility and integrity. If not, I'd be willing to kick in a few bucks to make it worth their while

16. **Hun**
November 20, 2008

    I found that the History Channel often sends out biased documentaries just like the Fox Channel, using interviews to deliver the message 'they' like. PBS seems to be much more careful.

17. **Charlie C**
November 20, 2008

    It's useful to look at this from a different perspective. What we have been talking about here is how to turn entertainment into education, a tough row to hoe. Following Janne's lead, maybe scientists should be asking how to we use modern tools to explain our ideas clearly. The scientists should take the lead, as has started to happen with YouTube. However, so far, most of the YouTube scientific lectures are pointed at fellow professionals. The REAL challenge is how can modern animation, graphic packages, YouTube video clips, etc be combined, by scientists, to CLEARLY yet ACCUATELY explain basic and sometimes profound concepts. Maybe some scientists should be taking the lead and forming small teams of media experts in order to create professional, accurate, and yes, maybe even exciting expositions intended for the non-pro. You don't have to talk down to your grandmother, but you do have to really understand how to describe your topic clearly and simply. And, of course, to teach is to learn. Win-win.

18. **Mitch Miller**
November 20, 2008

    "I don't really see how an intelligent person can watch this thing and not come away with the impression that theoretical physicists are a bunch of idiots." [PW]

    This is too harsh of a statement, but there is a danger here. We have all probably seen programs on the history channel with PhDs talking about how aliens built the pyramids who are clearly idiots. While this program was no were near that level (most of the experts came of as very smart people trying to explain speculative ideas to lay persons) it is concievable a physics show in the future could approach alien/pyramid if the experts aren't careful.

19. **Coin**
November 20, 2008

    What confuses me is why we don't ever seem to have a situation where they just hire one of these physicists to *write the program script*. It's not like real scientists are magically incapable of writing for a popular audience; they let scientists write *books* for a popular audience all the time (and more than one of the participants in this program has done exactly that), so why not let scientists write tv programs? Why relegate the science people to "consultant" at best– Clifford Johnson etc were expressing concern that they weren't able to view and give feedback on the program before it aired, but why is it that people like Clifford Johnson are being kept so far away from the production of the program in the first place that "request minor changes once the thing's done" is the best they can hope for?

20. **Heineken**
November 20, 2008

    the FLT video was called The Proof. it was a very good Nova episode. there are also 2 (?) NOVA episodes on Einstein's work. very good stuff for the lay audience.

    i had some questions about the symmetry with modular forms, how to find an E series, etc.

    some of my favorite books combine equations and history, and are geared to the lay audience with a year or two of calc background. Prime Obsession being a good example.

21. **Chris W.**
November 20, 2008

    From Coin:

    > What confuses me is why we don't ever seem to have a situation where they just hire one of these physicists to *write the program script*.

    What I said previously:

    > [The History Channel] is a commercial enterprise, and it aims to draw and hold viewers. In the contemporary media environment that inevitably devolves to pandering and efforts to titillate and offer shallow diversions.

    Why would the producers of a History Channel program let a physicist muck up their efforts to target the audience they're trying to reach? They are media professionals, and they figure they know better than anyone how to reach the audience that upper management wants to reach. That includes controlling how the script is written. Consider what professional scriptwriters, let alone academics moonlighting as script consultants, often have to contend with when they write for film or television. Stories of scripts butchered at the behest of producers and directors are legion.

22. **lcs**
November 20, 2008

By the way, I stopped watching PBS/NOVA science programs years ago because of the obnoxious introduction of MTV style editing, background music or drums (always completely unnecessary) and other concessions to the 10 second attention span of today's under 50 audience. The last good PBS show was about Feynman.

23. **Peter Woit**
November 20, 2008

I'm not so sure this is such a simple story of "bad TV people ruin good physics". There's a lot of pseudo-science in the multiverse to begin with and precious little good physics. The physicists participating in this all seemed willing to, on camera, go on about how cool it is that there is some other universe out there with people exactly like you, but slightly different. It's not too surprising that the film-makers took this kind of thing and ran with it.

Clifford Johnson seems to have gone into this without having any idea of what many of his colleagues (e.g. Tegmark) have been promoting, writing:

"I have never ever heard of this "level x" business. I don't know who says that."

You don't need to have followed much of the multiverse mania to have run up against Tegmark's 4 levels of multiverse. And any exposure to multiverse mania should make one very careful about getting involved in a program promoting it.

Again, at least one physicist had no trouble recognizing this, insisting that he wouldn't participate unless he could be sure his words would not be used to promote this kind of pseudo-science, and dropping out when he couldn't get convincing assurances. Of those who participated in the program besides Clifford, I have no idea whether they feed betrayed by the program, or whether they actually think it made sense. Fillippenko is listed in the credits as "scientific consultant".

I tried to contact the History Channel about this, got nowhere. Maybe other people might have more luck. I'm curious whether any of the physicists involved have complained to anyone there about the program.

24. **Professor_D**
November 20, 2008

And we wonder why the general public (not to mention politicians) is virtually scientifically and mathematically illiterate. Whenever I catch the occasional NOVA, I'm always hoping that it will really be about science this time. But alas, it always seems to be more boring historical reenactments and other renaissance faire fluff more appropriate to something that should be shown on the History Channel. It's essentially like a glorified version of Wishbone (the dog that reenacts classics like Romeo and Juliet, and Treasure Island, with the neighbohood kids). Oh well. Just my two-cents worth on this stuff.

25. **Chris W.**
November 20, 2008

For some modest relief, BBC2 offers *Einstein and Eddington* [Physics World blog review]. The science is apparently lightweight, but at least it seems to be a well-done drama on an interesting historical period; the producers show a modicum of taste.

26. **abbyyorker**
November 20, 2008

The public loved "What the bleep do we know" (I guess – didnt it have a sequel?). That movie was not only nonsense – it was boring. Public has no clue about physics – even physics from the 20's. Some great science communicators can give a whisper of understanding but it is of little consequence. So let the theorists run wild – the wilder the better because the public will not (cannot) call them to account.

27. **csrster**
November 21, 2008

"I'm always hoping that it will really be about science this time. But alas, it always seems to be more boring historical reenactments and other renaissance faire fluff more appropriate to something that should be shown on the History Channel."

The dearth of good history broadcasting and it's replacement with costumised puff is a topic for another day …

28. **Chris Oakley**
November 21, 2008

FYI: *Einstein and Eddington* is on in the UK, BBC2, tomorrow night (November 22) at 9:10pm.

I didn't see *Parallel Universes*, at least not in this one. Although one of my body doubles in a parallel universe may have watched and enjoyed it, this strand of Dr. Oakley probably won't bother.

29. **trond**
November 21, 2008

Anything broadcast on History/Discovery Channel is mostly infotainment and it's unreasonable to expect peer-reviewed content (no pun intended on Chaos, Solitons & Fractals). As mentioned by the other posters, there are only two broadcasters, BBC and PBS, that still occasionally produce quality productions.

30. **Visitor**
November 21, 2008

"I tried to contact the History Channel about this, got nowhere. Maybe other people might have more luck. I'm curious whether any of the physicists involved have complained to anyone there about the program."

Perhaps it might worthwhile, and more productive, to contact the physicists involved. It would be interesting to know, not only their opinion of the show, but if they feel that they have any duty to do anything about it, or if they can just wash their hands of the matter.
If these people are sufficiently dissatisfied with the show's treatment of the subject, and if they could ante up enough money to put an ad in a magazine, trade journal, or some publication of that sort, it is – and let me make this statement sufficiently conditional – it is not impossible that the resulting bad publicity could serve as a caution to other tv producers considering traipsing down the same road as was done in this show.

31. **bane**
November 21, 2008

To Serifo,

Whilst funding bodies generally don't consider media appearances, unfortunately many career important committees like hiring, promotion or other-university-responsibility boards often consider only the volume of a scientist's media work (via the pile of clippings and lists of TV work they attach to the CV) and not their quality. In all honesty I can't say considering media profile in these kind of things is wrong per se, it's just that if you're going to consider it you really should evaluate it properly rather than just "weigh it".

32. **Tom Whicker**
November 23, 2008

This program was really terrible, but sadly just standard operating
proceedure for the History Channel. I had the misfortune of seeing
"The Earth's Black Hole" not long ago on the HC. This one was so
astoundingly absurd that even non-technical fans of the channel were
calling for a boycott. There were plenty of mis-quoted scientists
in this one also. Here is a list of those that appeared if anyone
wants to contact them and maybe get some kind of group action going:

http://en.wikipedia.org/wiki/Decoding_the_Past

33. **GR**
November 23, 2008

CUNY has a new NYC subway train poster with a life size color photo of Dr. Michio Kaku. It describes him as "co-founder of string field theory" & "international authority on theoretical physics".

34. **db**
November 24, 2008

The "Einstein and Eddington" drama on BBC 2 wasn't all that bad. It focused more on the political and social consequences of corresponding with "the enemy" than the physics, but what physics they did include wasn't outrageously mangled.

They took a fair number of liberties with the characters, but they were largely forgivable with the exception of one cringeworthy speech toward the end.

Overall it did a better job than most of the pop sci stuff out there, but the science was almost entirely incidental.

35. **WP**
December 11, 2008

As a writer-producer-director with a strong background in the sciences, I can see the validity of the points brought up. I personally thought the program was entertaining, but thought someone reached way up to pull out some of the stuff showcased on the show.

Now, I've always been a proponent of the idea of a 'multi-verse' yet some of the theories like 'well there may be other identical universes, they're just too far for us to see' should get the 'plasticman' award for reaching. That and the 'burning a hole in space-time with lasers' was way too sci-fi even for me.

I do agree with the poster who mentioned that once the script is in the network's hands its a done deal. I'm sorry to say that it's all 'infotainment' including NOVA which I thoroughly enjoy and respect. So at best, production personnel and scientists will strive to get in and keep in as much solid info as possible.

One good thing about all of this is there are young people watching this stuff and are building an interest in how all these things (i.e. physics, math, science in general) work and want to know more. I know that NOVA, Jaques Cousteau, Wild Kingdom and a host of other shows made the sciences very attractive to me. I'm sure it is having the same effect on others.

The good news is; those who end up exploring these disciplines will soon discover the 'smoke and mirrors' whipped up by these programs and one day they'll say, "Hey, there really wasn't a lot of good math in this program…."

36. **P**
January 20, 2009

The history channel tends to be pretty sensational in many of its programs. Sometimes, late at night, they have had programs about the illuminati and alien conspiracies. Their religion programs like to focus on the gnostic gospels. If a real historian would laugh at some topic as total bull, they will probably do a special about it on the history channel.

The Map of My Life

November 22, 2008
Categories: Book Reviews

Springer has just published an autobiography of Goro Shimura, entitled The Map of My Life. Shimura's specialty is the arithmetic theory of modular forms, and he's responsible for a crucial construction generalizing the modular curve, now known as a "Shimura variety". The book has a long section at the beginning about his childhood and experiences during the war in Japan. The rest deals mostly with his career as a mathematician, including often unflattering commentary on his colleagues. One of those who comes off the best is André Weil, who encouraged and supported Shimura's work from the beginning. They both ended up at Princeton, with Weil at the Institute, Shimura at the University.

The book contains extensive discussion of the story of what Shimura calls "my conjecture". This is the conjecture proved by Wiles and others that implies Fermat's Last Theorem. In the past, it has conventionally been referred to by various combinations of the names of Shimura, Taniyama and Weil, although more recently the convention seems to be to refer to it as the "modularity theorem". Shimura also claims credit for conjecturing the "Woods Hole formula" that inspired Atiyah and Bott to prove their general fixed-point theorem.

To get a flavor of the unusual nature of the book, here are some extracts from one section:

> Jean-Pierre Serre, whom I had met in Tokyo and Paris, was among the audience, and kept asking questions on the most trivial points, which naturally annoyed me…. Somebody told me that he had become frustrated and even sour. Much later I formed an opinion that he had been frustrated and sour for most of his life. As described in my letter to Freydoon Shahidi, included as Section A2 in this book, he once tried to humiliate me, and as a result gave me the chance to state my conjectures about rational elliptic curves. I now believe that his "attack" on me was caused by his jealousy towards my supposed "success" — my conjectural formula and lectures — at Woods Hole….
>
> In spite of the fact that my mathematical work was little understood by the general mathematical public, I was often the target of jealousy by other mathematicians, which I found strange. I can narrate many stories about this in detail, but that would be unpleasant and unnecessary, and so I mention only one interesting case…
>
> (*he then describes an encounter in which Harish-Chandra compares favorably Apery's result on the irrationality of ζ (3) to Shimura's work*.)
>
> Clearly he thought he finally found something with which he could humiliate me: To his disappointment, he failed. Did he do such a thing to other people? Unlikely, though I really don't know. But why me? To answer that question, let me first note an incident that happened in the fall of 1964. As I already explained, Atiyah and Bott proved a certain trace formula based on my idea. Bott gave a talk on that topic at the Institute for Advanced Study. In this case he clearly acknowledged their debt to me. In the talk he mentioned that Weyl's character formula could be obtained as an easy application. Harish-Chandra, who said, "Oh, I thought the matter was the other way around; your formula would follow from Weyl's formula." Bott, much disturbed, answered, "I don't see how that can be done." After more than ten seconds of silence, Harish-Chandra said "It was a joke." There was half-hearted laughter, and I thought that his utterance was awkward and did not make much sense even as a joke.
>
> It is futile to psychoanalyze him, but such an experience may allow me to express some of my thoughts. He was insecure and hungry for recognition. That much is the opinion shared by many of those who knew him. He did not know much outside his own field, but he was not aware of his ignorance. In addition, I would think he was highly competitive, though he rarely showed his competitiveness. From his viewpoint I was perhaps one of his competitors who must be humiliated, in spite of the fact that I was not working in his field. Here I may have written more than is necessary, but my concluding point is: He did so, even though I did nothing to him.

The book contains quite a few other unpleasant characterizations of other people, together with assurances that everyone else shared his view of the person in question. I know for a fact that in at least one case this is untrue:

> A well known math-physicist Eugene Wigner was in our department, and so I occasionally talked with him. He was pompous and took himself very seriously. That is the impression shared by all those who talked with him.

Wigner was still around when I was a student at Princeton and often came to tea. My impression of him was not at all that which Shimura claims to have been universal.

Update: An exchange between Shimura and Bott about the Woods Hole story can be found here.

Comments

1. **chickenbreeder**
   November 22, 2008

   Interesting. I will probably buy a copy of the book if it's not expensive.

   A book like this is not unprecedented. Andrei Sakharov's memoir has a lot of moments when he pointedly accused his colleagues of character flaws. Those include at least one for Zeldovich, who had been supportive of him most of the time and was considered by AS as a friend. In fact, I think this is exactly what a good memoir should be: Honest recollection of one's thoughts and feelings, at the end of one's career when there's nothing more at stake. This also serves as a release, or else one would have to bring those thoughts and feelings alone to the grave.

   (In the revised version of his memoir, Sakharov mentioned that he and Zeldovich eventually reconciled.)

2. **Coin**
   November 22, 2008

   Maybe not the most appropriate question to ask, but I cannot help but wonder– does Shimura's book have anything of detail to say about Taniyama, or attempt to offer any insight into his suicide?

3. **Neville**
   November 22, 2008

   It sounds like a rather sad book. Life should be a happy adventure.

4. **mathematician**
   November 22, 2008

Is there any reason to doubt the general picture portrayed by Shimura of a not always pleasant sociology. I would tend to take him at his word (with an appropriate grain of salt). Surely there are at least others in that circle who could confirm the existence of this kind of negative atmosphere.

I know nothing of this group, but I have seen and experienced negative things in my own circles, and I am acutely aware of how much people resent being informed of it.

5. **anonymous**
November 23, 2008

One is obviously reminded of Grothendieck's "Recoltes et Semailles", where similar "score settling" sadly occurs.

When I've got a grudge against someone, I take it up with him/her, not with the general public.

6. **MathPhys**
November 23, 2008

I know that Grothendieck's accusations of a certain person in R et S were completely unfounded.

Grothendieck was a very great man, but when he wrote R et S, he was too angry to see that others may have perfectly valid explanations for their actions.

In any case, the people that he attacked were all still around and active when R et S was distributed.

In Shimura's case, the situation is different as many of the people that he talks about are no longer in a position to defend themselves.

7. **Peter Woit**
November 23, 2008

Coin,

There's a bit in the book about Taniyama, no insight into the reason for his suicide.

mathematician,

I gave one reason to doubt Shimura's portrayals of other people's failings, based on my own personal experience. I've never met Serre personally, but Shimura's claim that he was jealous because of the Wood's Hole conjecture is hard to believe. The currency of renown among mathematicians is theorems, not conjectures….

8. **Per**
November 23, 2008

If the text above is true, then it is a bit sad that excellence in mental achievements does not imply growth of other aspects of personality.

Egomaniacs seem to roam the halls of famous mathematics institutions also.

9. **mathematician**
November 23, 2008

Peter, okay you have a point that Shimura's claims of the form "everyone agrees with me" are easily refuted by people disagreeing with him.

But his claims of the form "person X has certain negative characteristics" are not so easily refuted by people disagreeing with him. People may not like hearing such things, but if they know nothing of the people involved they should just remain agnostic on the issue. I object to people automatically rejecting his claims, just because they are negative.

I have personal experience of being severely mistreated by someone who is viewed positively by many people and who has received awards and recognition for being something completely different to what I am describing. Several years ago I was almost driven to suicide by this person, and I believe there is another case where the word "almost" does not apply. And yet I just have to keep my mouth shut about it, because of the backlash that there is to such negative claims.

10. **st**
November 23, 2008

The academe in previous decades was not nearly as open-minded as it is today. I am sure as a foreign Japanese scholar working in the US back then, he must have felt at least some form of subtle prejudice or discrimination. From the excerpts and letter to the AMS, it appears that Shimura has felt that his ideas were often ignored or belittled and would like proper credit for his work. Although he may come across as a bit paranoid in his writing, that doesn't mean his observations about his colleagues were inaccurate or baseless, though the assertion that "all" agree with his opinion is exaggerated.

11. **woit**
November 23, 2008

st,

If Shimura felt discriminated against by anyone for being Japanese, he doesn't mention that in his book.

12. **st**
November 23, 2008

Peter,

I mentioned discrimination because it was well known at the time. For example, in his paper *A History of Mathematics at Princeton University* (available here), Michael Seip writes:

> Gian Carlo Rota was an undergraduate mathematics major at Princeton in the early 1950's, a period marked by growth of the department and political upheaval in academia. He describes another, more ugly, notion of the times; nationality prejudices in academia. Many notable professors from Europe and the Far East were allowed to visit American universities, but not many were granted tenure. This bias was expressed at Princeton in several notable instances. The geometer Kunihiko Kodaira, from Japan, was appointed to a six-year term but was not awarded tenure. Though Kodaira, "whose work in geometry was revered by everyone in the Princeton main line", (Rota, p. 226) was an accomplished and productive researcher, several members of the Princeton faculty were opposed to his award of tenure on the basis of his nationality and he was voted down.

On a different note, the paper continues with this observation:

> "Bias also existed towards those men who studied subjects considered intangible at the time…"

This type of bias, unfortunately, is still pretty much alive today.

13. **Kea2**
November 24, 2008

Over the years, Shimura has increasingly isolated himself from other mathematicians. Like Grothendieck, he can write very well about nonmathematical things — for example, his memoir on Taniyama (BLMS 1989) is very moving — but I wouldn't give any more credence to his characterizations of other mathematicians than I do to Grothendieck's.

14. **Jean-Paul Billon**
November 24, 2008

Whatever the domain, to push one's ideas leads to be shot at will by people you were trusting to be fair great minds. Mathematicians are not outside of the current universe under this aspect. Having been a mathematicinas in a former life, I was hurt by the buzz that a very obvious fundamental theorem I was using to base on a new method of theorem proving was wrong. No luck for my detractors, it was a well known theorem proven by Shannon. Since I have been moving to industry and now to politics. Same game. But you learn how to get a thick skin. Whoever believes that fundamental science is clean from any blow under the belt is a naive. Novertheless, sciens is moetimes progressing, which is great. BTW, did you know that Einstein was barred from tenure in France when seeking asylum…?

15. **N. Nakanishi**
November 24, 2008

My impression of E. Wigner is common to P. Woit's one. In 1961, at Princeton I met him and reported that I had translated his famous essay entitled "The Unreasonable Effectiveness of Mathematics in the Natural Sciences" into Japanese. He was extremely polite to such a young Japanese as me.

16. **sz**
November 25, 2008

Mathematicians always behave like children, even great ones like Shimura are no exception. In this book Shimura portrayed himself more like an aged Tom Sawyer than a matured adult. I don't see anything wrong about it, I believe his words are sincere and truthful.

In my naive psycho-analysis, I think sometimes when a person describes another person, he unconsciously projects his own shadow over the others' image. In Shimura's opinion about Harish-Chandra, I very much see an image of Shimura himself.

But I do think this is different from Grothendieck's "Recoltes et Semailles", because Grothendieck is not a traditional mathematician. What he did in mathematics is, in some sense, not mathematics at all, but rather like mythology of Ramanujan…it's like he hits the weakness of standard mathematical thinking in some way. His anger towards the others maybe come from the fact that even those closest to him can not or would not follow his way of thinking.

17. **a.k.**
November 25, 2008

..discrimination was not only 'well-known at that time', it certainly influenced the mathematical community not only in those days. In difference to any other subclass of society, the existence of discrimination by race, class or gender among mathematicians is not a 'valid' subject of discussion, is is assumed to be non-existent due to the general 'level of civilisation', which one presupposes for mathematicians. This is -without any doubt- the (conscious or subconscious) reason why Shimura did not explicitly mention the term of discrimination in his book, although it seems to be clearly implicit. It is a common phenomenon for people experiencing discrimination to react in a way which seems 'paranoid' to the observer, even or especially if the actual formulation of the term 'discrimination' is, for some reason, not an accepted way to choose for the person in case, for personal or 'external' reasons.

18. **former mathematician**
November 30, 2008

I can't resist the opening to report a Shimura anecdote from the 50's or 60's, when it seemed that he was publishing at least one 45-page paper each month.

A mathematician was browsing through typewriter ribbons ath the university store. Shimura approached him and said, "Don't get that brand; get this brand. You will find it helps write better papers."

19. **Peter Woit**
November 30, 2008

fm,

Funny, typewriter ribbons seem to be a big concern of Shimura's. In his book, he describes one city in Japan where he worked as kind of a wasteland, with the main evidence being that he had trouble finding a place to buy typewriter ribbons.

20. **anonymous**
December 6, 2008

Jean-Pierre Serre was probably the greatest mathematician of the second part of this century. He remained very creative over the years. Of course there were very creative new comers, who started at the right time. But the fact that Jean-Pierre Serre lasted so long must be irritating to some…

21. **Betsy Devine**
December 9, 2008

What a strange characterization of Wigner, who was shy and so European-polite as to seem quite formal.

I remember in the mid or late 1970s when the talk of Princeton physics was Wigner's courting Pat Hamilton, the widow of another physicist who was soon to become his third wife. Joan Treiman told me that on summer evenings you could see them out strolling together, holding hands. I never saw this phenomenon, but it was a real pleasure to see them at physics parties looking both so, so happy. My impression was that Wigner's colleagues loved as well as admired him.

22. **st**
December 10, 2008

I wonder whether mathematicians, in general, are always aware of how they come across to others especially in social situations. I would imagine that this awareness is probably below average as a group, although there are some with average to superb social skills. This is because we're trained to be direct, specific, and honest in our communication. There's little room for "spinning" when proving a theorem. As Gian-Carlo Rota once observed, mathematicians have "bad personalities," make "terrible salesmen," and are "totally devoid of common sense."

To give an example from my own experience, I've pointed out several relatively minor mistakes to a speaker in public without realizing at the time that that might have been offensive. For us, it is almost an instinct to correct. So I can understand how someone "asking questions on the most trivial points" might just be trying to understand the material rather than to intentionally annoy. What is "trivial" to one expert may be puzzling to others not directly working in the area. Coupled that with the extra-sensitivity of a person who might have experienced discrimination, it is easy to give birth to a "grudge" without the other party being aware of any wrongdoing. Such misunderstanding is unfortunate indeed.

23. **Anonymous**
    February 7, 2009

    Goro Shimura recently published this book too:

    Link can be found here:

    http://www.tenspeed.com/store/?main_page=pubs_product_book_jph1_info&products_id=2465

24. **anon smith**
    February 27, 2009

    Professor Goro Shimura, one of the greatest mathematicians of our day, has written a wonderful book about something well outside of mathematics: an aspect of Japanese culture that should be of great interest for anyone interested in Japanese civilization. I congratulate him for writing so eloquently outside the field of mathematics and for showing his sincere passion for an art form that is unique to historic Japan and its tradition. This book will enlighten and delight any reader with interest in Japan, in fine porcelain, and in ancient traditions.

Notes on BRST V: Highest Weight Theory

November 23, 2008
Categories: BRST

In the last posting we discussed the Lie algebra cohomology $H^*(\mathfrak g, V)$ for $\mathfrak g$ a semi-simple Lie algebra. Because the invariants functor is exact here, this tells us nothing about the structure of irreducible representations in this case. In this posting we'll consider a different sort of example of Lie algebra cohomology, one that is intimately involved with the structure of irreducible $\mathfrak g$-representations.

Structure of semi-simple Lie algebras

A semi-simple Lie algebra is a direct sum of non-abelian simple Lie algebras. Over the complex numbers, every such Lie algebra is the complexification $\mathfrak g_{\mathbf C}$ of some real Lie algebra $\mathfrak g$ of a compact, connected Lie group. The Lie algebra $\mathfrak g$ of a compact Lie group G is, as a vector space, the direct sum

$$\mathfrak g=\mathfrak t \oplus \mathfrak g/\mathfrak t$$

where $\mathfrak t$ is a commutative sub-algebra (the Cartan sub-algebra), the Lie algebra of T, a maximal torus subgroup of G.

Note that $\mathfrak t$ is not an ideal in $\mathfrak g$, so $\mathfrak g/\mathfrak t$ is not a subalgebra. $\mathfrak g$ is itself a representation of $\mathfrak g$ (the adjoint representation: $\pi(X)Y=$), and thus a representation of the subalgebra $\mathfrak t$. On any complex representation V of $\mathfrak g$, the action of $\mathfrak t$ can be diagonalized, with eigenspaces V^λ labeled by the corresponding eigenvalues, given by the weights λ. These weights $\lambda\in\mathfrak t_{\mathbf C}^*$ are defined by (for $v\in V^\lambda,\ H\in \mathfrak t$):

$$\pi(H)v=\lambda(H)v$$

Complexifying the adjoint representation, the non-zero weights of this representation are called roots, and we have

$$\mathfrak g_{\mathbf C}=\mathfrak t_{\mathbf C} \oplus ((\mathfrak g/\mathfrak t)\otimes\mathbf C)$$

The second term on the right is the sum of the root spaces V^α for the roots α. If α is a root, so is $-\alpha$, and one can choose decompositions of the set of roots into "positive roots" and "negative roots" such that:

$$\mathfrak n^+=\bigoplus_{+\ roots\ \alpha}(\mathfrak g_{\mathbf C})^\alpha,\ \mathfrak n^-=\bigoplus_{-\ roots\ \alpha}(\mathfrak g_{\mathbf C})^\alpha$$

where $\mathfrak n^+$ (the "nilpotent radical") and $\mathfrak n^-$ are nilpotent Lie subalgebras of $\mathfrak g_{\mathbf C}$. So, while $\mathfrak g/\mathfrak t$ is not a subalgebra of $\mathfrak g$, after complexifying we have decompositions

$$(\mathfrak g/\mathfrak t)\otimes \mathbf C=\mathfrak n^+ \oplus \mathfrak n^-$$

The choice of such a decomposition is not unique, with the Weyl group W (for a compact group G, W is the finite group N(T)/T, N(T) the normalizer of T in G) permuting the possible choices.

Recall that a complex structure on a real vector space V is given by a decomposition

$$V\otimes \mathbf C=W\oplus\overline{W}$$

so the above construction gives |W| different invariant choices of complex structure on $\mathfrak g/\mathfrak t$, which in turn give |W| invariant ways of making G/T into a complex manifold.

The simplest example to keep in mind is $G=SU(2),\ T=U(1),\ W=\mathbf Z_2$, where $\mathfrak g=\mathfrak{su}(2)$, and $\mathfrak g_{\mathbf C}=\mathfrak{sl}(2,\mathbf C)$. One can choose T to be the diagonal matrices, with a basis of $\mathfrak t$ given by

$$\frac{i}{2}\sigma_3=\frac{1}{2}\begin{pmatrix}i&0\\0&-i\end{pmatrix}$$

and bases of $\mathfrak n^+,\ \mathfrak n^-$ given by

$$\frac{1}{2}(\sigma_1+i\sigma_2)=\begin{pmatrix}0&1\\0&0\end{pmatrix},\ \frac{1}{2}(\sigma_1-i\sigma_2)=\begin{pmatrix}0&0\\1&0\end{pmatrix}$$

(here the σ_i are the Pauli matrices). The Weyl group in this case just interchanges $\mathfrak n^+ \leftrightarrow \mathfrak n^-$.

Highest weight theory

Irreducible representations V of a compact Lie group G are finite dimensional and correspond to finite dimensional representations of $\mathfrak g_{\mathbf C}$. For a given choice of $\mathfrak n^+$, such representations can be characterized by their subspace $V^{\mathfrak n^+}$, the subspace of vectors annihilated by $\mathfrak n^+$. Since $\mathfrak n^+$ acts as "raising operators", taking subspaces of a given weight to ones with weights that are more positive, this is called the "highest weight" space since it consists of vectors whose weight cannot be raised by the action of $\mathfrak g_{\mathbf C}$. For an irreducible representation, this space is one dimensional, and we can label irreducible representations by the weight of $V^{\mathfrak n^+}$. The irreducible representation with highest weight λ is denoted V_{λ}. Note that this labeling depends on the choice of $\mathfrak n^+$.

Getting back to Lie algebra cohomology, while $H^*(\mathfrak g, V)=0$ for an irreducible representation V, the Lie algebra cohomology for $\mathfrak n^+$ is more interesting, with $H^0(\mathfrak n^+, V)=V^{\mathfrak n^+}$, the highest weight space. $\mathfrak t$ acts not just on V, but on the entire complex $C(\mathfrak n^+, V)$, in such a way that the cohomology spaces $H^i(\mathfrak n^+,V)$ are representations of $\mathfrak t$, so can be characterized by their weights.

For an irreducible representation V_λ, one would like to know which higher cohomology spaces are non-zero and what their weights are. The answer to this question involves a surprising "ρ – shift", a shift in the weights by a weight ρ, where

$$\rho=\frac{1}{2}\sum_{+\ roots} \alpha$$

half the sum of the positive roots. This is a first indication that it might be better to work with spinors rather than with the exterior algebra that is used in the Koszul resolution used to define Lie algebra cohomology. Much more about this in a later posting.

One finds that $dim\ H^*(\mathfrak n^+,V_\lambda)=|W|$, and the weights occuring in $H^i(\mathfrak n^+,V_\lambda)$ are all weights of the form $w(\lambda +\rho)-\rho$, where $w\in W$ is an element of length i. The Weyl group can be realized as a reflection group action on $\mathfrak t^*$, generated by one reflection for each "simple" root. The length of a Weyl group element is the minimal number of reflections necessary to realize it. So, in dimension 0, one gets H^0(\mathfrak n^+, V_\lambda)=V^{\mathfrak n^+} with weight \lambda, but there is also higher cohomology. Changing one's choice of \mathfrak n^+ by acting with the Weyl group permutes the different weight spaces making up H^*(\mathfrak n^+, V). For an irreducible representation, to characterize it in a manner that is invariant under change in choice of \mathfrak n^+, one should take the entire Weyl group orbit of the \rho – shifted highest weight \lambda, i.e. the set of weights

\{w(\lambda +\rho),\ w\in W\}

In our G=SU(2) example, highest weights can be labeled by non-negative half integral values (the "spin" s of the representation)

s=0,\frac{1}{2},1,\frac{3}{2}\2,\cdots

with \rho=\frac{1}{2}. The irreducible representation V_s is of dimension 2s+1, and one finds that H^0(\mathfrak n^+,V_s) is one-dimensional of weight s, while H^1(\mathfrak n^+,V_s) is one-dimensional of weight -s-1.

The character of a representation is given by a positive integral combination of the weights

char(V)=\sum_{weights\ \omega} (dim\ V^\omega)\omega

(here V^\omega is the \omega weight space). The Weyl character formula expresses this as a quotient of expressions involving weights taken with both positive and negative integral coefficients. The numerator and denominator have an interpretation in terms of Lie algebra cohomology:

char(V)=\frac{\chi(H^*(\mathfrak n^+, V))}{\chi(H^*(\mathfrak n^+, \mathbf C))}

Here \chi is the Euler characteristic: the difference between even-dimensional cohomology (a sum of weights taken with a + sign), and odd-dimensional cohomology (a sum of weights taken with a – sign). Note that these Euler characteristics are independent of the choice of \mathfrak n^+.

The material in this last section goes back to Bott's 1957 paper *Homogeneous Vector Bundles*, with more of the Lie algebra story worked out by Kostant in his 1961 *Lie Algebra Cohomology and the Generalized Borel-Weil Theorem*. For an expository treatment with details, showing how one actually computes the Lie algebra cohomology in this case, for U(n) see chapter VI.3 of Knapp's *Lie Groups, Lie Algebras and Cohomology*, or for the general case see chapter IV.9 of Knapp and Vogan's *Cohomological Induction and Unitary Representations*.

Comments

1. **D R Lunsford**
   November 24, 2008

   Well what did you ever determine intuitively about the bifurcation of the world represented by W direct bar W? There are many candidates, but they come down to matter vs. antimatter. I mean how did you come to think about it? This issue comes up again and again.

   -drl

2. **Peter Woit**
   November 24, 2008

   drl,

   This is just the completely conventional way mathematicians think of what it means to put a complex structure on a real vector space.

   I should have commented somewhere about the fact that the whole highest-weight theory set-up is very much analogous to quantum field theory, with the highest weight vector playing the role of a the vacuum vector. You get something just like the particle-anti-particle business.

   In the case of affine Lie algebras, the analogous construction is exactly one that comes from 1+1 d QFT.

3. **newcomer to QFT**
   December 9, 2008

   "The answer to this question involves a surprising ρ – shift", a shift in the weights by a weight ρ, where

   $$\rho=\frac{1}{2}\sum_{+} roots} \alpha$$

   half the sum of the positive roots. This is a first indication that it might be better to work with spinors rather than with the exterior algebra that is used in the Koszul resolution used to define Lie algebra cohomology."

   See Ettienne Rassart's thesis:
   http://www.math.cornell.edu/~rassart/pub/EtienneRassart_Thesis.pdf

   for another way of avoiding the ρ-shift without using spinors.

The Landscape at Princeton

November 24, 2008
Categories: Multiverse Mania

The Princeton Center for Theoretical Science has been having a mini-symposium on the string theory Landscape, and as part of this today hosted a “panel discussion” on the topic. It turns out that there’s not a lot of support for the Landscape in Princeton.

Michael Douglas was the only real Landscape proponent in evidence. He gave a presentation on the state of Landscape studies, beginning by noting that landscapeologists keep finding more possible string vacua. Evidently the 10^500 number always quoted for the number of semi-realistic vacua is no longer operative, with latest estimates more like 10^(10^5) or higher. Douglas acknowledged that this pretty much removes any hope of making predictions by using experiment to fix this freedom and end up with non-trivial constraints. All that’s left is the idea of doing statistical calculations, but there the problem is that you don’t know the measure. He ended up mainly talking about cosmology, partly about the hope that maybe cosmology would constrain the possible vacua, as well as going over various ideas for putting a measure on the space of vacua. None of this really seems to lead anywhere, with all proposed measures having a rather ad hoc character. Douglas advocated just trying to count all vacua with the same weight, since at least one might hope to calculate that.

Tom Banks began by claiming that the effective field theory picture used in the landscape is just not valid. He also pointed out that if the landscape arguments were valid, the landscape would be disconfirmed by experiment, since 10-20 of the Standard Model parameters are unconstrained by anthropics, but take unusually small values, not the random distribution one would expect. Banks takes the attitude that the CC probably has an anthropic explanation, but not particle physics or the SM parameters. He also attacked the usual claims that different vacua are all states of the same theory, arguing that they instead correspond to different theories. Finally, he pointed out that the one prediction that landscapeologists had claimed they would be able to make, the scale of SSYM breaking, hadn’t worked out at all (Douglas now acknowledges that this can’t be done).

Nati Seiberg then argued that, as one gets to deeper and deeper levels of understanding of particle physics, one might reach a level where the only explanations are environmental and have to give up. He sees no reason for that to be the case now, with the main problem that of EWSB, and nothing to indicate that anthropics has anything to do with the problem. Rather, the problem is there because we haven’t had high enough energy accelerators (the LHC should change that), and the problem is hard. He ended by saying that the appropriate response at the present time to anthropic arguments like the Landscape is to just ignore them.

The last speaker was Nima Arkani-Hamed, who I suppose was chosen as a proponent of anthropics. He didn’t live up to this, saying that he pretty much agreed with Seiberg. Like Banks, he finds the anthropic explanation of the CC a plausible reason for why no one has come up with a better idea. He did say that thinking about anthropics and the Landscape has led people to look at some possiblilities for particle physics that otherwise would not have been examined. About the cosmological issues brought up by Douglas, his opinion is that there’s probably no point to thinking about these questions now, doing so might be like trying to come up with a theory of superconductivity in 1903. As far as EWSB goes, he believes the LHC will show us a non-anthropic explanation for its scale.

He explicitly attacked the discussion of measures that Douglas had engaged in as “not fruitful”, saying that he didn’t see any “endgame”, that it was wildly improbably that these could predict anything about particle physics. He also doesn’t see why our vacuum should be typical, joking that some of the least typical people in the world (Linde was mentioned) are most devoted to claiming that our universe is typical. He went on to argue for the currently fashionable enterprise of studying S-matrix amplitudes, arguing that looking at the local physics embodied in Lagrangians was no longer so interesting, that instead one should be trying to understand questions where locality is not manifest.

Finally, Arkani-Hamed ended with the statement that string theory is useful as a way to study questions about quantum gravity, but “unlikely to tell us anything about particle physics”. This is an opinion that has become quite widespread among theorists, but news of this has not gotten out to the popular media, where the idea that string theory has something to do with the LHC keeps coming up.

So, all in all, I found myself in agreement with most of the speakers. On another positive note, the math and physics book collection at Labyrinth (which has replaced the U-store bookstore) has improved dramatically.

Comments

1. **anon.**
   November 25, 2008

   “Evidently the 10^500 number always quoted for the number of semi-realistic vacua is no longer operative, with latest estimates more like 10^(10^5) or higher. Douglas acknowledged that this pretty much removes any hope of making predictions by using experiment to fix this freedom and end up with non-trivial constraints.”

   But isn’t the mainstream string theorist argument now the exact opposite, i.e. “the bigger the landscape, the better”? See:

   http://motls.blogspot.com/2006/06/top-twelve-results-of-string-theory.html

   According to Joe Polchinski (who is has done research in string theory and written a large textbook on the subject as you are aware), the twelfth top result of string theory is precisely:

   “The existence of the landscape, a *large enough set* of metastable solutions that the cosmological constant can adjust to a value small enough as to allow organized structures (which require many bits and many cycles). [hep-th/0603249]” (Emphasis added.)

   Therefore, the bigger the size of the landscape, the higher the probability that it can be somehow adjusted to fit observed physics, in principle. If the landscape size was really small, string theory could be falsified easily, and these experts (who appreciate a really big landscape over a small one) wouldn’t waste their time on it.

2. **Peter Woit**
   November 25, 2008

   anon.,

   Landscape proponents want at least 10^120 vacua, so as to anthropically solve the CC problem. They would like to get a prediction about something else, and for this the much larger numbers being considered now are a problem.

3. **Shantanu**
   November 25, 2008

   Peter, can you post a link to the conference site/talks if available?
   thanks

4. **Peter Woit**
November 25, 2008

Shantanu,

The only info about the conference available on-line that I know of is here:

http://pcts.princeton.edu/pcts/bigbang/Program-11-21-08.pdf

at the panel discussion, Douglas has quite a few slides, Banks just a few, Seiberg and Arkani-Hamed made informal remarks. I don't think anything was recorded, or that more will be available on-line.

5. **Tony Smith**
November 26, 2008

Peter, the PCTS conference link you gave was interesting in what was omitted:
Witten was not listed as being involved.

Has Witten taken a clear position on the Superstring Landscape,
or
does it appear that he is just letting it hang itself and drift slowly in the wind ?

Tony Smith

6. **Aaron Bergman**
November 26, 2008

Witten is on sabbatical at CERN.

7. **Peter Woit**
November 26, 2008

Tony,

As Aaron points out, Witten is not in Princeton this year. My impression is that he, like just most of the theorists in Princeton, is no fan of the landscape, and even if he were around, might steer clear of much involvement in this kind of program. What I find remarkable about the landscape story is how its proponents have managed to give the public the impression that it is the dominant point of view among string theorists. In actuality, I think most string theorist's attitude toward it is that of Seiberg: it should just be ignored.

8. **M**
November 27, 2008

ignoring the string landscape implies ignoring string theory

9. **Sumar Ongi**
November 27, 2008

I may be wrong, but I think not too long ago Arkani-Hamed was actively working with landscape-inspired models. (I even vaguely remember his calling this kind of work "staring the monster in the face", or something like that, in an interview.) If so, his change of opinion about those models (and about the relationship between ST and Particle Physics) is quite remarkable. More generally, it seems to me that all the opinions quoted above from well known theorists about these matters reflect a tidal change of mind in the string community.

10. **Arun**
November 27, 2008

I think it is a sign of progress that 10^500 has morphed to 10^100000. It is much more effective in keeping more people from wasting their time on it. Let a small contingent of researchers keep working on this and improving the intractability results of the landscape.

11. **Tony Smith**
November 27, 2008

Sumar Ongi said "… Arkani-Hamed was … calling … landscape-inspired model … "staring the monster in the face" …"
and
Peter said "… Nima Arkani-Hamed … went on to argue for the currently fashionable enterprise of studying S-matrix amplitudes, arguing that looking at the local physics embodied in Lagrangians was no longer so interesting …".

Is Arkani-Hamed's "landscape-inspired model" that is "the currently fashionable enterprise of studying S-matrix amplitudes"
actually
his MARMOSET approach described in hep-ph/0703088
and discussed in a RESONAANCES blog entry dated 1 July 2007 entitled "Nima's Marmoset" ?

In view of the success of the Standard Model Local Lagrangian,
is it reasonable to say that "local physics" of "Lagrangians" is "no longer so interesting" ?

Tony Smith

12. **Peter Woit**
November 27, 2008

Tony,

You're mixing up three completely unrelated things (landscape-inspired models, studying S-matrix amplitudes, and MARMOSET), with nothing much in common except that Arkani-Hamed has worked on them.

13. **hmmm**
November 27, 2008

What exactly is it that there are N=10^{many} of? (What does "possible string vacua" mean?) How are they counted? Does each one of these N "vacua", have their own continuously variable parameters (like the standard model has), in which case, why don't they say there are infinitely many and be done with the counting?

Finally, is there any sense in which string theory "predicts" that these "vacua" actually exist in physical reality, or is it just that some people choose to add the separate and additional hypothesis that things that might be theoretically possible, are guaranteed to actually exist as part of physical reality? (And couldn't you just make a "landscape" out of the standard model and its own continuously variable parameters, or out of whatever variable model you like?)

It just seems that some of these theorists are taking their models way too seriously, and have too narrowly focused on a limited range of possibilities.

14. **Peter Woit**
November 28, 2008

hmmm,

For the answers to you first questions, see review articles by Douglas and others. Plenty of string vacua do have continuous parameters, but these are supposed to correspond to massless fields. We don't see these, so you ignore those vacua. The 10^500 or larger number is a counting of vacua that aren't in obvious conflict with experiment.

Whether these "vacua" are legitimate metastable ground states of the full non-perturbative string theory is controversial. No way to resolve the controversy, since no one knows what the full non-perturbative string theory is.

I've gone on at length elsewhere why this is different than the standard model, but the obvious point is that the string landscape is thoroughly non-predictive about anything, whereas the standard model makes a vast number of detailed, testable (and tested) predictions.

15. **dan**
November 30, 2008

PW,
Do you support research aimed at writing down "full non-perturbative string theory"?

What can full non-perturbative string theory tell us that current perturbation series can't?

regards,
dan

16. **Peter Woit**
December 1, 2008

dan,

Sure, some people should continue thinking about this. The problem right now is that, besides gauge/string duality, no one has any very promising ideas. There are a long list of things that people would like string theory to do (provide a predictive TOE, a full quantum theory of black holes, a string dual to QCD, etc, etc.), but no one can tell whether this will work, because no one is sure what string theory "is", outside of perturbation theory. If that question had a definitive answer, one could start getting agreement on what string theory is good for, and what it isn't good for, which would be very helpful.

17. **Gordon McCabe**
December 5, 2008

Do anthropic explanations include cosmological natural selection (a la Lee Smolin)?

18. **hmmm**
December 5, 2008

Gordon McCabe Says: "Do anthropic explanations include cosmological natural selection (a la Lee Smolin)?"

No. Definitely not. They are completely different.

Anthropic explanations are more akin to postulating that the universe generates a huge number of random configurations of matter, some of which happen to be elephants.

Cosmological natural selection is more akin to the usual natural selection and its explanation for elephants.

19. **Highspin**
December 6, 2008

10^500 might be "the number of semi-realisitic vacua in ST usualy quoted", but this is the number of vacua in a more or less typical KKLT flux compactification senario. There are a possibly infinite number of Calabi-Yau's etc. that you could use to play that game. The question is then what are the superselection sectors, but no one knows that I guess…

Two years ago in Les Houches Summer school, Nima was still a very strong proponent of anthropic argument, and not only for the CC. It was fun listening to him, even a 3 in the morning, and it got me thinking a lot. I'm glad if he changed his mind though.

20. **Gordon McCabe**
December 7, 2008

I recognize that cosmological natural selection (CNS) is distinct from the anthropic principle (AP); in CNS, a life-permitting universe is a typical member of the universe population, whilst according to the AP, it is a very special member of the population.

However, I wondered if CNS falls under the aegis of anthropic explanations for the purpose of the arguments in this article. I'm thinking of the following:

"He also pointed out that if the landscape arguments were valid, the landscape would be disconfirmed by experiment, since 10-20 of the Standard Model parameters are unconstrained by anthropics, but take unusually small values, not the random distribution one would expect. Banks takes the attitude that the CC probably has an

anthropic explanation, but not particle physics or the SM parameters."

Can cosmological natural selection explain those Standard Model parameters, or does it leave them unconstrained, just like the anthropic principle?

21. **hmmm**
    December 7, 2008

    To Gordon McCabe:

    Good question, I don't know. You would have to go to the original source. I would guess there is much less written on CNS than (various versions of) AP.

    But I would think that for Standard Model parameters that are unconstrained (or loosely constrained), it wouldn't matter. Their values could drift (between generations) with neutral effect, and so CNS would not "explain" their specific values, nor would those values falsify or confirm CNS. (Just like selection-neutral genes.) There are still plenty of (SM and cosmological) parameters that could falsify CNS.

    I'm not an expert. I just read some things and formed my own conclusions.

Latest From the LHC

November 25, 2008
Categories: Experimental HEP News

A talk at CERN today by Jorg Wenninger gives an update on the problems at Sector 34 and more information about what the prospects are for restarting the machine next year.

The cause of the accident has been identified as excessive resistance in a busbar interconnection between two magnets. Looking at logged data from before the accident, evidence for this excessive resistance was seen. Checking all the other sectors, a hint of a similar problem was found in one other cell, and that dipole will be replaced.

50 magnets are in the process of being removed from Sector 34, all to be out by Christmas. To avoid future similar accidents, the quench protection system is being upgraded, and the commissioning procedures will include a systematic search for excessive resistance problems. These measures can be implemented before next summer. There is also a plan is to add pressure release valves on every dipole cryostat, but this is highly problematic since it will require warming up all the sectors and likely would not allow the LHC to run with beam during 2009. The summary for 2009 plans reads:

> Plan A:
>
> - Restart in (late) summer of 2009 with beam.
> - Beam intensity and energy limited to minimize any risk.
>
> Plan B:
>
> - No beam before a complete 'upgrade' of the pressure relief system is implemented on all sectors.
> - Excludes beam in 2009.
>
> Final decision in February?

On a more cheerful note, tonight PBS will be broadcasting a documentary about the search for the Higgs at Fermilab called The Atom Smashers. It looks like this program should be about 10^(10^5) times better than a recent one featuring theorists. One of the filmmakers has a blog here. With the LHC out of commission for a while, the Higgs search at the Tevatron is where the action is, and the experimenters there may be the ones to find the Higgs or rule it out.

Update: Two more recent presentations with information (including pictures!) about the LHC accident, repairs and plans for the future are here and here. For now, the plan is for the machine to be cold again next July.

Comments

1. **Thomas R Love**
   November 25, 2008

   Peter, Thanks for mentioning "The Atom Smashers". I checked the local listings and it is not playing, but it is available on DVD, so I shelled out $27.05 and hope to have it in a week.

2. **observer**
   November 25, 2008

   I tought that The Lorentz force and its consecuences (quenching, resistance and of course heating to name a few)were a solved problem, looks that it is not, well we have to wait how the upgrades go, is getting interesting now.

3. **Patrick**
   November 26, 2008

   The sentence "the experimenters [at the Tevatron] may be the ones to find the Higgs" could probably be rendered more acccurate by adding "or to confirm the LEP hints at a mass around 115-116 GeV/c2". Actually, if the Higgs boson mass *is* at 115 GeV/c2 (as current insignificant Tevatron excesses seem to point to;-), it is only through a combination with LEP that the TeVatron might be able to confirm the observation (with a combined significance of 3 sigma). It actually turns out that many of the Tevatron current experimeters were LEP experimenters in 2000, so it's probably what would happen in this happy situation!

4. **Roger**
   November 26, 2008

   If the Higgs were to be discovered by the Tevatron or Tevatron+LEP this would not be a happy situation, especially if no exotic physics was found at the LHC.

   We sold the idea of the LHC as a means of discovering the Higgs – for the sake of future funding it would be best if the LHC find it or disprove its existence.

   I realise, however, we live in the real world and I would be thrilled if the Tevatron picked the Higgs up first but this would cause problems later down the line when we ask for money for big projects.

5. **A.**
   November 26, 2008

   Sigh. I thought a bonus to landing another postdoc this year was that, as the LHC was coming online, I might still be in academia when the fallout of "we found/didn't find the higgs/susy" etc etc started up.

   Also, the incoming director general of Cern, Rolf-Dieter Heuer, gave a talk on the LHC, this week, in Dublin. You can now stream it from http://www.rds.ie/cern.

6. **Bont**
   November 27, 2008

   I couldn't find what is quoted here from 'talk at CERN today.' Was it updated recently?

7. **Peter Woit**
   November 27, 2008

Bont,

The information quoted was from around page 45 on that document. Right now it appears to be having trouble loading, presumably because it has been slashdotted

http://science.slashdot.org/science/08/11/26/2135203.shtml

8. **Bont**
November 27, 2008

Mr. Woit,

When I loaded it yesterday, that page didn't exist 🙂 Anyway, now it is available. Thank you very much.

9. **observer**
November 27, 2008

Off-topic but I am sure interesting. I just read in the news (canadian television) that Stephen Hawking is coming to the Perimeter Institute by January of 2009.

10. **Patrick**
November 28, 2008

To Roger:

If the LHC were not to discover anything new, I agree entirely that we would not be in a happy situation ! It would probably mean the end of particle physics, irrespective of anything else.

I disagree, however, on the fact that the LHC was sold to discover the Higgs. It was sold as an extraordinary step in energy towards discovery of TeV new physics. Let's put it right: as repeatedly demonstrated in the past 15 years, an e+e- collider with energy of the order of mH + mZ + 30 GeV would be in much better a position to discover and finely study the Higgs boson than any hadron machine ever. In this perspective, the sooner the discovery is confirmed, the better !

(Note: I am not a TeVatron experimenter, but an LHC experimenter, former LEP experimenter)

11. **Logan**
November 28, 2008

Hey I just found your blog and am pleasantly surprised. Thanks for letting me read your thoughts and ideas!
Good to know that the cause of the accident was found and that steps to prevent it from occurring again are being taken. It would be unfortunate if we didn't see another beam in 2009, but they gotta do what they gotta do.
Hahaha 10^(10^5) better? – looks like I should find a video of that broadcast! Thanks for the heads up.

12. **Clayton**
November 28, 2008

Hello — I'm one of the co-directors of "The Atom Smashers." Thanks for the link to my blog about the making of the film! I'm not sure about "10^(10^5) times better" than the other film you mentioned (that's quite a tall order), but I hope our film was enjoyable to those of you who watched. If you missed it, or are interested in buying a copy (the dvd for sale is the 73-minute "director's cut" with some extras, including the video Leon Lederman's made to try to convince Ronald Reagan to support the Superconducting Super Collider), you can do that by visiting our site at http://www.theatomsmashers.com. Also the film will be shown again on PBS on January 27.

We struggled with depicting many of the complexities of the fascinating search for the Higgs from the standpoint of Fermilab — it was a great story to tell with many interesting people and complex issues. We're interested to hear your thoughts.

13. **mike**
November 28, 2008

Maybe CERN can be convinced to double their efforts in bringing the LHC online sooner. An argument can be made that if landscape theory is proven with the LHC, we can then transport ourselves to a parallel universe where our trillion $ debt suddenly becomes a trillion $ surplus which will then pay for the additional time spent on the LHC. I think this is referred to in political circles as a self-fulfilling investment plan (or alternatively a scientific bailout)!

14. **SpaceTime**
November 30, 2008

Mini-blackholes http://www.aip.org/pnu/2008/split/871-1.html

The Atom Smashers did broadcast on our local OTA (Over the Air) PBS station here WPBT in South Florida but it played on an OTA digital only channel (2.2) so if your using cable/dish you may not get it. PBS often repeat shows on other digital channels and they also run on a sister station channel 17 WLRN.

This was an excellent show and pointed out the chronic anti-intellectual trend.

15. **Paul Collins**
December 3, 2008

It looks like access to the second presentation is restricted. The first one was very informative, however.

16. **woit**
December 3, 2008

Thanks Paul,

Linking from here to a previously public CERN source of technical information about the state of the LHC seems to often lead them to shut off public access. This is too bad. In the past, their policy was mostly to be very open with technical information about exactly what was going on at the LHC. It appears that this has changed, in response to the huge amount of press attention they are getting, and because of the accident this past September.

17. **Paul Collins**
December 5, 2008

Looks like the second one is available again.

From Quarks to Strings

December 2, 2008
Categories: Uncategorized

There's a new preprint on the arXiv from Polyakov, entitled From Quarks to Strings, in which he tells the story of his involvement with string theory over the years. He begins:

> In the sixties I was not much interested in string theory. The main reason for that was my conviction that the world of elementary particles should allow field theoretic description and that this description must be closely analogous to the conformal bootstrap of critical phenomena. At the time such views were very far from the mainstream. I remember talking to one outstanding physicist. When I said that the boiling water may have something to do with the deep inelastic scattering, I received a very strange look. I shall add in the parenthesis that this was a beginning of the long series of "strange looks" which I keep receiving to this day.
>
> Another reason for the lack of interest was actually the lack of abilities. I could not follow a very complicated algebra of the early works on string theory and didn't have any secret weapon to struggle with it.

After the asymptotic freedom breakthrough, Polyakov quickly saw that a non-perturbative understanding of gauge theory was needed. He (independently from Wilson) developed lattice gauge theory, but acknowledges that, unlike Wilson, he did not have the Wilson loop criterion for confinement. In 3d, he worked out the dual-superconductor picture, where monopoles are responsible for confinement, and he has interesting comments about efforts over the years to use instantons in 4d, something that works for the N=2 supersymmetric case (Seiberg-Witten, and Nekrasov), but not for QCD itself.

The strong-coupling lattice expansion was one thing that encouraged him to look for a gauge/string duality as a way to solve QCD. One idea was to write dynamical equations for the Wilson loop (later called Migdal-Makeenko equations) and find a solution to these as a path integral over surfaces, thus a string theory. About this he writes:

> This action is called now the Polyakov action, demonstrating the Arnold theorem,stating that things are never called after their true inventors.

He derived the critical dimension for the string, but was much more interested in trying to understand non-critical strings, especially the four-dimensional string as a tool for studying gauge theories, and the 3d string as a tool to solve the 3d Ising model. Later he realized that it was natural to think of the Liouville mode as a fifth dimension, and by 1996 was studying the idea of using warped 5 dimensions to get gauge/string duality. Here's how he describes what he was doing in the lead-up to the breakthrough by Maldacena which led to the AdS/CFT conjecture:

> At this point I was certain that I have found the right language for the gauge/ strings duality. I attended various conferences, telling people thatit is possible to describe gauge theories by solving Einstein-like equations (coming from the conformal symmetry on the world sheet) in five dimensions. The impact of my talks was close to zero. That was not unusual and didn't bother me much. What really caused me to delay the publication () for a couple of years was my inability to derive the asymptotic freedom from my equations. At this point I should have noticed the paper of Igor Klebanov in which he related D3 branes described by the supersymmetric Yang Mills theory to the same object described by supergravity. Unfortunately I wrongly thought that the paper is related to matrix theory and I was skeptical about this subject. As a result I have missed this paper which would provide me with a nice special case of my program. This special case was presented little later in full generality by Juan Maldacena and his work opened the flood gates.

He goes on to make some intriguing comments about the questions of integrability, and that of how to truly understand and derive gauge/string duality from first principles:

> The problem of reproducing gauge perturbation theory from the string theory side remains unsolved (and extremely important).
>
> Why should we care about the derivation from the first principles ? After all, in physics we value not so much the proved theorems but correct and powerful statements. However, in this case the lack of the derivation really impedes progress. We do not know how far the gauge /string duality can be extended and generalized. The enormous accumulation of special cases has been useful but not sufficient for deeper understanding. This is why I think that establishing the foundations is one of the most important problems in the field.

Polyakov seems to have always been a skeptic about the idea of using the 10 dimensional superstring to construct a unified theory, instead hoping that some understanding of the 4d non-critical string might lead somewhere. Like most string theorists, he takes the attitude that we need to wait for the results from the LHC and hope that a new clue will emerge and tell us how to make progress along these lines:

> As for the problem of string unification, it seems to me that non-critical strings may have some future. However, it may be wise to wait for some more information about Nature (specifically about supersymmetry) which we expect to get from the LHC.

Comments

1. **Shantanu**
   December 3, 2008

   Peter, do you know of or have you read anywhere about Polyakov's views/opinions about LQG?

2. **Peter Woit**
   December 3, 2008

   Shantanu,

   No, I've never heard anything about what Polyakov might think of LQG.

BRST News

December 2, 2008
Categories: BRST

I should finish writing the next installment of the Notes on BRST series soon, but thought I'd post here about two pieces of BRST-related news, concerning the "B" and the "T".

- The "T" in BRST is I.V. Tyutin, whose Lebedev preprint N. 39 from 1975 is considered to be one of the first uses of what was later to become known as BRST symmetry. This paper was never published and the preprint has not been widely available (in particular, I've never seen a copy of the original). This evening one of the new preprints in the arXiv hep-th section is a copy of the 1975 preprint, making it now available on the web.
- The "B" in BRST is Carlo Becchi, who together with Camillo Imbimbo has written an article on BRST for Scholarpedia entitled Becchi-Rouet-Stora-Tyutin symmetry. Scholarpedia has the interesting feature of making available (here) the discussion between reviewers and authors of the article, which can be enlightening.

Comments

1. **.**
   December 2, 2008

   «in particular, I'd never seen a copy»

   typo?

2. **Peter Woit**
   December 2, 2008

   More like bad grammar or something… Will improve.

3. **pedant**
   December 4, 2008

   `I had never seen' is good grammar, since you did see a copy before writing this post. 🙂

4. **N. Nakanishi**
   December 5, 2008

   Previously, I discussed the literature concerning the names of BRS and BRST. See Opening Address — "BRS" OR "BRST" ? — of "BRS Symmetry, Proceedings of the International Symposium on the BRS Symmetry on the Occasion of Its 20th Anniversary Held in Kyoto, Japan, September 18-22, 1995" edited by M. Abe, N. Nakanishi, and I. Ojima. Stora's paper in this volume also contains his comment on Tuytin's work.

Notes on BRST VI: Casimir Operators

December 3, 2008
Categories: BRST

For the case of G=SU(2), it is well-known from the discussion of angular momentum in any quantum mechanics textbook that irreducible representations can be labeled either by j, the highest weight (here, highest eigenvalue of J_3), or by $j(j+1)$, the eigenvalue of $\mathbf{J\cdot J}$. The first of these requires making a choice (the z-axis) and looking at a specific vector in the representation, the second doesn't. It was a physicist (Hendrik Casimir), who first recognized the existence of an analog of $\mathbf{J\cdot J}$ for general semi-simple Lie algebras, and the important role that this plays in representation theory.

The Casimir Operator

Recall that for a semi-simple Lie algebra $\mathfrak g$ one has a non-degenerate, invariant, symmetric bi-linear form $(\cdot,\cdot)$, the Killing form, given by

$$(X,Y)= tr(ad(X)ad(Y))$$

If one starts with $\mathfrak g$ the Lie algebra of a compact group, this bilinear form is defined on $\mathfrak g_{\mathbf C}$, and negative-definite on $\mathfrak g$. For a simple Lie algebra, taking the trace in a different representation gives the same bilinear form up to a constant. As an example, for the case $\mathfrak g_{\mathbf C}={\mathfrak{sl}(n,\mathbf C)}$, one can show that

$$(X,Y)=2n\ tr(XY)$$

here taking the trace in the fundamental representation as n by n complex matrices.
One can use the Killing form to define a distinguished quadratic element Ω of $U(\mathfrak g)$, the Casimir element

$$\Omega=\sum_iX_iX^i$$

where X_i is an orthonormal basis with respect to the Killing form and X^i is the dual basis. On any representation V, this gives a Casimir operator

$$\Omega_V=\sum_i\pi(X_i)\pi(X^i)$$

Note that, taking the representation V to be the space of functions $C^\infty(G)$ on the compact Lie group G, Ω_V is an invariant second-order differential operator, (minus) the Laplacian.

Ω is independent of the choice of basis, and belongs to $U(\mathfrak g)^{\mathfrak g}$, the subalgebra of $U(\mathfrak g)$ invariant under the adjoint action. It turns out that $U(\mathfrak g)^{\mathfrak g}=Z(\mathfrak g)$, the center of $U(\mathfrak g)$. By Schur's lemma, anything in the center $Z(\mathfrak g)$ must act on an irreducible representation by a scalar. One can compute the scalar for an irreducible representation (π,V) as follows:

Choose a basis $(H_i, X_{\alpha},X_{-\alpha})$ of $\mathfrak g_{\mathbf C}$ with H_i an orthonormal basis of the Cartan subalgebra $\mathfrak t_{\mathbf C}$, and $X_{\pm\alpha}$ elements of $\mathfrak n^{\pm}$ in the $\pm\alpha$ root-spaces of $\mathfrak g_{\mathbf C}$, orthonormal in the sense of satisfying

$$(X_{\alpha},X_{-\alpha})=1$$

Then one has the following expression for Ω:

$$\Omega=\sum_i H_i^2 + \sum_{+\ roots} (X_{\alpha} X_{-\alpha} +X_{-\alpha}X_{\alpha})$$

To compute the scalar eigenvalue of this on an irreducible representation (π,V_{λ}) of highest weight λ, one can just act on a highest weight vector $v\in V^{\lambda}=V^{\mathfrak n^+}$. On this vector the raising operators $\pi(X_{\alpha})$ act trivially, and using the commutation relation

$$=H_{\alpha}$$

(H_{α} is the element of $\mathfrak t_{\mathbf C}$ satisfying $(H,H_{\alpha})=\alpha(H)$) one finds

$$\Omega=\sum_i H_i^2 + \sum_{+\ roots}H_{\alpha}= \sum_i H_i^2 +2H_{\rho}$$

where ρ is half the sum of the positive roots, a quantity which keeps appearing in this story. Acting on $v\in V^{\lambda}$ one finds

$$\Omega_{V_{\lambda}}v=(\sum_i\lambda(H_i)^2+2\lambda (H_{\rho}))v$$

Using the inner-product $< \cdot,\cdot>$ induced on $\mathfrak t^*$ by the Killing form, this eigenvalue can be written as:

$$< \lambda,\lambda>+2< \lambda,\rho>=||\lambda+\rho||^2- ||\rho||^2$$

In the special case $\mathfrak g = \mathfrak {su}(2),\ \mathfrak g_{\mathbf C}=\mathfrak sl(2,\mathbf C)$, there is just one positive root, and one can take

$$H_1=h=\begin{pmatrix}1&0\\0&-1\end{pmatrix},\ X_{\alpha}=e=\begin{pmatrix}0&1\\0&0\end{pmatrix},\ X_{-\alpha}=f=\begin{pmatrix}0&0\\1&0\end{pmatrix}$$

Computing the Killing form, one finds

$$(h,h)=8,\ (e,f)=4$$

and

$$\Omega=\frac{1}{8}h^2 + \frac{1}{4}(ef +fe)=\frac{1}{8}h^2 + \frac{1}{4}(h +2fe)$$

On a highest weight vector Ω acts as

$$\Omega=\frac{1}{8}h^2 + \frac{1}{4}h=\frac{1}{8}h(h+2)=\frac{1}{2}(\frac{h}{2}(\frac{h}{2} +1))$$

This is 1/2 times the physicist's operator $\mathbf{J\cdot J}$, and in the irreducible representation V_n of spin $j=n/2$, it acts with eigenvalue $\frac{1}{2}j(j+1)$.

In the next posting in this series I'll discuss the Harish-Chandra homomorphism, and the question of how the Casimir acts not just on $V^{\mathfrak n^+}=H^0(\mathfrak$

n^+,V), but on all of the cohomology H^*(\mathfrak n^+,V). After that, taking note that the Casimir is in some sense a Laplacian, we'll follow Dirac and introduce Clifford algebras and spinors in order to take its square root.

Comments

1. **Tony Smith**
   December 4, 2008

   Peter, will you be discussing the physical significance of all the Casimir operators of all groups used in physics model-building,
   or will your discussion be restricted to the quadratic Casimir ?

   Tony Smith

2. **woit**
   December 4, 2008

   Tony,

   The next topic, the Harish-Chandra homomorphism, deals with the story of the higher-order Casimirs. But after that, what I'm writing about deals with Dirac operators. In some sense, from then on, the story is about the square root of the quadratic Casimir, not the higher order ones.

3. **Coin**
   December 5, 2008

   Woit: Small note, looking at the listing for your BRST category I find that BRST IV got left out, you may want to fix that.

4. **James**
   December 8, 2008

   I'm sorely missing my usual fix of "scandal-mongering or stirring up trouble of one kind or another", and after some months of cold-turkey and BRST treatment, I wonder if you could explain the motivation, and long-term prognosis, of all of this group-therapy?

   As an outsider I would ask: is this some technique for creating new potential QM theories, or a way of quantising classical models, or to try to put existing QM theories on a surer maths footing, or is it just cute maths in itself?

5. **Peter Woit**
   December 9, 2008

   Coin,

   Fixed.

   James,

   I'm back from a trip, and have tried to do a bit more stirring up of trouble. Unfortunately, it's getting harder to stir up trouble in this area. Nothing much new is happening, and I and pretty much everyone else is tired of the same old topics.

   Now that I'm back, and classes are over for the semester, I'll try to pick up the pace, and get to the point with the BRST postings, ending with some more explicit speculation about what all this might be useful for. Still in the realm of speculation, it might be useful for getting some sort of new insight into the non-perturbative quantization of gauge theories such as the standard model, as well as useful in purely mathematical problems about representation theory.

6. **D R Lunsford**
   December 15, 2008

   Peter, am I right that SO(3,3) has Casimirs of rank 2,3, and 4? Looking forward to the next installment.

   -drl

7. **woit**
   December 15, 2008

   drl,

   The Lie algebra of SO(3,3) is a real form of the Lie algebra of SO(6,C)=SL(4,C). Just finished writing up the next posting, where I at least mention that the higher Casimirs of sl(n,C) are of degree, 2, 3, etc. up to n, so, yes in your case they are of degree 2,3 and 4.

Educational Malpractice

December 9, 2008
Categories: Uncategorized

According to the New York Times, Scarsdale High School has decided to get rid of their Advanced Placement classes, including AP Physics, replacing them with a new curriculum that cost "$40,000 to bring in 25 professors from Harvard, Yale, New York University and other top colleges."

> "We have the luxury of being able to move beyond the A.P.," John Klemme, Scarsdale's principal, said in a recent interview. "If people called it a gold curriculum in the past, I refer to this version as the platinum curriculum."

What's the change in this new "platinum curriculum" as far as physics is concerned?

> Physics students now study string theory — a hot topic in some college courses that is absent from the Advanced Placement exam.

Comments

1. **rrtucci**
   December 9, 2008

   It's a smart move. By the time these kids graduate, investment banking will be in vogue again.

2. **Charles Siegel**
   December 9, 2008

   …I assume this is an "ideas in physics" type of course they're running, with no math? Because if they aren't even doing AP Physics, how the hell are they doing String Theory? Even the simplest non-popular accounts require quantum mechanics…

3. **A.J.**
   December 9, 2008

   The NY Times article doesn't really offer enough information to draw any firm conclusion. I'd guess that the regular physics course is spending a few weeks at the end sketching contemporary topics, and that this has been sexed up to "studying string theory" for the purposes of the article.

4. **Thomas R Love**
   December 9, 2008

   Like this site, the high school classroom is not "a place for people to promote their favorite ideas about fundamental physics."

5. **Peter Woit**
   December 9, 2008

   Charles and A.J.,

   I couldn't find a syllabus on the Scarsdale site for the physics class, so I don't see support for either of your conjectures. It seems that what this is all about is that many high school teachers are unhappy with being forced to teach to a rigid syllabus set by the AP exams. These new courses are meant to replace AP courses, with the students often taking the corresponding AP exams anyway. So, presumably this is not a "no-math" course. No idea how much time they devote to string theory, hard to believe it's a huge part of the course.

   One does wonder whether teaching string theory in a high-school AP-level class is the idea of the Scarsdale physics instructors, or the consultants hired by the school district.

6. **Rob**
   December 9, 2008

   Folks, I think teaching "string theory" – whatever that might mean in high school – is really besides the point. Scarsdale deserves praise for phasing out the ridiculous AP curriculum and actually teaching kids something meaningful beyond how to, you know, memorize facts and best use practice exams. Besides, pretty much every reasonable university has 'exposure' courses in something like string theory/complexity theory/what have you.

7. **Peter Woit**
   December 9, 2008

   Rob,

   If Scarsdale wants to abandon the set AP curriculum and do something better, that's fine. But, if their idea of something better is to feed trendy hype to their students instead of solid science, which is what they are doing if they are replacing conventional topics with "string theory for high school students", then that is educational malpractice.

8. **Rob**
   December 9, 2008

   Peter,

   If I had to guess, I'd say that they offered up "string theory for high school students" as a sound bite for the usual bad reasons. If they are indeed teaching a 'Scientific American-esque' course on string theory, rather than a fancy sounding low-level physics or mathematics course – yes, that's a serious issue. That said, I don't see a problem with something like an optional lecture series on the topic (not that kids are going to get anything out of it).

   They should teach a good course on quantum mechanics or stat mech. It (1) – would sound catchy, (2) – would be accessible enough for high-schoolers, and (3) – would be really useful for most future scientists and engineers.

9. **Rob**

December 9, 2008

Rather, the knowledge in those areas would be "…really useful for most future scientists and engineers." I have some doubts about a class for high school students.

10. **GR**
December 9, 2008

I can't imagine this working, at least not without some serious changes to the whole curriculum. As I recall, AP Physics was a constant struggle for the teacher to teach the calculus the students should have received in their math classes.
I don't really understand the scorn that is heaped on the AP curriculum, especially in physics. It seems that a lot of it comes from people who never actually took an AP class? In my view, it's a fairly solid curriculum which covers basic mechanics and E&M. And it was a challenge to fit all that in one school year. The exam isn't bad either, and unlike the dreaded GRE actually has some questions which aren't multiple choice.
I think a lot of it has to do with the quality of the instructor, and the lack of high school teachers with dedicated physics backgrounds. I was fortunate enough to go to an excellent (public) high school, which had one of the only two teachers in the STATE who were certified to teach actual physics, not just "science". I was also fortunate enough to take an International Baccalaureate (now that really ought to be the "beyond the AP" curriculum, but that's a whole other discussion) physics class after AP which went over waves, stat mech, and some basic quantum.
That's the sort of thing students need: an AP class to teach the basic fundamentals and only after that classes that teach more contemporary topics.

11. **D.**
December 9, 2008

I doubt they're getting more than a day or two of string theory.
Just for background, Scarsdale is one of the best public high schools in the country, and has long employed PhDs to teach their math & science classes.
Students there have structured their lives to be attractive to good schools, so if this change somehow harms test scores or admissions rates it will immediately vanish.

12. **Jack Lothian**
December 9, 2008

As an aside, at different times my wife has been a director of a neighboring school board and the chair of our local school board. Her experience is that many parents are obsessed with high math & science marks for their children but most do not actually value what their children learn in these classes. There is constant pressure from parents, teachers and students to dumb down these subjects so students can get better grades. In her experience, individual weak teachers are not the focus for most parent complaints. As long as the students get good grades everything is OK with most parents. Tough teachers or tough exams are the flash point for almost every crises my wife has faced concerning these subjects. I am sure there are some over-the-top teachers & exams but it is strange that my wife has never received a complaint about a teacher or exam that doesn't challenge the students enough. How relevant this point is to the discussion I do not know but I do know science and math is constantly under attack in our public schools.

13. **JC**
December 10, 2008

Jack Lothian,

This exact same thing has been happening in community colleges too. Students and their "helicopter parents" will revolt and try to get the tough instructors fired. Frequently the tough instructors will get several reprimands from the administrators, before eventually being fired for being "too tough".

Though back to the subject, string theory has not been mentioned at all in the physics courses taught at community colleges. (Not even by instructors who are believers in string theory).

14. **Jake the Snake**
December 10, 2008

They are teaching string theory to high school students? Oy! What is the woild comin' to?

The string theorists are really getting desperate. I always thought that contemporary theoretical physics was beyond the grasp of even many of the brightest high school students. To truly understand and appreciate Newtonian physics, one needs a background in calculus, and most American high school students do not learn calculus until senior year if at all. (The concepts of Newtonian mechanics are simple enough that calculus or any higher math is unneeded. Entry-level algebra would suffice, but to get a good working understanding requires analytical geometry and calculus and the like.) Not to mention such 20th century theories as quantum mechanics or relativity theory.

To truly understand higher physics would require not only advanced algebra, analytical geometry, probability, statistics, calculus, etc. but discrete mathematics, matrix operations, linear algebra, differential geometry, and the like. High school students should be able to understand the basics of quantum theory, especially as it applies to chemistry, but relativity requires very advanced geometries! Relativity would not make sense within Euclidean geometry. To say nothing of string theories with the many warped dimensions…

As I understand it, AP type physics courses generally cover advanced Newtonian physics with introductory lessons on relativity and quantum physics. They might explain what special relativity postulates or the meaning of E=mc^2 for instance. A high school level course might be set up as "Fundamentals of Modern Physics" (i.e. quantum theory, relativity), but without the math and physics background, such courses would be pop-sci introductions. Such an approach would be acceptable for more established theories such as relativity or quantum mechanics, but it would be a bad idea to apply said format to controversial theories like string theory and its variants. If you ask me, assuming it is true that high school students study string theory, it looks like these kids are being indoctrinated into the string cult.

Regarding what Thomas R. Love said, it seems apparent to me that he is a disgruntled string theorist displeased with your blog. After all, he accuses you of censorship, claiming, "[this site] is not a place for people to promote their favorite ideas about fundamental physics," implying that you stifle dissenting viewpoints. While I support his right to comment, I would disregard that statement. Dr. Woit, your blogging does a great service to the physics community. Keep up the good work!

15. **woit**
December 10, 2008

Jake,

Thomas Love is not a string theorist. I certainly do stifle certain kinds of dissenting viewpoints, ruthlessly censoring comments that are off-topic and go on about the author's favorite ideas. On the whole, I think people appreciate this policy…

16. **Mitch Miller**
December 10, 2008

I don't think we can put too much stock in an article that describes string theory as "a hot topic in college courses."

17. **Thomas R Love**
December 10, 2008

Jake the Snake: the phrase: "a place for people to promote their favorite ideas about fundamental physics." occurs just below "Leave a Reply".

I was just quoting Peter. His book is a must read for any aspiring physicist.

To call me "a disgruntled string theorist" is an insult. I do mathematical physics. String theory is not physics. It is not even sound mathematics.

18. **somebody**
December 10, 2008

I agree that it would be horrible to feed the kids Kaku-style nonsense. But if at all they are to hear *anything* about the frontiers of theory, it is reasonable that they hear about string theory. The most reliable judge here is the consensus of the scientific community (NOT consensus among ignorant haters, like we have on this thread). Even if we believe that Peter is a messiah who could look into the future, at this point in time, the reasonable thing to teach kids is the consensus of the community, not the views of a minority of dissenters. The discussion here would have more content, if it focussed on the hype aspect rather than on teaching string theory altogether.

19. **Peter Woit**
December 10, 2008

somebody,

I agree with you that the consensus of the scientific community should be a deciding factor here. This is not the same thing as the consensus of the string theory community. From talking to many, many scientists, my impression is that the consensus of the scientific community is that the picture of unifying particle physics with strings in extra dimensions is a very speculative idea that doesn't seem to be working out. This is not what "dissenters" think, this is what most physicists who are not string theorists think. Even among string theorists, I see an increasing number who have given up on string theory for unifying particle physics, instead seeing it as a tool for studying strongly coupled qfts, and as a toy model for understanding issues in quantum gravity.

There are several different problems with the idea of teaching string theory in high school, but one of them is precisely that the speculative picture of unification still vocally promoted by an increasingly small number of physicists does not have support from the rest of the scientific community.

20. **Jake the Snake**
December 10, 2008

To Peter Woit and Thomas. R Love, I apologize for the misunderstanding.

Mr. Love, I'm sorry that I misunderstood your above comment. I was not aware of your meaning as you did not provide the context. I did not know it was a Woit quote. I thought you were insinuating that Woit CENSORS differing viewpoints (i.e. string theory), as opposed to applying skepticism to FILTER out bad ideas, and you were implying that he similarly wanted to keep string theory out of the classroom for ideological reasons.

I took you for someone on the side of Lubos Motl, not our side. As it turns out you are not a string theorist (thank you for clarifying) and, the quote in context means that the classroom, like this blog, should be "no-nonsense zones." What I took to be anger or displeasure at string theory being excluded was in fact agreement that certain ideas are not appropriate. Correction appreciated!

Dr. Woit, I appreciate the reply. First off, this is your blog and your right to moderate comments. I do not believe what you do is "censorship" so much as "filtering." I agree that irrelevant and off-topic comments, or messages that are disrupting are fit for deletion. I am sorry I did not understand Thomas' comment.

21. **David Metzler**
December 10, 2008

As a math Ph.D. who teaches math and physics in high school, I am curious about the proposed Scarsdale curriculum, and in what way it is supposed to be superior to the AP curriculum. I agree with various previous comments that the string theory part is unlikely to be more than a tiny part of the course, and was hyped in the article by someone who thought that it was a major selling point.

Rob, I wonder exactly why you think the AP (Physics) curriculum is ridiculous. I'm not wedded to it, and I do dislike the rigidity, but I can't say that it emphasizes simply memorizing facts…could you elaborate?

For those who don't know, I'll note that there are two AP Physics courses: AP Physics B is a non-calculus based course, which covers basic mech and E&M as well as a smattering of waves, fluids, and modern physics. I haven't taught that one. AP Physics C covers less material—just Mech and E&M—but uses calculus (which is usually taken concurrently—tricky but manageable) and makes students work deeper and more involved problems.

22. **MathPhys**
December 11, 2008

They may be using the words "string theory" in a very general sense. They may start with something on the electromagnetic interactions and gravitation. Followed by something on the nuclear forces, then the idea of unification (which would allow them to include some basic mathematics, group theory, etc). It doesn't have to be too bad.

23. **somebody**
December 11, 2008

Peter says: "This is not what "dissenters" think, this is what most physicists who are not string theorists think."

Looks to me like you are trying to do a statistics of the high energy community, AFTER excluding the string theorists. Isn't that the same thing as putting in your answer by hand? Besides, I am not willing to take your anecdotal evidence (that you know "many, many scientists" who are against string theory) at face value. A much more objective measure is the preprints that turn up on hep-th everyday. The overwhelming majority of them are string-inspired.

Also I want to emphasize that the relevant consensus here is that of the people who are competent enough to understand the problems and merits of the various ideas in high energy theory. NOT the generic scientific community. (nobody asks particle physicists about plasma physics, if you notice). And within the high energy community, it would be ridiculous to claim that string theory is not a major theme, because your blog itself is afterall a dissent against this consensus.

So again: if at all the kids are to hear *anything* about the frontiers of fundamental physics, it is reasonable that they hear about string theory. It is one thing to emphasize that the theory is tentative, quite another to turn it into a war against tentative ideas. It is precisely the ideas that are not fully understood that trigger the imaginations of both a seasoned researcher and a beginning student. People who are trying to make the curriculum "safe" are not getting what drives human curiosity. Me, I see nothing wrong with a little mention of string theory in school, as long as it is not hyped up drivel.

24. **Peter Woit**
December 11, 2008

somebody,

Despite what string theorists like to think, there are many physicists and other scientists competent to evaluate whether the idea of string theory unification has had any success or not. Even if one is not an expert in the technicalities of a subject, one can listen to the case made by its proponents, and see what evidence they put forward. Here, it is inarguable that 25 years of intense effort has led to absolutely zero in terms of predicting anything or explaining anything about beyond the SM particle physics. Any competent scientist can read books like Susskind's "Cosmic Landscape", and see that the subject is at a dead end.

This is the problem with promoting teaching string theory as the "frontier" of the subject. It may be be interesting, but it's not a good use of high school student's small amount of time studying science to be explaining a complicated speculative picture that doesn't work. The problem isn't just with the fact that if people do this, they will hype the subject, the problem is with the whole idea.

25. **somebody**
December 11, 2008

I gave you an objective way to measure the opinions of the hep-th (NOT string) community: papers everyday on the arXiv hep-th. You say that people n other fields of physics who get the story of string theory from popular science books should also count. I simply disagree. This is not how it is ever done in any other field, so why should high energy theory be a popularity contest? Expertise counts!

Once again: My claim is that if we plan to teach high school students something about the cutting edge of fundamental physics, the scientific consensus that string theory is relevant should not be ignored. You keep repeating your opinions as though they were facts, but that still does not make them the opinions of the community.

26. **Somebody else**
December 11, 2008

Somebody,

The consensus of the scientific community is that 'string theory' is just speculation until there is some experimental evidence for even a small part of it.

Here by 'scientific community' I mean something broader than 'high energy particle theorists', but not much broader than 'professional physicists' (surprisingly to some the preceding two categories are not the same).

There are thousands of scientific ideas that ought to be presented to high school students before they are exposed to something like string theory.

27. **Peter Woit**
December 11, 2008

somebody,

If you talk to your non-string theorist physicist colleagues, I think you'll find that their sources of information about string theory range from popular books to colloquium talks by string theorists to having taken a course on the subject to having worked on it and stopped. I also think you'll find a great deal of resentment about the way string theorists are dealing with their own failures by telling their colleagues that they are just too ignorant to appreciate what they are doing.

The bottom line here is that, after 25 years of intense work, there is zero evidence for string theory unification. You don't have to be an expert to understand that this is not a promising situation, or that this is not something you want to present to high school students, unless you're trying to teach them about the sociology of science, not science itself.

28. **anon.**
December 11, 2008

Now I'm tempted to start a blog dedicated to complaining about how, after 20-odd years of work, there has been no definitive success in building a theory of high-Tc superconductors. What are these condensed matter theorists wasting their time on, anyway? They better not try to tell me I don't have the expertise to judge them.

29. **Coin**
December 11, 2008

anon: One obvious difference here is that there is experimental proof high-tc superconductors actually exist.

30. **Peter Woit**
December 11, 2008

anon,

I haven't noticed condensed matter theorists with unsuccessful high-Tc superconductivity models trying to get them taught in high school…

31. **Thomas R Love**
December 11, 2008

This from Zwiebach's "A First Course in String Theory":

"When we think about teaching string theory at the undergraduate level the main question is "Can the material really be explained at this level?" …the basics of string theory can be well understood with the limited tools acquired in the first two or three years of an undergraduate education.

…A First Course in String Theory should be accessible to to anyone who has been exposed to special relativity, basic quantum mechanics, electromagnetism and introductory statistical physics.

While the mathematics for string theory is discussed at

http://www.superstringtheory.com/math/index.html

This includes real analysis, complex analysis, group theory, differential geometry, Lie groups, characteristic classes, homotopy, , fiber bundles and more.

I seriously doubt there is any high school student with this background in physics and math.

32. **Yatima**
December 11, 2008

I remember, 20 years ago, reading high-level overflight articles about string theory in "Science & Vie". Meanwhile: learning the basics of calculus, complex and real analysis in advanced math courses. Fighting english and french grammar. Studying the causes of WW1 (yes, really). Reading mandatory novels for literature classes. Cramming (really badly taught) economics 101 for the 'bac. … And our physics teacher frantically trying to give us a 1-hour overview of things not seen in the classroom before students left for exams and the wider world beyond — the ones seen being mechanics, heat, LCR electronic circuits (no memristor yet), simple Newtonian mechanics and the Compton effect – with the mathematics just starting to be useful. Ah, youth.

String Theory? Under these circumstances? Shurely not.

33. **srp**
December 11, 2008

I believe you could teach some real high-energy physics in high school if you cut into the subject the right way. In college (about thirty years ago) a visiting professor named Irving Reichert taught our "Physics for Poets" class out of his manuscript for an innovative introductory textbook. The main idea was to teach the correct physics at the particle level first by focusing on the basic known conservation laws (momentum, strangeness, charge, etc.). Special relativity was assumed from the outset. Typical problems were about particle collisions and what could go in or out from different interactions, how to interpret bubble chamber data, etc. (This was all handy when we took a field trip to Brookhaven–we had some idea what all that gear was for.) Lots of high-school algebra was involved, but nothing more advanced.

One of his pedagogical points was that he could do quite a lot without having to use any calculus (even though I was taking multivariable calculus at the time). Another was that students who had been excited by media reports of cutting-edge discoveries in high-energy physics were turned off by intro courses' usual diet of frictionless blocks sliding down planes and so on. A third was that the Newtonian stuff was an approximation so why start with that? (I should point out that he was not a high-energy guy himself–I think he told us he worked on the magnetic behavior of electrons in helium and could do his experiments on a tabletop.)

I've never been able to find his textbook, so perhaps it wasn't published. He was a heck of a teacher, though, and I appreciated his innovative spirit. In any case, he showed that you could teach high-energy topics in a more-than-superficial manner without advanced math.

34. **J.F. Moore**
December 11, 2008

David Metzler alludes to this above (and as an AP teacher he clearly knows), but I thought I would clarify since some people seem to have the wrong concept of what AP Physics C (the calculus-based one) is: There is no modern physics taught at all, certainly no quantum, no relativity, not even optics or stat mech. There's no time. It could be the students' first physics class, and half is devoted to mechanics and half to E&M. It is supposed to be the equivalent of a rigorous freshman college series, not some overview of 'exciting topics'.

While there may be room at some high schools for more advanced physics, it seems silly to replace a foundation class.

35. **AngryPhysicist**
December 12, 2008

Hmmm…yes, well, call me old fashioned, but wouldn't it be better to ditch the AP in favor of actual college undergraduate level physics? You know, the good old Lagrangian and Hamiltonian formulation of mechanics, thermodynamics, classical field theory, electromagnetism…the good stuff!

There really is no need to be teaching string theory at all (I think some of the comments above more or less overlooked that point of view — ditching the AP is not bad, but replacing it with string theory should be seen even by string theorists as nonsensical; why not teach them quantum physics before classical?).

36. **David Metzler**
December 12, 2008

Thanks to J.F. Moore for the expansion/clarification of my comment. I'm a bit confused by AngryPhysicist's comment; AP physics is intended to be (and, if taught well, is) equivalent to standard college freshman physics, not the more advanced treatment he alludes to, usually taken in the junior year. That would be quite a tall order for a high school class.

One thing that's very important to remember in discussing freshman/AP physics is that most people taking such a class are not going to be physics majors, much less physicists. The majority will be engineers, or other types of scientist. Hence there are two strong motivations for spending a lot of time on blocks and pulleys (which a previous commenter seemed to suggest was boring). First, even for future physicists, you have to start with familiar examples to build intuition about Newtonian mechanics, before you go to Hamilton/Lagrange, much less QM. Second, I really want the guy building the bridges to understand that stuff!

37. **AngryPhysicist**
December 13, 2008

The AP subjects don't handle things such as Lagrangian or Hamiltonian mechanics, much less classical field theory! Consider discussing symmetries of a classical system using diffeomorphisms much less! So why would you skip this to go to string theory? Especially since everyone acknowledges these subjects are fundamental to more advanced physics.

That's why I remarked "Why not teach them quantum mechanics before classical mechanics?" It makes no sense to teach them string theory without having them know the basics of quantization of gauge systems and general relativity, which requires knowledge of quantum theory and classical field theory, which requires knowledge of the Lagrangian and Hamiltonian formulations of mechanics. To just skip all this is nonsense.

So is it good to abandon the AP? Well, I think the entire public education system in the US needs to be completely redone…so it's a moderate step forward. Is it sensible to replace AP physics with lectures on string theory? Not in the least.

(By the by, the "intention" is to have the AP be "equivalent" to college work just like how eating out of a dumpster is "equivalent" to being inoculated.)

38. **somebody**
December 13, 2008

"The AP subjects don't handle things such as Lagrangian or Hamiltonian mechanics, much less classical field theory! Consider discussing symmetries of a classical system using diffeomorphisms much less! So why would you skip this to go to string theory?"

I think the purpose of teaching something like this is not to leave the students powerful enough to start computing, say, scattering amplitudes. It is merely to let them know that physics is not a finished business. You don't have to be able to talk string theory, in order to talk *about* string theory. The value is more inspirational, in order to emphasize that there are plenty of questions (that are not merely details), which we still don't understand. As a kid, I distinctly remember being fascinated by the idea that general relativity was not consistent with quantum mechanics and that resolving this puzzle might be at the heart of resolving the puzzles behind big bang and black holes. I think this is the kind of context where one could drop a line about string theory in a high-school curriculum.

Back to Peter and the blind-leading-the-blind debate:

THE overwhelmingly major candidate for the cutting edge of fundamental theory is string theory, despite repeated claims by Peter otherwise. You cannot simultaenously make the claim that departements all over are being hijacked by string theory, while at the same time claim that string theory is not popular. It is simply ridiculous, no matter how many of your devoted followers here agree with you. Facts are simply not decided on the basis of democracy. About whose consensus is relevant: I am old-fashioned with these things and have to stick with my claim – that high energy physicists are better qualified to judge high energy physics, than people who have learnt about it from popular books and expositions. Shrug!

39. **Pawl**
December 13, 2008

Somebody, Anon.,

You are correct that facts are not determined by democracy — but that also means they are not determined by which research groups have the most members, nor by how many arxiv papers there are. They are determined by nature.

String theory, despite its mathematical successes, and the extraordinary cleverness its researchers have shown at working on its internal problems, has very little to show about physics beyond the speculative.

There has also ben a considerable lack, until recently, of frank self-criticism within string theory. There are plenty of statements from prominent proponents of string theory which reflect very poor judgement in this regard. So the credibility of judgments by the in group has been seriously cast in doubt by their own actions.

If you want to defend string theory at this point, it would be best to explain exactly what you are defending, and what previous claims you think were excessive. Remember, most of us have in mind overblown prior claims, statements by prominent string theorists to the effect that nothing but string theory can possibly be right (and so work on other approaches ought to die out), that wherever string theory leads (e.g., the landscape) must be right, etc. — which many of us feel are indefensible positions.

40. **Jonathan Vos Post**
December 13, 2008

Meanwhile India and Pakistan and China and Iran are teaching according to a "Plutonium Curriculum." As someone planning on a second doctoral program, namely a Doctorate in Educational Leadership, I find the American position indefensible, in both scholarly and Pentagon senses of the word. India had a multiverse theory, in Sanskrit, a millennium ago. Now, instead, they have more computer programmers than does Silicon Valley, and an Indian flag on the Moon (with the Chandrayyan-1). China had the theory that there were innumerable planets in the universe, then moved on — and had their first "space-walk" this Fall. We do not have the luxury to gaze into our stringy navels. There is a real world, with real competition.

41. **themanwithaplan**
December 13, 2008

"and the extraordinary cleverness its researchers have shown at working on its internal problems"

You take out Witten, Vafa, and a few others, and their cleverness probably goes below the average in sciences!

42. **somebody**
December 14, 2008

Pawl says: "You are correct that facts are not determined by democracy — but that also means they are not determined by which research groups have the most members, nor by how many arxiv papers there are. They are determined by nature."

I am with you till here.

Pawl continues: "String theory… has very little to show about physics beyond the speculative. etc. etc."

Now you see, this is no nature's truth, this is merely your opinion, presented as a fact. And therein lies the rub. When we want to make a policy decision, like what to teach students etc., the closest thing we have to nature's facts are the collective opinions of the relevant group of specialists, not yours or my oipinion. ArXiv etc. are good objective measures of this collective opinion. Replacing that with a more coarse (unspecialized) form of democracy, is what I am against.

About your other comments: Since you have made an effort to be reasonable, I will return the favour and respond to them as reasonably as I can. I think these are precisely the kind of questions asked by people who are not directly involved with the premises and challenges of high energy theory, so it might be useful to answer them. But these are off-topic (in as far as any discussion of the string controversy can be off-topic on this blog), so I will post them as a separate message.

43. **MathPhys**
December 14, 2008

While all of us are painfully aware of the severe shortcomings of string theory on the experimental verification front, there is simply no denying its incredible attractiveness to intelligent young people, who are too idealistic to study anything that would prepare them for a job and want nothing other than to learn the most fundamental physics. Every teenager/20 something believes that maybe he/she will crack the problem.

Over and over and over again, young people keep on approaching me saying "I want to learn string theory. Where should I start?"

Under the circumstances, all I can do is to make them aware of its problems, aware of the job prospects, then tell them about the 5 or 6 basic textbooks on string theory and hope that at least they will learn some conformal field theory and/or get a global view of modern mathematical physics.

We can use string theory as an excuse to introduce an incredibly large amount of very good mathematics and mathematical physics.

44. **somebody**
December 14, 2008

Dear Pawl, you say that string theory is speculative. But what exactly do you mean by "speculative"? In my opinion, string theory is probably THE most conservative extension of well-understood physics. It contains only the most well-accepted ingredients of physics (like say Feynman diagrams), with a single extra piece: that particles are extended strings. Everything else follows automatically (if one is infinitely smart).

I think what you might possibly mean by "speculative" is not so much that the theory is full of ad-hoc constructions, but that some of the *solutions* of the theory could give rise to a lot of exotic physics, like branes and extra dimensions and what not. (Incidentally, notice how this question is subtle and requires some training to even pose correctly.). But having exotic solutions is not an unknown situation in physics. General relativity has them too. This is hardly a reason to discard it without further thought.

The way I look at it, the experimental difficulties with string theory are basically a problem that ANY theory of the Planck scale will have to face. The real question, which gets usually drowned in the caricatured attacks, is whether we know enough to extrapolate to Planck scale from what we know at low energies. I personally think it is NOT ad hoc speculation to extrapolate, but again, this is precisely the question that I think requires some expertise to form a fair judgement call.

Now, about self-criticism in string theory. Here I think you are confusing two closely related, but fundamentally different phenomena. I think you are conflating the public hype of string theory in the media with the workings within the community. Again, the hep community is a LOT more stringent than what one might think from reading about string theory in the popular media. I cannot think of even one idea that got popular, which was accepted without sufficient scrutiny. Every SINGLE valid criticism of string theory (and its resolution) has come from *within* the community, not from outside, as many people would like you to believe. In particular, I think Smolin and Woit did a good job of counterbalancing the hype, but scientifically, there were absolutely no surprises from either of them. There are even main-stream figures who are vocal against some of the widely held notions within the community. Like Tom Banks. They are not booed out of court, but that is because they have more than superficial things to say.

About previous excess claims by string theorists. This is again a hype issue and a phenomenon you can only see in the public media. Within the community, the amount of attention an idea receives has always been roughly correlated with the intrinsic value of the idea, and has never been dependent on the hype. Most of what goes as tall claims in the media, did not get much traction within the community. In fact I cannot think of even one example for that happening. For instance landscape statistics gets an inordinate amount of attention in the media, but most people within the community are not really into it, because they would like to have a better way to pose the problem. Landscape is something we still don't understand well in string theory, but usually not for the reasons that it gets bashed for, in superficial debates.

PS: By the way, Peter, I have to say that I respect your integrity when posting these comments. My previous message actually got stuck in the wordpress filter, and I was worried that maybe you censored it, but happily, you liberated it. Despite the fact that I disagree very much with you on much of your views on string theory (while agreeing very strongly with you when you fight the string hype), it is refreshing to know that you are not going down the easy path of censoring stuff – which is a trap very easy to fall into. Of course, this does not mean that I am going to go easy on you in the future, ha! Cheers and respect!

45. **ex AP student**
December 15, 2008

I'm a mathematician who took AP "Physics C" decades ago.

For those not in the USA, "AP physics" means a calculus-based one year class in Newtonian mechanics (no Lagrangians or Hamiltonians) and basic electromagnetism. The standard textbook was, and as far as I know still is, Resnick & Halliday.

The real problem with both the physics AP and whatever Scarsdale etc might replace it with, is that (1) the mathematical prerequisites are acquired concurrently rather than in the preceding year, and (2) the nominal prerequisite in physics (a qualitative, non-calculus one year class in physics standard in US high schools that offer AP Physics) is a joke and completely superfluous for the AP material. What works much better is to eliminate the year of joke physics entirely and replace it with a full year of calculus (in AP terms, "Calculus BC").

46. **Pawl**
December 15, 2008

Dear Somebody,

I very much appreciate your continuing this dialog thoughtfully. You raise quite a few points, and I will do my best to respond relatively briefly.

It's curious that you take issue with the statement that string theory has little to show about physics beyond the speculative, to the point you doubt whether we mean the same thing by "speculative." As I noted before, string theory does have mathematical successes, and it has internal successes — remarkable cohesions, etc. But it hasn't had any predictive successes, which is how we usually judge physics.

So I would say that all of string theory's contributions to physics are speculative — inspired guesses, which may or may not turn out to be right. (And the track record in science is of course that far more speculations — even beautiful ones — turn out to be wrong than turn out to be right.)

Your comments lead me to think you don't really think of string theory as speculative in this sense. (And that would explain much of the rest of what you have written.) So I am going to speak to that. I don't want to trash string theory as an approach to quantum gravity, because I do think it is a reasonable one to investigate. What I am arguing for is viewing its prospects reasonably.

To begin with, it is very rare in physics that fundamental theory can make much progress without experimental input. We simply are not clever enough to anticipate what nature will do most of the time. Moreover, it is very rare to be able to successfully guess what will go on many orders of magnitude from what has been experimentally investigated. There are simply too many ways nature can surprise us in the intervening regimes. So the fairest evaluation of **any** theory of Planck-scale quantum gravity would be considerable skepticism.

As to string theory particularly, again, reasonable people can work on it, but there are also good reasons for thinking that it may well be entirely misdirected. As you say, it is rather conservative in some respects. Its basic physical idea — moving from point to string (or brane) interactions — seems hardly to be the sort of profound insight that one would expect to be the lynch-pin of a successful theory of quantum gravity. (Compare, for instance, the sorts of wrenching new physical insights which went into creating quantum theory and relativity.) Too, its premise is that one can quantize gravity without centrally dealing with interpretational issues in quantum theory. That might be true, but it could also very easily be wrong. And the reliance on supersymmetry and extra dimensions — well, that just might be wrong, too.

The point here is not to discard string theory (although, again, the only fair assessment of **any** theory of quantum gravity at this point is that it is likely to be wrong). The point is that other approaches should be investigated. This has sometimes been acknowledged by string-theorists, but the demographics of the subject (who is hired, what the standards of review are) tell a different story. Very nearly all of the quantum-gravity eggs have been put in one basket, and that is simply the wrong strategy for the community to adopt. And they've been put in that basket because of aggressive actions from the top of the string-theory community. The effect may well be to have made the world safe for string theory, but to have retarded the prospects for getting the right quantum gravity theory.

The general mode of proceeding in string theory has been copied from particle physics, for obvious historical reasons. That is, write down Lagrangians, do path integrals, don't worry too much about foundational questions. That worked in particle physics, partly because it all turned out to be quantum field theory, but more because there was a continual confrontation with experiment which weeded out a lot of erroneous theories. (But even there, the weeding was slow, and at many times progress was very hard.) But it's not at all clear that that is the right way to proceed in trying to quantize gravity.

I've spent some time trying to give a down-to-earth view of string theory's prospects, because of your hesitation in accepting that it was speculative. I want to quickly address another point you make.

I do take your point that some of the hype from the string-theory community is for external consumption (or is simply self-serving), and I know string theorists who smile or cringe when they hear it. On the other hand, there are cases of internal hype (for example, the claims of "obvious finiteness," an argument which was made vociferously as one way of justifying the large amount of resources going to string theory).

But my main concern is not so much the minutiae of the arguments as the big picture. String theorists seem not to have taken seriously (by which I mean, as far as allocating funding, hiring, etc.) that it would be more important to try to encourage a diversity of work than to concentrate work on one theory.

47. **David Nataf**
December 17, 2008

When I was taking high school physics we studied topics like the inclined plane, the pulley, charged rods and the Bohr atom.

48. **ishi**
December 22, 2008

this has probably been said, but the web tutorial mentioned above (superstringtheory) and general articles by Witten in Physics Today seem to me as a nonphysicist (who took physics to do math biology, which uses alot of the formalisms, from cond-mat to wick rotation) seem actually to me to be pretty good introductions which could be used as a 'special topic' day (or a few) to generate interest and motivation to learn the required math ,which is pretty universal for any field, at least at the intro level. I was amazed to see string theory was based on basic ideas like the classical wave equation, and duality in e and m. You can even get your lagrangian and action principle, etc.

I think maybe wikipedia may be just as good for many things, maybe even better than MIT open courses.

49. **somebody**
December 25, 2008

Dear Pawl, I just saw your message but thought I will reply anyway since I have some time. Your arguments are really against high energy theory altogether (as you probably realize), and not specifically against string theory. You haven't really got the distinction I was trying to make between speculative constructions which are often quite arbitrary, and a solid, robust theory which builds from the fundamental ideas of physics and merely cranks the formalism (in some sense) and churns out extraordinary insights (like gauge-gravity duality).

This distinction is what makes string theory so extremely appealing to so many. And a lot of the critics merely brush it off as "speculation" (while often making some concessional words about the mathematics of string theory), and equate it in value to a lot of other cock-and-bull stories that theorists come up with. This incomprehending nature of the critics is the reason why it is a little hard to take them seriously.

The sociological ways in which theory and experiment interact need not always be the same. At least a few times in history, our problem was not so much that we took our theories too seriously, but that we didn't take them seriously enough. Quantum field theory was not trusted for a long time because of the infinities (from 40s to 70's almost) unttil the standard model was unravelled. Big bang was considered so speculative that nobody even cared enough to look for its signature.

My point is that it is merely your personal claim when you say that experiments and theory have to interact in a certain sequence. Of course, experiments are necessary in physics. But the challenges facing science will change qualitatively as generations come and go. It is quite likely that in the future, experiments are going to be hard and theorists will have to work very hard (and perhaps long) in order to relate theory and experiment. The light is going to be only at the end of the tunnel. This is just the way things are going to be. Does that mean that high energy theory has to be shut down? If there came a stage where it was provably impossible to connect theory and experiment, I would consider that possibility. But otherwise, I would vote for pushing forward, because I think theory is our last line of attack against the frontiers of our ignorance about the universe. Shutting it down will be the end of curriosity. If you think objectively about why exactly society sponsors things like mathematics, particle physics etc., I think you will find that it is not such a horrible thing to do, to support research in theoretical high energy physics of this kind.

It is in such a context that one has to really weigh the balance of judgement against a theoretical idea. How robust is string theory is the real question. Not cheap caricatures, like "no experiment, so string theory is useless". These are just another form of veiled hate against theoretical physics itself, the not so uncommon hate against the top of the food chain, if you know what I mean. It was there back when theory was still dealing with experimentally accessible things, but then the criticism took a different form. Then it was "no practical use", now it is "no experiment".

In the above two paragraphs, I was taking on your experiment-above-all-else argument: the claim that string theory is useless because there are no experiments. (This is actually not even fully true considering the recent RHIC etc. experiments.) The real problem with these shallow criticisms is that they *totally* fail to see that a good theory teaches us deep insights and understanding about the things that we already knew, but thought were distinct. String theory has taught us that gravity and gauge theory can be understood in terms of each other (notice that between the two they exhaust all of dynamics.) Then thereare things like black holes, etc. where string theory has truly found ways to get past previous challenges. Notice that none of these things have anything to do with the fancy math aspect. If you believe that this sort of understanding is without value, then you are correct that string theory is not for you.

50. **Pawl**
December 26, 2008

Dear Somebody,

Thanks for responding. However, I don't really think you've understood my points. I won't reiterate them, but I will try to sketch what seems to be preventing commuication.

Rather than take up what I have said, you are entirely misrepresenting me (saying I am against high-energy physics altogether) and shifting the discussion to a straw man (people who "brush off string theory as 'speculation'"). You spend your post discussing what "the critics" of string theory say without taking up with what I say.

It is also disturbing that when I raise some criticisms of string theory, you amplify this into a question of whether theoretical physics is worthwhile or not.

Please reread what I have written.

51. **somebody**
December 27, 2008

Sorry Pawl, I cannot concede that easily that I am misinterpreting you. Here is an exact quote from from your previous message:

"To begin with, it is very rare in physics that fundamental theory can make much progress without experimental input. We simply are not clever enough to anticipate what nature will do most of the time. Moreover, it is very rare to be able to successfully guess what will go on many orders of magnitude from what has been experimentally investigated."

So yes, you were against high energy theory, not merely string theory, as I claimed. Your whole message was predicated on the deep pessimism that we are probably not smart enough, so it is unlikely that we will understand the (remaining) mysteries of the universe. This argument has been valid and compelling for most of history, but curious people who are not intimidated by the unknown usually find a way around. Is this going to happen this time as well? I don't know. But I am not going to bet on the mediocrity of humans like you do. Experiments are hard, yes, but there might be possible experiments which we are missing because our theory is not well-developed. People could have probably done the Aharonov-Bohm experiment in the early 1900's. But they didn't. Why? Because there was no way anyone could have thought about it before quantum mechanics.

The bigger point, as I said before, is that experiment and theory can interact in many ways.

The reason why I did not dissect your claims point by point is because it didn't seem like a useful thing to do. So instead I took a few of your more interesting points and responded to them. The overwheleming impression I got from your message is that you have opinions about things you are not very familiar with (sorry, I am honestly not trying to put you down, but I fear this is relevant to our discussion!). Not just when you talk about the "hype" regarding finiteness of string perturbation theory, but also when you say how research should be diversified. In principle the latter sounds like a good idea, but I really do not know of a much better way to run the scientific process in HEP, than the way it is currently. Not that the current one is perfect, but I suspect any artificial redistribution of the limited resources is only bound to make it worse.

52. **Pawl**
December 27, 2008

Dear Somebody,

Your response is becoming unproductive and rude: to say you will not answer my points because of your "impression" I don't know what I'm talking about.

I am completely baffled as to how you interpret the paragraph you quote as me being against high-energy theory. (However you manage to do this, it reflects what is in your own mind and not what I have written.)

There is no point in continuing unless you are willing to answer my points, and to try to understand what I have written.

53. **Peter Woit**
December 27, 2008

Somebody,

You are just ignoring real issues and making up straw men to attack. Pointing out the difficulties associated with trying to do theoretical physics without experimental input is not exactly saying something controversial, nor is it "against particle theory". Despite the impression one might get sometimes from hep-th, much of particle theory is very much involved with the question of what might or might not be seen at experiments that are being done now, or will be in progress soon (e.g. the LHC).

Not all particle theory needs to be so directly related to experiment, but anyone who works on scientific questions divorced from the discipline of contact with experiment (I include myself here) needs to be very aware that they are out on a limb and think carefully about what this means. Group-think, hype-mongering, and an unwillingness to admit failure are a very real problem here.

As to "artificial redistribution of the limited resources", my point of view is that that's exactly what string theorists have been trying to achieve, by attacking anyone who points out the failure of the string theory unification program in hopes of avoiding the conventional implications of that failure for the the distribution of resources. This tactic is no longer working so well.

54. **somebody**
December 27, 2008

Peter: "Pointing out the difficulties associated with trying to do theoretical physics without experimental input is not exactly saying something controversial, nor is it "against particle theory". "

That was not the claim, and this is a strawman. Nobody disputed the difficulties. The question is when do you decide that a research program is fruitless.

Your second paragraph I agree with, but not the third.

Peter: "As to "artificial redistribution of the limited resources", my point of view is that that's exactly what string theorists have been trying to achieve, by attacking anyone who points out the failure of the string theory…"

You have a book written, you are invested in this thing, your name is built on the anti-string campaign. So I am not sure that the anonymous string theorist who is trying to while away a weekend in sickbed is the one with an agenda to protect. Scientists usually are passionate about what they think they know. I didn't realize that I was defending my funding when doing the blogospheric dogfights against random people who think ignorance is a point of view.

Again, you are translating what is really a scientific debate into a sociological thing.

55. **TCO**
January 3, 2009

There is nothing stopping teachers from doing some extra material along with AP, other than the capability and background of the students (which is unlikely to be quite college level in all respects). however my teacher in AP chem did particle in a box, point group symmetry and solid sphere packings. All of which I think are extra. She still had time to spend half the class talking about school politics and how to behave at football games. We loved her! And lots got 5s.

Ap Physics is a whole nother kettle of fish. My school did not have it. We had PSSC phsycis (science emphasis, but pre-calc) and had HPP physics (liberal arts slanted). I had taken PSSC and Calc BC and so took the AP exam anyhow. I placed out of one semester of mechanics (I had to horse up with rotational mechanics, but the eqyations are very analagous to straight line stuff…and calc based problems had already been touched on within the calculus class.) I tried to cram the E&M, but it was too tough. got a 4 on mechanics and a 2 on E&M.

In service academy, where I went, the entire student body had to take 'zoics and it was nominally in the sophomore year so that differential and integral calc were done before the start. And during the second semester (E&M), multivariable calc had just been covered and diff EQs were being covered. This seemed to work.

Physics majors and EEs took a harder 3 semester curriculum in place of that Halliday and Resnick style (think it was actually Giamoato or somehting like that) curriculum.

AP or any advanced class should be more for that mass of engineering students than for the 'zoics majors. They will be otrutued enough with classical E&M as they go through.

Status of Superstring and M-theory

December 11, 2008
Categories: Multiverse Mania, Uncategorized

A write-up by John Schwarz of his Erice lectures from this past summer has now appeared on the arXiv, with the title [Status of Superstring and M-theory](). In his second lecture, Schwarz provides a good review of the various attempts to do "string phenomenology" by trying to find a "string background" that doesn't conflict with known particle physics. He devotes particular attention to the newest of these backgrounds, so-called "F-theory local models", providing a summary of the rather complicated constructions involved. Schwarz doesn't describe any experimental predictions of such models, just noting:

> It will be very interesting to see what predictions can be made before the experimental results pour in and whether they turn out to be correct.

For more discussion of these models and the question of whether they predict anything, see here.

Schwarz begins with an account of his interactions with Sidney Coleman at Aspen and elsewhere:

> I recall him once saying that there are three things that he does not like, all of which are becoming popular: supersymmetry, strings, and extra dimensions. Obviously, my views are quite different, but this did not lessen my regard for him, nor did it harm our personal relationship. In fact, I respected his honesty, especially as he did not try to impose his prejudices on the community.

About the anthropic landscape issue, he has this to say:

> Perhaps the absurdly large number of flux vacua that typically arise in flux compactifications has discouraged people from trying to construct viable particle physics models. In fact, this large number of vacua has motivated the suggestion that various parameters of Nature (such as the cosmological constant) should be studied statistically on the landscape. I don't really understand the logic of doing this, since this approach seems to assume implicitly that Nature corresponds to a more or less random vacuum. This in turn is motivated by some vague idea about how Universes are spawned in the Multiverse in a process of eternal inflation. Then the story gets even more entangled when the anthropic principle is brought into the discussion. Some people are enthusiastic about this approach, but I find it fundamentally defeatist. It is not the way I like to think about particle physics.

Meanwhile, public promotion of the Multiverse continues, with the opinion pages of Britain's The Independent today featuring a piece by Bernard Carr entitled Fifth dimensions, space bubbles and other facets of the multiverse. Carr describes the "growing popularity" of the multiverse proposal, ending with:

> But is the "multiverse" a proper scientific proposal or just philosophy? Despite the growing popularity of the proposal, the idea is speculative and currently untestable – and it may always remain so. Astronomers may never be able to observe the other universes with their telescopes and particle physicists may never be able to detect the extra dimensions with their accelerators. So, although some physicists favour the multiverse because it may do away with the need for a creator, others regard the idea as equally metaphysical. What is really at stake is the nature of science itself.

Carr characterizes some multiverse proponents as atheists favoring something that doesn't seem to fit into the conventional scientific method because it gives an answer to the argument from design for a deity. For more about this all-too-common argument for the multiverse, being promoted by Susskind and others, see here. In answer to such claims about religion being promoted by physicists, New Scientist this week is running a sensible piece by Amanda Gefter entitled Why it's not as simple as God vs the multiverse. It makes the obvious point about the multiverse-God dichotomy:

> Science never boils down to a choice between two alternative explanations. It is always plausible that both are wrong and a third or fourth or fifth will turn out to be correct.

Update: For more multiverse mania, see today's colloquium at Perimeter here. The intense promotion of this pseudo-science continues, but I don't think it's getting any traction.

Update: Yet more media attention to the God vs. Multiverse debate, now from the Guardian.

Comments

1. **Shantanu**
   December 11, 2008

   Peter, I watched this nice talk on testing inflationary models
   http://pirsa.org/08110021/ and near 46th minute where the
   speakers talked about testing consistency of string theory, there was a lot of
   discussions. Do you think these tests are really testing string thoery?

2. **Peter Woit**
   December 12, 2008

   Shantanu,

   Because of the low audio quality, I found it hard to hear exactly what was going on in that discussion. But, as far as I can tell, what's going on is that the speaker and others are talking about their "dream" to "constrain string theory". So, in their most optimistic scenario, all they will ever be able to do is to, amongst the infinite complexity of the landscape, say that some regions of it are consistent with various string inflation scenarios, and some aren't. This can never actually produce any evidence for or against string theory itself, it just takes physics where some string theorists want it to go. String theory is accepted as the "best guess" as to how the universe works, taught to high schools students, and fundamental physics becomes reduced to just parametrizing observations with string vacua, vacua that are capable of parametrizing anything that is ever likely to be observed. Doesn't really look like science to me….

3. **Samuel Prime**
   December 13, 2008

   You wonder if the enormous advancements in physics (and science) are prompting researchers to delve into, speculate, and think about superquestions and more adventerous ideas that, by nature, go beyond the currently accepted methods of science. It's hard to imagine science not evolving in its methods. (Causality was an integral part of classical physics 100 years ago but now no longer—or rather is still part of relativity but not quantum theory.)

   Perhaps new and/or additional methods will need to be added in order for us to advance (or define) our 'knowledge' of such huge unknowns (and which are so intriging to us humans). (Perhaps string theory is a symptom of this trend to 'push' the methods of science.) Of course, as Socrates would say, I know nothing, but I do think it is safe to say that science in 200, or even 500, years from now will not be the same as it is today. Our perspective will change. Perhaps even our methods and manner of perception.

We are hitting the boundaries of experiment. (More specifically, experiment that can be humanly done.) These many superquestions and supercuriosities show that we are more curious by questions that now seem untestable by our current state of abilities, at least to a high degree of certainty. Nevertheless, the big curiosities could play a role how the methods of science develop.

I have wondered if the development science will become similar to that of mathematics. (Research in high energy physics is already beginning to look like that!) For example, even though the Axiom of Choice and the Continuum Hypothesis are unprovable from the other Zermelo-Frankael axioms of set theory, mathematicians generally feel free to assume them in their work (even though they are 'untestable'). So if in science we hit boundaries of experiment and the unknowable, we may have to find ways to penetrate such boundaries.

4. **Chris W.**
December 13, 2008

Samuel Prime: *(Causality was an integral part of classical physics 100 years ago but now no longer—or rather is still part of relativity but not quantum theory.)*

It sounds like you're confusing causality with strict determinism. Causal structure is very much a part of quantum theory. (By the way, see Scott Aaronson's latest post.)

More to the point: When delving into superquestions and more adventurous ideas leads a scientist—or someone who claims to be a scientist—to start regarding testability against empirical observation as fundamentally irrelevant, it becomes utterly self-defeating. Some theorizing about the multiverse flirts with that attitude. Metaphysical speculation is (in my opinion) an inescapable part of formulating scientific questions that seek deep understanding, but the ultimate goal is a challenging confrontation with observation, hopefully with a successful outcome for the theory in question.

5. **Esornep**
December 14, 2008

It's rather amusing that Schwarz can dismiss the nucleation of bubble universes ["This in turn is motivated by some vague idea about how Universes are spawned in the Multiverse in a process of eternal inflation"] just after his reverential remarks about Coleman, in view of the fact that Coleman, with De Luccia, was the author of the massively influential paper on which all current multiverse theorizing is based. Yes, Coleman, who was opposed to higher dimensions, etc etc etc, started the whole multiverse ball rolling. Coleman was quite explicit about the possibility that our own Universe is a bubble, and of course there is no reason for such a bubble to be unique.

6. **Samuel Prime**
December 17, 2008

Chris W., could you please tell me where I mentioned determinism so that it would be confused (by yourself) with causality? Any educated physicists who read my comment would know what I meant by causality in the contrasting contexts of relativity and QM that I stated.

As for the unhappy comments toward the multiverse idea, I would hardly consider Steven Weinberg as among those 'self-defeating' physicists. In his essay Living in the Multiverse, Weinberg notes:

> There is also a less creditable reason for hostility to the idea of a multiverse, based on the fact that we will never be able to observe any subuniverses except our own. Livio and Rees and Tegmark have given thorough discussions of various other ingredients of accepted theories that we will never be able to observe, without our being led to reject these theories. The test of a physical theory is not that everything in it should be observable and every prediction it makes should be testable, but rather that enough is observable and enough predictions are testable to give us confidence that the theory is right.

It seems that two standards are being applied. One standard to theories we don't like, where we raise the standards to too rigorous a level (and requiring each idea to be testable), and another standard to theories we like and where we do not pick on them for not being testable in every respect. One could have picked on the Big Bang theory for the idea of an infinitely small point or particle when it exploded, even though that idea by itself alone is untestable.

As for myself, I can't say I believe in the multiverse idea. However, I am open to the concept. It can't just be an idea that stands by itself, but along with a broader mathematical framework, coupled with other hypotheses, so that the whole theory (not just one single idea) can either be tested or related to other known or accepted facts or scenarios that may be worthy of examination and further study.

I don't know of any grand and successful physical theory in which all of its ideas and concepts are each testable. So let's not push the testability thing too far or it will haunt our efforts.

7. **anon.**
December 17, 2008

'The test of a physical theory is not that everything in it should be observable and every prediction it makes should be testable, but rather that enough is observable and enough predictions are testable to give us confidence that the theory is right.' – Weinberg, quoted above.

That's wishy-washy brane-washing, because there is no unequivocal definition of 'enough' and 'us'. It's easy for string theorists to find even today 'enough' ad hoc prediction of gravity, etc., in string theory to give *them* confidence (to the point of arrogance) that their pet theory or religion is the right one. The fact that they are confident that they are right isn't worth much, however. Try applying Weinberg's statement to flat-earth theory, Piltdown man, cold fusion, Phlogiston, Caloric, etc.

8. **Jake the Snake**
December 17, 2008

The God v. Multiverse issue is an amusing one. Intelligent Design types love the dichotomy because it "forces" atheists to accept their God or postulate a weird hypothesis about multiple universes as an alternative explanation for the apparent "design" or "fine-tuning." Likewise, some atheists might not like it because they do not understand the real nature of the God-Multiverse dichotomy and think they may be compelled to postulate a multiverse, while those who do believe in a multiverse think they have an answer to the arguments of intelligent design. The problem is that any hypothesis which appeals to either a supernatural entity or multiple cosmoses fails the test of Occam's razor. Fortunately, the dichotomy between God and Multiverse is simply not true.

There is no compelling to declare either of the two scenarios true, or, on the contrary one can combine the two. Bradley Monton considered the ramifications of doing this to resolve the so-called problem of evil.

http://bradleymonton.wordpress.com/2008/12/03/the-many-universe-solution-to-the-problem-of-evil/

Why would Monton, who claims to be an atheist, attempt the above argument? It seems to be a thought experiment. As Monton reveals, he did not come up with the proposed solution, but he analyzes it briefly and considers the multiverse proposal to be an unsatisfactory answer to the problem of evil.

9. **Samuel Prime**
   December 20, 2008

   > ” … there is no unequivocal definition of ‘enough’ and ‘us’. “

   Yes, there is a practical one: the general consensus of the physics community. I’ll grant you, though, that string theory has much more work to do before it proves its mettle (if it is the right approach). (I think they know that themselves, even if some are arrogant, just as arrogance can be found on any side of an issue.)

   I had an arrogant and mean math teacher in school, but for some reason I separated his arrogance from my fascination for math. After him there was a much kinder teacher, and that made it all the more sweeter.

10. **TCO**
    January 3, 2009

    Skepticism about string theory has penetrated the popular culture. Its days of supremacy in funding and tenure positions are numbered. You should not feel defeated or defensive. You’ve practically won already.

    Lubos is smart and young so he can find something useful to do, still. Though if he doesn’t have tenure yet, I would be rather worried. Harvard is bad enough but with his field falling out of favor…anyhow, it just means he’ll latch on at a less prestigious school after Harvard dings him in a couple years.

Notes on BRST VII: The Harish-Chandra Homomorphism

December 15, 2008
Categories: BRST

The Casimir element discussed in the last posting of this series is a distinguished quadratic element of the center $Z(\mathfrak g)=U(\mathfrak g)^\mathfrak g$ (note, here $\mathfrak g$ is a complex semi-simple Lie algebra), but there are others, all of which will act as scalars on irreducible representations. The information about an irreducible representation V contained in these scalars can be packaged as the so-called *infinitesimal character* of V, a homomorphism

$$\chi_V: Z(\mathfrak g)\rightarrow \mathbf C$$

defined by $zv=\chi_V(z)v$ for any $z\in Z(\mathfrak g)$, $v\in V$. Just as was done for the Casimir, this can be computed by studying the action of $Z(\mathfrak g)$ on a highest-weight vector.

Note: this is not the same thing as the usual (or global) character of a representation, which is a conjugation-invariant function on the group G with Lie algebra $\mathfrak g$, given by taking the trace of a matrix representation. For infinite dimensional representations V, the character is not a function on G, but a distribution Θ_V. The link between the global and infinitesimal characters is given by

$$\Theta_V(zf)=\chi_V(z)\Theta_V(f)$$

i.e. Θ_V is a conjugation-invariant eigendistribution on G, with eigenvalues for the action of $Z(\mathfrak g)$ given by the infinitesimal character. Knowing the infinitesimal character gives differential equations for the global character.

The Harish-Chandra Homomorphism

The Poincare-Birkhoff-Witt theorem implies that for a simple complex Lie algebra $\mathfrak g$ one can use the decomposition (here the Cartan subalgebra is $\mathfrak h=\mathfrak t_{\mathbf C}$)

$$\mathfrak g=\mathfrak h \oplus \mathfrak n^+ \oplus \mathfrak n^-$$

to decompose $U(\mathfrak g)$ as

$$U(\mathfrak g) =U(\mathfrak h) \oplus (U(\mathfrak g)\mathfrak n^+ + \mathfrak n^-U(\mathfrak g))$$

and show that If $z\in Z(\mathfrak g)$, then the projection of z onto the second factor is in $U(\mathfrak g)\mathfrak n^+\cap\mathfrak n^-U(\mathfrak g)$. This will give zero acting on a highest-weight vector. Defining $\gamma^\prime: Z(\mathfrak g)\rightarrow Z(\mathfrak h)$ to be the projection onto the first factor, the infinitesimal character can be computed by seeing how $\gamma^\prime(z)$ acts on a highest-weight vector.

Remarkably, it turns out that one gets something much simpler if one composes $\gamma^\prime$ with a translation operator

$$t_\rho: U(\mathfrak h)\rightarrow U(\mathfrak h)$$

corresponding to the mysterious $\rho\in \mathfrak h^*$, half the sum of the positive roots. To define this, note that since $\mathfrak h$ is commutative, $U(\mathfrak h)=S(\mathfrak h)=\mathbf C$, the symmetric algebra on $\mathfrak h$, which is isomorphic to the polynomial algebra on $\mathfrak h^*$. Then one can define

$$t_\rho (\phi(\lambda))=\phi(\lambda -\rho)$$

where $\phi\in \mathbf C$ is a polynomial on $\mathfrak h^*$, and $\lambda\in\mathfrak h^*$.

The composition map

$$\gamma=t_\rho\circ\gamma^\prime: Z(\mathfrak g)\rightarrow U(\mathfrak h)=\mathbf C$$

is a homomorphism, known as the Harish-Chandra homomorphism. One can show that the image is invariant under the action of the Weyl group, and the map is actually an isomorphism

$$\gamma: Z(\mathfrak g)\rightarrow \mathbf C^W$$

It turns out that the ring $\mathbf C^W$ is generated by $dim\ \mathfrak h$ independent homogeneous polynomials. For $\mathfrak g=\mathfrak{sl}(n,\mathbf C)$ these are of degree $2, 3,\cdots,n$ (where the first is the Casimir).

To see how things work in the case of $\mathfrak g=\mathfrak{sl}(2,\mathbf C)$, where there is one generator, the Casimir Ω, recall that

$$\Omega=\frac{1}{8}h^2 + \frac{1}{4}(ef +fe)=\frac{1}{8}h^2 + \frac{1}{4}(h +2fe)$$

so one has
$$\gamma^\prime(\Omega)= \frac{1}{4}(h +\frac{1}{2}h^2)$$

Here $t_\rho(h)=h-1$, so

$$\gamma(\Omega)=\frac{1}{4}((h-1)+\frac{1}{2}(h-1)^2)=\frac{1}{8}(h^2-1)$$

which is invariant under the Weyl group action $h\rightarrow -h$.

Once one has the Harish-Chandra homomorphism γ, for each $\lambda\in\mathfrak h^*$ one has a homomorphism

$$\chi_{\lambda}: z\in Z(\mathfrak g)\rightarrow \chi_\lambda(z)=\gamma(z)(\lambda)\in \mathbf C$$

and the infinitesimal character of an irreducible representation of highest weight λ is $\chi_{\lambda + \rho}$.

The Casselman-Osborne Lemma

We have computed the infinitesimal character of a representation of highest weight λ by looking at how $Z(\mathfrak g)$ acts on $V^{\mathfrak n^+}=H^0(\mathfrak$

$n^+,V)$. On $V^{\mathfrak n^+}$, $z\in Z(\mathfrak g)$ acts by

$$z\cdot v = \chi_V(z)v$$

This space has weight λ, so $U(\mathfrak h)=\mathbf C$ acts by evaluation at λ

$$\phi\cdot v=\phi(\lambda)v$$

These two actions are related by the map $\gamma^\prime: Z(\mathfrak g)\rightarrow U(\mathfrak h)$ and we have

$$\chi_V(z)=(\gamma^\prime(z))(\lambda)=(\gamma(z))(\lambda + \rho)$$

It turns out that one can consider the same question, but for the higher cohomology groups $H^k(\mathfrak n^+,V)$. Here one again has an action of $Z(\mathfrak g)$ and an action of $U(\mathfrak h)$. $Z(\mathfrak g)$ acts on k-cochains $C^k(\mathfrak n^+,V)= Hom_{\mathbf C}(\Lambda^k\mathfrak n^+,V)$ just by acting on V, and this action commutes with d so is an action on cohomology. $U(\mathfrak h)$ acts simultaneously on $\mathfrak n^+$ and on V, again in a way that descends to cohomology. The content of the Casselman-Osborne lemma is that these two actions are again related in the same way by the Harish-Chandra homomorphism. If μ is a weight for the $\mathfrak h$ action on $H^k(\mathfrak n^+,V)$, then

$$\chi_V(z)=(\gamma^\prime(z))(\mu)=(\gamma(z))(\mu + \rho)$$

Since $\chi_V(z)=(\gamma(z))(\lambda + \rho)$, one can use this equality to show that the weights occurring in $H^k(\mathfrak n^+,V)$ must satisfy

$$(\mu +\rho)=w(\lambda + \rho)$$

and thus

$$\mu=w(\lambda + \rho)-\rho$$

for some element $w\in W$. Non zero elements of $H^k(\mathfrak n^+,V)$ can be constructed with these weights, and the Casselman-Osborne lemma used to show that these are the only possible weights. This gives the computation of $H^k(\mathfrak n^+,V)$ as an $\mathfrak h$ – module referred to earlier in these notes, which is known as Kostant's theorem (the algebraic proof was due to Kostant, an earlier one using geometry and sheaf cohomology was due to Bott).

For more details about this and a proof of the Casselman-Osborne lemma, see Knapp's *Lie Groups, Lie Algebras and Cohomology*, where things are worked out for the case of $\mathfrak g=\mathfrak{gl}(n,\mathbf C)$ in chapter VI.

Generalizations

So far we have been considering the case of a Cartan subalgebra $\mathfrak h\subset \mathfrak g$, and its orthogonal complement with a choice of splitting into two conjugate subalgebras, $\mathfrak n^+ \oplus \mathfrak n^-$. Equivalently, we have a choice of Borel subalgebra $\mathfrak b\subset \mathfrak g$, where $\mathfrak b =\mathfrak h \oplus \mathfrak n^+$. At the group level, this corresponds to a choice of Borel subgroup $B\subset G$, with the space G/B a complex projective variety known as a flag manifold. More generally, much of the same structure appears if we choose larger subgroups $P \subset G$ containing B such that G/P is a complex projective variety of lower dimension. In these cases $Lie\ P=\mathfrak l \oplus \mathfrak u^+$, with $\mathfrak l$ (the Levi subalgebra) a reductive algebra playing the role of the Cartan subalgebra, and $\mathfrak u^+$ playing the role of $\mathfrak n^+$.

In this more general setting, there is a generalization of the Harish-Chandra homomorphism, now taking $Z(\mathfrak g)$ to $Z(\mathfrak l)$. This acts on the cohomology groups $H^k(\mathfrak u^+,V)$, with a generalization of the Casselman-Osborne lemma determining what representations of $\mathfrak l$ occur in this cohomology. The Dirac cohomology formalism to be discussed later generalizes this even more, to cases of a reductive subalgebra $\mathfrak r$ with orthogonal complement that cannot be given a complex structure and split into conjugate subalgebras. It also provides a compelling explanation for the continual appearance of ρ, as the highest weight of the spin representation.

Comments

1. **capitalistimperialistpig**
   December 17, 2008

   I don't know anything about the subject, but it sounds like a suitable title for a "The Big Bang Theory" episode.

2. **D R Lunsford**
   December 18, 2008

   This presentation of Casimirology is really first rate.

   -drl

This and That

December 17, 2008
Categories: Experimental HEP News, Uncategorized

This week's Nature has a nice cover story on Lyn Evans, who has been leading the construction of the LHC. The story mentions one of the problems of his high-profile job:

> Evans has found himself the subject of more than one ad hominem attack in physics chat rooms and blogs; he knows because he Googles to find out.

While beam commissioning won't start up again at the LHC until at least next July, at the Tevatron things have been going extremely well. Last week they set a new luminosity record, accumulating 74 pb^{-1}. For more about this, there's a posting at Symmetry Breaking.

The Boston Globe has an interview with Lisa Randall, who is writing the libretto for an opera to be entitled "Hypermusic Prologue: A projective opera in seven planes".

Lieven le Bruyn has a posting about David Mumford and the so-called "Red Book", the notes for his course on algebraic geometry. This includes a reproduction of Mumford's picture of Spec Z, together with explanations of what all the squiggles mean. From this posting I also learned about a wonderful book on the topic of "Five Centuries of French Mathematics", available here.

Taking a look at the Theoretical Particle Physics Jobs Rumor Mill, things are looking quite bad for tenure-track jobs in string theory or, more generally, any formal work on quantum field theory. It seems that what US physics departments most want now are cosmologists and "astro-particle physicists". One place that plans to do a lot of hiring in this area is Arizona State, which is advertising for 8-10 new faculty appointments in these areas, and a similar number of postdocs, to be hired over the next 5 years. All of a sudden the field of "string cosmology" starts to make a lot more sense.

One organization that may need a lot of string theory instructors is the Maharishi Central University which will offer "Unified Field Based Education":

> The groundbreaking curriculum of Maharishi Central University is based upon the most advanced scientific knowledge of our age: the discovery of the Unified Field. During the past quarter century, modern physics has explored progressively more fundamental levels of nature's functioning at the atomic, nuclear and sub-nuclear scales, culminating in the recent discovery of the Unified Field—a single, universal field of nature's intelligence at the foundation of the universe.
>
> This Unified Field, or "E8xE8 superstring field," is the crowning achievement of fifty years of advanced research in quantum gravity theory, and is expressed most concisely in the following, compact Lagrangian, or "super-formula," presented, for simplicity, in the super-conformal gauge…

The summary of the curriculum goes on to explain how the superstring field "provides the long-sought, mathematically rigorous, interdisciplinary foundation for all the sciences, and for the whole field of academic study," and that "Without such knowledge, the entire field of education is essentially baseless."

The plan seems to be to build 50 universities, one in each state, with a construction cost of $16 million each. They're looking for investors, who are told that each university will enroll 200 students who will pay $45,000/year, generating an income of $9 million per year, so "This will render financing completely risk free." This money-raising effort is related to the one discussed here.

The first such university is being built at the "exact geographic center" of the US, a point about 12 miles northeast of Smith Center, Kansas. The news from Raja Robert Wynne, Mayor of Maharishi Vedic City and Raja of Invincible New Zealand, Armenia, Kenya, Pakistan, Iraq, Vanuatu, Liberia, and Burundi for the Global Country of World Peace, is that there are 10 buildings now under construction. From an AP article about this, according to founding president John Hagelin

> "The ultimate vision is 40,000 students. We're probably not interested in something smaller than 10,000 students"… He said it would take more than $100 million to start up the university – which he had wanted to have open two years after construction began – and that kind of money isn't easy to find amid a national banking crisis. Because of that, he said, a more reasonable estimate would be that the university will open in five to 10 years.

The locals seem to not be very happy about all this, worried by the presence of a Mexican construction company with Mexican workers at the site. One such Kansan is the Rev. Dennis Lambert, whose church is nearby, who says "We consider them to be a cult". The AP article explains that

> Lambert was among a small group of people who in 2006 dug up what they believe to be a Hindu idol on a rural property that meditators had once owned about 10 years ago. The figure, a hollow metal animal, contained fake jewels symbolic of the nine planetary gods, he said.
>
> "The fake jewels were crushed and the metal deal was destroyed with heat," Lambert said. "It was believed to have demonic influence and that's the way we dealt with it."

Comments

1. **csrster**
   December 18, 2008

   Rev. Lambert scares me more than Dr. Hagelin.

2. **Ralph**
   December 18, 2008

   "Invincible New Zealand"?

   Us NZers are about as capable of defending ourselves against a serious invasion as we are of stopping the decay of a meta-stable vacuum, in either case we just hope we're far enough away from northern hemisphere stuff like overpopulation and the LHC that it doesn't effect us 🙂

3. **Sultan**
   December 18, 2008

   The fake jewels were crushed and the metal deal was destroyed with heat," Lambert said. "It was believed to have demonic influence and that's the way we dealt with it."

   So, heat destroys demonic influences? I should think that they would be used to it.

4. **MathPhys**
   December 18, 2008

“Because of the extraordinary significance to mankind of Dr. Nader Raam’s scientific breakthrough that consciousness is the basis of human physiology, he was awarded his weight in gold and crowned in October 2000 as “Maharaja Adhiraj Rajaraam, First Ruler of the Global Country of World Peace.” Shortly thereafter, Maharaja Adhiraj Rajaraam’s Global Administration honored Dr. Hagelin as Minister of Science and Technology (2001). ”

Are these guys for real?

5. **Arun**
December 18, 2008

It would be amazing if the ancient Hindus knew of the nine planets – but they didn’t. They knew the five visible to the naked eye. The other four are the sun, moon, Rahu and Ketu.

http://www.navgraha.org/

“”Nav” or “Nava” means “nine”. Graha is sometimes translated as “planet”, but the Sun, Moon, and Rahu and Ketu are not “planets” according to modern astronomy. “Graha” is sometimes translated as “celestial body”, but Rahu and Ketu are not celestial bodies either. A third translation is celestial god or demi-god, but again, Rahu and Ketu are Asuras not Devas. Rahu and Ketu are further believed to be only positions in the planetary paths. A fact common to all navagrahas is that they have relative movement with respect to the backgound of fixed stars in the zodiac belt.”

More precisely, two of the “planets”, Rahu and Ketu, are the lunar nodes (the two points where the moon’s orbit intersects the ecliptic). They “cause” solar and lunar eclipses.

The graha do not fit into the category of “demonic” or “godly” either, as noted above.

It is a conceptualization of the world outside the domain, beyond the grasp of Rev. Lambert.

6. **themanwithaplan**
December 18, 2008

This is something right out of Monty Python’s holy grail quest.

Peter, a question: do you know who funds these guys? Their hilariously-inept (and comical) invitations for multi-million dollar fundings surely don’t net anything, howcome they’ve been running for so long? Who’s giving them all this money, cause I also have some magical beans to sell…

7. **Peter Woit**
December 18, 2008

themanwithaplan,

I’m also quite curious about the finances of this organization. As far as I can tell though, their grandiose plans to raise lots more money aren’t working out.

8. **Sumar Ongi**
December 18, 2008

That Hagelin entertains those projects is nothing unusual, it seems he’s been doing so for at least a decade now. His sources of funding are admittedly a more interesting research topic.

But Randall? An *opera* ??!!! That’s … well…, surprising, to say the least.

9. **Markk**
December 18, 2008

The Tevatron info was interesting. The fact that they have bumped luminosity by a factor of six in the last few years by what seems to be process improvements means, to me, that we are still on the upslope of the learning curve in knowing how to mess with high power particle beams. Long term that is good news for LHC hopes as they will climb that curve too.

I think it is a reason to keep the Tevatron going. People always want the next thing, the Higgs or whatever, but this is new high energy physics data and a bunch of it. We aren’t getting stuff like this anywhere else and won’t for a while. It will be a good set of data to use as a check and to find little things that we don’t know to look for now. Maybe Fermilab could try to start pioneering an open standard format for this information and create an open archive, like CERN was looking at a while ago.

10. **Chris W.**
December 18, 2008

That Hagelin entertains those projects is nothing unusual, it seems he’s been doing so for at least a decade now. His sources of funding are admittedly a more interesting research topic.

Hmmmm. Maybe something interesting will come out of the impending litigation over Bernard Madoff’s Ponzi scheme. 🙂

11. **DB**
December 19, 2008

“But Randall? An opera ??!!! That’s … well…, surprising, to say the least”

She probably got the idea from John Adam’s Doctor Atomic (2005), about Oppenheimer and the Manhattan Project.

12. **A.**
December 21, 2008

A summary of news on the Tevatron from Beate Heinemann at the UK Annual Theory Meeting:

No deviation from the SM in diboson production, preliminary data for a CP violating phase measured in B_s -> J\psi\psi decay might not agree with the SM prediction, this will be watched, and there is still no concensus on whether the CDF results indicate new physics or a miscalculated background.

Dave Charlton gave a status on the LHC which just confirms what Peter said in his post.

13. **woit**
    December 21, 2008

    Thanks A.,

    Many of the talks at that meeting look quite interesting, and slides are available there.

    I found the perspective of the talk on String Phenomenology and the LHC to be kind of amusing. It ends with

    "LHC can give us important information on the structure of the MSSM string landscape!"

    The philosophy seems to no longer be that experiment can test string theory. All experiment can ever do is tell us "where we are" in the landscape, with string theory and the string theory landscape now having the status of untestable axioms of physics.

14. **Chickenbreeder**
    December 22, 2008

    I noted that you no longer have the link to Arizona State job announcement. Are those 8-10 jobs not real after all? They just seem too good to be true since all I have heard is hiring freeze.

    I believe the scarcity of jobs in theoretical physics is correlated with the ill wind of economic downturn that's blowing over elite universities. Even Harvard and Stanford announced major cutback and hire freeze due to endowment loss.

15. **Peter Woit**
    December 22, 2008

    Chicken Breeder,

    The ASU job announcement is still there. I have no idea if budget problems will cause them to cut back and hire fewer people.

    All of US academia is having budget problems, especially public universities since state and local budgets are having problems.

    Harvard has announced major budget cuts, supposedly because their endowment has gone down $8billion this year. Unclear to me why this is such a huge problem, since it went up $6billion last year…

16. **BlissCat**
    December 22, 2008

    I sincerely hope to see the opening of Maharishi Central Universities everywhere. The idea of an underlying interdisciplinary foundation in education is very appealing to me, but sorely lacking in academia today, so that we are often left with an unbalanced curriculum which cannot fully develop a student's brain. In contrast, students who befit from the sort of complete education Dr Hagelin is talking about, develop their inner potential through the practice of Transcendental Meditation, increasing creativity, learning ability, the ability to relate to others, and cultivating the total potential of their brain. That to me is the basis of success in life. Click here for a description of scientific studies on the benefits of Transcendental Meditation in education:
    Transcendental Meditation & Brain Potential

17. **Henriette Hagelin-Chandra**
    December 23, 2008

    Well, BlissCat, since you seem to have the secret of success, I don't doubt you'll be donating a couple million of your dollars to our Maharishi C.U. We appreciate your financial support.

18. **Kay**
    December 23, 2008

    The National Institutes of Health have granted $24 million to study Transcendental Meditation for the prevention and treatment of heart disease, hypertension, and stroke – they wouldn't do that for a money scam! The beneficial effects of TM have been studied by independent researchers at Harvard, Standford, Yale, UCLA Medical School, and literally hundreds of leading institutions, and published by the American Medical Association, the American Heart Association, the International Journal of Neuroscience, etc. So don't let these fear-based claims get to you. See for yourself: Transcendental Meditation : Ask the Doctors. TM is safe, effective, and affordable with scholarships.

19. **Damru10**
    December 24, 2008

    Where does the money come from? Good question. The money comes from wherever it is. You compose a proposal for your idea, locate funding sources, then make your pitch.

    Imagine the pitch the Visionary Physicists(VP) of CERN gave to the Banker(B) who represents the universities that funded LHC.
    VC: We want €500Billion.
    B: Why?
    VC: To discover the Higgs Boson.
    B: When you find it, can you show it to me?
    VC: No. It's intangible to the five senses.
    B: Intangible? Well, what good is it, then?
    VC: It will prove my theory of the universe is correct.
    B: Let me get this straight. You want €500Billion for philosophical reasons. Right?

    That's gotta be a tough sell. The LHC visionaries at CERN must have told a very convincing story to get that kind of money. Of course making a pitch like this is predicated upon the money people actually listening to you.

    The regular contributors to this blog are scientists. As scientists you have to be painfully aware of Galileo's plight. He offered a heliocentric theory of how the universe functions to the ecclesiastical power elite who were deeply entrenched in a geocentric view. The Vatican was so threatened by Galileo's ideas that they contained him under house arrest for the rest of his life. Scientists would not want to duplicate the mistakes of the past. Would they? They would listen with an open mind to new ideas and evaluate them based on the science. Right? No real scientist would dismiss a new idea based solely upon here-say.

If anyone wants to hear what Dr. Hagelin is actually saying, here are his lectures on Unified Field Based Approach to various topics, including Health Care, Education, Defense, Poverty Removal, Global Financial Bailout Plan, Architecture, Agriculture and Administration:
http://www.gfcny.net/video/

For a review of the science behind the technologies of consciousness that Dr. Hagelin is recommending, examine the more than 600 peer reviewed studies, performed by over 215 independent universities and research institutions, that have been published in more than 160 peer-reviewed scientific journals and edited books.:
http://www.TruthAboutTM.org/truth/Research/TMResearchPublications/index.cfm

Remember Arthur Schopenhauer's 3 stages of truth:
"First, it is ridiculed.
Second, it is violently opposed.
Third, it is accepted as being self-evident."

20. **Kay**
December 24, 2008

Wow, thanks, Damru10, for your thoughtful, intelligent reply. It brings this discussion to a whole new level.

21. **Shantanu**
December 24, 2008

Peter, there are 2 other string-related talks at PI
See
http://www.pirsa.org/08120032/
Introduction to string theory

http://www.pirsa.org/08110034/
(This one is about eternal inflation)

22. **archie**
December 24, 2008

Regarding Hagelin and Maharishi Central U.: Damru10's Schopenhauer quote bears repeating:

"All truth passes through three stages.
First, it is ridiculed. Second, it is violently opposed.
Third, it is accepted as being self-evident."

23. **me**
December 24, 2008

Bullshit that is (eventually) accepted by mainstream also passes by the exact same stages.

How interesting.

24. **anon.**
December 25, 2008

Pseudoscience indeed passes through Schopenhauer's 3 stages, but the order is reversed:

"First, it is accepted as being self-evident.
Second, it is violently opposed.
Third, it is ridiculed."

Think about flat earth, epicycles and the sun orbiting the earth, witchcraft, Caloric fluid heat theory, etc. In a primitive society, even simple natural phenomena like fire, magnets, and gravitation can be held up by priests as "evidence" of their secret wisdom and supernatural craptrap powers. As more evidence accumulates that such things might not need religious belief in uncheckable theories, they can be successfully opposed and finally ridiculed.

25. **ishi**
December 25, 2008

i do have to give credit to the maharishi types; they published a 2 page ad in the wash post for their theory of everything (maybe the standard model lagrangian—i forget). you rarely see any math in the paper (though recently on a math ed article they did have something from complex variable theory). soetimes you take it from whatever source (dumpster diving theory of education).

i actually imagine meditation in general is ok. somewhat like sleeping. they did try to stop crime in DC by meditating (you need the sqrt of the population to get it to stop. (presumably this follows from the LL#s). but we still got the bush administration.

26. **Kay**
January 3, 2009

Please, it's not helpful to speculate that Transcendental Meditation might be like sleep. The brain wave patterns during TM are extraordinarily different from sleep. Here is what actually happens: The technique allows the mind to settle inward beyond thought to experience PURE AWARENESS, the source of thought, also known as transcendental consciousness. This is the most silent and peaceful level of consciousness — one's innermost Self. In this restfully alert state, the brain functions with significantly greater coherence than in sleep or ordinary waking state.

The superior person settles his mind
as the universe settles the stars in the sky.
By connecting his mind with its subtle origin,
he calms it.
Once calmed, it naturally expands,
and ultimately his mind become as vast and immeasurable
as the night sky.
— Lao Tsu

For decades, John Hagelin has been a proponent of a complete unified field theory of all the known matter and force fields of nature. But he has gone much, much farther, by proposing that this unified field at the basis of matter and energy is the SAME as the field of pure awareness described above – the source of thought, experienced while transcending. That silent, transcendental, holistic experience has been described by the great minds of all civilizations, from ancient Vedic seers to Taoist masters, from Greek philosophers to Christian saints, from Renaissance poets to modern artists. It is a universal experience which connects all beings. Hence it has a measurable 'field effect' capable of calming violence and radiating peace in the environment.

Notes on BRST VIII: Clifford Algebras

December 21, 2008
Categories: BRST

Clifford Algebras

Clifford algebras are well-known to physicists, in the guise of matrix algebras generated by the γ -matrices first used in the Dirac equation. They also have a more abstract formulation, which will be the topic of this posting. One way to think about Clifford algebras is as a "quantization" of the exterior algebra, associated with a symmetric bilinear form.

Given a vector space V with a symmetric bilinear form $(\cdot,\cdot)$, the associated Clifford algebra Cliff $(V,(\cdot,\cdot))$ can be defined by starting with the tensor algebra $T^*(V)$ ($T^k(V)$ is the k-th tensor power of V), and imposing the relations

$$v\otimes w + w\otimes v = -2(v,w)1$$

where $v,w\in V=T^1(V),\ 1\in T^0(V)$. Note that many authors use a plus instead of a minus sign in this relation. The case of most interest in physics is $V=\mathbf R^4$, $(\cdot,\cdot)$ the Minkowski inner product of signature (3,1). The theory of Clifford algebras for real vector spaces V is rather complicated. Here we'll stick to complex vector spaces V, where the theory is much simpler, partially because over $\mathbf C$ there is, up to equivalence, only one non-degenerate symmetric bilinear form. We will suppress mention of the bilinear form in the notation, writing Cliff(V) for Cliff$(V,(\cdot,\cdot))$.

For a more concrete definition, one can choose an orthonormal basis e_i of V. Then Cliff(V) is the algebra generated by the e_i, with multiplication satisfying the relations

$$e_i^2=-1,\ \ e_ie_j=-e_je_i\ \ (i\neq j)$$

One can show that these complex Clifford algebras are isomorphic to matrix algebras, more precisely

$$Cliff(\mathbf C^{2n})\simeq M(\mathbf C, 2^n),\ \ \ Cliff(\mathbf C^{2n+1})\simeq M(\mathbf C, 2^n)\oplus M(\mathbf C, 2^n)$$

Clifford Algebras and Exterior Algebras

The exterior algebra $\Lambda^*(V)$ is the algebra of anti-symmetric tensors, with product the wedge product $\wedge$. This is also exactly what one gets if one takes the Clifford algebra Cliff(V), with zero bilinear form. Multiplying a non-degenerate symmetric bilinear form $(\cdot,\cdot)$ by a parameter t gives for non-zero t a Clifford algebra Cliff$(V, t(\cdot,\cdot))$ that can be thought of as a deformation of the exterior algebra $\Lambda^*(V)$. Thinking of the exterior algebra on V of dimension n as the algebra of functions on n anticommuting coordinates, the Clifford algebra can be thought of as a "quantization" of this, taking functions (elements of $\Lambda^*(V)$) to operators (elements of Cliff(V), matrices in this case).

While $\Lambda^*(V)$ is a $\mathbf Z$ graded algebra, $Cliff(V)=Cliff^{even}(V)\oplus Cliff^{odd}(V)$ is only $\mathbf Z_2$-graded, since the Clifford product does not preserve degree but can change it by two when multiplying generators. The Clifford algebra is filtered by a $\mathbf Z$ degree, taking $F_p(Cliff(V))\subset Cliff(V)$ to be the subspace of elements that can be written as sums of $\leq p$ generators. The exterior algebra is naturally isomorphic to the associated graded algebra for this filtration

$$\Lambda^p(V)\simeq F_p(Cliff(V))/F_{p-1}(Cliff(V))$$

$\Lambda^*(V)$ and Cliff(V) are isomorphic as vector spaces. One choice of such an isomorphism is given by composing the skew-symmetrization map

$$v_1\wedge v_2\wedge\cdots\wedge v_p=\frac{1}{p!}\sum_{s\in S_p}sgn(s)v_{s(1)}\otimes v_{s(2)}\otimes\cdots\otimes v_{s(p)}$$

with the projection $T^*(V)\rightarrow Cliff(V)$. Denoting this map by q, it is sometimes called the "quantization map". Using an orthonormal basis e_i, q acts as

$$q(e_{i_1}\wedge e_{i_2}\wedge\cdots\wedge e_{i_p})=e_{i_1}e_{i_2}\cdots e_{i_p}$$

The inverse $\sigma=q^{-1}:Cliff(V)\rightarrow \Lambda^*(V)$ is sometime called the "symbol map".

This identification as vector spaces is known as the "Chevalley identification". Using it, one can think of the Clifford algebra as just an exterior algebra with a different product.

Clifford Modules and Spinors

Given a Clifford algebra, one would like to classify the modules over such an algebra, the Clifford modules. Such a module is given by a vector space M and an algebra homomorphism

$$\pi: Cliff(V)\rightarrow End(M)$$

To specify π, we just need to know it on generators, and see that it satisfies

$$\pi(v)\pi(w) +\pi(w)\pi(v)= -2(v,w)Id$$

One such Clifford module is $M=\Lambda^*V$, with

$$\pi(v)\omega=v\wedge\omega - i_v\omega$$

where i_v is contraction by v. This gives the inverse to the quantization map (the symbol map σ) as

$$\sigma: a\in Cliff(V)\rightarrow \pi(a)1\in \Lambda^*(V)$$

$\Lambda^*(V)$ is not an irreducible Clifford module, and we would like to decompose it into irreducibles. For $dim_{\mathbf C}V =2n$ even, there will be a single such irreducible S, of dimension 2^n, and the module map $\pi:Cliff(V)\rightarrow End(S)$ is an isomorphism. In the rest of this posting we'll stick to the this case, for the odd dimensional case see the references mentioned at the end.

To pick out an irreducible module $S\subset \Lambda^*(V)$, one can begin by choosing a linear map $J:V\rightarrow V$ such that $J^2=-1$ and J is orthogonal ($(Jv,Jw)=(v,w)$). Then let $W_J\subset V$ be the subspace on which J acts by +i, $\overline W_J$ be the subspace on which J acts by -i. Note that V is a complex vector space, and now has two linear maps on it that square to -1, multiplication by i, and multiplication by J. W_J is an isotropic subspace of V, since

$$(v_1,v_2)=(Jv_1,Jv_2)=(iv_1,iv_2)=-(v_1,v_2)$$

for any $v_1,v_2\in W_J$. We now have a decomposition $V=W_j\oplus \overline W_J$ into two isotropic subspaces. Since the bilinear form is zero on these subspaces, we get two subalgebras of the Clifford algebra, $\Lambda^*(W_J)$ and $\Lambda^*(\overline{W_J})$. It turns out that one can choose $S\simeq \Lambda^*(W_J)$.

One can make this construction very explicit by picking a particular J, for instance the one that acts on the element of an orthonormal basis by $Je_{2j-1}=e_{2j},\ Je_{2j}=-e_{2j-1}$ for $j=1,\cdots n$. Letting $w_j=e_{2j-1}+ie_{2j}$ we get a basis of W_J. To get an explicit representation of S as a Cliff(V) module isomorphic to $\Lambda^*(\mathbf C^n)$, we will use the formalism of fermionic annihilation and creation operators. These are the operators on an exterior algebra one gets from wedging by or contracting by an orthonormal vector, operators a_i^+ and a_i for $i=1,\cdots,n$ satisfying

$$\{a_i,a_j\}=\{a^+_i,a^+_j\}=0$$

$$\{a_i,a^+_j\}=\delta_{ij}$$

In terms of these operators on $\Lambda^*(\mathbf C^n)$, Cliff(n) acts by

$$e_{2j-1}=a_j^+-a_j$$

$$e_{2j}=-i(a^+_j+a_j)$$

The Spin Representation

The group that preserves $(\cdot,\cdot)$ is $O(n,\mathbf C)$, and its connected component of the identity $SO(n,\mathbf C)$ has compact real form SO(n). SO(n) has a non-trivial double cover, the group Spin(n). One can construct Spin(n) explicitly as invertible elements in Cliff(V) for $V=\mathbf R^n$, and its Lie algebra using quadratic elements of Cliff(V), with the Lie bracket given by the commutator in the Clifford algebra.

For the even case, a basis for the Cartan subalgebra of $Lie\ Spin(2n)$ is given by the elements

$$\frac{1}{2}e_{2j-1}e_{2j}$$

These act on the spinor module $S\simeq\Lambda^*(\mathbf C^n)$ as

$$\frac{1}{2}e_{2j-1}e_{2j}=-i\frac{1}{2}(a_j^+-a_j)(a_j^++a_j)=i\frac{1}{2}$$

with eigenvalues $(\pm\frac{1}{2},\cdots,\pm\frac{1}{2})$. S is not irreducible as a representation of Spin(2n), but decomposes as $S=S^+\oplus S^-$ into two irreducible half-spin representations, corresponding to the even and odd degree elements of $\Lambda^*(\mathbf C^n)$.

With a standard choice of positive roots, the highest weight of S^+ is

$$(+\frac{1}{2},+\frac{1}{2}\cdots,+\frac{1}{2},+\frac{1}{2})$$

and that of S^- is

$$(+\frac{1}{2},+\frac{1}{2}\cdots,+\frac{1}{2},-\frac{1}{2})$$

Note that the spinor representation is not a representation of SO(2n), just of Spin(2n). However, if one restricts to the $U(n)\subset SO(2n)$ preserving J, then the $\Lambda^*(W_J)$ are the fundamental representations of this U(n). These representations have weights that are 0 or 1, shifted by $+\frac{1}{2}$ from those of the spin representation. One can't restrict from Spin(2n) to U(n), but one can restrict to $\tilde U(n)$, a double cover of U(n). On this double cover the notion of $\Lambda^n(\mathbf C^n)^{\frac{1}{2}}$ makes sense and one has, as $\tilde U(n)$ representations

$$S\otimes \Lambda^n(\mathbf C^n)^{\frac{1}{2}}\simeq\Lambda^*(\mathbf C^n)$$

So, projectively, the spin representation is just $\Lambda^*(\mathbf C^n)$, but the projective factor is a crucial part of the story.

The above has been a rather quick sketch of a long story. For more details, a good reference is the book *Spin Geometry* by Lawson and Michelsohn. Chapter 12 of Segal and Pressley's *Loop Groups* contains a very geometric version of the above material, in a form suitable for generalization to infinite dimensions. My notes for my graduate class also have a bit more detail, see here.

In the next posting we'll see what happens when one chooses $V=\mathfrak g$, and studies the Clifford algebra $Cliff(\mathfrak g)$

Comments

1. **John**
   December 23, 2008

   It seems only few years back that Clifford Algebras have been getting more attention…

   I'm wondering what their primary applications are.

2. **Peter Woit**
   December 23, 2008

   John,

   The primary applications of Clifford algebras have always been to construct spinors (and thus the spin representations of orthogonal groups, from which all other reps of these groups can be constructed) and write down the Dirac equation. Much more speculative has been the idea of trying to use them to understand internal symmetries. Lots of people have tried to do this, with varying results.

   In physics, whenever one works with anti-commuting variables, when one quantizes them, what one gets is a a Clifford algebra. So, the things really are everywhere.

   In mathematics, the use of Clifford algebras to do representation theory, more generally than just for orthogonal groups, has been getting more attention in recent years, and that's part of the story I'm trying to write about here.

3. **John**
   December 23, 2008

Peter, I'm sure you know of David Hestenes' works using Grassmann and (I think also, although I'm not sure) Clifford Algebras…

Aren't Clifford Algebras then used to reformulate some areas of physics? If they are, I'm wondering which areas and (perhaps) how?

Also, are the Clifford Algebra reps a key component of BRST quantization (Just wondering.)

John

4. **Peter Woit**
December 23, 2008

John,

Grassman algebra = exterior algebra

I haven't looked closely at the work of Hestenes. From what I've seen much of it exploits the relationship between the exterior algebra and the Clifford algebra explained in this posting.

Clifford algebras are a crucial part of the approach to BRST I'm explaining in these notes. If you look at standard treatments of BRST, you'll find that some of them crucially use Clifford algebras, others avoid this.

5. **John**
December 24, 2008

Thanks.

6. **andy.s**
December 24, 2008

Hestenes (and also Doran and Lasenby at Cambridge) rewrites basic physics using Clifford algebra to replace vector algebra.

So, for angular momentum $L = r \wedge p$ instead of $L = r \times p$, for example.

It's fun stuff. Doran and Lasenby use this to derive a gauge theory of gravity in flat space.

Physics Murder Mystery

December 27, 2008
Categories: Uncategorized

After the recent news that Lisa Randall is writing the libretto for an opera, there's further evidence that particle theorists in Cambridge are moving in the direction of creative writing. Today's Wall Street Journal has a feature article about various people's plans for 2009. One of these is Frank Wilczek, who writes:

> I'm writing a physics murder mystery. The idea is that two men and two women from Harvard and MIT collaborate and discover dark matter. It's clear that they should win a Nobel Prize, but according to the rules of the prize, only three people at most can share.

This is an entertaining idea for a plot, and perhaps it has some personal resonance with Wilczek. For much of his career, he was well-known to be one of the people responsible for a definitely Nobel-prize caliber discovery, but he did not have a Nobel prize. By some counts, there were four people (Gross, Politzer, 't Hooft, Wilczek) who had a hand in the discovery of asymptotic freedom back in 1973. It was only with the award of a Nobel for related work to 't Hooft and Veltman in 1999 that the numerical obstruction to an asymptotic freedom award was removed, with the award going to the other three in 2004. Over this quarter century or so, surely it did not occur to any of the four that it might not be an entirely bad thing if one of them didn't live to a ripe old age….

Next month I'll be spending a week or so in Paris, partly for vacation, partly to attend a conference about Grothendieck's mathematical legacy, to be held at the IHES, a place I've never before visited. There's a murder mystery about the IHES that I've heard about but haven't yet read, so I hope to get a copy in France. The author is Nicole Gaume, who worked for the IHES director, and was forced out when a new director (Marcel Berger) came on the scene. Under the pen-name Margot Bruyère she wrote a roman à clef featuring the mathematicians of the IHES and the murder of a new director. The book first came out under the title *Dis-moi qui tu aimes (je te dirai qui tu hais)*, but was republished in 2002 under the new title *Maths à mort*. For more information about the book, see here.

Update: I just noticed that Wilczek has posted on his web-site an essay about Hermann Weyl's Philosophy of Mathematics and Natural Science that will be the introduction to a new edition of the book appearing next year.

Comments

1. **anon.**
   December 27, 2008

   "Writing a book for a general audience connected Randall with a new set of people in fields outside of physics. One of them, the Spanish composer Hector Parra, intrigued Randall by asking if she would try writing a libretto for an opera about her work. The resulting piece, a collaboration with the artist Matthew Ritchie, is scheduled to debut in Paris at the Georges Pompidou Centre this summer, then travel throughout Europe in the fall. …

   "The piece has the puzzling title of "Hypermusic Prologue: A projective opera in seven planes," the seven planes referring to space and to the opera's seven acts. The work's broader goal is to suggest new approaches to both science and art. The old-fashioned form of opera, Randall and her colleagues hope, can become a vehicle for modern science, using sound and voice to re-create the many dimensions that physicists now explore."

   It's interesting that string theorists are so successful in popularizing their ideas that people *now actually ask them to convey the ideas by media such as opera*. Lisa can't be blamed, since she was asked to write it. But will anyone ever ask people working on alternatives to string theory to write operas to popularize those? Is it obviously the Matthew effect (Matthew 25:29), i.e. once an idea has attained a critical mass of publicity, nothing can prevent a media frenzy about it that ignores alternative ideas completely. Extra dimensions have so much buzz, they're in fashion. Does opera want to be fashionable, or is it really the best medium for exploring the physics of string theory?

2. **Coin**
   December 27, 2008

   Incidentally, if this question is sufficiently on topic: Has anyone read Wilczek's "The Lightness of Being", is it recommended? Does it do much to teach physics concepts or is it more a philosophy-of-science sort of thing?

3. **Peter Woit**
   December 27, 2008

   Coin,

   It's not designed for people who already know a lot about particle physics, but more aimed at (among other things) giving the general public some idea of the insights from the Standard Model about where mass comes from. I liked the fact that it's free of the kind of speculative hype and science fiction that too often characterizes popular books about physics these days.

   For a more technically sophisticated reader, I'd recommend Wilczek's "Fantastic Realities", which I wrote about here:

   http://www.math.columbia.edu/~woit/wordpress/?p=394

4. **Chris W.**
   December 27, 2008

   > It's interesting that string theorists are so successful in popularizing their ideas that people *now actually ask them to convey the ideas by media such as opera*. Lisa can't be blamed, since she was asked to write it.

   Lisa Randall is not a string theorist. Of course one can argue that Kaluza-Klein inspired models of the sort for which she has become known have become about as trendy (and problematic) as string theory itself, but the distinction should be maintained.

5. **Thingumbob**
   December 28, 2008

   It was certainly eye opening to see in Wilczek's survey of Weyl such an emphasis on Leibniz' philosophy. I believe we would be hard pressed today to find a mere handful of theoreticians who comprehend the legacy and vector of method running through Leibniz, Gauss, Dirichlet, Riemann, Cantor, Hilbert to Goedel, et al.

6. **Cormac O Raifeartaigh**

December 28, 2008

Hi Peter, interesting post as always.
Lochlainn spent a year at IHES in the seventies, working with Louis Michel – that is also where his book on the group structure of gauge theories was written. All the family have happy memories of both Bures and the IHES.
Re Nobel, I've never understood hy the Swedish Academy have stuck to such a literal interpretation of the rules, it's really a pity..

7. **theoreticalminimum**
December 28, 2008

Maybe you could enquire how things are going with IHES' edition of "Récoltes et semailles" when you are in Bures-sur-Yvette? 🙂

8. **Jonathan Vos Post**
December 29, 2008

One should not be surprised by the subgenre of Physics Murder Mystery" given precursors such as:

(1)
"Special Topics in Calamity Physics"
by Marisha Pessl
ISBN13: 9780670037773
ISBN10: 067003777x
"is a darkly hilarious coming-of-age novel and a richly plotted suspense tale told through the distinctive voice of its heroine, Blue van Meer. After a childhood moving from one academic outpost to another with her father (a man prone to aphorisms and meteoric affairs), Blue is clever, deadpan, and possessed of a vast lexicon of literary, political, philosophical, and scientific knowledge"

(2)
Einstein Year – a year celebrating physics – Who killed Prof Jaeger?
"Rufus Jaeger, a world-famous physicist, is killed while demonstrating a live quantum experiment on stage – but was it an accident? Follow news@nature.com's murder mystery to track down the killer."

(3)
I. Goldhaber-Gordon and D. Goldhaber-Gordon, "Schrodinger's mouse- trap – Part 6: A cryptic response", Nature 433, 805 (2005). Chapter of a serialized physics murder mystery.

(4)
Innes, Michael The Weight of the Evidence. 1943, Harper/Perennial. A somewhat ordinary murder mystery, but the murder was committed using a meteorite in a university setting.

(5)
Preuss, Paul Broken Symmetries. 1983, Pocket Books. A novel of science, politics, and intrigue surrounding the building of a giant particle accelerator in Hawaii. (A 1997 sequel is entitled Secret Passages.)

(6)
Benford, Gregory "Matter's End" in Matter's End. 1994, Bantam. Physicists in India find that protons do decay as predicted by some Grand Unified Theories, with dire consequences for reality.

(7)
Egan, Greg Quarantine.1992, Harper Prism. A sophisticated detective mystery that addresses serious ideas in the interpretation of quantum mechanics.

(8)
Lem, Stanislav The Investigation. 1959, Avon. A novel that considers the philosophical implications of quantum mechanics: what if a mystery is unsolvable in principle?

(9)
Niven, Larry "All the Myriad Ways" in All the Myriad Ways. 1971, Ballantine. Works out some of the implications of the many-worlds interpretation for solving murder mysteries.

(10)
The Physics of Murder by Don Light.

I could go on at length, given my Physics training at Caltech , my coauthorship with Isaac Asimov (who wrote many Mystery stories and books, and several popular books of Physics and Chemsitry), and my father and myself having been Active Members of Mystery Writers of America. But I hope that these 10 data in an existence proof show that Lisa Randall, whether or not you can narrowly call her a String Theorist, is wiring in a grand tradition, and should be welcomed by scientists and the literati alike.

9. **Andrew Gelman**
December 29, 2008

Hi, Peter. This story is consistent with the finding that not getting the Nobel Prize reduces your expected lifespan by two years. The fited article frames it as that winning the prize increases your lifespan, but so many more eligible people don't get it than do (and the No comes year after year). I'd guess that it's a net reducer of scientists' lifespans. Even setting murder aside.

10. **Jon A.**
January 1, 2009

Peter, is the IHES conference open to the public, or do you have to be a registered guest?

I am a math PhD student, and will be traveling in the area. I would like to attend the talks on Monday.

11. **Jon A.**
January 1, 2009

I just found out that the registration is closed for the conference. Thanks.

12. **coolstar**
March 12, 2009

It's probably not well know to regular readers here, but there's a similar "4 person" problem in astronomy now. The four are Marcy & Butler and Mayor & Queloz for their work in discovering extra-solar planets. I strongly suspect that if THAT number was reduced by one (not that I'm hoping for that), then the survivors would have a good shot at the Nobel. Unlike the example given in physics, the chances of a "good" solution to this seems unlikely.

Notes on BRST IX: Clifford Algebras and Lie Algebras

December 29, 2008
Categories: BRST

Note: I've started putting together the material from these postings into a proper document, available [here](), which will be getting updated as time goes on. I'll be making changes and additions to the text there, not on the blog postings. For most purposes, that will be what people interested in this subject will want to take a look at.

When a Lie group with Lie algebra \mathfrak g acts on a manifold M, one gets two sorts of actions of \mathfrak g on the differential forms \Omega^*(M). For each X\in \mathfrak g one has operators:

- \mathcal L}_X: \Omega^k(M)\rightarrow\Omega^k(M), the Lie derivative along the vector field on M corresponding to X

and

- i_X:\Omega^k(M)\rightarrow\Omega^{k-1}(M), contraction by the vector field on M corresponding to X

These operators satisfy the relation

di_X+i_Xd={\mathcal L}_X

where d is the de Rham differential d:\Omega^k(M)\rightarrow \Omega^{k+1}(M), and the operators d, i_X, \mathcal L_X are (super)-derivations. In general, an algebra carrying an action by operators satisfying the same relations satisfied by d, i_X, \mathcal L_X will be called a \mathfrak g-differential algebra. It will turn out that the Clifford algebra Cliff(\mathfrak g) of a semi-simple Lie algebra \mathfrak g carries not just the Clifford algebra structure, but the additional structure of a \mathfrak g-differential algebra, in this case with \mathbf Z_2, not \mathbf Z grading.

Note that in this section the commutator symbol will be the supercommutator in the Clifford algebra (commutator or anti-commutator, depending on the \mathbf Z_2 grading). When the Lie bracket is needed, it will be denoted _{\mathfrak g}.

To get a \mathfrak g-differential algebra on Cliff(\mathfrak g) we need to construct super-derivations i_X^{Cl}, {\mathcal L}_X^{Cl}, and d^{Cl} satisfying the appropriate relations. For the first of these we don't need the fact that this is the Clifford algebra of a Lie algebra, and can just define

i_X^{Cl}(\cdot)=

For {\mathcal L}_X^{Cl}, we need to use the fact that since the adjoint representation preserves the inner product, it gives a homomorphism

\widetilde{ad}:\mathfrak g \rightarrow \mathfrak{spin}(\mathfrak g)

where \mathfrak{spin}(\mathfrak g) is the Lie algebra of the group Spin(\mathfrak g) (the spin group for the inner product space \mathfrak g), which can be identified with quadratic elements of Cliff(\mathfrak g), taking the commutator as Lie bracket. Explicitly, if X_a is a basis of \mathfrak g, X_a^* the dual basis, then

\widetilde{ad}(X)=\frac{1}{4}\sum_a X_a^*_{\mathfrak g}

and we get operators acting on Cliff(\mathfrak g)

{\mathcal L}_X^{Cl}(\cdot)=

Remarkably, an appropriate d^{Cl} can be constructed using a cubic element of Cliff(\mathfrak g). Let

\gamma= \frac{1}{24}\sum_{a,b}X^*_aX^*_b_{\mathfrak g}

then

d^{Cl}(\cdot)=

d^{Cl}\circ d^{Cl}=0 since \gamma^2 is a scalar which can be computed to be -\frac{1}{48}tr\Omega_{\mathfrak g}, where \Omega_{\mathfrak g} is the Casimir operator in the adjoint representation.

The above constructions give Cliff(\mathfrak g) the structure of a filtered \mathfrak g-differential algebra, with associated graded algebra \Lambda^*(\mathfrak g). This gives \Lambda^*(\mathfrak g) the structure of a \mathfrak g-differential algebra, with operators i_X, \mathcal L_X, d. The cohomology of this differential algebra is just the Lie algebra cohomology H^*(\mathfrak g, \mathbf C).

Cliff(\mathfrak g) can be thought of as an algebra of operators corresponding to the quantization of an anti-commuting phase space \mathfrak g. Classical observables are anti-commuting functions, elements of \Lambda^*(\mathfrak g^*). Corresponding to i_X, \mathcal L_X, d one has both elements of \Lambda^*(\mathfrak g^*) and their quantizations, the operators in Cliff(\mathfrak g) constructed above.

For more details about the above, see the following references

- A. Alekseev and E. Meinrenken, The non-commutative Weil algebra, Invent. Math 139, 135-172 (2000), or [arXiv:math/9903052]()
- E. Meinrenken, Clifford algebras and Lie groups, [2005 Toronto lecture notes]()
- G. Landweber, Multiplets of representations and Kostant's Dirac operator for equal rank loop groups, Duke Mathematical Journal 110, 121-160 (2001), or [arXiv:math/0005057]()
- B. Kostant and S. Sternberg, Symplectic reduction , BRS cohomology and infinite-dimensional Clifford algebras, Ann. Physics 176, 49-113 (1987)

Comments

1. **Shantanu**
   December 31, 2008

   Peter this maybe a bit OT, But your thoughts about this paper by
   Mcelrath
   [http://arxiv.org/abs/0812.2696]() ?

Thanks

2. **lewallen**
January 1, 2009

   Thanks a lot for the pdf! It’s great and very convenient.

3. **Peter Woit**
January 2, 2009

   Shantanu,

   Sounds pretty implausible, but I know very little about it. For some discussion with McElrath, see the comments at

   http://blogs.discovermagazine.com/cosmicvariance/2008/12/27/gravity-emergesfrom-neutrinos/

   One problem is that of universality, raised by Bee, which McElrath acknowledges he doesn’t yet have an answer for.

Dis-moi qui tu aimes (je te dirai qui tu hais)

January 2, 2009
Categories: Book Reviews

A colleague has very helpfully provided me with a copy of the murder mystery set at the IHES that I wrote about recently here, and I've just finished reading it. Since I'm not much of an afficionado of this genre of fiction, I can't really evaluate how good a murder mystery it is. But as a memoir of the IHES during the 1980s, it is excellent. A claim at the beginning of the book that "any resemblance to real persons is just coincidence" seems to be one of the few things in it (besides the murder) that is fiction. As far as I can tell, the descriptions of all characters correspond precisely to someone at the IHES during that period, with only the names changed. I'm guessing that all or most of the anecdotes about these characters also correspond to reality.

It's a roman a clef, so here's the key for the major characters:

Andre Grusin = Leon Motchane
Henrik Dekker = Nicolaas Kuiper
Charles Bouleaux = Marcel Berger
Antoine Fleuret = Alain Connes
Jacob Zuram = Barry Mazur
Boris Grekov = Mikhael Gromov
Jacques Chevalier = Pierre Deligne

Among the minor characters, I suspect

Joe Bub = Dennis Sullivan
David Amir = Ofer Gabber
Albert Toudy = Adrien Douady

I don't think I'll be giving away too much of the plot to mention that, since the novel was written nearly twenty years ago, back when string theory was a hot topic, one of the plot twists involves string theory. There's a discovery that "superstring theory is renormalizable and predicts that gluonic interactions are colorless".

Comments

1. **lieven**
   January 2, 2009

   i know it's pathetic but the 'que' should be a 'qui'. Anyway, I'd love to read the book in whatever language (it's out of print), now that I've read Dixmier's testimonial :

   « Une partie du charme de ce livre provient de l'environnement psychologique inhabituel. Bien sûr, les passions classiques se déchaînent ; mais aussi des passions moins classiques. Margot Bruyère, à l'évidence, connaît bien le milieu qu'elle décrit. »
   Jacques Dixmier

2. **Peter Woit**
   January 2, 2009

   Thanks Lieven, typo fixed.

   Dixmier's blurb is accurate.

   Is it really out of print? It was republished in 2002 as "Maths a Mort", and that version still shows up on the publisher's web-site:

   http://www.aleas.fr/index.php?option=com_content&task=view&id=16&Itemid=2

   and FNAC

   http://livre.fnac.com/a1355591/M-Bruyere-Maths-a-mort?PID=1

   and amazon.fr

   http://www.amazon.fr/Maths-à-mort-Margot-Bruyère/dp/284301056X/

   Margot Bruyere has a web-site:

   http://www.margot-bruyere.fr/

   and the first chapter is online there:

   http://www.margot-bruyere.fr/en/docs/maths.pdf

3. **lieven**
   January 2, 2009

   thx peter. i did find the website and read the first chapter. the fnac-website sucks and amazon.fr says its unavailable. i'll try to get hold of it somehow. atb :: lieven.

4. **lieven**
   January 2, 2009

   my apologies to fnac.fr. they've promised delivery in 3 weeks…

5. **Margot Bruyère**
   January 4, 2009

Happy new year! Bonne année!
I just discover your website and the comments on my book “Dis-moi qui tu aimes” alias “Maths à mort”, and I am happy to state that most people enjoyed it.

I regret that my english is too poor to translate it. But if somebody thinks it’s worth to do it and have it print, I would be very gratefuf… but I do’nt know a translator (Mary Turner seems to have given up) neither a publisher.

6. **Peter Woit**
   January 5, 2009

   Margot Bruyere,

   I also definitely enjoyed the book, thanks for writing it!

   I know nothing about the market for murder mystery books, but perhaps a press that publishes mathematics books would be interested. Maybe Princeton University Press, perhaps Jacques Chevalier could help…

   Peter

7. **Margot Bruyère**
   January 7, 2009

   Thank you, Peter, for reading the book. I am happy that you appreciate it. Maybe I will try to have the book translated! But I am working now more on history than mathematicians.

   I enjoyed very much the time I worked at IHES, but I enjoy also very much the life I have now in Brittany.

New Year Links

January 5, 2009
Categories: Uncategorized

There's a conference going on in Jerusalem now on the topic of Particle Physics in the Age of the LHC. Some slides and other talk materials are here, video may start appearing here. Not clear when the "Age of the LHC" is; unfortunately we're still a ways away from first collisions, even farther from new physics. Next year, starting in May, the KITP will be running a program on The First Year of the LHC, which may also be jumping the gun a bit, at least to the extent that the topic is LHC physics results. Last year's LHC program, Physics of the Large Hadron Collider, has a web-site that still begins with the counter-factual "The Large Hadron Collider (LHC) will begin operation by the end of 2007."

Also next year, the KITP will be running another supposedly LHC-related program, entitled Strings at the LHC and in the Early Universe. I wonder what the KITP director thinks of this, since he's on record as thinking it unlikely that the LHC will have anything to say about string theory. A much less dubious KITP program about string theory is the one starting today, with the title Fundamental Aspects of String Theory. This program focuses on the current lack of understanding of what string theory really is:

> Over the last decade, string theory has seen important conceptual and technical advances on a host of long-standing problems involving non-pertur-bative and strongly-coupled physics. However, the fundamental ingredients of superstring theory and M-theory are still not well understood, and this five month program will be directed at these open questions.

The first week will be devoted to introductory talks about string field theory and the pure spinor formalism, two quite different attempts to give a new and different formulation of string theory.

Also starting today is the big annual meeting of the AMS, held this year in Washington, DC. One of the important features of this meeting is that many institutions, especially smaller ones, do their initial interviews for next year's jobs at the meeting. This coming hiring season promises to be an exceptionally brutal one for job candidates, with financial problems leading to freezes and reduced hiring at many places. One resource for young mathematicians on the job market is the web-site of the Young Mathematicians Network.

I wrote about Witten's talk on quantum Yang-Mills theory at the Yau birthday conference here. A write-up of the talk is now available as a preprint here.

There's a new book coming out this month that I'm looking forward to reading, Graham Farmelo's biography of Dirac, entitled The Strangest Man: The Hidden Life of Paul Dirac, Quantum Genius. Nature Physics has a review here.

This week's Science Saturday featured John Horgan and George Johnson discussing the state of science journalism and what it has to do with blogging. As science journalists, they take exception to the point of view common among scientists that their job is just to try and accurately transmit to the public the claims being made by scientists.

Comments

1. **MathPhys**
   January 5, 2009

   Thank you, Peter, for the link to Dirac's biography. There is another by Kragh and a number of biographical sketches by others, but I never thought they were adequate.

   Best wishes for 2009.

2. **John Armstrong**
   January 5, 2009

   To expand on the brutal hiring season: word on the street here at the Joint Meetings is that fully one third of the employers have pulled out of the job fair. And as for me: I quit.

3. **anon.**
   January 6, 2009

   Thanks for the link to Farmelo's book on Dirac. I'm surprised that you are keen to read about the guy, seeing how much he argued against mainstream QFT developments after his own work. (E.g., quotations from Dirac here and here.) Dirac also claimed that there was some argument for a physical Dirac sea of virtual particles in the vacuum:

   'Physical knowledge has advanced much since 1905, notably by the arrival of quantum mechanics, and the situation has again changed. If one examines the question in the light of present-day knowledge, one finds that the aether is no longer ruled out by relativity, and good reasons can now be advanced for postulating an æther. . . .

   'We must make some profound alterations to the theoretical idea of the vacuum. . . . Thus, with the new theory of electrodynamics we are rather forced to have an æther.' – P. A. M. Dirac, 'Is There an æther?', Nature, v168 (1951), pp. 906-7.

   'Infeld has shown how the field equations of my new electrodynamics can be written so as not to require an æther. This is not sufficient to make a complete dynamical theory. It is necessary to set up an action principle and to get a Hamiltonian formulation of the equations suitable for quantization purposes, and for this the æther velocity is required.' – P. A. M. Dirac, 'Is there an æther?', Nature, v169 (1952), p. 702.

   In addition, there was his large numbers hypothesis with the argument that G varies with time. In addition, he was an electrical engineer before studying mathematics. Surely all this crackpottery makes the guy unworthy of serious attention?

4. **Peter Woit**
   January 6, 2009

   John,

   Thanks for the report from the meeting. Good luck to you and others dealing with the job situation. About the only encouragement I can provide is to note that just about all of the many people I know who have left academia over the years look back on that as a good decision…

5. **Peter Woit**
   January 6, 2009

anon.,

Adopting the standard of ignoring any of the great physicists of the past if some of their ideas don't hold up well half a century later would make the study of the history of physics a great deal simpler…

6. **Thomas Larsson**
   January 7, 2009

   Scientists are not remembered for their mistakes and blunders, but for their successes. The list of great physicists doing fundamental mistakes is long. E.g., Lorentz and Poincare were leaders in ether theory, Kelvin ridiculed Einstein's discoveries, Einstein denied QM, Heisenberg's Urfeld theory, Witten promoted string theory 🙂 , and many others. The only safe way to avoid wrong ideas is to not have any ideas at all.

7. **theoreticalminimum**
   February 24, 2009

   Dear Peter,

   I was just wondering if you have had the opportunity to read Farmelo's book on Dirac yet. It would be nice to know what you think about it. I am very much looking forward to buy a copy of the book, since Dirac was so special a human being.

8. **Peter Woit**
   February 25, 2009

   No, haven't got a copy of the book yet. Hope to soon, and probably will write something about it after I get to read it.

Best Job in the US

January 6, 2009
Categories: Uncategorized

According to the latest JobsRated listing released today, the best job in the US is that of mathematician. Pay is good, stress is low, and you don't have to get your hands dirty, but can sit in front of a computer monitor all day. Nice work if you can get it. The job of physicist is significantly less desirable: down at number 13, not quite as good as working as a philosopher (number 12), but a bit better than being a parole officer (number 14).

Comments

1. **A.**
   January 6, 2009

   Cute! If you have tenure, sure, being a mathematician (or theoretical physicist, the day-to-day routine isn't very different) is a sublime job.

   A postdoc position, on the other hand, well I'd guess that would come much further down the list! Pay is OK, stress — mostly from agonising over where the next postdoc is coming from — is high, and you have to get your hands dirty working on trendy stuff you don't really care about in order to get noticed. I've got a heacache just thinking about it.

2. **Sandro**
   January 6, 2009

   "Pay is good, stress is low, and you don't have to get your hands dirty, but can sit in front of a computer monitor all day."

   They forget to say that there are high chances you get obsessed with what you are doing, and that actually what you are working on never leaves you alone…

3. **Cormac O Raifeartaigh**
   January 6, 2009

   Parole officer? I think that tells us a little about this study…

4. **John Armstrong**
   January 6, 2009

   Stress is low? Bull. Like A. points out, that only goes if you've got tenure. The job market is horrific this year.

5. **John Armstrong**
   January 6, 2009

   Looking into their methodology indicates that the mathematician's "mid-level" income is quoted as \$94K, with a "growth potential" (span from low to high income) of "160%", for whatever that means.

   I don't think this is intended as describing academic mathematics, but mathematics as a wider occupation. I'd say academics should be listed separately.

6. **Peter Woit**
   January 6, 2009

   For information on academic math pay-scales, see

   http://www.ams.org/employment/facsal.html

   \$94K would be a typical salary of a tenured person at a university with doctoral programs. Liberal arts and other colleges pay less. Math jobs in the financial industry pay quite a bit more,

7. **Nugae**
   January 6, 2009

   Being a mathematician sounds to me to be about the most stressful job around. Sure, you get paid, and you get to sit around in a nice warm office, but you risk spending years of your life on something that never works out. Or on proving something that someone else, with different tools and insights, proves in 5 minutes.

   Most jobs have a built-in worthwhileness indicator that gives you some sort of an answer to "have I been wasting my time today / this week / this month / this year?". Mathematics doesn't.

8. **Peter Woit**
   January 6, 2009

   Nugae,

   Mathematicians outside of academia are often working on things that quickly get evaluated as to their "worthwhileness". My friends who build models used by hedge funds in their trading strategies are an example. As for academics, you forget that a big part of the job is to teach students. And, with every test they take, you see whether you are doing a good job.

   Pure math research may not work out, or may be hard to evaluate, but one of the main reasons that people do it is that they enjoy the process. You learn new things as you go, even if you don't end up where you had hoped.

9. **st**
   January 6, 2009

   If you're a math major and want a secure high-paying job, be an actuary or a biostatistician. Mathematics research is not a job; it's a vocation, a calling — like monkhood.

10. **John**
    January 6, 2009

    Hi Peter,

    Does this mean that Quant jobs are no longer worth pursuing?

    Regards

11. **Sandro**
    January 7, 2009

    "Pure math research may not work out, or may be hard to evaluate, but one of the main reasons that people do it is that they enjoy the process. You learn new things as you go, even if you don't end up where you had hoped."

    I agree. The problem is when things don't end up anywhere, and it happens quite often…

12. **Peter Woit**
    January 7, 2009

    John,

    From what I hear, people are still getting quant jobs, but they are quite a bit harder to get. Then again, all jobs are harder to get right now….

13. **Steve Myers**
    January 8, 2009

    I was a math major who ended up doing all kinds of work (mostly in electrical field) & have ended up acquiring and analyzing data & writing technical papers with a job with good pay & benefits. (I dropped out of grad school.) My oldest son, who is very gifted mathematically (invented his own form of integral calculus) chose Computer Science & does well (very good pay). My brother became a high school math teacher. So there are many ways to go for math guys. And you can always do pure research on your own for the pure pleasure of it.

14. **John**
    February 5, 2009

    Many people love working in a work place. I appreciate your honesty in this blog.

Dyson on Birds and Frogs

January 7, 2009
Categories: Uncategorized

Next month's Notices of the AMS has an essay by Freeman Dyson entitled Frogs and Birds, which was written for his planned Einstein Public Lecture. In it, he divides mathematicians up into two species: birds, who "fly high in the air and survey broad vistas" (i.e. seek abstraction, unification and generalization), and frogs, who "see only the flowers that grow nearby" (i.e. study the details of specific examples).

Dyson himself is resolutely a frog, but writes that "many of my best friends are birds", and argues that both birds and frogs are needed to do justice to the breadth and depth of the subject of mathematics. Frog that he is, his essay covers a variety of quite different special topics that have drawn his attention, linked together only weakly by the bird/frog theme. These include a discussion of the roles of complex numbers and linearity in quantum mechanics, a proposed idea about how to attack the Riemann hypothesis (try and enumerate 1d-quasicrystals, since the zeros of the zeta function have this structure), and a collection of profiles and anecdotes about various mathematicians and physicists (Besicovitch, Weyl, Yang, Manin, von Neumann).

Personally I suppose I fit well into Dyson's bird category, but among the best mathematicians that I know, the frog/bird distinction is often unclear. Many of them make their reputation by proving rather abstract and general theorems, but these proofs are often the result of a huge amount of detailed investigation of examples. I agree with Dyson that both points of view are needed, and see the most successful cases of progress in mathematics coming from mathematicians who avoid the temptation to fly too high into arid abstraction, or sink too deep into irrelevant detail.

Dyson includes a long section on string theory, which I'll include here:

> I would like to say a few words about string theory. Few words, because I know very little about string theory. I never took the trouble to learn the subject or to work on it myself. But when I am at home at the Institute for Advanced Study in Princeton, I am surrounded by string theorists, and I sometimes listen to their conversations. Occasionally I understand a little of what they are saying. Three things are clear. First, what they are doing is first-rate mathematics. The leading pure mathematicians, people like Michael Atiyah and Isadore Singer, love it. It has opened up a whole new branch of mathematics, with new ideas and new problems. Most remarkably, it gave the mathematicians new methods to solve old problems that were previously unsolvable. Second, the string theorists think of themselves as physicists rather than mathematicians. They believe that their theory describes something real in the physical world. And third, there is not yet any proof that the theory is relevant to physics. The theory is not yet testable by experiment. The theory remains in a world of its own, detached from the rest of physics. String theorists make strenuous efforts to deduce consequences of the theory that might be testable in the real world, so far without success.
>
> My colleagues Ed Witten and Juan Maldacena and others who created string theory are birds, flying high and seeing grand visions of distant ranges of mountains. The thousands of humbler practitioners of string theory in universities around the world are frogs, exploring fine details of the mathematical structures that birds first saw on the horizon. My anxieties about string theory are sociological rather than scientific. It is a glorious thing to be one of the first thousand string theorists, discovering new connections and pioneering new methods. It is not so glorious to be one of the second thousand or one of the tenth thousand. There are now about ten thousand string theorists scattered around the world. This is a dangerous situation for the tenth thousand and perhaps also for the second thousand. It may happen unpredictably that the fashion changes and string theory becomes unfashionable. Then it could happen that nine thousand string theorists lose their jobs. They have been trained in a narrow specialty, and they may be unemployable in other fields of science.
>
> Why are so many young people attracted to string theory? The attraction is partly intellectual. String theory is daring and mathematically elegant. But the attraction is also sociological. String theory is attractive because it offers jobs. And why are so many jobs offered in string theory? Because string theory is cheap. If you are the chairperson of a physics department in a remote place without much money, you cannot afford to build a modern laboratory to do experimental physics, but you can afford to hire a couple of string theorists. So you offer a couple of jobs in string theory, and you have a modern physics department. The temptations are strong for the chairperson to offer such jobs and for the young people to accept them. This is a hazardous situation for the young people and also for the future of science. I am not saying that we should discourage young people from working in string theory if they find it exciting. I am saying that we should offer them alternatives, so that they are not pushed into string theory by economic necessity.
>
> Finally, I give you my own guess for the future of string theory. My guess is probably wrong. I have no illusion that I can predict the future. I tell you my guess, just to give you something to think about. I consider it unlikely that string theory will turn out to be either totally successful or totally useless. By totally successful I mean that it is a complete theory of physics, explaining all the details of particles and their interactions. By totally useless I mean that it remains a beautiful piece of pure mathematics. My guess is that string theory will end somewhere between complete success and failure. I guess that it will be like the theory of Lie groups, which Sophus Lie created in the nineteenth century as a mathematical framework for classical physics. So long as physics remained classical, Lie groups remained a failure. They were a solution looking for a problem. But then, fifty years later, the quantum revolution transformed physics, and Lie algebras found their proper place. They became the key to understanding the central role of symmetries in the quantum world. I expect that fifty or a hundred years from now another revolution in physics will happen, introducing new concepts of which we now have no inkling, and the new concepts will give string theory a new meaning. After that, string theory will suddenly find its proper place in the universe, making testable statements about the real world. I warn you that this guess about the future is probably wrong. It has the virtue of being falsifiable, which according to Karl Popper is the hallmark of a scientific statement. It may be demolished tomorrow by some discovery coming out of the Large Hadron Collider in Geneva.

I don't know where Dyson got the estimate of ten thousand string theorists; my own estimate would be more like one to two thousand (with the number strongly dependent on how you decide who is a "string theorist"). The large yearly Strings200X conferences that bring together a sizable fraction of active string theory community tend to draw roughly 500 people.

The Princeton-centric assumption that there are lots of string theory jobs embedded in his question "And why are so many jobs offered in string theory?" is quite problematic, as any young string theorist on the job market could explain to him. There actually aren't a lot of string theory jobs out there, and a lot of Ph.D.s in the subject being produced, leading to a lot of ex-string theorists now working in the financial industry and elsewhere. These days, if you are going to choose your field based on where the jobs are, you become an LHC phenomenologist or a cosmologist. If you want to be a string theorist, you better be a string phenomenologist or a string cosmologist. Also rather unrealistic is Dyson's "it could happen that nine thousand string theorists lose their jobs", due to tenure in the academic system. Even if a consensus develops over the next few years that string theory was all a big mistake, twenty years from now there will still be a cadre of (older) people working in the field.

Dyson's idea, that 50-100 years from now, a new revolution in physics will show how string theory fits in may be right. It also may be that this has already happened, as much of the field has moved into the study of gauge-string dualities, where string theory provides a useful approximation for strongly coupled systems, and the idea that it unifies particle physics is falling by the wayside.

Comments

1. **Rene Meyer**
   January 8, 2009

Dear Peter,

Thanks for pointing out that nice piece of an essay. As a stringy person myself I would however disagree with Dyson saying that string theorists are too narrowly trained to be useful in other parts of science. People who enter a PhD program in string theory in a reknown place (need not to be Princeton) are good physicists already from their undergrad time, as the students normally interested in string theory are interested in deep questions of physics in general, and for this they need to understand the basics of physics to a good extend, which in turn means they have done their homework already as an undergrad. This is at least my experience. So they are well-trained physicists, and can in principle with some preparation time work in any field of theoretical physics. Also, for understanding the implications of string theory one needs to be well-trained in nearly all subjects of basic theoretical physics, which there are mathematical methods, classical and quantum field theory, statistical physics and gravity/GR. The average string theorist is well-trained in all these subjects. What makes the change of subject away from string theory even to closely related areas like "quantum gravity" (for the purpose here just summarizing all attempts to quantum gravity except string theory), classical gravity or pure cosmology is, in my experience, rather the attitude of other communities towards string theory, which is often full of prejudices originating from hearsay, the failure to understand even the stringy basics and the dissatisfaction thus created. Its often a one-way road: String theorists understand what other people are doing, but the other people dont understand string theory. With such a structure, it is nearly impossible to change for a postdoc in a different field, as everybody takes only the people whose work he knows and understands. This is partly also true in string theory, of course.

Another remark: Not only string theory is cheap, whole theoretical physics is, if you dont need supercomputers. So poorer universities can higher also other people…

2. **somebody**
   January 8, 2009

   Thanks for Dyson's interesting article, Peter. The essay was overall balanced I think, so I will state a few issues with which I disagree.

   1. People don't go into string theory for jobs. I was very sternly warned that going into string theory is dangerous, careerwise, and no string theorist I know had any illusions about it when they got into it either. I suspect that Dyson is forgetting why he went into mathematics and theoretical physics back when he was an excitable young man, but thats just my guess.

   2. While agreeing with the possibility that string theory might be only part of the full picture, I would like to point out that since we don't have the benefit of hindsight, the only working strategy we have available for progress NOW is to push our current theories to their limits. Revolutions, almost by definition, cannot appear on demand. To give an example: It took almost twenty years of work on seemingly pointless things like gauged supergravities, many dimensions, superconformal algebras, large N gauge theories etc. before it all finally came together in gauge-gravity duality.

   3. String theory being superspecialized is also a weird claim. One of the challenges that an aspiring string theorist faces is in fact the ridiculous amount of physics and math they have to learn. I think the real problem is not that string theory is superspecialized, but that some string theorists are superspecialized to one subfield or even to one problem. But this is not just a feature of the string theory community …

3. **Peter Woit**
   January 8, 2009

   Rene,

   I agree that Dyson's explanation that string theorists get hired because they are cheap doesn't explain anything, since they are no more or less expensive than other theorists.

   As for the somebody's point that string theorists don't go into the subject because job prospects are good, that's certainly right, more so now than ever. The question though is a relative one. The job situation in theoretical physics in general has been terrible for a long time. If you decide you want to try for a career as a theoretical physicist, despite the odds, does going into string theory maximize your chances? I think now someone who wants to maximize their chances is more likely to go into phenomenology or cosmology. But if your interest is in fundamental, more mathematical and formal approaches, you still may not have any real alternative to string theory. The job situation for string theorists is bad, but jobs in formal theory that is not string theory are pretty much non-existent in physics departments, at least in the US.

   My impression of the background of string theorists is that it varies widely. Some do actually know a lot of physics and mathematics, and have a good chance of changing fields if they want to. Others don't, and suffer from the disability of not knowing how little they know, making it highly unlikely that they'll ever be able to do what it takes to move to another field.

4. **Jonathan Vos Post**
   January 8, 2009

   I love Dyson's paper. To me, though, the strong point is the detailed and insightful History of transdisciplinary unifiers in Mathematics, and the surprise that this is useful in understanding Physics. I feel that Context trumps Content here. But I agree with the comments that we'll have a batter idea in 50-100 years what this was all about, in the late 20th century. If I'm very lucky, I'll be here in 50 years (aged 107). Nobody yet has been 157, so I'm not going to invest much effort in what might be known in 100 years.

5. **Rene Meyer**
   January 9, 2009

   Dear Peter,

   I have to disagree on the job situation. I just applied for my first postdoc, got some offers, but it was a very competitive thing. Nowadays string community in Europe and the US/Canada is very competitive on the postdoc level already, not to speak about tenure track. I have a little bit of insight into other fields of theoretical physics, where people are handled around to their next postdoc until they are old enough for tenure track. They do not have to go through all the pain of applying worldwide without knowing whether they should get a job at all. Long story short: In my opinion the job market in the string community is more competitive and less connection-driven than in other fields of theoretical physics.

   But feel free to disagree.

   Best, Rene.

6. **Peter Woit**
   January 9, 2009

   Rene,

   I don't disagree at all that the job market for string theorists is now highly competitive.

7. **Haelfix**
   January 9, 2009

   I don't really understand why he thinks lie groups are a failure for classical physics, or why the analogy makes much sense =/

8. **anon.**
   January 10, 2009

   Dyson's 50-100 years guess is for a revolution is identical to the guess in his 1981 essay Unfashionable Pursuits:

   '… At any particular moment in the history of science, the most important and fruitful ideas are often lying dormant merely because they are unfashionable. Especially in mathematical physics, there is commonly a lag of fifty or a hundred years between the conception of a new idea and its emergence into the maintsream of scientific thought. If this is the time scale of fundamental advance, it follows that anybody doing fundamental work in mathematical physics is almost certain to be unfashionable. …'

   – [Dyson's 1981 essay, Unfashionable Pursuits]

9. **nbutsomebody**
   January 10, 2009

   The number 10000 may come from "Brief History of Time". Somewhere it is written that 10000 people are doing string theory world wide, at least that what I remember.

   About the job situation, it is really bad in string theory. There should not be any question about that. But the situation was probably a little different in late nineties.

10. **Grant**
    January 21, 2009

    Peter,

    You write "Personally I suppose I fit well into Dyson's bird category" which makes me wonder, which fields of mathematics/physics did you unify, generalize or abstracted to a higher level? By looking at your record on spires, it's not at all clear what you mean. It seems to me your research has been pretty focused on a single topic, topological aspects of non-abelian gauge theories, which to me seems to fit the frog category.

    Your thoughts?

    Thanks for reading,
    Grant

11. **Peter Woit**
    January 21, 2009

    Grant,

    My early research was on topological effects in lattice gauge theory, but my general area of interest is trying to find new ways of understanding the underlying structure of the standard model using modern mathematics and representation theory. One of my problems in life is that my bird-like tendencies have led me to spend a large fraction of my time flying around at too high altitude trying to learn about different kinds of mathematical structures that might be useful, instead of getting down to ground and working out details and publishing.

    The current ideas about BRST that I'm working on and trying to get down to detail with involve algebraic ideas that are very far from the topological and geometrical ideas that I started with. It has taken me a long time to start to appreciate the algebraic approach.

    In any case, I recognize my tendencies (if not my accomplishments..) in Dyson's description of birds, find his description of "frogs" much more alien (e.g. when he says that he had no interest in the relations between number theory and QM that were pointed out to him)

12. **Phil Harmsworth**
    January 26, 2009

    Peter,

    I'm not sure that I follow Dyson's analogy between Lie groups and string theory. As I understand it, Lie was trying to create a Galois theory for differential equations. The 'disappointment' to which Dyson refers stemmed from his perception of a lack of recognition for his work, and not so much from a lack of applications.

    I also don't understand his description of Lie groups as a failure in classical physics, although admittedly it took until the 1940s before Lie's ideas started to be applied systematically (to hydrodynamics). (Refer the CRC Handbook of Lie Group Analysis of Differential Equations for an overview of more recent applications.)

    I think that it may be more accurate to say that for both classical and quantum mechanics, Lie's ideas needed further development and refinement. For example, the theory of representations of Lie algebras (to which Dyson refers to as natural language of particle physics) was a considerable advance from Lie's concept of 'infinitesimal groups'. As 'Not Even Wrong' describes, it took a considerable period for these ideas to be understood and accepted.

    If there is indeed an analogy for string theory, perhaps it is that the necessary tool or tools for progress already exist in contemporary mathematics, but they may also need further development. And it may not be at all obvious at the outset that these tools are in fact useful at all.

13. **Academic Career Links**
    March 20, 2009

    A related but somewhat different kind of division can be found in the essay [Two Cultures of Mathematics] by W.T. Gowers.

Science Channel Inks Deal With Physicist Michio Kaku

January 8, 2009
Categories: Uncategorized

Fresh from his leading role in the History Channel's Parallel Universes (if you missed it on TV, the DVD is available here), according to a press release today, Michio Kaku will now be appearing regularly on Discovery's Science Channel:

> The Science Channel has signed a multi-year agreement with theoretical physicist Dr. Michio Kaku. As part of the deal, the Discovery-owned network will produce a 10-part series based on his New York Times bestselling book Physics of the Impossible and has exclusive television rights to Kaku's other works for adaptation in series and specials.
>
> In addition, Kaku will become the host of "Sci Q Sundays," the Sunday programming block that explores scientific news and topics.

The show has a web-site here, including instructions on how to build a time-machine.

Comments

1. **Sakura-chan**
   January 8, 2009

   He's great at the capitalism game.

2. **J.F. Moore**
   January 9, 2009

   I actually forced myself to read Physics of the Impossible, so I could legitimately criticize it. I dread having to be the killjoy when people watch this series and get excited about the absurd stuff. Not that I mind straightening people out, they just seem to dislike boring mundane reality.

3. **MathPhys**
   January 9, 2009

   Kaku is *"one of the founders of string field theory"*, but that's too technical, so let's not split strings and just say *"one of the founders of string theory"*.

   The title of a future TV series could be

   "From Veneziano, Susskind and Nambu to Green, Schwarz and Kaku."

   It rhymes!

4. **anon.**
   January 9, 2009

   It would be good to know if there are any surveys of what really motivates most students to become interested in modern physics. If it really is down to sci fi wormholes, Planck scale manifolds, and spin-2 gravitons in string theory, then Kaku is needed in a sense to continue inspiring student interest in physics (one can hope that some of those students will defect back to reality at some stage, instead of worshipping unchecked speculation).

   In some cases the best-scoring students of theoretical physics started off by scoring well in mathematics, and then found a lot of interesting applications for those skills in physics. This is a different line of approach from a deep desire to understand phenomena, which probably is the route taken by those majoring in experimental/applied physics. If you start off with phenomena that you measure and plot, then find an equation for, then develop a theory that explains the equation and predicts other things, you end up with real understanding.

   I hope that this will happen once experiments indicate what is really occurring with electroweak symmetry breaking. The big strength of the standard model is in its experimental basis and experimental checks, which so far haven't extended to the Higgs mechanism. Hopefully LHC experiments will this year focus attention on building theories to explain, and checkably extrapolate predictions from, real phenomena. This is the opposite of what string theorists do when they produce mathematical models of Planck scale speculations and then use those models to produce uncheckable predictions.

5. **D R Lunsford**
   January 9, 2009

   J. F. Moore,

   Yes I think a lot of us have experienced that. I have a friend who is all over these news releases and I'm almost embarrassed to tell him the truth.

   anon.

   No I think serious physicists start out with wall sockets and fire and such. 🙂

   Peter,

   Was hoping for some science news of the new large radio noise discovered by ARCADE (Absolute Radiometer for Cosmology, Astrophysics, and Diffuse Emission) beyond press release hype. Standard modelers will be discomfited.

   -drl

6. **Chris W.**
   January 9, 2009

   Michio Kaku is to physics what Kenny G is to jazz. (Kenny G majored in accounting in college.)

7. **MathPhys**
January 10, 2009

If Nambu has a Nobel prize, why not Kaku?

PS Plagiarized from *"If QED got a Nobel prize, why not QCD?"* which was the catch phrase of the Gross *et. al* lobby, or so have I been told.

8. **Ari Heikkinen**
January 10, 2009

That's funny, as I opened TV today there happened to be a document about CERN and LHC, which was (basicly) touted as a "time machine", "capable of looking into the creation itself", "creating conditions of big bang", etc. and those comments were (apparently) from people working on the project. Atleast in that "how to build a time-machine" piece there's a section that reads "practical problems with time travel".

9. **Will**
January 10, 2009

I dont think people should expect people like Edward Witten or Steven Weinberg to champion physics causes. They have better things to do. Michio Kaku is doing a descent job of trying to popularize fundamental science and it is not an easy job. He may not be anywehre near a top physicist, but he has done commendable work in physics. You cannot expect people to really appreciate ADS CFT or mirror symmetry or dualities. You have to view his shows as not a lecture that consist all facts (which would be impossible) but entertainment with descent amount of good science for people who are talented and interested to get enthused about great progress made by modern physics, string theory and mathematics.

10. **MathPhys**
January 11, 2009

Will,

Seriously speaking, I wholeheartedly agree with you.

11. **Jimbo**
January 11, 2009

In his attempts to popularize theoretical Physics, Kaku really creates a bad image for the field. It is very unpleasant when people hear what I do and say "oh, so you're like Michio Kaku?".

12. **MathPhys**
January 11, 2009

Okay, so we need someone to popularizes theoreticall physics without giving a nutty used car salesmanic impression.

13. **Will L**
January 11, 2009

I think TED presentations are much more effective at inspiring students to become scientists than these pop programs.

14. **trond**
January 11, 2009

Isn't most of his previous documentaries mostly speculations too (he label himself as a futurist), so I guess these are just a continuation of those.

Another one that have been profiling himself is Brian Cox with some BBC documentaries. The latest was "What time is it?". The documentary style is much more personal and I actually learned something from it (and speculative ideas were stated as such). With Kaku it's just no-holds-barred, but then again he has to work within the contraints of commercial media.

15. **J.F. Moore**
January 11, 2009

The problem is with the hyperbole and poor logic Kaku uses when he is popularizing science. I can't conceive of the likes of Carl Sagan being so careless and silly, even though they are both dynamic lecturers and are good at capturing the fantastic scope of science.

For example, Kaku regularly chimes in on a notoriously nutty AM radio show and feeds people's fantasies that there have been extraterrestrial visitors. The question is whether more harm than good is being done ultimately.

16. **MathPhys**
January 11, 2009

Kaku ***is*** an extraterrestrial visitor.

17. **D R Lunsford**
January 13, 2009

You know in India, they have instead of Kaku, Jayant Narlikar (some might remember his work with Hoyle on the steady-state cosmology). He's the soul of restrained reason, and holds a position of near veneration there while continuing his career, emeritus. I can't help but see something deeply disturbing about our culture in Kaku's goofy omnipresence.

JFM, yes the comparison to Sagan was apt. I heard Sagan issue some whoppers about relativity, but on the whole he was enthusiastic and sane at the same time.

-drl

18. **Amos**
January 13, 2009

Since there are more physicists than physics jobs, I don't understand why recruiting more students to the field is a net-positive.

In all events, the people who watch popular science programs (including, occasionally, myself) do so because (a) we want to understand nature, and (b) the accumulation of such an extraordinary understanding of nature makes us proud of the species.

It serves those people no purpose to have a popular science program that fails to educate and fails to illuminate.

19. **King Ray**
January 13, 2009

It sort of reminds me of L. Ron Hubbard, a science fiction writer who decided the best way to get rich was to create a religion, Scientology.

Here we have physicists creating a religion called string theory and profiting from selling it in schools and to the public.

20. **Jake the Snake**
January 13, 2009

In my opinion, Michio Kaku seems like a very nice person. He appears to be a perfect gentleman, but I have trouble taking him seriously as a scientist. As a pop-sci guy he is decent, but Isaac Asimov was WAY better in that respect! Having read through *Physics of the Impossible*, I couldn't help but think that Asimov would have written a book just like it, only a bit better.

Unfortunately, Kaku has become THE pop-sci guy. It would be great if he quit theorizing and became a sci-fi writer. In fact, most string theorists ought to look for work writing science fiction should their area of research ever be marginalized. (…Except maybe for Lubos. Any attempt at fiction by him might be disturbing.)

Then again, it makes sense that the **History Channel** of all channels would select Kaku as their science guy. The History Channel has been notorious for having more shows about UFOs, cryptids, Nostradamus, and other occultic or paranormal stuff than actual history.

21. **MathPhys**
January 14, 2009

Kaku does not just *seem* like a nice man. He *is* a nice man. He's also a very smart man. Too bad he decided to get into the UFO business.

22. **Christine**
January 15, 2009

Asimov and Sagan were not only skilled science popularizers. They represent *schools* of science outreach. I wonder who is doing the job today at the same level of quality.

23. **Peter Woit**
January 15, 2009

Jake,

The History Channel was responsible for the recent awful program about parallel universes, but it's the Discovery Channel that is involved here. Not clear there's much to choose from between them….

24. **smart and nice**
January 15, 2009

how "smart and nice" can one be to promote time machines at the expense of physics?

a physicist's "niceness" and "smartness" should be judged relative to how "smart" he is regarding physical reality, and how nice he is to his peers who are engaging physical reality.

there is a certain type of media-physicist who has evolved who is now doing more damage than good on multiple levels. they suck up all the attention and funding, at the expense of true physicists.

truly, what has kaku truly contributed to physics?

25. **Russ**
January 15, 2009

I don't often watch shows on Discovery or the History channel – but sometimes I do and I admit to hearing things that make me groan. These channels by their very nature are for a wide popular audience. Physics (hell, speaking as someone with an M.A. in history) and History for that matter , are difficult subjects for a mass audience to digest . Physics in particular utilizes mathematics that is way beyond college calculus. If Kaku can translate for a popular audience even a small portion of what we agree to be physics then I think he is doing a service. I challenge 'Smart and Nice' to quantify in dollars just how much Kaku has damaged physics funding? I grew up watching Sagan and he is still a hero in my mind, as far as popularizing astronomy. Who knew how much the guy liked refer? 😉

26. **Jake the Snake**
January 15, 2009

@ MathPhys:

Well, I can't say I know him personally. He *COULD* be a prick and I wouldn't know, I simply would not conclude something based on my impression, though he does seem like a good person. However, you appear to have known him and that he is indeed a nice guy does not surprise me. And he would have to be very intelligent to wrap his mind around higher physics concepts like string theory. The problem is when scientists like Michio Kaku use their brilliance and personality to act as PR for bad ideas. As long as string theorists have agreeable and intelligent spokespeople, they have PR.

@ Christine,

Isaac Asimov is probably the most logical person whose writings I have read. In fact, if Asimov had a time machine, he would totally own Aristotle in rational argument! The genius of Asimov lies in his ability to explain even the most esoteric concepts in relatively simple ways that make sense. Kaku, for all his intelligence, would have a tough time catching up to Asimov though he tries to fill his shoes.

Dr. Woit,
Actually, I naively believed that the Discovery Channel, History Channel, Animal Planet, etc. all came from the same corporate entity as if one company had a virtual monopoly on educational or informational programming on American Cable. I later learned that Discovery Channel and History Channel are not owned by the same

company.

Discovery Communications, LLC is the sole owner of the Discovery Channel (and Animal Planet, TLC, the Science Channel, Military Channel, Discovery Health, etc.) while the History Channel is part of A&E network, a joint venture of National Broadcasting Corporation, Disney, and the Hearst Corporation. Yes, *that* Hearst, as in William Randolph Hearst, the turn-of-the-20th-century propagandist infamous for his invention of yellow journalism and perhaps inspiring such notorious media figures as Joseph Goebbels and Rupert Murdoch. The heritage of the History Channel might explain its preference for sensationalist programming.

Between Discovery affiliated networks and History Channel, I could almost sense a world of difference. Discovery Channel actually has plenty of great quality shows, especially *Mythbusters*, which is an excellent exposition on how to apply the scientific method. The (ironically titled) History Channel, on the other hand, is much more eager to air programs about the Loch Ness Monster, Nostradamus, exorcism, or UFO. Perhaps it is not surprising that HC would retain Michio Kaku as their go-to physicist. A "serious" scientist willing to give serious attention to spooky topics. But what does asking Kaku to host a show on the Science Channel say about Discovery? Until I get to see the show, I really can not say.

27. **Christine**
    January 15, 2009

    Jake the Snake,

    I started reading Asimov at 11 years old (fiction and non-fiction) and I am certain that he was responsible for my formidable interest in science. Asimov is supreme.

    Best,
    Christine

28. **Marion Delgado**
    January 17, 2009

    On the one hand, he's one of the people that turned me off string theory before I ever heard of Peter Woit.

    On the other hand, I now think that he's simply more up-front about the seemingly fanciful results you can get from the landscape and the latest predominant ideas in string/etc. theory. So paradoxically I am less bothered by him than I was.

Quick Posting, European Edition

January 15, 2009
Categories: Uncategorized

It's semester break and I'm in Paris this week, but I have a few moments to post on some topics that may be of interest:

People here in Europe seem quite normal, but Lubos has just won an award for Best European Blog.

Some details of the proposed new US stimulus package have just been released. Science is one of the main beneficiaries, with the DOE getting:

> $1.9 billion for basic research into the physical sciences including high-energy physics, nuclear physics, and fusion energy sciences and improvements to DOE laboratories and scientific facilities.

This should dramatically change the situation at Fermilab, which has been operating so far this year under a low continuing resolution level of funding. For the immediate future, the funding situation in the US for HEP and science in general looks bright, although this presumably will come to a screeching halt whenever the federal government stops financing everything by printing a trillion dollars a year.

Travis Brooks of SPIRES has produced Top Cites lists for 2008. He has a blog posting about this here, and the list of most heavily cited papers during 2008 is here. For the first time in many years, a new hep-th fad is having visible impact, with Bagger-Lambert at number 37, and three other papers on the same topic in the top 50.

Over at the Edge web-site, there's an interview with Frank Wilczek.

Comments

1. **Tony Smith**
   January 15, 2009

   At Edge, Wilczek said "… The time is right for an assault on the process of aging. A lot of the basic biology is in place. …".

   Would it really be a good thing for every human to live forever (barring accident or homicide)?

   Maybe individual human auto-termination with an upper-limit life span of 120 years or so
   is something that is beneficial for human society ?

   Tony Smith

2. **Cormac O Raifeartaigh**
   January 16, 2009

   I'm a European blogger and I've never heard of this award!
   I had a look at the winners and two points spring to mind

   (i) Although I read his science, I think it's a pity Lubos won – his style of rubbishing those who disagree with him will do little to convince those who doubt the usefulness of weblogs

   (ii) Most of the other winning blogs seem pretty humdrum!

3. **anon.**
   January 16, 2009

   People here in Europe seem quite normal, but Lubos has just won an award for Best European Blog.

   And another climate-change denier won the "Best Science Blog" category. I hope these results just reflect excess zeal and/or cheating from the denialists, and not their actual prevalence in the population….

4. **James**
   January 16, 2009

   I had also not heard of the European Blog award, and groaned when seeing that Motl had won it. However, there was a sigh of relief when the link explained that neither the UK or Ireland were involved (I'm from the UK).

   Auto-termination, or Euthenasia as it's called round these parts, is often up for discussion in Europe. Some recent cases made the news, usually involving countries such as Switzerland which allow it, and questioning whether family members who assist in the travel of the (what's the right word?) 'subject' from countries where it is illegal are criminals. This is a non-trivial question to say the least.

   However, if the human lifespan was unlimited by our own genetics and other biological factors (so ignoring environmental hazards such as asteroid collisions with the Earth, the explosion of the sun, the ultimate demise of the Universe, or dodgy electrics in the shower unit) this would reposition the whole euthenasia issue: should somebody be allowed to end their life becuase they were just too bored after 4000 years of living, and can't stand hearing the same crap songs played over and over again?

5. **Chris**
   January 16, 2009

   It should be Bagger-Lambert.

6. **Peter Woit**
   January 17, 2009

   Chris,

   Thanks, typo fixed.

7. **cormac**
   January 17, 2009

   I should have said – it's not Lubos's writings on climate science I enjoy, far from it…

8. **Shantanu**
   January 17, 2009

   interestingly enough 6 out of the top cited 10 papers are in (observational) astrophysics.

9. **Charles**
   January 20, 2009

   Does any erudite person here entertain me about how to use Anyons to do quantum computing? I read the Frank Wilczek's interview. Does the collective bosonic behavior of electrons at low temperature is theoretically predicted by his QCD theory or just experimentally achieved a technically smaller scale of what called Bose-Einstein condensate but with electrons instead of liquid Helium?
   Any other Fermionic material demonstrates collective bosonic behavior?
   Any other theoretical guy currently plays with idea like Bosonic matter near blackhole hehaves collectively as Fermionic matter?
   When I was young I have fantasized that the Universe would be very dull if everything can be classified as "black or white". This kind of large scale collective behavior sounds more intriguing to me. Thanks.

   Charles Hui

The Worst Jobs in Science: Theoretical Physicist

January 19, 2009
Categories: Uncategorized

After the recent news that being a mathematician is the best job in the US, next month's Popular Science magazine has come out with a list of the worst jobs, not overall, but in the sciences. "Theoretical Physicist" makes the list, right in between "Monkey-Sex Observer" and "Vermin Handler". Here's the text about this:

> For much of the past century, physics was an exciting, wide-ranging exploration. But to be a theoretical physicist today, you pretty much have to stake your career on one incredibly popular but pretty much unprovable notion: string theory. Since the idea that the universe is composed of small vibrating "strings" gained a following in the 1970s, the theory, which in some forms posits 10 dimensions and seeks a unifying "supersymmetry," has captured the theoretical-physics community in the U.S. The easiest way to earn an appointment is to dive head-first into a branch of string theory, which dominates the top programs at Princeton, MIT and other influential institutions. The problem is, we simply have no idea if we're on the right track, because the theory isn't verifiable.
>
> Lee Smolin, a physicist at the Perimeter Institute for Theoretical Physics in Waterloo, Canada, who investigates quantum gravity and string theory, believes that the physics monoculture is stifling. "Science has become too risk-averse, and its progress is being hurt as a result," he says. When CERN's Large Hadron Collider restarts later this year, however, it could end the waiting, helping to confirm parts of string theory — or dash it altogether. If supersymmetric particles called sparticles are bashed into existence: yay! But if the W boson particle does not react as hoped, that damages a central pillar of the theory. Across the U.S., whole careers are boiling down to the chance that a big box comes up with something.

It's true that, for string theorists, a lot is hanging on the question of whether sparticles are found at the LHC. If none are seen, I suspect that will pretty conclusively finish off in most theorist's minds the idea that string theory unification can be connected in any way with observations. The business about string theory and W-bosons is utter nonsense, presumably coming from this.

As mentioned here repeatedly, claims that hiring in particle theory is dominated by string theory are behind the times. String theorists are now yesterday's fad, with terrible job prospects if they don't have a permanent position. Today's fads are LHC phenomenology and cosmology (news from the rumor mill about two new jobs is that UCSB wants "candidates with interests in phenomenological aspects of particle physics and related areas of astrophysics and cosmology", Rutgers wants "a focus on LHC physics, broadly conceived.") String theory is on its way out in American universities it seems, but the long-standing pattern of fad-driven hiring isn't. Which is one thing that makes the idea of trying for a career in theoretical physics these days about as appealing to many smart young people as the idea of going into the vermin handling business…

Comments

1. **Bourgeois Nerd**
   January 19, 2009

   This may be a stupid question, but from a non-academic who reads this blog, I've been dying to ask: what exactly IS a "phenomenologist"?

2. **Peter Woit**
   January 19, 2009

   Bourgeois Nerd,

   Phenomenology: theoretical work closely tied to experiment, e.g. computation from a given model of specific predictions about what a certain experiment will see.

   contrast to so-called "formal" theory, which is about trying to better understand theoretical ideas: investigation of toy models, unphysical examples, development of new calculational techniques, etc.

3. **steve newman**
   January 20, 2009

   String Theory R.I.P.
   Will Dark Energy and Dark Matter and Inflation be next?
   Why do dead ends have such a long life?
   Hoping for a re-birth of physics.

4. **csrster**
   January 20, 2009

   Humorous stuff, but obviously not quite accurate if you happen to be, for example, a theoretical condensed-matter physicist.

5. **chris**
   January 20, 2009

   @steve newman

   i think you will hear a lot about dark matter/energy in the years to come. main reason is that they are observable.

   "Why do dead ends have such a long life?"
   Some famous guy once said (i don't remember who) that obsolete ideas only die with their supporters.

6. **Cormac O Raifeartaigh**
   January 20, 2009

   Hmm… the implication of the excerpt is that if SUSY is seen at LHC, this is a victory for ST – this is not necessarily true, as you know.

   Conversely, Peter's comments seems to imply that if SUSY is not seen at LHC, that rules out ST – but that doesn't work either, as only low -energy SUSY can been ruled out at the LHC!!

7. **mr**
   January 20, 2009

@chris

“Some famous guy once said (i don’t remember who) that obsolete ideas only die with their supporters.”

I think it was Max Planck, wasn’t he? (Not sure about the exact phrasing, though)

regards.

8. **TheorPhys**
January 20, 2009

Theoretical physics is for sure beautiful, but the sad story is that (unless the LHC finds something radically new) we are almost at a saturation point in the knowledge that man can have about fundamental laws of Nature. Maybe I am too pessimistic, but at least in the last 30 years this seems to be the trend.

But the real problem is that the education system does not easily provide young people alternative careers and easy ways to transfer from one subject to another one…once they get stuck in some dead end.
After all physics is a very broad field and this would be definitely possible.
Once upon a time science and knowledge were broad concepts. If you were a scholar you could study very different topics, with reasonable freedom.
If you think of Enrico Fermi he has been working in almost all fields of physics, as a theoretician and an experimentalist too, for example.
Today even a young Fermi could be probably stuck in some dead end, or following some fashion just to get a job.

The reality is that nowadays competition and excessive specialization have introduced also in science an industrial logic….and this kills scientific creativity and flexibility and even the pleasure of doing science.

9. **Peter Woit**
January 20, 2009

Cormac,

Logically, low energy supersymmetry is pretty much independent of string theory. But, sociologically, it’s a very different story, and my comment was more a sociological one.

TheorPhys,

The fact that it is hard for theorists to change fields is a huge problem. The effects of it may be worse for senior people than for junior people, with many senior people getting stuck in a failed dead-end research program. This leads to a seriously damaged field, as it affects the training and job prospects for young people. Anything that makes it easier for people to move on to something else when the field they are trained in stops being fruitful would be a good thing. But the danger of faddishness is that everyone will just move to the same new hot topic….

10. **TheorPhys**
January 20, 2009

Dear Peter,
I completely agree with you.

However, I would say faddishness is not such a big danger. If somebody is having the feeling of wasting time and energies and being in the wrong track , I can understand insisting just for a few years, but not too much. If instead, you feel ok in some field you would not even think of changing. After all physics should be driven by passion, and I have seen many theorists (even very bright people) being frustrated by the wild speculations that they have to do nowadays.

I think everybody who is in charge of science policy and in the universities should be aware of this problem and think about solutions of this issue.
Otherwise the only alternative for a theorist would just be finance, which is a bit sad.

11. **terry**
January 20, 2009

this is so ridiculous it’s not even wrong. the world of physics is huge compared to the small number of self-promoting, self-indulgent particle “theorists.” most theorists are happily solving problems in condensed matter, nuclear, fluid mechanics, lasers …, real problems with real applications and real jobs. the fact that some research universities have been parasitized by string theorists is only really relevant to a few competing clans.

sadly, this type of coverage reflects badly on all physics, not just string theory. if you can do it, and you want to do it, there are few jobs that are better than being a physicist–even if you have to settle for being a theorist.

12. **TheorPhys**
January 20, 2009

Dear Terry, I disagree with you.
Not all fields in physics have real problems with real applications and real jobs.

There are many fields, apart from string theory, which do not meet these requirements. Mainly those related to theory, to mathematical physics, to experiments in fundamental physics (do experimental particle physics or experiments in astrophysics, gravitational waves… have always real applications?).

13. **David Katz**
January 20, 2009

This is what I’m hearing from fellow physicists in the UK too. A recent chat with a friend of mine lead to string theory being described as “going through a period of navel gazing”. Hopefully some of that wonderful string theory money can now be put towards condensed matter, or fields of particle physics such as lattice QCD that might actually stand a chance of telling us something about physics, rather than sometimes beautiful mathematical models that stand no chance of ever being proved.

14. **Bee**
January 20, 2009

Anybody looking for an LHC phenomenologist going cosmologist, here is one, and in addition I am looking for a job.

So, Lee has now turned from a researcher into an “investigator”. That’s an interesting twist.

15. **D R Lunsford**
January 20, 2009

BN, this is a good question and shows you are paying attention.

Phenomenology is not equivalent to "experimental" – for example, the original theory of electron spin proposed by Pauli was a phenomenological theory. Only when Dirac showed how it came from a basic idea could spin be said to have a firm theoretical basis. Essentially, phenomenological theories have terms in them that are only heuristically justified – this does not make them bad theories, but progress usually comes from giving a theoretical basis to what was formerly phenomenology.

-drl

16. **Cormac O Raifeartaigh**
January 20, 2009

Peter: Yes, apologies, I see you what you mean.
Bee: What's this about a new job? Wanna come to Ireland? Have a look at the Dublin Institute for Advanced Studies..

17. **mathematician**
January 20, 2009

As a mathematician, I sometimes get physics envy.

And physicists sometimes get mathematics envy.

Of course, string theorists get both.

18. **Aaron Bergman**
January 20, 2009

These days, string theorists get job envy.

19. **Ari Heikkinen**
January 20, 2009

Just tell young people to get an engineering degree, that way they'll be solving practical problems in the real world. Or even better, tell them to acquire good software engineering skills and they won't ever be unemployed for long.

20. **JC**
January 20, 2009

Ari Heikkinen,

Engineering jobs vary significantly between different areas. It can also be trend driven to some extent too, just like physics.

Also many engineering majors don't even get the opportunity to ever work as an engineer. No big surprise as to why many engineering majors ended up in computer programming jobs over the last 25-30 years. Though with that being said, outsourcing has been eating away at salaries and jobs for many years.

21. **banerjee**
January 20, 2009

Many tenure-track positions in engineering are going to physics graduates these days. If a string theorist (or any other type of physics theorist) feels the need to be in academia can can't get a physics position, one path is to get some post-doctoral experience in a good engineering department and then switch to that field.

We in engineering are always looking for people with good skills and ideas in subjects that are not well understood by engineers.

22. **Troublemaker**
January 20, 2009

Humorous stuff, but obviously not quite accurate if you happen to be, for example, a theoretical condensed-matter physicist.

Well, you have to bear in mind that on this blog, and every other string theory or anti-string theory blog, "physics" = "high-energy theory." I like this blog and I like Peter, but he, his friends, and his enemies are all very high-energy-theory-centric in their conception of what it means to do physics.

23. **Shantanu**
January 20, 2009

Peter or others, anyone know about the job prospects for those doing neutrino physics or neutrino phenomenology.
Also are there many papers connecting string theory
with neutrino physics for which we have lots of data and will soon have more?

24. **bob**
January 20, 2009

what fraction of physicists are theorists? what fraction of physicists are string theorists?

i don't think string theorist is a bad job, and even if so, i don't think "theoretical physicist" should be branded a bad job even if string theorist is a bad job.

25. **Cormac O Raifeartaigh**
January 21, 2009

Terry: "the world of physics is huge compared to the small number of self-promoting, self-indulgent particle "theorists." Most theorists are happily solving problems in condensed matter, nuclear, fluid mechanics, lasers …, real problems with real applications and real jobs"

I don't think that's fair. First of all, particle physics is a fundamental field that bears close examination, in the sense that everything is made up of atoms, but atoms are not made up of semiconductors (say). Secondly, it turns out to be a very difficult field, accessible by only the very best students – the maths of gauge theory is way beyond most physicists, and therefore commands a certain respect. Thirdly, how do you know what the future applications will be? There have been many stunning applications of discoveries in particle physics, from nuclear power to the use of accelerators in medicine

If the public show a disproportionate interest in subjects like quarks or strings, I think it's for the same reason that they find cosmology so interesting… human curiosity concerning 'fundamental' questions..

26. **Peter Woit**
January 21, 2009

Shantanu,

String theory has nothing to say about neutrino physics.

As for job prospects for theorists working on neutrinos, I think it's a small group of people and a small number of possible jobs. Prospects should be better than if you're a string theorist, but beyond that I don't know.

27. **somebody**
January 21, 2009

A few pointers –

1. Jobs are overall bad in particle theory, and if anyone here is implying that phenomenologists have a cakewalk, they are delusional.

2. About the increased focus on phenomenology (much of it string-inspired, incidentally) being a "fad". It makes perfect sense to me that on the eve of the LHC, people care more about the TeV scale than the Planck scale. Choosing the word "fad" to describe this, is again, another way in which a perfectly valid SCIENTIFIC judgement is being (consciously?) misinterpreted as a sociological issue.

3. A significant fraction of the current HEP faculty in big universities is composed of string theorists and string-inspired people. To claim that the focus in recent years on phenomenology is an indication that they are done with strings, is beyond ridiculous. As I said, there is a very real reason why we all should be concerned about phenomenology/cosmology: the LHC and WMAP.

4. Since the departments that hire phenomenologists contain a significant fraction of senior string theorists, can we at least have a consensus now that not all senior string theorists are blinded ideologues who try to "suppress" everything else?

5. Cormac's previous message was excellent.

28. **Thomas Larsson**
January 21, 2009

Secondly, it turns out to be a very difficult field, accessible by only the very best students – the maths of gauge theory is way beyond most physicists, and therefore commands a certain respect.

Come on, gauge theory is not that hard. And remember that it was a squalid state physicist who invented the Higgs mechanism.

Thirdly, how do you know what the future applications will be?

This can be answered be a simple energy argument. The original fields to which QM was applied – atoms and molecules, solid state, chemistry – typically deal with energy scales of order eV, i.e. scales found on earth, and the practical applications at terrestial energy scales are too numerous to list. Nuclear physics is at the keV scale, i.e. typical energies of the sun. Solar scales have far fewer applications than terrestial scales, but since the sun is rather close to us there are still a few important ones – nuclear power and weapons, NMR, and maybe something more. In contrast, 45 years after the quark model, HEP has very few practical applications. Off the top of my head, the only one that I can name is the use of synchroton radiation in medicine (muon catalyzed fusion didn't work out, did it?), and this is very low high energy – MeV physics rather than GeV or TeV physics. The energy scales that will be probed at the LHC are typical of supernovae and the big bang, and will remain irrelevant for terrestial phenomena in the forseeable future.

Any good physicist who makes such an energy considerations will realize that applications of GeV physics will remain in the very distant future. There might of course be spin-off effects, like CERN inventing the WWW, space science inventing cool materials, and military science inventing the internet. But such spin-offs do not have anything to do with the core science.

There have been many stunning applications of discoveries in particle physics, from nuclear power to the use of accelerators in medicine

Since when did particle physicists adopt nuclear physics? I remember a rather controversial talk 20 years ago, when a local string guru (let's just call him "Veneziano's advisor") argued that nuclear physics was unmodern and its funding should be redistributed to more modern physics.

Medical applications remain at the MeV scale.

29. **Peter Woit**
January 21, 2009

somebody,

You really like to argue against statements that no one has made…

No one claims that "phenomenologists have a cakewalk", just that there are more jobs out there per person for phenomenologists than for others kinds of particle theorist.

No one claims that string theorists "are done with strings". They're not. I suppose many of them will keep working on string theory until retirement, no matter what happens.

No one claims that "all senior string theorists are blinded ideologues who try to "suppress" everything else." Sure, many of them are now the ones agreeing to try to hire phenomenologists and cosmologists. Besides their openness to difference though, another consideration might be that other people in their department (who have a lot to say about tenure-track hiring) would not put up with hiring another string theorist, for various reasons, including perception that a phenomenologist is more likely to get grant funding these days.

WMAP data came out 6 years ago, the LHC was initially supposed to be producing data a couple years ago. These two experiments have a lot to do with the general trend that started quite a few years ago in the direction of cosmology/phenomenology, but nothing much to do with the dramatic acceleration of this trend (to proportions that I think make accurate the description of “faddish”) during the last couple years.

30. **Bee**
January 21, 2009

Hi Cormac,

Thanks. I checked the website, it says deadline was in December. Too bad, I would have sent my docs had I known. Best,

B.

31. **Cormac O Raifeartaigh**
January 22, 2009

Thomas:
Re ‘Gauge theory is not that hard’, rather than get in too a silly argument about what constitutes ‘hard’, allow me to point out that in physics departments the world over, only the very best and mathematically able students are pointed in the direction of particle physics, and there are good reasons for this..
Re applications, it is the spin-off effects that I’m talking about. It’s always a mistake to consider only the direct applications of new science as it is in the getting of the results that the technical breakthroughs occur..
Re nuclear power, how quickly we all forget the history of particle physics. The splitting of the nucleus using the Walton linear accelerator was the first hint of a new source of energy, as you know. More recently, I believe Carlo Rubbia published an innovative design for a modern reactor based on work he did at CERN…

32. **Sumar Ongi**
January 22, 2009

>HEP has very few practical applications.

To add one more application to “terrestrial” phenomena (not necessarily very practical, though): the theory of weak decays in nuclear physics. And, since about 10 years ago, the theory of few-body nuclear interactions has been based on QCD.

>Since when did particle physicists adopt nuclear physics?

There was an article in Physics Today some months ago about the new theoretical approaches to nucleon-nucleon interactions and the impact they have had on *experimental* nuclear physics. Very interesting stuff, I’m sorry I can’t give the exact reference now.

That whole business started with a couple of papers by Weinberg from the early 90s, showing how low-energy QCD effective theories could be applied to the nucleon-nucleon problem. The field has matured a lot since then. Besides, since some years ago, lattice computations are also being applied to those systems.

In fact, the field of low-energy hadron physics has been more or less merged with high-energy (or “relativistic”) nuclear physics since a couple of decades ago. Many particle physicists in those areas routinely publish in nuclear physics journals like PRC, NPA, EPA, etc.

On top of that, AdS/CFT – inspired models are also being applied to some nuclear physics problems, though I’m not sure how much has been done in that direction. Finally, let’s not forget that string theorists have adopted nuclear/quark matter as their favorite application of N=4 SQCD…

33. **Thomas Larsson**
January 22, 2009

Indirect spin-off effects can just as well be used to argue for increased military spending – after all, military science led gave us arpanet turned internet. This is very different from direct applications of low-energy physics such as transistors, lasers or nuclear power, where it is the physics itself that has applications.

Fair enough. I don’t see a problem with HEP (at least high energy HEP) lacking practical applications; knowledge can be valuable without increasing GNP. However, I see a problem with the statement that we cannot know whether HEP will lead to practical applications, when it hasn’t happened for 45 years and there are simple arguments why this has to be the case.

34. **Dan M**
January 22, 2009

>allow me to point out that in physics departments the world
>over, only the very best and mathematically able students are
>pointed in the direction of particle physics, and there are good >reasons for this..

This statement is false. The very best and mathematically able students of whom I am aware are directed to choose their own fields, since they are the ones who are presumably most qualified to do so. In my experience, they usually choose something other than particle physics. And there are good reasons for this…

35. **Cormac O Raifeartaigh**
January 22, 2009

“In my experience”
It would be interesting to know your position and experience. (Mine are on the link). Here in Europe, we generally try to give students the best advice, cognisant of individual inclination and talent, but also of which fields tend to be difficult to make an impact in..perhaps a different approach is employed in the US

36. **somebody**
January 22, 2009

Peter, the primary effect contributing to the recent interest in phenomenology is LHC. You suppressed that in your original post, but after I pointed it out, you now claim that you were talking about a secondary effect. You also claim that I “really like to argue against statements that no one has made.” I don’t have the power of hindsight that you have, Peter. If you keep qualifying your previous statements when a new point is made, I will always be chasing a moving target.

Sorry about the sarcasm, but the last paragraph of your original post was the worst propaganda-driven drivel I have read here in a while. It is a pity because there are so many things about the sociology and the public relations aspects of string theory community which I find appalling …

Unparticles would qualify for a “fad”, but not phenomenology in general.

The whole issue you raise is pretty much vacuous, because it is only you who see such easy boundaries between phenomenology and fundamental theory. Considering the fact that Seiberg and Vafa are both working on ways to connect with TeV scale physics these days, I would say that your straightjackets are designed for mediocrity.

37. **Ari Heikkinen**
January 23, 2009

JC: "Though with that being said, outsourcing has been eating away at salaries and jobs for many years."

Here in Europe firms don't generally outsource their best engineers. Is the situation in the US really that bad that they oursource even their best employees to save money?

38. **JC**
January 23, 2009

Ari Heikkinen,

It depends on the particular firm, and who is calling the shots in these firms. In firms run by management consisting of mainly folks who only have an accounting and/or finance background, they tend to be more into the outsourcing thing.

39. **Eugene Stefanovich**
January 23, 2009

Regarding outsourcing… (for those contemplating trading a "high-stress" job in theoretical physics for a "lucrative" employment in industry)

My company (USA, software for microelectronics industry) hired quite a few engineers in Shanghai last year. Mostly for quality assurance and for programming of peripheral features. When (if) economics goes south this year, the management will have a choice: either layoff their core developers in the US, or cut fat overseas. Although 1 engineer in the US costs as much as 3-5 engineers in China, something tells me that my bosses would prefer the latter option. (In similar circumstances a few years back, they decided to shut down a division in France, rather than touch their US employees). So, it may sound paradoxical, but I think that outsourcing, actually, increased my job security.

40. **mike**
January 24, 2009

The vermin handling business sounds like a safer profession. Although there are some vermin that can bite (ouch!) – the fangs that some string theorists exhibit towards scientists of differing opinions makes the theoretical physics field much more dangerous!

41. **Doug Natelson**
January 25, 2009

Cormac – I would agree that many of the most mathematically skilled students tend toward theory, often high energy theory. Still, I think it's borderline pejorative to assert that these students are, because of their mathematical sophistication, "the best". Your taste and bias are influencing your choice of words. Frankly, the implication that high energy theorists are inherently superior to other physicists (let alone other scientists) is an example of what many perceive as arrogance.

42. **Cormac O Raifeartaigh**
January 26, 2009

That is not what I said and I am not a particle physicist. What I actually said is that here 'only the best students the best and most mathematically able students are encouraged in the direction of particle physics' – not just because it is a difficult field but because it is very difficult to make an impact in this field as it has attracted a great many physicists over the years (this is also true experimentally).
I absolutely agree that there are many other important and rewarding fields – that's the point!

43. **Cormac O Raifeartaigh**
January 26, 2009

Re main topic of this thread, I keep meaning to say there is a very nice interview with Peter Higgs in the September issue of New Scientist at
http://www.newscientist.com/article/mg19926732.100-the-man-behind-the-god-particle.html

On the difficulty of the theory, he states "…there was a problem for me when the bandwagon started to roll in 1972. Because I'd written an influential paper, people tended to assume I understood far more about the subsequent theory than I did and I found it increasingly hard to keep up".
He describes then taking an interest in supersymmetry, but again found that tough going too. "I realised the only people who were producing anything that was worth doing was the generation that had just got their PhDs. After some years, I gave up"..
Of course, Higgs is a famously modest man, but it makes you think all the same..

44. **Shantanu**
January 26, 2009

Peter, any interesting or newsworthy from last week's PI conference on black holes and quantum physics http://www.pirsa.org/C09002 and from the string theory related talks in this meeting?
Getting the correct value of black hole entropy and information loss paradox
is supposed to be a triumph of string theory.

45. **King Ray**
January 27, 2009

Peter, what do you think of this?

http://www.foxnews.com/story/0,2933,483477,00.html

46. **anon.**
January 27, 2009

"Theoretical Physicist" makes the list, right in between "Monkey-Sex Observer" and "Vermin Handler".

That's because theoretical physics has lost glamour. It used to be dominated by interesting personalities and rapid progress. Now things have slowed down and you have the boring geniuses left.

“Men of genius are often dull and inert in society; as the blazing meteor, when it descends to earth, is only a stone.” – Henry Wadsworth Longfellow

47. **Cormac O Raifeartaigh**
January 28, 2009

King Ray: I think the only sentence we need bother with is:

“We conclude that … the growth of black holes to catastrophic size does not seem possible”.
What a clear example of a journalist paying no attention to what the scientists actually said. Hopefully, Obama will declare a fatwah on FOX news…

48. **Bruce**
February 4, 2009

Peter,

In your post you point out that the claim that hiring in particle physics is dominated by string theory is behind the times. I don’t dispute this, but I wonder if you can quantify this statement or is it simply obvious to folks like you who wander widely in the field?

49. **Peter Woit**
February 4, 2009

Bruce,

Up to the minute data on particle theory hiring into tenure-track jobs is available here:

http://particle.physics.ucdavis.edu/rumor/doku.php

You’ll find that job descriptions this year often explicitly ask for LHC phenomenologists or cosmologists, whereas a few years ago string theory was often specifically mentioned. You’ll also find very few string theorists on the short lists for jobs this year.

I think if you talk to any young string theorist who is on the job market, you’ll get more confirmation of the problem

50. **Just Asking**
February 9, 2009

So Regge and Veneziano weren’t doing phenomenology? And we haven’t all been waiting with baited breath for almost 30 years now to probe the TeV scale?

You know, Boltzmann committed suicide shortly after his contemporaries reclassified him from “physicist” to “applied mathematician.” His theory did, after all, rely on the reality of unobserved “atoms” and “molecules.”

Hey, I have a great idea, let’s further encumber the minds of young people in an already anti-intellectual cultural climate with a pop science “debate” which reduces untold billions of man-hours of careful thought into the credo of some “parasitic” cult.

That will surely prevent the Boltzmann’s of today from hanging themselves while on Summer vacation.

51. **woit**
February 9, 2009

Just Asking,

I don’t think the Boltzmanns of today are likely to consider hanging themselves because unification via a 10d superstring doesn’t seem to work. They might just work on something else.

As for who is doing anti-intellectual pop science, that’s something I tried to avoid in my book and here. It would be a good idea if those promoting string theory considered doing the same…

52. **Robert**
March 10, 2009

I just received my Bachelor of Science in Physics and have been reading about string theory. It seems to me that all the talk about string theory being unprovable or unverifiable is overstated. Many theories in physics are based upon indirect evidence. For example, no one has ever seen a proton or neutron or electron, yet much of 20th century science is based upon the assumption that these particles exist.

I don’t understand string theory fully- yet, but I have read enough to know that if a theory explains events in the universe accurately and the math works it is worth pursuing.

Personally I find it pretentious that some physicists have the nerve to tell other physicists what they should be studying. If string theorists want to study string theory what is wrong with that as long as they follow the scientific method?

53. **Peter Woit**
March 11, 2009

Robert,

Of course indirect evidence is acceptable in science. The problem is that there is no such indirect evidence now, nor is there any plausible proposal for how to get such indirect evidence.

Scientists all the time argue for and express scientific opinions about which scientific ideas are promising and showing success, which ones are not working and why. Evaluating what works and what doesn’t is an important part of what scientists do. This is different than “having the nerve to tell other physicists what they should be studying”. Other physicists can make up their own mind about what to study based on the arguments they hear.

If you are interested in string theory, I suggest you read and think about the arguments being made both by enthusiasts and skeptics, and make up your own mind. I don’t see the point of complaining that both sides of the argument are available to you.

54. **Puttputt**

March 20, 2009

It can be solved by outsourcing theoretical physics. PARTY ON AMERICA!

Lots of Unrelated Topics

January 27, 2009
Categories: Uncategorized

I'm trying to finish writing up something about equivariant cohomology for the BRST project, slowed down by realizing there was something interesting about this that I didn't understand. Soon that should be sorted out….

In the meantime, here are various other things that might be of interest:

The El Naschie/Elsevier saga continues, latest here.

There's a new book out from Cambridge University Press entitled On Space and Time, which has chapters from different authors stretching from solid physics to theology, with lots of quantum gravity in between. The editor, Shahn Majid, is blogging here, on a site run by Cambridge.

Evidence for time travel has appeared in the British newspaper The Independent, which recently published an editorial by Mike Duff about string theory that appears to have come through a worm-hole connected to about 13 years ago.

There really are good reasons that theorists who insist on devoting their lives to absurdly speculative models of extra dimensions which have nothing to recommend them other than not being obviously inconsistent should stop promoting these things in the press. One of these reasons is that doing this tends to lead to articles like this one on Fox News.

British theorists at Durham are getting some new funding.

American scientists are lobbying for their piece of the stimulus pie that should be cooked and ready to serve within the next couple weeks. An editorial by David Gross and Eric Kandel is here, a letter from 49 Nobelists here. The latest news indicates that the NSF and DOE are still in line for massive short-term budget increases.

In France, President Sarkozy argued in a speech that French scientific research needs to be reformed, with the economic crisis that originated in the U.S. a good opportunity for the French to modernize and do things more the way they are done over here. Many French scientists are reacting with "shame and anger", and planning on joining a general one-day strike this Thursday. I know little about the problems and virtues of the French research system, but perhaps scientists there should tell Sarkozy it's a deal if he is willing to put up the sorts of cash the current U.S. administration is discussing.

There have been a few physics arXiv preprints that seemed worth a mention recently, although all of them have been discussed extensively by Lubos, who seems to be saner these days:

- Smolin and Ellis argue here that if you take the landscape seriously you end up predicting a negative cosmological constant, falsifying the idea. This is along the same lines as other such wrong predictions (e.g. proton decay), and since their existence hasn't slowed down the spread of landscape ideology, I doubt one more will do the trick.
- Several authors here find 10^{668} as a lower bound when calculating the number of possible vacua in a certain class that might give the standard model at low energies. This is high enough to make getting any predictions (other than the wrong ones…) impossible, but along the same lines as previous estimates which didn't slow down the landscapeologists. No reason to think this one will either.
- Petr Horava has an interesting proposal for a new candidate sort of quantum gravity here, one with Lorentz breaking at short distances. I don't know if this is any more testable than other such proposals. Even though his proposal has absolutely nothing to do with string theory, it's rather amusing that the author finds it necessary to somehow invoke string theory:

> Given this richness of string theory, it might even be logical to adopt the perspective in which string theory is not a candidate for a unique theory of the universe, but represents instead a natural extension and logical completion of quantum field theory. In this picture, string theory would be viewed – just as quantum field theory – as a powerful technological framework, and not as a single theory.
> If string theory is such an apparently vast structure, it seems natural to ask whether quantum gravitational phenomena in 3 + 1 spacetime dimensions can be studied in a self-contained manner in a "smaller" framework. A useful example of such a phenomenon is given by Yang-Mills gauge theories in 3 + 1 dimensions. While string theory is clearly a powerful technique for studying properties of Yang-Mills theories, their embedding into string theory is not required for their completeness: In 3 + 1 dimensions, they are UV complete in the framework of quantum field theory.
> In analogy with Yang-Mills, we are motivated to look for a "small" theory of quantum gravity in 3+1 dimensions, decoupled from strings.

So, the idea seems to be that now string theory is a "logical completion" of qft, although not needed to describe any of the forces we know about.

For two new survey talks at UCSB by Edward Frenkel about geometric Langlands, see here. He also gave an interesting talk there as part of the KITP string theory program, on recent work (summarized here) that has relations to both geometric Langlands and to the pure spinor formalism.

Update: One more. The Chronicle of Higher Education has an article on SCOAP^3, the plan to make the entire high physics literature open access by coming up with \$14 million/year to pay off the publishers. I don't really see this. The idea seems to be that the money is needed to get peer review, and the size of the literature is about 10,000 papers/year. So, cutting out the publishers, referees could be paid \$1,400/paper to do peer review. This might dramatically increase the quality of refereeing, or at least the take-home pay of many physicists. Some quotes:

> But Mr. Mele says journals still play a crucial role in the professional life of scientists, even though readership has declined. "We do not buy journals to read them, we buy journals to support them," he said. "They do something crucial, which is peer review."
>
> Without journals, he asks, how would colleges evaluate the work of scientists to know whom to hire or whom to promote? And how would other scientists know which of the thousands of preprints contain the most important findings?
>
> "What we are really paying for here is for a service of peer review," he said.

but here's the problem, at least in the U.S.…

> The librarians praised the goals of the project, but some asked whether it was sustainable. After all, if the journals make their contents free online, why should college libraries use their shrinking resources to pay for them?
>
> Some librarians at public institutions say they cannot participate even if they want to. "Most states require that public funds allocated for purchasing have to be used to actually purchase something," said Dennis Dillon, associate director for research services at the University of Texas at Austin. That is certainly the case in Texas, he said. "They can't be used to pay for something that everyone already has for free."

Update: For more LHC-related hysteria generated by publicity-hungry academics, see this. It originates with a group at the Institute for the Future of Humanity, last seen promoting the idea that we live in a simulation.

Comments

1. **anon.**
   January 28, 2009

   '… by coming up with $14 million/year to pay off the publishers. I don't really see this. The idea seems to be that the money is needed to get peer review, and the size of the literature is about 10,000 papers/year. So, cutting out the publishers, referees could be paid $14,000/paper to do peer review. This might dramatically increase the quality of refereeing, or at least the take-home pay of many physicists.'

   $14 million divided into 10,000 is $1,400, not $14,000. It will help those who are peer-reviewers, but will it help those trying to publish unfashionable ideas? I'd imagine that putting such a price tag on peer-review will make for a lot of the 'I'll review your paper if you review mine' culture, great for the mainstream but not so good for backwaters.

2. **Eugene Stefanovich**
   January 28, 2009

   "journals … do something crucial, which is peer review"

   Wrong. Scientists peer review each other. The contribution of journals is limited mostly to a technical work of sending e-mails back and forth. This job can be easily performed by a not-so-sophisticated web application. Paying $ millions to publishers is just beyond ridiculous.

3. **Peter Morgan**
   January 28, 2009

   Journals also provide an archive of their papers. If my library has a subscription, I go to the journal web-site for the paper, instead of writing to the author for an offprint. My library doesn't have an on-line archive of their own, whereas they used to have their own carefully maintained paper copy archive.

   Journal editors also act as doorkeepers, a task in which they are scrupulously supervised. Editors are expected not to pass papers to peer review if they are obviously substandard. I've seen at least one nasty blast at an editor for sending one of my early papers to a referee. You have to pay for your doorkeeper.

   Journals also apply minimal copy editing and standardization of format. Over time, new technical developments sometimes require changes to such policies, a process that requires expertise, time, and resources. The move to the web was a major such transformation of the process, which was handled well or badly by different journals, but it certainly cost money.

   There are numerous administrative tasks associated with any large-scale organization, which do not go away by the application of fine sentiment. It's not just peer review that is provided by journals, it's organization of peer review, knowing who can be sent any given new paper, knowing how to deal with new ideas well in the journal environment, etc. There is still the printing process to take care of, even if many fewer copies are printed and offprints have become almost a memory.

   I presume that a journal editor could list a significant number of other tasks that scientists themselves would be loath to spend time on. I am not associated with journals in any way, and some publishers act corporately in a way that encourages criticism, but the idea that journals just provide peer review is unthinking.

4. **Peter Woit**
   January 28, 2009

   anon.

   Oops… I suppose I should not write some of these things quite so fast. Fixed.

5. **Peter Woit**
   January 28, 2009

   Peter Morgan,

   Sure, the peer review process needs not just referees but editors to choose them (or decide a paper is not worth bothering them with).

   The arXiv already handles the archiving function well, and it's becoming debatable whether printing up copies of papers is a useful function.

6. **Pawl**
   January 28, 2009

   A few comments on the Fox News/LHC business, where, bizarrely, they report on an arxiv preprint concerning the possible lifetimes of micro black holes.

   I think it's a little unfair to say the large-extra-dimension proposals (which generated all the fuss by predicting micro black holes) have nothing positive to recommend them. They are attempts to solve a deep problem (the hierarchy problem), and that problem could have some really radical solution. (True, beyond that, they have a lot of negative baggage.)

   I do agree however that much of the problem comes from the fact that these proposals were discussed without anything like the appropriate caveats about how unlikely they are to be correct. It's not merely that they involve speculation, it's that they are so preliminary it's hard to say much about them. (There is no real theory yet, only some ideas and models. The ideas are so radical that they almost certainly cannot be reconciled with known physics.)

7. **Peter Woit**
   January 28, 2009

   Pawl,

   The original problem here came about because of the hype from theorists about the possibility of the LHC producing black holes, something absurdly unlikely, but you wouldn't know this from the sales job done by some theorists. Given the recent hype, I don't think it's especially bizarre that Fox News would pick up on a paper by three theorists about "the Possibility of Catastrophic Black Hole Growth in the Warped Brane-World Scenario at the LHC". I do think it's a bit bizarre that any theorists would post a paper with this title on the arXiv if they didn't want to get on Fox News.

8. **MathPhys**
   January 28, 2009

Now I'm starting to really like El Naschie. The man has guts.

9. **Shantanu**
February 2, 2009

Peter, I see there are 2 back-to-back colloquia on string theory .
Could you give us a summary if there was anything new in it?
Also anything interesting in the KITP program on string theory.

10. **Peter Woit**
February 2, 2009

Shantanu,

I'm embarrassed to admit that I was planning on attending the Gubser colloquium last week, but somehow got busy with something else that day and forgot about it. I'll probably try and go to Vafa's to see what he has to say.

So far, of the KITP talks I found the Nekrasov and Frenkel ones interesting, but haven't found time to write about them. I see Polchinski is giving a general talk about string theory today…

11. **Jason**
February 5, 2009

Anyone know what happened to John Baez's

http://golem.ph.utexas.edu/category/2008/11/the_case_of_m_s_el_naschie.html

? Did he take it down due to legal threats from El Naschie? I really want to read it. If anyone has it cached, please send it to hasten dot jason at gmail dot com. Thanks!

12. **woit**
February 5, 2009

Jason,

That's weird, I don't know what the story behind that is. The El Naschie story just gets odder and odder….

13. **Shantanu**
February 7, 2009

Peter, thanks for the info and I look forward to a description of
Vafa's talk. Also did you find anything hyped/intereresting or otherwise
in the string theory talks in the PI conference on black holes and
quantum physics?
Thanks

14. **Peter Woit**
February 7, 2009

Shantanu,

Sorry, but my interest in many quantum gravity issues is just not very great, and the black hole information loss business is something I'll happily leave to other people to debate. There are too many interesting things out there to learn about and think about, so one has to make choices, and that's one of mine…

CERN: The View From Inside

January 27, 2009
Categories: Experimental HEP News

Tommaso Dorigo has a new post up on Information control from CERN, where he discusses a Physics World interview by Matthew Chalmers of the head of communications at CERN, James Gillies.

Gillies addresses what CERN sees as a problem: information coming out first in blogs rather than from the CERN director through official press releases. One aspect of this is the release of information about experimental results, and Tommaso discusses this question on his blog. Unfortunately, I think CERN and the LHC are still quite a ways away from having any experimental results that need to be protected. For the rest of this year, the LHC will be getting a lot of attention from high energy physicists, but what they will be interested in is the question of how the machine is progressing towards the goal of colliding beams at a useful luminosity. For most of the history of the project, CERN's information policy was remarkably open: the slides from presentations made before the technical committees guiding the project were posted in locations that, while not advertised, were easy to find and did not require a password to access. Anyone with a serious interest could follow along and get first-hand technically accurate information about what was happening.

Things changed rather dramatically after the accident last September 19th. Publicly accessible logbooks were edited to remove information, and public access to the websites of the technical committees was shut off:

> **Who ordered links to photos and some presentations to be password protected after they appeared on blogs?**
>
> wanted the CERN community to receive the news from him before it was made more widely available, so access to slides was temporarily restricted. People just hadn't realized how much in the spotlight we are now.

Gillies doesn't address the issue of why these websites have now been restricted, a policy that appears to be permanent and go beyond a "temporary" restriction. According to Chalmers:

> CERN's new director general told staff on 12 January, that from now on people would hear about events first from him, not the press.

This kind of tight control of information about what is happening at the LHC seems to me to be a misguided policy. The best and most timely source of information for CERN staff about the LHC should be first-hand information from the engineers and physicists working on the project, not whatever has made its way up the chain of command and then been laundered for public consumption. Shutting off access by physicists to accurate technical information and making the DG the only source of news about what has happened at the LHC is likely to just encourage unchecked rumor.

Next week at Chamonix there will be an LHC Performance Workshop, and the slides are supposed to be publicly available here as the presenters post them over the next few days. These slides should give an accurate picture of where the project is and what a realistic proposed schedule for the rest of the year would look like. According to the Physics World interview, CERN's plan is that "a realistic schedule will be announced" after the Chamonix workshop. Of course, by then, many people will have already have a good idea about what this schedule will be, that is, if the slides are not password-protected….

Comments

1. **rumors about chamonix**
   January 28, 2009

   Well, one additional piece of news about CERN restrictive policies is that CMS is closing the outside access to their twiki page.

   Since this is going to happen on February 2nd, there is still time to dump the whole site on some disk and make a mirror. Of course, that will not allow access to future additions to the page, but still…

   On a different note, in Chamonix the plans for 2009 running (and beyond) will be laid down. There is considerable focus on whether 6 TeV c.m. would be enough to beat the Tevatron results on Higgs and other searches, given 100/pb of data or so. It seems 6 TeV are insufficient, while 10 TeV would fit the bill. However, 10 TeV will be very risky for the machine, which will not be totally safe from possible additional incidents like the one of Sept. 19th. My guess is that they will declare they're going for 10 TeV this summer, and then settle for 8-9 TeV this fall.

2. **DB**
   January 28, 2009

   I have been harbouring a suspicion for some time that the LHC management are more concerned about the long term operational viability of the LHC than they are letting on.
   There is a question as to whether the machine can fulfill its operational goals or whether is will be so prone to breakdowns that its effectiveness may be seriously compromised.
   However, I don't want to exaggerate this, it's just a minor nagging doubt for now.
   Debates as to whether the quench suppression systems really are up to the job, in which case the last incident was just a freak occurrence, are among those that you might expect to see aired in internal meetings. Management would not want the outside world to get hold of such candid discussions for obvious PR reasons and this could explain the "temporary" access restrictions which are still in place. While I don't like it, I can understand the need for internal teams to feel they can freely and candidly discuss all possibilities without the prying eyes of the media who, let's face it, can only be trusted to adopt the most sensationalist spin.

3. **Peter Woit**
   January 28, 2009

   DB,

   The only sensationalist spin about the LHC I've noticed in the media is that surrounding black holes, and that is driven by theorists. The media coverage of LHC progress, problems and the accident seems to me to be have been quite responsible, with the accurate first-hand information available from the technical committees and the links to that provided by blogs helping to ensure this. When there is trouble, the media attention is going to be uncomfortable for CERN, but they're the ones who have gone to a lot of trouble to get this media attention before the machine is operational, while they're in problem-fixing mode.

4. **Yatima**
   January 28, 2009

Groan. I can understand the urge to “control and restrict” information about the internal processes of CERN, even though the primary reason for this will be the bog-standard one that you encounter time and again in industry, public organizations and politics, namely, preventive ass-covering and the urge to not make your money sources ask too many questions that you cannot honestly answer. While there evidently will be no darker motives behind any such policy change (Dick Cheney has not being tapped for this, right?), I’m sure a lot of fringe elements will be energized and will start looking for the “story behind the story” and the coverup motives in a heartbeat. I’m sure it’s easy for everyone to list a few scare scenarios not out of place in an X-Files episode. It is doubtful that this will result in a positive message or responsible media coverage in the long run.

5. **onymous**
January 28, 2009

The only sensationalist spin about the LHC I’ve noticed in the media is that surrounding black holes, and that is driven by theorists.

Is it? My sense is that the person who has been most consistently pushing the possibility is Greg Landsberg, an experimentalist. There is a bit of theory work at one edge of the community but I think few theorists would call this a plausible possibility. Most of the work by competent people in recent years was either driven by the need to produce a safety report once the crackpots got too vocal, or by people arguing that the original estimates were too optimistic even in the implausible models where it could happen at all.

6. **Warren Platts**
February 1, 2009

One thing I’ve found conspicuous by its absence is an analysis by CERN of CERN’s culture, and what role that culture played in the explosion, er, “incident” of Sept 19. When NASA reviews what happened when a shuttle blows up, there is in addition to an analysis of the proximate, mechanical causes, there is also an analysis of how NASA culture might be involved, e.g., pressures to launch despite icicles hanging from rockets. Yet there’s be nary a word from CERN regarding its culture. The only clue that there are cultural issues came from Herr Heuer’s promise to be more cautious than his predecessor. Why would he say such a thing? It wasn’t the fault of a busbar splice after all? All this information control and spinning is not healthy–not to mention undemocratic.

7. **csrster**
February 2, 2009

Trying to stop physicists from talking about their work? Welcome to the exciting world of cat-herding!

No Chamonix For You

January 30, 2009
Categories: Experimental HEP News

In the last posting I linked to the web-site for next week's LHC Performance workshop at Chamonix, where the state of efforts to recover from last September's accident and plans for this year will be discussed. As in many previous cases, linking to an authoritative information source about what is going on at the LHC had the effect of it being quickly shut off to the public. I guess CERN really is serious about the idea that information about problems at the LHC is now only supposed to come from the DG's office.

So, from now on, I'm sorry to have to do this, but I won't be linking to any such information sources that people point me to or that I run across. Instead, I'll try to continue to post here authoritative information that comes my way, without indicating its source. Today, I'll just note that I've heard from an authoritative source about the current informed guesses for when the LHC will be able to start doing physics. The current hope is for first usable collisions at 5 TeV (per beam) in October, with two months for a physics run at that energy before winter shutdown. Peak luminosity would be a few times 10^{31}, integrated luminosity a few tens of pb^{-1}.

Update: CERN does seem to be making an effort to put out more information about the status of the LHC through their press office. Yesterday there was this update posted as "breaking news", not waiting for the next issue of the weekly bulletin. The news in the update is uniformly good, telling us about how it has been "a good week". What will be interesting to see in the future is whether less encouraging news makes it out to the public…

Update: The web-page denying access to the Chamonix slides has been changed, it now reads:

> This site is temporarily password protected during the duration of the LHC workshop but will be re-opened immediately after the workshop.

I guess that CERN still wants the news of whatever is presented at Chamonix to first come from their press office, but realizes that making available the detailed technical discussion behind this news is a good idea.

Comments

1. **onymous**
   January 30, 2009

   5 TeV c.m. energy, or 5 TeV per beam?

2. **Peter Woit**
   January 30, 2009

   5TeV/beam (I'll clarify that in the text)

   This is based upon the assumption that they don't decide that for safety's sake they need to run at a lower energy.

3. **mike**
   January 31, 2009

   I realize that October is a rough estimate but from my experience in technical project management I am guessing this means possibly spring of 2010 (figuring in winter shutdown and unknown delays) before new physics starts happening? So it looks like Fermilab is our best hope this year for the Higgs search and other new physics – is there any news on this front?

4. **Peter Woit**
   January 31, 2009

   mike,

   The main news from Fermilab is that the Tevatron is performing extremely well, significantly better than projected, with nearly 6000 pb^{-1} collected already. Even if the LHC does collect some data late this year, it won't be enough to say anything about a Standard Model Higgs. For the Higgs search, it looks to me as if the Tevatron will be the only game in town for quite a while, with a real possibility of either ruling out or finding the Higgs if it is in the expected mass range, before the LHC has a chance to weigh in.

5. **B**
   January 31, 2009

   It's important to remember that the Tevatron can only say the SM Higgs exists or doesn't exist at 3 sigma. Impossible for anything more then that.

6. **Coin**
   February 1, 2009

   So, 2 weeks in october at 5 TeV/beam followed by the winter shutdown?

   Just to be clear, wasn't this basically exactly the intended plan for last year, before the accident?

7. **Chris Oakley**
   February 1, 2009

   > 5TeV/beam (I'll clarify that in the text)
   >
   > This is based upon the assumption that they don't decide that for safety's sake they need to run at a lower energy.

   What – safety's sake in the sense that they don't want to run the machine above the threshold at which planet-swallowing black holes are created?

8. **Mikael**
   February 1, 2009

No, safety's sake in the sense not to do further damage to the machine, which would cause further delays.

9. **Peter Woit**
February 1, 2009

Coin,

Two months, not two weeks. The hope is to go as late as possible, into December, before the winter shutdown.

Yes, this is pretty much the 2008 plan, the accident has cost them most of a year to deal with

10. **SMD**
February 1, 2009

The major issue at CERN nowadays seems to be how little confidence they have in their enterprise. They are doing the same thing with information that Karl Rove et. al. did during the last presidential administration. Filtering all information is the route to spinning information and conveniently forgetting unpleasant truths. What they are doing in effectively prohibiting the dissemination of information is destroying their own credibility. GO FERMILAB!!

11. **JoAnne**
February 1, 2009

Don't count on 10 TeV center of mass this Fall – they are seriously considering 8 or even 6….

12. **Spear Mark the Second**
February 1, 2009

So little attention has been paid to the technical evolution of the LHC. On the good side is the enormous risk that was taken… CERN really has bet the farm on the LHC. Good for them! Amazing amounts of terrific engineering and applied science have been performed.

On the other side… it has been under-resourced. Hard to say that when billions $ have been spent. Still, the amount of money should be compared to the difficulty of the task. A lot of the fun of CERN has been sacrificed to try to make this a successful project… CERN used to have way more variety and a really fun scientific environment for experimental physics. Devotion to one big project kind of squeezes much of the elan out; also, the boundary between team members and outcasts who complain too much gets moved around to fit PR concerns.

Germany has been a bit lukewarm… resources into HERA and Linear Colliders… great to maintain diversity in the program, but many folks knew LHC was the main event, and was hurting.

Experimental physics is hard, really hard, much more challenging than string theory or SUSY. It will be a great success if LHC gets 14 TeV and good luminosity by 2011.

CERN is going the way of Fermilab and Cornell… dreadfully wanting to chase the great physics, but being unable to get the appropriate level of resources for a `Swiss Watch' startup. They will never give up! But what happens is delays and conversion of operating funds to finish the project. Run 2 at FNAL went through the same thing. Cornell never made a deadline that I can recall.

13. **mike**
February 1, 2009

Does anybody know when Fermilab might have definitive results on a Higgs mass? I know there is a lot of peer-review that has to be completed before the final results are published but perhaps something will leak out by the end of this year?

14. **Peter Woit**
February 1, 2009

mike,

Last summer, the Tevatron experiments released an analysis claiming that (using data from both experiments) they could exclude (at 95% confidence level) a Higgs mass in a very narrow range, essentially, exactly 170 GeV. I haven't heard anything about improvements on this, but they have a lot more data to analyze and are accumulating more all the time. With more data and improvements in the analysis, over the next few years one would expect them to be able to exclude increasingly large ranges of possible Higgs masses (or, see evidence of a Higgs). It appears quite possible that they'll be ahead of the LHC in this game until at least 2011 or so. Unclear if they'll be able to go all the way down to 114 GeV, the lower limit set by LEP.

15. **Nobody**
February 2, 2009

Dear Peter,
I think you perfectly describe the situation at CERN. I must say that I find one of the worst things of last year was the continuous exposure to "Official Press Releases", which as everybody already knew turned out to be one falser than the other. All of this without the arrogant management ever feeling the need to apologise with the Staff for the release of false and self-contradictory information. I really do hope the new DG manages to change this policy; but this will be a difficult task for him.
P.S. Your blog has been the place where one could find the most updated and complete information about CERN. Thank you.

16. **Peter Woit**
February 2, 2009

Nobody,

Thanks, I'm glad this blog has been a useful source of information. I hope that CERN decides to make as much technical discussion of the ongoing LHC project as possible publicly available. Blogs like this one can then provide the service of helping to point people to where to look for such up-to-date information.

17. **Anon**
February 2, 2009

Hi,

Did you attend the Grothendieck conference at the IHES? If so, I was wondering if you could write up a summary of what happened. I'm interested to hear what when on.

Thanks.

18. **Peter Woit**
February 3, 2009

Anon,

I went out to the IHES for parts of two days of the conference. There was a video-camera there, and I have been hoping the IHES will make available video of the talks, don't know if they have the capability or intend to. Maybe I will try and write something about the talks that I heard. This history was quite interesting, with Cartier, Deligne, Illusie, Serre and others who worked with Grothendieck during the 60s in attendance. Deligne gave a fascinating talk on motives, which is one thing I'd like to write about, but the topic is quite a challenge, and I haven't gotten up the energy yet…

19. **Anon**
February 5, 2009

Hey,

I actually found a blog that's covering the Grothendieck conference:

http://homotopical.wordpress.com/

20. **Peter Woit**
February 5, 2009

Anon,

Thanks, that's great. I'll try to find time soon to put up a posting about this, and maybe add something about motives and Deligne's talk.

21. **Chris Austin**
February 6, 2009

Returning to the topic of the LHC: if there is a new effect, such as TeV-scale gravity, which turns on very rapidly, modulo smearing by parton distributions, perhaps first collecting a lot of data at, say, 8 TeV c.m. energy, before moving up to 14 TeV, might be very helpful for trying to disentangle what is happening.

What is String Theory?

February 3, 2009
Categories: Uncategorized

Yesterday Joe Polchinski gave a lunch-time talk at the KITP on the topic of What is String Theory? No answer to the question, but he provided an outline of three topics being discussed at the current KITP workshop program that have something to do with it.

- String field theory: he wrote down the Witten open-string action and advertised that as the best candidate for a definition of string theory that could go on a t-shirt. He noted some of the problems with this, especially how to understand closed strings, which are somehow "emergent", "hidden in the measure" on string field space, which one doesn't really understand.
- The Berkovits pure spinor formalism for quantizing the superstring: if you want a consistent theory, you need supersymmetry, and Polchinski explained that the quantization of both supergravity and the superstring are ferociously complicated subjects. He hopes that the Berkovits formalism will provide a more lucid (perturbative) quantization of the superstring, one allowing a proof of finiteness at higher loops. This topic doesn't really address the "what is string theory?" question, since it is supposed to be equivalent to other ways of quantizing the superstring, and only valid perturbatively.
- AdS/CFT and integrability: here there's an answer to the "what is string theory?" question, but it's in some ways a disappointing one for the idea of a single string theory that unifies everything and goes beyond QFT. If you believe the full gauge/string duality speculative framework, there are lots of string theories, each of which is defined by fiat to be a certain QFT. If this is right, perturbative string theory is just a tool useful in the study of some strongly-coupled QFTs, and non-perturbative string theory isn't really a subject distinct from QFT. If you want to unify physics starting from thinking about the SM, at short distances you have a weakly-coupled QFT, with no role for string theory. And, in this picture, there are lots of string theories…

At the end, someone asked about the LHC and supersymmetry, Polchinski responded that string theory didn't require LHC-scale supersymmetry, but if supersymmetry was discovered at the LHC then there would be a "sociological" effect encouraging to string theorists. I also noticed recently that Polchinski has a web-page On some criticisms of string theory.

In his discussion of the pure spinor formalism, he noted that supersymmetry doesn't seem to "resonate" with mathematicians, but that pure spinors are more something they recognize. This is certainly true, with supersymmetry something frustratingly close to some standard mathematical constructions, but quite different in other ways. Pure spinors occur naturally when one tries to construct spinors geometrically. Projectively, the space of pure spinors is SO(2n)/U(n), a space which has some quite beautiful properties. In the Borel-Weil geometric construction of representations, spinors are holomorphic sections of a line bundle over this space (for details of this, see the chapter on spinors in *Loop Groups*, the book by Pressley and Segal).

For the superstring, one is interested in the case of n=5, and a certain sigma model with target space the space of pure spinors. There's a more general class of sigma models of which this is a special case, and for more about some of the interesting connections of this to other subjects, see the recent KITP talks by Nekrasov and Frenkel. The Frenkel talk is especially interesting, since it involves several other quite beautiful related ideas. He describes one motivation for studying some of these sigma models that comes from geometric Langlands. While he was at Santa Barbara, Frenkel also gave two nice survey talks about geometric Langlands, see here.

Update: Clifford Johnson explains here that not only do we not know what string theory is, but we can't even say anything useful about what it isn't, other than "it is not a theory of strings". The problem with this situation, according to him is:

> people who don't know what they're talking about, and sometimes with an axe to grind, shouting loudly (and sometimes deliberately misleadingly) about it.

Update: More thoughts from Clifford on the question of how to deal with string theory critics.

Comments

1. **Sumar Ongi**
   February 3, 2009

   > He hopes that the Berkovits formalism will provide a more lucid (perturbative) quantization of the superstring, one allowing a proof of finiteness at higher loops.

   Can one infer from this that, in Polchinski's view, there's no complete proof of finiteness yet?

2. **Peter Woit**
   February 3, 2009

   Sumar,

   He was specifically asked about this, and did not claim that a complete proof of finiteness exists.

3. **Alberto G.P.**
   February 3, 2009

   Like some banks, do you think string theory is in crisis, today? What is the opinion of the main theoretical high energy physicists?

4. **Peter Woit**
   February 3, 2009

   Alberto,

   I think the analogy with some banks is apt. One part of the U.S. financial crisis seems to be the fact that some of the largest banks are "zombie" institutions, insolvent but unwilling to acknowledge this and draw the necessary conclusions. The idea of string theory unification is also bankrupt at this point, but being propped up because it would be very inconvenient for many people to acknowledge this and they don't see any palatable alternative to soldiering on. One difference is that the banks could (and probably will) have their insolvency problem fixed by pouring in lots of government money. This won't work with string theory.

   Among "main theoretical high energy physicists" you'll find a wide spectrum of opinion about string theory, from strong defenders to those even more unhappy about its state than I am. One thing that I think is undeniable is that the spectrum of opinions has shifted in recent years toward a much more skeptical take on string theory's prospects.

5. **Coin**

February 4, 2009

Is String Field Theory still an area of active work and development? It seems when I try to look up material on string field theory I mostly find stuff from the 80s to late 90s (Witten's work in the area seems to come from this time period as well if I'm not mistaken?).

6. **chapieau**
February 4, 2009

Recently I have got the feeling that string theory is going to become a purely mathematical subject. It could be condensed in two "Theorems" to be proved:
1) Under very general assumptions every reasonable field theory can be embedded in a string theory which makes its UV (high energy) dynamics consistent.
2) Maldacena conjecture shows that both the problem of "solving" string theory and the problem of strong coupling of gauge theories are equivalently complicated. Solving one solves the other.
Waiting for some brilliant mathematician to demonstrate the theorems, phycicists are going back to (LHC) phenomenology and toward bottom-up approaches

7. **Kea**
February 4, 2009

One assumes that Clifford's *people who don't know what they are talking about* includes everybody that is not a respectable string theorist, which is to say that it includes nobody who disagrees with the basic stringy party line. This includes a rapidly increasing number of very respectable physicists from *other* areas of physics, most notably experimental. But of course, they don't really know what they're talking about because they have not really studied string theory. Heh, maybe if they did they would appreciate the necessity of forgetting about contact with experiment, and then appreciate the beauty of the mathematics, and be thankful for their now superior understanding of the fundamentals of physics. Yeah, right.

8. **Tony Smith**
February 4, 2009

Peter, you said:
"… Projectively, the space of pure spinors is SO(2n)/U(n) …
For the superstring, one is interested in the case of n=5 …",
which sort of confuses me.

As to the math (not so confusing), Reese Harvey (in his book Spinors and Calibrations) says
"… the set Cpx(n) of orthogonal complex structures on R^2n has two connected components Cpx+(n) and Cpx-(n), with
Cpx(n) = O(2n)/U(n)
Cpx+/-(n) = SO(2n)/U(n)

Harvey calls O(2n)/U(n) "the twistor space (at a point on a manifold), i.e., the twistor fiber",
and says(I am using "n" instead of Harvey's "p" here):
"… Let PURE(n) .. denote the set of all pure spinors …
consider the complex case …
PURE_C / C* = Cpx(2n) = O(2n)/U(n)
…
The square of a pure spinor represents the associated null plane in
/\R(n,n) …".

In the relevant case of n=5, it seems to me:

O(10)/U(5) which has 45 – 25 = 20 real dimensions is the twistor space,
and it has two components (for the two mirror image half-spinors of Spin(10)) that each are SO(10)/U(5) with 45-25 = 20 real dimensions.

Since PURE_C /C* = O(10)/U(5) has 45-25 = 20 real dimensions,
it has 10 complex dimensions.
Since the C* of complex scalars is 1-complex-dimensional,
PURE_C has 10+1 = 11 complex dimensions.

Since Spin(10) half-spinors are 16-dimensional,
11 of the 16 dimensions of Spin(10) half-spinors are PURE (in the real context).

That is consistent with the table in vol. 2 of the book Spinors and Spacetime, by Penrose and Rindler, that lists in table B.65 on page 453 for the even dimensional vector space n=10
the dimension d_10 of the space of pure spinors as 11
and the dimension of half-spinors as 16.

If my understanding stated above is roughly correct,
then it seems to me that the math/geometry is quite beautiful,
but
what really confuses me is how it is applied by superstring people:

Do they use the SU(5) as a GUT ?
If so, how do they deal with proton decay experimental results ?

Do they use the twistor space as generalized Penrose twistors ?
If so, how do they deal with massive fundamental particles ?

Do they just ignore concrete stuff like proton decay and particle mass,
taking the Clifford Johnson attitude "We're still working on it" so that their highly trained minds won't be corrupted by such messy detail stuff ?
(That reminds me of the HHGG where Vroomfondel said
"… our brains must be too highly trained …".)

Tony Smith

9. **Walter Mondale**
February 5, 2009

Dude, you're obsessed with string theory. Ok, we get your point. It's unprovable. But man, do you have to spend day after day, week after week, year after year, going on and on and on about this? I mean I can see a scientist being really into his research, but this is a scientist being really into "unresearch". You are just gushing about how a theory is not satisfactory. I'm not even saying you're wrong… but.. don't you ever get tired of this and think, maybe I'll go take up a new hobby like sailing or something….

10. **anon.**
February 5, 2009

'… don't you ever get tired of this and think, maybe I'll go take up a new hobby like sailing or something …'

Sailing is a good way to be alone with chicks for a few hours, but you're missing the basic string theory point. The flaw in string theory is *not* just 'it's unprovable' as you claim to grasp. The flaw is that it's not *falsifiable*. You can't prove models true in general, they are just approximations. E.g. quantum electrodynamics can't be proved, instead the theory survives experimental checks which have the capacity to falsify theories. The crisis in string theory is caused by the fact that it has been hyped as a self-consistent quantum gravity and unification theory without even any experimental support at all, never mind proof.

11. **somebody**
February 5, 2009

Alberto, look at the title of this blog, man. What do YOU think Peter thinks about the "crisis" in string theory?

Coin, in the last decade, there were two developments in string field theory that I consider "major". One was from Ashoke Sen's beautifully simple idea of tachyon condensation, which gave us the basic conceptual understanding of SFT that we have now. The second is Schnabl's solution of the closed string theory vacuum, which is more technical, but has created some powerful new tools and has made previously intractable questions within reach.

anon., Walter wasn't talking about string theory, he was talking about Peter's obsession with it. So it is not fair to say that he is "missing the basic string theory point", whatever that might be.

I have one quibble about Polchinski's talk – it seemed to focus entirely on the big-machines of formalism. Perhaps he was outlining the topics in the KITP program schedule.

Either way, he is looking for a way to answer the real question "what is string theory?", and not give the audience an exposition about "what is string theory (currently)?". This seems to have been lost on some of Peter's fans.

One interesting point I was reminded of in his talk was that there seems to be no simple way to derive 4D supergravity.

12. **Peter Woit**
February 5, 2009

Tony,

You have the geometry right. The twistors here are different than Penrose's, they're the 10d analog of what he is doing in 4d.

The SU(5) isn't the SU(5) in GUTs. This story is all about the basic question of quantizing the 10d superstring. To get GUTs or any particle physics you need to add more structure (compactification, for one thing).

13. **Walter Mondale**
February 5, 2009

Um, duh yes I know it's not disprovable either. This is given a subtle hint in the title "Not Even Wrong." Don't talk down to me, you spoon. The point is that spending years blogging and writing books about this subject is excessive. At this point in time, is there really anything else to add? Sure, there's some new conference or lecture by some renowned genius. But does it really truly add something new to the debate to point out for the 3,209th time how the latest development is still not satisfactory, because this too can't be proven or disproven via experiment? I mean he lives in NEW YORK CITY for crying out loud, there's so much to do. He could be getting laid every night. But no, instead he does this….

14. **Peter Woit**
February 5, 2009

Walter,

Odd, there's nothing in this posting about the proving or disproving string theory by experiment. You seem to be quite obsessed by this.

Also curious is how many people are concerned that my writing critical blog entries about string theory is interfering with my sex life (for instance, I recall the immortal words of Nima Arkani-Hamed as reported by Lubos "What's wrong with these people? Why don't they choose f***ing instead of writing about things that they don't like and they don't understand?"). Thanks for your concern, but rest assured, your worries about this are misplaced.

One not-so-obvious thing I should perhaps point out is that, especially after you've been doing it for a while, writing blog postings takes very little time. This one took less than a half an hour. Significantly more time was taken up by watching Polchinski's talk on-line, but it was an interesting and clear summary of the latest thinking on some quite technical issues, so definitely worth the hour or two I spent on it.

I'd be very glad if someone else would take over the job of providing a counterweight to the huge amount of hype and misleading promotion of string theory going on, which is doing continuing and serious damage to particle theory. For now it seems worthwhile to devote at least a few hours a week to this project, since the damage to my sex life is minimal. Much more serious actually is my recent fascination with reading financial news and following the ongoing economic crisis. That I've really got to stop….

15. **Syksy Rasanen**
February 5, 2009

The comic posted by Clifford is amazing. The fact that it is promoted by someone who identifies with the 'bitch-slapper' is symptomatic. Peter, nothing you write about 'string theory partisans' could possibly give a worse impression than this – this is self-parody of the highest order.

16. **yoyoq**
February 5, 2009

walter, what is worse ? :

the guy with the blog complaining about
string theory or the guy who writes into the blog complaining
about complaining about string theory

17. **Thomas R Love**
February 5, 2009

    Joseph Polchinski has an article on arXiv.org with the title "What is String Theory?"

    http://arxiv.org/abs/hep-th/9411028

    154 pages! From 1994!

18. **db**
February 5, 2009

    yoyoq> or the guy who writes into the blog complaining
    about complaining about complaining about string theory?

    Sorry.

19. **Coin**
February 5, 2009

    "Somebody", thanks for the explanation.

    I had not realized tachyon condensation was an idea from String Field Theory. Is everyone who works with tachyon condensation doing String Field Theory then, or is that just where the idea originated?

20. **Alberto G.P.**
February 5, 2009

    Peter,

    Thanks for the response!

    somebody,

    As I am a pedestrian, and my English is limited, I can only formulate very simple questions, questions that even a child would understand. As you seem a smart guy, I also have a question for you. In the last decade, what do you think has been the progress to obtain the electron mass using string theory?

    Thanks.

21. **Peter Woit**
February 5, 2009

    Coin,

    "tachyon condensation" just means that the usual perturbative vacuum state of fluctuations about zero field is unstable (there are tachyons) with a stable vacuum state given by fluctuations about some non-zero field (condensation of the tachyon). This is a general phenomenon in field theory, not specific to string field theory.

    Alberto G.P.,

    In the last decade there has been no progress towards obtaining the electron mass in string theory. There has however been a great deal of progress towards showing that you can get any electron mass you want in string theory.

22. **somebody**
February 5, 2009

    Peter, nice try. Yes, tachyon condensation exists outside of string field theory. So does gauge invariance, BRST symmetry, action functionals, … , addition, multiplication, and a zillion other things. 🙂

    "Coin": the point was that the tachyon condensation I meant was specifically the tachyon condensation that is relevant in string field theory. The one that takes you from a spacefilling D-brane to the closed string vaccum.

    Alberto, I will ignore your sarcasm and asssume it is the arrogance of the beginner. I recommend that you read my comments on this thread if you really want an answer:

    http://www.math.columbia.edu/~woit/wordpress/?p=740&cpage=1#comments

    The short answer is this: QFT doesn't predict the elctron mass either, but that hardly makes it non-preditive. I will ignore Peter's rhetoric, this has been discussed before, he has been told and knows full well whats up, and he just want to insinuate to his readers that predicting the electron mass is necessary for a theory to be predictive.

23. **Coin**
February 5, 2009

    woit: OK. Would it still be correct though to say that in order to apply tachyon condensation to String Theory, one must be using the String Field tools?

24. **Peter Woit**
February 5, 2009

    Coin,

    It's a field theoretical concept, so you need some sort of field. I don't know what it would mean in perturbative string theory.

25. **Jack Lothian**
February 5, 2009

Walter,

While I agree with many points made on Peter's site & I often enjoy reading some of the discussions, I too feel that at times the discussions become repetitive & circular. Whenever I feel like this, I tend to stay away from the site for a few weeks or a month. When I come back I can usually pick up the threads with a renewed interest.

While I understand some of the points you are making, I do not understant the anger or hostility. Peter is not forcing you come to his site. It is your free choice, so why are you angry with him? He is just doing something that he enjoys doing & the world is free to ignore it or read it.

26. **Walter Mondale**
February 6, 2009

I am not angry at him, I don't really know why you are saying that. I was annoyed at that other guy who talked down to me, but not Woit. I'm just kind of amazed at the spectacle of the blog, but I agree, if that's what he wants to do, that's what he wants to do. And he says his sex life is satisfactory, presumably from all the blog-related groupie activity. What more can one ask for?

27. **Alberto G.P.**
February 6, 2009

somebody,

Do you know if there is a solution of string theory that explains the Bhabha scattering, like QED?, Do you think I'm arrogant for asking? Maybe, 'the new science' doesn't need basic questions.

I think the problem is not with the string theory, but, with the philosophy of science.

BTW, I am not a professional physicist. I'm only a physics buff. I try to study physics as a hobby, but in a level much lower than it would be needed for studying string theory, because I'm completely unable to understand its math background. At the future, maybe, string theory will explain the other physics theories, and all the experiments, that the more conventional theories have already explained, then, I'll ask for forgiveness, even though, there were no new predictions. Meanwhile, I will come back to my Greiner's books.

Good luck.

28. **David B.**
February 6, 2009

Dear Alberto:

To the extent that string theory is compatible with gauge invariant interactions, and that the low energy effective field theory of string processes is indistinguishable from ordinary quantum field theoretical models, the Bhabha scattering is predicted with the same shape as it has in quantum electrodynamics.

The hard issue is not to say that one has quantum field theories in string theory that look sufficiently similar to what we see, but to what extent one can predict parameters that in the standard model are numbers that one measures (theoretical inputs) and for which there is no theory.

29. **Thomas R Love**
February 6, 2009

Peter,

You have mentioned this before, but it bears repeating since it echoes Polinski's title. In a 2005 article in Nature:

http://www.sns.ias.edu/~witten/papers/Unravelling.pdf

Witten wrote:

We still don't know where all these ideas are coming from — or heading to.
One day we may understand what string theory really is.

Witten doesn't know what string theory is.
Polenski asked the question, but did not answer it, so he doesn't know either.

I have observed that string theory's PR (Public Relations)
Is better than string theory's PR (Physical Review).

"Walter Mondale" wrote:

Dude, you're obsessed with string theory. Ok, we get your point. It's unprovable. But man, do you have to spend day after day, week after week, year after year, going on and on and on about this?

Walter: Peter is like the boy who said: "The Emperor has no clothes", he just has to keep saying it until the world gets the message. There are "string theorists" who treat the subject more like a religion. They have to, it is not a science.

I read Peter's book "Not Even Wrong" soon after it came out. It is a must read for all physicists. So, Walter, read the book!

30. **st**
February 6, 2009

I am baffled by the often use of analogy between string theory and religion. The two could not be more different. One is supposed to be a (correct/incorrect/not even wrong) theory about the physical world, and the other is about the supernatural, which as such, can never be proved or disproved by the scientific method.

31. **someody**
February 7, 2009

Thomas Love says : “Witten doesn’t know what string theory is. Polenski [sic] asked the question, but did not answer it, so he doesn’t know either.”

What is your point? If we understood string theory (at least at the level we understand QFT), then we would know immediately whether it was right or wrong and that would be the end of the string “controversy” nonsense. The point precisely is that string theory as it stands is incompletely understood. And no, this is not a secret known only to Witten and Polchinski.

32. **Alberto G.P.**
February 7, 2009

somebody,

After the response by David, and the response that a known string theorist gave me, I have realised that string theory is a model that can explain both QFT (Y-M theories) and general relativity in a consistent mathematical framework. Namely, we can explain all the laws of the fundamental physics/nature, although we can’t explain the fundamental parameters, using string theory. So, I have to apologise for my ignorance, for me the polemic is over.

33. **Shantanu**
February 7, 2009

somebody and other string theory enthusiasts, could you provide answers to the following questions ?

o Why hasn’t string theory solved the cosmological constant problem?

o What does string theory predict for the value of Theta_13, a quantity which is soon going to measured?

o Does string theory violate equivalence principle? (I have heard both yes
and no to this answer)

Thanks

34. **someody**
February 7, 2009

Alberto, just saying that the parameters of low energy physics are not fixed is perhaps a bit too generous a view of the current state of string theory. It is true, but the real problem is that we don’t really understand the true nature of string theory. The situation is very much like that of the proverbial blind men and the elephant. We are stuck to certain partial descriptions and specific tools, and because of this model-building is extremely hard. This is one of the reasons why “fixing all the parameters” in a useful way hasn’t happened.

35. **Peter Woit**
February 7, 2009

Alberto,

You have been misled. There’s an active campaign on to mislead people in this manner. The whole point of Polchinski’s talk is that there is no known mathematically consistent version of string theory that does what they hope (provide a unification of the SM and gravity).

Shantanu,

The answers to this are well-known:

1. the anthropic landscape “explains” the CC (and if you don’t like this, well, no one else can explain it).

2. string theory does not predict SM parameters.

3. light moduli fields will violate the equivalence principle. Like everything else, you can get whatever you want in string theory, have these or not have these…

somebody,

You are being misleading. The problem is not that you can’t “fix all the parameters”, the problem is that you can’t fix ANYTHING at all. Nothing, zero, zip nada. The excuse that “we don’t really understand the true nature of string theory” is getting very old. Everything you do learn about string theory provides more and more evidence that it can never predict anything about particle physics.

36. **someody**
February 7, 2009

Shantanu, my current view on string theory is that it is really a paradigm for building UV complete models. But if this paradigm is going to be useful, it should have a better (non-perturbative) definition, where we have the freedom to build models following ideas instead of being stuck with specific technical tools. The existence of such a definition is what made QFT useful for UV-INcomplete model building.

It is because it is a paradigm, that you can have many possibilities (“anything you want” in Peter’s pejorative description) in string theory. But note that QFT can also have anything you want, because it is also a paradigm. But because of the non-perturbative definition available in QFT, model-building was still fruitful.

One problem with not having a non-perturbative definition is that we can solve many problems (including the ones you point out) in string theory individually, but putting them all together becomes a huge challenge. This is the reason why more specific predictions at this stage are perhaps not very meaningful.

To Peter: I was talking about stabilizing (fixing) moduli in a specific vacuum. You are again confusing this with fixing fundamental constants dynamically, and accusing me of being misleading.

Peter also says: “The excuse that “we don’t really understand the true nature of string theory” is getting very old.”

The “excuse”? Should we apologize to you, Peter? 🙂

Some of these problems are truly hard, just wishing otherwise is not a particularly useful strategy for progress. Many smart people have been battling quantum gravity for 50 or so years, and it is not so unreasonable that the problem is genuinely hard. In particular, the “what is string theory?” we ask now is certainly not the same “what is sstring theory?”

37. **Peter Woit**
February 7, 2009

somebody,

Feynman said it well: "String theorists don't make predictions, they make excuses". This has nothing to do with apologizing, it has to do with being willing to admit that an idea doesn't work and move on to something else. In science this happens all the time and requires no apology. Most ideas don't work out.

The reason string theory predicts nothing, and the Standard Model is the most highly predictive physical theory known to man has nothing to do with the different statuses of their non-perturbative formulations. You can in principle write down arbitrarily complicated QFTs, and get, more or less "anything you want". If you did that, no one would take you seriously. Instead, it turns out that a quite simple gauge theory works precisely, on the nose, passing literally thousands of non-trivial experimental tests. In string theory on the other hand, all versions of the theory that are simple enough to analyze don't come even close to looking like physics. Instead, one has to invoke more and more complicated "string theory backgrounds" in order to avoid contradiction with experiment, never actually getting a calculation of anything you can compare to experiment.

"more specific predictions at this stage are perhaps not very meaningful".

You really insist on saying things in the most misleading way possible. More specific than what? Again, string theory predicts absolutely zero about particle physics. The problem is not that it is not specific enough, only giving you rough information about particle physics, it is that it gives you NO information about particle physics at all. Nothing.

38. **somebody**
February 7, 2009

Peter, I will ignore your appeals to authority and vitriolic comments. Lets stay focussed on the science.

Peter: "You can in principle write down arbitrarily complicated QFTs, and get, more or less "anything you want"…. Instead, it turns out that a quite simple gauge theory works precisely, on the nose, passing literally thousands of non-trivial experimental tests. etc."

How do you even call a QFT "simple" if you only had a bunch of rules for writing down messy scattering amplitudes? The non-abelian gauge symmetry of the non-perturbative description is what gives any sense to the statement that the theory is simple because the gauge groups are! "Simplicity" is essentially a meaningless concept when there is no non-perturbative definition (like in string theory, currently). This is one reason why it is worthwhile trying to answer the question "what is string theory?" and come up with a non-perturbative definition.

Peter: "You really insist on saying things in the most misleading way possible. More specific than what? Again, string theory predicts absolutely zero about particle physics. The problem is not that it is not specific enough, only giving you rough information about particle physics, it is that it gives you NO information about particle physics at all. Nothing. "

You are attacking a paradigm for not being a model. As a paradigm, non-abelain gauge invariance, chiral fermions, multiple generations etc. (all in the context of a theory that also includes gravity!) are all to be found in string theory. Your argument is like saying that QFT (as a paradigm) predicts nothing. But standard model, a specific QFT, is indeed predictive. So sttempts to gain model-building powers in string theory are well justified.

Last but not the least, you have a tendency to repeat claims many times hoping that this will turn them into facts. I just wanted to point out that this is not the case. 🙂

39. **Tumbledried**
February 7, 2009

Dear Peter, Somebody,

Many apologies to interject, but I would like to suggest that we suspend for the time being this increasingly vitriolic exchange and focus on some other matters instead.

Dear Somebody,

I am quite interested to hear your opinion as to whether you consider it reasonable, considering the status of string "theory" as a definite work in progress which even the world experts do not have a good intuitive feel for, as to look at other alternatives to string theory.

There are certainly many other models and theories out there that people are working on, and I assume a fair proportion of these may contain valuable insights about the nature of reality. I would be quite interested to hear from you whether it would be worth the time and energy to take a serious look at some of these, even though they are not The One True Theory that has been the focus of tens of thousands of person hours since the late 1970s.

For instance theology and pure philosophy has consumed much more of human time and effort over the centuries than string theory has for the last 30 years. Many extremely brilliant people, far more gifted than you or I, spent time on such things, debating endless metaphysical questions that were doubtless considered most profound at the time. Yet arguably we are no further than we were in such matters since the time of the ancient Greeks.

Dear Peter,

I greatly admire your tenacity in these matters. It is unfortunate, but it seems in these times that sometimes a point needs to be repeated over and over again in order for people to finally understand it.

Best Regards,
Tumbledried

40. **Peter Woit**
February 7, 2009

Thanks Tumbledried,

I confess that I'm infuriated by these arguments claiming that something that predicts nothing is the same as something that predicts just about everything we understand about fundamental physics. Once a science is taken over by people claiming that black is white since they're both kind of like grey it's pretty much all over. It's very sad to see what has happened to particle theory over the last 25 years.

41. **Luca**
February 8, 2009

I am an Italian student of Philosophy at the University of Studies of Milan. Recently I have read the book Not Even Wrong in the Italian translation and I found it very stimulating. In my university there are not many professors that study these themes and when they do it, they seem more worried to talk of old questions of early 20th century, i.e. of the paradigmatic change in physics due to the introduction of quanta. To be honest, I find more interesting the modern physics, because it seems to be in a critical phase, as the book suggests.
Reflecting on these themes, I have reached a personal opinion about the actual problem of theoretical physics. For me the problem is this: which form should a theory of everything have? There are two ways to answer this question: 1) it should be able to predict a lot of new phenomena; 2) it should be able to describe the world for what it is, without justifying it (this is the thesis I support). The problem is if a theory of everything would be scientific in accord with our normal scientific codes. I suspect not. A theory of everything does nothing but say that what it is there is because there must be or there would not be. Probably it seems a play on words, but it is not. A theory of everything, in my opinion, would not be predictive, but descriptive, and it would be a strange sort of theory in which the physical parameters have those values only because if they had not them, our universe would not exist. Hence the anthropic principle is a consequence of these assumptions. From this theoretical point of view, the most coherent string theory would not be a theory that provides us with results, but a theory that describes those results were already found. I personally regard a theory of everything as a natural language. It is only a logical connection of sentences, words and punctuations, and it cannot make predictions. However, in all circumstances (experienced before or completely new), we are able to adapt the language in order to make a detailed description of the situation, even if we have no idea why the situation is the way it is. Our linguistic code provides us with a system that can face every circumstance and also accept new terms in it without this fact implies the collapse of the whole theory (in this case, language). This is probably the form a theory of everything should have: for every new observation there is a way to accommodate the syntax of the theory without compromising the theory itself.
The problem is if this way to proceed is real science or not, and I am really dubious of it.

Kind Regards

Luca Ferrari

42. **somebody**
February 8, 2009

Tumbledried, thanks for providing the necessary distraction and saving Peter from himself. 🙂

I don't have much to say about your comments equating string theory to theology because it is in fact true that for an outsider, the difference is hard to tell. All the elaborate formalism just seems like pointless crap. To see that there is indeed direction (and a very clear one) you need a bit of expertise, I am afraid.

But, your comments regarding alterantives, I can make a few comments. I think there is a widespread misconception that there are many ignored alternatives. I think that most good string theorists would be eager to look into alternatives IF it had even *some* of the good features of a fundamental theory. But most people don't seem to realize how difficult it is to come up with even a mildly promising alternative. It is not like there are tons of alternatives out there and theorists are ignoring them. Quantum gravity is a difficult problem and despite half a century of work, there are only very few ideas which are not stillborn. The self-interest of theorists motivates them to consider new promising ideas very seriously. I don't even know one promising alternative idea out there which is not getting the attention it deserves. Another interesting fact is that almost all the ideas that seemed promising, seem to arise naturally in string theory. Supergravity, kaluza-klein theories, etc. were separate ideas initially, but they arise automatically in string theory.

43. **Shantanu**
February 8, 2009

Somebody, many people whom I know who used to work on string theory
have left string theory and switched to astrophysics, gravitational waves, LQG for precisely the same reasons Peter is talking about.

44. **Tumbledried**
February 8, 2009

Dear Peter,

You are most welcome.

Dear Somebody,

Thanks for your polite and considered response. I must admit that I was perhaps being a bit too honest with you, but you are quite correct, I am an outsider, or at least not a string theorist.

I am reassured by your response that, at least the part of the stringy community you frequent, are quite open to new ideas, providing that they are reasonably promising. This seems acceptable to me. It is of course disheartening to hear that there do not appear to be any promising alternative directions to string at the moment. Nonetheless I remain optimistic that the open problem of further unification is not impossible and some bright academic may find some different way of approaching it.

Best Regards,
Tumbledried

45. **klaus**
February 9, 2009

shantanu: people say a lot of things about why they leave a subject, partly to protect their own egos. most people I know, who left string theory, left it because the job market is brutal, and not because they got fascinated by something else. i am sure there are some people who do it because of the reasons you suggest, but the overwhelming majority that i have had first hand experience with, do not belong to that category, and leave because staying on is tough.

46. **chris**
February 10, 2009

dear somebody,

the argument of paradigm vs. model you invoke is rather interesting. however, there is one small but in my opinion rather essential point that you are missing in your comparison to early quantum field theory. you say:
"How do you even call a QFT "simple" if you only had a bunch of rules for writing down messy scattering amplitudes?"
which is partly true (because scattering amplitudes display the same sorts of symmetries just in a more hidden way) but not quite captures what early (<1970) qft looked like. keep in mind, that in qft the lagrangean was there right in the beginning. and step by step people came to grasp with how to evaluate path integrals with it. that's the story. and if you look at paradigms: there were a few. s-matrix theory, bootstrap, hadronic strings… all of them failed. advancement came about through better methods to do the path integral and that was it.

judging string theory is not up to me – future generations of experimentalists will certainly do that – but i am sceptic towards claims of paradigm shift without the paradigm fully visible. it leaves the same taste in my mouth than forward references like ‘we will show this in one of our next publications’. my gut feeling is that anounced revolutions rarely happen.

47. **Daniel de França MTd2**
February 13, 2009

This is certainly offtopic, but worth posting.

http://www.youtube.com/watch?v=MyVyDUdzvMY&feature=channel

This is a video of Levni Yilmaz talking about his private life with his father in a very creative bud sad and nostalgic way.

He is one of Yilmaz sons(that guy of the theory of gravitaion). He is a huge hit on Youtube with several million of visits, and talks about thing of daily life, no science.

48. **somebody**
February 14, 2009

Chris, I was busy and didn’t see your comment. Anyway, the point was not how history unfolded in the case of quantum field theory. The point was that the simplicity of a theory is not necessarily visible when you look at scattering amplitudes. A Lagrangian type description is where that is manifest, which we do not have yet in string theory.

Also I am using the word “paradigm” as just another word for “framework”. One of my suspicions about string theory is NOT that it is too much of a paradigm shift, but that it is probably not enough of one.

Finally, wanting a non-perturbative definition is not the advertisement for a coming revolution, it is the statement of an actual open problem.

49. **chris**
February 17, 2009

dear somebody,

thanks for your reply and clarifications. what you say sounds very reasonable to me.

Chamonix For You

February 6, 2009
Categories: Experimental HEP News

The 2009 Chamonix workshop on the commissioning of the LHC has just finished, ending with a message from the Director General, and the opening to the public of the web-site with slides from the meeting (bearing the warning "The Chamonix workshop was an open exchange of views and opinions. All the presentations made at the workshop are available here. The views expressed in individual presentations do not necessarily represent those of the CERN management.")

Here's the press release and message from Rolf Heuer:

> Many issues were tackled in Chamonix this week, and important recommendations made. Under a proposal submitted to CERN management, we will have physics data in late 2009, and there is a strong recommendation to run the LHC through the winter and on to autumn 2010 until we have substantial quantities of data for the experiments. With this change to the schedule, our goal for the LHC's first running period is an integrated luminosity of more than 200 pb-1 operating at 5 TeV per beam, sufficient for the first new physics measurements to be made. This, I believe, is the best possible scenario for the LHC and for particle physics.
>
> There were discussions in Chamonix between accelerator and detector physicists on several important issues. Agreements were reached whereby teams drawing from both communities will work together on important subjects, such as the detailed analysis of measurements made during testing of magnets on the surface.
>
> Since the incident, enormous progress has been made in developing techniques to detect any small anomaly. These will be used in order to get a complete picture of the resistance in the splices of all magnets installed in the machine. This will allow improved early warning of any additional suspicious splices during operation. The early warning systems will be in place and fully tested before restarting the LHC.
>
> Another important topic for the future was the radiation hardness of electronics installed in the service areas and the tunnel. For many years, particle detector electronics have been designed to cope with events such as loss of beam into the detectors. Until now, this has not been necessary for the accelerators, but will become so when the LHC moves to higher beam intensity and luminosity. Again, with detector and accelerator physicists working closely together, the experience gained from the detectors can be applied to the LHC itself.
>
> As the Bulletin reported on 30 January, opening up a magnet in which an anomalously high electrical resistance was measured made the reason for the anomaly immediately obvious – a splice had not been correctly made. This is one of two such splices that were identified in the five sectors tested, and as a result the magnet containing the second will also be removed from the tunnel for repair. Since resistance tests can only be conducted in cold magnets, three sectors remain to be tested: sector 3-4 where the original incident occurred and the sectors on either side. Within sector 3-4, the 53 magnets that are being replaced in the tunnel will all be tested before cool down, and the sectors either side will be cooled down early enough to intervene if necessary with no impact on the schedule. This leaves around 100 dipole magnets that we'll not be able to test until September and a correspondingly small chance that we may find further bad splices that will need to be repaired before operation.
>
> The Chamonix workshop involved a lot of work by many people. Much progress has been made, and the management now has all it needs to make an informed decision next Monday on LHC restart. I'd like to thank all those involved, and I will be writing to you again early next week to let you know our decision.

Looking at a few of the slides, it seems that the schedule for work this year has slipped, with the current plan that the machine will be cold in August, checkout in September, with powering tests in Sector 34 taking place in parallel with the checkout, which will end the third week of September. Beam commissioning will not be able to start until then, and the assumption has been that it would take two months to commission the beam and begin collisions for physics. If first collisions are not until late November, it's clear why they want to run over the winter. The main consideration evidently is cost, they will have to come up with 8 Million Euros more for more expensive power.

This assumes not all sectors are warmed up. Warming them all up to install the quench protection they would like would add another 5 weeks to the schedule.

Update: CERN has a press release today confirming the new schedule:

> The new schedule foresees first beams in the LHC at the end of September this year, with collisions following in late October. A short technical stop has also been foreseen over the Christmas period. The LHC will then run through to autumn next year, ensuring that the experiments have adequate data to carry out their first new physics analyses and have results to announce in 2010. The new schedule also permits the possible collisions of lead ions in 2010.

The decision was made to go ahead while installing additional relief valves in the four sectors that have been warmed up, leaving installation in the four remaining sectors for next year.

Update: There was a talk today at CERN by Lyn Evans on LHC status and future plans. The current plan for upgrading the LHC luminosity involves a "Phase I" in 2013 that would double the luminosity, and an upgrade of the accelerator complex that would be completed in 2017 and allow further luminosity increases.

Comments

1. **zzz**
   February 6, 2009

   With the economic recession, factories running at lower capacity, you would think electricity would be cheaper this year.

2. **Ari Heikkinen**
   February 10, 2009

   So they have no results, it's not even running and they've not even proven the machine even works and they're already talking about upgrades? I guess that's one way to secure tax money..

3. **Peter Woit**
   February 10, 2009

   Ari,

   The lead time to design and build this kind of upgrade is very long, so you need to do this in parallel if you want the upgrade to happen in the next decade. I'm sure the great majority of people's time and money is now going into getting the LHC to work, with the upgrade on the back burner and going nowhere unless the LHC project itself is successful.

4. **DB**
   February 11, 2009

   I just looked at the "Maximum Credible Incidents" and "Worst case beam incidents and protection" presentations, neither of which seemed to offer much reassurance against repeats of the September incident or possibly worse events.
   But at least the discussions are out in the open.

5. **Mikael**
   February 13, 2009

   Find here a direct link to the video of Lyn's talk:
   http://cdsweb.cern.ch/record/1160614/

6. **Steven Colyer**
   February 15, 2009

   Speaking as an engineer with 30 years of "real-world" understanding of Applied Experimental Physics and NOT asking for forgiveness regarding my skepticism, I can forsee a tremendous amount of setbacks for the LHC coming on-line before 2009, maybe 2010 and 2011 is more realistic, AND they will not like the results they get when so, when Supersymmetry and the Higgs Boson are found NOT to exist.

   I will not engage in speculation and rumor that there is a massive cover-up that they did indeed ran an experiment to conclusively prove the Higgs exists, but it failed, and this is all a massive cover-up. Conspiracy theory is not my thing; this set-back was a real one IMO. A machine as large as the LHC is going to have massive problems, this latest setback will be far from the last one, I'm afraid.

   Speaking as a lover of Science and Physics, it amazes me that Physicists have allowed Mathematicians (string Theorists) to take over their field, save for the good work by Woit, Smolin, et al., congrats gentlemen, people are listening. The analogy I like to make is you don't call a Mechanic a Wrench or a Screwdriver, those are the tools he (or she) uses.

   Time is NOT a 4th dimension of space, it is its own thing. Treating it mathematically as a 4th dimension does help though.

   I do not believe Gravity is a force either, so I do not believe in the Graviton. However treating Gravity AS a force (and therefore the theoretical graviton) does make the equations work out nicely.

   Gravity is a unification of matter and geometry. Einstein showed us the HOW of Gravity in General Relativity; you good people are working on the WHY. And in that regard, there is a heck of a lot more work being done at Waterloo's Perimeter Institute and other places than in America, which is sad. Check out "Quantum Graphity" and "Causal Dynamical Triangulations" where the most sense IMO is being done, not Strings.

   I DO believe in Quantum Mechanics as well as the Standard Model. QM may be strange but it needs to be explained better. Begin with Uncertainty, which makes all the sense in the world if you consider the Electron (and everything else) to be a "particle" in the sense that it's a bundled up wave, nothing more. Treating the Electron as a 0-dimensional "point" particle may make the equations work out nicely, but they're not real. All I ask is: Get Real.

   We and the Universe are basically 5 things: Up quarks, down quarks, gluons, photons, and electrons. Good luck figuring out how they came to be … well that's you job, isn't it? And my and the public's primary interest.

Money For Everything

February 12, 2009
Categories: Uncategorized

It now appears that the final US stimulus bill will include very large amounts of spending on scientific research. See here for a copy of the conference agreement. It has \$3 billion for the NSF, \$1.6 billion for the DOE office of science, and \$1 billion for NASA. These amounts are to be spent on top of the regular budgets (about \$6 billion for NSF, about \$1.6 billion for DOE office of science, as well as $400 million for ARPA-E, and \$17 billion for NASA). Basically, the government agencies responsible for funding math and physics research are receiving a one-time influx of money, of order half their annual budget, to be spent as quickly as possible. It will be very interesting to see what they do with it…

Update: More here.

Comments

1. **Ryan Dickherber**
   February 12, 2009

   First it was billions, then it was zero, now it is billions again. The bill has still not actually passed. Dare we be excited yet?

2. **Peter Woit**
   February 12, 2009

   Ryan,

   It wasn't ever zero, although the Senate version had much less in it for science than the House version. Whatever comes out of the House-Senate conference will be the final bill, and should pass. The document linked to and information released by Pelosi's office claims these are the numbers in the conference version, but the bill has not been completely finalized. So it remains possible that things will change, but it looks most likely now that this is what is going to pass.

3. **Yatima**
   February 12, 2009

   Yeah well, the first thing to do is transform the cash into something other than USD in order to have a safe stash when inflation decides to go hyper. Otherwise, excellent news.

4. **Alp**
   February 12, 2009

   Maybe someone should distribute this money to researchers as "bonus" before most of them leave to become quants.

5. **drunk**
   February 13, 2009

   Just because they put a budget together does not automatically mean there is money. US federal gov is already in technical bankruptcy, deep in deficit and debt. Even the interests must be borrowed. Every cent of this budget must be borrowed, or printed out of thin air. Or taxed. Are they going to tax people another $1T? Are there any more foreigners stupid enough to loan the US gov one more dollar after being screwed big time by Wall Street and see their own economies crashed by the spread of the US the financial crisis globally? So the Federal Reserve is going to print another $1T, after printing $2T last year, thus inflate the economy and reduce the worth of all USD by the same amount. This budget is a lot of smoke folks.

6. **Coin**
   February 13, 2009

   Ryan: The House has passed the conference version. The Senate votes at 5:30 eastern time.

7. **Peter Woit**
   February 13, 2009

   All,

   There are a thousand places on the internet now full of people having the same unenlightening discussions about economics. Please don't do the same thing here. If it's not specifically about math/physics, it doesn't belong here.

8. **Coin**
   February 13, 2009

   So, I am incredibly relieved to see that this funding made it back into the final bill. I wonder, though: It seems like I've been repeatedly told that between funding tricks, "continuing resolutions" and failure to correct for inflation, the budgets for these science funding agencies have been basically effectively falling for years. So we give them a 50% one time boost from their *current* budget– where does that put them in relation to the budget they really ought to have if science budgets hadn't been getting shafted lately?

   Something I find interesting, from that link it looks like the NIH gets nearly 2/3 of the additional science funding, like ten billion dollars altogether. Is this interesting or does it just reflect that the NIH has a larger grant budget to begin with?

9. **Peter Woit**
   February 13, 2009

   Coin,

   It's just not accurate to say that science funding has been falling for years. For the NSF, take a look at

   http://www.aaas.org/spp/rd/nsfauth09p.pdf

Ignore the 2009 data point, which corresponds to an increase for physics that didn't happen,

Total math + physics NSF funding has been basically flat (in constant dollars) for many years. From what I remember math has seen some increases, HEP has decreased in constant dollars. The problems of HEP are somewhat specific to the field, not indicative of the overall science-funding pattern.

The NIH is much bigger than the NSF, spending about $30 billion on medical research vs. the NSF $6 billion. This is not surprising. Voters are much more interested in figuring out how to avoid a painful death from disease than in understanding the origin of electroweak symmetry breaking.

10. **chickenbreeder**
February 14, 2009

The money distributed through the stimulus bill has to be spent within 120 days (the point being to give the economy a jolt.) This means it will not benefit people who are just about to submit a proposal to NSF. The money will likely be used to fund proposals that have already been reviewed and deemed "fundable if funds are available". If you have one of those in the hands of your NSF program manager this may indeed be a god-sent. Otherwise, it's not that useful. I believe this is also the situation with other basic research programs at NASA, DOE, etc.

11. **grant pending**
February 14, 2009

chickenbreeder, where did you see that, and how exactly does it (120 day time limit) apply to NSF?

12. **grant pending**
February 14, 2009

Okay I found one, though it's a month old, and may not be up to date.

http://www.technologyreview.com/blog/editors/22510/

13. **Steven Colyer**
February 15, 2009

How much of the $$$ do you think will be pumped into even stronger battle-ready infra-red lasers at Sandia or DARPA, not that any of us will ever know of it?

14. **mike**
February 15, 2009

So it doesn't seem to be mentioned here, but is there some consensus that the money to DOE and NSF will then stimulate QFT or mathematical physics research? Will string theory be revived from this (similar to how an infusion of steam causes bacteria to grow faster)?

15. **Peter Woit**
February 15, 2009

mike,

I don't think anyone knows yet how DOE and NSF plan to spend the money. I've heard speculation that one thing that may happen is that they'll be funding a higher proportion of grants, i.e. grants that would have not quite made the cut under the regular budget now will be funded. If this is true, all subfields will see more grants, but this shouldn't change the relative distribution of resources among different topics.

So, one guess would be that the stimulus package will mean more postdocs (which are funded by grants) in all subfields, including string theory. Tenure-track jobs may be a different story, since they are typically funded by university budgets, not grant money. I don't see any reason for the recent trend of physics departments wanting phenomenologists and cosmologists rather than string theorists and mathematical physicists to change.

16. **hungry for updates**
February 28, 2009

(bump)
News? Rumors? Anything?

17. **Peter Woit**
February 28, 2009

A more recent posting gives the FY2009 numbers that have just been decided on, and the overall FY2010 number proposed for NSF.

Basically NSF and DOE now have a lot more money to spend than usual. As far as I can tell, they still haven't decided what to do with it. The job situation is bad, so I hope they figure out how to fund some positions for young people for the next academic year.

The Landscape for Undergraduates

February 14, 2009
Categories: Multiverse Mania

I was in a local Barnes and Noble today, and noticed that there's a new, second edition out of Barton Zwiebach's A First Course in String Theory, which is the textbook for MIT's course 8.251 String Theory for Undergraduates. The new addition includes a 10 page section explaining the details of how to compute numbers of vacua in the landscape based on flux compactifications, and arguing that this provides an explanation of the value of the cosmological constant. Landscape ideology has now made it to the undergraduate level.

However, this only seems to be the case at MIT. A couple years ago I wrote about undergraduate string theory courses here, noting that there was an increasing trend to offer them, with MIT, Caltech, Stanford and Carnegie-Mellon providing examples. Recently this seems to have turned around, with Caltech, Stanford and Carnegie-Mellon not offering such a course this year. Somehow I don't thinking adding coverage of the landscape to the textbook is going to encourage physics departments to teach this material to undergraduates.

Comments

1. **Shantanu**
   February 14, 2009

   Are there any published reviews for this book? Also how was Vafa's
   colloquium?

2. **Joseph B.**
   February 15, 2009

   Hi PW, I feel obliged to mention that at least one of those undergrad string theory courses that you mentioned 2.5 years ago was meant to be an `experiment' in whether or not such a string theory course is feasible.

   It was initiated by undergraduates who were excited by Zwiebach's book and ended up being pushed through despite the reservations of some of the hep-th faculty. Ultimately the course evolved into a de-facto string-theory-for-grad-students (i.e. a proper string course) taught out of Polchinski and the experiment as an undergrad course was generally deemed a failure. I believe the department hasn't bothered removing the course title from their catalog, even though the course has not been offered since.

   So to be fair to [at least one of] the physics departments you mentioned, it's not necessarily that the departments thought "Hey, let's teach our undergrads watered-down-strings," but rather that there was a lot of student interest and the department was willing to give it a try. In fact, the biggest proponents from the faculty were non-theorists who were working with undergrads to explore possible innovations to the undergrad curriculum.

   I just wanted to explain that the offering of such a course in the past should not [necessarily] be interpreted as representative of some deeper ideological agenda so as not to give the wrong impression of my old department… especially when it was largely an experiment initiated by naive (but enthusiastic) undergrads rather than the powers-that-be.

3. **Peter Woit**
   February 15, 2009

   Shantanu,

   This was the first I had heard of the second edition. One review of the first edition was in the September 2005 Physics Today.

   Vafa's colloquium talk made a lot of dramatic claims, including putting up a detailed spectrum of what supersymmetric particles the LHC should see at what masses. But since it was a colloquium talk he gave few details, and nothing at all about the crucial question of how supersymmetry gets broken. It gave me a better idea of the general philosophy of what he is trying to do. They're assuming particle physics and quantum gravity are decoupled, giving up on the idea that the properties of which compactification Calabi-Yau you choose have any effect below the Planck scale. Particle physics then comes from the choice of various branes, and I guess his claim would be that the consistent possible choices of branes give some testable predictions. The lack of detail in the talk made it hard to tell to what extent this is true. I took a look at one of his papers that features the particle spectrum he showed. There seemed to be a lot of assumptions going into that plot that were not mentioned in the colloquium talk.

   Joseph B.,

   Thanks for the explanation. It has been my experience over the years that much of the enthusiasm for string theory comes from students and non-theorists whose knowledge of the subject comes from the large number of popular books, articles, TV programs, colloquium talks, etc. about string theory that hype the subject and give a misleading impression about it. Maybe someone should write a book trying to correct that and provide a more accurate description of what is really going on with the subject…

4. **Robert**
   February 15, 2009

   Thanks for informing about the second edition of this nice and usefull book.
   Yes, the landscape is mentioned, as of course it should in a book about strings, but the main change in the more than 100 additional pages seems to be contained in three new chapters about

   A look at relativistic superstrings

   String theory and particle physics

   Strong interactions and AdS/CFT

   Great that this accessible text is prevented from outdating so soon in this rapidly moving field.

5. **capitalistimperialistpig**

February 15, 2009

I'm an old physicist with no expertise in particles, gravity, or strings and I read Zwiebach I with great enthusiasm. It's development is exceptionally clear, too slow for theorists I imagine, but just right for me. There are lots of accessible (OK, easy) problems. By contrast, I found Polchinski rather impenetrable – it assumed too much that I had either forgotten or never knew. I think it's a great book for someone like me who has no designs on being a string theorist but would just like to understand some of the concepts beyond the popular level.

Its singular feature is that you don't need a sophisticated knowledge, or indeed any knowledge, of QFT – and if you don't have that you will learn a little QFT in reading it.

I'm sure its not a course for every undergrad, but it might be a worthwhile option for those who can't wait to learn QFT and GR first.

6. **woit**
February 15, 2009

CIP,

The book seemed to me a good place to start for someone trying to learn how to quantize a string (except for the part about how to get particle physics out of string theory, which is fundamentally misleading: why go on like that about an idea that doesn't work in a textbook aimed at impressionable undergraduates?).

My problem is not with the book, but with the idea that a course based on it makes any sense as part of a regular undergraduate curriculum. Seems to me that students should be encouraged to learn QFT and GR first. There's a long list of complicated speculative ideas that haven't worked out very well that don't belong in an undergraduate curriculum no matter how fond some theorists are of them or how much publicity they have gotten. By putting a subject in in the undergraduate curriculum, we implicitly say to students that this is a solid and important part of our field. There's nothing in the Zwiebach book that would make clear to a student that this is material of a completely different nature than what they are getting in the rest of of their courses.

7. **somebody**
February 15, 2009

Peter, we are talking about ambitious, smart undergraduate physicists who are eager to see whats up at the frontier. Do you seriously think that these kids don't have the cojones to see the pros and cons of string theory at the superficial level that it is discussed on this blog?

"Indoctrination of the youth" works a lot better at an even lower level. Who watches out for the far less prepared readers of your blog (or Kaku's books for that matter), who have to take your word (which I am sorry to say, is often misleading)?

8. **woit**
February 15, 2009

somebody,

Well, undergrads with adequately-sized cojones can read the many sources of string theory hype and this blog, then make up their own minds. From what I've seen they're doing that, and making a different judgment than you about who is being misleading. Women students are doing this too, same result…

9. **capitalistimperialistpig**
February 15, 2009

Peter,

I don't actually disagree, nor do I necessarily recommend it to undergrads, though come to think of it, I did recommend the course to my then undergrad son, who took it. So far as I can tell, it didn't do him any more damage than the rest of the physics curriculum – at least he didn't become a string theorist.

10. **somebody**
February 15, 2009

Peter, the point was that you started out implying that the students are being taken for a ride by this course. Now you agree with me that they are perfectly capable of making up their own minds about this.

I have no further comments if you are not going to change your mind again. 🙂

11. **woit**
February 15, 2009

somebody,

We've only agreed about the large-cojoned and female undergrads, there still are the ones with small cojones to be concerned about…

12. **Coin**
February 16, 2009

Joseph B.: Out of curiosity, would it be particularly unusual for a motivated undergrad to simply enroll in a grad-level string theory course?

(In CS at least it seems to be considered no big deal for a fourth-year undergrad to take a low-level grad course, but I don't know how things work in physics departments…)

13. **CWJ**
February 17, 2009

"Seems to me that students should be encouraged to learn QFT and GR first."

They are–at least, when I was graduate advisor in my physics department, and the current grad advisor deploys the same advice–but they don't want to hear this. Especially the ambitious but less talented ones. The smarter students are much more flexible and more willing to listen to good advice, I find.

Too Many Topics

February 15, 2009
Categories: Uncategorized

Last Friday at the KITP there was a celebration of Stanley Mandelstam's 80th birthday, with talks available here, and some messages from other physicists here. Geoffrey Chew recalls how Berkeley hired Mandelstam away from Columbia, where no one was very interested in what he was doing, in 1958. The next year the same thing happened with Steven Weinberg…

Recently I've noticed two books on a narrow topic not of general interest, but perhaps of interest to readers of this blog: histories of US math departments. They are:

- Recountings: Conversations with MIT Mathematicians, about MIT.
- Mathematics at Berkeley, about Berkeley.

Perhaps of even more esoteric interest, later this year Princeton University Press will publish Mathematicians: An outer view of the inner world, a book of photographs of mathematicians by Mariana Cook. Some of the photographs are available at her web-site here.

Via Ars Mathematica: Fulton's Algebraic Curves is available for free online. It's a good place to start if you're looking for a challenging introduction to algebraic geometry at the (quite) advanced undergraduate level.

Some wonderful expository pieces about areas of mathematics:

- Ben Webster on higher categories and knot homology.
- Vaughn Jones on operator algebras and TQFT.
- Cliff Taubes on Seiberg-Witten Floer homology, in a review of the recent book by Kronheimer and Mrowka.

The AMS Notices has an article about the current state of every mathematician's favorite tool: TeX.

Les Houches this year will have a summer school devoted to lattice gauge theory.

For the latest on the question of whether the Tevatron will manage to see the Higgs or rule it out, see excellent postings by Tommaso Dorigo here and here. The bottom line is that by the time the LHC has enough data to start saying something about the Higgs, the Tevatron experiments will have over $10fb^{-1}$ of data to analyze, which may, if improvements in their analysis work out, give them a two-thirds chance of seeing the Higgs at 2 sigma level over the entire expected mass range, or a 50/50 chance of seeing it at 3 sigma level over a large range, including a small range just above the 114Gev LEP limit. The Tevatron may remain very competitive with the LHC for some signals far longer than people have been expecting. And, at least for the next 18 months, the US stimulus legislation may make Fermilab better funded than CERN for a change…

I fear I've been remiss about not reporting on the IHES Grothendieck conference that I attended a couple days of when I was in Paris last month. Luckily, there's a new blog here with a report.

Protests and strikes in France over Sarkozy's attacks on the French scientific research system continue, see an English language report here. Some people may have misunderstood my previous mention of this. While it's not a topic I'm well-informed about, Sarkozy's argument in favor of moving to something supposedly more American, featuring a market-based, no central government regulation ideology has an obvious problem if you've been reading the newspapers.

Update: Video of the IHES Grothendieck talks is available here.

Comments

1. **Manel**
   February 16, 2009

   « Fulton's Algebraic Curves is available for free on-line.»

   It's been on-line for quite some months now.

   «It's a good place to start if you're looking for a challenging introduction to algebraic geometry at the (quite) advanced undergraduate level.»

   Is this really advanced undergraduate? Is it a good place for someone interested into Physics to learn AG from?

   It thing Griffith & Harris is probably more suited for a physicist due to it being more intuitive.

   Does anyone have suggestions of good AG introductions for physicists?

2. **Aaron Bergman**
   February 16, 2009

   I think G&H is probably the way to go. It stays away from the Zariski topology which I generally think is a distraction for physics applications.

3. **Peter Woit**
   February 16, 2009

   Manel,

   It's not really an appropriate book for physicists. You have to realize that a large part of algebraic geometry is commutative algebra, since mathematicians want to do geometry over different fields. not just over the complex numbers. Fulton does the algebraic part of the story seriously, but to follow this you need to have at a minimum a serious course in abstract algebra, which physicists normally don't.

   Physicists generally are only interested in geometry over the real and complex fields, and often general manifolds, not algebraic varieties. For them, Griffiths and Harris is good, since it approaches algebraic geometry from the point of view of complex manifolds. For something at a more undergraduate level, sticking to complex manifolds, but more along the lines of algebraic geometry, you could try Frances Kirwan's "Complex Algebraic Curves".

Another way to explain the difference in point of view is that, for one complex dimensional spaces, physicists mostly want arbitrary Riemann surfaces, but algebraic geometers are mostly interested in algebraic curves. There's a big overlap of the two subjects, but physicists rarely care about the algebraic part of it, whereas for mathematicians this is a crucial part of the subject.

4. **Javier**
   February 17, 2009

   I have just made a blog entry on the subject of algebraic geometry and physic (specifically string theory), concretely tis:

   http://freelance-quantum-gravity.blogspot.com/2009/02/nvitation-to-algebraic-geometry.html

5. **Manel**
   February 17, 2009

   Peter and Aaron Bergman, thank you for your elucidating explanations.

6. **milkshake**
   February 17, 2009

   Onion-caliber news: Tom Hanks has been invited to push the button on the repaired LHC. Hanks got that part because of the upcoming Hollywood thriller – where Hanks averts the plans for blowing up Vatican with 250 milligrams of antimatter pilfered from CERN. The movie was filmed at CERN and among other things it also feature the antimatter sinister canister and some CERN people were involved in advising the movie crew so that they get the Pontiff-blowing science right.

   http://www.nature.com/news/2009/090216/full/news.2009.104.html

7. **Coin**
   February 17, 2009

   milkshake: That is… odd. Especially given that CERN actually still has up on their website a fun but somewhat testy document talking about all the inaccuracies in the Angels and Demons book…

Mission Accomplished

February 18, 2009
Categories: This Week's Hype

A few years ago the asset value of string theory in the market-place of ideas started to take a tumble due to the increasingly obvious failure of the idea of unifying physics with a 10/11 dimensional string/M-theory. Since then a few string theorists and their supporters have decided to fight back with an effort to regain market-share by misleading the public about what has happened. Because the nature of this failure is sometimes summarized as “string theory makes no experimental predictions”, the tactic often used is to claim that “string theory DOES make predictions”, while neglecting to explain that this claim has nothing to do with string theory unification.

A favorite way to do this is to invoke recent attempts to use conjectural string/gauge dualities to provide an approximate calculational method for some strongly coupled quantum systems. There are active on-going research programs to try and see if such calculational methods are useful in the case of heavy-ion collisions and various condensed-matter systems. In the heavy-ion case, we believe we know the underlying theory (QCD), so any contact between such calculations and experiment is a test not of the theory, but of the calculational method. For the condensed matter systems, what is being tested is the combination of the strongly-coupled model and the calculational method. None of this has anything to do with testing the idea that string theory provides a fundamental unified theory.

The yearly AAAS meeting is the largest gathering where scientists present results to the press and try and draw attention to recent scientific advances. This year’s meeting was held over the past weekend and featured a program Quest for the Perfect Liquid: Connecting Heavy Ions, String Theory, and Cold Atoms. While the presentations were largely a serious attempt to explain this area of research to the public, the fact that this has nothing to do with string theory unification somehow doesn’t seem to have been mentioned, with the result one would expect. The program was reported on under the headline A first: String theory predicts an experimental result, with the story beginning:

> One of the biggest criticisms of string theory is that its predictions can’t be tested experimentally–a requirement for any solid scientific idea.
>
> That’s not true anymore.

Another report entitled A prediction from string theory? at Physics World starts off:

> Skeptics find much to complain about in string theory, but perhaps their most stinging criticism has been its inability to be falsified by experiment. A few years ago, one string theorist even told me that a particle accelerator big enough to “see” a string would be so large that its opposite ends would be causally disconnected. So this is not a problem we’ll be solving any time soon.
>
> Yet even if we’ll never see a string in the lab, it turns out that string theory does make a few predictions about how matter should behave at the quantum level…

The dramatic news that claims that string theory can’t be tested have been refuted was then spread widely by Digg, so much so that the Symmetry Magazine site featuring the story crashed. The discussion on Digg showed what got through to the public from the efforts of the scientists involved:

> Without a testable hypothesis it was only a String MODEL. Now we truly have a String Theory.
>
> Michio Kaku just had an orgasm.
>
> Brian Greene’s next book will be titled “Told You So Bitches!”

The one string theorist involved in all this was Clifford Johnson, who gives a minute-by-minute description of his participation here. It ends by invoking the phrase made famous by the last US president:

> Mission accomplished. (Hurrah!)

Update: There a better story on this at Ars Technica, which avoids the misleading “test of string theory” claim.

Update: Another story about this is Experimenting With String Theory?, where the author for some reason also missed the fact that this has nothing to do with unification, writing:

> So there you have it: finally, a potential concrete way to experiment with the predictions of string theory. But I’ll let the expert say that:
>
> “This is the first time string theory can help experiments,” Johnson said. “We haven’t proven string theory, but have found a place where string theory has been a modest guide and making testable predictions.”

Another string theorist has a long blog entry about this here, where the punch-line is:

> And it is just manifestly wrong to say that the lab tests of the predictions of AdS/QCD or AdS/CMT have nothing to do with string theory’s being the unifying theory of gravity and other forces and matter, or a theory of everything, if you wish. They have everything to do with it.

Update: Chad Orzel has sensible things to say about this here, in the context of a more general debate about the role of science journalists. In the comment section Moshe Rozali’s comment I suspect reflects the feelings of most string theorists about this:

> As for the specifics of your example, I would comment on it, but I decided to go and extract my own wisdom tooth instead. I think that would be much more fun.

Comments

1. **Stephen**
   February 18, 2009

   I know a few condensed matter theorists that are very interested in dualities, and a friend of mine is doing his thesis work technically in string theory, but in trying to work out a QCD dual (his five word summary, it’s obviously much more involved). In a sense, the ability to probe strongly coupled systems using ideas from string theory is a big step, since prior to the advent of duality, from the best of my studying, it was mostly ad hoc attempts to tackle the limit. It’s also opened up a lot of avenues of intellectual thought, and brought a lot of nice connections with mathematics.

   That said, their PR ranges from mumbles of confession that it’s probably not good for anything to downright intellectually dishonest claims to predictions that are a consequence of a formalism more than for a unified theory.

2. **A.J.**

February 18, 2009

Peter,

Clifford said explicitly that what he's doing doesn't constitute a test of string theory. First linked article, 5th & 6th paragraph. "string theory testable?" is a hook introduced by the science journalist. It's a sleazy rhetorical tactic, but that's show business for you. I don't see this is any worse than suggesting that honest research like AdS/QCD is just a PR tactic carried out on behalf of some unnamed "string theorists and their supporters."

3. **Peter Woit**
February 18, 2009

A.J.,

I in no way suggested that "AdS/QCD is just a PR tactic carried out on behalf of some unnamed "string theorists and their supporters"" and I really resent that accusation. You know very well that's not what I think and that's not what I've ever written. I've no problem with work on AdS/QCD. Some of it's very good, some of it's over-hyped, like any other scientific field. But I am critical of people who go to the press promoting this work in a way that they know full well is misleading. If you've spent twenty-five years going to the press promoting a speculative idea about string theory as a fundamental theory, when you go to them about a completely different use of string theory it's part of your job to explain that they're different things. The quote from Clifford that you mention doesn't do that.

The first story I linked to has a note from the author that explicitly thanks Clifford for helping her fix inaccuracies in the original version of the piece that she posted. If he had a problem with the whole "finally string theorists find a way to test their theory, despite what critics say" story line, he could have had that inaccuracy fixed.

Look, the many bogus stories about "tests of string theory" that have appeared in recent years are not innocent mistakes caused by journalists who, despite the best efforts of string theorists, are so stupid they can't understand what they are told, and so pig-headed that they insist on putting misleading headlines and lead paragraphs on their stories.

4. **A.J.**
February 18, 2009

Hi Peter,

Sorry for the bombast. I hope you're not too offended. I was rather annoyed by the blunt contradiction between the quotes from Clifford and your claim that string theorists weren't making it clear that use of string theory in heavy ion physics is not related to any uses string theory might have in very smale scale physics.

I think we'll have to agree to disagree about the degree to which string theorists are to blame for the misleading headlines. I should clarify my position a little though: I don't think these are innocent mistakes on the part of the journalist. I don't think the journalists are that dumb. I _do_, however, think that journalists write these sorts of headlines, knowing that they're misleading, because the hype sells the article. "Aspects of string theory useful in heavy ion physics" just isn't going to attract as many diggs.

5. **Peter Woit**
February 18, 2009

A.J.,

Sorry, my reading of the two paragraphs you point to is rather the opposite. Acknowledging that this work does not prove string theory unification isn't the point. Instead of just stating that the research under discussion has nothing to do with the string theory unification, Clifford is claiming that it does (using the logic: "we don't understand string theory, maybe comparing AdS/CFT-motivated approximations to experimental results in heavy-ion physics will help us understand string theory, and once we understand string theory, we'll see how to do string theory unification"), He's welcome to that bit of wishful thinking, but when he uses it on non-experts in the way quoted, it's not at all surprising that what they take away is the message that string theory unification is moving forward due to this first connection between string theory and experiment.

One thing about having a blog is that if something you have to say gets seriously misinterpreted, you can correct the record. We'll see if that's what Clifford chooses to do.

6. **somebody**
February 19, 2009

As a one-liner, "RHIC might test string theory" is okay I think. But it certainly comes with the caveat that it is not a test of string theory *in the context of unification*.

But your version " … the research under discussion has nothing to do with the string theory unification…", is wrong-er in the other direction. When you say that string theory is merely a "calculational method" for heavy ions, you are profoundly misrepresenting the problem. Nobody knows how to get the stringy "method" starting only with the field theory. The string theory used here is the same one that originated from attempts at unification, applied in a different context, through Maldacena's gauge-gravity duality. You are essentially saying that gauge-gravity duality is merely a computational tool.

As I have said repeatedly, you adamantly refuse to recognize the UNDERSTANDING we have gleamed through string theory, while knocking it for the lack of experiments. At RHIC, what people are trying is precisely to put this understanding to work and actually get some low energy experiments out. This is another example where thinking about the (experimentally inaccessible) Planck scale, opens up new understanding about *low energy physics* and makes previously unanticipated experimental tests a possibility. Indirectly this could give us confidence in our ideas about the Planck scale.

Finally, try not to be so negative. In your hurry to attack string theory, you seem to have lost sight of how amazing this RHIC/ALICE thing actually is. 🙂

7. **Gil Kalai**
February 19, 2009

"…any contact between such calculations and experiment is a test not of the theory, but of the calculational method."

This is not entirely true. Successful applications of ST calculations in other areas can be regarded as a (weak) support for the theory itself. The boundaries between "calculational methods" and "conceptual understanding" are often not clear cut.

8. **Peter Woit**
February 19, 2009

somebody,

I regularly report on the new understanding that progress in learning about string theory has led to. For particle theory, that new understanding is the existence of the landscape, and the inherent uselessness of string theory as a theory of unification.

I know very little about heavy ion physics, but I have to admit that I find it pretty funny to see the way string theorists now go on about how certain calculations in the subject are one of the most exciting developments in physics. Way back when (mid-eighties), I did some finite-temperature lattice QCD calculations (all this means is that you look to see what happens when one of the dimensions is smaller than the others). This supposedly was of relevance to the heavy ion experiments then being planned. I remember what most string theorists attitude at the time was about non-perturbative QCD calculations of possible relevance to heavy ions. As far as they were concerned, such a subject was about as not-exciting as could be imagined. Things have changed…..

9. **somebody**
February 19, 2009

Peter says: "I did some finite-temperature lattice QCD calculations ….. I remember what most string theorists attitude at the time was about non-perturbative QCD calculations of possible relevance to heavy ions. As far as they were concerned, such a subject was about as not-exciting as could be imagined."

Peter, I understand your frustration (we have all been there when working on unfashionable things), but I think the reason for this is scientific. The properties of heavy ions become of *conceptual/theoretical* ineterest when we are using something absolutely unprecedented (black holes) to attack the problem. Lattice approaches, while tremendously useful and important, did not require such a new theoretical perspective. The interesting message from the viewpoint of fundamental theory here is not the heavy ions themselves, but that heavy ions are telling us something about the two basic structures in physics: gauge theories and gravity.

10. **Peter Woit**
February 19, 2009

somebody,

The story about my past was about something that amuses me now, and has nothing to do with frustration, then or now. 20-some years ago I was working on doing calculations of topological invariants of lattice gauge fields and running Monte-Carlo simulations. I knew nothing about heavy-ion physics, and it wasn't a topic that interested me (there are lots of topics in science the study of which I have sufficient respect for to know I don't want to put in the time required to make a contribution to them). The fact that string theorists weren't interested either wasn't to me at all remarkable. Seeing the way some of the same people I knew then now go on about the subject now is pretty funny.

The one constant seems to be the level of hype being spouted. Back then it was hype about a grandiose, fundamental, mathematically revolutionary theory that would provide a unified theory of everything, now it's hype about an "absolutely unprecedented" crude approximation technique to predict the viscosity in a quark-gluon plasma. Perhaps you can see the humor…

11. **CWJ**
February 19, 2009

"but that heavy ions are telling us something about the two basic structures in physics: gauge theories and gravity."

That is sheer nonsense. That's like claiming studying eigenmodes in, say, bridges tells us something about quantum mechanics which also uses eigenmodes.

Duality is a tool, like eigenmode analysis, and if it proves useful in heavy ions, that gives one confidence that it may prove a useful tool in other areas. But successful application of duality to heavy ion physics does not mean that it validates, in any way, the underlying picture of string theory–only the usefulness of the tool.

If you wonder why Peter, and much of physics, is suspicious of string theory, it's exactly because of overreaching statements like this.

12. **somebody**
February 19, 2009

CWJ: "That is sheer nonsense. That's like claiming studying eigenmodes in, say, bridges tells us something about quantum mechanics which also uses eigenmodes."

Lets ignore the over-assertiveness.

About the analogy: in this case it does not work. Because if eigenmodes were difficult to access experimentally (which is the situation with string theory), it makes perfect sense to study them where we can, to see what we can learn. Eigenmodes are a really bad example in this context, because they are one of the easiest objects to do experiments with: remember that almost everything we know about physics comes from perturbation theory around the harmonic oscillator eigenmodes of systems.

Peter, no need to clarify, I wasn't implying that you were "frustrated" in any profound way.

13. **CWJ**
February 19, 2009

somebody: No, eigenmodes are a really good example, because it illustrates the confusion of mathematics with physics–which is what you were doing.

"remember that almost everything we know about physics comes from perturbation theory around the harmonic oscillator eigenmodes of systems"

Not true (although a narrow view typical of particle theorists). Perturbation theory is a very useful tool, but it's not the only tool. For example, in my field, nuclear structure, there are many people whose entire career revolves around applying group theory to everything. They couldn't do perturbation theory to save their lives, but they have productive, meaningful careers that teach us a lot about physics.

14. **somebody**
February 19, 2009

The whole reason why research is often on non-perturbative stuff is BECAUSE we understand things well when perturbation theory is valid. This is your second example where you are confusing cause and effect.

Also, asseriveness is cool, but substance is better. I have tried to be polite, but your hostility towards the (perceived) top of the foodchain is really showing.

15. **cwj**
February 19, 2009

somebody: "The whole reason why research is often on non-perturbative stuff is BECAUSE we understand things well when perturbation theory is valid. This is your second example where you are confusing cause and effect."

Umm… no.The reason research is carried out on applying group theory to physics is because nature seems to exhibit deep symmetries, which is of fundamental interest completely independently of how well perturbation theory works. This is true in particle physics, in nuclear and molecular physics, in condensed matter physics, and so on.

somebody, are you claiming to be top of the food chain?

16. **Peter Orland**
February 19, 2009

There is a point being overlooked in this discussion. AdS/CFT methods have not solved any of the hard non-perturbative problems in QCD or condensed-matter physics. These methods work with large bare coupling constants (that is, the coupling with the UV cut-off is large), where there is no universality. Qualitatively, I don't see that this activity is better or worse than lattice strong-coupling expansions. One strong-coupling method is no more universal than another.

AdS/CFT methods will probably give some more interesting results (for CFT's especially). But, in spite of early optimism, I don't think these techniques are going to be useful in studying fixed points of non-supersymmetric theories.

17. **Troy**
February 20, 2009

Peter,

I truly don't understand what your problem is (apart from the non-scientific issue of "science publishing", "public perception of science", "PR of science" and such topics, which are, clearly, not natural science. I will use the terms "science" and "natural science" as synonyms for this post.).

Yang-Mills theory was not invented to describe parts of the standard model. Does it make it bad that it is used for something completely different today than what it was invented for? Of course not. Similarly, does it make string theory bad that it is not used for what it was invented for? Of course not.
Don't worry about the Yang-Mills example, I (or you) could come up with literally hundreds of ideas and topics that found their "real" application somewhere else than was originally intended.

You might say that the original researchers on Yang-Mills (prior to the standard model applications) didn't hype their work as much as string theorists hyped their work. But this point, may it be valid or invalid, is completely a non-science point. It has to do with PR, press, journalism, etc, but it's definitely not science itself.

So please be honest and say that you don't have a science point to make, but rather you'd like to comment on PR, press, journalism and similar topics.

Best,
Troy

18. **Thomas Larsson**
February 20, 2009

What has AdS/CFT explained about asymptotically free, non-conformal gauge theories? By "explained" I mean explained in the past tense, not hopes for the future.

19. **somebody**
February 20, 2009

Peter O. says: "But, in spite of early optimism, I don't think these techniques are going to be useful in studying fixed points of non-supersymmetric theories."

Fixed points are easy because they are always conformal and conformal theories are what AdS is directly good for. The more interesting part is actually to get to a confining theory, like QCD. Also supersymmetry is already broken, because we are at finite temperature.

20. **Peter Woit**
February 20, 2009

Troy,

Actually, Yang-Mills theory was invented to describe part of the standard model (the strong interactions)…

This posting is about the problem of scientists misleading the public about certain scientific issues. If the idea of someone pointing out that this is going on bothers you, you might want to skip a lot of the postings on this blog.

21. **somebody**
February 20, 2009

cwj: "The reason research is carried out on applying group theory to physics is because nature seems to exhibit deep symmetries, which is of fundamental interest completely independently of how well perturbation theory works."

Understanding dynamics is the pre-eminent goal of physics, my friend. You are not seeing the woods for the trees.

"somebody, are you claiming to be top of the food chain?"

No, I am claiming that you are at the bottom of it. 🙂

22. **cwj**
February 20, 2009

"Understanding dynamics is the pre-eminent goal of physics, my friend."

And if dynamics has a deep group symmetry, then all the perturbation theory in the world won't help you.

Talk about your hostility, somebody. We can trade insults all you like, but I'd be happy to compare funding and citation profiles with you. I'm pretty sure I'm much higher up the food chain than you.

23. **Peter Orland**
February 20, 2009

Somebody said:

“Fixed points are easy because they are always conformal and conformal theories are what AdS is directly good for. The more interesting part is actually to get to a confining theory, like QCD. Also supersymmetry is already broken, because we are at finite temperature.”

What you are saying is incorrect. Perturbative properties of fixed points are easy, as you say. But the challenge in QCD is not finding confinement in a strongly-coupled cutoff theory. Wilson showed that thirty-five years ago.

The real problem is to show that confinement and a mass gap persist to weak bare coupling near the fixed point. This is what is needed to find the string tension and the mass gap after renormalization. This is where the AdS/CFT/QCD approaches have gone nowhere. I’ve worked on this problem a long time, and take my word for it – it is something I understand.

24. **Peter Orland**
February 20, 2009

Sorry if you could see the steam coming out my ears, somebody. I have explained (or at least tried to explain) this stuff at the end of my field theory class. That is why I reacted strongly to the implication that I don’t understand it.

25. **Troy**
February 20, 2009

Peter,

“This posting is about the problem of scientists misleading the public about certain scientific issues. If the idea of someone pointing out that this is going on bothers you, you might want to skip a lot of the postings on this blog.”

What bothers me is not that you point out anything. What bothers me is that continue to insist that this blog and book and similar polemic is part of your scientific activity. I argued, that it is not scientific activity. You apparently don’t deny this. Then, it would be best to not portrait yourself as a scientist making scientific points, but rather as a popular science writer, science journalist, etc.

Best,
Troy

26. **Peter Woit**
February 20, 2009

Troy,

This blog contains a wide mix of things, from notes on my current main research activity about interpreting BRST symmetry in terms of Dirac cohomology (more to come soon, sorry that has slowed down…) to expository comments and pointers to information about topics in math and physics that interest me, to news of various kinds, to attempts to counter efforts to mislead people about certain topics in physics. You don’t hear here about my personal life, you do hear about the areas of math and physics that my professional life is devoted to.

Popular science writers and science journalists don’t have the kind of training or professional background necessary to understand what is going on in cases like this one and to see that they are being misled. Scientists who do have such a background owe it to the public to speak up when they see this kind of thing going on. I would argue that this is part of the responsibility of a scientist, and, yes, legitimate scientific activity.

Funny, but you seem utterly unconcerned about the topic of this posting, the fact that the public is being misled about what is going on in a certain scientific research area. Instead of trying to defend what you know to be indefensible, you choose to (anonymously) write in to attack me. It continues to amaze me how much of this kind of behavior I’ve seen the past few years. The combination of having some physicists going out and actively misleading the public, and then others anonymously attacking anyone who objects to this is not exactly doing much for the public perception of this field.

27. **Peter Woit**
February 20, 2009

I’ve deleted the latest of the Peter O./somebody exchange. This kind of discussion is just not enlightening anyone about anything. Enough.

28. **Troy**
February 20, 2009

Peter,

I’m not attacking you. I’m not unconcerned about the topic of your post. I don’t know why you got that impression. I explicitly said that it doesn’t bother me that you make a point about anything.

What does bother me though is that you portrait this part of your work as science. In my opinion it is not. It is science journalism, popular science writing, etc, etc. This is not an attack on you, since science journalism and popular science writing is nothing bad. I don’t see how you can interpret this as an attack.

My point is that your observations and points about string theory and the state of particle physics are not science per se. This is because they don’t go through the same academic standard and filter as other scientific work, for example, peer review. You seem to be concerned a lot about the behavior of certain physicists. Do you seriously think that being concerned about someone’s behavior is a legitimate science project? Is it really scientific work? I don’t think so.

To illustrate my point: suppose you did nothing else at your university only wrote this blog and your book, would you be entitled to your salary? I don’t think so. This is hypothetical because I know you teach. But for the sake of argument, let’s just suppose you only wrote this blog and wrote your book. Do you seriously think you are entitled to your salary at a math and/or physics department whose mission is to further scientific knowledge through original research? I can only hope, that you also don’t think so.

Best,
Troy

29. **Peter Woit**
February 20, 2009

Troy,

You insist on completely ignoring my point that the postings here are of several different kinds, having various and different relations to “doing science”. For example, the notes on BRST are explicitly research science of the kind you insist I don’t do, so I find your (anonymous) comments an attempt to attack me by denying that I am a scientist who does science. I have no idea who you are or what your motivation for doing this is.

Some things I post here are journalism. Yes, I report news that I find interesting and not readily available elsewhere.

And to repeat myself again, I do happen to think that countering attempts by scientists to mislead other scientists and the public about scientific issues is a worthwhile scientific activity, and it’s not journalism. I’m not calling up experts, asking them what they think and reporting on this. I’m using my own training, expertise and scholarship to make scientific judgments and argue for them.

No, I don’t think Columbia University should pay my salary purely to support my writing a book and a blog. They are only one part of what I do, and the other parts (service to the department, teaching, and research) are significantly more important and central to the purposes of the university. However, I do think the book and blog are significant parts of my scholarship, and of some value to others as such. It is also part of the purpose of the university to support such activities. I’m not the only faculty member at a research university who has written a book which is not a research monograph. I’m extremely lucky and grateful to have colleagues and a university administration that is supportive of what I do.

30. **Troy**
February 20, 2009

Peter,

No, I still don’t attempt to attack you. This blog indeed has some scientific content, but in my opinion it does not qualify as genuine science research activity. You mentioned BRST, which is indeed a scientific topic, but presenting it in a blog, without peer review, without going through the usual channels of academic research, it remains the same category as science journalism and popular science writing. For example, if you would submit your work on BRST to a science journal where others would have the chance to seriously review it and it would get published, it would be a different story. But so far you did not do that. Even if you did, the publication on the blog, in my opinion, still doesn’t count as scientific research.

It seems to me you have a negative view of science journalists and that is why you want to exclude yourself from this group. I see it differently. Science journalists serve an important and relevant purpose and should be acknowledged for their effort. It’s very important that we have science journalists. Some of the science journalists were scientists themselves, some of them have a PhD. I think you are also this kind of science journalist or popular science writer. There is nothing wrong with that.

You seem to imply that a journalist is a person who has no expertise and only calls up experts, cut-n-pastes, etc. This view is pretty offending to several science journalists. Good science journalists are not like this and they can be pretty smart and as I’ve said can even have a PhD just like you.

In order to be clear, in order to not mislead the public, I think it would only be fair if you would present yourself as a science journalist or popular science writer to the public and not as an active researcher. This is because your most important views and messages (regarding string theory, etc) are not in the form of original scientific research, but rather in the form of blog posts and essays. Again, there is nothing wrong with this, this is not an attack on you, but the fact must be stated that you operate in an entirely different way than ordinary scientists who are doing active research.

Presenting yourself otherwise would be misleading to the public and a science journalist would need to come around point this out to the same public.

Best,
Troy

31. **blog reader**
February 20, 2009

Troy, please shut up. You’re just repeating yourself over and over like a cracked record. Give readers credit for being able to read various things and to intelligently draw their own conclusions.

32. **Peter Woit**
February 20, 2009

Troy,

I have a high opinion of science journalists, but I’m not one. I’ve written exactly one semi-popular book and have no interest or intention to write another. The book was intended for publication by a university press. String theorists put a stop to that and it ended up getting published by a trade publisher and getting much wider attention. The blog is aimed at other physicists and mathematicians who share my interests. What I wrote in the book and what I write on the blog is based upon training that includes a Ph.D. in particle theory from Princeton, and 25 years since then during which I’ve spent most of my waking hours thinking about and teaching mathematics and physics. There’s a huge amount of material on this blog and in the book, anyone can judge for themselves whether it is reliable and I know what I’m talking about.

I have no idea who you are, or why you are on this campaign to claim that I am not a scientist, a campaign conducted from behind the mask of anonymity. Frankly I find this extremely creepy

33. **A.E.**
February 21, 2009

This is like saying that experiments have proven the existence of Hilbert Space, because Quantum Mechanics works so well. Hilbert Space is a mathematical tool, that can be used any where including in QM, that a mathematical tool can be used to calculate something in the different context has nothing to do with the questions that mathematical tool was created to respond.

But then again, this has been a constant theme for string theorists. It was created to help with understanding Nuclear forces, failed there, then it was branded as a tool for QG, resolving divergence issues, failed there too, then it was re-branded as a theory of every thing, that unifies all other fields, spectacularly failed there. now it is being re-branded as a computational tool to calculate stuff in nuclear physics. Here is my question, how many times a formalism must fail for it to be abandoned? Isn’t 40 years enough and devotion of the best minds of physics, creating some of the most intractable body of intellectual work to-date enough ?

34. **anonymous fool**
February 21, 2009

Peter,

It’s a false argument that anonymity discredits the writer. What discredits a poster is failure to write in good faith; anonymity may or may not be a manifestation of a lack of good faith. There are perfectly reasonable motivations for writing anonymously, not least of which is that it does not seem important that it is I who am writing what I am writing – another way of putting it is that the anonymously written thing can be judged on its merits, not in terms of the perceived merits of who has written it.

35. **Peter Woit**
February 21, 2009

anonymous fool,

I'd rather not have repeated now the abstract discussion of anonymity on blogs that has taken place here and elsewhere on several occasions. People can judge for themselves what they think, in general, and in the specific. My judgment is that the kind of thing "Troy" is doing is both unprofessional and creepy.

somebody,

I've deleted your last comment because you refuse to stop attacking people as incompetents who don't understand basic facts about QFT.

36. **a**
February 21, 2009

Troy, some years ago neither scientists nor science journalists wanted to openly talk about the problems of string theory. Somebody had to do it. Peter did it.

37. **Joey Ramone**
February 21, 2009

Troy writes, "This is because your most important views and messages (regarding string theory, etc) are not in the form of original scientific research, but rather in the form of blog posts and essays."

String Theory's most important triumphs are not in the form of original scientific research which makes physical prediction and can be tested, but rather in the form of blog posts, essays, pop-sci books, and PBS miniseries.

Regarding the importance of peer-review, my favorite examples are Bruno being burned at the stake, Galileo being placed under house-arrest, Socrates being sentenced to death, and Boltzman being called a crackpot. He ultimately committed suicide before he ever knew of the vast triumph of his ideas. On his tombstone is s=klogw.

Thank goodness that Troy is of a far more civil era, where the tacit perseuction of those who speak and write about the truth regarding string theory is limited to anonymous blog posts, and the rejection of books reporting on reality by univeristy presses.

This blog is continually reviewed by thousands of peers. Its words might not fund some antiquated journal's cartel, where universities are forced to pay tens of thousands for papers–indeipherable papers that are all too often judged and juried by a nepotistic, self-serving system; and are generally published two years after the fact.

This blog contains philosophy. It contains sociology. It contains science and math. It contains entertainment and journalism. And it is making a far greater contribution to the realm of science than those who are all too happy to run with the disingenous stringster hype/headlines so as to sell copies of their magazines and run up their "diggs."

This blog is important *because* it is coming from a genuine, unique perspective. It takes more courage to honestly criticize the system than it does to conform to it, but those who do oft make a greater and more enduring contribution.

38. **Peter Woit**
February 21, 2009

Thanks Joey,

It's a great honor to be so appreciated by one of my musical heroes, even if it is from beyond the grave….

39. **Thompson**
February 21, 2009

Has anybody seen the new data from the Fermi (formerly Glast) satellite indicating a frequency dependent speed of light? This effect has also been seen previously by the MAGIC telescope, and could be the first experimental probe of string theory (more generally, quantum gravity).

40. **rhofmann**
February 21, 2009

Joey and Peter, let me express it this way. An attack strategy such as issued by Troy clearly expresses the fact that there is nothing constructive both, scientifically and sociologically, to be expected from his camp. It is a sad example of screaming helplessness. As a real physicist I am grateful to you, Peter, for all the informed and uncompromising postings of your blog and for the valuable service your book has done to support physical truth. High regards and thank you!

41. **aliaspg**
February 21, 2009

Troy wrote "What does bother me though is that you portrait this part of your work as science."

I may be wrong, but I do get the impression that this statement is linked to a very narrow view of what science is.

A discussion about the question whether a certain theory X has fulfilled its promises and expectations is an intrinsic part of scientific activity – if it is done by people who know what they are talking about, i.e. scientists.

The internet (blogs etc.) has made these discussions much more public, but doesn't change the fact that they are an intrinsic part of scientific activity.

42. **woit**
February 21, 2009

Thompson,

The paper you mention

http://www.sciencemag.org/cgi/rapidpdf/1169101v1.pdf

does not claim to have shown a frequency dependent speed of light, for that they need data from bursters at different distances. What they do do is give a limit on the scale of such an effect: it has to be at an energy level larger than 1/10 the Planck mass.

If such an effect does really show up, that would be exciting, but not an "experimental probe of string theory". Problem is, string theory is consistent with pretty much any scale. For an old posting about MAGIC and yes, yet another "finally a test of string theory is found" media story, see here:

http://www.math.columbia.edu/~woit/wordpress/?p=591

43. **Will**
February 21, 2009

I completely agree with Troy. At some stage, Peter should recognize that he is not exactly an active scientist but an active science journalist whose views are somewhat unobjective. The notes in BRST is of some value, but that doesnt qualify you to claim that you have done work on topics like quantum gravity and so on. If you have done so, you should put in preprint or publish it out. I dont think in this case you will have an excuse to say, the string theory community did not allow you to publish your work. As we all know, that you have done anything of value as far as scientific publication in a long time. I seriously think that you should

44. **Will**
February 21, 2009

….(remainder of the above post)

I seriously think that Peter should not be excessively opposed to anything that is of value that comes out from ideas like ADS CMT and so on. I am really concerned about the negative impact on scientific research by these attacks. If there is something objective that you have to say, please publish. Dont try to sway public opinion and do damage to the spirit of scientific research.

45. **Joey Ramone**
February 21, 2009

Hello Will,

You write, "I completely agree with Troy. At some stage, Peter should recognize that he is not exactly an active scientist but an active science journalist whose views are somewhat unobjective. "

Again, one could easily say, "(Insert Name of String Theorist Here) should recognize that he/she is not exactly an active scientist but an active science journalist/science-fiction writer whose views are somewhat unobjective."

But Peter actually often defends Ed Witten's genius, as well as the valuable mathematical contributions that the original String Theorists made, and the funny thing is he does it far better than most of the younger string theorists, who sometimes seem to forget their lines.

I sympathize with Peter, because as a founding member of the Ramones, I can say we did it not for titles and tenure, but for truth, beauty, and art.

Wikipedia reports: http://en.wikipedia.org/wiki/The_Ramones

"The Ramones were a major influence on the punk rock movement both in the United States and Great Britain, though they achieved only minor commercial success. . . In 2002, the Ramones were voted the second greatest rock and roll band ever in Spin, trailing only The Beatles.[9] On March 18, 2002, the Ramones—including the three founders and drummers Marky and Tommy Ramone—were inducted into the Rock and Roll Hall of Fame."

Now just as American Idol has replaced authentic rock, modern string theory has replaced authentic physics. We rocked out from the soul, and Peter writes from the soul. He has a sense of humor and is fun to read–my favorite part of NEW is the part where he is at Princeton and he sees Ed Witten up ahead in-between Jadwin and Fine. Witten goes up some stairs, and when Peter goes upstairs, Ed Witten is suddenly gone. Perhaps Witten really does have superpowers, or is other-worldly, Peter reasons.

Woit speaks for an entire generation of physicists who were oft muscled out of academia because they took the hard path–because when the choice came–to support questionable obfuscation and peer-review groupthink for a salary, title, tenure, and benefits, or to call it as they saw it, they chose the nobler route. And the Lubos Motls of the world–the end result of the Orwellianification of physics– were unleashed on them.

And that's why I'm supporting Peter from beyond the grave, because I know how it feels, and I wouldn't have had it any other way.

At the end of the day, it is not the Rock'n'Roll Hall of Fame, nor the honors in Spin and Rolling Stone that give us honor–but it my band–the Ramones–who give honor, meaning, and street-cred to them.

At the end of the day, it are not all the millions of refereed, peer-reviewed papers on String Theory that give honor to Princeton, Harvard, and Columbia, but it are bold intellectuals such as Peter by whom the academy is exalted, furthered, and remembered.

Now Troy and Will–I forgive you, as you remind me of all the jocks in high school who laughed at us; but I can't speak for Johnny and Dee Dee, who might not be so kind.

Will–you write, "I am really concerned about the negative impact on scientific research by these attacks. If there is something objective that you have to say, please publish. Dont try to sway public opinion and do damage to the spirit of scientific research."

Peter publishes far more than the vast majority of physicists. His output is truly phenomenal on this blog, and again, it is peer reviewed to a far greater extent than the vast majority of string theory papers–even anonymous snarkers without advanced degrees in physics are allowed to review and comment on the blog posts. It is also read far more than string theory papers, and it is having a far more positive influence on science than all those papers, and that–that is what bothers you most. Peter uses words and math in the pursuit of truth, so naturally, time is on his side, as it was on ours–again, Wikpedia writes:

"The Ramones made little commercial impact, reaching only number 111 on the Billboard album chart. The two associated singles, "Blitzkrieg Bop" and "I Wanna Be Your Boyfriend", failed to chart at all. At the band's first major gig outside of New York, a June date in Youngstown, Ohio, approximately ten people showed up."

String Theory has generated hundreds of millions and is revered by thousands of fanboys at Digg, and American Idol has also generated hundreds of millions of dollars and is revered by many of the same fanboys, but there is some sort of a mystical "super symmetry" in this world, wherein it are those misfit bands who play to ten people in Youngstown, Ohio–who play with heart, soul, and meaning–who end up defining and era and revolutionizing the world of music–not the winners of American Idol.

Peter running this rock'roll blog through modern peer-review would be akin to my band–the Ramones–audtioning before Simon Cowell and the American Idol judges.

It ain't ever goin' to happen–neither in this world nor the next–not even in your landscape's favorite multiverse.

46. **Peter Woit**
February 21, 2009

Well, there certainly appears to be a range of opinions on the Peter Woit issue.

"Will",

I guess it's clear what some string theorists have decided is the right tactic to deal the Peter Woit problem: try to convince people he doesn't know what he's talking about since he's merely a journalist. I don't think this is going to work for you.

You miss the whole point of the posting, which is not in any way critical of AdS/CMT research. What I'm criticizing is the highly misleading way it is being promoted. If you actually care about the reputation of this field, you might want to stop worrying about me, and see what you can do to stop people from going to the press with absurd claims about it. This kind of thing may work with some of the public, but it makes you a laughing-stock among your colleagues, and may have something to do with why no one wants to hire string theorists at the tenure-track level anymore.

47. **Will**
February 21, 2009

Hi Peter,

There are always going to be people in any fields and subfields who will exagerrate things they do.I dont see that this as a major issue. Also, I dont think this is a problem that is unique to string theory community. I tend to think that string theorist tend to do this lesser than most other discipline.

It is clear that ADS/CFT has helped us understand different physical phenomena in different energy scales. It has given us valuable too to understand phenomenas that were thought to have no relations what so ever. Now, ADS CFT was borne out of string theory. I dont know if you just despise the word "string theory". You have been around physicists enough years to understand that we are more or less an opportunist and we tend to work in whatever we can put our hands into.

In my opinion the term string theorist is too narrow.As you know, only a small fraction of people work in the fundamental theory. Most tend to work in other aspects where they think they can make progress.

48. **Will**
February 21, 2009

continued….

If some person decides to do a press release about whatever they do, this doesnt reflect the position of the whole community. You should know better than that. String theory is not an organization with a governing board which gives decree for certain press releases. Now, for you to suggest that the whole of string theory community is just working as some giant corporation is just silly.Now your critique of string theory tends to be always social. May be you should try to study the new developments rather than nit pick and focus on buzz words. It would be better than commenting on a cartoon picture in string theory, press releases or Polchinski's colloquim talk.

49. **Peter Woit**
February 22, 2009

Will,

Again, this was not a post criticizing AdS/CFT research but was about a specific egregious example of misleading hype. The huge area known as "string theory" has been heavily damaged by the amount of overselling and hype that has gone on for 25 years now, with a serious loss of credibility. You might want to think about what can be done about that, and whether reacting to new even more egregious hype by complaining that someone is criticizing it is a fruitful thing to do.

Believe it or not, I do spend significant effort trying to learn about what is going on in many parts of string theory, especially the more matematical end, but also others. I wish there was more interesting to report from this effort, but this seems to be a very quiet time for the subject.

50. **N. Nakanishi**
February 22, 2009

I believe that the aim of this blog is to discuss scientific topics and to communicate various opinions, but not to criticize particular person's activity. I think that the action of Troy and Will is unfair because they criticize not Peter Woit's opinion but his personal activity, keeping their names anonymous. The natural guess based on their anonymousness is that they are totally non-scientists.

51. **H-I-G-G-S**
February 22, 2009

Peter,

You said "Actually, Yang-Mills theory was invented to describe part of the standard model (the strong interactions)"

Not true. Go back and read the original paper. YM theory was invented to reconcile isospin invariance (which had new experimental results supporting it) with the concept of local fields. They give the impression that they think global rather than local isospin invariance is inconsistent with local fields. In any case, they decide to see what they can deduce if isospin invariance is made local. I don't believe the claim that they were trying to describe the strong interactions is supported by any statements in their paper.

52. **Peter Woit**
February 22, 2009

H-I-G-G-S,

Actually I have read the paper, and seriously studied the history of that era. I'm well aware that what they were gauging was isotopic spin, but it really is true that what they were hoping to get was a theory of the strong interactions. There's nothing in the paper about the weak interactions, the introduction is all about the strong interactions (nucleons, mesons, no leptons), then they state:

"We then propose that all physical processes (not involving the electromagnetic field) be invariant under an isotopic gauge transformation…"

It was only several years later, after V-A, that people like Schwinger and his student Glashow started working on the idea of using Yang-Mills theory to get a theory of the weak interactions.

If you don't believe me, maybe you'll believe David Gross, who writes in http://psroc.phys.ntu.edu.tw/cjp/v30/955.pdf

"The application of Yang-Mills theory to the strong interactions-the original motivation for the theory- was even trickier"

53. **Peter Woit**
February 22, 2009

One more, in case you also think Gross doesn't know what he's talking about:

See Weinberg's

http://arxiv.org/abs/hep-ph/0401010

"Yang and Mills [9] in 1954 constructed a gauge theory based not on the simple one-dimensional group U(1) of electrodynamics, but on a three-dimensional group, the group SU(2) of isotopic spin conservation, in the hope that this would become a theory of the strong interactions."

54. **Troy**
February 22, 2009

Peter,

""""I have a high opinion of science journalists, but I'm not one.""""

Okay, let's break this down more. Now that we agree that being called a science journalist is not an insult, hopefully you will stop thinking that I'm attacking you. I'm not.

""""The blog is aimed at other physicists and mathematicians who share my interests. What I wrote in the book and what I write on the blog is based upon training that includes a Ph.D. in particle theory from Princeton, and 25 years since then during which I've spent most of my waking hours thinking about and teaching mathematics and physics.""""

This is all true. But this doesn't mean that your current activity is not closer to science journalism than to original scientific research.

""""There's a huge amount of material on this blog and in the book, anyone can judge for themselves whether it is reliable and I know what I'm talking about.""""

All true again. I don't say you don't know what you are talking about either. I did not say that and I never implied it. All I'm saying is that writing a blog and popular science book, even with the strong credentials you have, does not constitute scientific activity, in my opinion. To be clear, there is nothing wrong with this, this is not an attack, not a creepy ad hominem, etc, nothing like that, it's a simple observation that doesn't contain anything insulting.

""""I have no idea who you are, or why you are on this campaign to claim that I am not a scientist, a campaign conducted from behind the mask of anonymity. Frankly I find this extremely creepy""""

Let's not get into the discussion of posting anonymously, this will only sidetrack the discussion.

I did not say you are not a scientist. I certainly start getting the feeling that you are a slightly bit paranoid. Again, I did not say you are not a scientist. What I said is that your most recent activity, writing a blog and a popular science book, does not constitute original scientific research activity.

Example: Terence Tao is a scientist. He writes a blog. His blog does not constitute original scientific research, based on which one could label him a scientist. We think about him as a scientist for other reasons, for example his papers, theorems, etc, etc, all the usual stuff. But he is not spending 100% of his time on original science research, he does other stuff too. He does popular science too, popular science writing, science journalism, etc. His blog is an example of his popular science activities. I bet he wouldn't be offended if somebody would point it out that his blog writing activity is popular science writing. In fact he knows this.

Just as John Baez knows that his This Week's Find is popular science writing, aimed at scientists, but something that does not meet the strict requirements of rigorous science. But he would not be offended by this. In fact, that's exactly the reason he does what he does. He wants a free form of exchange of ideas without the burden of rigorous academic channels and there is nothing wrong with that. In fact, it's pretty good that he is doing what he is doing. He is also a scientist and large part of his time is spent on original scientific research. His blog writing activity is something else though.

So back to you. Your blog and book writing activity is the same. It's popular science writing or science journalism. You have a PhD. You graduated from a good school, you wrote a couple of papers. So you are a scientist. But in the last 5-10 years, you did not carry on your scientific research, instead you are writing this blog and writing a book and publish drafts of some scientific ideas as blog postings. There is nothing wrong with this. But Terrence Tao and John Baez does this type of popular writing and original research on top of that. You apparently dropped original research and stick with popular writing. That's a difference between you and Terrence Tao or John Baez.

I simply don't understand why it's so hard for you to admit this. It's not a stigma. It doesn't make the value of your writings any smaller. Nobody will think less of you. But if you continue to go against the facts I outlined above maybe we don't agree as to what constitutes original science research and what constitutes popular science writing.

I think I made my definitions clear (Terrence Tao's blog or This Week's Find are both popular science writings, aimed at scientists, for example). If you disagree, please let me know what you think is original scientific research and what is popular science writing.

Best,
Troy

55. **Peter Woit**
February 22, 2009

Troy,

I've already answered you repeatedly. I'm no Terry Tao, but I currently spend many hours a week working on original scientific research in the narrowest possible sense (my work on BRST and Dirac Cohomology), and I have been doing this my entire adult life. Some of this has appeared on the blog, more will in the near future, as well as ultimately in more conventional forms. If you want to describe me as a scientific researcher who has a problem with not writing things up for publication out of a combination of laziness and not being willing to publish things that he's not happy with, that would be fine.

This is a posting about a rather egregious example of string theory hype. Instead of discussing that, you're on an obsessive and off-topic campaign to convince the world that I'm not a scientific researcher but a journalist. I don't know anything about who you are, or why you are trying to do this. It really is creepy though.

56. **Sung Lee**

February 22, 2009

Troy,

Your tactics is so transparent. What you are trying to implicate is that Peter is not a scientist but a scientific journalist, therefore his criticisms about string hypes are less credible.

I presume a lot of regular readers of this blog are professional physicists or mathematicians. I myself is a mathematician. Even without your pointless effort, I believe people can use their own judgment to see whether Peter's argument is credible or not. If you have a problem with his criticisms, you just need to point out what the problem is with your own rationales. If you are a string theorist, I would personally like to hear about your expert counter argument on Peter's criticisms. While I agree mostly with Peter's criticisms about string hypes, I believe it is still too early to dismiss string theory itself. I personally am interested in string theory and am trying to understand it along with standard QFT.

If you really have something to say to counter argue with his criticisms, please do. Otherwise, quit it.

Best,
Sung Lee

57. **anon.**
February 23, 2009

The H-I-G-G-S and Troy arguments are of the philosophy:

'When in hole, keep digging.'

No matter how many failures there are, no matter how many false claims are exposed, no matter how much spin and hype is proved vacuous, there is no apology. Far from it…

58. **Eugene Stefanovich**
February 23, 2009

Re: Troy vs. Peter

The naked Emperor is funny, but the naked Emperor throwing insults is truly hilarious.

59. **Joey Ramone**
February 23, 2009

Many people accused my band the Ramones of not being a real band because we did not put out top-40 hits like Boy George in the 80's.

I recall getting a snail-mail fan letter from Troy. Back then we did not have email and blogs, so people had to pay for stamps and envelopes when they sent anonymous words belittling us. I saved "Troy's" letter. Here it is:

"Joey–So back to you. Your band and record-making activity is the same. It's alternative rock or punk rock. You learned some chords. You got yourself a decent guitar, you wrote a couple of songs. So you are a musician. But in the last 5-10 years, you did not carry on your higher musical duties as did Billy Idol and Tears for Fears, instead you are performing for a dozen fans in Toledo Ohio and ignoring the corporate music scene. There is nothing wrong with this. But Adam Ant and Madonna also perform in Toledo and compose corporate top-40 music on top of that. You apparently dropped corporate top-40 music and stick with punk/alternative. That's a difference between you and Adam Ant and Madonna."

Without Troy's insights, I doubt we'd ever been inducted into the Rock'n'Roll Hall of Fame.

60. **Will**
February 23, 2009

I think everyone is misinterpreting Troy's point. I think Troy's claim is that Peter does not have credentials like Shelly Glashow to counter string theory. This does not mean that Peter is not entitled to his opinion. I think Peter has done a good job in pointing out websites, lectures etc in both math and string theory.

61. **Will**
February 23, 2009

My last thought is that I hope Peter would show the details in the flaws about the new development in arguments in string theory as opposed to touching on tangential point. Anyways, there I rest my case and I will not say more in this post.

62. **Peter Woit**
February 23, 2009

Will,

I don't think anyone is misinterpreting Troy's attack on my credentials. Anyone who wants to judge arguments about string theory based purely on the credentials of who is making them should ignore me and listen just to Nobel prize winners and senior faculty at places like Harvard and Princeton. On the other hand, if you want to make your own judgment about this scientific controversy, you might want to read what I have to say, what string theorists have to say, and make up your own mind. One thing you may notice is that often, in cases like this posting, string theorists don't bother to try and defend the indefensible hype coming from their colleagues that I am pointing out, and instead try and attack my credentials. This tactic hasn't worked out for them so far, but they keep at it…

63. **Joey Ramone**
February 23, 2009

I remember when Troy and Will used to follow us around after concerts, yelling at us from behind masks–"You are no Depeche Mode! You are no Duran Duran! You are no Dexy's Midnight Runners!"

Boy, did that creep the band out.

It turns out they were fans of Tears for Fears, but attacking our credentials never did get Tears for Fears into the Rock'n'Roll Hall of Fame.

And attacking Peter probably won't save string theory.

64. **Aaron Bergman**
February 23, 2009

Are you bashing Dexy's Midnight Runners? Because if you are, I'm not sure I can stand for that. I mean, they have the toora and the loora. You can't beat that shit.

65. **Will**
February 23, 2009

Peter,

Well, I almost feel compelled to make one more argument. If you see nobel lauretes like Weinberg, Thooft, Nambu, Gross, Gell Mann and others in particle physics tradition we know for a fact that they tend to support string theory.

In fact a number of them have worked in the fields and there is no denying this. These are not conspiracy theories. Now, obviously like with any discipline, there are skeptics within particle physics community but their criticism are more constructive and not outright negative.

Other criticism come from subdisciplines of physics. For instance people like Philip Anderson have opposed even building particle colliders: his battles with Weinberg about this issue and the congress hearings are well documented. As good as people like Anderson or other condensed matter physicist are, their skepticism is only good to understand their taste.

If one looks at physicist who have thought deeply about the problems in quantum gravity, it would be misleading to claim that most distinguished physicist are against string theory. In fact the evidence suggests quite the contrary.

66. **Peter Woit**
February 23, 2009

Will,

Deciding a scientific question by counting up how many Nobel prize winners are on each side of the question is one way to do it, another is to actually learn about the subject and make up your own mind. By the way, you don't seem to mention Veltman, Wilczek, Glashow,, and 't Hooft doesn't seem to me to be much of a fan of string theory unification. I have no idea what Kobayashi and Maskawa think.

In recent years I've had the honor of discussing the issue with many eminent physicists, including a sizable number of Nobelists. They each have their own take on the situation, I think it's a mistake to simplify any of their views to "for" or "against". One thing to keep in mind is that there isn't any real disagreement about the fact that string theory has not worked as a unified theory. Some remain optimistic that this will change, others are very skeptical.

To get back to the topic of this posting, the misleading story about string theory and experiment. There I don't see any disagreement. I can't think of any physics Nobelist who would want to defend the "string theory finally makes contact with experiment" headlines as not being misleading.

67. **Joey Ramone**
February 23, 2009

Yes–like the String Theorists of your era, Dexy's Midnight Runners had all the funding in the early eighties. They had all the lighting, intricate stagesets, professional choreography, fx, makeup & hair, complex story, and cool costumes:
http://www.youtube.com/watch?v=RXLHUThBib8

All we had were our leather jackets and a single camera which didn't move, and we didn't even dance:
http://www.youtube.com/watch?v=wMD7Ezp3gWc
The only time I ever met Weinberg, T'hooft, Nambu, Gross, and Gell Mann was during the filming of this video. You can see them around 1:00-1:10.

When one juxtaposes these two videos it is quite surprising that we–the Ramones–were the ones who ended up in the Rock'n'Roll hall of fame–not DMR. I would have never guessed, as you know how those "hall of fame" things go. Twenty years from now, when people watch an Elegant Universe, they will probably be surprised that nobody won a Nobel for String Theory.

68. **Aaron Bergman**
February 24, 2009

You can talk about the Rock and Roll hall of fame all you want, but you never had a fiddle in your band. Never had and never will.

69. **H-I-G-G-S**
February 24, 2009

Peter,

Perhaps Yang and Mills were hoping to develop a theory of the strong interactions. Perhaps not. Where is the evidence that they were? You don't cite any statements from their actual paper. You don't direct me to any historical documents where they were interviewed about their thoughts. If you did I would be happy to have a look and I might be convinced that this was indeed their motivation. Instead you argue by appeal to a higher authority, in this case Gross and Weinberg. Of course when they argue about the importance of string theory you do not agree with them, but when they support a point you like they are suddenly experts who cannot be disputed. Gross was 13 years old when the Yang-Mills paper was published. Why do you think he should know what they were thinking?

PH

70. **Peter Orland**
February 24, 2009

H-I-G-G-S,

I once read the Yang-Mills paper when I was much younger (though I was probably older than thirteen. Maybe I was twenty. Or seventy. I can't recall). They were trying to describe vector mesons, which were regarded as fundamental mediators of the nuclear force (along with the pion). Sakuri and other people were thinking of the Yang-Mills particle as rho mesons.

The mass was inserted by hand, since they didn't know about Y-O-U at that time.

As far as I know, the first people to try to apply the idea to the weak force were Schwinger and Glashow (his student).

71. **Pawl**
February 24, 2009

Re: Will's comments on unification and particle physics

There is a premise here — which seems to be unquestioned by many superstringers — that the problem of quantizing gravity is enough like a particle physics problem that it will be solved by a particle-physics-type approach. True, the electromagnetic, strong and weak interactions were all described by quantum field theory, but we may need something of a different (and arguably deeper) character to tackle gravity — which affects causal relations in a way the other forces do not.

From this point of view, one should be especially cautious about particle physicists who endorse some particular quantum gravity program, because there is the question of whether they are so conditioned by successes in their own field they may not appreciate the very real possibility that it may be necessary to move beyond it.

It might be interesting to consider how string theory is viewed by relativists. While there certainly are some relativists who do string theory, it seems to me that on the whole relativists have at best a wait-and-see attitude towards string theory.

(I agree with Peter that citing what other people think is really a distant second to having an informed opinion of one's own. I am trying to make the point that to have an informed opinion it would be a mistake to rely only on expertise in particle physics.)

72. **Will**
February 24, 2009

Pawl,
I was answering Peter's point that senior faculty at Harvard and Princeton were overwhelmingly against string theory.

I was pointing out names just to show that, in fact the balance of people with serious credential tips towards string theory and others who are skeptical make constructive arguments as oppose to whining about what a new scientists magazine thinks about string theory because a string theorist did not do a "good job" in a press conference. Again, any one string theorist is not representative of the entire field and these are mind numbing arguments.

Pawl, I however agree with you that everyone should make an informed decision and forum such as this almost inevitably engage people in discussion which has less to do with science but press releases, who said what and so on. I have said repeatedly, that
scientific debates are won in black boards as opposed to sociological comments in blogs or press releases.

Hence, I am urging people to make more scientific arguments as opposed to just playing the game which now seems old and not at all constructive.

73. **woit**
February 24, 2009

Will,

I never claimed that "senior faculty at Harvard and Princeton were overwhelmingly against string theory", something which is very much not true. I just wrote that if all you care about is credentials, they're the people you should be listening to, not that they as a group agree with me about string theory or anything else.

74. **Peter Woit**
February 24, 2009

H-I-G-G-S,

Wow. The refusal of some particle theorists to admit it when they're wrong about something is nothing short of spectacular.

Actually I did cite a relevant statement in the paper. My claim that Yang and Mills were thinking about the strong interactions is based upon my knowledge of the history of the period, not on the quotes from Gross and Weinberg. I quoted them because they are very serious scholars, with a deeper knowledge of the history than my own, since they are personally closer to it.

This whole topic seems worth a blog posting…

75. **Pawl**
February 24, 2009

Will,

You'll notice that my arguments are either directly scientific or go to the question of whether the authorities others cite are likely to have considered all pertinent scientific issues.

If you do want to encourage rational argumentation, I suggest you write courteously, avoiding terms like "whine."

76. **Mitch Miller**
February 24, 2009

Peter wrote:
"This whole topic seems worth a blog posting"

I know you probably don't take requests but some posts about the historical development of particle theory topics and how people reacted to things when they were first presented would be very interesting (i think) to alot of your readers and is something that is not found in detail very easily online. So if you were leaning in that direction I think it could make for some good posts.

77. **Sebastian Thaler**
February 24, 2009

Hi Peter,

I thought you'd get a kick out of this job posting for a "short-term science research assistant" that recently appeared on Mediabistro.com. Whoever posted the job lumps string theory in with meditation, clairvoyance, and psychokinesis:

http://www.mediabistro.com/joblistings/jobview.asp?joid=87703&page=1

78. **TCO**
March 1, 2009

pop culture has already moved on and decided that string theory is unphysical and untested. you won the meme war. don't let some Motl dead enders dissuad you. Even if the job holding onto professors don't agree…the public has already written of string theory. It's stock price is in the crapper. Trading on the pink sheets now. Even just regular physicists think so.

79. **Troy**
March 1, 2009

Peter,

TCO and Joey Ramone illustrate my point very well. They both talk about popular culture. It might be the case that the standing of string theory in pop culture is declining but just as it did not matter one bit what the standing of string theory is in pop culture when this standing was high, it does not matter one bit now when this standing is low. This is simply because pop culture and science are two different things and, for me at least, science is infinitely more important when discussing the merits of a scientific field.

There is nothing wrong with pop culture. In fact, I like it a lot. But it has nothing to do with science.

And in the last years you have contributed a lot to pop culture but exactly zero to science. As soon as you decide to contribute to science again I'd be more than happy to change my opinion.

You, in fact, promised almost a year ago that you will write up your ideas on BRST in the form of a paper by the summer of 2008:

http://www.math.columbia.edu/~woit/wordpress/?p=673&cpage=1#comment-36514

At that time you still recognized that writing this blog is less important than science itself. You wrote "I probably should be spending less time on the blog…." and it seemed you wanted to dedicate more time to science. This seemed encouraging but now there is still no paper, not even an arxiv posting and you even seem to have changed your mind and you started to think that blogging is part of your scientific activity.

What happened? What made you change your mind? Why don't you say today that "I should be spending less time on blogging and more on writing science research articles."?

Given your super paranoid reactions, let me reiterate:

1. I'm not attacking you.
2. I'm not saying you are not a scientist
3. I'm not questioning your credentials (in fact, I wrote that your credentials are strong)
4. I'm not bringing in personal/character/etc issues into the discussion
5. I'm not posting anonymously because I'm "hiding"
6. I'm not a string theorist (in fact, I have never written a paper on string theory)
7. I'm writing anonymously because I believe it's possible to discuss the merit of my posting on merely the posting itself, regardless of who the author is

Best,
Troy

80. **Peter Woit**
March 2, 2009

Troy,
Your obsesssion about this topic really is creepy. Latest news about the BRST project is that I spent yesterday working on rearranging the paper, based on some better understanding of what is going on that the last couple months of work has led to, this week hope to get more written.

81. **Troy**
March 2, 2009

Peter,

I'm eagerly waiting for the arxiv preprint, since this is a very interesting topic!

Best,
Troy

On History

February 24, 2009
Categories: Uncategorized

When learning about various ideas in mathematics and physics, I'm always fascinated by the history of these ideas and eagerly read whatever I can find on the subject. Partly this is because my understanding of ideas is often enlightened by finding out where they came from, especially what problems they were invented to solve. It's also true that the history of these fields is a huge and remarkable story, in many ways far more intricate, subtle and surprising than any novel ever written, and can be appreciated as such. It's quite possible that I've spent more time on this than is healthy, since there are good reasons for the fact that many scientists wait until late in their career to develop serious historical interests. Time spent studying history is not time spent developing new ideas.

One peculiar aspect of the present state of particle theory is that our current best fundamental physical theory, the Standard Model, is getting so old that fewer and fewer active physicists have any first-hand knowledge of its history. To a large degree, this history spans just about exactly a quarter-century, from renormalized QED in 1948 to asymptotic freedom in 1973. Before 1948 all we had were first-order calculations in QED, by 1973 the full Standard Model was in place. Physicists who finished a Ph.D in 1973 are now in their early 60s and soon will be getting to retirement age. First-hand understanding of where the Standard Model came from is now not part of the background of particle physicists in the most active stage of their careers.

One reason I started thinking about this is a recent exchange in the comment section of the last posting, sparked by my referring parenthetically to the fact that Yang and Mills had developed Yang-Mills theory (in 1954) in the context of trying to describe the strong interactions. The SU(2) gauge theory they wrote down didn't work for this purpose, since what was needed was an SU(3) theory of quark colors, something that had to await at least the discovery of quarks. The SU(2) gauge theory of isotopic spin they were considering ultimately did find a role in the electroweak part of the Standard model, but this idea got started only after the symmetry properties of the weak interactions became clear later in the 1950s. Schwinger and his student Glashow were among the first to work on this idea, with the correct theory not appearing until 1967 after the role of the Higgs mechanism was understood.

Anonymous commenter "H-I-G-G-S" reacted to my allusion to this history as follows:

> You said "Actually, Yang-Mills theory was invented to describe part of the standard model (the strong interactions)"
>
> Not true. Go back and read the original paper.

Well, I have read the original paper, as well as a lot of secondary literature about it. The paper begins with a discussion of the symmetry properties of the strong interactions of nucleons and pions, which was the main topic of the day in 1954, due to the large number of strongly interacting states being discovered at accelerators. Nothing about the weak interactions, which was a different topic, with the symmetry properties of such interactions not understood until a few years later.

I devoted a few minutes to Googling "Yang-Mills" and "history", and turned up quotes from David Gross and Steven Weinberg explicitly stating that the strong interactions were the motivation for Yang and Mills and posted comments with those. It seems though that "H-I-G-G-S" is not satisfied with this, recently responding:

> Perhaps Yang and Mills were hoping to develop a theory of the strong interactions. Perhaps not. Where is the evidence that they were? You don't cite any statements from their actual paper. You don't direct me to any historical documents where they were interviewed about their thoughts. If you did I would be happy to have a look and I might be convinced that this was indeed their motivation. Instead you argue by appeal to a higher authority, in this case Gross and Weinberg. Of course when they argue about the importance of string theory you do not agree with them, but when they support a point you like they are suddenly experts who cannot be disputed. Gross was 13 years old when the Yang-Mills paper was published. Why do you think he should know what they were thinking?

I had actually cited a relevant statement from the paper, but I'm not sure what if anything could possibly satisfy "H-I-G-G-S". Perhaps there is a published interview where Yang makes the kind of explicit, unambiguous statement about his motivations that "H-I-G-G-S" requires and maybe someone with enough interest can dig this up. Since "H-I-G-G-S" insists on anonymity, all I know is that he or she is from a major metropolitan area home to major universities, and appears to be a particle theorist who has been around for a while, although not long enough to know much history. Despite this, he/she has rather definite ideas about what this history is, coupled with a steadfast skepticism about any information which might indicate these ideas don't correspond to historical reality.

I don't know to what extent the case of "H-I-G-G-S" reflects the general understanding of the historical roots of the Standard Model among active theorists working on trying to extend it. Much effort on this blog has been devoted to trying to puncture the historical narrative that has solidified over the last 25 years about the supposed march forward of such speculative ideas such as extra dimensions, supersymmetry and string theory. Perhaps it would also be a good idea to worry about misconceptions concerning the history of successful parts of the subject, as well as the unwillingness of many particle theorists to give up such misconceptions.

Update: Here are some suggestions for reading about the history of the Standard Model, ordered very roughly from more popular to more technical:

- *The Hunting of the Quark*, Michael Riordan
- *The Second Creation*, Robert Crease and Charles Mann
- *Inward Bound*, Abraham Pais
- *50 Years of Yang-Mills Theory*, edited by 't Hooft
- *Pions to Quarks: History of Particle Physics in the 1950s*, edited by Brown, Dresden and Hoddeson
- *The Rise of the Standard Model: Particle Physics in the 1960s and 1970s*, edited by Hoddeson, Brown, Riordan and Dresden
- *History of Original Ideas and Basic Discoveries in Particle Physics*, edited by Newman and Ypsilantis

For the early history of gauge theory, there is

- *The Dawning of Gauge Theory*, Lochlainn O'Raifertaigh

Comments

1. **Thomas R Love**
   February 24, 2009

   This is a great time to be studying history. Besides google, there is scholar.google.com which links to papers that reference the paper you searched for, as does Phys. Rev. And most major journals are online. I can find more articles in one hour online than I used to find in a week at the library.

   I've been studying history to see where things went wrong. The introduction of quarks met with the same criticism as strings do today. The discussion of EPR through the ages is fascinating.

   Some of the most fascinating papers have no citing articles.

Yes, I am in my 60's, 63 to be precise and without the interruption of the Vietnam tour, I would have had my PhD in 1973. But then I would have been part of the establishment I am now criticizing. I am not a fan of the standard model, I am bothered by the idea that there are conserved quantities which are not defined for all particles and not conserved in all interactions (isospin, strangeness…)
and it is exciting that people long ago had the same reservations.

One of my cousins has a PhD in history and she says "Nothing changes faster that the past" both because scholars keep digging up new material but also because there are some who keep rewriting the past.

2. **anon.**
   February 24, 2009

   The widespread ignorance of the history of the ideas of particle physics is disgusting. The first application of Yang-Mills SU(2) to attempt to deal with weak interactions was by Schwinger and Glashow in 1956, see Glashow's Nobel Prize lecture here:

   'Schwinger, as early as 1956, believed that the weak and electromagnetic interactions should be combined into a gauge theory. The charged massive vector intermediary and the massless photon were to be the gauge mesons. As his student, I accepted his faith. … We used the original SU(2) gauge interaction of Yang and Mills. Things had to be arranged so that the charged current, but not the neutral (electromagnetic) current, would violate parity and strangeness. Such a theory is technically possible to construct, but it is both ugly and experimentally false [H. Georgi and S. L. Glashow, *Physical Review Letters,* 28, 1494 (1972)]. We know now that neutral currents do exist and that the electroweak gauge group must be larger than SU(2).

   'Another electroweak synthesis without neutral currents was put forward by Salam and Ward in 1959. Again, they failed to see how to incorporate the experimental fact of parity violation. Incidentally, in a continuation of their work in 1961, they suggested a gauge theory of strong, weak and electromagnetic interactions based on the local symmetry group SU(2) x SU(2) [A. Salam and J. Ward, *Nuovo Cimento,* 19, 165 (1961)]. This was a remarkable portent of the SU(3) x SU(2) x U(1) model which is accepted today.

   'We come to my own work done in Copenhagen in 1960, and done independently by Salam and Ward. We finally saw that a gauge group larger than SU(2) was necessary to describe the electroweak interactions. Salam and Ward were motivated by the compelling beauty of gauge theory. I thought I saw a way to a renormalizable scheme. I was led to SU(2) x U(1) by analogy with the appropriate isospin-hypercharge group which characterizes strong interactions. In this model there were two electrically neutral intermediaries: the massless photon and a massive neutral vector meson which I called B but which is now known as Z. The weak mixing angle determined to what linear combination of SU(2) x U(1) generators B would correspond. The precise form of the predicted neutral-current interaction has been verified by recent experimental data. …'

3. **big vlad**
   February 24, 2009

   Peter, this H-I-G-G-S fellow is a typical internet troll, I wouldn't waste time on him. Though he did allow you to make a point I suppose…

   Thomas R Love, I'm not sure I understand your reservations about the standard model. Isospin and strangeness are approximately conserved quantities (that is, they are not conserved, but they almost are). And surely they are defined for all particles? The strangeness is the number of strange quarks minus anti strange quarks, for example. Or am I mixed up?

4. **Peter Woit**
   February 24, 2009

   big vlad/Thomas Love,

   Please stick to the topic of history. Again this is not a forum for people to explain their favorite unconventional ideas to the world.

5. **Michael**
   February 24, 2009

   Peter,

   Wasn't Gross' remarks made at a celebration of Yang's 70th birthday — was he present?

6. **Peter Woit**
   February 24, 2009

   Michael,

   Yes, that's true. Almost certainly Yang was there.

7. **Pawl**
   February 24, 2009

   I agree that there's a great deal of interest attached to sorting this out.

   I have a general impression (perhaps someone can correct me about this) that in the mid-50's there was less theoretical urgency felt in moving beyond the four-Fermi theory of weak interactions than in trying to develop the theory of strong interactions. The reason was that the non-renormalizability of the theory was not widely perceived as a fundamental failing (only later did that general sentiment develop), and, given the weakness of the force, the f-F treatment seemed a good start. On the other hand, the strong force was — well, strong, and so worries about finding a consistent way of doing perturbations were much more timely.

8. **Peter Woit**
   February 24, 2009

   Pawl,

   I think what's most relevant is that in the early 1950s there was a huge amount of experimental data coming in about the strong interactions, with all sorts of new particle states being discovered, and hardly any theory at all to account for them. Because of this, just about all theorists those days were focused on the strong interactions. There was much less data concerning weak interactions, and the four-fermi theory accounted for what was known, with the question of higher loop calculations in that case a purely academic one.

   Yang and Mills didn't need to explain in their paper that they were thinking about the strong interactions, that would have been obvious to everyone. If they were thinking at all about the weak interactions, they would have mentioned that explicitly.

9. **Pawl**
February 24, 2009

Peter,

Thanks. I think we're saying almost the same thing.

10. **Shantanu**
February 24, 2009

Peter and others , I agree history of physics is a fascinating subject.
In cosmology, history of cosmic inflation is also very interesting
and quite differnent form what appears in textbooks with very important papers by Kazanas, Starobinsky and Sato (and probably others) before
Guth's paper.

11. **publius**
February 24, 2009

On the question of wether or not is healthy to devote time to study history of physics, i believe that history itself (a "one loop" reflexion:) shows that really deep thinkers like Galileo, Newton, Maxwell or Einstein were quite interested an well informed on that respect. Probably the usefulness of that kind of knowledege depends on what yo do: if you are a problem solver working on a given predetermined conceptual background (a "local thinker", a man of detail)it is probably OK not to know anything about history, but if you are a big picture "strategic" thinker, you better be aware of your discipline history.
At any rate, any self respecting phys grad student should have some exposure to phys history, at least as an antidote to the dreadful possibility of being some sort of phys "energyzer bunny" stuffed with batteries and set to go in the place and direction of choice of his PhD advisor, as many wannabe string theorists in this days.

12. **McPint**
February 24, 2009

I think Abraham Pais's Inward Bound has something to say on this. In the discussion on page 585 of the introduction of Yang-Mills theory the discussion over the next few pages is exclusively phrased with respect to nucleons and to vector mesons as the exchange particles. This surely gives the context for the discussion above.

Perhaps also reference 172 in that chapter also has something interesting to say – this reference is "C.N. Yang, Selected Papers 1945-1980, p 19, Freeman, San Francisco 1983" where the page reference describes Yangs recollections of a seminar he gave in Princeton where Pauli was very critical and Pais was also present.

This would perhaps give the definitive answer?

13. **Sumar Ongi**
February 24, 2009

> Yangs recollections of a seminar he gave in Princeton where Pauli was very critical and Pais was also present.

that story is so well-known, yet so funny… Pauli was asking Yang about the mass of the intermediate vector mesons (now gluons), probably knowing that they were massless and therefore a killer for the theory (there are no massless hadrons…). Yang responded he wasn't sure of the answer. Apparently, Pauli was so insistent and hostile with his questions that Yang just sat down at the front row and stopped talking! Then Oppenheimer encouraged him to continue delivering his talk, which he did.

14. **Indrajeet**
February 24, 2009

Hi,
Can anybody suggest me a good book on history of particle physics?…..Other than Pais' "Inward Bound"

15. **D R Lunsford**
February 24, 2009

Pauli did it first, before Yang-Mills, in some letters to Pais, "Meson Nucleon Interaction and Differential Geometry" (written "to see what it looks like", in three days in July. See O'Raifeartaigh, "Dawning of Gauge Theory". Like Gauss, he did not publish.

-drl

16. **sz**
February 25, 2009

Peter,

I have a copy of "Selected Papers by C.N.Yang, 1942-1980", in which Yang made extensive comments on every paper there, mainly with historical interests. The comments on the Yang-Mills are on p19…Maybe this gentleman H-I-G-G-S can go to the library to find it out.

17. **chris**
February 25, 2009

http://universe-review.ca/R15-21-YangPauli.htm

18. **Rajagopal**
February 25, 2009

@Indrajeet: I found 'Second Creation' by Robert Crease and Charles Mann to be a very interesting history of particle physics etc. Covers the period from early 20th century to early eighties (only) – very little, if not nothing, about string theory IIRC. Maybe others (physicists in particular) can comment on how good an account of history it is.

Rajagopal

19. **Cormac O Raifeartaigh**
February 25, 2009

Hi Peter, interesting post.
In the book 'It Must be Beautiful' (Granta Books), Christine Sutton has a lovely chapter on Yang-Mills theory, outlining the development of the theory, its initial failure and its modern use in particle physics.
There is also a nice reference to Lochlainn's book on the history of gauge theory!

20. **DB**
February 25, 2009

Chris Quigg deals with this issue in his "Gauge Theory of the Strong, Weak and Electromagnetic Interactions", p.55. where he cites the original 1954 paper by Yang and Mills.:

I'l paraphrase his remarks a bit:
"A free nucleon Lagrangian, written in terms of the composite fermion fields for the proton and neutron, has an invariance under global isospin rotation, and the isospin current is conserved. Thus one has complete freedom in naming the proton and the neutron (in the absence of electromagnetism), but only at a single point in space-time. Once freely chosen, the convention must be respected everywhere throughout space-time.
This single restriction may seem, as it did to Yang-Mills, at odds with the idea of a local field theory. Furthermore, we have seen that electromagnetism possesses a local gauge invariance, and that by imposing a local symmetry on a free-particle Lagrangian it is possible to construct a correct theory of electrodynamics. In analogy with electromagnetism we are led to ask whether we can require that the freedom to name the two states of the nucleon be available independently as every space-time point. Can we, in other words, turn the global SU(2) invariance of the free field theory into a mathematically consistent local SU(2) invariance"

He then goes on, following Yang-Mills, to construct the non-Abelian Yang-Mills Lagrangian for the nucleon, pointing out the standard defect, namely that as a consequence of local gauge invariance, the Yang-Mills quanta are massless vector bosons and of infinite range and therefore cannot serve as a successful phenomenological description of the strong nuclear force which involves the exchange of massive particles.

So of course, as you pointed out, Yang-Mills were trying to develop a phenomenological description of strong interactions, specifically a SU(2)-isospin gauge theory, but hit a brick wall once the quanta their theory produced were massless vector bosons.

Lochlainn O'Raifeartaigh (Cormac's father) in his beautiful monograph "Group Structures of Gauge Theories" also refers to the 1954 paper (p.79)

"It is now generally accepted that Weyl's gauge principle can be used to describe the strong and weak interactions and well as the EM ones by generalizing U(1) to other compact Lie groups. The first extension of the principle, to the isospin SU(2) group, was made by Yang and Mills (1954)"

21. **Christine**
February 25, 2009

Moriyasu's book "An Elementary Primer for Gauge Theory" is the shortest path to learn the subject that I know of and it nicely outlines the history involved.

> In 1954, C. N. Yang and R. Mills took the bold step of proposing that the strong nuclear interaction be described by a field theory like electromagnetism which is exactly gauge invariant. They postulated that the local gauge group was the SU(2) isotopic-spin group.
>
> (…)
>
> On the tenth anniversary of the Yang-Mills theory, there was still no successful gauge theory of the nuclear forces. A significant breakthrough had been achieved, as we saw in chapter VII, by finally solving the problem of the zero gauge field mass through spontaneous symmetry breaking. And the weak interaction, not the strong force, appeared to be the best candidate for a gauge theory. Yet the insurmountable difficulty of the "non-renormalizable" infinities still plagued both the weak interaction and quantum field theory. Nevertheless, the final step toward a successful gauge theory was taken almost simultaneously by Steven Weinberg at MIT and Abdus Salam at Imperial College, London. They boldly ignored the problem of the "non-renormalizable" infinities and instead proposed a far more ambitious unified gauge theory of the electromagnetic and weak interactions.
>
> The idea of unifying the weak and electromagnetic interactions into a single gauge theory did not originate with Weinberg and Salam. It had been suggested much earlier by Schwinger and Glashow. As we noted in chapter VI, Glashow and Schwinger had also pointed out that the leptons should carry weak isotopic spin like the mesons and baryons involved in the weak decays. Thus much of the detective work had already been done in untangling the weak interactions and laying the logical foundation for a gauge theory. However, the essential ingredient missing in all of these earlier theoretical attempts was an understanding of the origin of the gauge field masses. Weinberg and Salam were the first to realize that the Higgs mechanism could supply the last piece of the puzzle for a unified gauge theory.

22. **Indrajeet**
February 25, 2009

Rajagopal,
Thanks a lot. I will have a look at it.

23. **Peter Woit**
February 25, 2009

I've added to the posting a list of books that I've found most useful for learning some of the history of the standard model.

24. **Tony Smith**
February 25, 2009

Peter, would you consider adding to your list of books

Constructing Quarks: A Sociological History of Particle Physics
by Andrew Pickering (Chicago 1984)

Tony Smith

25. **Chris Oakley**
February 25, 2009

Yang-Mills is sometimes called Yang-Mills-Shaw theory on this side of the pond:

http://www.hull.ac.uk/php/masrs/reminiscences.html#Anchor-3.4-55015

26. **H-I-G-G-S**
February 25, 2009

Peter and minions,

I may not have been clear, or perhaps you are deliberately misunderstanding me. In either case it might be good if I amplify my remarks. I'm quite aware that isospin is a symmetry of the strong interactions and that Yang and Mills were discussing strongly interacting particles like the neutron, proton, and pion. They were obviously trying to make isospin a local symmetry in the context of the strong interactions. So I'm sure they viewed what they were doing as related to the strong interactions. If that's all you mean by saying that "Yang-Mills was invented to describe the strong interactions" then I have no quibble with you. But your statement, and the statement in many of the quotes in the comments sound quite a bit stronger. It makes it sound like they were trying to develop a full theory of the strong interactions based on gauging isospin. While this has a certain historical resonance to it since Yang-Mills theory ended up being the basis for QCD, I suspect it overstates the case. For example, at the time the strong interactions were known at large distances to be mediated by pion exchange. Yang and Mills make no attempt to describe the pion or its affects in their theory. They don't try to describe the neutron or the proton. What they do is to gauge isospin and then speculate that the apparently massless isovector spin one particle they find might acquire mass by some mechanism. So it seems they were trying to describe features of the strong interactions, and perhaps new particles resulting from the strong interactions, but I still see no evidence that they had any reason to think they were developing a full theory of the strong interactions.

What undoubtedly gave their theory a strong boost was the discovery 7 years later of a massive spin one isovector partice, the rho meson, just as predicted except for the awkward question of its mass. This, along with the work of Sakurai around 1960 gave Yang-Mills theory a strong boost, but even then it was used to describe features of the strong interactions such as Vector Meson Dominance and not as a comprehensive attempt to describe the full spectrum and structure of the strong interactions.

In general I suspect that history is often more complicated than the "just so" kind of descriptions that Christine quoted which make it sound like Yang and Mills almost had QCD except for using SU(2) instead of SU(3).

27. **Peter Woit**
February 25, 2009

H-I-G-G-S,

The historical record of the context of the Yang-Mills paper is unambiguous: they were hoping to get a theory of the strong interactions with a gauge theory of isospin, which they showed would imply that interactions were due to exchange of isospin 1 vector mesons. This idea doesn't work, which they clearly realized since they didn't pursue it after the original paper.

Your comments here about this being "not true", telling me to go read the paper, that you "don't believe the claim that they were trying to describe the strong interactions", that maybe they were not trying to develop a theory of strong interactions, and that Gross might not know what he was talking about since he was only 13 in 1954 are just completely ridiculous. All you are doing is providing an amusing example of how some particle theorists don't know the history of their field, are quick to accuse others of not knowing what they are talking about, and completely incapable of admitting it when they make a mistake.

No one claims anywhere that Yang and Mills "almost had QCD", you're trying to justify your own absurd comments by putting sillier ones in other people's mouths.

28. **Christine**
February 26, 2009

H-I-G-G-S wrote:

In general I suspect that history is often more complicated than the "just so" kind of descriptions that Christine quoted which make it sound like Yang and Mills almost had QCD except for using SU(2) instead of SU(3).

No. I just included some excerpts that I thought being relevant for the discussion, but evidently one should read the book to have the complete outline that Moriyasu offers. In any case, I hope the following excerpts will complement my previous ones:

> Although the Yang-Mills theory failed in its original purpose, it established the foundation for modern gauge theory. The SU(2) isotopic-spin gauge transformation could not be regarded as a mere phase change; it required an entirely new interpretation of gauge invariance. Yang and Mills showed for the first time that local gauge symmetry was a powerful fundamental principle that could provide new insight into the newly discovered 'internal' quantum numbers like isotopic spin. In the Yang-Mills theory, isotopic spin was not just a label for the charge states of particles, but it was crucially involved in determining the fundamental form of the interaction.
>
> (…)
>
> In retrospect, it is clear that both the original theory of Weyl and the Yang-Mills theory failed because they were too early for a gauge theory at their respective times. When Yang and Mills proposed their revolutionary idea for an SU(2) gauge theory of the strong interaction, an adequate understanding of the essential properties of the nuclear forces was still lacking. The final successful rediscovery of gauge theory had to wait nearly ten more years for several crucial experimental and theoretical developments to provide the necessary clues.

From my previous quotes and the present ones, I see in no place any claims that you suggest. I agree that Moriyasu's book is a very concise and elementary treatment of gauge theory but I fail to see where he got history unbalanced. Since I just included excerpts, reading the book is advisable. In any case, Peter has included a nice list of references on the matter.

Thanks,

Christine

29. **Christine**
February 26, 2009

I still see no evidence that they had any reason to think they were developing a full theory of the strong interactions.

Cao in his "Conceptual Developments of 20th Century Field Theories" puts the matter under two main motivations (pages 273-274):

> The Yang-Mills theory emerged entirely within the framework of the quantum field programme, and was motivated by two considerations. **First**, more and more new particles were discovered after the Second War, and various possible couplings among those elementary particles were being proposed. Thus Yang and Mills felt it necessary to have some principle to choose a unique form out of the many possibilities being considered. The principle suggested by Yang and Mills is based on the concept of gauge invariance, and is thus called the gauge principle. **Second**, in choosing a proper gauge symmetry, Yang and Mills were driven by curiosity to find the consequences of assuming a law of conservation of isospin, which was thought to be the strong interaction analogue of electric charge.

Since I am not certain whether this conciliates Peter and H-I-G-G-H's points of view, I let the matter with you.

30. **Peter Woit**
February 26, 2009

Lubos weighs in supporting H-I-G-G-S against Gross and Weinberg here

http://motls.blogspot.com/2009/02/history-of-yang-mills-theory-and.html

According to him, the desire of Yang and Mills to come up with a theory of the strong force had nothing to do with their gauging isospin. He does draw some historical lessons, noting correctly that a theory developed for one purpose may turn out not to work for that, but find use elsewhere. For instance, a theory once thought to be a spaceship capable of giving a TOE may turn out to be a toaster capable of approximately describing the viscosity of a quark-gluon plasma….

31. **Peter Woit**
February 26, 2009

Tony,

The list I put up is certainly not exhaustive, and "Constructing Quarks" is also a good history of subject. There are quite a few others, but I'll leave that list as a personal one of the things I most enjoyed or found useful.

32. **H-I-G-G-S**
February 26, 2009

Peter,

Your statement "The historical record of the context of the Yang-Mills paper is unambiguous:" is certainly true, but it is also not what we are talking about. I agree that the context is clear, but the motivation and thoughts of Yang and Mills, which is the topic under discussion, are not.

The historical record is rarely unambiguous on any topic, and you have yet to cite a single iota of primary material, by which I mean statements in the original paper or interviews with Yang and Mills or their colleagues near the time they were doing the work. I'm just as well acquainted with the secondary material as you are, but its not much help in trying to figure out what Yang and Mills were actually thinking at the time.

33. **Peter Woit**
February 26, 2009

H-I-G-G-S,

This is just ridiculous. Yang, like almost every other major theorist of the day, at that time was trying to find a theory of the strong interactions. The paper explicitly mentions this context, and every knowledgeable person who discusses it describes it as an effort to come up with a strong interaction theory, except you.

You completely ignored the what I quoted from the paper itself:

"We then propose that all physical processes (not involving the electromagnetic field) be invariant under an isotopic gauge transformation…"

What physical processes do you think he's talking about? If it's not the strong interactions, it's gravity or the weak interactions. If you're claiming that Yang had the idea of getting a weak interaction theory by gauging the right SU(2) a couple years before Schwinger, that's a dramatic new advance in the history of the subject. Except I don't think anyone will believe it except you.

At the end of the paper, they discuss experimental limits on the mass of the vector boson in their theory. They explicitly say it should decay into pions with a lifetime less than 10^-20 seconds. This is a strongly interacting particle.

34. **D R Lunsford**
February 26, 2009

It's always astonished me how ignorant people are of physics history, how quick to make pronouncements when it's patently obvious that they have never bothered to even learn what the papers were, much less read them. This is hardly new – it was true when I was in school 25 years ago.

Of course everything you say about Yang-Mills and their motivation – which had to be enormously strong in the face of Pauli's criticism (he himself had tried the idea a year or so earlier but did not publish it) – is entirely correct.

-drl

35. **Thomas**
February 26, 2009

Yang makes a few remarks concerning his motivation in his "Selected Papers (with commentary)" book. He says that as more and more hadrons were discovered there was clearly a need to find a general principle that would constrain the possible interactions, and he was looking at local isospin gauge invariance for this purpose. He also remarks that Pauli's unpublished note was called "Meson-Nucleon Interaction and Differential Geometry".

This idea is not totally wrong, by the way (if the Higgs mechanism is added). If the rho meson is a gauge particle, then the rho-pi-pi, rho-N-N, etc couplings are related. These relations are experimentally satisfied to a reasonably good (20%, or so) accuracy (for reasons that are not understood very well).

36. **Sumar Ongi**
February 26, 2009

> This idea is not totally wrong, by the way (if the Higgs mechanism is added).

The idea of gauging isospin and making the rho triplet massive by means of the Higgs mechanism was, I think, what Weinberg had in mind before realizing that it could be applied more successfully to leptons.

In any case, isospin symmetry cannot be spontaneously broken in QCD (like chiral symmetry is), according to a theorem by Vafa and Witten.

37. **H-I-G-G-S**

February 26, 2009

Thomas,

The reason for those relations is starting to be understood as a consequence of string dual models of QCD where isospin is in fact a gauge symmetry in the higher-dimensional dual theory.

38. **Shantanu**
February 27, 2009

Peters and others , see these talks at PI which does touch upon some historical aspects.
http://pirsa.org/09020027/
http://pirsa.org/09010009

39. **a quantum diaries surivivor**
February 27, 2009

Hi Peter,

I think the way we study even scientific matters in Italy is different from the way these are studied elsewhere. Maybe this is a hindrance, maybe it is an advantage – but I feel cultured by having studied the Standard Model not as it is now, but how it was developed – and I was 7 years old when the theory was sealed.

During my studies, by no means deeper than those of most of my italian colleagues, I for instance took great pleasure in understanding in deep detail what drove the choice of V-A over S-T, for instance, and the historical developments of the mid fifties.

As for Yang-Mills theory: I have never had any doubt that their aim was understanding strong interactions. I thank Professor Antonio Bassetto for teaching me a thing or two in his powerful course of Quantum Field Theory.

Cheers,
T.

40. **Ghame**
February 27, 2009

H-I-G-S-S, that's interesting. Is SU(6) symmetry also a gauge symmetry in the dual model?

41. **H-I-G-G-S**
March 1, 2009

Ghame,

Which SU(6) do you mean? The SU(6) of six flavors (u,d,s,c,b,t) or the non-relativistic SU(6) mixing rotations and isospin?

42. **Ghame**
March 2, 2009

H-I-G-G-S, I meant the nonrelativistic SU(6). The reason why I asked is that the octet meson-octet baryon-decuplet baryon couplings obey SU(6) with pretty good accuracy. So, if the pretty badly broken isospin relation between rho-pi-pi and rho-N-N couplings comes from a gauge symmetry, why not the SU(6) relation for decuplet couplings? Sure, SU(6) is not an internal symmetry so it seems unreasonable for it to be a gauge symmetry but,t what do I know about dual models?

HEP Budget News

February 24, 2009
Categories: Uncategorized

These are dramatic times for news about the US HEP budget, with the FY2009, FY 2010 budgets and stimulus package all coming together at the same time. The final stimulus package was very favorable for DOE and NSF, providing an extra $1.6 billion for DOE science and $3 billion for NSF.

Today there's a draft FY2009 budget out of Congress, news about it here from Adrian Cho at Science magazine. HEP at DOE was down in FY2008, at $721 million (after a $32 million supplemental appropriation). For FY2009, which is half over, the draft budget has $796 million. There should be stimulus package funding on top of that. A proposed FY2010 HEP budget is being presented to OMB this week, and the President's FY2010 budget proposal to Congress should be released in April. In the same draft, NSF research will get an overall increase of $362 million to a total of $5.18 billion (see here).

Today HEPAP is meeting in Washington, with presentations starting to appear online here. There are no decisions yet about what the supplemental funds will be used for, but according to the slides the guiding principles are to accelerate ongoing construction projects and update labs, increase operations and support of experiments at user facilities like Fermilab, and fund "selected research programs", minimizing commitments in out-years. A program to support graduate students and early career scientists is under discussion.

More from HEPAP and more details about the FY2009 budget should be available soon.

Update: An outline of the FY2010 budget proposal from the President is now available. The proposed NSF budget is $7.045 billion, an 8.5% increase from the recent FY2009 omnibus legislation. The $3 billion from the stimulus package is on top of this. No detailed numbers, but priorities listed include "substantial increases for NSF's prestigious Graduate research Fellowship and Faculty Early Career Development programs." and increased "support for promising, but exploratory and high-risk research proposals that could fundamentally alter our understanding of nature, revolutionize fields of science, and lead to radically new technologies." Sounds kind of like FQXI….

Comments

1. **String-freak**
   February 28, 2009

   Hi,
   This might be irrelevant here. But the list of speakers for Strings 2009 is out on its website.

Yang-Mills and Wikipedia

February 26, 2009
Categories: Uncategorized

I was recently looking up references about the history of Yang-Mills theory in order to write about it here, and one thing I ran into was the Wikipedia entry for Yang-Mills theory . It has three sections, the first two of which are standard material, but I was surprised to notice that the last section is completely unconventional, promoting the ideas of Marco Frasca and referencing two of his papers. It was written by an anonymous "Pra1998", who I'm guessing is Frasca himself.

I've never tried to edit Wikipedia entries before, but I thought it would be a good idea to remove this material, which is not the sort of thing that belongs there. My edit was immediately reversed. I tried again, justifying this in the discussion section, but the material is still there. At this point, I give up, lacking time to deal with this and any understanding of what mechanisms are available in Wikipedia to deal with such a situation.

Over the last few years I've been finding myself consulting Wikipedia entries more and more, especially ones on mathematics. The quality of the mathematics entries is often shockingly high. In the past if one ran into mention of some mathematical concept one didn't know about, tracking down a readable account of it was often insanely difficult. Now, one can often just look it up in Wikipedia and find a well-written, concise explanation of just the sort needed. It's a wonderful and incredibly valuable resource, and I'm mystified about how such a high quality is achieved and maintained. I hope the same mechanism, whatever it is, can work for the Yang-Mills entry.

Comments

1. **Marco Frasca**
   February 26, 2009

   Dear Peter,

   I have not appreciated your behavior. What is present in that section is a classical solution of Yang-Mills equations that is also present in the Smilga's book

   http://www.amazon.com/Lectures-Quantum-Chromodynamics-V-Smilga/dp/9810243316/ref=sr_1_1?ie=UTF8&s=books&qid=1235660829&sr=8-1

   These are solutions with all equal components. Also the title of the section gives immediately hints of the content. You can remove, as I have already said, all the references about my work and the section still gives useful information that you arbitrarily removed without any clear judgement.

   This is unacceptable and relies on your authority. No good for someone criticizing this kind of behaviors.

   Marco

2. **Peter Woit**
   February 26, 2009

   Marco,

   I see you don't deny being the author of this material. I've no interest in wasting time arguing with you about your ideas, here or anywhere else. I'll just point out that it's incredibly inappropriate for you to try and insert them into a Wikipedia entry in this way. Please do not do this.

3. **Marco Frasca**
   February 26, 2009

   Dear Peter,

   My regret now is for Wikipedia. Vandals intervention is just begun.

   Marco

4. **Turing E.**
   February 26, 2009

   Peter,

   First off, you are an expert in the field. As such, that in itself gives you some weight when it comes to editing articles in the field. Check out http://en.wikipedia.org/wiki/Wikipedia:EXPERT for more information. Be sure to put your credentials on your user page and you can even link to the Wikipedia article about you!

5. **Oakland Peters**
   February 26, 2009

   Peter,

   You are absolutely correct about the quality of the mathematics articles on wikipedia. Their quality has increased steadily over the last five years or so. At this point, it would not be a stretch to say that wikipedia is the most readable guide to mathematics on the web — at least up to the level of graduate level applied mathematics (past that point the articles tend to resume the standard level of mathematical incomprehensibility).

   However, the physics articles of wikipedia have not shown the same level of growth. This sort of disagreement has been frequent, and reoccurring.

   Why is this? Does the physics community lack general agreement on what should and should not be considered established knowledge?

6. **none**
   February 26, 2009

   In my experience, it is hopeless to try to argue with people like Frasca who don't speak English but who think they do. They just get more and more angry and incoherent.

   Wikipedia is very much overrated for just the reason you found: there is a tremendous amount of partisan wrong information on it that knowledgeable people are too busy to correct.

7. **Torus**
February 26, 2009

Somebody also vandalized The Dispersive Wiki page for Yang-Mills with this nonsense.

8. **Marco Frasca**
February 26, 2009

Just a question. Why do you consider an authority a person with just 15 publications in 26 years?

http://www-spires.fnal.gov/spires/find/hep/www?rawcmd=find+a+woit%2C+p&FORMAT=WWW&SEQUENCE=

Marco

9. **Peter Woit**
February 26, 2009

Marco,

The problem is that your idea of vandalism and other people's is very different. Most people consider what you are doing to these online encyclopedia entries, trying to stuff them with your ideas and references to your papers, to be vandalism.

Oakland,

Actually I've found even the most advanced mathematical material to be often quite good. Some of this is very specialized and inherently requires a lot of background to appreciate, but the authors do a very good job with it.

One reason for the difference may be that high-level mathematics tends to repel non-experts, whereas certain areas of high-level fundamental physics attract a large number of people who want to flood any high-quality information source with their own ideas. It's not going to be very rewarding to put work into this kind of expository writing if it's going to be vandalized by people trying to promote themselves.

10. **anon.**
February 26, 2009

I don't see why he would feel the need to spam Wikipedia with his ideas. His papers get plenty of citations from some guy who is *also* named (and this is a crazy coincidence!) Marco Frasca.

11. **A.J.**
February 26, 2009

Marco,

Please don't try to change the subject to Peter's credentials. That's a loser's tactic.

You need to make the case that the material you inserted belongs in the main article on Yang-Mills theory. I don't think this is possible. The main Yang-Mills article is supposed to be a broad overview of the most important parts of the subject. It's not an appropriate place for this sort of specialized material.

12. **D R Lunsford**
February 26, 2009

You might as well forget it with physics – there are too many people – students I suspect – with agendas. I tried with the Dirac equation and while at first the presentation was said to be clear, I found that it was almost immediately vandalized – someone who knew neither German nor Grassmann kept changing "Lineale Ausdehnungslehre" (correct) to "Lineare.." (incorrect) – it became comical to see how quickly the change back to the correct form would be reversed – so I just gave up. I suspect there are far fewer people in math who are willing to blunder ahead with no understanding. WP tends to be very good also with history and popular culture. Forget physics for the most part.

-drl

13. **Ryan Dickherber**
February 26, 2009

Maybe you can't correct the Wikipedia article, but this post is a fine substitute. Anyone who delves even slightly deeper than the Wikipedia article will come across it and realize the Wikipedia article is not the end of the story.

14. **Joe S**
February 26, 2009

Ah, a chance to be moderately on topic!

I suspect this kind of controversy is why my daughter's first year university professor warned his students not to cite authoritatively anything in wikipedia in their research.

15. **db**
February 26, 2009

I'm not qualified to argue the merits of the particular work, but wikipedia does have a no original research policy:

http://en.wikipedia.org/wiki/Wikipedia:No_original_research

My reading of it is that you don't get to post your own work, and you shouldn't get around the restraint by citing your own work.

A bit of a shame, as I have a wonderful theory about….

16. **Coin**
February 26, 2009

Dunno if this helps, but in the past when I have run into a situation where someone is deadset on keeping something incorrect in a wikipedia article, I have found that the quickest and most effective thing to do (since edit reversal wars are simply unwinnable) is visit #wikipedia on irc.freenode.net and ask for help. Wikipedia does have a layer of editors and people who understand how to navigate wikipedia's arcane conflict resolution mechanisms who seem to be legitimately interested in keeping the site clean, the problem is getting their attention…

Of course if you have a highly trafficked science blog, that probably is a good way of calling attention to problems too…

17. **Chris W.**
February 26, 2009

Well, at least the disputed section ("Integrable solutions of classical Yang-Mills equations and QFT") has been helpfully annotated. 🙂

Marco: Out of simple courtesy it seems to me that you should back off on this and present this material on your blog. Using a Wikipedia article on a major topic to present a non-standard or relatively esoteric sub-topic strikes me as a disservice to the readers of Wikipedia, and a fairly obvious attempt to freeload off of whatever claim to authority and reliability Wikipedia has managed to earn. (Of course, some may be confusing notoriety with authority and reliability.)

18. **Yatima**
February 26, 2009

Gentlemen,

Is there really a point in getting into debates about the contents of "Wikipedia" also known as "Jimbo Wales' Big Bag of Trivia Online?". I'm sure there are better venues for research or publication (maybe scholarpedia?). I do not deny that there are interesting nuggets to be found in there, however, for an (I'm afraid) realistic take on Wikipedia by "yoof" culture one need just google "wikipedia + scribblings on a truck stop bathroom wall" (readers who can't stand 'shock jock' talk should beware of clicking on the first link which is displayed).

19. **Headbomb,**
February 26, 2009

Hello, after you pointed out the problems at Yang-Mills theory, I reviewed the contributions of Pra1998, and there is another one that contains publications from Frasca (Perturbation theory, found here
http://en.wikipedia.org/wiki/Perturbation_theory). If you people could comment on these additions, it would be much appreciated:

On the Perturbation theory article

here

and

here.

Thanks to all who helped pointing out the problems in the Yang-Mills theory article. I also would like to invite you all to join the Physics WikiProject:
http://en.wikipedia.org/wiki/Wikipedia:WikiProject_Physics

20. **ll**
February 26, 2009

that's why math is my favourite subject in high school: it's fair, u can't cheat easily in front of truth and greatest mind of human beings.

21. **ML**
February 27, 2009

It is surprising for me as a young mathematician the love for Wikipedia by grown ones.

Of course Wikipedia is a great source when coming to a first approach to something that one does not know or haven heard about (specially for the youngest or for people who are far away from the neuralgic points in math research).
But nothing more than that!

The articles there have no referee, and anyone can write them trying to sell his own ideas. This is a global community and for me is ok. Why is it ok for me? Just because it is just a first approach. Then one can go to the arxiv or to mathscinet and look for a nice paper and take the effort to learn. It is a long process but we are being paid for it.

I just think that Wikipedia is the effort of many trying to help others, or even selling themselves (it does not matter), but just a tool. Like a good or not so good (depending on the people) conversation on maths.

I just hope that in the future the people will NOT end up citing wikipedia in their papers as a source!

22. **davetweed**
February 27, 2009

ML mentioned "Then one can go to the arxiv or to mathscinet and look for a nice paper and take the effort to learn. It is a long process but we are being paid for it." I entirely agree that it takes long effort to learn something in detail, but it's worth remembering that not everyone who uses even relatively abstract mathematics is being paid for it (and even if their employer accepts spending time figuring out what's already in the literature that's applicable, we may only be being paid to "learn enough to produce results from it" rather than truly understand it). I think that people tend to forget that wikipedia is an attempt to make a sort-of encyclopedia, and the same conventions ought to apply. Would you cite a printed encyclopedia article? I would only if the material was so well-known that the only reason for including a citation was to keep reviewers happy. Otherwise, there's going to be more focussed sources that are more suited to the task in hand.

23. **Engineer**
February 27, 2009

Scholarpedia actually promises to address some of the peer-review issues that wikipedia has. Unfortunately it does not have the following or diversity of articles that wikipedia has. I think that if a true scholar is concerned about the bias in the wikipedia articles, than a better way to alleviate their frustration is to volunteer to write and maintain an article on scholarpedia, especially if they have the credentials. There are some additional rules about how one gets elected/selected to be a curator, so I don't know how hard it would be to actually gain the power to control an article, but if people are interested it might be worth checking out.

http://www.scholarpedia.org/

24. **Don**
February 27, 2009

Gentleman,
I can't understand what is the real point about this post… Is there any mathematical/physical problem with those classical-YM solutions?
I am sorry, but all this argument here doesn't seems to be based just on scientific grounds…
Cheers
Don,

25. **tomate**
February 27, 2009

Italian wikipedia has the same problems:

http://it.wikipedia.org/wiki/Teoria_quantistica_di_Yang-Mills

A personal thought. I think scientific articles in wikipedia are both too technical and not enough. I mean: from a generalistic encyclopedia I would expect the articles to be accessible to laymen, at least in the first few sentences. From a specialistic encyclopedia I would expect rigour and carefulness. It seems to me that there is a sort of clash among opposite forces in Wikipedia that mediates these two positions with the result that articles are neither simple nor correct, and in the interstice anything can insert. Moreover, they almost never are fluent: different sections of an article are decorrelated one from another, there often is not a coherent development.

(Of course, it's not an easy task to simplify sentences like

Yang-Mills theory is a gauge theory of quantum field theory based on the SU(N) group.

but we have scientists who do a great job popularizing stuff.)

26. **Peter Woit**
February 27, 2009

Don,

See the discussion about this question in the talk section of the Wikipedia article. Independent of the issue of the merits of the material involved, I don't think it's hard to see that it's a very bad idea to allow people to add sections to Wikipedia entries about the most central concepts in physics promoting their own ideas and referencing their own papers.

27. **Don**
February 27, 2009

Peter, you have a point.
However, I just want to emphasize mine:
it seems me that since one can not prove that Dr. Frasca was the person that introduced the "heretical section" in Wikipedia, one should not cruficy a person that, in principle, is "Not even wrong" =)
Cheers,
Don.

28. **woit**
February 27, 2009

Someone is submitting anonymous comments here defending Frasca and attacking me, coming from the same IP address as Frasca's first comment here. They have been deleted.

29. **Peter Woit**
February 27, 2009

I'm shutting off comments on this section, and will delete any comments on this blog re: Frasca, since I don't want to waste any more of my time dealing with him, and he is flooding this blog with anonymous comments. If you want to debate his Wikipedia entry, do it using Wikipedia's discussion feature.

Worth Reading

February 28, 2009
Categories: Uncategorized

Lots of wonderful blog postings about math and physics out there worth reading, with a small sample including these:

- Jester on SUSY and the Higgs.
- Dmitry Podolsky has some very useful guests posts on various topics, including chirality on the lattice (here and here), and 3d-gravity.
- Rigorous Trivialities has lots of nice expository postings about algebraic geometry, with the latest by Columbia's own Matt DeLand on the K-theory of coherent sheaves.

From CERN, here's a report from JoAnne Hewett about the summary session discussing the Chamonix workshop on the state of the LHC and plans for getting it up and running. The current schedule, which is tight, has collisions starting in November of this year, and running for nearly a year until late 2010. To accumulate an amount of data that would allow some significant new results (about $50pb^{-1}$) should take six months or so, until mid-2010. The hope is to get to 2-300 pb^{-1} later in 2010 before shutting down. This would not allow the LHC to do better than the Tevatron on the Higgs. For that, we'll probably have to wait for data from the 2011 run. By the time this data is in, the Tevatron should have about $10fb^{-1}$ to analyze, and may already have seen evidence of the Higgs.

Also at CERN, there has recently been a conference on the topic From the LHC to a future collider. Lots of interesting talks about future possiblities, including the ILC, CLIC, colliding LHC protons with electrons, as well as the possibility of a muon collider.

More and more areas of mathematics have a blog, here's one for motivic homotopy.

Bert Schroer has updated two of his long articles that discuss both the sociology and conceptual framework of quantum string theory: String theory deconstructed, is dedicated to Philip Anderson and has a new section about history of the subject in Germany, and String theory and the crisis of particle physics, which is dedicated to Juergen Ehlers.

I hadn't realized that Physics World has a blog. Among the latest entries are two reports (here and here) about Lenny Susskind's recent talk to 700 people in Bristol about Darwin and the Cosmic Landscape. Susskind is still at it selling string theory and the multiverse to the public, no matter how unconvinced his colleagues may be:

> Just as there is a vast landscape of biological designs, our best theories of physics imply an equally vast landscape of universe designs. String theory provides an analogue of DNA for the universe and modern cosmology makes use of a principle of mutation that creates a tremendously large multiverse.

It seems that

> The central tenet of Susskind's talk was that string theorists should look to Darwin because he "set the standard for what an explanation should be like".

Funny, I always thought it was physics itself which set such a standard for the biological sciences, but I guess the idea now is to give up on that and have them be the gold standard.

While many theoretical physicists in their later years try and go for the Einstein look, according to one of the Physics World bloggers, Susskind is doing a good job of looking like Darwin.

Comments

1. **anon.**
   February 28, 2009

   The argument Susskind makes for the large size of the multiverse being a benefit to physics is basically the very small positive value of the cosmological constant. You need a large multiverse to accommodate the possibility of such a small positive value arising by accident. String theory predicts 10^500 vacuum states, so it is (allegedly) inconceivable that it doesn't contain states with suitably small positive cosmological constants. This is similar to evolution, where a vast number of DNA mutations occur, but we only get to see those which survive. The other universes in the multiverse were either unsuitable for the evolution of human observers, or else they are indeed populated by people in the same condition as ourselves, pondering on the string landscape!

   But the clever thing about evolution is that it makes falsifiable predictions. E.g. if conditions change, the individuals best able to adapt are those to survive and reproduce, so a new strain emerges which is adapted to the new conditions. If you use a new disinfectant inefficiently on bacteria, the survivors will be those which are more resistant, so you evolve a new hardy strain.

   > Funny, I always thought it was physics itself which set such a standard for the biological sciences, but I guess the idea now is to give up on that and have them be the gold standard.

   It might not be the most quantitative science, but biology does make checkable predictions, and is built upon experiments and observations, not speculation. If string theory was really analogous to evolution theory, there wouldn't be any problem.

2. **Shantanu**
   February 28, 2009

   Peter , did you find anything interesting in Maldacena's talk at
   KITP in the string theory conference?

3. **Marion Delgado**
   February 28, 2009

   This brings to my mind the question of what you thought/think of Lee Smolin's possibly evolving universes theory. For instance, selection of possible universes for having black holes?

4. **Peter Woit**
   February 28, 2009

   Shantanu,
   I haven't followed closely the topic of Maldacena's talk, CFT methods applied to LHC collider physics.

Marion,
I was never a big fan of Smolin's CNS version of the multiverse, which involved some analog of "natural selection" and was supposed to be predictive, unlike the string theory landscape. From what I remember Susskind was a critic of CNS, I'm surprised to see him invoking Darwin and natural selection. As far as I know, the string theory multiverse has an anthropic selection aspect, but no notion of Darwinian evolution.

5. **Hartford Wheeler Dealer**
   March 1, 2009

   Having recently completed Not Even Wrong, beneath the heading "Worth Reading" is perhaps not an unfitting place to post? It is a fairly rare occurrence that the author of a book is so accessible to his or her audience. Please allow the following comments.

   A central contention is that string theory has an increasingly dubious place within the realm of science. I have deliberately chosen not be buy Lee Smolin's Trouble With Physics despite knowing its reputation of more bibulously imparting similar content for the less knowledgeable learner. Only about 1/3 of the earlier book is absorbable to me though.

   Some things perhaps can be done better. Despite any impression that a scientist tends not highly prioritize floridity in their expression, your comparison (analogy/simile?) criticizing the 'Beauty' of string theory takes the form of a cliché at first. Also, an example involving postmodern humanities departments probably has multiple motivations behind it; though none of them are obvious? There may not have been ground to reference Lyotard?

   My hardcover copy, published by Basic Books, has a yellow spine and black cover. The image under the title is not explained? Maybe the small circular object represents some type of compactified multidimensional space?

   The overall lucubration is exciting. A response saying "I hope you are right" would not seem appropriate. Nevertheless, the implications have the potential to be incredibly important! Its impacts must be being felt.

6. **Robert**
   March 11, 2009

   Susskind doesn't look like Darwin, but he and Alain Connes look alarmingly similar. Has anyone ever seen them in the same room?

Living With Infinities

March 3, 2009
Categories: Uncategorized

Steven Weinberg has a new preprint out entitled Living with Infinities, which is the written version of a recent talk given in memory of Gunnar Källén. Källén was a Swedish mathematical physicist, who died in a plane accident in 1968 at the age of 42. For more about him, see this by Ray Streater.

Weinberg begins by recalling his first first trip to the Bohr Institute in 1954, where he met Källén, who suggested a research problem involving an exactly solvable QFT model invented by TD Lee. The solvability of this model made it possible to use it for investigating renormalization outside of perturbation theory. Källén and Pauli showed that the model was non-unitary, Weinberg showed that it had states with complex energies.

In his talk, Weinberg describes Källén's work during the 1950s investigating the question of how QED gets renormalized, outside the context of perturbation theory. Källén found an argument showing that at least one of the renormalization constants must be infinite but Weinberg notes that Källén never claimed the argument was rigorous, and concludes: "As far as I know, this issue has never been settled." He goes on to give the now conventional Wilsonian description of the non-perturbative situation and the possibility that no non-trivial continuum limit exists. This question is now considered somewhat academic, since it is assumed that QED gets unified with other interactions and ultimately with gravity at energies below those at which the behavior of the coupling becomes problematic.

Weinberg states in his abstract that he will present his personal view on how the problem of infinities may ultimately be resolved. Here's what he has to say about this:

> My own view is that all of the successful field theories of which we are so proud — electrodynamics, the electroweak theory, quantum chromodynamics, and even General Relativity — are in truth effective field theories, only with a much larger characteristic energy, something like the Planck energy….
>
> None of the renormalizable versions of these theories really describes nature at very high energy, where the non-renormalizable terms in the theory are not suppressed. From this point of view, the fact that General Relativity is not renormalizable in the Dyson sense is no more (or less) of a fundamental problem than the fact that non-renormalizable terms are present along with the usual renormalizable terms of the Standard Model. All of these theories lose their predictive power at a sufficiently high energy. The challenge for the future is to find the final underlying theory, to which the effective field theories of the standard model and General Relativity are low-energy approximations.
> It is possible and perhaps likely that the ingredients of the underlying theory are not the quark and lepton and gauge boson fields of the Standard Model, but something quite different, such as a string theory. After all, as it has turned out, the ingredients of our modern theory of strong interactions are not the nucleon and pion fields of Källén's time, but quark and gluon fields, with an effective field theory of nucleon and pion fields useful only as a low-energy approximation.
> But there is another possibility. The underlying theory may be an ordinary quantum field theory, including fields for gravitation and the ingredients of the Standard Model…

He then goes on to describe the "asymptotic safety" scenario where the renormalized couplings approach a non-trivial fixed point as the energy cut-off is taken to infinity, a fixed point which presumably cannot be studied in perturbation theory, writing:

> Other techniques such as dimensional continuation, 1/N expansions, and lattice quantization have provided increasing evidence that gravitation may be part of an asymptotically safe theory.

and referring to papers by Reuter/Saueressig, Percacci, and Litim (Percacci has a web-page about asymptotic safety here). He ends with the conclusion that, since string theory might not have any role in a fundamental theory, with only QFT needed to understand quantum gravity:

> So it is just possible that we may be closer to the final underlying theory than is usually thought.

In these days of string landscape ideology, this possibility is an important one to keep in mind.

Comments

1. **Shantanu**
   March 4, 2009

   Peter you mentioned
   "This question is now considered somewhat academic, since it is assumed that QED gets unified with other interactions and ultimately with gravity at energies below those at which the behavior of the coupling becomes problematic."
   Does this unification solve the problem?
   Also a related question (re QED). Are you worried about issues such as the
   Landau pole?
   thanks

2. **Peter Woit**
   March 4, 2009

   Shantanu,

   The problem that Weinberg explains is the Landau pole problem, and I agree with his description of it.

   Conventional guts don't solve the problem, because they require a Higgs sector, and this is not asymptotically free, so has the same problem as QED.

3. **Peter Orland**
   March 4, 2009

   Shantanu,

   Yes, unification solves the problem for QED. The Landau pole of QED occurs at a higher energy than the unification scale. Unfortunately the standard model still has a problem at high energies, because of the HIggs and the U(1) sector.

   Peter: I think K\"{a}ll\'{e}n would have called himself a theoretical physicist, not a mathematical physicist. In his day, the latter referred to people who used rigorous methods to prove things about physics. There was much interaction between the two subfields, but there was a clear distinction.

4. **Sulfer Surfer**
March 4, 2009

It is remarkable that Weinberg is also the one who did much to bring the anthropic principle into physics. One of the things that has made him a great scientist, time and again, is that he seeks to understand the universe without preconditions on what form the answer will take.

5. **N. Nakanishi**
March 4, 2009

Peter: Your description concerning the Lee model is not precise. The original Lee model, in which the V particle is of positive norm,
is unitary if cutoff factor is introduced. What Kallen and Pauli investigated is the indefinite-metric Lee model, in which the bare V particle is of negative norm. In this model, there are three cases for the physical V particles: one or two real eigenvalue(s), one of which is of negative norm, one double real eigenvalue, which is Heisenberg's dipole ghost, and two mutually complex-conjugate eigenvalues. For details, see my review article, Prog. Theor. Phys Suppl. 51 (1972), Sec.12.

6. **Chris Oakley**
March 5, 2009

SW seems to have a knack of capturing the mood of the moment – lauding String Theory when everyone (except Peter, of course) was working on it and then supporting the sceptics when PW's & Smolin's books came out. But being a trendsetter himself, his measurements of the state of the art have a habit of forcing the system into a quantum state, and no doubt now that he is saying that people should be going back to QFT basics, a large number of researchers will now do just that. It is high time.

7. **Peter Woit**
March 5, 2009

Nakanishi,

Many thanks for the added information about the Lee model, it's not something I knew about before this, quite interesting to hear more about it.

Peter,

Actually I think Kallen is an interesting case on the borderlines of classification. He wrote a well-known rather phenomenological book about particle physics in the mid-sixties which I learned a lot from as a student. The bulk of his work was investigation of fundamental issues in QFT, although not at a mathematician's standard of rigor. Later on I think he worked on more mathematical issues concerning analytic behavior of amplitudes, some published in mathematical physics journals. So, I think both conventional particle theorists and mathematical physicists can claim him as one of their own.

8. **Peter Woit**
March 5, 2009

Chris,

I'd love to count Weinberg as an enthusiast for my book and arguments about string theory, but I'm afraid the evidence doesn't really support that. After a few years working on string theory himself, he did vote with his feet and move into cosmology, something that probably encouraged that trend, which is ongoing. But it's my impression that his attitude for many years has been one more of agnosticism on the string theory/QFT question, that work on both is justified, with string theory remaining as much a possible TOE as any other idea on the subject. As far as I can tell, pointing out that string theory unification has failed as an idea about finding a TOE is not on his agenda.

9. **dir**
March 5, 2009

the landau pole problem has two aspects, i think. one is that whether is it really there? the pole apears from perturbative calculation, but the pole itself is in non-perturbative region. lattice peole said that the landau pole can exist. the other is that even if it exists (almost for sure), does this imply the theory is meaningless? maybe not. it can be an evidence of some phase transition which has to be described by a totally new theory.

10. **Chris Oakley**
March 5, 2009

Peter,

Didn't Weinberg say that "the sceptics are right" when yours and Smolin's book came out 2 1/2 years ago?

As for Kallen, IMHO he did much more than average in trying to make QFT rigorous. He did not succeed, of course – but then no-one else has either.

11. **Peter Woit**
March 5, 2009

Chris,

That doesn't correspond to anything I remember, but I've mercifully forgotten some details of the "string wars".

12. **dan**
March 5, 2009

The Nova program on string theory, hosted by Kaku and Greene, Weinberg was interviewed and asked and answered with a qualified endorsement of string theory.

13. **anon.**
March 5, 2009

Weinberg 2.5 years ago mentioned that there are critics of string theory who perhaps are motivated because they want some of the money string theory is getting. He then added 'perhaps they are right' to want funding for alternatives. He didn't say string critics are right in dismissing string theory, and he still supports string theory.

See 15 July 2008 interview of Weinberg by Dawkins at http://www.youtube.com/watch?v=kNpiX8XQhJM where Weinberg states at 9 mins 12 seconds:

'It's been a little disappointing that it hasn't led to any specific breakthrough in understanding what we already know, but it's still the best game in town.'

14. **Chris W.**
March 6, 2009

Off-topic: Ilya Piatetski-Shapiro, died Feb. 21 in Tel Aviv (*New York Times*).

15. **mike**
March 8, 2009

Peter,

Has there been any success in resolving perturbative issues with the two-dimensional models and conformal QFTs?

16. **Peter Woit**
March 8, 2009

mike,

There are lots of 2d models you can solve exactly, and infinities are much easier to deal with (no coupling constant renormalization), so there isn't the same kind of problem as in 4d.

17. **Thomas R Love**
March 10, 2009

Peter, Thanks for the info on Gunnar Kallen. After reading some more about his work, I finally ordered two of his books, QED and Elementary particles.

Latest on the Higgs

March 10, 2009
Categories: Experimental HEP News

The news media are full of stories about the observation at the Tevatron of “single top” production, at a rate consistent with that expected from the Standard Model. There are talks at Fermilab going on about this today, and the papers are here and here. For an expository account, you can’t possibly do better than this one from Tommaso Dorigo.

While these results represent an experimental *tour de force*, they just confirm what is expected based on the standard model. Much more exciting would be if the Tevatron experiments can tell us something new about the Higgs and the Standard Model, and it looks like that may be coming this Friday afternoon, when a joint talk by the two experiments entitled “Higgs Results from CDF and D0” is scheduled at Fermilab. The two experiments have each collected about 5 fb^{-1} and started announcing limits on the Higgs based on analysis of up to 4 fb^{-1}, but this is not quite enough for either experiment to be able to on its own exclude at 95% confidence level the Higgs at any particular mass. For this, one needs to combine the data from the two experiments. This was done last year, with results announced last August based on 3 fb^{-1} per experiment of data. This analysis allowed exclusion of the Higgs at 95% confidence level only in an extremely narrow range, basically just at exactly 170 GeV.

I’m guessing that what will be announced on Friday is exclusion of a Higgs over a much larger mass range. For a preview of this, see page 24 of the slides of a recent talk, where a graph shows what things would look like if you took twice CDF’s data set. This would come very close to excluding a range from 160-165 GeV, and perhaps within reach of excluding a region as large as 155-175 GeV, if not now, with only a moderate amount of more data and effort. It will be very interesting to see what they have…

For more evidence that this is what we’ll be hearing, Newsweek reports that:

> This week scientists at Fermilab in Batavia, Illinois, will announce new data that not only narrows the gap between them and the coveted God Particle, but also suggests that the LHC may not be particularly well placed to make the discovery at all. The finding is a public-relations blow to the LHC and tarnishes Europe’s newly burnished image as a leader in Big Science….
>
> The Higgs, the new Fermilab data show, does not exist for a portion of the upper range, putting it in the Tevatron’s cross hairs and suggesting that the LHC may be more peripheral to the search than previously thought. “We’ve made their jobs a little bit harder,” says Fermilab physicist Dmitry Denisov, “because we’ve excluded the region they’re good at.”

As the Tevatron shows that it can exclude the Higgs in the higher end of the expected mass region, where the LHC has a huge advantage, that means that either there is no Higgs (and presumably something else more interesting to find), or it exists in the lower part of the expected mass range (above the LEP limit of 114 GeV), where it is hard to find, but the Tevatron is not at such a disadvantage to the LHC. In any case, even if things work out as currently planned, the LHC will not start accumulating the kind of luminosity needed to compete with the Tevatron in this game until their 2011 run. It now appears highly likely that the Tevatron will be running at least through FY 2011 and possibly longer(for more about this, see here).

Comments

1. **Xerxes**
   March 10, 2009

   The physics is quite interesting, but politically the spin is completely stupid. If the Fermilab public-relations machine cannot see the short-sightedness of a “the LHC is a big waste of money” message, then we will shortly be doomed to repeat the harsh lessons of the SSC.

2. **Cormac O Raifeartaigh**
   March 10, 2009

   Well said Xerxes. That passage in Newsweek could have been put a lot more diplomatically.

   Speaking of science and the media, is there any chance you could all stop talking about “single top” quark production? This sounds awfully close to single quark production, guaranteed to confuse the rest of us…

3. **Stan**
   March 10, 2009

   Frankly, I just wish people would stop calling the Higgs the “God Particle.” It’s ridiculous and only serves to confuse people. Lederman is a smart guy and a great speaker, but his whimsical name for the Higgs is far to appealing to newspaper writers who want to allude to the “science vs. religion” fights in a strictly scientific story.

   (The last thing we need if the Higgs is discovered is a bunch of smug editorials laughing at physicists who “think they found God.”)

   I also wonder how much of the Fermilab vs. CERN friendly professional rivalry is getting misinterpreted and magnified by the popular media into something more like a race or conflict. Even if CDF and D0 actually find the Higgs, it will be at such low significance you’ll want the LHC experiments to confirm it and check its properties. Still, Fermilab wants to show what their machine can do, and so is pushing it to the limit in the few years left to run. Conflict is exciting, and so I’m not surprised the media is pushing that angle.

4. **a quantum diaries surivivor**
   March 10, 2009

   Hi Peter,

   thank you very much for the appreciation of my account of the single top discovery.

   As for the Higgs, although I sometimes let go myself with optimism about CDF, I think that one should be a bit cautious about the over-optimistic interpretations of the Tevatron claims, for one reason.

   Take the single top discovery plots and for one moment imagine that we jump four years ahead in the future and what we’re looking at is a Higgs signal instead than single top: would you claim that such excess is an observation ? Yes, five sigmas are still five sigmas, but those global combined-multi-likelihood distributions contain just too many ingredients to be conclusive, if they claim to be the discovery of something that might not be there. Only because single top cannot possibly NOT be there, those plots can be really believed, in my humble opinion.

Mind you, I am not saying there is anything wrong in either the CDF or the DZERO analyses (although in my blog I explain why the DZERO result is slightly less convincing than the CDF one). All I mean to say is that the Tevatron will never be able to show a conclusive signal for the Higgs, because the Signal to noise ratio is really too small: only by combining many channels, and cooking up powerful complicated discriminants, can the Tevatron experiments obtain results on the Higgs.

The LHC advantage is there: eventually, they will have enough statistics to show a signal that has all the characteristics needed to convince everybody.

To stress the point: somebody in CDF in 1993 had seen the first evidence for top pair production by using a likelihood discriminant. Now, those were quite different times, and likelihoods were still looked at with suspicion. But back then the top quark was still not compelling, it had already been claimed falsely once, and most people were really, really sceptical with a signal shown only on a not easily interpreted variable: until a counting experiment showed a signal, no publication was allowed.

Cheers,
T.

5. **Shantanu**
March 10, 2009

I looked at the agenda pointed to my Peter. The
antimatter gravity experiment looks cool.
Hope it gets funded.

6. **Nobody**
March 11, 2009

To the first two posts: you are absolutely right; but certainly statements like "In one year, we will be competitive. After that, we will swamp them." (Lyn Evans at the AAAS meeting, middle of February) do not help to mantain the relationship on a pure scientific level

7. **Peter Woit**
March 11, 2009

Thanks Tommaso!

You make a very good point. My main reaction to seeing the details you explained of the single top result was something like "Wow, those plots are not exactly obviously convincing of anything…", and presumably any Tevatron Higgs "observation" will be of a similar nature.

But, if the Higgs really is down at 115 GeV, is it really true that the LHC will be able to see it in a really convincing way? How much luminosity will this take?

About the public relations aspects of this: I don't see any problem at all with a well-publicized Tevatron/LHC competition, trash-talking and all. Given that it's often the same people on both sides of this, I can't believe it will generate any serious bad blood among the physicists involved. It might generate some real public interest and appreciation for the subject, more so than the uncomplicated "wondrous new machine finds obscure particle" story that has been pre-sold already, and doesn't draw the public in that much. It's also true that the way the LHC story was going, Fermilab and US HEP in general were in danger of being left for dead as road-kill, something which is not exactly healthy for the subject.

8. **Coin**
March 11, 2009

The finding is a public-relations blow to the LHC and tarnishes Europe's newly burnished image as a leader in Big Science

Although I don't really see anything wrong with the idea of a friendly LHC/Tevatron rivalry, this specifically seems like a weird interpretation for Newsweek to take. From anything I've ever been able to tell there seem to be an incredible number of Europeans working at Fermilab and an incredible number of Americans working at CERN.

9. **John Baez**
March 12, 2009

If the Higgs is found, what can we do to keep the public interested in particle physics? How can we possibly top the "God particle"?

Here's my suggestion: say that "sparticle" stands for SATAN PARTICLE!

10. **PV**
March 30, 2009

Europe lost many good scientific minds during World War II so CERN was developed to bring those minds back and establish Europe as a center of scientific power. Big Science is never really about science. NASA is another example of Big Science but its roots are in the Cold War. If you want real science, it begins with good conceptual physics. If we had any real conceptual idea of what the Higgs is we would know how likely it is of finding it before the experiment.

11. **J.J. van der Bij**
April 3, 2009

There has to be a Higgs field, but not necessarily a particle.
The Higgs field may have been seen at LEP (Phy. Lett. B638, 234 (2006)).
In this case neither the LHC or the Tevatron can see the Higgs.

Particle Fever

March 12, 2009
Categories: Uncategorized

LHC media fever continues this year, with at least three books out or on the way:

The Quantum Frontier: The Large Hadron Collider by Fermilab experimentalist Don Lincoln.

Collider: The Search for the Worlds Smallest Particles by Paul Halpern.

and

The Large Hadron Collider by Lyn Evans, who knows a thing or two about the subject.

There's also a documentary entitled Particle Fever being made about the LHC, produced by theorist David Kaplan, who "has discovered some of the most recognizable extensions to the standard model of elementary particles." The film web-site has bios for five physicists who will feature prominently in the film: three theorists well-known for their work on large extra-dimensional models, one experimentalist from CMS, and one from ATLAS. The ATLAS experimentalist is described as "a leader in the search for extra dimensions." I can't find anything about the Higgs on the web-site, maybe they've already given up on that and left it to the Tevatron…

The descriptions of the theorists include "responsible for some of the wildest theories about the nature of gravity, cosmology and fundamental particles", "a leader in the fields of supersymmetry, extra dimensions and new forces… has become a controversial figure by questioning experimentalists' traditional methods of analyzing the data" and "the most likely to win a Nobel Prize after the LHC data is interpreted." The experimentalist description is rather more modest ("has been involved in detector R&D and construction, software development and physics data analysis"), nothing about any possible Nobel prizes. If you had to choose whether to be a theorist or an experimentalist, the choice looks easy.

Comments

1. **H-I-G-G-S**
   March 12, 2009

   You forgot to mention "is considered the greatest theoretical particle physicist of his generation." How do you say chutzpah in Farsi?

2. **woit**
   March 12, 2009

   H-I-G-G-S,

   I think the percentage of physicists who consider Nima "the greatest theoretical particle physicist of his generation" is at least a couple of orders of magnitude larger than the percentage who think it likely that extra dimensions will be seen at the LHC. I won't speculate on the absolute magnitudes of these two numbers…

Twistor Fever

March 12, 2009
Categories: Uncategorized

It's becoming clear what the hot new topic in particle theory is these days: the use of twistor space methods to try and understand scattering amplitudes in Yang-Mills and gravity theories, especially the maximally supersymmetric versions. This evening on the arXiv there are two closely related papers on the topic: Scattering Amplitudes and BCFW Recursion in Twistor Space by Lionel Mason and David Skinner, and The S-Matrix in Twistor Space, by Arkani-Hamed and collaborators (there's also a third, much more distantly related paper, this one). The Arkani-Hamed et al. paper gives an extensive discussion of motivation for this work in the introduction: the structure of scattering amplitudes for these theories is remarkably simple in twistor space, leading to the question of whether one can formulate the full theory directly in twistor space somehow, giving a different sort of holographic dual than the AdS/CFT one.

A very influential version of this idea goes back to Witten's 2003 paper Perturbative Gauge Theory as a String Theory in Twistor Space, where the dual theory investigated was a topological string theory. This most recent work doesn't appear to use topological string theory, although Arkani-Hamed et al. are rather cagey on the topic of what sort of twistor space theory is at issue. They promise a forthcoming paper entitled "Holography and the S-matrix", with:

> a completely different picture for computing scattering amplitudes at tree level than given by the BCFW formalism, that we strongly suspect is connected with a maximally holographic description of tree amplitudes that makes all the symmetries of the theory manifest but completely obscures space-time locality.

The history of using twistor-space to study gauge theory goes back a very long ways. Penrose started using twistor techniques to study gravity back in the mid-sixties, and after the 1975 discovery of self-dual solutions to the YM equations (instantons), it became apparent that twistor-space techniques could be used to solve them, turning the problem of solving these non-linear equations into a problem in algebraic geometry, that of constructing certain holomorphic vector bundles. Atiyah was among the mathematicians who got interested in this, and his beautiful 1979 lecture notes *Geometry of Yang-Mills Fields* remain a wonderful introduction to the mathematical side of the subject. While still a post-doc, Witten worked in this area, coming up in 1978 with an interpretation of the full YM equations using supersymmetry. Work on this topic was what brought Atiyah and other mathematicians into contact with physicists, including Witten, beginning a quite remarkable period of very successful interaction between the two camps.

Twistors were among the things I started thinking about at the end of my graduate school days in the mid-eighties, for a completely different reason, one that has nothing to do with the current interest in the subject. I was interested in the problem of how to put spinors on a lattice, and the twistor geometry story gives a beautiful way of thinking geometrically about spinors. This idea never really got anywhere, although I did notice some relations between the geometry of the standard model gauge groups and representations that I wrote about back in 1987 and still find quite remarkable. It will be interesting to see what new ideas emerge from this latest wave of interest in thinking about quantum field theory in twistor-space.

Comments

1. **M**
   March 12, 2009

   Twistors are in the air… Earlier today, for no particular reason I dug up the lectures by Cachazo and Svrcek on their work with Witten. Great timing!

2. **Bee**
   March 13, 2009

   You would make a good trendscout. The last months I've heard a 'twistor' here or there and was wondering if that takes off. Well, it seems like it does.

3. **Higher Life**
   March 13, 2009

   All this perturbative field theory fashion is so retro. When will people go back to really cool stuff like the landscape or warped extra dimensions?

4. **Peper Pan**
   March 13, 2009

   You should mention Freddy Cachazo, not just "Arkani-Hamed and collaborators ". The main contribution is due to both Freddy and Nima, plus Freddy is the expert of this specific area. 🙂

5. **Peter Woit**
   March 13, 2009

   Peper,

   You're right, the way I wrote this is not fair, but either writing all the names or picking and choosing who to label things by are both also problematic. Now I see why this field is full of acronyms. Instead of "Arkani-Hamed et al." I'll write "ACCK"….

6. **Artful Codger**
   March 14, 2009

   My experience suggests that when a paper contains exclamation marks which do not refer to the factorial, this is a sure sign that something is being hyped. I seem to recall that this principle worked very well, for instance, in the case of the now completely dead and forgotten "PP wave" craze. This too shall pass.

7. **piscator**
   March 14, 2009

   re:artful codger, i think the presence of NAH on the author list is also often a good sign that something is being hyped ….

8. **Adam Helfer**
   March 14, 2009

   Peter,

   The twistor-Yang-Mills correspondence grew out of a gravitational version by Penrose (the "nonlinear graviton"); it was Richard Ward who worked out the electromagnetic and Yang-Mills analogs — and I think Ward may have been stimulated to take up the Yang-Mills case by discussions with others. At any rate, Ward deserves mention here.

9. **Peter Woit**
   March 14, 2009

   Adam,

   Thanks. I wasn't trying to give a detailed history here or even mention all the crucial people involved (because there were quite a few of them, including several physicists).

10. **twisting the night away**
    March 17, 2009

    I've always thought that the history of twistor methods as applied in physics contains a valuable lesson for string theory, albeit on a smaller scale.

    Twistors started as an attempt to formulate a nature. Not a bit of it, but the whole thing. This didn't quite work in the form conceived, although it obviously had an inherent beauty. This led to ever increasing mathematical sophistication, with only brief touches with physics. Sounds familiar?

    Of course, as recent developments appear to show: twistors remain a very clever idea that now might find it's right place in physics. In my opinion, strings is something similar in nature.

11. **Peter Woit**
    March 17, 2009

    twisting,

    Actually, in some ways the history may be the opposite. Twistors were initially a tool in general relativity, with some hope for using them to quantize gravity. There was little if any interest in the idea of getting particle physics or a TOE out of them. They did have an impact on particle theory, but purely as a technical tool for constructing instanton solutions, which could then be used in semi-classical approximate calculations in Yang-Mills theory.

    In the recent work, they also a appear as a technical tool, better variables for scattering amplitudes. But I still think there's a chance that twistors are much more than a technical tool, but instead a fundamental insight into how geometry and physics are connected. Maybe someone will make some progress on the question of how to reformulate QFT in these variables in an interesting way, and twistors will move from being a technical tool to a fundamental idea behind a TOE.

12. **Adam Helfer**
    March 17, 2009

    Here is a link to an article by Penrose on the origins of twistor theory. You'll see that the motivations were diverse, including both insights from g.r. and hopes of integrating quantum theory.

    Twistor diagrams were an early part of this, growing in part out of spin-networks. The diagrams can be viewed as attempts to define scattering amplitudes for massless fields of pure helicity; associated with this was an attempt to develop a theory of massive particles with such massless constituents. (See e.g. Hughston's *Twistors and Particles*.) That attempt didn't gain much traction, partly because the twistor diagrams were so hard to evaluate that that limited the degree to which the theory could be developed.

    It has been challenging, but not impossible, to adapt twistors to g.r. The most full-bore approach is via CR structures (Penrose again, with notable contributions by LeBrun, Sparling and Mason), which associate to a Cauchy surface (or more generally an acausal hypersurface) a CR twistor space. The mathematics is very interesting, but so far I am not aware of any contributions to physics from this.

    Another approach in g.r. is to define twistors on spacelike two-surfaces of interest as a first step towards defining energy-momentum and angular momentum quasilocally (Penrose again). I've recently built on these ideas to give a treatment of angular momentum at null infinity.

13. **Joe Hucks**
    March 17, 2009

    In case it might be useful, I did about a third of my dissertation on a chiral twistor formulation of the closed bosonic string in 3+1 dimensions:

    http://adsabs.harvard.edu/abs/1994PhDT…….128H

    (In Chapter 3, the application of twistor variables to the study of massless spinless particles is reviewed and discussed, and is generalized to give a chiral twistor formalism with first-order Lagrangian for the closed bosonic string in four space-time dimensions.)

14. **twisting the night away**
    March 18, 2009

    Joe,

    there were even earlier papers by Shaw on applications of twistors in string theory that I know of (sorry can't open your thesis 🙁). Now only generalise these to AdS_5xS_5 🙂 On that note, Berkovits's pure spinors are very close cousins of 10D twistors.

    Peter,

    I would agree that the scale is completely different (at least two order of magnitude in numbers of people involved for one). However, as measured in usefullness in physics there is a parallel I think: if string theory does not connect to the real world, it will devolve into mathematics in a similar way. Just classify the world's living, working twistor theorists today: most of the Penrose group went and worked on other things than physics. Of course, you and I would come down on different sides on whether strings is a good idea in the first place 🙂

15. **Academic Career Links**
    March 20, 2009

    For those interested: there is a fairly recent mini-survey on twistors by Penrose himself.

16. **twisting the night away**
March 23, 2009

Choice quotation from Penrose about recent twistor developments from the article mentioned by Academic Career Links:

"My reaction to these developments, however, is to feel rather like someone on safari in Africa, observing through binoculars a herd of water-buffaloes rampaging around at a safe distance away, when suddenly I realize that they have changed direction and are now heading straight towards me!"

🙂

New Higgs Mass Limits

March 13, 2009
Categories: Experimental HEP News

The new combined CDF/D0 Higgs mass limits are out, there's a paper here. At a confidence level of 95%, a standard model Higgs is excluded for a mass range between 160 and 170 GeV. At a confidence level of 90%, the range excluded is 157-181 GeV. Precision electroweak measurements already constrain the Higgs mass to lie below 185 GeV (at 95% confidence level).

Taken all together, it now looks likely that, if there is a standard model Higgs, its mass is in the region 114-157 GeV. With the data they have analyzed so far, the Tevatron experiments are only able to say that the cross-section for producing a SM Higgs over this mass region cannot be more than 2-3 times the SM value. They still have more data in hand to analyze, and the machine continues to run well. It will likely stay in operation at least a couple more years, possibly doubling the number of collisions already collected. The paper promises:

> The sensitivity of our combined search is expected to grow substantially in the near future with the additional luminosity already recorded at the Tevatron and not yet analyzed, and with additional improvements of our analysis techniques which will be propagated in the current and future analyses.

Now, we just need to hope that they don't find the SM Higgs in this remaining region, which would make things really interesting…

Comments

1. **Shantanu**
   March 13, 2009

   Peter, doesn't this rule out (or severely constrain) MSSM? If I remember right, MSSM has a some upper bound on Higgs boson mass in the region already excluded, but maybe others should clarify.

2. **Peter Woit**
   March 13, 2009

   Shantanu,

   I don't think this says much of anything about the MSSM. There the SM-like Higgs is supposed to be light, you have to tune things to get it above the LEP limit.

3. **Anon**
   March 13, 2009

   Unrelated, but the talks at the IHES conference on Grothendieck are available online at:

   http://video.google.com/videosearch?q=grothendieck&hl=en&emb=0&aq=-1&oq=#q=grothendieck&hl=en&emb=0&aq=-1&oq=&start=10

4. **Peter Woit**
   March 13, 2009

   Thanks!

   A somewhat better link for these might be

   http://www.dailymotion.com/visited/Ihes_science/1

5. **Nameless**
   March 13, 2009

   According to the Fermilab press release, they expect to triple the integrated luminosity by the end of 2010, bringing it up to 10 fb^-1 per experiment.

   If it's not there, CDF/D0 should be able to rule it out at 95% CL before LHC has a chance to say anything definitive. If it's there and it's sufficiently heavy (say, 150 GeV), they might be able to claim evidence of Higgs at 3 sigma. However, 10 fb^-1 would be insufficient to claim discovery at 5 sigma, whatever the mass.

6. **chris**
   March 16, 2009

   "Precision electroweak measurements already constrain the Higgs mass to lie below 185 GeV"

   you forgot a very essential 'standard model' here. in SM extensions, there is no problem obtaining Higgs mass as large as 600GeV

7. **Arun**
   March 17, 2009

   Happy 5th birthday!

8. **diver**
   March 18, 2009

   I remember Alain Connes has a paper in which he also predicts the mass of Higgs. Does anyone know what was his prediction? and whether it is still within the viable range?

9. **Peter Woit**
   March 18, 2009

   diver,

Connes's prediction was for about 170 GeV, which is in the ruled out range. This was even true last summer, and Connes acknowledges this, see

http://noncommutativegeometry.blogspot.com/2008/08/irony.html

Old Enough for Kindergarten

March 17, 2009
Categories: Uncategorized

Today is the fifth anniversary of the start of this blog, something that has caused me to go back and take a look at some of the early postings, and meditate a bit on what has happened during the past five years.

The first posting was content-free, just an experiment to see if the software worked. The inspiration for starting the blog included the examples of Jacques Distler's Musings, which had been around for a while, and Sean Carroll's Preposterous Universe, which he had just started. At the time I had finished writing the book *Not Even Wrong*, and was in the process of getting it published. The initial idea behind the blog was that it would be a place to comment on and share information with others about topics in math and physics that interested me, including following the on-going story of string theory, which plays a crucial role in the intersection of the two subjects.

A few days later, the first substantive posting was a discussion of a talk by David Gross at CUNY on *The Coming Revolutions in Fundamental Physics*. Gross had been giving similar talks for several years (you can see a version from 2001 here), and continues to do so to this day (in a few weeks, he'll be at UC Davis, see here). I don't see anything I'd want to change in my posting from five years ago, and find this in itself somewhat remarkable. One thing that I'm sure has changed in the more recent versions of the talk is that they don't include the prediction that 2007-8 will see a headline in the New York Times about the discovery of supersymmetry at the LHC. One feature of many theorist's talks in recent years has been consistently overly optimistic predictions about when results from the LHC will arrive.

The next posting was an attempt to balance the previous one with something positive and uncontroversial, a discussion of the importance of understanding electroweak symmetry breaking, along with speculation that this might end up having something to do with our still imperfect understanding of chiral gauge symmetry at a non-perturbative level. I found the reaction to this posting truly bizarre, and it gave an inkling of some of the strange chapters to come in what some started to refer to as the "string wars". Over the years I'd heard from some people that quite a few string theory enthusiasts were convinced that the only possible explanation for skepticism was the ignorance of skeptics. String theory is certainly a remarkably complex and difficult subject, and many skeptics will freely admit to not understanding the subject well, but my own personal experience talking to string theorists was that they were well-aware that there were good reasons for skepticism. Over the years, even many experts who had worked on the subject had come to the conclusion that string theory unification was not as promising as initially hoped, and had moved on to work on different things.

A few months later, Harvard's recently promoted faculty member Lubos Motl started up his blog, The Reference Frame, which kicked the pathological nature of the discussion of string theory up to a whole new level. By the way, it seems that the main character of one of the most popular shows on US television is based on Lubos, and there's a campaign to get an Emmy for the actor portraying this character. You really couldn't make stuff like this up.

Five years later, some things have definitely changed. String theory remains a very powerful political force in the theoretical physics community, but the very public debate over the problems of the subject has taken a huge toll. Perhaps the most accurate indicator of how an academic field is doing in the marketplace of ideas is how many universities are investing in tenure-track appointments in the field. At least in the US, the situation here for string theory is dire. I may be missing someone, but taking a look at the latest information about particle theory tenure-track positions in the US available here, I don't see any string theorist even making it to the short lists. At least in the US these days, if you want a permanent position in particle theory, you need to be doing something in phenomenology or cosmology. From what I hear, a common situation in physics departments is that the argument for string theory that "let's wait for the LHC results for vindication" has been taken to heart, with departments figuring that now is not the time to hire in string theory, deciding instead to wait a few years and see if it collapses completely or gets revived by whatever comes out of the LHC.

One sad aspect of all this is that it includes a generalized backlash against the use of sophisticated mathematical ideas in particle theory. Many physicists have drawn the conclusion from the failure of string theory that the problem was too much mathematics, rather than a wrong idea (even string theorists are moving away from mathematics: unlike many years, I see no mathematicians listed as speaking at Strings 2009). Maybe LHC results will point the way forward, but if not, and progress instead requires a deeper mathematical understanding of quantum field theory, the only place for people to get hired working on this will be mathematics, not physics departments, and this is a less than ideal situation for many reasons.

The devolution of string theory unification into pseudo-scientific argumentation about the multiverse is another cause for physics departments to shy away from the subject. This has also been deadly for the public perception of the subject. For this week's example, see a story in the Boston Globe which compares the scientific status of string theory with that of alchemy:

> And at the cutting edge of modern physics, string theory purports to offer a complete but possibly unprovable explanation of the universe based on 11 dimensions and imperceptibly tiny strings.
>
> Alchemists wouldn't recognize the mathematics behind the theory. But in its grandeur, in its claim to total authority, in its unprovability, they would surely recognize its spirit.

Searching the NSF physics awards database for the strings "multiverse" or "anthropic" turns up nothing, and I suspect that even the proponents of this research are well aware that their colleagues want nothing to do with it. For funding they may have to turn to other sources, including the Templeton Foundation, which recently financed a meeting at a resort in the Cayman Islands which brought together people from the world of business and philanthropy with an array of physicists, including the multiverse crowd. A report on the meeting, with some slides of presentations, is available here.

A somewhat related piece of news is that yesterday the Templeton Foundation announced that Bernard d'Espagnat is the latest winner of its $1.4 million Templeton Prize. d'Espagnat has a long career of serious work on the philosophy and interpretation of quantum mechanics, but what makes him eligible for the prize is having indulged in a certain amount of obscurantism concerning quantum mechanics, coupled with an indulgent attitude towards religion:

> Classical physics developed by Isaac Newton believes it can describe the world through laws of nature that it knows or will discover. But quantum physics shows that tiny particles defy this logic and can act in indeterminate ways.
>
> D'Espagnat says this points toward a reality beyond the reach of empirical science. The human intuitions in art, music and spirituality can bring us closer to this ultimate reality, but it is so mysterious we cannot know or even imagine it.
>
> "Mystery is not something negative that has to be eliminated," he said. "On the contrary, it is one of the constitutive elements of being."
> …
> "I believe we ultimately come from a superior entity to which awe and respect is due and which we shouldn't try to approach by trying to conceptualize too much," he said. "It's more a question of feeling."

I'm looking forward to seeing what happens over the next five years. Surely we'll finally start seeing results from the LHC and maybe they'll re-invigorate particle physics. The wide variety of work on mathematics inspired by quantum field theory may also lead to progress of one sort or another. As ever, obscurantism and pseudo-science will find proponents, but I don't think they'll make much headway in the scientific community, even with funding from the wealthy. Undoubtedly things will happen that I can't possibly imagine at this point. I hope that they're positive things for mathematics and physics, or, at least, entertaining.

Comments

1. **Shantanu**
March 17, 2009

Peter what will happen if LHC sees nothing, not even a Higgs?
Will that reinvigorate particle physics? Also you haven't mentioned
anything about neutrino physics in this post or in most other blog posts.
Do you think neutrino physics tells us nothing about particle physics?
Non-0 neutrino mass has been the
most (only) new discovery in particle physics in the last 10 years
and that has invigorated particle physics much more, I think.

Or do you think neutrino physics does not tell us much about electroweak symmetry breaking and other stuff?

2. **Peter Woit**
March 17, 2009

Shantanu,

If the LHC just sees a SM Higgs, that won't reinvigorate particle physics at all. The best-case scenario is no Higgs, which means there is something else behind electroweak symmetry breaking, and the LHC should be able to go searching for this something else, whatever it is.

Non-zero neutrino masses are a clue about what lies beyond the standard model, but unfortunately just a clue, one we don't know how to interpret. Unless there's some unexpected new result from neutrino experiments, I fear this will remain the case.

Maybe the best non-LHC bet for something dramatic would be from the experiments looking for dark matter. But I think these remain a long shot.

3. **Bee**
March 17, 2009

Happy bloggiversary 🙂

4. **Shantanu**
March 17, 2009

Thanks, Peter. BTW is non-zero neutrino mass evidence for physics beyond standard model? I thought so, but Tomasso and others pointed out to
me at that non-0 neutrino mass can be accomodated in the standard model (which was news to me).
Do you agree with Tomasso and others?

5. **Peter Woit**
March 17, 2009

Shantanu,

You can just treat neutrinos the same as other leptons, giving them mass by introducing right-handed neutrino fields and Yukawa couplings to the Higgs. This isn't really a significant extension of the minimal, no neutrino mass standard model, and people often mean this when they say "standard model" now. If you do this, there's no reason for neutrino masses to be so much smaller than other masses, but then again, you have no idea why any of the Yukawa's have the values they do.

Due to the fact that a right-handed neutrino field transforms trivially under SU(3)xSU(2)xU(1), there are other interesting possibilities for how to get a mass term, ones where the smallness of the mass would be natural, and these involve introducing new physics. It's in this sense that non-zero mass is a "clue".

I looked around for a minute and found this nice explanation of the neutrino mass situation:

http://library.lanl.gov/cgi-bin/getfile?25-04.pdf

6. **bryan**
March 17, 2009

congrats on 5 great years!

7. **Simplicio**
March 17, 2009

Alchemy isn't provable? I don't think that's really fair to alchemy. It was provable, and its practitioners ran experiments of a sort, it just happened to eventually be proven wrong.

String theory, on the other hand, is not even….well you get the idea.

Happy bloggoversery.

8. **Roger Schlafly**
March 17, 2009

So what were alchemists proved wrong about? That gold and lead were made of the same fundamental particles? It is true that they never turned lead into gold, but they weren't necessarily wrong to try.

9. **Low Math, Meekly Interacting**
March 17, 2009

Happy fifth!

10. **Aaron Bergman**

March 17, 2009

Neutrino masses can be accommodated in the standard model without the addition of extra fields. The point is that the mass arises from a higher dimension operator and is thus suppressed by the inverse of a cutoff scale at which you expect new physics. Since that scale's a bit less than the Planck scale, there's good reason to expect it isn't the standard model all the way up to quantum gravity.

11. **D R Lunsford**
March 17, 2009

I have thoroughly enjoyed the blog over the years. I hope in five years we can talk about really new things.

I think some of the backlash against the culture of mathematics, as it exists inside physics, is justified. People are not taught to think physically and so cook up all sorts of preposterous ideas. Physics is not blameless for endorsing wishful thinking all too often. I am amazed that it required so many years for a coherent voice about the matter (you) to emerge. Anyway it is good that you had the courage to do all this.

-drl

12. **capitalistimperialistpig**
March 17, 2009

Peter,

Has anyone besides Lubos really suggested that Sheldon Cooper (the string theorist on *The Big Bang Theory*) is based on Lubos? I consider myself pretty well acquainted with both characters and I don't see the resemblance – except that they are both well off the mass shell.

13. **capitalistimperialistpig**
March 17, 2009

But,

Happy Blogoversary!

14. **Tumbledried**
March 18, 2009

happy blogiversary Peter!

15. **Kasper Olsen**
March 18, 2009

Happy 5, Peter!

16. **says:**
March 18, 2009

So this blog is Old Enough for Kindergarten.

What is string theory is Old Enough for?

17. **Engineer**
March 18, 2009

Congratulations on 5 years. It is rare that a professional can keep a popular public blog without it (sadly) undermining their career. Regarding string theory, my opinion is that it does show that there is a way to unify quantum mechanics and general relativity, however, at first glance the whole idea of little strings being the answer is childish in its simplicity. It was probably a mistake for physicists to call it string theory.

I would argue that outside your dislike of string theory, you do share the same sort of contempt for the misuse of physics that your infamous rival Lubos has. I hate to say this, but there is a small circle of blogs that offer some of the most intellectually stimulating entertainment that one could ask for, and Not Even Wrong is a well established member.

However, I will also happily say this, my own education stopped prematurely due to my own career and obligations, and if it weren't for Not Even Wrong, The Reference Frame, Back Reaction and Cosmic Variance, and a few other blogs, I doubt my recent progress in self-education in physics would have been remotely successful or even possible.

Looking forward to your 10th anniversary!

18. **Peter Woit**
March 18, 2009

Thanks CIP,

I've only watched the show a few times, and the first episode I saw had Sheldon going on about the superiority of string theory to LQG, and how stupid LQG supporters were. At the time, my first thought was "Wow, somebody at this show has been reading The Reference Frame…"

19. **theoreticalminimum**
March 18, 2009

Thanks for sharing so much during those past 5 years Peter. Through your blog, you have made it possible for people like me to have a better appreciation of contemporary and historical ideas in physics and mathematics research. We wish you all the mental stamina to keep it going for a few more years.

20. **Steve Esser**
March 18, 2009

I'm not informed enough to comment often, so this seems like a good opportunity to say thanks for an interesting and informative blog (I enjoyed your book as well).

Best regards,
– Steve Esser

21. **John**
March 18, 2009

Ther are some string theory-educated people on the faculty short-list : Harold Steinecker, David Shih, Sebastian Franco

22. **mathematician**
March 18, 2009

Peter Woit, thanks for the blog. It's one of the better ones on the Internet.

I miss Lubos. I used to run into him at his old job and listened in on some of his QFT lectures. It's too bad he left the USA (and possibly academia). The wit and humor overbalance the arrogant craziness, I.M.O.

re: alchemy, it's just chemistry done in secret. Alchemy would have become modern chemistry and physics had there been open publication.

23. **Johan**
March 18, 2009

Thanks for this excellent blog. I am a mathematician with a casual interest in physics.

I am glad to have your critical perspective on string theory as a contrast to the frequently over-enthuiastic treatment in the media.

24. **Jacob DeGoede**
March 18, 2009

Hi Peter,
Thank you for your blog wich is a source of inspiration to me.

Jacob DeGoede

25. **a quantum diaries surivivor**
March 18, 2009

I cannot but join the cheering crowd for these brilliant, exhilarating, instructive, constructive five years. Keep it going strong, Peter!

Cheers,
T.

26. **AM**
March 18, 2009

I came across your blog quite by accident a few years back and have been a visitor on and off ever since. Cheers Peter on a very nice blog.

27. **Dmitry**
March 19, 2009

Dear Peter,

Being a blogger-beginner, I do appreciate how hard it is to keep coming up with a new stuff for months (many persons report writer's blog after a couple of weeks 🙂). You kept coming up with new stuff for years – you ARE a marathon runner.

Cheers and congratulations,
Dmitry.

28. **Shantanu**
March 19, 2009

Peter, thanks for the link on neutrino mass. Btw what do you (and others)think
about strong CP problem in QCD. I haven't seen much posts on this.
Or is this a solved problem? Would you classify this as a holy grail of physics?
Is the axion the only accepted solution? do you think there is any connection between strong CP problem and problem of electroweak symmetry breaking?

29. **Peter Woit**
March 19, 2009

Many thanks to all for the nice birthday greetings…

Shantanu,

If I had to pick a holy grail, it's the mechanism of electroweak symmetry breaking, including understanding where fermion masses come from. The strong CP problem is more of a hint than a holy grail, like the small size of neutrino masses. Maybe it's a hint about axions, but adding new fields just to solve this particular problem isn't very convincing. It's also possible that somehow, once we understand the origin of fermion masses, the problem will go away. If the bare mass of the lightest quark is zero, there is no strong CP problem. This doesn't seem to be true, but maybe it's also a hint…

30. **Daniel**
March 19, 2009

5 years of anti-scientific ranting? Enough already. Try finding a job you can be productive in.

31. **Academic Career Links**
March 19, 2009

Thanks for all the interesting posts & greetings!

32. **a**
March 20, 2009

hi Peter, I remember that 10 years ago the situation in theoretical physics was discouraging: we had a big problem, but nobody wanted to openly talk about it, keeping working on the usual topics. It is impressive to see how much the situation improved now.

Probably your blog played a key role, and without this new powerful tool that you managed in a great way, all criticisms would have been ignored.

33. **Syksy Rasanen**
March 20, 2009

Happy birthday, Not Even Wrong! Thanks for your informative and useful blog. (You know, in some countries, children are already at school at the age of five.)

34. **Joey Ramone**
March 20, 2009

Happy Birthday Peter's Blog!

Yes–I remember when we tried to get $1.4 million for our art, but the foundations supporting science, physics, and philosophy turned us down. Good to see they are now supporting, "art, music and spirituality can bring us closer to this ultimate reality, but it is so mysterious we cannot know or even imagine it."

Yes–it is so mysterious we cannot know nor imgaine it, which is why I gave up on physics and wrote "I wanna be sedated."
http://www.youtube.com/watch?v=wMD7Ezp3gWc

We're applying to Templeton to get a second camera for our next video, a mobile tripod, and more postdocs than shown in this shoot.

35. **Marcus**
March 20, 2009

"hi Peter, I remember that 10 years ago the situation in theoretical physics was discouraging: we had a big problem, but nobody wanted to openly talk about it, keeping working on the usual topics. It is impressive to see how much the situation improved now.

Probably your blog played a key role, and without this new powerful tool that you managed in a great way, all criticisms would have been ignored."

Could you be specific? What are some objective indications that the situation has improved?

Can you name any prominent theoretical physicists whose research direction has changed for the better, by your standards? Or any other concrete signs of improvement?

I don't doubt you are right, a. By my criteria there has been an big improvement. What I want is to understand your criteria.

36. **Russ**
March 20, 2009

I hope I'm not too late to wish your blog a happy birthday! I'm a relatively new reader of your blog and an amateur maths-physics follower. I didn't realize the stakes against string theory until I started reading 'Not Even Wrong' . I really do hope mathematicians and physicists make some progress in TOEs and all before I'm too old to care 😉 ! Meanwhile, I will keep reading, wondering, and learning from your blog. Thanks!

Russ Van Rooy

37. **Joey Ramone**
March 20, 2009

Hello Marcus,

One of the invaluable services Peter has performed has been to provide a moderate, well-reasoned, honest voice. It is not easy sifting through the volumes of hype of our era and then summarizing it all in concise words, while enduring anonymous attacks from well-funded apponents and their hired fanboys. We had the same problem with Dexie's Midnight Runners and Bon Jovi in the early eighties but we ended up in the Rock'n'Roll Hall of Fame. Just as the univeristy presses were told not to publish Peter's book, the major labels were told to steer clear of our rock'n'roll. But truly, it is the music that makes the label, not the label that makes the music. And by rejecting Peter's book while embracing strings/convulted/obfuscatory math, the university presses took themselves down a notch, just like AIG/Merrill/Fannae Mae. This is sad, because math is a great and powerful entity; and now people are losing trust in it, just as people are losing trust in music because of American Idol and the Jonas Brothers.

The good news is that Peter is spearheading a renaissance in the university, where cleared of the string theory (antitheory) administrators, the schools will again be able to focus on more exalted maths and further real physics:
http://www.youtube.com/watch?v=BL4od3xzthM

Towards the end you can see the LHC creating black holes in a brillaint explosion where it neither proves nor disproves string theory, which is beside the point, as far before that we see proof that Phenomenologists (in black leather jackets) Sing and Dance Better Than String Theorists.
http://blogs.discovermagazine.com/cosmicvariance/2005/07/21/do-phenomenologists-sing-and-dance-%20%20%20%20better-than-string-theorists/

Over the past five years Peter's blog has shown that science can also be pursued in the spirit of truth-seeking, in addition to driving traffic to websites to sell ad space. Rock-rock-rock-rock-rock-and-roll-high-school!

38. **Thomas Dent**
March 20, 2009

> "One feature of many theorist's talks in recent years has been consistently overly optimistic predictions about when results from the LHC will arrive."

In what universe could this be a meaningful or fair criticism? Is Gross is at fault for failing to anticipate – by some type of clairvoyance, I suppose – that the LHC schedule would slip by several years? Back in 2001, I seem to remember the plan was that operation would start in 2005/6 and data start flowing in 2006/7. Why would one not take this at face value?

Just what attitude should Gross and other theorists have taken to avoid these cheap shots with five years' hindsight? 'LHC will probably be late and break down, so there's no point trying to plan how we're going to deal with the data'?

Someone who never goes on the record about what the state of the field might be in 5 years' time will never be wrong – but such a person also deserves no voice in allocating scientific resources. Many big experiments have to be scientifically motivated and planned 15 years in advance. In context, the quoted remark is more than myopic.

And who could ever have predicted that mathematical physics would suffer from a prolonged publicity campaign against a field where large numbers of the most talented mathematical physicists work?

39. **Peter Woit**
March 20, 2009

Thomas,

In my original 2004 posting, I pointed out that Gross's prediction of a 2007 announcement of discovery of supersymmetry was unrealistic, given the schedule in place then, even ignoring the fact that schedules on projects of this kind almost always slip. The criticism of his prediction as unrealistic was a rather minor one, but I continue to think it was valid. More generally, I think particle theory as a whole has suffered for a long time from too much wishful thinking about how the LHC is any day now going to solve all problems.

The current bad reputation of string theory comes not from my blogging, but from the combination of the way it was (and continues to be) overhyped, together with its complete failure as an idea about unification, and the unwillingess of its proponents to admit publicly what has happened. Sure, it was possible to predict that this would cause serious damage to the willingness of other physicists and society in general to support particle theory, especially in its more abstract forms. The hope that this could be stopped before it did a lot of damage was one motivation for why, back in 2001, I started pointing out the problem.

40. **Chris Oakley**
March 20, 2009

Hi Joey,

We have had all sorts of people contributing to this blog: not just high-energy physicists, but engineers, philosophers, science writers, and all the possible shades of loons. Despite this, they have all had one thing in common: they have all been alive. With your posting, though, the blog can claim something unique – a contribution from a dead person. I look forward to more insightful comments from you, and hope that you can persuade some of your fellow dead people to contribute also. Personally, it would be the insights of Paul Dirac and Ernst Stueckelberg that I would most value. So if you could have a word …

41. **Simon Cowell**
March 20, 2009

Joey Ramone, that was indulgent nonsense.

42. **Joey Ramone**
March 20, 2009

Thanks Simon. Congrats on American Idol and all the educational shows it has helped inspire:
http://www.math.columbia.edu/~woit/wordpress/?p=1520
http://www.math.columbia.edu/~woit/wordpress/?p=1237

Chris–it is a little known fact that Dirac was our ;lead science advisor on this video for rock 'n' roll high school:
http://www.youtube.com/watch?v=EiDvKqRjYn0&feature=related

Not content to merely have the leading science/hollywood experts of our day and age advise our artistic creation, we hopped on Michio Kaku's time machine and went back in time to ask Dirac questions. You can see the results about five seconds before the end.

We knew we could do better than those who brought you Dr. Manhattan in The Watchmen–who owes his blue glow to quantum black hole creation in an LHC experiment designed to probe higher dimensions, test string theory, and prove the existence of parallel universes.

It has been a lot of fun hanging out with Einstein, Feynman, and Bohr. None of us can stop laughing. Please do keep it up everyone! Never in the history of mankind has such supreme satire been conducted for so long with such straight faces. Ha!

43. **Revlin**
March 20, 2009

I'm happy to have discovered your blog a few weeks back and I hope it continues for at least another 5 years. I know you're one of those complex math types so the content is reflective of that, but I'm more turned on by the introspective articles like this one. I like that you brought up the trend of private groups funding "far out" research, because I think this is an area where academia has really fallen behind. Organization like Templeton and the Singularity university are going to leave university sponsored research in the dust, not just in the ideas that they validate, but in the way those ideas are actually applied to the broader society.

I have some gripes about your characterizations:
"d'Espagnat has a long career of serious work on the philosophy and interpretation of quantum mechanics, but what makes him eligible for the prize is having indulged in a certain amount of obscurantism concerning quantum mechanics, coupled with an indulgent attitude towards religion"

First off, the quote you provided mentions nothing specifically concerning religion, but does however address "art, music and spirituality." I don't see where you're coming from when you label these memes "obscurantism" considering their prominence and scope in the course of human development. I'll realize this is a somewhat specifically targeted blog, but surely you are aware that the topics you cover here (bosons, fermions, twistors, etc.) are exponentially more obscure to most people on this planet than music and art. And why is that?

Is there a connection between the waning interest in academic research, the growing enthusiasm for private research, and the widening gap between "mainstream", scientific theory and the various ontological theories that the public has locked on to? Why is occultism growing in variety, scope and sheer numbers of believers, concurrent with "real" scientists' efforts to explain everything through empirical means? In my mind it's a failure of the educational system to bridge the divide between scientific data about reality and public perception of reality. Can science explain my experience of the world?

I think the groups that attempt to answer these questions will become increasingly relevant to the common person as the years roll on, regardless of the legitimacy of their answers. If scientists cannot explain the implications of their research to the life of the individual, someone else will, and they will be believed, crack-pot of not.

44. **Revlin**

March 20, 2009

"…crack-pot or not."

Also, I wish there was some way to edit a submitted comment. Thanks for your reference to the Cayman Islands conference. The site you linked to has some really interesting blog topics:

http://www.fqxi.org/community/forum/topic/427

http://www.fqxi.org/community/forum/topic/429 (check out the 'Quantum Darwinism' link)

http://fqxi.org/community/forum/topic/414

http://www.fqxi.org/community/forum/topic/401

45. **Cormac O Raifeartaigh**
March 20, 2009

Happy birthday Peter, well done and long may you keep it up. I myself have already started to repeat myself, two weeks shy of my 1st birthday!

Re "D'Espagnat says this points toward a reality beyond the reach of empirical science", I'm surprised. This looks like a fairly clear misunderstanding of HUP – as you know, it is not an uncertainty in our knowledge of position/momentum, but an inbuilt fuzziness in the quantities themselves…some philosopher!

46. **Eugene Stefanovich**
March 20, 2009

Peter,

almost five years ago I asked myself: "Is there a single person in the world who is independent and courageous enough to stand up against the string theory hype?" I started to Google word combinations like "string theory", "wrong", etc. The first thing I found was your blog. I have been reading it non-stop since then. Congratulations!

47. **stj**
March 21, 2009

Hi Peter,

happy birthday for this very informative blog.

Concerning d'Espagnat being awarded by the Templeton prize, it is of no surprise since, you might not know, he is member of "Université Interdisciplinaire de Paris" (UIP) [1]. On their website, the presentation of UIP is as follows: "The mission of the Interdisciplinary University of Paris (UIP) is to disseminate and bring into contact different visions of the world based on the study of contemporary scientific paradigms, mainly in the area of astrophysics, quantum physics, the theory of evolution, the neuro sciences and the philosophies of the spirit." In fact, UIP is the French equivalent of the Templeton Foundation (and they have partnerships). The veiled-reality concept from d'Espagnat was largely used by UIP members (including d'Espagnat but especially Jean Staune, the perpetual General Secretary of IUP) to advocate reconciliation between science and religion [3]. See also the article in Science [4].

[1] http://www.uip.edu
[2] See eg http://www.templeton.org/templeton_report/20080723/
[3] See eg http://www.uip.edu/gpss_major/spip.php?article73
[B. d'Espagnat, "The concept of 'Veiled Reality' and its relevance to the dialogue between Science and Religion"]
[4] http://sciencenow.sciencemag.org/cgi/content/full/2009/316/1?rss=1

48. **Peter Woit**
March 21, 2009

Revlin,

I don't think obscurantism and pseudo-science are an increasing problem among the general public. There have always been many sources of this, and they don't seem to be doing any better than in the past. The coverage of science in the media and the understanding and respect the public has for science seem to me as healthy as ever. The problem I do see is an increasingly large number of respectable scientists engaging in this kind of thing, and that's very dangerous. If you don't have real progress to promote, it's tempting to start hyping things which are not scientific progress, and too many people have been doing that.

49. **Revlin**
March 21, 2009

"the understanding and respect the public has for science seem to me as healthy as ever. "

Ah, but what is "science" in the public mind? What you call "obscurantism and pseudo-science" is what many others commonly infer as being based on "science." In a certain loose way it is based on science, but would you say it *is* science or in any way *validated* by science?
You are voicing my exact point when you say, "The problem I do see is an increasingly large number of respectable scientists engaging in this kind of thing, and that's very dangerous." This trend is going to continue in the upward direction and the division between what these guys profess and what you would call "real" science will become more and more blurry. Remember your entry on *What the Bleep*? Remember your frustration at having been in a minority group of critics of the scientific legitimacy of this films statements (relative to the millions who said, "Hmm, that was really interesting?"–how many people recommended the film to you?) That is evidence of the wide gulf between your circle of colleagues and *everyone else*.
I also don't see how you can say the general public is well informed about anything, especially through observation of states of media coverage, which are so disparate from real public perception. Try going to the mall and asking a random sampling of people what they think about the federal reserve central banking system. The breadth and consistency of ignorance about most worldly info is unbelievable, even when that info is overtly related to everyone's day-to-day existence.

50. **Peter Woit**
March 21, 2009

Revlin,

Maybe I'm an incurable optimist, but my take on "the general public" seems to be different than that of a lot of my Ivy League classmates. What I've seen over the years of talking to people is that, sure, they're often ill-informed about science, economics, whatever (although they'll often surprise you with how much they know). At the same time, they often are well-aware that they don't know much about these subjects, have a lot of respect for them, as well as common sense about them.

But, in the end, I'm in some ways an elitist. I just don't think it's very important if the general public is well-informed about, say, elementary particle physics. It's important that they have a general idea what the subject is about, and an openness to the idea that it's worth studying, even if they don't want to do so themselves. The serious problem worth worrying about is the quality of the thinking on a subject by its experts. If experts in particle theory are devoting their time to studying Boltzmann Brains, that's a big problem, far worse than whatever misconceptions the general public may have.

Similarly, the general public may not understand how the Federal Reserve works, but it's not clear that that's important. What is important is what happens with the people who do understand how it works, e.g. those who run it. We're in the middle of a world-historical disaster caused by the experts who run large financial institutions, including those most expert at the technicalities of how they work. That the average person does not understand these technicalities is both not surprising and mostly irrelevant.

51. **Revlin**
March 21, 2009

"We're in the middle of a world-historical disaster caused by the experts who run large financial institutions, including those most expert at the technicalities of how they work."

Experts who made, are making, and (if the trend continues) will continue to make a lot of money. The disaster is not a result of the experts failing in their understanding and capability to manipulate market systems. Perversely, the failure is a result of their *success* in diverting as much capital from the general public (including their own share-holders) to their pockets as possible. They are so good at this that they're *still doing it*, and being highly compensated for doing it well. So, what's the problem? The Big Picture.
"Experts" who have no interest and no incentive to consider the Big Picture will act with no heed or care for the needs of others. I see this happening in engineering consistently. If global change is the supposed big issue of our times, why is so much money being poured into developing tech for telecoms, popular entertainment, and non-renewable resource harvesting and so little going towards alternative resource management? Lack of incentive. Lack of public insight. Why is *Watchmen* backed by a $120 million budget while public universities across the country are struggling to keep their doors open?
The public gives a damn about inane, big-budget action movies. They do not give a damn about particle physics. You don't think this is a problem?

52. **D R Lunsford**
March 22, 2009

More should be written on the relation of the failure of fundamental science to that of financial practice, which I am calling "the hyperverdant castastrophe" because so many ex-physicists and mathematicians ("quants") were involved. Of main interest is the role of the appropriate press in both cases, both internally (journals and whitepapers) and externally (OMNI and the Wall St. Journal).

-drl

Collider Smackdowns

March 21, 2009
Categories: Uncategorized

If you're interested in particle physics and not regularly reading Tommaso Dorigo's blog, you should be. His latest posting reports on incendiary claims by Michael Dittmar of the CMS collaboration that recent Tevatron Higgs mass limits are wrong and not to be believed. According to Dittmar, the Tevatron is basically useless for looking for a SM Higgs, with only the future LHC experiments ever having a chance to see anything or produce real limits. You can look at the slides and the blog posting and make up your own mind. From what I can tell, Dittmar doesn't make a strong enough case to show that the Tevatron results are wrong. It remains true of course that the statistical significance of the limits being set ("95% confidence level"), is right at the edge of what is normally taken as capable of seriously ruling something out.

In the latest New York Review of Books, Freeman Dyson, in context of a review of Frank Wilczek's The Lightness of Being, engages in his own smackdown of particle physics at colliders. Here's what Dyson has to say about the LHC, and colliders in general:

> Wilczek's expectation, that the advent of the LHC will bring a Golden Age of particle physics, is widely shared among physicists and widely propagated in the press and television. The public is led to believe that the LHC is the only road to glory. This belief is dangerous because it promises too much. If it should happen that the LHC fails, the public may decide that particle physics is no longer worth supporting. The public needs to hear some bad news and some good news. The bad news is that the LHC may fail. The good news is that if the LHC fails, there are other ways to explore the world of particles and arrive at a Golden Age. The failure of the LHC would be a serious setback, but it would not be the end of particle physics.
>
> There are two reasons to be skeptical about the importance of the LHC, one technical and one historical. The technical weakness of the LHC arises from the nature of the collisions that it studies. These are collisions of protons with protons, and they have the unfortunate habit of being messy. Two protons colliding at the energy of the LHC behave rather like two sandbags, splitting open and strewing sand in all directions. A typical proton–proton collision in the LHC will produce a large spray of secondary particles, and the collisions are occurring at a rate of millions per second. The machine must automatically discard the vast majority of the collisions, so that the small minority that might be scientifically important can be precisely recorded and analyzed. The criteria for discarding events must be written into the software program that controls the handling of information. The software program tells the detectors which collisions to ignore. There is a serious danger that the LHC can discover only things that the programmers of the software expected. The most important discoveries may be things that nobody expected. The most important discoveries may be missed.

He goes on to somehow count Nobel prizes for experimental results in particle physics, with the conclusion:

> The results of my survey are then as follows: four discoveries on the energy frontier, four on the rarity frontier, eight on the accuracy frontier. Only a quarter of the discoveries were made on the energy frontier, while half of them were made on the accuracy frontier. For making important discoveries, high accuracy was more useful than high energy. The historical record contradicts the prevailing view that the LHC is the indispensable tool for new discoveries because it has the highest energy.

His argument that proton collider physics is problematic because of the huge backgrounds and difficulty of designing triggers just states the reasons why these are complicated and difficult experiments. Despite the difficulties, they have produced a huge number of new physics results. He doesn't give the details of how he is counting and categorizing Nobel Prize winning results, so that part of his argument is hard to evaluate.

In opposition to colliders, Dyson wants to make the case for passive detectors, with his main example Raymond Davis's discovery that the neutrino flux from the sun is 1/3 what it should be. I don't really see though why he sets up such experiments in opposition to high energy accelerator experiments. Right now many of them actually are accelerator experiments (for example MiniBoone), with an accelerator being used to produce a beam of neutrinos sent to the passive detector. Dyson's point that if one is very smart and lucky one may get indirect evidence about physics at high energy scales from passive detectors looking at cosmic rays is valid enough, but there is no shortage of people trying to do this, and it is every bit as problematic as working with colliders. There are inherent reasons that such experiments can't directly investigate the highest energies or shortest distance scales the way a collider experiment can. It's extremely hard to come up with a plausible scenario in which cosmic ray experiments will give you any information about the big remaining mystery of particle physics, electroweak symmetry breaking.

While I agree with Dyson that the huge sales job to the public about a new golden age of physics coming out of the LHC is a mistake. I don't see any reason to believe that if it fails cosmic ray experiments are going to get us to a golden age. If and when particle physics reaches a final energy frontier, with higher energies forever inaccessible to direct experiment, hopes for a golden age are going to rest on theory, not experiment, and recent experience with such hopes isn't very promising.

Update: This Sunday the New York Times will have a profile of Dyson, see here.

Comments

1. **TSM**
   March 21, 2009

   Very naive newbee question by why did the LHC designers decide to go with a proton/proton system versus proton/anti-proton? Wasn't the SSC suppose to be proton/anti-proton?

2. **TSM**
   March 21, 2009

   For got my last part: are anti-proton/proton experiments less noisy with regards to background?

3. **Shantanu**
   March 21, 2009

   Peter, proton decay is one example of a non-accelerator particle physics
   experiment which probes physics at very high energies. Or you are no longer sanguine about proton decay being discovered?
   Also any comments on Raphael Bousso's colloquium at PI?

4. **Peter Woit**
   March 21, 2009

   TSM,

   I believe the SSC was also a proton-proton machine. The problem with antiprotons is that it's hard to accumulate them and get an intense enough beam, limiting the luminosity one can achieve. As one goes to higher energies, the cross-sections one is interested in fall off, and you need higher luminosity. Maybe someone more expert than I can address the issue of relative backgrounds.

Shantanu,

I think the most likely end result of proton decay experiments will just be that protons don't decay at any rate one can ever hope to observe. If so, these experiments won't tell you anything about high energies, other than that baryon number stays conserved. If proton decay is observed, that would be extremely interesting, and would give the sort of thing Dyson is hoping for.

As far as I can tell, Bousso's talk is just more of the same. I don't see any hope for getting real science out of that.

5. **Bee**
March 22, 2009

Sounds like what he is actually saying is: hey folks, even if the LHC fails, be prepared to spend more billions on a lepton collider.

6. **anon**
March 22, 2009

Hi Peter

You said:
"It remains true of course that the statistical significance of the limits being set ("95% confidence level"), is right at the edge of what is normally taken as capable of seriously ruling something out."

As far as I was aware it is only in the social or medical sciences that 95% or two sigma is considered even worth mentioning, in astronomy its usually 3 sigma and in particle physics five sigma. Many in astronomy think it should be five sigma as well. I think two sigma should never be taken as more than the slightest of a hint that something could be ruled out.

7. **AOJ**
March 22, 2009

In comparing the potential of cosmic ray experiments to collider experiments, you should remember cost scale. Auger, the biggest and most ambitious cosmic ray experiment had a price tag between $100 and $200 million. If you threw LHC-sized funding at a building a single, amazing cosmic ray detector, you would get immediate breakthroughs in multiple areas of astrophysics and quite likely particle physics as well. The center-of-momentum energy for collisions of the highest energy cosmic rays is ~100 TeV. We certainly won't be probing that scale in collider experiments any time in the foreseeable future.

8. **Peter Woit**
March 22, 2009

AOJ,

Yes, but the luminosity of cosmic ray experiments is way too small at TeV and above center of mass energy scales to be useful, and building a 50xAuger that cost as much as the LHC wouldn't change that. You would learn more about astrophysics, but I don't believe you'd learn anything about TeV-scale particle physics phenomena like electroweak symmetry breaking. For a 120 Gev Higgs, how big a cosmic ray experiment would you have to build to see evidence of it?

9. **David B.**
March 22, 2009

TSM

Antiprotons are expensive to produce and maintain. At very high energies the collisions are dominated by gluons rather than quarks, so most of the events will be from glue collisions that do not distinguish protons from antiprotons.

For some types of searches, it is better to have a proton anti-proton setup, if the physics depends heavily on having quark-antiquark collisions at high energies.

If you are looking for colored particles or the Standard model Higgs, proton-proton will do just as good a job as proton anti-proton.

10. **Chris Oakley**
March 22, 2009

I am not quite sure what Dyson is getting at vis a vis choice of particles as at TeVs one tends to produce a godawful mess regardless of whether the beams are p/p, e+/e- or whatever.

11. **The Real Deal**
March 22, 2009

I don't know if Dyson is smart or dumb.

First the dumb part:
Sitting on his high armchair in comfort, he criticizes the LHC in hindsight, even before it's operational. The LHC costs some $4B to build, another $1B to run experiments, a dozen years to design and built, by thousands of physicists and engineers. The most sophisticated of intellectual efforts contributed by people who actually sweat it out instead of setting pretty in armchairs. Vast amount of public money, reputation, national dreams poured into it. It had better work. It had better produce. No further hyping necessary. The scale of the effort is 'hype' enough. Dyson should simply shut up and stop looking dumb.

Now the smart part:
If Dyson is so smart educating us all the reasons why the LHC should not be built, why don't he propose his superior design to replace it? Try getting $10B to build it. And let other do the criticizing in their armchairs.

12. **Observer**
March 22, 2009

…It had better work. It had better produce…
But what if not. We all have seen the main problems in the first run of the LHC are due to basically Quenching and heating, both of this problems relatively "basic" despite

all the intelectual effort behind them.

13. **Zathras**
March 23, 2009

The Real Deal,

Dyson has been criticizing p-p colliders for about 20 years now. I myself went to a talk of his in 1991 where he criticized the SSC for being as such. He is not coming late to this, and he has proposed alternative designs (a lepton collider). He just hasn't been able to convince enough scientists of what he perceives as the superiority of lepton colliders.

14. **milkshake**
March 23, 2009

Dyson is mildly non-conformist – he also likes to write against the global warming agenda orthodoxy. I bet he finds the neatly- designed passive detector experiments far more marvelous than accelerator physics.

15. **a quantum diaries surivivor**
March 23, 2009

Hi Peter,

I feel obliged!

I would like to stress, to answer anon's comment 6 above, that HEP physicists do not use 95% CL limits as a claim of anything. However, I agree that unfortunately the importance of such results gets unduly overestimated. So much so that the occasional Dittmar can feel compelled to refute them and waste his reputation in meaningless talks.

Cheers,
T.

16. **John K**
March 23, 2009

TSM
re : PBar (Antiproton) -P vs, PP

Advantages of PP (vs. PBarP)
–much easier to get intense P beam than PBar beam, so can get to much higher luminosity (interaction rate) –perhaps a factor of
hundreds or even thousands
–does not need the Pbar production system (which, in the case of the Fermilab PbarP collider, involves a very complicated system of production target, collector (lithium lens), cooling ring, accumulator, etc.
–LHC at 7+7 TeV will have a much higher production crosssection of new physics, especially above 200 GeV–and can reach masses not available in the 1+1 TeV Tevatron

Disadvantage of PP (vs. PBarP)
–PBarP need only one ring of magnets (PBar and P goes in opposite direction in the same ring)
–in low masses (such as 100-200 GeV, where the Higgs is expected to be), Signal to background is somewhat better
than PP

17. **Chris Oakley**
March 24, 2009

John K,

Another advantage of the p-p_bar system is surely that the resonances are likely to be more interesting, having zero charge and baryon number. Did they not use that (in an e+e- system) to investigate the J/Ψ? Would this still be relevant at much higher energies?

18. **Coin**
March 24, 2009

The machine must automatically discard the vast majority of the collisions, so that the small minority that might be scientifically important can be precisely recorded and analyzed. The criteria for discarding events must be written into the software program that controls the handling of information. The software program tells the detectors which collisions to ignore. There is a serious danger that the LHC can discover only things that the programmers of the software expected… The most important discoveries may be missed.

I don't think this is a very compelling argument. As Peter points out this is certainly a challenge for the LHC operators, but it's hardly a reason to cast aspersions on the LHC's results.

First off, the dangers of selectively discarding data sound entirely possible to mitigate. Even knowing very little about particle accelerators but knowing something about software I can think of basic ways to test for such a problem, like running tests with different triggers. Meanwhile [I actually got a chance to ask one of the USLHC bloggers about this exact problem once] and part of her response was:

> We can study these efficiency and biases using a very detailed Monte Carlo simulation of the trigger and detector. And we validate this simulation with the actual data using well-measured physics channels such as W, Z decays and QCD processes. As most SUSY models predict very energetic and multiple jets of particles, the trigger is expected to be very efficient and non-biased. But these are statements that we have to confirm before we can hope to publish.

I take this last sentence to mean that part of the published data for the LHC will be their evidence that their trigger scheme is not biasing or eliminating signal from the data. If Freeman Dyson thinks that evidence is inadequate, he can write a paper presenting his argument and it will be a big deal and everyone will read it because he is Freeman Dyson.

Second off, it kind of seems like this would be one of the easiest things about the LHC to upgrade. As far as I know the only reason for the automatic discarding of data is that storing and processing all that data is impractical. But what is practical in data storage changes all the time. If this is shown to be legitimately a problem it seems like it would be much easier to just slap in some fancy new hard drives than it would be to, say, increase the LHC's luminosity.

19. **Coin**
March 24, 2009

Sounds like what he is actually saying is: hey folks, even if the LHC fails, be prepared to spend more billions on a lepton collider.

Are there currently any plans to start building any lepton colliders at any point?

20. **Peter Woit**
March 24, 2009

Coin,

I don't disagree with Dyson that the necessity of using triggers is a serious problem for colliders, but there really isn't any choice, and it's up to the experimenters to do the best they can within the constraints imposed by the necessity of the triggers.

The nature of the problem is not that you won't be able to trust the results coming out of these experiments. If they look for an sparticle with certain properties, the trigger will be designed with this in mind, and the limits they quote or evidence they find should be trustworthy. The problem is with new physics that is not expected, that it might get missed because no one is looking for it. Dyson's point is that in something like a proton-decay experiment, you look at every event and make sure you understand it. New physics will be quickly identified, no matter what it is. In a collider experiment, one can imagine that there might be new physics that no one has thought of, with an experimental signature that gets thrown out by the triggers. I don't have much of a feel myself for what the likelihood of this might be, but I imagine it's a worry that keeps some of the experimentalists up nights when they design triggers.

There are two major on-going projects to design a lepton collider (ILC and CLIC). The current situation though is that no decision to build such a thing is likely until several years from now, after results from the LHC are in. Knowing what if any new physics is in the accessible energy range is crucial for the design of such a machine.

21. **Coin**
March 24, 2009

Ah, I hadn't realized the ILC was a lepton collider.

22. **srp**
March 24, 2009

Dyson has written eloquently in the past about what he sees as the "ecological" error of putting too high a percentage of a field's resources into a single project. That's one reason why he opposed the SSC, criticized the USSR's giant optical telescope, etc. Read his brilliant essay in From Eros to Gaia on the subject (I can't remember the exact essay title–it had something to with first, second, and third worlds).

23. **Thomas D**
March 30, 2009

Perhaps Dyson hasn't noticed that many theorists have been busy trying to think of classes of theories that 'standard' triggers and cuts miss, and have been working with the experimentalists to ensure that as little as possible gets away.

Also that experimentalists have been developing 'model-independent' search techniques that look for excesses over background regardless of whether they fit a favourite model or not.

I wouldn't expect anything different from someone who has been retired from research in particle physics for a long time. Dyson is extremely old and clever and respected but(/and) has no reason to keep up with recent developments.

The idea of trying to use cosmic rays to do electroweak scale particle physics is utterly impractical, because you would need literally square miles, if not cubic miles, of tracking detectors high in the atmosphere to see any interesting interaction. Auger is great by any cosmic ray detector standards but it just measures the overall parameters of a huge cloud of hadrons and photons.

I understand that p-pbar collisions were disfavoured for LHC as they would produce significantly more hard q-qbar events which from the point of view of new physics are almost entirely background.

24. **Andrei**
April 16, 2009

Don't you get the feeling that the Dittmars at CERN are getting scared? Until now they had this sort of monopoly on the Higgs boson as they thought that the Tevatron would never say anything meaningful on the matter. Not that the Tevatron has tresults, CERN is getting ants in the pants. Especially since the LHC is as far from running as it was 2 or 3 years ago.

I think running deadlines should be set by the funding governments not some scientists and contractors interested in prolonging their jobs. In other words: operate by x date or lose it.

The Nature of Truth

March 24, 2009
Categories: Multiverse Mania

Seed Magazine has a video and transcript up of a discussion between cosmologist Paul Steinhardt and philosopher of science Peter Galison advertised as *The physicist and the historian discuss the nature of truth as theoretical models of the universe become increasingly difficult to test*.

Steinhardt is no fan of the anthropic landscape and makes a general attack on the idea of eternal inflation, explaining why he prefers his cyclic model:

> The original idea — the way it's often talked about in literature and textbooks, even the way we talk to students — is that inflation makes everything in the universe the same. What we've learned is that inflation actually divides the universe up into little sectors that are all different from one another. Some regions of space would be habitable like ours, but others would be inhabitable; still others would be habitable but would not have the same physical laws or the same distributions of matter that we see here…
>
> Because you have an infinite number of everything, you have no rigorous mathematical or statistical way of computing a probability — it's not even a sensible question to ask. So people are in the process of trying to regulate this infinity. For example, they try to invent a rule for deciding probability that makes what we see likely. But there's no way of deciding why that rule instead of some other one. They simply keep trying until they've found the answer they wanted. Some people are going down that path and are prepared to declare victory if they find something they think works.
>
> Others take a different path. They accept the infinity of infinities and the fact that they can't find any measure for deciding whether our circumstance is more probable or not. They'll be satisfied with the fact that at least some patches look like what we see, and will declare victory on that basis.
>
> Personally, I don't find either of these approaches acceptable, which is why I have developed an alternative picture in which the big bang is not the beginning. A big bang repeats at regular intervals of a trillion years or so, and the evolution of the universe is cyclic.

The two then get into a philosophy and history of science discussion, starting with Steinhardt's:

> We've been talking about an example in which you have a complex energy landscape and an infinite number of possibilities for the universe. But we have no real explanation as to why things are the way they are, because it could have been different.
>
> So it has no power. And without real explanatory power, it's not interesting to me. But I'd be interested to hear how this has played out in the history of science.

and Galison's response:

> We have that sort of split right now among the string theorists. One side says, "Look, what's really scientific is to say there's this infinite or very huge number of craters to be imagined in some landscape, each of which carries different physical particles and different physical laws and so on. And we happen to live in one of them."
>
> But the other says, "You've given up! You've given up the historical project of science. We went into string theory because we wanted to produce a theory that had one parameter, or very few movable parts. And now instead of a glider you've got a helicopter with 10,000 little pieces that have to move exactly the same way. If the slightest thing goes off, it falls to the ground in a heap of burning aluminum."
>
> It's really an interesting moment in that way.

Steinhardt describes the current situation as follows:

> I think it's historic. There's a certain community that feels, "This is an 'aha' moment. Science has to change. We have to accept that science has limits. There's only a certain amount that we'll be able to predict. Beyond that we're going to accept that we live in some special corner of space in which seemingly universal laws — including Newton's law of gravity — are just local environmental laws that aren't really characteristic of the whole."
>
> Other groups say, "Hold it, this is failure. We either find ways of fixing the problems in those theories, or we scrap them and replace them with something else."

The source of the problem here is not actually eternal inflation, but string theory. It is the fact that one needs to postulate a huge landscape in string theory in order to have something complicated and intractable enough to evade conflict with experiment that is the problem. Once one has this, and populates it with eternal inflation, then one has a pseudo-scientific framework with no explanatory or predictive power. Galison notices that string theorists are dividing up into those who follow this path, and those unhappy with it, but it is only Steinhardt who makes the obvious point that what's going on here is just garden-variety scientific failure. The failure though is not attributable to the general idea of inflation, but rather to the string theory-based assumption that fundamental physical theory involves a hopelessly complicated set of possibilities for low-energy physics.

Comments

1. **srp**
   March 24, 2009

   Straight up now:

   1) One could argue that it's too soon to give up looking for "simple" theories that a) unify all of high-energy physics and that b) deduce most of the empirical parameters from just a few, but that string theory isn't the answer to this problem. (This is my understanding of the Woit position.)

   2) One could argue that someday string theory will become that simpler elegant theory and that it is by far the leading candidate to achieve unification and parameter reduction. (This is my toned-down understanding of the Motl position.)

   3) One could argue that no fruitful approach to unification and parameter reduction has been found after a lot of effort and that there are reasons to think that the task is impossible (e.g. a small cosmological constant that is hard to derive from first principles). Then trying to develop a theory of why the parameters are what they are is like trying to develop a first-principles theory of why the earth has its specific circumference or why Britney Spears is blond–a misguided exercise in futility. (This is my understanding of the Susskind position.)

   4) One could propose some theory of one's own that allegedly solves the problem but has been ignored by everybody for various political and sociological reasons. (This is my understanding of the position of some of the commenters on this blog.)

Other than existential or philosophical commitments, is there any rational basis for choosing amongst 1) – 4)? Because what I mostly see is a lot of foreign tourist syndrome–people repeating their assertions LOUDER and S-L-O-W-E-R in the hopes that their interlocutors will understand the invincible correctness of their view.

2. **Peter Woit**
   March 24, 2009

   srp,

   Personally, I don't think this has anything to do with existential or philosophical commitments, rather with the fact that 2) is wishful thinking, 3) is pseudo-science, and 4) are generally being ignored for good scientific reasons. But that's just me….

3. **srp**
   March 25, 2009

   I guess I think that objections to 3) ARE purely philosophical or existential (having to do with the definition of science). Further, they rest on a very particular view of science that is really most relevant in physics and chemistry–that science doesn't study the particular histories of things but seeks only general laws that apply in all times and places.

   I personally find this ideal rather congenial but there is no way to make it a necessary condition for work to be scientific. Otherwise structural geologists who explain how a particular mountain came to be would not be scientists. Not to mention the issue of evolutionary histories in biology.

   In my opinion, the ideal of ahistorical and universal explanations in physics was really placed in danger by two related phenomena: The end of steady-state cosmology and the interpenetration of cosmology and physics. When I was growing up, I just assumed that the universe was eternal and had always existed; talk of "in the beginning" or "the beginning of time" seemed to me nonsensical, poetical, or mythological. In such an eternal universe, we don't explain "where things came from"–the universe has no history and just IS. In such an environment, it wouldn't be possible to explain why the particles have the masses they do (or any other empirical parameter) by appealing to historical accident.

   Then came the Big Bang theory, which necessarily opened the door to the possibility of such developmental accidents. "The symmetry just happened to break this way" and similar arguments become ways of saying that "stuff happens" when an explosion happens and creates the universe.

   The impact of the new cosmology on physics was delayed a bit because at first the flow of ideas was mostly from physics to cosmology. But as the Big Bang and its attendant ideas (e.g. inflation) took hold of the imagination, and as physicists started speculating about high-energy regimes beyond the range of their accelerators, they started to use ideas from cosmology to make sense of the physics. I'm not sure this intimate embrace is healthy for either field (I worry that each assures itself that the other has the answers to its deepest problems rather than solving them directly), but it was probably inevitable.

   Now that physicists turn to cosmology to think about things like ultra-high energy regimes where forces are unified, and given that cosmology is historical given the Big Bang, the door is open for thinking that lots of facts about the universe–particle masses, forec strenghts, etc.–are accidents not derivable from first principles or models with fewer parameters. It almost seems natural, even if you don't believe in multiple universes.

   After all, we turn to historical accident to explain the strength of the Earth's magnetic field, the size of the Sun, and so on. Yet no one argues that the people studying these things are unscientific because they don't deduce these facts from basic symmetry principles. So I think that the general discomfort with these ideas is philosophical and existential.

4. **Aleksandar Mikovic**
   March 25, 2009

   I liked the description of philosophies behind the various proposals for the Theory of Everything given by Srp, and it is good that people are aware of various approaches. Note that even the usual scientific method is based on the beleif that our world can be described in a certain way. However, formulating a TOE is not just another scientific theory, because the domain is the whole Universe and everything in it, and this clearly is not the usual situation. A good analogy is when somebody tries to apply the usual quantum mechanics to the whole universe, and consequently encounters problems of how to define the measurement (no outside observers), how to define probabilities etc. Every scientist has a dream that the Universe can be described by a set of equations which can fit on a t-shirt, but our Universe may not be of this kind. But even if our universe is not described by a simple mathematical structure, I don´t think that would be the end of science. This situation would require a more general way of thinking then the usual scientific way.

5. **thought**
   March 25, 2009

   I think some people need to realize the difference between everything that can exist, and everything that does exist.

6. **Rick Ryals**
   March 25, 2009

   It is a historically recorded fact that scientists ignore certain types of theories for political and sociological reasons, and you can bet your last buck that there will be no complete theory of quantum gravity, and no ToE, as long as this remains the case:

   http://dorigo.wordpress.com/2008/06/23/

7. **Zathras**
   March 25, 2009

   I also like srp's description of the various options. Peter, could you explain further why you would contend #3 is pseduoscience?

8. **Peter Woit**
   March 25, 2009

   Zathras,

   I think I've already done this ad nauseam on this blog. There's nothing wrong with the idea that some things are just an accident of history and you can't compute them. But if you want to do promote a theory and claim that it is science, it has to have some predictive power. If your theory is nothing but an elaborate excuse for not being able to predict things, then it isn't science. If you look into what is going on with the string theory landscape, that's what you find.

9. **Zathras**
   March 25, 2009

Peter, I agree with your stand on Susskind's view on string theory here. This, however, is not what #3 says (although it is part of what Susskind says; there is a discrepancy here). Susskind has two points; one negative (described in #3) and one positive (effectively demolished by you). My question was what do you think of the negative theory described in #3, and why is it pseudoscience?

10. **Joey Ramone**
March 25, 2009

String theory is pseudoscience.

It will be around for a decade or so yet, mostly promoted by physicists in their 70s, some of their past postdocs yet writing grant proposals, and journalists who will slowly realize that it no longer sells magazines like it used to, as it just isn't "cool" anymore, due to the old men hyping it. Brian Greene wore a cool black leather jacket much like mine in an Elegant Universe, and the DVD sold very well, but he probably won't be inducted in the Rock'n'Roll Hall of Fame. He might yet win a Nobel, but in literature or maybe even economics, for mining higher dimensions for millions.

11. **Peter Woit**
March 25, 2009

Zathras,

It seems to me that I'm just repeating myself, but again, the point is that a theory has to have some conventional predictive power about something or else it's not science. If you can't even in principle test it and find out if it's wrong, it's not science. The theory that the fundamental constants were chosen on a whim by the Jolly Green Giant has a great deal of explanatory power but predicts nothing.

It's bad enough to promote pseudo-scientific explanations, worse to do so as a bald-faced attempt to evade the consequences of failure.

12. **Zathras**
March 25, 2009

Peter,

I understand what you're saying. The underlying assumption you are using, however, is that a predictive theory to find these constant (a) exists and (b) can be found. This kind of assumption does not seem to me to be scientifically based, since no such theory of everything has been found.

The inherent problem in the debate is that science self-selects those people who believe the assumption above. If someone did not believe it, they probably would not go into, or stay in, science. I know this from personal experience. I left graduate school at Caltech for mainly these reasons. I lost my faith that such theories could be found, and so I left science.

Again, I am not defending Susskind's defense of string theory. It is not defensible. The lack of an ability to find a testable theory does not allow one to ironically spin out elaborate B.S. and call it science. The negative theory stands on its own. It can be falsified by the finding of such a theory. Since this has not occurred, and I doubt it ever will, the negative theory is what I will place my stake in.

13. **thought**
March 25, 2009

I don't see what people like in srp's classification of possibilities into cases 1)-4). The cases are not mutually exclusive and don't even come remotely close to covering the possibilities.

On the other hand they have the interesting recursive characteristic that srp's classification itself clearly falls into case 4).

14. **Peter Woit**
March 25, 2009

Zathras,

I honestly have no idea which if any of the parameters in the SM can be computed from first principles, and which are historical accidents. I'm not making any assumptions about that at all. I do believe though that the SM is not a final theory and we can do better. That's a working assumption based on history, looking at the problems of the SM, and having some very general ideas about what a better theory might look like that I think are worth pursuing. People who think the SM can be improved upon and who want to try should work on this, those who think it's hopeless should do something else, just not invoke the failure of one idea as proof of hopelessness.

15. **anon.**
March 25, 2009

'The underlying assumption you are using, however, is that a predictive theory … (a) exists and (b) can be found. This kind of assumption does not seem to me to be scientifically based, since no such theory of everything has been found.' – Zathras

You only know what exists and what can be found once you've found them. Scientific research is exploration, and no explorer ever knows in advance for certain what will or can be found. You try to find things. If you stop science because you are uncertain whether you will make progress, you're finished straight off.

I don't think that Woit is implicitly assuming that a theory of everything that makes falsifiable predictions exists and can be found; merely that science is about searching for such things.

If you find a road turns out a dead end, you should admit failure, and try to find a new pathway. You can't know what you are going to find in advance. That's the point of science. Religion is about believing claims that can't make falsifiable predictions.

Scientists should pursue ideas they find interesting, but they shouldn't believe in their validity until they have solid evidence.

Some guy essentially discovered X-rays (causing fluorescence outside a cathode ray vacuum tube) before Roentgen, but he didn't bother to publish (or feared censorship?) because it seemed to contradict the reigning theory of solid impenetrable atoms! That's what you get from believing unproved orthodoxy.

16. **Peter Shor**
March 25, 2009

My opinion (which probably shouldn't count for too much): It's clear that the Standard Model isn't the final theory because it doesn't account for gravity. Previously, any time we've unified two existing theories, this has increased our predictive power – think of Maxwell's equations, special relativity, general relativity, the SM. It would be very disappointing if that didn't happen with the unification of the SM with gravity.

17. **milkshake**
March 25, 2009

There is just not enough to go on. If they cannot propose any signature of the multiverse spawning business then they should say so (and try to do something about the problem).

Not too long ago geologists used to argue whether the continents are moving and whether meteorites produce craters. These things are hard to watch in action but the indirect evidence got pretty good eventually.

18. **neo**
March 25, 2009

The issue is no doubt philosophical–that is why it is not science. As a committed Copernican, I recognize that I live in a privileged environment (the inhabitable zone of a star), and I have no problem with the Anthropic principle as an explanation. So what is wrong with the landscape? the difference is that I know that the (much greater) nonhabitable zone exists–I can observe it. Those who invoke the landscape assert non-habitable universes that we cannot observe, nor have any hope to observe. That is why it is not science. The question is whether Steinhardt's cyclic universe theory is any different.

19. **Artful Codger**
March 25, 2009

[a] Inflation is well-supported observationally
[b] Many models of Inflation are eternal
[c] It is therefore perfectly "scientific" [in the sense that it flows from a theory with observational support] to believe in the existence of large numbers of other ["Coleman-De Luccia"] universes.
[d] Standard QFT makes the *observed* value of the cosmological constant seem utterly bizarre [See Polchinski's 2006 article on the arxiv].
[e] I conclude that the anthropic explanation of this particular number is very plausible.

What I find strange is this: detractors of the anthropic explanation of the CC never seem to be able to propose an alternative. Dear Peter: would I be correct in assuming that you believe that one day somebody will come up with a physical theory that allows one to *compute* the value of the CC from mathematics? This means that *mathematics* is "designed" to allow our existence. This is far more bizarre than the anthropic explanation.

I have sympathy for your points about the role of string theory here, *though* as you can see above, string theory really doesn't have much to do with it at all! What I mean is: if *anyone else* had invoked the anthropic principle, say the LQG people, then certainly the macho string types would have started testiculating about the flabby philosophizing of their opponents. But one should not allow these sociological phenomena, striking though they may be, to obscure the fact that the anthropic explanation of the facts is actually the *least* weird alternative.

20. **Peter Woit**
March 25, 2009

Artful Codger,

No, I have no idea whether the CC is the kind of thing computable from first principles. Maybe the anthropic explanation of its size is correct, who knows? But if you want to claim scientific evidence of an eternal inflation scenario producing a multiverse with a distribution of CCs that allows such an anthropic explanation, you have to come up with a conventional scientific prediction. If this is science, there has to be some way, at least in principle, to put it to a convincing experimental test. Tell me some measurement that someone will someday be able to go out and make that your theory gives a distinctive prediction for. If you can't do this, what you are doing is just making up untestable speculative scenarios, with no way to ever find out which corresponds to reality. That's not science, it's pseudo-science.

And, like any pseudo-science, it's harmless as long as it's ignored by most of the scientific community, which so far seems to be the case (do you have any idea how idiotic most physicists think this kind of thing is?). It's not harmless if it becomes a dominant ideology, used to prop up a failed research program.

21. **Artful Codger**
March 25, 2009

"And, like any pseudo-science, it's harmless as long as it's ignored by most of the scientific community, which so far seems to be the case (do you have any idea how idiotic most physicists think this kind of thing is?)."

Sure, "most physicists" think, or pretend to think, that this kind of work is idiotic. That means nothing. Phil Anderson seems to think that *all* fundamental physics research is idiotic, and I guess that most condensed matter physicists agree. So what? If we allow physics research to be directed by the inferiority complexes of the collective, then we really are doomed. By the way, these same physicists whose help you are enlisting might apply the same words to the kind of research *you* favor. In fact, I'm pretty certain they would.

Calling something idiotic is not terribly scientific. Coleman certainly was no idiot. The fact that our best theory of the early universe [Inflation] leads to unpleasant consequences is not something we should ignore.

Experience suggests that suspending judgement on such matters is not a productive way to go. It's better to identify the most plausible explanation of the facts, and then push it until it works or something better comes along.

22. **srp**
March 26, 2009

Kepler attempted at one point to explain the structure of the solar system by mapping it to nested Platonic solids. Thus the sizes of the various planetary orbits would be a consequence of fundamental mathematics.

Today we think that is crazy, not just because the particular geometric model is unmotivated but because the entire category of explanation is believed to be impossible and misguided. There IS no fundamental, ahistorical explanation for the sizes of the particular planetary orbits in the solar system and nobody sensible wastes his time trying to find one.

We understood this for centuries not because we had data about other stars' planets (we didn't) but from general principles about Newtonian theory and what it could or could not do. The assertion that there is no way to derive specific planetary orbits from first principles is not pseudo-science at all, even if it generates no empirical predictions.

So, since we don't need to rely at all on string theory or multiverse ideas to do it, it can't be pseudo-science to tell people to stop looking for ahistorical explanations of things that are, on theoretical examination, very unlikely to be derivable from first principles. It may be bad advice, in that a clever or lucky person might actually stumble upon some deeper structure, but the argument against such a quest can be made using evidence. That evidence is the nature of existing theory and the nature of the phenomenon in question. It may even be possible to prove that a given class of theories cannot deduce the phenomenon.

I agree that "explaining" an accidental fact by reference to unobservable other universes doesn't add much to our understanding of anything. The urge to do so, I believe, stems from the psychological conflict between physics's deep ambition to explain everything with ahistorical laws and the advent of Big Bang cosmology which gives the universe an unavoidable history. The multiverse and evolutionary universe ideas are halfway houses between historical (accidental) and ahistorical (lawful) explanations– we can imagine a structure or process that makes local accidents part of a broader lawful scheme, which makes some people feel better even if it has no additional scientific payoff.

23. **anon.**
March 26, 2009

'Today we think that [Kepler's nested solids solar system] is crazy, not just because the particular geometric model is unmotivated but because the entire category of explanation is believed to be impossible and misguided. There IS no fundamental, ahistorical explanation for the sizes of the particular planetary orbits in the solar system and nobody sensible wastes his time trying to find one.' – srp

The Titius-Bode law was formulated to predict planetary radii in the solar system and lots of people tried to make sense of it theoretically. Theories of the solar system have been formulated which attempt to model the planets condensing out of a contracting, rotating cloud of gas, and one thing such models try to predict is that empirical law.

Kepler tried all kinds of data-fitting explanations before formulating the three laws of planetary motion, which served Newton so well. I don't think that we need to be biased 'believe' that any approach is impossible and misguided: either it works and is useful, or it doesn't. The whole solar system idea of Aristarchus (250 BC) with the earth rotating daily and orbiting the sun was believed by the mainstream to be impossible and misguided for 17 centuries. Thus there is no science in being prejudiced and disbelieving ideas without evidence, any more than in believing string theory without evidence. What you believe or disbelieve is not relevant for science, which isn't about beliefs.

24. **theoreticalminimum**
March 26, 2009

I just stumbled upon this by accident. The list of participants, and the mention of "God" are somewhat intriguing.

25. **stringph**
March 30, 2009

If Steinhardt's 'cyclic' model – whatever its exact guise is nowadays – were scrutinized with the same rigour as he spends on inflation (or eternal inflation or landscape/eternal inflation) he would never have proposed it in the first place.

It relies on a mathematically unsound concept of infinite past time to a much greater extent than eternal inflation does. (What is the number of our current cycle?) Steinhardt likes to claim there is an 'attractor' behaviour from one cycle to the next – but attractors only attract in one direction: either the behaviour diverges in the past or the future. A past- and future-infinite number of cycles, of which we inhabit a specific one, is not only mathematically problematic, it is dynamically unrealizable.

The scalar potential necessary for the 'cyclic' model to work is even more complicated and absurdly fine-tuned than most inflation or dark energy models. I don't know of any paper where it is written down algebraically. Steinhardt likes to criticize inflation models based on the form of their scalar potentials, but if you never write down your own model in closed form this criticism can be evaded…

And to get a hot Big Bang and restart the cycle one needs a process that (unlike inflation) cannot even be modelled meaningfully in field theory. They call it 'brane collision' and like to send the field value off to infinity while it is occurring, hoping that it will somehow spring back for a fresh new day.

If one compares string theory/ eternal inflation/ landscape ideas to a certain US car manufacturer, Steinhardt's logic seems to be 'GM cars are disappointing and unreliable, therefore I prefer to travel by camel'.

Latest Langlands

March 24, 2009
Categories: Langlands

The site at UBC collecting the work of Robert Langlands is now no longer being maintained. There's a new site now at the IAS. It includes some interesting recent short articles of various kinds that I hadn't seen before, including a short autobiographical memoir, an expository piece written for *Pour La Science*, and another piece which includes extensive speculative remarks about his current thinking on the topic of the "Langlands Program".

The expository piece includes remarks about the remarkable centrality of representation theory both in number theory and quantum theory:

> La leçon que nous voulons tirer de ce dicton, "il se trouve derrière tout nombre quantique une representation d'un groupe", c'est que tomber en mathématiques ou en physique sur les représentations d'un groupe, c'est souvent tomber sur une veine d'or à laquelle il faut tenir corps et âme.

("The lesson we would like to draw from this motto 'behind every quantum number is a group representation', is that when one comes upon group representations in mathematics or physics, one has often come upon a vein of gold, which one must pursue body and soul.")

On the geometric Langlands front, earlier this month the Clay Mathematics Institute organized a series of talks at RIMS in Kyoto by Bezrukavnikov, Gaitsgory and Nakajima about various aspects of the subject. Unfortunately notes from the lectures don't seem to be available anywhere that I have looked.

Last week the KITP in Santa Barbara hosted a mini-conference on Dualities in Physics and Mathematics, with some of the talks devoted to topics relating geometric Langlands and quantum field theory.

Comments

1. **Aaron Bergman**
   March 25, 2009

   At the UCSB conference, there were also a number of really good talks on wall-crossing formulae in math and physics which are well worth checking out.

The Expanding Universe (of Cosmology Centers)

March 26, 2009
Categories: Uncategorized

The past couple months I've seen announcements of the founding of two new cosmology centers at US universities, and I realized that there has been quite a lot of this going on over the past few years here in the US. Going back 5 years or so, I count at least a dozen:

Texas Cosmology Center, Austin (March 2009)

Center for Particle Cosmology at the University of Pennsylvania (January 2009)

Bruce and Astrid McWilliams Center for Cosmology, Carnegie Mellon (May 2008)

Astrophysics and Cosmology Center, Los Alamos (January 2008)

Berkeley Center for Cosmological Physics (December 2007)

Center for Cosmology and Astroparticle Physics, Ohio State (October 2007)

Beyond Center, Arizona State (September 2006)

Moore Center for Theoretical Cosmology and Physics, Caltech (April 2006)

Center for Cosmology at UC Irvine (June 2005)

Kavli Institute for Cosmological Physics, Chicago (March 2004)

Kavli Institute for Particle Astrophysics and Cosmology, SLAC (October 2003)

Center for Education and Research in Cosmology and Astrophysics, Case Western (October 2003)

The job market being what it is, if you're a string theorist you better be an incredible genius (and lucky) to find employment. On the other hand, if you're a cosmologist, well, it doesn't look that hard…

Update: A commenter points to one more:

Institute for Gravitation and the Cosmos, Penn State (August 2007)

This one replaced a previous "Institute for Gravitational Physics and Geometry", part of a trend in physics: cosmology hot, geometry not.

Comments

1. **rrtucci**
   March 26, 2009

   Jesus! how many cosmologists does the US need!

2. **Aki**
   March 26, 2009

   Has anybody heard of a Finnish professor Kari Enqvist? He seems to be a true string believer and famous in the circles or at least they say so on this page (see the link, sorry it's in Finnish). The string theory research is funded by Academy of Finland, so live long string theory con artism. ;-(

   http://www.aka.fi/fi/A/Tiedeyhteiskunnassa/Huippuyksikot_/2008—2013/Teoreettisen-fysiikan-tutkimuksen-suomalainen-huippuyksikko/

3. **chickenbreeder**
   March 26, 2009

   I don't believe there are that many jobs available for cosmologists, either. The phenomenon (of growth in numbers of cosmology centers) simply reflects the trend that U.S. universities are now run by career administrators and buraeucrats. I am positive physicists in those universities didn't initiate the establishment of cosmo centers. Administrators and bureaucrats, on the other hand, have nothing else to do so they constantly reorganize their universities as a way to pretend to be relevant. Since they don't have any professional skill all they rely on is perceived fades and buzz. So, if X University has a cosmology center we want one, too. Cosmology is actually a small thing if you count how many schools and centers of "sustainability" have been created in the last 5 years. I bet it numbers at 100s, even though no one knows what sustainability really means.

4. **AcademicLurker**
   March 26, 2009

   What chickenbreeder said.

   At my own university, there seems to be constant pressure to move toward a center based organizational model and away from a traditional department based one.

   One of the reasons, I suspect, is that the faculty have less and the administration more power in such an arrangement.

5. **anon**
   March 26, 2009

   The grass always looks greener on the other side!

6. **Shantanu**

March 26, 2009

Peter you missed some more 🙂
Institute for Gravitation and Cosmos Penn State (2007)
Tufts Institute for Cosmology (this one has been in existence for almost 20 years)
I am sure there are many more

7. **Peter Woit**
March 26, 2009

Thanks Shantanu,

I did miss Penn State, will add it. Pre-2003, there are a small number of others I didn't try and list, including Tufts, NYU, and Columbia's ISCAP.

chickenbreeder,

These centers are typically not just re-shufflings of existing positions, but involve new money and positions. Notice that a bunch of them have someone's name on them, and it's not the name of a physicist… That's one source of the new money, another is "seed money" from universities. A typical way these centers get funded is by the university agreeing to put up some number of millions of dollars to hire a few people and get the thing going, with the idea that after a while it will be successful enough to attract grants that will fund it for the future. From the university's point of view, putting money into such a center is an investment (or a bet…) that they hope will in later years pay off in new outside funding for the university. I don't believe the impetus behind these things is just from administrators.

8. **Shantanu**
March 26, 2009

There is also another center at
Georgia Tech (although not called a "cosmology") which opened last Fall, with emphasis on gravitational waves and particle astrophysics (which for all practical purposes can considered
be cosmology)

9. **Peter Woit**
March 26, 2009

Thanks Shantanu, but that one seems somewhat different. In this day and age, going on about high energy particles and labelling what one does "relativistic astrophysics" seems very retro.

10. **Chris W.**
March 26, 2009

Are they all getting funding from the Templeton Foundation? 🙂

11. **The Real Deal**
March 27, 2009

Not to be outdone, Perimeter not only expanding its cosmology program but erecting a brand new 55,000 sq ft building soon. One needs appropriately fancy facility to accommodate none other than Stephen Hawking. Who else can claim Hawking on staff? The writings on the wall – strings is out, cosmology is in. And money trumps everything.

12. **Cormac O Raifeartaigh**
March 27, 2009

Re "a bunch of them have someone's name on them, and it's not the name of a physicist…"
I'm glad you said that, I was getting worried at all these important names in cosmology that I've never even heard of!

More seriously, there is a point that has not been mentioned – cosmology has truly come of age in the last decades and the convergence of cosmology and particle physics is a relatively late development (when I was a student, cosmology was still an 'out-there' branch of physics). IN this regard, cosmology institutes might be a good thing for particle physics, if the atter is done there – it'sprobably easier to convince uninformed skeptics to fund institutes that study the 'universe' as opposed to the 'atom' – while maintaining particle research!

13. **Shantanu**
March 27, 2009

Peter, in this age particle astrophysics is really intertwined with cosmology
and neutrino/gamma ray telescopes can tell you about nature of dark matter.
With grav. waves also, a whole lot of cosmology can be done. A few other similar centers whose names are not "cosmology" (in recent years) which have come up are
Center for Particle Astrophysics (Fermilab) (and many of its members are
involved in cosmology). Also MIT's center for space research was renamed to
Kavli Institute of astrophysics and space research with main emphasis on
determining nature of dark matter and energy.

14. **Mike**
March 28, 2009

Has anybody actually detected gravity waves yet? The last I heard LIGO was still looking and not seeing the waves from GRBs.

15. **Thomas D**
March 30, 2009

At current sensitivity LIGO expects to see signals from things like –

SN1987a – ie any SN at comparable distance;
Neutron star and black hole binary mergers up to some number of (M)pc;
GRBs above some threshold on energy release over distance.

The fact that no signals arrived is not really a surprise as the number of such events per year in our vicinity is not large. We may be lucky tomorrow with another supernova. But basically the detector is performing as expected.

Future experimental sensitivity is expected to be one (soon) or two (not soon) orders of magnitude better. Since detectability goes with 1/distance, the expected number of events goes up roughly with the inverse cube of the sensitivity. Just wait a few years.

Anything Goes

March 26, 2009
Categories: Multiverse Mania

Yesterday at the KITP Wati Taylor gave a talk entitled [Freedom and Constraints in the Landscape of Intersecting/Magnetized Branes](). During the talk he explained in detail the problem of lack of predictivity caused by the landscape. As far as anyone knows, to the extent you can calculate anything, you can get whatever you want: "Anything Goes", and string theory is useless for ever predicting anything. He was looking at some particular classes of vacua, chosen for their computational tractability, and hoping to find some constraints among the quantities one can compute. There's no known reason to expect this, but one can compute anyway and hope. The end result was the expected one: you can get whatever you want. Here are some quotes from the talk:

> So, We're really in a very challenging situation where we don't really know how to define the theory, we don't know what the set of solutions are, and even if we did we would have a very hard time making a sensible statement about what that means for predictions…
>
> Every piece of data we have so far I would say is consistent with the notion that everything is pretty much uniformly and randomly distributed in the landscape.

There was extensive discussion of the predictivity problems and overwhelming evidence string theory can't ever predict anything below the Planck scale (this wasn't discussed, but I don't see how it predicts much above the Planck scale either). For some reason there was no drawing of the obvious conclusion that one should just give up on the idea and try something else.

Comments

1. **capitalistimperialistpig**
   March 26, 2009

   The counsel of dispair is too bitter a drink to swallow in one gulp..

2. **Jim Clarage**
   March 26, 2009

   If "everything is pretty much uniformly and randomly distributed in the landscape" I wonder which space is larger:

   1) landscape space of possible string theories

   2) space of possible random text-strings with a given maximum length, corresponding to the possible TOE Lagrangians one could type on a piece of paper.

   Monkeys and Shakespeare and keyboards come to mind.

3. **capitalistimperialistpig**
   March 26, 2009

   er, despair.

4. **srp**
   March 27, 2009

   I think they're taking Margaret Thatcher's old TINA position–there is no alternative.

5. **Nameless**
   March 27, 2009

   Did we move away from 10^500 string theories to infinite number?

   If we did not, the problem is not that string theory is useless for ever predicting anything, it's that the structure is too big and complex. But it should still be theoretically possible to answer the question whether there is at least one in 10^500 that has the right particle content and predicts low-energy constants in agreement with observations. Maybe not at this stage (we need 500*ln(10) = 1150 bits of information), but eventualy. We have 19 constants just in SM, some, such as electron mass, are known to 10 digits. Neutrino oscillation means that we get at least 7 there (three masses, four MNS parameters). If LHC finds supersymmetry, the number of parameters will blow up. Sooner or later we will get to the point where the number of string theory false vacua within the boundaries of observed constants is either 1 or 0. And that will be the definitive test. In the mean time it's a valid avenue of research to analyse the landscape.

   Does anyone have a reference to an article that initially computed the 10^500 number?

6. **Bee**
   March 27, 2009

   Odd. This doesn't quite seem to agree with what Dienes and Lenneck have found. See arXiv:[0804.4718]() or PIRSA:[08120000]().

7. **Cormac O Raifeartaigh**
   March 27, 2009

   Vintage Woit, well said. My namesake tells me you're in Dublin next month – I'll see if I can get a day off in the big smoke…regards Cormac

8. **Masochistic Troll Feeder**
   March 27, 2009

   Who cares whether string theory can ever predict anything below the Planck scale? It's supposed to be quantum gravity, so the real test is what it predicts ABOVE the Planck scale!

   Do you throw out the Standard Model because it does not predict biology? If we ever discover life on other planets, it will likely have very different biology from our own. All these possible different biologies will of course be consistent with the Standard Model, just like many different low energy field theories are consistent with string theory, but the Standard Model does not select out a unique possible biology, just like string theory does not select out a unique low energy physics!

9. **Peter Woit**
March 27, 2009

Bee,

What Dienes is doing is a bit different. He's counting vacua and hoping for correlations. Taylor explicitly says he doesn't think counting is relevant: if you don't know the details of what cosmological process is producing our universe, you don't know what the right probability measure on vacua will be, no reason to believe it's the simple counting measure.

Nameless,

I don't have a specific reference at hand, but look up Michael Douglas's papers on the landscape and either he's the first to come up with this number, or he refers to the paper that did. People (e.g. Denef) have come up with constructions of vacua that produce much larger numbers of vacua than 10^500, effectively the number you would have to examine is infinite.

Hi Cormac,

I'll be in Edinburgh April 21-23 (for the conference in honor of Atiyah), then the 24th will stop for the day in Dublin on the way back to NY. I'm hoping to extend that and stay for another day or so. Once I've got definite plans I'll contact you and maybe we can get together.

10. **Daniel de França MTd2**
March 27, 2009

Yesterday, Renata Kallosh finaly proved (a physicist proof) that N=8 Sugra is renormalizable at all loops and for all legs. It seems that a certain the E7 symmetry is never broken, and delays the firt non renormalizable singularity up to infinity and rendering the theory renormalizable.

Here is the article:

http://arxiv.org/abs/0903.4630

11. **Joey Ramone**
March 28, 2009

Yes–I have been talking with Feyman about how ridiculous the string theory folly is getting. You know how Feyman once said that science is like creating in a straight jacket. And just the other dya he said string theory is like taking the jacket off, saying and doing anything in the landscape, calling it science, and then putting the straight jacket on your critics.

Remember that one band which tried to get famous by just removing all the frets, putting their fingers anywhere on the guitar neck, and proclaiming it to be music? I don't remember them either. And I'm pretty sure they weren't state funded nor tenured either.

12. **Aaron Bergman**
March 28, 2009

Dissing fretless guitars now? You're still bitter about that whole Fleetwood Mac thing, aren't you?

13. **Joey Ramone**
March 29, 2009

We were all for innovation, rebellion, and rock'n'roll, but even we had our rules honoring the mathematical vibrations of strings. It just wouldn't be fun walking out there on stage with strings vibrating in an infinite array of possibilities, all over the landscape, and then shouting at the press, "THIS IS BEAUTIFUL, ELEGANT MUSIC!" Sure, some of the press would write wahatever we said just so they could join us on the bus later, but would we really want them there? I mean they don't even trust their own ears so how can we trust them? Would you want them on your bus around your groupies?

Peter wrote, "For some reason there was no drawing of the obvious conclusion that one should just give up on the idea and try something else," and my good friened Einstein stated, "Insanity: doing the same thing over and over again and expecting different results." Maybe Feynman was more right about those straight jackets than he even knew.

14. **Aaron Bergman**
March 29, 2009

Well, you shouldn't stop thinking about tomorrow. It'll soon be here.

15. **Joey Ramone**
March 30, 2009

Yes–tomorrow will be better day as string tehory fades away.

I have also had the opportunity to converse with a lot of other classical economists–folks like Adam Smith and Ludwig von Mises. They see a lot of parallels between the snarky math of String Theory and Wall Street. Smith and von Mises dealt in ideals, ideas, integrity and *words* that meant things. String Theorists and Quants deal in math that never quite proves their fundamental postualte taht they are smarter than us, but rather which proves the exact opposite. They have given math a bad name and this makes me angry. Good Pythagoreas comes up with the theory of harmonies on vibrating strings and then they corrupt and convolute it all into a discordant landscape, while bankrupting a nation and currency with their silly little equations which have absolutely no predictive power, robbing them of math's higher beauty, which is married to the power of prediction. Riding on Einstein's, Smith's, et als. coattails, the modern snarkyalarks levelethey pay d physics and finance. Newton stood upon the shoulders of giants to see further, and they stood upon the shoulders of giants to cut the giants' heads off. And now we're supposed to buy their "Elegant Universe" DVDs and bail them out, while they pay Lubos to call us names.

Speaking of Lubos, not only did he inspire that character on The Big Bang (perhaps String Theory's greatest lasting contribution), but back in the eighties we were able to hop on Michio Kaku's time machine and travel to 2012 and then back again with Lubos so that he could play the cop at 50 seconds in this 80's video for Rock'n'Roll High School:
http://www.youtube.com/watch?v=c5vh0QHUA1w

16. **Mikael**
March 30, 2009

Hi Peter,
about predictivity of string theory:
Imagine we had one million string vacua, whose low energy physics content look exactly like the standard model. (We can discuss later how realistic this is.) Now, we could say, that predictivity is totally lost, when we want to go to high energy physics. On the other hand, let's compare the status of string theory in this situation with quantum field theory. In quantum field energy there is simply no way at all to go to high energy physics. QFTs are even understood today as the low energy effective theories of some unknown high energy theory. So computational issues aside we can use one of our one million string vacua and it is just as good as the standard modell. One could argue that it is even better because it is a theory of gravity as well and you can calculate general features of gravity like the black hole entropy.
Questions:
1. Is there an error in this line of thought? Is the error that in the assumed standard model string vacuum you just had to many parameters to fit?
2. Do you think my assumption is realistic to have string vacua, which look exactly like the standard model in the low energy range?

17. **Peter Woit**
March 30, 2009

Mikael,

One problem with this sort of argument is that it's just not true that string theories with a choice of one of these vacua gives you a consistent theory at any energies. You don't even in principle know what the theory is outside of perturbation theory, so you don't know that you have a consistent unified theory at arbitrarily high energies. Even if you did, the freedom of choices presumably would allow you to get all sorts of different physics at very high energies.

The Standard Model is a gauge theory with one of the simplest possible choices of groups and representations, and it is this simplicity that makes it highly predictive and gives an extensive array of very strong tests that it has passed. In "string vacua" constructions, simple choices don't look like the real world, so people have to make constructions more and more complicated in order to evade making a prediction that can be compared to existing data.

There's all the difference in the world between these two situations, and going to high energy changes nothing. String theory is no more predictive there than at low energies. The only difference is that at high enough energies you can make sure your predictions can't be checked.

18. **Mikael**
March 30, 2009

Peter,
let's forget about high energies. Let's just compare string theory and QFT in energy regions explored by today's colliders. Is it conceivable that we arrive at a situation, where we can say, string theory did not keep its promise but it is not worse than QFT?

19. **Peter Woit**
March 30, 2009

Mikael,

Again, at collider energies, quantum gauge theory is a huge success. You pick one of the simplest ones, it predicts all sorts of things, they all come out true on the nose. In string theory instead, you get no predictions, because you have to make your model more and more complicated in order to evade getting a wrong answer. The difference between these is the difference between dramatic success and dramatic failure.

20. **Shantanu**
March 30, 2009

Peter (and other QG enthusiasts), what do you people think of
a href="http://arxiv.org/abs/0806.0665″>this
Hogan has given talks on this which are available
at here?
This paper however as been refuted
here
Have you seen any papers by string theorists claiming this as a prediction
of string theory?

21. **Peter Woit**
March 31, 2009

Shantanu,

I haven't seen this claimed as a "prediction" of string theory. For one thing, just being "holographic" doesn't mean you're doing string theory. Personally, I find it hard to believe that you can get any serious prediction just by invoking "holography", which is a very general concept. Because of that and my general lack of interest in quantum gravity, I haven't looked into exactly what Hogan is doing.

22. **Durendal**
April 4, 2009

Right, this is just like how classical mechanics is totally worthless to predict anything, because a lagrangian can be anything I want it to. A lagrangian can be any function at all! How can we expect to ever find a lagrangian that describes reality when they can be anything we want!

23. **Herodotus**
April 6, 2009

Not quite. Not only, once the Lagrangian is chosen, the predictions of the classical mechanics are known, but the useful Lagrangians are simple. If one had to use a completely different Lagrangian to predict the fall of an apple compared to the behaviour of the Moon, or even the fall of different apples, then we would have a "stringy" classical mechanics.

24. **Shantanu**
April 8, 2009

Peter, from listening to one of his talks, the kind of holography is a

prediction of Bank/Susskind et al's Matrix theory models .
(see the audio of his talk at
here
Anyhow do u know of any string theorists who are citing this tentative result from GEO-600 as an "evidence" for string theory?

25. **Peter Woit**
April 8, 2009

Shantanu,

I haven't seen any string theorists claiming this.

In any case, this can't be claimed as a prediction of such matrix models, since they haven't been made to work at all in realistic cases (four large dimensions).

Origin of the World

April 1, 2009
Categories: Multiverse Mania

Arizona State University's new Origins Initiative is starting off this year with some mind-expanding programs, including an Origins Symposium that will start tomorrow. The number and quality of speakers who will be making the trip to Tempe is remarkably high. The event will be webcast, so the rest of the world can get the inside dope by following along here.

Cosmology will be the main topic on the first full day of the Symposium, with Science Friday broadcasting live at 11am a panel discussion on "Physicists and the Origin of the Universe". The afternoon program will be in three parts, with the last part about new observational methods. The first two deal more with the chronic heady topics and pipe dreams of theorists ("How Far Back Can We Go?" and "Is our Universe Unique, and how can we find out"), and will have a break for tea and brownies, finishing up at 4:20 with Glashow (whose title is the blunt "Is Particle Physics Over?") and Vilenkin ("Mediocrity as a principle").

I hear that refreshments will be provided by Tempe's own ChebaHut and there will be an exhibit featuring work of local artists. Arizona is putting on quite a show, with a major effort to attract cutting-edge researchers in physics to the state, including the recent announcement of proposed new legislation.

Update: The final paragraph above was inspired by the posting date, and is pure fantasy. In addition, I have no idea whether brownies will be served during the break tomorrow, or what might or might not be in them. I do look forward to seeing what the various speakers will have to say, and am sure they will be addressing the multiverse/pre-big-bang topic without the aid of any mind-altering substances, difficult as that may be to believe.

Comments

1. **Shantanu**
   April 1, 2009

   Thanks, Peter for the link. I will be watching the cosmology talks.
   In 2003 there was another very awesome conference at Case on Cosmology (which I attended).
   here
   and there is a discussion of anthropic principle (which is the first time I saw it)

2. **AO**
   April 1, 2009

   "finishing up at 4:20"?? is that serious??

3. **artist**
   April 1, 2009

   The question of the origin of the world was solved long ago:
   http://www.math.columbia.edu/~woit/wordpress/?p=1777#comments

4. **artist**
   April 1, 2009

   I mean
   http://www.musee-orsay.fr/en/collections/works-in-focus/search/commentaire/commentaire_id/the-origin-of-the-world-3122.html

   [Warning: NSFW]

5. **Jeffrey McGowan**
   April 1, 2009

   OK, it's not nearly as good as Courbet, but still…

   http://arxiv.org/PS_cache/arxiv/pdf/0903/0903.5321v1.pdf

6. **DatUCLA**
   April 2, 2009

   best april 1st post ever

7. **Joey Ramone**
   April 2, 2009

   If we can get Paul Davies' time machine working on time, the Ramones will be headlinig the big bash at the Origins Initiative!

   We have a new song :

   Rock-rock-rock-rock'n'roll cosmology center!

   "Rock N Roll Cosmology Center"

   Well I don't care about string theory
   Rock, rock, rock'n'roll high school
   'Cause that's not where I wanna be
   Rock, rock, rock'n'roll high school
   I just wanna have some multiverse kicks
   I just wanna get some multiverse chicks
   Rock, rock, rock, rock, rock'n'roll high school

Well the girls out there knock me out, you know
Rock, rock, rock'n'roll landscape
Cruisin' around in my time machine GTO
Rock, rock, rock'n'roll high school
I hate all predictions and old principles
Don't wanna be taught to be no fool
Rock, rock, rock, rock, rock'n'roll high school

Fun fun rock'n'roll cosmology center
Fun fun rock'n'roll cosmology center
Fun fun rock'n'roll cosmology center
Fun fun, oh baby

The album is called "A Cosmology Center in every Multiverse."

Hope to see you at the conference!

Towards the end of the video, the LHC is turned on and it
1) proves string theory
2) finds seven multiverses
3) locates 17 higher dimensions
4) proves the anthropic principle
5) creates baby black holes which become bouncing universes
6) explodes before any of this can be recorded

Joey

8. **Joey Ramone**
April 2, 2009

P.S. Here is the video for Rock'n'Roll Cosmology Center: http://www.youtube.com/watch?v=DhRALq8IsL4

Towards the end of the video, the LHC is turned on and it
1) proves string theory
2) finds seven multiverses
3) locates 17 higher dimensions
4) proves the anthropic principle
5) creates baby black holes which become bouncing universes
6) explodes before any of this can be recorded

9. **John Hungaton**
April 9, 2009

In one of the discussions in the Origin conference on Monday, it was suggested that Sundrum's model "must be right", as it "got him tenure". This sort of reasoning is almost unbelievable…

On the other hand, Brian Greene made it clear that one should talk about "string conjecture", not "string theory". He was not giving the impression to be completely sure that the whole endeavor is on the right track. Now, if Greene does not believe in it, who does?

John

10. **woit**
April 9, 2009

John,

I'm pretty sure the comment about Sundrum was a joke. On the other hand, in an environment where no one is coming up with models that make contact with experiment, maybe a model is "successful" if it gets one tenure.

From what I've seen, Brian has always been careful to claim that string theory is a conjecture, and it's not appropriate to "believe" in it. I think he and most string theorists desperately want to find some sort of prediction of the theory that would allow it to be tested, and are very frustrated that that has not turned out to be possible so far. To what extent he and others in their heart of hearts are getting to the point of doubting that the whole idea is on the right track is an interesting question I don't know the answer to.

Various News

April 6, 2009
Categories: Experimental HEP News, Multiverse Mania, Uncategorized

The CERN Bulletin has been providing weekly updates about the progress of LHC repairs, with the latest one here. One thing they don't seem to have mentioned is that it looks like the schedule has recently slipped by nearly a month. The schedule approved in early February had checkout of the machine in week 38 (week of Sept. 14) and first beam week 39 (week of Sept. 21). The latest draft of the schedule (see page 42 of this presentation) has checkout in week 42 and beam in week 43. So, it looks like the latest plan calls for injection of a beam around October 19th, collisions sometime in November.

I today heard an odd rumor about a problem this past weekend with the on-going repairs in sector 34, but there may be nothing to it, so I'll try to stick to only mongering confirmed rumors.

The blogging world continues to expand with new institutional initiatives to set up blogs. There's a new version of Quantum Diaries out, along the same lines as a similar site set up back in 2005. That's the one that Tommaso Dorigo survived, along with some other physicists who have kept blogging, including Gordon Watts and Peter Steinberg. Unlike the 2005 incarnation, this version seems to be restricted to experimentalists, no theorists allowed. It also uses the same address as the old site, which unfortunately seems to no longer be accessible. Whether it is possible or desirable to set up a mechanism to ensure the availability in the future of blog content is an interesting question.

The AMS has set up a blog for mathematics graduate students, which so far mostly consists of professional advice.

One piece of news that might be interesting to some of these graduate students is that the NSF has just announced a plan to use some of the stimulus money to provide 30 two-year postdocs aimed at students on the job market this spring who have not yet found employment. The money is being funneled through the various institutes supported by the NSF, with the idea that the jobs will generally be hosted at other institutions, which will provide a mentor and possibly teaching opportunities. The deadline to apply for these jobs is very soon (April 10), there's more here, here, here and here.

One unusual thing about these postdocs (compared to usual NSF postdocs) is that it appears they are available to anyone who is getting their degree from a US university, not just US citizens or green-card holders. It also seems to be possible to hold the postdoc outside the US, at some MSRI-affiliated institutions such as the University of Toronto. Adding up the cost of the 30 postdocs comes to maybe $3 million or so, leaving open the question on everyone's mind in academia: what about the other %99.9 percent of the $3 billion in NSF stimulus money? Where's that going to go?

I always wondered who the Pupin building housing the physics department here at Columbia was named after. Here's the scoop.

The Origins symposium at ASU is finishing up today. It was webcast, but if you missed it archived video is starting to appear here. The Science Friday segment featured Michael Turner responding to Steven Weinberg's claim that some anthropic argument is just common-sense with the remark that "some of us chafe at using anthropic and commonsense in the same sentence". I haven't yet seen the full multiverse discussion from later last Friday, but presumably that will be available soon.

Update: One more. Today at CERN they're celebrating Carlo Rubbia's 75th birthday. Webcast going on right now, slides here.

Update: Hamish Johnston at Physics World asked CERN spokesman James Gillies about the delay in the draft schedule:

> He also said that CERN is now looking for ways to make up the extra time identified by Bailey and he said that the repair team are confident of having the LHC running towards the end of September as planned.

Update: Yet one more: New Scientist has an interview of Witten by Matthew Chalmers here.

Comments

1. **Coin**
   April 6, 2009

   Whether it is possible or desirable to set up a mechanism to ensure the availability in the future of blog content is an interesting question.

   Well, let's just check archive.org…

   > We're sorry, access to http://www.quantumdiaries.org/ has been blocked by the site owner via robots.txt.

   Oh… hm.

2. **Bee**
   April 6, 2009

   http://user.web.cern.ch/user/news/2009/090406.html

3. **Michael**
   April 7, 2009

   Hi Peter,

   the rumor about a problem in LHC sector 34 has some truth, in so far as there is a magnetic which will take longer to repair than originally planned. However, I have it on good authority that sector 34 is not on the critical path, so this problem magnet will not delay the LHC schedule further. (I was personally very worried about further delays, so I asked someone in a meeting for details, which he delivered.)

   regards,
   Michael

4. **Thomas R Love**
   April 7, 2009

   Columbia has a site about Pupin:

   http://www.columbia.edu/cu/physics/about/main/one/michaelpupin.html

I read his book:
The New Reformation: From Physical to Spiritual Realities – Michael Pupin –
many years ago. He refers to scientists as prophets.

5. **Thomas R Love**
April 7, 2009

According to the Mathematics Genealogy Project:

http://genealogy.math.ndsu.nodak.edu/id.php?id=72169&fChrono=1

Pupin had a graduate student by the name of Robert Millikan.

6. **Nigel Cook**
April 7, 2009

Pupin independently rediscovered and patented Heaviside's original 1875 discovery that long telegraph cables could be made suitable for distortionless speech (long distance telephones) by increasing the inductance (e.g., putting inductance coils at intervals – merely amplifying the signal just amplifies the distortion, because the inductance effect is frequency dependent). Heaviside didn't have funds to apply for a patent. Pupin's patent in the USA made $1 million.

Heaviside in England, despite formulating Maxwell's equations in vector calculus, was simply ignored by Sir William Preece, head of the Post Office Telecommunications. Preece instead used public money to fund his own incorrect theory that the long-distance telephone voice distortion was due to the cable design, and tried for years to overcome distortion with better cables, setting back the introduction of long-distance phones in England by 20 years. Heaviside became increasingly rude towards Preece:

'If you have got anything new … you need not expect anything but hindrance from the old practitioner even though he sat at the feet of Faraday. Beetles could do that … . But when the new views have become fashionably current, he may find it worth his while to adopt them, though, perhaps in a somewhat sneaking manner, not unmixed with bluster, and make believe he knew all about it when he was a little boy!' – Oliver Heaviside, 10 March 1893.

Preece had just stated in his 1893 IEE Presidential address: 'I took the opportunity to formulate the theoretical views of electricity that I had acquired at the feet of Faraday.'

7. **Peter Woit**
April 7, 2009

Thanks Michael,

From looking at the schedule, I had assumed that fixing sector 34 was on the critical path. If that's not true, do you know what it is that now is considered the most likely source of delays?

That sounds like a different rumor about a sector 34 problem, one better founded than what I heard…

8. **KevinS**
April 7, 2009

from the link…

While studying electrical engineering at Columbia, Pupin became interested in Maxwell's equations. After graduation in 1983, he went to Cambridge, England in order to work under James Clerk Maxwell in 1883 (looks like time travel was possible then). Alas, when he arrived at Cambridge, he found out Maxwell died four years earlier in 1979. This was the reality of trans-Atlantic communication at that time. There has been some improvement since then. (though, it would seem, not in editing)

damn…1979…I could've tried for a signed edition from Maxwell.

9. **Andrei**
April 8, 2009

There is no way the LHC will run this year. Last time it took 6 months or more to cool each sector. To have beam in September, Cern would have to start to cool them now. Sector 34 is still under repairs so the cooling of this sector will realistically be sometime in july or August. You might be better off covering the new batch of results expected from the Tevatron in August. The most optimistic expect a Higgs 3 sigma anouncement. After all if the LHC was running the source of excitement would come from its results not from the simple fact that it is running. And having a low power beam cannot be called "running" in any case. Some of you might have to face a brutal truth: the LHC is a bit of a failure.

10. **Peter Woit**
April 8, 2009

Andrei,

If you look at the draft schedule, they certainly have a detailed plan for the cooldown, and a lot of experience from last year in how long it takes. My guess would be that, if nothing else goes wrong, in October they'll be able to circulate a beam, colliding beams in November. They may be able to take some data late this year, but it will be at very low luminosity. It's also true that even during 2010 the luminosity will be rather low, and energy only 5 TeV per beam. For many things (especially the Higgs), it won't be until 2011 that they start getting useful data.

This doesn't mean at all that the LHC is in any way a failure, it just means that it is taking longer to get running than the official estimates originally indicated. This isn't surprising in the least for a project of this complexity. In the long run, how long it takes to get the thing working is not important, what is important is whether it works. Even if it takes 10 years to get the thing working at design luminosity, if it then makes important discoveries, it will be a huge success.

Official schedules are almost always overly optimistic, naturally reflecting estimates based on assuming that nothing significant goes wrong. Something almost always does go wrong, but, with some time and effort, whatever it is gets fixed. The huge media attention on the LHC may be more of an education for the public about how major scientific projects get done than some people intended.

11. **Sumar Ongi**
April 10, 2009

> Update: Yet one more: New Scientist has an interview of Witten by Matthew Chalmers here.

Well, *interview of Witten* turns out to be a wild overstatement. The article contains a couple of unremarkable sentences said by Witten, surrounded by several tens of lines of mostly meaningless chit-chat by the author, who doesn't seem to be a specially gifted writer. I think a more correct description is *a little bit of trivia about Witten*.

12. **Peter Woit**
    April 10, 2009

    In defense of Chalmers, I know a large number of science writers who have tried unsuccessfully to get a comment out of Witten about the "String Wars". He's the first one I know of to have any success at all with that…

13. **Shantanu**
    April 10, 2009

    Peter, I agree with Andrei. If LHC keeps getting delayed, it will be hard to attract grad. students (and postdocs) towards it as they need data for their thesis and no one likes their Phds to get extended and that is not a good sign.
    These grad students would gravitate towards particle astrophysics, neutrino physics or cosmology instead of LHC.

14. **Shantanu**
    April 10, 2009

    Peter, while looking at CERN site found these acadameic training lectures on "String theory for pedestrians"
    here
    Have you seen it? any comments?

First Principles

April 10, 2009
Categories: Book Reviews

I learned recently from Sabine Hossenfelder's blog that there's a new book out by Howard Burton, entitled [First Principles: The Crazy Business of Doing Serious Science] (she has some comments on the book [here]). It's a fascinating and entertaining book. I couldn't put it down this morning, so took a very long breakfast during which I finished reading it.

Burton got a Ph.D. in theoretical physics at the University of Waterloo, which led him (like most Ph.D.'s in theoretical physics) to need to find some sort of employment doing something else. He was saved from getting wealthy in the financial industry by Mike Lazaridis of RIM, who was on a list of people that Burton wrote to asking if they had any job openings. It turned out that Lazaridis was starting to think about the idea of funding a theoretical physics institute, and decided that Burton was just the person to hire to look into the idea.

One of the most interesting parts of the book is Burton's description of the process he went through of talking to a large number of theorists around the world to get their ideas about the state of theoretical physics, and about what sort of well-funded new institute would be viable and make sense. An interesting problem to have, and one that he got a lot of good advice about from many people.

One of the main motivating ideas behind the founding of Perimeter was to support work on foundational issues that normally don't get funded because they are considered "too hard" to make progress on. The other was to encourage openness and communication between groups that normally don't talk to each other. Burton was quite struck by the situation with superstring theory (this was back in the late 1990s, long before the recent "string wars"):

> …what did shock me was my growing awareness that the field was rife with dissention and sociological barriers. Superstring theorists, for example, did not interact in any meaningful way with people pursuing other approaches to the problem, and vice versa. Worse still, the groups were downright hostile to one another, lobbing ad hominem and defamatory attacks across one another's bows, condemning mountains of work with a dismissive (and often ignorant) wave of the the hand while trumping up the claims of their own theories well beyond any defensible level… they were simply refusing to engage with one another, separating off into rival sects like high school gangs…
>
> My first, albeit indirect, encounter with superstring theory was a perfect case in point. As a PhD student, I spent a good deal of time talking with Nemanja Kaloper… Actually Nemanja did most of the talking, particularly once he discovered that I had gone over to "the dark side" by opting to spend time learning various non-superstring approaches to quantum gravity instead of his beloved superstring theory. For Nemanja, this was nothing more than a time-wasting combination of obstinacy and simple-mindedness: superstring theory simply was quantum gravity; trying to learn quantum gravity without string theory made about as much sense as writing music without notes…
>
> The more I kept my eyes open the more I realized that Nemanja's behaviour was hardly unusual: indeed such counterproductive squabbling and rampant dogmatism existed on all sides of the issue, making it hard to see how any genuine progress in any direction might be attained in the near future…
>
> The Olympian heights of pure reason, when examined in more detail, turned out to be reducible to a furiously contested form of highly esoteric tribal warfare.

The story of how Perimeter grew out of these ideas and came into being is a fascinating one, and Burton tells it with a sense of humor in a very entertaining way. He became executive director of the institute, all the while lacking the usual sort of credentials as an eminent researcher that would be expected for such a position. The book ends with a short epilogue discussing the fact that he was forced out of this position in June 2007:

> The official reason given for my departure was that contract negotiations broke down, but I think it's fair to say that such a justification hinges on a particularly loose interpretation of the world "negotiations".

He speculates that the reason for this was the book he was writing:

> So what on earth happened?
>
> What happened, so far as I can determine, is the book you hold in your very hands. Bizarre as it may seem, it appears that a major preoccupation of the institute's board of directors for the first six months of 2007 was what to do about this pernicious book, followed closely, presumably, by how to get rid of its author who had the brazen temerity to once again bring the dark story forward publicly.

I know nothing about what really happened, but if Burton is right that the book played such a role, that's extremely odd. The book makes a wonderful case for Perimeter and what it has been doing, portraying it (accurately I think) as a big success. It does so in a way far more effective than the kind of PR materials such institutions usually hire professionals to produce.

Perimeter does seem to have become quite a success, playing an increasingly large role in the theoretical physics community. It now has a new director (cosmologist Neil Turok), plans to expand, and nine prominent members of the theoretical physics establishment have recently signed on as "Distinguished Research Chairs". If anything, one might worry that the institution is in danger of too much conventional success, merging with the establishment that it was set up to provide somewhat of a challenge to. Their advertisements for new faculty specify that they are looking for people in "Cosmology and Quantum Information". I don't know much about the quantum information business, where they seem to be leaders, but these days the idea of a well-funded institute for cosmology isn't exactly revolutionary (see [here]).

I'm curious to see what happens with Perimeter now that it's entering adulthood, and glad to have read Burton's book which does a great job of telling the story of its birth and infancy.

Update: Lubos has a [posting] about this, although it's clear he hasn't read the book. According to him Burton "managed to write a public text that exposes pretty much all the business (and personal) secrets of the Perimeter Institute. He wants to earn money by publishing this sensitive stuff."

If you buy the book hoping to find out the "business (and personal) secrets" of PI, I think you're going to be very disappointed.

Comments

1. **Jonathan Vos Post**
   April 10, 2009

   Thank you for shedding light on these beginnings. This is much better than a hazy, rose-colored "creation myth" growing up as penumbra to an organization.

2. **Bee**

April 10, 2009

Odd indeed, isn't it? The only part of the book that is critical about PI is for all I can tell that epilogue.

Howard's assessment about groups that don't talk to each other etc sounds very familiar doesn't it? I think it's good it is being said by somebody who has not been personally involved with either of these research topics. It would be interesting to know whether similar problems exist in other fields of science.

From what I have seen in the last years, PI is just simply an excellent place (I don't get paid for saying that, it's true). They have some growing pains though that I am afraid will persist for some while. It won't be easy for Neil.

3. **Matt Leifer**
April 10, 2009

Having recently made hires in quantum gravity and quantum foundations, I wouldn't worry too much about PI losing its unconventional edge. They usually just focus on a couple of subject areas for hiring each year because they don't have the resources to conduct a comprehensive search in all subject areas all of the time. They have recently lost Michael Nielsen in quantum information, meaning that quantum information and cosmology are currently the smallest groups, consisting of one researcher each.

4. **Chris W.**
April 10, 2009

Where is Nielsen going (or where has he gone)?

5. **Kea**
April 10, 2009

If you look at the PI website it says "Cosmology" OR "Quantum Information". I would be much more impressed if the conjunction had in fact been used.

6. **Matt Leifer**
April 10, 2009

Chris W.,

As far as I am aware Nielsen has not taken another job, but has decided to focus on writing at the moment. See his blog http://michaelnielsen.org for more news.

7. **anon.**
April 10, 2009

I can't say I know Nemanja well, but I don't see how any reasonable person could think he's dogmatic.

8. **Chris W.**
April 10, 2009

It does so in a way far more effective than the kind of PR materials such institutions usually hire professionals to produce.

There seems to be an inescapable dynamic at work in organizations, such that when they become prominent they become susceptible to a banal and paralyzing risk aversion—a stealthy form of risk-taking in itself.

From Helen Keller, as quoted by former NASA program manager Donna Shirley:

> "Security is mostly a superstition. It does not exist in nature nor do the children of men as a whole experience it. Avoiding danger is no safer in the long run than outright exposure. Life is either a daring adventure or nothing."

9. **Joey Ramone**
April 12, 2009

Boy oh boy does Lubos go on and on and on.

If it weren't for String Theory's ruse, which Lubos served for a short bit, would we even know who he is? He is definitely a case study of something–I'm not sure what yet–but something.

He reminds me of all those eighties bands that grabbed onto the passing fads of hairspray and makeup.

Then Kurt Cobain came along in his thriftstore sweaters, power chords, and simple production values and restored rock back to its proper roots, just as Woit has been retruning physics and philosophy to its natural, cordial roots. With Cobain and Woit, again one could see the humanity of the endeavor exalted over the corproate-state's machine, reminding us of the reasons we love physics, philosophy, and rock'n'roll music; as the truth sets us free.

I hang out with Cobain now and then, and while he has never heard of Lubos' blog, he sometimes plays the bongos with Feynman.

Here's Dee Dee Ramone covering Curt Cobain's Negative Creep, as sometimes we mentors become students, as our studnets become mentors: http://www.youtube.com/watch?v=MAabjXoiJhk

I wish that the String Theorists/antitheorists knew this–there is a time for everyone to step down and salute the new. But perhaps humility is harder for those yet waiting to succeed.

10. **mike**
April 12, 2009

"which led him (like most Ph.D.'s in theoretical physics) to need to find some sort of employment doing something else."

— Is it really that bad in theoretical physics? Gives new meaning to the term "is the end in sight for theoretical physics?".

11. **Peter Woit**
April 13, 2009

mike,

Yes, the job situation of theoretical physics PhDs is bad, and has been bad for a long time. There's nothing really new about this though, it has been the situation since about 1970…

12. **Jack Lothian**
April 13, 2009

True Peter, I left Physics in the early 70s because there were no jobs. Universities, junior colleges, etc were seeing massive drops in enrolment in the hard sciences, the aero & space sector was in a slump & we were between electronic innovation periods. It was a waste land for physics in particular & hard science in general. I remember when a recruiter showed up at our university & gave a presentation, it appeared every graduate student in physics attended and we were all looking for that job that did not seem to exist. When I left, I had the choice of a junior college or a long post-doc journey with no definite job at the end or a career change. Because I worked in Statistical Mechanics & most educated persons do not really know what that means, I got hired as an expert in statistics and eventually became a survey consultant. Physics was always my first love though & if I thought I had a real shot at doing it, I would have. My career change though did turn out to be a good second option.

Sadly my experience is that science & math get lip service but little real support in our education system & culture. Most middle class families that I know want their children to get good marks in math & science in order to get into medicine, law or business schools. Science is a poor second choice & hard to do as well, so most students and parents see it as a dead end for a bright kid. Almost anything else is preferable.

13. **Paul Frampton**
April 13, 2009

My knowledge of Burton and PI stems from two visits: one day in 2004 then four months in 2005. That seminar was (I suspect) the last in Waterloo's post office. I learned theoretical physics at PI. Howard had a PhD in physics, was not an active researcher, yet deserves credit for making almost everyone happy. I met Mike Lazaridis whose idea and resources underly PI. A little bird said he wanted editorial control of Howard's book which I look forward to reading having ordered it from Canada. Turok is co-inventor with Steinhardt of cyclic cosmology a big idea.

Strings Strike Back

April 14, 2009
Categories: This Week's Hype

The February AAAS press event (discussed here) designed to get out the word that the critics are wrong and string theory is making predictions about physics that are getting tested has finally made it to Slashdot, via an article in Science News by Tom Siegfried.

Siegfried has been making his living selling string theory hype since at least the mid-nineties when he wrote quite a few articles for the *Dallas Morning News* with titles like "Physicists sing praises of magical mystery theory". In 2000 he published The Bit and the Pendulum: From Quantum Computing to M-theory, which somehow manages to put together quantum computing, consciousness, and string/M-theory. His next book, in 2002, was Strange Matters: Undiscovered Ideas at the Frontiers of Space and Time, 300 pages of solid hype for problematic speculative ideas, with branes and superstrings playing a leading role. More recently, he has been at work hyping cosmic superstrings in the pages of *Science* magazine (see here) and trashing me and my book for claiming that string theory doesn't make predictions (see here).

Most of the Science News article actually gives a reasonably sensible description of the story of attempts to use string duals and holography to study strongly coupled systems in 3 and 4 dimensions. But in the concluding paragraphs this story is shanghaied into service in the string wars, in a section entitled "Strings strike back" which begins:

> In recent years it has become popular to criticize string theory as out of touch with reality. Popular books have been written by scientists, some prominent and others not so prominent, arguing that string theory makes no predictions that experiment can test, that its fundamental objects can't be observed, that physicists have wasted their time on an enterprise that isn't even scientific to begin with.
>
> Such arguments leave an impression of utter unfamiliarity with the history of science. In times past, the same kinds of aspersions were cast against quarks, neutrinos, even the very existence of atoms. Superstrings are in good company.

You see, some critics of string theory are such ignorant idiots that they question the existence of superstrings even though any student of history knows that they are no more problematic than quarks, neutrinos and atoms. And experiments at RHIC show that string theory does make predictions, ones that have been successfully tested by experiment….

Update: I just read through some of the comments by Slashdot readers. The level of hostility towards string theory and string theory hype is remarkable.

Update: Commenter Hendrik points to a new piece from New Scientist where they have helpfully gathered together in one place all the outrageous string theory hype that has appeared in their pages in recent years.

Comments

1. **Tom**
   April 14, 2009

   Hi Peter, if you generalized the last sentence down to "The level of hostility is remarkable.", it could be used to accurately describe the comments for ANY Slashdot story.

2. **Peter Woit**
   April 14, 2009

   Tom,

   Good point. The level of hostility and ignorance in Slashdot comments is often remarkable. What struck me here is how one-sided things are in this case. At least among the nerd population that reads and comments on Slashdot, string theory has a big PR problem….

3. **Aaron Bergman**
   April 14, 2009

   I would say that slashdot commenters being against something is generally a sign that it's on the right track.

4. **Anon**
   April 14, 2009

   I do not take sides in the "string wars" but I don't really like his following statement.

   "Such arguments leave an impression of utter unfamiliarity with the history of science. In times past, the same kinds of aspersions were cast against quarks, neutrinos, even the very existence of atoms. Superstrings are in good company."

   It smacks of confirmation bias. What about all the other theories that were correctly rejected?

5. **csrster**
   April 15, 2009

   Anon: yes, it's the old "they said Einstein was mad" argument. They also said my uncle Morty was mad – but he actually was mad.

6. **Dennis Towne**
   April 15, 2009

   Yes, the level of hostility toward string theory is rather high among technical minded people and skeptics; I myself have shown the light to a number of technical and less-than-technical people.

   The critical, deciding factor for most of these people is in fact that disconnect from reality, from experiment. It's one thing to have the standard model, which has predicted many things to ridiculous accuracy for decades; it's another to have an idea which has clearly predicted very little, if anything.

   We might not be physicists, but we're not stupid. We know how to glean the most likely truth from the things we see, and we know where to place credibility:

   1) The standard model is questioned, in that 'corner case' effects, near the edge of our testing regime, are not explained or predicted accurately. The vast bulk of data supports it. The few detractors of the standard model can only point to corner case failures. This would be considered 'highly credible'.

2) String theory, in general, predicts at best only ‘corner case’ effects, which are near or beyond the edge of our testing regime. The few strong detractors of string theory make legitimate points regarding its foundation, not its details; the vast bulk of string theory moderates agree with the detractors, but simply maintain that regardless, string theory appears the best path. This would be considered ‘less than highly credible’.

As a software engineer, the string theory process so far reeks to me of software overdesign; by trying to reach too far, they have doomed the project. Incremental, small, and beautiful changes are the norm in software. Perhaps this is why Lisi’s work attracted such attention.

7. **Chris Oakley**
April 15, 2009

> Incremental, small, and beautiful changes are the norm in software. Perhaps this is why Lisi’s work attracted such attention.

I disagree. The reason that Lisi’s work attracted so much attention is that Lee Smolin said it was awesome, and he has a lot of influence. It is very far from being minimal and, like string theory, any beauty evaporates once one tries to apply it to the real world (although in neither case has this been successful). The point in its favour is that it is less arcane that ST.

8. **Joey Ramone**
April 15, 2009

I remember when Siegfried tried to get on the Ramones tourbus for some inside scoops.

While the String Theory machine evolved (in an anthropic manner) to support and promote sycophantic, soulless, sellout, scienceless hypesters, we wanted to keep our rock’n’roll pure and clean. It spoke for itself. Hypesters could only damage our cred. We didn’t need to create careers for non-scientist hypesters who actually do a vast disservice to science while personally profiting by hyping antitheories which sap all the funding that ought be going to true scientists–to all the postdocs and grad students and young professors struggling out there.

The only time I met Sigfreid was when he hopped on Michio Kaku’s time machine to sneak on the set for the music video for “I Wanna Be Sedated.”

He’s dressed like a scientist/doctor (in a white coat) around 1:02 in the video and he’s bothering us. I almost called cut as it was creepy.

http://www.youtube.com/watch?v=8FxaJKm9sdI

Sometimes people would do anything just to touch us, and then there are those who would do anything just to touch a string theorist, or be liked by one. We generally kept them off our tour busses and out of our videos.

9. **ohwilleke**
April 15, 2009

Compared to commentary on newspaper and political blog sites, the commentary at slashdot is downright calm and rational. If the same story were posted at the Denver Post, you would surely see complaints in the comments about how science is a conspiracy of liberal immigrants, and is based upon the non-biblical belief that pi is equal to something other than three. The really clever ones might complain that the numeral system used represents a false pro-Islamic bias. The main flaw of both sides of that debate is a lack of evidence or sourcing which isn’t necessarily customary in Internet comments as opposed to academic writing.

Apart from Tom Siegfried, who isn’t the world’s best public face for string theory, string theorists have done a pretty poor job of popularization and P.R. in the past half a dozen years or so, despite being the dominant school of thought in academic theoretical fundamental physics. Given that almost all fundamental physics research is ultimately funded with taxpayer dollars one way or another, this is a very bad choice to be making by default. Quantum gravity advocates, in contrast, have done a decent job in that time frame of bridging the gap between expressing ideas in academic journals, and popularizing the gist of what is important about those articles to the educated lay public. The version of the story that can be told in half a dozen Science News sized articles matters a great deal to popular understanding, something that few physicists since Feynman have realy understood well — most people will never read the book length version, even if it is readable. Not enough people are writing stories that briefly summarize the most important half a dozen or so key experimental data points that make someone say “string theory, not something else” and most importantly experimental data that is suggestive of a resolution of the string theory v. LQG debate.

There are also serious semantic issues going on in both Siegfried’s article and the larger discussion. It is very hard to tell how narrowly or broadly string theory is being used as a term. What components of the theory are definition (>4 multi-dimensionality, perhaps), and what are not (e.g., ironically, strings themselves as opposed to mathematially similar fine structures, and both LQG and string theory both involve what could be called strings although no one familiar with the discussion would ever describe LQG as a subset of string theory). In other words, it is not always easy to distinguish which parts of the theory are “moving parts” and which are a common core — something that is complicated by the fact that versions since rejected remain out there like ill considered blog posts, for all and sundry to see and recirculate without realizing that they are out of date. When one refers to superstrings, does one mean a supersymmetric theory, or simply that these strings being the ultimate cause of everything are especially cool. “String theory” has become the “cloud computing” of theoretical physics.

One of the more insightful of the slashdot comments notes that there is not just one string theory, but instead many variations on the theme. As a result, new evidence sometimes merely sorts the stack of available sub-theories without ruling any overall approach out.

10. **Joey Ramone**
April 15, 2009

Yes ohwilleke, but should it really be all about PR?

You write, “Apart from Tom Siegfried, who isn’t the world’s best public face for string theory, string theorists have done a pretty poor job of popularization and P.R. in the past half a dozen years or so, despite being the dominant school of thought in academic theoretical fundamental physics. Given that almost all fundamental physics research is ultimately funded with taxpayer dollars one way or another, this is a very bad choice to be making by default. Quantum gravity advocates, in contrast, have done a decent job in that time frame of bridging the gap between expressing ideas in academic journals, and popularizing the gist of what is important about those articles to the educated lay public. ”

Instead of PR shouldn’t physicists concentrate on physics?

It seems that equations such as F=ma and E=mc^2 have a way of generating PR on their own.

We never used a PR firm–we just rocked out. And long after the Jonas Brothers and Britney Spears and Quantum Gravity PR specialists are forgotten, we will be remembered.

It is funny how physicists are seeking to imitate pop stars as opposed to physicists who advanced physics such as Feynman, Fermi, Dirac, Einstein, Bohr, and Boltzman, who would have been embarrassed to devote their precious time to pondering PR strategies for untsetable antitheories.

Life is short and if one pursues empty PR, one ends up with empty PR, which becomes more and more embarrassing for certain “physicists” as they age, causing them to engage in bizarre behavior, hyping nontheories to the press in most creative manners.

I would advise everyone to rock out for truth, and so would Feynman.

11. **Hendrik**
April 16, 2009

String theory strikes back on a new front: check the article in NewScientist and in particular the Witten interview. Seems he hasn’t read your book yet.

12. **luny**
April 16, 2009

Aside from everything else, what exactly is the prediction that string theory made about RHIC?
That viscosity over entropy density (eta/s) is 1/4 pi?

Well, this is not anymore a prediction (see, for example,
http://arxiv.org/abs/0812.2521): eta/s in theories with string duals can go to lower values to 1/4pi, perhaps all the way to 0 (or quantum mechanics could prevent this. But this was known way before string theory
(see,eg Phys.Rev.D31:53-62,1985).

That eta/s is “low” in a strongly coupled theory? Well, thats a pretty obvious point that transcends string theory.
It is cute that AdS/CFT reproduces many phenomena also observed in hydrodynamics, but there is NO AdS/CFT result that can be sensibly compared with data and used to make a prediction. NONE. Not one. If anyone disagrees, please give an example.

AdS/CFT is,currently, a very interesting conceptual exercise. Perhaps tomorrow someone WILL extract predictions relevant to heavy ion collisions out of it. But it hasnt happened yet. And to claim it has is dishonest Public Relations.

13. **Peter Woit**
April 16, 2009

For those who can’t get enough, and want to follow yet another battle in the string wars (inspired by the Witten interview Hendrik mentions), see this posting at Cosmic Variance.

http://blogs.discovermagazine.com/cosmicvariance/2009/04/09/string-wars-the-aftermath/

14. **Mitsis**
April 16, 2009

Peter,

I do not understand your position on “string duals” fully.
Is it not true that the string theory of AdS CFT is really the same as the string theory one would use for unification?
Certainly the compactification is very different (the boundary of AdS instead of our flat space etc.) but the kinematics etc. and the whole formalism is the same – no ?

So if AdS CFT turns out to work correctly it would be a good argument for string theory. Is this not true?

15. **Peter Woit**
April 16, 2009

Mitsis,

The string theory side of AdS/CFT gives you gravity in 5 dimensional AdS space, not four dimensional space. For this and many other reasons you can’t use it for unification. The 4d physics of the theory is supposed to be N=4 SYM (no gravity), this may be a useful approximation to QCD, but it’s not a unified theory.

If you believe in much much more general conjectures about gauge duals of string theories in different “string vacua”, then you could imagine that there are gauge theory duals of the kind of string theory used in unification. These would be 3d gauge theories, and looking for them is an active field of research. As far as I can tell though, if it is successful, all you will get is a different parametrization of the “Landscape”, an infinite number of complicated qfts, corresponding to the infinite number of complicated “string vacua”.

16. **Andrei**
April 16, 2009

String theory does not have a PR problem. On the contrary most of the media gobbles up any crazy thing someone like Witten cares to say. I think the problem with strings is that it is not new as some people misleadingly say. It’s been around for about 40 years. And in all this time it has proven itself to be nothing more then mental pollution. Besides there are much more interesting and much newer aproaches to quantum gravity based on superconductivity. I think it is time string theory was filed in the round file (garbage can) and other approaches given a chance.

17. **Dennis Towne**
April 16, 2009

Chris Oakley,

When I talk about Lisi’s work, yes it’s less arcane, but the real hook can be found in his TED talk about it. It sounds plausible, it seems reasonable, and what little he predicts (the missing particles) make sense. But most importantly, his presentation -looks- cool, with nice symmetric/geometric visuals that people immediately understand.

That it has some gaps and handwaving in the middle is irrelevant; it has everything it needs to sell well to the techie crowd. Only a solid “this doesn’t match reality, even though it looks cool” is likely to change that.

18. **Thomas Larsson**
April 16, 2009

“So if AdS CFT turns out to work correctly it would be a good argument for string theory. Is this not true?”

But does it work correctly? In a recent discussion here, I became aware of the paper arXiv:0806.0110v2. Therein, the following statements are proven:

1. AdS/CFT makes a prediction for some quantities c'/c and k'/k, eqn (5).

2. This prediction is compared to the exactly known values for the 3D O(n) model at n = infinity, eqns (28) and (30).

3. The values disagree. Perhaps not by so much, but they are not exactly right.

This may be expressed by saying that the d-dimensional O(n) model does not have a gravitational dual (an euphemism for "AdS/CFT screwed up"?), at least not in some neighborhood of n = infinity, d = 3, and hence not for generic n and d. There might be exceptional cases where a gravitational dual exist, e.g. the line d = 2, but generically it seems disproven by the above result. In particular, I find it unlikely that the 3D Ising, XY and Heisenberg models (n = 1, 2, 3) can be treated with AdS/CFT.

19. **DB**
April 16, 2009

I note that Witten expects any useful new insights in String Theory to come from a younger generation. Well at least he's realistic, unlike his wife. Theoretical physicists over the age of 45 rarely make significant fundamental contributions. Even the smartest are burnt out by then.

20. **Aaron Bergman**
April 16, 2009

this doesn't match reality, even though it looks cool

How about "it doesn't look cool when you look closely at it and it's not even a quantum theory"?

21. **Joey Ramone**
April 16, 2009

Trying to defend classic, epic physics at Discover's Cosmic Variance blog would be like trying to promote the Ramones at a Jonas Brothers concert. I wonder if the same parent company owns both the Jonas Bros. and Discover as well as Disney and Miley Cyrus?

22. **Joey Ramone**
April 16, 2009

DB writes, "I note that Witten expects any useful new insights in String Theory to come from a younger generation. Well at least he's realistic, unlike his wife. Theoretical physicists over the age of 45 rarely make significant fundamental contributions. Even the smartest are burnt out by then."

Well, no doubt that Witten's a genius, but what useful "old" insights into String Theory came from the older generation, as far testable physics goes?

Kudos to Witten for advancing mathematics, but it's kindof like Milli Vanilli saying that they expect any useful new insights into lip-syncing to come from the younger generation.

23. **Chris W.**
April 16, 2009

Joey, you can go back to being dead now, thank you.

24. **Marion Delgado**
April 17, 2009

What do the leading actual string theorists say when someone asks them if Tom Siegried's very strong claims are correct?

25. **Sumar Ongi**
April 17, 2009

Although not directly related to the topic of this post, there's been a recent development in the field of SM extensions that, I think, shouldn't go unnoticed.

In the family of models known as Little Higgs Models, the naturalness problem is solved by making the Higgs be a pseudo-Goldstone boson of a certain global symmetry group that is spontaneously broken above the electroweak scale, where a sector of the model becomes strongly coupled. In fact, the Higgs boson is the lightest of those pseudo-Goldstone bosons, and is especially and naturally light due to the particular implementation of spontaneous symmetry breaking adopted in these models, the so-called "collective" symmetry breaking.

The Higgs boson does not get quadratically divergent corrections to its mass because it is a pseudo-Goldstone, and it also remains light with respect to the global symmetry breaking scale (unlike the other pseudo Goldstones in the model) due to the collective symmetry breaking.

A very recent preprint shows that this collective breaking mechanism is unstable under renormalization. Thus, unless the top quark couplings are fine tuned in an unnatural way, the Higgs boson does receive quadratically divergent corrections. To quote the authors of the preprint "This defeats the purpose of introducing the model in the first place." The preprint goes on to show that this problem is generic, therefore affecting all Little Higgs models.

All in all, it seems that Little Higgs models have been ruled out (so to speak).

26. **Sumar Ongi**
April 17, 2009

Well, as expected, the link came out wrong. It should be,
http : //arxiv.org/abs/0904.1622

Quantum tunnelling of a new, third kind could finally put string theory to the test

April 20, 2009
Categories: This Week's Hype

The whole "finally, a way is found to test string theory" business is starting to become a complete joke. See the latest such nonsense:

Quantum tunnelling of a new, third kind could finally put string theory to the test

which is based on this preprint.

Note: I'll be traveling this week, first to Edinburgh, where a celebration of Sir Michael Atiyah's 80th birthday is going on, then stopping in Dublin on the way back to New York.

Update: As usual, Slashdot can be relied upon to promote the latest "predictions from string theory" hype.

Comments

1. **Felipe**
   April 20, 2009

   Dublin? Is there a talk planned?
   Are you going to be around Trinity College by any chance?
   Cheers.

2. **Peter Woit**
   April 20, 2009

   Felipe,

   No, the trip to Dublin is really just my vacation. I will be around Trinity meeting someone there Friday, might be at tea that afternoon…

3. **David G.**
   April 20, 2009

   Hi! Just bookmarked your blog. I picked up the book yesterday. Mainly looking for more info (I liked your posts and the links, along with the sidebar links). Esp. interested in book reviews and recommendations for my library and reading.

   Have a great trip to Eire! – David in Houston

4. **bz**
   April 21, 2009

   It's not quite fair to criticize this paper as a joke in testing string theory. In the preprint, the only time the word "string" comes out is in page 2:

   "tunneling of the 3rd kind can serve as a tool to search for physics beyond
   the standard model in the form of light minicharged particles. The latter arise naturally
   and consistently1 in many extensions of the standard model based on field and string
   theory"

5. **Marion Delgado**
   April 21, 2009

   bz: the paper is definitely not a joke, the hype might be.

   The remnant question is: Does this "third kind of tunneling" exist and would it bolster string theories?

   First, we KNOW from experience that no matter what, any result won't be taken as weakening, let alone falsifying, string/etc. theory. Partly because there's such a large variety of possible string theories.

   Second, we DON'T know whether discovering this conjectured tunneling would help bolster string theory generally, because it's unclear from the article to what extent string theories uniquely posit that.

6. **Coin**
   April 21, 2009

   So do I understand this "third way" tunneling paper correctly that they're not proposing a new effect, they're just deriving a previously unnoticed emergent behavior of old effects?

   I don't think I quite understand their remark on page 2 which bz quotes– how exactly is it that this effect could allow us to more easily detect previously undiscovered particles, as they claim?

7. **Engineer**
   April 22, 2009

   The author of the referenced paper also has another one about checking for anomalies in the coulomb field.
   The author does state that both field theories and string theory allow for mini-charged particles.

   I am for any tests that give null results, however I have to agree with Peter that there are a lot of recent discussions about angels on the head of a pin, including some over at Cosmic Variance.

http://arxiv.org/PS_cache/arxiv/pdf/0904/0904.1547v1.pdf

Atiyah-Singer String Index Theorem

April 21, 2009
Categories: This Week's Hype

Just made it to Edinburgh for the Atiyah conference. It seems that someone at a local newspaper really wants to get my goat. See the story headlined [World's Great Minds Gather to Celebrate Atiyah's Birthday].

Update: Will try and write more about the conference soon. At least one string theorist [argues] for the new name for the index theorem, on the grounds that it is used in string theory. When I get back to Columbia I think I'll tell my Calculus students about the Taylor string series….

Comments

1. **Experimentalist**
   April 21, 2009

   I'd not be too worried if I were you, the Herald hasn't much of a reputation for Science reporting.

   If you're at the public lecture at the RSE this evening drop by the Particle Physics for Scottish Schools display exhibits- I'll be demonstrating some basic particle physics to the public and I'd love to hear your take on our work! I'm very keen to strip the hype from HEP and show the public that it can be fascinating without the need for media sensationalism.

2. **Chris Oakley**
   April 21, 2009

   On the subject of goats, did you know that last March – for the first time – there was an [Oxford vs. Cambridge Goat Race], on the same day as the Boat Race?

3. **alex**
   April 21, 2009

   Sorry, I'm probably a bit slow today (maybe every day), but what do you object to in the article? They've added the word string to the Atiyah-Singer Index Theorem, but that's hardly disastrous. Or was it something else?

4. **Peter Woit**
   April 21, 2009

   alex,

   The Atiyah-Singer index theorem is one of the central results of 20th century mathematics, and probably my favorite theorem in mathematics. It has nothing to do with string theory. Clearly the theorem was renamed to annoy me. It's a plot I tell you.

   Experimentalist,

   Will try and stop by, but I may not get there in time….

5. **Dan**
   April 21, 2009

   Is alex being sarcastic? The troubling part is not the shoddy journalism but the natural question: "Where on earth would the journalist even get the idea that this is the name of the theorem?" The most likely answer is that the mistake was caused by too much string theory hype.

6. **Mitch Miller**
   April 21, 2009

   Calling it the Atiyah-Singer String index theorem is pretty crazy. Atiyah obviously has spent alot of time thinking about string theory (much of that thinking was very fruitful for math and physics/ST) but the index theorem is not part of that group.

7. **Cormac O Raifeartaigh**
   April 21, 2009

   A Freudian slip? my guess is that the journalist first wrote 'The Atiyah String Index Theorem', then corrected the omission of Singer without understanding his mistake!

8. **Bored Busybody**
   April 21, 2009

   The Herald's online editors have been notified of the gaffe. It will be interesting to see if they respond.

9. **Experimentalist**
   April 21, 2009

   Thanks for dropping by, Peter. Enjoy the rest of your time in Edinburgh!

10. **M**
    April 22, 2009

    About string jokes, there is today a new string paper in the string hep-th arXiv (0904.3101) with a string abstract where they claim a string prediction for the amount of string CP violation. Reading the paper, one can see that the string Harvard authors just assume a qualitative string texture for the string Yukawa matrices of the string quarks and of the string leptons to estimate the string Jarlskog invariant.

11. **alex**
    April 23, 2009

Dan

I wasn't being sarcastic, I think the article is quite good. In a short space it does a reasonable job of explaining who Atiyah, Singer, Witten and Higgs are and what they've done. It says that the LHC will be used to look for the Higgs boson. It doesn't say that the LHC will be used to look for evidence of string theory.
The mistake with the theorem's name is not a big deal, I imagine Cormac's guess is not far from the truth. To all but a vanishingly small fraction of humanity "Atiyah-Singer index theorem", "Atiyah-Singer string index theorem", "Atiyah-Singer string theorem index", "Atiyah string singer theorem" etc are all equivalent complicated sounding gobbeldy gook.
Peter's conspiracy theory amused me, but the harrumphing from others about "shoddy journalism" and a "gaffe" doesn't seem to be intended as humour. I think it is likely to convince any journalist reading it that you lot are going to flick peanuts whatever they do.

12. **Peter Woit**
April 23, 2009

alex,

The printed version of the article has a sidebar with the headline "How huge is this piece of string?" which repeats the name "Atiyah-Singer String Index Theorem" and describes its significance for math and physics as huge. This wasn't a typo, it was a misunderstanding based upon being subjected to some of the standard hype about string theory, mathematics and physics. I don't blame the author of the piece, the problem is the hypsters who are happy to spread this sort of misinformation.

13. **Thomas R Love**
April 23, 2009

Googling 'Atiyah-Singer String Index Theorem' yields only one site other than NEW (or references to NEW):

http://www.gabbr.com/blogs/2009/4/26979/Michael-Atiyah:-80th-birthday/

There is nothing on scholar.google.com

So this nonsense is not widespread.

14. **Benni**
April 23, 2009

By the way Peter, what do you think about the journal "progress of physics"?
http://www3.interscience.wiley.com/journal/109802073/issue

These days, that journal publishes almost only string phenomenology (but sometimes also articles of witten).

In the early view section
http://www3.interscience.wiley.com/journal/109802073/issue

there is now an article about String theory and the LHC by the journal Editor D. Lüst:
http://www3.interscience.wiley.com/journal/122304902/abstract

What do you think of it?

15. **Eric Habegger**
April 23, 2009

"It seems that someone at a local newspaper really wants to get my goat. "

Didn't know you had a goat. It seems very odd for a Columbia professor to have a goat. It must be a very attractive goat. What's his/her name? Is this some new fashion among the east coast professional elites to acquire goats?

16. **csrster**
April 24, 2009

"It seems that someone at a local newspaper really wants to get my goat. "

Don't worry Peter, I'm sure you're also getting their goat by referring to them as a local newspaper when they consider themselves a national newspaper. The best way to annoy them is to refer to them by their old name, The Glasgow Herald.

17. **Chris Oakley**
April 24, 2009

> Is this some new fashion among the east coast professional elites to acquire goats?

The latest round of grant cuts have put many academics in the New York area below the poverty line, and without the chickens, pigs and goats they keep they would probably not survive.

18. **Jeff Moreland**
April 24, 2009

I hope you don't tie your goat up with string.

19. **Coin**
April 24, 2009

I always assumed it would be difficult enough just to keep a dog in Manhattan.

20. **Eric Habegger**
April 24, 2009

The Scotch are big connosoires of goats from way back. A word to the wise: its generally not a good idea to bring your best goats to Scotland. They are known for trying to get it.

21. **Jack Lothian**
April 26, 2009

    Eric, my grandmother was very scottish & proud of it & one of her favourite phases to her grandchildren was “you are not scotch you fool, you drink scotch, you are a Scot”. Reading your remark, I could hear her again, accent & all.

22. **Chris Oakley**
April 27, 2009

    OK – since we are all in a pedantic frame of mind let me point out that the Scottish – or at least the ones that live in Scotland – never, ever drink “Scotch” although they do drink rather a lot of “Whisky”.

23. **Low Math, Meekly Interacting**
April 27, 2009

    No true Scotchman?

Atiyah80

April 27, 2009
Categories: Uncategorized

Last week I was in Edinburgh for a few days and managed to attend the last two days of the conference in honor of Sir Michael Atiyah's 80th birthday. Atiyah is now retired, but he was one of the dominant figures in mathematics during the second half of the twentieth century, as well as perhaps the person most responsible for bringing together mathematicians and physicists around issues of common interest in geometry and physics. His interactions with Witten played an important role in several major developments, including the whole idea of "topological quantum field theory". One major part of my mathematical education was spending quite a lot of time for a few years reading through Atiyah's collected papers. He is at all times a very lucid writer, with his expository writings quite marvelous and uniformly worth reading.

The biggest news at the conference was the announcement by Mike Hopkins of his solution (with Mike Hill and Doug Ravenel) of most of an old problem in topology that goes back to the sixties, known as the Kervaire invariant problem. Hopkins in his talk labeled the new theorem a "Doomsday Theorem", because it nearly finishes off the subject it deals with, by ruling out the existence of a certain class of possible interesting topological invariants in all the remaining open cases except one. I wasn't looking forward to trying to explain this here on the blog, since what is involved are issues in stable homotopy far beyond my expertise, so I was pleased to find this morning that others have beat me to it with explanations. The slides from his talk are here, John Baez has a posting here (including a comment from Hopkins here), and the news was spread to the ALGTOP mailing list here.

Another report of impressive progress on a problem was Simon Donaldson's talk on the problem of showing that a Fano manifold has a Kahler-Einstein metric if and only if it is stable. This is one of the big open problems in complex geometry, and Donaldson discussed the appropriate notion of stability and outlined a strategy for getting a proof. He is not claiming a proof, with significant work still to be done, but experts seem to believe that the goal is now within sight and the next few years will see a resolution of this problem.

Unfortunately I arrived too late at the conference to hear Witten's talk, the slides of which are available here. He is continuing his work of the past few years on Geometric Langlands. Dijkgraaf gave a nice talk reviewing a wide range of topics connected in one way or another with topological strings. Perhaps his slides will soon become available, but the topics covered were similar to those of his talks at UCSB last spring (see here). Vafa gave a rather clear explanation of his program to try and get particle physics out of local F-theory models. I'm not convinced this does more than reinterpret GUT extensions of the standard model using quite complicated constructions, but you can see for yourself here. He didn't talk about the crucial question of whether this approach makes distinctive predictions about supersymmetry breaking testable at the LHC.

One evening was devoted to a public program about the Higgs particle, with a panel discussion featuring Higgs himself. It was not clear to me how much got through to the public about the electroweak symmetry breaking issue and what we hope to learn at the LHC. As always, some of the public wanted to know about what string theory has to do with this question. Unfortunately they were not given the simple, accurate and easy to understand answer "nothing at all."

Update: The web-site for the conference is here, and conference organizer Andrew Ranicki has set up another web-site here for various materials associated with the conference.

Update: Videos of the talks at Atiyah80 are now available at the web-site linked to above.

Comments

1. **John**
   April 28, 2009

   Hi Peter,

   Very good post! Very useful too!

   Can you suggest the most important / relevant papers to read from his collected works?

   Regards.

2. **David Edwards**
   April 28, 2009

   In the spring of 1972 I was a post-doc at IAS. One day Mrs. Atiyah dropped by my house. I enthused about how deep and broad Michael's work was. Then, getting reflective, I said "But he hasn't done any mathematical physics". She immediately piped up "He will, he will!"

3. **former mathematician**
   April 28, 2009

   Hopkins's talk is already referenced in the Wikipedia article on the Kervaire invariant. I'm impressed!

4. **woit**
   April 28, 2009

   John,

   Here are some suggestions, taken from among the expository pieces. Most of the non-expository research articles are also quite readable, and include many classics. As an example, his paper with Bott on Yang-Mills in 2d is chock-full of remarkable ideas and results.

   Algebraic topology and elliptic operators (Comm. Pure. Appl Math XX (1967))

   Topology of elliptic operators (Proc. Symp. Pure. Math 16)

   The index of elliptic operators (1973 AMS Colloquium Lectures)

   Algebraic Topology of operators in Hilbert Space (LNM 103)

Classical Groups and classical differential operators on manifolds. (CIME Varenna 1975)

Geometry of Yang-Mills Fields (Pisa 1979)

The moment map in symplectic geometry (Durham Symposium 1984)

Anomalies and index theory (Lecture notes in physics 208)

Topological aspects of anomalies (Symposium on Anomalies, Geometry, Topology, 1984)

New invariants of 3 and 4 dimensional manifolds (Weyl symposium 1988)

The Geometry and Physics of Knots, CUP 1990.

5. **Successful Researcher**
April 28, 2009

Thanks for the post and the reading suggestions in the comments!

6. **John**
April 29, 2009

Thanks Peter!

Reading Atiyah is certainly no time spent unproductive! He's written seminal works on a whole eclectic body of math and physics topics.
Admittedly, I also take a more-than-passing interest in what he has researched upon and written on over the years, along with E. Witten (my interest/s lie in tandem to such (gargantuan! and needless-to-say awe-inspiring) figures).

Please do provide more such posts. Recommended readings are even more welcome.

Regards.

(PS: Slightly off-topic, but if you have a Facebook account I would like to add you to my friends list.)

7. **Daniel de França MTd2**
April 29, 2009

So, witten has been working on the Langlands program for 32 years. Nice to know that.

8. **Stevem**
April 29, 2009

Peter, was the conference held at the James Clerk Maxwell building on the Kings Buildings campus or somewhere else? Just curious.

9. **Felipe Zaldivar**
April 29, 2009

May I point out the recently published monograph: "Stable Homotopy around the Arf-Kervaire Invariant" by V. Snaith (Birkhauser Verlag, 2009) where we can find some history and background on the (non)-existence of framed manifolds of Arf-Kervaire invariant 1 in 2^n-2 dimensions, with the known exceptions?

Math goes quite fast, sometimes, as Mike Hopkins' breakthrough shows!

10. **Peter Woit**
April 29, 2009

Stevem,

The conference was held in a new building, the "Informatics Forum".

11. **Peter Woit**
April 29, 2009

Thanks Felipe,

For an interesting survey of the state of the Kervaire invariant problem before last week, see the introductory chapter of the Snaith book, which is available courtesy of Springer here

http://www.springerlink.com/content/t75067/front-matter.pdf

12. **Andrew Ranicki**
April 30, 2009

I am now maintaining an Atiyah80 home page http://www.maths.ed.ac.uk/~aar/atiyah80.htm
concerning the conference itself (including photos, cake, gift, wine label etc.) and subsequent mathematical developments.

13. **Peter Woit**
April 30, 2009

Andrew,

Thanks, I'll add that link to the main body of the posting. And thanks for all your work organizing the wonderful conference!

14. **Successful Researcher: How to Become One**
June 1, 2009

For those interested: Witten has recently posted a preprint based on his Atiyah 80 talk.

The Strangest Man

April 29, 2009
Categories: Book Reviews

When I was in Edinburgh I picked up a copy of Graham Farmelo's new biography of Dirac. It's entitled The Strangest Man: The Hidden Life of Paul Dirac, Quantum Genius, and is not yet available in the US. I read the book on the plane trip back to New York and very much enjoyed it. While I've read a large number of treatments of the history and personalities involved in the birth of quantum mechanics, this one is definitely the best in terms of detail and insight into the remarkable character of Paul Dirac. I gather that Farmelo had access to many of Dirac's personal papers, and he uses these well to provide a sensitive, in-depth portrait of a man who often is reduced to a bit of a caricature.

The book is less of a scientific biography than the other book about Dirac I know of, Helge Kragh's 1990 Dirac, A Scientific Biography, and emphasizes more the development of Dirac's personality and the story of his relations with others, especially with his father, his mother, and his wife (who was Wigner's sister). I learned quite a lot about Dirac that I'd never known before, including for instance the story of his work on the atomic bomb project during WWII.

Dirac is responsible for several of the great breakthroughs in 20th century physics. At the age of 23, while still a graduate student, he took Heisenberg's ideas and found the fundamental insight into what it means to "quantize" a classical mechanical system: functions on phase space become operators, with the Poisson bracket becoming the commutator. This remains at the basis of our understanding of quantum mechanics, and Dirac's textbook on the subject remains a rigorously clear explanation of the fundamental ideas of quantum theory. Two years later he found the correct relativistic generalization of the Schrodinger equation, the Dirac equation, which to this day is at the basis of our modern understanding of particle physics. This equation also turns out to play a fundamental role in mathematics, linking analysis, geometry and topology through the Atiyah-Singer index theorem. Around the same time, Dirac was one of the people responsible for developing quantum field theory and quantum electrodynamics, as well as coming up with an understanding of the role of magnetic monopoles in electromagnetism.

The period of Dirac's most impressive work was relatively short, ending around 1933. By 1937, the year he married, Farmelo reports Bohr's reaction to reading Dirac's latest paper (on the "large numbers hypothesis"):

> Look what happens to people when they get married

Farmelo discusses a bit the question of why Dirac never later achieved the same sort of success after the dramatic initial period of his career. There may be a variety of reasons: the open problems got a lot more difficult, marriage and celebrity changed the way he lived and work, the war intervened, etc. For the rest of his career, Dirac took the attitude that there was something fundamentally wrong with QFT, and this may be why he stopped making fundamental contributions to it. He believed that a different sort of dynamics was needed, one that would get rid of the problems of infinities. He never was happy with renormalization, either in the form used to do calculations in QED after the war, or the more sophisticated modern point of view of Wilson and the renormalization group.

Unfortunately, some of the later parts of Farmelo's book are marred by an attempt to enlist Dirac in the cause of string theory. This starts with the claim that Dirac's work on "strings" during the fifties should be seen as a precursor of present-day string theory. These "strings" occur in the context of QED and magnetic monopoles, where they are unphysical artifacts of a choice of gauge, and have very little to do with the modern-day interest in physical strings as a basis for a unified theory.

Farmelo sees string theory as a resolution of the problem of infinities that Dirac would have approved of:

> What would surely have impressed Dirac is that modern string theory has none of the infinities he abhorred.

I don't see any reason at all to believe that Dirac would have been impressed with the idea of resolving the problems of QFT that bothered him by replacing it with a 10-dimensional theory that, despite the endless hype, has its own consistency problems (its perturbation expansion diverges, just like that of QFTs, and, unlike QFT, a 4d non-perturbative theory remains unknown). String theory was around for at least a dozen years before Dirac's death, I'm sure he had heard about it, and there is no evidence he took any interest in the idea. Farmelo reports the reaction Pierre Ramond got from Dirac in 1983 when he tried to sell him on the idea of replacing 4d QFT with a higher-dimensional theory:

> So he asked Dirac directly whether it would be a good idea to explore high-dimensional field theories, like the ones he had presented in his lecture. Ramond braced himself for a long pause, but Dirac shot back with an emphatic 'No!' and stared anxiously into the distance

The book ends with long discussion of Dirac and string theory that I think is seriously misguided, but it does include a mention of the fact that many physicists are unconvinced by the idea of string theory unification. Veltman is quoted, and the last footnote in the book refers the reader to *Not Even Wrong*.

Dirac is famous among physicists for his views on the importance of the criterion of mathematical beauty in fundamental physical law, once writing:

> if one is working from the point of view of getting beauty in one's equation, and if one has really sound insights, one is on a sure line of progress.

Farmelo believes that Dirac "would have revelled in the mathematical beauty" of string theory, but this is based on an uncritical acceptance of the hype surrounding the question of the "beauty" of string theory. "String theory" is a huge subject, and one can point to some mathematically beautiful discoveries associated with it, but the attempts to use it to unify physics have led not to anything at all beautiful, but instead to the landscape and its monstrously complex and ugly constructions of "string vacua" that are supposed to give the Standard Model at low energies.

I very much share Dirac's belief that fundamental physics laws and mathematical beauty go hand in hand, seeing this as a lesson one learns both from history and from any sustained study of mathematics and physics and how the subjects are intertwined. As it become harder and harder to get experimental data relevant to the questions we want to answer, the guiding principle of pursuing mathematical beauty becomes more important. It's quite unfortunate that this kind of pursuit is becoming discredited by string theory, with its claims of seeing "mathematical beauty" when what is really there is mathematical ugliness and scientific failure.

Ignoring the last few pages, Farmelo's book is quite wonderful, by far the best thing written about Dirac as a person and scientist, and it's likely to remain so for quite a while. Definitely a recommended read for anyone interested in the history of the subject, or some insight into the personality of one of the greatest physicists of all time.

Comments

1. **Shantanu**
   April 29, 2009

   Peter, thanks for pointing to this. Also in this post, you forgot to
   mention Dirac's contributions to GR. There is a good exposition on that
   in here
   Does this book discuss Dirac's contributions to GR?

2. **Peter Woit**
   April 29, 2009

   Shantanu,

   Yes, the book does discuss a bit his contributions to GR. What I wrote is by no means intended as a comprehensive list. Dirac did significant work on many topics, but I think the general notion of quantization and the Dirac equation are the two that are both of greatest importance, and cases where the insight is purely his, no one else was doing the same thing around the same time.

3. **Chris Oakley**
   April 29, 2009

   I have to applaud Farmelo for having the courage to point out that Dirac was weird and probably mildly autistic – people normally just try to make excuses for his odd behaviour.

   I have only skimmed the book so far (tho' it is near the top of my reading list) and am disappointed to read here that the author has tried to enlist Dirac in the String Theory Roll of Honour. I do not blame him for that in itself (I am just as guilty as any in claiming "support" by the great man): it is just that I did not expect that Farmelo, as an adjunct physics professor not obviously attached to any String program, would have a horse in that particular race.

4. **Eugene Stefanovich**
   April 29, 2009

   I must disagree that Dirac slowed down after 1933. For me, his most precious (and, unfortunately, underappreciated) work is P. A. M. Dirac, "Forms of relativistic dynamics", Rev. Mod. Phys. 21 (1949), 392.

5. **Jules Moulin**
   April 29, 2009

   A strange story about Dirac's equation is told by Thomas Racey, who with Battey-Pratt deduced the Dirac equation from a slightly expanded version of Dirac's belt trick: see E.P.Battey-Pratt; T.J.Racey, Geometric model for fundamental particles, International Journal of Theoretical Physics, 19:437-475, 1980. They deduced all the details of Dirac's equation from a few simple topological ideas. They then sent a copy of the paper to Dirac, but unfortunately, Dirac never answered.

   Racey then turned to other research topics. Only a few scattered people are trying to revive interest in the approach.

   Jules

6. **Thomas R Love**
   April 29, 2009

   I thought some of Dirac's best work was his 1951 Letter to Nature:"Is there and Ether" and his idea that the velocities of the ether provide potentials. Thanks Jules, I've thought for a long time that "Geometric Models" one of the most important papers that never developed a following.

   Dirac presented his last talk at the 2nd New Orleans Conference on Quantum theory and Gravitation in 1983. I went up to him carrying his book on General Relativity and asked for an autograph. He refused saying "I don't do that".

7. **MathPhys**
   April 29, 2009

   Many scientists and artists are probably slightly autistic.

8. **Francois Vanderseypen**
   April 29, 2009

   Made me laugh: '…string theory as a resolution of the problem of infinities that Dirac would have approved of…'. I don't have the right to claim the opposite but it's fun to see how these days anything and everything is used to defend string theory. Like many, I reckon he would have liked the mathematical integrity/beauty of string theory but that doesn't make it a physical theory.
   Soon there will be a book about Boltzmann who anticipated string theory, a book about Wolfram's NKS which proves the statistical advantages of the landscape…?
   I love you blog, keep writing, you're my hero.

9. **cynic**
   April 29, 2009

   'As it become harder and harder to get experimental data relevant to the questions we want to answer, the guiding principle of pursuing mathematical beauty becomes more important. It's quite unfortunate that this kind of pursuit is becoming discredited by string theory, with its claims of seeing "mathematical beauty" when what is really there is mathematical ugliness and scientific failure.'

   In 1930 he wrote:

   'The only object of theoretical physics is to calculate results that can be compared with experiment.'

   – Dirac, *The Principles of Quantum Mechanics,* 1930, page 7.

   But he changed slightly in his later years and on 7 May 1963 Dirac actually told Thomas Kuhn during an interview:

   'It is more important to have beauty in one's equations, than to have them fit experiment.'

   – Dirac, 'The Evolution of the Physicist's Picture of Nature', Scientific American, May 1963, 208, 47

   Other guys stuck to their guns:

   '… nature has a simplicity and therefore a great beauty.'

   – Richard P. Feynman (*The Character of Physical law,* p. 173)

‘The beauty in the laws of physics is the fantastic simplicity that they have … What is the ultimate mathematical machinery behind it all? That’s surely the most beautiful of all.’

– John A. Wheeler (quoted by Paul Buckley and F. David Peat, *Glimpsing Reality,* 1971, p. 60)

‘If nature leads us to mathematical forms of great simplicity and beauty … we cannot help thinking they are true, that they reveal a genuine feature of nature.’

– Werner Heisenberg (page 2 here)

‘A theory is the more impressive the greater the simplicity of its premises is. The more different kinds of things it relates, and the more extended is its area of applicability.’

– Albert Einstein (in Paul Arthur Schilpp’s *Albert Einstein: Autobiographical Notes*, p. 31)

‘My work always tried to unite the true with the beautiful; but when I had to choose one or the other, I usually chose the beautiful.’

– Hermann Weyl (page 2 here)

10. **Lowell Brown**
April 29, 2009

I once asked Dirac how he invented the Dirac Equation. He said:

I found that a times sigma sub x plus b times sigma sub y plus c times sigma sub z — quantity squared — was a squared plus b squared plus c squared, and I marveled at the beauty of this result.

11. **Thomas Larsson**
April 30, 2009

The circle is the most beautiful geometrical shape. Hence planets must move in circles, or in circles around circles.

A misguided concept of beauty held progress back for 1500 years. Something to ponder when arguing for beauty.

12. **Chris Oakley**
April 30, 2009

Lowell,

This version of WordPress seems to allow you to write that in HTML, or the generalisation:

σiσj=δij+iεijkσk

I feel a piece about “the square root of the Klein-Gordon equation” coming on. But don’t worry – I’ll put it on my web site and not here.

13. **publius**
April 30, 2009

While it seems clear that Dirac was not seduced by the idea of extra dimensions, as shown by his answer to Ramond, and it also seems to be no evidence of interest from him on string theory during his last years, he was certainly one of the firt researchers (if no the first) to consider fundamental relativistic extended objects in physics, introducing a relativistic brane, motivated partly by the hope to get rid of just those troubling infinities in particle theory. I believe the reference is
Proc. R. Soc. London A 268(1962)57
This reference is quite more pertinent than Dirac’s string to make a connection between Dirac and string theory (which possibly Dirac himself would have rejected…) It would be surprising if the author of the book does not mention it.

14. **Chris Oakley**
April 30, 2009

Forget that – it did the _… in the preview, but not in the final result.

15. **Peter Woit**
April 30, 2009

publius,

Farmelo does mention the 1962 paper in this context. In it Dirac was trying to interpret the electron as a charged conducting surface, with the muon an excited state. The connection to current ideas about branes seems rather slim….

16. **none**
April 30, 2009

The Dirac-Born-Infeld action, a nonlinear modification of the Maxwell action, shows up in string theory as part of the effective action for a D-brane.

17. **Carl**
May 1, 2009

Thomas,

the smarter physicists used “beauty” in physics to mean “simplicity” (see the citations above) which is the real thing to achieve. Beauty is maybe misguided, simplicity is not. Over the years, the description of physics has become simpler and simpler. Maybe also more beautiful, but most of all, simpler.

18. **dir**
May 1, 2009

mentioning infinities in qft, i once read a small book by yukawa who also tried to use extended objects to replace point-particles. maybe this trend had an influence to japanese theoretical physicists, including nambu?

19. **davetweed**
May 1, 2009

With regards to Chris Oakley's comments, it is rather strange that people insist on ascribing medical conditions to people where they cannot possibly know enough details to make a meaningful medical diagnosis, rejecting the idea that many people can be weird without some having some medical condition to explain it. (Autism is a relatively precise medical diagnosis, and someone not acting with the general societal level of gregariousness and following of social conventions doesn't automatically imply they have some degree of autism.)

Note that I'm not arguing either that Dirac would or wouldn't have been diagnosed as autistic by a specialist had he been examined by one, but rather about the cavalier "well those anecdotes sound vaguely like they fit with autism, so he must have been mildly autistic" approach.

20. **D R Lunsford**
May 1, 2009

I have to disagree as well that Dirac was spent in the 30s. "Forms.." as mentioned is an extremely important work. His book on GR remains the most concise exposition imaginable. His attempts to rid classical EM of inconsistencies were heroically motivated and fascinating to follow. The LNH remains an extraordinary idea. He was the first person to rigorously explore anti-deSitter space (early 60s) ("A Remarkable Representation of the 3+2 deSitter Group"). Very late in life he was working on infinite dimensional representations of the Lorentz group. Like Beethoven, he knew only one direction, forward.

I agree that connecting the "Dirac string" as it came to be called with string theory, is analogous to connecting General Relativity with General DeGaulle.

-drl

21. **D R Lunsford**
May 1, 2009

T R Love,

This work behind this article in Nature was no doubt his "New Classical Theory of Electrons" which appeared in the Royal Society Proceedings in three papers – early 50s. Basically he posited an equation of state such that the current was proportional to the potential,

J = -k A

so one has a sort of superconducting vacuum in the London sense. It's an extremely interesting series if only as a demonstration of how to think physically, and how intractable is the problem of the electric charge.

-drl

22. **Thomas Larsson**
May 1, 2009

Carl,

A circle is simpler than an ellipse. Thus Ptolemy was right. And since we remember him after 2000 years, I bet he was really smart, too 🙂

The problem with simplicity or beauty is that it lies in the eyes of the beholder. Beauty is an important motivation and inspiration, but eventually it must be up to experiments to decide whether Nature agrees with your subjective notion of beauty. E.g., it really doesn't matter how fearful a symmetry that SUSY might be; if the Tevatron finds no light Higgs and the LHC finds no sparticles, SUSY is dead nontheless.

One of my favorite models is the CFT with c = 1/2, which describes the 2D Ising model at criticality. It consists of three fields, belonging to three separate Virasoro representations (with h = 0, 1/16 and 1/2). This model is thus not unified in the sense that all fields belong to the same multiplet of the relevant symmetry, but it is unified in the weaker sense that the representations are connected by fusion rules. If this is the way that things will work out in Nature (and it does for the 2D Ising model), then the idea of GUTS (putting everything in the same multiplet) may be fundamentally misguided. And so may SUSY, since fusion rules of a purely bosonic symmetry can connect both bosonic and fermionic fields.

23. **Greg Sivco**
May 1, 2009

Dirac was the way he was because of an extremely strict father. Read the Wiki entry and it explains that precisely. I look forward to reading the book, P.A.M. Dirac is one of our all-time favorites. The list of his accomplishments (right hand side, Wiki again) is so amazingly large. Is there anyone who did more? I often debate with myself if either he or Max Born loved Mathematics more. Shrug, I'll never know, but they're up there amongst the Early Titans of 20th Century Physics.

Thank you, Peter for your excellent review, we have been looking forward to this book for a long time, and I will most assuredly skip the last "string section" on purpose. That New Scientist article is depressing enough … I see no reason to add to the masochism. 🙂

24. **Carl**
May 3, 2009

Thomas,

beauty might lie in the eye of the beholder, simplicity does not.
The inverse square law is simpler than epicircles.

A single QED Feynman diagram is simpler than Maxwell's equations.

The issue now is how to continue this…

Carl

25. **Georges Melki**
September 20, 2009

I would like to add to the list of quotes by Cynic regarding the importance Dirac assigned to the "beauty" of a physical theory. My quote is taken from a very good book :"

Paul Dirac-The Man and his Work", Peter Goddard,Ed.(Cambridge UP), page 89:
"In 1977, he explained his attitude in a particularly vivid way when describing his affinity with Erwin Schrœdinger(with whom he shared the Nobel Prize:
…Schrœdinger and I both had a very strong appreciation of mathematical beauty, and this appreciation of mathematical beauty dominated all our work.It was a sort of act of faith with us that any equations which describe fundamental laws of Nature must have great mathematical beauty in them. It was like a religion with us. It was a very profitable religion to hold, and can be considered the basis of all our success.
It has to be admitted that in most hands and at some times in the development of science this approach can be dangerous."
I believe this to be as clear and concise as possible…
On the other hand Peter, I prefer your review of the book to that of the New York Times!

The Only Game in Town

May 1, 2009
Categories: Multiverse Mania, This Week's Hype

This week's New Scientist has an article promoting the string theory multiverse, starting off with positive comments from Brian Greene, and continuing with a claim that the majority of physicists now embrace the idea:

> Greene's transformation is emblematic of a profound change among the majority of physicists. Until recently, many were reluctant to accept this idea of the "multiverse", or were even belligerent towards it. However, recent progress in both cosmology and string theory is bringing about a major shift in thinking. Gone is the grudging acceptance or outright loathing of the multiverse. Instead, physicists are starting to look at ways of working with it, and maybe even trying to prove its existence.

In his promotional book on the subject, Susskind is able to come up with exactly one bit of information that the string theory multiverse hypothesis provides, a prediction of the sign of the spatial curvature of the universe (others don't think that even this bit is there, see this by Steve Hsu). The New Scientist article ends:

> …says Susskind. "If it turns out to be positively curved, we'd be very confused. That would be a setback for these ideas, no question about it."
>
> Until any such setback the smart money will remain with the multiverse and string theory. "It has the best chance of anything we know to be right," Weinberg says of string theory. "There's an old joke about a gambler playing a game of poker," he adds. "His friend says, 'Don't you know this game is crooked, and you are bound to lose?' The gambler says, 'Yes, but what can I do, it's the only game in town.' We don't know if we are bound to lose, but even if we suspect we may, it is the only game in town."

The arguments for string theory have evolved over the years, with the "it's the only game in town" one being made starting fairly early on. Weinberg seems to be willing to go for a new variant of this, that not only is it the only game in town, but it's probably crooked (i.e. can't possibly work, is obvious pseudo-science…), and this doesn't matter, one should continue anyway.

It has become increasingly clear to me in recent years that there is a large cohort of people who have so much invested in string theory that they will never, ever give up on the idea of string theory unification, no matter how clear it becomes that the game is crooked and not legitimate science. They will be active and with us for a long time, but the idea that there has been "recent progress in both cosmology and string theory … bringing about a major shift in thinking", causing the majority of physicists to sign on to this is nonsense. Quite the opposite is true, with the increasingly obvious problems with string theory causing non-string theorists to shun the subject and avoid hiring anyone who works on it.

The New Scientist article is also available here, and if you want more recent multiverse promotional material, there's this. Finally, a panel discussion on this was held at the Origins symposium at ASU recently, and is now available on-line.

Update: The torrent of string theory hype seems to continue unabated, with claims that the Planck satellite will tell us something about string theory (see here):

> The results could also offer insights into the much vaunted string theory – science's big hope for a unified theory of everything. The idea involves a complex 11-dimensional universe, with seven 'hidden' dimensions on top of the four observable dimensions of space and time.
>
> Professor Efstathiou said: "The potential for fundamental new discoveries that will change our understanding of physics is very important and that is what I'm really hoping for with Planck.
>
> "We might find signatures of pre-Big Bang physics. We might find evidence of cosmic defects – superstrings in the sky.
>
> "Unravelling the physical information may tell us something about the warped geometry of the hidden dimensions."

Comments

1. **Lucy in the Sky**
   May 1, 2009

   Oh my God… You are really a sick puppy. Vitriolic, unfounded and really pathetic banter. Do us (= scientists) a favor and offer us when rejecting (whatever you do not like and that's your perfect right) a valid – or at least interesting – alternative. This blog is really becoming the prime example of the emperor with(out) his clothes. PLEASE: publish a scientific paper and BUILD instead of destroying…

2. **DB**
   May 1, 2009

   Examine the psychology of these people who are doggedly promoting string theory. They are desperate. The vast majority are over 45, the age beyond which it is extremely rare for a mathematical physicist to make significant contributions. They have devoted most of their professional careers to string theory. They need a new generation to continue the quest or they will have wasted their research lives.
   Egotism, the refusal to face the consequences of their folly, and the terrifying realization that they will never be more than footnotes to science unless the next generation can make the breakthroughs building on their work is what drives them on. Worse, the failure of the past thirty years risks being held up as a model of how not to do theoretical physics. So they have to succeed.
   The fact that they are sucking young talent into the same futile exercise is of no concern to them.
   It's clearly a form of fanaticism. Almost religious in nature.
   Weinberg is only lukewarm, his place in history is secure.

3. **Peter Woit**
   May 1, 2009

   LSD,

   Sometimes I do think of giving up on trying to do anything about the endless hype and pseudo-science going on under the banner of "string theory", mainly because it seems hopeless and there is no way to have an effect on it.

   Then, I hear from people like you that I've managed to really upset, who seem to feel that I have great destructive powers, and I get the strength to go on. Thanks.

4. **Jo**
   May 1, 2009

   "Sometimes I do think of giving up on trying to do anything about the endless hype…."

   Peter, One thing you could do is encourage people to actively seek alternatives (or help find one yourself).

5. **Peter Woit**
   May 1, 2009

   Jo,

   I do work on alternatives myself, spending far more time on that kind of work than on the blog. The whole point of the hype campaign is to convince people that they should not seek alternatives, that string theory is the "only game in town".

   That Weinberg is admitting that the game is probably crooked and unwinnable seems newsworthy, and pointing people to that news seems like a good way to encourage them to look for alternatives.

6. **Lucy in the Sky**
   May 1, 2009

   "I do work on alternatives myself, spending far more time on that kind of work than on the blog. " I am impressed by this 15+ years of time&efforts and the results which emerged from it… http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=find+find+a+p.+woit .

7. **Peter Woit**
   May 1, 2009

   Thanks again LSD. Responding to criticism with anonymous personal attacks does a good job of convincing people that proponents of this particular game have neither a scientific case nor the guts to stand up publicly for it. Your destructive power is far greater than mine….

8. **Successful Researcher**
   May 1, 2009

   Is there an interview or something where Weinberg is more verbal on the string theory and landscape? In the NS article he is just quoted to say "We just can't make a list of 10^500 things. That's more than the number of atoms in the observable universe.", and I fail to read much of an attitude out of this…

9. **Peter Woit**
   May 1, 2009

   Just remembered that "Lucy" had been here before, leading to a similar exchange. He or she really doesn't like it when I say critical things about multiverse research.

   http://www.math.columbia.edu/~woit/wordpress/?p=717

10. **Peter Woit**
    May 1, 2009

    SR,

    For Weinberg's views on the multiverse, probably best to read this

    http://arxiv.org/abs/hep-th/0511037

    Recall that he's generally the one credited with pointing out that the size of the cosmological constant could be understood by an anthropic argument. I suspect that, like Gross and many others, to some degree he recognizes how problematic the whole string landscape subject is, but one naturally tends to think more highly of one's children than is reasonable…

11. **Eric Habegger**
    May 1, 2009

    Regarding the endless hype of string theory, it seems to me that much unwarranted help is being enlisted by periodicals such as New Scientist. I subscribed to it for about a year but realized quickly that my interest in physics was not being at all well served by it. I had to actually get my credit card replaced to end their tireless automatic resubscription.

    It seems to me a good plan to hit string theory where it hurts is to stop subscriptions to pseudoscience periodicals such as New Scientist. That may hit string theory harder than large amounts of well reasoned arguments against it.

12. **Peter Woit**
    May 1, 2009

    Eric,

    I don't think the problem is New Scientist. They're generally reporting reasonably accurately what physicists, often prominent ones, say to them.

13. **coolstar**
    May 1, 2009

    Very timely post Peter as I just NOW read on the Bad Astronomy blog how the Planck mission will test string theories of cosmology.

14. **Peter Woit**
    May 1, 2009

    coolstar,

    Looking at that blog, it seems to me that Phil Plait avoids making the claims about string theory that you state. Perhaps Planck will see evidence for gravitational waves in the CMB, that would tell us something new about the Big Bang, and that's what he's excited about. Wouldn't tell us whether strings have anything to do with anything though….

15. **Observer**
May 1, 2009

Keep up the good work Peter, thanks for let us know why string theory does not work. It is apalling how some called "(= scientists)" can't see the objective of your blog

16. **David G**
May 1, 2009

Thanks Peter. It's spotlighting the hype which makes an interested non-scientist like myself want to delve deeper into the alternative material, which this post does very well.

17. **Shantanu**
May 1, 2009

Peter ,see this nice talk by Krauss on inflating , string theory etc.
he points out how one can/may never test inflation with CMB experiments.
http://www.stsci.edu/institute/itsd/information/streaming/archive/STScIScienceColloquiaSpring2009/

18. **Pawl**
May 1, 2009

Lucy, Jo,

Your criticism of Peter's publication record rather than his arguments only shows how little you have to offer in support of the "multiverse." (It's also not a self-consistent position, as you don't offer your own publication records to be examined.)

The argument you make, which seems to be popular among string theorists, is that one should "build, not destroy." This is not science. Poor ideas should be criticized. Proponents of ideas should be frank about their limitations — and not encourage hype, whcih was the whole point of Peter's post. Some ideas are so poor that it is really better to give up on them and work on something else.

In fact, what Peter reported seems to go well beyond hype and into a level of serious misrepresentation — New Scientist suggesting, presumably on the basis of their interviewees' comments, that most physicists accept the multiverse.

It's hard to see Peter's points there as objectionable at all.

19. **Mark Stuckey**
May 2, 2009

Peter,
I am glad you're doing this work and I believe it's valuable. I've heard fm more than one researcher who believed stupor string theory was draining talent and thwarting progress in physics, e.g., "Is string theory a futile exercise as physics, as I believe it to be? … The sad thing is that, … other avenues are not being explored by the bright, imaginative young people, and that alternative career paths are blocked." P.W. Anderson, NY Times, 4 Jan 05. So, I believe it's important that someone takes the time to communicate flaws in SST.

As for your own attempt at an alternative to SST unification, what are you working on? I see you had pubs in lattice gauge theory, are you working on a discrete method now? Maybe this isn't the place to discuss it based on the warning that "this is not a place for people to promote their favorite ideas about fundamental physics." If so, please email me your answer. Thanks.

20. **Peter Woit**
May 2, 2009

Mark,

No, I haven't worked on lattice gauge theory in a long time, and I don't have any particular ideas about discretization as helping with fundamental issues.

Recently I've been working on a variant of the BRST approach to handling gauge symmetry, using something mathematicians are calling "Dirac Cohomology". Still sorting out some things about this that confuse me. Now that classes are almost over, I hope to devote the summer break to finishing something I'm writing on the subject.

21. **coolstar**
May 2, 2009

Peter: perhaps you're correct about Bad Astronomy, Planck, and string theory but on a second and third reading, and on following the links, I'll still have to disagree. I've obviously been wrong before, but less than average about that particular blog. And of course, what you've said about gravitational waves and Planck is correct (I certainly didn't mean to imply that Planck wasn't a worthwhile mission).

22. **Joey Ramone**
May 4, 2009

Who is the greater musician–he who wins American Idol and sells a million records or he who plays from the heart, come hell or high water, whether there are three people in the audience or 30,000?

Who is the greater physicist–he who publishes millions of indecipherable, fundaemnatlly-erroneous and intenionally- misleading papers in meaningless, groupthink journals, or he who speaks the simple, honest truth regarding physics and physical reality?

Truly, many "scientific" journals have been hijacked by the same financial "geniuses" who brought us the decline of the economy, giving a bad name to mathematicians, physicists, and economists.

23. **hmm...**
May 4, 2009

`Joey Ramone', you seem to be having trouble maintaining a consistent character.

24. **Andrei**
May 5, 2009

One can only hope that the new minds are steering clear of string theory. In the beggining the string people were all confident (read arogant) about how their theory will explain everything. Now they are desperately clinging at straws. Looking for any kind of confirmation even marginal ones. This is a good sign. String theory is on its death bed. The question is how long will it linger before someone gives it the final dose of painkiller. The idea that Planck will confirm some aspects of string theory is absolutely pathetic. The final nail in the string's coffin must come from alternative theories. Otherwise all those tenured types with their lives vested in it will continue to push it forward. And their task is not hard because by definition string theory is impossible to disprove. So I agree that this blog should also describe and discuss alternate theories.

25. **Joey Ramone**
May 5, 2009

Hmmmm writes, "`Joey Ramone', you seem to be having trouble maintaining a consistent character."

Hey Hmmm–you try fathering/furthering/defininining the American punk rock/alt.-rock movement, never selling out, getting inducted into the Rock'n'Roll Hall of Fame, and then taking up physics in the afterlife.

It are all the string theorists who have trouble maintaining a consistent character, as they have displaced and suppressed their own characters and curiosities to serve the greater goals of the group–never has physics, like rock'n'roll, been furthered by corporate-state groupthink. The String Theorists are always waiting for their marching orders from their antitheory, physicsless elders, who command them to show their loyalty by anonymously attacking Peter Woit's blog, which makes for good entertainment. Imagine if Einstein or the Ramones had had to take dictation from seventy-year-olds going on and on about the Anthropic Principle and Multiverses–the very opposite of physics–when there were in their twenties!

Imagine if Lenny Susskind or Michio Kaku were in charge of writing the Ramones' lyrics! Brian Greene wouldn't be so bad–he wears cool black leather jackets in his music video for The Elegant Universe. If he grew his hair out he could definitely audition to be a roadie.

26. **Thomas R Love**
May 5, 2009

The best thing NEW did was to steer new graduate students away from string theory into other fields where they might actually do some real physics.

Peter, are you going to write a sequel?

27. **Peter Woit**
May 5, 2009

Thomas,

No plans for a sequel. For one thing, little has changed since 2002, when most of the book was written. There would have to be something new to write about for me to get interested in thinking about a sequel. Maybe if LHC results change things dramatically.

Someday I would like to write a technical book on representation theory, QM and QFT, but that project is also a long ways off right now.

28. **Tim**
May 6, 2009

Hello Peter,
sorry, my comment is obviously a little bit off topic: String theorists tend to claim that "string theory contains all of general relativity". As far as I understand this, this statement reduces to "string theory contains massless spin-2-particles". It seems to me that the impression that this implies all of general relativity contains can be traced back to the Lectures on gravitation by Feynman who showed that one can indeed derive the field equations from this fact. But is general relativity not much more than just the field equations? Can a spacetime containing a black hole even be generated by gravitons? This seems to be one of the key arguments of the LQG-people, but has this question even been asked by some string theorists?
On the other hand do the field equations of general relativity enforce consistency of the energy-stress-tensor and the spacetime geometry, meaning the energy-stress-tensor generates spacetime curvature which back reacts on matter = energy etc. To postulate some spacetime and putting some strings in it that may or may not generate gravitons seems to completly ignore this point.
I would be very grateful to any link or hint to further discussions of these points,
with kind regards,
Tim (interested layman)
P.S.: Could it be that string theory is mostly popular in the US has to do with the fact that Feynman did not publish his lectures, but that they were passed around from hand to hand?

29. **Peter Woit**
May 6, 2009

Tim,

I don't think the availability of Feynman's lectures had anything to do with it, the argument was well known and I believe repeated in many sources. The sociology of particle theory in the US has always been more trendy than in other countries, with a big emphasis on hiring people working on the "cutting edge". Actually, at the moment, I think this trendiness is cutting against string theory in the US, as the perception has grown that string theory is dormant, with cosmology and phenomenology the "cutting edge". String theory seems to me to be doing better in other countries, where there is more willingness to hire people in field perceived as "prestigious", even if not currently very successful.

String theorists certainly do worry about the question you ask, and there have been extensive debates over the issue of "background dependence" in string theory here and elsewhere. I don't want to get those started again, unless some commenters willing and able to provide a well-informed, balanced discussion free of hype and wishful thinking magically appear.

Perhaps the main reason I have little interest in arguing these issues is that if you can't use the criterion of solid mathematical consistency or that of agreement with experiment, you can't ever be shown to be wrong, So you may be doomed to a future of arguing over who has the "best" not truly consistent quantum gravity theory that can't be tested, with no objective way to decide. I'll leave this to others.

30. **Aaron Bergman**
May 6, 2009

In addition to containing gravitons, it is possible to demonstrate that string perturbation theory makes sense precisely when the background spacetime satisfies Einstein's field equation. The gravitons are the perturbations around the background.

31. **Michael T.**
May 6, 2009

Thanks Peter for the link to the Origins Conference at ASU. I did find Sheldon Glashow's comments most interesting. What I got from it was that the cancellation of the Texas collider created a 17 year pause in experimental physics. With no real data to construct new theories it seems that the imaginations of some very bright people over the years got a bit carried away which eventually led to untestable notions like string "theory". If the devil truly does find work for idle hands then with a nod to Occam it is the most simple and most human explanation of why ST has garnered so much attention.

32. **Paul James**
May 7, 2009

The claim of "only game in town" may be exaggerated. String theory uses, as Witten says, 4 ideas: supersymmetry, higher dimensions, duality, and stringiness. If one leaves out the first two or three, which have little counterpart in experiment, another game in town pops up: to produce a unified model based on stringiness alone, but in three spatial dimensions.

I would be ready to take a long bet that this game has better chances than any one that uses 11 spatial dimensions.

33. **Tim**
May 7, 2009

Hi Peter,
thanks for your answer, I was kompletly unaware that the thing with the gravitons is common sense,
I looked at severel sources and found no reference I could follow, e.g. wikipedia or several
books on string theory. Mr. Kaku in his two books on string and M-theory e.g. does not mention
the origin of his starting point at all (I mean the axioms that any quantum theory of gravity
needs to live in Minkowskian spacetime and contain massless spin two particles aka gravitons).

Of cource since I am not affiliated with any physics department I am a little out of the loop 🙂

It is completly understandable that you do not like to repeat any extensive discussions that
obviously did take place, but one wonders if it would be possible to give a little overview over
the conclusions if there were any, or over the arguments of both sides (or a link to that), for someone who was not involved.
All I did came up with is that "background independence" for string theory seems to mean that one can choose
any spacetime one likes to start with, while the LQG idea is that a spacetime is a classical state
and a quantum state should be a wave function living on some space where each "point" is a
classical spacetime that is a valid solution of general relativity.

If this post happens to fuel any hostility on your blog please accept my apologies.

34. **christina philosina**
May 8, 2009

Hello Peter,
In fairness to Steven Weinberg, I believe you completely misinterpreted his little joke about the poker game, and more importantly, his attitude towards string theory. If one reads the New Scientist article, the correct interpretation becomes obvious. It's not, as you say, that he admits string theory is 'crooked and you are bound to lose' and that means he thinks string theory 'can't possibly, work, is obvious pseudoscience'. Rather, he feels that physicists can't win in the sense that it may well be that the fundamental values of nature cannot ever be predicted by theory because they are randomly determined in each universe of the multiverse. THAT is regarded by physicists as a 'crooked' game because for at least a century they operated on the assumption that a 'fair' universe would be one whose values had to be what they were, and it was the job of physicists to create a theory that predicted those values. It pains and sometimes infuriates them to think that that glorious undertaking may be doomed to failure. And think about it Peter—why on earth would Steven Weinberg embrace a theory he considered obvious pseudoscience?

Christina P

35. **>2**
May 9, 2009

christina philosina, are you saying that Steven Weinberg doesn't recognize a false dichotomy for what it is? I find this hard to believe.

36. **christina philosina**
May 9, 2009

>2, your comment mystifies me.
What is the 'false dichotomy'? There's a real dichotomy: 1)We live in the only universe there is, whose mathematical values for the forces and constituents of nature MAY all be derivable from first principles or 2)we live in one of many (or an infinite number) of universes, whose fundamental values are randomly affixed.
Until the strong version of the anthropic principle (implying the existence of a 'fine-tuning' God) led atheistic physicists to propose a multiverse as a rebuttal in the 1970's, no one ever considered the notion of other universes. The only issue was: could all quantitative aspects of the (one and only) universe be deduced? Physicists fervently hoped so, since only then could their theories have a sense of 'completeness'. Steven Weinberg certainly shared this craving, but now he is reluctantly accepting the notion that a number of lines of argument (string theory's 10^500 solutions is just one) suggest the unfortunate truth may very well be that we live in a multiverse. If so, any Theory of Everything will have to have deeply irritating arbitrary elements, determinable only by experiment. 'The only game in town' would then be 'crooked.'

37. **>2**
May 9, 2009

christina philosina you forgot 3), 4), 5), …

38. **christina philosina**
May 9, 2009

>2:
What third (or fourth—or fifth!!) option do you have in mind that is scientifically or philosophically relevant to this discussion? Or do you always just reflexively reject

any Either/Or as too simplistic? While I admire the almost poetic brevity of your comment, there IS such a thing as being TOO succinct.
And I notice that Peter has been more than succinct, he's been utterly silent in the face of my having pointed out his error in interpreting Steven Weinberg's poker game joke as indicating that Weinberg believes that string theory is pseudoscience. Hmmm.

39. **Pawl**
May 9, 2009

Christina,

Having read the NS article myself, it seems to me that Peter has a direct reading of it and you have rather an interpretation based on what you think Weinberg must have meant. (Your interpretation could well be right; the article might have misrepresented Weinberg's meaning by juxtaposing two quotes.)

I think bringing in religious issues can only obscure the matter — if it's hard enough to agree on objective issues, the discussion will become impossibly fragmented if you add a religious element.

It is of course possible in principle that certain things about the universe are not knowable. (Science recognized this in one precise sense when it found that the results of quantum measurements could only be predicted probabilistically.) But the aim of science is to say as much as possible, and so giving up on making predictions in an area is the very last thing a scientist should do — and it should not be done without a very strong argument for the failure of predictability *even in principle* for the situations considered.

The "mutliverse" ideas motivated by strings suffer from a basic misjudgment. They are predicated on the notion that string ideas are so convincing that it is preferable to consider a major retreat from the goals of science rather than to call strings into question.

This from a "theory" which is not at all a theory in the strict sense, and which has no phenomonological successes. (String-inspired mathematical tricks — which do appear to be useful — cannot count as much evidence for strings as a theory of nature.)

40. **Peter Woit**
May 9, 2009

christina,

The reason for Peter's silence is a bit different than you think. For the last 24 hours he has been having a life: taking advantage of the break in the weather to go on a long bike ride, going out to dinner with friends, sleeping in and having a lazy morning, and doing laundry. He just got into the office, where the main item on the agenda is to make up Monday's final exam for his class. Tonight will be taken up with going to see Star Trek with some friends, Sunday devoted to a day-long celebration of mother's day and to meeting up with Tommaso Dorigo.

So, there's a short window of opportunity here to respond to your comment. Of course I understand that part of what Weinberg was doing was making the standard argument for the anthropic multiverse ("maybe we can't calculate things we hoped to, they're just environmental"), and couching this as "the universe has put us in a crooked game". But unfortunately, that's not all he was saying. He explicitly identified "the only game in town" as string theory, and is making the very popular argument for string theory that, even though it predicts nothing, it should not be abandoned as a failure. The idea seems to be that, since it provides a framework that explains why it can predict nothing, we should believe in it since we don't know how to do better. Personally I think this is pseudo-science. I don't think Weinberg thinks of this as pseudo-science, probably because he still has some hope that string theory will somehow lead to a legitimate scientific prediction of some kind. Some people like him still think this, lots of others, including me, think this is just wishful thinking.

While writing this, I saw the comment from Pawl come in, which I agree with.

41. **christina philosina**
May 9, 2009

Peter, all I said was "Hmmm"!!!
And I do admit to having a soft spot for a roguish yet gallant Highwayman-like character defying the authorities in pursuit of, not Bess, the landlord's daughter, but, far nobler, True Science! (Remember The Highwayman, from Junior High? "Then look for me by moonlight, Watch for me by moonlight, I'll come to thee by moonlight, though Hell should bar the way!" And isn't it amazing how swiftly String Theory has become the dare-not-challenge Establishment, and how thoroughly dissenting views like yours have taken on the stigma of heresy?)

However, one should not let admirable zeal lead you to mischaracterize someone's comments so that it converts (in this case) Steven Weinberg from a reluctant supporter of string theory into someone seriously impugning it. Let's review, for the record, what you said about Weinberg in your original post:

"The arguments for string theory have evolved over the years, with the "it's the only game in town" one being made starting fairly early on. Weinberg seems to be willing to go for a new variant of this, that not only is it the only game in town, but it's probably crooked (i.e. can't possibly work, is obvious pseudo-science…), and this doesn't matter, one should continue anyway."

You now acknowledge, in your last post, that Weinberg doesn't actually think it's pseudo-science; I think I gave a far more plausible interpretation of his joke in my previous posts. As for whether string theory has been prematurely crowned 'the only game in town' is a separate issue–you have advanced powerful arguments in support of the notion that the Physics Establishment has shown reckless haste in its embrace of it, a haste that I think would have propelled Thomas Kuhn, were he still alive, into a possible revision of his belief that theories were always accepted only long AFTER the evidence in support of them justified acceptance.

Pawl,
You fault me for introducing religion into this discussion. Religion subtly lurks at the heart of the acceptance of string theory!! Or more precisely, the antithesis of religion —the fierce materialism of most physicists. It is delightfully ironic that the very thing that makes string theory scientifically unpalatable—the prospect that the 10^500 solutions are irreducible—makes it philosophically irresistible (if you're an atheist). Atheism, the intelligent and informed person realizes, is now untenable without a multiverse—the strong version of the anthropic principle demolished traditional atheism (one universe, without a God being necessary to explain our presence). Incidentally, one reason for the quick embrace of inflation theory was its mechanism for the spawning of other universes. Of course, string theory with 10^500 solutions doesn't prove there IS a multiverse, it simply is consistent with one. Dark energy, as a cosmological constant, WOULD be evidence. And of course, a multiverse would not DISPROVE the existence of God, only make him unnecessary as an explanation for the existence of intelligent life in our universe. Then one would have to decide if one wants to use Occam's Razor on God.

42. **Pawl**
May 9, 2009

Christina,

You seem to have a number of premises (about what atheism or theism is and how they bear on science) which you think the rest of us share. They are certainly far enough from my understanding that I can only guess at what you have in mind.

You seem to have in mind that having a scientific perspective must mean thinking the world is so far from being god's special creation that a multiverse is an appealing idea. I don't know any scientist who holds that view (although perhaps there are some). Virtually all scientists (and the "virtually" is there only to do multiverse proponents the courtesy of not dismissing them, for the purposes of this discussion, as scientists), including atheists, do believe in a world of natural laws and order.

You seem to want to turn this into a theistic/atheistic debate. I decline to do so.

Brane Science

May 2, 2009
Categories: This Week's Hype

There's a nice article in Nature News about the solution to the Kervaire invariant problem mentioned here. It's an excellent and accurate description of the result and its significance, except for the last paragraph, on "Brane science", where the author can't resist following the convention of appending some nonsensical hype about string theory:

> Because the new approach involves looking at topological problems of a manifold from the perspective of a space that has one more dimension, it is analogous to the use of one-dimensional strings as the basis of zero-dimensional (point-like) fundamental particles. Similarly, it has become popular for cosmologists to study the behaviour of space-time from the perspective of higher-dimensional 'branes' that interact with one another. This is why studying the Kervaire invariant problem might offer useful mathematical techniques to fundamental physics.

Update: This news is now featured on the AMS web-site, together with the misleading hype about strings and branes:

> Ball explains "although it looks at face value to be extremely abstruse, the mathematics involved in the solution might be relevant to quantum theory and string theory, not to mention brane theory, which has been invoked to explore some issues in Big Bang cosmology."

Comments

1. **newcomer to QFT**
   May 3, 2009

   For a framed 4k+2 manifold Kervaire defined a refined intersection pairing (on middle dimensional homology). However the refinement is non-trivial only if the exist certain elements in the stable homotopy groups of spheres that are represented by elements in Ext groups. Hill-Hopkins-Ravenel show these elements don't exist for 4k + 2 != 2,6,14,30,62, or 126.

   The case 4k + 2 = 6 is very relevant to string theory. See for example
   Hopkin's ICM address: math.AT/0212397

   Loosely speaking, the existence of certain quadratic refinements is essential for string/M-theory to avoid potential anomalies.

   Of course the new results are about k >> 1, so its not clear what the implications for physics will be.

   There's much to learn about tmf and index of Dirac operators on loop space and the new results are very exciting.

   Claiming that the hopeful wish:

   "studying the Kervaire invariant problem might offer useful mathematical techniques to fundamental physics."

   is "nonsensical hype" seems flippant at best.

2. **Peter Woit**
   May 3, 2009

   newcomer to QFT,

   That an old result about a topological invariant of 6d manifolds has some application to string/M-theory is not surprising, but the new Hopkins et. al result is about non-existence of similar topological invariants of manifolds with dimension 254 and higher. There simply is zero reason to believe that this has anything to do with string theory. The one possible connection to physics that Hopkins mentioned is not about string theory, but a speculative idea about a possible 4d QFT.

   It is also just absurd to describe this kind of mathematical work as being based on the idea "analogous to the use of one-dimensional strings as the basis of zero-dimensional (point-like) fundamental particles". This is not just nonsensical hype, but actively misleading.

3. **Austin**
   May 3, 2009

   Some elementary explanations by John Baez why this problem is string theory:

   http://golem.ph.utexas.edu/category/2009/04/kervaire_invariant_one_problem.html

4. **HarryD**
   May 3, 2009

   Hi,

   I just found that your book "Not Even Wrong" was recently translated into Korean. Since it is not easy to translate 'not even wrong' into Korean, the title of the book is "The truth of the superstring theory" (in Korean). Congratulations.

5. **Peter Woit**
   May 4, 2009

   Austin,

   You're misreading Baez's posting, and presumably the same misreading is what led to the Nature News article.

   Baez explains that there are important relationships between level 2 cohomology theories (elliptic cohomology, tmf) and 2d QFT. This goes back to work by Witten, where he investigated index theory on the loop space of a manifold. Index theory on the manifold itself can be understood in terms of supersymmetric quantum mechanics, going to the loop space means one's path integral is now an integral over world-sheets, so a 2d QFT (which is different than string theory, but maintaining that distinction seems to be a lost cause…).

The new work of Hopkins et. al involves a level 4 cohomology theory, which (speculatively) might have something to do with some sort of quantum field theory on 4d manifolds. Baez refers to such a theory as involving “3-branes”, just because for any n-dim QFT, one can call its n-1 dim boundary conditions “n-1 branes”.

So, what Baez was explaining was a speculative connection between this mathematics and 4d QFT. 4d QFT is not string theory.

If you want to make the argument that any QFT in any dimension d has dimension d-1 boundary conditions and people have called these “branes”, and “branes” also occur in work on string theory, so any QFT is really string theory, I suppose you can do that. But it’s extremely misleading….

6. **Jr**
   May 5, 2009

   Well, they are not exactly hyping string theory. Rather Nature is hyping the result by claiming a connection to string theory.

7. **H-I-G-G-S**
   May 5, 2009

   Typical blog post.

   You are at a meeting where they are discussing blah blah blah or there is an article about blah blah blah. Various smart and accomplished people have done the following interesting thing which is being discussed. Unfortunately these smart and accomplished people are under the delusion that their work has some connection to string theory. Whereas I, PW know that is impossible because nothing interesting or useful can possibly be connected to string theory.

   At some point I would think most of your readers would draw the obvious conclusion.

8. **Peter Woit**
   May 5, 2009

   Jr,

   The hype does go in both directions. There’s some of both here, with the author claiming that “the new approach” of Hopkins et. al. is somehow based on the idea of replacing points by strings (which is simply untrue).

   H-I-G-G-S,

   Do you know anything at all about the mathematics involved here and what its connections to 2d QFT and to string theory are? If you’d like to discuss this, please go ahead, it’s an interesting subject. If you don’t, you might think for a minute about whether using anonymous blog comments to attack people who do know something about this is a good idea or not. I don’t know who you are, but if you don’t have the excuse of being a high school student, I find your behavior on this blog both shameful and unprofessional.

LHC On Schedule

May 7, 2009
Categories: Experimental HEP News

A couple weeks ago I linked [here](#) to a draft LHC schedule that had about 3 weeks slippage from the previous schedule, which has beam commissioning starting again on September 21 (week 39). This was due to delays in getting the new quench protection system in place, which meant that powering tests could not start until later than planned. The latest news is that a way has been found to get some of the powering tests done earlier, and then get the bulk of the tests done in 11 rather than 14 weeks by adding shifts and working on sectors in parallel. The latest schedule thus is able to stick to the September 21 start date.

The working assumption remains that it will take a month or so from that date to get colliding beams and the possibility of starting to get some data. The plan now is to run through the normal winter shutdown period for about a year until the late fall of 2010, hoping to collect 100-500 $pb^{\wedge -1}$ at 10 TeV center of mass energy. This week in Berkeley there's a workshop on Physics Opportunities with Early LHC Data. At the projected luminosities there's not much hope of competing with the Tevatron on the search for the Higgs, but the LHC would be able to push up current Tevatron limits on masses of some superpartners (gluinos).

There are also recent postings about prospects for the LHC from Tommaso Dorigo, and John Conway at Cosmic Variance.

At the KITP in Santa Barbara, there had been plans to have a program on The First Year of the LHC, starting in May of next year. The delay in LHC startup has caused that program to be pushed back, with a new startup date of June 6, 2011.

The latest CERN Bulletin has news and video of the recent transport of the final replacement magnet for the damaged sector 34. All the necessary refurbished magnets are now in the tunnel, and work on the interconnections is on-going.

In other CERN news, I hear that the Austrian government has decided, for budgetary reasons, to withdraw from membership in CERN by the end of 2011. This decision still needs to be ratified by the parliament, so perhaps there is some hope of getting it over-turned.

Update: Maybe not all is well. I hear that new problems have turned up with some of the busbar connections. It turns out that in some cases the way the superconductor was soldered in some interconnections melted the solder connecting superconductor and copper. This could be a problem during a quench. Investigation of the problem is ongoing, and it will take a couple weeks before data is in, analyzed and conclusions can be drawn.

Comments

1. **Andrei**
   May 7, 2009

   Ah so the pressure beggins to gather. Well CERn'd better start running soon and show some spectacular results (as opposed to just hype). Last year the Brittish were cutting down money for this kind of research. Only recently Stephen Chu announced his estimate for the ILC to be about 25 billion. In contrast to Barrish's estimate of about 6 bil. I don't think Chu really believes the large figure. It is more of a veiled message. These big projects need an infusion of legitimacy soon or they in for a rude awakening. Perpetual delays and cute computer simulations just will not cut it anymore.

   As to the LHC schedule I wouldn't get too excited. The first year of data will be inconclusive or more measurements of what nature is not. Then at the end of 2010 after a year of "operation," (I.m sure CERN will say it was a great success just like they did last year despite the fact that the machine blew up in their face) the LHC will stop for a year to install some additional hardware. So in reality we are looking at a de facto additional delay of at least 2 years before some legitimate results will issue from CERN.

2. **Andrei**
   May 8, 2009

   Well here is an update on this. Altough the official reason was budgetary reasons, the Austrian government has actually in creased science funding and (quote):

   The government will use its contribution to CERN — roughly €17 million per year, or 2% of the laboratory's budget — to make up some of that shortfall and to begin participation in other international collaborations in physics, sociology and biotechnology. Among the projects that may benefit are the European Biobanking and Biomolecular Research Resources Infrastructure project, the European X-ray Free Electron Laser near Hamburg, Germany, and the Facility for Antiproton and Ion Research in Darmstadt, Germany.

   here uis a link to that article: http://www.nature.com/news/2009/090508/full/news.2009.459.html.

   A final decision to withdraw is expected in the fall. CERN would better make the LHC run in the fall. According to the above article they are now affraid that other nations will follow suit.

   Well what did they expect? So far, in contrast to the promise to elucidate the secrets of the universe, CERn has been sinking money in one embarrasing failure after another. But that should have been foreseen by European governments because the LHC was sold in France as a job creation tool and not a scientific experiment. I wonder how long the other goverments will tolerate a perpetually delayed project intended to keep French workers in weine and croissants.

3. **Andrei**
   May 8, 2009

   Sorry for not proofing my comments. I am rather tired.

Witten Away From the Energy Frontier

May 7, 2009
Categories: Uncategorized

Edward Witten has been visiting CERN this past academic year, and it seems that besides continuing to work on things related to geometric Langlands (see his recent talk at Atiyah80), he also has been returning to his roots as a phenomenologist, and taking a wide interest in a range of phenomenological questions being discussed at CERN.

Next week CERN will be hosting a workshop on New Opportunities in the Physics Landscape at CERN, to discuss experiments at CERN over the next 5-10 years that are NOT directly related to the LHC. Witten will open the workshop with a talk on *Perspectives in the Physics Landscape away from the Energy Frontier*, and his slides are already available. He comments on a variety of topics, including CMB measurements relevant to inflation, neutrino masses and mixings, proton decay, CP violation and axions, and dark matter candidates. All in all, it's a quite comprehensive survey of how possible non-LHC results might address beyond Standard Model physics questions, mostly from the point of view of the now conventional speculative framework of Supersymmetry/GUTs/String theory.

Comments

1. **Haelfix**
   May 7, 2009

   Witten did some beautiful work in phenomenology, including a bunch of obscure but still highly relevant gems like his work on neutrino physics.

2. **M**
   May 8, 2009

   I wonder what is the opinion by Witten about using a novel non-scaling FFAG or a SC-linac feeding a system of accumulator and compressor rings to limiting the duration of pulses below a few msec in a 50 Hz proton driver?

3. **Greg**
   May 9, 2009

   The most saddening aspect of Witten's slides are that he is excited about the many possible theoretical options. (See the slide before last, for example). One day we will have the final theory, and we will know that all options except one are wrong. So how can one be excited about the existence of many options? It just means that unconsciously, one has given up on distinguishing the true from the false, one has given up on finding the correct option.

   Witten was my hero in the 1990s, when in an interview he stated that he decided to dedicate his life to finding unification. Now he is excited about many options. In short, he has given up. That is incredibly sad!

   It is sad that in order to remain friends with colleagues, we all feel forced to cite all their toy models, as weird as they may be, without saying that they are wrong. Worse, we do not use these mistakes to narrow our search, but we use them to widen it. That is a disservice we do to students – and to ourselves.

   We will never find the toe if we do not "cut the crap". Anybody has the right to publish on any topic, but we need to make use of all these mistakes to proceed. We (the community of physicists) are not doing that, at present.

   Another person, who liked to cut the crap has given up doing so: Motl. We can all notice that he cuts the crap only in topics far from his own field. In string theory, he rarely does so. Thus he will not advance to the toe, even if it is inside string theory! (remember his post many years ago were he speculated the direction to go? He did not follow it up himself, nor did he ever correct the aim.) All these are examples were people prefer to stay friends than to search for the true theory.

   Peter, you cut the crap, and we all admire you for this. Let us go on – life is too short. We now need to clarify the mistakes done in the past and provide guidance, for ourselves and others, into the right direction for the search.

   Woul it be possible to open up a new speculative tag on your blog, called something like "lesson of the week" (partner of "hype of the week") in which you/we all clarify (1) which ideas have to be dropped from the search for the toe, and (2) which ideas have to be retained?

   Nobody is doing this in public in the whole world; who else but you can do it?

4. **anon**
   May 9, 2009

   Who are you? You're supposed to teach Edward Witten, 339 papers and 87,000+ cites old, what is the right path to take and what's wrong? Hilarious!!! Just that he said other options are exciting doesn't mean the options are right..

5. **Francis**
   May 9, 2009

   If I read the post, Greg is not telling Witten what to do, he is writing only about his own sadness. Greg's point and reason for sadness is that Witten has given up searching for the toe.

6. **anon**
   May 9, 2009

   oh really? as I read it, the word 'sadness' seems to enrobe his firm conviction that Witten has given up on unification and instead is promoting other people's ideas to stay in their good books, which I think is plain accusal of a great scientist.

7. **Francis**
   May 9, 2009

   No, it is much worse, Greg is saying that all physicists, including himself and "anon", are too kind to false ideas in order to remain friends, and that this is the reason that the toe has not been found.

8. **BigG**

May 9, 2009

You people should be careful about using the term TOE. First of all the name is misleading as it would not be a theory of everything. Second, even as physicists intend it, there is no certainty it is possible or even an existent. If a point was reached that was labeled TOE, there is no way of being certain that's it. A further objection is that a TOE would be something that is not subject to further investigation, implying something that exists independent of anything else. This concept is highly problamatic. Physics lost its way when it decided philosophy had nothing to offer.

The biggest problem with physics today is those attempting to push the edge of theoretical physics are still stuck in the mindset that made the standard model so successful. This kind of thinking would not have been beneficial in the early 20th century and is holding back progress today.

Its a difference between technicians and thinkers.

9. **Peter Woit**
May 9, 2009

Greg,

The whole problem with beyond the SM physics is that no one has any really compelling ideas. Witten gives a good survey of some of what is out there, but that doesn't mean that he's claiming they are convincing. Like everybody, he's hoping for some unexpected experimental result that will show the way….

10. **anon**
May 9, 2009

It would be better if you speak for yourself Mr. Greg, rather than for the whole physics community as such and Witten in particular, I think Woit's point explains it better..

11. **anon2**
May 12, 2009

I think its important to contrast these two statements…

"Who are you? You're supposed to teach Edward Witten, 339 papers and 87,000+ cites old, what is the right path to take and what's wrong? Hilarious!!!"

vs.

"Science is the believe in the ignorance of the experts"
-Feynman

12. **Paul**
May 18, 2009

But is there any physics beyond the standard model? (I'm counting neutrino masses as part of the standard model.) This is not sure at all. Some argue that no argument for the existence of physics beyond the standard model is really watertight. A likely situation is that the LHC find one Higgs and that is it, and none of Witten's experiments finds anything. We would not be smarter than we are now.

Then Greg's proposal for a "cut the crap" approach might be useful. In fact, if higher dimensions and supersymmetry are dropped because they are "crap", then there are only extremely few options left. But nobody seems to want to explore them.

13. **FNesti**
May 29, 2009

I agree that Witten's slides were quite disappointing.

Lack of new ideas or hints.. (ok, this is hard for everybody, but I was expecting something more.) All theories on equal footing – those with predictions and those with none. Superficial analyses..
And more importantly: the lack of insight, of taste, of understanding of the relations among things, of hyerarchy among problems..

It was something like a student's talk, just a list of possibilities – as if one studied phenomenology just the night before.

So the sad point to me is to realize that, after having provided outstanding contributions to Physics, one can go so far to loose the contact with the real problems. That is to me, the main lesson – be careful not to loose too much contact with physics. Then, no doubt Witten will be back on topic very soon if he wants.

14. **Peter Woit**
May 29, 2009

FNesti,

I don't think Witten's problem is having lost "contact with physics". He's very much in contact with the activities of phenomenologists (and always has been), and his talk reflects the conventional wisdom of the field. To expect some insights into phenomenology from him that are dramatically deeper than those of other experts is unreasonable.

One could perhaps make a stronger argument that he (and many other theorists) are too much in "contact with physics" now, that until something new comes out of experiment, time is better spent doing what they are best at, trying to get a better understanding the formal underpinnings of the subject. But, I agree with other commenters that offering Witten advice about how he should be choosing to spend his time is rather silly…

15. **FNesti**
May 30, 2009

Hi Peter,

well, I do was expecting something more – some concrete points:

Neutrino masses – is it really a surprise the "largness" of the neutrino mixings? (to me this is a naive and misleading observation..) should this be mentioned in a talk?

GUT – in reality there is no prediction of the GUT scale, beyond say 10^14. There are models where proton decay can be suppressed and you can lower the scale .. there are models where the additional mattter content before GUT pushes the scale up to MPL. What one can say is just that the idea is not ruled out by unobservation of proton decay, a fairly weak statement (sad for older generations, not for the new ones). Also SUSY is really a non-theory, when you look at the few complete real models you find big uncertainties from soft terms.. or any sort of fragments in the desert so no real connection with the weinberg angle.. no real connection between GUT and neutrino masses. I would have hoped to see some of this noticed.

p-decay – again – there is no prediction from real GUT models – e.g. it is not true that dim.5 operators are challenged by the SK limit.. they are too model dependent (btw dim5 does not give p->e+pi !) and thus there is no need to invoke extra dimensions or Split-SUSY or string models, where – again – there is no prediction.

DM – we all know there are many candidates, but a solid evidence is already there (DAMA – incompatible with the WIMP picture – waiting to be confirmed whether it is really DM). Similarly for other excesses like ATIC… Would have been nice to have those new evidences put in perspective.

So in any topic I look I am not satisfied.. what can I say.

Yes, probably I am also not satisfied with the “conventional wisdom”…

Whatever Works

May 7, 2009
Categories: Uncategorized

The latest Woody Allen film, *Whatever Works*, was shown recently at the Tribeca Film Festival. I missed it there, but it looks like I'll have to see it when it comes out in theaters later this year. It features the conventional Woody Allen theme of a gorgeous young woman falling in love with a misanthropic Manhattanite old enough to be her grandfather. But this time, the Woody Allen character is a string theorist. Here's part of the plot summary:

> A former Columbia Professor and self-proclaimed genius who came close to winning a Nobel Prize for Quantum Mechanics, Boris fancies himself the only one who fully comprehends the meaningless of all human aspirations, and the pitch-black chaos of the universe….
>
> Boris once had a picture-perfect life. A world-renowned physicist teaching String Theory at Columbia, he was married to Jessica (Carolyn McCormick), a brilliant and beautiful, rich woman, and lived in an opulent uptown apartment. But Boris's good fortune didn't alleviate his perpetual feelings of despair, and one night, in the midst of an argument with Jessica, he leapt out the window. To his great disappointment, he landed on a canopy and survived. Afterwards, he divorced Jessica and moved downtown.
>
> One night, Boris is about to enter his apartment when he is approached by a young runaway, Melody St. Ann Celestine (Evan Rachel Wood), who begs to be let into his apartment….

Comments

1. **Some guy with a laptop**
   May 7, 2009

   Dear Professor Woit,

   do you happen to know that Woody Allen is a huge believer in String Theory? But seriously, it is true.

2. **BigG**
   May 8, 2009

   Woody Allen is not qualified to come to a decision on string theory. Who cares what someone like him thinks about string theory. For the layperson, the only position they can reasonably take is that string theory is highly problematic and if they must should tend towards not accepting it.

3. **Russ Van Rooy**
   May 8, 2009

   Well, that is so Woody! America's best known, crotchety existentialist would live in a universe embedded in the backdrop of a 10 to the 500th power vacua embracing landscape – how else could it be? Woody is the crankiest of cranks and anything that can make his outlook even more depressing is what works.

4. **Chris Oakley**
   May 8, 2009

   Au contraire, Allen's *Slepton production from gauged orbifold flux tube brane compactifications in 198+465 dimensions in the early universe* is widely recognised as the most significant contribution to String Theory since Feynman realised his shoelaces were undone before being called in to interview at Princeton in 1937.

   In my humble one, though, the best thing that Woody Allen ever did – and even funnier – was this, purporting to be the memoirs of Hitler's barber:

   The Schmeed Memoirs

5. **Timothy P. Keller**
   May 8, 2009

   Good Morning,

   Prof. Woit is much too modest.

   Never mind what Woody thinks of
   string theory, the truly interesting
   question is : what real life personage
   is 'Boris' modeled on ? The answer
   is : Prof. Woit! himself !

   Of course, as is well known, mathematicians
   have a debonair charm, which gives them
   the edge over physicists in matters of the
   heart – More particulary, that 'mad poet'
   ambience of the typical algebraist, is just
   madly attractive to the Melodys of the world.
   (I did my dissertation on quadratic forms over
   fields … umm,hmm, …)

   So good to see popular culture finally
   coming to the realization of what we've
   know all along.

   All the best, Tim

6. **Peter Woit**

May 8, 2009

Thanks Tim, but I'm having difficulty finding any points of contact between Boris and myself:

No Nobel Prize prospects
No teaching of string theory
No ex-marriage to rich woman
No opulent apartment
No suicide attempts
No perpetual feelings of despair
No current marriage to gorgeous young woman young enough to be my granddaughter.

Wait a minute though, reading the full piece again, there is something about "irritating his still-loyal friends with his never-ending tirades about the worthlessness of absolutely everything." Maybe Woody reads this blog sometimes….

7. **Chris Oakley**
   May 8, 2009

   > irritating his still-loyal friends with his never-ending tirades about the worthlessness of absolutely everything

   Yeah … though I am prepared to substitute "entertaining" for "irritating"

8. **warp speed!**
   May 8, 2009

   hi Peter. Ignoring your idiotic rants against string theory and its lack of testability, some serious but modest string theorists silently developed for mankind a practical application of M theory:

   http://www.sciencedaily.com/releases/2009/05/090507175838.htm

9. **Peter Woit**
   May 8, 2009

   Thanks warp speed!

   You've inspired another quick blog posting…

10. **BiggerG**
    May 9, 2009

    BigG: `Woody Allen is not qualified to come to a decision on string theory. Who cares what someone like him thinks about string theory.'

    He is about as qualified as Peter Woit is. They have authored the same number of string theory papers. And Woody still has a somewhat bigger audience…

11. **Peter Woit**
    May 9, 2009

    I'm guessing BiggerG is the same person as the one responsible for the "Peter Woit: crackpot or not?" thread over at Tommaso Dorigo's blog:

    http://www.scientificblogging.com/quantum_diaries_survivor/blog/lesser_jetflying_clown_rubbia

    I encourage all people who want to discuss this endlessly fascinating topic to do so over there and help out Tommaso's traffic numbers. Attack Woody as a pedophile here, me as a crackpot there…

12. **Kea2**
    May 9, 2009

    I am shocked that anyone would question Woody Allen's knowledge of physics. Here is what he wrote in The New Yorker, July 29, 2003.
    "I am greatly relieved that the universe is finally explainable. I was beginning to think it was me. As it turns out, physics, like a grating relative has all the answers. The big bang, black holes, and the primordial soup turn up every Tuesday in the Science section of the Times, and as a result my grasp of general relativity and quantum mechanics now equals Einstein's — Einstein Moomjy, that is, the rug seller. How could I have not known that there are little things the size of "Planck's length" in the universe, which are a millionth of a billionth of a billionth of a billionth of a centimetre? …."

13. **Mitch Miller**
    May 15, 2009

    From the trailers it looks like Larry David is going to play the lead role. You can see the trailer on Hulu.

Star Trek Warp Speed?

May 8, 2009
Categories: This Week's Hype

To continue with the string theory/movie theme, a commenter just wrote in to tell about some new ideas for using M-theory to create a warp-drive. These are contained in some papers from the past year or two by string theorists Richard Obousy and Gerald Cleaver (see here, here and here). Today, as a tie-in to the release of the new Star Trek movie, Baylor University issued a press release with the title 'Star Trek' Warp Speed? Two Baylor Physicists Have a New Idea That Could Make it Happen, which states:

> String theory suggests the universe is made up of multiple dimensions. Height, width and length are three dimensions, and time is the fourth dimension. Scientists believe that there are a total of 10 dimensions, with six other dimensions that we can not yet identify. A new theory, called M-theory, takes string theory one step farther and states that the "strings" actually vibrate in an 11-dimensional space. It is this 11th dimension that the Baylor researchers believe could help propel a ship faster than the speed of light.

Interesting to know that there's a "new" theory called "M-theory". Maybe it will replace the old one that has been around for 14 years or so. In any case, while the Woody Allen film is not out, the new Star Trek is, and when I go see it tomorrow night, the fact that it is based on solid science will be reassuring.

Update: Sadly, no explanation in the Star Trek movie of how M-theory was used in the design of the warp drives. However, according to EETimes, a Star Trek warp drive is already in the works.

Comments

1. **Chris Oakley**
   May 8, 2009

   If you go to it for the science you may be disappointed [how weird that that should be the case for a Star Trek movie]. My favourite in this regard was the young Spock saying that he "estimated that he had a 4.6% [or whatever it was] probability of succeeding" in his mission. See Feynman's "What do you care what other people think?" for insightful comments on assigning probabilities to the unquantifiable/imponderable. You will also be introduced to "red matter" which looks like tomato ketchup, but is altogether more scary as it turns stars and planets into Black Holes. I can't remember whether it had the same effect on hot dogs, but to play it safe, if the movie theatre dispenses ketchup from large, glass spheres then *don't order one*.

   Nonetheless, the movie is a fine piece of drama and some of the visuals are breathtaking. Well worth the admission price!

2. **Coin**
   May 8, 2009

   Isn't the 11th dimension in M-Theory just the dilaton field?

   So they're saying they can create the effect of an Alcubierre drive by manipulating the dilaton field?

   …Does that even mean anything?

3. **BigG**
   May 8, 2009

   This is truly an embarassing time for physics. These crap popular articles read like something you'd find in a post-modern studies journal. First of all, you shouldn't speak about inflation like it is fact. Secondly, the use of terms like "tearing the fabric of space" and "shrinking space-time" are meaningless in this context. Regardless if they have a meaning among professionals, when popular articles use such terms its is simply to give people who know nothing something to do at cocktail parties. Its about promoting your program to the public than truth. There are very few true great thinkers anymore. In a time when physics needs seers it is being dominated by craftspeople.

4. **milkshake**
   May 8, 2009

   Coin: Isn't the 11th dimension in M-Theory just the dilaton field?
   Cleaver: [pause] These go to eleven.

5. **LMAO**
   May 8, 2009

   From the article

   "The Baylor physicists estimate that the amount of energy needed to influence the extra dimensions is equivalent to the entire mass of Jupiter being converted into energy.

   "That is an enormous amount of energy," Cleaver said. "We are still a very long ways off before we could create something to harness that type of energy." "

   I am now convinced that at least some string theorists shouldn't be allowed to speak in public.

6. **Janne**
   May 8, 2009

   That red matter sure is mysterious, but I guess Angels & Demons next week will straighten things out for the layman.

7. **Andrei**
   May 8, 2009

   Red matter? I guess it is a spin on "red mercury." Remember that super destructive substance the Russians had invented? Which was another red herring. I wonder why in Hollywood psychologys all evil things or villains are either red or green. The green has been explained by some comentators as resulting from a perceived association of the color green with gas swamp, which is suposedly green (I've never been to a swamp). The Romulans had green ships so did the Borg. And remember Star Trek Nemesis. That was an ovwerload of green. But what's with the red? Is it because of the Romans with their capes or the Soviets or perhaps the Nazis. Or because the evil tea sucking British troops had red uniforms back in revolutionary times? For once I'd like to see a b;ue or say orange villain or ship.

8. **Ralph**
May 9, 2009

Cool, does this mean that implementing a working warp drive is a test of string theory, and that if we can't send people to Alpha Centauri and beyond by next year, then the string people will shut-up and let the LHC work out what reality is like instead?

9. **Chris Oakley**
May 9, 2009

Some script that was cut from the final version:

NERO

You b*****d, Spock! You could have saved my planet! If you had arrived twenty minutes earlier, you could have created a black hole with your Red Matter which would have swallowed up the supernova!

SPOCK

Let me get this straight – you seriously think that a few drops of tomato ketchup could have prevented a star from exploding. Am I right or am I right?

NERO

That is correct, pointy-eared freak!

SPOCK

What are you? A Trekkie fan? Believer in String Theory? I suppose that you think Elvis is still alive, having been abducted by anal-probe-inserting aliens?

NERO

Indeed! He is our prisoner! And will be until we find out who killed JFK!

10. **Bubos**
May 9, 2009

Shame on Chris Oakley who should have the brains to know that Star Trek is backed by, and actually starred in, by Stephen Hawking. So it's factual as Hawking radiation.

(Hawking wouldn't have got any Nobel Prize if it was just a stringy type speculation nobody could check, unmeasurably submerged in the intense gamma ray background radiation spectrum of space!!)

11. **Chris Oakley**
May 10, 2009

Hi Bubos,

1. Stephen Hawking appearing in Star Trek to me signifies that he needs the money/publicity, not that he endorses the "physics".

2. Even if he did, I would not care that much. I am afraid that the clip is misleading you in claiming that he ranks with Einstein and Newton as he is not even close. Those who claim to the contrary I think you will find are publicists for his book, not physicists.

12. **Thomas Larsson**
May 11, 2009

"Hawking **wouldn't have got** any Nobel Prize" ???

Maybe it is because I am a non-native English speaker that I have problems parsing this sentence, but it seems to me that you believe that Hawking has got a Nobel.

13. **db**
May 11, 2009

@Thomas Larsson

I am a native english speaker, and can't parse it either. It looks like the output of a physics oriented dada engine to me.

14. **Larry**
May 12, 2009

Peter,

You will be happy to find that you and Lee Smolin have a new soul mate:

Oswaldo Zapata:

On Facts in Superstring Theory. A Case Study: The AdS/CFT Correspondence

http://arxiv.org/abs/0905.1439

Congratulations, the anti-string horde is growing with more and more fine warriors! I'd be interested to know and I suppose other readers as well, what you think.

Larry

15. **Austin**
May 12, 2009

Dear Larry, the historical essay you mentioned is actually – decently – pro-string theory.

There is a whole blog by the same author dedicated to these ideas. I assure you that neither Peter Woit nor Lee Smolin can be found among the 20 authors of the popular and semipopular books about string theory that the author discusses.

http://spinningthesuperweb.blogspot.com/

16. **Tim**
May 12, 2009

Dear Larry, Austin,

after skimming the essay I got the impression that the author does not
make a clear statement if he is pro or con the way the Maldacena conjecture
“evolved” from a conjecture to a “fact even mentioned in undergraduate courses”.
His point is that this process did not follow what was common sense
in Mathematics/Physics in the 20th century (mathematical theorems need proof,
physical theorems need experimental verification, to be accepted by the scientific community).
Is this impression correct? Or does the author propose that
human kind just developed a brand new way to establish scientific truth? 🙂

P.S.: I think we can forget about this Star Treck thing as public relations gone wild.

17. **Peter Woit**
May 12, 2009

Austin,

Actually Zapata quotes my book at least a couple times in the first essay. He wrote recently to tell me about his project, and I pointed out to him that one of the quotes was out of context and misleading. I started writing a blog posting about this, will finish it soon. Yes he’s “pro-string theory”, but he does seem to agree with me that hype, propaganda, and public relations are an important part of string theory. But, for him I guess, that’s a good thing…

18. **Tim**
May 12, 2009

Hello Peter,

I understand that Zapata got his Ph.D. for working on a topic in string theory, so I am not surprised that he is a supporter – but I am quite surprised that he does not seem to have a problem that “hype, propagande” etc. are important parts of string theory. How come? As I said before from his first essay his personal viewpoint did not become clear to me.

19. **PhilG**
May 15, 2009

The FQXi have launched an essay contest on “What’s Ultimately Possible in Physics?” This could take speculation and hype to a whole new level.

http://fqxi.org/community/essay

Spinning the Superweb

May 12, 2009
Categories: Uncategorized

Oswaldo Zapata is a young string theorist who recently got his Ph.D. in the subject in Rome. He recently wrote to me to tell me about some essays on the history of superstring theory that he has written, which he is starting to put up on a web-site he calls [Spinning the Superweb](). I'll be interested to follow the rest of the essays. He has also posted the first of these on the [arXiv]().

Zapata's history is largely concerned with the question of how string theory has achieved acceptance in certain circles despite its failure to satisfy the conventional criteria normally demanded of a successful scientific theory. Reading him, you might initially get the idea he is a string theory skeptic unhappy with what has happened:

> From the previous examples we have learnt some important things about the development of string theory. Firstly, as research progresses in a given topic, an explicit reference to the unsolved problem tends to disappear from the literature. For instance, we saw how the quantization of gravity is considered by string theorists to be an accomplished task that does not deserve further study, or even a mention. Secondly, while research advances, the initial problem changes in such a way that it becomes increasingly difficult to unravel the convoluted relationship connecting the final problem to the original one. This was illustrated by our second example concerning string theory and the unification of the forces. Originally the idea was to extract the standard model from superstring theory, an investigation encouraged during the second half of the eighties by the promising results obtained from the heterotic string. Then, by the mid-nineties, the goal was to determine the unique vacuum of the mother of all the theories, the M-Theory. And, more recently, the focus was on the right "environment" of the anthropic solution. Things have changed, but the fundamental query remains unsolved: how do we get the standard model from string theory? With these examples we have learnt something else: this occurs while an "outward" discourse (from the "inside" to the "outside" of the professional community) proclaims that the theory has solved such problems. Indeed, in this movement disadvantages have been transmuted into virtues…
>
> At first, a hypothesis is made, explaining openly its significance as well as its difficulties. At this stage no one is sure of the real value of the conjecture, however, it is interesting enough to drive a significant part of the physics community to devote itself to its development. Step by step "evidence" accumulates and after a while the string theory fact emerges. String theorists have created in this way their own nature: a supersymmetric world, a big bang with all the fundamental forces combined, a multi-dimensional universe, and so forth.

Zapata appears to be claiming there is such a thing as a "string theory fact", which is somehow different than the usual scientific notion of "fact", one that requires experimental confirmation.

Among the other unusual aspects of the string theory story that Zapata recognizes is one that has often struck me. This is a subject so complicated that very few people actually understand what is going on, including many of the people working on it. As a result, overhyped claims in the popular media play a big role, with few people able to evaluate them properly:

> In fact, string theory is so complex that experts are neither able to understand entirely the main developments nor to follow its rapid growth. In general, practitioners feel confident only in a specific subfield. People working on the AdS/CFT correspondence or twistor theory, for example, do not comprehend the whole area, even though they can be extremely competent when tackling the particular problems of the subfield. Because of this, paradoxically, those that have provided the evidence in support of superstrings do not fully grasp it. Many do not understand the AdS/CFT correspondence completely but they believe in it; it is a matter of fact. A fact in string theory is a shared belief that something is unquestionably true. What I will try to show here is that string theorists often base their beliefs on what they have seen proclaimed everywhere. This ubiquitous discourse includes technical seminars and articles, which I will call the in-in discourse, as well as popular speeches and books, the out-in discourse. Furthermore, I will try to convince the reader that string theorists start to internalize the rules of the game long before they become experts; by means of a discourse that embraces the whole society. I will dub this the out-out discourse when the information comes from non-experts, and the in-out discourse when it comes from professional physicists.

Zapata goes on to give a truly remarkable description of the sociology and psychology of how people get into string theory. Remember, this is coming from a young string theorist:

> The discussion above suggests that many string theorists have begun their careers with a biased view of the subject. How they conceive the theory during their formative years depends crucially on previous contact with materials intended for the general public and, later on, on the systematic training given by senior members of the community. We have seen how these two stages in the education of future string theorists coincide at one point: they present new subjects as confirmations of the most fundamental claims of the theory. The theory has succeeded in: quantizing gravity and unifying all the fundamental forces of nature. In addition, it explains the thermodynamics of black holes and has also demonstrated a precise gravity/particle physics correspondence. This is what is taught. Even though young string theorists can feel sometimes uncomfortable with the weakness of some arguments, the challenge usually exceeds their skills. Moreover, in such a competitive field there is no time to digress by asking fundamental questions. When finally the young researcher becomes a full member, with many more resources at hand to tackle fundamental issues, it turns out that they are probably working on a specific topic with its own problems. And, not surprisingly, all these investigations assume the validity of the basic claims of the theory. The once controversial claims are no more questioned; they have been internalized as matters of fact. Eventually, the young researcher becomes an accomplished theoretician; it is now their turn to protect the theory and contribute fervently to the in-out discourse. This final step consolidates further the scientific fact and, very importantly, guarantees the reproduction of well-trained newcomers. This long and tortuous process of internalizing the rules of the game is sociological, but unavoidably also psychological. As I said above, a fact in string theory is a deep and sincere belief, and nobody can dispute certain issues without at the same time denying their own self.

With belief in string theory based on this sort of psychology, it's not surprising that defending it from skeptics can't be done with the usual sort of scientific discourse, but requires propagandistic techniques:

> What I've described in this section is an alternative strategy of validation that string theorists have persistently employed in order to preserve what they consider a worthwhile field of research. The purpose of this is to protect the theory from attacks from defenders of contending models; attacks due in part to theoretical and experimental shortcomings. It is not an exaggeration to say that string theory uses propaganda, more or less as Galileo did in his times: ''He uses psychological tricks in addition to whatever intellectual reasons he has to offer. These tricks are very successful: they lead him to victory.''

Describing a New York Times article on the Maldacena conjecture, he writes:

> This article, and many others of the same sort, reinforce, willingly or not, the social belief that superstring theory is ''on the right track.'' In this case, the circle of believers is expanded thanks to the participation of non expert actors: science writers and interested readers. This sympathetic environment, which will be illustrated further in the next essays, has been vital for the development of the theory. It must be mentioned that this out-out discourse does not originate independently from professional string theorists. In general, it simply reproduces the in-out discourse of the experts. I do not mean to suggest that string theory popularizers are scientifically illiterate, I just want to highlight that the substance of what they say reflects the opinion and enthusiasm of string theory specialists. In such an abstract area, things could not be any other way. As a consequence of this discourse, a favourable disposition regarding superstrings has permeated into the public domain. The lay public's attitude functions as a support for the internal discourse. What is more, the layman's view of superstrings is sometimes internalized by experts on the theory and then works as a reconfirmation of the old belief. To put it differently: the out-out discourse is not only oriented to popular audiences but towards experts as well; the out-out discourse is also an out-in discourse. Consequently, "non-pure" conceptions penetrate and modify the theoretical development of the

field. I will call this the in-out-in process. Notice that unlike the in-out-•••-in process explained above, the in-out-in process only concerns the movement of ideas (of course, persons are also involved here, but not in the sociological sense meant before). In this way, with contributions from the in and the out, the creeping belief in the accomplishments of superstring theory is gradually confirmed…

> The effects of these kinds of comments on the theory are two-fold. On one side they create a favourable background for the theory to develop, on the other they send a clear message to string theorists that they are doing right, that nature is really as they think it is. I must confess that this hypothesis is hard to prove. However this is what the next essays try to do. Before moving on to these more detailed discussions, I would like to observe something that a string theorist would be unlike to deny: when a newspaper says that colleagues at Harvard are dancing "La Maldacena," they feel more confident about their own results. Something similar occurred when David Gross was honoured with the Nobel Prize for physics in 2004. My experience was that the general mood among string theorists was very optimistic. They felt that this award was somehow recognition of their own efforts in string theory. Evidence in support of this claim is varied: from technical seminars to public speeches, and from published articles to forwarded emails.

All in all, Zapata does an excellent job of explaing why string theory has been the subject of such a long-term relentless campaign of hype and propaganda, one that continues to this day.

In his essay, he concentrates on the story of AdS/CFT, the one place that string theory has had some real success. As part of this, he engages in some propaganda himself, quoting me out of context in a misleading way. When I wrote in my book about string theory as a "failed project", I was referring to its failure as an idea about unification, not describing AdS/CFT as a failure.

All in all, Zapata's essay is something quite remarkable: a view from the inside of what things look like to someone who is both a true believer, as well as a clear-eyed observer of how string theory has gotten to where it is today. I suspect though that his history is already starting to be out-of-date, with the same phenomena that he describes looking very different to the rest of the world. Most physicists have begun to lose patience with the hype and propaganda surrounding string theory, and want nothing to do with a supposedly scientific subject full of true believers acting on a new and non-standard concept of what is a scientific fact and what isn't.

Comments

1. **Spiny Norman**
   May 12, 2009

   "I will call this the in-out-in process. Notice that unlike the in-out-•••-in process explained above, the in-out-in process only concerns the movement of ideas (of course, persons are also involved here, but not in the sociological sense meant before)."

   You do realize, Peter, that this is a crude (but funny) joke?

2. **Peter Woit**
   May 12, 2009

   Norman,

   The thought has occurred to me that the whole thing is a rather fantastic joke of some kind. But I can't figure out what kind…

3. **Leonard Ornstein**
   May 12, 2009

   Peter:

   Oswaldo doesn't seem to accept the distinction between belief in a mathematical conjecture, which may ultimately be 'provable' as a theorem, 'merely' through consistent application of 'axiomatics', and assurance of completeness; and 'belief in' a scientific conjecture (model), which requires some degree of empirical inductive support to warrant any 'degree of belief', and can never be 'completely' verified.

4. **Sandro**
   May 12, 2009

   I think I've got to agree with Leonard Ornstein. He makes actually a good point in distinguishing the two kind of "beliefs": indeed, in my opinion, mathematical beliefs like that homological Mirror Symmetry must be there for pairs of mirror Calabi-Yau n-folds are well worth pursuing, and of course knowing what has been done by String theorists can be quite useful. This is my mathematician's view of the subject, though…

5. **Peter Woit**
   May 12, 2009

   Sandro and Leonard,

   The big problem with the Zapata essay is that he doesn't distinguish between two completely different things:

   1. The AdS/CFT conjecture, which is an essentially mathematical conjecture about a relationship between two different mathematical structures, and for which there is quite a lot of evidence, with many people working on gathering more evidence and making the conjecture more precise.

   2. String theory as a 10/11d unified theory of particle physics and gravity. For this, not only is there no positive evidence, there is now a lot of evidence that this can't work.

   Case 1. is a rather conventional story about how "belief" in a mathematical conjecture becomes stronger as it passes various tests, you understand it better and make it more precise. It is case 2. that is highly problematic, where you have lots of people "believing" in an idea about the fundamentals of physical reality. Zapata gives a good explanation of how hype, propaganda, PR, and hiding behind extreme complexity have led to this much more problematic sort of "belief". I'm still having trouble believing myself that he has a positive view of this second situation.

6. **Peter Shor**
   May 12, 2009

   I just realized: part of the problem of string theory may be that the sociology of physics departments does not let people work on purely mathematical things like AdS/CFT without some claim that they are connected to the laws of physics.

7. **Coin**

May 12, 2009

So… reading Zapata's Arxiv essay (which only seems to contain the first of Peter's blockquotes– what are the other quotes from?) something that seems to me important is that he doesn't seem to be *judging*, particularly, anything he's writing about– taken by itself the paper seems to mostly just be describing a sociological process, a process where a conjecture becomes a fact just because everyone's given up on trying to prove it.

Ornstein/Sandro/Woit above raise some objections having to do with the idea that in some contexts (say mathematics vs physics) this process or something like it might be *appropriate* more so than others, and Zapata doesn't really acknowledge this distinction? But it seems like the question of whether and when this process is appropriate is separate from the question this *particular* essay seems intended to address, which is more just whether the process is happening at all. I think this can be an interesting and valuable question by itself– is String Theory progressing with time on its central questions, or just gradually shrugging them off?

This said the main question I'd ask about Zapata's paper is whether the process he describes is unique to String Theory. It's certainly not hard to come up with examples in mathematics where a very large body of work springs up around a conjecture which is proven either much later or not ever at all. Even in physics I seem to hear a lot about incidents where questions about the mathematical validity of some quantum physics procedure or other (renormalization and "integrating over the singularity" in path integrals come to mind) just got kind of dropped over time without ever really being addressed. I have a strong suspicion if you tried to take the analysis Zapata performs on string theory, of catching unsolved problems in the act of being "disappeared", and applied this to a number of other sciences in their early stages, they would not come across very differently.

(Of course, even if the kind of behavior Zapata describes is widespread within math and/or science, this wouldn't necessarily reflect well on string theory anyway– that is, I'm not trying to suggest string theory gets an "everybody does it" pass. It seems like string theory's unique status, that of simultaneously being widely assumed to be central in science yet performing relatively poorly in terms of contact with experiment, means we might reasonably demand higher expectations of string theory than we might some other young sciences. For example it might be that, I don't know, the theory of motives is on just as shaky theoretical grounds as AdS/CFT due to an analagous failure to prove the underlying conjectures; but motives do not get the sort of central, "only game in town" treatment in mathematics that strings get in physics, and are in fact kind of obscure. Or it might be that the early historical stages of QFT brushed over some critical questions of consistency and finiteness, in a way similar to what string-theory based quantum gravity studies have done; but in QFT the reason why this was forgiven is that QFT was actually producing practical numerical results, in a way which I don't think string theory QG can.)

8. **John Baez**
May 12, 2009

Peter Shor wrote:

> I just realized: part of the problem of string theory may be that the sociology of physics departments does not let people work on purely mathematical things like AdS/CFT without some claim that they are connected to the laws of physics.

Yes. The mere fact that a mathematical structure is beautiful and interesting is typically not enough for physicists; it's supposed to be related to the real world. At first glance this sounds entirely sensible, but it has a bad side-effect: even when physicists are studying a mathematical structure with no clear relation to the real world, they feel psychological pressure to act like they're studying the real world – or even to *believe* that they are. This creates misleading rhetoric – or even self-deception.

It's not just a problem in string theory; it's also true in loop quantum gravity.

This is one reason I've quit going to quantum gravity conferences and started focusing on pure mathematics. If it's "merely mathematics", the need for certain kinds of rhetoric is eliminated.

9. **BigG**
May 13, 2009

This choice of term "real world" needs to be done away with. What does real mean? What does world mean? Is math not real? Not part of the world?

10. **Tim**
May 13, 2009

@Coin: I totally agree that Zapata does very carefully avoid any judgment, maybe in order to not offend anyone – seems to be done extremely clever, at least to me.

11. **Sandro**
May 13, 2009

"[…]even when physicists are studying a mathematical structure with no clear relation to the real world, they feel psychological pressure to act like they're studying the real world – or even to believe that they are."

This is exactly the problem.

"This is one reason I've quit going to quantum gravity conferences and started focusing on pure mathematics. If it's "merely mathematics", the need for certain kinds of rhetoric is eliminated."

And this is exactly the solution.

12. **Peter Shor**
May 13, 2009

Sandro says (quoting John Baez)

"'This is one reason I've quit going to quantum gravity conferences and started focusing on pure mathematics. If it's "merely mathematics", the need for certain kinds of rhetoric is eliminated.'"

"And this is exactly the solution."

This is, unfortunately, not the solution for many string theorists. Mathematicians have a similar, although different, problem: to claim you're really doing mathematics, and be accepted by most mathematicians, there has to be some pretense of mathematical rigor. Physicists don't require mathematical rigor; historically, the connection to the real physical world has presumably been a good substitute for this. While some string theorists would fit fine in mathematics departments (and some already are), I would guess that most could not.

For a non-string theory example, Michel Talagrand has recently proven some of the claims of the physicists using the replica method using mathematically rigorous techniques. This work is acclaimed among mathematicians, but physicists are completely unimpressed.

13. **Chris W.**
May 13, 2009

Following up on Peter Shor's latest comment, there is an obvious parallel to the above in the relationship between the work of physicists and engineers. That is, a nagging and possibly fundamental issue of understanding for a physicist might be—in fact, typically will be—of little or no interest to engineers. In some cases this applies to entire subfields.

More to the point, the kinds of shifts of attention and effort that Zapata describes can be regarded as shifts of *problem formulation*, in the hope of finding a better avenue for understanding and technical progress. I've mentioned it before in comments on this blog, but it's worth repeating an observation of Shiing-Shen Chern. In a volume published during the Einstein Centennial (1979) there is a reminiscence by Chern about Einstein, partly based on discussions Chern had with him when he was at IAS (1943-1948). Chern observed that problems generally (always?) come to a mathematician in a reasonably well-stated form, whereas a substantial part of the task of a theoretical physicist is to arrive at a good problem formulation before attempting to apply any refined technique or method of attack. This is especially true at the frontiers. (I and others would argue that metaphysical ideas often play an essential role in arriving at these problem formulations.)

Of course, the central issue and recurring theme on this blog has been whether this process of redefining the problem—in physics and science generally—should be allowed to extend so far as to undercut the very possibility of refutation by observation of the proposed solutions.

14. **Chris W.**
May 13, 2009

PS: I'm pretty sure the book I have in mind is *Some Strangeness in the Proportion: Centennial Symposium to Celebrate the Achievements of Albert Einstein*, edited by Harry Woolf (Addison-Wesley, 1981). It's listed on Amazon.

15. **Sandro**
May 13, 2009

@Peter Shor,

sorry about the misunderstanding: I wasn't referring to a common string theorist, but to "physicists (who) are studying a mathematical structure with no clear relation to the real world". In most cases these people have the necessary rigor to be called mathematicians (unless you define a mathematician to be someone that can't read physics papers), and there's just a psycological barrier, as John says. I agree with you that many sting theorists would struggle in a maths department.

16. **Peter Shor**
May 13, 2009

Hi Sandro,

Sorry to have misunderstood you. I agree that most of the quantum information theorists (for example) in physics departments are working with sufficient mathematical rigor to be accepted by mathematicians.

On the other hand, the replica method in statistical mechanics is completely non-rigorous, and it has led to some amazing mathematical results which I don't think could have been found if the theorems hadn't been discovered first by the replica method, and then proved rigorously by mathematicians. Some of the physicists using it are studying statistical mechanics systems which have nothing to do with actual physics, but which arise in computer science, and I don't think they've suffered any adverse consequences. Of course, the replica method is also very useful for systems which arise in the real physical world, so there's no need to justify the whole field.

17. **Sandro**
May 14, 2009

Hi Peter Shor,

I completely agree with you. The situation I've in mind is that of a physicist who realizes at certain point that he's actually a working mathematician, with prevalent interests being on the maths, and decides to go for pure maths. Of course, his background in physics will allow him to realize that some results in physics, like those implied by the replica model in statistical mechanics you mention, can have an impact also on mathematics, or at least constitute results which are worth investigating from a mathematical point of view. In an ideal world, such a person should not have any problem fitting in a good mathematics department, but I'm not sure if this is always the case…

18. **davetweed**
May 14, 2009

I think the discussion between Sandro and Peter Shor slightly misses the point. Isn't the problem not that people who want to switch from physics to mathematics can't, but that it's very difficult for a physicist to say "I think this mathematical structure may be useful in modeling some physics, but I can't demonstrate this yet. I want to keep studying this IN THE HOPE IT WILL LEAD TO PHYSICS rather than purely for its own sake (ie, mathematically), but it's not successful physics yet". It's a version of the problem in many disciplines that there's arguably a bit too much short-term-ism you can't really get the time to "build up some conceptual tools for discipline X (that aren't yet convincing)" within the X department itself.

19. **H-I-G-G-S**
May 14, 2009

This supposed history of AdS/CFT omits one of the three foundational papers on the subject (hint: the paper appeared about a week before the Witten paper he discusses at length). He also seems ignorant of many of the ideas and results that led up to Maldecena's conjecture. A clear-eyed observer? Looks to me more like one eye only partially open.

20. **Joao Leao**
May 14, 2009

Peter,Leonard,Sandro, etc..

I am afraid you are splitting hairs you don't have! A conjecture, in either math or physics, may be more or less *plausible* as it is supported by more or less plausible arguments. But it is not *a fact* until it meets the standard of a mathematical proof or a modicum of empirical data in its favor. What Oswaldo points out is that this is no longer the case as far as the Maldacena correspondence is concerned and even your reaction to his observation supports him! Since BMN asserted it as such the AdS/CFT correspondence is an *undisputed fact* and no one holds any doubts about it, including yourselves! This in spite of the fact that there is no mathematical proof of it or any real world experimental results to support it. No one in his right mind would even spend time trying to find a proof or, more crucially, the likely limits of its applicability. Funny that BMN are still kind of cautious about it obtaining in the non-supersymmetric gauge theory case but who doubts that today after all the great heavy-ion collision "duality predictions"? 3,000 papers could not be wrong, could they?

It seems to me that OZM is hardly making a joke but instead an insightful and judicious analysis about something he has directly witnessed: the production of "string theoretical facts" by a mix of social pressure, dogmatism, popular hype, PR, and tabloid level journalism. I think what he is saying deserves better attention than you seem willing to give it.

21. **David B.**
May 14, 2009

Dear Joao:

Few of the experts believe the correspondence is a fact in the mathematical sense of having a proof. If you want a real explanation of why a great part of the field has moved on, read the post I made on my blog. Calling something a fact has more than one meaning.

There are also plenty of people trying to find a proof of it, all of them rather respectable physicists.

22. **Joao Leao**
May 15, 2009

Dear David,

Thank you for your kind response and for pointing me to your blog entry. I read the very interesting exchanges and marveled at your clever "bayesian belief argument". Unfortunately I have the same qualms with it that other people well expressed on your blog. But I do not think that you have to believe a conjecture to be true to do relevant work on it. Think of the Artin and Langlands conjectures or, more gloriously, of the Riemann Hypothesis. And, mind you, I fully recognize that there is considerable heuristic support for some form of a gauge/gravity duality. I only object to this compulsion to state as established fact something that you know it is not quite yet so! In this I have to concur with Oswaldo and point to this as a form of bad faith that has regrettably become the norm in and around the string community.

I am glad to know that there are people still hunting for the proof, though. If it is not asking too much maybe you can point us out one or two, no? Thanks in advance.

23. **David B.**
May 15, 2009

Hi Joao:

For recent work, there is a huge community that is using the hypothesis of integrability to try to bootstrap the AdS/CFT correspondence for the maximally supersymmetric case. Some of the main proponents and characters are Beisert, Dorey, Kazakov, Minahan, Staudacher, Tseytlin, Zarembo (but there are many more). The discussion is rather technical at this point, but the aim has always been to prove in a rigorous way various key aspects of the AdS/CFT correspondence.

For other setups, Gopakumar et al. have been studying how to rewrite Feynman diagrams in four dimensions so that the AdS geometry becomes more manifest.

Maldacena and collaborators have found a huge number of the pieces of the AdS/CFT dictionary.

Berkovits and Vafa have a different starting point from string actions.

I am also aware of various puzzles that have been raised by Giddings regarding locality on sub-AdS scales, and Polchinski has been studying various problems related to understanding black holes and the problem of information loss (even if at the level of toy models). One of the key missing links that people really want to get their hands on is deducing the flat space S-matrix of string theory from SYM (recovering the Veneziano amplitude of flat space).

My own work in the last few years has been on starting from field theory and trying to deduce everything else.

Don't be deceived by outward appearances: people in the field are very aware that this is a conjecture. People take it to be true in the same spirit as the Riemann hypothesis, and as such there are a lot of applications that spring forward from the equality of two apparently different formalisms.

A lot of the work assumes it is correct to make deductions about situations where there are no other computations one can do. Because in some situations one really does not know what quantum gravity is, many papers assume that the definition of quantum gravity in AdS spaces is exactly via a QFT in fewer dimensions. This would sound as a tautology, but there is still the burden of showing that the semiclassical intuitions about quantum gravity are indeed correct in the appropriate limits.

No matter how one looks at it, if one really wants something close to a proof (real understanding of what's going on), one has to solve problems at strong coupling in one way or another. This is just extremely hard in general, but the community of people looking into this issues is huge. Although sometimes it is hard to tell what is going on from the way that the papers are worded if you are not working on the field itself.

24. **Joao Leao**
May 16, 2009

Hi David,

Thanks so much for the references. You provide a very detailed guidebook. I will withhold judgement until I can read through some of the work you point out. I am aware this is hard work and that it can no longer be considered as part of sting theory apologetics since it aims to set a general context for strong coupling regimes in general. Still I maintain that that is precisely why one needs to be extra careful in separate facts from fictions of convenience.

Thanks again for indulging me,
-Joao

25. **Amir**
May 22, 2009

Hi Peter
I was wondering where you got the quotes that you've put on your blog here. I'm going through the arXiv paper and can't find any reference to for example this sentence:

"Even though young string theorists can feel sometimes uncomfortable with the weakness of some arguments, the challenge usually exceeds their skills. Moreover, in such a competitive field there is no time to digress by asking fundamental questions."

The above quote is also nowhere to be found on the author's blog.

Best, Amir

26. **Peter Woit**
May 22, 2009

Amir,

The quoted material is in the fourth paragraph from the bottom of this page of the author's blog

http://spinningthesuperweb.blogspot.com/2008/05/1-on-facts-in-superstring-theory-iii.html

27. **Amir**
May 23, 2009

Thanks, Amir

New York Events

May 12, 2009
Categories: Uncategorized

I'm afraid that most of you have already missed one event here in New York involving someone who blogs about high energy physics. This was Tommaso Dorigo's visit this Sunday to New York for a few hours. Luckily for you, he blogs about it, with pictures, here. I'm quite pleased to have finally gotten a chance to meet him in person. My mother feels the same way.

There is something else though here in New York, next Monday, that you still haven't missed. I'll be talking and answering questions at an event organized by the Center for Inquiry, which will take place at the Brooklyn Society for Ethical Culture in Park Slope. More information about the event is available here.

Comments

1. **a quantum diaries surivivor**
   May 12, 2009

   The pleasure was mine, Peter! And your mother is a charming lady, sends her my regards.

   Cheers,
   T.

2. **Chris Oakley**
   May 13, 2009

   Peter,

   The company you choose to keep is entirely your own decision, but you should be careful of being associated with Professor Dorigo as no less an authority than Dr. Motl (formerly of Harvard University) has exposed him as an amateur and a fraudster.

3. **Successful Researcher: How to Become One**
   May 14, 2009

   Are there any plans on making the video of the second event available online?

4. **Peter Woit**
   May 14, 2009

   Not that I know of…

Austria May Leave CERN

May 12, 2009
Categories: Experimental HEP News

I mentioned this here when I first heard about it, but by now more information is available. Last Thursday the Austrian government announced their intention to withdraw from membership in CERN, effective late 2010. This decision still needs to be approved by the parliament. An official statement from CERN is available here, news stories here and here, blog postings many places including here.

The cost to Austria of CERN participation is not extremely large (less than 20 million Euro/year, roughly similar to the cost of running the math department here at Columbia), and this decision came as a surprise to the physicists in Austria who will be most affected by it. Unfortunately, joint efforts like CERN that produce fundamental scientific knowledge with no direct applicability suffer from an inherent structural problem. After leaving CERN, Austria will still benefit from knowledge produced there, even if they are no longer paying for membership. In times of budgetary problems, a government could rationally decide to cut-back on its contribution to organizations that it believes will manage to go on without its help. The problem here is not so much the loss of Austria's contribution, which is a budgetary problem CERN can find some way to deal with, but the danger that other members of the European community may decide to follow suit. If a lot of other European governments make the same calculation as Austria, CERN could not survive.

A letter signed by representatives from all the particle physics groups in the UK is going to the Austrian government, asking for reconsideration of this decision, and presumably similar efforts will come from the rest of the CERN member states. The Austrian Institute for High Energy Physics has set up a web-site dealing with the issue here, and an on-line petition here.

If the decision is not overturned, CERN will be in a very uncomfortable position with respect to collaboration with Austrian physicists. While cutting off contacts goes against all traditions of the field, continuing them would encourage other states to follow Austria's example.

Update: It looks like the decision has been overturned, and Austria will stay in CERN. There's a news story in German here.

Comments

1. **karl**
   May 12, 2009

   Hi Peter, are you sure about the 20 Mios for the Math Department?
   b.t.w. the actual CERN Membership is somewhat less for Austria, rather around 16 Mios. it adds up to 20 if they also close the Institute for High Energy Physics HEPHY. But I wonder if you should be sympathetic to the plan: just as you want to shoot down string theory there are scientists who want to shoot down experimental high energy physics. I guess in the US you do have experience with people like that.

2. **Peter Woit**
   May 12, 2009

   karl,

   That was an order of magnitude estimate. I checked into this a bit, and if you add all costs up, maybe 10 million dollars would be closer. So, I edited the post a bit to reflect this.

   I understand that HEP has always had a problem that it's an expensive business, and other scientists sometimes believe that if the budget for HEP were to be cut, there would be more for them.

3. **J.F. Moore**
   May 12, 2009

   The Tragedy of the (unreguated) Commons manifested as usual.

   Peter, regarding HEP we can only hope that the likely budget increases to all science in the next few years will remove that kind of pressure. Most sensible scientists know already that total science funding is not a conserved quantity, and further that coupling within and between departments is weak at best (e.g. zeroing out ILC would not lead to a commensurate increase in cancer research).

4. **DB**
   May 13, 2009

   It's ironic. Just six months ago, Fermilab and the Tevatron was being written off, and the consensus was that it would be converted into a user facility in 2010. Since then we've seen substantial new funding from the new administration which seems to indicate a genuine HEP future for Fermilab beyond 2010, the serious problems at CERN and their subsequent mishandling by the PR department, the latest CDF/D0 limits on where the Higgs is likely to be found (if indeed it exists) which place it in energy ranges not so favourable for the LHC.

   Now come the Austrians and a new set of (as yet unconfirmed) rumours indicating that there are additional problems with some magnets and that the new CERN schedule may have to be further delayed.

   While I don't welcome any of these difficulties for CERN, one side-effect is a revitalisation the sense of competition and rivalry between US and European groups which has always been an important and healthy feature of HEP research.

   There is now a possibility, and it is by no means as remote as once thought, that the Tevatron will discover the Higgs before the LHC.

5. **Andrei**
   May 13, 2009

   I agree with DB here. The best science happens when there is healthy competition. I.e. pressure to perform. I realize most of the scientists there who are not really in it for the science but more for the cushy jobs are absolutely apoplectic about the new pressures. But in my view the pressure on CERN from a revitalized Tevatron and the threat of withdrawal are welcome and overdue. The only reason CERN got away with so many program failures was their perceived monopoly on physics. This is manifested everywhere where physicists take this high horse attitude: support the LHC or you do not understand this and that and the other. This pressure is a blowback that tells the high horse people either you are in the game and produce physics or let somebody else do it. LET THERE BE PRESSURE. PLANCK SCALE PRESSURE OF

POSSIBLE.

6. **jpd**
   May 13, 2009

   yes, einstein made his great breakthrough competeing with all those other patent agents

7. **karl**
   May 13, 2009

   Dear Andrei,
   I have spent several years at CERN, believe me, everybody there would
   be happy with feeling the scientific pressure of rival program to the LHC in the US! to blame the lack of this pressure and not to mention the continuing budget cuts CERN had to face over the last decade for some of the technological problems is an original ansatz.

8. **Peter Woit**
   May 13, 2009

   Andrei and respondents,

   Enough. I've deleted the last few comments here since they transmit no information, just anonymous hostility and abuse. Please stick to the topic at hand.

9. **Cormac O Raifeartaigh**
   May 13, 2009

   Hi Peter, re
   "After leaving CERN, Austria will still benefit from knowledge produced there, even if they are no longer paying for membership."

   This argument against joining CERN has been employed by government mandarins in Ireland for decades now. The result has been twofold: the decimation of research in experimental particle physics (an area that Ireland had a proud tradition in, from Walton to O Ceallaigh etc) and a feeling among european scientific partners that we are not pulling our weight…

10. **art**
    May 13, 2009

    re:jpd

    "yes, einstein made his great breakthrough competeing with all those other patent agents"

    it is relevant to point out that Einstein was in race to the finish with Hilbert to find field equations for general relativity.

11. **Pedro**
    May 13, 2009

    «it is relevant to point out that Einstein was in race to the finish with Hilbert to find field equations for general relativity.»

    And lost.

12. **neo**
    May 13, 2009

    This is not tragedy of the commons (the over use of a depletable resource) but the free rider problem (refusal to pay for a public good that benefits all). Sorry to be picky.

13. **Marion Delgado**
    May 14, 2009

    I was going to say what neo said.

    I can add one thing – free riders are a subset of the prisoner's dilemma.

    And prisoner's dilemmas are solved by either an outside coordinator or by negotiation over a series of transactions.

    The 2nd case is what Peter Woit is discussing.

14. **srp**
    May 14, 2009

    If it discovers the Higgs, CERN should claim a process patent on using the particle to create mass and then force Austria to pay up big if it still wants to have inertia.

15. **http://sos.teilchen.at/**
    May 15, 2009

    thx for putting up the link for the petition!

    I'm a physics student at the university of vienna and I have to say it's all just politics.
    They decided to leave CERN without asking anyone who would even know what a taylor series is…

16. **wolfgang**
    May 18, 2009

    It was just announced that Austria will *not* leave CERN and remain a member.
    Science minister Hahn could not convince the (larger) part of the coalition government, including the chancellor, of his plan.

17. **Peter Woit**
    May 18, 2009

Thanks Wolfgang,

Good news. I’ll add an update to the posting about this.

Feynman Diagrams and Beyond

May 16, 2009
Categories: Uncategorized

The Spring 2009 IAS newsletter is out, available online here. It includes the news that the IAS is stealing yet another physics faculty member from Harvard, with Matias Zaldarriaga moving there in the fall.

The cover story of the newsletter is called *Feynman Diagrams and Beyond*, and it starts with some history, emphasizing the role of the IAS's Freeman Dyson. It goes on to describe recent work on the structure of gauge theory scattering amplitudes going on at the IAS, emphasizing recent work by IAS professor Arkani-Hamed and collaborators that uses twistor space techniques, as well as Maldacena's work using AdS/CFT to relate such calculations to string theory. Arkani-Hamed (see related posting here) says he's trying to find a direct formulation of the theory (not just the scattering amplitudes) in twistor space:

> We have a lot of clues now, and I think there is a path towards a complete theory that will rewrite physics in a language that won't have space-time in it but will explain these patterns.

and explains the relation to AdS/CFT as:

> The AdS/CFT correspondence already tells us how to formulate physics in this way for negatively curved space-times; we are trying to figure out if there is some analog of that picture for describing scattering amplitudes in flat space. Since a sufficiently small portion of any space-time is flat, figuring out how to talk about the physics of flat space holographically will likely represent a real step forward in theoretical physics.

One IAS member who is also working in this area is Emil Bjerrum-Bohr, a great-grandson of Niels Bohr, and the newsletter has an article about him and the various members of the Bohr family who have been at the IAS at one point or another.

For one more piece of news related to Feynman diagrams, Zvi Bern et al. have a new paper out where they explicitly construct the four-loop four-particle amplitude, for N=8 supergravity, and show that it is ultraviolet finite in both 4 and 5d. This provides yet one more piece of evidence for the ultraviolet finiteness of N=8 supergravity. Remember all those claims made for string theory that it is the only way to tame the short-distance fluctuations of a quantum theory of gravity?

Update: One of the authors of the four-loop paper wrote to me with some comments about it, which he gave me permission to post here:

> I just wanted to point out what I see as two of the interesting things with this calculation:
>
> 1) Honest four-loop QFT calculations in (massless) gauge and gravity theories are now possible, if not exactly trivial. This isn't just "big fancy computers." Sure, computers help with the book-keeping of the calculation, but no computer in the world could have accomplished this by naively marching through Feynman diagrams (just look at the size of the expression of 3-graviton Feynman rule in your favorite gauge, and do vertex counting on the number of distinct graph topologies). Rather, this is due to advances in understanding how to manipulate lower-loop and tree-level scattering amplitudes to get (complete) higher-loop scattering amplitudes.
>
> To understand how powerful this is, consider the following: the construction of the four-loop four-point N=4 super-Yang-Mills amplitude required (as input) nothing more complicated than the Parke-Taylor expressions for MHV three-, four-, and five-gluon scattering amplitudes in four dimensions — not even requiring the (very nice) recursion relations for higher point trees mentioned in the IAS piece above. (Verification, of course, required more 🙂). If you've seen the Parke-Taylor expressions you'll know how simple they are! The construction and verification of the four-loop N=8 supergravity amplitude requires only knowing the four-loop four-point N=4 super-Yang-Mills amplitude.
>
> Even had we not gotten the nice result regarding the tame UV behavior, getting to the point where these types of calculations are doable is I think important in its own right, and possibly even more important in the long-run. I should probably point out that these types of approaches can and are being generalized to more physical theories, like the exciting high-multiplicity one-loop QCD work going on.
>
> 2) Maybe there's a perturbatively finite (point-like) QFT of gravity in 4D. This is exciting as it suggests that QFT could be a more powerful framework for describing the universe than people have been giving it credit for recently. We do believe that, if it is perturbatively finite, it will be so due to some previously unrecognized symmetry or dynamical mechanism that once understood should greatly improve our understanding of gravity. There does seem to be some connection with the very good scaling behavior of tree-level pure-graviton amplitudes in theories related to Einstein-Hilbert gravity.
>
> That being said, we really don't have anything to say about its non-perturbative behavior. Really. Nothing at all. It absolutely could require non-perturbative completeness from string theory. It could already be non-perturbatively complete in a way that's best described by a string theory in certain regimes (emergent string theory if you like). Maybe it only works with higher-dimensional invisi-pink elephants. I really don't know; it's not what we're after right now. I certainly encourage people to consider working on non-perturbative N=8 questions if they're curious!
>
> Not to be overly contrarian, but I wouldn't characterize any of this as a blow against string theory, and I don't think most string theorists see it as such. String theorists have, on the whole, been very supportive of this line of research (even if it might mean a small technical modification of certain sentences in the introduction of certain texts 🙂). Besides one of our collaborators (Radu) also being a practicing string-theorist, we've met with a lot of support from all sorts of people who appreciate calculation, and are honestly curious about the results. Besides, there have been very strong string-theorists actively working on understanding this from the string-side. In terms of community support, i.e. not just good individuals here and there, Zvi's been invited to talk at Strings '09, Lance talked at '08, and I think Zvi talked at '07 if I remember correctly.
>
> I, of course, can't help but flinch a little when people glibly say string theory is the only way to talk about gravity (which is manifestly wrong, e.g. the CFT side of the AdS/CFT *duality*). Most thoughtful string-theorists I've met who say something similar, however, are using it as a shorthand for a much more long-winded statement which is accurate. Namely they're compressing a statement regarding the level of understanding we've gained about gravity and gauge theories and non-perturbative solutions through string-theoretic analysis, which we haven't from anywhere else. As we can see by my comment here, there are perils to giving in to long-windiness, so I tend to refrain from giving them too hard a time about it. There is trouble of course when similar statements are mindlessly parroted by the thoughtless, but the thoughtless tend to generate grief generically in any case.
>
> John Joseph M. Carrasco
> http://www.physics.ucla.edu./~jjmc/

Comments

1. **bla**

May 20, 2009

no comments? this N=8 SUSY being finite idea looks interesting, I’d appreciate more info on this. Is it really a blow to string theory?

2. **Peter Woit**
   May 20, 2009

   bla,

   Evidence for the finiteness of N=8 supergravity has been around for a few years now, I first wrote about it here:

   http://www.math.columbia.edu/~woit/wordpress/?p=268

   One reaction to this possibility from string theorists is to argue that N=8 supergravity has problems non-perturbatively. Another is to basically just ignore all evidence that there are QFTs with sensible perturbative expansions and keep on repeating the argument that “string theory is the only known way” to get a finite theory of quantum gravity.

3. **Vince**
   May 20, 2009

   For more information on this topic and its relations to string theory, look at these posts:

   http://motls.blogspot.com/2006/10/finiteness-of-supergravity-theories.html
   http://motls.blogspot.com/2007/04/decoupling-n8-d4-supergravity.html
   http://motls.blogspot.com/2008/04/n8-supergravity-lance-dixons-puzzle.html
   http://motls.blogspot.com/2008/07/two-roads-from-n8-sugra-to-string.html

Why Colliders Have Two Detectors

May 20, 2009
Categories: Experimental HEP News

Last year the D0 collaboration at the Tevatron published a claim of first observation of an Ω_b particle (a baryon containing one bottom and two strange quarks), with a significance of 5.4 sigma and a mass of 6165 +/- 16.4 MeV. This mass was somewhat higher than expected from lattice gauge theory calculations.

Yesterday the CDF collaboration published a claim of observation of the same particle, with a significance of 5.5 sigma and a mass of 6054.4 +/- 6.9 MeV.

So, both agree that the particle is there at better than 5 sigma significance, but D0 says (at better than 6 sigma) that CDF has the mass wrong, and CDF says (at lots and lots of sigma..) that D0 has the mass wrong. They can't both be right…

For a detailed discussion, see here, here and here.

Comments

1. **ObsessiveMathsFreak**
   May 20, 2009

   5.4 sigma is pretty accurate (~0.999999 probability of data being within this many standard deviations of the mean if my statistics is still correct).

   But how many of these experiments do they have to run in order to find a particle again?

2. **Dmitry**
   May 20, 2009

   Hi,

   I am from CDF, so I am biased.
   But: D0 result cannot be right for two reasons:
   1) they observe relative production rate of Omega_b/Xi_b to be almost 1 (0.8). Normally you expect a penalty of about 1/10 in production rate for additional s-quark. A picture which quite consistent across many experiments for different species (Xi_b/Lambda_b), (Xi_c/Lambda_c), (Omega_c/Xi_c).
   2) The mass they measure for Omega_b is way off of Xi_b (expect difference to be ~0.2GeV) which is again is observed in other systems (B_s vs B, Xi_b vs Lambda_b, Omega_c vs Xi_c, Xi_c vs Lambda_c)

   There is third, reason – theoretically these states are studied very well and there is no really a wriggle room for the Omega_b mass. It is pretty firm 6.05 +/- 0.01 GeV. If observed mass is significantly different it means tons to Heavy flavor physics. This means that HQET does not work for baryons – a statement which is a very bold statement since HQET has been a very precise tool in describing property of heavy flavors so far!

   (1) and (2) make D0 observation a very extraordinary claim and therefore require extraordinary scrutiny.

   CDF has just provided this scrutiny by performing a simple cut based analysis on a sampe which is almost 4 times bigger than one used by D0. Nothing in the claimed region and a nice peak in the anticipated region with good precision matching theory expectations.

3. **Dmitry**
   May 20, 2009

   Also: people should not get fooled too much by > 5*sigma claims. There have been way too many cases so far of > 5sigma claims that dissolved or failed to be reproduced. Independent confirmation is the only way to really establish am effect.

4. **zanzibar**
   May 20, 2009

   Dmitry says:

   "(1) and (2) make D0 observation a very extraordinary claim and therefore require extraordinary scrutiny. "

   What is the effect of *not* subjecting all measurements to the same "extraordinary scrutiny". A bias towards orthodoxy?

5. **a quantum diaries surivivor**
   May 20, 2009

   Hi all,

   first of all many thanks to Peter, who is always very generous with links to my site. At least I can say I bought him a beer already… I hope I will have a chance to buy him a dinner another time, although he'll probably try to fight for the check.

   Second, Dimitry is right, but there are more reasons, experimental ones, to say that the DZERO result is unfortunately wrong this time.

   1) First of all, the DZERO significance is computed by taking the probability of the -2 log (L_s+b/L_b) variation between the likelihood of a fit with signal and background to a likelihood with background only. Their signal has variable amplitude AND mass in the fit, so the s+b fit has TWO degrees of freedom more than the background-only one, but they compute the significance as if the delta log L distributed as a chisquared with ONE degree of freedom. Their true significance is 5.05 sigma, not 5.4 as mentioned in the paper.

   2) second, the systematic part of the mass uncertainty in the CDF mass measurement is below one MeV, the one of the DZERO mass measurement is more than tenfold. This means that if one of the two experiments got the mass wrong, it must be DZERO, since the statistical uncertainty is well-measured in both cases, and the two measurements are totally inconsistent. To be clear, if you had to inflate the systematics of one of the two experiments with a k-factor as the PDG does when they get their averages, you would have to inflate DZERO's with a K=5, or CDF's with a K=60. Your pick.

3) third, CDF analyzed three times more data. If the rate measured by DZERO were right, CDF would have seen more than thirty events of signal in its dataset, in a sample which counts 35 (where CDF measures 12 of signal). This is utterly unlikely. If, instead, the CDF rate is right, then DZERO would have seen only five or six events in their data due to Omega_b production. They saw more, and that is not a too unlikely fluctuation.

4) then theory agrees with CDF, disagrees wildly with DZERO both in rate and in mass. This of course can only be taken in consideration after all the rest.

Best,
T.

6. **Dmitry**
May 20, 2009

Hi Tomasso,

I am not in argument with you, just my observations:

1) 5.05*sigma vs 5.4*sigma is really a nitpicking.

2) you assume that both experiments see the same particle and ask yourself who could be wrong on its mass CDF or D0. But given > 6.5 sigma discrepancy between CDF and D0 we can be sure they see *different* particles. So the question on who got Omega_b mass wrong is kind of irrelevant.

3) Rate is the killer argument. that put a big question over the D0 measurement. Huge rate and extraordinarily high mass put a big question on their result.

CDF of course looked for Omega_b all along even back in fall of 2007 (with 2.2 fb) . There was empty space in D0 mass range and something promising at 6.05 which was decided to be left alone until we had more data and therefore significant signal.

The real interpretation of CDF result:
– discovery of Omega_b
– ruling out of structure @ 6.110 GeV previously reported by D0

Given discrepancy with expected mass D0 must have called their work not a "First Direct Discovery for Omega_b" but "Evidence for a new particle in Omega, J/psi mass spectrum".

They justified that what they see is Omega_b by taking a broad sample of theoretical works going back 20 years. Where you may indeed find someone who thought that its mass could be 6.120. But since then the predictions have narrowed down significantly to 6.05 +/- 0.01 (based on input from measured states like Lambda_b, Sigma_b, Xi_b). Having assumed that this is expected particle they automatically overestimated the significance of the signal.

7. **Dmitry**
May 20, 2009

– ruling out of structure @ 6.165 GeV previously reported by D0

I put wrong mass there….

8. **Dmitry**
May 20, 2009

and last post: The cool thing about CDF analysis is that the Omega hyperon (for Omega_b search) was actually tracked in silicon allowing for precise Omega_b vertex determinations and cutting down on combinatorial background. This is relatively sophisticated technique that provides high purity sample of Omega combined with pretty simple cut based analysis later on gave CDF enormous confidence in this result.

9. **a quantum diaries survivor**
May 23, 2009

dmitry, 5.05 differs from 5.4 by a factor seven in probbility! nit-picking? maybe, but enough to fire the PRL reviewers, if you ask me!
cheers,
T.

10. **Dmitry**
May 23, 2009

It is nitpicking cuz it does not do anything to prove that the result is wrong. Maximum and errata could be issues – "oops, this is 5.05 not 5.4 sorry, but still > 5., so we are cool".

My point is that D0 Results is Not Even Wrong (what a proper board we are discussing this stuff!), so poking small holes in it is waste of time.

The production rate and mass value measured by D0 are so outrageous, as to should they be true we should be throwing Standard Model away!!! This is what is implied by D0! This is like all of a sudden someone tells you that 2+2 is not 4 anymore but 5 (not even 4.000000001), this is how badly it is violated. I am surprised than none pointed this out. As soon as I learned that they got almost the same strength signal as Xi_b in the same size sample I told our B-group conveners – be cool, this is rubbish and stopped paying attention.

One cannot call Omega_b something that does not look like Omega_b.
As soon as you remove that condition, and estimate probability of getting 17 events above background in any random place in wide mass range you will go below 5 sigma immediately.

In this view, I recommend you to peruse D0 Xi_b observation, which I still don't quite believe (so shell-shocked I was when I learned that we got scooped in 2007 – myself and Pat were authors of CDF analysis) .
Note, their Xi_b/Lambda_b production rate is 2 times larger than ours (granted with errors big enough as not to cause a stir). Fluctuation?
Does D0 thrive by fluctuations? B_s mixing infamous "first ever double sided limit"? First singe top 3 sigma evidence? Xi_b was lucky fluctuation (at the "right mass") and Omega_b was unlucky one. Payback time 🙂

11. **a quantum diaries surivivor**
May 25, 2009

Hi Dmitri,

I take it that you did not read my last post on the matter, which shows conclusively what you are arguing, from a statistical standpoint.

I however insist: the error in the D0 PRL had to be spotted by PRL reviewers. They get poor grades for that.

Cheers,
T.

12. **Dmitry**
May 25, 2009

Tomasso,

Lets be simple. D0 has signal S = 17.8 on top of background B=1.75*6. =10.5 (looking by eye at their plot).

converting probability of no signal to give signal into Gaussian significance is roughly the same as calculating:

significance = S / sqrt(B) = 17.8/sqrt(10.5) = 5.5 sigma

So I, personally, do not really doubt their figure for significance. Or rather I do not put *too much emphasis* on it. You probably know statistics better. IMO ratio of likelihoods is not a probability to get signal from background by background fluctuation even if you recast this ratio to look like chi2 difference.

But *I do not want to argue this*. Read my post. You're right, they should have put 5.05 sigma in paper. Whatever.

I shoot in different direction – mass is off from theory by at least 4.4 sigma (even inflating theory error by at least 1.5). So according to this the probability that what D0 was seeing Omega_B is
Erf(4.4/sqrt(2))=1.1e-5. This is low. Given that, and very high relative production cross-section (w.r.t to Xib) I would (If I was on D0 editorial board) have required more data to be analyzed, just to be sure. A valid demand, given that by 2008 D0 has twice more data compared to the sample they used to publish their Omega_b observation. May be they would have found it in the same place where CDF found it, then!

Cheers,
Dmitry

Various and Sundry

May 21, 2009
Categories: Uncategorized

HEPAP is meeting in Washington today, talks starting to become available here. Things are very different now than in past years, with huge budget increases for all areas of HEP at the NSF and DOE.

FQXI has awarded quite a few mini-grants, the list is here. They also have a new essay contest, on the topic What is possible and impossible in physics?

Some worthwhile expository mathematics pieces:

Motives—Grothendieck's Dream
The Theory of Witt Vectors

At least one mathematician is a viscount and has a coat of arms.

Witten has a new paper on the arxiv entitled Geometric Langlands From Six Dimensions, an expository account of a rather special 6d superconformal theory and how its existence implies SL(2,Z) symmetry of N=4 SYM, and thus duality in geometric Langlands theory. He remarks that there isn't a widely used name for this theory, calling it the "six-dimensional (0,2) model of type G".

This week there's a workshop on Topological Field Theories going on at Northwestern, with David Ben-Zvi lecturing on *Topological Field Theory, Loop Spaces and Representation Theory*. I hope he'll soon follow his standard practice of putting notes up on his web-site.

Tomorrow I'll head up to Cambridge for the weekend, to visit my brother and his family and to attend the Perspectives in Mathematics and Physics conference being held in honor of Is Singer's 85th birthday.

Update: Notes from the Northwestern workshop are available here (from Evan Jenkins) and here (from Alex Hoffnung).

Update: David Ben-Zvi has posted notes from his talk and some others at the Northwestern workshop here.

Comments

1. **Evan Jenkins**
   May 21, 2009

   I've been putting up handwritten notes for the Northwestern TFT workshop here. Should my handwriting prove unbearable, Alex Hoffnung has been typing notes, which are available (albeit currently without drawings of pairs of pants and the like) here. Someone has also been recording audio, but I'm not sure how good the quality is or when it will be made available.

2. **Peter Woit**
   May 22, 2009

   Thanks Evan, that's great.

3. **H-I-G-G-S**
   May 22, 2009

   To maintain your reputation for being fair and balanced you might want to add the following to your description of the (0,2) theory.

   "Until relatively recently, it was believed that four was the maximum dimension for nontrivial (nonlinear or non-Gaussian) quantum field theory. One of the surprising developments coming from string theory is that nontrivial quantum field theories exist up to (at least) six dimensions. "

4. **Cplus+**
   May 23, 2009

   J. Freese in a rather technical article on predictive promiscuity in deductive applications has named "vampirical hypotheses" those which can not be killed by mere evidence.

   The phenomenon extends beyond string theory.

5. **Alex Hoffnung**
   May 24, 2009

   Regarding notes from the TFT workshop, several people are working on some version of notes/reports from the Lurie and Ben-Zvi lectures in a more expository form than they currently exist (also with pictures). I hope to have these up soon.

6. **Hendrik**
   May 25, 2009

   You may interested in the thesis of Patrick Costello (here) which just appeared on the ArXiv regarding the mathematical aspects of quantum BRST. Its focus is on the functional analytic aspects of current quantum BRST-methods, in particular the Kugo-Ojima approach (although there is also some analysis of the Hamiltonian approach).

7. **Peter Woit**
   May 26, 2009

   Thanks Hendrik

   That's very interesting. There clearly are a lot of remaining mysteries in the BRST story…

8. **David G.**

May 31, 2009

Dr. Abhay Ashtekar's 60th birthday conference 'Abheyfest 2009' is next weekend. Anyone attending?

Latest on the LHC

May 28, 2009
Categories: Experimental HEP News

The Resonaances blog has a report from Planck 2009 on a talk about the status of the LHC. The slides of the talk explain the problems with training quenches that have necessitated initially running the machine at 5 TeV per beam instead of the 7 TeV design energy. They also explain the analysis of what caused the accident last September: bad soldering of the interconnections between copper bus-bars connecting the magnets.

There has been an ongoing campaign to check the quality of the interconnections by careful measurements of the resistance, with slide 44 noting:

- Ongoing race to identify and repair faulty joints.
- Unfortunately poor quality joints are localized in many places – likely to slow down progress with the machine re-commissioning.

It remains unclear exactly how many joints will have to be opened up and repaired, and what impact that will have on the re-commissioning schedule. While this remains to be decided, the latest draft schedule I've seen has about 1-2 weeks of slippage from the current official schedule, with powering tests on all sectors not finished until the first week of October, whereas the official schedule now envisages first circulating beam the week of September 21. The Planck 2009 talk just says "Beam commissioning scheduled to resume in September or October 2009".

For some misinformation about the LHC schedule, see here.

Comments

1. **roland**
   May 28, 2009

   "It remains unclear exactly how my joints will have to be opened up and repaired"

   Get well soon!

2. **woit**
   May 28, 2009

   Thanks roland, my joints are now fine and the posting is fixed…

3. **ANdrei**
   May 29, 2009

   So what is the difference between "beam commisioning" and "circulating beam.?

4. **Peter Woit**
   May 29, 2009

   Andrei,

   My understanding of the terminology is that beam commissioning starts when they first try and circulate a low energy (450 GeV?) beam around the ring. They got this far last year. From there, one has to get stable circulating beams in both directions at high energy (4-5 TeV), get them to collide, and get the luminosity in each beam up to a usable level. This is scheduled to take a month or so.

Singer Birthday Conference

May 28, 2009
Categories: Uncategorized

Last weekend I was up in Cambridge attending the conference in honor of Is Singer's 85th birthday. Singer has had a very long and distinguished career in mathematics, much of it at MIT, where he arrived as one of the first Moore instructors back in 1950. Besides a wide range of purely mathematical contributions, Singer was responsible for bringing together mathematicians (including Atiyah) and physicists starting back in 1976, at first around questions related to instantons. He has run a joint physics and mathematics seminar for about a quarter century, at Berkeley while he was there, then back at MIT. Unfortunately, this past year will have been the last year of the seminar, partly due to Singer's imminent retirement, partly due to a shift in the interests of Boston area physicists towards phenomenology and away from mathematics.

Jim Simons, an old friend and student of Singer's, played an important role at the conference, as master of ceremonies at the dinner, and as a financial backer. Back in 1975 it was his lectures to physicists at Stony Brook that got Yang and ultimately Singer interested in the question of the relation of gauge theory to geometry.

While in Cambridge, I picked up a copy of a new book, Recountings, which has interviews with many MIT mathematicians (including Singer), and does a good job of portraying the history of the MIT math department over the past 50 years or so.

Of the conference talks I managed to get to, probably the best was that of Mike Hopkins, who gave a blackboard talk about the Kervaire invariant problem. This one was a lot more accessible than his talk last month at the Atiyah80 conference, where he unveiled his dramatic new results with Hill and Ravenel (more about this story here). In the MIT talk, Hopkins concentrated on explaining the background and significance of the problem, as well as giving some of the philosophy of the proof, which uses what he describes as a "designer" cohomology theory.

Some quick notes on a few of the other talks that I made it to:

- Atiyah described some of the history of how Singer got him interested in physics, then went on to promote a very speculative idea about a non-local version of the Dirac equation.
- I can't say that I really understood Polyakov's talk, but it was along the lines of this.
- Cumrum Vafa talked about his local F-theory models and attempts to understand the hierarchy of particle masses this way.
- Michael Douglas gave a rather odd blackboard talk: no equations, no math, just pretty much straight promotional material about the philosophy of the landscape.
- Richard Kadison mostly reminisced about working with Singer, leading into a description of what is known as the Kadison-Singer problem.
- Greg Moore talked about work with Dan Freed and Jacques Distler. You can see their versions of the talk here and here.
- Wati Taylor gave another landscape talk, similar to the one discussed here.
- Orlando Alvarez gave a talk about work with Paul Windey, based on this paper.

Comments

1. **Barry Cunningham**
   May 28, 2009

   I just finished Recountings a few weeks ago and really enjoyed it. I attended MIT from 1966-1971 as an undergraduate. I was fascinated to get some insight into what was really going on then and learn some more about some professors I knew (or briefly met), both those directly interviewed (Hartley Rogers, Ken Hoffman, Harvey Greenspan, and Arthur Mattuck) and those they mentioned (e.g., Frank Peterson, George Whitehead, Harvey Friedman, Gerald Sachs, Dave Benney, Dirk Struik).
   I know! Just another old guy reminiscing.

2. **Thomas R Love**
   May 29, 2009

   Sorry to hear that the Physics-Mathematics seminars will end with Singer's retirement. I attended 4 quarters of the seminar when it was a Berkeley and it sparked my research on geometry and physics. I left when he started with supersymmetry.

3. **Walter Mondale**
   May 29, 2009

   It would appear that you are a "math groupie".

4. **Thomas**
   May 30, 2009

   Exist texts of Atiyah's and Hopkin's talks?

5. **Peter Woit**
   May 30, 2009

   Thomas,

   The talks were being recorded on video, I don't know if and when this will be publicly available.

6. **Andrew Ranicki**
   May 31, 2009

   Videos of the talks at Atiyah80 are now available from http://www.maths.ed.ac.uk/~aar/atiyah80.htm

7. **Peter Woit**
   May 31, 2009

   Thanks Andrew,

   I'll add a link to the posting about that conference.

Role Reversal

May 29, 2009
Categories: Multiverse Mania, This Week's Hype

It used to be that *New Scientist* had somewhat of a reputation for publishing misleading articles about speculative physics, and *Science News* was a more stodgy but reliable publication that stuck to serious physics. Recently there has been a role reversal. *New Scientist* is running a long, relatively sensible article about the use of AdS/CFT methods in condensed matter physics, entitled [What string theory is really good for]. It avoids the usual “String theory finally makes predictions!” hype that some string theorists have been trying to promote. *Science News* on the other hand, is now being run by Tom Siegfried, who is quite a fan of string theory hype, the more speculative the better. Last month was [Strings Fight Back] at *Science News*, this week it’s multiverse madness, with a cover story on [Infinity], which promotes the latest multiverse/Boltzmann Brain pseudo-science. Towards the end of the article, David Gross is allowed a few words as skeptic, arguing that we don’t understand string theory, so can’t be sure it leads to this mess: maybe some missing insight will get string theorists out of it. Siegfried responds with the thought that the “missing insight is merely realizing the need to master the inconveniences of infinity to resolve the cosmic conundrums.”

Update: The New Scientist article makes it to [Slashdot] where, as usual, it gets transformed into nonsense:

> His theory states that the known universe is only a 2D construct in anti-de-Sitter space, projected into 3 dimensions.

Comments

1. **Russ**
   May 29, 2009

   This guy Tom Siegfried apparently actually has some amount of training in chemistry and physics. You wouldn’t know it, through, from such statements as “Such calculations encounter a major impediment, though: To test whether the universe is the way it is because it’s a good place for men and women to be born and die, scientists must learn how to cope with infinity.”, and of course the gem from the end of the article that Peter quoted above.

   Man, I hate science journalists.

2. **woit**
   May 29, 2009

   Russ,

   I think it’s important to remember that journalists like Siegfried don’t just make this stuff up. The multiverse/Boltzmann Brain business, together with the idea that it is achieving some fundamental breakthrough in physics by thinking about infinity was fed to him by reputable physicists.

No Landscape and No Math in Rome

May 29, 2009
Categories: Uncategorized

Strings 2009 is about three weeks away, and it will bring 450 or so string theorists to Rome. The topics of the talks at the Strings 200x conferences give a good idea of what the hot topics in the field are, and this year's talk titles are now available. What's big this year are scattering amplitudes, as well as the usual AdS5/CFT4 topics, supplemented by the more recently popular AdS4/CFT3. As far as phenomenology goes, the hot topic is definitely local F-theory models, with three separate talks on the subject.

One topic that is not hot is anything mathematical, with no research talks by mathematicians or Witten, and little about mathematically significant topics such as mirror symmetry. What also seems to no longer be hot is either string cosmology or the landscape. No cosmology, multiverse or Boltzmann Brains are to be found among the research talks, although Brian Greene will give a public lecture about the issue of possible multiple universes.

Comments

1. **gues**
   May 30, 2009

   So what about Horava gravity theory? TBC ? when confirmed?

2. **David Ben-Zvi**
   May 30, 2009

   Hi Peter – Three talks that stand out to me as mathematically significant are those by Gaiotto, Nekrasov and Ooguri (not to say others aren't, these are closer to things I've been exposed to..). The math of N=2 d=4 gauge theories continues to be very inspiring, with new relations to representation theory, algebraic geometry, Hall algebras, Donaldson-Thomas invariants etc emerging recently, and these three speakers are very much in the forefront of the subject and influencing many mathematicians from what I can tell.

3. **MathPhys**
   May 31, 2009

   I thought the talks by Antoniadis and Narain on their work on topological amplitures would be primarily of mathematical interest as well.

4. **Peter Woit**
   May 31, 2009

   Hi David and MathPhys,

   I was of course exaggerating the point with both "No Landscape" and "No Math". Just as some of the talks surely contain material of use to landscapeologists, the ones you mention will contain material of real interest to mathematicans. But, still, just as none of the talks are primarily about the concerns of landscapeologists, I don't see any of the talks as having primarily mathematical interest, and suspect that relatively few mathematicians will be able to get something out of them. Looking through the 400+ long participant list I only saw a couple people I recognize as working in math departments.

   In any case, I tend to believe in the existence of a large area of overlap between math and physics, making putting things in one or the other category often impossible. My perception though is that, while string theorists have in some past periods been interested in reaching out to and interacting with mathematicians, that is less true recently.

   David's comment about the mathematical significance of recent work on N=2 d=4 gauge theories is a good case in point. I'm sure he's right, but also suspect that he's one of a very small number of mathematicians able to appreciate these connections. I hope he'll continue to do a great job of explaining this to me and others.

5. **no**
   June 12, 2009

   wow, No Chinese either.

Interview With Simons and Yang

June 1, 2009
Categories: Uncategorized

Steve Miller pointed me to a fascinating interview with Jim Simons and C. N. Yang, available on YouTube here.

Simons tells the story of how he got kicked out of his job at the IDA in 1968 over his opposition to the Vietnam War, and ended up at Stony Brook as chair of the math department there. He and Yang collaborated on raising money to support anti-war efforts.

They describe how Yang went to Simons to try and find out about fiber bundles and what they might have to do with gauge theory. Simons started by referring Yang to Steenrod's *The Topology of Fibre Bundles,* which Yang couldn't make any sense of (Simons admits he never made it all the way through the book himself). This did in the end lead Simons and Yang to some real understanding of how vector potentials in gauge theory and connections on bundles were the same thing, with monopoles examples of topologically non-trivial bundles. Simons lectured at Stony Brook in 1975 on this, and a paper later that year by Wu and Yang included what became known as the "Wu-Yang dictionary" relating terminology in gauge theory and geometry. Singer learned about this soon thereafter when he visited Stony Brook, and went on to spread the news to Oxford, MIT and elsewhere.

Simons also describes what is going on with plans for the new Simons Center for Geometry and Physics, including some of the thinking that led him to decide to support this. The official ground-breaking ceremony for the new building there was held last week, you can follow construction progress here.

Comments

1. **Rene Meyer**
   June 11, 2009

   Well, it might have been different in 1968, but certain recent comments by C.N. Yang on what a great achievement of "Chinese science" the mainland chinese development of the atomic bomb actually was let me doubt that Mr. Yang always was a genuine pacifist…

2. **Peter Woit**
   June 11, 2009

   Rene Meyer,

   I have no idea what statement of Yang you allude to, or what his opinions are about nuclear weapons, either US or Chinese. What was discussed in the interview was not pacifism, but opposition to the Vietnam war in the late 1960s. Simons explicitly says that he is not a pacifist, would have supported the involvement of the US in WWII. I assumed the same of Yang, but don't know anything about his views on this, or on politics in general.

3. **Rene Meyer**
   June 11, 2009

   The comments of Yang are to be found here in English language:

   http://english1.people.com.cn/english/200004/12/eng20000412_38790.html

   I have to say that I read an even stronger statement in chinese language somewhere at some news portal, but I cant find this statement right now. Now since the People newspaper is the official mouthpiece of the CCP, I can not guarantee that the atomic bomb was not added to the there-presented list due to a suggestion of the editor or who-knows whom, just to show the necessary amount of patriotism. The chinese are generally very proud of being a nuclear power. However, if he really thinks himself that developing weapons of mass destruction is a great achievement of science and technology, in my mind his moral integrity as a scientist has to be questioned. After all, it would probably have been better if nobody had invented those things. But maybe it was just a mis-represented statement of an old man, who knows.

4. **Peter Woit**
   June 11, 2009

   It seems to me that Yang was just noting that development of nuclear weapons requires some significant scientific and technical expertise, while making no comment about the morality of such weapons. He could be dedicated to the abolition of nuclear weapons or support their development, I don't see anything he had to say addressing this.

   Again, I don't know what his political views are, and get the impression that, whatever they are, he tends to keep them to himself. The Vietnam War story surprised me a bit, but then the late 60s were an unusual time. The issue of the war was tearing the US apart, and it would have been difficult to not take a position on it.

5. **Jeremy**
   June 19, 2009

   The interview is indeed fascinating. Thanks for the link, Peter.

The Music of the Superstrings

June 1, 2009
Categories: Uncategorized

String theorist Oswaldo Zapata continues (see here for an earlier posting about this) his remarkable series of essays about string theory and how it came to dominate research in theoretical high energy physics. The latest one, entitled The Music of the Superstrings is about the metaphorical use of classical music to promote superstring theory, and it concludes:

> Metaphors are powerful rhetorical tools. But, at the same time, they are much more than that. Indeed, when used astutely, that is, when anchored in deep shared meanings and aspirations, they can create an enthusiastic army of supporters to the discourse displayed. This has been one of the strongest weapons of string theorists in the battle for the control of future research in high energy theoretical physics.

Zapata examines how and why string theorists have chosen to advertise string theory to the public by claiming a deep connection to music, especially to classical music. He recalls the many ways this analogy has been promoted by many different string theorists, from Brian Greene, who has made it a prominent part of his popular explanations of the theory, to Edward Witten, who told an interviewer in 1988:

> In the case of a violin string, the different harmonics correspond to different sounds. In the case of superstring, the different harmonics correspond to different elementary particles.

I've always found this kind of thing grating, for a reason that Zapata doesn't address. Statements like Witten's give people the impression that the known fundamental particles of nature somehow correspond to the harmonics produced by vibration of a string. This is rather misleading, since all known particles correspond to the lowest energy state of the string. The quantum states corresponding to "harmonics" of a string are all supposed to be at unobservably high energy. The way the theory is sold to the public, via the musical metaphor that electrons and muons are different "harmonics" of a string vibrating at different frequencies makes it seem that such particles can be matched to the characteristic behavior of the harmonics of a string-like mechanical system, which is simply not true.

Zapata also now has a Reactions page, where he has posted links to commentary about his essays, as well as some comments from Bert Schroer in a recent preprint.

Comments

1. **Matt Leifer**
   June 1, 2009

   Personally, I've always found comparisons to classical music rather pretentious and off-putting. If you want to get me interested in a subject then you would be much better off comparing it to punk rock 🙂

2. **jpd**
   June 1, 2009

   paging Mr. Ramone….Mr. Joey Ramone

3. **piscator**
   June 1, 2009

   >This is rather misleading, since all known particles correspond
   > tothe lowest energy state of the string. The quantum states
   >corresponding to "harmonics" of a string are all supposed to be
   >at unobservably high energy.

   um, in string theory gravitons and gauge bosons are harmonics of the string. the ground state with no oscillators is tachyonic.

   piscator

4. **woit**
   June 1, 2009

   piscator,

   Yes, I know. My point is that the way this is advertised to the public is misleading: the different notes coming from the different harmonics of a vibrating string in a stringed instrument do correspond in a quantized theory to different sets of states. But those different states are not the known different particles.

   Put differently, string theorists go on about an analogy with the musical notes in Bach string concertos, failing to mention that, as far as their strings are concerned, there's only one note.

   You can argue that this is justifiable poetic license, but, as Zapata explains, it is license taken in the cause of creating "one of the strongest weapons of string theorists in the battle for the control of future research in high energy theoretical physics."

5. **Tom Whicker**
   June 1, 2009

   Yes, grating is the word for it!

   The vibrating string metaphor really falls apart if you consider the internal forces involved in a real vibrating string. Do quantum strings have an internal structure that gives them a characteristic bulk modulus? If so, then the string is certainly not
   a fundamental particle. And in the quantum world, what provides the needed connections to each end of the string so that it may be tensioned? I seem to recall that Randall spoke of strings being anchored at one end to a brane….but if that is so, then how
   do strings move freely as particles?

   One thing that made Greene's "Elegant Universe" so hard to watch was the repeated use of bad metaphor. For instance, ants walking on the surface of a long piece of wire says nothing of substance regarding "compactification of dimensions".

How about a full issue of the blog dedicated to bad (and good, if they exsit) string metaphors?

6. **Hendrik**
June 1, 2009

In response, some classical composers have taken up strings (in the physics sense), e.g. here. Maybe it has to do with all the hype about beauty.
NewScientist is back onto strings too.

7. **Bert Schroer**
June 2, 2009

The metaphor to a musical string is actually correct, but in a very queer unintended sense. But the string is not in spacetime, it rather "sits" in the little Hilbert space "above" one point i.e. at the place which normally is reserved for the spin indices. And in some contorted way a small number of string theorists (Martinec,…) know that, but by the time they made this observation the metaphor of "those little wiggling strings" was already so powerful within the community that they called it an "invisible string" of which only one point is visible thus deriding decades of year of foundational research on quantum theory, including the work of Bohr and Heisenberg (who are left to rotate in their graves).
String theory can of course not be corrected by correcting the terminology and calling those objects what they really represent namely the first illustration of infinite component fields or wave functions (which Fronsdal, Barut, Kleinert,..in the 60 looked for in vain). One really needs the incorrect metaphor and not the real thing in order to make sense of those Feynman-like tube pictures which leeds into one of these abominable "catch 22" problems.
Question: do you think that there ever will be a description of those tube rules in terms of operators in a Hilbert space? This is what every object in QT admits, in the entire quantum theory there has been no exception to this rule.

8. **robert**
June 2, 2009

Witten's off piste lingusitic abilities are well known; I've never heard mention made of his musicianship. Is he an accomplished performer or avid listener, by any chance?

9. **Giotis**
June 2, 2009

Peter, my feeling is that you are exaggerating on this one. They just wanted to convey the basic idea to the public. Such pictorial representations are often used by scientists. I don't think that their intention was to mislead anyone and I don't see how an intuitive image intended for the public, could possibly influence the trends in theoretical physics.

10. **Peter Woit**
June 2, 2009

Giotis,

The problem is that it doesn't actually convey the basic idea to the public very well, but gives them a wrong idea about what is going on. While I think this is misleading, I don't think that's the intention.

As for whether I'm exaggerating: the fact is that I thought about writing something on the topic in my book about the problems with the musical metaphor, decided it wasn't important enough to write more than a sentence about. Note that this posting is about Zapata's essay, he's the one arguing that this kind of misleading metaphor has influenced trends in theoretical physics.

11. **Giotis**
June 3, 2009

I see. I misinterpreted then.

12. **Bert Schroer**
June 4, 2009

Giotis and Peter
You both came very close to what what is the core of the string problem. It is often said that a pointlike free quantum field is a collection of oscillators. This is a somewhat perilous characterization because no student who knows the QM of the oscillator would be able to reconstruct a free field from this indication. The most important aspect of the quantum field is its causal localization in one spacetime point; if it is a free field one most add the information that it obeys a specific equation of motion. From these (or similar formulated) data one can uniquely construct a free field.
The vibrating oscillators are then the Fouriercomponents of the pointlike field, but it is physically clearer to make the linear decomposition into creation/annihilation operators a(p), a(p)*. In every state, in particular in the vacuum state, there are fluctuations of these oscillators. Most aspects of these oscillators are unphysical, but certain fluctuations have physical consequences. For example the fact that the sum (integral) of all the vacuum fluctuations diverges is not of direct physical relevance, it is equivalent to the fact that the field A(x) is not an operator but a more singular object namely an "operator-vaued distribution" with excellent mathematical credentials.
As far as localization in the material sense is concerned, the field A(x) is localized in one point and there is no trace of the wiggling from the oscillators, the Wiggling comes when you look at expectations of products A(x_1)…A(x_n) in states (the most important state being the vacuum).
Behind these remarks there are two very different concepts of localization namely the Born localization of QM (in the relativistic setting the Born-Newton-Wigner localization) which comes with wave functions and the causal localization which in the above example is the point x i.e. independent of any state in which the local observable is studied. Nobody in his right mind would say that the oscillator wiggling in states has anything to do with the localization of an observable. The causal localization concept has a completely intrinsic meaning, there is a new mathematical theory which allows to liberate this meaning from the contingency of what field-coordinatization one is using: modular localisation.
What are Nambu-Goto strings or their supersymmetric counterparts? They turn out to be infinite component pointlike wave functions/fields. String theorists know that they only use a different name they call them (those who looked at the localization aspect) "invisible strings" or "strings of which only one point is visible". Why? because at the time they realized this the string mataphor was already so strong that they coud not resist the suck which they themselves created. What helped this incorrect metaphor was the fact that the classical Lagrangian is indeed that of a string. But all the work of Bohr Heisenberg … was done to liberate QT from false analogs to classical physics.
Where did all the oscillator degrees of freedom go which correspond to the classical string? They went into the rich infinite components i.e. they are in the inner space which metaphorically one likes to picture "above" the localization point (see my previous remark). There is nothing, I repeat, there is absolutely nothing which resembles a wiggling string in spacetime.

13. **Joe Dimaggio**
June 4, 2009

"This is rather misleading, since all known particles correspond to the lowest energy state of the string."

In other words… the string-music analogy falls flat.

You have to love my triple entendre 🙂

14. **Bert Schroer**
June 4, 2009

Peter, Giotis and Joe,
The analogy to a an acoustic string would be a legitimate metaphor (metaphors are never correspondences but at best analogies) if strings would really be what string theorist claim they are, namely stringlike extended objects (as opposed to the pointlike fields of QFT).
But they are not, and some string theorists (Martinec, Dimock,…) have seen this by explicit calculations. Since at the time of these calculations they were already committed to strings (the community influence which Zapata describes so vividly) they called it an "invisible string" or "a string of which one sees only the c.m." They should have used the name "infinite component wave function" or (in second quantization infinite component pointlike fields). These objects were searched for before by Fronsdal, Barut, Kleinert…but without success because they were using group theoretical methods. When you permit quantum mechanics of infinitely many oscillators you can do it but only in d=26 resp. 10
Now if the result of ST would have been a string in spacetime than the analogy would be tolerable at least in order to reveal some flavour of that theory to the public. But the crucial point is that it isn't and to use the musical analogy with an infinite component field (with all the higher frequencies having gone into the inner ("little" biut now infinitely large) Hilbert space namely that which goes with the indexing of the components is a very misleading metaphor.
Or in Peter's words, if you apply the analogy to the infinite component pointlike fields (called string) than you must also allow it for a few component i.e. standard QFT, but you should not do this in either case.
I would expect a blogger telling me that names are hollow words, so what string theorists discovered (by a tube analogy to Feynman graphs) is a finite interaction between infinite component wave functions. This is their great discovery, something which one cannot do with a finite component field.
My answer would be: show me an operator realization. Every prescription in QT permits a formulation in terms of operators in Hilbert space other wise it has nothing to do with QT. Witten looked for such a description over many years and did not find any. Anybody in the blogger community who believes that such a thing exists?
The relation between the above strings and the way how to implement interactions in terms of pictures is hopeless, a genuine "catch 22" situation if you know Joseph Heller's novel.

15. **A.J.**
June 4, 2009

Dr. Schroer,

It's not quite true that the strings are always point-like objects in spacetime. They can wrap cycles, and this is essential for mirror symmetry.

16. **Bert Schroer**
June 4, 2009

A.J.
Ihave to disappoint you, all of them are really infinite component wave functions or fields. Real strings also exist in QFT but they are very very different. You can call the objects of ST "invisible strings" or "strings of which only the c.m. point is visisible" (look up the computations of Martinec or Dimock if you find my computation to sketchy or suspect), but that is just a metaphoric circumscription id infinite conponent pointlike objects.
I expected your reaction which is typical, because how can a young man (assuming that you did not live through pre 1980 particle physics) look through an extremely plausible metaphor which was solidified over 5 decades not only by its proselytizes of ST as Gross and Witten, but also by the silence of all the others including several Nobel laureates.
How can a young man believe in what somebody like me, a nothing, says about string localization? You probably will not even find it worthwhile and look up the references by string theoreticians where they showed that the (graded) commutator is that of a pointlike field (which is the definition of pointlike localization) which they then unfortunately expressed in the described way. But physics is a democratic science and I have no doubt that at latest at the great ST and TOE crash which has to happen because there is simply no progress without it, you will remember me.
I am just trying to back up Zapata's cutting insider report in how facts are created in a community (the only place where this is possible) by massaging conjectures. I am using my 40 years experience as a research scientist in an era where at the end only the truth counts independent of who said it, but where the formation of gigantic communities (as compared to individual researchers up to the 80s) may delay this for a very long time.
A.J. do you seriously believe that I would write such things in public if I would not have clarified this issue with all my colleagues who do understand QFT on a profound level? Please do me a favor and look up any reference about localization in ST (take the two above) and come back if you find anything else than invisible strings or strings which come with only one point and let me know. For a twitter session where you just day things without backing them up by a proof or at least an argument, time is too valuable. If you want to know how real strings look like, this can be explained and the result is that they have nothing to do with ST.

17. **A.J.**
June 4, 2009

Dr. Schroer,

Please explain how a string wrapping the circle once is a localized entity. The notion of center of mass is a bit meaningless when the spacetime lacks an affine structure.

18. **Sulfur Surfer**
June 5, 2009

Prof. Schroer's technical claims about string theory are so manifestly ridiculous as to provide no motivation to read the tomes that he claims are necessary to understanding QFT (and there are many hundreds, perhaps thousands, of theorists using QFT correctly and productively without the formalisms that Schroer claims are necessary). Indeed, AJ has provided two counterexamples, and the long strings that appear along excited Regge trajectories are another: these behave in every physical respect like string-like objects.

19. **Bert Schroer**
June 5, 2009

Dear A.J.
The metaphor of the invisible wrapped string (with only one point is visible) is not mine but the phrasing of those string theorist theorist who really computed the spacetime extension of the string and found just a point. I am not using such misleading metaphors because the english language offerers a word for what it really is: an infinite-component pointlike field or better wave function.
This blog discussion is impeded by the weight of almost 50 years of metaphors because when you hear the word "string" you think automatically in terms of those little wiggling closed or open things, don't you? What I am asking you is to delete this in your memory and find our for yourself. The only reason I mentioned those computations done by string theorists because you trust them more them me. But the result of the computation is the same, the only difference is that they phrase the result in that (misleading) metaphor whereas I present the result without using metaphors.

By the way to see the uncorrectable metaphoric Maldacena AdS-CFT conjecture cannot be dealt with in terms of a simple calculation, one needs more demanding structural tools: but since it does not come music (besides that of its string theory parents) one would have to open a separate section.
Of course all these aberrations would not exist if the sociological conditions for particle physics would be similar to what they were at its best times namely by individual researchers as Pauli, Schwinger, Kallen, Lehmann, Weinberg,..and not by globalized communities with a guru at their leader.

20. **Bert Schroer**
June 5, 2009

When I came back to my office just now and looked at this blog, I realized that my last contribution intersected with sulfur's. It so happened that in anticipation of my contribution and against his will he gave me a helping hand by confirming that particle physics these days is a matter of guru-directed communities who measure the validity of ideas in terms of community size and/or viewing rates.
I expect however that possible further contributions of this Asperger Neird will be less useful and it would be a good idea if he can be kept out of this series blog in which every statement should be proven or documented following the example set by Oswaldo Zapata (who showed that string theory and objectivity do not necessarily have to be contradictory).

21. **Giotis**
June 5, 2009

Personally I don't quite understand where the problem is. The EOM of the string is a wave equation. The solution to the EOM is expressed in terms of Fourier series. Upon quantization the commutation relations of the Fourier modes are those of the harmonic oscillator creation and annihilation operators. We have an infinite number of creation operators and by acting on the ground state (of the string i.e. no oscillations) they create the infinite set of basis states that span the infinite dimensional Hilbert space. Each quantum state of the string is described by a wavefunction (that satisfies the Schrodinger equation) and can be thought as a one particle state. Thus there should be the corresponding quantum field in space-time (the realm of String Field theory).

But that doesn't change the fact that the string is *really* the fundamental one dimensional object that sweeps a worldsheet in spacetime or wraps around a circle. Likewise in QFT the fundamental object is the field and its Quantum states can be interpreted as particles. Anyway I'm a little confused by these assertions.

22. **Bert Schroer**
June 5, 2009

Sure Giotis, a string of ST like a pointlike field with infinitely many component i.e. an infinite component field in the sense of the book on QFT by Bogoliubov, Oksak, Todorov,… But those degrees of freedom which reside in the e.g. Nambu-Goto Lagrangian are not the oscillator degrees of freedom which you see if you look at the creation/annihilation operators of a finite component free field. Those oscillator degrees of freedom which are in that classical N-G string rather go into the inner Hilbert space i.e. they create the infinite internal dimension which has nothing to do with the creation/annihilation operators of fields. The reason why those people who tried infinite component fields before string theory did not get anywhere is because they did not use vector-valued quantum mechanical variables to built up this infinite dimensional Hilbert space, but they tried it with noncompact group theory. It is very tragic that the dual model people did not realize that the Veneziano solution for a dual amplitude led precisely to those infinite component pointlike objects (the infinitly many poles whose residua are formed according to Gamma function properties), we would have been much better of.
I agree with you that words are all hollow, but this ceases if words generate series misunderstandings. This is the case here. The oscillators of quantum fields nobody would call strings, they are pointlike localized and that they contain oscillators you only see through taking their expectation values in states (e.g. vacuum fluctuations), e.i. these fluctuations are their spacetime mark. With other words the oscillation (fluctuations) comes in through states, there is nothing oscillating in the observable pointlike localized field A(x), i.e. the a and a* do not start to fluctuate by themselves.
The role of the oscillators which are characteristic for string theory is very different since they create an inner Hilbert space (which accounts for all those infinitely many spin components) and to speak about fluctuation is physically completely meaningless, their role is just to catalog the particle tower.
There are two remarks which one could add here
1) there are genuine stringlike objects; e.g. the electricall charged fields in their sharpest-localized form. They are very different from ST.
2) dismissing strings/infinite component field as hollow words has its end when you come to the way in which string theorists introduce interaction via tube recipes for those infinite component string wave functions.
3) The incorrect picture of ST can be traced back to the idea of source and target space (world sheet). You cannot embed a chiral conformal current with 26 internal vector indices into a 26 dimensiomal spacetime, when you do this the chiral degrees of freedom go into the internal space, i.e. the resulting 26 dimensional object is an infinite component pointlike field in 26 dimensions. This is where the tragic journey into metaphor-land begun.

But I think I answered your question and I do not want unsolicited enter related subjects.
Resume: the strings you mean when you make this comparison with oscillators (and vacuum fluctuation) in quantum fields are perfectly compatible with the true nature of what ST call strings and which they should have called infinite component fields, but they are not compatible with those metaphoric pictures of ST as an object which has an intrinsic (state independent) extension in form of a string.

LHC: status and commissioning plans

June 1, 2009
Categories: Experimental HEP News

The LHC's Mike Lamont has just posted a preprint here based on a recent Moriond talk describing the current status of the LHC commissioning. Here is his discussion of the on-going campaign to identify bad splices and figure out what to do about them. From it, I gather that the big unanswered question is what to do about possible bad splices in sectors that have been cooled down, since warming up the sectors would significantly delay the scheduled startup.

> One additional danger that has recently surfaced is a bad electrical contact between the copper of the busbar and the U-profile of the splice insert on at least one side of the joint. Combined with a bad contact between the cable and the copper this leaves the splice without an alternate route for the current in the case of a busbar quench – in a good splice the current can flow in the copper removing the danger of excessive resistive heating in the quenched superconductor. A good contact between the Rutherford cable joint is assumed (i.e. less that 2 nano-ohm).
>
> Such situation can be detected by measurements at warm using low current and a nanovoltmeter across short segments of the machine. Under such circumstances the current flows in the copper and the resistance of a good joint is around 12 micro-ohm. Extensive measurements of the four warm sectors (May 2009) have revealed 16 segments with excess resistance of over 30 micro-ohm. The relevant interconnects have been opened. Individual splice measurements have revealed resistances of 30 – 50 micro-ohms. All such splices have been re-done and re-measured.
>
> Warm quadrupole measurements started in May 2009. Measurements at at 80 K in sector 23 are also ongoing at this time. The measurements at 80 K are more difficult and show a lot more signal variation – the resistivity of copper falls by a factor of 7.5 at this temperature. The question of what to do if suspect splices are found at this stage of re-commissioning is to be addressed.

No Comments

Multiverse News

June 1, 2009
Categories: Multiverse Mania

Some items from around the multiverse:

- Srednicki and Hartle have a new preprint on hep-th about Science in a Very Large Universe. Like many other multiverse papers, it doesn't really have any equations in it, so it's a bit hard to figure out what their argument is. Maybe readers can figure it out from the conclusion:

> It is no surprise that information about us is required to make predictions for our observations. Our data suggest that we are located some 13.7Gyr from a Big Bang. To make a reliable prediction from that information, we have to assume that it describes our physical situation. If the universe is rife with delusion, we must assume that we are atypical in order to have predictive and testable scientific theories. Indeed, it is only by making such assumptions that we are able to do science in a very large universe. We imagine that even Copernicus would have agreed that it was necessary to assume that Ptolemy was not deluded in his observations of the planets.

The authors thank about a dozen or so other theorists for their help with this.

- World Science Festival 2008 here in New York was a huge success, and I suspect that the 2009 version starting June 10 will be too, which is great. Of the many events, one where I might have a difference of opinion with some of the panelists will be a session on Infinite Worlds: A Journey through Parallel Universes, sponsored by the Templeton Foundation.
- Seed magazine had a story a couple months ago about the theological implications of the multiverse.
- Astrophysicist Jeffrey Zweerink has a book out called Who's Afraid of the Multiverse?
- Over at FQXI there's a recent blog entry discussing the question of whether God might be "unsure as to whether He is really just a brain floating in a vat?"
- Somehow I missed this one last year. Wheaton College held a research symposium on String Theory and the Multiverse: Philosophical and Theological Implications.

Update: For a more skeptical and philosophical take on the multiverse, there's The Unique Universe, a piece by Lee Smolin that just came out in the latest Physics World.

Update: The proceedings of the Wheaton College conference on string theory, the Multiverse and theology are available here. They include audio recordings of the discussions, and inform us that string theory

> implies that physical reality is far vaster and possesses greater grandeur than ever imagined.

Comments

1. **perverse**
   June 2, 2009

   The universe IS rife with delusion … that it is one of a multiverse.

2. **Shantanu**
   June 2, 2009

   Peter are u planning to attend the 2009 Amaldi meeting ?
   2 weeks from now

3. **Peter Woit**
   June 2, 2009

   Thanks Shantanu, I didn't know about that. I'll be around those days, may try and catch some of the talks.

4. **Arun**
   June 2, 2009

   Re: Lee Smolin *As we attempt to realize those principles, we seek a notion of law that cannot be applied to an imagined universe within a multiverse, and which cannot be imagined to hang around timelessly waiting for a universe to begin that it can then govern. Given that the universe only happens once, we must try to imagine a new kind of law that applies only that one time. Such a law need not — and should not — have any sense in which it exists outside of time. Nor could it be conceived of as apart from the universe it describes.*

   Our universe began with low entropy – it applies to this universe, not imagined universes within a multiverse; it is not hanging around waiting for a universe to begin, it comes into being with the Big Bang. It cannot be conceived of as being apart from the universe it describes. Talk me down.

5. **GR**
   June 2, 2009

   Good lord. I may just be a beginning graduate student, but my bullshit detector reads off-scale high when I try to parse that. My best guess is that it means "in order to do science, our observations have to match up with reality"?

   Who knew Aristotle was the first string theorist. It's sad that theoretical physics is rife with third rate philosophy hacks trying to come to grips with solipism.

6. **Coin**
   June 2, 2009

   Why is the brain always in a vat? Is this really the most efficient and safe way to house a trapped brain? It seems like a neutrally buoyant brain floating in fluid might still tend to drift and hit the side of the vat, or something. And would a brain suspended in nutrient fluid actually even successfully be able to intake the fluid? In the human body the "nutrient fluid" has to keep continuously flowing via external force and if it ever goes stagnant we call this a stroke. Right? Why not just keep the arteries connected to the brain and pump fluid through those?

7. **Chris W.**
   June 2, 2009

GR: I can guess, but would you mind being more specific about what triggered your bullshit detector?

8. **GR**
June 2, 2009

Chris:
Well, to be fair, when I first wrote that I'd only read the excerpt. I've read the whole thing now and my opinion hasn't really changed. Of course, I may just lack the sophistication needed to understand the paper, but it reads like an exercise in metaphysics, not physics.

In any case, the sentence "To make a reliable prediction from that information, we have to assume that it describes our physical situation." is what made me skeptical. I think they're questioning the idea that the laws of physics are the same everywhere in the universe. Fine, but what really is the point? There's not much use in doing cosmology if they aren't.

Again, from my perspective as a beginning graduate student, all of this wild speculation about multiverses just sounds like the string theory crowd making excuses for why their theory doesn't come up with predictions…

9. **Chris W.**
June 2, 2009

Okay, so you were talking about the excerpt from Lee Smolin's article in Arun's comment (6/2, 10:42 am), and have since read the entire article.

I get the impression that you're fairly new to this discussion. Lee Smolin is not a string theorist, although he has done some work in the field. The whole point of his article is explain why certain assumptions implicit in our conventional understanding of quantum theory lead us to a multiverse in the context of quantum gravity and cosmology. He is trying to find a way past those assumptions, precisely because the idea of a multiverse severely undercuts or eliminates the possibility of making testable predictions.

Is his article an exercise in metaphysics? Absolutely! In this context metaphysics is unavoidable. Modern physics inherited some key metaphysical ideas from the ancient world, most notably atomism, and found ways to give them empirical depth and substance, ie, to build testable theories around them. I think the same process must be repeated now, although the required ideas will almost certainly have a much more recent origin, and may have to be invented anew.

Smolin is not questioning the importance of the universality of the laws of physics across space-time. What he is emphasizing is the potentially disastrous effects of separating the fundamental laws from *effective* laws in such a way that only specific effective laws can be tested, but are also assumed to be highly contingent on how the observable universe is situated in a hypothetical multiverse. The result is that a failure of any effective theory simply results in selecting another effective theory from a vast ensemble allowed by the fundamental theory. Potentially, then, the latter therefore can never be tested; it devolves into a generator of effective theories, with unlimited latitude for accommodating actual observations.

Of course string theory (not to mention particle physics and cosmology), considered as a formal discipline, is a vast subject, and one can bury oneself in mastery of parts of it while ignoring these issues. Smolin and others are trying to resist this attitude. Mathematical formalism can be just as metaphysical in its relationship to nature as ideas expressed in prose, even if its logical structure is tighter and its definitions are more precisely stated.

10. **GR**
June 2, 2009

That quote was from the first bullet point in Peter's post, not Smolin's article, which I haven't had time to read.

11. **Dude**
June 3, 2009

Peter, can you please consider using a widget in the sidebar that shows the titles of your posts for your archives? The same thing for your categories would also be nice.

In the past, I used a widget called Wp-dTree. The wordpress.org page is here: [Wp-dTree 3.5](). One of the tabs near the top shows some screeshots.

Here's a blog that's using Wp-dTree: [here]().

You have posts that I may want to refer to in approximately 14.34 years, when I've achieved greatness of some sort, probably via American Idol, or Britain's Got Talent, singing as fat woman. Or occasionally, I might like to surf through old posts of yours in a non-linear manner.

I could link to your posts now to separate them from the myriad of posts on your blog, but this could be a waste of my time, considering there's a possibility I'll move to Sudan, or never move to Britain, and lose my chance of competing on American Idol or Britain's Got Talent as a fat woman, and never achieve the greatness I desire, and therefore never have any real use for your posts.

12. **chris**
June 3, 2009

Chris W,

i am not aware of even one single instance in modern physics where pondering about metaphysical questions resulted in progress of any sort. all examples that i am aware of show that progress is usually reached through detailed understanding of experimental data. metaphysics is usually produced in the aftermath of a breakthrough to digest what was learned or put it in some sort of perspective. this is very natural, but i seriously question its usefulness for any sort of scientific progress.

13. **A.**
June 3, 2009

The discussion above reminds me of the following exchange I overheard in the department, the other day:

Senior string theorist: "The definition of "science" has nothing to with modern physics."

Postdoc physicist: "I'd say that modern physics has nothing to do with science."

Senior string theorist: *looks like he has just been slapped*.

Sigh. Seriously.

14. **Peter Woit**

June 3, 2009

Dude,

Thanks for the suggestion. I've implemented it for the archives, where it looks quite useful.

15. **Dude**
June 3, 2009

Peter, thank you very much. That's the main thing I wanted, but here are two more requests/suggestions, although you may not care about implementing them.

#1: Please increase the width of the sidebar as wide as can (but to where it still looks good). Wp-dTree truncates the post titles based on the width of the sidebar. This isn't of ultimate importance since the complete title is shown if the mouse cursor is hovered over a truncated post title, but it would make viewing titles faster.

#2: Include the sidebar in every view. For example, the sidebar is shown if I enter using your main blog URL, but if I click on a post, the sidebar isn't shown. If I was surfing lots of old posts, I wouldn't have to go back to your main page each time.

Suggestion #2 would also be good for those who aren't familiar with you, and who enter a page through a search engine. If the sidebar is there, a person may see something that interests them.

To do the above, you'd have to edit the CSS, and I can't tell you how to do that. The last time I did it, it took me a long time to figure out how to mess with CSS.

Thanks again.

16. **Peter Woit**
June 3, 2009

Dude,

I don't think I'll change anything more right now, have been trying to keep the changes from the default theme to a minimum, which makes upgrading the version easier.

17. **Mitch Miller**
June 3, 2009

A's comment sort of reminds me of George W. Bush's comment on the free market.

"I've abandoned scientific principles to save the scientific system."

18. **GR**
June 3, 2009

Chris W:

Now that I've read Smolin's article, I don't have a problem with it. Mainly because it offers a concrete prediction: the universe maximises the production of black holes. I'll be damned if I can ferret out any sort of prediction from Srednicki and Hartle.

If string and multiverse theorists want to play the metaphysics game, they're more than free to. But they shouldn't call it physics. This whole probability game reminds me of that Feynman quote about the license plate…

19. **Chris W.**
June 3, 2009

— from 'Chris': "i am not aware of even one single instance in modern physics where pondering about metaphysical questions resulted in progress of any sort."

1) Since the 18th century, metaphysical considerations have remained in the background, and even in their intellectual biographies most scientists downplay or omit any mention of their influence. At most, they might acknowledge having studied philosophical writings in which metaphysical presuppositions are discussed. In addition, there is the question of what assumptions one considers to be metaphysical. I doubt that most working scientists spend much time thinking about the demarcation. They're more concerned with distinguishing between ideas that might prove fruitful for the formulation (!!) and solution of scientific problems. In practice, "metaphysical" is often considered a synonym for "useless" or "sterile". Indeed, most metaphysics, like much thinking in many areas, is of dubious value. ("Ninety percent of everything is crap. ")

2) The metaphysical ideas that have most important to modern science (not just physics) so far have ancient roots, and are so embedded in our cultural and intellectual heritage that are rarely consciously considered, except by philosophers and historians.

3) Avoidance of metaphysical assumptions, interpreted as an uncompromising operationalism, was very much part of the philosophical background of quantum mechanics. Einstein's stated motivations for the development of special relativity provided considerable impetus for this, along with the influence of the Vienna Circle. (Einstein later repudiated this attitude.) This is deeply embedded in modern scientific culture, so that metaphysical assumptions, when employed, are rarely acknowledged or even recognized as such.

4) Re your remark: "all examples that i am aware of show that progress is usually reached through detailed understanding of experimental data." The question becomes, how is that understanding achieved? Perhaps it is considered to be in poor taste to discuss it publicly, but when the necessary ideas don't exist, somebody has to wrestle with the obstacles to understanding, including the possible role of an unquestioned metaphysical framework, or the lack of such a framework. Of course, as soon as empirically testable assertions can be made, they take center stage, and most researchers *not directly involved in developing the key ideas* take little interest in their background.

I hope these observations serve to indicate why consideration of metaphysical presuppositions is so routinely avoided, even when it could be illuminating, and even essential, both in formulating and in appreciating important scientific ideas.

20. **Observer**
June 3, 2009

…"The definition of "science" has nothing to with modern physics."
Are string theorists slipping their discipline into religious faith…? Now that sounds amazing .

21. **John Duffield**
June 3, 2009

Ouch. Is that what passes for HEP these days? The Srednicki/Hartle paper read like a spoof. It falls over in the first paragraph when it claims that “there is a quantum probability for these data to exist in any spacetime volume”. No, there isn’t. There is no evidence whatsoever for any kind of xerographic multiple copies of me sitting at this desk. There is no evidence whatsoever for an infinite number of Boltzmann brains magically popping into existence. These speculations do not serve as the basis for a typical/atypical computation or experiment dressed up with Bayesian respectability. This isn’t science. There are no testable predictions. These guys are riding the many-Worlds misinterpretation of quantum physics and they surely don’t even understand that the fine structure constant alpha $\alpha = e^2/2\varepsilon0hc$ is a running constant or why. So when they say “It is only the combination of fundamental theory and an assumed xerographic distribution that is predictive and testable by observation” it’s just castles in the air.

22. **anon.**
June 3, 2009

they surely don’t even understand that the fine structure constant alpha $\alpha = e^2/2\varepsilon0hc$ is a running constant or why

It’s not as if one of them wrote a textbook on quantum field theory, after all! Oh, wait.

23. **Titanium Dragon**
June 3, 2009

Your extract has the scent of nonsense clinging to it, but it actually does make sense. It is an incredibly complicated restatement of a very basic principle. It could be summarized more simply as follows:

In an infinitely large universe, any set of physically possible events will occur somewhere. Therefore, it is possible that all of our conclusions are the result of coincidence, so we can only be as confident in our data as probability allows.

Or, in other words, it is possible (if vanishingly unlikely) that our observations of physical phenomena are coincidental. Of course, this is a worthless statement, as we all already knew this fact; its basic science that we are never absolutely certain of anything.

It is also worth noting that this is not, in fact, limited to an infinite universe, as it can be true in even a finite universe, so the whole multiverse part is completely extraneous to this statement. We could simply say:

We can only be as confident in our data as chance allows us to be.

And be done with it.

24. **Titanium Dragon**
June 3, 2009

Incidentally, the linked-to article strikes me as pretty vapid itself, I’m afraid. Of course, so does the entire timelessness argument. He claims that we evoked chess into existence, as opposed to it always existing in some timeless state. Both strike me as completely meaningless, unscientific ideas. Indeed, both are equally falsifiable; we cannot confirm the existence of something until we do so by definition, so we cannot check if it was there “beforehand”; ergo both are meaningless.

Really, the argument strikes me as a very metaphysical one which doesn’t make unique predictions about the universe. What in that article is really worth considering? That we cannot know the initial conditions of the universe? Isn’t that something which is generally suspected because of the massive amount of energy involved, as well as our inability to observe the universe as a whole and the fact that our laws of physics break down before we go back all the way to the big bang?

25. **jpd**
June 4, 2009

i think i agree with you ,but quick question

re: “In an infinitely large universe, any set of physically possible events will occur somewhere”

if the probability of a physically possible event is infinitely small, couldn’t it be ruled out in a (not sufficiently) infinitely large universe?

in other words: a boltzmann brain is ridiculous.

26. **Shantanu**
June 4, 2009

Peter you found anything interesting at
“New Prospects for Solving the Cosmological Constant Problem”
conference at PI?
It has a wide spectrum of talks from anthropic principle to LHC etc.

27. **Peter Woit**
June 4, 2009

Shantanu,

I have the same problem with explanations of the CC that I have with quantum gravity in general. The only way that I can see that any purported explanation would be convincing would be if it also implied other non-trivial facts about observable physics. I haven’t seen anything like this, either from the anthropic people or from other attempted CC explanations.

28. **alex**
June 5, 2009

re: “In an infinitely large universe, any set of physically possible events will occur somewhere”,

or as Hartle and Srednicki put it,

“in the universes considered in contemporary cosmology the following often hold:

1. The probability is near unity that our data D0 exist somewhere.

2. The probability is near unity that our data D0 is exactly replicated elsewhere many times."

The reasoning behind this claim isn't specified but it seems to be that given an infinite number of tries every possible outcome will occur. But his is not so. Even in an unbounded universe you have a countable number of planets, brains, desks or whatever else you include in "our data D0". If the number of allowed states D0 is not countable, which would seem to be the case, then it is impossible for all of them to actually happen and the chance of there being another me sitting at an identical desk looking at an identical night sky somewhere out there beyond the observable universe is not close to unity, it is zero. At least if you consider D0 being "exactly replicated" as they state. There are some loopholes. You might think of a multiverse with uncountably many universes within it. Or better, you might consider states similar to but not exactly the same as D0. Maybe you'd want to consider the probability of states where the expectation values of all possible measurements are within epsilon of those for the state D0, and see if you can keep the probability of there being one at unity whilst epsilon tends to zero. Though you might then worry about chaotic systems and the amplification of arbitrarily small differences in initial conditions. I guess you might also consider that you are talking about some sort of reduced density matrix for each candidate subsystem and the implications of that. But they don't.

29. **GR**
June 5, 2009

Peter,

Have you seen the latest rehash of the string-theory-in-CM on slashdot?

30. **Peter Woit**
June 5, 2009

GR,

Hadn't seen that. It's the New Scientist article I wrote about here a while ago, which wasn't bad, although Slashdot as usual manages to do a good job of garbling it with nonsense like:

"His theory states that the known universe is only a 2D construct in anti-de-Sitter space, projected into 3 dimensions."

31. **Claver**
June 8, 2009

re: "In an infinitely large universe, any set of physically possible events will occur somewhere"

In the real and measurable universe only certain events happen. That is, doesn't such a universe exhibit a bias? Such a bias permitting and excluding events.

If the universe exhibits such a bias today, why shouldn't an observer some where afar off not see the same bias? And, why should that bias change?

I mean is the universe 'compelled' to pick different events tommorrow? Implying that the universe 'remembers' which events happened yesterday? (Which gives rise to an interesting observation: information from the past influences events of today.)

Put in another way, if in an infinite amount of spacetime EVERY set of physically possible event will occur, then surely every event can be repeated, within the same infinite universe. In which case the probability of an event occurring in an infinite universe can be more than 1 because
'who can measure how many times, in an infinite universe, a particular event is repeated?'Is there an observer? If not, then saying …

"In an infinitely large universe, any set of physically possible events will occur somewhere"

is saying something that cannot be physically proven. It is an unobservable and unmeasureable statement?

i.e if ANY set of physically possible events WILL occur then there is a probability that ALL will occur.

I thought that in physics (real and measurable) the notion of an infinitely large universe was purely a mathematical construct? A consequence of the inherent mathematical difficulty – in expressing ongoing repetitive structures – carried over into physics from mathematics.

Or is this just being naive?

32. **Dr Lewney**
June 9, 2009

Hi Peter,

Did you happen to have a look at the last page of the June issue of Physics World containing Lee's excellent article? (I'm afraid it wasn't picked for online publication.)

Cheers

Mark

33. **Peter Woit**
June 9, 2009

Hi Mark,

I'm afraid I haven't seen a physical copy of the June issue of Physics World yet. What's on the last page?

34. **Dr Lewney**
June 10, 2009

It's a light-hearted article on my Schools Lecture Tour which mentions this very blog! Send me a private email and I can reply with the PDF.

Cheers

Mark

35. **Matthew Putman**
June 14, 2009

i am wondering if you had a chance to get to any of the World Science Festival this year? And if you do have any issues, i am curious about them. I thought that this year the events were rather brave in presenting opposing theories. I say brave, because there is a danger when presenting ground breaking and interesting science to a group most interested in the artistic interpretations of it. They might tend to misunderstand scientific discourse, relating it to differences in religion. In Religion the wrong party goes to hell. In science, they jump on board the proven theory and try to improve it. This is why scientific debate is always positive, and why religious debate always a waste of time.

36. **Peter Woit**
June 15, 2009

Matthew,

I was out of town for most of the World Science Festival this year, so just saw some reports in the newspaper, which indicated that, like last year it was a great success. The high level of public interest in this kind of thing is pretty amazing.

String Universality

June 7, 2009
Categories: Multiverse Mania

There's a new paper out on the arXiv this evening, advertising a new concept called "string universality". The authors argue that in six dimensions, by use of appropriate compactifications, any consistent 6d supergravity theory has a string theory realization. They go on to conjecture that the same might be true in four dimensions. As for the implications of "string universality", they write:

> If it is correct, or even close to correct, that string universality holds in six dimensions, then in this case we seem in some sense to be in the worst possible situation vis a vis low-energy predictions. If every possible consistent theory can be identified from low-energy considerations, and all of these theories can be realized in string theory, then string theory would seem to have no predictive power for low-energy physics…

Not being able to predict anything sounds bad for string theory. But wait, they go on to explain why not only is this not a deadly problem, it's actually a "strength of the theory". You see, there's "symmetry and elegance" to a principle that is consistent with absolutely everything and constrains nothing. Some worrywarts might have problems with such a principle since it can't be tested, but, just because something can't be tested doesn't mean it's not right, no?

> This may seem like a very awkward situation for string theory. It should be emphasized, however, that there is no reason a priori why a theory of quantum gravity relevant at the Planck scale of 10^{19} GeV should make any prediction for physics at the scale of 1 TeV, 16 orders of magnitude below the quantum gravity scale. String theory is valuable as a framework for describing quantum gravity. If in fact, string theory can be used to provide a UV completion of essentially any low-energy theory whose coupling to quantum gravity does not violate some basic principle like unitary via anomalies, this can be seen as a strength of the theory. There is a certain symmetry and elegance about the notion of a quantum gravity theory which provides for the production of essentially all possible low-energy behaviors in some regime of the theory or region of the metaverse.
>
> If indeed, string theory can give rise to such a wide range of low-energy behavior that predictions at the TeV scale cannot be made precisely, it may bother some scientists that this makes the theory difficult to test. But, on the other hand, this does not make the theory any less likely to be correct. It just makes it more difficult to verify.

Comments

1. **fh**
   June 8, 2009

   I actually think this would be great, this is a good place for String theory, not as a unique theory that gives immidiately testable predictions but as a generic framework like QFT.

   Just as in QFT you specify gauge groups and representations to obtain a theory, in ST you specify a geometry/compactification.

   It's not a theory of everything but a theory of every theory.

2. **MathPhys**
   June 8, 2009

   I think it's a beautiful idea: Strings form a completion of effective, low-energy field theories, and (hopefully) offer the correct way to make the loop diagrams finite order by order.

3. **Engulf and Devour**
   June 8, 2009

   Why stop there? String Theory should not be satisfied until it has subsumed all of the *in*consistent theories as well.

4. **Peter Woit**
   June 8, 2009

   MathPhys,

   But that's not the idea the authors are claiming is beautiful. Their new idea "string universality", for which they claim "symmetry and elegance", is that of a "theory which provides for the production of essentially all possible low-energy behaviors in some regime of the theory or region of the metaverse."

   In the past, a physical theory was considered to be "symmetric and elegant" if it explained a large number of observable phenomena with a small number of constructions. What is new here is the discovery that you can get "symmetry and elegance" at the opposite extreme: a theory which explains no observable phenomena using extremely complicated constructions. They couple this with a very powerful new principle of how to do science: just because a theory can't ever be tested doesn't make it any less likely to be correct. This principle has wide possible applications, promising to bring back together science and religion, healing the terrible split which occurred with Galileo.

5. **Peter Shor**
   June 8, 2009

   If this in fact can be proven (or at least shown to the satisfaction of the string theorists), this would be a significant advance; it would also put a stop to the incessant "test of string theory" press releases.

6. **Chris W.**
   June 8, 2009

   They couple this with a very powerful new principle of how to do science: just because a theory can't ever be tested doesn't make it any less likely to be correct.

   Yeah. I'll bet that the IDers will be all over this, not to mention the astrologers and diehard believers in psychic phenomena. Talk about riding coattails…

   > …… But, on the other hand, this does not make the theory any less likely to be correct. It just makes it more difficult to **verify**.

Notice (yet again) the way the author skates right by the essential emptiness of "verification" without testability. This is precisely what galvanized Karl Popper as a young man, as he observed the depressing contrast between Freudian psychology and Marxism on the one hand, and what he saw happening at the same time in theoretical physics, where people actually seemed to care about checking for the possibility that their ideas might be wrong, ie, might imply assertions that contradict observations and results of experiments. More precisely, they cared enough to *formulate* ideas that were logically capable of contradicting observations and results of experiments.

Richard Feynman died in February of 1988, but even then he smelled a large rat.

7. **A.J.**
June 8, 2009

If this in fact can be proven (or at least shown to the satisfaction of the string theorists), this would be a significant advance….

Not to mention a great piece of mathematics.

8. **Peter Woit**
June 8, 2009

Peter Shor,

There's no more claim here than anywhere else to be able to show "string universality" in 4d. What is remarkable in this paper is the claim that "string universality" would not be a reason to give up on string unification, but instead would be evidence of "strength", and a "symmetric and elegant" scientific picture of the world, with the fact that it could never be checked not a problem. Most string theorists I talk to claim the argument for string theory unification these days is "we still don't understand string theory", that when we do, maybe it will somehow work out. They acknowledge that if, once string theory is completely understood, it has nothing to say about particle physics, then the game is over, you can't claim string unification as successful science.

I don't think there's any prospect of press releases about "tests of string theory" stopping. Even now, these pretty much all just ignore what is known about string theory.

One other peculiar thing in this paper is the wording of the implication of "string universality" as "predictions at the TeV scale cannot be made precisely". The problem being discussed is whether ANY predictions, no matter how crude, can be made, not the question of precise predictions. It's unclear to me why the authors use this language that doesn't correspond to what they are talking about.

9. **M**
June 8, 2009

why the 6d case is even worse than the 4d case? Naively I would guess that to get 4d one needs to compactify more extra dimensions so there is more arbitrariness about how to choose the extra dimensions: one M-theory in 11d, 5 string theories in 10d, …, 10^500 not-even-wrong theories in 4d.

10. **Peter Orland**
June 8, 2009

fh just repeated the far too common retort on this blog…

"just as in QFT you specify gauge groups and representations to obtain a theory, in ST you specify a geometry/compactification."

NO! There is the SUBJECT "Field Theory" about the properties of different field theories. This is not the standard model, which is a, single, not family of, not class of, not generic, not a googol of, but ONE field theory. That's the theory.

11. **Leonard Ornstein**
June 8, 2009

It seems that the distinction between possibly beautiful math, which doesn't require empirical confirmability; ultimately, only 'logical proof' – and science, which requires empirical testability, no matter how beautiful a model (or 'theory') – is still not appreciated by many of the posters above!

12. **Benni**
June 8, 2009

Well, i do not think that it is important for a theory of quantum gravity, to make predictions at the TeV scale. Someone who is interested in predictions at the TeV scale should work on ordinary phenomemology, and not on any quantum gravity.

The aim of theories of quantum gravity are just to give possible models that describe a possible quantum version of gravity, and mayme come up with additional fields.

Research on quantum gravity is mostly purely foundational research that deals more with mathematical consistency and mathematical innovations than phenomenology.

13. **Peter Woit**
June 8, 2009

Benni,

I don't disagree with you that string theory has nothing to say about particle physics (other than as a possible calculational tool in QCD). However, many of the people who work on the subject seem to think otherwise and disagree with you.

14. **Nige Cook**
June 8, 2009

'What is new here is the discovery that you can get "symmetry and elegance" at the opposite extreme: a theory which explains no observable phenomena using extremely complicated constructions. They couple this with a very powerful new principle of how to do science: just because a theory can't ever be tested doesn't make it any less likely to be correct. This principle has wide possible applications, promising to bring back together science and religion, healing the terrible split which occurred with Galileo.'

This humorous sarcasm is just not funny anymore, just sad.

15. **Marcus**
June 8, 2009

Nigel, your sadness surprised me, so I had to react. That passage made me laugh out loud. "healing the terrible split which occurred with Galileo" is perfect!

The tone is controlled for maximum effect. On occasion Peter gets it just right. Perhaps if we can laugh at the way this branch of physics is going, things are not so bad. I have the impression that you are sad because you are pessimistic about the decline of empiricism in theoretical particle physics. The decline in standards.

But other branches of science are quite healthy and that includes other types of physics (does it not? You may know better than I about that.) So be of good cheer. If I may venture a prediction, things will all turn out fine.

16. **Thomas Larsson**
June 9, 2009

This humorous sarcasm is just not funny anymore, just sad.

Not even fun.

17. **Chris W.**
June 9, 2009

Benni said: *Well, I do not think that it is important for a theory of quantum gravity, to make predictions at the TeV scale. Someone who is interested in predictions at the TeV scale should work on ordinary phenomemology, and not on any quantum gravity.*

Consider a rephrasing of this for an earlier era:

Well, I do not think that it is important for a theory of relativity that is consistent with Maxwell's electrodynamics, to make predictions at relative velocities small compared to the speed of light. Someone who is interested in predictions at these velocities should work with ordinary Newtonian kinematics and dynamics, and not with [Lorentz-invariant] relativity.*

I trust that the problematic nature of the second formulation is apparent. 🙂

———————
(* of necessity, an alternative to Galilean relativity)

18. **A.J.**
June 9, 2009

Well, I do not think that it is important for a theory of relativity that is consistent with Maxwell's electrodynamics, to make predictions at relative velocities small compared to the speed of light. Someone who is interested in predictions at these velocities should work with ordinary Newtonian kinematics and dynamics, and not with [Lorentz-invariant] relativity.*

Most models of quantum gravity — including the string theories Benni was talking about — predict that the effects of quantum gravitational phenomena at the TeV scale are negligible, just as relativity predicts minimal deviation from Newtonian mechanics.

If you want easy access to TeV scale phenomenology, quantum gravity isn't likely to help much, for the same reason that QCD doesn't do much for people studying solid state physics.

19. **Daniel de França MTd2**
June 9, 2009

Nigel Cook, if you look for research into Graphene in arxiv, you will see SciFi materializing in front of you.

20. **Coin**
June 10, 2009

NO! There is the SUBJECT "Field Theory" about the properties of different field theories. This is not the standard model, which is a, single, not family of, not class of, not generic, not a googol of, but ONE field theory. That's the theory.

Okay, so here's what I don't get.

Let's say the 4D string universality conjecture above turns out to be true, and every 4D SUSY particle theory has a compactified string theory equivalent. Let's say that we ignore the infinite landscape of theories this opens up, and pick a *single* one of these compactifications– let's say we arbitrarily pick a compactification that precisely corresponds to the MSSM.

It does seem basically impossible to go from this point to any new predictions about low-energy particle theory– our MSSM-string model will only give us the same predictions that the MSSM would if we were using field theory tools, by definition.

But, there is one difference, which is that our MSSM-string model will incorporate supergravity. Our MSSM-string model will necessarily include a graviton; and it's understood to be impossible to calculate anything concerning gravitons with field theory, whereas string theorists claim that under their formulation it *is* possible.

This means our MSSM-string model will allow us to do supergravity calculations in a setting where all the MSSM particles exist: we will have the ability to do all the low-energy things we could in field theory, since as 4D universality would tell us we can convert any field theory into a string compactification, and we will have new abilities at the SUGRA scale. Is this not useful?

21. **anon.**
June 10, 2009

Our MSSM-string model will necessarily include a graviton; and it's understood to be impossible to calculate anything concerning gravitons with field theory, whereas string theorists claim that under their formulation it is possible.

Nonsense. It's perfectly possible to calculate things involving gravity in field theory — that's what GR, or SUGRA, *is*. The difficulty sets in only (a) near the Planck scale or (b) in subtle situations like the correlations that encode information in Hawking radiation. We also know that super-Planckian scattering doesn't do much except make black holes, so there's some sense in which the only regime where quantum gravity could be probed is right in the vicinity of, but not much above, the Planck scale. (This is a little cavalier.)

22. **Peter Woit**

June 10, 2009

Coin,

The standard argument that string theory is “better” because it provides a consistent “ultraviolet completion” of a QFT has several problems with it:

1. It’s not true. Recall that we don’t know what non-perturbative string theory is, and we do know that perturbative string theory is not fully consistent. What is being sold as the new piece that will provide consistency is not a known theory, but an unknown one that is just conjectured to exist.

2. Such a supposed completion is not unique, you probably can come up with 10^500 of them. Not only do you get no predictions at 1 TeV, you get no predictions at any scale, since you’ve always got lots of possibilities.

3. Claiming that adding a complex superstructure that can’t be tested somehow is an improvement is just bad science.

What I really don’t understand about some string theorists these days is why they just can’t face up to standard scientific practice: if you pursue conjecture X and find that it is basically empty and can’t be tested, you admit failure and move on. You don’t start writing about “X universality”, promoting X as a great scientific success, an improvement over other ideas.

23. **halliday resnick teacher**
June 10, 2009

I enjoy lurking here and thought I’d venture a question:

Peter Woit says:
June 8, 2009 at 7:53 am

…a “theory which provides for the production of essentially all possible low-energy behaviors in some regime of the theory or region of the metaverse.”

I looked up “metaverse,” and found no physics references to this term, only references to a “cyberpunk fiction virtual world,” that is presumably emulated in Second Life, and such applications. Yet, the authors of the paper in question seem to be using it in a physical sense. Can anyone clarify? Are “metaverse” and “multiverse” being used interchangeably by some physicists?

24. **Peter Woit**
June 10, 2009

hrt,

In the usage of people interested in this, “metaverse” just seems to be a less popular variant of “multiverse”. I don’t know why these authors decided to use it.

25. **Markk**
June 11, 2009

Off the topic of this post but perhaps related. Have these papers
been causing any kind of stir around the theoretical physics community? They seem to be new or perhaps renewed approaches to unification with interesting fallout like inflation. Looking at these with limited understanding it seems like there are some promising approaches to larger scale theories. Hat tip to Ars Technica for their article. To this layman’s eye this seems vaguely related to things like the double relativity and such that have been around.

Nature Physics, 2009, DOI: 10.1038/nphys1298
PRL, 2009, DOI:10.1103/PhysRevLett.102.161301

PRL, 2009, DOI: 10.1103/PhysRevLett.102.221301

26. **Peter Woit**
June 11, 2009

Markk,

Yes, lots of people have started writing papers about this kind of quantum gravity model. Presumably it will soon get sorted out whether there’s a serious inconsistency in the idea. But even if it’s consistent, this looks to me like just one more untestable quantum gravity theory. It explains nothing about observable particle physics. For more discussion of it, best to find a blog run by someone with more interest in quantum gravity than me.

Latest From the LHC

June 9, 2009
Categories: Experimental HEP News

Last week was the annual Fermilab User's Meeting, for all sorts of interesting talks see here. These included a talk by Sergio Bertolucci giving recent news about the LHC status. This week CERN is hosting a CERN-Fermilab Hadron Collider Physics summer school, talks here, including one from Jorg Wenninger about the LHC status.

The main concern now involves bad soldering of some of the 1700 or so bus-bar inter-connections between magnets. One of these seems to have been at the origin of the accident last September. For the sectors (four of them) that are warm, such bad joints can be identified relatively easily, by their higher-than-normal resistance, and repaired. Unfortunately, for the four sectors that are now cold, identifying such bad splices is much more difficult. Warming up these sectors and cooling them back down is a time-consuming process that could significantly push back the LHC schedule.

Late last week the decision was made to start warming up sector 45. Measurements at 80K had identified 3 cases of anomalously high resistance. The plan is to warm up the sector, take measurements which can be compared to the measurements made when the sector was cool, and fix splices as necessary. For this particular sector, things can be rearranged so that warming it up and cooling it back down will not seriously impact the schedule.

For the other three cold sectors though, the situation remains unclear. They're gathering more data and analyzing it, trying to understand better what is going on, as well as analyzing the question of whether it's possible to go ahead and find ways to run the machine safely, even given the possible existence of somewhat iffy interconnections.

The latest version of the schedule, from mid-last week, has powering tests ending in mid-October. So, as long as it does not turn out to be necessary to warm up more sectors, late October is the time-frame for trying again to circulate a beam and begin beam commissioning.

Update: There's a video of Wenninger's talk last week available here, where he gave some more details in the question session afterwards. Sector 45 will be warm and ready for measurements next Monday (June 22). If the results show good correlation with what was measured at 80K, at the end of the month the temperature of the three remaining sectors will be stabilized at 80K and measurements will be made on those.

About the current schedule for when to try and circulate a beam, he says "I know that officially it's still September but I have problems to sell that…" with a better guess of sometime in October (assuming the three sectors at issue don't need to be warmed up). He also remarks that it will be a while (2012?) if ever before the machine operates at 7 TeV. 5 TeV is the likely energy at the beginning, with a possibility of going up to 6 TeV.

Comments

1. **DB**
   June 10, 2009

   Isn't is surprising that such a straightforward test, i.e., the test for anomalously high resistance wasn't part of the quality control procedure during the initial construction phase?
   Makes you wonder.
   I'm also surprised that it has taken so long to identify this issue. Had it been flagged earlier there would have been plenty of time to warm up the sectors, test and fix.
   Perhaps there is something I'm missing.

2. **Yatima**
   June 10, 2009

   >Perhaps there is something I'm missing.

   You are probably underestimating the vagaries of real-world engineering. Especially for "once-off" projects of high complexity, things are left out, checks are not made, problems are not considered due to failure of imaginations, paperwork is lost and of course short-cuts are made due to budget and time constraints. Moreover, a lot of the stuff being designed is bleeding edge, so you cannot just expect production-run quality in any case.

3. **Steve Myers**
   June 10, 2009

   How are these connections soldered? I am familiar with control & construction work from 5 VDC to 13000 VAC & know a good electrical tech can tell an "iffy" joint by just looking. Of course, a resistance test proves it.

4. **Vladko**
   June 10, 2009

   I'm wondering if they could use some kind of isolated thermal suits to go inside the cooled down areas and do the repairs. If people can repair the Hubble space telescope in orbit I would hope this to be a simpler operation.

5. **Peter Woit**
   June 10, 2009

   Vladko,

   My understanding is that the problem is that there is not yet a reliable way of figuring out which of the cooled-down interconnection might be problematic and need repairs.

6. **ObsessiveMathsFreak**
   June 10, 2009

   You'd think there would be a better way to do things by now. The idea of having to shut down the entire LHC for weeks to deal with even a slight error resembles a programming project in which the most minute change requires a complete recomplication. If the LHC fails again it's going to look very bad for the entire project.

   Whats with the cool down times anyway? How long are the times at other accelerators like the Tevatron?

7. **Tom Whicker**

June 10, 2009

These are not your ordinary solder joints! The interconnects are layered with alloys of platinum if I recall correctly. Modelling behavior as it is cooled may be a difficult problem.

As an aside, these are anxious times for any project that depends on quantities of PGMs. Take a look at the five year
chart for platinum:
http://www.kitco.com/charts/liveplatinum.html

8. **Yatima**
June 11, 2009

>>The idea of having to shut down the entire LHC for weeks to deal with even a slight error resembles a programming project in which the most minute change requires a complete recomplication.

You meant to say “recompliation” I suppose (note that recompilations indeed took significant time a few decades hence, when I first hit the keyboards, but today are measured in minutes, which is nice as we then get “continuous recompilation” and the attendant quality improvements – but that’s another story).

Your comparison is wrong of course – I would say you could compare the innocent-sounding “solder joint fixing” with opening up, reviewing and redesigning a sizeable part of a software architecture for an embedded system which in this case is partly live. Any prospect of doing that will put cause sweat to appear on any project manager’s brow.

9. **Yatima**
June 11, 2009

Ouch, I misspelled “recompilation”, too. I blame my lousy keyboard and the infuriatingly tiny blog text area!

10. **Tom Whicker**
June 11, 2009

I cannot now locate the info on PGM metal use in the LHC, other than the use of Platinum for the temperature sensors. Sorry for the mis-information.

But there are plenty of exotic alloys throughout the machine. In the interconnection assemblies alone there are some quarter million components!

The number of connections is given at 123,00: http://cerncourier.com/cws/article/cern/29012

11. **Steve Myers**
June 12, 2009

A note from February CERN COURIER: Meanwhile, work continues apace on the repairs at the LHC. At the end of January, a dipole from sector 1-2, which had been identified as having an internal splice resistance of 100 nΩ, was opened up after removal from the tunnel and was found to have little solder on the splice joint. It is likely that a similar small resistance was at the root of the incident in sector 3-4. The LHC teams can now detect a single defective splice in situ when a sector is cold and they have identified another dipole showing a similar defect in sector 6-7. This sector will be warmed up and the magnet removed. Each sector has more than 2500 splices, but the resistance tests can only be conducted on cold magnets. Three sectors remain to be tested: sector 3-4, where the incident occurred, and the adjoining sectors, 2-3 and 4-5.
Looks like standard splicing. And, yes, platinum & platinum alloy used in high temp thermocouples.

12. **Sorinis**
June 18, 2009

So no operations this year after all. I’m willing to wager that the other sectors will need to be warmed up. So all these articles talking about how the LHC will valiantly run through the winter are now moot. I see from the article that Peter is not convinced that the LHC will ever run at 7 tev. This project is deeply disapointing both qulitatively and quantitatively.

13. **Peter Woit**
June 18, 2009

Sorinis,

It still seems possible they will not have to warm up the other sectors, looks like we’ll find out within the next couple weeks.

From what I’ve read, getting all the magnets to reliably work at high
enough fields to do 7 TeV may never be possible, but maybe something like 6.5 TeV is, and the difference of .5 TeV is not of big significance.

I suspect that the LHC will ultimately be a significant scientific success, although it may be a public relations failure. Promoting the thing as possibly discovering extra dimensions, and making a big public fuss over it while it is still being tested and debugged (something that always takes longer than expected…) seems to me to have been a mistake.

14. **Tomatonator**
June 18, 2009

I know this may not help, but it took over two decades for the 200-inch Hale Telescope on Mount Palomar to be completed, and it turned out to be a pretty useful instrument for astronomy.

And then there is the Hubble….

15. **Sorinis**
June 18, 2009

Peter,

Would you at least agree that the design seems unsound. If you must spend months warming and cooling every time there is a bad wire you are looking at years of stoppages for every little flaw immaginable . . . like a bad wire. God forbid you get something major like the explosion that was not an explosion last year. If this were an airplane, many people would be dead by now.

16. **Sorinis**
June 18, 2009

With regards to my latest comment above. I know because it has been pointed out that even wires at the LHC are special and made up of gold and platinum and angel dust. But I cannot imagine a smaller increment of tecnology than a wire. Except perhaps a bolt. But I bet even bolts there are fashioned by the elves from precious metals.

17. **Peter Woit**
June 18, 2009

Sorinis,

It's not at all clear that the design is unsound. So far the fact that it takes time to warm up and cool down sectors has not been the main thing delaying them (rather it has been things like bad magnet support design, bad PIMS, bad splices). Their problems have been pretty much the kind of thing you would expect when you first try and run an extremely complicated piece of equipment that has never operated before.

It's not an airplane, it's a tricky piece of scientific apparatus, and its problems have put no one in physical danger. When the thing is running, people are kept far away, and it should be able to explode perfectly safely, if that's what happens.

String Universality Reloaded

June 12, 2009
Categories: Multiverse Mania

I still haven't figured out yet what the arXiv's trackback policy is, since trackbacks to my blog entries sometimes appear there, sometimes not. One example in the "not" category is my recent posting about the Kumar-Taylor paper on "String Universality", which now has trackbacks to postings by Jacques Distler (recently seen here) and Dmitry Podolsky. The ways of the arXiv remain mysterious, but I can't help recalling that my original problems with them seemed to have to do with powerful people who did not like having their multiverse pseudo-science disrespected. Even without the trackback, I'm wondering if the authors of the paper somehow heard about my comments and felt they needed to be addressed, since a new version of the paper has just appeared.

The most extensive changes are to the section on "predictivity" discussed in my posting. Here's some of the added text:

> It may be that string universality holds for four-dimensional theories with supersymmetry, but that supersymmetry breaking mechanisms lead to a constrained subset of non-supersymmetric low-energy theories in 4D.
>
> It is possible that the dynamics of string cosmology may define a natural measure on the space of string solutions, which would favor some solutions over others. Currently, however, we lack a mathematically complete or background-independent formulation of string theory. It is likely that significant progress in this direction will be needed to understand the cosmological measure on the string landscape. In this brief discussion, we describe the situation for predictivity in the absence of such a breakthrough.

Some other changes:

> This may seem like a very awkward situation for string theory.

has been replaced by

> If we were living in six dimensions, then this would seem like a very awkward situation for string theory.

and the assurance that string theory would explain anything seen at the LHC has been toned down a bit, with

> any new and unexpected phenomena found in experiments at higher energies should be realizable in the string theory context

replaced by

> any new and unexpected phenomena found in experiments at higher energies may be realizable in the string theory context

Comments

1. **Sakura-chan**
   June 14, 2009

   100% off topic, but when you search for "terence tao" on google, the two related search suggestions at the bottom are grigori perelman and not even wrong.

2. **Tom Whicker**
   June 14, 2009

   The comments from Clifford Johnson and Jacques Distler are defensive; at times almost desperate. The level of denial
   is palpable.

   Even though string theory is now more than 30 years old, Clifford claims that it cannot be questioned because it is still in naiscent form and must be protected from prying eyes:

   "The point, Peter, is that we don't know whether it is right or wrong either, nor what the outcome of the entire program of research will be, ultimately. But we're not (or at least, that large percentage of the field I know and trust) are not presuming the answer at the outset, like you are. That's why it is called research. We don't know.

   But next he turns 180 degrees and lambasts critics of string theory, saying they do not speak with enough detailed
   equations and scientific rigour:

   "So, Peter, come out from behind that silly figleaf and tell us: How is the actual scientific argument for your claims coming along then? Have you written a paper yet? Constructed a demonstration the community can discuss? Is anyone going to see it soon?
   Try to do some science, Peter. Please try. Back up your claims with science and not obfuscation if you want them to be taken seriously. Until then, you're wasting everyone's time."

   All the while Clifford proudly proclaims that he is just too busy to read either book from Woit or Smolin.

   The taunting, sneering tone from Johnson and Distler is something I haven't heard since teenage locker-room days.
   If this is what the world of academic physics has come to; I'm so glad I left that world some time ago!

3. **George Dorn**
   June 15, 2009

   I am also flabbergasted at the tone used by what would appear to be grown up people on various blogs devoted to what would appear to be science and scientific arguments. If nothing else going back and reading all this stuff has provided hilarious entertainment. I have always been a little sceptic when historic accounts of scientific breakthroughs presented the community as conservative and childishly clinging to old ideas but I must admit that it all seems so much more plausible in the light of all these exchanges.

4. **Peter Woit**
   June 15, 2009

   Tom Whicker,

One of the most surprising things to me about the "String Wars" has been the strange behavior of the few well-known string theory bloggers. Besides the Distler/Johnson business, there's always Lubos, whose activities (see next posting) seem to embody Hunter Thompson's slogan "When the going gets weird, the weird turn pro."

5. **Arnaud**
   June 15, 2009

   Is String Theory really 30 years old? That would mean papers before 1980?

   A.

6. **Peter Woit**
   June 15, 2009

   Arnaud,

   Work on string theory as a theory of the strong interactions started in 1970, as a unified theory including gravity 1974. It was only in 1984 that it became incredibly popular, once Witten started working on it. We're about to hit the 25th anniversary of that event…

7. **nbutsomebody**
   June 16, 2009

   Lubos is a lunatic, and right now he is somehow out of scientific community. His views may well be dismissed as paranoia. However I am shocked to see how comparatively well respected guys like Clifford Johnson etc behave towards criticism. It is really a shame to string theory community, a big shame.

8. **GR**
   June 17, 2009

   Ostensibly string theorists are funded by NSF-type grants, and apparently have been for the past 20 years… is there no one at the institutions that's asked for tangible results? Or is the problem that almost everyone in HEP theory these days is doing string theory?

   As a grad-school-bound student, I can safely say that the diatribes by people like Lubos and Johnson are a big turn off for theory in general and HEP in particular. I (naively and optimistcally) want to do science and discover something new and cool, not bicker endlessly about what the "right" way to do physic is. I don't want to immerse myself in what (again, from an outsider's perspective) looks like groupthink and wagon-circling.

   And I know most undergraduates feel the same way about string theory. We're not dumb, we read PT and blogs. In the end, string theorists might have doomed themselves through this nasty and childish behavior.

   Also, as a side note, what's with the proliferation of extra dimensions? What's the historical justification for this avenue of research? Has anyone ever been able to show something about 4D from all this n-dimensional balderdash?

9. **nbutsomebody**
   June 17, 2009

   "Has anyone ever been able to show something about 4D from all this n-dimensional balderdash?"

   No, not in anything related to unification and standard model.

   However there is something called holography. Which may help us to aspects of gauge theory like QCD.

10. **anon**
    June 17, 2009

    "We're not dumb, we read PT and blogs."

    You contradict yourself.

11. **Rhys**
    June 17, 2009

    "However I am shocked to see how comparatively well respected guys like Clifford Johnson etc behave towards criticism. It is really a shame to string theory community, a big shame."

    So because I'm studying string theory I should feel shame at the actions of others engaging in the same pursuit? That's absurd, but it is not unusual to see this attitude endorsed, implicitly or explicitly, in various blogs (posts and comments).

    But to comment in dot points on the actual subject matter of the post:
    – As Peter admitted, the paper has no bearing on models of 4D physics.
    – The paper seems only to deal with the low energy theory ('massless' fields, roughly speaking). Stringy signatures will always appear at the compactification or string scale.
    – Nothing seems to have been proved.
    – It would be worth working on string theory, as a theory of quantum gravity/geometry, even if it could produce all low-energy effective theories.
    – I agree that if the swampland is empty, low-energy model building from string theory is somewhat pointless.

12. **nbutsomebody**
    June 17, 2009

    Rhys,
    I agree to your physics points. However you said
    "The paper seems only to deal with the low energy theory ('massless' fields, roughly speaking). Stringy signatures will always appear at the compactification or string scale."
    This is not strictly true. It depends on the particular theory.

    "So because I'm studying string theory I should feel shame at the actions of others engaging in the same pursuit? That's absurd, but it is not unusual to see this attitude endorsed, implicitly or explicitly, in various blogs (posts and comments)."

If you are just “studying” string theory, you may not have to bother about them. However, if you are a “string theorist”, you may have to share a little responsibility. There is nothing absurd about it, your credulity and scientific standard is indeed judged (at least in part) by looking at the scientific community you belong to. Look these guys are not some nuts who are doing fringe science. They are the main stream guys. People read their papers, they arrange conferences, they have money to heir postdocs/visitors/students. They appear in front of press, they act as refree and the list goes on and on. Like it or not they are the spoke person for the community, hence if you belong to the community they speak for you too :(. Only way to get rid of this uncomfortable situation, I believe, is a overt opposition to their erratic behavior.

13. **Peter Woit**
June 17, 2009

Rhys,

What I find remarkable here are not the arguments over remaining hopes that string theory somehow can say something about particle physics, but the notion that “string universality” is somehow a positive thing, rather than a simple expression of failure. When an idea doesn’t work, you’re supposed to give up on it, not promote it as some new kind of science.

14. **anon.**
June 18, 2009

‘When an idea doesn’t work, you’re supposed to give up on it, not promote it as some new kind of science.’

As people keep pointing out, dogmatic belief systems with no evidence from start to finish like religions, don’t operate like rational science. When they fail to a achieve something, the failure is interpreted as a proof of decisive knowledge that it is impossible for anybody ever to achieve that thing.

E.g., if string theory can’t predict everything, then that’s not the failure of string theory. It’s proof that nature is simply not predictable. Don’t you see that, you fucking moron?

15. **Mitch Miller**
June 18, 2009

Peter,

I apologize in advance as this is pretty far off topic, but I have a question on the slides that you posted on the other thread. On slide 30 you say that violations of WW scatering at the LHC could actually be good for string theory, I don’t see how that is possible. The standard model is already falsified because of gravity, what is not known is the energy scale at which naive GR+SM stops being valid. If violations are found, it seems like string theory would be dead since AFAIK string theory can’t handle Lorentz breaking at this (possibly any?) scale or unitarity/analyticity breaking.

I think I agree with the spirit of what you are saying, this is sort of lame as a test of ST since it is such a long shot to begin with but I’m not sure if I am missing something else. And feel free to ignore/delete if too off topic. Thanks.

16. **Peter Woit**
June 18, 2009

Mitch,

The only point I was trying to make was that the claim made in that case about a test of string theory was actually about a test of basic QFT axioms. There was an argument on various blogs about this way back when, but it still seems to me that a Lorentz-breaking string theory ground state is possible. Unlike QFT, you don’t have a non-perturbative definition, or agreed upon axioms.

As a matter of sociology, I am quite convinced that if the LHC sees something that violates QFT axioms, this will be touted as evidence for string theory…

17. **Mitch Miller**
June 18, 2009

Thanks, that clears it up. It will be very interesting to see how people react if something crazy actually does happen at the LHC.

La Tête dans les Étoiles

June 15, 2009
Categories: Uncategorized

This past weekend the city of Villeneuve-sur-Yonne organized a science festival entitled La tête dans les étoiles; les pieds sur terre! It featured Lubos Motl speaking on "Physics at the Planck Scale", and the Bogdanoff brothers on "The Beginning of Time".

Comments

1. **Arnaud**
   June 15, 2009

   Well, I'm not sure if you posted that in jest, Peter?

   I am slightly ashamed at a town in my country of origin putting on such a mish-mash, looking at the program, Lubos is mixed with a farmer's market and wild bird conservation society. I think rather than hear the Bodganov I'd prefer to join the "cheese workshop"! Then again, Villeneuve sur Yvonne is hardly a hotbed of scientific debate…
   I think you previous comment about weird and weirder hits the mark.

   What you won't know is that the infamous Brothers used to present a science and SiFi kids show on French TV in the 80s, which I used to love, as well as I (probably) a good portion of the Gallic physicists currently using your site. I suppose that's one of these embarassing childhood stories they tell at weddings.

   How on Earth did you find out about this anyway? too much time on your hands, mate.

   A.

2. **Peter Woit**
   June 15, 2009

   Arnaud,

   I've been following the activities of the Brothers for quite a few years ago, and quite a few people have told me of their fond memories of the TV series.

3. **theoreticalminimum**
   June 15, 2009

   It's funny Lubos still hasn't stopped being a Harvard professor. Makes me smile. Thankfully the kids were already half asleep by the time he and the crackpot brothers gave their talks, and obviously Hubert Reeves would have made the biggest impression (although I am intrigued how he could have got involved..).

4. **Chris Oakley**
   June 15, 2009

   > … wild bird conservation society

   Maybe they are the ones who invited Lubos.

5. **John Baez**
   June 16, 2009

   Is this their only performance, or have they teamed up to form a kind of traveling circus? Does anyone know? I'm in Paris for two months — it might be fun to see them if they come to town.

6. **MathPhys**
   June 16, 2009

   Are the B Brothers heavy users of plastic surgery? or is it the Elephant Man syndrome?

7. **Peter Woit**
   June 16, 2009

   Hi John,

   I don't know about any more appearances, but the new Bogdanov book
   "Au Commencement du Temps" is coming out tomorrow.

   And on Saturday at 3:20pm, the France 3 channel is having a program about the Bogdanovs, with them interviewed in their Paris "gothico-medieval" home.

8. **Fabien Besnard**
   June 16, 2009

   Arnaud, that's Villeneuve-sur-Yonne… Villeneuve-sur-Yvonne probably hosts other kinds of shows…

9. **observer**
   June 16, 2009

   Never more appropriate the saying " …This is a place for everything and every thing will fit in its place". weird at best.

10. **Cormac O Raifeartaigh**
    June 17, 2009

That was I call a neutral post. Magnificent restraint…

11. **YBM**
June 21, 2009

Would you believe this, Peter (I guess you did): again, there is a lot of "quotes" of you in their book.

What's hilarious is that they now present themselve as mainstream string theorists, quoting t'Hooft and Witten at every page.

12. **Peter Woit**
June 21, 2009

Thanks YBM, I'll have to at some point get a copy and find out what I have to say about the Bogdanovs. If it's anything positive about their research, it doesn't correspond to my actual views.

I see from the publicity for their book that

"ils enseignent la physique primordiale à l'université Megatrend de sciences appliquées de Belgrade."

so I guess they're no longer in Riga (where I'll be visiting in a couple weeks) at the "Mathematical Center of Riemannian Cosmology"

http://phys-maths.edu.lv/

Funny, but the web-site of Megatrend University

http://www.megatrend.edu.rs/vest_strana.php?v=806&language=1

seems to think that they're professors at Bourgogne University…

13. **YBM**
June 21, 2009

I doubt that they are actually teaching there, anyway they are registred there, but, as you noticed, it's a plain lie to present themselve as professors from Bourgogne Univ.

Megatrend is not a real university, it's a private school of economics with a bit of engineering. By chance it happens that its director is the translator of the serbian edition of "Avant le Big Bang".

Same dirty old tricks as usual. These days in France, the Bogdanov are playing a lot with policital influence (they are members and have responsabilities in the presidential party, they get a new TV show on the main public channel by direct order from the french presidency, etc.)

As disgusting as ever…

Summer Programming

June 18, 2009
Categories: Uncategorized

As far as academics are concerned, summer has started, which means that there are lots and lots of conferences going on. The past couple weeks we've had two here in the Columbia math department, one in algebraic geometry, and another covering various related topics in hyperbolic geometry and knot theory.

Next week 450 or so physicists will gather for the big annual string theory conference, and a couple weeks ago there were more than 400 participants at the big annual SUSY conference, SUSY 2009. For more on this, see reports from Jester and Sabine. Jester's take on the conference included:

> Over 400 participants, not counting squatters. 42 plenary speakers, most of whom witnessed the glorious days when supersymmetry was conceived. Seven parallel parallel sessions to cover every aspect of supersymmetry that has not yet been covered thoroughly enough. Royal coffee break menu fully adequate to the royal conference fee. And so on and on since 16 years and into the future.
>
> Meanwhile, there is no single hint from experiment that supersymmetry is realized in nature… but that should not upset anyone. As my fellow blogger skillfully put it, supersymmetry is the "shining beacon", the "raison d'etre" and for this reason "the conundrum is how it will be discovered, not if". That's why every year we come together to enjoy old familiar faces and old familiar talks. The point is, while waiting for the inevitable, to maintain that kind of spirit that David Lodge praised in his books.

At Santa Cruz this week, there was a conference in honor of the 60th birthdays of Tom Banks and Willy Fischler, blogging from David Berenstein.

At Penn there's a summer school on Geometry of Quantum Fields and Strings, blogging at Rigorous Trivialities.

On the multiverse front, Thibault Damour has a survey article about constancy of physical constants, where he makes the reasonable argument that checking such constancy is one of our few ways of getting insight into the origin of these parameters of physical theory. Sometimes you see the claim made that string theory predicts time-dependence of constants (since they are moduli parameters), other times the claim is made that string theory does make one prediction, that such constants won't change (due to energetics of the landscape). Damour summarizes this as

> However, there is no firm prediction for the observable level of EP violation. Actually, the current majority view about the "moduli stabilization" issue in String Theory is to assume that, in each string vacuum, the coupling constants are fixed by an energy-minimizing mechanism which is generically expected to forbid any long-range violation of the EP. This, however, makes EP tests quite important: indeed, they represent crucial tests of a widespread key assumption of string-theory model building. This exemplifies how EP tests are intimately connected with some of the basic aspects of modern attempts at unifying gravity with particle physics. Some phenomenological models (inspired by string-theory structures, or attempting to understand the cosmological-constant issue) give examples where the observable EP violations would (without fine-tuning parameters) be just below the currently tested level.

The "string universality" principle discussed here recently is related to Leibniz's "Principle of Plenitude":

> all logically possible "things" (be they objects, beings or, even, worlds) have a tendency to (and therefore must, if one does not want contingency – be it God's whim – to reign) exist.

So, experimental measurements are important not because they can tell us whether string theory is right or wrong, but because they can tell us which kind of string theory is right….

The latest Seminaire Bourbaki was last week, here's a summary of the talks. Edward Frenkel has posted his survey of Gauge theory and Langlands duality on the arXiv.

Earlier this week I learned from my colleagues one obscure piece of mathematical culture that I had been unaware of. Physicists have the famous story of how Alpher and Gamow brought in Bethe as co-author to improve the author list, but it turns out that mathematicians have a somewhat different story of this kind. At lunch one eminent algebraic geometer started snickering when someone (using standard terminology) brought up the well-known ring associated to an algebraic variety due to David Cox. At this, it was pointed out that Cox was co-author of a famous paper with Steven Zucker, and the story goes that this came about because Cox had decided once he heard of Zucker that a Cox-Zucker paper just was asking to be written. A supposedly authoritative source on the internet claims:

> Cox and Zucker were admitted as grad students to Princeton in precisely the hope that they would someday collaborate. This kind of forethought is why Princeton is Princeton.

For a related mention of this, see the Journal of Improbable Research.

Comments

1. **mifune**
   June 18, 2009

   It was Alpher and Gamow who brought in Bethe.

2. **Shantanu**
   June 18, 2009

   Peter, I think I mentioned this, but another meeting in Columbia next week.

3. **hackenkaus**
   June 18, 2009

   Nati Seiberg I'm sure will be shocked to learn he is turning 60.

4. **woit**
   June 18, 2009

   mifune and hackenkaus

   Oops, thanks for the quick corrections, now fixed.

5. **Felipe Zaldivar**
June 18, 2009

You may be interested in the following picture:

http://hopf.math.purdue.edu/pictures/princ70.gif

6. **Peter Woit**
June 18, 2009

Thanks Felipe,

Well, at least part of that story seems to be true…
Interesting to see that Frank Wilczek was in the same year.

7. **Tomatonator**
June 19, 2009

Here is an article from 2008 on Ralph Alpher, who may have been a credit victim of the Alpher, Bethe, Gamow joke:

http://philosophyofscienceportal.blogspot.com/2008/03/credit-where-credit-is-due-big-bang.html

and this:

http://philosophyofscienceportal.blogspot.com/2008/05/book-review-big-bang-origin-of-universe.html

8. **?**
June 19, 2009

My english is not good enough to understand why "Cox-Zucker" is funny, can somebody please explain?

9. **!!**
June 20, 2009

> funny, can somebody please explain?

If you read it backwards you'll notice an allusion to an old Yiddish joke first quoted in English by Shakespeare.

10. **Peter Woit**
June 20, 2009

?,

Here's a hint: everyone who tried to explain this explicitly here had their comment intercepted by the automatic spam detector.

Beyond that, I'm not convinced that your English is really that bad…

11. **Henry Tucker**
June 22, 2009

Felipe, where can one find scans of those Princeton entering mathematics graduate student group photos? I've seen them around the web and have been curious.

12. **Felipe Zaldivar**
June 22, 2009

Hi Henry!

I found that one picture when looking for something else at the "Hopf Archive", a site created by Clarence Wilkerson with news and an album of photographs of fellow algebraic topologists, but I don't think that there are more of those "entering class photos" in that site: http://hopf.math.purdue.edu/

News from CERN, Witten Interview

June 18, 2009
Categories: Experimental HEP News

The new CERN Bulletin is available, and it contains a link to a recent video interview of Witten, who has been visiting CERN during the past year.

Some other things in this issue:

- Bill Gates recently visited the LHC, bringing his son Rory. Gates announced:

> I just bought the rights to the Feynman 'Messenger Lectures' that he gave in Cornell in the 1960s. The BBC filmed Feynman giving what I think are the best physics lectures I have ever seen. So we are going to make these lectures free for anyone to watch.

- There's news about the LHC restart here and here, including the warming up of sector 4-5 discussed in postings here recently. Here's what Director General Heuer has to say:

> The bottom line is that we remain on course to restart the LHC safely this year, albeit at reduced energy.
>
> A tremendous amount of work has been done to fully understand the splices in the LHC's superconducting cable, one of which was the root cause of the incident last September that brought the LHC to a standstill. We've learned a great deal. It's mostly good news but there's also plenty of food for thought. The good news is that all the measurements done so far indicate that we will be ready by September or October to run the LHC safely at around 4-5 TeV per beam. If new evidence appears in the meantime to suggest otherwise, we'll modify the energy for this year's run accordingly. The food for thought is that the same tests tell us that before we can run safely above 5 TeV, more work is needed, and this will be carried out in a shutdown starting in Autumn 2010.
>
> Many of you will have heard, or seen on the LHC web pages, that we're warming up sector 4-5. This sector can be warmed and re-cooled within the time remaining before we inject the first beam of 2009 into the LHC, and doing so will give us increased confidence that we fully understand the splices. We're warming up this sector because we have developed a new non-invasive technique for investigating the splices. The sector has been measured at a temperature of 80K, indicating a suspect splice or splices. By warming the sector, the results of the test can be checked at room temperature, thereby validating the procedure at 80K. If the 80K measurements are validated, any suspect splices in this sector will be repaired.

From this, it sounds like the current plan is to not wam up the cold sectors, but reach a decision on what the highest energy they can safely run at in case of a quench, given their understanding of remaining problems with bad splices. This might mean running below the planned 5 TeV/beam.

- On July 1 CERN will be providing training for its employees on how to deal with the press.
- On July 2 there will be a talk by Steve Myers on the LHC status

Update: While the CERN DG indicates readiness of the LHC for start-up in "September or October", the most up-to-date schedule (which somehow came into my possession this morning…) shows powering tests in sectors 4-5 and 8-1 going through the end of October, so machine checkout and first beam not until the beginning of November.

Update: There's now a press release. According to this, they are now planning to start up at some energy in the range 4-5 TeV, currently 2-3 weeks behind schedule (so start-up second half of October).

Comments

1. **chris**
   June 19, 2009

   very interesting

   the marketing speak they adopted (good news vs. food for thought) and also the ever so slightly decreasing beam energy (4TeV now – what will it end up being?) and slipping schedule (first beam end of december as it seems).

   maybe it was not such a good idea celebrating September 10 2008 so publicly. or is Holger Nielsen right after all?

2. **Sorinis**
   June 19, 2009

   Chris

   It might end up beeing the same energy as the Tevatron. Wouldn't that be funny? It is also interesting that Peter has seen the latest schedule but Heuer somehow has not. Makes you wonder about the lax attitude at CERN. I think you are right. CERN has a long history of declaring success before even operating. We've gone from discovering all the secrets of the universe including extra dimensions to a really humiliating media spin about a freefloating schedule and ever decreasing energies.

   They may have lectures on dealing with the press but you can't put a positive spin on this.

3. **Cplus+**
   June 21, 2009

   le Déluge commence.

   From Luca Trevisan:

   "Two days ago, the Chancellor announced that because of the cuts expected as a consequence of the state-wide budget crisis, UC Berkeley needs to cut about $100 millions. Next year, we should expect a complete freeze on hiring, layoffs of administrative staff, strong cuts to student aid, increased tuition, and salary cuts of 8%. For 2010-2011, rumors are that the sun will go dark, it will rain blood from the sky, and then the locusts will come and eat us alive. Unfortunately, 2011-2012 will be much worse…"

   If the markets resume their decline during the next two years, nearly all the University Endowment funds, which to date have been only badly damaged, will be unable to contribute to cover operating expenses and the non-public universities will experience the same or posssibly worse.

4. **Thomas R Love**

June 21, 2009

I was happy to hear that Gates will make Feynman's Lectures available. Our library has them on film but the film is in very bad shape. It will be a pleasure to watch them again but without having to change the film reels!

Manufacturing universes in a fractal multiverse

June 21, 2009
Categories: Multiverse Mania

I gather that the World Science Festival here in New York a week or so ago was a great success, although I was out of town for most of it. The one part of the program I was dubious about (Infinite Worlds) seems to have come off even more one-sided than planned, since David Gross couldn't be there.

There's a report on this at Ars Technica from someone who was sitting near Cameron Diaz while watching the program. Philosopher of science Nick Bostrom did point out the obvious, that for the multiverse to be science it has to predict something. Someone seems to have convinced the author of the piece that there actually is such a prediction:

> Early in our Universe's history (before the mulitiverse's inflation pulled things apart), it was possible that the Universe bumped into a neighboring one. If that's the case, there should be remnants of that event buried in the cosmic microwave background. Less than a month from now, the ESA's Planck mission should arrive at the L2 Lagrange point with instruments sensitive enough to pick up this signal.

So, I guess in a couple years from now, we'll know if there is a multiverse or not…

For another report, see here.

Sean Carroll reports here on some other parts of the festival, including the panel on Time Since Einstein, where he explained to the audience that "the fact that an a splattered egg cannot turn back into a pristine unbroken egg is the best evidence we have that we live in a multiverse."

Comments

1. **milkshake**
   June 21, 2009

   regrettably, Dr. Carroll cannot take back the splattered egg remark because it would collapse our multiverse

2. **ManyMe**
   June 22, 2009

   I think the nonsense of Sean Carroll is the best proof for the multiverse.

   If there is only one universe, it would be very unlikely that it contains physicists who make such statements.
   It is much more likely that there is a large number of universes with silly physicists trying to explain eggs and such – and we just happen to be the one with Sean Carroll in it …

3. **bjm**
   June 22, 2009

   It seems to me that whenever nature is able to do something, it does it more than once.

4. **Tim**
   June 22, 2009

   Is there a reference to a paper to back up Sean Carroll's claim?

5. **Peter Woit**
   June 22, 2009

   Tim,

   There's a paper with a student from 2004, extensive promotion of the idea in Scientific American and elsewhere, and he's writing a book on the subject. There are some links here:

   http://www.math.columbia.edu/~woit/wordpress/?p=689

6. **Tim**
   June 22, 2009

   Hello Peter,
   thanx for the link, unfortunatly I did not really understand the paper by Collins/Chen, but I think I understand now a little better the motivation of Lubos Motl to write in one and the same sentence about a paper "I don't understand it, but I am absolutly shure it's crap" as he does time and again in his blog…

7. **Tomatonator**
   June 22, 2009

   I am amazed at how catty scientists can get regarding each other and their work.

   I guess you are all human, too.

8. **Peter Woit**
   June 22, 2009

   Tim,

   I think I understand the paper, but just don't see how it adds up to a scientific explanation of anything. I see that Sean has a website up for the book

   http://preposterousuniverse.com/eternitytohere/

including a copy of the book prologue

http://preposterousuniverse.com/eternitytohere/prologue.html

where he quotes the reaction of one physicist to a talk of his about this, a reaction which I'd guess is pretty common.

The main problem I see here, with much of the multiverse stuff, including Sean's, is that extremely speculative ideas that most physicists don't think deserve to be called science are being heavily promoted to the public, often in a misleading way. There was always a bit of this problem with string theory, but this is far worse.

9. **Arun**
   June 22, 2009

   …"the fact that an a splattered egg cannot turn back into a pristine unbroken egg is the best evidence we have that we live in a multiverse."

   Since the microscopic laws of physics are (effectively) time-symmetric, all that I come up with is that there are far fewer initial conditions where splattered eggs unsplatter than where splattered eggs remain splattered. This is a fact independent of cosmology. Whole pristine eggs do not arise from a process of unsplattering, other methods are far more probable.

   I need to broaden the question – "explain a universe in which eggs and observers like myself can arise", before there is a mystery. It remains a mystery. But it is an even deeper philosophical mystery as to how postulating an infinite number of other universes so that the asymmetry in initial conditions can somehow be erased*** constitutes an explanation.

   *** The fact that there are far fewer initial states that lead to splattered eggs unsplattering than initial states where splattered eggs remain splattered is simply a matter of counting and so remains true in each universe in the multiverse.

10. **Rodrigo**
    June 23, 2009

    Look at this:

    http://dilbert.com/strips/comic/2009-06-23/

Templeton Foundation Strategy and Planning

June 22, 2009
Categories: Multiverse Mania

Most physicists are rather dubious about whether "multiverse" research is deserving of any support since it's not clear that it is even science. Because of this, such research has been finding financial support in recent years not from conventional sources like the NSF, but from the Templeton Foundation, a very wealthy organization devoted to the goal of bringing together science and religion. Some examples of this funding include the World Science Festival program mentioned in the last posting, and the Foundational Questions Institute, which provides grants, many of them for multiverse research.

I'd always wondered if there was some kind of organized effort by Templeton to push this kind of research, and and got a partial answer to this question recently when I took a look at some of their web-pages. Last year they organized two days of conferences at the Royal Society in London, associated with their choice of cosmologist and Catholic priest Michael Heller for their 2008 Templeton Prize of a million pounds. The preparatory readings page of the second day's conference provides a link to a document entitled Towards the Establishment of the Philosophy of Cosmology at Oxford. There's also a link to "Password Protected Papers" on the topic of Toward a New Philosophy of Cosmology, but these papers aren't really password protected since the password ("multiverse") as well as the user name ("universe") are given right next to the link.

If you follow this link, you are taken to the web-site for "A Strategy and Planning Workshop of the John Templeton Foundation", held in May 2007 at the Royal Society. The purpose of this workshop is described as:

> To bring cosmologists and philosophers together to review the 'state of the field' of the philosophy of cosmology and to explore the most effective ways of developing and enriching the philosophy of cosmology. How might the John Templeton Foundation contribute to field development?

and here are some other extracts from that page:

> The John Templeton Foundation would like to help develop this field. We are considering the creation of substantial research support opportunities in the philosophy of cosmology. In addition, our expectation is that there is some need for infrastructure, and the Foundation would be interested in helping to support development of a suitable context for a flourishing field….
>
> …the afternoon session, a discussion of strategy for field development. The Foundation is serious about providing resources if we can find a strategically-effective plan to make a difference.

This afternoon session was described in the program as follows:

> *Goal*: Finally, we explore what is needed to develop the field of philosophy of cosmology in a dramatic way over the next decade. Our initial thought is that the field needs some infrastructure development as well as basic research support. Because this work must bring together such disparate fields, it is not easy working through basic university structures. It may be that beyond core research projects, we need to support the creation of major long-term initiatives or centers, and help establish training for a new kind of scholar as well as faculty positions and perhaps other elements of infrastructure such as book series or a journal to take two common elements of the scholarly enterprise. Also needed, perhaps, are ways of making it more visible to the public in ways that improve on current popular presentations.

The afternoon talks were by Priyamvada Natarajan, a Yale astrophysicist described as "currently a member of the advisory panel for the Dialogue on Science, Ethics, and Religion of the American Association for the Advancement of Science (AAAS) and has an abiding intellectual interest in understanding issues in spirituality and science", and 2004 Templeton Prize winner George Ellis who is described as "co-author of *On the Moral Nature of the Universe: Theology, Cosmology, and Ethics"* and "editor of the *Far Future Universe: Eschatology from a Cosmic Perspective*." The respondent was the Reverend Keith Ward, described as "his main work is a four-volume comparative theology from OUP", and "His most recent book, published in 2006, is *Pascal's Fire – religious understanding and scientific faith*." The evening program was devoted to a celebration of Bernard Carr and the book based on three Templeton-funded conferences that he edited, Universe or Multiverse?

In a whitepaper prepared for the strategy session, Carr argues not just for the anthropic principle, but for the necessity of a "new science":

> In one sense the current debate today about the scientific status of M-theory and the multiverse proposal is nothing new. We've seen that progress on the outer and inner fronts has always been controversial, so perhaps history is just repeating itself. However, there is another sense in which the current situation is very special. This is because today — for the first time — the boundaries at the largest and smallest scales have connected. They are unified through quantum gravity and so the two science/philosophy frontiers have merged.
>
> One might argue that this merging represents the completion of the scientific process. This is why the symbol of cosmic uroborus is so powerful: it represents both the evolution of our knowledge of the universe and the triumph of physics in producing a unified view of the world. Have we therefore reached the endpoint of science just as the macroscopic and microscopic frontiers merge? I doubt it. Personally I believe this merely represents a transformation in the perceived nature of science.

He then explains why Templeton money is needed:

> Whether one redefines science to include exotic new ideas is not just a semantic issue. It also has practical implications because research in topics deemed to be "unscientific" is unlikely to be funded through the usual channels. For example, even in my own field, I have sometimes found that I cannot obtain a grant for a research project because some funding council has changed the definition of what constitutes "astronomy". There will always be research areas (especially cosmological ones) which straddle the border between science or philosophy and this is why JTF's initiative to support such areas is so important. It can promote or nurture ideas which have not yet been accepted into the world of legitimate science.

Carr is a past president of the Society for Psychical Research, and in its proceedings recently published a paper entitled *Can Psychical Research Bridge the Gulf Between Matter and Mind?*. In the white paper he advocates the idea that Consciousness is somehow part of this new science:

> Another feature of the new paradigm – and here I am definitely venturing beyond the boundaries of current science – is likely to be mind. One feature of the Universe which is noticeably absent in the current paradigm of physics is consciousness….
>
> …even the mention of the C word was taboo until recently. On the other hand, one might be sceptical of physicists' claim to be close to a "Theory of Everything", when such a conspicuous aspect of the world is neglected.
>
> Certainly physics in its classical form cannot incorporate consciousness….
>
> But what has this to do with cosmology? At first sight, developments in cosmology and particle physics might appear to have diminished the status of mind. The more we understand the Universe, from the vast expanses of the cosmos to the tiny world of particle physics, the more irrelevant humans (and hence minds) seem to become. Curiously, however, in recent decades cosmology has brought about a reversal in this trend, suggesting that mind may be a fundamental rather than incidental feature of the Universe. I'm referring here primarily to the Anthropic Principle…

He ends with some comments on theology, beginning with:

> Of course, most scientists are even more uncomfortable straying into the domain of theology than philosophy, so the G word (God) is usually regarded as even more taboo than the C word. However, since science seems to be coming to terms with the A and C words, perhaps the same will happen with the G word. Maybe there is a gradual process of desensitization in which the A, C and G words become successively accepted!

The whitepaper by Ellis deals with the crucial question of whether untestable speculation is science:

> There is also a reverse flow, whereby the development of the philosophy of cosmology – pushing the philosophy of science to its limits – may well have useful influences in wider realms of philosophy. Indeed it must be so, as cosmology helps us understand the nature of being human by clarifying the overall physical context through which we come to have our existence. So a useful part of the whole enterprise may be to try to develop that link: the different ways in which our understanding of the universe helps shape our views of humanity, and the ways that the philosophy of cosmology may help shape the philosophy of science. This may be particularly useful in terms of those aspects of physics which also face problems of testability for fundamental reasons, and so where some physicists are proposing to lessen the degree of rigour usually demanded in a scientific proof, decrying usual scientific criteria of testability as they do so (string theory comes to mind).

The whitepaper of Simon Saunders is an earlier version of the proposal for a new Oxford program available on the 2008 conference site. It proposes a new masters level 2 year course in philosophy of cosmology, with Templeton funding 2 3-year postdocs, a 5-year research fellow, a visiting scholars program, buy out time for those teaching in or administering the program, and funding for fellowships for graduate students. He also proposes to spend about 20,000 pounds a year on an outreach program to promote "philosophy of cosmology" in schools.

I haven't seen any evidence that this planning and strategy session led to anything. For one thing, there does not seem to yet be a "Philosophy of Cosmology" program at Oxford, although perhaps Templeton will at some point fund such a thing. But, this session does give a good idea of where some people would like to take theoretical physics, and indicates Templeton's interest in the idea of heavily funding ventures to promote such a "new science".

On a somewhat related note, see today's PZ Myers posting entitled [The name "Templeton Foundation" needs to become a mark of failure](#).

Comments

1. **Matt Leifer**
   June 22, 2009

   Hmm. I do actually think that developing the philosophy of cosmology is a good idea, but not if support for the multiverse is taken as a foregone conclusion. We need people who are willing to engage in a thorough criticism of these ideas as well.

2. **Tony Smith**
   June 22, 2009

   Peter, about Templeton and FQXi and the multiverse,
   you mention "… the Templeton Foundation … funding … the Foundational Questions Institute [FQXi] …"
   and
   the FQXi web site (including blog entries by Anthony Aguirre, who appears to be an official FQXi spokesman regarding the essay contest) says in part:
   "… FQXi Essay Contest …
   The theme for this Essay Contest is:
   "What is Ultimately Possible in Physics?" …
   the challenge will be to maintain high relevance by focusing on *ultimate* possibility …
   the focus of these essays should be
   more about physics and what is ultimately possible *in physics*
   rather than what is ultimately possible *using physics*, i.e.
   technologically attainable in some way
   …
   an essay that is centrally about how a multiverse limits physics ultimate predictive power — or doesn't — would feel … relevant
   …
   Entries will be accepted from May 15, 2009 to October 2, 2009
   …
   This forum category will contain discussions of essays submitted to FQXi's 2009 essay contest. Please check back in early June to join the discussion! … There are no topics yet in this category. [as of Monday 22 June 2009 around 5:30 PM EDT] …".

   It seems interesting that even well after early June has come and gone,
   and well over a month after the essay contest opened on 15 May 2009,
   and even though FQXi's spokesman explicitly approved the topic
   " … about how a multiverse limits physics ultimate predictive power — or doesn't …"

   there are NO essays on that (or any other, for that matter) topic on the FQXi Forum web page where they are supposed to appear for discussion.

   Does that mean that few essays have been submitted
   or
   does that mean that FQXi is having difficulty screening/refereeing submitted essays to ensure that they "… explore the most effective ways of developing and enriching the philosophy of cosmology …"
   and "… to promote such a "new science" …"
   ???

   Tony Smith

3. **Peter Woit**
   June 22, 2009

   Tony,

   I have no idea how FQXI is dealing with their essay contest, you have to take that up with them, not here.

4. **Eric Habegger**
June 22, 2009

Even though the multiverse idea seems intrinsically silly I actually don't think the idea of developing a philosophical structure that accompanies some of our newer knowledge is a bad idea at all. For instance, I think people should understand that the multiverse idea is an essentially amoral concept. That is, it projects that anything that has a probability of occuring "does" occur, even if not in our universe. Supposedly it occurs with a statistical rate that coinsides with the initial probability.

But the whole idea gives one, if one chose to, the ability to rationalize doing bad things because someone will commit that abomination somewhere. So it gives the excuse that you can do anything you want if it will give you an advantage. I think besides being a really stupid idea the multiverse is an essentially destructive philosophical stance. I think everyone should understand that.

I am much more comfortable with one universe, one event occuring out of the initial probability of events, and a world which can be better or worse for that event happening. It gives all of us a responsibility for living a life that will provide a better world in the future.

5. **Peter Woit**
June 22, 2009

Matt,

If people want to discuss the "philosophy of cosmology", that's fine, but they should do it as philosphers, according to the intellectual standards of philosophy. The problem is that there are quite a few physicists, some motivated by a need to find some way to avoid admitting that string unification has failed, who want to engage in something that is not science, claiming it to be a "new kind of science". Unfortunately, the Templeton organization seems interested in funding and encouraging this (or maybe not, maybe the "planning and strategy" workshop convinced them that this would be a mistake…).

Eric,

An excellent example of something which is not science and has no place in a science department.

6. **David H. Miller**
June 22, 2009

Peter, you said:
>If people want to discuss the "philosophy of cosmology", that's fine, but they should do it as philosophers, according to the intellectual standards of philosophy.

Putting all flippancy aside (a bit difficult in this area!), are there really any "intellectual standards of philosophy" generally accepted among academic philosophers?

Can the pomo folks, any lingering existentialists, the growing group who hark back to the ancient Greeks, the old "analytic philosophers" of the twentieth century, and the few philosophers who have some literacy in science really agree on any intellectual standards at all?

Don't get me wrong – I respect and like personally some individual philosophers. For example, I am friendly with Colin McGinn, even though Colin is more pessimistic than I about solving the problem of consciousness. Colin tries to seriously learn about science before he comments on it, he separates his own speculations from established science, he adheres to standard norms of reason and evidence. He's a good guy, even if you disagree with him.

But I am doubtful if he is the norm among contemporary philosophers.

Of course, I suppose this goes back to your ongoing central theme, Peter: science has outstripped other intellectual disciplines by adhering to one central rule – our speculations have to be checked by experiment.

Take that away, and chaos looms.

Dave Miller in Sacramento

7. **rrtucci**
June 23, 2009

So, why do religious people like Templeton believe in a multiverse? Where does the Bible talk about it?

8. **cos**
June 23, 2009

Maybe they just view it as solidarity among faith-based ideologies.

9. **Tim Solton**
June 23, 2009

Anybody who has ever tried to write down a definition of the term `universe' will see that his definition does not allow him to define the word `multiverse'.

It is a bit like first saying `everything' and then saying `even more everything'.

I find it appalling that a person with high school degree is not ashamed to use such words. But, "ex falso quodlibet". Maybe that is the real motto of the Templeton Foundation?

Tim

10. **asdfasf**
June 23, 2009

"Philosophy of science is about as useful to scientists as ornithology is to birds."

11. **Tim vB**
June 23, 2009

Hello,
actually there is already a very nice work addressing the program of the Templeton Foundation, see "The Physics of Christianity" by Frank J. Tipler. He has a nice proof of

the existence of god using cosmology and some concepts from QM/QFT, it basically goes like this:
The universe will collapse (big crunch), all intelligent live forms will unite in the process to form one entity, called the omega point. The heating universe will then fuel the ability of the omega point to process information, so it's intelligence will diverge to infinity. Now imagine that your brain could process information twice as fast as it does now, that would mean you would have twice as many thoughts per second than you have now. Tipler's hypothesis is, that you would experience a second therefore as twice as long as you do now. For the omega point this means that, while the (proper) physical time of the existence of the omega-point is finite, it's consciously experienced time will be infinite since it's information processing ability will diverge. Note that the concept of "time as experienced by a consciousness" does not correspond to any concept of time as an observable in conventional physics, so we really are expanding physics as a science in one of the direction the Templeton program would like to.
Now, what does the omega point do with his abilities? It will of course emulate everything that ever existed before in the universe, including you and me (our original selves are probably long dead and gone by then).
Since QM says that identical systems cannot be distinguished, an emulation of you cannot be distinguished from you, so it is really you. So you and me will be resurrected by the omega point, and be judged by it (why that necessarily happens escapes me at the moment, but hey, I read the book some 15 years back).
So here we arrive at god and judgment day by scientific reasoning.
Tipler doesn't use any stringy concepts, so you could probably throw in some stringy buzzwords and resell the theory to Templeton, if you would like to get funding from them (I won't, so here you go).
Maybe, if you choose the buzzwords carefully enough, you could even post it in hep on the xArchive without too many people dying from a laugh attack.
Have fun,
Tim

12. **chris**
June 23, 2009

Tim, 'multiverse' is a very respected concept for decades – among science fiction and fantasy writers.

look. at e.g:
http://en.wikipedia.org/wiki/Eternal_Champion
http://en.wikipedia.org/wiki/Plane_(Magic:_The_Gathering)#Dominia
http://en.wikipedia.org/wiki/Chronicles_of_Amber#The_Amber_Multiverse
http://en.wikipedia.org/wiki/The_Chronicles_of_Narnia#The_Narnian_universe

i am always surprised about the nonchalancy with which respected scientists have recently started pushing this 'scientific' term that for decades was so firmly rooted in the area between fiction and obscurity. they could have at least cooked up another word to avoid the obvious connotations.

13. **Tim vB**
June 23, 2009

Hi Chris,
the core of the problem seems to be that there can't be any interaction between different universes in the multiverse as Sean Carroll repeatedly pointed out, so his theory does not set the stage for an interesting scifi or fantasy story, as the central part of that would be some kind of interaction between the different worlds.
Actually I remember some scifi stories where humans encounter aliens that experience a "reversed arrow of time", so one should probably check that no copyright is violated if one alters the whole story in this direction.
Nonetheless I think there have been quite a few interesting interactions between science and science fiction, e.g. the novels of Jules Verne may well have inspired scientists and engineers alike, but he openly admitted that he was writing novels, not research papers 🙂
Kind regards,
Tim

14. **Yatima**
June 23, 2009

"Now imagine that your brain could process information twice as fast as it does now, that would mean you would have twice as many thoughts per second than you have now."

Cool, I will be able to watch twice as many Battlestar Galactica DVDs in the same amount of time!

Seriously though, even if your "mind" is sped up by a factor x – and it makes sense to assume that the perceived world would consequently speed up by 1/x – you are limited at least by the speed of light and the uncertainty relationships. As any fule know, except those misguided souls looking for ways to circumvent the "Turing Barrier" or other such B.S., there should be a maximum number of bits processable in this universe:

http://www.scottaaronson.com/democritus/lec20.html

15. **Matt Leifer**
June 23, 2009

I think it would be fun if someone were to submit an article about why multiverse theories cannot be confirmed by physics to the FQXi essay competition. I am fairly sure that it would be taken seriously because FQXi does have a large degree of independence from Templeton (disclosure – I have accepted money from FQXi in the past and am a member). There are a number of anti-multiverse people involved with FQXi, so it is not completely crazy to think that such an essay might win something.

Peter,

"If people want to discuss the "philosophy of cosmology", that's fine, but they should do it as philosphers, according to the intellectual standards of philosophy."

I agree with this 100%. What I am saying is that people who are rigorously trained in philosophy really SHOULD study these issues in order to find out whether the pseudo-philosophy promoted by some contemporary physicists really stands up to scrutiny, or at least to give them a hard time about it.

Part of the problem with this is that the most common route into philosophy of physics involves doing a physics undergraduate degree followed by a philosophy masters and Ph.D. This is why you will find vastly more philosophers studying the foundations of nonrelativistic quantum theory than any truly "modern" physics, e.g. quantum field theory, general relativity, cosmology, strings, etc. That's not to say that there are no philosophers studying these topics, just that there are fewer of them than there ought to be. They also tend to be people with two Ph.Ds.

Personally, as someone who is sometimes active in the foundations of quantum theory community, I have to say that the input of philosophers is tremendously useful. They play a key role in moderating the fantastical claims that physicists sometimes make and they are good checking whether arguments hold water. I'd cite Tim Maudlin's work on Bell's theorem and the Oxford group's work on probability in the Everett interpretation as prime examples of good philosophical work (even if I don't agree with all of their conclusions). It would be very useful to have this sort of quality of philosophical work in cosmology and other areas of "modern" physics.

Therefore, I do partly agree with Templeton that there needs to be a special grad program for people who want to do philosophy of cosmology, which teaches them the conceptual underpinnings of modern physics in a rigorous way whilst at the same time covering the standard philosophy material. However the program should definitely not be one-sided or designed to produce multiverse yes-men.

David,

"Putting all flippancy aside (a bit difficult in this area!), are there really any "intellectual standards of philosophy" generally accepted among academic philosophers?"

Yes. Pomo types tend to steer clear of topics like philosophy of physics. Although it is true that these people often misappropriate terminology from modern physics without understanding it, they are usually not explicitly analyzing physics when they do so. In fact, since science does not occupy a central position in their worldview, they are much less likely to study it seriously than people trained in analytic philosophy. Therefore, the vast vast majority of philosophers of physics are in the western analytic tradition and their style of argument meshes quite well with that of theoretical physics.

As for standards, perhaps you should read an issue of Stud. Hist. Phil. Mod. Phys. and see what you think. Sure, you'll probably find a few meaningless papers, but not more than in an average issue of Phys. Rev. Lett. and there are bound to be a few gems in there too. Another method of checking would be to attend a conference on the foundations of quantum theory and make a note of the number of crackpot talks given by physicists vs. the number given by philosophers.

16. **Tim**
June 23, 2009

Hello Yatima,
well, unfortunatly BSG ended with season 4, so there is not so much to gain here…
the interesting point here is, that Tiplers theories allow critique based on physical arguments,
some of these he even addresses in his books (including yours on an upper bound of information
contained in a finite volume of spacetime, see Bekenstein, resp. an upper bound on bits that could be processed in a collapsing
universe). I would not know how to formulate citique in any comparable way on the multiverse theories.
However, the supposed infinity of information processed means that everything that
is physically possible will be emulated by the omega point, according to Tipler,
so that's a little analogy to the multiverse ideas.
Regards,
Tim

17. **Syksy Rasanen**
June 23, 2009

Peter,

George Ellis is not a proponent of the multiverse, quite the contrary. See for example

http://arxiv.org/abs/astro-ph/0602280

and

http://arxiv.org/abs/0811.3529

18. **Bee**
June 23, 2009

http://www.dilbert.com/strips/comic/2009-06-23/

🙂

19. **Peter Woit**
June 23, 2009

Syksy,

I didn't mean to imply that Ellis was a multiverse proponent. Thanks for clarifying that.

20. **Mitch Miller**
June 23, 2009

Do multiverse supporters think it is a coincidence that the multiverse was put forth as a serious idea at a time when experiments can't help narrow down the phase space? It seems like it was obvious from the start that the standard model would only be 1 particular solution out of a large family of possible solutions but I don't think anybody started talking about the mulitverse at that point since people were content to do experiments and fix all free parameters of the standard model.

If the multiverse is some deep insight about nature, it is quite fourtunate that we can't do the relevant experiments!! If we could, we would probably be content to describe our string theory vacuum and just throw away all the other solutions.

21. **Peter Woit**
June 23, 2009

Matt,

I do think philosophers could make a major contribution here, by critically examining the issues surrounding multiverse research. Unfortunately I don't see much of that happening, and to the extent it does, multiverse proponents like Susskind take the attitude that scientists should ignore philosophy of science when it challenges what they are doing.

In any case, this doesn't appear to be what interests Templeton. Their motivation seems to be to increase multiverse "research" and blur the issue of what is science and what isn't.

I hope someone does submit the kind of article you suggest to the FQXI competition, and I'm sure they would take it seriously. There is something very weird though about having to explain to scientists the most basic fact about what science is. The reaction I've seen so far from multiverse proponents makes me rather pessimistic that rational argument has anything to do with the issue at this point. Some people just inherently like doing speculative pseudo-science, and some string theorists are going to grasp at any straw that allows them to avoid admitting failure.

22. **Lee Smolin**
June 23, 2009

I have no objection to Oxford or anyone instituting a program in philosophy of cosmology, but what the documents you quote seem to miss is that throughout the history of philosophy a number of the most influential philosophers have been driven by cosmological concerns. So it would not be good to isolate "philosophy of cosmology" from philosophy itself.

An example of a cosmological concern is how a law of nature is to be formulated, understood, or tested if it is to be claimed to apply to the whole universe, and not just a portion of it. Certainly Leibniz in his Monadology was thinking about these issues, and his principles of sufficient reason and the identity of the indiscernibles only make sense in a cosmological context. What was at stake in the relational/absolute debate between Leibniz and Newton's followers were different approaches to cosmological issues. Mach's principle is cosmological as is Einstein's understanding of general relativity he formulated in response to it. And C S Pierce's arguments for evolutionary explanations for laws were in response to cosmological concerns.

Coming closer to our time, Everett, deWitt and Wheeler's claims for the MWI were explicitly cosmological, and much of the literature by philosophers on the MWI by Sanders and colleagues reflects them. And the debates about the nature of time, among philosophers as well as physicists are also cosmological. Roberto Unger, with whom I have been working recently, is an example of a philosopher with cosmological concerns. And a number of philosophers have written on the anthropic principle, multiverse issues, and the subtleties of reasoning about probabilities and possibilities, much of it strongly critical of naïve ideas of physicists.
To my mind the debates on these issues among physicists could only be improved by the inclusion of good philosophers who have critical and thought out views on these issues. I personally have learned an enormous amount from listening to philosophers who have been kind enough to critique things I've written. At the first two meetings that the Templeton Foundation sponsored on the AP, at Cambridge and Stanford, philosophers were conspicuously missing, despite proposals by some of us that they be included. The reason given was that philosophers would not be able to follow the technical issues, but my sense is the opposite, contemporary philosophers of physics are mostly well educated in physics. The effect was rather to protect some physicists from the blistering attacks a good philosopher might give to their proposals, unaware as they seem to be how naïve they sound to someone educated in the history of philosophy and science. My sense is that the best thing the Templeton Foundation could do now is support venues where the physicists who have been proposing multiverse and anthropic ideas have to listen to criticisms of these views by good philosophers.
More generally, while I would never say that every physicist needs an education in philosophy, I would certainly recommend that any physicist who wants to publically speculate on issues that have a long history in philosophy, and who wants to actually contribute to progress, would be well advised to know that history before wasting everyone's time with naïve views that don't stand up to basic criticisms.

23. **Tomatonator**
June 23, 2009

It should be obvious why the Templeton Foundation supports the multiuniverse concept: Heaven and Hell and all those other afterlife places of the JudeoChristian tradition can now be considered real places that just happen to be alternate universes.

Like the creationists, slap a little science on your ideology to make it look legit – ironically to the very groups that otherwise disdain and do not understand science.

The worst part is that, just as UFO fringe elements tarnish the serious study and search for alien life, these guys will do the same to multiverse studies.

Lee, I do like seeing your acceptance and appreciation of branches of learning outside physics. We need to get back to having well-rounded teachers and students who know more than just one field.

24. **Brian**
June 23, 2009

Tim Solton,

The term "atom" originally referred to an indivisible piece of matter. It's just that we ultimately learned that the things we'd gotten used to calling called atoms actually weren't atomic, and it wasn't worth changing the terminology. By extension, it's not unreasonable to refer to an element of a multiverse as a "universe." If multiverses are something you want to talk about, of course.

Brian

25. **Dave Miller**
June 23, 2009

Matt wrote to me:
>As for standards, perhaps you should read an issue of Stud. Hist. Phil. Mod. Phys. and see what you think. Sure, you'll probably find a few meaningless papers, but not more than in an average issue of Phys. Rev. Lett….

Hmmmm. Even when I was a doctoral student thirty years ago, there was an awful lot of nonsense making its way into PRL! Damning with faint praise?

Seriously, I tried to make clear that I was not condemning all philosophers: I explicitly mentioned McGinn as a good guy, and, yes, I have seen a number of intelligent philosophers writing on QM — as you imply, often writing more intelligently than many physicists write.

I was just addressing Peter's point about "intellectual standards of philosophy." Even if we restrict ourselves to philosophy of science alone, we have to face up to the fact that Feyerabend was one of the best known philosophers of science in the previous generation.

And even Karl Popper, who did make some good points, was ludicrously uninformed when it came to his writings on quantum mechanics.

Yes, there are some bright and thoughtful — I'd go so far as to say some who are brilliant – philosophers writing in the philosophy of science today. On the other hand, I am still skeptical that there are decent, generally-accepted, "intellectual standards of philosophy" that prevail within the profession of philosophy as a whole. A profession that honors Feyerabend, Popper, Foucault et al., even though it also includes truly informed folks such as McGinn, Jeff Barrett, Mike Redhead, John Searle, etc., has a problem with standards.

I'm making a narrow sociological point about a lack of serious standards that prevail throughout the profession of philosophy taken as a whole.

Now, if you want to argue that physicists also now have a similar lack of standards (maybe worse when they write on the philosophy of quantum mechanics!), well….

Dave

26. **milkshake**
June 23, 2009

the style is not postmodern – is a rather traditional New Age doppel (lightly hopped with Aquinas to compliment the sponsors)

27. **cormac**
June 24, 2009

I had a look at Saunder' paper, thanks for the link. Actually, reading down through the syllabus, the breakdown of topics looks very interesting – looks like a decent introduction to philosophy for any physicist. I didn't see any evidence of foregone conclusions, wouldn't mind doing this course myself!

28. **Steve Esser**
June 24, 2009

Lee Smolin spoke to something I noticed: Templeton funds some science and alot of theology, but skips over non-theistic philosophy for some reason. I hope they do reconsider this.

29. **concerned cynic**
June 30, 2009

Lee Smolin wrote:

"At the first two meetings that the Templeton Foundation sponsored on the AP, at Cambridge and Stanford, philosophers were conspicuously missing, despite proposals by some of us that they be included. The reason given was that philosophers would not be able to follow the technical issues, but my sense is the opposite, contemporary philosophers of physics are mostly well educated in physics. The effect was rather to protect some physicists from the blistering attacks a good philosopher might give to their proposals, unaware as they seem to be how naïve they sound to someone educated in the history of philosophy and science."

I cannot believe that the absence of philosophers was at the insistence of Templeton people. The physicists must have been the ones to insist on this. Lee, if your cynical conjecture as to the motivations of the physicists who organised those conferences is correct, that is breathtakingly damning and further evidence that you and Woit have put your fingers on a real and grave problem in the temple of theoretical physics.

Disclosure: I admit to being a stuffy Popperian in my views on how to do science. I also believe that doing physics is like ballroom dancing, you need a partner. And theorists have to dance with experimentalists; nothing happens worth noticing until the two pair off.

The activities of the Templeton Foundation do not bother me as much as they do Peter. As a private entity, they should be free to pursue their ends as they see fit. Acknowledging Templeton Foundation support should not be dismissed as the Mark of Cain. Here's hoping that other foundations emerge that support other viewpoints on theoretical physics. Let us not forget the good people who created Lee's employer, the Perimeter Institute.

If Oxford were to start a program in the philosophy of cosmology, that would be fine by me, as long as Joe Silk were invited to be part of it. And I think that Oxford should hire students, and students of students, of Sciama's to help staff it.

The apparent "fine-tuning" of the facts and laws of physics as summarized, e.g., in Paul Davies's recent Goldilocks book, deserves to be included in Smolin's list of fundamental open questions in physics. In particular, hypothesizing a vast multiverse is not a satisfactory explanation. I do not think we are anywhere near a satisfactory naturalist explanation of the universe and the laws of physics. We are like Dorothy in the Emerald City, not knowing that there is a man behind the curtain.

The Man behind the Curtain could be taken to be a God-like entity, but I do not take that as evidence confirming the monotheism we have inherited from the Middle East. If the Universe was willed into existence by a Creator, who fine tuned the laws of physics to make intelligent life possible, we cannot conclude that the Creator values each of us as individuals. It would not justify religious ceremonies or customs, would not make an eternal soul and an afterlife more or less likely. It does not help us know more about how we should conduct our lives. It does not even assure us that God is unique. God may have left clues in the laws of nature, but that tells us nothing about the veracity of the scriptures of historical religions. It is evident that actual religions contain a huge element of human imagination.

The universe began, a finite proper time ago, in a state of extreme simplicity / symmetry. As it expanded, a fantastic cascade of broken symmetries has resulted in the very complex reality we observe. The universe has gradually unpacked itself. In my view, the creation myth with which the Bible begins is consistent with these facts — this may be nothing more than a coincidence. Regardless, Templeton funded scholars should feel free to explore this further.

30. **Chris W.**
June 30, 2009

From Lee Smolin:

> More generally, while I would never say that every physicist needs an education in philosophy, I would certainly recommend that any physicist who wants to publically speculate on issues that have a long history in philosophy, and who wants to actually contribute to progress, would be well advised to know that history before wasting everyone's time with naïve views that don't stand up to basic criticisms.

Man, what a relief it is to hear a physicist say that. Nothing grates on me more than people who espouse philosophical positions, especially in areas long considered by explicitly philosophical thinkers, while dismissing philosophy as irrelevant or fruitless.

31. **Chris W.**
July 5, 2009

See Steve Hsu's post "Feyerabend on the giants".

Gina Says

June 23, 2009
Categories: Uncategorized

At the height of the string wars a couple years ago, one of the participants was a mysterious anonymous commenter going under the name "Gina". Earlier this year Gil Kalai wrote to me to reveal that he was the person behind "Gina", and that he had put together a book based on these blog discussions, to be entitled "'Gina Says,' Adventures in the Blogosphere String War". He has now put the first part of manuscript up on his blog, the posting is here.

Back in January he sent me a copy of what he had written, I haven't checked to see what changes might be in the version available now. Instead of writing something about this here now, I think I'll just include part of my e-mail to him back in January, which gave my reaction to the project then:

> Hi Gil/Gina…
>
> Thanks for sending me the draft of the book. I read through it quickly, amused to relive again some battles of the string wars. When people ask me if I'll write another book, often I've answered that I was considering just cutting and pasting together a lot of things from my blog, other blogs, and my e-mail, all of which told a rather amazing and often amusing tale. Funny to see that you've done something a bit like this yourself. During this period I also remember often telling people that I felt like I was living in a comic novel.
>
> Actually, I've no intention of publishing anything about the "String Wars", although happy if other people want to. I'm rather glad that they have died down, and I'm trying to devote my time instead to a research project I'm quite excited about (the BRST stuff I've started writing about on the blog).
>
> Some comments about issues you raise, and some added context for some of these stories:
>
> In my book, I tried to avoid saying much at all personally critical about string theorists and their behavior, the sort of thing that Lee Smolin did more of. I generally agree with what Lee wrote, but, in the past my personal contacts with string theorists were mostly with quite reasonable people that I didn't think it appropriate to criticize in this way. After my experiences in the "string wars" though, I ended up feeling that Lee actually didn't go far enough; that, individually and as a community, there are very real behavioral and ethical problems in how all too many string theorists do business. My impression is that the "string wars" brought a lot of this out into the open, and have damaged the perception of string theory among physicists and the wider community, more so than anything Lee wrote. Like Lee, what I was hoping our books would lead to would be a serious discussion of the issues involved. There was some of this, but all too much name-calling and bad behavior.
>
> Some context about Clifford Johnson: independently of each other, both Lee and I wrote to him when our books were in draft form, asking if he would be willing to take a look at them, and let us know if there was anything we had wrong. He just ignored my e-mail, and I gather Lee got a similar response. He appears to be a rather nice guy, and I found this response kind of odd, it was one reason for my mistaken guess that he was the Cambridge referee. I still find his behavior exceedingly strange: how can you write long blog entries denouncing books you refuse to read? He seems to have an ability to refuse to acknowledge the existence of inconvenient realities that goes beyond anything I've seen before.
>
> …
>
> In your fantasy of the future, you mention my book being translated into Czech. Funny, a publishing company there did buy the rights a year or so ago, and I think they will be bringing it out. Sometimes reality and fantasy are indistinguishable in this story…

Update: There's a posting about this over at Physics World.

Comments

1. **Tim vB**
   June 23, 2009

   Hello Peter,
   now that was hilarious, but I wonder why the "string wars" have cooled down? Is everybody exhausted from going in circles?
   Regards,
   Tim

2. **karl**
   June 23, 2009

   Hi Peter,

   Why don't you declare victory? Time seems ripe. Note that the annual string conference is going on this week in Rome, but this year it has basically zero impact on the blogosphere or elsewhere in the internet.
   Lubos says almost nothing and even you ignore the event totally. Nobody seems to be live-blogging, no videos or audios of the talks, not even pdf's. Looks as if strings do not rise emotions anymore. Hmm, bad prospects for a new book on the "string wars" ?

3. **Peter Woit**
   June 23, 2009

   Tim vB and karl,

   I think exhaustion has a lot to do with it. At this point the arguments on both sides have been made repeatedly and extensively, and no one seems to now have any new arguments to make. I'm not about to write another book on the subject.

   The problems of string theory have gotten a fair amount of attention, and there is a much more skeptical attitude about the subject now, among all parties except for string theory's hard-core fans. Some string theorists seem to be hoping that since attention has died down, maybe they can go back to issuing some hype for the subject. So, the blog still has a role to play…

   Unfortunately I don't think my point of view on this has been victorious, since the problems of string theory have caused a backlash among physics departments against mathematical and formal studies, not only of string theory, but of QFT also, which is a shame.

It is odd that there’s nothing on the internet yet about Strings 2009. No video, no slides, no blogging. Some years ago Jacques would live-blog the conference, this year his only blog entry so far has been about finding someone to go to dinner with. Presumably at some point he’ll write something about the talks.

4. **Aaron Bergman**
June 23, 2009

This seems to be a copyright violation waiting to happen.

5. **AcademicLurker**
June 23, 2009

“During this period I also remember often telling people that I felt like I was living in a comic novel.”

Peter,

On another blog we were discussing comic novels set in academia and noticed that the famous academic satires like David Lodge’s Small World (and many others) focus almost exclusively on the humanities, with science largely ignored.

Perhaps you could write a novel to complement Not Even Wrong. There’s definitely an unfilled niche:)

6. **Peter Woit**
June 23, 2009

AcademicLurker,

There’s definitely an excellent comic novel in this story, and the idea of writing such a thing in 21st century epistolary form (i.e. as blog comments and e-mail) did appeal to me. One reason I didn’t pursue that idea was that I’m pretty sure that I don’t have the skill to put together a really good piece of fiction in any form, the other was that reality as usual seems to me more strange and fascinating than fiction. What novelist could possibly come up with Lubos Motl?

Still, maybe I should have done my own version of Gil’s cut and paste job, there’s some wonderful material. In any case, it’s all still out there for anyone who wants to try….

7. **Thomas R Love**
June 23, 2009

Thanks for another interesting link, Peter. I read the book and had quite a few laughs, but then I went to Gil’s web site and read his list of publications. Nothing on string theory, so I have to wonder what his motivation is. Gil posted as Gina here. Did he ever write anything here under his own name?

8. **Peter Woit**
June 23, 2009

Thomas,

Gil did post some comments under his own name “Gil Kalai”. He’s not a string theorist, and I think makes pretty clear his motivation, which was basically an interest in trying to understand the lively controversy.

The “Gina” comments were always frustrating to deal with. For one thing, the anonymity is always frustrating, partly because not knowing what someone’s background is, it’s hard to know at what level to try and interact with them. How much do you need to explain? Not knowing who you are dealing with makes this a more difficult and time-consuming process.

As recounted in the book, I ultimately decided to put an end to Gina’s comments. They sometimes raised interesting points and were worthwhile, but towards the end more often were attempts to enter into technical debates, without any expertise in the issues. This led to lots of confusing misconceptions being thrown around, encouraging other not very expert commenters to add theirs, and generally led to me being put in the position of allowing a lot of uncorrected misinformation here, or having to put in a lot of time to straighten it out. I was trying to encourage what at that time was sometimes a serious discussion of the issues between people fairly expert in them, and felt that Gina’s comments were interfering with that.

9. **tomate**
June 23, 2009

It is quite odd to hear from overseas the string war is cooling down, and that time is ripe to declare some form of victory. Here in Italy, at the periphery of the empire (not only from a scientific point of view) such a discussion is yet to come, and problably will never come. It is amazing to see how much scientific discussion goes on the internet in the US and in other more civilized european countries; we are almost illiterate in comparison, so no wonder that Strings 2009 is held in Italy, and that it has no resonance on the web.

10. **Peter Woit**
June 23, 2009

tomate,

I think there’s nearly unanimous agreement that the string war has cooled down, unfortunately not so unanimous about which side won…

It does seem pretty 20th century that nearly halfway through Strings 2009 not a single blog posting has yet come from any of the 450 people attending (or are they all on Twitter now…?). But I don’t think you can blame that on the Italians. There are physicists there from the very center of the Empire (Austin, Texas) who for some reason don’t seem to be blogging.

11. **David**
June 24, 2009

Ever occur to anyone that measuring blog interest is not actually a measure of scientific impact or in fact anything at all.
Hhhm, perhaps we should use other criteria.

12. **themanwithaplan**
June 24, 2009

Having read some of it, I have to say that it leaves one with a distaste for what Kallai has done. By selecting which blog entries to show and which to omit, he could have tried to give a narrative to his writeup. But the way he has written it now, it is very sleazy: after showing snippets of blog responses, "Gina" then tells us what she thinks about it…. sometimes sneering at the blog posts, sometimes indicating how idiotic the writer of the post must be.

The role of kallai should have been the one of a silent commentator, above all the comical aspects of this whole story, whose simply organizing and showing us what happened. But gil is not satisified with that, he lacks any sense of decent restraint. He simply cannot resist jumping into the story, and rolling in the mud himself.

Peter, you should have not given space to such idiocracy by gracing it with a blog post. From what you wrote above on your idea of such a project, I got a different impression, one closer to what I just said. Gil is just using this whole story to get *his* last word on everything in there.

13. **Peter Woit**
June 24, 2009

themanwithaplan,

I don't know if my version of this would have been more objective than Kalai's, but I do think it would have been a lot funnier….

14. **Tim vB**
June 25, 2009

As entertainig as all of this is: please keep in mind that there are people out there, and will be, for years to come, who are primarily interested in the arguments that were exchanged.
If anyone thinks right now "well, just go to your local string guru and then to your local QFT of whatever non-string-guru": Extrapolating and abstracting my experience boils down to this: Go to professor X at the physics faculty of university y in city z and ask about string theory. You will get a sermon living up to the late Ajatolla Chomeini, be it pro or contra string theory, but a sermon it will be.
How is one supposed to make any sense of this?
To give a complete account of the string wars, you would probably have to team up a string theorist, a non-string-physicist, a philosopher fond of epistemology, a sociologist and some talented writers from Hollywood.

15. **Hendrik**
June 26, 2009

I ran into "Gina" in a hot debate on Cosmic Variance some years ago. I found that "she" dissipated the debate by asking for instruction at an elementary level. She was often a distraction from the main scientific issues under discussion. Later on "she" actually emailed me when the debate ran out of puff, asking me to post more basic explanations. Overall, I think she added substantially to the noise level of the debate.

16. **D R Lunsford**
June 26, 2009

Peter – you say the arguments have exhausted you, and I totally understand – but the social behavior of string theorists is the real problem and not the details of the (non) theory, which any reasonable person can see to be bad science in general and awful physics – it's the terrible effect on careers and science itself generated by this entrenched majority that needs to be exposed, attacked, and put to an end. The real problem to explore, is how this all came about and how it persists.

-drl

17. **Chris Oakley**
June 28, 2009

Hendrik,

Ah … who could forget that one? 530 comments over 2 months and practically the entire Fundamental Physics blogosphere, and then some, chiming in.

I did not object to "Gina"'s contributions, though. But as a general principle I do not think that people should be allowed to post anonymously, especially when they are being contentious.

I like the final comment (#530) – more than 6 months after the original posting, which was my old pal William Shaw realising that I had taken his name in vain in mentioning his work on 4D Superstrings.

18. **Gil Kalai**
June 29, 2009

This is a nice reference to my book, Peter. Thanks. The issue of anonymous posting (and allowing various cyber or collective or even computerized entities) is interesting. I remember that a year ago or so we briefly discussed it here.

The marathon 530 comments long thread over the cosmic variance that Hendrik and Chris referred to was indeed unusual and interesting. Close to the end (from 446 or 445 to around 490) There was an interesting long discussion on renormalization which featured different attitudes and different personalities that perhaps could only come together on blogs. There were also several interesting subthreads on other matters.

19. **Peter Woit**
June 29, 2009

Hi Gil,

I did take a look recently at that marathon comment thread. Probably it is the best single location for people to get an idea of the discussion going on during that period. Unfortunately the rest of it is spread around quite a few other different blogs and postings. You gather together some of it, a useful project I might get to someday would be to put together a page of links.

20. **Tim vB**
June 30, 2009

Hello Gil, Peter,
some random thoughts about this monster thread:
– This is the first time I think I got a good idea of the kind of discussions at that time, I did not find anything comparable on my own.
– This thread should probably have been split into one for the experts, and one for posting elementary questions at textbook level (Gil, the questions about renormalization

seem to be quite off topic to me).
– It would seem that everybody has to learn how to communicate using a blog. Personal insults, sublte irony etc. will easily be misunderstood by at least some of the participants, spawning responses that could kill the whole thread.
The posts that basically repeat former postings strike me as particularly odd, this may make sense in a personal conversation, if you get the impression that you have not been heard, but in a blog everyone can just scroll to the top of the page…
– Personally I would be most interested in an excerpt of the arguments of both sides, since I fear I have neither the time nor the energy to read all the postings (let alone understand everything).

21. **Aristarchus**
July 6, 2009

Peter, would you like to comment on the following quote from
the post here:

http://motls.blogspot.com/2009/07/against-philosophy-2009.html

"Unfortunately, the world's crackpots – including some of their leaders such as Peter Woit and Lee Smolin – can never understand that their philosophical preconceptions trying to dictate a priori what science should look like may be wrong and are wrong.

"Physics cannot respect Smolin's opinions that it shouldn't be based on deep and accurate mathematics; it ignores his desire for the laws to respect his own version of the background independence; Nature cannot respect Woit's or anti-quantum zealots' opinion that physics must avoid all concepts that are too hard for their small brains to visualize them or to improve their everyday life; science disagrees with Woit's dumb illusions that correct physical theories are obliged to guarantee that physicists must always have sufficient resources to test these theories (or even cheaply or soon). Nature has no such obligations. Even if these people managed to write one correct sentence – which is as likely as in the case of the proverbial monkey writers – the method how they arrived to such a sentence is scientifically flawed."

22. **Tim vB**
July 7, 2009

Hello Aristarchus,
Hopefully Peter will have time to answer you himself soon, but: Is it really necessary
to comment on this quote? I always liked the attitude "let everybody publish and
let everybode else make up her/his mind about it".
During the last few years I got the impression that this attitude is a luxury
that people working in hep cannot afford, at least not all the time.
In the case of Lubos Motl I think we can.
The only remarkable point in this quote seems to be "science disagrees with Woit's dumb illusions that correct physical theories are obliged to guarantee that physicists must always have sufficient resources to test these theories".
This seems to be a relapse to pre-Aristotelian concepts about what science is and what it is not (no need to discuss that it is also quite impolite, right?).

23. **Aristarchus**
July 7, 2009

Tim, I was not trying to be rude. I want to hear the other side of the story and I wanted to quote what was said EXACTLY without editing so no one can accuse bias.

24. **Peter Woit**
July 7, 2009

Still traveling, back at work on Thursday. Tonight I've got decent internet access and some free time for the first time on this trip.

Aristarchus,

Lubos is pretty much nuts on the topic of string theory and this is so obvious that it's usually not worth the time to respond to his endless rants. As for this one:

1. I firmly believe that at a fundamental level physics is based on deep mathematics, not on things that humans are readily able to visualize. The problem with string theory is not that it uses too sophisticated mathematics, it's that it is a wrong idea about unification, and no amount of mathematical technology can fix this.

2. The problem with string theory is not that it can't be tested today, but that it is inherently untestable, no matter how high an energy accelerator we ever figure out how to build. There's nothing wrong with working on ideas you don't know how to experimentally test, but you have to have some plausible explanation about how, if your research program works out, someday your ideas can be tested. In the case of string theory, in 1984 there was such a proposal, but what has been learned about the theory since then has shown that it doesn't work, leading instead to landscape pseudoscience. In short, the problem with the project of getting string theory to the point of being testable is that by any measure of progress, the derivative is the wrong sign. 25 years of research has just made the whole thing more and more implausible the more we learn.

25. **Anonymous**
July 7, 2009

"but that it is inherently untestable,"

Can you explain? What if there is a way?

26. **chris**
July 8, 2009

"but that it is inherently untestable,"

Can you explain? What if there is a way?

well, nobody can show any way up to now. as Peter said, 25 years ago it looked more or less like there would be some predictive power but now this hope is dwindling by the year as the landscape is explored and no preferred point seems to be found.

27. **Aristarchus**
July 8, 2009

Is it true what I once read, that in order to ever have a chance to detect a string, we would need to build a particle accelerator the diameter of the Milky Way galaxy?

When I see how much trouble they are having with the LHC….

28. **Peter Woit**
July 9, 2009

Aristarchus,

A particle accelerator the diameter of the Milky Way could not test string theory. The problem is not one of technology…

Strings 2009 Out of Juice?

June 23, 2009
Categories: Strings 2XXX

I had a suspicion that Strings 2009 wasn't going to be scientifically very active, since not much has been going on in that field recently, but I still found it surprising how little news from the conference was making its way out to the internet. The conference is nearly half over, no evidence of any activity has appeared on the conference web-site, and until now I couldn't find anything at all on the internet discussing what is going on there.

However, something did just turn up. Over at the Bad Astronomy and Universe Today Forum, under the topic heading "Off-Topic Babbling", there have been two communications from Terry Giblin. In the first, written on Sunday, Terry writes that he's on the road to Rome for the conference, and since it looks like he'll be late, he'd like someone from the stage or audience to call him on Skype so he can ask the opening speaker (David Gross) a "strings question on quantum tunneling and singularities."

That doesn't seem to have worked out. In his latest communication, he says that he finally did make it to Rome, where he reports:

> It would appear that I have not missed much over the past two days, yesterday the internet at the conference was only working slowly.
>
> Today they had a power outage, so the conference was cancelled.
>
> I hope I have more success tomorrow or the coming week.
>
> Its amazing to think you can change the outcome of a conference, without being physically present………

Update: A twitter from Marco Baumgartl has made it out to the internet, bringing more confirmation of problems:

> I'd loved to give you live updates from the Strings 2009 conference, but they have severe wifi problems there, can't connect

Update: An anonymous correspondent attending the conference reports that disorganization is a problem, and that there have been no major announcements. David Gross's talk listed 8 fundamental problems for string theory and gave the string community a grade of A-F for progress on each problem (this sounds familiar, I vaguely recall him giving such a report card at some other talk a few years ago, I wonder if the grades have changed…)

The atmosphere was much like in other recent years: some disillusionment in the air, but people continuing to work along similar lines. The talks were described as sketchy and mostly incomprehensible to much of the audience, with many of the younger string theorists rather bitter about the lack of much of an attempt on the part of the speakers to give clear explanations and put their work in any sort of context (the audience of 450 has widely varying backgrounds, this is not a specialist conference).

Update: Well, the conference is over now, but still no slides of talks, or any blogging from anyone there, other than Jacques Distler's attempt to find someone to go to dinner with a week ago. Reports I've gotten from the conference describe Vafa's talk as "Good, in fact too good to be true", and claim that Arkani-Hamed showed up two hours late for his talk, then went over time by 20 minutes.

Update: Still nothing on the conference web-site about the talks, or on blogs. Physics World does have a report from Edwin Cartlidge, who noted that the scientific talks were appropriately held at the University of St. Thomas Aquinas, named after the great Scholastic philosopher. He also reports on the public talks held yesterday. Witten appears to have decided the best thing to do was to not talk about string theory, but instead talk about particle physics, the LHC, dark matter and supersymmetry. He left string theory to Brian Greene, who somehow convinced Cartlidge that what this is all about is "that 10500 is somewhat bigger than 10120, and that's a measure of how much we don't understand dark energy."

> Greene pointed out that string theory requires an extra 6 (or 7) dimensions of space in addition to the three that we are aware of. Helpfully, these dimensions are so small that we can't see them, but unhelpfully there are rather a lot of ways of curling these extra dimensions up – some 10500 different ways as it turns out. And we would have to study all 10500 if we want to find out whether or not string theory describes the real world.
>
> For Greene, all is not lost, however. He pointed out that 10500 is somewhat bigger than 10120, and that's a measure of how much we don't understand dark energy. In a nutshell he argued that if we happen to live in one of the few of the 10500 universes where conditions are just right for us to exist then there's a damn good chance that we could have such an apparently statistically unlikely dark energy. For Greene, this suggests we might be on the right lines with string theory. Others may be less convinced.

Update: The exponent problem has been fixed.

Comments

1. **M**
   June 23, 2009

   Any word on the food? People do not go to conferences for free wi-fi really.

2. **tomate**
   June 24, 2009

   Still sure you can't blame our medieval gap? I think this might be symbolic of the status of research and university here.
   At least the food will be just fine.

3. **Per**
   June 24, 2009

   My girlfriend is Italian she told me before it started – the organization is so gonna break down. Guess that's what happened. It's hard not to like Italians, but their skills in organizing and planning sometimes lacks a bit behind 🙂

4. **Tim vB**
   June 24, 2009

   Hello Peter,
   "I had a suspicion that Strings 2009 wasn't going to be scientifically very active, since not much has been going on in that field recently…"

Asking with honest interest, since I do not follow the field intensely: This statement is based on what observation(s)?
Regards,
Tim
P.S.: A little note on my background to provide some context to my question: I graduated in theoretical physics from the University of Heidelberg and stumbled upon your book and Lee's while trying to decide what to do next, in order to get a second resp. n'th opinion on string theory. Almost everything I know of the subject itself comes from Barton Zwiebach's book – I know it is addressed at toddlers, but one has to start somewhere.

5. **H-I-G-G-S**
June 24, 2009

Good to see that you have such reliable sources. In one his postings Mr. Giblin says "This equation also implies that an electron and a photon cannot, exist together." He seems to be a crank. Wifi problems, yes. Conference cancelled, I doubt it.

6. **Peter Woit**
June 24, 2009

H-I-G-G-S,

We Report. You Decide…

Tim vB,

I've been following the field quite closely for 25 years, and don't think it's a very controversial statement that the last few years haven't seen any major developments. The planned titles of the talks are online, and from that there doesn't appear to be anything dramatically new, although one can't really know without having access to the talks themselves.

7. **Bee**
June 26, 2009

Totally off-topic: On mouseover the "latest comment" feed in the sidebar says "Last comment was 39 years, 6 months ago…"

8. **woit**
June 26, 2009

Hi Bee,

That bug has been around for a while, you've encouraged me to try and fix it. Right now it looks like it is fixed for all but this posting, maybe my adding this comment will do the trick….

9. **Peter Woit**
June 26, 2009

Fix seems to have improved matters, but not completely fixed the problem. I think I'll not spend more time on this now though…

10. **brief aside**
June 27, 2009

Peter,

I have been reading your lecture notes on Hamiltonian Mechanics and Symplectic Geometry, which I found on your site. In particular, 'Quantum Field Theory for Mathematicians: Hamiltonian Mechanics and Symplectic Geometry'

On page 7 I came across the equations z = p + iwq and… z(t) = Cexp(iwt). I would be thrilled if you could please tell me where I can get more material on these equations. Do you have indepth lectures on this? If you have any free material could you please direct me to it. Thanks.

11. **brief aside**
June 27, 2009

I mean those equations in particular. Thanks.

12. **Peter Woit**
June 27, 2009

brief aside,

The equations you mention are just the standard trick of expressing a pair of canonical variables in terms of a complex variable. This is in almost every quantum mechanics book, when you solve the harmonic oscillator by writing annihilation and creation operators (a and a^*) in terms of position and momentum operators. For some long expository related pieces by John Baez, see

http://math.ucr.edu/home/baez/photon/intro.htm

and, at a higher level

http://math.ucr.edu/home/baez/harmonic.html

I hope this helps. I'm sorry, but I don't have the time or energy now to devote this blog to basic expository questions like this. Some day in the distant future I would like to write up some much more detailed lecture notes on QM and representation theory, but that project is a ways off…

13. **Tim vB**
June 28, 2009

Hello Peter,
the article by Edwin Cartlidge seems to be a joke along the line "you all know that no one can possibly make any sense of what these nerds say".
This is unfair to Brian Greene and to string theory (not that I am a fan of one or the other).

Meanwhile Lubos has blogged about the conference, so if there were anything dramatically new we would know by now,
therefore confirming your expectations.
He talks about applications of string theory in many body problems,
with experts telling us that "string theory computes things that we
acutally see in experiments", any opinion on this claim?
Regards,
Tim

P.S.: Almost every journalist misunderstands 10^500 as 10500, disregarding the hat
(humans tend to ignore signs they do not understand 🙂
Maybe we should all switch to "10 to the power of 500", at least when
writing an email to a non-specialist.

14. **Simplicio**
June 28, 2009

17985 is an exact measure of the amount by which I don't understand where 10120 comes from.

15. **Peter Woit**
June 28, 2009

Tim vB,

It's quite true that sometimes in this business you can't tell what's a joke and what isn't. That isn't a good thing….

Applications of AdS/CFT duality to studying strongly coupled models that may have something to do with heavy-ion physics and condensed-matter physics has become a large and active subject. I confess to not having a lot of interest in it, since it says nothing about particle theory and nothing mathematically interesting. From what I have seen, some of this may definitely be useful, but there also appears to be a lot of hype going on. I was interested to see that Lubos describes some of this negatively as "an industrial activity", which "has become a routine".

Lubos's blogging about the conference talks seems to be based purely on the titles of the talks. When and if the talks are on-line, perhaps he'll have some more substantive comments.

16. **observer**
June 28, 2009

I am currently half-watching a program on the Science channel about Hawking's endorsment of string theory and how it's the best candidate for TOF. The program calls the development of string theory a "break through" discovery that unites gravity and QCD. There is no evidence presented but I am not sure the listening public cares. Will it matter if Strings 2009 collapses but the spoon-fed public absorbs it as the current theory of everything? The violin and banjo sounds demonstrating the theory are a nice touch (even if its truly a horrible analog).

17. **Michael Thaddeus**
June 28, 2009

For what it's worth, Lubos has some discussion of the Strings 2009 talks on his blog… but I preferred his pronouncement about Michael Jackson: "His pale skin could have been caused by a skin disorder."

18. **Tom O'Bulls**
June 29, 2009

Lubos M has a simple rule in deciding who he likes, viz. he just has to ask how similar they are to himself. Hence his worship of intensely insecure and obnoxious historical figures like Pauli and Feynman. The fact that he liked Michael Jackson is therefore deeply revealing.

19. **nbutsomebody**
June 29, 2009

String 2009 is really out of juice. At least string theorists are not at all talking about it. Not only in blogosphere but also in real life. May be it is time people stop a yearly Strings meeting and go for a longer cycle like 2-4 years.

20. **Anonymous**
July 1, 2009

It was interesting to hear Horava's talk on his new approach to quantum gravity. The idea is unrelated, and most likely incompatible with string theory. It left many of us (including apparently Ed Witten) unconvinced, but I think it is nice that string theorists are willing to devote time to considering such radical departures.

Peter, I have not seen you mention this development, which strikes me as an interesting new turn in the string theory debate. Perhaps it is does not interest to you because Horava's approach does not have much to say about electroweak symmetry breaking. Do you have any thoughts on this?

21. **Peter Woit**
July 1, 2009

Anonymous,

I have the same problem with the Horava proposal as with all quantum gravity proposals that say nothing at all about unification and particle physics: I don't see how you can ever know if they are right. Like lots of ideas though, it might be worth studying since you might learn something useful. This particular idea doesn't involve anything particularly mathematically interesting, so it's not something I'd personally want to pursue.

The sociological story is a bit interesting. Even though this has nothing to do with string theory, and would kill the main argument for string theory, it still is getting a respectful hearing from string theorists, who write papers on it, and invite the speaker to Strings XXXX. If Horava was not a fairly prominent string theorist, I doubt this idea would be getting as much attention. And Lubos would not be polite….

22. **Chris W.**
July 1, 2009

As a PS on Peter's comment, a number of prominent condensed matter theorists (eg, Volovik, Laughlin) have developed broadly similar ideas over the past decade. Volovik has recently commented on Horava's proposal.

23. **Chris W.**
July 1, 2009

Also see this review by Volovik:

Emergent physics: Fermi point scenario
(arxiv:0801.0724)

24. **Anonymous**
July 1, 2009

I do not think there is any convincing evidence that particle physics at accessible energy scales has much to do with quantum gravity. At one time some people hoped that string theory could have some explanatory power in this regard, but not so much anymore. So I do not think it is quite fair to judge a theory of quantum gravity according to its particle physics implications, anymore than it is fair to judge QED based on it's implications for geophysics.

On the mathematical side, Horava mentioned a connection between the RG flow in his theory and the famous Ricci flow. I am not knowledgeable in these matters and am not sure how seriously to take his analogy. Do you have any thoughts on this?

25. **Peter Woit**
July 1, 2009

Anonymous,

The problem with your geophysics/QED analogy is that geophysicists deal with experimentally accessible quantities. If your quantum gravity is not going to say anything about physics except at completely inaccessible scales, there is a very real issue of how to pursue the subject as a science.

I don't know about Horava's case. In the case of 2d non-linear sigma models, the relation between the Ricci flow and the renormalization group flow is well-known.

26. **Chris W.**
July 1, 2009

Speaking of problematic analogies, Leonard Susskind has a new article in *Physics World*:

Darwin's legacy

I think you can guess where it ends up.

27. **Tim vB**
July 2, 2009

Hm, I think I do not want to diskuss if if the idea of the multiverse etc. meets any established standards of science,
but I think it clearly does not meet established standards of science fiction (it is just not interesting enough).
Maybe we should sharpen the title of the blog to something like "not even entertaining" or "not even funny".

28. **nbutsomebody**
July 6, 2009

Slides of the talks are available now. Talks look pretty boring and most speakers are randomly selected without any real reason.

29. **jmars**
July 8, 2009

nbutsomebody,

who precisely do you have in mind?

Peter,

can you give an assessment of Vafa's proposal? It really seems to give a lot of very detailed predictions; since you are the leading critic of string theory unification, this F-theory business (which I assume you are following with great attention) is probably worth a whole blogpost. Don't take it as a criticism, but the actual research of the leading string theorists about the particle physics phenomenology should probably be much more relevant for the readers of your blog than a press-release from a university in a remote Dutch province… 🙂 On the other hand, it'd be good to have an unbiased and detailed account of the work of Heckman and Vafa from an outsider's point of view.

30. **Peter Woit**
July 8, 2009

jmars,

I wrote about this here

http://www.math.columbia.edu/~woit/wordpress/?p=697

when it was first getting started, more than a year ago, and recently quoted opinions I heard reported second-hand from Strings 2009: "Too good to be true".

You can certainly pick some string-inspired model, make lots of choices so that you get something that doesn't obviously disagree with experiment, and then try to show that with your choices things are constrained enough to make predictions and test the idea. That's what the people working on this are doing. What I've seen of it looks to me highly implausible, but they see it differently and are making a very hard sell of the idea. My impression is that many people share my skepticism (one other blog that has covered this is Resonaances, with a similar point of view).

In any case, this is a story that can't go on forever. The true believers here claim they can make predictions that will be tested reasonably soon. Let them make their predictions and see what the LHC says. But note that this is in no sense a “test of string theory”: this is just one of many possible string theory scenarios, if it fails it won’t show string theory to be wrong.

Some very recent work (arXiv:0906.4672) claims to show that the versions of this idea studied so far don’t actually work, that you need to make even more non-minimal choices to get things to work. It may be that the scenario being studied intensively now will turn out to be inconsistent, and people will lose interest after some more iterations of adding complexity to the story to evade contradiction.

31. **Terry Giblin**
    August 9, 2009

    who was the opening speaker at Strings 2009?

    Kind Regards

    Terry Giblin

Conferences

June 29, 2009
Categories: Uncategorized

In a couple days I'll be leaving for a week-long trip to Riga and St. Petersburg. I'll be in St. Petersburg from July 5-8, and will give a talk on BRST and Dirac Cohomology at a conference there. I'm finishing up a draft of a paper on the subject now, will try and write about it here after I get back from the conference.

The Strings 2009 information blackout continues. Still no talks or blogging on-line, although a new twit has made it out, summarizing the conference:

> more effort being put into contact w experiment•hope for LHC•fundamental issues need more attention•much optimism

although that could have functioned as a summary of pretty much every Strings XXXX conference held during the past 20 years.

Even if no information about the Strings 2009 talks ever gets out, many of the same speakers are speaking at uncountably many other strings conferences scheduled before and after the big one, conferences that are likely to have web-sites that make the talks available. Here are some of them:

Florence

Warsaw

Saclay

Porto

Lisbon

Benasque

Frascati

Potsdam

Wroclaw (Robert Helling will be blogging)

Crete

Santa Barbara

For less technical and more audio-visual enlightenment, you might want to check out the following:

A CBC podcast interview with Lee Smolin.

A Bloggingheads diavlog with Sean Carroll and Mark Trodden.

Comments

1. **Robert**
   June 29, 2009

   Please move my name one line down, I am in Poland, not in Potsdam.

   So far, I have not heard much worth reporting.

2. **Tim vB**
   June 29, 2009

   What is "string phenomenology"? Had a look at some of the slides of the talks in Warsaw, there seem to be
   a) talks about string theory,
   b) talks about (IMHO) totally different topics (like the Navier-Stokes equation and turbulence, see Y. Oz – "Gravity and the Shape of Turbulence") that the author tries to connect to string theory in a rather ad hoc way.

3. **Peter Woit**
   June 29, 2009

   Thanks Robert, fixed.

4. **anon.**
   June 29, 2009

   Do you speak any Russian? Just wondering how you will chat at the bars in St Petersburg? (I'd like to visit Russia myself, but am apprehensive about the language.)

5. **Peter Woit**
   June 29, 2009

   No Russian. I fear that any extensive conversations with the locals will be restricted to those who speak English (or French I guess…).

6. **Ilya**
   June 29, 2009

   Hi Peter, I've been following your blog for some time and I should say that I rather share your attitude to the string theory hype.

I'm a physics undergrad at Cambridge but now on vacation back home in St Petersburg so it'd be great to meet you here. What date is your talk?

7. **Peter Woit**
   June 29, 2009

   Hi Ilya,

   I'll be at the conference definitely all day next Monday, my talk is scheduled for 15:40. Come by and find me there….

8. **The Vlad**
   June 30, 2009

   "No Russian. I fear that any extensive conversations with the locals will be restricted to those who speak English (or French I guess…)."

   Speaking from personal experience, don't hold your breath waiting for a local to speak English there. It took me a lot of sign language to get where I wanted to go, most of the time.

9. **Chris Oakley**
   June 30, 2009

   Hi Vlad,

   Your native language being what – Romanian?

10. **Chris W.**
    June 30, 2009

    Somewhat off-topic: Oswaldo Zapata has posted his 3rd essay, "Superstrings, The Most Beautiful of All Existing Theories?"

    See the post here.

11. **Coin**
    June 30, 2009

    I'm sure you will find it entirely unhelpful to know that your trip to Russia more or less precisely coincides with Barack Obama's.

12. **The Vlad**
    July 1, 2009

    @Chris Oakley

    LOL…Aussie actually mate 🙂

13. **the h bar**
    July 9, 2009

    I just noticed that they have uploaded most of the Strings 2009 talks to the official site. It was about time:

    http://strings2009.roma2.infn.it/cgi-bin/roma_program.pl.cgi

14. **Marcus**
    July 9, 2009

    @the h bar,
    All I can find using the link you gave are the PDF files for the slides. I don't find any video. Does anyone have a link to a video of David Gross' talk?

15. **A.**
    July 10, 2009

    Lightcone 2009 is currently going on in Sao Jose dos Campos, Brazil. Organisation: amusing.

    Participants: old boys network.

    Talks: the postdocs are putting in a lot of work and delivering nice talks. The old crowd are mostly putting up "walls of text" and things are a little rambling.

    Hot topics: "lightfront holography", which relates lightfront hamiltonian mechanics to AdS/CFT giving AdS/QCD.

LHC Status

July 1, 2009
Categories: Uncategorized

If you want to keep up with the latest news on the LHC status, tomorrow at 3pm Geneva time there will be a webcast of a talk by Steve Myers. The abstract reads:

> The status of the LHC will be presented. This will include the repair of sector 34, the ongoing consolidation work in the other sectors, and the progress with the new Quench Protection System. The results of recent resistance measurements of the copper stabilizers will be presented.
> The plans for powering the LHC and the tunnel access restrictions will also be discussed. Finally the planning for the start-up and the programme for future operational consolidation work will be detailed.

One crucial piece of news will be what was learned from the resistance measurements in the sector that was just warmed up, and whether warming up of the other sectors will be required.

I'll miss this since I'll be on a plane.

Comments

1. **Sorinis**
   July 1, 2009

   Let's prepare for major dissapointments and hope for the best.
   Could it be that they are going to operate ahead of schedule? (I can even write that with a straight face.)

2. **Greg Sivco**
   July 4, 2009

   Major disappointments? You mean other than 2007 was to be start-up and 2010 is now presented as the soonest?

   How does 2020 grab you, realistically? I'm willing to take bets on the over/under as to when the LHC really takes off. I'm betting on 2020 personally, so we'll set the over/under at 2018. Because my God gentlemen, if something as simple as WELDS are off, what the heck else is wrong that we (and they) don't about yet?

   The defection/return of Austria is a bit telling IMO. Do they know something we don't, but have too much class to admit publicly? Too many cooks, maybe? I don't know, I'm just speculating, just like bootstrapping String Theorists.

   I am not criticizing LHC management. Nor am I being a pessimist or skeptic, just being Realistic. As a Mechanical Engineer, I assure you there is always this ONE guy who shows up at work in projects of this complicatedness (rather than complexity), and his name is Murphy. You may have heard of him, he has a Law ascribed to him.

   The Pauli Effect. It rules, alas.

   But the other particle accelerators were eventually developed, so don't lose hope. The LHC's day will come, sooner or later. Probably later, which is bad news for those putting all their particle physics eggs in the one basket that is the LHC.

   OK, so what do we spend our time on in the meantime, anyone?

   QCD? That would be my call. Quarks and Gluons are after alll …. 99.9 something percent of all there is (not called Dark Matter and Dark Energy by Mass/Energy), right? Much work to be on there I think. Who are the top people working on QCD?

   And what about LQG and CDT? What's new there?

3. **Sorinis**
   July 4, 2009

   I think Frank Wilczec is the best source in OCD. LQG sounds like phrenology to me. Also I think the CERN management were abysmal. I'm thinking robert Aymar. This is project failing because of quality control issues and not "complicatedness." And that is just not acceptable. Perhaps this idea of intenational cooperation in science is not the best way. If there was competion between nations you would see much bettrer and spectacular developments. At thus point even seeing a development (even if not a spectacular one) would be something.

4. **Greg Sivco**
   July 4, 2009

   Ah, thank you Sorinis, much appreciated. "Frank Wilczek, who along with David Gross and H. David Politzer, was awarded the Nobel Prize in Physics in 2004 for their discovery of asymptotic freedom in the theory of the strong interaction."

   Awesome, because Asymptotic Freedom is awesome. Mind blowing in fact, but there must be a reason. In the meantime, there is much David Mermin-ish "Shut up and calculate" work to be done, and if anyone is looking for someone to help in that regard, I am available.

   I assumed early management of the LHC would be abysmal, but I also assumed they would learn from their mistakes. I think they are (or am I just WISHing they are?). IMO it's not a CERN thing, just a human thing. I am open minded enough to be proven wrong, pls advise.

   As far as "International Co-operation" goes, it seems to be working well enough for the ISS. Granted it will be slower than America with Mercury/Gemini/Apollo and the Soviet Union with their programs in the 60's (less bosses, … less BS), but still …

   I understand LQG is speculation. The difference between the advocates of LQG and Strings is that the LQG guys admit that, and the String guys don't, they promote their stuff as "fact."

   Bull. I am SO grateful Peter wrote his book. What an eye-opener. I am especially happy that Ed Witten, who along with Schwartz and Green was responsible for the first Superstring revolution and singlehandedly for the second one, seems to be backing off from Strings, if not completely.

Much diplomacy will be involved. These things have to be handled del-l-l-icately, so said the Wicked Witch of the West. 😉

5. **jks**
   July 4, 2009

   122 slides from the talk are available from the second link in Peter's post. Interesting reading. Among other things, there is a new quench protection system being deployed along side the existing one. Maximum machine energy versus spice integrity seems to be an ongoing concern, but with much new data and understanding.

6. **Sorinis**
   July 4, 2009

   Am I wrong or did Myers basically say that there is no schedule any more? It is all up in the air?

7. **pessimist**
   July 5, 2009

   Greg,

   I am with you – from one engineer to another (I am electrical), I can testify to you that Mr. Murphy is always on the job, waiting for somebody to ignore him. In my business we call it de-murphizing the project – that means planning and checking to excruciating detail which reduces the probability of a Murphy event. Design teams are always doing reviews on each other and the project lead is making everybody do feasibility tests as a part of their design. Based on the report so far, I have a feeling Mr. Murphy has free run of the LHC! As painful as it sounds, the only way to solve the problem once a project has been hosed is to carefully pull each weed out one at a time without messing up the rest of it. It's at least 4 years from turn-on time before all of the bugs are removed.

8. **Sheepdip**
   July 6, 2009

   Off topic for this thread but I thought you'd want to see this new claim:

   http://esciencenews.com/articles/2009/07/06/physical.reality.string.theory.demonstrated

9. **Coin**
   July 6, 2009

   Sheepdip: Trying to respond in short because Peter seems to dislike taking threads too far offtopic, but: One thing I'll note is that the grandiose and wildly inaccurate claim of the headline– "Physical reality of string theory demonstrated"– does not appear to be a claim anyone with Leiden University has made. It was added by ScienceDaily (where I think this article originated?). In fact, it wasn't even a claim made in the ScienceDaily article itself– the claim appears only in the headline, and is in no way supported by anything in the article itself (which is much more conservative in its phrasing). A consistent problem in science journalism is that someone will write a decent article and then an editor will tack on an "exciting" but wildly inaccurate headline which changes the tenor or content of the piece itself. (Actually this is a frequent problem in many other kinds of journalism as well, especially in politics– most people writing in traditional journalism have no control over their own headlines.) If you ignore the headline and just look at what Leiden University team actually *did* you'll find it doesn't have anything to do with "demonstrating the physical reality of strings" (although what they *did* do sounds actually pretty awesome and like a legitimately positive development for AdS/CFT as a research program).

10. **Anonymous**
    July 7, 2009

    Here's a better article on it:
    http://www.physorg.com/news166097923.html

11. **Marcus**
    July 7, 2009

    Here is the actual journal article:
    http://lanl.arxiv.org/abs/0904.1993
    It does seem that we've been hyped again by irresponsible pop-sci journalism. It's not evidence of string "physical reality" at all. Just another application of the AdS/CFT mathematical tool applied to something else besides fundamental particle physics.

This Week’s Hype

July 7, 2009
Categories: This Week's Hype

A Leiden University press release headlined [Physical Reality of String Theory Demonstrated] is being picked up and used to generate news stories in the media.

It starts off:

> String theory has come under fire in recent years. Promises have been made that have not been lived up to. Leiden theoretical physicists have now for the first time used string theory to describe a physical phenomenon.

which follows the usual dishonest and misleading template for attempts to deal with string theory’s failure as a unified theory. The idea is to put out a press release announcing that string theory has finally lived up to its promise and shown its critics to be wrong, because of evidence it may work as an approximation method for some strongly coupled condensed matter or nuclear physics model. The fact that this has nothing to do with string theory’s continuing utter failure as a fundamental theory is carefully not mentioned, ensuring that non-expert readers of the press release will be misled.

A common excuse for this is that it is being done by journalists, with the scientists involved having no responsibility for the misleading material. In a news media story, conceivably this could be the case, but a university press release is something different. University researchers have both the right and the responsibility to ask for the retraction of a university press release that mis-characterizes their work. When they don’t do this, they make themselves responsible for actively misleading the public about this subject.

Comments

1. **Marcus**
   July 7, 2009

   A poster at Physicsforums with pseudonym “Civilized” has taken issue with this post:
   http://physicsforums.com/showthread.php?p=2263776#post2263776
   ==quote==
   On his blog Woit says:

   [The idea is to put out a press release announcing that string theory has finally lived up to its promise and shown its critics to be wrong, because of evidence it may work as an approximation method for some strongly coupled condensed matter or nuclear physics model.]

   I guess he doesn’t know that the AdS/CFT duality is an exact correspondence, not an “approximation method.” String theory consist of equations that describe relativistic quantized strings, and AdS/CFT allows us to test these equations. The only remaining question is whether quantized relativistic strings exist in nature on the scale of the Planck length, and AdS/CFT will continue to be silent on this subject, but the fact remains that the AdS/CFT duality can be and is actively being used to test the exact equations of string theory.
   ==endquote==

   Several things about this puzzled me, one being why wouldn’t the person come here and comment directly? The PF thread had no link to this particular post (although string hype and bad journalism was being discussed.) So it seems like here is a better place to post fulminations against this N.E.W. post and aspersions against the author. It also seemed like a straw man ploy, you didn’t say that AdS/CFT was approximate but he pretends you did and makes a show of correcting your “mistake”. Anyway here it is if you care to reply.

2. **chris**
   July 8, 2009

   Dear Peter Woit,

   the researchers often have no control over the headline of the press release of their own institute – i know this from bitter experience. and once it got snapped up by some of the larger media outlets it is a very very steep uphill battle to get anything revoked. basically, they won’t do it on the grounds that nobody understands it anyways and retraction is always a statement of sloppy work on the part of the media.

   of course you are right that they should do all they can to stop it anyways, but probably the only way of finding out whether they did is asking them directly.

3. **ManyMe**
   July 8, 2009

   I am working on a new theory of everything based on the Ising model. It already predicts gravity and I am sure it will contain the standard model and it will also describe supersymmetry and all kinds of new particles.

   So far there is no evidence whatsoever for my new theory, but
   it turns out that the Ising model is very useful in cond. mat. physics, in particular to understand phase transitions of ferromagnets.

   I take this as evidence that I am on the right track with my theory of everything.

4. **anon.**
   July 8, 2009

   “may work as an approximation method for some strongly coupled condensed matter or nuclear physics model.” – Woit

   “AdS/CFT duality is an exact correspondence, not an “approximation method.”” – Civilized

   Civilized fails to understand that you can use an exact correspondence between conformal field theory and anti-de Sitter as an approximation method to systems that don’t exactly correspond to either. Duh.

5. **ManyMe**
   July 8, 2009

I forgot to mention that I will put out a press release about it soon.

6. **John F. McGowan**
July 8, 2009

Is this that remarkable?

As I understand, the string theories started out as models of nuclear resonances, mesons, and so forth in the 1960's. There were and are many empirical observations of nuclear resonance behaviors and properties that can be explained by modeling the nuclear resonances as vibrating strings of some sort of subatomic nuclear matter.

It is my impression that Veneziano's original string formula was a model for nuclear scattering data.

The Lund Monte Carlo which at least used to be heavily used in experimental particle physics to model hadron jets and so forth is based on a string model of the mesons.

The modern interpretation is that QCD somehow yields a tube or string of flux that connects the quark and anti-quark in a meson. There used to, at least, be numerous theoretical attempts to show that QCD produced string-like behavior either through an exact analytical solution to QCD or some type of approximation.

While there is little (no?) empirical evidence to support superstrings as a unified field theory, there has been evidence of string-like behavior in strong nuclear forces for forty to fifty years.

It does not seem surprising that mathematics that had its roots in modeling strong nuclear interactions might be successfully applicable to nuclear physics.

7. **Peter Woit**
July 8, 2009

chris,

The problem isn't just the headline, it's the entire lead paragraph and context in which the story is put. I haven't heard of any cases where researchers have tried to get string theory hype about their work retracted and failed. One case I know of where a researcher complained about this, the story was retracted and rewritten, for more, see

http://blogs.discovermagazine.com/cosmicvariance/2006/02/27/any-publicity-is-good-publicity/

If the researchers involved in this case are unhappy about the misrepresentation of their work, there are many ways that they can make that known. I haven't heard anything…

8. **karl**
July 9, 2009

@ John:

Yes, string theory was originally intended as a theory of strong nuclear interactions. And now it is back to describe precisely this: QCD in the strongly coupled regime. So is this remarkable?

Well, YES!
and the story behind all this is truly revolutionary. Note that there was at least one revolution involved in that story that lead to several Nobel prizes: non-abelian gauge theories. How they first killed strings and now resurrect them, making a sidestep into the fifth dimension is amazing. It took 40 years or so to develop all this (and a community of several hundreds of scientists). It is a complicated story and it is still being written and probably we will not know for another 40 years if it is "wrong". Is it worth being told? Definitely!

Many non-string theorists think it is (jan zanen is a condensed matter physisicst). b.t.w. just now, something like the creme de la creme of condensed matter physics is gatherning right now at kitp santa barbara to learn string theory (and to teach condensed matter theory to string theorists).

Don't get fooled: science is a tough job. As a scientist you can do a valuable job even if you don't yet know if your theory is right or wrong!
After all, the only rule of science is "anything goes"!

9. **Chris Oakley**
July 9, 2009

Karl,

I am under the impression that fundamental physics nowadays is dominated by hippies and comments like yours do nothing to assuage this belief.

Anything goes – really? What about if it does not fit known experimental facts? Or even makes no predictions at all? What about if it is mathematically inconsistent?

And why should researchers in a relatively healthy branch of physics wish to co-opt ideas from a failed research program in another?

10. **David B.**
July 9, 2009

Hi Chris:

High TC superconductivity has not been solved theoretically. There is agreement that it requires a strongly coupled system, for which very few theoretical techniques actually work.

The AdS/CFT setup provides new ways to understand strongly coupled systems that serve as toy models for real world physics. The AdS/CFT ideas are not a 'failed research program' as you describe it.

Toy models usually provide insight that is unavailable otherwise. That is why Condensed matter theorists are learning this stuff, and this is why string theorists are learning condensed matter physics.

11. **karl**
July 9, 2009

Dear Chris,

yes really “anything goes” (P. Feyerabend)!

and if our theory does not fit any experimental facts, why do you care?
evolution will take care of us, or at least of our theories 😉

(although I admit that last point is more Popperian than Feyerabend)

12. **Chris W.**
July 9, 2009

So, the string theory community (or a portion thereof) has become a collection of applied *mathematicians*, currently assisting condensed matter *physicists*.

Nice to have that cleared up…..

13. **Coin**
July 9, 2009

Ugh, so I made a post in the previous thread which now looks pretty stupid. In that thread someone posted a reprint of this press release; looking around I was able to find the same article printed more or less verbatim on several other websites including sciencedaily, but wasn't able to find the actual original Leiden University press release they were actually copying. Trying to be charitable, I made a post attempting to pin this on an overenthusiastic journalist somewhere who then got picked up by some other sites. But no, the whole thing was copied verbatim from the Leiden University release Peter linked. Which just… I don't even know. There really is no excuse for something like this coming out of a university PR department– I realize university PR departments aren't great but I feel like this article crosses a couple of lines.

What's really fascinating me at this point is the thing that tripped me up in the last post– that this press release got more or less literally copy-and-pasted and posted as a story on so many internet news sites with no indication of authorship more clear than, say, (as sciencedaily put it) “Adapted from materials provided by Leiden University”, or “Source: Leiden University”, or other tags written such as might lead the naive or foolish (i.e. me) to assume that the story had been written *using* Leiden as a source, not that Leiden had simply outright written the article. More interesting yet is that so many of these sites, including a couple that one might think of as at least attempting respectability, printed this article seemingly without even having read it– not only accepting its premises as accurate without stopping to question them, but in most cases not even bothering to *proofread* it. Is this sort of thing happening more often than I thought?

14. **Peter Woit**
July 9, 2009

Coin,

Traditionally what a lot of media outlets really do is just take press releases and wire service reports, and print them without much editing. The main function of the editor/reporter is just to sort through the mountain of such things and identify the ones of interest to their readers. These days the web is full of sites that do little but reproduce press releases and stories from elsewhere. This Leiden press release is now on dozens of sites.

By now I've seen a lot of press releases like this, and it's pretty clear how they get generated:

1. Press office at university has someone whose job it is to find stories like this to write. Publications like Science/Nature/PRL encourage such press releases, so press office person gets notified that someone at their university has a new result tha might be worth a press release.

2. A story about how your university's researchers have found a possible new approximation method for dealing with certain condensed matter models doesn't sound very sexy. Press officer notices the “string theory” angle, knows that “string theory” is sexy, as well as controversial because it has been criticized as not predicting anything about nature.

3. They talk to researchers, planning to put together a story about how “university X researchers have solved the great problem in the sexy subject of string theory, how to use it to predict something”.

4. Researchers play dumb. They're not the ones who wrote the headline…

15. **Roger**
July 9, 2009

I think you're being a little unfair Peter.

There are many things researchers should do but, for various reasons, don't – this is one of them. If it helps them raise their profile inside the university then this type of stuff may still be for the good even if a distorted message comes out. Furthermore, arguing with a PR department to make sure they get it right takes time and energy and is frequently a fruitless task.

Also, your comments would carry more weight if you took the time to publish and disseminate (inside and outside the community) your own original research instead of just criticising other researchers who do publish.

16. **Arun**
July 9, 2009

Roger, what you're saying seems to me to be analogous to “don't criticize credit default swaps, go and create your own”.

17. **Peter Woit**
July 9, 2009

Roger,

The misleading message of the title and lead paragraph of this press release is not some random distortion introduced by an ignorant journalist that busy researchers don't have the time and energy to get fixed. Some string theorists have decided that the way to deal with criticism of string theory's failure as an idea about unification is not to answer it honestly, but to engage in a campaign to mislead the public about the topic. I'll keep pointing this out as long as people keep doing this.

I've just got back from a trip disseminating my own original research, I'll write more about this here soon.

18. **Shantanu**
July 9, 2009

Peter any thing interesting from PLANCK 2009, TAUP 2009 or AMALDI 8 comferences?

19. **Tim vB**
July 10, 2009

Hello karl,

"yes really "anything goes" (P. Feyerabend)!"
Surely you were joking, right? However, what Feyerabend said in "against method" was that you cannot decide if some activity is science by judging the methods that are used. "against method" should be understood as "against the enforcement of certain methods as the only ones that may be used by scientists". Feyerabend would say that astrology and astronomy do not differ much in the methods used, but in the results that are obtained (useful or not useful). So, if you get your best ideas by throwing chalk at the blackboard, you do that. But don't claim that the dots you get are useful results, Feyerabend would not agree 🙂

"evolution will take care of us, or at least of our theories (although I admit that last point is more Popperian than Feyerabend)"
Thomas Kuhn is your friend here, Popper was always rather idealistic and supposed that a theory dies by falsification, not by loosing all fans due to a plane crash or something like that.
BTW, I do not want to wait that long! Please? Let's hope string theorists find a different way than dying in dealing with their research program.

20. **Roger**
July 10, 2009

Arun, I'm saying nothing of the sort .

Publishing and disseminating work directly exposes a scientist to the machinations of university PR departments. Opinions backed up by direct experience are often more solid than those given by people without direct experience of the processes they're criticising.

Peter – I can't comment on this individual case. However, in my experience, university PR departments jealously guard their right to determine "the message". I've been involved in a few cases where it just wasn't worth the effort to try to correct their misconceptions.

21. **nbutsomebody**
July 25, 2009

After all this hype it seems that the paper (arXiv:0904.1993) was indeed wrong as confirmed in arXiv:0907.2694.

22. **Thomas Larsson**
July 27, 2009

FWIW, there is a new paper on the arxiv 0907.4238 that references NEW (ref 82).

23. **concerned cynic**
August 20, 2009

University PR departments have a mission: to get what researchers do be known and talked about, and to increase the likelihood of success of future grant applications by researchers employed by the university. The preceding somewhat pedantic sentence deliberately omits "telling the truth about what researchers are doing." That's the job of the researchers themselves, via what they upload to arXiv and eventually publish.

I learned what I say here when I was first intereviewed by a journalist employed by university PR, in 1986. It quickly dawned on me that the person across the desk had been carefully briefed to promote an agenda. Roger above is indeed right: "university PR departments jealously guard their right to determine 'the message'."

A Montreal category theorist told me a few years ago that the Canadian equivalent of the NSF will no longer award grants to category theorists. String theorists, fearing a similar fate, have enlisted university PR departments in their defence. All's fair when faced with the prospects of having one's department closed (yes, simply because physics is not a popular major), of having one's PhD program shut down. or of having to teach undergrad service courses. To tell, in effet, a natural scientist "You will not be awarded another grant unless you make substantial changes to your research agenda" is akin to banishing him/her to Coventry.

Those who don't like Smolin's book patronize it by saying that Smolin does sociology. Those who don't like Woit's book may have similar opinions. But the problem of string theory is as much economic as sociological: the dearth of tenure track positions in theoretical physics. Publishing a lot of papers does not assure one of a job; you need to find a department that also likes your work. And we tend to like work that supports our pet agendas. This puts too much power in the hands of the tenured conformists.

Various and Sundry

July 10, 2009
Categories: Experimental HEP News, Multiverse Mania, Uncategorized

For the latest on the status of the LHC, see the July 2 talk of Steve Myers mentioned here earlier, and a July 8 talk (slides, video) that has some more recent news. The question of what to do about bad splices is still up in the air. The current plan is to make measurements at 80K during the next few weeks on the three sectors that have not been warmed up, then present options during the second week of August to the DG and the experiments. The decision to be made will be about how long to delay the start-up to fix more splices. If more splices get fixed, the machine can safely be run at higher energy. The optimistic scenario now seems to be that it will be possible to run at some energy in the range of 4-5 TeV/beam, without introducing further delays in the current draft schedule (the latest schedule has the machine ready to start circulating beams around the end of October). Gordon Watts has more detail in a recent post, including one of the relevant plots showing the energy vs splice resistance trade-off.

Two items on the multiverse front:

- Lenny Susskind gives new depth and meaning to the word “chutzpah” with an article in Physics World on Darwin’s Legacy. It seems that Darwin’s legacy for physics is the field of string theory anthropic landscape pseudo-science. Luckily, I don’t think creationists normally read Physics World.
- Sean Carroll’s book “From Eternity to Here” is now scheduled to appear next January. It has a Facebook page and a mission statement:

> You can turn an egg into an omelet, but not an omelet into an egg. This is good evidence that we live in a multiverse. Any questions?

String theorist Oswaldo Zapata has posted the third part of his essay on the history of superstring theory, dealing with the question of the “beauty” of string theory. Basically he argues that it was only in 1999, after it started to become clear that string theory unification wasn’t working out, that a publicity campaign about the “beauty of string theory” got started:

> During the late eighties and early nineties, and motivated by the relative success of the heterotic superstrings, string theorists were submerged in intricate and endless computations trying to recover the standard model using a ‘‘top-bottom’’ approach. At that time no one was talking publicly about a beautiful construct. In fact, the theory was in an ugly impasse and mathematical consistency was the only remote trace of beauty…
>
> In this section we have seen that, in contrast to what is currently claimed, string theory was not always considered to be a beautiful theory. The public recognition of the beauty of the theory is recent, dating from around 1999, and it was due mainly to the convergence of two factors: a favourable context, “internal” and “external,” and an acute sense of opportunism.

From the Publisher’s Weekly review of Graham Farmelo’s life of Dirac:

> In 1955, Dirac came up with a primitive version of string theory, which today is the rock star branch of physics.

The opera Hypermusic Prologue: A Projective Opera in seven Planes, libretto by Lisa Randall, had its first performance last month in Paris. It will be presented again in Barcelona in November.

FQXI seems to like to have conferences for their members at scenic volcanic locations in the mid-Atlantic. This year it’s the Azores, here’s their schedule, talks to appear here, blogging here (Sabine Hossenfelder) and here.

Strings 2009 finally got around to putting slides from most of the talks online here. The one talk that seemed to have something new wasn’t about strings, it was Arkani-Hamed’s talk on Holography in Flat Space: Algebraic Geometry and the S-matrix, based on work to appear with Cachazo, Cheung and Kaplan. It’s based on studying the structure of amplitudes in twistor space, and the talk includes many exclamation points, and the claim that “SOME POWERFUL MATHEMATICAL STRUCTURE IS AT WORK!”. More specifically:

> Very natural and beautiful mathematical structure – intersection theory and Schubert Calculus – seems to lie at the heart of tree and loop gluon scattering amps!

From the slides it does appear that there’s some nice mathematics at work here, I look forward to seeing the paper.

Comments

1. **Tim vB**
   July 10, 2009

   Citing the last paragraph of the article by Lenonard Susskind:
   “Whether string theory with its huge landscape, and eternal inflation with its reproducing pockets of space, will prove to be correct is for the future to decide. What is true is that as of the present time, they provide the only natural explanation of the universe that lives up to the standard set by Darwin.”
   Currently I am greatly enyoing his book “An Introduction to Black Holes, Information And The String Theory Revolution”.
   How can a capable physicist and author like him write an article like that? This is completly beyond me.
   A key of evolution is the survival of the fittest, with the definition of “the fittest” being constantly in danger to become a tautology (“Well, the fittest were the ones that survived, what else?”).
   Did Leonard Susskind give somewhere a definition of what being the fittest universe of the multiverse means (boy, I hope it is the one we live in!) and how a process of selection would work?
   At least he admits that string theory has still to prove itself.

2. **Daniel de França MTd2**
   July 10, 2009

   Peter,

   In your opinion, do you think Nima, Cachazo and others are trying to find a non-string quantum gravity, but that uses susy just as a scattering symmetry rule?

3. **ManyMe**
   July 10, 2009

   you cannot turn an omelet into an egg, but you can turn stupidity into a book.
   This is good evidence that we live in a multiverse, because stupidity is infinite and a single universe could not contain all of it.

4. **Peter Woit**
July 10, 2009

Daniel,

My understanding is that one motivation is to get some sort of analog of AdS/CFT for flat space (rather than anti-deSitter), but, unlike AdS/CFT, this is supposed to a weak-weak duality, so it's something very different. The CFT side I guess is N=4 SYM, or maybe N=8 supergravity, but I have no idea what they are expecting on the "AdS" side, other than that it would involve twistor space.

5. **Count Iblis**
July 11, 2009

If all the technical problems with the LHC are fixed in time, Murphy's law suggests that a swine flu outbreak may cause further delays.

6. **Jim Clarage**
July 11, 2009

"You can turn an egg into an omelet, but not an omelet into an egg…" ergo proof of multiverse.

Bah.

Maybe you or I can't turn an egg into an omelet, but the universe does this all the time. Or are we to presume eggs came preformed as initial conditions of the big bang? Farmers and chicken coops do this reaction millions of times each day. I'm not being flippant, but pointing out the obvious observation which wrinkles this entire argument. Higher order biological laws create low entropy eggs from initial states with much higher entropy than the proverbial omelet, viz., disordered solutions of amino acids and H2O.

7. **Jim Clarage**
July 11, 2009

Time's arrow edit. I meant:

Maybe you or I can't turn omelets into eggs, but the universe does this all the time…

8. **Pawl**
July 11, 2009

Jim,

I'm not endorsing the multiverse, but the argument about the second law depending on low entropy in the early Universe is a good one and almost by definition as valid as the second law is.

Chickens do produce eggs daily, but with a significant expenditure of energy. Where does the energy come from? Plants, which derive their energy from the Sun, a star. That the Universe is populated with stars (and is not a much more random structure) is a result of the near-uniformity of matter in the early Universe **together with** the fact that the gravitational field was of such a special type as to allow that near-uniformity. If a more "random" gravitational field had been there, the form of the Universe would be entirely different (no stars and no chickens, probably).

The near-uniformity of the matter does contribute some entropy, but it is the extreme specialty of the gravitational field which makes the early Universe a low-entropy state (compared to a "random" Universe).

9. **Greg Sivco**
July 12, 2009

Even Lubos Motl disagrees with Susskind re Anthropic/Darwinism if you check out his website.

Is the situation still as Peter wrote in his book re String Theorists that there are 2 camps: Anthropic (Susskind, Polchinski) and Not-Anthropic (Gross, Witten)?

"How can a capable physicist and author like him write an article like that? This is completely beyond me." … Tim vB

Might I suggest==> "Cognitive Dissonance?" That's a psychology term about why the human brain finds it difficult to accept things when evidence is shown that a fast-held belief is wrong. Happens all the time and has no correlation to IQ. Synonyms include "lying to oneself" or "Rationalization."

Peter's book explains the Anthropic in String Theory beautifully: in a nutshell, it's a desperate measure to explain the non-predictive and unfalsifiable String Theory. Essentially, it's desperation.

10. **Tim vB**
July 13, 2009

Hello Greg,
I know about cognitive dissonance: In the EU every package that contains cigarettes or cigars must by law have a big label saying something like "smoking can kill you" or "smoking can cause cancer" etc.
I once asked a heavy smoker what his opinion of this was, he answered "of what?".
He had not noticed…(the labels are big, you cannot miss that by chance).

11. **Greg Sivco**
July 14, 2009

Tim, that's a nice analogy but I was thinking of Leonard Susskind and String Theory, because as far as I can tell the "String Theory War" is either over or coming to a close, and ST isn't the winner. Motl, Susskind et. al. will obviously disagree. If the Anthropic Landscape doesn't spell D-E-S-P-A-R-A-T-I-O-N, what does?

There's an outside chance (think: Quantum Tunneling) that Witten will bring his incredible intellect back to falsifiable and predictive Physics, indeed I think I see signs (but do not know …. I'm speculating), but I don't think Susskind ever will. He was there at the beginning, and he strikes me as a very stubborn man. Which isn't a bad thing all the time, actually. His is also an incredible intellect.

Nor do I think String Theory should completely go away. I just don't think it will be falsifiable or predictive until 2200, 2300, or even 2400. Can I hear a 2500? Shrug, who knows?

Leonard Susskind will speak at the Holographic Cosmology Conference that will run from tomorrow through the 18th at The Perimeter Institute. I hope Peter or someone links to his talk.

12. **Shantanu**
July 14, 2009

Peter, have you seen this
paper ?
It is an example of a string theorist venturing into LQG.

13. **Greg Sivco**
July 14, 2009

Thank you for that link, Shantanu, good stuff. I especially liked this line in the abstract:

"This suggest [sic] the possibility of a relation between Loop Quantum Gravity and supersymmetric string theory, where the Barbero-Immirzi parameter and field of the former play the role of the supersymmetric axion in the latter."

Well, what can we say? Lee Smolin, former author of string theory papers and ever the diplomat, mentions in TTWP that LQG and St need not be combative, but co-operative i.e. complementary, the point being (if I read Lee correctly) that the difference between (some but not all) Strings and LQG is one of scale, with LQG (and Quantum Graphity and CDT especially) operating at the smaller scale.

But good luck trying to get String Theorists (the hardcore ones) to agree with that! Their field has gone from something very specific, namely: trying to explain quarks and the Strong force until Quantum Chromodynamics kicked them to the curb, to getting more and more general until we arrive at the present with Anthropic Landscape, in which case everything is possible and we may as well embrace Many Worlds. Which I will not, nor do I think most reasonable people will.

14. **Peter Woit**
July 14, 2009

Please, remember this is not a general physics discussion board, and the owner has a rather limited degree of interest in quantum gravity.

15. **Greg Sivco**
July 14, 2009

Thank you, Peter, my interest in QG is primarily as a hobby. It's fascinating, yet not as important as those branches of Mathematics and Physics that push knowledge a bit farther than where we are. It would be nice if we have a 1925-1929 situation where the curtains of ignorance are vastly pushed back, and years such as those may well be in hand, but who knows?

I love your weblog, so much so I couldn't find time to read your book until a month ago, and of course I am juiced as a rookie reader.

And what can I say about your book; it's awesome, and pleasantly surprising, as your road to this stuff we're talking about is the same as mine, from Astronomy to Physics to Mathematics.

Best of luck to you by the way on your exploration of BRST Dirac Cohomology. Sounds enticing.

Specifically, I have completed a 10-month general overview of current Physics/Mathematical physics, and find it is now the time to choose a specialty, with QG forever a hobby that I will spend time on, time willing. I have narrowed it down to the following fields. your opinion, please:

– Math: Representation theory of 4D space-time gauge symmetries and diffeomorphism group representation theory as related in the last two paragraphs of your book. How has that developed since publication. Where is the most current work needed?

– Physics: The Quantum Hall Effect. Low temperature sure, but incredible how the quantum world can be made manifest in the macroscopic. I'll work on this one by myself, what a puzzle.

16. **Peter Woit**
July 14, 2009

Greg,

The paper on Dirac cohomology is intended to be a toy model for the interesting case of 4d. You study representations by studying an invariant, their Dirac cohomology. To do this in 4d requires new ideas: everything is parametrized there by the space of background connections and metrics and you need to figure out what to do about that. Right now I'm thinking about the easier case of 2d…

I think we're still quite a ways away from even knowing what the right way to think about such representations is, so this is a difficult subject for someone to study. It's too wide open right now, and the relevant mathematics is not even clear. That quantum Hall effect looks like a much more straightforward problem…

17. **M. Wang**
July 14, 2009

The most relevant application of Darwin's theory on high-energy physics is the evolution of the physicists landscape. Here, natural selection works through paper publications, and the fittest are those who can keep publishing the most, however meaningless these papers may be. Susskind and his cohorts are successful survivors of this process. Although I admit this result is not pretty, but hey, that's part of Darwinism.

18. **observer**
July 14, 2009

…..Wait a minute; with the DNA you can identify a person from a million, with ST (and branes), still you cannot do anything concrete just hope for some right arrangement to be correct. Sorry but lame analogy.

BRST and Dirac Cohomology

July 11, 2009
Categories: BRST

For the last couple years I've been working on the idea of using what mathematicians call "Dirac Cohomology" to replace the standard BRST formalism for handling gauge symmetries. So far this is just in a toy model: gauge theory in 0+1 dimensions, with a finite dimensional Hilbert space. Over the last few months I've finally got this to the point where I think I understand completely how this should work, at least for this toy model. I talked about this last week in St. Petersburg, and have a preliminary version of a paper on the subject, which is available here. Next weekend I'm leaving for a trip to Shanghai and Hong Kong (the plan is to be in Shanghai for the July 22 total solar eclipse, which will be visible there). After I get back at the beginning of August I'll work on the paper a bit more, hoping to have a final version done by the beginning of September, when the academic year starts.

The paper uses quite a lot of mathematical technology, so I fear most people will find it hard to read. This fall I hope to get back to finishing the Notes on BRST I was writing up, the idea behind those was to give a more expository account of this subject. That project got bogged down when I realized there was something I was still confused about, and after getting unconfused it seemed like a good idea to get the basic ideas down on paper, since the expository project might take a while to complete.

First of all, what this is and what it isn't. It's a toy quantum mechanical model, with gauge symmetry treated using some new ideas from representation theory which are related to BRST, but different. It's not a QFT, and not a treatment of gauge symmetry in the physical case of four space-time dimensions. I've been thinking about how to extend this to higher dimensions, but this requires some new ideas. Next on the agenda is to try and get something that works in 1+1 dimensions, where one can exploit a lot that is known about affine lie algebras and coset models. There also appear to be interesting possible connections to geometric Langlands in that case.

Given a quantum system with G-symmetry, the BRST method allows one to gauge a subgroup H, picking out the H-invariant subspace of the original Hilbert space using Lie algebra cohomology methods. The proposal here is to do something different, picking out a subgroup H of symmetries one wants to keep, and gauging the rest. In the special case where Lie G/Lie H is the sum of a Lie subalgebra and its conjugate, the method proposed here reduces to the standard BRST method, but it is more general.

An algebraic version of the Dirac operator plays a role here somewhat like that of the BRST operator in the standard formalism. One difference is that the square of this operator is not zero. However, it is in the center of the algebra of operators acting on the Hilbert space, so its action on operators squares to zero. This sort of thing has been studied a bit before in the physics literature, in the context of supersymmetric quantum mechanics models, but I do believe that the interpretation here as a method for handling gauge symmetry is new.

One thing I want to add to the paper is some comments about the relation to the physical Dirac operator. The point of view on the Dirac operator explained here that comes out of representation theory seems to me perhaps the most intriguing part of this story. Remarkably, this Dirac operator is in some sense a quantization of the Chern-Simons form. The full story of how to use this in higher dimensions remains obscure to me, but there is some hope it will bring together the physical Dirac operator, something like BRST, and something like supersymmetry in a new way.

Comments

1. **Austin**
   July 11, 2009

   Peter, what are the testable predictions of the generalized physically unphysically topological Woit-Dirac-Kostant-BRST-supersymmetry non-nilpotent cohomology-non-cohomology and how can one falsify it, aside from pure mathematical proofs that it cannot work?

2. **Peter Woit**
   July 11, 2009

   "Austin"

   The problem with string theory is not that it doesn't now make testable predictions, but that 25 years of intensive effort have shown that it inherently can't.

   Whether ideas related to Dirac Cohomology will ever lead to progress on doing better than the Standard Model, I don't know. I think it's reasonable to hope that they might lead to a better understanding of the Standard Model and its relation to fundamental mathematics. This seems to me worth working on and I'll keep doing that. If 25 years from now this hasn't led anywhere, but there are institutes of Dirac Cohomology and thousands of physicists working in the field, I'll be the first to criticize this.

   Quantum gauge theories remain a largely unexplored rich mathematical territory. Exploring this may lead to new physics, or it may lead to new mathematics. What I'm writing about is just one idea about how to make progress on this. We'll see if it gets anywhere.

   And, with that, from now on I'll delete any more stupid anonymous comments to this posting. If you want to discuss the ideas I'm writing about here, I'm happy to do so. If you want to engage in juvenile argumentation, you'll have to do it under your own name here, or find another posting.

3. **Per**
   July 11, 2009

   Congratz

   You must be a bit nervous now tho. All those string theory people has critized you over and over again for not publishing anything. Now it comes, so I am sure they'll all jump on it.

   Best of lucks.

   // phd student.

4. **Christine**
   July 11, 2009

   Peter,

   Sounds interesting, but it will make a somewhat hard reading for me. I understand that it is just a toy model for the moment, but I missed the main motivation for it. Do you envision any methodological advantages over BRST or perhaps a possible connection to the Langlands programme?

Best,
Christine

5. **Tumbledried**
July 11, 2009

Per,

In my opinion, there in nothing wrong with pursuing a speculative line of enquiry – that is what research is supposed to be all about. "Search", and "search again" ie Re-search. Results are never guaranteed. In fact the chance of a particular line of reasoning being successful, in absence of some very good heuristic/intuitive/formal/physical reason, is very, very small.

And even if there are grounds to believe something to be true, that does not ensure success. String theory is an example of this. There originally were relatively good grounds and motivation to pursue it as a topic – 30 to 40 years ago. But it is pure foolishness in an area **where results are not guaranteed** to put all resources into one particular topic.

The whole scientific community benefits if a variety of ideas/approaches are posed to attack fundamental questions. Contrariwise, the whole community suffers if all avenues of attack save a few – which furthermore have yet to be vindicated (if ever!) – are stifled.

So the burden of proof is not on Peter in this case, since I do not think he is advocating/demanding that everybody drop everything and work on BRST.

tumbled

6. **Peter Woit**
July 11, 2009

Per,

Thanks, but I'm not worried about that. The problem with working on something rather abstract and far from what other people are used to is that it's likely to be ignored. If string theorists decide to learn about Dirac cohomology in order to criticize what I'm doing with it, so much the better…

Christine,

The BRST treatment of gauge symmetry has various problems, especially non-perturbatively. Maybe Dirac cohomology avoids these, too soon to tell. It also may be useful in a non-perturbative formulation of chiral gauge theory, something which doesn't really exist at the moment. Another possible application would be to make sense of the vague idea that spinor fields are the analogs of ghost fields for local translation symmetry. All of this is rather speculative at the moment.

One approach to geometric Langlands uses BRST, perhaps Dirac cohomology gives something of mathematical interest there, including a closer connection between geometric Langlands and 2d gauge theory. Again, all speculation at the moment…

7. **Michael Zeiler**
July 12, 2009

Off topic, but you mentioned that you'll be in Shanghai for the total solar eclipse on July 22.

I just posted some eclipse maps with lines of equal duration for totality and local circumstances at

http://www.eclipse-chasers.com/tse2009maps.php

The map for Shanghai is TSE2009_China_120E_123E.pdf. If you need to dodge clouds and drive east, then you also want TSE2009_China_117E_120E.pdf

For a larger view, try Regional_TSE2009_CentralChina.pdf and Regional_TSE2009_EastChinaSea.pdf

Clear skies!

8. **Peter Woit**
July 12, 2009

Thanks Michael,

The plan so far is to watch the eclipse from downtown Shanghai, we'll see if that works out…

9. **a quantum diaries surivivor**
July 12, 2009

Another eclipse! I am extremely envious! I still have to see one… I seem to be on the opposite side of the world when one occurs. Have fun!

T.

10. **Tipster Dude**
July 13, 2009

Peter, if you add this WordPress shortcode in the HTML of a page, [archives], it will list links to all your posts on one page, which would be around 750 links.

WordPress shortcodes.

So you could create a page such as "Archives", and then create a link to that page in one of your link categories, if inclined.

Formalizing theory from a heavy mathematician's perspective ==> magical mystery tour for all but a few select physicists. Serves them right.

11. **Thomas Larsson**
July 20, 2009

I cannot say that I understand what you are writing. However, it seems to me that you make the same crucial assumption in Dirac cohomology as in BRST: that the relevant algebra has representations. This becomes problematic if you want to study current groups in multi-dimensions. Classically, there are no problems; the representations typically act on fields over spacetime, i.e. on sections of some bundles. However, after quantization you run into various infinities, which can not be removed by normal ordering. This problem seems independent of whether you consider Dirac or BRST cohomology.

One can construct representations acting on quantum fermions parametrized by a classical background gauge field. This idea, which you seem to favor, has been put forward by Jouko Mickelsson. Alas, I think it is a bad idea, at least if you want to study fundamental physics. It may work within a limited scope, but fundamentally everything must be quantized. The introduction of a classical gauge field is therefore manifestly unphysical.

The reason why Jouko considered this type of representations is a no-go theorem by Pickrell. IMO, the moral of this theorem is that no interesting representations of current groups can be constructed within the framework of QFT, but one must consider a more general framework instead. Since I know that you don't want me to mention my own work, let me link to an old paper by Rao and Moody, where they construct vertex representations of current algebras. The thing to note is that these representations do not depend on some background gauge potentional, so Pickrell's no-go theorem is evaded. However, the Rao-Moody representations can not arise in QFT.

12. **Jonathan**
August 5, 2009

Were you in Shanghai for the eclipse? I made it West to Wuhan and saw a few fleeting glimpses, a few km South West had perfect skies, but such is life. Third eclipse, third cloudy eclipse – I'll get my corona yet!

13. **Peter Woit**
August 5, 2009

Hi Jonathan,

Yes, I was in Shanghai, which was cloudy, except for a few short glimpses of the partial phases (one just after totality). I can't complain too much though: this was the fifth eclipse I've tried to see, the previous four had clear skies.

14. **Peter Woit**
August 5, 2009

Thomas,

I agree that the background gauge field needs to be quantized. But it appears to me that what you naturally are getting is not a representation, but a family of representations parametrized by the background field. Perhaps this can be quantized by a path integral sort of construction, basically integrating over the background fields.

Interestingly, in geometric Langlands, Frenkel argues that you should think of the group as acting not on a representation, but on a category of representations.

15. **Thomas Larsson**
August 7, 2009

Peter,

I strongly disagree, and much prefer the Rao-Moody class of representations. They cannot arise in QFT (their extension is proportional to the second Casimir rather than to the third), but we know that QFT must break down anyway since it is incompatible with gravity. The extra ingredient that must be added is the observer's trajectory in (n-1)-dimensional space. This is secretely encoded in the n-1 null roots, defined in R-M's Definition (3.3).

Given that every experiment involves an observer, and none fundamentally involves classical fields, I believe that representations are more physical if they depend on the former rather than on the latter. But then again, my opinion might be colored by the fact that I have invested 15 years of my non-career into this class of representations.

Latest From the LHC

July 15, 2009
Categories: Experimental HEP News

Here's an announcement from the CERN DG Rolf Heuer sent out to CERN employees today:

> The foreseen shutdown work on the LHC is proceeding well, including the powering tests with the new quench protection system. However, during the past week vacuum leaks have been found in two "cold" sectors of the LHC. The leaks were found in sectors 8-1 and 2-3 while they were being prepared for the electrical tests on the copper stabilizers at around 80 K. In both cases the leak is at one end of the sector, where the electrical feedbox, DFBA, joins Q7, the final magnet in the sector.
>
> Unfortunately, the repair necessitates a partial warm-up of both sectors. This involves the end sub-sector being warmed to room temperature, while the adjacent sub-sector "floats" in temperature and the remainder of the sector is kept at 80 K. As the leak is from the helium circuit to the insulating vacuum, the repair work will have no impact on the vacuum in the beam pipe. However the intervention will have an impact on the schedule for the restart. It is now foreseen that the LHC will be closed up and ready for beam injection by mid-November.

This is an extra two week or so slip with respect to the latest draft schedule I'd seen. In addition, the question of how to deal with defective splices remains open. Efforts now are directed towards determining what the maximum safe energy is, assuming that the cold sectors are not warmed up, with the plan to have an answer to this question by the second week of August. Part of this effort involves study of possible changes in the parameters that determine how quenches are detected and dealt with, in order to optimize the maximum safe energy.

Update: The latest CERN Bulletin is out, with more about this.

Comments

1. **ObsessiveMathsFreak**
   July 16, 2009

   I'm beginning to think that the LHC will never run as advertised. It's too big, has too many single points of failure, takes far too long to repair. It's like a giant software project, where a single tiny bug can crash the system, but that takes six weeks to recompile from source, even if only the slightest change needs to be made.

   I don't think these issues are teething troubles. The design of the LHC was probably too optimistic about the potential for errors. There has been a years delay already, and the machine hasn't even done anything yet.

   Is it reasonable to say at this point that this device will at some point be in continuous operation for, say, five years? How many potential gremlins will have to lie dormant for that long? Hundreds? Thousands? More than a million? Has there ever been a single project in human history that has run so smoothly for so long?

   Admittedly, I'm being very pessimistic. I'm sure that these issues have been discussed, and indeed have been surmounted in other colliders. But the LHC is over 4 times larger than the next largest particle accelerator. By my offhanded reckoning, with the same instruments, wouldn't that mean that it is only likely to run for a quarter the length without failure. Have our instruments become four times more reliable in the meantime?

   The cooldown time is not so big a problem as the fact that almost every component in the machine is a single point of failure. I honestly don't think that humans can reliably operate such delicate devices on such a large scale.

2. **Peter Orland**
   July 16, 2009

   OMF,

   I don't think we should be so pessimistic. Yes, it is a complicated project, hence many unanticipated delays, screw-ups, etc. The people who built this machine are smart and dedicated, however, and the set-backs don't imply it will never work.

3. **Charles**
   July 16, 2009

   Obviously the previous poster decided that his own personal paranoia is important to the rest of the readers here, and also to the running of the LHC.

   Amyway, since some of the sectors to be warmed up because of the leaks can then also be checked for bad splices, the reliability of the whole machine should increase considerably.

   Charles

4. **Greg Sivco**
   July 16, 2009

   Whoa, man, " the fact that almost every component in the machine is a single point of failure."? That's a bit much, but I do hear you that there is an outside chance (very small, I hope) that the damned thing will never start up.

   The fact straight up though regarding that is: we don't know. Experimental Physics, Applied Ahysics, and Engineering are extremely well-defined fields. I say let's give the Euros in those fields a break, and hope, rather reasonably hope, they slay their dragons and get the sucker up and running, later if not sooner. We really don't want the machine to go the way of the SSC, right?

   In the meantime … there much work to be done. We have a sweet opening between the death (or dimishment) of String Theory, and the LLC producing particles that raise more questions that answer.

   Peter is working on BRST Cohomology. Good for you, Peter. Go for the gold.

5. **Peter Woit**
   July 16, 2009

OMF,

I think it’s way too early to start worrying. In a huge, complicated project like this, you’re going to run into all sorts of problems. So far, they’re all things that are readily fixable, it just takes time (except maybe the magnets not getting to 7 TeV, it’s possible the machine will end up being a 6-6.5 TeV machine). Getting the LHC up and running, and working reliably at design luminosity may very well end up taking a few more years. The publicity campaign that encouraged people to think the machine was going to be working and producing (safe) black holes any day now may have been a mistake. Realistically, this is a process that takes a lot of patience.

6. **Sorinis**
   July 16, 2009

   We all apreciate optimism. But this project is as dead as a can of spam. Notice too that the latest delay (in a long series to come) is getting leaked now after Heuer announced that his new budget was aproved. Yes it is huge and complicated. But it is years over schedule, it will not run at design energy for the foreseeable future . . . and there is no credible start date at any energy. You can define failure more acurately.

7. **Loco**
   July 16, 2009

   Sorinis and greg, you’re so pessimistic it’s hard not to take it as personal paranoia. As PW himself stated (and you don’t think PW is especially naive don’t you?) you’re going to run into all sorts of problems when you build a new collider. “So far, they’re all things that are readily fixable, it just takes time.”

   An extra 5 years before full luminosity at 7 Tev is unlikely but would not be so surprising (remember the now glorious Tevatron?), and it doesn’t matter. You’re talking about a 40 years project. Who cares if it takes 45?

   Anyway don’t worry, don’t change anything in your attitude. That explains a lot about why the LHC is not in the USA and why the SSC went the way it did.

8. **Shantanu**
   July 16, 2009

   Loco, the basic point made by Sorinis and Greg is correct. If LHC is delayed,then people will start losing interest (esp. grad. students/postdocs) and will gravitate towards particle astrophysics/cosmology/gravitational waves. In fact AFAIK the top cited experimental HEP paper is not aparticle physics experiment, but WMAP. In fact in the last 8 months or so most hep-ph papers seem to be PAMELA rather than TEVATRON or LHC
   (of course this is anecdotal and I have not done a count).

9. **Sorinis**
   July 17, 2009

   I think the tevatron is due to announce some higgs results towards the end of august. Peter, are you covering htat?

10. **Mikael**
    July 17, 2009

    OMF,

    your analysis is very superficial.

    A vacuum leak is a trivial problem which has been overcome during the last cool down. Remember, the machine was already completely ready once proving you wrong.

    The bad splices are annoying because completely fixing them would require a complete warm up of the machine. They just don’t want to do it now just because they want the machine be running now so everything can be tested including the detectors.

    Once the bade splices are fixed, they are just fixed and do not interfere with anything.

    There may be a couple of other problems ahead, but their number is surely finite.

11. **Loco**
    July 17, 2009

    Shantanu, I’d say you have a generous way of extracting “the basic point” made by some folks here.

12. **Nobody**
    July 17, 2009

    Sorry Mikael,
    we all saw one week after what it was ready for. And please do not talk of bad luck; in the past they had a similar accident on a test bench, but it looks like the potential consequence of it on the final machine was totally neglected.

13. **Peter Woit**
    July 17, 2009

    Sorinis,

    If the Tevatron has new results on the Higgs this summer, I’m sure I’ll post about them when they come out. If someone at CDF or D0 has preliminary results now they’d like to share, feel free to e-mail me…

14. **Greg Bishop**
    July 17, 2009

    Shantanu, who cares if some marginal hangers on are discouraged that experimental physics is hard? That’s a good thing. It leaves more money for those that actually care.

    Certainly the string guys typing arbitrary ‘science free’ formulas (IMHO) into computers could use some company on World of Warcraft. That’s (probably) the more useful activity they perform for society, after all the Orcs aren’t going to defeat themselves.

15. **Shantanu**

July 17, 2009

Greg, one counter-example to your point is "Gravity Probe B" which was launched after almost 40 years. Are you really looking forward to their results?
Also if an experiment keeps getting delayed
it risks not getting funded (rather than the opposite way) (another example been the DUMAND experiment

16. **student**
July 18, 2009

Shantanu,

As a student on ATLAS who is waiting for data to come in to complete a thesis analysis, I think you have a point. However, to assume that the delays of the LHC are driving students and post-docs to different fields is a bit of a stretch in my opinion. Some American students have switched to Tevatron experiments recently (as they're nearing the end of their degree and simply can't wait any longer for data to come in, and unlike they're European counterparts, aren't allowed to graduate on a Monte Carlo analysis), but I've yet to hear of a single person moving entirely away from collider experiments, simply because they're discouraged by the delays.

17. **Shantanu**
July 18, 2009

student, if such delays continue then obviously you will see more people
migrating away from collider/accelerator physics. as I mentioned WMAP
papers have more HEP citations than any particle physics experiment.

18. **Pessimist**
July 20, 2009

The migration to astrophysics seems unlikely by students already in the pipeline; however, we might have seen an exodus to the Tevatron had the real extent of the damage been made public earlier. Recall that the "He leak" was to be fixed, and LHC ready for beam in "early spring". Now we see a steady dribble of two week schedule slippages.
To compound the misery, the detectors have been closed and are going through periods of 24 hour shifts to be ready for beam in summer ….fall … winter …..
We all know this is a difficult undertaking, but the management of information is starting to have a real effect on the participants lives.

19. **Sorinis**
July 21, 2009

Yes Peter, I'll email you what I've got when I have it. But only if you stop erasing my rants about the LHC. 🙂 That is the deal with Satan. I had such high hopes for it and the europeans just ruined everything.

20. **Coin**
July 22, 2009

> people will start losing interest (esp. grad. students/postdocs) and will gravitate towards particle astrophysics/cosmology/gravitational waves

Do grad students and postdocs really have the ability to so completely shift their research focus within the timeframe that the final burst of LHC delays has covered (1-3 years)?

…and if some people did shift their focus as you describe… would that even be a bad thing? There's some really important stuff happening in astrophysics and cosmology.

21. **Hal**
July 28, 2009

Don't forget the paper that argues that the LHC will never be operated (at close to full luminosity) due to a time travel cancellation effect from the future.

http://www.fqxi.org/community/forum/topic/230

Every time I read about new delays and new problems I wonder if the idea might just be crazy enough.

22. **nigel**
August 5, 2009

Are you unable to log on from China due to internet filtering? Hope you start blogging again soon.

23. **Peter Woit**
August 5, 2009

nigel,

I did have internet access periodically in China, no posting just because I was on vacation… Internet filtering did keep me from following Lubos's blog.

Weinberg at CERN

July 17, 2009
Categories: Uncategorized

Steven Weinberg was visiting CERN recently and gave a talk entitled The Quantum Theory of Fields: Effective or Fundamental? He discussed the ups and downs of the "market price" of quantum field theory, showing a decline since a peak in 1984, followed by a conjectured increase in the future. He also described the history of his work that led to the modern point of view on the role of QFT as an effective theory.

He ended with comments on the "asymptotic safety" approach to quantum gravity, noting that it is quite possible that string theory is not needed, that the world can just be described at a fundamental level by quantum field theory (and thus his conjecture that QFT may come back into fashion as a fundamental theory):

> I don't want to discourage string theorists, but there's just the possibility that maybe that isn't the way the world is, that the world is much more like we've always known, that is, the Standard Model and General Relativity.

Update: Weinberg has a new paper on the arXiv, covering much the same material as this talk.

Comments

1. **Charles**
   July 17, 2009

   The talk is somewhat confused. If the world is really described by the standard model plus general relativity, why do we need to discuss asymptotic safety at all? This is not made clear by Weinberg.

2. **Peter Woit**
   July 17, 2009

   Charles,

   This is all about the renormalizability problem of gravity. The asymptotic safety idea allows you to make sense of GR as a quantum theory, despite the renormalizability problem. It's a matter of some controversy whether this actually works and does what one would like.

   My own interpretation of what Weinberg is saying (which he might not agree with…) is something like: "unifying the SM and quantum gravity via string theory may very well be the way to go, but given that this hasn't been a success so far, it's a good idea to keep in mind that there's another possibility: QFT by itself might be all that is needed, with asymptotic safety one way the gravity renormalizability problem could get solved".

3. **V_NO**
   July 17, 2009

   Lubos Motl wrote interesting comment on this talk, pointing out that quantum gravity as local quantum field theory is not compatible with the results of black hole thermodynamics. Are there any loopholes to solve this problem?

4. **Peter Woit**
   July 17, 2009

   V_NO,

   What Weinberg is talking about is the standard claim for string theory that it is needed to deal with the perturbative renormalizability problems of quantum gravity. That may not be true, either because of asymptotic safety, or also because of possible perturbative finiteness of some supergravity theories.

   Non-perturbatively, you don't really even know what string theory is, I don't see how one can claim it's the only possible way to deal with non-perturbative problems in quantum gravity. The problem is more complicated than just invoking black hole thermodynamics, which was originally discovered as a semi-classical phenomenon in QFT.

   Sorry, but the Lubos rants on this subject tend to be more ideology than science. It's quite clear that he would equally vociferously make the opposite argument if he felt that was the "pro-string theory" position.

5. **Peter Woit**
   July 17, 2009

   Getting ready to get on a plane to China. I'm not going to be able to deal with comments here for a while, so will shut off comments for now.

   If you want to discuss Lubos's claims that Weinberg is wrong on this point of QFT, you can do that at his blog.

Back

August 5, 2009
Categories: Experimental HEP News, Uncategorized

I've spent most of the last month traveling, first to Latvia and Russia, then to China, finally to Seattle. Back now, looking forward to staying in one time zone and not seeing the interior of a plane for a while.

In China I visited Shanghai, Suzhou, Hangzhou, Yellow Mountain, Hong Kong and Macao, all of which was an amazing experience (thanks to John Baez for, among other things, urging me to search out the few remaining sections of old Shanghai). The weather unfortunately was less than ideal, with record heat in Shanghai, rain at Yellow Mountain, and clouds the day of the eclipse. Still, it was a lot of fun to be in People's Square and see the city go dark for 6 minutes. Here's one view from about that moment:

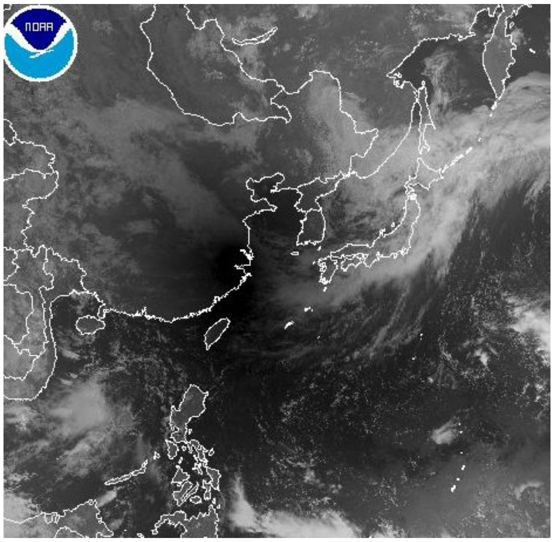

After getting back to New York from China, I turned around and went out to Seattle to attend my friend Nathan Myhrvold's surprise 50th birthday party. This was held at the new lab of his company, Intellectual Ventures, and among the organizers were Bill Gates and Lowell Wood. In attendance were many of the luminaries of the technology and culinary worlds, with Wylie Dufresne of New York's WD-50 one of several chefs who came in to attend the party and serve amazing food to the guests. Not the sort of party I normally attend…

Regular blogging will resume imminently. Things seem to have been rather quiet the past couple weeks anyway. The news of delays at the LHC reported here earlier has been getting more media attention. There's a very good article about the LHC problems by Adrian Cho at Science here, and the New York Times ran a front-page story yesterday. For some reason, the Times decided that it was important to quote what prominent theorists have to say about this, including:

> "I've waited 15 years," said Nima Arkani-Hamed, a leading particle theorist at the Institute for Advanced Study in Princeton. "I want it to get up running. We can't tolerate another disaster. It has to run smoothly from now."

Gordon Watts has some comments about this here (including pointing out that "running smoothly from now" is probably not in the cards), and there's also a good posting at Resonaances.

My understanding is that the LMC (LHC Machine Committee) was meeting today to go over all that is known about the splices problem and discuss the question of what the highest energy is at which the machine can safely run in its current state. A smaller group of people, in consultation with the experiments and the director, will then have to decide either to run at that energy, or accept further delays for repairs to allow running at a higher energy. It's not known how long that decision will take, but presumably it will come soon. If no further repairs are to be made, the current schedule has the machine ready for injection of a beam in mid-November.

Update: It's 3.5 TeV/beam.

Comments

1. **Chris W.**
   August 6, 2009

   Welcome back!

   (Are you going to re-open comments on the previous post ["Weinberg at CERN"] or just let that sleeping dog lie?)

2. **Haelfix**
   August 6, 2009

   Yea the last post does deserve some scrutiny, b/c its interesting and there are some good arguments against that conclusion.

3. **Peter Woit**
   August 6, 2009

   I think I'll just leave that one closed. This particular topic in quantum gravity has been discussed repeatedly on many blogs, the likelihood of anyone having anything new and enlightening to contribute on the subject seems small, and Lubos has provided a venue to argue with Weinberg about this.

4. **sorinis**
   August 6, 2009

When is the 3.5 TeV happening? In November?

5. **woit**
August 6, 2009

 Current schedule is to try and inject a beam at low energy mid-November, with about a month required to get usable colliding beams at 3.5 TeV. But they may be shutting down for a while over Christmas/New Years…

6. **Geometrick**
August 6, 2009

 Is 3.5 TeV in the range for supersymmetry? When will we finally get some hard evidence of the existence or non-existence of SUSY?

7. **dir**
August 7, 2009

 you missed beijing?

8. **Peter Woit**
August 7, 2009

 dir,

 I figured it best to leave Beijing for my next trip to China….

9. **graviton383**
August 7, 2009

 With this reduced center of mass energy and the relatively low integrated luminosity that is expected, it may take some reasonable time to generate sufficient SUSY signals
 beyond the bounds that already exist from the Tevatron. However, the real issue is not that but how long it will be for the detectors to be understood well enough to make any new physics claims.. if the new physics is at all subtle as would be the case for general SUSY.

10. **dir**
August 7, 2009

 peter,
 if you come to beijing, let me know. i'm an expert in food in beijing.

11. **ESPOL**
August 24, 2009

 The power of the storms always cause us to wonder, nature always reminds us that we are small and limited

12. **Shantanu**
September 3, 2009

 Peter, some other summer conferences/schools
 o NEw England Particle Physics summer retreat
 http://physics.bu.edu/NEPPSR
 (see esp. the talk on physics beyond standard model)

 o Abhay fest
 http://gravity.psu.edu/events/abhayfest/
 Maybe you could report if you found anything interesting in these meetings

Various Items

August 6, 2009
Categories: Uncategorized

- CERN just issued a press release announcing the decision about the energy for the initial LHC run: 3.5 TeV/beam.

> The procedure for the 2009 start-up will be to inject and capture beams in each direction, take collision data for a few shifts at the injection energy, and then commission the ramp to higher energy. The first high-energy data should be collected a few weeks after the first beam of 2009 is injected. The LHC will run at 3.5 TeV per beam until a significant data sample has been collected and the operations team has gained experience in running the machine. Thereafter, with the benefit of that experience, the energy will be taken towards 5 TeV per beam. At the end of 2010, the LHC will be run with lead ions for the first time. After that, the LHC will shut down and work will begin on moving the machine towards 7 TeV per beam.

- The Lost University will be running two courses on physics, starting September 22. The more advanced one will be taught by Jeremy Davies, based on Lynne McTaggart's book The Field:The Quest for the Secret Force of the Universe

> This book cites scientific experiments that examine the Zero Point Field, which is believed by some to be a universal energy source that connects everything.

The other physics course will be taught by Sean Carroll, Clifford Johnson and Nick Warner. It's entitled Introductory Physics of Time Travel and will be the prerequisite for a later course on "Advanced Physics of Time Travel". One of the texts will be Sean Carroll's forthcoming book.

More about this from Clifford Johnson here.

- The Anacapa Society, an organization aimed at promoting theoretical physics research at undergraduate institutions, has taken up permanent residence at Amherst, more here.
- Tommaso Dorigo is trying to stir up trouble again, this time by pointing out the intriguing fact that, looking at events with just electron-positron pairs, both D0 and CDF have seen more than expected at an invariant mass around 720 GeV. Not that this is statistically significant or anything…
- Talks from the recent FQXI conference in the Azores are available here.
- Talks from the on-going Newton Institute workshop on Non-Abelian Fundamental Groups in Arithmetic Geometry are available here. Commentary from Jordan Ellenberg about Deligne's talk is here. Jordan notes that:

> When I was first giving public lectures, someone gave me the hoary advice that I should quell nervousness by imagining the members of the audience in their underwear. Strange to think that, in this new broadband world, *most of them actually are*.

Comments

1. **spear Mark the Second**
   August 6, 2009

   We're finding out the LHC was pushed a bit far… too much reach on not enough money. An honored tradition, in its way… the original main ring at FNAL, the SLC at SLAC, and pretty much the whole CESR/CLEO program at Cornell. They all caught up by using operating funds to complete the job.

   CERN historically has gotten the resources for the reach they proposed. But now there is no competition, so why not? The only question is whether the public will turn against the LHC because the hype got a bit out of control last year. If Conan O'Brien makes a joke out of a couple billion for contacts as bad as an american toaster, that will be bad.

2. **Shantanu**
   August 6, 2009

   Peter, welcome back. Any comments on the recent review on quantum gravity
   (esp. the section on string theory) posted by Thomas in which your book is cited?
   Also videos of Planck 09 conference are online.

3. **Peter Woit**
   August 6, 2009

   Shantanu,

   See the next posting…

25 Years On…

August 6, 2009
Categories: Uncategorized

Five years ago the 20th anniversary of the "First Superstring Revolution" was being celebrated, and I wrote some postings about this history (see here, here and here). This month is the quarter-century anniversary, and I haven't seen any evidence of anyone celebrating.

25 years later, it's pretty clear that the anomaly cancellation discovered by Green and Schwarz doesn't actually provide at all the kind of explanation of some aspects of the standard model that people got excited about back then. 1984 also saw the beginning of endlessly repeated and overhyped hand-waving arguments that string theory is needed to cure the perturbative ills of quantum gravity. It turns out that these arguments don't actually work either. The latest issue of Science has an article about recent discoveries showing that N=8 supergravity amplitudes are much better behaved than expected. Zvi Bern compares the widespread belief that string theory is needed to deal with perturbative divergences to the belief that ulcers are caused by spicy food. While you could make a plausible hand-waving argument for this, it turns out that the real culprit is a bacterium, not enchiladas. According to the article:

> The work doesn't disprove string theory, but it has string theorists backpedaling a bit in their criticism of quantum field theory. "At certain points, our understanding has been incomplete, and we may have said things that weren't right," says John Schwarz of the California Institute of Technology in Pasadena. "That being said, the fact is that we still need string theory."

Forced to concede that a quarter-century's worth of argument about perturbative amplitudes was wrong, Schwarz tries to shift ground by claiming that the QFT problems are non-perturbative. That may very well be, but until one actually has a viable non-perturbative version of string theory, it will be hard to argue that string theory is the only way to go.

A new review article about quantum gravity describes how things are 25 years after the revolution, with many if not most string theorists having given up hope that string theory has anything to say about unification:

> A personal anecdote might best convey the current state of affairs. Early in the spring 2007 semester my University of Florida colleague, Charles Thorn, began a seminar by announcing his belief that:
>
> *String theory is just a technique for summing the leading terms in the 1/N expansion of QCD.*
>
> After years of hearing more ambitious assessments this was so shocking that I checked to be sure I had understood correctly. Charles confirmed that I had; in his current view, the effort to regard superstrings as a fundamental theory of everything was a blind alley. Later that year I related Charles' pronouncement to string theory colleagues on three continents and solicited their own opinions. About half of them agreed with him, more often the younger people.

Update: One more, from Martha Stewart, Some Pearls of Wisdom on String Theory.

Comments

1. **Chris W.**
   August 6, 2009

   Richard Woodard's review article is quite a piece of work—106 pages, and very readable.

2. **Chris W.**
   August 6, 2009

   PS: From page 62:

   > The most recent results about N = 8 Supergravity [88] mean something is wrong with our thinking about what sorts of divergences can happen. No one knows how high the cancellations extend in N = 8 Supergravity, or if they might apply to more realistic models. We could be witnessing the start of a revolution. The latest progress on this subject is so new that there are no books or long review articles. I highly recommend the short article by Bern, Carrasco and Johansson [100].

3. **Mirko Tarollo**
   August 7, 2009

   In 1997, at Strings in Amsterdam, I happened to sit near Nathan Seiberg. He was extremely angry because Hawking had said that N=8 supergravity was a candidate theory of everything, on par with superstings. Seiberg was really extremely angry, and when I asked to explain, he stopped talking to me.

   The day after, Seiberg gave his talk. He was still angry. I have rarely seen a man so angry. He was a missionary, not a speaker. Strings were his religion. I wonder how he deals with all this at present.

4. **Chris Oakley**
   August 7, 2009

   Hawking was saying that N=8 Supergravity was the answer in 1980.

   It's just one of the things he does.

   There is no point in anyone getting angry about it.

5. **Daniel de França MTd2**
   August 7, 2009

   How could N=8 SUGRA be a theory of everything? How could it hold the particles of the SM, any papers on that?

6. **Peter Woit**
   August 7, 2009

   Daniel,

There’s a long literature from the early 80s about this, and the problems with it.

What’s more important is that the N=8 supergravity calculations show that the conventional arguments about the perturbative problems of gravity QFTs are just wrong. The source of these new cancellations is not known, they don’t seem to necessarily require N=8 supergravity.

7. **Aaron Bergman**
   August 7, 2009

   What’s more important is that the N=8 supergravity calculations show that the conventional arguments about the perturbative problems of gravity QFTs are just wrong. The source of these new cancellations is not known, they don’t seem to necessarily require N=8 supergravity.

   I think that goes a bit too far. For example, I don’t think anything is going to make the divergence in pure gravity go away.

8. **Peter Woit**
   August 7, 2009

   Aaron,

   I wasn’t saying anything about pure gravity. Again, the point here is just that the standard “only string theory can make QG perturbatively finite” argument appears to have fallen apart. It remains very unclear what is causing these cancellations, and thus for what class of theories they might give perturbative finiteness.

   For some relevant speculation, see section 9.3 of 0907.5418 where the authors (for the case of N=4 YM) write: “This perhaps suggests that SUSY is not playing a particularly crucial
   role in the story”. They find different helicities coming not from supersymmetry , but from different charts on a Grassmannian.

   If I were to indulge in unsupported speculation, it would be that there is some class of QFTs including both gauge theory and gravity for which divergence problems in perturbation theory go away, due to symmetries we do not yet understand. Our world is described by one of these, one that has these still mysterious symmetries (as well as others…).

9. **MathPhys**
   August 7, 2009

   In 1986, at ICTP, I heard Michael Green say

   “Mr Seiberg, look around you. The sky is blue. The birds are singing. Do you really believe you explain all this with E8XE8?”

   It was great.

10. **Eric Habegger**
    August 8, 2009

    Peter,

    You said:
    “If, I were to indulge in unsupported speculation, it would be that there is some class of QFTs including both gauge theory and gravity for which divergence problems in perturbation theory go away, due to symmetries we do not yet understand. Our world is described by one of these, one that has these still mysterious symmetries (as well as others…).”

    I have it on good authority that Martha Stewart agrees with you. Oh wait… I might be mistaking her with someone else.

11. **Yatima**
    August 8, 2009

    “What kind of baking sheet do you suggest using for cookies?”

    That should be reformulated!

    “What kind of branes do you suggest using for cookies?”

    Ok, while we are on the subject of rank sillyness and it’s Saturday, let’s have some popular culture:

    http://www.youtube.com/watch?v=63S3vacxI9c

    Look out at 7:30 for “Depending on what we find, we _might_ just disprove string theory – that would really make my day!”

12. **Hontas Farmer**
    August 9, 2009

    Peter: Have you by any chance read this paper by Robert Wald “The Formulation of Quantum Field Theory in Curved Spacetime.” http://arxiv.org/abs/0907.0416 ? In one of your earlier postings to which you have linked in this one, you wrote of U Chicago doing a press release which sang the praises of string theory. A sure sign of change would be what Wald writes in that paper. He describes in detail an algebraic approach to QFT in curved space time which reminds me very much of loop quantum gravity. As if he has thought of a general formalism which describes that employed by LQG and which could work for any other QFT.

    Yatima: I love the straight answer she gives to the shredded financial documents question. Like it doesn’t bother her at all.

13. **Haelfix**
    August 9, 2009

    Certainly the conjectured finitiness of N=8 Sugra is intriguing, and I suspect will be proven sometime soon with the twistor techniques that are hot in development.

    Otoh its somewhat strange to disparage string theory using this particular theory, as its well known that it contains extended objects (pbranes and so forth) in its spectrum.

    It really begs the question.

    I think you would need to find another finite field theory that includes gravity, which explicitly does not include string theory asymptotic states to make the claim robust.

14. **jpd**
    August 9, 2009

    sounds like you are adjusting to string theory not being unique

15. **Big Vlad**
    August 10, 2009

    Haelfix,

    I have heard these arguments about N=8 sugra's relation to string theory. Could you explain a bit more? You're saying that it is known that there are stable extended field configurations in the theory. This is cool, but so what? The basic entities in the theory are still fields, not quantized strings. So isn't Peter justified in claiming the latest developments are a blow to string theory?

16. **twisting the night away**
    August 10, 2009

    Big Vlad,

    All arguments about N=8 being a 'finite field theory' are about perturbation theory *around a flat background*.

    Now you need a solution to the classical field equations to start a well-defined perturbation theory, but there are more solutions out there than the trivial one. One class of this are pbrane solutions, which you should think of as very close cousins of instantons in Yang-Mills theory. These are regular saddlepoints of the action and should be included in the full theory somehow.

    Is this important from a physical point of view? Probably, but even in the technically much simpler Yang-Mills theory related questions are hard to answer.

    Now there is an embedding of N=8 into string theory (apparently). That is, the perturbation theories is claimed to match in a certain limit. Strings also have non-perturbative effects which are to an extent understood, especially for extremal black holes.

    If N=8 is finite, a hard-core string theorist might therefore claim that is due to the fact it is string theory anyway and strings provides information about the non-perturbative sector. Really proving this is very hard to do since the limit taken can lead to unwanted effects (in principle). So hard, that to my mind at the moment it is more a question of faith than anything else.

17. **Peter Woit**
    August 10, 2009

    Hontas Farmer,

    Yes I've looked at the Wald paper. What Wald is doing is quite interesting, but it doesn't involve quantizing the metric degrees of freedom. I have no idea what Wald thinks of string theory, no reason to believe that he had anything to do with the press release of five years ago (he's in a different research group, GR, not particle theory, than the string theorists).

18. **Hontas Farmer**
    August 10, 2009

    You are correct, in the paper he show no work of his own on quantum gravity. The formalism he describes could work as well for Quantum gravity, as it would for any other QFT. If I understood it correctly all one needs to do is provide a algebra of observables for their theory. No metric, or Hilbert space is specified or required. The observables could be the operators and algebra of LQG, or QED.

    I wouldn't call Wald's formulation a theory of everything, it's more like a theory of anything.

    Was he not the overall department head of physics at U Chicago five years ago? I assumed that if he was he would have had to sign off on a press release. Shows what little I know.

19. **Peter Woit**
    August 10, 2009

    Hontas Farmer,

    What Wald describes assumes the existence of a fixed background metric (unquantized). You can't use his formalism to quantize the metric.

    An assumption that a department chair would have to sign off on press releases I think assigns far more power to that position than it carries in most American research universities…

20. **chris**
    August 11, 2009

    Aaron,

    this is not going too far. there are even claims that classical gravity could eventually be renormalizable perturbatively. essentially the argument goes like this: you have divergencies at all loop levels, but their prefactors have relations that you can describe with a finite number of equations. so you need to introduce an infinite number of counterterms, but with a finite number of parameters giving relations between them. so in a classical sense, this theory is not renormalizable, but it would still be predictive since the number of parameters is finite.

    even the perturbative nonrenormalizability of QG is far from being settled (let alone the nonperturbative one). and this relatively recent discovery of bern about N=8 SUGRA cancellations is another strong hint that we don't understand any of this as well as we should.

21. **Tim vB**
    August 11, 2009

    Hello Peter, Hontas Farmer
    Sorry if I take the discussion too far here…
    QFT in curved spacetime has been a reasearch topic at least since the 1970'ties,

you just try to quantize fields in a fixed curved spacetime instead of Minkowski spacetime.
The idea is to generalize QFT a little bit to include classical gravity effects on fields while
neglecting effects of matter on gravity, i.e. on the spacetime metric tensor.
Since there are many approaches to this topic in Minkowski spacetime, you can ask, which ones work best
in this generalized setting, Robert Wald says that algebraic QFT is what you should use.
In algebraic QFT, the basic object you work with are causal nets of von Neumann algebras.
Have a look at "Quantum Field Theory in Curved Spacetime and Black Hole Thermodynamics" from him for this approach,
or at Birrell, Davies: "Quantum Fields in Curved Space" for approaches that most people trained in "classical" QFT
will probabliy find more familiar.
All of this has nothing to do with LQG or string theory, it is just an attempt to combine two well known
frameworks (QFT and GR) without changing one of them in any radical way, to see if anything could be learned from it.
The most famous results obtained here are the Unruh-Effect, Hawking-Radiation and the formula for black hole entropy.
What keeps irritating me is that some string theorists seam to treat these as established facts while mostly ignoring
the whole framwork and some key insights of QFT in curved spacetime, e.g. that the particle concept is highly problematic.
If anyone could point out a discussion of this topic ("what do string theorists think of QFT in curved spacetime") to me I would be very thankful.

22. **Guest**
August 11, 2009

"what do string theorists think of QFT in curved spacetime"
I think that a more relevant question would be "what do string theorists think of STRINGS in curved spacetime" and the short answer is "They think about it quite a lot and consider it to be a very important problem". Quantizing strings in AdS is one of the most outstanding problems, not only because the background metric is curved but also because of the presence of the background RR flux. One particular non-trivial background where strings were successfully quantized, i.e. the string spectrum was found and multiparticle transition amplitudes were computed using the CFT techniques, is the so-called pp-wave background. For a reference see this paper:
http://arxiv.org/abs/hep-th/0202109

23. **James**
August 12, 2009

Off topic: but it seems that cosmic variance has fallen out of favour amongst your physics weblog links?

24. **Peter Woit**
August 12, 2009

Thanks James,

That's very odd and kind of disturbing. It looks like a bunch of links have disappeared (also interactions.org and Lubos) and I have no idea why. Will try and fix later today…

25. **Tim vB**
August 13, 2009

Dear Guest,

thanks for the answer and the link, I will take a look at the paper.
What I mean by "QFT in curved spacetime" is taking GR as it stands, choose
a "physically realistic" spacetime in the sense of classical GR and put quantum fields on it.
"Physically realistic spacetime" in this context usually means a 4 dimensional causal (let's agree on globally hyperbolic)
classical spacetime. In this sense the moment you think about AdS (5 dimensional or AdS_5 × S_5 as in the paper you referenced), you are not doing "QFT in curved spacetime".
I do not wish to imply that AdS and quantizing strings in AdS is irrelevant, but can we agree that it is a totally different topic than
"QFT in curved spacetime" as understood here?
I would still be interested in an answer to my original question then…

26. **Guest**
August 13, 2009

To Tim vB, if you are interested in this topic, check out these arXiv papers: http://tinyurl.com/mdxobn

27. **James**
August 15, 2009

Re Links: some have been restored but, for example, Chad Orzel is still left out in the cold.

Incidentally, I'm not just trying to nitpick! As well as visiting your blog because I enjoy reading it, I use it as a jumping off point to other good blogs (except I can't if the links have vanished!)

Please restore order to my blogverse! 🙂

28. **Guido**
August 20, 2009

Peter wrote:" … the anomaly cancellation discovered by Green and Schwarz doesn't actually provide at all the kind of explanation of some aspects of the standard model … "

Once I made the mistake to email Distler for some details of the anomaly argument. Though I sent him a friendly mail, I got no answer, but a series of really unfriendly ad hominem remarks. He is clearly a person to avoid contact with, even if you want to talk about string theory.

29. **Tim vB**
August 20, 2009

Hi Guido,
please do not judge people by an episode like this, I am very glad that some string theorists like Jaques Distler

still care to discuss and explain basic topics online.

This Week's Hype

August 12, 2009
Categories: This Week's Hype

From Softpedia this week, the news is of a Universal Theory of the Universe in the Works. According to the article,

> The theory of quantum mechanics was devised around 1920, and explains all this, but without accounting for gravity. Therefore, uniting the two ideas has since been an effort taken on by a large number of physicists. Now, an international group believes it is closer than ever to finally managing a breakthrough.
>
> Professors A.A. Coley, from the Dalhousie University, in Halifax, G.W. Gibbons at the University of Cambridge, in the UK, and C.N. Pope at the Texas A&M University, in the United States, led by young mathematician Sigbjørn Hervik, at the University of Stavanger, in Norway, believe that string theory is the best option physics has at bringing the two together.

It's hard to tell what this is based on, but the only paper I see with those authors is this one, which doesn't really have much of anything to do with string theory.

The source of the Softpedia article is one from Science Daily entitled A Grand Idea About the Universal Universe that tells us that:

> A mathematician in Norway and international fellow scientists have now conceived a grand idea about the universal universe. They have developed a method that may provide answers to universal problems and characterize and describe the universe….
>
> "The problem is that quantum mechanics does not include gravity and the theory of relativity does not include quantum mechanics", Hervik says.
>
> Many attempts have been made to find a unifying theory of both. String theory is the best candidate so far, according to Hervik.

Ultimately this all goes back to yet another university press release, this one about Hervik's Universe.

Comments

1. **Bee**
   August 12, 2009

   Well, quantum mechanics doesn't "include" electrodynamics either. These two just happen to be compatible if done the right way.

2. **Vid**
   August 12, 2009

   "…which doesn't really have much of anything to do with string theory."

   Don't know about the rest, but where does the notion of quantum correction come from if not string theory?

3. **Justin**
   August 12, 2009

   On one hand, Hervik talks about string theory being the best candidate for unifying quantum mechanics and gravity, but, from what I can gather, what he's actually doing is looking for an alternative to string theory.

   In fact, if you read further in the ScienceDaily article, it looks like his work might be more in line with approaches like loop quantum gravity rather than string theory, in that he's looking for operators that produce a discrete spectrum of spactime:

   > "The idea is to construct curvature or projection operators that split geometry into small entities. It is a tool or a method based on mathematical formulas designed to find such operators."

4. **Peter Woit**
   August 12, 2009

   Vid,

   I think the notion of "quantum correction" comes from quantum mechanics, not string theory.

   From what I can tell, these authors are just looking at a class of solutions to a generalization of the Einstein equations. There's no indication in the paper of why this generalization is what you are supposed to get from a quantum gravity theory or from string theory in particular, so no good reason to refer to "quantum corrections". The idea seems to be to find things that are solutions to "almost any set of covariant equations involving the metric and its derivatives", so you have solutions to all conceivable quantum gravities, but it's not clear to me that that makes any sense.

   In the standard story about how string theory is supposed to give corrections to the Einstein equations, one normally is talking about six dimensional compactification spaces, and modified equations such that solutions are no longer quite Ricci flat. This paper is about 4d and something completely different.

   Justin,

   It's hard to tell from the article what Hervik is referring to, but I see no reason to believe it has anything to do with the discrete spectrum ideas from LQG.

5. **H-I-G-G-S**
   August 13, 2009

   Thank goodness you're back so you can protect us from string hype we wouldn't have otherwise noticed.

6. **a-n-o-n**
   August 13, 2009

   and Thank goodness H-I-G-G-S is back to provide the yang to the yin to keep me distracted during work..

7. **Tim vB**
August 14, 2009

Taken directly from the University press release:
"Naturally, it is personally very satisfactory for me to discover something new in the world of mathematics on a par with Columbus' discovery of America".
It would seem we need another item in the crackpot index:
20 points for comparing your discovery to Columbus' "discovery" of America, 5 points for any signs that you do not know that the Vikings "discovered" America some hundred years before Columbus.

8. **Chris Oakley**
August 14, 2009

Yeah, but the Vikings didn't do much about it. You know – a few Magic Mountain rides, Disneyworld and then they were back in their longboats for the return trip to Norway.

9. **capitalistimperialistpig**
August 14, 2009

If you don't want to become the math/physics version of late night comedy material, think twice (thrice -etc) before you talk to your schools press office about your latest brilliant idea – at least not until you collect a Nobel or two!

10. **Tom Whicker**
August 14, 2009

Hervik points out the great adaptability of string theory:

"What we can do, however, is to consider the problem from another angle as if we did not know what the theory could be. We can describe phenomena, for example the universe, as a consequence of the unknown theory, in spite of the fact that we do not know what the exact theory is", Hervik explains.

11. **Sebastian Thaler**
August 15, 2009

Peter,

The new book STRING THEORY FOR DUMMIES, which is being released in November, includes a list of "Ten String Theory Skeptics". You are included, along with Richard Feynman, Lee Smolin, Robert B. Laughlin, Roger Penrose, Lawrence Krauss, Sheldon Glashow, John Moffat, Andreas Albrecht, and Joao Magueijo.

12. **G**
August 17, 2009

Embrace the duality.

13. **John A**
August 18, 2009

"Embrace the duality"

Then there'll be two books?

"The new book STRING THEORY FOR DUMMIES, which is being released in November, includes a list of "Ten String Theory Skeptics"….

I've operated on the assumption for a while that "String Theory for Dummies" is rather like "Sexual intercourse for Eunuchs", that is, if it were possible to explain string theory to actual dummies, then it wouldn't be string theory or they wouldn't really be dummies.

14. **Chris Oakley**
August 18, 2009

It could be dummy in the sense of a life-size sex toy, and the book is an instruction manual for turning one into a string theorist for those kinky enough to want that.

15. **Tim vB**
August 18, 2009

There already is a book in the same spirit, "String Theory Demystified" by D. McMahon, and yes, it works quite well for everyone
who had problems with the introductory quantum field course or needs a book one can hold with one hand only
(because the other hand has to hold that cocktail while you sit at the pool) but nevertheless would like to learn a bit about string theory.
But what is the point in listing sceptics (or fans) ?
Of course you could try to weigh the most prominent 100 sceptics and fans (like in Lev Landau's logarithmic classification) of string theory and compute the likelihood of the theory to be true…
BTW: Most people find books with a QED title more impressive than string theory, so maybe you would rather like to read some of these at the pool.
If you are unlucky the person you try to impress is a software developer and thinks you didn't get the concept of a string variable when you failed to learn to program BASIC in highschool, and that is not funny.

Various and Sundry

August 20, 2009
Categories: Uncategorized

Back from a final short summer vacation, with no further travel plans for the indefinite future. Some things I've recently come across that might be of interest:

- Tommaso Dorigo has posted his contribution to a session on "Blogs, big physics and breaking news" held at last month's World Conference of Science Journalists in London. There's a recording of the session available here. Besides Tommaso, one speaker was Matthew Chalmers, who talked a bit about the "String Wars", including the role of blogs in it. The last speaker was CERN's James Gillies who discussed CERN's efforts to do a better job of putting out information about progress on the LHC project, under some pressure from the phenomenon of others disseminating such information if they don't…. They've done a much better job of this recently, putting out informative press releases almost immediately after major decisions are taken. I'm glad to hear that he finds the role of blogs to have been a positive one.

For a recent LHC update, see these slides from a talk at the Lepton-Photon Symposium. On page 35 there's a copy of the latest detailed schedule that I've seen, which one can compare to the continuing updates on progress here.

- Also at Lepton-Photon, here's a talk by Shamit Kachru about using AdS/CFT to build technicolor-type models of electroweak symmetry breaking that involve strongly coupled gauge theories. He and his wife Eva Silverstein will be leaving Stanford and joining the KITP in Santa Barbara this fall, see the press release here.

For lots more about the KITP, its programs and its finances, see this presentation by David Gross to the NSF.

- I see there's an interesting sounding workshop at the Fields Institute this fall, but it scares me to see that it is described as a celebration of Allen Knutson's 40th birthday. I seem to have gotten old very quickly, with conferences now devoted not only to people younger than me, but to people much younger than me that I recall meeting when they were just starting graduate school…
- My nomination for the all-time highest quality discussion ever held in a blog comment section goes to the comments on this posting at Secret Blogging Seminar, where several of the best (relatively)-young algebraic geometers in the business discuss the foundations of the subject and how it should be taught.
- There's a long and well-informed article here on the multiverse, bringing together the "What the Bleep" crowd, mainstream physicists, theologians, and the logo of the Stanford Institute for Theoretical Physics (the one Kachru and Silverstein are escaping from).
- For a selection of the latest in cutting-edge applications of new internet technology related to physics, there's Gordon Watts with his Deeptalk, the nLab site of the n-category cafe, and the Twitter feed of Cosmic Variance.

Comments

1. **M**
   August 21, 2009

   As string theorists like to say, String Theory is a conceptual framework that unifies all good ideas, like supersymmetry, extra dimensions and now theology. In the multidisciplinary conference you pointed out, theology gave a decisive contribution to the string swampland program, pointing out a common feature of all string vacua: "the multiverse would not contain morally unacceptable worlds, such as those in which evil significantly predominated over good"

2. **Chris Oakley**
   August 21, 2009

   What about the Land of Oz, where, until Dorothy arrives, the Munchkins are ruled by the Wicked Witch of the East?

3. **Ben Webster**
   August 21, 2009

   I wouldn't read too much into the Knutson conference. Mostly it's just that he has a lot students who are wags.

4. **Tim vB**
   August 21, 2009

   Hi Chris,
   the evil witch of the east was killed and the evil witch of the west was defeated in the end, right? So that is a conclusive proof of the statement.
   The "multiverse" idea is too embarrassing and too easy to make fun of, so I will try to refrain from doing that.
   But it would be nice to have a careful explanation of some of the errors in the "Pluralistic Universe" that one could refer to, namely:
   – It is 10 to the power of 500 string vacua, not 10500, does anyone know a popular article that get's that right?
   – The multiverse idea that originates from the string landscape has no connection whatsoever to the multiple universe interpretation of quantum mechanics.
   – To be a Boltzman brain does not imply that one has to be a solipsist, or the very existence of philosophers that are not solipsists would prove that they are not Boltzman brains.
   – What does it mean to prove the existence of the multiverse? How could one do that?
   I would devote the rest of my life to try to un-cook an egg if that could prove the mutliverse idea to be wrong, but I suppose any success would just prove the multiverse idea to be true:"Hey see, we live in one of the universes that allows that kind of thing, what a triumph for the theory".

5. **Bob**
   August 21, 2009

   Surely Gross did not present all 180 slides to the NSF? Inflicting death by stupefaction is a guaranteed way to make sure KITP will never get another dime!

   LOL

6. **Peter Woit**
   August 21, 2009

   Thanks Ben,

   I feel better already….

7. **E. Silverstein**
   August 21, 2009

Dear Peter,

You write that Shamit and I are "escaping" from the Stanford Institute of Theoretical Physics, but nothing could be further from the truth.

The SITP is a phenomenal group of physicists. These are people who played a leading role in brining us early universe inflation, phenomenology beyond the Standard model, and holography and string theory. Together we have had tremendous fun and real synergy, for example developing primordial cosmology and its potential connections with string theory, among many other topics. We also treasure our interactions with students and postdocs, and the open, broad, and ambitious style of research in the group, as well as its unique sense of humor. In short, we have been delighted to be part of SITP and SLAC theory as well as the broader departments and university.

KITP/UCSB is also a spectacular, unique institute with great physicists who we deeply admire, as reported at length in the press release you cite, and we indeed decided to try that amazing new experience.

Best wishes, Eva Silverstein

8. **Peter Woit**
   August 21, 2009

   Eva,

   Sorry for the inaccurate implication, that was just my own (possibly defective) sense of humor at work. Good luck with the move to Santa Barbara!

   Peter

9. **Allen Knutson**
   August 21, 2009

   he has a lot of students who are wags.

   Surely that counts for something.

Percontations

August 24, 2009
Categories: Multiverse Mania

The Templeton Foundation has recently been sponsoring a series of Bloggingheads diavlogs, under the name Percontations. This week's episode is Fiddling With the Knobs of the Universe, and it has cosmologist Anthony Aguirre and string theorist Clifford Johnson doing their best to hype string theory and the landscape. Critics are dismissed as people who believe obviously wrong things like "if it's statistical it's not science".

Johnson argues that string theory landscape research is just like any other kind of science, capable of making testable statistical predictions, predictions based on generic properties of the theory (e.g. T-duality), and predictions of some parameters based upon fixing others by observation. He neglects to mention that decades of work by people trying to do such things have shown that there are very solid reasons why they don't work. Not only have no predictions come out of this, but the reasons why have become clear.

While hyping the landscape, he acknowledges that string theory has had a problem with hype in the past. "We all bought into it to some extent" that string theory was going to give the Standard Model, and it was bad that this was promoted in the press as a polished, definitive story of how the world works. He claims to be happy that this has been backed away from in the last several years (although he never seems to have been happy about the existence of string theory critics who have raised the issue of the problems publicly).

In a recent posting, Johnson partially resolves a mystery I'd always wondered about, that of why he left Cosmic Variance. He explains that one reason was that Cosmic Variance was taking "the obnoxious route of calling someone an idiot or stupid for their religious beliefs at the outset." Bloggingheads has recently featured Sean Carroll and Mark Trodden of Cosmic Variance also discussing cosmology and the multiverse (see here and here), but these episodes were not sponsored by Templeton.

Comments

1. **NonScientist**
   August 24, 2009

   "He neglects to mention that decades of work by people trying to do such things have shown that there are very solid reasons why they don't work. Not only have no predictions come out of this, but the reasons why have become clear."

   This may be an omission, but I don't think it's a fault. How many theories or other scientific discoveries have come to light after such a small quantum of time as decades were not fruitful?

   Don't confuse this as a slight to, or promotion of string theory. I'm more speaking about Johnson's desire to promote his research.

2. **Marcus**
   August 24, 2009

   Was "Bloggingheads diavlogs"
   a diavolical Sleudian Frip?

3. **Peter Woit**
   August 24, 2009

   No Marcus, "diavlog" is the word the bloggingheads people came up with to describe these things.

   NonScientist,

   I think promoting one's research by going to the public and making an argument for it that neglects to mention there are well-known and well-understood reasons why this argument doesn't work is not just an omission, but a fault.

4. **Phil**
   August 24, 2009

   Marcus,

   Diavlog is a second-order blend, by the way: it blends dialog and vlog, with the latter element representing a blend of video and blog. Or make that third-order, since blog blends Web and log.

   http://languagelog.ldc.upenn.edu/nll/?p=1684

5. **Marcus**
   August 24, 2009

   thutch

   that means thanks very much.

6. **M**
   August 25, 2009

   I would say that the main problem was not the hype, but that hyping the Unique Theory with the Unique Vacuum, string theorists collectively misunderstood the physics of strings. Being very smart in formal stuff and doing good physics are two different things.

7. **alex**
   August 25, 2009

   lapimacuote

   that means the last paragraph is missing a closing quote (for the one that opens "the obnoxious…)

8. **Peter Woit**
August 25, 2009

Thanks alex, fixed.

I'm learning all sorts of new words….

9. **Tim vB**
August 25, 2009

Just listened to the "Fiddling With the Knobs of the Universe" episode.
Another interesting remark of Clifford Johnson is that one should not expect anymore that string theory will explain all input parameters of the standard model (that would mean that one's concept of what a scientific theory should be like or could do for you is too narrow), or that string theory will be a theory of everything (because it is no longer clear what that would even mean).
Seems to be quite a strategic withdrawal.

10. **per**
August 25, 2009

Well, I think Clifford deserves credit if he left Cosmicvariance for the reasons states in his post. Starting out a discussion with arrogance usually does not lead down a fruitful path (which I am sure you Peter are aware of after all the pro / con string theory discussions)

P

11. **Peter Woit**
August 25, 2009

Tim,

At this point, I think just about all string theorists think it unlikely that "string theory will explain all input parameters of the standard model".

But this is really a misleading way of putting things. All evidence is that if the landscape picture is correct, string theory not only can't explain "all" parameters of the standard model, but it can't explain a single one. It's completely vacuous, exactly what you would expect of a wrong idea.

In dealing with the public, string theory proponents often use this misleading tactic, referring to string theory's problems with predicting everything, when the problem is that it predicts nothing.

12. **Tim vB**
August 25, 2009

Hello Peter,
agreed, Clifford uses a very interesting multiple tactics approach that clearly shows that he has learned some politics during the string wars:
– repetition of well known hype arguments ("strings are beautiful, string theory is the only game in town"),
– references to tremendous successes of the theory and of (verified?) predictions (maybe I missed the point but I think he does not explain which ones),
– a nearly complete retreat from the original research program ("you cannot expect that string theory has to explain anything, e.g. the electron mass, and fits on a T-shirt", "string theory is just a tool that can be of use in many different contexts"),
– and the promise that the string theory hive is still open to new ideas, as any scientists should be ("as soon as something more promising shows up, we will stop the string theory business and think about that").
I've always wondered if the "only game in town" is some sort of insider joke, because according to the original anecdote the player that plays that game loses all the time…

13. **DB**
August 25, 2009

Propaganda was a major weapon in the arsenal of String Theorists during their period of advance, as they acquired funding and tenured positions. It's just as important during their retreat, to preserve funding and protect the careers of postdocs and non-tenured staff as far as possible.
They are masters of cynical media manipulation and spin.
The long-term damage to the discipline of theoretical physics is hard to quantify but is substantial.

14. **Nigel Cook**
August 25, 2009

I think that the string theorists are able to ride over critics easily because they have got the big advantage of a critical mass. They have each other to discuss ideas with. Whenever you have a group of people all enthusiastic about something, they can't resist talking the easy road to ignore critics.

E.g., the 'only game in town' claim is infuriating because it's untrue. But when you point out that it's untrue, they always (without any shame) shift to claiming that it's the 'best claim in town' by arguing that it's had more people, money, and time spent working on it than all other speculative ideas.

15. **Andrew Zimmerman Jones**
August 25, 2009

When I was researching my upcoming book, String Theory for Dummies, I found the landscape to be one of the hardest things to deal with logically. I finally used an analogy of the "texas sharpshooter" – he shoots at a wall and then draws the target around where the bullets landed. You basically look around where you're at and then redefine that position as the goal of what you were working toward. This doesn't necessarily mean he's a bad shot, but it certainly makes him look like a far better shot than he is in reality.

16. **Chris W.**
August 25, 2009

Despite or rather because of its accuracy, I don't think string theorists will be too fond of that analogy, Andrew. Good luck…

17. **nbutsomebody**
August 26, 2009

Clifford is completely vague in what he is saying. The information content is zero but the comedic content is high. is it a stand up (phone up maybe) comedy !

"String theory or method of string theory" (humm subtle, a person of finer articulation)
"It is telling us to grow up" (have I heard it correct, hope not !)
"Interesting to regime of physics … blah blah" (zzz, sleepy),
String theory USEFUL (🙂)

well, what is going on?

Anyway such a bad performance by a string theorist is really concerning.

18. **chris**
August 26, 2009

Tim vB,

string theory indeed was tremendously successful recently. i actually think it is close to a breakthrough right now. give it just a few more years and you will see, that it is a fabulous phenomenological model for strongly coupled electron systems. and maybe – in some more distant future – it will come close to lattice gauge theory in describing the IR part of QCD.

19. **Tim vB**
August 26, 2009

allow me to be a bit naive and demagogic here: If you could replace the standard model lagrangian
with, say, the nambu-goto lagrangian of naive string theory with one free parameter
(e.g. the string tension), that would be beautiful. I would be very grateful if I would
not have to memorize the standard model lagrangian any more.
So let's assume for the moment that the impression "string theory is beautiful" stems from this observation.
If you give up the hope that string theory will ever explain resp. replace the standard model, why is it still beautiful?
This is one of the main points I do not get, I mean: Clifford cites many of the arguments pro string theory
that originated from the hope that the standard model could be explained by it, while (nearly) giving up all the hope that this could ever happen.

20. **it works!**
August 26, 2009

I don't know whether you guys have come across this claim, but one string theorist told me he did string theory "because it works." I don't know what he meant by that, but I kept quiet just for the sake of politeness….

21. **Sorinis**
August 26, 2009

http://cdsweb.cern.ch/journal/article?name=CERNBulletin&issue=35/2009&number=1&category=News%20Articles&ln=en

Here we go again . . . for the millionth time.

22. **Peter Woit**
August 26, 2009

Sorinis,

That's not really news, just a good explanation of the complexity of the LHC startup schedule.

Latest estimate I've seen has first beam injection around Nov. 18, with sector 67 the last one to be ready (its cooldown recently had to be interrupted to fix a short-circuit).

23. **Peter Orland**
August 26, 2009

Chris,

I have hear such promises before…

Right now string theory can describe strongly-coupled gauge theory (by the way, so can non-numerical lattice gauge theories). OK, fine, one can play with the idea and try some phenomenology. There are also stringy ideas for summing planar diagrams, and a lot of other interesting ideas.

BUT… no one can show that string theory methods gives the RIGHT strongly-coupled gauge theory, i.e. that obtained by integrating out all the short-distance degrees of freedom from QCD, starting near the UV-free fixed point. My impression is similar for approaches to condensed-matter physics.

Promising a breakthrough doesn't count as much as delivering one. Any good physicist who truly has a breakthrough in the works, would work on finishing it, and not making promises (that only spurs the competition).

24. **Marcus**
August 26, 2009

Peter Orland,
you took Chris's post seriously, as perhaps was proper, but to me it seemed humorous, somewhere on the scale between subtle and sidesplitting. He said:

"…string theory indeed was tremendously successful recently. i actually think it is close to a breakthrough right now. give it just a few more years and you will see, that it is a fabulous phenomenological model for strongly coupled electron systems. and maybe – in some more distant future – it will come close to lattice gauge theory in describing the IR part of QCD."

25. **Peter Orland**
August 26, 2009

Hi Marcus,

It could be that I'm the guy in the room who doesn't get the joke (especially if it's on him), but in this case, I suspect Chris was serious. I think he is expressing the feeling of a lot of younger people in the field. If I'm right about this, they need to hear that promises mean nothing (except in the public-relations world), and only results count.

Regards,
Peter O.

26. **onymous**
August 26, 2009

Peter Orland said:

BUT… no one can show that string theory methods gives the RIGHT strongly-coupled gauge theory, i.e. that obtained by integrating out all the short-distance degrees of freedom from QCD, starting near the UV-free fixed point.

Indeed, one knows that they do *not*, and the reason, amusingly, is their failure to be sufficiently stringy. Calculability in these models relies on a hierarchy between the AdS curvature scale and the string scale. On the field theory side this means a large gap in operator dimensions that does not exist in QCD. In principle one can imagine a model that gets closer to QCD — or even *is* QCD — but in such a model this hierarchy would not exist, and one would have to do string theory rather than effective field theory in the AdS background. So far this is an insurmountable technical obstacle. The AdS/QCD models remain compelling and amusing toy models, but everyone working on them should (and, for the most part, does) understand that they are, in many ways, toys.

As far as I can tell all the same objections apply to AdS/CMT. They might be valuable toy models that shed light on interesting phenomena, but they are unlikely to ever be models of real physical systems until the technical obstacle of doing string theory on highly curved spacetimes is overcome.

27. **TomInCalif**
August 26, 2009

Peter wrote: "… string theory proponents often use this misleading tactic, referring to string theory's problems with predicting everything, when the problem is that it predicts nothing… "

As a layman in this field, I have a question: Can the proponents even construct all the correct parameters of our actual universe, in a consistent way, just by twiddling whatever "free parameter knobs" exist in string "theory" ?

Or is even that challenge akin to recreating all the content of the Sunday NY Times, from a pile of burned ash?

28. **Greek**
August 26, 2009

Hi,
as a European practitioner in the field that you may call String Theory, it is amusing to see that the String Wars is – perhaps – more than an urban legend! I see that (some) serious people take it seriously and (sometimes) use serious arguments. So here is mine:

perhaps there is reason to be optimistic – whichever side of the trench one sees oneself. On the one hand, it is by now obvious to the honest physicists that the Scherk-Schwartz leap to connect the spin-2 string state to the graviton is probably misleading. It has lead to an enormous amount of work, much of which is of high quality both in physics and maths, however it has produced a "doomsday" religion regarding string theory: i.e. that string theory is a T.O.E. This nicely fit the old "particle" ideas regarding "effective field theory", which are based on the philosophical conceptions such as the "atomic" theory of the ancient Greeks! Unfortunately, both the ancient Greeks and the modern non-Greeks are proven – by experiment – wrong.

On the other hand, the honest physicist now observes that String Theory rather helps in the deconstruction of the old particle physics "effective field theory" philosophy! Indeed, "fundamental d.o.f" such as strings and (M)branes describe low energy phenomena in condensed matter. So what is "fundamental", the string or the superconducting vortex observed in the lab? But this is supporting the idea (see P. Anderson etc) of "emergent physics".

So, if the recent AdS/CMT ideas work – even a little bit – String Theory will close a circle and get back to where it started; to a model describing the "phenomenology" of matter!

Then, for one that is not a string-religion-fanatic, there is a clear winner in the String vs LQW war. It is String Theory of course – but viewed from this newer perspective. Indeed, many of the young generation view things that way – so in 10 years time when the old guard will retire the field will be completely different.

So, cheer up, indeed.

PS: Sorry for the long comment, I am completely new to blogging.

29. **Peter Woit**
August 26, 2009

Greek,

Thanks a lot for the interesting comment. I agree with what I take to be your perspective that a lot of the string theory community, especially the younger part, has given up on the idea of using string/M-theory in 10/11 dimensions to unify physics, and moved on to trying to do other things, typically related in one way or another to AdS/CFT duality.

That's fine, but to me there still remains the problem that, non-perturbatively, QFT still remains mysterious in many ways, often in ways that AdS/CFT has no relevance to. It would be nice if the giving up on one failed idea could lead to people being willing to investigate a variety of new ideas, not just one particular one that historically grew out of the TOE failure.

TomInCalif,

The problem is that the string theory machine used to construct "string theory backgrounds" that might look like our world turns out to have to be very complicated, without much calculational control. So, the state of the art is that as far as anyone can tell, in principle you can get our physics with the right parameters (and just about any parameters you want) out of such a "string theory background". But actually doing this in a precise and well-defined way is way beyond current calculational capabilities, and to me it has never been clear that these calculational problems are not problems of principle, not just practicality.

30. **Greek**
August 26, 2009

Hi Peter,

sure one should try new ideas, away from AdS/CFT. In fact, a particularly interesting clue has recently emerged in d=3. It seems that one needs to go beyond the usual group-based gauge-theory structures to understand three-dimensional theories. More complicated structures (based on 3-algebras) seem to be needed. Other ideas are welcome too!
e.g. keep the term "systolic geometry" for the time when it will be applied to critical systems and the black-hole entropy..

Again on the philosophical side (its very late here in the "old world" so I am allowed): this ear looks very much like the beginning of the 21s century when people would construct everything from machines -then came quantum mechanics. It also looks like the 70's when people were seeing UFOs everywhere and nuclear science would explain everything – then came quantum gravity to spoil the party. It seems that in the coming decade something else will emerge that will change our current views.. hopefully.

31. **Pawl**
August 26, 2009

Greek,

I am puzzled by your comment that String Theory "viewed from this newer perspective" is the winner in the "String vs LQW [sic] war." Do you mean LQG (Loop Quantum Gravity)?

If you mean that String Theory provides a more successful approach to quantum gravity, I don't really see, based on what you've written, why you would hold that view — much less how it could be presented as something more than an opinion. But perhaps I'm just not understanding your point.

32. **Greek**
August 26, 2009

Pawl,
my point was that "dreams of a quantum gravity" (to paraphrase Weinberg) is not such a big thing after all.. There is much more to physics than QG.i.e. turbulence to just name a cliche' example. Even if LQG provides the path to QG (which I believe is at least as doubtful as the opposite statement), it will not be relevant! On the other hand, ideas and maths that are spin-offs of String Theory touch upon much more physical structures (turbulence included!). That is why I think that the winner in String Theory. I hope I am clearer now.

33. **Pawl**
August 26, 2009

Greek,

Thanks; I understand you now.

I do not really agree that reconciling quantum theory with gravity is "not such a big thing" — although I agree strongly that there are many other important problems in physics. (I'll point out too that I was not arguing that LQG was likely to be correct, but I was comparing its progress versus string theory's on quantum gravity.)

34. **chris**
August 27, 2009

Peter Orland,

unfortunately i am not as young as you imply – i wish i were. and just to clarify:

if i was working on a candidate TOE and after 20 years discover that it might be a passable effective description for some solid state physics or quark gluon plasma, i'd not really call it a breakthrough.

35. **Peter Orland**
August 27, 2009

Well, Chris, I have to say, if it were true that string theory could solve real problems, I would consider that a success, not a disappointment. Having said that, I don't see any sign of success.

Any new solution in theoretical physics is a breakthrough. I personally would be much more happy and excited to solve Yang-Mills or High-T_{c} superconductivity than find a FUT (Fully Unified Theory – a more strident-sounding moniker than the melifluous TOE), since the former are related to experiments. I don't understand the feeling of some physicists that all life begins at the Planck scale.

What I said above (and see onymous's comment also) is that string-theory ideas have hit a wall in field-theoretic applications. In fact, they hit this wall very soon after the ideas were proposed.

The promise of a breakthrough means little. I wish people would stop making promises. People on the verge of breakthroughs don't advertise – they break through!

ICM 2010

August 27, 2009
Categories: Uncategorized

The International Congress of Mathematicians is held every four years, and the next iteration, ICM 2010, will be held about a year from now, in Hyderabad, India. These are huge conferences, planned well in advance, with 1465 mathematicians already pre-registered.

The list of speakers gives a good indication of what the mathematical establishment views as the most important research activity of the past four years, and this list is now available here. There are a large number of parallel sessions, and a limited number (20) of plenary talks.

The winners of the Fields medal are announced at the ICM, at the same time as the composition of the committee that made the choice (the chair of the committee, Laszlo Lovasz, is known). One way to help guess who will win a Fields medal is to take a look at those on the speakers list who are under forty. I'm not privy to any inside information, but many people think Ngo is a shoo-in for his work on the fundamental lemma (he's a plenary speaker), and there's some speculation about Jacob Lurie (who is a parallel session speaker).

This year there's a new prize to be awarded at the ICM, the Chern Medal, for "an individual whose accomplishments warrant the highest level of recognition for outstanding achievements in the field of mathematics." This one, unlike the Fields, comes with a significant amount of money ($250,000, and another $250,000 for the medalists favorite mathematical organization).

Comments

1. **Manel**
   August 27, 2009

   "there's some speculation about Jacob Lurie"

   What has we done?

   Regarding the Chern Medal, is it for people <40 years old as the Fields Medal?

2. **Marcus**
   August 27, 2009

   I see Matilde Marcolli is one of around ten invited speakers in the Mathematical Physics section.
   She just posted http://arxiv.org/abs/0908.3683
   Early Universe models from Noncommutative Geometry

3. **egan**
   August 28, 2009

   In the ICM list of plenary speakers why there is "USA" for Ngo Bao Chau?

   He is vietnamese and studied in France under Gerard Laumon : http://en.wikipedia.org/wiki/Ngo_Bao_Chau

4. **Peter Woit**
   August 28, 2009

   egan,

   I believe Ngo is now at the IAS in Princeton.

5. **Peter Woit**
   August 28, 2009

   Manel,

   The Chern Medal has no age restriction.

   For Lurie's work, see his home page, or search the n-category cafe or secret blogging seminar for blog entries about this.

6. **Manel**
   August 28, 2009

   Thanks Peter.

   PS: I obviously intended to write "What has **he** done?"

7. **simplyAnonymous**
   August 28, 2009

   Actually, 4 years ago the math blogosphere wasn't as developped as it is today, so let's hope the gossiping still remains minimal.

   Yet, since I'm no insider, and since your post is an invitation for gossiping 🙂 let me venture an opinion, namely that I'm not surprised to see Avila getting a plenary, he has the typical Field medalist pedigree (PhD aged 21, still only 30 today, many major papers and large output, former IMO gold medalist… ticks all the boxes).

8. **french anonymous**
   August 28, 2009

   Avila…

9. **egan**
August 29, 2009

>>>I believe Ngo is now at the IAS in Princeton

Peter,

So what ? Ngo is vietnamese and not american.
If you look at the ICM speaker list for Artur Avila you will see he is brazilian. He work in Paris but near his name on the ICM list there is “Brazil” and not “France”.
Near Ngo’s name it should be “Vietnam” and not “USA”.

10. **Peter Woit**
August 29, 2009

egan,

Avila’s CV has him with a French CNRS position, but now on leave from Paris and affiliated with IMPA in Rio. The countries listed seem to be not citizenship but location of current academic affiliation.

11. **harrison**
August 29, 2009

Irit Dinur looks like a shoe-in for the Nevanlinna (she’s under 40, right? she certainly looks it…) although it’d be fantastic if she won the Fields. (Why do I say this? Because she’s a plenary speaker without the status of a Goldwasser or a Wigderson, and her work on PCP certainly qualifies her for a Nevanlinna. It’s possible that Aharanov could get it, though, although I’m less certain that she’s under 40.)

Manjul Bhargava is conspicuously absent from the speakers list, although I’m pretty sure he has another cycle of eligibility. Chandrashekhar Khare was born in 1968, and is ineligible. Avila has two more cycles of eligibility, and most of the time the Fields committee tends to wait in such cases. (Think of Deligne; think of Drinfeld, who could have won in ’82, ’86 or maybe even ’78 without anyone batting an eye.) Lurie also has two more cycles. I think by far the best bet is on Ngo, with Avila strongly in the running.

12. **simplyAnonymous**
August 29, 2009

Yes, there are many deserving candidates obviously, so indeed it may ultimately push things more and more towards recipients in their late thirties, with twenty-something winners not occuring anymore.

Looking at the list of speakers a little more and crossing it with winners of prizes like European Math Soc. and Salem, it looks like Assaf Naor especially is another strong young contender, at least for future cycles.

13. **anon**
August 29, 2009

Kisin?

14. **Voltberg**
August 29, 2009

Manjul Bhargava, Ben Green, Cedric Villani and Bao Chau Ngo.

15. **perestroika**
August 30, 2009

cedric villani or alexander kuznetsov

16. **anon**
August 30, 2009

Christopher Hacon..
(Lurie and Venkatesh in 2014?)

17. **Walter Mondale**
August 30, 2009

I think they’d wait for Lurie… his work still is unpublished and he’s well under 40. Avila is a good candidate although young himself.

18. **Huron Lovett**
August 30, 2009

I’ve heard rumors about Kiran Kedlaya from MIT. And sure enough, he’s an invited speaker in the number theory session.

19. **chickenbreeder**
August 30, 2009

Slightly related, any news about Grigori Perelman lately?

20. **Peter Woit**
August 31, 2009

chickenbreeder,

I haven’t heard anything at all about Perelman recently. There the interesting question is what Clay is going to do about the million bucks for the proof of the Poincare conjecture.

21. **terence**
    September 2, 2009

    yun zhiwei

22. **terence**
    September 2, 2009

    yuan xinyi

23. **Chicken**
    September 3, 2009

    Look at the papers of Kiran Kedlaya, given that Laszlo Lovasz is the chair he would be my best bet with Bao Chau Ngo who is a certainty

24. **Sunny**
    September 4, 2009

    I guess all those Clay Math scholars and award winners are all potential candidates. Don't you think it would be interesting to have a first female Fields Medal winner in the history of math? The third winner's name may not have been heard of by us in the US.
    As you can see from the list of speakers in each field, there are many new names. But the ultimate question goes to "what has he or she done so significant?"

    If for age priority, the chance that Venkatesh and Rodnianski are also good candidates. But you will never know. Ngo is the only certainty.

25. **Sunny**
    September 4, 2009

    By the way, Who would be the candidates for Chern Medal? Serre?

26. **Walter Mondale**
    September 4, 2009

    I don't think there are any certainties in the fields medal this time around. Last time there was Perelman who was pretty certain, Tao was pretty certain to get one eventually, but I don't see now anyone standing head and shoulders above the rest… I guess we'll have to just wait and speculate 🙂

27. **anon**
    September 5, 2009

    I was under the impression that the Fields was given to people who solved big or hard problems. So, that rules out some of the people listed, right?

28. **Anonymous**
    September 5, 2009

    I was under the impression that the Fields was given to people who solved big or hard problems. So, that rules out some of the people listed, right?

    It's hard to say. The committee has a lot of discretion, and there's no clear cut-off for what constitutes a "big or hard problem". Some of the people mentioned here seem an awful lot more likely to me than others, but that's what you'd expect from blog comments, and there's enough unpredictability that I wouldn't bet my life savings against the less likely candidates.

29. **Andy**
    September 5, 2009

    With regard to hard problems, it is useful to recall the old joke : Thurston proved that you could win a Fields medal without ever writing out a proof, but then Witten proved that you could win one without even stating a theorem!

    That said, Ngo is the only person I feel at all confident about.

30. **observer**
    September 8, 2009

    Bao Chau Ngo, is working on Langlands and solved some important issues, and that was probably a reason why he was brought to the IAS. Avila could be a deserving choice too. As athird choice I think Jacob Lurie could be in the run.

31. **Andreas**
    October 4, 2009

    If you ask me: As Richard Schoen is a plenary speaker, in my opinion Simon Brendle from Stanford is a good guess for the fields medal. As far as I know he is not even 30 years old.

32. **Sunny**
    October 8, 2009

    Simon Brendle is a great mathematician. But they have given it last time to Perelman (in differential geometry/geometric analysis). So it may not be likely to give the Prize to mathematicians who are in the same field again unless they have some astonishing breakthrough comparable to the resolution of Poincare Conjecture. Brendle's works are excellent but he's got many competitors in the field. He has a chance though, but maybe not this time.

    For the field of Number Theory, it's competition is even more intense: Bhargava, Venkatesh, Soundararajan, Mark Kisin, Ben Green, etc. As Lovasz in the chair position, Sudakov is a good candidate for combinatorics (though not sure if he's still under 40 by the time). For Algebraic Geometry, there are many competitors as well: Christopher Hacon, Jacob Lurie,

    I think all those who are Clay, AMS, AIM, or EMS Award Recipients under 40 are stronger candidates. Reading the ClayMath winners, Rodnianski, Ian Agol and D. Calegari are another strong candidates for it.

    I think all the above mentioned are currently in the US. (Ngo is a sure fire; Avila comes in 2nd;) But Europe is getting very strong in math nowadays as well as Asia.

Well, I am more curious about the Chern Medal than the Fields. It seems to me somehow that this is a Prize for “an individual whose accomplishments warrant the highest level of recognition for outstanding achievements in the field of mathematics” So this means a lot. It seems to be THEEE Prize in math. I wonder who have the chances?

33. **Pascal J.**
October 8, 2009

What do you think about one of these Bonn guys? Take for example Bringmann.

String Theory Skeptic

September 2, 2009
Categories: Uncategorized

The latest Forbes magazine has an article entitled String Theory Skeptic, which gives me a lot more credit for the problems of string theory than I deserve.

The article as I just saw it online appears to have a minor editing problem, with the quote

> It's common in physics for people to have incredibly ambitious ideas that don't pan out but lead to rich mathematical ideas that end up being very useful.

which is attributed to Peskin in the middle of the article, appearing a second time at the end, right after a quote from me. In any case, even if Peskin is the one who said it, not me, it's something I very much agree with, and perhaps a good summary of the string theory situation.

Update: I gather that the Peskin quote is the "knockout quote" of the piece, set off and summarizing things, with the online formatting what makes it appear to be in the body, at the end.

Comments

1. **Doug Natelson**
   September 2, 2009

   That sounds like Peskin. He taught me graduate mechanics.

2. **DaveC**
   September 2, 2009

   Peskin wrote a theory of 'Cooper pair mass' which had condensed matter theorists I know wincing at its 'ambitious' nature, ie, the fact that it completely ignores pretty much everything that's understood about superconductivity in real stuff!

3. **dan**
   September 2, 2009

   "Princeton's Witten declines to discuss Woit, saying in an e-mail that he prefers to debate these issues only with "critics who are distinguished scientists rather than with people who have become known by writing books.""

   OUCH. I infer that Witten remains a strong string theorist believer

4. **Peter Woit**
   September 2, 2009

   dan,

   From what I've seen, my impression is that Witten has decided he'd rather not debate these issues at all, a choice I respect. If he only debates people at least as distinguished as himself, he may not have anyone to argue with…

5. **Garrett**
   September 3, 2009

   Not a bad article. They made things sound pretty grim for strings. And apparently you're equal to Q?

6. **Chris Oakley**
   September 3, 2009

   I think I get it. God talks only to Witten, and Witten talks only to God. String Theory is God's revelation to Witten, and he will only impart more to mankind when they are ready.

7. **dan**
   September 3, 2009

   Ok but Marcus has posted over at PF that Witten was having "doubts" about strings and was working on other fields but apparently that's just wishful thinking.

   "String theory was a bubble waiting to be pricked," says Woit, 51. "The fundamentals just weren't there anymore."Fans of string theory are well aware that the tide has turned. said his very brightest string theory graduate students are having trouble getting work.

   Is that true? No wonder you've become a lightening rod for string theorists like LM & others

8. **H-I-G-G-S**
   September 3, 2009

   You've got to love how the logic progresses here:

   "Princeton's Witten declines to discuss Woit, saying in an e-mail that he prefers to debate these issues only with "critics who are distinguished scientists rather than with people who have become known by writing books."

   "If he only debates people at least as distinguished as himself, he may not have anyone to argue with…" -PW

   "I think I get it. God talks only to Witten, and Witten talks only to God. " -CO

9. **Marcus**
   September 3, 2009

To dan,
What I reported over at PF is a matter of record. I don't speculate about Witten's state of belief or non-belief, as you unfortunately do. Like several other top researchers he has given clear signs recently that his interests are not confined to string. (Perhaps they never were, and we are simply being reminded of an obvious fact.)

At Strings 2007 he presented his work on 3D QG. He did not attend Strings 2008. He did not present a paper at Strings 2009, but gave a public lecture which was about nonstring physics. His talk this summer at Cern was about prospects for new physics away from the high energy frontier.

I think it's ridiculous to focus such attention on Witten or to speculate about one person's state of "doubt" or non-doubt. You implied I ascribed "doubt", but I don't recall even using the word. I think you need to look at the overall picture. That includes looking at the makeup of recent research by *several* prominent people who were formerly more focused on core string topics. What are they actually doing? Where is the significant creative work occurring, in which areas and directions?

10. **Peter Woit**
September 3, 2009

Garrett,

The photographer asked me to write something on the board, I think what I wrote was an expression for a BRST-like operator as a supercommutator with a Dirac operator. Then I stood in front of it…

H-I-G-G-S,

I suspect that Witten would be even less likely to be willing to debate anonymous blog commenters than book-writers.

11. **Martin**
September 3, 2009

Hi Peter,

Wow! It seems string theorists must really be on the defensive if Witten isn't active in this field anymore.

Have you considered what you will blog about when nobody is interested in string theory anymore? Reminisce about the times when an unkind comment by you provoked angry retorts: "I always used to enjoy a good bash with old Lubos", etc.?

Cheers Martin

12. **Peter Woit**
September 3, 2009

Martin,

I suspect string theory will be around in various forms for quite a while. However, I can already reminisce about the the days when string theorists argued that string theory had won the battle in the marketplace of ideas…

13. **Marcus**
September 3, 2009

Wow! It seems string theorists must really be on the defensive if Witten isn't active in this field anymore.

?

Inaccurate premise. The comment does not appear to make sense even as sarcasm.

14. **Peter Woit**
September 3, 2009

One can debate whether this matters, but from what I can tell, Witten has not given up on the idea of string theory based unification, but, not seeing how to make progress on this, has generally been working on other things.

The main problem for string theory unification in recent years is not me, nor Witten, but just that evidence against the workability of the idea continues to mount. The high visibility of string theory landscape anthropic pseudo-science has also taken its toll.

15. **David Nataf**
September 3, 2009

Why would Forbes Magazine have an article on string theory?

16. **Peter Woit**
September 3, 2009

David,

They have a column called "Ideas and Opinions", presumably they thought their readers might find this story interesting. This is far from the first time they have had articles related to string theory, see for instance

http://www.forbes.com/2008/10/07/nobel-prize-physics-oped-cx_mk_1007kaku.html

http://www.forbes.com/forbes/2002/0204/068.html

I once remarked to a friend of mine that business magazines seemed to have an unusual interest in my critique of string theory. He pointed out that they tend to be on the lookout for contrarians and contrarian ideas. If you could have sold string theory short a few years ago, you could have made a lot of money….

17. **Mitch Miller**
September 3, 2009

Interesting to see Baez's crackpot index quoted by a journalist in a major publication.

18. **Don Murphy**
September 3, 2009

I believe the interest from business magazines stems from their believe that there may be an exploitable technology resulting from string theory that could make them all rich some day.

19. **Don Murphy**
September 3, 2009

Sorry for the typo in the previous post: believe vs. belief

20. **Peter Woit**
September 3, 2009

Don,

I seriously doubt that a significant number of the readers of Forbes or any other business magazine are so far gone that they think string theory technology is an investment opportunity worth considering.

21. **dan**
September 3, 2009

"The main problem for string theory unification in recent years is not me, nor Witten, but just that evidence against the workability of the idea continues to mount. The high visibility of string theory landscape anthropic pseudo-science has also taken its toll."

"said his very brightest string theory graduate students are having trouble getting work. "

Is there any evidence that the reason fewer openings offered to string theory is specifically the result of bursting the string theory hype?

22. **Don Murphy**
September 3, 2009

Perhaps they're not that far gone, yet. But I do know they mine seriously all that goes on in the world of scientific theory hoping to find that one nugget that will emerge as potential technology. I mean think about it. To the uninitiated the thought of strings as the fundamental unit of existence could mean the ability to create life from scratch, to become the architect of new worlds. Science fiction? There is no doubt. But that is what all the hype has been about.

23. **John Baez**
September 3, 2009

What I really like is how they used the crackpot index to determine that Peter is *not* a crackpot. It does indeed work both ways.

He's never sent anyone a letter in green ink saying that he has a revolutionary theory he wants them to examine, but they have to promise first not to tell anyone about it. Nor has he threatened string theorists with show trials in which they will be forced to repent of their misguided views. Nor has he called Witten a "self-appointed defender of the orthodoxy".

Of course this doesn't mean he's *right*. But the writer at Forbes was wise to run him through these tests before writing the article.

24. **Koray**
September 3, 2009

"Princeton's Witten declines to discuss Woit, saying in an e-mail that he prefers to debate these issues only with "critics who are distinguished scientists rather than with people who have become known by writing books.""

Ouch. I infer that Witten is calling Smolin not a distinguished scientist?

25. **Marcus**
September 3, 2009

John Baez wrote: *But the writer at Forbes was wise to run him through these tests before writing the article.*

It is reassuring to see a member of the financial community prudently performing such due diligence regarding his source. Perhaps you could also provide financiers with a similar checklist for detecting nutcase derivatives and credit swaps.

26. **intrigued**
September 3, 2009

Dear Peter,

What do you think of the work done in twistor theory and twistor-string theory by Arkani-Hamed and collaborators?

Do you also feel that research in this direction is futile? If not, why is it different from the situation in string theory?

27. **capitalistimperialistpig**
September 3, 2009

There have been some famous debates in physics and astronomy: Bohr vs Einstein, the nebulae, the big bang. String theory isn't really ready for that – you need at least something that can be tested, at least in principle. I suspect that there are a couple of Nobelists who would duke it out with Witten, but he doesn't seem to be willing to take them on either.

He's probably right. What would they talk about? Einstein formulated a number of clever thought experiments to challenge Bohr and clarify the foundations of QM, but what could either side say on strings?

'Tis pretty!
'Tis not!

The problem, as someone might once have mentioned, is that so far, at least, string theory is not even wrong.

28. **Peter Woit**
September 3, 2009

intrigued,

The work you mention certainly isn't futile. Unlike string theory, it's not making grandiose but unsupportable claims about unifying all of physics. Instead it's an investigation of the structure of 4d gauge theory amplitudes using new ideas about how to work in twistor space rather than the usual space-time. We know 4d gauge theory is important and part of real physics. Ideas about how to reformulate it to uncover new structure are obviously interesting, with final significance depending on what new structure one finds.

The idea of using replacing space-time variables with twistor variables goes back to Penrose 40 years ago, and I've always found it very intriguing. Lots of people are working on this now, and there seem to be some quite beautiful new mathematical ideas emerging. So far this seems to all be about scattering amplitudes, I'm curious whether there will be some way to reformulate the theory itself in twistor variables.

29. **chris**
September 4, 2009

"Perhaps you could also provide financiers with a similar checklist for detecting nutcase derivatives and credit swaps."

This, indeed, is a fabulous idea!

30. **milkshake**
September 4, 2009

The Forbes article uses the String Wars as illustration how theoretical physics is actually done – I think its unfortunate.

31. **Marcus**
September 4, 2009

The Forbes article uses the String Wars as illustration how theoretical physics is actually done – I think its unfortunate.

Has something abnormal occurred? If so, what exactly was it?
What I remember goes back to 2003—a vehement attack on Loop conducted at sci.physics.research: numerous reasons why it could never work and why people working on it must be feebleminded. The inappropriateness of this attack—which appeared phony or diversionary—is what got me interested in Loop in the first place.

2003 was before Peter's blog, or book, and before Lee's book which did not appear until 2006. What had, however, happened was the KKLT result and Susskind's anthropic string landscape response. These were intrinsic string infirmities, not warfare originating from outside.

Making it look like the ensuing gradual deflation was a war between two rival camps is either cheap journalism or a face-saving cover. The most trenchant criticism of string has always come from senior physicists with no connection to Loop or any other rival approach. I assume these key critics have been motivated to dump anthropery, get the bubble agony over and done with, and restore credibility to the field. So "War" is simply the wrong journalistic cliché, maybe string "Blister" would be a better image.

But is this not how science is actually done? theoretical physics being no exception. How otherwise do scientific communities correct for mistakes and restore integrity?

32. **The Wombat**
September 4, 2009

G'day,

Jeffrey Kooistra had an article in the September 09 issue of ANALOG titled THE TROUBLE WITH PHYSICS.

He discusses some of the same topics as here.

You might find it of interest.

The Wombat

33. **Marcus**
September 4, 2009

Wombat, thanks for the pointer to this book review:
http://www.analogsf.com/0909/altview_09.shtml

34. **The Baron**
September 4, 2009

"Analyzing why people play golf is like exploring the intricacies of string theory – there are so many permutations lacking scientific observation that physicists or golfers can pretty darn well say anything they like and the explanation might stick."

–Bill Gross, CEO of Pimco

http://www.pimco.com/LeftNav/Featured+Market+Commentary/IO/2009/Gross+Sept+On+the+Course+to+a+New+Normal.htm

35. **Martin**
September 6, 2009

Hi Peter,

"… The work you mention certainly isn't futile."

How far along are Arkani-Hamed and friends in making testable predictions for twistor theory?

Cheers,
Martin

36. **Janne**
September 6, 2009

Why would Forbes be interested? Maybe because these days economics and theoretical physics have much in common. Similar challenges are faced in both fields.

37. **Peter Woit**
September 6, 2009

Martin,

Working on better understanding a theory that has passed thousands of experimental tests is rather different than working on a very speculative idea where you need to find some sort of connection to observable physics. In this case you are starting with an already tested theory, so the real question is whether the methods you are working on allow you to do computations more efficiently, or give other insight into the theory.

38. **onlooker**
September 6, 2009

The Forbes article appears to be the first instance of bad press that Witten has ever gotten. He earned it with the catty personal attack on Woit (and/or Smolin) and the implication that scientist skeptics of string theory are too dumb for their criticisms to be entertained.

39. **Marcus**
September 6, 2009

I didn't hear catty, and I didn't even hear bad press. I heard the tense restlessness of someone being forced into an uncomfortable role. The journalist was bugging him by casting him as Mister String Theory. I can imagine someone would hate to be put into that role, especially now.

Everything Witten has done lately, in major appearances, has pointedly signaled that he's not wearing the string label. I listened to three 90-minute lectures here at UC Berkeley in 2006 where he didn't once mention string or M-theory. Then at the end someone on the other side of the hall, I couldn't tell who, asked what about string, and he gave them a one sentence answer conveying hope, but not conviction.
Sure he writes some string papers but he also does Langlands.

What may have sounded like disdain for less eminent fellow academics could just as well have been disdain for the journalist. What business had the journalist suggesting that Witten debate string with anybody? whether grand or modest. He has other things to think about.

40. **Paul Jackson**
September 7, 2009

Marcus commented that: "you need to look at the overall picture. That includes looking at the makeup of recent research by *several* prominent people who were formerly more focused on core string topics. What are they actually doing? Where is the significant creative work occurring, in which areas and directions?"

As the string bubble collapses I get the impression that string folk, so adept at rationalising theory, are migrating into cosmology. Let's hope that they don't infect this now evidence-based subject with their bubble-bursting disregard for confirmed predictions.

41. **antipodal**
September 7, 2009

Not sure what thread to post this in, but it seems to be the kind of thing you like commenting on.

Magnetic monopoles detected in a real magnet for the first time

http://www.physorg.com/news171209923.html

42. **Peter Woit**
September 7, 2009

antipodal,

If you look at the paper in question, you see these aren't actually magnetic monopoles, they're

"emergent quasiparticles resembling monopoles"

which is much less interesting. I'll leave discussion of them to condensed matter physicists.

43. **onlooker**
September 10, 2009

(marcus)
"I didn't hear catty, and I didn't even hear bad press. I heard the tense restlessness of someone being forced into an uncomfortable role. The journalist was bugging him by casting him as Mister String Theory. I can imagine someone would hate to be put into that role, especially now.
"

You're inventing silly, forced explanations for a silly, unforced error by Witten. He is quite precise with language and knows quite well how to express concepts such as not being Mr String Theory, or Mr Strings-Only, or not wishing to debate string theory, or string theory's critics not having points worthy of debate. What he actually said doesn't have any such charitable interpretation, which is why the piece was explicitly and implicitly "bad press" as it concerns this particular utterance by Witten.

It's not all bad; the article has the usual material, some of it from Woit, about how brilliant Witten is. This one remark was very far from brilliant , and it is easy for any reader, physicist or not, to understand that.

44. **Marcus**
September 10, 2009

==quoting onlooker==
September 6, 2009 at 1:48 pm
The Forbes article appears to be the first instance of bad press that Witten has ever gotten. He earned it with the catty personal attack on Woit (and/or Smolin) and the implication that scientist skeptics of string theory are too dumb for their criticisms to be entertained.
==endquote==

I may have misinterpreted and you may have summed things up correctly.

I think of Paul Steinhardt as the string critic par ex.
His reply to the 2005 Edge question
http://www.edge.org/q2005/q05_print.html#steinhardt
is the most concentrated critique I've seen. It is hard hitting, focused and constructive. I'd like to hear his views now, almost five years later.

I'd like to hear a threeway conversation between Steinhardt, Peskin, and Strominger. They are all three about the same age (born around 1952-1954) and might be prepared to discuss matters openly.

Debate is not quite the right word. But something is missing. The string episode may have been a folly, and whatever it was, no one seems to want to be held accountable.

Leonard Susskind continues to mythologize. David Gross tears his hair with quiet dignity. Witten has better things to do. Maybe senior superstars should not be expected to take responsibility.

But a frank discussion by two or three slightly junior stars could help clear the air. I'm interested to know what you think, onlooker.

45. **Shantanu**
September 10, 2009

Hi
Peter,
Have you looked at Susskind's most recent colloquium at PI?

46. **Peter Woit**
September 10, 2009

Shantanu,
From a quick glance it looks like the standard story he has been selling for years now, without finding a lot of buyers. He seems however to have become delusional, calling it the "post-standard-model" standard model.

47. **onlooker**
September 10, 2009

(marcus writes)
"
But a frank discussion by two or three slightly junior stars could help clear the air. I'm interested to know what you think, onlooker.
"

There's an accumulated public explanatory burden resulting from all the evangelism and PR that dominated the discussion since the 1980's. The smartest people (or at least, an incredibly talented group by any math/science research standard) have been working on this for decades with thousands of papers and no physical output. Either they know something we don't, and can explain it both in general terms and with specific refutations of any technical criticisms by skeptics; or there is a serious problem with the structure of the field.

I don't think it matters how senior the researchers are who address these points. It could be Lubos Motl for all I care. But any string respondents will need to do more than reiterate cliches about "the only game in town", or point to emerging mathematical structures and holography as clear indicators of progress toward a unified physical theory. Having better computational tools in QFT is nice but that isn't what the publicity was about for the past 20 years. If the theory was heavily and deliberately oversold it would be refreshing to have some public admission that such is now the consensus (or becoming the consensus) within the field.

48. **Shantanu**
September 12, 2009

Peter, its interesting that Lee introduced him (from what I could tell).

Mathematicians: An Outer View of the Inner World

September 7, 2009
Categories: Book Reviews

A friend recently loaned me a wonderful book, the recently published [Mathematicians: An Outer View of the Inner World](), which consists mainly of photographs of mathematicians by Mariana Cook, paired with a page of comments from the mathematician being photographed. For more of the photos, see Mariana Cook's [web-site](). The comments typically deal with the story of what led the person into mathematics, or a summary of their career, or some general thoughts on mathematics and the pleasures of studying it.

Many of these mini-essays are well-worth reading. The Viscount Deligne describes working with Grothendieck and contrasts this to some of his later experience:

> When I was in Paris as a student, I would go to Grothendieck's seminar at IHES and Jean-Pierre Serre's seminar at the Collège de France. To understand what was being done in each seminar would fill my week. I learned a lot doing so. Grothendieck asked me to write up some of the seminars and gave me his notes. He was extremely generous with his ideas. One could not be lazy or he would reject you. But if you were really interested and doing things he liked, then he helped you a lot. I enjoyed the atmosphere around him very much. He had the main ideas and the aim was to prove theories and understand a sector of mathematics. We did not care much about priority because Grothendieck had the ideas we were working on and priority would have meant nothing. I later met other areas of mathematics where people were worried about doing something first and were hiding what they were doing form one another. I didn't like it. There are all kinds of mathematicians, even competitive ones.

Michele Vergne has an intriguing comment about the "quantization commutes with reduction" question, which is a fundamental issue for how symmetries work in quantum physics. When you have a gauge symmetry, do you get the same thing if you first eliminate the gauge variables (go to the symplectic reduction) and then quantize, or if you quantize and then take the gauge-invariant subspace? This turns out to be a remarkably interesting mathematical question. Perhaps the best way to think about its physical significance is to take it as a criterion for any viable notion of exactly what "quantization" is, and how it is supposed to interact with the notion of symmetry.

> Today I can see a dim light on a problem that has been on my mind for a long time. This is the assertion: quantization commutes with reduction. It was a beautiful conjecture of Guillemin-Sternberg, which was clearly true, but revealed itself hard to prove in general. I was able to prove an easy case. A much more difficult case was then proved by another mathematician ten years ago, using surgery. For me, this method via cuts is ugly. I would have liked to prove this conjecture with my own methods. Long after the full proof was found, I kept reorganizing my own arguments in all possible ways. If I repeated them over and over, the difficulties were bound to disappear. But they did not. These ceaseless failed attempts left a scar. I do still hope to discover where exactly the difficulty was, and today I feel I know the small hole where the difficulty was hiding. I think it can be grasped easily. Then, maybe, I will be able to formulate and prove the theorem in a much more general way. True, for thiat I need someone else's idea, but just recently, I used a brilliant idea of one of my students to explain a very similar phenomenon. I believe it can also be used to understand this case. Anyway, I will try. Tomorrow.

Comments

1. **theoreticalminimum**
   September 7, 2009

   Nice 🙂

   I actually came across Cook's 'Mathematician Gallery' some time ago, when I was looking for a photo of Don Zagier. May I point out that that she published another photo book [Faces of Science](), which is worth having a look into. By the way, all the pictures figuring in the galleries can be viewed in higher resolution if you right-click on any one of them, and select "View Image".

   It's quite nice to see young brilliant mathematicians like Tao and [Mirzakhani]() in the album 🙂 (following on your post about matters ICM, may I say that I carry the hope that the latter might become the first female Fields medalist). My favourite picture is that of my *maître*, J.-P. Serre 🙂

   I would finally suggest [The Unravelers]() as a great addition to Cook's book (but it's exclusively about mathematicians who have spent time at the *Institut des Hautes Études Scientifiques*).

2. **Anon**
   September 7, 2009

   Thanks for the post. The comments by Deligne were interesting.

3. **MathPhys**
   September 8, 2009

   Yes, that's a very nice comment by Deligne.

   PS I didn't know he became a Viscount.

4. **Deane**
   September 8, 2009

   I first noticed Cook's photos on the front cover of a Princeton University Press mailing. It had photos of authors of books listed in the mailing, but they were the nicest photos I had ever seen of mathematicians. I even emailed PUP asking if there was any way I could get a poster version of the cover or the individual photos. I am still disappointed that none are available. But I did pre-order the book (Amazon had it at a very reasonable price). I agree that the photos and essays are both wonderful.

5. **Chris W.**
   September 10, 2009

   Loosely related: The [Oded Schramm Memorial Conference]() was held at Microsoft Research on August 30-31.

   One of the talks given was this:

   > **Random triangulations as dynamical variables in quantum mechanical models**
   > [Michael Freedman]()

Abstract: I'll discuss a simple calculation which Oded showed me how to do and its implications for taking quantum mechanical models off lattice.

6. **Chris Oakley**
   September 11, 2009

   Happy birthday, Peter!

   I am actually in NY now working for the client. As we are not far from the WTC site I was a bit worried that Al Qaeda might be back for old time's sake. But then I realised they would not use our calendar. September 11, 2001 was 23rd Jumada all-thani 1422 (give or take a day), the anniversary of which would have been on June 16.

7. **Peter Woit**
   September 11, 2009

   Thanks Chris, both for the birthday greetings and for the obscure calendar reassurance…

8. **Chris Oakley**
   September 11, 2009

   A pleasure, and I hope that it is a better day than the one eight years ago. FWIW the dates of 23 Jumada al-thani, according to the Saudi calendar are

   1422 11-Sep-2001
   1423 01-Sep-2002
   1424 21-Aug-2003
   1425 09-Aug-2004
   1426 29-Jul-2005
   1427 18-Jul-2006
   1428 08-Jul-2007
   1429 27-Jun-2008
   1430 16-Jun-2009
   1431 06-Jun-2010
   1432 26-May-2011
   1433 14-May-2012
   1434 03-May-2013
   1435 23-Apr-2014
   1436 12-Apr-2015
   1437 01-Apr-2016

9. **YBM**
   September 13, 2009

   About mathematicians and would-be mathematicians, you could be amused to know that the Bogdanov brothers are now claiming that Alain Connes is stealing their key ideas without quoting them.
   http://forums.futura-sciences.com/lectures-scientifiques/335812-commencement-temps.html

Latest From the LHC

September 11, 2009
Categories: Experimental HEP News

Things seems to have been going well at the LHC recently, with the current schedule expecting injection of beams in a little more than two months from now, on Thursday November 19. After that, the plan is for a week and a half of beam commissioning at 450 GeV, and 450 GeV collisions at the beginning of December. The machine will then be ramped up to first 1 TeV, then 3.5 TeV, with 3.5 TeV collisions on December 14.

Soon after that (December 17), the machine will go into a technical stop period for the holidays, starting back up January 7. From then on, the plan is for a month of more commissioning work and pilot physics. The first regular physics run at 3.5 TeV will last about 3 months, with expected luminosity of 54 pb^{-1}. Then in May, the energy will be increased to somewhere in the range of 4-5 TeV, with a run beginning in June at that energy lasting until mid-October, with expected luminosity of 274 pb^{-1}. The machine will then be reconfigured for a one-month run with heavy ions, and then go into a long shutdown at the end of November.

Anyway, that's the latest plan, reality may turn out differently. For up to the minute information on how things are going, you can follow along here. The last sector to be ready is now supposed to be sector 67, which is in cooldown, the magnets currently around 200K.

See here for a recent Science Magazine story on the subject from Adrian Cho.

Comments

1. **dan**
   September 11, 2009

   "and then go into a long shutdown at the end of November."

   What is the benefit of a long shutdown?

2. **Peter Woit**
   September 11, 2009

   dan,

   A lot of work on the machine may be needed in order to get it to run not at 3.5-5 TeV/beam, but at the design energy of 7 TeV/beam. This may, for instance, require warming up and then cooling down the entire ring, which would be a time-consuming process.

3. **neo**
   September 12, 2009

   How did they fix all those badly soldered connections?

4. **dan**
   September 13, 2009

   ok, if everything goes according to schedule, when will data from LHC start pouring in? Presumably the Tevatron will continue to hunt for Higgs/sparticles

5. **Peter Woit**
   September 13, 2009

   dan,

   Well, the schedule has enough data to be interesting coming in during 2010, but not enough to really compete with the Tevatron on the Higgs. Tommaso Dorigo has a relevant posting about this

   http://www.scientificblogging.com/quantum_diaries_survivor/worst_nightmare_scenario_cern_150_gev_higgs

6. **dan**
   September 15, 2009

   Thanks,

   "f superpartners don't show up at 7 TeV/beam"

   What year at the earliest if everything goes to schedule would the LHC collect enough data from 7 TeV collusions due to luminosity to be able to show or suggest sparticles or rule it out?

7. **Peter Woit**
   September 16, 2009

   dan,

   Since we don't the masses of superpartners, there's no answer to your question. Depending on different choices of the masses, you can come up with cases where the LHC should see something quickly, or not for a very long time, if ever.

8. **dan**
   September 16, 2009

   Hi,

   I am well aware that SUSY could be broken at any scale.

I was thinking if MSSM or next to MSSM Distler seems to promote, and if SS is the correct explanation for EW stabilization. What range of mass would rule out SS as the explanation for higgs hierarchy-EW based on LHC energy and luminosities?

9. **Peter Woit**
   September 16, 2009

   dan,

   As far as I know, you can push all the superpartner masses high enough to be invisible at the LHC, at the cost of requiring higher amounts of fine-tuning to explain the hierarchy problem. So, depends what degree of fine-tuning you are willing to accept.

Bloggingheads Diavlog With Craig Callender

September 13, 2009
Categories: Uncategorized

There's a new Bloggingheads Diavlog up today, where philosopher of science Craig Callender and I discuss the topic of Philosophy and the String Wars. Regular readers of the blog will just get to see me make the same points as usual in video format, more interesting might be to hear Craig's point of view on some of this. We agree about the anthropic principle.

Those who follow science-blogging controversies will have heard that certain science bloggers have announced a boycott of Bloggingheads, based on the fact that two creationist/ID types had recently been allowed to participate. I heard about this after agreeing to do this latest one, and initially the idea of such a boycott sounded to me completely bizarre. Why would anyone boycott a media outlet that produces a lot of serious and interesting content on the grounds that two out of its hundreds of participants were cranks (I can't think of ANY completely crank-free media outlet)? So, I recently read much of the on-line discussion, including that of the original boycotters, some non-boycotters (here and here), and the discussion here with Bloggingheads founder Robert Wright, who put up a clarification of the organization's policy here. After wading through all this, I concluded that, yes, the boycott thing is completely bizarre. For one take on the question that I pretty much fully agree with, see this one by John Horgan.

Comments

1. **Thomas R Love**
   September 13, 2009

   Interesting discussion. Even the ID types should be allowed to speak, then we can all see how silly their ideas are.

   I especially enjoyed your discussion of the beauty of mathematics.

2. **David Nataf**
   September 13, 2009

   I don't know Peter, I think "bizarre" might be a bit strong. Progress is not inevitable on a global scale and certainly not on a local scale, I don't think it's obvious what the best way to deal with these kinds of questions is.

   What the Mainstream media does on GW for example is present two sides as equal, get a quote from each, etc. Is that parity the right way to go?

3. **Roger Schlafly**
   September 13, 2009

   Complaints about Behe seemed to have caused the boycott, but he is not even a creationist in the sense of a young Earth creationist. He accepted much of evolution. His views were clearly labeled as being outside the mainstream. Yes, the boycott is bizarre. It is odd that anyone would be so threatened by Behe.

4. **Peter Woit**
   September 13, 2009

   David,

   I just don't think what Bloggingheads is doing is anything like giving parity to cranks. See their announced policy, which is what the boycotters find worth boycotting.

   Roger and others,

   Please avoid any temptation to start debate here about creationism, ID, evolution, Behe, etc. etc. It will be ruthlessly suppressed. My strongly held belief is that the last thing the internet needs is more discussion of this in more places…

5. **Geoff**
   September 14, 2009

   I can think of at least four different completely crank-free outlets: Physical Review, Classical & Quantum Gravity, Journal of Mathematical Physics and of course everyone's favorite ArXiv. So should a string oriented cosmologist peruse any of the above and come across a paper about LQG they should immediately conclude that the outlet has been taken over by crackpots and stop publishing in those places. No, really, please stop publishing.

   Now if you'll excuse me I've got to go back to the kitchen where I'm trying to get my clock to run backwards by unscrambling an egg.

6. **Peter Woit**
   September 14, 2009

   Geoff,

   There are several reasons to characterize this particular boycott as "completely bizarre"….

7. **Giotis**
   September 14, 2009

   This bizarre boycott has a simple explanation. Some people are obsessed with certain topics and their obsession dim their judgment. You are not obsessed with the whole ID thing and thus you have correctly identified their bizarre behaviour.

8. **wonderment**
   September 15, 2009

   Peter, I think you missed the point by being too cerebral about this "boycott." I followed the controversy closely on Bheads, and my impression is that it has little to do with science v creationism and much to do with a personal misunderstanding between Bob W. on the one hand and Carl and Sean on the other.

   After the first creationist appeared, Bob has a conference call with people who included Carl and Sean. Carl and Sean felt (perhaps mistakenly) that Bob had committed to pulling the plug on future creationists. Then when Behe appeared, they felt betrayed. They quit defending a principle, but there was a lot of emotional baggage. The issue got further muddled when other bloggers, like PK Myer, boycotted Bheads out of solidarity with Carl and Sean.

9. **Peter Woit**
   September 15, 2009

   wonderment,

   I agree that the whole boycott thing cannot be understood “cerebrally”, it’s pretty clearly irrational…

The Holy Patron of String Theory and its Holy Grail

September 14, 2009
Categories: Uncategorized

Science News is running a long interview with Murray Gell-Mann, who will be celebrating his 80th birthday tomorrow. Gell-Mann was arguably (Feynman is one who would argue..) the most influential figure in theoretical particle physics throughout the 1950s and 1960s. In the interview, he gives the standard story about the cosmological constant/supersymmetry/hierarchy problem, expecting superpartners to be accessible at the LHC design energy, although perhaps not at its initial energy of 3.5 TeV/beam. If superpartners don't show up at 7 TeV/beam, he says:

> Well, we'd have to see exactly how bad it is. I mean how high up you go and still don't find anything and so on. But yes, one might have to discard this whole line of reasoning.

Gell-Mann describes himself as not a string theorist, but someone who thought it was promising and continues to do so, claiming:

> I was a sort of patron of string theory — as a conservationist I set up a nature reserve for endangered superstring theorists at Caltech, and from 1972 to 1984 a lot of the work in string theory was done there.

He speculates about what is missing in string theory as follows:

> I am puzzled by what seems to me the paucity of effort to find the underlying principle of superstring theory-based unified theory. Einstein didn't just cobble together his general relativistic theory of gravitation. Instead he found the principle, which was general relativity, general invariance under change of coordinate system. Very deep result. And all that was necessary then to write down the equation was to contact Einstein's classmate Marcel Grossmann, who knew about Riemannian geometry and ask him what was the equation, and he gave Einstein the formula. Once you find the principle, the theory is not that far behind. And that principle is in some sense a symmetry principle always.
>
> Well, why isn't there more effort on the part of theorists in this field to uncover that principle? Also, back in the days when the superstring theory was thought to be connected with hadrons rather than all the particles and all the forces, back in that day the underlying theory for hadrons was thought to be capable of being formulated as a bootstrap theory, where all the hadrons were made up of one another in a self-consistent bootstrap scheme. And that's where superstring theory originated, in that bootstrap situation. Well, why not investigate that further? Why not look further into the notion of the bootstrap and see if there is some sort of modern symmetry principle that would underlie the superstring-based theory of all the forces and all the particles. Some modern equivalent of the bootstrap idea, perhaps related to something that they call modular invariance. Whenever I talk with wonderful brilliant people who work on this stuff, I ask what don't you look more at the bootstrap and why don't you look more at the underlying principle. . . .

Lubos Motl seems to have calmed down a bit recently, and his latest posting is about the Gell-Mann interview. He describes Gell-Mann as not just a patron of string theory, but a holy patron of string theory, with the comments quoted above "the holy word". They inspire him as he continues to work a few hours a day towards finding the holy grail of string theory: some fundamental principle that defines the theory non-perturbatively.

Searches for such a principle go back at least 25 years, to 1984 and the explosion of interest in string theory as a unified theory. After the first efforts to base unification on a Calabi-Yau, it soon became clear that more was needed than string perturbation theory. Just one of many such attempts that I remember was that of Friedan/Shenker in 1986, who hoped that in some sense the moduli space of all Riemann surfaces would somehow carry a unique vector bundle with flat connection. There were many others.

Lubos entered the field ten years later, after discoveries about dualities had led to Witten's conjecture of the existence of an "M-theory" that would reduce in various limits to the known string theories. At the time, the hot candidate for such a theory was something called Matrix theory, and Lubos made his reputation with work on this. His thinking these days grows out of the "M-theory" conjecture that he first started working on as an undergraduate 13 years ago, and probably reflects well the kind of speculative hopes that drove this area of research from the beginning:

> It also seems extremely likely that some UV/IR links – modeled by the modular invariance in the context of perturbative closed strings – will be important for the formulation of the ultimate principle. Non-perturbatively, it seems obvious that such a link will have to constrain the black hole microstates, i.e. the generic high-mass particle species in any theory of quantum gravity. The spectrum and detailed structure of the black hole microstates must be linked to low-energy fields and all of their higher-order interactions. These conditions will admit a limited number of solutions that will coincide with the allowed configurations of string/M-theory.
>
> Moreover, it's conceivable that we won't be able to work "fully on the worldsheet" or "fully in the spacetime". I feel that the ultimate set of consistency rules for quantum gravity will work "simultaneously" for the generalized worldvolumes as well as spacetime. So I am spending a lot of time by attempts to import some lessons – and methods to derive or generate new degrees of freedom – from spacetimes to the worldvolumes, and vice versa.
>
> Modular invariance, mutual locality of operators, Dirac quantization rules, similar conditions, and their generalizations play an important role. But it remains to be seen whether there is a concise, ultimate principle or set of principles, why it generalizes the conformal symmetry (and modular invariance) in the perturbative limit, and why it admits old perturbative solutions as well as new, non-perturbative solutions such as the 11-dimensional vacuum of M-theory.
>
> Of course, one of the most obvious testing grounds for such new sets of ideas is the exceptional U-duality group of M-theory on tori – i.e. the maximally supersymmetric supergravity. The exceptional groups are pretty and they must have a pretty cool explanation in terms of a structure we still don't fully know.

Like Gell-Mann, Lubos expects the right theory to emerge not from choice of a specific set of dynamical degrees of freedom, but by a "bootstrap": discovery of some sort of consistency conditions that uniquely pick out the right theory. The idea is that you don't have to get to fundamental variables at the bottom of things to rest your theory on, but can by some other means "pick yourself up by your bootstraps". Since this doesn't work in real life, I've always wondered why its advocates didn't pick a more convincing name…

Lubos ends his posting with:

> I think that some kind of bootstrap is needed to determine what "M" and its structure of symmetries really is. Is there a third person in the world who cares about this possibly most important question of science? These core topics of string theory are currently understudied at least by two orders of magnitude.

The question of why so few string theorists work on this question is an interesting one. The M-theory conjecture drove string theory research for many years. My own suspicion is that the fact of the matter is that most string theorists have just given up on it. The AdS/CFT correspondence appears to give a non-perturbative definition of string theory in a particular background (in terms of a QFT), and string theorists are more interested in investigating that than in continuing the so-far futile search for "M-theory". In addition, arguments of landscapeologists indicate that if you did find the conjectured "M-theory", it might be a useless untestable "theory" that could explain just about anything.

Physicists with a sense of history also have another good reason to be suspicious of calls for a new "bootstrap" program. This idea was all the rage during the sixties, but ended up a dismal failure. The conjecture that some known powerful principles (analyticity, crossing, etc..) would have a unique solution satisfying them just turned out to be wrong as a way of understanding the strong interactions. There are lots of possible solutions, and finding the right theory requires identifying the correct one: an SU(3) gauge theory with a specific, very beautiful set of geometrical degrees of freedom. This theory remains poorly understood, and the project of better understanding it recently has revived some of the bootstrap ideas, but in the context of trying out a new choice of geometrical degrees of freedom (twistors). This is now the hot idea of the subject, but it's no longer one that promises unification via string theory. I suspect Lubos will be increasingly lonely in the pursuit of the dream of his youth, as his colleagues mostly give up on it and move on.

Comments

1. **Phil**
   September 14, 2009

   Peter,
   your link on Lubos' page refers to this page:

   http://lubos.motl.googlepages.com/crackpot-not-even-wrong.html

   So I don't think that he'd calmed down….

2. **capitalistimperialistpig**
   September 14, 2009

   As a one time student of Lubology, I would say that there have always been two Lumos, the vengeful prophet and the insightful scientist – Dr. Lubos and Mr. Motl. Good Lumo writes nice biographies and elegant explanations all the while pondering the deep questions of physics. Evil Lumo is obsessed with his many enemies. One cheers for the good guy, but you have to know the other one is waiting with … whatever.

3. **Thomas Larsson**
   September 14, 2009

   "Just one of many such attempts that I remember was that of Friedan/Shenker in 1986, who hoped that in some sense the moduli space of all Riemann surfaces would somehow carry a unique vector bundle with flat connection."

   I met Friedan in 1990, at a conference to which I travelled with a mathematician friend who was very excited about the Friedan/Shenker work. Friedan himself did not show any interest to discuss it though. He dismissed his work as "very abstract", and it was clear that this was not meant in a positive way.

4. **Franz**
   September 15, 2009

   I can testify that Lubos has NOT calmed down. I have copies of very recent emails in which Lubos told an aquaintance (on Lubos' posting about Gell-Mann's interview) that "careful observations" show that the world has either 10 or 26 space-time dimensions and that whoever believes that space has 3 dimensions has a "lethal disease" and is full of "blinded religious dogmatism". (I am not making this up.) Despite believing in 3 dimensions, my acquaintance does not seem offended, but he sure is astonished.

5. **a quantum diaries surivivor**
   September 15, 2009

   CIP, I agree – Lubos gives the impression of having two personalities. I think it bothers him a bit if I bring this up when he starts talking foul in my blog. He mostly behaves with me now. Right, Lubos ?

   Recently, I have also found out that he drops the conversation if I threaten him physically. He was suggesting that my male organ is undersized, and I offered to pay him a visit and have him experience it, adding that he should prepare lots of Lube, shave carefully, and use little makeup. I think that was too much for him to handle, and he dropped off the thread.

   Cheers,
   T.

6. **Peter Woit**
   September 15, 2009

   Tommaso,

   Now, now, this is supposed to be a PG rated blog, as well as one that firmly espouses non-violence (at least in the string wars).

   Interesting to see from Lubos's latest comments on his posting that he's upset not only about me and Smolin, but about the recent behavior of the powers that be in particle theory:

   "This is about forces that have penetrated almost everywhere into the "official" scheme of things. This is about the Chamberlainian attitude of the physicists who actually know what the truth is. It is about the drivers that lead people, especially the young ones, to decide about their "focus" and "excitement" which is so rarely real these days. All these things are wrong and I don't want to be a part of it."

   Particle theory is in a sad way these days, and, as usual, I suspect that Lubos and I agree about more things than most people realize…

7. **TomK**
   September 16, 2009

   Would it be a good idea to place a short warning within this posting to warn future readers what will happen when they follow that particular link?

8. **Franz**
   September 16, 2009

   Peter, as you cited already, in the fast comment section on his posting on Gell-Mann, Lubos criticizes the patrons of physics: "it is about the drivers that lead people, especially the young ones, to decide about their "focus" and "excitement" which is so rarely real these days."

   At the beginning of that comment section he behaves in exactly the way that he criticizes: he writes incorrect and offensive statements about a researcher and his unification attempt. I am really shocked by this double standard. Lubos turns out to behave in exactly the same way as the people he criticizes.

   Let us hope that Gell-Mann's proposed search for an underlying principle will be undertaken by more people. Lubos narcissistic personality disorder will prevent him to find any such principle. We need professional scientists to achieve it, people who are focused and determined.

9. **big vlad**
September 16, 2009

I am really shocked by this:

"I am really shocked by this double standard. Lubos turns out to behave in exactly the same way as the people he criticizes."

10. **Tim vB**
September 16, 2009

concerning Lubos: One basic rule of psychology is that what you dislike most with other people are your own biggest faults.
BTW: Since it seems that Lubos doesn't know: Gell-Man took the name quark from the novel "Finnegan's Wake" by James Joyce,
but Joyce himself did probably hear the word during his stay in Austria, it is a common German word meaning "curd cheese".

11. **Dave Miller**
September 16, 2009

Peter wrote:
> Particle theory is in a sad way these days, and, as usual, I suspect that Lubos and I agree about more things than most people realize…

I've always suspected that. Lubos comes from that Central European milieu where, in print, you take no prisoners, but I've always suspected that you guys might actually have a pleasant dinner together (if you're ever in Sacramento at the same time, you have a standing invitation).

I took a freshman course on particle physics from John Schwarz, who I thought was a truly nice guy, during the early days of string theory (too early for John to tell us frosh about it). I think Gell-Mann does deserve credit for "protecting" what was, after all, an interesting idea – even if it later became a fad.

I've always had this nagging suspicion myself that there is some underlying geometric-style symmetry behind all of the string/post-string math. Maybe Gell-Mann, Lubos, and I are all still blinded from our youth by having learned about how Einstein worked out GR.

Or, maybe, Lubos will actually stumble upon something.

Dave Miller in Sacramento

12. **chris**
September 16, 2009

but Joyce himself did probably hear the word during his stay in Austria, it is a common German word meaning "curd cheese".

i don't think he heard that in Austria, since it's called "Topfen" there, not quark 🙂 afaik, he read a grocers ad "Musterquark fuer 3 Mark" and transformed it into "3 quarks for muster mark".

13. **Tim vB**
September 16, 2009

Hi Chris,
I stand corrected, so the ad was a German one, since the currency in Autria would have been Schilling, not Mark, right?
Anyway, Murray Gell-Man was looking for a name for some mysterious entity and ended up with the name of a milk product, which is an anecdote worth telling in every class on QFT 🙂

14. **Chris Oakley**
September 16, 2009

According to this you're all wrong about quarks as "three quarks for muster Mark" refers to derisory squawks directed against King Mark, the Cornish king whose bride – the Irish Princess Iseult (or Isolda) – turned out to be far more interested in Tristram (or Tristan), one of his knights, than the king himself. One of my ambitions is to make a movie of Wagner's version of this legend (his opera *Tristan und Isolde*) but as I have just turned 50, and have yet to make any movies, it is looking ever less likely.

BTW: perhaps the only thing I agree with Jacques Distler about is that giving air time to Lubos or to comments about him is a Bad Thing. Can I propose a similar policy here?

15. **Kea**
September 16, 2009

They serve quark cake at some mountain restaurants in the Bernese Oberland in Switzerland … and maybe further afield, but I'm not sure.

16. **Marcus**
September 16, 2009

Yes, deliciously rich, with butter and higg-yolks.

17. **Jack Sarfatti**
September 18, 2009

Parts of string theory will survive and be testable. It is basically the quantization of the classical Einstein-Cartan localization of the 10-parameter Poincare group + supersymmetry. I am on way to Trinity College, Cambridge and will elaborate on this another time. Agreed on the MPD of Lubos who has a webpage from Rutgers that he is a transexual ET – humorous of course. If Lubos did not exist I would have had to invent him as a larger-than-life character in a Pynchon or Borges story. 🙂

Various and Sundry

September 16, 2009
Categories: Uncategorized

- General Relativity and Gravitation has a special issue on quantum gravity, available here.
- Some out-takes from photographs taken for the recent Forbes article are here. You can see what part of my office looks like…
- I haven't regularly been following the TV show *Big Bang Theory*, which features a main character (Sheldon) inspired by Lubos Motl. Someone who has is Bad Astronomer Phil Plait, who is interviewed here, with the following exchange:

> Alan: My geek barometer question for the Big Bang Theory is, Do you ever pause it and look at the board and try to decipher the equations?
>
> PP: I don't need to pause it, just a quick glance. Actually, it's all really advanced stuff, like string theory and more. Actually I don't think it's string theory because Sheldon said some nasty things about string theory in the past. But I never really understand it. There's some other things that they've got in there that I recognize.

I assume Phil is just confused, but if things have gotten to the point that Sheldon is saying nasty things about string theory, it's really in trouble…

- Physics World has two interesting interviews by Matin Durrani on-line, one with CERN Director General Rolf Heuer, the other with CERN head of communications James Gillies.

One topic discussed by Heuer is CLIC, and CERN's hope to be the place where the next generation electron-positron collider gets built. Here's a recent presentation about CLIC's status. If one were the wildly optimistic sort, one could see R and D on this finished next year, a complete design by 2016, construction starting in 2018 and first beam in 2025.

- Even further down the road than CLIC would be a muon collider. Fermilab now has a web-site devoted to the topic.
- You might want to keep up with the activities of the Bogdanovs here.
- John Hagelin's Global Financial Capital of New York (or someone they sold to recently) seems to be selling its building, which includes 3 stories configured as luxury apartments. $45,000,000 and it's all yours. For some more of Hagelin's activities over the last few years, there's this.
- The New York Times Book Review has a nice review of the recent biography of Dirac I wrote about here, which is now out in the US.
- If, unlike Dirac, you prefer your spinors real, there's a very interesting review article in Nature by Frank Wilczek, entitled Majorana Returns. I hadn't realized that these things now seem to be finding a place in condensed matter physics and even quantum information processing.

Comments

1. **John Armstrong**
   September 16, 2009

   Phil's confused. Sheldon (Jim Parsons) is a strong proponent of string theory. He's repeatedly slammed recurring character Leslie (Sara Gilbert) for her advocacy of loop quantum gravity.

   And actually there's plenty of equations on the boards that are on the level of quantum field theory, or even just some integral identities (ostensibly in the service of QFT). But they *are* all reasonably accurate, especially when you consider that this isn't even a sci-fi series, but a straightforward situation comedy.

   One particular exchange deserves pointing out. One morning Sheldon wakes up, walks past his board, and is pulled back by the recognition that the direction of an inequality has been changed. He expresses outrage, at which point Leslie (who has evidently stayed the night with Sheldon's roommate) wanders in and says, "Oh yeah, I did that. Now your quarks are free at high energies" (or something similar), which is actually reasonably close to what the change would do.

2. **Veronica Mars**
   September 16, 2009

   Yeah, I allways check what's on the board! 😀
   And no, he never said something bad about string theory, as far as I remember.

3. **capitalistimperialistpig**
   September 16, 2009

   I follow the show pretty closely, always try to read the equations, and have never heard Sheldon bad mouth strings.

4. **chris**
   September 17, 2009

   John Armstrong,

   i'd love to see 't Hoofts reaction to that particular joke 🙂

5. **Kevin**
   September 17, 2009

   Who cares what a stupid and unfunny TV show like "Big Bang Theory" says? It's not even worth mentioning.

6. **onymous**
   September 17, 2009

   Yeah, I allways check what's on the board! 😀

   As if Veronica Mars would misspell 'always'.

   The odd thing was that in the bit where Sheldon is slamming loop quantum gravity, you can see on his whiteboard in the background that there's something about the Immirzi parameter.

7. **Thomas Larsson**

September 17, 2009

LM did [some work](#) on the Immirzi parameter. So maybe he is the model for Sheldon after all 🙂

8. **The Vlad**
September 17, 2009

The Motl paper contains a sentence starting with a lovely phrase that I don't recall seeing in a scientific paper before:

"Morally speaking, the perturbation…"

9. **Adam Helfer**
September 17, 2009

This use of "morally" definitely exists in a subsector of physics and mathematics. I believe Michael Atiyah and Raoul Bott used it, and I recall some around them using it. I wonder if to some degree Atiyah was influenced by his brother Patrick, a legal scholar whose work on torts inevitably touches on the issue.

10. **Coin**
September 17, 2009

The article about the CLIC requires some sort of CERN account to view it.

How does the CLIC fit into the sort of overall scheme of collider plans? Like, how does it compare to the ILC? Wikipedia's page on the CLIC says it would be like the ILC but run at a higher energy. What is the reasoning for building the ILC then? Is the idea that the CLIC is just the ILC's successor?

11. **Peter Woit**
September 17, 2009

Coin,

I guess CERN management seems to have decided that the recent update on CLIC's status was a bit too recent for public consumption…

CLIC uses a very different acceleration technology, one that still has not yet been definitively proven to work as hoped. In principle one could start building the ILC very soon, if the money were available, whereas CLIC is a decade or more from that point. I guess the current thinking is that if the LHC soon discovers something at low enough energy to be studied by the ILC, it would be worth going ahead with the ILC, not waiting one or two more decades for CLIC.

12. **D R Lunsford**
September 17, 2009

Sigh, the first article in the gravity volume is on superstring perturbation theory :/

Amazing there is no mention of Finkelstein in the Wheeler plaudits. He of course made the current state of the GR art possible by re-inventing black holes in a way accessible to calculation. I would have thought Wheeler would have mentioned that as it happened in the '50s

-drl

13. **David Garvin**
September 17, 2009

David Saltzberg is the science advisor and there is a really interesting interview with him about his input. Re: the whiteboards:

"I send the set designers the material for the white boards and they put it up during the week before the taping. Sometimes the scribblings pertain to the topic of the show. For example, in the episode where the boys buy a time machine replica, the equations for time travel using wormholes, are on the whiteboards. One mathematician blogger criticized the way they were adding spins on the whiteboard as clumsy, but he also recognized that this is the way physicists would actually do it."

http://the-big-bang-theory.com/saltzberg.interview/

14. **anon.**
September 18, 2009

Sheldon's defense of string theory: http://www.youtube.com/watch?v=FMSmJCKaaC0

15. **Chris Oakley**
September 18, 2009

I saw two episodes of the show on the flight home (NY to London) yesterday, which included the above clip. I was hoping that it would be squirm-making, but it wasn't. The only thing that they've accurately captured is the arrogance. Sheldon and Leonard are far too normal. Some of the physicists and mathematicians I worked with (was – ?) were total freaks, and it might actually be funnier to tell it like it was/is, e.g. a mathematician who is perfectly capable of saying nothing for an entire evening (I knew a few), to name but one example of eccentric behaviour.

16. **Deane**
September 18, 2009

"morally correct" was widely used at Harvard, when I was a math graduate student there (this is also when Atiyah visited Bott regularly and often and gave "secret seminars" in Bott's office). It means a proof that is incomplete or incorrect, yet it elucidates the critical insight or idea that motivates the theorem.

Although Bott probably did use this term, I believe it was used more often for analytic proofs of algebraic geometry theorems, where the rigorous proof required a more algebraic and, for some of us, totally opaque proof.

There was another noun used in a similar spirit, maybe "yoga"? It would mean the overall philosophy behind a particular proof. I recall Melrose at MIT using this regularly.

This, I think, was a reaction to how much time and effort was being devoted by students to learning about and building mathematical machinery, as well as filling in all the rigorous details of a proof. Although these are all necessary steps to becoming a research mathematician, people such as Atiyah and Bott stressed that, after you had mastered the skills for constructing rigorous definitions and proofs, then once you understood the critical idea or insight underlying a given theorem, you could usually construct a complete proof of that theorem yourself without having to read every line of the proof given in a book or paper.

The most extreme expression of this view was by David Kazhdan. He would constantly show one of his graduate students a book or paper and say, "See this? You should know everything in it, but don't read it!"

17. **Peter Woit**
September 18, 2009

Hi Deane,

I definitely remember hearing "morally correct" from Bott way back when, probably also from others. "yoga" I believe I first heard from Blaine Lawson. From the context I took a "yoga" in mathematics to be a package of results and techniques that required a certain amount of training and exercise to know how to apply, but were powerful tools in the hands of adepts, and reflected some deep aspect of mathematical reality. Think homological algebra…

18. **Jeff McGowan**
September 18, 2009

"Moral" seems to me to be quite common in the math community. It was certainly used by a number of folks at the Graduate Center when I was a student there in the late 80s. Scott Wolpert from U MD uses it all the time, he got his doctorate at Stanford in the mid 70s. My colleague Eran Makover uses it, both his undergraduate and grad degrees are from Hebrew U.

Yoga in math I don't know, other than http://math.gc.cuny.edu/Faculty/dodziuk/
(my advisor, I can do one too, but there are no existing photos of that 🙂

19. **Giotis**
September 18, 2009

This character is inspired by Lubos? Why are you saying that?

20. **anon.**
September 18, 2009

Giotis, see: http://physics.about.com/b/2009/03/04/big-bang-theory-emmy-movement.htm

21. **Felipe Zaldivar**
September 18, 2009

Hi Deane and Peter, the expression "yoga" is widely used by Grothendieck and Serre (and their school) from the early 60's (see, e.g. the Grothendieck-Serre correspondence, p. 146 in a letter from April 1, 1964 in the AMS translation).

22. **Geoff**
September 20, 2009

As a string theory critic it appears your priorities are backwards. On your "top shelf" it appears you have Kaku, Polchinski, Zwiebach, and Becker Becker Scwarz. Below that it looks like Peskin and Schroeder and possibly Ashok Das. And below that it looks like t'Hooft's 50 years of Yang Mills. Shouldn't you apply a parity operation to your book shelf?

23. **Shantanu**
September 20, 2009

Peter and others, an interesting conference at TAMU in November
on 50th anniversary of ADM formalism
http://adm-50.physics.tamu.edu
Peter: do you think ADM formalism has had an impact on string theory?

24. **ManyMe**
September 21, 2009

Does anybody else here think string theory is an [unphysical pile of garbage?](#)

Characterising Science and Beyond

September 21, 2009
Categories: Multiverse Mania

This week the Templeton Foundation is funding yet another conference on the Multiverse, this one is entitled [Philosophy of Cosmology 2009: Characterising Science and Beyond](). The conference is also celebrating the 70th birthday of Templeton Prize winner George Ellis. The conference web-site includes a [page]() showing the book covers of recent multiverse books, noting that:

> The selection of books shown here (at both the popular and technical level) demonstrate the fact that the notion of the Mutliverse is becoming increasingly mainstream.

Ellis has expressed some skepticism about the question of whether the multiverse idea is testable, but, as usual with these Templeton conferences, there seem to be rather few skeptics invited. On the other hand, there do seem to be quite a few philosophers of science, and some philosophers of religion, (Alex Pruss of Baylor and Robin Collins of Messiah College), which I guess is appropriate.

Sean Carroll, who seems to have overcome his [earlier qualms]() about Templeton funding, is live-blogging the conference (see [here]() and [here]()). He notes that Ellis is worried that the multiverse may be inherently untestable and thus not science, but doesn't himself think this is worth worrying about. Presumably he'll continue tomorrow, covering the rest of the conference.

Update: Sean Carroll's live-blogging of the Templeton conference is the lead item of the front-page news on their [web-site]().

Comments

1. **Tom S.**
   September 21, 2009

   Dr. Woit. I see you discuss posts that seem to link the concept of the multiverse with religion. (Or at least with templeton.)

   Not that it matters, but I was just curious, are you implying that since some religious people see the concept of a multiverse strengthening their beliefs a sign that the concept of the multiverse is a bad thing?

   For a specific example, once you pointed out that mormons are liking the multiverse. Are you suggesting that since some mormons find a use for the idea this proves the idea is bogus?

   Not that I care, I was just wondering how a scientific idea supporting religious views in the minds of some make it so that such a scientific idea should be frowned upon.

2. **Peter Woit**
   September 21, 2009

   Tom S.,

   My problem with multiverse research is that it's mostly not science and is often conducted with the motivation of trying to use pseudo-science to explain away the failure of string theory as a unified theory.

   What religious people think about the multiverse is pretty much irrelevant to me, and no I don't think the fact that Mormons believe something is any kind of argument for or against it. Some religious people may think the multiverse gives an argument for their faith, others an argument against their faith (e.g. one can use the multiverse to counter the argument from design). Either way, their interest in the topic is motivated by something other than science, something I don't care about.

   What I do find worth pointing out is the role being played by organizations like Templeton. They have an agenda to bring together religion and science, and are implementing it by funding this kind of conference. The participation of philosophers of religion is just one indication that something funny is going on here, something that has nothing to do with legitimate science.

3. **Marcus**
   September 21, 2009

   Why is Joe Silk involved?

4. **Peter Woit**
   September 21, 2009

   Marcus,

   It's a cosmology conference at Oxford and he's a cosmology professor at Oxford…

   There are several mainstream cosmologists participating.

5. **Marcus**
   September 21, 2009

   Of course I know he's at Oxford. Even if disinclined he'd almost be obliged to participate, if for no other reason than to nominally honor George Ellis.
   But I think that such a slanted meeting, greased with Templeton fat, is no real honor to Ellis. It is sad to see people like Ellis and Silk drawn in. Just my personal view. I have enormous respect for both Ellis and Silk—think of them as scientists of the great integrity. Sad to see them roped into a event that looks rigged. Used.

6. **Robert L. Oldershaw**
   September 21, 2009

   Einstein advocated combining Spinoza's "religion" with science.

   He said: 'Religion without science is blind; science without religion is lame', or words to that effect.

If the Templeton Foundation was promoting this specific, and far more mature, form of combining science and "religion", would you still object.

Is there any credible evidence for a more dubious hidden adgenda pursued by the TF?

Might TF be promoting the Deism of Democritus, Spinoza, Jefferson + Franklin, and Einstein rather than Theism? There is a profound difference between the two, but both could be labeled "religion".

RLO

7. **Peter Woit**
September 21, 2009

I don't think the problem here is the Templeton Foundation, the problem is the toleration of pseudo-science by mainstream scientists. Templeton funds some real science, all sorts of things that have nothing to do with science, and some pseudo-science. If they didn't have prominent scientists signing on to participate in their pseudo-science activities, there would be no real problem.

It's unclear to me who at this conference besides Ellis is skeptical about the claims of multiverse studies to be science. Presumably there are quite a few. I get the impression though that the idea behind this conference was a somewhat misguided one: to bring together philosophers and physicists to examine the "is the multiverse science" question. The problem is that it's not a philosophical question, it's a physics question: do you have a theory that has any hope of giving what is needed? No one does, and as far as I can tell, the reasons for this are not being examined. They're the same reasons string theory can't predict anything about non-cosmological physics.

8. **Marcus**
September 21, 2009

Dear RLO,
what the Templeton Foundation is promoting is neither honest religion nor honest science.
You ask for evidence of (what I would call openly harmful and misguided, but not hidden) agenda. Look at the program of talks. It is slanted to promote multiversalism with a speaker lineup with celebrated proponents not balanced by strong critics.

Had they wished, I expect the organizers would have had no difficulty finding speakers to argue that religion has no need for a multiverse, nor does multiversalism do religion any good.

Likewise they could have found speakers (e.g. Paul Steinhardt) to argue that acquiescing to a landscape of multiple versions of physics is a betrayal of scientific values and not even opportune: Aside from some specialists in a stalled line of research who would benefit by abandoning the goal of a predictive as well as explanatory physics?

Left to itself, without the support of Templeton money, the multiverse speculation of the years 2003-2007 would have dried up. We wouldn't be hearing it discussed. Science has self-regulatory mechanisms. For instance the string theory landscape was almost entirely excluded as a topic from the main conferences Strings 2008 and 2009. A big change compared with 2005. Templeton funds tend to drag the clock back.

"Religion" is fine. "Deism" is fine, they're not the issue. I think Jefferson Franklin, and Einstein would have seen though the multiverse hype, and seen Templeton for what it is, a corrupting manipulation.
In my humble opinion.

9. **Mark**
September 21, 2009

I attended the Wheeler Symposium: Science & Ultimate Reality. (Mar 2002) which was funded by TF. Lots of great talks. Zeilinger explained a new delayed choice experiment he had carried out and Smolin argued that LQG was the long sought quantum gravity, to which Cecil DeWitt replied smiling, "This is probably the Xth time I've heard that announcement. Let's hope you fair better than your predecessors." Bryce DeWitt was there. Raymond Chao. There was only one mention of anything to do with the sponsor — someone said this was the first time he'd heard of talks motivated by the phil-anthropic principle. I'd attend a conference like that regardless of who funded it — Satan, the Pope — it was a great conference!

10. **Kea**
September 22, 2009

Well, I am fairly certain that Roger Penrose is also skeptical of such multiverse ideas.

11. **anon**
September 22, 2009

Marcus,

The Templeton foundation just provides the funds for a proposed conference. They don't have any say in who is invited. I attended and it was a good workshop with a lot of participants for and a lot against the multiverse idea. Peter, I bet if you attended one of these workshops you might actually soften your stance a bit on the whole multiverse issue. I got the impression that even Ellis did and he's probably even a bigger sceptic than you.

12. **Peter Woit**
September 22, 2009

anon,

The problem with Templeton funding is not that they choose the speakers, but that they use their money to try and legitimize a pseudo-scientific subject as science, a project then carried out by legitimate scientists. I believe that the majority of physicists consider most multiverse research to be pseudo-science, and would object to a conference about it being funded out of standard peer-reviewed sources.

From Sean Carroll's account, of the many speakers there, the only one who spoke about the problem of multiverse research not being science was Ellis (were there others?), and you say the conference may be causing him to soften his stance. From your account, the Templeton money is having the desired effect….

13. **anon**
September 22, 2009

Peter,

Roger Penrose, Robert Brandenberger and a number of philosophers where talking against the multiverse. Some speakers like Joe Silk where taking a more neutral position. I disagree with you about the multiverse being pseudo science. I think it provides the only solution to the cosmological constant problem. Until someone comes up with an alternative explanation I think people will be interested in the multiverse. the hope is that eventually some other signature will be found. People once said "don't study quarks because you can never see an individual quark so its not science". I think its good to be skeptical but then there are also advantages to keep an open mind on things. I don't agree with religion being mixed with science but as long as the templeton foundation keeps a hands off approach to the projects they fund, I think it is nice to have the opportunity to have a conference about some slightly more speculative issues as well the usual hard nosed stuff.

14. **Peter Woit**
September 22, 2009

anon,

The cosmological constant is about the only "prediction" of the multiverse anyone seems to be able to come up with. Here's my alternative theory of the cosmological constant: "I have absolutely no idea whatsoever what determines it, so it's equally likely to be any value". That theory makes exactly the same experimental predictions as the multiverse theory. They're both equally vacuous, and if I were to organize a conference devoted to my theory, it would be a conference devoted to pseudo-science.

This is really the heart of the matter: unless your "multiverse theory" is something with non-trivial content, i.e. it makes different predictions than the "I have no idea what is going on" theory, all you are doing is cloaking your ignorance in grandiose nonsense and trying to promote yourself as doing science when you aren't.

The argument about quarks is silly, this has nothing to do with the problem of direct vs. indirect observability. The quark hypothesis was a non-trivial one that made a lot of strong, testable predictions which worked. If Gell-Mann had started going on about his quark theory, which was functionally the same as the "I have no idea what is going on in the strong interactions, could be anything", people would have (rightly) dismissed him as a crackpot.

15. **anon**
September 22, 2009

Peter,

I disagree with your description of the multiverse solution to the cosmological constant problem. If there is a potential landscape which covers a broad enough range of values then somewhere the overall vacuum energy will be small enough to allow galaxies to form. Inflation then provides a natural mechanism to populate the landscape and then string theory KKLT extra dimension stabilization provides the landscape. There are several ways this scenario could get into difficulty. For example if gravity waves are detected by PLANCK and supersymmetry is detected by the LHC, see recent papers by Linde and Kallosh. Also if SNAP (or one of its reincarnations) finds w not -1 that will also be hard to incorporate into the above picture. Yes that is not directly falsifiable of the landscape but I really think this Popper view of science is very overly simplistic. In practice I think science works more by induction and finding supportive evidence than falsification. Also another interesting ways that more supporting evidence could be found for the landscape is in studies like http://arxiv.org/pdf/0712.2454 .

16. **Peter Woit**
September 22, 2009

anon,

Do you really believe that the din of rejoicing from string theorists if supersymmetry is found at the LHC would be affected at all by a subsequent discovery of gravity waves by Planck? Or that string theorists will proclaim string theory dead if SNAP finds w not equal to -1?. Of course not. In both cases, finding string theory "scenarios" that could reproduce whatever is found is not likely to be hard, if it hasn't already been done.

The Hall et. al. set-up is rather complicated and I haven't followed its details. From spending a little time looking at it I don't see any use of any fundamental theory of the multiverse, their fundamental multiverse theory is effectively the same theory as my "I got no idea what is going on" theory. They do seem to be doing something potentially interesting, putting into the anthropic mix some information about the physics of electroweak-symmetry breaking, but, again, I haven't invested the time to follow the details.

By the way, a more useful sort of multiverse conference would be one that dumped some of the philosophers in favor of physicists, and concentrated on examining this issue of whether the "string theory landscape" is capable of being tested in any conventional manner. All claims I've seen so far about this appear to be bogus, sorting them out and seeing what's really there would be a public service.

17. **anon**
September 23, 2009

Peter,

The multiverse explanation of the size of lambda is not the same as a theory saying "I have no idea of what lambda is". Such a theory would have a prior with a uniform distribution of lambda over some large range of values. The multiverse calculation puts the prior probability of lambda being proportional to the fraction of matter that collapses into galaxy sized objects or larger. This gives a prior distribution whose peak is quite close to the Omega_lambda \approx 0.7 that is observed. This was done BEFORE the supenovae measurements surprisingly showed Omega_lamda \approx 0.7s, see eg
http://arXiv.org/pdf/hep-th/0511037 for a history and references. With a bayesian analysis your "I have no idea of what lambda is" would be hugely disfavored compared to the multiverse calculation.

18. **Peter Woit**
September 23, 2009

anon,

By "prior" I mean the probability distribution of values that you get out of your underlying model of the multiverse, before you fold in your favorite way of doing anthropics, selection effects, etc. In the string landscape model of the multiverse, people take this distribution to be flat over the relevant range.

19. **Shantanu**
September 23, 2009

Peter or anon or others, Could one of you point whether we can test anthropic predictions with Tevatron or LHC?
Thanks
shantany

20. **Peter Woit**
September 23, 2009

Shantanu,

Depends what you mean by “anthropic” predictions. Predictions based on no fundamental theory, just anthropics, can’t possibly be wrong, they are true by definition.

If you take as your fundamental multiverse theory my “I don’t know what’s going on, it’s all equally likely” theory, you can get some sorts of statistical predictions, e.g. that we won’t observe things to be out in the tails of statistical distributions. What the string landscape people do is start with this, then when something is found to be out in a tail (e.g. proton decay), they say “well, we don’t actually know what the distribution is”.

There are no predictions about anything at any energy (Tevatron, LHC, Planck) that the people promoting this kind of research will stand behind, saying “if we don’t see this distinctive experimental signature of my theory, it’s wrong”. This is why it’s pseudo-science.

21. **anon**
September 24, 2009

Peter,

I would prefer to say its work in progress. Yes its not producing testable predictions at the moment, but its an interesting area worth exploring. Maybe one day there will be some testable predictions, but it may take time. You may think that its not worth exploring, others have a different view.

22. **Peter Woit**
September 24, 2009

anon,

The reason there are no testable predictions about cosmology is the same reason there are no testable predictions about anything else. People have been working on this for 25 years, and everything they have learned has just made it clearer and clearer that this is an inherently vacuous conceptual set-up that can never predict anything. There’s some point at which, if you refuse to admit the failure of an idea you are working on and insist on keeping going, you become a pseudo-scientist. That has happened already in this field.

23. **Robert L. Oldershaw**
September 24, 2009

Sad, but true.

Maybe it is time to back to go back to basic science based on testable predictions. And if tests give negative results, one does not “adjust” the model, but tries new ideas.

Speculation is fine by me, so long as it is clearly labeled as such and not treated as ‘the next big thing’. Unfortunately we have been conflating beautiful science like General Relativity with junk bond science like “string theory”.

RLO

24. **anon**
September 24, 2009

Peter,

I’m not a string theorist but I think you are being too harsh. Research into such large energy and time scales is obviously going to be quite open ended. I don’t think you or anyone else can say it will or wont eventually become a testable theory. My impression as an outsider is that string theory is the best theory of quantum gravity and its intersection with the multiverse and inflation may eventually lead to some very fruitful results.

25. **Peter Woit**
September 24, 2009

anon,

Like most outsiders, your impression reflects very well the hype being put out on this subject, often funded by Templeton. Again, it would have been much more helpful to science if the conference you attended was not devoted to hyping pseudo-science, but instead to a serious examination of the what is known about attempts to describe the multiverse using the string theory landscape. Enough is known to make it very clear that it is a dead-end. Even most string theorists I believe now have begun to agree about this. The people promoting string theory pseudo-science are often not string theorists….

26. **Giotis**
September 24, 2009

Peter, what is the majority opinion? At the end of the day this is what counts. If somebody is not crazy but the majority thinks he is crazy, they are gonna lock him up no matter what. So unless the majority of physicists thinks otherwise string theory ‘hype’ as you say is not a pseudo-science.

27. **Peter Woit**
September 24, 2009

Giotis,

Based on my discussions with many scientists, string theorists and not, I’d say that string theorists are now divided between those who think there still is hope for 10/11d unification and those who have given up on it as a lost cause and are now doing AdS/CFT or something else, with perhaps this last group approaching a majority. Non-string theorists generally take the attitude that string theory itself is too technical for them to evaluate, however they almost uniformly recognize that invocation of the anthropic multiverse is an admission of failure.

Take a look at the “multiverse” conferences and see who is funding them. They are rarely funded by granting agencies that peer-review grants, since getting such a proposal past a peer-review panel is very difficult. Instead they’re funded by organizations like Templeton whose explicit mission involves the promotion of pseudo-scientific ties between religion and science.

For some similar data about this, see this quote:
“in his current view, the effort to regard superstrings as a fundamental theory of everything was a blind alley. Later that year I related Charles’ pronouncement to string theory colleagues on three continents and solicited their own opinions. About half of them agreed with him, more often the younger people.”

from page 58 of this review article

http://arxiv.org/abs/0907.4238

28. **anon**
September 24, 2009

Peter,

I think you are indulging in a bit of sophistry here. Ok you don't like string theory, and some of your arguments are perhaps persuasive, but I think a lot of what you are saying, especially about the sociology, is quite misleading as well. Anyway I'm exhausted with these blog based arguments and will now get back to my own slightly quieter area of research.

29. **Peter Woit**
September 24, 2009

anon,

I can see why you might not trust my views on the sociology, but I don't why you believe that Richard Woodard is lying.

I urge you to look into this for yourself: don't ask the people who attended that conference what they think, virtually none of them know much about string theory. Ask experts who have worked on string theory what they think about the string theory landscape/multiverse. If you do this outside the Bay area, I suspect you might find the result enlightening.

Media Commentary

September 24, 2009
Categories: Uncategorized

Tonight will be the premiere of a new TV series called *Flashforward*, based on a novel with a plot that involves the Alice detector at CERN. CERN has put up a web-site about this, to reassure people that CERN isn't about to change time around. The web-site is along the same lines as the one they put up about Dan Brown's Angels and Demons, to reassure people that CERN wasn't producing quantities of antimatter that could be used in a bomb.

Dan Brown has a new novel out this week, entitled The Lost Symbol. The plot evidently revolves around a researcher in "Noetic Sciences", who is quite the expert on "What the Bleep" pseudo-science, as well as string theory. Here's where she learns that string theory was known to the ancients:

> …I want to study cutting edge THEORETICAL physics. The future of science! I really doubt Krishna or Vyasa had much to say about superstring theory and multidimensional cosmological models."
>
> "You're right, they didn't." Her brother paused, a smile crossing his face. "If you're talking superstring theory …" He wandered over to the bookshelf yet again. "Then you're talking about THIS book here." He heaved out a colossal leather-bound book and dropped it with a crash onto the desk. "Thirteenth-century translation of the original medieval Aramaic."
>
> "Superstring theory in the thirteenth century ?!" Katherine wasn't buying it. "Come on!"
>
> Superstring theory was a brand new cosmological model. Based on the most recent scientific observations, it suggested the multidimensional universe was made up not of THREE … but rather of TEN dimensions, which all interacted of vibrating strings.
>
> Katherine waited as her brother heaved open the book, ran through the ornately printed table of contents, and then flipped to a spot near the beginning of the book. "Read this." He pointed to a faded page of text and diagrams.
>
> Dutifully, Katherine studied the page. The translation was old-fashioned and very hard to read, but to her utter amazement, the text and drawings clearly outlined the EXACT same universe heralded by modern superstring theory – a ten-dimensional universe of resonating strings. As she continued, she suddenly gasped and recoiled. "My God, it even describes how six of the dimensions are entangled and act as one?!" She took a frightened step backwards. "What IS this book?"
>
> Her brother grinned. … "The complete Zohar."

(Thanks to Greg Sivco for the transcription).

Perhaps CERN-TH may want to put up another Dan Brown web-site at CERN to reassure people that the strings in 10d stuff has nothing much to do with reality and isn't likely to lead to whatever trouble it leads to in the novel.

For more on this, Salon has a book review entitled Dan Brown swaps pseudohistory for pseudoscience.

Comments

1. **Chris Oakley**
   September 24, 2009

   Oddly reassuring that. The next time someone tries to convince me that Dan Brown's writings are based on fact, I will be able to point to this. I suppose the story must have a counterpart to Silas (the Opus Dei fanatic in *The Da Vinci Code*). Who could fulfil that role, I wonder?

2. **jpd**
   September 24, 2009

   "the complete zohar" is that a new saying,
   similar to "the whole enchilada" ?

3. **Greg Sivco**
   September 24, 2009

   Thank you, Peter.

   Dan Brown is a highly intelligent man and a product of one of the Phillips Academies and Harvard and that is was vexes me … that he should have read more and not promoted a theory that is quite obviously: dust … save for Susskind, Polchinski, Witten (diplomatically), Motl, et. al. .

   The quote Peter mentions is a "flashback" when Katherine was 19 (she's 50 in the "real time" of the novel, heh, not far off from Peter and me), so doing the Arithmetic (not even Math), the year where that conversation goes down is 1978. Whatever. QCD was the sex then, and QCD was the first thing that kicked ST to the curb.

4. **roland**
   September 24, 2009

   Man, Dan Brown. That is so lame.

5. **Frank Quednau**
   September 24, 2009

   This sounds utterly unreadable. People interested in history probably feel the same with the other books

6. **Chris W.**
   September 24, 2009

   Michael Crichton has a similar pedigree, and also produces schlock for mass consumption. I think they both know what they're doing, even if their readers don't. Indeed, Crichton wrote a piece for *Science* a few years ago in which he responded to scientists' complaints about how they're portrayed in the movies by saying, in essence, "Just get over it; you can't expect anything better from this medium. That's not what it's about."

7. **H-I-G-G-S**
September 24, 2009

Michael Crichton had. Not has.

8. **Yatima**
September 24, 2009

Dan Brown. Pah! Lovecraft was there first ..

"A room was easy to secure, for the house was unpopular, hard to rent, and long given over to cheap lodgings. Gilman could not have told what he expected to find there, but he knew he wanted to be in the building where some circumstance had more or less suddenly given a mediocre old woman of the Seventeenth Century an insight into mathematical depths perhaps beyond the utmost modern delvings of Planck, Heisenberg, Einstein, and de Sitter."

Not half bad for a story written in 1932.

9. **Jeff McGowan**
September 25, 2009

I believe Brown went to Amherst, not Harvard, FWIW. Probably only means something to those of us who went to Hampshire 🙂

I tried to read a page of Da Vinci code, yow it was painful. Actually beyond the "so bad it's funny" level of bad, which I guess is some sort of accomplishment…

10. **David Schaich**
September 25, 2009

Unfortunately, it means something to those of us who went to Amherst as well.

I had hoped that the long delay between books meant that someone was forcing Brown to, you know, check facts and stuff. Guess not. Yow.

11. **Arun**
September 26, 2009

Its a work of fiction!

12. **Greg sivco**
September 27, 2009

Very good Jeff and David, quite correct he graduated Amhurst in '86. His chief protagonist is Robert Langdon, Professor in "Symbology" at Harvard.

TLS IS a work of fiction, an action-adventure treasure-hunt thriller in the traditions of the films "Indiana Jones" and "National Treasure" and the Bond films. Brown however emphasizes a higher degree of Intelligence from the reader, such that those of average IQ who complete the book will feel themselves so much smarter than "scientists."

Because "real" scientists reject Noetic Science, and Brown makes them feel smarter for having read the book. I mention the book because with 5 million copies in the first printing (which I do believe they will sell), I fully expect the book to be the most read in the next month, and just wish to prepare us for having this book and its conclusions brought up at dinner parties and other social get-togethers in the next month or two.

Among the odder "Noetics" in the book is an experiment to measure the mass of the human soul. A dying man is put into an adult sized incubator similar to one we use with premature babies, with a highly sensitive scale. After he passes, his weight noticeably decreases by micrograms as his soul escapes. So says Noetics.

I have finished the book and like it for what it is … fiction. Brown himself is no conspiracy theorist … the Scottish Rite Masons come off rather well except for the villain, who is mentally ill but sane enough to do great damage. The best part for me are the many puzzles it presents, most of which I could solve before Brown spills their beans 2 chapters ahead. The worst part for me is the New Age-presented-as-fact stuff. But in order to criticize I feel it important to read the book.

"Truth is stranger than fiction; Fiction has to make sense." … Leo Rosten

Not necessarily.

13. **Vronsky**
September 29, 2009

An [appropriate comment] on the Dan Brown comments.

14. **Janne**
September 29, 2009

"Non-Euclidean calculus and quantum physics are enough to stretch any brain; and when one mixes them with folklore, and tries to trace a strange background of multi-dimensional reality behind the ghoulish hints of Gothic tales and the wild whispers of the chimney-corner, one can hardly expect to be wholly free from mental tension." -H.P. Lovecraft (The Dreams in the Witch House: And Other Weird Stories)

15. **Chris Long**
October 2, 2009

Look mac, these ain't just higher dimensions, they're wider, more luxurious dimensions. You interested? Best you'll find on the black market.

16. **Ken McKenna**
October 9, 2009

The Zohar (Hebrew: זֹהַר, lit Splendor or Radiance) is the central work of Kabbalah. It is a commentary on the Torah, written in medieval Aramaic. The Zohar is not one book, but a group of books; these books include scriptural interpretations as well as material on what some of its adherents term theosophic theology, mythical cosmogony, and mystical psychology. Its scriptural interpretations have been described as an esoteric form of Midrash (Rabbinic commentary on the Tanach), a description that is consistent with the view of many Kabbalah cultists that it is the concealed part of the so-called "Oral Torah."

In its own weird way the Zohar advances a view of the nature of God, the origin and structure of the universe, the nature of souls, redemption, the relationship of “true self” to the “light of God,” the relationship between the “universal energy” and man, and God-knows-what-else. In short, the “Complete Zohar” is a really rich literary and religious compost.

But there’s no string theory there. A pity, that. Otherwise, I suppose, Madonna might be a string theorist. And while she may be a bit over the hill as an entertainer, her involvement might have really livened things up.

The Edge of Physics

September 24, 2009
Categories: Uncategorized

Nature this week has two stories about the Perimeter Institute. There's a long one entitled The edge of physics, which emphasizes Perimeter's wealth and success, starting off:

> Working at the Perimeter Institute for Theoretical Physics comes with certain perquisites. Whenever recruits arrive at the Toronto airport, for example, they are met by a limousine and driven west along Canada's Route 401 into the rich farmlands of Ontario. Eighty-five kilometres later, the limousine works its way through the streets of the town of Waterloo, and lets them out in front of a sleek building of black, green and glass squares that stands next to a pond in Waterloo Park. Stepping inside, the recruits find wall-to-wall blackboards, working fireplaces, a sauna, multiple dispensers of free coffee and the Black Hole Bistro, which serves free lunches on Wednesdays.

Neil Turok is the new director, and he plans to double the full-time faculty from 12 to 25. The institute already has more theory postdocs than anywhere else in the world (44) and is aiming for a research staff of 250, including visitors. For comparison, the Princeton IAS has 5 permanent faculty in physics and about 20 postdocs. Perimeter has an endowment of 200 million Canadian dollars, a figure they hope to double.

The same issue has a review by Joao Magueijo of Howard Burton's book about his experience as first director of Perimeter (my own review is here). Magueijo's take on Perimeter is rather scathing, seeing it as a "sad tale", having sold out on its original anti-establishment concept:

> The institute's aim was to "make waves, big waves", and it got off to a promising start. Burton — a youthful outsider who had only just finished his physics PhD went about his job with maverick flair, challenging the scientific establishment, attacking its tribalism and allergy to innovation. Here was an opportunity to do things differently: to promote originality, to flatten hierarchy and empower the young researchers actively driving the field. It sounded utopian, but it was worth a try.
>
> Unfortunately, reality failed to comply with Burton's plan. The best days of this haven of free-thinking came while it was still a 'theoretical' theoretical physics institute — before the scientists arrived. The anecdotes Burton narrates in the chapter 'The Trouble with Physicists' ring hilariously true. But there was also a fatal flaw in Perimeter's concept — scientists tend to define 'originality' as what they personally do. Soon the institute's quest for novelty became hijacked by the agendas of the field's usual culprits, and Burton himself came under attack from them….
>
> Burton tried to replicate the US establishment in Canada, but he was often outbid and exploited by opportunists who used Perimeter as a trampoline to boost their US careers.
>
> By the time Perimeter matured, five years later, the divide between the quixotic first hires and the new wave was painfully evident. The openness of the early days was replaced by Princeton-style hush-hush and invitation-only meetings. The idealists openly confessed that they wished they could find another benefactor, to "start anew and this time do it right". Something had gone wrong: the sought utopia had become a dystopia.
>
> Scientific originality has become big business: being anti-establishment sounds great. Yet few want to take the risks necessary to achieve it. Originality is encouraged in public pronouncements only to be punished when practical decisions are made. Perhaps Perimeter's tale proves that there is no recipe for original science: it happens anarchically and by accident, in spite of, rather than because of, scientific institutions.

Update: Sabine Hossenfelder, who recently spent three years at PI, has her take on the Nature articles here.

Comments

1. **tom s.**
   September 24, 2009

   Speaking as someone who knows nothing about the internals or personnel at PI, how does one tell brilliant, quixotic, anti-establishment hires from flakes? I do remember that the first public talk was by Roger Penrose, whom I have always thought closer to the second.

2. **tytung**
   September 24, 2009

   1. It's hard to distinguish anti-establishments from cranks
   2. The established people are unhappy that such anti-establishment young people can be allowed to possess such a good environment/opportunity.

3. **peter shor**
   September 24, 2009

   The "right" way to do anti-establishment science is first to get tenure by doing brilliant establishment work, and then to do your truly original, anti-establishment stuff. Otherwise, everyone will ignore you because you don't have any credentials.

   This is easier said than done.

4. **volunteer**
   September 25, 2009

   @peter shor

   Yes, but what about grants?

5. **Peter Jackson**
   September 25, 2009

   What Perimeter really needs to be doing is not just ensuring nice jobs for scientists to persue their own agendas, but something no academic institutes do, properly check out what's really out there.
   At present everything original gets thrown into the crackpot pit if it's not direct from academia (and some that is!) without even a look.
   Perhaps the best unification of GR and QM we have is Einsteins comment that we dont yet "know 1,000th of 1% of what nature HAS REVEALED to us" and the Quantum tendancy for everything that CAN happen TO happen. Somewhere out there the answer lies, someone, who perhaps thinks differently, has found it, yet it's all

being ignored and consigned to the pit without a glance.
It just needs a policy and minimal time checking over models and evidence proffered. It should be the duty of all staff to spend say an hour a day doing so!
Perimeter has become as self serving and non-inclusive as most other institutes. This includes, yes, the Royal Society, which started with free thinkers like Sir Christopher Wren, but is now as packed with clones as anywhere else. I hope Neil Turok can get back to fundamentals, but I'm not holding my breath.

6. **bane**
September 25, 2009

Peter Shor's comment may well be true in "no scutwork or lab equipment" disciplines like mathematics, theoretical physics and theoretical computer science. In other areas of research, volunteers comment about grants is highly significant.

As a side-bar, I think it's probably noticeably easier to find a place to be "orthogonal-establishment" (with respect to your immediate colleagues, not the whole field) rather than "anti-establishment": if the rest of the faculty don't really understand what you're doing but it looks to have solid foundations then they're generally reasonably supportive. On the other hand if your work is, however politely, suggesting what they're doing is misguided then there's a lot more criticism. I've sometimes wondered if that's why some of the most "original" thinkers seem to be situated in strange departments in smaller places where everyone else in the department seems to work in other areas: if they were somewhere bigger or with more related faculty they'd probably face more friction.

7. **Bee**
September 26, 2009

Hi Peter,

Thanks for the link.

Tom, Tytung, Peter Shor:

Using "anti-establishment" as a criterion to select promising scientists it a completely misguided attempt. It is not only easy to fake, it isn't any scientific criterion whatsoever. It just sometimes happens to be a beneficial attitude for those scientists who chose to take the path less traveled, mostly out of a sense of self-protection. After all, they are smart people who know they are making their life unnecessarily complicated, so there's a tendency to rationalize that with an anti-authoritarian, anti-establishment, anti-something philosophy. That however is neither a necessary nor a sufficient ingredient to find people who have the qualification and courage to work on what the NSF calls "transformative" research (and what others like to call risky research).

There is a very nice song by a German group called "Rebell" (rebel), the chorus being "I am against it, no matter what it is." Unfortunately, this is an equally dumb attitude as brainlessly swimming with the stream.

There are a whole bunch of other criteria that I have found frequently being thrown around, and that are nothing but secondary criteria that are to some extend correlated with what one is actually looking for, yet shouldn't be confused with it. You can count to that "independence" which is easily confused with being unable to work with collaborators, and "broadness," a frequent change of topics, which for some people happens because they jump on any boat that crosses their path.

Bottomline is, there is no way to summarize the necessary characteristics for a scientist likely to cause a great breakthrough with a couple of adjectives.

Best,

B.

8. **steph handel**
September 26, 2009

is garret lisi perhaps the only successful "anti-establishment" physicist who has been affiliated with PI?

9. **Steve Satak**
September 26, 2009

I believe it was CS Lewis who wrote, in a passage I cannot now find, that the more original a person tried to be, the less likely he was to achieve his goal. The truly original, if you ask them, are found to have been focusing on the task at hand, *not* being 'original'.

10. **Phil Warnell**
September 27, 2009

Hi Steph,

"Is garret lisi perhaps the only successful "anti-establishment" physicist who has been affiliated with PI? "

I don't know if the term "anti-establishment" is the right word , rather than unorthodox or ones not following the current trend. If taken as the latter Lee Smolin could be seen as such a person or Lucien Hardy, both of which are not simply associated with Perimeter, yet are actually faculty. If you want a more poignant example it would be someone like Antony Valentini ,who after completing his doctorate spent many years in the wilderness outside of mainstream academia. I believe it was Lee Smolin who was instrumental in having him come to Perimeter as a visitor, which turned out to a visit of a few years. After this he struck out on his own again and received some funding from a a source that is designed to lend such people assistance. One should also be mindful that Einstein spent a few years working in a patent office until his papers of 1905 changed his destiny and with it the course of physics.

Best,

Phil

11. **Bee**
September 27, 2009

Last time I looked, Garrett wasn't affiliated with PI.

12. **Erik Anderson**
September 27, 2009

I suspect that Magueijo is right. PI is going totally mainstream. PI's mission now seems more focused on accruing prestige than than supporting self-directed individuals who are unaccommodated by existing research programs.

Speaking of G. Lisi, does anybody know what ever became of his idea to establish “Science Hostels” ?

13. **Peter Woit**
    September 27, 2009

    I’ve just deleted several content-free comments about Garrett Lisi, both anonymous attacks or claims for his genius. If you have something interesting to contribute about Perimeter, please do so. If you have a stupid comment about Garrett, find someplace else…

14. **Phil Warnell**
    September 27, 2009

    Hi Erik,

    I suspect that Magueijo is right. PI is going totally mainstream.

    When Howard Burton’s contract as director of Perimeter wasn’t renewed, I would say it marked a point in its evolution and not so much indicative of a change in direction. Burton acted mainly as an instrument of Mike Lazadisis’ vision and I would say he did a commendable job a far as the initial stages where concerned.

    Likewise, I see Turok representing simply the one chosen to shape Perimeter through the next phase of its development. Its current expansion demonstrates the continued commitment to the vision and yes the growing pains will continue. I would ask you to be more specific, as to name names of who you would recruit as faculty and those as post docs. I find Magueijo’s comments not much more than sour grapes and find him to hold no less of an elitist attitude than those he criticizes. I don’t mean this so much as a criticism of him, yet more to acknowledge that as demonstrated by many sharp minded, free thinkers, comes a feeling that one’s direction being the only correct one.

    One could say that the vision of what Perimeter represents to be is the product of one man and that being Mike Lazardisis, which continues to be the case. The thing that Lazardisis’ vision is focused around is expressed in what he conveyed to Burton when he hired him; that being he believes the world is on the verge of something groundbreaking and earth shaking and he would like to help expedite its fruition. Mike is a man of action and not one of analyzing methodology, past knowing that in any endeavour or enterprise of scale one is required to have a clear vision, which must be shared by those who are to bring it about.

    Thus I argue that the main tension we are feeling here is that since Mike is not one of those who will actually have it come to be, he should have no part other than to be the banker. I would counter this opinion with asking if Neil Armstrong would have stepped on the moon as early as 1969 without having the force of vision and will supplied by John F. Kennedy. One thing I can assure is if Mike contunues and is allowed to play his role PI will become the place and accomplish the things he has imagined. This dynamic will assure it, as the normal model of academia has seldom been able to manage, for it places goals above the people who serve achieve them and not the other way around.

    Best,

    Phil

15. **Erik Anderson**
    September 28, 2009

    Phil,

    Thanks for your thoughtful remarks. It just so happens that last March I did send a letter of recommendation in support of an applicant for a PI faculty position. Suffice to say that the limousine was not summoned. You may now wonder if I too have a sour-grapes complex, but that’s not where I’m coming from.

    I agree with the notion that we are long overdue for groundbreaking innovations in theoretical physics, but I don’t believe that the endeavor can be properly compared to the Apollo program. Cultivating innovation has little in common with a massively blueprinted engineering project. Nor is lavish spending required. Frugality, on the same budget, would go so much further.

    You have a nice selection of favorite books listened on your webpage. May I recommend to you Norbert Wiener’s classic “Invention: The Care and Feeding of Ideas” (a manuscript from the 1950’s published in 1993)? The culture of ‘Megabuck Science’ was coined by Wiener to describe pathological tendencies of industrial science. Woit’s and Smolin’s most recent books have convinced me that parallel tendencies have spilled over into the theoretical sciences as well.

    Kind Regards,

16. **Kea**
    September 28, 2009

    Just a warm bed, a desk and enough food to eat. Is that too much to ask?

17. **Phil Warnell**
    September 28, 2009

    Hi Erik.

    First let me thank you for your considered response. Thanks also for the books you recommend and I will surely have a look. I would most certainly agree with you that one can’t directly compare the race to the moon with fundamental research, for as you remind the former being more the development of technology, rather the new science; although there was some of that as a result.

    No what I meant mostly is that Lazardisis is the one who holds the vision and the will to maintain it, which I find is something common to both. The other thing I believe they share, is each has to reach some critical mass before they begin to show promise. This current expansion has more to do with growing the scope of the specialties, rather than expanding the numbers in of each discipline and I find that to be a good thing.

    To be honest, I find if anything has changed, not for the better, is the public outreach component of PI’s vision ,of which I’ve been a grateful benefactor almost since it’s beginnings. From my own observations it has shifted under from the direction of Burton , who emphasised the humanizing of scientists and along with it there science, to mainly being a PR imitative to promote and guarantee funding. I find this to be a mistake for I believe that science’s success is ultimately best achieved when a greater sector of the populous not only see it as a methodology for expanding our knowledge, but also as one for guiding us through every day life’s decisions. I’m thus reminded in this regard when Carl Sagan warned *“It is suicide for a society that depends on science and technology to know nothing about science and technology.”* . This I also find to be a important part of the mission to realize PI’s vision, for which I think Burton had the better feel.

    Best,

    Phil

18. **Chris W.**
September 28, 2009

Lest his last name be confused with an obscure medical disorder, here is the correct spelling of Mike’s name:

— Mihalis “Mike” **Lazaridis**

19. **Thaler**
September 28, 2009

> Just a warm bed, a desk and enough food to eat. Is that too much to ask?

If that’s all you need, there are plenty of places offering it. But I suspect that you, and most people, want much more than just that.

Most people want their warm bed, desk and food to be placed in Oxford, Cambridge, Princeton, Stanford or CERN. And that’s a completely different business.

20. **Erik Anderson**
September 28, 2009

Hi again, Phil

each has to reach some critical mass before they begin to show promise.

Doubtful in the latter case. Which massive programs of support produced Kepler, Newton, or Einstein?

Genius springs forth at the level of the individual — not collectives. So if philanthropy can be massive, then it ought to be massively parallel, widening the diversity of individual pursuits. This mode of support, I would think, would best enhance the prospects of genuine discoveries.

But as a rule, money goes where money is. If Lazaridis wishes to make PI into another Princeton, then the redundancy he thereby creates does not advance the cause.

– E

21. **Kea**
September 28, 2009

If that’s all you need, there are plenty of places offering it.

Oh, yeah? WHERE?

22. **Andy**
September 28, 2009

tytung,

only cranks cannot tell non-establishment scientists from cranks.

23. **Jarmo Makela**
September 28, 2009

Steve Satak,

I think that the quotation from C. S. Lewis you are referring to, is this:

“Even in art and literature, no man who bothers about originality will ever be original: whereas if you simply try to tell the truth (without caring twopence how often it has been told before) you will, nine times out of ten, become original without ever having noticed it.” (C. S. Lewis: “Mere Christianity” p. 190 (Macmillan Publishing Company, New York, 1960))

24. **Erik Anderson**
September 28, 2009

Kea,

Perhaps Thaler was making an oblique reference to *homeless shelters*. 😉

Lisi’s ‘Science Hostel’ concept sounded more promising.

At any rate, I agree with the spirit of your previous post. Dedicated researchers do not have a dire need for limousines, glass facades, saunas, fireplaces, etc. But I would add one item to your list of essentials: good Internet connectivity.

Cheers,

– E

25. **Chris Oakley**
September 28, 2009

Erik,

I disagree. *Poor* internet connectivity is far better. Then one might get some work done rather than pointlessly leaving comments on Science Blogs. Oh … but it looks as though … never mind … too late now (my own theoretical phys research is pretty much on ice anyway).

I agree with C S Lewis about originality. Seeking out “original” researchers is only likely to turn up cranks. Equally, looking for the “big ideas” is also, IMHO, doomed to failure. Bear in mind that the non-universality of time and space was only generally accepted decades after Lorentz wrote down his transformations. And Heisenberg tried very hard to avoid non-commuting operators before finally acquiescing to matrix mechanics. No: I think that one should only take the “big” idea on when one is certain that nothing else will do.

26. **Thaler**
September 28, 2009

> Oh, yeah? WHERE?

Peripheral places. Maybe Northern Ireland, or Bosnia, but those are still Europe. Provincial universities in Latin America or Southeast Asia (Colombia, Peru, Philippines, …) I have no idea about Middle East or Africa.

Many, if not most, of those places don’t advertise positions in Physics Today but some of them are willing to hire. You might try going to a conference in the region and finding out what’s available.

The break is over, I’ll submerge now…

27. **weichi**
September 28, 2009

Isn’t all this talk about how to organize research groups pretty much beside the point? Theoreticians at Perimeter face the same problem faced by theoreticians elsewhere: history has demonstrated that unless your name is is Albert Einstein and you are working on a relativistic theory of non-quantum gravity, you aren’t going to make any important theoretical breakthroughs in the absence of new experimental results.

28. **Kea**
September 29, 2009

Thaler

Thanks!! I do look for these jobs, all the time, but they are not advertised. I might just find some conferences in peru or south east asia to go to! Of course, travel costs will be tricky to arrange … but maybe I will manage.

29. **Phil Warnell**
September 29, 2009

Hi Erik,

Which massive programs of support produced Kepler, Newton, or Einstein?

Not so much programs, rather technology, which in this case came with the advent of the printing press. And yes I would agree that breakthroughs are often exclaimated by what happens to present as being a few individuals. However, this hypothesis quickly starts to show cracks if you try to say that those you mentioned could have done it alone, that is without the accumulation of the thoughts and ideas of others. I personally believe we have reached a juncture where the problems we are presented with will only be solved by the collaboration of many minds, along with their ideas and not be attributable to those of any one individual. One could argue that Quantum Mechanics and its extensions marked the juncture where this became first to be evident.

As for Perimeter simply representing being redundant, as such places having already been realized, I would point out it expands the overall numbers able to remain dedicated to fundamental research, instead of them being forced to find employment in things for which they haven’t been specifically trained for or more importantly what they enjoy. I also find it funny that many envision Perimeter somehow as being the Ritz hotel of research centres, for although it has excellent resources and perhaps a few niceties, it does provide what the average person would call a lavish existence. I can see many here have expressed the opinion that without some pain there can be no gain, with which I might agree if you were talking about athletes rather than scientists.

Best,

Phil

30. **Phil Warnell**
September 29, 2009

Hi Erik,

” for although it has excellent resources and perhaps a few niceties, it does provide what the average person would call a lavish existence. ” </i?"

Sorry that should have read:

” for although it has excellent resources and perhaps a few niceties, it doesn’t provide what the average person would call a lavish existence. ” </i?"

This is one reason I prefer blogger over wordpress, as I could have simply erased the whole thing. Thus I find technology can make a difference:-)

Best,

Phil

31. **Mark Stuckey**
September 29, 2009

If PI is simply trying to serve as a launch site for young theorists (nothing wrong with that, but it is a duplication of effort), the theorists will have to work in established areas to guarantee success when they return to the “heat bath.” You shouldn’t really expect these researchers to find a new paradigm under those circumstances. If PI rather wants to facilitate the development of a new paradigm in physics (whatever that might be), they can drill or refine. Right now PI is drilling. Rockefeller made his fortune refining (oil in his case), letting others take the risk of drilling. PI could rather serve as a refinery, which would be something new in the community and it would have a higher probability of contributing to whatever becomes the next paradigm in physics.

32. **Marcus**
September 29, 2009

and it would have a higher probability of contributing to whatever becomes the next paradigm in physics.

will there be a next paradigm if nobody drills?
It sounds reasonable that one design a strategy to be more certain of having contributed, after one recognizes what the next paradigm was, after the fact. I'm skeptical though. Sounds like playing for credits rather than what Robert Frost was talking about—"for heaven and the future's sakes".

33. **Erik Anderson**
September 29, 2009

Phil,

"*this hypothesis quickly starts to show cracks if you try to say that those you mentioned could have done it alone, that is without the accumulation of the thoughts and ideas of others*."

But that's *not* what I tried to say. The pertinent moral is that none of these gentlemen made their mark as a result of some massively *collaborative* research program. They unquestionably had good access to previous knowledge (unfairly delayed in Kepler's case), but were otherwise working on their own.

Marcus,

"will there be a next paradigm if nobody drills?"

Well put. Individual 'explorers' do still exist, though no practical incentives exist to be one. The risk they assume is total. Institutions, however, are risk-averse. Hence the disconnect which Magueijo describes.

The next paradigm is inevitably coming. But it will surely be brought about by a risk-taker — not by practical "men of action."

Cheers,

34. **Haelfix**
September 29, 2009

PI is a worldclass facility. Most everything there is modern and schick and it probably costs a nice penny for the administrators and Canada.

Unfortunately the truth is that in science, going down establishment roads typically yields far more steady, consistent results and revenues than 200 wild idea people, where 1 person of the lot has a legitamate breakthrough every ten years or so.

Particularly so in fundamental theory, where there is no lucrative patent at the end of the process that could recoup the massive losses.

They made the correct decision by bringing in Turok and a few establishment people to balance things out. Of course this will irratate some, but its really the only way for the place to make economic (not necessarily scientific) sense in the long run

35. **weichi**
September 29, 2009

In the driller/refiner metaphor, aren't the drillers represented by experimentalists, and the theorists in general are the refiners? So that the risk-taking you want to encourage is by *experimentalists* at least as much as it is by theorists?

Of course, funding experimentalists is a lot more expensive than theorists (especially the high-energy kind), and their duds are a lot more obvious …

36. **Erik Anderson**
September 29, 2009

No, the role of experiment in this metaphor is to test whether a new well has produced real oil or snake oil. 😉

37. **Phil Warnell**
September 30, 2009

Hi Erik,

My point being that we have entered a stage where collaboration has to be increased, both within and between the disciplines, with PI marking an effort where this is being attempted as a methodology. I would say it might also help if they expanded their faculty to include foundational mathematics and scientific philosophy. The trick of course is how to have these people interact positively or better yet even want to. This is the challenge that lays at the heart of the success or failure of it all, that is as far as I'm concerned.

Best,

Phil

38. **J.F. Moore**
September 30, 2009

"The best days of this haven of free-thinking came while it was still a 'theoretical' theoretical physics institute — before the scientists arrived."

This seems to me a really strange statement. Is there a missing word I wonder, or are there actually theoretical physicists who don't consider themselves scientists?

39. **Peter Woit**
September 30, 2009

J.F. Moore,

I think the author meant that the best days were when it was just a concept, before theorists were hired and it became a reality…

40. **Erik Anderson**
September 30, 2009

Phil,

Put that way, I think your point is an excellent one. The need for an interdisciplinary outlook today is acute. Meanwhile, the breadth and sheer bulk of today's knowledge is such that one wonders if a single individual can ever acquire enough of it.

To solve the trick of making it possible for people to productively share knowledge, I suggest that this mode of collaboration must have organic roots — i.e., spontaneously grown from the bottom-up — not orchestrated from the top-down. It's a matter of which people want to associate with whom *in the first place*, and then: can they all be brought together. It's *not* a matter of hiring top-experts in various fields and hoping for the best. Indeed, the kind of experts needed must include those who are deeply *dissatisfied* with their own respective fields — which is to say: unrecognized experts.

Cheers,

– E

41. **Bruce Bartlett**
September 30, 2009

Just to mention that Neil Turok has, of course, been the driving force behind the African Institute of Mathematical Sciences (AIMS), which he essentially founded six years ago. AIMS is situated just outside Cape Town in South Africa and basically offers a nine-month postgraduate training programme for students from all over the African continent in the math sciences (that includes physics!). Nowadays there are also opportunities for postdoctoral researchers. It's been a big success, so kudos to Neil Turok. Off hand, if anyone is interested in coming to sunny Cape Town (world cup next year!) and delivering a three week postgrad lecture course, or simply spend some research time at the institute, visit the website.

42. **Peter Woit**
September 30, 2009

Thanks Bruce,

I was thinking of trying to get some mention of Turok's role in AIMS into the posting but didn't quite get around to it. Thanks for mentioning it here, AIMS seems to be a remarkable institution that deserves more attention and support.

43. **Andy**
September 30, 2009

Interesting and important institution, the African Institute of Mathematical Sciences (AIMS), and certainly ambitious—I quote from their job opening "Director: AIMS Next Einstein Initiative":

"By developing scientific talent in Africa, AIMS-NEI aims to discover people of rare creative genius, capable of revolutionary advances in various fields of human endeavour. By adding entrepreneurship and leadership skills to the existing AIMS curriculum, AIMS-NEI aims to catalyse the creation of spin-off companies and thereby to help generate wealth creation. As well as the Next Einstein, we hope to discover the Next Gates, Brin and Page in Africa."

44. **Chris W.**
September 30, 2009

The next paradigm is inevitably coming. But it will surely be brought about by a risk-taker — not by practical "men of action." (Marcus)

Very often, practical "men of action" seem to regard themselves as the real risk-takers, and regard the supposed theoretical paradigm-creators as impractical dreamers who can't function in the "real world" — right? That certainly seems to be true in the corporate world, which academic institutions increasingly resemble.

Here is a question: Why wasn't Albert Einstein dismissed as a crank, albeit an unusually talented and articulate crank? It seems that some of his eminent elders had to be willing to go out on a limb for his work to be published and acknowledged. His confidence and perhaps his sanity could have been crushed simply by being ignored for a couple of decades.

45. **Erik Anderson**
September 30, 2009

Here is a question: Why wasn't Albert Einstein dismissed as a crank, albeit an unusually talented and articulate crank?

Because… Einstein's ideas did not threaten anyone else's entire career? Even as late as 1929, Michelson was *still* hunting ether with an even *larger* interferometer.

46. **Marcus**
September 30, 2009

Chris W.
You have a quote there about "the next paradigm" which is actually not from me but from Erik
http://www.math.columbia.edu/~woit/wordpress/?p=2313&cpage=1#comment-50444
which is perfectly all right with me. Hardly worth mentioning, unless Erik objects.

I like the point you made which was essentially about the seriousness and honor of Einstein's community elders—the scientific ethos that prevailed at the time.

I have no way of measuring to what extent, if any, our scientific culture is different from those German Victorians of the class of 1890-1910.

But you are telling me that some cultural intangibles helped to motivate Einstein and get him recognized. He expected that the scientific world would "play fair" and take an objective look, even if he contradicted received wisdom.
And he was right. The physics community of the time did play fair. It recognized that he had done something important and it rewarded him.
This is probably worth a lot, even compared with having lots of fellowships to support unconventional research.

Giving an outsider recognition *at the possible cost of one's own prestige or one's friends' prestige* being open-eyed to recognize the possible merit of an alien idea, is an act of seriousness. In our culture it may be that keeping one's own prestige, funding, and public image intact is more important than that kind of integrity. The "we're the only game in town" mentality, where anything positive said about a rival approach is met with hostile rebuke.

Should be possible to measure these differences in scientific culture by some objective means, and compare different historical eras. I wouldn't know how or if it has been done. But your second paragraph was pertinent. Thanks

47. **Chris Oakley**
October 1, 2009

Read Einstein's Mistakes for a warts-and-all account of the great man and the environment in which he (eventually) prospered. He did not actually get particularly good grades but his relentless energy and curiosity won the professors over. This is something that could not happen now as the technical bar is so much higher. Creative students with less than excellent grades now would be weeded out very early on. This is probably inevitable, but I am sure that something is lost in the process.

48. **A.J.**
October 1, 2009

The next paradigm is inevitably coming. But it will surely be brought about by a risk-taker — not by practical "men of action." (Marcus)

It's not clear to me that people fall clearly into one category or the other. Planck and Schrodinger, for example, both had long histories of "establishment" work before they found themselves with the opportunity to do something extraordinary.

49. **Erik Anderson**
October 1, 2009

I am enjoying the ongoing momentum of this discussion (no big deal that a snippet of what I wrote keeps being attributed to Marcus).

To A.J.,

Ironically, the importance of Planck' 1900 discovery was not initially recognized by Planck himself. He presented it as a curious phenomenon — not as an epoch-defining discovery. Planck's demeanor was thoroughly unassuming. He did not even believe in the atomic hypothesis until very late in his life.

Who are the risk takers? The risk takers are those theorists who, in the course of their research, make definite predictions which can be incontrovertibly arbitrated by future experiment. The risk is, of course, that experiment can rule against them.

Those who are not risk takers are those theorists who immunize themselves from any such liability. Their theories are sprinkled with enough free parameters to weasel out of any experimental result. These folks are not in the business of sticking their necks out; they are in the business of covering their asses.

50. **Peter Woit**
October 1, 2009

Erik and others,

Please, this discussion has gotten too far from any significant connection to the topic of this posting, the Perimeter Institute.

Investigating the Nature of Matter, Energy, Space and Time

September 29, 2009
Categories: Uncategorized

On Thursday on Capitol Hill, the House Subcommittee on Energy and Environment of the Committee on Science and Technology will hold a hearing with the title [Investigating the Nature of Matter, Energy, Space and Time](). Witnesses will be Hugh Montgomery of the Jefferson Lab, Lisa Randall of Harvard, Pier Oddone of Fermilab and Dennis Kovar from DOE. A webcast of the hearing should be available.

To brief the subcommittee, someone put together the hearing charter available [here](). It does a reasonably good job of explaining at a popular level what particle and nuclear physicists are working on and what problems they are trying to solve. Unfortunately the part of the document on particle physics is marred by some stale string theory hype, with the subcommittee told that:

> Unification was Einstein's great, unrealized dream, and recent advances in a branch of physics known as string theory give hope of achieving it. Most versions of string theory require at least seven extra dimensions of space beyond the three we are used to. The most advanced particle accelerators may find evidence for extra dimensions, requiring a completely new model for thinking about the structure of space and time…
>
> Understanding the very early formation of the universe will require a breakthrough in physics, which string theory may provide.

Selling the US investment in machines like the Tevatron and the LHC as being about extra dimensions seems to me to be a mistake. Very few physicists believe it likely that this is what the LHC is going to find, and the failure to find promised extra dimensions at the LHC will not be helpful in a few years when the US particle physics community is trying to convince Congress to fund a next-generation accelerator.

The charter doesn't explain what the LHC is really good for and why physicists are so excited about it: finally the energy-scale of electroweak symmetry breaking is being reached, with the promise of finding out what sort of physics is behind this phenomenon and responsible for mass. There's no need for string theory hype to justify the interest and importance of this sort of very fundamental research, bringing in failed highly speculative ideas is likely to actually be counter-productive. The case for the current and planned US particle physics program is a very strong one, I hope the witnesses are able to make it clearly and forcefully to the subcommittee.

Update: The hearing is going on now, with a break for a vote. The webcast is [here](). The first question from Representative Vern Ehlers, a physicist, was for Lisa Randall, and he asked if there is any experimental proof or corroboration of string theory. Instead of answering the question with a straightforward "No", and explaining that string theory makes no predictions, Randall did what she could to obfuscate the issue. She answered by going on about how, while string theory was speculative, it has led to ideas testable at accessible energies: supersymmetry and large extra dimensions. I suspect her answer left Ehlers and others still confused about the issue he was asking about. Avoiding public acknowledgment of the failure of string theory unification seems to extend even to Congressional testimony….

Update: There's a press release [here]().

Update: The video webcast of the hearing is now available from the hearing web-page.

Comments

1. **Adam Helfer**
   September 29, 2009

   While (I agree) the charter is generally a good one, and really as good as one can expect for something like this, as long as we're caviling:

   The charter repeatedly identifies questions about "the nature of matter, energy, space and time" with particle physics. What happened to relativity?

   It's hard to see how particle physics tells us more about the nature of space and time than relativity (since particle physics requires relativity for its formulation); and while one can argue that somehow quantum-field-theoretic Hamiltonians tell us truly deep things about energy, it's not clear that these should be regarded as more important than understanding energy in general relativity….

   Of course this view in the charter is no very serious matter. It would have been nice to see something a bit more balanced, though — presumably the document was drawn up by staffers in consultation with physicists, who could have demonstrated a broader perspective.

2. **capitalistimperialistpig**
   September 29, 2009

   I worry about the chances for selling fundamental physics to Congress when a large fraction of it seems to be stuck somewhere in the dark ages. It's hard to pitch the quest for knowledge to those who are clinging to ignorance with both hands and teeth.

   The traditional gimmicks – national security and nation prestige – look a little shopworn by now, not to say slightly dishonest. The only notion that looks solid to me is the claim that the frontier of science is also the frontier of technology. The backwoods crazies might not understand that but their corporate sponsors might.

3. **Greg Sivco**
   September 30, 2009

   Interesting choice of witnesses. Lisa Randall's testimony may be key, as she is first, from Harvard, which is highly respected in the halls of Congress, and second, an attractive blond, which definitely interests Congressmen.

   Anyone know her current stand on String Theory? I'm sure she still wants Supersymmetry and the Higgs boson to be proven, but besides that she's always struck me as strangely yet diplomatically 50/50 on Strings (before it was fashionable to be so), accepting just enough of it to support hers and Sundrum's 5D Gravity brane theory. I got that impression from reading her book Warped Passages and her 56-min. Charlie Rose interview, which I must review as its been almost a year since I watched it. Here it is again, for reference:

   http://video.google.com/videoplay?docid=-45154219728824809&sourceid=searchfeed%20#

   Finally, I hope at least one of them mentions the (mild but significant) brain drain from America to Europe since the SSC's cancellation as well as the USA's obsession with Strings. Fingers crossed.

   Also, i hope at least

4. **Bee**
   September 30, 2009

   "In your experience, how important is it in obtaining funding that your project or research area is well covered in the media?"

   Very important/somewhat important: 23.5%

   Results of a survey conducted by UCSB survey center in April 2009 among active researchers in physics in North America. The final number of respondents amounted to 1816, which corresponds to response rate of 14.42%.

5. **Marcus**
   September 30, 2009

   That is a telling 23%. Makes one think. Is there an online source for that survey report? Or some online article that refers to the finding?

   The question sounds as if it could have two different interpretations.
   How important is media coverage to funding *of your field collectively as a whole?*

   versus

   How important is media coverage to obtaining funding *for your own individual project?*

   I wonder if there are tax-base (national or regional) differences in the role played by media in science funding.

   I don't feel like judging anybody on this score, approvingly or dis. To the extent the perception is real it places a burden of responsibility on the press. Relying on the honesty and integrity of the press does not work consistently in politics. One wonders…

   Is life better in Sweden?

6. **Gphillip**
   September 30, 2009

   I just watched a program on the American TV cable "Science" channel titled "Parallel Universe". It turned out to be a one hour propaganda film about how string theory has already solved all the questions of Physics. While I object to calling this mess a "theory", as it really seems to be more of a hypothesis, I'm really more concerned that with propaganda like this, another generation of great physics minds will be sucked down the black hole of the "theory" that explains anything, real or not. I've never seen a PR campaign like this in science. It's astounding.

   Lisa Randall is just being diplomatic and I understand that. Speaking ill of string theory in public is professional suicide in most circles. But to propagate this unproved hypothesis to the public and even to the halls of Congress as the only path to a theory of everything is to bind physics to a theory that may never lead to the advancements that physics promises.

   I feel like I'm living in the Dark Ages and anyone who claims that string theory isn't, and probably can't be proven (or disproven), must be burned at the stake as quickly as possible. In time, this will all pass and other ideas will be considered and tested. I just feel sad for all the young minds destined to be wasted chasing this Holy Grail.

7. **Peter Woit**
   September 30, 2009

   Gphillip,

   By now there are quite a few of these sci-fi shows hyping string theory in rotation on the various science TV channels. The one you mention is due to the BBC, and from 2002. My impression is that by now most string theorists find these to be an embarrassment, but not enough of one to do anything about trying to get them off the air. One of them may be a source for whoever put together tomorrow's hearing charter…

8. **sh**
   September 30, 2009

   Shouldn't they have used a different ordering of what to investigate at least?

   http://en.wikipedia.org/wiki/MEST_%28Scientology%29

9. **J. Reay**
   October 1, 2009

   Frankly, any argument that will precipitate funding from Congress for basic science is a welcome effort. Kudos to those who fight for basic research dollars!

10. **Bee**
    October 1, 2009

    Hi Marcus,

    No, life isn't better in Sweden, but at least it's life.

    Unfortunately, the survey results presently aren't available anywhere . I'm sitting on piles of data with the best intention to bring them into a useful format. So stay tuned, more details are to follow. Best,

    B.

11. **capitalistimperialistpig**
    October 2, 2009

    Perhaps the Congressman should have asked Randall to name one specific experiement that bears on the validity of string theory, and explain how it does.

12. **Shantanu**
    October 2, 2009

Peter or others, Is the video available? I tried to view the webcase, but could not.
Also Peter, what do you think of the fact (in the last year) that experiements with wrong results such as DAMA are getting more citations than most collider/accelerator phsyics experiments? Is there really such a dearth of data in experimental HEP.

Mathematics and Religion

October 1, 2009
Categories: Uncategorized

Unlike physics, mathematics has managed to remain immune from efforts to promote pseudo-scientific agendas, financed with the goal of mixing up science and religion. I don't see any reason to believe this is going to change, but I just noticed that the Templeton foundation is funding a program here in New York later this month on the topic of Mathematics and Religion.

The program will take place at the Philoctetes Center, which is run out of a townhouse on the Upper East Side and supports a variety of activities that you can read about here. The organization ran into serious trouble with its funding recently since its investments were managed by Bernard Madoff. A year before the scandal broke, Philoctetes sponsored a panel discussion (accessible here) on *The Future of the Stock Market*, which featured Madoff as a panelist. Because of these losses, the Center has had to look for funding elsewhere, and has found some from the Templeton Foundation.

One notable thing about the Mathematics and Religion panel is that it doesn't include much at all in the way of mathematicians. Of the six participants, one is Max Tegmark, a physicist prominently involved in Templeton-funded multiverse studies, but the only mathematician is Edward Nelson. Nelson is quite far from the mainstream of mathematics, with a religion-infused recent paper entitled *Warning signs of a possible collapse of contemporary mathematics*, available here. Unlike the case of multiverse pseudo-science, which has drawn support from leading figures in the physics community, this sort of point of view about mathematics has attracted zero interest among mathematicians.

The Mathematics and Religion panel isn't any threat to mathematics, and is part of a larger and much more worthy program about mathematics at Philoctetes funded by Templeon. In November there will be a panel discussion on Mathematics and Beauty that sounds interesting, I might even try and make it over there to see it (last year I did attend a talk at Philoctetes given by Barry Mazur). The Mathematics and Religion panel is associated with something more serious, a talk by Loren Graham on his book Naming Infinity. It's a book I read earlier this year, but don't think I ever got around to writing about here on the blog. I wasn't completely convinced by some of the claims it makes about the relation between religious practices and the work of certain Russian mathematicians. The story it tells about the religious sect of "Name Worshipers" and the history it recounts of one part of the Russian mathematical community are quite fascinating.

Comments

1. **Bee**
   October 1, 2009

   "Certainly, if one needs to believe that beyond the appearances of the world there lies a permanent and transcendent reality, there is no better choice than mathematics. No other conception of reality has led to so much success, in practical mastery of the world. And it is the only religion, so far as I know, that no one has ever killed for."
   ~ Lee Smolin, The Life of the Cosmos

   Sorry, couldn't resist 🙂

2. **ObsessiveMathsFreak**
   October 1, 2009

   Is this a good time to bring up the tale of Hippasus and the Pythagoreans?

3. **Peter Woit**
   October 1, 2009

   OMF,

   No.

4. **Chris W.**
   October 1, 2009

   From that essay of Edward Nelson:

   > The most impressive feature of Cantor's theory is that he showed that there are different sizes of infinity, by his famous diagonal argument. But Russell applied this argument to establish his paradox: the set of all sets that are not elements of themselves both is and is not an element of itself. Actually, Russell's paradox was in response to Frege's work, not Cantor's. Frege gave a clear and precise account of his work, making it possible for Russell to show it was wrong, whereas Cantor's work was in parts so vague and imprecise that, as Pauli said of another theory, it was not even wrong.

5. **Per**
   October 1, 2009

   The paper by Edward Nelson was really nice. I had never heard about it before, thanks for sharing it. It's nice with some sort of fresh trippy take on contemporary mathematics. God knows it's lacking in physics…

6. **inc.**
   October 1, 2009

   I had a brief interaction with E.N. some time ago; I certainly respected him on the basis of his papers. Hmm, unfortunate that he would find himself in such company.

7. **Janne**
   October 2, 2009

   The title reminded me of Gödel's ontological proof.

8. **Aleksandar Mikovic**
   October 2, 2009

   Bee´s quote of Lee Smolin's apology of platonism was a pleasant surprise, because very few physicists are platonists. This is contrary to mathematicians, where a lot of them are platonists, which is understandable.

The reason why physicist are not platonist is the idea that everything can be explained from a finite set of elementary constituents (elementary particles, or strings, or something else) moving in space, subject to a finite set of laws of motion and structure. However, when analysing this framework, one has to decide whether the laws of motion are independent entities or they are simply random regularity patterns in the motion of the elementary constituents.

If one chooses the second option, this means that our world can disappear tomorrow and that the ultimate explanation for any phenomena is that it is a random fluctuation. On the other hand, the first option leads to platonism, i.e. there exists a separate realm from spacetime, where abstract ideas like mathematical structures and laws live. Furthemore, it is then natural to identify our physical world with a mathematical structure, as Max Tegmark has proposed, which explains Wigner's question of unreasonable effectivness of mathematics in physics.

Now, why still many physicist do not accept a platonic metaphysics and stick to the chaotic universe metaphysics? My guess is that in a platonic metaphysics the idea of God naturally appears, and many physicist are opposed to it because it is an unscientific idea. However, it is an irony that the scientific metaphysics, i.e. a materialistic metaphysics, leads to the conclusion that the ultimate explanation for anything is that it is a random event, which is contrary to the very spirit of science.

9. **mark a. thomas**
October 2, 2009

It should be noted (myth?) that Hippasus was strangled or drowned by the Pythagorean order for revealing to the world the irrational construction of the dodecahedron. To keep the nature of mathematics divine they then set their sights on the continuum rather than that of the discrete whole numbers. It seems ancient maths had connections to religions or mysteries.

10. **Benni**
October 2, 2009

It seems that Nelson does indeed want to publish real mathematical work. Yet what he wants to write seems not to be ready.

He has a better discussion of his program here. The content is the same, however this is aimed at mathematicians:

http://www.math.princeton.edu/~nelson/papers/hm.pdf

There, one can read that he wants to give a proof of something which he is still working on.

Maybe he goes to such conferences since he is somewhat struggling with his proof.

11. **Benni**
October 2, 2009

Nelson writes in

http://www.math.princeton.edu/~nelson/papers/hm.pdf :

The goal is to produce an explicit superexponentially long recursion and prove that it does not terminate, thereby disproving Church's Thesis from below, demonstrating that finitism is untenable,
and proving that Peano Arithmetic is inconsistent.

It seems that he has problems with this and then he goes to templeton conferences.

12. **Joao Leao**
October 2, 2009

(Aleksandar Mikovic says:
"Now, why still many physicist do not accept a platonic metaphysics and stick to the chaotic universe metaphysics? My guess is that in a platonic metaphysics the idea of God naturally appears, and many physicist are opposed to it because it is an unscientific idea. However, it is an irony that the scientific metaphysics, i.e. a materialistic metaphysics, leads to the conclusion that the ultimate explanation for anything is that it is a random event, which is contrary to the very spirit of science.")

I do take issue with the notion that platonism in particular or idealism in general imply theism. Plato was quite careful in separating god from (the) good and should not be punished by what the neoplatonists and the doctors of the church made of his thought. (The same may be said of the frequent but abusive "equation" of materialism with atheism). That may explain why, even with a majority of platonists among mathematicians, Templeton has not been able to recruit any mathematicians to its campaign gatherings, as Peter noticed. God is NOT a mathematical idea!

13. **Benni**
October 2, 2009

By the way, I think that Nelson's claims in http://www.math.princeton.edu/~nelson/papers/hm.pdf

"The goal is to produce an explicit superexponentially long recursion and prove that it does not terminate, thereby disproving Church's Thesis from below, demonstrating that finitism is untenable,
and proving that Peano Arithmetic is inconsistent."

are quite ambitious. Since the link above does not contain that much fuzzy religious statements as Nelson's other paper, can someone report what is about those claims:
http://www.math.princeton.edu/~nelson/papers/hm.pdf

From what I see, he is indeed using only logic. Yet I'm only a physicist.

So: can some real mathematician report, if Nelson's program has any chance to succeed? Or are there errors in the above pdf?

I'm just curious. Benjamin

14. **Vidkun**
October 2, 2009

Now, Nelson is surely something of a philosopihcal extremist, but he is without any doubt a sound mathematician; he has many results throughout mathematics, logic and mathematical physics.

Also the "problem of induction" in the talk linked led him to develop his Predicative Arithmetic. I don't think many besides Nelson himself regard it as relevant to the foundations of mathematics, but his results were used when others (Sam Buss for example) began developing Bounded Arithmetic, which has strong connections to complexity theory and (variations of) the P=NP problem. Hence, the "zero interest among mathematicians" is rather unfounded (if mathematical logicians are to be included).

I like to view Nelson as a prime example of how philosophically untenable positions could lead to valuable results. (Compare how Einstein's and Heisenberg's dogmatic positivism gave us Relativty and Matrix Mechanics.)

15. **Mark Stuckey**
October 3, 2009

"If a 'religion' is defined to be a system of ideas that contains unprovable statements, then Gödel has taught us that, not only is mathematics a religion, it is the only religion that can prove itself to be one." J.D. Barrow in Between Inner Space and Outer Space, Oxford University Press, 1999, p 88.

16. **BigG**
October 3, 2009

Mark,

That is not a good definition of religion because there are many counter examples that are certainly not religion. If you accept this definition than philosophy and physics would be religions which is ridiculous. The point of a definition is to show somethings' defining characteristic; what makes it different from what is not it.

The definition of religion is a system that contains a doctrine of liberation; i.e. a soteriology.

17. **Mark Stuckey**
October 3, 2009

BigG,

I don't know about philosophy, but here is a quote about physics:

"Many scientists are deeply religious in one way or another, but all of them have a certain rather peculiar faith – they have a faith in the underlying simplicity of nature; a belief that nature is, after all, comprehensible and that one should strive to understand it as much as we can. Now this faith in simplicity, that there are simple rules – a few elementary particles, a few quantum rules to explain the structure of the world – is completely irrational and completely unjustifiable. It is therefore a religion." Sheldon Glashow in The Quantum Universe, co-produced by WETA-TV and The Smithsonian Institution, 1990.

18. **BigG**
October 3, 2009

Mark,

Does Sheldon Glashow of J.D. Barrow saying something make it true? I'm interested in following the idea to its conclusions. The original definition you posted said a "system of ideas that contains unprovable statements." By your logic its hard to find a system of thought that isn't a religion. Why? Because all systems of thought have some unjustifiable assumptions. Physics assumes the validity of the experimental method. Can physics use the experimental method to establish the experimental method? Can light illuminate itself? Can darkness obscure itself?

19. **Peter Woit**
October 3, 2009

Any more comments about religion will be deleted. The internet is full of other places those interested in this kind of thing can have this kind of discussion.

20. **Tony Smith**
October 4, 2009

Bee quoted Lee Smolin as saying that "… mathematics … is the only religion, so far as I [Lee Smolin] know, that no one has ever killed for …".

Whether or not math may have been involved in violent persecution of Pythagoreans, etc,
it seems clear that Nazi Germany did severely persecute what it defined as "Jewish mathematics". In his book "History of Mathematics: A Supplement" (Springer 2007) Craig Smorynski said:
"… the change of mathematical direction … would reach an extreme in the 1930s with the nazi distinction between
good German-Aryan anschauliche (intuitive) mathematics
and
the awful Jewish tendency toward abstraction and casuistry.
…
the proponents of this distinction had to dance some fancy steps in explaining how the abstract mathematics of David Hilbert … was not the bad abstraction of the Jews. …".

Tony Smith

21. **Maynard Handley**
October 4, 2009

"
The story it tells about the religious sect of "Name Worshipers" and the history it recounts of one part of the Russian mathematical community are quite fascinating.
"

Uhh, no. Perhaps the story could be fascinating, but the actual book Graham produced is, let's face it, a waste of time if you are interested in the mathematics. We learn that there are some bizarre orthodox christian practices, that some Russian mathematicians were strongly orthodox and interested in these practices, and that some of the mathematicians were gay.

What we NEVER learn, which would have made the whole exercise worthwhile, is exactly what these Russian mathematicians did that was so spectacular compared to western mathematicians. Sure, we are told over and over again that name worshipping led to mathematics that was too "daring" for French and German and US mathematicians, but we are never told WTF that mathematics was.

This ridiculous, to put it bluntly, c**k-teasing approach to writing history has put Graham on my list of authors to be permanently avoided. Don't waste my time for 150 pages and then fail to deliver the only damn thing that made me interested in your book in the first place!

(BTW do not be confused. Loren Graham is NOT Lauren Graham!)

22. **Elbadudedansky Brodudensky**
October 4, 2009

You can't corrupt math, although it's not because there aren't mathematicians who wouldn't corrupt it if it could be corrupted.
Numerous mathematicians become just as illogical and biased as the pack, once they stray from math.

What can be corrupted is the application of math, and many people confuse the application of math with math.

Because the most useful of math is tied into the real numbers, the foundation of which is standard set theory, standard logic, and the natural numbers, then how could any corruption of note not be logically tracked down? A corrupt construct would most likely be at a higher level than the real numbers, and anything at a lower level, what would be the purpose of that? Very few people have a beef with the way we count or with the law of the excluded middle; it might be better to say that very people would even want to have the awareness that they could be aware of such things.

Also, other than the general status of "mathematician," there's no glamor in math. It's like a foreign language with thousands of dialects, where those fluent in one dialect can't communicate in most of the other dialects.

People will sit for hours listening to talks about physics and astronomy, because it involves the physical world, and there's something to try and visualize. With math, there's no pleasure in hearing someone talk in a foreign language; it's no fun to have your eyes glaze over.

And most people barely tolerate useful math. Why would they tolerate useless math? Especially if it tweaked the useful math in a bad way.

But you don't have to corrupt math to corrupt math-based science. You just have to find a set of unproved assumptions and a mathematical model that will do what you want it to do. The logic behind the math will be solid. And in fact, given your unproved assumptions, why couldn't you get the desired conclusions? At most, you might merely need some additional unproved assumptions. With the right credentials, you'd be good to go.

23. **Elbadudedansky Brodudensky**
October 4, 2009

In the above, please feel free to replace "real numbers" with any other popular and useful algebraic structure.

Bourbaki Archives

October 2, 2009
Categories: Uncategorized

I'd recently been wondering whether the archives of the Bourbaki group would be put on-line, and today noticed that there's a project to do so, with results available here. One can read copies of "La Tribu", internal reports on the activities of the group, up through 1953. There are a wide variety of interesting mathematical documents, often consisting of attempts to write up one subject or another, efforts that sometimes made it into the published books, often not.

One subject that Bourbaki struggled with over the years was that of how to set out the foundations of differential geometry. My colleague Hervé Jacquet likes to tell about how Chevalley at one point made an effort to do so, with the peculiar starting point of defining things in terms of "cubes". I wasn't sure whether to believe him, but here it is. According to Borel, in 1957 Grothendieck presented the group with his own take on the question of manifolds:

> Grothendieck lost no time and presented to the next Congress, about three months later, two drafts:
>
> Chap. 0: Preliminaries to the book on manifolds. Categories of manifolds, 98 pages
>
> Chap. I: Differentiable manifolds, The differential formalism, 164 pages
>
> and warned that much more algebra would be needed, e.g., hyperalgebras. As was often the case with Grothendieck's papers, they were at points discouragingly general, but at others rich in ideas and insights. However, it was rather clear that if we followed that route, we would be bogged down with foundations for many years, with a very uncertain outcome.

I don't see these documents on the list, perhaps documents from the later years are still to appear.

The documents often start out with some unvarnished comments, here's an example, from Chevalley's report on a text about semi-simple Lie algebras:

> Au moment d'écrire ces observations, je me demande si ce ramassis des méthodes les plus éculées et les plus pisseuses, ces résultats les moins généraux possibles établis de la manière la plus incompréhensible possible, ne sont pas un canular intrabourbachique monté par le rédacteur. Même s'il en est ainsi, je me laisse prendre au canular et présente les observations suivantes.

Comments

1. **Marcus**
   October 2, 2009

   éculées –down at the heels, shabby
   canular — hoax

2. **monolingual pleb**
   October 2, 2009

   Curiosity got the better of me so I googled "babelfish" and got the following translation of that french stuff:

   "At the time of writing these observations, I wonder whether this bunch of the most worn down methods and more the baby girls, these results the least general possible benches in the most incomprehensible possible way, is not a hoax intrabourbachic assembled by the writer. Even if it is thus, I let myself take with the hoax and presents the following observations."

   Perhaps our blog host can let us know how faithfully it represents the original french.

3. **Tom O'Bulls**
   October 2, 2009

   It's deeply interesting, and extremely telling, that Bourbaki couldn't handle the most important and profound branch of modern mathematics, namely differential geometry. They ought to inscribe this passage on Bourbaki's tombstone.

4. **Marcus**
   October 2, 2009

   Au moment d'écrire ces observations, je me demande si ce ramassis des méthodes les plus éculées et les plus pisseuses, ces résultats les moins généraux possibles établis de la manière la plus incompréhensible possible, ne sont pas un canular intrabourbachique monté par le rédacteur. Même s'il en est ainsi, je me laisse prendre au canular et présente les observations suivantes.

   As I write these observations, I ask myself if this grab-bag of outworn and infantile methods, these results with the least possible generality established in the most incomprehensible manner, isn't a Bourbaki in-house hoax perpetrated by the writer. But even if it is, I will let myself be taken in by the hoax and present the following observations.

   I think pisseuses refers to infants of the wet-diaper type—pissy ones—an earthy expression.

5. **Marcel**
   October 3, 2009

   Concerning the volume on differential deometry, here is a short extract from a 1990 French radio programme in which Laurent Schwartz and Jean-Pierre Serre shed some light on the matter (at least for those who understand French): http://www.fileden.com/files/2009/10/3/2591723/schwartz_serre.mp3

6. **T.**
   October 3, 2009

   Marcus's translation is perfect, including his interpretation of "pisseuses" (sic).

7. **J**

October 4, 2009

The definition in Chevalley's Bourbaki draft is in fact not contrived in any way: it simply specifies a (smooth) manifold as a space which is locally homeomorphic to an Euclidean space, and which carries a distinguished collection of "smooth" functions, in such a way that _locally_ the smooth functions match (via the local homeomorphisms) the infinitely differentiable functions on Euclidean space. Choosing (the interior of) a cube as the local model of a smooth manifold (as the "standard" piece of an Euclidean space) is certainly among the most natural choices.

Chevalley's definition is effectively the same as that of manifolds as (locally) ringed spaces which are locally isomorphic to Euclidean space with its sheaf of smooth functions, only leaving the machinery of sheaves (of rings) out. Among other things, this definition has the benefit of putting smooth manifolds on into the same framework as schemes, analytical spaces etc – usually define as ringed spaces locally isomorphic to a model (such as the spectrum of a commutative ring in the case of a scheme). It also provides a direct route to "synthetic" differential geometry: carrying out the basic constructions such as those of tangent spaces and connection "algebraically", in the same way they are normally done in algebraic geometry (EGA IV).

The above hints at some of the issues, not entirely straightforward, that Bourbaki was facing: how general (and generalizable) framework to choose? Minimum needed for a few future volumes (such as Lie groups) or a basis for a significant expansion of the book series into different types of geometries? In the end, they only published a "Fascicule des résultats", a quick fix for the former use.

It seems that this outcome was to a large etent due to some of the same difficulties Bourbaki faced in writing a book on algebraic topology (which they never finished): sheaves, and category theory in particular, were difficult to embed into the structure of the series at that stage – it would have needed a major overhaul of much of the earlier volumes. Some rather interesting results of this can be seen in Algèbre Ch. 4, where Symmetric tensor functor is mentioned in subsection headings in spite of functors never having been defined nor appearing in the actual text. And the chapter (10) on homological algebra is a curious example of categorical thinking without categories. And all this in spite of many of the key people behind category theory, sheaves etc. were also key members of Bourbaki.

8. **Peter Woit**
October 4, 2009

J,

I think the reason people don't do it Chevalley's way, specifying a cube in R^n (or any other figure in R^n) is that the intersection of two cubes is not a cube. Surely you can do it Chevalley's way, as well as lots of others, but there are good reasons that way of doing things is unpopular.

Looking at all the Bourbaki drafts of a differentiable manifolds book, I can't avoid the conclusion that the lesson is that one shouldn't let algebraists write books on differential geometry. Bourbaki was dominated by algebraists, and I think this was one one of its major weaknesses. It led them to miss out on lots of important areas of mathematics (including the connections of mathematics to physics via geometry).

Besides differential geometry, where they were pretty hopeless, they also never really did even algebraic geometry. This wasn't their fault though, since the foundations of that subject were in flux at the time.

9. **J**
October 5, 2009

Peter,

Rather too harsh criticism on Bourbaki, I would say.

First, whether one uses cubes or other shapes (or just open sets) as the local models for manifolds is not really crucial to Chevalley's approach – even starting with cubes can always saturate by finite intersections. The inconvenience is of the same order of magnitude that one meets in defining the metric topology using balls defined by the metric: the intersection of two balls is not not a ball either.

Instead, it is the definition of smooth manifolds (or other geometric creatures) in terms of (the sheaf of) distinguished functions on them that is important. While it is to certain extent a matter of taste whether to start from the sheaf of functions or from charts, both viewpoints are quite important. Their equivalence is of course rather trivial – it is more the viewpoint that matters. One should also note that the "usual" definition in terms of charts runs into a rather similar issue with intersections: intersection of (the domains) of two charts is not in general (a domain of) a chart – unless one uses a saturated atlas. Finally, as regards this particular topic in Bourbaki, it is fair to note that they eventually settled for the "traditional" definition, and explain the sheaf viewpoint a few pages later, in the published Fascicule des résultats".

Second, whether one likes his (differential) geometry with more or less algebra is also a matter of taste – I've sometimes had the feeling that differential geometry is too important to be left to people with background too much in analysis… But more seriously, expecting a completely universal coverage of all important topics in mathematics by Bourbaki would of course be unrealistic – even if they had continued at the same pace from late 60's onwards. It is moreover clear from a careful look at the Bourbaki archives that a great many later ababdoned topics were planned for inclusion, so seeing a deliberate exclusion from the project is not really correct. Rather, the project seems to have run out of steam by the early 70's, with many plans unrealised. Specifically for the connections between geometry and physics that you presumably have in mind, they were discovered at a time when Bourbaki had already become rather inactive. Not writing on gauge theory in the sense of Atiyah and Donaldson in the late 60's probably should not coutn against them…

As for algebraic geometry, it is fair to note that the biggest names of the field were Bourbaki members (Grothendieck, Serre, Weil,…). While the subject being in flux was a big reason for a book on algebraic topology getting stuck, I think there was an even clearer reason it made no sense for Bourbaki to write a book on algebraic geometry: Grothendieck's EGA (written in by him and the arch-Bourbakist Dieudonné) was already an essentially Bourbaki-style treatment of the subject. No need to duplicate that.

10. **ObsessiveMathsFreak**
October 5, 2009

Considering that Bourbaki never wrote a book on geometry, it's not too surprising that they never got around to differential geometry. For a group supposedly committed to a rigorous presentation of the foundations of mathematics, it was always a serious omission.

Bourbaki had difficulty with geometries of all kinds because they chose to work through the medium of set theory. While some may disagree with me, my own opinion is that set theory and geometry are fundamentally incompatible at the elementary level. That is to say, they are two independent and unconnected ways of doing mathematics and ne'er the twain shall meet. You really cannot talk about geometric elements like points, lines, planes and circles by describing them as sets (though many do!), and sets cannot be encompassed at all by geometrical descriptions. The fundamental disconnect seems to me to be that sets have no notion of order or position, which is what geometry is all about.

My main point is that there is no one road, royal or otherwise, to every place in mathematics. Bourbaki tried to carve a path to all topics using set theory and they unsurprisingly failed. I cannot be done. Mathematics cannot be reduced a linear axiomatic presentation and moreover we have known this since the days of Godel and before.

This isn't to say that Bourbaki's results have been a waste. The Bourbaki framework and style make it possible to rigorously discuss advanced concepts and structures in a way that simply wasn't available before.

On the other hand, Bourbaki has in some way made it *impossible* to talk about anything without smothering the discussion in formality. To whit; what is a manifold? Or a group? Or better yet, what is the determinant of a matrix?

The answers Bourbaki gives to these questions, while correct, leave something to be desired.

11. **Tony Smith**
October 5, 2009

It may be, as J said, that Bourbaki "… seems to have run out of steam by the early 70's, with many plans unrealised …",
but
from the late 70's onward some very geometrical works, such as the books "Manifolds All of Whose Geodesics Are Closed" (Springer 1978) and "Einstein Manifolds" (Springer 1987) were written by Arthur L. Besse whose wikipedia entry states:
"Arthur Besse is a pseudonym chosen by a group of French differential geometers, led by Marcel Berger, following the model of Nicolas Bourbaki. A number of monographs have appeared under the name.",
so
maybe you could say that the geometric view,
in the form of exposition of concrete examples without obscuration by formal abstraction,
has been carried out by a very close relative of Nicolas Bourbaki.

For example, the preface of the 1978 book describes such visualizable geometric structures as "… les espaces projectifs sur les nombres reels, sur les nombres complexes et sur les quaternions (ainsi que l'esoterique plan projectif des octaves de Cayley) …".

Individuals acknowledged to be related to Arthur L. Besse include:
Genevieve Averous, Pierre Berard, Lionel Berard Bergery, Marcel Berger, Jean-Pierre Bourguignon, Yves Colin de Verdiere, Andre Derdzinski, Annie Deschamps, Dennis M. DeTurck, Paul Gauduchon, Nigel J. Hitchin, Josette Houillot, Hermann Karcher, Jerry L. Kazdan, Norihito Koiso, Jacques Lafontaine, Rene Michel, Pierre Pansu, Albert Polombo, Pierrette Sentenac, John A. Thorpe, and Liane Valere.

Tony Smith

12. **weichi**
October 5, 2009

OT, but no Nobel prize rumor-mongering this year? You're losing your touch, Peter!

13. **Peter Woit**
October 5, 2009

weichi,

I retired from the Nobel prize predictions business after this:

http://www.math.columbia.edu/~woit/wordpress/?p=84

My only prediction for this year is that it's not going to be in particle physics, since that was last year's field, and there hasn't been anything prizeworthy in particle physics for a while now.

14. **Successful Researcher**
October 5, 2009

It's not exactly about the Bourbakis but I just learned some very sad news about another great mathematician: Israel Gelfand is no longer with us.

15. **observer**
October 5, 2009

At 96 and after living a very long and fruitful life M.I. Gelfand (working to the very end) passed away. He has been and will be remembered as one of the few truly great mathematicians of the 20 century. Despite of being jewish, managed due to his mathematical ability to stay in Moscow university even in times of anti-semitism. His mathematical work is truly impressive. Rest in peace.

16. **Deane Yang**
October 5, 2009

I didn't read the definition of manifolds using cubes. I see some attempts at defending this by saying it is no worse than using anything else. I'm not sure I agree. But could someone try to articulate exactly why Bourbaki would think that using cubes was somehow *better* than using, say, arbitrary open sets of R^n?

17. **Christine Dantas**
October 6, 2009

The Nobel Prize in Physics 2009

Charles K. Kao – "for groundbreaking achievements concerning the transmission of light in fibers for optical communication" (1/2 Prize);

AND

Willard S. Boyle and George E. Smith – "for the invention of an imaging semiconductor circuit – the CCD sensor" (1/4 Prize each).

Congratulations!

18. **Peter Woit**
October 6, 2009

I seem to be 100% on my Nobel prize predictions. This one was for something I know absolutely nothing about.

I hope someone more able to do justice to him than me will write about Gelfand. He was one of the major figures in developing twentieth century representation theory.

19. **Successful Researcher**
October 6, 2009

For starters one can look here.

20. **Thomas R Love**
October 9, 2009

Bourbaki did write a volume on differential geometry:

Sorry it took so long for me to find it

Varietes differentielles et analytiques – Paragraphes 1-7 (Elements de…
by Nicolas Bourbaki

from

http://www.librarything.com/work/7952041

21. **Peter Woit**
October 9, 2009

Thomas,

Yes, there is a Bourbaki text on the subject, but it’s short and just what they call a “fascicule de resultats”, not a detailed treatment with full proofs of the sort they produced for other parts of mathematics.

Latest From the LHC

October 7, 2009
Categories: Experimental HEP News

Things have been going fairly well at the LHC, with no major problems encountered recently as the machine is being prepared for operation. The last two sectors (34 and 67) are almost cool (see more about this here). Not mentioned in the CERN Bulletin article is that there has been about a week and a half slippage with respect to the schedule of a month ago, with the current schedule having powering tests finishing in the last two sectors around November 20. Attempts to circulate beams and begin the beam commissioning process should begin shortly after that.

CERN has also recently decided how to handle the media campaign for this second attempt to start up the machine. Unlike last year, there will be no media event associated with the first circulation of beams, just press releases issued at that time, at the time of first collisions at 450 GeV, and at the time the beam energy is raised to a world record (above that of the Tevatron, 1 TeV). There will be a media event planned for first collisions at 3.5 TeV/beam, but the date for this will only be planned about 2 weeks before it happens, and confirmed a day or two before the event. It's possible that this will happen later in December, just before the holiday shutdown, but maybe it's more likely for January. CERN has a web-site set up for the media on this topic, see here, where all they say "The first high energy collisions will most likely occur at a date after mid-December 2009."

In other LHC news, there has been an ongoing campaign to simulate the bad interconnections that are still known to be there in the machine, and these simulations have led to much more confidence that the potential dangers in the case of a quench are understood. The simulations show that operation at 3.5 TeV/beam should be safe, but going up to 5 TeV/beam without fixing the interconnections (which requires warming up the sectors involved) still seems risky.

Comments

1. **P**
   October 7, 2009

   Hi Peter,

   When they say 3.5 TeV / beam, does that mean the energy for each beam or is the collision energy (i.e, 2*3.5 TeV) ?

2. **Peter Woit**
   October 7, 2009

   P.,

   They plan to run at 3.5 TeV/beam, for a total collision of energy of 7 TeV. This is 3.5 times the Tevatron energy (1 TeV/beam), half the LHC design energy (7 TeV/beam).

3. **Steve Myers**
   October 8, 2009

   I caught part of a History Channel piece on LHC. They did show the soldering technique (quickly, though). Looks like pressurized hot box. Might be worthwhile to see the whole show.

4. **zanzibar**
   October 9, 2009

   Here's the lastest news from CERN:

   Headline: "Swiss atom lab: Physicist held on terror links"

   http://www.msnbc.msn.com/id/33241768/ns/world_news-terrorism

   (Nothing but the facts ma'am).

5. **zanzibar**
   October 9, 2009

   And from the BBC

   Headline: "'Al-Qaeda-link' Cern worker held"

   http://news.bbc.co.uk/2/hi/europe/8299668.stm

   Apparently, the suspect is a 32-year old non-CERN collaborator on LHCb.

   He and his brother were arrested in south-east town of Vienne (100 km outside of Geneva).

   CERN is cooperating with French authorities in the investigation.

6. **BDO Adams**
   October 13, 2009

   I'm sadden they haven't fixed the interconnections yet. If there going to have to reheat and recool each section before they can get to 5 GeV. The LHC isn't going to get full power in 2010 at all. Is it? Still there is be plenty of room for new particles from 1 to 7 TeV.

7. **Peter Woit**
   October 13, 2009

   The LHC definitely won't get to design energy (7 TeV/beam) in 2010, they may or may not get to 5 TeV/beam. In any case, even the starting 3.5 Tev/beam is 3.5 times the Tevatron energy, so a new energy range will be begin to get explored. In any case, my guess is that much of 2010 will be taken up with understanding the behavior of the machine and of the detectors, getting both working reliably.

8. **Rob**
October 14, 2009

BDO,

Unfortunately it's not that simple. When you consider the production of new particles at a collider you need to consider the collision as two incoming partons (quarks & gluons), not two incoming protons. Even though the Tevatron has a center of mass energy of roughly 2 TeV, it can only produce particles of roughly a few hundred GeV. This is because it is extremely unlikely for a parton to have a large fraction of the total energy of the proton.

If you take into account the fact that the tevatron has recorded roughly 100 times more data than the LHC will be able to record the first year you'll quickly come to the conclusion that nothing new can be discovered the first year. For example: The tevatron has a greater chance of creating a 500 GeV resonance in 5 inv fb than the LHC does (at 7 TeV) in 50 inv pb.

Unfortunately it's going to be a few years before the really interesting results start coming in.

Sounds Familiar

October 8, 2009
Categories: Uncategorized

From a recent blog posting by economist Brad DeLong, entitled The State of Economics in the 2000s Analogized…:

> But I think there also has to be an explanation in terms of the sociology of academic disciplines. And in that light, it seems to me that if I were a journalist, I'd consider writing a piece comparing freshwater economics to the other major recent case in which an academic discipline went completely off the rails, namely English departments' swing into postmodernism in the '80s and early '90s. Offhand, there seem to be some real similarities, e.g.:
>
> 1. In both cases, the people involved maintained, credibly, that you couldn't really assess the work in question without putting a lot of effort into understanding it.
> 2. In both cases, that required mastering difficult stuff. (In econ, all the math and models; in pomo lit stuff, mastering the literally incomprehensible language in which a lot of that stuff was written.)
> 3. In both cases, that deterred a lot of people on the outside who were generally puzzled and skeptical, but didn't want to spend years getting into a position in which they could credibly say: yes, this is, in fact, nuts.
> 4. So in both cases practitioners were largely insulated from criticism they had to take seriously.
>
> Relatedly, in both cases it took shocks from the outside to expose the problems in this (in the case of English, things like the Sokal hoax; in the case of econ, the near-collapse of the global economy.)
>
> Both cases involved a lot of arrogance, and a generally dismissive attitude towards other approaches. Since, in both cases, practitioners were able to seize significant amounts of control over a discipline before their approach crashed and burned, this did real damage to the disciplines in question (leading to, e.g., large chunks of previous disciplinary history being forgotten.)

In the last sentence DeLong identifies clearly what is most sad and disturbing about this kind of story.

Update: As a commenter points out, the text quoted is from DeLong's blog, but is not his own words, he's quoting someone else.

Comments

1. **Marcus**
   October 8, 2009

   That quote from Brad DeLong is beautifully lucid. Joe Polchinski could have it tattooed on his bottom, for string aspirants to read.

2. **hackenkaus**
   October 8, 2009

   Oh great, now that people are finally making the connection between freshwater economic theory and string theory, it can't help but draw Lubos into the debate, which will of course be very productive.

3. **spammophobia**
   October 8, 2009

   The effects of the global crisis on economic thinking and modeling are still to be seen, but what was the net effect of the Sokal affaire on postmodern literature? I guess nothing much has changed actually, but a more, and better, informed opinion would be interesting.

4. **Chris W.**
   October 8, 2009

   It should be pointed out that the text was *quoted* by Brad DeLong, from an email sent to him by one 'chunkyreesewitherspoonlookalike@gmail.com'. His post makes this clear.

5. **Carl Futia**
   October 9, 2009

   I'd be careful about accepting DeLong's (second hand) views as gospel.

   To suggest that economists, especially macro-economists, have taken a collective wrong-turn sometime during the past 30 years is silly.

   As a group, economists are just as smart as physicists. They are just as dedicated to testing their theories against evidence – even though this is much harder to do in economics because controlled experiments are next to impossible to conduct.

   DeLong would have you believe that the current economic crisis proves that economics as currently practiced is bunkum. Does it make sense to imagine that DeLong knows the truth and that everyone who disagrees with him is an idiot?

   Why should economists be able to prevent economic crises? Do they have magical powers? Can they give marching orders to the US government and to you and me?

   The physics of hurricanes is reasonably well understood. Can physicists prevent hurricanes?

6. **Chuckles**
   October 9, 2009

   (…in the case of English, things like the Sokal hoax; in the case of econ, the near-collapse of the global economy..)

   And in the case of physics, what exactly?
   The disapperance of Madagascar?
   I am not a strings fan, but to compare the nonsense that is pomo with the voodoo that is much of macroecon to strings is absurd. At least string theorists arent going around telling us how to think, trying to change society b causing us to experience joussiance (whatever the hell that is) or giving so called economic advice to governments

around the world that ends up damning the lives of millions.

7. **Bob Levine**
October 9, 2009

And I'd be VERY careful of accepting Carl Futia's views, firsthand or not, as gospel. I think I'm a bit more persuaded by the arguments of Paul Krugman—who does know a thing or two about Nobel Prize level economics—given at

http://www.nytimes.com/2009/09/06/magazine/06Economic-t.html?_r=1&pagewanted=all

that indeed, macroecons of the freshwater persuasion got it spectacularly wrong during the past 30 years. Krugman gives specific instances where Chicago-school economists ignored evidence, actual occurences in the real world, to a degree that even the most devoted string theorist would be unlikely to indulge in if contraindications of comparable strength were to emerge from the LHC.

One of Krugman's highly relevant points is that many economists were attached to extremely unrealistic models of economic behavior because the simplifying assumptions associated with 'perfect market' models of the economy allowed them to create wonderfully ramified mathematical models that were too beautiful to be false. Sound familiar?

8. **Armozel**
October 9, 2009

Well I'm glad that someone else is starting to chime in a fundamental crisis that has existed probably since Marshall (and Walras). The attempt to model human behavior in the form of catallactics (interpersonal exchange) by assuming static equilibrium and other physics concepts into the attempt has made more errors than it has solved.

Lets take the idea of the price system as an example. Most theories revolving it simply assume prices exist with *no* concrete explanation (I say most, because at least the Austrians have taken a stab at it, going far back as Carl Menger's own work on the issue), but the theories some how make many conclusions about the nature of how prices move, how capital is influenced (and vice versa), and so on. How can any of these theories survive if they can't start with the basics (how the price itself gets started? how money started and so on?).

Then there's the problem of the implied separation of micro and macro sphere phenomena. Not even in biology (in particular, evolutionary biology) is there such a division, but economists get away with it with little or no criticism. And any criticism that's leveled on this issue is either ignored or attacked in a derisive manner. It's issues like these that makes economics less of a science and more of political ideology meets perennial RTS game sort of situation (where those with the most "street cred" and the loudest voice gets to set the tone of the discourse, and the rest get drowned out like the Austrians and even some Post-Keynesians).

9. **srp**
October 10, 2009

Anyone who accepts the critiques of Krugman and DeLong at face value is making a mistake. The freshwater models certainly have many limitations, but it's not as though the alternative neo-Keynesian models predicted the current crisis any better or explain it any better after the fact. Krugman isn't much of a macro expert in the first place and, for example, was actually cheerleading for the creation of a housing bubble a few years ago.

The truth is that the Keynesian and freshwater types have converged a lot in the last couple of decades in the formal properties of their models. None of these models does a very good job of dealing with financial shocks.

10. **Bob Levine**
October 10, 2009

@srp:

"The freshwater models certainly have many limitations, but it's not as though the alternative neo-Keynesian models predicted the current crisis any better or explain it any better after the fact…
The truth is that the Keynesian and freshwater types have converged a lot in the last couple of decades in the formal properties of their models. None of these models does a very good job of dealing with financial shocks."

I'm not sure that predictive success is really at issue (and I don't think Krugman is really arguing that Keynsian economics predicts financial shock better than any other kind of economics). I think the issue is rather that the freshwater world-view assumes that the largely unregulated market is going to work, that any problems the rest of us face are simply normal corrections, and so on. The insulation of this best-of-all-possible-worlds picture of reality—the kinds of denial that Krugman documents—from events in the actual world, the picture of the Infallible Market that it promotes, is really the problem. And it's just that extreme insulation for empirical challenge that makes de Long's and Krugman's observations relevant to the issue of string theory's overall stance.

11. **Jack Lothian**
October 15, 2009

As someone who has studied and worked in both Physics and Economics, I do believe that economics has gone off the rails. First, it is a pseudo-science – there are no provable theories and no repeatable experiments. Second, most of the basic assumptions (especially at the micro-level) are at best approximations and these assumptions clearly break down at times. It is obvious that people, even in large groups, are not rational & that irrational feed-back loops exists that swamp classical economic optimality cobstraints.

Up until recently, most economists, especially macro-economists, understood these point but starting somewhere around Friedman, economics found religion and this polarized and distorted the field. Capitalism and free-markets are the magic economic hand of god. Today the talk about competition, deregulation, efficient markets & the "magic hand" has become a dogma that is not to be questioned and it need not bother explaining difficult events such as the various "economic bubbles" that keep happening. After all, these events are a result of man's imperfections & we just need to try harder to be pure and everything will turn out perfect.

As an aside, the physicists and mathematicians who recently moved into the field are sophisticated modelers, use to modeling large multi-body problems of various types. In truth, their methods and temporary success say nothing about the underlying economic theories and they brought no real science to economics.

Short News Items

October 9, 2009
Categories: Uncategorized

Mathematician Jim Simons is retiring from the job of running the hedge fund Renaissance Technologies. Construction of the building for the Simons Center for Geometry and Physics is proceeding, with opening scheduled for next fall.

An Algerian physicist associated with the LHCb experiment at CERN has been arrested on charges of having associations with al-Qaeda. The media freak out and CERN issues a statement.

I. M. Gelfand died on Monday at the age of 96. For more about him, see here, here and here.

The fourth and latest installment of Oswaldo Zapata's essay on the history of superstring theory is here.

In Geometric Langlands news, Dennis Gaitsgory is running a seminar at Harvard this fall, with notes and other materials on-line here.

Emanuel Kowalski points out that, morally, Princeton's Peter Sarnak has a blog.

Update: One more.

Comments

1. **Yatima**
   October 9, 2009

   Ah, terrorism!

   So said physicist is "under suspicion of links to terrorist organizations". This could mean he once phoned a guy who has phoned a guy living next door to Bin Laden's driver. On the other hand, the press release does not even mention persons of Middle Eastern origin at all.

   Could be it's just a weird dadaist project to push Dan Brown's latest book.

2. **zanzibar**
   October 9, 2009

   Peter,

   What's your source for saying the arrestee is Algerian? According to all the sources I've seen he's Algerian-born, but could have French citizenship. He probably holds dual citizenship.

   The AP release states he is a French citizen.

3. **Peter Woit**
   October 9, 2009

   zanzibar,

   I think you've probably looked at more sources about this than I have. By "Algerian" I just meant "from Algeria", not his citizenship.

4. **anon.**
   October 10, 2009

   "I'd suggest shutting up and letting him initiate the conversation. He probably won't do this the first night or the second night, but after five nights of silence, I bet he says something. If he doesn't do it, then he doesn't want to talk with you during dinner."

   Thanks for the "update" link. Pity there is no mathematical proof to back up the advice from mathematicians.

Witten on Analytic Continuation of Chern-Simons Theory

October 10, 2009
Categories: Uncategorized

I was down in Princeton last Thursday, and attended a wonderful talk by Witten, which I'll try and explain a little bit about here. Presumably within a rather short time he'll have a paper out on the arXiv giving full details.

The talk concerned Chern-Simons theory, the remarkable 3d QFT that was largely responsible for Witten's Fields medal. Given an SU(2) connection A on a bundle over a 3-manifold M, one can define its Chern-Simons number CS(A). This number is invariant under the identity component of the group of gauge transformations $\mathcal G$, and jumps by 2π times an integer under topologically non-trivial gauge transformations. The QFT is given by taking CS(A) as the action. The path integral

$$Z(M,k)=\int_{\mathcal A/\mathcal G} dA e^{ikCS(A)}$$

is well-defined for k integral and gives an interesting topological invariant of the 3-manifold M. One can also take a knot K in M, choose an irreducible representation R of SU(2) of spin n/2, and then define a knot invariant by

$$Z(M,K,k,n)=\int_{\mathcal A/\mathcal G} dA e^{ikCS(A)}hol_R(K)$$

where $hol_R(K)$ is the trace of the holonomy in the representation R, around the knot K (this is the Wilson loop).

To simplify matters, consider the special case $Z(K,k,n)=Z(S^3,K,k,n)$, which can be used to study knots in $\mathbf R^3$.

These knot invariants can be evaluated for large k by stationary phase approximation (perturbation theory), and for arbitrary k by reformulating the QFT in a Hamiltonian formalism, and using loop group representation theory and the Verlinde (fusion) algebra.

One thing that has always bothered me about this story is that it has never been clear to me whether such a path integral makes sense at all non-perturbatively. At one point I spent a lot of time thinking about how you would do such a calculation in lattice gauge theory. There, one can imagine various (computationally impractical) ways of defining the action, but integrating a phase over an infinite dimensional space always looked problematic: without some other sort of structure, it was hard to see how one could get a well-defined answer in the limit of zero-lattice spacing. In simpler models with similar structure (e.g. loops on a symplectic manifold), similar problems appear, and are resolved by introducing additional terms in the action.

What Witten proposed in his talk was a method for consistently defining such path integrals by analytic continuation. The idea is to complexify, working with SL(2,**C**) connections and a holomorphic Chern-Simons functional, then exploit the freedom to choose a different contour to integrate over than the contour of SU(2) connections. By choosing a contour that is not invariant under topologically non-trivial gauge transformations, and only modding out by the topologically trivial ones, Witten also managed to define the theory for non-integral k, making contact with a lot of mathematical work on these knot invariants, which treats them a Laurent polynomials in the square root of

$$q=e^{2\pi i/(k+2)}$$

The main new idea that Witten was using was that the contributions of different critical points p (including complex ones), could be calculated by choosing appropriate contours $\mathcal C_p$ using Morse theory for the Chern-Simons functional. This sort of Morse theory involving holomorphic Morse functions gets used in mathematics in Picard-Lefshetz theory. The contour is given by the downward flow from the critical point, and the flow equation turns out to be a variant of the self-duality equation that Witten had previously encountered in his work with Kapustin on geometric Langlands. One tricky aspect of all this is that the contours one needs to integrate over are sums of the $\mathcal C_p$ with integral coefficients and these coefficients jump at "Stokes curves" as one varies the parameter in one's integral (in this case, x=k/n, k and n are large). In his talk, Witten showed the answer that he gets for the case of the figure-eight knot.

Mathematicians and mathematical physicists have done quite a bit of work on SL(2,**C**) Chern-Simons, and studying the properties of knot-invariants as analytic functions. I don't know whether Witten's new technique solves any of the mathematical problems that have come up there. He mentioned the relation to 3d gravity, where the relationship between Chern-Simons theory and gravity in the Lorentzian and Euclidean signature cases evidently still remains somewhat mysterious. Perhaps his analytic continuation method may provide some new insight there. It also may apply to a much wider range of QFTs where there are imaginary terms in the action, making the path integral problematic. I'd be very curious to understand how this works out in some simpler models, such as the loop space ones. In any case, it appears to be a quite beautiful new idea about how to define certain QFTs via the path integral.

Update: Witten's slides for the talk are available here, video here. For slides from other talks at the workshop the talk was part of, see here.

Comments

1. **Bruce Bartlett**
   October 11, 2009

   Sounds great. Looking forward to when the paper comes out.

2. **Lefschetz Thimble**
   October 12, 2009

   I don't see the magic words to convince everyone you know the mathematics.

3. **Peter Woit**
   October 12, 2009

   Thanks L.T. for giving us the magic words.

   (the commenter must have attended the talk, during which Witten joked that if you want to show people that you're expert in this subject, you refer to one of these integration cycles as a "Lefschetz Thimble". I considered doing this in the posting, but decided against it, since not only am I not an expert on this, but it's one part of Morse theory I've been completely ignorant about…)

4. **Daniel Doro Ferrante**
   October 14, 2009

   Hi Peter,

For those who are interested, the talk is already online (i watched it yesterday),

• Analytic Continuation of CS Theories (aka, *Lefschetz Thimble* 😉).

It’s a very stimulating and sophisticated lecture, definitely worth watching.

Incidentally, it reminds me of some works that go in this “general direction”, namely: arXiv:hep-th/9612079 and arXiv:0710.1256 [hep-th].

5. **Peter Woit**
   October 14, 2009

   Thanks Daniel,

   I also see that a pdf of the slides is available, will add links to the posting.

In Search of the Multiverse

October 12, 2009
Categories: Multiverse Mania

The ongoing pseudo-scientific multiverse mania continues, with the recent publication in the UK of a new book by John Gribbin promoting this to the public: In Search of the Multiverse.

Gribbin expounds at length the usual string theory anthropic landscape/multiverse ideology, carefully avoiding introducing any mention of the fact that there might be quite a few scientists skeptical about it. On the crucial question of testability he invokes Raphael Bousso, who:

> hopes, and expects, that there will be ways to extract such broad rules of the behaviour of matter at what are low energies compared to the Big Bang, but high by the standards of everyday life, from string theory.

There's no indication given about what these broad rules implied by string theory might be, just a hint that whatever they are, we're not going to be able to test them anytime soon:

> even the the technology of the Large Hadron Collider may not be up to the task of testing such predictions.

Like many multiverse fans, Gribbin wants to mix together the many worlds interpretation of QM and the string theory anthropic multiverse in cosmology, attributing this insight to Susskind, and ending the next to last chapter of his book with:

> This pulls together everything discussed in this book so far in such a pleasing way that it is tempting to end it here. The Cosmic Landscape of string theory is just the many worlds theory of David Deutsch writ large, and with inflation included within itself.

Unfortunately he doesn't end the book there, but adds a final chapter promoting his own interpretation of the significance of the multiverse. His idea is that we are the product of a baby universe created by some race of superior beings:

> The intelligence required to do the job may be superior to ours, but it is a finite intelligence reasonably similar to our own, not an infinite and incomprehensible God. The most likely reason for such an intelligence to make universes is the same as the reason why people do things like climbing mountains or studying the nature of subatomic particles using accelerators like the LHC – because they can. A civilization that has the technology to make baby universes might find the temptation irresistible, while at the higher levels of universe design, if the superior intelligences are anything at all like us there would be an overwhelming temptation to improve upon the design of their own universes.
>
> This provides the best resolution yet to the puzzle Albert Einstein used to raise, that 'the most incomprehensible thing about the Universe is that it is comprehensible.' The Universe is comprehensible to the human mind because it was designed, at least to some extent, by intelligent beings with minds similar to our own. Fred Hoyle put it slightly differently. 'The Universe,' he used to say, 'is a put-up job.' I believe that he was right. But in order for that 'put-up job' to be understood, we need all the elements of this book.

Personally, I think there's an air-tight argument against this: any race of superior beings that produced a universe in which science descended into this level of nonsense would immediately wipe out their creation and start over. Since we're still here, there can't be such a race operating out there.

Gribbin also has a Sci-Fi novel entitled Timeswitch coming out soon.

For two reviews of the book, see here and here.

In other multiverse news, FQXI has a story here promoting Andrei Linde, Renata Kallosh and their work on the string theory multiverse. Linde and a collaborator have a new paper How many universes are in the multiverse? on hep-th (by the way, why are these things not in qr-qc, since they're "quantum cosmology" if anything is?). They come up with a number of 10 to the 10 to the 375 for the number of universes, and seem to argue that one needs to analyze all these to come up with predictions:

> But when we study quantum cosmology, evaluate the total number of universes and eventually apply these results to anthropic considerations, one may need to take into account. Potentially, it may become very important that when we analyze the probability of existence of a universe of a given type, we should be talking about a consistent pair: the universe and an observer who makes the rest of the universe "alive" and the wave function of the rest of universe time-dependent.

Comments

1. **theoreticalminimum**
   October 12, 2009

   That's rather sad. John Gribbin is an author whose many books I grew up reading. I don't think I will grow up with this one.

2. **roland**
   October 12, 2009

   "The Universe is comprehensible to the human mind because it was designed, at least to some extent, by intelligent beings with minds similar to our own."

   I guess following the line of universe builders leads to a final source(?). The highest beings there should live in a universe that is uncomprehensible to them. How did their human- like minds develop there? How can they have technology if they can't comprehend their environment. That whole line of reasoning is just absurd.

3. **neo**
   October 12, 2009

   Absolutely right, Roland. Like every "intelligent design theory", it seeks to explain by invoking the inexplicable.

4. **Tim vB**
   October 12, 2009

   You could of course have an infinite hierachy of creators, if each level acts twice as fast as the level below (the higher the more advanced), you could still have our baby universe at the bottom created in finite time, right? No need of some highest entity.
   In that case we should hasten to create our own baby universes, or else we would have to live with the fact that we are the most stupid human like creates of infinite

universes.

5. **Chris Oakley**
October 13, 2009

> … any race of superior beings that produced a universe in which science descended into this level of nonsense would immediately wipe out their creation and start over.

This may be about to happen. Hopefully they will replace it with a universe with no Teletubbies.

6. **Stephen**
October 13, 2009

This argument:
"'the most incomprehensible thing about the Universe is that it is comprehensible.' The Universe is comprehensible to the human mind because it was designed, at least to some extent, by intelligent beings with minds similar to our own."

is actually exactly the same argument invoked by theologians: The Universe is comprehensible to the human mind because it was designed, at least to some extent, by intelligent beings with minds similar to our own=God. The reason being that he is the creator and he has created us similar to himself, we are his image (or even more: he is our father, according to christian theology).

7. **Stephen**
October 13, 2009

and by the way:

the reason why people do things like climbing mountains or studying the nature of subatomic particles using accelerators like the LHC – is not just because they can: but because it is beautiful (and, who knows, perhaps useful).

8. **Oxonian**
October 13, 2009

I am an atheist because I am unpersuaded by the arguments and evidence for theism, not because theism "sounds silly". It is arguments and evidence that I would like to see offered against Gribbins's hypotheses, however "silly" they may sound. Yet as far as I can see, all we are given in that respect is an invocation of falsifiability, which is besides the point since there are observations that could affect the probability that those hypotheses are true or false. So, Peter, could I kindly ask you to articulate, at least roughly, the reasons why you reject Gribbins's ideas? This may actually persuade me and others that these ideas are indeed wrong, moving the discussion forward.

9. **Peter Woit**
October 13, 2009

Oxonian,

The problem with Gribbin's ideas is that they are pseudo-science, not science. They don't actually have anything substantive to say about observations. Sure, it may be true that our universe may have been constructed by a higher intelligence, but this is an hypothesis that can be tested in any legitimate scientific way.

10. **Oxonian**
October 13, 2009

Peter,

Thanks for the response. It is not clear to me why such a hypothesis could not, in principle, be tested in a legitimate scientific way. I agree with Richard Dawkins that the hypothesis that there is a god should be treated like a scientific hypothesis; and I agree with Victor Stenger that this hypothesis is disconfirmed by the available evidence. I don't see why a hypothesis such as Gribbin's (sorry for the misspelling above) should not be treated similarly.

Just out of curiosity, do you think the many worlds interpretation of quantum mechanics is also pseudo-scientific? If so, what do you make of claims by Max Tegmark and others that this interpretation is actually scientifically testable? (If you have written about this in the past, feel free to direct me to the relevant writings.)

11. **Peter Woit**
October 13, 2009

Oxonian,

There is no trivial reason why you can't get predictions out of string theory multiverse models, you need to understand what they are and what their problems are. The bottom line though is that, despite a lot of work, proponents of this have come up with nothing. There is nothing in the Gribbin book in terms of predictions except the assurance from Bousso that he expects his work to be successful and to find something.

I haven't seen Tegmark's claim that many-worlds is testable, so don't know what that refers to. To the extent that many-worlds precisely agrees with other QM interpretations, not postulating completely different physics we mysteriously can't quite test, I don't think it's pseudo-science. But if so, depending on one's taste, one might not find it very interesting…

12. **Rakavolver**
October 13, 2009

"Personally, I think there's an air-tight argument against this: any race of superior beings that produced a universe in which science descended into this level of nonsense would immediately wipe out their creation and start over. Since we're still here, there can't be such a race operating out there."

Superior doesn't mean "good," there is no correlation. They may be the types with horns, not halos, or supra-being equivalents of non-serious high school sophmores in Chemistry lab playing with bunsen burners.

13. **TomInCalif**
October 13, 2009

It's "Turtles all the way down…"

14. **Arrow**
October 14, 2009

Oxonian: "I am an atheist because I am unpersuaded by the arguments and evidence for theism, not because theism "sounds silly". It is arguments and evidence that I would like to see offered against Gribbins's hypotheses, however "silly" they may sound."

Ockham's razor.

15. **Aristarchus**
October 14, 2009

I always thought that God was a grad student from another universe who made ours for his thesis project. We're now sitting on some immense beaker on some dusty shelf in another reality, as our Creator goes in search of a postdoc and a future wife.

I have no proof of this, but these days who needs to?

16. **Charles Hardin**
October 14, 2009

I'm always amazed at the number of people willing to make sweeping statements about a book without actually reading it! I have read it, and it makes a lot of sense. Gribbin isn't so stupid that he hasn't thought of all these points, and discussed them! And as he says, if there is only one Universe, where do quantum computers do their calculations?

17. **Rakavolver**
October 14, 2009

Quantum computers, being quantum computers in our universe, make their computations in our Universe. Was that so hard?

I believe you are referring to Shor's Algorithm, which is rock-hard Mathematics, and Deutsch's interpretation of same, which is speculation.

Would you care to elaborate? Because all of this refers to possible FUTURE Quantum computers far more sophisticated than the ones we have now, and can theoretically can achieve, but have not. Hello, Quantum decoherence?

Gribbin not stupid by any means, but he has the right and the privilege to be wrong, just like everyone else, including you and me.

18. **Rakavolver**
October 14, 2009

"The Cosmic Landscape of string theory is just the many worlds theory of David Deutsch writ large, and with inflation included within itself."

Oh, good Lord. In one sentence, three controversial subjects are combined (NOT "unified", because to call them so would be a insult to TRUE UNIFIERS such as Dirac who unified (without quotes) Special Relativity and Quantum Mechanics), thanks but no thanks, via semantics. Let's break them out. This is (sadly) fun:

– The Anthropic Landscape Mis-Theory of California String Theory
– The Many-Worlds Mis-Theory of Hugh Everett II
– The Inflation Damn-Near-Theory but often Mis-Theory of Alan Guth

Why am I NOT surprised, since this is written for the "middle-brow" "intelligent laymen" target audience that is the English, that this book will NOT sell? In England, anyway.

So sad.

19. **neo**
October 14, 2009

I would describe Everett's many worlds as an "interpretation"of a rock solid theory, QFT. I don't quite know what the anthropic landscape should be called. An interpretation of a speculative theory?

20. **Charles Hardin**
October 17, 2009

Peter Woit says, "many-worlds precisely agrees with other QM interpretations" in other words, other QM interpretations precisely agree with many-worlds. So it passes all the tests other interpretations pass. AND it solves the puzzle of what makes the wave function of the Universe collapse. You can't just dismiss it because you don't like it.

Rakavolver describes inflation as a mis-theory — yet it has passed several experimental/observational tests.

And the President of the Royal Society takes anthropic ideas seriously.

Let's use reason in this debate, not emotion.

21. **Tim vB**
October 18, 2009

@Charles Hardin
Hope the following is not too controversial:
QM is a mathematical model, if you describe your experiment, I can employ that model and calculate the possible outcomes. If there is an error on my or your part we will disagree, find the error, fix it and finally agree, within a relatively short timespan.
An interpretation of QM is an explanation of why the model works. You and I can believe in different interpretations, that does not affect the your experiment or my calculations. I think this is meant by "many-worlds precisely agrees with other QM interpretations".
You cannot rule out an interpretation by experiment.
While the many-world interpretation has many interesting aspects, I am puzzled why I have the impression to live in only one of the many-worlds. Any interesting comments on that? (I knwo some proponents of many-worlds, but they all dismiss this question as metaphysical).

22. **David J. Williams**

October 23, 2009

Deutsch's contention is that a quantum computer would literally be "larger" than any one classical universe:

http://www.edge.org/documents/archive/edge78.html

Interesting interview, regardless of whether you agree with him.

Embarrassing Crackpottery

October 12, 2009
Categories: Multiverse Mania

A while back I noticed that the arXiv had allowed the posting of the preprint Card game restriction in LHC can only be successful!, yet another in a sequence of crackpot articles about the LHC from Holger-Bech Nielsen and Masao Ninomiya. That these authors have managed to get the previous articles in the series published in the International Journal of Modern Physics A presumably has something to do with the fact that Ninomiya is an editor of the journal. I didn't post anything about this, on the grounds that embarrassing crackpottery from well-known physicists that no one except them takes seriously is best ignored.

Unfortunately, this particular piece of nonsense has been picked up by the New York Times, which tomorrow is running a story about it under the title The Collider, the Particle and a Theory About Fate. The writer, Dennis Overbye, presumably contacted some physicists to find out what they thought of this. If any of them told him this was just nuts and an embarrassment, that didn't make it into the story, instead there's:

> …craziness has a fine history in a physics that talks routinely about cats being dead and alive at the same time and about anti-gravity puffing out the universe.
>
> As Niels Bohr, Dr. Nielsen's late countryman and one of the founders of quantum theory, once told a colleague: "We are all agreed that your theory is crazy. The question that divides us is whether it is crazy enough to have a chance of being correct."
>
> Dr. Nielsen is well-qualified in this tradition. He is known in physics as one of the founders of string theory and a deep and original thinker, "one of those extremely smart people that is willing to chase crazy ideas pretty far," in the words of Sean Carroll, a Caltech physicist and author of a coming book about time, "From Eternity to Here."

Perhaps it would be a good idea if physicists would remind journalists that often things that seem to be crazy really are crazy.

Update: See more here from Tommaso Dorigo. I should have mentioned that his posting from a couple years back Respectable physicists gone crackpotty was linked to in the article by Overbye, who had an accurate take on the subject from at least one source.

Update: Somehow I knew that Slashdot could not possibly resist this nonsense.

Update: Sean Carroll has a long defense of the Nielsen-Ninomiya papers as not crackpot at all, but crazy in a positive way:

> There's no real reason to believe in an imaginary component to the action with dramatic apparently-nonlocal effects, and even if there were, the specific choice of action contemplated by NN seems rather contrived. But I'm happy to argue that it's the good kind of crazy. The authors start with a speculative but well-defined idea, and carry it through to its logical conclusions.

As for the argument that prominently-placed New York Times stories promoting crazy ideas about physics might be problematic, Sean is having none of it. He argues that the public is able to differentiate between speculative ideas and solidly tested science, so it's not a problem that:

> My own anecdotal observations are pretty unambiguous — the public loves far-out speculations like this, and happily eats them up.

Comments

1. **ConcernedCitizen**
   October 12, 2009

   It's rather shocking that such garbage actually gets published in journals.

2. **James D. Miller**
   October 12, 2009

   I'm an economist not a physicist so please forgive me if this seems stupid.

   But if the many world's hypothesis of quantum physics is right and if the LHC would destroy the world if it ever became operational then wouldn't we get the result in the paper? In fact, doesn't every time the LHC breaks down increase the probability that the LHC is destroying alternative quantum branches?

3. **Peter Woit**
   October 12, 2009

   James,

   The nonsense coming from Nielsen-Ninomiya is not the popular multiverse nonsense, but their very own private nonsense about "backward causation". It doesn't have anything to do with destroying the world, it's about the production of Higgs particles causing an effect that goes backward in time to stop this production.

   If over the next few years, we see more and more unlikely things happening to keep the LHC from operating, that would be an argument for a many-worlds scenario in which an operational LHC destroyed the universe. Hey, if things got unlikely enough, I might even start believing this….

4. **theoreticalminimum**
   October 12, 2009

   Boy, another let-down 🙁
   Peter, go easy on my childhood memories!
   Overbye was another author I like; what's going on with these people these days? Are they struggling to find truly interesting things to talk about?! :-\

5. **capitalistimperialistpig**
   October 12, 2009

   This looks like fodder for a dramatic TV series in the mold of *Flash Forward*. Maybe the authors are pitching such a project.

6. **abby yorker**

October 12, 2009

Seems like there must be plenty of Higgs produced by cosmic rays over the years. Are these avoided by the theory too?

7. **Tim vB**
October 12, 2009

Just read the paper and laughed my head off: Come on now, this is a pretty good joke and parody of some off-shell stringy style, could not be more obvious.
Well, they could have ended each sentence with a smily, if TeX supports that – don't know.
Do you really think you have to point out that this is not "real science"?
Well, obviously you cannot be too obvious if you do not want to be taken seriously 🙂 which would be the meta joke of the whole affair.

8. **Amitabha**
October 13, 2009

This must be the Eternity machine that Asimov was talking about. Perhaps somebody had gone back to him from the days after the LHC.

9. **a quantum diaries surivivor**
October 13, 2009

Hello Peter,

thank you for linking my piece, which does not make me too proud but did make me chuckle upon reading it back.

if the LHC ends up not running at all for a string of accidents big and small, I'd still not caress the idea that we should start playing card games. It will just be a demonstration that we have built a toy too big to play with.

Every student who has seen the relation between curvature radius and bending dipole field, and the dependence of synchrotron power loss with curvature, has the tools to wonder how large an accelerator humans can build. Unfortunately, though, those are not the only parameters. The space-time failure probability d2F/dVdt depends on the volume of the device, and this becomes a hindrance to scalability. It is obvious if you go to four dimensions and examine time instead of space dependence, trying to ask if you can keep the total failure probability small while increasing the duration of the experiment indefinitely.

The LHC might just be beyond the limit of spatial dimensions allowed by present-century technology. It would be very sad, but I'd argue that we shouldn't draw more conclusions from it, other than some of the project leaders should have known better 😉

Cheers,
T.

10. **Bee**
October 13, 2009

The only thing that's crazy about "anti-gravity puffing up the universe" is journalists who call it anti-gravity.

11. **Umair Rahat**
October 13, 2009

This is the top story on the front page of NewYorkTimes.com as of 4 AM EST, I could not have been more shocked. Even worse is such a paper in a real scientific journal, and then an article written by a physics and cosmology writer Dennis Overbye in a newspaper as NY Times. This type of nonsense needs to be seriously condemned. Even an article of the new-age-movement claims on LHC would have done far more satisfactory result, since even it wouldn't have aligned such utter crackpottery to physics.

And whats going on with this incredibly illogically warped piece of logic this paper is; With the title "Card game restriction in LHC can only be successful!", and even the exclamation mark hints on what is to come. Is this a joke or is this "a new game" of writing scientific papers?

12. **Tim vB**
October 13, 2009

During a recent discussion with (non-scientists) friends I found out that the statement "we hope to observe new physics at the LHC" was interpreted to mean that some events could happen at the LHC that never ever have happend before in the whole universe. That is probably the reason so many people are willing to believe any story anyone with a Ph.D. is telling. Please help and tell your friends that the universe has and probably will be operating at much higher energies than the LHC without distroying itself 🙂
I am getting scared that so many people seem to believe the paper of Nielsen and Ninomiya could NOT be a joke/parody. How could that be? They talk about:
– backreaction of future events leading to the dismissal of the SSH by the congress of the USA,
– a vacuum bomb (meaning a bomb that works through the creation of a new QFT vacuum state),
– tossing cards to find out about the future of the LHC (I admit that there live people who believe in that 🙂
– "Now it is sometimes explained that SSC had bad luck because of
various stupidities or accidents, but had it been a card game nobody could come up with such foolish cards."
– "Therefore, if LHC fails for a reason other than a random
number game, we would have not even truly learned that our theory was right even
though we would say 'it is remarkable that the present authors wrote about the
failure while LHC still looked to be able to work.' "
Ok, if anyone remains that does not believe in a joke (or call it parody or political comedy) I give up, I'll first throw away all my papers and textbooks, get a banking job in London and I won't look back.

I'm not so shocked about the New York Times, obviously the authors were not able to comprehend basic 20th century physics (but all of those kind of articles contain misconceptions).

@James D. Miller
The multi universe interpretation of quantum mechanics does not seem to have anyting to do with all of this, but: Your conciousness appears to live in one universe only, right? (If not, it is called schizophrenia I think). So if you believe in the mulit universe interpretation, you need some explanation why your conciousness seems to constantly choose only one of them to live in. Depending on this explanation, if the LHC destroyes some of the universes, your conciousness could equally well choose the path to one of those, so you would just wake up one morning and realise you don't exist anymore. You would need to assume some mechanism that ensures that your conciousness stays on the right path.

13. **Paul Jackson**
    October 13, 2009

    Wonderful stuff from Nielson and Ninimiya. They should write a book about it — perhaps a romantic novel called BBC rules.

    But isn't it about time evidence-based physics was due for a revival? Then we could do away with backward causation, black holes that form only at the end of eternity, slathers of stringy stuff, so-far unobserved dark energy, and even poor dead-and-alive cats.

    And get on with optic fibres and charge-coupled devices —our real business, don't you think?

14. **ManyMe**
    October 13, 2009

    The paper states that the model
    "begins with a series of not completely convincing … assumptions".

    I think they are right about that.

15. **chris**
    October 13, 2009

    you have to give them credit where credit is due though. "Miraculosity" is certainly the most hilarious observable ever seriously suggested by a physicist 🙂

    oh, and another thing: the "theory" doesn't really fall under 'not even wrong'. it has a very characteristic experimental signature and could quite easily be disproved by an experiment which would cost a few pennies. so what's the real problem? the embarassment of Heuer having to draw from a few stacks of cards? 🙂

16. **chris**
    October 13, 2009

    another thought

    maybe they are begging to be awarded an ignobel price?

17. **Thomas Larsson**
    October 13, 2009

    the "theory" doesn't really fall under 'not even wrong'. it has a very characteristic experimental signature and could quite easily be disproved by an experiment which would cost a few pennies.

    Really? If such an experiment only costs a few pennies, why hasn't it been done already? I might be willing to fund it myself.

18. **Aristotle Pagaltzis**
    October 13, 2009

    Perhaps it would be a good idea if physicists would remind journalists that often things that seem to be crazy really are crazy.

    Would that it were so simple. Unfortunately, a lot of modern physics fare like curved space-time, particle-wave duality and other firmly established ideas seem equally crazy to laymen whose only perspective is their everyday intuition.

19. **Jacques Lacon**
    October 13, 2009

    Here's a hypothesis: this sure reads to me like an elaborate Bayesian joke at the expense of the LHC and the Higgs and a certain sector of the theoretical physics community. I think they are with you Peter, not against you, and this is a Sokolesque parody. At any rate I found it quite funny.

20. **SteveB**
    October 13, 2009

    I have always liked well written science fiction. It gives me ideas, gets me thinking outside the box, provides very interesting connections to possible happenings in reality, and it satisfies that need to have that feeling of "wow, that's freaky!" on occasion. I do not have to accept it as "real", and I like that. Science fiction probably was the biggest influence on me having a career in science.

    However, I really do like my science and my science fiction to be separate. I applaud Peter for calling it out. It is embarrassing that apparently some people can no longer make that separation.

21. **Rakavolver**
    October 13, 2009

    Agreed SteveB, and Greg Egan of Perth, Australia and friend/collaborator of the incredible John Baez is the best one currently writing.

    Well, this was quite amusing and thank you for this Peter, but to get serious for a bit regarding crazy crackpottery.

    Everyone should encourage all their "intelligent laymen" friends (especially those in power) to read the following list:

    http://en.wikipedia.org/wiki/List_of_topics_characterized_as_pseudoscience

    Then, have them read John Baez' crackpot index, so:

    http://math.ucr.edu/home/baez/crackpot.html

    Finally, let's get serious. Of all the important things said by anyone in the last 50 years, the single most important thing anyone has said was said by noted General Relativist Stephen Hawking, who essentially said we have to get off this 8000-mile rock or we're doomed.

    It won't be easy, it won't be cheap, but it IS important. And take a wild guess WHOM our species will depend on to make that happen? Richard Branson? Yes, a bit, but mostly ….. YOU guys.

If you're intelligent enough to have read this website in the first place and far enough down this thread in the second place, then you're capable of making a difference. What have to done today or this month or year to make that happen, for example say, a lunar colony, come about?

What this has to do with crackpottery is simply this:

Peter, PLEASE write a rebuttal to that article, or one of you, please, and politely insist it be front page next week on the NY Times. Because if it doesn't, Hawking's worst nightmare may come to pass. Cheers.

22. **Charlie C**
October 13, 2009

The paper is not even funny.

23. **Yuri Danoyan**
October 13, 2009

"Influence from the Future" idea proposed by C.D.Froggatt and H.B.Nielson in 1996. http://arxiv.org/abs/hep-ph/9607375.
I don't think it is full crazy idea.My be older generations of particles not signals from the Past, but signals from the Future?Some hints about future transformations in the world of particles next epochs?
They are not ancestors, but the descendants ?

24. **Rakavolver**
October 13, 2009

From Yuri's link:

"Our cleanest prediction is the Higgs boson mass being 149 +/- 26 GeV, which in addition assumes that the pure Standard Model is valid until the Planck scale. Adding the assumption that the two vacuum states needed in our model come about naturally requires a strong first order transition between them; together with the assumption of the Planck units being fundamental, this leads us to also predict the top quark mass as 173 +/- 5 GeV, and a more precise value of the Higgs boson mass as 135 +/- 9 GeV."

Those are three rather strong assumptions, eh?

25. **timmo**
October 13, 2009

I'm sure this is a silly question, but I just don't understand what, *exactly*, is the experiment they propose?

I find it further confusing that they keep talking about either drawing a card, or drawing a number from a *quantum* number generator. If the experiment (whatever it is) can be performed by drawing a card, then it could also be performed by a normal, classical random number generator. Why should it be either a deck of cards or a quantum computer?

26. **Geoff**
October 13, 2009

Clearly they left out this reference:

http://www.physics.nyu.edu/faculty/sokal/transgress_v2/transgress_v2_singlefile.html

27. **neo**
October 13, 2009

I think that this paper is a joke (funny or not) by Nielsen and Ninomiya, but apparently Overbye didn't get it.

28. **Peter Woit**
October 13, 2009

If it's a joke, it's part of an act the authors have been putting on now for several years without breaking character…

29. **Tom O'Bulls**
October 13, 2009

Chris said: "oh, and another thing: the "theory" doesn't really fall under 'not even wrong'. it has a very characteristic experimental signature and could quite easily be disproved by an experiment which would cost a few pennies. so what's the real problem? the embarassment of Heuer having to draw from a few stacks of cards?"

As somebody pointed out on Bee Hossenfelder's blog, the problem is that you have to *trust* the person doing the experiment, and that the Higgs has not reached back in time to compel him to conceal the evidence that the "experiment" will reveal. What if the Higgs turns him into a liar? What if the Higgs turns him into an alcoholic?

I still think it's a joke. Somebody evidently pointed out to the authors that the SSC was cancelled because of the fall of communism [!]. To this the riposte is that the Higgs caused the fall of communism. Come on, it has to be a joke.

30. **Yatima**
October 13, 2009

So it's in the NYT? So what. The NYT is the Nigerian Yellowcake and Iranian Imminent Nuclear Threat Newspaper. It will get the hoi polloi confused and into a funk, but that's the way the cookie crumbles.

Now, my banker buddy seems to have decided that he doesn't believe in QM and has locked on to the "Theory of Elementary Waves", another member of the Not Even Wrong category. What do I do? Torture him with Lagrangians? Oh my…

31. **Kea**
October 13, 2009

This simply MUST be a joke. Human stupidity may be infinite, but I find it hard to believe that these guys could be THAT idiotic. And why would they admit it was a joke just yet? The paper is published, they are getting serious press, and the farce just keeps getting more and more ridiculous.

32. **Christine**
October 14, 2009

I would not doubt that even average crackpots find that paper highly embarrassing. That makes some crackpot claims appear respectable scientific works in comparison. I can only conclude that the effective result of that paper is the fact that it undoubtedly expands the limits of crackpotism. John Baez will have to re-scale his index.

33. **D R Lunsford**
October 14, 2009

A comment to Overbye's article sums up the damage done to physics from within and from without:

"I absolutely love physics for just this reason. It's totally crazy. Just like this life. Who knows anything, and it's all magic. Fantastic!"

That is what the intelligent layperson understands about the very thing that underpins most of the modern world.

-drl

34. **Rimus**
October 14, 2009

Oh! And there's not only "miraculocity". There's also the "degree of remarkableness" The more I read it, the better I like it!

35. **Aristarchus**
October 14, 2009

Apparently somebody read Einstein's Bridge and took it seriously:

http://faculty.washington.edu/jcramer/E_Bridge.html

36. **Rakavolver**
October 14, 2009

As bad as the paper is, the article is far, FAR worse.

It's bad enough The New York Times, a once great newspaper, has degenerated as much as it has. It's bad enough all newspapers will be dead and ONLY on-line in the next ten years, but this is really scraping the bottom of the barrel. The journalist takes the impish "respected" Physicists, who are obviously making a joke, at their word.

Do we really have to read about Kurt Vonnegut's FICTIONAL work as "real" Physics? What an insult to the late wonderful Vonnegut. Do we REALLY have to know the author is a life-long Boston Red Sox fan and believes in "jinxes"? I think not.

Worst of all is his taking completely out of context wonderful quotes by Niels Bohr and Albert Einstein. They were great men and deserve better.

I admire the way Europe, in its cute way, is trying to compete with America economically. We'll see how that works out. But in Physics, Europe has already won and will continue to do so. The LHC WILL start up, and the results will be wonderful.

Keep the faith my European cousins.

37. **Aristarchus**
October 14, 2009

THIS is why the Higgs Boson is messing with the LHC – we threatened it!

http://abstrusegoose.com/118

38. **Coin**
October 14, 2009

If over the next few years, we see more and more unlikely things happening to keep the LHC from operating, that would be an argument for a many-worlds scenario in which an operational LHC destroyed the universe. Hey, if things got unlikely enough, I might even start believing this….

Like "intelligent design", it's a theory which is attractive for its nearly unbounded explanatory power. I would note that similar anthropic "otherwise, all life in the universe would have ended" reasoning could prove equally valuable in explaining the difficulties in Democratic efforts to pass a health care reform bill, or Terry Gilliam's recent inability to complete a movie.

39. **Hal Porter**
October 14, 2009

AAArgh!!

I just lost my (very witty) post a moment ago when I hit the wrong key.

Gist:

I thought it was a joke, also.

Now I learn otherwise.

40. **Sean's doppelgänger**
October 14, 2009

Stop the eye rolling, *dammit*!

41. **Thomas Larsson**
October 14, 2009

Maybe the infinite improbabability drive isn't so unrealistic after all; I'll make myself a nice hot cup of tea.

42. **Joe Eckard**
October 15, 2009

Laugh away. I laughed too. A lot of knee-jerk thinking in these comments. Nielsen is one of the fathers of string theory. He is no dummy, nor a crackpot. He is willing to think. He is willing to doubt what "everyone knows." We could all learn from his fearlessness. He is not afraid of being mocked. He might be correct.

43. **Bob Levine**
October 15, 2009

"Sean is having none of it. He argues that the public is able to differentiate between speculative ideas and solidly tested science …"

Carroll is dead wrong if he believes that. There's an interesting, well-established branch of psychology which studies peoples' pictures of the physical world, based on their intuitive perceptions of what their senses tell them; I've seen it referred to as 'folk physics', by analogy I suppose with 'folk taxonomy'—how people conceive of the biological domain based on their perceptions, and so on—and one of the depressing conclusions from this research seems to be that a substantial fraction of people in the collective subject pool actually do believe that when you drop a bomb out of a plane flying at 30,000 feet, the bomb travels six miles straight down to land at the site the bomber was directly over when the bomb was released. With that kind of foundation, just where does SC get the idea that people are sophisticated enough to recognize the line between the already improbable-sounding tenets of rock-solid modern physics, on the one hand, and nutcase-level fantasizing on the other??

Scientists tend to be rather insulated about the poplular perception of the content of their respective fields… but this is just flat-out bizarre.

44. **chris**
October 15, 2009

"Nielsen is one of the fathers of string theory."
yes
"He is no dummy, nor a crackpot."
yes
"He is willing to think."
yes
"He is willing to doubt what "everyone knows.""
yes
"We could all learn from his fearlessness."
yes
"He is not afraid of being mocked."
yes
"He might be correct."
no way

45. **Aristarchus**
October 15, 2009

We live in a world where many people not only diss evolution but believe that stains on a wall are signs from their god, so whether this is a joke or not, it cannot be taken lightly because way too many people are already off the science wagon (or were never on to begin with).

Or is this just in America? Are Europeans smarter?

46. **Haelfix**
October 16, 2009

I have to hand it to the authors. You would be very hard pressed to come up with a simpler idea that violates more laws of physics so completely and thoroughly.

This one manages to more or less include every single fundamental principle that I can think off, including special relativity, unitarity, cause and effect, the measurement principle, Occams razor, heck quantum mechanics and logic/observation in general.

Kudos. (And Seans defense, even muted as it is, makes me reconsider my Bayesian prior that we are living in 'The Matrix' by maybe a tenth of a percent)

47. **Tim vB**
October 16, 2009

@Aristarchus
I can only speak on behalf of the Germans: Recent polls show that about 30% think that the sun moves around the earth.
There is no "intelligent design" debate because Germans do not care about religion at all (especially in the east, Communism left it's mark) or do not take it seriously enough to engage in any debates about it.
Oh, and I think the German department of defense did not order an anti-gravity weapon because they have not enough money, not because they are smarter than the Americans.
Remember that highschool education takes you to Faraday and no further and imprints a deep unease and frustration and distrust in most students regarding everything that sounds like science. If the fate of the human race would depend on a layperson to understand more of the word quantum than remembering the last Bond movie we would be doomed.

48. **Aristarchus**
October 16, 2009

I thought this blog response to the recent Higgs Boson LHC issue was both
interesting and informative even beyond the main story:

http://backreaction.blogspot.com/2009/10/science-writers-and-public-bizarre-love.html

49. **Proxi**
October 20, 2009

The LHC will fail over and over again and will be closed before 2018.
Just wait and you will see.

Just for fun: draw these cards! 🙂

Nielsen-Ninomiya and the arXiv

October 14, 2009
Categories: Multiverse Mania

Because of the New York Times article discussed here, four recent papers by Nielsen and Ninomiya have been getting a lot of attention in the blogosphere. Pretty much all of it has been unremittingly hostile, when not convinced that these papers must be some sort of joke (except for this from Sean Carroll). I just noticed that these papers have gotten some attention from administrators of the arXiv, who have decided to reclassify three of them, presumably since the appearance of the NYT article.

The first in the series, arXiv:0707.1919 was originally posted in hep-ph, with a cross-listing to hep-th (see the Google cache of Oct. 5), but has now been re-classified as gen-ph (cross-listed as hep-ph and hep-th). Similarly, arXiv:0711.3080 has been reclassified from hep-ph to gen-ph, cross-listed to hep-ph (see Google cache of Sept. 12). I'm not sure what arXiv:0802.2991 was originally classified as, but the Sept. 3 Google cache has it as the same as now, gen-ph, cross-listed to hep-th. Finally, the most recent one, arXiv:0910.0359, was originally classified as hep-ph (Google cache of Oct. 7), now it has been re-classified to gen-ph, cross-listed to hep-ph.

While the arXiv administrators seem to be indicating that they share the common opinion that these are crackpot papers, one thing there does remain constant: trackbacks appear there to various press stories and blog postings about these papers, but trackbacks to this blog seem to be censored.

Update: Trackbacks to blog postings here on this Nielsen-Ninomiya subject have now appeared. The ways of the arXiv remain mysterious to me. About all I can tell is that trackbacks to some sources appear more or less immediately, presumably automatically (for instance the trackbacks to the original NYT article). For other sources, e.g. this one, they only appear in batches, often several days later, presumably after someone has gotten around to considering the matter…

Comments

1. **Kea**
   October 15, 2009

   But gen-ph is supposed to be for papers that no serious physicist would read. Why do they bother, given the publicity these papers have?

2. **ManyMe**
   October 15, 2009

   I am not surprised that Sean Carroll defends the Nielsen-Ninomiya paper.
   He is the expert on the arrow of time problem and the one who can proof the existence of the multiverse by making an omelette.
   And he has a book to sell…

3. **Tim vB**
   October 15, 2009

   What Sean Carroll says is IMHO correct, the critical path of a hoax is that there is enough content to let it appear to be something interesting, something one can discuss, while leaving enough prove of nonsense to reveal the hoax later. That he does it shows clearly how far off the track some (small) part of the string theory community is. Hey, but we can discuss this too, cant' we?
   First of all: What is time? To stay focused, let's add "in GR" to the question. Well, you have events, pick A and B, and these can have a causal relationship, meaning: Either A nor B can influence one another, xor (excludive or) A can influence B xor B can influence A. Lets consider A can influence B. Now and only now you can say "A occurs before B".
   If someone says "we have future and past and the future influences the past" doesn't that mean that she does not use time as a causal relationship? IMHO the authors should therefore explain what concept of time they use before we can take the diskussion any further. (Since Sean mentons the "non-locality of string theory" this has probably been done, in that case please provide a link, I was not aware of any of this).
   And shurly you noticed that the card drawing game depends on the actions taken depending on the outcome. Meaning, if you draw "don't operate the LHC" and then decide "well, that was a test, let's try again", the experiment is spoiled. It only works if you are really really shure about acting accordingly to what the card says. This connection of the outcome of an experiment to ones state of mind (after the execution of the experiment) clearly takes all physical theories way beyound their realm of applicability.
   If any of the authors reads this, I'd rather be more interested if you have your fun, or if you are embarrassed that the scientific community fell and continues to fall for your hoax (or both, this "or" is not exlusive 🙂
   Does anyone know a desperate unemployed postdoc willing to produce papers of increasing nonsense to test experimentally what will be accepted by established journals? (or the arXiv? or Sean? or LM? – strike the last, the Bogdanovs did that already). If so, I'll provide my contact information and will happily share my thoughts on the topic.

4. **DB**
   October 15, 2009

   The Nielsen-Ninomiya papers remind me of an old Larry Niven story, *Rotating Cylinders and the Possibility of Global Causality Violation*. That title was in turn borrowed from a serious paper by Tipler.

5. **Bob Levine**
   October 15, 2009

   "The Nielsen-Ninomiya papers remind me of an old Larry Niven story, Rotating Cylinders and the Possibility of Global Causality Violation. That title was in turn borrowed from a serious paper by Tipler."

   I remember that story—very good, one of Niven's best I thought at the time. But there's a bit of difference between that and the N/N paper, no? In that story, the relevant planet's sun goes nova to stop the "tojan horse" give of time-reversal causality from being presented to the enemy. The point was that the universe doesn't allow time-reversed effects to impinge on its unidirectional causal structure. What N/N seem to be suggesting is the contrary: that the universe itself resorts to time-reversed causal events…

   I gotta say, I find Niven's take on it a good deal more plausible than the N/N story…

6. **Yatima**
   October 15, 2009

The arxiv doesn’t keep a history of its classifications? That’s lousy.

7. **Peter Woit**
October 15, 2009

Yatima,

As far as I can tell, they don’t. I think it’s very unusual for this sort of change to be made, especially for papers that have been posted for a long time (since 2007 in this case).

8. **Coin**
October 15, 2009

DB: I imagine someone’s already mentioned this in one of the previou threads, but isn’t this entire thing basically the plot of Einstein’s Bridge?

9. **zevans**
October 15, 2009

Stephen Baxter – Time – is this idea in reverse, various particle physics events ’caused’ by ‘downstreamers’ ie future selves.

10. **anon.**
October 16, 2009

“… trackbacks appear there to various press stories and blog postings about these papers, but trackbacks to this blog seem to be censored.”

Your blog doesn’t leave the right impression about string theory, and burying the head in the sand (censoring trackbacks) is the natural reaction to bad news exhibited by the ostrich.

11. **Alejandro Rivero**
October 16, 2009

Yep, penalties in the Arxiv block files seem to be ethernal; yesterday I did not remember my actual password and tried accidentally a blocked one, from years ago: it was there, after five or six years. And I guess it will be after 10 or 30. Probably it is because such bans are “legacy”, from orders received or executed by previous sysadmins.

12. **Peter Woit**
October 16, 2009

Trackbacks to these two blog postings appeared today, I’ve added an update about this to the posting.

13. **Per**
October 16, 2009

Hi Peter,

I think your attitude is in general a bit to negative. There is no harm in putting these papers on the arXiv. The people who wrote them are respectable scientists, and allowing something like this from them wont open the door for any crackpot to upload something.

14. **srp**
October 16, 2009

The best story about causality from the future is Benford’s Timescape, which as a bonus has actual realistic scientists as its protagonists.

15. **Peter Woit**
October 16, 2009

Per,

I agree that there are few enough serious scientists posting utter nonsense like the last Nielsen-Ninomiya paper that there’s no danger they will overwhelm the arXiv. I was just a bit surprised to see it originally posted in hep-ph, thought the moderator would not allow it there, especially since it was number 4 in a series. Not surprised that when it drew attention, it was moderated elsewhere.

The real problem though is not posting of crackpot papers on the arXiv, there will always be some of those. It’s when these things get taken seriously by some physicists, and picked up by the media that there’s a problem.

16. **Pawl**
October 17, 2009

The NN papers have been criticized for, among other things, introducing “miraculosity” [actually they use “miraculocity”]; if I understand the papers correctly, this term is supposed to refer to the violations of conventional causality, specifically predictability.

It seems to me that “miraculosity” in a similar (not quite identical) sense should be a problem for brane-world physics. After all, disturbances could appear, to us on the brane, out of the bulk with no apparent (four-dimensional) causation. To suppress these effects, one would need, not only extremely strong constraints on the dynamics, but a theory of what the initial conditions in the bulk were. Ensuring that unwanted “miraculosity” is not present is presumably an extremely strong constraint on branes; I don’t know whether the theory is developed well enough to speak realistically to this.

17. **Chris W.**
October 17, 2009

Pawl’s comment points up what might be the principal value of papers like those of Nielsen and Ninomiya.

If the proposed theory is afflicted with clearly unacceptable side effects, especially side effects that inspire heated discussion, people might well be prompted to ask what protects some other theories—apparently more worthy of being taken seriously—from similar pathologies. It may be that without further constraints—perhaps unrealizable or unacceptably ad hoc constraints—these other theories will also turn out to be vulnerable. The end result may therefore be to identify good reasons to *rule out* several heretofore plausible ideas, some of which already have multiple arguments in their favor.

If this happens, it constitutes genuine progress. Of course, brane-world physics already offers multiple reasons to worry about its ultimate viability.

18. **Delusion**
October 22, 2009

I argued against a theory of infinite universes a few months ago. If I may briefly summarize myself:

If infinite universes exist, we must have some means (now or in the future) to access to them to matter. Infinite universes who we cannot exchange energy, matter, or information with are functionally equivalent to this being the only universe, and is therefore metaphysical garbage.

So, if infinite universes exist, every possible reality that is consistent with fundamental laws of physics is present, an infinite number of times. If infinite universes can have different physical laws, that doesn't change anything; I'm merely taking the most conservative version of this fantasy into account.

Since we are assuming that these universes are accessible to one another, there are literally an infinite number of universes whose most powerful inhabitants consider it their sole mission in life to destroy every other universe, and also an infinite number of universes bent only on destroying our very specific universe, and also an infinite number of universes bent only on destroying you and I in this specific universe.

We're here, so the infinite universe theory is out.

In your experience, do most string theorists believe in an infinite universe theory where everything can physically happen does, in another universe? Even so much as the results of chance interaction between fundamental physical particles? Do they perhaps assume an infinite universe theory where the universes don't have any access to one another, and if they don't, how do they attempt to reconcile this?

Or is it the case that most string theorists who believe in multiple universes believe in a finite number of universes (though that number may be staggering), along with Nielsen-Ninomiya?

To me, the entire subject of infinite universes (or an amount large enough to be functionally similar to infinite) and the idea that we are another race's model universe, or that we will all be virtually resurrected from a perfect data source smacks of wish fulfillment and a desire for a scientific gloss to wrap around what is effectively a science-fiction version of immortality and afterlife.

It can make for good science fiction, I suppose.

Short Items

October 17, 2009
Categories: Uncategorized

- There's a wonderful new research mathematics site: Math Overflow. For some discussion of it, see here and here.
- For yet another wonderful new site about research mathematics, there's the French Images des Mathématiques.
- Why is there nothing in theoretical physics anywhere near as good as the above two sites?
- Via Flip Tanedo, an NPR story about Berkeley's parking spaces for Nobelists. He neglects to mention that, starting with Vaughan Jones in 1990, Berkeley started providing equivalent parking spaces for Fields medalists.
- It looks like multiverse mania is not just an American phenomenon, since there's a new popular book on the multiverse out in Germany Die verrückte Welt der Paralleluniversen, by Tobias Hürter and Max Rauner. For a synopsis in English, see here. The authors have a blog, Multiversum.
- The Perimeter Institute has just announced more details of their expansion plans. The new 55,000 square foot expansion of their building will be named the Stephen Hawking Centre at Perimeter Institute. They have doubled the number of Distinguished Research Chairs to 20, with ten new appointments announced here. Director Neil Turok is giving a talk about their plans today, video should be on-line soon.
- This week at Perimeter they're having a Quantum to Cosmos Festival. It started off Thursday night with a discussion by 9 physicists organized around "what keeps them up at night". String theorist David Tong explained that he used to be kept up at night worrying about whether string theory unification could ever be tested, scientifically justifying the subject. Nowadays though, he says he sleeps fine since he no longer needs to worry about this: even if string theory unification is untestable, string theory research can be justified because it provides approximate calculational methods that might be useful in nuclear or condensed matter physics.

Comments

1. **Patrick Tam**
   October 18, 2009

   One of the creators of mathoverflow told me that stackoverflow.com inspired him. Stackoverflow is similar except for programming. Eventually, stackoverflow offered a service (http://stackexchange.com/) to run sites similar to it. The mathoverflow folks decided to use that. So math use to be in the same position (without good sites) you find theoretical physics. It is conceivable that the theoretical physics community will eventually set up something similar with the stackexchange service.

2. **Anton Tykhyy**
   October 18, 2009

   Actually there already exists a physics site, the problem being that it's still empty — http://physics.stackexchange.com/

3. **PhilG**
   October 18, 2009

   Math Overflow needs TeX support to make it worth using.

   I much prefer the MathLinks forum which is an offshoot of The Art of Problem Solving. It is based around maths contest style questions but you will find discussions about maths problems at all levels there. It has been around for a few years and has over 70,000 registered users thanks to excellent TeX features and an friendly open atmosphere.

4. **Thomas**
   October 18, 2009

   Don't UseGroups (nowadays aka Google Groups) fulfill the same purpose as MathOverflow/StackExchange?
   And they seem to be quiet alive and kicking!

5. **Tim vB**
   October 18, 2009

   Skimmed "Die verrückte Welt der Paralleluniversen", the good thing is that this book does not hype the multiverse, but is just an attempt to draw some money from the existing hype. With some luck this thing will not be published in English. God, I think I lost 2 IQ point by just reading pages 100 to 150.

6. **Bob Levine**
   October 18, 2009

   "Nowadays though, he says he sleeps fine since he no longer needs to worry about this: even if string theory unification is untestable, string theory research can be justified because it provides approximate calculational methods that might be useful in nuclear or condensed matter physics."

   So we've gone in a few short years from 'the only game in town' to computational heuristics?? Wow, how the mighty are falling… reading this statement from Tong, I could not get out of my mind the horrendously vivid scenes from Sergei Bondarchuk's 1968 8-hour ultramasterpiece version of WAR & PEACE depicting the deathmarch retreat of Napoleon's Grande Armée after the disastrous march on Moscow. If I were DT, I don't think I'd be sleeping particularly well at all…

7. **joey**
   October 18, 2009

   > calculational methods that might be useful in nuclear or condensed matter physics.

   might…!? what did he mean, *might* be useful? Just *might*, like in *might* be testable one day?

   I don't think I'll be able to sleep tonight…

8. **Chris W.**
   October 18, 2009

   From the math***overflow*** FAQ:

   > **What is this? Who are you?**
   > Math Overflow runs on Stack Exchange, the hosted service that provides the same software as the popular programming Q&A site Stack Overflow. The hosting cost is paid from the research funds of our generous benefactor, Ravi Vakil of Stanford University.

> Keeping Math Overflow clean, civil, and on topic are its moderators: David Brown, Daniel Erman, Anton Geraschenko, Scott Morrison, and Ben Webster. If you have any problems with the site, contact Anton Geraschenko (geraschenko@mathoverflow.net).

As a long-time reader of Joel Spolsky's blog, and occasional reader of stack**overflow**, I have to wonder why the people behind this new site felt compelled to ape the *name* of Spolsky's and Atwood's original site. After all, the term *stack overflow* has a particular meaning and significance to programmers, whereas *math overflow* means nothing, really. The first offshoot of stack**overflow** based on the Stack Exchange platform is called server**fault**; it's for system administrators and IT professionals. The common theme is that of a breakdown or failure that somebody is trying to understand and resolve, although the scope of the questions on these sites is broader than that.

9. **teaser**
October 18, 2009

So does this mean that string theorists will be absorbed into the condensed matter physics departments at universities everywhere? That should help the department numbers for these folks – and their funding.

10. **Aristarchus**
October 20, 2009

Stephen Hawking's position at Cambridge has been replaced by a string pioneer:

http://www.guardian.co.uk/science/2009/oct/20/stephen-hawking-michael-green-cambridge

Higgs, Dark Matter and Supersymmetry: what the LHC will tell us

October 19, 2009
Categories: Uncategorized

The Council for the Advancement of Science Writing is holding a New Horizons in Science conference right now in Austin. This morning Steven Weinberg gave a talk, now available online, with the title Higgs, dark matter and supersymmetry, what the Large Hadron Collider will tell us. He described the Higgs as something definitely expected, supersymmetry as a much more speculative possibility, but had nothing to say about string theory during the talk. In the question session, Tom Siegfried of Science News asked him about why he hadn't mentioned string theory, and what its prospects now were, 25 years after first being heavily promoted to the press. Weinberg answered:

> It's developed mathematically, but not to the point where there is any one theory, or to the point that even if we had one theory we would know how to do calculations to predict things like the mass of the electron, or the masses of the quarks. So, I would say, although there has been theoretical progress it's been, I find it disappointing. One of the hopes would be that the LHC would provide a clue to something we're missing in superstring theory and I think there supersymmetry is the most likely place to look.
>
> One of the troubles with superstring theory is that although in a sense the theorists think there is only one theory, there are an infinite number of approximate solutions of it and we don't know which one corresponds to our world. But at least in a large variety of the solutions of superstring theory there is supersymmetry visible at low energies, and if we see supersymmetry at low energies, superstring theorists may be able to derive from it some kind of clue as to how to solve these theories. But I haven't talked about it in this lecture because I don't see how that would work, it would be.. I mean I couldn't say that that was likely with any degree of sincerity, and certainly the LHC and any other accelerator that we can imagine being built will not get up to energies which are high enough so that we can directly see the structures that are described by superstring theory, the strings or the D-branes or whatever it is. Those will not be accessible at the LHC, so any clue we get will be very indirect.
>
> I myself, well I was working on superstring theory in the 80s and gave it up because I… I moved into cosmology, which in the last couple of decades has had the excitement that elementary particle physics had in the 60s and 70s, a wonderful coming together of theory and observation. Cosmology now reminds me of the excitement that I felt when I was younger and doing particle physics.. and it's a pity that superstring hasn't developed better. I still think it's the best hope we have, I don't know of anything else. My own work very recently has been trying to develop an alternative to superstring theory as a way of making sense out of quantum gravity at very high energies. But even though I'm working on this I still find superstring theory more attractive, but not attractive enough…

Siegfried gives an account of the talk here. It includes a new remarkably convoluted and misleading way of referring to the fact that string theory predicts nothing at all about observable physics:

> But despite a quarter century of intense effort, superstring theory has not produced a cohesive and clear guide to testing its fit with all the observable features of physical existence.

Comments

1. **sfjp**
   October 19, 2009

   Well, it's a shame that in such conferences speakers do not have a tie-microphone. Hanging a hand microphone is so much dated, and so much painful for old people…

   Whatever, wehn I was a young student, I attended in Paris a conference by Alfred Kastler, about the origin of quantum theory. I must say, what he told us then was so much unexpected and unheard of, that I am still muling his lesson, thirty five years after. Weinberg is a too well known teacher and Noble price, what he can say is much less original… (putting away the fact that I find his books about QFT ones of the less pedagogical, except for references)

2. **Bob Levine**
   October 20, 2009

   "Siegfried gives an account of the talk here. It includes a new remarkably convoluted and misleading way of referring to the fact that string theory predicts nothing at all about observable physics"

   Sigfried's conspicuous lack of—to put it delicately—intellectual candor, so far as string theory is concerned at least, is the main reason that I let my subscription to Science News lapse. If the periodical is going to be turned into just another mouthpiece for string theory 'irrational exuberance', what's the point of supporting it? His skewed choice of coverage (including one particularly contentless last-page essay by some guy whose name I've succeeded in forgetting that made some empty dismissive comments about Lee Smolin's book) shows that he's pretty much just a shill for an agenda in science that has become far more a matter of ideology than a real research program—one that is willing to commit itself to gaining some battle-tested results, as Wheeler put it, before claiming ANY degree of success.

   But I'm glad to see that SW has finally said something publicly about his doubts. I wonder how the latter will affect his pronouncements about the anthropic principle and the predictive possibilities of physics as a science—he was pretty bullish on the AP for a while, as I recall…

3. **chris**
   October 20, 2009

   Bob,

   i think you are mixing things up a bit here. afaik, Weinberg *invented* the anthropic principle (or to put it better was the first one to point out how it can be applied to particle physics/string theory). with so many things that he has pioneered, he really does not need to advertise or push anything (except his book maybe). i always found him extremely honest and lucid in his statements.

   just look how he is talking about the asymptotic safety scenario in this very citation given here. he invented that too, you know.

4. **Bob Levine**
   October 20, 2009

   " with so many things that he has pioneered, he really does not need to advertise or push anything (except his book maybe)."

   Chris—
   My impression was that Brandon Carter and John Barrow were the ones who introduced the AP into the discourse in physics, and that Weinberg's comments invoking the AP were connected with the landscape interpretation of string theory, which seemed to be the only way to give a realistic interpretation to the vast number of possible

vacuum states implied by the theory (or program, or whatever the right term for it is). I could be quite mistaken about that, for sure. But I wasn't thinking of SW's promoting or advertising the AP so much as perhaps dropping it altogether. After all, if you no longer take string theory seriously as a promising direction in pursuing a 'final theory', then a lot of the motivation for the landscape, and hence the AP, disappears along with it. And it certainly sounds as if Weinberg no longer has anything like the view of ST he was articulating earlier. So it makes me wonder if we'll be hearing anything further about the AP from him, or if he instead views the whole AP/landscape quagmire as a dead end.

"i always found him extremely honest and lucid in his statements."

I wouldn't disagree at all with this. It's Siegfried who I find profoundly, um, *disingenuous* in the way he uses SCIENCE NEWS to try to minimize/deny the foundational problems that ST faces (whether it wants to or not).

5. **PhysicsPhile**
October 20, 2009

Bob,

My impression from reading Weinberg's recent papers is that he still believes that the multiverse is the only available explanation for the cosmological constant problems. Note that the multiverse is an independent concept from the string theory landscape. But the latter does give an explicit construction for the former.

6. **Peter Woit**
October 20, 2009

What I found interesting about Weinberg's talk and comments was that, whatever his views on the multiverse, AP, extra dimensions, string theory landscape, etc. might be, he decided not to mention these at all in his talk. He takes a very different approach than many other prominent figures in theoretical physics, who think it's a good idea to get the press and the public "excited" by feeding them all sorts of highly speculative nonsense. Because of this latter approach, people like Siegfried go out and write books based on such nonsense, and have a completely misguided view of the subject. Also as a result, both questioners wanted to ask Weinberg about string theory, which he hadn't talked about, not about the solid science he did talk about.

7. **Peter Woit**
October 20, 2009

Bob,

I think the Science News piece you refer to is this one:

http://www.sciencenews.org/view/generic/id/45196/title/Comment__Five_problems_in_physics_without_the_definite_article

which described Lee Smolin's book as "silly attacks on string theory", but which I'd describe as a silly attack on Lee Smolin…

8. **Bob Levine**
October 20, 2009

Peter—yes, that's the one. I'd already pretty much decided that in good conscience I couldn't really continue supporting SN, and it's been in the back of my mind for a while that if Siegfried is pursuing this kind of partisan agenda in one branch of science, what sort of illusions of consensus is he trying to promote in biology, psychology and so on?…

…and to PhysicsPhile: yes, point taken, except that the AP seems to have been most aggressively flogged of late in connection with the string landscape business, and reading Peter's citation of SW's evident disillusionment with ST suggests that maybe the drunken orgy phase of ST—and its sociological domination of the field in terms of grantsmanship, hiring and the like—is about to come to an end, if even high-profile advocates such as Weinberg are having second and third thoughts.

"Also as a result, both questioners wanted to ask Weinberg about string theory, which he hadn't talked about, not about the solid science he did talk about."

An old, wise professor of mine in graduate school once commented bleakly that you can't kill a bad idea. That's doubly true I think if giving up the bad idea leaves you in a relatively unsatisfactory, frustrating situation given your ambitious hopes for a real working GUT (which is presumably why Siegried and others like him want to keep the party going).

But is it too much to hope that the various bits and pieces we've been reading about here (David Tong's comments, Weinberg's frank acknowledgement that ST has, in terms of real physics, gone nowhere, and the rest) are a sign that the town drunk is beginning to sober up at last?

9. **Cormac O Raifeartaigh**
October 20, 2009

what did he say about DM?

10. **Michael Good**
October 20, 2009

Please blog your thoughts about Green's new post.

I'm interested in what you think about it!

http://www.guardian.co.uk/science/2009/oct/20/stephen-hawking-michael-green-cambridge

11. **Peter Woit**
October 20, 2009

Cormac,

You can watch the video, but from what I remember Weinberg just made conventional points, explaining some of the astrophysical evidence, the possible connection to supersymmetry, and the possibility of production of dark matter particles at the LHC.

Michael,

Green seems like a reasonable choice for the post. While I complain about string theory and the faddish way it has been pursued, it's worth noting that there's a major difference between Green and most other string theorists: he was doing important work moving the field forward at a time that this was a very unfashionable thing to be doing. He and Schwarz deserve a lot of respect for that.

One odd thing I saw mention of somewhere is that Green is only 4 years younger than Hawking, so presumably will be stepping down from this position rather soon after taking it.

12. **Marcus**
October 20, 2009

PP said
…impression…recent papers is that he still believes that the multiverse is the only available explanation for the cosmological constant problems…

Impressions of what someone still believes—rather than what they actually say—can certainly be tricky. I couldn't find anywhere that he actually says that, in his ten papers from the last four years. A lot has happened during that time, so earlier than that would not seem exactly "recent".

http://arxiv.org/find/grp_physics/1/au:+Weinberg_S/0/1/0/2006,2007,2008,2009/0/1

Maybe you can point out for us which of these papers actually says that he still believes such and such. I could easily have missed a reference.

If on the other hand you are using your intuition to guess another person's state of belief, without his explicit statement, that's…well…different. Tells us more about you than about Weinberg.

We should then probably be discussing your own ideas, rather than projecting them on him.

The two *most* recent papers of Weinberg, both from 2009, and his July 6 talk opening a conference at Cern, all cited extensively some work by Reuter, Percacci, and others on the "asymptotic safety" treatment of gravity—allowing Newton's G and the c.c. to run with the renormalization group flow. According to a paper of Reuter et al that Weinberg cited, this offers another way to understand the cosmological constant. I don't know what Weinberg thinks or believes, but he explicitly said in his Cern talk that his current research focus is on asymptotic safety (a concept he introduced in 1976) and its relevance to cosmology.

On the other hand, I've seen nothing from him recently either of multiverse or anthropic argumentation. If you have please provide a reference.

13. **Peter Woit**
October 20, 2009

Marcus,

In his comments at the end of this talk, Weinberg mentions that he is working on asymptotic safety, but says "I still find string theory more attractive". It's a reasonable interpretation of this to think that he hasn't changed the views about the CC, multiverse, string theory landscape that he wrote about in "living in the multiverse" back in 2005. If he has come to the realization that there is something fundamentally misguided about most multiverse research, he hasn't said so publicly, as far as I know.

In any case, I think, as shown in this talk, Weinberg is careful to keep in mind the distinction between what is solid science, with real evidence behind it, and what is highly speculative, and thus likely to be wrong. And all explanations of the size of the CC now available are in the latter category, no matter which of them is your favorite.

14. **Frank**
October 20, 2009

Peter, Weinberg is careful, but not in all statements. For example, he gives talks since 30 years saying that the essence of nature is its symmetry (repeating what Heisenberg said). Symmetry is surely important, but whether it is of fundamental importance is questionable. Weinberg's stress on symmetry and its role was the reason that so many researchers looked for larger and larger symmetries (GUTs, supersymmetry, string theory). It is not at all clear that this is the right approach for fundamental physics – especially since none of these symmetries has shown up. The search for larger and larger symmetries is usually taken for granted, due to Weonberg's constant stress on the issue – but is it the correct approach? Maybe the LHC can give us a hint about this fundamental issue.

If the LHC does not find any new symmetry, then a complete change of mind might be required. Maybe there is no larger symmetry in nature, apart from those that are known?

15. **Peter Woit**
October 20, 2009

Frank,

Weinberg is far from the only one who thinks symmetry is a fundamental principle. This was a very fruitful idea throughout the 20th century. My own view is that it will continue to be one, but one has to keep in mind that new ways to exploit symmetry principles may need to be found. The way gauge symmetry is realized in the electroweak theory is a good example, it's rather subtle and still not completely understood non-perturbatively.

So, if Weinberg is pushing symmetry as a principle, I'm with him. But to get something new it's likely to require something much more non-trivial than just replacing SU(3) by SU(5).

16. **Frank**
October 20, 2009

Peter, I understand what you say: but people have tried GUTs, supersymmetry, quantum groups, BRST, and so many other ideas. None worked. It is quite courageous to think that more symmetries – apart from the gauge symmetries of the standard model – play a role in nature. Thousands of people have looked for more general symmetries, but none has been found. So the question arises: is a larger symmetry really needed?

17. **Peter Woit**
October 20, 2009

Frank,

My own point of view is just that there are fundamental questions about how to handle things like gauge symmetry in QFT that remain poorly understood. They're deep questions both about mathematics and about physics, so worth understanding. It's true that such efforts haven't yet led to any breakthroughs in physics, and maybe they never will. But, historically, looking for new ways of exploiting symmetry principles has been very fruitful, I don't you think you need to be a wild-eyed optimist to suspect this might still lead somewhere interesting.

And, it's not like there are a lot of other ideas out there which are working out…

18. **Chris W.**
October 20, 2009

I think one can make a strong case that the asserted symmetries that have been most fruitful are those that express a *principle of indifference or non-observability*, for which empirical support was already in hand. Special relativity is the obvious case in point.

Asserting that "nature loves mathematical symmetry" strikes me as simply missing the point, and research programs based on such an assumption are likely to get lost in a mathematical labyrinth. Of course, if one loves mathematical structure for its own sake, one may not care to escape from that labyrinth. [🙂]

19. **Eric Habegger**
October 20, 2009

Peter, you said,
"In his comments at the end of this talk, Weinberg mentions that he is working on asymptotic safety, but says 'I still find string theory more attractive'. In any case, I think, as shown in this talk, Weinberg is careful to keep in mind the distinction between what is solid science, with real evidence behind it, and what is highly speculative, and thus likely to be wrong. And all explanations of the size of the CC now available are in the latter category, no matter which of them is your favorite."

It seems to me your observation that his work on the reason for the value of the small CC is probably wrong is somewhat misleading. For one thing it ignores the fact that there is a vital difference between the CC being small and "zero" which was assumed until 10 years ago. The CC is now simply small and the current estimate is about 0.7
.

Much of the work in particle physics, including the SM assumed a zero CC, though it wasn't really part of the SM. This new value of the CC really does require a rethinking of physics and I think it really is a gross exageration to just minimize it and say asymtotic safety "still" has nothing to do with it. When Stephen Weinberg says he finds string theory more satisfying than this new direction, to me it just means he would rather have not had the CC turn out to be non-zero. But now that it is new directions are required because the facts have changed. That doesn't mean he wouldn't have preferred the previous fact, i.e. the CC = 0, to still be in force.

20. **Peter Woit**
October 20, 2009

Eric,

I see no reason to believe that Weinberg's preference for string theory over asymptotic safety as an explanation for quantum gravity has anything to do with the CC. In any case, he explicitly says that string theory, while better than known alternatives, is not attractive enough an idea to be convincing.

The point I was trying to make is that Weinberg, unlike an increasingly large number of people in theoretical physics, understands very clearly the difference between solid ideas which we have good reason to believe are true, and highly speculative ones that are probably wrong. He was carefully avoiding going on about the latter to this group of journalists.

Chris W.

Sorry, but that's just not true. Most uses of symmetry principles in physics involve using the symmetry group to decompose states into non-trivial representations of the group. The action of the symmetry group is very much observable.

Actually, most well-known observables in QM are the infinitesimal generators of symmetry group actions on states (momentum, angular momentum, charge). Non-observability is only relevant in the case of local gauge symmetry, where you expect states to transform trivially (although even in this case, the situation is much more complicated).

21. **Christine Dantas**
October 21, 2009

Eric Habegger wrote:

It seems to me your observation that his work on the reason for the value of the small CC is probably wrong is somewhat misleading. For one thing it ignores the fact that there is a vital difference between the CC being small and "zero" which was assumed until 10 years ago. The CC is now simply small and the current estimate is about 0.7.

To be more precise: observations require, apart from the usual radiation, baryonic and dark matter components, a "dark energy" component, which in dimensionless terms requires a energy density of 0.7 (relative to the critical energy density). One can treat it as a fluid and see that it leads to a "repulsive" gravitational effect. There are several possibilities for this effect. In one of them, usually regarded as the simplest one, is not a fluid, but the cosmological constant. In this case, the theory introduces a length scale related to the dark energy density. There is a dimensionless combination of that length scale, G, c and the Plank constant, and it is this dimensionless combination that is required (observationally) to be of the order 10^{-123}. What people believed previously was, despite of being a puzzling fact per se, that this dimensionless combination was exactly zero. However, what current observations require is that this number is today extremely small in fact, but not zero. This requires a big fine tuning and consequently a big problem to solve.

22. **Christine Dantas**
October 21, 2009

To sum up my previous comment: to begin with, for many decades, the cosmological constant was thought to be exactly zero (although no one had an explanation for that), then came observations leading to a large energy density component, to which several explanations have been offered. The cosmological constant came as one of them, but then it introduces a natural dimensionless combination which is extremely small.

23. **Physicsphile**
October 21, 2009

Christine,

If you want some other source for the dark energy, you still have to explain why the cosmological constant is then zero as vacuum fluctuations and phase transitions imply it should be 10^{many} orders higher. The nice thing about the multiverse/landscape (not necessarily string) explanation is it kills these two birds with one stone. But I do agree with Peter that with our present state of knowledge it is still not a testable explanation. Never the less it is the only one available so I think it shouldn't be treated as irrelevant.

24. **Christine Dantas**
October 21, 2009

Physicsphile,

I agree that if for some reason the dark energy problem "goes away" (whether observationally or by some alternative well-tested explanation not involving the CC) , it is clear that it will still be necessary to understand why the CC is zero. This problem existed before the dark energy problem, and may be independent of it.

As I see it, the CC problem must be explained through quantum gravity; classically the CC can be gauged away, this tells something. I do not think that the multiverse/landscape solves the problem. Conceptually, many more problems arise than vanish. You are entitled to disagree on this.

25. **Peter Woit**
October 21, 2009

Please enough about the CC. This has nothing to do with Weinberg's talk, where he didn't mention this topic at all, for good reason…

26. **Christine**
October 21, 2009

Peter,

Just a suggestion, but of course you run your blog as you find correct. If you thought that the CC was off-topic, why have you not cut the matter from the very root? I thought it was reasonable to make a few clarifications about someone else's comment about the CC, so I took my time to write here, feeling that it was a constructive thing to do. It's not the first time that this happens. I know this is difficult to manage, since I have a blog myself. But at the same time it is hard to receive a "enough of this!" at one's face, when one is just trying to be constructive on a matter *already* accepted and published in the comment's section.

27. **Peter Woit**
October 21, 2009

Hi Christine,

I wasn't criticizing your clarifications, I just wanted to discourage other people from pursuing this topic further here.

It's natural for a discussion to evolve away from the original topic, but I really want to stop comment threads here from evolving into a general physics discussion board, unrelated to the postings. The main reason for this is simply that moderating such discussions is difficult, time-consuming and unrewarding work, something I don't have the time or interest to be doing. It's hard enough to figure out how to sensibly moderate comments that are on-topic…

28. **Marcus**
October 21, 2009

I think it would be a good idea to focus on what SW actually said, and not try to mindread about what he does or does not "still believe". The discussion has been interesting, especially Christine's comment, but we could waste our opportunity to learn, if we don't pay adequate attention to the actual soundbites. Some are just repeats of what we've heard before, but others could indicate a shift of perspective or a change in the zeitgeist weather.

Here are some essential snippets, some oldish, some newish:
==excerpts==
It's developed mathematically, but not to the point where there is any one theory, …

… although in a sense the theorists think there is only one theory, there are an infinite number of approximate solutions of it and we don't know which one corresponds to our world.

… in a large variety of the solutions of superstring theory there is supersymmetry visible at low energies, and if we see supersymmetry at low energies, superstring theorists may be able to derive from it some kind of clue …

… I don't see how that would work, it would be.. I mean I couldn't say that that was likely with any degree of sincerity, …

I myself, well I was working on superstring theory in the 80s and gave it up because I… I moved into cosmology, which in the last couple of decades has had the excitement that elementary particle physics had in the 60s and 70s, a wonderful coming together of theory and observation…

… My own work very recently has been trying to develop an alternative to superstring theory as a way of making sense out of quantum gravity at very high energies. But even though I'm working on this I still find superstring theory more attractive, but not attractive enough…
==endquote==

What mainly impresses me is a certain earthy openness, he's willing to expose his hunches—feelings about what is exciting now (why cosmology rather than particle physics) and what is likely or not likely (deriving clue-help for string from appearance of susy at low energy).

29. **j.**
October 22, 2009

Hello, Peter. Could you devote an entry in the blog to comment this recent "development"? I'm really interested in your opinion about it. Thanks!

30. **Peter Woit**
October 22, 2009

j.

Already done, see

http://www.math.columbia.edu/~woit/wordpress/?p=2172

Physicists Calculate Alternative Universes

October 22, 2009
Categories: Multiverse Mania

According to a story in the Stanford Daily, the recent arXiv preprint mentioned here and discussed many other places on the web has given us two new scientific celebrities:

> Two of Stanford's physicists, Professor Andrei Linde and postdoctoral researcher Vitaly Vanchurin, have garnered recent celebrity-status in the scientific community for their recent discovery of the maximum number of alternate universes.

Instead of consulting experts in this field and getting quotes about how significant this pseudo-science is, the writer asks Stanford students, who do a much better job than the experts:

> "I personally find the concept intriguing, but I think we should be wary of scientists who can use it as a way to write things off and stop looking for deeper answers to physical phenomena," Lauren Janas '12 said…
>
> Some Stanford students are not entirely convinced of Vanchurin and Linde's complicated methods.
>
> "I'm quite skeptical," Frank Liu '13 said. "I think it's hard to tell how many universes there exactly are."

The story ends with the mystifying news that the authors hope "that in the future, they can work with modular observations to confirm their findings."

For more media coverage of the multiverse, see here.

Update: Oops, last link was broken, now fixed.

Comments

1. **The Vlad**
   October 22, 2009

   Well, I'm no physicist, but as a computational neuroscientist, I find the following part of the article offensive:

   Linde stressed that their estimate may not be comprehensible to the human brain.

   "Our brain is the final computer to which all the data about the universe is going," she said.

   Linde estimates that the brain's limit to how many alternate states can be processes is 10^10^16, far less than10^10^10^7.

   "If we concentrate on the information our brain can acquire, we're going from real physics to a little bit more philosophical issues," Vanchurin said.

2. **Yatima**
   October 22, 2009

   No need to be offended I would say. It just doesn't make any sense. The article's quotes look like they were cut & pasted by someone with ADHD.

   Still, the incongruous citation of "the number of brain states" makes me suspect that there are Boltzmann Brains involved. Somehow.

3. **Chris W.**
   October 22, 2009

   Meanwhile, Steve Hsu throws a bone to anthropism.

4. **Yatima**
   October 23, 2009

   >>Meanwhile, Steve Hsu throws a bone to anthropism.

   Nice. A whole library of rather well-grounded reasoning about why life should not be improbably at all (starting with Dysons' Origin of Life and ending with modern analysis of genetic algorithms) is thrown out of the window in a handwaving blogpost which also cites Gödel who apparently was caught momentarily under the delusion that life evolves in an environment in thermodynamic equilibrium.

   I want to not believe.

5. **Bob Levine**
   October 23, 2009

   "A whole library of rather well-grounded reasoning about why life should not be improbably at all (starting with Dysons' Origin of Life and ending with modern analysis of genetic algorithms) is thrown out of the window in a handwaving blogpost which also cites Gödel who apparently was caught momentarily under the delusion that life evolves in an environment in thermodynamic equilibrium."

   This is yet another instance of the dark side of The (Internet) Force. Hsu's piece is at best a series of shallow connections fired off in thinking-aloud mode, based in part on the assumption that Gödel's authority based on his consistency/completeness theorems for formal systems gave him some kind of authority in evolutionary biophysics. The Gödel metric notwithstanding, his unsupported assertions on the physics of life strikes me as being kind of on the same level as Hsu's own stream-of-consciousness text. Someone will turn 'cite' Hsu in support of some other bit of dubious semi-science, which will be embedded in yet more vague nonsense, and so it goes.

   This kind of public venting of what is in most cases half-baked speculation that couldn't get to first base against even casual peer review makes the use of the Internet very dodgy for people who have no particular expertise or experience in whatever branch of science is involved. I'll bet Hsu is a lot more careful when preparing his work for that kind of review at, say, Physical Review Letters, but on his blog he can let his hair down and indulge in what is, in polite terms, cocktail party chit-chat. A lot of the public, trawling for information on the internet, probably can't tell the difference. Watch for Hsu's comments to be mirrored endlessly in the creationist blogsphere till hell freezed over by people who are under the impression that his and Gödel's comments constitute actual support for ID.

6. **Tim vB**
October 23, 2009

Did you cry or did you laugh when you read the article? Couldn't make up my mind yet, but I think the quote
"I think it's hard to tell how many universes there exactly are."
would be a good starting point for any Monty Python scene.
How about: Mr. X gained celebrity status by calculating the number of whoffels in the multiverse, "it is probably a prime number" he stated, and conjectured that gaining more insight would probably require to work harder.
Anyone interested to write an article about that?

7. **Christine Dantas**
October 23, 2009

Dantas' Axiom: the number of alternate universes is exactly zero.

In strange times, some people may find what is self-evident as shocking. Now *that* must turn me into a celebrity.

8. **Stephen**
October 23, 2009

It is sad how much Linde has gone away from real physics into wild speculation.

I also point out some sentences:

"Linde describes this as the most accepted theory regarding the creation of the universe."

— inflation is not "creating" any universe, it is just a period of very fast expansion

"Even though the universe is generally infinite in the mathematical sense"

— who told them this? God? or some observation?

"If we concentrate on the information our brain can acquire, we're going from real physics to a little bit more philosophical issues,"

— from real physics?!?

9. **Bob Levine**
October 23, 2009

'"If we concentrate on the information our brain can acquire, we're going from real physics to a little bit more philosophical issues,"

– from real physics?!?'

I know… it's scary, really. When people start talking this way, showing how much they're presupposing, without question or even pro forma caution, whatever pistache of strange assumptions and a priori is current in their hermetically insulated research communities, you start wondering how long science will be able to maintain the credibility it took so many centuries to earn. What the social theory hacks were unable to do, real scientist may well wind up doing to themselves.

Sabine Hossenfelder, in her Blog, has made the case that it's not necessarily the best thing for science to be conducted totally out in the open, so that every single step from initial hunch to decisively confirmed (or not!) prediction is available for public consumption/comment/kibbitzing and so on. I think Linde and Vanchurin would have done *much* better taking her cautions to heart before going public with this stuff…

10. **Arrow**
October 23, 2009

It's amazing that such speculative nonsense actually passes of as physics.

Their absurd statements about human brain are especially appalling, the capacity of human brain would have nothing to do with the "number of universes" even if the latter made any sense. Why do they compare the "states which a brain can handle" with the supposed number of universes? What are they trying to say that each "state of the brain" can somehow represent or comprehend one universe?! This is absurd.

Multiverse idea is dealt with by Ockham Razor.

11. **ObsessiveMathsFreak**
October 23, 2009

A 12 and a 13 year old showing better critical thinking skills than the majority of the theoretical physics community? I think it's time for everyone to take a step back.

12. **neo**
October 23, 2009

"most accepted theory", "popular theory"

It is settled then. We can decide on how many universes there are simply by voting on it.

13. **Peter Woit**
October 23, 2009

OMF,

Those indicate the graduation date of the Stanford students interviewed (2012, 2013), not their ages.

14. **Aristarchus**
October 23, 2009

The so-called reporting I have seen from college students in their local papers who one day hope to become professional journalists concerns me deeply.

So many of them are communications majors who know little about science and it shows. Apparently doing some homework when writing up a story is passé these days.

15. **Peter Woit**
October 23, 2009

Aristarchus,

The young journalist here did a better job than many professionals in terms of coming up with people who could give a sensible response to this particular bit of multiverse mania. The problem with these stories is not the journalist, but the activities of members of the scientific establishment that the journalists are reporting on.

16. **Bob Levine**
October 23, 2009

"The problem with these stories is not the journalist, but the activities of members of the scientific establishment that the journalists are reporting on."

So the question is, *why* are these established scientists doing this sort of thing?

A number of Peter's recent blogposts bear all seem to bear on this question. We have prominent physicists writing about backwards causality mediated by Higgs bosons (talk about a 'God particle'!), exact calculations of bounds on the number of alternative universes… someone has too much time on their hands, is what it looks like. And we all know the reason for that: no new data, no real anomalies to explain, nothing to compute on and get either a match with theory or a discrepancy. The Devil makes work for idle hands… or, another way to look at is is the way people in acquatic isolation tanks, in full sensory deprivation experiments, start to hallucinate. The senses need input and stimulation; if they can't get the real thing, some people think, they'll generate their own spurious signals just to have something to process.

Which is why people like Weinberg have switched fields, in effect, to cosmology. There's actual data there, there are observed effect that need explaining and bear on theoretical models. As long as particle physics lacks something comparable, they'll hallucinate landscapes and backwards causality and all sorts of wondrous things. Please, *please*, let the LHC finally come on stream, and stay there…

17. **Peter Woit**
October 23, 2009

Bob,

Problem is, it's the cosmologists as much if not more than the particle physicists who are hallucinating (Linde, Vanchurin are cosmologists, and the only person who seems to take Nielsen-Ninomiya seriously is a cosmologist…).

18. **changcho**
October 23, 2009

So this is what passes for Physics at Stanford nowadays. Ah, and also the Landscape of course…

19. **Bob Levine**
October 23, 2009

"Problem is, it's the cosmologists as much if not more than the particle physicists who are hallucinating (Linde, Vanchurin are cosmologists, and the only person who seems to take Nielsen-Ninomiya seriously is a cosmologist…)"

Yes… maybe the whole culture of the field has been contaminated by the data-shortage-induced crisis in high energy physics? For several generations, that's the branch of the field that been the flagship of the physics enterprise; if hep starts chasing white rabbits, maybe other theoretically inclined branches are tempted to join in the pursuit?

20. **Tim vB**
October 24, 2009

@Bob Levine
"… no new data, no real anomalies to explain, nothing to compute on and get either a match with theory or a discrepancy."
There are many open problems in hep and cosmology you can work on without engaging in highly speculative ideas with no hope to ever make contact to experiment. That is why I cannot understand why anyone would sit down and try to calculate something like the number of universes in the multiverse. If asked why they did this Linde and Vanchurin will probably not answer "we were bored" or something similar – would be nice if someone at Stanford asked them, I would be interested in the result 🙂
With regard to the Family guy episode: This is hardly new, the whole series Sliders was built on this concept and there are even Star Treck episodes (original series as well as Next Generation) about the existence of multiple universes. Try to calculate what string theory says about the warp drive or quantum torpedoes, that would be slightly more original. ("String theory predicts the warp drive? Bah, LQG predicts the trans worp drive, bruhaha!").

21. **Tim vB**
October 24, 2009

Hello Peter,
Off topic:
there is an interview with Michael Green here:
http://www.guardian.co.uk/theguardian/2009/oct/24/michael-green-new-lucasian-professor
(How do I post a link? Do I just enter the href HTML-Node?).
It is completly unremarkable, at least to me, with the notable exception that Green tells us his opinion about the critics of string theory:
cit. begin:
"Woit [Peter Woit, author of Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law] is a blogger – he runs an anti-strings blog, he's an ex-physicist, a PhD I think. He's at Columbia – a systems manager or something [he also teaches mathematics]. So he's not a professional physicist. He has strong views about string theory, which he's entitled to, and he blogs them. And good for him.
"The other one [Smolin] is a physicist who has a view of physics other than string theory and wants to promote that. And the media made a big song and dance about this, which seemed to me to be completely off-scale with what we experience anywhere in any university. The subject's thriving."
cit. end
What good does it do Green (or Susskind or anyone else) to tell people that Peter is a "computer programmer" or "systems manager"?
You probably need to pass some specific initiation ceremony to be entitled to have an opinion about string theory, a Ph.D. in particle physics is not enough.

22. **teaser**
October 24, 2009

Does anybody know how many epicycles were needed to make the Earth-centered solar system match observation? I guess it would make sense that to account for all of the alternate universes, one would need a much bigger number of epicycles than what we used for the solar system.

23. **Thomas Larsson**
October 24, 2009

Mainstream medieval science was up to 13 epicycles when Kepler tried ellipses. However, epicycle theory, with any number of cycles, only works if planets move in closed orbits, which is only approximately true.

24. **Bob Levine**
October 24, 2009

"There are many open problems in hep and cosmology you can work on without engaging in highly speculative ideas with no hope to ever make contact to experiment."

Tim—that's true without any question. But my guess is, it's the TOE which has been promoted as the Holy Grail of physics, and the burning goal of a generation of physicists who have, from what I can tell, believed that such a theory was just a little bit out of their grasp. A genuine TOE, a theory that worked, from which all of the observed forces of nature would in effect fall out as theorems, would constitute the single most important result ever in science—I can't really see any argument on that point—and, conversely, success in any 'lesser' project—what used to be called normal science—just can't begin to have the glamour of grand unification, the 'final theory', however persuasive such results might be in terms of goodness of fit with experiment.

That kind of thinking is bound to have a distorting effect on the climate of opinion in any collective activity. And the result is that for a lot of physicists, switching research activities to something perceived as being less absolutely fundamental would be a come-down of sorts, an admission of failure…

25. **Aristarchus**
October 27, 2009

Hawking says there were an infinite number of quantum beginnings:

http://www.dailygalaxy.com/my_weblog/2009/10/were-there-an-infinite-number-of-quantum-beginnings-stephen-hawking-says-yes.html

And who is going to argue with him?

26. **Nigel Cook**
October 29, 2009

"Mainstream medieval science was up to 13 epicycles when Kepler tried ellipses. However, epicycle theory, with any number of cycles, only works if planets move in closed orbits, which is only approximately true."

Arthur Koestler's 1959 "The Sleepwalkers: A History of Man's Changing Vision of the Universe" actually counted the epicycles up and found 40 in Ptolemy's Earth-centred-system in his "Almagest" of 150 A.D., versus 80 in Copernicus's solar system of 1500 A.D. (which used circular orbits with epicycles instead of ellipses like Kepler). This was contrary to the prevailing history of science, which insisted that Copernicus was accepted on the basis of Occam's Razon due to having the fewer epicycles than Ptolemy. Actually sometimes more complex theories are closer to nature and there were different reasons why Copernicus was preferred. (Viz: Mercury and Venus are always observed from Earth to be on a bearing within 90 degrees of the position of the sun, a fact which is explained very simply in the solar system model by Mercury and Venus having orbits closer in to the sun than the Earth's orbit. Additionally, the apparent size of the Moon seen from Earth in Ptolemy's model should vary by a factor of two monthly due to its epicycles, when in fact it doesn't appear to vary in size. Copernicus's more complex model was preferred because it didn't modelled nature right, not because it was the simplest.)

Master of the Universe

October 24, 2009
Categories: Uncategorized

A couple days ago I got an odd phone call, from a reporter at the Guardian, asking me to comment on the appointment of Michael Green as Lucasian Professor at Cambridge. I told the reporter that I wasn't a really appropriate person to be asking; for one thing I've never met him personally. I did say that from what I knew of his scientific career, he was a quite good choice. He and John Schwarz made great progress in understanding string theory, working on it at a time that this was a very unpopular thing to do. In my view much of the problem with particle theory the past 25 years has to do with the lack of sufficient talented people willing and able to work on the kind of unpopular research that Green and Schwarz took up.

Several people have now pointed out to me the new story in the Guardian, [Michael Green: Master of the Universe], which makes clear the reason for that phone call (although none of my comments made it into the story). There's the usual hype about string theory: "the subject's thriving", and the latest news is that it may lead to better understanding of high temperature superconductors and thus help solve the world's energy problems. In a sidebar, the claim is made that:

> The Large Hadron Collider, at Cern, could provide evidence for the theory by analysing the collisions of fundamental particles at high energies.

although Green admits:

> …that really is wildly optimistic, and I suspect that's not going to happen.

Green deals with criticism of string theory with a laugh and ad hominem attacks on Lee Smolin and me as "two particular people who don't have any particular reason to be knowledgeable about the subject." As for the idea that it might be a good idea for people to look for alternatives to string theory (much the way he and Schwarz worked in the early 80s), his comment is "But there *is* nothing else."

Green seems to be not completely sure I have a Ph.D. For those interested in the question of my qualifications, there's an old blog entry [here]. It should perhaps be updated to note that, while I'm still responsible for the Math department computer system, I no longer have the odd title of "Director of Instruction", but was moved to a non-tenured faculty position as "Lecturer". Recently I was promoted to the position of "Senior Lecturer", still non-tenured, but with a long-term contract.

I wish Green the best with his promotion.

Comments

1. **Marcus**
   October 24, 2009

   Congratulations on your appointment to the Senior Lecturer position!

2. **Marcus**
   October 24, 2009

   A propos the interview in the Guardian, someone mentioned that MG is 63 and the Lucasian retirement age is 67. So it seems we can look forward to four year of ex cathedra pronouncements such as "The subject's thriving," and "There is nothing else." 🙂

3. **Peter Morgan**
   October 24, 2009

   Thanks for the link to the old description of your academic life. Congratulations also on your new appointment.

4. **Sebastian Thaler**
   October 24, 2009

   Apparently class is not a job requirement for the Lucasian Professorship.

5. **Jas**
   October 24, 2009

   I am following your blog for a while now. I am an Undergrad. student.
   It appears that particle physics is dying. But tell me about yours opinion on loop gravity. Thats I guess producing few good results.

6. **C.K.**
   October 24, 2009

   @Jas

   John Baez in week280 of his This Week Finds summes most recent developments in LQG and related theories. The summary is that everything was plagued with problems of different, but equally profound kind like the Strings, but was recently successfully started from scratch. That also means theory is still undeveloped stub.

   I thus think Green may be right saying there is nothing else, in a sense that alternatives are so undeveloped as to not even mimic working. LQG, despite 20 years of development is far from including Standard Model, and Causal Dynamical Triangulations are far from including anything at all, presently being just a very profound and nice, but only curiosity.

7. **James**
   October 24, 2009

   Peter, the interview shaws that Green has no dignity. Green criticizes you because you say (more or less) that
   (1) "string theory is wrong because it has no relation to experiment" Tthis is deeply dishonest.
   (2) "you have no knowledge of the subject"; this comment shows what string theory really is: the belief of a sect.

   Peter, I counted a handful of Nobel Prize winners in particle physics who say the same what you say. And I count only one (Gell-Mann) who promotes it a little. But that is the same Gell-Mann who said that "Bohr brainwashed a whole generation of physicists that quantum theory has no problems." A handful of serious people against one crazy old man. Nobelists thus have clearly spoken out against string theory. Nobody dares to attack them. Instead, string theorists attack you. You will see: the LHC will put string theory aside as a wrong theory. Green's appointment is the swan song of string theory; please be strong and be patient.

8. **C.K.**
October 24, 2009

What Green says is not much arrogant compared to what you people here put on his lips, and downright nothing next to how you attack him for it.

@James

Peter on numerous occasions described why LHC won't settle issue of validity of String Theory. That means it will not be decisively disproved. And as long as nothing else will show up, people will do strings.

Appointment of Green underlines this. On the one hand it is deserved reward for the man's work and there was surely noone else in Cambridge more adequate to take the post. On the other, it is also convenient temporary solution for Cambridge.

9. **theoreticalminimum**
October 24, 2009

I think one important thing to take from this interview is the way Green and Schwarz were spending their time, "more leisurely", thinking about the problems they were faced with. It seems like they enjoyed a long quiet time span of relatively relaxed collaboration without the pressure of publishing, and could think hard about certain problems. I think this leisurely and periodically thought-intensive method of working is what is sorely lacking today in high-energy theoretical physics. Big problems have been solved in mathematics by people who spent about a decade working on these problems, e.g. Wiles and Perelman. There are no such examples that I know of in theoretical physics. Of course, theoretical physics and mathematics are very different fields of intellectual endeavour (even though they are intricately related in many ways, more so now that ever before), but I think this raises an important question (one, of course, which has been raised many times by many people, e.g. you Peter, L. Smolin, A. Connes, to name the few I have heard voicing their concern about this state of affairs), which is the following: are we spending enough time thinking about specific problems? Many problems in physics are hard to define as in mathematics, but I know there are many others which are really well-defined and precisely framed in mathematical physics. Are there people spending enough time looking at these problems? Poincaré and Hadamard have written about the psychology of discovery in mathematics, and I wonder what they would have said if they were alive to witness the pace of things in research in theoretical physics, and more specifically string theory, today.

10. **Roger**
October 24, 2009

Peter, have any string theorists like Green addressed what you actually say in your book?

11. **Anon**
October 24, 2009

Another thing to take away from this interview is to be very careful when speaking with a reporter. I imagine Green regrets this comment. It's the sort of snarky comment I can see making to a colleague as a semi-joke but would be embarassed to see repeated in public. (Perhaps one shouldn't make such comments in the first place.)

12. **Peter Woit**
October 24, 2009

Roger,

There's a review written by a post-doc (Aaron Bergman), and a few comments in a review by Joe Polchinski, but that's about all I can think of in terms of serious responses from string theorists.

13. **Peter Woit**
October 24, 2009

Anon,

Perhaps you're right. I suspect that if I'd had something unpleasant to say about Green to the reporter, then I might have been quoted…

14. **Thomas Larsson**
October 25, 2009

James:

Of the living theoretical particle Nobels, David Gross is the only one who is a leading string theorist, and a string proselyzer. Perhaps you can say that he is in string theory what H A Lorentz was in ether theory.

Gell-Mann, Weinberg, and perhaps Wilczek are (or have been) pro-string, but do not actively pursue the subject. This also goes for David Politzer, but he does not really count.

Glashow and Veltmann appear hostile to string theory.

What 't Hooft and Ken Wilson think is anybody's guess.

15. **Thomas Larsson**
October 25, 2009

And then I forgot that last years Nobel prize went to particle theory. Nambu was working early string theory in the 1960s, I think.

16. **ettore**
October 25, 2009

In regards to t'Hooft, this is what he said ten years ago, when he received the Nobel (you can check it in the Nobel site, in his autobiography)

"In 1984, the superstring revolution took place. Many of my colleagucs were enchanted by the coherence of the mathematical structures they saw in this theory. Would this not be exactly what we are looking for, a new paradigm that naturally generates the gravitational force and an apparent complete unification of all interactions?

But to me, superstring theories presented as many new problems as they may solve; I still cannot quite fathom the fundamental logical coherence of these ideas. The short distance structure is as mysterious as it was before and the predictive power of these theories was disappointing, to put it mildly."

17. **Peter Woit**
October 25, 2009

Thomas,

I don't think it's accurate to say that Wilczek is or has been "pro-string". He has been careful to avoid entering into this controversy. I've never seen anythng anywhere about Politzer's views on the topic.

18. **observer**
October 25, 2009

Take ll this with a grain of salt. First, professor Green Is at 3 or four years from retirement (due to age). So most probably they are giving him the Lucasian Chair as a farewell present.

19. **capitalistimperialistpig**
October 25, 2009

I'm not sure how leisurely Schwarz and Green were able to be. This was a time when early string theorists like Ramond were being dumped by top departments. Schwarz hung on because Gell-Mann insisted, or so G-M claims.

20. **Thomas Larsson**
October 26, 2009

One of the benefits of living in Stockholm is that one can attend the Nobel lectures. My impression of Politzer's views are based on what he said at that occasion, although I don't remember any details.

21. **Anonymous Undergrad**
October 26, 2009

Hi Peter,

[This is somewhat off-topic, but I'd like to inquire about your perspective/s on studying gauge theory as an uninitiated undergrad.

…

– Any words of advice?
– Any specific title/s that come most readily to mind, (e.g. 'The Geometry of Physics', 'Topology, Geometry, and Gauge Fields: Foundations', etc.)?]

Regards,
Anonymous Undergrad

22. **Chris Oakley**
October 26, 2009

Appointing a 63-year-old to a professorship that has traditionally been given to relatively young people indicates to me that there was no obvious (relatively) young person to give the job to. Green would certainly have to take his share of the blame for that, being partly responsible, at least indirectly, for making it impossible for young scientists to work on anything other than the blind alley of String Theory.

23. **Tim vB**
October 26, 2009

@Anonymous Undergrad:

If you would allow me to offer my opinion 🙂
Have a look at John Beaz' blog:
http://math.ucr.edu/home/baez/FUN.html#general_physics
(note the item "books" under Miscellaneaous Fun Stuff).
As an undergraduate interested in theoretical physics you should definitely read Naber: „ Topology, Geometry, and Gauge Fields" (both volumes, Foundations and Interactions). But do that after you had an introduction to quantum mechanics, quantum field theory and differential geometry.

24. **Kris Krogh**
October 26, 2009

Hi Thomas,

The Nobel lectures are on-line. Politzer's is here:

http://nobelprize.org/nobel_prizes/physics/laureates/2004/politzer-lecture.html

On string theory there is only this:

The realm of the conjectured "unification" of the forces of the Standard model, the realm of their possible unification with gravity, and the basic physics of String Theory, the most widely pursued approach to a physics more fundamental than the Standard Model, are all more than a dozen orders of magnitude further away.

25. **Anonymous Grad Student**
October 27, 2009

My impression is that Politzer doesn't much care either way. Last time I talked to him, he was much more interested in Biology anyway. He will however be taking part in the following event in Pasadena tomorrow evening, if anyone in SoCal is interested in hearing his opinions.

`At 7 p.m. on October 28, science writer K.C. Cole will engage in a discussion of the creation of ideas and the sources of inspiration for the next generation of physicists with Nobel Laureate David Politzer, Caltech's Tolman Professor of Theoretical Physics. The event will take place in Hameetman Auditorium, in Caltech's new Cahill Center for Astronomy and Astrophysics.'

26. **Chris W.**
October 27, 2009

Here is the official announcement on the Caltech events site.

27. **Tim vB**
October 29, 2009

Isn't the following statement of Green from the interview worthy to be cited in the main blog entry?
cit.:
Furthermore, string theory, Green contends, "isn't simply something that will, once tested, be either verified or disproved. It's become much more than that".

28. **Allan**
November 8, 2009

Let me run a few words from a post above through the time machine:

"The Lorentz transform so far from including anything at all, presently being very nice, but only curiosity."

I don't always agree with Peter, in general, about the lockstep interplay of experiment and theory. It's a wonderful conjunction when it happens, but just as often, physics enters a wilderness period where the two fail to cross-pollinate until someone brilliant comes along. I suspect the present challenge is not how to best return to the road, but instead how to best thrive in the wilderness until a new road forward emerges.

On one hand, if the available formalisms restrict the potential solution space, you're crazy not to investigate this, regardless of a short term disconnect with experimental corroboration. (Careerist calculations aside, the short term could be fifty years when you're recently passed a road sign lettered "Nirvana 10^70".)

For string theory, initially the formalism had the appearance of a constrained solution: "hey, it only works in 10 dimensions!" Twenty years later we've added the footnote: "but with perhaps in excess of 10^500 solutions". This field has been tilled vigorously for a generation, maybe it's time to let this particular field lie fallow for a while. Worth doing, but not at warp speed, with the foot of the public treasury pumping the accelerator.

It won't surprise me if an insight derived from the formalisms provides an essential clue to the path forward; nor will it surprise me that the path forward turns out not to be string theory itself. The question is not whether string theory is the final answer, but it's prognosis as a useful (or even essential) stepping stone to whatever comes next. Depends on whether this wilderness can be circumvented, or not. I think not.

A problem for physics during its wilderness periods is that its innate immunity to institutionalism breaks down. Physics hasn't developed a strong social immunity to institutionalism, because when physics is going well, it doesn't need one: experiment wields the scythe.

Where string theorists are presently in denial is that after twenty years with little to show as corroboration, the program has now plunged deep in the heart of institutionalism. Physics–falsely in my opinion–prides itself on immunity from this, based on a few rapturous decades here and there. The occasional rapturous decade has earned physics a certain kind of immunity from public scrutiny despite large inputs of public funds. Like the bankers, there's an ingrained attitude among the endowed: let the good times roll.

Most egregious, in my view, are the monoculture apologists: string theory is all we've got, so we have no choice but to continue. When this kind of fungus sprouts up, I can understand an impassioned banging of the shoe on the podium demanding a return to experimental confirmation. Is that possible? Can we roll back time to the glorious sixties? Our drugs have become increasingly expensive. If CERN wasn't a great adventure in determining the largest machine we can almost build, would it have been funded on physics agenda alone?

Concerning the article, I was miffed by Aida Edemariam leaving out an essential paragraph after quoting the remark "The subject's thriving." Based on what criteria, Mr Green? That the jungle is now lusher, thicker, and more impenetrable than ever? That funds are flowing to the most expensive physics project in human history? That the profession has become so abstruse that nothing short of a brilliant, full time practitioner within the inner sanctum is qualified to venture an opinion on whether results obtained over the past two decades justify the cost and talent expended?

Linguistics went through the same expansionary bafflegab: many of the explanatory frameworks Chomsky put forward turned out to be Turing complete, which translates to unlimited horizons for graduate students to elaborate the unconvincing.

My question is this: what if it turns out that the only way forward is for physics to bunker down for a while and function as a social institution, without the luxury and glory of its former immune system? It seems wrong to have a man endowed with a prestigious chair whose insight into institutionalism doesn't seem to run any deeper than "we're successfully spending a lot of money and all our critics are twits who barely completed boot camp at the world's most famous universities".

There's nothing wrong with training smart people to confront the ardours of life at the edge of the jungle (harrumph: that word doesn't mean what I thought it meant) . Like the NASA program, physics has successfully spun off a fair amount of its futuristic gear to the benefit of other disciplines. Fair enough, whether or not physics beats its own jungle back anytime soon.

The unavoidable question becomes: if experiment ceases to wield the scythe, what social criteria must take its place? Who decides? It seems from Mr Green's perspective that the New York Yankees are the only game in town, having no credible competition. Meanwhile, the rapture of the molecular biologists is performing a few blocks down the street and there's actually a pennant at stake that means something.

Latest from the LHC

October 25, 2009
Categories: Experimental HEP News

This weekend successful tests of injection of a beam from the SPS into the LHC were performed. The beam only traveled through a few of the sectors before being dumped, since all sectors of the machine are not yet ready for beam commissioning.

A week or so ago the decision was made to start beam commissioning with the magnets only fully commissioned to 2kA. This means that the machine will be limited to operation at 1.1 TeV/beam this year. The current schedule has commissioning to 2kA finishing November 16, attempts to circulate 450 GeV beams starting November 23. On December 7, the beam energy would start to ramp up to 1.1 TeV. 1.1 TeV/beam collisions would start Dec. 14, with shutdown for Christmas/New Year's starting Dec. 16. This means that 2009 will not see physics collisions, but will perhaps see collisions at energies marginally higher than that of the Tevatron.

By the end of the year, 2 sectors will be commissioned to 6kA, the magnet current needed to run the machine for physics at 3.5 TeV/beam. The rest of the sectors will be commissioned to 6kA and the energy ramped up to 3.5 TeV/beam starting after the shutdown ends in January.

Update: Some more from the latest schedule. January 7 will be the start of recommissioning after the shutdown, and current plan is to have the machine ready for physics collisions at 3.5 TeV/beam by February 8.

Update: The date to begin beam commissioning again by circulating a beam in the LHC is now set for Friday November 20.

Comments

1. **Martin**
   October 26, 2009

   Hi Peter,

   do you know what the reason is for this further delay?

   Cheers,
   Martin

   PS Where do you think will the Higgs be found first: Tevatron or LHC?

2. **Peter Woit**
   October 26, 2009

   Martin,

   I believe the delay in getting all sectors commissioned to 6kA has to do with the new Quench Protection System.

   The Tevatron may rule out a larger range of Higgs masses, but it seems unlikely that, even if the Higgs is there, they'll have convincing evidence of its existence. That should require the LHC, and at least a couple more years.

3. **Coin**
   October 27, 2009

   This means that 2009 will not see physics collisions, but will perhaps see collisions at energies marginally higher than that of the Tevatron.

   Could the LHC uncover new things despite running at the same energy as the Tevatron simply due to higher luminosity?

4. **Peter Woit**
   October 27, 2009

   Coin,

   The LHC will only briefly be running at Tevatron energies, during the commissioning process on the way to 3.5 TeV/beam. During this period it will be running at extremely low luminosity, not in any way competitive with the Tevatron.

   By the way, I see that the NYT seems to have gotten the news about the plan to run at only 1.1 Tev/beam this year from somewhere…

   http://topics.blogs.nytimes.com/2009/10/27/the-cosmic-countdown-in-geneva-goes-on/

News from HEPAP

October 27, 2009
Categories: Experimental HEP News

Last week there was a meeting of HEPAP held in Washington, presentations are available here.

HEP has done very well recently in recent US federal government budgets, due to the stimulus and large deficit spending going on to fight the recession. The FY2010 DOE budget has been passed by Congress, and it includes $810 million for HEP (up 2% from $797 million in FY2009), and there is also $232 million in stimulus package money currently being spent on HEP. The FY2010 NSF budget has not yet made it through Congress, but the Administration request for NSF physics research is up by 9% from FY2009.

DOE is planning to keep running the Tevatron now at least through FY 2011, since it is likely to be competitive with the LHC in the Higgs search business at least that long. The current Fermilab long-term planned run schedule is here.

DOE will keep supporting ILC research through FY2012, but the plan to make a decision about building it at that time now seems to be off the table. The LHC will have just begun producing results, and the current estimates of the ILC cost are so high that making the case for it will be very difficult. A story in Science quotes William Brinkman, the head of DOE's Office of Science as saying:

> With all the contingencies, you're talking about $20 billion. In my opinion, that price pushes it way out into the future, and onto the backburner.

Funding for new high-energy accelerators is likely to mainly be devoted to participating in any upgrade of the LHC at CERN, and the Project X/muon collider proposals at Fermilab. There will be workshops at Fermilab next month to discuss Project X and the muon collider. Brinkman in his HEPAP talk notes that the HEP community will have to come up with a compelling scientific case for these projects, which will largely revolve around an expanded neutrino program.

There was also discussion of a report from PASAG (the Particle Astrophysics Assessment Group). For discussion of the issues surrounding proposed experiments relevant to particle astrophysics and cosmology, see stories from Eric Hand at Nature News here and here.

Comments

1. **Yatima**
   October 28, 2009

   Unfortunately, the long-term signs are not good. It is rather likely that recessions are not fought with (Keynesian) deficit spending but with (Austrian) hands-off management and the return to a "save&invest" mentality. The next ten or so will most probably be a period of stagflation. Once this becomes clear I hope DOE will not quickly revise its spending plans downwards. As for liberating 20 billion+ (an initial estimate?) for ILC, well, the SSC was killed at 12 billion in 1993 dollars (that's arund 18 billion USD in 2009), when the overall economic situation was actually far, far better. So, no.

2. **srp**
   November 3, 2009

   And still no money for early development research on advanced accelerator concepts. You guys need to learn about decision trees…

Short Mathematical Items

October 29, 2009
Categories: Uncategorized

- Riemann submitted his paper on the Riemann Hypothesis October 19, 1859, and it was read by Kummer at the meeting of the Berlin academy on November 3. AIM is organizing a celebration of the 150th birthday of the Riemann Hypothesis, with a "Riemann Hypothesis Day" on November 18th. Talks will be given on that day at many institutions around the world, a list is here.
- The Royal Society in Britain has announced the appointment of six "Royal Society 2010 Anniversary Research Professors". Two of them are mathematicians: Timothy Gowers, of Cambridge, and Andrew Wiles, who will be leaving Princeton to take up the position at Oxford. Wiles has this comment about his current research:

> Over the last several years my work has focused primarily on the Langlands Program a web of very influential conjectures linking number theory, algebraic geometry and the theory of automorphic forms. I am trying to develop arithmetic techniques that will, I hope, help to resolve some of the fundamental questions in this field. I am delighted to be appointed a Royal Society Research Professor in their anniversary year and I look forward to the opportunities this will give me to further my research.

- I spent a couple days earlier this week up in New Haven, attending a conference celebrating Gregg Zuckerman's 60th birthday. Zuckerman's specialty is representation theory, and he's well-known in that subject for several ideas that have been important in the modern understanding of infinite dimensional representations of semi-simple Lie groups. He also has done quite a bit of work in mathematical physics, work which includes a classic paper (Proc. Natl. Acad. Sci. U.S.A. 83 (1986), pp. 8442–8446) with his Yale collaborators Howard Garland and Igor Frenkel explaining some aspects of the BRST quantization of the string in terms of semi-infinite cohomology. As far as I know, he was the first person to study (in a 1986 paper "Action principles and global geometry") the field theory with Chern-Simons action that Witten was to make famous two years later when he worked out its significance as a TQFT giving interesting 3-manifold and knot invariants.
- An hour or so ago I went out for a walk, stopped at the bookstore, and noticed that there's a new book out about Grigori Perelman, entitled Perfect Rigor. It looks worth reading, perhaps they'll be a longer blog post about it sometime soon…

Comments

1. **Marcus**
   October 29, 2009

   I see that slides can be downloaded for 3 out of the 18 talks given at the Group Representations conference you attended at Yale.
   http://www.liegroups.org/zuckerman/slides.html
   In particular Garrett Lisi's talk is one of those for which the slides are available.

2. **John Armstrong**
   October 29, 2009

   Unfortunately I couldn't be there for my advisor's conference. Unemployment is a harsh and forbidding landscape.

3. **Marcus**
   October 29, 2009

   J.A. I've read your blog and some of the comments re the current math job market. Sounds very tough–as if they are almost forcing pure math PhD's to go back for applied courses of some type. Sad you were unable to attend your advisor's 60th-birthday conference. Hope things improve soon.

4. **Sakura-chan**
   October 29, 2009

   I am going to order that Perelman book right away!

5. **Tim vB**
   October 30, 2009

   The anniversary of the Riemannian hypothesis should be celebrated with a lecture by Allain Connes in Göttingen, but it semms neither Connes nor Göttingen is involved in this? Too bad.
   Just re-read Riemanns original paper (in Edwards, "H. M. Riemann's Zeta Function"). As you all probably know already, what is called "Riemannian hypothesis" is only a remark in the paper, which is about the "number of primes less than a given magnitude". At the end of the paper Riemann compares his formula with the known number of primes smaller than 3 million! referencing the work of Gauss and Goldschmidt. Wow, they computed all primes lower than 3 million without a computer! (And he mentions that he himself tried to prove his hypothesis: "One would of course like to have a rigorous proof of this, but I have put aside the search for such a proof after some fleeting vain attemps, because it is not necessary for the immediate objective of my investigation". What would he have said if someone had told him that this would become one of the most popular open problems of the 21st century?).

Perfect Rigor

October 30, 2009
Categories: Book Reviews

I just finished reading author Masha Gessen's new book about Grigori Perelman, Perfect Rigor: A Genius and the Mathematical Breakthrough of the Century. It's a short but very well done account of the life of Grigori Perelman, how he came to prove the Poincare Conjecture, and what has transpired since.

The book is really not about mathematics, but about mathematicians and their culture, especially that of Russian mathematicians. Only one chapter deals with the mathematical content of the Poincare Conjecture, with the bulk of the book about Perelman and his career. Perelman's talent's were recognized early, and were nurtured in Leningrad by a system designed to train students for mathematical competitions. He won a gold medal at the International Mathematical Olympiad in 1982. The institutionalized anti-Semitism of the Soviet mathematics establishment of this period is described in detail in the book, together with the intense efforts made by Perelman's supporters (including Alexandrov) to overcome this. He did his graduate work at the most prestigious institution in Leningrad, and then went on to a research position there at the Steklov Institute.

Gessen never managed to interview Perelman himself, but did talk to many if not most of the mathematicians he interacted with. He was brought to Courant by the intervention of Gromov, and for a few years worked there, at Stony Brook and at Berkeley. By the end of this time, he had started to develop a significant reputation in the math community, but he chose to return to Steklov and pretty much dropped out of sight, communicating with very few people for several years. It was during this period that he developed his proof, finally posting what could be described as a detailed outline in a series of three papers submitted to the arXiv.

The story of what happened then is rather remarkable, but it's a story I'm pretty familiar with since I got to watch much of it from up close (Perelman's preprints and the question of whether he really had a proof were discussed intensively here at Columbia, where Richard Hamilton and John Morgan are among my colleagues, and quite a few other people work in this area). Gessen does a good job of telling this story, adding some details I was unaware of.

Perelman turned down the Fields medal awarded him for this work, and sadly, he seems in recent years to have cut himself off from even his closest friends in the math community. Indications are that he is no longer actively working on research mathematics. The book contains speculation from several mathematicians who know Perelman about his thought processes and the reasons for his behavior, but they remain somewhat of a mystery. Some amount of paranoia seems to be at work, together with an intense distaste for any sort of politics, even the most innocuous workings of the mathematical community and its institutions.

The last chapter of the book has some news I hadn't heard. Last year, Jim Carlson, who runs the Clay Mathematics Institute and is responsible for the process that will determine the award of the million-dollar Millennium prize for the proof of Poincare, traveled to St. Petersburg. He talked to Perelman on the phone, but Perelman refused to meet with him. According to the book, Clay was planning on convening a committee to decide on the prize this past May, with a report planned for August. Presumably this all has already happened by now, and perhaps Carlson has already made another trip to St. Petersburg in a last attempt to see if Perelman can be convinced to accept the prize. Perhaps we will be finding out the results soon…

Update: Today's Wall Street Journal has an article by Gessen about Russian mathematics that summarizes part of her book.

Comments

1. **Giotis**
   October 30, 2009

   The poor, lonely, eccentric genius who proves the Poincare conjecture, denies the Fields medal, the money, the honours and the whole world and disappears in oblivion.

   This is the stuff that legends and myths are made of.

2. **tst**
   October 31, 2009

   "This is the stuff that legends and myths are made of."

   And you think Perelman is unaware of that?

   The math he did is first rate, of course. But the drama adds nothing.

3. **milkshake**
   October 31, 2009

   tst: Its not a pose, he withdraws from things which he does not want to deal with. The unpleasant part of the drama was not his making anyway.

4. **Tom O'Bulls**
   October 31, 2009

   What finally happened re Yau and his legal case?

5. **ds**
   October 31, 2009

   Giotis: why do you think he is lonely? Some people can be quite happy on their own and even prefer it, and even choose it as a lifestyle.

   I find it infinitely irritating how the majority of people equate being alone with loneliness. Some people just aren't so shallow that they need constant stimulation and distraction from themselves in the form of others and things. They may restrict their attention to just a few people.

   Perhaps Perelman sees a corrupt institution before him and chooses not to participate. Maybe this should make us stop and think;; instead we just marginalize him and say he is a "poor, lonely genuis," which is saying don't take him seriously, he is brilliant in math, but that same brilliance makes him completely incompetent in social matters.

6. **ds**
   October 31, 2009

   tst:

   "And you think Perelman is unaware of that? "

Do you think he is really so petty as to be motivated by that? I don't think so. You see, this is another attempt at marginalization. Society, the norm, sees an individual who refuses to play by it's rules, and it tries to dismiss him, explain him away as mal-adjusted or petty. That way we don't have to think, we can just carry on as usual; in our petty competitive way, chasing prizes, positions at prestigious universities, and so on.

7. **ds**
   October 31, 2009

   i should just add at the end of the last sentence of my previous message: "or whatever particular aspect of our behavior Perelman may be repulsed by.."

   Sorry for the SPAM!

8. **Giotis**
   October 31, 2009

   ds, I was referring to the way the general public might perceive his life and not to his actual reality.
   In any case I don't give a negative tone to the word 'lonely'. The idea of the lonely hero is omnipresent in literature and in popular culture, captivating human minds throughout the centuries.

   But yes, you are right; a loner is not necessarily lonely although an old saying alleges that if someone lives all alone and doesn't feel loneliness, is either mad or god.

9. **Paul Titze**
   November 1, 2009

   Why not challenge Grigori with another mathematical challenge that's suited to his skills with an even bigger prize? He might be so enticed by it that he'll come out of the woodworks 🙂

   Paul.

10. **D R Lunsford**
    November 1, 2009

    Does any single person understand all the details of the P.C. proof?

    -drl

11. **Sakura-chan**
    November 1, 2009

    The claymath website continues to refer to it as a conjecture. I wonder when they'll dub it as a theorem.

12. **milkshake**
    November 1, 2009

    prizes are apparently not what he is after; maybe they should try to convince him to move to IAS

13. **Peter Woit**
    November 1, 2009

    milkshake,

    Perelman appears to have no interest in a position at the IAS (or anywhere else for that matter).

    Paul,

    He seems rather offended by the whole concept of the prize he already has just about won, I don't think another one would inspire him in any way.

    Sakura-chan,

    Perhaps the Clay web-site will be updated soon…

    drl,

    The mathematicians who wrote up detailed versions of the Perelman argument (Morgan-Tian, Cao-Zhu, Kleiner-Lott) presumably understand the full details, and so perhaps do other people who have read these documents and worked in this area. The Morgan-Tian book was especially carefully refereed, by several of the best mathematicians in the world (ever wonder why Terry Tao has given talks on the proof and written about it in his blog?). I don't know if these referees divided up the sections of the proof, or each of them went through the full thing.

    In any case, this proof now has been about as carefully examined as any.

14. **Christine Dantas**
    November 1, 2009

    If I may make a guess, I think that Perelman would accept a position as a teacher for the very young, smart children, still uncorrupted from "politics" that invades everything and everywhere from the adult world. Although such a position could be seen by many people as an absurd downgrade (teaching kids is usually regarded as a minor profession by society), I think that he would probably enjoy such a fresh air. This is something that I think he needs. And such a "reboot" would perhaps motivate him to return working in mathematical problems again and have his results published independently in the arxiv.

    He has shown clearly not to be motivated by prizes or fabulous positions at all.

15. **Gigel**
    November 1, 2009

    Why is everyone trying to "fix" him? So he can become yet another useful tool for humanity, since if you don't work for society, something must be clearly wrong with you? Maybe he plays world of warcraft all day and that's what he enjoys. Or maybe he's simply crazy. Why don't we just forcefully hospitalize him and make him better, stuff him with meds until he agrees to hold a position at IAS and maybe prove 2-3 more theorems, right? That would make US feel better about ourselves, seing how well our society is working like an ant farm. Well what if he doesn't give a crap about teaching or working in math anymore?

Nobody owes anything to society, unless they personally feel they do.

And btw, he never refused the million bucks. In fact, it was never offered to him. As far as he is concerned, he proved the conjecture, his work is done. If the institute is unable to solve its own policies, that's not his problem. He just said he won't discuss it or anything, because it's done. Finito. Nothing to talk about. Whether it's published where it has to be or not. Now give him the money or back out of the deal because of minor issues.

He'll never beg or ask for the money. That's all.

16. **vincent**
November 1, 2009

I agree with "tst" that the drama aspect should have been irrelevant.

Three other people got Fields medals along with him. He is getting the most publicity out of them all! Self-interest might not be the full story, but its silly to think that he has none of it.

17. **Tom O'Bulls**
November 1, 2009

"Three other people got Fields medals along with him. He is getting the most publicity out of them all! "

That's because their work, outstanding as it is by normal standards, is utterly negligible compared to Perelman's. That was really a bad year to win a Fields.

18. **Andy**
November 2, 2009

Tom O'Bulls — While I agree that Perelman's proof of the Poincare conjecture was amazing, no one who knows anything about math would claim that the work of Tao, Okounkov, or Werner is "utter negligible" compared to it.

19. **milkshake**
November 2, 2009

Christine, you are right – a math Olympiad club, no pressure.

20. **Serifo**
November 2, 2009

I think Perelman and Grothendieck are examples of pure natural thinkers , any young physicist or mathematician should reflect about their stories .They are not motivated by international awards , fame , academic prestige or New York times magazine !

21. **Aristarchus**
November 2, 2009

"But yes, you are right; a loner is not necessarily lonely although an old saying alleges that if someone lives all alone and doesn't feel loneliness, is either mad or god."

Why not both? Even the Bible says that God made humans for companionship and worship.

Boy, he must have been lonely.

22. **abc**
November 2, 2009

I think that people should leave him alone. I guess he just loathes being a public figure. One cannot get the million dollar prize, Field medal etc. and than just disappear. Once you start to play the game, you are already deeply in it, so he refused everything from the very beginning. I would even go on and say, since it has become increasingly difficult for him to avoid publicity, he had to become more reclusive than he probably ever wanted

23. **observer**
November 2, 2009

Cantor, Godel, Sidis, Erdos,Grothendieck,Nash,Von Neumann, Perelman to name just a few of the great mathematicians and all of them had some very peculiar view of the world and some eccentricities to say the least, it seems to came with the profession itself.

24. **Deane Yang**
November 3, 2009

"They are not motivated by international awards , fame , academic prestige or New York times magazine !"

And who is? Anyone who wants any of these things is not going to become a pure mathematician.

I don't really understand either the attacks on Perelman or the effort to put him on a pedestal. There is no question that he's a great mathematician and that it is a loss for the rest of us that he has decided to turn his back on us. But he is not the only one who has done this, and his decision should be treated with respect. He's just a human being trying to lead his life as best as he can.

25. **RMA**
November 3, 2009

Does Dr. Perelman even like people talking about his personal life (as opposed to his mathematical ideas) on public forums?

26. **milkshake**
November 3, 2009

… the paparazzis at his mother's house and random freaks (snapping his pictures on subway with a cell phone) would be a more acute nuisance than some little Internet gossip

27. **Aristarchus**
    November 3, 2009

    I now consider him to be one of my few personal heroes. He did the math for the math and told society to go jump in a lake.

28. **Russ Van Rooy**
    November 4, 2009

    Sounds like shades of Alexander Grothendieck. A great biography of Grothendieck in English is long over due by the way.

Latest from the LHC

November 3, 2009
Categories: Experimental HEP News

The latest official news from CERN about the LHC schedule that I've seen is this from DG Rolf Heuer, who doesn't give specific dates other than "second half of November" for circulating beams, collisions at injection energy soon thereafter, and, if all goes well, "high-energy collisions" before Christmas. He doesn't specify what the value of "high-energy" is.

Physics Today has this story, which has a lot more detail than available officially, including a quote described as "a statement on the CERN web-site":

> This means that 2009 will not see physics collisions, but will perhaps see collisions at energies marginally higher than that of the Tevatron…

which was picked up by the New York Times here, and reported as:

> The lab now says the first collisions, before Christmas, will be even lower, due to delays in finishing a system to protect the powerful superconducting magnets from explosive failures. The initial collisions will be at 1.1 trillion electron volts per beam, just barely above the energy of the Tevatron collider now running at CERN's rival, the Fermi National Accelerator Laboratory outside Chicago.

I can't find that quote on any CERN site, but it and the other details of the story do seem awfully familiar.

Unofficially, what's known about the schedule at the moment is:

Next weekend (Nov. 7-8): Second injection test. If sector 67 is ready, beam will travel through this sector (and possibly even through sector 56) as well as the two (sectors 23 and 78) tested during the first injection test.

November 20th: First attempt to circulate beams at the injection energy of 450 GeV.

Early December: Collisions at 450 GeV.

Mid-December: Ramp to 1.1 TeV, collisions at 1.1 TeV/beam.

December 16th: Stop of beam commissioning for end-of-year break.

January 4: Restart after end-of-year break. About three weeks for hardware commissioning to 6kA, 3.5 TeV/beam.

Late January: Beam commissioning at 3.5 TeV/beam.

Early February: Collisions at 3.5 TeV/beam. First physics run soon thereafter.

Update: Not sure what to make of this. At first I found this hard to believe, but there's another story here.

Update: I guess this actually happened: here's something from CERN.

Update: Looks like they will be able to get a beam through 4 of the LHC's 8 sectors this weekend.

Commenter Yatima points to this at the Register. If you believe the Register (not necessarily a good idea…), CERN's Sergio Bertolucci is promoting the idea that the LHC will open a portal to other dimensions, so:

> Summarising, then, it appears that we might be in for some kind of invasion by spontaneously swelling and shrinking spherical or wheel-shaped creatures – something on the order of the huge rumbling stone ball from Indiana Jones – able to move in and out of our plane at will. Soon the cities of humanity will lie in smoking ruins, shattered by the Attack of the Teleporting Juggernaut-tyrants from the Nth Dimension.

The writer asks LHC Machine Coordinator Mike Lamont what he thinks of all this. He suggests reading Lisa Randall's book.

Comments

1. **Stefan**
   November 3, 2009

   Have there been any comments about the electricity bill expected for this winter, and how this could affect the schedule?

   It seems the main supply from France could be especially expensive this time, as 25% of French power plants currently are offline and France will have to import electricity starting mid-November until at least end of January.

2. **Peter Woit**
   November 3, 2009

   Stefan,

   I haven't seen anything recently about that. Earlier this year, when they decided to not shutdown for the whole winter, they considered this cost issue in detail.

   But, since things are taking longer than expected, and a physics run won't start until at least February, the additional energy cost should be even less than they were projecting. For much of the next three months, the machine will probably be off much of the time, and running at lower energies when it is on. I would guess that that will keep their energy bill down.

3. **cea**
   November 4, 2009

   http://washuu.net/cea-bang.htm

A hydrogen bubble chamber at the CEA (Cambridge Electron Accelerator, Cambridge, Mass, USA) really did explode (1965), hydrogen leaked out of the BC, one person died, so the caution CERN is displaying is well-founded.

4. **Yatima**
November 6, 2009

I would say that caution is justified. The blowout of last year could easily have led to loss of life and limb (Are people allowed in the tunnel of live ring collider? There may be some Bremsstrahlung, which can be shielded against though.

Anyway, more LHC weirdness from El Reg:

http://www.theregister.co.uk/2009/11/06/lhc_dimensional_portals/

5. **Bob LaBla**
November 6, 2009

(Are people allowed in the tunnel of live ring collider?)

No, the ring and detector areas are off-limits when the beam is on!

6. **Paul Guinnessy**
November 9, 2009

Peter,
I went back to my original notes. The 450 GeV and a lot of the other details were mentioned at an accelerators meeting in DC a couple of weeks ago (disclaimer, our CEO chaired a session related to it last Monday), plus there are details on CERN's web site if you know where to look.

You were right that the quote in the piece came from your blog. I apologize for the mistaken attribution and it has been corrected.

I do not know where the NYTimes got their piece, but there's been a lot of coverage in the European press, and I suspect they got it from there rather than from us.

7. **Paul Guinnessy**
November 9, 2009

I should also add that CERN's press office has been very helpful in confirming the revised schedule. Sometimes it pays to pick up a phone and ask 🙂

8. **Peter Woit**
November 9, 2009

Hi Paul,

Thanks for writing and for clearing up the attribution of the quote. I'm perfectly happy to have my words attributed to CERN, but they may feel differently…

It's true that reporters do something very important that bloggers don't: call up authorities and get information + public statements from them. Maybe some of my postings can at least help make it clear what questions to ask.

9. **Martin**
November 10, 2009

Hi Peter,

I assume that these delays are indeed the last. If you had to make an educated guess: When will LHC find the Higgs?

Cheers,
Martin

10. **Peter Woit**
November 10, 2009

Martin,

The 2010 run won't deliver enough luminosity to see the Higgs, so the earliest possibility would be data from runs in 2011 and later, analyzed in 2012 and later.

Of course, much more interesting would be to not find the Higgs, but to find something else.

11. **Martin**
November 10, 2009

Peter,

thanks for your quick reply.
What would you guess are the chances that Fermilab will beat them to the Higgs?

Cheers,
Martin
PS What other things instead of the Higgs are you hoping for?

12. **Peter Woit**
November 10, 2009

Martin,

One place you could read about this question is here http://www.scientificblogging.com/quantum_diaries_survivor/worst_nightmare_scenario_cern_150_gev_higgs

especially note the graph showing the probabilities of 2-sigma evidence as a function of Higgs mass.

But 2-sigma is very marginal evidence of a particle. You need 3-sigma to claim “observation”, 5-sigma for “discovery”. My impression is that 3-sigma is possible, 5-sigma isn’t.

About what I’m hoping for: evidence of some mechanism for electroweak symmetry breaking that no one (including me) has thought of yet…

News From NSF THY

November 5, 2009
Categories: Uncategorized

A presentation at a recent SLAC Users Group meeting included some of the following data about NSF support for HEP theory:

Theory funding (including cosmology and astro-particle physics) for FY 2008: \$11.68 million. For FY 2009, \$11.31 million + \$2.3 million from the stimulus legislation.

In FY 2008, these grants supported 128 senior personnel, 84 postdocs and 104 graduate students. For FY 2009 the numbers were 184 senior personnel, 50 postdocs and 70 graduate students.

During FY 2008, 24 out of 57 new submitted proposals were funded, 17 out 21 renewals were funded.

Group grants were categorized as 11 phenomenology, 11 strings, 2 cosmology, 1 general.

Individual grants were categorized as 17 cosmology, 12 strings, 9 phenomenology, 3 astrophysics, 2 lattice QCD, 3 general.

So, as far as NSF HEP grants go these days, if you're not doing cosmology, string theory, or phenomenology, basically you're out of luck…

NSF THY has a new program manager who started Oct. 1. It's Keith Dienes of the University of Arizona, whose research in recent years has focused on the "string vacuum project". He'll be giving a colloquium at Fermilab next month on Probing the String Landscape, which is advertised with the abstract:

> We are currently in the throes of a potentially huge paradigm shift in physics. Motivated by recent developments in string theory and the discovery of the so-called "string landscape", physicists are beginning to question the uniqueness of fundamental theories of physics and the methods by which such theories might be understood and investigated.

Since the late eighties, the two institutions in the US most heavily invested in string theory have been Princeton and Rutgers. Recently they have been moving aggressively to try and diversify, especially in the direction of LHC phenomenology, with the hiring of Nima Arkani-Hamed at the IAS and Matt Strassler at Rutgers. Last year the two institutions collaborated on a proposal for a new Physics Frontier Center with a budget of \$1 million or so per year. This would be called the PARTICLE Center (Princeton And Rutgers Theory Institute for Collaboration with LHC Experiments) and would aim to be the main US center for LHC phenomenology. The proposal promoted the possibility of experimental anomalies to be discovered by the LHC in fall 2009, quickly followed by PARTICLE physicists inventing a model that would explain the data and predict a subtle effect that would require a new triggering strategy to see. The result of this would be a surprising measurement that would explain supersymmetry breaking.

Anyway, that proposal doesn't appear to have been funded, with reviewers rather dubious about the idea of retraining Princeton and Rutgers string theorists as LHC phenomenologists, as well as the idea of devoting significant new resources to funding the Princeton and Rutgers theory groups, centralizing LHC phenomenology efforts there. However, two new year-long grants for \$130,000 each were awarded to Strassler and Arkani-Hamed, who promise to use them to "create the nucleus of an LHC center on the East Coast" at Princeton and Rutgers. One of the goals of these grants is listed as "to help in the process of … retraining postdocs from more formal areas of high-energy theory", since the job market for young string theorists has more or less collapsed.

Comments

1. **Mitch Miller**
   November 5, 2009

   "This would be called the PARTICLE Center (Princeton And Rutgers Theory Institute for Collaboration with LHC Experiments)"

   I would have funded it based on the acronym alone.

2. **Bob Levine**
   November 5, 2009

   Re Dienes' comment—isn't part of the semantics of 'discover/discovery' an entailment that whatever you're talking about demonstrably exists? Talking about the 'discovery' of something is only felicitous if that something is really *there*, no? Or does Dienes really believe that the landscape is just, well, *necessarily* true?

3. **Peter Woit**
   November 5, 2009

   Bob,

   I wouldn't so much quarrel with the use by Dienes of the word "discovery". One can "discover" a previously unknown feature of a speculative model, and that's how I think he's using the term.

   More dubious is actually the adjective "recent" applied to this "discovery". The Bousso-Polchinski paper that set this off was written nearly ten years ago, in an earlier millennium than the current one.

4. **Pawl**
   November 5, 2009

   Dienes' abstract seems not to quite make sense. If we're "in the throes" of a paradigm shift to landscapes, there's nothing "potential" about the hugeness of the change. (Or maybe there's a "landscape lite" alternative I'm not aware of….)

   There's also the timing issue Peter touched on: people are not "beginning to question" uniqueness of fundamental theories — those who wanted to question it have done so, with limilted success in convincing the rest of the community. (In fact, this sentence of Dienes' sounds depressingly like what appears to be the standard-issue wishful thinking common among some of those advocating the multiverse in public, as opposed to scientific, fora.)

   True, Dienes does go one to say

   > I will also discuss some of the questions to which it has led, and the nature of the controversies it has spawned.

   which at least sounds more objective.

5. **Kea**
   November 5, 2009

   Ah, the way of the world. Retrain the people who did what they were told, rather than hire one single person who was critical, in the right direction, all along.

6. **Serifo**
   November 6, 2009

   Humm , perhapes there is a significant number of people in NSF HEP lobbying for string theory , cosmology and phenomenology ! 🙂

   Regards

7. **Robert**
   November 6, 2009

   any news about consequences of the birds' droppings on a cooling aggregate of LHC?

8. **Matt**
   November 6, 2009

   Peter,

   Any sense of why lattic QCD and astro-particle physics make up such a small grants in HEP theory? With these two subfields having access to the greatest amount of applicable experimental data to fit, I am a bit at a loss of how few grants there are in these areas compared to strings & phenomenology. Also a bit interesting the difference between group and individual grants for cosmology as well.

9. **Peter Woit**
   November 7, 2009

   Matt,

   astro-particle physics overlaps with astrophysics, and that is funded separately from HEP. Lattice QFT is a subject that has never really been pursued at the most prestigious places in the US (for example Harvard, Princeton), and has always been seen as a minority and rather un-sexy subject to be working in. It's an interesting question why this is.

10. **SteveB**
    November 7, 2009

    Like lattice QFT, there is a similar phenomena in fluid dynamics. There is the establishment majority that solves the Navier-Stokes equations with various techniques and a small offshoot that reformulated fluid flow into a Boltzman lattice model. At the end of the day both converge to the right answer and it is nice to have two separate methods for consideration. There is a bit of acrimony between the two camps, and sometimes more than a bit. In my opinion, NS is the most popular since it lends itself to easier application of the necessary approximations and simplifications for solving various different phenomena (turbulence, energy, multiphase, etc.) with a reasonable amount of computer power.

Simons Postdoctoral Fellowships

November 9, 2009
Categories: Uncategorized

The Simons Foundation will be funding new postdoctoral positions at various institutions starting next fall. Details of one of these, at the University of Texas, have been announced, with more to follow in coming weeks. These are three-year postdocs, with a first-year salary of $70K/year.

Comments

1. **Stephen**
   November 10, 2009

   Why is every institute funding postdocs and no one is funding real jobs (i.e. long term)?

   When people look for jobs outside academia, in private companies, they normally look and find long-term jobs. Or at least, if they are doing well after some initial time, they can stay longer.

   Now, why a university cannot work in the same way?

   If you have a Ph.D. and looking for an academic job, this seems impossible for many years, so that you have to struggle all the time with applications, relocating, maintain a personal life, relationships, friendships, etc… and in the end you do not know when you will have a permanent job.

   I find all this completely crazy: if somebody works well, why he cannot stay in the same place more than 2-3 years?
   If a Ph.D. is not enough to get a job immediately, this probably means that there are too many people with a Ph.D. or that the whole system is dysfunctional.

   There is probably the wrong idea that putting people under this kind of situation and pressure they will work more. But competition and fear should not be the motivation to do research….Some competition is good, but when it is too much, this is exactly what kills the creativity, the freedom of thought and the passion.
   And gives people (even the good ones) motivations to leave science.

   There is also the idea that forcing people to travel will make them acquire an international experience. True. But wouldn't be easier and better to have just funding for visit and short stays/sabbaticals?

2. **Franckle**
   November 10, 2009

   Dear Stephen, I completely agree with you. I'm a person who works with bursts of great talent when outside there's the sun in the sweet air of the morning… or I can be depressed thinking about the strange system you described so well in which I'm immersed. Thanks Stephen, words are important…

3. **Andy P.**
   November 10, 2009

   Stephen — because of tenure, hiring someone for a "permanent" position is essentially making a 35-40 year commitment. It's not realistic for the NSF or a private foundation to make that kind of commitment.

   Trust me, the situation in math and the sciences is much better than other parts of academia. We can at least get temporary positions that pay a decent wage and have benefits. In (say) English, you essentially have to live in dire poverty adjuncting for years until you can find a permanent job.

4. **Stephen**
   November 10, 2009

   Hi Andy,
   suppose that you are a doctor working in a hospital or a teacher in high school (or I could give many more examples). Is it unrealistic that you get a permanent job? If not, why having a Ph.D. (and maybe even one or two years of postdoc) is not enough to be trusted to get a job in academia?

   The fact that other parts of academia are worse, does not make things any better. It just make things worse for the poor guys who are in the other fields.

   I believe many of us just got accustomed to be treated in this way.
   Personally I think it is unfair and nonsense, and also counterproductive.

   Many people have also short memory: after suffering for many years and finally getting on the other side with tenure, will they do something to hire young people offering permanent jobs? Unfortunately what happens is that tenured people will try mostly to get grants in order to hire people for temporary jobs.
   It is a self-replicating system, unless somebody is willing to try and change it.

5. **Ming**
   November 10, 2009

   In view of the fact that there are many more Physics PhD's than available faculty positions in Physics, I suppose some form of "weeding" process must be implemented. From what I've seen, the ones who eventually made it to tenure are usually very talented (a necessary, but not sufficient condition) and either very excited and committed to whatever they're doing in just the right kind of research (the ones that are in fashion and get funded) or very ambitious in getting a faculty position no matter what it takes, be it personal or professional sacrifices. If you (like me) don't fit into either category, then it's extremely unlikely you'll end up with a permanent faculty position in Physics.

   Is it unfair? Guess so, but who said life is fair? And besides, there are always other careers for Physics PhD's outside academia. Is it unhealthy for Physics that the top academic jobs in Physics are only given to those who're fashion-chasers often with questionable professional ethics? Yup I think so, but it'll take a literal revolution to change the system…

6. **Andy P.**
   November 10, 2009

Stephen — do you really think that senior faculty can do all that much to increase tenure-track hiring? I can assure you that most departments fight like dogs for every tenure-track line they can get. The money simply isn't there.

I agree that the job situation in math can be frustrating, but blaming it on tenured faculty with "short memories" is simply wrong. Getting grants and hiring postdocs is really all they can do.

Just give thanks that if you end up leaving academia, you will be able to get a very nice job. Your salary will probably be better than most tenured faculty. This is simply false for most other parts of academia.

7. **Peter Shor**
November 10, 2009

Dear Stephen,

When you get a job in a private company, you can be fired or laid off at any time. In fact, I know a number of people who have been laid off and have had to scramble for another job (and they didn't always get one right away). It's not fun. So in some sense, a three-year postdoc position is more secure than many other jobs.

The biggest problem is that there is a big supply of PhD's in physics, and not anywhere near as much demand. In computer science, most PhD's go either directly to tenure-track positions or industry. Only top universities can get reasonable people by offering them postdoc positions, because graduating PhD's have so many more good opportunities than they do in physics.

So if you want a better job market in physics, discourage people from going into physics (say, start up a big String Wars brouhaha to make the field look less attractive …).

8. **POM**
November 10, 2009

Dear Andy,

if the senior faculty cannot help to increase tenure track hiring, then who else can do it ? It is certainly unfair to blame "Senior faculties" as a whole to have short memories, but one can certainly blame some of them (and – to my experience which is quite large on that matter – not only few of them) no to care at all to what happens to the "youngs in their field".

I have heard many times this kind of answer "yes but you know… life is unfair". So what ? One should just shut up and accept it ? This is the kind of attitude which is expected to be promoted in science ?

Many academics consider themselves as bright and clever people, with high morality and self-declared ability to select "the best one(s)". But very often the final decision is mainly politics. This is this hypocrisy that I (and other long-term postdocs) find unbearable:

-on the one side there is a community which keeps on claiming that its main selection criterium is "scientific excellence",

– on the other side as soon as you start hearing what is really going on inside (some) hiring committees, you discover that often more than "scientific excellence" (whose definition by the way varies depending on whom the position has been designed for) what is important are dirty politics, local interest, or simply "délit de sale gueule" (or age segregation, that is unsaid because it is illegal, but that "naturally" applies).

Surely academics in physics or maths is not worst than other segments of the society. But at least in the bank industry nobody claims to be driven by high principles. The aim is to make profit. Full point.

9. **Thomas Larsson**
November 11, 2009

In a steady-state situation, each advisor will on average give birth to one new advisor in the next generation. So if an average professor produces ten PhDs, 90% of his students will not get tenure themselves. It is really as simple as that.

10. **POM**
November 11, 2009

Except that a human society is a not a gas, not even a perfect one. There are many and various interactions between its components, from very short to long range. Moreover "Academy" is certainly not an isolated system. It is neither homogoneous nor isotropic. One does not understand much of it by simply taking global averages.

11. **Chris Oakley**
November 11, 2009

To expand on TL's comment, one should not forget [Weil's Law of university hiring](#) (under "Quotations") either. On average, therefore, the quality declines.

12. **Bob Levine**
November 11, 2009

'Except that a human society is a not a gas, not even a perfect one. There are many and various interactions between its components, from very short to long range. Moreover "Academy" is certainly not an isolated system. It is neither homogoneous nor isotropic. One does not understand much of it by simply taking global averages.'

But TL's comments don't depend on a human society or an academic subcomponent being a perfect gas. The situation is much simpler: under normal conditions, the demand for university faculty will correlate closely with the size of the student population, so that only radical increases in the size of the latter, or some other very unusual combination of factors, will significantly affect the intake rate for faculty. The last time I can recall that happening was in the wake of Sputnik, when all that NDEA Title 4 money poured in to create hundreds of new departments in many fields, and many of my colleagues received calls—as *graduate students*—from department heads pleading with them to accept academic positions in their departments. That's going to happen extremely rarely in any given century. Yet, in line with TL's point, if each professor trains on average only two Ph.D.s in his or her academic career, it would require something at least as spectacular as Sputnik to absorb the whole candidate pool into the tenure-track professoriate—and this at a time when administrators are trying to reduce the size of the tenured/tenure-track faculty by hiring part-time faculty on short-term contracts with minimal benefits and often terrible working conditions. It's a mistake to blame senior faculty for this—we often fight like hell to minimize such practices. But university administrators are committed, in my view, to deprofessionalizing their faculties, and they use every trick in the book to do so.

There's just a fundamental problem here: a department's size is one of its chief guarantors of survival, at a time when graduate programs are being merged or shut down completely. But the only justification of increasing size—hiring more faculty—is that you've admitted more students and need instructors to teach them, not just at the undergrad level but in your graduate programs. And that means you wind up creating way too many Ph.D.s for the market. If senior faculty can be blamed for anything, it's for promoting excessive growth in their programs; but if they don't, those programs themselves may well get the chop. And I've yet to hear of any constructive suggestions to solve this conundrum that were particularly realistic…

13. **Stephen**
November 11, 2009

Dear Andy,
if the money simply isn't there, there are two viable options:
either trying to get more money, or to train less Ph.D. students (or both). Just enough to cover the demand.

Moreover, for instance, many senior people are frustrated with having too much teaching. Teaching often implies teaching to undergrads, also to non-physics students. There is certainly demand for this. Then, why not hiring more young people for mixed long-term positions (teaching+research), so that the load would be less also for tenured people?

14. **Coward**
November 11, 2009

Stephen: I'd love to hire some young people to reduce my teaching load! And where am i supposed to get the money for this, especially in the current economic crisis with most universities in very dire budgetary situations? If anything, I think universities will feel pressure to increase teaching load to save money on faculty salaries.

15. **Lee**
November 11, 2009

Professors are to blame when they hire and retain low-salary postdocs and graduate students while giving them exaggerated prospect of their landing a tenure track job later. The false advertisement comes in the form of overstating the impact of the proposed research to be done together (this project will save the world), and understating the hiring criteria of perceived lower-rank universities (you will be a professor in some state university). Selling the professor's research and training package to a postdoc in this manner is nothing short of selling stock or house with bubbled price, and is downright unethical if the broker intentionally does so to a new customer in the market.

The sad thing is, most of academic research has extremely low value, from the society's point of view. Adding a drop of knowledge in the ocean of existing knowledge is not a good bet. Its best bet for market performance is leveraging its educational value, but letting postdocs to gain teaching experience is what most professors don't like to do – they want isolated and illusioned postdocs in their lab producing results for next NSF funding. This is unethical.

16. **Keith Schilling**
November 11, 2009

"Why is every institute funding postdocs and no one is funding real jobs (i.e. long term)?"
totally agree – however given the state of the economy it shouldn't be too big of a surprise is it?

17. **Michael Gogins**
November 11, 2009

I believe this situation has mixed causes. In the first place, contrary to what many in the system believe, society values university research very highly and funds it at a high level — even if it is not the even higher level that some in the system might prefer.

The funding comes partly because of an expectation that university research is required to win future wars. This is a realistic expectation. However, since we have not had a war that has been decided by university research since 1945, this feeling is dying out with the generation of leaders who feel the motivation, although the reality of the situation in fact persists (think about space battle, or information warfare).

Of course warfare, however important it may be, is an intrinsically unscientific motivation, and accepting funding on this basis is sickening the sciences. The same goes, to a lesser extent, for funding based on expectation of better medicine, or a more productive economy, or maintaining "national competitiveness."

Right now the life sciences are being perverted by a search for means of extending the human life span. I am decidedly in favor of doing this, personally, but I expect a good deal of dishonesty to enter medical science as a result.

The right way to fund the sciences – or the arts, for that matter – is to donate money to the best people for whatever it is that they, personally, feel is worth doing. Since they are of course the only ones even remotely capable of deciding what is worth doing! This is an essentially elitist view and does not sit well in a democratic system. It is ultimately founded on a contemplative, non-utilitarian view of how talented people should spend their time.

The tenure system for all its faults reflects this elitist view, inherited from a far more contemplative age. But it is to be expected that in a crowded world of well-fed, relatively well-educated people, long-lived people there will many more people who want to do this kind of work than there are places for them.

The postdoc system in my view is an uneasy compromise between the elitist and utilitarian views. The postdoc system is sort of like boot camp or hazing, and it does more or less get unserious or mediocre people out of the way. But from the outside, where I sit, it would seem to create a very poor environment in which to contemplate and think. The worst consequence is the inflation of "style" or "fashion" in science where there should be only a small place for it, along with party feeling, purges, and the whole 9 yards.

We desperately need multiple approaches to really hard problems, but the utilitarian, competitive approach, perhaps paradoxically, does not naturally support this. Generous private funding has in fact been necessary to supplement the support of alternative approaches to physical theory in North America.

If you don't want this environment, you will have to find other ways to discourage unsuitable or superfluous people. And better ways to encourage the most suitable people! But in our society, whatever you do will be perceived as unfair.

I don't know what to say about that. Good luck all,

Mike Gogins

18. **Peter Woit**
November 11, 2009

It should be pointed out that Simons has funded permanent positions: the new center for geometry and physics at Stony Brook will have 7 permanent positions (director, 3 physicists, 3 mathematicians).

19. **POM**
November 11, 2009

"If senior faculty can be blamed for anything, it's for promoting excessive growth in their programs; but if they don't, those programs themselves may well get the chop. And I've yet to hear of any constructive suggestions to solve this conundrum that were particularly realistic…"

What I blame (some of) the senior faculties for, is the unbearable lightness with which they manage the hiring process. Indeed the job market is hard. This is precisely why one should expect the hiring process to be rigorous, clear and clean. And to a large extend this process is in the hands of senior academics. I have little experience with the american system, but I have the feeling that in (at least some part of) Europe, applicants are rather used as canon fodder, for the benefits of local interest that one is aware of once the process is over.

Just a simple illustration: why is it so difficult to obtain a constructive opinion on one's application, which goes beyond the useless "your research interest did not meet the priority of our institute. We wish you success in your future academic life" ? Is it because the professional-life of some non permanent staff is not worth the time a permanent researcher would spend answering him ?

20. **Alejandro Rivero**
November 11, 2009

Of course it is possible for private companies to fund long term positions:

"Lucas, in his will, bequeathed his library of 4,000 volumes to the University and left instruction for the purchase of land whose yielding would provide £100 a year for the founding of a professorship"

But they can also endow, at the same price, a fellowship.

21. **Andy P.**
November 11, 2009

POM – In the United States (I don't know about Europe), there is often an explicit policy forbidding people from sharing the reasons someone wasn't hired. It can very easily lead to lawsuits, even if the reasons are totally reasonable.

22. **Stephen**
November 11, 2009

It was not my intention to blame tenured professors, in a generalized way.

I just wanted to remind that, from the point of view of young Ph.D.'s life, it is much better to have opportunities for long term jobs. And I think very few people would disagree with this statement.

Some people made the point that there is no money for this (for permanent jobs or mixed research/teaching positions).

The solution to this conundrum is actually trivial: the money which is spent for hiring postdocs could be spent for paying salaries for long term jobs.

Moreover, many people would by far prefer to have a long-term jobs, even if they were paid a little less. So departments would not have to spend more than what they spend now. Maybe even less.

23. **Andy P.**
November 11, 2009

Stephen –

I don't things are quite as simple as that. Let's assume that all postdocs were converted to tenure track jobs (which will never happen). OK, there would be a spurt of hiring for a couple of years. After that, tenure-track hiring would revert to its usual lousy state, but there would be no postdocs. In the end, young people would be even worse off.

The cold, hard truth is that academia is not growing. If anything, it is shrinking. There will never be a time in which there are enough permanent jobs for all new PhD's to get them.

I suppose one could argue that the solution is to shrink graduate programs, but I assure you that if graduate programs shrank, then so would departments, and tenure-track hiring would slow down even more.

The only solution I see is for universities to do a better job at preparing their graduate students for non-academic employment.

24. **Stephen**
November 11, 2009

hi Andy,
so basically you are saying that a large fraction of graduate student which end up out of academia is necessary to maintain the system.

I might agree for those fields in which graduate studies naturally lead to other career paths (for example law, medical studies, engineering etc… have other options, in a natural way).

For those fields which are mostly academic (such as theoretical studes, maths, etc..) what I am saying is that basically the number of Ph.D. should be equal to the number of academic/teaching positions, by increasing the number of jobs and/or by reducing the Ph.D. if the former is impossible.
It is better, in my opinion, to have a bottleneck (if any is really needed) at the beginning of the studies rather than later on.

I would not worry about the number of graduate students being smaller.

25. **Jeremy**
November 11, 2009

Stephen asks:
"Why is every institute funding postdocs and no one is funding real jobs (i.e. long term)?"

The problem is partially because of the "senior faculties". If the senior faculties are forced to retire when they are 60 years old (like they are in Japan), or 65 years old (like they are in the U.K.), there will be a lot more "real jobs" available.

26. **David B.**
November 11, 2009

Jeremy:

US law forbids mandatory retirements except in some professions requiring certain kind of fitness level (police officers, etc), were not retiring the individuals put others at risk. This is part of the anti-discrimination laws based on age. Suggesting that it be changed to fix academia is at best wishful thinking.

27. **Nicoletta Sabadini**
November 11, 2009

How is it possible that all these apparently intelligent and talented students don't understand that they have voluntarily entered into a mousetrap? The fact that someone has studied for ten years does not imply that anyone should pay them to do the same thing for ever.

To make an example, the conservatoriums are full of students of piano who are skiiled and study for many years, but the need for concert pianists is very limited.

This does not excuse my colleagues who, for their own personal interests, are still advising students with false promises to enter into a process which starts with a PhD and ends with disappointment.

28. **peter shor**
November 11, 2009

Stephen says:

> Moreover, for instance, many senior people are frustrated with having too much teaching. Teaching often implies teaching to undergrads, also to non-physics students. There is certainly demand for this. Then, why not hiring more young people for mixed long-term positions (teaching+research), so that the load would be less also for tenured people?

I don't see how the economics of this works. Are you suggesting that we have two classes of tenured faculty, one with high teaching load and the other with low teaching load? Try selling that to the administration at your university.

Otherwise, hiring more tenured faculty to reduce the teaching load requires more money.

29. **peter shor**
November 11, 2009

Stephen says

> Some people made the point that there is no money for this (for permanent jobs or mixed research/teaching positions).
>
> The solution to this conundrum is actually trivial: the money which is spent for hiring postdocs could be spent for paying salaries for long term jobs.

But the tradeoff is something like 20 postdoc positions for one permanent position. This isn't going to make a dramatic increase in the number of tenured faculty.

[How I get this figure: a tenured professor is going to occupy a position on the order of 10 times the length of a typical postdoc, and the salary is on the order of twice that of a typical postdoc.]

30. **POM**
November 11, 2009

Nicoletta,

I have got a good friend who is a conductor. Certainly he will never conduct the Berlin Philarmoniker (although I wish he will one day :-)) but he found a stable position as an orchestra-conducting teacher in a good conservatorium, he is making concerts, he plays with various "ensembles". In brief, he is doing music.

In maths or theoretical physics this is an "all or nothing" alternative: either in a short window of time after your phD you find a permanent position, or you are supposed to leave research. And a high-school teacher is not with respect to a researcher what an orchestra-conducting professor is with respect to the Berlin Philarmoniker conductor. When my friend plays with a good-although-not-famous ensemble, he is making music. When one is teaching physics at high-school level, one is not doing research (which does not mean that teaching is not a beautiful job, but this is a different job than research).

That is what is extremly difficult to swallow in the academics world: it is a kind of random process, in which some people are "elected", others are "damned", and when you ask why you are damned, you are answered "well, you know, life is unfair". Again, this would be fine if this were not happening in a community which bases all its legitimacy on its supposed "rational attitude". From people that have been "elected" because they were "the best", one is entitled to require higher level of explanation than just "life is unfair".

31. **Walter**
November 12, 2009

I don't think that from a financial viewpoint the ratio of postdocs to tenure-track positions should be viewed as 20 to 1 or anything like that… Many of these postdoc positions are recurring and have been around for decades.

Still I think it's a financial issue. When there's a budget crunch it's simply cheaper to hire a temporary person than a tenured track, say when someone retires. Over time this leads to more temporary jobs. Another issue worth mentioning for the US is that there's a lot more flow from abroad than vice versa, and this makes the job market tighter. Not that this is necessarily a bad thing, it keeps American science and math dominant. But it's not good for domestic PhD's

32. **Thomas Larsson**
November 12, 2009

"I would not worry about the number of graduate students being smaller."

Alas, advising grad students is an important part of a professor's job. Less grad students => less work for professors => layoffs.

Implicit in this discussion is the assumption that a PhD in theoretical physics is totally worthless outside of academia, and that theoretical physics PhD's are too incompetent to find non-academic jobs. Whereas there might some truth in this, I don't really buy it.

33. **Tim vB**
November 12, 2009

@Thomas Larsson
From my own experience on the job market:
Companies do always look for an expert with specific experience and training. A Ph.D. in physics means you are trained to do research in physics, and there are very very few jobs outside academia that involve serious research in physics!
So the typical situation is something like this: A company looks e.g. for a software programmer with knowledge and experience in the development of big object oriented software systems using Java and a three tier architecture with a web client and a relational database.
If they can get one who fits the picture, fine. If not, they will settle for someone with a Master in computer science.
If they don't get one, they may in certain circumstances take someone with a physics Ph.D., if he/her can convince them that he/her doesn't need at least one year of training before he/her becomes productive.
So, our Ph.D. will mostly be considered to as a kind of fallback plan C. Look at the job market and decide for yourself: What are the chances?

34. **Peter Shor**
November 12, 2009

Hi Walter,

Say, over a thirty-year period, with the same amount of money you can fund 1 tenure-track position or 20 3-year postdocs. So if there are currently funds for 400 postdoc positions per year, and you use all this money for tenured faculty, you get 20 new tenured faculty positions a year. What you've done is replaced 300 very disappointed postdocs by 280 very disappointed new PhD's. How much of an improvement is this?

35. **Dave**
November 12, 2009

A new faculty member has several years to attract enough funding, produce his/her first batch of graduate students, and get sufficient amount of research done before the tenure review process kicks in. It's a busy time, especially when one factors in the service and teaching commitments. A postdoc offers a newly minted PhD an opportunity to beef up his/her record as a researcher and time to mature some more as a scientist and a person with relatively few distractions prior to stepping onto the junior faculty treadmill. It may be more of a blessing than a curse.

36. **Walter**
November 12, 2009

Peter.. I know what you mean… adding one tenure-track job this year will result in 20 people not getting postdocs this year. But the way I look at it is that the number of people who can continue on in the profession is determined by the number of tenure-track positions overall. By the pigeonhole principle, most of these 20 people would end out having to leave the profession anyhow unless they can find a way of becoming "life-long postdocs." My opinion is that this should be done earlier rather than later, which is the opposite of current trends.

37. **Walter**
November 12, 2009

Incidentally, from a financial standpoint the 1:20 thing isn't how it works. Most departments have "postdoc lines" which cost a certain fraction of a tenure-track line. So from a financial standpoint a postdoc line is 1/3 of a tenure-track line, and so on.

38. **Thomas Larsson**
November 13, 2009

Tim vB: Based on my own experience, I would say that the chances for a theophys PhD to eventually find a job in the private sector is close to 100%.

Of the 20+ people that got a PhD from the theoretical physics dept at KTH, Stockholm, and were born between ca 1955-1965, I know two who ended up in academia (or stayed longer than I did). One is a professor in solid mechanics, and the other works with biophysics, not sure if he is professor. The remaining 90% have got jobs elsewhere. A lot of people ended up working for Ericsson, which is the local high-tech giant, and nobody is AFAIK flipping hamburgers at McDonalds.

However, things might be somewhat simpler because KTH is an engineering school, like MIT or Caltech, so we have a marketable undergraduate degree to fall back upon.

39. **Tim vB**
November 13, 2009

Agreed, the best insurance against unemployment is an academic degree, and one in the "hard" sciences or in engineering is even better.
My point is: As a physics Ph.D that drops out of academia, you will notice that you spend about 5 years of your life learning stuff that is no longer of any use to you.
And you will spend a few hard years to learn all the stuff that you need for your new job.
Eventually you will catch up with your collegues, but the feeling that the decision to study physics instead of e.g. computer science was wrong will never quite fade away…

40. **Thomas Larsson**
November 13, 2009

Personally I feel rather grateful to the taxpayers, including those spending their days flipping hamburgers at McDonalds, for giving me almost ten years (four years in grad school + a four-year postdoc + some extra time – hey, my academic career was longer than lubos') to pursue my interests, and giving me the opportunity to make a substantial discovery of lasting value. That the physics community hasn't appreciated this discovery (the math community has been more open-minded) is hardly something that you can blame on the taxpayers.

In SUSY We Trust

November 11, 2009
Categories: Uncategorized

New Scientist has an article in the latest issue entitled In SUSY we trust: What the LHC is really looking for, which promotes the idea that the LHC is going to discover supersymmetry. Only supersymmetry enthusiasts are quoted. I'd be curious to see some data on what the distribution of views of particle theorists is on this issue (one piece of evidence that supersymmetry skepticism is in the majority is here). Among bloggers, at one end of the spectrum is Sean Carroll, who gives a probability of 60%, at the other is Resonaances, with 0.1%. Personally, I'm with Resonaances, at least as far as conventional supersymmetric models go. The main arguments against supersymmetry, ignored in New Scientist, are that supersymmetry breaking is both necessary and hideously ugly, and if this was going to solve the hierarchy problem, we'd have seen evidence already at the Tevatron.

The article does a good job of recounting the pro-supersymmetry arguments (hierarchy problem, unification of couplings, dark matter candidate), but then goes completely off the rails with an absurd claim that supersymmetry explains confinement:

> Supersymmetry's scope does not end there. As Seiberg and his Princeton colleague Edward Witten have shown, the theory can also explain why quarks are never seen on their own, but are always corralled together by the strong force into larger particles such as protons and neutrons. In the standard model, there is no mathematical indication why that should be; with supersymmetry, it drops out of the equations naturally.

At least we'll know one way or another within a few years from now…

Comments

1. **Eric**
   November 11, 2009

   I would disagree with the statement that if supersymmetry solves the hierarchy problem then it should have been seen by now at the Tevatron. The Tevatron simply does not cover the entire allowed parameter space. Additionally, even if you think that supersymmetry breaking is ugly, then alternative models such as technicolor are even uglier. In any case, the indications that the Higgs has a light mass do favor SUSY.

2. **factcheck**
   November 11, 2009

   The author of the article doesn't even list the correct institutes of the people in the article. Another excellent piece of journalism by the new scientist.

3. **Peter Woit**
   November 11, 2009

   factcheck,

   The only inaccuracy of the kind you mention that I noticed was the description of Seiberg and Witten as affiliated with Princeton University rather than the IAS in Princeton, but that's rather small potatoes…

4. **Bill K**
   November 11, 2009

   Eric says: "In any case, the indications that the Higgs has a light mass do favor SUSY."

   SUSY favors a light Higgs. Which is not at all the same.

5. **Eric**
   November 11, 2009

   Bill K,
   SUSY favors a light Higgs, and the experimental data favor a light Higgs. For non-SUSY alternatives, there is no reason for the Higgs to be light. By light, I mean less than 135 GeV. As Peter says, time will tell and hopefully we'll find out in the next few years.

6. **Pawl**
   November 11, 2009

   Another curious aspect of the NS article is that they mention Brout and Englert as well as Higgs, but leave out Guralnik, Hagen and Kibble (compare here) .

   I can't really fault them for this — it's really a question of whom they've talked to.

7. **Thomas**
   November 12, 2009

   Of course, there is also the opinion of Veltman, who stated, as recently as October 2009, that he would bet that the Higgs does not exist; in his book on particle physics he also writes that supersymmetry is only "a figment of the human mind".

   The LHC will show us who is right.

8. **Thomas Larsson**
   November 12, 2009

   In a more recent post, the Jester said something very striking:

   "If supersymmetry is relevant at the weak scale it is in general very uncomfortable with a heavy Higgs. Well, they keep telling you that the upper limit in the MSSM is 130 GeV. But that requires stretching the parameters of the model to the point of breaking, while the natural prediction is 90-100 GeV. Indeed, not finding the Higgs at LEP is probably the primary reason to disbelieve that supersymmetry is relevant at low energies."

9. **Arrow**
November 12, 2009

The probability of SUSY being correct is 10e-27 and that of Higgs being correct 10e-9.

However with probability of 10e-2 a newly discovered particle will be wrongly identified as Higgs boson and with 10e-3 a newly discovered particle will be wrongly identified as superpartner of one of SM particles.

10. **Haelfix**
November 12, 2009

The actual 'bayesian' measure on how to determine what mass of the Higgs is most natural for supersymmetry is a complicated story. Impossible to do unless you restrict the range of models (so people talk about the constrained MssM and so forth). For instance there are models that predict a ~80-90 GeV Higgs that are not actually excluded by experiment, b/c their decays are so strange and elusive.

The generic tendency is to prefer a light Higgs, but it is by no means exhaustive and assigning 'weights' to models is a mess.

As far as what most physicists prefer, well if you add phenomenologists, it also gets messy. Everyone has their favorite pet model, but a generic MssM probably beats the nearest competitor (say extra dimension models) 3 or 4 to one.

It has the benefit of simultaneously having the most explanatory power, and in some ways the simplest solution to the variety of problems that are out there.

11. **Mary**
November 16, 2009

The article has either an explicit snub or was very poorly researched. Along with Peter Higgs, Francois Englert, and Robert Brout there was another team that deserves as much, if not more, credit for the discovery of the mass boson. Gerry Guralnik at Brown University, Dick Hagen at University of Rochester, and Tom Kibble of Imperial College London wrote a paper in the same volume of Physical Review Letters in 1964 that laid out the boson in clear terms, lent mass to the gauge particle, and showed how Goldstone theorem was avoided. All three papers were recognized as milestone papers by Phys Rev Letters 50th anniversary.
http://prl.aps.org/50years/milestones#1964

Additionally, all six were recently awarded the 2010 J. J. Sakurai Prize for Theoretical Particle Physics "For elucidation of the properties of spontaneous symmetry breaking in four-dimensional relativistic gauge theory and of the mechanism for the consistent generation of vector boson masses".
http://www.aps.org/units/dpf/awards/sakurai.cfm

Steven Weinberg, who is well versed in this history and quoted throughout Ananthaswamy's article, credits all three teams as recently as a speech he delivered last month. Ironically, this speech occurred at the Council for the Advancement of Science Writing's annual symposium.
http://blogs.physicstoday.org/newspicks/2009/10/higgs-discovery-likely-at-lhc.html

With such an extensive view of history in this article, missing this GHK team certainly seems sloppy at best – or with motivation (at worst).

12. **Peter Woit**
November 16, 2009

Mary,

Personally I think squabbling over credit for this amongst these three groups of six people is a bit absurd. The basic idea is not due to any of them, but is due to Phil Anderson, who discovered it a year and a half earlier. And yes, I know that most particle theorists didn't understand/believe him, mistakingly thinking that his mechanism required breaking relativistic invariance. They were wrong, he was right, and showing this in detail is all that these six people did.

13. **Larry**
November 17, 2009

Here is a recent historical perspective with actual physics work that outlines some of the "Higgs" and GHK work above. Good read.

http://arxiv.org/abs/0907.3466

PDF:
http://arxiv.org/PS_cache/arxiv/pdf/0907/0907.3466v1.pdf

14. **Peter Woit**
November 17, 2009

Larry/Mary (who share an IP address),

Whoever you are: I see no good reason for you to be posting anonymously, and using different pseudonyms to make it look like comments are coming from different people is dishonest and reprehensible.

The paper you refer to is quite interesting, it answers several questions in my mind about the history of this. However, I should say that I don't find the reaction of the authors to being told that they were scooped by Higgs, Brout-Englert, and even earlier Anderson to be very creditable. Guralnik describes Higgs/Brout/Englert as "aimed in the correct direction, they did not form the basis for serious calculation", but I suspect few other theorists would agree with that evaluation.

He also admits that Anderson's work was brought to his attention, and writes: "In general these comments are correct. However, as they stand, they are entirely without the analysis and verification needed to give them any credibility." and uses this to justify not even referring to Anderson's work. This seems to me not at all the way to behave in the face of evidence that someone else understood the crucial points and worked out an example demonstrating them a year and a half before you did.

15. **Tim vB**
November 18, 2009

I'd like to take the opportunity to ask a little question about anonymity:
I'd like to stay anonymous (well, semi-anonymous since "Tim" happens to be my first name in real life) to the readers of some blogs, but not necessarily to the hosts. Don't the hosts have access to the email address that I enter for each post?
This gives away my full name, plus you got my email address (you can actually contact me via this address).

16. **Peter Woit**
November 18, 2009

Tim vB,

I don't have any problem with people like you who identify themselves to me via their e-mail addresses, but use a semi-anonymous name publicly.

I do have a problem with people who use anonymity dishonestly, engaging in "sock-puppitry" to try and advance their cause.

17. **magicmike**
November 22, 2009

Hi Peter,

I'm a little confused about why you think that the article goes "completely off the rails" with an "absurd claim" that supersymmetry explains confinement. Is it just that you don't agree with the rigor of Seiberg and Witten's computations in their 1994 paper?

18. **Peter Woit**
November 22, 2009

magicmike,

The problem of explaining why quarks are confined is a problem about QCD, a non-supersymmetric theory. Invoking supersymmetry as an explanation of why quarks are confined is nonsense.

The Seiberg-Witten story is about N=2 supersymmetric Yang-Mills, which has rather different behavior than QCD. I don't think there's any problem with their results, but they don't explain confinement in QCD.

19. **Peter Orland**
November 22, 2009

There is an even older 3+1-dimensonal model which displays confinement; the (non-supersymmetric) Georgi-Glashow model with sufficiently small monopole mass and large enough (renormalized) U(1) coupling. Polyakov discussed such models in 1977. As I understand things, confinement in the Seiberg-Witten theory is very similar (and importantly some quantities can be determined exactly).

If the monopole mass is too large, the magnetic monopoles of this model are just particles. These monopoles condense if the mass is small enough (but not zero), leading to confinement. What is much better known is that the 2+1-dimensional model confines for any coupling or mass. But there is confinement even in 3+1-dimensions, under the right circumstances.

As Peter W. says, confinement in QCD is very different from that in such models.

Witten on J Street

November 11, 2009
Categories: Uncategorized

The latest New York Review of Books (December 3 issue, not yet online) contains an article by Edward Witten entitled “The New J-Lobby for Peace”. It’s about [J Street](), an organization set up last year to lobby in Washington in favor of Middle East peace. Witten is on the organization’s advisory board. For more about his views on J Street from last year, see [this article]().

At the moment, he’s both hopeful that J Street will start to have an effect, and fearful that it might be too late, writing:

> The rise of J Street gives strong promise that Jews with a more liberal outlook on the Israeli-Palestinian problem will now have a voice in the American political system.
>
> The real question about J Street may be not whether it will grow but whether it is simply too late. Numerous trends, including the spread of Israeli settlements, the increase of the Palestinian population, the rise of Hamas, and growing Orthodox influence in Israel, may be putting a two-state solution out of reach.

Witten has been involved in this issue for a long time, on the board of Americans for Peace Now since 1991. It’s great to see such a prominent member of the physics/math community standing up on this issue, and encouraging that he sees something positive happening. I share his hopes for J Street, as well as his fears that it may be too late.

Sorry, but I’m not allowing comments on this posting. While I think this is a very important issue and wanted to make people aware of Witten’s article, I don’t want to host political discussions on this blog, especially on this topic.

Update: The Witten article is now available on-line [here]().

No Comments

A Line on String Theory

November 12, 2009
Categories: This Week's Hype

According to the Harvard Gazette, it seems that string theory predicts a very distinctive experimental signature that should be easily observable at the LHC. The claim is that string theory predicts that the LHC should produce stau particles, with a lifetime of a minute or so. I'm no experimentalist, but I'd think a charged particle with no strong interactions, a mass of many hundreds of GeV, and long-lived enough to go all the way through the detector, should stick out like a sore thumb. This might be the kind of thing you only need one of to claim discovery of a new particle, and could even be expected to show up very early after the LHC is turned on.

So, at least if you believe the Harvard Gazette, we may be only a few weeks away from having an experimental result that will settle the string theory question once and for all. Either Vafa and collaborators will be getting the 2010 (or 2011 at the worst) Nobel prize, or string theory's prediction will have been wrong and we can say goodbye to the theory for good. Next year should be exciting…

Update: Some commenters were pessimistic that the first year LHC would produce these supposed staus at an observable rate. If I read this presentation correctly (page 54), only 40 inverse pb are needed to produce 3 events of a 200Gev stau. Maybe this model will get verified or killed during 2010. From the same conference, see Michael Peskin's summary talk for more about what the LHC might see in 2010.

Comments

1. **Eric**
   November 12, 2009

   The long-lived stau occurs in gauge-mediated supersymmetry breaking with a high scale of SUSY-breaking where the gravitino is the LSP and the stau is the NLSP. This is the type of scenario that is characteristic of the F-theory compactifications that Vafa has been studying with Heckman.

2. **a quantum diaries surivivor**
   November 12, 2009

   Hi Peter,

   "Next year should be exciting" –> if you are gullible enough 😉

   There are indeed searches for decay signals of long-lived particles designed in CMS and ATLAS. The idea being that you get signals when the beam is off! You trap these particles in the detector material, and then they decay at their leisure.

   I doubt anybody would stake a decent meal, let alone their scientific reputation and line of research, on such a prediction. It is just fun to know that we'll be able to knock such a thing off the board with little early-beam data, but not overly exciting.

   Cheers,
   T.

3. **Peter Woit**
   November 12, 2009

   Hi Tommaso,

   This thing is not just long-lived, but it's also charged, so you should see it go through your detector, not have to wait for it to decay, right?. How hard would it be to recognize a, say 200 GeV stable charged particle? And how much luminosity at 7 TeV would be needed to produce a few such particles (assuming MSSM or something like it)?

4. **Javier**
   November 12, 2009

   It is important to clarify tht if that signature isn't actually found it is not string theory what would be falsified but only the F-theory approach of Vafa.

   Still, whatever we would see (or not see9 is quite interesting because this F-theoretical approachsare a whole area of string phenomenology and, nowadays, the best developed one.

   I would add that this F-theory approachs also have concrete implications in cosmology, concerning drk matter and what can find FERNI. In particular it is expected that the ATIC and PAMELA aparent signatures of WIMMPS would be false.

   By the way, this conclusions were presented by Vafa in the slides of the last string theory 2009 in a very assequible way.

5. **piscator**
   November 12, 2009

   look. the chain of reasoning that says that metastable staus are a real prediction of F-theory GUTs (let alone of string theory) is euphemistically described as dubious. i think this is clear to anyone who has studied them.

   btw, there seems to be no text in the linked article?

6. **Peter Woit**
   November 12, 2009

   Javier,

   I see, so the headline of the story is wrong, this isn't a prediction of string theory. Any idea why someone would put an incorrect headline on a story like this?

   piscator,

Maybe you have an idea about how to reconcile the headline with your “euphemistically described as dubious”? Link works for me, giving a web-page with text.

7. **Eric**
   November 12, 2009

   I think the point is that this signature is characteristic of a specific mechanism for breaking supersymmetry. If this is observed at LHC, it would be strong evidence for gauge mediation and could suggest that the specific type of F-theory compactifications considered by Vafa and his collaborators is the right direction in which to pursue phenomenology.

8. **Toma Susi**
   November 12, 2009

   I’m not an expert, but aren’t these pretty big assumptions?

   “Vafa and Heckman devised two constraints that greatly narrowed the possible string universes. First, they assumed that gravity does not have to play a role in the unification of the other three forces. And second, they assumed that one property of string theory, called supersymmetry, is present at the energy levels generated by the LHC.”

9. **piscator**
   November 12, 2009

   >>Maybe you have an idea about how to reconcile the headline with your “euphemistically described as dubious”?

   I think the author or sub-editor has a good future writing articles for New Scientist.

   The link just gives me a large picture of Vafa with no text, although have just discovered the article by looking at the html source.

10. **Thomas R Love**
    November 12, 2009

    Like piscator, when I followed the link I did not get the article, but the entire mag is at:

    http://news.harvard.edu/gazette/wp-content/uploads/2009/11/HarvardGazette11.12.091.pdf

11. **Peter Woit**
    November 12, 2009

    Toma,

    Low-energy supersymmetry has always been advertised as a positive feature of superstring unification. Decoupling from gravity is essential to get anything. Without this, your particle physics predictions depend on the details of your Calabi-Yau, fluxes, etc, and you basically can’t predict anything (this is the landscape problem).

    You can certainly give up one or both of these things, but if you do, it seems likely that you’ll never predict anything that can be observed.

12. **a quantum diaries surivivor**
    November 12, 2009

    You are right, Peter. If it is charged, it can also be seen with other -more mundane- techniques, such as the large ionization it leaves in the tracker. All in all, the limitations in the detection of such things is pretty much driven by their production rate, and so the model details.

    Cheers,
    T.

13. **anon.**
    November 12, 2009

    “It would be the smoking gun for our stringy models,” Vafa said.

    I hope that’s a misquote. It’s a signature of a wide range of gauge-mediated models. “Smoking gun” usually means a direct implication, not a hint. It’s also not at all clear that F-theory GUTs necessarily imply gauge mediation, and there are many issues (like moduli stabilization) that are glossed over in extracting something resembling an effective low-energy theory from their string constructions. They end up with something that is very close to minimal gauge mediation (and signatures that have been studied for more than a decade), but it’s not at all clear that this is the only thing one could get from similar constructions.

    The one hint of high-scale physics that one could hope for in this scenario would be a measurement of the stau lifetime (which would require the sort of trapped particle Tommaso mentions), which would be an indication of the gravitino mass and hence of the fundamental scale of SUSY breaking. For Vafa and company, this is expected to be just below the intermediate scale, roughly as high as gauge mediation can possibly be pushed.

14. **Shantanu**
    November 13, 2009

    Peter , there have been some interesting conferences at PI

    http://www.pirsa.org/C09025
    http://www.pirsa.org/C09026
    anything interesing in this ?

15. **Theodor**
    November 13, 2009

    Something to smile about:

    A well-known physicist writes: (standard) “particle theory predicts supersymmetry”. (This statement is found on http://insti.physics.sunysb.edu/~siegel/vs.html)

    It shows how working 30 years in the same field distorts your perception of reality…

16. **Martin**
November 13, 2009

Hi Peter,

the article states that "Supersymmetry, which was first discovered in the context of string theory,…".
A question for the history of string theory buff:
Did string theory really give birth to supersymmetry?

Cheers,
Martin

17. **Peter Woit**
November 13, 2009

Martin,

No, that statement is not really true. 4d space-time supersymmetry of the kind relevant to the LHC was first discovered by Golfand-Likhtman (in 1970, published in 1971) and Akulov-Volkov (1971/72).

Independently, in 1971 Gervais and Sakita recognized a version of 2d world-sheet supersymmetry in the new fermionic string theory. This led to Wess and Zumino rediscovering 4d supersymmetry in 1973.

For more details, see

http://cerncourier.com/cws/article/cern/28388

18. **Tom Rizzo**
November 13, 2009

Hi,

To directly produce an essentially stable 200 GeV stau would require the 14 TeV LHC to run full out, i.e., 100 fb^-1. The production cross section for the pair production of heavy states by only electroweak processes is quite small. It is more likely that such an object might be found in the cascade decay of a strongly produced SUSY state. At 10 TeV, 100 pb^-1 barely lets you pass the LEP limit for such an object.

19. **Peter Woit**
November 13, 2009

Thanks Tom,

Looking at the Vafa-Heckman papers more carefully, I see that they are claiming a mass range of about 50-300 GeV, less tha 172 GeV if it's going to be the NLSP. I don't see a cross-section, but for the case where a neutralino is the NLSP, they quote 3 x 10^2 fb (presumably at 14TeV) for the production of the kind of strongly produced SUSY state you mention, with branching ratios "relatively large" to the neutralino.

So, OK, maybe not in the first few months…

20. **anonymous**
November 13, 2009

Look, it's obviously extremely irresponsible of anyone to claim that
string theory or specifically F theory predicts something so
specific at this point. That signal could suggest
gauge mediated SUSY breaking, consistent
with many UV completions even within
string theory. We'll be lucky if
SUSY itself is cleanly diagnosed very soon (as opposed to
other kinds of new physics). Gravity-mediated
SUSY breaking is a bit more UV sensitive, as is high
scale Gauge mediation. CMB data has a certain
amount of UV sensitivity. People should try to be responsible
about making accurate, measured (so to speak)
statements about these directions. It's not all or nothing;
the fact that there are not known smoking gun predictions
of F theory or string theory phenomenology does not mean
that we can't test specific scenarios and learn as
much as we can from the data
as well as from the structure of the theory.

21. **Andrew**
November 14, 2009

I'm sure if the LHC produces pink bunnies, string theory can be modified to explain it.

22. **Javier**
November 15, 2009

Hi Peter, some clarifications.

First of all, this F-theory is "landscape free". By this I mean that the requirement of gravity decoupling is enough to allow a very restricted number of choices that resemble the standard model, with concretes values of couplings and masses, unifying to SU(5). This is contrary to the landscape idea that, even after that decoupling (i.e. working in"local models" that intend to engnier the known physic) there are a very large number of different string vacua that result in a low energy effective lagrangian implementing the standard model, but with very different values of the coupling constants (and possibly other quantities) for each of them, which implies that string theory doesn't predict any value for them. This kind of landscape is different from that implied in the solution of the tiny cosmological constant. I must admit that I still don't understand all the details, and in particular I am not sure how the F-theory GUT's are expected to face the cc problem.

The second questions is that the claim of Vafa is that the peculiarities of his F-theory GUT's are so specific that it could be possible to distinguish them for the other similar SUSY models with gauge mediation SUSY breaking, precisely the contrary to what many commenters have said here. The actual paper (I am not sure if that was the one you were referring to in one of your comments) is :http://arxiv.org/abs/0903.3609

I am not the most qualified person to give a authoritative opinion about this topic,but as far as I understand it if the LHC fits the predictions of Vafa there is not too much window for other theories that string theory, in the F-theory in this specific limit to account for the known physic. If it gets falsified one would have to rely on other type II-B brane constructions, that have many restrictions to be unified theories so one would possibly have an standard model tht doesn't unify, or, more probably, natures is not described by type II -b string theory in weak or strong coupling. That is too much to say. Possibly mirror symmetry would also almost invalidate type IIa constructions. As a result possibly if this F-theory GUT is not verified that would mean that nature would be expected to realize some heterotic (M theory heterotic) string vacua. Said this It would be really fine if most informed people as Motl or Distler (or whoever) would comment on what I say.

23. **Gphillip**
November 22, 2009

    I'll give any and all takers 1000 to 1 odds that these particles will never be found. The reasoning is that if these massive charged particles take 1 minute or so to decay, we would be swimming in them from cosmic ray collisions with our atmosphere. We wouldn't even need the LHC. Just the detector pointed at the sky should work fine.

24. **Peter Woit**
November 22, 2009

    Gphillip,

    The number of cosmic rays with energy sufficient to produce interactions of higher energy than that studied at the Tevatron is non-zero but quite small. Since the probability of producing such a new particle is also very small, you're not going to see these things in cosmic rays.

    This problem is very general, and it's why you need accelerators like the LHC: you need to produce not just high energy collisions, but a lot of them, and you can't get this from cosmic rays.

25. **Gphillip**
November 24, 2009

    Perhaps I've misunderstood something here. At 10 ^13eV cosmic rays strike the atmosphere at a rate of a few per sq. meter per year. Many more at lower energies. There's about 197 million square miles on the surface of the earth. That's a very big non-zero number.

    I was just joking about pointing a detector at the sky, but if that many heavy charged particles are being generated, and take 1 minute to decay, I believe we would have (or at least could have) detected them by other means. But as I said, perhaps I've misunderstood something.

26. **Peter Woit**
November 24, 2009

    To get LHC energies in the center of mass you need cosmic rays of something like 10^17 ev. They're rare. And it's not like you can afford to instrument the entire earth's surface.

    Once you have one of these, the probability that it will produce this kind of new heavy particle is extremely low. Remember, at the LHC, you're talking about a few events/year out of a collision rate of 10^9/sec or some such.

    You can do the math, but there's no way you're going to get a non-negligible answer…

27. **Gphillip**
November 24, 2009

    Of course you are right. At or above 10^17 eV only about 10 cosmic rays per sq. kM strike the atmosphere per year. It's enough to eliminate "the LHC will devour the Earth" conjecture, but not enough to generate many particles if only a few are generated per year from 10 ^ 9 collisions per second. I suppose this eliminates the possibility of finding some stuck to my refigerator magnet. I stand corrected.

On the Defensive

November 13, 2009
Categories: This Week's Hype

There's another article here about Michael Green succeeding Hawking as Lucasian chair. It emphasizes the idea that this is all about more funding for string theory:

> MICHAEL Green, the 18th holder of Cambridge University's Lucasian Professorship of Mathematics, is clearly a man with weighty issues on his mind.
>
> He apologetically darts out of our meeting to speak to a colleague about how to submit the paperwork for a 1.5 million euros (£1.35 million) grant application he has just heard has been approved by the European Union.
>
> "I suppose it sounds like a lot of money," he explains, "but it's not that much really compared to the billions spent on some research. Our work is theoretical – we're very cheap."
>
> The money will go towards research on Michael's specialist subject, string theory…
>
> "I have been thinking about how I can make use of such a prominent position to benefit my colleagues. It is difficult to find funding at the moment, especially for subjects which don't obviously have an immediate application for something that will make money.
>
> "But the people who discovered magnetism and electricity had no idea what they could be used for. The MRI scanner wouldn't exist without particle physics. There are so many spin-off industrial investments in things that are being researched, and we need more of this."

Another blogger has the following comments about this:

> There's only so far that one can run away with this. People "…who discovered magnetism and electricity…" had, in their corner, empirical evidence to at least tell them if they are on the right path or not. This is where the analogy to pursuing String Theory breaks down and the similarity ends. I don't believe that there has been, in the history of physics, a study in a field of physics that has gone for so long, and garnered THIS much attention, that has been totally devoid of any empirical evidence which indicates one way or the other that it is on a right path. For many of us who value physics as being guided by empirical evidence, this is the most troubling aspect of String theory.

To be fair, Green notes that it's not all about cashing in for himself and his colleagues, that he would also like to finally have some success with the science:

> But, ever the academic, Michael's eyes twinkle as he admits his "pie in the sky" dream for his tenure of the Lucasian Professorship is not about money, but a breakthrough in the application of his beloved string theory.
>
> "We need something which at the moment doesn't seem to be a fundamental phenomenon," he explains. "To find something we know already, but find an undetected explanation out of string theory. It is a radically new theory; what it needs is a radical new prediction."

I'm not sure though that describing a nearly forty year old theory as "radically new" is really accurate. Any sort of prediction would be radically new.

Also in the business of defending string theory is Sean Carroll, who has a video and transcript up on the Edge web-site on the topic of "Why does the Universe look the way it does?". It's unclear to me what this has to do with the topic, but for some reason much of the talk is taken up with a defense of string theory. It's the usual misleading hype, at great length, leading up to a peculiar defense of the idea that even once you have shown that a speculative theoretical idea is vacuous and can give you anything that you want, you should keep studying it anyway:

> How do you show that a theory is not right if you can get anything from it? My answer to that is we just don't know yet. But that does not imply that we will never know.

From here it's on to the multiverse and his idea that it explains why you can't unscramble an egg, and that one is doing observational cosmology over breakfast:

> The reason we find a direction in time here in this room or in the kitchen when you scramble an egg or mix milk into coffee is not because we live in the physical vicinity of some important object, but because we live in the aftermath of some influential event, and that event is the Big Bang. The Big Bang set all of the clocks in the world. When we go down to how we evolve, why we are born and then die, and never in the opposite order, why we remember what happened yesterday and we don't remember what is going to happen tomorrow, all of these manifestations of the difference between the past and the future are all coming from the same source. That source is the low entropy of the Big Bang…
>
> I like to say that observational cosmology is the cheapest possible science to go into. Every time you put milk into your coffee and watch it mix and realize that you can't unmix that milk from your coffee, you are learning something profound about the Big Bang, about conditions in the very, very early universe. This is just a giant clue that the real universe has given to us to how the fundamental laws of physics work. We don't yet know how to put that clue to work. We don't know the answer to the who done it, who is the guilty party, why the universe is like that. But taking this question seriously is a huge step forward in trying to understand how the universe that we see around us directly fits into a much bigger picture.

Update: Carroll this week will be on a lecture tour in Australia giving talks on the Big Bang/egg unscrambling business. The first will be in Sydney where the "internationally-renowned theoretical physicist" will give the 2009 Templeton Lecture.

Comments

1. **GeofF**
   November 13, 2009

   I think the flushing of a toilet is a much better analogy than scrambling an egg.

2. **Tom O'Bulls**
   November 14, 2009

   Thanks for directing my attention to Sean Carroll's extremely interesting article. He really raises some very profound questions, and it's good to know that there are people thinking about genuinely interesting problems like the arrow of time, and not just about really boring things like [mumble].

3. **M**

November 14, 2009

GeofF, an even better analogy is: by flushing 1.5 million Euro down a toilet we learn about cosmology. Maybe it can get funded.

4. **piscator**
November 14, 2009

‘cashing in’, ‘all about more funding for string theory’? This is a parallel universe – do you know the state of funding for particle theory at this moment?

The UK government has thrown squillions at its banks and is in danger of losing its AAA credit rating. It also wants funding for all research (including even the humanities God help them) to be based on economic impact.

The idea that this environment is one of squidgy slush funds for string theory is so far from the truth it is comical.

5. **Peter Woit**
November 14, 2009

piscator,

I understand that the context of this is one of a difficult budget environment for science in the UK. Still, the article’s focus on the Lucasian chair as a vehicle for Green to raise money for his colleagues and his field is remarkable, if only as a sign of the times.

6. **John Rennie**
November 14, 2009

Re the low entropy at the Big Bang: if gravity were replusive then the state of maximum entropy would be an (approximately) uniformly distributed gas. Doesn’t LQG predict that gravity becomes repulsive as you wind back time towards the Big Bang? In that case isn’t it possible that the low initial entropy is due to gravity flipping from a repulsive to an attractive force?

JR

7. **Tim vB**
November 14, 2009

Here are two quotes from the article that I have some trouble making sense of:
“…with applications in countless different spheres.”
“The theory … has met with many successes over the years.”

Should’nt the first one be “with applications, maybe, sometime in the far future, if the theory succeeds”?

Reading the second one, I thought in this context a success should be something that is of interest to non-string-theorists. The only way I can make sense of this statements is that Green meant that “string theorists succeded in solving many problems that string theory had resp. created”, meaning that, from the point of view of a string theorists, there were many breakthroughs. I could concur with that, but I suspect that this not not what most reades will think.

P.S.: Green uses his position to fund string theory, this is hardly a surprise. Isn’t that what most people in similar situations do?

8. **Kea**
November 14, 2009

Oh, I’ll be in Melbourne for that Carroll talk … but I’ll be busy doing something too interesting to attend … like my washing, maybe.

9. **Marcus**
November 14, 2009

Oh, I’ll be in Melbourne for that Carroll talk … but I’ll be busy doing something too interesting to attend … like my washing, maybe.

LOL

And remember that doing your laundry is a giant clue as to to how the fundamental laws of physics work.

10. **Will Orrick**
November 14, 2009

The reason for the name “Templeton Lecture” is described at this University of Sydney website:

“In 1990, Charles Birch was awarded the Templeton Prize for progress in religion. He donated part of the proceeds of the prize to establish the annual Templeton Lecture at the University of Sydney, under the auspices of the Centre for the Human Aspects of Science and Technology. This generous gift has brought a succession of distinguished speakers to Sydney.”

11. **Peter Woit**
November 15, 2009

Thanks Will, I was wondering what that was about.

12. **chris**
November 16, 2009

JR,

on a cosmic scale gravity IS repulsive.

13. **John Rennie**
November 16, 2009

@chris: I assume you’re referring to dark energy, but this has only recently starred dominating and certainly wasn’t a factor near the Big Bang, or indeed during the formation of galaxies.

Of course there may eventually be a big rip. If so presumably the state of maximum entropy would again be a roughly uniform distribution. That's assuming the concept of entropy has any meaning in such a radically non-equilibrium system!

14. **Alejandro Rivero**
November 16, 2009

TInking about being a "nearly forty year old theory" … is there some plan to celebrate the anniversary. If we count from Susskind "violin string or organ pipe", we should prepare, as it was submitted in 11 July 1969 and published early 1970. If we count from Veneziano, we are already late. We could count from Ramond 1971, as it paves the way to D=10, susy etc.

15. **Thomas Larsson**
November 16, 2009

Veneziano's and Virasoro's advisor is no longer with us.

16. **Chris Austin**
November 20, 2009

Forgive me if this is a silly comment, but isn't the reason that we remember the past but not the future, connected with the fact that field equations in Minkowski signature have well defined solutions when boundary conditions are specified on just a single spacelike surface, i.e. a Cauchy surface? Then, if the boundary conditions on the Cauchy surface do not include non-random correlations between the initial values of the fields at distant points, the time direction away from the Cauchy surface is the direction of increasing time, and we are forced to use retarded potentials in solving, e.g., Maxwell's equations in the presence of classically moving charged particles.

If we wanted to use advanced potentials, or mixed advanced / retarded potentials, the boundary conditions on the Cauchy surface would have to include carefully set up incoming spherical waves for the moving charged particles to absorb, and these would have to be correlated with the future motions of the charged particles. Thus Minkowski signature field equations automatically create correlations away from the Cauchy surface, if there are no correlations on the Cauchy surface.

Furthermore, if the boundary conditions on the Cauchy surface are random, the state of a memory device at any time will in general be correlated with events in its past light cone, and in particular, with events on its past worldline, going back to the Cauchy surface, but cannot be correlated with events on its future worldline except under special conditions of shielding, since events on its future worldline will in general depend on the boundary conditions on the Cauchy surface outside its current past light cone, which have not yet had any impact on the memory device.

Thus the fact that we remember the past but not the future points to nothing more than that the boundary conditions of physical fields have been specified in the simplest possible way, namely randomly on a Cauchy surface, with no correlations.

The reason we can't unscramble an egg is different. First, we have to get the ordered state, i.e. the yolk separated from the albumen. Such locally highly nonrandom states can arise out of initially random states by a long series of ratchet-like processes such as evolution, or by forces that tend to separate the components of a mixture such as a colloid, due for example to differing densities, or high interface energies. Once we have the egg, it is easy to scramble it, because a wide variety of crude driving motions, such as shaking or stirring, can all effectively turn the initial ordered state into a disordered state, via chaotic motions. However the fact that it is difficult to unscramble an egg is a specific property of the yolk / albumen mixture, and tells us nothing at all about other systems. For example, if we "scramble" oil and water together, the mixture will soon separate, due to the differing densities of oil and water, and the relatively high surface energy at the oil / water interface.

A Brilliant Darkness

November 14, 2009
Categories: Book Reviews

Joao Magueijo has a new book out about Ettore Majorana, entitled A Brilliant Darkness. It's a lot of fun to read, and could be described as an example of Gonzo history of science. While it contains a lot of factual information, much of which I was unaware of, it's probably best to think of it like the works of Hunter S. Thompson. Not a good place to go for authoritatively accurate information about, e.g., Las Vegas or the 1972 US Presidential campaign, but a highly personal investigation that manages to get to the heart of the matter, finding emotional if not literal truth.

For some examples, here's Magueijo on Majorana's upbringing:

> Jokes and pranks aside, one should not get the impression that Ettore's youth was a happy one. It was dire. Between the priests and his parents, his basic humanity was destroyed. He was brought up by social outcasts and grew monstrously distorted, lacking social skills and independence, full of ineptitude. People like him — when they don't become criminals, drug addicts or psychopaths — *can't help* being intellectually superior. But they're "Frankensteins," artificially gifted, clever "against nature." And like the literary monster, behind the bestial genius lies a very different nature: tender in a way that can never be fully realized; longing for love, knowing full well that it will always be denied; a furnace of kind emotions that the ogre exterior will always screen.

and here's his account of his own trip on the ferry where Majorana presumably killed himself in 1938 at the age of 31:

> Back on deck, I realize what a gloomy figure I must cut: pensive and stark, staring at the sea. Maybe the insomniac brigade is worried I might be contemplating suicide. A girl comes out to smoke and waits to be chatted up. I move to the rear. What a sad bastard I must look, refusing to play the game of life, shouting and fucking, throwing up against the wind. I watch the wake for a long time, the cigarette butts flying past me into the night, like fireflies from Mars. In our world of the "normal", anyone who thinks is likely to appear suicidal. And yet, suicide or not, we will all be there one day, not just Ettore. We are all the same, only in different seasons.

Majorana was born in 1906 in Sicily, went to Rome for his studies. His career as a physicist basically spanned just the years 1928-1933, much of which was spent working in Fermi's famous group in Rome. For Magueijo, Fermi is one of the villains of the piece, with Majorana a genius much his superior. Unlike the rest of the group, Majorana wasn't interested in experimental work, nor much interested in publishing his ideas, about which Magueijo claims:

> That's how he never got credit for Heisenberg's theory of nuclear forces and the neutron, the Weisskopf-Pauli second quantization of the complex scalar field, or the parity-violating properties of the neutrino, which earned Tsung Dao Lee and Chen Ning Yang the Nobel Prize some thirty years later. They could all have been named after Majorana. But because he never published his work, only the Majorana neutrino — his inseparable soul mate — carries Ettore's name today.

In 1933 Majorana traveled to Leipzig to work with Heisenberg, then to Copenhagen to work with Bohr. When he returned to Rome, some combination of physical and mental health problems led him to become a recluse for several years. He emerged from this state in 1937 to take up a professorship in Naples, but a few months later disappeared after embarking on a ferry taking him from Palermo back to Naples.

Majorana's most important scientific work appeared in a 1932 Nuovo Cimento paper motivated by the desire to find a replacement for the Dirac equation that would solve the problem of its negative energy states (a problem which disappeared in 1932 with the discovery of the positron). In this paper, Majorana investigated for the first time infinite dimensional representations of the Lorentz group, ones whose role in physics, if any, remains mysterious. As part of this work, he discovered the possibility of a real representation of the Clifford algebra and thus a version of the Dirac equation in which a particle is its own anti-particle. Whether this possibility is realized in the case of neutrinos is one of the big open questions of the subject. We know that there must be neutrino mass terms, but we don't know if they're of Majorana or Dirac form.

Magueijo does a good job of describing this important physics at a popular level. He also gives a lot of space to the various myths that have grown up around the story of Majorana's disappearance. There's a whole subculture out there devoted to them. He wisely decides not to sign on to any of these or create his own, concluding:

> And as with the neutrino, Ettore's story is also elusive. Even if we found out for sure what actually happened to him, we'd never know *why* he did it — which is far more important. This absence of a final truth shouldn't sadden us: At leas we don't harbor delusions of omniscience. When I got on that plane to Sicily, I promised myself only this: I won't raise my leg and urinate over my little territory in Ettoreland; I won't invent a solution that is not needed.

Comments

1. **D R Lunsford**
   November 14, 2009

   Now THIS sounds like a good read!!

   BTW it's not quite right to say the problem of the negative energy states was alleviated by the appearance of the positron. Dirac for example kept working at "getting an equation" (as he would say it) with purely positive energy right up into the 70s. See the papers "A Positive Energy Relativistic Wave Equation" from about 1972 or so.

   ok here http://www.jstor.org/pss/77762

   a fascinating work.

   -drl

2. **einblicke**
   November 14, 2009

   I was greatly inspired by the works of infinite dimensional representation of the Lorentz group by Majorana, since the mathematical theorm says that the finite dimensional representation of a non-compact group (like the Lorentz group) is non-unitary, which violate the spirite of quantum mechanics, and I once thought that it may be the key obstacle for our reconciling the quantum mechanics and the relativity. However, an infinite dimensional representation of the Lorentz group is unitary, which I once thought as a possible path, but little progress.

   Today's quantum fields theory of reconciling the quantum mechanics and the relativity is going another way in which the particle number but rather the inner product can be negetive and be interpreted as anti-particle.

3. **mike**
   November 15, 2009

Will the LHC be able to tell the difference between neutrinos of the Majorana and Dirac form? Will will we be able to get an accurate mass reading from one of the detectors?

4. **Yatima**
November 15, 2009

While we are on the memory lane:

http://www.independent.co.uk/news/obituaries/vitaly-ginzburg-nobel-prizewinning-physicist-who-helped-the-soviet-union-develop-the-hydrogen-bomb-1820387.html

"Ginzburg was a pioneering theoretical physicist who often deprecated his own abilities in mathematics, yet made seminal contributions in a number of areas of physics, including quantum theory, astrophysics and radio-astronomy. Following years of nominations, he won the 2003 Nobel Physics Prize for developing the theory behind superconductors; materials which allow electricity to pass without resistance at very low temperatures. His work had been done with his fellow physicist Lev Landau, but he had died in 1968 and awards were not given posthumously. He shared the prize with the British-American Anthony Leggett and the Russian-born US scientist Alexei Abrikosov."

5. **Peter Woit**
November 15, 2009

mike,

The LHC is not useful for studying neutrino masses, they're quite small and you don't need LHC energy to produce neutrinos. There's a wide range of non-collider experiments going on to study neutrinos, including some that in principle can distinguish between Marjorana and Dirac.

6. **Stephen**
November 15, 2009

I strongly recommend this classic book on Majorana:
"The Moro Affair and The Mystery of Majorana" by Leonardo Sciascia.

From the book, among other things, apart from the suicide hypothesis, there appear to be some other explanations: for instance the fact that he may have started secretly religious life, or that he may have escaped to argentina. I like to think that the alternative hypothesis to the suicide are true.

7. **Cormac O Raifeartaigh**
November 17, 2009

I enjoyed Jao's first book, Faster Than the Speed of Light. I suspect a lot of reviewers missed the fact that he is a very good science writer because they doubted his premise. He has a lovely, clear, but chatty style…

8. **Kay zum Felde**
November 18, 2009

I find it interesting how Magueijo conclude, that Majorana must become a superior physicist because of his hard youth.

I myself am a physicist too and suffer from a schizoaffective psychosis and when the illness is on its top, I've often good ideas as pictures in my mind, that don't work, because in such a state my mathematical mind does not work correctly. Afterwards I usually cannot remember my ideas. This is not that problematic since as I implied they wouldn't work correctly. What I wanted to say too, I can imagine the lonelyness of Majorana, especially the longing for love, even it cannot delivered.

Best Kay

9. **jun**
November 19, 2009

I too liked Joao's first book. I remember getting pretty excited about VSL. I was a bit surprised when a well-known string theorist disparaged his ideas.

10. **tomate**
November 21, 2009

The short novel by Leonardo Sciascia that Stephen cites is a great pieace of italian literature devoted to alternative inquire into the subject, so I wouldn't call it "subculture". The main point of Sciascia (maybe a bit too fictionary, but deep and accurate) is that Majorana could have disappeard to subtract himself from the hands of the fascist regime, since he problably foresaw the strategic power of the nucleus that was not yet totally unravelled by Fermi himself. In fact Mussolini had explicit orders to find him alive, when Fermi brought his experimental knowledge in the US, being his wife jew. History could have taken a quite different turn…

11. **tomate**
November 22, 2009

…and I point out this new article that appeared in the arXiv, for the italian readers:

http://arxiv.org/abs/0911.0933

The author is credited for being the most prominent expert about Majorana's legacy; the materials Leonardo Sciascia worked on come from himself. It would be interesting to know if Magueijo got in contact with this guy for his own work…

LHC Update

November 19, 2009
Categories: Experimental HEP News

Yesterday the LHC Hardware Commissioning Coordination Team announced the end of the 2009 Hardware Commissioning Campaign as all 8 LHC sectors were declared commissioned and ready for beam. A two day checkout period is now underway, which should have the LHC ready for beam at 17:00 Friday. Friday evening and night should see beams threaded around the machine in both directions. Saturday the plan is to capture a circulating beam in one direction, Sunday in the other direction. Celebratory drinks are scheduled for 17:00 Monday in the CERN Control Center.

Update: Up-to-date news about beam commissioning is here, and hopefully CERN won't shut off outside access to it. Normally I try and avoid providing links that might be in danger of becoming non-public, but in this case, since the New York Times is linking to this, I suppose I should too.

Update: The LHC Portal is a site with a lot of links to CERN information. For more about the site, see here.

Update: Beam is in the LHC and has made it part way around, to IP3. One place to follow progress is here.

Just as I finished writing that, I see it's now at IP5.

Update: After a short stop to recover from a magnet quench, the beam has now gone all the way around the ring, making two turns.

Comments

1. **T.**
   November 20, 2009

   Nooooooo way, apparently another magnet has quenched..?!

2. **Peter Woit**
   November 20, 2009

   T.,

   Magnet quenches are fairly common. The problem last year was the failure of the system that protects the machine when they happen.

3. **katanakun**
   November 20, 2009

   follow the LHC beam circulation on http://twitter.com/cern

   LHC is back 🙂 🙂 🙂

4. **T.**
   November 20, 2009

   Thx Peter. well this protection system now appears to be doing its job properly -at the very least it didn't fail immediately, this would have been death by ridicule.

5. **Chris Stephens**
   November 22, 2009

   Cool mention of my site. I hope you guys enjoy it. If you want to keep track of daily happenings the best spot is http://cmsdoc.cern.ch/cms/performance/FirstBeam/cms-e-commentary09.htm which is on my site under CMS and Live Log

Higgs Escapes Part of Exclusion Region

November 19, 2009
Categories: Experimental HEP News

This past winter a combined analysis of data from the two Tevatron experiments showed at 95% confidence level that the Higgs mass could not be in the range 160-170 GeV. This was a better result than expected: statistically the experiments should not have been able to exclude any of the mass range, but were helped by a downward statistical fluctuation.

Today a new and improved combined analysis was released using more data, and the new result is that there has been a reversion to the mean, no more help from statistical fluctuation downwards. Statistically, this time they should have been able to exclude 159-168 GeV, but now the fluctuation is a bit upwards, so the actual exclusion region is 163-166 GeV. In essence, better data has shown that the likelihood of a 160-163 or 166-170 GeV Higgs, something that was previously assigned a probability of a bit less than 5%, now has a probability a bit more than 5%. So, any putative Higgs particle in those mass regions has now escaped being tarred with the unfair label of "excluded".

If the Higgs is actually there at a certain mass, as one gets closer and closer to having sufficient data to exclude its existence, one should find oneself doing nowhere near as well as expected as far as excluding that mass. A thoroughly irresponsible person might see some significance in the fact that, unlike the analysis from earlier this year, the new improved analysis with more data does a worse job of exclusion than expected over much of the low mass range, peaking at 1.5 sigma or so for the mass range around 135 GeV.

Update: More detail and rank speculation about this from Tommaso Dorigo here.

Comments

1. **Peter Lee**
   November 19, 2009

   Just wondering if this means that Alain Connes' Non-Commutative Geometry theory predicting a Higgs mass of around 170 GeV might still work out?
   I enclose a copy of his posting in August 2008 when an earlier result excluding a Higgs mass of 170 GeV came out.

   IRONY

   In a rather ironical manner the first Higgs mass that is now excluded by the Tevatron latest results is precisely 170 GeV, namely the one that was favored in the NCG interpretation of the Standard Model, from the unification of the quartic Higgs self-coupling with the other gauge couplings and making the "big desert" hypothesis, which assumes that there is no new physics (besides the neutrino mixing) up to the unification scale. My first reaction is of course a profound discouragement, mixed with an enhanced curiosity about what new physics will be discovered at the LHC.
   I'll end with these verses of Lucretius:
   Suave, mari magno turbantibus aequora ventis,
   e terra magnum alterius spectare laborem;
   non quia vexari quemquamst jucunda voluptas,
   sed quibus ipse malis careas quia cernere suave est.
   —————————————————
   [Pleasant it is, when over a great sea the winds trouble the waters, to gaze from shore upon another's tribulation: not because any man's troubles are a delectable joy, but because to perceive from what ills you are free yourself is pleasant.]
   Posted by AC at 3:57 PM 20 comments

   Monday, August 4, 2008

2. **piscator**
   November 20, 2009

   maybe the thoroughly irresponsible person would see 1.5 sigma worth of significance? 😉

3. **Martin**
   November 20, 2009

   Hi Peter,

   I have heard of particles with a signal at the 5 sigma significance level disappearing into noise.
   How often in your experience does it happen that a particle is found in its (e.g. 2 sigma) exclusion zone?

   Cheers,
   Martin

4. **Peter Woit**
   November 20, 2009

   Peter Lee,

   This just improves the bad odds a bit for the 170 GeV prediction of Connes from 5% to something a bit bigger.

   Martin,

   There aren't many examples I know of of this kind, so hard to say. In principle, one time in 20 this should happen. There's also the phenomenon to keep in mind that when experimentalists err, it generally is on the side of thinking their results are more accurate than they really are…

5. **A quantum diaries survivor**
   November 21, 2009

   Hi Peter,
   thanks for the irresponsible link 😉

I think that the 1.5 sigma at 115 GeV are a way more interesting thing than the narrow exclusion, because of the electroweak fits and because of the LEP II excess.
Also, a 160 gev Higgs will be seen or wiped off the board in a few months by LHC, so the 115-140 gev region retains less ephemeral value.
Cheers,
T.

First Collisions at the LHC

November 23, 2009
Categories: Experimental HEP News

Things evidently went extremely well over the weekend at the LHC, with simultaneous circulating beams achieved this morning. Speculation is that first collisions (at the injection energy of 450 GeV/beam) are imminent. Places for up to the minute information include here, here and here.

Update: It looks like first collisions have been seen at the LHC. Announcement comes from a muzzled blogger….

Update: Modified posting title.

Update: For a series of talks about events during the first few days since beam injection at the LHC, see here. Progress was dramatic during the first few days, although it has slowed up recently. As data starts to come in, the first scientific task for the experiments is to re-discover the Standard Model. So far, CMS has managed to rediscover the pi-zero.

Comments

1. **Garrett**
   November 23, 2009

   The CMS e-commentary site just splashed a collision image, then pulled it, then put it back. Probably good to mirror that one…
   http://sifter.org/~aglisi/albums/LHCfirstcollisions.png

2. **Marcus**
   November 23, 2009

   Here is the official site's picture.
   http://cmsdoc.cern.ch/cms/performance/FirstBeam/pictures221109/CollisionEvent.png
   Good you have it mirrored.

3. **Yatima**
   November 23, 2009

   Why are they so secretive? We know they are just playing around; this not some sort of political kabuki where everybody has to protect cherished anatomical parts before the poll results are in … or is it?

4. **Marcus**
   November 23, 2009

   Yatima, I laughed out loud reading your post
   Why are they so secretive? We know they are just playing around; this not some sort of political kabuki where everybody has to protect cherished anatomical parts before the poll results are in … or is it?

   I like the turns of phrase. But I have to say that from my perspective I think CERN is now being admirably open. They are being a lot better than they could be. There is an impressive amount of web access. And they also (so far as I know) have been allowing Chris Stevens' unofficial "LHC Portal" website which assembles a huge amount of stuff.

   I think there's been some improvement and they've gotten more enlightened—this openness will, I think, benefit CERN and science generally (assuming it continues.) The public relations benefit of allowing a kind of fly-on-wall participation via web will greatly outweigh any possible temporary embarrassments.

   But obviously my sense of proportion about this is very different from yours.

5. **Ralph**
   November 24, 2009

   They ramped a beam up to 560 GeV also. I guess it will take a few attempts to get it ramping smoothly to over a TeV.

6. **Coin**
   November 24, 2009

   The USLHC blog has some more pictures, which appear to be official releases (?), of what they call "candidate" collision events.

   What does "candidate" mean in this context?

   Trying to make sure I understand: Normally the LHC would use two opposing particle beams, and "collisions" would be between the particles of the two beams. In this case they were only testing 1 beam, however because the vacuum in the detector chamber is necessarily imperfect the beam would have collided with like a marauding atom or something to produce the "candidate" collision. Is this right?

7. **Peter Woit**
   November 24, 2009

   Coin,

   These "candidate" collisions are real collisions, the experiments are just being overly cautious about what they call something that has not yet been carefully analyzed.

   They have two beams in the machine, and got them to cross at the correct interaction points inside the detectors. These are now real colliding beams of the sort the machine is designed to produce, only problem is that the energy/beam is 450 GeV, not 3.5 TeV, and the luminosity is something absurdly low. They have made a huge amount of progress very quickly, this was supposed to take much longer.

   Now all they need to do is to ramp the beam energy up to 3.5 TeV, and work on getting a usable luminosity (a lot more collisions). The machine's quench protection system is only commissioned up to 1.2 TeV/beam, so that should be as high as they can go before Christmas. After Christmas, they'll first have to work on commissioning to 3.5 TeV, then ramping up the beam energy.

8. **Coin**
   November 24, 2009

   Thanks for clarifying.

9. **Mitch Miller**
   November 25, 2009

   Another general question, when they get the energy and luminosity up, do they just sweep the whole energy range and collect data? From what I have read, it seems like it takes an incredible amount of data at to show an unobserved particle exists, so maybe they guess energies and focus on the ones where the not yet signifigant data looks most interesting?

10. **Peter Woit**
    November 25, 2009

    Mitch,

    In these experiments the collision energy is fixed. They will basically always run at the highest beam energy the machine can achieve. There's no structure in the proton-proton cross-section at these energies like a resonance that would make it desirable to run at a specific energy.

    The products of a collision are very complicated, and the search for new particles is all about trying to find evidence for it amongst these collision products. You want as a high a collision energy as you can get, because that will allow you to produce the highest possible mass particles, in the highest numbers.

11. **Mitch Miller**
    November 25, 2009

    Thanks for the answer Peter.

12. **Paul Wells**
    November 25, 2009

    Mitch,

    In electron-positron collisions where (to current knowledge) the particles have no sub-structure the energies ARE tuned e.g. at LEP to Z0 resonance.

    In proton-proton collisions the energy is already smeared out over the quarks and gluons so there is no need to tune.

Short Items

November 27, 2009
Categories: Uncategorized

- To get an idea of what's going on at CERN not at the LHC, but at the theoretical end of things, take a look at the presentations at the recent CERN-TH retreat.
- I was worried that this blog marked the end of the distinguished series of publications of W. C. Gall. Fortunately, I see that there is now more.
- A year ago I attended a talk at NYU by IAS director Peter Goddard on the early history of the IHES and how it was inspired by the Princeton Institute. Cormac O'Raifertaigh reports here on a recent talk by Goddard at the Dublin Institute of Advanced Studies, about how it too grew out of a similar inspiration.
- Via Jordan Ellenberg, there's news of a claimed proof of Leopoldt's conjecture, with details available at a blog entry by Minhyong Kim at the new London Number Theory blog.
- Among courses this semester the world over I wish I could attend, one would be Eckhard Meinrenken's on Lie Groups and Clifford algebras. Luckily he's producing lecture notes, updating those from a previous version of the course.
- Next Tuesday the Science Channel will continue it's great tradition of programming about fundamental physics with the premiere of a new show called Sci-Fi Science featuring Michio Kaku. The first evening's episodes will explain

> a loophole in Einstein's theory of relativity that shows how a spacecraft could travel at warp speed.

followed by

> Dr. Kaku is on a mission to design a gateway to a parallel universe – but which type should he visit? MIT cosmologist Alan Guth explains his recipe for creating your own universe in the lab, and physicist Neil Turok explains how a parallel universe is only an atom's length away from us.

To their credit, sometimes they do actually have some real practical science which is not science fiction: yesterday they had Frank Wilczek on this show.

Update: Lubos has more Kaku.

Comments

1. **DavidZS**
   November 27, 2009

   How the hell does Kaku get ANY respect ?
   The guy's clearly on LSD.
   How is it these people still got their positions at Uni's?
   Science is dead and these guys are kicking down it's tomb stone

2. **Cormac O Raifeartaigh**
   November 28, 2009

   Funny thing is, I really enjoyed Kaku's book on Einstein. I was stuck in a hospital for 8 hours once waiting to see a doctor, read the thing several times, really enjoyed it.

   Thanks for the plug for the blog post on Goddard lecture Peter, it was a really interesting talk. Actually, one point the speaker didn't touch on is the downside of the institutes. One problem the Dublin IAS suffers has is size – it's so tiny there is no admin support whatsoever for organising conferences etc, so the professors do absolutely everything themselves. Also, the Institute does not advertise its successes at all well for the same reason- it is much better known outside Ireland than inside. For example, Lochlainn's work is almost completely unknown in his own country – the last 3 collections of Irish scientists ignored him completely.

3. **neo**
   November 28, 2009

   We are all agreed that Kaku's ideas are crazy, but are they crazy enough? To quote a famous man.

   OT–Kaku may be crazy, but his geography is better than that of Lubos. The latitude of England is indeed northern Canada (James Bay), not southern Canada as Lubos claims.

One Reason Science is Having Trouble Banishing Religious Thinking

November 29, 2009
Categories: Uncategorized

Andrew Sullivan, under the title String Theory and Miracles quotes part of a blog posting entitled One reason science is having trouble banishing religious thinking at the Democracy in America site (the original posting text is not there right now, may reappear) which notes that the spectacle of physicists widely promoting to the public the string theory multiverse is having the following effect:

> It's not always readily apparent to non-physicists why this kind of talk is less supernatural than a belief in the persistence of the soul after death….
>
> But strictly in terms of how the argument between theists and atheists plays out in the public domain, there is a different quality to the tenets that are emerging on the atheistic, particle-physics side of things these days.

The string theory multiverse pseudo-science has done a huge amount of damage to the interests of string theory within the academic community, but it also threatens to do damage to the understanding and image of science among the public. Unfortunately, while there is more and more physics content in US popular media, it is often in the form of string theory-based pseudo-scientific nonsense rather than real science. For examples of this, see a new article in the Denver Post which catalogs some of this (while arguing that it's a good thing):

> TV is working through the shock of the age of terrorism and dismay at the broken boundaries of science, right before our eyes. Parallel universes? Bending time? Alternate dimensions? Some heavy-duty thoughts are seeping into prime time every week.
>
> To the extent that TV reflects the culture at large, these shows seem to be saying we're on the cusp of major change — technological, scientific, political or emotional. We may not have answers but we're aware of expanding questions. In 2009, it has become accepted for folks on the couch to converse about the space-time continuum. Not that we understand string theory, but we recognize it when it pops up in TV scripts, peppering a spy thriller. "Lost" pushed the way with its dialogue about "moving the island," leading fans to discuss time-shifting, wormholes and Einstein's relativity theory.
>
> Really.
>
> The newer shows are picking up the string (theory) and running with it. There are hopeful signs in all this. The sci-fi series depict humans taking control of the planet, voting in favor of free will and standing up for the species. Maybe TV can provide some wishful thinking.

Comments

1. **Bob Levine**
   November 29, 2009

   Whether or not, as H.G. Wells observed, a frightful queerness has come into life in general, it certainly seems to have come into scholarship and science. Once upon a time, it seemed that the humanities were pretty much lost for good, drowning in the incoherence and irrationality enthusiastically sponsored by postmodern crit-babble, the kind that was mercilessly dragged into the light by Alan Sokal in his famous expose of SOCIAL TEXT. But there was always the forward march of science to console yourself with… and now it turns out that that what purports to be the leading edge of fundamental physics is aggressively hyping 'fashionable nonsense', as Sokal's 1997 book terms it—nonsense which is every bit as nonsensical as any of the deconstructionist gibberish that Sokal eviscerated more than a decade ago.

   It's implicit (and sometimes explicit) in Peter's work, Lee Smolin's work and that of other observers of the frightful queerness that is contemporary theoretical physics that this is the sort of trap science can fall into when it doesn't have real data to bang its head against and make sense of, and maybe that's enough to explain it. But the side-by-side train wrecks of scientific and literary/cultural theory suggests something more, some kind of intellectual nihilism that I find very, very depressing.

2. **Tim vB**
   November 30, 2009

   I don't think it is scary if the general public grossly misunderstands some concepts from theoretical physics.
   Think about Star Treck, since the invention of quantum mechanics and the multi-world interpretation there have
   always been fantasies about travelling to a parallel universe. It is slightly annoying if someone at a cocktail party tries to explain the uncertainty principle to you because that special someone was enlighted by a Pherry Rhodan comic strip the he or she read in the morning paper. But neither scary or depressing.
   You know what really scares me? A well known and established professor of theoretical physics publishes a scientific paper, claiming that an extra term in the Lagrangian in the path integral of the universe could explain why the LHC broke, and, after recovering from a fit of laughter I find that there are many intelligent people out there who did take this seriously (not believing it was true, of course, but believing that it was not meant as a joke).
   You know what I'm talking about.

3. **Peter Woit**
   November 30, 2009

   Tim vB,

   I think the danger now is not that the public misunderstands what some prominent theorists are up to, but that they do understand it.

4. **Mark Wallace**
   November 30, 2009

   It is hard to stop all the silly speculation, because lack of evidence does not seem to discourage TV popularization or sci-fi plots. Of course there are a lot of ragged edges around quantum mechanics that really need to be worked on, but string theory does not appear to have made much progress in the quarter century or so since I first had it explained to me.

   However, I do think there is merit to applying the same ways of thinking that have been effective for analyzing molecules to larger systems. This leads to a lot of unusual and nonsensical suggestions, but few people have the intuition to avoid the nonsense. It is not clear (to me, at least) that we fully understand why tools like Schrodinger's equation have been so successful predicting chemical and electrical behavior. String theory may have been a distraction, but (as an engineer) my perception is that our understanding of quantum theory and it implications for larger systems is still at a very primitive stage of development. The viewing public is not really naive — they sense the confusion, and are drawn to it just like kids chasing a fire engine.

5. **Anonymous**

December 1, 2009

For most folks, even, say, a professional biochemist, the only basis for accepting the claims of high energy physicists is the authority mechanism – the claims are made by people with nice titles at respectable institutions, and there is some evidence (e.g. transistors) that claims made by similar people have translated into something tangible which would not have existed without those people. The professional biochemist, by projecting her own professional experience, is perhaps better situated than the `man on the street' to judge which authorities should be trusted, but in the end it is a matter of picking one's authority.

For many people, the people with fancy titles in elitist expensive institutions are inherently untrustworthy authorities – are not these essentially the same people who run the banks? – and something like the bible, or the minister expounding it, seems more accessible and more believable. Black holes can't be seen with the naked eye anymore than can gods.

I certainly do think there is a basis for accepting certain authorities over others (transistors, h-bombs, genome sequencing, etc.. all attest to the superiority of certain paradigms), but I think also that the scientific community ignores the need to explain to world at large why its authorities should be considered credible, and dismisses too easily the credibility the world at large gives other authorities. Why should a man who can't add fractions (most folks) listen to some nonsense about colored quarks?

You are absolutely right that quasi-religious pseudoscientific multiverses live in the same intellectual multiverse as do things like intelligent design, and they make far more difficult the task of communicating why scientific authorities should be trusted.

6. **Gphillip**
December 1, 2009

I love science fiction. It's great entertainment and I see nothing wrong with it at all. I'm even willing to accept possible conclusions from science and math that can't be tested with current technology. After all, when Einstein's equations predicted the possibility of a gravitational collapse, he did not believe it really would exist in nature, much less someday be observed and studied from Earth. Even still, Einstein's theories were based on sound observations about time and light, space and gravity. Then those theories were peer reviewed and verified by additional observations, many of which he proposed himself. This is the difference between science fiction and the scientific method. In my own humble opinion, a scientist should not propose a conjecture as fact without it being based on sound observation and without proposing a way that it could be falsified or verified.

This is the line between science fiction and the scientific method. When scientists cross that line, they may as well describe the universe the way my elders once did, by saying, "that's just the way God made it". Just to be clear, I have no problem with anyone who wishes to explain the cosmos that way, but it's not science. Just as a particle can be both a wave and a solid bit of matter at the same time, I can allow for multiple explanations for the same thing.

My concern is that true science is not based on any sort of faith. It must be based on observations and include tests that can prove or falsify its predictions. This is an easy line to draw and many of today's so-called scientists have morphed into nothing more than evangelists or science fiction writers. When Author C. Clark first proposed satellites, there was no science to back it up. It was a great idea, years before it's time, but it was science fiction. If scientists want to write science fiction, that's fine. Carl Sagan did a great job with "Contact". But for goodness sakes, let's not confuse a belief by faith, or science fiction, with the scientific method. If we do, science will lose all contact with reality and all those that call science nothing more than a popular paradigm will be justified in their accusation. In the end, scientists have a professional responsibility to separate science from religion and science fact from science fiction. I'm very disappointed in those scientists who fail in this professional responsibility.

7. **Bob Levine**
December 2, 2009

"In the end, scientists have a professional responsibility to separate science from religion and science fact from science fiction. I'm very disappointed in those scientists who fail in this professional responsibility."

I think it's a bit trickier than that.

If you ask the physicists who go on about the multiverse and contact between alternate universes and so on about what they're doing, they would probably tell you that they're not doing science fiction, but exploring possibilities that are already within the scope of what they regard as 'normal science'. The problem is that what's regarded as normal science has changes significantly in the last thirty years or so. Once upon a time, normal science meant systematically extending the reach of one's best hypotheses and analytic frameworks, those which had proven most sucessful to date, to novel problems suggested by new data, or an innovative take on old, previously unresolved problems, to yield a predictively satisfying result. You departed from this MO only when you weren't getting anywhere and needed a *really* crazy alternative to standard models and concepts to make a breakthrough, and you didn't regard that alternative as established until it had proven itself through further application. We all know from experience that most ideas that are just too god to be false wind up in the dustbin sooner or later. And the more drastically such ideas depart from the previous body of 'battle-tested' knowledge, the more likely they are to be discarded. You can count genuine paradigm shifts in physics on a proper subset of the fingers of one hand.

Something has changed that conception of normal science. The lack of new critical data allowing a push beyond the standard model—we're stil sitting on tenterhooks waiting to see what the fate of the Higgs is!—combined with the incredible pressure, in the wake of electroweak unification, to finally finish the bloody job, cook up a successful TOE, and go home—seems to have led physicists to redefine normal science as a glass bead game with only the most indirect connection to any actual *facts*. Once your scientific culture changes in that direction, conflating thought experiments with real experiments, and possible worlds with the real world starts to become 'normal', and we get to… well, to where we are in the OP here.

And it's not helped, of course, by the fact that some people feel that the right way to address string theory's 10^500 vacuum states is to assert, completely seriously, that that outcome has ontological consequences which we must accept. Get all these factors interacting for long enough and the whole view of science that took us all these centuries to build up is in jeopardy…

8. **Gphillip**
December 2, 2009

BL, generally I believe we are in agreement. The question is what is science? What separates the fantastic thought experiments of Einstein from Star Trek fiction? What separates scientific fact from a belief through faith? Perhaps the question is even deeper as you suggest. If science hits a brick wall, is it acceptable to extend its reach with thought experiments that have no observational basis and mathematics that are the equivalent of proving that 0/0=every number?

In my own humble opinion, science is a profession. The pursuit of that profession is guided by the scientific method. Those who claim to be pursuing science, but do not use the scientific method are quacks. They may be well educated and well meaning quacks, but they are quacks nonetheless. I'm not saying that valuable insight cannot be gained by other methods, but rather that those methods are not science and anyone who claims to be doing science by other methods has lost their way along the path to scientific progress.

While I understand there is a great push to complete the job of unifying the fundamental forces, I do not agree that moving outside the scientific method is the correct course. It wouldn't surprise me if it takes a very long time to collect the experimental data to complete this work. It could even be possible that the solution is many orders of magnitude more complex than anyone envisions, or that we may not be able to complete this work for many generations to come. In any case, the unification of the fundamental forces is not the only ongoing scientific work in physics. I do not believe that these attempts to go outside the scientific method are justified when considered in the balance of all the harm that fundamental science in all fields will suffer if scientists stray from the path of the scientific method.

That is of course, my own opinion. Others my feel it is justified to scrap the scientific method if there is any hope at all of solving this vexing problem. I don't begrudge them their attempt at solving the problem, I only object to their calling themselves scientists. I really don't know what to call them. Aspiring profits? Pseudo scientific evangelists? I don't mean any disrespect by calling them quacks, but it's the only term I know that applies to would-be scientists who have abandoned the scientific method.

If, in the end, the only solution to this problem depends on the assumption of something that can never be observed, then in my opinion it can be said that there is no scientific solution to the problem. There may be many problems that have no scientific answer, but that doesn't justify throwing out the scientific method, since there are so many more problems that the scientific method can solve.

But I do not believe we should be willing to accept defeat this early in the game. I firmly believe that if all the time and money expended on trying to come up with pseudo scientific answers to the problem was dedicated to pursuing answers through the fundamental scientific method, the correct answer , if there is one, will be found much faster and with much less damage to other branches of real science. Perhaps we just need to be a little more patient and wait for the results to come out of the LHC over the next few years. Perhaps there is another experiment either here on earth or out in space that will point the way. But in any case, let's not accept throwing out the method that gave us the transistor and the end of small pox, just to pursue a very remote possibility of advancing a few individuals careers.

It is said that we stand on the shoulders of scientific giants. So just how did they become giants? In every case it was through the rigorous application of the scientific method. I can't think of a single scientific "giant" that became so by proposing solutions that can never be observed. Perhaps I need to adjust my thinking though and accept the existence of new pseudo scientific media and internet giants. I can accept that for entertainment purposes, as long as they quit claiming to be performing real science.

9. **Bob Levine**
December 2, 2009

@GP:

"I'm not saying that valuable insight cannot be gained by other methods, but rather that those methods are not science and anyone who claims to be doing science by other methods has lost their way along the path to scientific progress…While I understand there is a great push to complete the job of unifying the fundamental forces, I do not agree that moving outside the scientific method is the correct course…. In any case, the unification of the fundamental forces is not the only ongoing scientific work in physics."

Right, and that's the crucial nub of the issue. What you're saying here is an affirmation of the conduct of "normal science", as it used to be understood, as the default strategy for obtaining hard, defensible conclusions—what used to be called *results*. But in high-energy physics, there is now quite a different ethic in place, where mathematical validations of highly abstract conjectures are seen as normal—regarded as just the latest avatar of what Dirac did with Clifford algebras way back when, and are themselves claimed to be results with *physical* content. Even more central is what you say about unification not being the only ongoing work (of importance) in science. Again, normal scientific practice in the past didn't aim directly at such unification, which was rather an earned result based on patient accumulation of confirmations for progressively more general characterizations of physical phenomena. Plenty of impressive reputations were earned on the basis of normal science, and not a few Nobel Prizes (Hans Bethe comes to mind as a good case in point).

But that's not the way things are seen within a sizable chunk of the professional physics community, apparently. A cash-strapped field with far more Ph.D.s than permanent (or even impermanent) jobs for them, a generation-old theoretical framework that no one seems to be able to take the next step from, operating in a general zeitgeist driven as much by celebrity as genuine earned authority, is almost certainly going to try to solve its problems by changing the rules of the game to play in the way that we're talking about. In the case of Kaku & Co., the counter would almost certainly be, whatever increased people's interest in science is good, and as you mentioned in the other thread, the program is clearly labelled 'Sci-fi'. What worries me is that people may not recognize that not only the concepts (wormholes to other universes, time travel, etc.) but the 'physics' applied to those concepts on the program, are part of the 'fiction' in the 'Sci-fi' label….

LHC at the High Energy Frontier

November 29, 2009
Categories: Experimental HEP News

A few minutes ago, one of the beams of the LHC was ramped up to an energy of 1180 GeV, besting the Tevatron's top beam energy of 980 GeV.

Update: Actually the beam was lost at 1040 GeV, which is still a record high energy.

Update: A few minutes ago both beams were successfully ramped up simultaneously to 1180 GeV.

Update: Wow, that was quick. First publication based on LHC data is now out, from ALICE, based on data gathered a week ago. Nothing at all unexpected, this is just based on 284 total events, at the already well-studied energy of 900 GeV.

Comments

1. **Ralph**
   November 29, 2009

   They just got beam 1 to 1180GeV with 2e9 protons. They started with both beams but lost about 90% of beam 2 around 700GeV.

2. **D R Lunsford**
   November 29, 2009

   What exactly happens when they "lose the beam"? (I've been studying physics for decades and still have almost no clue about what happens at real accelerators 🙂

   -drl

3. **hmmm**
   November 30, 2009

   Has the LHC rediscovered the Standard Model then?

4. **lost**
   November 30, 2009

   DRL –
   "Lose the beam" –
   "The" beam is typically many bunches circulating around the circumference.
   (And obviously two counter-rotating beams in the LHC.)
   In the present case, possibly just one bunch per beam just to verify that the LHC works.
   I do not know.
   Anyway, to "lose a bunch" –
   It may hit the wall of the beam pipe (vacuum chamber) or some aperture.
   Possible reasons –
   – The steering went wrong during the energy ramp and the beam centroid went off-center (typically).
   – The bunch was injected badly and did not line up nicely with the reference orbit. (Then the beam would be lost immediately not at 700 GeV.)
   – There is also beam loss from beam-gas scattering. This is a slow process ~ diffusive. An entire bunch is not lost this way.

   During normal operations, after a fill is over the beams are `dumped' by targeting them onto some beam stop. This is also a beam loss.
   Beams can also be deliberately aborted during emergencies (a quench?).

   When a hadron beam hits a target (wall, beam stop, …) it generates artificial radioactivity.
   This is at least one reason that accelerators are interlocked and everyone must be out before operations can begin.
   (Electron beams produce much less artificial radioactivity but they produce much synchrotron radiation ~ "bremsstrahlung" ~ also a reason everyone must be out.)

   Eventually several parts of the LHC will become radioactive. This is a fact of life for hadron accelerators.

5. **Pawl**
   November 30, 2009

   Peter,

   Would it be possible to give a quick summary of what the sub-screens, etc., in the vistars link you previously gave signify? Or give a link to an explanation? (I can work out some, but not all of it.) Thanks.

6. **Peter Woit**
   November 30, 2009

   Pawl,

   I don't know what the graphics visible on the LHC1 vistar are representing, maybe someone else does. The screen has semi-reliable info about whether the beams are in the machine, at what energy and what intensity. Perhaps the text messages from the operators are the most useful indications of what is going on.

   hmmm,

   They're still quite a ways from rediscovering the standard model (in the sense, for instance, of being able to see W's, Z's and top quarks)

7. **from CERN**
   November 30, 2009

hmmm, for the moment LHC rediscovered the pion

8. **Ralph**
November 30, 2009

Re the graphics on LHC1 vistar these are normally BTV-something:

From http://lhccwg.web.cern.ch/lhccwg/Bibliography/UsefulAcronyms.htm "BTV: Beam Observation TV Monitors based on Screens"

From what I can make out, they have thin screens in front of the beam dumps, presumably with cameras pointing at them, and the images show the beam profile from a recent beam dump.

9. **Ralph**
November 30, 2009

Here's a description of the various BTVs: http://ab-dep-bi-pm.web.cern.ch/ab-dep-bi-pm/?n=Activities.BTVLHC

10. **Yatima**
December 1, 2009

And here's how the LHC "beam dump" looks like. Useful when you have to get rid of your hadrons quickly:

http://lhc-machine-outreach.web.cern.ch/lhc-machine-outreach/components/beam-dump.htm

11. **Martin**
December 1, 2009

Peter,
Regarding the first publication: arXiv:0911.5430v1 [hep-ex] you mentioned.

How stringent are the requirement for an arXiv publication?
This paper just says that they've managed to achieve a few collisions and that 2 parameters are what they expected.

Where's the new physics? Which hypothesis were they testing?
Surely they weren't trying to falsify Holger B. Nielsen's and Masao Ninomiya's paper on "Search for Effect of Influence from Future in Large Hadron Collider" (http://arxiv.org/abs/0707.1919)?

12. **chris**
December 1, 2009

Martin,

arXiv is a preprint server and as long as there is one endorser (basically everyone who has ever put a few decent articles on arXiv) you can upload your preprint there.

also you should keep in mind, that accelerator physics and detector physics in itself is a topic. verifying that you have collissions in a new accelerator and that your new detector can make sense of them is worth a publication certainly.

13. **Paul Wells**
December 1, 2009

Martin,

I think another reason for the rapid publication is that many of the people on the LHC teams are PhD students who need a publication of some sort to graduate and they have been waiting a very long time for LHC data. IMHO – good luck to them all !

14. **Yatima**
December 2, 2009

Apparently the LHC suffered a large-scale power failure, but apparently nothing bad came out of it:

http://www.theregister.co.uk/2009/12/02/lhc_power_failure_again/

15. **student**
December 2, 2009

You can expect similar Min Bias papers from all LHC experiments in the very near future. Alice was simply a lot more aggressive in getting the paper done quickly, but the DG of CERN is asking for all experiments to have them out soon.

16. **Bob Levine**
December 2, 2009

"I think another reason for the rapid publication is that many of the people on the LHC teams are PhD students who need a publication of some sort to graduate and they have been waiting a very long time for LHC data. IMHO – good luck to them all !"

What I wonder is, just how much glory do you *really* get as approximately 1/800th author of a paper which, when the five pages of names and affiliations and the page and a half of acknowledgements are references are subtracted, comes to not quite five and a half pages? Is anyone's career really going to be advanced on that basis?

17. **Peter Woit**
December 2, 2009

Bob,

This has nothing to do with glory, just with many universities not wanting to award a Ph.D. to someone until the point where they have some real data to show.

18. **Me**

December 2, 2009

“Is anyone’s career really going to be advanced on that basis?”

I can assure you, they do advance on that basis.

Sci-Fi Science

December 2, 2009
Categories: Uncategorized

I watched the first two episodes of Michio Kaku's Sci-Fi Science show last night (for a review, see here). The format of the show is that Kaku uses supposedly real physics to design on his laptop a revolutionary new device, then unveils it at the end of the show to a group of sci-fi fans for what I guess is supposed to be a form of peer review. The adoring fans are suitably impressed. In the first episode the device was a warp drive, in the second a portal to other universes, based on a big accelerator and "negative matter". In both cases "negative energy" played a big part.

Neither episode involved a non-negligible amount of legitimate science, instead treating the physics in a completely misleading way. The second episode included participation by Max Tegmark, Alan Guth and Neil Turok. I wonder if they've seen the final product and what they think of it.

Comments

1. **Jeff McGowan**
   December 2, 2009

   Well, someone I work with (Thomas Roman) does research on negative energy (together with L. Ford at Tufts). They have publications going back more than a decade giving clear, small, bounds on negative energy density (see for example http://prola.aps.org/abstract/PRD/v55/i4/p2082_1). Tom frequently gives talks explaining that you won't ever get a stable wormhole, or a warp drive, for exactly this reason. You would think Kaku might have bothered to look for papers about this, I mean Phys. Rev. D isn't exactly obscure.

2. **theoreticalminimum**
   December 2, 2009

   I wonder why Turok bothers getting involved in this kind of stupid stuff, especially when Kaku is the orchestrator. Is he not busy enough helping out African students?! :-\
   sigh

3. **neo**
   December 2, 2009

   Kaku is over the top, but on the positive side, lots of kids may be turned on to science by shows like this. Soon enough they will learn that real science is not like comic books.

4. **Gphillip**
   December 2, 2009

   I'm OK with this because it has "Sci-Fi" in the title. As long as they are clear that it's just having fun with Science Fiction (yes, I know that's questionable), I'm not going to get bent out of shape. If they had titled it "Si-Fi Pseudo-Science", I would have sent them a fruit basket.

   So have fun kids, but be sure to pay special attention to that thing called the scientific method in school. If you think this science stuff is fun, you'll need that method to stay on the narrow path of real science. Those who have strayed off the path and are wandering in the weeds of M-Branes (and you know who you are), go back and review.

5. **D R Lunsford**
   December 2, 2009

   neo – kids destined for science, math, or engineering do not get turned on by BS, today or yesterday or tomorrow. Rather the opposite – they get turned onto it by looking through telescopes, playing with machines, taking things apart, and so on. The brain must be involved from the first minute. It's not a sensual thing.

   -drl

6. **zanzibar**
   December 2, 2009

   Suggested edit to avoid a double negative:

   "Neither episode involved a non-negligible amount of legitimate science,…"

   "Neither episode involved more than a of legitimate science,"
   spattering
   iota
   quantum
   smoot
   smidgin

   I like smidgin, it being the most quantative, although iota does have the fewest letters.

7. **Giotis**
   December 2, 2009

   "Neither episode involved a non-negligible amount of legitimate science,…"

   My mind will explode…

8. **Pawl**
   December 2, 2009

   Oh, I dunno — it was not unmeaningless.

[Cribbed from Thurber]

9. **jpd**
December 2, 2009

i thought it was just me, its actually a triple negative.
how about
"Both episodes involved a negligible amount of legitimate science, …"

10. **Peter Woit**
December 2, 2009

Everyone's a critic. Come on, y'all knew what I meant….

11. **Me**
December 2, 2009

"I wonder if they've seen the final product and what they think of it."

No publicity is bad publicity.

12. **Belizean**
December 2, 2009

Yeah, Kaku is a bit of a media whore and an embarrassment. But I'm sure that he long ago realized that there's no market in commercial TV for shows about legitimate science. He's simply adapted to that sad reality.

When I was a child, there was a physics guy on TV who I found absolutely mesmerizing. But he wouldn't last two minutes in any of today's TV markets.

13. **ChuckO**
December 2, 2009

To show you how things have changed, I'm 65 and, when I was in high school, I lived in the Dayton, Ohio area. During my sophomore year in high school, the local public television station showed a college course on introductory quantum mechanics at 6 in the morning. I used to get up to watch it and try to follow along. It was over my head, but I still managed to learn a lot. I don't see anything like that on PBS these days. Even the science shows, like Nova, have been dummied down from what they used to be.

14. **David Pennell**
December 2, 2009

Isn't Kaku's non-science usually derivable from some interpretation of string theory?

15. **Peter Woit**
December 2, 2009

David,

I don't remember much about string theory in these TV programs, perhaps string theory is too much like real physics to be useful in making warp drives and portals to other universes.

Branes do play a role though, as part of the whole other universes business.

16. **DZS**
December 2, 2009

So honestly, you guys here who practice real science.
What specific parts can you point to that's completely falsified?

17. **Peter Woit**
December 2, 2009

DZS,

Kaku's portal to another universe is based upon constructing a scaled up version of the LHC using the solar system asteroid belt. When the beams collide, they supposedly tear a hole in the fabric of space-time and open up a wormhole to another universe. This is complete nonsense, there's no reason to believe that such a machine would do this. He then goes on to say that he will stabilize the throat of this wormhole by putting in "negative matter". There is no such thing as "negative matter".

Etc….

18. **DZS**
December 2, 2009

What about Jeff McGowan's comment?
He seems to state that negative matter is real, but doesn't possess such properties?
Conflicting views?

I think someone should gather up all of Kaku's major mistakes and when the season is over, recap every episode and it's flaws.
Then make an article about it, presenting this evidence for the layman / future scientists…

19. **Peter Woit**
December 2, 2009

Jeff's comment was about negative energy, not negative matter. Nowhere does he state that negative matter is real.

If someone were to do what you suggest, I suppose it would be a public service. It would also be a huge waste of time, since I see no reason to believe it would have any effect on Kaku or the Science Channel's willingness to keep promoting this nonsense.

20. **Jeff McGowan**
December 2, 2009

Yes, Peter is right, nowhere did I mention, or mean to mention, negative matter. Humble mathematician that I am I would never posit such a ridiculous idea. Now perhaps imaginary matter might be a fruitful idea 🙂

21. **DZS**
December 2, 2009

Sorry I misread his comment.
Well then I guess it means Kaku is wrong about negative energy on top of the existence of negative matter all together.

I understood such a task would be tiring and obviously it wouldnt stop the channel exploiting pseudoscience for money, no.
However it would most likely be picked up by most science related sites.
If he is truly as wrong as you guys state, it WILL be picked up by science sites ofcourse.
Kaku is one of the most known scientists of the 21th century, he lying that much would no doubt stirr up controversy.

It's not a fruitless effort or else, why would you bother to write your book and keep this blog?

22. **buz**
December 2, 2009

It has been my impression that many people, especially young, but also older, develop something that they *think* is an interest in science, but is really a type of 'Cosmic Exhilaration Syndrome'. I think Kaku might be someone in academia who has a special appeal with this fan base.

It becomes important to make a distinction between science and scientists, which are often boring but good, and 'cosmic exhilaration infotainment and entertainers'.

String theory seems like an area of study where it is hard to tell the difference. The idea of 11 dimensions and many universes is exhilarating and entertaining for many, and there is no evidence for it. However there are publications, citations and professorships and scientific awards and fellowships giving these ideas credibility. But the people who read this blog have heard enough about all that.

23. **Javier**
December 2, 2009

The existence or not of traversable wormholes is a topic of research addressed by some people, mainly in the general relativity community. Some major nowadays names are, for example, M. Visser or Francisco S.N. Lobo.

As buz says the idea is not new at all and goes back to Einstein himself. Famous people, as Feynman, consider it curious enough to talk about it in his book on general relativity.

The recent line of interest goes back to a paper of 1988 by M.S. Morris and K.S. Thorne (tow of the authors of one of the most famous textbook on general relativity). The research on the topic follows the scientific method. That is, is based on solutions of a well established theory, general relativity, and discussing it's properties in order to search experimental evidence of their possible existence.

Obviously wormholes are independent of string theory. Still some ideas suggested by string theory (and in general, studies in cosmology, specially dark energy) play a role suggesting candidates for the key missing ingredient that mentions Jeff McGowan. From this computer I have not access to the paper he links, but I have read many papers that offer possible reliable candidates for that kind of mater violating NEC conditions.

At most you could call it search for exotic physics, but certainly wormholes are only a little bit stranger than black holes. Independently of everyone's preferences I seriously doubt that any serious theoretical physicist could say for sure that they don't actually exist.

24. **Peter Woit**
December 2, 2009

Javier,

The question is not whether there are wormhole solutions in GR.

Do you think any serious scientists believe wormholes can be created by colliding particles at 10^7 TeV, then using "negative matter" particles to stabilize them?

25. **Javier**
December 3, 2009

Well, if we haven't technology to send people to mart with guaranties we definitively can't send people to kuipier belt to build an particle collider.

But that is a engineering problem, not a first principles one. If that would be the only problem it wouldn't bee too different, in essence, to the theoretical works in, for example, Bose-Einstein condensates or nanotechnology many years before it could be available in practise. Well, possibly the main difference is economical. It is far more expensive (not to say difficult) to build such accelerators. Governments would need a very good reason to do so.

Actually I think that someone would find, sooner or later, better ways to make such high energy colliders that simply increasing it's radius. Suppose for a moment that such a technical development is achieved and the 10 ^7 TeV could be available for an earth based device. Wouldn't your viewpoint about the idea change if so?

The most serious problem of that idea are to see whether the physic is totally solid. The most common idea is that wormholes are continuously created and destroyed in the "quantum foam". Supposedly high energy collisions could increase that planck scale w-h to macroscopic sizes. Obviously the problem is that "quantum foam" is a byproduct of a quantum theory of gravity and I am not sure if there are any detailed calculations in some of that theories (string theory or whatever) deriving a cross section for the creation of a measurable wormhole. I know that two or three years ago some people presented papers claiming to do that for energies available at the LHC . I read one of the papers and they used some exotic type of branes supposedly derived from string theory. They didn't give details on the nature of that branes and referred to another papers. I didn't follow that other papers but I read a lot of critics about them in a thread in physic forums. Maybe there are other papers on the subject, but I am not aware of them.

And, of course, still the key ingredient to stabilize the w-h, exotic matter, isn't available. without it we could search for signatrues of w-h in colliders, but not stabilize them, and they wouldn't be useful for "practical" engineering purposes.

Resuming, nowadays it could be considered a serious (although exotic) topic for science, but going to the practical side is only Si-Fi, precisely what the title of the program says ;-).

B.T.W I didn't see the show and don't know what they mention. As far as I know in principle it could be possible that a wormhole would connect parts of the universe arbitrarily far away. That includes the possibility of connecting regions out of causal contact. In particular they could connect different universes of the multiverses appearing in eternal inflation sceneries. And one could, in principle, use the wormhole to study that other universes so they would be experimentally measurable science them.

In the economic side wormholes would be potentially useful. If one mouth of the w-h would be connected to a point of great energy (near the surface of the sun for example) they could be used as a source of energy. And, of course, they could be used as a weapon.

So, one would go back to the arguments about the lack of danger of creation of black holes. If w-h could be created in colliders they could be created by collisions with cosmic rays and there is not evidence of them, so maybe , after all, there are good reasons to not be too enthusiastic with the proposal of Kaku and his solar system sized LHC.

S you can guess from this long topic I consider fun the idea of the wormholes, but only as an exotic line of research complementary of more well established topics of interest.

26. **Paul Titze**
December 3, 2009

Myself I don't like the way they convey ideas to the public which are plain wrong such as time travel or stabilizing wormholes with negative matter etc, think the aim is to entertain people at best, however I hope people realise this is all fantasy. I'm not sure why he talks about all this when real Physics dealing with the real world is far more interesting however that's me.

There's also a good post on this at:

http://badphysics.wordpress.com/2009/11/15/how-to-time-travel-4/

Cheers, Paul.

27. **Janne**
December 3, 2009

Who watches TV anyway. Any curious kid nowadays will find lots of good material online, for example Leonard Susskind's brilliant lectures on general relativity and quantum mechanics.

28. **Aaron Davies**
December 3, 2009

alcubierre drive, i assume?

29. **Tim vB**
December 3, 2009

Let me try to clarify a bit the "negative energy" versus "negative matter" issue:
"Negative matter" is mostly used to describe something with opposite properties to "normal matter", e.g. the graviational force between normal matter is always attractive, so the gravitational force between normal matter and negative matter would be repulsive. Up to now there is no reason to believe that negative matter in this sense, i.e. with this property, exists.
Well, since Einstein told us that matter is just a manifestation of energy, wouldn't that mean that "negative energy" does not exist, either?
The point is, that "negative energy" means something different: Let's say we look at Minkowski spacetime with quantum fields on it in the vacuum state. "Vacuum state" means that the expectation value of energy is zero.
But you can create spacetime regions where the expectation value is even lower! One example is the Casimir effect:
If you calculate the energy-stress-tensor you will get negative values! This phenomenon has been verified experimentally, therefore it is an accepted topic to talk about in the physics community.
If you want to, you can visualize the vacuum state as emptyness with quantum fluctuations, with virtual particles constantly created and destroyed. In a region with negative energy this happy dance is supressed. (IMHO this is hardly more than a metaphor).
HTH

30. **Adam Helfer**
December 3, 2009

Tim is correct in saying that relativistic quantum field theories predict states with negative renormalized energy densities, and that the simplest treatment of the Casimir effect (between two idealized perfect plane conductors) is an example of such a state. However:

(a) For realistic models of conductors, it is less clear that one gets a negative renormalized energy density between them;

(b) Experiments have verified the Casimir *force*, but not the Casimir *energy density*; a Cavendish-type experiment to measure the energy density would probably require plates around a light-year on a side;

(c) While it is often assumed (especially by people who would like to create exotic general-relativistic effects) that one can use something like the expectation value of the renormalized stress-energy operator as a source for Einstein's equations (this is called the semiclassical approximation), we don't really know that this is correct. (In fact, it's fairly clear that it *can't* be right in a number of situations.)

It's worth pointing out that the renormalized stress-energy operator is in fact a rather singular object many of whose properties are unintuitive and ill-understood. While it's fun to think of science-fictiony implications, it would be prudent to be bear in mind our limited understanding of this.

31. **Tim vB**
December 3, 2009

Agreed, thanks for the clarification.
I hope this discussion explains to the general public why physicists call "negative matter" science fiction and "negative energy" a research topic.

(I should mention that I did not watch the show with Kaku and don't intend to, I just assumed that he used the concept of "negative matter" in the same sense as I did in my previous post).

32. **S.C. Kavassalis**
December 3, 2009

Thanks Paul for linking me over here. I agree that this is another example, again from Kaku, of popularizes doing a disservice to science. Science fiction can sometimes serve as great inspiration for people to enter into the world of physics, but if that is where their knowledge base comes from, then they are deluding themselves. There is so much beauty in general relativity understood well; if people spent the time to express the wonder in actual solutions to the Einstein equations, then they wouldn't need to pad these specials with meaningless, misleading, fluff.

33. **Simple Mind**
December 3, 2009

i'm just a simple person with nowhere near the education or brain power as most of you on here. i actually despise mathmatics. i am hower a realist and one how and learn from the past. i believer most of the comments made above about how crazy this guy is and how his science is flawed are the exact same attacks that newton and einstien faced. the earth is round. yeah right what a crazy idea that is. lets kick him out of the church and put him to death. the earth revolves around the sun. that guy was considered a crazy outcast as well. shoot even tesla was considered crazy for his experiments. however look at what was acomplised by these people for believing in their crazy stuff. 99% of what we have today was considered crazy and usuless at one point in time. Radio waves, crazy. the horseless cariage, crazy. telephone, crazy. electricity, crazy.
So although i may not understand, or sometimes even agree, i don't think he deserves to be call outlandish or crazy, because most all of the "science" shows him to be wrong. countless others were considered wrong too and look where that got us.

Please don't correct my grammer, spelling or typing. i did not proof this no do i care to.

34. **Steve Myers**
December 3, 2009

I have a son who became a computer scientist by building controls for his electric train & moved on to a Radio Shack assembly language setup; another son went in to physics because he wanted to "discover the rules of the universe" and first started out at 3 by noticing the colors of a sunset & asked, "Who painted that?" I work with a bright scientist who began by picking up trashed electronic parts & tried to get them to work; I know a biophysicist who began by noticing moths on trees (and who spent his honeymoon in the Amazon forest). I don't think too many people are led to science through science fiction or phony science.

35. **DaveC**
December 3, 2009

It's my understanding that serious doubts have recently been cast on the claims of Casimir force detection. It was realized by the people who made the claims (Lamoreaux, Capasso) that there were electrostatic effects that had not been taken into account.

36. **Adam Helfer**
December 3, 2009

DaveC,

I am not an expert on the current experimental status, and so I cannot speak directly to the point you raise — but, equally, that means I should have written a bit more cautiously, and not claimed that the force had been "verified," but rather that there was some experimental evidence for it.

I believe the situation is further complicated by uncertainties about the modeling of real systems which are essential for interpreting the experimental results — and I'm not an expert on those details, either. (And perhaps the electrostatic effects you have in mind are part of that modeling….)

37. **Pawl**
December 3, 2009

Simple Mind,

Of course, it's wrong to dismiss ideas out-of-hand as "crazy," and it's true that some excellent ideas have been dismissed that way by people who should have known better.

But the take-home lesson from that is to think things through carefully. Usually if someone can give a good reason for an idea being crazy, it really *is* crazy. It's when someone short-circuits the thinking process that the mistakes are made. (The lesson is *not*, as you seem to suggest, that all ideas are equally good or equally likely.)

Scientists are human beings, and sometimes they do go off half-cocked. But science at its best considers ideas carefully and gives carefully conisdered reasons for whether thet're good or bad, likely or unlikely. The results are all around you.

38. **Bob Levine**
December 3, 2009

"But the take-home lesson from that is to think things through carefully. Usually if someone can give a good reason for an idea being crazy, it really is crazy. It's when someone short-circuits the thinking process that the mistakes are made. (The lesson is not, as you seem to suggest, that all ideas are equally good or equally likely.)"

This is a crucial point. It *may* be true that a lot of what survives in science may have seemed crazy when it was first proposed—I'm not at all sure that that's been true in general—but even if it had been, that in no way implies that crazy ideas that violate well-tested physical principles are likely to be correct, any more than someone surviving a gunshot wound implies that getting shot is likely to improve your chances of living to a ripe old age.

And so far as the actual examples SimpleMind gives are concerned, I'm a little confused about what s/he could be thinking of. Exactly who thought Einstein's ideas were crazy? His early work was controversial when it first appeared, but was recognized as foundational quite soon by the established leading physicists of the time, and was taken to be a non-negotiable starting point for the extension of quantum mechanics to high-energy physics. As for the general theory of relativity, it was recast within a decade of its publication in textbook form by one of the leading astrophysicists of the day, Sir Arthur Eddington. Again, there was plenty of controversy, but if any of the major physicists of the era regarded the theory as crazy, I'd be VERY interested in getting citation of the source. And similarly with Newton: he became in short order one of the most eminent figures in the Royal Society and very early on in his career had developed a formidable reputation—in fact, he was regarded as something of a national treasure and in his own lifetime members of the RS went so far as to accuse Leibnitz of having plagiarized Newton's creation of differential and integral calculus. Who with any distinction referred to Newton as crazy? Hooke thought he was wrong about the nature of light, but that's a very different thing.

There's absolutely no basis for comparing Kaku's flim-flam on the Science Channel with the foundational work of either Newton or Einstein. I've no idea where this strange legend came from that either of the latter were considered to be wrong, crazy or anything similar by the great majority of people who were experts in the field. The majority of stuff that sounds like nonsense is almost always going to turn out to be just that.

39. **Gphillip**
December 3, 2009

Simple Mind

Don't worry, I'm not into correcting others grammar. I live in that glass house and don't want to supply anyone with rocks. We only tease Peter about his writing because he's made so much more money at it than the rest of us.

I don't believe you are that simple, as you went right to the heart of the issue. I'm not sure if anyone ever called Newton crazy. I doubt it as he was a rather dour sort and rarely got close enough to any other human for them to call him names. They certainly called Einstein crazy until they read his papers. I've heard of hardened scientists actually crying from seeing the sheer beauty of his equations for the first time. Of course, Dr. Kaku is no Newton or Einstein, but he is a trained scientist and knows a lot of his "info-tainment" is based on pseudo science. He presents these Si-Fi shows almost tongue-in-cheek, to make a profit. That's fine; I'm no communist . But when a trained scientist presents science fiction to the lay public for entertainment, he is walking a very thin line. Please allow me to explain why I believe this is so.

Often times a scientist will request funding for a project on not much more than his own reputation. If his reputation is damaged, either by himself or by someone else, his ability to perform real science is hampered, if not completely shut down. When a trained scientist promotes pseudo science, he not only damages his own reputation but that of everyone else in his field. You don't really have to worry about good ideas being quashed. Oh, we may fuss and squall like a bunch of children, but in the end, the best ideas always make it to the top, even if they come from a patent clerk fired from his university post.

Please understand, this is not about name calling. It is about professional ethics. If a doctor performs unnecessary surgeries, it not only hurts his reputation, but also the reputation of all doctors. If a lawyer encourages his client to lie on the witness stand, it harms the reputation of all lawyers. Each profession is guided by a central principal. In medicine it is to, "Do no harm". In science it is to use the scientific method to discover and present scientific facts. When we allow the Doctor to operate, we must trust him to do the right thing. We have to trust him because we can't understand what he is doing or why. When the scientist makes a claim, it is expected that he at least believes it to be true. If he is deliberately lying, he destroys that trust between the lay public and his profession. It boils down to a professional responsibility.

As I have said elsewhere, it's ok for a scientist to have fun with science fiction. Carl Sagan did a great job with his Si-Fi book "Contact". But Dr. Sagan knew right where that line of professional responsibility was. He used science to discover and tell the truth, and he never crossed the line. He walked right up to it on his fantastic series "Cosmos", but he never crossed it. If Dr. Sagan told me there were "billions and billions" of stars just like ours, I'd believe him without question. If Dr. Kaku told me the same thing, I wouldn't know if it was true, or just entertainment. Dr. Kaku is no Carl Sagan.

As I've said before, I'm not going to get too bent out of shape since Dr. Kaku had "Si-Fi" in the show title. But most scientists probably believe that Dr. Kaku has stepped over the line of professional responsibility. Please understand, this is not elitism, it is a simply a matter of professional ethics. In science, there is a responsibility to tell the truth, even if it doesn't support your theory. That's often painful and the temptation is always there to falsify data to get the funding that should rightly go to a more deserving project. Once scientists begin stepping over that line, scientific progress will grind to a halt and the progress we have achieved, like ending small pox and exploring the surface of Mars, will grind to a halt with it. If you know someone who bears that burden of professional responsibility, like a doctor or lawyer or engineer or even a professional scientist, talk to them sometime about their burden. Every single one will have had a crisis of ethics where they could have made more money or had an easier time of it if they had just compromised their principles.

Well, that's just my own humble opinion. If you disagree with it, that's your right. But the next time you go to the doctor to get a critical procedure done, let's just hope it's not another Doctor Kaku planning to inject you with negative matter just to see what happens. OK, that was extreme. I apologize.

40. **anon**
December 3, 2009

> Again, there was plenty of controversy, but if any of the major physicists of the era regarded the theory as crazy, I'd be VERY interested in getting citation of the source.

Well, according to Heisenberg, Schroedinger kept on saying that Einstein's photons were absurd, and that he would quit being a physicist if photons were true, until as late as 1925 or so.

41. **Tom Whicker**
December 3, 2009

Right now we have a generation of college-age people who typically believe that WATER is a fuel and that the oil cartels killed Stan Meyers, and killed the Genepax car (ran on pure water and had water as exhaust). There is a wide-spread belief that the concepts of thermodynamics and conservation of energy are "old school" if not an outright conspiracy by big oil to hide the existence of all types of over-unity power machines. Dr. Kaku is no doubt a hero of this audience…

42. **Zac**
December 4, 2009

The show is doing it's job. It's meant to be informitive, and drum up interest in the subject, and it's doing both. Are his argument's iron clad? Not by a long shot, but considering the word "Sci-Fi" is in the title and mearly the subject of theoretical physics is in itself, well, theoretical, I can give him a slight pass. You can extend that pass even further when you remember there is a time limit, and editing that he doesn't have control of.
Do I wish there was more science to the show itself? Yes, I do but I understand that he would much rather have the show air then it end up on the cutting room floor.
So it might seem a bit blasphemous but I know of 3 people who expressed interest after watching, and 1 of them went out the next day and bought a few books. So if Kaku needs to be the Elvis of Physics, then so be it if it means even one more person takes true intrest.

43. **Simple Mind**
December 4, 2009

maybe crazy wasn't the propper phrase to describe the folks i mentioned , well at least a few of them. however my point is, it sometims it takes and outlandish person to come up with some outlandish ideas, that later turn into something real and believed in. sure his ideas of a light saber my seem far fetched, and they are. but his thearetical science may later lead to the discovery of something far more profound that might be the "next big thing". i agree with what someone above me just said. I paraphrase: "sure his science may not be the most accurate, but i might inspire people". if a show like this, that deals with the theoretical science of Sci-Fi of the popular culture, can inspire just a few people to take science, physics, mechanics, and any number of other types of avenues seriously, one of them may come up with the "next big crazy idea". The next major life saver, the next major wepon that can keep our troops safer or that wild secret to the cosmos. I think it's a great show, he has a excellent personality and he has a way of makeing the impossible not seem so difficult. It's entertaining. If someone can take a subject such as theoretical physics and make it entertaining, i'd call him a genious. that subject will generally put people to sleep. I will continue to keep watching the show, because it's entertaining. hower, i do keep in mind that he's dealing with sci-fi, not sci-reality.

I enjoy reading the comment on this page, and enjoy reading the ones that make me realize how much i don't know. I appreciat a forum where no one is attacking each other and calling names. i'm glad i found this place.

44. **Grétar Amazeen**
December 4, 2009

I thinks shows that put the emphasis on proper science and the truth inspire people much more to become scientists that what Kaku is doing. How many people became scientists because of Sagan or Attenborough? Their programs were, and are, proper science and try to display a sense of exitment for grasping the truth, not far fetched fiction.

just my 2 cents…

45. **Robert L. Oldershaw**
December 4, 2009

Jacob Bronowski's "The Ascent of Man" was a remarkable televised science series that celebrated science, art, literature and culture in an intelligent way. A good example of how it can be done right.

46. **Arnaud**
December 7, 2009

To Peter, about his reply to DZS on Dec 2:

This may be where physicists get it wrong w.r.t. PR, IMHO. I would like to compare that with a more salient side of science to normal people, i.e. climate modelling, and of course climate change.

As I'm sure you're aware, there are huge issues going on in the public representation of Earth science given the massive political impacts. As a result, this particular scientific community has reacted by engaging massively with the public and expending a significant amount of time in public forums relentlessly exposing the quacks, denalists and other lobbyists who tend to repeat the same lies and create a travesty of the scientific method. They may (and do) feel sometimes that's it is hopeless, but I believe it has had a very positive impact.

Now, in one way what you are doing through this blog is similar in the field of particle physics and string theory, although, fortunately (?) for you, it seems there are no powerful lobbies on either side of the debate, just overdimensioned egos, quantum-level bank accounts, and faltering careers…

But my point is, in an information-driven society, I think it is not enough to show the truth (in the scientific meaning of the term) and debate in specialised forums. One also has to expose lies and combat them through generic public debate. But I guess this isn't a thing theoretical physicists are interested in, as that appears a thankless and idiotic task (which I can wholehartedly understand).

Are we on the edge of a "New Middle Age" as a French writer recently wrote? I don't know, but I do think scientists of the 21st century should be prepared to fight their corner for rational thinking (if not rationalism) against New Age & populist claptrap, a bit like scientists of the 16th to 18th century sometimes had to fight it against religious and political censorship.

Keep this in perspective, though: The price to pay for upholding reason now is addressing idiotic comments in the blogosphere rather than being burned at the stake. That's gotta be an improvement 🙂

A.

47. **changcho**
December 9, 2009

"…Jacob Bronowski's "The Ascent of Man" was a remarkable televised science series…"

Which of course influenced Sagan tremendously when doing his Cosmos series; Sagan was a big Bronowski fan.

For the most part, Kaku is shamelessly promoting pseduo-science; another thing altogether.

48. **ABProduction**
December 12, 2009

Could all of you really claim your vocation hasn't been a little bit influenced by sci fi? Does the true science, which have been developped all along the 19 and 20 th century, not benefit from the pop culture of sci fi. I am sure it has a great part in your own motivations. Of course every one here ask by himself questions about nature, the sci fi yields the idea that science is enough powerful to change the world. I don't know a lot of kids who decided by themselves to become the creator of a new method in analysis. Why so much space scientist in united states and russia? (and more why so much space scientist?) Why every one loves astrophysics and nuclear reactions, and just few people love condensed matter or atomistic? Because of an insane imagination. I was in a GR course looking the potential for the schwarzschild solution, and I suddenly wondered why last year I wasn't so much in love with potential of di-Hydrogen. The curves are the same in essence (I mean they are potential that's all). Now I explained myself why, I still prefer the schwarzschild one, that is not a crime.
Methods are austere, motivations are passions. (applause, cry, so beautiful).
Sorry for english, I used to love it before I realized I was a piece o…

49. **ABProduction**
December 12, 2009

I have too add to the text just above that carl sagan is just frozen, I definitely don't like him and his talk about universe and universal and so and so.

50 Years of Nobel Memories in High Energy Physics

December 4, 2009
Categories: Uncategorized

Over the past two days, CERN has been hosting a program consisting mostly of talks by Nobel prize winners in high energy physics, under the title 50 Years of Nobel Memories in High Energy Physics. It has been a while since anything Nobel Prize worthy has been discovered in HEP, so the speakers of necessity are all getting on a bit in years. The talks pretty much all seem worth paying attention to. Many of them are now on-line, and of the few I've had a chance to look at, the comments by Burt Richter about the ILC/CLIC issue were notable, as well as Veltman's explanation of the possible significance of not finding a Higgs.

I started watching the webcast towards the end of David Gross's talk, in time to hear him give his usual praiseworthy defense of physics against anthropic pseudo-science. Weinberg's talk by video-conference was unfortunately cut short, since he thought it would be an hour long, but it was just scheduled for half an hour.

CERN DG Heuer ended the program by thanking everyone, and looking forward to the first run of LHC collisions at reasonable intensity with the detector magnets on (although only at 450 GeV), which is now scheduled for Saturday.

Update: Videos of the talks are now available here and I've watched a few of them. One interesting thing I noticed was Frank Wilczek's talk about QCD, where his response to a question about AdS/CFT was "I'm not as impressed as I should be."

Comments

1. **Thomas Larsson**
   December 4, 2009

   Gall et al's (or is it al et Gall?) observation about the Nobel prize singulariaty is quite on the spot.

2. **Domenic Denicola**
   December 4, 2009

   Newbie question after reading Glashow's talk: he says that "Furthermore, the Higgs mechanism per se cannot fully explain electroweak symmetry breaking." What is meant by this?

3. **Cormac O'Raifeartaigh**
   December 4, 2009

   Re 'it has been a while since anything Nobel Prize worthy has been discovered in HEP, so the speakers of necessity are all getting on a bit in years'
   it seems the time gap between theory and experiment gets larger the deeper down we go. Sad to think whole careers come and go in this timeframe e.g. if supersymmetry is observed at the LHC, almost none of the pioneers will be around to see it (except Zumino)

4. **Peter Woit**
   December 4, 2009

   Domenic,

   I'm not sure what Glashow had in mind. There are various problems of principle with introducing an elementary scalar Higgs field (it's not asymptotically free, cosmological constant), there's the hierarchy problem, then there's the basic problem that you lose all ability to predict particle masses, since they are given in terms of arbitrary Yukawa couplings to the Higgs.

5. **Ricci Pieces**
   December 4, 2009

   http://insti.physics.sunysb.edu/~siegel/parodies/sam/sam.html <– Check out the section called Nobel Prize Singularity.

6. **Ralph**
   December 4, 2009

   The schedule for the weekend (http://lhc-commissioning.web.cern.ch/lhc-commissioning/news/LHC-news.htm) is interesting – 450GeV collisions mixed with ramps (presume to 1.18TeV).

   Hopefully if all goes well, they might just tweak the schedule a bit to collide at the higher energy as well…

7. **Shantanu**
   December 4, 2009

   Peter, I don't see a link to the videos of talks. Could you(or others) point me to it?
   Thanks
   shantanu

8. **Peter Woit**
   December 4, 2009

   Shantanu,

   I watched some of the talks on the live CERN webcast site. Presumably the videos will be made available later, but I don't think they're up now.

9. **zhaphod**
   December 5, 2009

   I don't understand why time would be an issue and they have to cut Weinberg's talk in the middle. It is a singular opportunity to preserve for posterity the talks by these great scientist. I think CERN should do more in this regard.

10. **dan**
December 6, 2009

So are there any seriously considered alternatives to higgs (simple scalar or composite?)

11. **Peter Woit**
December 6, 2009

dan,

The general assumption is that, to get electroweak symmetry breaking, one needs to add some sort of new fields to the fermions and gauge bosons of the standard model, with dynamics such that either an elementary or composite field gets a vacuum expectation value that breaks the symmetry. The most popular ways to do this are an elementary Higgs scalar, or a new QCD-like sector, with pions playing the role of the Higgs field (technicolor). I don't know much about other ideas, the prejudice seems to be that they end up being equivalent to one of these two alternatives.

It wouldn't surprise me though if it turns out that electroweak gauge symmetry breaking is a much more subtle business, with some currently unsuspected way of making it work. Hints from the LHC about this would be most welcome…

12. **dan**
December 6, 2009

Thanks for replying,
I know that SUSY is the proposed solution to simple scalar higgs fields, the other is fine-tuning, but do technicolor models (i.e top color assisted technicolor) or composite higgs field also suffer from the hierarchy problem, and need SUSY or fine-tuning to remedy them?
Is there a reason SUSY seems preferred to fine-tuning?

13. **Peter Woit**
December 6, 2009

dan,

Other than the problems with elementary scalar fields, it's not clear to me that there really is a hierarchy problem. One has no evidence for a GUT scale, nor an understanding of quantum gravity, so the smallness of the electroweak scale relative to these may not be of any particular significance.

14. **dan**
December 6, 2009

Thanks, just to clarify, if there is no GUT or planck scale, then Higgs large radiative contributions do not apply?

15. **Van**
December 6, 2009

Hi Dan,
The large radiative corrections do apply, even if there is no GUT or planck scale. I presume that what Peter has in mind is that there is some 'subtle' property of QFT that is currently not recognized which would make these corrections small. I presume he applies the same logic in his idea that electroweak symmetry breaking will not involve a Higgs-like mechanism.

16. **Peter Woit**
December 7, 2009

dan,

The "large radiative corrections" problem is a problem with having an elementary scalar field. It's just one of the reasons for not being happy with this way of implementing electroweak symmetry breaking.

17. **dan**
December 7, 2009

Are there serious proposals for breaking electroweak symmetry breaking that do NOT involve elementary scalar fields and is also consistent with current evidence such as W-W scattering?
thx in advance

18. **Peter Woit**
December 7, 2009

dan,

I don't think current data about WW scattering constrains much of anything, my understanding is that this is something interesting to look at at LHC energies. Technicolor and its variants are the most well-known alternatives to scalar fields, long ago I had a guest blog entry here from Robert Shrock about the state of constraints on such models, see here:

http://www.math.columbia.edu/~woit/wordpress/?p=614

A Black Future

December 5, 2009
Categories: This Week's Hype

Tom Siegfried, the editor of Science News, seems to have decided to join with Michio Kaku in the science-fiction as science business. He marks the startup of collisions at the LHC with A Black Future, an article about how "the Large Hadron Collider might help humans explore the cosmos". Here "exploring the cosmos" doesn't mean understanding how the cosmos works, it means building an interstellar spaceship to travel across it.

The argument seems to be that the LHC will produce black holes, and a recent paper by Crane and Westmoreland suggests that black holes can be used to power a space-ship. Siegfried somehow manages to drag Steven Weinberg and supersymmetry into this, with a claim I don't understand that the Crane-Westmoreland idea "may be realistic only if cosmic physics incorporates a mathematical framework known as supersymmetry."

Comments

1. **Chris Oakley**
   December 6, 2009

   The Crane-Westmoreland paper is listed under "gr-qc" (General Relativity and Quantum Cosmology) on arXiv. Personally, though, I think they should create a new category "bs" for this kind of thing – maybe they have not because it would get too crowded.

2. **J. E. Connett**
   December 6, 2009

   Hi Peter,

   This is a bit off-this-specific topic, though maybe it does relate to hype –

   There is a web commentary group called the Hydrino Study Group
   Forum (HSG),

   http://forum.hydrino.org/index.php?sid=f5b283266ad2a24080ac110d43cc9087

   which is devoted to examining the theory and experiments of Randell L.
   Mills, MD, and his company, Blacklight Power (which has attracted $60M
   in venture capital funding). Mills has published over 70 papers in
   peer-reviewed journals.

   One of the regular posters to the HSG Forum, an electrical engineer
   named John Barchak, frequently cites your blog as evidence that
   quantum mechanics is hopelessly wrong. I wonder if you could clear
   the air on this a bit –

   Do you think QM is hopelessly wrong?

   Are you aware of Dr. Mills' work and his 1000+ page book, "Grand Unified
   Theory of Classical Physics" ? What is your view of this?

3. **Peter Woit**
   December 6, 2009

   J.E. Connett,

   I don't know who John Barchak is, but citing anything I've written on my blog as evidence against QM is complete nonsense. Quantum Mechanics is in a very real sense the best scientific theory we have. It is a beautiful, fundamental theory, closely intertwined with the deepest concepts in mathematics. It has been tested with an accuracy and completeness that nothing else in science can match.

   As for Mills and his hydrinos, I see no reason to believe any of it, encourage others to ignore this kind of pseudo-physics, and to avoid trying to discuss it here…

4. **DZS**
   December 6, 2009

   Peter Woit,

   You write a lot about the Multiverse mania.
   What's your take on Many Worlds Interpretation of quantum mechanics?
   There are atleast as many dogmatic proponents of it as there is of string theory, amongst the worst: David Deutsch, Max Tegmark and I think Michio Kaku is too.

5. **Peter Woit**
   December 6, 2009

   DZS,

   My take on Many Worlds is that I'm not very interested in something which is purely an "interpretation" of QM. If it's fully equivalent operationally to other "interpretations", arguing about which is better doesn't seem to be a worthwhile activity. There is an interesting issue about the "interpretation of quantum mechanics", that of understanding how classical physics emerges from a fundamental quantum mechanical theory, but I don't see that Many Worlds has anything useful to say about this.

   As far as I can tell, despite some people's attempts to mix up the two, Many Worlds has nothing to do with the multiverse hypothesis.

   Sorry, but this is an off-topic issue, one that lots of people want to debate. It's not one I'm expert on or interested in moderating a debate about, so please find somewhere else to discuss this.

6. **DZS**
December 7, 2009

Sorry,

It's just MWI bothers me atleast as much as String Theory.
A lot of parallels when you look at it from a sociological view, with it's dogmatic proponents etc. but I understand this is a place to discuss String Theory.

Thanks for your opinion though

7. **Arnaud**
December 7, 2009

Chris O,

Are you referring to factual errors in that paper, or to their overall subject? I have to say myself I can't comment on the facts but from a cursory glanced of the paper it sounded like an interesting read.

Such papers are obviously not meant to advance the fields of physics with new insights, and maybe the paper would be better published in an astronautics journal, for instance.

But this works reminds me of the work of this past NASA group who was looking into "advanced" propulsion concepts and such like. Not useful currently, but keeps your mind open, as long as the science is right, of course.

After all, the works of Tsiolkowsky when published were probably a similar level of anticipation about technological steps needed for a given application, i.e. space travel. In fact one can argue today we have yet to realise his vision fully, but his insight into potential developments of space travel were astonishing.

A.

8. **mark**
December 7, 2009

Hi,

I also noticed some time ago:

http://www.physorg.com/news174293159.html

another idea for the LHC and insterstellar travel. Good that there is a way to experimentally test this idea right now…

9. **Chris Oakley**
December 7, 2009

Arnaud,

I have no reason to believe that there are factual errors in the CW paper *per se*. But the tone is highly misleading. *If* we had a quantum theory of gravity (we don't), or had stumbled upon artificial black hole production by chance in the laboratory (we haven't), then it might make sense to talk about the practicality of making a propulsion system based on the latter. But since neither is the case, the authors are in reality in no better a position than the scriptwriters for *Star Trek* in working out the feasibility of such a scheme. This is a paper for gullible journalists, not scientists, and providing equations and references serves merely to confuse the issue.

10. **Tim vB**
December 7, 2009

I considered myself warned after reading this in the introduction: "The conclusion we reach is that it is just on the edge of possibility to do so, but that quantum gravity effects, as yet unknown, could change the picture either way."

I'm not sure anymore if papers like this one are supposed to be a joke or not.

11. **Adam Helfer**
December 7, 2009

The CW paper is serious science of a highly speculative but appropriate sort (on the question of whether human interstellar flight is possible within certain parameters). The authors clearly don't expect their analysis to be definitive, but aim to stimulate work leading to a fuller understanding of the issues.

I think the authors' tone would be understood and not misconstrued by anyone in the field, and given the intended readership (gr-qc) I don't see a problem. Of course, anyone who is not in the field should be cautious about her or his understanding of the paper's context, the likelihood its arguments are correct, and its implications.

That the authors are aware of the potential of quantum gravitational effects to alter their conclusions is a strength rather than a weakness. There are plenty of papers on related issues whose authors seem unaware that on dimensional grounds quantum gravitational effects could be large enough to change their results.

12. **Peter Woit**
December 8, 2009

Adam,

Physicists posting wildly speculative papers about space travel and black holes to the arXiv isn't unusual or surprising. Science News publishing an article claiming these have something to do with the LHC is…

13. **Adam Helfer**
December 8, 2009

Peter,

I agree. The point I was making was that just as some of those not in the field should be cautious of forming a Crane-Westmoreland-LHC-SUSY-Weinberg amalgam, so equally should others be cautious about the sorts of criticisms they make of Crane and Westmoreland.

14. **schof**
December 9, 2009

I feel as if there might be another subject which regularly assumes the existence of certain results or axioms that are only true in some cases, or have yet to be shown.

Mathematics rings a bell…

15. **Louis Crane**
December 10, 2009

Dear sir,

I completely agree that that sentence in the Science News article is misleading. We did not use supersymmetry in our calculations. I am very skeptical about supersymmetric theories.

If LHC surprises us and produces black holes, all the calculations in our paper will need redoing, and the project will be much easier than it now seems.

On a personal note, I do not understand why contributors with no actual points to make think it appropriate to curse at us in public. I do not think the internet justifies the end of civility.

16. **Chris Oakley**
December 10, 2009

Hi Louis,

Thanks for dropping in.

This is the abstract of your paper:

> We investigate whether it is physically possible to build starships or power plants using the Hawking radiation of an artificial black hole as a power source.
> The proposal seems to be at the edge of possibility, but quantum gravity effects could change the picture.

As I do not need to tell you, an artificial black hole has never been produced. There is, I believe, recent strong evidence for the existence of black holes as astronomical objects, but since the supporting experimental evidence for GR is entirely in cases of a weak gravitational field it is entirely possible that the Schwarzschild solution does not apply to such objects. What is more, the densities of such objects are probably such that one could not ignore quantum effects. As I also do not need to tell you, we do not currently understand gravity at the quantum level. Thus our understanding of black holes from both the theoretical and experimental standpoint is extremely poor. Despite this, using semi-classical arguments Stephen Hawking was able to demonstrate that if such objects exist according to the laws of GR, then they must radiate. However, since we do not really know what is going on here, there could also be a myriad of other effects, one or other of which could making Hawking radiation irrelevant. We will not know for sure until we have a workable quantum theory of gravity.
So, if one substituted, "The proposal seems to be at the edge of possibility, but quantum gravity effects could change the picture." with, "The proposal assumes that one can produce black holes artificially – something that has never been done, and that Hawking's semi-classical model suffices to describe their behaviour – which seems highly unlikely," then it would be less likely to confuse journalists.

17. **Tim vB**
December 11, 2009

Adam Helfer, Lois Crane,
my apologies if what I said came out wrong.
IMHO the paper we talk about clearly states what is assumed and clearly states what the current status of these assumtions is, so I don't think it should cause any confusion in the physics community.
What Tom Siegfried wrote however seems to be an unjustified blend of ideas in order to attract the attention of a lay audience,
which is what motivated Peter Woit to blog about this in the first place, as he already pointed out.
However: I suspect that a highly speculative paper about harnessing black holes to propel spaceships will
look like a result of some playfully spent idle time to most scientists outside the field, which is what I meant when I said "is it a joke?" (I did not intend to imply that the paper is ludicrous, sorry for the imprecision.)

18. **Robert Frost**
December 11, 2009

Perhaps arxiv.org needs a new category:

"hep-pseudoscience [toy ideas, fantasies, untestable speculations].

Just a thought.

19. **RM**
December 14, 2009

Perhaps one way to put it is this: when physicists who normally publish meaningful physics, sometimes take a detour into the bizarre, its probably ok – and maybe even worth reading. However, if the bizarre becomes routine, one feels like revoking the tenure system. I don't know enough about general research published by Crane etc so would not like to comment on this particular piece.

But K(a)ukoo-land is quite another matter…!

Spreading Wild Rumors

December 7, 2009
Categories: Uncategorized

One effect of Nature's embargo policy is to encourage the spread of wild rumors. It's almost part of the mission statement of this blog to participate in this, so I feel I must link to this.

Update: Nature unequivocally denies that they will be publishing a CDMS paper on the 18th. The collaboration does plan to reveal the results of their latest run on that date. The wild rumor that they have seen WIMPS and will reveal all on the 18th remains in force…

Update: There's now a statement on the CDMS web-site, saying that there will be two talks on December 17, and plans to submit a paper to the arXiv before the talks.

Comments

1. **Low Math, Meekly Interacting**
   December 7, 2009

   Probably a good link for the future would be the point spread on Jester's two proposed options and whatever others fit some reasonable Bayesian cutoff.

2. **Low Math, Meekly Interacting**
   December 8, 2009

   Oh well.

3. **Ricci's Pieces**
   December 8, 2009

   That's quite interesting. I can't wait until the 18th!

4. **Yatima**
   December 8, 2009

   X-mass is early and in the multi-GeV range.

5. **Coin**
   December 8, 2009

   Nature unequivocally denies that they will be publishing a CDMS paper on the 18th

   Which of course does not rule out the possibility that they will be publishing a CDMS paper on the 19th DUN DUN DUNNNN

6. **Jess Riedel**
   December 10, 2009

   Does releasing a pre-print before their talk suggest that their results aren't monumental?

7. **blogging tips**
   December 16, 2009

   17 december will be here tommorow – we will see

News From the LHC

December 8, 2009
Categories: Experimental HEP News

Just saw this from Tommaso Dorigo, which made me realize that I should get to work and write a blog posting before the CERN press office people wake up. The big news today is that the first collisions at a record energy higher than that of the Tevatron have occurred, marking a first step into new territory past what has been the energy frontier in HEP for a very long time. Two beams of two bunches each were successfully ramped up to 1.18 TeV, and although one of the beams was lost after a while, before that point some collisions at a total energy of 2.36 TeV were observed. An event display from ATLAS is here.

Over the next few days, the main objective of the beam commissioning team is to try and increase the intensity per bunch and provide a large number of collisions at 450 GeV/beam to the experiments. They'll also continue tests in which they ramp up the energy to 1.18 TeV/beam, and if all goes well should be able to provide the experiments with an equally large number of collisions at that energy. The experimenters are gleeful about finally having real data to play with. CMS has publicly announced the re-discovery of the pi-zero, and I assume that by now they're working their way through the 1950s, already rediscovering kaons and other particles with strange quarks. If they manage to get a significant amount of data at 2.36 TeV, there must be some sort of cross-section they can measure at an energy that just beats out the Tevatron, although with this size of a collision sample, it won't be one that anyone cares much about…

Beam commissioning work for 2009 should end on December 16, and the accelerator complex will be shut down over the holidays, re-starting January 4. During January the LHC will go back into hardware commissioning mode, with a month-long plan to commission the new quench protection system in all sectors, allowing operation at 3.5 TeV/beam. The injectors are supposed to restart on February 5, LHC beam commissioning restarting on February 8. The way things have been going, they may have at least some 7 TeV collisions happening quite quickly after that.

Update: This morning again a pair of 1.18 TeV beams were stored and brought into collision at the LHC. This time CMS as well as ATLAS is publicly saying that collisions were seen, with event displays here. CERN's twitter feed is saying over a million collisions at 900 GeV, 50,000 at 2.36 TeV. Still no press release from CERN gloating about how they now are doing physics at higher energy than Fermilab.

Comments

1. **Steve**
   December 8, 2009

   Hi, Peter. Could you explain what it means for a beam to be "lost"? I've heard that term used few times, but I don't understand the relation between that and just dumping the beam. Is there some sense in which the beam is "lost" but still circulating and waiting to be shunted to the dump system?

   Cheers,
   -Steve

2. **Peter Woit**
   December 9, 2009

   Steve,

   I think in this context "lost" means more "lost control", presumably the operators could tell you what happened to the beam particles. When a beam is "dumped", that's a controlled shutdown of the beam, by directing its particles to a safe location to be stopped.

   There are all sorts of instabilities than can disrupt a particle beam and lots of complex mechanisms for stabilizing the beam and controlling it. When it's "lost" I think this normally means that there was some sort of failure leading to a destabilization and loss of control of the beam.

3. **Nobody**
   December 9, 2009

   It appears that the beams were never declared as "stable" (neither they were intended to collide); does this mean that ATLAS keeps his detector switched on even when the beam is not under complete control and can do weird things?

4. **Peter Woit**
   December 9, 2009

   Nobody,

   My understanding is that in this situation the detector is not fully operational, with some parts switched off for protection. But enough of the detector is working to see the evidence of a collision that ATLAS showed.

5. **jks**
   December 10, 2009

   Vistars Op comments at 02:01:01 on 11-12-2009: "FIRST HIGH INTENSITY STABLE BEAMS". Looks like five bunches each in B1 & B2 (@ 450 GeV) with the FBCT (fast beam current transformer) intensity saying about 6e+10 with a slow rolloff to 4e+10 and hour and 1/2 later. Nice.

6. **a quantum diaries surivivor**
   December 11, 2009

   Hi Peter,

   thanks for the link 🙂
   Yes, you assume well. Atlas and CMS are working their way through the fifties, literally.

   By the way, when the beam is lost it is indeed what you explained. There are so many components in a proton synchrotron that may cause the beam to get progressively out of sync and lost, there is not much to explain.
   As for stable beam: LHC is indeed producing stable beams, and in fact the data that ATLAS and CMS are analyzing come from those short periods of time when machine operators take their hands off the controls and go for a coffee. Normally, they try to study the beams and by doing so the conditions are not suitable for data taking.

Cheers,
T.

7. **a quantum diaries surivivor**
December 11, 2009

Oh, and just before I forget: you might want to read my latest posting on the events of this week. Not really releasing any insider information, but I hope I have given the flavour of what is going on at CERN these days.

http://www.scientificblogging.com/quantum_diaries_survivor/science_making

Cheers,
T.

8. **Mehmet Tezgel**
December 15, 2009

Hey it's 2.36 TeV not GeV . Greetings from Darmstadt, Germany 😀

9. **Peter Woit**
December 15, 2009

Thanks Mehmet!

fixed…

Various and Sundry

December 13, 2009
Categories: Uncategorized

- The proof of the fundamental lemma by Ngo has made it onto Time magazine's list of the top ten scientific discoveries of 2009. Ngo will be visiting Columbia in the near future, and I might even end up understanding what this is about. He's giving the Ritt lectures here later this week, and will be Eilenberg visiting professor for the Spring 2010 term, giving a series of weekly lectures.
- The collaborative work on the Density Hales-Jewett theorem initiated by Timothy Gowers on his blog has made it into today's New York Times magazine's survey of the "Annual Year in Ideas".
- Tony Zee's book Quantum Field Theory in a Nutshell will be coming out in a second edition next year, featuring some new material covering recent advances in computing scattering amplitudes in gauge theory. The new preface is available here and has some interesting comments from Zee about the book and about QFT books in general. It also contains a response to those Amazon reviewers described as "nuts who do not appreciate the *Nutshell*". I suggest that Zee get a blog, it gives one an excellent way to respond to nuts who misunderstand and don't appreciate one's book…
- Last weekend there was a meeting held at Rutgers in memory of I. M. Gelfand, with some materials available here.
- A couple weeks ago there was a very good article in *Science* magazine by Adrian Cho about recent discussions of the possibility of a muon collider. Since muons are much heavier than electrons, one can in principle use a storage ring to collide them without the problem of synchrotron radiation loss that limits the energy of electron-positron rings to the LEP energy scale. The fact that muons are unstable and decay fairly quickly is a huge problem. Besides making it difficult to use the "cooling" techniques needed to produce a usable beam intensity, the decay products create a very challenging environment for a detector to operate in, as well as producing neutrino intensities so high they are capable of causing problematic levels of radiation wherever they emerge from the earth.
- C. J. Mozzochi has a page here with links to many of his wonderful photographs of mathematicians, mostly in action at various conferences or lecture series.

Update: One more. Last night I watched a spectacularly bad Sci-Fi movie, Annihilation Earth, brought to the world by the Syfy channel. I don't think it's a movie that really can be spoiled for you, so here's a plot synopsis: three supercolliders in Geneva, Orleans and Barcelona are providing power for Western Europe. Scientists who designed them realized that in a certain configuration the critics were right, and the Higgs field would get out of control and form a black hole that would destroy the earth. Evil Arab terrorists hack into one of them and reconfigure it to self-destruct. The remaining two are all that is keeping the Higgs field from expanding exponentially and causing the black hole that will annihilate everything. One of the scientists refuses to believe the other when he explains this to him, because of the color of his skin and the fact that he's an Arab too (although he doesn't look it). So, in the final scene he shuts down one of the remaining super-colliders and the Earth is annihilated. I guess the film-makers should be congratulated on this innovation in sci-fi film-making, ending the film with the scientists not saving the Earth but destroying it.

Comments

1. **Ricci's Pieces**
   December 13, 2009

   Wow, I am pleasantly surprised that Ngo's work is being recognized by Time magazine. Certainly this is huge for the Langland's Program.

2. **Tim vB**
   December 14, 2009

   Time magazine chose Edward Witten as one of the most influential personalities of the year (or the decade or the century, I don't remenber) some years ago.
   They must have a mathematician or physicist in their editorial staff.
   This kind of recognition is of course great news to the whole math/physics community, not only to the Langland's program (either the reader already knows about Langland's or (s)he will have forgotten about it seconds after reading the article :-).

3. **Arun**
   December 14, 2009

   I suggest that Zee get a blog, it gives one an excellent way to respond to nuts who misunderstand and don't appreciate one's book…

   But moderating the comments is a painful chore, not to be wished upon even one's worst enemy….. 🙂

4. **sfjp**
   December 14, 2009

   About "Earth annihilation": that's why, by backward causation, the LHC will fail before producing any Higgs….:-)

5. **D R Lunsford**
   December 14, 2009

   Ok Peter, so why are there beekeepers in this flick? Maybe they are B-meson keepers. Huh?

   -drl

6. **John Baez**
   December 15, 2009

   … but for some reason backwards causation did not prevent people from making this movie.

7. **John**
   December 15, 2009

   Hi Peter,

   I was thinking of purchasing Zee's book to start learning some quantum field theory: do you have any specific suggestions regarding the work and/or beginning qft texts?

   Regards,
   John

8. **Chris Oakley**
   December 15, 2009

Annihilation Earth at least has the positive feature of making the public think that particle accelerators contribute to the electricity grid rather than being a massive drain on it.

9. **Peter Woit**
December 15, 2009

John,

I like Zee's book a lot, but the problem I see with it is that if you're just beginning in the subject it doesn't really have the kind of technical detail you need to be able to do computations yourself. It might be best used as a supplement to one of the standard, more computational texts (I like Pierre Ramond's book since it is concise, but it doesn't address lots of topics, a more comprehensive book with details is Peskin-Schroeder). Another recent QFT book I like a lot is V. P. Nair's.

10. **D R Lunsford**
December 15, 2009

Peter, I had the same complaint about Kaku's book (which, believe it or not, is eminently sane). A fairly recent book that I have is by Michele Maggiore – very good, sort of a Sakurai for modern times. For relativistic QM the book "Advanced QM" by Schwabl is excellent.

-drl

11. **srp**
December 15, 2009

Re the muon collider: I always hear these gee-whiz statements about how neurtrinos are sooo hard to detect because they don't interact with anything and they can go through the Earth without noticing it, etc. Now you tell me that the measly decay products from an accelerator are intense enough to cause radiation issues? I demand a popular science refund!

12. **Steven Colyer**
December 15, 2009

Thank you for the excellent textbooks, Peter. I also own "Nutshell" as Zee refers to his book, and find it as others do a book than likely began in intent as a non-technical popular overview of humanity's greatest 20th century achievement, QFT, then morphed somewhere along the way into a "near"-textbook without quite achieving true textbook status.

As a fine introduction to those who have mastered the Mathematics of Quantum Mechanics, I enjoyed "Quantum Field theory Demystified, a Self-Teaching Book", by David McMahon, available in bookstores and targeted to the very intelligent layperson.

As early as page 3 he makes mention of the Klein-Gordon equation with its obvious flaws, and moves quickly into the Dirac Equation. On page 4 he explains that while "x-carat" is considered an operator and "t" a parameter in QM, and one would expect the promotion of "t" to operator status in QFT, that instead "x-carat" is considered a parameter as well, and McMahon goes on from there through its slim but lovely 261 pages.

13. **Coin**
December 16, 2009

They call it "science fiction" but so much of the time the message seems to be "science is evil and will kill us all".

14. **Tim vB**
December 16, 2009

@John on QFT books:
I completly agree that you should definitly read the nutshell, it's a great exposition of many beautiful concepts, but you need a supplement that gets you calculating.
Try Voja Radovanovic: "Problem Book Quantum Field Theory" (consisting of standard problems and solutions on an introductory level).

15. **andy.s**
December 16, 2009

The problem with the "Demystified" series is that the publisher seems to have cheaped out on providing a proof reader. Did you know that the charge on the strange quark is +2/3? Me neither!

Little goofs like that are all throughout the text.

16. **Haelfix**
December 16, 2009

I loved Zee's book. Actually after quantum mechanics, I recommend a pedagogical introduction to particle physics (alla Griffith) to learn Feynman diagrams and how to compute various quantities like cross sections. Then read Zee, but don't do any of the exercises.

From there, Peskin and Schroeder is the standard field theory textbook and the one where you should spend time doing exercises and all the funky integrals. From that point on where you go depends on preference and what you are going to do in physics. I like Weinberg, b/c he goes over the whole material from scratch in a logically cohesive way and by that point you should be good enough to follow it without too much of a hitch.

17. **Gphillip**
December 16, 2009

I suspect fans of HEP and the LHC probably laughed all the way through the Annihilation Earth Si-Fi show, if they made it all the way through. I tried to watch, but failed to make it to the end. Thanks for the summary, now I don't have to watch the rest of that silly stuff. Oh well, at least they didn't have a real Physics PhD playing the lead.

18. **Coin**
December 18, 2009

I have an open-thread kind of question. Would anyone be able to recommend any current science books which would be a good Christmas gift for someone? I was thinking about something that would serve as kind of an introduction to the things the LHC might be looking for, and so was thinking of Oerter's "The Theory Of Almost Everything" which is sort of an intro to the standard model, but that book is like two or three years old and I don't know if something more appropriate or explicitly LHC-centric might be out by now. (Also it's cheap now so I was wondering if I could find something to pair it with.) I should note the person I'm thinking of giving this gift to has like a master's in earth sciences so it's safe to give something math-heavy (or even just about math, if there are any math or math-history books worth recommending that came out this year).

19. **Steven Colyer**
    December 22, 2009

    One book I would NOT recommend is the recently released "String Theory for Dummies" by Andrew Zimmerman Jones (which many here would say is the most appropriately named book of all time), because of its rather heavy PRO-strings bias, and its obviously heavy influence by Leonard Susskind. Peter, have you reviewed that book yet?

    On the positive side, I would start with journalist Louisa Gilder's The Age of Entanglement, which covers the Quantum 11 (I include Weyl) from Planck through Dirac and on into the 21st century (with stops along the way with Feynman and Bohm) in novel form. Reads like a novel, and is vastly entertaining and inspirational, albeit math-free.

    After that, Peter's own Not Even Wrong is excellent. I call attention to the middle so-called "hard parts" in the middle which are a brilliant example of expository writing regarding the development of Quantum Field Theory beginning with Weyl and ending with Atiyah. Of course the whole book is great but those are my favorite bits. Again, no maths, but that's the joy, to tell such a story in prose form.

    I would then move on to the 2009 Revised Edition of The New Quantum Mechanics by Tony Hey and Patrick Walters, with the briefest introduction to the mathematics involved. My one flaw with the book is a mere 2-1/2 pages devoted to Quantum Gravity because they only mention String Theory. Peter would probably say 2-1/2 pages is the appropriate proportion to devote to QG in a 200+ page book about QM. 🙂

    Finally, for the Math buff, there's The Road to Reality by Sir Roger Penrose. Although Sir Roger speculates quite a bit, he is polite enough to warn you ahead of time that he's doing so. It is chock full of Math and his introduction to spinors is a classic.

20. **AlBme**
    December 23, 2009

    Regarding "Annihilation Earth": The Sci-Fi channel started out with great promise, but badly deteriorated over the years through poor management — with some notable exceptions like "Battlestar Galactica". When the Sci-Fi Channel changed its name to SyFy, it became one of the worst network name changes in television history. It was so appropriate to learn from my Polish friends that "syfy" in polish is the plural for "a dirty mess" — but, dirty in the way feces is dirty.

    There's good sci-fi and bad sci-fi. This movie doesn't have enough 'sci' to even qualify as bad sci-fi.

String Theory and Heaven

December 16, 2009
Categories: Uncategorized

I hadn't heard much about Dinesh D'Souza since the Reagan era when for some mysterious reason his views were widely promoted in the media. He has continued since then to play the role (supposedly according to the New York Times Magazine) of "one of America's most influential conservative thinkers." His last book was *The Enemy at Home: The Cultural Left and its Responsibility for 9/11*, and he has a new one out called Life After Death: The Evidence. In a recent magazine interview he explains the main thesis of the book, that string theory has vindicated Christian theology by proving the existence of heaven and hell (they're out there in the multiverse somewhere).

> **How might science explain heaven and hell as places that could exist?**
>
> Scientists now posit through string theory the presence of multiple realms, multiple dimensions. One of the implications of the big bang is that space and time had a beginning, and that space and time are properties of our universe. If that's true, then outside our universe or beyond our universe, there would be different laws of space and time, or no space and no time.
>
> The idea that our universe may not be the only one and that there may be other universes operating according to different laws is now coming into the mainstream of modern physics. So the Christian concept of eternity, which is God outside of space and time, is rendered completely intelligible. It opens up possibilities that would have seemed far-fetched even for science fiction a century ago.

Update: Here's the link to the interview: String Theory and Heaven.

Comments

1. **Pawl**
   December 16, 2009

   Boldly going where angels fear to tread.

2. **andy.s**
   December 16, 2009

   Of course Christianity posits only 3 universes, ours, Heaven and Hell(*). This is more parsimonious than 10^500, making Christianity more likely than string theory.

   I wonder what the vacuum state for hell is?

   ——————————
   (*)Limbo and Purgatory being submanifolds of Hell proper.

3. **point**
   December 17, 2009

   You'd better behave yourself, Peter, or you might get sent to a universe with branes, supersymmetry and a negative cosmological constant.

4. **Geoff**
   December 17, 2009

   It is completely obvious from reading scripture that the vacuum state for heaven must be calculated using the Bogolubov transformation. As any self respecting Christian will tell you, Bogolubov is Russian for God's love. It is equally obvious that the vacuum state for hell is ill defined since hell is clearly expanding and thus creating new damned souls 🙂

5. **Rutger**
   December 17, 2009

   There is nothing new about claims like this, see the 'physics vs eastern philosophy' craze a while back. Further, I bet there are people who have written that the standard model proves there is no god/1 god/many gods. To suggest that string theory is the only theory to fall foul of these types of wild extrapolations is not quite fair (not fair and balanced? :)).

6. **Francois Vanderseypen**
   December 17, 2009

   It's good to have a laugh 🙂 The tragedy is the influence some people have and give an aberrant picture of reality. Very often, not the truth matters but the perception of things.

7. **Bob Levine**
   December 17, 2009

   " …So the Christian concept of eternity, which is God outside of space and time, is rendered completely intelligible."

   What I find bizarre is that D'Souza seems to think that the existence of multiple subuniverses, corresponding to all those different vacuum states, somehow implies that the contents of at least some of those universes are exempt from causal relationships ('outside of space and time'). Is there any reason whatever to believe that string-based models of the 'landscape' include such oases of noncausal being?

8. **Alejandro Rivero**
   December 17, 2009

   The main difference between Kaluza Klein and Modern String Theory is that the former is a theory of "needed extra dimensions", while the later is a theory of "wasted extra dimensions". Given that there is no role for the extra dimensions in string theory, this proposal is as good as any other. Any conceptual vacuum, g-d fills it.

9. **andy.s**

December 17, 2009

Damnation : i.e. |Mortal Sin>, is one of the eigenstates of a person's wave function (which layman refer to as a 'soul').

Other eigenstates: |Original Sin>, |Grace>, |Venal Sin>.

10. **Stephen**
December 17, 2009

All this may sound ridiculous, but I find the following sentences meaningful:

"outside our universe or beyond our universe, there would be different laws of space and time, or no space and no time."

"So the Christian concept of eternity, which is God outside of space and time, is rendered completely intelligible."

One may argue about the word "completely", but the line of reasoning is actually showing that a concept which was very mysterious in theology, becomes illuminated by modern physics in a beautiful way.

However this is not new, and it has nothing to do with string theory but just with other concepts, such as general relativity and the possibility of having theories with coupling constants which take different values from the ones that we observe.

11. **Chris Oakley**
December 17, 2009

andy.s,

Not to be pedantic, but what you say is only true in the Heisenberg Picture as

$|\text{Mortal Sin}\rangle = \exp(iH' t)\ |\text{Original Sin}\rangle$

where H' is the unperturbed Hamiltonian plus a deviation towards wickedness (obviously t = Day of Judgement here). In the Schrödinger Picture, they are the same ket, measured at different times.

12. **Yatima**
December 17, 2009

Sounds like a schizophrenic is free-associating on the couch. Charming.

Dante has been in that general vicinity a long time ago anyway, with his universe a 3-sphere, hell at one end, and heaven at the opposite end:
http://dx.doi.org/10.1119/1.11968

13. **h**
December 17, 2009

As any Hindu can tell you, you are all (not even?) wrong.
The world is flat, and is supported on the back of a turtle,
which swims in an infinite sea, which resides in … the multiverse?
http://en.wikipedia.org/wiki/Turtles_all_the_way_down

Why be Christian and die only once?
As a Hindu you can have multiple lives and visit the entire multiverse!

14. **Barry Cunningham**
December 17, 2009

This just reflects the fact that even the flat-earthers have noticed what many others have known for a long time: string theory is a religion. The only wrinkle is that since they believe there is only one true religion, they think it must be theirs.

15. **Scerny**
December 17, 2009

Sorry Peter, this will be totally off topic, but of interest:

Does anyone know what happened to Lubos Motl's blog? Currently it's down, blogspot saying the blog can not be found.

Scerny Pvrizek

16. **Tim vB**
December 17, 2009

You can justify Christianity by conventional physics, you don't need string theory:

The-Physics-of-Christianity

One thing I don't understand is: Do the authors expect every Christian to get a physics Ph.D.? Or may true believers just believe without understanding anything of the scientific foundations of their religion?

17. **Koray**
December 17, 2009

If physics tells us that there may be other universes, it still doesn't render "God outside of space and time" completely intelligible just as "outside of Houston, Texas" doesn't mean somewhere else in Texas, or the US, or the earth.

18. **Hal Porter**
December 17, 2009

As a non physicist, does string theory posit an immortal soul that wafts off to another universe after we shuffle off this mortal coil?

And what are the implications for the trinity, the virgin birth, and the rising of the dead after the great shout.

Rattle my bones and tie them together with strings!

19. **anon**
December 22, 2009

D'Souza has published a book after "The Enemy At Home" and before "Life After Death". It was called "What's So Great About Christianity". I read it, and I thought it was very good, and I don't even believe Christianity. I also don't have the same political views as D'Souza; mine are libertarian, which D'Souza rejects. Dinesh D'Souza is a very serious thinker, and it is unwise to discuss him dismissively. I haven't read "Life After Death", but my concern is that Woit's original entry might not accurately describe the main thesis of the book.

20. **Bob Levine**
December 22, 2009

"Dinesh D'Souza is a very serious thinker, and it is unwise to discuss him dismissively. I haven't read "Life After Death", but my concern is that Woit's original entry might not accurately describe the main thesis of the book."

Anon, just look at the content of what D'Souza says in the passage Peter quotes. The theory that D'Souza explicitly refers to, and which he takes to be synonymous with 'Science', is, like all other physical theories, a statement of a set of constraints on dynamical variables defined in terms of space and time. The 'multiple universes' D'Souza alludes to are just those possibilities which conform to those constraints. And D'Souza believes that among the members of this set of possibilities conforming to those constraints, there will be at least one which is subject to NO constraints on (entities definined in terms of) space and time, since there supposedly is *no* space or time in this member of the set of universes. Serious or not, what kind of thinking is involved here?

21. **Peter Woit**
December 22, 2009

anon,

I've added the link to the full interview of D'Souza, which I forgot to include when I originally wrote this posting.

As to questions like that of whether D'Souza is a serious thinker or an idiot, my preference is to let people read for themselves what he has to say, and make up their own minds. To me personally, this seems like an open-and-shut case…

22. **luny**
December 22, 2009

A cursory reading of the bible shows God is not "outside space and time". He creates the world, messes around all kinds of people to "test them" (Adam+Eve,Job,Abraham,…), realizes humanity is going to the dogs, floods the world after arranging an ark, and finally sends his son to save humanity.
He might be on a different brane, but his timeline is the same as ours

23. **anon**
December 22, 2009

I think it's a mistake to underestimate D'Souza's intellect, or to describe him in a mocking or dismissive way, but I suspect that you, and many other physicists, will be predictably hostile to D'Souza without even doing a serious reading of his work. There are, however, two factual errors in the blog post. One is about which book was his last one. The second is about the "main thesis" of his newest book. The publisher's description is:

"Drawing on some of the most powerful theories and trends in physics, evolutionary biology, science, philosophy, and psychology, D Souza shows why the atheist critique of immortality is irrational and draws the striking conclusion that it is reasonable to believe in life after death. He concludes by showing how life after death can give depth and significance to this life, a path to happiness, and reason for hope."

The main thesis of the book is that it is reasonable to believe in life after death. Using ideas from string theory to defend that main thesis is only a small part of the book's contents.

24. **Peter Woit**
December 22, 2009

anon,

My apologies about any inaccuracies. I haven't read the book, but I did read the full interview with D'Souza that I linked to and quoted from. He goes on there in a way that makes clear that he has no understanding at all of what he is talking about. That he has written a book claiming that the "cultural left" is responsible for 9/11 just seems to me additional evidence that he's an idiot. Maybe he's only an idiot on some days, a serious intellect on others. Those who believe this are welcome to investigate the rest of his writings.

25. **anon**
December 22, 2009

I've read "The Enemy At Home", and it isn't his best book, but it's not a complete joke either. It had some parts I found convincing, others not so much. The title they chose for that book is not a very good one, and not very representative of the contents of most of the book. Regnery often has a lot of input about the books they publish and how they are marketed, and I suspect that it might have been their choice of title, perhaps to help sell the book to consumers of an "anti left" political persuasion. I certainly have many disagreements with D'Souza, but I don't regret reading his books. I doubt you'll read his book, but none of his books that I've read are simple or bad enough to warrant a quick ad-hominem dismissal.

26. **David H. Miller**
December 23, 2009

Peter wrote:
>Maybe he's [d'Souza] only an idiot on some days, a serious intellect on others.

Peter, I've followed his career for a long time: let's just say he is no dumber than Limbaugh.

Seriously, I've seen him give a good speech (on C-SPAN), good timing, some funny lines. And, being an ignoramus on science is probably par for the course for political pundits (cf. Gore's comment on the earth's core temperature).

The country is full of guys with emphatic political or cultural opinions who have broad public platforms (Tom Friedman, Deepak Chopra) but who are not exactly experts (or even literate) in any real field of scholarship. To give a recent example, CNN a couple weeks ago had Bill Nye "the Science Guy" on as an expert on global warming (for whatever it is worth, Nye believes in global warming, although he could not say anything coherent about the actual science).

D'Souza may be no worse than the others.

Dave Miler in Sacramento

27. **David H. Miller**
December 23, 2009

anon wrote:
> [N]one of his books that I've read are simple or bad enough to warrant a quick ad-hominem dismissal.

Please contact Dinesh's publisher and offer that comment as a blurb for his next book.

Dave

28. **Eric Baird**
December 25, 2009

If a "broad" interpretation of Multiverse theory allowed Heaven and Hell to be out there somewhere, then presumably it'd also have to allow Valhalla, Zeus's Great Hall atop Mount Olympus, the Elysian Fields, Samsara, Tian, and every other religion's special places.

So the mainstream Christian idea that there's only one true God, and only one way to get salvation, would be kinda broken.

29. **Tim vB**
December 26, 2009

When I read the interview Mr. D'Souza lost me with the very first point he tried to make: The concept of hell contradicts Freud hypothesis that religion may be explained by wishful thinking. Nobody likes the concept of hell? If there is a life after death, what happens with all the bad guys that lived rich and happy lives on earth? Surely you want them to be punished?! So of course you need a concept like hell if you want to sell your idea of a live after death!
Mr. D'Souza may have oversimplified his ideas, after all it is an interview – but he failed to convince me that he has anything interesting to say about religion or

30. **Tim vB**
December 26, 2009

…philosophy, and he proved that he does not understand theoretical physics at all…
(I accidentally submitted my comment before I had finished…)

Life in the Multiverse

December 17, 2009
Categories: Multiverse Mania

The latest Scientific American features a cover story on Life in the Multiverse: Could the strange physics of other worlds breed life? The magazine earlier this year fired a third of its staff and replaced its editor (the new editor has a column this month about the Multiverse and Star Trek).

I don't think one can blame the new editor for this though. Over the last few years, Scientific American has made multiverse pseudo-science stories a staple of its coverage of science. See for instance Parallel Universes, The String Theory Landscape, The Great Cosmic Roller-Coaster Ride and Does Time Run Backwards in Other Universes?

Update: Alejandro Jenkins and Gilad Perez, the authors of the Scientific American piece, pointed out to me something that I really should have made clear in this posting, that their arguments about the possible implications of a multiverse are of a different nature than those in previous Sci Am articles. They are arguing not for or against a multiverse, but against some popular anthropic arguments that try and explain the values of fundamental constants as being necessary for life. Anyway, my apologies to them for not making this clear, and here's something they sent me explaining in more detail their point of view:

> The title of our article in the current issue of Scientific American –as well the first bullet for the "Key Concepts" — might give the impression that our work argues for the reality of the multiverse, but this isn't really the case.
>
> Our research suggests skepticism about the usefulness of anthropic selection arguments when applied to particle physics (see the references below). The anthropic argument seems reasonably convincing when applied exclusively to the cosmological constant, as Weinberg did in 1987. But our own work shows that the parameters of the strong and weak interactions could be significantly different from what they are, without there being any obvious obstruction to the evolution of organic life. This means that the anthropic principle might not be enough to explain the microscopic laws of particle physics. In this sense, then, our story counter-balances the claims made previously by other experts about many of the parameters of the Standard Model being obviously "fine-tuned for life" (and therefore admitting an anthropic explanation).
>
> Our work is simply based on varying the parameters of the Standard Model and trying to understand how things would change from what we see in our world. It is true, though, that this intellectual exercise is motivated in part by the expectation from inflationary models (and from certain speculative proposals for the physics at the Planck scale) that the fundamental physics might produce many distinct universes besides our own.

Comments

1. **Tim vB**
   December 17, 2009

   The logic about the "anthropic principle" applied to blockbusters is sound. Harrison Ford as Dr.Kimble in the Fugitive didn't have luck, his story is the logical consequence of the fact that all the parallel Kimbles didn't make it to the theaters (would you watch a movie called "catched after 5 minutes"? See?).

2. **Pawl**
   December 17, 2009

   I notice the Parallel Universes link (referring to an article by Max Tegmark) contains the header

   > Not just a staple of science fiction, other universes are a direct implication of cosmological observations.

   I don't know if Tegmark himself makes this claim. (You have to pay to read the article, and, given that header, I'm not inclined to do so.)

3. **srp**
   December 18, 2009

   I read the Tegmark article when it was new, I think. It actually performed the service of distinguishing among the many versions of multiple-universe thinking at different levels of theory. One of them, if I recall, was pretty funny–a simple combinatorial argument about the number of atoms and possible arrangements versus the size of the observable universe that essentially applied the pigeonhole principle to show that everything should be duplicated somewhere in this universe.

4. **Belizean**
   December 22, 2009

   I think we need to distinguish between

   1) the multiverse postulated to resolve a conceptual problem in elementary quantum mechanics (for which there is abundant experimental confirmation), and

   2) the multiverse proffered to excuse the dearth of explanatory power in string theory (for which there is no experimental confirmation).

   You really need the original multiverse to explain how a quantum computer can perform a quantity of parallel computations that exceed the number of particles that comprise it (or even exist in our universe), or to make any sense out of quantum cosmology.

2010 LHC Schedule

December 17, 2009
Categories: Experimental HEP News

The LHC shut down yesterday for an end-of-year break after a very successful initial period of beam commissioning at beam energies of 450 GeV and 1.18 TeV. Tomorrow at CERN there will be public reports about the state of the LHC and the initial results from the experiments. I gather that by now all sorts of particles have been rediscovered, including kaons and lambdas, here are some details from Jim Pivarski.

There's now a tentative schedule 2010 out. Hardware commissioning of the new quench protection system, allowing beam energies up to 3.5 TeV, will begin on January 4, and be completed by February 15. A new checkout to prepare for beam commissioning will take place Feb. 17-19, and next injection of a beam into the LHC should be around February 20. Commissioning of 3.5 TeV beams and some pilot physics runs at that energy should take a month or so, with the first regular physics runs at 3.5 TeV/beam beginning around March 25. A tentative month-long shutdown to reconfigure the machine to run at higher energy (up to 5 TeV/beam) is scheduled for May 3-June 2.

From January 25-29 machine experts will meet in Chamonix to discuss whether to try and run at 5 TeV/beam in 2010, and how to implement this if it seems feasible. Plans will also be made for the late 2010-2011 shutdown. This will require deciding what to do about all the problematic splices in the machine in order to allow operation at the design energy of 7 TeV/beam, as well as understanding how much retraining of the dipoles will be needed in order to get to that energy. Current plans call for a "long shutdown" in 2013-4 to begin some upgrades of the LHC, and this is another topic that will be discussed.

While news coverage of the LHC in science magazines like Science News has been a mixed bag, often focussing on extra-dimensional speculation irrelevant to the actual science that will get done there, there's a quite good new article here, in a surprising location: Vanity Fair. The LHC has become a real celebrity…

Comments

1. **Ralph**
   December 17, 2009

   Which is more important – Increasing energy or increasing luminosity?

   There seems (a) there is a pretty good likely-hood of seeing interesting physicals already at 3.5TeV/beam, (b) seeming new physics at any energy is likely to require significant luminosity.

   So wouldn't it be sensible to push luminosity rather than energy for now?

   [Especially if one were parochial, and say, wanted to ensure beating tevatron to seeing the Higgs]

2. **Chris W.**
   December 18, 2009

   The author of the *Vanity Fair* piece, Kurt Anderson, is a novelist and journalist and host of the public radio program *Studio 360*, of which I'm quite fond. It nominally focuses on the arts, including architecture, but is very wide ranging, and almost always interesting.

   Somewhat off-topic: See this interview with Vanessa Gould, the director of the film *Between the Folds*, which was recently shown on PBS.

3. **Chris W.**
   December 18, 2009

   PS: From Vanessa Gould's statement about her film:

   > As a documentary project, this film has been less about telling a story and rather about finding an idea—layers of ideas. Everyone involved in this project has been incredibly energized by the challenge of making a documentary film about ideas. All along, we knew its central themes would speak to different people in different ways, as any film about ideas should. Therefore, it was of great importance that its themes were presented subtly and flexibly, so that every viewer could experience the film in ways that were both universally resonant and personally meaningful.

4. **Bill K**
   December 18, 2009

   "Which is more important – Increasing energy or increasing luminosity?"

   Depends on what you're looking for. In the case of a low mass Higgs (115 – 140 GeV) go for luminosity. You're actually better off running at reduced beam energy for this since the QCD background increases more rapidly with increasing energy than the signal. To search for more exotic things you obviously need enough beam energy to produce them in the first place.

5. **Steven Colyer**
   December 29, 2009

   Frank Wilczek gave an interview to the New York Times here. In in, he mentions experiments will be run at the LHC regarding Quantum Chromodynamics, for which of course he won the 2004 Nobel Prize in Physics.

   Also interesting and of note is a novel he is working on his spare time regarding four people who discover something significant, but Alfred Nobel's will (I think it was Nobel's will, if not it's just policy) allows for only three winners for any one award.

   Although the article doesn't mention it, could that reflect on Frank's award? Although he won the 2004 NPP along with David Gross and David Politzer, I'm curious if any of you know how close t'Hooft and Sid Coleman were considered for the same award. Thanks in advance.

   And darn it if that article doesn't make me want to explore the latest work in Axions. How's that going?

6. **NP Nonwinner**
   January 3, 2010

   Alfred Nobel's will was vague on details. Click on the link "excerpt of the will" (with English translation) http://nobelprize.org/alfred_nobel/will/index.html

See also Wikipedia, which quotes the paragraph, in English
http://en.wikipedia.org/wiki/Nobel_Prize

The full text of the will (English translation) is here
http://nobelprize.org/alfred_nobel/will/will-full.html

The details about the prizes were vague, so it took 5 years to sort out the legalities.
Nobel's will said nothing about "three people" and indeed it said "in the previous year" and it is says "benefit to mankind" (like confinement and asymptotic freedom in QCD?).
So …
a) the "previous year" business was set aside from the start
b) "benefit to mankind" was interpreted loosely. Mostly the Nobel Prizes are awarded for academic research. Thomas Edison contributed probably more benefit to mankind than any scientist, but he never won a Nobel Prize. (But Marconi won the NP.)

t'Hooft – it was known by all that he also derived the negative slope of the beta function in QCD (leading to confinement) but
it is also recognized that
– he did not make the connection to the (known) expt results on deep deep inelastic scattering (he did not pursue the consequences of the minus sign much)
– he already had a Nobel Prize (in 1999)

Sid Coleman? — do not know.

Has Dark Matter Finally Been Detected?

December 17, 2009
Categories: Experimental HEP News

No.

The CDMS experiment today reported the observation of two events, with an expected background of .8 events (I gather this is a 1.5 sigma result, but there is no arXiv preprint yet). Based on this, the Guardian [reports](#) that "Hunt may well be over for mysterious and invisible substance that accounts for three-quarters of mass of universe" and Science News has [Experiment Detects Particles of Dark Matter, Maybe](#). Science News quotes Craig Hogan as saying the results are "potentially very exciting", and the Guardian has "If they have a real signal, it's a seriously big deal." Unfortunately they don't have a real signal, so it's not a seriously big deal.

I just noticed that the [New York Times](#) is also covering this, but more soberly, describing the results as "faint hints". They do quote Gordy Kane, who describes the mood at the KITP in Santa Barbara as "a high level of serious hysteria", which he then embodies by claiming "It seems likely it is dark matter detection, but no proof."

For those unfamiliar with the terminology experimentalists use to characterize signals of various statistical significance, here's a summary:

5 sigma: discovery

3 sigma: observation

1.5 sigma: noise

Update: Scientific American gets it right [here](#).

Update: The paper is [here](#). It includes the information that "Reducing the revised expected surface-event background to 0.4 events would remove both candidates." There really literally is no signal here.

Update: Ethan Siegel's [blog posting](#) about this explains the appropriate scientific response to the news that two events were observed when .8 were expected:

> Well, La-dee Frickin' Dah!

Adrian Cho at Science has an excellent piece about the story: [Wimpy Evidence for Dark Matter Particles](#). He quotes two experimentalists who explain the significance of this (Richard Gaitskell: "Nobody should be attempting to say that this is evidence" for dark matter, and Edward Thorndike: "Absolutely not" an observation of dark matter). There's also this comment from theorist Joseph Lykken:

> Even so, Joseph Lykken, a theorist at Fermilab, says he's relieved that CDMS has finally seen something. WIMPs are predicted to exist by theories involving a principle called supersymmetry, which posits a heavy partner for every particle currently known. Had CDMS continued to see nothing, the results would have undermined those theories. So seeing something is better than seeing nothing, Lykken says.

Lykken seems to be ignoring the fact that the new CDMS results, two events and all, rule out yet more of the supersymmetry parameter space. For an explanation of this, written when the last CDMS results came out, already causing problems for supersymmetry, see Tommaso Dorigo's posting [SUSY more unlikely by the new CDMS II results](#).

Comments

1. **Van**
   December 17, 2009

   Actually, I think the correct answer to this question is 'maybe'. The events could be from dark matter, it's just not possible to say this definitively. However, considering the null results up to this point, these results are tantalizing and of interest. We'll have to wait a short while for better statistics, but it's very encouraging to be this close.

2. **Phil**
   December 17, 2009

   umm, " … three quarters of mass of universe … " ?
   That IS a sigma 5 observation the Guardian is making for the dark matter proportion in the universe isn't it ?

   p.s. – Sorry about overstepping bounds in a previous post, Dr. Woit.

   Thanks

3. **Amitabha**
   December 18, 2009

   I guess observation was ruled out as soon as Nature denied they would publish anything by CDMS today.

4. **Martin**
   December 18, 2009

   Oh well, the excitement is over. I had actually hoped for a bit more.
   Peter, do you know what caused these impatient scientists to publish a paper with such a low significance value?

5. **David B.**
   December 18, 2009

   Dear Martin:

   In experimental physics, you publish what you have. If a search comes out empty, you set new limits on detection, if you have some partial information, it could be noise or real signal. Either way, it must be reported. The grant agencies process can not let people wait for discovery before an announcement.

   And by the way, they were not impatient. The data is from a run starting on 2007, and they waited two years to see the events in the corresponding window where detection was possible. They did a blind analysis: they set up the complete analysis of systematics etc on the control region before opening the data in the discovery region. As far as I can tell they did a great job and presented it the right way.

6. **Peter Woit**
   December 18, 2009

   Martin,

   David B. is right, this is not a case either of impatient scientists, or of a paper that shouldn't have been published. Null results and somewhat better bounds on dark matter are very much worthwhile science.

   The only odd thing going on here is anyone portraying this as something other than a null result.

7. **SpearMarktheSecond**
   December 18, 2009

   Not quite a true null result… a true null result would be another zero, like (more or less) their previous 3 results.

   There are two good events in their signal box, which was defined blindly on calibration data. About a 23% chance the background fluctuated up. That summarizes the situation better than more qualitative things like `discovery' `observation' etc which is a terminology that has only started being used in the past few years, and are by no means universally accepted in particle physics.

   Obviously rumors leaked out that there were good events in the signal region this time. Given the importance of this experiment and result, it is easy to understand the excitement. Frankly, this excitement was more justified than all the excitement during the past 30 years about SUSY and string theory; at least this was a real experiment, carefully done.

   The paper is scientifically perfectly correct. Don't blame the authors for the excitement… the excitement arose naturally and actually is a sign that people still want real data and not mere mathematical virtuosity.

8. **Peter Woit**
   December 18, 2009

   SpearMarkII,

   Actually this is a null result. The two events go away if you tighten the cuts to the point where you don't expect any events (i.e. from .8 to .4 expected events).

   I'm all in favor of excitement over experimental results, be they negative or positive, and happy to have helped spread the unfounded rumors that led to some of this excitement. But when an experiment finds no evidence for something, having people going on about how it might really be something is not a good idea. Same as when a theoretical idea doesn't work out and having it promoted as still a possibility isn't good either…

9. **Coin**
   December 18, 2009

   So if more events are seen at the same point, then this will constitute evidence in favor of a dark matter particle?

   In the hypothetical situation where the results are not statistical noise, will having seen this negligible number of events now– and thus "knowing where to look"– aid CDMS or LHC researchers in any way by allowing them to conduct their experiment in a way that will ensure further signals at that point are not overlooked? (Alternately, is there risk that there will be an effect where since CDMS or LHC researchers "know to look there", they will be subconsciously more likely to commit experimental error that would reinforce the idea there's a dark matter particle there?)

   Even though the balance of the evidence is against the detection being real, is there value in theorists checking now to see which models are compatible with a dark matter particle at that mass, in case more events are seen?

10. **Gphillip**
    December 18, 2009

    I am excited and I can't help it. I'm excited people are interested in real physics. I'm excited we are debating real experimental results instead of multiverses and dreamworlds. I read three papers on multiverses this week and all three proposed different mechanisms and not any of them had any way to test their conjectures. Hats off to the CDMS team for conducting real science and reporting their results honestly.

11. **Peter Woit**
    December 18, 2009

    Coin,

    There's not much point in running the same apparatus much longer, you'd have to do this for quite a few years to get significantly better statistics than they already have. Better to put the time into building a more sensitive detector, and that's what they are doing ("SuperCDMS").

    I don't think the observation of these two background events helps one know what a real dark matter event would look like. All it does is tell you more about what the difficult to handle background looks like. Theorists have studied intensively the question of which models are compatible with the null results of this experiment and others. For more, see the old Tommaso Dorigo posting I linked to.

12. **SpearMarktheSecond**
    December 18, 2009

    Peter, hard to say. 2 events when 0.4 is the expected background is 2.3 sigma or so (single sided). Doesn't quite qualify as null. This experiment is in the uncomfortable in-between region, and folks want to editorialize one way or another. Rick Gaitskell is on a competing experiment (LUX), and Ed Thorndike at a LUX institution (Rochester). Lykken is at a CDMS institution. They all fall into line. You, Peter, are averse to excitement in the string theory community.

13. **Peter Woit**
    December 19, 2009

    SpearMarkII,

You have the numbers wrong, look at page 4 of the paper. The two events correspond to cuts with expected background of .8 +-.1 +- .2. They go away and there are zero events if the cuts are tightened and the expected background is reduced to .4

14. **Physicsphile**
December 19, 2009

SpearMarkTheSecond,
The CDMS paper says
"the probability of observing two or more background events is
23%"
In this case the distribution is not Gaussian so it doesn't strictly make sense to talk about sigma. But it is interesting to compare to the canonical Gaussian experiment of an assumed mean and standard deviation, sigma.
Then one says, OK if this was the true distribution and we took a sample, what would be the probability of drawing a sample with a value a greater distance from the mean than the observed one. If the probability is small, this leads you to start doubting the model. In this gaussian case, if you have drawn a sample 1.2 sigma away from the mean then there would be about a 23% probability of drawing a sample a further distance from the mean. So as a useful shorthand you can call a 23% chance of your data coming from background events as 1.2 sigma result. You may think its better to just stick to probabilities and that is fine but when you start talking about 3 sigma you are already talking about a probability of about 0.0026 which is starting to get a bit hard to visualize and once you start getting down to such small values you probably have a lot of data which means, by the central limit theorem, your distribution is probably pretty close to Gaussian anyway.

So you may say "well 23% thats less than 50% so it is a hint". But bear in mind there are lots of experiments looking for any one thing, so if you have 20 experiments, one of them is going to get a 2 sigma (5%) result on average. Also, it is generally not possible to eliminate systematics to beyond about a sigma so a two sigma result could easily be a one sigma result. So I think it really is worth having some reasonably strict criteria to when you are going to start taking an experimental detection seriously. I think a lot of theorists are too eager for new results though and that's why you get some fairly unwise comments in cases like this.

15. **SpearMarktheSecond**
December 19, 2009

Peter, I was responding to your statement that at 0.4 expected backround, the two events are cut. They are just barely cut, so if the cut is at 0.4001 or so, two events remain. Had the cut been placed there, this could have been a >2 sigma result.

The cut variations are all unfair, however, Peter, mine just above and yours. Your argument that tightening the cuts cut the events and therefore it is a null result is as bogus as my response that had the cuts been tightened to a point just before the two were cut it would have been >2 sigma.

My original point is what I stick too: it is not a null result (because 2 events in the signal region were seen) but not a significant result because 23% chance of background fluctuating. All the rhetoric of words is BS here. From my perspective, this one experiment is more valuable than all the theory of the last 20 years, however.

Physicsphilie, of course, but everyone translates from confidence levels to sigma because that is established shorthand, whether gaussians apply or not. And I think the real reason the field has come to demand 5 sigma etc is that we don't really trust our confidence interval calculations. In reality 3 sigma is just fine if we trusted the underlying understanding of the statistics. That we demand 5 sigma is a technique like in the very old days `multiplying by pi' to get a more conservative limit.

16. **Peter Woit**
December 19, 2009

SpearMarkII,

Where in the paper does it say that the two events remain at an expected background of .4001? You seem to just be making up numbers.

One other number that is in the paper is what happens if you loosen the cuts: no new event appear until an expected background of 1.7.

These numbers all correspond to exactly the sort of thing you expect to see if there is no real signal.

17. **Michael**
December 19, 2009

A statistical significance of 23% is very little, to be sure, and one could not call this "evidence" for a signal. Even if one supposes that the experimenters were conservative in they way they interpret their results (ie, with systematics, background estimates, etc.), this result is far from what one could designate to be a signal.

Are one or two of those two events actual dark matter scattering events? There is no way to tell at present. The only way to make progress is to take more data! Happily for all of us, the Xenon experiment will have relevant data rather soon, so we don't have to wait until SuperCDMS is ready and able to record new data.

So – if some of us think this is a null result, while others want to see a signal – what are the *predictions* for Xenon? 😉

18. **Peter Woit**
December 19, 2009

Michael,

Good point, someone should start circulating rumors about Xenon100, which may be reporting results in "early 2010", and is supposed to be an order of magnitude more sensitive than CDMS.

I don't know if they'll see anything, but, if they're 10 times more sensitive than CDMS, my prediction is that they won't see the 20 events you'd expect if the two CDMS events are real…

19. **teaser**
December 19, 2009

From the Xenon100 experiment page:

"A WIMP search in the current XENON100 will be first performed in 50 kg fiducial target with 40 live-days of exposure, to reach a WIMP-nucleon spin-independent cross section at 6 x 10-45 cm2 for 100 GeV/c2 WIMPs"

Is the expectation of a dark-matter candidate mass at 100 GeV/c2 based on cosmological estimates of "missing mass"? Also, is there a theoretical expectation for the cross section and is it possible that it's lower than what we think? If so, is there any way to know if we will ever detect a WIMP?

20. **Anon**
December 19, 2009

Unrelated to the post, but I think you'd find this comic funny:

http://xkcd.com/171/

21. **teaser**
December 19, 2009

I just read the CDMSII abstract and answered one of the questions I posted previously on cross-section:

The upper limit (based on current and previous data) for cross-section is 3.8 x 10 ^-44 cm2 for a WIMP mass of 70 GeV/c2. Is there any chance that the dark matter candidate has a mass greater than 100 GeV/c2 or is the lifetime to short to interact with other particles?

22. **Rien**
December 20, 2009

teaser: the mass can be both lower or higher, and the cross section can vary by orders of magnitude depending on what model for dark matter you're considering. Look at Fig 4 in the new CDMS paper. The colored area represents different parameters in the MSSM, so even within the MSSM the WIMP can have very different properties depending on in what part of parameter space you are.

23. **SpearMarktheSecond**
December 20, 2009

That the events are cut very suddenly at an expected background of 0.4 is in Lauren Hsu's talk at FNAL, see http://cdms.berkeley.edu/hsu_091217_FNAL.pdf , page 41. Still, the point is that the cut they used was determined blind and represents the fair value. Peter, your observations about loosening or tightening are hindsight and statistically biased.

Forgive me if I question your expertise, Peter, concerning what to expect about the behavior of a real signal. I doubt you've ever had to stand up and announce a new experimental result before a skeptical audience. Certainly your assertion that I make up numbers belies your noobility.

Michael… follow the impurities in Xenon100's LXe… doubt they're gonna get the sensitivity they advertise.

And if all the money spent on string theorists salaries and grants over the past 20 years had been spent instead on initiatives like CDMS, Xenon, LUX, Coupp, etc, science would have advanced one heck of a lot further in this area…

24. **Peter Woit**
December 20, 2009

SpearMarkII,

I have no idea who you are, although am quite willing to believe your expertise in this area is much greater than mine.

On the other hand, I think I have more than enough expertise to understand the numbers put out by this experiment. I don't question the accuracy of the numbers or the huge effort and expertise they represent. It's quite clear though what it means when an experiment looks for a signal that will revolutionize physics, expects one background event, instead sees two, and the two events go away when the cuts are modestly tightened (I don't see how the graph you point to has both events going away exactly at .4, but, whatever….).

25. **point**
December 20, 2009

They had a few little detectors? Why didn't they just have bigger detectors, and more of them?

26. **Peter Woit**
December 20, 2009

point,

money.

27. **jpd**
December 20, 2009

reminds me of a fellow grad student
TAing a lab class. one of his student
did an experiment with two data points
and wrote something like
'this proves the linear relationship'

28. **SpearMarktheSecond**
December 21, 2009

One event is cut at an expected background of about 0.35 and the second at an expected background of about 0.18, according to Lauren Hsu's plot. I suppose the neutron background should be added in, which probably raise those to about 0.43 and 0.26. Had CDMS chosen a tighter cut prior to unblinding, they easily could have ended up with a >2 sigma result, but not a >3 sigma result. Or, if they had chosen an expected background of 0.1, they'd have had a null result. They chose what they chose based on an optimization of their sensitivity that was done with no knowledge of their actual signal events… shown in some of the other CDMS talks this week. Very fair and very correct.

Then the unblinding of this CDMS exposure returned a result between null and significant. We all used to discuss, in the pre-blinding days, how results like this simply wouldn't get published… there was a strong publishing bias favoring true null results or truly significant results. Luckily blinding has helped allow the community to accept results in this in-between region.

Sure some people got too excited. Now other people are getting too cynical. I think the right way is to say, well, there is a 23% or so chance the background fluctuated, and so the CDMS results are interesting but not conclusive and not null either.

29. **chris**
December 21, 2009

Dear Peter,

amongst all the 'hype bashing' you do in this blog, i find the bashing of this particulat CDMS result as the most necessary piece. really.

it might seem as legit advertisind of the CDMS people to blow their result slightly out of proportion. well, this is how we (the trained scientists) react to it. 1.5 sigma is nothing. you don't have to say much more.

but the general public: oh my! you know, they trust us (scientists). and if these people go around telling their story about how they found 'dark matter' but there is a 25% chance that is was not dark matter, then guess how an average intelligent person reacts to that: oh, 75% chance that it was DM. and you know what: they are right. 1.5 sigma taken literally mean there is a 3:1 chance that this is the discovery of the century. i would get freakin' excited by that, too if i didn't know better.

if you think i am nitpicking or so, i am not. really. i spent a good part of the weekend trying to explain to laypeople why they should not get excited about a 75% chance of DM detection. and yo know what: almost nobody understood. and you know what's even worse: the people supporting me in this discussion were the 'einstien was wrong' nutcases and the 'all research is bad' types. the laypeople seriously interested in physics didn't follow me at all. and why? because they rightly invoked that these CDMS people know much better than i what they were doing and if they claim 75% chance, then this is so.

i am so depressed. seriously. i mean, what have we come to? where is the rigour of past days, where you would have to fight a steep uphill battle against an extremely conservative established opinion that you could only win with the correct theory as your companion.

i have somewhat accepted that nowadays you can claim multiverses, backward causation and all that experimentally unsupported nonsense if you include the small cautionary note that it is speculative. but it seems that this constant flow of the 'excitement' drug to the public has spoiled their perception so much that even hard-fact data based experimentalists can't resist the urge of pumping up their non-results instead of humbly reporting that they set better limits on DM. don't they see that in the long run this undermines the credibility of science? aren't they afraid about the long term effects of their claims, once DM limits have moved by a factor 10 and shown their results to be a fluctuation at the 5-sigma level? what do they say to joe the plumber then when he asks them about the 75% certainty they claimed?

i really hoped that experimentalists by their very nature would be cautious against hypes. i am really depressed about this.

30. **point**
December 21, 2009

point says: They had a few little detectors? Why didn't they just have bigger detectors, and more of them?

Woit says: money.

point: Hmmm, so I wonder what their approach would be to try to attract more money.

31. **Peter Woit**
December 21, 2009

SpearMarkII,

Your interpretation of that plot from the talk is in direct contradiction with the unambiguous statement in the paper on page 4 which I've quoted here earlier and pointed out to you:

"Reducing the revised expected surface-event background to 0.4 events would remove both candidates while reducing the WIMP exposure by 28%."

32. **no point**
December 21, 2009

Whatever detector one builds, it will always be `little' by the standards of the next generation.
I imagine that CDMS-II was not little when it was designed.
As I understand CDMS-I was at Stanford ('very little'?)
One demonstrates that the design and experimental concept are fundamentally sound (fundamentally WIMPY?)
and proposes a next-generation experiment.

33. **Physicsphile**
December 21, 2009

Chris, as far as I am aware the CDMS have not been hyping their result, that's been done by others not related to the experiment.

SpearMarktheSecond, usually in statistics you have the null hypothesis and the alternative hypothesis. You assume the null hypothesis and if your data are sufficiently badly fit you can interpret this as evidence for the alternative hypothesis. So in the CDMS case the null hypothesis is that there was no detectable dark matter (ie either dark matter does not have the properties to be detected by this experiment or there simply was no dark matter passing through this experiment) and the alternative hypothesis was that there was detectable dark matter. Assuming the null hypotheses one gets a 1.2 sigma result. Such a fit to the null hypothesis is not considered bad, the usual convention being that a 5 sigma or greater result is needed to refute the null hypothesis. Perhaps if it was 3 sigma I think you would be justified to refer to this as "CDMS results are interesting but not conclusive". I think it is more accurate to say the CDMS results are consistent with the null hypothesis that dark matter does not have the properties to be detected by this experiment.

34. **SpearMarktheSecond**
December 21, 2009

No Peter, it is an issue of significant figures (0.4 was quoted, not 0.40 or 0.400, meaning 0.3 to 0.5), consistent with not giving ultra-precise information so as to give the illusion of false precision and tuning of the cuts.

Physicsphile, sure, you're right, CDMS could not reject their null hypothesis. We can agree to disagree about whether 2 events in the signal box is interesting. Perhaps after Xenon, LUX, and Coupp have reported we'll know (assuming they can solve all their purity and discrimination issues)… all sorts of early observations are kept track of for years in the lore of experimental particle physics.

35. **dan**

December 21, 2009

Is the present non-detection of CDM at various detectors a problem for DM models, based on expected flux, probability, distribution, sensitivity?

36. **graviton383**
December 23, 2009

Dan, the answer is 'No'. Even in the somewhat limited supersymmetric DM model with neutralino LSPs the new limit from CDMS hardly makes a dent in the allowed parameter space. It only really starts to do so if the limit get about 1000x stronger..but that will be years from now. In the meantime the LHC will have a lot to say about most models with DM partcle candidates & is more likely to compress their parameter spaces if nothing is seen.

37. **dan**
December 23, 2009

thanks for replying. It sounds like LHC timetable won't be collecting statistically meaningful data for another 6-10 years out.

Until that time, isn't there time to build several thousands of these detectors?

Would increasing the size of the detectors, or increasing the number of detectors around the world, improve sensitivity? Would say having a null result from a couple thousand of these detectors all around the world, shielded from cosmic rays, neutrons, ground radioactivity, constrain it?

38. **Peter Woit**
December 23, 2009

dan,

I think the LHC time-table is shorter than this, with them collecting a significant amount of data at or near their design energy in 2012.

The cost of reproducing these detectors by the thousands, as well as installing and operating them around the world would be prohibitive. New, better detectors are being planned and built now, but the way they achieve higher sensitivity is by making the detector larger, not building a lot of smaller ones. Given the lead time necessary to design and build these things, the competition for the LHC is going to be those that are already being built and close to or already in operation.

39. **dan**
December 23, 2009

PW, thanks for replying. I was also thinking in terms of luminosity and collecting enough collision events to be statistically meaningful, say 3 bar. LHC by 2012 will have collection enough data by 2012 to discover DM and/or Higgs?

I understand that building larger detectors improves sensitivities, and I understand it is expensive and time consuming. What I had in mind is that other countries would pay for it, i.e 10+ built in Japan, Korea, China, India, Germany Britian, Italy, France, Russia, Mexico, Australia, would be paid by those respective countries.

Is there a reason that specifically the DM candidate neutralino has not been found in Tevatron? Is it that Tev lacks energy or luminosity?

40. **Peter Woit**
December 23, 2009

dan,

I think those countries aren't interested in spending more on this. Britain is planning on cutting spending on this kind of research, not increasing it.

The luminosity and energy you need to discover the neutralino depends on your model (supersymmetric theories have lots of extra undetermined parameters, and for different parameters you get different experimntal signatures). From what I can tell, immediately after the CDMS result was released, phenomenologists started writing papers calculating what the experimental signature would be for their favorite models, putting out almost half a dozen papers/day on the arXiv this week. It's a complicated business, consult those papers for the latest news on this topic.

41. **Stephen**
December 24, 2009

Already 9 papers on the arxiv on the cdms results…

42. **pushmepullyou**
December 31, 2009

Hey Woit,
Get back to work!

I'm kidding, and I'm too ignorant to comment on the subject matter here, but I enjoy reading NEW.

Best Wishes and I hope you aren't troubled by illness or sadness.

Two Books by Bloggers

January 3, 2010
Categories: Book Reviews, Multiverse Mania

The holidays are coming to an end, so expect a return soon to the usual somewhat irregular posting frequency.

Over the past week or two, one thing that I did was get a chance to read new books by two of the most prominent physics bloggers around: Chad Orzel (who has been blogging since 2002, now at Uncertain Principles), and Sean Carroll (since 2004, now at Cosmic Variance).

Orzel's new book is entitled How to Teach Physics to Your Dog, and he has a website with all sorts of material about the book here. I guess it's generally agreed that a cute dog improves just about any sort of material. While Brian Greene in his *Elegant Universe* Nova special introduced general relativity by trying to discuss it with a dog, concluding that "No matter how hard you try, you can't teach physics to a dog", Orzel takes a very different tack, structuring his book around conversations with his dog about quantum mechanics. The dog ends up with a solid intuitive understanding of quantum physics and presumably the idea is that the reader should be able to do as well as the dog. The book is a quite good, non-technical, exposition of some of the paradoxical aspects of quantum mechanics, emphasizing the subtleties of the relationship between the quantum and classical views of reality. His expertise in experimental atomic physics gives him an excellent understanding of these issues, and he does a good job of conveying some of this to the reader.

Among the best features of the book are enlightening treatments of the quantum Zeno effect, quantum tunneling, entanglement and quantum teleportation, as well as careful treatment of some crucial subtleties of the subject. If you want to go beyond the usual explanation that the uncertainty principle is about how measurements must change the state of a system, and find out how one can use quantum mechanics to measure a state without changing it, this is a good place to start.

By the end, I observed myself ending up in a linear combination of two possible states describing my feelings about the dog thing: about equal amplitudes for charming and annoying. Even now that we're in a different decade, I haven't yet collapsed into one state or the other.

The other new blogger-book is Sean Carroll's From Eternity to Here, which has its own website here. I confess to being somewhat mystified by this book, and a bit surprised by its contents. Carroll is a very smart guy, with a serious dedication to making the wonderful science of his professional field (cosmology and particle physics) accessible to the general public. Given this, my expectation was that the book would be mainly devoted to telling the conventional scientific story of some part of our current understanding of these subjects, with perhaps a more positive take than mine on the possibility of exciting new discoveries in the near future. I also expected him to include some material on his highly idiosyncratic ideas about the arrow of time.

It turns out though that this rather long book is heavily oriented towards making the case for unconventional claims about physics, with essentially no discussion at all of what is happening on the experimental side of the subject. The LHC appears only in a footnote explaining that it won't destroy the earth, and there's virtually nothing about the hot topics of dark matter, gravitational waves, or the cosmic microwave background. In a final footnote, Carroll explains that he decided not to write about these experiments because

> it's very hard to tell ahead of time what we are going to learn from them, especially about a subject as deep and all-encompassing as the arrow of time.

Carroll's problem is that the questions that he has chosen to highlight in the book may be "deep and all-encompassing", but they're of a sort one might describe as "philosophical" rather than scientific. Much of the book is devoted to arguing that in order to understand the local (in time) question of why entropy increases, one must understand the global puzzles pointed out by Roger Penrose associated with gravitational entropy, the Big Bang and inflation. More succinctly, the explanation for why an omelet doesn't turn into an egg somehow involves understanding the Big Bang.   Even after reading the book, I remain unconvinced that the global problem has to be solved to explain the local problem, and unfortunately there's no scientific way to resolve my difference of opinion with the author. No conceivable experiment can provide evidence one way or another about which of us is correct.

After making the case that one needs to understand the low entropy of the early universe to understand everyday physics, Carroll goes on to propose his own theory, the "Ultimate Theory of Time" of the book's subtitle. It's a version of the usual "multiverse" argument: one explains some mysterious distinctive feature of the universe by positing that we live in a multiverse without this distinctive feature, which just occurs as a dynamical accident in our particular universe. The problem is that this particular explanation is not a conventional scientific one, since it is immune to experimental investigation, and, as far as I can tell, few physicists take it seriously. Carroll's one scientific paper on the subject, (written in 2004 with his graduate student Jennifer Chen) received a lot of publicity on the internet and in Scientific American, but doesn't seem to have yet been published, despite being listed on his CV as submitted to Phys. Rev. D.

The book seems likely to get a lot of public attention, but I'm not sure this is a good thing for the public understanding of science. It raises fundamental issues in physics, which naturally attracts people's interest, but then addresses them in a rather post-modern yet pre-scientific manner, avoiding contact with either mathematics or experiment. Probably the best way to think of *From Eternity to Here* is as an extended essay in the philosophy of science, and as such I'd be curious to hear what philosophers expert in the subject make of it.

Update: Scientific American has an interview with Carroll, in which he addresses objections like mine as follows:

> The following statement is very true: To understand the second law of thermodynamics, or how the arrow of time works in our everyday lives, we don't need to ever talk about cosmology. If you pick up a textbook on statistical mechanics, there will be no talk about cosmology at all. So it would be incorrect to say that we need to understand the big bang in order to use the second law of thermodynamics, to know how it works. The problem is, to understand why it exists at all requires a knowledge of cosmology and what happened at the big bang.
>
> Once you assume that the universe had a low entropy for whatever reason, everything else follows, and that's all we ever talk about in textbooks. But we're being a little bit more ambitious than that. We want to understand why it was that way—why was it that the entropy was lower yesterday than it is today?
>
> To understand why the entropy was lower yesterday really requires cosmology. And I think that if you sit down and think about it carefully there is absolutely no question that that is true, yet a lot of people don't quite accept it yet.

After having sat down to think about it carefully, I still don't quite accept it…

Comments

1. **wolfgang**
   January 3, 2010

   >> It raises fundamental issues in physics, which naturally attracts people's interest, but then addresses them in a rather post-modern yet pre-scientific manner

   I would think this is true of (almost) all popular books about physics (including your own)? They are not research papers but rather more or less interesting stories about science.

2. **Domenic Denicola**
   January 3, 2010

   Although I haven't been through Carroll's blog posts on arrow of time recently, I do recall them being pretty sensible and agreeing with the general consensus at the time that I read them. That is, it's general accepted that you need to explain why entropy was low at the beginning of the universe: the Second Law of Thermodynamics alone does not suffice, without the addition of a Past Hypothesis. There's a neat argument showing this that involves phase space that I'm sure you've seen?

   Maybe you could explain more what kind of statements you found unlikely? (Apart from the multiverse "ultimate theory," of course.)

3. **anon**
   January 3, 2010

   > but doesn't seem to have yet been published, despite being listed on his CV as submitted to Phys. Rev. D.

   See arXiv:gr-qc/0505037.

4. **Franca**
   January 3, 2010

   Peter,

   thermodynamics books say that entropy is a concept defined for physical systems. A system is a set of matter enclosed in a boundary. But is the universe really a physical system? Many say yes, but is this really so? I remember a friend saying that Weinberg's opinion is that the concept of entropy is not applicable to the universe. If that is true, all discussions on the topic are not serious. What is the the "official" view on this topic?

5. **Adam Helfer**
   January 3, 2010

   I haven't read Carroll's book, but I don't see that the arrow of time enters in explaining why omelettes don't turn into eggs. That would be going from a larger to a smaller region of phase space, which is improbable unless either the dynamics or the initial conditions are very special, no matter *which* way one moves in time. (Maybe there's another idea here I'm not appreciating, though.)

   The mystery of the arrow of time (in this context) is rather why there are eggs (very low-entropy objects) in our common experience. Basically, that implies that there must have been still lower entropy in the past. That does indeed lead back to questions about the early Universe. It also inevitably leads to global issues.

   In the standard hot big bang, of course, the material degrees of freedom are thermalized. But the degrees of freedom corresponding to gravitational radiation are almost entirely unexcited — in fact, it is precisely this which is required for the homogeneity and isotropy of the Universe. Thus while the matter is in a high-entropy state, potential gravitational radiation is in a very low entropy state, and this very low entropy is moreover what provides the entire Friedmann–Robinson–Walker background (on which the evolution of galaxies, stars, chickens and eggs is described).

   Notice that the global character of the conditions — the homogeneity and isotropy — is intimately bound up with the low-entropy character of the gravitational degrees of freedom. This is contrary to what happens in more familiar thermal situations, where a homogeneous system is typically a high-entropy state. The difference is due to the attractive nature of gravity.

   (I know this is a bit lengthy, but the question is an involved one.)

6. **Tom O'Bulls**
   January 3, 2010

   "More succinctly, the explanation for why an omelette doesn't turn into an egg somehow involves understanding the Big Bang."

   I haven't been able to get hold of the book yet, but I'd be amazed if it contains any such claim, because that would directly contradict SC's numerous explicit statements to the contrary in his blog postings. There he says that the usual probabilistic arguments explain why high entropy states [almost] never evolve into low entropy states. *But* if you accept this, then you are obliged to explain where low-entropy states observed in our universe came from. The answer, of course, is that they came from something about the big bang that we don't yet understand.

   As for whether physicists take SC's ideas seriously: you have to take into account the *extremely* mysterious fact that physicists are apt to get very emotional about this issue; and I'm not just talking about cranks like Lubos Motl. Many people get upset when told that something as basic as the second law of thermodynamics is not fully understood. Partly this is due to "cosmology envy" — some people working at the more boring end of physics resent the idea of those pointy-headed cosmologists muscling in on their territory.

   Anyway, it is certainly not the case that SC's ideas are rejected because they challenge some orthodoxy; there *is* no orthodoxy on this matter. Common sense tells us that the second law implies that the universe must have begun in an extremely low-entropy state, and that this has to be explained if we are to make any sense of early-universe cosmology. But most people just don't want to think about this.

7. **Peter Woit**
   January 3, 2010

   anon,

   The preprint you mention is a 5 page non-technical essay for the Gravity Research Foundation essay competition.

   Domenic (and others),

   I don't disagree that to understand cosmology you want to explain the low entropy at the Big Bang. I just continue to not see why this explanation, whatever it is, is necessary to explain the 2nd law of thermodynamics. I'm not the only one (the book's introduction contains another example), and the problem seems to me to be that questions like this inherently can't be adjudicated by any conceivable experiment. Arguing about them thus tends to be a rather pointless activity. We're really in the realm of philosophy of science here, not science.

   "Tom O'Bulls",

   From your IP address it seems likely you're one of the people on the very short list of physicists known to have some sympathy for Carroll's arguments about the multiverse. I really wish you and others would use your real names here, especially if you intend to make arguments about what your colleagues think of all this.

8. **Peter Woit**
January 3, 2010

For those asking for an explicit example of a statement from Carroll that I disagree with, a good one is his Facebook summary of the book's argument:

"You can turn an egg into an omelet, but not an omelet into an egg. This is good evidence that we live in a multiverse. Any questions?'

9. **Ricci's Pieces**
January 4, 2010

Hi Peter,

As a mathematician, I am extremely uncomfortable with some of the multiverse claims by Sean Carroll. I think his textbook is marvelous, it was the first textbook I used to learn General Relativity, but I find the multiverse concepts meta-physics at best and it is a bit disheartening to see, let's face it, one of the more popular physicists so die hard about it.

10. **Bee**
January 4, 2010

I find it interesting to lead a discussion in a book rather than scientific papers that are (for one or the other reason) inaccessible for most people. As long as it's clear to the reader which part is backed up by solid science and which is speculation. It's a much less constrained medium that offers more freedom. Didn't read either book though, so can't say much.

Does it really bother you that the paper didn't get published?

Btw: Happy New Year 🙂

11. **Lee Smolin**
January 4, 2010

I look forward to reading Sean's book, but I am confused by some of these accounts of the issue. Even if the entropy of the universe as a whole can be defined (which I am confused about) and increases over all, the important thing for eggs, chickens and cooks is that nevertheless there are large regions far from equilibrium today.

This is in turn due to the fact that there are stars with cosmological life times, which pour hot photons into cold space. The existence of stars is due to both some fine tuning of the parameters of the standard model and the fact that gravity is universally attractive and long ranged so that gravitationally bound systems have negative specific heat.

The fact that stars exist is likely not due to the extreme homogeneity of the early universe, were the initial density fluctuations quite a bit larger I suspect there still would have been stars, possibly a higher density of them, and sooner.

Nor is there a simple thermodynamic argument that applies to gravitationally bound systems or the gravitational degrees of freedom. The canonical ensemble cannot be applied to systems with negative specific heat such as gravitationally bound systems. It can also be shown, given only that matter has positive energy, that gravitational radiation cannot come to thermal equilibrium within any finite time, nor can it be contained by any material within a finite volume. So it is unclear that equilibrium thermodynamics is ever relevant for the gravitational degrees of freedom.

Thanks,

Lee

12. **Adam Helfer**
January 4, 2010

Peter,

I think there is an issue which is physics (whatever its implications for philosophy), that is, to explicate the Second Law and its relation to the arrow of time. I also think that while it is wise to be wary of discussions which cannot lead to experimental verification, what we're doing at this stage is rather trying to clarify our understanding of some of the theory. Hopefully a deeper understanding will suggest experiments.

Let me begin with two possible formulations of the Second Law:

1. Systems tend to move from smaller volumes of phase space to larger ones (or tend to spend more time in larger volumes than smaller ones);

2. Over time, entropy increases.

The point I want to make is that the first one admits a time-symmetric interpretation (indeed, the form in parentheses is explicitly time-symmetric), while the second does not. Thus the first is insensitive to the arrow of time, but not the second; they really reflect slightly different concepts. The second form captures something that the first form misses, precisely because it does implicate the arrow of time.

I would also say that the first statement is plausible and largely an intuitive statement about probabilities. (Of course, subtle questions about issues like coarse-graining are hidden in its implicit assumption that we have selected certain phase volumes.) So I would contend that the thing to be explained about the Second Law is the "something extra" which is added in passing from the first to the second version.

What has been added is brought out most forcefully by viewing the movie backwards: going backwards in time, we find entropy decreases. That is what is puzzling. So the puzzle of the Second Law is why the initial entropy was so low. And this is a cosmological issue.

I appreciate that it seems that there is a considerable leap between everyday thermodynamic phenomena and the early Universe, and I think this is perhaps why their linkage seems hard to accept. However, this is really because we are highly prejudiced about what we consider to be "everyday thermal phenomena." Had the Universe been a more "random," average (from the phase-space view) one, it would look nothing like ours. Stars and galaxies would probably not exist (or would be rare). The fact that we can build heat engines and refrigerators (and, more spectacularly, that chickens and eggs — the original points of discussion — exist) is really due to the low entropy we still have available to us to exploit, and this ultimately came from earlier — cosmological — times.

13. **H-I-G-G-S**

January 4, 2010

This is too funny. Peter "my credentials don't matter" Woit is drawing conclusions about a commenters views on physics based on their IP address.

HNY
PH

14. **Peter Woit**
January 4, 2010

Bee,

I don't know why the the paper hasn't been published, so it's unclear what the significance of that is. I am curious though what the reaction to it is from experts in the subject. The book does acknowledge that some of what is in it is not widely accepted by other physicists, but doesn't really explain why.

15. **Adam Helfer**
January 4, 2010

Lee,

I agree with most of your points. You'll notice that I have phrased things in terms of phase-space volumes and not (except for small systems, and a reference to the conventional treatment of the hot big bang) in terms of thermodynamic entropy and equilibrium. I acknowledge that this means dodging or postponing certain questions — but that's often what allows one to make progress.

16. **anon**
January 4, 2010

A rather trivial comment, just for the record. As someone else once pointed out in this blog, turning omelettes into eggs is almost as trivial as the inverse process. Just finely chop the omelettes and feed a hen with them, she will turn them back into eggs, increasing the overall entropy of the barn with heat and waste in the process…

17. **Peter Woit**
January 4, 2010

Adam,

I understand the argument, but it just doesn't seem to me to provide a significant coupling between the global cosmological problem and the local physics problem. According to that argument, whatever the solution to the global problem turns out to be, it's not going to affect in any way our understanding of the local physics, and no possible local experiment is going to help solve the global problem.

By the way, the contrast to Penrose is interesting. Penrose has been pointing out the big bang entropy problem for a long time, but, unlike Carroll, it leads him to speculative ideas that do directly affect local physics (modifications of quantum theory with potentially observable local consequences).

H-I-G-G-S,

I've been repeatedly struck by how much lower the level of discussion is on physics blogs than on mathematics blogs. One major difference is that serious mathematicians participate in blog comments using their own names, and I think this has a lot to do with the problem (and you're a good example of it….)

18. **Christine Dantas**
January 4, 2010

What is Carroll's definition of gravitational entropy? Under which constraints is it extremized?

If one goes back to the very basics of gravitational relaxation from the point of view of statistical mechanics, several issues arise, as mentioned by Smolin, which I fully agree. [A brief review (with relevant references) can be found, e.g., in section 4.2 of astro-ph/0604544.] The point is: those issues arise in a very well-studied subject.

[There are in fact open questions. For instance, if one goes back to the very basics, as my paper above points out, mesoscopic energy constraints may be operating during gravitational relaxation. This has been discussed previously by other authors, like e.g. Kandrup et al. If it turns out that this effect is confirmed in observations, then it is just an example of why one should be careful concerning gravitational entropy and so on.]

Given that there are still down-to-earth issues that still must be elucidated in this beautiful area of real physics, it is not clear to me the logic to adhere to speculations such multiverses in order to reach whatever conclusions about whatever.

19. **neo**
January 4, 2010

Somewhere, out there in the multiverse, someone or something is turning omelets into eggs, as we speak.

20. **Bee**
January 5, 2010

The second law of thermodynamics does not say the entropy (of an isolated system) increases, but the entropy does not decrease.

Peter: Is Sean's book well written? Like in: worth reading or will catch dust?

21. **Thomas R Love**
January 5, 2010

Peter wrote:

"I've been repeatedly struck by how much lower the level of discussion is on physics blogs than on mathematics blogs."

Some how almost everyone feels qualified to comment on current physics, but few feel qualified to discuss mathematics. I saw this happen in an undergraduate course on The Philosophy of Science during a discussion of quantum mechanics. After some very wierd comments were made by several members of the class, I asked the class to raise their hands if they had taken a course on QM. Mine was the only hand raised. Not even the instructor had taken QM. But they were more than willing to discuss topics about which they were ignorant.

22. **Adam Helfer**
January 5, 2010

Peter,

Yes, Penrose deserves a great deal of credit for bringing the cosmological problem forward (and I know what I've written here is certainly influenced by his ideas). One used to hear cosmologists talking about the high entropy of the early Universe, with no mention at all of the gravitational degrees of freedom. I haven't read Carroll's book, but I believe that in the past he has acknowledged Penrose's influence.

I agree also that Penrose's speculations about links to observational physics are very interesting. While I do feel that there is an argument that cosmology does call for some reconsideration of the usual notion of experimentation (one cannot expect to produce, much less reproduce, phenomena on truly cosmic scales), one should approach this very cautiously, since one risks throwing out the foundation for scientific progress. I think something like Penrose's Weyl Curvature Hypothesis is an excellent example of an idea which could in principle be testable but nonetheless have cosmological implications.

Bee,

You are right, that is the standard formulation; I probably should have phrased it that way to avoid confusion. (But the distinction has no effect on the argument. Also it is arguably mathematical hair-splitting, since: no real system is perfectly reversible; the entropy is perfectly well-defined as a precise real number only under idealized assumptions which do not hold for real systems; etc.)

23. **Peter Woit**
January 5, 2010

Bee,

The book is well-written, and people may find lots of the various things discussed useful. My problem is just with the main thesis that the book is structured as an argument for.

24. **Peter Woit**
January 5, 2010

Thomas,

That's part of the problem. But there's a further problem with physics blogs that a significant number of the informed people with reasonable credentials who post comments have decided to do so anonymously. Cloaked in anonymity, people tend to take a lot less care about what they write…

25. **Bee**
January 6, 2010

Adam: In what sense does the whole universe conform to your idea of a "real, not idealized system?"

26. **Bee**
January 6, 2010

Peter: Thanks. I think I'll put it on my reading list. There is a nonvanishing chance I'll open it before the end of the century.

27. **Adam Helfer**
January 6, 2010

Bee,

The Universe is certainly a real system. The sense of "idealized" which is relevant here is whether the entropy can be well defined. The Universe is certainly not a system in thermal equilibrium, so it's certainly very far from "idealized" in the usual sense one wants for such a definition.

The arguments I gave earlier are based on using (logarithms of) phase-space volumes as substitutes for entropy. But that doesn't really sharply define entropy, because of the question of just where to cut off the phase-space cells (the coarse-graining issue). Only in certain idealized limits does this ambiguity in the phase-space approach contribute zero uncertainty to the definition.

28. **Bee**
January 7, 2010

Adam: What I was aiming at is that unlike all other systems the universe is perfectly isolated. Yes, you have to coarse-grain. But is that fundamental?

29. **Adam Helfer**
January 7, 2010

Bee,

Since we don't really have a really firm idea of how to define the entropy of the Universe, I don't think we're in a position to talk about what is fundamental. For the purposes of the arguments I was giving, some sort of coarse-graining is essential, because one needs to decide when two different states count as macroscopically the same.

It's true that one might try to consider extending the idea of entropy so as to treat the entire Universe in some "fundamental" sense. However, this is a very big problem, and it's not clear that it would have a solution. In fact, it's hard to see how one would define the entropy of an ordinary (say classical mechanical) system which was not in thermal equilibrium.

30. **Bee**
January 8, 2010

Adam: If the problem is not fundamental, why is it worrisome? What you say is exactly what I meant: you have to decide what different microstates count for the same macrostate, that determines your entropy (leaving aside all other problems for the moment). This notion stems from thermodynamics and that's where it has its use, but what's its use for the whole universe, a system that by definition is all there is and exists once in exactly one microstate?

31. **Adam Helfer**
January 8, 2010

Bee,

I'm not sure what your point is. If you are arguing that entropy is not a useful concept in its application to the entire Universe, that's pretty much my point of view. If you are asking about why concerns about the Second Law are fundamental, I'd say that the arrow of time is a fundamental issue — but I'd add that understanding the link between the Second Law and the arrow of time may well take us beyond the bounds of the strict applicability of the concept of entropy and ordinary thermodynamics.

32. **Bee**
January 9, 2010

Adam: Yes, is what I'm saying. I think we agree anyway.

33. **Maynard Handley**
January 10, 2010

Here is, I think, a more precise way of stating Bee's point.
Entropy, (IMHO) carefully defined, is a property of probability distributions (or, if you prefer) measures.
We can connect it to physics because, for blocks of space containing certain fields and subject to certain boundary conditions, we can construct an argument for what the PDF of the system should be, and we can calculate the entropy of that PDF.
(This requirement, well-defined PDF as a property of a few variables is also why we need thermal equilibrium. If you're going to have a non-equilibrium system, in theory you COULD define/assume/calculate an associated PDF, and this might even work for something like turbulence, but good luck doing it for a general situation. We can get away with this for the most part because when you're dealing with a steam engine or whatever, the number of coherent degrees of freedom is so small compared to the number of incoherent degrees that treating the system as in equilibrium basically works OK.)

When you expand this argument to the entire universe it basically falls apart. What is the PDF of states of the universe? What are even the allowed possible states, let alone a claim regarding what probabilities to assign to each possible state?
But if you don't have a PDF, you don't have an entropy.

Untangling String Theory

January 7, 2010
Categories: This Week's Hype

The Times of London recently sent one of its reporters out to a pub to learn about string theory from Michael Green, with results available on-line here. Green does a good job of trying to explain some physics over a few beers, and admits that:

> I think, historically, when there has been a big change in a theory there is usually some qualitatively new phenomenon which will distinguish the theory. This has not happened for string theory, which is one of the reasons some people wonder whether it is real physics.

There's an associated slide show that supposedly gives a step-by-step guide to string theory. It explains that there are ten extra dimensions of very small size, necessary because:

> Beautiful as the idea sounds, when string theory is applied in the ordinary three spatial dimensions it doesn't work mathematically, predicting the wrong numbers for constants such as pi and the speed of light. It also predicts that the whole Universe should disappear.

I do wonder what string theory's prediction for the value of pi is…

According to the Times, the LHC has something to do with all of this, since:

> Scientists hope that the smashing together of particles at the Large Hadron Collider may reveal hints of the strings lying within them.

Over at the Los Angeles Times, Steve Giddings somehow neglects to mention string theory while arguing that the LHC

- "could open new frontiers in understanding space and time"
- "might produce dark-matter particles so we can study their properties directly and thereby unveil a totally new face of the universe."
- "might also shed light on the more predominant 'dark energy,' which is causing the universe's expansion to accelerate."
- "may reveal … the existence of a completely new type of dimension — what is called 'supersymmetry'".
- "may find evidence for extra dimensions of a more ordinary type, like those that we see — still a major revolution."
- "could produce microscopic black holes."

In case all of these discoveries seem a bit abstract and useless, there's the possibility of

- new energy sources, means of space travel or communication, or amazing things entirely unimagined.

Over at Uncertain Principles, some of the Giddings arguments about spin-offs leave Chad Orzel rather grumpy.

Finally, also on the nothing-to-do-with-string-theory front, New Scientist has an article about this paper from Science, where the authors find some sort of relation I don't understand between a representation of E8 and some phenomenon at the critical point of a quasi-one dimensional Ising ferromagnet.

> Although E8 does show up in string theory calculations, observing the symmetry in magnetic crystal experiments does not provide any evidence for string theory itself, Konik says.
>
> "The fact that you see this particular symmetry in this spin chain doesn't say anything about string theory per se," he says.

Comments

1. **anon**
   January 7, 2010

   your link to the times article gives a 404 error page

2. **Peter Woit**
   January 7, 2010

   The London Times first link sometimes requires a reload to work properly for me, and the second link sometimes doesn't work at all (but it's a pop-up which can be found on the main story page, if you can get to that…)

3. **Ignatz Thugg**
   January 7, 2010

   Is it worth $15 to read the Science article?

4. **Peter Woit**
   January 7, 2010

   Ignatz,

   I very much doubt it, unless you have very specific and unusual research interests in quantum criticality and no other way of getting access to a copy of Science.

5. **Bee**
   January 8, 2010

   "We observe only three spatial dimensions in everyday life, but what if, on the subatomic scale, there are tiny, curled up, extra dimensions, deeply embedded within space, and so small that they can't be seen? What if there were ten of them?"

   The explanation is badly formulated, but I believe "them" in the last sentence refers to "dimensions" and not "extra dimensions." They don't say one needs 10 **extra** dimensions of very small size as you write.

6. **Paul Wells**
   January 8, 2010

If you define pi as the ratio of circumferance of a circle to its diameter and if the space is non-Euclidean I can see why this comment might make sense – especially in the context of a popular article where you might not want to go into details of space-time curvature etc.

7. **Tim vB**
January 8, 2010

@Paul Wells
Sure, but that would be a phenomenon of non-euklidean geometry resp. of general relativity, this has nothing to do with string theory.
And of course the definition of pi is and stays to be the "ratio of circumferance of a circle to its diameter in euklidean space time", if you insist on a geometric definition (cumbersome).
If you leave out the "in euklidean space time" you don't get the definition of a constant.
I think the author needed another "constant" beside the velocity of light to get the rhetorical figure "enumeration", and pi was the first "constant" that came to mind, from the remnants of highschool.

8. **Steve myers**
January 8, 2010

Since it is common in checking smoothness of a fixture table by comparing circumference to diameter of several "circles" it is best to treat pi as a constant defined by something like 4 * Integral (0 to 1) of dx/(1 + x^2). I guess I never realized the power of string theory — it can change math constants. Is Euler's number next? (Note: we also check the eccentricity of rolls of material by seeing how close the ratio approaches pi.)

9. **Will Orrick**
January 8, 2010

The E8 work was inpsired by A. Zamolodchikov's results on integrable massive perturbations of conformal field theory. One of the simplest conformal field theories is the minimal model M(3,4) which corresponds to the critical point of the two-dimensional Ising model (or an equivalent one-dimensional quantum spin chain). What the authors have apparently done is to realize such a spin chain experimentally. There are two integrable perturbations of M(3,4), one of which corresponds to moving away from the critical temperature, the other of which corresponds to turning on an external magnetic field. Zamolodchikov discovered a connection of the latter to E8 which can be used, for example, to compute the mass ratios of the fundamental excitations in the massive theory. I have not yet read the Science article, so I'm not sure what aspects of this picture the experimentalists were able to measure.

10. **Anonymous**
January 8, 2010

"I do wonder what string theory's prediction for the value of pi is…"

Perhaps this might help:

http://arxiv.org/abs/0903.5321

11. **petergreat**
January 8, 2010

I think arXiv needs to add a rating system like in Youtube, so papers like this will be discouraged.

12. **Bob Levine**
January 8, 2010

"I think arXiv needs to add a rating system like in Youtube, so papers like this will be discouraged."

@petergreat: this is a terrific bit of *satire*. Look at the footnotes:

[11]I could have checked the original references, but it was
easier to look everything up on Wikipedia.
[12][And who isn't? (with the passage in text footnoted as [12] reading, 'Inspired by string theory[12]'…

Reread it for what it is: a great spoof on the kind of efforts that we're always reading about on Peter's blog…

13. **Tim vB**
January 8, 2010

ROFL!
(The author missed the pi=3 statements from the bible.)
It's very telling that
a) someone wrote this and uploaded it to the arxiv,
b) I would add here that someone did not notice that it is a parody, but I'm not so sure: @petergreat, did you perhaps mean you wanted parodies to be discouraged?
c) someone felt the need to point out that this is indeed a parody.

You know, it's really up to you if you get angry, cry, run away or laugh. I'm glad that some people out there chose to laugh.

14. **The Lone Haranguer**
January 9, 2010

(The author missed the pi=3 statements from the bible.)

He was wise. See http://www.math.ubc.ca/~israel/bpi/bpi.html, On The Rabbinical Exegesis of an Enhanced Biblical Value Pi, of where the author shows that a value of 3.1415094…, accurate to 3 parts in 200,000 can be derived.

15. **Tim vB**
January 9, 2010

Interesting! Finally some numerology that makes sense.

I may be a sceptic of numerology, espacially since I read the novel "Foucaults Pendulum" by Umberto Eco, but in this case I believe it.

So let me correct my statement and say: The author should mention the Hebrew value of pi according to verse 1 Kings 7:23 of the Bible.

16. **Thomas Larsson**
January 9, 2010

    April 1st 2009 was a Wednesday. Why did he upload already on Monday?

17. **Chris Oakley**
January 9, 2010

    Thomas,

    If he posted it from a computer travelling close to the speed of light, or close to a Black Hole, it may have been April 1st in his frame of reference.

    Who knows – maybe there are frames of reference (near a cosmic-scale superstring?) where it is always April Fool's day.

18. **Thomas Larsson**
January 9, 2010

    Chris, I think the explanation is simpler than that. IIRC, the deadline for submission to the arxiv is midnight GMT. If Scherrer made his submission between 0 and 6 am GMT on March 31, it would be received and time-stamped at Cornell on March 30, but appear in the April 1 listing in Germany.

19. **Chris W.**
January 9, 2010

    NPR has some coverage of the research described in that *Science* paper.

20. **Bob Scherrer**
January 11, 2010

    Papers need to be submitted a day early in order to appear on Apr. 1. If you look back for several years previously, you will see that several spoofs get published on the arXiv every Apr. 1. The arXiv administrators are obviously aware of this and have tolerated it thus far.

Simons Foundation News

January 8, 2010
Categories: Uncategorized

The latest AMS notices has the [news]() that the Simons Foundation is now spending about $40 million/year in mathematics and related theoretical fields. This is being done under a program being run by David Eisenbud at Berkeley, and the first initiative has been the funding of [new postdoctoral fellowships]() (there's an earlier posting about this [here]()). How the rest of the money will be spent remains undecided, with a request going out for suggestions.

This fall the program will fund 15 mathematics and 10 theoretical physics 3-year postdocs, as well as 9 2-year postdocs in computer science. A similar number of new positions will be funded next year. The postdocs will pay very well, at 70K/year for the mathematics ones, 65K/year for those in physics and "gauged to attract the highest caliber of applicants" for computer science.

The departments chosen for the postdocs have not been officially announced, but a little googling turns up the following ones that have job ads specifically mentioning the Simons fellowship.:

Mathematics: Berkeley, Cal Tech, Cambridge (UK), Columbia, Harvard, Michigan, MIT, Northwestern, Stanford, Texas (Austin), UCLA, Yale

Physics: Berkeley, Cal Tech, Chicago, MIT, NYU, Santa Barbara, Texas (Austin), Yale

Computer Science: Carnegie Mellon, Cornell, MIT, Princeton

The job market for the usual sort of teaching jobs at academic institutions has not been doing well recently, especially at US state universities facing budget problems. On the other hand, the job market for mathematics and theoretical physics, at least at the post-doctoral level, may do better than that in some other disciplines. We may be returning to an eighteenth-century model where this kind of research is supported not by public universities, but by the great private fortunes of the day, those being produced in dominant new industries such as finance (Simons) and telecommunications (Lazaridis).

Comments

1. **John Armstrong**
   January 8, 2010

   Oh sure, the market for math postdocs improves *after* I've been squeezed out…

2. **Abhijnan Rej**
   January 9, 2010

   I think the Simons Postdoc at Northwestern starts from the academic year 2011!

3. **chickenbreeder**
   January 9, 2010

   The eighteenth century model of private sponsorship may turn out to be the better one. The current model is that the government collect all taxtpayers' money and give it to NSF/DOE, allowing a small number of like-thinking program managers to run the show. This highly centralized model not only leads to lack of diversity but also bureaucratic waste. This can be reverted by giving private foundations big tax break if they devote themselves to sponsorship of science.

   If 20 super-rich people do as Simons and Larzadis do, it may be enough to change the landscape of science research in the U.S.

4. **Arun**
   January 9, 2010

   2010 Federal Budget:
   National Institutes of Health $30.8 billion
   FDA $0.51 billion
   Centers for Disease Control $6.4 billion
   DOE Office of Science $1.6 billion
   NSF $7 billion

   20 Simons foundations: $0.8 billion

   Even if government spending is 10 times less efficient, it would take more than 200 Simons to match it.

5. **anon**
   January 10, 2010

   "…the job market for mathematics and theoretical physics, at least at the post-doctoral level, may do better than that in some other disciplines. We may be returning to an **eighteenth-century model where this kind of research is supported not by public universities, but by the great private fortunes of the day,** those being produced in dominant new industries such as finance (Simons) and telecommunications (Lazaridis)."

   Did pencils and paper cost fortunes in the 18th century? I can understand certain experiments costing fortunes, but not theory…

6. **Peter Woit**
   January 10, 2010

   Arun,

   Large scale experiments are still beyond the financial resources of almost all of the largest fortunes (although, extrapolating trends of the past couple decades, that may not be true in the future). But $40 million/year is a significant fraction of US government support for pure math and theoretical physics, and it's less than 1% of Simons's wealth/year.

7. **Tim vB**

January 11, 2010

@anon
Don't forget that other parts of society need talented people, too. If you put too much money into mathematics and theoretical physics you will cause a brain drain elsewhere 🙂

8. **Kay zum Felde**
January 11, 2010

Hi,

I don't know, if it is good, that money is now spent by the industry. They decide how the money is used. Physicists should decide, how they use their money.

9. **SteveB**
January 11, 2010

anon,

How much theoretical physics is done with only pencil and paper today. I would expect that a significant fraction of the "theory" work done today uses computational models to extract information and big computers do not come cheap — ~$1M to purchase and several $100K per year to power, cool, and support.

SteveB

10. **Peter Woit**
January 11, 2010

SteveB,

Very few theoretical physicists use that kind of computer power, in almost all cases a workstation or server that costs only a few thousand dollars is all that is needed. For mathematics and theoretical physics, what is costly is people, not equipment.

11. **Mitch Miller**
January 11, 2010

Is there a specific reason why the math ones pay a bit more?

12. **Peter Woit**
January 11, 2010

Mitch,

I wondered about that myself. Maybe Simons likes mathematicians more… I believe the math Simons fellowships involve teaching one semester course/year, possibly this is not true for the physics ones, and that has something to do with the difference

13. **Eric Baird**
January 11, 2010

Well, IAS Princeton was founded by a department store owner, the Perimeter Institute was set up by the Blackberry guy, and the Nobel Prize was founded with a bequest from an arms manufacturer.

14. **Arun**
January 14, 2010

OK, American Mathematical Society:
http://www.ams.org/notices/200807/tx080700809p.pdf

"Federal support for the mathematical sciences is slated to grow from an estimated US$468.59 million in FY 2008 to an estimated US$516.8 million in FY 2009, an increase of 10.3 percent."

so Simon is adding 10% to that. Significant but it doesn't change "the landscape of science research in the U.S", only that of mathematics and theoretical physics.

And on the other hand, the very same document tells us

> According to the Science and Engineering Indicators, 2008 Edition, in FY 2006, only 34.6 percent of full-time mathematics faculty, having doctoral degrees, received federal research support.

So non-federal government sources fund most math. faculty. Simon is one more.

15. **Peter Woit**
January 14, 2010

Arun,

The fact that 34.6 % of math faculty receive federal research support doesn't mean that the rest get non-federal research support. Most of them probably get no research support.

Also, keep in mind that Simons is supporting pure math research, not applied math research, and the federal support for that is a lot less than the total of about $500 million (maybe half?, NSF DMS is about $250 million). In addition, he is funding mathematics in other ways, e.g. the Simons Center, for which he gave $60 million to start the thing up two years ago.

Unless there's a mountain of money around that somehow I've never heard about, at the moment there is no other private source of funding for pure math research in the US on anything like the scale of the new Simons Foundation activities.

The Entropy Decade

January 11, 2010
Categories: Multiverse Mania

We're only a week and a half into the new decade, but already I'm seeing a trend…

A few days ago Sean Carroll's book From Eternity to Here came out, promoting the idea that understanding time and cosmology is all about understanding entropy. The same day saw Erik Verlinde's arXiv preprint On the Origin of Gravity and the Laws of Newton, which argues that

> Gravity is explained as an entropic force caused by changes in the information associated with the positions of material bodies.

Verlinde is a well known string theorist, and the paper is somewhat of a repudiation of the motivating idea for string theory unification, that string theory predicts gravity since it has a spin two massless state. But even with the main motivation gone, all is not lost for string theory, since

> The presented ideas are consistent with our knowledge of string theory, but if correct they should have important implications for this theory as well. In particular, the description of gravity as being due to the exchange of closed strings can no longer be valid. In fact, it appears that strings have to be emergent too.

This is discussed in blog postings here, here and here, and yesterday even made it to Slashdot.

Today, it's yet more entropy, with The Entropic Landscape by Bousso and Harnik, which propounds the Entropic Principle, that:

> the number of observers is proportional, on average, to the amount of entropy produced.

and claims that this principle quantitatively predicts six important aspects of cosmology.

While much of physics in the last century was dominated by a highly successful program to identify fundamental degrees of freedom of nature and understand their dynamics using increasingly deep and sophisticated mathematical formalisms, now the trend appears to be very different. Many of the most well-known theorists are pursuing research programs with the remarkable features that:

- You don't need to have any idea what the fundamental degrees of freedom are.
- You don't need any fundamental dynamical laws either.
- You can do everything with high school mathematics.

The last century was a hugely successful one for physics, whether this new order will be equally successful remains to be seen.

Update: More analysis of the Verlinde paper here, and Verlinde now has a blog and a twitter feed about it.

Update: Verlinde is adding explanations of points in his paper and conducting a discussion of it on Lubos Motl's blog here. He now says that, to explain quantum gravity

> I am not sure that string theory is the way to go.

Even though under his new framework string theory explains nothing about any fundamental physics, Verlinde refuses to give up on it, arguing that:

> It should also be emergent, and it is nothing but a framework like quantum field theory.
>
> In fact, I think of string theory as the way to make QFT in to a UV complete but still effective framework. It is based on universality. Many microscopic systems can lead to the same string theory. The string theory landscape is just the space of all universality classes of this framework. I have more to say about it, but will keep that for a publication, or I will post that some other time.

Comments

1. **zanzibar**
   January 11, 2010

   "I have one word for you son… biology!"

   The future of physics lies where experiments will be done, or where data is already awaiting analysis. Interpolation, not extrapolation.

2. **Chris W.**
   January 11, 2010

   Erik Verlinde's paper should arguably be regarded as a review article providing an accessible overview of some fairly well-known work done by others over the past 15 years. In particular, his references include the following:

   [7] T. Jacobson, "Thermodynamics of space-time: The Einstein equation of state," Phys. Rev. Lett. 75, 1260 (1995)
   arXiv:gr-qc/9504004

   [14] T. Padmanabhan, "Thermodynamical Aspects of Gravity: New insights," arXiv:0911.5004 [gr-qc], and references therein.

   Padmanabhan has developed these ideas in several papers.

   In addition, see the papers of Ariel Caticha, including the following:

   "The Information Geometry of Space and Time"
   arXiv:gr-qc/0508108 [26 Aug 2005 (v1)]

   "From Entropic Dynamics to Quantum Theory"
   arXiv:0907.4335 [24 Jul 2009 (v1)]

   I would also offer the general comment that arguments from thermodynamics and statistical mechanics played an extremely important role in arriving at an understanding of the microscopic dynamics of matter in the early 20th century. I think it is eminently reasonable at this stage to suppose that explorations of the deep interconnections of thermodynamics, entropy, and gravitation will play an equally important role in arriving at the *unified* understanding of the dynamical underpinnings of gravity and quantum fields that we don't yet possess.

3. **Peter Morgan**
January 11, 2010

The definition of entropy is hyperplane dependent, as is its thermodynamic dual, temperature. That's a problem. [As a matter of definition, something that is not hyperplane dependent would not be entropy, although clearly it is conceptually possible to introduce "measures of randomness" that are not associated with phase space. The vacuum state of QFT on Minkowski space is Lorentz and translation invariant, and introduces Gaussian fluctuations that are hyperplane independent, but quantum fluctuations are distinct from thermal fluctuations (which, again, are not Lorentz invariant).]

As far as empirical success is concerned, SR, GR, and QM were all inspired by empiricist approaches, of different kinds, but at least partly inspired by the 19th Century epitome, thermodynamics (in contrast to statistical mechanics). The descent back into model-building realism, particularly since 1950, say, is questionable (Chad Orzel has an IHE article in which he characterizes Physics as being about models — I imagine Pierre Duhem is turning in his grave). The descent into Platonic realism about mathematical models is even more questionable. So, the great successes of the 20th Century could be said to be more rooted in the 19th Century, and a return to an updated empiricism might be a good thing.

4. **Chris W.**
January 11, 2010

Peter, your link to Chad Orzel's IHE article seems to be null. Could you post a follow-up with a correction?

5. **Chris K.**
January 11, 2010

In the context of mentioning Padmanabhan, he also had an attempt to refute the massless spin-2 in "From Gravitons to Gravity: Myths and Reality" http://arxiv.org/abs/gr-qc/0409089 to which Deser cared to reply recently http://arxiv.org/abs/0910.2975

6. **Hans-Peter**
January 12, 2010

Peter, your irony is honest, but some of it may be a little bit misplaced.

You don't need to have any idea what the fundamental degrees of freedom are.

Of course, this attitude is wrong. We need to know the fundamental degrees of freedom. Not looking for them is a way to avoid solutions; you are right on this.

You don't need any fundamental dynamical laws either.

This point is more delicate. If macroscopic dynamics, such as space-time curvature, is a statistical effect, there is a certain independence from the fundamental dynamics. So this point may be an acceptable one, at least partly or temporarily.

You can do everything with high school mathematics.

This point is also delicate. "everything" is surely too much. But Verlinde's paper is, in large part, the non-relativistic version of Jacobson's. And nonrelativistic gravity is done, as we all know, with high-school maths. What is wrong here? There is only one thing that is wrong: that since 1995, when Jacobson wrote his paper, almost nobody was interested in his result. Both Jacobson and Verlinde show that, independently of the specific microscopic degrees of freedom, gravity results from their statistical properties. This is an old idea, going back to Smolin and others.

You are right to say that this cannot be the end of the story: we need to find the underlying degrees of freedom. But there are many people who do not even think that such degrees of freedom exist. For them, Jacobson and Verlinde are good: they make them start thinking.

Besides, there should be something about Jacobson and Verlinde that you should really like: these arguments are very much 3+1-dimensional. Verlinde talks about string theory as well, but in fact everything is 3+1-dimensional. (Jacobson, for example, does not mention string theory at all.) In fact, their work is discretely pulling the rug under the idea of higher dimensions. I at least, was very much astonished to see a string theorist like Verlinde writing this, because this means that he is slowly, but definitely leaving string theory – even though he does not yet admit it.

7. **Andrew Thomas**
January 12, 2010

If we are considering the fundamental level of reality, and asking the most fundamental questions about dynamics, we come up against the question "What decides how things change?" At this fundamental level, the physical laws can seem somewhat arbitrary (for example, the amount of charge on an electron). In fact, at this most fundamental level, the only principle which seems likely to describe dynamics seems to come from mathematics not physics: a system will have many more possible disordered states than ordered states, so a system which changes state randomly will most likely move to a more disordered state.

While the second "law" of thermodynamics is "just" a statistical principle, it is a mightily powerful statistical principle! This is because the basis of the second law – that "disorder will increase" – seems so obvious, and seems to appeal to a fundamental, platonic principle of mathematics. For this reason, the second law manages to appear even more fundamental and unbreakable than the other physical laws, which seem rather arbitrary in comparison. Hence Arthur Eddington's famous quote: "If someone points out to you that your pet theory of the universe is in disagreement with Maxwell's equations – then so much the worse for Maxwell's equations. If it is found to be contradicted by observation – well, these experimentalists do bungle things sometimes. But if your theory is found to be against the second law of thermodynamics I can offer you no hope; there is nothing for it but to collapse in deepest humiliation."

At the most fundamental level, I would just imagine physical dynamics are described by change of entropy – I can't imagine any more fundamental principle which could possibly describe change.

8. **Jack Sarfatti**
January 12, 2010

The award winning 2004 Ph.D. dissertation of Tamara Davis from University of New South Wales (P.C. Davies on her committee) Figs 1.1 & 5.1 are very relevant to this discussion. The dark energy density we detect in our past light cone is the inverse area of our future event horizon in our future light cone. Therefore, it's our future event horizon that must be the hologram screen acting retro-causally in the sense of Wheeler-Feynman's total absorber. Indeed the increase in area of our future horizon from inflation to its asymptotic constant de Sitter value in the causal diamond of our observable universe (defined by T. Davis) explains why the thermodynamic arrow of time is in same sense as the cosmological arrow and why the entropy of the early universe is so relatively small at the moment of inflation.

9. **Chris W.**
January 12, 2010

(I found Chad Orzel's most recent IHE article; I'm not sure it's the one Peter Morgan had in mind.)

10. **Peter Morgan**
January 13, 2010

Hans-Peter, you react fairly strongly against Peter's "You don't need to have any idea what the fundamental degrees of freedom are", but I think this is a significant aspect of a statistical approach.

We can achieve an empirically adequate model with effective degrees of freedom, whereas at some point in the future we will need to introduce a model that has different DOFs to achieve empirical adequacy to updated experimental data. Although it is possible that there is an endpoint to this process of refinement, there is no guarantee that there is.

Chris W., sorry about the link problem. I had in mind the IHE article referred to in Chad's "Uncertain Principles" blog post, http://scienceblogs.com/principles/2010/01/dog_physics_and_academic_blogg.php.

11. **Kevin Knuth**
January 13, 2010

I would be more inclined to suggest that this is the entropy century!

Entropy is critical because of its relationship to inference. We are finding that the "laws" of physics are rules that tell us how to make the best inferences given our current states of knowledge. Probability and entropy are central to this point of view since probability is a measure on the space of logical statements that one can make about a system, and dually, entropy quantifies (not technically a measure) the relevance of questions one can ask about a system.

There is nothing physical about entropy. This has led to great confusion in the past. Entropy quantifies the amount of information one would need to know the particular details about a given model.

Similarly, the idea that information drives a system is faulty. Deriving laws based on probability and entropy does involve information that the one performing inference possesses, but ultimately the system is merely "driven" in the most probable direction. The fact that we get the right answer doesn't mean that the information is driving the system. It means that we took into account all of the relevant information in making our predictions, and that this relevant information was sufficient to make an accurate prediction.

The result is that we don't get a theory of *why* something evolves the way it does. Instead we get a theory telling us which is the most probable scenario.

I have been pursuing these ideas for the last decade now along with Ariel Caticha and others, and am astounded at what the community involved in this research has uncovered. This new perspective of physical law as inferential reasoning brings a unity and utility that previous interpretations lack. My belief is that these ideas will break through the stagnation that theoretical physics saw in the last century and propel us to new heights.

12. **capitalistimperialistpig**
January 13, 2010

The only substantial clue we have as to the link between quantum mechanics and gravity is black hole thermodynamics, so entropy looks like a sensible place to start. Many of the deepest ideas in physics (Faraday's field concept, Einstein's relativity) came not out of sophisticated math but careful analysis of the foundations of physics.

13. **Chris W.**
January 13, 2010

See Erik Verlinde's latest post (13 January), scroll down to the following paragraph, and read from there:

> If the previous papers [Jacobson, et al] had made the emergence of gravity so clear, why are people still regarding string theory as the final theory of quantum gravity? Somehow, not everyone was convinced that these similarities mean something, or at least, people had no clear idea of what they mean.
>
> …… … … …

14. **Tim vB**
January 14, 2010

Verlinde states that he does not believe that string theory is a fundamental theory any more, because gravity is not a fundamental force, but he also states that string theory has a right to exist as a UV completion of QFT, and as a tool that gives valuable hints.
So his statement is a repudiation of string theory as the most fundamental theory, but not of string theory as a unification of the four forces (note that we have to drop the ubiquitous "fundamental" here).

I'll pick just two examples of the many points that confuse me, maybe someone likes to fill me in on Verlindes intentions:

First point:
"Many physicists believe that gravity, and space-time geometry are emergent. Also string theory and its related developments have given several indications in this direction."

This comes as a surprise, because in my naive understanding of string theory I always thought that the existence of spacetime is one of the axioms/starting points of the theory, so that an emergent spacetime is excluded from the very start. Was that a misconception?

Second point:
Verlindes talks about space to be emergent, but supposes microscopic dynamics including the existence of "time". If you start from GR, then this is impossible: Either you have space and time or you have none. Spacetime itself describes causal relationships, i.e. relationships of events, and this requires both notions. How does one reconcile GR with having time at a microscopic level and space as an emergent macroscopic phenomenon?

On a side note I don't quite understand the amount of attention this paper receives. What's so fascinating about it?

15. **Chris W.**
January 14, 2010

Tim vB,

I'm not the best person to respond to your queries, but I'll take a stab at it.

Point 1: In string theory's original formulation, a background spacetime was indeed assumed, but as work has pointed towards a deeper, non-perturbative formulation (M-theory) it has become doubtful that this can be maintained. Edward Witten has been fairly explicit recently that spacetime must ultimately be emergent. Exactly what this means, and what such a theory should look like, has remained obscure.

Point 2: "If you start from GR…" Well, you don't necessarily start from GR. Your starting point may be something that incorporates some primitive notion of time or causal ordering, which is applied in describing the evolution of some sort of pre-geometric system, out of which spacetime arises as a sort of coarse-grained or "mean-field" approximation to the underlying structure. Again, no one is sure whether, or exactly how, this can work, although a number of provocative ideas have been under investigation for quite a few years. Causal set theory is one example.

Why all the attention? Interest in the deep interconnections of entropy, thermodynamics, and gravity goes all the way back to the 1960s, when the so-called 4 laws of black hole mechanics were formulated, essentially as theorems in general relativity. Ostensibly they simply described the geometry of balck holes, but it was quickly noticed they bore a striking resemblance to the laws of thermodynamics. As a graduate student of John Wheeler, Jacob Bekenstein decided to take this analogy seriously and conjecture that they were in fact the laws of thermodynamics in a geometrical guise. He drew out some of the implications of this, and his conjecture was soon vindicated by Stephen Hawking's discovery that a semiclassical analysis of quantum fields near the horizon of a black hole implied that the black hole had a finite temperature measured at a distance (asymptotically), confirming Bekenstein's suspicions. This intimate 3-way relationship between general relativity, quantum field theory in curved spacetime, and thermodynamics has been widely regarded ever since as a key guidepost towards a theory of quantum gravity. This has been reinforced in the last years by the work of Ted Jacobson and others (see earlier comments). Erik Verlinde's prominence as a string theorist, and his remarks in the paper and on his blog about string theory in this context, suggest a significant shift in outlook on his part with respect to string theory and quantum gravity in general. (He goes out of his way to make this clear.) Of course this shift is not definitive, but we're in a revolutionary phase in fundamental physics; little if anything can be taken for granted.

16. **Mark**
January 14, 2010

"This comes as a surprise, because in my naive understanding of string theory I always thought that the existence of spacetime is one of the axioms/starting points of the theory, so that an emergent spacetime is excluded from the very start. Was that a misconception?"

To TimVB,
You should watch the talk by Moshe Rozali on "Backgound Independence in String Theory" at Loops 07:

http://www.matmor.unam.mx/eventos/loops07/

Or read the corresponding paper:
http://arxiv.org/abs/0809.3962

17. **D R Lunsford**
January 14, 2010

quote "Entropy, thermodynamics and gravity go back to the 60s…"

The first widely accessible book on GR was R.C. Tolman's "Relativity, Thermodynamics, and Cosmology" from the mid 30s.

-drl

18. **Chris W.**
January 14, 2010

DRL,

That quote was taken out of context. I wasn't talking about understanding and applying thermodynamics in astrophysics and cosmology, where gravity and special relativity (relativistic energies) are important. I was talking specifically about the discovery—in the 1960s—of the unexpectedly thermodynamic character of a particular class of solutions in classical general relativity.

19. **John Baez**
January 14, 2010

Hans-Peter wrote:

"…since 1995, when Jacobson wrote his paper, almost nobody was interested in his result."

I guess I'm almost nobody. But seriously: everyone in loop quantum gravity was very interested in Jacobson's result; the problem was figuring out something to do with it!

20. **Tim vB**
January 15, 2010

Chris W., Mark,
thanks for the answers, that was very helpful.
At this moment I have not digested all of the offered material, so I will refrain from asking more questions with one exception:

Chris W. wrote:
"Well, you don't necessarily start from GR. Your starting point may be something that incorporates some primitive notion of time or causal ordering, which is applied in describing the evolution of some sort of pre-geometric system, out of which spacetime arises as a sort of coarse-grained or "mean-field" approximation to the underlying structure."

It makes much more sense to me to understand Verlinde in the way you suggest: If he talks about the microscopic dynamics and it's concept of "time", then it is not the macroscopic notion of time connected with the emergent spacetime, but a different concept like the "causal set structure" that you pointed out.
But I'm not sure if that's what Verlinde means, because he mentions "space", and space only, as an emergent concept.
And he uses "time" to denote the causal structure of the microscopic dynamics, which made me think that he rolls back the unification of space and time of relativity theory.
Maybe this is a simple misunderstanding caused by my overly pedantic interpretation of his use of language.

21. **Chris W.**
January 15, 2010

From Tim vB: *And he uses "time" to denote the causal structure of the microscopic dynamics, which made me think that he rolls back the unification of space and time of relativity theory.*

The unification of space and time in relativity has come to be viewed in a somewhat different light as a result of the study of the dynamical structure of general relativity ("geometrodynamics"). You should find out more about the ADM formalism.

The meaning and status of time in general relativity is a vexed subject, that has received much attention from physicists and philosophers of physics. How it will all shake out is not at all clear, of course (notwithstanding my own biases 🙂).

22. **drme**
January 15, 2010

Wow! Verlinde posting in Lubos' blog?? Is Lubos still considered an important voice in the field? Amazing!!

23. **Chris W.**
January 17, 2010

Verlinde's Jan. 15 post is now on his blog. It appeared first on Lubos' blog.

Also, although the linked titles on his main page don't work, the three posts can be accessed as separate files:

15 Jan: Entropic forces and the 2nd law of thermodynamics
13 Jan: Essential points of the paper
12 Jan: Logic of the paper

24. **Mark Wallace**
January 19, 2010

I enjoyed Verlinde's paper. I was not familiar with the earlier work he references by Jacobson, so of course it really got me thinking. It immediately raised the following questions for me:
(1) If you accept that gravity is emergent, then what is wrong with the Standard Model? Why do strings need to be added to all the QFT that makes good experimental predictions?
(2) Having spent most of my life working with computer codes, I am intuitively comfortable with ideas like the holographic principle. Similar types of information constraints come up all the time in algorithms I work with, so it does not seem surprising to see something similar in nature. However, starting with the holographic principle doesn't seem quite right when the author claims 3 dimensional space as an emergent phenomena. I am willing to go along and follow the logic of deriving spatial dimensions as emergent phenomena, but there wasn't enough in Verlinde's paper to suggest a good route. I was kind of expecting a derivation of spatial dimensions based on information-theoretic type arguments, but I could not find it.
(3) On the other hand, time was taken as a given. I was surprised, since I would have expected for entropy based arguments that time would be one of the (many) emergent phenomena, and that 3d space and the holographic principle would be the givens.
One way or another, I liked the paper and think I may have learned something. One we start down the route of saying Newton's equations are like the ideal gas law, we need to be very careful, in any given context, to say what is a "given" and what emerges. The logic can get very confused if, in the process of a single argument, the author is not perfectly consistent about the givens.

25. **Chris W.**
January 20, 2010

The New Scientist article that Verlinde said was in the works is now on the website. (See Verlinde's Twitter feed.)

The author, Martijn van Calmthout, avoids dealing with some muddled aspects of the story, but overall the article isn't bad.

The muddle is inherent in the opening paragraphs:

> WHAT exactly is gravity? Everybody experiences it, but pinning down why the universe has gravity in the first place has proved difficult.
>
> Although gravity has been successfully described with laws devised by Isaac Newton and later Albert Einstein, we still don't know how the fundamental properties of the universe combine to create the phenomenon.

This skates right by the fact that the only forces—in a Newtonian sense—in general relativity are the non-gravitational forces that take particle trajectories away from geodesics in spacetime. So strictly speaking, talk of an entropy *force* must be replaced by another locution if one is to properly understand the connection of "the flow of entropy" to geodesic deviation, ie, to spacetime curvature.

This is just one aspect of this very provocative line of thinking that could use some clarification in accounts aimed at a general readership. Ted Jacobson's original paper avoids this problem, at the cost of using some relatively dense technical argumentation, and no explicit use of the holographic principle.

26. **Chris W.**
January 20, 2010

PS: Actually, footnote 5 in Jacobson's paper makes the following reference to the holographic principle:

> Another argument that might be advanced in support of the proportionality of entropy and area comes from the holographic hypothesis[6, 7], ie, the idea that the state of the part of the universe inside a spatial region can be fully specified on the boundary of that region. However, currently the primary support for this hypothesis comes from black hole thermodynamics itself. Since we are trying to account for the occurrence of thermodynamic-like laws for classical black holes it would therefore be circular to invoke this argument.

Iranian Theoretical Physicist Assassinated

January 12, 2010
Categories: Uncategorized

An Iranian theoretical physicist named Masoud Alimohammadi was assassinated in Teheran Tuesday. Alimohammadi's publication list indicates that he began his career specializing in conformal field theory, and more recently had been working on questions in general relativity. Initial news reports inaccurately characterized him as a "nuclear physicist" and speculated that he was assassinated because of his association with the Iranian nuclear program, but there seems to be absolutely no reason to believe this.

Comments

1. **John Armstrong**
   January 13, 2010

   This sort of misunderstanding is disturbingly common. A friend of mine was almost disallowed from boarding her flight in Seattle because she had in her backpack an introductory quantum mechanics textbook.

2. **Thomas R Love**
   January 13, 2010

   It seems much more likely that he was assassinated because of his association with the Iranian political opposition.

3. **obvious**
   January 13, 2010

   Umm, JA, obviously the Iranian government knew what kind of physicist he wasn't when they decided to assassinate him.

4. **Chris Oakley**
   January 13, 2010

   I do not think there are any misunderstandings. See here for example. Either the Iranian government itself or an extremist group supporting the official line but not under their direct control (LUnatics But supporters Of the System – LUBOS for short) carried out the attack to eliminate an opposition supporter and thereby intimidate others. Ali Mohammadi was a convenient choice because they could then blame Mossad or the CIA. He of course could not have made any significant contribution to Iran's nuclear programme, but the public are not going to know that, are they?

5. **ManyMe**
   January 13, 2010

   Actually Lubos wrote about this too.
   But I am a bit confused about what he is trying to say.
   Does his contempt for murder depend on the number of string theory citations somebody has on spires?

6. **Alejandro Rivero**
   January 13, 2010

   For a contrary to Armstrong comment: I was allowed to follow by French Police after finding that my only possession was a book onC-* algebras.

   I was running, wearing black sport clothes, during a rainy night in an isolated area. So police stop me, asked to produce an ID card, which I had forgotten, and only after I show the contends on my backpack, the springer yellow book was proof enough that I was a mad scientist and not a burglar.

7. **Tim vB**
   January 13, 2010

   ManyMe,

   that's not how I understood Lubos, it's just that he was not sure if the victim could be of use to Iran's nuclear program – and that stopping that program, the assasination of the scientists who contribute could be a better alternative than, say, declaring war on Iran.
   His statement is a bit odd, because
   a) I agree with you that – given the papers the victim published – it is pretty clear that he was not involved in applied nuclear research and
   b) I sincerily hope that there are alternative approaches that will succeed to stop Iran developing nuclear weapons, without hurting or killing anyone – and I would never say that "assasination of certain people might be the most human way" to achieve anything.

   All in all the interpretation Chris Oakley cites is obvious enough, let's hope that the Iranians don't fall for the lies of their government.

8. **Serifo**
   January 13, 2010

   Well , you don`t have to be an expert on nuclear physics to work on a nuclear program ! Have you forgoten the Manhatan project ? Pure mathematicians (e.g. von Neumann) and theoretical physicists (e.g. Richard F.) all worked on the project…

9. **Tim vB**
   January 13, 2010

   Serifo,
   no, of course I have not forgotten that. And of course I cannot be sure if what I said is right, but given the information I have I think it is likely to be right.
   John von Neumann did work on hardware design of computers, too, and was successfull, because he was a pioneer. Today you would have to beat thousends of specialists with many years of experience to make a significant contribution: Very unlikely that you are able to do that as a theoretical physicist!
   During the 19th century you could make groundbreaking discoveries in chemistry on your attic. That time is gone.
   And I think it is similarly unlikely that someone interested in quantum gravity or the like would even be asked to help with a nuclear weapons program, or be assasinated if he did help.

(And yes, I know that there were plans to kill Heisenberg during world war II because some people were afraid he could succeed in developing a nuclear weapon – and that he never came close to do so, although he tried).

10. **Bob Levine**
January 13, 2010

@Serifo

"Have you forgoten the Manhatan project ? Pure mathematicians (e.g. von Neumann) and theoretical physicists (e.g. Richard F.) all worked on the project…"

Theoreticians and mathemeticians were necessary to the work of the Manhatten Project because the question of whether an explosive chain reaction based on specific materials and mechanical configurations was even possible had not been established—that was what Trinity was all about: an existence proof. Theoretical modeling was crucial to determine what kind of engineering was necessary. Generating a spherical shock wave via an explosive lens around a plutonium core to get around the predetonation problem, for example, was a novel idea that only emerged, courtesy of Seth Neddermeyer, after extended discussions based on considerable mathematical extrapolation. Things couldn't be more different now—the possibility of such processes has been proven far too often; the major engineering stages and corresponding components are known. You probably need mostly high-level nuclear engineers at this point to build a deliverable fission device, at least. The *last* person you need is a GR theorist.

11. **Simplicio**
January 13, 2010

Chris's explanation seems overly clever. A pro-gov't group assasinated Alimohammadi both because killing an reformist supporter would intimidate other members of the opposition, and because people would think Mossad/CIA/etc were behind it? So people are supposed to think the CIA is killing reformists? That doesn't make much sense.

Plus, he didn't seem to be particularly well known for his support of the reformists. You'd think the gov't would choose someone a little more high-profile if they're trying to intimidate.

Maybe he just owed money to a loan-shark or somethng.

12. **Chris Oakley**
January 13, 2010

> A pro-gov't group assasinated Alimohammadi both because killing an reformist supporter would intimidate other members of the opposition, and because people would think Mossad/CIA/etc were behind it? So people are supposed to think the CIA is killing reformists?

Not quite correct. Read *The Independent* article. The story put out by the Iranian government and media (they do not have free speech, remember?) is that Al Mohammadi is a loyal son who was brutally murdered by foreign agents who thought that he was part of Iran's nuclear weapons program.

13. **Serifo**
January 13, 2010

Tim v B , Bob Levine

" And I think it is similarly unlikely that someone interested in quantum gravity or the like would even be asked to help with a nuclear weapons program, or be assasinated if he did help. "

Unlikely but possible !! There are a lot of theoretical physicists (string theorists in particular) working on wall street , most of them had to perhaps learn some economics in order to apply their capacity of buiding abstract models for real world purposes ! A good nuclear program will always require good technical assets , theoretical physicists and mathematicians (even as assistants to calculate or build abstract models) are excelent assets ! In fact even if not needed directly in a nuclear program , because of their mathematical background , theoretical physicists and mathematicians may help the army generals in their strategic plans.

As for the assasination of Mr. Alimohammadi :

If it is a CIA – Mossad job (most likely Mossad) , then the main goal is clearly to eliminate any techinical asset that may be involved in Iran`s nuclear program. By " eliminate " I mean killing important assets , or scare other assets to leave (or not be part) of the nuclear program !

If it`s Iran`s internal job , then the main goal is to diverge the attentions of the Iranian people from the internal political and economic problems. Blaming the " zionist " and CIA is very appealing to Iranian people !

14. **Tim vB**
January 13, 2010

Serifo,
again, I cannot know if I am right…
I graduated in theoretical physics, started working as a software developer – and could easily make (a lot) more money if I accepted one of the job offers by EADS, on several weapons programs (e.g. developing software that steers rockets).
I wouldn't even have to move, I could stay right where I am now!

So I think I know what you mean 🙂
It's just the combination of several facts that make me think that your theory is less likely (it's not necessary to repeat what these are, is it?).
In the case of Iran the CIA could have invested massive amounts of money to turn around the elections.
They did it in the past in other countries, several times, with success. Such a story would have better chances to convince me.

15. **Simplicio**
January 13, 2010

@Chris

I think you're missing my point. Your explanation gives the theorized gov't allied assasins two motivations for killing the Professor which are contradictory. That they targeted him both so that reformist intellectuals will know they're willing to murder people and be intimidated, and that since he's a professor they can tell the public he was killed by Mossad to keep him from working on nuclear projects.

But who's going to be intimidated away from supporting reformists if they think he was killed for his nuclear work?

16. **Mike**
January 13, 2010

Simplicio ,

No, I think you're missing the point. They tell the world that the US and Israel did it because he was a physicist — it's what a lot of people want to believe anyway. Of course, that doesn't work internally, with their own people — they know better (at least the opposition does) and they are already taking it as a warning.

17. **Simplicio**
January 13, 2010

Maybe, but that's what I mean by it being overly clever. The plan is dependent on one group of people seeing through one dastardly ruse to a second, deeper contradictory plan to try and interpret out a vaguely threatening message. I just don't think actual people make plans like that, especially the types of people who use random violence and murder as a form of intimidation, a tactic which doesn't exactly profit from subtlety. YMMV, of course.

Plus, as I said, the Professor seems a poor target as far as intimidating reformers, even without the layers of obsfucation. While he was a supporter, he doesn't appear to have been terribly vocal about his support, possibly signing a petition and stating that the gov't should treat student protesters with less violence (which I imagine is a pretty common sentiment amongst Iranian professors).

18. **Chris Oakley**
January 13, 2010

Simplicio,

I have no idea who carried out the attack. But if it was a loan shark or other local criminal I doubt that the Iranian media would have made such a big deal of it. It will not have been the CIA or Mossad – even they are not stupid enough to see this guy as a threat. So what are you left with?

19. **JK**
January 13, 2010

The Iranian state has a long record of terrorising opposition through random murders, not claiming responsibility and often disguising them to look like crime. There are many cases where the truth has never come out. Presumably the idea of the random pattern is to increase the effect of terror. (For an example where a section of the security service went out of control and the truth was exposed google chain murders.)

Killings have fallen back a bit in recent years, but with the new upsurge in opposition I would be surprised if murders didn't start to rise.

As for blaming it on the Americans / Israelis / British that is what the Iranian state always does in every circumstance, regardless of truth or plausibility (and of course there have been some foreign interventions in Iran that are not too popular with the people there, giving the accusations some resonance – but I won't take the blog any further away from physics.)

20. **Peter Woit**
January 13, 2010

I'm shutting off comments here, since the signal to noise ratio seems to have hit zero (if not gone negative).

Please all: stop submitting comments if you don't actually know anything about the subject of the posting.

Templeton Foundation News

January 15, 2010
Categories: Uncategorized

The Simons Foundation isn't the only one announcing funding opportunities in math and physics. The Templeton Foundation's list of funding priorities for 2010 is here, with applications opening February 1. In math and physics the topics they want to support research in are:

- Quantum Physics and the Nature of Reality

and

- Foundational Questions in the Mathematics Sciences

At least for 2010, they seem to have lost interest in the Multiverse.

Templeton is also supporting a member of the Harvard Math Department in a big way, with a grant of $10 million to math professor Martin Nowak to fund a program in Foundational Questions in Evolutionary Biology.

Comments

1. **Marcus**
   January 15, 2010

   In the grant description, I'm curious what this means:
   ==quote==
   "missing knowledge problems", such as in the origins of biological creativity, the deep logics of biological dynamics and biological ontology, and understandings of teleology and concepts of ultimate purpose in the context of evolution.
   ==endquote==

   Here's the grant description link that Peter gave:
   http://www.fas.harvard.edu/~fqeb/
   It links to a page on the principal recipient Martin Nowak:
   http://www.ped.fas.harvard.edu/people/faculty/

2. **Peter Woit**
   January 15, 2010

   Marcus,

   I think the next sentence elucidates the meaning:

   "These specific kinds of knowledge are directly relevant to a wide range of philosophical and theological discussions and debates."

   or roughly translated:

   "If I can relate my specialty of the mathematics of dynamical systems applied to biology somehow to theology, there's 10 million bucks in it…"

3. **Yatima**
   January 15, 2010

   Hmmm… "teleology and ultimate purpose in the context of evolution". Well, that river opens up into cargo cult ocean. Looking for purpose in evolution is like looking for a sculptor in a limestone cave.

   Seriously though, hasn't Arthur C. Clarke or NewScientist been all over that problem already?

4. **neo**
   January 15, 2010

   IMO, this is something best judged by results rather than intent. And if the money is wasted (the results are bogus), too bad for science, but it is Templeton's money to waste (except for the tax deduction).

5. **Low Math, Meekly Interacting**
   January 17, 2010

   Perhaps the most significant aspect of Darwin's achievement was to make the make the absence of the need for teleological arguments virtually irrefutable. This in the mid-19th century. Resuscitating teleology is about as progressive as bringing the luminiferous aether back to explanations of Lorentz transformations.

6. **Anonymous P**
   January 17, 2010

   There may be room for disagreement about the value of Martin Novak's research, but I've looked at it, and it certainly falls under the category of real science. the reference to teleology appears to be misleading here.

   P

7. **The Baron**
   January 18, 2010

   The class I took from Professor Nowak was probably the most fascinating one I've ever enrolled in. The interdiscliplinary nature of the scholarship coming out of the Program for Evolutionary Dynamics is simply stunning, as is Professor Nowak's intellect. A well-deserved award.

Particle Theory in Midtown

January 17, 2010
Categories: Uncategorized

Particle theory is about to have a significantly higher profile in midtown Manhattan, with the launch of two new programs this spring:

- The CUNY Graduate Center at 34th St. is starting up an Initiative for the Theoretical Sciences, with a program of colloquia, workshops and public lectures in various areas of theoretical science. In early April there will be a workshop on Emerging problems in particle phenomenology.
- A few blocks away, at the 27th St. Stony Brook Manhattan campus, the Simons Center for Geometry and Physics will start having seminars February 12 under the title Simons Center Seminars in Manhattan.

Comments

1. **Thomas R Love**
   January 17, 2010

   The three speakers at the Simons Center Seminars in Manhattan are all string theorists. Surprise, surprise, suprise!

2. **Peter Woit**
   January 17, 2010

   Actually, they might all be better characterized as "ex-string theorists", with McGreevy recently working on condensed matter physics, Komargodski on supersymmetry breaking and Cachazo on qft scattering amplitudes.

   This reflects pretty well the "hot" topics in particle theory: string theorists are fleeing the failed program of trying to get unification or even quantum gravity out of string theory, and moving into other subjects, sometimes hoping to find a use for methods that grew out of their earlier string theory research.

3. **Mike**
   January 23, 2010

   What I found interesting w.r.t. the Simons seminar announcement is that no title of the talks, let alone abstracts is posted. Why should people attend, then?

4. **A.J.**
   January 23, 2010

   Mike,

   I suspect the organizers will post the titles and abstracts when they get them from speakers.

Big Think

January 18, 2010
Categories: Uncategorized

A little while ago I did an interview for Big Think, and they just put it up here today, with some editorial comment here.

I really don't like watching or listening to myself, so I'm not about to go through the interview and see exactly how what I tried to say came out and later got edited. If I said something unclear or nonsensical, perhaps someone will let me know. Regular readers of this blog are unlikely to hear anything they haven't read before. Big Think has their own commenting system, and you can comment there if you wish.

Comments

1. **Tim vB**
   January 18, 2010

   Did not spot anything unusual – but if you really want to show off, here's a little hint: The s-t-combination in German is usually pronounced as sh – t (sh like in hush), so Einstein is more like Ein-sh-tein and less like Ein-s-t-ein 🙂

2. **Bee**
   January 19, 2010

   43 minutes? Forgive me for not watching it. At least the first minute the video is out of synch with the audio. (Or maybe the speed of sound just changed by some orders of magnitude?)

3. **Bob Levine**
   January 19, 2010

   There's a written transcript of the interview provided as well. Peter's comments seemed quite good-natured and—at least as far as personalities went—non-judgmental, while still making the relevant points about the dead-end nature of string theory. And the comments about the Higgs, and why in some sense one doesn't really want the Higgs field to be the source of particle massiveness, was nicely done, I thought. The only problem with the interview that I could see was that in some places the editing seemed a bit spotty….

4. **GVF**
   January 19, 2010

   You talk much too fast for a recording.

5. **Eshan Shah**
   January 19, 2010

   Mr Woit, could you simply sum up the arguements for why string theory is not a 'scientific' theory?

6. **D R Lunsford**
   January 19, 2010

   Interview is OK but the streaming content is very badly done – unsynced audio and video and high CPU usage. Oh well.

7. **Peter Woit**
   January 19, 2010

   Eshan Shah,

   I don't claim that string theory is not a scientific theory. "String theory" refers to a wide range of different activities, many of which are completely scientific.

   The controversial part of string theory is the speculative attempt to use it to produce a unified theory. I would claim that such an attempt is scientific, the problem is that it has conclusively failed. What is unscientific is the attempt to evade this failure by invoking the "anthropic landscape". That's not science since it can't predict anything or even in principle be tested.

8. **Chris W.**
   January 19, 2010

   An example of that spotty editing:

   "Why do they all have different masses? We don't understand why the electron has a certain mass, **quartz** has other masses."

   Hopefully they'll make another pass on editing the transcript.

9. **Thomas R Love**
   January 20, 2010

   I watched the whole thing, in three sessions, after all they break up the 43 minutes in to 6 segments. Like you said, Peter, nothing new. But it is nice to review once in a while.

10. **David Levitt**
    January 20, 2010

    Peter,

    I am a long time fan of your blog and book, but have not commented previously. I am commenting now because of the rather bland nature of the above comments. What is wrong with giving you some applause and support? I thought it was a great interview – very fair, informative and focused. I would recommend it as a very concise summary of the current situation. I doubt it could be improved on.

11. **petergreat**
January 20, 2010

Indeed nothing new in it. I'm under the impression that no one has anything new to say about HEP. Not until LHC starts to give results.

12. **neo**
January 20, 2010

I found it very interesting and informative, although the disconnect between the video and audio was disconcerting. And BTW Chris W., Peter said quarks not quartz.

13. **Chris W.**
January 20, 2010

"Peter said quarks not quartz."

I know; I was blaming it on the transcriptionist. (I've seen some other sloppy "phonetic" transcripts recently.)

14. **Peter Woit**
January 20, 2010

Thanks David!

Actually, I'm glad that some of the conflict over string theory has died down, and Peter Woit attackers and supporters are no longer fighting in the streets (or the blogs…)

A combination of exhaustion and waiting for the LHC has put a lot of these debates into a state of suspended animation. This has now been going on for a while, and may continue for quite a while longer, which has its pluses and minuses. Anyway, I'm glad to every so often have the opportunity to restate a point of view on these issues which I've always thought is actually a fairly mainstream one in the physics community, even though for many years it wasn't getting much public attention.

15. **Paolo**
January 21, 2010

To date, I'm still failing to see what's the point of this war against string theory. Let's suppose for the sake of argument that those scholars are all completely and fully wrong and that the "theory" (I know Peter probably doesn't consider it a theory at all) doesn't make sense: so what? Don't we have better things to do than spending some of our precious time attacking those poor chaps? Like, for example, working on better ideas in the same area, or just enjoying the life and doing something completely different? Really, I do not understand. Probably I never will.

16. **Peter Woit**
January 21, 2010

Paolo,

Actually, sure, string theory is a theory, just one that doesn't work as a unified theory…

I'm glad I took the time to write the book, and to have some effect on causing there to be a needed debate about what was going on with string theory. But at this point, personally I'm taking your advice, spending very little time on string theory, instead working on better ideas and enjoying life. Arguably I'm spending too much of my time enjoying life and not enough working…

17. **Ryan Budney**
January 21, 2010

I'm another long-time follower of your blog. I thought the interview was excellent. You were clear and measured. You qualified your statements but didn't sound too much like a politician. 🙂 Very nice.

On my end your voice wasn't quite synced with the image but that's a purely technical issue of production.

Various and Sundry

January 21, 2010
Categories: Uncategorized

- The latest New Scientist has an article about Erik Verlinde's "entropic gravity", with enthusiastic remarks from Robbert Dijkgraaf and Stanley Deser. Gerard 't Hooft expresses pleasure at seeing a string theorist talking about "real physical concepts like mass and force, not just fancy abstract mathematics". According to the article, the problem with Einstein's General Relativity is that its "laws are only mathematical descriptions." I guess a precise mathematical expression of a theory is somehow undesirable, much better to have a vague description in English about how it's all due to some mysterious entropy. There's even an editorial about this:

> Now we could be closing in on an explanation of where gravity comes from: it might be an emergent property of the way objects are organised, much as fluidity arises as a property of water…. This idea might seem exotic now, but to kids of the future it might be as familiar as apples.

In a new preprint, Lee Smolin uses Verlinde's work in a very different way, to show that Newton's law of gravity must emerge from the microscopic quantum gravity approach Smolin favors, that of loop quantum gravity.

- Also on the New Scientist/entropy front, there's a review by Craig Callender of Sean Carroll's new book. I'd been wondering what philosophers of science would have to say about the book, and the reaction to Carroll's multiverse explanation of the arrow of time was about what I suspected it would be:

> Daring to speculate in the absence of well-confirmed theory, Carroll jumps from clue to clue, from black hole physics to string theory to the holographic principle, until he arrives at his destination: an eternal "mother space-time" from which a multiverse of baby universes are continually bubbling up and pinching off. The mother space-time is a high entropy vacuum that gives birth to universes like our own, some of which we can expect to begin with low entropy. Problem solved, says Carroll, because that is natural.
>
> Carroll seems slightly embarrassed by the many leaps of faith he asks of his reader in proposing this solution, and the prose of Part IV sometimes reads like the pitch of an honest used-car salesman: "This car is a dream! True, the tyres are bald, brakes unsound and transmission sticky, but you'll love it!"
>
> Carroll and other peddlers of multiverses make us an offer: we will explain the unexplained if you add vast unconfirmable matters of fact into your ontology. In this case that includes a host of disconnected baby universes, an eternal mother universe entirely unlike ours, and half a dozen unknown mechanisms to get all this working. Assuming this explains the low entropy past – and with so much unknown it is hard to be sure another conspiracy isn't lurking within – is this a good deal?
>
> In most cases I don't think so. Why is Manchester United perennially a good soccer team? Surely most solutions of the laws of physics don't have them winning so much. How unnatural (and unfair) those initial conditions are! Nonetheless, a frothy sea of baby universes tempts no one. We shrug and say, that's just the way it is. Sometimes it is best not to scratch explanatory itches.

- Witten now has a long preprint out about his beautiful recent work on analytic continuation of Chern-Simons theory that I wrote about here last fall.
- My colleague Johan de Jong has been working for a few years now on what he calls the Stacks Project, which aims at a detailed, foundational exposition of the theory of algebraic stacks, beginning with the necessary algebraic geometry. He has structured this along the lines of an open source software project, encouraging contributions to the project from other algebraic geometers. The latest addition to the project is a blog.
- The filmmakers who brought us What the Bleep Do We Know? have recently completed a new film, entitled Ghetto Physics: Will the Real Pimps and Ho's Please Stand Up!. According to Cornel West "This intelligent and intelligible film is a must-see for all of us." There may be a theatrical release this year.
- A huge proportion of the mathematics research literature is now controlled by the publishing company Springer Science + Business Media. Last April there were reports that the owners of the business had it up for sale for about \$2.9 billion. The CEO denied these reports, stating "We are not for sale, there is no truth in Springer being sold". Last month came the announcement that Springer was being sold, to two private equity firms from Sweden and Singapore. The price was about \$3.4 billion, with the new owners also taking on \$2.9 billion of the company's debt.

It's not clear if there are any implications for mathematics publishing, with this perhaps just a transfer of control of the mathematics literature from one group of private equity firms to another.

- In the next couple months Princeton University Press will publish a short new popular book on string theory, Steve Gubser's The Little Book of String Theory. It is only 184 pages long and appears to be somewhat similar to efforts like The Complete Idiot's Guide to String Theory, String Theory Demystified, and String Theory for Dummies, but less technical, with less graphics, and a lot shorter.

According to the promotional material, the author

> describes efforts to link string theory to experimental physics and uses analogies that nonscientists can understand. How does Chopin's Fantasie-Impromptu relate to quantum mechanics? What would it be like to fall into a black hole? Why is dancing a waltz similar to contemplating a string duality?

and

> After reading this book, you'll be able to draw your own conclusions about string theory.

The introduction is available here, and ends with this description of recent debates over string theory:

> I don't aim to settle any debates about string theory in this book, but I'll go so far as to say that I think a lot of the disagreement is about points of view. When a noteworthy result comes out of string theory, a proponent of the theory might say, "That was fantastic! But it would be so much better if only we could do thus-and-such." At the same time, a critic might say, "That was pathetic! if only they had done thus-and-such, i might be impressed." in the end, the proponents and the critics (at least, the more serious and informed members of each camp) are not that far apart on matters of substance. everyone agrees that there are some deep mysteries in fundamental physics. nearly everyone agrees that string theorists have mounted serious attempts to solve them. And surely it can be agreed that much of string theory's promise has yet to be delivered upon.

- For two wonderful but very different short memoirs by mathematicians about aspects of their research work, see William Stein's Mathematical Software and Me: A Very Personal Recollection, and Michael Harris's A Mathematical Dream and Its Interpretation.

Update: The Onion carries the news that World Physicists Complete Study of Physics. The quote from a physicist is:

> Yeah, that about does it for physics. All done. Math can pretty much take it from here.

Update: Robert Helling gives his take on the Verlinde paper here. It reminds him of a certain proof that reaches an unreasonable conclusion using the rules "time=money" and "money is the root of evil". I noticed this via an arXiv trackback. Funny, for some reason there are no trackbacks to my postings on this topic

Comments

1. **Ricci's Pieces**
   January 21, 2010

   I get a sense that you are not so into the entropy/gravity connection, but I think it's going to be a very fruitful area for mathematical General Relativity in the coming years. There have been about a dozen mathematical papers on the subject, most of them coming from the viewpoint of Perelman's entropy functional, Ricci Flow and all those things. It may not be Perelman's entropy functional, maybe a modified entropy functional, but this is one of the most mysterious areas in geometric analysis right now.

2. **Coin**
   January 21, 2010

   I wonder if it's a sign of trouble when you one has to specifically brag that one's work is "intelligible".

3. **D R Lunsford**
   January 22, 2010

   That piece about math software was really entertaining! I still have my working HP-71B from student days and still use my TI-92+ to do algebra in spacetime on the bus and train. I'm sort of surprised he never used Derive, which I find incredibly useful – a sort of LISP dialect that is specifically for math – the array processing is magnificent and better even than APL, and of course it handles the complex domain effortlessly.

   I heartily endorse the SAGE project, which is both fun in itself and very useful as well.

   -drl

4. **wolfgang**
   January 23, 2010

   I do not find Lee Smolin's argument very convincing.

   Verlinde considers the change in entropy dS for displacements dx assuming a holographic principle. But in his calculation he implicitly assumes the geometry of a smooth and indeed flat geometry.

   There is of course nothing wrong about that, but if Lee Smolin wants to use this argument, then he has to first show that there is a reasonable limit of loop quantum gravity, which reproduces this smooth and (almost) flat spacetime and I dont see that.

5. **John Rennie**
   January 23, 2010

   I think you're being a bit harsh when you say:

   I guess a precise mathematical expression of a theory is somehow undesirable, much better to have a vague description in English about how it's all due to some mysterious entropy.

   No-one is suggesting the existing mathematical models should be abandoned. The point being made is that the entropic approach may give us some physical insight into those mathematical models.

6. **Thomas Larsson**
   January 23, 2010

   Lubos has decided that Verlinde's paper is junk, cf http://motls.blogspot.com/2010/01/erik-verlinde-why-gravity-cant-be.html . While I don't have neither the energy nor the motivation to check his arguments, they agree with my own prejudice.

   To my embarrassment I must confess that I often find a lot of sense in what Lubos writes – in many ways, he is often expresses an extreme caricature of my own opinion.

   (I once knew how to make anonymous links to Lubos' site, but I have forgotten how.).

7. **Thomas Larsson**
   January 23, 2010

   Just copy the link into your browser's address field and you should get there.

8. **Frank**
   January 23, 2010

   Peter is right to criticize that the entropic formulation by Jacobson and Verlinde is not deep, because describing space-time as a thermodynamic limit does not tell (almost) anything about the microscopic constituents.

   When Bernoulli deduced the ideal gas law from atoms, he was able to show that gases are made of atoms whizzing around. But as suggested by many, almost any microscopic degree of freedom at Planck scale will give the proper thermodynamic limit: loops, ribbons, etc. all yield a limit that then leads to Jacobson's and Verlinde's argument.

   It is highly probable that the Jacobson/Verlinde argument is not able to distinguish between different microscopic models of quantum gravity. There is one exception though: the argument eliminates all theories with higher dimensions. In my view, the only conclusion about new physics that can be drawn from the Jacobson/Verlinde argument is: space-time is emergent and is made of microscopic degrees of freedom that fluctuate in 3+1 dimension.

   Quantum gravity people will say: we knew this since (at least) 15 years. And they are right. However, if the exploration of quantum gravity were the right path to find the microscopic degrees of freedom, they would have been found long ago. In fact, quantum gravity does not allow to deduce much about the microscopic degrees of freedom.

   The paper by Verlinde does not change the situation at all. Except that it confirms that superstrings are not the right microscopic degrees of freedom, because they do not live in 3+1 dimensions. But Peter would not call this a new result 🙂

9. **Frank**
January 23, 2010

I think that Lubos is wrong. Unruh's proportionality between acceleration and temperature implies that gravity can be seen as an thermal/entropic effect. There is little doubt about it. If you want to get rid about the gravity-entropy relation, you must get rid of Unruh radiation – and that is impossible.

On the other hand, this does not tell anything new, as I argued in my previous comment. The reason that Lubos is against the connection between gravity and entropy is clear: he understands that the Jacobson/Verlinde argument undermines string theory, because it excludes higher dimensions. Worse, through Verlinde's simplification for Newtonian gravity, EVERY physicist now understands that higher dimensions are out! This is Lubos' nightmare: a simple argument that suggests that string theory is wrong. Even worse, the argument is made by one of the world's most distinguished string theorists! We can all guess what will happen: Lubos will start discrediting the argument with the same anger with which he discredits global warming. Watch the show.

10. **Peter Woit**
January 24, 2010

Unfortunately, whenever I mention the Verlinde business, it's all anyone seems to want to comment on. I get dozens of comments (most of which I delete) submitted by people who want to discuss their own ideas about how the fundamental problems of physics can be solved using simple ideas from thermodynamics and high school math. Moderating these comments depresses me, so please stop doing it. Unless you have something really new, interesting, and very directly related to the Verlinde story, please don't post comments on the subject here.

11. **Roger Schlafly**
January 25, 2010

So do you agree that the more serious string theorists are not far apart from you on matters of substance? And that you are not far apart from them?

12. **Rhys**
January 25, 2010

Frank, which part of Verlinde's argument singles out 3+1 dimensions? I have just been going through the early parts of the paper to try to get a better feel for his argument, and no part of it seems to depend on the dimensionality of spacetime.

13. **Peter Woit**
January 25, 2010

Roger,

I don't see any disagreement with most serious string theorists about whether string theory now gives a viable unified theory (it doesn't). Where we disagree is on the question of whether to be optimistic or pessimistic about prospects for the future of this idea. I haven't seen anything in recent years that changes my pessimistic take on the situation, and I think even the more optimistic string theorists would agree that nothing learned recently adds to the case for optimism. Other than a small number of people who believe the landscape is serious physics, the majority of string theorists recognize that it's a disastrous end-point for the idea of unification via string theory.

14. **Lee Smolin**
January 25, 2010

Dear Wolfgang,

From your comment you misunderstand the argument I make. I begin the conclusions by saying: "It is important to emphasize that I have not shown here that classical spacetime emerges from loop quantum gravity, as we have assumed that there is a classical spacetime in the exterior region where we make measurements. What has been shown is that if there is a classical spacetime that emerges then Newton's law of gravity is necessarily satisfied. " Perhaps you will find the actual argument, which is to that limited conclusion, more convincing.

Lee

15. **wolfgang**
January 26, 2010

Dear Lee,

>> What has been shown is that if there is a classical spacetime
>> that emerges then Newton's law of gravity is necessarily
>> satisfied.

but this includes several assumptions which you make on p. 9 and p.10, for example
".. the excitation is moving in a classical spacetime. We assume that then it can be described as a particle."

Later on the same page you assume a specific value for a fudge factor f to actually reproduce Newton's law.

Since I do not immediately see that all these assumptions follow naturally from l.q.g. (obviously I know much less about it than you), I did not find the argument very convincing.

16. **Frank**
February 9, 2010

Rhys, the whole Verlinde argument only works in 3d. There are many ways to see this. One is to check Jacobson's original argument, which only works in 3d; another is to recall the relation between 3d and the "square" in 1/r^2.

As I predicted, Motl is fighting with all his energy. Now Verlinde is already a "crackpot". It seems that this attitude is shared by many. (It makes Jacobson a crackpot as well.) I can only shake my head in dismay. Even Seiberg, some time ago, has written a long paper on emergent space-time. It is now obvious since 15 years that space and gravity are due to some microscopic degrees of freedom, and now that there is a simple argument to show this, many dismiss it.

Wheeler spoke of "space-time foam" long ago. Then came the string theorists, and they replaced foam by compactified 11d space. But they were wrong. Wheeler was right. That is the simple truth. Space-time foam exists, and its thermodynamics produces gravity. Space-time is not a compactified 11d space.

17. **Peter Woit**

February 9, 2010

Frank,

Sorry, but I'm with Lubos on this, and I don't believe your argument that this only works in 4d. You can hope (as Seiberg does) to get space-time to "emerge" from a higher dimensional string theory, or from "space-time foam", from bits, from all sorts of things, with all sorts of different possible dimensions "emerging" at different distance scales. But by assuming just thermodynamics + some version of holography, you are starting with almost zero input. From this you will get almost zero output, not stringent limitations on the nature of the fundamental theory, for instance on its dimensionality.

Physics of the Universe Summit

January 26, 2010
Categories: Uncategorized

The New York Times today reports on a *Physics of the Universe Summit* held a week or so ago in LA. According to the Times, participants stayed at "a Hollywood hotel known long ago as the 'Riot Hyatt,' for the antics of rock stars who stayed there." Talks were a couple miles south at the SpaceX factory, Larry Page of Google was there "handing out new Google phones to his friends", the magician David Blaine performed card tricks, and Bob Dylan's son Jesse showed some sort of film about the LHC. The only other information about this that seems to be available on the web is Sean Carroll's blog posting here, where he gives a link to the slides of his talk.

Optimist Gordy Kane claimed that the LHC will soon discover supersymmetry, making physics on the verge of seeing "the bottom of the iceberg". Lisa Randall (who evidently has a new book planned about science and the LHC) argued instead for focusing on less grandiose small problems. She was skeptical about supersymmetry, pointing out that we should have seen various evidence of it by now, and that the "wimp miracle" of a stable superpartner explaining dark matter doesn't work well "without some additional fiddling with its parameters." Joe Lykken summarized the situation as:

> We're confused, and we're probably going to be confused for a long time.

Comments

1. **willman**
   January 26, 2010

   "less grandiose small problems" – does this mean that Randall has given up hope for finding evidence of large extra dimensions?

2. **Bee**
   January 26, 2010

   Gosh, if the meeting was as fuzzy and content-free as the article, it was a total waste of time and money. Always the same people telling each other always the same things. Avoid groupthink? Figure out what it is to begin with.

3. **Harrison**
   January 26, 2010

   unreal, the nyt reporter offers virtually zero insight from a meeting he spent days covering.

4. **Peter Woit**
   January 26, 2010

   Harrison,

   I disagree completely. The article seemed to me to do an excellent job of capturing the mood and discussions of this kind of meeting (for example, that people spend time arguing "is this subject depressing right now or not?"). It even included a reasonable explanation of some of the more technical arguments, for example Randall on the problems of supersymmetry.

5. **Michael**
   January 26, 2010

   If anything proves that we need new data, the quoted statements from this "conference" do!

   The hubris of trying to replicate a speech by David Hilbert with a conference called "The Physics of the Universe" is a bit too much for my taste. Staging contending camps of optimists and pessimists to duel on-stage is rather contrived. Have some of these people simply been to too many conferences and are no longer stimulated by the usual discussions among experts of concrete problems and puzzles?

   One could also complain about some very sloppy statements – CMS is not Maria Spiropulu's detector, and the LHC experiments have only looked at a few thousand min-bias events at 900 GeV. This is a far cry from "recapitulating much of the physics of the 20th century." This is hyper-hyperbole.

   Lisa Randall's comment is the most interesting one, given that her fame comes from some very big and bold ideas. It would be nice to know what are the small problems that interest her – I guess we will see.

   I am most grateful for Mark Wise's reminder that we have no reason to be depressed given the accomplishments of the Tevatron program and the turn-on of the LHC. Hopefully only a few people will be blasé about that. I can think of a few hundred, or even a few thousand experimenters who are very excited to finally get a chance to probe physics at much higher energy scales. Who knows – maybe conferences next year will focus on the interpretation of those data rather than on overblown big questions…

6. **M**
   January 27, 2010

   by the way, why the NYT journalist always writes about Maria Spiropulu?

7. **John Baez**
   January 27, 2010

   Physics "of the universe"? As opposed to what?

   I thought all physics was supposed to be about the universe. But I guess I'm just old-fashioned.

8. **Peter Woit**
   January 27, 2010

   John,

As opposed to “physics of the multiverse” I suppose…

9. **A quantum diaries survivor**
January 28, 2010

   Michael, agreed – cms is not Maria’s own, and it has not recapitulated much of last century’s physics! But neither “few thousand minimum bias collisions” is a fair description. Not even the “50,000” that Dennis reported about in his NYT piece.
   Cms collected, as you know, over 500,000 good collisions. This is nothing compared to what is in store for this year, of course, but it is enough to keep us busy with not totally irrelevant physics measurements. W and Z bosons will have to wait, but misrepresenting the situation one way may be as nocuous as misrwpresenging it the other way.
   Apart from that, I share much of your feelings.
   Cheers,
   T.

LHC Update

January 27, 2010
Categories: Experimental HEP News

Those responsible for the LHC machine are having their yearly meeting this week in Chamonix to discuss the state of the project and plans for the future. Last week a subgroup met to discuss plans for beam commissioning to 3.5 TeV/beam, starting next month. The current schedule envisages beam commissioning to restart around February 19, and best estimate is that it will take about a month to establish safe, stable 3.5 TeV beams and begin extended runs for physics purposes. There's a plan for a big media event when first collisions are achieved at 3.5 TeV/beam, something that may require discouraging experiments from announcing observation of high-energy collisions that happen before the planned moment (evidently this is what occurred last year, when Atlas saw 1.18 TeV/beam collisions before they were supposed to…).

This year's schedule includes a possible one-month stop mid-year to increase the beam energy from 3.5 to 5 TeV, but based on the discussions at Chamonix, this looks very unlikely. The most serious problem with the LHC remains the bad splices which are known to exist in the machine, as well as sectors where definitive measurements of all the splices have not been possible (they would require warming up the sector, causing delays of months). The current knowledge of the splices leaves no room for error, even at 3.5 TeV, and going to 5 TeV would require warming up parts of the machine, something which cannot be done during a 1-month stop.

Discussions are beginning about how long a stop for repairs should be planned for after this year's run ends in November. To be able to run at 5 TeV/beam will probably require keeping the machine off until May 2011 to fix splices. Going to the design energy of 7 TeV may require even more extensive work on the splices, work that could keep the machine off for all of 2011, with startup again in 2012. To get above 5 TeV, work also needs to be done on retraining the magnets through repeated quenches. Not much of this would be needed to get to 6.5 TeV/beam, but to go all the way to 7 TeV, problems that are still not understood with magnets from one manufacturer will have to be addressed.

Update: From the Chamonix summary talk, there are two main scenarios now being considered. In the first, the energy of the machine would stay at 3.5 TeV/beam this year and next, with .1-.5 fb^{-1} integrated luminosity in 2010, 1 fb^{-1} in 2011, then a year-long shutdown in 2012 to fix all splices before moving to 6.5-7 TeV/beam. In the second, splices would be fixed in stages, running for only 5 months in 2011, at 5 TeV/beam, 1 fb^{-1} integrated luminosity.

There will be a summary session at CERN next Friday.

Comments

1. **Coin**
   January 27, 2010

   Peter, thank you, this blog really did at some point become the best source of LHC updates I've found anywhere.

2. **Amos**
   January 27, 2010

   One question I've had about all of this since Resonaances first broke the "one manufacturer's splices" part of the story — these things under warranty?

3. **Peter Woit**
   January 27, 2010

   Amos,

   I'm pretty sure that these things come with no warranty. They're extensively tested before being put in the tunnel, but I'd guess there's no warranty after that.

4. **Ralph**
   January 28, 2010

   6 to 12 months to repair splices sounds pretty pessimistic – warm-up about 1 month, cool-down about 2 months, so that's about 3 to 9 months of actual work?

   If it really is that major, then putting the work off until other planned work (e.g., the 2014ish upgrade of the inner triplets?) is carried out, would sound attractive. There's plenty of interesting stuff that can be done at 3.5TeV/beam at the mean time (especially if working at a lower energy allows being more aggressive with the beam current).

5. **Michael**
   January 28, 2010

   Let me try to balance the somewhat gloomy cast of Peter's post by pointing out Ralph Heuer's (Director General of CERN) new set of goals for the 2010-11 run: there should be 500 pb-1 delivered to each experiment at high energy (which is expected to be close to 10 TeV, ie, maybe 9 TeV) and one considers even 1 fb-1. With such a data set, the LHC experiments could surpass the achievements of the Tevatron experiments, both in terms of standard model measurements and searches for new physics.

6. **Peter Woit**
   January 28, 2010

   Michael,

   Thanks for pointing this out. The splice problem directly affects the possibility of increasing beam energy, but not the prospects for increasing luminosity. Beam commissioning went very well last year, and the same may be true this year.

   For the most relevant Chamonix presentation about the effects of the splice problem on prospects for 5 TeV/beam, see

   http://indico.cern.ch/getFile.py/access?contribId=2&sessionId=0&resId=1&materialId=slides&confId=67839

   My reading of it is that going to 5 TeV/beam will require warming up sectors and is not going to happen in 2010. There are ideas discussed that could be implemented with a 1 month stop, but it looks like they would only allow an increase to 3.8-4 TeV/beam.

   Ralph,

   I think the fundamental problem is that there are 24000 splices in the LHC main circuits, and if something has to be done to a significant fraction of them, that's a time-consuming job.

7. **PhilG**
January 28, 2010

It will be very hard to hide the fact that they have collided at 3.5 TeV unless they cut public access to most of the status displays and logs.

8. **Tom Rizzo**
January 29, 2010

You must have a magic wand for reading technical slides..I couldn't find those 2 run plans in the Chamonix summaries at all…inside info?? 😉

9. **Peter Woit**
January 29, 2010

Look at Mike Lamont's talk on integrated luminosities, and the summary Steve Myers talk.

10. **Ralph**
January 29, 2010

On splice fixing, I just noticed the following from http://indico.cern.ch/getFile.py/access?contribId=8&sessionId=2&resId=1&materialId=slides&confId=67839 :

Conclusion: For safe running around 7 TeV, a shunt has to be added on all 13 kA joints

In other words, it looks like its not just a matter of fixing a limited number bad splices, they're going to put an additional copper piece around every single high current splice between magnets.

(Thankfully they don't seem to be talking about the splices inside the magnets also – presumably the protection diodes give extra margin there).

11. **robert**
January 30, 2010

i have one question why they have down till february ? what is that for

12. **Peter Woit**
January 30, 2010

robert,

Last year the new quench protection system was not yet fully operational, but they decided to go ahead and start beam commissioning at lower energy (1.18 TeV). This month they're finishing commissioning the quench protection system, and getting things ready to operate at 3.5 TeV. They should be ready to start beam commissioning again sometime in February.

Are There Cosmic Microwave Anomalies?

January 28, 2010
Categories: Uncategorized

No.

The WMAP team has just released a new set of papers based upon seven years of data from their experiment. For a summary of how this new data has sharpened some of their previous results, see the Cosmological Interpretation paper. They have also gone over claims by many groups to have found deviations from the standard cosmological model in their earlier data sets (for example claims to have found "the unmistakable imprint of another universe" which "points to string theory being on the right track.") In a paper entitled Are There Cosmic Microwave Background Anomalies, the WMAP team reports:

> In most cases we find that claimed anomalies depend on posterior selection of some aspect or subset of the data… We examine several potential or previously claimed anomalies in the sky maps and power spectra, including cold spots, low quadrupole power, quadropole-octupole alignment, hemispherical or dipole power asymmetry, and quadrupole power asymmetry. We conclude that there is no compelling evidence for deviations from the LCDM model

They give a humorous example of the problem that plagues typical claims to have found such anomalies, showing that the CMB sky map clearly contains the initials of Stephen Hawking, "aligned neatly along a line of fixed Galactic latitude."

Update: For the WMAP team's summary of its new results for the public, see here.

Comments

1. **Bee**
   January 29, 2010

   Coincidentally, I used the SH in the CMB example in my recent post Is physics cognitively biased?.

2. **Robert**
   January 29, 2010

   The only question I have is why the aliens don't know that Steven's initials are SWH, at least according to his email address.

3. **Christine**
   January 29, 2010

   A simple search in the arxiv with "cmb" and "anomalies" gives 110 hits. Several have been published in peer-review journals.

   I have read none of these, thanks to my skeptic-meter, which is becoming exponentially more severe with time, or specially when other people's skeptic-meter goes down in the opposite direction.

4. **Low Math, Meekly Interacting**
   January 29, 2010

   However, there have been no sightings of the Virgin Mary in the CMB (yet), which is somewhat encouraging.

5. **neo**
   January 29, 2010

   No virgin mary, but an axis of evil (the devil?) has been sighted.

6. **Nige Cook**
   January 29, 2010

   "… clearly contains the initials of Steven Hawking …"

   Typo: it's spelt "Stephen": http://www.hawking.org.uk/

   You escaped 5 points on item 8 of the index of Prof. Baez, since he only credits you if you misspell Hawking's surname, see: http://math.ucr.edu/home/baez/crackpot.html which gives

7. **Peter Woit**
   January 29, 2010

   OK, typo fixed. I tend to get this wrong because my brother spells it the other way…

8. **Luca Signorelli**
   January 30, 2010

   While I've always suspected most of the claimed anomalies weret at best wishful thinking, I can't help being disappointed – the "void" looked really as some interesting mystery to study…

9. **Jeff McGowan**
   February 2, 2010

   OK, question from a mathematician. I was looking at the WMP results page that Peter links to, and found it quite interesting. I'm confused however by the neutrino result, they say the WMP data give you 4.34 \pm .87 species of neutrino, and the standard model gives you 3.04, so it seems to me the WMP data is indicating there is a problem with the standard model, no?

10. **Peter Woit**
    February 2, 2010

Jeff,

Not really. See
http://www.math.columbia.edu/~woit/wordpress/?p=2597
for an explanation of the signficance of an experimental result being 1.5 standard deviations away from the expected value.

11. **Jeff McGowan**
February 2, 2010

Peter,

Thanks, I was assuming the \pm was more like 3 standard deviations, silly me 🙂

Jeff

12. **steve newman**
February 6, 2010

hi-
what do you make of this article which appeared a week ago?

has this been,/ will it be/, responded to by anyone??

it seems like a serious questioning of whole map, and therefore
of all the conclusions drawn from the map.

———————————————————————

"A remarkable inconsistency between the calibrated differential time-ordered data (TOD) of the Wilkinson Microwave Anisotropy Probe (WMAP) mission, which is the input for map-making, and the cosmic microwave background (CMB) temperature maps published by the WMAP team is revealed, indicating that there must exist a serious problem in the map making routine of the WMAP team. This inconsistency is easy to be confirmed without the use of WMAP map-making software. In view of the importance of this issue for cosmology study, the authors invite readers to check it by themselves."
[762] arXiv:1001.4643
Title: Inconsistency between WMAP data and released map
Authors: Hao Liu, Ti-Pei Li,
1Key Lab. of Particle Astrophys., Inst. of High Energy Phys., Chinese Academy of Sciences, Beijing
2Department of Physics & Center for Astrophysics, Tsinghua University, Beijing, China
3Department of Engineering Physics & Center for Astrophysics, Tsinghua University, Beijing, China

http://arxiv.org/PS_cache/arxiv/pdf/1001/1001.4643v2.pdf

13. **John Baez**
February 13, 2010

I'll make an exception and give Peter 5 crackpot points anyway. He can afford them.

LHC Update, More

January 29, 2010
Categories: Experimental HEP News

According to [John Conway](), the decision coming out of Chamonix is to go with the first of the two scenarios described [here](): stay at 3.5 TeV/beam, then a long shutdown to fix all the splices. The idea is to run at 3.5 TeV during 2010 and 2011, stopping for shutdown either when 1 fb^{-1} has been accumulated, or end of 2011, whichever comes first. The LHC will thus be off throughout 2012, coming back in 2013 for a run at or near the design energy of 7 TeV/beam.

With the Tevatron counting on having around 12 fb^{-1} of data at 1 TeV/beam by October 2011, it should remain competitive with the LHC for many sorts of searches, including the search for the Higgs, for much longer than expected. This should be true for more than 3 years from now, until after the LHC has accumulated a significant amount of data at full energy in 2013. The current planning is for Tevatron operation only through FY2011, I wonder whether this will change…

Update: Science has a story from Adrian Cho [here](). The D0 co-spokesperson says the decision on running the Tevatron in 2012 "won't have to be made for several months." CERN experimenters are quoted as saying that they will still be searching for supersymmetry and extra dimensions. I haven't seen any studies of exactly what 1 fb^{-1} at 7 TeV will make possible in terms of doing better than Tevatron limits on such processes and on the Higgs.

Comments

1. **DB**
   January 29, 2010

   The LHC won't even be runnng when the Aztec calendar ends in 2012? There goes one doomsday theory! 🙂

2. **Teva**
   January 30, 2010

   "The current planning is for Tevatron operation only through FY2011, I wonder whether this will change…"

   Let's bear in mind that many particle accelerators became famous (or perhaps I should say many significant HEP expt discoveries were obtained) for reasons completely different from why they were proposed. Pre-WW2 machines didn't usually have names, but post-WW2, the Bevatron was designed to produce the antiproton (it did, and a physics NP), but it really became famous for Luis Alvarez and the bubble chamber and finding many resonances. The AGS proton synchrotron at BNL was built to reach higher energies, but nobody anticipated the 3 NP winning discoveries it produced (J of J/psi, CP violation, 2 types of neutrino species) — there was also the Omega- (Eightfold way). The AGS runs to this day (as an injector for RHIC). SPEAR? Nobody really expected the psi of J/psi, nor the tau lepton (although the idea of a charmed quark had been floated). The SLAC linac and deep inelastic scattering? The PS at CERN was a proton synchrotron of comparable energy to the AGS, and discovered weak neutral currents (Gargamelle bubble chamber), while the SPS at CERN was again a higher-energy proton synchrotron, but it really made history as a p-pbar collider. The muon g-2 storage ring at CERN indeed stored muons and gave a high-precision value for (g-2)_mu, but after that it was used for ICE (Initial Cooling Experiment) to demonstrate the feasibility of stochastic cooling.

   The Tevatron? It has produced the top quark, although the existence of the top was predicted as soon as the b quark was discovered (1977), albeit without a precise prediction for the mass. So … if the Tevatron finds a signal (or possible evidence for a signal … this is very subjective), then indeed the funding profile of the Tevatron will change. Why not?

3. **Pietro**
   January 31, 2010

   Nobel Prize winner Veltman (who won the Prize on issues around the unification of the weak interaction) thinks that the Higgs does not exist, and says so in each of his talks, citing a paper of his from 1991 which he wrote with his daughter. He predicts that there are some subtle changes in longitudinal W and Z boson scattering, but nothing else to be discovered at energies of a few TeV.

   If he is correct, both machines will see very little new physics. It will be fantastically interesting to see who is right: the majority or the "lonely" researcher Veltman.

4. **Ralph**
   February 2, 2010

   Re: "The current planning is for Tevatron operation only through FY2011, I wonder whether this will change…"

   My guess is that the only thing that is certain is that plans will change, the question is how 🙂

   If we get to the end of 2011 and it looks like something interesting is in sight, it might make sense to run LHC at 2*3.5TeV through 2012 also and aim for many fb^{-1} at that energy – in which case keeping Tevatron running would be pretty much irrelevant?

5. **Anon**
   February 4, 2010

   The "rule of thumb" is that you need ~3 times more luminosity at 7 TeV center-of-mass energy to get the same sensitivity as at 10 TeV center-of-mass energy. This is approximate and depends on production mechanism and the mass of the thing you're trying to discover and the backgrounds which can scale differently. Keeping this in mind there's a write-up from last year with some interesting information at:

   https://espace.cern.ch/acc-tec-sector/Chamonix/Chamx2009/papers/MFL_9_01.pdf

   and

   https://espace.cern.ch/acc-tec-sector/Chamonix/Chamx2009/talks/MFL_9_01_talk.pdf

   You should bear in mind that most of the these studies were done quickly making assumptions about how things scale with energy, rather than full simulation.

   In short for Higgs, LHC will overtake Tevatron in sensitivity at high masses but will struggle to be competitive at low masses with 1 fb-1. It seems the Tevatron will not discover a low mass Higgs at the 5 sigma level, but may see evidence for a low mass Higgs with 10-12 fb-1 of data. For SUSY and exotics, LHC should be in new territory.

In reply to the comment about Veltman, there are many people who have studied theories without a Higgs, he may have been the first, but it's an entirely conventional avenue of research now. In many ways the most exciting discovery would be one that nobody has thought of, and discovering nothing would also be enormously important since the whole Standard Model would need a re-think.

Particle Theory Job Market

January 29, 2010
Categories: Uncategorized

Erich Poppitz has updated his statistics on the high energy theory job market to include data from 2009. He counts hirings to tenure-track faculty jobs, using data from the Theoretical Particle Physics Jobs Rumor Mill. For 2009, out of 12 hires listed on the Rumor Mill, he counts 9 as in high-energy theory, 3 as cosmologists. Of the 9 high energy theorists, he counts 7 as in phenomenology, 2 as in string theory. I'm not sure exactly who he is counting as a string theorist, probably Easson (string cosmology) and either Elvang (now working on QFT amplitudes) or Shih (supersymmetry breaking). It appears that it is now essentially impossible to get a permanent job in a physics department if you're working on the more formal end of string theory (or string phenomenology, for that matter). You pretty much have to work in cosmology or phenomenology to have some sort of job prospects.

The academic job market in general in the US is in a terrible state, and this is reflected in the change from an average of around 20 hires per year in recent years to 9 in 2009. It looks like the situation won't be any better for 2010. The imbalance between the large number of new PhDs and postdocs, and very few permanent jobs is quite remarkable. According to the postdoc rumor mill, this year already 8 people have accepted postdocs in Princeton, at the university and the IAS, making this small segment of the community large enough to fill almost all the available permanent jobs.

The US economy remains on its knees due to the economic crisis triggered by the blow-up of debt instruments, especially those designed by quants often coming from a physics background. Luckily for physics PhDs who now have no hope for a job in academia, what I hear from my financial industry friends is that, unlike the rest of the economy, their companies are doing quite well, embarking on new rounds of hiring.

Comments

1. **Tony Smith**
   January 29, 2010

   Peter, you said "… The US economy remains on its knees due to the economic crisis triggered by the blow-up of debt instruments …"
   and
   also that you "… hear from … financial industry friends is that, unlike the rest of the economy, their companies are doing quite well …".

   Have your friends explained how it is that the financial industry, where "the blow-up of debt instruments" occurred,
   has avoided being "on its knees" and is instead "doing quite well"?

   Tony Smith

2. **Peter Woit**
   January 29, 2010

   Tony,

   This is one of the great mysteries of the modern world, but for informed discussion of it, I suggest you frequent one of any number of blogs devoted to the issue. I'd like to stick to math and physics here.

3. **Robert McNees**
   January 29, 2010

   A number of people who have worked on strings (or stringy topics) are responding to this by looking for jobs at Universities and Colleges that don't show up on the Rumor Mill. These are positions at good schools, with reasonable teaching loads and lots of time for research. Yet many of us never hear about them in grad school.

   The last few years really have been bad if you go by the Rumor Mill, but the picture isn't _as_ bad if you consider the full range of jobs that are out there. Though frankly, with the number of PhDs that are being produced, it's hard to imagine a market that could produce enough jobs to keep everyone happy.

4. **neo**
   January 29, 2010

   Fascinating how these accelerative cycles work. In Economics, it was game theorists. Game theory was hot and every department was clamoring for game theorists. New PhDs responded accordingly and became game theory specialists. For a while it was good, but then departments became top heavy with game theorists, especially when the discipline turned more empirical. But the game theory train had too much momentum. The system kept churning out game theorists. Is the same thing now happening with string theorists in Physics?

5. **Erich Poppitz**
   January 29, 2010

   In response to the post and one of the comments: I treat particle theory, phenomenology, and cosmology as one group, and string theory and lattice as the other two. In cases such as those mentioned, I allow myself to assign fractions of a person to different groups (some subjectivity definitely creeps in then; also, I notice the 2009 percentages don't add up to 100, my apologies, will fix). As for looking at jobs not on the rumor mill my only response is that these are very hard to track.

6. **Mesa**
   January 29, 2010

   Well as someone involved in said industry, I can tell you that it wasn't the design of the instruments that was particularly the problem, but the leverage with which they were applied. The decisions to lever up the exposure to these instruments were made by MBAs, not PhDs. IE bankers not quants.

7. **Francois Vanderseypen**
   January 30, 2010

   Did my PhD 15 years ago and it took me two years to find a job thereafter. There is life outside the academic world but it takes a while to make the shift. I have compassion with all those who finish their PhD in math or hep these days and cannot stay in research; it's not fair. The commercial world does benefit from all these smart people but it's sad for hep and there is much hidden (personal and/or psychological) pain underneath all this.
   Even without the economic factor, the whole publish or perish atmosphere is a destructive element on the long run. The current state is the result of an incorrect perception

about what 'research' is, I think. Probably it'll take a long time before the hep community has made a shift again.

8. **whaddyagonnado**
January 30, 2010

Not all phds apply to be postdocs. Many people start a phd with no intention of a career in academia. So it's not such a great problem that there are so many phds being churned out.

A bigger issue is the proportion of permanent/faculty jobs compared to postdoc positions, because once you've started a postdoc you're committed to the academic path. You can still get out of course, but it's more painful than when you're fresh out of grad school.

9. **Michael**
January 30, 2010

Just a piece of information: there is a major decline in the number of jobs for experimentalists in particle physics, too, based on the data posted at http://www.freewebs.com/heprumor/index.html . There appear to be 9 openings in 2009-10 compared to 16 in 2008-9, 31 in 2007-8 and 24 in 2006-7.

10. **Bob Levine**
January 30, 2010

Several years ago a rather chilling essay appeared in PHYSICS TODAY, whose author laid out a number of quite reasonable (and deliberately optimistic) assumptions about the future demand for physicists in the U.S., based on retirement rates, expansion prospects for academic programs, employment patterns in the private and military/government lab sectors, and several other parameters, and then worked out what the rate of Ph.D. production would have to be just meet these requirements. It's been a while, but as I recall, he concluded that if each senior faculty member currently employed in a university with a doctoral program in physics turned out, on average, *two* Ph.D.s over a whole career, that would be about right to meet all the need for physicists—academic, industrial, military—for the next generation at least. The obvious problem was, he noted, that no graduate program in physics in its right mind would throttle back its doctoral graduation rates to that level.

And this was *way* before the current economic implosion we're going through….

11. **Thomas Larsson**
January 30, 2010

In a steady-state situation, each advisor will on average give birth to one new advisor in the next generation. So if each advisor on average produces 10 PhDs over his career, 90% of them will not become advisors themselves.

I don't understand how that can be a surprise for anybody.

12. **Paul Wells**
January 30, 2010

Sorry -but regarding the comment " I have compassion with all those who finish their PhD in math or hep these days and cannot stay in research; it's not fair." I don't agree. I don't think there is an entitlement to work in the fields of particle physics or any other area. The current funding "crunch" (= rationalization) in particle physics has been obvious for many years. Experiments cost too much and theorists have been delivering too little on their former promises. Nine sounds about right for particle physics theory new-hires.

Physics is a fantastic field and great training for many fields. Just look at number of Physics grads working in electronics and computer areas.

If particle physics is going through a lean period – change to solid-state ! Or even consider going into industry and help the U.S. economy.

BTW I have a PhD in Physics. Left academia years ago and wish I had done it earlier.

13. **Frederik Denef**
January 31, 2010

Things are a little better than what the rumor mill suggests. Last year, three of our own string theory postdocs got faculty jobs: one in Alberta, one is Austin and one in Milan. All three are doing mathematically oriented string theory. There have been several fairly recent string hires in Europe, places like the Simons center will end up hiring quite a few too, and the hiring freezes at places in the US will not last forever.

14. **Robert McNees**
January 31, 2010

Frederik: At least one of those jobs was in a Math department, right? Was that job listed on the Rumor Mill? I'm curious about the number of jobs over the last few years that support stringy stuff, but weren't on the Rumor Mill.

15. **Peter Shor**
January 31, 2010

Thomas Larsson says:

> In a steady-state situation, each advisor will on average give birth to one new advisor in the next generation. So if each advisor on average produces 10 PhDs over his career, 90% of them will not become advisors themselves.
>
> I don't understand how that can be a surprise for anybody.

This makes the quite unreasonable assumption that the **only** jobs that a PhD physicist is qualified for is tenured professorships in PhD granting institutions. If this is the case, it's quite clear we're teaching the wrong stuff in our PhD programs.
For one thing there are lots of non-PhD granting colleges who need to hire faculty to teach physics to undergrads. The requirement for these faculty is generally that they be PhD physicists (and some of them at the better colleges are actually incredibly good PhD physicists). There are also lots of jobs for PhD physicists in industrial labs, which also don't grant PhDs.

So I would guess that the steady-state number is at least 3 or 4, and the distribution should be biased towards PhDs at top universities.

On the other hand it seems quite likely that we are granting too many degrees in physics.

16. **Thomas Larsson**
January 31, 2010

What???
I never claimed that PhD physicists were unfit for anything but professorships at PhD-granting instutions. On the contrary, 90% better be qualified for other jobs, in industry, government, education or academia. Note that I used the word "advisor" rather than "professor", thus excluding professors at non-PhD granting institutions.

17. **Arun**
January 31, 2010

Some 40% or more of graduate students in physics in the US are(? – definitely used to be) from overseas. Assuming that Asian universities are staffing up, and economic growth continues there, some number will be taken up there.

18. **anonymous**
January 31, 2010

I can also vouch for the market being far better than this suggests. Three present/former postdocs I know from Stanford got faculty jobs last year: one at Heidelberg, one at DESY, and one at Ecole Normale Superieure in Paris. (Not to double count the one who Frederik mentioned who is now in Milan). So if you're out there thriving in your research and this message stream panics you, relax.

19. **anonymous**
January 31, 2010

Oops, I should have stressed: two of those can only be called string phenomenologists, and the third does very formal mathematical string theory. [The one now in Milan, who should not be double-counted, also does very formal mathematical string theory].

20. **Peter Woit**
January 31, 2010

It's interesting to hear that the job situation is better in Europe (at least on the continent, one hears lots of bad news from Britain). At some point things presumably will pick up again in US physics departments, but in the meantime, it sounds like those looking for a job in this field need to learn a foreign language (German, French, Italian, or mathematics…).

21. **Garbage**
January 31, 2010

I think there are 2 points here. 1) Whether string theorists should be learning a new language/field (or returning home); 2) Whether we should tell PhD students (and postdocs) in universities other than Harvard, Princeton, Stanford, etc. (very short 'etc.' indeed according to Poppitz), they should seriously consider life outside academia.

On the other hand, this year postdoc jobs was unusually good (although arguably bleak for string theorist.) Not sure whether this will be a trend or not, only the future will tell…

22. **ObsessiveMathsFreak**
February 1, 2010

There are probably lots of interesting jobs and research programs outside of theoretical particle physics which in fact result in things that are actually useful. Not to be too blunt about it, but the field of particle physics—and theoretical physics in general—has not made leaps and bounds in recent years.

There are countless unsolved problems and phenomena in low temperature physics, astronomy, optics, fluid mechanics, etc. There were the bread and butter problems of physicists in days gone by and perhaps the world would be better served by young minds researching in these fields rather than adding yet more impenetrable papers to the hep-th heap?

23. **Trent**
February 2, 2010

I'm surprised to see that nobody mentioned the obvious: having lots of PhD's and few jobs is good for competition. Only the best people will get hired and in order to end up with the best one needs to start from a large pool. Exactly how things are right now. Problem?

24. **Arun**
February 2, 2010

"…having lots of PhD's and few jobs is good for competition. Only the best people will get hired and in order to end up with the best one needs to start from a large pool. Exactly how things are right now. Problem?"

Yes, the field becomes much less attractive, and good people will do something else. Over time the field is impoverished.

25. **Christine**
February 2, 2010

Trent wrote:

(…) Only the best people will get hired (…)

Define "best". Sometimes merit is undervalued against one's social capital and other interests, as science is a social activity after all. With time, too strong a competition may probably "naturally select" not the honest-type of scientist but the more aggressively ambitious-type (namely, that who aims at success and power above all).

26. **Francois Vanderseypen**
February 2, 2010

From an evolutionary point of view the idea that natural selection picks out the best, Trent is right. In the same fashion, World Wars also help nature in selecting (according to some dictators) the 'better' ones. So, the key issue in all this is the criteria you handle in this process. What is 'better' or 'best'? What is the norm? I think often criteria are used (to pick out PhD's) which are very subjective and politically loaded. The criterium 'creativity' or 'deep/visionary' is usually irrelevant.
Most of all, perceiving this (selection) process in this way disposes the fact that science is a human activity and PhD's have feelings, families, responsibilities, dreams… Discarding this dimension of life (and doing research is part of life, no?) is ignoble as far as I'm concerned.

27. **Paul Wells**
February 2, 2010

U.S. PhD takes too long. In U.K PhD takes 3 years. IMHO this is better for society and makes it easier to transition to non-academic career.The issue might be that U.S. PhD is too long and too narrow. Being cynical PhD students are just being exploited as cheap labour.

28. **babel**
February 2, 2010

"The criterium 'creativity' or 'deep/visionary' is usually irrelevant."

True, because you can't quantify it. How would you select people based on creativity, or vision? You'd just create more opportunity for corruption and bias.

Do you think the great Renaissance artists were hired based on some vague criteria like the above? No, they were hired based on previous works. And at the sole discretion of the employer. It was a free market of art. That's how you get masterpieces.

Not by imposing or saying hey more jobs should be available or whatever. Or that this and that should be the criteria. If a university wants to be competitive, it will be in its own interest to hire the most capable. Eventually natural selection will provide the best criteria. No one forced anyone to get a PhD, so no one should be issuing any sort of demands or expectations. If it's anyone's fault, it's the fault of everyone who encouraged students to get a phd in the first place.

I do agree however that getting a phd should be harder, as should getting an undergrad degree. Giving them out is bad for everyone. If you pick a random bum of the street and give him a phd, he'll still be unqualified for advanced work, but he won't lower himself to flipping burgers anymore either. Since he has a phd and all.

Yet people complain that too few people have access to higher education. Make up your mind.

Best,
A future phd student in mathematical string theory.

29. **Me**
February 2, 2010

"Eventually natural selection will provide the best criteria."

natural selection doesn't provide best criteria, only sufficient ones for survival. That's all.

30. **Trent**
February 2, 2010

Christine and others,

Imagine you are sitting on a hiring committee. You need to decide who to hire from 50 applicants and you have 3 jobs to fill. You will surely rank the candidates according to some criteria that you think is the most reasonable. Naturally you will pick the 3 best candidates. So, naturally, you will have the definition of 'best'. It will the definition that you think is the most appropriate.

Would you prefer to have the criteria given — or dictated — by somebody? Would you be happier if somebody told you who to hire?

I'm sure the answer to both questions is no. If *you* are on a hiring committee *you* will define 'best'.

This is exactly the current system, there are no uniform guidelines dictated or suggested by somebody outside the decision makers, each university, each department, each hiring committee and each member of the hiring committee makes up his/her mind and defines what is 'best'.

Christine, you wrote,

"
With time, too strong a competition may probably "naturally select" not the honest-type of scientist but the more aggressively ambitious-type (namely, that who aims at success and power above all).
"

Do you have any facts to back this claim up? Factual, quantifiable facts I mean.

Trent

31. **martibal**
February 3, 2010

To Trent

"I'm sure the answer to both questions is no. If *you* are on a hiring committee *you* will define 'best'."
So don't call it "the best", call it "the one that at instant t and place x best serves my interest". And it could be the one who seems to produce the most interesting papers, or the cutest one with whom I am sleeping and is supposed to marry me (but will left me as soon as he/she is hired), or the one that will help me in my administrative task, without challenging my scientific ideas.

"Would you prefer to have the criteria given — or dictated — by somebody? Would you be happier if somebody told you who to hire?"
Yes, I would be happy that the committee makes its decision on clear and public criteria. "the best" is an empty word. Clear criteria need not to be dictated from the outside.

When you are in hiring committe, do you take the time to really study the applicants' papers ? I guess no because you are very busy and it would take several days/weeks to fully study the work of 50 candidates. And of course one week of a hiring committe member (who, by the way, has been at some time pointed out as "the best", hence considers himself that his opinion needs not to be justified) is worth more than the work-lifetime of 50 candidates.
The most honnest thing I have heard on hiring committe is from a professor explaining that, once a short list is made, all the candidates would fit the position. The process is then totally random, and it would not be less fair to just throw a dice.

32. **Thomas Larsson**
February 3, 2010

Natural evolution selects the fittest. Fittest is the person that was selected.

33. **Peter Woit**
February 3, 2010

Please, enough mounting of hobbyhorses about the fairness/unfairness of how hiring decisions get made.

34. **sourgrape**
February 3, 2010

If Peter Woit can get a job, why can't I get one?

35. **Peter Woit**
February 3, 2010

sourgrape,

I suspect you have a job. And, life is unfair…

36. **Alexey Petrov**
February 3, 2010

Very strange study — looks to me that the data is not accurate — even if we consider Particle Rumor Mill as a source (note that some particle jobs were also listed on the Nuclear Rumor Mill).

For example, in 2001, when I got my job, there were 31 openings in the US with 21 first-time hires — that is according to the Rumor Mill. Erich lists only 19 — why? Not to mention that my job does not exist according to page 2 of his study…

37. **Alexey Petrov**
February 3, 2010

To add to my post above — there were 6 new hires advertised on the Nuclear Rumor page that went to particle theorists (if you consider QCD theorist as particle theorist). And there is also Astro rumor page that aslo lists cosmology jobs. So the studies are at least not accurate… yet it is clear that the number of faculty jobs in particle theory went down quite a bit compared to some years ago…

38. **Erich Poppitz**
February 4, 2010

In response to Alexey's comments: yes, there is an inherent inaccuracy stemming from use of Theory Rumor Mill data only. I have now put the disclaimer on top of the page. On his concrete questions about the memorable 2001: yes, indeed there was an omission, and the correct number is 20, so we split the difference (as for his job, it was listed on p.2, which I have made clearer now).

39. **Doug Natelson**
February 5, 2010

I can't comment on high energy hiring in particular, but having served on and chaired search committees, I can talk about hiring in more general terms. Regarding martibal's comment above…. If the search committee has done its job well, then yes, you would hope that everyone on the short list is "above the bar", so to speak. By that, I mean that everyone on the short list would (a) stand a reasonable chance of research success (as much as anyone does; there is some luck involved, after all); (b) not be an embarrassment in the classroom (and ideally would be an excellent teacher and clear communicator); (c) not have major problems getting along with colleagues; and (d) be able to interact well with students; not necessarily in that order.

That doesn't mean that choosing among the short-listed people is random. After a visit (with individual conversations w/ faculty members) and (in our department's case) two talks (one to the whole department and one about research plans to the search committee), it becomes clear that the different short-listed candidates bring different strengths to the table. One might be a more dynamic, interactive person; one might be more creative or visionary in terms of research; one might fit very well into the perceived research needs of the department. At this point, it's a matter of the search committee really refining what they want and need, having seen the choices before them.

For what it's worth, in my limited experience I think in general that hiring practices in academia are certainly no worse than in other professions. Committees may tend not to be bold or adventurous, but at least there's a process, as opposed to the whims of a HR department + keyword search of resumes. The ratio of applicants to academic positions makes the process painful from the applicant side far more than the hiring process itself.

40. **Chris Oakley**
February 6, 2010

Here is one possible job opportunity: teaching string theory to Anne Hathaway.

41. **martibal**
February 6, 2010

Is this a tenure track, or just a 1+1 year postdoc ?

42. **Chris Oakley**
February 13, 2010

Martibal,

You would have to check with her agent.
Another female celebrity who might be into it is Courtney Love.
She apparently likes "watching quantum physics videos on YouTube".
I must admit that I did not know there were any, but now I will check it out.

Various and Sundry

February 6, 2010
Categories: Uncategorized

- Now that the plan for running the LHC over the next few years is in place, one can start to get an idea of what new physics might emerge from it between now and 2013. For the question of the Higgs, Tommaso Dorigo does some analysis here, going back to 1999 Tevatron projections to see how reliable they were. He concludes that the 1999 projections were accurate for the mass range above 135 Gev. Below that, they depended on assuming a silicon detector upgrade that never was funded. His bottom line is that he sees the Tevatron as ultimately able to rule out the Higgs at 95% confidence level over the entire relevant mass range, but unable to come up with convincing evidence of its existence if it is in the lower part of this mass range. For this, the LHC will be required, but this will have to be after the move to higher energy in 2013:

> The LHC experiments will be unable, in my opinion, to make up in two years of data taking, and with the 3.5 times larger energy, for the 8-year advantage in running time of the Tevatron. The Higgs boson will be unlikely to be discovered before 2013, and it will probably be a sole LHC business; however, until then the Tevatron will retain the better results as far as the mass exclusion range is concerned.

- Operating on a different reality plane is Michio Kaku:

> "We're beginning to test string theory with the large Hadron collider outside Geneva, Switzerland, costing ten billion euros, the most expensive machine that science ever created. That's what I do for a living," said Kaku in a recent conference call interview from New York.

This is from a story mainly about Kaku's new TV show on the Discovery Channel, accurately entitled Fact or Fiction? Physicist Dr. Michio Kaku blurs the line between science and science fiction.

- NPR has recently started up a project called 13:7 Cosmos and Culture. It's a blog "set at the intersection of science and culture." Unfortunately, NPR's conception of the intersection of physics and culture is occupied by Stuart Kauffman, who has a series of posts arguing that the physical universe cannot be described by physical laws (see here and here). In the most recent one, Kauffman takes up the complicated subject of decoherence and the emergence of classical behavior in quantum systems, and claims to have (inspired by Karl Popper) an argument based on special relativity showing that decoherence cannot be described by any fundamental law of physics. This is supposedly experimentally testable:

> As it happens these ideas may have testable consequences, for they should be more marked as the relative velocities of the event A and one or two receding detectors increase toward the speed of light. And, since quantum decoherence is easier if the quantum processes in the "environment" are locally abundant, they should be more visible in that case. These are testable consequences of Popper's original idea and my use of it with credit.
>
> I hope the experiments are done.

For more about all this, Kauffman refers to his article here from the Edge web-site, where he argues that that the brain is "quantum coherent", and:

> Reversibility of the coherent to decoherent-classical to recoherent quantum states are essential to my hypothesis for I wish the brain to be undergoing such reversible transformations all the time.

He gets around problems with time-scales by noting that:

> The time scale of neural activities is a million times slower, in the millisecond range. But it takes light on the order of a millisecond to cross the brain, so if there were a dispersed quantum decohering-recohering mind-brain, reaching the millisecond range is probably within grasp of a quantum theory of the mind-brain system.

I suppose it is true that it might take light a millisecond to cross one's brain, if one's brain were about 200 miles across…

- Normally I don't think I can ethically post gossip about mathematician's love lives here, but once it has already appeared in the media…
- Some ex-colleagues from here at Columbia are among those launching the Journal of Unpublishable Mathematics. From what I hear, they haven't yet published anything, but have had nominations.
- Last week the algebraic geometer Eckart Viehweg passed away at the age of 61. His wife Helene Esnault is also an algebraic geometer, and recently posted an article on the arXiv based on joint work, with a heart-breaking abstract.

Comments

1. **Peter Shor**
   February 6, 2010

   Kauffman's right (in some sense). Decoherence cannot be described by any fundamental law of physics, as it is an emergent phenomenon (presumably fully explained by the fundamental laws of physics, although I don't believe we completely understand it yet).

2. **Peter Woit**
   February 6, 2010

   Peter Shor,

   That's not the argument Kauffman is making. He's not saying that decoherence can't be "described" by fundamental laws, he's saying that it can't be entailed by fundamental laws (it doesn't "emerge" from anything):

   "Since Newton we scientists, particularly physicists, have believed that all that unfolds in the universe is entailed by the fundamental laws of physics. I believe this view is false and its implications deeply alter our world view, heal the breech between science and the humanities, much of it discussed in my book "Reinventing the Sacred"."

   His argument is that relativistic causality implies this, and that this argument has experimental implications which can be tested. I don't see enough detail to figure out what these supposed experimental implications are.

3. **anon.**
   February 6, 2010

   I suppose it is true that it might take light a millisecond to cross one's brain, if one's brain were about 200 miles across…

   Well, light does take longer to cross a medium with a high index of refraction. I have little trouble believing that Kauffman's "mind-brain" is so incredibly dense that the speed of light within it is reduced by a factor of a million.

4. **Chris Oakley**
February 6, 2010

Kauffman could be referring to [Marvin, the paranoid android](), who, apparently, has a "brain the size of a planet".

5. **John Romeo Alpha**
February 6, 2010

Maybe he means that light traveling the same twisted path as regular neural activity would take a millisecond, so that this "dispersed quantum decohering-recohering mind-brain" could accomplish something in a millisecond which is not possible with a real brain and its pokey neural activities.

6. **Abhijnan Rej**
February 6, 2010

Hi!

Eckhart's death is really sad… I knew Helene and Eckhart for some time now, and both of them had the German algebraic-geometric community in place, with their common sense, sharp remarks (I remember seeing Helene stop Pierre Cartier short!) and just good will for people starting out!

BTW, their work on surface singularities is completely nontrivial!

Obi

7. **Bee**
February 7, 2010

I skipped over most of what Kauffman wrote, but in a nutshell he seems to be saying (correct me when I'm wrong), if decoherence happens on a hypersurface of simultaneity then this surface is observer dependent, meaning if you boost high enough in some frame it will not be simultaneous. That's hardly a new observation is it? Besides that the detector does provide a reference frame and thus I cannot see any fundamental problem with SR, I frankly don't quite get why this means it cannot be described by "physical law." Unfortunately, Kauffman's essay isn't really insightful, and it's quite unclear what he wants to observe and conclude. It might be it's just in principle unobservable, or eventually an empty statement. I mean, consider the usual EPR type setting, with an entangled pair of spins. You measure one in a detector, what happens to the spin of the other particle? Or, more importantly here, when does it happen? Problem is, it's entirely irrelevant what happens to the other particle if you don't measure it. Well, I guess what I'm saying is I don't know what he's saying.

8. **Ulla**
February 7, 2010

Kauffman talks of 'order for free' or self-organization in biology. self-organization is done by an entropy minimizing process, seen as instanse in the formation of virus shells and protein-folding.
[http://www.edge.org/3rd_culture/kauffman06/kauffman06_index.html]()

The automized process demands an earlier input of entropy, as an information or a codex, though, so this is only seen as a delayed response in my eyes. The order is there implicitly, and the folding makes it explicit.

Physical forces outside fundamental forces would peak in the direction – a Universe created by God, or the creationist wiev. This seems not be the case.

So the question is of the emergence of the Universe or not, the emergence of information. The same as Verlinde spoke of and Lubos discuss in
[http://motls.blogspot.com/2010/02/entropy-information-and-mixed-states.html#more]()

And that is not outside the fundamental laws at all.

Interesting though that he speaks of new biology 🙂

9. **Peter Shor**
February 7, 2010

Hi Peter,

I know that's not the argument Kauffman is making. I just found it ironic that he could be right (in one sense) and totally wrong at the same time.

10. **Peter Shor**
February 7, 2010

I should have remembered that irony (like entanglement) cannot be transmitted over the Internet.

11. **Bill K**
February 7, 2010

"Some ex-colleagues from here at Columbia are among those launching the Journal of Unpublishable Mathematics. From what I hear, they haven't yet published anything, but have had nominations."

Of course not, Peter. The actual publication of a paper which cannot be published would create a logical paradox.

12. **mathphys**
February 9, 2010

Testing string theory at the LHC oustide Geneva, the most expensive machine that science ever created, is an honorable way to make a living. I'm touched by Prof Kaku's modesty.

13. **Coin**
February 9, 2010

The Emperor’s New Mind strikes again 🙁

14. **John Baez**
February 13, 2010

Peter wrote:

> I suppose it is true that it might take light a millisecond to cross one’s brain, if one’s brain were about 200 miles across…

Are you accusing Stuart Kauffman of having a big head?

> Some ex-colleagues from here at Columbia are among those launching the Journal of Unpublishable Mathematics. From what I hear, they haven’t yet published anything…

Just as you’d expect, given the title. Perhaps our library can afford this one.

Expanding Crackpottery

February 9, 2010
Categories: Uncategorized

Lubos Motl is getting rather concerned (yes, I know about what pops up when I link to his blog…) about recent trends in theoretical physics, especially the implications of recent work (discussed here) of well-known string theorist Erik Verlinde. He claims that many other string theorists share his concern:

> The people whose knowledge and opinions about physics are close to mine are finally beginning to realize the worrisome trends affecting the quality and character of the research in theoretical physics. I have a significant number of e-mail exchanges with these folks – and let me assure everyone that you're not alone.

In many ways I also share Lubos's concern. Like lots of people, over the years I've been deluged with examples of what I'll call "unconventional physics", in a spectrum ranging from utter idiocy to serious but flawed work. Much of it shares the all-too-common feature of making grandiose claims for new understanding of fundamental physics, based on vague ideas that often use not much more than a few pieces of high-school level physics and mathematics. The beautiful and deep physical and mathematical ideas that go into the Standard Model are ignored or thrown out the window. In many cases, it's hard not to suspect that the authors have decided that they can replace modern physics, without bothering to take the trouble to learn what it is. This kind of thing is pretty easy to quickly identify and decide to ignore, and it ends up having no impact on the scientific research community.

In recent years though, some theorists who definitely understand and have made contributions to modern physics have started promoting research which looks depressingly like the typical sad examples of "unconventional physics". Many of the products of the ongoing multiverse mania fit into this category. Lubos is getting quite worried to see that a very talented and well-known leader of the string theory community, Erik Verlinde, seems to be engaging in this sort of research, and getting positive attention for it. Within a month of its appearance, Verlinde's "Entropic Force" paper has already generated a dozen or so preprints from other physicists on the same topic. It could easily end up being the most influential (in the sense of heavily referenced) paper of 2010. Seeing this coming from a string theorist he admires is worrying Lubos and his correspondents.

While I agree with Lubos that this is something worth worrying about, his interpretation of the problem is characteristically irrational. In his posting, he argues that this is all due to the influence of the "notorious crackpots" Lee Smolin and Peter Woit. I don't see how I'm supposed to be responsible for prominent string theorists taking up dubious lines of research I strongly disagree with, other than perhaps having some responsibility for driving them over the edge. In any case, Lubos concentrates his attack on Lee Smolin, arguing that he's the one mainly responsible for this, an idea which is completely absurd. While Smolin is surely more sympathetic than I am to research like that of Verlinde, he's a serious scientist and not one with a lot of influence over Verlinde and the string theory community. Lubos's argument that this is all a left-wing plot organized by the far-left radical hippie Smolin is just laughable. One merciful thing about the string wars always was that positions people took were uncorrelated with their political ideology, keeping politics out of it.

Unlike Lubos though, I'm not convinced that I understand what the source of the problem is. My diagnosis of the current state of the field remains what it was when I wrote my book quite a few years ago: the lack of relevant experimental data coupled with the faddish pursuit of a failed idea about unification has led to a disturbing situation. In a very deep sense though, I just don't understand why talented physicists react to this by engaging in things like anthropic string landscape research, or vague arguments about "entropic forces". Lubos is right to notice that this situation has recently become more disturbing. A debate about the causes of this involving people more sober than Lubos would be a good idea. Twenty-some years of string theory hype in the scientific literature and popular press did a lot of damage, and if this gets replaced by hype of ideas even more dubious than string theory unification, things will go from bad to worse. Maybe the LHC will save us, but if this is what it takes, it looks like we're stuck for a few more years.

Comments

1. **Daniel de França MTd2**
   February 9, 2010

   This story of relating general relaitivty , to holography and thermodynamics, is god-know-how-many-years-old. There are several papers concerning this matter by Raphael Bousso which has between 100-500 citations. It was intensively used as tests to know if black holes of higher dimensions in SUGRA had the correct entropy. And this not counting just relating GR with thermodynamics, which is even older. Jacobson had papers about this 16 years ago.

   Now, I don't get is what all this fuzz is all about since Verlinde merely made a paper about a super simplified case, using Newtonian gravity…

2. **capitalistimperialistpig**
   February 9, 2010

   My guess is that Lubos has done more damage to the reputation of string theory than you and Lee combined. It hardly improves the credibility of your science if a practitioner behaves a lot like the crazy guys on the street corner talking about the end of the world.

3. **capitalistimperialistpig**
   February 9, 2010

   I have to say, though, that your snide remarks about the entropy work remind me a lot of what I heard about black hole radiation and Beckenstein's entropy back when – and yes, I am that old.

4. **Bee**
   February 10, 2010

   Well, if you believe Lubos, Lee is single-handedly responsible for every evil in this world. It's quite funny actually. Unless you're Lee I suppose.

   Anyway, you know what disturbs me about this isn't even Verlinde's paper. I don't really know what to make out of it, but he has evidently put work into it, it's thought through, maybe he has a point to make, I don't know. I don't get it, but maybe it shouldn't be dismissed too easily. It probably took quite some courage to put this out. But thing is, I'd usually have ignored it. What I find disturbing is how quickly people are jumping on the topic. I mean, look at this, it's a matter of weeks! The thing goes through the blogs, is in New Scientist, and so on, and so on. I mean, really, what's this? The-Making-Of the topic for 2010? After we had Horava-Lifshitz and Unparticles (and whatever happened to that?). I take some comfort of living in a relatively hype-free North European country (there is one person here who reads blogs! No really! But he didn't know about Cosmic Variance. And he didn't read your book either. And some other guy read your book but thought Lee wrote it. Or the other way round, forgot.) Now guess what I found on the printer yesterday. Yes, that's right, Verlinde's paper 🙂

5. **QED**
   February 10, 2010

Verlinder did nothing wrong! He has every right to publish
his results. If you get angry it is your problem. I don't see
the moral level of our physics community is evidently lower
that in 70's or 60's. There are always some very interesting
papers, some less interesting papers, so nothing worths
making fuss about.. In this regard, Verlinder's paper is
much better than many not-even-wrong papers.

6. **Per**
February 10, 2010

Hi Peter (if I may)

You are way to negative!

Sure, perhaps the Verlinde paper is nonsense, but as Bee said, he did 1) put serious work into it 2) he has atleast prior to this proven he is not an idiot (no matter what one think of string theory) 3) those two facts combined, it wouldn't really make any sense if he put something really stupid out.

Of course, it could happen, but this off hand dismissal just because it has a totally novel approach I find somewhat, hmm, unscientific.

7. **fh**
February 10, 2010

I'm with bee, the hype is disconcerting. BUT I think the paper itself is refreshing, at least compared to the myriads of conceptually trivial papers that are mathematically sophisticated this is a paper that tries for come real conceptual discussion. And it doesn't come out of nowhere Ted Jacobson, Black Hole Entropy, holography, etc.. This paper is in good company.

No the problem isn't with this paper but with the fetishisation of highly technical calculations (not Maths most of the time mind you, just calculations) that add no insight whatsoever. Philosophy is considered a derogatory term (see Polchinskis rant against "word ideas"), and as a result conceptual discussion is automatically labelled crackpot.

It can be. Crackpottery often pretends to be conceptual discussion (though not always, see the Bogdanov affair), but it can be distinguished from genuine, measured, precise debate. Fundamental physics just has forgotten how preferring instead to focus on more "objective" measures like technical skill.

(BTW, since somebody will misread this, technical skill is absolutely highly important, but it seems to me it is emphasized to the detriment of everything else, including, BTW sound mathematical reasoning)

8. **Michael Varney**
February 10, 2010

@capitalistimperialistpig

Lubos is pretty much not taken very seriously by many string theorists I know. They recognize him because of his blog, political leanings and his attacks on others more than for any recent physics contributions.
I would worry little about any damage to the reputation of string theory due to Motl. Many consider him to be traveling the road to becoming a crank.

This is pretty obvious from his post moderation, and his contention that people take his blog more seriously than peer reviewed papers. =)

9. **Peter Morgan**
February 10, 2010

There are reasons why people are getting onto Verlinde's paper. Partly it's his prestige, but it's also partly because there *are* previous works that are mathematically interesting that people are pointing out, and links into other research directions will be developed progressively. Verlinde has definitely not walked into a vacuum. I imagine that 't Hooft and others who have been working with stochastic or random fields will watch developments with interest, as I shall. We'll find out in a few years (or decades) whether Verlinde has marred his reputation or pointed out a path that becomes interesting and empirically relevant as it becomes increasingly thought through, both mathematically and philosophically.

Although I take your point that "The beautiful and deep physical and mathematical ideas that go into the Standard Model are ignored or thrown out the window", I think it's a little too much. A new approach to Physics has to pick *something* that it can do in an interesting way, then it has to construct a Correspondence Principle between itself and QM/QFT/the standard model that is mathematically elegant and that works well enough to be useful. The second step is very difficult to do well enough, the more so as the conceptual distance is greater. Lee Smolin's valley has to be crossed. If you had held Planck or Bohr to your standard, we might never have got from classical Physics to QM, since, emphatically, none of that work reconstructed all of classical Physics. The new QM more-or-less solved the Correspondence Problem with classical Physics in the late 1920s, but we still live with the slight failings of our understanding of how that works precisely.

Despite these reservations, good post!

10. **joel rice**
February 10, 2010

Maybe it just makes more sense to put heavy theorizing on hold
until the LHC can weed out the unproductive paths .

11. **Peter Woit**
February 10, 2010

joel,

I think that has been a lot of people's attitude for nearly a decade (at one point the LHC start date was 2005), and it hasn't worked out well. Right now, if everything goes according to plan, it seems likely that significant new information from the LHC won't come until data from the 2013 run is analyzed and released, possibly in 2014. So, best case scenario is four more wasted years. And then, what if the LHC data just agrees with the Standard Model?

12. **Fabio**
February 10, 2010

Its not just formal theory that is having quality control problems. A large portion of the phenomenology community spends it's time shamelessly chasing the statistically insignificant experimental anomaly du jour. Some phenomenologists I know have privately indicated disgust at the situation. I'm not sure exactly how Lee Smolin can be blamed for this, but there must be a way, because he is the devil. But citations for crappy papers are better than no citations for crappy papers not written, so it will continue to get worse before it gets better.

13. **johan couder**
February 10, 2010

Perhaps the source of the problem is identified in your book: "It is also true that there are no alternatives to superstring theory that one can easily learn and quickly start doing research into. Other ideas remain very little developed, and many of them require dealing with a whole slew of different mathematical ideas that are not part of a physicist's normal training." (p. 269 in my copy). "…, since readable expository material about much of modern mathematics is sorely lacking.", why spend (waste?) years of learning exotic mathematics, if high-school math(s) will do for the time being?

14. **Peter Woit**
February 10, 2010

Per,

I'm not critical of the Verlinde paper because it is "totally novel" (which it isn't), but because I read it and don't believe you can get a serious scientific theory of the gravitational force from what he starts with (thermodynamics + vague ideas about holography + high school mathematics).

As I wrote in my posting, I don't understand why a smart, capable scientist is putting out this sort of paper, just as I don't understand why a lot of other smart, capable people pursue multiverse pseudo-science.

15. **F**
February 10, 2010

There are a number of instances where simple 'high school' calculations can provide significant insight and/or lead to significant advances in a subject. The Jones polynomial for knot invariants can be derived using elementary ideas in geometry (basically three Reidemeister moves) and some trivial algebra. See Kauffman's work for an easy introduction. However, before the Jones polynomial was discovered in the 80's, mathematicians toiled with the Alexander polynomial for over 60 years by studying a very formal branch of mathematics dealing with the fundamental group theory of the complement space of a knot embedded in 3-space. I recall it took me a whole semester and about 100 pages of formal theory to just arrive at a rigorous understanding of the Alexander polynomial knot invariant. And despite all this effort, it can't tell the difference between the trefoil and its mirror image!! (something the Jones polynomial can). Despite its simplicity the Jones polynomial is connected with deep mathematical ideas.

Is this the fate of string theory??

In any case, one can slave away for decades on a highly sophisticated and formal subject that is considered 'acceptable' for academic research – and make progress – but if an an alternative idea comes along wrapped up in some simple 'high school' mathematics, we shouldn't be so hasty in rejecting it as 'nonsense', especially when there are deep physical insights underlying the arguments….

16. **F2**
February 10, 2010

I don't really see what's the trouble here. Why would I complain about someone investing his or her (private!) time in a new approach? If the Higgs ect. is there, then good for the core Standard Model physicists. This model works quite well for what it should do and of course people like that. If one wants to go beyond that in any way – why do other people have a problem with someone who thinks about something not semi-containing the old theory? If it doesn't work out, I guess it was worth a try still. If it's just high school mathematics, as you say, then people won't stick to it like to the string theory unification approach caus then people understand it and understand that it won't work. I think it's a bigger problem, that some physicists don't want to share their last two or three decades of lifetime with something that might seem far fetched, because they learnt something else, the past three decades. old story.

17. **Peter Morgan**
February 10, 2010

"I read it and don't believe you can get a serious scientific theory of the gravitational force from what he starts with (thermodynamics + vague ideas about holography + high school mathematics)." So should we have told Einstein not to worry his little head about a trivial, vague thought experiment that compared and contrasted acceleration and gravity, and not to look for a mathematics that makes that intuition into something powerful and useful? Of course not every intuition comes to anything, but, even if you might be right about Verlinde, you're sounding a little testy here. We can celebrate Verlinde's voyage even if he might sink. It's a strange, troubling sport to see you make this particular common cause with Lubos.

To me, the holography is inessential, but the thermodynamics maps directly to geometry, suggests Poincaré's "Science and Hypothesis", chapter IV, published in English in 1905, which then, I think, can go beyond high school mathematics. So yes, not so new. On the other hand, string theorists might find Poincaré's conventionalism a little indigestible.

18. **Peter Woit**
February 10, 2010

Peter Morgan,

Personally I find comparing Verlinde's "entropic force" paper to Einstein's GR papers just absurd.

In this case, I think Lubos's evaluation of this sort of research is accurate, and most of the theoretical physics community likely feels the same way. Maybe the ideas Verlinde (and you in your comment) are promoting represent where theoretical physics is headed, but if so I don't think that's a good thing.

19. **Thomas Larsson**
February 11, 2010

There are only two kinds of physicists today – those with bad ideas, and those without ideas. Crackpots vs. mediocracy.

Lubos proves that these sets have a non-vanishing intersection.

20. **P**
February 11, 2010

Standard model is very successful but IMHO it has already reached it's limits and all attempts to extend it further will fail. I also doubt LHC will find any new physics though it also likely won't find Higgs or SUSY neither.

It is however not true that there are no experimental observations which need explaining, there are plenty of deep foundational questions which are still unanswered, like: what aspect of physical reality is captured by wavefunction? what is described by fine structure constant? why SM parameters have the values they have? and many more…

I believe such questions have to finally be answered if we are to see further progress. They cannot however be answered in the framework of SM (or the answers would have been found by now), they require a novel approach based on different foundations. And this approach will likely look like crackpotism to those accustomed to SM at first.

This is why I believe unconventional thinking is good and the ideas like ones proposed by Verlinde should be encouraged even if they turn out to be wrong. Wrong ideas can still be beneficial if they inspire others or at least inform them that such and such approach does not work for following reasons.

This doesn't mean however that anything is worth exploring, speculations which cannot be tested even in principle like multiverse are still worthless and should be discouraged.

Finding successor to SM certainly won't be easy, it may very well be the most difficult task ever faced by physics what is really unfortunate is that those who chose to work on it face not only technical difficulties but also sociological barriers. Many in physics not only actively avoid foundational questions they also associate such work with crackpots and tend to shun those who try to work on them. This combined with publish or perish attitude makes such work very problematic for young researchers who are most likely to find solutions missed by their predecessors.

All this may significantly delay progress and could also mean that eventual breakthrough will have to come from outside mainstream physics.

21. **ObsessiveMathsFreak**
February 11, 2010

> In a very deep sense though, I just don't understand why talented physicists react to this by engaging in things like anthropic string landscape research, or vague arguments about "entropic forces".

Well, what's their alternative? They could spend another 20 years of their life battering their heads against the rock of modern particle physics with very few results and recognition to show for it; or they could produce and almost as substantial document about "entropic forces" in a hundredth of the time nd for ten times the press. It's a no-brainer from a game theory perspective at least.

Of course there's option 3, which is to research in some less grandiose, but no less worthy, branch of physics, e.g. battery research, and produce results potentially of great and immediate worth to mankind. However, this may be quite difficult to do after living in a dreamworld for 30+ years.

22. **Marcus**
February 11, 2010

I think this whole thread is unfair to physics. Key people in the entropic force business are Ted Jacobson and Jerzy Kowalski-Glikman. Both have solid integrity, neither show any interest in publicity, both are clearly guided by genuinely serious scientific consideration. The cynicism of some of these comments is shameful.

If you want a good quick overview of the thermodynamics of geometry, read Jerzy K-G's 5-page February 2010 note:
http://arxiv.org/abs/1002.1035
I think there is no better source if you want to know what is going on in that area. Among other things he offers specific fundamental degrees of freedom as called for in Verlinde's heuristic.
It is appalling that people badmouth Verlinde's paper merely because he does not offer specific tangible degrees of freedom to carry the temperature and entropy he talks about. His paper is a valuable and courageous opening game. Tangible details will be and already are forthcoming.

23. **Christine**
February 11, 2010

Peter,

What do you think about Padmanabhan's line of research? (See summary, e.g., here). He has been working on a thermodynamical interpretation of gravity for quite some time now. Do you consider his line of research as dubious as Verlinde's? If not, why? If yes, why?

Thanks.

24. **Peter Woit**
February 11, 2010

Christine,

My interest in arguments about quantum gravity that say nothing about particle physics or unification has always been limited, so I haven't followed closely research like Padmanabhan's. He appears to be engaged in a serious investigation of some of the puzzles raised by the relation between thermodynamics and event horizons in GR, and that's a perfectly legitimate area of research. His recent Verlinde-like claims to derive GR purely from considerations about entropy strike me as likely to be rather circular, or just dimensional analysis, but I haven't looked into this carefully. In any case, just saying "gravity comes from entropy" seems to me a rather empty and useless statement. Unless you have some idea how to use this to say something new about physics, testable at least in principle, few people are going to be interested (unless you're a well-known string theorist promoting the idea…).

25. **Daniel de Franca MTd2**
February 11, 2010

Peter,

That seemed to be a qualitative work, and Verlinde did cite Jacobson in his paper, so this is not like he is coming from nowhere. The follow up papers did seem to get the idea and produce things with much higher quality of math. So, it doesn`t seem that the overly simplified approach from Verlinde was any kind of barrier to development.

And just to reinstate what I said before. This links of gravity to thermodynamics are not a new topic, rather, they are quite common and involve several hundreds of papers. Take a look at the cite count of Bousso`s papers and you will see the ones involving gravity, holography and thermodynamics approaching 500 citations, most of them by string theory, or sugra papers.

Speaking of simplicity, I downloaded his 1st paper on general covariant entropy the day it was out on arxiv and printed because it was extremely simple, and it was one of the very few things I could understand from arxiv.org, being on the 2nd semester of my course.

I guess what is catching people`s attention it is that this paper it is that this concept is starting to be widely cited used outside string theory.

26. **Bee**
February 12, 2010

"The important thing in science is not so much to obtain new facts as to discover new ways of thinking about them." ~Sir William Bragg

27. **German guy**
February 20, 2010

If physicists were sophisticated enough to have a sophisticated vocabulary, like mathematicians, then it wouldn't be a problem, bro. For example, if "conjecture" was required usage in the physical sciences, then people could conjecture all day long, and over a period of years the good conjectures would rise to the top.

As it is, you moron physicists use a binary vocabulary where everything is science of some sort (speculative, etc.), or it's crackpottery. And we know why that is, don't we? Because physicists are supposed to be smart like mathematicians, but yet the state of modern experiment for the glory physics doesn't allow many conjectures to be fleshed out, thus no glory with a tight vocabulary, which is completely unacceptable for those seeking glory. Thus the loose definitions used.

But you're not doing a thing to fix the fundamental problem, so keep on licking those big black boots to maintain establishment acceptability, and I'll keep on doing the important things I do.

28. **Mihai Pomarlan**
February 24, 2010

irt. the Bragg quote:

"you should keep an open mind, but not so open that your brain falls out" – Richard Dawkins.

I see several commenters rushing in to defend Verlinde's paper, and for all I understand of those comments, they defend it because it's a new approach.

As if that in itself is a justification. The plethora of "new approaches" is precisely the problem these days, because most of them are junk. If this one is new, why is it worth it? Does it explain some new and puzzling obervation?

Or if it's not new, as references to similar and much older papers are present, then why the fuss?

And can anyone explain it to someone with an Engineering background like me?

(loose quote) "if you can't explain it in an undergraduate-level course, then you don't understand it" – Feynmann.

LHC Update

February 11, 2010
Categories: Experimental HEP News

A new schedule for operation of the LHC is out. It has sector tests of injection into the LHC starting the evening of Feb. 17, circulating beams again around Feb. 22, about 6 weeks for beam commissioning, then physics starting April 5. On October 18 the LHC would be stopped for two weeks to set up ion beams, which would then run for four weeks, with an end-of-year stop starting Nov. 29.

Last year there there was an estimate of about 10 days to establish collisions at 3.5 TeV/beam, but the latest estimate is more conservative, about 25 days. So, at the earliest, probably about March 4, more likely around March 19. Complicating the matter is CERN's plan to have first collisions broadcast worldwide on LHC First Physics Day, which is supposed to be a mid-week day announced a week in advance. So, the beam commissioning team is going to have to first get to the point where collisions are possible, then spend the next week of work being very careful to avoid stray collisions that one of the experiments might pick up and someone might blog about…

CMS collaboration members were asked for their best estimates of when first collisions would occur, with results plotted here. Lots of optimists voted for around March 1, the date that got the most votes was April 1.

Comments

1. **neo**
   February 11, 2010

   "…the date that got the most votes was April 1."

   Clearly some CMS members have a good sense of humor.

2. **PhilG**
   February 12, 2010

   They will be able to spend that week doing something useful at lower energy such as increasing the number of bunches

3. **Bill K**
   February 12, 2010

   "On October 18 the LHC would be stopped for two weeks to set up ion beams, which would then run for four weeks, with an end-of-year stop starting Nov. 29."

   Is it known at what energy the ion beam run will be conducted?

String Theory for Undergraduates at Brown

February 11, 2010
Categories: Uncategorized

A few years ago various US universities decided it was a good idea to offer a course on string theory for undergraduates (see here), but in recent years most of these seem to have been dropped from the curriculum. Brown University is going in the other direction, offering Physics 1970C, String Theory for Undergraduates, this semester. A report from a Brown undergraduate on Lubos's blog gives me some encouragement to continue blogging:

> Life is carrying on naturally. In fact, if I hadn't been reading eg woit's blog, I would've suspected we're still in the middle of a stringy revolution! We even just started a new string theory course for undergrads, and I and quite a few other undergrads held a string theory seminar. Interest in stuff like LQG is completely zero. So in the press you have woit, smolin blahblahblahing, ok. But in the meantime, you have ads/cft, and the whole twistor reformulation of yang mills in terms of contour integrals over grassmannians (inspired by twistor string theory). Even condensed matter physicists accept string theory as one of the greatest things that happened to physics.

The undergraduates at Brown have a String Theory Study Group, Facebook group here.

Update: The undergraduate string theory courses are now facing some competition. LSU is offering an undergraduate Introduction to Loop Quantum Gravity.

Comments

1. **Paul Wells**
   February 11, 2010

   I am not a great fan of superstring theory but I am also not sure this course is a bad idea.

   I think it is good for students to be exposed to the fact that Physics is an evolving lively subject that is often controversial.

   How about a course with basic ideas from three different approaches to quantum gravity – LQT, strings and maybe Dynamic triangulation ?

   Lets face it – the standard model is -well- pretty standard. I think there should be some courses in a Physics degree that excite and risk being wrong.

2. **Peter Woit**
   February 11, 2010

   Paul,

   The main problem with teaching undergraduates about proposals for how to quantize classical GR is that to even understand what the problem is, they first need to understand classical GR as well as how to quantize a classical field theory. The number of undergraduates who understand GR and QFT is quite small, and they would be best off in graduate courses.

   Encouraging undergraduates to do coursework on the various controversies over quantum gravity that haven't led anywhere interesting in recent years, before they have the background necessary to appreciate the crucial technical issues, seems to me to be a bad idea.

3. **Max**
   February 11, 2010

   I've been a long time reader of this blog, this is my first comment.

   I'm in this class! I'm a senior at brown. The class about 5 undergrads and 3 grad students, taught primarily by a postdoc (Ari Pakman) and by prof. Antal Jevicki when he is available.

   I'll answer anything you want about it, if you have further curiosities. I'm personally rather skeptical of string theory , but only based on what I consider to be second-hand information. I've never sat down and tried to understand the maths. I thought this class would be good to not only close that gap.

   Where did you find that quote?

4. **Peter Woit**
   February 11, 2010

   Max,

   The quote is from "gigel", commenting at Lubos's blog on his recent posting about "expanding crackpottery" that I blogged about here.

   Good luck with the course, I'm sure you'll learn something from it (although I think students would be much better off learning quite a bit of quantum field theory before tackling string theory).

   The only real textbook I know of at this level is Zwiebach's, so I'm a bit curious to hear if some other source is being used as a text for the course.

5. **Max**
   February 11, 2010

   The source is indeed the zwiebach book. I'm sure that most people in the class are going to have holes in their physics fundamentals, but it is being taught in a manner that will hopefully have a chance to address those shortcomings. I don't expect to have a robust understanding of string theory or ads/cft at the end of the semester, but I'm excited for the chance to learn first-hand what everyone is getting so worked up about; I might not get another chance.

6. **Peter Woit**
   February 11, 2010

   The problem I see with trying to teach AdS/CFT at this level is that students don't even know what the CFT is, and giving them that kind of background is not going to be possible in a course like this.

AdS/CFT is an interesting topic, but, like much of string theory, it is surrounded by vast amounts of hype. Enjoy, but don't believe everything you hear…

7. **gigel**
February 11, 2010

Hi Peter,

I think quite a few of the undergrads in the course do know both qft and gr, so this seemed like a decent opportunity to build on both.

You can't deny that string theory has been pretty useful in all sorts of places. I don't even think that the whole unification bit is the most amazing (possible) feature of string theory. You can take it as a tool to solve condensed matter problems, or just a new framework to test your undergrad knoweldge of lagrangians. I'm not sure where doubt has any room in this.

The two topics in the quote above are just a few examples where string theory did prove useful. Can you possibly deny this in any way?

8. **Peter Woit**
February 11, 2010

gigel,

On its main selling point (unification), string theory has been an abysmal failure, something which Zwiebach's book doesn't really acknowledge.

Sure, string theory is an interesting exercise in how to quantize an infinite dimensional classical system. The problem is that it is really a quite difficult exercise (due to the infinite-dimensional invariances of the system). Zwiebach covers some of this, but it seems to me that most people's time would be better spent first understanding more physically relevant QFT examples. If Brown undergrads really do understand QFT at the appropriate level, they should be ready for a graduate-level course.

The use of AdS/CFT as a tool to solve condensed matter systems is a hot research topic, but it remains very unclear how powerful it is. Your claim that "condensed matter physicists accept string theory as one of the greatest things that happened to physics" is pure hype and nonsense.

9. **petergreat**
February 11, 2010

String theory as a tool to solve condensed matter systems? As far as I know, this topic has become hot among particle theorists, but has not had any real impact on condensed matter physics. Until condensed matter physicists think AdS/CFT has solved problems that they can't solve before, and until AdS/CFT becomes an essential part of condensed matter physicists' tool set, this is just another speculative research direction.

10. **gigel**
February 11, 2010

Ok I was obviously hyping that part.

Peter, people sometimes are eager to see research or more "fun" topics before they reach 30 and complete their phd. Following the standard courses very often drives people away from physics. Finding about Noether's theorem in grad school? Finishing your SB in physics and not knowing what the weak and strong forces are (ie, about half the forces in physics)?

It happened that many undergrads here were interested in the topic, so the university offered the course. No one attempted to brainwash us or drag us into the bottomless pit of string theory research. It was student requested.

11. **Trent**
February 11, 2010

Slightly off topic, but Peter, how is your research going on BRST?

You said something like a year ago that by the summer of 2009 you will have publishable results, at least a preprent, what happened to that?

Cheers,
Trent

12. **Pawl**
February 11, 2010

It's certainly good when people try to learn about the issues. On the other hand, learning what string theory *is* (while difficult enough) is not the same as getting the right background to assess its prospects. For that one needs understanding of general relativity and quantum field theory at a deep — really, a foundational — level.

Very few physicists work at such a level. There are many really excellent physicists, but virtually all work is done on problems which are solved by technical acumen within existing frameworks, or modestly extended ones. So it is very hard to point a student to someone or some reference which would help with a critical foundational assessment of string theory (beyond the semi-popular literature).

I should point out that a main tenet of string theory has been that gravity *can* be quantized by a modest extension of existing framework (getting a quantum field theory over strings, rather than over points). (This is not "modest" within the class of quantum field theories, but it is extremely modest from the point of view of what might potentially be involved in reconciling quantum theory and gravity. That might be something entirely different from quantum field theory.)

13. **petergreat**
February 11, 2010

I second Trent's concern. I do hope Dr Woit will publish more papers. Both this blog and the related book have been excellent, and the world has already received the message. Maybe it is a good time to move on.

14. **gigel**
February 11, 2010

Pawl, you are forgetting about holography. It might turn out that the better definition of string theory and quantum gravity comes from ads/cft. There are many possibilities, not just the old "everything is made of tiny vibrating strings singing along".

Whichever way you put, ads/cft is just to remarkable to brush off. At least that's what I think. Typical unification pales in comparison.
I mean, on the one hand, what, everything is not made from point particles, but from strings. ok. On the other, you have two completely different theories living in different spaces and being equivalent. That's the real wow.

15. **Pawl**
February 11, 2010

Gigel,

As duality between quantum field theories, holography is interesting; as a statement about gravity, is on far shakier ground.

In fact, the history of the subject is something of an object-lesson in the point I was making. It was originally introduced by particle theorists who wanted to regard black holes as just another species of particle — and showed little awareness of the differences in the causal structure of black-hole space-times from others. This led to misunderstandings which persist to this day, and also made the physical basis of the theory fuzzy.

Its recent manifestations are still rather fuzzy. (There is, for instance, the over-arching question of what the entropy really is and whether one can meaningfully speak of the entropy of a general gravitational configuration. There are also serious technical problems.)

Some good might come out of it, but what I am arguing for here is the necessity of an assessment which fairly faces up to the problems. Its proponents would do well to demonstrate their own critical awareness by bringing these forward.

16. **gigel**
February 11, 2010

And besides, the theory really encourages you to look at many different topics, including in mathematics. I'm not sure why anyone would call that "wasting time". Mathematics is useful to anyone studying anything, and I mean anything, not necessarily science.

I'm pretty sure you can put all the math and techniques to good use in other places if string theory were to somehow fail (though there's no way that could happen since it's intrinsically related to YM, in at least two vastly different ways).

Pawl, I'm a very big fan of being critical and pointing fingers at the problems. But no one is hiding them, like some people would say. I mean, most papers have a dedicated section at the end on remaining open problems. In fact, that's probably everyone's favorite section of a paper. What more would you want?

Sure, you probably won't hear what the difficulties of ads/cft are in popular press, if that's what you're referring to. But people do know about them. And how do you get people to solve them? You obviously don't hold a talk and start with "so we have this thingy, but man it has so many problems! so, any volunteers to help us out?". You would probably want to say something like "ok we have this thing, we could use it for this and this and this. who wants to develop it further?" Notice the difference?

I think this is pretty much valid in all fields of science. Think of eg quantum computing, which would be the extreme example.

The "publish or perish" thing probably isn't helping much either. If people need to put out a few papers per year, they don't really have much time left to tackle the truly formidable conceptual problems. A "we rederive the relations of aaa et al using absolutely nothing new" still sounds better than "ya, i thought really hard about this for a couple of years, I even asked some friends, but didn't get anything. sorry. next time, I promise". But of course everyone knows about this. Not an easy to solve issue.

In the meantime, what's wrong with some undergrads having fun while they still can 😛 . Either they'll like it and be happy to have found something worth their time and effort, or they'll smell something fishy and cross out a potential research topic early on. It's not like we're learning something completely useless. The majority of people in het are still into strings, so it's sensible to prepare undergrads for the outside world accordingly. As in absolutely any other field.

17. **JC**
February 11, 2010

On a slight tangent, are there any universities which officially offer a regular undergraduate course on quantum field theory?

I suppose if such an undergraduate course on qft is offered, the easiest way to do it would be to do canonical quantization of the scalar fields with tree level and 1-loop calculations of phi^3 interactions in 6 dimensions.

18. **Pawl**
February 12, 2010

Gigel,

You bring up a number of points.

(1) Sure, mathematics is useful. It's not the same as foundational physics, however.

(2) You want to be "a big fan of being critical," but you also would, it seems, view it as a surprise if string theory "somehow turned out to be wrong;" you cite connections with Yang-Mills. This is mixing mathematical results and fundamental physics. The math is reasonably secure; the idea that we have reason to be less than skeptical of string theory as a fundamental physical theory is not supportable.

I really have to underline how gross this is. It is extremely hard to get physics right in (for example) an energy regime which is half an order of magnitude beyond what has been experimentally investigated; there is, for example, no consensus at all on what results will emerge from the LHC. We're usually simply not clever, or imaginative, enough to guess; nature surprises us. If (as in string theory) one aims to describe the Planck scale — well, it's good to be ambitious, but simple experience shows that it is far more likely one will get it wrong — and seriously wrong — than right. Any claims of naturality of beauty that string theory has are no defense here: beautiful theories are (to borrow a phrase of Haldane's) slain by ugly facts all the time.

I've limited myself to generalities which would apply to *any* speculative fundamental theory, but I could also list specific reasons for being especially dubious about string theory.

(3) Yes, papers mention open problems. It's the problems they *don't* mention which I'm concerned about. I don't think the authors have any conscious intent to conceal; it's rather that they're unaware of the problems, or of how serious they are.

(4) I was not referring to difficulties with ADS/CFT, but to two other things. The first were the original arguments (ca. 1993?) about holography and black holes, which depended heavily on supposing CPT invariance held but didn't take into account that the time-reverse of a black hole is a white hole. (This mistake is still being made in somewhat different contexts.) The second was the cavalier assignment of entropy to regions in space-time.

(5) It's important to be quite frank with students (whether research students or undergraduates) about the chances of success (in the sense of progress on fundamental physics), and not just to be a cheerleader.

19. **Thomas Larsson**
    February 12, 2010

    No study of string theory is complete without reading the introduction to [hep-th/0204131](hep-th/0204131), in particular subsection 1.6. And keep in mind that the author is the founder of the string theory group at Rutgers.

20. **Anonymous**
    February 12, 2010

    With reference to "some encouragement to continue blogging", I follow your blog and Jester's for the great "executive summaries" you both provide of what's going on in high energy physics.

21. **Paul Wells**
    February 12, 2010

    Peter,

    I take you point – but how are you going to attract people to people with just the same old courses on Electrodynamics, Optics and Thermodynamics ?

    After my first degree i had no exposure to GR, weak interaction etc other than what I read for myself.

    I think that there is a need to compromise academic rigor here against the need to give people some education about what the cutting edge of Physics is about.

    Otherwise people might end up going into Engineering or (gulp) even Biology…

    I get frustrated when I see Physics taught as if it were Ancient Greek rather than a lively active discipline.

22. **Paul Wells**
    February 12, 2010

    [sorry meant people to Physics of course]

23. **Peter Woit**
    February 12, 2010

    Trent, petergreat,

    What I'm working on is still the idea outlined in toy-model form in

    http://www.math.columbia.edu/~woit/brstdirac.pdf

    I haven't finished that paper since I'm still not quite clear on where it is going and I want to understand this better before finishing it. The next step is to understand the case of affine Lie algebras, and the relationship to what people have done in geometric Langlands. I've been learning a lot about this, especially about the D-module approach to representation theory, and this has given me a new perspective on this material. I'm quite excited about this, but still in the middle of it, hope to have an updated and maybe final version of the paper this spring, we'll see…

24. **Peter Woit**
    February 12, 2010

    gigel/Paul,

    I'm in no way opposed to teaching material beyond 19th century physics to undergraduates. If they graduate without being exposed to some version of Noether's theorem (i.e. that symmetries imply existence of notions of energy, momentum, angular momentum, charge… and their conservation), that's educational malpractice.

    Also not against fun. There are a lot of ways though to choose interesting and challenging material about modern physics to try and explain, without picking a 40 year old set of speculative ideas that haven't worked out and are in the process of being abandoned by the community. If you want to present something cutting-edge that is going to get ambitious undergrads a jump on hot topics in current research, string theory is a peculiar choice these days.

25. **joel rice**
    February 12, 2010

    It might be fun and useful to have a course on
    "what is screwed up with the Standard Model".

26. **gigel**
    February 12, 2010

    Pawl,

    It would be very hard to believe that all the results from ads/cft and twistor string theory are just mathematical coincidences. It is much more likely there is something going on which we still have to uncover completely. The fact that ads/cft is a complete equivalence really does say you can't kill string theory.

    But also, you can't really make the distinction between "mathematical" connection and "physical" connection as you suggest. The Standard Model is also just a mathematical connection with nature. It's a model, a translation of nature to our own language. It's good because some calculations you do with it turn out correct. String theory might be a different translation of Yang Mills. In one of its manifestations.

27. **Pawl**
    February 12, 2010

    Gigel,

    You seem determined not to distinguish between mathematical and physical successes. While physics is indeed cast in terms of mathematics, there is a huge difference.

Physics makes testable connections with the world in ways which are as precise as possible. String theory — as a fundamental physical theory — has no successes at all in this regard.

The position you have been taking is indeed one which has at times existed within the string-theory community. However, it amounts to an abandonment of the basic goals of physics, and the motivation for this radical step is nothing more than aesthetic attachment to a “theory” with no phenomenological successes!

28. **Peter Woit**
February 12, 2010

Pawl, Gigel,

Please, enough of the tired pro/anti-string argumentation, unless someone has something new to say.

29. **CWJ**
February 12, 2010

I don’t know what other advanced elective courses Rutgers offers, so it’s hard to put this in context.

As a physics professor in a small department, I occasionally get students coming to me expressing interest in string theory. Tellingly, they are rather intellectually incurious about the steps before they get to string theory. They’ve heard of GR, maybe even taken a class in it, but they haven’t really heard of quantum field theory (due to budget cuts we can’t offer such a course), and they certainly haven’t heard of gauged field theories. And they seem completely uninterested in such topics; they seem to view gauged field theories, not as the theoretical triumph that they or, not as a rich field of ongoing investigation, but as a pesky inconvenience on their way to becoming the next Einstein. There’s no real intellectual curiosity; they just want to be the next Einstein.

I can see string theory (or LQG) as being part of a more general survey course, which would include QFT, the standard model and gauged field theories, and ending with quantum theories of gravity. And if Rutgers or LSU do this but call it a “string theory/LQG for undergraduates” to draw in more students, more power to them.

30. **JC**
February 12, 2010

CWJ,

This has been going on for more than 20 years. Back when I was in grad school, it was common to find grad students (and some highly motivated undergrads) who had the mentality that quantum field theory, gauge theories, classical GR, etc … were pesky “inconveniences” in their quests of being the next Einstein. I remember a few students who didn’t even bother taking the quantum field theory courses, and went straight to studying stuff like string theory papers (various Schwarz lectures), and later the Green, Schwarz, Witten superstring theory books.

31. **Paul Wells**
February 13, 2010

Well Peter, my undergraduate degree didn’t cover Noether’s theorem. The address is University of Cambridge, Cambridge, U.K if you want to sue them for educational malpractice :-).

I think universities in general do a lousy job of keeping the interest of their students in Physics. I don’t believe final year undergraduates can’t cope with the basics of GR, Higgs mechanism etc. The details can wait for grad school.

Most undergraduates don’t become Physics professors. I think it is a disservice not to give them at least a flavor of Physics developments after 1915. Maybe things have improved since 1985 when I graduated but I doubt it.

By the way I think the (well-deserved) success of your book and others like it show there is a huge demand for this kind of course. I know Feynman tried it with QED. Maybe not strings but at least a few Feynman diagrams and an outline of the standard model.

32. **JC**
February 14, 2010

Paul Wells,

Several of my former colleagues have taught undergraduate particle physics courses using the Griffiths textbook on elementary particles. Their courses typically covered tree level calculations in the Standard Model, along with whatever background stuff that was needed to understand such calculations (ie. Higgs mechanism, SU(2) and SU(3) groups, Dirac equation, etc …). They didn’t bother going extensively into renormalization, other than the simple minded approach taken in the Griffiths book.

In the end, it was essentially a course in calculating Feynman diagrams by being given the Feynman rules, without going through much of the quantum field theory formalism. Not entirely satisfactory, but it was the least they can do without making the course too difficult.

33. **Chris Oakley**
February 14, 2010

Paul,

You may be talking about Cambridge specifically as the Physics course you did is embedded in the Natural Sciences Tripos which -even if you take all the physics options – has a lot of non-physics stuff in it, giving you less time to (e.g.) study the Higgs mechanism. In the first year of any physics course one has to do a lot of calculus, linear algebra and other mathematics, plus thermodynamics, electromagnetism and classical mechanics. I don’t see how one can avoid that. In the second year, one needs to study quantum mechanics, atomic physics, nuclear physics, statistical mechanics, solid state physics, special relativity, more electromagnetism and optics. Again, I don’t see how one can avoid that. So – maybe in the third year, bearing in mind that all the lab work will be a commitment throughout – one could get more up-to-date. At Oxford the particle physics 3rd year option (at least in 1980) got us pretty up-to-date, but it was an experimentalists course as relativistic QM and QFT were not taught. Neither was GR. I am in two minds about teaching GR to undergraduates, but certainly think that the theory of continuous groups should be taught – and preferably before even one starts on quantum mechanics. One should know what SU(2) is before studying angular momentum in QM. Noether’s theorem? I am not sure. That would have to be embedded in a QFT course – and to do that in sufficient depth would require cutting out a lot of important other stuff.

34. **Trent**
February 14, 2010

Peter, I can’t believe you said this:

”
Please, enough of the tired pro/anti-string argumentation, unless someone has something new to say.
”

What?!? Using this argument you could close your blog!
If you mean the above genuinely why don’t you fold the tent?
I’m kinda confused.

Trent

35. **anon**
February 14, 2010

> Noether’s theorem? I am not sure. That would have to be embedded in a QFT course – and to do that in sufficient depth would require cutting out a lot of important other stuff.

Noether’s theorem appears first in classical mechanics. It can be explained in 5 to 10 minutes to students who already have studied cyclic coordinates and their relation to conservation theorems. Proving it requires only a chain-rule differentiation and takes just one line.

I’ve always wondered why textbooks like Goldstein or Landau and Lifshitz do not even mention Noether’s theorem, whereas they spend a lot of space on much more abstruse topics with many fewer practical applications.

36. **JC**
February 14, 2010

anon,

re: Noether’s theorem

We were given Noether’s theorem as a homework problem in an undergraduate Lagrangian mechanics course.

37. **JC**
February 14, 2010

Chris Oakley,

Maybe I should have went to Cambridge for undergrad. 😉 Back when I was an undergrad, it felt frustrating not doing any modern physics stuff earlier in the curriculum.

For example, we didn’t do any quantum mechanics until the tail end of junior year (ie. third year). That junior year quantum course basically covered modern physics topics (ie. Planck, de Broglie, Bohr, etc …) and some simple solutions to the Schroedinger equation (ie. free particle, square well, Heisenberg inequality, etc …) at the end of the course.

For most of the first three years of physics undergrad, it was largely classical physics done in successive courses with more mathematics added in each year. So by the time we took that first quantum course in junior year, we already had taken: 2 courses in classical mechanics (ie. with and without Lagrangian mechanics), 1 course on electromagnetic theory (ie. solving Laplace’s equation), 1 course on complex analysis (ie. resides, Cauchy’s theorem, etc …), 1 course on partial differential equations, 1 course on physical optics (ie. solving Maxwell’s equations, etc …), and 1 course on classical thermodynamics + heat transfer (without any statistical mechanics).

I suppose learning all that mathematics and classical physics stuff in extensive detail before ever doing quantum mechanics, does take some of the “deus ex machina” out of quantum mechanics. (For example, the analogy between Poisson brackets and the commutators of operators in quantum mechanics). But unfortunately it didn’t leave much time for the more modern courses like particle physics, nuclear physics, solid state, etc … By the time we were able to take the particle and/or nuclear physics courses in senior year (4th year), it was the last semester before we graduated. We all had a bad case “senioritis” by then. 😉

I remember several former classmates I knew in my freshman undergraduate year who decided to transfer to another university, because our university didn’t do any “cool” modern physics stuff in sophomore or junior year (2nd and 3rd years). They were very impatient for the most part, and wanted to do quantum mechanics early on. Years later I found out a few of these same former classmates who transferred out, were also into string theory years later. They were the same ones who thought that it was a brilliant idea to skip the quantum field theory and general relativity courses, and go straight to reading string theory papers.

I ended up taking the particle physics course (along with the nuclear course) in the last semester of senior year. It was largely an “experimentalist” course, where quantum field theory and relativistic quantum mechanics were not covered at all. At the time it seemed kind of disappointing.

38. **changcho**
February 16, 2010

“If they graduate without being exposed to some version of Noether’s theorem (i.e. that symmetries imply existence of notions of energy, momentum, angular momentum, charge… and their conservation), that’s educational malpractice.”

Well, I didn’t see that as an undegrad; had to wait to become a grad student to see Noether’s theorem.

39. **Amos**
February 16, 2010

Peter, on the BRST point: You said here, and I’ve seen a few other references that seem to track your language, something along the lines of, that BRST “comes close” to explaining why there are fermions. Could you explain what you mean by that? I’ve been trying to understand it, but its clearly far deeper into the math than I’ve been able to penetrate.

40. **Tom**
February 17, 2010

@JC says: “On a slight tangent, are there any universities which officially offer a regular undergraduate course on quantum field theory?”

IIRC, Caltech offers Ph205 (?) which I’m told is basically a QFT course.

Undergrad physics majors who stayed the course — ie, didn’t change majors halfway through their years — and kept pace on their math courses, would be taking Ph205 in 4th year.

Ph205 was nominally a graduate course, but beginning graduate courses were also populated with 3rd/4th year undergrads.

41. **JC**
February 17, 2010

Tom,

When I was in grad school, it was not unusual for highly motivated undergrads to take graduate level courses. When I took quantum field theory in grad school, there were several undergrads enrolled.

With that being said, what I was asking in my previous question is whether there was an undergraduate course on quantum field theory which was not cross-listed with the graduate qft course.

Now that I thought about it more, I suppose such an "undergrad qft" course could in principle cover quantum electrodynamics done using canonical quantization. What I had in mind was Sakurai's "Advanced Quantum Mechanics" book.

42. **DaveB**
February 18, 2010

When I was at UMIST in the mid 1990s we touched on qft in the 2nd year, and there was an optional particle physics course in the 3rd year.

43. **Me**
February 18, 2010

Now that I thought about it more, I suppose such an "undergrad qft" course could in principle cover quantum electrodynamics done using canonical quantization. What I had in mind was Sakurai's "Advanced Quantum Mechanics" book.

Thats what I had in my 4th year of undergrad. Renormalization was done using dimensional regularization. Almost no loops but we had some SM stuff.

44. **JC**
February 18, 2010

Me,

We never got around to renormalization in senior year undergrad quantum mechanics. The furthest we got was Dirac's equation and various solutions (ie. plane waves and hydrogen atom).

45. **Peter Woit**
February 18, 2010

Amos,

On a very vague level, one can speculate that, since we use fermionic fields (valued in the Lie algebra of the gauge group) to deal handling gauge symmetry (a la BRST, or Fadeev-Popov), physical fermionic fields might also have an interpretation as a means for dealing with some symmetry. The natural symmetry to consider is diffeomorphism symmetry, or local translations. These fields though are spinor valued, which makes them rather different. I've played with various ideas for trying to do this, have some new ones I'm working on in the context of the BRST=Dirac cohomology framework. The fact that I still don't completely understand this is one thing keeping me from finishing the BRST-Dirac paper.

46. **Me**
February 18, 2010

JC,

My degree was a 5 year degree late previous century so this wasn't even in my senior year. (though for some people it was).

We barely did renormalization but I sure remember dimensional regularization and people being baffled about integrating in d+e dimensions where d is an integer an e->0.

We did basic QED and some SM. Mainly calculating tree-level approximations to weak-force related events.

It wasn't QFT as we later studied since things were motivated from Dirac's equation and then upwards and not starting from a Lagrangian and QFT and deriving the rules. Things were very heuristic.

I didn't like it very much but we certainly did some long calculations…

47. **Dave Nott**
February 21, 2010

Does anybody have web references for the twistor reformulation of Yang-Mills in terms of contour integrals over Grassmannians?

48. **Peter Woit**
February 21, 2010

Dave,

Look at

http://arxiv.org/abs/0912.0539 + earlier papers by same authors

and

http://arxiv.org/abs/0912.4912 + earlier papers by same authors

Completely Off-Topic

February 12, 2010
Categories: Uncategorized

I'm heading off to New Orleans tomorrow morning, will be there for Mardi Gras, back next Wednesday. Light to no blogging for the duration.

Since I'm already off-topic, I can't resist promoting my friend Alexei Karamazov (aka Mark Ettinger)'s show, which opened last night here in New York at the Minetta Lane theater. The Flying Karamazov Brothers travel the world with a wonderful show that could best be described as demented vaudeville featuring some of the best juggling around. They're here in New York City for the first time in many years, through March 7. For one of the first reviews, see here. Some of their previous shows featured references to string theory, this one doesn't. Go to their web-site to learn more, then go buy tickets and help me recoup some of the money I put up as an investor to help make this happen…

No Comments

The Little Book of String Theory

February 21, 2010
Categories: Book Reviews

Back from New Orleans, and there are now three books I've read recently that I'll try and write reviews of. The first is The Little Book of String Theory, by Princeton's Steve Gubser. The author has a web-site for the book here, and the introduction is available.

While trying to cover a huge amount of complicated material, the book is quite short, with 162 pages of text, in a small format. Gubser has chosen to deal in a radical manner with the problem of deciding whose work to reference, and whose name to mention in connection with various discoveries. There are no footnotes or end-notes, no bibliography of any kind, and no mention of the names of any string theorists, or any living physicists at all for that matter. The history of the subject pretty much only appears in a few of Gubser's comments about early parts of his own career.

To somehow counterbalance its main focus on a highly sketchy treatment of an intricate and very abstract subject, the book periodically introduces some very concrete and explicit numerical computations, starting with a first chapter devoted to explaining in detail the equation $E=mc^2$. Unfortunately, none of these calculations have anything at all to do with the topic of the book, string theory. The central section of the book, about branes and duality, contains no such concrete calculations, but instead largely consists of page-long paragraphs recounting in words the intricate structures that occur in this subject. I find it hard to believe that anyone not already familiar with this topic will get much out of this kind of discussion.

Gubser intensively uses analogy to try and convey some understanding of the material, and has a fondness for analogies based on his mountain-climbing experience. Here's an example, based on a climb to the Aiguille du Midi:

> The ridge we climbed is famously narrow, heavily trafficked, and snow-covered. For some reason everyone seems to climb it roped up. I've never quite approved of the practice of climbing roped when no one is tied to a solid anchor. If one person falls, it's hard for the others to avoid being pulled off their feet. Usually I think it's better to trust yourself and climb unroped, or else anchor and belay. But I'll admit that I climbed the ridge roped up to my climbing partner like everyone else. My partner was a very solid climber, and the ridge isn't really that tough.
>
> In retrospect, I think that roped teams climbing a narrow ridge provide a good analogy to the Higgs boson, which is one of the things LHC experimentalists hope to discover.

The point here is that the top of the ridge is supposed to be like the unstable maximum at zero of the Higgs potential, but it seems to me that few are likely to get much real understanding out of this kind of analogy. Similarly, the chapter on GR and black holes opens with a chilling story about a fall while climbing near Aspen, but it's hard to see how it adds much to the reader's understanding of the subtleties of the modern understanding of gravitation.

The book is advertised as "a non-technical account of string theory and its applications to collider physics." The last chapter is about recent attempts to use AdS/CFT as an approximate calculational technique in heavy-ion physics. This is Gubser's specialty, and he does a good job of giving a hype-free explanation of the state of the subject, for instance:

> The second reason why it is tricky to compare a prediction of the gauge/string duality with data is that the string theory computations apply to a theory that is only *similar* to QCD, not to QCD itself. The theorist has to make some translation between one and the other before he or she has a definite prediction to give an experimentalist. In other words, there's some fudge. The best attempts to handle this translation honestly lead to predictions for the charm quark's stopping distance that are either in approximate agreement with data, or perhaps as much as a factor of 2 smaller. A similar comparison can be made for viscosity, and the upshot is that the gauge/string duality produces a result that is either in approximate agreement with data, or perhaps a factor of 2 away from agreement.

While giving a reasonable account of the heavy-ion collision story, the description of the relation of string theory to the much more interesting question of what happens in proton-proton collisions at the Tevatron or LHC energy frontier is actively misleading hype. What he is really describing is supersymmetry, and while he begins with the arguable:

> Supersymmetry predicts many other particles, and if they are discovered, it would be clear evidence that string theory is on the right track.

he then goes on to claim that:

> What is exciting is that string theorists are placing their bets, along with theorists of other stripes, and holding their breaths for experimental discoveries that may vindicate or shatter their hopes…
>
> If it is found, many of us would take it as confirmation of superstring theory

There's no discussion of the issue of the supersymmetry breaking scale, or acknowledgement of the fact that string theory does not at all require this scale to be low enough for superpartners to be observable at LHC energies. The fact of the matter is that string theory makes no predictions at all about what the LHC will see, and Gubser's claim that string theorists have some sort of LHC prediction they are betting on is just not true. There is no bet here that string theorists can possibly lose: if superpartners are found, they are likely to trumpet this as "confirmation of string theory", but if not, they'll fall back on the accurate statement that string theory predicted nothing about this.

Throughout the book, Gubser is on the defensive about the issue of string theory's lack of predictivity, invoking highly strained and dubious analogies as excuses. One chapter begins with a discussion of Roman history and its effects on our present-day culture. He then argues that our many centuries remove from this history is somehow like the way string theory makes predictions at high energies, not low energies. I don't see the analogy (we have lots of evidence for Romans, none for strings), and in any case the problem with string theory is not that it can't predict what happens at low energies, but that it can't predict anything at any energy. In another chapter he compares current string theory unification models to the BCS theory of superconductivity, noting that the BCS theory doesn't work for high-temperature superconductivity. I'm not sure what to make of this analogy, since BCS is a successful theory, string unification models aren't. The only point of it seems to be the hope that something new will be discovered experimentally (analog of high temperature superconductivity), and some unknown version of string theory will describe it.

Like pretty much all of his colleagues at Princeton, one thing Gubser wants nothing to do with is the multiverse and the anthropic string theory landscape. While he explains the moduli-stabilization problem, the landscape and the multiverse are not discussed, and anthropic argumentation is dismissed with:

> Altogether, I find myself unconvinced that this line of argument is useful in string theory.

In the next posting, I'll write about another new popular physics book, one that I think is much better and much more readable, although it takes the West Coast multiverse interpretation of string theory as gospel, ignoring the views of Gubser and his Princeton colleagues.

Comments

1. **DaveB**
   February 22, 2010

   The idea of travelling on a ridge roped up with no anchors is simple. If one person falls off the ridge then the other is supposed to somehow find the willpower to jump off the other side, leaving both mountaineers hopefully alive but dangling a few metres below the ridge line. It isn't very good for ropes or nerves.

   I'm not sure if that has any relation to particle physics, but modern climbing ropes can hold strong forces, and are thin enough to be stringy…

2. **Trent**
   February 22, 2010

   Peter, I wrote this in a previous thread but maybe you missed it, I'm genuinely interested:

   Peter, I can't believe you said this:

   "
   Please, enough of the tired pro/anti-string argumentation, unless someone has something new to say.
   "

   What?!? Using this argument you could close your blog!
   If you mean the above genuinely why don't you fold the tent?
   I'm kinda confused.

   Trent

3. **Peter Woit**
   February 22, 2010

   Trent,

   Much of the blog is not about the string theory controversy, and I'd be quite happy if the fraction of postings on this topic went to zero. Unfortunately, there continues to be a very active campaign by string theorists to mislead people about the subject, and I think there's a continuing need for some sort of response to this. If someone else wants to take on this job, or if string theorists decide to stop the hype, I'll stop writing about this and will stick to other subjects (including the more positive aspects of the string theory story).

   In the meantime, I'll just keep doing what I'm doing, which is not to continually bring up this tired subject for no reason, but to respond to string theory hype as it appears.

4. **Tim van Beek**
   February 22, 2010

   What is the intended audience of this book? The author writes on his homepage that his book is supposed to be a non-technical account for the layperson, but in the introduction we find statements like

   "An example of an anomaly is that if there were particles similar to neutrinos, but electrically charged, then certain types of gravitational fields could spontaneously create electric charge."

   Or

   "When heavy ions collide head-on, it is even more chaotic than a proton-proton collision. it's believed that protons and neutrons melt into their constituent quarks and gluons."

   Aren't people who don't have a degree in physics scared away by this introduction?

5. **Peter Woit**
   February 22, 2010

   Tim,

   The level of knowledge expected of the reader is very uneven throughout the book, with an explanation of scientific notation one place, explanations that require a sophisticated knowledge of physics to understand in others.

   Note that I don't necessarily think this is a bad thing. I certainly did some of this in my book, and see nothing wrong with different parts of a book being accessible to different audiences, with some parts of the audience only able to follow parts of the book. In general, I think the best philosophy is to make explanations as simple as possible, but no simpler. For example, explanations that use analogies that are so over-simplified that they don't capture much at all of what the true story is seem to me a bad idea (the Higgs mechanism analogy I quoted in the post is an example).

6. **milkshake**
   February 22, 2010

   The problem with using such analogies in popular writing is that the people who are specialists in a highly technical field but are trying to explain it to the general public have hard time gauging the usefulness of their analogies and the clarity of their expose (because it is difficult for them to correctly approximate the thinking of a person within the intended audience). Then there is a normal problem with vanity, or the author gets carried away in a chapter that deals with his favorite subject, etc.

   Years ago when I was reading the "Elegant Universe" – a book that was generally agreed to be a good piece of popular writing – I remember that I was at times put off by what I felt was too verbose and even somewhat condescending explanation of simple stuff like photoelectric effect or curled dimension – but when the analogies and more detail would have been actually helpful in the esoteric later parts dealing with branes, holographic principle, dualities and M-theory, the expose became suddenly very sparse and reverted to "here is what was found" or " the current thinking is that".

   A specialist writing a popular science book – especially when he is new to this business – should at least distribute the manuscript copies to his friends and colleagues from other departments (including humanities) and ask them to participate in anonymous poll that would, for example, correlate the level of readers technical training level and professional background with their assessment of what they got out of the book, so that one could maybe re-adjust the detail level in particular chapters or rethink the used analogies. Of course working with a competent editor also helps.

7. **Chris W.**

February 23, 2010

I’m afraid that at some point the reason for these shortcomings becomes: “It’s just not that easy, I don’t have time for this, the book is getting too long, we have to stick to such-and-such a publication date, …”

It doesn’t help when the majority of one’s colleagues take a dim view of taking time away from professional activities to write books for a general audience.

A serious effort to write accessible material on these topics should be collaborative, so no one person has to take on too much of the job. Ideally, that would be happening in a medium like Wikipedia. Unfortunately, there is reality to contend with.

That said, perhaps the best reason to keep writing such books is that it sends the message that people in the field care enough to try, and thereby inspires young people (one hopes) to keep going into the field, or at least care to support it as taxpayers.

The Edge of Physics

February 22, 2010
Categories: Book Reviews

The second book about physics or math that I finished reading recently is Anil Ananthaswamy's The Edge of Physics: A Journey to Unlock the Secrets of the Universe. The author has a blog devoted to the topic of the book, as well as a web-site, which includes some wonderful photos of the experiments discussed.

The bulk of the book is devoted to the author's description of his travels to visit experimental projects around the world devoted to learning more about cosmology, particle physics, dark matter and dark energy. These include CDMS at the Soudan Mine in Minnesota, the Lake Baikal neutrino telescope in Siberia, the Very Large Telescope at Cerro Parranal in Chile, Mauna Kea in Hawaii, the Square Kilometer Array in South Africa, BESS at McMurdo Bay and IceCube at the South Pole in Antarctica, as well as the LHC and Planck satellite. Ananthaswamy is a quite good writer, and does an excellent job of describing the settings of the experiments and what they are trying to measure, as well as the scientists who are working on them.

Unfortunately though, he doesn't stick to the impressive experimental story going on, but wraps everything in a heavy dose of string theory/multiverse hype. None of the experiments he visited actually are capable of saying anything about string theory or the question of whether or not there is a multiverse. Most of them are investigating subjects like dark matter, which are of great potential interest, but have nothing at all to do with string theory or the multiverse. The only one for which there have been claims of such relevance are cosmological measurements of the spatial curvature of the universe, with Susskind claiming a prediction of the sign (but not the magnitude, experimentally it seems to be zero). This "prediction" actually doesn't work, see this paper.

It's too bad that among the string theorists Ananthaswamy interviewed, none included the many prominent ones such as Gross or Witten, or pretty much anyone at Princeton (see for example the book by Gubser reviewed in the previous posting) who could have explained to him the actual situation. So, if you're interested in what's going on at the experimental frontiers of this subject, this is a good book to read, as long as you skip all the parts about theory…

Update: Ananthaswamy describes here how decided to deal with the problem of writing about experiments, yet wanting to address string theory.

Comments

1. **neo**
   February 23, 2010

   Thanks for the reviews. I am always in the market for science books, so the info is useful. Sounds like Ananthaswamy's book is good for its descriptions of these mega-research projects, and I ordered it for that reason. I am used to skipping over string-multiverse hype–you have to do it every day reading the science press.

2. **a quantum diaries surivivor**
   February 23, 2010

   Hi Peter,

   If I recall correctly, Anil interviewed me for two hours over the phone three years ago… It was the beginning of the "new scientist – economist" higgs cry-wolf crisis. I tried to explain things to him but later saw them misreported. I do not know whether it was because he wanted to give more emphasis to the excess CDF was seeing or what.
   Of course, this says nothing of Anil's ability as a journalist. Just for the record…

   Cheers,
   T.

The Quants

February 25, 2010
Categories: Book Reviews

The third book I recently read that has some math or physics content is Wall Street Journal reporter Scott Patterson's The Quants: How a New Breed of Math Whizzes Conquered Wall Street and Nearly Destroyed It. It's a very lively and entertaining telling of a story which features quite a few mathematicians who have gone on to make (and then sometimes lose) absurd amounts of money using mathematical models to try and exploit market inefficiencies. Jim Simons and his large group of mathematicians and other Ph.D.s at Renaissance play a significant role, and among other mathematicians who make an appearance is Neil Chriss, co-author of Representation Theory and Complex Geometry, one of the most well-known books on geometric representation theory (now available as a "Birkhauser Classic").

Patterson's story emphasizes heavily the relationship to gambling. He writes extensively about Ed Thorp, who developed the theory of card-counting, did well with this at casinos, then moved on to the hedge fund business. Just about everyone profiled in Patterson's book is described as having read and been inspired by Thorp's 1962 book on card-counting (*Beat the Dealer*). Many of them are serious poker players, and the book opens by describing the scene at the one of the recent Wall Street Poker Night Tournaments. These are yearly events (Chriss and Simons are among the organizers) that bring together quants and professional poker players to play high-stakes poker, with proceeds donated to Math for America.

The subtitle of the book puts the blame for the financial crisis on this kind of activity, but there's not much evidence given to justify this. Most of the book is about various hedge funds, and the stories of failure are pretty much the same old story of Long Term Capital Management's failure back in 1998. Finding some sort of market inefficiency and exploiting it tends to work for a while, but sooner or later either others start doing the same thing or patterns change, sometimes very quickly. If one has gotten greedy and started using too high levels of leverage, one can get in trouble fast. The best-run hedge funds (for instance, Renaissance) managed to stay out of trouble, others didn't. How much of a public problem all this is remains unclear. To a large extent the failures just lead to some rich people (and universities like Harvard) becoming less rich, while some hedge-fund owners and employees see their income go down but get to keep the fees earned while they were taking too much risk. It's very clear why a lot of mathematicians and physicists go into this.

None of this though seems to have had a determining part in the disastrous financial crisis of recent years and its ongoing effects. The book has little to say about a more significant failure that involved a different group of quants, those responsible for the bad mathematical models used to justify the mortage securitization business. From what I can tell, there the story is that if there's a lot of money to be made creating a financial instrument carrying large risks obscured by complexity, it's not hard to find people willing to help you sell it by creating bad mathematical models of its behavior.

The story of *The Quants* is a remarkable one, whether or not the people described have some responsibility for the current state of the financial industry and the dangers still embedded in it. While reading the book I couldn't help thinking that it would be a good idea if the best of them would play a little less poker and take on another pro bono task, that of coming up with a good understanding of the current pathologies of the financial system, and models useful in the task of figuring out how to change it to something more socially desirable.

Comments

1. **milkshake**
   February 25, 2010

   …when you have good understanding of the current pathologies of the financial system, the natural thing is to try to make money from it for your fund.

   Do you expect people from Renaissance to go public with their models – or to complain to the regulators that certain trading schemes shouldn't be allowed?

2. **chris**
   February 26, 2010

   very well said. this pretty much sums up the defect of our current system.

3. **Frank Quednau**
   February 26, 2010

   @milkshake
   What do you mean by "the natural thing" ? Altruism is also a "natural thing". "Socially desirable" would be to expose those pathologies to work towards a system that does *not* only work for a few and leaves the major part of roughly 7bn. people in its wake.

4. **AJ Scruffles**
   February 26, 2010

   I'm waiting for the paperback to come out in England – but what i'm interested in is the implication/accusation that quantitative analysts were responsible for the crash.

   I recall a recent television program on the crisis that used NASA's imperial/metric mistake as a framing analogy before cheekily adding that some of the people who had designed these faulty financial products were themselves rocket scientists. You wonder if there are moves to scapegoat the quants or whether it's just an interesting angle.

   Also, you allude to the social value of this kind of work. In the UK the most common destination for science graduates (in applicable fields) is banking. The discrepancy between employment opportunities and starting salaries in the financial sector to any other is (or was) mind boggling. I've never really seen this issue raised in the mainstream media. Perhaps it's less apparent in the U.S. but it's surely indicative of a wider social problem.

5. **Anonymous**
   February 26, 2010

   @above: Indeed I've seen many people graduating in math, material science, neuroscience, biology and so on getting into banking. I'm simply amazed at how banking in the UK is such a large industry that absorbs a considerable proportion of all science graduates.

6. **mathphys**
   February 27, 2010

   "Representation Theory and Complex Geometry" is based on lectures that V Ginzburg gave as a visitor at the U of Chicago before he eventually became a professor there. N Chriss was a graduate student who took lecture notes that formed the basis of the book.

7. **Chris Oakley**
February 27, 2010

LTCM blew up because they started getting into products (e.g. merger "arbitrage" and gilt swap spreads) that they did *not* have a mathematical model for worth the name. In the end it turned out that the LTCM math PhDs – despite having larger genitalia (financially speaking, of course) – were *less* good at the seat-of-the-pants trading done by the other market players.

I agree with Peter that the quants who did the *real* damage were not these (relative) small-time gamblers, but those whose models gave AAA ratings to horse-excrement sub-prime mortgage bonds. These were sold in the trillions to pension funds, insurance companies and sovereigns like Iceland, and the fallout from their crash will be felt for decades to come.

8. **george harrison**
February 27, 2010

like string theory, is not the problem that the mathematical models are incapable of prediction, but only capable of wealth transfer?

9. **Peter Woit**
February 27, 2010

George,

I don't think the situation is at all like string theory. Mathematical models in finance make very definite (although statistical) predictions. The problem with many of the bad models wasn't that they didn't predict anything, but that they made specific predictions about levels of risk that were just wrong.

String theory isn't a very effective means of wealth transfer, typically transferring rather puny postdoc salaries to a bunch of people, more substantial senior faculty salaries to a few. On the financial industry scale though, all these numbers would be rounding errors…

10. **Haelfix**
February 28, 2010

I have a few quant friends, and the story I heard was a little different. Contrary to media reports, most of the models were *not* wrong. Including some of the highly publicized ones. Many of these were optimized for rather short timeframe corrections.

Instead they were really very accurate, which is a large part of the problem and caused a bit of a selffulfilling prophecy. The models had a number of fallbacks to prevent catastrophic loss. They also had well defined regimes of applicability where the model was supposed to be valid.

What happened was within the span of about a week, the market entered a phase where one by one, the fallbacks of the models triggered (not necessarily a big deal at first glance), and then the regimes of applicability were passed.

At that point, you had firms that had been basing a large percentage of their forecasting and risk assessment on these things, all of a sudden lose complete predictive power. This caused a panic, which triggered more rapid selling and more models to trigger selloffs (too much redundancy on systems that are too similar).

Economically, the panic occurs not necessarily on fundamentals, but b/c the computer all of a sudden returns negative infinity within a timespan that is much too quick for rational human activity and assessment. In short, what should have been a much more gradual correction, instead turns into a near world destroyer.

But I think the point is, its not the computers or the programmers fault really. The CEO's just didn't listen to the fine print, nor did they appreciate just how herdish wall street really is. Moreover the best models arguably saved their companies millions of dollars by getting the firms to remove their money or rethink their risk exposure before the real selloff's took place.

11. **Peter Woit**
February 28, 2010

Haelfix,

Thanks, although this "the model was fine, people using it just needed to read the fine print which said there was a significant probability it might blow up in their face" argument sounds a lot like someone covering their ass ex post facto.

From what I can tell, there were a wide range of models in use, some of which did what they were supposed to (Renaissance's Medallion fund doesn't seem to have done too badly). Others seem to have been just absurdly bad, including those used to create many AAA-rated mortgage backed securities.

12. **SteveM**
February 28, 2010

LTCM collapsed primarily because they were massively overleveraged and because they had way too much faith invested within a piece of solvable stochastic analysis that is emperically wrong since markets are not perfectly Gaussian. It is also ironic that Merton and Scholes were harsh critics of the Kelly propertional betting criterion, which is essentially a compromise between optimization of gains, relative to a given edge, while minimising risk. Ed Thorp on the other hand used the Kelly criterion as a guide when he played blackjack so that he would not overbet and wipe out his edge. If you have $2000 in liquidity or capital at the blackjack table for example, you can't bet more than $20 a time or else volatility or negative fluctuations can wipe you out and ruin the player edge from card counting–its true for a modest blackjack bankroll and its true for a massive hedge fund portfolio. Thorp's hands-on experience in blackjack seemed to have made him very risk adverse when he subsequently ran his successful hedge fund. Simons and his crew did'nt actually build the Medallion fund at the heart of Renaissance: this masterpiece was constructed by the brilliant Elwyn Berlekamp and Berkeley number theorist, the late James B. Ax–hence "Axcom Trading"–both of whom I guess vastly underestimated its power when they sold everything off to Simons. Interestingly, Kelly, Thorpe and Berlekamp are all very closely associated with Claude Shannon of Bell labs, the father of information theory.

13. **SteveM**
February 28, 2010

Haelfix, the quants who constructed debt instruments like mortgage-backed securities have to take some blame for the current mess but only some since they were still quite a way down the "food chain". But regardless of how good the models might have been, in reality there was simply no way to check the quality of hundreds of thousands of mortgages in these pools or whether the reality behind that triple-A mortgage rating on a six-bedroom luxury villa was that it had been sold to a Walmart's shelf packer. The people who pushed these insane subprimes also have to take a lot of blame as is everyone in the mortgage-backed securities chain. I guess sheer greed just got in the way.

A huge part of the mess is also due to the "American Dream" mentality: the average American (and European) consumer living way beyond their means on credit, fuelling a lifestyle and standard or living they had'nt actually earned but which they thought they were entitled to almost as a birthright. Asset bubbles, especially real estate ones, are always due to too much credit so also blame Alan Greenspan for adjusting interest rates and flooding the system with easy money/credit and creating the credit superbubble in the first place; of course, one reason for doing that was the fear that markets would collapse following the meltdown of the aforementioned LTCM. Also when LTCM got bailed out others realized they could also take massive risks and also get bailed out if it all went wrong–a win-win situation for them and the start of the "too big to fail" mentality. Also, Clinton's financial advisers in the late 90s, who helped dismantle the Glass-Steagall banking act of 1933 to "free things up".

The US is now in such a crushing black hole of debt it has forever past the "horizon" of ever returning to a vibrant free-market economy–all is can do now is default on its debt or inflate/hyperinflate it away, with dire consequences. And then there is Greece and the Euro and of course China…OK I won't rant on:) But this giant Ponzi scheme of moving money, printing money, credit, selling debt, buying debt, stimulus packages, massive bailouts and TARP is now the ultimate bubble and it simply can't be sustained–the worst is probably still to come.

14. **Chris Oakley**
March 1, 2010

> LTCM collapsed primarily because they were massively overleveraged

Agreed.

> and because they had way too much faith invested within a piece of solvable stochastic analysis that is emperically wrong since markets are not perfectly Gaussian

Not agreed. Let us take the case of merger arbitrage – one of the things that killed LTCM. Company A decides to buy company B, exchanging x shares of B for a share of A. That means that if the share price of company A is pA, then the share price of company B should be pA/x, right? If it is not then there should be a free lunch: if it is lower then buying B and shorting A will give you money for nothing and if it is higher then buying A and shorting B will give you money for nothing. This is merger arbitrage, and it is not rocket science. The problem is this: what if they change their minds about the ratio before the deal goes through? Mathematical models will not help you here (eavesdropping on board meetings would, but that is illegal). The other major thing that killed LTCM was gilt swap spreads. The 5-10 year interest rate for interbank loans in sterling was historically much too high relative to the 5-10 year interest rate for UK government debt (gilts) and did not reflect the relative credit quality. LTCM, along with everyone else, knew that this difference had to come down (this difference was determined by markets, and more specifically by the fact that many investors were only allowed to buy government debt, forcing gilt prices up and therefore yields down). LTCM were right – eventually the spread did reduce, but not quickly enough, and in the meantime their convergence trade helped to bankrupt them. Unless they could have known what every player in that market was doing, and going to do, they could not only guess when the convergence was likely to occur. Mathematical models, again, would not have helped.

15. **Gene Caldwell**
March 1, 2010

I think part of the problem was that the input parameters to the models representing assumptions about risk were provided to the modellers by the business and finance people.

16. **Eric Dennis**
March 1, 2010

I think it must be satisfying to some people to speculate that quant finance models — whether for algorithmic trading or mortgage securitization — have the power to cause the kind of economic upheaval experienced over the last two years. In fact, they don't have that power. They were a secondary factor in it.

The manifest proximate cause of the contraction was too much bad debt, especially mortgage debt. Models didn't generate this debt. The bad debt was assumed because the prices for it, known as interest rates, were too low. Interest rates were too low because the Fed decreed them to be so (negative, in real terms) for an extended period after the dotcom implosion (itself a previous iteration of the same scenario). It would be valid to impugn the macroeconomic models consulted by the Fed in making their decisions, but these models are really just an impotent formal rationalization for the back-of-the-envelope Keynesian/Monetarist guesstimates that actually drive monetary policy.

Securitization was simply a new mechanism by which the excess green paper printed by the Fed (i.e. created out of thin-air when the Fed bought securities in "open market operations") was goosed into the larger economy. The new mechanism made the process easier, but it didn't create all the bogus liquidity. The Fed did. It is true that models used by the ratings agencies to declare a lot of this garbage debt perfectly safe are also nonsense, premised on naive assumptions of macroeconomic stability. That certainly didn't help things. But everyone knew these models were nonsnse to begin with. That's what you get from quants (or anyone else) working for the ratings agencies, which are more creatures of government than of Wall Street due to their special status as an oligopoly mandated by federal regulation.

17. **MBS Quant**
March 3, 2010

There is a fundamental misunderstanding here. The reason the bond market blew up (and have since recovered) was mainly because of a massive "depricing" (spread widening) during the crisis across the board, way over and above fundamentals (which MBS quants concentrate on).
Structuring 101: Suppose you have a 1000 subprime mortgages, each at 10% interest and $100,000 original balance. Your model tells you that at worst 30% of these loans will eventually default. Now you create two $50 millon bonds A and D backed by these loans. You structure the bonds so that A will pay the buyer 500 of all 1000 cashflows (principal and interest) that come in every month, and bond D gets the remaining of the cashflows each month. If your model is right, as far as A is concerned defaults do not matter, you have a low risk bond, since it would take more than 50% of the loans defaulting for this bond to lose a penny. This is how you shift collateral default risk from bond A to bond D. Yes the collateral is garbage but statistically only half or less of it, and that is key. Based on this risk profile bond A gets an AAA rating and since it is paying a nice 10% interest, it trades at a premium. Bond D, on the other hand, holds all the collateral risk and it gets (say) a D rating. D trades at a very low price (high yield) and is bought by "savvy" investors who (1) want the very high yield and/or (2) are betting on their belief that your model is too pessimistic (i.e., they believe they have a better default model).
Now the housing market tanks and many of the 1000 loans start defaulting at a high pace, looking like you will indeed get 30% or maybe even 50% defaults. Note that unless defaults reach 51%, bond A will receive the same cashflows. What happened next was that bonds like D got crushed, as they should, but the market dumped all MBS bonds, and bonds like A (and all bonds for that matter) got also crushed in the process, because of supply and demand. Nobody wanted to hold any of these things. Since then, the market, specially for bonds A-like, has greatly recovered. A lot of hedge funds loaded up on A-like bonds when they got spectacularly cheap, priced way below their fundamental value.
MBS bond prices come from 4 factors: cashflow structure, modeled losses, modeled prepayments, and a fourth factor which is a "technical spread" (or how much investors are willing to (not) pay for the perceived un-modeled risk, liquidity, demand/supply of the bond). MBS "quants" concentrate on modeling the first 3, the fourth one was historically contained and this time it blew out (dragging down bonds like A) because of unprecedented market panic and hysteria (spiraling as institutions were forced to mark(price) to market their bond portfolios). Yes, default models are based on history, and it is true that modelers now understand that the fourth component can blow out and bonds may trade like stock, regardless of the fair value of the underlying cashflows, making modeled prices at that point irrelevant. Your car that gets you to work every day may not be worth a dime after all, if nobody wants to buy it. Can one say that A bonds should be rated F because they are backed by sub-prime loans and lost so much market value at the bottom of the crisis? Well, the hedge funds may certainly hope so, because they would be buying AAA cashflows being sold for dirt all day long. What programs like TARP (initially) and banks tried to do was to warehouse these bonds and hold them to maturity (realizing their cashflows) or until the market returns from trading bonds like they were GM stock…

18. **Sigge**
March 4, 2010

Here a presentation from Professor Philip Jorion (aka MR Value-at-Risk) that gives an interesting discussion about what caused the financial crisis. There is one example that may be particularly interesting where you can see that a tranche of a CDO security was giving credit rating AAA when a more appropriate assumption about the default correlation for the underlying loans would give BBB.

http://efmaefm.org/0EFMSYMPOSIUM/Nantes%202009/efm_sympo2009.shtml
Click on Keynote PPT slides to download the presentation

19. **Edward K. N.**
March 4, 2010

The LTCM predictive models did not account for the collapse of the ruble. It is not likely that exogenous variables can be incorporated into any useful model.

Nassim Taleb discusses the limitations of statistical theory in his "Black Swan."

The recent books about the crash have disclosed that senior management were clueless about the methods used by the quants and therefore did not comprehend the extent of the risks.

20. **anon**
March 5, 2010

> The LTCM predictive models did not account for the collapse of the ruble. It is not likely that exogenous variables can be incorporated into any useful model.

The influence of the ruble on the real estate market in the US is a subject that has just begun to be explored.

Now that water has been found on the Moon, maybe real estate markets will stage a spectacular rebound. Watch out for a huge surge in the price of subprime mortgages.

21. **Tim van Beek**
March 5, 2010

In case you did not notice, Scott Patterson was on the Daily Show on March 4th.

22. **Chris W.**
March 5, 2010

… coming up with a good understanding of the current pathologies of the financial system, …

The fundamental question might well be this: If the agents participating in the system use models to guide their actions, and worse yet, if they use models as a selling tool —eg, to provide bogus justifications for rating bonds as triple-A, while the underlying assumptions are being invalidated by the actual behavior of market participants— can models *ever* be expected to be reliable guides to the actual behavior of the financial system?

I seriously doubt it……

23. **Geoff**
March 8, 2010

Hi Peter –

Thanks for book review since I probably would have missed it. What I found interesting (page 115) is that Renaissance employs a number of people who originally worked on speech recognition and are clearly well versed in Shannon information. Achim Kempf was published a series of papers utilizing some of the same technology but in a completely different context – quantum gravity.

"Information-theoretic natural ultraviolet cut-off for spacetime"

http://arxiv.org/abs/0908.3061

A perimeter seminar can be found here and Susskind can be heard asking questions.

http://pirsa.org/09090005/

I'm curious whether you find the approach promising.

24. **Peter Woit**
March 8, 2010

Geoff,

Yes, that's an interesting insight into the kind of thing Renaissance is interested in.

Personally I don't find the kind of approach to quantum gravity you mention very promising, for various reasons. One is that I don't see that throwing out geometry in favor of information theory really buys you anything, but mainly I'm skeptical that "quantum gravities" that don't have anything to say about unification will ever be testable or useful for anything.

LHC Update

February 27, 2010
Categories: Experimental HEP News

Beam commissioning has started for 2010, with beam back in the LHC starting early Sunday morning. The plan is for roughly a month until colliding beams at 3.5 TeV/beam.

For the latest news, see here and here.

Update: Please, everyone, stop e-mailing me and posting comments here with the "news" (e.g. here or here) that the LHC will shutdown for a year or more in the future to fix bad splices. This is not news, it was announced by CERN back in January (see here).

Comments

1. **lhc**
   March 2, 2010

   It is curious that there is so much verbosity on "Quants" etc and so few comments on the various LHC update posts. Kudos to PW for diligently staying up-to-date and spreading the word on the LHC commissioning. You see now (I hope!) the difficulties of the reality of bringing a state-of-the-art superconducting collider to fruition. May I suggest for your further reading two articles (Part I and II actually) directly connected with the same subject, viz the problems and ultimate failure of the ISABELLE superconducting collider at BNL in the 1970's. (Today RHIC occupies the old ISABELLE tunnel.)

   Parts I and II
   http://www.springerlink.com/content/r114825754r38578/
   http://www.springerlink.com/content/824344k282615438/

   Well worth reading. You may need a journal subscription. Crease is a professor at Stony Brook and the resident historian of BNL. The consequences of the ISABELLE demise affect the US HEP community to this day. It is a lesson in leadership as well as pure physics. Don't complain if the CERN management is cautious about the LHC.

2. **DaveB**
   March 3, 2010

   The BBC will be broadcasting a programme about the last year at the LHC this evening.

   http://www.bbc.co.uk/programmes/b00qvpb9

   It will probably be available for download after being broadcast, although it may be restricted to UK IP addresses.

3. **Bill K**
   March 3, 2010

   "Don't complain if the CERN management is cautious about the LHC."

   More frequently I've heard the opinion that the previous CERN management showed a lack of caution, and that now the LHC is headed on a prudent course. To be fair, the pressure from all sides has been intense, to produce results in the form of papers, theses and press releases.

   The present plan of 1/fb at 3.5 TeV seems reasonable. But note that along with the reduced energy, this is well below the design luminosity of 10/fb/year. One should realize that finding the Higgs with only 1/fb of data is unlikely, and that any new results will primarily be more mundane measurements in b quark and t quark physics.

   In addition to the other problems, an underlying concern is the unknown effect that prolonged operation will have on the solder connections.

4. **SpearMarktheSecond**
   March 5, 2010

   I've heard the LHC collaborations will focus on J/Psi physics in their early studies. Yippie, back to Spear circa 1974!

5. **PearsAnjoutheThird**
   March 9, 2010

   It's a calibration. When LEP II was pushing to sqrt(s) in excess of open W production, still they stopped at sqrt(s) = M_Z0 first to calibrate the detectors at the start of a run.

First Hint That the Multiverse Really Exists

March 3, 2010
Categories: Multiverse Mania

Multiverse mania rolls along, with New Scientist this week running a cover story entitled Touching the Multiverse. They advertise the story by claiming that "we reveal the first hint that the multiverse really exists".

It turns out that this is a promotional effort for the work of Raphael Bousso, with the first hint that multiverse really exists a paper of his from more than 3 years ago that purports to "predict" the observed value of the cosmological constant. This in some way improves on Weinberg's 1987 anthropic argument, which to this day remains about the only piece of evidence backing up multiverse mania. The New Scientist article also reports on his more recent attempts to better justify the 3 year-old calculation with this decade's buzz-word ("entropic principle") as well as that of the last decade ("holographic").

Comments

1. **MyrtleParker**
   March 3, 2010

   Uhm, you insinuate that the guy went back and revisited his old paper to update it with new buzzwords when the original three year old paper's title is, "Predicting the Cosmological Constant from the Causal Entropic Principle?"

   You positing some form of new time travel theory here?

2. **Peter Woit**
   March 3, 2010

   MyrtleParker,

   You're right, Bousso had the "entropic principle" buzzword several years ago, before other people. It's the addition of "holographic" buzzword to the story that seems to be more recent.

3. **Bee**
   March 4, 2010

   I guess one could say the first hint that the multiverse exists is that the universe exists? 😉

4. **Dave Buettner**
   March 4, 2010

   I recently bumped into a couple of theorists I know. They were jubilantly celebrating a recent victory after a long and arduous effort. Using string theory, they were able to calculate the precise number of universes in a non-infinite version of the multiverse. Their work is yet to be published because, although they believe peer review will be a breeze, an embarrassing imaginary term appeared which they have yet to account for. However they believe they can work around it by adding another dimension. Anyway, the correct number (real part) of universes is 7,477.

5. **MyrtleParker**
   March 4, 2010

   Dave, I heard something similar although the details are a bit different. I heard that the team had used string theory to determine that the number of universes in the multiverse was in fact finite. However, they could not determine the exact number. Rather different starting assumptions lead to a landscape of finite values numbering near 10^500. Definitely finite though. So there is that.

6. **Peter Woit**
   March 4, 2010

   The notion of "counting the number of universes" in string theory is completely ill-defined. There's no reason to believe that the various constructions of "string vacua" known now are the only ones. Some of these constructions give an infinite number of vacua. The only hope of getting finiteness is to put in a cut-off at a certain scale, beyond which you expect the approximate calculation to be wrong anyway. Any numbers you get depend on the arbitrarily chosen scale.

7. **MyrtleParker**
   March 4, 2010

   Peter, I should have put tags around the post above 🙂

8. **MyrtleParker**
   March 4, 2010

   Err, should be 'sarcasm' tags.

9. **Peter Woit**
   March 4, 2010

   MyrtleParker,

   This is a subject where it's very hard to tell what's a joke and what is supposed to be serious science….

10. **MyrtleParker**
    March 4, 2010

    Indeed… that's what makes it so painfully funny.

11. **neo**
    March 4, 2010

    Infinite, schminfinite. Can we even know whether the set of possible universes is a countable set?

12. **Jeffrey McGowan**
    March 4, 2010

    Physicists know what countable means, really? 🙂

13. **Amanda Gefter**
    March 5, 2010

    Hi Peter,

    While I can't speak to the way the article was marketed, I was really writing about Bousso's paper from last year, Complementarity in the Multiverse (arXiv:0901.4806), which argues for a duality between a global picture of the multiverse and a local causal patch measure that only considers what a single observer could see. I found this really interesting, because if it holds it circumvents the problem of unobservable universes. It would allow you to do multiverse physics without reference to anything unobservable beyond your own horizon. I thought this was an intriguing new spin on the debate about whether the multiverse can be considered science.

    Anyway, I always love to hear your opinions on these matters! Thanks for mentioning the article.

    Cheers,
    Amanda

14. **Peter Woit**
    March 5, 2010

    Hi Amanda,

    Thanks for the further explanation, I should have been a lot clearer in my posting. I just don't see how this helps in terms of testability, since it doesn't change Bousso's original CC calculation or make any other testable predictions as far as I can tell. So, not much of a new hint…

    I should have also noted that you start the article describing George Ellis's skepticism about the multiverse. That there are skeptics about this, probably in a large majority, doesn't always get mentioned. Glad to hear you enjoy the blog!

15. **Ron Macken**
    March 6, 2010

    Maybe this is too basic a question, but what is leading physicists towards a multiverse model? I'm familiar with the anthropic principle, but am not sure if there are any other logical arguments for a multiverse.

16. **Peter Woit**
    March 6, 2010

    Ron,

    What is leading string theorists to multiverse models is not logic, but desperation. If your theory can't predict the value of X, you can try and explain this away by invoking a multiverse, with the value of X a random artifact of our particular universe. With string theory this gets taken to an extreme: the theory predicts nothing, but instead of giving up on it, some string theorists are trying to invoke a multiverse as an explanation for not predicting anything.

17. **neo**
    March 6, 2010

    But doesn't chaotic inflation theory also lead to a multiverse, independently of string theory?

18. **John Baez**
    March 6, 2010

    Jeffrey writes:

    Physicists know what countable means, really?

    Yes, but different physicists use different definitions, depending on how high they can count.

19. **Peter Woit**
    March 7, 2010

    neo,

    The idea of "eternal" inflation leading to a multiverse has been around for a long time, independent of string theory. It didn't get a lot of attention though, since it was (and is) untestable. By itself, it doesn't say anything about observable physics (except perhaps, could be used to justify the anthropic CC argument). There were initial hopes that the string theory version of the idea would be able to predict something (e.g. whether supersymmetry breaking scale is high or low), but that has not worked out.

20. **Zathras**
    March 8, 2010

    Jeffrey McGowan says:
    March 4, 2010 at 6:42 pm

    Physicists know what countable means, really?

    Well, since irrational numbers aren't observable, strictly speaking, a Physicist would say it doesn't matter!

21. **Michael T.**
March 8, 2010

@Ron. It seems that the main argument in support of the multiverse is not so much scientific but philosophical. That is it can neatly side step the fine tuning argument invoking theism.

22. **noel**
March 8, 2010

It still seems that the multiverse explanation is better than fine tuning. Anthropic principle always > theistic principle. So it's not like we're not making progress..
What's better, assuming that our solar system has 9 planets and that other systems might have a different number, or that our system has 9 because it's somehow unique and special? It's always good to expect that not everything we see around us has a particular reason for being the way it is. Accidents do happen.

The issue is not more philosophical here than anywhere else. For example, who predicted that the standard model will be described by gauge theories? No one. We just assume a priori that this works, and it does work, and you can make predictions from there. Similarly, who can predict which of the vacua describes our universe? No one, but if we find the right one in there, it might turn out that it works, and we could make other predictions from there.

I'm not a fan of the multiverse, I'd like all parameters to be predicted from a simple principle, but nature doesn't give a s*** about what I like and hope. Similarly, I'd like there to be a reason why gauge theories work, in the form of a very abstract and general no-go theorem. If people can't come up with something like that, I'm not very sure we should be expecting a selection principle for the vacua. It's the same problem, just different names.

23. **Peter Woit**
March 8, 2010

noel,

Personally I think the reason gauge theories work is that they embody a very deep truth about the relationship between mathematics and physics, one that we only partially understand.

But, that reason is not the reason gauge theories are a successful physical theory. They're successful because if you look at some of the simplest possible gauge theories, described by a small number of parameters, you get a consistent theory that makes an infinite number of very non-trivial predictions, and every one of these that can be tested comes out exactly right, on the nose. From a small input, you get a huge non-trivial output.

The string theory landscape is just completely different. There, the simplest examples don't look like the real world. You have to make your constructions more and more complicated in order to evade contradiction by experiment, until you have something so complicated that you can't predict anything. This is exactly the kind of thing you expect to happen when you pursue a wrong idea.

Sure, we don't know a priori whether a fundamental theory exists that is more predictive than the standard model. Based on past history, pursuing the search for such a thing seems likely to work out. But, no matter what kind of theory you want to pursue, if you're going to do science you need some way of testing your ideas against the real world. The utter failure of the string theory landscape to meet this kind of Science 101 standard is very telling. Comparing it to the situation with gauge theory is just absurd.

24. **Giotis**
March 9, 2010

Well you can't test everything. There are physical and technological limitations. It's just the way things are. People don't do Physics of the 19th or 20th century anymore. This doesn't mean of course that is forbidden to construct theories for certain regimes; you must keep making progress at all fronts and push the limits of knowledge. There are other ways to evaluate a theory i.e. Its Mathematical and theoretical consistency, its explanatory power and the lack of better alternatives.

25. **Peter Woit**
March 9, 2010

Giotis,

The problem with string theory is not that you can't test everything, it's that you can't test anything at all, because it predicts nothing. Arguing that you're going to replace experiment by mathematical consistency doesn't help here, since you don't have a mathematically consistent theory (you don't what string theory is non-perturbatively, can't even write down conjecturally what the theory is that's supposed to be producing the landscape).

All arguments like this about the string theory landscape that I've seen are just attempts to excuse what is an obvious failure. This is a really bad idea.

26. **Christine**
March 9, 2010

push the limits of knowledge

You can only push the limits of knowledge if nature validates back. Otherwise, it is not knowledge that you are gaining, but only the ability to construct good (?) hypotheses.

There are other ways to evaluate a theory i.e. Its Mathematical and theoretical consistency, its explanatory power and the lack of better alternatives.

That is a distortion of the scientific method. What you describe is a working hypothesis, not a theory. Only when the hypothesis offers predictions and/or effectively explain observed phenomena is that you have a *theory which describes that phenomena*. Such a theory evidently may only be valid at certain regimes and be eventually generalized or revised or an entirely new theory be formulated in its place. This is what pushing the limits of knowledge is about. And it happens exactly because of the scientific method.

Well you can't test everything. There are physical and technological limitations.

We all acknowledge that. So perhaps it is time to acknowledge the difference between speculation, hypothesis and theory as well.

27. **Giotis**
March 9, 2010

"The problem with string theory is not that you can't test everything, it's that you can't test anything at all, because it predicts nothing."

Peter, the problem (?) with string theory currently is not that it predicts nothing but that it predicts everything.

This “everything” though has explanatory power and that’s important. You can’t just close your eyes to that fact.

28. **Peter Woit**
March 9, 2010

Giotis,

Predicting everything and predicting nothing are the same thing.

A theory that predicts either nothing or everything has no explanatory power at all.

29. **neo**
March 11, 2010

Peter,

Predicting everything correctly is definitely better than predicting nothing. I do not follow you.

neo

30. **Bob Levine**
March 11, 2010

@neo

‘Predicting everything’ in this instance means predicting *anything*. If no what what the situation is, your theory predicts that it’s one of the possibilities—if it’s compatible will anything at all—then that theory makes no predictions, because it rules nothing out.

Top Cites 2009

March 8, 2010
Categories: Uncategorized

Travis Brooks of SLAC's SPIRES database has a blog posting today announcing the availability of various lists of the high energy physics papers most heavily cited during 2009. A full matrix of links to this data is here, data broken out by arXiv subfield is here.

It's hard to over-emphasize how much the particle theory parts of these lists are dominated by classic papers on AdS/CFT, in particular Maldacena's original 1997 paper. It now has over 6600 citations and during the next year or so should pass Weinberg's 1967 paper as the most heavily cited particle physics paper of all time. One remarkable thing about this paper is that in recent years the number of citations of it has increased to new highs, reaching 731/year in 2008. Even at the height of theoretical activity surrounding the Standard Model back during the late 1970s, none of the classic papers of that subject (such as Weinberg's) reached even half the citation rate of the Maldacena paper. Similarly, during the explosion of interest in string theory after 1984, none of the papers from the first superstring revolution reached half the Maldacena rate.

Among the top 25 entries in the 2009 overall top-cite list, the leading theory papers are 97-98 AdS/CFT classics at positions 3, 8 and 9, as well as Randall-Sundrum extra dimension papers from 1999 at 14 and 20. Among the top 50 entries, there are only two hep-th papers that are not from the last millennium: at number 33 one of the papers on superconformal Chern-Simons/supergravity duality, and Horava's Lorentz-breaking gravity proposal at number 38 (there's a very recent article about this at FQXI).

Looking just at the articles cited in hep-th during 2009, gauge-gravity duality is again completely dominant. The top 3 are AdS(5)/CFT(4) classics , the rest of the top 9 are about the lower dimensional AdS(4)/CFT(3) case (except for an AdS/CFT review article). To find something not about gauge-gravity duality, one has to go down to number 10, the KKLT paper that set off the landscape craze.

Taking a look at recent hep-th lists of postings, there seems to be no let-up in the AdS/CFT dominance. The only recent paper on another topic that seems likely to make the top ten of the 2010 listings is Erik Verlinde's January paper on entropic gravity, which two months later already has 40 citations.

Comments

1. **top50**
   March 8, 2010

   Very many of the top 50 in 2009 are astrophysics papers. (WMAP figures promimently ~ good for WMAP!) I suppose that's an indicator where the new physics is. As for Maldacena v. Weinberg, I suppose this is an indication of publication inflation? (Substitute favorite deragatory term if so desired…) There was no arXiv in the good old days, quite likely many of the citations are to arXiv postings, also there are many more conferences/symposia/whatever these days (and easily available online), so there are overall many more publications these days. Not a uniquely string theory phenomenon.

2. **Roger Schlafly**
   March 8, 2010

   I am surprised that Witten only has 2 papers in the top 50. I thought that his papers were very widely cited.

3. **Peter Woit**
   March 8, 2010

   Roger,

   Witten has written many highly influential papers, but there is no single one that stands out, and they tend to be on the mathematical end of the subject, where fewer papers are published. In these days of all AdS/CFT, all the time, very little that is not AdS/CFT is getting heavily cited in hep-th. It's not surprising that the Witten papers that show up are his AdS/CFT-related one and one other.

4. **neo**
   March 8, 2010

   It is not hard to understand why Maldacena's paper is so widely cited. It is a striking and tantalizing conjecture that attracts string theorists and particle theorists alike.

5. **fernighan**
   March 9, 2010

   It is not hard to understand why Weinberg's paper is so widely cited. It is a striking and tantalizing THEORY OF NATURE that attracts string theorists and particle theorists alike.

6. **anon.**
   March 9, 2010

   It's a bit silly to compare citations to Weinberg versus citations to Maldacena. In any field there is a body of accepted knowledge that one doesn't bother to cite. No one cites Weinberg for the Standard Model any more than they would cite Feynman, Schwinger, Tomonaga or Dyson to justify doing quantum field theory, or Einstein to explain relativity. It's fully integrated into the infrastructure of the field. This is far more important than any number of citations.

   (I imagine that at some point people will stop explicitly citing the original AdS/CFT papers and just assume that it's known, but — maybe just because they're already there in the TeX code everyone recycles, maybe because it isn't in a standard textbook yet — so far it looks like everyone will keep doing it.)

7. **Peter Woit**
   March 9, 2010

   anon,

   I was comparing the citation rates for Maldacena and Weinberg during comparable periods, about a decade after they were written, at a time when research in HEP was dominated by the topic that they started. Weinberg's 1967 paper (after getting no citations in the first few years, but that's another story…) reached a high of 351 citations/year 13 years later, in 1980. Maldacena's 1997 paper may not have hit its highest point yet, but was at 731 citations/year 11 years later, in 2008. This increase of more than a factor of two is partly explained by an increase from 1980 to 2008 in the number of papers written in HEP, but I think also reflects the historically very unusual domination of hep-th by research into one particular set of ideas.

The other comparison one can make is just the integrated number, where Weinberg is about to lose out, despite a 30 year head start. The future is hard to predict, but it seems just about certain that the Maldacena paper will be the most heavily cited HEP paper written during the 20th century, and may very well end up being the one most heavily cited during the 21st.

8. **dan**
March 9, 2010

re Maldacena – Is there any experimental evidence or empirical evidence that would suggest this level of citation is bad for HEP?

9. **Peter Woit**
March 10, 2010

dan,

In and of itself, I think the high citation rate is historically unusual, but from that you can't conclude it's "good" or "bad". Personally, I think the way the field is dominated by one very specific research topic is a reflection of an unhealthy situation. Many would argue though I think that this is the way it is just because AdS/CFT is the only good idea out there that has not been fully investigated.

10. **M**
March 10, 2010

it would be interesting to renormalize to the total number of citations in the field per year

11. **N**
March 10, 2010

That requires an index of inflation, to be able to state a baseline "the comparisons (of Maldacena and Weinberg?) are in constant 1990 citations". What of the citations of the papers of Albert Einstein (the statistics must be available somewhere) … how to compare? Pre-WW2 physics was "small science" and a comparison of citations based on inflation-adjusted numbers would still be impossible to formulate meaningfully. I do not doubt that in the decades to come things will get much worse.

12. **Peter Woit**
March 10, 2010

M and N,

It would be interesting to normalize these statistics, if anyone has good data on the yearly size of the HEP literature I'd be interested to see it. The comparison between the peak Weinberg and current Maldacena numbers is from 1980 vs 2008. There was a huge historical expansion in the size of physics research employment during the 60s, at least in the US. I'd suspect that there has been an increase from 1980 to 2008, but not as large, and concentrated outside the US and Western Europe.

More relevant may be that the Weinberg and Maldacena papers are not comparable because the Weinberg one was of much wider relevance, getting cited by phenomenologists and experimentalists as well as whatever the 1980 equivalent was of hep-th.

13. **piscator**
March 10, 2010

I think it would be interesting also to normalise by the number of citations per paper. When I read older papers I never see the long{recent work on the Buggins model includes [1-55]} reference lists that often occur in the current literature. Maybe that is because with the arxiv people are more aware of the literature, with electronic texts it is easier to cut and paste references, and there are also more people to be offended if you don't cite them.

There is also an unhealthy herd effect on citations when it comes to fashionable topics that seems particularly prevalent in hep-ph but is also present in hep-th.

As mentioned by others the comparison of absolute citation numbers between Maldacena and Weinberg doesn't mean very much, any more really than the relative price of a Hershey bar in 1998 compared to 1967.

In the period I have been in the field, which is not so long, I've noticed that the size of the topcite percentage bins in the SPIRES playground has grown by around 50%. Acording to SPIRES close to 1 paper in 5 is a topcite now. Glad to see we're all doing high impact work then 🙂

14. **M**
March 11, 2010

hi Piscator. Dividing by

N(t) = (average number of citations per paper) = (average number of references per paper)

allows to remove the effect of longer citation lists, which is mostly a consequence of informatics (easier to ask for citations, easier to say yes, easier to access papers).

But also the number of papers per author increased, in part because computers allow to write more useless papers, in part because the field expanded.

To remove both effects of informatics one should therefore divide by the average number of references per author per year.

15. **Haelfix**
March 12, 2010

Citation count (at this level) is usually not a good indicator of what a paper means to our understanding of nature. There are plenty of stringy papers that are far less cited, and ultimately more important.

Instead citation count here is a sort of combination of luck factors and sociological ones. For instance, if your paper is the final word on the subject, and is part of the way nature work, you will get a lot of citations, but not nearly as much as if you only discover a portion and leave questions unanswered.

Likewise, sometimes it turns out that a research direction is too technically challenging, and you hit a lot of brick walls. Consequently, less people will be inclined to take the plunge.

AdS/CFT hits that perfect middle ground, where its not too hard to work in and spawns fruitful research, yet is by no means perfectly well understood.

16. **SpearMarktheSecond**
March 15, 2010

Perhaps unheralded is the tendency to truly bury contributions in experimental particle physics. Talented young people first get their contributions buried under the collaboration, where first-authorship for the prime movers has been largely abandoned. Second, the papers get rolled up into the Review of Particle Physics.

So for me, the true source of who does what in experiment is the record of doctoral dissertations and internal collaboration notes, where the latter is generally not public. Perhaps those notes should become public, say, 1 year after they are written. The public pays for them.

There has been some discussion by theorists that it is hard to get young LHC researchers interested in new ideas. There are at least two ways to view this… on the one hand, the system discussed above gives little recognition to the people who do the work. On the another hand, the theory community has lost touch with real experiment in their 30 year or so journey through string theory, and suffers cognitive dissonance now when they look at how experimental collaborations make their sausages.

Short Items

March 9, 2010
Categories: Uncategorized

- Bill Thurston teams up with Issey Miyake for their fall ready-to-wear collection based on the Geometrization Conjecture (via Quomodocumque). Youtube video here.
- Why String Theorists Should Switch Fields to Quantum Computing.
- This month's AMS Notices has an interview with last year's Abel Prize winner, Mikhail Gromov. This year's Abel prize will be announced March 24, see here for the committee making the choice. Also to be announced soon is the winner of another million dollar prize, the Templeton Prize.
- Yet more string theory in popular culture.
- From UT Austin, various interesting new lectures in their GRASP series. These include a nice expository talk by David Ben-Zvi on the Fundamental Lemma. Ngo is lecturing about this weekly here at Columbia. In the fall he will move to Chicago and take up a permanent position there.
- This year's Talbot workshop will be on one of my favorite topics, Twisted K-theory and Loop Groups. At MIT there's a preparatory seminar here, and a page for the workshop here.
- This has been mentioned here before, but one can't stop marveling at the Math Overflow phenomenon.
- There's an interesting interview with Alan Sokal here.

Update: Some helpfully just pointed me to this review of a new novel by Ian McEwan in which

> characters mention M-theory, Nambu Lie 3-algebra and coincident M2-branes

Looks like something I'll have to read when it comes out here in a couple weeks.

Comments

1. **Fabio**
   March 10, 2010

   I'm not sure why quantum computists would want to encourage string theorists to join them. Quantum computer theorists, like string theorists, seem to be a dime a dozen these days. That would just make them a nickel a dozen.

2. **harsha**
   March 10, 2010

   The Grasp video server seems to be down.

3. **David Ben-Zvi**
   March 11, 2010

   Harsha – Thanks for the comment.. are you still having problems with the GRASP page? it seems to work on my computer (but depends on having Quicktime 7 I think)

   Peter – thanks for advertising the lecture! I hope it's broadly accessible – it's a fundamental piece of mathematics whose implications are widely advertised but not widely understood, but I think there are really beautiful (and simple!) new ideas in Ngo's work that should have a lot of impact.

4. **Peter Woit**
   March 11, 2010

   David,

   Video is working now for me, on a Windows machine, after installing quicktime7 (not working on a linux machine, presumably because quicktime7 is not installed there).

   Thanks for helping to make the Fundamental Lemma story more accessible. The relationship to geometric Langlands is remarkable, and I think you're right that this will lead to a lot more in the future. But the literature on the subject has been extremely difficult for non-specialists to follow.

5. **agoose**
   March 11, 2010

   There's more!
   http://abstrusegoose.com/244

   It gets worse
   http://abstrusegoose.com/238

   There seems to be quite a bit of QM/GR in this
   http://abstrusegoose.com/240
   http://abstrusegoose.com/242

6. **harsha**
   March 12, 2010

   I get a 10060 disconnected error with Quicktime 7.6, but since it works for you it is probably a network problem on my end.

7. **boreds**
   March 12, 2010

   I read a short story about the same character in the New Yorker, and liked it a lot:

   http://www.newyorker.com/fiction/features/2009/12/07/091207fi_fiction_mcewan?currentPage=1

8. **Hendrik**
   March 27, 2010

A side issue you may find interesting, is that Garrett Lisi's theory seems to be in trouble: see [here]() or at the [Arxive]().

Strings 2010

March 11, 2010
Categories: Strings 2XXX

Strings 2010, this year's version of the big annual string theory conference, will be held next week in College Station, Texas. There's a university press release about this here. Normally the conference is held in the summer at places like Rome, Madrid, Paris, Kyoto, etc. and attracts about 4-500 string theorists. This year's time and location may keep attendance down (although College Station is a lot cheaper place to stay than Rome…).

Unlike most years, there have been no promotional public lectures arranged. It also appears that there is no summary talk scheduled. In recent years, these have often been given by David Gross (who won't be talking this year) or by Robbert Dijkgraaf (who is busy with another project, video here, for which he might want to recruit help from fellow string theorist Lubos Motl). Many of the talk titles are now available. In the past, sometimes the hot topic was mathematical and mathematicians were in attendance, but this has no longer been true for a while now. This year the hot topic is condensed matter physics, with several talks scheduled on attempts to apply AdS/CFT techniques to superconductors.

It turns out I'm going to be relatively nearby, but a week later, giving a talk for the public the evening of March 24th at Collin College in Plano.

Starting up this week and continuing through May, the KITP is hosting a string phenomenology program entitled Strings at the LHC and in the Early Universe. The program blurb somehow neglects to mention that string theory doesn't actually predict anything at all about LHC physics or cosmology. To get a good idea of the topics that researchers in this field are discussing, online talks are here, starting with two rather general discussion sessions, one led by Blumenhagen, the second by Ovrut. As far as connecting to real physics goes, the state of the art seems to be much like it was a quarter century ago, with people struggling to find ways to come up with string theory-motivated constructions that are not in obvious disagreement with experiment. To achieve this requires going to ever more complicated models, which often contain various particles not in the Standard Model. In terms of making LHC predictions, one has no idea if this is a good or bad thing.

Update: The Strings 2010 talks will be web-cast. There's now a participant list. With 192 participants, this will be the smallest Strings XXXX conference in many years.

Comments

1. **Anonymous**
   March 11, 2010

   High Tc Superconductor is already a hot field for many many years, now with string theorists joining…

2. **Sam**
   March 12, 2010

   Looks like you're coming to my neck of the woods – I live in Collin County and Plano is very convenient.

   From the press release

   http://www.ccccd.edu/news/PressReleases/2009-2010/20100310distinguishedSpeaker.html

   the lecture is open to the public. I'm going to try to attend.

3. **George H.**
   March 16, 2010

   "Robbert Dijkgraaf (who is busy with another project, … for which he might want to recruit help from fellow string theorist Lubos Motl)."

   Funny!

   It would surely make Patrick Michaels happy.

4. **lun**
   March 17, 2010

   As a heavy ion physicist with an interest in AdS/CFT, the presentations I saw absolutely shocked me.

   The tone of the speakers, and I am talking about the leading lights of string theory, was essentially "we learned everything there is to learn about heavy ions, so we are not interested in it anymore, let us do condensed matter physics".

   While AdS/CFT was, and still is, a promising field, its "objective" impact to understanding the system created at RHIC is zero: I mean, there are neat calculations of highly idealized models, but there is no evidence that these models actually apply to the real world. Worse than that, there was very little effort to _look_ for this evidence. For the string theorists to now behave as if the job is finished, and they can concentrate on something else, is simply preposterous. It would be bad enough to admit failure (its premature, this approach has potential!), but they behave as if it was an unqualified success (and surely enough, PR annoucements will continue to claim this). Heavy ion phenomenologists (not to speak of the experimentalists) have every right to be extremely annoyed at this attitude.

   No idea how the condensed matter community will receive this interest. The people I know working on high-Tc superconductivity and related problems (a sample of ~5) have either never heard of AdS/CFT, or are dismissive of it. From my admittedly naive outsider perspective, the models described in the talks are largely too "artificial" to teach us anything we do not know already (in comparison the heavy ion models are actually pretty simple and natural, something I have in the past found very attractive), so it looks very much a repeat of the heavy ion performance: A few technical calculations of models that "look like" the system they claim to rapresent, little phenomenological effort, and a lot of PR.

   Honestly, if this is the best the field can offer, we are in deep trouble.

5. **Shantanu**
   March 25, 2010

   Peter and others, see this talk by Stanley Deser on overview of quantum gravity
   http://online.kitp.ucsb.edu/online/colloq/deser1/rm/flashtv.html

Around 40th minute there is some discussion of string theory.

LHC Update

March 12, 2010
Categories: Experimental HEP News

There's now a tentative date set for first high energy (3.5 TeV/beam) collisions at the LHC: it's March 30th.

Comments

1. **nessuno**
   March 12, 2010

   I don't know where you got this info. If it is from where I believe, it is followed by a (TBC) (TuBerCulosis? To Be Confirmed ?). Of course everyone is hoping that the moment comes as soon as possible; but perhaps, as history has shown, to start putting a lot of mediatic emphasis on things that cannot really be planned so precisely is not the best strategy.

2. **Peter Woit**
   March 12, 2010

   nessuno,

   That's why I described it as a "tentative date"… I agree that there's a good chance it will change.

3. **Bill K**
   March 13, 2010

   "… perhaps, as history has shown, to start putting a lot of mediatic emphasis on things that cannot really be planned so precisely is not the best strategy."

   "Plans are useless, but planning is indispensable." — Eisenhower

HEPAP Meeting

March 12, 2010
Categories: Experimental HEP News

HEPAP is now meeting in Washington, presentations available here. Like the rest of science, HEP has been doing very well in the federal budget, including a temporary increase due to the stimulus program. Excluding stimulus money, the president's FY2011 request has total DOE HEP funding up 2.3% (theory is up 3.8%) over FY2010. This is about 10% over the FY2007 level. At the NSF, the proposal is for a 2.8% increase in physics research spending in FY2011, up 20% since FY 2007.

The NSF will be funding several "Physics Frontier Centers", with five-year renewable awards of 1-5.5 million $. Pre-proposals are due in August.

The DOE has been emphasizing Early Career Programs, with 14 "Early Career Awards" to tenure-track physicists made in HEP in FY1010. Six of these went to HEP theorists, pretty much all in phenomenology, with funding for string theorists not popular these days it seems.

With the particle theory job market a complete disaster, particle theorists somehow managed to convince the DOE that the answer to the problem is to produce more particle theory Ph.Ds. There is a new program of HEP Theory Fellowships funding (with two-year fellowships) an additional five students this year, five more each year in the future. So I guess, steady-state, the idea is to add 10 more theory Ph.D.s/year, into a job market where the total number of permanent jobs/year is about 10.

Update: Science magazine has a story here about budgetary problems of the DUSEL project.

Comments

1. **Joseph Smidt**
   March 12, 2010

   Well, even without the extra DOE fellowships there would be a steady stream of particle theorists. They seem pretty set on going into particle theory, money or not.

   For instance, here at UC Irvine there is a constant scare among the particle theory grad students that if there isn't enough TA positions in the coming quarters, they may have no source of funding at all. (At least, this is what they tell me. They always TA and have been warned there might not always be enough TA slots for everyone.)

   However, even with the realization that they may have to be a grad student with no funding at all, including no TA position, at some point in their grad career, there is always a steady stream of people who sign up and say "Well, I didn't become a physicist for the money."

2. **Anonymous**
   March 12, 2010

   I'm starting a PhD in string theory. Is it possible to switch to phenomenology for postdoc, if the job market continues to favor phenomenology? I don't like this trend. What they call "phenomenology" is stuff that is as speculative as string theory, yet somehow they convinced everyone that it is down-to-earth physics. Yeah LHC is going to produce results soon, lets focus more on phenomenology! And do lots of "model building" out of nowhere to predict what will happen! I won't object to this trend if LHC has really found something very interesting that needs investigation, but such "warming up" before LHC results is not very rational, and is not fair for more formal theorists.

3. **Avidan**
   March 12, 2010

   What is the logic of supporting tenure-trackers if they have a job, while those whose research the DOE should really be supporting, are people without a tenure-track and that, despite them not doing string theory, are forced out of the system to do finance ?

4. **Peter Woit**
   March 12, 2010

   Avidan,

   One justification for creating these well-funded grants to tenure-track physicists is that a lot of the money will go to their employers (mostly universities, sometimes government labs), encouraging said employers to turn these tenure-track positions into permanent ones, and/or create more tenure-track ones.

   Put differently, if this grant program didn't exist, there would be six more tenure track theorists without a grant, in danger of not getting tenure (with one permanent particle theory job disappearing), and six institutions that would think twice before agreeing to another tenure-track line in particle theory.

5. **David B.**
   March 12, 2010

   Dear Peter,

   This complaint is very unfair to both theorists and the DOE.

   The past few years have seen a steady erosion of the support in individual grants, to the point where many theorists can not pay to support students any longer. The salaries of students and researchers has gone up, and the grant amounts have been steady or declining.

   The fellowship program is to remedy somewhat that situation. It is competitive and will be similar to some extent to the NSF graduate fellowships.

   Also, the early career award is the new name for what used to be the OJI awards. These are the Junior Investigator grants that the DOE has been awarding for many years past under a new name.

6. **Peter Woit**
   March 12, 2010

   David B,

   The idea of the OJI/Early Career grants seems like a good one to me. Anything that supports more tenure-track or permanent positions is helpful.

On the other hand, the huge imbalance between the number of particle theory Ph.Ds awarded and the number of permanent positions has always seemed to me very unhealthy for the field. If the trend is that DOE/NSF money and university budgets are tighter and able to support fewer permanent faculty in particle theory, they should also be supporting fewer graduate students.

Among the various problems that the field of particle theory suffers from, too few Ph.D. students being trained for the available permanent positions is not one of them. I understand extremely well that from the local point of view of any particular faculty member at any particular university, more Ph.D. students in their subject is a good thing. That doesn't mean it's a good thing globally.

7. **A String Theorist**
March 12, 2010

Agreeing with the first comment about the steady stream of particle theorists:

On our graduate admissions committee there has been an ever-increasing pressure to try to throttle back the number of HET students we admit, but it never works (mostly, because of people who write an ambiguous statement of interests, or who simply lie about what they want to work on.)

This year we tried a quota of three for the HET group, but now we are hearing back from admitted students and at least eight of them have listed HET as their first area of interest. Meanwhile the various (condensed matter, astro) experimentalists in our department are in serious trouble because they can't attract enough students to staff their equipment.

So, there is still huge demand for particle theory Ph.Ds. Apparently, despite the hype, young people still recognize the discovery of string theory as one of mankind's greatest intellectual achievements.

P.S. I also certainly agree with Anonymous 1:37 — I distinguish between "hard" and "soft" phenomenology, the former comprising actual data analysis and Monte Carlo coding, while the latter is not only more speculative than string theory but also significantly less useful.

8. **Paul Wells**
March 15, 2010

I think it is unhealthy that the state of Physics seems to depend so much on LHC results.

What if LHC just discovers the Higgs with a mass of 120 GeV ?

What are all those LHC experimentalists and string theorists going to do next ?

I think there should be a better balance with areas such as dark matter searches, LIGO, condensed matter, low temperature physics, neutrino physics etc.

In addition I am not sure that an experiment with thousands of PhDs provides good training for those PhDs.

9. **Peter Woit**
March 15, 2010

Paul Wells,

Unfortunately, prospects for progress in HEP and thus for it remaining a healthy scientific field, do depend strongly on LHC results. This is an unavoidable aspect of the science and of how difficult it is to do experiments at the energy frontier.

In the US, funding for LHC research is a relatively small part of the reseach budget, with a lot of attention going to the subjects you suggest.

10. **Paul Wells**
March 15, 2010

Point taken – but at some point building larger accelerators is just impractical.

In addition neutrino measurements are pretty much guaranteed to give useful data for theorists.

Maybe accurate neutrino masses /mixings would give a clue as to why there are 3 generations of leptons ?

I think this might be more profitable than building bigger and bigger accelerators that may not discover anything interesting.

11. **Peter Woit**
March 15, 2010

Because there is no viable way to compete with the LHC at the energy frontier, US HEP is already putting its emphasis on the kind of neutrino experiments you suggest (Fermilab's next generation "Project X" is being designed largely to produce an intense neutrino beam).

If there really is nothing interesting at LHC energies, and surprises in neutrino physics, maybe this will work out very well for the US…

12. **Per**
March 15, 2010

It's a nice thing that the current administration at least live up to one of its promises…

13. **Peter Woit**
March 15, 2010

Per,

Yes, but note that this recent increase in science spending is taking place in the context of large increases in all federal spending to fight the recession. As (and if…) the recession ends, there will have to be significant budget cuts in coming years to bring down the current large deficits. It's when decisions have to be made about what to cut that we'll find out how dedicated the administration (and the Congress) is to science spending…

14. **Markk**
March 17, 2010

"I think it is unhealthy that the state of Physics seems to depend so much on LHC results."

What ever gave you that idea? Most of the physicists and most of the papers and most of the work done in physics wouldn't even notice if the LHC imploded tomorrow. Well they would notice and be sad but it wouldn't directly affect their work. It is only High Energy Particle Physics and its theorists which depend on LHC. That is literally a small fraction of physics. Typical of the attitude of many science newspeople though to really focus on that area alone.

15. **chris**
    March 18, 2010

    Markk,

    actually (and naturally) it is really the HEP-experimentalists. a sizeable fraction of us HEP-theorists are doing perfectly fine without LHC.

Millennium Prize to Perelman

March 18, 2010
Categories: Uncategorized

The Clay Mathematics Institute announced today the award of the Millennium Prize to Perelman for the proof of the Poincare Conjecture. The award was made based on the rules set up when the prize was created: a Special Advisory Board (Donaldson, Gabai, Gromov, Tao and Wiles) made a recommendation to the CMI Scientific Advisory Board (Carlson, Donaldson, Margulis, Melrose, Siu and Wiles) that was accepted.

On June 8 and 9 Clay will host a conference to celebrate at the Institut Henri Poincare.

The initial press release says nothing about the question many have been wondering about for years: who will get the million dollars? Does all of it go to Perelman? Did he accept it?

Update: According to alexbellos on Twitter, Carlson says that Perelman has been informed of the award of the prize, but there has been no response yet from him about whether he will accept it.

Comments

1. **Nigel**
   March 18, 2010

   Since he declined the Fields Medal in 2006 despite two days of fruitless attempts to make him accept, maybe they'll take the easy option and mail him the check for the Millennium Prize?

2. **David Rysdam**
   March 18, 2010

   Why have many been wondering about the million dollars? Are the rules unclear or something?

3. **Dr.Kathrine Martinez-Martignoni**
   March 18, 2010

   I am really very happy for him.
   Will be Prof. LOUIS DE BRANGES the next one to receive this "premium" for his demonstration of the Riemann hypothesis?
   I hope so.
   Best,
   dr. kathrine (Switzerland).

4. **literalman**
   March 18, 2010

   There's a question about the $1,000,000 because Perelman refused the Fields previously — see
   http://en.wikipedia.org/wiki/Grigori_Perelman#The_Fields_Medal_and_Millennium_Prize

5. **Peter Woit**
   March 18, 2010

   Kathrine,

   There are very specific rules governing the Millennium prize, including a process that begins when a complete proof has been written down and refereed according to the standards of major journal or its equivalent. In this case, Clay helped fund the book written by Morgan-Tian to make sure that this was done. In the de Branges there is no such refereed paper.

6. **Roger Schlafly**
   March 18, 2010

   What took them so long? Perelman posted his proof 7 years ago, and it was published in a peer-reviewed journal 4 years ago. The Clay rules say only to wait 2 years.

7. **John Baez**
   March 18, 2010

   Yay! That's great news! He deserves it. Whether he wants to take the money or not, that's up to him…

8. **neo**
   March 18, 2010

   Recognition of his great accomplishment is all that matters. Whether or not he wants to accept it is up to him.

9. **Chris Oakley**
   March 19, 2010

   > Yay! That's great news! He deserves it. Whether he wants to take the money or not, that's up to him…

   It could be that the Clay Mathematics Institute, under pressure to cut costs, is now only allowed to award prizes to those who are not likely to accept them.

10. **Sakura-chan**
    March 19, 2010

    Hamilton should get some of that too.

11. **Ricci's Pieces**
    March 19, 2010

    Just leave him alone. Why can't people understand, he feels like accepting these prizes are beneath him. It's almost sad at this point. Everyone knows he completed Hamilton's Ricci Flow program. I'm even more stunned that Gromov was on that committee; the one person I thought understood Perelman.

12. **Peter Woit**
    March 19, 2010

    Ricci's Pieces,

    The task of the committee Gromov was on was mainly to get experts to go through the proof and make sure that it is a valid proof of the claimed theorem. Checking the proof is important and requires experts like Gromov. I see no rational reason Perelman should object to this activity, quite the opposite. He should be pleased that other mathematicians are working hard to understand his ideas and their implications.

    As for the decision to award him a $1 million prize, the thing may be beneath him or not, but once the prizes associated with problems were set up, when one of the problems is solved, that must be acknowledged. That's what Clay has now done. If Perelman wants to either ignore them or reject the award, he's free to do so.

    I'm struck actually by how low-key this announcement was. There appears to have been no effort to get this wide distribution in the media, and the $1 million is not mentioned at all in the press release.

13. **Simplicio**
    March 19, 2010

    @Ricci's: I don't see how they can not at least try and award him the prize. It was set up to go to whomever proved Poincare's conjecture, and that was Perelman. Its not like the Field's medal where its more of a judgement call.

    Will be interesting to see if he takes the money though. A million bucks is a lot to turn down, especially since he's now reportedly unemployed.

14. **capitalistimperialistpig**
    March 19, 2010

    Einstein once described Dirac as living on the knife edge between genius and insanity. Guys like Perelman and Groethendieck seem a bit over that edge. I don't think that their motivations can be interpreted in conventional terms. It would seem sad, at least to me, if Perelman, like Groethendieck, has lost interest in mathematics

15. **Ricci's Pieces**
    March 20, 2010

    When I mentioned Gromov, I meant much more in the sense of politically and not mathematically. I know a lot of Gromov's work was used in Perelman's work and naturally you would ask someone like Gromov to check the work or parts of the work. However, in Gessen's recent book on Perelman, it was very apparent that Gromov supported Perelman's stance on the Fields Medal situation. On the other hand, I guess Clay has their hands tied; they have no choice but to award it to Perelman.

16. **amateur**
    March 20, 2010

    I think there is a difference between recognition and fame. If Perelman has stated he wanted recognition, I think that is a very different thing than being famous.

    I would argue that Perelman's refusal to accept the Fields medal and apparent refusal to accept the Clay prize has nothing to do with those things being "beneath him". I think he his trying to make a statement about purity of math, and any exchange of money effectively cheapens math and makes a mathematician look like a prostitute. Although that might not be the precise way to phrase it, I can see it as a principled position and I suspect it is one that is closely aligned to Perelman's.

17. **professional**
    March 20, 2010

    > and any exchange of money effectively cheapens math and makes a mathematician look like a prostitute.

    More than a principled position it seems to me juvenile, if not downright childish. Nobody thinks of Euler as a prostitute, even though he worked for Catherine the Great, and the Clay institute is certainly not a brothel.

    I very much doubt you're accurately representing Perelman's ideas.

18. **amateur**
    March 20, 2010

    Perhaps your right, I humbly submit my apology if I caused offense.

19. **Soarer**
    March 20, 2010

    Uncertain of its validity, yet some russian media's indicating that perelman has declined the award.
    http://english.pravda.ru/society/stories/19-03-2010/112643-perelman-0

20. **professional**
    March 21, 2010

    Come on amateur, we're just discussing ideas, no need to apologize. Prof. Perelman certainly has a very uncommon, and maybe extreme, view of the mathematical community which probably very few people, aside from him, fully understands. I just don't think your rather simple interpretation of his behavior could possibly reflect his way of thinking.

21. **martibal**
    March 22, 2010

    I guess there is not so much money in Russia for science right now. Would it be so "impure" to take the money to help other young (or less young) mathematician in his country ?

22. **amateur**
March 22, 2010

I have read several articles and a few books, and they all seem to say basically the same thing. He really just wanted his work checked, he was upset that some competing mathematicians tried to claim his work as their own, and he feels isolated from the broader mathematical community. I think if my suggestion that accepting money might make him feel like a prostitute is childish, it would seem to fall in line with some of his behavior already. However, just because one might perceive such behavior as childish, I would argue that because of his apparent situation of being exceptional at math, he probably has never fully developed strong personal skills, simply because there are probably fewer than 10 people in the world he can have discussions with about the type of math he's interested in (probably less if you consider the language barrier). I think under those situations, a person would behave very childishly, particularly when you consider that a few of the people he can converse with behaved equally childishly and tried to take claim for his work.

23. **professional**
March 22, 2010

From what I've read, I think one of the things Perelman is reacting to is his perception that sometimes positions, prices, and credit are given for political reasons, whereas often talented and deserving people don't get the credit (and money…) they deserve because they are not well connected politically.

If so, his attitude of withdrawing from mathematics could not be more wrong. He's just leaving a vacuum that will be filled by others, perhaps less deserving.

I fully agree with you that he should have accepted the price and use the money to help young people who lack support to develop their careers as mathematicians. Even more, if he had accepted the Fields medal he could have used the prestige and political influence that comes with it to try and change some of the things he thinks are wrong with the community.

24. **andreask**
March 22, 2010

..as far as I understood it should be well-known that Perelman was far from being that stereotyped lack-of-personal-skills-mathematician, it seems quite clear he never visited any new-age-manager-seminars on 'development' of 'personal skills and potential', but his love for literature, including Dostojewskij and Tolstoj is one of his few personal convictions that made it through the media and seem to have been, in fact, directly initiated by Perelman himself. Possibly those are some of the few hints Perelman gave to mathematical community, knowing that the ones that he aimed at would understand and the ones he did not aim at, would refuse to understand. One could call this childish, but in Dostojewskij's perception of reality (compare 'Notes from the underground', 'The Idiot' etc.) characters similar to that one being 'performed' by Perelman were the great and unrecognized heroes of the Russian society during the 19th century.

25. **Common Sense**
March 22, 2010

> One could call this childish, but in Dostojewskij's perception of reality (compare 'Notes from the underground', 'The Idiot' etc.) characters similar to that one being

I would like to suggest to Prof. Perelman a much more effective course of action, though definitely less literary.

Accept the prize, and donate the money to the children of Haiti, or whatever needy place you like. There's no lack of starving children in the world that could use a million dollars worth of food.

Then, with a few thousands of dollars of the prize money, publish and ad in a large newspaper saying: "Dr. so-and-so, president of whatever mathematical institution, is a corrupt asshole".

Mind you, that message would reach very far and would be heard loud and clear by millions. After that, he can go back to his favorite cave with the pleasing satisfaction of a job well done.

26. **Peter Woit**
March 22, 2010

I'd be very interested to hear from anyone who actually knows something about Perelman and his thinking on the prize issue. But most of the comments here seem to be pure speculation, informative perhaps about the commenter, but not about Perelman. Enough.

27. **jester**
March 23, 2010

here is the link. it seems he refused the money (or prize).

http://www.dailymail.co.uk/news/worldnews/article-1259863/Worlds-cleverest-man-turns-1million-prize-solving-mathematics-greatest-puzzles.html

28. **amateur**
March 23, 2010

http://www.dailymail.co.uk/news/worldnews/article-1259863/Worlds-cleverest-man-turns-1million-prize-solving-mathematics-greatest-puzzles.html

29. **Joseph**
March 23, 2010

Being a mathematician myself, I doubt in his situatin I would have refused the prize, acting on moral high ground, as Common Sense suggests, or otherwise – but he gets all my respect for mocking the lusts of this world.

30. **Eugene Stefanovich**
March 23, 2010

Take a look at two video clips on http://www.lifenews.ru/news/18018

In the first one a reporter talks with Perelman on the phone and asks if he decided to take the Clay prize. Perelman's response is that he has not decided yet. If he is going to accept the prize the Clay Institute will be the first to know. The clip is dated March 23, 2010.

The second clip shows Perelman walking to a neighborhood grocery store. A (different) reporter is stalking him trying to ask questions. And Perelman is doing his best to avoid the annoying encounter.

31. **Richard**
    March 23, 2010

    Good grief, that obnoxious “news crew” behaving more like paparazzi probably does nothing but drive him further underground. He should have wasted some of the food he bought on the camera’s lens.

32. **D.**
    March 23, 2010

    The site linked directly above is a fascinating microcosm of another culture. Fully half the women commenting are proposing to bear his children. I am fairly certain that recent US Fields medalists did not inspire this response in the general public.

33. **a**
    March 24, 2010

    “Even more, if he had accepted the Fields medal he could have used the prestige and political influence that comes with it to try and change some of the things he thinks are wrong with the community.”

    I don’t think you could be more wrong about this. Perelman gets little if any prestige or political influence from the Fields medal, especially in the mathematical community, unless it’s the kind of influence that he doesn’t want in the first place. On the contrary, it’s he who would give prestige to the medal.

34. **amateur**
    March 24, 2010

    I think the media should just leave him alone.

35. **student1729**
    March 25, 2010

    If Diogenes was around today walking with his lamp in Saint Petersburg looking for an authentic, honest human being he would actually find one.

36. **Chris**
    April 4, 2010

    Come on, what’s a million dollars these days? Peanuts. It wouldn’t buy you an apartment in London (or just a tiny apartment, perhaps). And I hear that Moscow is the most expensive city in the world, which is probably where Grigori Perelman would like to buy an apartment. Big deal. They should increase the prize to a 100 million USD.

High Energy Beams at the LHC

March 19, 2010
Categories: Experimental HEP News

At 5:23 am in Geneva this morning, for the first time the two LHC beams were ramped up to high energy, the 3.5 TeV/beam that they plan to run at for the next two years. These are the highest energy (per particle) beams ever created by human beings, significantly surpassing the value at which the Tevatron operates (.98 TeV/beam) as well as the record achieved last fall (1.18 TeV/beam) during the early stages of beam commissioning.

From now on, work will continue on preparing the machine to operate at higher intensity (for now they are using low-intensity pilot beams). For the next week or two, one of the challenges will be to carefully avoid any interesting collisions between particles in the two beams, since a major media event is being organized around the first collisions, and the event is tentatively scheduled for March 30.

Update: CERN press release is here.

Update: CERN has confirmed in a press release that first collisions will be attempted on March 30.

Comments

1. **amateur**
   March 19, 2010

   Its a very exciting moment in human history, it is sad that so few understand how important it is.

2. **Dr. E**
   March 20, 2010

   Awesome!

   I heard this news while flipping through the AM dial last night!

   They were talking about it on some conservative radio talk show, instead of health care and politics.

   Science is a great uniter and exalter, and hopefully the LHC will inspire us to focus on all those greater things we have in common–the mystery of physical reality–rather than those things that divide us.

   Does anyone know the general odds that people are laying for and against the LHC finding new science?

3. **Bill K**
   March 21, 2010

   Dr. E says: Does anyone know the general odds that people are laying for and against the LHC finding new science?

   A certain online betting site quotes these odds for finding the Higgs:
   by 31 Dec 2010 – 11 percent
   by 31 Dec 2011 – 17 percent
   by 31 Dec 2012 – 22 percent
   by 31 Dec 2013 – 29 percent

   Of course these are way too optimistic. "Finding the cause of electroweak symmetry breaking by 2015" would be a good bet.

4. **SteveB**
   March 22, 2010

   It appears they got a short circuit today. "Cycling the machine/ Short-circuit detected on the main circuit in SPS – under investigation/More news asap."

   It does not say if this is serious, or a normal, every-day, minor glitch. I cannot judge.

5. **Peter Woit**
   March 22, 2010

   SteveB,

   A bit later : Short circuit in SPS has been identified and problem solved… Beam should be back in half an hour.

   With this kind of huge and incredibly complicated apparatus, finding short circuits may be a not uncommon occurrence.

6. **Barzin**
   April 3, 2010

   I heard the news too, but I don't know what they understand from that test? does it solve any problem in physics? or not yet!!

7. **Peter Woit**
   April 5, 2010

   Barzin,

   Not yet! It will take a year or so to accumulate enough data to start to be interesting, probably several years and an upgrade to full energy in 2013 to get results that will answer the questions we're most interested in, about the Higgs mechanism.

More Prizes

March 26, 2010
Categories: Uncategorized

As far as I can tell, it's still unclear if Perelman will accept the $1 million Millennium prize awarded to him last week. This week brings news of two more million dollar prizes:

- John Tate is this year's winner of the Abel Prize, worth 6 million Norwegian Kroner, which is a bit more than $1 million. Tate is now 85, recently retired from UT Austin (he spent much of his career at Harvard). He is a major figure in the development of algebraic geometry and number theory during the second half of the last century. His Princeton Ph. D. thesis, which pioneered the use of Fourier analysis on the adele group in the study of number theory, could easily be the most widely read and used doctoral thesis in mathematics.
- The 2010 Templeton Prize, worth 1 million British pounds, or about $1.5 million, was awarded to biologist Francisco Ayala. Remarkably, the Templeton Foundation describes the prize-winner as someone who has "vigorously opposed the entanglement of science and religion". I had thought that the main goal of the Templeton Foundation WAS "the entanglement of science and religion", so this is a bit surprising. Ayala has done admirable work over the years refuting creationism and intelligent design.

There was a bit of a kerfuffle over the fact that the announcement was made at the National Academy of Science (Ayala is a member), with Sean Carroll quoted as:

> Templeton has a fairly overt agenda that some scientists are comfortable with, but very many are not. In my opinion, for a prestigious scientific organization to work with them sends the wrong message.

Science magazine has an article here about the award and about what some scientists think of Templeton's activities, including the following:

> Even those who are put off by Templeton's mission agree that the foundation does not attempt to influence the outcomes of the research and discussions it sponsors. "I am not enthusiastic about the message they seem to be selling to the public—that science and religion are not incompatible; I think there is real tension between the two," says Steven Weinberg, a Nobel Prize–winning physicist at the University of Texas, Austin, who has been an outspoken critic of religion. "But for an organization with a message, they are pretty good at not being intrusive in the activities they fund. I don't wish them well, but I don't think they are particularly insidious or dangerous."

Comments

1. **Sakura-chan**
   March 26, 2010

   They should have given Tate the money when he was younger so that he could enjoy it more.

2. **Anonymous**
   March 27, 2010

   @Sakura-chan:

   First, there *was* no Abel Prize when Tate was (much) younger, since it only exists since 2003.

   Second, the Abel Prize is not the only prize around. Overall, you might say that we now have the Fields medal for young mathematicians, the Abel prize for older ones, and the Wolf prize (which Tate got as well) for people somewhere in between. So what's wrong with that?

   Third, I doubt money was in any way a motivation for Tate to do the work he got the prize for.

3. **yota**
   March 27, 2010

   >>>*the Abel prize for older ones*

   There will be competition between the Abel prize and the Chern medal : http://www.mathunion.org/general/prizes/chern/details0/

   Chern medal will be awarded during International Congress of Mathematicians like the Fields medal.

   What do you think about this new prize ?

4. **Vince**
   March 28, 2010

   I am pretty sure for $1.5 million atheistic scientists will find good reasons why Templeton is not so bad after all.

5. **tytung**
   March 28, 2010

   As long as the scientist need it and they provide him with the money, I don't think there is any problem with it. Who knows, maybe the next Einstein will turn out to be a templeton funding beneficiary?

6. **ninguem**
   March 28, 2010

   @Sakura-chan: I believe Tate has had a very happy and trouble-free life and, especially after taking up the Chair he had at Texas, no money worries. One could argue that the money could be better spent elsewhere but prizes like this add visibility to math. I don't think the press would have taken this much notice if the amount was, say, 100K. But I confess to have mixed feelings about these huge prizes.

7. **Peter Shor**
   March 28, 2010

   One million-dollar prize will get a lot more publicity then ten $100,000 prizes, and so this is a lot better for the donor. I'm not sure that it's better for the field, even though mathematics publicity is good in general.

8. **chris**
March 29, 2010

i don't really get it why Templeton money is thought of as untouchable by some. just imagine, if you had a hundred million bucks to spare, and you would really like to know what science has to say about religion. would it be so unreasonable and bad if you just set up a foundation to fund people who want to find out by doing some research?

sure, it might be all politically motivated. but you are free to turn away from it the moment this becomes obvious.

i personally find Templeton-like money to be about the 'cleanest' possible source of funding nowadays. it seems to be spare money donated by someone who really would like to know. this is what science is all about after all. now think of the other funding sources: DoE, NSF, DoD – and their equivalent counterparts in other countries. don't they have an agenda? or is it so much cleaner than Templetons?

as a grad student, i was financed by the DoE nuclear stockpile stewardship program. nowadays i get grant money by basically promising that we can beat other countries in supercomputer applications and give ours a competitive edge. maybe many of the readers here are much closer to the true spirit of fundamental science in their funding sources, i don't know. but for me, i feel that I have no right really to despise Templeton-funding.

9. **Diz**
March 30, 2010

i don't really get it why Templeton money is thought of as untouchable by some. just imagine, if you had a hundred million bucks to spare, and you would really like to know what science has to say about religion. would it be so unreasonable and bad if you just set up a foundation to fund people who want to find out by doing some research?

The concern is mostly that they do not in fact "really want to know", but rather are trying to push a foreordained conclusion, i.e. compatibility between science and Christian doctrine.

Most detractors would put very long odds on a Templeton award being presented for findings that the two are incompatible, or that modern science vindicates Hinduism or Zoroastrianism or the worship of Marduk, however compelling a case was made.

10. **Cathy O'Neil**
March 31, 2010

Go John!! Woohooooooo!!!

First High Energy Collisions at the LHC

March 29, 2010
Categories: Experimental HEP News

Current schedule is for first 7 TeV center of mass collisions tomorrow (Tuesday) at 9:17 am Geneva time. Injection of the beams will take place after 2 am, ramp up to 3.5 TeV/beam from 3-4 am. For more details of what has been going on recently at the LHC, see here. The schedule for tomorrow is here, a link to the planned webcast is here.

CMS e-commentary is here, ATLAS control room blog here.

Update: I just woke up, a couple minutes after first collisions were observed, 12:57 Geneva time. Collisions are going on now.

Comments

1. **Michael**
   March 30, 2010

   I tried to post a digest of Mike Lamont's talk on the commissioning of the LHC machine at Collider Blog.

   Last night there were non-colliding beams, and CMS and some of the other experiments were able to measure the position of the beam using the tracker – the position was within 1 sigma of the correct position.

2. **Eudox**
   March 30, 2010

   In my country the local media says that the LHC collisions is recriating the Big-Bang. Is that true or just another hype?

3. **Peter Woit**
   March 30, 2010

   Eudox,

   It's hype. The conditions created at the LHC are not those of the big bang, and the questions we hope it will answer have little to do with the big bang.

4. **Sakura-chan**
   March 31, 2010

   Electro-weak symmetry breaking sounds so unsexy to the media.

5. **Paul Wells**
   March 31, 2010

   Well, I don't think "recreating the big-bang" is a bad analogy.

   Presumably it refers to (possibly) creating a Quark Gluon Plasma in ALICE.

   Science Journalists don't have an easy job and I think it is better to give a distorted picture than no picture at all.

6. **a quantum diaries surivivor**
   March 31, 2010

   Paul, I have to concur in general. However, there are ways to convey the importance of what the LHC is doing that have nothing to do with the big bang (which, by the way, is deceiving, since we are doing particle physics and not cosmology down here).

   The LHC is "studying what breaks the symmetry of subatomic force carriers." The LHC hopes to "produce new so far unseen particles that may teach us more about the organization of the world at its tiniest distance scales". The LHC will "collide protons at an energy never achieved so far by Man". The LHC will "allow physicists to prove their conjectures on the origin of mass". The LHC might "permit us to discover a new world of subatomic bodies, symmetric to ours but so far undiscerned".

   All of the above is still hype, but slightly more informative and more sober. It took me thirty seconds, and I did a rather sloppy job. I am sure journalists could do that too.

   Cheers,
   T.

7. **Christine**
   March 31, 2010

   Unfortunately, the most part of the world population has no idea whatsoever about what each of these means:

   – "breaks the symmetry" (symmetry? Is that an illness or something? Is there a way to repair it?)

   – "subatomic" (don't know even what an atom is, much less what a "sub" means)

   – "force carriers" (who are these strong guys?)

   – "organization of the world" (the politics of our planet?)

   – "protons" (what?)

   – "mass" (ah, weight)

   – "subatomic bodies, symmetric to ours" (esoteric!)

This is not a criticism at your attempt, which is better than connecting to the big bang (something that most people recognize as — "see? The scientists are just getting close to what religion already said"). There is a lot of deep ignorance out there. The best to be done, perhaps, would be:

"The scientists are colliding one type of tiny particle against another to see what is inside them; this is the strongest collision ever attempted, and they hope to learn from it something about these and other particles that make up everything around us".

8. **SteveB**
March 31, 2010

I agree we should not be too critical of the media. And I do not think we need to denegrate the general public.

Everyone must have experienced the frustration of having to dumb down your explanations and using less than perfect analogies to try to convince even very smart people on a subject, if they are outside of your own speciality. I know I have had to use some statements that are not quite correct because I did not have the 10 minutes (or 2 pages) I needed to do it right. I think science reporters are in an especially tough role because they have to try to write to both scientists in other fields and also to the general, educated public.

I do not think the uneducated public typically chooses to read the science reporting, just as I do not choose to read Hollywood gossip. With something as news worthy as the LHC, the reporters *do* have to broaden their audience a bit. Not easy.

I think Peter did an excellent job explaining difficult material in his book and I have been trying to keep his methods for approaching complex issues in mind when I present in my own specialty.

I will also mention (with a bit of pride) that my 14 year old daughter is really enjoying the Sean Carroll Dark Matter, Dark Energy, Teaching Company Courses lecture series. She doesn't quite get how space-time can be expanding, but otherwise is following the material quite well. She also reads New Scientist every week. So, I might suggest you think of the "public" as having many smart, educated people in it, rather than dwelling on the not-so smart and not-so educated portion.

SteveB

9. **Arun**
March 31, 2010

It is better to let the reader know that they definitely don't understand something than to mislead them with wrong words and wrong analogies.

10. **Tim van Beek**
April 1, 2010

In Germany, in the news headlines, the LHC was called the "big-bang machine" (the name LHC was mostly left out), telling the audience that "scientists reacreate the big bang and that, for god's sake, the first collisions proove that the machine does not destroy the earth".
And I'm pretty sure that even this description was too boring for most people.
My favorite anecdote about science literacy is this: Someone with a Ph.D. in computer science asked me about the LHC, and as a little check I first asked back "what is the difference between velocity and acceleration?".
Answer: "There is none, if I want velocity and/or acceleration, I hit the gas pedal."
True story.

11. **a quantum diaries surivivor**
April 1, 2010

We could discuss what is the best way to communicate to the public what the LHC is and what it does for decades without reaching a consensus.

My two cents are that there are maybe 85% of people in our planet who we will never reach with our science talk; and an additional 14.5% who is reachable and receptive. We should invest on reaching that 14.5%, by telling them true facts, as hard as they may be.

So I think Arun summarized it best.

Cheers,
T.

12. **milkshake**
April 1, 2010

Do you think there is a chance of observing events that produce a massive particle from the dark sector? Because if you tell a curious kid that the majority of stuff in our galaxy probably is of some weird type that we know almost nothing about – and we hope to produce this thing and watch its disappearing act, I think the kid is going to be awed by the idea more than by some vague hype about big bang. Its almost like finding out that two thirds of your own family is actually populated by aliens.

13. **John Baez**
April 2, 2010

The LHC is "studying what breaks the symmetry of subatomic force carriers."

I'm afraid I have to agree with Christine that this would mean nothing to most people. You could say the LHC is "studying what rotates the electricity of nuclear momentum particles" and it would make just as much sense.

14. **Arun**
April 4, 2010

Dennis Overbye in the New York Times (Week in Review) has this
http://www.nytimes.com/2010/04/04/weekinreview/04overbye.html
"A Primer on the Great Proton Smashup".

Pretty good, I thought. In particular:

"Physicists suspect that the laws of physics evolved as the universe cooled from billions or trillions of degrees in the first moments of the Big Bang to superfrigid temperatures today (3 degrees Kelvin) — the way water changes from steam to liquid to ice as temperatures decline. As the universe cooled, physicists suspect, everything became more complicated. Particles and forces once indistinguishable developed their own identities, the way Spanish, French and Italian diverged from the original

Latin.

By crashing together subatomic particles — protons — physicists create little fireballs that revisit the conditions of these earlier times and see what might have gone on back then, sort of like the scientists in Jurassic Park reincarnating dinosaurs. “

Short Items

April 2, 2010
Categories: Uncategorized

Planning on getting back to writing some longer postings, but for today, here's a collection of quick news and links:

- I hear from number theorists that Princeton's Manjul Bhargava has some breakthrough results on the ranks of elliptic curves. I was out of town and missed his colloquium talk here, reports were that it was quite impressive. Here's the main result, from the talk abstract:

> There is a standard conjecture, originating in work of Goldfeld, that states that the average rank of all elliptic curves should be 1/2; however, it has not previously been known that the average rank is even finite! In this lecture, we describe recent work that shows that the average rank is finite (in fact, we show that the average rank is bounded by 1.5).

- There's an intriguing new paper out from Frenkel, Langlands and Ngo, describing some tentative new ideas about how to prove functoriality using the trace formula. I gather that this combines ideas from Ngo's proof of the fundamental lemma, ideas of Langlands about "Beyond Endoscopy", and ideas originating in the geometric Langlands program. The paper is clearly largely written by Langlands (one hint is that it's in French, be grateful it's not in Turkish…).
- Besides this new work, Ed Frenkel also has a new film coming out, entitled Rites d'Amour et de Maths. Here's the plot summary from IMDB:

> Is there a mathematical formula for love without death? The film 'Rites of Love and Math' is a sprawling allegory about Truth and Beauty, Love and Death, Mathematics and Tattoo, set on the stage of Japanese Noh theater. About the directors: Edward Frenkel is Professor of Mathematics at University of California at Berkeley and one of the leading mathematical physicists in the world. Reine Graves is a talented French filmmaker who has directed a number of original and controversial films that have won prestigious awards. Having met in Paris, Frenkel and Graves decided to create a film showing the beauty of mathematics. But how to do this without getting bogged down in technical details of the subject that could scare away non-specialists? Looking for the right metaphor, they came across the idea of making the tattoo of a mathematical formula. What better way to show the beauty of the formula than by letting it merge – literally – with beautiful female body! They found the aesthetic language for expressing this allegory in the enigmatic film 'Rites of Love and Death' (a.k.a. 'Patriotism') by the great Japanese writer Yukio Mishima, which had a very unusual and mysterious history of its own (banned for over 40 years, it came out on DVD in the Criterion Collection in 2008). The exquisite imagery of Mishima's film and the original idea of Frenkel and Graves have led to the creation of 'Rites of Love and Math.'

- Harvard finally has a female tenured math professor: Sophie Morel.
- This week's Science Magazine has an article about Sabine Hossenfelder's work (also see her blog posting here) purporting to show that you can't get linear terms in deformed Special Relativity, making deviations from standard Special Relativity unobservably small. Personally, this is the sort of thing I don't know enough about to offer an informed judgment on, but I'm curious to hear what experts think.
- Nature has an article about social scientists studying the LHC project.
- The KITP is now running a program on Strings at the LHC and in the Early Universe, which is a bit odd, since string theory predicts nothing at all about either topic. They've had promotional Blackboard Lunch talks by Cvetic and Brandenberger claiming otherwise (Brandenberger's title was "Testing String theory with Cosmological Observations"). Taking a look at them, I don't see anything at all that corresponds to a "test of string theory".

Update: One more. See here for Jester's summary of what particle theory came up with during the noughties, which has to have been the most depressing decade for the subject in a very, very long time.

Update: There are two new papers (see here and here) on the arXiv this evening that address Sabine Hossenfelder's arguments about DSR (she also has a new paper summarizing her argument, here). In one of these, Lee Smolin argues that, at least in some cases, the paradoxes pointed out by Hossenfelder can be eliminated if one studies wave-packet propagation instead of classical propagation.

Update: There's another unusual paper on the arXiv this evening, by Longo and Witten, entitled An Algebraic Construction of Boundary Quantum Field Theory. It's an algebraic QFT paper, written in a rigorous mathematical style, quite out of character with typical papers from Witten.

Comments

1. **Egads Itwerks**
   April 2, 2010

   "Harvard finally has a female tenured math professor: Sophie Morel."

   Honestly, Peter, who cares about Harvard anymore? Columbia is more important, as are most other Universities. Harvard isn't to be faulted for hiring Lubos, no, they lost cred the day they gave George W. Bush a Masters degree. No rational person can respect them now. The once proud institution gives new meaning to "yesterday's newspaper."

2. **Li Shenjang**
   April 2, 2010

   According to the new 2010 NRC rankings (yet to be released to the wide audience), Harvard is not even in top 5 anymore. So the answer to the obvious question "would it hire S. Morel if she were a he" is probably a resounding "yes".

3. **Peter Woit**
   April 2, 2010

   Li Shenjang,

   What's going on with the NRC rankings? I remember that already several years ago they were late being released…

   And, Egads, Harvard still has a great math department, whether or not it's in the NRC top 5.

4. **Bob Levine**
   April 2, 2010

   The NRC ranking are due to be released late this spring, at last report. My understanding is that the format will be somewhat different; rather than a strict order on departments, graduate programs will be grouped into tiers. It does seem a bit more sensible, doing it that way.

5. **Shantanu**
April 3, 2010

Peter, what do u think about Deser's talk at KITP? Looking forward to your comments. I think Brandenberger's talk was about String gas cosmology(an alternative to standard inflationary model)

6. **M**
April 3, 2010

The social scientists write about CERN "its two restaurants live up to their reputation for offering some of the best food of any physics canteen in the world."

This is more irrealistic than the CERN airplane invented by Dan Brown

7. **Bee**
April 3, 2010

Hi Peter,

Thanks for the link. Since I just saw that Lubos commented on my recent paper, I hope you will kindly host my reply to him since I know it's pointless to leave it at his blog.

It is of course complete bullshit that this recent paper, or any of my previous papers, is the result of communication with Lubos. I did indeed have an email exchange with Lubos about DSR at some point. I have repeatedly, and fruitlessly, tried to tell him that the model I've been working on does not have an energy-dependent speed of light. [That model dates back to 2003] as one can easily see from my publication list. Lubos has failed all the years to grasp this simple fact. His misunderstanding is documented on various of my and his blogposts. This isn't the place to explain what's behind my model, but who is interested can check out [this blopost]. Up till today I don't know if Lubos really didn't understand that or if he just pretended to so he had a reason to proclaim I'm dumb.

To address another particularly nonsensical remark in Lubos' blogpost: Neither Lee Smolin nor anybody else has ever "pressured" me to write a paper. That I started writing papers on DSR after I went to PI is obviously because I started to get more interested in the topic. I first tried to figure out what the relation is to my model, documented [here] and [here]. I then wrote two papers about what I think are the biggest problems of DSR, the so-called [soccer-ball problem] and [the formulation in position space]. The latter paper was a precursor to the more recent paper.

Even though I've thought an energy-dependent speed of light to be incompatible with observer-independence (which is why I didn't consider the option in my 2003 paper) it wasn't so easy to show after all. Sometimes I thought it might work, but then again it didn't make sense, then I tried something else, that didn't work either, then I thought I might have missed something, etc.

It adds to this that I'm not particularly happy poking holes in other people's models, I'd rather have something more promising to offer myself. The recent paper actually came about because I was trying to figure out if there is a way to distinguish deformations of Lorentz-invariance from breaking of Lorentz-invariance by taking measurements in two different frames (say the Earth and a satellite), but I ran into some problems there. I wrote up my notes on that and dumped them on the arXiv. I frankly didn't expect them to get this attention, in particular not since the problems with locality had been pointed out previously by Unruh and Schützhold (and in my earlier paper). It's just that nobody ever put the numbers in. That's what I did. Turns out the nonlocality is far larger than even I expected it to be. So it seemed worth writing up.

Best,

Sabine

8. **Chris Oakley**
April 3, 2010

Bee,

If you insist on taking Lubos seriously, Peter does have a "Censored Comments from the Reference Frame" entry which I think still accepts comments:

http://www.math.columbia.edu/~woit/wordpress/?p=412

Although it is an old post, new comments will show up in the right-hand column here.

9. **Peter Woit**
April 3, 2010

Bee,

Thanks for the further explanations of this.

Chris,

The current configuration of the blog shuts off comment threads after some predetermined length of time. I'd rather people don't respond here to Lubos's rantings, unless it's on-topic (as Bee's comment very much was).

Shantanu,

Deser seemed to me to be saying nothing new or surprising.

10. **hv**
April 3, 2010

«It adds to this that I'm not particularly happy poking holes in other people's models, I'd rather have something more promising to offer myself.» Lubos' offer is simply the plain old Lorentz invariance. His point often being, if there's nothing reasonable and/or justified to offer, then don't! And pointing holes in models should be compulsory in any theoretical science, especially if you know they're there! It makes issues in science less foggy. Is Lubos harsh in his discourse? Yes, he his! But afaik his points are correct. Mind you, Nature isn't polite either.

11. **Peter Woit**
April 3, 2010

hv,

The problem with Lubos is not just that he's impolite. He's a fanatic, and like all fanatics is devoted to misunderstanding and ignoring facts that disagree with his preconceptions. The rudeness makes it extremely unpleasant for anyone who tries to take on the task of correcting something he is wrong about, and the fanaticism ensures that he'll never acknowledge error.

Enough though about Lubos. It's depressing to put up postings mentioning a range of interesting topics, then find that all people want to discuss is Lubos the minute some reference is made to him.

12. **Sacre Bleu !!**
April 3, 2010

> what particle theory came up with during the
> noughties, which has to have been the most depressing decade
> for the subject in a very, very long time.

The idea being, I gather, that any progress in particle
physics (say, advances in jet algorithms or QCD perturbative
computations) is extremely boring, whereas any incremental
result in some obscure subfield of number theory is
fascinating, especially if it has been written in some
Quartier Latin's dialect of French.

13. **Lubos Motl**
April 3, 2010

An anonymous unfriendly person – a reader of yours, Peter – has sent me a link to your blog, Peter. I haven't heard about the blog for years but it's good to see it's still alive. But not too good. 😉

Interesting conversation. Concerning your frustration: Well, let us admit that I am by far the most interesting topic you have ever discussed on your blog, so the mindless zombies who keep on visiting your crappy website even years after it's been shown that you are just a grumpy dishonest crank are unsurprisingly excited by this topic. Get used to it. You owe everything to me.

The whole point of this collapsing website has always been to poison people and slander physics and physicists. The whole point was to claim that there was nothing interesting or valid going on in theoretical high-energy physics – which mostly means string theory. The whole point was to make people frustrated, bored, upset, and misled.

So it is pretty surprising to hear that you think that you are offering "interesting topics". You have never offered any interesting topics or ideas in your whole life, Peter. Even the most hardcore mujahideens who still keep on visiting this site know that which is why they're trying to discuss the only remotely relevant interesting topic which just happens to be myself at this point (although I would surely modestly disagree that I am the most interesting topic – but among the topics that are accessible to limited people who have been brainwashed by your stupidities for 6 years, I probably am). 😉

[arguments/threats directed at someone else deleted]

14. **Bee**
April 4, 2010

hv:

The thing is that what's "reasonable" and "justified" is not an objective criterion. Different people may have different opinions on that, and these opinions will typically differ depending on the context of their work and their interests. One of the key motivations to consider the particular type of deviations from Special Relativity that my paper comments on is that Special Relativity does not respect the invariance of any (finite, non-zero) energy scale. If you believe the Planck mass is a physically meaningful UV regulator, then it should be the same in all restframes, so the argument. You can't do that with usual Lorentz-transformations. Thus, you need a modification of Special Relativity to achieve the invariance of the Planck scale.

Now I've argued (in one of the papers I mentioned above) this is not the right way to think about the invariance of the Planck scale, but then I do not think it's an "unreasonable" motivation either to examine such deviations from Special Relativity. Best,

B.

15. **Ricci's Pieces**
April 4, 2010

It is ridiculous to say that Harvard is not important anymore. They have Jacob Lurie! I'm not a category theory person, but it cannot be denied that what Jacob Lurie is doing is ground breaking work. Yes, Columbia has some impressive people, but Harvard is still one of the best math departments in the nation.

16. **anon.**
April 4, 2010

One of the key motivations to consider the particular type of deviations from Special Relativity that my paper comments on is that Special Relativity does not respect the invariance of any (finite, non-zero) energy scale. If you believe the Planck mass is a physically meaningful UV regulator, then it should be the same in all restframes, so the argument.

On the contrary. Lambda_{QCD}, or maybe 4pi Lambda_{QCD}, is a UV scale if you're talking about pion physics. And it's not Lorentz invariant. Is this a problem? Of course not; it's only if some *invariant* quantity is larger than Lambda_{QCD} that it's sensitive to the UV completion (QCD). It's true that this is simple field theory, not deep and mysterious quantum gravity, but I don't see why the fact that quantum gravity is deep and mysterious should lead you to conclude that all the rules have to change.

17. **anon.**
April 4, 2010

To clarify, my "it's not Lorentz-invariant" means "it's not Lorentz-invariant if viewed as a cutoff *energy*." The point, of course, being that the cutoff is not in energy but in invariant kinematic quantities.

18. **anon.**
April 4, 2010

I now recall that I made the same point on your blog several years ago, Bee. I'm not sure if I'm failing to convey the point or if you have some argument I've missed.

19. **?**
April 5, 2010

Somebody must ask this: the 1st female math professor at Harvard is there because she was the best candidate, or because Harvard now has a radical feminist president and a lot of $ for "diversity efforts"?

20. **Peter Woit**
April 5, 2010

?,

Morel is an excellent mathematician, not at all out of place among the list of other Harvard senior faculty. One could also note that she started her career with admission to the ENS in Paris, which is by competitive exam. If one is going to try and make the case that affirmative action is a problem at American universities, she's not a helpful example.

21. **willman**
April 5, 2010

Anon,
Bee's conclusion, as I understand it, is that the Planck energy does not in fact need to be an invariant of Lorentz boosts (see p. 18 of her "Box-problem" article); thus, I don't see any actual disagreement between you and her on this point. Bee also claims that the Planck scale needs to be observer-independent if this scale is truly fundamental; but I'm not sure why this claim is any more controversial than the idea that taking the velocity c of light as fundamental involves taking c to be observer-independent.

22. **chris**
April 6, 2010

thanks for this very interesting link to the sociology of the LHC project. i really loved this sentence:

"Social scientists say they earn the trust of the physicists at CERN by immersing themselves in the culture, just as they would with any other population."

23. **Thomas Larsson**
April 6, 2010

The Longo-Witten paper made me raise an eyebrow, too. Unfortunately I never managed to become confortable with AQFT, so I will have a hard time reading it.

24. **Amos**
April 6, 2010

It would be helpful if someone were to put together a post discussing in detail this issue of the observer-dependence of the Planck scale, and the different sides of the question.

Solar

April 5, 2010
Categories: Book Reviews

Ian McEwan's new novel Solar is now out, with a plot featuring Michael Beard, an aging theoretical physicist. Beard won a Nobel prize early in his career for the "Beard-Einstein Conflation", which supposedly involves some unexpected coherent behavior in QED, based on the discovery of a structure in Feynman diagrams that involves E_8. An appendix of the novel reproduces the Nobel presentation speech, evidently it's the work of physicist Graeme Mitchison (see here).

As the novel opens in 2000, Beard is in his fifties, having spent quite a while coasting on his Nobel Prize. He's director of an alternative energy research center, and one of the activities there employs postdoc theorists to evaluate unconventional ideas that are sent in, largely by cranks:

> Some of these men were truly clever but were required by their extravagant ambitions to reinvent the wheel, and then, one hundred and twenty years after Nikola Tesla, the induction motor, and then read inexpertly and far too hopefully into quantum field theory to find their esoteric fuel right under their noses, in the voids of the empty air of their sheds or spare bedrooms – zero-point energy.
>
> Quantum Mechanics. What a repository, a dump, of human aspiration it was, the borderland where mathematical rigor defeated common sense, and reason and fantasy irrationally merged. Here the mystically inclined could find whatever they required and claim science as their proof.

The postdocs have little interest in the old history of Beard's discovery about QED, but instead baffle him by making "elliptical references to BLG or some overwrought arcana in M-theory or Nambu-Lie 3 algebra." McEwan seems to have gotten this from Mike Duff, who is thanked in the acknowledgments. There's the obvious problem though that Bagger-Lambert-Gustavsson, a hot topic while the book was being written, dates from 2007, so is anachronistic as a topic of discussion in 2000. Beard's reaction to what the postdocs work on is:

> Some of the physics that they took for granted was unfamiliar to him. When he looked it up at home, he was irritated by the length and complexity of the calculations. He liked to think that he was an old hand and knew his way around string theory and its major variants. But these days there were simply too many add-ons and modifications. When Beard was a twelve-year-old schoolboy, his math teacher told the class that whenever they found an exam question coming out at eleven nineteenths or thirteen twenty-sevenths, they should know that they had the wrong answer. Too messy to be true. Frowning for two hours at a stretch, so that the following morning parallel pink lines were still visible across his forehead, he read up on the latest, on Bagger, Lambert and Gustavsson – of course! BLG was not a sandwich – and their Langrangian description of coincident M2-branes. God may or may not have played dice, but surely He was nowhere near this clever, or such a show-off. The material world simply could not be so complicated.

To some extent, *Solar* is an entertaining comic novel of physicists and the alternative energy research business. The dominant theme though is a topic not all will find interesting: Beard's personal life, which involves five failed marriages over the years. At the end of the book, Beard is in his sixties, a fat, unpleasant slob, with two younger women fighting over the possibility of being number six on the list.

Comments

1. **Nigel**
   April 5, 2010

   Second sentence:

   "… based on a the discovery of …"

   You need to decide whether it is "a" or "the" discovery.

   "… a topic not all will find interesting: Beard's personal life … At the end of the book, Beard is in his sixties, a fat, unpleasant slob, with two younger women fighting over the possibility of being number six on the list."

   I suppose it would be interesting why two women would fight over such a person … if it wasn't just fiction.

2. **Peter Woit**
   April 5, 2010

   Thanks Nigel, fixed.

   The phenomenon of attractive younger women throwing themselves at much older unattractive and unpleasant men is one that seems to have been exhaustively dealt with in literature. Whether it correlates to anything statistically common in reality, I have no idea. To be fair to McEwan, in this book, one of the two women is portrayed as unattractive herself…

3. **roland**
   April 5, 2010

   I think no "real" physicist would think of a primary school exam that features non integral solutions when confronted with some new theory that he thinks is too messy or complicated or whatever to be true.

4. **Kea**
   April 6, 2010

   So all crackpots are men? Phew, that let's me off the hook.

5. **cormac**
   April 6, 2010

   Hi Peter,
   I just finished 'Solar' too! I must say I thought it was a very good book indeed, in many ways.
   First and foremost, I thought the description of the main character utterly plausible, and most impressive for a non-scientist. I also thought McEwan's antihero a much more complex character than you imply.
   There were several other themes I liked – the conversion from gw sceptic to convictee was very convincing. Also the tension between genuine believer and later

commercial interest was very well done – a theme that few scientists seeem to be aware of.
Re personal angle, well it is a novel! It's a book I look forward to reading a second time..

6. **Peter Woit**
   April 6, 2010

   Thanks Cormac,

   I'm afraid I was rather unfair to the novel in limiting my remarks about the non-theoretical physics parts of it to some easy snark. I agree to some extent with your comments and hope they go a ways to giving a better idea of the rest of the book.

7. **Andrew L.**
   April 6, 2010

   First off,big fan of your blog,Peter. A second year graduate student in pure mathematics at Queens College and The City University Of New York Graduate Center.
   Never read the novel under consideration-sadly,literature,once a great love of mine,has long since been back seated to my personal life. But to answer your question from my own limited experience, attractive younger women are attracted to fat old slobs of questionable character for one very simple motivation that takes a myriad of forms: riding the storm.They feel attaching themselves to such a man who's regarded as an eminent person will share that eminence with them and increase thier importance in society's eyes. It's why rock stars marry 20 something models who they are old enough to be grandfather to,why Anna Nicole Smith married a 90+ year old billionaire,and why half the Senators,lawyers in Cambridge and doctors on Park Avenue have second wives younger then thier daughters.It's not really just about wealth,although that's the simplest and most obvious example.It's about hooking thier car to a train everyone gathers around to watch when it pulls into the station.

8. **Jen**
   April 8, 2010

   Aw, like some people (including yourself) the non-science stuff is reviewed rather harshly.
   I found it quite an intriguing novel, also because of the science involved. I'm a senior EE major/ philosophy minor at the U of southern calif (at the moment at the ETH in Zurich, switzerland, which – by the way – is a waaaay cool university) and I love my physics classes and ever since my friends heard of that, all presents I'm getting for birthdays etc involve physics in some way or other. Solar was one of them.

   anyhew, the main reason why I'm writing a comment is that I'm one of those 20-something women involved with an older, not always pleasant to others guy, and that I MUST protest! Our relationship is not even a publicly known fact, so I'm NOT doing it to get some glory or money or whatever.
   you know, love can sometimes be even harder to describe than the laws of physics. and people sometimes fall in love completely irrationally, creating really bad matches. but what's a mere mortal to do?

9. **Jen**
   April 8, 2010

   haha, sorry for the unfinished sentence there! of course, it's supposed to say
   "like some people, including yourself, wrote …"

   sorry! tired me produces half-way sentences. brain thinks, fingers too tired to keep up.

Freaky Physics Proves Parallel Universes Exist

April 6, 2010
Categories: Multiverse Mania

Fox News has decided that some recent experimental atomic physics work showing that quantum mechanics works as expected (for a sane discussion of the science, see here) proves that parallel universes exist and that time travel may be feasible. In an article entitled Freaky Physics Proves Parallel Universes Exist, Fox News writer John Brandon develops this idea with help from Sean Carroll and Fred Allan Wolf (aka Dr. Quantum):

> The multi-verse theory says the entire universe "freezes" during observation, and we see only one reality. You see a soccer ball flying through the air, but maybe in a second universe the ball has dropped already. Or you were looking the other way. Or they don't even play soccer over there.
>
> Sean Carroll, a physicist at the California Institute of Technology and a popular author, accepts the scientific basis for the multi-verse — even if it cannot be proven.
>
> "Unless you can imagine some super-advanced alien civilization that has figured this out, we aren't affected by the possible existence of other universes," Carroll said. But he does think "someone could devise a machine that lets one universe communicate with another."
>
> It all comes down to how we understand time.
>
> Carroll suggests that we don't exactly feel time — we perceive its passing. For example, time moves fast on a rollercoaster and very slowly during a dull college lecture. It races when you're late for work . . . but the last few minutes before quitting time seem like hours.
>
> **Back to the Future**
>
> "Time seems to be a one-way street that runs from the past to the present," says Fred Alan Wolf, a.k.a. Dr. Quantum, a physicist and author. "But take into consideration theories that look at the level of quantum fields … particles that travel both forward and backward in time. If we leave out the forward-and-backwards-in-time part, we miss out on some of the physics."
>
> Wolf says that time — at least in quantum mechanics — doesn't move straight like an arrow. It zig-zags, and he thinks it may be possible to build a machine that lets you bend time.

Update: Matt Springer has a more detailed analysis of the Fox News article entitled The Worst Physics Article Ever. For some reason his critique skips over the multiverse part, implying that that's the one part of the article that makes sense…

Comments

1. **changcho**
   April 6, 2010

   This is.. not even wrong.

2. **milkshake**
   April 6, 2010

   The time-bending machine from Dr. Quantum + Fox News: It would be lot more feasible to build a machine that bends the public perception of reality… Wait, it already exists.

3. **Chris Oakley**
   April 6, 2010

   Reminds me of graffiti scrawled in one of the men's toilets in college in the Einstein centenary year (1979):

   Space is straight, Einstein was pissed

   [NB: pissed (Brit. sl.) = drunk]

   Underneath, someone had then written

   No, space is bent: God was pissed.

4. **Matt Leifer**
   April 6, 2010

   Wow, this is an incredibly garbled article. I sort of expect it from Dr. Quantum who often produces stuff that skirts the edges of quantum flapdoodle, but I would have expected Sean to be more careful (or at least more highly attuned to to possibility of an out of context quote).

5. **hasan**
   April 9, 2010

   I know jack shit about physics, but when i saw the article was from fox i immediately suspected bullshit. That suspicion was confirmed when i googled for more stories on the subject and couldn't find any. God, i wish google would stop linking to Fox "News" Pieces. I was so excited about the parallel universe thing too.

$2 Million For The Nature of Time

April 6, 2010
Categories: Uncategorized

FQXI has just announced that it will be awarding another series of large grants, of size \$50K-\$100K, totaling about \$2 million. These grants will be targeted at research into "The Nature of Time." Initial proposals are due June 14, grants will start January 2011. For more details, see here and here.

At this point, I'm a bit curious about where the FQXI money is coming from. They started up in 2006 with a seed grant from the Templeton Foundation of about \$8.8 million, and that grant was supposed to finish at the end of last year. Going forward, I haven't seen any indication of whether they're still operating on the seed money, or have new money from Templeton or other sources.

Comments

1. **Kea**
   April 6, 2010

   I received their bulk email about this, even though I asked to be taken off their mailing list years ago. Anyway, I submitted a proposal, because it was easy enough, but I doubt it will get past the admin people … an institution is required.

A Tear at the Edge of Creation

April 6, 2010
Categories: Book Reviews

There's a new book out this week by Marcelo Gleiser, entitled A Tear at the Edge of Creation. Gleiser blogs at the NPR site 13.7, and that site also has a review of the book from his fellow blogger Adam Frank.

Gleiser started out his professional life as a string theorist, enchanted by the prospect of finding a unified theory, and for many years that motivated his research:

> Fifteen years ago, I would never have guessed that one day I would be writing this book. A true believer in unification, I spent my Ph.D. years, and many more, searching for a theory of Nature that reflected the belief that all is one.

Over the years he began to become disillusioned with this quest, not only with string theory, but also with other closely associated ideas (e.g. GUTs and supersymmetry) about how unification is suppose to happen. Hopes that GUTs would give predictive theories of inflation or proton decay have fallen by the way-side, and about supersymmetry he is "very skeptical":

> The fact that the particle has so far eluded detection doesn't bode well. To make things worse, results from the giant Super-Kamiokande detector in Japan and the Soudan 2 detector in the United States have ruled out supersymmetric GUT models, at least the simpler ones, based again on the proton lifetime. If SUSY is a symmetry of Nature, it is very well hidden.

About string theory itself, Gleiser refers to my book and Lee Smolin's, and comments that:

> Responses from notable string theorists were of course highly critical of the books and their writers. Some were even offensive. I find this sort of dueling pointless. People should be free to research whatever they want, although they should also reflect responsibly on whether their goals are realistic.

Ultimately, Gleiser came to the point of view that all hopes for a unified theory are a misguided fantasy, and explaining this point of view is the main goal of the book. In an interview on his publisher's web-site, he says:

> After years searching for a "final" answer, as a scientist and as a person, I realized that none exists. The force of this revelation was so intense and life transforming that I felt compelled to share it with others. It completely changed the way I think about Nature and our place in it.

In his book, he argues repeatedly against the fundamental nature of symmetries in our understanding of physics, seeing the failures of GUTs and supersymmetry as a failure of the idea of getting unification out of larger, more powerful symmetry laws. For him, symmetries are always just approximations, never exactly true principles. He claims to be more interested in asymmetries, in failures of symmetry laws, seeing in asymmetry a fundamental explanatory principle about the universe and humanity's role in it.

Personally, I find myself in strong disagreement with him about this, and don't see much evidence in the book that the abandonment of the search for symmetries that he advocates leads to any positive route to greater understanding of the universe. I agree with him about the failure of the most popular ideas about how to use symmetry to get beyond the Standard Model, but disagree with him about the implications of this.

The problem with both GUTs and supersymmetry is that one posits a new symmetry only to be faced immediately with the question of how to break it, with no good answer. To be successful, any new symmetry principle needs to come with a compelling explanation of how it is to be realized in fundamental physics. A repeated lesson of the development of the Standard Model was that major advances came not only from coming up with new symmetry groups, but through coming up with unexpected ways of realizing them (e.g. spontaneous symmetry breaking and confinement in gauge theory). I don't believe that the gauge symmetries of the Standard Model are approximations, but rather that they are among our most powerful and fundamental physical principles, and that much work remains to be done to understand their full implications. The failures that have discouraged Gleiser have also discouraged many others, and a resulting abandonment of symmetry-based attempts to find a better, more unified, fundamental theory would be a shame.

Comments

1. **Claire**
   April 7, 2010

   Hi Peter,

   there is another option, between the idea of Gleiser and yours: that the known gauge symmetries are exact, but that no higher symmetries exist. So far, experiments point more in this direction.

   After all, Gleiser's experience is not that unification is misguided, but only that supersymmetry-based unification (thus with this particular higher symmetry) does not work.

   Claire

2. **James**
   April 7, 2010

   "Gleiser started out his professional life as a string theorist, enchanted by the prospect of finding a unified theory, and for many years that motivated his research."

   This does not appear to be a true statement, as can easily be checked by looking at Gleiser's papers on SPIRES. It seems that he has worked in cosmology most of his 30 year career and did not start out as a string theorist as you have claimed. It would seem that you have simply asserted this in order to find yet another way to throw mud at string theory.

3. **Peter Woit**
   April 7, 2010

   James,

   To quote from page 149 of the book:

   "Together with my Ph.D. advisor, John G. Taylor, I even wrote one of the first papers on how superstrings could explain the Big Bang, back in 1985."

I don't think I've mischaracterized Gleiser's attitude toward string theory at all. He's quite critical of the whole general idea of trying to find a unified theory, including the specific approach of string-theory based unification, and that's a subject he has worked on and thought about.

4. **DaveB**
   April 7, 2010

   Off topic, but I noticed that your posting about the Paul Dirac biography isn't listed in the book reviews section.

5. **Peter Woit**
   April 7, 2010

   DaveB,

   I only started using the "book review" tag relatively recently. Will add it to the Dirac bio posting, others as I notice them.

6. **jpd**
   April 7, 2010

   is that "tear" as in cry a tear? or a tear in my shirt?

7. **Peter Woit**
   April 7, 2010

   jpd,

   tear as in my shirt is my guess. For one thing, the cover of the book features a torn photograph.

8. **younghun park**
   April 7, 2010

   Hi Peter
   Could you explain the reason why you believe that gauge symmetry is perfect?

9. **Giotis**
   April 7, 2010

   My understanding is that the very nature of the Big Bang itself forces us to search for a unification scheme above a certain energy.

   Regarding symmetries Susskind says pretty much the same with Gleiser in this interview:

   http://www.closertotruth.com/video-profile/Why-do-we-Search-for-Symmetry-Leonard-Susskind-/1493

   But when he talks about symmetries he is referring to global and not gauge symmetries. He doesn't consider gauge as real symmetries in the strict sense. At least this is what I understood.

10. **rrtucci**
    April 7, 2010

    I don't get it. Why is it called 13.7 ? I thought all NPR radio stations in the US are by convention near 90FM. 1/137 ?

11. **theoreticalminimum**
    April 7, 2010

    rrtucci: 13.7 as in 13.7 billion years, estimated age of the universe.

12. **Peter Woit**
    April 7, 2010

    Giotis,

    I don't see how the Big Bang has anything to do with it.

    yonghun,

    Adding gauge-non-invariant terms to the Standard Model action is believed to ruin fundamental consistency properties of the theory such as unitarity. There's also neither any experimental evidence for such terms, nor any theories of them viable enough to motivate people to even bother to look for such effects.

13. **Giotis**
    April 7, 2010

    Peter, what do you mean when you say that the Big Bang has nothing to do with the unification of forces?

    This is pretty much conventional wisdom. To see what I mean check this article in Wikipedia for example regarding the evolution of forces during the Big Bang.

    http://en.wikipedia.org/wiki/Timeline_of_the_Big_Bang

    Or maybe you mean something else?

14. **Signs of the Times**
    April 7, 2010

    The era of "shut up and calculate" is long gone. Now it seems to be
    "Don't compute, just blather on"

15. **Peter Woit**

April 7, 2010

Giotis,

What we can observe about the Big Bang just doesn't give us any useful information about unification and Beyond Standard Model physics. What gets a lot of attention is that people have been hopeful that we might observe things like monopoles or cosmic strings that were remnants of BSM physics produced at early stages of the Big Bang, but nothing like this has ever been seen. All our observations of the Big Bang are completely consistent with the SM. Nothing we see about the Big Bang is helpful for understanding LHC energy physics, much less higher energies.

16. **Peter Morgan**
April 7, 2010

As an empirical matter, I take an observed symmetry as a starting point: to say that a real object is circular or square starts a discussion in terms of a particular mathematical object, after which we can introduce more subtle measures of how close the real object is to being an ideal circle or square. If we start with a real object that is close to being a circle, it's relatively natural to use representations of the rotation group to express how close the object is to being a circle. When we later discover that an object is close to being an ellipse, it may become relatively natural to introduce other, different measures of how close the object is to being an ellipse.

In the more abstract arena of correlations between observables at time-like separation, which have traditionally been discussed from a starting point of a Hamiltonian or Lagrangian presentation of a free quantum field dynamics, it is relatively natural, and quite empirically effective, to introduce gauge-invariant deformations of that type of presentation, while keeping the CCRs undeformed. That is by no means true if we adopt a different mathematical starting point for discussing correlations at time-like separations, or if we were to decide, more radically, that it is worthwhile to discuss something other than correlations, expected values, and other statistics as a universal representation of empirical data.

The idea of representations of a symmetry group that act (linearly or non-linearly) on a representation space is sufficiently general as a way to organize calculation that it seems likely to be part of at least the next generation of mathematics that is used to describe what mathematical object the world is like, and how close the likeness is, just as a matter of correspondence. But in the more removed future we might have to say that some more abstract mathematical concept has taken over from "symmetry" as the label du jour.

Gleiser, I suppose, has to some extent given up the quest to find some mathematical object that is more like the way the world is, in a tractable and useful way, than what we currently have.

17. **Bill K**
April 7, 2010

All our observations of the Big Bang are completely consistent with the SM. Nothing we see about the Big Bang is helpful for understanding LHC energy physics, much less higher energies.

One of the most important observations from the Big Bang is inflation, and the hypothetical particle which causes this, the inflaton, is not a part of the Standard Model.

18. **John Baez**
April 7, 2010

Peter wrote:

> All our observations of the Big Bang are completely consistent with the SM.

What about the evidence widely believed to support the inflationary cosmology? I don't think inflation can be explained using only Standard Model physics.

19. **neo**
April 8, 2010

So is the search for symmetry and unification a good approach to doing physics, or not? In some cases, the search for them has led to profound discoveries. In other cases, they have proved a wild goose chase and wasted productive years by great scientists. In still other cases, symmetry and unification were the result but never the motive.

20. **Kea**
April 8, 2010

Peter, classical symmetries rely on the classical concept of spacetime. Do you not think there is a good chance that the non local formulation of the SM (which must exist according to the evidence from twistor theory) might employ distinct, albeit necessarily deeper, principles?

21. **Frank**
April 8, 2010

John,

there is *no* observational evidence for inflation. As explained in the wikipedia article you cite, there is evidence for flatness, homogeneity, a specific fluctuation spectrum, etc., etc.. But many cosmological models gives those same results, also models without inflation. (This is not mentioned in the wikipedia article, but any cosmologist will confirm this.)

To claim that cosmology provides any evidence beyond the standard model is wishful thinking!

22. **Peter Woit**
April 8, 2010

Bill K and John,

My comment about SM and the Big Bang was perhaps slightly too strong. The problem with inflation is that it gives us very little extra to go on, basically something like "there's some kind of other physics, which can be crudely modeled by an inflaton with an effective potential we have some very minimal information about". The original hope was that this inflaton would correspond to a particle that fit into the GUT scenario, and that then inflation would be experimental evidence for unification, but this has not worked out.

Kea,

Twistor theory is a very good example of a different way of realizing symmetries, one that could turn out to have deep significance for unification.

23. **Physicsphile**
April 8, 2010

Cosmology very strongly points towards the existence of dark matter which cannot be explained by the SM.

24. **Peter Woit**
April 8, 2010

Physicsphile,

The main evidence for dark matter comes not from cosmology but from observations of relatively nearby galaxies. As with inflation, what we know about the Big Bang gives us only very minimal information about dark matter and what sort (if any..) of non-SM physics it might come from.

My point remains that, despite a lot of hype promoting the idea that the way to get information about unification is to study cosmology, this hasn't really worked out.

25. **Physicsphile**
April 8, 2010

Peter,

The rotation curve of galaxies is an important piece of confirming evidence for dark matter but by itself it does not show that the dark matter is not a SM particle.

To do that you need Cosmic microwave background data or a combination of galaxy survey data and big bang nuclear synthesis data.

26. **Remus**
April 8, 2010

> combination of galaxy survey data and big bang nuclear synthesis

I'm pretty ignorant on these matters, but nucleosynthesis falls squarely within the SM of particle physics. In fact, it is pre-SM physics, since it was first developed before the SM was even formulated.

27. **Thomas R Love**
April 8, 2010

Gleiser's statement:

'After years searching for a "final" answer, as a scientist and as a person, I realized that none exists.'

This is classic sour grapes. He is saying that he failed to find such a theory and since he is so smart, no one else can either.

Conservation laws are the foundation of physics and conservation laws follow from continuous groups, therefore groups form the foundations of physics.

28. **Paul**
April 8, 2010

Personally I don't see symmetry as fundamental to unification. Symmetry is an important property but it is just an abstraction forming only part of a proper description of any object.

SM is mostly based on guesswork and since it is much easier to guess a symmetry then to guess all the other details of a description symmetry has played an important role. But now that we know all the relevant symmetries the task is to elucidate all the other details of description of fundamental physics.

29. **chris**
April 9, 2010

a highly esteemed colleague once said that all of particle physics is mislead by the higgs mechanism. most people expect the symmetry to increase by going to higher energies, where actually experience tells us that it is exactly opposite. in solid state physics you can plainly see this. and also in high energy physics it is to be expected: the modern view of renormalizability has told us that no matter what happens at the high scale, at low energies we will end up with the SM lagrangean with only the few renormalizable terms that it contains.

it would be very strange in my opinion, if this symmetry generating mechanism, that is already discovered, would be unused by nature and instead of a proliferation of terms we would see them thinning out at high energies. to me, this scenario seems strongly disfavoured by the renormalizability of the SM.

30. **Physicsphile**
April 9, 2010

Remus,

big bang nuclear synthesis data indicates that baryons only make up about 5% of the energy density needed to close the Universe. Redshift surveys indicate that non-relativistic matter makes up about 30%, so 25% of this is inferred to be non-baryonic matter.

Independently, the CMB can constrain both quantities. Some assumptions are needed to do this in each case, as with all science. If you combine more data sets you can then reduce the number of assumptions.

31. **Remus**
April 9, 2010

Physicsphile,

I agree with you that Big Bang nucleosynthesis predicts an amount of baryonic matter that, when compared with the matter needed to explain astronomical observations of gravitational effects, turns out to be just about 5% of the mass/energy density of the universe.

But nucleosynthesis is a collection of purely SM processes. In fact, since the synthesis of nuclei occurs at typical energy scales of tens of MeV, it is pretty insensitive to the high energy structure of the SM, or its extensions. For example, nuclear synthesis occurs in the Sun all the time, and it's quite independent of the precise value of the top mass.

Chris,

the SM has a larger symmetry group than any of its lower-energy effective theories. At a scale of, say, 1MeV, particle physics looks like QED + Fermi model + nuclear physics. At the electroweak scale, it's all just a single gauge theory (with a product group, so not unified yet). So, at least in this case, higher energy leads to more symmetry. Which comes to show that the RG-flow point of view you are taking does not prohibit that from happening.

By the same token, it might well happen that at 1TeV particle physics looks like the SM + a bunch of effective interactions, which are all derived from a single theory with an even larger symmetry group at some higher scale. Or it might well be that something else happens…

32. **JC**
April 10, 2010

I took a look through this book earlier this evening. It consists of a lot of short chapters about stuff unrelated to particles, cosmology, strings, etc …

Overall it reads a bit like a rambling monologue, with some nuggets about his change of heart about unification and the "eureka moment" which led to it.

33. **Bill K**
April 10, 2010

Remus,

A moment spent googling 'big bang nucleosynthesis' will help clear up your confusion. Yes, it's true that the nuclear reactions are low energy. However the net yield of light isotopes produced (particularly deuterium) is sensitive to the temperature curve of the cooling big bang, and this in turn is influenced by whether you use the standard model or one of its extensions. So the amount of primordial deuterium that one observes has a direct bearing on particle models.

34. **Remus**
April 11, 2010

Bill, there's no confusion at all. Element abundances in the Universe depend on nucleosynthesis rates and on the thermal history of the Universe. Nucleosynthesis data do not disagree with the SM. The thermal history of the Universe is another story, to which I made no reference in my posts.

35. **Physicsphile**
April 12, 2010

Remus,

I never claimed BBN was not explained by SM processes.

36. **Chris W.**
April 12, 2010

An interview with Marcelo Gleiser is here.

He may have a case to make, but judging from this interview it isn't much of one.

Three Mysteries

April 13, 2010
Categories: Favorite Old Posts, Uncategorized

There's a well-known list of high-profile problems in fundamental theoretical physics that have gotten most of the attention of the field during the past few decades (examples would be the problems of quantizing gravity, solving QCD, explaining dark energy, finding a model of dark matter, breaking supersymmetry and connecting it to experiment, etc.). Progress on these problems has been rather minimal, and in reaction one recent trend has organizations such as FQXI promoting research into questions that are much more "philosophical" (for instance, they are now asking for grant proposals to study "The Nature of Time"). In this posting I'd like to discuss a different class of problems, ones which I believe haven't gotten anywhere near the attention they deserve, for an interesting reason.

The three problems share the characteristic of being apparently of a purely technical nature. The argument against paying much attention to them is that, in each case, even if one were to find a satisfactory solution, it might not be very interesting. It's possible that all one would discover is that the conventional wisdom about these problems, that they're just "technical" and thus not of much significance, is correct. The argument for paying more attention is that the technical problem may be an indication that we're doing something wrong, that there is something of significance about the Standard Model that we haven't yet understood. Achieving this understanding may lead us to the insight needed to successfully get beyond the Standard Model. At the moment all eyes are on the LHC, with the hope that experiment will lead to new insight. Whether this will work out is still to be seen, but in any case it looks like it's going to take a few years. Perhaps theorists with nothing better to do but wait will want to consider thinking about these problems.

Non-Perturbative BRST

The BRST method used to deal with the gauge symmetry of perturbative Yang-Mills theory does not appear to generalize to the full non-perturbative theory, for a rather fundamental reason. This was first pointed out by Neuberger back in 1986 (Phys. Lett. B, 183 (1987), p337-40.), who argued that, non-perturbatively, the phenomenon of Gribov copies implies that expectation values of gauge-invariant observables will vanish. I've written elsewhere about a different approach to BRST that I'm working on (see here), which is still at a stage where I only fully understand what is going on in some toy quantum-mechanical models. My own point of view is that there's still a lot of very non-trivial things to be understood about gauge symmetry in QFT and that the BRST sort of homological techniques for dealing with it are of deep significance. Others will disagree, arguing that gauge symmetry is just an un-physical redundancy in our description of nature, and how one treats it is a technical problem that is not of a physically significant nature.

One reaction to this question is to just give up on BRST outside of perturbation theory as something unnecessary. In lattice gauge theory computations, one doesn't fix a gauge or need to invoke BRST. However, one can only get away with this in vector-like theories, not chiral gauge theories like the Standard Model. Non-perturbative chiral gauge theories have their own problems…

Non-perturbative Chiral Gauge Theory

Since the early days of lattice gauge theory, it became apparent that chiral symmetry was problematic on the lattice. One way of seeing this is that naively there should be no chiral anomaly on the lattice. The problem was made more precise by a well-known argument of Nielsen-Ninomiya. More recently, it has become clear that one can consistently introduce chiral symmetry on the lattice, at the cost of using fermion fields that take values in an infinite dimensional space. One such construction is known as "overlap fermions", which have the crucial property of satisfying relations first written down by Ginsparg and Wilson. This kind of construction solves the problem of dealing with the global chiral symmetry in theories like QCD, but it still leaves unsolved the problem of how to deal with a gauged chiral symmetry, such as the gauge symmetry of the Standard Model.

Poppitz and Shang have recently written a nice review of the problem, entitled Chiral Lattice Gauge Theories Via Mirror-Fermion Decoupling: A Mission (im)Possible? They comment about the significance of the problem as follows:

> Apart from interest in physics of the Standard Model — which, at low energies, is a weakly-coupled spontaneously broken chiral gauge theory that does not obviously call for a lattice study — interest in strong chiral gauge dynamics has both intensified and abated during the past few decades. From the overview in the next Section, it should be clear that while there exist potential applications of strong chiral gauge dynamics to particle physics, at the moment it appears difficult to identify "the" chiral theory most relevant to particle physics model-building (apart from the weakly-coupled Standard Model, of course). Thus, the problem of a lattice formulation of chiral gauge theories is currently largely of theoretical interest. This may or may not change after the LHC data is understood. Regardless, we find the problem sufficiently intriguing to devote some effort to its study.

In a footnote they compare two points of view on this: Creutz who argues that the question is important since otherwise we don't know if the Standard Model makes sense, and Kaplan who points out that if there is some complicated and un-enlightening solution to the problem, it won't be worth the effort to implement.

You can read more about the problem in the references given in the Poppitz-Shang article.

Euclideanized Fermions

Another peculiarity of chiral theories arises when one tries to understand how they behave under Wick rotation. Non-perturbative QFT calculations are well-defined not in Minkowski space, but in Euclidean space, with physical observables recovered by analytic continuation. But the behavior of spinors in Minkowski and Euclidean space is quite different, leading to a very confusing situation. Despite several attempts over the years to sort this out for myself, I remain confused, and can't help suspecting that there is more to this than a purely technical problem. One natural mathematical setting for trying to think about this is the twistor formalism, where complexified, compactified Minkowski space is the Grassmanian of complex 2-planes in complex 4-space. The problem though is that thinking this way requires taking as basic variables holomorphic quantities, and how this fits into the standard QFT formalism is unclear. Perhaps the current vogue for twistor methods to study gauge-theory amplitudes will shed some light on this.

On the general problem of Wick rotation, about the deepest thinking that I've seen has been that of Graeme Segal, who deals with the issue in the 2d context in his famous manuscript "The Definition of Conformal Field Theory". I saw recently that he's given some talks in Europe on "Wick Rotation in Quantum Field Theory", which makes me quite curious about what he had to say on the topic.

For some indication of why this confusion over Minkowski versus Euclidean spinors remains and doesn't get cleared up, you can take a look at what happened recently when Jacques Distler raised it in related form on his blog here (he was asking about it in the context of the pure spinor formulation of the superstring). I'm not convinced by his claim that the thing to do is to go to Euclidean space-time variables, while keeping Minkowski spinors. Neither is Lubos, and he and Jacques manage to have an argument about this that sheds more heat than light. It ends up with Lubos accusing Jacques of behaving like Peter Woit, which lead to him being banned from commenting on the blog. While this all is, as Jacques describes it "teh funny", it would be interesting to see a serious discussion of the issue. Since it in some sense is all about how one treats time, perhaps one could get FQXI funding to study this subject.

Update: Lubos Motl has immediately come up with a long posting explaining why these are all non-problems, of concern only to those like myself who are "hopeless students", "confused by many rudimentary technicalities that prevented him from thinking about serious, genuinely physical topics." If I would just understand AdS/CFT and Matrix theory I would realize that gauge symmetry is an irrelevance. Few in the theoretical physics community are as far gone as Lubos, but unfortunately he's not the only one that thinks that concern with these "technicalities" is evidence that someone just doesn't understand the basics of the subject.

Comments

1. **Interested**
   April 14, 2010

   "Others will disagree, arguing that gauge symmetry is just an un-physical redundancy in our description of nature, and how one treats it is a technical problem that is not of a physically significant nature."

   Interesting. Please elaborate on that argument.

2. **Trent**
   April 14, 2010

   Peter, you write,

   "More recently, it has become clear that one can consistently introduce chiral symmetry on the lattice, at the cost of using fermion fields that take values in an infinite dimensional space."

   This is complete rubbish. Fermions in the overlap formulation have exactly the same number of components as for example fermions in the Wilson formulation. At each space time point they have 4 Dirac indices and 3 color indices.

   What you might be confusing with overlap fermions are domain wall fermions. However domain wall fermions do not solve the chirality problem exactly, there is a residual breaking. In the limit when the residual breaking goes to zero one recovers overlap fermions, which have, as stated above, the same number of components as conventional Wilson fermions. No surprise there, because overlap fermions are based on Wilson fermions, only the Dirac operator is not the Wilson matrix, but a function of it (related to the sign function, if someone was curious).

   Trent

3. **Trent**
   April 14, 2010

   You mentioned Distler's blog, where I also commented and pointed out that the initial assertion of Distler is incorrect. He also admits this, so this issue is very clear and there is no confusion. All of this happened before Lubos entered the scene though 🙂

   Basically, Distler was claiming that in Minkowski space \eta_\mu^\mu = d – 2 whereas in Euclidean space \eta_\mu^\mu = d so he thought there was a problem somewhere.

   But actually there is no problem at all, as I pointed it out to him and he admitted to being wrong, since also in Minkowski space we have \eta_\mu^\mu = d and there is no minus 2 anywhere.

   This is basically elementary linear algebra or vector calculus or whatever you want to call it, at any rate part of QED 101 material. I was surprised to see Distler getting confused about such simple things, but good thing is he quickly admitted to being wrong. Gentlemanly, one could say.

   This is also my take on your issue. What is exactly wrong with Wick rotation and analytical continuation once it's done properly (with positivity constraints)? In lattice gauge theory the constraint is called "reflection positivity". Please state a specific problem or specific quantity one can not calculate in a Euclidean setting. Things like "oh, it's so mysterious" doesn't count 🙂

   Trent

4. **Peter Woit**
   April 14, 2010

   Trent,

   Apologies for the imprecise statement about the overlap formalism and the other ideas about how to deal with chirality that it grew out of. For a detailed explanation by an expert of the point I was referring to, see Neuberger's hep-lat/9808036, where his summary of the situation goes:

   "At the base of this progress lies a world consisting of an infinite number of fermions, all but one having very large masses. This infinity is fully under control and consequently can be completely eliminated."

   As for the Wick rotation question, I should make it clear that this "mystery" is of a different nature than the two others. It's not that there's a well-defined problem of non-existence of a needed non-perturbative construction (at least in flat space-time). It's rather that I just don't see a clear explanation of the relationship between the QFTs in Minkowski signature and Euclidean signature, especially when dealing with chiral fermions. What I'm looking for is something that treats the spinor geometry in a coordinate-invariant way, so would make sense even in a curved background. Maybe such a thing exists and I'm just unaware of it, but the literature I've looked at doesn't seem to contain such a thing. The discussion between Lubos and Jacques indicates that maybe I'm not the only one confused by this topic.

5. **Peter Woit**
   April 14, 2010

   Interested,

   This is a complicated topic. Two quick points about it are:

   1. In principle, you are supposed to be able to get rid of the problem of gauge symmetry by working just with gauge-invariant variables. The problem with with this argument is that the (gauge-dependent) space of connections (gauge fields) is a linear space that we know how to quantize, and with coordinates that are local and transform simply under Lorentz transformations. If you try to eliminate the gauge degrees of freedom, go to the "reduced space" and quantize this instead, you lose all these nice properties and encounter intractable "technical" problems.

   2. In a Hamiltonian formalism, you should be able to deal with gauge symmetry by just looking at states invariant under the gauge group. However, something more subtle is going on when you actually try and implement this. Complexifying, there are no gauge-invariant states, so the standard Gupta-Bleuler trick is to impose invariance only under half the gauge symmetry. BRST deals with this issue in a trickier and very interesting way.

6. **Trent**
April 14, 2010

Peter, you write

"What I'm looking for is something that treats the spinor geometry in a coordinate-invariant way, so would make sense even in a curved background."

Such a thing certainly exists and is well-known, it's called the spinor bundle of a Riemannian manifold. The metric can be curved or flat, doesn't matter. The spinor bundle is defined as long as a certain Stiefel-Whitney class is zero and the various choices are parametrized by another Stiefel-Whitney class. So there is no mystery at all, in my opinion, around defining spinors for a curved manifold.

Trent

7. **Peter Woit**
April 14, 2010

Trent,

Of course I know how to define the spinor bundle on a curved manifold. The problem is Wick rotation or analytic continuation.

To have the usual notion of analytic continuation, relating sections of spinor bundles over a Riemannian signature space and over a Minkowski signature space, it seems to me you need to represent the two spaces as subspaces of a complex manifold of twice the (real) dimension, and the spinor bundles as restrictions of a holomorphic bundle. This is why I mentioned the Grassmanian and twistor theory. There you have exactly this, and even better, the (half)-spinor bundle is just the tautological bundle over the Grassmannian.

This is all just a story about classical fields, and what you really want is a quantum field theory. That's where I start to get confused….

8. **anonymous**
April 14, 2010

Just to point out a recent progress towards resolving the Neuberger problem done in http://arxiv.org/pdf/0710.2410 and http://arxiv.org/pdf/0812.2992 . Though it is true that the Neuberger problem is a technical problem (just for the lattice), they want to resolve it to do proper simulations then – the same motivation as to have chiral gauge theory on the lattice: to do non-perturbative computations for QCD in a practical way.

9. **Interested**
April 14, 2010

Thanks Peter.

My impression of your post was that you were explaining that guage symmetry doesn't have a physical basis.

On something a little unrelated, I would just like to let you know my impression is that, outside particle physics, nobody cares about a theory that doesn't make experimental predictions. As such shouldn't it be called the String Hypothesis (hypotheses??)?

So keep fighting the good fight.

10. **Interested**
April 14, 2010

Sorry

My impression is that *others* believe guage symmetry doesn't have a physics basis. I am aware (If I remember your book/blogs correctly) that you think that symmetry is the key to making progress.

11. **Peter Woit**
April 14, 2010

Interested,

I don't think the term "has a physics basis" is very well defined, but it's true that I think properly understanding the implications of gauge symmetry will give deep insight into fundamental theory. Others think it's a technical device that can in principle be ignored.

Yes, calling string theory a "theory" has led to all sorts of confusion and trouble. But, that's far off-topic, please try and keep this posting string-free unless strings have something to do with one of these three problems.

12. **CKK**
April 14, 2010

What Lubos describes is in fact one of the great achievements of modern physics – the realization that problems too hard for conventional methods may always be solved by psychology – all one has to do is convince enough peers that the problem is a non-issue, a technicality, a shortcoming of mathematics which has nothing to do with real physics, and the problem will simply go away.

This clever method has been very successful recently, particularly in string theory, where it is by far the most popular technique with a large number of devoted fans.

13. **ijc**
April 14, 2010

I sometimes appreciate what Lubos has to say, but his reaction to these issues betrays his disappointingly un-mathematical character. The issues are technical matters, but that does not make them less important, unless we are to be satisfied with doing only calculations according to symbolic replacement rules, without any definition a meaning for the formal objects.

At no point did you (Peter) even attack the un-rigorous establishment in theoretical physics, perhaps you accept it as necessary ever since Newton introduced calculus without defining it, and yet Lubos' reaction is quite defensive. He really does seem to prefer physicists who are so intoxicated by their own egomania that they just consider their mastery of the replacement rules to suffice for understanding:

LM: ” More experienced and powerful physicists have no trouble with the formalism and they can instantly penetrate it and get to the actual physics.”

That’s right, LM and his powerful friends have no concern over writing nonsense such as complex measures on function space; not only do they have no concern, but they judge anyone who wants to properly contextualize these “analytic continuations” into a mathematical space as a bad student. More powerful physicist have no trouble with replacement rules like “t – > i T”, and they KNOW that these kinds of rules are as complete and total of an understanding as there could ever be.

14. **Peter Woit**
April 14, 2010

ijc,

Historically I think there is good reason for the standard prejudice amongst physicists to just charge ahead and not worry about what appear to be technicalities. You may get somewhere this way that you wouldn’t have gotten to if you had got hung up thinking about something that didn’t turn out to be really necessary.

After 30 some years though of charging ahead and getting nowhere (or farther and farther from anywhere one would like to be), this may be an unusual historical moment in which it would be a good idea to go back and take a closer look at those things one doesn’t completely understand, even if someone has a plausible sounding argument about why this is likely to be a waste of time.

15. **mathphys**
April 14, 2010

When I try to read Motl’s post, my browser comes up with a warning message that says “To protect your security, Internet Explorer blocked this site from downloading files to your computer. Click here for options…”.

I think he puts so many ads and so forth on his web pages it’s impossible to read now.

16. **Peter Woit**
April 14, 2010

mathphys,

For the past couple days, Lubos’s blog has been hanging and crashing one of the browsers (the gnome browser, epiphany) on the Linux machine I use at work. In this he joins Jacques, whose blog for a year or so has been giving an error message when you try to connect to it with epiphany. Firefox does still work on both of them.

A week or so ago, I noticed a loud noise at home periodically, finally realized that it was occurring whenever I had a browser window open on Lubos’s blog. Turns out it was the processor fan on my Windows machine, which started cranking at top speed to keep the processor cool as it tried to deal with Lubos’s web-pages…

17. **N. Nakanishi**
April 14, 2010

Peter,
I would like to point out that non-perturbative covariant BRS-quantization of gauge field theory was formulated in a satisfactory way more than thirty years ago. For reviews, see

T. Kugo and I. Ojima, Prog. Theor. Phys. Suppl. 66 (1979) 1.
N. Nakanishi and I. Ojima, Covariant Operator Formalism of Gauge Theories and Quantum Gravity (World Scientific, 1990).

The trouble of Gribov copies is nothing more than a defect of the path-integral approach, just like the doubling problem of fermion poles in the lattice theory. The path-integral approach cannot give a complete quantum field theory, because in its own framework one cannot prove the unitarity of the physical S-matrix. The path-integral approach induced such inadequate notions as instantons, axions, etc. It is generally inconsistent with field equations, and therefore violates the Noether theorem, because of the use of T*-product instead of T-product. This fact induced incorrect appearance of various anomalies such as gravitational anomaly, FP ghost-number anomaly, etc.

18. **Peter Orland**
April 15, 2010

N. Nakanishi

Many people have noticed that the Gribov problem occurs in Hamiltonian quantization of non-Abelian gauge theories – not just in path integrals. The problem of copies means there is an essential difficulty in solving Gauss’ law, for most gauge conditions. Gribov copies are not an artifact of any particular formalism. The only way to avoid the problem is not fixing the gauge. This is fine for some purposes (strong-coupling lattice, Monte-Carlo), of course.

19. **Spivak**
April 15, 2010

Lubos wrote an article regarding three mysteries you suggested, I am very much interested to hear your counterargument.

20. **A.**
April 15, 2010

Agree with Peter W. that nonperturbative BRST is an important problem, esp. in regard to Gribov and the difficulty of quantising on the reduced space of gauge fields. As already illustrated by the opposing opinions, here, of whether Gribov is relevant nonperturbatively, this is a hard topic to make progress in. Also agree with Peter O. that the copies are in no way attached to a particular formulism. It’s easy to find examples in the literature which make no reference to the path integral approach.

One issue which makes this area difficult to persue is the reluctance of large parts of the community to care about it — I’ve found that the average reviewer is typically very hostile to any paper which tries to make a point of the importance of Gribov. Often the reaction is just “go to the lattice” and everything will ultimately be fine (even when the reviewer admits that there are still copies on the lattice).

21. **abc**
April 15, 2010

Hi,
As rightly pointed out by A., Gribov copies do exist on the lattice too (e.g. Recently, Gribov copies are fully classified on the lattice though for a ‘toy’ model http://arxiv.org/pdf/0912.0450). Their effects on the gluon propagators on the lattice is an active research area mainly being done by several groups in Germany, Australia and Brazil. And many issues need to be settled down there.

Best,
abc

22. **Matti Pitkanen**
April 15, 2010

mathphys said: When I try to read Motl's post, my browser comes up with a warning message that says "To protect your security, Internet Explorer blocked this site from downloading files to your computer. Click here for options…".

I have similar problem with Mac. For several times I have got the warning that the loading of the contents of the page puts my computer to a non-responsive state.

23. **Thomas Larsson**
April 15, 2010

Firefox with NoScript, Adblock Plus and WOT (not Woit): Lubos' page loads rapidly and without problems on both Ubuntu and Windows, even on a old machine.

24. **Peter Woit**
April 15, 2010

Spivak,

Responding to Lubos in detail would be a full-time job, but here are a few other comments:

1. On the Wick rotation issue he's mostly arguing with Distler, not me. I think we agree that this is a crucial part of QFT, with QFTs not well-defined in Minkowski space. Exactly how this works for chiral spinors, non-perturbatively, in non-flat backgrounds, seems to me to be something still not completely understood. Lubos just asserts that it works without providing a reference to where one can find the details.

2. He claims that these problems are just lattice artifacts, that some other non-perturbative formulation of gauge theory won't have these issues. The problem is that there is no such thing as a completely well-defined non-perturbative definition of chiral gauge theory outside of the lattice framework. The lesson of many years of work on understanding the problem of chirality on the lattice is that it's not a lattice artifact, but something inherent in the use of a cut-off, some version of which is needed to make sense of the QFT. His comments about the problems of supersymmetry on the lattice are irrelevant, this is a question about a non-supersymmetric theory.

3. The general criticism he and others make of anyone trying to work on these issues, that they're just second-raters working on uninteresting technicalities, unable to, like real men, work on AdS/CFT, I think I've dealt with in the posting and earlier comments.

25. **Chris Austin**
April 15, 2010

Hi Peter,

What is your opinion on Peter Hirschfeld's paper "Strong evidence that Gribov copying does not affect the gauge theory functional integral," Nuclear Physics B 157 (1979) 37-44, which he wrote whilst an undergraduate at Princeton, with advice from Curtis Callan, if I remember correctly?

Best,
Chris

26. **Peter Woit**
April 15, 2010

Hi Chris,

I'd never seen that paper, just took a look at it. Peter O. is more of an expert on this question, and maybe he knows if there is a problem with this Peter H. paper, and if so what. From my quick look, there are several things to worry about in his argument (non-compactness of the gauge group, existence of the kind of coordinates he wants, whether the intersection number he gets is zero…). In any case, if his claim is that one can just ignore the contributions of Gribov copies, that seems to conflict with the large amount of later work on the subject.

27. **F**
April 15, 2010

"Non-perturbative QFT calculations are well-defined not in Minkowski space, but in Euclidean space, with physical observables recovered by analytic continuation."

Peter, your comment above is not quite correct. Euclideanization is ONE way to handle non-perturbative calculations in QFT, but not the only way. Light cone quantization of quantum field theories is a standard tool that can be used to obtain non-perturbative solutions without a Wick rotation of the time coordinate. In this way, one can avoid a lot of the subtle issues you mention arising from changing the space-time signature of a theory.

28. **anon.**
April 15, 2010

Just to be precise about the issues of BRST and Gribov problems — they don't affect the nonperturbative definition of the theory since, as you say, the lattice can always calculate gauge-invariant quantities. They are relevant when people want to investigate, e.g., the "nonperturbative gluon propagator" in some particular gauge. But there's no deep reason to think such things are good physical quantities to study.

Similarly, the Gribov problem exists for many gauges in the continuum (e.g. Landau gauge), but not for others (Weinberg's book, for instance, skirts the problem by using axial gauge).

So I think really the only problem is that one cannot write down an action that is simultaneously *manifestly local*, *manifestly Lorentz-invariant*, and *gauge fixed* without running into Gribov problems. (Although Zwanziger has a formalism attempting to do this by adding more auxiliary fields, which I've never fully digested.)

Whether one things these problems are interesting is a matter of taste — I find them somewhat interesting, but I doubt they hold the key to a more solid nonperturbative understanding of QCD, since any such understanding should be expressible in terms of only gauge-invariant quantities.

29. **Peter Orland**
April 15, 2010

anon.

The reason Gribov copies are interesting is the following. The physical configuration space of Yang-Mills theories are gauge orbits, not gauge fields. A gauge orbit is an equivalence class of gauge connections, the equivalence relation being gauge-equivalent.

If you don't want to find coordinates on the space of orbits, then you don't have to worry about the Gribov problem. If you do, then you do have to worry.

So whether or not it is important depends on how you want to study gauge theories. In perturbation theory, you can ignore copies, which correspond to large gauge transformations. If you study gauge-invariant functionals (like Wilson loops) and somehow have a calculational scheme avoiding gauge fixing, you can bypass the issue altogether. Unfortunately, no one has such a scheme for QCD.

The Gribov problem is present in axial gauges as well, though open boundary conditions (instead of periodic boundary conditions) ameliorate the situation considerably.

30. **N. Nakanishi**
April 15, 2010

Peter Orland,
Please give me a reference, if any, which shows that the Gribov problem is relevant to the operator formalism of the
BRS-quantized gauge theory. I note that in the operator formalism, of the BRS-quantized gauge theory, c-number solutions to the gauge fixing condition are totally unnecessary; the canonical quantization can be carried out exactly in the same way as in the non-gauge theory.

31. **Peter Orland**
April 15, 2010

N. Nakanishi,

Respectfully, I don't have to give you a reference. The Gribov issue is very general, and it isn't my problem whether the methods you advocate are right or wrong. That is for you to work out.

Anyway, Peter W. insists this is not a physics discussion board, so I will just say this:

You need to show that large gauge transformations are properly taking into account in your formalism (that is how copies arise). Everything I have seen on BRS (except Herbert Neuberger's work) deals with infinitesimal gauge transformations. For example, how do you handle the Gribov horizon, where the Fadeev-Popov determinant vanishes?

32. **N. Nakanishi**
April 15, 2010

anon,
You claim "one cannot write down an action that is simultaneously manifestly local, manifestly Lorentz-invariant, and gauge fixed without running into Gribov problems".

Such a theory does exist! It is the Kugo-Ojima theory (see,
Prog. Theor. Phys. Suppl. 66 (1979) 1-130).

33. **N. Nakanishi**
April 15, 2010

Peter Orland,

It seems to me that you do not understand the fundamental nature of quantum field theory. In quantum field theory, symmetries are described by Lie algebra but not Lie group; that is, I mean that only infinitesimal transformations are
relevant. To recognize this point is particularly important in supersymmetry, because there is no "supergroup". The same is true for the local gauge symmetry: finite-size local gauge transformations are irrelevant to the quantum gauge theory. The c-number gauge trasformation is a purely classical concept; in quantum gauge theory, there exist only the BRS transformations. So, it is totally unnecesary to take account of the Gribov copies in the proper framework of quantum field theory. The FP determinant is a consequence of the path-integral formalism; it is a misleading notion based on the classical gauge theory.

34. **Peter Orland**
April 15, 2010

N. Nakanishi,

Yes, call me a dummy. Other people have said worse.

Only infinitesmal transformations are relevant? Not so. Global properties of symmetry groups are important, even in the theory of spin.

The Fadeev-Popov determinant, can be derived from the Haar measure of a lattice theory. There is nothing misleading about it. Is the lattice theory just a classical concept to you (or a misleading notion)?

35. **Peter Orland**
April 15, 2010

Anyway, I will not comment further on this thread. I am sure Peter W. is tired of insults flying around.

36. **N. Nakanishi**
April 16, 2010

Peter Woit,

Since Peter Orland does not wish to continue the discussion, I would like to have your opinion. Do you still assert that the non-perturbative BRS-quantization of gauge theory is not yet formulated?

Peter Orland's comments provide no logical basis for claiming that the Gribov problem is relevant to the operator formalism of the BRS-quantized gauge theory. What he commented are nothing more than his feelings. (Reply to his question: The lattice gauge theory is essentially based on the path integral. There is no BRS-formulated lattice theory.)

37. **Z**
April 16, 2010

Hello, I find the subject of gauge symmetries very interesting but also very confusing, I tried asking Lubos about some of the details of his argument about gouge symmetries being just technicalities but without luck so far, perhaps someone here could be more helpful, here is what is bothering me:

On one hand Lubos argues that gouge fields represent unphysical degrees of freedom and it agrees with what I've read generally. But on the other hand it is also said that gouge bosons mediate interactions, for example a photon is a gouge boson associated with U(1) symmetry. Isn't that a contradiction? Photons are certainly physically real, so why are they modeled by gouge fields which should only describe unphysical states?

Lubos also claims and Peter seems to (at least partly) agree that one can reformulate the theory without any gouge degrees of freedom but if that is indeed the case that what will happen to the photon and other gouge bosons in such a description? What will mediate forces?

Finally the gouge symmetry associated with photons – U(1) represents quantum phase and while there may be no way to directly observe this phase is not really unphysical as it does affect experimental outcomes (position of interference fringes in in double slit experiments or Aharonov Bohm variants for example).

So are gouge degrees of freedom completely unphysical or do some of them describe physically real phenomena?

38. **Peter Woit**
April 16, 2010

anon,

While it's possible to argue that one can just ignore problems with handling gauge symmetry in vector-like theories like QCD (not that I'm convinced, one may very well learn something important by taking these problems seriously), this is not at all the case for chiral gauge theories (like the Standard Model…).

Nakanishi,

Yes, I'm afraid I agree with Peter Orland. I don't believe the operator formalism for QCD is now well-defined in the sense that the path integral formalism is through lattice gauge theory. The lattice formalism gives a completely well-defined calculational set-up that one can even put on a computer (although the calculation may not be practical). A possible addition to my list of mysteries would be that of the relationship between the operator formalism and the path integral formalism. This is much trickier than often assumed and working out details of this might be very interesting.

Z.

gauge fields include both physical and gauge degrees of freedom. In some sense the problem is that there is no clean way to separate them.

39. **anon.**
April 16, 2010

Peter Orland, I don't understand your comment "If you study gauge-invariant functionals (like Wilson loops) and somehow have a calculational scheme avoiding gauge fixing, you can bypass the issue altogether. Unfortunately, no one has such a scheme for QCD." What about the lattice? Or do you just mean a continuum scheme?

40. **anon.**
April 16, 2010

I think the strongest hint that the Gribov problem might be relevant comes from evidence that at least in some gauges, configurations like center vortices that should be relevant for confinement (based on arguments that don't depend on a choice of gauge) are localized on the Gribov horizon. There seems to be something interesting and poorly understood lurking there.

41. **Peter Orland**
April 16, 2010

anon.

I meant a non-numerical (i.e. analytic) scheme, lattice or not.

42. **Z**
April 16, 2010

Thanks for the answer Peter.

43. **Rondeau**
April 16, 2010

"The same is true for the local gauge symmetry: finite-size local gauge transformations are irrelevant to the quantum gauge theory."

In perturbation theory, in principle, only infinitesimal gauge transformations are relevant because finite gauge transformations are just compositions of infinitesimal ones. But globally, not just locally, there are obstructions to that naive approach to gauge transformations.

But even in the perturbative quantum theory there is at least one finite gauge transformation that is crucial: the one connecting unitary gauge (in which the theory is manifestly unitary), to a suitable covariant gauge (in which the theory is manifestly renormalizable). Without that transformation there is no consistent quantum theory.

44. **N. Nakanishi**
April 16, 2010

Peter Woit,

You confuse the existence of the formalism and that of the method of numerical calculation. The former is a fundamental theoretical problem, while the latter is merely a practical one. The existence of the satisfactory formalism is very important as physics even if one cannot solve it. Indeed, the covariant operator formalism of the non-Abelian gauge theory clarified various theoretical problems (e.g., formulation of the condition of color confinement, resolution of the dilemma between the Higgs mechanism and the Goldstone theorem
in the manifestly covariant theory, proof of the charge universality in the electroweak theory, etc.). On the other hand, I think that the lattice theory cannot show even the angular-momentum conservation law. The lattice theory discusses quark confinement but not color confinement.
Anyway, you should read a review of the covariant operator formalism of BRS-quantized gauge theory. If you give me your mailing address, I am willing to send you a copy of my book.

Rondeau,

You confuse the problem at the operator level and that at the representation level. Since the path integral does not discriminate them, the people working only in the path-integral approach often misunderstand this point. The finite-size c-number transformations are not well-defined at the operator level. They are often recovered at the representation level; the physical equivalence (but not unitary equivalence!) of unitary gauge and covariant gauge can be shown after the representation space (i.e.,state-vector space) is introduced.

45. **Peter Orland**
April 16, 2010

OK, against my better judgement, I will jump back in. I am already regreting it.

Mr. Nakanishi, stop telling other people they are confused. It does not make you look knowledegable. It just shows you have an agenda.

So here are the issues:

1) The lattice formalism does not require numerical simulation. It is just a gauge-invariant regularization of gauge theories. I repeat – numerical issues are irrelevant. Go and read Wilson's paper from 1974. He does no simulations in that paper.

2. It is not true that the lattice implies the use of the path integral. There is a Hamiltonian (Kogut-Susskind) lattice formalism where the same issues arise (in solving Gauss' law).

3. Let me say it again – the Gribov problem occurs in the Hamiltonian formalism too.

4. The issue of large gauge transformations has nothing to do with one formalism (path integrals) or another (Hamiltonians). In the Hamiltonian approach, as I said before, it arises in the measure on the space of states. States are singlets under gauge transformations. ALL gauge transformations, not just infinitesimal ones. There is still the issue of large gauge transformations.

This is fundamental to gauge theories and you can't invent any formalism to avoid it. If you say some method removes it, the method is plain wrong (or at least, your interpretation of the method is wrong). For example, there will be no theta vacua in any formalism of gauge theories ignoring large gauge transformations. Different n-vacua are large-gauge transformation-equivalent to a pure gauge. That is how theta vacua arise. Once again, this is just as true in a Hamiltonian as a path-integral formalism

5. Though one does not need to path integral in these arguments, why you dismiss path integrals mystifies me. I suggest you look at the books by Simon, Glimm and Jaffee on the subject. These people actually proved theorems about quantum mechanics and field theory, using path integrals.

6. Now in this forum, I can't prove any of this to you. All I can say that it has been generally known since the seventies. The blogosphere is not a good teaching environment. I am not able to prove it to you without our actually working through the math together (and I am pretty sure you would not be willing to do that anyway). So instead of repeating your pronouncements, you should read and understand some of the literature (not just the literature you are promoting).

6. Last and most important. I don't know if you are still doing research in this field. If you are, listen! I am saying something you should hear, even if you don't want to.

46. **N. Nakanishi**
April 17, 2010

Mr. Orland,

In this forum, discussions should be scientific ones. OK?

1. It is Peter Woit who emphasized the importance of numerical aspect of the lattice theory.

2. & 3. May I understand that what you call Hamiltonian formalism is the formulation in the Schroedinger picture? The Schroedinger picture is good in quantum mechanics (because of von Neumann's theorem), but very bad in relativistic quantum field theory. The Schroedinger picture not only violates manifest covariance but also suffers from the difficulty of vacuum polarization. Even worse is the fact that it is unclear what representation is adopted.

4. The covariant operator formalism of gauge theory is formulated in the Heisenberg picture. It is BRS invariant. It is not invariant under local gauge transformations. Hence the Gribov problem is totally irrelevant to it.
The theta vacuum is a pathological consequence of the path-integral approach. What is physically sensible is the variation of the action but not the action integral itself.

5. I know that path integral was used in the constructive field theory, but the models treated there are such simple ones as scalar field theory. The trouble with path integral arises in more realistic theories such as quantum gauge theory and quantum gravity. The path-integral approach cannot be a complete theory because within its own formalism one cannot prove the unitarity of the physical S-matrix. The hermiticity of the action cannot be defined in it.

6. Non-scientific.

47. **Thomas**
April 17, 2010

> The theta vacuum is a pathological consequence of the path-integral approach.

So there is no strong CP problem, theta vacua are just an illusion?

48. **Anon.**

April 17, 2010

"The covariant operator formalism of gauge theory is formulated in the Heisenberg picture. It is BRS invariant. It is not invariant under local gauge transformations."

Typically, one starts with gauge invariance, and then gets to BRST as a way to handle gauge invariance when quantizing. If you take the gauge-fixed BRST theory as the definition of your theory, then yes, there is no ambiguity. You are essentially truncating your definition of the theory at that level.

As long as non-perturbative effects are hard to test experimentally, your theory might survive, I don't know. Conceptually though, it sounds like you have no notion of a non-perturbative definition. You are just ignoring the unknown. Taking gauge invariance seriously all the way has lead to correct ideas in the past, eg.: Aharonov-Bohm effect, which is intimately tied to monopoles, theta angles and such.

"The theta vacuum is a pathological consequence of the path-integral approach. What is physically sensible is the variation of the action but not the action integral itself."

That cannot be entirely true because we know (experimentally) that tunneling exists in quantum mechanics.

More generally, path integrals are extremely natural from a lot many points of view. Anomalies, renormalization group, lattice gauge theory etc. are all most natural from a path integral viewpoint.

49. **Peter Woit**
April 17, 2010

Nakanishi,

Thanks for the offer of a copy of your book, but I already have bought one, several years ago. It and Henneaux-Teitelboim are the only two books I know that address the issue of treating gauge symmetry via BRST, beyond Feynman diagrams. Henneaux-Teitelboim I think I understand quite well, your book much less so. Henneaux-Teitelboim try and connect the Hamiltonian and path integral formalism, but do so using (like others who try and do this) phase space path integrals that are ill-defined.

Despite some effort, I still haven't completely understood exactly what the assumptions are that go into the covariant operator formalism. I started my career working on lattice gauge theory, so one virtue it has for me is familiarity, but the other is that it is precisely well-defined (independently of its use in numerical calculations). One thing I learned by doing numerical calculations is that setting them up on a computer forces you to make sure you understand very precisely what you are doing, there is no room at all for ambiguity.

The lattice approach to QCD is precisely defined, even if you can't prove things about it. If the covariant operator formalism for QCD is as well-defined, and gives different answers, this would be very interesting. One problem you would have then is that as far as I know, no lattice computation that can be compared to experiment has given anything incompatible with what is observed. If you claim there are no theta-vacua, one thing you have to explain is the success of Witten's argument for the eta-prime mass.

There is conventional wisdom about how the path integral and operator formalisms are supposed to be two ways of treating the same theory, in principle compatible. If someone could sort out carefully what is going on here, that would be very interesting.

50. **Peter Orland**
April 17, 2010

In any case with no theta vacua, the U(1) problem in QCD has no solution.

By the way, there is a formulation of unitarity within path-integral lattice gauge theories. It is called reflection positivity and was first considered in gauge theory by Osterwalder and Seiler.

Whether or not one takes the Kugo-Ojima formulation seriously, I don't believe that it forbids theta-vacua, unitary gauges or Gribov problems. If it does, it is purely perturbative. A formulation which does forbid such things is not going to be unitary, except in perturbation theory. An analogy is to treat the double-well Hamiltonian perturbatively, ignoring tunneling. The result for the spectrum is going to be wrong, unless tunneling (a nonperturbative process) is taken into account.

51. **Peter Orland**
April 17, 2010

Sorry to have prolonged this discussion Peter.

52. **Rondeau**
April 17, 2010

"Witten's argument for the eta-prime mass."

This is probably a stupid question, but wasn't that 't Hooft's argument?

"Sorry to have prolonged this discussion Peter."

I'm sure I'm not the only one who found it interesting.

53. **Peter Woit**
April 17, 2010

Rondeau,

't Hooft showed that the argument that the eta-prime should have low mass is ruined by the anomaly. Witten actually gave a formula for the mass, arguing from the 1/N approximation. I guess Veneziano did this independently, it is known as the Witten-Veneziano mass formula.

54. **N. Nakanishi**
April 18, 2010

Thomas,

That is right. The non-existence of strong CP violating term is confirmed by the non-existence of neutron's electric dipole moment. Axions are also non-existent.

Anon,

The Kugo-Ojima formalism provides us the manifestly covariant formulation of the Standard Theory. It is important to recognize that local gauge transformation is a classical concept, because it contains arbitrary functions, i.e., functions chosen by human being. Quantum field theory is the fundamental theory; it must be independent of human will. The quantum version of local gauge invairance is nothing but BRS invariance. Local gauge invariance is recovered in the sense of gauge independence of the physical S-matrix.

The AB effect establishes experimentally that gauge potential is the fundamental quantity. Monopoles and theta vacua are merely hypothetical things.
Tunneling exists in quantum mechanics. But this does not imply that instanton is physically sensible in quantum field theory. The reasoning used here is no more than analogy. Can you prove the conservation of total probability in the process in which transition via instanton really occurs?

"Natural" is merely your feeling. You must logically show why. Path integral is no more than the generating function of Green functions. Anomalies, renormalization group, lattice theory, etc. can be considered also in the framework of operator formalism, because Green functions can be defined in it.
What I am emphasizing is that it is quite dangerous to make physical considerations based on the path integral beyond the extent justifiable by the operator formalism.

Peter Woit,

Thanks for buying a copy of my book; please read it. The Kugo-Ojima formalism is nothing but a natural extension of the manifestly covariant formalism of abelian gauge theory (QED and Higgs model) to the non-abelian gauge theory. Nothing is conceptually difficult.

I think that the lattice approach is an important tool for QCD. What I stress is that the lattice gauge theory cannot be a fundamental theory because the angular momentum conservation cannot be proved within its own framework (Remember the importance of partial-wave analysis.).
I don't know much about the problem of eta-prime's mass, but it is a matter crucially dependent on approximation method. What I assert is the matter of the fundamental principle of the theory. Field equations and canonical commutation relations are independent of a constant term of the action. This is a completely non-perturbative statement.

Path integral and operator formalism have some delicate differences. If you are interested in this problem, please see the following paper:
M. Abe and N. Nakanishi, Perturbative or Path-Integral Approach versus Operator-Formalism Approach, Prog. Theor. Phys. 102 (1999), 1187.

Peter Orland,

There is no strong CP violation; there are no axions.

To repeat, the Kugo-Ojima formalism is non-perturbative, manifestly covariant, unitary, and totally irrelevant to the Gribov problem.

Tunneling is possible in quantum mechanics, because the representation is unique. In quantum field theory, there are infinitely many representations. It is impossible to tunnel to different representations.

55. **Peter Orland**
April 18, 2010

I never said their were axions and did not comment on strong CP violations. But a theta-term is possible to add to the QCD action.

As I said, if the Kugo-Ojima formulation has the properties you say, then it is wrong (but I don't believe your statement that it does have such properties).

Tunneling is present in field theory (and not just gauge theories); provided the WKB factor is non-zero.

56. **Rondeau**
April 18, 2010

thanks for your answer

57. **N. Nakanishi**
April 18, 2010

Peter Orland,

If you claim that my assertion is wrong, you must point out where and why it is so. It is non-scientific to claim "wrong" merely based on your feeling.

58. **Peter Woit**
April 18, 2010

Nakanishi,

It is difficult to prove things in 4d lattice gauge theory, but you can do computer calculations and check whether what you get satisfies properties you expect. In particular, as you take the coupling to zero, you can check whether the breaking of rotational symmetry introduced by the lattice disappears as expected. My understanding is that this is what happens.

The reference you give to a discussion relating the covariant operator formalism and path integral formalism is for a 2d theory. The divergence structure of 2d QFT is quite special, and, especially in the context of conformal field theories, there is quite a bit known about the operator formalism and its relation to the path integral (this is a beautiful subject mathematically). However, the 4d divergence structure is quite different and much worse. I don't see evidence that you are able to explicitly construct operator algebras and representations with the properties that you expect to have. The advantage of the lattice version of the path integral is that it is an explicit construction, and you can check that it has the properties you want, analytically in a few cases, numerically in others.

59. **Peter Orland**
April 18, 2010

Actually I have no obligation to point that out. It is you who is making claims that the conventional view on gauge theories is wrong, so it is you who has an obligation to prove it.

The very loaded term "non-scientific", in its simplest form means making assertions without evidence. I have evidence that I what I am saying is true. You claim to have other evidence. Only doing the mathematics can say who is right.

My research is in field theory, and I have some confidence in my assertions. I do my best to be careful in what I do. I make mistakes, but after a lot of checking and reworking, I try to fix them. Though I am not perfect, I work hard to understand the issues. If I am sure something is true after reading well-written and convincing papers and my own hard calculations, why should I believe someone who says it is all wrong?

Perhaps I can best express my reaction to your assertions though quoting the following:

http://xkcd.com/675/

60. **N. Nakanishi**
April 19, 2010

Peter Woit,

I am not concerned with the matter of expectation or hope based on a finite number of numerical calculations. What I am saying is that the angular momentum conservation must be exactly established if the theory is fundamental.

Thanks for quick reading of my paper. The subject of this paper is not to discuss the divergence problem but to clarify concretely the pathological aspects of the perturbative or path-integral approach in 2d quantum gravity.

The original topic which you proposed is non-perturbative BRS. I don't understand why you adhere to lattice approach so much. There is no BRS-formulated lattice theory!

Pete Orland,

I have never said that your work is wrong, because I don't know your work at all. Probably, it is better to stop the discussion with you.

61. **Peter Orland**
April 19, 2010

I was not talking about my own publications. But OK, you are right, this
has gone on too long.

62. **D R Lunsford**
April 19, 2010

This topic has been one of the best in the history of this blog. I wish there were more sane discussion on the net.

-drl

63. **mathphys**
April 20, 2010

Dear Nakanishi san and Peter Orland,

Please continue the discussion. I personally found it instructive as I didn't realize there were any controversies in these matters. There must be a way to talk about these things without getting all worked up.

64. **N. Nakanishi**
April 21, 2010

mathphys,

If you are interested in my point-of-view, please read the following paper:
N. Nakanishi, Method for solving quantum field theory in the Heisenberg picture, Prog. Theor. Phys. 111 (2004), 301-337 (Invited paper).

As for Orland's opinion, please ask him.

65. **Peter Orland**
April 22, 2010

D.R. and mathphys

I will not succeed in doing these topics justice, but I will try to give a summary below.

The first person to compute the effects of tunneling in gauge theories was Polyakov, who studied the problem for QED on a Euclidean lattice in 1975 (Physics Letters B), and in the O(3) Yang-Mills theory broken to a U(1) symmetry in 1977 (Nuclear Physics B). The lattice case was studied in the Hamiltonian formulation in

Qed On A Lattice: A Hamiltonian Variational Approach To The Physics Of The Weak Coupling Region.
S.D. Drell, Helen R. Quinn, Benjamin Svetitsky, Marvin Weinstein, (SLAC) . SLAC-PUB-2122, Jun 1978. 64pp.
Published in Phys.Rev.D19:619,1979.

So the path integral is absolutely and most definitely not essential.

Attempts to do full-fledged calculations of tunneling between n-vacua in four-dimensional Yang-Mills (instantons) are highly suggestive, but inconclusive, mainly because classical conformal invariance leads to infrared divergences. Some progress was made in the 2-dimensional O(3) sigma model, but even there the result is problematic (I am actually working on this problem some of the time).

Gribov worked in the continuum, but the Gribov problem is present in the Hamiltonian, as well as the Euclidean lattice formulations. The gauge fixing problem can be thought of as a spin-glass. If you think of minimizing TR A^{2} with respect to gauge transformations, the result is Lorentz gauge (on a 4D lattice) or Coulomb gauge (on a 3D lattice). This problem is finding the minimum (or classical-ground-state) energy of a spin glass, with a non-Abelian spin-space. The gauge field plays the role of the spin-glass frustration. Such problems are well-known not to have a unique minimum.

There is a lot more to say, but I hope this will give you some idea of what the problems are about.

66. **Chris Austin**
April 22, 2010

Hi Peter W,

Thanks for the reply on Peter H's paper.

What specifically were you thinking of, halfway down the Older Comments page, where you said, "The lesson of many years of work on understanding the problem of chirality on the lattice is that it's not a lattice artifact, but something inherent in the use of a cut-off, some version of which is needed to make sense of the QFT"? Is there some other non-lattice kind of short-distance cut-off that causes fermion doubling, quadrupling, etc., in simple treatments?

Best,
Chris

67. **mathphys**
April 22, 2010

Peter O,

Thank you for recalling these classic papers. Great works.

68. **Peter Woit**
April 22, 2010

Chris,

Besides the lattice, I don't know of any simple ways to write down a cut-off gauge theory non-perturbatively.

What I had in mind with that comment is that (take a look at the Neuberger paper about the chiral fermion problem) the anomaly is really a topological problem, one that can be reduced to a problem about how you count states in the spectrum of the Dirac operator. Any kind of naive discretization of the Dirac operator will somehow run into trouble with the anomaly, with doubling one possibility.

Exactly how species multiply to cancel the anomaly depends on what one does. If you do something like Kogut-Susskind fermions, another way of thinking about what you are doing is that you are discretizing differential forms, not spinors, and for these there is no anomaly, and you expect to get spinors with multiplicity given by the dimension of the spinor space. Always seemed to me that what one should look for is some formulation that inherently involves the geometry of spinors, not that of forms, but I've never had a good idea along those lines…

69. **Chris Austin**
April 22, 2010

Thanks Peter

70. **younghun park**
April 24, 2010

Hi Peter
Your book "Not Even Wrong" and your blog have made me
think about the future of physics.
I have read your book several times.
As you point out, Standard Model is the successful theory
which needs to be improved the more.
But I think Anyone doesn't know how to improve this theory.

Maybe, String theory is suggested as the alternative to
Standard Model with the difficult problems.
I feel you believe that String theory is the failed theory.
You and your fans have confidence that String theory can't
be the alternative to SM and that is not the true science.
I agree to your idea.

But, what is alternative to SM?
Is there any good idea to improve SM?
The insight to be ontained from researching for the improved
Gauge theory makes this situation better really?

I want to ask these questions of you and many visitors to this blog.

My thought is that physics can't go ahead any longer
as far as it is confined in the concept "Field".

"Field" is the good concept to explain the electic and magnetic
phenomena. Dirac and high energy physists developed this "Field"
as abstact concept to describe the particle creation and annihilation
in sub atomic world.

In fact, several problems in SM are relevant to Field concept.
The infinity appeared in the caculation is not irrelevant to using the Field.
Lattice gauge theory also be made for escaping from the infinity appeared
in the caculation with using Field.

Is there any concept alternative to Field to describe the High-enery physics?

I feel that theorists must begin to think what is the alternative to Field.

How about my thought?

71. **N. Nakanishi**
April 24, 2010

Younghun park,

The concept of quantum fields is very successful, so it is too hasty to give it up. Learn from history! Newtonian mechanics was very successful, but to formulate quantum mechanics it was essential to rewrite it into analytical dynamics, which was achieved by treating spatial coordinates and conjugate momenta on an equal footing. Likewise, to formulate general relativity it was essential to rewrite special relativity into the four-dimensional tensor analysis, which was achieved by treating space and time on an equal footing.

Now, quantum field theory unified the concept of particles and that of forces. Next, we should proceed to unifying the concept of fields and that of spacetime. That is, I suggest that we should rewrite quantum field theory by treating quantum fields and spacetime on an equal footing, before a possible breakthrough is found.

The symmetry between quantum fields and spacetime was found to hold in the BRS-formulated manifestly covariant formalism of quantum gravity in the de Donder gauge. There is a 16-dimensional supersymmetry IOSp(8;8) on the basis of of the supercoordinates consisting of spacetime coordinates, gravitational B field, FP-ghost and FP-antighost, though, of course, most of the generators of this supersymmetry are spontaneously broken, and the graviton is one of Nambu-Goldstone bosons.

72. **Peter Woit**
April 24, 2010

yonghun park,

This discussion really has nothing to do with the topic of this posting, and I don't want to run a general physics discussion board here.

73. **tbs**
April 24, 2010

Hi,
Firstly, thanks to Peter Woit to write this post. All of them are seriously under-investigations by the field/ theorists. Sometimes making serious research public does help, although many times don't! Anyway, I am not writing this to discuss those social science issues. I just wanted to point out some efforts to obtain a BRST-formulated lattice theory, mainly by evading the Neuberger's 0/0 problem. Firstly it was Testa's paper http://arxiv.org/pdf/hep-lat/9803025 which tried to restore BRST symmetry on the lattice. Meanwhile, Schaden and Baulieu showed that gauge fixing was a Witten-type TQFT http://arxiv.org/pdf/hep-th/9601039. Schaden then went on used this important interpretation to restore BRST symmetry on the lattice using coset space construction http://arxiv.org/pdf/hep-lat/9805020.
There were some other efforts too e.g. http://arxiv.org/pdf/hep-lat/9709154, but Neuberger had counter argumented http://arxiv.org/PS_cache/hep-lat/pdf/9801/9801029v2.pdf . I don't know the current status of this approach.
Using Curcci-Feerrari gauge, in http://arxiv.org/pdf/0807.0480, it was also tried to evade the Neuberger problem.
Very recently, the Neuberger problem seems to be evaded using redefinition of the gauge-fixing on the lattice in http://arxiv.org/pdf/0710.2410 and http://arxiv.org/pdf/0812.2992 (someone has already mentioned these papers above). The current research is to do simulations with this BRST-formulated lattice theory. The Kugo-Ojima theory is indeed appreciated in many of these papers.
It should be noted that relatively 'low no. of citations' to Neuberger's original papers should not be taken as low interest in this problem. But rather as a difficult problem to be addressed.
If you are interested in some debates on relavant issues on these topics, there is a blog http://marcofrasca.wordpress.com/ where the author sometimes do discuss about these topics – though the blog seems to be inactive for the past few months. Note that I am not the blogger of that blog nor actively involved in this particular research nor here to defend/offend any of the above approaches. But closely watching and eager to see the progress.
Hope this helps.
tbs

74. **Marco Frasca**
April 25, 2010

Dear tbs,

Thank you very much for citing my blog as a reference for people working in the field. This blog is inactive for the simple reason that, in my place of work, has been installed Websense and a lot of sites that could be helpful for this activity are no more accessible. E.g. I can read this blog but not the one from Backreaction. Last but not least, I cannot see pictures, photos and formulas making the management really involved and restricted in a small range of time in the day that is generally not available for other reasons. I am able to put down these lines from my home computer.

I think that this is not the right place to cite my blog for the simple reason that me and Peter have had some questioning last year about my work. A couple of formulas of mine appeared in Wikipedia and Peter intervened to get them removed. This produced some fuss here and and in the blogosphere concluded with an intervention by Terry Tao. Curiously enough this was beneficial to my work and, now, I should say that Peter helped me to make things work.

Marco

75. **N. Nakanishi**
April 25, 2010

tbs,

I think that Neuberger's 0/0 problem is simply caused by his erroneous assumption. There is no reason for assuming that the BRS-invariant measure is a trivial one with respect to the
FP-ghost and antighost variables, that is, it may contain c-bar c terms.

Of course, the existence of the BRS-invariant measure is merely an assumption. In exactly solvable models, we often encounter the BRS anomaly in the path-integral approach, but not in the operator-formalism approach.

Tevatron vs. LHC

April 20, 2010
Categories: Experimental HEP News

The news from Fermilab is that the Tevatron has set a new luminosity record, with a store last Friday that had an initial luminosity of 4.04 x $10^{32}cm^{-2}s^{-1}$, or, equivalently, 404 inverse microbarns/sec. For more about this, see a new posting from Tommaso Dorigo, where someone from Fermilab writes in to comment that they're not quite sure why this store went unusually well.

Over in Geneva, commissioning of the LHC continues. There, the highest initial luminosity reported by ATLAS is 2.3 x $10^{27}cm^{-2}s^{-1}$, or 200,000 times less than the Tevatron number. I haven't seen a recent number for the total luminosity delivered by the LHC to the experiments so far, but I believe it's a small number of hundreds of inverse microbarns. The Tevatron is producing more collisions in one second than the LHC has managed in the three weeks since first collisions.

The current plan for the LHC is to devote most of the rest of 2010 to increasing the luminosity of the machine, with a goal of reaching something somewhat lower than the Tevatron luminosity (1-2 x $10^{32}cm^{-2}s^{-1}$) by the end of the year. Then the plan is to run flat out at this luminosity throughout 2011, accumulating data at the rate of about 100 pb^{-1}/month, and ending up with a total of 1 fb^{-1}. The hope is that this will allow them to be competitive for some measurements with Fermilab, the lower luminosity compensated by the advantage of a factor of 3.5 in beam energy that they now enjoy.

The Tevatron has already produced over 8 fb^{-1} of data, and the current plan is to run the machine through the end of FY 2011, reaching at least 10 fb^{-1}, and then shut it down for good. The LHC is supposed to go into a long shutdown throughout 2012, not coming back into operation until 2013. Even if all goes well, it likely will not have accumulated enough data to decisively compete with the Tevatron until late 2013 or 2014. Under the circumstances, it's hard to believe that there aren't plans being proposed at Fermilab to keep the Tevatron running for several more years, until 2014. The machine should then be able to end up with a total of 15-20 fb^{-1} worth of data, which could be enough to allow them to see evidence of the Higgs at the 3 sigma level over the entire possible mass range.

Shutting down the Tevatron as planned would free up funds for other experiments, and free parts of the accelerator complex for use by neutrino experiments. It will be interesting to see whether instead of this, the decision gets made to go for a bid to outrace the LHC to the Higgs and other high energy frontier physics over the next few years.

Update: The integrated luminosity seen by the ATLAS detector for the current run so far is about 400 inverse microbarns, 350 at CMS (where they have 300 worth of recorded data).

Update: This morning the LHC has started delivering collisions with stable squeezed beams to the experiments, initial luminosity 1.1-1.2 x$10^{28}cm^{-2}s^{-1}$.

Update: Integrated luminosity delivered to each experiment at the LHC is now up to around 1000 inverse microbarns.

Comments

1. **Verified Armonyous**
   April 20, 2010

   Do you think the comparisons of instant luminosity you make are in any way significant?
   I think you can make your point on the need to run Tevatron longer (with which I fully agree) without trashing the LHC.

2. **Peter Woit**
   April 20, 2010

   I'm not in any way trashing the LHC, which is undeniably the long-term future of the field and is moving forwards towards fulfilling that role. What I'm doing is trying to put out the best numbers available about the current situation and what can be expected from the two machines over the next few years as well as noting the obvious questions these numbers raise.

3. **milkshake**
   April 20, 2010

   It is worth to point out that Tevatron is not obsolete yet, and it is not unfair to make a direct comparison because LHC promise of delivering breakthrough results soon has been over-publicized and meanwhile Fermilab has had hard time with funding…

4. **luminosity**
   April 20, 2010

   "The Tevatron is producing more collisions in one second than the LHC has managed in the three weeks since first collisions."

   LEP ran from 1989-2000. One good weekend in 1994 produced as much integrated luminosity as the whole of the 1989 run. See the chart at bottom of page for integrated luminosity at DELPHI for LEP-I ("Z0 factory")
   http://hepunx.rl.ac.uk/~adye/thesis/html/node9.html

   See also here for LEP-II integrated luminosity 1994-2000
   http://accelconf.web.cern.ch/Accelconf/e00/PAPERS/TUOBF101.pdf

   It is a matter of history that 70% of the integrated luminosity of LEP was delivered during its last three years of operation. All of this merely underscores the *enormous* technical challenge of commissioning and operating a (large) new accelerator. The Tevatron itself ran for years with instant luminosities of approx 1E30. So LHC delivers 2.3E27 now? Perhaps the right viewpoint is that 2.3E27 is better than zero.

5. **Paul Wells**
   April 21, 2010

   From a global perspective I think it would be more efficient to shut down Tevatron now.

   LHC will find the Higgs eventually if it exists.

   This reminds me of SLC and LEP competing to find the number of neutrinos from the width of the Z.

Why not shift resources now to neutrino work ?

Is this just a matter of ego ?

6. **eff**
April 21, 2010

Who is to define "efficiency" ?
SLC and LEP — again a too-narrow focus on HEP expt (# neutrinos and Z width) and neglect of accelerator technology. SLC also pioneered a new acceleator principle/technology, that of a linear collider. These are not "turnkey" operations that one can buy off the shelf nor can they be designed "on paper" with computer simulations.

7. **Peter Woit**
April 21, 2010

Paul,

By the same argument, any piece of scientific apparatus should be shut down as soon as there's a viable plan for something better. That's not the way science is done, for good reason. It's not because of ego, but because actually getting results is important, so you don't just stop before you have them when you think someone else will have a better shot in the future.

In this case, no one knows how long it will actually take the LHC to reach the point of producing data that makes the Tevatron irrelevant. The history so far is that things have taken significantly longer than planned.

In addition, for some sorts of measurements, higher center of mass energy is not so crucial. For example, for a low mass (115 GeV) Higgs, my understanding is that the higher energy of the LHC doesn't really help.

It's also true that having multiple confirming observations of something is quite valuable. If a Higgs is seen, it's going to be a difficult signal, and seeing it at four different experiments will help confirm it is really there.

The argument for shutting down the Tevatron is not the LHC, but that once it has collected a certain amount of data, it's not worth spending a lot more money to collect only marginally more data.

8. **Dr.Kathrine Martinez-Martignoni**
April 21, 2010

Peter,
do you think that americans feel themselves a little bit "frustrated"
to constate that LHC has already surclassed Tevatron in the mass
media propaganda?
Dr.Kathrine M.

9. **Peter Woit**
April 21, 2010

Katherine,

I don't think this is a nationalistic issue. Among Americans, most non-physicists don't know or care about the question, and most physicists are quite excited by the fact that the LHC is finally getting into operation and very much looking forward to what it will find.

Tevatron vs. LHC is not exactly a hostile competition, since the experimentalists involved on both sides are often the same people (many work on both the Tevatron and LHC detectors). It's also not much of a nationalistic competition, since many European physicists work on the Tevatron experiments, many Americans on the LHC experiments.

10. **Verified Armonyous**
April 23, 2010

There you have it. Now people have started quoting your unfair comparison, akin to comparing your reading skills with those of a three month baby.

11. **nessuno**
April 23, 2010

Armonyous,
I don't think the post is an "unfair comparison"; rather, it is a representation of the real current situation. By the way, I found a similar post some days ago (http://blog.vixra.org/2010/04/10/lhc-needs-more-luminosity/). But if you want to compare, Tevatron is like an old 8 tons truck running at its best and delivering stuff to the customers. LHC is like a brand new 40 tons truck, but only tested with a 4 grams load. And the plan to increase the load is, for the time being, unpredictable. So, if the customers think it is still useful to get some stuff within a predictable delay, they should keep the old truck running. Only when the new one delivers what it has promised, the old truck can be dismissed. After all SLC was not stopped before LEP produced decent luminosity.

12. **Verified Armonyous**
April 23, 2010

Sure, I'm fully convinced and in favor of keeping Tevatron running for much longer. Still I believe the wording of the comparison was not very fortunate and was certainly unnecessary to make the point. Only a theoretician could speak like that.

13. **th**
April 23, 2010

A true blue theoretician wouldn't mention the LHC or Tevatron at all. Everything could be deduced from pure Platonic intellectualism (like strings?). I am much impressed by PW's diligent efforts to present up-to-date reports on the progress/status of LHC and Tevatron (also RHIC, as with the private donation of funds by Simons to keep the machine running in 2006).
http://www.math.columbia.edu/~woit/wordpress/?p=328

Unfair comparison? Not at all!

Kudos to PW!

14. **Coin**
April 23, 2010

Could eventually it be possible to do a single statistics analysis / bump hunt using merged data from the LHC and Tevatron?

15. **Peter Woit**
April 23, 2010

Coin,

They run at different energies, and one is a proton-proton, the other a proton-antiproton machine. So, doesn't really make sense to combine the data.

If they do both end up with some sort of marginally significant Higgs signal, there probably is some sort of statistical analysis that would quantify the improved significance of the Higgs signal taking into account the two analyses.

16. **Coin**
April 23, 2010

Interesting, thanks.

17. **Bill K**
April 23, 2010

one is a proton-proton, the other a proton-antiproton machine.

This has always left me scratching my head, for the following reason.

1) Suppose that back in 1975 or whenever, someone had said to you, "Hey, let's build a proton-antiproton collider at Fermilab." Wouldn't your reaction have been, "No, of course not, that'd be like shooting yourself in the foot. Just think how difficult it would be to generate the antiprotons and collimate them into a usable beam. You'd never be able to produce enough luminosity that way." And yet the Tevatron does so, admirably.

2) Given that success, suppose that back in 1985 or whenever, someone had said to you, "Hey, let's build a collider at CERN and make it a proton-proton machine." Wouldn't your reaction have been, "No, of course not, that would just complicate the magnet design. Look what a great job the Tevatron does using antiprotons."

3) Now suppose in 2010 someone says to you, "Hey, let's build an even larger machine." Given that both the Tevatron and the LHC are now remarkably successful — of the two designs p-p or p-anti p (never mind the other options!) do you see either of them as having a real advantage, and if so which one and why?

18. **Remus**
April 23, 2010

In the case of the LHC, I guess it *had* to be proton-proton because the same accelerator was intended to accelerate, and collide, heavier nuclei as well.

19. **Ralph**
April 23, 2010

Well, LHC just got to 1.2e28 🙂 Still a way to go to 1e34…

Of course, if Tevatron decides to make it a race, LHC might decide to play along and put off the 2012 shutdown; even one year could well be enough to increase the luminosity by a factor of ten at this stage of the LHC…

[LHC needs to do its repairs before it gets too irradiated though – I wonder if that is a constraint?]

20. **Peter Woit**
April 23, 2010

Bill K,

My understanding is that the reason the LHC is a proton-proton machine is that, as you go to higher energies, you want higher luminosity since cross-sections of interesting processes are falling off with energy. So the LHC design luminosity is 10^34 cm^-2s^-1, and there's no way to get to this kind of luminosity with anti-protons. The Tevatron luminosity is very much limited by the ability of the accelerator complex to accumulate and store anti-protons in a beam.

It looks like, as Ralph mentions, this evening they've had significant success, producing stable colliding squeezed beams of higher intensity, so getting a significant increase in luminosity.

21. **pbar**
April 24, 2010

Bill K ~ "1) Suppose that back in 1975 or whenever, someone had said to you, "Hey, let's build a proton-antiproton collider at Fermilab." Wouldn't your reaction have been, "No, of course not, that'd be like shooting yourself in the foot. Just think how difficult it would be to generate the antiprotons and collimate them into a usable beam. You'd never be able to produce enough luminosity that way." And yet the Tevatron does so, admirably."

We need a serious history lesson here! The idea of colliding proton-antiproton beams WAS FIRST PROPOSED AT FERMILAB IN 1976, using stochastic cooling to increase the phase-space density of the pbar beam to a useable level (i.e. sufficient luminosity in collisions), and the Fermilab management KICKED THEM OUT.

The proponents of p-pbar colliding beams were Carlo Rubbia, David Cline and Peter McIntyre. See for example Peter McIntyre's home page
http://faculty.physics.tamu.edu/mcintyre/

"In 1976 Prof. McIntyre was the first to propose the possibility of making colliding beams of protons and antiprotons using the large synchrotrons at Fermilab and at CERN. This work led to the discovery of the weak bosons at CERN in 1982."

McIntyre says "Fermilab and CERN" but is was Fermilab that they turned to first. The Fermilab people openly laughed and told them that stochastic cooliing violated Liouville's theorem, etc., much as Bill K says above.

So Rubbia and Cline went to CERN, which was hungry for a Nobel Prize, and CERN was willing to try the stochastic cooling idea (which idea had been invented by Simon van der Meer who was a CERN engineer in the first place). CERN converted the SPS into the SppbarS, it worked and produced the W and Z and led to the 1984 Nobel Physics Prize for Rubbia and van der Meer.
http://nobelprize.org/nobel_prizes/physics/laureates/1984/

All of this is well-documented. A book which describes the history of these events is "Nobel Dreams" by Gary Taubes
http://www.amazon.com/Nobel-Dreams-Deceit-Ultimate-Experiment/dp/1556151128

The Tevatron did not yet exist in 1976. It would have been necessary to convert the Fermilab Main Ring synchrotron into a p-pbar collider (as CERN did with the SPS). Pbar beams were first circulated at Fermilab in 1985, and the Tevatron became operational around then.

Why were Rubbia etc kicked out by the Fermilab management? At least part of the reason is that Robert Wilson, who was then the director of Fermilab, had devoted significant resources to Rubbia's expt at FNAL, leading to the high-y anomaly (go look THAT up on your own!) which was later debunked by CERN. This followed on from the "alternating neutral currents" fiasco, also perpetrated by Rubbia at his expt at FNAL (go look THAT up on your own, too!). Rubbia had a history of bogus grand claims leading to embarrassment at FNAL. The high-y anomaly business broke in 1977. When in 1976 Rubbia came up with yet another hair-brained (hare-brained?) scheme — it was obviously a very speculative and difficult idea — and was obviously no longer focusing on high-y, to which Wilson had devoted significant resources, Wilson realized that Rubbia was again off on another tangent. Robert Wilson lost his temper with Rubbia and kicked him out. It is a matter of history and irony that Robert Wilson, despite his many talents, gave priority to the high-y anomaly (which was a false phenomenon), and rejected the pbar stochastic cooling idea (which led to a Nobel Prize).

The SSC was proposed as a proton-proton collider for precisely the same reason that the LHC is a proton-proton collider, exactly as PW says — it is difficult to produce antiprotons in sufficient quantity (and phase-space density) to attain adequate luminosity. The disadvantage of a proton-proton collider (RHIC, LHC) is that one needs two rings instead of one. This doubles (approximately) the cost of the final ring in the accelerator complex — LEP was a single ring — but it simplifies the beam production, storage and acceleration process. So there are trade-offs.

But never doubt that the idea of colliding proton-antiproton beams was first proposed at Fermilab, which famously rejected the idea. Wilson resigned as director of FNAL around 1977, and Leon Lederman became the director. There was the famous "Armistice Day" meeting at Fermilab in Nov 1977, where everyone was invited to speak openly, and Lederman decided as a result that FNAL would not attempt to compete/race with CERN to make and collide pbar beams. It is a tribute to Robert Wilson's foresight that he left enough space in the Fermilab Main Ring tunnel to build a second ring — he visualized the two rings as a p-p collider — but the Tevatron instead became a single-ring p-pbar collider. But Wilson also kicked out Rubbia for proposing stochastic cooling and p-pbar collisions instead of focusing on the high-y anomaly.

Bill K #2 — "Given that success, suppose that back in 1985 or whenever, someone had said to you, "Hey, let's build a collider at CERN and make it a proton-proton machine." Wouldn't your reaction have been, "No, of course not, that would just complicate the magnet design. Look what a great job the Tevatron does using antiprotons.""

Understand first the history of the Tevatron. Understand the history of the idea of colliding p-pbar beams.

Rubbia and the bogus grand claims – after discovering the W and Z, the UA1 collaboration went on to discover supersymmetry (monojets) and also the top quark (44 GeV). Remember those?

CERN punished Rubbia for these transgressions by making him the DG. Bogosity pays!

22. **a quantum diaries surivivor**
April 24, 2010

Hi all,

although I do not share pbar's rather negative view of Carlo Rubbia (who after indeed several fiascos and some dangerous politicking to steer the labs into financing his endeavours, hit on the right idea and over the course of a winter became the world's expert of antiprotons, becoming the engine that brought the SppS to existence) his reconstruction is rather accurate.

Regardless of neutral currents, y anomalies, monojets and top quarks, every ounce of Rubbia's Nobel prize for showing the world that W and Z bosons were there is well-earned.

Cheers,
T.

23. **pbar**
April 24, 2010

My intent was really the history of p-pbar collisions and Fermilab, and Rubbia got more mention than I intended.

BTW "surivivor" –> survivor (unless it's deliberate?)

24. **P**
April 25, 2010

Thanks for a very interesting history lesson.

25. **Paul Wells**
April 28, 2010

Peter,

Do you have a reference to what the LHC luminosity optimizations actually are and what future optimizations are planned ?

Naively one would think a proton proton collider should have a huge advantage over a antiproton proton collider since protons are easier to make than anti-protons…

Thanks
Paul

26. **Peter Woit**
April 28, 2010

Paul,

There are lots of sources of up-to-date info online at CERN about plans for what needs to be done to increase the LHC luminosity towards the ultimately planned design luminosity. I'm not linking to them since there's an unfortunate history of CERN shutting off access to such information sources after seeing links from blogs.

The main issue for luminosity increases this year at the LHC is the machine protection system. They're starting to enter into a regime of "unsafe beams", where a loss of control of the beam could lead to very serious damage to the machine. Because of this they plan to be very careful and go about this slowly, being sure they completely understand and control the behavior of the machine at one luminosity level before moving to the next.

Podcasts

April 26, 2010
Categories: Uncategorized

In case you're tired of reading me going on about the same topics and instead would like to listen to me going on about such topics, there are now two new options:

- A couple weeks ago I did a podcast with the folks at the Rationally Speaking web-site, talking to Massimo Pigliucci and Julia Galef (their podcast site is here, direct link to my segment here). In the near future they'll be doing a different podcast on the topic of the Anthropic Principle. Pigliucci is a philosopher of science and comments on this here.
- Last month I visited Collin College in Texas, and they have a podcast up from an interview I did there. The site is here, link to interview here.

Comments

1. **milkshake**
   April 26, 2010

   I liked the podcast from Collin College better – the interviewer must have had the structure of the interview planed out in advance, she kept the questions short and to the point and she was not plugging her own observations. One of signs of professional interviewer is that she/he is patient and leaves most of air in the room for the guest.

   I had impression that it was harder to get your points across clearly in the Rationally Speaking conversation. Debates can be quite messy to follow when the subject is philosophy and there are several participants some of which are more eager to listen to themselves than to others.

2. **Peter Woit**
   April 26, 2010

   milkshake,

   They're just different. The Collin College one was basically just an interview, with questions discussed in advance, so I could just make points I wanted to make that I thought would be accessible to as many people as possible. The Rationally Speaking one was intentionally more of a conversation, and I think got into some more of the subtleties of the issues. It was recorded after we had dinner together and talked quite a bit, so ended up, for better or worse, being somewhat of a continuation of that conversation.

   Because some of these debates about string theory end up getting into significant questions about the philosophy of science, I'm always interested to hear what professional philosophers of science think about the debate. In this case, Pigliucci is a philosopher who has thought a great deal about these issues, and I was enjoying discussing them with him, not just putting out my own point of view.

3. **milkshake**
   April 26, 2010

   The Rationally Speaking debate felt a bit like split-screen pundits on CNN competing for limited air time, a style which made it harder for anyone involved in that debate to make their basic points across in a concise way, and then to elaborate them in more detail before getting cut off. It was somewhat ineffective format not because the ideas were discussed in more depth but because not everyone involved seemed willing to stop and listen what the others had to say, then formulate a good reply.

   By the way, philosophy of science is not such a inaccessible field, it has to do with practical usefulness of an idea and the scientific integrity, these are concepts that anyone can understand.

4. **Joe Joe Bob**
   April 28, 2010

   I prefer to read.

   Get to work, slacker. Hunt up interesting information and make more of a contribution to the market place of ideas, understandable or otherwise, provacative or mundane, original or mere reporting. With a kickback job like yours, having hours to ponder the deep and mysterious in your ivory tower office, surrounded by your yellow library of Springer textbooks and monographs, there's no excuse for you turning into a slacker.

Dark Matters

April 28, 2010
Categories: Uncategorized

Initial 2010 data from the Theoretical Particle Physics Jobs Rumor Mill indicates that the particle theory job market remains as trend-driven as ever. This year, it seems that if you want a tenure-track job in the US, you must be working on phenomenology. And not just any sort of phenomenology, your work has to be about dark matter. Of the seven theorists offered tenure track jobs so far, no less than 6 are phenomenologists working on dark matter. The seventh is Davide Gaiotto, who has been working with Witten and others at the IAS on mathematically quite interesting topics that use N=2 and N=4 supersymmetric gauge theory. His offer is from Stony Brook, where much of the funding comes from Jim Simons of Renaissance Technologies. Simons is putting profits from the world's most successful hedge fund to work keeping alive the idea that the intersection of mathematics and physics is still worth pursuing, so not everyone has to become a dark matter phenomenologist.

(By the way, the rumor mill seems to indicate that Kachru and Silverstein are leaving the KITP, heading back to Stanford. Is that right?)

If you're a young theorist who wants to remain in the field, you better get to work on dark matter phenomenology. I'm afraid that this blog won't be of much help, you should carefully follow Resonaances, which has the latest news and rumors.

Update: It seems that my point about the dominance of dark matter hiring has even more backing than I thought, since Sergei Dubovsky evidently has an offer from Stony Brook (and other places). So, that makes it seven out of eight for dark matter so far this year.

Comments

1. **M**
   April 28, 2010

   Woit writes:
   "If you're a young theorist who wants to remain in the field, you better get to work on dark matter phenomenology."

   That makes no sense. If you want to remain in the field then stay in the field. If you want to do phenomenology, then you go ahead and work on dark matter and other venues.

2. **graviton383**
   April 28, 2010

   Shamit & Eva will be back in Stanford-land this Fall.

3. **Avidan**
   April 29, 2010

   M, what you say makes no sense:

   How do you suggest non-dark-matter people will stay in the field (which I define as academic high energy theory) if they have no job ? Fill out a lottery card ? get a day job as a software engineer ?

   You know, physicists also usually eat, pay bills, and even (maybe to some extent rarely) have families…

4. **Rien**
   April 29, 2010

   Of course, it could be because dark matter is where there is something interesting going on at the moment, what with recent and coming data. There is only so much you can do while waiting for LHC.

   Some people, as weird as it may sound, still think physics is related to experiments.

5. **Peter Woit**
   April 29, 2010

   Rien,

   I won't argue against the idea that dark matter is a hot topic for good reason. This doesn't mean it's a good idea for US physics departments to only hire people working on the latest, hottest topic. They've been doing this now for a very long time, and this has a lot to do with the sad current state of the field.

6. **M**
   April 29, 2010

   What I mean is that you should follow what you like, not what is trendy. Your research topic should not be dictated by what is popular. If you have gained the basic knowledge in physics to understand the current problems then you ought to be able to come up with a few ways to tackle at least one of them. Then you try to solve the problem and in the end you should learn something: why your thing worked or why it did not. These problems do not live in the academia. They are free for anyone to try.

   If you want to get an academic job, then you have to play the game and do cartwheels. That game changes every year / decade / generation / etc. The situation is nasty; the lottery ticket summarizes it well. Nothing is fair in life. But if you want to understand something you actually like, surely you do not need to be in the academia. Just follow the rules of the scientific method and you should be able to stay away from crackpotery.

7. **Rien**
   April 29, 2010

   Peter, of course I agree that this trend-following is not good. I believe one should try to hire smart people foremost. But actually, many of the people on the list are not working on only dark matter but phenomenology in general.

8. **Theorist**
   April 30, 2010

Peter, have you considered that you might be confusing causation and correlation? Is it possible that the people hired are simply the best candidates and the reason that dark matter is a hot topic is (in part) because the best young people are working on it?

I know part of the theme of this blog is to expose the shadowy conspiracy that is high energy theory, but it is possible that the dark overlords don't control every hire.

9. **Peter Woit**
April 30, 2010

Theorist,

I didn't say anything about dark overlords or any conspiracy, I'm well aware of how the hiring systems in both physics and math work. What's happening is a lot of people with the same point of view independently making the same decision about priorities. And sure, the decisions of the best young people about what to work on and of the older people doing the hiring are both part of the picture.

What I was pointing out is the end-result of the whole process: virtually everyone working on the same topic. I think there's a good case to be made that this end-result is not a healthy one, that people should be acknowledging this and thinking about what can be done about it. I take it that your conclusion is that nothing's wrong, there's no reason to be concerned about this kind of pattern, it's just the result of everyone making the best possible decisions. Others may disagree…

10. **Anonymouse**
April 30, 2010

However, Rien's point is valid — if you look at the phenomenologists you classify as 'working on dark matter', I would have to concede that every one of them has a paper on the subject. However, less than half of them have worked for, say, 50% of their career (measured in publications) on the subject, and several of them not in the past year or two. What makes for a dark matter theorist? Or even virtually everyone working on the same topic? I also find the faddishness unfortunate, but I'm not seeing more than a vague trend here.

I think it is fair to say that phenomenology remains more attractive to search committees than more formal work, as has been true for a few years now. Dark matter is an important component of phenomenology in general. Particularly given the slow start-up of the LHC. Wanting a general phenomenologist is not being faddish, it's being smart in the sense that someone with a track record of being able to follow interesting results is, long term, a much better investment.

Also let me point out that at least three of the jobs listed on the HEP theory rumor mill (Carnegie Mellon, Case Western, and Oregon) state they are looking for someone specifically with interests in cosmology. That pre-selects them for dark matter and away from HEP theory. It may be that this just shows they were more organized about following a fad than other places, but I doubt it.

11. **Peter Shor**
May 1, 2010

Maybe the question should be not: why do Physics departments hire only people working on the latest trendy fad but: why do Math departments avoid hiring based on the latest fads? From what I've seen of other disciplines, in many of them hiring is significantly more tilted to hot areas than it is in Math departments. There may be something in the mathematics culture that discourages this.

Of course, from a personal perspective I probably should be asking: How can we make quantum computing the hot topics in Physics departments?

12. **Peter Woit**
May 1, 2010

Peter Shor,

Yes, mathematics does seem to avoid excess faddishness, it would be a very good idea for particle theorists to think about why this is and whether they could learn anything from it. My own speculative ideas about the sources of the difference are:

1. The ratio of people to jobs is better in math. In particle theory there are more people with excellent credentials then jobs, so hiring committees can add the criterion of working in what they see as the most promising area. In math if you tried to add this criterion, you'd typically end up not being able to hire the best people.

2. For 40 years 1. has been true, so the permanent faculty now consists completely of people chosen using the criterion of having made their career by working on the latest hot topic. Not unreasonably, they take the attitude that the best people are the ones who work on the hot topic, those not doing so are those that just can't cut it.

3. Particle theory now has a well-developed ideology about what speculative ideas (supersymmetry, extra dimensions, GUTs) are promising, limiting the attention of those in the field to a relatively narrow range of ideas.

4. Mathematicians have a culture which values expertise, and the idea that really understanding a particular subject is very difficult and requires many years of work. When a subject all of a sudden becomes hotter, say after Wiles or Perelman's work, you see some more activity in it, but people from other fields don't jump into the now hot one, since they don't believe they can do it successfully, and others would tend to not take them seriously. Particle theorists often have a much more arrogant attitude, convinced that they can jump into something they don't know about and a couple months later be writing useful papers on the subject. Witten can do this, lots of other people think they can….

13. **Peter Shor**
May 2, 2010

Peter Woit,

In addition to your points, there may be another factor in play. In mathematics, you can expand the boundaries of mathematics in many directions, all of which are potentially worthwhile. In physics, there's only one "right" answer, so some directions of exploration are preordained to be blind alleys. This can be used to justify the attention to hot topics; historically, at any time there have only been a few directions of research which led to the next step in discovering the laws of nature. (Of course, these weren't always the "hot" topics at the time, but with hindsight this is easily forgotten.)

14. **theorist2**
May 2, 2010

I think Peter Shor makes an important point. However close the ties between physics and mathematics are, they are fundamentally different disciplines. Research in each field is carried out in very different manners. Given that there is only one universe, unless we digress into multiverse discussions, there is only one "right" answer. Whether a theorist's best guess is correct or not can only be judged by experiment. While some ideas are more promising and more mathematically compelling than others, which in turn creates trends in high energy theory, if in the end of the day the ideas are falsified by experiment then they are ultimately wrong. Therefore, it might not be wise to take clues from Mathematics departments as far as hiring practices are concerned.

It would be hard to compare the "faddishness" of dark matter or phenomenology hires to string theory or "formal" theory hires. What are the most interesting experimental results, or those ideas which have the most interesting experimental consequences, will presumably always drive the field to a large part as it always has. That is of course when there are interesting experimental prospects or results (outside of those times who knows what the best guide should be). A number of questions could be relevant: Does HET do worse as far as results go in the long term compared to other areas of theoretical physics? Is hiring in HET that different than in other fields of theoretical physics? How different are hiring practices in more related disciplines like Biology and Chemistry(rather than Math)?

15. **Peter Woit**
May 3, 2010

Peter Shor,

It's true that this kind of physics is different in that ultimately there will be just one correct better theory. But, it's quite likely that there are multiple possible ways to get from here to there, and the current situation is that we don't know at all what the right direction to go is. In such a situation, focusing on one particular highly speculative direction and making anyone who works on anything else jobless is probably not a good idea.

One reason particle theory is so faddish is that (in the distant past…) fads were driven by experimental results. When a ground-breaking unexpected experimental result arrives, there's a very good reason to focus on it and what it means. Unfortunately, I think what is going on now is experimentally-driven faddishness with no significant experimental results. If someone actually had a dark matter signal, that would justify what is going on, but there is no solid evidence for such a signal out there right now, and the idea that it is just around the corner remains quite speculative.

16. **Remus**
May 3, 2010

> historically, at any time there have only been a few directions of research which led to the next step in discovering the laws of nature. (Of course, these weren't always the "hot" topics at the time, but with hindsight this is easily forgotten.)

Maybe to easily… One can turn the question around, and ask how many laws of nature were found that way.

General Relativity? QED? QCD? Electroweak Theory? Antimatter?
Renormalizability of the SM? Asymptotic freedom?

As far as I know, none of them was a hot topic immediately before they were discovered. They became a hot topic afterwards, and no further natural laws were found in those directions.

17. **Peter Shor**
May 3, 2010

Peter Woit,

My post seems to have been misinterpreted. I didn't mean to say faddishness was a good idea. What I meant to say is that there are reasons why over-confident physicists who think they know where the next breakthrough is coming from think it's a good idea. Remus understood exactly what I was trying to say.

18. **Verified Armonyous**
May 5, 2010

Why do you count Dubovsky for DM ?

19. **Peter Woit**
May 5, 2010

Because:

1. That's how my source, who was very well-informed about Dubovsky and his work, counted him.

2. Of his last 5 papers, only one isn't about phenomena that involve dark matter candidates.

20. **Verified Armonyous**
May 6, 2010

I would say exactly the opposite, that only one among his last 5 papers deals directly with DM.

21. **Anonymouse**
May 6, 2010

Here are my take on the stats, listing the person with the offer on the rumor mill, number of papers about dark matter, total number of papers, and a paper about dark matter in the last 12 months (yes or no):

Chang: 6 dark matter, 22 total, Almost 100% DM this year

Dubovsky: 6 dark matter, 52 total, Yes to DM this year

Gaiotto: 0 dark matter, 46 total, No DM this year

Graham: 4 dark matter, 14 total, Yes to DM this year

Kong: 4 dark matter, 32 total, Yes to DM this year

Papucci: 4 dark matter, 22 total, yes to DM this year

Schuster/Toro: 4 dark matter, 13 total, yes to DM this year

Toro's papers are complete subset of Schuster's (Schuster has 4 more, 2 of which are dark matter), so I listed them together. Also, it amused me to do so.

I was strict in choosing papers about dark matter, meaning I did not accept work on other aspects of cosmology as a dark matter paper. In a couple cases I also avoided articles that had dark matter in the title but were clearly about collider physics (but only a couple like that). You can quibble about what was meant by various comments made above, but given the over-all discussion I believe that this is fair. I did not separate out published from unpublished, which does represent an over all bias of some kind.

I draw a very different conclusion from these data: forget working on dark matter, just get Arkani-Hamed or Dimopoulos to support you. Particularly if your CV is on the weaker side. I understand that numbers do not capture impact or any number of other important metrics by which research should be judged, but I feel I know the individuals well enough to judge them that none of them clearly deviates by huge amount in terms of impact that would substantially change the picture the numbers is painting. The two who impress me the most as scientists actually have the largest number of publications independently of that fact.

To reiterate: I don't see a huge jump on a dark matter fad this year. Dark matter is interesting, and there is data available, questionable in interpretation though it may be. A large number of phenomenologists worked on dark matter in the last two years (amny of whom already had faculty positions, and still more who did not receive one), and I believe this is just intelligent people following interesting results.

22. **A Phenomenologist**
May 6, 2010

The phenomenologists who got jobs this year are all highly deserving candidates and their work extends way beyond "Dark Matter". It is extremely silly to label these people as "dark matter phenomenologists". In particular, Peter Woit's counting of Dubovsky's last five papers and labeling four of them as Dark Matter papers is just plain asinine. Perhaps's Peter Woit's illustrious publication record of five papers in the last 20 years makes it difficult for him to count papers and understand their content.

It is high time that young people who are either guided by data, or whose work leads to new experimental probes of physics are rewarded with faculty positions. In fact, I find it very odd that people whose work explicitly deals with theories that cannot possibly describe the world or involve dualities between worlds that are also not our own world continue getting jobs. They ought to find themselves positions in a dual world.

23. **Peter Woit**
May 6, 2010

"A Phenomenologist",

You seem to be rather upset that even one out of eight of these jobs is going to a non-phenomenologist.

It's interesting to see that now that phenomenologists have vanquished their enemies and successfully won the political fight in HEP theory, at least some of them have adopted the arrogant and juvenile attitude that in the past so endeared many string theorists to their peers.

24. **noname**
May 9, 2010

I'm surprised that you haven't corrected your wrong statement about Dubovsky.

25. **Anonymouse**
May 9, 2010

I guess there are many scarred people out there on both the phenomenological and "non" (in all forms) sides. I think saying phenomenology has won is not accurate. Winning at the moment, to be sure, but that particular pendulum has swung back and forth at least twice in my memory.

Add Johannes Walcher to the list of faculty positions, not dark matter and not phenomenology. And Chris Jackson, phenomenology and some dark matter component.

Without the polemic of "A Phenomenologist", which I think is pointless, I am also surprised that Peter Woit is not interested in the debate as to whether his assessment that dark matter earned people jobs this year is accurate. He seems to studiously avoid it, which may be because I think the facts just don't bear it out. Which is his choice, but he is the one who made the pronouncement to begin with.

Phenomenologist: Even among the people getting jobs in your camp this year, are you really sure they are at all relevant for the real world? Despite the upswing in phenomenological positions, there is still very very little representation in the past ten years for people who do work on the Standard Model. People seem to be happy reward inventing unlikely or baseless theories for how nature could be, but the work of studying how it is seems to get very little attention.

26. **Peter Woit**
May 9, 2010

Anonymouse,

I haven't responded to your arguments partly because I've been out of town, but mainly because I just don't have anything significant to add. Yes, the world is complicated and "the people getting jobs are dark matter phenomenologists" is an over-simplification of a complicated situation. On the other hand, I still believe that the point I was making in the posting is quite valid: particle theory hiring is often very faddish, and this year the fad is phenomenology, with an emphasis on work relevant to dark matter.

Some other people's arguments I haven't responded to just because long experience has shown it's a waste of time trying to have an intelligent discussion with people who want to carry on a hostile argument from behind the cover of anonymity.

27. **Anonymouse**
May 9, 2010

That's fair enough.

28. **Eric**
May 9, 2010

I only have one thing to add, which is that hiring choices are frequently determined by which candidates are considered to have the best chances of bringing in grant money. Research topics which seem to have the best chance of getting funding are frequently those which some consider to be faddish.

Bohmian Spat

April 29, 2010
Categories: Uncategorized

Here's a story from the boundaries of conventional physics of the sort I normally try to resist paying any attention to, but couldn't quite help myself this time:

Last week I noticed amongst the e-mail from Jack Sarfatti that clutters my (and many other people's) mailbox some forwarded messages about a kerfuffle involving the withdrawal of a conference invitation to Brian Josephson. Josephson is a Nobel Prize winner but, on the other hand, he seems to think that [this sort of thing] makes sense. In one of the messages, I noticed that Josephson defends himself by pointing out that his talks often don't involve paranormal phenomena, giving as example a recent [Hermann Staudinger lecture] in Freiburg (Staudinger was a chemistry Nobelist, also my great-uncle).

This mini-scandal has now made it to a [Times Higher Education story] today, which starts off:

> An extraordinary spat has broken out after a Nobel prizewinning physicist was "uninvited" from a forthcoming conference because of his interest in the paranormal.
>
> Details of the conference in August for experts in quantum mechanics sounded idyllic. Participants were due to discuss "de Broglie-Bohm theory and beyond" in the Towler Institute, which is housed in a 16th-century monastery in the Tuscan Alps owned by Mike Towler, Royal Society research fellow at Cambridge University's Cavendish Laboratory.
>
> Last week, any veneer of serenity was shattered. Conference organiser Antony Valentini, research associate in the Theoretical Physics Group at Imperial College London, wrote to three participants to say their invitations had been withdrawn.

The current situation seems to be that Josephson, David Peat and Jack Sarfatti were un-invited, but now Josephson and Peat have been re-invited.

I had never heard of the [Towler Institute] before, but it sounds like a beautiful place, which physicist Mike Towler has admirably made available as a site for hosting small meetings and conferences. From the information on its web-site, this looks like the kind of place I'd find it very difficult to turn down an invitation to, no matter what the conference topic.

The [conference] at issue will be held at the end of the summer, and deals with what is known as "de Broglie-Bohm theory". One can read about this many places, including this [site] of Mike Towler's. The conference summary itself refers to the de Broglie-Bohm theory's "fringe nature in modern physics", and for more about why it is controversial see Towler's lecture [Not even wrong: Why does nobody like pilot-wave theory?].

After spending a little time learning about it many years ago, I quickly decided that I personally didn't like pilot-wave theory, partly because it seems to me that it throws out all the deep, amazing and experimentally verified links between modern physics and mathematics that motivate what I love about the subjects, getting nothing much in return. I don't see a good reason to believe that research in this area is going to lead to something interesting, but those who do have every right to keep trying. As they do so, they face serious problems in distinguishing crackpot from non-crackpot efforts, as this story makes very clear. Note that I have no intention of putting any time into this problem myself, so in this case I'm adopting a uniform policy of just deleting all comments arguing for or against de Broglie-Bohm. If that's a topic you like to argue about, do it elsewhere.

There's more [here] from Chad Orzel.

Comments

1. **willman**
   April 29, 2010

   Just to get a little clarification/enlightenment, I'll try a question: do you dislike the GRW approach to quantum mechanics (and related "spontaneous collapse" approaches like Philip Pearle's) as much as you do the de Broglie-Bohm approach, or do you find GRW's work more palatable than that of pilot-wave theorists (and if so, why)?

2. **Anonymous**
   April 29, 2010

   I remember hearing a talk given by Brian Josephson on "Mind Matter Unification". After the talk a student challenged Josephson with the question whether his theory is experimentally falsifiable. Josephson thought for a while, and answered, "The energy of the Universe may be different when mind is taken into account". According to people who know him, Josephson lost interest in conventional physics soon after (or even before) he was awarded the Nobel prize, but he is a very smart person and he's still doing integrals in his non-conventional research.

3. **Stephen**
   April 29, 2010

   The abstract of Brian Josephson's paper (i.e. "this sort of thing") starts "A model consistent with string theory is proposed for so-called paranormal phenomena such as extra-sensory perception (ESP)…". Is there a hint of irony here?!

4. **Peter Woit**
   April 29, 2010

   willman,

   In general I'm not much of a fan of attempts to modify the basic structure of quantum mechanics in order to deal with the measurement problem (precisely because this basic structure fits so well with basic structure in mathematics). From the little I know of them, the "spontaneous collapse" models have more modest goals and are less about replacing the whole QM formalism with something that I find radically less appealing, but I know very little about this.

5. **Bob Levine**
   April 29, 2010

   There seems to be a bit more to this story than the impression conveyed by the initial reports, on the basis of which Orzel asks the entirely reasonable question, what were the organizers thinking? A big part of the answer seems to be contained in Towler's long post in response to the THE story and attendant correspondence—specifically, that Josephson and Sarfatti were NOT formally invited. According to Towler, they 'they asked me if they could attend, which is very different.' Indeed it is. A bit more information about what *actually* happened is probably warranted before any conclusions are drawn from all this.

6. **willman**
April 29, 2010

Thanks, Peter.

7. **milkshake**
April 29, 2010

They got worried about being lumped together with Esalen… I feel sorry for the organizers, they seem generous. Maybe they should look into bringing in younger people instead; summer workshop would be also a less conspicuous way to invite someone with fringe ideas

8. **Alex R**
April 30, 2010

The comments following the Times Higher Education story are not to be missed, for entertainment value at least, containing comments from Sarfatti, Josephson, and Towler among the usual anonymous suspects…

9. **John Baez**
April 30, 2010

That paper by Josephson may have been moved from the hep-th section of the arXiv to the general physics section — see his complaints here.

10. **Phil Warnell**
May 1, 2010

Dear Dr. Woit,

As Antony Valentini was himself fated to existing for years outside the protected walls of the Ivory Towers, it is indeed curious to find him at the centre of all this. I would be the first to say such bickering as being completely unnecessary and yet also would insist that transparency should not be forsaken in situations where an opinion is rendered, by citing any which others may have to express as being irrelevant in respect to the topic.

Sincerely,

Phil

11. **Antony Valentini**
May 2, 2010

I would like to make a public statement about this.

The fuss stemmed from a private email that I wrote to Prof. Brian Josephson on the 19th April 2010, regarding a conference (about the de Broglie-Bohm interpretation of quantum mechanics) which I am co-organising with Dr. Mike Towler. The matter has recently erupted into the public domain with the publication of a rather misleading article in Times Higher Education.

Conference organisers are sometimes required to make difficult judgements, and of course mistakes can and do occur. The email I wrote was an attempt to deal with a difficult and complex organisational problem internal to the conference. It was not intended as a literal statement of my views about the scientific status of research into the ‘paranormal’. Nor did the wording accurately convey the nature of Prof. Josephson’s early association with the conference.

For the record, and contrary to what many are claiming: I am not in principle opposed to the careful and scientific investigation of alleged anomalies, whatever they may be. This view seems to me entirely obvious and uncontroversial.

Some will ask why I wrote an email apparently ‘dis-inviting’ a participant. Normally, such a step would of course be a regrettable breach of basic etiquette, and the recipient could reasonably complain strongly (and in private) to the organisers. However, as many will have learned from Dr. Towler (who started planning the conference before I got involved), certain alleged ‘invitees’ were in fact never formally invited.

Even so, some may ask why certain people became associated with a conference that is outside their domain of expertise, and which was never intended to be about the paranormal. Others feel driven to suggest that I was forced to write the email by a sinister power, and attempt to portray this episode as a bigoted attempt to suppress radical ideas. Some have simply concluded that there were probably good (if obscure) reasons for my writing the email, while others have seen fit to make comments without knowing the full (and private) facts behind the case.

In my view, if I may say, these matters are the business of the conference organisers and not of anybody else.

Prof. Josephson took the regrettable step of posting my email, in full and with author signature, on his website. (The author information and some of the text has now been removed.) This act encouraged a storm of protest from some of Prof. Josephson’s associates, partly in the form of a large volume of misleading emails sent to all the conference participants as well as to dozens of others (including journalists) and partly in the form of postings on various websites, including one that by any reasonable standard can only be described as deliberately defamatory.

Private correspondence (whether by conventional or electronic mail) should be treated as private, and should not be placed in the public domain without the author’s consent. The internet is an evolving medium, and one can query the suitability of standard constraints in this context. However, I suggest that we all take a deep breath, and ask ourselves if it is wise to blur the distinction between private and public correspondence in this way.

It is my view that a private matter between Prof. Josephson and myself has been brought into the public domain in a manner that is inappropriate and improper, as well as unhelpful and deeply misleading.

Some will regard my attitude as old-fashioned. For the other side of the argument, I can recommend a book by Lee Siegel, whose title speaks for itself: ‘Against the Machine: Being Human in the Age of the Electronic Mob’.

12. **csrster**
May 4, 2010

That’s a shame as the conference otherwise sounds like a Bohmian Rhapsody.

13. **Janne**
May 4, 2010

Theoretical physicists seem to have this strong paranormal ability to assess the mental health status of people they haven't even met.

14. **M. Wang**
May 5, 2010

Dr. Woit, can you elaborate on "all the deep, amazing and experimentally verified links between modern physics and mathematics that motivate what I love about the subjects"?

15. **Peter Woit**
May 5, 2010

M. Wang,

I'm referring to the close relationship between between representation theory and quantum mechanics, and the still not well-understood issue of how these work in the case of gauge symmetry and qfts like the Standard Model.

16. **Dave Miller**
May 7, 2010

Peter,

I've been interested in Bohm-de Broglie theory since my undergraduate days (alas, more than three decades ago!).

I share your aesthetic feelings that Bohm-de Broglie is unlikely to really give fundamentally deeper insight into the nature of QM. However, even granted that, I have always found it interesting that there does exist this mathematically consistent theory that reproduces all the results of standard QM. I think that this is worth exploring from the viewpoint of foundations, even if, as I suspect, it will not in the end, replace standard QM.

I have also followed both Valentini's and Sarfatti's work over the years. Valentini's work is normal physics: e.g., mathematical results are proved from clearly stated assumptions, etc.

I think I can honestly and diplomatically say that a different characterization would be needed to describe Sarfatti's work (Jack himself might even agree).

These guys are engaged in different activities, very, very different activities. Trying to include those activities in the same conference would be like holding a joint conference on superstrings and digital signal processing (and I hope I didn't just start a new string subfield!).

All the best,

Dave Miller in Sacramento

Oy vey

April 30, 2010
Categories: Multiverse Mania

Last Friday City College held a symposium here in Manhattan celebrating physics at City College. I was able to attend just the morning session, which began with a quick rescheduling of Anton Zeilinger for David Gross, who had overslept. Gross finally did make it and gave a talk on "The Frontiers of Particle Physics". He says he's taking bets in favor of supersymmetry being seen at the LHC, with 50/50 odds, and expects first evidence for supersymmetry within a year or two. By the time he got to the part of his slides about string theory he was over time, so he bypassed them, flipping ahead several slides at once.

Unfortunately I seem to have missed the real fireworks, which were at a panel discussion that afternoon. There's a report at Scientific American, entitled [Star physicists trade barbs over cosmological model](). Alan Guth was there, promoting the multiverse and the anthropic explanation of the CC. Gross was having none of it:

> "In reaction to that last talk—oy vey,"…
>
> Gross called Guth's concept of eternal inflation somewhat speculative, noting that if other universes do exist, they are causally disconnected from ours—"every goddamn one of them." As such, Gross added, talk of other universes "does bear some resemblance to talking about angels."

Comments

1. **Nathaniel**
   May 1, 2010

   Hmm…Gross says that Guth's multiverse bears some resemblance to talking about angels…huh, what about string theory? Seems like both theories are speculative unless there is some experimental evidence behind either of them. Kind of like the pot calling the kettle black.

2. **Mantis**
   May 1, 2010

   +20 sanity points for Gross.

   Nathaniel, string theory was supposed to describe our Universe but failed and it's this failure that prevents it from making testable predictions. Multiverse OTOH is all about things unobservable even in principle and from the very start, so even if it works perfectly it's still completely worthless untestable speculation.

   Multiverse ideas are a complete waste of time and resources, a pathology, a nonsensical fashionable crap which doesn't deserve to be associated with physics of natural sciences in general.

   Science is based on empiricism, if something cannot make a connection to experiment even in principle it is not science, it is theology.

   And I don't mind people studying theology but I do mind theology masquerading as physics especially if it's consuming funds which society devoted to physics.

3. **Verified Armonyous**
   May 3, 2010

   Mantis, although I share your point of view regarding landscapism, I was not aware that string theory has failed.

   Perhaps one could say that string theorists have failed so far in delivering what they had promised. In spite of that, I think string theory is too good not to have anything to do with physical reality and it's just a matter of time that this will be proven to be the case.

   Then one can debate about how much manpower should be devoted to this enterprise
   and other sociological issues but the shear revolutionary potential of string theory is beyond doubt.

4. **Peter Woit**
   May 3, 2010

   VA,

   It's not "string theory" that has failed (that term now applies to so much that it is becoming meaningless). What has failed is the speculative idea of starting with string theory in the critical dimension (10) then finding a consistent "string vacuum" that keeps 4 dimensions large and somehow deals with the other six. 25 years of work on this have provided strong evidence that this can't provide a predictive framework (leading to the multiverse nonsense as a desperate way out). Going to M-theory just makes things worse by providing even less predictivity.

   Anyone who wants to claim that this speculative idea has not failed needs to provide a plausible scenario in which it can be salvaged and turned into a success. I think the only way this can be done is through arguments of the sort "the general idea of string theory is so wonderful that there must be some unknown new insight into it which will come along and save the situation." These are more wishful thinking than science.

5. **Austin**
   May 3, 2010

   Peter Woit said,

   "Anyone who wants to claim that this speculative idea has not failed needs to provide a plausible scenario in which it can be salvaged and turned into a success."

   Correct me if I am missing something, but I see no problem with the following scenario: like any other physical theory, you use observation to constrain the free parameters of the theory, then you can make predictions. Technically difficult, but plausible I think.

6. **Peter Woit**
   May 3, 2010

   Austin,

You need to explain why no one has done this so far, and provide a plausible scenario for how this is going to change in the future.

The state of the art is that in various "string vacua" you can calculate very crude things like number of generations, but find that you can get any number you want. And you don't even know if you want 3 or 4 (maybe there's a fourth higher mass generation??).

For some more detailed numbers, like fermion masses, as far as I've ever been able to tell, the state of the art is that you can't compute these reliably and accurately (say, to better than 1 %). There also seems to be no reason to even try, since the evidence is that even if you could do this for any specific string vacuum, among the 10^500 possibilities you could get whatever number you wanted.

In other cases (vacuum energy), people working in this area have explicitly given up. Generically computations give something wrong by absurdly large numbers of orders of magnitude, leaving the only way out the one Gross describes by "Oy vey!".

7. **Austin**
   May 3, 2010

   I don't know why no one has done this, but it's a plausible scenario, I think.

   "…among the 10^500 possibilities you could get whatever number you wanted."

   Yes, but most or all of those 10^500 are at odds with observation. Thereby observation can cut the 10^500 possibilities down to fewer (perhaps zero).

   Regarding vacuum energy: I thought (admittedly I'm not familiar with this technically) that various string vacua allowed different values of the vacuum energy, (just like fermion mass and number of generations) some of which are negative, some of which are positive but too large ("by absurdly large numbers of orders of magnitude"), and some of which may be in the experimentally allowed range.

   Still it's just a plausibility argument, I assume that actually making this work technically would be beyond difficult.

8. **Fabien Besnard**
   May 3, 2010

   Mantis said : "Multiverse ideas are a complete waste of time and resources, a pathology, a nonsensical fashionable crap which doesn't deserve to be associated with physics of natural sciences in general."

   This is a rather dogmatic point of view. MWI interpretation of QM is in some sense "saner" than wave function collapse. More generally, science is full of entities that can't be observed but are theoretically useful, so one must keep an open mind.

   As I see things, the problem is more with anthropic explanation than with multiverse theories. Saying that something is anthropically explained amounts to saying that it happened merely by luck, so it is equivalent to say that there are no reason for it. It is fine if there really is no reason, but this means we gave up explaining that thing, it does not mean that we explained it.

   So adopting a multiverse theory for the sake of anthropic reasoning is not a good deal. But if you have other, compelling reasons, to adopt a multiverse theory, and that theory says something (like the cosmological constant) is environmental, then it is ok for me.

9. **Peter Woit**
   May 3, 2010

   Austin,

   You should think carefully about the significance of numbers like 10^500. Say you are wildly successful, and you manage to calculate the 20 or so parameters of the standard model each to 1 % accuracy. You'd expect each such achievement to cut out 99% of the possible vacua. Doing all 20 calculations successfully and imposing the results, you expect to have 10^460 viable possibilities left.

   There are very good reasons most particle theorists have given up on this, whether or not they're willing to use the word "failure" to describe the situation.

10. **Peter Woit**
    May 3, 2010

    Fabien,

    The kind of cosmological multiverse motivated by the string theory landscape that Guth is promoting and Gross is "Oy vey"ing about is something different than the multiverse of the MWI interpretation. They're two quite different things.

11. **Austin**
    May 3, 2010

    Peter Woit said,

    "Say you are wildly successful, and you manage to calculate the 20 or so parameters of the standard model each to 1 % accuracy. You'd expect each such achievement to cut out 99% of the possible vacua."

    I don't follow this reasong. "1% accuracy" is not the same as "1% of all possibly allowed values" and therefore is not the same as cutting out 99% of them.

    I'll try a simple counterexample to clarify what I'm saying. Take some free parameter of the standard model, say unit charge e, that theoretically can have any real positive value. Measuring the value of e even to 50% uncertainty eliminates a fraction of the previously allowed values that is arbitrarily close to 100%. See what I mean?

    10^500 is a huge number, but (as far as I know) before observational constraints, the standard model allows uncountably infinite possible values for its free parameters. In light of this, calling 10^500 big is a bit like a black hole calling the kettle black, isn't it? At least 10^500 is finite.

12. **Peter Woit**
    May 3, 2010

    Austin,

    One can make pointless arguments about things like finiteness of the number of string theory vacua all day, but the bottom line is that the LHC is starting up and despite more than a quarter century of intense effort by thousands of smart people, string theory has led to no predictions at all about what it will see. If you actually spend time understanding string vacuum constructions and how they work (or don't), the reasons for this become apparent. If you want to claim that this situation is not a failure, you need to come up a plausible explanation for why things have not worked out so far, but will work out in the forseeable future.

13. **Fabien Besnard**
May 4, 2010

Peter said :
"The kind of cosmological multiverse motivated by the string theory landscape that Guth is promoting and Gross is "Oy vey"ing about is something different than the multiverse of the MWI interpretation. They're two quite different things."

I know, but Mantis was saying that any multiverse theory was junk, on the ground that other universes are unobservable. I replied because it's a rather common point of view, which I think is completely misguided.

14. **Verified Armonyous**
May 4, 2010

Peter said :
"Anyone who wants to claim that this speculative idea has not failed needs to provide a plausible scenario in which it can be salvaged and turned into a success."

I'm not a string theorist myself and there are people better qualified than me to defend string theory. But as far as I know string theory is by far the only serious game in town when it comes to quantizing gravity, which is a notoriously difficult and non trivial task.

It also seems clear that much much more work will be needed to make definitive progress in proving that this theory has something to do with the real world.

At this point, what do you propose to do? To abandon the most serious candidate we have to solve this problem, simply because you're not patient to wait longer? I don't think your position on this is very serious. Some dose of criticism on the string community might even be healthy but you shouldn't throw the baby out with the bath water.

15. **Bill K**
May 4, 2010

At this point, what do you propose to do? To abandon the most serious candidate we have to solve this problem, simply because you're not patient to wait longer? I don't think your position on this is very serious. Some dose of criticism on the string community might even be healthy but you shouldn't throw the baby out with the bath water.

String theory has been looking for a baby for 25 years, but it is all bath water.

Neither "abandon" or "wait" has anything to do with the way research is conducted. When a theory runs into a dead end, as string theory has, you don't abandon it, but you do put it aside temporarily. And you don't wait, you just try to find something that's more productive.

Witten has been quoted as saying that string theory was ahead of its time, and maybe that's the problem. Maybe we just need to take things in the proper order, and after understanding what comes after the Standard Model we'll be better able to appreciate what, if anything, string theory is good for.

16. **Pawl**
May 4, 2010

VA,

At the risk of repeating old arguments (but yours are old too):

(a) "String theory is by far the only serious game in town when it comes to quantizing gravity" is inaccurate.

(b) The sense in which string theory was a hopeful approach to quantizing gravity was that it seemed to be an approach along the lines that quantum field theorists were familiar with which looked like it might tackle the problems which seemed most bothersome to quantum field theorists. It was far less convincing to relativists even in its heyday — something which is only fair to take into account in assessing its strengths.

(c) If we're going to make progress on quantum gravity we need to have a hard-headed assessment of when to give up on poor approaches. At some point the argument that one approach is "the best thing going" becomes a weaker one than the judgment that that approach seems to have little going for it and quite a lot against it: it becomes an error not to cut one's losses and reconsider the question and the entire approach.

(d) We are likely to make little progress on quantum gravity until the right breakthrough occurs. It is very hard to figure out what sort of steps the field as a whole could take to try to hasten this. But the first thing would be to set a standard of examining carefully and fairly the strengths and weaknesses of different ideas.

17. **Verified Armonyous**
May 5, 2010

Bill K, make up your mind. Either string theory is bath water or ahead of its time. Let me trust Ed on this one.

"Neither "abandon" or "wait" has anything to do with the way research is conducted."

Exactly. And when you stumble on a theory ahead of its time you don't wait to be ready to deal with it. You jump on it and devote all your effort to understand it. Taking things in its proper order is for bureaucrats, not for curious scientists eager to find the right theory behind Nature.

Pawl, it's fine for arguments to be old as long as they are right. Your (a) is not even an argument but simply an statement. Without further qualifications it's useless.

(b) hahaha

(c & d) The breakthrough will come from more work, not from your clear definition of when to give up or how to precisely weight the worth of different ideas.

18. **jpd**
May 5, 2010

a)
arxiv search for "quantum gravity" AND NOT "string"

returns:
Your query resulted in too many hits, only 1000 hits are being displayed. These are not necessarily the 1000 most recent papers. We recommend that you try a more specific search.

b) “hahaha” isn’t even a statement

19. **Verified Armonyous**
May 5, 2010

jpd

a) Oh, you convinced me! Now it’s clear to me that there are much better ideas out there on how to consistently quantize gravity. Just with one click! I’ll use your insightful method in the future!

b) Sure it is.

20. **Peter Woit**
May 5, 2010

VA,

Excellent job of driving the intelligence level of a discussion down to zero. Enough.

String Vacuum Project 2010

May 3, 2010
Categories: Multiverse Mania

I've written before about the String Vacuum Project (back in 2006 and 2008), and there was a story about it in Nature. This week they are having an SVP 2010 Spring Meeting at the KITP, talks available here.

A proposal to the NSF for funding of the String Vacuum Project was first made five years or so ago, but I had heard that this and later versions hadn't been successful. In recent years perhaps the main proponent of the project has been Keith Dienes of the University of Arizona, who organized its last meeting in Tucson two years ago. Dienes started work as a program manager at NSF last fall. Maybe it's just a coincidence, but the SVP now has funding through an NSF grant for $150K this year, with the grant paying for bi-annual meetings (of which I guess the KITP one is the first). While "the PIs are proposing a String Vacuum Project (SVP) network with eight geographic nodes", the grant sponsor is the University of Arizona, where the co-PI for the grant is Shufang Su, a phenomenologist who doesn't seem to have any history of working on string vacua.

Well, at least stimulus funding is helping get the SVP off the ground…

Update: After looking through some of the workshop talks, it's very unclear to me what the "String Vacuum Project" actually is. At this point it appears to just be a mechanism for getting the NSF to fund three graduate students working on string phenomenology. From the talk by Michael Douglas you learn that it's very unclear what a string vacuum even is. It appears to involve an intractable large unknown space (including e.g. "all six manifolds"), with an unknown effective potential on it, with disagreements among practicioners about whether the effective potential is a sensible thing to look at.

Not surprisingly, the discussion session about what the project should be doing was a sad thing to watch. One of the main topics was the SVP Wiki, which people hope to improve. Maybe it's been moved somewhere else, but the only address I know for it (here) has been down for quite a while.

Update: More discussion showing the current level of understanding (nil) of string vacua here.

Update: There is now a new String Vacuum Project web-site.

Comments

1. **Coin**
   May 3, 2010

   From a physics outsider's perspective the SVP certainly *sounds* like the kind of thing the string theory community ought to be encouraged in. If the landscape is unavoidable then we ought to at least make some sort of effort to determine what it looks like?

   The thing that interests me about the SVP is the attempt to do that census or sampling of string vacua. The NSF grant says they will be funding students to work on that? I'm not sure I can see much sign of that particular effort in the SVP talk descriptions but I don't think I'd know what to look for. I wonder, are they still planning on making that online database of vacua they've analyzed?

2. **Peter Woit**
   May 3, 2010

   Coin,

   The problem is that there has already been a great deal of effort put into looking at string vacua, with discouraging results. Most string theorists acknowledge this, and don't work on this sort of thing.

   When the SVP started many years ago, part of the plan was to try and gather enough data to make statistical predictions of some sort. The initial hope was that you could at least tell whether supersymmetry breaking was likely to be at high or low energy scale. That failed and people seem to no longer believe statistical predictions are possible, so I don't see what you would do with an accumulated database of string vacua. It's rather unclear to me what the specific plan of the SVP project is now, it will be interesting to see what comes of their discussions at the KITP this week.

3. **Dave**
   May 4, 2010

   Indeed surely a coincidence, since NSF program officers abide very strictly by conflict of interest regulations.

4. **Fabio**
   May 4, 2010

   I've just completed the CPP (Cubic Polynomial Project). I've enumerated every cubic polynomial with complex coefficients, and have created an online database of results which is integrated into Wolfram Alpha. Just go there and type in a polynomial, it will show you the results of my analysis. Now I am applying for NSF funding to do the same for quartic polynomials.

First Results From XENON100

May 4, 2010
Categories: Experimental HEP News

The XENON100 dark matter experiment now has a paper out reporting their first results, from a test run of 11 days. They claim a 90% confidence level exclusion of 50 GeV WIMPs with a spin-independent elastic cross-section above 3 x 10^{-44} cm^2, which one can compare with the recent CDMS limit of 3.8 x 10^{-44} cm^2 at similar mass (70 GeV). At the time of the CDMS result many seemed to believe that the two events seen were not background (see here), with Gordon Kane claiming “it is likely it is dark matter.” The New York Times has an article today, with Kane now commenting on XENON100 “if they see a signal, it will be unambiguous”, by which I presume he means that their full data will conclusively show whether the CDMS events could have been a signal. Given that they are seeing nothing at all now, at 10 times greater sensitivity later, they may very well see an ambiguous signal…

The XENON100 result also appears to rule out claims from the DAMA and CoGeNT experiments to have seen some sort of signal. For more on this, as always in dark matter issues, Resonaances has the best coverage of the story.

Update: Physics World reports on objections to the XENON100 claims from a CoGeNT physicist and others, see this arXiv preprint.

Comments

1. **lcs**
   May 4, 2010

   “It’s the strongest statement about dark matter today and it reads: we have looked here and there and over there but didn’t find nothing,” Rafael Lang of Columbia University, wrote…

   I guess Columbia doesn’t teach remedial English.

2. **Anonymouse**
   May 5, 2010

   Ics:

   Rafael Lang is a postdoctoral researcher. Columbia didn’t teach him anything, in that he never took a course there. He works there.

   Oh, and he is German. Sorry that some people have less than perfect English… for it being at least his second language, I would say he does reasonably well…

3. **John Baez**
   May 7, 2010

   Suppose XENON doesn’t see WIMPs even when it reaches the maximum sensitivity that people have planned for it. What would this tell us? What kinds of WIMPs would be ruled out as a plausible explanation of the amount of cold dark matter that we seem to see? And roughly when would this happen?

4. **Bill K**
   May 7, 2010

   By definition, a WIMP interacts with nucleons via the weak nuclear force, implying an elastic cross section of the order of 10^-44 cm2. So already an order of magnitude improvement in the experimental limit would start to make things uncomfortable.

   On the other hand, there’s so much uncertainty surrounding the properties of dark matter, and so much interest, that a sequence of much larger experiments beyond XENON100 (LUX 300 kg, XENON1T 1 ton, LZS 1.5 tons, LZD 20 tons) have been studied/ planned/proposed. So my guess is the search for WIMPs will go on for another 5 years at least.

5. **Anonymouse**
   May 9, 2010

   WIMPs may have originally meant “weak nuclear”, but that is a strong version of the definition compared to the way the term is used today. Call it more “roughly weak nuclear” or even “sub-weak nuclear” more accurately.

   Xenon100 will start to become sensitive to backgrounds at roughly one year of running. So end of this year, expect their big jump in improvement. It will take a bite out of, say, MSSM parameter space, but not one that will make SUSY practitioners uncomfortable.

6. **Bill K**
   May 10, 2010

   Anonymouse, my understanding is that the observed abundance of dark matter particles as thermal relics of the Big Bang implies that their self-annihilation is of the order of the weak nuclear force. And hence they are usually considered to be WIMPs in the strict sense (the so-called ‘WIMP Miracle’) This conclusion holds regardless of whether they arise from supersymmetry or something else.

7. **Anonymouse**
   May 10, 2010

   Bill K,

   We sell the WIMP miracle as a pretty picture, but it is far from encompassing the spectrum of possibilities for dark matter or WIMPs. That said, I think it is likely to turn out the WIMP miracle does explain the relic abundance of dark matter.

   However, when one gets precise, the thermal abundance actually requires “sub-Weak strength” interactions. Literally weak stength leads to a cross section which is too large, causing the WIMPs to stay in equilibrium longer, and they are depleted in the Universe by too much (e.g. there is too little dark matter to match observations). So in fact if we take the WIMP miracle seriously, defining WIMP to be “weakly interacting” in the strict sense is in fact excluded by measurements from WMAP and other experiments.

It’s true that none of this requires supersymmetry, but my hope is that my answer above provides context which is useful to someone like John Baez, who asked what Xenon100 can do and on what kind of time scale

LHC Update: Bing Bang Machine Could Confirm or Disprove String Theory

May 5, 2010
Categories: Experimental HEP News, This Week's Hype

Today's CERN LHCC meeting had a wide-range of reports about how the machine is doing (1 nb^{-1} now, 10 nb^{-1} over the next 5 weeks), what the experiments are seeing (charm, Ws), and what physics might be possible with the 2010-11 run (limits on some supersymmetric and other more exotic scenarios).

Reuters this evening reports on the meeting, headlined with the typical delusional nonsense about string theory and the LHC which we're in for several years of {"Could confirm or disprove string theory").

Update: This story has made it to various media outlets, including one that has it as Collider on Track With Bing Bang Research. Title of posting edited appropriately.

Comments

1. **Interested (I guess I'll keep that moniker)**
   May 6, 2010

   Can you clarify something?

   The link you provided quotes an LHC director stating they may find "large extra dimensions, string balls and heavy slow-charged particles".

   Do string theories have as precise definitions of those things listed as the particles the Standard Model predicted? My impression is that if the definitions are loose or even if they just come up with another thousand guesses they will eventually 'find' something.

2. **Peter Woit**
   May 6, 2010

   Interested,

   String theory doesn't predict anything about what the LHC will see, but it is consistent with all sorts of things in an infinitely long list of exotic non-standard model possibilities. That's what is being referred to here.

3. **unreuters**
   May 6, 2010

   At least Reuters reports on the thing at all.
   Let's not complain about *everything*.
   It's overall a good article, there's no need to quibble about a few extra dimensions here and there.
   For example:

   "The CERN collisions, some 200 million since March 30, in the 27 km (16.8 mile) LHC tunnel are recreating on a miniature scale what happened within nano-seconds of the Big Bang 13.7 billion years ago that created the galaxies, stars — and life."

   This is not bad at all. Quite good, some might even say. Why not? And this —

   "Six ultra-sophisticated detectors around the LHC record how the particles behave after being smashed together, transmitting the data for analysis to laboratories at CERN and in other research centres around the globe.

   Already the machine has identified many elements included in the so-called Standard Model that physicists created during the 20th century for how they believed the cosmos should and does work, said CERN scientist Andrei Golutvin.

   ……..

   "To me, it is a miracle that the LHC is detecting the particles we expected from the Standard Model so early in this experiment. It shows just how well the LHC is functioning," said Golutvin, spokesman for the LHCb."
   .

   Really that is all quite good.

   Once upon a time, in a land not so far away, the Daresbury lab invited reporters on a tour, showing them their sychrotron and their van-de-Graaf generator (which was the world's tallest). An article subsequently appeared on the "world's tallest synchrotron." Eh …

   As for the LHC and the Big Bang, it was not so different for the SSC and the Big Bang. Indeed, I once was asked by some high school students in (near) Chicago, "We are confused about the reports on the SSC. It says the SSC will reach temperatures not seen since the Big Bang." So I replied "Yes." "But," said the students, "the SSC is a *superconducting* machine. It operates at *cryogenic* temperatures close to absolute zero. How can it possibly reach the temperatures of the Big Bang?"

   Oy double vey … ?

   I said the first thing which popped into my head, which was "Why me?"

4. **Peter Woit**
   May 6, 2010

   Unreuters,

   Typically, as with this one, the article itself isn't bad, but it's sitting under a headline of ludicrous hype about string theory. Most people read the headline, don't read the article.

5. **unreuters**

May 6, 2010

That is true of very many things besides string theory. Reuters is a business, and it has to sell, and the headline has to attract attention, and Reuters is not alone in this. There are many misrepresentations of things much worse than string theory, typically political subjects … but I drop the subject here.

6. **Robert**
May 6, 2010

   Typically the headlines are not even formulated by the same people as the article.

7. **Paul**
May 7, 2010

   "The CERN collisions, some 200 million since March 30, in the 27 km (16.8 mile) LHC tunnel are recreating on a miniature scale what happened within nano-seconds of the Big Bang 13.7 billion years ago that created the galaxies, stars — and life."

   This is not ok, the claims that LHC is a "Big Bang machine," or studying Big Bang are pure BS. There is no scientific reason to link it to the hypothetical Big Bang, the collisions recreated are ubiquitous in the Universe and the results won't tell us anything about the Big Bang itself.

   The conditions nanoseconds after the hypothetical Big Bang were completely different as the Universe was squeezed to an incredibly tiny fraction of it's current size and we have no idea how dark matter, dark energy, visible matter or other as yet undiscovered mass and energy types behave in such conditions. And invoking hypothetical inflation breaks the link to Big Bang even further.

   The link to Big Bang is nothing but a publicity stunt, the LHC is not studying Big Bang and won't tell us anything new about it, claiming that it will is ensuring that the public will see it as a failure.

8. **kumar**
May 7, 2010

   An answer would be very welcome for the long awaiting physics community. But the real question, if LHC results are against string theory, whether will it be sufficient enough to convince the string theorists?

9. **a quantum diaries surivivor**
May 7, 2010

   Stop it, people. We insist with our century-old mistake – we give the same old image of a snotty-nosed set to whomever cares to read our mutterings. Let us instead start trying to offer the media with images that are as sensationalistic as they need them, but correct.

   Much harder than criticizing, ain't it ?

   Cheers,
T.

10. **Joao Leao**
May 7, 2010

    BING Bang? Is this in homage to Bing Crosby or did Microsoft buy the naming rights to the Big Bang? (With editors like this who needs copywriters?)

11. **John Baez**
May 7, 2010

    Yes, it's "Bing Bang".

    Sing along with me: Uh Eeh Uh Ah Ah Ting Tang Walla Walla Bing Bang!

12. **Yatima**
May 7, 2010

    Next: Rappers rapping about the "Bling Bang". Ok, this is silly. Back to work.

13. **thomas**
May 7, 2010

    unreuters: awwww that hyschool studence story is so cute ^____^

14. **Claver**
May 7, 2010

    Might it not be a 'bling-ing' bang? It wouldn't just 'bling' would it? Or, maybe a 'blings' bang?

15. **Chris Oakley**
May 9, 2010

    BLING, from the Urban Dictionary:

    > As of 2007, commonly used to designate expensive new compact electronic accessories. Best examples are: 1) small, feature-packed cell phones. 2) small, high-megapixel digital cameras with huge RAM and bright LCD displays. 3) High-end MP3/video players. 4) Particle accelerators.

16. **Eric Baird**
May 14, 2010

    So if the LHC is now "Bling", where do we attach the big gold chain?

Applying String Theory to Quantum Information Theory

May 10, 2010
Categories: This Week's Hype

There's a remarkable article by Mike Duff in this month's CERN Courier, arguing the case that string theory does too have important applications: in Quantum Information Theory. The claim seems to be that since the same algebraic structures appear in black-hole entropy calculations in string theory and in the analysis of certain cases of the entanglement of qubits, this provides an application of string theory to Quantum Information theory. There's some remarkably obscure algebra involved, from exceptional structures such as E7, the octonions and the Fano plane to Cayley's nineteenth century work on hyperdeterminants. Besides the very complicated mathematics and physics, what I don't understand about this is the claim that if the same classical algebraic structure gets used to do a calculation in string theory and in subject A, it means that string theory is being applied to subject A.

While the mathematical physics story Duff tells may be of some interest (to learn more about it, there are review articles here and here), unfortunately he can't resist the temptation to shanghai it into service in the string wars. He gives a less-than-honest description of the problem with string theory:

> The partial nature of our understanding of string/M-theory has so far prevented any kind of smoking-gun experimental test.

The problem with string/M-theory is not that it is missing a "smoking-gun experimental test", it is that it is missing any kind of experimental test whatsoever, which is rather different. He defends the failure of string theorists to come up with any experimental test after more than 25 years of work by thousands of physicists writing tens of thousands of papers with a comparison of the situation to that of the time lag between the 1935 Einstein-Podolsky-Rosen paper and J.S. Bell's work 29 years later. One obvious difference is that, before Bell, hardly anyone tried to come up with such a proposal, unlike the case of string theory, where the issue has been the central one in the field since 1984.

Under the heading "Further Reading", one is referred to Duff's 2007 debate with Smolin, which I posted about here, based on second-hand reports from those in attendance. Until now I don't think I'd seen the transcript of the debate, which is available here. I notice that Duff sums up his argument as follows:

> The trouble with physics ladies and gentleman is that Lee Smolin and Peter Woit having lost their case in the court of science, are now trying desperately to win it in the court of public opinion. Thank you.

It seems to me that Duff, having lost his case in the court of his physicist peers, where string theory unification is widely seen as a failure and young string theorists are just about unemployable, is now trying desperately to win it with tendentious argumentation in the court of popular opinion (well, at least in the pages of the CERN Courier…).

Update: Lubos seems to mostly agree with me about this:

> So the role of the qubits, or the arguments of the hyperdeterminants, are "physically" completely different.
> …
>
> A superficial similarity of one aspect is very far from a full-fledged mathematical equivalence.

Update: John Baez's latest TWF has an explanation of some of the mathematics involved here.

Comments

1. **bohm (not)**
   May 10, 2010

   EPR and Bell – actually I think it was David Bohm – remember him? -who (re)formulated the EPR paradox in terms of spins, which were easier to visualize. This was done approx 1952(?)
   http://www.ge.infn.it/~zanghi/DJB.pdf

   EPR formulated their gedanken experiment using positions and velocities, but all(?) the expts use spins. Much simpler.

   EPR 1935, Bohm c. 1952, Bell 1964, Aspect 1982, GHZ 1989
   Progress takes place approx every 10-15 years (on average?).

2. **sf**
   May 10, 2010

   If these really are "important applications" then you'd think that us quantum information theorists would have heard about them…

   Maybe we're just incapable of understanding them.

3. **kevinfp**
   May 11, 2010

   It just happens that you and Lubos agree with each other. Does it mean that the two of you are necessarily right?

4. **Trent**
   May 11, 2010

   Peter,

   You continue to be the funniest person ever 🙂

   You write,

   "It seems to me that Duff, having lost his case in the court of his physicist peers"

   What exactly makes you say that?

   "where string theory unification is widely seen as a failure"

Any data to back that claim up? And even if true, let's suppose string theory unification doesn't work, how does that make string theory as a whole a failure? If there were 1000000000000 other true applications of string theory in other subjects, would you still claim that string theory is a failure just because it didn't fulfill it's original goals?

"and young string theorists are just about unemployable"

And you conclude what from that? 10 years ago string people were very much employable while hard phenomenologists were "just about unemployable". Same goes for loop quantum gravity (and not just in the last 10 years but they are pretty much unemployable even today). Would you have said that this is a signal that phenomenology or loop quantum gravity is a failure? Aren't you taking a superficial metric heavily influenced by fashions and trends (employability) and judge a whole field based on it if it suits your objectives (as in string theory) and do not do the same thing when it doesn't (as in phenomenology and loop quantum gravity)?

"…. now trying desperately to win it with tendentious argumentation in the court of popular opinion"

Are you really comparing yourself to Duff? Please check the archive how many scientific arguments does Duff make and how many do you make. Then do another piece of homework and check how many blog posts and other activity that is designed to effect public opinion does Duff make and how many do you make. See, this is one of the reasons I find you very funny 🙂

Cheers,
Trent

5. **Peter Woit**
May 11, 2010

Trent,

I didn't write that "string theory is a failure", specifically because "string theory" now refers to very many quite different things, some of which have had successes. However, the idea of using string theory in higher dimensions to get a unified theory has failed, and most physicists are aware of this. If physics departments thought that string unification was still a promising research program, they would still be actively trying to hire young people in the subject.

Duff knows the subject is in trouble, but instead of acknowledging failure and moving on, he's trying to evade the consequences of this failure with over-hyped and misleading claims. If he can't even get Lubos to defend them, you know he's gone way too far…

6. **pwlub**
May 11, 2010

"It just happens that you and Lubos agree with each other. Does it mean that the two of you are necessarily right?"

To borrow from George Carlin –
If PW and Lubos agree with each other in the middle of a forest and there is no woman around to hear them, are they both still (not even) wrong?

7. **Peter Woit**
May 11, 2010

kevinfp and pwlub,

Lubos isn't exactly a reliable source, but if he won't defend a certain piece of string theory hype, you're going to have trouble finding anyone who will.

Similarly, if he ever starts acknowledging global warming, I suggest you immediately head for higher ground…

8. **John Baez**
May 11, 2010

Personally I think it's almost a waste of time worrying about whether this counts as an "application of string theory". String theory is obviously in deep trouble when it comes to making any sort of contact with experiment, and at this point I've decided to take pity on that severely wounded horse and stop beating it. There's a lot of cool math in string theory, and there's a lot of cool math in Duff's work regardless of whether one considers it "string theory".

While Peter may consider it "remarkably obscure algebra", the 56-dimensional representation of E7 is a beautiful thing. I've discussed it [here](). I explained how it's related to a certain 57-dimensional manifold, the lowest-dimensional manifold on which E8 acts nontrivially. I also explained how it's built up from two copies of the exceptional Jordan algebra and two copies of the real numbers (56 = 27 + 27 + 1 + 1). It's called the "Freudenthal algebra" and it's been studied quite a lot. It's great to see new things being done with it – there's a nice introduction by [Bianca Cerchiai and Bert van Geemen](). To my mind, when it comes to beautiful structures like these, arguing about their possible "applications to physics" is far less enjoyable than actually learning about them.

9. **Trent**
May 11, 2010

Peter,

You continue beating your favorite dead horse. So I ask again, more sharply this time, so that you will perhaps not avoid responding:

Who cares if string theory does not work as a unified theory of the 4 forces?

Let's assume string theory has 1000000000000 fruitful applications outside of its original goal (unification), but does not work for its original goal (unification). Who cares?

If I set up a theory to prove the Poincare conjecture (never mind about Perelman for a second) but eventually end up proving the Riemann conjecture with it, do you think mathematicians will go around writing blogs about me being a sucker and all my efforts were in vain because I could not prove the Poincare conjecture using my methods, which was my original goal?

So, is this clear what I'm trying to say? If not, I'll clarify, but it seems to me you understand me perfectly well, you just don't want to answer or reply or comment on this very point because it's inconvenient for you.

"If physics departments thought that string unification was still a promising research program, they would still be actively trying to hire young people in the subject."

Again, what's your point? You argue by authority and majority but science is neither based on authority nor is democratic. By the same argument you would have concluded 5 years ago that string theory is the best thing in town. Or by using the same argument you would have concluded in year X that the best research program in that year is the one which gets the most hires. How sensible is that?

On another issue in my post you did not respond either. You countered the claim that you are courting public opinion instead of doing research by saying that Duff is doing exactly this. I gave you and your readers 2 homework assignments: (1) check the amount of noise you and Duff did in the past 3 years that can be considered "courting public opinion", call these N_W and N_D (2) check the amount of research results you and Duff had in the past 3 years, call these R_W and R_D. I'm pretty sure everybody will find

N_W >> N_D
R_W >> R_D

In other words, the situation of Duff and you is completely different, can not be compared.

10. **Trent**
May 11, 2010

I've just seen John Baez's reply and I fully agree.

The point is, who cares if string theory does not work as unification or does not make contact with experiment? It does lots of other great things, things for which it was not invented. Is that a bad thing?

11. **Trent**
May 11, 2010

Ooooops, my inequalities got mixed up.
Correctly, they are:

N_W >> N_D
R_W < < R_D

Cheers,
Trent

12. **Peter Woit**
May 11, 2010

John,

There's no necessary contradiction between your claim that this sort of algebra is beautiful and mine that it is "remarkably obscure".

Trent,

Once string theorists give up writing popular articles designed to mislead people about what string theory is and what it isn't, I'll stop pointing this out. The unwillingness of influential string theorists to own up to what has happened is a continuing source of damage to the subject.

The reason I pointed out that string theorist's colleagues have decided to stop hiring them is that you asked for evidence that physicists believe string theory to be a failure. That the latest fad is now dark matter phenomenology is not necessarily a reason to believe it will be any more fruitful than string theory. The sort of thing I personally have the most hope for, "formal" QFT research, is far more unpopular than either string theory or phenomenology.

String theory does do some interesting things, and I'm in favor of people investigating them and writing intellectually honest articles about them. In the case of what Duff is writing about, I don't actually see string theory doing anything, just a lot of hype on his part.

Duff's summation against Smolin and myself was not that we're not doing research, but that, within the physics community, the arguments for doing string theory had won out over those against the subject. That may have been true before the past half dozen years or so, but now the shoe is on the other foot. Instead of making an honest technical argument for string theory research that could convince his now-skeptical colleagues, Duff has decided to write a misleading semi-popular article, exactly the sort of behavior he wanted to accuse Smolin and me of.

13. **CWJ**
May 11, 2010

Trent writes,

"If I set up a theory to prove the Poincare conjecture (never mind about Perelman for a second) but eventually end up proving the Riemann conjecture with it, do you think mathematicians will go around writing blogs about me being a sucker and all my efforts were in vain because I could not prove the Poincare conjecture using my methods, which was my original goal?"

But that's a sloppy analogy. Suppose, having successfully proved the Riemann conjecture, you then went around saying, 'This suggests that the Poincare conjecture is true, too.' That's what string theorists often imply–that the success of their methodology in another area means that string theory–as applied to particle physics–is correct. And they have totally failed to establish the latter.

It's perfectly fine that the math invented from string theory is applied to other areas. That's great. But that establishes *nothing* about the hypothesis that strings are the fundamental constituents of matter.

14. **Peter Woit**
May 11, 2010

cwj,

I agree, but it's also important to note that what Duff is talking about is NOT math discovered by string theorists. There are a few examples where you can make the case that string theory research has uncovered important new ideas in mathematics. This isn't one of them.

The contrast between this reality and the string theory partisan argument "first assume string theorists prove the Riemann hypothesis" is rather striking.

15. **Roger Schlafly**
May 11, 2010

Trent, there is no need to clarify. Your argument consists of absurd hypotheticals and ad hominem attacks. If string theory had some merit that you wanted to promote, then you would address those merits.

16. **Coin**
May 11, 2010

“Suppose, having successfully proved the Riemann conjecture, you then went around saying, ‘This suggests that the Poincare conjecture is true, too.”

And this Duff article is clearly suggesting this. I mean, look at it. After four paragraphs about QIT he suddenly jumps in with “Strings, branes and M-theory: If current ideas are correct, a unified theory of all physical phenomena will require some radical ingredients in addition to supersymmetry”. He is clearly here not arguing string theory is an interesting mathematical tool potentially leading to technical advances in various areas. He is talking about M-theory as a physical model of the universe: string theory the hypothesis, not string theory the mathematical toolset. The following [dense] section where he lays out the mathematical similarities between QIT and string theory is specifically named “Falsifiable predictions?” and says “Nevertheless it cannot be denied that such a prediction in string theory would be welcome” before launching into his math dump. It seems like a not mathematically inclined person (or even someone mathematically inclined but not able to follow his argument– which honestly I can’t, I understand the article up to the “falsifiable predictions?” header fine and then everything that follows flies completely over my head) would look at that section introduction, see a bunch of dense mathematics and walk away assuming that he had just presented an argument that the string theory hypothesis falsifiably predicts [something to do with 3-bit entanglement]. Actually, I’m not totally certain he _isn’t_ trying to argue this. (He does back off to “so the esoteric mathematics of string and M-theory might yet find practical applications” at the end of the section, but if that’s the goal then why the big header talking about falsifiable predictions of M-theory?)

Overall I guess it seems weird to complain about Peter attacking the “dead horse” of string theory unification but not seem so worried about theoretically more-significant publications such as the CERN Courier publishing articles supporting that same “dead horse”.

17. **Kea**
May 11, 2010

Perhaps instead of bickering like stupid little boys, you could all spend some time reading, for instance, the papers of Levay, who is mentioned in Duff’s article. Sure, Duff is deluded about strings, but he has done some cool stuff here and if it turns out that the QI is in fact at the foundations of QG, then it will be relevant. And won’t you want to know something about it?

18. **John Baez**
May 11, 2010

Peter wrote:

> John,
>
> There’s no necessary contradiction between your claim that this sort of algebra is beautiful and mine that it is “remarkably obscure”.

True. I was trying to say, not only that it’s beautiful, but that it’s not “remarkably obscure”.

There’s a tiny smidgen of algebra that’s extremely well-known: the stuff people are forced to learn in math grad school, and the stuff theoretical physicists use all the time.

Then there’s a bunch of algebra that’s a bit less well-known, but still actually quite famous to people who care about algebra: stuff like E7, the octonions, the Fano plane, and Cayley’s hyperdeterminants. (I don’t know what Cayley’s hyperdeterminants are, but I keep running into people talking about them, so I don’t call them “extraordinarily obscure”: I just say I haven’t learned about them yet! The mere fact that they were discovered by a famous dude like Cayley means they aren’t all that obscure.)

Then there’s a vast amount of algebra, about 100 times as much, which has been worked out by mathematicians we’ve never heard of, and can be found in papers that sit largely unread in math journals. *That’s* the really obscure stuff: the kind of thing where merely citing an example would instantly multiply by 1000 the number of people who’d ever heard of that thing. Okay, I’ll give an example: 3-PAPLs. I’d call those fairly obscure.

Anyway, none of this is very important. I just wanted to say: you’ve got enough justifiable complaints against Duff without labelling his algebra “extraordinarily obscure”. In fact he’s studying quite famous algebraic structures, and apparently doing cool stuff with them.

19. **PhilG**
May 12, 2010

All Duff says is “While string theory and M-theory have yet to make readily testable predictions in high-energy physics, they could find practical applications in quantum-information theory.” and “So the esoteric mathematics of string and M-theory might yet find practical applications.” Notice the “could” and the “might”. There is no definite claim, just an intriguing idea that he is trying to get people interested in.

The maths behind this is very cool and yet not fully explored. It is just the kind of thing that mathematical physicists should be interested in. There are connections between hyperdeterminants and cohomology theory for example. The 2x2x2x2 hyperdeterminant is a degree 24 polynomial and this is connected to the “mysterious” appearance of this number in the theory of elliptic curves and bosonic string theory. Binary codes appear when the hyperdeterminants are extended to invariants of E8. This raises many questions about what else could be going on here.

Woit says that string theory as a unified theory has failed while others like Duff say that it has not suceeded yet. Half full or half empty? take your choice. Duff is just promoting the idea that there could be a fundamental connection netween M-Theory and quantum information theory that could answer some questions about both. He does not know enough yet to make a definite claim but he thinks it is worth mentioning. How can this justify such an attack?

20. **mathematician**
May 12, 2010

Wow, I hadn’t realized they were doing so much string theory in the 1800’s!

21. **Trent**
May 12, 2010

Peter,

Okay, let’s look at formal QFT. It’s been a subject that has been around for at least 30 years, but let’s say 25.

Can you please explicitly state what has been the original motivation 25-30 years ago?

And can you please explicitly state what has been achieved from these original goals? And what has been achieved as a byproduct or accidental result?

Cheers,
Trent

22. **Trent**
May 12, 2010

Coin,

"
Overall I guess it seems weird to complain about Peter attacking the "dead horse" of string theory unification but not seem so worried about theoretically more-significant publications such as the CERN Courier publishing articles supporting that same "dead horse".
"

The CERN Courier is not a research journal. Scientific discussion is done in peer-reviewed research journals. So it's okay for CERN Courier to talk in lay-man terms and in hyperbole. As far as actual science is concerned this is pretty much irrelevant just as Peter's blog is irrelevant. This is what Peter is missing, he is doing science journalism about science journalism. See, he is 2 steps away from real science. 1 step is science journalism. Writing popular postings about science journalism is 2 steps away from actual science.

He completely lost perspective, which is not surprising because he himself is not doing any actual original research, the only thing left for him is popular science journalism, but he insists on calling it scientific activity.

When you are far from a certain topic it's hard not to lose perspective. Example: Peter frequently complains about the multiverse. I actually agree, this multiverse business is pretty silly, but if you actually look at the archive and see how many papers there are on the multiverse you will quickly find out that it's a fringe minority. So why bother? Lee Smolin and Peter wants to let people choose their research topics independently and free from pressures. So why not let some weirdos choose esoteric topics such as the multiverse? I personally think it's silly but who cares? It's a fringe minority and what is important some of these people already proved that they are capable of doing real research (such as Linde) so who are we to tell them what to do? Similarly, 't Hooft already proved that he is good, so who is entitled to instruct him that he should not be working on his crazy weirdo deterministic quantum mechanics stuff?

Cheers,
Trent

Cheers,
Trent

23. **Peter Woit**
May 12, 2010

Trent,

By formal QFT I just mean the investigation of the structure of QFTs, often using mathematical methods. This has been going on not for 25 years, but since the invention of the subject back in the 1930s. An example of success would be Veltman-'t Hooft's work on the quantization and renormalization of gauge theories, which was crucial for the Standard Model. Current ideology is that we understand completely gauge theories, that formal theoretical work on their structure is a waste of time. I disagree.

Your comments about the CERN Courier make exactly my point about Duff now doing what he was criticizing Smolin and me for 3 years ago.

I may be beating a dead horse, with the particle theory community completely in agreement with me about the multiverse and string theory unification. I do find some encouragement to continue when I see that instead of just boring people and making them go away, it continues to upset some who want me to stop interfering with the continuing production of hype like Duff's. It's also interesting to note that while half the criticism I get is that I should be ignored since I'm a marginal figure who doesn't know what he is talking about, the other half seems to be that I should be ignored since I'm just saying well-known things that almost everyone in the community agrees upon.

24. **responsible outreach**
May 12, 2010

"So it's okay for CERN Courier to talk in lay-man terms and in hyperbole. As far as actual science is concerned this is pretty much irrelevant just as Peter's blog is irrelevant."

It is far from irrelevant and far from okay (well, lay-man terms are okay, but not hyperbole). Science may not be "done" in the New York Times or the CERN Courier, but it still matters what they say about it because it is the de facto medium through which it is explained to the people who are paying for it. Billions of dollars are spent on projects like the LHC and if we are going to make up bogus feel-good stories about how it may soon verify string theory or how possible applications of string theory in quantum information bolster its potential as a theory of everything, then this amounts to something like fraud.

If we're going to accept that, we should spend the money on something else, perhaps a subsidy for science fiction writers which may get us a better story for far less money. We should also be prepared to butt out and shut up if the Pentagon tell us that things are going well in Afghanistan or BP tells us that the oil spill is under control and not their fault. After all, what do we lay people know that they don't? Do we have better ideas of what to do?

We expect the public media to take seriously their responsibility to report things as accurately as they can. Scientists in turn have a responsibility to help them in this process when it comes to reporting science, whether it is through direct contact with journalists or outreach publications like the CERN Courier. It does not matter that the people reading it know less than you do, do not have better ideas, or do not participate in the science itself.

25. **Trent**
May 12, 2010

Peter,

I thought you mean algebraic field theory. Okay, so let's stick to basic questions about QFT in general. As an example let's take your example, 't Hooft and Veltman. And let's look at the time frame of string theory: let's say past 20 years.

Question 1: What was achieved in the field of "basic QFT structures" in the last 20 years that is comparable to 't Hooft and Veltman?

Question 2: If the answer is "not much", then would you conclude that the field of "basic QFT structures" is dead, because if anything could be achieved, this would have been done in the last 20 years?

Please answer both questions specifically.

You write

“Your comments about the CERN Courier make exactly my point about Duff now doing what he was criticizing Smolin and me for 3 years ago.”

It seems you didn’t get my point which I illustrated with inequalities as well. These two things are not the same: (1) Duff says “Woit is just producing misleading popular science journalism and should be ignored” (2) Woit says “Duff is just producing misleading popular science journalism and should be ignored”. Again, these two things are not the same, although you might lump them together superficially.

Why?

Because Duff is actually doing original research and you don’t. This is not an ad hominem attack, but a factually correct description of reality. So, if we completely ignore Duff as a whole we lose lot of good research and perhaps miss some irrelevant popular science journalism. If we ignore you, on the other hand, we only miss out on irrelevant popular science journalism and nothing else. I hope it’s clear why the situation is not symmetric and it’s perfectly legitimate to have different points of views on you and Duff, although superficially it might look like both of you are doing similar things.

You also write,

“I may be beating a dead horse, with the particle theory community completely in agreement with me about the multiverse and string theory unification.”

You don’t understand my point about the dead horse. The particle theory community is not in complete agreement with you. Reason being is that you think string theory is bad for particle physics and popular science as well. The particle theory community, on the other hand, simply doesn’t care about the multiverse and string unification, because the multiverse business is a fringe topic practiced by a handful of people and whether string unification works or not, is completely irrelevant, nobody cares. If it works, great, if it doesn’t, who cares, something else will work and string theory is useful anyway for other things.

Please note again: you are not in complete agreement with the particle theory community, for the above reason.

You also enlightened us with

“It’s also interesting to note that while half the criticism I get is that I should be ignored since I’m a marginal figure who doesn’t know what he is talking about, the other half seems to be that I should be ignored since I’m just saying well-known things that almost everyone in the community agrees upon.”

You are definitely a marginal figure who doesn’t know what he is talking about, because you are beating a dead horse 🙂 You think string unification is so central to particle physics that all that is good or bad with the subject has to do with string unification. Your interpretation of the social aspects of particle physics is also wrong, if there are no surprising and striking experiments for 20 years, it’s natural that people will move to more formal and mathematical things, such as string theory. Now with the LHC string people get hired less, which is very natural, because new experiments demand new phenomenologists. This phenomenon has nothing to do with the hype, promise, originality, scientific merit, etc, of string theory. In fact, this could have been predicted easily:

no experiments -> more string theory (and other formal stuff)
new experiments -> less string theory (and other formal stuff)

It’s very simple. This is the major reason Lee Smolin and you are both wrong about the social aspects of the particle theory community (not to mention that Lee Smolin is as far removed from experiments as any string theorist).

I hope it’s clear why the two reactions from the particle community to you are compatible: (1) Woit should be ignored because he doesn’t know what he is talking about in general (2) Woit should be ignored because he attributes an enormous amount of importance to well-known things, which are however not very relevant.

Cheers,
Trent

26. **Peter Woit**
May 12, 2010

Trent,

I don’t know who you are, or why you are so obsessed with me and how I choose to spend my time. You’re quite right however that I should be spending more time on my research, so will now do so and stop responding to you, since doing so has been a complete waste of time.

27. **Quantum Information Theorist**
May 12, 2010

Kea says:

Sure, Duff is deluded about strings, but he has done some cool stuff here and if it turns out that the QI is in fact at the foundations of QG, then it will be relevant. And won’t you want to know something about it?

On several occasions when I have tried to suggest to string theorists that quantum information theory might have something to do with the foundations of QG, I have been laughed out of the room.

28. **A String Theorist**
May 12, 2010

Trent says:
“and whether string unification works or not, is completely irrelevant, nobody cares. If it works, great, if it doesn’t, who cares, something else will work…”
Are you nuts? Must of us work on String/M-theory because we sense that it WILL work out and is Fundamental Real Physics. Nice that it is useful elsewhere, but if it were not our best candidate for Unification, it WOULD be a monumental waste of talent and time as far as HEP is concerned.

29. **H-I-G-G-S**
May 13, 2010

That horse isn’t dead, he’s restin’

30. **Thomas Larsson**
May 13, 2010

He is pining for the fjords

31. **Geoff**
May 17, 2010

This is completely off topic as I was unsure where to post it. For those that appreciate wildly inaccurate headlines, you might appreciate this article from Pravda about Perelman:

http://english.pravda.ru/society/stories/07-05-2010/113310-perelman-0

32. **Coin**
May 17, 2010

Geoff: Perhaps I shouldn't pick at this, but do I read this correctly that their source for the entire part on page 2 is some random person who posted on their web forum who claims to know Perelman? I'm particularly fascinated by the bit about the crosses and rosaries and how his "apartment is heavily decorated with icons". That's the apartment he shares with his mother, isn't it? Isn't Perelman's family Jewish?

I showed this article to a friend and they suggested that maybe Pravda was trying to lure Perelman out of hiding by printing an article so intentionally inaccurate that he was provoked to step forward to correct it.

33. **Andrew Zimmerman Jones**
May 18, 2010

In regards to Peter's comment:

> *I didn't write that "string theory is a failure", specifically because "string theory" now refers to very many quite different things, some of which have had successes. However, the idea of using string theory in higher dimensions to get a unified theory has failed, and most physicists are aware of this.*

When I was writing ***String Theory For Dummies***, one of the string theorists who was performing a technical editor role (not Daniel Robbins, who was co-author on the book) claimed that the majority of string theorists *never* claimed that it would unify physics under a single "theory of everything," which I found odd, of course, since most string theorists who talk about it in the public forum use precisely this sort of terminology.

Despite this, the string theorist in question did claim that string theory would still work to unify the four forces under QFT … so I was never particularly clear on his real complaint. I think he just fundamentally didn't like the phrase "theory of everything" itself.

This particular string theorist seemed to take personal affront to a lot of phrasing choices, though. For example, he strongly objected to the suggestion that Richard Feynman received the Nobel Prize for the creation of Feynman diagrams, despite the fact that both the Nobel committee and Feynman himself specifically discussed Feynman diagrams when discussing the work that brought him there.

Toward the end of the editing process, I began to think that he was just looking for reasons to complain. (And no, it was not Lubos. 🙂

34. **Anonymous**
May 19, 2010

In my opinion it may be true that most string theorist are already disillusioned about the prospect of unification, as claimed by Trent and Jones. Hardly any string theory professors I met are fanatic about string theory. One of them teaching a course on string theory openly makes it very clear to students that string theory may have nothing to do with unification, but still it is worth studying for various other reasons. The PhD students doing string theory, as far as I can tell, don't have much faith in string unification either, although when asked about what they are doing by some non-physicist, they always answer they are trying to unify nature's forces.:-) Actually they choose this route simply because they want to get into formal particle theory, and given the current environment it does seem to be a natural choice for someone whose interest lies in this area.

35. **srp**
May 19, 2010

Slightly off topic of QIT, but I would be interested in our host's opinion about this:

http://www.scientificamerican.com/article.cfm?id=simple-twist-of-fate

36. **Peter Woit**
May 19, 2010

srp,

The SciAm article is a bit behind the times, with the string theory/twistor ideas from seven years ago. More recently, there have been quite interesting uses of twistors, but in QFT, not string theory, see some comments here

http://www.math.columbia.edu/~woit/wordpress/?p=1705

37. **Kea**
May 20, 2010

Actually, the SciAm article refers to some recent progress on Hodge's diagrams, although it does not do a good job of explaining this.

Short Items

May 25, 2010
Categories: Uncategorized

- Beam intensity at the LHC continues to increase with successful collision this morning of beams containing 13 bunches of protons, producing an initial luminosity of about 1.5 x $10^{29}cm^{-2}s^{-1}$. This is about a factor of 1000 below the goal for later this year, 2000 below typical luminosities at the Tevatron. The current plan is for proton-proton collisions this year until November 1, then a shift for a while to heavy ions. Integrated luminosity is about 10 nb^{-1}, see a graph here.
- Last week this result (also see here) from DZero got a lot of attention in the press, including a front page story in the New York Times. The claim is of observation of a CP-violating effect not predicted by the Standard Model, with a significance of 2-3.2 sigma, depending on exactly what numbers you look at. If I had to bet, I'd bet that this, like lots of other purported violations of the SM over the last 35 years, will ultimately disappear. Unfortunately, it seems that CDF is unlikely to be able to confirm or disconfirm this. For blog postings from people who actually know something about this, see here and here.
- There's an interesting interview with Shing-Tung Yau at Discover. This fall he has a new book coming out: The Shape of Inner Space.
- My colleague Brian Greene is keeping very busy, with the World Science Festival here in New York next week, and filming a four-part NOVA series based on is book *Fabric of the Cosmos*.
- If I weren't planning a trip to South America to see another eclipse in July, I'd probably be trying to find an excuse to go to Paris that month, and ICHEP 2010 might do the trick. Now, it seems even physics conferences have blogs.
- Herbert Neuberger has a very nice survey here, based on a colloquium talk, of our understanding of non-perturbative QCD.
- Frank Quinn has a long essay here which has quite a few interesting things to say about mathematics, mathematics research, mathematics education, and relations to physics. Quinn is a topologist who has interacted with physicists since the late 80s, in the context of topological quantum field theory. I learned about this from a posting at the n-Category Cafe, where some illustrious commenters have an interesting exchange about TQFT.
- For a long time now, particle theory has been divided up between phenomenology and string theory, with formal QFT an increasingly marginalized subject. At the same time, more and more mathematicians have been studying QFT long enough to become quite expert at it. In the future it seems conceivable that this will ultimately lead to new discoveries about QFT coming out of math, not physics departments. For an indication of how things are going, take a look at the recent MSRI workshop in honor of Alan Weinstein. Videos of some of the talks are supposed to be available at some point. I'm most looking forward to seeing what Graeme Segal has to say about Geometric aspects of the positivity of energy in quantum field theory, and curious to know what Reshetikhin had to say about Hamiltonian structure of gauge theories (it appears that video of that talk will not be available). Next week Reshetikhin is teaching a master-class on gauge theories in Amsterdam (web-site here), and he taught a course on QFT there last fall

Updates:

- See Resonaances for some news that makes the DZero supposed SM violation look less likely. Some support for the claim came from earlier data showing similar SM violation in another channel, but the latest from CDF is that, with more data, this has gone away.
- Latest from the LHC is that on Tuesday morning a peak luminosity of about 2 x $10^{29}cm^{-2}s^{-1}$ was reached. Integrated luminosity is now about 16 nb^{-1}.
- Nature has an interview with Brian Greene about the new orchestral work from Philip Glass inspired by his recent children's book.

Update: A more skeptical report on the DZero result from Adrian Cho at Science Magazine is here.

Comments

1. **Jonathan**
May 25, 2010

   Where are you heading to to see the eclipse? I'll be in Chile at the time visiting some groups in Santiago but might see if I can go see the eclipse. That would make the last three in a row!

2. **Peter Woit**
May 25, 2010

   Hi Jonathan,

   I'll be with a group going down to Patagonia from Buenos Aires. In Patagonia the eclipse will be happening near sunset, and the probability of clouds is high, so the plan involves getting on a chartered plane to get above the clouds at eclipse time. Not cheap, but cheaper than the other South American way of seeing the eclipse, which would be to fly to Easter Island and see it there. But those flights, and any place to stay on the Island, are quite expensive.

3. **Coin**
May 25, 2010

   "The current plan is for proton-proton collisions this year until November 1, then a shift for a while to heavy ions"

   So I'm not sure I understand what the heavy-ion searches are about.

   The proton collisions, as I understand, the point is to probe the particle spectrum. This will likely require a long, high-luminosity run before anything "new" can be found, because a large number of events have to be logged in order to establish a background which interesting events can be distinguished against.

   The heavy ion collisions though as I understand have nothing to do with the particle-spectrum search, and the goal there is to create a quark–gluon plasma (which I assume, like the exotic particles in the proton searches, will last only a short amount of time and be analyzed only by the particle spray it leaves behind?). Will the heavy ion experiments have the property the proton experiments have, where the machine has to run for a very long time in order for any conclusions to be drawn at all?

   Is there somewhere that would be a good source for learning about how the quark gluon plasma analysis works and exactly what we are hoping to learn from it?

4. **Peter Woit**
May 25, 2010

   Coin,

   Good questions. I've seen very little about what people will be looking for in heavy ion collisions at LHC energies. Maybe there's a knowledgeable reader out there…

5. **Tim van Beek**
May 26, 2010

> In the future it seems conceivable that this will ultimately lead to new discoveries about QFT coming out of math, not physics departments.

Is it a coincidence that this statement is similar to the closing words of the (draft of the) preface of “Mathematical Foundations of Quantum Field and Perturbative String Theory” that you can read [here](#) ? (Last paragraph on the page, right above the “authors” list).

6. **Bill K**
   May 26, 2010

   Why, recreating the conditions of the Big Bang, of course. 🙂

   Knowledgeable I’m not, but I can point you to a good summary of what they’ll be looking for:

   http://www.bnl.gov/npp/docs/Hunting%20the%20QGP.pdf

   And I have some opinions. Under the constraints that the LHC is currently operating, I think ALICE is much more likely to produce new and interesting physics than the other experiments. And I think the two months they’ve allocated for it is far too little. Switching over to lead will be more than just flipping a switch – another lengthy tuning process will be needed.

7. **Peter Woit**
   May 26, 2010

   Bill K,

   I’d be curious to see something more LHC-specific. What will the LHC see that RHIC can’t? What will this tell us? Are their predictions of AdS/CFT and other methods that will be tested?

8. **Anonymous Grad Student**
   May 27, 2010

   I attended the MSRI workshop. Forgot I had made some sketchy notes of Graeme Segal’s talk. Here are some scans to tide you over till the video is posted. (No notes for Reshetikhin’s talk, sorry.)

   http://i46.tinypic.com/2s6pjs7.jpg
   http://i46.tinypic.com/r8ukid.jpg
   http://i46.tinypic.com/e9c2md.jpg

9. **Anonymous Grad Student**
   May 27, 2010

   Also, there were no results in Reshetikhin’s talk. He gave the impression that he’s still in the process of learning gauge theory, and just gave some examples of them. I think the talk even ended with “Results: to come”. Possibly why the video isn’t being posted.

   Should have mentioned, take the notes above with a grain of salt. I’m sure there some misrepresentation in there, especially towards the end 🙂

10. **Adam Helfer**
    May 27, 2010

    AGS,

    Your notes look interesting, but I couldn’t tell specifically what was going on. Was Segal trying to look at properties of Hamiltonians of quantum fields in curved space-time? If so, in the case of generic space-times (no timelike Killing vector), these Hamiltonians are pathological, even for linear quantum fields. They’re not only non-positive, they’re unbounded below (this is an ultraviolet problem) and they do not exist as self-adjoint operators, but rather in the weak sense.

11. **Peter Woit**
    May 27, 2010

    AGS,

    Thanks a lot for the notes and the report!

    Adam,

    I think what Segal is interested in is trying to make sense of “Wick Rotation” for a curved space-time. Euclidean signature methods are needed in the path integral formulation of QFT, so having an appropriate notion of how to analytically continue from Minkowski to Euclidean signature is crucial. Even in flat space this is an interesting question which I’m not convinced is resolved when one has spinor fields.

    One could imagine thinking about this by complexifying space-time, and looking at holomorphic objects, but Segal is doing something different. He’s taking a fixed space-time manifold and complexifying the space of metrics on it. From what I can tell, he has some good understanding of what happens in d=2, which is interesting for 2d QFT and string theory, not sure what he has for d=4.

12. **JE**
    May 27, 2010

    Surely off-topic and not really sure of its significance, but just in case… news reports broke in Spain announcing that Professor Francisco Santos of the University of Santander has disproved the Hirsch Conjecture.

13. **Coin**
    May 27, 2010

    JE, that is very interesting… Wikipedia’s “Hirsch Conjecture” article seems to have some information on this already (they link a blog post by Gil Kalai who noticed this as early as May 10):

    > The result is to be presented at the conference 100 Years in Seattle: the mathematics of Klee and Grünbaum… a counter-example was found, using a 43-

dimensional polytope of 86 facets with a diameter of more than 43.

The conference starts July 28, the abstract for Santos' paper is here.

14. **Adam Helfer**
May 27, 2010

Peter,

Thanks. I suppose I should wait for MSRI to finish posting the video before going too far in discussing this, but if one doesn't have some very strong link between the physical metric and whatever the auxiliary structure is (in this case, apparently the space of complexified metrics), it's hard to see what the physical content of the mathematics is.

On the issue of Euclideanization (or, to put it in more physical terms, the pseudo-Lorentzianization) of Minkowski space, I don't know if this comment helps, but the two two-component spinor spaces, that is, the primed and unprimed spaces, have well-defined analytic continuations. What changes is the way these spaces are related to the relevant sense of choices of orientation. In each case, the two spin spaces essentially correspond to the two eigenspaces of the duality operator on two-forms. In the Lorentzian signature, the eigenvalues are plus or minus i, and the spaces are conjugates of each other, and neither is preferred (without more information). In Euclidean signature, however, the eigenvalues are plus or minus one, and so there is an invariant distinction.

15. **Researcher**
June 3, 2010

Another, very sad short item: Vladimir Arnold, a great Russian mathematician, passed away today in Paris, France.

16. **Anonymous Grad Student**
June 9, 2010

Just a heads up. The video for Graeme Segal's talk (along with others from the conference) is now available.
http://www.msri.org/communications/vmath/VMathVideos/VideoInfo/4666/show_video

I couldn't get the streaming video to work, but the download link works fine.

More Quantum Information Theory From String Theory

May 27, 2010
Categories: This Week's Hype

Claims made recently in the CERN Courier that string theory can be applied to Quantum Information Theory (see here) are being followed up with a new paper entitled Four-qubit entanglement from string theory which appears to claim that, despite what some might think, string theory is falsifiable since it makes experimentally testable predictions about Quantum Information Theory:

> Falsifiable predictions in the fields of high-energy physics or cosmology are hard to come by, especially for ambitious attempts, such as string/M-theory, to accommodate all the fundamental interactions. In the field of quantum information theory, however, previous work has shown that the stringy black hole/qubit correspondence can reproduce well-known results in the classification of two and three qubit entanglement. In this paper this correspondence has been taken one step further to predict new results in the less well-understood case of four-qubit entanglement that can in principle be tested in the laboratory.

Previous papers along these lines about the three-qubit case involved some algebra that I referred to as "remarkably obscure", a comment that "was like waving a red flag in front of a bull" as far as John Baez was concerned, leading him to some expository comments about the subject in his latest This Week's Finds in Mathematical Physics. About the string theory claims he comments:

> Unfortunately, Duff gets a bit carried away. For example, he says that string theory "predicts" the various ways that three qubits can be entangled. Someone who didn't know physics might jump to the conclusion that this is a prediction whose confirmation lends credence to string theory as a description of the fundamental constituents of nature. It's not!

Unlike the three-qubit papers, this latest one sticks to mathematics that is not particularly obscure. The mathematics invoked is the quite beautiful subject of the classification of nilpotent orbits in a Lie algebra. I've been trying to learn more about some related topics in recent months, having to do with the role of nilpotent orbits in representation theory. Part of this story involves what are now known as "finite W-algebras", and these have a BRST definition. I've been curious about the relation of this to the BRST/Dirac Cohomology relationship I've been working on.

The mathematical problem at issue here is that of classifying $SL(2,\mathbf{C})^4$ orbits on the four-fold tensor product of $\mathbf{C}^2$. For an exposition of this problem aimed at mathematicians, see these lecture notes by Nolan Wallach. In the new paper, the authors claim that the Kostant-Sekiguchi theorem implies that this classification is the same as that of orbits of SO(4,C) on its Lie algebra, and this latter classification also classifies certain sorts of black holes in supergravity, but I haven't checked the details of this. It's a complete mystery to me why the use of the Kostant-Sekiguchi theorem to relate the straight-forward mathematics used in QIT to a black hole classification problem is going to somehow turn string theory into falsifiable, experimentally testable science.

Comments

1. **from CERN**
   May 30, 2010

   Despite the paper on CERN Courier, the seminar at CERN was attended almost only by some string theorists

2. **anonymous**
   June 2, 2010

   Pete, sorry if OT. From the June 2010 Scientific American issue:

   http://www.scientificamerican.com/article.cfm?id=interactive-12-events

3. **Peter Woit**
   June 2, 2010

   anonymous,

   The one of the SciAm 12 "events that will change everything" that has to do with particle theory is the possible discovery of extra dimensions, that is assigned a probability of 50% by 2050. It's true that would be a huge event in the history of science, but I think most theorists would put the probability one or more orders of magnitude below 50%.

   The article mistakenly claims that supersymmetry requires 10 dimensions, mixing up supersymmetry and string theory (string theory isn't mentioned at all).

   There's a quote from Arkani-Hamed:

   "So while extra dimensions would be a terrific discovery, at a deeper level, conceptually they aren't particularly fundamental'.

   In some sense I agree, although perhaps with a different point of view. Extra dimensions don't solve any fundamental problem that we care about, much more interesting would be a conceptual breakthrough that would solve such problems.

4. **Bill K**
   June 2, 2010

   Extra dimensions are the last thing we need. It's hard enough trying to explain the existence of the first four!

5. **Confused Student**
   June 3, 2010

   Does the inverse scattering transform method for kdV equation make falsifiable predictions from the Schrodinger equation and hence validate the basic equation of quantum mechanics? Is this the major success of quantum mechanics?

6. **anonymous**
   June 3, 2010

   Thanks for your answer, Pete.

Althought the article must be judged by its genre (journalistic science counterfactual forecasting?) when reading it i had the impression that present physics much more open than i think it is.

For instance the 50% issue: i´m not even sure that to assign probabilities to such (or similar) events is correct. If it is, 50% must be interpreted as if we had not any information about if the universe has 3+1 dimensions or more, but as you say “most theorists would put the probability one or more orders of magnitude below 50%”. This issue is not a fair coin, it´s clearly biased !

On the other hand despite Arkani-Hamed comment I still find highly unsatisfactory that SM is compatible with extradimensions, if only for parsimony.

In any case, congratulations for your blog!

7. **bane**
   June 4, 2010

   @anonymous.

   The assginment of probabilities to those events is perfectly foundationed interpreting them as Bayesian probabilities incrorporating whatever theoretical prior beliefs. The biggest difficulty is that (assuming that if extra dimensions exist they will be detectable by 2050) it seems almost all relevant physicists either believe theories that say there are extra dimensions with a tiny chance of being wrong, or that there aren’t extra dimensions for parsimony reasons with a tiny chance of being wrong. So all this probability really represents is what the writer (Sci Am, Peter) thinks the relative proportions of the two groups is.

8. **Pawl**
   June 4, 2010

   bane,

   The problem is not just prior theoretical prejudices. It is that the “large extra dimensions” scenarios are not at all developed theories, but guesses about what might possibly be achieved if somehow the apparently enormous problems involved in developing theories to support the scenarios might be overcome.

   Any reasonable estimate of their chance of being true must take into account: (a) the magnitude and complexity of these difficulties; and (b) the odds that, even if those are overcome, the original guesses will turn out to have been pretty much correct — something which is hardly assured given the depth of the unresolved problems.

   The 50% figure is preposterous from this point of view.

9. **anonymous**
   June 8, 2010

   bane,

   It is not that i want to revive the probabilities interpretation debate, but i´m classical or ortodox on this: let the probabilities for real or theoretical systems which fit into Kolmogorov axioms.

   On this sense the posibility of asigning probabilities to such “universe property events” as the number of dimentions seems tied to the belief either on some kind of multiverse or sequential toss, on which 50% of the universes have extradimensions and 50% not. This makes no sense if the universe had unique properties (i.e. the universe properties are always the same after every sequential or parallel universe toss).

   On the other hand the sentence “I think most theorists would put the probability one or more orders of magnitude below 50%” seems clear enough.

Quick Links

June 3, 2010
Categories: Uncategorized

- Several blogs have now pointed out the wonderful snarXiv site, which automatically produces random plausible-sounding hep-th entries. It's something along the same lines as the famous Postmodernism Generator. For entertainment, you can try playing snarXiv vs. arXiv.
- HEPAP is meeting in Washington today, presentations here
- At Fermilab, the User's Meeting continues today, talks here.
- Last week was Brookhaven Forum 2010, talks available here.
- Talks by Michaels Atiyah and Green at the Young Researchers in Mathematics 2010 meeting are available here. Atiyah reminisces about his early career and offers a lot of helpful career advice for young mathematicians. For the future, he's betting that topology and symmetry will remain crucial themes in mathematics, with solitons continuing to be interesting since they live in the intersection. Green gives a very standard promotional talk on string theory. For some reason he explains to the young mathematicians the story of the string theory anthropic landscape explanation for the CC, then remarks that he personally finds it to be a cop-out.
- The Edge web-site has a wonderful discussion with my Columbia colleague Emanuel Derman, covering his early career in particle physics, his days as a Quant, and thoughts about the current state of the finance business.
- From Fabien Besnard, the news that Vladimir Arnold died today in Paris.

Comments

1. **ignatz thugg**
   June 3, 2010

   try this link:
   http://pdos.csail.mit.edu/scigen

2. **Tim van Beek**
   June 4, 2010

   To stand a chance at "snarXiv vs. arXiv" I would need the abstracts, I guess, with the titles alone I don't get above 70%.
   What got me in the first run was, for example, "non-compact compactification" (that's probably too absurd to be computer generated).

3. **CWJ**
   June 4, 2010

   snarxiv is hilarious. Another one, though more sad (because the posters are completely serious), is viXra.org.

4. **nbutsomebody**
   June 4, 2010

   Arnold's death is really a sad news.

5. **D R Lunsford**
   June 4, 2010

   Thanks for pointing out vixra.org – some of that stuff is really entertaining 🙂 and some of it really is serious. That is, it's not a joke.

   -drl

6. **Sakura-chan**
   June 5, 2010

   DR,

   One of the regulars on that site is a Mister Victor Porton, who believes his math discoveries have earned him the Abel prize:

   http://www.mathematics21.org/abel-prize.html

7. **Peter Shor**
   June 5, 2010

   Thanks for the link to snarXiv. One of the papers that got me had "new old inflation" in the title … I figured that one couldn't possibly be real. Wrong!

8. **Marcus**
   June 5, 2010

   When you get 4 out of 4 right it calls you "Nobel Prize winner" and then if you get 5 out of 5 it says "Ed–is that you?", beyond that I have not tried to go.

9. **Chris Oakley**
   June 6, 2010

   Porton's request reminds me of a Russian beer ad I saw once. I cannot remember the brand, but the poster showed a bottle with the caption: "Buy our beer. We need your money."

10. **Ed**
    June 6, 2010

    > and then if you get 5 out of 5 it says "Ed–is that you?"

    No. The Ed sentence is random for 100% results.

11. **Marcus**

June 6, 2010

Ed, I only made 5 tries in all and got 5 out of 5, all I know is that is what it said. Didn't explore further. You could be right.

12. **PhilG**
June 6, 2010

I got to a Nobel Prize straight off, then got them all wrong after that until I was worse than a monkey.

13. **Hendrik**
June 7, 2010

Another topic:- Witten's string-twistor marriage is getting an airing at:

http://www.scientificamerican.com/article.cfm?id=simple-twist-of-fate

Where Do They Go?

June 6, 2010
Categories: Uncategorized

HEPAP has had a Demographics Committee since about 1999, charged with gathering data on what happens to young people in the US who enter the field of High Energy Physics (both theory and experiment). The latest report from the committee is here, but it contains more questions than answers. The data gathered show that only 10-20% of HEP graduate students end up with permanent tenured positions at HEP institutions, and the other 80-90% in some sense "leave" the field.

The committee seems to have had very little success at finding out what happens to the "leavers", perhaps because its data-gathering method is based on questionnaires filled out by one person at each institution. Very typically, once someone "leaves" academia for a different career track, within a few years their ex-colleagues no longer know where they are or what they are doing. On the other hand, in the age of Google and Facebook, tracking people down has become rather easy, so it's unclear why an effort hasn't been made to do this, if not for everyone in the database, than at least for a randomly chosen statistically significant sample.

I'd certainly be curious to see some real data, but based on my personal experience I'd guess that the 80-90% number sounds right, with "leavers" going into a wide variety of different careers. The financial industry may be the most popular, but I also know many who have gone into the computer or telecommunications industries, as well as other fields in academia.

By this count, I and others who have ended up in mathematics departments count among the "leavers". I'm very happy with how my own rather unusual career path has worked out, but have generally advised others that it relied too much on good luck for anyone else to try and emulate it. A few days ago I heard from someone at the Perimeter Institute who told me about a new "hybrid research/IT position" that they are trying to fill. The job listing is here, with a detailed description of what they are looking for here. They seem to be a looking for a candidate with both a research Ph.D. and IT experience, which is a somewhat unusual combination. A Ph.D. with little IT experience but the right skills (quick learner with patience, common sense, enjoys working with technology and helping others with their technical problems) who is interested in the position might want to try and convince them that most of the IT skills get picked up on the job anyway…

Comments

1. **John Armstrong**
   June 6, 2010

   tracking people down has become rather easy, so it's unclear why an effort hasn't been made to do this

   It's perfectly clear: people like me who have been cast out of the academy are apostate.

2. **Kea**
   June 6, 2010

   And the job sites are still full of ads begging for ~~exploitees~~ students.

3. **Tim van Beek**
   June 7, 2010

   > a new "hybrid research/IT position"

   That sounds familiar, the software company that I work for offers similar jobs, where you have to combine some familiarity with a field that uses IT heavily (finance, telecommunication, car producers), with some IT knowledge. You evaluate new technologies and create solutions for standard problems. In this case this could be something like "we need a programming language for a multithreading Monte Carlo simulation of lattice gauge theories"…and you would have to be able to recommend a programming language, an integrated development environment, a visualization and evaluation framework, a set of trusted pseudo random generators etc.

   (Preferably as a single download self explaining package 🙂
   It does not make any sense if everyone who needs to do a Monte-Carlo simulation looks for and evaluates pseudo random generators all by himself, and it is impossible to do that for every component you need anyway.
   Same goes for all the other IT-topics of interest to the research community.

4. **Anon**
   June 7, 2010

   Can someone clarify the Departure Table (page 7) for me? In the first column, we have HEP, Other Physics, blah blah blah. What does the HEP row mean? Does the HEP row mean that they were initially 115 students? But then how can one lose 118 students to Unknown (last row of the first table on page 7)?

5. **Anon**
   June 7, 2010

   Oops! Ignore the previous post. I just found out that departure just means change of institution or rank. So HEP is just change of institution to another one.

6. **Thomas R Love**
   June 7, 2010

   peter wrote:
   I'm very happy with how my own rather unusual career path has worked out, but have generally advised others that it relied too much on good luck for anyone else to try and emulate it.

   Writing an excellent book didn't hurt your career

7. **Peter Woit**
   June 7, 2010

   Thomas,

   I only was able to write the book because I had a permanent academic job that gave me the time, energy and people to talk to necessary to learn about, then write about, the topics of the book. Finding such a position required a certain amount of luck.

8. **D R Lunsford**
June 8, 2010

Well I never had a ghost of a chance to “stay” vs “go”, for many reasons – but I don’t feel as if I myself went anywhere – I’m still doing physics the way I learned it – but I strongly feel that academic life left me, and not the other way.

I feel much worse for engineers than I do for us physicists.

-drl

9. **Belizean**
June 8, 2010

The thing that irks me is that NSF, DoD, etc. are still funding efforts to increase the number of graduates in fields of Science Technology Engineering and Mathematics (STEM). I’m forced to mention this in just about every grant application that I write.

They should instead fund efforts to *decrease* education in STEM fields, as there is very obviously a glut.

10. **naezileb**
June 9, 2010

“Science” includes also things like biology and biotechnology. There may be a glut of graduates in HEP theory, but is there a glut across all of STEM?

As for the 80-90% “leavers” if the statistics reveal that the leavers earn three times as much as the stayers (D R Lunsford?), there may not be great incentive to advertise the fact.

11. **D.**
June 9, 2010

Compare the guy on his third postdoc to the same guy joining a hedge fund soon after his thesis defense and the multiple is more like 8, unfortunately. This doesn’t address opportunity cost etc.

Prerequisites

June 9, 2010
Categories: Uncategorized

The Abstruse Goose recently provided an excellent summary of how to go about learning string theory. It starts with "String Theory for Dummies", here. Tommaso Dorigo's latest say of the week is somehow relevant.

Comments

1. **ampm**
   June 9, 2010

   There's commentary on those who wish to use mathematics to study gauge theories, too.

   http://abstrusegoose.com/105

2. **Giotis**
   June 9, 2010

   He forgot to include Supersymmetry and Supergravity.

3. **Coin**
   June 10, 2010

   I would be legitimately interested in an entry-level introduction to string theory, especially if it focused on string theory as a mathematical framework rather than string theory as a physical theory.

4. **Tim van Beek**
   June 11, 2010

   @Coin: You could try "A mathematical introduction to string theory. Variational problems, geometric
   and probabilistic methods." by Albeverio, Jost, Paycha, Scarlatti.
   This is a mathematically sound exposition of the quantization of the relativistic bosonic string using the Polyakov functional integral (so it is not an introduction to "string theory", as the title implies, but only to one aspect, but mathematically rigorous).

5. **Coin**
   June 12, 2010

   Tim, thanks! I'll try to take a look and see if I can handle it…

Through the Wormhole with Morgan Freeman

June 9, 2010
Categories: Uncategorized

The Science Channel is starting up yet another show on physics tonight, with Michio Kaku's Sci-Fi Science and Into the Universe with Steven Hawking being joined by Through the Wormhole with Morgan Freeman. The topics being covered by Freeman are the usual ones: Black Holes, Aliens, Is Time Travel Possible? What Happened Before the Big Bang? etc.

The series unfortunately first starts out by bringing religion into it, with an episode called Is There a Creator?

> Did our Universe just come into being by random chance, or was it created by a God who nurtures and sustains all life?

I gather that the episode begins with speculative physics elements that include Alan Guth on the multiverse and Garrett Lisi on E8 unification, but then moves on to speculative God stuff, with a neurophysiologist followed by the "maybe we're just a simulation" business. The New York Times today has a depressing review, by a writer who wants more of the God part and less physics:

> …this opening installment, which is supposed to be about whether there's a Creator, almost immediately degenerates into theoretical yakking by scientists about unified theories of this and missing particles of that.
>
> Especially with recent news coverage of that particle accelerator near Geneva, it seems as if we'd been hearing about this type of physics for a long time, and the discussion never does go anywhere or have much practical relevance. Anybody got a particle big enough to plug that busted oil pipe in the Gulf of Mexico?
>
> Anyway, after about half an hour, Mr. Freeman's show does get intermittently interesting because it turns itself more directly to the Creator question. (Questions are pivotal to this series; future episodes include "How Did We Get Here?" and "Are We Alone?") Doesn't answer it, of course, but does check in on an assortment of scientists who have an assortment of theories.
>
> One thinks our idea of God is a kind of neuropsychological tic and plunks a ridiculous-looking contraption he calls a God helmet on research subjects' heads to try to prove it. Another suggests that we're nothing but a computer simulation created by our own descendants. If this program can stay away from same-old science and work this territory — theories that sound a little bit crackpotish, a little bit geniusy — it might set itself apart in an increasingly crowded genre.

Update: Chad Orzel weighs in here.

Comments

1. **Grétar Amazeen**
   June 9, 2010

   I don't know what is sadder, the show or the review. The world needs another Carl Sagan desperately!

2. **Garrett**
   June 9, 2010

   Sadly, I'm no Carl Sagan.

   I hope this episode isn't terrible. I've only seen parts of it. I gave the producers lots of advice, and even introduced them to the Science & Entertainment Exchange. They had their own ideas and agenda though, and did their own thing. I'm mostly just hoping I don't get What the Bleeped, or pronounced a physics prophet.

   The only thing about it that I know is good is that Morgan Freeman will talk a bit about Lie groups. That I'm happy about. The God stuff, not so much.

   And, err, there is a decent shot of me surfing.

3. **Amos**
   June 9, 2010

   How's the e8 work coming then?

4. **Garrett**
   June 9, 2010

   E8 work is going well. Short paper coming soon — probably out next week.

   (Peter, feel free to curtail any inappropriate threads if we're drifting off topic.)

5. **Mike**
   June 9, 2010

   I confess I am a mere consumer of such programming, and though I have not derived any of the force calculations referenced, I am impressed with the show. "More questions than answers" was promised, but a deeper dive than expected is the result. I will now research into Lie groups, because I really want to understand the math behind them, though I probably never will, fully. Was that really the north shore you were shot at–excellent surfing there!

6. **Chris Oakley**
   June 10, 2010

   Morgan Freeman talking about Lie groups? I just checked his wiki page and his qualifications in this regard seem to be that (i) he was a mechanic in the USAF and (ii) he has played God twice in movies. (i) requires (only) a working knowledge of O(3) and its subgroups, so I guess that he is getting in under (ii) which, I suppose would qualify him for just about anything (anything *good*, anyway).

7. **Tim van Beek**
   June 10, 2010

> The New York Times today has a depressing review, by a writer who wants more of the God part and less physics…

Well, there *has* been a lot of coverage about the LHC, all the physicists *keep* on saying the same things, the discussion *is not* moving anywhere, and, frankly, I would like a well done documentary about different notions of "God" in the world religions of the present day and the past **much** more than anybody telling the same over and over again how the LHC could detect additional dimensions.

Yes, it is depressing, but that's not the fault of the reviewer 🙂

8. **Jeff Olszewski**
June 10, 2010

Well Dr. Lisi, your comments are spot on as usual. However, while it is true that you are no Carl Sagan (yet), I wouldn't undercut the influence that your theory is having today, and will likely have in the future on physics. A buddy called me up last night and said "get to the TV now, Morgan Freeman is doing a spot on Dr. Lisi". I enjoyed the show, even the God speculation, because I'm not a hard core physicist. One thing for sure, your legend is growing, in no small part because you are likely the only world class physicist who also surfs. Remember the movie line "Charlie don't surf"? Well, "Garrett surfs" may someday be a T-shirt. Keep at it!

9. **Chris Oakley**
June 10, 2010

Jeff,

Your comment suggests a remake of *Apocalypse Now* with the String Theorists as the VC, Peter Woit as Col. Kurtz and Garrett Lisi as Lance the surfer.

E.g. Captain Willard [not sure who that would be] looking at Peter's dossier on the boat:

> At first, I thought I had wrong dossier. I couldn't believe they who they've had for their enemy! Third generation Harvard, top of his class. Numerous degrees. About a thousand prizes. Etc, etc… I mean, I know this man, or at least I think I know him better than others do, but… I couldn't connect who he was then with who is he now.
>
> He had an impressive career. Maybe too impressive … I mean perfect – Harvard, Princeton, PhD with Callan. Oh! He was being groomed for one of the top slots of the corporation. Professor, Head of Department, anything … In '04 he returned from a lecture tour and things started to slip. The report to the DoE was highly restricted. Seems they didn't dig what he had to tell them. During the next several months he made four requests for transfer back to physics. And he was finally accepted. Physics blogging? He was 44 years old, jack. Why the f-ck would he do that?

10. **Peter Woit**
June 10, 2010

The horror… the horror…

11. **MN**
June 10, 2010

"One thing for sure, your legend is growing, in no small part because you are likely the only world class physicist who also surfs."

This is so scary in so many ways.

Lisi is no "world class physicist" (he seems honest and might even agree) and the fact that surfing makes one's "physicist's legend" status grow just shows that fans such as "Jeff Olszewski" are far more dangerous to science than your average creationist crank.

Science is about describing the physical world.
Lisi made no advances on this (public at least).
Surfing and other wet dream fantastic irrelevancies such as the trash this post describes are just substitutes for the ignorant masses and these masses just keep asking for more (see the NYT review).

"The horror… the horror…"

Indeed.

12. **anon.**
June 10, 2010

There are actually a lot of world-class physicists who surf. Which way the causation goes between this and the KITP being located on a beach, I don't know.

13. **Thomas Larsson**
June 10, 2010

Hm. If KITP is located in the same building as the ITP was when I was a grad student, it is not located on the beach, but on the UCSB campus on the top of the cliffs. Now, *I* was frequently located on the beach (some of the tan still remains after 27 years), but that didn't prevent me from being the only one in my class to pass the qualifier as a freshman.

14. **Chris Oakley**
June 10, 2010

MN,

Your comment is mean-minded as Lisi is certainly a world-class physicist.

The question is more whether he is on to something or not.

Personally, I doubt it, but if unrealistic expectations have built up, then he is probably just as embarrassed as you are.

15. **Tim van Beek**
June 10, 2010

> …Lisi is certainly a world-class physicist.

I vote to make physics an olympic discipline, then we would all know who is world-class and who is only replacement bench material 🙂

16. **Anonymous**
June 10, 2010

Found this blog in a Google search for this show.
Just wanted to share my opinion:
This first episode's religious theme was a turnoff to me. I watch the Science Channel for science, not for religious BS, so I decided to skip this one. I may watch (some of?) the other episodes, but I wasn't very attracted to spending an hour watching "speculative God stuff".

17. **H-I-G-G-S**
June 10, 2010

Dr. Lisi has world-class fame as a physicist, even if no actual physics was done while achieving this fame. I'm pretty sure that is all that matters to the producers of this show.

18. **Hendrik**
June 10, 2010

Garrett, good to hear E8 is going well. Maybe I missed it, but was there an answer to:

http://arxiv.org/abs/0905.2658

19. **Shecky Riemann**
June 10, 2010

Are you sure that guy is writing for the NY Times?… sounds more like the NY Post!

20. **Risco**
June 11, 2010

"I think that's what is so fascinating with that episode especially with Lisi's theory of creation because he himself is an atheist and he totally accepts and understands that his theory can prove the existence of God and that's one of those wonderful conundrums. We wanted to explore God and science, not God versus science."

That's from the producer. Garret?

21. **Risco**
June 11, 2010

http://www.surfingtheuniverse.com/Surfing_the_Universe/Welcome.html

And, ?

22. **D R Lunsford**
June 11, 2010

Shecky, the poor guy is just the theater and TV critic – and that's the problem with these shows – they are so inchoate and filled with idle speculation that they cannot be reviewed based on any meritorious content, as could say the NOVA shows about the standard model, broadcast in the late 70s. These science shows are no better in fact than the endless stream of drivel about chucucabras, ghosts, aliens, angels etc. that also gushes from these cable networks.

-drl

23. **Ken**
June 14, 2010

I have no extensive education in physics, theoretical or otherwise. And after second year calculus, I was lost. But I must say I did enjoy this first episode of "Through the Wormhole." The theories presented were done in an interesting way without any perceived bias.

When the discussion of the origin of the universe crops up, the intelligent design debate is guaranteed to be close at hand. As Aristotle said, "It is the mark of an educated mind to be able to entertain a thought without accepting it."

I am the general public to whom I believe the series is targeted. I liked it and will watch the rest.

24. **david**
June 16, 2010

It seems obvious that this NYT writer knows next to nothing about the genre of theoretical physics. Apparently all he wants to hear is that the God he learned in Sunday school created everything on the SCIENCE channel. He denounces science based on the fact that the answers to all the questions havent been discovered in the 2 years the LHC has been working like it should be been all figured out over night. He doesn't want to hear about "theoretical yakking by scientists about unified theories of this and missing particles of that." Einstein died with this question in his head. Perhaps getting an understanding for a subject before spitting out an ignorant review on it would be a swell idea.

25. **Marc**
June 23, 2010

When Garrett says of the producers "They had their own ideas and agenda though, and did their own thing" he hits the nail on the head. There are some interesting gathering of facts, but i am quite sick and tired of pseudo science shows like "monster hunters" and "ghost investigators" and such, posing as real science to push an agenda. Unfortunately a small part of shows like these leak out as sound bites and get twisted and used. For instance, i had a discussion with a co-worker; he cited the existence of the "God Particle" to prove that scientists are forced to admit the existence of his christian god!
oh well, the world will end in a couple of years anyway…

26. **Researcher**

June 27, 2010

Just a small side note: Garrett's new preprint is already out there:

http://arxiv.org/abs/1006.4908

27. **Garrett**
June 28, 2010

Yes, this paper answers the question of whether or not a generation of fermions embeds in E8. Curiously, Distler's lengthy response came within minutes of the paper appearing on the arxiv. Getting a few days' lead on new papers is one of the perks of being on the arxiv board I suppose.

28. **Peter Woit**
June 28, 2010

Thanks Garrett,

The introductory section of that blog posting is pretty remarkable. I'm still wondering what argument the hep-th moderator uses to justify censoring trackbacks to this blog, thought maybe it was that I'm sometimes obnoxiously snide. But now, it's clear that that can't be the argument….

29. **H-I-G-G-S**
June 28, 2010

Garrett,

The time stamp on Distler's blog energy is 9:17pm. I presume this is CST since he is in Texas. ArXiv papers are available starting at 7PM CST, so that is 137 minutes, a quantity of time rarely referred to as "within minutes." I don't see any reason why he couldn't have written the post in that amount of time. After all, your recent paper in no way contradicts the results of his paper with Garibaldi.

30. **Peter Woit**
June 28, 2010

H-I-G-G-S,

Lubos is the only one in the business who can read papers and write blog articles at that length (with graphics!), that fast…

In any case, as far as I know there's no rule against arXiv moderators reading papers as they come in. If you care about this issue, you should ask Jacques himself.

31. **jpd**
June 28, 2010

"your recent paper in no way contradicts the results of his paper with Garibaldi" i agree with that, you agree with that, i even think Distler agrees with that. i am not sure why he takes it so personally. its just a question of definitions.
until a prediction comes out, both Lisi and Distler are not even wrong.

32. **H-I-G-G-S**
June 28, 2010

Peter, I don't care about it, I was simply correcting the impression left by Garrett's incorrect description of the time involved. People who are careless with the truth about small things also tend to be careless with larger things, in my experience.

jpd, it is not a question of definitions, it is a question of physics. In particular it is a question of how chiral fermions work, the difference between real and complex representations, and the physical implications of having mirror fermions or anti-generations in Distler's language. Here Distler is definitely correct and Garrett is engaging in wishful thinking.

Not In Our Lifetimes

June 9, 2010
Categories: Uncategorized

A report from one of last Saturday's events at the World Science Festival has string theorists Brian Greene and Shamit Kachru admitting that they'd be surprised to see experimental evidence for string theory in their lifetimes:

> John Hockenberry, the panel's moderator, asked Greene if he thought experimental evidence would come during his lifetime.
>
> "I'd be surprised," said Greene.
>
> "And in your lifetime?" Hockenberry asked Kachru.
>
> "…I'd be surprised," conceded the young physicist reluctantly.

For more reports about the same panel discussion, see here and here.

Comments

1. **ludwig**
   June 9, 2010

   Ludwig Boltzmann saw no evidence for the atomic hypothesis in his lifetime, underlying his work on kinetic theory, microstates and entropy and the Maxwell-Boltzmann distribution.

   And he committed suicide.

   Which is one way to guarantee that one won't see results in one's lifetime.

   A role model for the ages?

2. **Chris Oakley**
   June 9, 2010

   The moderator seems to have avoided the obvious follow-up question, i.e. "Why the f*** are you still working on it, then?"

3. **Mantis**
   June 9, 2010

   For the vast majority of theorists the fact their theories cannot be verified in their life time is a blessing!

   Only a tiny amount of theoretical ideas turn out to be correct in the end. If verification were straightforward almost all theorists would see their vacuous ideas butchered by experiments.

   The fact there is no way to separate the wheat from the chaff brought progress in fundamental physics to a halt.

4. **Wyman**
   June 9, 2010

   Boltzmann saw no evidence of the atomic hypothesis? The man pioneered statistical mechanics! Just because there was still resistance to the atomic hypothesis does not mean there was no evidence–Dalton saw evidence of the atomic hypothesis before Boltzmann was even born.

5. **Thomas Larsson**
   June 10, 2010

   I expect to see the disproof of supersymmetry, and thus indirectly of string theory, at the LHC in my lifetime.

6. **Anonymous**
   June 10, 2010

   Can we even see a *defining equation* of string theory within our lifetime?

7. **Giotis**
   June 10, 2010

   The real power of String theory is that it *explains* things in a consistent way and does not just describe them. This is essential for a TOE. If someone could give a better explanation for the origin of the field content in 4D for example then we could compare the two approaches. Until then String theory is the only viable explanation and thus it would be very interesting to see where it can lead.

8. **Peter Shor**
   June 10, 2010

   Giotis,

   I keep hearing this, and I keep asking how string theory explains the black hole information paradox (how does information get out of a black hole?), and I keep getting completely incoherent answers. Maybe I'm dumb, but it certainly doesn't explain everything to me.

9. **Bob Levine**
   June 10, 2010

@giotis:

The real power of String theory is that it *explains* things in a consistent way and does not just describe them.

While I'm not dead keen on a lot of what she says, Nancy Cartwright in one of her books notes that explanatory success accompanied by descriptive failure is a bad way to do science, at least over the long haul. Given its detachment from any observable context which could constitute an empirical test, it's hard to see any descriptive efficacy in ST. And we've yet to be given any real reason to believe that the theories in physics which *do* connect with observation fall out as theorems from ST. So exactly what is the explanatory triumph you appear to be attributing to ST which redeems its descriptive failure ?

10. **a quantum diaries surivivor**
June 10, 2010

Let me play the devil's advocate for this once.

The question involved the time the two scientists think -or are willing to state they expect- to live, together with the time before they think a first evidence for string theory will concretize. So they had to give a statement on the ratio R=T_s/T_d between the time to an evidence for string theory and the time to their death, and they both said they would be suprised if this ratio R turns out to be smaller than 1.

If you consider it this way, claiming that sihce they believe that R>1 they should work on something else makes as much sense as saying that the Tevatron should not search for the Higgs boson, given that the expected reach on the ratio R between the observable Higgs rate and the expected SM one is larger than unity…

Cheers,
T.

11. **Dave B**
June 10, 2010

Or maybe they think that we will all be eaten by a black hole before we get to the energies we need to test string theory 🙂

/gets coat & sneaks away

12. **Peter Woit**
June 10, 2010

I think that what is more relevant than specific estimates of the distance to the goal of string theory unification is the time evolution of these estimates.

1985: String theory enthusiasts were very excited, many were convinced that evidence would be found within a few years, say less than five.

2000: A typical estimate might be more like 20 years to go. See
http://www.longbets.org/12 where in 2002 Kaku and others were willing to bet money on superstring unification before 2020.

2010: Given normal life expectancies, we're now talking about more than 40 years from now as an estimate from string enthusiasts about when they will have evidence.

13. **dan**
June 10, 2010

@Thomas Larsson – how would LHC DISPROVE SUSY? If SUSY is not seen couldn't that be consistent with a breaking scale above LHC energies?

Are there genuine particles or phenomena the LHC could plausibly see, such as technicolor, or Top-quark assisted technicolor, 4 generations of SM, preons, higgless EW breaking, or the non-detection of SUSY partners such as little Higgs, that would definitively discredit SUSY?

14. **Mitchell Porter**
June 10, 2010

I spent the last month studying something out of string theory – the F-theory GUT models – because they very clearly have a chance of looking like reality. It seems inevitable either that the F-theory program will issue in the discovery of string-theory backgrounds which are completely consistent with experiment and cosmological observation, and in which all Standard Model parameters have a concrete explanation in terms of compactification, brane configuration, and various "fluxes"; or that the program will result in an impossibility theorem, to the effect that such models can't do the job after all. I'd expect one outcome or the other within a few years, and obviously it's big news either way.

So I struggle a little to understand what Greene and Kachru are thinking. But the panel discussion was about extra dimensions, so probably they were talking about direct experimental evidence for extra dimensions, such as missing energy. The F-theory models prove that you can already interpret particle physics in terms of specific higher-dimensional phenomena, so I would have thought there's a very good chance that we'll have indirect arguments for extra dimensions very soon.

15. **anon.**
June 11, 2010

I haven't followed all the latest papers, but early on F-theory GUTs ignored all moduli stabilization problems, and had at best a hand-wavy argument for preferring (fairly high-scale) gauge mediation to gravity mediation. (My experience is that gauge mediation tends to be delicate — it's difficult to stabilize all moduli and cancel the C.C. without breaking SUSY.) So there's a long chain of tenuous arguments that end up predicting — minimal gauge mediation! Which, of course, predates F-theory GUTs by many years. Seeing it wouldn't convince anyone of anything about high-scale physics, beyond the zeroth-order question of what scale SUSY is broken at.

16. **Rudigger**
June 11, 2010

I'd be surprised if string theory makes a definite prediction in my lifetime.

17. **Anon2**
June 11, 2010

Even if you can't get falsifiable *predictions* from string theory, you could still falsify some of the assumptions it is built upon. E.g. spin-2 gravitons seem to everyone to be logical and necessary and require some kind of stringy framework unlike the spin-1 vector bosons in the Standard Model, but suppose gravity doesn't conform to the expectations of Pauli and Fierz, and isn't spin-2. It's looked logical to Ptolemy to model the sun and stars daily orbiting the Earth… things did not turn out to be that simple.

Like Ptolemy's epicycles, string theory is an ad hoc mathematical explanation for a widely held prejudice which still hasn't a shred of experimental evidence behind it. Maybe we need a new Kepler.

18. **AdSCFTfan**
June 11, 2010

OK, maybe string theory has not predicted anything definite about the real world yet. But via the AdS/CFT correspondence it has made many precise predictions about some gauge theories at strong coupling, like the N=4 supersymmetric Yang-Mills.

These predictions are testable. In fact, some of them have been tested by solving for some qauge theory quantities as functions of the coupling and comparing with string theory at strong coupling. It works!

Maybe this is not enough to satisfy some of you critics, but I find this amazing.

19. **Fluffy Eschaton**
June 11, 2010

AdSCFTfan: Woit has been fairly consistent in saying that it's string *unification* he thinks hasn't panned out, not all the mathematical developments associated with string theory.

20. **Remo**
June 11, 2010

> @Thomas Larsson – how would LHC DISPROVE SUSY? If SUSY is not seen couldn't that be consistent with a breaking scale above LHC energies?

Dan, if no hint of susy were observed at the LHC it would mean, as you say, that its scale is significantly higher than 10 TeV. In that case it would lose most, if not all, of its phenomenological appeal.

21. **AdSCFTfan**
June 11, 2010

Fluffy Eschaton:
Thanks for the clarification. Why is this blog still called `Not Even Wrong' then? Them there harsh words…
Isn't it time to admit that string theory gets some things right? Hey, you never know: the real world may budge next!

22. **Mitchell Porter**
June 12, 2010

anon., what I anticipate is that a specific higher-dimensional effect may become the standard explanation for a specific unexplained feature of the Standard Model. The analogy is with SU(5) GUT explaining the weak mixing angle and supersymmetric GUTs providing exact unification of running coupling constants. For example, the F-GUT explanation of flavor hierarchies (arXiV:0906.0581).

23. **Giotis**
June 13, 2010

Dan is right. Even if LHC falsifies SUSY in 4D it doesn't mean that SUSY doesn't exist in 10 or 11 dimensions i.e. in the energy realm of string/M theory. Supersummetry is inevitable in String theory for internal consistency reasons and not because it is a good candidate for physics beyond the standard model in 4D.

So even if LHC falsifies SUSY in 4D they could always construct models that break all the SUSY in 4D upon compactification. They will have to search for a different kind of vacua that's all.

24. **dan**
June 19, 2010

@ Remo, Michio Kaku, Lubos and other string partisans have argued that it is merely a limitation of collider technology that prevents experiments above 10 TEV scale. If SUSY is broken above 10 TEV, and there were colliders that could explore this scale, wouldn't it be phenomenologically interesting? Maybe not stabilizing EW scale but perhaps DM candidates?

@ Giotis the successor to CDMS II is SuperCDMS. It evidentally has the sensitivity to detect neutralinos over its entire parameter space.
ref:

http://arxiv4.library.cornell.edu/abs/1005.0761

SUSY dark matter in light of CDMS II results: a comparative study for different models
Authors: Junjie Cao, Ken-ichi Hikasa, Wenyu Wang, Jin Min Yang, Li-Xin Yu (Submitted on 5 May 2010)

"(i) For each model the new CDMS limits can exclude a large part of the parameter space allowed by current collider constraints; (ii) The property of the allowed parameter space is similar for MSSM and NMSSM, but quite different for nMSSM; (iii) The future SuperCDMS can cover most part of the allowed parameter space for each model."

If the LHC does not find neutralinos, and the future SuperCDMS (and other direct and indirect dark matter detection) also fails to detect neutralinos with sensitivity to detect them over its parameter space, is there a plausible SUSY model (4D or 11D, planck-scale, etc.) that could account for both null results?

Can SUSY be a symmetry of nature broken at any scale if neither LHC nor SuperCDMS detects them?

25. **Thomas Larsson**
June 20, 2010

"Can SUSY be a symmetry of nature broken at any scale if neither LHC nor SuperCDMS detects them?"

From Frank Wilczek's [Future summary]() from 2001, p 20:

"Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new R-odd particles. Even in the focus point scenario, there must be several accessible to the LHC."

26. **Giotis**
June 22, 2010

@Dan

Yes of course. Supersymmetry has been well established by String theory as a theoretical concept at high energies.

Whether it would be observed at the low energies of LHC is not vital for Supersymmetry although psychologically this would be bad news for SUSY and String theory.

Moreover the vast majority (if not all) of String theory models have been constructed for N=1 SUSY in 4D so a failure in LHC would have I guess a significant impact in 4D model building.

LHC Update

June 11, 2010
Categories: Experimental HEP News

See here for a new status report on the state of beam commissioning at the LHC. About two weeks ago a peak luminosity of about 2 x $10^{29}cm^{-2}s^{-1}$ was reached, using beams with 13 bunches, but each bunch relatively low intensity. Since that time, efforts have been directed at increasing the bunch intensity towards nominal values. During this process, there has been little new physics data gathered, although the plan has been to interleave physics runs on weekends with beam commissioning during the week.

On Wednesday, this plan was changed, with the focus now completely devoted to commissioning beams with nominal bunch intensity. The hope is to have collisions of high intensity bunches in two weeks or so and then provide a 4 week period of stable running for physics during August, at luminosities an order of magnitude higher than now available. The limiting factor will be the stored energy in the beam, which is approaching Tevatron levels. To go higher will require much more testing of the beam protection systems.

This week in Hamburg there was a conference people have been waiting a long time for, Physics at the LHC 2010, the first conference devoted to the presentation of LHC experimental results. So far the results are based on very low luminosities, so are only able to check that the machine is working and rediscover various well-known features of the Standard Model. As the luminosity increases, it will start to become possible to observe or rule out various exotic particles at masses not reachable by the Tevatron, as long as the cross-sections are high.

Mostly such things are unmotivated and not expected to turn up, but one story to watch is that of supersymmetry, as the LHC becomes capable of observing strongly-interacting superpartners at masses beyond the Tevatron's reach. The MSSM includes a huge number of unknown extra parameters, so the masses of these things are unknown. Given one motivation for supersymmetry, that it is supposed to stabilize the electroweak scale, one would expect superpartners to readily show up at the new mass ranges being investigated by the LHC. The problem with this is that it's hard to understand why they haven't already been seen at the Tevatron, or indirectly in other experiments through the effects of higher-order processes. Taking current non-observations into account, even if one has faith that superpartners exist at masses the LHC can probe, it doesn't seem likely that the earliest LHC data will be able to see them (for more specific analysis along these lines, see for instance this talk).

I'm quite dubious that the LHC will ever see superpartners, but many prominent theorists claim that this is likely to happen. It would be interesting to pin them down on what LHC luminosity is needed to see what they are expecting to see.

Update: A particularly noteworthy talk is that of Mike Lamont, where he gives estimates of the progression of integrated luminosity this year. They're significantly more pessimistic than similar estimates from early this year, with about 50 inverse pb this year instead of 200, but still on track for 1000 inverse pb by late 2011.

Comments

1. **lhc**
   June 11, 2010

   It is a delicate balance, to spend time to test and commission the machine (higher energy, more bunches, etc) — which may result in possible failure of the strategy — and to run a stable machine for HEP, at a lower energy and luminosity.

   LHC and finding superpartners — I look at my marriage and I realize that finding a superpartner is indeed a very speculative notion.

2. **Geoff Brumfiel**
   June 11, 2010

   So just out of curiosity Peter, what are you putting your money on the LHC finding first then?

3. **Peter Woit**
   June 11, 2010

   Geoff,

   Unfortunately I don't see any reason to expect new strongly interacting particles, a new resonance, or anything else visible with the amount of data the LHC is likely to produce this year and much of next year. I would love to be wrong…

   By late next year possibly and more likely in 2014, they should start having the data necessary to either see a Higgs, or rule it out and maybe see something else that is responsible for electroweak symmetry breaking, that's when it seems likely that things will get interesting. An at least during next year, for the Higgs they'll be in competition with the Tevatron.

Millennium Prize Update

June 11, 2010
Categories: Uncategorized

The Clay Mathematics Institute this past week sponsored a conference in Paris devoted to celebrating the proof of the Poincare Conjecture. This included a short ceremony on June 8th awarding the Millennium prize to Perelman, which included several laudations explaining his achievement.

The question of what happens to the million dollars remains unresolved, with the following statement issued yesterday:

> The Clay Mathematics Institute has no plans for the Millennium Prize funds other than to respectfully wait for Dr. Perelman's decision. No deadline has been fixed for his decision, and nothing has been said or will be said about the possible use of the funds. Please see the text and laudations below for what is truly important: Dr. Perelman's great gift of a solution to the century-old conjecture of Henri Poincaré, and Thurston's geometrization conjecture. Their solution was celebrated at a conference in Paris held June 8-9, 2010. Those present send their congratulations and best wishes to Dr. Perelman.

Comments

1. **Syzygy**
   June 11, 2010

   Among the laudations for Perelman from several very distinguished mathematicians, I would like to bring attention to Thurston's: It is the only one to hint that perhaps what is most admirable about Perelman, even beyond his awesome mathematical strength, is his courage to stay true to his convictions and to challenge all this relentless chasing after fame and external recognition at the expense of caring for our "deeper needs". I'm glad that someone of Thurston's calibre thought to make this point clear. The other laudations, while full of praise for Perelman's mathematical heroism, do not say anything essentially fresh.

2. **D R Lunsford**
   June 11, 2010

   Peter, for you next book, I wish you would write up an historical exposition of this and related work, understandable to non-specialists with some grounding in advanced mathematics.

   -drl

3. **Fred**
   June 11, 2010

   The Institute should make public a deadline for Perelman to accept his prize. If no response, they should just give the money to charity. If Perelman doesn't want the money, this would be a perfect opportunity for him to make a tangible contribution to humanity by accepting the prize money and immediately giving it to charity. This is a million dollars here. Do you know what a million dollars could do? These people should get out of their ivory towers and look around at the world.

4. **Peter Woit**
   June 11, 2010

   drl,

   I'm afraid this is a topic way beyond my competence. There's at least a couple popular books already (by Szpiro, Gessen) and various survey articles at different levels. But the kind of geometric analysis behind Hamilton's work and the Perelman proof is way beyond me. I know just enough about it to know the kind of time commitment that would be needed to really understand it…

5. **Dr.Kathrine Martinez-Martignoni**
   June 11, 2010

   Why don`t they send this "Millennium Prize" (one millions US dollars)directly to the mother of Perelman who is living in a very poor way in St.Petersburg?
   Best wishes,
   Dr.Kathrine M.(Switzerland).

6. **Roger Schlafly**
   June 11, 2010

   It is funny how everyone says that Perelman turned down the prize when he has said nothing. It is also funny how people like Fred try to tell Perelman what to do.

7. **ghengis.khandada**
   June 11, 2010

   Perelman once said the greatest reward for proving the Poincare conjecture is the proof itself, and that the best way to honor him/his work is to re-direct our time and energy to understand the proof. Perelman gives me tremendous hope by being a purist to the end.

8. **Fred**
   June 11, 2010

   Roger,

   I just meant that as a suggestion for Perelman, that's all. If he really wanted the money/prize, he would have said something by now. He clearly doesn't want the money, so I offered the suggestion that the Institute just give the money away to charity. The other suggestion, mentioned above, is that he money go to his mother instead. If Perelman wants all this hoopla to lead to something good, then my suggestion was for him to accept the money and give it to people who need it, since he clearly doesn't want it. You can find plenty of people/causes that would benefit from the money. It's a good opportunity for him to make this happen. I don't know about you, but I think that's a good idea.

9. **BigG**
   June 11, 2010

Fred,

Given the number of needy people in the world what Perelman is doing is extremely selfish. It's really cool he's not accepting the money on principle, but he could redirect the Clay Inst. to donate the money or accept it on the grounds he will donate it. Wittgenstein did something similar and for similar reasons.

As for the other guy, why bother responding to people who obviously are not seriously interested in what you have to say? If there is no serious engagement with the issues or interest in a disscussion there's no basis for debate.

10. **Haelfix**
June 12, 2010

Alternatively the Clay inst. could just create a new question (there are many worthy ones) and have instant funding.

11. **Anonymous**
June 12, 2010

This Fred and BigG sillness reminds me of piles of angry letters Grothendieck recieved upon his decline of Crafoord Prize. The particularily absurd one was from a woman accusing him of robbing French people from Kenseyian stimulus the economy would recieve would he spend his prize money in France.

Also, Dr Kathrine, Perelman probably cares more about his mother than you do about her. I'd also like to point that Perelman recievied a few prizes in the nineties and is moderately wealthy by Russian standards.

12. **Bob Levine**
June 12, 2010

"This Fred and BigG sillness reminds me of piles of angry letters Grothendieck recieved upon his decline of Crafoord Prize. The particularily absurd one was from a woman accusing him of robbing French people from Kenseyian stimulus the economy would recieve would he spend his prize money in France.

Also, Dr Kathrine, Perelman probably cares more about his mother than you do about her. I'd also like to point that Perelman recievied a few prizes in the nineties and is moderately wealthy by Russian standards."

Well said on all counts, Anonymous!

This thread raises a question which is much more general than the question of what Perelman is going to do—namely, what is the virtue (or otherwise) of these huge cash prizes that are associated with specific prizes for scientific discovery? Does the prestige of the physics Nobel, for example, really depend on the attachment of approximately $1,500,000 US to it? Suppose the prize were reduced to $10,000, but everything else about the award were left intact—would that alter the role of the Nobel in singling out the absolutely highest level of achievements in physics and the other disciplines it's awarded for?

Suppose the answer to that one is 'No'. The next question one would want to ask would be, does the award of the Nobel actually promote achievements in the Nobel disciplines which otherwise would not be reached, or not reached until much, much later?

My impression is that Perelman's point in declining the Field Medal, and (in all likelihood) the Millenium Prize, is to forcefully express his own judgment on these questions: a sound result is the only authentic reward of scientific investigation. Everything else is, well, hype. Does anyone *really* want to argue that the opportunity to support this or that charity trumps taking and maintaining such a principled position?

13. **BigG**
June 12, 2010

It's absolutely not silly. This western idea of the lone genius can lead to extremely selfish thinking. When you have homeless and starving people who cares about your purity as a mathematician. The European enlightenment was a good thing but individual self-determination can be taken too far. It may be a long time before people like you understand this.

14. **Jack Lothian**
June 12, 2010

Turning down a prize is selfish??? While I presume accepting it isn't selfish?? That is silly. Forcing Perelman to accept the prize & donate it charity would be unethical and morally unacceptable. Yet you feel this should be done on his behave & against his wishes because it makes you feel better? While I would not make the same decision as Perelman, I respect his right to make his decision.

Truth is, his decision awes me. I wonder if maybe he sees further then me even in non-mathematical issues.

15. **Coin**
June 13, 2010

It seems to me that by not accepting the prize, Perelman is effectively donating a million dollars to the Clay Institute. This seems like a not unreasonable use of a million dollars to me.

16. **BigG**
June 13, 2010

Jack you haven't read what I said closely. No one can make him accept the money but not doing so on some principle of the purity of maths is selfish. It has nothing to do with making me feel better and by saying this your unfounded assumptions while reading become clear.

The idea that a principle of the purity of math is somehow more important than helping others is selfish. Who cares about math in comparison with relieving the suffering of others? On a personal level, the decision by itself to stand up for one's principles is admirable. But given the money and what can be done with it, there are times when a principle such as this needs to be put aside.

There are two points of view of this principle, one for himself and one for the discipline of math. Both these are less important than helping those in need. In comparison, who cares about Perelman and who cares about math. They are meaningless from this point of view.

I would also be in awe of Perelman from a personal perspective of standing up for one's principles. But there is nothing to be in awe of when you consider how much could be done.

Consider a thought experiment. His mother is sick and too poor to receive treatment. He turns down the prize because of some personal principle. Is he worthy of awe now or completely selfish? This is exactly the situation he's currently in.

You need to think about what's being said. When your perspective begins to go from me to them you will understand my point.

17. **Sansa**
June 13, 2010

For the people condemning Perelman for not accepting $1M and giving immediately to charity, why aren't you also condemning Landon Clay for not giving $7M to charity?

(Also BigG consider a thought fact: Russia has UHC)

18. **Fred**
June 13, 2010

Bob Levine:

"My impression is that Perelman's point in declining the Field Medal, and (in all likelihood) the Millenium Prize, is to forcefully express his own judgment on these questions: a sound result is the only authentic reward of scientific investigation. Everything else is, well, hype. Does anyone *really* want to argue that the opportunity to support this or that charity trumps taking and maintaining such a principled position?"

Suppose this money could be used to educate people about science in a third world country. I believe this kind of thing trumps such a principled position because holding such a position accomplishes nothing. Accepting the money in an effort to make peoples' lives better accomplishes something. It takes money away from rich people, using it in the service of others.

Here's a suggestion for the Institute. If Perelman doesn't accept the prize money in two weeks, the Institute will use the money to develop schools that educate people about science and math. Schools in countries too poor to have them.

19. **BigG**
June 13, 2010

Sansa,

I'm not condeming Perelman. You need to read what I'm saying more closely. I never used the term condem. Another simple analogy will make the point clear. If you over pay for something and I say 'you paid too much for that' there's no condemnation. That's your choice. All I'm doing is stating my opinion that you paid too much money. These words don't imply anything else.

Your comment about Russa having UHC misses my point completely. Its an abstraction from people in general to someone in particular that makes clearer how he would be selfish not to accept the money if someone needed it.

Fred seems to be the only one who understands my point. I don't care if you agree or not with me but most of those who disagree don't understand my point. As a result, when they reply they make claims that are not supported by what I wrote.

"I said he would be lonely, & he said he prostituted his mind talking to intelligent people."

20. **John Bacon**
June 13, 2010

> "Who cares about math in comparison with relieving the suffering of others?"

I do. In the end Perelman's work is far more important than the lives of thousands if not millions of humans. Certainly yours or any commenter here.

Human life is severely over-rated. Most humans are trash and contribute nothing to understand the world and advance knowledge only concerning themselves with stupid societal pursuits such as kicking a ball into a net or deceiving other people for social/financial status.

In the end Perelman's work trumps all of this trash and so does is right to refuse the prize for whatever reasons he sees fit.

> His mother is sick and too poor to receive treatment. He turns down the prize because of some personal principle. Is he worthy of awe now or completely selfish?

Both.

People who aren't openly selfish always have insidious ulterior motives that make them far more dangerous than ones that those who are selfish.

21. **Peter Woit**
June 13, 2010

Enough, the comments here reveal all too much about the commenters, but nothing at all about Perelman and his situation. Don't submit comments unless you have something informative about the topic of the posting to contribute.

22. **basic arithmetic**
June 14, 2010

To put things into perspective, $1miilion represents about 0.00001% of the USA's (annual) GDP.

23. **Tim van Beek**
June 14, 2010

At the end of the 1990ties I spent a few weeks at the institute Laue-Langevin in Grenoble, France, and got to know some scientists from Russia, they had grants and tried to save as much money as they could, because at that time you could live like a year in Russia with the money you had to spent during one month in Grenoble. They did research, too, but one told me that the main reason for him to go to Grenoble was to get French money and use that to fund his research back home.
Perelman's story reminds me of him, maybe he would have refused the million dollar price, too, if having this amount of money would mean that he could not concentrate on his research as much as he liked to, and if he did not need it because his savings were enough to survive.
I have always admired the Russian experimentalists, who were able to produce results despite their unfavorable economic situation.

24. **ML**
June 14, 2010

I live in Stcokholm, just an hours flight from St Petersburg, where
I have good friends well connected in mathematics. I wanted to invite Perelman to Stockholm as a short-term guest, with no commitments from his side. But the message from Perelman was that he was not willing to discuss anything related to mathematics.

25. **The Vlad**
June 14, 2010

I think the interesting questions are: Is he still doing math? And if so, what fantastic insights and technical advances is the academic community missing because of his reclusivity? Presumably he is documenting his putative findings in such a way that those who eventually follow him–should his work ever see the light of day–can build on that work.

26. **Pilot**
June 16, 2010

With apologies to Peter ("Don't submit comments unless you have something informative"), here is my suggestion: Perelman should accept the award, take the money and declare a million-dollar-prize of his own for his favourite problem in mathematics. Or better, award it to the next Henri Poincare. He certainly deserves at least ten million dollars and Perelman can start by redirecting his one? 🙂

27. **Arun**
July 1, 2010

http://news.yahoo.com/s/ap/20100701/ap_on_sc/eu_sci_russia_math_genius

Grigory Perelman, a reclusive Russian mathematics genius who made headlines earlier this year for not immediately embracing a lucrative math prize, has decided to decline the cash.

Perelman's decision was announced Thursday by the Clay Mathematics Institute in Cambridge, Mass., which had awarded Perelman its Millennium Prize.
….
….

Jim Carlson, institute president, said Perelman's decision was not a complete surprise, since he had declined some previous math prizes.

Carlson said Perelman had told him by telephone last week of his decision and gave no reason. But the Interfax news agency quoted Perelman as saying he believed the prize was unfair. Perelman told Interfax he considered his contribution to solving the Poincare conjecture no greater than that of Columbia University mathematician Richard Hamilton.

"To put it short, the main reason is my disagreement with the organized mathematical community," Perelman, 43, told Interfax. "I don't like their decisions, I consider them unjust."

28. **Benni**
July 1, 2010

Perelman now has officially declined the clay prize.

According to recent press articles, Perelman told:
http://www.spiegel.de/wissenschaft/mensch/0,1518,704113,00.html

Perelman thinks that Richard Hamilton deserves the prize similarly than does Perelman. Perelman says the decision of the Clay institute therefore would not be fair. As a consequence, he refused to accept the prize.

This seems to be a reasonable decision. It is always doubtful, if an achivement like this in mathematics can be attributed to one single person who can be given an avard. Often, there are more people involved in a proof than just one person.

29. **Tim van Beek**
July 1, 2010

I've read that article, too, but, oddly enough, there does not seem a link or hint to the source. Surely, no one from the spiegel online editorial staff talked to Mr. Perelman in person, right?

30. **Arun**
July 1, 2010

This version reports Perelman saying that he had as many reasons to accept as to decline the prize and that is why it took him so long to make a decision. I'm glad he considered taking the award.

Source: Tanjug News Agency

Russian mathematician Grigory Perelman decided to reject the Millennium Prize worth $ 1 million from the American Clay Mathematics Institute, the Interfax News Agency reported.

"I declined the award. **I had as many reasons to accept it as to turn it down.** That's why it took me so long to make the decision" Perelman said.

The main reason for his decision to reject the award was its "unfairness".

"I think my contribution in solving the problem of the Poincare conjecture was not greater than the one of the American mathematician Richard Hamilton."

http://www.allvoices.com/contributed-news/6208384-russian-math-genius-rejects-1-million-prize

Predictions From David Gross

June 14, 2010
Categories: Uncategorized

Video of David Gross's talk at the Physics at the LHC 2010 conference is now available. He devotes much of the talk to reviewing predictions he made back in 1993 of what would happen by 2008, and making new predictions for what will happen by 2020.

The 1993 experimental predictions that didn't work out could mostly be explained away by the SSC cancellation, which pushed investigation of TeV scale physics into the future at the LHC. In 1993 Gross predicted two light Higgs particle and superpartners (due to supersymmetry), new Z-mesons (i.e some new U(1) gauge fields) and "There will be cloudy evidence of superstrings." His 1993 predictions about theoretical developments related to string theory didn't work out very well:

> String field theory will begin to be a useful tool and will illuminate the underlying symmetries of the theory. (*not at all, he admits*)
>
> New mechanisms of string supersymmetry breaking will be discovered leading to new and definitive low energy models. (*new maybe, certainly nothing definitive*)
>
> The conceptual revolution arising from the nonperturbative formulation of string theory will be in full swing, revolutionizing the concepts of space-time geometry. ("*on its way, but it hasn't really revolutionized our concepts*")

For 2020, Gross makes one striking non-scientific prediction: The US will join CERN and there will be a joint plan to build a linear collider at CERN.

His experimental predictions include a repeat of the 1993 ones (superpartners, new Z-mesons, and the Higgs, although now he only mentions one Higgs), except that he has now given up on even "cloudy" evidence of superstrings showing up at the TeV scale. His theoretical predictions include a much scaled back version of the 1993 theoretical predictions about string theory: "If we're lucky, string theory will start to be a theory with predictions (right now we don't understand it)".

As far as supersymmetry goes, he says that he is "totally convinced that SUSY should be there, at the 50% level" and repeats his offer to take bets at 50/50 odds. One of the main motivations for this is the argument that SUSY can provide a suitable dark matter candidate, and he predicts observation of dark matter by 2020 at non-accelerator experiments.

Finally, he ends up with an attack on the anthropic explanation of the CC, and predicts that a better one will be found by 2020.

Update: See Lubos, who has a lot more energy than I do, for a transcription of Gross's predictions.

Comments

1. **capitalistimperialistpig**
   June 14, 2010

   CC?

2. **Peter Woit**
   June 14, 2010

   CC = Cosmological Constant

3. **Roger Schlafly**
   June 14, 2010

   I am totally convinced, at the 0.1% level.

4. **capitalistimperialistpig**
   June 14, 2010

   Thanks. I figured chess club and corporate conspiracy wouldn't cut it.

5. **Bee**
   June 15, 2010

   A linear collider at CERN? Has he ever looked at a map of the area?

6. **clic**
   June 15, 2010

   CLIC – the CERN LInear Collider or Compact LInear Collider
   homepage
   http://clic-study.web.cern.ch/clic-study/

   see also
   http://public.web.cern.ch/public/en/research/CLIC-en.html

   It is a project *by* CERN, even if the eventual location is not physically at Geneva.
   Moreover, the CLIC project has been under study for many years.

   Gross knows what he is talking about, as far as a linear collider at CERN is concerned. Whether or not the US will join CERN is a highly political question, and by 2020 is yet another question, but CERN has certainly demonstrated up to now that it (CERN) is prepared to go it alone (w/o USA), for CLIC. Whether CLIC itself will be ready (w/ or w/o USA) by 2020 is another question. A lot of course depends on the outcome from the LHC ~ the Higgs … at what mass? It may (will?) take years to accumulate and analyze the LHC data.

7. **Dr. David A. Edwards**
   June 15, 2010

The video of David Gross's talk at the Physics at the LHC 2010 conference keeps stopping.

8. **Jess Riedel**
   June 15, 2010

   You have to admire his willingness to publicly asses his past predictions. That's rare, in HEP theory or elsewhere.

9. **a**
   June 15, 2010

   assess
   not quite the same as asses

God Particles Breeding Like Bosons

June 15, 2010
Categories: Experimental HEP News, This Week's Hype

Science news in the media today is full of stories about Fermilab finding no less than five Higgs particles: God Particles Breeding Like Bosons, The 'God Particle' may exist in five forms, Large Hadron Collider's rival project finds, US experiment hints at 'multiple God particles', Fermilab Experiment Hints at Multiple Higgs Particles. The source of these stories can be traced back to this preprint, whose authors then appeared on this radio program, leading to this Symmetry Breaking story.

On May 18 D0 claimed observation of CP violation in processes involving B-mesons of a sort that could not be explained by the SM, at a significance level of around 3 sigma. For an explanation, a good place to look is Resonaances. A violation of the SM is an extraordinary claim, so it requires some extraordinary evidence, and a 3-sigma result is not that extraordinary. The case for such a violation was strengthened by the fact that D0 and CDF had seen a 2-sigma violation of the SM in a similar CP-violating process. The May 23 theory theory preprint tries to explain these SM violations with a model involving two Higgs doublets. Two days later though, on May 25, CDF reported new results: with better data, their 2-sigma SM violation had gone away (now it is 0.8 sigma, completely consistent with the SM). Again, for a good explanation of this, see Resonaances. Somehow, the disappearance of one of the main reasons for taking all this seriously didn't make it into the Symmetry Breaking story, or any of the flood of ridiculous stories that appeared today.

Comments

1. **Bee**
   June 15, 2010

   Thanks for putting this into context… I came across the story in a (usually pretty good) German magazine (or rather, its online edition) and was surprised they'd write about an unpublished paper with unconfirmed results. Now I see where they got the story from…

2. **Dan**
   June 16, 2010

   My old advisor (particle physics) used to make the "Yogi Berra-esque" statement that "3 sigma events happen about half the time."

3. **Shantanu**
   June 18, 2010

   Peter have you looked at any of TASI 2010 videos/lectures
   which is on string theory?
   http://physicslearning2.colorado.edu/tasi/tasi_2010/tasi_2010.htm

4. **The broad higgs**
   June 18, 2010

   There are voices about this new result, to be announced at summer conferences + press release

5. **Peter Woit**
   June 18, 2010

   Thanks Shantanu, hadn't seen those. Looks like more of the same about the landscape, a lot more about attempts to apply gauge-gravity duality to condensed matter physics.

   the broad Higgs,

   Now that's a very interesting rumor…

6. **Tona**
   June 18, 2010

   The DZero collaboration does not claim that the 3 sigma result proves asymmetry, just that it MAY point to that. The collaboration is happily waiting for CDF or the LHC to cross check its analysis.

   The CDF paper released a coupld weeks ago and mention here and in the Resonaances blog does not confirm or disput the DZero result. It is like comparing apples to oranges.

   The CDF collaboration did release a sin(2beta_s) result a few weeks ago that looked at CP violation states and found the discrepancy between the amount of particles that decayed to matter versus antimatter in line with the Standard Model, the opposite of what DZero's study found. The CDF study is an update of a 2007 meson decay analysis that both CDF and DZero conducted where both found discrepancies in the preference for decays to matter and antimatter. The recent CDF update of this analysis, which is being submitted to the scientific journal Physical Review Letters, found a smaller discrepancy than in 2007. DZero has plans to conduct its own update.

   However, comparing the updated CDF study and the current DZero asymmetry result is like comparing apples and oranges. True, both studies look at CP violation in meson decays, which can be used to search for "new physics" beyond the Standard Model. However, while the studies look at the same underlying physics, they use vastly different approaches and analysis methods that renders it impossible to draw a conclusion from the CDF study that would deny or confirm the DZero asysmmetry result, according to CDF leaders. Also, the uncertainties of both measurements more than "cover" the disagreement – so neither rules the other one out.

   What the CDF study does do is highlight the need for CDF to conduct an apple-to-apple type study of the current DZero result that would have the potential to validate or invalidate it.

   While there was some initial discussion, even among CDF collaborators, about whether CDF could perform the same search because the magnet construction in its detector differs from DZero's, CDF collaborators believe they can reach the same level of sensitivity as DZero to the decays.

   CDF conducted a similar apple-to-apple type study several years ago, but with a much smaller dataset, not enough to make a judgment either way, according to collaboration leaders. The DZero result is based on more than 6 inverse femtobarns in total integrated luminosity, corresponding to hundreds of trillions of collisions between protons and antiprotons in the Tevatron collider.

However, the success of CDF's past study, leads CDF collaborators to believe they can conduct a larger, comparable analysis, although in a slightly different fashion, than the past CDF study and the current DZero study.

The CDF collaboration announced at the Fermilab Users' Meeting in early
June that it will perform this analysis with its full data set. The duration of the study will depend on the amount of preliminary work conducted, but collaborators estimate a result in time for a presentation at the winter 2011 physics conferences if the CDF search turns out to be as competitive as DZero's.

You can read an expanded version of this at the updated symmetrybreaking post: http://www.symmetrymagazine.org/breaking/2010/06/04/could-dzero-result-point-to-multiple-higgses/

7. **Peter Woit**
June 18, 2010

Tona,

Thanks for clarifying this. However, it should be pointed out that the theory paper about the "five Higgs particles" makes a big deal about the older D0/CDF result indicating non-SM CP violation in a different process, discussing it extensively on the first page of the preprint. The bottom line in this discussion on the first page is that, combining old and new, the new D0 result is 2.5 sigma from the SM. The authors then combine this 2.5 sigma with the old 2.1 sigma deviation to argue that there is a violation at 99.9% confidence level. This argument gets blown away by the new, better CDF measurement, and I assume the authors at some point will revise their preprint to take this into account.

Even with independent 2.5 and 2.1 sigma deviations, it seems to me to be a bad idea to go on the radio trying to sell the highly speculative explanation they have. I don't know when the radio interview was, but if it was after the release of the CDF result then it was an even worse idea. I listened to the first few minutes of the interview and the extent to which the interviewer had no idea what was going on and the physicists little interest in explaining the highly speculative nature of their work made the whole thing almost comical.

Various and Sundry

June 21, 2010
Categories: Uncategorized

- Last week's symposium in Paris on Symmetry, Duality and Cinema now has talks available on-line. These include a very nice survey talk by Edward Frenkel, including some comments on his recent work with Langlands and Ngo that involves an analog of the trace formula in the geometric case. The symposium also included a showing of Frenkel's film *Rites d'amour et de maths*, and the Huffington Post has an article about the film here.
- Some notes from this year's Talbot workshop on Twisted K-theory and Loop Groups have started to appear here.
- For the last three weeks or so, the LHC has not been taking data, but instead working on commissioning the beam at higher intensity. This process should end soon, and the plan is to have physics runs this summer with up to 24 bunches/beam, getting an integrated luminosity of around 5 pb^{-1}.
- For an excellent article on the science job market in the US, see The Real Science Gap. The author quotes from this posting about 2009 particle theory hiring. Numbers for 2010 so far look similar.
- The authors of the preprint that led to the "Five Higgs" world-wide hype-fest (see previous posting) have put out a revised version of their preprint. The first version gave as major motivation corroborating SM-violating data in another channel from earlier experiments, but this has gone away in recent CDF data. The new version removes most of the discussion of this, leaving one paragraph that seems odd to me, since it refers to the recent CDF data as "strengthening the case for physics beyond the SM", although that data only disagrees with the SM at the .8 sigma level.
- In Denmark it seems that they take their quantization of moduli spaces very seriously, with the Center for the Topology and Quantization of Moduli Spaces at Aarhus now joined by a new Center for Quantum Geometry of Moduli Spaces.

Comments

1. **Researcher**
   June 21, 2010

   Just a minor quibble: the letter p in *The Real Science Gap* is not linked.

2. **Peter Woit**
   June 21, 2010

   Thanks, fixed.

3. **nbutsomebody**
   June 22, 2010

   Peter,

   I had an impression that job market in Mathematics is better than that of HEP. Partially because less number of people graduating. Please correct me if I am wrong.

4. **Peter Woit**
   June 22, 2010

   nbutsomebody,

   I don't have any good numbers, but the job market seems to be much better in math than in HEP theory. The ratio of permanent jobs at research universities to people getting Ph.Ds at the top universities is much higher in math. In very rough numbers I'd guess that typical top US universities are each producing about 5 HEP theory Ph.Ds and 10 pure math Ph.Ds, who will end up competing for about 10 permanent HEP theory positions and an order of magnitude more permanent math research positions.

5. **Andy**
   June 24, 2010

   CQGM in Arhus is a continuation of CTQM, with the same director but under a different name.

String Theory Fan

June 21, 2010
Categories: Uncategorized

One of the weirder battles of the String Theory wars became known to some as “trackbackgate”, referring to arguments over the arXiv’s policy of not allowing trackbacks to this blog. I’ve mercifully forgotten the details of the story, other than that I wasted a lot of time arguing the issue with the authorities at the arXiv and Cornell, an argument that I finally lost. If you’re interested in the history, a couple blog entries you could start looking at would be this and this.

At some point the arXiv’s policy changed, and some trackbacks to blog entries here started to appear, as well as trackbacks to all sorts of media stories that linked to the arXiv. A little bit of checking seemed to indicate that trackbacks would appear if I linked to papers not in hep-th, but wouldn’t appear when I linked to hep-th papers. A recent example would be this blog entry, which linked to and discussed this paper. This made me a bit curious about what the arXiv current trackback policy might be, but from past experience I figured that trying to contact them to find this out was unlikely to get me anywhere.

One day recently it occurred to me that a way to find out something about this would be to start up another blog, write a posting linking to an hep-th paper and see what happened. It’s quite remarkable how little time it takes to start up a blog, so an hour or so later String Theory Fan was on the web, with an About section:

> This blog will be devoted to discussing the latest exciting developments in string theory, our best hope for a fundamental unified theory of particle physics and quantum gravity.
>
> The author is an academic actively studying this fascinating subject.

a first blog entry spouting hype about string theory, the multiverse and how uninformed critics were, and a second one linking to and superficially summarizing a randomly chosen recent multiverse paper.

Well, I still don’t know what the arXiv’s policy is about trackbacks to hep-th papers, but the data shows that while Not Even Wrong doesn’t seem to qualify, String Theory Fan does:

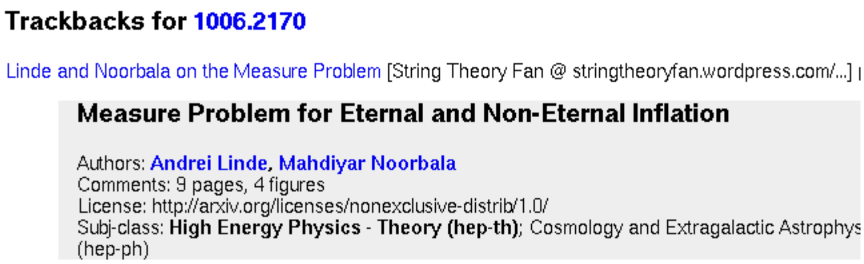

Trackbacks for 1006.2170

Linde and Noorbala on the Measure Problem [String Theory Fan @ stringtheoryfan.wordpress.com/...]

Measure Problem for Eternal and Non-Eternal Inflation

Authors: **Andrei Linde, Mahdiyar Noorbala**
Comments: 9 pages, 4 figures
License: http://arxiv.org/licenses/nonexclusive-distrib/1.0/
Subj-class: **High Energy Physics - Theory (hep-th)**; Cosmology and Extragalactic Astrophys
(hep-ph)

Comments

1. **Kea**
   June 21, 2010

   The shady activities of the arxiv are now well established. Fortunately, alternative sites such as vixra make the arxiv essentially redundant, even if professional ass kissing keeps it alive.

2. **Wyman**
   June 21, 2010

   I do hope research universities keep this in mind when Cornell comes to them begging for help paying arXiv’s server bills.

3. **Bee**
   June 22, 2010

   Well, that’s a little bit embarrassing for the arxiv admin isn’t it?

   In any case, it must be difficult for them to deal with the trackbacks. They probably want to preserve a certain quality standard so will try to avoid trackbacks to random crackpot sites. You can’t automate quality standard, so they will have to check them by hand. This inevitably brings in personal opinions and subjectivity and will occasionally cause problems. Not saying there’s a good reason to not list trackbacks to your blog in particular, I’m just saying the problem is one where such things are prone to go wrong every now and then.

4. **Stucker**
   June 22, 2010

   Peter,

   I’m usually critical of your activities, but this time, I have to admit, my hats off! 🙂

   Well done!

   Best,
   Stucker

5. **Syksy Räsänen**
   June 22, 2010

   Excellent, Peter.

6. **Anon2**
June 22, 2010

LMAO. Very funny. Their “policy” is string worship. But they probably don’t even realise how subjective they are.

7. **Fabio**
June 22, 2010

Hilarious. But you should have held out for a supportive link from Lubos before you spilled the beans.

8. **String mafia**
June 22, 2010

to proof that you are a real scientist, the next step is publishing a paper about the Multiverse with your colleague Sokal

9. **Peter Woit**
June 22, 2010

Fabio,

While Lubos approves of the string theory fanboy aspect of the blog, he’s not big on the Multiverse, so I doubt he would endorse “String Theory Fan”. To find someone willing to endorse both string theory fanboydom and Multiverse pseudo-science, it seems you need an arXiv hep-th moderator.

By the way, does anyone know why most multiverse papers like the one I linked to are in hep-th and not gr-qc? I’d have thought qc=quantum cosmology was exactly their topic, and they typically have no connection to HEP. Maybe the gr-qc moderators wouldn’t allow them…

10. **Alejandro Rivero**
June 22, 2010

Bee, the problem is that they automate quality standard, in other sense: once some decision about blocking has been evolved into code (Perl or any other matching algorithm, I dont know), it seems very hard for the next ArXiV moderators to review and remove them, specially when/if moderators and programmers change with time. For robot scans, for instance, it seems that the ban is kept ethernally. Death penalty for your IP.

11. **Peter Woit**
June 22, 2010

Alejandro and Bee,

The little information I have about this is that yesterday someone connected to the “String Theory Fan” blog from an arXiv trackback administrative page, and looked at all the “String Theory Fan” pages. Soon after that, the trackback appeared. This was definitely not automated, but I have no idea who was doing this, or what criteria they were using to decide whether “String Theory Fan” was trackback-worthy.

I’m not very surprised they found the site trackback-worthy, partly because they now seem to be operating under a very loose standard, allowing trackbacks from all sorts of postings from non-scientists (e.g. Slashdot). What is surprising is that even under this newer loose standard, my postings are still often being judged unworthy. These decisions seem to be being made manually, not automatically, but, again, I have no idea who is making them or what criteria they are using.

12. **Anon2**
June 22, 2010

“… I have no idea …”

Very tactful of you! (Surely, you don´t need top be Einstein to work out from the list of arXiv advisers, those who are old enemies of your blog.)

13. **Peter Woit**
June 22, 2010

Anon2,

I’m sure a certain member of the physics arXiv advisory board from the University of Texas has something to do with all this, but exactly what remains a mystery. Some of those who know him tell me he’s a reasonable guy, whose hands are tied by the true powers-that-be in this business. Seriously, I don’t actually have any idea what arXiv policy on this now is, or who is implementing it.

14. **nbutsomebody**
June 22, 2010

An excellent step.

15. **Roger Schlafly**
June 23, 2010

I am not sure your experiment proves much, except that a trackback can be approved quickly. Maybe someone dislikes you personally, or thinks you are a physics traitor, or thinks you are too negative about mainstream physics, or is offended that you attack sacred cows, or sees you as a threat to physics funding. It might be useful to do an experiment to distinguish these possibilities. Maybe you (or some reader) can start a blog that directly attacks the multiverse as unscientific, or says that the LHC is a big waste of money. Maybe you could throw in some Lubos-style rants.

16. **Janne**
June 23, 2010

Once again the true colors come out. Pathetic.

17. **younghun park**
June 23, 2010

Roget Schlafly

Your critic on Peter may be right.
I agree that Someone seems to dislike him and his act.

But, his writings make us think about what is true science.
Someone likes the concept of Multi-universe
and the others dislike the concept of Multi-universe.

It's not important that someone likes or dislike
that concept. The important thing is whether we are
going on the right way or not.
His blog makes us think about that question.

"Which way Now physics is going on?"
"That way is right or not?"

Maybe, it will be answered after the result of experiment
shows up through LHC.

I believe his critic on current physics will make current physics more healthy. I hope to see his active critical life.

18. **Benni**
June 23, 2010

Peter, If you want your trackbacks to appear, then make your comments at scirate.com.

This is a site where one can comment on arxiv papers. Trackbacks from that site are allowed, and any comment on a paper will automatically produce a trackback to the comment linked to the paper. (unfortunately, the comments are sometimes uninformed. For example, if someone does not like a paper, he can leave misleading claims on scirate on the article. Those rants then automatically produce a trackback that gets linked to the paper on arxiv.

19. **El Punko Rubio Herreras**
June 23, 2010

Yo, that is so el unprofessional to have done that, hombre. You will come to regret that, el mucho, grande mucho.

20. **Gphillip**
June 23, 2010

Their policy seems very clear. You are on the wrong side of the argument. You can either change the fundamental physics of the Universe, or learn to live without trackbacks. Trackbacks are so great, I'd suggest you change the fundamental physics of the Universe. Of course there are others who have been trying to do just that for decades, but I'd say you have just as much chance at it as they do. Good luck!

21. **sheldon**
June 24, 2010

Why exactly is it so important to have trackbacks from the arXiv to this blog? I am reminded of the quote by Sheldon Flender from the Woody Allen film "Bullets over Broadway"

http://www.imdb.com/title/tt0109348/quotes

Sheldon Flender: [bragging] I have never had a play produced. That's right. And I've written one play a year for the past twenty years.

David Shayne: Yes, but that's because you're a genius. And the proof is that both common people and intellectuals find your work completely incoherent. Means you're a genius.

22. **Peter Woit**
June 24, 2010

sheldon, Benni

I don't actually think it's of much importance to have trackbacks from the arXiv to blog entries here, so am not about to devote any more time to arguing the issue with them or figuring out some way around what they are doing.

But the arXiv itself is of central importance these days for the health of particle theory and several other academic fields. How they handle the tricky issue of moderation is of interest, so I thought devoting a couple hours to this test project was worthwhile. But, that's probably already more time than was sensible….

23. **Neville**
June 24, 2010

This episode is a hoot! Thanks for the entertainment.

Life is supposed to be fun.

24. **Lenny**
June 25, 2010

Peter,

While you are right that your experiment suggests that arxiv's trackbacks are not managed in a serious way, I'd join with others in agreeing that nor are they a serious thing about which to worry. Moreover, there is a danger to feeding the caricatural anti-establishmentarianism manifested by some of those on the fringe of legimitate science – there is a difference between wild, credible ideas and junk – and for the arxiv to be useful it is essential that the latter be excluded – that's not the old boy network – that's good management.

25. **Tim van Beek**
June 25, 2010

The arXiv is a central part of the hep-th community at least, so the way it is run and moderated is an important topic, certainly.

IMHO the next step would be to set up a blog like "string theory fan" and include watered-down versions of original rants, for example:

"LQG is just stupid. I know it because I once met a LQG expert, talked to him and he was just unable to understand what I said. Plus their Hilbert space is not separable."

And

"Modular localization in axiomatic QFT is plainly wrong, because it is not part of string theory, therefore I don't know what it is, which proofs that it has to be wrong. If it were right, it would be part of string theory. And it would be supersymmetric".

(In case anyone thinks that this is certainly not "watered-down": The relevant blog archives are all available online).

26. **Steven Jones**
June 25, 2010

i know this is a bit dark, and i might be wrong, or not even wrong, but sometimes i get the feeling that if we threw all the blog posts and comments and trackbacks into the trash, physics would not be harmed in any way, and may even be enhanced, as the snarky, handwaving, entrenched regimes of failure faded to naught, allowing new ideas to blossom.

never have so many insiders been paid so much to do little more than set up criterion and networks to define others as crackpots, beginning with the premise that their own non-theories, decades-old dead ends are non-crackpottery.

a little humility across the board would be a great thing, for until one comes up with a meaningful postulate and equation, how does one know that one is not a crackpot?

i suspect that this reality eats at a lot of people in modern physics.

for instance, when this is respectable research at major institutions:
http://www.amazon.com/Eerie-Silence-Renewing-Search-Intelligence/dp/0547133243/ref=sr_1_1?ie=UTF8&s=books&qid=1277494776&sr=8-1
http://www.amazon.com/Physics-Impossible-Scientific-Exploration-Teleportation/dp/0307278824/ref=sr_1_1?ie=UTF8&s=books&qid=1277494816&sr=1-1

what constitutes crackpottery?

I mean just look at the titles.

Physics of the Impossible: A Scientific Exploration into the World of Phasers, Force Fields, Teleportation, and Time Travel

The Eerie Silence: Renewing Our Search for Alien Intelligence

Imagine Einstein, Bohr, Feynman, Dirac, or Pauli having anything to do with the above titles….

27. **kevin strings**
June 25, 2010

why the above titles reflect most exalted physics!

crackpottery would be something like:

The Eerie Silence: Renewing Our Search for Phasers, Force Fields, Teleportation, and Time Travel

or

Physics of the Impossible: Renewing Our Search for Alien Intelligence

now books like these would be crackpottery!

28. **Peter Woit**
June 25, 2010

I don't think the two titles you mention are very highly thought of at most research physics departments, and their authors are not modern-day analogs of Einstein, Bohr, etc.

For a better example of something that is mysteriously highly thought of at certain physics departments (although not so highly thought of at many others), see

http://www.amazon.com/Cosmic-Landscape-String-Illusion-Intelligent/dp/0316013331

29. **Hank**
June 26, 2010

Certainly a damaging finding. I remain unconvinced that entrenched bias will lead to better science.

Summer School

June 26, 2010
Categories: Uncategorized

This is the time of the year when young particle physicists usually want to go to school, more precisely, a "summer school" held in some pleasant location. These typically have a series of survey lectures on the hot topics of the subject, aimed at the level of advanced graduate students and postdocs. These days, the lectures are often available on-line in some form, so anyone interested in learning some more about currently active research topics can do so, even if they have to miss the summer travel aspect of the school.

Here's a partial list of some of the larger such programs:

Theoretical Advanced Studies Institute in Elementary Particle Physics: String theory and its applications, Boulder, Colorado June 1-25

Summer school on structures in local quantum field theory, Les Houches, June 7-25

50th Cracow School of Theoretical Physics, Zakopane, Poland June 9-19

2010 European School of High-Energy Physics, Raseborg, Finland June 20 -July 3

Summer School on Mathematical String Theory, Blacksburg June 21-July 2

Cargese Summer School on String Theory: Formal Developments and Applications, Cargese, Corsica June 21-July 3

PASI School on Quantum Gravity, Morelia, Mexico June 23-July 3

Prospects in Theoretical Physics: Aspects of Supersymmetry, Princeton July 19 – July 30 (why Princeton as a location to travel to for the summer is a bit of a mystery…)

Cargese Summer School on Physics at TeV Colliders, Cargese, Corsica July 19-July 30

International School on Strings and Fundamental Physics, Munich July 25-August 6

PSI Summer School on Particle Physics: Gearing up for LHC Physics, Zuoz, Switzerland, August 1-7

SLAC Summer Institute on Neutrinos, Stanford, August 2-13

Clifford Johnson is blogging from the quantum gravity school in Morelia, where he's shocked to find the "totally bizarre" situation that the students there aren't very enthusiastic about string theory. He attributes this to their ignorance:

> Here's the really odd thing about all this … : While this is a school on Quantum Gravity, after talking with the students for a while one learns that in most cases the little they've heard about string theory is often essentially over 20 years out of date and almost always totally skewed to the negative, to the extent that many of them are under the impression that string theory has nothing to do with quantum gravity at all! It is totally bizarre, and I suspect it is largely a result of things that are said and passed around within their research community.. So there are a few students here and there who have some familiarity with strings, huddling together at times for warmth in a sea of miscommunication, misinformation, and strange preconceptions.

I find it extremely hard to believe that the students at this school are ignorant of claims that string theory is a unified theory including quantum gravity, more likely they're just unconvinced and more interested in other approaches.

Lubos reacts to this by noting that Clifford is finally encountering reality:

> Clifford Johnson seems to be surprised that almost all the students have been brainwashed by various anti-stringy misconceptions. Clifford has clearly been living outside the reality at least for 4 years, and so have many other serious high-energy physicists.

and then goes beyond Clifford, arguing that the problem is not just ignorance, but sub-normal intelligence:

> There's no string theory group in Mexico – another fact that shouldn't be shocking given Mexico's average IQ around 85. The IQ increment needed to go from non-stringy quantum gravity to string theory is around 20.

Comments

1. **Mantis**
   June 26, 2010

   Lubos rant is really hilarious.

2. **alfredo**
   June 26, 2010

   Hi Woit,

   you write:

   "I find it extremely hard to believe that the students at this school are ignorant of claims that string theory is a unified theory including quantum gravity, more likely they're just unconvinced and more interested in other approaches."

   I would love to agree with you on this, but I can't. It is a fact that the high energy and gravity community in Mexico is out dated and has promoted a negative attitude towards not only string theory but basically any new physics idea in the past 20 years. By this I do not mean that there has been a critical attitude (that would be nice), it is just negative as a matter of "principle".

   One of the many consequences of this (the one that I think hurts the most) is that students usually work on old and uninteresting (in the sense of the community) problems. This in turn disables them in terms of competition in the international market. In Mexico it is still relatively easy to find a position at a state University. There is no much competition.

Of course there are many other factors involved, but I do think the attitude of the old guard has been essential.

Now, regarding Lubos. I don't think you should take it to seriously when he comments on the IQ stuff. In some sense, I wish that was the problem! He likes to tease, and the you fall!! 😉

Un abrazo….

3. **Anonymous**
June 26, 2010

You have to be up to date with current literature for your ideas to be taken seriously. That's about the only difference between a crackpot and a serious scientist with a non-mainstream research interest. Even if the current literature (such as string theory) may turn out to be crap, people who think they can work on something (such as quantum gravity) without knowing the current literature is destined to reinvent the wheel again and again and make little progress.

4. **Ricardo**
June 27, 2010

Hi Peter,

Regarding Lubos comment there is a string theory group in Mexico, although small, and they even have annual meetings:

http://fejer.ucol.mx/elena/mexicuerdas/index.html

I am a grad student at UNAM, and you're right when you say there is a group of people "unconvinced and more interested in other approaches" when it comes to strings as a unifying theory, I myself am one of them. I should also mention that people working in string theory in Mexico are more geared towards duality than finding a TOE, at least in my perception.

I agree with Alfredo (Hi Fefo) that there is a tendency from the old school to resist change, but then again I know people who are working on loops, noncommutative geometry and some other less "trendy" stuff

5. **Peter Woit**
June 27, 2010

Alfredo and Ricardo,

Thanks a lot for the comments on the situation in Mexico, very interesting to hear. I'm still dubious though about Clifford's claim that Mexican students "are under the impression that string theory has nothing to do with quantum gravity at all!".

As for whether Lubos is serious, that's always hard to tell, but I do think that the ideas that string theorists are smarter than other theorists, and that skepticism about string theory indicates a lack of intelligence are among his deepest and most sincerely held beliefs.

6. **Roger Schlafly**
June 27, 2010

Why is that hard to believe? Did someone get a Nobel prize for using string theory to solve quantum gravity? Is there some scientific paper that proves that there is some relation?

7. **Bill K**
June 28, 2010

"people who think they can work on something (such as quantum gravity) without knowing the current literature is destined to reinvent the wheel again and again and make little progress."

It depends on who you are. Feynman used to say, if you read what's in the literature you'll just repeat the same mistakes they made.

8. **Cesar Laia**
July 1, 2010

Just crashed in your blog, and I am really shocked with the IQ stupid joke.

I had to see it, and rest of the text by Lubos did not help.

9. **Lee Brown Jr.**
July 2, 2010

I really enjoy this blog. I have been working independently on quantum gravity after being out of school and completely out of physics for a decade. I think I am representative of the newer generation.

One major reason why I avoid string theory, is that the best minds have worked on it for 30 years, and to what end? There is no result you can point to and say that it was a real contribution to our understanding of Nature. Not only that, there is nothing you can point to as a significant calculation.

If guys like Witten get nowhere (from an outsiders perspective), then what chance is there that a more human mind will do better? Moreover, if an idea is truly insightful and meritous, then usually it reaps rewards in fairly short order. SR, GR, Schrodinger Equation, the Dirac Equation, QED, QCD , all contributed soon after proposition.

Younger researchers are not as emotionally and professionally invested in string theory. Abandoning it for more fertile, more tractable pastures only makes sense.

10. **Valentina**
July 2, 2010

It really "amuses" me to see where the link about Lubos takes you to.
I'm Italian so I apologize for my English but, as we say in my country, I think the guy needs a good one, where one means psychiatrist.

His pretending to be some kind of superior being just testifies the huge inferiority complex he must have.

Sorry to be so rude. I'm really "intolerant" towards racist people.

11. **Marcus**
July 2, 2010

I'm curious to know what the mix of students at the PASI quantum gravity school was like. Can anyone say roughly what percentage North/South American? Or what universities they mostly came from?

I'm happy that there was finally such a panamerican QG school. Already in Europe there have been several QG schools of this type—one or two weeks, for graduate students (and others) starting research and wanting an introduction to active topics. I think the first European QG school was in spring of 2007 at Zakopane ski resort in Poland. About time this practice gets a start in this hemisphere.

12. **Ian**
July 5, 2010

Check out:
http://www.phdcomics.com/comics/archive.php?comicid=1335

13. **Chris Oakley**
July 6, 2010

Ian,

The issue, ultimately, is funding. If, in industrial research, your boss offered to pay £1,000 for you to go on a conference/workshop/symposium in Barbados/Bermuda/Bahamas you would probably say, "No thanks, I'd rather have the money – then I can go to one of these places with my family and not have to sit through a whole lot of boring seminars." This option is not available in academia – no cash is on offer if you choose not to go. So think of the whole conference/summer school thing as a charity that provides exotic vacations to those on meagre salaries.

14. **Charles**
July 14, 2010

The is also the school in Argentina:
http://dl.dropbox.com/u/2114657/school2010/index.htm

LHC vs. Tevatron Update

June 28, 2010
Categories: Experimental HEP News

Over the weekend the LHC had a first successful physics run with nominal intensity beams, in 3 bunches. A peak luminosity of about 5 x $10^{29}cm^{-2}s^{-1}$ was achieved, and the total integrated luminosity per experiment is now around 30 nb^{-1}. While this is quite a bit behind optimistic schedules of earlier this year, it may now be possible to much more quickly increase the LHC luminosity as the number of bunches is increased. The current plan foresees an integrated luminosity of about 1 pb^{-1} in July, and another 3 pb^{-1} in August.

The report about this from BBC News has the LHC's Mike Lamont trash-talking about the Tevatron:

> "It's clear that the LHC is the new boy in town, but in two years running we're going to put Fermilab out of business," operation group leader Mike Lamont told BBC News.

John Ellis is enthusiastic about the possibility of producing black holes:

> Professor Ellis added that as the luminosity increases, one of the things physicists at Cern will be looking for is a mini- black hole.
>
> "It would be absolutely, fantastically exciting if we produced black holes at the LHC," he said.
>
> "Then we would test our ideas about gravity, quantum physics, string theory. This would be much more exciting than finding a… Higgs boson or even dark matter."

Meanwhile, over in Batavia, the Tevatron has been regularly operating at peak luminosities of 3-4 x $10^{32}cm^{-2}s^{-1}$, nearly a 1000 times that of the LHC, accumulating integrated luminosity of around 50 pb^{-1} a week. They're getting the total number of collisions produced at the LHC this year about every couple of minutes. So far this year they are doing even better than planned, with over 2000 pb^{-1} of integrated luminosity in FY 2010. Last week, the Physics Advisory Committee met to consider plans to get in Mike Lamont's face, and keep operating the Tevatron past its planned closing date of end FY 2011, possibly for another three years. This would take their total data set from about 10 fb^{-1} to possibly as much as 20 fb^{-1}. With this amount of data they expect to be able to provide 3-sigma evidence for a Higgs over the entire expected mass range, as well as stay ahead of the LHC in several different measurements, including the sort of possible non-SM CP-violating effects that recently have been in the news.

Update: More about CERN's competition with the Tevatron here:

> The LHC now has to produce as many collisions as possible in the next two years in order for the various experiments at CERN to essentially prove their worth among other established particle physics laboratories.
>
> The past failures of the LHC weighed heavily on operations group leader Mike Lamont who talked about some of the criticism from the media.
>
> "The Americans in particular can be quite aggressive," he told Deutsche Welle.
>
> "It's quite clear that we're competing with the States, and we've had setbacks, and you can see journalists occasionally being aggressive about that," he said. "I mean 'You're spending taxpayers' money, and you're still messing up,' which can be a fair comment."
> …
> Only if the experiments meet their goals for collected data by 2012, Lamont said, would CERN pull ahead of the research performed at the US-based Fermilab, a particle accelerator located near Chicago, Illinois that measures 6.3 kilometers in circumference.
>
> "We've got reach in energy, but they're still sort of chasing at our heels," Lamont said. "So if we can collect enough data in 2010 and 2011, we essentially put them out of business, then we can relax in 2012 and fix the properly."

Update: The latest news (01:51) on the LHC Vistar doesn't sound good: "soon access in US15 for fire brigade". The beam was lost around 01:00, soon after beams had been ramped to 3.5 TeV. US15 is an underground service cavern next to the ATLAS detector.

Update: Not clear what that was about, but as of 4:30 things are back to normal and they're getting ready to inject another beam.

Comments

1. **VVAA**
   June 29, 2010

   Wow, you seem disappointed that things went back to normal!

2. **Peter Woit**
   June 29, 2010

   Not at all, I'm very glad the problem wasn't serious, and they're now back in business, as of a few minutes ago colliding beams at even higher luminosity. That's great.

   I hope both the LHC and the Tevatron do great physics over the next few years, in a friendly and vigorous competition…

3. **nbutsomebody**
   June 29, 2010

   It is unlikely that Elis takes the proposal of black hole production very seriously. Last time I heard him (in february) he was very critical about the idea.

4. **informed bitch**
   June 30, 2010

   Dear Peter,

   the problem of the Tevatron experiments with the Higgs search is that they will have to face degrading performances of their silicon detectors in the second part of a 20/fb dataset. Silicon sensors are sensitive to the radiation dose they get, and those of CDF and D0 were not designed to withstand such high integrated fluxes. They are currently working fine, but signs of increased leakage currents are clear, and bias voltages need to be knocked up. Signal to noise has already decreased. For a while things will not directly affect the b-tagging efficiency, but we are on untested ground to a non negligible extent.

Another issue is that the predictions that are being sold for the Higgs reach at the Tevatron are based on 2xCDF, which assumes that D0 will eventually catch up with the better sensitivity that CDF is showing in the low-mass analyses. I know how hard CDF has worked on the low-mass region, and have serious doubts that D0 will ever match CDF there, especially given the serious undermanning of D0.

In summary, the reach for the Higgs at low mass is, IMHO, not as good as it is made to appear.

Best regards,

IB

5. **Bill K**
July 5, 2010

"A peak luminosity of about 5 x 10^29/cm^2/s was achieved, and the total integrated luminosity per experiment is now around 30/nb. While this is quite a bit behind optimistic schedules of earlier this year, it may now be possible to much more quickly increase the LHC luminosity as the number of bunches is increased."

Does the luminosity go up quadratically as the number of bunches is increased? I.e. does each bunch going one way get to collide with each bunch going the other way?

6. **lumo**
July 6, 2010

Luminosity and bunches ~ typically yes.
The luminosity goes as the product of the number of particles in each bunch, divided by the cross-sectional area. For convenience assume the beams have equal cross-sectional areas, say an ellipse with rms semi-axes (sigma_x, sigma_y). Assume also the bunches collide head-on and the longitudinal axes are coincident (not merely parallel). This is all fairly typical operation. Then if the circulation frequency is f_rev, the luminosity is

L ~ N1 N2 f_rev / (sigma_x sigma_y) * (factors of pi, etc)

Hence the units of luminosity are cm^-2 s^-1.

Suppose there are B1 bunches in one ring and B2 in the other ring, then

L ~ B1 B2 N1 N2 / (sigma_x sigma_y) * (factors of pi, etc)

Typically B1 = B2. Otherwise the operation is less than optimal. Also typically N1 = N2. In practice this is only approximate because some particles are lost during the acceleration process, one cannot guarantee that the transmission efficiency is equal for both rings. But one can say N1 ~ N2.

Because the beams are proton-proton, they are in two separate rings, and collide only at the interaction points. This is all the same for LHC and RHIC.

But in a p-pbar collider, or an e+e- collider, the counterrotating bunches pass through the SAME ring, in opposite directions. Then if there are B1 = B2 = B bunches, they collide at 2B points.
This leads to unwanted collisions in the ring arcs, which disrupt the beams. So the beams have to be artificially separated where collisions are not desired.

But to answer your question ~ yes.

7. **PhilG**
July 6, 2010

The bunches in one direction do not intersect with every bunch in the opposite direction because they are in separate rings and are only brought together at the points where the experiments are housed.

The exact number of interactions per turn depends on the filling scheme, i.e how they arrange the pattern of bunches in the buckets. For large number of bunches you can assume one collision per turn per bunch. This means the luminosity increases linearly with the number of bunches, not quadratically.

8. **lumo**
July 7, 2010

Mea Culpa! I stand corrected.
You are absolutely correct that a bunch in one ring does not collide with every bunch in the other ring. FYI in RHIC for the polarized protons, the polarization pattern is ++– in one ring and +-+- in the other ring. That way one gets all 4 spin combinations (++) (+-) (-+) (- -). I don't know if there are any unpolarized bunches.

9. **Chris Oakley**
July 7, 2010

Lumo,

I assume you cannot be Lubos Motl ("Mea Culpa" is not something he would be capable of saying), but this nickname is often used by him. Can I therefore suggest that you use another to avoid confusion? Or, better still, just use your *real name!*

Perelman Turns Down Millennium Prize

July 1, 2010
Categories: Uncategorized

The Clay Mathematics Institute today announced that Perelman has turned down the one million dollar Millennium prize:

> On June 8-9 CMI held a conference in Paris to celebrate the resolution of the Poincaré conjecture by Grigoriy Perelman. Dr. Perelman has subsequently informed us that he has decided not to accept the one million dollar prize. In the fall of 2010, CMI will make an announcement of how the prize money will be used to benefit mathematics.

There are various media stories appearing about this, based on an AP report, with a bit more detail:

> Jim Carlson, institute president, said Perelman's decision was not a complete surprise, since he had declined some previous math prizes.
>
> Carlson said Perelman had told him by telephone last week of his decision and gave no reason. But the Interfax news agency quoted Perelman as saying he believed the prize was unfair. Perelman told Interfax he considered his contribution to solving the Poincare conjecture no greater than that of Columbia University mathematician Richard Hamilton.
>
> "To put it short, the main reason is my disagreement with the organized mathematical community," Perelman, 43, told Interfax. "I don't like their decisions, I consider them unjust."
>
> …
>
> Carlson said institute officials will meet this fall to decide what to do with the prize money. "We have some ideas in mind," he said. "We want to consider that carefully and make the best use possible of the money for the benefit of mathematics."

Comments

1. **Simplicio**
   July 1, 2010

   And somewhere, Hamilton is wondering why Perelman doesn't just take the money and give him half a million bucks.

2. **Sakura-chan**
   July 1, 2010

   They should distribute it to the remaining 6 problems.

3. **"Shecky Riemann"**
   July 1, 2010

   gotta love the eccentricity of so many prodigy mathematicians…

4. **Dr.Kathrine Martinez-Martignoni**
   July 1, 2010

   This Perelman is really a very strange man.
   I can write only this because it is the first time that a man
   (a genius in mathematics but anyway always a man)is so "crazy"
   to refuse one million US-Dollars.
   I am really completely fascinated by this man.
   Good luck Dr.Perelman!!!
   Dr.Kathrine M. (Switzerland).

5. **David Santo Pietro**
   July 2, 2010

   My respect for Perelman is increasing exponentially. He has his principles and he sticks to them regardless. Seriously, how many people these days could turn down a million dollars on principle. Not only that, but he does so in a classy and non confrontational way. When given the opportunity to publicly rub the prize in the noses of the establishment mathematical community, Perelman chooses to rightly give credit to Hamilton in his brief statement. This even after he has had to endure mathematicians claiming credit for his work. When people talk about Perelman they sometimes do so in a condescending way, as if he was "crazy". From everything I've seen, Perelman has the clearest state of mind of anyone in this whole saga.

6. **Deane**
   July 2, 2010

   For me the greatest tragedy is not Perelman turning down the Millennium Prize but the apparent prospect of Perelman refusing to do any more mathematics, both because he seemed to love it so much and because he has and presumably would continue to contribute so much to the field. I am still hoping that Perelman is just toying with us, and in fact he is still pursuing his passion in the subject.

7. **Daniella**
   July 2, 2010

   This bitter, wierd little man named Perelman continues to manipulate the media and the good general publics sentiment by whoring out his persona. The man is clearly so desperate for fame, notoriety and being remembered after his own death that little else matters to him.

   Instead of graciously receiving the monies from this award and doing good deeds with it – such as charitable donations, helping his own family or himself he has selfishly chosen to spit in the face of the establishment once agin.

Principled? Sure – it's just a pity that his "principles" are geared towards ensuring his own fame at the cost of everyone else. When I studied with him at University he was not well liked by anyone and to be frank this is exactly why he was universally shunned. This media attention is further fuelling his lust for fame and power unfortuantely, just as he'd hoped.

8. **csrster**
July 2, 2010

Simplicio – There is a precedent. That's what Banting did with his Nobel Prize money for the discovery of insulin, sharing it with his assistant Charles Best. History seems concur with Banting's view that Best was equally deserving of the prize.

9. **chris**
July 2, 2010

i guess the most logicl recipient of the money should now be the institute he worked for. they sure could use that money.

10. **Sam I Am**
July 2, 2010

That a man turns down a prize that seems to him exorbitant and inappropriate should be respected, and one with curiosity should try to understand his motivations.

Perelman is right (provided he really said what the newspapers have apparently quoted him as saying) that Hamilton contributed as much as he to the resolution of the Geometrization Conjecture to which Perelman's work lead – Hamilton laid out the program of using Ricci flow, and proved fundamental results showing the viability of the approach, and even identified the obstacles which later Perelman showed how to overcome.

Perelman is right that such prizes are unfair and unjust. For every mathematician who receives such a prize there are several more who do work of similar depth who receive nothing. The sociological effects of such prizes are not always positive for the mathematical community, even if they are largely positive for the recipients (they can be negative for those who do not receive, but know they could have, who sink into bitterness). Vershik wrote something along these lines in a Notices of the AMS.

The notion that Perelman should accept the prize and give it to charity, and that by not so doing he is doing something wrong, is ludicrous, based on a precious and oppressive moralizing. He has not asked for the prize, he has not wanted the prize – it is not his to decide what to do with what is not his – if one thinks that such money would be better given to a charity than to a mathematician – then one's quarrel is with the Clay Institute, not with Perelman.

11. **Zilog**
July 2, 2010

Daniella, your statement is completely unfair. He's not desperate for fame – he IS famous already. And it's his ownr right to refuse what he thinks doesn't belong to him. That charity-things are just side-effect of money-hungry nature of other people, and it's good that Perelman doesn't obey them. Narrow-minded enough (your post).

12. **Mitch Miller**
July 2, 2010

I have no problem with him turning down anything, but I wonder why he decided to put the proof on the Arxiv in the first place. It seemed like he had problems with the math community before he got the attention based on this proof.

13. **killbill**
July 2, 2010

Being weird is cool in math and physics. Erdos is the paradigmatic example, but we all know people like him among our colleauges, who like to be eccentric for the sake of eccentricity. Perelman is entirely entitled to do whatever he wants with the award, but it is silly for his admirers to insist that he does not care about fame etc. You don't get that good without having incredible ambition. He is certainly more famous than most other Field's medalists because of his antics.

Case in point: My girlfriend, who is a veterinarian and couldn't care less about math, told me today after reading the news about this "crazy genius" mathematician who doesn't want a million dollars.

Ultimately, this entire thread is silly, because the guy is of course a genius. But it is fun to psycho-analyse him anyway.

14. **Eric Habegger**
July 2, 2010

I think there might be an added element that brings a reasonableness to the mystery that is Perelman. I think it makes sense because it also fits in with his mathematical talent. I absolutely agree with his ethics but the essence of the divergence from most of us in giving up 1 million dollars is that almost all of us would weigh the two elements, our morals vs our enjoyment of the luxuries 1 million could bring, and come down on the side of the money.

I wouldn't presume to read his mind but I think when you have such a rich an inner intellectual world as he does one's outer environment isn't quite as important as it might be to most of us. At least it makes me feel better to think about him that way. I tend to think some others bitter conclusions about Perelman reflects more on their subconscious knowledge of their dependence on the outer world for their contentment.

15. **David Santo Pietro**
July 2, 2010

Daniella, are you saying that Perelman devised a plan to get famous and remembered after his death that included dropping out of the mainstream mathematical community, moving in with his mother into an apartment in Russia, stopping all contact with the outside world, working on a problem no one could solve for a century, then after he solves it not talking to anyone about it or accepting awards for it? If this was truly all part of a grand scheme Perelman devised to get remembered after his death, he is an even greater genius than we are giving him credit for.

You seem to dislike him personally, and maybe it is warranted (professional mathematicians aren't necessarily the most personable people in the world), but I don't see how you can say that someone who has almost no contact with the outside world is a "media whore".

16. **Dr.Kathrine Martinez-Martignoni**
July 2, 2010

Daniella,
in this world there are people that ,for a million Us dollars, can kill
theirs mothers …and,fortunately,there are also (few) people who can refuse 1 million Us dollars!!!

The world is so nice , so beatiful and so interessant because there are so many different people who think differently.
I think simply that you have to respect Perelman (and his decisions) even if you don`t like him.
Dr.kathrine M.

17. **Rana**
July 3, 2010

Actually, this refusal makes some sort of sense. Hamilton was the one who laid out the program, and as astounding as Perelman's solution was, Hamilton should have been awarded the Millennium as well. I wonder why the Clay institute didn't include him.

18. **Alejandro Rivero**
July 3, 2010

At least now I understand why my own solution was not acceptable. I suggested to allow Perelman to propose a new problem to be managed by Clay institution. But if he does not consider that the mechanism of prices is fair nor good to promote maths, then of course he would not like to propose any.

19. **Lee Brown Jr.**
July 4, 2010

I say give the money to his mom. She's probably earned it.

20. **Anonymous**
July 4, 2010

Perelman has won my heart by giving credit to Hamilton. That's the standard of ethics that every respectable scientist must have.

21. **Pierre N.**
July 4, 2010

Chapeau bas to Perelman! In a very different way, this is as impressive as his proof. He just earned the Field prize of intellectual honesty as only he can judge how much he owes Hamilton.

22. **altf**
July 5, 2010

I agree with rana. Hamilton has made a great contribution to solve this problem (and also generally to mathematics) by his introduction of Ricci flow. For me, Perelman is on finishing touch (although it is the most difficult part at times), but Hamilton is the real founder.

23. **nbutsomebody**
July 5, 2010

A really off topic discussion. Have you seen this
http://scienceblogs.com/principles/2010/06/the_physics_of_the_imbecile.php

it seems Kaku is going along with Deepak Chopra in a new age hype.

24. **John Baez**
July 5, 2010

I think we should force Perelman to take the money.

25. **Coin**
July 5, 2010

I'm not sure how Perelman could communicate "just leave me alone" any more clearly than he already has.

26. **JG**
July 6, 2010

Perelman was widely regarded as the best geometer of the last century, even before his proof of geometrization – see his incredible proof of the soul conjecture, for example.

27. **Will Farnaby**
July 21, 2010

Perelman is not "an eccentric": he presents Asperger's syndrome – see also Glenn Gould, for example – and, like Gould, he's made the world a better place in his own particular way.

Various and Sundry

July 7, 2010
Categories: Uncategorized

- It seems that Jean-Pierre Serre now spends his time commenting on blogs.
- For those interested in particle physics history, there's an interesting article by George Zweig here about his role in the discovery of quarks (which he called "aces").
- There's a very nice new survey article by Mikhail Shifman about QCD, especially about hopes to exploit supersymmetric models to better understand non-perturbative issues.
- Blogger String Theory Fan still gets hits from the trackback to his blog entry over there. Trackbacks to hep-th papers from here still seem to be censored, but people find out about the postings anyway. For instance, the authors of this recent paper have put out a revised version adding a reference to the earlier calculation in the math literature pointed out here.
- If you like listening to talks by Nobel Laureates, there's a whole bunch here.
- One person who is more than distinguished enough to be a Nobel Laureate but isn't one since he made the mistake of being born too late is Edward Witten. Last week he was in Europe collecting other well-deserved medals: the Lorentz Medal in Amsterdam and the Newton Medal in London. Evidently he was giving two talks, one for the public and one more technical. The public one was entitled *String Theory and the Universe* and probably not to my taste. It should appear at some point here, but for now there's a report here at Physics World. Michael Green introduced Witten with the accurate title of "Master of the Path Integral". The more technical one may have better shown off Witten's mastery; it had the fascinating title of *A New Look At The Path Integral of Quantum Mechanics*, and I'm hoping it will appear soon here (or maybe a commenter who has heard the talk in Amsterdam or elsewhere can tell us more about it…)
- While I'm not sure how strongly Witten feels about string theory these days, there's not much ambiguity in the case of Michio Kaku. He was on the Colbert Report Monday night and has a recent blog entry arguing that We Physicists Are the Only Scientists Who Can Say the Word "God" and Not Blush:

> As you know, I work in something called String Theory which makes the statement that we are reading the mind of God. It's based on music or little vibrating strings thus giving us particles that we see in nature. The laws of chemistry that we struggled with in high school would be the melodies that you can play on these vibrating strings. The Universe would be a symphony of these vibrating strings and the mind of God that Einstein wrote about at length would be cosmic music resonating through this nirvana… through this 11 dimensional hyperspace—that would be the mind of God. We physicists are the only scientists who can say the word "God" and not blush.

If you're in New York and want to help him defeat a Cyborg Army on July 16th, see this.

- As for me, I'm heading soon for Patagonia to try and see another eclipse. After that I'll be traveling in South America for a couple weeks, won't be able to help with the Cyborgs since I should be somewhere around Lake Titicaca on the 16th. Comments may get shutdown temporarily here for a while, partly because of the hundreds of spam comments coming in here each day, not all of which get caught by the spam filter, making some on-going maintenance necessary.

Comments

1. **Tim van Beek**
July 8, 2010

> Michio Kaku…was on the Colbert Report Monday night

…and he got applause for announcing the upcoming invention of invisibility cloaks and time travels, so all you physicists: Get to work! You don't want to disappoint the customers!

(But if you are a little bit like me, you might want to hide behind a nearby bush, crying).

2. **DaveB**
July 8, 2010

Good luck with the weather Patagonia.

I have been planning to go climbing or trekking there for years, but it has a reputation as one of the wettest places on the planet.

3. **Doug Henning**
July 8, 2010

W.r.t. Witten, don't you mean born too early, since experimental evidence for strings won't be found until those light-year-radius particle accelerators get built a thousand years hence?

P.S. Enjoy Patagonia. Good luck hanging out with gauchos, rheas, titanosaur bones, and assorted Nazis-in-hiding.

4. **Dave B**
July 8, 2010

Don't forget the patagonian welsh!

5. **Mark Decker**
July 8, 2010

Michio!
"Reading the mind of God" sounds dangerously close to those fanatics who "hear the voice of God." Thanks for the heads up. At least we know how grandiose your string delusion has become.
And although you can make statements like that without blushing – please know that I am embarrassed for you.

6. **Roger Schlafly**
July 8, 2010

I don't understand the Witten comment either. Has any Nobel Prize ever been given for the sort of work that Witten does?

7. **Peter Woit**
July 8, 2010

Witten's work is not just mathematical, but covers a lot of ground. The more mathematical end of it has been the most successful, but that's partly because, in the thirty-some years of his career, no particle theorist at all has had the kind of success that leads to a Nobel Prize. If Witten had been born ten-twenty years earlier, I'd bet that he would have played some sort of important role in the development of the Standard Model, of a sort that would have involved a Nobel prize.

8. **twistingthenightaway**
July 9, 2010

Witten's "new look" is all about A-branes and provides a neat explanation of some things hinted at earlier in a paper of him and Gukov. He will publish it in due time I guess. Nothing new about quantum mechanics… that is, apart from Witten's clear explanation of some of the more arcane points of path integral lore.

p.s. As it was a whiteboard talk nothing will appear on the workshop website.

9. **AK-47**
July 9, 2010

Serre just corrected an absurd misinterpretation of the word character by "Nigel" . To say that Serre now spends his time commenting on blogs is a tad hyperbolic….

10. **schieghoven**
July 9, 2010

"Witten's "new look" is all about A-branes…"

I don't quite agree. Mostly the talk (at Imperial) was about deforming path integrals into the complex domain in order to improve the convergence… the A-branes came in at the end, as an application. I thought the preceding ideas were valuable in their own right.

11. **Mitch Miller**
July 9, 2010

Witten could have had a shot at a Nobel for asymptotic freedom if he had just decided to work for Gross right after undergrad.

12. **D R Lunsford**
July 9, 2010

There is an earlier retrospective by Zweig from 1980, here:

http://www-hep2.fzu.cz/~chyla/talks/others/zweig80.pdf

-drl

13. **Mike Shain**
July 10, 2010

Don't forget to read "In Patagonia" by Bruce Chatwin. Look at the reviews on Amazon.

14. **Shantanu**
July 10, 2010

Hi, Peter or anyone else,
Does someone know of any interesting new results or blogs form GR19 conference?

15. **D R Lunsford**
July 11, 2010

How's the weather in Argentina/Chile? Looks good for Easter Island. The eclipse perfectly brackets the World Cup Final, so 2012 may be early. If the Moai start blinking, run.

-drl

16. **Anonymous**
July 11, 2010

Witten could have had a shot at a Nobel for asymptotic freedom if he had just decided to work for Gross right after undergrad.

Asymptotic freedom is great, but discovering something that others would have discovered anyway (in hindsight) doesn't make you a genius of Witten's caliber. Einstein was a genius because without him, it might have been several decades later that GR was discovered.

17. **chimpanzee**
July 12, 2010

It was raining Sat Easter Island, partly cloudy for Sunday. Eclipse was clear (some wispy clouds), cloud interference @3rd contact. D. Fischer (German science writer) had clear skies from Patagonia:

http://twitpic.com/photos/cosmos4u

Cook Islands (Mangaia) & French Polynesia (Hao atoll) had cloud issues, but some got clear skies:

Spaceweather

18. **chris**
July 12, 2010

witten is a bit too formal for a physics nobel. but the proof of the cake is the eating … let's wait and see.

19. **martibal**
July 12, 2010

Anonymous: it seems that Hilbert was not far from discovering GR as well. And also special relativity may have been discovered, maybe not exactly in this form, by Poincare. This does not deny anything to Einstein genius, but making a great discovery is also a matter of luck, being at work at the right moment in time.
What about if physics (at least high energy physics) is just entering a period of one or two centuries without major discoveries ? This might be the case, not due to the lack of bright minds, but because nature and our current knowledge (e.g. we do not have the good experimental tools to get new information, and not the good mathematical tools to understand our current theories in a way allowing to make significant progress) may be such that there cannot be major discoveries in this specific argument before ages.
I have just read (do not remember where) that Einstein refused to be instructed in social science (and I guess biology as well ?) because he thought these were areas to well understood and there was nothing to discover here. Maybe high energy physics is to well understood at the moment and this is not the subject where big discoveries will be made.

20. **Peter Woit**
July 12, 2010

Unusually clear skies here in Patagonia, all the way down to the horizon. So we had perfect conditions and a beautiful eclipse just before sunset. Headed back to Buenos Aires, then on to Bolivia and Peru.

21. **Chris W.**
July 14, 2010

> I have just read (do not remember where) that Einstein refused to be instructed in social science (and I guess biology as well ?) because he thought these were areas to well understood and there was nothing to discover here.

I seriously doubt it. If anything, he thought that these fields were simply not amenable to the kind of theoretical understanding that he had come to admire in physics, and were therefore not to his taste. More precisely, they were replete with mind-numbing empirical detail, and lacking any examples of effective and testable unifying theoretical frameworks to make sense of all these observations.

22. **Sakura-chan**
July 15, 2010

Hi DRL,

In that paper of Zweig that you linked to, in the epilogue on page 36 he talks about his appointment to a leading university being blocked by a senior theorist. Is he talking about Chew?

23. **Charles**
July 15, 2010

You should have gone to Bariloche to do some trekking…it´s very cheap (or maybe visit el Glaciar Perito Moreno, but that is a lot more expensive).

24. **martibal**
July 16, 2010

Chris W.: well, my source may not be very reliable indeed, since this is the french wikipedia page:
“Il fait ses études primaires et secondaires à la Hochschule d’Aargau en Suisse, où il obtient son diplôme le 30 septembre 1896. Il a d’excellents résultats en mathématiques, mais refuse de s’instruire en biologie et en sciences humaines, car il ne perçoit pas l’intérêt d’apprendre des disciplines qu’il estime déjà largement explorées. Il considère alors la science comme le fruit de la raison humaine et de la réflexion.”
But this statement is not referenced, and does not seem to appear on the german or english version.

25. **Peter Woit**
July 16, 2010

Charles,

Spent the morning before the eclipse at Perito Moreno, which really is spectacular.

26. **Martin**
July 18, 2010

Judging from his website, wikipedia entry, the ever growing book no the subject and his scientific publications, Hagen Kleinert would surely think that he deserves the title “Master of the Path Integral” ;-).

27. **D R Lunsford**
July 21, 2010

Sakura-Chan, I doubt it. I have my own know^h^h^h^hopinion on who it is but will not spread rumors in public.

-drl

28. **anon.**
July 22, 2010

Hope you start blogging again soon. There is little worth reading on the internet at present.

29. **Peter Woit**
July 23, 2010

anon,

Thanks, although I seem to have chosen a good time to go on vacation. In Peru on the Amazon now, heading back home soon, at work again on Monday. Will attempt to resume your regularly scheduled programming soon after that.

New Higgs Results From the Tevatron

July 26, 2010
Categories: Experimental HEP News

Just got back from vacation this morning. Luckily I managed to be away for the blogosphere-fueled Higgs rumors, returned just in time to catch the released results which appeared in a Fermilab press release minutes ago. The ICHEP talk in Paris announcing these results will start in about half an hour, slides should appear here.

The bottom line is that CDF and D0 can now exclude (at 95% confidence level) the existence of a Standard Model Higgs particle over a fairly wide mass range in the higher mass part of the expected region: from 158 to 175 GeV. If the SM Higgs exists, it appears highly likely that it is in the region between 114 GeV (the LEP limit) and 158 GeV. The most relevant graph is here. It shows an excess of about 1 sigma over the entire region 125 GeV to 150 GeV, which unfortunately is nothing more than the barest possible hint of something actually being there.

Comments

1. **Wild Goose Chase**
   July 26, 2010

   Why is there a difference between “expected” and “SM=1” in the linked graph?

2. **bane**
   July 26, 2010

   On a different topic, one thing I’d have expected you to comment about is the recent “finding” that the proton’s charge radius appears different from that predicted by QED.

   http://en.wikipedia.org/wiki/Proton#Charge_Radius

   Presumably either the proton’s charge radius is described by something other than current QED model, or the mathematical model used to infer the charge radius from a proton+meson is wrong, both of which look like new clues about some issue in the area of field theories that you’re interested in. Any thoughts?

3. **Peter Woit**
   July 26, 2010

   WCG,

   The experiments are not seeing a signal, so the plot is of the 95% confidence level limit they can put on a signal/divided by the signal expected for a SM Higgs. The “expected” value for this is based on the expected performance of the detectors. Around 165 GeV they expect to be able to put a limit on the size of the signal below the SM value, and they do, ruling out the existence of a Higgs at that mass. Around 125 GeV they expect to only be able to get a limit about 1.8 times the SM value, so don’t expect to be able to rule out a Higgs at that mass.

   If there really were a Higgs at a certain mass, one expects that the experiments would start to see an excess above expected background, and this would make their 95% confidence level worse than expected. However, the excess they are seeing is still so small as to be quite consistent with no real signal.

4. **Peter Woit**
   July 26, 2010

   bane,

   That’s on a list of things to learn more about and see if there’s anything interesting to say. But I just got back from vacation a few hours ago….

   Update: for those who really want to discuss the proton charge radius question, this really isn’t the place.

5. **SpearMarktheSecond**
   July 27, 2010

   The q^2 employed in the old proton charge radius measurements is probably of order 1 GeV^2, while the new muonic hydrogen measurements probably have a q^2 of order the muon mass squared, or 0.01 GeV^2.

   Would it be very surprising if QCD caused evolution of the charge radius between those values of q^2?

   As for the Higgs, someday I’d like to dig out all the august testimony to Congress during the SSC days, where the leading figures of the field argued passionately for the SSC’s energy and luminosity, in order to test the Higgs hypothesis sufficiently.

   The LHC was not sufficient, they said. Of course a bunch of blurry estimates of the Higgs mass happened between then and now, and have pointed at the light Higgs and thus new physics soon. But maybe it was all Luck not being a Lady, and instead, we have a 800 GeV Higgs + some strongly interacting electroweak bosons, and now trace of new physics, and thus a desert.

6. **Eric Habegger**
   July 29, 2010

   I just watched Veltman’s video presentation about the Higgs at the Lindau Conferernce here:

   http://www.lindau-nobel.org/WebHome.AxCMS

   I thought it was an excellent presentation that gives a jargon free explanation of the search for the Higgs. It really is an outstanding talk and I came away with a much better understanding of the issues involved. I’m still digesting his final conclusion about why he thinks there is no Higgs. I’ll give a hint: It involves the corrections to the scattering amplitudes of the vector bosons that would require a Higgs particle. Second hint: the highest sigma for the Higgs is at one particular energy where there is no scattering correction required. I think he is onto something. I like the guy.

   Eric Habegger

7. **higgs**
   July 30, 2010

   Veltman has disbelieved in the Higgs since the beginning. (This despite the proof of renormalizability of Yang-Mills with SSB etc.) But does he offer any credible alternative for the gauge boson mass and their longitudinal polarization modes?

Witten Talk and Interview

July 27, 2010
Categories: Uncategorized

In conjunction with his receipt of the Newton Medal, Edward Witten gave a public talk in London (now available on-line here), and an interview (available as part of a pod-cast here).

Witten's talk was a rather polemical argument for string theory, in which he laid out his reasons for still feeling that string theory is on the right track. The video is in two parts, with the first part not especially interesting since it is pretty much word-for-word the standard arguments for string theory that he and others have been making since the mid-eighties. The second part has some more interesting content, including Witten's comments on the evolution of his own personal relationship to the subject. This started in the early eighties, before the 1984 "First Superstring Revolution", when he began studying string theory, feeling that it was an approach to unification that deserved more attention than it was getting.

A recurring theme in his talk is that "string theory" has gone through unexpected changes in perspective over the 30 years he has been working on it, with the unspoken argument being that some new change in perspective may yet make the current deadly problems of string unification go away. He takes an ambiguous attitude towards attempts in recent years to argue for a change in perspective to the pseudo-scientific "landscape", explaining the arguments of proponents while not signing on to them. In the podcast he says "I don't know if this is the right picture of the universe", in the talk it's "to my thinking we still need more clues to have a better picture whether that is the right interpretation."

It's interesting to compare Witten's pro-string theory arguments to the somewhat similar ones of his much less mild-mannered thesis advisor David Gross (see here for a posting about a recent talk by Gross). Unlike Witten, Gross is clear where he stands on the anthropic landscape, denouncing it as pseudo-science. One other crucial difference has to do with their discussion of upcoming LHC results. Here, Gross argues strongly that the LHC will see supersymmetry, and is willing to put money on the table to back this up. Witten's talk barely refers to the LHC, and while he argues that a point in string theory's favor is its relation to supersymmetry, all he's willing to say about supersymmetry or extra dimensions at the LHC is "it might happen if we're fortunate enough." The next interesting part of the string theory story may very well be what happens in 2013-4 when it becomes clear that supersymmetry and extra dimensions are not going to be seen at LHC energies. After paying off his debts, I wouldn't be surprised to see Gross take this as an opportunity to back off from the idea of string theory unification. On the other hand, in his talk Witten seems to be positioning himself for carrying on as before, by now making arguments for string theory that don't at all involve low-energy supersymmetry.

Witten ends his talk with the argument that we still know very little about what string theory is, and this implies that it remains an excellent subject for young physicists to start to work on. I suspect he sees all too well that among physicists the tide has changed, with students turning to other subjects as jobs in string theory dry up and prospects for progress on string theory unification look increasingly dismal. He's trying to counter this by restating the arguments that have continued to keep him interested in the subject.

Update: Clifford Johnson has some very interesting and accurate comments on Witten and on how others react to him here.

Comments

1. **Sakura-chan**
   July 27, 2010

   Thanks for posting this Peter! I agree, the second half is more interesting than the first.

2. **Chris W.**
   July 27, 2010

   I second those thanks.

   For me, the overwhelming impression is one of ongoing stagnation, combined with a melancholy nostalgia for the excitement that once surrounded the subject.

   I really wonder if a truly good and insightful idea in this field would be recognized as such by its current practitioners.

3. **Claude**
   July 28, 2010

   In a recent video interview, David Gross said explicitly that he would still believe in supersymmetry even if the LHC did not find it. So it seems that your opinion about Gross changing his mind if experiments contradict theory is somewhat optimistic.

   In addition, it is a pity how little string theorists are following the advice of Murray Gell-Mann, who suggested to clarify its basic principles, in the same way that Einstein clarified the basic principles of general relativity before starting calculations. A search on arxiv essentially leads to zero results.

4. **Bee**
   July 28, 2010

   Makes me wish I could have a look at the future and see what people will think about string theory in 30 years from now. Will you still be blogging then and keep us updated? 😉

5. **Theorist**
   July 28, 2010

   "The next interesting part of the string theory story may very well be what happens in 2013-4 when it becomes clear that supersymmetry and extra dimensions are not going to be seen at LHC energies."

   You don't know that. I agree that there's no reason to believe in SUSY or extra dimensions of any kind at the TeV scale (unless the latter is related to EWSB), but you make that statement with an (apparent) certainty that far exceeds what you know for sure.

   On a related note, 2014 is WAY too early to be ruling out anything that Tevatron won't have already excluded.

6. **Shantanu**
   July 28, 2010

HI Peter,
I haven't looked at the talk, as am travelling. However did Witten discuss any high energy prospects for string theory (such as proton decay, neutrino oscillations). 5 years ago, I attended [this](#) where Witten talked about proton decay predictions in some string theory model. Also I am guessing he said nothing about loop quantum gravity?

7. **Pmer**
   July 29, 2010

   after seeing the talk I am more sympathetic to string theory, since

   1. unifies gauge, diffeomorphism and super symmetries
   2. interactionns are global only, so e.g. space could be emergent 3. it is not some theory that gets quantized, but starts with both parameters a' and h (just imposing a commutator is not principled)

   these are important to the foundations of quantum mechanics

8. **Mitchell Porter**
   July 29, 2010

   "Dismissing" the landscape is itself not a scientific act. It is a question of fact as to whether string theory inherently predicts chaotic inflation; it is a question of fact as to whether we live in such a universe; and as things stand, an answer of yes to both questions is consistent with all the evidence.

   I can understand people being skeptical about anthropic reasoning – though Weinberg's argument is a good one – but you don't get to reject string theory just because it suggests that particle physics is highly contingent. Practical field theory is about model-building meant to explain observations; in the era of the landscape, so is string phenomenology. In other words, it's actually a return to business as usual, except that now you have a class of models that contain gravity.

9. **Peter Woit**
   July 29, 2010

   Theorist,

   Sure, that's my prediction, confidence level 95%…. For people who do expect supersymmetry to show up at the LHC, I'm quite curious to know what amount of data they will need to see before they admit it's not going to happen. In particular, what exactly are the terms of David Gross's bet? I was using the date 2014 based on the expectation that by then there should be about 10 inverse-fb of analyzed data at design or near design energy (6.5 or 7 TeV/beam). If no signs of supersymmetry are seen in that data set, I'd think that prospects for them showing up in 100 or even 1000 inverse fb would be slim.

   The LHC is going to be around for a long time, a very long time if you count various proposed upgrades. If those saying they are willing to bet on supersymmetry at the LHC are including the full life span of the machine with upgrades, betting with them would not be a good idea for someone middle-aged like me since I might not be around to collect.

10. **Peter Woit**
    July 29, 2010

    Shantanu,

    In the talk you refer to, Witten's conclusion is negative:

    "There is no hope of getting a real answer right now, because even if string theory is correct and even if nature is based on one of its GUT-lik realizations, there are far too many possibilities."

    He described string theory as the "only real idea about quantum gravity", which I guess was a comment on LQG (I told you the talk was kind of a polemical one…).

11. **Peter Woit**
    July 29, 2010

    Pmer,

    In Witten's talk he described the speculative hopes like the ones you mention that he still holds for the theory. I characterize his talk as polemical since he makes repeated overly-strong claims, not even mentioning the reasons for skepticism about them. I thought for a minute about examining these claims and explaining the problems with them, then realized "wait a minute, this is a complicated topic that I wrote a whole book about…"

    Mitchell Porter,

    Witten gave an exhaustive list of the reasons he thinks string theory is still worth pursuing. "String phenomenology" was not mentioned, I think for good reason.

12. **Chris Austin**
    July 29, 2010

    Hi Mitchell Porter,

    I had been under the impression that within the context of the KKLT or similar landscapes, it is not possible or practical to point to a specific model and say, "This specific model definitely has a cosmological constant small enough to be consistent with observations," the problem being that large positive and negative contributions, of very different origin, have to cancel to a precision of about 1 part in 10^{120}, and neither type of contribution is controlled to anywhere near the necessary degree of precision.

    If this is incorrect, or no longer the case, what would be a good reference to look for examples in?

13. **hmm**
    July 29, 2010

    Just a perhaps obvious remark about the previous comment by C. Austin
    :

I believe it is correct, but it is not a specific limitation of string theory.
In no current framework for theoretical physics do we have the ability
to compute contributions to vacuum energy to sufficient accuracy to claim that one would definitely reproduce today's vacuum energy to high precision.

It is important not to confuse specific problems of string theory (of which there may be many, or may be none), with general difficulties that face all theoretical models, and are not specific to one framework.
I believe the small value of the vacuum energy is in the latter category,
if one wants a normal predictive theory that explains the value, and does not want to resort to Weinberg arguments and vacuum multiplicity.

14. **Mitchell Porter**
July 30, 2010

Chris, I think you're right, but matching the observed cosmological constant is likely to be one of the last considerations in building a phenomenological model from string theory, precisely because it is supposed to arise from a complicated near-cancellation of many positive and negative contributions. Just reproducing the qualitative structure of the Standard Model in string theory is quite difficult and this offers guidance to the model builder that is far more direct. There are only a handful of ways to do it (that are known) and there are plenty of other issues to preoccupy the people working on each such possibility. At the moment I'm interested in orbifold models and there are a few papers on getting the cosmological constant there, e.g. hep-th/0603088. But it's an issue for the final stage of string phenomenology, along with particle masses and moduli stabilization.

15. **dan**
July 31, 2010

Mitchell Porter
How close are string theorists to "reproducing the qualitative structure of the Standard Model" along with such string phenomenology, along with particle masses and moduli stabilization and cc?

16. **Mitchell Porter**
July 31, 2010

I can't say. I'm a neophyte and remote from the centers of research. But from the literature I can say that what people try to do is to find string vacua which produce the "minimal supersymmetric standard model" (MSSM) or a supersymmetric GUT which reduces to the standard model. This is the qualitative part and has been done several times in several ways. Usually such models must contain extra particles that haven't been seen, and so you have to find vacua where those extra particles are heavy or otherwise undetectable at present energies.

So far as I can see, people are not yet presenting further refinements of these models in which *all* the standard model parameters are derived at once, such as the masses of the *observed* particles, but instead this final step is being tackled piece by piece. There are a lot of papers on how to get a heavy top quark, there are a few papers on how to give the up and down quarks small nonzero masses, and so on. It's all very incremental, and also framework-dependent (a mechanism for giving quarks mass in one class of model may not be transposable to another class of model), and even dependent on basic theoretical progress (e.g. arXiV:0707.1871).

17. **Bruno Galileo**
August 3, 2010

In the history of all of science, has a hunch such as string theory ever received so much attention and funding?

Truly, string theory and its handwaving ethos shped physics over the past twenty-thirty years. Even LQG adopted some of its tenor and tone, often stating in its own defense, "hey if string theory can be not even wrong then we have every right to be not even wrong too!"

One must wonder about all the lost opportunities. What physicists were shut out from the academy and funding? How many bright young minds were lead down a seemingly dead-end street? How many gained tenure not by science, but by politics?

As Witten was an undergraduate history/politics major, it would have been interesting to hear his take on how string theory politicized and polemicized science.

Best,

Bruno Galileo

18. **CNX**
August 5, 2010

As Thomas Kuhn stated in his book about Scientific Revolutions, in many cases new ideas/theories do not just replace the old ones by being more successful, but they simply outlive them: they only get established when the older generation of supporters for the one theory gradually die out and the younger generation prefers the new theory. I think this is the most likely scenario how String theory will fade out, if it does at all, and this can take a very long time(Too long for middle-aged people to witness?). However, if the theory continues to win the souls of the younger physics students, whether by its rosy prospects(?), virility(?) or career pressure from its established, influential practitioners, then it may still be able to survive many more generations. In that case, even during the entire lifetime of a younger person such as me fundamental theoretical physics may continue in its current shape, monopolized by one seemingly promising theory which does not live up to its huge expectations.

19. **horace**
August 5, 2010

> in many cases new ideas/theories do not just replace the old ones by being more successful, but they simply outlive them:

In many cases, like… what?

20. **Max Planck**
August 6, 2010

hello all,

i believe the quote you were looking for comes from max planck:

A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up with it. –Max Planck

21. **Mark Decker**

August 6, 2010

A great collection of quotes. This single post should serve as a resource for anyone on the fence and looking for string theory opinions from scientists who haven't lost their sanity.

Getting back to the Witten topic: Everyone talks about how much of a genius he is but yet he is a string theorist. What contributions has he made that will stand the test of time when string theory eventually falls by the wayside? I'm not being a wiseguy and I'm not taking shots at Dr. Witten – I was just wondering if anyone could clear that up?

22. **Peter Woit**
August 6, 2010

Mark Decker,

I wrote extensively about Witten and his non-string theory achievements in my book.

23. **Pmer**
August 15, 2010

Witten made clear in his talk that one cannot start with a classical theory and then quantize it to get M-theory. Rather, you have to start with alpha' and hbar together.

Can anybody expand on this? What is he talking about?

24. **Peter Woit**
August 15, 2010

Pmer,

I think this is just one aspect of the fact that we don't actually have any definition of M-theory or really know what it is. We really just have a conjecture that a theory exists with certain properties.

For first-quantized string theory, you can proceed in the usual way, "quantizing" a classical theory of strings. But this can only give you interactions through a series expansion in the string coupling (expansion in the genus of the string world-sheet) one that doesn't converge. M-theory, among other things, is supposed to give a theory valid at non-zero string coupling. One doesn't know how to get M-theory by "quantizing" a classical theory and Witten suggests will be impossible. But until you understand what the theory is a lot better, I don't see how you can be sure that it might not have some formulation that could be interpreted as a "quantization" of something.

Short Items

August 4, 2010
Categories: Uncategorized

- There has been some recent progress on increasing the LHC luminosity. Recent physics fills have peak luminosities around 2.5 x $10^{30}cm^{-2}s^{-1}$, total integrated luminosity is above 500 nb^{-1}, with a goal of getting to 1000 nb^{-1}=1 pb^{-1} this week. The current goal is to get to peak luminosity of around 1 x $10^{32}cm^{-2}s^{-1}$ this year, but there are only about 12 weeks left in this year's proton run. To achieve next year's goal of 1 fb^{-1} in integrated luminosity, they will need to get to peak luminosities around 2 x $10^{32}cm^{-2}s^{-1}$.
- According to a new preprint entitled It's On, with only 70 nb^{-1} of analyzed data ATLAS has already been able to rule out some parts of the huge parameter space of supersymmetry models, beyond that already ruled out by the Tevatron. These limits come from looking for missing transverse energy.

A story at Ars Technica says:

> John Ellis was quite a bit more optimistic; he expects that we might be seeing new physics once we've obtained somewhere in the neighborhood of a trillion events, which may happen as soon as this autumn. Since the Higgs boson, the ostensible target of the LHC, is in a noisy place, in terms of the other particle decays with similar signatures, we may actually end up seeing supersymmetry first. Since the experiments are so well-tuned, it may only be a matter of hours before it's flagged, and the rumors start to filter out.

A trillion events is about 10 pb^{-1}.

If there's no sign of supersymmetry in this year's LHC data, how discouraging will this be for those who expect to see supersymmetry at this energy scale?

- Besides supersymmetry, something else that experimentalists will be looking for in the initial LHC data will be a fourth generation quark. The Tevatron has been able to put limits of 300 GeV or so on the mass of such a thing, see Tommaso Dorigo's latest posting for more about this topic.
- Capitalist Imperialist Pig has a review of a movie with the title *String Theory*. It seems that this is actually a very popular movie title, used by at least two feature-length films (here and here) as well as three shorts (here, here and here). For some reason (as far as I can tell), no one has yet used *Not Even Wrong* as a film title.
- *Colliding Particles* is a well-done on-going series of films featuring experimentalists working at the LHC. There are six of them so far, and they're available on-line here.
- While I was away Erik Verlinde made the New York Times with his "entropic" theory of gravity. There's also a talk at ICHEP available here. This week he's promoting this at SciFoo, going on at the Googleplex, see a report here:

> So far all this is just an "intuition", Verlinde says. Now he needs to find the mathematics to prove it. Then he shrugs and says perfectly matter-of-factly that this was how Einstein started out too.

I really don't get this at all….

- From David Berenstein I learned about Jonathan Rosenberg's comic series Scenes From A Multiverse (some randomly chosen examples here and here).
- Finally, from Gordon Watts, a wonderful tale of the tenure process.

Update: One more. There's a very informative long piece by Stephen Hawking here about his scientific career.

Update: See Resonaances for more about the "It's On" paper.

Comments

1. **onymous**
   August 4, 2010

   If there's no sign of supersymmetry in this year's LHC data, how discouraging will this be for those who expect to see supersymmetry at this energy scale?

   In 2010 data? Not discouraging at all. In 2011 data? Fairly discouraging. In 2014 data? Enormously depressing.

2. **DB**
   August 4, 2010

   Since supersymmetry is in a permanent state of being always "just around the corner", failure to observe it will only strengthen such convictions.

   It is a self-reinforcing cycle.

3. **Peter Woit**
   August 4, 2010

   DB,

   I think a lot of that (although not all…) has been the LHC (or SSC) being "just around the corner", which has been the story for a long time now. So, failure to find evidence for supersymmetry at the LHC when it finally is working will significantly change the environment.

4. **DB**
   August 5, 2010

   I would say that the environment for SUSY and especially for String Theory (which is umbilically bound to SUSY) has already changed very significantly since you (and Lee Smolin) launched your crusade. Failure to find SUSY will consolidate this trend.

   But the true believers have an ace up their sleeve: split supersymmettry – it knocks the ball into the next court and keeps the faith alive. However, they won't be able to claim its around the next corner anymore, more like around the next galaxy!

5. **Roger**
   August 5, 2010

   DB – Split SUSY still predicts new physics at TeV energies, eg a stable gluino.

6. **DB**
August 5, 2010

Thanks Roger, I must have been thinking of Supersplit Supersymmetry :))

7. **SpearMarktheSecond**
August 5, 2010

Energy trumps luminosity easily in the beyond the SM search business… twas ever thus. But the Tevatron is the remnant of the US HEP program, so, it gets a bit more hype than is deserved.

The light Higgs is one kinda sorta exception. Amazing the level of hype the light (<2M_W) Higgs has produced.

8. **Anonymous**
August 5, 2010

In the article Hawking says he was lucky not to do a PhD project in particle physics at that time, because the Cambridge school was in support of the futile S-matrix program, and his work would have been forgotten by now…

Maybe today's graduate students starting a project in string theory should think about Hawking's words? (Though nobody knows for sure)

9. **ozanam**
August 6, 2010

here is an excerpt from a recent article by Stephen Hawking:

"….There was an exciting period culminating in the Les Houches summer school in 1972, in which we solved most of the major problems in black hole theory.

This was before there was any observational evidence for black holes, which shows Feynman was wrong when he said an active field has to be experimentally driven. Which is just as well for M theory [a relative of string theory that involves membranes]. "

Peter, comment ?

10. **Peter Woit**
August 6, 2010

oznam,

The problem with the idea of unification in 10/11 dimensions using string/M-theory is not that it isn't experimentally driven. It's that it doesn't work.

11. **Shantanu**
August 6, 2010

ozanam,
Eventually we did find observational evidence for black holes.
Also there were many papers on proposed observational tests of black holes by Salpeter, Zeldovich etc, which had specific predictions which were later vindicated.
whereas in string theory its more than 30 years without a single prediction

12. **stan**
August 6, 2010

"The problem with the idea of unification in 10/11 dimensions using string/M-theory is not that it isn't experimentally driven. It's that it doesn't work."

What does this even mean? String theory has many solutions, embedded in 10/11 dimensions, that look essentially like our world, and that unify the gauge and gravitational interactions into a common calculable framework. The problem is that there is too much flexibility in constructing such scenarios, and hence it is not predictive (at least so far), But I have a hard time interpreting "it doesn't work" as anything more than a debating stance.

13. **Peter Woit**
August 6, 2010

stan,

In science "not predictive" = "doesn't work". This is scientific methodology 101, not a "debating stance".

After more than a quarter-century of learning more and more reasons why string theory unification can't ever predict anything, claiming that it's reasonable to believe that things are going to some day turn around and string theory unification will become predictive, now that's a debating stance…

14. **stan**
August 6, 2010

Peter,

I don't think I'm being overly pedantic by suggesting that saying"it doesn't work" is either too vague or else misleading, if the goal is to actually impart some understanding here. There are obviously huge distinctions between theories which are a) internally logically inconsistent b) make definite and wrong predictions c) make no predictions at all (by themselves). Theories of type a) or b) either have to be discarded or modified. Theories of type c) are not necessarily wrong, but they do have to be supplemented by extra information, incorporated into some other larger theory, etc. in order to be viewed as successful scientific explanations.

These are pretty basic distinctions, but I honestly don't know which of these you are driving at (or maybe you just don't care). I personally believe that string theory is correct but of type c). I think we are going to need new ideas and physical input to make progress, but that our current understanding of string theory will be incorporated into this. I guess you think I'm just an irrational, deluded, string partisan.

The analogy with QFT is often made. It is of course nice that our world is described by a fairly simple gauge group. But it's easy to imagine a world governed by some hideously complicated gauge group and matter representations, maybe even strongly coupled. In such a world it could take ages before QFT would make successful predictions. Would you say that "QFT doesn't work" in this world? This is of course more or less what many people were saying in the 1960s after failing to understand the strong interactions.

15. **Peter Woit**
August 6, 2010

stan,

I've devoted a lot of time over the years to carefully writing a book on exactly the topic of what is wrong with string theory unification (and what many people consider far too many blog postings on the same topic). If you don't agree with or understand the argument of the book or the blog postings that's one thing, but you seem to be pretending they don't exist.

The argument you make that QFT is inherently no more experimentally testable than string theory is one I've answered many times, despite the obvious absurdity of claiming that the most successful scientific theory we have is not distinguishable from one that predicts nothing. Sure, you can imagine a universe governed by QFTs of such complexity that conventional scientific method would be useless. That's not the world we live in. The currently popular idea that we live in a universe governed by a string theory with solutions of such complexity that conventional scientific method is useless is logically possible, just like the idea that we live in a simulation run by aliens, but it's not science.

You're welcome to believe, as many highly competent physicists do, that some new advance is going to come along and fix the problems of string theory unification. But you should first admit that, given our current understanding of the subject, the problems are deadly and this "doesn't work". Maybe someone will figure out how to make it work, but I don't think the last quarter century of history is at all encouraging, so there's a lot to be said for getting people to move on and look for something else.

16. **stan**
August 6, 2010

Peter,

My interpretation of your position is that you firmly believe that the true physics lying behind the Standard Model is "simple" in some sense, and so the correct theory describing it will lead to testable predictions in short order. Since String Theory has not provided any such predictions it must be the wrong theory. That's fine, but I doubt you can put forth a shred of evidence supporting this — it's an aesthetic judgement. I don't feel compelled to hurl insults at you based on this.

What are these "deadly problems" you mention that I must admit to? As I said at the start, the problem with string theory model building is that it is too flexible, allowing for too many ways of constructing worlds that look like ours, hence not being predictive (exactly the same problem faced by all existing QFT based attempts at unification by the way). Everyone agrees additional input is needed, the disagreement is whether this means that string theory is incompatible with whatever this new input is.

17. **Peter Woit**
August 6, 2010

stan,

Sure, my belief is that particle theory will continue to advance in the same way that it always has: if we find a more accurate description of nature than the Standard Model, it will be one sufficiently simple for us to analyze it and confront it with experiment in a conventional scientific manner. This isn't just "an aesthetic judgment", it's more about my understanding of what it means to do science. Claiming that string theory unification involves some new way of doing science, necessitated by the complexity of its solutions, is just a cop-out and refusal to acknowledge failure.

The fact is that string theory unification allows no reliable and predictive computations of any kind. Why this is requires a long analysis of an incredibly complicated set-up, but whatever you think the reasons for this situation are, those are the "deadly problems". Again, you may believe someone will someday make them disappear, but right now they're there and looking at the history of the subject gives no reasons for such optimism.

18. **stan**
August 6, 2010

Peter,

We're starting to go in circles here. Let me just note that if we somehow were able to determine that we lived in String Theory Vacuum X of the sort that people have studied, there would be lots of predictions we could make. You think this could never happen, either because string theory has nothing to do with our world, or because even if it does it is still inherently incapable of making predictions — I'm still not clear what your position on this is.

People will jump en masse into any new line of thought that could lead to beyond the Standard Model predictions, based on string theory or otherwise. The fact that no attempt so far has been successful in this sense is seen by some as evidence that no simple answer is lying just around the corner. I personally think that expecting to find enlightenment based on greater mathematical sophistication is a poor judgement. Since you argue based on historical precedent I will do the same: I can't think of a single example in modern physics where a testable prediction has emerged from someone pursuing a mathematically minded approach. Advances always seem to come from applying old math to deep new physical insights. It's actually quite remarkable how unsuccessful mathematicians have been in directly advancing physics. It's amazing that all of known physics can be understood using math no more complicated than basic differential geometry and simple group theory. I say all this to challenge the viewpoint promoted by your book and your blog (and to prove that I have read them, at least in part).

19. **CWJ**
August 6, 2010

stan said: *"I can't think of a single example in modern physics where a testable prediction has emerged from someone pursuing a mathematically minded approach"*

I can:

General relativity.

The Dirac Equation.

There are probably scads more.

20. **jpd**
August 6, 2010

whats so special about 'modern physics'
eg fourier and newton ?

21. **stan**
August 6, 2010

General relativity.

The Dirac Equation.

Neither of these are good examples. Riemannian geometry was at least 50 years old when Einstein used it. Everything in physics obviously uses math that was new and sophisticated at one time, but I was looking for examples involving new mathematical ideas.

Neither did Dirac use any sophisticated math. He wanted a Lorentz invariant first order wave equation, and found that he needed objects obeying anti-commutations relations. By trial and error he found that some 4 x 4 matrices did the trick. I don't think he needed to consult with mathematicians or read math papers to do this!

22. **stan**
August 6, 2010

whats so special about 'modern physics' eg fourier and newton ?

Up until one or two hundred years ago, there wasn't much distinction between a mathematician and theoretical physicist. Since then things have diverged, and mathematicians have adopted a style different from physicists. I think history shows that their style has not led to advances in the search for new physical laws.

23. **Peter Woit**
August 6, 2010

stan,

The problem with the argument that "if we could determine the string theory vacuum" all would be well is that we can't determine the string theory vacuum. The reasons for this are complicated, having to do with inherent limitations on what is calculable for each possibility in the current string theory set-up, together with the huge number of possibilities.

I have looked closely at attempts to do string theory phenomenology and to make progress on getting to something that would do what you argue for. Everything I've seen is ridiculously far from being useful. The fact of the matter is that string phenomenologists have had 20 years to prepare for the opening of a new energy scale at the LHC, it's here, and they have no predictions at all. If this isn't failure, I don't know what is. The idea that new LHC physics is going to match some string-inspired vacuum and justify string theory is so far-fetched that even most string theorists I know dismiss it as completely implausible.

You're right that my own judgment about ideas worth pursuing involves trying to get inspiration from mathematics. This has worked out sometimes in the past (e.g. general relativity), but it's true that most of the time it has not been fruitful. I'd argue that this is because historically experiments were throwing up things that disagreed with theory, and pursuing these as hints was far more likely to lead to progress than pursuing mathematical hints. The problem now is that we don't have much in the way of such hints from experiment, so may not have any choice but to try and make progress along other, more difficult lines. In any case, I think this is a time people should be trying all sorts of different approaches, since no one has good evidence that of any particular speculative approach working out.

24. **stan**
August 6, 2010

Peter,

I agree with you that it is very hard to get particle physics predictions out of string theory. Ergo, if our world is described by a complicated string theory vacuum, doing fundamental physics is going to be very difficult. The non-sequitur comes when you present this as evidence that our world is not described by such a string theory vacuum. This makes no logical sense unless you are starting from the axiom that physics should be simple, in which case the whole argument is circular.

Actually, I hope you're right, and I wished I shared your optimism.

25. **Bruno Galileo**
August 6, 2010

woit writes, "You're right that my own judgment about ideas worth pursuing involves trying to get inspiration from mathematics. This has worked out sometimes in the past (e.g. general relativity), but it's true that most of the time it has not been fruitful. "

actually, general relativity was inspired not by mathematics, but by the very *physical* reality of gravity.

you will recall that einstein hired mathematicians to figure it out.

to not take my word for it, but heed freeman dyson:

In Disturbing the Universe, Freeman Dyson writes, "Dick [Feynman] fought back against my skepticism, arguing that Einstein had failed because he stopped thinking in concrete physical images and became a manipulator of equations. I had to admit that was true. The great discoveries of Einstein's earlier years were all based on direct physical intuition. Einstein's later unified theories failed because they were only sets of equations without physical meaning. Dick's sum-over-histories theory was in the spirit of the young Einstein, not of the old Einstein. It was solidly rooted in physical reality." In The Trouble With Physics, Lee Smolin writes that Bohr was not a Feynman "shut up and calculate" physicist, and from the above Dyson quote, it appears that Feynman wasn't either. Lee writes, "Mara Beller, a historian who has studied his [Bohr's] work in detail, points out that there was not a single calculation in his research notebooks, which were all verbal arguments and pictures."

Hype or Not Hype?

August 6, 2010
Categories: This Week's Hype

The high point of my expertise in condensed matter physics was about thirty years ago, when I studied the subject in order to pass one of the general exams at Princeton. At the party after the test was graded, Phil Anderson came up to (after a fashion…) compliment me, noting that he was glad to see that even though I hadn't been able to solve one of the condensed matter problems, I had known enough to realize that the calculation I was trying to do was giving a result that couldn't be right and had written that on the test.

Since then, my little understanding of the subject has slowly decayed over the years, so I'm in no position at all to evaluate claims made about new advances. Recently there has been a lot of interest in applications of gauge/gravity duality to certain condensed matter systems, and this week there's a new article out in Science (not available on the arXiv itself, but based on this arxiv preprint), together with a press release from MIT. This has led to news stories headlined String Theory Explains Superconductors, and String theory and black holes show a possible path to practical superconductors. This latest story starts off:

> A leading candidate for room temperature superconductors is the copper compound cuprate, but no one knew how cuprates facilitated superconductivity…until some brave souls looked inside a black hole and broke out the string theory to explain how they work.

So, hoping that there might be someone expert on this out there and willing to comment, what's the verdict: hype or not hype?

Comments

1. **Professor Elephant, Ph.D.**
   August 6, 2010

   Hype. Without much doubt.

2. **Dr Ron**
   August 6, 2010

   Well, it's 30yrs since I worked in condensed state physics for my PhD, and it had pretty well lost its way back then. To my casual glance, this is the application to the condensed state of some peculiar substances of some of the well-established mathematics that has been developed in the course of studying string theory, just like we happily fiddled with Green functions way back when. Perfectly plausible. Damn all to do with String theory per se, but it makes a better headline.

3. **Peter Orland**
   August 6, 2010

   It sort of makes sense to use a new tool (i.e. AdS-string methods) to try to study as many problems as possible, and that's my impression of what is going on.

   Like you, Peter, I not a condensed-matter insider, but I try to keep up with cond-mat. In the two or three years many people have tried to use string theory in condensed matter physics, but I don't think there is a definite outcome. The big problem in high-T_c (from my ignorant standpoint) is non-Fermi-liquid behavior (in many contexts). I am not aware of any definite progress on this front using any method. I don't have any specific comment on the paper you mention, though.

4. **Peter Orland**
   August 6, 2010

   OK, I just glanced at the paper. It seems to be an attempt to describe a non-Fermi-state using anti-deSitter methods. It is not a microscopic approach, though. With no microscopic theory (e.g. a Hamiltonian for the electrons/holes) it isn't clear to me what it all means.

5. **Peter Orland**
   August 6, 2010

   I suppose one can have an effective theory (like the Ginzburg-Landau approach). I can't see whether this string-AdS model is even an effective theory in that sense, since it seems to just start with some high dimensional system. Maybe a real non-Fermi-liquid should behave the way the authors claim, but it is not clear to me.

6. **neo**
   August 6, 2010

   I don't know whether it is hype, but it seems simple to decide. Theorists need simply to show how string theory provides a single prediction about superconductivity that is not predictable without string theory but which is or eventually can be confirmed. Then everyone will pay attention.

7. **Roger Schlafly**
   August 6, 2010

   Peter, you are going soft on us. The p.r. even has Polchinski saying physicists can throw in some gauge/gravity duality whenever they cannot understand a system. Do you really think that there is some chance that black holes will explain the conductivity of rare metal compounds?

8. **Anonymous**
   August 7, 2010

   It would be interesting to find out Xiao-Gang Wen's opinion about AdS/Condensed matter. Originally a student of Ed Witten, now a professor in the condensed matter theory group at MIT, his research has spanned string theory, quantum gravity, and (NON-string related) high Tc superconductors. (Though I'm not sure whether he still follows string theory.)

9. **spin fluctuations**
   August 7, 2010

   I have recently been involved in the new iron superconductors. Judging by who gets invited to give lectures at conferences, it seems theorists who do first principles calculations have made important contribution in this field than people who study various models. There are some papers written by Sachdev, Viswanath, etc. about superconductors, but most people don't seem to pay attention to them.

In superconductors, the most important questions are what are the pairing interaction and symmetry. I don't think string theory has anything interesting to say about that and people in condensed matter theory and experiment don't seem to care about AdS/CMT correspondence.

10. **nbutsomebody**
August 8, 2010

Hype 75%
Not hype 10%
Who knows ? 15%

11. **nbutsomebody**
August 8, 2010

My previous rating was for the usefulness of AdS/CMT activity. If the question is about pairings in cuprate etc. (what peter asked) then

Hype 99.5%
Who knows ? 0.5%

12. **anonymous**
August 9, 2010

The claims are overstated in the press release, but AdS/CFT does provide an interesting handle on strongly coupled field theory at finite density, a subject that goes back several years. The MIT group's paper as well as one or two other papers in this area have found some of the hallmarks of strange metal behavior, but in models that are known not to be the correct theory for the cuprates or any other real material. The hope is that some aspects of the AdS/CFT theories can survive an extrapolation to real materials, but there has not been a concrete proposal for a systematic way to improve the AdS/CFT models to make them realistic.

13. **ab**
August 9, 2010

Not hype.

14. **Avidan**
August 9, 2010

You gotta be kidding, of course its hype. I hear numerous AdS talks on this superconductor crap and unfortunately read more on high-Tc than most of the speakers (and their advisors).

The definition of high-Tc superconductors according to string- theorists is: any gravity background, which if applied on with the correspondence, that is unlikely to work except for N=4, will produce something with funny dispersion relation.

Oh God, what has science has come to. God bless wall-street for giving these people something to do once they figure out that there are no jobs in this swindle.

15. **nbutsomebody**
August 9, 2010

ab,

Most of it is hype. Are you kidding ?

16. **Smurf**
August 9, 2010

The authors of the paper managed to construct a gravity background which seems to be dual to a system with non-Fermi-liquid-like behavior. (This is not the first paper on the subject though.) Everything else is hype, but what is not hype in today's media?

17. **nbutsomebody**
August 9, 2010

Although unrealistic the non-fermi liquid part is reasonably interesting. However any connection with cuprate superconductors and claim that gravity duals may be used to understand the high-Tc pairing mechanisim is pure bullshit !

18. **somebody**
August 10, 2010

"However any connection with cuprate superconductors and claim that gravity duals may be used to understand the high-Tc pairing mechanisim is pure bullshit !"

Its not clear to me that in high-Tc superconductors (which are strongly coupled) "pairing" is a good description because particles are an inherently weakly coupled notion. At best "pairing" can count the charge of the condensate in terms of the fundamental charge, but beyond that it is not really useful.

Anyway, the message that I draw from the holographic superconductor business is that many of the features of high-Tc superconductors are universal, and captured by gravitational systems via AdS/CFT. This is scientifically interesting. Nobody in the field is under the illusion that they have a theory for any *specific* cuprate. The problem is morally as hard as finding the standard model among the vacua of string theory.

The hype in the public media (if it exists in this context) is such an omni-present problem that I will leave that fight to braver souls.

19. **Anonymous**
August 10, 2010

AdS/QCD is remarkable because it is the *only* theoretical tool (not counting lattice QCD) which calculates quantities such as entropy / viscosity ratio. According to the MIT press release, AdS/CMT is also the *only* model that offers an explanation of the behaviour of "strange metals" such as linear dependence of resistivity on temperature. (In the article, Liu says "There's really no theory of how to explain that") If this is indeed the case, I would say they are probably on to something!

20. **Peter Orland**
August 10, 2010

Anonymous,

A strong-coupling calculation of the ratio of entropy to viscosity is found analytically (not numerically), without using the AdS correspondence in

http://arXiv.org/abs/0810.4181

21. **Peter Orland**
August 10, 2010

My point is not that one can't learn from the AdS correspondence in some contexts (I believe one certainly can). It is that it is just one tool among many (such as that mentioned above for the entropy/viscosity ratio), and is heavily oversold as solving heretofore unsolved problems. It's high time to solve some of these strongly-coupled problems, not to sell the proposed solution.

22. **anonymous**
August 10, 2010

There are already multiple ways to explain linear resistivity in the holographic setting (the one in the MIT press release, and Lifshitz theories and generalizations to name two). This may be theoretical progress, but the effective theory of real high Tc materials is an open problem, and still of great interest.

23. **somebody**
August 10, 2010

Sorry but I might be missing something. Seems to me that strong coupling expansions of the kind in that paper (0810.4181) are believable only a-posteriori. It is essentially a purely classical computation (even though naively it uses the path integral in terms of Wilson loops). It is not clear what renormlaization means here (in a theory like QCD this is not forgivable), it is not clear where the confinement-deconfinement transition is etc. The latter is essential because it is only in the deconfined phase that one can talk about viscosity to entropy ratio.

I agree it is not a worthless calculation, but to call it "another technique" seems a bit too much. Why did nobody do it until AdS/CFT? The authors quote the AdS/CFT result explicitly and one of the authors has written papers on string theory …

24. **nbutsomebody**
August 11, 2010

"Anyway, the message that I draw from the holographic superconductor business is that many of the features of high-Tc superconductors are universal, and captured by gravitational systems via AdS/CFT. This is scientifically interesting. Nobody in the field is under the illusion that they have a theory for any *specific* cuprate. The problem is morally as hard as finding the standard model among the vacua of string theory."

really ?, many features of high-Tc superconductors are universal, and captured by gravitational systems via AdS/CFT ? This is the most standard hocus-focus most AdS/CMT people will write in the introduction of their papers (well, I have to do it too :)). The whole point is that there is no reason to believe such claims.

25. **nbutsomebody**
August 11, 2010

It is undeniable that viscosity and entropy ratio calculation is a big triumph for string theory. Other applications of AdS/QCD, while not exact, are also sufficiently interesting. AdS/QCD is a standard part of various QCD and lattice literature. Most reviews contain some holographic results etc. atleast for a comparison purpose.

However the question here not so much with AdS/QCD but with AdS/CMT. Is it possible to learn something novel about condensed matter system by applying ideas of AdS/CFT ? I would like to stick with a resounding nooo…

26. **chris**
August 11, 2010

somebody,

your argument is really priceless. yes, 0810.4181 does make uncontrolled approximations – but at least it starts out with the right theory.

but well, if you think that this is far worse than some estimate in an N=4 maximally supersymmetric extension of QCD…

27. **somebody**
August 11, 2010

" many features of high-Tc superconductors are universal, and captured by gravitational systems via AdS/CFT ? This is the most standard hocus-focus most AdS/CMT people will write in the introduction of their papers … The whole point is that there is no reason to believe such claims."

Really? All you have is that ideological pronouncement?

Before AdS/CFT, how often has anyone been able to theoretically produce even the condensate curve of a strongly coupled superconductor? Getting generic (universal, if you prefer) condensates, conductivity, gap, etc. are simple via AdS/CFT. I am not repeating a talking point I read somewhere: it is simple and I know how to do it. The point is that this is still not enough in understanding any *specific* high-Tc superconductor.

To check what you call a "claim" for its veracity, you just need to open one of the papers and read beyond the "hocus pocus" in the introduction.

28. **somebody**
August 11, 2010

" … but well, if you think that this is far worse than some estimate in an N=4 maximally supersymmetric extension of QCD…"

All this is at finite temperature, so SUSY is broken. The bigger problem potentially is that N=4 SYM is conformal, whereas QCD is confining. But at finite temperature conformal invariance is broken and the deconfined phase is what we are after.

But yes, I agree that it is a matter of judgment.

29. **Peter Orland**
August 11, 2010

somebody,

We've been over this before. Yes strong-coupling approximations are not controlled approximations. But AdS/QCD is also a strong-coupling approximation.

This is a renormalization-group issue. The only correct way to make a strong-coupling approximation is to integrate out all the short-wavelength degrees of freedom. Unlike at weak coupling (near the fixed point, where the theory becomes simple), this means the effective theory is filled with irrelevant operators. Until that is done, I can't trust the claim that a lattice or stringy strong-coupling method comes from QCD. I don't know if you work on AdS/ACD, but many who do work on it, to their credit, understand this point.

30. **Peter Orland**
    August 11, 2010

    Anyway, the only reason I brought up the lattice paper on entropy/viscosity is to point out that AdS is not the *only* (claimed by one of the many Anonomouses) method for doing analytic calculations. The real problem is that there is *no* analytic method which is clearly correct.

    I think the situation is similar in condensed-matter physics. Ultimately the goal is to calculate using a microscopic Hamiltonian, not a model. Does this mean string methods are not useful? Of course not. It just means they haven't solved these problems yet.

31. **somebody**
    August 11, 2010

    Peter O., I am not sure I understand what you have in mind. For instance, it is not clear at all to me what you mean by microscopic description. From one of your previous messages it looks like what you want is to understand high-Tc superconductors in terms of a Hamiltonian for electrons and holes. Electrons and holes are useful only when the theory is close to a free limit. It is meaningless to talk about them away from it where the degrees of freedom can be completely different. If it was a UV free theory or something, there would be some meaning to saying that they *are* the true degrees of freedom (like quarks in QCD), but here we only care about the IR of the theory.

    In fact, this is the whole reason why it is not a Fermi liquid: high-Tc superconductors have charge carriers that don't behave like free particles on a fermi surface. It is a mistake to think that the microscopic Hamiltonian for superconductivity is comprised of electrons and holes: the microscopic Hamiltonian is that of the standard model of particle physics. It is indeed an unsolved challenge to see how the (dressed) electrons and phonons of BCS theory arise from the standard model. But we don't care, because that is not what we mean by solving superconductivity. What we mean when we try to "solve" superconductivity, is precisely to identify the right degrees of feedom where the problem can be understood quantitatively: we are trying to GUESS the degrees of freedom, because deriving it is impossible. This is what BCS did, for usual (weakly coupled) superconductors: they guessed that they were phonons etc. In a strongly coupled high-Tc system, if a gravitational/stringy system can be the right degrees of freedom, then that answers the problem as completely as BCS theory. Yes, a dual black hole is more dramatic than a phonon, but that is hardly an argument against it.

    Wanting to have a theory in terms of electrons and holes, as you say in a previous message is simply not the right approach, IMHO. If there is a string vacuum which can reproduce a /specific/ superconductor, then that /is/ the theory we want, and the problem is solved. I personally think it is a looooooong shot, but it does seem useful for understanding some generic features.

    PS: As a final comment, I use the word "hole" loosely, because you used it. Doping and superconductivity seem somewhat distinct to me: but I hardly know any condensed matter, so what do I know?

32. **nbutsomebody**
    August 11, 2010

    "To check what you call a "claim" for its veracity, you just need to open one of the papers and read beyond the "hocus pocus" in the introduction."

    Actually, I wrote few of them 🙂

    AdS/CMT does not produce a condensation curve which may not be reproduced by Landau-Ginzberg theory. However you are quite correct that calculation of conductivity and especially the mass gap etc. is indeed a novel result. I personally find it very interesting. However the results one gets is rather unphysical and mass gap is actually zero. It changes to a non-zero value as we shift to a non-abelian p-wave holographic superconductor. I do not want to go into more technicality as Peter does not like a lot of technical discussion in this blog. However the bottom line is quite clear that it is unclear (sorry for the pun) whether there is something generic about strongly coupled superconductors. Hence we may not say much about high-Tc etc from the study of holographic superconductors. The goal in high-Tc business is to learn about some specific superconductors. Generic results possibly do not have much role to play.

    Also look at Peter Orland's post about uncontrolled nature of whole AdS/CFT business. Another point is that here the system we are looking at is rather different than conventional superconductors. It is a lorentz invariant system, there is no lattice, it is a large-N gauge theory and the list goes on and on. The whole holographic superconductor business is actually just the study of bose condensation in a holographic context. That is why it is sometimes called holographic superfluid and has more closer relation to flavor superfluid phase in QCD phase diagram.

    To set the context correct there was no debate about the relevance of studying holographic superconductors. It is possibly interesting and relevant. Although it is unlikely that it would provide a deep insight in cuprates.

33. **Peter Orland**
    August 11, 2010

    "Wanting to have a theory in terms of electrons and holes, as you say in a previous message is simply not the right approach…"

    It's not? I thought it was the goal. Isn't that what we're supposed to do is describe phenomena in terms of the most basic theory we can?

34. **nbutsomebody**
    August 11, 2010

    Peter Orland,

    "It's not? I thought it was the goal. Isn't that what we're supposed to do is describe phenomena in terms of the most basic theory we can?"

    I think so. That is what I have been informed by my colleagues in condense matter.

35. **somebody**
    August 12, 2010

Peter O., contrary to the comments of the other somebody, there is strong experimental evidence that the system cannot be captured by a weakly coupled Fermi liquid. That is, free electrons etc. cannot be a good description in a high-Tc superconductor. (for eg. linear resistivity above Tc cannot arise from a weakly coupled description.)

I emphasize that even in the BCS theory, the degrees of freedom (not sure why you call them microscopic, they are strictly effective) were *guessed*. (Dressed) Electrons and phonons there, are effective, in no sense fundamental, and also we do not know how to derive them by integrating out some fundamental theory.

The thing about BCS that made things useful, was not that it was fundamental, but that these effective field theory d.o.f were weakly coupled.

36. **somebody**
August 12, 2010

"Actually, I wrote few of them "

So you were being disengenuous, and not ignorant. You have got me between the devil and the deep sea …

"AdS/CMT does not produce a condensation curve which may not be reproduced by Landau-Ginzberg theory."

No condensate curve can ever be /not/ produced by some Ginzburg-Landau theory. So this point is moot.

"However you are quite correct that calculation of conductivity and especially the mass gap etc. is indeed a novel result. I personally find it very interesting. …."

You seem to be going back and forth between this and the other extreme. About your other comments like p-wave etc.: the real high-Tc superconductors are d-wave, so I would say it is encouraging that a hard gap can be found by going from s to p.

But anyway, there are other problems when you get to d-wave because the spin is too high etc. My point: that some general characteristics of superfluid/superconductors can be captured by holographic methods is not bad. This already warrants further inevstigation. I am not going to do it myself pbbly, because I prefer more fundamental questions, but I am glad someone is doing it.

37. **Peter Orland**
August 12, 2010

Yes, somebody (capitalized Somebody?), we know it isn't a Fermi liquid. The disagreement is not a technical one, so there is no need for a technical justification.

Physics is supposed to explain things, as well as describing them. It's fine to derive Green's functions from a scheme, but you need to derive them from a theory. Schemes and theories are not (always) the same.

AdS methods are another tool and more power to the people who use them. The point people are making on this thread is that AdS/CMT is not enough. You have to justify it on the basis of something you know about matter. Similarly AdS/QCD needs to be justified from QCD. That, in a nutshell, is the problem.

38. **somebody**
August 12, 2010

I do not agree that the Fermi liquid issue is merely a technical point. The fact that high-Tc is a non-Fermi liquid is of fundamental significance in this discussion. You seem to want to apply the particle/hole/phonon idea in a situation where I see no reason why it should be applicable.

Also, why are you not unhappy that nobody knows how to derive BCS theory from standard model or QED? Why is BCS any more explanatory, than descriptive in that sense? Is it because you have a mental picture of a metal with free electrons etc. in it?

"The point people are making on this thread is that AdS/CMT is not enough. You have to justify it on the basis of something you know about matter."

From my side, it looks like you are just uncomfortable with the idea of a dual description, where the original degrees of freedom have re-organized (or at least re-expressed) into something completely different. You rationalize that discomfort by looking suspiciously at AdS/CFT.

Wanting a full proof of AdS/CFT is a noble thought and certainly interesting. But note that in the history of QFT it would've been a bad idea to first make the path integral rigorous. Working with it instead, was what lead to progress especially in the second half of the last century. My strong prejudice is that trying to prove AdS/CFT in a full way is a bad idea at this stage. I am convinced that AdS/CFT is correct, not just because of the specific and detailed tests, but also because the beautiful way in which too many different things (like details of gauge theories, gravity and black hole thermodynamics) tie together.

Also, this thread is really about the specific issue of condensed matter applications, not the general viability of holographic methods. I am not interested in the latter discussion because having worked with various aspects of gauge-gravity duality, I think a blog discussion is unlikely to change my opinions on it.

39. **Peter Orland**
August 12, 2010

Somebody, my discomfort isn't with dual descriptions (which I have worked on, by the way). I am not trying to cast suspicion on this idea or that idea. I am trying to cast suspicion on declarations of victory.

I and nbutsomebody (whoever he or she may be) are just trying to make a non-technical, unbelievably simple, point.

In physics we try to understand how behavior emerges from theories. No one is saying we should derive BCS directly from the standard model, which would be a very foolish thing to try to do. But B, C and S derived superconductivity from a theory of Nature (electrons and phonons). We can understand how this theory arises from QED, another theory of Nature. This in turn arises from the standard model, yet another theory of Nature. There are some mathematical gaps (the second law of thermodynamics, crystallization), but we have a pretty good picture of how it all this fits together.

Ultimately, we would like to understand non-Fermi-liquid behavior from a Theory of Matter. Phenomenological models are stepping-stones, not the answer. This is very basic to science.

Here is a better analogy than BCS. Kepler's laws are great, but they aren't enough to explain how the solar system works. A theory of mechanics and gravity is more fundamental. Maybe in the 17th Century, someone thought science should settle for Kepler's laws. Wasn't it a good thing some scientists didn't believe that Kepler's laws "explained" astronomy? I think so.

Having taught courses for non-physicists, I know that many non-technically-trained people understand this. Don't you?

40. **somebody**

August 12, 2010

"But B, C and S derived superconductivity from a theory of Nature (electrons and phonons). We can understand how this theory arises from QED, another theory of Nature."

No, we don't know how to get BCS from any more fundamental theory. This is the point. You are just comfortable with the idea that BCS *might* be derivable from QED, because the ingredients are not too exotic. The degrees of freedom are not the same as in QED, the vacuum we are working with is horrendously complicated (a metal), we really have NO idea. BCS were clever to ask the correct (effective) questions to get the correct (effective) answer.

If (a big if) holography delivers, this will precisely be the situation with high-Tc. The only difference is that the description of the system is in dual variables, in terms of black holes in AdS. It will be phenomenological, exactly to the same extent that BCS theory is. The comparison is not at all with Kepler's laws. It would indeed be great if we could derive effective theories from fundamental theories, but that is generically not possible with most mesoscopic phenomena.

In particular, it is not clear to me that the phonons etc. are somehow useful for any purported "microscopic" description.

"Having taught courses for non-physicists, I know that many non-technically-trained people understand this. Don't you?"

🙂

41. **Peter Orland**
August 12, 2010

What a lot of baloney!

Somebody, if you don't think that science uncovers deeper descriptions, then why do it?

42. **Peter Orland**
August 12, 2010

… and since you don't want to argue the real issue and prefer to hide in a smokescreen of jargon…

BCS applied mean field theory and a Bogoliubov transformation to a model of electrons and phonons. They didn't start with the answer, they derived it! BCS is not a phenomenological theory. I don't know where you came up with the nonsense above.

43. **somebody**
August 12, 2010

"Somebody, if you don't think that science uncovers deeper descriptions, then why do it?"

Its money for nothin, and chicks for free in physics. Thats why. 🙂

You will have better luck with me, if you phrased your commentaries as commentaries and not rhetorical questions. Lets not re-interpret my criticisms about specific misunderstandings you had, into some deep and profound differences in our "views" on science. Science is of course about deeper truths, what I am saying is that these deeper truths do not fit with your prejudices.

I imagine that there will be hell to pay for my glib comment above: Woit gets all politically correct when string theorists are being tongue-in-cheek. So I will be bowing out.

44. **Peter Orland**
August 12, 2010

My questions were not rhetorical. I was really curious.

You misinterpret my disagreement with you as a technical issue. It's really not technical. That's why I tried to turn the conversation to plain words, not jargon.

45. **somebody**
August 12, 2010

"BCS applied mean field theory and a Bogoliubov transformation to a model of electrons and phonons. They didn't start with the answer, they derived it! BCS is not a phenomenological theory. I don't know where you came up with the nonsense above."

Aaaaaargh, you drag me in again! 🙂

Where do you have phonons and dressed electrons in any fundamental theory, sir? QED doesn't have them! Is this so difficult to understand? What you call a derivation is what I call BCS theory already. That derivation is trivial, and one can essentially do away with it by just writing down the relevant operators once one identifies the degrees of freedom. The point is that it is forbiddingly difficult to derive it from a (microscopic) theory like QED where the degrees of freedom look different: photons, fermions, etc, are different from the phonons etc. of BCS. This is why BCS is an *effective* theory.

46. **Peter Orland**
August 12, 2010

Yes, the degrees of freedom look different. No, we can't rigorously derive electron-phonon systems from QED. But that doesn't mean we have no understanding of how they arise from QED.

I apologize if I have made you so frustrated. My friends (it's hard to believe I have any, I admit) and relatives all tell me how frustrating I can be.

There are outstanding problems in deriving certain results from deeper models. Some of us fall back on approximations, and sometimes hand-waving arguments. A small number of people try to prove propositions and theorems to make some of the connections more carefully (like how states of matter appear in more fundamental theories). I am not saying it is easy.

Your viewpoint seems to be that we can't really explain anything. Well, it's very tough, but occasionally we can.

47. **somebody**

August 12, 2010

Phew, at least we seem to be beginning to understand each other…. lol! Thanks for playing and thanks for staying honest.

Just one comment:

“Your viewpoint seems to be that we can’t really explain anything.”

Not all phenomena, just mesoscopic ones. In general I think trying to truly derive mesoscopic phenomena can be quite hard, but I am not sure thats the equivalent of surrendering. It is usually a detail thing, rather than a concept thing – or so we hope. Because usually, we in particle physics regard the low energy physics as somehow understood, even though it is not really understood. From what you say, it seems like this reductionist hope is ultimately what you vote for too.

But the interesting thing about AdS/CFT is that it is the great equalizer in energy scales: quantum gravity can be dual to things at lower energy scales. Its a bit like concept and detail are dual to each other. This reverse direction of the arrow is what is interesting from a fundamental physics point of view. A detailed explanation of high Tc superconductivity is secondary (at least to me) – no matter how interesting it might be, from a practical point of view.

48. **onymous**
August 12, 2010

Maybe an actual condensed matter expert can correct me, but my understanding is that the CM community doesn’t have even a *toy* calculable model that exhibits all the basic features of the cuprate phase diagram — antiferromagnetism, the pseudogap, superconductivity, a Fermi liquid phase at higher doping — or a good guess at the universality class of the suspected underlying quantum critical point. So *if* AdS/CMT could give a toy model that encompasses all of these phenomena, it might be of interest. My (not thoroughly up-to-date) understanding is that AdS/CMT can give toy models of critical non-Fermi-liquid-like phenomena, tunable to achieve any desired scaling behavior, and hence isn’t so close to doing this. But one could imagine it might be possible, and would count as a success.

As far as matching to *the* correct theory, though, AdS/CMT is dual to something with large N and large ‘t Hooft coupling, and thus is in the same situation as AdS/QCD; it’s a toy model that exhibits a lot of the right phenomena, but it’s *very* far from being *the* system one is interested in, and there’s little concrete hope for bridging the gap in the foreseeable future. This doesn’t make it useless, though; toy models are a worthwhile part of physics.

49. **nbutsomebody**
August 12, 2010

Somebody,

“No condensate curve can ever be /not/ produced by some Ginzburg-Landau theory. So this point is moot.”

It can be found. I am surprised that you do not know it.

50. **nbutsomebody**
August 12, 2010

“So you were being disengenuous, and not ignorant. You have got me between the devil and the deep sea …”

No I was not. It is natural in a scientific literature to speculate when it is explicitly mentioned as so. It becomes disingenuous when those speculations are advertised as a important scientific finding in the popular media.

“My point: that some general characteristics of superfluid/superconductors can be captured by holographic methods is not bad. This already warrants further inevstigation.”

Not bad and not too great either. There are various investigations going on even on less important matter. Surely it deserves some investigation and no hype.

“You seem to be going back and forth between this and the other extreme.”

I am not the center of the universe, nor are you. Just the fact I find something interesting does not mean it is extremely important.

51. **nbutsomebody**
August 12, 2010

Somebody,

“About your other comments like p-wave etc.: the real high-Tc superconductors are d-wave, so I would say it is encouraging that a hard gap can be found by going from s to p.”

Not a bad point ! I think one should write it up and I more or less know how to calculate it. Any interest of doing a “less fundamental” work?

52. **nbutsomebody**
August 12, 2010

onymous,

Very nice description. This is also my realization although I am not a CM expert. However as you have pointed out it would be nice to have model which produces all the feature of cuprates phase diagram in one place. It would be nice if some CM expert correct those speculations.

53. **Seth Thatcher**
August 16, 2010

Peter, I just discovered your blog tonight, or rather this morning and find it extremely helpful both in posts and comments to understanding the ongoing temporal battles in theoretical physics. I am a layman and must admit that I have been taken in by all the Michio Kaku’s of the world and their seeming confidence in the truth of string theory. It is only upon reading this blog that I come to realize that there is a sizable contingent of physicists who regard string theory as bunk. So now I must ask the question-What, if any, theories are there that take the place of string theory in whole or in part? That question is not meant insult. I understand that many physicists may think string theory is incorrect while not having any ideas to what might be correct given the fascinating and elusive nature of our universe. Are there competing theories? Where can I read about them? Thanks in advance and I will be visiting often even if I only understand about 1/100th of what is discussed here.

54. **Yatima**

August 16, 2010

>>What, if any, theories are there that take the place of string theory in whole or in part?

No-one knows. Indeed, is is not at all certain that we already have the mathematical tools to do it yet.

I recommend Roger Penrose's "The Road to Reality" as pretty good math-based "introduction". He doesn't refrain from proposing his own ideas in that Phone-Directory-Sized Wonder, but that's ok. One of the books that take a few years to read.

55. **SteveB**
August 16, 2010

Seth,

I recommend reading Peter's book: Not Even Wrong. Very good reading. The standard model description is good and especially how mathematicians have contributed to the physics. Peter is very reasonable about asking for some verifiable predictions from a theory. I have read Peter's book twice (so far). Also, I recommend Lee Smolin's book, The Trouble With Physics. I just finished it and it is also very good.

56. **Peter Woit**
August 16, 2010

Seth,

There are various competing ideas about how to get a quantum theory of gravity. Lee Smolin has written about some of them. There's an interesting more recent idea that a certain standard supersymmetric version of general relativity may avoid the usual arguments that standard quantum field theory can't give quantum gravity, but I haven't seen a popular exposition of that.

It remains true that there aren't any good competing ideas about unification: how to put the Standard Model and quantum gravity together. The excitement over string theory was mainly due to its promise to achieve this, a promise which hasn't worked out. So, right now we're in a situation where every known proposal for a unified theory has very serious problems. My own point of view is that this means that people should be casting a wide net to look for new ideas, and not spending more time on failed ones. But in no way do I claim that the problem is that there is some great idea about unification that is being ignored in favor of string theory.

57. **stan**
August 16, 2010

… how to put the Standard Model and quantum gravity together. The excitement over string theory was mainly due to its promise to achieve this, a promise which hasn't worked out.

Oh dear, here we go again. String theory does of course provide fully explicit and calculable solutions that unite the Standard Model and Quantum Gravity (there is admittedly some fine print here, which we can discuss if need be), and I'm sure Peter knows this. The hope was that this problem was sufficiently constrained as to admit a more or less unique solution, hence being predictive — a hope that seems not to be realized. So string theory succeeded in unifying the Standard Model and Quantum Gravity. What failed was the dream that this would be enough to make experimental predictions. Any attempt to go beyond the Standard Model has to face up to this fact. The truth is that only String Theory has been pushed sufficiently far to encounter this fact of Nature.

58. **nbutsomebody**
August 16, 2010

Stan,

It would be nice to know more detail about the string theory compactification you are talking about.

59. **Peter Woit**
August 17, 2010

nbutsomebody,

I suppose one could point out again to "stan" that the statement that string theory unification hasn't worked out is an uncontroversial one that even most string theorists agree with. But I fear that would be a waste of time…

Enough about string theory unification hype, unless someone has something new to say. Otherwise, please return to the topic of this posting, whether claims that AdS/CFT explains high Tc superconductivity are hype or not.

60. **Shawn Halayka**
August 23, 2010

"There's an interesting more recent idea that a certain standard supersymmetric version of general relativity may avoid the usual arguments that standard quantum field theory can't give quantum gravity, but I haven't seen a popular exposition of that."

That does sound interesting. I wonder if it would mean that hidden dimensions are effectively fictitious.

Do you know where to find documentation on this idea?

61. **martibal**
August 24, 2010

Well, this comment is off topic regarding AdS/CFT and high Tc superconductivity, but totally in the topic regarding "Hype or not Hype". Here is an article (in french)

http://www.rue89.com/2010/08/23/les-freres-bogdanov-exagerent-le-succes-de-leur-dernier-livre-163583

explaining how the Bogdanoff are exaggerating the success of the their new book (taking into account what they claim, they should have sold 50 000 copies in one single night !).

What could be interesting for the readers of this blog, or at least funny, is the photo at the top of the article. It is amusing to see who-you-know on one of the most renown online-newspaper in France.

62. **Chris Oakley**
August 24, 2010

Re: the photo of the Bogdanov bros. with a former Harvard Assistant Professor – who is destroying who's credibility?

63. **martibal**
August 25, 2010

Chris: good question ! It is fair to say that the photo is from 2008, and the former Harvard Assistant Professor has probably not been asked by the online-newspaper the authorization to publish the photo.

64. **Thomas Larsson**
August 25, 2010

I spent a week in Paris on vacation this summer, and noticed that the Bogdanoff's new book was among the most exposed science books.

65. **martibal**
August 25, 2010

They are pretty popular in France (mostly because of their science/ science fiction show on TV 25 years ago) and their are still very present in the medias. Some people claim this is because they are quite close to one of the son of Sarkozy (at least they pretend to have worked with him on one of their TV program). It is just completely frightening to imagine they could be some scientific counselor of the president…

Gossip

August 17, 2010
Categories: Uncategorized

As the date for announcement of the 2010 Fields Medals approaches, gossip about who the winners might be has been circulating. Math Overflow is by far the best internet site for authoritative discussion between knowledgeable mathematicians, but, unlike this site, they have a "no gossip" rule, leading to the closing of discussion threads like this one.

You can bet on who the Fields Medalists might be here. I assume there's no bet possible for Ngo since he's a sure thing…

Update: It's Ngo and Villani, also Elon Lindenstrauss and Stanislav Smirnov. For the announcement and information about the work of the prize winners, see here. Accurate rumors about this don't seem to have started circulating until the ICM announced the winners to the press late Tuesday. This information was embargoed until today, breaking the embargo didn't seem sporting…

Update: Best blog by far for following this is that of Timothy Gowers, who was on the committee that picked the Fields medal winners, and promises to tell us about Cedric Villani's outfit.

Comments

1. **Nameless**
   August 17, 2010

   Isn't it conventional to invite future recipients of the Fields Medal as speakers?

   If that is the case, we can probably rule out Cedric Villani. In fact, the only people under 40 on the list of invited plenary speakers are Avila, Ngo, and possibly one or two ladies (Dinur and Plofker) whose ages I can't readily determine. Plofker does not appear to have done anything worth a Fields, and Dinur is a better match for Nevanlinna.

2. **Nameless Too**
   August 18, 2010

   @Nameless: no, the invitations are made long before the Medals are finalized. Extra plenary slots are held open for Medalists who were not already invited. Among the 2006 laureates, only Tao was scheduled to give a plenary; Okounkov and Werner were both scheduled originally for 45-minute talks, and were moved to other slots after the prizes were announced.

   I find it telling that those three names (Ngo plus the two at the betting site) have been circulating loudly for a few months without (to the best of my knowledge) any informed suggestion to the contrary. If someone were in the position of being anointed by incorrect rumors, there might be some attempt to dispel the resulting attention. There might yet be a fourth medal as well. Anyway, we'll know in less than 24 hours.

3. **Peter Woit**
   August 18, 2010

   Much higher quality rumors are now circulating, although embargoed by the ICM until 2am tonight New York time.

   In the righteous cause of stamping out ignorance and error, right now I'll just say that rumors about Artur Avila seem to have no substance.

4. **Oisin McGuinness**
   August 18, 2010

   Manjul Bhargava was a plenary speaker at ICM 2006, giving a nice account of his 'Higher Composition Laws' (the 4th paper in this series came out in the Annals of Mathematics in 2008), and was given the AMS Cole Prize in 2008 for this. There are 5 recent (since May) preprints on the ArXiv with very deep applications of his methods (e.g., 100% of quintic fields have S_5 as Galois closure); that on the boundedness of the average rank of elliptic curves (with Arul Shankar) is particularly amazing and wonderful. (They get a bound of 1.5 for the average algebraic rank; previous results on the average analytic rank pioneered by Armand Brumer 20 years back were a) higher, b) conditional on GRH etc.) So he deserves a Fields medal. However, as noted in the discussion here last year, he will still be eligible in 2014, and these papers probably came out too late for affecting the choices this year.

   But I'll still be disappointed if he's not on tomorrow's list!

5. **Anonymous**
   August 19, 2010

   If the list of speakers' names and attendees' names at the ICM web site are accurate, that would filter out some of the candidates. Manjul's name has not been there for several weeks, so are a bunch of others. It seems that Jacob Lurie's name just appeared recently, if my memory is correct.

6. **Tooth Fairy**
   August 19, 2010

   Winners:
   Elon Lindenstrauss
   Ngô Bảo Châu
   Stanislav Smirnov
   Cédric Villani

7. **Voltberg**
   August 19, 2010

   two out of four a year ago. Not bad !!!

8. **Sasha Gantil**
   August 20, 2010

Who was on the Fields Medal Committee?

9. **Michael Thaddeus**
August 23, 2010

Laszlo Lovasz (chairman), Corrado de Concini, Yakov Eliashberg, Peter Hall, Timothy Gowers, Ngaiming Mok, Stefan Müller, Peter Sarnak, Karen Uhlenbeck.

10. **Tooth Fairy**
August 24, 2010

I don't know where else to ask, so I will ask this here. Do you think that there is a change of the type of work that is getting the fields? My impression is that the medal was given for work that finds applications to physics. (are these really needed applications from the physics viewpoint ?) Furthermore, the connection is with statistical mechanics, not string theory. Are the powers behind the math scene signaling the new direction they think math will profit to explore? Or is it part of the previous scheme of things?

11. **Peter Woit**
August 24, 2010

Michael,

Thanks a lot for finding that and posting it!

Tooth Fairy,

I don't think you can conclude that much about trends in mathematics from these choices for the Fields. I believe that any Fields medal committee would have chosen Ngo, but that another committee could very easily have chosen different people for the other three. The choice of these three reflects who is on the committee and who they're getting advice from, as they try and choose 2 or 3 people amongst maybe a dozen or so equally distinguished young mathematicians. There's a sizable randomness factor here. As far as the connections to physics go, analysis has always drawn inspiration and problems from physics, going all the way back to the beginnings of the subject, there's nothing new here.

I suppose you could draw one conclusion, that the lack of a Fields medal for any of the more esoteric string theory or qft-related topics many people are working on reflects a judgement that there hasn't been anything really dramatic coming out of that area in the last few years.

12. **math_lambda**
August 24, 2010

@Tooth Fairy: as an outsider I'd say the spirit of the prize hasn't changed, it's just that what current mathematicians do is more varied than 40 or even 20 years ago. It's still that great math prize which rewards great proofs.

Werner's medal and now Smirnov's medal are really awards for the probability side of things, the insight on brownian motion geometry. Given the wide range of math used to model physical phenomena (and, as a result, the number of exciting mathematical problems coming from physics), it's bound to happen from time to time that some medalists have results of physical significance. Same with Villani: definitely it's subtle analysis first and foremost, with delicate estimates and innovative new norms.

So I don't think at all it's a signal or anything regarding applications to stat phys v strings, it's all about recent great mathematical results.

Geometric Langlands at the KITP

August 17, 2010
Categories: Langlands

There's a very interesting program going on at the KITP discussing recent work of mathematical interest on 4d supersymmetric gauge theories (N=2 and N=4). These include various connections of 4d gauge theory to geometric Langlands uncovered by Witten and collaborators a few years ago, as well as last year's conjecture by Alday-Gaiotto-Tachikawa of a relation between 4d gauge theory and 2d Liouville conformal field theory. In his introductory talk, Edward Frenkel discusses the possibility of a relationship between these ideas and the much earlier ideas about 2d conformal field theory that were inspirational at the beginnings of research on geometric Langlands (about which he has written extensively).

Yesterday and today Witten gave two talks on some new work. The first was about the very basic problem of how you quantize a finite dimensional symplectic manifold, which he approached using the phase-space path integral. The idea was similar to that described in a 2008 paper with Gukov, where the quantum mechanical problem gets turned into a 2d topological QFT problem. The innovation here is that he does this explicitly at the level of the path integral, using the kind of techniques for complexifying the problem, using holomorphicity and choosing appropriate path integral integration contours, that he pioneered in his recent paper on Analytic Continuation of Chern-Simons Theory. The second talk applied these ideas to the case of Chern-Simons theory. The path integral there is somewhat like the phase-space sort of path integral, and he expressed it in terms of a 4d QFT. He claims to be able to thus solve a well-known problem, that of how to get a QFT that gives Khovanov homology, which is a topological invariant with Euler characteristic the Jones polynomial. Unfortunately I get lost at the end when he has to go to 5 dimensions and perform some duality transformations. I gather he'll have a paper about this relatively soon, and I'll try again to see exactly how this works then.

Perhaps a collection should be taken up to buy a new camera for the KITP. The resolution of the one they have now been using for years is such that you often can't quite read what the speaker is writing on the blackboard. Still, it's wonderful to be able to follow along as they quickly put a lot of high-quality talks on-line.

Comments

1. **Kea**
   August 17, 2010

   Well, it's great to see Frenkel introducing the categorical viewpoint so early on! What a cool talk archive.

2. **Jim Martindale**
   August 18, 2010

   Did you see Prof. Witten's notes from his 8/16 "A New Look At The Path Integral Of Quantum Mechanics" lecture at
   http://online.kitp.ucsb.edu/online/duallang_m10/witten/pdf/Witten_LanglandsQFT_KITP.pdf
   ?
   Are they any help?

3. **Peter Woit**
   August 18, 2010

   Jim,

   The notes aren't Witten's, but taken by someone in the audience. They do help with the blackboard readability problem, but by the end of a long action-packed talk, one can see that the note-taker's ability to write everything down starts to flag…

4. **Kea**
   August 19, 2010

   Hmmm, given the way the camera keeps re-focusing, I am wondering if perhaps the problem is with the eyesight of the camera person!

   So towards the end of Witten's second lecture (about getting Khovanov homology from a 5d theory, where the extra dimension came from using D branes in a IIB string theory) he concludes that (S duality for) N=4 SYM is 'universal' in a nice sense, related to Langlands somehow. And then he discusses some '6d' M theory theories, from which one can get (by a 1d reduction on a circle) (i) the Khovanov theory or (ii) another case that reduces down to Chern-Simons …

   I confess that I skipped some of the talk here. Does this mean that to understand the reduction 'Khovanov to CSFT' we need to talk about the M theory picture?

5. **oarobin**
   August 25, 2010

   peter,
   have you seen and do you have any comments on the video "String Theory for the Scientifically Curious with Dr. Amanda Peet" available at
   http://www.youtube.com/watch?v=PpQngpaHamg

   especially the Q & A section at the end.

6. **Peter Woit**
   August 25, 2010

   Thanks oarobin,

   Hadn't seen that. Quite something, I think it deserves a new edition of "This Week's Hype"…

This Week’s Hype

August 25, 2010
Categories: This Week's Hype

I’m rather busy these days with a move to a new apartment, but maybe there’s time for a quick edition of “This Week’s Hype”.

A commenter on the previous posting points to Amanda Peet’s recent talk entitled String Theory for the Scientifically Curious. In the question and answer section, she responds to someone who asks her to comment on Phil Anderson’s claim that string theory makes no falsifiable predictions. She describes this claim as “absolutely fundamentally completely utterly wrong” and says that Anderson should “be smacked around the head” for saying it. She then goes on to a vigorous and extensive personal attack on Lee Smolin.

Her argument that string theory really is falsifiable is that a paper by Distler and collaborators shows this and has been published. The paper she is referring to is this one, which started off as a preprint with the title Falsifying String Theory through WW scattering, but was only published after a forced change of title to “Falsifying Models of New Physics Via WW Scattering”. One reason for this is that there’s actually nothing about string theory in the paper. Evidently Peet just saw the preprint, not the published version. If you want to know more about this particular piece of hype, see blog postings here, here, and here.

For more Amanda Peet in action, there’s a classic video from the KITP, blogged about here.

Comments

1. **chris**
   August 26, 2010

   that’s incredible.

   what an utter lack of respect and discussion culture. somtimes i wonder what the scientific community has come to.

2. **Felipe Zaldivar**
   August 26, 2010

   I was appalled for the level of aggression in A. Peet answers to people asking some questions after the talk. Is this really necessary? Has this field of Physics sunk to such level or is she just a “singularity” in the realm? I did not watch the whole video, but if someone is interested, see from 1:19 to 1:25 min in the video.

3. **H M**
   August 26, 2010

   She claims that Phillip Anderson has an agenda, there are not enough smart people working on superconductors and therefore he wants to attract potential string theorists (aka the smartest people in the world) towards Condensed Matter physics. Such arrogance seems very prevalent among string theorists.

4. **Fabio**
   August 26, 2010

   I’ve never known Peet to voice an opinion that didn’t come directly from one of her mentors. In this case she seems to be aping some of Lenny’s earlier personal attacks against Smolin. Of course, if you’re an insecure lesser figure in a field, loudly and publicly attacking apostates is an easy way to earn some plaudits from your peers.

5. **srp**
   August 26, 2010

   (I apologize if this double posts–there was a glitch the first time.)

   I was very confused by this post; a Google search quickly confirmed that the Amanda Peet I’ve heard of is someone you’d be very surprised to find had an opinion on string theory…

   It might be a good idea to say “Amanda Peet (not the famous actress)” just as a post about an actor named Ed Witten would be well-advised to tell the reader it was not referring to the more-famous one.

6. **Edward**
   August 27, 2010

   She’s hardly a lesser light, Fabio. That she has the same viewpoint as her supervisor and peers is, of course, a common phenomenon and not necessarily indicative of mindless parroting. As for her level of aggression, unfortunately this (and thick skin) is what is often required to succeed in physics, especially in the rarefied atmosphere of high-energy. Anderson is no less tenacious/vicious.

7. **Peter Woit**
   August 27, 2010

   Edward,

   I’ve spent a lot of time in the “rarefied” atmosphere of academia, both in high energy physics and mathematics, and I’ve never seen this kind of public behavior (except from Lubos Motl, who didn’t last long, and a bit of it from Lenny Susskind).

   There’s a reason that serious academics don’t respond (in public) to substantive arguments about science by engaging in personal attacks on those they disagree with and saying that they need to be “smacked”. Doing this makes it clear to everyone in the audience that you probably don’t have a substantive response to the argument. Doing it and adding as your only substantive response that your claim is true because it was published in a paper in Phys. Rev, when a referee explicitly wouldn’t allow that claim to appear there makes it crystal-clear what is going on.

8. **hep-ph**
   August 27, 2010

   Anderson testified to the US Congress in 1987 against the construction of the Superconducting Super Collider and does deserve a good smack in the face, regardless of his views on string theory.

9. **onymous**
August 28, 2010

I wish people would stop talking about smacking Phil Anderson. Have you *seen* the man lately? He doesn't look like he would hold up to much physical violence. But he's pretty intellectually combative, so maybe fighting with words would be more productive….

10. **anon**
August 28, 2010

Hello Dr. Woit,

Why does it matter whether or not the string theory makes falsifiable predictions?

Suppose the latest version of general relativity fits all experimental evidence, and none of its predictions are falsified. Suppose the same were true of the latest version of the standard model of particle physics.

On another blog, reference frame by Lubos Motl, I read the statement that string theory is the only mathematically consistent way to unify those theories. Suppose a correct, extensive mathematical proof of that statement is given, covering every last mathematical technicality. Perhaps it will appear soon on that blog, or maybe I missed the reference to it.

Then, wouldn't string theory be correct and a great achievement even without having any falsifiable predictions that are specific to string theory, and not to the pre existing models it unified?

If stars and particles must be made of the same matter, and if the Lubos Motl assertion is proven, then wouldn't it be logically mandatory that string theory is accepted even if it does not introduce any new falsifiable predictions?

This is probably a dumb question and I have only a basic, introductory level, familiarity with physics at this time. It's just that "no falsifiable predictions" is one of the main complaints here, but the people who promote this research don't advertise new predictions, they advertise the prospect of less contradictory models for existing predictions that already fit empirical evidence.

11. **Anon2**
August 28, 2010

anon, a lack of falsifiability is actually a BONUS to theoretical physicists, allowing them to sleep soundly at nights, safe in the knowledge that their theory CANNOT be falsified.

Woit just doesn't grasp the fact that BEING FALSIFIED is not good for business if one is a theoretical physicist wanting fashionable non-falsified research on one's CV.

12. **Giotis**
August 28, 2010

Well she may be a little harsh but she has her own unique authentic style. I like this, we don't have to do PR all the time. Our world is too hypocritical as it is and we need authentic people like Amanda Peet to speak their mind.

13. **Peter Woit**
August 28, 2010

anon,

There's nothing wrong with unfalsifiable theories, they just tend to not be scientific theories, but something else. And no, string theory at this point does not provide a mathematically consistent unification anyway.

The weird thing about the Amanda Peet attack on Anderson over the falsifiability issue is that most string theorists would acknowledge that, in its present state, string theory doesn't make falsifiable predictions. The controversy is not over this, but over the question of whether there's reason for optimism that string theory unification may someday make predictions. I see no good reason to expect this, string theorists typically are more optimistic.

14. **Peter Woit**
August 28, 2010

Giotis,

I have no problem with Amanda Peet speaking her mind if she wants to. The problem is with what's in her mind….

15. **anon**
August 28, 2010

I was under the impression that string theorists wanted a logically consistent way of getting the same predictions one already has from GR and particle physics. I thought they were concerned about GR and QM contradicting each other, not with either one of them coming up short when it comes to fitting experimental data. If GR and QM already count as scientific and falsifiable if wrong, then a model that gives the same answers as them should also.

Ad hominem responses and impugning motives are not what one is supposed to do. However people still do them. It might be a case where the audience gets only what the lecturer thinks the audience has enough background to understand.

16. **Giotis**
August 29, 2010

"I was under the impression that string theorists wanted a logically consistent way of getting the same predictions one already has from GR and particle physics."

Anonymous you are right of course but the issue here is that String theory has so many components you can manipulate that practically you can come up with any physics you want in 4 dimensions. Moreover the attempts to derive the 4D physics we know seems too contrived from string theory perspective although I would say this is a subordinate issue. In any case if there was a natural *unique* solution of String theory that would produce the 4D physics we know then of course nobody could seriously criticize String theory regardless of any new falsifiable predictions. It would be truly a triumph that no one could deny since the real purpose of a unified theory is to explain our world in the deepest fundamental level. But unfortunately this is not the case at least so far.

Note though that for many people this plethora of solutions is a blessing more than a curse. It gives for example a natural explanation to certain puzzles of nature like the small value of the cosmological constant and the presumed fine tuning of our Universe in general.

17. **chris**
August 31, 2010

reading some of the comments here i really wonder if there is not even a consensus within the physics community any more about threats of physical violence being inacceptable.

i mean seriously: we all know the game of shaping the future of research by the economic stranglehold on postdocs. so is this not enough? now physical violence comes into the game again? i am totally appalled.

"Anderson testified to the US Congress in 1987 against the construction of the Superconducting Super Collider and does deserve a good smack in the face, regardless of his views on string theory."
apart from the fact that this could have saved billions of $ from being pumped into a doomed project: what is it that you want to express with this statement? that scientists should not speak their honest opinion lest they get beaten up by the opposing mafia-gang of their peers? or that a democratic decision on science project funding is a nuisance that should be done away with?

Everything is Emergent

August 26, 2010
Categories: Uncategorized

During the past year Erik Verlinde has made a splash (most recently in the New York Times). with his claim that the reason we don't understand gravity is that it is an emergent phenomenon, an "entropic force". Now he and Peter Freund are taking this farther, with a claim that the Standard Model is also emergent. Freund has a new paper out on the arXiv entitled "Emergent Gauge Fields" with an abstract:

> Erik Verlinde's proposal of the emergence of the gravitational force as an entropic force is extended to abelian and non-abelian gauge fields and to matter fields. This suggests a picture with no fundamental forces or forms of matter whatsoever.

Freund thanks Verlinde, who evidently has much the same idea:

> I wish to thank Erik Verlinde for very helpful correspondence from which it is clear that he independently has also arrived at the conclusion that not only gravity, but all gauge fields should be emergent.

He remarks that this new theoretical idea is remniscent of Geoffrey Chew's failed "bootstrap program" of the sixties:

> It is as if assuming certain forces and forms of matter to be fundamental is tantamount (in the sense of an effective theory) to assuming that there are no fundamental forces or forms of matter whatsoever, and everything is emergent. This latter picture in which nothing is fundamental is reminiscent of Chew's bootstrap approach , the original breeding ground of string theory. Could it be that after all its mathematically and physically exquisite developments, string theory has returned to its birthplace?

It's very unclear to me why this is supposed to be a good thing. In his Nobel prize lecture, David Gross, a student of Chew's explains:

> I can remember the precise moment at which I was disillusioned with the bootstrap program. This was at the 1966 Rochester meeting, held at Berkeley. Francis Low, in the session following his talk, remarked that the bootstrap was less of a theory than a tautology…

Comments

1. **Peter Shor**
   August 26, 2010

   How can everything be emergent? To the best of my understanding, emergent laws of nature are derived by considering the statistical mechanics of the fundamental laws of nature. If there aren't any fundamental laws, you can't have any other laws emerge from them.

2. **Paul Murray**
   August 26, 2010

   So, they seem to be saying that the fundamental forces can be reduced to some other kind of thing from which they emerge. Ok … so what's new? Isn't this exactly what string theory has been hypothesising all along?

3. **Morton**
   August 26, 2010

   The preprint is just words. Compared to that, even Christoph Schiller's approach is better. He claims to deduce quantum theory and the three gauge groups from emergence, in four dimensions…

4. **Marcus**
   August 26, 2010

   Mockery by reference to that strange sometimes-very-funny Jonathan Foer book "Everything is Illuminated".

5. **chris**
   August 26, 2010

   full circle back to bootstrap theories?

   oh my, this one is completing a much larger full circle: full circle back to Aristoteles, where physical theories are suggested in vage analogies without the inhibiting rigour of mathematics.

6. **slw**
   August 26, 2010

   Okay, so there is a deeper underlying reality for which our current understanding of the universe is simply an higher-level abstraction. That's good to know, I guess. Are there any hints in the paper what the deeper, simpler reality could be? Or is this simply one of those mathematician anecdotes with the punchline "a solution exists!".

7. **Frank Quednau**
   August 26, 2010

   Looks like philosophy isn't far away now. I do have to chuckle a bit. Seems to me the whole physics body has no clue right now as how to move on in understanding that silly thing called reality.

   If you're trying to find a more fundamental explanation to features of reality, isn't it mandatory to declare those features you deemed fundamental as "emergent"?

8. **Sam**
   August 26, 2010

   Verlinde's proposal is somehow quite compelling. To say that the gravitational equations are emergent is not so different than saying that the Navier-Stokes equations are emergent. What one means is that Navier-Stokes are some equations that are derived from the macroscopic behavior of enembles of things described microscopically in quite a different way, subject to constraints like conservation of energy. The gravitational equations are not so different … at least this is what Verlinde is arguing.

The philosophizing ones are those who speak of ‘fundamental forces’ while not expressingly clearly or precisely what that means. Whatever that means, the equations describing the motion of a fluid certainly do not describe fundamental forces/laws, and the suggestion at the bottom is that we need to think of and derive the gravitational equations from a similar point of view. What’s so terribly unreasonable about that?

9. **DB**
August 26, 2010

Good news for Fritjof Capra and his Tao of Physics baloney.

10. **cyd**
August 26, 2010

Er, the Standard Model IS emergent. That’s what it means to be a renormalizable quantum field theory, and that’s the basic lesson of the renormalization group.

11. **SteveB**
August 26, 2010

I am a bit fuzzy on what the term “emergent” means in a mathematical or physics context. There are some good clues in the previous responses but is there a precise definition?

12. **cyd**
August 26, 2010

> I am a bit fuzzy on what the term “emergent” means in a mathematical or physics context.

The standard meaning of “emergence” in the physics context comes from Phil Anderson’s 1974 article “More is Different”. In the context of the Standard Model, “emergence” refers to the statement that the Standard Model is an effective field theory. This is not a free-floating claim; it arises from the renormalization group’s insights about what renormalization really means.

13. **Ricardo**
August 26, 2010

After reading the paper Plato’s theory of forms came to my mind, it very much reminded me of the long, involved arguments you had to read in high school philosophy class to determine whether they were valid or invalid.
Leaving the physics apart, I can’t even begin to see if the paper is logically correct, but the thing that really bothers me about these emergent “models” is that I fail to see what is the gain, what do they have to offer besides what we already have from supersymmetry, strings, etc,?

14. **H M**
August 26, 2010

The idea that gravity and gauge fields could be low-energy emergent degrees of freedom is FAR from new and there has already been done lots of work on this. See for example the work of Xiao-Gang Wen (http://dao.mit.edu/~wen/) or the book “The Universe in a Helium Droplet” by Grigory E. Volovik (where he shows that many features of the standard model can emerge as effective low-energy model of superfluid 3He, it is of course just a toy model).

In Condensed Matter physics emergent gauge fields and relativistic symmetries are nothing new, and has been measured in many different systems. The mechanism Verlinde proposes might of course be new (although its very hand-waving right now), but the idea of emergent standard model is not his.

@Paul Murray: No, emergence is not what string theory is about. You are confusing the fact that string theory (or another microscopic theory) should reduce to SM at long distances, with the concept of emergence (which is very different and very subtle).

15. **H M**
August 26, 2010

@Ricardo
Quote: “the thing that really bothers me about these emergent “models” is that I fail to see what is the gain, what do they have to offer besides what we already have from supersymmetry, strings, etc,?”

You have to understand the difference between emergence and reductionism. The prevalent idea in high energy physics is that everything (up to a limit) can be split apart and reduced into more and more fundamental particles (or strings). For example the proton is build out of three quarks, which again might be build out of more fundamental things.

The idea of emergence is VERY different. If a particle is not fundamental it is not necessarily build out of smaller particles, it can be a COLLECTIVE degree of freedom. There are many examples of collective phenomena in physics (specially pioneered in Condensed Matter Physics), and these “particles” cannot be described in a reductionist way. Quantum solid and liquids, are microscopically non-relativistic and build out of many-particles interacting with EM. But they can have low energy degrees of freedom which are much more symmetric, ie have Lorentz invariance and emergent non-Abelian gauge bosons.

Thus the answer to your question is that; what emergent models can offer is to describe emergent phenomena! IF gauge symmetries and gravity are not fundamental, but emergent collective degrees of freedom, they cannot be described by, say, strings. Wither the content of the standard model is fundamental or emergent is not known (though most people tend to prefer non-emergence).

16. **noel**
August 26, 2010

So we had emergent gravity, we knew about Kaluza-Klein compactification of gravity….connect the dots r serious business rite? Why exactly did he have to communicate with Verlinde about this trivial observation?

17. **Ricardo**
August 26, 2010

@H M
Ok, thanks for the clarification, but my point is this: as I understand Verlinde’s proposal is based on the holographic principle, which is based on strings, so if you use that to show that the emergent degrees of freedom are not strings, isn’t that self-contradictory or circular reasoning at best?

18. **H M**
August 26, 2010

@Ricardo

Oh I'm sorry, I misunderstood your question.

Well I don't think that the holographic principle is necessarily based on string theory, the original proposal by 't Hooft didn't as far I can know (http://arxiv.org/abs/gr-qc/9310026). But it is apparently, mostly implemented in stringy environment (I must say that I know very little to nothing about this).

With that said, it seems like Verlinde want to, as you say, put in string theory into his mechanism. This also sounds odd to me, but I need to study his papers more carefully before commenting on this.

19. **Roberto Percacci**
August 26, 2010

Quoting H M:

> what emergent models can offer is to describe emergent phenomena!

Yes, that would be useful, but much of the literature does not address the issue of constructing workable emergent models for gravity. It seems to me that those that go furthest in this direction are people that do not usually talk much about emergence. For instance, you could argue that causal dynamical triangulations is currently the most successful way of actually calculating emergent properties of gravity.

20. **Shantanu**
August 26, 2010

FWIW, Erik Verlinde's talk on his paper is online at
http://online.itp.ucsb.edu/online/joint98/verlinde2/

21. **Haelfix**
August 27, 2010

The word 'emergent' is one of those horribly loaded words in physics, that means about ten different things depending on the context.

For instance, the way I interpret the word, would be that the author of that paper claims that the gauge fields of the standard model and gravity (eg the W, Z, gluons, graviton etc) were in fact not fundamental and instead collective degrees of freedom.

The classical reply to that, is to point out the Weinberg-Witten theorem precludes exactly this scenario.

Having said that, I don't think that's what Peter Freund is saying at all, and I have no idea what to make of his paper. I think perhaps I will now associate the word 'emergent' with 'opaque, excessively verbose papers' and be satisfied to leave it at that.

22. **onymous**
August 27, 2010

It's not clear that Weinberg-Witten applies here; certainly it doesn't apply any more than it would to the original emergent gravity idea. We know that both gravity and gauge fields in AdS can emerge from the boundary theory, which has a stress tensor and conserved currents but in one lower dimension, so that the assumptions of Weinberg-Witten are evaded. (The gauge fields can either live on branes, or arise in the Kaluza-Klein manner exactly as Freund suggests.) What isn't clear is how any of this concretely works in any setting other than AdS. Verlinde proposes that gravity in flat space emerges from lower-dimensional physics (I think), in a mysterious unspecified way. If this is true, it would seem almost mundane to have gauge fields also emerge in this way.

23. **Kea**
August 27, 2010

In the UCSB talk, Verlinde takes some time to argue that gravity and gauge theories should emerge from a new kind of matrix theory that reflects the entropic screen information. This talk is not as good as the Perimeter one (although it has slightly more substance) because he tries too hard to cater to the stringers in the audience.

24. **Chris Austin**
August 27, 2010

In agreement with onymous, it seems to me that the Weinberg-Witten theorem, "Limits On Massless Particles," Phys. Lett. B 96 (1980) 59-62, does not exclude the possibility that the graviton could be a collective degree of freedom in a combinatorial system such as causal dynamical triangulations.

The assertion in H M's first two comments above, that non-relativistic condensed matter systems in the laboratory can have low energy collective degrees of freedom that behave like non-Abelian gauge bosons with Lorentz-invariant dynamics, and that such behaviour has been measured in many different systems, is completely new to me. It would be very interesting to see some references on this.

25. **Haelfix**
August 27, 2010

Mmm, terminology and loaded vocabulary! A system where dualities are present is a different type of 'emergence'. You wouldn't really say that the gravitons in ads/cft were composite would you? But you could say that the bulk was "emergent" from dynamics that lived on the boundary.

Likewise its kind of odd to call a lattice approach 'emergent'. For instance I consider lattice gauge theory the very definition of what qft means. But then its not particularly standard to say that lorentz invariance is 'emergent', even though thats explicitly what happens when you take the continuum limit of a typical qft.

In any event, playing word games doesn't increase one's knowledge of the physics, instead they're supposed to be defined by the context and the associated mathematics… Unfortunately it seems such a specific realization is considerably more difficult to produce, than currently in vogue publishing standards….

26. **Roberto Percacci**
August 27, 2010

> its kind of odd to call a lattice approach 'emergent'.

maybe so, but it's not at all obvious that by throwing together a lot of little triangles and stirring you get something that looks on average like a smooth manifold. That's why I think it is correct to use the word here.

> for instance I consider lattice gauge theory the very definition of what qft means.

As "cyd" pointed out above, the form of the lagrangian of the standard model could be said to be an emergent property: whatever nonrenormalizable stuff you started with at high energy would be enormously suppressed by the time you arrive at low energy. This is exactly the behavior that occurs in statistical systems with a second order phase transition, which is undeniably an example of emergence. And by the way, something similar could happen in the case of gravity, if it had a suitable fixed point…..

Concerning the Weinberg-Witten theorem, it does not forbid "induced gravity", which is another concrete example of "emergent gravity" (you integrate out matter fields in a background metric, and in so doing you generate an action for the metric). WW explicitly point this out in a footnote.

27. **Jim**
    August 27, 2010

    Dear Dr. Shor,

    Wheeler was the first to state Freund & Verlinde' idea clearly:

    "The only Law, is the Law that there IS no Law".

28. **Marcus**
    August 27, 2010

    I liked Percacci's two comments here, so I started a discussion thread at Physicsforums to follow up with perhaps a bit more detail:
    http://physicsforums.com/showthread.php?t=424552

29. **Norea Armozel**
    August 28, 2010

    What I find annoying by the use of the term emergence is that it dilutes its original meaning for both natural and social sciences. In both sciences, emergence means there are some laws which are fundamental, but that the resultant 'sub'-laws come about as an interplay/relationship between the given fundamental laws. This is especially true when considering sociological studies of extralegal institutions and some such (I'm not sure what would be the analog in physics or chemistry… Friction perhaps?).

    It's like emergence is the new synergy or whatever. X_X

30. **Einblick**
    August 28, 2010

    The claim that the gauge field is emergent is not new, I think. X.G.Wen has argued that the gauge field dof can be emergent from the string-net liquid. And I think that Verlinde's entropy gravity is only classical gravity, until we get to know what are the fundamental degrees of freedom and how they behave, since we don't have the notion of quantum thermodynamics, but rather quantum statistics.

31. **martibal**
    August 28, 2010

    Can anybody explain (in a nutshell) what bootstrap theory was ?

32. **John McVirgo**
    August 28, 2010

    These two great physicists, although they did brilliant work in their youth, are rather past it when generating creative ideas that have some resemblence to reality.

    Erik Verlinde is 48

    Peter Freund is… 73

    Let's wait and see what the young post docs have to say.

33. **Wyman**
    August 28, 2010

    That is an awful bit of ageism right there. Do we really want to ignore the contributions of anyone over the age of 40?

34. **Chris Austin**
    August 28, 2010

    With reference to attempts to view an Abelian gauge field as emergent from something else, perhaps it is fair to note that in 1861 James Clerk Maxwell developed a theory of magnetic lines of force as rotating vortices subject to Newtonian dynamics, with tiny particles like ball bearings separating neighbouring vortices to eliminate friction, and that this mechanical analogy apparently helped him to develop his field equations published four years later. See
    http://upload.wikimedia.org/wikipedia/commons/b/b8/On_Physical_Lines_of_Force.pdf

35. **Chris Austin**
    August 29, 2010

    With reference to the one sentence in which Freund mentions supersymmetry, perhaps a pointer to the way forward might be the papers of Cioroianu, Diaconu, and Sararu, e.g. [0903.0259](0903.0259), in which they use the Batalin-Vilkovisky apparatus to show that any theory in 11 dimensions that nontrivially couples a massless symmetric tensor, Rarita-Schwinger field, and 3-form gauge field, without altering their degree of freedom counts, must automatically be supersymmetric and coincide with the CJS theory after field redefinitions, and thus also have vanishing cosmological constant in 11 dimensions, by Bautier, Deser, Henneaux, and Seminara.

    In other words, if you could adapt the Neuberger Dirac operator to a Majorana spinor living on the links of causal dynamical triangulations in 11 dimensions, and also add a 3-form gauge field, perhaps simply as a number living on the 3-simplexes, and ensure that the system is nontrivially coupled in the infra-red, then the Cremmer-Julia-Scherk theory should automatically emerge at long distances.

If combining Neuberger and CDT turned out to require a combinatorial tangent space or vielbein, perhaps the appearance of combinatorial structures called oriented matroids in the Gelfand-MacPherson construction of combinatorial Pontrjagin classes, math/9204231, and MacPherson's subsequent theory of combinatorial differential manifolds, might provide a clue, the connection being that Pontrjagin classes are involved in the subtleties of discretized spinor fields via the Atiyah-Singer index theorem. Various types of connection between oriented matroids and M-theory have already been considered by J. A. Nieto, e.g. hep-th/0603139.

36. **Jack Lothian**
August 29, 2010

I am not sure that I am understanding "emergence". I find the discussion in this thread opaque. The group & topology theory discussion goes over my head but statistical mechanics is something that I am familiar with. So, is emergence like the stable vortexes observed in turbulent flow – it arise out of statistical averaging of complex multi-body interactions? If so, like turbulence, does this imply that our universe is in a quasi-equilibrium state? Are phase transitions possible?

I am not sure how any reporter can comment on this stuff. I have an ancient masters degree in Statistical Mechanics plus I worked my whole life in science related topics yet this discussion comes across as voodoo science to me. So what is emergence – in layman's terms? Can someone tell me?

37. **Chris Austin**
August 29, 2010

Jack,

To a condensed matter physicist, "emergent" refers to properties of large collections of atoms that have no analogue for one or a few atoms.

To a high energy physicist, "emergent" refers to the large distance or low energy properties of a model system whose microscopic definition has a simple mathematical structure. For example the microscopic degrees of freedom of lattice gauge theory, which is used in computer simulations of quantum chromodynamics, consist of a 3 x 3 unitary matrix on each link of a hypercubic lattice, together with a spinor for each type of quark on each vertex of the lattice.

As Roberto Percacci pointed out above, currently the most successful emergent model of gravity is the causal dynamical triangulations of Ambjorn, Jurkiewicz, and Loll. This is a completely combinatorial system, that consists simply of a list of a finite number of vertices, together with a list of selected quintuples of these vertices, each of which represents the four-dimensional analogue of a tetrahedron. If two selected quintuples have four vertices in common, they are the four-dimensional analogue of two tetrahedra that share a common triangular face. The list of selected quintuples is required to be such that for each quintuple in the list, and each set of four of the five vertices of that quintuple, there is exactly one other quintuple in the list that also has those four vertices. The list of selected quintuples then represents a triangulation of a four-dimensional curved space-time.

To see how the properties of a four-dimensional curved space-time can be coded into such a list of selected quintuples, consider the example of a two-dimensional curved surface. Randomly select a finite number of points sprinkled on the surface: these are your vertices. Associate to each vertex its Voronoi cell, which is the set of all the points of the surface that are closer to that vertex than to any other vertex. Then define a triple of vertices to be selected if and only if their Voronoi cells meet at a corner.

The list of the triples of vertices selected in this way gives a triangulation of the curved surface, and if you sprinkled your vertices finely enough, you can approximately study the curved surface simply by counting vertices, edges, and triangles. For example, the distance between two vertices is proportional to the number of edges in the shortest path of edges between those two vertices. And if you count the number of vertices up to some distance from a fixed vertex, and the number is smaller than you would find for a flat surface, then the surface is positively curved at that vertex, and conversely it is negatively curved if the number is larger.

Ambjorn, Jurkiewicz, and Loll found Monte-Carlo rules for randomly modifying the list of selected quintuples in their simulations such that on average, the lists of selected quintuples they generate look at large distances something like triangulations of smoothly curved four-dimensional "spacetimes". Thus these four-dimensional curved "spacetimes", governed at large distances by something like Einstein's classical action for the gravitational field, emerge from simple combinatorial rules for combining the four-dimensional analogue of tetrahedra in a semi-random manner.

In their latest article with Goerlich, they appear to hint at a connection with the "entropic gravity" ideas of Verlinde which with the subsequent suggestion by Freund are the topic of this posting, although they don't actually cite Verlinde's paper.

38. **Jack Lothian**
August 29, 2010

That response was a bit higher level than I hoped but I think I get the reference to Statistical Mechanics: you are averaging constrained micro/local topology paths in some sense to infer macro characteristics for the surface? Also, this makes me think of things like the Ising model but not sure if that is a valid comparison.

I feel somewhat uneasy about this discour though, it seems to me infering macro characteristics from the bottom up is really sensitive to initial conditions, boundary conditions, # of dimensions, grid size, etc but this is several notches above what I usually deal with so I do not feel qualified to really debate this.

I still wonder how any reporter could understand this stuff – I suspect they would only react to the word "emergent" & they would interpret the term as it is used in common language.

39. **Jack Lothian**
August 29, 2010

Oh, Chris,

I forgot to say: thanks for taking the time to respond – I do appreciate the effort even if it challenged me a little more than I anticipated. Also, I think that I sort of get the gist of the argument.

Thanks

40. **younghun park**
August 29, 2010

Peter
As jack Lothian points out above, the meaing of the word "Emergent' is seemed to be ambiguous.
What do you think this word mean in physics?

41. **martibal**
August 30, 2010

Chris:

I understand that, in some limit, the triangulated space-time looks like a smooth manifold. If I understand well, the list of selected quintuple gives you the curvature, right ?

But where does Einstein equation comes from ? Do you interpret the "randomness" as a stress energy tensor (i.e. one side of Einstein equation) that allows you to compute the curvature (i.e., roughly speaking, the other side of Einstein equations) ? If so, does this mean that various "randomness" (I am not very familiar with random objects but I believe the randomness is given by a probability law ?) correspond, in the classical GR picture, to various matter distributions ?

42. **Marcus**
August 30, 2010

Martibal: "I understand that, in some limit, the triangulated space-time looks like a smooth manifold."

No. Why should it? The CDT approach (which Chris was discussing) uses triangulation and lets the size of the triangles go to zero.
But it does not result in a smooth manifold model of spacetime.

There is no logical or mathematical reason that it should, and it does not.

The CDT authors (Loll Ambjorn et al) describe the spacetime model they get as "fractal-like" at small scale. The dimensionality, which is 4D at large, goes down continuously to around 1.9 or 2.1 at small scale. A similar spontaneous reduction of dimensionality occurs in spatial slices. Since they generate universes by computer simulation, they are able to study what happens by looking at concrete realizations. An approximately smooth deSitter spacetime emerges at large scale as a kind of path-integral "average" of many non-smooth cases.

43. **martibal**
August 30, 2010

Marcus: sorry I am not so familiar with the subject so my formulation may be not correct.

Cris Austin said: "Ambjorn, Jurkiewicz, and Loll found Monte-Carlo rules for randomly modifying the list of selected quintuples in their simulations such that on average, the lists of selected quintuples they generate look at large distances something like triangulations of smoothly curved four-dimensional "spacetimes"."
This is what I meant by saying that at in some limit (I should better say: at large) the triangulated manifold (my formulation was misleading. I should say: the fractal spacetime obtained as the limit of a triangulated spacetime with triangles of size zero) looks like a smooth manifold.

So let me try again :
I understand that at large scale, the fractal spacetimes look like a smooth manifold (e.g. de Sitter). In other terms, the smooth structure emerges from an appropriate average on fractals. And so does the curvature.

What I do not understand is: where does Einstein equation comes from ? I mean Einstein equation determines the metric, but not the topology. So if you average in a different way on the same set of fractal spaces, can you obtain – at large- various metrics on the same topological manifold ?

44. **Marcus**
August 31, 2010

Martibal,
Here is a seminal paper by Tullio Regge (1961) "General Relativity without Coordinates":
http://www.signalscience.net/files/Regge.pdf

Here are Loll's papers
http://arxiv.org/find/grp_physics/1/au:+Loll/0/1/0/all/0/1

Loll and friends don't use all identical building blocks, but they almost do. They use two types. Morally it's like all identical—in the 2D case imagine identical equilateral triangles.

You can tell the curvature at a point by counting how many triangles meet there. 6 ==>flat. 5==> pos curv. 7==>neg curv.

Einst Hilb action is essentially an integral of curvature. Regge inspires us to evaluate it by counting identical triangles and the points where they meet. If there is just the right proportion of triangles to points then on average it is flat (every point is surrounded on average by 6)

The upshot is that we can evaluate the E-H action, or the Regge version of it, just by counting. That extends to higher, like 4D.
Loll team *could* do it with all identical equilateral 4-simplices. But for technical reasons they use two classes of identical non-equilateral 4-simplices.

It only makes the counting slightly more complicated. Main thing is that the E-H or Regge action is evaluated simply by counting different types of simplex—a linear combination of census-data. Comparing how many big ones versus how many little ones where the big ones meet. (I'm oversimplifying but it's sort of like that).

Then they introduce different shuffling moves where you have this huge assemblage of simplices ("triangles") and at each point the computer looks to see if it wants to perform a re-arrangement, and *tosses a coin*. The decision to make the move or not is random.
Different probabilities for different moves. Re-arrangement of simplices can change the census data, change the average number meeting around whatever they meet around. Change the overall action.

So the probabilities of the various moves (some of which insert or remove simplices) are adjusted so it is a path integral Monte Carlo scheme.

Individual spacetimes result from millions and millions of shuffles. The individual spacetimes are not deSitter. Only the average of many individual spacetimes is deSitter.
An individual spacetime (after say a million shuffles at every location) can be studied. It will have "fractal-like" features at small scale. They are not strictly speaking fractals, people call them "fractal-like" at small scale. The dimensionality goes down at small scale.
Think of the radius to volume relation of a wad of crumpled paper.
At large scale the mass of paper within a certain radius goes up as the cube. But at small scale the mass of paper goes up as the square of the radius. The exponent of the radius-volume relation is one measure of dimension. But it is not like crumpled paper it is only remotely analogous.

Here is the paper I found most helpful
http://arxiv.org/abs/hep-th/0105267

For lots of intuitive analogies and color illustrations there is a Sci Am by Loll and friends
http://www.signallake.com/innovation/SelfOrganizingQuantumJul08.pdf

Here is a recent review or status report:

http://arxiv.org/abs/1007.2560

Reading Loll papers is better than reading what I say. They are very clear writers. But maybe this (imperfect and dashed off) will get you started.

45. **chris**
August 31, 2010

HM,

I am strongly disagreeing with your proposed meaning of emergence as applied to high energy physics. you say that the predominant picture in HEP is that of reductionism: if this is so, the what about e.g. QCD? how can reductionism be the guiding principle, when quarks are unobservable? is the effective theory of protons and pions not also "emergent"? you could argue that these degrees of freedom are not 'colective', but just look at the high temperature phases: emergent phenomena wherever you look. and this is hardcore, partially experimentally verified high energy physics.

and the specific criticism that string theory is in contradiction with emergence: the current main line of string research – as far as i know – is duality stuff. the boundary QFT of a bulk string theory – this satisfies every single criterion you set up for calling a phenomenon "emergent".

in the end, without any concrete theory behind it it is just more words.

46. **martibal**
August 31, 2010

Marcus: thanks for the detailed answer !

47. **Yasha**
September 1, 2010

I am a mathematician, with only a bit of training in general relativity, but I wanted to make a few comments/questions. I believe one of the first people to suggest gravity is an emergent phenomenon was Sakharov although he called it induced gravity http://en.wikipedia.org/wiki/Induced_gravity. How does this relate to ideas of Verlinde? Why is this not mentioned or discussed?

Of course the description of induced gravity in wikipedia is unintelligible: We are starting with a pseudo-riemannian 4 manifold or 3 manifold? I am guessing 3 manifold, since in a 4 manifold, the gravitational field is already implicit and there is nothing to induce. (I suppose that's why they say Riemannian.) I am also guessing for Sakharov the universe needs to be globally hyperbolic.

48. **Coin**
September 2, 2010

Looking at the Freund paper– the way he generalizes entropic-gravity to entropic-everything is just using the old Kaluza-Klein dimensions-as-forces thing, right? But people don't actually *use* the Kaluza-Klein dimensions in normal mainstream physics, right? Weren't there problems with the theory, like stabilization? Wouldn't an attempt to explain forces as Kaluza-Klein dimensions for entropic gravity run into the same problems that explaining forces as Kaluza-Klein dimensions for *ordinary* gravity did? Am I missing something?

49. **Shawn Halayka**
September 5, 2010

Hi Coin,

This problem is treated from a Kaluza-Klein perspective in Chapter 10.3 "Flux compactifications: Moduli stabilization" of String Theory and M-Theory: A Modern Introduction by Becker, Becker, and Schwarz (that is, THE John Schwarz).

Whether you are a fan of string theory or not, this chapter is a good read.

Researchers Discover How to Conduct First Test of "Untestable" String Theory

September 1, 2010
Categories: This Week's Hype

A couple people this morning pointed me to today's press release from Imperial College, headlined Researchers Discover How to Conduct First Test of "Untestable" String Theory and subtitled "New study suggests researchers can now test the 'theory of everything'". In case you miss the headline and subtitle, and thus the point that string theory is now testable due to the efforts of Imperial College researchers, the rather short press release repeatedly drives the point home:

> The new research, led by a team from Imperial College London, describes the unexpected discovery that string theory also seems to predict the behaviour of entangled quantum particles. As this prediction can be tested in the laboratory, researchers can now test string theory…
>
> Using the theory to predict how entangled quantum particles behave provides the first opportunity to test string theory by experiment…
>
> The discovery that string theory seems to make predictions about quantum entanglement is completely unexpected, but because quantum entanglement can be measured in the lab, it does mean that at last researchers can test predictions based on string theory…

There's a blog posting written about the preprint of this paper when it first appeared here, in which I pointed out that the result worked out in the paper is just an example of a well-known piece of mathematics that comes down to classifying nilpotent orbits. This is based on a famous 1971 theorem of Kostant-Rallis, and Nolan Wallach worked out in detail here the specific example considered by Duff et al. in lecture notes for a 2004 summer school. The initial preprint didn't refer to this mathematical literature, but a revised version was soon issued in which a reference to the Wallach notes was added to the bibliography. There's no trackback to the discussion on Not Even Wrong at the arXiv listing for the paper due to the arXiv's censorship policy, but perhaps one or more of the authors of the preprint are regular readers here…

I have no idea how this paper is supposed to contain a "test" of string theory. The simple quantum mechanics problem at issue comes down to classifying orbits of a group action on a four-fold tensor product, exactly what Wallach worked out in detail in his notes, as an example of Kostant-Rallis. If you do an experiment based on this and it doesn't work, you're not going to falsify string theory (or Kostant-Rallis for that matter). By now there's a long history of rather outrageous press releases being issued about the discovery of supposed "tests" of string theory. This one really takes the cake…

Update: The press release is having its intended effect, generating stories headlining false claims about string theory . So far today, there's String Theory:Testing the Untestable? , New study suggests researchers can now test the 'theory of everything', Scientists Say They Can Now Test String Theory and Researchers Devise the First Experimental Test of Controversial, Confusing String Theory. There's even UK Scientists discover way to test untestable string theory, which has the test already performed:

> Scientists at the Imperial College London have managed to conduct the first string theory test, destroying previous beliefs that it was untestable….
>
> The discovery will please physicists, most of whom consider string theory the best available for explaining the universe.

Unfortunately, no details on how the test turned out…

Update: The subtitle on the press release has been changed. It used to be "New study suggests researchers can now test the 'theory of everything'", now it's "New study presents unexpected discovery that string theory may predict the behaviour of entangled quantum particles."

Update: No press campaign for a "finally string theory is testable" claim is complete without a Slashdot story (actually, stories, here and here):

> Big news for theoretical physicists who are fed up with the inability to test String Theory…

Update: Lisa Grossman has a story about this at Wired Science. She went to the trouble of contacting well-known string theorists for their opinion, which is unanimous that this is not a "test of string theory":

> "Already I can imagine enemies sharpening their knives," Duff said.
>
> And they are. A chorus of supporters and critics, including Nobel laureate and string theory skeptic Sheldon Glashow and string theorists John Schwarz of Caltech, James Gates of the University of Maryland, and Juan Maldacena and Edward Witten of the Institute for Advanced Study in Princeton agree that Duff's argument is "not a way to test string theory" and has nothing to do with a theory of everything.

I'm still trying to figure out what the supposed test of string theory is, since I can't find such a thing in the published paper. The Wired article has a bit more explanation from Duff:

> Whether the result is some fundamental principle or some quirk of mathematics, we don't know, but it is useful for making statements about quantum entanglement.

As far as I can tell, we do know where their results come from, a "quirk of mathematics" known as the Kostant-Rallis theorem, applied to the invariant theory question that comes up in quantum entanglement.

The article also contains quotes from me, saying about what you'd expect.

Update: Science News has completely uncritical coverage of the "First Test of String Theory" claims.

Comments

1. **Kea**
   September 1, 2010

   Gee, that is totally outrageous! And especially disappointing, given the real scientific interest of this work. I know that at least one of the young authors on these papers is not a great fan of string theory, which is to say that he understands the quantum information theory is just that – quantum information theory.

   This is not surprising for Imperial though. They are known to care a lot more about their reputation (preventing certain people from speaking there) than about the actual science.

2. **Anonymous**
   September 2, 2010

   How can they claim this is the *first* test of string theory? The first test was quark-gluon plasma at RHIC.

3. **rrtucci**
September 2, 2010

I too think that dishonesty in academic press releases is a serious problem. Such practices erode the public's trust in science and their understanding of it. They confuse non-scientists, blurring in their minds the difference between someone advocating homeopathy and someone advocating Newton's equation. Non-scientists start thinking that scientists are lying when scientists tell them that vaccines don't cause autism, because after all, they've seen scientists lie or exaggerate in other contexts.

I hope someone starts a website like http://www.PolitiFact.com or http://www.FactCheck.com. It could have a truth-o-meter, a la poltifact, for each press release or newspaper/magazine article that any concerned scientist entered. (A good source of press releases is http://www.eurekalert.org). How to arrive at the truth-o-meter rating would require some thought (maybe by poll?). The truth-o-meter ratings for each university department could be compiled together. My impression is that some university departments are worse offenders than others. It would be nice to give each university department a grade to embarrass bad offenders into behaving better.

4. **lun**
September 2, 2010

In this, unfortunately, string theory is hardly unique.
Yale University characterized the (probably bogus in any case) discovery of topological parity-violating effects in heavy ion collisions as
"Yale scientists break the laws of physics" (!!!!!!!!!!!!!!)
(http://www.sciencedaily.com/releases/2010/03/100329214740.htm)

In our day and age, there is the perception that public relations determine funding. The people who write these announcements are neither scientifically competent nor so interested in objectivity.

THese are the results.

5. **piscator**
September 2, 2010

Anonymous's comment is either deeply depressing or wonderfully tongue-in-cheek. Unfortunately I can't decide which….

6. **rrtucci**
September 2, 2010

Correction: I meant http://www.factCheck.org, not http://www.factCheck.com

7. **nul**
September 2, 2010

This is the BNL/RHIC press release about parity-violating effects in (polarized) p-p collisions at RHIC.
http://www.bnl.gov/rhic/inside/news.asp?a=1588&t=today

Overall much more toned-down compared to "Yale scientists break the laws of physics" but also much less accessible to a general (non-physics) audience. From what I see in the BNL press release, it does not sound bogus. There can be bubbles in a QGP where parity is locally violated (but averaged over many bubbles the parity violation is zero). Why not?

As for string theory and QIT etc. — yes, publicity seems to be an essential ingredient for funding. And yes, string theory is not unique in this trend.

8. **Marcus**
September 2, 2010

It seems the new Hawking book hypes M-theory, the multiverse, etc. It goes on sale within week (7 September) and is currently #9 among all books Amazon sells. The Amazon page has a two-paragraph quote from Hawking summarizing the book's message as he sees it.
To me this suggests we are about to be engulfed in a wave of buzz, misleading half-truths, and public gullibility. But maybe it won't be that bad.

9. **Mark Decker**
September 2, 2010

Looks like people from all sides are jumping all over this story. It's too bad that string theory itself doesn't get the same critical analysis that this test claim is receiving.
rrtucci: Thank you for checking your facts on the factcheck address.

10. **lun**
September 2, 2010

nul… of course the "discovery" is reasonable. The problem is that the same _observed_ effect can be reproduced without parity-violating bubbles, via much more mundane effecs such as local momentum conservation and jet fragmentation.
See http://arxiv.org/abs/1008.4919 or http://arxiv.org/abs/1008.3846
These ideas appeared well before the public relations announcement.

It of course does not mean that parity-violating bubbles are excluded, or that this research is worthless (it is in fact extremely interesting and stimulating, and I hope it continues, eg by designing more direct experimental signatures of this effect), but it annoys me that PR announcements trumpeting great discoveries are made almost at the same time as preprints explaining the "discoveries" with more mundane mechanisms appear online.

This is the mirror image of the string theory announcements… or at least, a proof that misleading PR is far from unique to string theory.
It pervades modern science. And it will come to bite all scientists in the back, because in the long-run, it fosters (un)healthy skepticism: Maybe all scientists are full of it. Which they are not.

11. **Chris W.**
September 2, 2010

In the so-called real world this is known as marketing and healthy self-promotion.

See Feynman's example from advertising in his essay "Cargo Cult Science". Apparently many current university press releases on research at their respective institutions barely maintain that dubious standard.

12. **dexmachina**
September 3, 2010

I'm afraid the Slashdot story is my fault. I really should know better than to trust popular science stories. I've linked to this page from the Slashdot post to try to undo some of the damage.

13. **Another Mike**
September 3, 2010

dexmachina, I saw your correction link right away. Not RTFA pays off! I consider this a great way to promote this site and point out an abusive press release.

Hawking Gives Up

September 7, 2010
Categories: Book Reviews, Multiverse Mania

David Gross has in the past invoked the phrase "never, never, never give up", attributed to Churchill, to describe his view about claims that one should give up on the traditional goals of fundamental physics in favor of anthropic arguments invoking a multiverse. Steven Hawking has a new book out this week, called The Grand Design and written with Leonard Mlodinow, in which he effectively announces that he has given up:

> We seem to be at a critical point in the history of science, in which we must alter our conception of goals and of what makes a physical theory acceptable. It appears that the fundamental numbers, and even the form, of the apparent laws of nature are not demanded by logic or physical principle. The parameters are free to take on many values and the laws to take on any form that leads to a self-consistent mathematical theory, and they do take on different values and different forms in different universes.

Thirty years ago, in his inaugural lecture as Lucasian professor, Hawking took a very different point of view. He argued that we were quite close to a final unified theory, based on N=8 supergravity, with a 50% chance of complete success by the year 2000. A few years after this, N=8 supergravity fell into disfavor when it was shown that supersymmetry was not enough to cancel possible ultraviolet divergences in the theory. There has been a recent revival of interest as new calculational methods show unexpected and still not completely understood additional cancellations that may fully eliminate ultraviolet divergences. Hawking shows no interest in this, instead signing on to the notion that "M-theory" is the theory of everything. The book doesn't even really try to explain what "M-theory" is, we're just told that:

> People are still trying to decipher the nature of M-theory, but that may not be possible. It could be that the physicist's traditional expectation of a single theory of nature is untenable, and there exists no single formulation. It might be that to describe the universe, we have to employ different theories in different situations

The book ends with the argument that

- Our TOE must contain gravity.
- Supersymmetry is required to have a finite theory of gravity.
- M-theory is the most general supersymmetric theory of gravity.

ergo

> M-theory is the unified theory Einstein was hoping to find. The fact that we human beings – who are ourselves mere collections of fundamental particles of nature – have been able to come this close to an understanding of the laws governing us and our universe is a great triumph.

This isn't exactly an air-tight argument…

The book begins in a more promising manner, with a general philosophical and historical discussion of fundamental physical theory. There's this explanation of what makes a good physical model:

> A model is a good model if it:
>
> 1. Is elegant
> 2. Contains few arbitrary or adjustable elements
> 3. Agrees with and explains all existing observations
> 4. Makes detailed predictions about future observations that can disprove or falsify the model if they are not borne out.

The fact that "M-theory" satisfies none of these criteria is not remarked upon.

The book is short (about 100 pages of actual text, interspersed with lots of color graphics and cartoons), and contains rather little substantive science. There are no references of any kind to any other sources. The discussion of supersymmetry and M-theory is often highly misleading. For example, we are assured that

> various calculations that physicists have performed indicate that the partner particles corresponding to the particles we observe ought to be a thousand times as massive as a proton, if not even heavier. That is too heavy for such particles to have been seen in any experiments to date…

With no references, one has no idea what these "various calculations" might be. If they are calculations of masses based on the assumption that the supersymmetry and electroweak-symmetry breaking scales are similar, they typically predict masses visible at the Tevatron or LEP. I suspect that the logic is completely backwards here: what is being referred to are calculations based on the Tevatron and LEP limits that require masses in the TeV range.

As for the fundamental problem of testability of M-theory, here's the only thing we get:

> The theory we describe in this chapter is testable…. The amplitude is reduced for universes that are more irregular. This means that the early universe would have been almost smooth, but with small irregularities. As we've noted, we can observe these irregularities as small variations in the microwaves coming from different directions in the sky. They have been found to agree exactly with the general demands of inflation theory; however, more precise measurements are needed to fully differentiate the top-down theory from others, and to either support or refute it. These may well be carried out by satellites in the future.

This looks like one of many dubious claims of "testability" of multiverse theories, which tend to founder on the measure problem and the fact that one has no idea what the underlying theory actually is. Without any details or references though, it's hard to even know exactly what the claim is here.

One thing that is sure to generate sales for a book of this kind is to somehow drag in religion. The book's rather conventional claim that "God is unnecessary" for explaining physics and early universe cosmology has provided a lot of publicity for the book. I'm in favor of naturalism and leaving God out of physics as much as the next person, but if you're the sort who wants to go to battle in the science/religion wars, why you would choose to take up such a dubious weapon as M-theory mystifies me. A British journalist contacted me about this recently and we talked about M-theory and its problems. She wanted me to comment on whether physicists doing this sort of thing are relying upon "faith" in much the same way as religious believers. I stuck to my standard refusal to get into such discussions, but, thinking about it, have to admit that the kind of pseudo-science going on here and being promoted in this book isn't obviously any better than the faith-based explanations of how the world works favored by conventional religions.

For some reviews of the book showing a bit of skepticism, see ones by Craig Callender, Fred Bortz, and Roger Penrose. For much more credulous reviews, see for example James Trefil (who evidently has his own multiverse book coming out). The Economist has a news story about this, which assures us that Hawking is

> a likely future recipient of the Nobel prize in physics (if, as expected, his 1974 theory that black holes emit radiation despite their notorious all-engulfing gravitational pull is confirmed by experiments at the Large Hadron Collider in CERN).

Update: There's a new posting at physicsworld.com by Hamish Johnston that brings up the issue of the potential damage caused by this to the cause of science funding in

Britain:

> This morning there was lots of talk about science on BBC Radio 4's Today Programme — but I think it left many British scientists cringing under their duvets.
>
> Hawking explained that M-theory allows the existence of a "multiverse" of different universes, each with different values of the physical constants. We exist in our universe not by the grace of God, according to Hawking, but simply because the physics in this particular universe is just right for stars, planets and humans to form.
>
> There is just one tiny problem with all this — there is currently little experimental evidence to back up M-theory. In other words, a leading scientist is making a sweeping public statement on the existence of God based on his faith in an unsubstantiated theory…
>
> Physicists need the backing of the British public to ensure that the funding cuts don't hit them disproportionately. This could be very difficult if the public think that most physicists spend their time arguing about what unproven theories say about the existence of God.

Update: Today's Wall Street Journal has a quite positive review of the book by Sean Carroll.

Update: See here for John Horgan's take on the Hawking book:

> I've always thought of Stephen Hawking—whose new book The Grand Design (Bantam 2010), co-written with Leonard Mlodinow, has become an instant bestseller—less as a scientist than as a cosmic, comic performance artist, who loves goofing on his fellow physicists and the rest of us…
>
> Toward the end of the meeting , everyone piled into a bus and drove to a nearby village to hear a concert in a Lutheran church. When the scientists entered the church, it was already packed. The orchestra, a motley assortment of blond-haired youths and wizened, bald elders clutching violins, clarinets and other instruments, was seated at the front of the church. Their neighbors jammed the balconies and seats at the rear of the building.
>
> The scientists filed down the center aisle to pews reserved for them at the front of the church. Hawking, grinning ear to ear, led the way in his motorized wheelchair. The townspeople started to clap, tentatively at first, then passionately. These religious folk seemed to be encouraging the scientists, and especially Hawking, in their quest to solve the riddle of existence.
>
> Now, Hawking is telling us that unconfirmable M-theory plus the anthropic tautology represents the end of that quest. If we believe him, the joke's on us.

Comments

1. **Marcus**
   September 7, 2010

   Perceptive review in NYTimes:
   http://www.nytimes.com/2010/09/08/books/08book.html
   "…the real news about [the book] is how disappointingly tinny and inelegant it is."

2. **Greg Sivco**
   September 7, 2010

   Excellent, Peter. This is the objective review we have been waiting for. I feel so bad for Hawking, whom I otherwise admire. FOX News' review showed its usual stripes in choosing what had to be the ugliest picture ever taken of Professor Hawking in its horrible unfair and unbalanced review, presented as usual to engender its usual fear-mongering policy.

   Nothing more to add really, except for Roger Penrose's excellent opening words in his review of same which you linked to, which are:

   "In *The Grand Design*, Stephen Hawking gives his perspectives on physical reality and expectations for **future fundamental physics**, ably assisted by the fine science writer Leonard Mlodinow. These issues are made accessible to general readers via apposite analogies. Nonetheless, I doubt that adequate understandings can arise in this way. This applies particularly to "M-theory", a popular (but fundamentally incomplete) development of string theory …"
   … Roger Penrose, 4 Sept. 2010

   I get SO sick and tired of SuperStrings' M-Theory being presented as "theory", rather than the "idea of a theory" that it really is.

3. **Cale**
   September 7, 2010

   off-topic so i hope you find it interesting:
   sean carrol has made this talk here:
   http://www.youtube.com/watch?v=GFMfW1jY1xE&feature=player_embedded
   in which he partly discusses the Boltzman brains problem but also talking about how baby universes can be created out of vacuum due to quantum fluctuations given endless amounts of vacuum and endless amounts of time. Does this immediately ring untrue to you for some reason?

4. **Yatima**
   September 7, 2010

   "The fact that we human beings – who are ourselves mere collections of fundamental particles of nature – have been able to come this close to an understanding of the laws governing us and our universe is a great triumph."

   This begs the question of "how close is that" and is there a metric space involved?

   Reminds me of a short story (by Alastair Reynolds I think) wherein *actual* understanding of the physical laws was made unreachable by the fact attaining full understanding immediately caused the understanding agent to collapse into a black hole, taking his knowledge with him. [That would mean "understanding" has to do with energy density, which would open novel ways of thinking about thinking. Turing Machines won't do for that.]

5. **Marcus**
   September 7, 2010

   That review of the Hawking potboiler was so perceptive and sharply written that it put the reviewer, Dwight Garner, on the map for me. He's good. I normally don't read book reviews, but I'm going to start following his. He does non-fiction. As a sample here is his list of 10 favorite non-fiction books from 2009:
   http://www.nytimes.com/gift-guide/holiday-2009/giftguide-garner/list.html?pagewanted=print

What he picks, and the one or two-sentences he says about each, can give you an idea of what he's like.
Here again is the link to his review of "The Grand Design"
http://www.nytimes.com/2010/09/08/books/08book.html

6. **Peter Woit**
September 7, 2010

Cale,

Sean Carroll's views on the arrow of time etc. are definitely off topic, but I was going to point out that, unlike me, he is interested in the God thing, and has a recent posting about Hawking's book from that point of view, which has attracted a huge number of comments. People interested in that aspect of the book are encouraged to discuss it there.

7. **Anonymous**
September 7, 2010

Though Hawking is not a string theorists himself, through his numerous popular books starting from "A Brief History of Time", he may be the single person most responsible for spreading the hype of string theory to the general public. As an iconic figure in front of mass media, his endorsement of string theory means a lot. I'm sure string theorists feel indebted.
As for N=8 supergravity, at least according to Lubos, you can't get any realistic phenomenology out of it. So finiteness may not be the only issue that affects people's opinion of SUGRA.

8. **Christian Takacs**
September 8, 2010

I am not a physicist, I am not a mathematician, but I am a student of philosophy, history, logic, science, and the human mind. With all due respect, It seems that Mr. Hawking does not know the difference between Mathematics and Reality. They are not the same thing, and can't be the same thing by their very nature …and by definition in any respected dictionary.
For Mr. Hawking to be calling upon "infinite" unknowable, unobservable, unmeasureable multiverses to resolve any physics problem in this reality is ridiculous… unless his intended genre is actually fantasy or metaphysics.

9. **Joe Bob**
September 8, 2010

Does any serious physicist take Hawking very seriously anymore?

10. **chris**
September 8, 2010

What always irked me about Hawkings arguments is that they would rank rather highly on the crackpot index due to his frequent mentioning of Einstein.

11. **DB**
September 8, 2010

The real problem is that many very successful theoretical physicists become agents of negative influence when they pass the age at which they are capable of making fundamental breakthroughs, roughly the age of forty to forty-five. As they age, they become an embarrassment to the field.

Einstein, post 1930, was the prime example and his malign influence is felt today in the obsession with TOEs and mathematical beauty. Dirac and Heisenberg showed similar symptoms in their dotage.

Driven perhaps by a touch of megalomania, and stripped of their ability to recreate their past success they venture into the intellectually sloppy regions of speculative physics.

12. **Anon2**
September 8, 2010

Hawking is just following the fashion trend set by Susskind's "Cosmic Landscape" and Dawkin's "God Delusion" in claiming that the scientific theory of string disproves God by providing a large landscape of parallel universes, so that fine tuning can be by anthropic self-selection (with no need for a prayer-deaf old man in a beard).

More exciting news: Roger Penrose's new book "Cycles of Time" is published in the UK on 23 September. Penrose hasn't given up!

13. **Chris Oakley**
September 8, 2010

I was feeling depressed about the state of theoretical physics when I heard a voice say, "Cheer up! Things could be worse!" So I cheered up, and, sure enough, things *got* worse. Hopefully Penrose's book will be better.

14. **BigG**
September 8, 2010

First, its always to good have alternative view points and people following paths outside the mainstream. And if the people who do this are respected physicists all the better.

Second, all this critisism of Hawking and Einstein is unfounded. The same things were being said about Einstein in 1905. Further, in the early 20th century the same kind of people were saying there's only mechanics and e&m; all that's left of physics is to figure out this particular case or that. The use of the word 'crackpot' is highly over used. Yes, there are certainly people who are way out there but the same was said by the old generation of the new in the earth 20th century. Besides, how many of you who criticize Einstein or Hawking have ever written anything close to what they've done. I apologize for going off topic but its shameful to read this stuff. People who know how science works would never make the criticisms you are. Imagine youself 100 years ago defending strict determinism in the face of quantum theory.

15. **wolfgang**
September 8, 2010

>> the age at which they are capable of making fundamental breakthroughs, roughly the age of forty to forty-five.

Planck was 42 when he discovered quantum theory in 1900.
Born was 43 when he formulated matrix mechanics.
Einstein was 45 when he wrote about what we now call Bose-Einstein statistics.
Hahn was 59 when he discovered fission.
Bethe proposed neutrino oscillations when he was 80.

Just saying.

16. **Arun**
September 8, 2010

Imagine youself 100 years ago defending strict determinism in the face of quantum theory.

You mean the "God does not play dice with the world" Einstein?

The criticism of the physics here is that it has ZERO experimental or observational content, which is rather the opposite of the situation with quantum mechanics a century ago.

17. **younghun park**
September 8, 2010

Peter
From your writing, I don't think Hawking gave up the unified theory. Like many physicist, he has had the dream on the unified theory, the theory of everything.

I feel that the thing he gave up is the hope to reach one perfect
theory by using current methods, supersymmetry, string theory and so on. He seems to ask us how the unified theory can be made.He seems to long us to find the key toward that theory.

I have learned that physics aims at describing the nature by simple, very simple idea. I have fallen in love with physics due to that point.

I believe that physics can advance continuously when it aims at the unified theory even if that is impossible.

18. **stan**
September 8, 2010

It is interesting to note that the timing of David Gross' exhortations to "never give up" coincided quite closely with the cessation of his research output. Who's doing the giving up here?

19. **BigG**
September 8, 2010

Arun,

You've taken that quote out of context. Einstein was working within QM to find some kind of derterminism. My post was talking about those of the older generation who opposed it merely on belief or speculation, like those here are criticising Hawking.

20. **Giotis**
September 8, 2010

Why Hawking relates M-theory to the Multiverse? From what I read for the book in various places is like if M-theory directly implies and explains the Multiverse and the Multiverse concept could not be understood without it. This is not true of course.

The KKLT model which triggered the multiverse frenzy is in the good old IIB. The counting of the flux vacua by Douglas which resulted the notorious 10^500 number was performed in IIB also. M-theory is not needed to derive the Multiverse and to say that M-theory (and M-theory alone) implies it (and thus explains why God is not needed) is simply wrong. On the contrary I may add direct compactifications of M-theory to 4 dimensions (i.e. on G2 manifolds) and its vacua are not well understood.

21. **Chris Oakley**
September 8, 2010

Hi BigG,
Let me see if I have understood you correctly. Around 1905 Einstein explained the photoelectric effect – an experimental result – by proposing that light, as well as being a wave governed by Maxwell's equations, was also a quantum of energy $h\nu$. Notwithstanding the success of the interpretation, some of the older generation of physicists never accepted this because they refused to try to wrap their minds around the idea that something could be a wave and a particle at the same time.

105 years later Hawking is proposing that because a theory, or at least framework for speculation, called M-theory, something that he has never worked on, is incapable of predicting anything we need to adjust our definition of "prediction" so that it does, and if we cannot do that then we are just like those who could not accept wave-particle duality in the early part of the 20th century.

Have I got this right?

22. **Mark Decker**
September 8, 2010

BigG,
Your comment: "People who know how science works would never make the criticisms you are" is absolutely absurd.
It is the very people who know how science should work (and haven't lost their way) who are the ones making these critical comments. Don't confuse the actual word "Science" with modern day individuals who have the credential to refer to themselves as "Scientists" as they spew endless nonsense.
I like Chris Kennedy's quote (on the majority of Theoretical Physicists today): "They have become heroes to the stupid and laughingstocks to those who know better."

23. **MyrtleParker**
September 8, 2010

"This could be very difficult if the public think that most physicists spend their time arguing about what unproven theories say about the existence of God."

My estimation of Stephan Hawking has gone so far down because of this and his times article. Really sad.

There really is very little difference between so-called scientists engaging in vapid anthropic/Multiverse/M-Theory speculation and religion.

24. **MyrtleParker**
September 8, 2010

Sean Carroll's praise of Hawking and absurd statements that science understands the creation of the universe is equally shameful. Our knowledge of the laws of physics break down completely with the Big Bang. He should know better. Looks like he has really bought into the physicist as well known ego-hype-charlatan. Very sad.

25. **Consumer Reviews**
September 8, 2010

Interesting review! One thing is for sure, Stephan Hawking is a fascinating personality.
Kind regards.

26. **DaveC**
September 8, 2010

Steven Hawking and Sean Carroll both have something in common with an increasing, perhaps even overwhelming, majority of theorists nowadays. They have never once had the experience of either predicting or convincingly interpreting the results of a good experiment (nor have they come up with any original mathematics themselves). Perhaps Hawking is one of the oldest of whom this could be said. After spending a career playing mind games with others in the same boat, and being praised for doing so, it's not too surprising that they end up like this.

It's hard not to see a big crunch ahead for physics, as generations are trained by people who haven't had this experience, compounded with all the other overwhelming pressures on us nowadays to live and breathe hype, pretending our work will answer the mysteries of the universe, solve the energy crisis in one blow, allow us to simulate reality, or whatever, in order to get funded and attract students.

27. **neo**
September 9, 2010

Stephen's (not Steven's!) accomplishments speak for themselves. Why he has embarked on this exploitation tangent is beyond me. His approach seems to be the substitution of one form of theology for another.

28. **longchenpa**
September 9, 2010

so if an actual physicist who is hundreds of times smarter than you says things that go against your pet peeve, of course, something must be wrong with him.

29. **neo**
September 9, 2010

longchenpa:

That is appeal to authority. Stick to arguments. No one is so smart that they cannot be wrong.

30. **BigG**
September 9, 2010

Chris & Mark,

No you don't adjust anything. My point is we don't know what the future holds. We don't know for sure if Hawking's prediction will turn out to be true. Let him follow his own path. Why are so many people afraid? If his theory is proven its accept it, if not its rejected. The method will take care of the problem. The whole point I'm making is you don't know. That's what I mean by people who know science wouldn't say what's going on here. The only certainty in science is what was will most likely be overturned to some extent.

I have two qurstions:

1) can you predict the future
2) what have you contributed to physicss besides criticism.

If you want to voice a disagreement this is reasonable but the hostility is unfounded. based on the comments here I'm not sure you know how science works.

31. **Arun**
September 9, 2010

BigG:
If his theory is proven its accept it, if not its rejected. The method will take care of the problem.

What he proposes falls outside the "method". That is the problem.

32. **Coin**
September 9, 2010

Actually, the NYT review linked a couple times above strikes me as darn weird. The author seems to spend more time expressing concern that a multiverse theory would potentially render God unnecessary, or concern that Hawking&Mlodinow think it would, than anything else about the book. In short he doesn't seem concerned with whether the multiverse hypothesis is good science, he's just upset by its perceived consequences– its theological consequences even? Now of course if H&M "started it" by bringing up atheism on every page or something then that doesn't strike me as good science writing, but the NYT reviewer doesn't give much indication that was the case– he just seems offended the subject was at some point broached and doesn't seem concerned with the actual substance of the book. I feel like I didn't learn much about the book by reading that review.

A question, do we really know how much of this book was written by Hawking?

33. **Wavefunction**

September 9, 2010

On a related note, what do you think of Shing-Tung Yau's new string theory book "The Shape of Inner Space"?

34. **Peter Woit**
September 9, 2010

Wavefunction,

I did read the Yau book quite recently and plan to write about it here very soon. About all I'll say now is that I liked the book a lot more than I expected (the opposite of the case with Hawking's).

35. **Mark Decker**
September 9, 2010

BigG'
You ask: "Why are so many people afraid."
I don't think they are afraid. I'm certainly not. And I don't consider my statements to be hostile either. I think most people are responding the way they are out of frustration. I can understand why anyone who just wants to get a responsible discussion going on these matters sees the Hawking book as just another setback. However, I don't see this as a setback. I think the lines have been drawn and there are two camps. The Greene-Kaku-Hawking, etc… camp who get all of the media attention including guest spots on CNN, shows on PBS, etc.. and the other camp, who are a little more responsible when it comes to investing themselves in certain theories. The latter camp, although more scientifically minded than the former, have been reduced to an underground movement. No big lights or fancy promises of extra dimensions. Just people who want to get to the truth. That's actually fine with me. Let the masses be entertained with science fiction. As long as there is an outlet for the continuation of this underground movement (like Woit's blog) I'm happy.
Can I predict the future? No
And to answer your other question: My criticism is my contribution to physics, and I think an important one.

36. **Marcus**
September 9, 2010

Coin,
you miss the point of the first 1/3 or so of the NYT review, which took Hawking to task for "Godmongering"—getting books talked about by issuing provocative pronouncements about God. Pro or anti, doesn't matter.

To illustrate: Hawking and Mlodinow had a byline piece in the WSJ (3 September) with the heading
Why God did not create the universe
which was described as an excerpt from their book. The article contrasts modern science with primitive superstitions concerning natural phenomena, raises questions about why the universe is the way it is, and proceeds as follows:

==quote==
… Luck in the precise form and nature of fundamental physical law is a different kind of luck from the luck we find in environmental factors. It raises the natural question of why it is that way.

Many people would like us to use these coincidences as evidence of the work of God. The idea that the universe was designed to accommodate mankind appears in theologies and mythologies dating from thousands of years ago. In Western culture the Old Testament contains the idea of providential design, but the traditional Christian viewpoint was also greatly influenced by Aristotle, who believed "in an intelligent natural world that functions according to some deliberate design."

That is not the answer of modern science. As recent advances in cosmology suggest, the laws of gravity and quantum theory allow universes to appear spontaneously from nothing. Spontaneous creation is the reason there is something rather than nothing, why the universe exists, why we exist. It is not necessary to invoke God to light the blue touch paper and set the universe going.
==endquote==

If it matters to you, it is clear they STARTED IT as you said in your post, and that they are USING theological provocation to get attention (e.g. from WSJ readers) and build sales.

The reviewer, Dwight Garner, is not critical of the atheism *per se* but of the cynical potboiler aspect. He is just as critical of the seemingly "pro-God" teasing that Hawking used in his 1988 book. Godmongering can be worked either way. Ambiguous pronouncements aimed at firing up discussion.
http://online.wsj.com/article/SB10001424052748704206804575467921609024244.html

The NYT reviewer spent only about 1/3 of the article on the cheap theological provocation. He then ripped into the book's other superficial potboiler characteristics.

To make it clear that it was not simply the atheism *per se*, I will quote briefly from the NYT review.

==quote Dwight Garner review==
In "A Brief History of Time" Mr. Hawking also dabbled in what the science writer Timothy Ferris has called "Godmongering." Mr. Hawking… ended "Brief History" by declaring that the discovery of a unified theory of physics could help us to **"know the mind of God."** It was a line that — cynically, some thought — allowed glints of fuzzy sunshine to warm the cold blade of his thinking.

Mr. Hawking's new book, "The Grand Design," … has already made headlines … thanks to a **different sort** of Godmongering. This time Mr. Hawking has, we're told, declared God pretty much dead…
==endquote==

I read it as a kind of teasing which involves taking phony pro- or anti- *poses* that, partly because of their deliberate ambiguity, contribute neither to science nor to theological discussion.

37. **Marcus**
September 9, 2010

Since I quoted a few lines of the NYT review I should probably give the link:
http://www.nytimes.com/2010/09/08/books/08book.html
I gave it earlier but you'd have to scroll back quite a ways to find it.

38. **Chris W.**
September 10, 2010

For more on the discussion in the U.K. of Hawking and Mlodinow's book, see this Physics World blog post by James Dacey.

39. **Anonymous**
September 10, 2010

Leonard Susskind also debunked Intelligent Design as an "illusion" based on the idea of string multiverse. The non-uniqueness of string theory is obviously our best weapon in fighting religious dogma.

40. **Anon2**
September 10, 2010

Newton could have "gone anthropic" to predict gravity on the basis that people would have drifted off into space without it and would have been unable to move with too much. He could thus have done rough and ready calculations of the order of magnitude of the gravitational acceleration needed to keep most people's (not string theorist's) feet on the ground long enough for them to behave sensibly and thus exist.

However, Newton didn't waste his time on such pseudoscientific anthropic arguments. Lee Smolin made a comment here a few years ago (Not Even Wrong) claiming that every "anthropic" prediction is vacuous. E.g. Hoyle claimed to anthropically predict a significant cross-section for carbon synthesis in stars from the fusion of three alpha particles (necessary because of the beryllium bottleneck) in order to produce the carbon-12 for life. But all he was really doing was making a very rough and ready *ad hoc* prediction from a theory to explain the observed carbon abundance in the solar system.

41. **Nono**
September 10, 2010

> The non-uniqueness of string theory is obviously our best weapon in fighting religious dogma.

The *only* weapon to fight religious dogma is the scientific method. Take a look at the book "Facing Up" by S. Weinberg.

42. **notan**
September 10, 2010

"the apparent laws of nature are not demanded by logic or physical principle"

Most of us are aware of the fact that the apparent laws of nature are not demanded by logic since, if they were demanded by 'logic' then this 'logic' could not be logic. To put it differently, if some theory gives you some information about the (laws of) nature it can't be logic in any sense reasonable sense of the word "logic" (unless you think you could learn something about nature while staying in your bedroom).

So we are left with

"the apparent laws of nature are not demanded by physical principle"

Then you should stop searching for physical principle and find another job.

43. **zanzibar**
September 10, 2010

RE: Bethe's work on neutrino oscillations –

I saw him talk on this when he was indeed 80 years old. I remember it being inspirational – deep physical insight and mathematics. Very impressive.

I also remember watching the difficulty he had backing his rental car out of a space in the parking lot afterward. He was alone, and though I admired his independent spirit, I do remember a certain measure of concern I had for the other drivers who had to subsequently share the road with him.

For a mathematical counter-example against youth, see E.T. Bell's "The World of Mathematics", chapter 12 – "Invariant Twins, Cayley and Sylvester" –

http://books.google.com/books?id=Bnh3FKIKT9AC&pg=PA340&lpg=PA340&dq=bell%27s+men+of+mathematics+cayley+sylvester&source=bl&ots=44L8UFK08L&sig=kr4d3gjz31fgC_hv9vteJeiVCnKTLncEMKblgfLnczECw&sa=X&oi=book_result&ct=result&resnum=2&ved=0CBkQ6AEwAQ

44. **Marko Amnell**
September 11, 2010

Anon2 wrote:
"Newton could have "gone anthropic" to predict gravity on the basis that people would have drifted off into space without it and would have been unable to move with too much. He could thus have done rough and ready calculations of the order of magnitude of the gravitational acceleration needed to keep most people's (not string theorist's) feet on the ground long enough for them to behave sensibly and thus exist. However, Newton didn't waste his time on such pseudoscientific anthropic arguments."

Newton was undoubtedly a great scientist but he was not a paragon of scientific rationality. Quoting Wikipedia:

"In his Hypothesis of Light of 1675, Newton posited the existence of the ether to transmit forces between particles. The contact with the theosophist Henry More, revived his interest in alchemy. He replaced the ether with occult forces based on Hermetic ideas of attraction and repulsion between particles. John Maynard Keynes, who acquired many of Newton's writings on alchemy, stated that "Newton was not the first of the age of reason: He was the last of the magicians."[40] Newton's interest in alchemy cannot be isolated from his contributions to science; however, he did apparently abandon his alchemical researches.[5] (This was at a time when there was no clear distinction between alchemy and science.) Had he not relied on the occult idea of action at a distance, across a vacuum, he might not have developed his theory of gravity."
http://en.wikipedia.org/wiki/Isaac_Newton

45. **BigG**
September 11, 2010

Hawking doesn't know what he's talking about when he criticizes philosophy. He also speaks about science as having proved something related to the origin of the universe. Like a lot of physicists he mixes up terminology. They should know better than to use meaningless terms like 'theory of everything' and 'nature of space and time'.

Arun,

there is no method. It is as real as the nature of space and time.

46. **Phil Warnell**
September 11, 2010

Dr. Woit,

You may find that Hawking is inconsistent with his theoretical preferences, yet he has now also apparently completely changed his philosophical standpoint, as coming to believe that science can dismiss the question why as being superfluous; whereas before he thought holding any opinion on such matters to be never the correct place for the discipline. It was not that long ago that he expressed this clearly in a critic he made in respect to Roger Penrose's views when it came to such matters when he wrote upon Penrose's invitation the following:

"Roger Penrose and I worked together on the large-scale structure of space and time, including singularities and black holes. We pretty much agree on the classical theory of General Relativity but disagreements began to emerge when we got on to quantum gravity. We now have very different approaches to the world, physical and mental. Basically he is a Platonist believing that there's a unique world of ideas that describes a unique physical reality. I, on the other hand, am a positivist who believes that physical theories are just mathematical models we construct, and that it is meaningless to ask if they correspond to reality; just whether they predict observation."

– The Large, The Small and the Human Mind- Roger Penrose (page 169)-Cambridge University Press (1997)

It has long been clear to me that Hawking's craving for notoriety has always been more important to him than the search for truth, scientific or otherwise. In such respect I think Sean Carroll's assessment to be the best, which he expressed in the summation of the review you pointed to in saying:

"Answers to the great "Why?" questions are going to be subtle and difficult. Our best hope for constructing sensible answers lies with scientists and philosophers working together, not scoring points off one another."

In my view, if any concern is to be had with books such this, it is to wonder how they serve the public at large able to distinguish the difference between science and religion, when we find the self appointed spokesman for the one built upon doubt places his faith in theory which claims as being reasonable although not able to be tested. It also has me to wonder if Hawking would ever be willing as Penrose was to open himself up to the criticism of one of his peers. I think if science demands anything of its practitioners, that is beyond adherence to the scientific method, it is to at least endeavour to maintain the integrity of the discipline, if not their own.

Best,

Phil

47. **Loki**
September 12, 2010

Perhaps the ALS has caught up with him, and he has lost his ability to rationally convey or understand arguments.

If this is the case, it would be a sad day indeed.

48. **Giotis**
September 12, 2010

I realize now after seeing the big publicity and the way the story is covered by large mainstream media (especially the Larry King interview) that this will have a huge impact in the long run. Hawking not only introduced String-M theory to the general public but more importantly he presented it as the celebrated undisputed theory which represents the climax of human knowledge. The uninitiated one is lead to assume that M-theory is the pinnacle of science endeavour and the only hope we have to understand the world. I'm sure String theorists are celebrating right now.

49. **Peter Woit**
September 12, 2010

Loki,

The problem with your hypothesis is that the pseudo-science Hawking is promoting is rather popular in some circles, so you would need an implausible amount of organic brain damage to explain this phenomenon that way.

50. **Benjamin Gal-Or**
September 12, 2010

Hawking love-story with the media and the masses intimidates many shy-away-from-the-media scientists, mainly by pushing the image of science far into mysticism and a misleading populism, which bluntly exploits the layperson ignorance of what is verifiable science. But the root problem is not Hawking but the many opportunistic scientists that support and even exploit such an approach to the fundamental pillars of verifiable science. Without their support the media would have failed to make an icon out of this false prophet; would at least resort to the audacity of truth: The entire Hawking festival does not even rise to the level of being wrong in the domain of verifiable science.

An unknown personal fact is added next:
Nature was among the first to publish a review on my "Cosmology, Physics and Philosophy" [1981, 1983, 1987, Springer Verlag], which, inter alia, contained criticism of Stephen Hawking's theories as lacking any possible future verification and some are not his but of J. D. Beckenstein.

The review was written by Hawking, who hinted there that instead of my not-easy-to-understand book he would write one for the masses , which he did in 1988 by omitting all mathematical equations and adding mysticism. [BTW: As far as I know some key Hawking's claims in science were first ridiculed by professor Yuval Neeman.]
["People University Online", Cf. my Facebook].

51. **mark davis tortino**
September 12, 2010

i wonder if hawking ever read feynman's cargo cult science speech?

"The first principle is that you must not fool yourself–and you are
the easiest person to fool. So you have to be very careful about

that. After you've not fooled yourself, it's easy not to fool other scientists. You just have to be honest in a conventional way after that.

I would like to add something that's not essential to the science, but something I kind of believe, which is that you should not fool the layman when you're talking as a scientist. I am not trying to tell you what to do about cheating on your wife, or fooling your girlfriend, or something like that, when you're not trying to be a scientist, but just trying to be an ordinary human being. We'll leave those problems up to you and your rabbi. I'm talking about a specific, extra type of integrity that is not lying, but bending over backwards to show how you are maybe wrong, that you ought to have when acting as a scientist. And this is our responsibility as scientists, certainly to other scientists, and I think to laymen.

So I have just one wish for you–the good luck to be somewhere where you are free to maintain the kind of integrity I have described, and where you do not feel forced by a need to maintain your position in the organization, or financial support, or so on, to lose your integrity. May you have that freedom."

http://www.lhup.edu/~DSIMANEK/cargocul.htm

52. **Rude Dude**
September 13, 2010

These are the problems with Mlodinow's other books.

How much of this did he write?

53. **chris**
September 13, 2010

neo,

"Stephen's (not Steven's!) accomplishments speak for themselves. Why he has embarked on this exploitation tangent is beyond me."

i bet it's for the money. what else could it be?

54. **Cotuios**
September 13, 2010

"Stephen's (not Steven's!) accomplishments speak for themselves."

What experimentally verified accomplishments were those?

55. **Bugsy**
September 13, 2010

Question from a non-physicist:
First of all, in the Feynman path integral theory, how in the world do you make sense mathematically of the constant branching of possibilities in all places and at all moments? (Since apparently quantum mechanics is not simply a diffusion process). Maybe the answer is that measurements take place only at discrete places and times. But then same question re the many-worlds theory (since then presumably this splitting takes place everytime and place). I mean is there a mathematical way of formulating this at all, even approximately? I am thinking that the number of possible worlds should certainly not exceed the continuum in cardinality (!) and should in fact lead to a measure space. Now naively the problem seems to be magnified enormously with the landscape idea… especially if it's infinity and not just 10^500. (???) Enlighten a bit please!

My impression is things are way too speculative for Hawking to put his considerable scientific and public weight on one side of the scale…unless of course he REALLY believes what he is saying.

56. **Chris Oakley**
September 13, 2010

Bugsy,

You are conflating things that have very little to do with each other.

1. The Feynman path integral. This is the notion that a system takes all possible paths from a state at an earlier time to a later time, but only the ones which follow the "correct" equations of motion are actually seen because of phase cancellations. It may or may not be rigorous, depending who you talk to. Either way, though, I personally have never understood (or seen the point of) it.

2. The measurement problem in quantum mechanics. Physical observables are only ever eigenvalues of some quantum operator. This does not require and has nothing to do with path integrals, or String Theory.

3. The Landscape in String Theory. The world is, as far as we know, 3+1 dimensional. String Theory is typically 10+1 dimensional. The process of losing dimensions is called compactification and current understanding is that the process can be done in about 10^500 different ways. The options are thus either (i) to give up or (ii) to say that because string must be right physics can therefore be reduced to choosing the right one of these compactifications.

Whether Hawking believes in (ii) or not is not so much the issue as whether anyone should care if he does. However I have noted generally a tendency for the elder statesman of the physics community (with the possible exception of Veltman) to support the status quo, whatever that may happen to be.

57. **Anthony McCarthy**
September 13, 2010

After reading here and other places, I went to my sister-in-law the aquatic biologist, with the happy news that now that physics has been freed from the requirement of actually being tied to physical evidence that she didn't have to go out this winter to do her sampling anymore. She wasn't as happy about it as I thought she would be, though she did take the opportunity to vent about theoretical physicists and cosmologists, their politics, their dirty politics and their hogging of funding. I think she might have felt better after that.

Being a complete outsider I have to say that the idea of an entirely artificial physics generating an entirely artificial mathematics to service it gave me a lot of entertainment while I was doing my chores this weekend. It came to me that the results might be a science that has has more in common with fan fiction than it does the natural universe. But that's only a musician's view of it.

58. **Jeffrey McGowan**
September 13, 2010

True story, I was at a math conference maybe 15 years ago, at a talk on something to do with Riemann surfaces, and the number 26 (or maybe 24) kept coming up. Someone asked "is that the 26 dimensions from string theory?" (back then, if I remember correctly, string theory somehow involved 24 maybe + 2 dimensions?). At least now they've got it down to 10 + 1. I've been told by several very good mathematicians who are much more involved than I am that there is very interesting math involved in string theory, then again, sometimes I get the impression that it's actually inconsistent, and you can prove anything you want to…

59. **Bruce Keener**
September 13, 2010

I'm just a layman, but do have an interest in what makes our world tick, and I tend to buy most of the popular physics books to try to understand current thinking.

I was very disappointed in Hawking's book. My full review of it is here.

But, in short, he makes no attempt to summarize the various ideas regarding the origin of our universe and to say why M-theory is a better candidate … he basically just says it is, like we are supposed to believe him. The problem is, many will just believe him, and will never know that there are competing theories. And to treat cosmic inflation and supersymmetry as fact is just downright deceitful, given that many of his readers will never know that there are many reputable physicists who dispute both. (For all I know, both theories are true, but they are certainly not established fact.)

60. **Peter Woit**
September 13, 2010

Jeff,

Actually there is some quite good mathematics related to string theory, problem is that it tends to be orthogonal to those parts of string theory that are supposed to give unification.

The number 26 of dimensions for the critical bosonic string does have a nice relation to the moduli space of Riemann surfaces. The 10 for superstring theory less so. This is kind of typical: as you focus on those aspects of string theory that might give unification, things become more and more complicated, less and less mathematical elegance is to be found.

61. **Jeffrey McGowan**
September 13, 2010

Peter,

It was a moduli space thing where the 26 came up, back then I had very little to do with that sort of thing, now I'm big into Teichmuller theory, go figure. I do know there is quite interesting math lurking around, interesting what you say about the loss of elegance. Maybe that's why I ended up in math, much easier not to end up jamming a square peg into a round hole.

62. **Bugsy**
September 14, 2010

Chris (Oakley),
Thanks for your patient response.
I guess I was conflating "many paths", "many worlds", and "many universes"! And wondering if anyone thinks all three belong in the same "model", and further wondering if any of that can be formulated mathematically in any sense at all. Is there an actual topological space,
or measure space, which should be phase space, acted on by the dynamics of time evolution?

To try your patience one more time:
from what you say it seems the number 10^500 is really meant to be finite, as it comes from topological or algebraic considerations, and is not a continuum. Does that mean the possible values for each of the various physical constants are also of that number? (Is there a map from the compactification space to a vector space of possible constants?)Thanks….!

63. **Thomas Larsson**
September 14, 2010

Cosmic Clowning: Stephen Hawking's "new" theory of everything is the same old CRAP
Hat tip: Lubos

64. **Magnus W**
September 14, 2010

However a church in Sweden does not mean the people in it are religious… might have clapped just because of his fame…

65. **steve newman**
September 14, 2010

On these blogs and elsewhere i often read word to the effect that
' string theory might not cut it as a physical theory, but it has led to very interesting mathematics".
Just what is this 'interesting math' ??
Can anyone refer me to a book or article for non-specialists that communicate this?
(I am mathematically and scientifically literate (phd theoretical physics, Columbia U 1966), so it needn't be a words-only book for a popular audience.) I follow current physics developments as an

interested outsider, and i'm genuinely interested in knowing about the interesting mathematics that is coming from superstring research.

66. **Peter Woit**
September 14, 2010

Steve,

My book "Not Even Wrong" has a bit about this. There's a new book by Yau and Nadis that just came out (The Shape of Inner Space) which has a lot more.

67. **Chris Oakley**
September 14, 2010

Bugsy,

I would rather not go there – it is too depressing. If it was me I would have thrown out the theory as soon as I discovered that it did not work in 3+1 dimensions. But yes, each string vacuum (=compactification) has its own mass spectrum and coupling constants. The only things that are common are c (the speed of light), h (Planck's constant) and G (the gravitational constant).

68. **Giotis**
September 14, 2010

Chris,

The gravitational constant is not common in all compactifications. It depends on the volume of the extra dimensions.

69. **John Baez**
September 15, 2010

Bugsy writes:

> from what you say it seems the number 10^500 is really meant to be finite, as it comes from topological or algebraic considerations, and is not a continuum.

Yes, it's supposed to be finite – but as far as I can tell, it's just a wild guess, or maybe a lower bound. It's gone up a lot during my lifetime. 🙂

However, I believe that beyond this large finite number of choices, there are also theories that involve choices that come in continuous families. The buzzword is "moduli space".

> Does that mean the possible values for each of the various physical constants are also of that number?

There are lots of theories with different numbers of particles, different kinds of particles, etc. Then you get to choose certain numbers governing the behavior of these particles.

70. **Henrick**
September 15, 2010

Hello Professor Baez!

Where do Hawking & Sean Carroll rank on your crackpot index?

71. **Glenn**
September 15, 2010

"The main reason I wrote about the Bogdanov story, (besides for its entertainment value), is that I think it shows conclusively that in quantum gravity in general, many people have lost the ability or willingness to recognize non-sense for what it is."

Could this quote from you, Peta, also describe Hawking's purpose for writing the book? If so, it reminds me of Picasso's descriptions of the kind of stuff that art galleries promote. I seem to recall an interview with the painter on "60 Minutes" where he confessed to trying to find how far the art world would follow him.

72. **Peter Woit**
September 15, 2010

Glenn,

The Bogdanov story was pretty different. They were producing sophisticated sounding gibberish, which required some expertise to recognize as such. The multiverse stuff being promoted by Susskind, Hawking and many others uses no sophisticated math, and is easy to understand. It's quite easy for even a non-expert to quickly realize that this is not legitimate science. Here I think the only reason that many people take it somewhat seriously is that they can't believe that smart experts would engage in something so obviously flawed. I'm mystified by this myself…

73. **Glenn**
September 15, 2010

Peter, my apologies for the last post addressing you as Peta. Although I feel that animals should be treated kindly, I have very different feelings about this handwriting recognition program on my tablet. It makes changes to previous text when I hit the submit button.

74. **tristes_tigres**
September 15, 2010

> Without any details or references though, it's hard to even know
> exactly what the claim is here.

Something like this ?

http://arxiv.org/abs/1009.2525

75. **Peter Woit**
September 15, 2010

tristes_tigres,

Presumably that's it, although the book was likely written a year or so ago.

I'll stack my guess that any such claim would have trouble with "the fact that one has no idea what the underlying theory actually is" up against the abstract's admission that the paper's "predictions" only apply to "a certain class of landscape models". M-theory itself is the final theory replacing religion since it can never be falsified.

76. **steve newman**
September 16, 2010

Re my request yesterday-
On these blogs and elsewhere i often read word to the effect that
' string theory might not cut it as a physical theory, but it has led to very interesting mathematics".
Just what is this 'interesting math' ??
Can anyone refer me to a book or article for non-specialists that communicate this?

and your answer-
My book "Not Even Wrong" has a bit about this. There's a new book by Yau and Nadis that just came out (The Shape of Inner Space) which has a lot more.

———————-

THANKS. that's exactly what i wanted. I read your book when it first came out but i had forgotten that it covered this. I am revisiting it now with great interest and will also check out the Yau book.
I'm glad this made me look at your book again. It does a really great job of reviewing the field.

77. **Chris W.**
September 16, 2010

More on a rather more humorous note from Physics World's James Dacey, excerpting this piece in the Guardian.

"Some subjects are so serious that one can only joke about them."

— Niels Bohr (see Wikiquote)

78. **John Baez**
September 17, 2010

Henrick writes:

> Where do Hawking & Sean Carroll rank on your crackpot index?

I don't usually go around publicly rating physicists, but I'll do it just for you. Both these fellows have their sins, but neither are 'crackpots' by any stretch of the imagination. And Hawking did such incredibly profound work in his earlier years that he deserves vast amount of slack now. Don't read his new book: read the book he wrote with Penrose, and get a beautiful description of black holes and why they radiate.

79. **John Baez**
September 17, 2010

Sorry, I somehow linked to a $390 *video* version of the Hawking-Penrose book *The Nature of Space and Time*. Or something like that. I have no idea what this video is — does anyone know?

The actual book is just $14.95.

Short Items

September 17, 2010
Categories: Experimental HEP News, Langlands, Multiverse Mania, Not Even Wrong: The Book

- The Fermilab Physics Advisory Committee recently recommended that the Tevatron be kept running for an additional three years (until 2014). By the end of that time it should be able to accumulate a total of 20 fb^{-1} of data, which would give sensitivity to a standard model Higgs at the 3-sigma level over the entire interesting mass range. The cost for this would be a total of about \$150 million, which would likely have to come out of Fermilab's $810 million/year budget. While the idea of continuing to do physics at the high-energy frontier and possibly beat the LHC to the Higgs, for less than 10% of the lab budget/year seems to be a no-brainer, director Oddone still may not be completely sold on the idea. Keeping the Tevatron going would set back some of the projects the lab has planned for its future in a post-Tevatron world. There's also significant concern about the future federal budget situation, and how to make sure that the best possible case is made for a future of Fermilab, in an environment where people may be looking for large, expensive programs that could be cut. For more about this, see Adrian Cho's article Higgs or Bust? in Science.

One huge consideration in this decision is that of what will happen at the LHC. CERN is facing its own budgetary problems, and has just decided to shut down during 2012 not just the LHC (for repair of magnet interconnections), but the entire accelerator complex. Work continues this year on trying to raise the luminosity of the machine, but progress is slow. They still are an order of magnitude lower than where they want to be by the end of the year, with only a few more weeks left before the machine is shutdown as a proton-proton collider and reconfigured for a heavy-ion run. If all goes according to plan, by late 2011 the LHC would have 1 fb^{-1} of data, enough to compete with the Tevatron in the Higgs search. But, so far, plans like this have turned out to be overly optimistic, with things taking longer than expected.

In today's CERN Bulletin and Fermilab today, Oddone and CERN DG Heuer issued a joint statement downplaying the competition between their labs:

> The press makes much of the competition between CERN's LHC and Fermilab's Tevatron in the search for the Higgs boson. This competitive aspect is real, and probably adds spice to the scientific exploration, but for us such reporting often feels like spilling the entire pepper shaker over a fine meal.

- CheapUniverses.com is now selling a Universe Splitter iPhone app for $1.99, complementing its other products. At $3.95, the Basic Universe:

> Using quantum physics, we split your universe into two branches, then we send you an email to inform you which branch you're in.

As celebrity endorser, they have Garrett Lisi explaining:

> The functioning of this app is in complete agreement with the many-worlds interpretation of quantum mechanics.

- The author is always the last to know such things, but I've heard rumors that someone intends to bring out a Czech edition of Not Even Wrong.
- High quality videos of talks from the Princeton IAS summer school on supersymmetry are available here.
- In Langlands-related news, there's an excellent new preprint by David Nadler about the fundamental lemma and Ngo's proof. This is one of the most ferociously difficult topics to understand in current math research, and Nadler's article is about the best expository piece on the subject that I've seen.

This semester there's a program on Langlands Duality in Representation Theory and Gauge Theory at Hebrew University.

There's a fascinating recent preprint by Kevin Buzzard and Toby Gee on The conjectural connections between automorphic representations and Galois representations. They conjecture a reciprocity sort of relation between algebraic automorphic representations and Galois representations, not just for GL(n), but for arbitrary reductive groups. This involves invoking a twist by "half the sum of the positive roots", a phenomenon that arises in various places in representation theory, often indictating that spinors are involved ("half the sum of the positive roots" is the highest weight of the spinor representation).

Comments

1. **Sven Johnston**
   September 17, 2010

   Will Lubos Motl be translating the Czech edition of Not Even Wrong? And if he does so, can we order the iphone universe splitter so that we can live in a universe where Garrett Lisis translates it?

   Feynman, Einstein, Newton, Galileo, Planck–how awesome for them that they do not have to live through our era which consisted of cashing out and corroding the legacy they created. And how sad for us.

2. **Peter Woit**
   September 17, 2010

   Sven,

   I'm not sure how many joke multiverse iphone apps at $1.99 one can sell, even with Garrett's endorsement. My suspicion is that all he gets for this is the amusement value, but if he's getting paid, it's probably less than the amount I'm getting for the Czech rights to NEW. In another universe this might be more than enough to pay for a nice dinner out in New York, but not in this one…

   So, at least as far as the topics of this posting go, I don't think there's a lot of cashing out or corroding of legacies going on. The news from CERN and Fermilab reflects a likely exciting time ahead for experimental results and research mathematics is a quite healthy subject.

3. **Sven Johnston**
   September 17, 2010

   ^^^^
   "The news from CERN and Fermilab reflects a *likely* exciting time ahead for experimental results and research mathematics is a quite healthy subject."

   This kindof sums of the *likely* sentiments of the last 40 years of zero progress in physics, save for new tv shows and iphone apps for our amusement. lzozlzllz

4. **neo**
   September 17, 2010

   About the universe splitter. I already know about the branch I am in. I want to know about the branches I'm not in.

5. **Theorist**

September 17, 2010

I would argue that not running the Tevatron is a no-brainer. They don't have the resources to do anything BUT a Higgs search. It's a waste of money to run the machine just to do one measurement.

6. **physicsphile**
   September 18, 2010

   Theorist,

   Well its a very important measurement. I think they should do it. What if the LHC has some faults and never works?

7. **Paolo**
   September 19, 2010

   About the "Universe splitter", just wanted to report that by *any* criteria, even the most relaxed ones, it's a robery, in the sense that it does *not* connect to any remote source of randomness: modulo all the "funny" wording, I expected it to remotely connect to a source using an hardware device like http://www.idquantique.com/true-random-number-generator/products-overview.html but in fact it doesn't access the web *at all*.

8. **Noname**
   September 19, 2010

   You keep insisting on how slow progress is with the LHC. Well, the increase in luminosity over the past months has been exponential, with a factor 10 of improvement per month (on average). Obviously this will not continue forever, but it's difficult to find an adjective more inappropriate than "slow" to describe their impressive progress.

9. **Peter Woit**
   September 19, 2010

   Noname,

   I don't think "slow progress" is an inaccurate description of the situation, if you look globally (compare where things are to projections put out early this year), or locally (what has been happening the past few weeks).

   I also don't think there's anything at all wrong with this, it's about what you expect for this kind of project, even when things are going well (and has been the case ever since the machine was first proposed). The important point is that progress is being made and everything indicates that sooner or later the machine will work as designed. That's great.

   However it's also true that those deciding how long to keep the Tevatron running might be paying close attention both to history and the current situation and making their own projections about what luminosity the LHC will achieve over the next couple years.

10. **Peter Woit**
    September 19, 2010

    Noname,

    I checked a bit to see if I was remembering things correctly, and found documents like this from early this year

    http://lhc-commissioning.web.cern.ch/lhc-commissioning/luminosity/MG_2010_parameters.pdf

    which states

    "Making reasonable operational assumptions regarding
    fill length, luminosity lifetime and machine availability,
    the total integrated luminosity for the year would be about
    hundred inverse picobarns."

    The integrated luminosity is now at 3-4 inverse picobarns, with six weeks to go.

11. **Amos**
    September 20, 2010

    Paolo: How do you know Universe Splitter isn't touching the web? It seems to, by which I mean that it takes more or less time to run depending on the quality of my internet questions.

    Assuming that it does do what it says (and that the many-worlds interpretation is correct) its an excellent solution to a lot of thorny dating problems.

12. **Buba**
    September 21, 2010

    3 sigma is very weak – it is not enough to constitute a discovery..

13. **Eric Daniels**
    September 22, 2010

    @Paolo: The Universe Splitter actually *does* contact the quantum device you mentioned. How do I know this? Because I wrote the code. In fact, I even purchased a backup machine in case the one in Geneva breaks down… much to my wife's dismay.

    If you're at all curious, but don't want to see me make a dime, you can have fun with the online, public version at cheapuniverses.com (it says it'll cost you $3.95, but it won't; it's free until I decide otherwise, which will probably be never).

    Enjoy!

14. **Joe Kriek**
    September 30, 2010

As someone involved in science education, I can only conclude that Hawking is doing science a service by stating, to put it very crudely, that the universe is self-creating, self-organizing, and self-explaining. If this message can reach millions of Americans, it's no bad thing.

The alternative is that millions of Americans with no science background will continue to believe that an unknowable, untestable intelligence outside the universe is responsible for the entire show.

This has nothing to do with how seriously M-theory should be taken within the science community, but it's important that the public understand there is no need to insert the supernatural into science.

All of this is obvious to those in the field, but to the millions whose exposure to science is via tv, pop science books, etc, Hawking is doing us a favor when, for sales reasons, he adds a dash of no-God spice to his book.

Science is not only about what can be tested, or what is likely to be testable in the future, it's also the ideas that scientists have while at work at scientists. As such, I can buy M-theory as more useful and helpful than theology.

15. **Peter Woit**
    September 30, 2010

    Joe,

    I don't think that a scientist "stating" something is or should be convincing of anything to anyone. The whole point of science is that we can (or should be able to) back up our statements with evidence. The danger of what Hawking is doing, making statements that he backs up with a bogus argument he has no evidence for, is that it will encourage people to believe that the knowledge claims of science have no different status than those of religion. You see this very clearly in the debate in England, where theologians just have to point out that there is no evidence for M-theory to win the argument.

16. **CT**
    September 30, 2010

    Joe,

    Both religion (Christianity) and any self-creation theory require faith to believe. A big difference here is that the former is open and clear about the requirement (see, for example, Hebrews 11:6 in the Bible), whereas the latter pretends to be a scientific fact, with no evidence to back it up, as Peter said.

The Shape of Inner Space

September 19, 2010
Categories: Book Reviews

Besides the Hawking book, which was a disappointment in many ways, I recently also finished reading a much better and more interesting book which deals with some of the same topics, but in a dramatically more substantive and intelligent manner. [The Shape of Inner Space] is a joint effort of geometer Shing-Tung Yau and science writer Steve Nadis. Yau is one of the great figures in modern geometry, a Fields medalist and current chair of the Harvard math department. He has been responsible for training many of the best young geometers working today, as well as encouraging a wide range of joint efforts between mathematicians and physicists in various areas of string theory and geometry.

Yau begins with his remarkable personal story, starting out with a childhood of difficult circumstances in Hong Kong. He gives a wonderful description of the new world that opened up to him when he came to the US as a graduate student in Berkeley, where he joyfully immersed himself in the library and a wide range of courses. Particularly influential for his later career was a course by Charles Morrey on non-linear PDEs, which he describes as losing all of its students except for him, many off to protest the bombing of Cambodia.

He then goes on to tell some of the story of his early career, culminating for him in his proof of the Calabi conjecture. This conjecture basically says that if a compact Kahler manifold has vanishing first Chern class (a topological condition), then it carries a unique Kahler metric satisfying the condition of vanishing Ricci curvature. It's a kind of uniformisation theorem, saying that these manifolds come with a "best" metric. Such manifolds are now called "Calabi-Yau manifolds", and while the ones of interest in string theory unification have six dimensions, they exist in all even dimensions, in some sense generalizing the special case of an elliptic curve (torus) among two-dimensional surfaces.

Much of the early part of the book is concerned not directly with physics, but with explaining the significance of the mathematical subject known as "geometric analysis". Besides the Calabi conjecture, Yau also explains some of the other highlights of the subject, which include the positive-mass conjecture in general relativity, Donaldson and Seiberg-Witten theory, and the relatively recent proof of the Poincare conjecture. Some readers may find parts of this heavy-going, since Yau is ambitiously trying to explain some quite difficult mathematics (for instance, trying to explain what a Kahler manifold is). Having tried to do some of this kind of thing in my own book, I'm very sympathetic to how difficult it is, but also very much in favor of authors giving it a try. One may end up with a few sections of a book that only a small fraction of its intended audience can really appreciate, but that's not necessarily a bad thing, and arguably much better than having content-free books that don't even try to explain to a non-expert audience what a subject is really about.

A lot of the book is oriented towards explaining a speculative idea that I'm on record as describing as a failure. This is the idea that string theory in ten-dimensions can give one a viable unified theory, by compactification of six of its dimensions. When you do this and look for a compact six-dimensional manifold that will preserve N=1 supersymmetry, what you find yourself looking for is a Calabi-Yau manifold. Undoubtedly one reason for Yau's enthusiasm for this idea is his personal history and having his name attached to these. Unlike other authors though, Yau goes into the question in depth, explaining many of the subtleties of the subject, as well as outlining some of the serious problems with the idea.

I've written elsewhere that string theory has had a huge positive effect on mathematics, and one source of this is the array of questions and new ideas about Calabi-Yau manifolds that it has led to. Yau describes a lot of this in detail, including the beginnings of what has become an important new idea in mathematics, that of mirror symmetry, as well as speculation ("Reid's fantasy") relating the all-too-large number of classes of Calabi-Yaus. He also explains something that he has been working on recently, pursuing an idea that goes back to Strominger in the eighties of looking at an even larger class of possible compactifications that involve non-Kahler examples. One fundamental problem for string theorists is that of too many Calabi-Yaus already, so they're not necessarily enthusiastic about hearing about more possibilities:

> University of Pennsylvania physicist Burt Ovrut, who's trying to realize the Standard Model through Calabi-Yau compactifications, has said he's not ready to take the "radical step" of working on non-Kahler manifolds, about which our mathematical knowledge is presently quite thin: "That will entail a gigantic leap into the unknown, because we don't understand what these alternative configurations really are."

Even in the simpler case of Calabi-Yaus, a fundamental problem is that these manifolds don't have a lot of symmetry that can be exploited. As a result, while Yau's theorem says a Ricci-flat metric exists, one doesn't have an explicit description of the metric. If one wants to get beyond calculations of crude features of the physics coming out of such compactifications (such as the number of generations), one needs to be able to do things like calculate integrals over the Calabi-Yau and this requires knowing the metric. Yau explains this problem, and how it has hung up any hopes of calculating things like fermion masses in these models. He gives a general summary of the low-level of success that this program has so far achieved, and quotes various string theorists on the subject:

> But there is considerable debate regarding how close these groups have actually come to the Standard Model… Physicists I've heard from are of mixed opinion on this subject, and I'm not yet sold on this work or, frankly, on any of the attempts to realize the Standard Model to date. Michael Douglas… agrees: "All of these models are kind of first cuts; no one has yet satisfied all the consistency checks of the real world"…
>
> So far, no one has been able to work out the coupling constants or mass…. Not every physicist considers that goal achievable, and Ovrut admits that "the devil is in the details. We have to compute the Yukawa couplings and the masses, and that could turn out completely wrong."

Yau explains the whole landscape story and the heated debate about it, for instance quoting Burt Richter about landscape-ologists (he says they have "given up.. Since that is what they believe, I can't understand why they don't take up something else – macrame for example.") He describes the landscape as a "speculative subject" about which he's glad to, as a mathematician, not have to take a position:

> It's fair to say that things have gotten a little heated. I haven't really participated in this debate, which may be one of the luxuries of being a mathematician. I don't have to get torn up about the stuff that threatens to tear up the physics community. Instead, I get to sit on the sidelines and ask my usual sorts of questions – how can mathematicians shed light on this situation?

So, while I'm still of the opinion that much of this book is describing a failed project, on the whole it does so in an intellectually serious and honest way, so that anyone who reads it is likely to learn something and to get a reasonable, if perhaps overly optimistic summary of what is going on in the subject. Only at a few points do I think the book goes a bit too far, largely in two chapters near the end. One of these purports to cover the possible fate of the universe ("the fate of the false vacuum") and the book wouldn't lose anything by dropping it. The next chapter deals with string cosmology, a subject that's hard to say much positive about without going over the edge into hype.

Towards the end of the book, Yau makes a point that I very much agree with: fundamental physics may get (or have already gotten..) to the point where it can no longer rely upon frequent inspiration from unexpected experimental results, and when that happens one avenue left to try is to get inspiration from mathematics:

> So that's where we stand today, with various leads being chased down – only a handful of which have been discussed here – and no sensational results yet. Looking ahead, Shamit Kachru, for one, is hopeful that the range of experiments under way, planned, or yet to be devised will afford many opportunities to see new things. Nevertheless, he admits that a less rosy scenario is always possible, in the even that we live in a frustrating universe that affords little, if anything in the way of empirical clues…
>
> What we do next, after coming up empty-handed in every avenue we set out, will be an even bigger test than looking for gravitational waves in the CMB or infinitesimal twists in torsion-balance measurements. For that would be a test of our intellectual mettle. When that happens, when every idea goes south and every

road leads to a dead end, you either give up or try to think of another question you can ask – questions for which there might be some answers.

Edward Witten, who, if anything, tends to be conservative in his pronouncements, is optimistic in the long run, feeling that string theory is too good not to be true. Though, in the short run, he admits, it's going to be difficult to know exactly where we stand. "To test string theory, we will probably have to be lucky," he says. That might sound like a slender thread upon which to pin one's dreams for a theory of everything – almost as slender as a cosmic string itself. But fortunately, says Witten, "in physics there are many ways of being lucky."

I have no quarrel with that statement and more often than not, tend to agree with Witten, as I've generally found this to be a wise policy. But if the physicists find their luck running dry, they might want to turn to their mathematical colleagues, who have enjoyed their fair share of that commodity as well.

Update: I should have mentioned that the book has a web-site here, and there's a very good interview with Yau at Discover that covers many of the topics of the book.

Update: There's more about the book and an interview with Yau here.

Comments

1. **Anon2**
September 20, 2010

Minor typo corrections:

"…Particularly influential for his later career was a course by Charles Morrey on non-linear PDEs, which he describes losing all of **it** students except for him…" {**its**}

"…anyone who reads it is likely to learn something and to get a reasonable, if perhaps overly optimistic summary of what **it** going on in the subject. …" {**is**}

"… Looking ahead, Shamit Kachru, for one, is **hopefuls** that the range of experiments under way …" {**hopeful**}

Just a comment about the claim Yau makes (which you agree with) that mathematics can provide inspiration when experimental data runs out:

"But if the physicists find their luck running dry, they might want to turn to their mathematical colleagues, who have enjoyed their fair share of that commodity as well." – Yau

To the naive public, the problems in string theory are mathematical. Einstein added a time dimension to Euclidean space account for gravity, Kaluza and Klein added a spatial dimension for electromagnetism, and string theory adds further spatial dimensions to build in the rest of particle physics. Then the mathematics is so complex due to the need to hide the extra dimensions in a Planck scale sized Calabi-Yau manifold, it becomes an uncheckable and ugly mathematical guess. Similarly, Lisi's E8 Lie algebra or older SU(5) GUT ideas have apparently come from mathematicians, and don't really lead anywhere.

In what way should physicists turn to mathematicians?

2. **Peter Woit**
September 20, 2010

Anon2,

Thanks, typos fixed.

The problems with string theory are not mathematical, but physical. Adding an extra dimension is a physical idea. Yes, the mathematics string theorists end up using is not only sophisticated, but quite complicated and rather ugly. This is because they're pursuing a wrong physical idea.

My own ideas about where inspiration can be found in mathematics are more in representation theory than in geometry these days (although the two subjects are closely related). I've written about them here often.

Please though, comments should be about Yau's book, generic "math sucks, no it doesn't" arguments are tedious and will be deleted.

3. **PM**
September 20, 2010

Peter: "Towards the end of the book, Yau makes a point that I very much agree with: fundamental physics may get (or have already gotten..) to the point where it can no longer rely upon frequent inspiration from unexpected experimental results"

It always puzzles me when I hear that it's the lack of experimental data that is the problem in physics. There is plenty left to explain about the data we already have: masses and coupling constants – still unexplained, structure of nuclei – still unexplained, origin of symmetries – still unexplained, dark matter and dark energy – still unexplained, and so on…

As far as I see it the lack of experimental data is not the main problem, sure more data would make things easier but it's not like we have nothing left to explain.

I would say current problems are mostly:

1. Cultural
First many people manage to convince themselves that there are no explanations to be found – QM is complete and final. Take the decay of unstable nuclei for example it is thought to be acausal and random – an idea which I find absurd, but if you are OK with it you won't look for a better explanation even though there may very well be one just waiting to be found.

This is often striking in retrospect when fairly straightforward generalizations or concepts took decades simply because people weren't looking for them.

Second the existence of certain candidate theories which cannot be tested makes many simply stop looking for alternatives until these candidates can be confirmed or ruled out, but it may take decades or centuries to do so so we need to keep looking for alternatives.

2. Foundational
I think physics is currently trapped in a deep local minimum so to speak and the road in every direction will be up a steep hill for a while. To me we have simply reached the limits of the current conceptual framework and further progress will require a radical rethinking of the very foundations.

This is problematic however as such alterations to foundations threaten to ruin all the intricate modern physics which rests upon them and since it works so well many feel the foundations should not be altered. But I think it's a necessary step and I expect that once the better foundations are found all the current results will be rederived from them and many more.

Two examples of such radical thinking from the past which I particularly like even though they failed are the geometrodynamics of Wheeler and Dirac attempt to reformulate classical theory without point charges. Those are the kind of imaginative and far reaching ideas which I think are needed to move us forward.

Those were also true unification attempts since they tried to modify two distinct pieces of physics to unify them unlike certain modern approaches which just try to come up with some "glue" to stick them together.

Finally I also like that they tried to improve the classical theory as I think this is where the problems are. I see quantum theory as simply emerging from an underlying classical theory and I believe it is this classical theory has to be improved to see progress..

4. **Peter Woit**
   September 20, 2010

   PM,

   My point is not that there's no experimental data still unexplained, but that for many years now we've had very little new such data. We have hints from experiment about what a better theory should look like, but the problem is that we haven't been getting new hints, so are stuck facing the same intractable problems. Something physicists should be thinking about is how to make progress under these circumstances, but instead the tendency seems to be to just keep doing the same thing, no matter how long goes by without it working.

   But, again, I encourage people to discuss Yau's book, not other unconnected speculative ideas about physics.

5. **Wouarnud**
   September 20, 2010

   Peter,

   Thanks for that. I may actually read this book, since, as you rightly say, it is rare to find a book that tries to explain complex ideas to people who don't have all the necessary math background without using false and crude metaphors (in my case the math was somewhat there too long ago, those particular gray cells have been reallocated to fascinating tasks like management plans and shopping lists).

   I was wondering first if Yau addresses a question I often think about, regarding the part about new directions in physics. One rather glaring experimental fact is that of dark matter and dark energy. As far as I understand, it is lacking from standard models and theoretical explanations of it are, to say the least, incomplete. As far as I've seen as a member of the general public, mathematical theories about these facts are fairly basic, rather non-specific and unimaginative (modified gravity et al). I don't really understand why this isn't the hottest of the hot areas in fundamental physics and mathematics today rather than String theory or any competing alternative. Does Yau mention that in his book?

   As an aside, working in a Space Agency, I once had a scientist making the point to me that since dark energy is 90% of the actual energy in the universe, it should receive 90% of the funding (including of course the mission he was promoting, a far-UV telescope if I remember well). It's hard to argue with that…

   Wouarnud.

6. **Peter Woit**
   September 20, 2010

   Wouarnud,

   Dark matter and dark energy are perhaps the hottest topics in fundamental physics. The problem is that no one has a really good idea about their origin, using geometry or any other argument. I forget whether Yau and Niadis say much about the problem in the book, but if so there's just not much that the study of Calabi-Yaus adds to the question.

7. **Giotis**
   September 20, 2010

   I've read only half of the book and I like it so far. Yau is focusing mainly on the mathematical aspects which was expected in some extend and indeed he makes an ambitious honest attempt to explain these difficult issues. On the other hand he is rather sketchy regarding the physical concepts and String theory itself; at least so far. The autobiographic parts are embedded in a natural way and they serve the overall scope of the book.

   BTW I've noticed that on page 125 he attributes the 10 dimensions of the theory to the Green–Schwarz mechanism although it is a direct consequence of superconformal invariance which is of central importance. He mentions conformal variance on page 152 but he doesn't relate it to the critical dimension.

8. **neo**
   September 20, 2010

   I thought it interesting that Yau, in his Discover interview, says that he ended up proving the Calabi conjecture because he began by thinking it was wrong, and that he had proven it so. Perhaps this is an approach that should be actively adopted more often.

9. **Coin**
   September 20, 2010

   Thanks for this review, this sounds like the kind of string theory book I have been wanting to read. I will try to give this one a shot. (And I'd actually be curious if there are any books which treat AdS/CFT in a similar non-physics-math-person friendly manner!)

   I do have a question, something that has perplexed me for awhile and I think I should try to get a better understanding of before I read Yau's book– just so that I know how to interpret what I'm reading:

   When I look at people talking about string theory, I see two different notions of how space is treated in string theory– how we get from ten or eleven dimensions down to the perceived four. Sometimes people talking about string theory seem to be using one of these notions, sometimes they seem to be using the other, sometimes I can't tell.

   The Calabi-Yau Manifold notion is the first. When string theory is described this way, "the universe" is in fact a ten dimensional manifold, it's just that six of the dimensions are compactified. This manifold isn't embedded in anything else, it stands alone as a background space by itself. Calabi-Yau theory describes this manifold's structure.

The other notion of space I sometimes see is this "M-Theory" picture, where there's an open ten-eleven-ish dimensional "bulk" filled with arbitrary N-dimensional "brane" objects. In this picture "the universe" is described as something "stuck to" (or similar vague phrasing) the outside of one of the branes; the structure of the manifold our universe *seems* to be located in winds up being dictated by how some less-than-ten-dimensional brane restricts our universes's strings.

How do I reconcile these two pictures? Are the Calabi-Yau manifolds of the first picture actually embedded in the "bulk" of the second? Are the manifolds of the first picture somehow dual to specific configurations of branes in the second? Or are these two different incompatible versions of string theory altogether? In short, when I'm reading about how the Calabi-Yau manifolds work, how (if at all) do I connect that to things people say about M-Theory-alikes?

10. **Robert**
September 21, 2010

Hi Peter,

a small correction:

by Yau's solution of the Calabi conjecture there exists a unique Ricci-flat Kahler metric in each Kahler class.

If you don't fix the Kaehler class (and the complex structure), the uniqueness statement is obviously wrong, e.g. if you take a Ricci-flat Kahler metric and rescale it you get another one.

11. **Chris Austin**
September 21, 2010

Coin,

In brane compactifications of the 5 ten-dimensional superstring theories, the 6-dimensional Calabi-Yau space is the bulk, and the brane, called a Dirichlet brane or D-brane, is the Cartesian product of the 3 space dimensions we live in, and a closed surface embedded in a topologically non-trivial fashion in the Calabi-Yau. For example, if the closed surface is a 1-dimensional closed loop, embedded in the Calabi-Yau so that it can't be contracted to a point without breaking it, then the brane is a 4-brane, meaning it has 4 spatial dimensions. Open strings are associated with matter fields and often have both ends attached to the brane, but free to move along it, while closed strings are associated with gravitational fields and are free to move through the bulk.

M-theory usually refers either to the strong coupling limit of type IIA string theory, which is d = 11 supergravity compactified on a circle from 11 to 10 dimensions, or the strong coupling limit of the E8 x E8 heterotic string, which is d = 11 supergravity compactified to 10 dimensions on a finite 1-dimensional interval, with a d = 10 supersymmetric E8 Yang-Mills multiplet on the 10 dimensional spacetime at each end of the interval. This last version is also known as Horava-Witten theory, and the 9 space dimensions at each end of the Horava-Witten interval are sometimes regarded as 9-branes, with the Cartesian product of the interior of the Horava-Witten interval, and the 9 space dimensions perpendicular to it, constituting the bulk.

Burt Ovrut, who Yau quotes twice in the extracts above, works on Horava-Witten theory, with 6 of the 9 space dimensions perpendicular to the Horava-Witten interval compactified on a 6-dimensional Calabi-Yau space. We live on the boundary at one end of the Horava-Witten interval, and the remaining 3 space dimensions perpendicular to the Horava-Witten interval are the 3 extended space dimensions that we see.

12. **Coin**
September 21, 2010

Chris Austin: Oh dear. Thank you for your helpful post, I am still a little confused. I think this is where I am getting stuck. You say: "In brane compactifications of the 5 ten-dimensional superstring theories, the 6-dimensional Calabi-Yau space is the bulk". Should one expect the six dimensions of the Calabi-Yau space in this case to be compactified?

13. **Chris Austin**
September 22, 2010

I should more accurately have said that the the bulk is the Cartesian product of the 3 extended space dimensions and the 6-dimensional Calabi-Yau space, and the brane is the Cartesian product of the 3 extended space dimensions and a closed surface embedded in a topologically non-trivial fashion in the Calabi-Yau space.

The six dimensions of the Calabi-Yau space are always compactified, but depending on the model, they can be as small as the Planck length around 10^{-35} metres, or as large as around 10^{-14} metres. In the latter case the closed surface embedded in a topologically non-trivial fashion in the Calabi-Yau space can be no larger than around 10^{-19} metres, so the Calabi-Yau space has to be able to support topologically non-trivial structures much smaller than itself. Calabi-Yau spaces with this property are sometimes called "swiss cheese" Calabi-Yaus.

The latter case might result in superstrings and/or quantum gravitational effects eventually being observed at the LHC, but I don't know whether Yau discusses this in the book.

14. **Nono**
September 22, 2010

Oh boy, Chris! Now I know what people mean when they talk about Rube Goldberg constructions. And you didn't even mention fluxes…

15. **Chris W.**
September 22, 2010

Someday this work will be looked upon—by physicists—in more or less the same way that some of the ideas in Kelvin's Baltimore Lectures are looked upon now. (Its mathematical interest is beside the point.)

16. **Geoff**
October 4, 2010

Hi Peter –

Yau mentions a general relativity class at Berkeley that had a profound influence on him. Would that have been R.K. Sachs' class?

Tevatron Funding

September 21, 2010
Categories: Experimental HEP News

The Fermilab web-site today has a message from Director Oddone about prospects for funding an extension of the Tevatron run after FY2011, as recommended by the Physics Advisory Committee. He has asked the DOE for additional funding of $35 million/year to pay part of the cost of an extended run, with the rest to come out of slowing down other planned experiments. If the DOE turns this down, it seems the plan is to shutdown the Tevatron next year.

This leaves prospects for the Tevatron's future very much up in the air, especially given the dysfunctional nature of the US federal budget process. With FY2011 about to begin, the Congress has yet to pass a budget, shows no signs of doing so anytime soon, and is in the middle of an election campaign dominated by calls for cutting federal spending. The general assumption is that they'll deal with this by continuing funding at FY2010 levels, until finally getting around to passing appropriations bills all together as part of an "omnibus" bill sometime deep into the fiscal year. The process of dealing with the FY2012 budget starts next February with the President's budget request, but again there's no reason to believe there actually will be a budget until long after they've already started spending the money. Luckily, the Fermilab people by now have many years of experience dealing with this system.

Comments

1. **Yatima**
   September 21, 2010

   I thought there was another Keynesian Stimulus Package of 80 billion in the pipe? Gentlemen, pick up those phones to the Imperial Capital!

2. **Kea**
   September 21, 2010

   So, you're speechless about the new CMS post SM result?

3. **Peter Woit**
   September 21, 2010

   Kea,

   Presumably you mean this:

   http://cms.web.cern.ch/cms/News/2010/QCD-10-002/QCD-10-002.pdf

   I'd be interested to hear from an expert about this, but it is not at all clear that, even if this is something new, it's inconsistent with the SM (the behavior of QCD in processes like this is very complicated and sometimes not well understood).

   Oh, just saw something about this from someone expert in the subject, see here

   http://blogs.uslhc.us/exciting-new-results-from-cms

4. **Kea**
   September 21, 2010

   Oh, so that would be the expert who says: *…but I think it's safe to say this is the first surprising result from the LHC, something that changes our paradigm.*

5. **Peter Woit**
   September 21, 2010

   I see that Slashdot is on the case, with

   LHC Spies Hints of Infant Universe

   http://science.slashdot.org/story/10/09/21/2013225/LHC-Spies-Hints-of-Infant-Universe

6. **M Brane**
   September 21, 2010

   There's a saying in europe:" nothing improves a woman's appearance faster than a man's " , and since the appearance of the rather masculine LHC , I have to say that the tevatron has certainly being trying her hardest to get noticed.

   However, there's only so much you can shorten your hemline , and the point comes where that's not enough to stop the person who's paying your rent from looking at a newer model.

   But there is also another saying in europe : "failure is the fuel of success", and as such the LHC has much to thank the tevatron for.

   Bon chance, tevatron. Bon chance mon ami.

7. **Theorist**
   September 22, 2010

   Peter

   Another expert opinion:

   Pythia isn't very good at QCD. There are lots of things we know it doesn't do correctly (anything having to do with heavy flavor, for starters). In this case, Pythia only includes tree level color correlations, which could be a problem (it seems like this is an issue with interjet radiation). On top of that, there could be issues with the color string model at LHC energies.

This isn't BSM physics.

On the topic of the post, unless Tevatron can demonstrate it has the manpower to do something other than look for the Higgs, it shouldn't get any more funding.

8. **Roger**
   September 22, 2010

   I agree with theorist. As an experimentalist who uses Pythia a lot to study final states, this type of result doesn't surprise me.

   Pythia is one of our most useful tools since it provides an excellent description of most final states. However, it isn't perfect and wouldn't necessarily be expected to describe the tails of distributions well.

   No Nobel prize will ever be gained by showing that Pythia doesn't work.

9. **Tommaso**
   September 23, 2010

   Hi Peter, all,

   yes, we cannot claim that what CMS sees is new physics just because there is a difference with Monte Carlo simulations. The CMS observation is interesting because it shows an effect not seen with lower multiplicities and lower energies, but this may well be the color coherence of partons radiating in the direction along which the color strings stretch. At high multiplicity the effect should in fact be more easily discernible.

   I have discussed the topic in a post here: http://www.science20.com/quantum_diaries_survivor/new_physics_cms_huhm

   Cheers,
   T.

LHC Update

September 24, 2010
Categories: Experimental HEP News

There's been great progress made recently at the LHC, with successful commissioning of "trains" of bunches, allowing significantly higher collision rates. Last night's fill produced an integrated luminosity of .684 inverse picobarns (or 684 inverse nanobarns), which can be compared to the total integrated luminosity up until this week of about 3.5 inverse picobarn. The highest instantaneous luminosity reached is now at about 1/5 the goal set for this year, with a further increase in number of bunches planned for this weekend. For more details, there's a message from the CERN DG here.

Update: The latest fill, with 104 bunches, recently started, with an initial luminosity now at 1/3 of the goal for this year.

Update: For more information about latest events at the LHC and upcoming plans, see here. Latest luminosity plots are here, now including highest instantaneous luminosity.

Comments

1. **hasti**
September 24, 2010

Thanks for this informative post. Could you please clarify how this '.684 picobarn' of integrated luminosity has been estimated? I'm asking because the luminosity decreases since a collision run started. If you could make a rough estimation to get this number, I'd be thankful. Or if you take this number from the official page, I'd be glad to see the links.
Hasti

2. **PhilG**
September 24, 2010

hasti, instantaneous and integrated luminosity during runs can be monitored on this page: http://op-webtools.web.cern.ch/op-webtools/vistar/vistars.php?usr=LHCLUMINOSITY

3. **lun**
September 25, 2010

You are forgetting the observation of long-range correlations in rapidity observed in p-p collisions
http://arxiv.org/abs/1009.4122
They are bound to challenge quite a few people's ideas, as these kind of correlations are associated with "collective" phenomena.

Of course, its also an experimental verification of string theory 🙂

4. **gast**
September 25, 2010

hi lun,

I`ve read somewhere that the fragmentation of some " Unknown Elongated Object" could have led to the correlations seen (an UEO)
🙂 …

But You should not une such s-words here :-)…

5. **Peter Woit**
September 25, 2010

Oops, there was an "inverse" missing in original posting, fixed.

lun,

For commentary on the CMS result, see comments of previous posting. The exciting thing about the latest progress at the LHC is the promise they'll be able to say something new about something other than QCD…

6. **lun**
September 26, 2010

Thanks, I missed those comments.

I have something to add: One important thing a lot of commenters are missing is that these structures were already seen at RHIC, in A-A collisions. They are generically understood as being due to two things:
a) "hotspots" in nuclei creating "strings" (see, string theory 🙂) which stretch longitudinally across the event. These create correlations in rapidity
b) Collective flow (the fluid flows "outwards") which focuses these correlations in angle.

p-p, however, throws a somewhat big wrench in this consensus:
Are hotspots have to be smaller than the proton size (ie, Lambda_{QCD})? Is there flow in proton-proton collisions? (If so, why is it missing in particle spectra?)
Or are we misunderstanding the origin of these structures in heavy ions too?

Honestly, I think this is physics at its best: Pesky experimental data throwing an established theoretical consensus in disarray.
Its a shame in areas of particle physics "other than QCD", such events are rare to non-existant.

7. **Tommaso**
September 26, 2010

The latest fill mentioned in your update has produced 1.06/pb of data, with initial luminosity of 3.5E31. I think it is worth noting that given these numbers, the LHC is the main factory of top quarks and Higgs bosons by now (as shown here: http://www.science20.com/quantum_diaries_survivor/lhc_surpasses_tevatron_top_and_higgs_factory).

Cheers,
T.

The End of Time

September 26, 2010
Categories: Multiverse Mania

I've been critical of multiverse pseudo-science because it doesn't make any testable predictions, but it seems that tonight there really is one. According to this new preprint, multiverse arguments guarantee that time will end, with the expected amount of time left before the end about 5 billion years and

> There is a 50% chance that time will end within the next 3.3 billion years.

The argument seems to be that multiverse arguments require introducing an artificial cut-off to get finite numbers, so the cut-off must be there and we're going to hit it relatively soon on cosmological time scales. The age of the universe is about 13.75 billion years, but we're getting near the end, already entering late middle-age to senior-citizen time-frame. One interpretation given of this result is that:

> we are being simulated by an advanced civilization with a large but finite amount of resources, and at some point the simulation will stop.

It turns out that you don't even need the whole apparatus of eternal inflation to see that time is going to end. All you need to do is to think about sleeping and waking up, which, according to the paper, leads to the "Guth-Vanchurin" paradox:

> Suppose that before you go to sleep someone flips a fair coin and, depending on the result, sets an alarm clock to awaken you after either a short time or a long time. Local physics dictates that there is a 50% probability to sleep for a short time since the coin is fair. Now suppose you have just woken up and have no information about how long you slept. It is natural to consider yourself a typical person waking up. But if we look at everyone who wakes up before the cutoff, we find that there are far more people who wake up after a short nap than a long one. Therefore, upon waking, it seems that there is no longer a 50% probability to have slept for a short time.
>
> How can the probabilities have changed? If you accept that the end of time is a real event that could happen to you, the change in odds is not surprising: although the coin is fair, some people who are put to sleep for a long time never wake up because they run into the end of time first. So upon waking up and discovering that the world has not ended, it is more likely that you have slept for a short time. You have obtained additional information upon waking – the information that time has not stopped – and that changes the probabilities.
>
> However, if you refuse to believe that time can end, there is a contradiction. The odds cannot change unless you obtain additional information. But if all sleepers wake, then the fact that you woke up does not supply you with new information.

Update: Lubos doesn't think much of the paper:

> But holy crap, if physicists don't lose all of their scientific credit by publishing this pure garbage and nothing else for years, can they lose their credibility at all? Does the institutionalized science have any checks and balances left? I think that all the people are being bullied into not criticizing the junk written by other people who are employees of the academic system, especially if the latter are politically correct activists. And be sure, some of the authors of this nonsense are at the top of it.
>
> This is just bad. I urge all the sane people in Berkeley and other places to make it very clear to Bousso et al. – and to students and other colleagues – that they have gone completely crazy.

Update: In other pseudo-science news, the latest Scientific American features a piece by Hawking and Mlodinow based on their recent book.

Update: Not only does New Scientist think this nonsense deserves to be covered in a lead article, they also have an editorial urging us not to "roll your eyes" about this.

Comments

1. **Arun**
   September 26, 2010

   The cutoff is a computational device – I don't see any physical reason for a discontinuity between the region within the cutoff and the region outside the cutoff (figure 1). All that appears to be happening is that no matter how I construct an ensemble of observers in the eternally inflating universe using a geometrical cutoff, a non-zero fraction of observers will hit the boundary. I don't see how that is interpreted as time coming to an end. In particular, I can have a universe in which every observer sees 1 o'clock and 2 o'clock, but there is no way for me to select a geometrical cutoff that contains only observers that see both events, i.e., a vanishingly small fraction of observers who see only one o'clock. If I believe the paper, then my cutoff actually truncates the multiverse, which is a god-like power to ascribe to physicists.

2. **Kea**
   September 26, 2010

   Um, and the arxiv bans papers on Koide formulae for neutrino masses? What universe am I in?

3. **Adam Helfer**
   September 26, 2010

   > But thought's the slave of life, and life time's fool;
   >
   > And time, that takes survey of all the world,
   >
   > Must have a stop.
   >
   > — Henry IV, Part 1, Act 5, Scence 4
   >
   > (Hotspur's dying speech)

4. **Christian Takacs**
   September 27, 2010

   Ok, I'm going to try very hard to be polite, and take this statement seriously: "There is a 50% chance that time will end within the next 3.3 billion years."
   Why is this prediction testible? Isn't there a 100% chance that anyone who takes this bet will not live long enough to find out if the prediction is accurate thus negating the

meaningful concept of making a prediction?

5. **slw**
September 27, 2010

If we are a simulation, we have no way of knowing what kind of laws of physics rule the universe of the beings that simulate us. It might very well be that in their universe conservation of energy does not hold, so they have in fact infinite resources for running the simulation. Perhaps the whole simulation is done to figure out how an intelligent species would adapt to a universe where resources are limited.

6. **lolphysicist**
September 27, 2010

When these people teach, what do the students gain? Does anyone have first-hand information, I wonder.

7. **Mitchell Porter**
September 27, 2010

This is an easy paper to mock, and I guess deservedly so.

Section 5.1 contains a clue to the paper they should have written. It lists assumptions, one of which must be incorrect, for the conclusion that "time ends" to be false. The second one is "Probabilities in an infinite universe are defined by a geometric cutoff".

So the paper they should have written would have been "Probabilities in eternal inflation can't be defined by a geometric cutoff", and this whole business about "time ending" would have been a one-sentence reductio ad absurdum of a particular line of argument.

8. **Nex**
September 27, 2010

Amazing how far one can drift away from rationality when one is surrounded by equally deluded peers.

The sloppiness of thinking in the paper is really remarkable, here is an example from the second paragraph:

"If it does occur in Nature, eternal inflation has profound implications. Any type of event that has nonzero probability will happen in□finitely many times, usually in widely separated regions that remain forever outside of causal contact. This undermines the basis for probabilistic predictions of local experiments. If in□finitely many observers throughout the universe win the lottery, on what grounds can one still claim that winning the lottery is unlikely? To be sure, there are also in□finitely many observers who do not win, but in what sense are there more of them? In local experiments such as playing the lottery, we have clear rules for making predictions and testing theories. But if the universe is eternally inflating, we no longer know why these rules work."

Is this for real? The rules work because the lottery is designed that way! For every winner there are thousands of non-winners, the infinite number of lotteries doesn't change a damn thing.

It's like claiming that if eternal inflation is true we no longer know why the probability of rolling 6 on 6 sided dice is 1/6!

And even if we ignore the atrocious lottery example and talk about probability in general the paragraph still makes no sense – we have an empirical way of defining probabilities and it works very well and is completely immune to eternal inflation metaphysics.

As for the main conclusion the fact that eternal inflation requires an ad hoc regulator to make any predictions means that it is a useless pile of philosophizing not that the time will end.

9. **Bugsy**
September 27, 2010

The authors run a serious risk of not being taken seriously. Is the universe inflating infinitely or merely the egos of those involved?

In fact maybe I should refrain from commenting as I admit to not reading more than the first paragraphs; I got stopped early on by the quotation cited by Nex, which t is so clearly complete BS. On the other hand, in infinitely many universes I will apparently "lose the lotto" and continue reading. In infinitely many of these same universes these authors will win jobs from places like Berkeley, Lawrence Labs, and be paid actual salaries…….

I close with a question, which should be testable: if physics articles on the arxiv keep inflating, won't the average information content approach zero exponentially fast???

10. **Bee**
September 27, 2010

So I wake up, and look at the others who've just woken up and those who are still sleeping, and see that it's 50/50. Whether or not "time will end." Don't see what's paradoxical about that.

11. **Chris Oakley**
September 27, 2010

I agree with Lubos. What garbage! The gist of their argument seems to be that because something (the end of the time) CAN happen in a large enough sample it necessarily WILL. MIT and Berkeley, eh? It may not mean the end of time, but it does seem to indicate the end of any standards in theoretical physics research.

12. **Carl Zetie**
September 27, 2010

I see this kind of "reasoning" all over the place from multiverse advocates and it's not just bad science, it's bad logic. Even my five year old son understands that when you get one random pick, "you get what you get and you don't get upset".

The glaring flaw is the assertion that "it's natural to consider yourself a typical person". THIS IS COMPLETE NONSENSE, and this cannot be repeated often enough and loud enough. Basically, we are taking a sample of size one from an allegedly very large population, and I know of no principle in probability that justifies one in drawing *any* conclusion whatsoever from this. (And no, the "Copernican principle" will not do).

In particular you cannot, as multiversians are fond of doing, reason about the population you were drawn from based on one outcome. Consider this analogy: you have a large sack of balls, some black, some white. You draw out one ball and it's white. What can you conclude about the proportion of black and white balls? Answer:

Absolutely nothing. (Well, OK, you can conclude that there was at least one white ball in there…). You can't even conclude that there are many universes, let alone that ours is typical.

13. **Jeff McGowan**
September 27, 2010

OK, I'm sorry, it used to be fun for mathematicians like myself to make fun of physicists, for example I was just at a conference celebrating Scott Wolpert's 60th birthday, and Peter Sarnak made a crack "he's a physicist, so he doesn't have to prove anything" about someone applying percolation theory to questions on nodal domains of cusp forms, and it got a big laugh. But stuff like this just ruins it all, I mean how can you make fun of people who take stuff like this seriously, at this point I'm just embarrassed for you all 🙂

14. **Carl Zetie**
September 27, 2010

Oh no, they didn't repeat the old nonsense about "in an infinite multiverse, everything must happen infinitely many times", did they? I dispatched of that chestnut in the *letters column of New Scientist*, for crying out loud. Could some friendly, patient mathematician explain to these physicists that infinities come in more than one size?

Not to mention the fact that if eternal inflation is driven by bubble universes born from existing ones, it's perfectly reasonable that the bubbles have physical laws not far distant from the parent, and that however often the bubbles are created, they never explore more than a tiny fraction of possible universes. It's even possible that they cycle through a finite set of configurations. All of this is at least as likely as the "everything must happen" nonsense.

15. **S**
September 27, 2010

The logic in this paper is a disaster. This "Guth-Vanchurin" paradox excerpt given here is just begging the question, and so is the assumption that the theory must result in "finite numbers". The logic is: I want finite results, so I will assume that finite results are correct, and then write a paper concluding time is finite.

I'm a liberal arts major. Come on, physicists.

16. **Anon**
September 27, 2010

The 'Guth-Vanchurin paradox' as written up by these people seems like nonsense but there is a very similar real effect in statistical physics based on Levy statistics.

In fact, in the last year I read of an experimental paper in PRL that observes the effect in some quantum dots.

Here is the essence: Imagine a particle (or quantum dot) with two states – glowing and not-glowing. The particle makes stochastic transitions between these two states and the amount of time spent in each of these states comes from two distributions G(\tau) and NG(\tau).

Let, say, G(\tau) have a long tail (like a Levy distribution). That is, there is significant probability of spending very large times in the glowing state. Let's say NG(\tau) does NOT have such a tail.

Then, if you take a population of such dots and observe the total brightness, you'll find that the total intensity actually increases with time even though the properties of each dot are time-invariant. This is because the longer you measure, the more of the tail of G(\tau) you start sampling…

The essence is that when you have a distribution of times like G(\tau), the length of the experiment you sets a growing cutoff on the distribution. The paper in PRL measures this effect in a real system.

My philosophical resolution of the paradox is that it is not possible to verify that a particle has the distribution G(\tau) in the first place without waiting a long time.. or something of that sort — what does it really mean to have a long tailed distribution of times and how would you verify it?

17. **Roger Schlafly**
September 27, 2010

> Christian Takacs says: Why is this prediction testible? Isn't there a 100% chance that anyone who takes this bet will not live long enough to find out if the prediction is accurate thus negating the meaningful concept of making a prediction?

Do you also object to predictions that our Sun will explode into a red giant in a similar time-frame? It is possible for a testable theory to make long-term predictions, so it is not enough to just argue that people will not live that long.

18. **Coin**
September 27, 2010

"How can the probabilities have changed?"

Because you changed the sample population and in doing so biased it arrrrgh

19. **M Brane**
September 27, 2010

Having a theory that relies on the multiverse is like making love to a beautiful married woman.

You wouldn't be doing it if you thought there was a chance you were going to be found out.

20. **Jeff McGowan**
September 27, 2010

The correct phrase in relation to this is "jumped the shark." See http://www.youtube.com/watch?v=MpraJYnbVtE

21. **Nono**
September 27, 2010

Well, although it's not April 1, has anyone considered the possibility that this is just a joke?

22. **Peter Woit**
September 27, 2010

Nono,

The problem with the joke hypothesis is that these authors have written quite a few papers already that aren't that different. If it's all a joke, they're keeping it up for a remarkably long time.

23. **luke**
September 27, 2010

@ Christian Takacs: Well if time ends there's nothing stoping the person 3 billion years in the future from just walking over and telling you right now, right?

24. **Christian Takacs**
September 27, 2010

Mr. Schlafly,
Quite honestly, I do object to long term "testible" predictions being made about anything 3.3 billion years into the future with any level of statistical accuracy worth mentioning. Please consider the very brief period of time humans have been observing the cosmos, and how many times over just the last 100 years the understanding of how this cosmos functions has changed.
To make such long term predictions with so little understanding of how gravity, light, space, and time function in this universe(not to mention as yet uknown variables) on such a large scale is pure hubris.
I believe that any math is still bound by logic; If you can't observe and measure something, you certainly can not make accurate models or predictions .

25. **Kea**
September 27, 2010

The difficulty with many successful so called theoretical physicists today is their complete lack of an education in philosophy and the humanities. They were selected mainly on their ability to mimic the computational skills of their teachers, with no regard for real scientific enquiry, mind boggling egos and a complacency that would astound any previous generation of human being.

How many of them have ever worked in a real laboratory? Or in industry? Who would have thought it was possible to wander so far from genuine scientific prediction. And when the public talk of massive funding cuts, these same peole cry foul for the future of their societies. Good riddance to it.

26. **Peter Woit**
September 27, 2010

Kea,

The weird thing is that many "multiverse" papers like this don't involve any computational skills beyond high-school level math. They consist of pages and pages of natural language text with a few trivial equations interspersed. The problem isn't that the authors are showing off computational technique without addressing philosophical issues. They're often addressing nothing but philosophical issues, but doing so in a naive and pre-scientific style.

27. **Roger Schlafly**
September 27, 2010

5B years will not be enough time for black holes to evaporate from Hawking radiation, but this story claims that the radiation has already been observed in the lab!

28. **Arun**
September 27, 2010

Do you also object to predictions that our Sun will explode into a red giant in a similar time-frame? It is possible for a testable theory to make long-term predictions, so it is not enough to just argue that people will not live that long.

"All men are mortal" is tested by looking at deaths today and not finding any reason that those alive today are somehow exempt from the same processes. Likewise, the theory and observation is that "stars like our sun do whatever" and so the prediction is that our sun will also do whatever.

29. **Awake**
September 27, 2010

> But if we look at everyone who wakes up before the cutoff, we find that there are far more people who wake up after a short nap than a long one.

I am unable to agree with this statement. Can anyone who agrees with this statement please explain why it might be true?

30. **Peter Shor**
September 28, 2010

Can I join the fun and make up a new theory of physics, too? My theory is that quantum mechanics and general relativity are irreconcilably incompatible. This will only become physically relevant at around the time when the first black hole evaporates, around 1060 years from now, at which point the universe will end.

Isn't this a much better theory than the one we're discussing? Eternal inflation is still a speculative idea, while both quantum mechanics and general relativity are extremely well tested experimentally.

But I guess a paper simply pointing out that there are serious problems with the probability measure for eternal inflation wouldn't have gotten anywhere near as much press as this paper did.

31. **Peter Shor**
September 28, 2010

10^60. Sorry

32. **Peter Woit**
September 28, 2010

Peter Shor,

I think you're missing the innovative aspect of this paper (and its similar cousins). In old-style rationalist argumentation, when you show that your hypotheses lead to absurd conclusions, you have to abandon one of more of your hypotheses. The innovation here is to turn a bug into a feature: when your hypotheses lead to an absurd conclusion, you write a paper (and possibly issue a press release) proclaiming that you have made a surprising and counter-intuitive discovery.

As long as you're careful to avoid dealing with anything experimentalists can check, and stick as much as possible to vague words, avoiding precision and equations, there's no danger of running into too-obvious-to-ignore logical contradiction.

33. **neo**
September 28, 2010

Peter Shor:

10^60. Phew. Thank goodness. You had me worried there for a minute.

34. **Jeff McGowan**
September 28, 2010

Well, you know it is more fun to work in a system with inconsistent axioms, since then you can prove ANYTHING 🙂

35. **Giotis**
September 28, 2010

I just want to remind to the people who mock Bousso (not the paper but the actual person) here and elsewhere that he is the author (together with Polchinski) of the celebrated paper "Quantization of Four-form Fluxes and Dynamical Neutralization of the Cosmological Constant".

This paper contains the only explanation the human race has so far for the observed value of the CC.

In any case I think a little respect is in order here.

36. **Chris W.**
September 28, 2010

If that paper hadn't been written by Bousso and Polchinski, I doubt it would be considered much of an explanation.

Of course, as an example of how one "explains" observations of fundamental significance in cosmology and astrophysics using M-theory, I suppose it's fairly typical.

(As for "the only explanation", that is also open to dispute, but never mind that.)

37. **Sol**
September 28, 2010

About it being "testable": ignore the long time period and think about if the prediction was there was a 50% change time will end tomorrow. Assume that's correct. Then half of the time, time keeps on going and we gain no additional information. The other half of the time, time ends — and we no longer exist to gain confirmation the theory was true!

38. **Peter Woit**
September 28, 2010

Giotis,

I'm not mocking Bousso, just the paper…

One common reaction at the time to the Bousso-Polchinski (and later, KKLT and Susskind) string theory anthropic landscape explanation of the CC, was that if you took that seriously, you were giving up on conventional scientific research and this would unleash all sorts of nonsense. There's now significant evidence for how that turned out, including this latest paper.

39. **changcho**
September 28, 2010

I didn't know that they accepted science fiction at the ArXiv – this idea could be turned into a great Star Trek episode…which is about the only (potentially) good thing about it.

40. **Dave Miller**
September 29, 2010

Kea wrote:
> The difficulty with many successful so called theoretical physicists today is their complete lack of an education in philosophy and the humanities. They were selected mainly on their ability to mimic the computational skills of their teachers, with no regard for real scientific enquiry, mind boggling egos and a complacency that would astound any previous generation of human being..

Hmmm…. tried reading any philosophy recently? Just last week, a philosopher on the Web was trying to explain to me how philosophers have proved time travel to be impossible. (No, I'm not shilling for time travel – I just question whether the philosophers have proved that it is impossible).

Kea also wrote:
> And when the public talk of massive funding cuts, these same peole cry foul for the future of their societies. Good riddance to it.

I think there may be a testable prediction there: when the public really finds out about all this, their reactions will be…?

Dave Miller in Sacramento

41. **Kea**
September 29, 2010

Dave, it is agreed that no academic discipline is immune to the disease of the times, but that is no excuse to ignore the discipline entirely!

42. **Shantanu**
September 30, 2010

Peter sorry for the spam but have a look at Andy Strominger's talk on string theory (at Harvard)

see the discussion around 50-55 mt. its a string theory report card
shantanu

43. **Peter Woit**
September 30, 2010

Shantanu,

Interesting looking talk, it's at

http://media.physics.harvard.edu/video/index.php?id=COLLOQ_STROMINGER_091310.flv&width=640&height=360

If I can find time soon to watch it, may write something about it here.

44. **Pawl**
September 30, 2010

And this in from The Economist's blog, commenting on Hawking's scientific credentials:

> …if, as expected, his 1974 theory that black holes emit radiation despite their notorious all-engulfing gravitational pull is confirmed by experiments at the Large Hadron Collider in CERN…

One can't fault Hawking for this bizarre assessmnent of "what is expected," but it is something of an index of how confused the public is.

45. **Dave Miller**
October 2, 2010

Kea wrote to me:
>Dave, it is agreed that no academic discipline is immune to the disease of the times, but that is no excuse to ignore the discipline entirely!

My reply to you disappeared, so I suppose Peter does not want a lengthy discussion on this.

So, I'll simply say that philosophy has been doing for two millennia what the string theorists have been doing for just a few decades.

And, yes, that indeed is a good reason for ignoring their discipline entirely.

46. **chris**
October 5, 2010

ah yes, i have been told in school that physics has two dangerous limits: zoology and philosophy. seems these guys hit one of the limits pretty hard.

47. **mekong**
October 8, 2010

"Hmmm…. tried reading any philosophy recently? Just last week, a philosopher on the Web was trying to explain to me how philosophers have proved time travel to be impossible."

@dave miller

Yes, one philosopher is representative of the entire discipline. How very logical of you to also accept his argument at face value, and then to make further inferential claims about an entire discipline. Especially when said discipline has multiple sub-areas, and sub-sub areas. The philosophy of time itself is split up into multiple conceptual areas [1], had you gone beyond your limited sample space of one "philosopher on the web" (who I'd wager good money wasn't a philosopher at all, though it's up to you to provide proof, please link it if this was an open web conversation …) you might have encountered the different and varied argumentative positions for philosophers of time (many of whom disagree on whether time travel can or cannot exist, and some — mostly of the empirical bent — deny whether it is meaningful to talk of completely).

Sounds like you need to read some more philosophy, namely informal and formal logic.

Notes

(1) And before someone goes "LOLPHILOSOPHERSCAN'TAGREE," let it be known that philosophy is primarily useful insofar as it keeps a philosophical inventory of arguments that are known to be useful, or useless (primarily the latter, especially for metaphysical concepts) and groups them together by category. In the case of philosophy of time, and its sub-areas, there are numerous conceptual schemes and arguments for-and-against different positions. So to say "philosophers as a whole reject time travel" shows an ignorant account of philosophy in general.

NRC Rankings

September 27, 2010
Categories: Uncategorized

One thing I've learned in life is that human beings are creatures very much obsessed with social hierarchy, and academics are even more obsessed with this than most people. So, any public rankings that involve oneself and the institutions one is part of tend to have a lot of influence. In the US, US News and World Report each year ranks the "Best Colleges", see the rankings for National Universities here. My university's administration tended in the past to express skepticism about the value of this ranking, which typically put us tied for 8/9 or 8/9/10. This year however, everyone here agrees that there has been a dramatic improvement in methodology, since we're at number 4.

For most academics though, the real ranking that matters is not that of how good a job one's institution does in training undergraduates, but the ranking of the quality of research in one's academic field. Where one's department fits in this hierarchy is crucial, affecting one's ability to get grants, how good one's students are and whether they can get jobs, even one's salary. The gold standard has been the National Research Council rankings, which were supposed to be revised about every ten years. It turns out though that the task of making these ranking has somehow become far more complex and difficult, with more than fifteen years elapsing since the last rankings in 1995. Since 2005 there has been a large and well-funded project to generate new rankings, with release date that keeps getting pushed back. Finally, last year a 200 page book was released entitled A Guide to the Methodology of the National Research Council Assessment of Doctorate Programs, but still no rankings.

Recently the announcement was made that all will be revealed tomorrow at a press conference to be held in Washington at 1pm EDT. I hear rumors that university administrations have been privately given some of the numbers in advance, to allow the preparation of appropriate press releases (see here for an example of a university web-site devoted to this issue).

The data being used was gathered back in 2005-2006, and the five intervening years of processing mean that it is rather stale, since many departments have gained or lost academic stars and changed a lot during these years. So, no matter what happens, a good excuse for ignoring the results will be at hand.

Update: University administrations have now had the data for a week or so and are providing it to people at their institutions. For an example of this, see the web-site Berkeley set up here. All you need is a login and password….

Update: (Via Dave Bacon) Based on the confidential data provided to them last week, the University of Washington Computer Science and Engineering department has released a statement characterizing this data as having "significant flaws", and noting that:

> The University of Washington reported these issues to NRC when the pre-release data was made available, and asked NRC to make corrections prior to public release. NRC declined to do so. We and others have detected and reported many other anomalies and inaccuracies in the data during the pre-release week.
>
> The widespread availability of the badly flawed pre-release data within the academic community, and NRC's apparent resolve to move forward with the public release of this badly flawed data, have caused us and others to urge caution – hence this statement. Garbage In, Garbage Out – this assessment is based on clearly erroneous data. For our program – and surely for many others – the results are meaningless.

The UW Dean of the College of Engineering has a statement here where he claims that, despite 5 years of massaging, the NRC data contained obvious nonsense, such as the statistic that 0% of their graduating CS Ph.D. students had plans for academic employment during 2001-5.

Update: Boston University has broken the embargo with this press release. They give a chart showing that almost all their graduate programs have dramatically improved their ranking since the 1995 rankings, while noting that the two rankings are based on different criteria, the NRC says you can't compare them, and the 2010 rankings in the chart are not NRC numbers, but are based on their massaging of the data. I suspect that the NRC data will be used to show that, like the kids in Lake Wobegon, all programs are above average.

For more on this story, see coverage by Steinn Sigurdsson at Dynamics of Cats.

Update: The NRC data is out, and available from its web-site. But, no one really cares about that, all they care about are the rankings, and the NRC is not directly putting those out. Instead, they've subcontracted the dirty work to phds.org, where you can get rankings here, using either the "regression-based" or "survey-based" score. In mathematics and physics, the lists you get are about what one would expect, with perhaps somewhat of a tilt towards large state schools compared to the 1995 rankings (most dramatically, Penn State was number 36 in 1995, number 8 or 9 this year).

Comments

1. **X**
   September 28, 2010

   The UW dean appears to be correct. I've been perusing the data set for schools that I have been connected with. There are some flagrant errors that seem to significantly impact the results. For example, they record that the Boston University physics department has a faculty that is 75% female. This gives them a huge bump in the "diversity" rating. In addition, despite having a median time-to-graduation of 6 years, they claim only 9% of physics grad students graduate within 6 years. Basic sanity-checking does not seem to have been applied.

2. **anon**
   September 28, 2010

   I have some doubts about the Penn State ranking, at least in math, where they made a similar jump. I think it's a fine dept, but to jump from 35 to 10th seems dubious to me. Perhaps they're strong in areas outside of my interests, but I wonder what's going on with they're data.

3. **anonymous coward**
   September 28, 2010

   There are some terrible flaws here, and I don't really have enough time to comment on the utter incompetence that these rankings have displayed.

   What I will say is this. This is an extremely confusing and misleading ranking, and to have ranges instead of a simple ranking has to be one of the dumbest things any ranking system has done. My guess is that they did this so that colleges can brag about how high they are possibly perceived to be. This is a ploy to make NRC a more popular ranking, but it won't happen, since no one's going to through all the trouble and confusion of phds.org to get a half-assed confusing-as-hell ranking system. US News, unfortunately, will become the standard for graduate school rankings. In a way, it already has.

   Also, there are serious data problems. anon above me states that Penn State jumped from 35th to 10th in math. Chicago went from 5th to 12-39 with a survey ranking of 27-57. This is despite the fact that Chicago is within the mathematics world reputed as top 5 in the United States and in the last year, just recruited a Fields medalist (which are quite rare within US institutions). Something must have gone seriously wrong with data collection.

Anyway, that is all I can say. It is just very disappointing that this was what I was waiting for for the last five years…

4. **JSE**
September 29, 2010

There's some good drill-down into the math numbers going on at my blog In particular, one commenter points out that all of Chicago's many postdocs were counted as faculty, while none of (e.g.) Yale's were. As you can imagine, this plays havoc with measures like "proportion of faculty holding external grants."

5. **Peter Woit**
September 29, 2010

Thanks JSE (fixed link),

The way faculty are counted seems to be one of the main weaknesses of this survey. They had some very elaborate counting scheme, involving "core", "associated", "allocated" faculty, and each institution had to figure out separately how to reconcile their own way of counting faculty with what the NRC wanted. The definition of "core" involved things like whether you were on phd committees, etc. It seems that different institutions made different choices about how to count, which in some cases benefited them immensely, in others hurt them a lot.

6. **rrtucci**
September 29, 2010

So what is the job and yearly budget of the National Research Communism agency. The wikipedia article on it is very short on details.

7. **Peter Woit**
September 29, 2010

rrtucci,

I don't know about the NRC in general, but this particular project is described as costing "well over $4 million", and I think that's the cost of the NRC people's effort. The cost to universities to get this data together, analyze it, and put out press releases might be an order of magnitude more than that.

8. **ObsessiveMathsFreak**
September 30, 2010

Looks like if you want a higher ranking from the NRC, you're going to have to grease a few palms. I suppose that technically a bribe or two to the odd council official could be marked under "research expenditure" since promoting the university's profile/ faculty salaries is all research is about nowadays.

These rankings are all nonsense anyway. The London Times brought out one of these lists recently and it's essentially an Anglo-Saxon Academy Award ceremony—and every bit as insular. The US and especially the UK were vastly overrepresented, as were most anglophone countries.

The UK had ~26 universities, while France has only 4, despite the fact that both have similar population sizes (~60 million) and development levels. Germany (~80 million) has about 14. Ireland (pop ~4 million) had 2. There were no Russian(~150 million) universities in the top 200—or Indian ones (~1.3 billion).

These rankings are bogus to begin with. Their only purpose is to satisfy the deep need for our society to reduce entire institutions and their collective efforts down to a single number, which can be compared to other numbers to produce a league table. It's a pathological disorder of the modern world (I blame the press) to seek such numbers regardless of whether they hold any real meaning(They are generally meaningless).

The effect of all of this is debilitating to education everywhere it is applied. Who is the "best"? Whoever has the higher "number". How good is an institution? Check its "number" on the table next to all the other numbers. Forget the other factors; there's a number. This is like publication counts/citations, only applied to entire universities and schools.

This is why I'm quite serious when I say that universities should allocate money to bribe the officials making these numbers. Better that than play the corrupting game of juking the stats; rushing to meet meaningless targets to up the number(s), fudging figures and cutting corners all the way, and likely wrecking the university in the process.. If your aim is to turn your institution into an elaborate machine for producing a higher number, then you might even succeed; but your institution will likely fail in the process.

On a deeper note, what does it say about our society if we base our collective decisions on numbers which mean nothing? Price Indices, league tables, stats of all kinds. Numbers without meaning(or mathematics), which emerge from calculations which make no sense, based on data which has been manipulated. And then we feed them all back into the systems which run our lives. All rather dystopian.

9. **anon**
September 30, 2010

Dear ObsessiveMathsFreak — Academics love of rankings (as much as many of us, such as myself, try to deny it) is, in my opinion, partially built from our own early success in school. More or less, everything you said could be applied to a test score achieved in fourth grade. My own realization that I _might_ be good at math started when I got a 51/52 in some test on fractions. What does that 51/52 really mean? Some different test might have score me 15/52. It's so arbitrary and meaningless etc etc.

However, what I do remember is my teacher's happy reaction to my performance, and that ultimately made a difference to me. I went off to do well in school (meaning, more meaningless A's), as do many of our peers. It's hard to shut off caring about grades after years of benefiting from them!

10. **rrtucci**
September 30, 2010

A guy named Jules posted the following very interesting comment about this issue in Scott A-Ronzoni's blog:

Deans and department chairs want to kill off any ranking system, because they do not want prospective graduate students to get this data. I hope that they are not successful now, because you can be sure they won't put anything in its place.

For example, UW does not report any of the NRC-requested data on its website. If you email them for it, they will not respond. Now they complain that their faculty count is wrong when they themselves inflated the count. Cry me a river.

As to MIT, I don't know what "found academic employment" means precisely. What counts is how many PhDs found tenure-track positions. I doubt this is 50%.

11. **Place Holder**
October 1, 2010

@rrtucci: Scott was talking about the MIT EECS department, in terms of “found tenure track jobs”. Tenure track jobs were much easier to find in EECS as opposed to math and physics.

12. **International**
October 3, 2010

Look at the tables of honors and awards recognized for adding points! The NRC claim that “starting in fall of 2005, NRC staff compiled lists of international, national and disciplinary awards in all the taxonomic fields …” so how come in more than 3 years they failed to find the academie des sciences in Paris, the Royal Society of London, and other leading and internationally respected sources of honors – in Cambridge MA alone there are 28 Fellows and Foreign Members of the Royal Society, and those associated with graduate programs are overlooked. Yet the Maple Leaf Farms Duck Research Award is included as Prestigious – doubltess meritorious on some scale, but in all fairness not at the level of the academie des sciences of Paris or the Royal Society of London, or many other awards completely overlooked by the NRC. I thought there was something about not disciminating against people based on nationality and national origin here in the US, but perhaps the NRC thinks it’s above the law as well as courtesy and thoroughness.

13. **Andrew L.**
October 3, 2010

I really don’t think I can improve on ObsessiveMathsFreak’s commentary-except to add I’ve found in my travels the reality of the quality of the top schools is often far removed in reality due to many factors:Not the least of which is the archaic practice of placement of wealthy “legacy students” in top programs who can barely read.This takes a very limited spot from deserving students of lower pedigree. It’s nothing new at prestigious universities,of course-but it’s certainly new in the sciences. I think this is one of the great unspoken factors undermining scientific training in this country.

14. **Peter Shor**
October 3, 2010

To Andrew L.

I don’t know for sure about other graduate departments, but I do know that the MIT Math Department does not admit any legacy students (and I strongly suspect that most other graduate programs do not). Undergraduate admissions is another matter.

As for the ranking, all I can say is that there are a number of instances where School A is ranked well above School B, and where I would advise most graduate students who had this choice to attend School B.

15. **Eli Rabett**
October 3, 2010

What obviously happened is that some departments figured out how to game the data entry and others ignored them (it was a lot of work).

16. **former chair**
October 5, 2010

It wasn’t always at the department level…the data collection was administered at the university level.

17. **Andy P.**
October 8, 2010

@Andrew L : I’ve seen you make this claim in other forums (eg on mathoverflow), and I have to call BS. I’ve either been a student at or been employed by several “top” departments, and I’ve got close friends at several others. I’ve never encountered any students like you describe.

Simply put, I think you don’t know what you’re talking about.

18. **Peter Woit**
October 8, 2010

Andy P.,

Agreed. I’ve been associated with my share of “prestigious” universities and have seen few if any incompetent rich kids in math and science classes. The main effect of legacy admissions (and it is of significant size) is to give a big help to people who are part of the large group with similar qualifications around the cut-off for admission. The number of those with much inferior qualifications getting in is small; for this I believe you need to be not only a legacy, but from a family with significant financial or political power. And in those few cases, the students involved tend to major in something other than math or science.

And this only applies to undergraduates. From what I’ve seen graduate admissions pretty much completely ignores considerations of legacy status or wealth.

19. **Anonymous**
October 8, 2010

Just one more comment … I have seen (very rarely) incompetent students admitted to graduate programs, but in at least one case this was because in their home country, they had a family with significant power, and this let them obtain incredible recommendation letters, excellent grades, and very good scores on standardized tests.

Grading String Theory

September 30, 2010
Categories: Uncategorized

Commenter Shantanu points to this video of a recent colloquium by Andy Strominger at Harvard, which includes some extensive comments on the current state of string theory. Strominger is one of the most prominent string theorists in the business, and has been working in the field for more than a quarter-century since the first "Superstring Revolution" of 1984. In the talk (at about the 52 minute point), Strominger gives a "report card" for string theory, where he assigns it 3 As, 2 Bs, 3 Ds and 2 Fs, for an average grade of about C. It gets an F for making no unambiguous testable predictions, a D for prospects of saying anything about LHC, an F for the CC (Strominger isn't sold on the anthropic landscape) and a D for cosmology. Some of the high grades are debatable, with an audience member pointing out to him that there was a tension between his A for "Not being ruled out as theory of nature" and F for no testable predictions. Strominger repeatedly claimed that most string theorists would agree with him on these grades (except maybe the F for the CC).

As an overall evaluation, he said that it was debatable whether this was a passing or failing report card, then arguing:

> But this is the only student in class, so if you flunk her you have to shut the school down.

Along the same lines, a bit earlier in the talk one of his slides characterizes all that theorists can do as "go home and watch TV" if they believe in the landscape ("String theory is everything") or if they think string theory is a failure ("String theory is nothing"). The positive argument he was trying to make is that there still is something for string theorists to do even after they are forced to give up on particle physics: they can try applying AdS/CFT and black holes to other areas of physics (nuclear physics, solid state physics, fluid mechanics).

I think Strominger is right that his grades and point of view about string theory are now conventional wisdom among leading theorists. What I find striking about this is the argument that if you are forced to give up on string theory, you have to "shut the school down" or "go home and watch TV". More than 25 years of working on string theory has left Strominger and others somehow believing that there is no conceivable alternative. The failure of string theory as a theory of particle physics leads them to the conclusion that they must not abandon string theory, but instead must abandon particle physics and try and apply string theory to other fields. The obvious conclusion that string theory is just one speculative idea, and that its failure just means you have to try others, is one that they still do not seem willing to face up to.

Comments

1. **Kea**
   September 30, 2010

   Yes, it is fascinating to see people dictating to Nature the impossibility of alternatives, merely on the assumption that nobody else could possibly do better than they have. I feel that Nature would rather we were somewhat more productive than TV couch potatoes, since TV watching uses less brain power than sleeping. Well, Nature has a simple cure for this kind of behaviour, othewise known as Extinction.

2. **Anonymous**
   September 30, 2010

   So what's the alternative theory that gets better grades on these issues?
   By the way, abandoning particle physics is not really a bad idea, if you accept the premise that a field of inquiry should only receive resources proportional to the amount of new experimental evidence arising in this field.

3. **hsearles**
   September 30, 2010

   Reminds me of the Karl Popper quote: "Whenever a theory appears to you as the only possible one, take this as a sign that you have neither understood the theory nor the problem which it was intended to solve."

4. **Peter Woit**
   September 30, 2010

   Anonymous,

   You could also argue that fields should get resources inversely proportional to amount of new experimental evidence, on the grounds that if there are lots of new experimental clues, any unfunded idiot will be able to make progress. It's only when there are few experimental clues that the work is hard, so should get more resources.

   Also, by your criterion, mathematical physics should never be funded at all.

   The argument that "sure, string theory is a failed idea, but since we don't have better ones, we're going to keep doing it anyway" has been the main thing keeping string theory research afloat for a very long time now. It wasn't a good argument 15 years ago, and with the landscape, etc., it's not getting any better.

5. **Roger Schlafly**
   September 30, 2010

   The alternative theory with better grades is the Standard Model.

6. **Yatima**
   September 30, 2010

   That's quite an interesting little talk but it's hard to figure what the linkage of black hole theory or string theory to solid state physics, fluid dynamics and whatnot is all about. It's also difficult to ascertain what the author classifies as belonging to string theory and what not. A colored diagram with the mathematical bridges linking the main idea blocks and equations would be useful.

7. **flunkemall**
   September 30, 2010

   The Standard Model is the baseline. The challenge is to find a successful extension. That determines the grades.

8. **Dave Miller**
September 30, 2010

Peter,

What do you think of the view that string theory is simply a framework for theories, but that you need some additional ideas to narrow it down to get a specific theory? It's an analogy to QFT: i.e., no one criticizes QFT on the grounds that thee are an infinite number of QFTs. We just accept QFT as a framework that, fortunately, includes one specific example that works extremely well — the Standard Model.

Perhaps even string theory enthusiasts should accept the fact that the community just needs to "sleep on it" for a few years to come up with some novel ideas. After all, QFT goes back all the way to 1930 or so, but it wasn't until the '60s that the Standard Model was actually invented.

Dave Miller in Sacramento

9. **neo**
September 30, 2010

I, for one, do not think physicists and mathematicians should give up on string theory. They should stop the hype and stop blocking the consideration of alternatives, but not give up. One should wonder though, absent evidence, when you go down a path that gets ever more convoluted, whether you are on the right path. At least the QFT folks were led into a labyrinthic theory by the existence of hard facts.

10. **DB**
October 1, 2010

"The obvious conclusion that string theory is just one speculative idea, and that its failure just means you have to try others, is one that they still do not seem willing to face up to. "

Indeed it is. But Strominger is 55 years old and therefore completely passed it in terms of making fundamental new contributions to theoretical physics. So for him to abandon String Theory is to acknowledge that his whole research career has been a failure. Since String theory has been going for thirty years most of his colleagues are in the same boat. Psychologically he, and his fellow workers can't face up to this. And practically speaking, it would mean the end of their mentoring role – the only meaningful role they have left. And this, more than any other factor, explains their bizarre committment to a failed theory.

11. **Jose**
October 1, 2010

"You could also argue that fields should get resources inversely proportional to amount of new experimental evidence…"

…in which case string theory should get infinite funding.

12. **Fabien Besnard**
October 1, 2010

I know that this is not "a place for people to promote their favorite ideas about fundamental physics", but surely the names "loop quantum gravity", on the quantum gravity side, and "noncommutative geometry", on the unification side, must have come to the ears of Strominger and other promoters of the TINA argument. I would understand if they said "there is no good alternative", since this would clearly be the expression of an opinion, but choosing instead not to even mention the other theories surely qualifies as propaganda.

13. **M Brane**
October 1, 2010

In many ways, string theorists are like wives and string theory itself is like the husband that beats them.

"But I love him"
or
"I can't leave him, because where would I go?"
or
"He only does those things because he's incomplete, and I can FIX him"
or
"I'm too old to start seeing someone new"

However in some violent relationships, the surprising happens: The husband leaves. So it may end up that string theory itself may leave the theorists and not the other way round.

Indeed since 1998 , when string theory said "I'm just going to the shop to get some CC-igaretes" , the theorists have been waiting for it to come back by keeping it's dinner warm in the landscape oven.

14. **Seth Thatcher**
October 1, 2010

M brane:

I think the abandoned wife of string theory has lately been internet dating "Multiverse" in "String theory's" absence to pick up CC-igarettes. "Multiverse" sounds good and is quite interesting, but he is elusive and refuses to meet her at the local Starbucks for a face to face. She's never seen him and can't verify who he is or that she he even really exists, but she's hopeful.

Seth

15. **Peter Woit**
October 1, 2010

Dave,

If you're going claim to just be a "framework", the question becomes whether you're a useful framework, i.e, whether you're able to provide a testable scientific explanation of anything. Claiming that "God did it" is also a framework, just not a very useful one.

The QFT framework historically used by physicists is not "pick any random, arbitrarily complicated QFT and try and use it. What is really powerful is the idea: pick one of a small number of the simplest QFTs and compare these to the world. This is what physicists have been doing since the earliest days of quantum theory, with great success in terms of getting a scientific explanation this way. The simplest string theories are just dead-wrong, so you're forced to look at more and more complicated ones, evading confrontation with experiment.

16. **Roger Schlafly**
October 1, 2010

So it appears that Strominger, Woit, Motl, and everyone else are in broad agreement about what is string theory, and what it has and has not accomplished. Motl even agrees with Strominger's report card. Controversy only arises when someone argues that a failed theory is still worth pursuing if all the experts say that it is the only game in town.

17. **Smurf**
October 1, 2010

DB

…therefore completely passed it in terms of making fundamental new contributions to theoretical physics…

Hidden conformal symmetry of the Kerr black hole (2010), Kerr/CFT correspondence (2008), Chiral Gravity Conjecture & its proof (2008), OSV conjecture (2004) & its proof (2006), 4D/5D black hole connection (2005),… Enough for you?

18. **JC**
October 1, 2010

At this point, I doubt people like Witten, Strominger, etc … are going to publicly renounce string theory. Nobody wants to admit their entire research career was all in vain.

From a more pragmatic perspective, they don't want to "nuke" the careers of their younger colleagues, previous students, and postdocs, whom are still climbing up the academic system (ie. looking for an assistant professor job, on tenure track, etc …). They also don't want to give the funding agencies any more excuses to reduce or outright cancel funding for string theory.

With that being said, the earliest I can think for somebody like a Witten or Strominger to renounce string theory, would be after they are retired and no longer have their offices at the university, IAS, etc …

For a die hard fanatical string theorist, they probably will not renounce string theory at all. Though if they do renounce, for whatever reasons (ie. seeing the light, etc …), it will most likely be when they are on their deadbeds.

19. **Dave Miller**
October 1, 2010

Peter wrote to me:
> The QFT framework historically used by physicists is not "pick any random, arbitrarily complicated QFT and try and use it. What is really powerful is the idea: pick one of a small number of the simplest QFTs and compare these to the world. This is what physicists have been doing since the earliest days of quantum theory, with great success in terms of getting a scientific explanation this way. The simplest string theories are just dead-wrong, so you're forced to look at more and more complicated ones, evading confrontation with experiment.

Yeah. I think what probably will happen, and probably should happen, is that people will gradually drift away from string theory, simply because no one really knows what to do with it. However, it seems to me quite possible that, forty years from now, some bright young kid will be working on LQG or non-commutative geometry or whatever the hot thing is then, and will see that it connects with string theory in a cool way, and that this was the missing idea that kept string theory from being useful.

Stranger things have happened: it took a long time after Riemann and Gauss worked out the initial ideas in differential geometry until Einstein saw how to use those ideas in general relativity. Of course, GR has an indefinite metric, as suggested by special relativity, and this gives a structure not imagined by Riemann or Gauss. It seems to me that something like that may happen someday in string theory: some old ideas banging around from string theory may mate with some entirely new ideas from some other source and produce some viable offspring.

I don't think I'm really arguing with you here: I don't think you've ever suggested that we should simply burn all the books and journals with string theory stuff in them to prevent future generations from being polluted with string theory! Obviously, string theory has not worked, but I do not think it is crazy to suspect that some ideas from string theory may eventually make it into real elementary-particle physics. At any rate, I doubt it is possible to hide the stuff that has been worked out in string theory from future generations of theorists, even if they find it of limited interest.

The other point I'd like to raise is basic math issues about string theory: I'm still bothered by the different quantization procedures (Gupta-Bleuler-like quantization, light-cone gauge, etc.), by the question of a true string field theory, by whether or not "M theory" actually exists, and, indeed, by the question of what string theory actually looks like in spacetime (the jump from 2-D quantization in the sigma-tau plane to actual 9+1 or 25+ 1 or whatever-dimensional spacetime is slid over pretty slickly in everything I have seen).

You recall Streater and Wightman's "PCT, Spin and Statistics, and All That"? There was nothing about real-world field theories there (certainly not gauge theories), just an attempt to figure out what sort of critters field theories are.

It seems to me that there are a few decades (at least!) of similar work in string theory, and, to my tastes, that might be worthwhile. Most of us don't pooh-pooh the work put into proving that phi^4 theory is trivial in 3+1 dimensions: it did not teach us anything about the real world, but it did teach us something about field theories.

Similarly, if someone could actually present "M-theory" in a well-defined way and show that it exists mathematically, I'd find that interesting as work in mathematical physics, even if it never connects with the real world.

Alas, string theory does not seem to attract too many people with the personalities of Streater and Wightman.

Dave

20. **Tom Whicker**
October 2, 2010

Chiral Gravity has been proved? Does this mean that Uncle Al has been vindicated???

21. **Anonymous**
October 2, 2010

Similarly, if someone could actually present "M-theory" in a well-defined way and show that it exists mathematically, I'd find that interesting as work in mathematical physics, even if it never connects with the real world.

By AdS/CFT, this is probably at least as hard as showing the existence of Super Yang-Mills theory. What a hopelessly difficult challenge.

22. **Dave Miller**
October 2, 2010

Anonymous wrote, re my earlier post:
>By AdS/CFT, this [showing M-theory exists] is probably at least as hard as showing the existence of Super Yang-Mills theory. What a hopelessly difficult challenge.

I should have been clearer with my Streater/Wightman example. I don't expect anyone to work out string theory with the level of math rigor that Streater/Wightman used.

But, it would be nice to have *some* demonstration that there is such a thing as M-theory at the same level of understanding we all used to accept for field theory – some single Lagrangian or *something* that really is a single presentation of M-theory. As far as I can tell, there have been rather wild jumps to possible phenomenology, brane-worlds, the landscape, and all that, without even knowing if M-theory exists in terms of the relaxed standards of rigor physicists usually accept (or used to accept).

Similarly, "first-quantized" string theory seems actually to be a classical field theory of strings (just as Klein-Gordon theory actually is a classical field theory that needs quantization). However, if you try to work out the details based on the standard approaches – light-scale "quantization," Gupta-Bleuler-like imposition of the constraints, etc. – well, I have not seen that done, and it looks to me to be fairly difficult.

I suspect that some string theorists would claim that this is obvious, and that I am just too dumb to see it.

Maybe so. In which case, surely one of them could have mercy on us poor dumb theorists and write it out for us at the same level as any of the old classic field-theory textbooks.

I don't think they can. I have most of the standard string-theory texts – GSW, Polchinski, etc. It's just not there.

If string theory is real, even at the level of mathematical physics, it should be possible to work this stuff out and write it up. Trying to do so might clarify a lot of what is going on.

I'm an agnostic on this: I don't know if the basic ideas of string theory make sense or not.

But, it would be nice if someone were actually trying to work this out.

Dave

23. **anonymous**
October 2, 2010

What grades does he claim are A's and B's?

There's a huge difference between LHC and cosmology.
LHC physics, while very important, is at an energy scale (TeV) for which string theory
may well have little to say. In very early universe cosmology there is a greater possibility for connection with data, since the subject depends on high energy physics (Planck-suppressed operators). In this subject, there are testable signatures of some well-defined theories; moreover the theory has motivated
new, more systematic studies of field theory and CMB signatures
which are central in that field. The speaker has not worked on this much, and may not have thought carefully about it.

It is surprising that he would claim to speak for others on this; I would guess the grades would vary wildly.

24. **Peter Woit**
October 2, 2010

anonymous,

Strominger's grading of string cosmology with a D I think accurately reflects both its lack of success and the general opinion of the subject of cosmologists, string theorists and others. The only disagreement among most physicists would be whether a D or an F is the right grade. Trying to claim that he doesn't know what he's talking about is just absurd.

JC,

I think giving a public colloquium in which you award your own subject Ds and Fs for its progress in saying anything about particle physics and describe the future of this subject as its adherents having to go home and watch TV is about as close to admitting failure as you are ever going to get. What's at issue now is what you do after admitting this failure. Strominger is arguing that theorists should leave particle physics and try applying techniques they have learned from string theory to other subjects. I'd be happier if he and others argued that people should be looking for new techniques.

25. **DB**
October 2, 2010

JC

"From a more pragmatic perspective, they don't want to "nuke" the careers of their younger colleagues, previous students, and postdocs, whom are still climbing up the academic system"

I think it's more insidious that this. The reason, I believe, for the relentless incantation of the mantra "this is the only game in town" is a cynical pr-style attempt to influence young talent into taking up String Theory. I think this is the only rational explanation for their behaviour. The old burnt-out hands, who now sit in very influential positions, know that the only real chance for progress is to attract the young talent that might be capable of making the breakthrough that they have failed to make and are now too old to realistically hope of achieving. The alternative is that their long toil of thirty years of research is consigned to the dustbin of history.

Putting myself in their place, I can see why they are driven to relentlessly spin and hype their field. If by some miracle a young genius was to sort out what appear to be completely intractable problems with the field, the shameful history of hype and spin would soon be forgiven and they would be celebrated as pioneering visionaries.

@smurf

Your filter as to what constitutes “fundamental new contributions to theoretical physics” appears to be set at a considerably lower threshold to mine. Historically, really important new breakthroughs by theoretical physicists over the age of 45 are extremely rare.

26. **piscator**
October 2, 2010

Not sure that anonymous really appreciates particle physics. The inflationary scales are no more Planckian than the TeV scales.

In large classes of models (all versions of gravity mediation), *all* the soft terms – i.e. the terms in the Lagrangian that in the best of all possible worlds the LHC will measure – arise as Planck-suppressed operators.

In both susy and cosmology, understanding the physics requires understanding Planck-suppressed operators. How well defined the models are is another question, but if you want to understand the origin and physics of these Planckian operators, there is still only one game in town (sorry Peter).

27. **anonymous**
October 2, 2010

piscator: TeV physics may or may not involve SUSY, which if it exists may or may not involve gravity mediation. Inflationary cosmology necessarily involves Planck-suppressed operators, in the cases protected by symmetry, an infinite class. However I do agree with you that particle physics is very interesting and we could get lucky in terms of connections to string theory. SUSY grew out of string theory as well, as a spinoff.

Peter: actually cosmologists do take this seriously. WMAP papers and various cosmo white papers written by the experts in the field refer at an appropriate level to string theoretic mechanisms and their corresponding signatures, for standard reasons. Moreover, string theory contributed in a clear way to the development of the field theoretic classification of single-field inflation mechanisms.
This is all standard by now in the cosmo community. Unfortunately, it is not absurd to say that string theorists including perhaps Strominger are not all caught up on this, but CMB physicists by and large are.

28. **piscator**
October 2, 2010

anonymous,

OK, we aren’t that far from each other. Personally I feel cosmo people place too much emphasis on string-motivated ideas, and pheno people too little, but we can agree to differ.

Inflation is in some sense twice as hard as susy breaking though, because for inflation you need controlled susy breaking to start with, and then controlled inflationary dynamics on top of that.

29. **careful**
October 2, 2010

Wow. The one thing I would think all theorists would have learned by the age of 55, is to never claim to speak for a whole community.

I know a lot of string theorists. I have even been called a string theorist on occasion. I am not sure what “grades” I would give this huge framework (which in my opinion has contributed a lot to theoretical physics, but is not yet in anything close to a finished form). But I am pretty darned sure that if you took a poll of string theorists, you would get a nice, diverse statistical distribution of answers about where the theory has done best/is most promising, and where it has been most deficient (or what areas are most overgrazed to little continuing benefit).

I suspect Strominger, for instance, might emphasize calculations of highly idealized “black hole” entropies (counts of field theory BPS states) as a great success, where others might instead be happiest with models of cosmology (which promise eventual contact of a more physical nature), or particle physics, or geometric engineering of various known and previously unknown field theory results, or even relations to pure mathematics. I am not advocating any of these views, but it is clear from the arXiv that many people view each as a prime motivation to work in this area.

This is as it should be; since there has been no smoking gun success at the level of making contact with nature, any agreement would be purely sociological, and I’m happy to say that we haven’t reached that level of groupthink yet.

30. **vasisdas**
October 2, 2010

Amusing colloquium but very political, with lots of preferential references to present and former Harvard people. Does he really believe that black holes are the harmonic oscillators of the 21st century? They seem pretty 20th century to me, just like Strominger’s powerpointless slides. I doubt that referencing Subir repeatedly for using black holes to solve CMT will help extend the lifetime of this peculiar fad. Of course, trying to explain the Big Bang is what string theory should be good for. He might not like the progress on this so far, but this is a far more likely arena for eventual string theory contact with real world than LHC energy scales. If LHC discovers some new physics that will be great, but having it distinguish between different BSM scenarios (let alone string constructions) is unlikely. So, grading both as D looks misleading.

31. **Shantanu**
October 2, 2010

Peter, here is another interesting talk by Avi Loeb on advice to young astrophysicists. Although this is more astrophysics based(although string theory and landscape is mentioned), the same method applies
to particle physics also
http://www.cfa.harvard.edu/itc/events/Loeb.Future_Frontiers.mov
Someone should give a similar talk about particle physics

32. **Anonymous**
October 3, 2010

As well as being an accomplished scientist, Loeb has a very open mind. Among other points, he thinks dark matter/dark energy may not eventually be proven correct, and our current understanding of cosmology leading to the theory of galaxy formation may well turn out to be another theory of epicycles.

Ironically, string theorists seem to be absolutely certain that there is a cosmological constant, while astrophysicists, who actually study this issue directly, at least do not view competing theories like quintessence as crackpottery.

33. **lun**
October 3, 2010

Dave Miller's comment is very appropriate given Witten's paper of a few days ago http://arxiv.org/abs/1009.6032

Strictly speaking, it is not on string theory at all, let alone of string theory as a theory of quantum gravity. However, objects which arise naturally in string theory are shown to be somehow equivalent to objects which arise in certain quantum field theories and even quantum mechanics.

Seiberg's dictum, if we find something beyond string theory well call it string theory, is not COMPLETELY unjustified.

34. **Chris W.**
October 4, 2010

The writeup that Avi Loeb refers to in his talk is arXiv:1008.1586.

35. **Geoff**
October 4, 2010

Hi Peter –

My bias is that string theory does too much violence to GR, but there is an interesting problem that makes me wonder if string theory isn't entirely useless from a computational standpoint. If I had been a student of DeWitt and he asked me to calculate graviton-graviton scattering as described here:

http://arxiv.org/abs/0805.2935

it would have taken over 500 terms and I'm quite sure I would have never gotten it right. If someone told me there was a quick way to get the right answer with only one term, I certainly would have used it without regard as to whether the underlying theory solved the unification problem.

36. **Coin**
October 4, 2010

"Among other points, he thinks dark matter/dark energy may not eventually be proven correct"

Hm. Loeb's general philosophy on research is fascinating, but I'm surprised to find people still doubting dark matter. I'd be curious how any alternative to the dark matter hypothesis could possibly be made compatible with the bullet cluster/MACS J0025 observations…

Also:

"SUSY grew out of string theory as well, as a spinoff."

Really?

37. **Peter Woit**
October 4, 2010

Geoff,

Sure, the applications of string theory giving sums of lots of Feynman diagrams in terms of one world-sheet diagram are impressive. It's worth pointing out that this is still a non-trivial calculation (to really connect to GR, you should quantize the 10d superstring and then extract 4d info from it).
Lots of things coming out of string theory are useful, just not the ones coming out of conjectured unification via string theory.

I don't know who Yau's GR instructor was, don't think he says in the book, may be wrong.

Coin,

Dark matter has nothing to do with this posting, which is one of the problems with string theory unification…

I wrote extensively about the history of 4d SUSY in my book. It was first discovered in the Soviet Union, through motivations having nothing to do with string theory. Later it was independently discovered in the West, there string theory was part of the motivation.

38. **np**
October 5, 2010

http://edition.cnn.com/2010/LIVING/10/05/sweden.nobel.physics/

It's NP (Physics) day today. Not string theory by any stretch of the imagination, but I suggest it's time for a new post.

39. **Peter Woit**
October 5, 2010

np,

Sorry, but the Nobel news is about something I know nothing about. Earlier this year I was at a series of talks which was supposed to include Geim, but he didn't make it because of the volcano. So, I remain ignorant and I fear you'll have to find better informed blogs for insight on today's news.

40. **zanzibar**
October 5, 2010

RE: Nobel prize (graphene) and Black Holes (!?)

Interestingly, Motl's blog on the Nobel prize contains material apropos this thread's topic:

//quote
————————————————————————-
Just four days ago, graphene was also mentioned on TRF because it appeared in Andy Strominger's talk – as an example of a physical system for which the string-theoretical AdS/CFT correspondence has been useful.

That's partly because graphene is a nearly perfect fluid – much like the other systems that are well covered by the holographic dual (and reinterpreted as a black hole). This fact is just another example of the tight relationships with string theory and its brand of quantum gravity that the most important state-of-the-art discoveries even in as faraway disciplines as atomic or condensed matter physics display.
————————————————————————-
/endquote

See:

http://motls.blogspot.com/2010/10/physics-nobel-prize-geim-novoselov.html?utm_source=feedburner&utm_medium=feed&utm_campaign=Feed%3A+LuboMotlsReferenceFrame+%28Lubos+Motl%27s+reference+frame%29

(Sorry about the double posting, but how do I quote material via this blog's editing interface?)

41. **ohwilleke**
October 5, 2010

The good news is that there actually more kids in the classroom (LQG and entropic gravity most notably) than there were a decade ago.

42. **MathPhys**
October 9, 2010

I personally liked Strominger's seminar and it is clear to me that he has thought about the issues that he was talking about, deeply and for a very long time.

It is natural that he referred frequently to contributions by Harvard people. The seminar took place at Harvard, the audience were mostly Harvard students and he was being patriotic, so to speak. It is also a fact that he was mostly talking about traditional, non-stringy physics.

His opinions on string theory in the final part of the talk were, well, his opinions and he's entitled to them. What's he expected to say? Someone else's opinions? I think he was very reasonable.

Various and Sundry

October 11, 2010
Categories: Uncategorized

Yet another random collection of topics of possible interest:

- Things have been going well at the LHC recently, and there's a new dashboard page at which progress can be followed. For the latest from the LHC, see the talks at last week's LHC Days in Split. The LHC machine is discussed here, with the news that restart of proton-proton collisions (after a November 1 stop for a heavy-ion run and holiday shutdown) is tentatively scheduled for February 4. Long-term plans for the machine are covered here, including projected running at 6.5 TeV/beam in 2013, 7 TeV/beam in 2014, and a possible rebuilding of the machine with new magnets that would give 16.5 TeV/beam in 2031.

Now that the experiments have 10 inverse picobarns of data, the search for supersymmetry can begin in earnest, and various talks cover this. According to Maria Spiropulu of CMS "the time between O(10) and O(100) inverse picobarns of well-understood data will be critical for the discovery and characterization of SUSY".

The other hot topic is that of how well the LHC will be able to compete with the Tevatron for discovery of the Higgs. Tommaso Dorigo discusses this here and here, using LHC projections given here.

- In news of non-scientific projects of mathematicians and physicists, Edward Frenkel has a screenplay out called The Two-body Problem. Lisa Randall has curated an exhibition in LA entitled Measure for Measure.
- Frank Wilczek is working on a murder mystery novel to be called *The Attraction of Darkness*, which will mix "science, music, sex, and murder." There was a recent Bloggingheads conversation with him here. His response when asked about his take on string theory: "It needs work."
- For commentary from Charles Day, an editor as Physics Today, about why their coverage of string theory has been sparse, see here. For Clifford Johnson's commentary on the commentary, see here.
- Later this month the Princeton Center for Theoretical Science will have a workshop on Rare Events in Computational, Financial and Physical Sciences, co-sponsored by the hedge fund D.E. Shaw. D.E. Shaw has been a large employer of mathematicians and physicists, but recently hasn't been doing so well, announcing the firing of about 10% of their staff.

The new documentary about the financial crisis that just came out, Inside Job, is surprisingly good, I highly recommend it.

- Past proceedings of the International Congress of Mathematicians are now available on-line here.

Comments

1. **Octoploid**
   October 11, 2010

   Thanks for the LHC dashboard link. This is a great
   page with all the relevant information nicely viewable.
   This makes the endless switching between the various
   OP Vistars pages largely obsolete.

2. **jal**
   October 11, 2010

   I hope everyone becomes aware of "INSIDE JOB". You will find out that reality is worst that the film.
   jal

3. **Anonymous**
   October 11, 2010

   For commentary from Charles Day, an editor as Physics Today, about why their coverage of string theory has been sparse, see here.

   I hope editors of Science and Nature also review why their coverage of string theory is sparse. And some official clarification from the Nobel committee would be also very welcome:)

4. **Jim Clarage**
   October 11, 2010

   Thanks for alerting us to the Past proceedings of the International Congress of Mathematicians. Having these papers available from my armchair next to the fireplace is almost science-fiction. The 1893 address by Felix Klein on "The present state of Mathematics" could almost be written today. Klein points out certain problems in astronomy where "even the professional mathematician finds here much to be learned." The problem he's referring to involves infinite determinants ("unendlichen Determinante") discussed in the paper by Burkhardt from that same year — also available online of course. The infinities physicists continue to toss at mathematicians e.g., Feynman diagrams and renormalization, come to mind.

5. **Benni**
   October 12, 2010

   Although it is off topic:

   Roger Penrose has published a new book. It will appear at 26.th of this month:

   http://www.amazon.de/gp/product/0224080369/ref=s9_newr_gw_ir01?pf_rd_m=A3JWKAKR8XB7XF&pf_rd_s=center-4&pf_rd_r=036A8N9W4SYJTTF5MH12&pf_rd_t=101&pf_rd_p=463375133&pf_rd_i=301128

   It is called "cycles of time", with the description saying:

   One of our most distinguished scientists offers a radical new theory of the origin, and ultimate end, of the Universe.

   Professor Sir Roger Penrose's groundbreaking and bestselling The Road to Reality provided a complete guide to the laws that govern our universe. In Cycles of Time, Penrose offers a completely new perspective on the often-asked question, 'what came before the Big Bang?' The answer that Penrose proposes involves a curious but fully rational way of looking at the expected ultimate fate of our acclerating expanded universe, and showing that its end can in fact be reinterpreted as the 'Big Bang' of a new universe.

6. **Benni**
October 12, 2010

The book has already appeared in the uk, therefore, amazon.uk page has reviews:

http://www.amazon.co.uk/Cycles-Time-Extraordinary-View-Universe/dp/0224080369

The good news is, that the book may contain interesting technicalities, since an amazon.co.uk reviewer writes:

"There is some hard maths however and this has been relegated to the Appendix (30 pages). This maths is very advanced and another of Penrose's technical books (Penrose and Rindler Volume 2) would be needed to understand it fully – so that is only for the experts. "

7. **Yatima**
October 13, 2010

>The good news is, that the book may contain interesting technicalities

Hell yes. They are all in the appendix (Penrose uses the word "banished"), but keep your skills in 2-spinor formalism and operator algebra handy.

Sadly, there is no bibliography.

The book is about Penrose's blueprint of an idea on how to connect the low-entropy Big Bang state to the high-entropy "end of times" state of an exponentially expanding universe. The states apparently can be made conformally equivalent assuming rest masses and charges all vanish to 0 as the universe ages.

8. **noname**
October 14, 2010

The LHC continues to make (very slow?) good progress and starts to fullfill its goals for 2010. Hey, make an effort and try to look cheerful when you report on that!

9. **Peter Woit**
October 14, 2010

noname,

You seem to have missed the lead-off sentence here:

"Things have been going well at the LHC recently" which, if I were the sort to editorialize, I'd say was great to hear….

They've reached their peak luminosity goal for the year, with a couple weeks to spare. I hope they can run for physics long enough during the remaining time to have something interesting in the data.

10. **Tommaso**
October 14, 2010

Hi all,

noname is of course reading another book. The LHC is performing surprisingly well, and we experimenters on the receiving end are simply delighted. 10^32 with 240 bunches means we have a much better emittance than predicted, which is great news and is not an ephemeral plus.

With 15/pb already in the bag and probably over 50 before the end of the year, we are with both foots into "discovery territory", if you buy into the theorists' hype that new physics is behind the corner. Sure, it is, but the corner is nowhere to be found.

Best,
T.

11. **Jacek**
October 14, 2010

couple weeks to spare = two weeks

12. **Tommaso**
October 14, 2010

Ah, and… Forgot to write the real reason for my above comment – thanks for the links Peter!

Cheers,
T.

13. **SteveB**
October 15, 2010

Thanks for the info. While the LHC dashboard looks promising it is not working well for me. The screen flashes every second and it seems to clear all but the top 20% of the image and then fills the rest in slowly so I get to see the bottom portion of the display for about 1/3 of second. Also, none of the buttons on the bottom of the dashboard work for me. I am using a fairly common OS and browser(Windows7 and IE8) and have no issues with other web sites. I have not yet checked the dashboard at home with snow leopard and Safari.

14. **noname**
October 15, 2010

I'm not reading a different book. I simply remembered previous posts here in which Peter was very lukewarm
in reporting progress at the LHC (he almost seemed happy thinking the Tevatron would always be on top). It's very difficult to read him totally excited about the LHC, which goes a long way in showing what type of physicist he is.

15. **Peter Woit**
    October 15, 2010

    noname,

    There have been times this year when the LHC was significantly behind its goals and I've reported that. At other times, like now, when it has been reaching its goals, I've also reported that.

    The current situation is that if all goes well, they'll end this year's pp run at about 50 inverse picobarns of integrated luminosity (versus expectations early this year of 100 inverse picobarns, see

    http://indico.cern.ch/getFile.py/access?contribId=26&sessionId=7&resId=2&materialId=paper&confId=76921

    Peak luminosities are now a bit above half the projected final value in the above document, may go higher in the next couple weeks.

    So, I think a fair summary is that things are going well, in some ways better than projections (as Tommaso mentions), in other ways not quite at projected levels. I'll leave overhyping this situation to others.

    Like everyone interested in particle physics, I'm following with great interest the progress of the machine and of the experiments. If there's any difference between my point of view on the LHC and that of certain other people's, it's just that I'm dubious about widely advertised supersymmetry or extra dimensional scenarios, and think the most likely important result to come out of the LHC will be an understanding of the origin of electroweak symmetry breaking. Looking at how difficult it appears to be to see the Higgs, this may take quite a while. If, on the other hand, a dramatic violation of the Standard Model shows up this fall in the first tens of inverse picobarns, that would be fantastic.

    As for what type of physicist I am, unlike "noname", I put my name on everything I write so that people can judge for themselves.

16. **PhilG**
    October 16, 2010

    The LHC was never "significantly behind its goals" this year. The 100/pb was just an estimate, not a goal.

    What has actually happened is that they have adapted their plans to take advantage of better than expected beam stability at high intensity. In the original plan that Peter linked to you can see that they expected to have to go to much higher bunch numbers using 50ns separation and lower beta* of 2m to reach the 100/μb/s target. In fact they were able to do it with less bunches seprated by 150ns and beta* of 3.5m. The good news is that it is relatively easy now to decarease bunch spacing and increase beta* giving a potential luminosity increase factor of about 5 times. They might do that early next year so that if the LHC runs with good efficiency they can reach the 1/fb target more quickly.

    Of course they have had to deal with many problems along the way, but no more than they had allowed time for. There have been no major problems and not a single unplanned quench. This has been a great year for the LHC and we have every reason to expect the next to be even better.

The Gathering Storm: Category 5

October 14, 2010
Categories: Uncategorized

Back in 2005 an illustrious group was organized to produce a report addressing the state of science and technology in the United States, resulting in what became known as the "Gathering Storm" report since it was entitled Rising Above the Gathering Storm. This report recommended that various steps should be taken to increase the number of science Ph.D.s produced in the US and going into the US labor market (while noting that there was no evidence of a shortage of such Ph.D.s).

Last month the group was back, now claiming that the gathering storm has become a hurricane of nearly category 5 intensity, with a new report entitled Rising Above the Gathering Storm, Revisited: Rapidly Approaching Category 5. Yesterday they appeared before the House Science and Technology Committee. In an analysis of what happened to their recommendations, they noted that the call for more Ph.D.s had been effective, with the NSF spending $475 million on graduate student funding during FY 2009-2010. As for the effect of this on their goal of more well-paid jobs for Americans, here's what they had to say:

> A paradox exists in the debate over whether there is a shortage of scientists and engineers or whether there are too many scientists and engineers for the jobs that are available. Most business leaders maintain the former; however, with regard to the more "conventional" functions of these fields it may well be that *de facto* there can no longer be domestic shortages of scientists and engineers. Firms facing this proposition are simply moving work elsewhere. Similarly, the observation that many scientists and engineers elect to pursue careers in other fields is in many instances simply reflective of the value placed on education in these disciplines by business, law, and medical schools and related employers and should not necessarily be decried. However, if the sole purpose of a PhD in science is considered to be to prepare future educators in science, then a surplus of scientists (often evidenced as a surplus of Post-Doctorate researchers) seems inevitable. The Gathering Storm recommendations are based upon the premise that federal investment in research must be doubled (the report's second highest priority recommendation)—in which case there will be commensurate increases in demand for researchers . . . and not solely for the purpose of providing educators.

It seems that the idea is that while there's no Ph.D. shortage at the moment, the Congress will double funding for scientific research over the next few years, so just maybe there could be a shortage in the future and this must be addressed right now.

As for the "paradox" that business leaders see a shortage of the kind of trained scientists and engineers they would like to hire at the wages they would like to pay, it appears to be the same paradoxical shortage I regularly encounter of first-class plane tickets to Paris available at the price I would like to pay for them.

In the real world, the latest Notices of the AMS has data showing the number of mathematics graduate students increasing from 10,883 in fall 2008 to 11,268 last fall. The situation graduating students face is described as:

> The job market for doctoral mathematicians took a decided turn for the worse during the 2008-2009 hiring season. For all mathematics departments combined, the number of full-time positions under recruitment during 2008-2009 for employment beginning in fall 2009 decreased 27%, dropping to 1,464 from 2,012 reported last year. This is smallest number of such positions reported since 1997 when it was 1,246. The number of tenured/tenure-track positions under recruitment during this period was 930, down 23% from the previous year's figure of 1,213. The number of full-time positions filled was 1,274, with 710 of these tenured/tenure-track positions. These figures are down 30% and 27%, respectively, from the figures reported for the 2007-2008 hiring season.
>
> For all mathematics departments combined, the number of new doctoral recipients hired for positions beginning in fall 2009 was down 13% from the previous year's number, to 656. Likewise, there was a decrease in the number of new doctoral recipients obtaining tenure-track positions for fall 2009 with 301 such hirings reported compared to 378 reported for fall 2008.

Comments

1. **Anonymous**
   October 14, 2010

   "As for the "paradox" that business leaders see a shortage of the kind of trained scientists and engineers they would like to hire at the wages they would like to pay…"

   I believe this is exactly correct and the aforementioned business leaders would then proceed to break the airline unions so they can get the price they would like to pay for that Paris trip. There is a shortage of various types of engineers in the US based on demand, but business leaders would rather setup operations overseas to get reduced labor. I have seen the first hand effects of this – unfortunately, it's not easy applying the kind of mind-games that business leaders like to play on scientific personnel when their minions are half-way around the globe and can't be forced to work late in their own time-zone. So they bring the jobs back to the US and secretly pray for the equilibrium to shift in the other direction (towards a surplus of people) by dumping personnel in the market, hoping that it will create the conditions they want. Not being very good at analyzing the dynamics, they don't quite understand why it doesn't work (including why many engineers leave the field altogether, reducing the supply). Maybe we should use NSF funds to properly educate business leaders, including requiring them to take (and pass) the real calculus that the rest of us take in school.

2. **Ralph Ballart**
   October 14, 2010

   Excellent report on just this topic
   http://www.miller-mccune.com/science/the-real-science-gap-16191/

3. **CIP**
   October 14, 2010

   The shortage, I think, is of graduate students profs would like to train for jobs that don't exist.

4. **Anonymous**
   October 14, 2010

   The problem is that since graduate student can be used as cheap labor, there are far more openings for graduate students than for faculty members. When the scientific institutions finish exploiting a graduate student, the student is dumped with little hope of landing a faculty position. He's also in effectively at a disadvantage if he seeks jobs elsewhere, because of the amount of time wasted in graduate school. I can't see how this problem can possibly be resolved in any manner.

5. **anonymous**
   October 15, 2010

   This is something I feel deeply and personally. I'm 27 and just drive as a courier currently because I can actually live well on it. I'd love to pursue a higher education in physics but the debt load and prospects seem crushing. What to do about housing and money, the intensity and deadlines all scare me, not the complexity. I love to learn naturally on my own, for fun, with the feeling of exhiliration and pursuit of deep understanding. Yes work can be a drag, but at least I can pursue art, music, reading, science and FUN in relative freedom.

In other words I'm doomed!

6. **Bugsy**
October 15, 2010

Thanks for the link and comments, Peter; the article linked to by Ralph Ballart is also fascinating. The comments there are also worth reading….
Interesting that on the AIP site and also the cited report site there seems to be no place for comments. Good for them, as a lot of us would no doubt call "BS" on the whole enterprise.

Bad news from across the seas: the Sarkozy regime is making life so unbearable for academics that I predict many (those who can) will try to "get out", though given the situation in the US now that could only affect the very top levels.

Some examples: general low pay, and now apparently those previously privileged CNRS researchers will now have to undergo "evaluations" every 6 months??? with similar hurdles on the way for all academics, the overwhelming emphasis being put on sheer numbers of publications…there are many opportunities in the new system for friends to help friends, in a desperate race for
grant money and hence survival, which will encourage all manner of abuse of power and influence….there is a general threat in the air that those who are a bit older or are "less productive" will be rewarded by a doubled teaching load….social services are being cut…and on and on.
So my prediction, other than many wishing they could leave the country, is an increased suicide rate among those who feel trapped in the present situation, for a multitude of reasons…

7. **gs**
October 15, 2010

As for the "paradox" that business leaders see a shortage of the kind of trained scientists and engineers they would like to hire at the wages they would like to pay, it appears to be the same paradoxical shortage I regularly encounter of first-class plane tickets to Paris available at the price I would like to pay for them.

Exactly. Back in the 1980s when IBMer Erich Bloch headed it, the NSF was stating that a boom in demand was a few years ahead and scientists would be writing their own tickets in the 1990s.

8. **Peter Woit**
October 15, 2010

gs,

Yes, I should have mentioned that there's a long history of these bogus claims about "shortages" of US scientists. I think the NSF one you refer to calculated that by now (2010), the US would be in truly desperate straits, with a shortage of about 700,000 scientists.

9. **Yatima**
October 16, 2010

@Anonymous: "I believe this is exactly correct and the aforementioned business leaders would then proceed to break the airline unions so they can get the price they would like to pay for that Paris trip."

Really now. Whatever happened to economic education? I know it has been in the dumps since at least WWII, but still.

What *would* happen is that aforementioned business leaders would proceed to set up an airline that could offer low, low rates to engineers wanting to get a correct price for plane ticket to Paris.

It's that simple.

What would furthermore happen is that unions would mess the plan up by demanding higher wages or cushy job conditions for employees that are also unions members, then the governement would demand special "green" taxes, income taxes or nonsensical antiterror measures so the airline would go bankrupt and we would be back at square one.

Then capitalists get blamed for "greed".

10. **Peter Woit**
October 16, 2010

Please, no more ideological rants pro or con unions. In the job market at issue here, for better or worse, the number of union members and people interested in trying to form unions is infinitesimally small.

11. **Charlie C**
October 16, 2010

"The job market for doctoral mathematicians took a decided turn for the worse during the 2008-2009 hiring season. For all mathematics departments combined, the number of full-time positions under recruitment during 2008-2009 for employment beginning in fall 2009 decreased 27%, dropping to 1,464 from 2,012 reported last year. This is smallest number of such positions reported since 1997 when it was 1,246. The number of tenured/tenure-track positions under recruitment during this period was 930, down 23% from the previous year's figure of 1,213. The number of full-time positions filled was 1,274, with 710 of these tenured/tenure-track positions. These figures are down 30% and 27%, respectively, from the figures reported for the 2007-2008 hiring season.

For all mathematics departments combined, the number of new doctoral recipients hired for positions beginning in fall 2009 was down 13% from the previous year's number, to 656. Likewise, there was a decrease in the number of new doctoral recipients obtaining tenure-track positions for fall 2009 with 301 such hirings reported compared to 378 reported for fall 2008."

It would be interesting to know what the equivalent numbers are for China. Does anyone have data on that?

12. **Iowa Beauty**
October 17, 2010

The Miller-McCune argument is pretty compelling so long as we continue to imagine that the only successful employment for a STEM doctorate is research and teaching in a world class university. The system is heavily biased toward that belief, but it's deeply unfortunate. Industry needs people both in research and product development that have the knowledge and skills of PhDs. But most new doctorates consider an industry job second class, PhD programs do little or nothing to prepare their graduates for non-faculty positions, and most industry disdains to nurture their employment outside the few genuine industry research labs still operating.

We need a track toward the end of PhD programs that actually prepares graduates for real-world positions – allowing them to specialize their skills in directions that would make them business savvy, or properly geared up for undergraduate education, or for the very most creative, research.

Of course, we also need industry that can see beyond next quarter's results, but I guess that's a topic for another place and time.

13. **David**
October 17, 2010

Doesn't the quoted text including "For all mathematics departments combined, the number of full-time positions under recruitment…."
show exactly what the problem is?

Their idea is that people with a PhD should go into industry, not get jobs in math departments. The math departments can quite obviously produce many more PhDs than the universities need, the over production is for the benefit of industry, and the graudate students should know that a majority of them will not get university jobs. This is already the situation in e.g. Germany.

14. **Anonymous**
October 17, 2010

@David: You're entirely correct that the majority of PhD students should expect to build their future career outside academia. However, unless you know you probably want to stay in academia, doing a PhD is a bad career move (in most case anyway). Is the industry really craving for PhD applicants rather than those with a Master's degree but several more years of work experience? I don't think so. Beside, doing a PhD already means losing several years of salary (Working anywhere else is better paid).

15. **bane**
October 17, 2010

There's another paradox in the process. People who've come up through the industry route know different things than PhDs — indeed the fact that doing academic research causes you to learn different useful stuff (in stuff like mathematics; dunno what it means for a theoretical physicist) is presumably why business people want PhDs. But in the interview process it's customary that you can only answer the questions you're asked, and you generally get interviewed by an industry person. They ask you the questions they know the answer to from their background, then reject you with the reason "you don't seem to know as much interesting stuff as I expect", completely missing the point that you might know different stuff they don't know.

If there's one good thing about interviews for academic posts, it's that the interviewers are very good at giving you opportunities to tell you about stuff YOU know about that THEY don't.

16. **Iowa Beauty**
October 17, 2010

"on the interview process it's customary that you can only answer the questions you're asked"

Man, not in my world. I always ask open ended questions, such as "what do you think you can do for me?" (after explaining the position), and I encourage people to demonstrate problem solving ability and creativity by telling me about their hardest and most interesting accomplishments. Anybody really wanting a job would take those openings and run with them.

That said, I do expect people to come to me prepared to relate their skills and knowledge to the position, and to understand that we hire people to accomplish business goals, not purely intellectual ones. I don't see that many PhDs, but the ones I do see seem as often as not to consider industry work a consolation prize, and not be as fully engaged as others.

And of course, everything above is anecdotal, so apply appropriate error bars.

17. **bane**
October 18, 2010

I suspect it may vary with field. I work with computers, which is perhaps unusual in that graduate level mathematics is often powering what the user sees. I've recently been to interviews with various software companies, including some of the most famous ones in the world, and ALL the questions have been "Here's this problem I like, solve it", no questions that were remotely open ended or picking up items I'd drawn attention to in the cover letter. Probably I wasn't right for any of those jobs, but what's annoying is the feeling that the interviewers didn't ask the more open-ended questions that would have given them a feel for what I could have done for the company. In contrast, every academic position interview has had a mix of "testing questions" and "show me something useful you know questions". If you're consciously asking open ended questions to get a better view of the potential employee then I think you stand above many other interviewers.

18. **Hutom**
October 18, 2010

Nobody retires. New positions are created by people dying or by expansion of departments. The real surprise is that so many new positions are still available every year.

19. **truth**
October 29, 2010

It's not even really the business leaders. They want BS in Comp Sci from India (for same reasons of cheaper). But wrt Ph.Ds in sciences or math, that is just professors and unis wanting to have research programs and needing the transiet slave labor for the faculty.

20. **Terra**
October 29, 2010

The tenured establishment welcomes anything that increases the numbers of PhD's produced because they've already got secure jobs, and they can hire more graduate students to do slave labor in both research and teaching.

Looking at from another perspective, if we're thinking ahead for the good of society, the older generation owes the younger generation a chance to be trained for careers that actually exist.

I got my Ph.D. in physics at a time when the APS estimated that 3% of Ph.D. students would get permanent jobs in the field. I went to an elite school, and out of a graduating class of 50 or so, I think two people got jobs. One was an affirmative action case (female child of a professor) and another (child of a professor) is working at a new university in the Middle East.

It gratifies the narcissism of the tenured establishment to believe “the cream floats to the top”, but they’re actually filtering for desperate people who will tolerate an intolerable environment. Something I’ve noticed is that an overwhelming majority (80%+) of the professors at this elite school have parents who were also professors.

I got into a discussion with a professor (who I hope is in English or some other un-quantitative field) who told me that, at best, he could manage to get somebody placed by a connection once in his career. Well, when tenure-track jobs are in a steady state, that one act of nepotism effectively lowers the chances of the son of construction worker to spend a life in science to zero.

21. **Bugsy**
November 1, 2010

Terra, don’t assume all kids of professors get jobs because of nepotism.
There are very simple reasons for kids to want to try a career similar to that of their parents–
it is something you grow up around so it seems natural, accessible and understandable and, if you are lucky, interesting (and that part can apply to academics’ kids just as well as to those of the owner of a small business). So the sample is bound to be (very) biased. Plus, knowing the ropes in an intuitive way can sure help out a lot in any career.

Beyond that, I agree with all else you said. And indeed, life can be tough in a dfferent way for a professor’s kid:
dreaming you can copy your daddy’s success in perhaps a very different epoch but then seeing your own chances for “a life” go down the drain is emotionally challenging to say the least (brutal and dehumanizing being much more like it!)

Bogdanov Update

October 18, 2010
Categories: Uncategorized

Earlier this year the Bogdanov brothers published Le Visage de Dieu, yet another book dealing with their ideas about the big-bang and pre-big-bang physics. This follows their earlier Avant le Big Bang, and L'équation Bogdanov : Le secret de l'origine de l'Univers? a book that they somehow got Lubos Motl to take credit for. I haven't seen the new book, but it seems that it takes off from George Smoot's comment that looking at the CMB was like "seeing the face of God." Somehow the brothers have managed to get well-known cosmologists Robert Wilson, Jim Peebles and John Mather to contribute pieces to the book.

In other Bogdanov news, I hear from a Paris correspondent that an interesting document has recently come to light, a report commissioned by the CNRS after the Bogdanov Affair made headlines back in 2002. For press coverage of this, see here, here, here. The CNRS critique, from November 2003 is detailed, harsh and devastating. For instance, about Igor Bogdanov's thesis (which ended up being published in multiple copies in several respectable journals, including the highly thought of Classical and Quantum Gravity), the conclusion is "la valeur de ce travail est nulle", this work is of no value.

Update: For another informative article about the Bogdanovs, from this summer, see here.

Comments

1. **Chris Oakley**
   October 18, 2010

   On the subject of Avant le Big Bang, we had a Horizon (popular science) programme here in the UK recently on the very topic. The title actually was "What happened before the big bang?" The obvious answer to this question is "I have no idea, and neither have you", which is more or less where they got to, but not before taking in extensive and largely contradictory contributions from Linde, Penrose, Smolin, Turok, and a few others I had not heard of. Still – it was interesting to see the world's largest vacuum chamber and the gravitational wave detector on the way.

2. **Anon2**
   October 18, 2010

   http://news.bbc.co.uk/1/hi/programmes/hardtalk/4631138.stm

3. **Anonymous**
   October 18, 2010

   What? Peebles contributed to Bogadanov's book?

4. **Anonymous Number 2**
   October 19, 2010

   It would be good to check whether Robert Wilson, Jim Peebles and John Mather *knew* that their pieces were being used for this book, before assuming they did.

5. **Anonymous Number 2**
   October 19, 2010

   The CNRS report on Grichka Bogdanov's math thesis describes his results in one page. It says (translating from the French) that they are "presented with great naivety and with errors which demonstrate the author's misunderstanding of a concept that is taught at the maîtrise level."

   "It would be easy, but cruel, to list the numerous errors which spatter this text, for example "SO(2,2) has no matrix representation" (p. 17). We shall content ourselves with observing that the space Σtop that he uses is simply the Cartesian product of R2 by (R3 × R3)/SO(3) and that this removes any potential meaning from the phrase "unique singular point" which is the essential [foundation] of the statements in this chapter, and [equally removes any potential] relevance of this study to the "Initial Singularity" of space-time."

   […]

   "In conclusion: one of the eight chapters of this thesis contains a construction at the level one might find in an undergraduate essay, but very badly written: the other seven contribute nothing to mathematics."

   The report spends much longer — 15 pages — dissecting Igor Bogdanov's physics thesis. It concludes:

   "Unfortunately, even a rapid reading evokes strong suspicions about the scientific contribution and about the quality of presentation of this work; and a deeper analysis only confirms these suspicions. It seems that the text contains no coherent construction. On the contrary, this work is a constellation of incoherencies, of serious confusions and of propositions so imprecise as to be incomprehensible. The conclusion of this report, based on the detailed analysis presented below, is clear and unambiguous: according to the criteria set out above, the value of this work is nil."

6. **Chris W.**
   October 19, 2010

   Well, I guess one could give them credit for pulling off a moderately successful scam, inasmuch as some reputable people felt compelled to spend valuable time exposing it, and granted them some publicity in the process. If you're enough of a narcissist, that feels like success. ("I don't care what they say about me, as long as they spell my name right.")

7. **Coin**
   October 19, 2010

   "Bogdanov" is hard to spell

8. **Anonymous**
   October 20, 2010

Does anyone have an actual link to the CNRS report?

9. **Peter Woit**
   October 20, 2010

   There is a link in the posting:

   http://www.marianne2.fr/docs/rapport_cnrs_bogdanoff.pdf

   It's unclear to me though whether this is the full CNRS report or just an appendix to it. If there is a more complete version of the report somewhere, I haven't seen it.

10. **MathPhys**
    October 20, 2010

    Peter, What do you mean by

    "This follows their earlier Avant le Big Bang, and L'équation Bogdanov : Le secret de l'origine de l'Univers? a book that they somehow got Lubos Motl to take credit for"?

    How do you know that Motl did not write the book? I thought he wrote on his web site that he did.

11. **Peter Woit**
    October 20, 2010

    MathPhys,

    I don't know what the real story of the "Bogdanov equation" book is, just that it must be a very odd one, and that in matters involving the brothers, things are often not what they are claimed to be.

    I don't actually have a copy of the book, but from its table of contents, some chapters sound like Lubos, others, not so much. The first chapter, available on-line, just doesn't seem to me at all like something Lubos would write. But, again, who knows….

12. **YBM**
    October 20, 2010

    Here is the first part of the CNRS report :
    http://www.marianne2.fr/attachment/62240/

    The full report is supposed to be published in a few weeks.

13. **Random**
    October 20, 2010

    FYI: the Marianne link to the CNRS report is just the overview, the non-technical pages. There should be an appendix for each thesis detailing the criticism. The CNRS is in the process of clearing it for release to the general public.

    The real question is why that report was left to snooze at the bottom of a drawer in the first place!

14. **Peter Woit**
    October 20, 2010

    Random,

    Interesting question!

    The report YBM links to does a good job of demolishing the idea that the Bogdanovs did any serious science, but seems to whitewash the failure that led to them both getting Ph.Ds. The names of those responsible for allowing this are blacked out, and the claim is made that this is such an unusual case that it's not worth doing anything to keep it from happening again.

    More interesting might be the internal discussions at the journals that published the Bogdanov articles….

15. **MathPhys**
    October 20, 2010

    ".. whitewash the failure that led to them both getting Ph.Ds. The names of those responsible for allowing this are blacked out,"

    These very pages that are linked (or something equivalent to them, but I think the same) were available a number of years ago without the blackouts. Parts of the reports on the theses were also made available.

    I remember one of them. It was like ".. this is revolutionary, very deep, …".

    "… and the claim is made that this is such an unusual case that it's not worth doing anything to keep it from happening again."

    The scary thing, Peter, is that it does happen on a regular basis. I can tell you of a specific example of a thesis (also on string theory) that was approved in the late 80's (the glory days of string theory) by very well known scientists at a very well known university.

    We now know that everything in this thesis was either wrong or plagiarized. None of the examiners had a clue. Those of us who suspected foul play in advance couldn't say a word. One becomes very unpopular for rocking this type of boat.

    But it also happens on a daily basis in papers that are being refereed for publication. How many referees are actually willing or able to re-derive the technical details of a technical paper. If it sounds reasonable, you approve it and hope for the best. But reasonable can turn out to be totally wrong.

16. **Bugsy**
    October 21, 2010

John Baez has some very interesting links on all this…last updated 2006:
http://math.ucr.edu/home/baez/bogdanoff/

This is one reason why advisers should never feel steamrolled into accepting or sticking with a student they feel is not up to the level, PARTICULARLY if they suspect the student is dishonest….someone with delusions or no scruples who can then parlay their PhD into money or influence. And who but a borderline sociopath would try that anyway?

17. **e.**
October 21, 2010

Bugsy, if someone is dishonest or wrong then this doesn't have to do with adviser ethics. It's much simpler. On the other hand what you suggest is tricky, since the inverse occurs a quadzillion times more often.

To the case itself — a friend of mine is a talented artist; yet I see her how her and her peers are willing to accept any piece of art, as long as it's self-proclaimed as abstract, no matter how silly, meaningless or downright bad it is (and I don't think that all abstract art is meaningless), reaching a point where they deny themselves the right to taste and critical ability. I still don't get why they do so, especially considering how this diminishes the value of their own works, although I have some ideas: a wish for paving acceptance for themselves, a wrong notion of abstract, and the rules of the market, among them.

I could end with a grande phrase outlining certain similarities but I think it'd be redundant 😐

18. **Chris W.**
October 21, 2010

Speaking of sketchy to non-existent journal reviewing standards, see the recent exploits documented on the SCIgen blog.

If you're thinking of establishing a publication record in computer science you could try SCIGen yourself. 🙂

19. **Sascha Vongehr**
October 23, 2010

Glad to see that people do not tire of pointing out how easy it is to publish absolute crap in the most respected journals, as the Bogdanovs did. I wish to support those here that stressed that such is not seldom and involving bad apples, but that the problem is systemic and growing.
My few cents to this subject I wrote about involving the subject of nanotechnology, where such is also going rampant (not quite as easy to use as SCIgen, but I thought of doing something similar with this paper generator):
http://www.science20.com/alpha_meme/pop_physics_free_nanotech_scientific_journal_article_generator

20. **clmasse**
October 25, 2010

"La valeur de ce travail est nulle"? I wasn't aware that French were the official language of the "physical community." A franco-french revenge about a cartoon character, or a real scientifical "huge problem"? To say the truth, in many year I've seen many valueless works, the least of them not being the supersting and other supercheries, without any bearing of any kind with empirical data. For me, controversies about for instance the climatgate is a better loss of time.

21. **martibal**
October 25, 2010

clmasse: i do not think the CNRS reports intended to be the voice of the physical community, but only the voice of CNRS, dealing with a problem internal to the french university (namely is it or not a scandal that a university delivered the Bogdanoff a PhD diploma ?). And one may also wonder whether they would have been such a mess if they were not (ex)-TV stars ? I mean, is their paper really the worst ever published in Class. Quant. Grav. or any other good journal ? At least from a surrealistic-poetry point of view, claiming that "the Big Bang singularity is aligned with Foucault pendulum" is not so bad 🙂

Also it should be emphasize that thanks to their TV program (which I find a crap) many people hear about science. Is it better to hear about almost-zero-value science, or not to hear about science at all ? Of course it would be better to hear about good science on tv, but there is no hope to find a science program prime time on some national network.

A last comment: it is also not so clear that they are dishonest. They worked several years on their PhD without being paid. So to some extend they may believe in what they say, even it what they say sounds total nonsense.

22. **MathPhys**
October 26, 2010

Many of the things they did 'after' their PhDs, including the Prof Yang alias e-mails, websites for research institutes that do not exit, etc, etc, make it clear (to me) that they are less than honest.

This Week's Hype

November 3, 2010
Categories: This Week's Hype

Way back in 1997, string theorists were already getting rather touchy about people pointing out string theory's testability problems. At that time, Gordon Kane published an article in Physics Today with the title String Theory is Testable, Even Supertestable in which he wrote:

> A decade ago in PHYSICS TODAY (May 1986, page 7), Paul Ginsparg and Sheldon Glashow raised this question dramatically, and effectively began a widely repeated myth that string theories, candidates for a primary theory, are not testable. Here I want to dispel this myth, and describe some of the many ways in which string theories are testable. If nature is supersymmetric on the electroweak scale, for which there is exciting but not yet compelling evidence, then string theories are even testable in essentially the same ways as traditional ones. All the tests I describe are doable now or in the foreseeable future with existing or proposed facilities or projects.

Kane went on to give a long list of testable things that string theory was going to predict, including as an example a detailed spectrum of superpartner masses, all in the range 50-300 GeV (he assures us that supersymmetry is a prediction of string theory, quoting something Gross and Witten wrote for the Wall Street Journal).

Now that LHC data has finally started to arrive, in amounts large enough to soon start seeing all of the superpartners advertised back in 1997, Physics Today has decided to put out a rather spectacular piece of string theory hype from Kane as their cover story, under the title String Theory and the Real World. In the story, the main theme is the same as 13 years ago: it's a myth that string theory doesn't make testable predictions. Now though, the many 1997 predictions are forgotten, and much of the article is devoted to a tendentious discussion of what it means to test a scientific theory. In the 2010 version, there's no longer a detailed list of things that string theory should be able to predict, instead Kane describes two specific predictions of string theory:

- the first one is about neutrino masses and is rather bizarre, describing work on one specific string theory compactification:

> We showed that in no case could the theory generate light but not massless neutrinos. That work represents a clear example of a test of string theory.

So, one specific string theory compactification is known to not look like the real world. That's a test of string theory????

- Kane advertises a recent paper of his from this past June (updated version of a couple weeks ago) about "non-thermal cosmological history". I'll leave it to readers to decide for themselves how compelling a test of string theory the paper provides.

Whatever one thinks of these latest "tests", the difference between what string theory tests looked like back in 1997 and what they look like in 2010 is rather remarkable.

Comments

1. **Anonymous**
   November 3, 2010

   "We showed that in no case could the theory generate light but not massless neutrinos. That work represents a clear example of a test of string theory." means that string theory predicts, correctly, that neutrinos have mass. I think you were confused by the multiple negations in the statement.

2. **Nex**
   November 3, 2010

   Anonymous you are the one being confused, if it said "string theory predicts light but not massless neutrinos." it would mean string theory predicts neutrinos have small mass (not massless =with mass).

   But the actual statement is the negation of the above since it says that in *NO* case could the theory generate light but not massless neutrinos.

3. **Jupiter**
   November 4, 2010

   I have been reading this blog with increased enthusiasm.
   Unfortunately, I do not have a background in theoretical physics
   and so it is difficult for me to evaluate the testability of this hypothesis.

   My basic question was:
   What is the endpoint, if there is one, of string theory? If there are major disagreements regarding the experimental evidence of a theory that has been around for 30 years, what can one conclude about the nature of the propositions that will be posited in the next 30 years?

4. **Bee**
   November 4, 2010

   I read the article yesterday, and found it to be really disappointing. I was very sure you'd comment on it 🙂 The most interesting sentence is this one

   "interested readers can find others in talks accessible from the website of the international String Phenomenology 2010 conference held in Paris this summer (http://stringpheno.cpht.polytechnique.fr)"

   I'd have expected that his article provides a summary of these efforts, rather than just advertising two of his own papers.

5. **Peter Woit**
   November 4, 2010

   Anonymous,

   I still think my interpretation of that "prediction" is correct. After what I quoted, the paragraph goes on:

   "Although the particular compactification we studied did not yield the desired neutrino masses, different compactifications may allow for neutrino masses consistent with experiment and offer explanations of observed neutrino properties."

Maybe though Kane is claiming that string theory "predicts" light but not massless neutrinos because they occur in one particular compactification. If so, "bizarre" would still be an accurate description of the argument.

6. **T.T.**
November 4, 2010

String theory is testable in a certain range of its fundamendal mass scale. Its tests are high on the LHC priority list and possible with only 2.9 inverse picobarns of data. See
http://128.84.158.119/abs/1010.0203
for latest results.
The statement that string theory is not testable is false.

7. **Peter Woit**
November 4, 2010

OK T.T.,

Since the LHC experiments have nearly 50 inverse picobarns collected, at the Winter conferences we should see announcements of the results of testing string theory. If the result is failure, I guess that's it for string theory, right?

Or maybe string theory gets to have it's own special kind of test, the kind where it doesn't matter if the test-taker fails…

8. **T.T.**
November 4, 2010

Peter,
The main premise of your blog was that "string theory is not testable". On many occasions you stressed that it does not matter for you whether it was right or wrong, so I will not answer your question. Furthermore, in experimental physics, we do not use the word "failure" to describe results — we rule out or confirm theoretical predictions.

If you want to make true statements, you'll need to add footnotes or small print.

It looks however that the main purpose of this posting was to make fun of Gordy Kane and other people who are enthusiastic about the prospects of the theory.

9. **Tim van Beek**
November 4, 2010

Peter wrote:

> …much of the article is devoted to a tendentious discussion of what it means to test a scientific theory.

I don't have access to physics today and can't read the article, but I would like to know what this discussion is about: If someone could provide a synopsis I would be thankful.

T.T. wrote:

> String theory is testable in a certain range of its fundamendal mass scale.

From the abstract of the paper you refer to:

> The data exclude new particles predicted in the following models at the 95% CL: string resonances, with mass less than 2.50 TeV…

AFAIK string theory can be adapted to not predict anything below 2.5 TeV (or XY TeV, for that matter), at least that is the state of the art right now. Therefore this is maybe a test of certain versions of string theory, but it is still not possible to falsify the theory. (My apologies to Peter if this remark starts the kind of discussion he has grown weary about).

10. **Peter Woit**
November 4, 2010

T.T.,

I think the person who needs to add small print to their claims is you. In the conventional understanding of what it means to "test a theory", when the experimental result differs from what the theory predicts that carries negative implications for the theory. And yes, people apply the word "failure" when this happens. If the LHC data differs significantly from standard model predictions, this will rightly be described as a failure of the standard model and be taken extremely seriously.

On the other hand, not even the most enthusiastic string theorist seriously expects the LHC to see strings in the first 2.9 inverse picobarns of data. Claiming that your theory is being tested by an experiment when you don't actually expect the experiment to see anything is just a not very honest abuse of language.

11. **Peter Woit**
November 4, 2010

Jupiter,

I think the endpoint of the history of claims about string theory being a unified theory of nature is already just about here: this is an idea that doesn't work. The lack of any support for such claims in the LHC data over the next few years should put the final nail in that coffin.

On the other hand, there will always be some people and some publications that will never admit what has happened. Extrapolating from the last two examples, by around 2025 we'll see Physics Today publishing a special issue by Kane on "String theory is too testable, darn it!". Hard though to even guess what the arguments for this will be by then.

12. **Manuel Bärenz**
November 4, 2010

T.T.,
I think Peter really has manners enough not to try to make fun of Gordy Kane. There are bloggers who do make fun of other people (most of us know at least one of

those…) but it appears that Peter does something far more intelligent – he points out that some people are making fun of themselves, without intention.
Switching from the meta-topic to the topic, I just wanted to add that there are AFAIK variants of string theory making predictions that are visible with the amount of luminosity you mentioned at the LHC. This is what some people mean when the talk about sensitivity of the LHC to string theory.
It is however not given that we *must* see evidence of this. I believe it takes an average string theory phenomenologist and two hours to set up some model that does explain why we don't see the signal. This is, why a reasonable man should not say that the LHC is sensitive to string theory, but at most to a tiny tiny fraction of some huge parameter space.

13. **Peter Woit**
November 4, 2010

Manuel,

Thanks, although I fear my last comment does show a certain willingness to make fun of this kind of thing. I agree though that mostly what's going on here is Kane (and Physics Today) choosing to embarrass themselves.

14. **Sam**
November 4, 2010

Peter:
There is no problem with the predictions changeing. This is exactly what we should expect of an evolving theory. You accuse the string theorists of claiming that they expect to see nothing, while saying their theory is being tested. Surely, that would not be a wise strategy for them, would it? Rather, the point is the unification of physics is a non-trivial exercise of the humans. It is absolutely incredible to think that a finite machine might reach the energies necesssary to probe this realm, but it is not necessary.

15. **Peter Woit**
November 4, 2010

Sam,

The problem isn't that string theory makes changing predictions, it's that it makes no legitimate predictions at all. Back in 1997, Kane wasn't making predictions, he was saying that in the future it would make predictions. Now that the future has arrived, all that has happened is that his past predictions of predictions have collapsed.

16. **MathPhys**
November 4, 2010

G Kane makes a living out of proposing experimental tests for supersymmetric (not necessarily string) theories. He did a lot of work on testing supersymmetric GUTs in the early 80's before superstrings. I heard him give talks on testing super GUTs in 1983 before the work of Green and Schwartz on anomaly cancellation in type I superstrings.

That's more than 25 years ago. I wish I could hear Kane admit once that he made a clear prediction that turned out to be wrong but he never does. That's not right.

Consider on the other hand Ch Thorne who is one of the true fathers of string theory (works on string since the late 60's) and who very clearly says that string theory as a unified theory of all interactions is not working.

We can do many things with it (come up with new, beautiful mathematics, for example), but at this stage, in this form, unification is not one of them.

17. **Richard**
November 4, 2010

For any who are looking for the original article by Gordon Kane but have not any clue where to access it, I have uploaded a copy on file-hosting websites

[*Sorry, but using a Columbia University web-site to provide a link to less-than-legal copies of documents is asking for trouble I don't want. If anyone has a better, legally non-problematic, link to this content, I can post that*]

18. **pt**
November 4, 2010

Write a letter to Physics Today about Kane's article. Quote from his 1997 article, for example the detailed spectrum of superpartner masses, all in the range 50-300 GeV. Point out that many 1997 predictions are forgotten in the 2010 article. Ask Kane how many of the 1997 predictions have been verified, or point out that none have been successful.

PT may not accept the letter, or may edit it heavily. KISS ~ I think you know what that means.

19. **Bee**
November 4, 2010

On the matter of scientific prediction, I just wanted to mention something I explained in my post [What is a scientific prediction?](). It is in practice very rarely the case that a theory is falsified. More often, it is what one could call "implausified": You constrain parameters so much the theory becomes implausible (most often because the parameters are unnatural). The problem with the LHC "predictions" is that if nothing is found, you can just go and say "naturally" we wouldn't have expected to see anything, because that particular model wasn't the right one or because this compactification scale, mass spectrum, etc wasn't plausible to begin with or swhatever. That's very likely what's going to happen within the next couple of years. (But you know, we would totally expect to see something at the ILC!!) I think you're actually better off sticking to cosmology, though even there you have the problem that the model doesn't uniquely follow from the theory, so there's always an escape route. In essence, one has to be careful what one calls a prediction of the theory itself and what a prediction of a particular model that is based on the theory. For all I know, everything you'll hear at the string pheno is of the latter, not of the former type. Consequently, if you try to reverse the logic, falsifying the model doesn't falsify the theory. (Besides that, it also isn't much of a "pre"diction to claim neutrinos have small but nonzero masses.) Not that this problem is unique to string theory in particular. In any case, it's unfortunate Kane is making his points so badly because I welcome the string pheno efforts.

20. **Coin**
November 4, 2010

So, I am looking at T.T.'s link and trying to understand exactly what the "string resonances" that have been excluded at the 95% level under 2.5 TeV are.

Poking at google I am seeing explanations that the thing being tested by the search described in T.T.'s link is not string theory in general, but rather the "low-scale string" scenario, where the "fundamental string mass scale" is in the TeV range. Is this correct?

It furthermore appears that even in a universe where string theory is correct, it would be more surprising than not if the "low-scale string" scenario were true. So in contrast to, say, Supersymmetry or the Higgs Boson (which, if they are not found at the LHC scale, this will be considered alarming and potentially threatening to the acceptance of those theories, because although you can still rescue the theory you have to do contrived things to the models to do so), if the fundamental string mass scale is not found in LHC ranges then string theorists would legitimately consider this basically to be the expected result, because the low-scale scenario is relatively exotic among string theories. Is this also correct?

Also, I am seeing suggestions that low-scale string theories require "large extra dimensions". Are the low-scale string theories, then, the same ones where the LHC is predicted to eventually generate the vaunted "micro black holes"? But it looks like the search is not looking for signs of micro black holes being created, it is looking for "string Regge excitations" (http://arxiv.org/abs/hep-ph/0001166 , reference [3] in T.T.'s link) of quarks and gluons. Is there a simple explanation of why these "excitations" occur in the scenario being tested?

21. **neo**
November 4, 2010

Taking these statements at face value, it is fair to say that string theory is indeed testable, or at least one specific compactification of it. Kane claims that this theory is inconsistent with light, non-massless neutrinos, and exactly that form of neutrino has in fact been observed, so this specification of string theory has been tested and refuted. Is this not right?

22. **Peter Woit**
November 4, 2010

neo,

The criticism of string theory is that it predicts nothing since you can find a "solution" that has any feature you want. Showing that there is a solution with the wrong neutrino masses doesn't exactly refute this.

23. **T.T.**
November 4, 2010

Strings vibrate. The quantized modes are Regge excitations. This is a universal property and is valid for all models, not a "feature" of any particular "solution". If Nature chose just one parameter, the fundamental scale of order TeV, LHC will discover string resonances in dijets and many other places. This is very simple, so if you have a scientific (as opposed to social group-thinking that you despise) argument why Nature made a different choice, I'll be glad to hear it.

24. **disproof**
November 4, 2010

The phlogiston theory was never disproved, per se. The luminiferous ether theory was also never disproved. They were merely forgotten or abandoned, because better theories came along which could explain the facts without increasingly artificial contrivances. So it will be with string theory. One can complain about the state of physics TTWP etc, and commit suicide as Boltzmann did, but that is neither here nor there.

25. **Mitchell Porter**
November 5, 2010

"The criticism of string theory is that it predicts nothing since you can find a "solution" that has any feature you want."

Wouldn't it be rather big news if a solution of string theory which looked exactly like the real world was found?

26. **Bee**
November 5, 2010

T.T.: Because there's no deep reason the relevant scale should be at a TeV.

27. **John**
November 5, 2010

At Mitchell Porter,

No it would not be big news if string theorists found such solution because it wouldn't explain much: the strength of such explanation would be similar to the Greek 'explanation' of positions of planets by epicycles. I am sure that the Greek thought in that time that this was the best exlanation one could get, just like string theorists now believe that science has to 'progress' through antropic reasoning… History has shown this type of 'ideas' to be wrong again and again and one should not take it seriously.

28. **Peter Woit**
November 5, 2010

Mitchell Porter,

If a string theory solution is found in which all the parameters of the standard model can be reliably calculated and they come out right, that will definitely make the newspapers.

So would the observation of a flock of pigs circling the CERN site at an altitude of 100 meters.

29. **Thomas Larsson**
November 5, 2010

Theorem [Larsson 2007, 2009]
Supersymmetry will not be discovered at the LHC.

Proof: String theory predicts supersymmetry (Witten 1984-2002). String theory predictions are always wrong. Hence supersymmetry does not exist, and will in particular not be found at the LHC. QED.

Corollary
Lubos Motl will lose his experimental-susy-by-2006 bet.

30. **Mitchell Porter**

November 5, 2010

Peter – there is an obvious contradiction between "string theory can be made to look like anything" and "string theory can't be made to look like reality". So what are you saying – that string theory *can* resemble reality, but that the calculations would be hard?

31. **John**
November 6, 2010

???? Mitchell Porter, I don't understand what is so difficult to comprehend from the last comments… There are really a few issues here
(a) String theory hasn't managed yet to resemble reality
(b) string theory contains 10^{500} solutions of which presumably the large majority do not look like reality
(c) even if string theory would solve (a), it probably never can *explain* (a) in the context of (b).

So like I said, this is the situation the Greeks had with epicycles, they could solve (a) but never got an explanation for it … until galileio and newton came by. Peter appears to think that solving (c) is never going to happen and I tend to agree with him.

Usually, a common feature of all scientific revolutions is that calculating out the old theories was *easy*. It was a piece of cake for Newton to verify the correspondence between the 1/r^2 force of gravity and the equal area swept out in equal times by planetary motions. It was simple for Einstein to deduce a Newtonian limit for the gravitational laws and the mere fact that the classical quantum correspondence is *not* so straightforward – in my mind – indicates that we are missing something. Consistency checks have always been easy … it was calculating the new phenomena which was usually a bit more difficult (see for example the three body problem in Newtonian gravity).

Why is this difficult to understand? It is not because people have a lack of good ideas that we should desperately try to defend the only things we can come up with so far…

32. **Peter Woit**
November 6, 2010

Mitchell,

The problem with "string phenomenology" is two-fold:

1. You can't actually calculate much at all reliably. Even in the limiting cases where perturbation theory is valid, an example of this problem is that you don't have explicit metrics for the Calabi-Yaus compactification spaces you would like to use. After 25 years of work on this, the state of the art is not much advanced. This is why even Kane has given up on his 1997 claims, and why it's highly implausible that all of a sudden you will be able to start exactly matching up string theory models and the Standard Model parameters.

2. As far as anyone can tell, when you are able to calculate something, you can get almost any values you want, by choosing a complicated enough string theory model. Even if you solved 1, there's no reason to believe that this problem would go away, and that all of a sudden a relatively simple model would appear that matched the standard model. One can't logically rule such a thing out, but believing in its existence at this point is pure wishful thinking.

33. **Eric**
November 6, 2010

Mitchell,

It's not really true that you can't calculate much reliably in a given string compactification. For example, in Type II string theory with D-branes on toroidal orientifolds, all parameters such as gauge and Yukawa couplings can be calculated reliably. Additionally, there are string models which strongly resemble the Standard Model. The problem is really of uniqueness. In order to fix the values of parameters such as couplings, the problem of moduli-stabilization must be addressed. This is on top of the problem of the huge number of possible vacua. Peter is right that the so-called top-down approach by which the Standard Model may be derived uniquely from string theory has thus far failed. However, the 'bottom-up' approach is alive and well.

Recent Hot Topics in Hep-th

November 4, 2010
Categories: Uncategorized

SLAC's SPIRES database has a link you can use to search for articles heavily cited during 2009 and 2010. Just looking at the hep-th papers, three are review articles of older work on applying AdS/CFT to condensed matter physics, three are about Erik Verlinde's claims that gravity is an "entropic force", and the rest are about Petr Horava's non-relativistic theory of quantum gravity.

The story of hep-th in 2009/10 seems to be that the only new ideas getting attention are ones coming from prominent string theorists who have become apostates advocating non-string theory approaches to quantum gravity. The idea of getting gravity out of simple thermodynamics didn't get much attention back in 1995 when Ted Jacobson was discussing it, partly because it didn't seem to go anywhere, partly because the conventional wisdom was that the spin-two massless mode of a string was the reason for gravity. Now that, fifteen years later, a prominent string theorist is promoting the idea (see his recent Harvard colloquium on the topic here), it is getting a lot of attention.

Those abandoning string theory as an explanation for quantum gravity do need to be careful in how they describe what they are doing; see for example the first part of the most heavily cited hep-th paper of 2009 (Horava's), which begins as follows:

> In recent decades, string theory has become the dominant paradigm for addressing questions of quantum gravity. There are many indications suggesting that string theory is sufficiently rich to contain the answers to many puzzles, such as the information paradox or the statistical interpretation of black hole entropy. Yet, string theory is also a rather large theory, possibly with a huge landscape of vacua, each of which leads to a scenario for the history of the universe which may or may not resemble ours. Given this richness of string theory, it might even be logical to adopt the perspective in which string theory is not a candidate for a unique theory of the universe, but represents instead a natural extension and logical completion of quantum field theory. In this picture, string theory would be viewed—just as quantum field theory—as a powerful technological framework, and not as a single theory.
>
> If string theory is such an apparently vast structure, it seems natural to ask whether quantum gravitational phenomena in 3 +1 spacetime dimensions can be studied in a self-contained manner in a ''smaller'' framework. A useful example of such a phenomenon is given by Yang-Mills gauge theories in 3 + 1 dimensions. While string theory is clearly a powerful technique for studying properties of Yang-Mills theories, their embedding into string theory is not required for their completeness: In 3 + 1 dimensions, they are UV complete in the framework of quantum field theory.
>
> In analogy with Yang-Mills, we are motivated to look for a ''small'' theory of quantum gravity in 3 + 1 dimensions, decoupled from strings.

Update: A commenter points out that Verlinde has just received a 2 million euro grant to support this kind of research, more info available here.

Comments

1. **Anonymous**
   November 4, 2010

   What a great analogy of Yang-Mills! 🙂

   Entropic gravity and Lifshitz gravity indicate that the subfield known as quantum gravity may enter a "model-building" era, as opposed to "formal" approaches like superstring theory. Yet another possibility is that with continued frustration, this subfield will shrink and become non-mainstream, much like how so few people are working on the million-dollar Yang-Mills existence problem because they don't see any hope of success.

2. **anon.**
   November 4, 2010

   It's a little odd to call these "hot topics". I think it's more that they're relatively non-technical ideas that are easy to work on, so they've been latched onto by not-very-competent people mostly in countries where publishing a large number of papers is required for employment but publishing quality papers is not. Horava-Lifshitz gravity, in particular, seems to be looked askance at by most good people. This isn't because of prejudice against non-stringy ideas but because the field has already had a few iterations of clever modified gravity ideas and, through painful trial and error, learned the lesson that they almost all fail in a universal way. So few people were surprised when it turned out that H-L gravity has the usual problem. More recently people (including those who successfully found the expected problem) have claimed to modify it in ways that make it sensible, at least at a linearized level. I would bet that the problems are still lurking in the background, waiting for someone to do a more careful analysis. If not, then I think if anyone really convincingly shows they are absent, the field would pay attention.

   My assessment would be that there *are* no hot topics at the moment. The only recent big advances of high quality are being made in technical areas with a high barrier to entry, so they're not picking up huge citation counts. In the hep-ph world, driven by experimental results, light (5 to 10 GeV-ish) dark matter is trending toward being a hot topic.

3. **Anonymous**
   November 4, 2010

   The only recent big advances of high quality are being made in technical areas with a high barrier to entry, so they're not picking up huge citation counts.

   What's your list, or a few examples, of these big advances?

4. **a**
   November 4, 2010

   What about http://arxiv.org/abs/0906.3219 and http://arxiv.org/abs/0901.3753 ?

   Somehow you don't mention these two recent developments, quite convenient indeed…

5. **Peter Woit**
   November 4, 2010

   a,

   The reason is simply that they don't appear in the SLAC recent topcites list I linked to. The Horava paper, of similar vintage, has about four times as many citations.

6. **Marcus**
November 4, 2010

There could be some problem with the Spires search because you asked for 100+ topicites with date = 2009. And the Alday et al paper mentioned by "a" has 111 cites (according to Spires.)

Since Spires knows that http://arxiv.org/abs/0906.3219 got 111 cites, why would it not have included it on the list? Looks like a bug in the search engine to me. (Certainly not an intentional omission on anyone's part.)

7. **Marcus**
November 5, 2010

The other 2009 paper that "a" mentioned is this:
http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=FIND+EPRINT+0901.3753
and it did not meet the search requirements, so there was no error.

You asked that 2009 papers have 100+ cites and this one has 97.
=============

What puzzles me is
http://www.slac.stanford.edu/spires/find/hep/www?rawcmd=FIND+EPRINT+0906.3219
with 111 cites.
It fits the terms you defined but did not appear on the list.

8. **Marcus**
November 5, 2010

There appears to be something wrong with the record for that paper.
When I tell Spires
FIND A GAIOTTO AND DATE 2009
then it finds the paper. (but says it is 50+ cites when it should say 100+)

On the other hand when I tell Spires
FIND A ALDAY AND DATE 2009
it does not find the paper at all.

Somebody should tell Alday about this so that he can write to Spires and get the record of his paper corrected.

Everybody benefits from the reliable operation of a database like that so we should thank "a" despite his sarcastic and suspicious tone of comment, for revealing the flaw 😀

9. **Bee**
November 5, 2010

I don't think the reason Verlinde's paper got so much attention is that he's a string theorist. It's just that he's formulated his claim by using only a handful of simple equations. Or possibly because people had nothing better to do.

10. **Peter Woit**
November 5, 2010

Marcus,

There are other examples easily found of papers with a bit more than 100 citations that are topcite 50, not topcite 100. I suspect that this is because they only periodically go through the whole database to update the topcite status. If so, this just means that for a 2009 paper to make the SLAC recent topcites list, it needs not just more than 100 citations now, but this had to be true a few weeks ago.

11. **anon.**
November 5, 2010

Anonymous asked:

What's your list, or a few examples, of these big advances?

Definitely the things "a" linked to: Alday/Gaiotto/Tachikawa (which I'm a little surprised has 100+ citations, but good for them!) and integrability. Plus other work of Gaiotto on N=2 supersymmetric field theories. Also progress in relating scattering amplitudes with Wilson loops, Grassmannian representations of N=4 amplitudes, continued advances in using twistors and momentum twistors, improved understanding of consistency conditions in supergravity… Probably there's a lot more that isn't coming to mind, since I'm not actively involved in any of these things. I would say there's been steady and significant progress in technical aspects of understanding quantum field theories, especially highly supersymmetric ones, for the last few years.

12. **BS**
November 5, 2010

Hi Peter, have you heard of the ERC grant for Erik Verlinde? This is quite a big success for him and the University of Amsterdam. Although I was very surprised to see that the research will be about emergent gravity.
http://www.science.uva.nl/english/home.cfm/7BA87E2C-CC3F-4C2A-84FA5A77B6AD6B27

13. **Peter Woit**
November 5, 2010

Thanks BS. I had seen from Verlinde's twitter feed that he got the grant, but had assumed it was one or two orders of magnitude smaller in size. $3 million or so is a huge amount for a grant of this sort. So, "entropic gravity" is now not only a hot-topic, but an extremely well-funded one….

14. **Vince**

November 5, 2010

Has anyone read this paper:
http://arxiv.org/abs/1009.5414

If there's nothing wrong with it, why is entropic gravity being funded?

15. **somebody**
November 5, 2010

Entropic gravity is really nonsense. I have tried to read the paper giving Verlinde the maximum leverage in what he *could have* meant, and I still can't come up with anything useful. Everything that was worth saying there was already said by Jacobson long ago. (In my opinion, of course!)

On the other hand, Horava's theory, you have to give him credit for trying something new… even if it is also likely to be unsalvageably wrong.

16. **Kea**
November 5, 2010

And will Verlinde give some of the money to all the starving people who have been working on related ideas for decades?

17. **Mitchell Porter**
November 6, 2010

In Erik Verlinde's recent talk at KITP, starting at 31 minutes, he switches to a non-entropic perspective involving individual pure states rather than ensembles of them. Listen especially to the start of the 37th minute, where he says that under the right conditions, ergodic motion in a phase space would be macroscopically indistinguishable from a genuine thermal state. So it looks as if he's moved to a deeper perspective that might evade the Motl-Kobakhidze argument.

18. **MathPhys**
November 6, 2010

"And will Verlinde give some of the money to all the starving people who have been working on related ideas for decades?"

When I read what Padmanabhan writes on the topic, I feel that there is probably a point to it.

Simons Center Inaugural Conference

November 5, 2010
Categories: Uncategorized

I was out in Stony Brook for the past couple days, to attend festivities surrounding the inauguration of the new building there which will house the [Simons Center for Geometry and Physics](). The Center is funded by Jim Simons, whose Renaissance Technologies is one of the world's most successful hedge funds. The plan is to ultimately have six permanent members (half in physics, half in math) and a director, as well as quite a few postdocs and visitors. The founding director of the center is John Morgan, who until recently was my colleague here in the math department at Columbia. Mike Douglas is a permanent member in physics, current and recent senior visitors include Nikita Nekrasov and Graeme Segal.

The building is quite attractive, with the two lower floors forming a public area that includes two auditoriums, an atrium and a dining area which will serve lunch and bring together the local math and physics communities. The three upper floors have offices and seminar rooms. Construction is nearly finished, but not quite, with part of the ground floor still walled off for some last-minute work. The atrium features a large mural containing a selection of historically important equations, the choice of which evidently was a major undertaking. Guests noted one typo, but luckily the current mural is a temporary printed one, with the plan to cut the equations in stone not yet implemented.

Tuesday night featured a gala opening event, with music provided by the Emerson quartet, and short speeches from various dignitaries. Simons was presented with an original edition of one of the works of Isaac Newton, with the comment that he had shown much greater success than Newton at turning things into gold. He gave a wonderful talk describing the early stages of interaction between math and physics that he was involved in as chair of the math department at Stony Brook in the early seventies. Evidently soon after his arrival he was invited to meet with Frank Yang, who had just started up an institute for theoretical physics there. Yang described his current research on gauge theory, which Simons claims to not have understood a word of. The same thing happened again a year later. However, the third time this happened, Simons all of a sudden realized that what Yang was talking about was something that geometers knew very well: a connection in a principal bundle. This led to a series of lunch-time lectures by Simons to the physicists, to visitor Is Singer getting interested and taking what he learned back to MIT and Atiyah at Oxford, and finally to the modern period of interaction between math and physics that began in the mid-seventies centered around questions related to instantons.

On Wednesday, the Simons Center hosted a day-long [inaugural conference]() (videos and slides of talks should appear on their web-site at some point). Appropriately, the first talk was an inspirational one from Michael Atiyah, going over a wide variety of different mathematical ideas. One theme was the quaternions, with Atiyah pointing out that Hamilton had written down a square-root of the Laplacian many decades before this trick was re-discovered by Dirac in writing down the Dirac equation. After recalling the relation between the division algebras (real and complex numbers, quaternions, octonions) and the Hopf invariant one problem, Atiyah suggested that the [Freudenthal magic square]() has a similar relationship to the Kervaire invariant one problem, with the recent complicated proof by Hopkins et al. an analog of the original Adams proof in the Hopf case, with the analog of Atiyah's "postcard proof" still to be discovered. He ended with some comments about ideas of Alain Connes about non-commutative geometry and the Riemann hypothesis, and suggested that the conjectured self-adjoint operator that could explain the Riemann hypothesis might be the Hamiltonian of quantum gravity. I noticed that Atiyah was supposed to be giving a talk at the IAS today with the impressive title of "Quantum Gravity and the Riemann Hypothesis", but it appears to have been canceled.

The second morning talk was a rather rambling and elementary one by Polyakov about topics related to Wick rotation, ending with the claim that in the gravitational case the usual ideas about Wick rotation fail and this has something to do with explaining the cosmological constant. The afternoon began with a talk by my colleague Andrei Okounkov using symplectic resolutions to study quantum cohomology, followed by Witten whose topic was "A New Look at Khovanov Homology". This talk covered similar material to the ones described [here](), involving a really beautiful story about various 3 and 4 dimensional topological quantum field theories. As in the earlier talks though, at the end there's a transition to 5 and 6 dimensional theories where I got just as lost as before. I had been hoping that the Simons Center talk would explain the ideas I was missing, but I fear that there's no way around digging into the details in Witten's [recent paper]() about this. The last talk was by Cumrun Vafa, who gave a very nice and elementary discussion of the "wall-crossing" phenomena in certain 2d qfts, as motivation for recent work on wall-crossing in 4d gauge theories.

The talks were uniformly of very high quality, it's wonderful to see that the Simons Center is off to a great start.

Update: I just did take another look at Witten's recent paper, and realized that the part involving 5 and 6 dimensional theories is not there, but in a paper in progress on "Five-branes and Knots". So, that story will just have to wait for now…

Comments

1. **Jason**
   November 6, 2010

   Walked out my door this morning, fired a bugler, palmed a handful of sand and said in lingua franca "Gee, how almost perfectly god damn delightful it is to be right."

   Just a nod from the not so tenured and noble for your work, expressions and teachings.

   Thank you

2. **Jim**
   November 6, 2010

   Great new ctr for theoretical physics, only one sad note.
   There are no stipends or fellowships for grad students.
   Seems hard to believe such a small contingent could get
   anything significant done.

3. **Ken Yokoyama**
   November 7, 2010

   Regretably!

   > Atiyah was supposed to be giving a talk at the IAS today with the > impressive title of "Quantum Gravity and the Riemann
   > Hypothesis", but it appears to have been canceled.

4. **Peter Woit**
   November 7, 2010

   Jim,

   The center won't teach classes or have its own students, but I'm sure Stony Brook physics and math grad students will participate in its activities, and they are already funded. Given the huge imbalance already existing between the number of students in this field and the number of jobs for them, funding more students isn't really a good idea.

5. **The Vlad**
   November 7, 2010

   @Peter Woit

   That's a viewpoint framed with the welfare of potential students in mind. Yet I would have thought you would be encouraging funding for more students and post-docs. After all, if the field is as stuck as you say it is, and assuming younger researchers (students/post-docs) are the most likely source of a major breakthrough, isn't it better to have more of them?

6. **Peter Woit**
   November 7, 2010

   The Vlad,

   More is not necessarily better. I don't see much evidence that really good people can't get into Ph.D. programs because there are too few grad student positions being funded.

   The post-doc situation is much more complicated. One aspect of it is that you need post-doc jobs structured so as to recruit the kind of person able to make a big advance, and give them an environment and time to do this. The Simons Center should be a good place for this. More post-docs, of the right kind, might be a good idea, but the mismatch between numbers of these jobs and numbers of permanent jobs will continue to be a problem worth worrying about.

7. **ghost**
   November 7, 2010

   Just a question which have non real link with the post: did you heard that the schrodinger institut in vienna could closed the next year?

8. **anonymous**
   November 7, 2010

   Here is a link with the text of an e-mail sent out by the ESI (Erwin Schroedinger Institute, Vienna, Austria) management about its eventual shutdown:

   http://prof-yura.livejournal.com/487154.html

9. **Chris Oakley**
   November 8, 2010

   The closing of the Schroedinger Institute does not seem necessarily to be a done deal – the process of making a measurement, however, could force it into a closed state.

10. **Ossicle**
    November 9, 2010

    Would love to hear or read the Atiyah talk, I wonder if it'll be available anywhere.

Once Before Time

November 8, 2010
Categories: Book Reviews

There's a new popular book out this week entitled Once Before Time: a whole story of the universe, by Martin Bojowald, promoting his ideas about "Loop Quantum Cosmology". It's a translation of the original German edition, Zuruck vor den Urknall, published last year.

The topic of the book is work by Bojowald on toy models using loop quantum gravity that avoid the Big Bang initial singularity of classical general relativity. For a much shorter version of all this, see his 2008 Scientific American cover story Big Bang or Big Bounce?

There's a very deep human desire to understand origins and thus to trace the history of the universe back before the earliest periods for which cosmological theory and observations have provided some degree of scientific understanding. Unfortunately this has led in recent years to a flood of over-hyped claims by physicists claiming to have a scientifically viable theory of what happened "Before the Big Bang". To qualify as legitimate science, such claims need to be backed up by some conventional sort of evidence. This might take the form of experimental predictions, testable either now or in principle in the future. It might also take the form of a highly constrained and beautiful theory whose success in other realms makes a compelling case that it could also explain experimentally inaccessible phenomena. I don't know of any example of such pre-Big Bang scenarios now being sold to the public that comes even close to having such backing.

The cover of Bojowald's book tells us about Loop Quantum Cosmology:

> Now the theory is poised to formulate hypotheses we can actually test.

I'm no sure exactly what that is supposed to mean, but it appears to be misleading hype, not corresponding to anything actually in the book. The text of the book itself wavers back and forth, sometimes explaining the overwhelming problems one faces if trying to extract some kind of prediction out of the LQC framework and emphasizing how speculative it all is, at other times expressing ungrounded optimism that somehow these problems will be overcome. It ends on an upbeat, hopeful note

> Will we ever, with a precision that meets scientific standards, see the shape of the universe before the big bang? The answer to such questions remains open. We have a multitude of indications and mathematical models for what might have happened. A diverse set of results within quantum gravity has revealed different phenomena important for revealing what happened at the big bang. But for a reliable extrapolation, parameters would be required with a precision far out of reach of current measurement accuracies. This does not, however, mean that it is impossible to answer questions about the complete prehistory of the universe. Cosmology as well as theoretical investigations are currently moving forward and will result in unforeseen insights. Among them might well be experimentally confirmed knowledge of the universe before the big bang.

but I found nothing in the book to justify this optimism. The few allusions to specific attempts to find some relation to something observable are vague and suffer from the book's nearly complete lack of any references to more technical sources of information.

About fifteen years ago, in The End of Science, John Horgan described the field of fundamental physics as degenerating into what he called "ironic science", something more like literature, art or philosophy than traditional science, pursued in a "speculative post-empirical mode". At the time I thought he was going too far, but Bojowald's book provides an unfortunate confirmation of the phenomenon Horgan was describing. It's written in a rather dense and sometimes impenetrable style, featuring quotations from Nietzsche, some science-fiction set off in italics, and a few pictures of contemporary art-works supposedly relevant to the argument. Attempts are made to claim a role for pre-Socratic philosophy, with LQC finally providing a means of going beyond the pre-Socratics:

> Otherwise, one can find among the pre-Socratics most of the elements of modern cosmology. Only with quantum gravity did truly new elements enter the game.

Aficionados of the loop quantum gravity – string wars will find various accurate comments about string theory and the sociology of science, and Bojowald also describes an interesting insider's point of view on the story of the development of loop quantum gravity and the scientific figures behind it. He's quite right that it's a fascinating possible approach to quantum gravity well worth pursuing, but the applications to cosmology seem to me not even close to being ready for prime-time and this kind of treatment in a popular book.

Update: There's a review by Brian Clegg at the Wall Street Journal here.

Comments

1. **Marcus**
   November 8, 2010

   Posted on arxiv today:

   http://arxiv.org/abs/1011.1811
   Observing the Big Bounce with Tensor Modes in the Cosmic Microwave Background: Phenomenology and Fundamental LQC Parameters
   Julien Grain, A. Barrau, T. Cailleteau, J. Mielczarek
   12 pages, 5 figures
   (Submitted on 8 Nov 2010)
   "Cosmological models where the standard Big Bang is replaced by a bounce have been studied for decades. The situation has however dramatically changed in the last years for two reasons. First, because new ways to probe the early Universe have emerged, in particular thanks to the Cosmic Microwave Background (CMB). Second, because some well grounded theories — especially Loop Quantum Cosmology — unambiguously predict a bounce, at least for homogeneous models. In this article, we investigate into the details the phenomenological parameters that could be constrained or measured by next-generation B-mode CMB experiments. We point out that an important observational window could be opened. We then show that those constraints can be converted into very meaningful limits on the fundamental Loop Quantum Cosmology (LQC) parameters. This establishes the early universe as an invaluable quantum gravity laboratory."

2. **Peter Woit**
   November 8, 2010

   Marcus,

   Seriously evaluating that paper requires more time and expertise than I have available. A quick look at it though indicates that it is discussing the possibility of features in a possible B-mode power spectrum in the CMB that would differentiate between a conventional inflationary potential scenario, and an LQC-inspired one. LQC doesn't seem to give any actual prediction of the size of these features, that's a free paramater.

   At this point, there's no observation of CMB B-modes at all, and these authors don't seem to be claiming relevance to anything measurable by current experiments

(Planck). So, the bottom line is that some possible next generation experiments (how many years out?) might see not only the power spectrum predicted by inflation, but some variation of this IF unknown numbers parametrizing our ignorance about LQC are in the right ranges.

I don't think any of that is much reason for optimism that we're going to be seeing pre-Big Bang effects anytime soon (or ever). But, even if Bojowald is much more optimistic, the fact remains that no specific results like that in this paper are described in the book. A serious description of CMB B-modes and the challenges of observing them and finding interesting signals in them would have made the book a lot more interesting.

3. **Marcus**
November 8, 2010

A serious description of CMB B-modes and the challenges of observing them and finding interesting signals in them would have made the book a lot more interesting.

A good point, Peter. Would make any book on the topic (LQG cosmology) more interesting. I haven't read Bojowald's book and can say nothing about it, but I have read the paper by Aurelien Barrau and friends. It's first rate.

4. **Daniel Tung**
November 9, 2010

Hmm..How about Penrose's book ? He also talked about the time before big bang, in which he suggested to be just the end of another period of universe, and that the same thing will happen to the end of our current universe.

5. **Bee**
November 9, 2010

Hi Peter,

You might find it interesting to check section 2.4.7 and 3.3 of my recent paper

http://arxiv.org/abs/1010.3420

It summarizes the work Marcus is also referring to. If you have a bounce scenario, the bounce would leave a distinct signal in the tensor mode spectrum. That is in principle observable, though, as always, it will eventually only constrain the parameters of the model. In this regard, also have a look at this paper for constraints from presently available data

http://arxiv.org/abs/1007.2396

Now one might plausibly say that the range in which the model can be tested is so far not a particularly strong constraint, but at least it's something. Best,

B.

6. **Tim van Beek**
November 9, 2010

> …the applications to cosmology seem to me not even close to being ready for prime-time and this kind of treatment in a popular book.

Here is what I know from newspaper stories published in Germany last year: Bojowald did LQC calcuations about the big bang for his PhD, which is an acceptable topic in the sense that the PhD candidate can prove that he/she understands some physics and can do reasearch/calculations on his/her own.

Then, for reasons unkown to me, several newspapers interviewed him or published articles about this work: My impression is that people simply wanted to publish something about theoretical physics for a change, and Bojowald's work held the promise to be interesting because it is about the "origin of the universe and everything". It would seem that some publisher succeeded in convincing Bojowald to use the ensuing publicity to write just another "popular science" book about highly speculative ideas that no one needs 🙁

Well, Bojowald *did* write an interesting (to theoretical physicists!) survey of this area in living reviews:
Loop Quantum Cosmology.

7. **Physicsphile**
November 9, 2010

I haven't read the book or the paper, but its fair to say the CMB B-modes potentially offer a unique new window into the early Universe. But, Unfortunately, their amplitude will probably be too small for us to ever detect.

8. **Marcus**
November 9, 2010

Here are white papers/websites discussing the proposed B-pol and CMBpol missions

http://cmbpol.uchicago.edu/
http://www.b-pol.org/index.php

There you can see what kind of time-frame and instrumentation they are talking about.

9. **Peter Woit**
November 9, 2010

Thanks Bee,

I just took another look at the book to see if I missed something. The section discussing possible early universe experimental tests is about four pages long and very vague. No specific mention of B-mode polarization at all (earlier in the book there's a vague mention of CMB polarization).

10. **Anonymous**
November 9, 2010

Horgan's "The End of Science" is really the end of Moore's law for science, the end of "one revolution after another" exponential growth for science, which prevailed the

20th century but can’t possibly sustain forever.

11. **k97**
November 9, 2010

“Now the theory is poised to formulate hypotheses we can actually test.”

How is any of this different from Kane 1997 or Kane 2010?

12. **Marcus**
November 9, 2010

How is any of this different from Kane 1997 or Kane 2010?

Rhetorical question? I haven’t read the book and I suspect neither have you, so we can’t actually tell *how it is different* (if it is different in kind) out of context.

One obvious difference COULD arise if Bojowald should point to a small body of literature by phenomenologists/observational cosmologists. I know of about a dozen papers along similar lines as the latest by Barrau and friends. The authors are not Loop community—they write about observational testing of other stuff including string.

Obviously Barrau would like to see the B-Pol mission funded. It was a proposal for the 2015-2025 timeframe, in response to a NASA call for proposals.

In the current economic condition what is going to happen to projects which take a closer look at the CMB?

I am not advocating. You asked *what is the difference*. I am replying that there is a hard dollar bread-butter difference. Personally I don’t think Kane 2010 was talking about one specific testing project, like putting one specific CMB observatory in orbit. In the 2015-2025 timeframe. Check the B-Pol mission link out for concrete detail.

So I think in answer to your question that if there is a difference, it is probably a difference in the degree of concreteness. And I think that Bojowald and Barrau and others are going to have to build on that, and make that point, if they are going to get Lqc early universe models tested by observation, or any other early universe models, in the present economic/political situation.

13. **Bee**
November 10, 2010

Hi Peter,

Well, I think the results are fairly new, so possible he just didn’t have them when the book was written. Best,

B.

14. **Peter Woit**
November 10, 2010

Bee,

Maybe that’s it, but then it’s still somewhat of a mystery why the cover of the book claims that LQC was “poised to formulate hypotheses we can actually test”, since I haven’t heard of any others besides the B-mode ones, and couldn’t find any in the book.

15. **cormac**
November 10, 2010

It’s not a great translation of the title, is it?

16. **chris**
November 12, 2010

Peter,

I really like your scepticism against all sorts of hype – not just string related. it is good to see that among all the “publish or perish” crazieness that has gotten hold of high energy during the dire dataless decades there is still a voice of reason.

LQG people are amazing. after 20 years without being able to find the Hamiltonian constraint they still feel superior to string theorists and point at their lack of progress.

17. **Marcus**
November 12, 2010

chris,
Please try to keep your comments reality-based 😀

LQG people are amazing. after 20 years without being able to find the Hamiltonian constraint they still feel superior to string theorists and point at their lack of progress.

I don’t see Loop researchers “pointing the finger” at stringers. They have plenty to do in their own field, and some temporarily cross over now and then if they see an interesting stringy problem to tackle. Can you cite some recent instance of finger-pointing?

There may or may not be some inner feelings of good fortune, or of having a superior rate of progress, but it’s hard to tell inner feelings. Every Loops conference so far has had a plenary talk by a string theorist. Could be their respect and openness is good policy.

The progress in Loop just in the past three years 2008-2010 has been remarkable. Approaches have converged and what has emerged is looking very much like coherent theory ready for testing.
See for example 1004.1780 and 1010.1939

Something that could, in fact, be falsified by astrophysical observation. But I think it would be naive to start making bets at this stage. My attitude is to keep my eyes open, watch the scene develop, and refrain from trying to pick winners. Emotional/partisan statements tend to sound stupid to me.

18. **Marcus**
November 12, 2010

I forgot. Loops 2009 in Beijing broke that pattern. They didn't have a plenary talk by someone from the string community at that conference. Otherwise it's been a regular custom starting in 2005 with Robbert Dijkgraaf. Just a little thing–may not seem very important to you if you have a "two-camps" mentality.

19. **Bob**
November 13, 2010

Marcus, no-one in the field will take the Mielczarek et al calculations seriously – from what I understand even among the LQC cohort he's considered a joke.

20. **Moore**
November 14, 2010

Horgan published a brief update to the End of Science in Discover magazine a few years ago for those interested:

http://tinyurl.com/323dfub

No, he doesn't really give any ground; yes, he references this blog.

21. **stavrogin**
November 14, 2010

marcus cites two of rovelli's papers as a proof of how a coherent picture is emerging in LQG. These papers have absolutely nothing to do with hamiltonian constraint of LQG. These works are summerising recent progress in spin foam models whose relation to canonical LQG to put it modestly, quite overhyped.

22. **Marcus**
November 14, 2010

Bob, what you posted is absurd. Mielczarek is not the lead author of the paper I mentioned, which follows in a line of some half-dozen by Barrau and Grain. Barrau is the senior author. Grain started out working with him when he was a PhD student as I recall.

The Barrau Grain papers are taken seriously, and cited, by LQC folks last time I checked. You can probably find a citation by Ashtekar.

The third author Cailleteau of the paper I mentioned is I believe a PhD student of Barrau, or postdoc working with him.

I don't remember seeing Mielczarek's earlier work (before this collaboration) cited in Loop papers, so can't offer an opinion, but it is irrelevant since this paper is out of Barrau's group.

Barrau gave a talk on this at ICHEP 2010 in Paris this summer.

Here is a 7 page paper on the topic published by Physical Review Letters if you want a sample:
http://arXiv.org/abs/0902.0145
Cosmological footprints of loop quantum gravity

Have to say, Bob, that your post strikes me as dishonest to the point of being mildly disgusting. Thanks for the opportunity to reply.

23. **Marcus**
November 14, 2010

Stavrogin,
What I said was *Approaches have converged and what has emerged is looking very much like coherent theory ready for testing.
See for example 1004.1780 and 1010.1939*

There has been considerable convergence of spinfoam LQG with the older canonical version. This is detailed in the two papers I mention here, which constitute a brief status report. The application to cosmology is now being based on the spinfoam ("path integral") formulation as you can see from recent papers by Ashtekar, Rovelli and others.

I haven't seen any claims that the convergence is complete or that there is an exact equivalence. Why don't you see if you can find some statement about it in either of those two papers which you would call "hype"? It has been an important trend, to which Lewandowski's work has contributed a lot, but the statements reporting on it that I have seen in the research literature have been amply qualified and rather cautiously worded. See if you can find a sample of "hype" to show us.

I think the main point to make is that the picture with (esp. spinfoam) LQG and its application to cosmology has changed significantly in the past couple of years and that there is a robust implication for cosmology that looks testable. (See Bee Hossenfelder's comments earlier.)

24. **Peter Woit**
November 14, 2010

Bojowald has a new paper out this evening claiming something about falsifiable predictions about the CMB based on LQC. It's

http://arxiv.org/pdf/1011.2779

and it's hard to compare its claims to those of the recent paper Marcus referred to, since Bojowald doesn't refer to it.

I agree with Marcus's general evaluation of comments like that of "Bob", and had thought of deleting that comment. Please, do not use anonymity to engage in personal attacks.

I took a quick look at the new Bojowald paper, but it looks like it would take a lot more time to figure out whether there's a there there than I have the willingness to devote to such a project. By now there's a long history of claims about quantum gravity imprints in the CMB. Some of the first I paid attention to were those of my colleague Brian Greene and his collaborators ten years ago

http://arxiv.org/abs/hep-th/0104102

I'm afraid I'm still a skeptic about all efforts of this kind. The information so far read off of the CMB is impressive, but corresponds to relatively low-energy physics, very far from telling us about higher energies scales, much less the Planck scale.

25. **Terra**
November 17, 2010

The proliferation of QG models in which we can actually compute things (that includes strings, LQG, and a number of other approaches) has lead us to believe that quantum gravity respects unitarity — since we've got no idea how we'd do quantum calculations without unitarity, we're like a bunch of drunks searching for our keys under the streetlight, because that's where the light is.

(Although most people work with orthogonal basis sets, unitarity is the property that's actually needed for a basis set to work… It's very possible to work with phase space representations that aren't orthogonal, although you open up another can of worms when you do that.)

Anyhow, if you believe in unitarity, there's no information loss in a black hole, no singularity in a black hole, and no singularity at the beginning of the universe…. These results hold up whatever model you're using.

Paul Ginsparg wrote an editorial where he pointed out that we don't have any proof that QG (in the real world) respects unitarity — all of the QG approaches we have now are still castles in the air. On the other hand, if you believe in unitarity, you get such wonderful results that it's hard to give up.

Of course, that's the tragedy of modern physics; the interior of black holes is one of the most exciting things right now, but nobody will ever get to go inside one, see what it's like and tell us the tale. 😉

This and That

November 9, 2010
Categories: Experimental HEP News, Langlands, Uncategorized

- There's a new preprint here explaining the scientific case for running the Tevatron past 2011. A couple weeks ago the P5 subpanel came out with its report on the subject, generating news stories "Momentum builds for Tevatron extension" and "Panel Wants U.S. to Chase 'God Particle'—If There's Money". The panel recommended extending the Tevatron run, but only if another $35 million/year in additional federal funding was provided to do it. Prospects for this are very unclear, with the next stage in the process a decision about whether to include this in the President's DOE budget request for FY2012, due next February.

The US for a long time now has operated under a bizarre budgeting system, where government agencies typically spend much of the time operating without a budget. FY 2011 began October 1, with the Congress still far from having come up with a FY 2011 budget, instead operating the government on a series of "continuing resolutions", which allow spending at the FY 2010 level. The latest of these expires December 3, after which there will undoubtedly be another one. Sooner or later an "omnibus bill" setting the actual budget will presumably get passed and one might think the result would correspond to the levels set by the appropriation committees of the House and Senate (which contain an increase of FY 2010 levels). Every other year though is an election year, so the Congress that had left town for weeks to campaign comes back after a post-election rest as a lame-duck organization, with lots of its members pushing for delaying everything until next year if their side did well in the election. This year the Republicans did very well at the polls arguing that the government spends too much money on "discretionary" things. Unfortunately for HEP, it's in the relatively small "discretionary" part of the budget. There will undoubtedly be a strong push from the Republicans to not wait for FY 2012, but to start cutting this year's budget, in the middle of the fiscal year. Given the dysfunctional nature of the US political system, it's anybody's guess how this will turn out. For comments from the Fermilab director about this situation, see here.

- One organization that doesn't have to worry about federal funding is FQXI, which was initially funded by the Templeton Foundation with a grant that was supposed to take them through the end of last year. I don't know where their money is coming from these days, but they have recently announced a new essay contest carrying $40,000 or so in prize money (the Gruber Foundation is one sponsor), on the topic "Is Reality Digital or Analog?".

Among other FQXI activities, next August its members will go on a cruise during which they will discuss foundational questions related to the nature of time.

- The LHC proton-proton run has ended for 2010, with an integrated luminosity of about 45 inverse picobarns. The plan is to restart around February 4 and collide protons for 9 months in 2011. It's possible that the energy of the beams will be raised from 3.5 TeV to 4 TeV. Detailed plans will be made at workshops at Evian and Chamonix (January 24-28). For recent news and some idea of long term plans, see this talk at the US LHC users organization meeting. It has the LHC shutdown to replace splices from December 2011-March 2013, and another long shutdown for a luminosity upgrade in 2016. In the very long term, there are discussions of upgrading the machine with new, higher field magnets that would allow operation at 16.5 TeV/beam, but this is probably about 20 years off…
- The theorists at CERN have been retreating, see here.
- There's an interview with Steven Weinberg in Scientific American, see here.
- Nature Physics has a review by Eva Silverstein of the Yau/Nadis book (that I discussed here).
- See Dennis Gaitgory's website here for some notes in progress on geometric Langlands.

Update:
One more: An interesting diavlog between Lee Smolin and Robert Wright at the Big Questions Online site.

Update: Tommaso Dorigo has a critical discussion of the paper making the case for an extended Tevatron run.

Comments

1. **Seth Thatcher**
   November 9, 2010

   Peter,
   Physics is my hobby, but politics is my area of expertise. Our system of budgeting is strange indeed. I am pleased with the direction of the mid-term elections and I expect to see spending dramatically decreased going forward. But, politicians have a way of disappointing. I write this post, however, to put forth the idea that funding the Tevatron is no run of the mill congressional earmark. It is one of the truly necessary and fruitful federal expenditures. How can we leave real science unfunded or underfunded when there is so much good that can come from it. Also, there is much pseudo-science being generated these days as you point out regularly. Let's not let crackpot science get any better foothold than it already has. In my humble opinion, we should give the Tevatron the funding it seeks. Let's find the Higgs first (if it exists)! A little scientific competitiveness is a good thing.

2. **bmenrigh**
   November 10, 2010

   @Seth

   I agree and I'm sure most readers on this site agree. It seems unlikely that Congress will agree though…

   Let's hope they do!

3. **DB**
   November 10, 2010

   Killing off the Tevatron is part of a bigger picture: putting the nail in the coffin of any grandiose plans that may exist to build expensive new accelerators on US soil.

   Killing off Constellation served a similar purpose in relation to NASA.

   However, the US will remain the world leader in String Theory, of that you may be certain.

4. **Kris Krogh**
   November 10, 2010

   The interview with Lee Smolin was refreshing, particularly his comments on time. His colleague, Carlo Rovelli, argues it is only an illusion and has claimed for years that loop quantum gravity will solve the "problem of time" by making time go away completely. (Reminds me of Frank Wilczek's remark that "String theory is promising. And promising, and promising.") Smolin takes the opposite (less fashionable) position that time may be something we need to accept as truly fundamental.

5. **Shantanu**
   November 11, 2010

Peter, any comments/interesting stuff about the conference
http://www.pirsa.org/C10023
which has talks by Weinberg (among others)?
shantanu

6. **Verified Armonyous**
November 11, 2010

Wow, no exclamation marks to spare for the outstanding performance of the LHC…A cold status report that would make jump any hypothetical student of yours into willing to work on high-energy Physics!

7. **Peter Woit**
November 11, 2010

VA,

Given that the projected total luminosity for 2010 was 100 inverse picobarns and they ended up with 45 or so, I don't think that a large number of exclamation points are really justified. They did get peak luminosities above their goal, and prospects for reaching their goal of 1000 inverse picobarns next year look good, but that's next year. I hope things work out well and they manage to reach that goal and more.

As for whether students should be excited about the future of HEP and go into it, that depends not on how much data the LHC produces, but what's in it. The time for exclamation points will be when there's a real non Standard-Model signature (or at least a solid SM Higgs signal).

8. **Peter Shor**
November 12, 2010

I can give my view about the topic in the essay contest: Reality is quantum, which is both (and neither) digital nor analog, in the same way that an electron or a photon is both (and neither) a wave and a particle. I probably won't enter it, though.

9. **Shantanu**
November 13, 2010

Peter, any comments about the Miniboone result which may be confirming LSND result? Is this an exciting development for
particle physics?
http://arxiv.org/abs/1007.1150

10. **Peter Woit**
November 13, 2010

Shantanu,

No, I think you need a statistically more significant difference from the Standard Model (with neutrino masses) to get excited.

Massive

November 10, 2010
Categories: Book Reviews

There's a wonderful new book about particle physics that has just come out, [Massive:The Missing Particle that Sparked the Greatest Hunt in Science](), by Ian Sample, who is a science correspondent for the Guardian. The topic is the huge open question currently at the center of particle physics: is the Higgs mechanism the source of electroweak symmetry breaking (and, at the same time, the source of the mass terms in the Standard Model)? The Tevatron and the LHC are now in a race to either detect the Higgs particle or rule out its existence, with one alternative or the other very likely to come through within the next few years.

Truly explaining what the Higgs mechanism is can only be done with mathematics and physics background far beyond that expected in a popular book, but *Massive* makes a good try at it. Sample does a wonderful job of telling about the history behind this subject. He's the first writer I know of who has gotten Peter Higgs to tell his story in detail. The original paper on the subject by Higgs was rejected by Physics Letters, but ultimately published by Physical Review Letters. There's a complicated priority issue one can argue over and that someday soon a Nobel committee may need to resolve, involving Higgs, Englert, Brout, Guralnick, Hagen and Kibble. My personal opinion is that it was condensed matter theorist Philip Anderson who first understood and described the Higgs mechanism, quite a while before anyone else.

Sample's book is full of wonderful stories about particle physics, and alludes to some that he can't give the details of:

> On June 8,1978, Adams marked the achievement in extraordinary fashion. He jotted down a poem about Rubbia and Van der Meer's efforts and sent it out as a memo. The poem — too offensive to reprint here — suggested that Rubbia had exploited van der Meer's brilliance to further his own career.

The footnote to this says that the memo is in the CERN archive, dated June 8,1978 and entitled "Approval of ppbar facility".

One of the later parts of the story involves the discussion of Higgs rumors on particle physics blogs, and debates among the experimental collaborations about this. With a little bit of luck, we may hope to see more of this soon.

All in all, the book is a great read, by far this year's best popular book that could be recommended to lay people who want some idea of what's going on in particle physics now and why it is exciting.

Comments

1. **Nameless**
   November 10, 2010

   Here's the poem (I'm not sure what's offensive about it):

   Clever Simon met a Bagman,
   Leader of a Team.
   Said Clever Simon to the Bagman
   I can cool a beam.
   Bagman said to Clever Simon
   I can use this scheme.
   By being first and talking faster
   Bigman I will be.
   Clever Simon is Simon van der Meer and Bagman is Carlo Rubbia.

2. **More Nameless**
   November 10, 2010

   The above poem is correct. Beaten to the post by Nameless!
   Adams also said in public that Rubbia was the 'entrepreneur' of the ppbar project and that Rubbia was a 'CERN staff member and well known transatlantic commuter'. This was deemed extremely offensive and Adams was forced to apologize for the above, in August 1978.

   Sir John Adams was a master accelerator builder at CERN in its early days.
   He died in 1984, just at about the time the ppbar project came to fruition, producing the W and Z and making Rubbia a Nobel Prize winning Bigman.
   http://sl-div.web.cern.ch/sl-div/history/sirjohn.html

3. **cyd**
   November 11, 2010

   > My personal opinion is that it was condensed matter theorist Philip Anderson who first understood and described the Higgs mechanism, quite a while before anyone else.

   Anderson's priority should obvious to anyone who has any knowledge of the subject. I'm surprised you seem to think there's any sort of controversy.

4. **Hamish Johnston**
   November 11, 2010

   It would be fantastic for Bell Labs to have two double Nobel winners! Go Phil!

5. **Narcissus**
   November 11, 2010

   "… the huge open question currently at the center of particle physics: is the Higgs mechanism the source of electroweak symmetry breaking (and, at the same time, the source of the mass terms in the Standard Model)?"

   It's not much of an "open question" if people are just searching for a Higgs boson in connection with electroweak symmetry breaking and particle mass! If Higgs is "the only game in town" on this subject, then it's a "closed question". Does the book give any discussion for alternative theories of mass? Does the book seriously discuss the physics implications if the *broken* (observed) electroweak "symmetry" is broken at all energies, i.e. if the speculative electroweak unification at 246 GeV (or whatever) doesn't occur? Does it clearly point out that the existing tests of the Standard Model with regard to electroweak unification merely validate the need for gauge boson mixing (not unification) of U(1) hypercharge and SU(2) isospin to produce the required vector bosons? The idea that electroweak symmetry may exist is much analogous to the "beautiful" mathematical idea of Plato that atoms must be regular geometric solids, or else God has missed a good idea. Well, God missed Plato's good idea. Until there is some evidence for electroweak unification beyond dreams, we should not limit our theories to one guesswork idea.

6. **Bugsy**
November 11, 2010

Adams died at 64; von Neumann and Feynman also died young. I have always wondered if exposure to radiation had anything to do with that….
maybe things weren't as safe back then as now?

7. **Seth Thatcher**
November 11, 2010

As a layman, I have been wondering why Higgs seems to get the honor when a simple reading of Wikipedia will show that others were first in theorizing the mediator of mass, if in fact it even exists. I was explaining this concept to my wife the other night as she drifted off to sleep. She made the mistake of saying something about CERN not realizing my deep interest in this subject. Well, lets just say it was like taking a sip of water from a firehose for her. I find these discussions utterly fascinating. Peter, thanks for the tip on the book and the blog.

8. **passb**
November 11, 2010

Phil Anderson's model was nonrelativistic (BCS and how the photon becomes effectively a massive field). Anderson never developed a relativistic model, although he may have made some statements that such a thing is possible. Also Anderson was (is?) terribly bigoted about HEP. Recall that Kobayashi and Maskawa were awarded the Nobel Prize, but Cabibbo was left out, despite Cabibbo clearly being first with a 2×2 matrix, and the 3×3 matrix is universally called the 'CKM matrix'. Cabibbo is a much clearer case of well-deserved priority. Many NP awards are controversial, even when the evidence seems clear.

9. **Nameless**
November 11, 2010

@cyd: anyone with real knowledge of this issue knows that extending Anderson's results from the condensed matter framework to the high energy one, namely, introducing the Lorentz group in the play, is a highly non-trivial task. This is, in fact, the reason why Anderson didn't do this himself.

Furthermore, Anderson's bigotry towards HEP just serves to murk the waters even more: if he ever made claims that this issue would be easily solved in HEP, he was wrong.

Aside from that, there's a very nice read on this topic, by one of its founders (Guralnik),

» "The History of the Guralnik, Hagen and Kibble development of the Theory of Spontaneous Symmetry Breaking and Gauge Particles", http://arxiv.org/abs/0907.3466

10. **Peter Woit**
November 11, 2010

Nameless,

One justification for Anderson's bigotry toward HEP might be the way HEP theorists handled references to his work. His 1963 paper clearly and explicitly laid out the physics of the Higgs mechanism (he claimed that this was independent of the issue of relativistic vs. non-relativistic, and was right). Among the 1964 papers, Higgs referred to Anderson, the rest didn't. Guralnik explains how he and collaborators dealt with this:

"At the same time, Kibble brought our attention to a paper by P.W. Anderson…. We did not change our paper to reference the Anderson work."

11. **Nameless**
November 11, 2010

@Peter,

For completeness' and context's sake, here's the full quote that you referred to (above), straight from the Guralnik paper I cited:

> At the same time, Kibble brought our attention to a paper by P.W. Anderson [26]. This paper points out that the theory of plasma oscillations is related to Schwinger's analysis of the possibility of having relativistic gauge invariant theories without massless vector particles. It suggests the possibility that the Goldstone theorem could be negated through this mechanism and goes on to discuss "degenerate vacuum types of theories" as a way to give gauge fields mass and the necessity of demonstrating that the "necessary conservation laws can be maintained." In general these comments are correct. However, as they stand, they are entirely without the analysis and verification needed to give them any credibility. These statements certainly did not show the calculational path to realize our theory and hence the unified electroweak theory. It certainly did not even suggest the existence of the boson now being searched for at Fermi lab and LHC. The actual verification that the same mechanism actually worked in non-relativistic condensed-matter theories as in relativistic QFT had to wait for the work of Lange [28], which was based on GHK. We did not change our paper to reference the Anderson work.

So, it's not that GHK simply ignored or hand-waived Anderson's work; quite the opposite, as Guralnik clearly explains above.

The bottom line is that you have to deal with PCT and Lorentz symmetries, and also Goldstone's theorem; and none of these play a role in condensed matter.

Thus, things are not as straightforward as they're made to sound…

12. **Nono**
November 11, 2010

> maybe things weren't as safe back then as now?

I don't know whether von Neumann or Feynman where significantly exposed to radiation, but Madam Curie and Fermi, to quote just two examples, were, and paid with their lives for it. Things were definitely not safe back then.

13. **Peter Woit**
November 11, 2010

Nameless,

After reading Guralnik's justification for not referring to Anderson, I went back and took a look at Anderson's paper. It's remarkably modern, with his understanding of the Higgs mechanism much as it's explained in textbooks today. I find Guralnik's excuses for not referring to it kind of ridiculous. It's rather rich that he and his co-authors have spent much of their lives complaining that other people don't refer to their work on this given the way they treated Anderson. I have no idea whether Anderson cared at all about this, but he would have been fully justified in being unhappy with their behavior.

14. **anon.**
November 11, 2010

The bottom line is that you have to deal with PCT and Lorentz symmetries, and also Goldstone's theorem; and none of these play a role in condensed matter.

Since when does Goldstone's theorem not play a role in condensed matter? Goldstone bosons are everywhere in condensed matter: phonons, magnons, ….

15. **Anonymous**
November 11, 2010

HEP is much more popular with mass media and attracts more bright young students. These are enough reasons to make condensed matter theorists feel bitter and under-acknowledged.

16. **famelessnameless**
November 12, 2010

Nameless:

Whatever Guralnik says, that's not an excuse to not even reference the paper! He could have referenced it, and could have pointed out whatever he wrote in the para of his that you quoted.

Not referencing such a paper once you're aware of it, and which is clearly doing something directly connected and useful to your own paper, is just plain dishonesty.

17. **beta**
November 12, 2010

It puzzles me slightly (i.e. I don't really lose any sleep over this since I have no aspirations to a Nobel) that everybody (i.e. you) discusses the Higgs only in the context of spontaneous symmetry breaking (and Goldstone bosons). That may be its immediate raison d'etre, but I see the Higgs is the culmination of a story that goes back to Ernest Rutherford. After the discovery of radioactivity by Becquerel, Rutherford found that there were three types of radioactivity, termed alpha, beta and gamma rays. This was circa 1899, and to this day we continue to seek the full explanation of beta decay. It is a great story of scientific advance, with many twists and turns, which have greatly altered our understanding of Nature at the most fundamental level. The Higgs boson is the culmination of a century-long quest. Does one doubt that the quest is worthy?

And yes, it is a great story, and makes for excellent press! HEP does appeal to the mass media, and schoolchildren, and for good reason. It has a central thread, which is easy to state and is obviously worthy, that of seeking to understand Nature at its most basic. And by that I mean not only particles but also the nature of space and time, and all this spills over into efforts to understand the Universe (or the Multiverse if you prefer). Condensed matter (theory or expt) has no such central thread.

18. **A.J.**
November 12, 2010

Feynman was nearly 70 when he died. That's close to the average for a male of his generation.

Presumably Bugsy meant to write "Fermi".

19. **jpd**
November 12, 2010

"Adams died at 64; von Neumann and Feynman also died young"
64 is the new young?
i feel better already

20. **46**
November 12, 2010

I'm on the way out!

21. **Mark Decker**
November 12, 2010

If the Higgs doesn't exist afterall (which is my bet) then the electroweak theory of Glashow, Salaam and Weinberg goes down the tubes. Then it's time to look at other theories. I know John Moffat has published one (sorry don't have link handy) as I am sure others have too.

22. **tubes**
November 12, 2010

GSW does NOT go 'down the tubes'. Remember that weak neutral currents have been observed, the W and Z have been found, numerous branching ratios have been measured (neutral to charged currents?), all in agreement with the GSW theory. These aspects of the theory will survive.

BUT if indeed the Higgs is not found (or rather is ruled out in a large area of parameter space), THEN one begins to search for alternative explanations for the existence of massive bosons. The alternative answer may not be a gauge theory (who knows?) but it must reproduce weak neutral currents and branching ratios etc.

Remember that the discovery of the photoelectric effect, and the failure of the wave theory of light to explain it, did not mean the wave theory went down the tubes. (Ok, you have to define a tube.) Instead the wave theory was woven together with the particle theory of light. More precisely, the discredited corpuscular theory of light was resurrected in a manner compatible with the wave theory.

23. **Nameless**
November 12, 2010

@anon. (November 11, 2010 at 7:54 pm): The constraints imposed by Lorentz symmetry and the Spin-Statistics theorem (PCT) render Goldstone's theorem quite a different beast in High Energy (compared to its Condensed Matter application) — I didn't mean to imply anything else other than this.

Indeed, it is these intricacies (Lorentz symm, PCT, Spin-Stat, etc) that render High Energy quite a different beast than Condensed Matter, even though most techniques can be applied in both. See, e.g., Goldstone Theorem in Nonrelativistic Theories (note that R. V. Lange worked closely with G. Guralnik — and is, thus, cited by Guralnik).

Case in point: this discussion about Anderson's claims in this topic of the "Higgs".

24. **Nameless**
November 12, 2010

@PWoit: i disagree: GHK was the only paper that counted the degrees-of-freedom in the problem of symmetry breaking: Higgs' first paper was classical and BE was semi-classical; GHK was the only quantum mechanical calculation.

Further, Guralnik was working with Lange and knew fairly well the issues in Condensed Matter (see above response to 'anon.').

In hindsight, however, vision is always 100% (this is the only part that's truly "ridiculous"). But, if you contextualize appropriately, things are not so straightforward. For example, there is a reason why Streater only cited Guralnik in this paper (see seciton 4: "The Guralnik Model"): Spontaneous Breakdown of Symmetry in Axiomatic Theory.

So, as you can see, what i'm saying here is a more widely known fact among 'old timers'… but, somehow, this information (and history, and details) got lost through the years.

25. **Nameless**
November 12, 2010

@Mark Decker: The result presently known as "Higgs boson" is a perturbative one. Unfortunately, only very recently some non-perturbative aspects of QFT have come to light, e.g., Complexified Path Integrals and the Phases of Quantum Field Theory.

In fact, as it turns out (check the ref above), you can have symmetry breaking without the so-called "Higgs", and without destroying any of the current structure of the Standard Model (namely, QFT and Gauge Theories).

So, not so quick to draw… 😉

26. **Grumblenik**
November 12, 2010

http://en.wikinews.org/wiki/Prospective_Nobel_Prize_for_Higgs_boson_work_disputed

Look at this!
But J J Sakurai Prize 2010 awarded to all six.

27. **Peter Woit**
November 13, 2010

For some other views of the history of the Anderson-Higgs mechanism, see this

http://www.scholarpedia.org/article/Englert-Brout-Higgs-Guralnik-Hagen-Kibble_mechanism

by Kibble, and an interview with Anderson here:

http://www.aip.org/history/ohilist/23362_3.html

He describes his paper published in 1963 as based on work he did during the summer of 1962, and says:

"So it was probably completed summer '62. Very little attention was paid to it except that in fact— well, Higgs reinvented it. In some ways the particle physicists tell me had less understanding; in some ways he had more. He certainly made a real model out of it where I had only a mechanism…

about the Anderson-Higgs phenomenon, if I may use the word. In the paper that I wrote I definitely said people have been worried about the Goldstone boson in broken symmetry phenomena. The Goldstone boson is not necessary. Here is the possibility of removing the Goldstone boson, mixing it with a gauge boson, and ending up with zero mass. [should be "non-zero" maybe a transcription error]…So I think I really understood the nature of the mechanism…

It was not published as a paper in the Condensed Matter Physics. It was published as a paper in Particle Physics. Brout paid attention to it. And he and Englert two years alter produced a model of symmetry breaking, which if you'll read carefully the summary of their work that t'Hooft and Veltman give (Nobel Prize winner this year), they say that they took off very much from the Brout-Englert paper, and there's no way Brout was not perfectly aware of my work and I would be surprised if the Brout Englert paper doesn't reference it rather than Higgs or along with Higgs. So in fact it didn't fall completely on deaf ears."

Actually, Englert-Brout (why not Brout-Englert??) doesn't refer to either Anderson or Higgs, in the case of Higgs because his paper had been rejected by Physics Letters and thus not yet published.

Higgs gives his version of the history here

http://www2.ph.ed.ac.uk/peter-higgs/history.shtml

properly recognizing Anderson's prior work, but claiming credit to be the first to discuss the mode that gives the "Higgs particle". He uses the term "Anderson mechanism" to refer to what is usually called the "Higgs mechanism".

28. **Peter**
November 13, 2010

What about Ernst Stueckelberg? Wikipedia says "In 1938 he recognized that massive electrodynamics contains a hidden scalar, and formulated an affine version of what would become known as the Abelian Higgs mechanism."

In the Discussion section somebody writes: "his despite the fact that he invented the renormalization group, despite the intermediate bosons, despite covariant perturbation theory, and despite the first Abelian Higgs mechanism, in 1957, remember this is same year as BCS, before Anderson, before Brout. His lack of recognition is a notable and sad fact."

29. **MP**
November 13, 2010

I can recommend another fantastic book for laymen (of which I am one) on this subject. Deep Down Things: The Breathtaking Beauty of Particle Physics by Bruce A. Schumm

This book does a great job of explaining the Standard Model, Gauge Theory, Lie Groups, and the basics of the Higgs Mechanism at a level that should be comprehensible to anyone who remembers a little high school algebra and trigonometry.

The Anderson-Higgs Mechanism

November 13, 2010
Categories: Favorite Old Posts, Uncategorized

One reason for this posting is that exchanges in the comment section of the previous one led me to look into some history, and I found some odd and possibly interesting facts I hadn't previously known. So, part of this will just be lifting of some links from comments in the last posting.

Another reason is that while the history may seem obscure, what's at issue is the central unsolved problem of particle physics: the nature of electroweak symmetry breaking, and no excuse for thinking more about this topic should be let to pass by. The Yang and Mills work on non-abelian gauge theory published in 1954 had one huge problem: in perturbation theory it has massless particles which don't correspond to anything we see. One way of getting rid of this problem is now fairly well-understood, the phenomenon of confinement realized in QCD, where the strong interactions get rid of the massless "gluon" states at long distances (they are relevant at short distances, visible in terms of jets seen at colliders).

By the very early sixties, people had begun to understand another source of massless particles: spontaneous symmetry breaking of a continuous symmetry. If the vacuum state is non-invariant under a continuous symmetry, you expect to find one massless state in the theory for each generator of the symmetry. These are called "Nambu-Goldstone" particles, and pions provide an example (only approximately massless, since the symmetry is approximate).

What Philip Anderson realized and worked out in the summer of 1962 was that, when you have both gauge symmetry and spontaneous symmetry breaking, the Nambu-Goldstone massless mode can combine with the massless gauge field modes to produce a physical massive vector field. This is what happens in superconductivity, a subject about which Anderson was (and is) one of the leading experts. His paper on the subject was submitted to Physical Review that November, and appeared in the April 1963 issue of the journal, in the particle physics section. It explains what is commonly called the "Higgs mechanism" in very much the same terms that the subject appears in modern particle physics textbooks and notes:

> It is likely, then, considering the superconducting analog, that the way is now open for a degenerate-vacuum theory of the Nambu type without any difficulties involving either zero-mass Yang-Mills gauge bosons or zero-mass Goldstone bosons. These two types of bosons seem capable of "canceling each other out" and leaving finite mass bosons only.

All that is missing here is an explicit relativistic example to supplement the non-relativistic superconductivity one. This was provided by several authors in 1964, with Higgs giving the first explicit relativistic model. Higgs seems also to have been the first to explicitly discuss the existence in models like his of a massive mode, of the sort that we now call a "Higgs particle", the target of active searches at the Tevatron and LHC.

Anderson tells his story here:

> So it was probably completed summer '62. Very little attention was paid to it except that in fact— well, Higgs reinvented it. In some ways the particle physicists tell me he had less understanding; in some ways he had more. He certainly made a real model out of it where I had only a mechanism…
>
> about the Anderson-Higgs phenomenon, if I may use the word. In the paper that I wrote I definitely said people have been worried about the Goldstone boson in broken symmetry phenomena. The Goldstone boson is not necessary. Here is the possibility of removing the Goldstone boson, mixing it with a gauge boson, and ending up with zero mass. …So I think I really understood the nature of the mechanism…
>
> It was not published as a paper in the Condensed Matter Physics. It was published as a paper in Particle Physics. Brout paid attention to it. And he and Englert two years later produced a model of symmetry breaking, which if you'll read carefully the summary of their work that t'Hooft and Veltman give (Nobel Prize winner this year), they say that they took off very much from the Brout-Englert paper, and there's no way Brout was not perfectly aware of my work and I would be surprised if the Brout Englert paper doesn't reference it rather than Higgs or along with Higgs. So in fact it didn't fall completely on deaf ears.

Note added 5/15/2013: I've heard from Martin Veltman that at the time they were working on the renormalizability of Yang-Mills, he and 't Hooft were not aware of the Brout/Englert work, or of the general issues about the Goldstone theorem and the Higgs mechanism that Brout/Englert and others were addressing. Veltman's Nobel lecture describes the history in detail, and has nothing like what Anderson describes (neither does 't Hooft's).

Given the background Brout had in condensed matter physics and Anderson's claim that "there's no way Brout was not perfectly aware of my work", it is quite surprising that no reference to Anderson occurs in the paper he and Englert published in Physical Review Letters. It arrived at the journal June 26, 1964 and came out in an issue dated August 31, 1964. In historical talks about this given back in 1997 (available here), Brout and Englert write:

> We knew from our study of ferromagnetism that long range forces give mass to the spin waves and we were aware, from Anderson's analysis of superconductivity , of the fact that the massless mode of neutral superconductors, which is also a Nambu-Goldstone mode, disappears in charged superconductors in favor of the usual massive plasma oscillations resulting from the long range coulomb interactions in metals. Comforted by these facts, we decided to confront, in relativistic field theory, the long range forces of Yang-Mills gauge fields with the Nambu-Goldstone bosons of a broken symmetry.
>
> The latter arose from the breaking of a global symmetry and Yang-Mills theory extends the symmetry to a local one . Although the problem in this case is more subtle because of gauge invariance, the emergence of the Nambu-Goldstone massless boson is very similar. We indeed found that there were well defined gauges in which the broken symmetry induces such modes. But, as we expected, the long range forces of the Yang-Mills fields were conflicting with those of the massless Nambu Goldstone fields. The conflict is resolved by the generation of a mass reducing long range forces to short range ones. In addition, gauge invariance requires the Nambu-Goldstone mode to combine with the Yang Mills excitations. In this way, the gauge fields acquire a gauge invariant mass!
>
> This work was finalized in 1964.

Very oddly, the only reference to Anderson's work that they give (their) is to a 1958 paper of his, not to the 1963 paper which had the same conclusions as theirs, a year earlier.

Brout and Englert don't give a full model, just assume existence of a scalar field with spontaneously broken symmetry, and specified couplings to the gauge fields. Working independently, Peter Higgs in July 1964 sent a paper to Physics Letters arguing that, even relativistically, Anderson's argument worked and there is no need for massless particles in the case of spontaneous symmetry breaking with a local symmetry. This paper was published, but a paper he sent a week later in which he wrote down an explicit model (the Abelian Higgs model) was rejected. It was later submitted to (August 31, 1964) and accepted at Physical Review Letters (published in the October 19, 1964 issue), where the referee (Nambu) made Higgs aware of the Brout-Englert paper, which Higgs refers to in a footnote. The Higgs paper does refer to Anderson's 1963 paper, writing in the introduction:

> This phenomenon is just the relativistic analog of the plasmon phenomenon to which Anderson has drawn attention.

Higgs gives his version of the history here, and refers to the "Anderson mechanism", writing:

> During October 1964, Higgs had discussions with Gerald Guralnik, Carl Hagen and Tom Kibble, who had discovered how the mass of non-interacting vector bosons can be generated by the Anderson mechanism.

Guralnik, Hagen and Kibble had been working on what Higgs calls the "Anderson mechanism" and Anderson the "Anderson-Higgs mechanism", writing a paper about it for submission to PRL. Guralnik gives his version of the history here (writing about the "Brout, Englert, Guralnik, Hagen, Kibble, Higgs phenomenon", Higgs last, no Anderson), Kibble's is here. In Guralnik's version:

> as we were literally placing the manuscript in the envelope to be sent to PRL, Kibble came into the office bearing two papers by Higgs and the one by Englert and Brout. These had just arrived in the then very slow and unreliable (because of strikes and the peculiarities of Imperial College) mail. We were very surprised and even amazed. We had no idea that there was any competing interest in the problem, particularly outside of the United States. Hagen and I quickly glanced at these papers and thought that, while they aimed at the same point, they did not form a serious challenge to our work.

His explanation for why they did not refer to Anderson is:

> At the same time, Kibble brought our attention to a paper by P.W. Anderson . This paper points out that the theory of plasma oscillations is related to Schwinger's analysis of the possibility of having relativistic gauge invariant theories without massless vector particles. It suggests the possibility that the Goldstone theorem could be negated through this mechanism and goes on to discuss "degenerate vacuum types of theories" as a way to give gauge fields mass and the necessity of demonstrating that the "necessary conservation laws can be maintained." In general these comments are correct. However, as they stand, they are entirely without the analysis and verification needed to give them any credibility. These statements certainly did not show the calculational path to realize our theory and hence the unified electroweak theory. It certainly did not even suggest the existence of the boson now being searched for at Fermi lab and LHC. The actual verification that the same mechanism actually worked in non-relativistic condensed-matter theories as in relativistic QFT had to wait for the work of Lange , which was based on GHK. We did not change our paper to reference the Anderson work.

See Guralnik's paper for a detailed discussion of those points which he feels Anderson, Brout, Englert and Higgs had missed about all this. It remains true that the full understanding of how this works non-perturbatively is rather tricky, especially in the chiral, non-perturbative context that is relevant to the Standard Model. It may very well be that there is some important piece of understanding about this that has been missing and will someday lead to a final understanding of the origin of electroweak symmetry breaking.

Update: For two other recent expository articles about this subject and its history, see here and here.

Comments

1. **Louis**
   November 13, 2010

   Nice post. Other related links on topic are below.

   http://www.youtube.com/view_play_list?p=BDA16F52CA3C9B1D

   http://en.wikinews.org/wiki/Prospective_Nobel_Prize_for_Higgs_boson_work_disputed

   http://www.aps.org/units/dpf/awards/sakurai.cfm

2. **Louis**
   November 13, 2010

   Weinberg video is also related and good view.

   http://www.youtube.com/watch?v=Zl4W3DYTIKw&p=BDA16F52CA3C9B1D

3. **Peter**
   November 13, 2010

   I have to post this again (I already did in the previous thread).

   What about Ernst Stueckelberg? Wikipedia says "In 1938 he recognized that massive electrodynamics contains a hidden scalar, and formulated an affine version of what would become known as the Abelian Higgs mechanism."

   In the Discussion section somebody writes: "his despite the fact that he invented the renormalization group, despite the intermediate bosons, despite covariant perturbation theory, and despite the first Abelian Higgs mechanism, in 1957, remember this is same year as BCS, before Anderson, before Brout. His lack of recognition is a notable and sad fact."

   Does anyone know more about this? I'm always amazed "Stueckelberg" pops up so many times in priority discussions. As far as I know, Feynman is the only Famous Physicist that acknowledges his work.

4. **es**
   November 13, 2010

   My information comes from "The Second Creation" by Crease and Mann. Ernst Stueckelberg was at the University of Zurich. Wolfgang Pauli was the big cheese at nearby ETH Zurich. From what I can tell, Stueckelberg suffered from low self-esteem. Pauli was a disaster for Stueckelberg. Pauli mercilessly put down Stueckelberg's ideas. Stueckelberg published his papers in French, and in obscure journals. It is also recognized that Stueckelberg's papers were very difficult to read. It is only with hindsight that we recognize much of Stueckelberg's contributions.

   On the subject of Yang-Mills and massive gauge vector bosons, it is a matter of record that Pauli formulated models of field theories with non-Abelian local gauge invariance before Yang and Mills. But Pauli also realized that the inclusion of a nonzero bare mass term for the gauge bosons violated gauge invariance and made the theory unrenormalizable. And on this basis Pauli rejected locally non-Abelian gauge theories as valid candidates for physics. It is a matter of record that Pauli attacked C.N. Yang on this basis. Read articles by Jeremy Bernstein on the subject.

   Pauli is perhaps admired for the phrase "Not Even Wrong" but Pauli also put down much that was Not Even Right.

5. **Peter Woit**
   November 13, 2010

   Peter,

What Stueckelberg did uses a scalar field to make a gauge field massive in a different way than the Anderson-Higgs mechanism. It's generally called the "Stueckelberg mechanism", and doesn't seem to be relevant to the electroweak symmetry breaking of the standard model. For more about it, see here

http://arxiv.org/abs/hep-th/0304245

6. **Louis**
November 13, 2010

Some other interesting notes in this plot that you may have (or not) uncovered.

The 1979 winners (AS and SW) primarily worked from the GHK paper due to the completeness and proximity to each other at Imperial College London. All six Sakurai winners are referenced in the 1979 Nobel talks by AS and SW. AS mentions that Kibble "tutored" him on these concepts.

It is odd that the Brout-Englert Historical Account posted in blog does not mention GHK – why introduce anything to the history that ruins "Nobel Math" of three. So this is in-line with how they treated PA.

t'Hooft and Veltman's story has changed over time. They referenced all three 1964 PRL papers and actually refer to it as the "Higgs-Kibble Mechanism" on page one of their Nobel Paper (see link below). Since being buttressed by their Nobel they have pushed a BEH award – particularly BE.

http://igitur-archive.library.uu.nl/phys/2005-0622-155148/13877.pdf

7. **egan**
November 14, 2010

Peter, if you were a member of the Nobel Prize comitee after the discovery of the Higgs boson at the LHC, what would be your decision ?
Who deserve the prize ? And don't forget the rule : no more than 3 names 😉

8. **bjm**
November 14, 2010

"Three papers written in 1964 explained what is now known as the 'Englert-Brout-Higgs-Guralnik-Hagen-Kibble mechanism' "
http://en.wikinews.org/wiki/Prospective_Nobel_Prize_for_Higgs_boson_work_disputed

I don't know a lot about physics but I know what's funny. Should that now be the "Anderson Englert-Brout-Higgs-Guralnik-Hagen-Kibble mechanism"?

9. **aebhghk**
November 14, 2010

In its own way the 2010 Sakurai Prize reflects the prejudices of HEP. Even though Anderson's 1963 paper was published in the Particle Physics section of the Physical Review, it is nevertheless viewed as condensed matter (and the work is of course nonrelativistic).

10. **Evgeny**
November 14, 2010

Just for the reason of completeness I would like to mention
the paper by A.A. Migdal and A.M. Polyakov in
Sov.J. -JETP 24 , 91 (1966)
"Spontaneous Breakdown of Strong Interaction Symmetry and the Absence of Massless Particles"

11. **Peter Woit**
November 14, 2010

egan,

In some sense the right Nobel for that has already been given (Weinberg-Salam), but if one wanted to go further back, I think Anderson + Higgs would be justifiable, Anderson for discovering the Higgs mechanism, Higgs for first writing down a relativistic theory implementing the Anderson mechanism and noticing that it had a physical mode that would be the Higgs particle.

But, in any case I'm hoping that what the LHC discovers involves some much more interesting mechanism for electroweak symmetry breaking than a Higgs field.

12. **Toma Susi**
November 14, 2010

Would make an awful acronym.

13. **Paul Klavan**
November 14, 2010

nice post peter–'tis why this is one of the best science blogs out there.

speaking of blogs, anderson should have blogged his work in 1962!

all kidding aside, i imagine the internet, and the numerous forums/blogs/archives will help us prevent such controversy in the future.

the knowledge that one cannot easily get away with forgetting to reference work will work wonders, as well as the permanent record via blogs/arxiv/groups/forums/facebook/blogs, all of which are dated and the vast majroity of which can be trusted, as it would be impossible to hack a facebook date!

thoughts? comments?

also, it appears journals are becoming less relevant, with their long delays vs. the immediacy of the internet.

14. **book**
November 14, 2010

FWIW there's a whole book on this stuff (reprints of papers). The papers by Anderson (1958, 1963), Nambu, Schwinger (2D electrodynamics) are all there. Migdal-Polyakov also. But the papers by Higgs, Brout-Englert and Guralnik-Hagen-Kibble are not reprinted. I suppose the book is about "dynamical" symmetry breaking ~ BCS superconductivity? ~ although in the HEP context I have no idea what would be a condensate bilinear in fermion fields.

http://books.google.com/books?id=1BSj1N3IMP8C&printsec=frontcover&dq=farhi+jackiw&source=bl&ots=kr-zPCoU8D&sig=nSqxex5tjf69WONZdvpukMl0n8g&hl=en&ei=L0fgTO37EoH6swOtgMmICw&sa=X&oi=book_result&ct=result&resnum=1&sqi=2&ved=0CBMQ6AEw

15. **Louis**
November 14, 2010

They used to be posted here but seem to be removed.

http://prl.aps.org/50years/milestones#1964

16. **N. Nakanishi**
November 14, 2010

Comment 1
I believe that the first person who pointed out that a gauge boson can acquire a nonzero physical mass in the relativistic quantum field theory is J. Schwinger. He solved 2-dimensional massless QED, called the Schwinger model; the gauge field becomes massive (PR 128(1962),2425). At that time, this was called Schwinger mechanism, but it turns out to be nothing but the dynamical Higgs mechanism.

Comment 2
Higgs did not resolve the dilemma between the Goldstone theorem and the Higgs mechanism. He merely avioded the
contradiction by adopting Coulomb gauge. The Goldston theorem holds in the manifesly covariant field theory, and the requirement of manifest covariance is very important backbone of the Standard Theory. I emphasize that the Nambu-Goldstone boson does exist in the electroweak theory.
It is merely unobservable by the subsidery condition (Gupta condition). Indeed, without NG boson, the charged pion could not decay into muon and antineutrino (or antimuon and neutrino) because the decay through W-boson violates angular-momentum conservation.

Comment 3
I know that it is a common belief that pion is regarded
as an "approximate" NG boson. But it is quite strange to regard pion as an almost massless particle. It is equivalent to regard nuclear force as an almost long-range force! The chiral invariance is broken in the electroweak theory. And as I stated above, the massless NG boson does exist.

17. **pidecay**
November 14, 2010

I would agree about Schwinger and his conclusions based on his model. GHK stated that in the Lorentz gauge a NG boson would exist but it would be "pure gauge" and not observable. (Higgs did not say this, in his 1964 paper anyway.)

The decay of a charged pion principally into (muon, mu_antineutrino) and not (electron, e_antineutrino) has long been regarded as a demonstration of the V-A structure of the weak interactions. The resulting helicity suppression is the reason that the branching ratio for the decay to (electron, e_antineutrino) is so small, despite having a much larger phase-space. "Indeed, without NG boson, the charged pion could not decay into muon and antineutrino … the decay through W-boson violates angular-momentum conservation." ??

18. **N. Nakanishi**
November 15, 2010

Explanation on Comment 2
Pion's spin is zero, while W-boson's spin is one. People usually understand that the pion decays into a muon and a neutrino through an intermediate state consisting of one W-boson. But this is forbidden by the angular-momentum conservation law in the rest frame of the pion. Note that conservation law (
possibly except for energy because it is canonically conjugate to time) must hold even in intermediate states.

19. **pidecay**
November 15, 2010

No good. But I shall let the bigshot experts deal with it, if anybody cares.

20. **Chris Oakley**
November 15, 2010

> But, in any case I'm hoping that what the LHC discovers involves some much more interesting mechanism for electroweak symmetry breaking than a Higgs field.

Amen to that. Has it occurred to anyone that all this scrabbling for credit for the Higgs mechanism might turn into scrabbling for the exit if the Higgs particle is not found?

21. **nohiggs**
November 15, 2010

"… scrabbling for the exit …" There's probably a Monty Python sketch for that. More likely there will be a generous attribution of credit to the *other* person. For example the Higgs may be renamed the Kibble, or the Brenglert, or all the HEP theorists will join forces to point the finger at Anderson. But it will he hard to conclude that the Higgs does not exist simply because it is not found. The mass limit (with 95% confidence) will be pushed up. Eventually the limit may reach outrageous levels, but that may require more than the LHC.

22. **chris**
November 15, 2010

Regarding Schwinger, there is one big problem in the argument that he discovered the dynamical symmetry breaking mechanism we usually call Higgs mechanism: in 2D there is no spontaneous symmetry breaking (Mermin-Wagner theorem). His paper is a well known landmark and recognized for its contribution towards understanding the confinement phenomenon and effective field theories (bosonization). On the other hand, it is also well known, that it misses one of the most prominent features of 2D QED (and 4D QCD), the theta-vacuum structure, entirely.

The Schwinger model is an early toy model study of QCD. I guess that therefore – by a very large leap of faith – one might even call it a grandfather in spirit to technicolor theories and therefore an early precursor study of a dynamical Higgs phenomenon. (This notwithstanding that the fact that the condensate one would like to associate with the Higgs is simply not formed in 2D because that would violate the Mermin-Wagner theorem) . But attributing to it the discovery of the Higgs mechanism is way beyond reasonable argument.

23. **erhard**
November 15, 2010

In all this discussion about the early years of the Higgs et alii "mechanism" we should not forget that another link was searched for, once in a while, by considering a scale covariant scalar field as a bridge to gravity, e.g.:
F. Englert, E. Gunzig, C. Truffin, P. Windey 1975 (Conformal invariant general relativity with dynamical symmetry breakdown,Phys. L. 57B, 73ff.) tried a link to Jordan-Brans-Dicke theory.
L. Smolin 1979 (Towards a theory of spacetime structure at very short distances, Nucl. Phys. B 160, 253ff.) started to play around with Weyl geometric gravity of the Dirac-Utiyama type.

Every ten to fifteen years similar ideas seemed to have popped up, but remained marginal annotations to the main discourse. If something came out of these attempts, would n't that be already a bit "more interesting" than the ordinary Higgs field; or do you expect even more, Peter?

24. **Peter Woit**
November 15, 2010

Erhard,

Well, if somehow the origin of electroweak symmetry breaking has to do with gravity that would be truly spectacular, one certainly couldn't expect "even more" than that.

25. **N. Nakanishi**
November 15, 2010

chris,

Your assertion that in 2D there is no spontaneous symmetry breaking is not valid in the indefinite-metric quantum field theory. The specialty of 2D is the non-existence of the 2-point funtion of a massless scalar field in the Hilbert space, but it does exist in the indefinite-metric space.

The theta vacuum business appears in the massive Schwinger model but not in the massless Schwinger model. Furthermore, the appearance of the theta vacua depends on the choice of the representation; it is possible to avoid the theta vacua.

I of course do not regard Schwinger as the discoverer of the Higgs mechanism. He is a precursor. Probably, Anderson should also be regarded as a precursor.

26. **chris**
November 16, 2010

Then please tell me the symmetry that gets broken dynamically in order to give the Schwinger particle its mass.

27. **N. Nakanishi**
November 16, 2010

chris,

The manifestly covariant formulation of the Schwinger model contains massless ghost particles. Gauge symmetry is broken so as to give a mass to the gauge particle. The ghost particle, which is unphysical because of the subsidiary condition, plays the role of the Goldstone boson.

The fact that Schwinger mechanism is nothing but a dynamical Higgs mechanism was shown by
K. R. Ito, Prog. Theor. Phys. 53 (1975), 817,
N. Nakanishi, Prog. Theor. Phys. 54 (1975), 840.
For a review, see N. Nakanishi and I. Ojima, Covariant Operator Formalism of Gauge Theories and Quantum Gravity,
(World Scientific, 1990), Sec.2.5.4.

28. **chris**
November 17, 2010

I am sorry, but there is no dynamical symmetry breaking in the Schwinger model. In your work, you explicitly broke the gauge symmetry by a gauge fixing condition. Of course ghosts will arise then – but they are unphysical.

The mass of the Schwinger boson is due to the anomaly term. This is not symmetry breaking, there was no symmetry to begin with.

29. **N. Nakanishi**
November 17, 2010

chris,

You seem not to understand the fundamental points of the quantum theory of a gauge field. Let me explain. Any classical theory which has local symmetry cannot be quantized without violating it by introducing gauge fixing. Instead, the theory should become BRS invariant; the BRS symmetry is the quantum version of local gauge symmetry. For the abelian gauge theory, FP ghosts decouple, so only the B field remains.
Although local symmetry does not exist in quantum gauge theory, global symmetry remains unbroken if spontaneous symmetry breaking does not occur. I emphasize that local gauge symmetry plays no role in the Higgs mechanism of the quantum gauge theory. Local symmetry is a classical concept!

The Schwinger mechanism is NOT an anomaly. It is not good to regard anything which you cannot understand as anomaly.
The Schwinger model has essentially the same mechanism as that of the chiral gauge theory.

30. **Shantanu**
November 20, 2010

See
http://web.mit.edu/dikaiser/www/Kaiser.WhoseMass.pdf
for an interesting historical retrospective on connection between
Higgs and Brans-Dicke theories and how people who worked on one

were completely ignorant of the other.
shantanu

31. **Greg S**
November 30, 2010

Here how I judge the three 1964 PRL papers discussed above by Peter – nice post and blog by the way.

1) Does paper have the mechanism?
2) Does paper have the boson?
3) Does paper show explicitly how Goldstone theorem is avoided?
4) Is paper accurate and error free?

Brout-Englert (PRL, August 1964)
1 – Yes, has mechanism
2 – No boson
3 – No – paper assumes Goldstone's theorem is correct throughout
4 – No. Messes up poles at top of page 322

Peter Higgs (PRL, October 1964)
1 – Yes
2 – Yes
3 – No. In his Physics Letters Paper PH stated Goldstone theorem could fail in radiation but does not show how. In PRL paper, PH does not pick a gauge nor did he not give a quantum mechanical argument to the Goldstone theorem which is purely a quantum phenomenon.
4 – Yes

Guralnik-Hagen-Kibble (PRL, November 1964)
1 – Yes
2 – Yes (bottom of p. 586)
3 – Yes. Shows explicitly how Goldstone Theorem fails in radiation gauge. This is large focus of their 1964 paper and explained in Guralnik's 2009 historical paper. Hagen also discusses on YouTube in Sakurai Prize lecture http://www.youtube.com/view_play_list?p=BDA16F52CA3C9B1D
4 – Yes (Points 1-4 are probably why Ian Sample in "Massive" (p.70) points out "some regard (GHK) as the most comprehensive version of the theory"

The papers have gone down in history together as they should. It is a shame the Nobel "rule of three" tries to segment these as TOGETHER they each brought different pieces to this and formed the basis of the standard model. Again, nice post Peter. Certainly Anderson's work was also critical to this whole phenomena and period.

A Geometric Theory of Everything

November 17, 2010
Categories: Uncategorized

The December issue of Scientific American is out, and it has an article by Garrett Lisi and Jim Weatherall about geometry and unification entitled A Geometric Theory of Everything. Much of the article is about the geometry of Lie groups, fiber-bundles and connections that underpins the Standard Model as well as general relativity, and it promotes the idea of searching for a unified theory that would involve embedding the SU(3)xSU(2)xU(1) of the Standard Model and the Spin(3,1) Lorentz group in a larger Lie group.

The similarities between (pseudo)-Riemannian geometry in the "vierbein" formalism where there is a local Spin(3,1) symmetry, and the Standard Model with its local symmetries makes the idea of trying to somehow unify these into a single mathematical structure quite appealing. There's a long history of such attempts and an extensive literature, sometimes under the name of "graviGUT"s. For a recent example, see here for some recent lectures by Roberto Percacci. The Scientific American article discusses two related unification schemes of this sort, one by Nesti and Percacci that uses SO(3,11), another by Garrett that uses E_8. Garrett's first article about this is here, the latest version here.

While I'm very sympathetic to the idea of trying to put these known local symmetry groups together, in a set-up close to our known formalism for quantizing theories with gauge symmetry, it still seems to me that major obstructions to this have always been and are still there, and I'm skeptical that the ideas about unification mentioned in the Scientific American article are close to success. I find it more likely that some major new ideas about the relationship between internal and space-time symmetry are still needed. But we'll see, maybe the LHC will find new particles, new dimensions, or explain electroweak symmetry breaking, leading to a clear path forward.

For a really skeptical and hostile take on why these "graviGUT" ideas can't work, see blog postings here and here by Jacques Distler, and an article here he wrote with Skip Garibaldi. For a recent workshop featuring Lisi, as well as many of the most active mathematicians working on representations of exceptional groups, see here. Some of the talks feature my favorite new mathematical construction, Dirac Cohomology.

One somewhat unusual aspect of Garrett's work on all this, and of the Scientific American article, is that his discussion of Lie groups puts their maximal torus front and center, as well as the fascinating diagrams you get labeling the weights of various representations under the action of these maximal tori. He has a wonderful fun toy to play with that displays these things, which he calls the Elementary Particle Explorer. I hear that t-shirts will soon be available…

Update: T-shirts are available here.

Comments

1. **Aaron**
   November 17, 2010

   Oh dear lord. Again?!?

2. **chris**
   November 18, 2010

   oh, that guy. thanks for pointing this out.

   by the way, Lisi seems to have gone into comercials lately…
   http://cheapuniverses.com/universesplitter/

3. **Peter Woit**
   November 18, 2010

   I'm deleting a sequence of comments triggered by an anonymous attack on one of the authors of the SciAm piece, Jim Weatherall. If you want to attack someone from behind anonymity here, it better be me, or it better be extremely fair, otherwise it's a good candidate for deletion.

   The exchange did contain links to something Weatherall wrote for Slate based on early Higgs rumors

   http://www.slate.com/id/2167563/

   and a blogposting he wrote about it featuring an exchange with Nima Arkani-Hamed.

   http://stevens.edu/csw/?p=42

4. **D R Lunsford**
   November 18, 2010

   I applaud this work if for no other reason than the work involved in juggling all the multiplets, although I think it is pretty clear that toying with gauge groups is not going to produce a breakthrough. The particle explorer is a fantastic toy!

   -drl

5. **anon**
   November 18, 2010

   Hmm.. Motl has a post just now about Weatherall, attacking him. He happens to have the very same two links (to Slate and the blog posting) you've retained here.

   Coincidence?

6. **Chris W.**
   November 19, 2010

   This wouldn't be the first of such "coincidences".

7. **Tristan Hubsch**
November 19, 2010

How is a mathematically rigorous disproof of a concrete proposal “really skeptical and hostile”? Facts simply **are**.

Or, are you suggesting that Distler-Garibaldi’s disproof is incomplete, (perhaps not unlike the very productive examples of some well-known past “no-go theorems”)? If so, please do share: we all might learn a great deal more.

8. **Peter Woit**
November 19, 2010

Tristan,

My understanding is that Garrett is well aware that his proposal has problems. In the Scientific American article he writes:

“All new ideas must endure a trial by fire, and this one is no exception. Many physicists are skeptical—and rightly so. The theory remains incomplete.”

I have no problem with skepticism, I’m skeptical about many of Garrett’s ideas too. If Jacques wants to make a clean technical argument showing the nature of the problems with Garrett’s proposal, that’s great, and could be potentially worthwhile. But I don’t see any reason for the hostile, sneering tone of Jacques’s blog posting explaining these points. This is not the way to professionally make a credible technical argument.

He and Lubos have done an excellent job of convincing many people that there’s something seriously wrong with string theory and string theorists. Instead of writing in here to complain about my accurate characterization of Jacques’s writings as “hostile”, you might want to think seriously about whether his behavior is worth defending, given how much it has cost your field.

9. **Garrett**
November 20, 2010

Peter: Thanks for posting on the SciAm piece.

Chris: It was an (unpaid) endorsement of a fun app, not a commercial.

D R Lunsford: Glad you like the Elementary Particle Explorer — I think it’s a fun and worthwhile educational tool.

Tristan: What is it, precisely, that you think Distler and Garibaldi proved? Their claim is that they proved “the theory doesn’t work.” But does that follow from what they technically proved, or is it in fact a lie, dependent on a hidden assumption?

Peter: Agreed. Except I see several reasons for Distler and Motl’s sneering tone, all of which speak to their character rather than that of those they malign.

10. **D R Lunsford**
November 20, 2010

I wonder if anyone remembers Feza Gursey, who was always finding suggestive patterns in the objects of algebra. I was entranced at first, and then just sort of decided that even if that was the right way, I preferred to not go crazy thinking about it, and so good luck. It seemed to me that nothing could possibly be that complex 🙂

So my point is, this goes back a long way and not much has come of it, because it doesn’t have a new dynamical principle.

-drl

11. **Casey Leedom**
November 20, 2010

I’m reminded of the old quote “Academic politics are so vicious precisely because the stakes are so small.”[1] It seems that so little has happened of real import in HEP over the last 30 years that practitioners are left squabbling for the scraps. It’s very sad. I think it was Peter who said something along the lines that when he started his career, the Standard Model had just had the finishing touches applied to it after nearly a century of bold, dramatic progress in the field of physics. Who knew that a whole generation of physics PhDs were going to see very little happen on their watch?

[1] http://ask.metafilter.com/80812/Academic-politics-are-vicious-because-the-stakes-are-so-low

12. **John Baez**
November 20, 2010

Garrett wrote:

> But does that follow from what they technically proved, or is it in fact a lie, dependent on a hidden assumption?

What do you think? I’ll guess you’re hinting it’s the latter. if so, could you be really specific about what hidden assumption they’re using, and why it’s wrong?

13. **Bugsy**
November 20, 2010

As a complete outsider, when I skimmed through some of the Distler-Lisi exchanges when Lisi’s first preprint came out, I was amazed to see Lisi keep his cool again and again while Distler consistently displayed a belittling, arrogant tone. It seemed to me that all concerned were getting something out of the exchanges, and since both were some of the very few people in the world who could possibly understand the arguments involved, why in the world would one of them seem to consistently view the other as an “enemy”? Now to prick Distler’s enormous pride a bit, if he were such a damn expert why would he need to rely on a coauthor to “do the math” for him in the new preprint? Methinks there is more behind his anger than greets the eye, some “hidden variables” of jealousy, bitterness and frustration that keep rearing their ugly heads and interfere with what should be simply a fascinating, INSPIRING intellectual debate. Lisi’s best move is to keep reading zen books for the philosophy and leave the snide remarks to others. Beauty is not only in the eye but in the heart of the observer. Nastiness in academia has driven many gentle but brilliant souls away from the ivy halls, Lisi will have to be strong inside to continue to weather all this with wistful good humor.

14. **Bugsy**
November 20, 2010

Addendum: just skimmed Lubos’ twin posts on the matter, and along with some (apparently well done) general exposition, he makes some concrete criticisms which (apparently) should be seriously addressed. I say apparently because of my own ignorance, and will now shut up and watch the discussion by those who know something unfold. I will just say to Lisi that if you write an article about your own work in a popular magazine, then you are setting yourself up for a really big fall IF your work turns out to be wrong. But that fall would be no problem at all, if all that really matters to you is what is so- in all its aspects. In skiing and surfing we fall all the time, right? The wave shows us the reality, we laugh and shake our heads, look at the amazing sky and keep on going…

15. **Peter Woit**
November 20, 2010

Bugsy,

Jacques has always been quite consistent in starting out his various blog postings on the topic by explaining that it is "mind-numbingly trivial", elementary, and intellectually far beneath him, even if he is requiring the efforts of a specialist. This is pretty funny stuff if you know something about the technicalities of the subject. If you don't know the technicalities, the humor still becomes pretty obvious when you see David Vogan writing to Jacques to explain to him why he has it wrong. For amusement, read Jacques's first blog posting on the topic:

http://golem.ph.utexas.edu/~distler/blog/archives/001505.html

The comic high point there though is not Jacques, but an anonymous string theorist who writes in:

"Nice post. It would make a good final exam question for a first course on baby Lie theory, too. (A 4th year undergrad course, these days, I should point out; something worth pondering.)"

There are some other hilarious parts of this story that unfortunately I'm not at liberty to discuss here. Over the years mathematicians have learned some beautiful and important ideas from string theory, they've also had some very amusing interactions with string theorists.

16. **Garrett**
November 20, 2010

DR: Yes, Gursey was one of the first to consider E8 for unification. I think what held people up was not knowing how to include gravity (that needed a new dynamical principle).

Casey: There have been several interesting developments in HEP, but yes, nothing as dramatic as in the previous 30 years.

17. **Garrett**
November 20, 2010

John: Yes, they are relying on two hidden assumptions:

(1) Having mirror fermions makes a theory unviable.

(2) There is no way of getting rid of mirror fermions.

The first assumption is wrong because we just don't know why particles have the masses they do, and there could be mirrors with large masses. Since what Distler and Garibaldi prove is that, under a direct decomposition of E8, there are mirror fermions, they then use (1) to claim this E8 theory is unviable. But that is a lie, based on (1), which they initially obscured. I am using this strong language because I talked to Distler in person and his strategy of obscuring (1) was made clear to me. (You might recall a lengthy and repetitive exchange on Urs' ncat post, during which Distler refused to acknowledge what was going on.) And Garibaldi was following Distler's word on the physics.

Issue (2) is more subtle, and harder to argue, since I haven't written up the precise mechanism yet. But I think what's really going on is that these mirror fermions may be related to usual fermions using an E8 gauge transformation, so that they never appear as mirrors but rather as another generation.

18. **Garrett**
November 20, 2010

Bugsy: Sage advice. But I suspect I am too gritty to completely take the high road. However, the concrete criticism has been seriously addressed. Here's a summary of criticism, and how it has played out:

(1) Impossible mixing of bosons and fermions.

The superconnection was quickly agreed to be viable.

(2) Violation of the Coleman-Mandula theorem.

The novel loophole in the theorem is addressed in the Scientific American article, and was described in more detail here. If nothing else, this new understanding opens the field for gauge-gravity unification.

(3) The dynamics is not described (with an action) invariant under the full E8 symmetry.

A couple of LQG researchers and I recently found such an action.

(4) Even one generation of fermions does not fit in E8.

This misconception, introduced and propagated by Distler and Garibaldi, is directly countered here. It was one of the more enjoyable experiences of my life to see this issue discussed in Banff with Garibaldi in attendance. I think it still pains Distler to use the standard terminology "fermions and mirror fermions," instead choosing to say "no chiral fermions," which is synonymous but misleading.

(5) The theory is not quantized.

The theory is compatible with the usual methods of quantum field theory. Although extensive renormalization calculations have not been carried out, I think they can and will be. The lack of a background spacetime does make this difficult though — a known problem with quantizing gravitational theories. In any case, the QFT of nonabelian Yang-Mills theories, and the geometry of BRST, is an extremely rich area that should be further explored.

(6) The theory does not accommodate three generations of fermions.

This issue was identified (by me) as the most serious problem from the beginning, with a potential solution coming from triality. As of a year ago I was discouraged, but with some insights gained at Banff I now think triality will indeed work. It's tricky though (for the noncompact case), and I'm working on that now.

Peter: Since Distler has been very effective at persuading physicists that there is nothing to this E8 stuff, it has been less funny from my perspective. But it has certainly been interesting. And my interactions with mathematicians have been fantastically enjoyable and informative.

19. **Mark**
November 20, 2010

Garrett, it is an established fact that your theory has a non-chiral spectrum. This is what Distler and Garibaldi rigorously proved. It has been known for 40+ years that in order to describe the real world the fermion spectrum must be chiral. Hence, unless you impose some extra symmetry that would forbid the vector-like mass terms, ALL fermions will get large GUT scale masses through quantum corrections and will automatically decouple at the scale far above the electroweak scale. Thus, the electroweak scale spectrum of your theory looks nothing like the real world. This is not a lie but a basic QFT argument that cannot be refuted just by waving your hands and saying that "The first assumption is wrong because we just don't know why particles have the masses they do, and there could be mirrors with large masses."

20. **Aaron**
November 20, 2010

Oh for christ's sake, Garrett. Do we really have to go through this again? You can call your 'superconnection' whatever you want (although you still have not acknowledged that the very BRST superconnection you cite and don't seem to understand is related to a Grassman symmetry), but that doesn't mean it makes sense. For example, are the currents that relate the fermions and bosons in your model Grassman or commuting? If they're Grassman, do they fit into an ordinary Lie algebra or a superalgebra?

For the Coleman-Mandula theorem, you need to address what happens in the low-energy theorem. In particular, what breaks the symmetry mixing spacetime and internal indices and at what scale?

For your paper with Lee, I'd be a lot more interested if it had fermions in it.

The idea of net numbers of generations has been standard in high energy physics for at least thirty years and probably longer. You're welcome to present a reason why your mirror fermions don't pair up with a high mass and disappear from the low energy spectrum, but until you do no one's going to care about the fact that you don't like the standard terminology; they're just going to ignore you.

For quantization, if it's so easy, why don't you sit down and compute something? Like the low-energy spectrum, say. If you're going to go around and talk about what particles your theory predicts at the LHC, I'd think you'd at least have the responsibility to compute a scattering amplitude or two.

Or, for that matter, what are the free parameters in your theory? What sets the Yukawa couplings? What sets the Higgs self coupling? Neutrino masses? The CKM matrix? The value of the cosmological constant? Even if you're not able to give numbers, can you at least explain the mechanism by which these parameters arise? Are they all undetermined? Are there relations between them?

And, seriously Peter. The math here is the computation of a few branching relations for the adjoint of various real forms of E8. Big fucking deal. There's probably a computer program out there that will do that for you these days. Must all these discussions end up focussing on how mean Jacques and all the other people on the internet are? We could just cut and paste from the old discussion on Cosmic Variance and save us all some time.

21. **Peter Woit**
November 20, 2010

Aaron,

Sure, the kinds of mathematical questions raised by Jacques are answerable with known mathematical techniques. But, no, they're not trivial, and only an idiot would think they'd make a good final exam question for an undergraduate course on Lie theory.

I've had extensive experience trying to have technical discussions with Jacques, just about all of which are publicly available on various blogs, so anyone who cares can read them and see for themselves what they think. Based on that experience, I can easily see why Garrett, who's a very polite, mild-mannered guy, describes what he encountered with a strong word like "lying". The problem with Jacques is not that he's mean (I hear that, like Lubos, he's a perfectly nice guy in person, the problem is just when he gets a keyboard in his hands). The problem is that he's unprofessional and dishonest. There are well-understood rules for how professionals conduct intellectual arguments (e.g. avoid ad hominem attacks, try to accurately describe your opponents arguments, etc.). Jacques and Lubos don't play by those rules. In Lubos's case it's immediately obvious, with Jacques it takes a while to figure out.

22. **mark davis tortino**
November 20, 2010

"There are well-understood rules for how professionals conduct intellectual arguments (e.g. avoid ad hominem attacks, try to accurately describe your opponents arguments, etc.). Jacques and Lubos don't play by those rules. In Lubos's case it's immediately obvious, with Jacques it takes a while to figure out."

Well, it's not an ad hominem attack to state that Garrett has never published a peer-reviewed paper on his "theory."

Nor is it an ad hominem attack to note that Distler *has* published a paper refuting Lisi's theory.

What *is* an ad hominem attack is to label the physicist publishing peer-reviewed papers as somehow being unprofessional, while cheering the one who does not publish peer-reviewed papers and receives all the "fantastic" hype as behaving professionally. What you are doing, Peter, is "attacking the man"–Distler–while ignoring his superior physics and math, which was published in a peer-reviewed journal.

23. **mark davis tortino**
November 20, 2010

Bugsy writes, "Now to prick Distler's enormous pride a bit, if he were such a damn expert why would he need to rely on a coauthor to "do the math" for him in the new preprint? Methinks there is more behind his anger than greets the eye, some "hidden variables" of jealousy, bitterness and frustration that keep rearing their ugly heads and interfere with what should be simply a fascinating, INSPIRING intellectual debate. Lisi's best move is to keep reading zen books for the philosophy and leave the snide remarks to others."

"enormous pride" is a snide, ad hominem attack.
"if he were such a damn expert" is a snide, ad hominem attack.
"methinks there is more behind his anger than greets the eye, some "hidden variables" of jealousy, bitterness and frustration that keep rearing their ugly heads " is a snide, ad hominem attack.

hey peter et al.–instead of the snide, ad-hominem attacks, why not try to refute distler's paper on a scientific, mathematical level, and then publish it like distler did?

seems distler is taking the high, professional road, while you guys are taking the snide low road…

24. **Peter Woit**
November 20, 2010

Mark,

I have no dog in the argument between Garrett and Jacques over chirality problems in what Garrett is trying to do. As I said, I'm in the camp of skeptics that Garrett has a workable unified theory. I agree that you can't automatically conclude from the fact that Jacques or Lubos argues in a dishonest and unprofessional way that they're wrong.

Again, I'm just telling you what my personal experience with Jacques is, going back now quite a few years, on several different topics. The relevant exchanges are in the public domain, if anyone cares.

25. **Tristan Hubsch**
November 21, 2010

Peter,

I'm glad that your skepticism is not as one-sided ("I'm skeptical about many of Garrett's ideas too") as it seemed to me; sorry about that: my bad. In turn, however, writing "you might want to think seriously" implies that you know that I don't. But, never mind.

Finally, thanks for a nice book; I've learned a lot about human nature.

Peace, out.

26. **John Baez**
November 21, 2010

Just for my own peace of mind, I'll try to summarize my own understanding of various problems that confront Lisi's theory, or indeed *any* attempt to pack all known particles into the adoint representation of E8.

I'll try to do this in a fairly lowbrow sort of way, so more people can understand what I'm saying. This increases the risk of oversimplifications, but doubtless I'll be corrected here if I make any mistakes, and even if I don't.

So here goes:

Most importantly, particles have very distinct personalities, whereas all 248 dimensions of E8 look alike. E8 is very symmetrical, that's why people like it. But this beautiful symmetry needs to be severely broken for anything like real-world physics to fall out.

First, particles come in two kinds: bosons and fermions. If we chop E8 into a bosonic part and a fermionic part we no longer have the whole symmetry of E8. Garrett writes:

> The superconnection was quickly agreed to be viable.

but I'm not sure anyone there agreed it was 'viable': Urs merely suggested this idea as a way forward, but as Aaron notes,

> …that doesn't mean it makes sense. For example, are the currents that relate the fermions and bosons in your model Grassman or commuting? If they're Grassman, do they fit into an ordinary Lie algebra or a superalgebra?

I see no way to get E8 symmetries that mix fermions and bosons in a model where the symmetry of E8 has been broken by deliberately chopping it into bosonic and fermionic parts.

Second, even among fermions, different particles have drastically different personalities: for example their masses, and the rates at which they turn into each other, which are described by the numbers in the Cabibbo-Kobayashi-Maskawa matrix and the Maki-Nakagawa-Sakata matrix. Similarly, in the realm of the bosons there's the Higgs mass and other numbers. Garrett's work has nothing to say about these. Without these numbers we can't do real-world physics. But the big problem is this: I don't see any way to get these numbers into the game without further breaking down the symmetry of E8. Why? Because again, E8 symmetry wants all particles to be alike, but these numbers describe how they're not.

Third, there's no way to pack all known fermions into E8 without positing two copies of E8 and giving every fermion a mysterious unseen partner called a "mirror fermion". To keep these rascals from being seen we could claim they're more massive than the guys we see – but no method for this has been described yet, to my knowledge.

(This should be vaguely reminiscent of the problem with supersymmetry, where every known particle gets a "superpartner", and then we have to wave our hands frantically and try to find a way for those superpartners to be a lot heavier, to explain why we haven't seen them. But at least in the case of supersymmetry, a method for doing this has been described. It's a completely ad hoc, ugly method that involves breaking supersymmetry by hand, and throwing over a hundred extra unknown parameters into the Standard Model! But it's still a method.)

Given all this, it should not be surprising that Garrett has not yet done calculations with his theory that reproduce the physics of the world we see around us. I am very glad to be doing something easy like trying to prevent global warming.

27. **mark davis tortino**
November 21, 2010

I have an idea! Lisis should:

a) publish some papers in journals
b) answer john baez et al.'s skepticism
c) present some testable predictions
d) have them tested
e) be labeled the next-einstein and move on to selling i-phone universe-splitter apps and t-shirts

But right now he is beginning with e), and naturally, this rubs some hard-working physicists doing a) the wrong way. 🙂

Add to this the powerful behind-the-scenes financial and media forces at play here, and that this has been going on for over three years with no new developments nor solutions to the problems from Lisi which teh greater community recognizes, and some physicists are a bit frustrated that they have to take time off of their research to get the simple truth of the E8 hype heard above the well-funded media train.

28. **Peter Woit**
November 21, 2010

Tristan,

My apologies for any mistaken assumptions, I should know better. Glad you liked the book.

29. **Peter Woit**
November 21, 2010

Mark,

The idea that what motivates Jacques is willingness to bravely stand up to a well-funded, powerful lobby promoting over-hyped ideas about unification to the public is very funny.

30. **mark davis tortino**
November 21, 2010

Yes Peter–it is perhaps funny. But true! And props go to Distler here for actually publishing sound peer-reviewed papers, which is what physics has ever been about, long before this era of anonymous wikipedia editors/blogs/postmodern/ironic/behind-the-scene machination physics and t-shirt sales.

And yes–I do realize how in certain ways Lisi was used by some to satirize the String Theorists, but two not even wrongs do not make a right GUT.

Best,

Mark 🙂

31. **Brian Hsu**
November 21, 2010

At this point in time, what distinguishes Lisi's claims from those of the Bogdonavs?

At any rate, Lisi has had almost four years to publish a single peer-reviewed paper since all the hype started with… so what exactly is he waiting for?

In the history of science, has an unpublished, unaccepted, and now refuted theory ever received so much attention and hype?

Is this a product of our times, whence politics has trumped physics?

Something like this would have been impossible, even ten years ago.

32. **Giotis**
November 21, 2010

"It's a completely ad hoc, ugly method that involves breaking supersymmetry by hand"

The SUSY breaks in the hidden sector sponteneously and is mediated to the visible via a certain mechanism; so this is not what I would call an ugly/unnatural ad hoc method.

33. **Brian Hsu**
November 21, 2010

From reading through the above, it seems that Peter is motivated largely by his past interactions with Distler, and that he finds Lisi to be a "nice guy." Does Peter think that bringing personalities into a discussion of physics is "professional?" It seems the professional viewpoint would be to note that while Distler has published a paper refuting Lisi's theory in a peer-reviewed journal, Lisi has yet failed to publish a paper supporting his theory. And so, I guess the best Lisi's supporters can do is to use ad hominem attacks. But is this professional. If so, what has the physics profession become, where spurious, unpublished science is hyped, and those who question it are attacked on a personal level?

34. **Peter Woit**
November 21, 2010

Brian,

The Bogdanovs (not "Bogdanavs") are the poster-boys for the problems with peer-review. They published five peer-reviewed articles, two of them in very well-known and highly-respected journals. One of them was given his Ph.D. on the basis of having published such peer-reviewed articles. The articles are complete gibberish, and very different than Garrett's. Garrett is making clear statements and proposals, some of which may be wrong and/or incomplete. What he's saying is so clear that Jacques can write a mathematical physics paper purporting to give a rigorous refutation of some of it.

The fact of the matter is that one can't judge this kind of research on whether it is peer-reviewed or not, you have to read the things, understand them and make up their own mind. In my posting I gave links people can follow to do this, and that's what they should do if they are seriously interested in this particular question. I've also noted in the comments that my experience with Jacques is that he doesn't follow the usual rules for intellectual debate, which you can check by reading my public exchanges with him. You may or may not find that relevant if you try and follow his arguments with Garrett.

35. **Peter Woit**
November 21, 2010

Giotis,

You're conjecturing an unobserved entirely different "hidden sector" of physics that spontaneously breaks the symmetry, interacting with ours through interactions cooked up to completely escape any currently observable effects.

Tastes may vary, but ugly/unnatural/ad hoc all seem to me reasonable adjectives to apply to this scenario.

36. **Brian Hsu**
November 21, 2010

 Thanks Peter,

 You write, "Garrett is making clear statements and proposals, some of which may be wrong and/or incomplete."

 Which of Garrett's "clear statements" are "right" and "complete?"

 I have read his work and while he stated that the LHC can test his theory, in reality his theory makes no predictions. Perhaps I missed that part? If so, please do share!

 And if peer-review is so completely broken, what should replace it? Media campaigns, anonymous wikipedia editors, and the suppression of all peer-reviewed articles mathematically refuting Garrett's theory?

37. **Peter Woit**
November 21, 2010

 Brian,

 As I've noted repeatedly, I'm somewhat of a skeptic on the subject of Garrett's theory. I'm not going to spend more of my time defending his work for him. All I'll say is that I've read his papers, I've read the Bogdanov papers and the two things are completely different.

 Obviously I'm not arguing for the suppression of anyone's articles, peer-reviewed or not. Given the broken nature of the peer-reviewed system, I don't think you can conduct an argument about the value of an idea by saying that X is peer-reviewed and Y isn't. Or by seeing that Z has made it into Wikipedia, or is the subject of an article in a popular magazine.

 You need people who know what they are talking about to discuss the issue as honestly and clearly as they can. In many cases things will come down to whether there's any hope that future work will fix known problems with some idea (and I think that's what's going on here, as well as in string theory). Reasonable people will differ, with those who believe problems can be overcome going on to try and do so.

38. **Giotis**
November 21, 2010

 Peter,

 Yes it's a matter of taste if you like but my main point is that you don't put the soft SUSY breaking terms in the langranian completely arbitrary and by hand as Baez said. There is a proposed theoretical mechanism/justification for the presence of these terms and in any case the SUSY breaks spontaneously which is not unnatural at least.

39. **Brian Hsu**
November 21, 2010

 Thanks very much for your time, Peter,

 I do not wish to take up more of it, but I feel that if Garrett Lisi has made clear statements and proposals, it would take but a moment for you to express them. For instance, Einstein and Newton made clear proposals: E=mc^2 and F=ma.

 You write, "Garrett is making clear statements and proposals, some of which may be wrong and/or incomplete."

 Which of Garrett's "clear statements" are right and complete?

 Thank you for your time.

40. **Peter Woit**
November 21, 2010

 Brian,

 Sorry, Garrett's ideas are a lot more complicated than F=ma, and I believe the same will be true of whatever ideas finally get us beyond the Standard Model.

 You appear to be a complete fanatic on the Garrett issue, posting seven highly repetitive and uninformed comments here in the past few hours. That's enough, I'm deleting the rest. If you want to carry on this kind of campaign, you'll have to do it somewhere else.

41. **ned**
November 21, 2010

 Mr. Woit,

 I'm no physicist, so of course I don't have the slightest idea of the merits of the arguments in this discussion.

 But on reading through this exchange I did notice something which was not mentioned:

 Some of the postings, though on the surface pretending to be about physics, seem to me to have a closer connection with another branch of science – psychology.

 What the real problem here seems to be is addressed quite beautifully in a single sentence – the last one in a recent article about Mr. Lisi in the Telegraph:

 "… [Lisi] tells me: "I've been spending every other day surfing or kitesurfing here in Maui." No wonder his peers are jealous."

 At least some of them.

42. **Garrett**

November 21, 2010

Before I address the many points that have been raised, I'd like to say something on the personal and meta level about where I'm coming from. Many people seem to be behaving as if this E8 Theory is being forced on them, or as if I am some kind of salesman or trickster. What I am is a physicist who struck out on his own to pursue a question (specifically, what spinor fields are geometrically) that was of little interest to the broader community. I worked on this question largely in isolation for ten years, self supported, never pushing my ideas on anyone, and happened to find something incredibly cool. If I seem pushy now it is because I'm excited about this E8 Theory and I want to share it. At the same time, I openly acknowledge its current deficiencies and where there is work left to do. Regarding the media… sometimes excitement tempered by scientific skepticism is too subtle for editors and tv hosts, and hype gets extremized, which is unfortunate. I do have a tendency to say "yes" to interviews, invitations, and speaking engagements — so I am complicit to that degree, but in my defense it is because I'm excited about what I've found, and where it may lead. Also, as prospective theories of everything go, this one does look pretty awesome on a tshirt. (No, I'm not going to make money off of them, nor have I made money selling anything — the shirts are for love.) That said, I will now throw myself back into this comment thread and address the points raised. I generally enjoy and benefit from the high level of discussion on this blog.

-Garrett

43. **Garrett**
November 21, 2010

Mark: You're wrong on several counts. Distler and Garibaldi proved that *their* version of E8 Theory has a non-chiral spectrum. It is my thinking that mirror fermions (and 64 other E8 root vectors) can be gauge transformed to another chiral generation. However, since this is yet to be formally established, I'm willing to play along with the possible existence of mirror fermions. And the bottom line is that the existence of mirror fermions has not been ruled out. So although they are bad, they do not make a theory wrong, merely unattractive — and I expect this to change so that the problem is irrelevant.

44. **John Baez**
November 21, 2010

Giotis wrote:

> Yes it's a matter of taste if you like but my main point is that you don't put the soft SUSY breaking terms in the lagrangian completely arbitrary and by hand as Baez said. There is a proposed theoretical mechanism/justification for the presence of these terms and in any case the SUSY breaks spontaneously which is not unnatural at least.

I don't really follow particle physics much these days, so I could easily be behind the times. Spontaneous symmetry breaking is, of course, much nicer than adding soft SUSY breaking terms by hand! I'd like to learn more about the state of the art.

Is the idea of spontaneously broken supersymmetry widely used in particle physics phenomenology, or is it just a proposal that's been studied in some toy models?

If the former:

Does someone know how to get a supersymmetric extension of the Standard Model where supersymmetry is broken spontaneously? If so, how do the superpartners get their masses? Is there some theoretical reason why all observed particles have superpartners much more massive than they are, rather than comparable masses – or is this fact somehow fed in by hand?

Etcetera, etcetera – lots of questions.

45. **mark davis tortino**
November 21, 2010

Garrett–in your Scientific American article, you claim that the LHC will test your theory.

How will it do this?

What are the specific particles and masses predicted by your theory?

Thanks!

46. **Aaron**
November 22, 2010

Sure. There are tons and tons of such models. The usual idea is that you have some hidden sector with a bunch of stuff that spontaneously breaks supersymmetry. The breaking is then communicated over to the MSSM by some sort of mediating particle. This is often a gauge field (gauge mediated susy breaking) or a graviton (gravity mediated supersymmetry breaking). It's a little old, but I think Martin's review hep-ph/9709356v5 has a pretty decent overview. As is often the case with these things, as soon as you solve one problem, another one will often pop up.

47. **Garrett**
November 22, 2010

Aaron:

> You can call your 'superconnection' whatever you want (although you still have not acknowledged that the very BRST superconnection you cite and don't seem to understand is related to a Grassman symmetry), but that doesn't mean it makes sense.

The superconnection I am using — the formal sum of a 1-form and a Grassmann field — is a well established mathematical construction. There are references cited in my papers, as well as from the link I provided. Such a superconnection is conventionally used in the BRST approach to YM gauge quantization. I am simply using the same well established mathematical formalism, with a different physical interpretation. I am sorry if it's unfamiliar to you — it's unfamiliar to most physicists, but it's an interesting geometric structure worth understanding.

> For the Coleman-Mandula theorem, you need to address what happens in the low-energy theorem. In particular, what breaks the symmetry mixing spacetime and internal indices and at what scale?

An excellent question, which is answered in detail in this paper with Lee and Simone. To summarize: the E8 connection obtains a nonzero VEV, separating the gravitational and gauge sectors of the connection, thus satisfying C-M at low energies.

> For your paper with Lee, I'd be a lot more interested if it had fermions in it.

It does. See Section 3.

For the rest of your questions, they're being addressed elsewhere in this thread — or they're pure snark. If I'm failing to address a question you have, try posing it more politely. In general, if you think E8 Theory is hopeless, then yes, do what you like and ignore it. Might I recommend you choose one of the other ToE's that have made fruitful progress over the last 30 years — oh wait, there are none. Maybe you can find one yourself that you like better.

48. **Garrett**
November 22, 2010

Mark:

> Well, it's not an ad hominem attack to state that Garrett has never published a peer-reviewed paper on his "theory."

No, but it's wrong. The paper with Lee and Simone lays out 90% of the theory, and was published in J. Phys. A: Math. Theor. 43 (2010). Lee and I tend to just put papers on the arxiv, but Simone thought it would be good to put it in a journal. There was no problem getting it published.

> why not try to refute distler's paper on a scientific, mathematical level

I did. The resulting paper is here, which I believe makes things crystal clear, as well as forming a more complete introduction to E8 Theory.

49. **onymous**
November 22, 2010

Aaron wrote:

This is often a gauge field (gauge mediated susy breaking) or a graviton (gravity mediated supersymmetry breaking).

This is not what "gravity mediated supersymmetry breaking" means. "Gravity mediated supersymmetry breaking" means "supersymmetry breaking mediated by any set of Planck-suppressed operators." In some sense, the minimal version of it is what's known as "anomaly mediation," but it encompasses a huge range of models (and some not-quite-models, like "mSUGRA" or "minimal supergravity" which is more of an ansatz than a model, and which unfortunately is most of what experimentalists have been setting limits on for decades).

The trouble with generic Planck-suppressed operators is the flavor problem. As John said, there are about a hundred parameters in the MSSM with soft SUSY breaking, but phenomenology imposes strong restrictions so that really only about 20 are completely independent. If you tried to wander very far outside of this low-dimensional subspace in the 100-dimensional parameter space, you would be in gross conflict with observations. To give an example, if selectrons and smuons are both light, they have to be almost the same mass. So gravity mediation requires extra structure to explain these phenomenological facts, and this structure must be present at or near the Planck scale and survive running down to low energies.

As for John's question:

Does someone know how to get a supersymmetric extension of the Standard Model where supersymmetry is broken spontaneously?

It's important to note that particles beyond those of the MSSM are needed for supersymmetry to be broken spontaneously, which is why models always involve a hidden sector. This was realized quite early on; the paper by Dimopoulos & Georgi that introduced the MSSM with soft SUSY breaking explained that without a hidden sector (i.e. if SUSY is broken spontaneously in the MSSM alone), the theory would always have a scalar lighter than the up or down quark.

50. **Garrett**
November 22, 2010

John: Thanks for this — I hope we can clarify a lot, and maybe even make some new progress.

> Most importantly, particles have very distinct personalities, whereas all 248 dimensions of E8 look alike. E8 is very symmetrical, that's why people like it. But this beautiful symmetry needs to be severely broken for anything like real-world physics to fall out.

Excellent point. Here's what happens. We start with an E8 principal bundle with connection (not a superconnection). The symmetry breaks when this connection gets a vacuum expectation value (VEV), [tex]A \simeq E_0[/tex] (I'm not sure if TeX is working here in the comments, as it once was), which leaves the curvature 0. (One way this could happen spontaneously, starting with an E8 invariant action, is described in the paper with Lee and Simone, but the particular mechanism isn't so important.) This spontaneous symmetry breaking picks out some directions in E8 as *special*, allowing all other generators in E8 to be identified (and named) with respect to these, based on their Lie brackets. Since it's key, let me describe this in more detail. If we describe the E8(-24) Lie algebra as

e8 = spin(12,4) + 128^+_S

then the VEV of the connection is [tex]E_0 = 1/4 e_0 \phi_0[/tex], in which [tex]\phi_0[/tex] is the VEV of a Higgs multiplet that transforms as a 12 vector under a spin(11,1) subalgebra of the spin(12,4), and [tex]e_0[/tex] is the 1-form frame field of deSitter spacetime, transforming as a 4 vector under a spin(1,3) subalgebra of spin(12,4), such that the nonzero VEV is in the complement of spin(1,3) and spin(11,1) in the spin(12,4) of e8. It had to be deSitter spacetime if the curvature of the connection is to be 0, with cosmological constant related to the Higgs VEV. Personally, I think this symmetry breaking mechanism — combining cosmogenisis with a Higgs model — is… awesome. I'd enjoy getting your feedback on it.

> I see no way to get E8 symmetries that mix fermions and bosons in a model where the symmetry of E8 has been broken by deliberately chopping it into bosonic and fermionic parts.

The "bosonic and fermionic" parts of the connection can only mix before spontaneous symmetry breaking — which is to say, before our universe technically exists. However, if an appropriate action has been chosen that is independent of the "fermion" parts of the E8 connection, then there is a prescription for replacing the "fermion" parts of the connection (1-forms valued in the parts e8 that we're calling the fermion part, based on [tex]E_0[/tex]) with Grassmann fields, which are identified as fermions (or pre-fermions, if you like). Now, based on our action, and on [tex]E_0[/tex], we could separate out the [tex]128^+_S[/tex] as the fermion part, or, as I consider preferable, we could break e8 up as

e8 = spin(4,4) + spin(8) + 8×8^+_S + 8×8^-_S + 8x8_V

and consider those last three blocks of 64 as pre-fermion Grassmann fields. This works because spin(4,4) + spin(8) is reductive in e8.

Ah, as I'm reading this, I see I have an email from you…

51. **Giotis**

November 22, 2010

John Baez,

A recent, up to date, small (20 pages) concise review with a comparison of the various mechanisms (pros and cons) and potential string theory realizations is the following

arXiv:1006.0949 by Alwis

52. **Aaron**
November 22, 2010

This is not what "gravity mediated supersymmetry breaking" means.

Geez. I go away for a few years and I already start forgetting things. Enh. Phenomenology was never my thing anyways.

For Garrett, you still haven't explained whether the infinitesimal generators of your symmetry are all commuting or if some or Grassman.

And do you still claim to be able to reproduce any part of the standard model action?

I do apologize for confusing your paper with an earlier one of Lee's which was fermion free, however. However, your paper seems more along the lines of Percacci's earlier paper where fermions are considered separately and not in the same multiplets as bosons.

I will probably have to bow out of this discussion now, however.

53. **Wolfgang**
November 22, 2010

"This works because spin(4,4) + spin(8) is reductive in e8"

How the Standard Model gauge group sits inside of spin(4,4) + spin(8) ?

"Personally, I think this symmetry breaking mechanism — combining cosmogenisis with a Higgs model — is… awesome."

Does that mean that you predict the cosmological constant is electro-weak scale in size?

54. **Rhys**
November 22, 2010

> The resulting paper is here, which I believe makes things crystal clear, as well as forming a more complete introduction to E8 Theory.

Except that you have still completely failed to answer Distler's criticism. Allow me to quote the relevant excerpt from your paper:

> Distler and Garibaldi prove that … when one embeds gravity and the Standard Model in E8, there are also mirror fermions. They then claim this prediction of mirror fermions (the existence of "non-chiral matter") makes E8 Theory unviable. However, since there is currently no good explanation for why any fermions have the masses they do, it is overly presumptuous to proclaim the failure of E8 unification – since the detailed mechanism behind particle masses is unknown, and mirror fermions with large masses could exist in nature.

This is completely misleading. For one thing, the phrase "mirror fermions" is ambiguous. There are models of particle physics which impose exact parity symmetry, which requires introducing so-called "mirror matter". However, in this case, the extra particles are charged under a different gauge group to the visible particles, and are easy to 'hide'. What you have is nothing like this.

In the standard model, no fermion mass terms are allowed, because they violate the gauge symmetry. The reason is that the left-handed fermions are in a complex representation of the gauge group. After electroweak symmetry breaking, all fermions are vector-like with respect to the remaining gauge symmetry, and mass terms can be written down. In practice, these come from Yukawa couplings to the Higgs field. However, in your model, the fermion content is doubled, such that the left-handed fermions now fall into a real representation of the standard model gauge group (in fact, R + R-bar, where R is the standard model rep). Therefore there is nothing to forbid mass terms for all the fermions, and in fact these should be generated radiatively in the absence of supersymmetry. So generically, all fermions in your theory should have masses roughly of the cut-off scale (probably the Planck scale here).

This is a serious problem, and why Distler rightly calls your model a 'zero-generation' model. You can't just wave your hands about it — you have to at least provide a solution in principle, or there is no reason to think your model is anything more than a pretty exercise in group theory.

Let me finish by explaining why this is so different to supersymmetry. Before SUSY breaking, there are no mass terms in the MSSM. For the fermions, the reason is the same as for the standard model, and the bosons are related to the fermions by SUSY. After SUSY breaking, nothing stops us writing down mass terms for the bosons, but those for the fermions are still forbidden by chirality. That's why it is natural for the (unseen) scalar partners to be significantly more massive than the standard model fermions. The Higgs is different, because there is an up-type Higgs and a down-type Higgs, and together they form a real representation, so one can write down a supersymmetric mass term.

55. **Urs Schreiber**
November 22, 2010

Garrett suggested to sombody:

> Might I recommend you choose one of the other ToE's that have made fruitful progress over the last 30 years okay, I will — oh wait, there are none.

I suppose you have followed Alain Connes' construction (here is a survey and links) of the standard model by a Kaluza-Klein compactification in spectral geometry. It unifies all standard model gauge fields, gravity as well as the Higgs as components of a single spin connection. Connes finds a remarkably simple characterizaiton of the vector bundle over the compactification space such that its sections poduce precisely the standard model particle spectrum, three chiral generations and all.

Alain Connes had computed the Higgs mass in this model under the big-desert hypothesis to a value that was in a rather remarkable chain of events experimentall ruled out shortly afterwards by the Tevatron. But the big desert is a big assumption and people got over the shock and are making better assumptions now. We'll see.

Apart from being a nice geometrical unification of gravity and the other forces (credits ought to go all the way back to Kaluza and Klein, but in spectral geometry their orginal idea works out better) Connes' model has some other striking features:

the total dimension of the compactified spacetime in the model as seen by K-theory is and has to be, as they showed, to produce exactly the standard model spectrum plus gravity: D= 4+6.

Now "as seen by K-theory" was shown by Stolz and Teichner and students to mean in a precise sense: as seen by quantum superparticles (here is some link — you can ask me for a better link). In fact what they consider is almost exactly the spectral triples that Connes considers, with some slight variation and from a slightly different angle. For the relation see the nLab entry on spectral triple (ask me to expand that entry…).

As also indicated at that entry: there is a decent theory of how to obtain a spectral triple as the point particle limit of a superconformal 2-dimensional CFT. Yan Soibelmal will have an article on that in our book Precisely because Connes' model turns out to have real K-theory dimension 4+6 does it have a chance to be the point particle limit of a critical 2D SCFT. That would even give it the UV-completion — as they say — that would make its quantization consistent (which, remember, contains gravity).

I think there is some impressive progress here. It is not coming out of the physics departments, though, but out of the math departmens. For some reason.

56. **Garrett**
November 22, 2010

(I'm continuing through the comments chronologically, picking up where I left off, trying not to miss anything directed to me that's important. Peter, thanks for allowing the discussion.)

John:

> Second, even among fermions, different particles have drastically different personalities: for example their masses, and the rates at which they turn into each other, which are described by the numbers in the Cabibbo-Kobayashi-Maskawa matrix and the Maki-Nakagawa-Sakata matrix. Similarly, in the realm of the bosons there's the Higgs mass and other numbers. Garrett's work has nothing to say about these. Without these numbers we can't do real-world physics. But the big problem is this: I don't see any way to get these numbers into the game without further breaking down the symmetry of E8. Why? Because again, E8 symmetry wants all particles to be alike, but these numbers describe how they're not.

Getting the CKM and MNS matrices is the goal, and it is true, E8 Theory is not there yet — and I have been completely candid about this at every opportunity. But I do think there is hope, and my work may soon have something to say about these. How can this possibly work? Well, it all has to start with symmetry breaking, as described in my previous comment. After that, the masses of all other particles are determined by how they interact with the Higgs.

> Third, there's no way to pack all known fermions into E8 without positing two copies of E8 and giving every fermion a mysterious unseen partner called a "mirror fermion". To keep these rascals from being seen we could claim they're more massive than the guys we see – but no method for this has been described yet, to my knowledge.

There is potentially a way to fit three generations of fermions into E8 and avoid the mirror fermion problem. The basic idea is that, using an inner-automorphism of E8 related to triality, we can independently gauge transform the 64 mirror fermions, and 64 pre-fermions in the 8_Vx8_V of a so(4,4)+so(8) subalgebra of e8, into generations of usual fermions that will all interact differently with the Higgs. I don't yet know if CKM and MNS will come out of this, but I'm working on it. In the meantime, yes, it's fair to say the model is incomplete, and the burden is on me (or some other researcher) to figure out how this can work, but it's premature to say it can't work.

57. **Garrett**
November 22, 2010

Mark:

> Lisi should: a) publish some papers in journals

Did that.

> b) answer john baez et al.'s skepticism

Working on it. But, I also encourage skepticism.

> c) present some testable predictions

The testable predictions are that if any new particles are found that don't fit E8, such as superparticles (which many expect to see), then this theory is wrong.

> d) have them tested

In progress.

> e) be labeled the next-einstein and move on to selling i-phone universe-splitter apps and t-shirts.

What people label me, whether it's crackpot, next-einstein, or surfer-physics dude, isn't really up to me. And I'm not selling anything. I am, however, helping friends sell things I think are cool, and see no problem with that. Also, I did get paid to write the SciAm article, and will use the money to buy a new surfboard.

> Add to this the powerful behind-the-scenes financial and media forces at play here

What powerful financial and media forces?

> and that this has been going on for over three years with no new developments nor solutions to the problems from Lisi

If you look at my list of (1)-(6) issues in my comment above, you'll see that there has been good progress on several of them.

58. **Garrett**
November 22, 2010

Brian: Peter has addressed your points. My only addition is that I have published work on the theory, without difficulty, with coauthors. But as the Bogdanovs showed, this means little.

59. **Garrett**
November 22, 2010

Peter:

> You need people who know what they are talking about to discuss the issue as honestly and clearly as they can. In many cases things will come down to whether there's any hope that future work will fix known problems with some idea (and I think that's what's going on here, as well as in string theory). Reasonable people will differ, with those who believe problems can be overcome going on to try and do so.

Precisely. Thank you.

60. **Garrett**
November 22, 2010

ned:

> "I've been spending every other day surfing or kitesurfing here in Maui." No wonder his peers are jealous."

My life hasn't been all roses. But I am working on making it easier for other scientists to come spend some time in Maui — that's what the Pacific Science Institute is about.

61. **Garrett**
November 22, 2010

Aaron:

> For Garrett, you still haven't explained whether the infinitesimal generators of your symmetry are all commuting or if some or Grassman.

The superconnection is the formal sum of a 1-form and an anti-commuting Grassmann field, both valued in different parts of some algebra. That algebra can be a Lie algebra, such as E8, or it can be a Lie superalgebra — the necessary restriction is that the 1-form be valued in a reductive subalgebra.

> And do you still claim to be able to reproduce any part of the standard model action

Yes, see the paper with Lee and Simone.

> I do apologize for confusing your paper with an earlier one of Lee's which was fermion free, however. However, your paper seems more along the lines of Percacci's earlier paper where fermions are considered separately and not in the same multiplets as bosons.

True. We wanted to make this less unfamiliar and upsetting to people by keeping the bosons and fermions separate.

> I will probably have to bow out of this discussion now, however.

Sorry to lose your input.

62. **Garrett**
November 22, 2010

Wolfgang:

> How the Standard Model gauge group sits inside of spin(4,4) + spin(8) ?

Excellent observation. It doesn't. The Standard Model and gravity fits in spin(12,4), and spin(4,4)+spin(8) fits in that. There are two SM generators, W^+ and W^-, that occupy the complement. When considering triality, one needs to use spin(4,4) and/or spin(8). I'm not yet sure whether those W's will remain in the complement as bosons, displacing two fermion degrees of freedom, or whether things will work some other way. A key idea that came from Banff is that the spin(4,4)+spin(8) subalgebra of e8 which relates to triality might be a different subalgebra than the one containing the Standard Model, including having different Cartan subalgebras. This will, of course, give a mixing mess, but the question will be whether that mess is the CKM and MNS mess.

> Does that mean that you predict the cosmological constant is electro-weak scale in size?

Yes! That seems terrible, but the hope is that the cosmological constant runs from this value at the unification scale down to the tiny value we see at low energies.

63. **Wolfgang**
November 22, 2010

"There are two SM generators, W^+ and W^-, that occupy the complement. When considering triality, one needs to use spin(4,4) and/or spin(8). I'm not yet sure whether those W's will remain in the complement as bosons, displacing two fermion degrees of freedom, or whether things will work some other way."

Didn't you explain that all the generators of e8 are either in spin(4,4)+ spin(8) or in one of the 8×8 blocks exchanged by your triality?

So, if some of 8×8 block is bosons, where are rest of fermions? And, if Ws live in 8×8 block, does that mean they form triplet under triality? Do different generations have different gauge bosons coupling to them?

Sorry for so many questions, but this is very confusing!

64. **Garrett**
November 22, 2010

Rhys:

> Except that you have still completely failed to answer Distler's criticism.

No, Distler and Garibaldi's claim is that one cannot even get one generation of fermions in E8. This paper answers that directly by showing explicitly how a generation of fermions does fit in E8. (And it explains it via a direct identification of generators, which is quite nifty.)

> This is completely misleading.

No, it is direct. It is Distler's language that is misleading. When he says "there are no chiral generations," what he actually means is that there is a generation and what he calls an anti-generation and I call mirror fermions. For him and you to use this twist of mathematical language to say "there are no generations" is a lie.

> For one thing, the phrase mirror fermions is ambiguous.

The top Google hits and I disagree.

> There are models of particle physics which impose exact parity symmetry, which requires introducing so-called mirror matter.

If I had said "mirror matter," then yes, it would have been ambiguous. But I did not.

> fermions in your theory should have masses roughly of the cut-off scale

That's such a fun word, "should." With that one word, you are presuming what nature does — when the truth is that we just don't know. It is fine if you want to say "it should not be," it is a lie to say "it can not be," at least until we know what nature actually does. And in that same phrase, you are incorrectly presuming, as do Distler and Garibaldi, that *my* theory must have mirror fermions in it, when I have said several times that I expect these to be gauge transformed to usual fermions.

> You can't just wave your hands about it — you have to at least provide a solution in principle, or there is no reason to think your model is anything more than a pretty exercise in group theory.

This is a valid point. I cannot say I have a complete theory of everything, and I do not, until these problems are solved. But I can and do wave my hands about how they might be solved in principle. And even if it ends up having been a pretty exercise in group theory, I won't have considered it a waste of time, because I think there is a lot here — especially the dodge of the Coleman-Mandula theorem via symmetry breaking — that is true about nature, even if we don't yet have the full picture.

65. **Steve**
November 22, 2010

This may not be the proper place to bring this up. But does anyone have a view on Penrose's 'conformal cyclic cosmology'? I am reading his book now and but have only seen a couple of arxiv articles mentioning it…..

66. **Garrett**
November 22, 2010

Urs: I agree that Alain Connes' model is fascinating and deserves more attention from physicists. But it has not been fruitful in making successfully tested new HEP predictions — nor has any model in the past 30 years. It was not my intent to be discouraging, or particularly disparaging of other ideas — I was mostly counter-snarking Aaron.

67. **Peter Woit**
November 22, 2010

Steve,

I've heard Penrose talk about this, but didn't really understand the point. I look forward to reading his book (it isn't out in the US yet), and might write about it then. But, I'm no cosmologist, best to find a blog run by someone who is to discuss the subject.

68. **Urs Schreiber**
November 22, 2010

> But it has not been fruitful in making successfully tested new HEP predictions — nor has any model in the past 30 years.

It has. Within weeks even. It was experimentally verified that the big desert assumption is inconsistent in this model with experiment.

It's an impressive model. And I didn't quite say that "it deserves more attention from physicists". Let them spend their time with what pleases them. Instead I mentioned this in reply to your insinuation that there is nothing promising in fundamentall model building out there besides your idea. It occurred to me that you might actually think that's true. And maybe because the most impressive progress in fundamental physics these days does not quite percolate through the physics community.

69. **Garrett**
November 22, 2010

Wolfgang (and John):

> So, if some of 8×8 block is bosons, where are rest of fermions? And, if Ws live in 8×8 block, does that mean they form triplet under triality? Do different generations have different gauge bosons coupling to them?

Here we are on the edge of what I'm working on. So I don't yet know what the complete picture is, and my remarks here will be speculative. We do know that under the decomposition

e8(-24) = spin(12,4) + 128^+_S

that one generation of fermions can be the 64^+_S rep (part of the above 128) of a spin(11,3) subalgebra of the above spin(12,4). And in fact, the known gravitational and Standard Model bosons can fit in a spin(5,3)+spin(6) subalgebra of spin(11,3). But, spin(5,3) and spin(7,1) don't have triality automorphisms. However, spin(4,4) and spin(8) do, so we can decompose e8 as

e8(-24) = spin(4,4) + spin(8) + 8x8_V + 8x8_+ + 8x8_-

and consider innner automorphisms of e8, corresponding to so(4,4) and so(8) triality, that interchange those three blocks of 64. If we put gravitational spin(1,3) in the spin(4,4), and strong su(3) in the spin(8), and the photon and the Z in both, then we're stuck with at least the W^+ and the W^- in that 8x8_V. And you're right that if we're identifying those three blocks of 64 by triality that we're probably going to be missing at least three sets of two fermion degrees of freedom to accommodate those W's. Maybe nature has chosen to exclude the right-handed components of neutrinos in this way? Or, an even weirder speculation, maybe that particular spin(4,4)+spin(8) is not completely in the spin(12,4)? There would have to be a lot of mixing angles to describe the geometry of how these spin groups are mutually related, but we want something like that to come out anyway that corresponds to CKM and MNS. The bottom line is that I don't know how this works yet, but it's really fun and interesting! (And please do correct me if I've made any mistakes here.)

70. **Garrett**
November 22, 2010

Urs: I didn't mean to insinuate that there aren't other promising models. But I don't consider a prediction proven false to be a "fruitful" prediction in the usual sense, though these impressive events and the positive aspects of Connes' model are not lost on me. I do consider this E8 Theory to be even more fascinating and promising, but I'm biased. Although, it indisputably looks really good on the new T-shirts. 🙂

71. **Tony Smith**
November 22, 2010

Garrett, about
e8(-24) = spin(4,4) + spin(8) + 8x8_V + 8x8_+ + 8x8_-
where you put
gravity spin(1,3) in the spin(4,4)
and
color su(3) in the spin(8)
but
are "stuck with at least the W+ and W- in that 8x8_V"

could you find within "that 8x8_V" a spacetime base
manifold that is an 8-dim Kaluza-Klein M4 x CP2
where
M4 is 4-dim Minkowski spacetime
and
CP2 is internal symmetry space.

Then since CP2 = SU(3)/U(2)
you would have the electroweak U(2) (weak bosons and photon)
naturally included in your structure,
and
the added benefit of getting not only fermions and bosons,
but spacetime itself as part of your E8.

The 8-dim Kaluza-Klein idea is not mine,
but is due to N. A. Batakis
who wrote Class. Quantum Grav. 3 (1986) L99-L105 in which he showed
that "… In a standard Kaluza-Klein framework,
M4 x CP2 allows the classical unified description of an SU(3) gauge field with gravity
… an additional SU(2) x U(l) gauge field structure is uncovered …
As a result,
M4 x CP2 could conceivably accommodate the classical limit of a fully unified theory
for the fundamental interactions and matter fields …".
Roughly, he uses the structure CP2 = SU(3)/U(2) with the local U(2) giving electroweak and the global SU(3) working for color since its global action is on CP2 which is, due to Kaluza-Klein structure,
local with respect to M4 Minkowski spacetime.
As to why Batakis is not well known and his model fell into obscurity,
Batakis never handled fermions properly in his model.
Since he had nothing to work with but M4xCP2 Kaluza-Klein,
he was reduced to introducing fermions sort of ad hoc by hand,
and he could not show that they worked nicely with his gauge bosons.
However,
since all your structures (spacetime, gauge bosons, fermions) would come from E8 they can be shown to work together nicely.

Tony

72. **Wolfgang**
November 22, 2010

"If we put gravitational spin(1,3) in the spin(4,4), and strong su(3) in the spin(8), and the photon and the Z in both, then we're stuck with at least the W^+ and the W^- in that 8x8_V."

If I understand what you are saying, there are three W^+s and three W^-s (the ones in the 8x8_V and their triality partners in the other 8×8 blocks). How to reconcile that with there being only one SU(2), whose gauge field corresponding to the diagonal generator occurs only once?

How does the SU(2) Yang Mills action look?

I understood that you wanted the physics of having three generations of fermions, but I don't think you want three generations of W's.

73. **Garrett**
November 23, 2010

Tony:
Your idea sounds pretty close to constructing a Cartan geometry starting from E8. However, if we mod E8 by spin(4,4)+spin(8), we get not only the 8x8_V but also the 128 spinor as the base, which is waaaay too big. It seems much cleaner to consider an E8 principal bundle over a 4D base. If it turns out that structure isn't rich enough, then I'm open to re-considering an 8D base and KK with CP2. I agree it looks pretty good, but I want to see what I can do with just a 4D base, E8, and triality first.

74. **Garrett**
November 23, 2010

Wolfgang:
You are understanding things perfectly.

But when looking for a mixing mechanism, I think it's probably good to have these issues in mind but not focus on them too hard. The W's are important, but they're not the only problem. We also have to either get rid of or give large masses to all the X bosons somehow — the various gauge fields other than those of the SM. And, of course, we want to mix the fermions (including mirrors) and get the CKM and MNS for them — all of this in one go, with limited options.

I think a good thing to try is going to be using the so(4,4)+so(8) decomposition to calculate a set of E8 inner automorphisms related to triality, then try applying those inner automorphisms back in the so(12,4) + 128 decomposition to see how it can mix elements. I want to see what is learned from trying that before focusing on specifics.

75. **chris**

November 23, 2010

> Also, I did get paid to write the SciAm article, and will use the money
> to buy a new surfboard.

this is the coolest statement on this blog yet 🙂

and kudos that you resisted invoking the anthropic principle … yet

76. **Steve**
November 23, 2010

Peter,
Penrose's book has been available on Amazon since late October.

77. **Wolfgang**
November 23, 2010

"The W's are important, but they're not the only problem. We also have to either get rid of or give large masses to all the X bosons somehow"

OK. But other gauge symmetries could be broken at Planck scale. Standard Model gauge symmetries are supposed to be unbroken down to low energies. That seems much more restrictive.

One more question:

In previous comments, you seemed to say that even if triality idea doesn't work out, E8 theory is still OK.

My understanding from trying to read distler-garibaldi paper is that they show two things,

1) 128 (out of 248) fields are fermions
2) fermion spectrum is non-chiral

so (assuming no triality), fermions are at best 1 generation and 1 mirror-generation of Standard Model. From this they conclude E8 theory is not viable.

Do you say E8 theory with 1 generation and 1 mirror-generation (even if no triality) is still viable theory of Nature?

If so, could you explain how?

78. **thomas schaefer**
November 23, 2010

The lack of response to Connes' theory is indeed interesting. I think the problem is that nonbody has been able to explain in a language that particle theorists can understand whether this is indeed a new idea (and if so, what the new idea is) or whether this is just a complicated way to formulate an old idea (GUT's or maybe Gravi-GUT's). Where is Witten when you need him?

79. **Wolfgang**
November 23, 2010

Sorry. I have one more question. About cosmological constant being electro-weak scale in size, you said:

"Yes! That seems terrible, but the hope is that the cosmological constant runs from this value at the unification scale down to the tiny value we see at low energies."

Does that mean the relation between electro-weak scale and cosmological constant is accidental feature of your classical lagrangian? Is there a more general lagrangian where they are independent parameters?

I ask because in renormalizable theory all counterterms should appear as possible terms in classical lagrangian.

80. **Thomas Larsson**
November 23, 2010

Didn't Connes predict a 170 GeV Higgs? Which was the first region to be ruled out by the Tevatron?

81. **Tony Smith**
November 23, 2010

Just to be clear,
my intent was not to suggest
"… mod E8 by spin(4,4)+spin(8) …"
in which case "… we get not only the 8x8_V but also the 128 spinor as the base …"
but to suggest
mod E8 by both spin(4,4)+spin(8) and also the 128 spinor
(that may be a 2-stage process)
so that
we get only the 8x8_V as the base
and then
to let the 8x8_V represent an 8-dim M4 x CP2 Kaluza-Klein.

Tony

82. **Urs Schreiber**
November 23, 2010

> The lack of response to Connes' theory is indeed interesting. I think the problem is that nonbody has been able to explain in a language that particle theorists can understand whether this is indeed a new idea (and if so, what the new idea is) or whether this is just a complicated way to formulate an old idea (GUT's or

> maybe Gravi-GUT's).

Yes. Generally my impression is that the number of theoretical physicists actively aware of or at least interested in the issues of what it means to find a conceptual or even axiomatic framework for fundamental physics is currently much lower than it used to be. It seems to me that in the early 90s or so the situation has been very different. In fact from that time date a few articles by string theorists who had read Connes, had understood what he is after and had tried to connect it to string theory.

Because the curious thing is: what Connes suggests is precisely the 1-dimensional version of the very idea of perturbative string theory (which is the 2d version of an even more general idea):

regard the algebraic data characterizing a d-dimensional super QFT as a stand-in for the geometric data characterizing the target space of which this QFT would be the sigma-model, if it were one. What in Connes's setup is a spectral triple is a vertex operator algebra for the string.

(References that discuss how to make this statement precise are at [spectral triple] and [2-spectral triple]).

> Where is Witten when you need him?

And why do you necessarily need him?

Lately Witten seems to be busy providing more evidence for the [holographic principle of higher category theory] (scroll down to see what i mean).

83. **Garrett**
November 24, 2010

Wolfgang:

> In previous comments, you seemed to say that even if triality idea doesn't work out, E8 theory is still OK.

What I was saying was slightly different. I do think the triality idea is going to have to work out for E8 Theory to be a *good* theory. However, since I only have some rough ideas on how triality might work out, I have been forced by critics to defend the theory without it. Without triality, the best I can say is that the theory is incomplete and unattractive, but not necessarily wrong. The best way to look at it, in my opinion, is that we currently know exactly how gravity and the Standard Model gauge fields along with one generation of fermions can embed in E8, which is incredibly cool. And there are some indications of how to get the other two generations, with a much tighter fit, but that is not yet clear.

> My understanding from trying to read distler-garibaldi paper is that they show two things, (1) 128 (out of 248) fields are fermions. (2) fermion spectrum is non-chiral. So (assuming no triality), fermions are at best 1 generation and 1 mirror-generation of Standard Model. From this they conclude E8 theory is not viable. Do you say E8 theory with 1 generation and 1 mirror-generation (even if no triality) is still viable theory of Nature?

This is a straw man setup. I disagree with Distler and Garibaldi at step (1) — they insisted on using this [tex]Z_2[/tex] grading of E8 even though I said the theory would rely on other options. However, even this silly straw man is not easy to knock down, because mirror fermions have not been completely ruled out, even if they make it ugly.

Essentially, I think E8 Theory is about half way done. We've got gravity, the Standard Model, a generation of fermions, and a nice symmetry breaking mechanism. And the triality-related gauge transformations I'm working with are very encouraging. It is kind of stupid to assess a half-done theory as if was supposed to be a complete theory of nature. It's like looking at a half-built house and saying "oh, that's no good — it's leaky." Rather, one needs to assess E8 Theory as a research program moving towards a complete ToE. And, from that point of view, it's doing pretty well.

84. **Garrett**
November 24, 2010

Wolfgang:

> Does that mean the relation between electro-weak scale and cosmological constant is accidental feature of your classical lagrangian? Is there a more general lagrangian where they are independent parameters?

The relation between Higgs VEV and cosmological constant is even more fundamental than the Lagrangian. If the bosonic connection is

[tex]H = \frac{1}{2} \omega + \frac{1}{4} e \phi + A[/tex]

then its curvature is

[tex]F = \frac{1}{2}(R – \frac{1}{8}\phi^2 e e) + \frac{1}{4} (T \phi – e D \phi) + F_A[/tex]

and the relationship between Higgs VEV and cosmological constant, [tex]\Lambda = \frac{3}{4} \phi_0^2[/tex], comes from [tex]F_0 = 0[/tex], which I consider more fundamental than the Lagrangian. One might be able to cook up a way to change that relationship, but I wouldn't recommend it.

85. **Garrett**
November 24, 2010

(Hmm, that "8211;" above is a "-". I don't know why it did that.)

86. **Garrett**
November 24, 2010

Tony: I think one is only allowed to mod out by subgroups.

87. **Wolfgang**
November 24, 2010

"This is a straw man setup. I disagree with Distler and Garibaldi at step (1) — they insisted on using this Z_2 grading of E8 even though I said the theory would rely on other options."

Maybe I expressed myself badly.

Fermions transform as Lorentz spinors. Your triality idea is to change how fields in the 248 transform under Lorentz group. Without triality (which remains to be worked out), fields transform according to the “naive” transformation rule. Do you agree that naive transformation rule gives 128 fermions or is even that part wrong?

Put differently: if the triality idea doesn’t work out, do you have another way to avoid distler-garibaldi conclusion?

“The relation between Higgs VEV and cosmological constant is even more fundamental than the Lagrangian. … One might be able to cook up a way to change that relationship, but I wouldn’t recommend it.”

If you don’t change it, how do you avoid cosmological constant of order the electro-weak symmetry breaking scale?

In earlier comment, you said “the hope is that the cosmological constant runs from this value at the unification scale down to the tiny value we see at low energies.” But if Higgs VEV and cosmological constant are tied together as you say how can one be big (250 GeV) and the other tiny?

88. **Tony Smith**
November 24, 2010

Garrett, you say “one is only allowed to mod out by subgroups”.

Maybe you and I are not using “mod out” in the same sense,
and maybe (since it is a term with which I am not very familiar)
I have been misusing it, so here is what I am trying to say in
terms of graded Lie algebras:

Consider Thomas Larsson’s 7-grading of E8 which is of the form
E8 = g_-3 + g_-2 + g_-1 + g_0 + g_1 + g_2 + g_3
with graded dimensions
E8 = 8 + 28 + 56 + (sl(8) + 1) + 56 + 28 + 8

The odd graded part of E8 has 8+56 + 56+8 = 64+64 = 128 dimensions
and corresponds to your 128 spinor.

The even graded part of E8 has 28 + 64 + 28 = 120 dimensions
and corresponds to your D8 Lie algebra so(4,12)

My first stage is to “mod out” the odd graded 128 spinor,

which leads to the next stage about the D8.

The D8 Lie algebra has a 3-grading which is of the form
D8 = g_-3 + g_-2 + g_-1 + g_0 + g_1 + g_2 + g_3
with graded dimensions
D8 = 28 + (sl(8) + 1) + 28

The odd graded part of D8 has 28 + 28 = 56 dimensions
and corresponds to your D4 + D4 Lie algebras so(4,4) and so(8)

The even graded part of D8 has 64 dimensions
and corresponds to your 8x8_V.

My second stage is to “mod out” the odd graded D4 + D4,

which leaves your 64-dimensional 8x8_V to represent
an 8-dim spacetime that can (by breaking octonionic symmetry
down to quaternionic) give you a M4 x CP2 Kaluza-Klein
with the Batakis structure giving you the U(2) from CP2= SU(3)/U(2).

Then you can construct a nice Lagrangian as follows:

Base Manifold from the 8-dim Kaluza-Klein in the 8x8_V

Gauge Boson terms from D4 + D4 “modded out” in stage 2

Fermion terms from 128 half-spinor “modded out” in stage 1.

Then,
if you look at the geometry of the octonionic/quaternionic
symmetry breaking down to 4+4 dim Kaluza-Klein you see
that Meinhard Mayer’s mechanism (Hadronic Journal 4 (1981) 108-152)
(he is physics professor emeritus at U. C. Irvine)
gives the Higgs scalar.

Tony

89. **Daniel de França MTd2**
November 24, 2010

Garrett,

What about this: whenever you talk about using triality on a part of E(8), it seems you are not talking about E(8) anymore, but a semiderect product of SO(8)XE(8), SO(8) being the group that “insert” the triality. Now, what do you think of this?

90. **Gregor Samorasky**
November 25, 2010

Hello Peter & Garrett,

Well, it seems that Peter is quite skeptical of Garrett's theory, and that Garrett is too, if less so. The question, then, is why does it keep getting so much attention and funding?

Peter writes, "
Tristan,

My understanding is that Garrett is well aware that his proposal has problems. In the Scientific American article he writes:

"All new ideas must endure a trial by fire, and this one is no exception. Many physicists are skeptical—and rightly so. The theory remains incomplete."

I have no problem with skepticism, I'm skeptical about many of Garrett's ideas too. If Jacques wants to make a clean technical argument showing the nature of the problems with Garrett's proposal, that's great, and could be potentially worthwhile. But I don't see any reason for the hostile, sneering tone of Jacques's blog posting explaining these points. This is not the way to professionally make a credible technical argument. "

Are there not a lot of other theories out there which we can be skeptical about? So why is Garrett's "theory" getting all the attention from television, magazines, and the press? Who is pushing/promoting this and why?

Insights? Ideas? Thanks!

91. **Garrett**
November 25, 2010

Wolfgang:

> Fermions transform as Lorentz spinors. Your triality idea is to change how fields in the 248 transform under Lorentz group. Without triality (which remains to be worked out), fields transform according to the "naive" transformation rule. Do you agree that naive transformation rule gives 128 fermions or is even that part wrong?

That is correct.

> Put differently: if the triality idea doesn't work out, do you have another way to avoid distler-garibaldi conclusion?

No, without triality, we're stuck with mirror fermions. But the Distler-Garibaldi conclusion that "the theory can't work" would still be untrue, because mirror fermions could exist. But I don't think they do — I think triality will work.

> But if Higgs VEV and cosmological constant are tied together as you say how can one be big (250 GeV) and the other tiny?

I haven't done the calculation, but perhaps they run independently, with the effective cosmological constant getting contributions from gravity, and the Higgs mass from Standard Model and other interactions.

92. **Garrett**
November 25, 2010

Tony:
If one were to try and build a universe by deforming the E8 Lie group, the nicest way to do it would probably be to use [Cartan geometry](), by which the base spacetime is modeled on the (too large) symmetric space obtained by moding E8 out by a subgroup.

Of course, you're also welcome to just start with an 8D base and a principal bundle, which is less restrictive, and play with different gradings and KK schemes as you are here.

93. **Garrett**
November 25, 2010

Daniel:
When I am talking about E8 triality I am talking about the triality outer automorphisms of the so(4,4) and so(8) subalgebras, and the corresponding inner automorphisms of E8.

94. **Garrett**
November 25, 2010

Gregor:
Since you cannot accept that the media has been attracted to a story about an unusual physicist who has come up with an interesting new theory, the attention must be because I am so incredibly handsome.

Happy Thanksgiving!

95. **Wolfgang**
November 25, 2010

"No, without triality, we're stuck with mirror fermions. But the Distler-Garibaldi conclusion that "the theory can't work" would still be untrue, because mirror fermions could exist."

Sorry. That I don't understand.

Without triality, you are stuck with one generation and one mirror-generation. I don't see how you can say that "works" as a theory of nature. Could you explain?

" But I don't think they do — I think triality will work."

Maybe it will. But it faces serious obstacles (see above discussion about W bosons).

"I haven't done the calculation, but perhaps they run independently,"

Every independently-running coupling constant corresponds to an independent term you can add to classical lagrangian. You just explained that in your theory electro-weak scale and cosmological constant are not independently adjustable coupling constants. So how can they run independently?

96. **chris**
November 26, 2010

>"No, without triality, we're stuck with mirror fermions.
> But the Distler-Garibaldi conclusion that
> "the theory can't work" would still be
> untrue, because mirror fermions could exist."

> Sorry. That I don't understand.

i guess he just means that you can build a model that looks like the SM at low energies without chiral fermions. and he is right – you can. you just need fine tuning, which is "ugly" but not forbidden.

and of course you can add 2 carbon copy generations and by hand add CKM mixing. it's not pretty – but who says that top-color assisted extending walking technicolor is? 🙂

97. **Peter Woit**
November 26, 2010

I've had to delete repeated anonymous comments by someone who couldn't be bothered to either look things up for himself or read Garrett's previous response to the same question:

http://www.math.columbia.edu/~woit/wordpress/?p=3292&cpage=1#comment-69686

98. **Tony Smith**
November 26, 2010

Garrett, please correct me if I misunderstand. It seems to me that what you are now doing is (using "mod" in your Cartan geometry sense):

Mod E8 / so(4,4) x so(8) to get 8x8_v + 128_s+

which by reducing the 128_s+ half-spinor of D8 into 64_s+ + 64s_s-
(is this done by looking at things in terms of D6 instead of D8?)
to get

E8 / so(4,4) + so(8) = 8x8_v + 8x8_s+ + 8x8_s-

Then you use the fact that both so(4,4) and so(8) have triality among their three 8-dimensional representations
8-vector
8-halfspinor+
8-halfspinor-

to construct a triality automorphism inside E8 among the three 64-dimensional things
8x8_v
8x8_s+
8x8_s-

so 8x8_s+ + 8x8_s- must inherit mirror-image halfspinor structure from both the so(4,4) and the so(8)
You do not identify them with fermion particles and antiparticles as would be natural from so(10) models,
but
you identify them with one fermion generation plus one antigeneration,
and you say that the antigeneration somehow is suppressed.
Does that not destroy the triality symmetry and so render it useless for further things, such as higher generations ?

Also, the 8x8_v must inherit vector structure from both the so(4,4) and the so(8)
and that vector structure includes signature,
so it seems to me to be an inconsistency because
the so(4,4) split signature (4,4)
Is not
the so(8) Euclidean signature.

Can you address these concerns?

Tony

99. **Daniel de França MTd2**
November 27, 2010

Garrett,

"Daniel:
When I am talking about E8 triality I am talking about the triality outer automorphisms of the so(4,4) and so(8) subalgebras, and the corresponding inner automorphisms of E8."

But what about the operators that do the outer automorphisms inside 8? Where will they find space inside E8 if everything is already busy with other fields?

Would you give me an example of that in a simpler theory?

Best,

Daniel.

100. **Garrett**
November 28, 2010

Wolfgang: (Thanks Chris.) Without triality it "works" in the same sense that the SO(10) GUT works — there are many predicted particles, and the three generations are left unexplained. For the cosmological constant and renormalization, the [tex]\frac{4}{3} \Lambda e e[/tex] term might be independent but related to the [tex]\phi^2 e e[/tex] term.

Tony: Your analysis is correct. However, keep in mind that an 8D vector is triality-equivalent to an 8D chiral spinor wrt so(8) or so(4,4).

Daniel: The triality operator is an element of the E8 Lie group, and can be used to gauge-transform between the vector and positive and negative chiral spinor parts of E8. I'm afraid I don't know of an example.

101. **Wolfgang**
November 28, 2010

Chris said:

"i guess he just means that … you can add 2 carbon copy generations and by hand…"

If that's what he means, then there is even easier solution:

1) Give large vector-like mass to the fermions that Garrett found "in E8" (distler-garibaldi theorem guarantees these fermions are in vector-like representation, so can be given a gauge-invariant mass).
2) Add 3 chiral generations "by hand".

Problem solved! No need for triality or other fancy mechanism.

Garrett said:

"Without triality it "works" in the same sense that the SO(10) GUT works — there are many predicted particles, and the three generations are left unexplained."

Unexplained, or unexistent? If you don't allow to add fermions "by hand", then I would say "unexistent." If you allow to add fermions by hand (as in SO(10) GUT), then "unexplained" is more accurate, but then why big fight with distler-garibaldi, who explain how to get rid of unwanted (and now unnecessary) fermions originally from E8?

"For the cosmological constant and renormalization, the $\frac{4}{3} \Lambda e e$ term might be independent but related to the $\phi^2 e e$ term."

What does "independent but related mean"? Are these independent terms in the action, or do they come from expanding a single term in fluctuations about VEV?

102. **Tony Smith**
November 29, 2010

Garrett and Daniel, the simplest example of inherited triality within a larger group containing so(8) is 52-dim F4 which is made up of:
28-dim so(8) D4
8-dim vector 8_v
8-dim +halfspinor 8_s+
8-dim -half-spinor 8_s-

Pierre Ramond describes F4 (mostly from the point of view of superstring theory, but the basic math structures are the same regardless of physics point of view) in hep-th/0112261 and hep-th/0301050. Please read the papers for a lot of very interesting details that may be relevant to E8 physics models. Here, in this comment, I will only mention a general F4 quote and a quote about fermions, bosons, and spacetime dimension.

In the earlier paper 0112261 Ramond says:
"… the triality of the … little group so(8) … links its tensor and spinor representations via a Z3 symmetry …
The exceptional group F4 is the smallest which realizes this triality explicitly …".

In the later paper 0301050 Ramond says:
"… In four dimensions, fermions and bosons are naturally differentiated, as fermions have half-integer helicities while the boson helicities are integers. …
In d+1 spacetime dimensions, fermions transform as spin representations of the transverse little group so(d-1), while bosons are transverse tensors.
As a result … In 9+1 dimensions, the little group is so(8), with its unique triality property according to which bosons and fermions are group-theoretically equivalent …
This triality is explicit in the F4 [containing] so(8) decomposition …".

In short, by Ramond's 0301050, the nice fermion-boson correspondence in E8 physics does NOT work with a 4-dimensional spacetime, but requires a 10-dim spacetime as in superstring theory (or at least an 8-dim spacetime if you reduce it to the so(8) vector structure),
so
Garrett's E8 over 4-dim spacetime structure will NOT inherit the nice fermion-boson correspondence and WILL violate spin-statistics
(unless its spacetime structure is enlarged to the 10-dim of string theory or at least to the 8-dim Klauza-Klein of Batakis).

Tony

PS – Note that an 8-dim Kaluza-Klein can be extended to 10-dim
by extending its 4-dim Minkowski part to a 6-dim Conformal spacetime.

103. **MarkusMaute**
December 1, 2010

"I find it more likely that some major new ideas about the relationship between internal and space-time symmetry are still needed."
Don't we have them already in noncommutative geometry ? (According to Alain Connes, NCG allows for breaking the chains of the Coleman-Mandula theorem). I wonder if non-commutative geometry plays any role in Lisi's E8 model, given its success ? (E. g. the derivation of the full-fledged Lagrangian of the standard model).

104. **Wolfgang**
December 2, 2010

It seems Garrett has lost interest in answering questions about his theory. But I'll ask this one anyway.

Nesti-Percacci study SO(3,11) theory. Their SO(3,11) gauge fields form a subset of E8 gauge fields. And their frame field (1-form with values in the 14 of SO(3,11)) also sits in E8.

So one might expect that their theory forms a subsector of yours. Is that correct? If so, do you agree with their analysis of that subsector? If not, where would you say their analysis is incorrect?

105. **Garrett**
December 2, 2010

Wolfgang:

> Unexplained, or unexistent? If you don't allow to add fermions "by hand", then I would say "unexistent." If you allow to add fermions by hand (as in SO(10) GUT), then "unexplained" is more accurate, but then why big fight with distler-garibaldi, who explain how to get rid of unwanted (and now unnecessary) fermions originally from E8?

Yes, if one is less ambitious, then there are many ways to make things work by hand. But I am not so interested in that. I want to figure out how the three generations derive naturally from geometry, and I think triality in E8 may do that.

> Are these independent terms in the action…

I suspect they might be independent terms that could arise in the perturbative expansion when doing the renormalization. But I'm just guessing. It would be really nice to also have a geometric understanding of why the action is what it is, which might shed some light on this question.

106. **Garrett**
December 2, 2010

Tony:

Funny you should mention F4… that's what I'm working on right now, as a warm up. The triality automorphisms of so(4,4), and corresponding automorphisms of the F4 Lie algebra, are tricky, but fascinating! You make an interesting point about 8D vs 4D spin-statistics; I'll ponder that.

107. **Wolfgang**
December 3, 2010

"But I am not so interested in that. I want to figure out how the three generations derive naturally from geometry, and I think triality in E8 may do that."

If one is going to add fermions "by hand", is hard to see any advantage of your "E8" theory over Nesti-Percacci SO(3,11). So, yes, your triality idea seems to be "last best hope" for E8 theory.

"I suspect they might be independent terms that could arise in the perturbative expansion when doing the renormalization."

In your paper, they come from expanding a single term in the action about VEV, so it is hard to see how that could be true.

On the other hand, Nesti-Percacci (this is why I have been looking at their paper) have a very different analysis of this subsector of your theory.

Could it be that their analysis is the correct one?

Assorted News

November 18, 2010
Categories: Experimental HEP News, Uncategorized

- HEPAP is meeting in Washington today, presentations available here. The idea of this regular meeting is for the US HEP community and the funding agencies to meet and plan for the future, something that's not easily done in an environment where these agencies have no budget at all for the current year, just an authorization to spend money at last year's rate that expires in a couple weeks from now. No one seems to be sure what funding prospects are for the next few months, much less the next few years. Fermilab is dealing with this situation by offering 600 of its staff incentives to quit or retire next month (see here). There's a new DOE Committee of Visitors report out here, it contains the bizarrely familiar recommendation of all such reports: the US needs to fund more HEP theory students (they don't explain why, or where the money should come from).
- In dark matter news, Princeton this week hosted a workshop on the subject, talks available here. Still no results from the latest Xenon100 run. This week's Nature has a nice review of the various searches for WIMP dark matter, with conclusion:

> With the advent of the Large Hadron Collider at CERN, and a new generation of astroparticle experiments, the moment of truth has come for WIMPs: either we will discover them in the next five to ten years, or we will witness their inevitable decline.

(Update: a commenter points out that this article is also available on the arXiv here.)

One new astroparticle experiment that is supposed to look for evidence of dark matter is Sam Ting's Alpha Magnetic Spectrometer, set to be launched in February, and described in a front-page New York Times article yesterday.

- Ten days after first collisions, Alice already has two papers out (here and here) with experimental results on lead-lead collisions at an energy more than an order of magnitude higher than ever before. String theorists are very enthusiastic about this (see here and here), claiming that what is being observed is "properties of a type that can be nicely captured using string theory models". I'd be quite curious to see any AdS/CFT based predictions that could be compared to these new results (or to forthcoming ones).
- For the latest from the LHC, see here. Current plan is to have a proton-proton beam back around February 21, followed by at least 2 weeks of beam recommissioning. The proton run would end in November, followed by another ion run. First estimates for 2011 are that the run will be at 4 TeV/beam, and a "reasonable" estimate of total luminosity would be 2.2 inverse femtobarns, double the initial goal. Even more optimistically, the possible "ultimate reach" for next year would be a luminosity that would give a total of 7.2 inverse femtobarns if sustained over the hoped for 200 days of running. This kind of higher luminosity would allow the LHC to see evidence of a Higgs over the entire expected range, as well as allowing it to finally overtake the Tevatron in the Higgs race. The experiments so far are reporting results that match exactly the Standard Model, more announcements to come at the Winter Conferences early next year.
- There's an interesting trend of our LA-based theorist-blogger-media-stars starting to resist making dubious media appearances. A few months ago Sean Carroll described storming off the set of a TV pilot here. Now Clifford Johnson (whose media mishaps include appearing as a scientific expert on the question of how big women's breasts need to be to crush beer cans, see here) tells us that Sometimes I Say No.

Comments

1. **luny**
   November 18, 2010

   Regarding the heavy ion predictions: While the "low viscosity" discussion has generated a lot of publicity, it is somewhat unsuitable for quantitative comparisons: The number of free parameters and uncertainities is such that determining the viscosity to much better than an 100% error will be very difficult. Equally, determining stringy corrections to the "infinitely strongly coupled" value is somewhat controversial, especially in view that we do not,trivially live in a CFT world.

   Thus, "AdS/CFT viscosity over entropy density=1/4pi" is a great publicity gimmick, but not really an experimentally verifiable prediction.

   The best hope we have for the latter is the interactions heavy quark physics. Here, there will be very high quality data at the LHC, AND the strongly coupled theory might allow something of a systematic expansion whose results are _qualitatively_ different from the perturbation theory.

   As a matter of fact, some of the "pioneers" on this live a few hundred meters from you:
   http://arxiv.org/abs/0706.2336
   http://arxiv.org/abs/0907.3062
   http://arxiv.org/abs/1009.2286

2. **Anonymous**
   November 18, 2010

   Thus, "AdS/CFT viscosity over entropy density=1/4pi" is a great publicity gimmick, but not really an experimentally verifiable prediction.

   Hasn't it been verified already at RHIC, with an error of around 1/N^2=10%?

3. **luny**
   November 18, 2010

   Not really,although there have been excellent tries (the latest effort is
   http://arxiv.org/abs/1011.2783).

   Baiscally, initial conditions in the transverse plane (currently controversial and well studied) give you an 100% or so error
   (viscosity is really ~1/4pi for "Glauber" initial conditions but more like 2-3/4pi for "Color Glass condensate" ones).

   That leaves controversial initial conditions which have NOT been studied:
   How much flow forms BEFORE hydrodynamic expansion? How good is the longitudinal ansatz examined by the hydrodynamic calculations? What about corrections to Navier-Stokes?
   These, together, could easily add another 100%.

   Also, some observables do not fit the hydrodynamic theory at all (google "HBT puzzle"), which leaves some doubt that theorists have radically misunderstood the data. Its not impossible.

4. **Nige Cook**
   November 19, 2010

   "… Clifford Johnson (whose media mishaps include appearing as a scientific expert on the question of how big women's breasts need to be to crush beer cans, see here) …"

I believe this is a bona fide piece of research. Crushing cans flat makes recycling more efficient, because you can then fit more empty cans into recycle bins, which don't then need to be emptied so often, thus reducing the carbon-footprint of the can recycling enterprize. It's also good entertainment, after a few beers.

5. **John Rennie**
   November 21, 2010

   For those of us without a subscription to Nature, the article on dark matter is available at http://arxiv.org/abs/1011.3532

6. **J. E. Connett**
   November 21, 2010

   Peter,

   On Dec 9, 2009, I sent a message to your blog regarding the
   Hydrino Study Group, a forum which has since been discontinued. My
   comments included the following:

   "One of the regular posters to the HSG Forum, an
   electrical engineer named John Barchak, frequently cites your blog as
   evidence that quantum mechanics is hopelessly wrong."

   I would like to post a retraction of that statement. Mr. Barchak has
   often quoted your blog on the HSG, and I think it is fair to say he is not
   a fan of quantum theory, but in fact his quotations from your blog are
   generally in support of his views regarding the Standard Model and String
   Theory and the Higgs boson, and not directly in support of his views
   regarding QM itself.

   Thanks,

   J.E. Connett

7. **Peter Woit**
   November 21, 2010

   Thanks John. I've added that link to the posting.

8. **Estrella**
   November 22, 2010

   Hi,

   I would like to know your take on this article purposing a real life application of string theory:
   http://www.scientificamerican.com/article.cfm?id=string-theory-tackles-strange-

9. **Peter Woit**
   November 22, 2010

   Estrella,

   I'm not a condensed matter theorist, so I really have no idea how significant this is. It doesn't have anything to do with using string theory to get quantum gravity and unification. It's plausible that AdS/CFT might lead to new approximate models that would have condensed matter applications. But, given the history of hype surrounding string theory, I'd like to hear about this from an expert.

10. **Estrella**
    November 23, 2010

    Thanks for your opinion, Peter. 🙂
    I am not an expert on the subjet either. I am just a Physics PhD student (on the Complex Systems area), so I am not an expert on String theory or Gravity either.
    I just was curious about the subject.
    BTW, I live in Montevideo, and I know people who works in an alternative to string theory unification, quantum loop gravity.
    Do you have an opinion in this theory?

11. **Chris W.**
    November 23, 2010

    Estrella,

    Search the blog for "loop quantum gravity" and "LQG". Some of the posts (and comments) you find will include links to critical papers on the subject, as well as discussion of LQG and occasional cautious comments by Peter. For background you should look up some review articles, such as those at LivingReviews.org.

12. **Marcus**
    November 23, 2010

    Estrella, since you live in Montevideo and know some LQG people, you have undoubtably heard of Rodolfo Gambini: a major figure in the field. His research group has come out with a number of interesting and creative papers over the past 10 years. Montevideo is the top LQG place in South America, and one of a handful of top places worldwide.

    Another person at Montevideo who has contributed notably to the development of LQG is Michael P. Reisenberger. He collaborated with Carlo Rovelli around 1996-1998 on the first *spin foam* papers, an approach to LQG which has become increasingly important in recent years.

13. **Estrella**
    November 23, 2010

Thanks for the links Chris.
Yes, Marcus, I know Rodolfo Gambini and Michael Reisemberger. And I heard of their ideas on some talks they gave. Just curious what other people not related to them thought on their theories.
Moreover, as I said before, I am not an expert on that area, so I am just curious to hear all the bells.

14. **Estrella**
November 23, 2010

By "Hear all the bells" I meant hear all the opinions, pro and con.

15. **Estrella**
November 23, 2010

BTW, I am not an expert on Gambini or Reisemberger ideas either. I just asisted to some talks, but I don't work with them or in their area.

16. **Marcus**
November 23, 2010

Estrella,

By "Hear all the bells" I meant hear all the opinions, pro and con.

I understand and I think you are asking a good question, but Peter's usual policy is to discourage us from discussing non-string quantum gravity here, because he doesn't feel that he knows enough about Loop-and-allied approaches to be a good moderator.

Personally I would say that Loop (and related spinfoam) quantum gravity and cosmology have changed radically since 2006 and have made remarkable advances. To where phenomenologists begin to study how test (and even possibly to falsify). Relative to the size of the field a few years ago, there are a lot of young researchers, new energy, and even more job openings than before. So it is an exciting time.

One way to hear various opinions is to go to Physicsforums, this part of it: http://physicsforums.com/forumdisplay.php?f=66
and ask questions. Then people will sometimes point you to discussions that have already occurred—and sometimes, instead of that, when you ask a question they will begin immediately to argue among themselves.

17. **Peter Woit**
November 23, 2010

Yes, please, enough about LQG right now. I have to keep reminding people this is not a general discussion board, I don't want to manage such a thing.

I think there's a lot interesting about LQG, but lack motivation to spend much time on quantum gravity theories that don't unify with the rest of physics and tell us where the standard model comes from. I'm willing to believe that everyone who has a quantum gravity for sale may be right, which means that there are at least 10^500 of them….

18. **Estrella**
November 23, 2010

Thanks for all the answers. 🙂

19. **Marcus**
November 23, 2010

Estrella, here are 281 LQG papers from the two years 2009 and 2010 assembled by a Spires database search.
http://www.slac.stanford.edu/spires/find/hep/www?
rawcmd=FIND+K+SPIN%2CFOAM+OR+DK+FIELD+THEORY%2C+GROUP+OR+DK+QUANTUM+GRAVITY%2C+LOOP+SPACE+OR+QUANTUM+COSMOLC

You can change the parameters as you wish–get the papers sorted by citations or in other ways, change the keywords, the timeframe. I have included spinfoam, group field theory, and loop quantum cosmology, which is the sector closest to being testable (by CMB observation).

20. **Anonymous**
November 24, 2010

@Peter Woit: You've called for hep-th community to diversify, and they did, but it seems that you're not impressed by any of the following: susy, string theory, LQG, Horava's theory, Verlinde's theory, Lisi's theory… So what exactly is the "diversity" that you long for? You're only impressed with stuff along the line of "elucidating the mathematical structure of non-supersymmetric QFT". If everyone is doing this, I'm sure the community will be truly diverse!

21. **Peter Woit**
November 24, 2010

Anonymous,

Susy and string theory are still by far the dominant research areas in the subject. LQG and Horava's theory also get attention, which is great. Verlinde's theory doesn't seem to me to be a theory at all, but maybe I'm missing something. Lisi's theory is being worked on by, well, Lisi. That's still not a whole lot of diversity.

You're right that if everyone was working on better understanding non-perturbative QFT using new mathematical methods, that would not be diverse. I think we're a long way however from having to worry about that particular danger right now….

22. **Loco**
November 25, 2010

@Peter Woit

If you ever comment on Penrose Conformal cyclic cosmology, could you put the links again? There's a 6σ claim going on:

http://fr.arxiv.org/ftp/arxiv/papers/1011/1011.3706.pdf

23. **Peter Woit**
November 25, 2010

Loco,

My only comment about that claim is that I'm not a cosmologist and have no way to evaluate it. If someone else can point to somewhere an expert in the subject has commented on this, that would be interesting.

24. **Derek Teaney**
November 25, 2010

Responding to

"Thus, "AdS/CFT viscosity over entropy density=1/4pi" is a great publicity gimmick, but not really an experimentally verifiable prediction."

Perhaps I can offer some perspective (I work in this field). I would not say that
1/4pi is a publicity gimmick. The importance of the AdS/CFT
result is that there exsist field theories (in any theory!)
where is \eta/s is as low as 1/4\pi. Before this calculation
in the AdS/CFT it seemed extreme to imagine that QCD
would have \eta/s less than 1.

The other important point is that the gradient expansion
which underlies the hydrodynamic results at RHIC would
not converge (for typical initial) conditions for \eta/s > 0.4.
For say η/s less than 0.3 the gradient expansion
does clearly converge, i.e. ideal hydrodynamics provides
the baseline result, the navier stokes theory provides a correction,
and the second order corrections are small.

Thinking about hydrodynamics in AdS/CFT led to a precise
formulation of second order corrections in hydrodynamics.

25. **Loco**
November 25, 2010

To date I found only one comment which seems interesting.

http://www.physicsforums.com/showthread.php?p=3003554

26. **Casey Leedom**
November 26, 2010

And back to your original blog posting, re "an interesting trend of our LA-based theorist-blogger-media-stars starting to resist making dubious media appearances," check out the latest Abstruse Goose:

http://abstrusegoose.com/321

Casey

27. **luny**
November 27, 2010

Derek… I do not disagree that 1/4pi is a very important result from a theoretical point of view which spurred quite a lot of interesting work.
It is however wrong to claim that "Before this calculation
in the AdS/CFT it seemed extreme to imagine that QCD
would have \eta/s less than 1". A "lower limit" on viscosity in a quantum field theory was argued for in the '80s, in the context of heavy ion collisions, from back-of-the-envelope arguments stemming from the uncertainty principle, see
Phys.Rev.D31:53-62,1985
(the limit is surprisingly similar to 1/4pi!).
So its not quite correct that the AdS/CFT result is the first result suggesting that a quantum field theory can exhibit hydrodynamical behavior at microscopic scales.

In any case, by "publicity gimmick" I mean that, while there have been a lot of popular science implying that string theory calculations can be quantitatively tested in heavy ion collisions, this has simply not been the case: The phenomenological uncertainity in hydrodynamic simulations, and the lack of knowledge of how to relate infinitely strongly coupled N=4 Nc=infinity SYM to not-quite-infinite-coupled N=0 Nc=3 QCD prevents any kind of real comparison at present.
I am not saying this comparison is impossible in the future, but at the moment it can not be done.

It of course does not mean this work is a waste of time! The Sine-Gordon model was also very important for field theory which led to important theoretical developments of QCD, but it would be a "publicity gimmick" to say it can be used as a quantitative test for anything.

28. **Derek Teaney**
November 27, 2010

Luny,

The calculation you refer to was a extrapolation of weak
coupling results where $eta/s >> 1$ deap to a regime where it is not valid.
So it was terribly unclear wether such field theories actually
exsist. The only thing which was clear from such extrapolations
was that when $eta/s \sim 1$ something must happen.

The 1/4\pi showed that there exist field theories (where you
can calculate at strong coupling) . This result gave a kind of
reason to suggest that hydrodyamics (as opposed to alternatives which don't seem to hang together anyway)

was actually responsible for the observed elliptic flow

You write,

” In any case, by “publicity gimmick” I mean that, while there have been a lot of popular science implying that string theory calculations can be quantitatively tested in heavy ion collisions, this has simply not been the case ”

This is true. Last comment, the heavy quark results you quote above
are really no better in this regard

The BMO Financial Group Isaac Newton Chair in Theoretical Physics

November 29, 2010
Categories: Uncategorized

I learned this morning from Matin Durrani's blog that the Perimeter Institute has announced today the first of what they expect to be five very well-funded Perimeter Research Chairs in theoretical physics. The next four will be named after Maxwell, Bohr, Einstein and Dirac (as well as whatever other wealthy individual or organization comes up with funding).

The BMO Financial Group is putting up $4 million and $4 million is coming out of the Perimeter endowment (which is mostly from Blackberry's Mike Lazaridis). An endowment of $8 million for a chair is quite high. It seems that typical numbers for endowment payouts these days are around 5%, so this would make available $400,000 or so a year to pay some prominent theorist. For comparison, the Simons Foundation has recently announced that it will fund endowed Math+X chairs aimed at mathematicians working at the interface with some other subject. Simons may be the wealthiest hedge-fund manager in the world, but he's a piker compared to the Canadian financiers, with only $1.5 million going to each chair (to be matched by $1.5 million from the institution that gets the chair, for a total of $3 million). Then again, it just may be that prominent mathematicians are dirt-cheap compared to prominent theoretical physicists.

The Perimeter Institute in recent years has moved away from supporting non-mainstream topics in theoretical physics, while expanding dramatically. The only two conferences announced there for the next year or so are on the topics of LHC physics and AdS/CFT, about as mainstream as one can possibly imagine. If they manage to fund what might be the five highest-paid theoretical physics positions in the world and hire the people they want into them, they will be well on their way to a dominant position in the subject. While, like most industries these days, the tactic here is to shower the top few people in the field with cash, they are also expanding their hiring at more junior levels. According to the rumor mill, last year out of a total of fourteen people hired to tenure-track positions in theoretical particle physics in North America, three of the fourteen went to Perimeter.

For more on this, see here, and an interview with Lazarides and the BMO CEO here.

Comments

1. **Gregor Samorasky**
   November 29, 2010

   Wow! Only "fourteen people hired to tenure-track positions in theoretical particle physics in North America"–in America and Canada combined!! Wow!!

   Who/where is still saying there will be a shortage of physicists?

   🙂

2. **Kalyan**
   November 29, 2010

   It would be nice if the 400K/yr funding would include 2-3 post doc salaries to both spread the wealth and support the chair holder.

3. **Peter Woit**
   November 29, 2010

   Kalyan,

   PI already has about 45 postdocs, and surely their new 400K/yr researcher will have some say in choosing these.

   Actually, if they're taking $4 million out of the endowment, that may mean a bit less money for postdocs.

4. **$$$$**
   November 30, 2010

   half would go in Canadian taxes?

5. **Mark Decker**
   November 30, 2010

   It is sad that Perimeter has become more mainstream. Kind of defeats their original purpose? Look on the bright side though. If string theory is eventually verified then movie theaters will be able to start showing movies in 11-D. I wonder what the glasses will look like?

6. **Robert Guttenberg**
   December 1, 2010

   Are these chair endownments higher than those at the IAS? I read Simonyi donated $25 million in 2005…

7. **Peter Woit**
   December 2, 2010

   Robert,

   The Simonyi $25 million was a general gift, not for an endowed chair. Looking at the IAS site, the last gift of an endowed chair that I see announced was at $5 million. It's unclear exactly how Perimeter plans to spend the $8 million, and at other institutions one doesn't know for sure if an announced gift for an endowed chair will cover the entire expenses involved.

   For another point of comparison, endowed chairs at Harvard Law School supposedly go for $4 million, and law professors tend to be among the best paid of academics.

   All in all, theoretical physics may not be doing so well intellectually, but it appears that the financial prospects of its stars are bright.

8. **rrtucci**

December 2, 2010

I think this is so wrong. A good theoretical physicist does does not crave for large amounts of money. Large amounts of money just distract and pervert him.

Anyway, how long can BMO continue their charitable contributions at high levels. Not very long, I dare say. Their products have become quite obsolete in the past year, due to Android and iPhone.

9. **rrtucci**
December 2, 2010

Also, good theorists usually thrive in places where there is good experimental physics going on too. (Think Bell Labs) My impression is that Canada has very little good experimental physics and technology going on. In my field of quantum computing, they are a giant flop. Perimeter institute specializes in turkey technology like quantum cryptography (which is snakeoil) and NMR quantum computers (which are non-scalable and an obvious dead end)

10. **Peter Woit**
December 2, 2010

rrtucci,

BMO is the Bank of Montreal, and as far as I know Canadian banks are doing fine. It's RIM, the company of Lazaridis that makes the Blackberry.

11. **rrtucci**
December 2, 2010

Yes. I should have said RIM.

Why is BMO doing this.? I thought Canadian banks are much less leveraged, and therefore poorer than American ones. Is Lazarides behind this BMO move, somehow?

12. **Peter Woit**
December 2, 2010

rrtucci,

On the scale that large financial organizations operate the highest paid theorist in the world is working for peanuts, and funding even the most highly endowed chair in the world doesn't involve coming up with what they consider a lot of money. I believe BMO's profits are a couple billion dollars a year or so.

13. **anon.**
December 2, 2010

I've been trying to think of anyone who is prestigious enough to live up to PI's expectations for these chairs, but also plausibly willing to leave their current job and move to Waterloo. I'm coming up short.

14. **Peter Woit**
December 2, 2010

anon.,

It's not just "leave their current job and move to Waterloo", but maybe "leave their current job, where things are kind of depressing, move to Waterloo, double their salary and be able to hire lots of people in your field". One advantage Perimeter has is that lots of other places are in financial trouble (think University of California, Great Britain and lots of the rest of Europe). Many US physics departments aren't so keen on speculative things like quantum gravity research these days (be it string theory, loop quantum gravity or anything else). So there are not a lot of places out there offering well-paid positions to theorists at a large and expanding theory group. The Simons Center at Stony Brook is in a somewhat similar position, but they're going after mathematicians and the mathematical end of theoretical physics, which is something Perimeter doesn't seem to have much interest in.

So, it's not an easily solved problem, but neither is it an insoluble one…

15. **Jack Lothian**
December 2, 2010

rrtucci,

I am not sure that being less levered makes one poorer? Some might argue the opposite. Canadian banks operate under tighter regulation than American banks. The few Canadian banks rival the larger American banks in size & have earned incredible returns year-in-year-out for over a century. Unlike in the US, in Canada, small banks do not exist. So despite the fact Canada is a tenth of the size of the US, our banks are very big by North Americam standards. I suspect our big 5 banks rank in the top 25 banks in North America.

Waterloo is not out in the Canadian wilderness. It is effectively a suburb of Toronto which is very big city even by American standards & it consistenly ranks well in various surveys of city quality. I agree it is not currently a hotbed of experimental physics research but there are a lot of very good universities situtated within a hundred miles of the PI. Waterloo is dead center in our largest concentration of universities. During the previous tech boom many of these universities did stand out.

Math Research Institute, Art, Politics, Transgressive Sex and Geometric Langlands

December 4, 2010
Categories: Uncategorized

I learned from a colleague last night about recent events bringing together the topics of the title of this posting, something that one wouldn't have thought was possible. Last Wednesday there was a showing in Berkeley of Edward Frenkel's short film *Rites of Love and Math*, together with the Yukio Mishima film *Rites of Love and Death* that inspired it. Frenkel is a math professor at Berkeley, and one of the leading figures in geometric Langlands research (which he describes as a "grand unified theory of mathematics"). He's also a wonderful expositor, almost single-handedly making the beauty of a subject initially renowned for its obscurity accessible to a much wider audience. Recently he has worked with Witten on relations of geometric Langlands to quantum field theory, and with Langlands and Ngo on relations to number theory. At the same time, while a visiting professor in Paris, he co-directed (with Reine Graves) and acted in this new film.

MSRI was one of the two sponsors of the showing of the film, but pulled out of this role recently, for reasons explained here by MSRI director Robert Bryant. He had found that some people in the math community were upset by the film and MSRI's involvement with it, feeling that it glamorized an objectionable view of the relationship of women to mathematics. There's a plan to organize some sort of event at MSRI to discuss the issues brought up by the film and the decision to withdraw sponsorship.

I still haven't seen the film, although I gather that a DVD will soon be available. Congratulations to all involved in this for finding a unique way to make mathematics and mathematicians look interesting and worthy of media coverage. I had no idea it was still possible to stir up controversy in the Bay area with art involving transgressive sex, and would never have thought that using research mathematics was the way to do it.

Update: Andrew Ranicki has written a review of the film for the London Math Society newsletter, available here. He identifies the notorious equation in question (5.7 of http://arxiv.org/abs/hep-th/0610149), and makes the comment that, sartorially, this film is a breakthrough, since, in other films:

> By and large, male mathematicians are portrayed as crazies who are smart and lovable, but badly dressed. Likewise for female mathematicians, although they tend to be better dressed. This said, in the □film under review, the actors are either very well dressed, or not dressed at all.

Comments

1. **feministisch**
   December 4, 2010

   I didn't see the movie. But what I read about it was more than enough to make me disgusted. There's enough (barely) hidden misoginy in mathematics as is. I'm glad the MSRI has withdrawn support.

2. **ObsessiveMathsFreak**
   December 4, 2010

   From the trailer, the film looks like a standard "high art" pornography flick to me. I'm not surprised that people objected to the MSRI being associated with it.

   Does this mean that mathematics isn't an art after all?

3. **Bugsy**
   December 6, 2010

   Between the right and left-wing thought police it seems there is no place left for art, expression or intellect in the US of A. For MSRI to cow under to uninformed anti-art politics is disgraceful. Not one of the critics seems to have seen more than the trailer, and no one accusing the film of "sexism" seems to have noticed that the director is, apparently, a woman.

4. **Chris W.**
   December 6, 2010

   I notice that NEW mentioned this film (and quoted from IMDB) in a post from last April — the day after April Fools Day.

   I guess that's why I didn't remember it. (*Cough*…)

5. **MathPhys**
   December 7, 2010

   He writes on her belly, pucturing her soft flesh with a sharp pen. She screams in pain. He continues to write.

   Since when is doing mathematics meant be physically painful to another person? I thought the whole point and main attraction of pure mathematics as well as all of pure science is that it's harmless. You don't abuse, exploit or hurt another person.

   And I didn't enjoy the sight of his backside. I only want to read his papers. I don't want to see his backside. But it was in the very first frame of the trailer, so it was forced on me. Maybe that's another reason why so many people were disgusted by the trailer. He made us see his backside.

6. **tulpoeid**
   December 8, 2010

   Impressive how people think that every single image in a movie is about conveying the maker's intention, without any reference to the overall plot or the symbolism or the hidden ideas. Also, impressive how people think that a woman can't direct sexist movies (ahem, just have a look at the number of women editors in glossy sexist magazines). Ok, has anyone watched the whole of it?

7. **Aki**
   December 8, 2010

   "Bodily violence is a displeasure done with the intention of giving pleasure"

   –John von Neumann

   http://www.reference.com/browse/john+von+neumann

8. **andreask**
December 8, 2010

I think, at least judging by the trailer, one has to contextualize the film, without contextualizing it, it is mainly an aesthetically appealing example of what germans would call erotic/exotic 'edel kitsch', by contextualizing, as it was already mentioned it does indeed tell the audience something about how women are represented my male mathematicians, also about how women of color are represented by white male americans. I suppose the rest of the film, granted its title, supports these views. Certainly one has to consider Yukio Mishima's original in the context of his biography, saying this, there is certainly a non-vanishing amount of narcissism and homo-eroticism also involved and that is of course also reflecting 'something' in (mathematical) reality..so this seems interesting in many ways.

9. **ahmed**
December 8, 2010

"You don't abuse, exploit or hurt another person."

Some poor grad students would disagree

10. **Richard Mlynarik**
December 8, 2010

Non-review of the movie makes the local free paper: http://www.eastbayexpress.com/ebx/erotica-intrigue-and-arithmetic-in-rites-of-love-and-math/Content?oid=2258787

11. **Chris Oakley**
December 9, 2010

From UC Berkeley News:

> In Frenkel's film, the mathematician faces a quandary familiar to theoretical scientists. He has found, at long last, the mathematical formula of love. But then he realizes that others could use his formula to cause harm — and that he must die to safeguard the world. He saves the formula by etching it into his lover's body.

Familiar to theoretical scientists? In my research years I do not remember having to confront this particular issue (although I thought that Lie groups were kind-of neat). But if another reader of this blog has discovered the mathematical formula of love and then realised that since it could be used to cause harm the only decent thing to do is to die having etched the formula into their lover's body, then I would be interested to hear. Oh – wait – you may be already dead!

12. **GeorgeDorn**
December 10, 2010

Offtopic but FYI: "The results may also allow experimental tests of string theory in the future." — http://www.physorg.com/news/2010-12-fahrenheit-neutron-stars-theory.html

Something for you to debunk perhaps.

Enjoy reading your blog. Keep up the good work.

Regards,
George.

13. **Kea**
December 10, 2010

George, so that would be something like the 1/4pi universal value. And no theory but string theory is capable of deriving a number like 4pi?

14. **Peter woit**
December 11, 2010

George,

I guess I do need to keep up on these things, so done. Glad you enjoy the blog!

Peter

15. **Thomas Larsson**
December 12, 2010

A very inspiring trailer!
http://vimeo.com/17736325

16. **Chris Oakley**
December 13, 2010

Thomas,

I see that you too are getting into this whole etch-your-equations-into-your-lover's-body-and-then-kill-yourself thing. All I can say is, don't do it! After all (i) if your equations turn out not to be harmful to mankind anyway, you will have died for nothing; (ii) thinking that one's equations are that important is in any case pretty arrogant; (iii) your wife/lover is entitled to an opinion on all of this <aside>interesting moral point: would a wife have cause to be angry if her husband killed himself after etching his equations into *another* woman's body?</aside>

17. **elsie**
December 16, 2010

Ok, so this is another movie about a non-white woman being subjugated and perhaps arguably tortured by a white man? *Yawn*. Oh please. I do not see this being a big hit outside of Berkeley and some mathematics circles, if only for the controversy. Now what really would have been interesting: the submissive woman gets her moxie, the tides turn, she subjugates the man, steals the formula, and…. could be a variety of more interesting endings.

18. **Kea2**
December 20, 2010

Mishima's film is generally seen as an extreme expression of his narcissism and ultra-romanticism; in other words, Mishima at his worst. It is a little odd that someone would want to emulate this.

19. **andreask**
December 20, 2010

elsie, indeed from an exterior-mathematical perspective this event bears nothing new at all, what I meant above saying the discourse around the film would be interesting in 'many ways' is mainly that it possibly sheds light on a certain degree of (again, in traits narcissistic) ignorance that the mathematical community would treat such subjects as 'gender' or 'critical whiteness studies', in fact Frenkel's remark, that he couldn't deal 'with any problem of the world at the same time' is quite a typical example of the manifestation of the 'white privilege', the unspoken racist discourse creates the pictures the audience has in mind when watching and the 'other' need to manifest an explicit counter-discourse to oppose the image. I actually wonder if the bare fact that the subjugated women in the film is non-white made any appearance in the mathematical discourse on the film yet and certainly the possible argument that Frenkel is just 'replacing' the male character in Mishima's original by himself without further consequences is quite misleading.

20. **Oh well...**
December 21, 2010

Most of those who have posted comments above have not seen "Rites of love and math". But apparently this is not considered as a serious impediment to expressing "expert opinion" and passing judgement.

I think many of these "experts" would be surprised, and perhaps even embarrassed, by their comments if and when they watch the film in its entirety.

For example, consider this from 'elsie':

"Ok, so this is another movie about a non-white woman being subjugated and perhaps arguably tortured by a white man?"

No, it's not. But 'elsie' is not really asking a question. He/she already "knows" what this film is about and in particular offers this brilliant suggestion:

"Now what really would have been interesting: the submissive woman gets her moxie, the tides turn, she subjugates the man, steals the formula, and…."

Well, 'elsie' might be surprised to find out that this is pretty close to what happens in this film.

SPOILER ALERT!

At the end of the film, the Mathematician kills himself and Mariko, the female character, walks away with his formula, leaving him dying on the floor.

The description of Mariko as "subjugated woman" is concocted by those who feel perfectly comfortable judging a film after watching a two-minute trailer.

It seems that these comments tell us more about the writers and their prejudices than about the film itself.

Another interesting comment from 'elsie':

"I do not see this being a big hit outside of Berkeley and some mathematics circles, if only for the controversy."

Interesting… Looking at the film Web site http://ritesofloveandmath.com, we find that the movie had a successful premiere in Paris last Spring and was selected at international film festivals, including a presigious festival in Spain in October. It has also been reviewed favorably in Le Monde, on Twitch.com (a popular Web site about movies), and in other publications…

Finally, about Kea2's comment:

"Mishima's film is generally seen as an extreme expression of his narcissism and ultra-romanticism; in other words, Mishima at his worst."

Really? I would be interested in seeing this claim backed up by something other than the Kea2's apparent self-confidence. In fact, Mishima's film was released on DVD in the United States by the Criterion Collection, which is a very important DVD series. And a cursory search on Google produces a large number of admiring reviews of his film.

21. **andreask**
December 21, 2010

so the questions remain a) why was the film thought to be advertised 'sufficiently well' by a trailer which conveys the impression that it contains at least to a certain degree misogynic content if it allegedly does not b) does commercial success or at least a 'succès d'estime' on european film festivals imply that the criticism presented above is wrong.

22. **Kea2**
December 23, 2010

@Oh Well. My comment was based on my knowledge and understanding of Mishima's oeuvre, not on a google search for the film.

23. **Oh well...**
December 24, 2010

@Kea2: Thanks for the clarification. In your original comment you wrote: "Mishima's film is generally seen as…" suggesting that you are not expressing your own opinion (to which you are certainly entitled), but rather a consensus view of the film. My point was that what you wrote is NOT the concensus view of Mishima's film.

24. **ak**
December 24, 2010

possibly Frenkel's remark

"(..)the film is an allegory in which the female character represents the truth and mathematics(..)"

exemplifies already the problematic traits in the general conception, the film is intensely absorbed in 'traditional' gender roles, the woman in 'patriarchal' societies representing the passive ‘truth’, ‘nature’ and the ‘unexplained’, and while this is obviously in perfect accordance to the film mainstream and good parts of its non-mainstream in Europe or the US, a mathematical environment still characterized by a grotesque under-representation of females has indeed reasons to oppose further amplification of what can clearly be understood as the ‘archaic truths’ of males and females in our societies.

This Week’s Hype

December 11, 2010
Categories: This Week's Hype

This week’s contribution to the long tradition of universities issuing press releases hyping non-existent “experimental tests of string theory” by their employees is from Duke University, which advertises “String Theory in a Lab“. This is based on a paper that just appeared in *Science* describing measurements of the viscosity of a Fermi gas. The paper explains the relationship of the measurements to string theory as:

> The measurement of the viscosity is of particular interest in the context of a recent conjecture, derived using string theory methods, which defines a perfect normal fluid.

referring to this paper which first suggested that gauge/gravity duality implied a value of $1/4\pi$ for the ratio of shear viscosity to entropy density.

In the press release, this connection to string theory has been promoted to a headline, as well as to the claim that:

> The results may also allow experimental tests of string theory in the future.

which I suppose is better than the usual claim in these press releases that what is being promoted is already an experimental test of string theory. It seems likely that one reason this isn’t yet an “experimental test” is that the data comes out 4 to 5 times higher than the string theory value.

Comments

1. **Amused**
   December 11, 2010

   Hi Peter,

   I`m just amused seeing You jump head on at every headline appearing somewhere containing the S-word, independent of what the context is …

   That`s so predictive and sure as death and taxes !

2. **Peter woit**
   December 11, 2010

   Amused,

   For years I’ve had a Google alert set up that lists new news stories involving the terms “string theory”. The long term pattern is that fewer and fewer of the things listed have anything to do with string theory itself. These days the majority come from music: “string theory” is very popular as the name of a musical group or event.

   Besides the music ones though, many of the rest are the all-too-familiar press releases promoting some kind of hype about a bogus “experimental test of string theory” (and the varying numbers of news stories that pick up the press release). I’m well aware that my pointing these out is all too predictable and kind of tedious by now, since I’m pretty bored with this myself. However, it still seems to me that someone should try and do something to slow down this phenomenon, and I’m not seeing anyone else interested in the job…

3. **Derek Teaney**
   December 11, 2010

   Peter,

   I have not read the article by Schafer et al. (I’m at home
   and its not free) but certainly the 1/4\pi result is not to be made
   light of. The 1/4\pi result does provide an “exact” example
   of a strongly coupled theory close to the quantum limit where
   the shear viscosity can be computed. Of course the 1/4\pi
   result does rely on
   the AdS/CFT and certain properties of black holes which haven’t
   been proven. Measuring the shear viscosity in fermions system
   close to unitarity does not prove the exsistence of strings or the validity of string theory. But, it does strongly suggest the inherent value in these methods as a tool of theoretical physics.

4. **Peter Woit**
   December 11, 2010

   Derek,

   I’m not making light of gauge-gravity duality, or its possible applications to computations in strongly-coupled quantum systems.

   I am making light of the endless less-than-honest attempts to use this to generate misleading press releases about “experimental test of string theory finally found!” Or, actually, I’m not really making light of it, I think it’s kind of disgraceful…

5. **Amused**
   December 12, 2010

   Ok Peter,

   the Google alert explains it …

   Perhaps nobody else does what You are doing because it`s really enough now and it`s the job of present and future experiments to decide what is right and what is not?

   Instead of trying to get any research on ST in particular or on all of the other approaches to quantum gravity which You seem not to be able to accept neither eliminated, it would be much more interesting to read about what You are working on and what You appreciate as good and fascinating news in physics …

Or perhaps Your intention is to merit a special chapter in the new
german popular science book “Kampfhähne der Wissenschaft” by
Heinrich Zankel ;-))) ?

See

http://www.amazon.de/Kampfh%C3%A4hne-Wissenschaft-Kontroversen-Feindschaften-Erlebnis/dp/3527325794

Seriously, I´d really prefere reading more about fascinating in interesting things You surely have to write about than seing this neverending picking on others …

At the present state of affairs, reading some physics text, on QFT for example, is much more promising for me to understand what is going on in modern physics.

Best wishes

6. **New**
December 12, 2010

Is the press release by string theorists? It looked like it was the experiementers at Duke who said (correctly) that it might be interesting from a string theory perspective. The Duke string theory group is into much more formal string theory, like CFTs for heterotic models etc.

In any event, I am not sure why this is hype. They say very clearly that the string theory computation is a lower bound and that the experiment is five times the bound or so. The actual claim is that “The results may also allow experimental tests of string theory in the future” which is hardly hype.

The reason why this is interesting is that standard approaches other than string theory give vastly bigger results, while the lower bound from string theory is within an order of magnitude of the experimental value.

What is so upsetting to Woit here is anybody’s guess.

7. **Peter Woit**
December 12, 2010

New,

The point is that this has nothing at all to do with experimentally testing string theory, either now or in the future, and those promoting this kind of hype know this, whether they’re condensed matter physicists or string theorists.

What one can’t now, but in principle might be able to test this way is the combination of some conjectured strongly coupled model for the system together with some conjectures from gauge/gravity duality about how solutions to the model behave. If such an experimental test fails it means either the model is wrong, or solutions to the model have different properties than conjectured.

The first alternative has nothing to do with string theory, and the second one is a mathematical question. Whatever the correct answer to the mathematical question is, there’s no “experimental test of string theory” there. If experiment disagrees with the gauge/gravity duality “prediction”, I doubt anyone is going to give up on gauge/gravity duality, instead they’ll give up on the particular strongly-coupled model used, or decide that the application of gauge/gravity duality in this situation is too naive.

8. **Chris Austin**
December 12, 2010

Hi Derek Teaney,

Would you be able to give the authors or title of the paper with page reference

Phys.Rev.D31:53-62,1985

mentioned by luny in response to your comment on the same topic here? I can’t find it on SPIRES just from the page reference.

9. **thomas**
December 12, 2010

Neither the heavy ion experiments nor the experiments involving dilute Fermi gases provided quantitative test of string theory right now. The heavy ion experiments do not produce an N=4 SUSY plasma at large N_c, and the Fermi gas experiments involve a scale invariant non-relativistic fluid, but not one of the systems for which a precise correspondence has been formulated.

The experiments nevertheless provide a path for testing the string theory framework. Ideally, this will happen when holographic dualities are extended to more realistic systems. Even if this turns out to be difficult, we can use the experiments to verify numerical calculations of transport properties (in lattice QCD or non-relativistic quantum Monte Carlo), and then check the dependence on the matter content numerically. If string theory correctly predicts the N=4 large N_c limit, and if at strong coupling the dependence on the precise matter content is indeed weak, then this would constitute a pretty non-trivial test of string theory as a quantum gravity framework, and a test of the holographic dualities.

Of course this does not directly tell us much about attempts at deriving physics beyond the standard model from string theory, but the Duke press release is actually fairly honest (on the scale of these things) in not claiming anything like this. The press release also points out connections to the physics of neutron stars. Here, the situation is not so different — some additional theory is required to extrapolate from cold atoms to cold neutrons.

Finally, as Derek points out, nobody can seriously doubt that string theory has been extraordinarily fruitful in thinking about strongly coupled fluids.

10. **Peter Woit**
December 12, 2010

Thomas,

You’re just exploiting ambiguities in language when you write about “testing the string theory framework”. Most people when they read that would assume that you are talking about the conjectured use of strings as elements of a fundamental theory, which is the way string theory has been advertised. You’re referring to something completely different, a string-theory-inspired mathematical conjecture that may or may not be right, and may or may not some day have a use as an approximate calculational method.

People have noticed that after 25 years the heavily promoted “string theory” predicts nothing and instead of admitting the reason for this failure, there’s an active campaign to avoid acknowledging this by misleading people with bogus claims of new “experimental tests of string theory”. I don’t think trying to justify these kinds of press releases is a good idea.

11. **Chris Austin**
December 12, 2010

With reference to my comment above, perhaps I should have pointed out that it would be interesting to see the arguments in the paper mentioned by luny, which luny states finds a limit that is surprisingly similar to 1/(4 pi), using back-of-the-envelope arguments stemming from the uncertainty principle, because Kovtun, Son, and Starinets find on page 5 that the uncertainty principle leads to a lower bound of about 1, rather than about 1/(4 pi), on the ratio of viscosity to entropy density.

12. **Derek Teaney**
December 12, 2010

Hi Chris,

The paper is a classic of our field

%\cite{Danielewicz:1984ww}
\bibitem{Danielewicz:1984ww}
P.~Danielewicz and M.~Gyulassy,
%“Dissipative Phenomena In Quark Gluon Plasmas,”
Phys.\ Rev.\ D {\bf 31}, 53 (1985).
%%CITATION = PHRVA,D31,53;%%

The point is though, that its based on extrapolation from weakly coupled theories to a regime where they are not valid, rather than
a valid scheme.

13. **Giotis**
December 12, 2010

Peter I’m curious; do you have any theoretical reasons to believe that String theory is wrong as a fundamental theory or the fact that it can’t be currently tested is an adequate reason for you to dismiss it altogether?

14. **Peter woit**
December 12, 2010

Giotis,

I wrote a whole book about this…

In brief, the mathematical structure of the standard model is quite marvelous and uses very deep mathematics. My guess is that going beyond the Standard Model will involve going deeper in those mathematical directions.

I never was a fan of the idea of throwing most of this out, with the idea that completely different objects (strings and extra dimensions) were what was really fundamental. When people first tried to get a unified theory from strings it was a reasonable (if not especially compelling) thing to try. It soon became clear though that this didn’t work. The lack of any predictions is just a symptom though of the underlying disease, which is that you have to choose more and more complicated backgrounds just to avoid contradiction with experiment. You never get more out than you put in, which is the signature of a bad idea.

15. **critic**
December 12, 2010

Peter wrote: For years I’ve had a Google alert set up that lists new news stories involving the terms “string theory”.

That’s quite an occupation you have invented for yourself. Is this the world’s newest profession?! If this is your occupation, of course you will always criticize these press releases, no matter what they say. Who cares anyway: you don’t have any working knowledge of string theory. It is easy to dismiss things you have no clue about!

Obviously, what you are doing is not stopping all the string theory chatter whether it makes sense or not. Quite the opposite, it brings it to the attention of a wider audience. And anyway, complaining is `un-American’ and rarely brings intended results.

You seem like a smart guy: find something better to do with your blog!

16. **Peter Woit**
December 12, 2010

Hi “critic” from Princeton, New Jersey,

You don’t have any substantive argument, but you seem to think an anonymous personal attack will help your cause. Thanks for the encouragement to keep devoting a small fraction of my time to this activity. If it’s causing behavior like yours, it must be having an effect…

17. **Peter Woit**
December 12, 2010

Many commenters claim to be annoyed that I’m spending too much of my time on this blog complaining about string theory. This seemed odd to me, since I couldn’t remember thinking or writing at all about the topic for quite a while until spending the fifteen minutes or so it took to write this post after reading the press release. So, I took at look at the last ten posts on the blog. No complaints about string theory in any of them, the only real reference to the subject was some links to postings about ALICE results in one of 5 parts of one multi-part post.

It’s pretty clear that as far as certain people are concerned, the problem is not that I’m spending too much of my time criticizing string theory, it’s that I’m doing it at all.

18. **luny**

December 12, 2010

To Derek and Chris, of course the limit in the paper is a “back of the envelope estimate”, since it uses an approximation beyond its range of validity.
However, while the “AdS/CFT limit” is certainly more accomplished, its physical foundations are exactly as shaky. We do not know to what extent does the strongly coupled theory with 4 supersymmetries and an infinite number of colors approximate any physical system. We do not know how stable is the “limit” against perturbative corrections (see, for example, http://arxiv.org/abs/0812.2521
Heck, we dont really know weather AdS/CFT is really “right”,its a conjecture.

So both approaches are based on assumptions and extrapolations.
If anything, the most interesting aspect is how close is the AdS/CFT result come to the “back of the envelope” result of 1985.

And Peter, keep ’em coming. Its true that the eta/s result is not to be made fun of, but its also true that a press release describing Fermi Gas measurements as “string theory in a lab” deserves as much making fun of as it can get.
I kind of disagree that this is exclusive to string theory though
(This whopper http://www.sciencedaily.com/releases/2010/03/100329214740.htm
is as hilarious as anything in your blog).
Unfortunately, in this day and age, scientists are under pressure of spinning anything they do into something fashionable, and these press releases are a byproduct of this

19. **New**
December 13, 2010

Woit: “If experiment disagrees with the gauge/gravity duality “prediction”, I doubt anyone is going to give up on gauge/gravity duality, instead they’ll give up on the particular strongly-coupled model used, or decide that the application of gauge/gravity duality in this situation is too naive.”

This is exactly the same as *not* throwing out quantum field theory because not all QFTs are experimentally relevant.

General predictions of QFT (like scalings of scattering amplitudes near fixed points etc.) still have value. Thomas is absolutely right that string theory as we underatand it now is a framwork and not a model.

20. **Peter Woit**
December 13, 2010

New,

Encouraged by thomas, you’re mixing up two completely different things:

1. string theory as a “framework” for producing TOEs that include the SM + GR in the low energy limit. The question of the value of such a “framework” is whether it predicts anything useful at all, allowing it to be tested, but that’s a different issue than the one addressed in this posting.

2. string theory as a “framework” for producing dualities between strongly interacting qfts and gravity theories in another dimension. Assuming this works as advertised, it can give a very useful approximate calculational method. That’s what this press release is about.

21. **Chris Austin**
December 13, 2010

Hi Derek, thanks for the reference.

22. **Mean and Anomalous**
December 13, 2010

I certainly do not see what the problem is with exposing over-hyped ‘scientific’ claims for what they are. Keep up the good work Peter.

23. **New**
December 14, 2010

Sorry Peter, I mixed up nothing. The press release said nothing about whether the potential predictivity of string theory was about Unification or not. You dragged that in, as your handy strawman.

24. **seth thatcher**
December 14, 2010

Peter, thanks for continuing to point out the hype associated with ‘tests’ of string theory. I’m as anxious as anyone to see progress in this area and have no animus toward string theory. Rather, as a layman, I prefer to read both sides of an argument so I can make an informed opinion of the science. As far as I can tell, you are the only one providing a reasoned alternative to string theory. It is a service to me. Of course, I also have no problem with those wanting to attack Peter’s viewpoint as long as it is based on logical objections of his analysis of the science. But most of these objections are not logic based.

25. **AL**
December 14, 2010

Very interesting……I have to agree with you in this case, since I published some work on the non-relativistic gas in question and calculated the ratio of viscosity to entropy-density and obtained about 6 times the conjectured “bound”, in agreement with experiments.
Though my calculation uses “standard” methods, I haven’t
checked, but I am pretty sure it wasn’t cited.

26. **AL**
December 14, 2010

Actually I just checked my paper again and obtained something between
4.5 and 5 times the bound, which is even better.

27. **New**
December 15, 2010

There are some calculations I have seen where people use the QCD Lagangian directly and try to do manipulations with it (or do naive manipulations with it inside the path integral) to get the eta/s ratio for quark-gluon plasma.

I have heard statements defending these approaches, like “at least these people are using the correct (i.e., QCD) Lagrangian, whereas AdS/CFT is using a conformal, supersymmetric theory”. This misses the point completely.

None of these computations are meangful. This is because asymptotic freedom and confinement make QCD an intrinsically quantum theory. The Lagrangian is an utterly meaningless thing to work with (outisde of lattice-based approaches) when the coupling is strong. Said another way, there are no classical, propagating Yang-Mills waves, unlike electromagnetic waves, so classical physics of this kind is useless. AdS/CFT is useful because it gives a direct handle on quantum QCD via classical gravity. At finite temperature, both supersymmetry and conformal invariance are broken and the theory is deconfined. So it is a far better candidate for comparison against the deconfined phase of QCD than any ill-conceived approach using the QCD Lagrangian.

Similar statements hold for any approach that claims to make predictions using “classical” approaches for strongly coupled quantum theories.

New/Somebody

28. **AL**
December 15, 2010

NEW, I agree, for gauge theories like QCD the Ads/CFT correspondence
has been very useful in offering new insights and even quantitative results. Although it remains a conjecture, there is a lot of evidence
that gauge theories are related to gravity in one higher dimension. But this post is about non-relativistic quantum gases, i.e. cold atoms, and it is fair to say that the attempts to use Ads/CFt ideas here are just not as successful.

29. **chris**
December 17, 2010

Dear Apple-Anny,

why don’t you sell oranges?

isn’t that kind of ridiculous?

so why on earth do people feel the need to pressurize an anti-string blogger into blogging about advances in QFT?

seriously people, if you are so bored, go somewhere else, read something different.

Oh and New,

“The Lagrangian is an utterly meaningless thing to work with (outisde of lattice-based approaches) when the coupling is strong.”

wow. why not tell this to Mr. Leutwyler? or to Mr. Goldstone? sure the QCD Lagrangean is entirely worthless at low energies. what a statement.

30. **AL**
December 19, 2010

There is excessive hype in the press release, but one has to distinguish between the media writers and the physicists. I looked at this paper, and it is a solid paper reporting interesting and difficult measurements in cold atoms. The paper itself does not try to promote string theory. I don’t see anything wrong with comparing with a conjecture on the lower bound of this quantity……it’s just interesting. Ads/CFT has certainly provided unique insights into Yang-Mills. Though the connection with relativistic quantum critical points of condensed matter is more speculative, it has nevertheless provided interesting perspectives on transport phenomena. This post was about cold atoms, which are non-relativistic and at LOW energy…..the success here has been nothing to write home about,
but the papers on the subject are honest and worthwhile attempts. Where things seem to go wrong is when the non-scientific media tries to make a splash.

Physicists Finally Find a Way to Test Superstring Theory

December 15, 2010
Categories: Experimental HEP News

More than ten years ago, the New York Times ran a story explaining that Physicists Finally Find a Way to Test Superstring Theory. At the time, the test was scheduled to start in 2005-6:

> In fact, it might be possible to concentrate so many heavy gravitons into a tiny volume of space that they would collapse in on themselves and create miniature black holes, those cosmic sinkholes from which nothing can escape. Experiments like this will be on the agenda when the Large Hadron Collider begins operation in five or six years at the CERN accelerator center in Geneva. "These black holes should be quite safe," Dr. Giddings said, for they would rapidly evaporate.

Today CMS released the results of the long awaited test of superstring theory, based on 35 inverse picobarns of data. It failed.

Update: Since this is getting wider than usual attention via Slashdot, I suppose I should remove tongue from cheek and make clear what is going on here. Claims such as the one in the 2000 Times headline always were nonsense: string theory unification failed long ago because it can't predict anything. Various physicists back then came up with "string theory inspired" models of extra dimensions that would in principle have observable effects at LHC energies. There never was any reason at all to believe these models (and they were no more "predictions of string theory" than anything else), but there was a lot of hype about them, often promoted to the media by people who should have known better. Now that the LHC is finally working, the result is exactly what everyone expected: these exotic phenomena that had no good reason to happen don't actually happen. It's great evidence that the LHC is working as expected, but not an experimental refutation of string theory.

Comments

1. **ted**
   December 15, 2010

   This is not failure. It's success in obtaining one more constraint. It's big business. A professor once told me that his whole academic career was to put increasingly stringent constraints on various models.

2. **Anonymous**
   December 15, 2010

   I suppose one could argue that the threshold of blackhole production based on superstring theory really starts at a higher energy? If this is so, wouldn't we also have seen blackhole production at the Fly's eye and other cosmic ray observatories (assuming they have the right detectors) where the energies can be much higher?

3. **Peter Woit**
   December 15, 2010

   Anonymous,

   The cosmic ray experiments observe only very small numbers of collisions at these high center of mass energies, and have very limited abilities to observe the products of the collision. I don't think they give any useful bounds on black hole production. The LHC results are based on trillions of collisions, and very sophisticated detectors.

   In case it's not clear, the 2000 Times headline always was complete nonsense. Even if you do believe in unification based on strings in extra dimensions, there's never been any good reason to think that the extra dimensions and associated black holes would show up at LHC-scale energies. This was a theoretical possibility that some people found useful as a selling point for string theory, but even they can't be surprised it's not working out.

4. **Tim van Beek**
   December 16, 2010

   Peter said:

   > This was a theoretical possibility that some people found useful as a selling point for string theory, but even they can't be surprised it's not working out.

   Ok, but nevertheless a lot of experimentalist teamed up and invested time and other resources to check it out, so there weren't sufficiently interesting alternatives, or were there?

5. **Peter Woit**
   December 16, 2010

   Tim,

   They're working on looking for a wide range of different things that would give evidence for something beyond the standard model. This is just one of the easier things to see, so the result is coming out first. Over the next year there should be a lot of similar null results in searches for exotic things that aren't expected to be there. But by late next year, there may be enough data to see the Higgs, and that's a very different story.

6. **Average Guy**
   December 16, 2010

   This is science at its best: formulate theories, make predictions, conduct experiments, … and deal with the more than occasional refutation. Some theories need "adjustments" (Newtonian physics) others are plain wrong (caloric, ether) but theories that allow to make predictions are always useful in advancing our knowledge, if only because they can be refuted by experiments and lead to better theories.

7. **Peter Woit**
   December 16, 2010

   Average Guy,

   The problem is that there never actually was a prediction made here. String theory unification failed a long time ago, not because of a bad prediction, but because it can't predict anything.

8. **basic philosophy of science**
December 16, 2010

This result puts constraints on possible hypotheses.

9. **Anonymous2**
December 16, 2010

As someone who actually did research on potential kaluza-klein gravitons produced by the LHC (another testable prediction of large extra dimensions), I thought I should clarify somethings that are glaringly incorrect in this summary:

First, there is a very good reason why these large extra dimensions might exist. They reduce the hierarchy problem to one of geometry. Also, this result doesn't even refute their existence, just reduces the parameter space.

In addition, you shouldn't lump all string theories into one, as if "string theory" would be refuted by the non-existence of large extra dimensions. I also disagree with your assertion that string theory failed because it couldn't predict anything. It in fact predicts quite a bit (for example, the existence of supersymmetric partners). While many predictions of, say, M-theory, can't be tested at energies below the GUT scale, that doesn't mean that none can. For example, many believe the LSP might be within reach of the LHC's energies.

The problem that many have with string theories – namely, that most of its new predictive power lies in the GUT scale – is a problem that faces many extensions of the Standard Model, because, well, that's unfortunately where the bulk of the new physics lies. Just because our experimental apparatuses on Earth are lacking doesn't mean it isn't science. If Einstein had lived a hundred years earlier and thought up relativity then (when no one had dreamed we would be able to measure the speed of light), it doesn't mean what he did wasn't science – it still made predictions that were testable in theory.

10. **Peter Woit**
December 16, 2010

Anonymous2,

Again, the suggestion that this refutes string theory was tongue-in-cheek, parroting the absurd and indefensible claims made over the years that this would be some sort of "test of string theory". You've got to admit though, if CMS had found something of this kind, we'd be seeing a lot of stories about "string theory found at the LHC". This is the usual way all these bogus "tests of string theory" go. If you see something, it's evidence for string theory, if you don't, that says nothing about string theory.

Saying that string theory predicts "the existence of supersymmetric partners" neglects to mention that it is completely silent on what breaks supersymmetry, not even able to say whether it is broken at the Planck scale or at some much lower scale. Saying "my theory predicts a symmetry", but then going on to admit that the symmetry is broken and your theory says nothing about how it's broken is kind of absurd. It's simply not true that string theory is predictive at the GUT scale, with the problem only how to extrapolate to low energies. We know how to extrapolate down from the GUT scale, the problem is that you can get anything you want at the GUT scale.

The question of whether one finds the large extra dimensions solution to the hierarchy problem compelling is to some extent a matter of taste. It's not something that ever appealed to me, and I like geometry, but tastes differ. I think it is a fact though that only a small minority of theorists ever claimed to find this idea appealing enough to lead them to expect evidence for it to show up at the LHC.

11. **Another Programmer**
December 16, 2010

I don't have a Google alert, but anytime I read anything about string theory in the general media, I always look up the reaction on your site and for comparison the site of that Czech guy (afraid to mention his name, don't want to restart the old wars). Please keep doing what you are doing here. In particular I really apreciate that you made an update that is more verbose and more accessible to the people who had strayed away from the real science to some nearby fields.

For this holiday season I was pondering some Marxist thoughts: How to make the physicists real owners of the tools of their trade? It seems like modern physicists are scientific sharecroppers who toil on the LHC-land owned by the absentee landowners and beholden under the opressive thumb of administrators always saying "publish or perish".

I'm trying to find an analogy in the human history where the distance (in tems of income) between owner/administrator and scientist/operator was as high as it is right now in the field of experimental physics. Make an IPO for the LHC and have the stock markets decide the further investments in research? How to make it work such that those expensive scientific tools like LHC are not under control of a secretive cabal that locked themselves in the ivory tower and doesn't want to talk to anyone outside?

12. **Peter Woit**
December 16, 2010

Another Programmer,

I encourage people to read both what I have to say and what Lubos has to say and make up their own minds on issues where we differ (we actually agree about physics more than many people realize). I see from one of his latest that I'm still a piece of "subhuman trash"…

I'm not convinced that lack of control over the means of production (the LHC) is the source of many problems. Just getting the thing to collide protons at high luminosity is a technological tour de force that I don't think could be done better or cheaper by anyone else. Similarly for the highly complex detectors. One can try and argue that someone out there is being kept from doing a really innovative analysis of the data, but all I know is that I have no ideas at all about how to do this better than those working there now.

13. **Eric**
December 16, 2010

Hi Peter,

I just want to say that your assertion that string theory has nothing to say about how supersymmetry is broken is wrong. In a fully worked-out model with all moduli-stabilized, it is in fact possible to calculate the supersymmetry-breaking soft terms. The problem is finding such a model and with knowing whether or not it is a unique solution.

14. **Peter Woit**
December 16, 2010

Eric,

Sure, at least for some classes of “string theory backgrounds” you can in principle stabilize moduli and calculate the soft supersymmetry breaking terms. But the problem is that it seems you can get pretty much whatever such terms you want by appropriate choice of “string theory background”. There’s also large classes of these backgrounds such that you can’t, even in principle calculate this kind of thing. It would be interesting if one could divide up the 105 or so dimensional space of these terms in the MSSM into values that could come from string theory and values that can’t, but I don’t believe that is possible.

15. **Ken**
December 16, 2010

Anonymous2,

“String theory doesn’t predict anything” is perhaps not the correct way to put it, however it is true that string theories lack significant predictive power. The problem is that a unified string theory requires so many arbitrary and independent variables that it effectively becomes a “fitting function” rather than a predictive model of reality.

As for predictions such as supersymmetry, string theories are hardly the only theories out there that require supersymmetry, and they face an occam’s razor problem on that count. Which of the “quite a bit” of predictions are *unique* to a string theory framework, and couldn’t be explained by something simpler?

Though IMO, if the problem of too many arbitrary variables isn’t solved, then there really isn’t any point talking about “predictions” of string theory in the first place.

16. **Eric**
December 16, 2010

Peter,

Yes, but the values of all parameters such as coupling constants and Yukawa couplings depend on the moduli VEVs. If such a model were found that could reproduce the correct values of these parameters with the moduli stablized, then I think the corresponding soft terms with be highly relevant.

17. **Giotis**
December 17, 2010

This article was referring to brane models with large extra dimensions. These models do not belong to String theory per se (although there are analogs in String compactifications where some of their benefits can be reproduced i.e. hierarchy). Every model with extra dimensions is not a priori related to String theory.

Of course the headline is obviously ridiculous.

18. **Chris Austin**
December 17, 2010

“If Einstein had lived a hundred years earlier and thought up relativity then (when no one had dreamed we would be able to measure the speed of light)”

In 1676 Olaf Romer and Giovanni Domenico Cassini found by observing the satellites of Jupiter that light took “about ten to eleven minutes” to cross the distance from the Sun to the Earth, and that distance was approximately known from simultaneous observations of Mars made in 1672 by Cassini in Paris and Jean Richer in French Guiana. The speed of light was estimated from this data by Christiaan Huygens, who died in 1695, 184 years before Einstein was born.

(Sorry for taking up an off-topic thread.)

19. **J. Patil**
December 17, 2010

The String theorist may have to pack up and look for another career. LHC has proven that they are on the wrong path. The early history of string theory also very interesting.
In 1968 a young Italian physicist was searching for an equation which would describe the strong force (the force that binds protons and neutrons). He found a book that contained an equation by a Swiss Mathematician named Leonhard Euler (1707-1783), which seemed to miraculously describe this force.
The discovery would eventually lead the physics community towards string theory, which has become a big part of theoretical physics. How did one guy’s mathematical gaming, translate into String theory and theory of everything?

20. **Markus Maute**
December 19, 2010

If you haven’t seen it yet, here is the latest of Roger Penrose on the subject:

Roger Penrose: Fashion, Faith, and Fantasy Part 7
http://www.youtube.com/watch?v=afsd3_PJais&feature=related

Have fun !

21. **Amos**
December 20, 2010

Peter – can you clarify the scope of the impact of this result?

I recognize that it can’t confirm or contradict string theory generally.

But does it at least exclude large extra dimension models? Or put a cap on the highest possible number of large extra dimensions? Or is it limited to large extra dimension models with particular features?

Those models may not be much of the landscape, or whatever, but they were certainly a pretty central topic of discussion for the past few years, so if that’s the exclusion it seems like it may be an even bigger deal.

22. **Peter Woit**
December 20, 2010

Amos,

For answers to your questions, you’ll have to consult someone who didn’t think these were contrived, implausible models not worth paying a lot of attention to. Can’t help you there.

23. **Amos**
December 20, 2010

    Can any of those people write coherent paragraphs in English? I tried reading Motls' blog post on the subject. It seemed to illustrate the proposition that the longest distance between two points isn't a straight line.

24. **SteveB**
December 21, 2010

    I happened to read a Letter to the Editor in New Scientist in the 08 December issue from noted string theorist Michael Duff and Nobel laureate Abdus Salam. They were commenting on a previous issue's article about the history of the relativity deniers in the early to mid 20th century. Then they took a crack at "us" — those of us who do not see String Theory as working out…

    Start of letter…

    'I enjoyed Milena Wazeck's analysis of the thought processes of those who denied Einstein's relativity (13 November, p 48). Yet it all sounded eerily familiar.

    Phrases such as "when people don't like what science tells them, they resort to conspiracy theories, mud-slinging and plausible pseudoscience" and "the increasingly mathematical approach of theoretical physics collided with the then widely held view that science is essentially simple mechanics, comprehensible to every educated layperson" call to mind the modern-day ramshackle alliance between unqualified scientists, the blogosphere and many science journalists when confronted with the academic consensus of superstrings and M-theory as the most promising candidates for unifying gravity with the other forces of nature. These people are quick to cry "this is not science", while themselves resorting to pseudoscientific alternatives.'

    …end of letter.

    Are we resorting to "pseudoscientific alternatives?". I do not think Peter is at all. I agree there is a strong message often found here that is similar to "this is not science."

25. **Peter Woit**
December 21, 2010

    Thanks SteveB,

    That's pretty amazing. But it's just from Duff, not from Salam, who died about 15 years ago. Duff is just invoking his title of "Abdus Salam professor".

String Theory Fails Another Test, the “Supertest”

December 17, 2010
Categories: Experimental HEP News

Wednesday’s CMS result finding no black holes in early LHC data has led to internet headlines such as String Theory Fails First Major Experimental Test (for what this really means, see here). At a talk today at CERN, yet another impressive new CMS result was announced, this one causing even more trouble for string theory (if you believe in purported LHC tests of string theory, that is…).

Back in 1997, Physics Today published an article by Gordon Kane with the title String Theory is Testable, Even Supertestable. It included as Figure 2 a detailed spectrum which was supposed to show the sort of thing that string theory predicts. Tevatron results have already caused trouble for many of these mass predictions. For example, gluinos are supposed to have a mass of 250 GeV, but the PDG lists a lower bound (under various assumptions) of 308 GeV. At CERN today, the CMS talk in the end-of-year LHC jamboree has a slide labeled “First SUSY Result at the LHC!”, showing dramatically larger exclusion ranges for possible squark and gluino masses. Over much of the relevant range, gluino masses are now excluded all the way up to 650 GeV. It looks like string theory has failed the “supertest”.

If you believe that string theory “predicts” low-energy supersymmetry, this is a serious failure. Completely independently of string theory, it’s a discouraging result for low-energy supersymmetry in general. The LHC has just dashed hopes that, at least for strongly-interacting particles, supersymmetry would show up just beyond the energy range accessible at the Tevatron.

Comments

1. **Glenn**
   December 17, 2010

   So, what’s the ‘motive’ towards “Supersymmetry” anyway? I see why people wanted to unify forces (electric force vs. weak force vs. strong force vs. gravity) but I’m not sure why having bosons vs. fermions is such a bad thing?

   And where are we in the strong plus electroweak unification theories anyway? The last I heard, combining them lead to predictions of proton decay that haven’t panned out.

   I always thought String Theory (TM) was supposed to resolve all this stuff.

2. **Peter Woit**
   December 17, 2010

   Glenn,

   Supersymmetry is a long and complicated story, I wrote a whole book to address questions like yours. Not much has changed since the book

   To all: please try and restrict comments to the topic of the post. While a general purpose place for people to ask questions and discuss particle physics would be a desirable thing, this isn’t it.

3. **Eric**
   December 17, 2010

   Hi Glenn,

   The main phenomenological motivation for supersymmetry is that it solves the hierarchy problem, which means that it prevents the Higgs mass from receiving large quantum corrections that would push the electroweak scale up to the Planck scale. Also, it improves the running of the gauge couplings to that they seem to unify at high energy. Finally, it includes a natural dark matter candidate.

   Peter: Looking at the supersymmetry particle spectra from some well-motivated string models, it appears that the viable parameter space prior to any results from LHC always has the gluino in the 1-2 TeV range. So, I don’t think that the current constraints represent an actual test that string theory has failed.

4. **Peter Woit**
   December 17, 2010

   Eric,

   The favored gluino mass in string theory models seems to have moved up from Kane’s 250 GeV back in 1997….

   I don’t doubt that string theory models can be found with whatever gluino mass one wants. Today’s news is presumably just the start of a long story during which superpartner mass limits will keep moving up, with favored string theory models keeping pace.

5. **Glenn**
   December 17, 2010

   Peter,

   Thanks, I own the book, read it, and even followed along pretty well for most of it, I think.

   I need to put it on the list to read again, along with “The Trouble With Physics.”

6. **Giotis**
   December 17, 2010

   Well 1997 was a long time ago…

   Currently and as far as I know String theory from a top down approach does not favour low energy SUSY in the sense that it is not expected these kind of vacua to be a majority in the landscape in comparison to vacua with other (high) breaking scales. I could even say that the opposite is true although this is debatable.

On the other hand low energy SUSY as it is well known is motivated mainly by theoretical considerations in order to solve problems like unification of couplings and the hierarchy. If it is excluded in LHC then one might naturally ask why we need SUSY after all if it doesn't solve the problems of the SM we want to solve. So SUSY as the most prominent candidate theory for physics beyond the SM will be strongly questioned. This will have at least a physiological impact to Supersymmetric theories in general like String theory.

7. **Peter Woit**
December 17, 2010

Giotis,

I see. If SUSY shows up at the LHC, string theorists will claim vindication. If it doesn't show up at the LHC, they'll just say it shows that the string theory landscape is right, that they knew very well SUSY wouldn't be at LHC scales since most of the vacua are at high supersymmetry-breaking scales.

8. **Mark**
December 17, 2010

Peter, if anything, this exclusion plot should only make Gordon Kane very happy because it excludes a region with light sfermions.

9. **Giotis**
December 17, 2010

Peter I understand but here is the point:

Let's say that we want to find a solution to the hierarchy problem and we have two ways for solving it, fine tuning (like the fine tuning of CC in the Bousso Polchinski model) and Low energy SUSY. Now in String theory there are models which can produce both mechanisms. Fine tuning may seem quite unlikely at first but given the plethora of vacua that can realize it this may well be the prediction of String theory after all and not low energy SUSY. This would be the case if we could estimate that the vacua which we obtain from these models are much more frequent and thus statistically favoured over the low energy SUSY vacua.

This is an over simplified picture of course and many other factors must be considered but it is the general idea. The fact that String theory is a Supersymmetric theory doesn't imply that String theory favours SUSY at LHC. You must first check the models and the statistics of the corresponding vacua. Of course if SUSY is found at LHC this would be very important for String theory but only as a theoretical framework regardless of any 'predictions'.

10. **anon**
December 17, 2010

Since landscape statistics is generally pulled out the nether regions of the body, the only way to judge any statement of the form 'most vacua are X' or 'X is more likely than Y in the landscape' is to see WHOSE nether regions it is pulled out from. A professor at Stanford — well, maybe I'll give it 2 minutes of thought. A grad student — not so much.

Given this is the only available criterion, I find it hard to judge statements made on a blog about what's common on the landscape.. I mean, how respectable is your nether region?

11. **anon**
December 17, 2010

as a string theorist (on the more mathematical end) and as a human being (who knows the general structure of issues being debated here), it drives me up the wall that clearly intelligent and thoughtful people like 'giotis' go through all these ridiculous constructions in clear denial of what is staring them in the face.

the world is so full of interesting ideas. science even more so, physics most so. Take a short walk and take the time to understand what a good non-high-energy physicist or scientist is thinking about — you'll be amazed at (and reminded of) how interesting ideas are formed and work out.

No twisting and turning and playing dodgeball for 20 years required.. You think up an interesting idea, try it, throw it out if it doesn't work and go on to something else.

It'll really seem like a breath of fresh air, compared to the landscape cr*p people here at talking about.

12. **Mitchell Porter**
December 18, 2010

anon @ 9.24pm, do you consider string theory itself a dead end, or just landscape statistics?

13. **Verified Armonyous**
December 18, 2010

anon @ 9.24pm, we would be happy to test our theories in our tabletop colliders on Fridays, but even a string theorist surely knows it's a bit more complicated than that. That's unfortunate but it's the only way when you're asking the most fundamental questions.

Glenn, Supersymmetry is the coolest thing you could expect to find at the TeV energy frontier. It is revolutionary (you heard about fermionic dimensions?), beautiful (people will wear the SUSY algebra in T-shirts :)), perturbatively well behaved (you can extend it to hugely large energy scales), solves neatly some SM problems, gives naturally a dark matter candidate, points to grand unification, passes switfly precision tests, etc.

14. **anon 924**
December 18, 2010

landscape statistics and related attempts to connect strings to particle physics are bad mathematics and bad physics. i think there are so many interesting things to think about and do in string theory and hep-th in general. I know many around me who do this.

On the other hand, it's seems stupid that some people need to twist themselves into pretzels and wave their hands and waste their youth writing the most non-rigorous worthless papers, trying to justify this as physics testable at the LHC, just so funding levels remain higher than it would if this were classified as mathematics or mathematical physics.

To all you younger landscape / string pheno people out there — do you really think your work will 1) be proven true? 2) be proven false? 3) neither but contributes a new physical IDEA? 4) none of the above?
My opinion is that (4) holds for all of the landscape. It's like biting into an air-filled pastry.

Oh, please stop with the “can’t test grand theories so easily”. My complaint is that pheno end of string theory is quackery with no rigor, not misunderstood genius that has come way before its time.

15. **Mark**
December 18, 2010

Dear anon 924,
String pheno is a huge field that has been around for a few decades before this landscape statistics bs came about. There are many far more important, in my opinion, and interesting problems in string pheno that people are working on. If you check out the talks at this year’s KITP program on string pheno you’ll see that landscape statistics is a rather marginal topic. On the other hand, understanding the mechanism of SUSY breaking is a very important problem and will remain one of the dominant topics in het in the years to come, especially if superpartners are discovered at the LHC. After years of research it is pretty clear that moduli stabilization and SUSY breaking are extremely closely related and one can already make some very specific statements about sparticle spectra based on very few known realistic scenarios.

16. **MathPhys**
December 19, 2010

“Fine tuning may seem quite unlikely at first but given the plethora of vacua that can realize it this may well be the prediction of String theory after all and not low energy SUSY. This would be the case if we could estimate that the vacua which we obtain from these models are much more frequent and thus statistically favoured over the low energy SUSY vacua.”

The above statement goes against the grain of every intuition that we have gained since Pierre-Simon Laplace wrote his book on probability theory.

17. **anon 924**
December 20, 2010

mark, i admit that string pheno is larger than landscape ramblings – but string pheno is often either rigorous and irrelevant to the LHC or very ad hoc and isn’t really ‘stringy’ anyway.

For example, to follow your example, i think there is VERY VERY little string theory has to add to the story of SUSY breaking.. besides in gravity mediation perhaps. i don’t think any serious honest person is working on stringy models of SUSY breaking.. even people with stringy knowledge and skills like Seiberg.

You can claim susy breaking is related to modulii stabilization in string theory but that’s like saying issue X is related to issue X + Y. True but I can ignore Y and work on X alone just fine.. modulii stabilization might have consequences for susy breaking but the latter can exist without the former.

18. **chris**
December 20, 2010

Mark,

i respect your opinion, but i can’t help noticing the irony in a statement like

“understanding the mechanism of SUSY breaking is a very important problem and will remain one of the dominant topics in het in the years to come, especially if superpartners are discovered at the LHC”

posted in a thread on the first huge exclusion sweep of the LHC.

i am asking this in honesty and not tongue in cheek (as i really would like to know): is your scientific judgement and gut feeling still pointing towards low energy SUSY?

in the 70s and 80s SUSY certainly had its appeal. we knew much less then about the TeV scale and even electroweak symmetry breaking. but looking at what has happened experimentally in between makes SUSY look like a dinosaur to me. W and Zs were found and later the top, the Higgs limit was pushed to 115GeV, the cc was found and anisotropies in the CMB. our view of the world has considerably changed and expanded since the invention of susy but still no trace of remnants of this elusive symmetry. are you not bothered by this or, in other words, was it to be expected that susy is so hard to find?

19. **Mark**
December 20, 2010

Dear anon 924,
The assumption that “modulii stabilization might have consequences for susy breaking but the latter can exist without the former.” is certainly not ruled out but appears to be very unlikely for very good reasons. Let us be conservative and assume that a given compactification is described by N=1 D=4 sugra. Then, decoupling moduli stabilization from SUSY breaking would require 1) Stabilizing all moduli by the superpotential only without relying on the Kahler potential. 2) Having an additional dial in the superpotential, essentially a constant, and using it to fine tune the total superpotential very close to zero so that the the resulting vacuum is very nearly SUSY Minkowski. 3) Breaking SUSY on top of that and decoupling the s-goldstino (aka a modulus) from the gravitino while maintaining a tiny CC (this is needed in gauge mediation to avoid a very light modulus) requires a rather contrived choice of a Kahler potential for the SUSY breaking field. While 1) is certainly possible, 2) is extremely non-generic because of the amount of fine tuning required to achieve SUSY Minkowski vacua. Furthermore, 1) + 2) would certainly kill any attempt to solve the strong CP problem via a QCD axion since all the axions would be rendered too heavy, as Joe Conlon has rigorously proved in the context of N=1 D=4. 4) Simply embedding a viable gauge mediation scenario into string theory is a rather daunting task in itself.
Therefore, unless you are so heavily invested in gauge mediation that you are willing to give up on naturalness, it is pretty clear from the top down perspective that gravity mediation is much more natural and that the problem of moduli stabilization is intimately related to the question of SUSY breaking.

20. **Mark**
December 20, 2010

“i am asking this in honesty and not tongue in cheek (as i really would like to know): is your scientific judgement and gut feeling still pointing towards low energy SUSY?”
Yes, there are at least four very compelling reasons to consider TeV scale superpartners. Stabilizing the gauge hierarchy, precision gauge coupling unification, radiative electroweak symmetry breaking, LSP as a dark matter candidate. The exclusion plot discussed in the posting considers a small portion in the parameter space of a very simplified, almost adhoc, set of GUT scale boundary conditions called msugra. Let us wait until the LHC collects at least 10/fb before we jump to premature conclusions.

“was it to be expected that susy is so hard to find?” In retrospect, if the SSC had not been cancelled, SUSY could have already been found a decade ago.

21. **piscator**
December 20, 2010

anon924,

If you have thought about the subject seriously, I find it hard to see how you can say string theory has little to add in susy breaking except perhaps in gravity mediation – so little except in the (arguably) best motivated type of susy breaking, where all the operators are coming from the Planck scale.

In any case every serious person knows (and has known for years) that in any context with a UV string embedding an honest discussion of susy breaking requires addressing moduli stabilisation. You're right it's possible to work on susy breaking in global field theory without thinking about moduli stabilisation. It's even possible to have a successful career doing so. There are lots of interesting results thereby obtained for susy breaking in worlds that don't have gravity. None of this means this has any real physics value to susy breaking in this world. It's like Feynman's example of the flow of dry water, it may be interesting qft but its missing a crucial part.

22. **MathPhys**
December 21, 2010

Dear Mark,

"In retrospect, if the SSC had not been cancelled, SUSY could have already been found a decade ago."

It's statements like this that give string theory and theorists a bad name.

23. **Eric**
December 21, 2010

Dear MathPhys,

Can you explain exactly how Mark's factual statement possibly gives string theory and theorists a bad name? If TeV-scale SUSY does exist, it would most certainly have been found at the SSC had it been constructed. Since it was cancelled, theorists have more or less had to wait for the LHC, although there was always an outside chance that it could have been seen at LEP or the Tevatron.

24. **Thomas Larsson**
December 22, 2010

In retrospect, if the SSC had not been cancelled, low-energy SUSY could have already been ruled out a decade ago.

25. **MathPhys**
December 22, 2010

Dear Eric,

To me, Mark's statement implied that

"There is supersymmetry, there is no doubt about it, but we were denied the chance to discover it".

It's this messianic approach to science that gives the subject a bad name. A better statement would have included Larsson's comment above

"In retrospect, if the SSC had not been cancelled, low energy SUSY could have either been found or ruled out a decade ago."

26. **Chris Oakley**
December 22, 2010

Not entirely. There is Occam's razor. If I posit that there is a real Father Christmas who charges around the sky on a sleigh pulled by reindeer, climbing down chimneys to leave presents – but only for children who have been good, then the onus is on me to prove it – not on you to DISprove it. Similarly, all experimental evidence so far leads one to the conclusion that there is no supersymmetry – its only value is as a band aid to patch up ailing theories that probably should have been binned instead. So unless someone can come up with something more compelling the default position should be that it does not exist.

27. **MathPhys**
December 22, 2010

On the one hand, supersymmetry is by and large the reason why quantum physics (starting with Witten's work on Morse theory in the early 80's to mirror symmetry) has been spectacularly successful in pure mathematics over the past 30 years.

On the other hand, after 35 years now, there is indeed not a shred of evidence of supersymmetry in the real world. It's a very peculiar situation.

28. **chris**
December 22, 2010

Hi Mark,

thanks for your honest answer. I see the standard arguments, but there is always this nagging feeling.

Let's take the unificaton of couplings. As far as I know, this is essentially still a 1-loop argument. But granted that it is true in the full, nonperturbative context, what does it tell us?
I am thinking in comparison of the Weinberg angle. It is really close to the predicted SU(5) GUT value. So close in fact that Georgi and Glashow in their original paper were about as sure as one can be that this is the true symmetry group of nature. Alas, it has been firmly ruled out by now.

So what does it tell me that 3 lines do intersect in one point instead of a larger region? unification looks not too bad if only very little is allowed to happen between the TeV and the GUT scale. I really find it very difficult to interpret anything furthter into it.

29. **Eric**
December 22, 2010

MathPhys,

I think you are misinterpreting Mark's statement. Saying that SUSY could have been discovered already if SSC had not been canceled is quite different than saying that it would have or should have, which is what you seem to think he said. I think it's fair to say that whatever new physics exists at the TeV-scale would have been discovered by now if the SSC had been built, SUSY or otherwise.

30. **MathPhys**
December 22, 2010

Dear Eric,

Yes, I agree with you. I took his statement more along the lines of "would have been discovered". I also wish he would have added "or otherwise" to make his statement more balanced.

31. **Mark**
December 22, 2010

Dear Chris,
"So what does it tell me that 3 lines do intersect in one point instead of a larger region?"
As you know, the unification idea, whether it is SU(5) or SO(10), is well motivated by the fact that the matter multiplets of the SM nicely fit into into complete GUT multiplets. This is a higly non-trivial clue and should be considered very seriously. However, for this idea to work: 1) the three gauge coupling must unify at some scale; 2) the unification scale must be high enough to suppress the proton decay. As we know now, if the SM is all there is between the EW scale and the GUT scale, this does not work. The proton decays too fast while the couplings do not actually unify.

Keep in mind that the MSSM was introduced to address the gauge hierarchy problem, first and foremost, and the other three properties I mentioned in my previous posting followed *automatically*.

In fact, to get the couplings to unify at 1-loop one does not need the full MSSM! Including light Higgsinos only can do the job to satisfy requirement 1). However, in this case the unification scale will be around 10^14 GeV – the same as the original non-susy GUT. This is deadly because of the proton decay. Including the MSSM gauginos changes the relative running speed and raises the scale at which the couplings unify, which is nice because the proton lifetime becomes much longer, hence satisfying requirement 2). On the other hand, at 1-loop, the MSSM matter multiplets play no essential role (they only change the slopes and thus the value of the unified coupling) as they come in complete GUT multiplets. So, if you only want to use SUSY for the purpose of preserving the gauge coupling unification you can completely decouple all the squarks and sleptons! This idea is called split SUSY and completely gives up on addressing the hierarchy problem but some people are taking this possibility seriously. A long-lived gluino is a robust prediction of such a model and it is actually being searched for at the LHC. It is perfectly possible to add a few heavy complete vector-like GUT multiplets to the largangian and not spoil the perturbative unification. Now, when you start to include 2-loop effects life becomes more interesting. In this case the running becomes slightly more sensitive and depending on the details of, say the gaugino spectrum at the unification scale, the couplings can unify even more precisely at 2-loops.

32. **chris**
December 23, 2010

Dear Mark,

thank you very much for your detailed response. As you explained so nicely it is indeed a very nice feature of the MSSM to address several problems of unification at once. although I do not see the gauge hierarchy problem as solved by the MSSM but rather shifted to another sector, it is indeed intriguing circumstantial evidence that this solution also results in a dark matter candidate and unification of couplings.

but my problem really is with the circumstantial nature of this evidence and i would like to once again pick the unification of couplings as the example. since i first saw this plot, i always wondered about one thing: unification seems to happen at almost the planck scale (logarithmically). it is nice that susy at 1-loop brings the 3 couplings together, but to be completely compelling i would have somehow expected this intersection of 3 lines to – so to say – intersect with a 4th line, the naively computed fundamental scale (plank scale). you see, intersecting 3 lines is not such a big deal. as you so nicely pointed out yourself, you need just one new fermion with the appropriate couplings. that's it. and the advantage of intersecting higher that 10^14 TeV is also a two-edged sword: yes, one can portray it as a success given the experimental knowledge we have, but in fact the success consists of nothing more than evading another experiment. from an unprejudiced, purely theoretical point of view unification at a lower scale would be preferred a priori because of its increased testability or , put differently, because it decreases the extent of the region one has to proclaim as being a dessert. that is, if this scale is not found to be the planck scale or given by another, independent experimental hint.

to me it seems that susy does get some things right but not quite all of them. and i am sceptical as to whether the features it does get right are not just some rather universal ones.

33. **Mark**
December 23, 2010

Dear Chris,
Once the three gauge couplings unify at ~10^16 GeV, they keep running as a single coupling, i.e. one line. This is because all the GUT scale extra states complete the formerly incomplete GUT multiplets. So the higgses and higgsinos instead of doublets become 5-plets and \bar 5-plets, the gauge bosons become 24-plets etc. So after the three couplings unify they no longer diverge and run as one. However, above the GUT scale the theory may no longer be effectively 4-dimensional so one needs to take into account the contributions from the KK modes, etc. This will change the shape of the running. The unification of the SU(5) gauge coupling and the gravitational coupling will eventually take place at a scale a few orders of magnitude above the GUT scale.

34. **Mark**
December 23, 2010

Dear Chris,

"from an unprejudiced, purely theoretical point of view unification at a lower scale would be preferred a priori because of its increased testability or , put differently, because it decreases the extent of the region one has to proclaim as being a dessert."

I would qualify this point of view as motivated by experimental testability accessible with the current technology rather than a purely theoretical. I'm fairly certain that testing the proton decay lifetime in the context of SUSY GUTs will eventually be possible as new expreiments will come online.

"although I do not see the gauge hierarchy problem as solved by the MSSM but rather shifted to another sector, it is indeed intriguing circumstantial evidence that this solution also results in a dark matter candidate and unification of couplings."

Indeed, SUSY does not "solve" the gauge hierarchy problem, rather it "stabilizes" the electroweak scale while it does not really explain its origin. In order to *really* explain the origin of the electroweak scale once needs to have a model of SUSY breaking where the only dimensionful input is the Planck scale. Back in the early 80s Ed Witten suggested an excellent idea where some strong gauge dynamics in the hidden sector generates a small scale of SUSY breaking in the visible sector via dimensional transmutation. This is referred to as dynamical SUSY breaking. I personally think that this is the best idea we have and I don't agree with the landscapeologists that the EW scale is there because some fluxes are extremely fine-tuned.

By the way, as I said in my previous posting, in addition to the precision unification and the LSP as a dark matter candidate, the MSSM also naturally explains why the electroweak symmetry is broken. Recall that in the SM the higgs potential has a negative mass squared term in order to generate a minimum where the higgs vev is non-zero. In the SM this tachyonic potential is put in by hand and has no dynamical explanation. In the MSSM, on the other hand, the negative mass squared term has a dynamical explanation. It is generated via radiative corrections, mainly due to the large value of the top Yukawa coupling. So, in the context of the MSSM, one can claim that at least one of the SM quarks must be very heavy in order for the EW symmetry to be broken. Again, this rather non trivial result was not the reason why the MSSM was introduced but comes automatically as a bonus.

HEP News

December 17, 2010
Categories: Experimental HEP News

Besides the dramatic new CMS results mentioned in the last two postings, there's other news from the high-energy frontier as it moves from Illinois to Geneva.

Earlier this week the MCTP hosted a workshop on LHC First Data. Today at CERN was the LHC end-of-year jamboree, talks available here.

Plans for next year's LHC run were made at Evian last week and will be finalized at Chamonix next month. Beam re-commissioning will start February 21, and it looks like the goal will be to run the machine at 4 TeV/beam (up from 3.5 this year) and accumulate a total luminosity of 1-3 inverse femtobarns. Instead of shutting down during 2012 to fix magnet interconnections, the plan now is for the LHC to continue running through 2012, accumulating enough data to definitively see or rule out a Standard Model Higgs and finally put the Tevatron out of business.

Today at Fermilab people are looking backwards, with a symposium celebrating the 25th anniversary of first collisions at the Tevatron. While a proposal has been put forth to keep the machine running through FY 2014, the budgetary situation looks increasingly likely to put them out of business, no matter what CERN does. The dysfunctional nature of the US federal budget process means that the laboratory is already several months into FY 2011, with no budget, operating under a "continuing resolution" that allows them to spend money at the same rate as last year. Last night, an effort to pass an "omnibus" spending bill for the rest of FY 2011 allocating total spending at the same level of FY2010 was defeated. This means that until February and the next Congress, Fermilab and the rest of the government will operate without a budget. At some point after that, the Republicans plan to try and pass a budget cutting spending from the FY2010 level. Fermilab could very well find itself this Spring finally finding out that its FY2011 budget has been cut, with only a few months left to get spending down to the appropriated level. Budgetary problems are not just affecting the Tevatron, with plans for an underground laboratory in South Dakota dedicated to neutrino and other experiments now up in the air as the NSF has withdrawn its support for the project.

President Obama did make an inspiring speech about his dedication to support Research and Development spending.

Comments

1. **HET student**
   December 17, 2010

   I think you meant 4 TeV beams, not Gev. This would be a disappointing performance by the LHC.

2. **Peter Woit**
   December 17, 2010

   Thanks HET student. Fixed.

3. **Nickle Berry**
   December 18, 2010

   I suppose Obama pans to pay for scientific research with tax cuts for the rich.

4. **Chris Austin**
   December 18, 2010

   Is anything more known about the little dip at M_Inv = 1.6 TeV and the following bump at M_Inv = 1.9 TeV in the 3rd plot, labelled ATLAS-CONF-2010-088, in the top row of the graphs on page 28 of the Atlas talk? The dip seems to be 3 sigma, and since the data follow a smooth curve, with a dip and a bump that is not in the Monte Carlo, one could argue that the significance is more than that. But M_Inv of what? The vertical axis shows Entries/0.1 TeV, but Entries of what? The integrated luminosity for the plot seems to be just 295 per nb, so has that signal now vanished, or is it still there?

5. **Anonymous**
   December 18, 2010

   "…the plan now is for the LHC to continue running through 2012, accumulating enough data to definitively see or rule out a Standard Model Higgs…"

   So what happens if the Higgs is ruled out by the end of 2012? What do we look for next?

6. **Chris Austin**
   December 18, 2010

   One possibility would be a fourth Standard Model generation, which would also allow the Higgs to be heavier.

   With reference to page 15 of the Atlas talk, it is not clear to me how the lack of structure in what appears to be the 2-jet invariant mass can exclude the production of excited quarks, by which I assume they mean quarks of a 4th Standard Model generation, with masses in the range 0.3 to 1.5 TeV.

   For from the graph of cross sections that appears on pages 6, 7, 9, 13, etc., it appears that sigma_b = sigma_{4.2 GeV} is about 3 x 10^5 nb at 7 TeV, and sigma_t = sigma_{172 GeV} is about 0.2 nb, so the heavy quark production rate scales with mass as about m^{-3.83}. So we expect sigma_{600 GeV} to be around 0.002 nb, which for an integrated luminosity of 3.1 per pb corresponds to about 6 events, against a background of 10^3 events. And we expect sigma_{900 GeV} to be around 0.4 pb, which corresponds to about 1 event, against a background of 100 events. And we expect sigma_{1500 GeV} to be around 0.05 pb, which corresponds to about 0.2 events, against a background of 6 events.

   So the graph on page 15 seems to me only to limit the couplings of 4th generation quarks to be less than values somewhat larger than the Standard Model couplings.

7. **neo**
   December 18, 2010

   Does it make any sense to continue to run the Fermilab TEV now that the LHC is operational? Can it expect to compete with the reams of data being turned out by the LHC detectors?

8. **Peter Woit**

December 19, 2010

neo,

Whether to keep the Tevatron operational is currently being debated. For the moment it is far ahead of the LHC in the Higgs search business, and the argument has been made that it could remain competitive for a few more years as it accumulates data faster (although at lower energy) than the LHC. But that was based on the assumption that the LHC would shutdown for at more than a year after reaching 1 inverse femtobarn in luminosity sometime next year. If the LHC acquires 3 inverse femtobarns in 2011 and keeps going through 2012, then it will definitively outclass the Tevatron in the Higgs search.

The way the US budget situation is going, this may be a moot point anyway. It's still unclear whether there will be funds to get through this fiscal year, much less funds to run the Tevatron in FY 2012.

9. **Chris Austin**
December 20, 2010

With reference to my first comment above, a Google search for ATLAS-CONF-2010-088 turned up the full report, "Search for new physics in multi-body final states at high invariant masses with ATLAS", dated August 21, 2010. They selected events with at least three objects such as jets or leptons in the final state and an invariant mass above 800 GeV and total p_T greater than 700 GeV. The initial large peak is due to the cut on m_Inv at 800 GeV and the fact that the Standard Model background is rapidly decreasing. The graph appears on pages 8 and 9. The dip at 1.6 TeV and the peak at 1.9 TeV are definitely not in the Standard Model background. The data follow a very smooth curve, i.e. with almost no scatter, that departs substantially from the Monte Carlo, so although the dip is only 3 sigma below and the bump is only 1 sigma above the Monte Carlo, one could argue that the significance of the whole smooth curve is more than 3 sigma.

The analysis used just under 300 per nb of data, but ATLAS now has 45 per pb, i.e. 150 times more, so if the trend shown in the graph has continued, it will be a discovery by now. There is no update available yet in the ATLAS Conference Notes, nor on arXiv. It would be very interesting to hear from someone in ATLAS about the current status of this signal.

The search was designed to look for evidence of TeV-scale gravity, in the shape of high mass states decaying democratically to Standard Model states. The discussion on pages 10 and 11 refers only to semi-classical black holes, for which no evidence was seen. The bump could be a Kaluza-Klein graviton in the Randall-Sundrum-1 model, or it could be a KK mode of the graviton multiplet in the geometry of subsection 2.5 of my article arXiv:0704.1476, which is totally different from the Randall-Sundrum geometry, but also has the lightest massive gravitational KK modes around a TeV: their wavefunctions are pushed up against our boundary of the Horava-Witten interval by the warp factor.

The only way I know to force the subsection 2.5 geometry to occur uses very large electric fluxes of the 4-form, whose dual 7-form fluxes wrap the de Sitter 3-sphere times a 3-cycle of the compact hyperbolic 6-manifold times the Horava-Witten interval. Choosing the quantized flux numbers to be about 10^{126} forces the cosmological constant to be about right, i.e. the de Sitter radius is forced to be about 10^{26} metres, and Newton's constant and the electroweak breaking mass are also forced to be about right.

The flux quantization condition is invariant under the full de Sitter group, but the fluxes are not. However it seems possible that the only significant observable Lorentz violations will be in the dynamics of the massive gravitational KK modes, so it would be worth checking if the size or position of the dip and the bump vary in synchrony with the Earth's sidereal rotation, i.e. the Earth's absolute rotation with respect to the stars. (A sidereal day is approximately 23 hours and 56 minutes.)

10. **Shantanu**
December 20, 2010

Peter, maybe slightly OT,
but see the slides of NNN 2010 conference at Toyama
http://www-sk.icrr.u-tokyo.ac.jp/NNN10/
See in particular the talks by Nomura and concluding talk
by Jung. (again emphasis of this conference is proton decay
and underground detectors)

Short Items

December 21, 2010
Categories: Uncategorized

- The Tevatron last week passed the milestone of 10 inverse femtobarns of luminosity delivered to the experiments. That's about 1.5 quadrillion collisions.
- Presentations from the Simons Center Inaugural Conference, discussed here, are now on-line.
- Luis Alvarez-Gaume and John Ellis discuss here the Higgs mechanism, its history and the question of who should get a Nobel prize if the Higgs particle is found There's the usual attempt to cut Anderson out of the picture (for more see here), I gather this is payback for his opposition to the SSC.
- The Cambridge City Council has passed a resolution congratulating Yau and Nadis on the publication of their book about Calabi-Yaus, *The Shape of Inner Space*.
- Barry Mazur and William Stein are working on a book entitled *What is Riemann's Hypothesis?*, with a rough draft available here.
- If you want to seriously learn algebraic geometry, maybe the best way would be to take Ravi Vakil's Math 216 course on-line here. OK, I should have told you about this at the beginning of the semester, because if you start now you'll be way behind. But, since it's on-line, maybe that doesn't matter. You could try and catch up…
- There have been various recent claims to see evidence of pre-big bang physics in the CMB (see here and here), although the significance level of these results seems to be about that of the discovery of Stephen Hawking's initials in the same data. Several preprints have already appeared criticizing the first of these claims, Sabine Hossenfelder deals with the second here. John Horgan blogs about this as "science faction" here, and discusses it with George Johnson here.
- Mike Duff seems to now be deep in Lubosian territory, publishing a letter to New Scientist that accuses those who don't accept the supposed "academic consensus of superstrings and M-theory" as being just like the crackpots and anti-Semites who refused to accept Einstein's relativity back in the 20s. According to Duff, the explanation for criticism of string/M-theory is that:

> when people don't like what science tells them, they resort to conspiracy theories, mud-slinging and plausible pseudoscience.

Update: The America COMPETES Reauthorization has just passed the House and will go to the president to be signed, something no one expected to happen a week or so ago, more details about the legislation here. I gather that it authorizes 5 to 7% increases for science agencies. Problem is that these are not the actual appropriations, which are still up in the air, awaiting action next year by the next Congress. But this does indicate that there is bipartisan willingness to at least pay lip service to protecting the research and development part of the budget.

Comments

1. **Garrett**
December 21, 2010

In the Simons talks, Atiyah's slide 17 caught my eye. I wonder what he was saying to accompany that.

2. **Peter Woit**
December 21, 2010

Garrett,

I don't remember Atiyah giving any more details about that in the talk than in the slides. He was explicitly operating in an extremely speculative mode…

3. **Roger**
December 21, 2010

Do you have any idea what Duff is referring to where he said:

> These people are quick to cry "this is not science", while themselves resorting to pseudoscientific alternatives.

The closest I found was this 2002 letter, where he seems to complain that an article mentioned quantum gravity and human consciousness, without mentioning M-theory.

4. **Peter Woit**
December 21, 2010

Roger,

The 2002 letter seems to claim that M-theory explains everything, except consciousness, and that Duff likes that situation.

In the new letter, the culprits are "unqualified scientists, the blogosphere and many science journalists", and I'm pretty sure I qualify. He doesn't specify what pseudoscientific alternative I'm resorting to, maybe he is strongly opposed to the use of representation theory, or any tinkering with the BRST formalism. One other possible guess is that in his mind loop quantum gravity is a "pseudoscientific alternative" and Lee Smolin is an "unqualified scientist", but who knows. To me Duff sounds a lot like Lubos does these days, so maybe you can consult Lubos's extensive writings for more specifics.

5. **Roger**
December 21, 2010

If Duff is referring to you, then he is worse than Lubos. Lubos posts crazy rants every day. He has probably said worse things about you, but it is clear that he just doesn't like your negative comments about string theory. Duff's letter is published in a respectable journal. Some editor had to approve it as making a reasonable point. If Duff is going to say, in print, that you are unqualified and pseudoscientific, then he ought to back it up. Duff seems more irresponsible to me.

6. **Tim van Beek**
December 22, 2010

Since the letter of Michael Duff is not the daily abreaction of a lonely and troubled soul, there must be a concrete reason that prompted it, several years after most people lost interest in the "string wars". Does anyone know what it is?

(And does anyone know what is supposed to be the unpopular truth that string theory tells?).

7. **Peter Woit**
December 22, 2010

Tim,

It's hard to guess what might cause someone in Duff's position to start writing public letters comparing their situation to that of Einstein and accusing those who don't believe in their speculative theory of incompetence. I've never seen anything quite this odd.

8. **neo**
December 22, 2010

Duff appears to be attempting to continue an unfortunate but effective tactic of lumping all skeptics of a theory with some crackpots and classifying them as "deniers". He has a tough go in this case though because there isn't a shred of string evidence to deny.

9. **Marty**
December 22, 2010

The Nature paper by Luis Alvarez-Gaume and John Ellis is certainly payback…but not for Anderson and SSC. It is payback for this below spat from the summer – both in France and Nature Magazine (on-line).

http://www.nature.com/news/2010/100804/full/news.2010.390.html

The authors do a good job of looking fair but not really discussioning the merits of each of the papers. One of the last posts from 11/30 seems to get it.

http://www.math.columbia.edu/~woit/wordpress/?p=3282&cpage=1#comment-71745

10. **Coin**
December 22, 2010

"Duff's letter is published in a respectable journal"

Do you actually think of the New Scientist as respectable, or for that matter a "journal"?

11. **Roger**
December 22, 2010

Yes, New Scientist is a respectable journal, and I would be annoyed if it called me an unqualified pseudoscientist. Duff obviously thought that it was respectable enough to publish his letter. If the letter had been more specific, then I would suggest that Peter write a reply. As it is, I think that it just reinforces the reputation of string theorists as being arrogant, elitist, and detached from reality.

12. **Nickle Berry**
December 23, 2010

New Scientist is not respectable in the sense that it is not viewed by experts as a trustworthy source of information about technical subjects.

13. **Mean and Anomalous**
December 23, 2010

I agree with N. Berry about New Scientist. In general, it can be said that it isn't a trustworthy source of information about technical matters.

14. **Vladimir Kalitvianski**
December 23, 2010

> there must be a concrete reason that prompted it, several years after most people lost interest in the "string wars". Does anyone know what it is?

Yes, I think it is namely the loss of interest in strings that makes him mad.

15. **John Rennie**
December 24, 2010

Speaking as a physicist not a mathematician, I found The Shape of Inner Space a fascinating book and I strongly recommend it.

For anyone interested in the Riemann Hypothesis I recommend Prime Obsession: Bernhard Riemann and the Greatest Unsolved Problem in Mathematics by John Derbyshire. The maths shouldn't tax anyone with a physics degree, and it does an excellent job of describing why the roots of the Zeta function matter.

16. **John Baez**
January 2, 2011

New Scientist is not what I'd call a "journal", because scientists don't publish research papers there. It's a news magazine that focuses on science, like Scientific American.

I don't consider New Scientist particularly "respectable" on the topic of physics. For example, they published an uncritical article on Shawyer's "EM drive", a proposed propulsion system that violates conservation of momentum. But their articles aren't all bad: they vary wildly in quality, and they're often quite interesting.

Ancient History

December 31, 2010
Categories: Not Even Wrong: The Book

Sometime around now is the tenth anniversary of my first foray into the business of public criticism of string theory. I wrote something up over the end-of-year holiday in 2000, and circulated it by e-mail to a list of prominent theorists (some of whom I knew, some I didn't), asking for advice. The main motivation was that it seemed to me that it had become clear that string theory had failed as an idea about unification and while this was increasingly well understood in the particle theory community, the news had not gotten out to the wider world. Instead, a fairly active campaign to promote string theory to the public continued unabated. This was a rather peculiar situation, one that I felt someone should do something about, and I was curious what my correspondents thought of it. Most responded with quite interesting comments on their views on the matter, and one of them put me in touch with an editor at Physics Today. After some back and forth with Physics Today it became clear that they were unlikely to publish anything on the matter, especially from me, so in February I posted what I had written to the arXiv as String Theory: An Evaluation.

Hard as it is to imagine, back in those days there were no physics blogs. Perhaps the closest thing was Usenet newsgroups, especially sci.physics.research, where John Baez and others had taken on the thankless job of moderating discussions which often addressed issues about string theory. The archive of these discussions is here, and some discussion of my arXiv piece broke out there, appropriately in a thread about Lie algebra cohomology. For my first posting joining that discussion, see here. This led to my first encounters with the surprising phenomena of Jacques Distler and Lubos Motl.

Scientifically, not much has changed in ten years, but the public perception of string theory has changed a lot and become much more realistic. The next decade will undoubtedly be dominated by the effects of whatever we learn over the next few years at the LHC, although I don't think this is likely to affect proponents of string theory unification very much. Many of the remaining defenders of this idea are by now pretty well dug in and make it clear that "never give up" is their policy, even if it involves abandoning all hope of understanding this universe and putting faith in the existence of others.

One outcome of this that I never expected ten years ago is that I now have a book that has been published in Czech, with the title Dokonce ani ne spatne. I can't read a word of it, which doesn't matter except that I'm intrigued to see that Martin Schnabl has written an afterword and wonder what he has to say. The publisher just sent me a few copies, but I can't think of anyone I know who reads Czech to give them to. Other than Lubos, of course, but I suspect he wouldn't appreciate the gesture…

Comments

1. **Oleg T**
   December 31, 2010

   I don't speak any Chekh but Google Translate helped me with an interview that Martin Shnabl gave to newspapers:

   http://translate.google.com/translate?u=http://www.lidovky.cz/nejde-o-moc-ale-o-dobrou-vedu-dp8-/ln_noviny.asp%3Fc%3DA101130_000112_ln_noviny_sko%26klic%3D240160%26mes%3D101130_0&hl=en&langpair=auto|en&tbb=1&ie=windows-1250

   He says that string theory is getting money in the United States because it is the best science that is available and it has no competition. Everyone else is just waving his hands and no one has yet challenged string theory as a science, Shnabl concludes.

2. **ObsessiveMathsFreak**
   December 31, 2010

   Were it not for the generally high profile of theoretical physics, I think the last ten years of controversy would probably be described as an academic storm in a teacup by most. However, given the central important of theoretical physics in the pantheon of the sciences, I think this storm was one well worth having.

   Given the intersection of science, (academic) culture, society and new technology(by which I mean blogs), I would also say that the last ten years of the String Theory debate will be a topic of some historical interest.

3. **milkshake**
   December 31, 2010

   It is not about power, but about good science
   November 30, 2010

   No-one invalidated string theory yet, says Martin Schnabl from The Institute for Physics of Czech Academy of Sciences.

   LN (the newspaper): Are top faculty appointments and generous grants really won mostly by the string theorists?

   That's true in US. Although I see the origin somewhere else than Peter Woit. According to him, the Stringmen have been usurping power and they are now holding onto their position no matter what. I just think they are doing the best science. But the situation in Europe is different. I have also won a large grant for string theory research myself but I was only second one in our field within Europe.

   LN: What's behind the string domination in US?

   String theorists are getting grants because they are bright, hard-working and creative. They would have been successful even if they focused on another research subject. In reality the grant awards are based more on results and individual track record rather than hopes and promises. The scientists decide for themselves what they want to work on.

   LN: But why are the physicists so often choosing string theory? Is there really no other alternative?

   Fundamental physics can be approached in two ways. Going from bottom up, we find that there are abundant possible extensions of our current particle physics model. Scientist are testing them by performing experiments in accelerators, for example at CERN. It is an expensive but a very solid way of doing things. The string theory represents the opposite approach, from top down. It is the only one that explains gravitation within quantum framework. Since 30s when the scientists first started thinking about this, they had not come up with any other theory that could handle this. The alternatives, even if proposed with serious intentions, are still just little more than handwawing. String theory is in agreement with all experiments so far. Even though the String theory is pretty complicated and we don't fully understand it yet, it seems to describe and unify all laws of physics. There is however a large number of possible solutions and each one of them can correspond to a somewhat different physics. That's why it's very hard to invalidate this theory

4. **Giotis**

January 1, 2011

BTW Peter, have you seen this recent small article in the pop-ph arxiv (http://arxiv.org/abs/1012.5417)? Your book is presented as a representative example of contemporary criticism towards String theory.

5. **jpd**
January 1, 2011

the abstract mentions "sinergy".
is that the finnish rock band ?

6. **Peter**
January 1, 2011

I would like to know how did you manage to write the book in a language you can not read.

7. **Peter Woit**
January 1, 2011

Thanks Giotis, hadn't seen that.

Peter,

There's a translator, who presumably has done a good job…

milkshake,

Thanks. I like the argument for string theory that "it's in agreement with all experiments so far." There are lots of competitors though which can say the same thing, for instance my unified theory which is much simpler. It goes: "I dunno…"

8. **milkshake**
January 1, 2011

I think there are some unintended funny bits in that interview:

… It is [accelerator experiments] an expensive but a very solid way of doing things. The string theory represents the opposite approach…

The verbatim translation gets even better: "It is an expensive but honest way of doing things. The string theory represents the opposite approach"

9. **Shantanu**
January 1, 2011

Peter and others, happy new year. anyhow what do you think of the fact that we do not have any experimental evidence for physics beyond standard model for more than 30 years? what do you think will be
the future of particle physics if LHC sees nothing?
Given the fact that we have not found anything are we on a completely wrong track (wrt supersymmetry, technicolor etc)?

10. **Thomas Larsson**
January 2, 2011

Being the one who started the spr thread on Lie algebra cohomology, let me recall how the discussion started.

Anybody can criticize string theory on the grounds that it makes no hard predictions, and that all weak predictions (extra-dimensions, susy, etc.) disagree with experiments. Such critique, albeit well motivated, is not very constructive. In contrast, my point was more original and based on mathematical discoveries (by myself and mathematicians) that at the time were quite novel: the Virasoro-like extensions (an element in the Lie algebra cohomology, hence the title) of the spacetime diffeomorphism algebra and its off-shell representation theory. The Lie algebras that appear in string theory are either one-dimensional (Gelfand-Kirillov dimension = 1) or classical (no extensions, representations not of lowest-energy type), and this can not be good enough for a reality which is both four-dimensional and quantum.

At the time I harbored the idea that physicists might be interested in new mathematics, especially with such obviously close ties to the symmetry principles of both GR and QM. Ah, the naivité of the youth.

11. **ThM**
January 2, 2011

Easy: scan -> OCR -> Google translate

12. **MathPhys**
January 2, 2011

The "problem" with string theory (in my very humble opinion) is the fact that it is so very successful mathematically.

It is a framework, a way of thinking (that uses "physics words" as B Schroer would say) that effectively unifies almost all of mathematics. You want a short cut to very large chunks of 19th and 20th century mathematics? Read Wittens (and Vafa's) papers!

How could that be? How can we explain that? I would say that this is the reason why die hard string theorists would never give it up.

Happy 2011!

13. **Jan Ebr**
January 2, 2011

Peter Woit: If you ever need help translating something of interest from Czech, just drop me a message. I am an occasional reader of your blog already for many years and I will be glad to help, if it is needed.

Anyway, I haven't read a "popular science" book in ages, so this seems like a good place to start. I may even do a review for our magazine.

It is not surprising to me, that it is published in Czech, as the anti-string-world-dominance feelings were always quite strong among many physicists around here. I am not implying that the community here is anyhow against string theory (after all, I think we all value having Martin Schnabl here), only that plurality of opinions is (also historically) highly valued and the view advocated by some, that the string theory is the only truth in the universe, is simply unacceptable.

Greetings,
Jan

14. **David Berman**
January 2, 2011

Hi,
I made a vow of not contributing to blogs but you make it so hard. Its the throw away comments that insult the hard work of so many. How can you say, "Scientifically nothing has changed", in ten years! Are you really saying the intellectual endevours of so many for so long mean nothing. That is just not true. I am made happy by the excellent work produced by my colleagues over the last ten years. The huge number of insights and ideas. I cannot fathom a psyche that fails to see the beauty and wishes to see fault. There isn't a Hollywood Eureka moment; but that isn't science. There is the hard work of many and the results they produce. I am thankful and so should you, since exactly where would you be without string theory?

(How do you spell endevour?)

15. **kiwi**
January 2, 2011

@Jan

you wrote: "I think we all value having Martin Schnabl here"

What about Lubos Motl?

16. **Peter Woit**
January 2, 2011

David,

Obviously the "scientifically nothing has changed" remark refers to what the posting was about: the argument I gave ten years ago that string theory unification was a failed idea. It's still a failed idea, for the same reasons as ten years ago. Actually, I was trying to be rather kind and not bring up the one thing that has changed, the promotion of landscape pseudo-science.

Outside of the unification failure, "string theory" is by now such a huge subject that of course there are plenty of interesting things learned over the past decade to point to.

And yes, I am saying that a lot of hard intellectual work by a lot of smart people turned out to be wasted as they worked their way into a dead end. But that's how science progresses, most of what one spends one's time trying to do doesn't work. The problem arises when people refuse to admit that an idea they put a lot of effort into has failed…

17. **Tim van Beek**
January 3, 2011

There is one common theme that keeps repeating itself in this discussion as exemplified by this statement…

> How can you say, "Scientifically nothing has changed", in ten years! Are you really saying the intellectual endevours of so many for so long mean nothing. That is just not true.

…besides Peter's point that string unification has failed. Of course there will always be some kind of progress in a scientific community consisting of at least 500 highly qualified scientists, progress that will be appreciated and occasionally celebrated. The question is if there are any results that are of interest to anyone outside of the community.

If you take any obscure method and calculate a scattering amplitude of a process that can be observed at a collider, this is a result that is of potential interest to experimentalists, regardless of the involved theory. But if all of the results are about answering questions that no one outside the community would ever have asked, and no one outside the community can do anything with the question and its answer, this will be of course observed as "no progress" from the outside.

In this situation there no point in lamenting the lacking interest in the progress of one's favorite topic.

18. **Somebdoy**
January 3, 2011

Time says: "But if all of the results are about answering questions that no one outside the community would ever have asked, and no one outside the community can do anything with the question and its answer, this will be of course observed as "no progress" from the outside. "

This is a reasonable querry from an interested outsider. The point however is that unification at the Planck scale is far away from our experimentally accessible scales, and we need quite a good control on the theory in order to make connections with low energy experiments. The "progress" that David talks about in string theory is (mostly) towards that goal: the goal of understanding the theory well enough. This is the necessary prerequisite to see whether it can say something about things of interest to "outsiders". By definition, such progress is of not direct interest to an outsider, especially if only the result at the end of the tunnel is considered as worthwhile.

Except for some universal statements (like those regarding black holes, and AdS/CFT inspired consistency checks) the detailed tests of string theory are very likely to require such an understanding. The reason to study string theory is that the universal things seem to work(!), which is way more than what other attempts at quantum gravity can claim.

Peter talks as if the experimental inaccessibility of string theory is an obvious fact. This is FAR from the case. Especially in light of the successes of string theory in producing the universal aspects of low energy physics, it is very worth investigating this matter in detail.

But I do think that David should have resisted the bait. Peter thrives on these… 🙂

19. **Peter Woit**
January 3, 2011

Somebody,

The problem with your argument is that all progress towards better understanding string theory in the past ten years has just given added force to the arguments for why string theory unification doesn't work. Most notably, 10 years ago one could have claimed "when we figure out how to stabilize moduli", then we will be able to make predictions. Instead what happened was that the moduli stabilization mechanism found gave us the landscape and a radically non-predictive unification scenario. Yes, this is progress towards understanding string theory, but it leads to understanding more clearly why string theory unification can't work.

As for claims about "the successes of string theory in producing the universal aspects of low energy physics", that's just ridiculous.

20. **David Berman**
January 3, 2011

Hi all,

I will resist the general discussion but in reply to Tim Van Beek, I couldn't quite figure out if you know that indeed progress in QCD scattering amplitudes is happening as a result of a stringy understanding. My colleagues at Queen Mary are amongst those that provide calculational tools for people interested in QCD amplitudes (I believe they are incorporated into the state of the art monte carlos at CERN). So indeed your example is completely poigniant to the discussion but perhaps not in the way you imagine. Or perhaps I've misunderstood you.

I will now gracefully bow out and now the holliday is over go back to my day job.

21. **El Cid**
January 3, 2011

Very fast damn Peter. You haven't wasted time. Peter, I hate you. You're just looking for money. I'm going to attack your blog. You hate physics, You hate the science.

22. **Mitchell Porter**
January 3, 2011

Peter Woit said

"the moduli stabilization mechanism found gave us the landscape and a radically non-predictive unification scenario. Yes, this is progress towards understanding string theory, but it leads to understanding more clearly why string theory unification can't work."

In fundamental physics, we are trying to learn about reality. We have a working model with all sorts of unexplained numbers. We may hope that all those numbers are uniquely determined for reasons we don't currently understand. But that is just a hope.

Meanwhile, we have string theory, with its landscape of solutions. Judging just by experimental adequacy – and not the apriori hope that all those numbers are uniquely determined – every single string vacuum which perfectly matches experiment is a candidate for the correct description of reality. This is true whether there's only one such vacuum, or whether there are infinitely many of them.

So what is the situation in string theory? There are many promising classes of vacua, but not even one vacuum has been *proven* to fully reproduce the standard model. This is a symptom of slow progress on topics like moduli dynamics, though such progress is occurring (e.g. see 1101.0108 at the arxiv today). Eventually we *will* know whether some of those promising vacua really do match experiment, or whether none of them do.

The moment that a string vacuum is found which is both mathematically tractable and fully consistent with experiment, then string theory will finally have delivered a candidate description of the world. If it produces a hundred or a million such vacua, they will all be possibilities – and most likely they will fall into classes which do make distinct predictions about higher energies, making them subject to future falsification. Finally, if it can be shown that *no* string vacuum can match experiment, then string theory really will have been falsified. Only then would you be able to say that string theory can't be the truth. And given the number of phenomenologically promising classes of vacua, it appears far more likely that string theory will instead provide the "standard models" of the future.

23. **Peter Woit**
January 3, 2011

Mitchell Porter,

"Finally, if it can be shown that *no* string vacuum can match experiment, then string theory really will have been falsified. Only then would you be able to say that string theory can't be the truth."

Well, since there's no way to computationally ever do that, there's no way to ever show that string theory is wrong. Your conclusion about a theory that can't be shown to be wrong is

"string theory will instead provide the "standard models" of the future."

My guess is that the idea of using a theory that can't be shown to be wrong as the "standard model" of the future is one that few physicists are willing to go along with.

24. **Mitchell Porter**
January 4, 2011

Peter,

Let's distinguish between falsifying string theory as a whole, falsifying a class of string models, and falsifying a specific string model.

So far as I can tell, the set of string models for which moduli dynamics are fully understood, and the set of string models that are of phenomenological interest, are completely disjoint. All of the latter, like the heterotic MSSM or the intersecting IIA braneworlds, still pose unsolved mathematical problems which prevent the calculation of standard-model masses and coupling constants.

Now let us suppose that with time, after sufficient mathematical progress, the models of phenomenological interest become fully tractable. This is when we can really talk about falsifying, not just specific models, but even whole classes of models, by *proving* that an empirically necessary conjunction of properties does not occur anywhere in the class.

When you say that "there's no way to computationally ever do that" (for the whole of string theory), you must be thinking of arguments like Denef & Douglas's, that the Bousso-Polchinski model of the cosmological constant would be NP-hard to test. But as Denef himself suggests, such complexity-theoretic results might be more like "no-go theorems", telling you how *not* to proceed.

In computer science, two things which can speed up search are (1) structure in the search space (2) search for a special property, and string phenomenology possesses both of these. Looking for the cosmological constant a la Bousso-Polchinski – a finely-tuned near-cancellation of numerous fluxes – must be just about the least efficient way to falsify string theory. The qualitative features of the standard model already suffice to rule out the vast majority of string vacua, and subtle facts like Koide's relation connecting the masses of the charged leptons will also be highly constraining.

When I say that string theory will supply the standard models of the future, I mean only that I expect some string vacua to pass all these tests. The standard model won't be "string theory, in which we believe because it contains gravity", it will be "string theory on background X, because it predicts the correct masses and coupling constants".

25. **Somebdoy**
January 4, 2011

Peter says: "As for claims about "the successes of string theory in producing the universal aspects of low energy physics", that's just ridiculous."

Alright, I'll bite. Here are some aspects of how string theory captures general features of low energy physics. While these will not impress you, I will keep an open mind that some of your readers are not as opinionated as you are.

All phases of string theory contain black holes. Whenever they are under computational control, the Bekenstein-Hawking entropy is reproducible microscopically. In the limit of small temperature of the black hole, even dynamical phenomena like Hawking decay rates and their greybody corrections can be produced from string computations. As Sen likes to emphasize, if there was a mismatch in *any* of the phases of string theory, then it would immediately have been game-over for string theory as a whole. Many spacetime singularities are naturally resolved in string theory. Chiral fermions, multiple generations, non-abelian gauge symmetry, hierarchies in Yukawa couplings etc. are all essentially automatic in any string compactification. Each of these things is a puzzle from a purely low energy perspective. Things like AdS/CFT and the qualitative matches with RHIC-like experiments is another generic test. Finally, all of these things are coming from a quantum theory that containes gravity – usually, attempts in this direction result in instantly-dead-at-birth theories. (Another one of the things that is lost on the layman is how remarkably gauge theories and gravity seem to mesh together to make AdS/CFT and string theory consistent, but this is more theoretical, so its not too relevant for this discussion.)

So yes, while you might argue that string theory hasn't made precise quantitative predictions yet, generic aspects of string theory do capture low energy physics. Its not "just ridiculous", sorry.

An over-riding theme in the things I mentioned above is that "whenever we understand string theory", things seem to work. This is the reason why it is interesting for some of us to work on understanding the theory fully.

26. **Peter Woit**
January 4, 2011

Somebody,

I see, the "successes of string theory" in low-energy physics that you are hyping have really nothing to do with the topic at hand: the failure of string theory unification. Black hole entropy tells us nothing about observable low-energy physics, and AdS/CFT is a completely irrelevant issue.

The one sentence you write that is relevant:

"Chiral fermions, multiple generations, non-abelian gauge symmetry, hierarchies in Yukawa couplings etc. are all essentially automatic in any string compactification."

is nonsense. You can find string theory "vacua" violating any of those, and if low-energy physics were different, I'm sure you'd be telling us that something else was "essentially automatic". For instance, in a non-chiral world, we'd probably be hearing a lot about how N=2 supersymmetry was crucial in string theory, so non-chiral couplings were "essentially automatic".

It's interesting that you don't mention the most famous argument of this kind: the "string theory predicts low-energy supersymmetry" argument. I notice that over the past ten years as the Tevatron has seen no evidence of supersymmetry, and as the arrival of relevant LHC results approaches, string theorists have started to back away from this argument. My prediction is that over the next few years they'll be running away from it, and claiming they never believed it. Unless of course, evidence for supersymmetry appears, in which case we'll hear something very different…

Mitchell,

You're creating for yourself a hypothetical world in which all the problems on which no progress has been made in 26 years (for very good reasons) have been solved, and then arguing on the basis of that. This is just wishful thinking, not science.

27. **Somebdoy**
January 4, 2011

"Black hole entropy tells us nothing about observable low-energy physics,… "

My low energy world contains gravity as well, Peter. And not just the standard model of particle physics. Since general relativity is part of low energy physics, we are forced to confront black holes. Its not an option.

About the rest, the claim was that string theory contains the ingredients of particle physics. That statement is true and non-trivial, notwithstanding your outbursts.

28. **Peter Woit**
January 4, 2011

Somebody,

We've gone from:

"the successes of string theory in producing the universal aspects of low energy physics"

to

"the claim was that string theory contains the ingredients of particle physics"

which is rather different. Sure, string theory contains "the ingredients of particle physics", along with 10^500 other things. The problem is that it has failed to tell us anything about these ingredients.

29. **Somebdoy**
January 4, 2011

There is only one particle physics in the real world. So the word "universal" doesn't make much sense in my original statement. I am pretty sure that you understood that what I meant was "generic".

If you want to be cynical … 🙂

30. **Eric**
January 4, 2011

I think it should be pointed out, lest someone get the wrong idea from the above arguments, that it is in fact possible to find string vacua which are very close to the observed Standard Model. This includes all of the features of the Standard Model such as chiral fermions, multiple generations, hieararchical Yukawa couplings, etc. as Somebody has pointed out. This by itself is reason enough to believe that string theory is on the right track.

What has not been accomplished up to this point is to uniquely fix the parameters of these models in order to determine the actual values of gauge and Yukawa couplings. The study of the mechanisms for doing this has been a very active area of research over the last decade and much has been accomplished. I fully expect to see a string model constructed in the next 10 years or so where this can be done. As to why our universe contains the three-generation Standard Model rather than some other gauge symmetry or matter content is a question that will take some time to answer.

31. **Giotis**
January 4, 2011

Somebody said:

"Chiral fermions, multiple generations, non-abelian gauge symmetry, hierarchies in Yukawa couplings etc. are all essentially automatic in any string compactification."

Such a statement is absurd of course and I find it strange that you could say such thing since as it seems you know quite a lot about the subject. Maybe the above nice properties are present in the models you know but this is exactly because they have been engineered to produce them. In any case I don't understand the point you are trying to make with similar statements.

In the current situation I think there are 5 options that someone could choose from regardless of any LHC findings:

1)Dismiss String theory as a framework for unification altogether.

2)Adopt the multiverse idea, eternal inflation, anthropic reasoning etc..

3)Dismiss the current concept of low energy effective vacua and wait until there is a better understanding of the theory hoping that this will eventually help us understand how our world emerges naturally from the theory.

4)Accept the validity of the current concept of low energy effective vacua and wait until a vacuum selection principle is found which will hopefully select our vacuum.

5)Do nothing of the above and continue to be skeptic.

32. **Tim van Beek**
January 5, 2011

David Berman said:

> I will resist the general discussion but in reply to Tim Van Beek, I couldn't quite figure out if you know that indeed progress in QCD scattering amplitudes is happening as a result of a stringy understanding.

This blog is mainly about string theory as a theory of unification (of the standard model and gravitation), and that's what my remark was about. If one counts the application of "stringy understanding" to QCD as a success of string theory is a matter of discretion, depending on one's understanding of what "string theory" comprises. In the sense of this blog's understanding of string theory as unification, it is not.

To pick another not too elementary example: One can use white noise calculus in order to give QFT an axiomatic foundation using the Osterwalder-Schrader axioms. One can also use white noise calculus to try to understand stochastic partial differential equations and turbulence in Navier-Stokes equations.

These are completely different physical theories, but of course one is free to claim that progress in QFT has led to a better understanding of turbulence in classical physics (and if you think both topics through and solve all problems, you could even win *two* millenia problem prices at once, think about that!).

33. **Somebdoy**
January 5, 2011

Giotis, the general context in which my statement is to be understood is the original Calabi-Yau compactification papers of Candelas, Horowitz, Strominger and Witten. As I repeatedly emphasized, the question was not what is possible[1], but what seems to arise essentially "automatically". This is admittedly a not very precise notion. For example, inherent in this claim there are some assumptions: like we demand four non-compact dimensions. But there exist large classes of string theory models where my statement is true, is the point. Its hard to define a measure on the space of theories, so it is not very meaningful to be more precise.

[1] There is a quote attributed to Coleman that string theory is not a theory of everything, but a theory of anything. (This is one of the things that bothers Peter and probably you.) This is in fact less true about string theory than quantum field theory, because UV completion puts constraints. The hope of the last decade (which seems completely unreasonable in hindsight) was that there will be a unique vacuum that will emerge. Instead it turns out that UV completion is constraining, but still we do have a large choice: in the industry-lingo, this is what distinguishes between the "swampland" and the "landscape". But a theory can be predictive without being unique. In fact, quantum field theory does just that, i.e, standard model, a specific quantum field theory is predictive. So string theory might turn out to be predictive if we could only wield it well.

I don't think your 5 bullet points cover all that is possible. Keep in mind that a theory doesn't have to predict the values of the couplings to make it a predictive theory. The choice of the vacuum might be anthropic or accidental or initial condition or whatever you want to call it, but after a finite number of experiments things can be predictive. The standard model contains 19 undetermined couplings, but (ideally) after 19 experiments to calibrate those numbers, the theory is predictive. String theory could work the same way: the choice of vacuum would determine the couplings, and indirectly we will be determining the vacuum by doing the calibration runs. We simply don't know if this situation is possible in string theory, which is why pronouncements irritate the practitioners. There are some subtleties here, like how much purely low energy experimenst will be able to fix the vacuum: I like the way Mitchell describes the situation in the last para of his post at 10:51.

Hope it was not too useless. I don't have the time to write a longer message.

34. **Giotis**
January 5, 2011

Somebody,

If there is no underlying principle which will point to a specific vacuum you could always change the vacuum to fit the experimental data by manipulating your model. That's the whole point of the criticism.

35. **Somebody**
January 5, 2011

Well, there is no underlying principle that uniquely picks out the standard model from the plethora of possible quantum field theories. But thats hardly a problem. As I said, uniqueness is not a necessary requirement for predictivity. You are thinking of computing the coupling constants, I am talking about fixing them experimentally. Please read my post again.

The point is whether you can fix a vacuum/model by doing a finite number of experiments. If you can, then all experiments after that are predictions. "Manipulating" a model as you call it, is what went into (for example) the construction of the standard model in the last century. One way to look at the question is whether this scenario can be realized in string theory. There is the possibility that string theory might provide the same kind of arena for UV complete model building that QFT provided for theories without gravity. Only time and hard work will tell. One trouble is that model-building in string theory as it stands is sketchy because we don't understand the theory well enough, so we have no good organizing principles (like gauge symmetry in QFT).

36. **Peter Woit**
January 5, 2011

Somebody,

The Calabi-Yau compactifications chosen for study in 1985 were chosen very specifically to have the right general characteristics to have some hope of giving the (supersymmetric) standard model at low energies. You can't point to this as providing "generic" features of string theory at low energies.

It's quite understandable that there was a flurry of excitement in 1985 when people started looking at these models. On the other hand, from the beginning there were also very obvious reasons to be skeptical. Not a single specific feature of the standard model was predicted (gauge groups, couplings, matter representations). As time went on, it soon became clear that there was so much freedom in how you chose your "string vacuum" that you could probably get just about anything. The only hard thing to understand is why 26 years later, people are still claiming this is a viable idea about unification.

As for the "string theory more predictive than qft" argument, it's a waste of time to keep pointing out why that's wrong. If someone wants to keep arguing that a theory that predicts nothing is more predictive than the most successful theory we have ever had, there's not much point in going there…

37. **pointless**
January 5, 2011

Hi Somebody (and others with similar intentions …),

isn`t it a bit pointless for You wanting to explain and discuss things in this blog? Your efforts will never change the positions of anybody here…

You have certainly more important things to do 😉 …

Happy New Year

38. **Somebody**
January 5, 2011

Hahaha! Its my holiday good deed. I like to think that I am talking to the lurkers who never post on this blog, but who might be more reasonable. Otherwise, it is easy to flip out and go on a shooting spree when faced with these relentless misrepresentations. 🙂

Cheers!

39. **pointless**
January 5, 2011

He he,

I sometimes silently lurk around 😉 …

Had to laugh about the joke linked to; most of these abstruse goose comics are fun.

I was just asking myself if the game they are playing here would lose it`s fascination if they could not evoke any response?

Anyway, enjoy Your holiday then 🙂

Cheers

40. **El Cid**
January 6, 2011

A question, If you say that you aren't an expert in string theory, what the hell are you criticising? You don't know of what you are talking about. You aren't a physicist, any longer. Maybe you've got in a Ph.D. in theoretical physics in a prestigious university long time ago, but now you don't want to learn theoretical physics. why don't you try to learn the hard and necessary math and then to try understanding string theory before to criticise the theory?
Because the only aim for you is to argue with the true physicists for laughing of them and your blog is only about sociology. Your book is for earning a lot of money with this dishonest behaviour. Because of this I hate you and I'm going to attack your blog. You are an enemy of science you are an enemy of the hard work.

41. **Shantanu**

January 6, 2011

Peter, probably you already know this. But Rutgers also archives their
HET seminars.
http://www.physics.rutgers.edu/het/video/het-video.html
there are two talks by Witten on this

42. **Igor Khavkine**
January 7, 2011

Somebdoy says: (January 5, 2011 at 6:21 am)
"But a theory can be predictive without being unique. In fact, quantum field theory does just that, i.e, standard model, a specific quantum field theory is predictive."

That's an excellent argument. In fact, it works so well that it also applies to not-necessarily renormalizable QFTs. It then puts gravity-as-a-QFT on the table as a theory of quantum gravity. That plus the standard model does an exceptionally good job of accounting for all known physics.

Obviously unknown physics cannot be accounted for, since we do not know what it is. It does sting a little bit that unknown physics currently includes the very interesting questions of the fate of evaporating black holes, the mass of the Higgs field, and the possible additional fields and interactions relevant in the early universe. But that can only be helped by looking for the answers in nature, rather than on paper.

Suddenly the rationale for using string theory at all, instead of plain old QFT, has evaporated…

43. **Eric**
January 7, 2011

I think probably the best way to view perturbative string theory is that it is really an effective theory which is valid up to some scale M_{string} < M_{Planck}. So, in this sense it isn't a complete unification theory, at least at the perturbative level, as Peter likes to say. However, I do believe it does take us a little closer to unification than do gauge theories/QFT by themselves since now gravity can be brought into the picture. It just doesn't take us close enough to say, uniquely predict the Standard Model, if that's even possible. What is needed is to find the theory which is valid at the Planck-scale, and to which perturbative string theory is an approximation. This sometimes goes by the name of "non-perturbative formulation" or "M-Theory".

44. **Somebody**
January 7, 2011

Khavkine says: "In fact, it works so well that it also applies to not-necessarily renormalizable QFTs. It then puts gravity-as-a-QFT on the table as a theory of quantum gravity… Suddenly the rationale for using string theory at all, instead of plain old QFT, has evaporated"

No, you will need an infinite number of experiments to fix all couplings if the theory is non-renormalizable. Thats not predictive, even in the best case scenario. UV completeness is key. Note that we are talking about all energies at once, not just low energies.

Here is one scenario: if we had all the control on string theory that we wanted, then we will be able to construct vacua which reproduce everything we see at low energies: standard model and black hole puzzles and cosmological constant and what not. The question that we don't know the answer to is, (1) is there any vacuum that allows this? (2) is there more than one vacuum which allow this? If the answer to (1) is no, string theory is ruled out. If the answer to (2) is yes, then it will not be within the realm of science to distinguish between these degenerate (at low energy) vacua without doing high energy experiments.

My strong suspicion is that if we fully understood string theory, it will actually enable us to relate low energy and high energy in more interesting ways, so the last setup may not even be realized. But even if it is, we will still have a candidate for a (universality class of) complete descriptions of Nature.

For the prupose of full disclosure, I should add that the "string phenomenology" as it is practised today is not really in line with this philosophy. It tries to construct vacua using more short-sighted approaches, because of our inadequacy in understanding non-perturbative string theory. But since it seems quite likely that the answer to question (1) is "yes", I think this might indeed be enough to settle that once and for all. But I suspect that answering (2) in any sort of useful way is going to require a real understanding of string theory, instead of the various bits and pieces that we currently refer to by that name.

45. **Igor Khavkine**
January 7, 2011

Somebody wrote: "No, you will need an infinite number of experiments to fix all couplings if the theory is non-renormalizable. Thats not predictive, even in the best case scenario. UV completeness is key. Note that we are talking about all energies at once, not just low energies."

Yes, thank you for pointing out the obvious difference between renormalizable and non-renormalizable theories. However, despite this difference, non-renormalizable theories are not any less predictive. In fact, once I pick any set of values for the infinite set of coupling constants needed to define my QFT, it is as predictive as desired and at all energies to boot. Whether this fixed theory is correct is, like any other hypothesis, to be tested by experiment. Do you still disagree?

Now, even if you agree with the above, you might take issue with exactly how the coupling constants were chosen. For the sake of argument, let me set all of them to zero, except the finitely many that have been directly probed by experiments to date; those I set to the measured values. I didn't have to use zeros, I could have used the outcomes of an imagined infinite sequence of coin flips. The point is that there is no known preferred mechanism for making that choice. And even if string theory is a candidate for such a mechanism, it is not known to be preferred to, for example, the two methods I just made up on the spot.

The other comments you've made rely on the hypothesis that the fundamental model we need will be a string theory. Now that is the hypothesis that IMHO severely lacks evidence.

46. **somebody**
January 8, 2011

Khavkine: "In fact, once I pick any set of values for the infinite set of coupling constants needed to define my QFT, it is as predictive as desired and at all energies to boot."

Hmm? How does one fix these coulings experimentally is the question. Couplings are fixed by EXPERIMENT, not by "picking" whatever value one wants. If you have a theory that had infinite number of couplings, one needs an infinite number of experiments to determine them, so the theory is not predictive. Simple as that. PS: Anomalies would rule out many theories, but this is even before that.

47. **somebody**
January 8, 2011

“Now, even if you agree with the above, you might take issue with exactly how the coupling constants were chosen. For the sake of argument, let me set all of them to zero, except the finitely many that have been directly probed by experiments to date; those I set to the measured values. I didn’t have to use zeros, I could have used the outcomes of an imagined infinite sequence of coin flips. The point is that there is no known preferred mechanism for making that choice.”

No, I don’t agree with the above. I agree that your setup is operationally okay at low energies, but is meaningless as a full theory. I expect that questions that we could ask at any energy should have reasonable answers. When all energies are allowed, your set up is *meaningless*.

Your claim is tantamount to arguing that the world is not predictive at a fundamental level. That was a valid argument at any stage of science when there was something we didn’t understand. But so far that argument has been wrong.

48. **Igor Khavkine**
January 8, 2011

Dear Somebody, I do not disagree that you are arguing from conventional wisdom. However, conventional wisdom needs to be shaken up and re-examined once in a while. With that in mind, perhaps you can be a bit more specific with your counter arguments when it comes to predictivity and meaningfulness at arbitrary energies.

I described two specific (though possibly appearing convoluted to some eyes) QFT models. What is it that these models cannot predict? A specific example would be nice. And what kind of question gets a meaningless answer at arbitrarily high energies? I cannot think of any such examples myself. After all, a fully renormalized QFT model yields n-point functions that are finite where expected and defined for all energies and all n. Moreover, any quantitative question posed comes down to the knowledge of n-point functions. So I’m puzzled by where your examples could come from.

“Your claim is tantamount to arguing that the world is not predictive at a fundamental level. That was a valid argument at any stage of science when there was something we didn’t understand. But so far that argument has been wrong.”

I’m sorry, I really do not see where you drew that conclusion from. I’m also completely stumped by the second sentence above. There are certainly many things in science we do not understand. One does not even need to stretch to meaning of the word “understand” to extremes. The particle/field content and their interactions that were relevant in the early universe is an example of something we do not understand. Though, still, I’m not sure how these factoids pertain to the discussion.

49. **Somebody**
January 8, 2011

Here is where your basic fallacy is (in my view):

“In fact, once I pick any set of values for the infinite set of coupling constants needed to define my QFT, it is as predictive as desired and at all energies to boot. Whether this fixed theory is correct is, like any other hypothesis, to be tested by experiment. Do you still disagree?”

One doesn’t fix the theory by devination and then check it against experient. The fixing itself done VIA experiment. And when you have infinite couplings there is no operational way to do it.

When you are truncating the theory to a finite number of operators the situation *qualitatively* changes. What I am saying is that the truncation can arise from either a UV complete theory or a non-renormalizable theory, but only one of them is really a theory (the UV complete one).

In any event, the situation regarding non-renormalizable theories being unpredictive is not something that I would refer to by the romantic term, “conventional wisdom”. I would say that it is an automatic consequence of what non-renormalizability means. So if you still don’t agree, maybe it is better that we agree to disagree.

Best wishes!

50. **Bugsy**
January 14, 2011

As usual, a great blog by Peter, giving space for fascinating comments from all sides. (I am speaking as a complete outsider to mathematical physics, and am grateful for this accessible window looking in). BTW: if Peter were more diplomatic and less provocative, I doubt the resulting discussions would be half as interesting!

What the M Stands For

January 3, 2011
Categories: Uncategorized

There's an explanation at the latest Abstruse Goose.

To recycle some of my own writing, from page 107 of NEW, the book:

> When I was a graduate student at Princeton, one day I was leaving the library perhaps thirty feet or so behind Witten. The library was underneath a large plaza separating the mathematics and physics buildings, and he went up the stairs to the plaza ahead of me, disappearing from view. When I reached the plaza he was nowhere to be seen, and it is quite a bit more than thirty feet to the nearest building entrance. While presumably he was just moving a lot faster than me, it crossed my mind at the time that a consistent explanation for everything was that Witten was an extra-terrestrial being from a superior race who, since he thought no one was watching, had teleported back to his office.

And, before anyone takes this seriously, I certainly don't believe this is the explanation for the "M" or that any actual teleportation occurred. To quote the next paragraph of the book:

> More seriously, Witten's accomplishments are very much a product of the combination of a huge talent and a lot of hard work. His papers are uniformly models of clarity and of deep thinking about a problem, of a sort that very few people can match. Anyone who has taken the time to try and understand even a fraction of his work finds it a humbling experience to see just how much he has been able to achieve.

Update: Clifford Johnson at Asymptotia points out a recent talk by Witten to a non-specialist audience about knots. It there is a Martian plot going on here, at least it has led to some wonderful insights about mathematics and quantum field theory that human beings might never have otherwise been able to figure out…

Comments

1. **wolfgang**
   January 4, 2011

   Peter,

   but maybe the truth is that *you* were sent by the Martians to confuse people about string theory 😎

2. **Peter Woit**
   January 4, 2011

   wolfgang,

   Actually, it's the Venusians who sent me, in a so far fruitless attempt to foil the Martian plot…

3. **MathPhys**
   January 5, 2011

   "And, before anyone takes this seriously, I certainly don't believe this is the explanation for the "M" or that any actual teleportation occurred."

   Did you really find it necessary to spell this out, Peter? You must have had such harrowing experiences in the past, you can no longer afford to take any chances.

4. **Nige Cook**
   January 5, 2011

   Dr Ed Witten's World of Mathematics biography states: "Witten, whom his students affectionately nicknamed "the Martian" because of his brilliance and soft voice, gave up his teaching duties in 1987 to concentrate on his research. … His wife reports that Witten does calculations only in his head."

5. **Chris Oakley**
   January 6, 2011

   MathPhys,

   You can't be too careful. When Steven Weinberg visited Oxford in 1983 there were a dozen or so alien space craft hovering above the city for a whole week. This may not have had anything to do with him, but the whole experience was quite unnerving. They only went when they found that the local shopkeepers were refusing to take their money, which was in plasma form.

6. **Thingumbobesquire**
   January 8, 2011

   Since M Theory is a product of Cambridge, it only make sense that it must like the M in MI5 stand for Mother. Hence its similarity to the pagan great mother worship of Gaia (which I believe is another project of like minded "thinkers.")

7. **Thomas Larsson**
   January 8, 2011

   There is of course Glashow's explanation: the M is really an up-side-down W, for Witten.

8. **Kris Krogh**
   January 9, 2011

   "it has led to some wonderful insights about mathematics and quantum field theory that human beings might never have otherwise been able to figure out"

   Peter, could you mention which of those insights you feel are important, and why?

A couple of decades ago, it was widely advertised that string theory had made a fundamental contribution to topology, of which mathematicians were in awe. The claim was that one topology could legitimately be converted to a different one via the singularity of a black hole. Haven't heard anything about that since. What's happened there?

9. **Yatima**
January 9, 2011

Okay. Linux does not have decoder for the Witten Talk. H.264? Patent-encumbered. HURRR!

10. **Peter Woit**
January 9, 2011

Kris,

That quote referred to Witten's talk about QFT and knots, and specifically I had in mind the work on this topic ("Chern-Simons-Witten theory") that won Witten a Fields medal. It doesn't have anything to do with string theory.

[joke]It seems that the clever Martian plot is to have Witten "discover" incredible deep and obviously significant insights into qft and math, thereby gaining a huge amount of credibility as a genius and guru of the field. This is then used to get people to believe the string theory unification stuff, which would not otherwise be taken seriously. [\joke]

11. **Marcus**
January 10, 2011

Malarkey

12. **zanibar**
January 10, 2011

NCZ

I would explain the joke more like this —

The Martians think that humans are becoming too advanced technologically — so they deliberately send an agent in disguise to lead the humans astray in order to halt any real scientific advancement …

Sort of a fake Mathematical Moses leading the Israelites into the desert, but not out of it.

KRLLL's credentials and following could have been established with "legitimate" science (unlike Peter's interpretation), the joke takes this as a given – obviously an insider's look at physics, going beyond merely copying buzz-words on the blackboard in panel 8.

The joke hinges on identifying Witten as KRLLL together with the implication that M-theory is really a Martian conceit.

The last three panels complete the joke. A classic parody of a classic sci-fi theme.

Reminds me of a truly great episode of the original Outer Limits – "O.B.I.T." (Out of Band Individuated Teletracer), including the finale.

See –

O.B.I.T. (wikipedia)

O.B.I.T. Epside Review (from David Schow's amazing site)

/NCZ

13. **Chris Long**
January 11, 2011

You can see Witten's talk just fine under Linux, e.g. just open the URL in Kaffeine.

Differential Cohomology at the Simons Center

January 11, 2011
Categories: Uncategorized

This week the Simons Center is hosting a workshop on Differential Cohomology and its applications in physics. I won't try and give an explanation of what differential cohomology is here, with a little luck the videos of the talks will soon be on-line. Very briefly, this subject is about an extension of the usual sort of cohomology theory that provides finer information. It was discovered independently by Deligne in an algebraic geometry context (his construction is often called "Deligne Cohomology") and by Jim Simons and Jeff Cheeger in a differential geometry context. The subject made its appearance in physics first through Wess-Zumino-Witten terms in non-linear sigma models and the Chern-Simons term in gauge theories.

Dan Freed's first lecture included an extensive discussion of one recent example that uses a generalized cohomology theory, and thus generalized differential cohomology, see here for details. Mike Hopkins discussed his work with Singer which led to this paper, and some ongoing work from a more generalized perspective. He started with some history, explaining that things began with a specific example he noticed in work on topological modular forms that Witten had found around the same time in work on the partition function of the fivebrane. He described this initial impetus as like discovering that they both were looking at the same intriguing specific tropical fish, with attempts to understand it leading to a huge ferocious formalism he characterizes as a shark that lept out of the tank.

In the afternoon, Jim Simons gave a wonderful description of the early history of his work on Chern-Simons invariants and Cheeger-Simons differential characters, leading up to recent work trying to prove that certain properties uniquely characterize this kind of theory. He began his talk by noting that only one small piece of chalk was available and complaining "I paid all this money for this place and all I get to use is one broken piece of chalk?". The story started when he tried to work out a combinatorial formula for the signature in 4 dimensions, by analogy with what one does starting with the Chern-Weil formula for the Euler characteristic. In the signature case, the evaluation of a 4d Pontryagin class leads to the study of a 3-form on the boundary, which he investigated with Chern, leading to Chern-Simons theory. This is much the same problem as the one that (more than a decade later) I started working on as a graduate student in physics, trying to figure out how to calculate the second Chern number of a lattice gauge field configuration.

Finally Krzysztof Gawedzi gave an interesting talk reviewing the by-now-extensive history of the use of this kind of mathematics in physics, including various incarnations of the notion of a "gerbe". Unfortunately I'm back in the city now, hope to follow the rest of the workshop via video at some point.

Comments

1. **Number Freak**
   January 12, 2011

   Does Simons still do research?

2. **Peter Woit**
   January 12, 2011

   Yes, he's been doing work with Dennis Sullivan the past few years, see

   http://arxiv.org/find/math/1/au:+Simons_J/0/1/0/all/0/1

HEP News

January 11, 2011
Categories: Experimental HEP News

While I was away at Stony Brook yesterday, every other blog and news source out there had a story you've surely seen about the DOE's decision to turn down a proposal to seek funding to keep the Tevatron running past the end of this fiscal year. This means that soon the long era of physics at the high-energy frontier pioneered and often dominated by the US will conclusively be over, probably at least for the rest of my lifetime. It will continue in Europe at CERN, with the LHC and whatever follow-on machines get designed and built there. In some sense this was bound to happen sooner or later, once the decision was made to pull the plug on the SSC. See Cosmic Variance for a long history of the Tevatron from John Conway. Also, see here for the latest from the director of Fermilab.

The US is throwing in the towel for a combination of reasons that include a desire to devote all resources to new ventures with more of a future, the fact that continued running would not dramatically increase the total size of the data set, and faith that the LHC will reach its goal of several inverse femtobarns of data at 4 GeV/beam over the next couple years. It's still somewhat difficult though to understand why, in order to save 5% of its HEP budget, the US is shutting down a machine that continues to produce important new results, some of which cannot be easily studied at the LHC. An intriguing example is CDF's recent data on asymmetry in the production of top-anti-top pairs. For an explanation of this you can't do better than to see the discussion at Resonaances. This result uses the fact that the Tevatron collides protons and antiprotons, allowing measurements that can't be done with proton-proton data from the LHC.

Unlike the CDF result, the latest LHC results just exclude more and more popular extensions of the Standard model. CMS yesterday (see here and here) released results (discussed earlier here) that rule out a range of once popular values for masses of supersymmetric partners. In this arena, the LHC is quickly moving to outclass bounds from the Tevatron.

Comments

1. **tev**
   January 11, 2011

   The response from the Fermilab director is here
   http://www.fnal.gov/pub/today/archive_2011/today11-01-11.html

   The statements by John Conway about the history of the Tevatron (circa 1976-83) are not entirely accurate. (Tommaso Dorigo may offer his own take on this?) There is no mention of BNL (Brookhaven National Lab) and ISABELLE. When Fermilab started, there was a conflict between FNAL and BNL as to who would get the next big proton machine. The eventual agreement was that Fermilab would do fixed-target (synchrotrons) and BNL would do colliders. So when John says that (paraphrasing) "The lab was engaged in a wide range of fixed target experiments, using the Fermilab Main Ring proton synchrotron as its workhorse …. But Europe pulled ahead – it already had the Super Proton Synchrotron, and plans to convert it into a proton-antiproton collider…."

   Well … not quite. *Fermilab* was not building a collider, but *USHEP* as a whole was. BNL was building ISABELLE 1976-83 (approx). The design was 500×500 GeV (sqrt(s)=1 TeV), with the explicit goal of discovering the W and Z. The project failed miserably, and when CERN produced the W and Z using the SppS (UA1 and UA2 expts), then in July 1983 the ISABELLE project was cancelled. The proposal for the SSC was born around the same time.

   After the cancellation of ISABELLE, Fermilab went ahead and converted the Tevatron (originally visualized as a fixed-target machine) to be also a p-pbar collider. This was in 1985. But by then CERN had locked up the Nobel Prize (1984 to Carlo Rubbia and Simon van der Meer).

   Read this about ISABELLE

   Pt I
   http://www.springerlink.com/content/r114825754r38578/

   Pt II
   http://www.springerlink.com/content/824344k282615438/

2. **SpearMarktheSecond**
   January 11, 2011

   All good things come to an end, and no doubt, the Tevatron was a very good thing. The issue now is… what is the US experimental particle physics program? The space-based dark energy program (SNAP/JDEM/WFIRST) is delayed to infinity, as is the linear collider. DUSEL hangs by a thread. NOvA is a bit of a wimper. Meanwhile, Italy has green-lighted a super-B factory, China has a charm factory, and Japan has taken over accelerator neutrino physics and kaon physics.

   Well, maybe AMS II will make a great discovery.

   Are we not physicists? DEVO.

3. **Peter Woit**
   January 11, 2011

   SpearMarkII,

   Excellent question. By the way, this made me realize that I have no idea why the US gave up on B physics. As far as I can tell, the Italians are planning on building a B-factory with spare change left over after their CERN contribution (and with no longer needed equipment from the US). What's that all about?

4. **Shantanu**
   January 13, 2011

   Maybe whole of HEP community will switch to neutrino physics,
   particle astrophysics or LIGO (which is funded)

5. **El Cid**
   January 15, 2011

   The future of physics and engineering in USA is in the field of inertial confinement fusion to achieve ignition with the new device NIF. Experimental HEP is RIP in USA.

The only hope is in theoretical HEP where USA is still the world leader by far.

More Short Items

January 16, 2011
Categories: Uncategorized

- There's an excellent article by Michel Berube about the Sokal hoax, fifteen years later, entitled The Science Wars Redux.
- The latest Notices of the AMS has a review of the recent Yau-Nadis book by Nigel Hitchin (for my take, see here).
- My colleague Brian Greene has a new book coming out soon, The Hidden Reality: Parallel Universes and the Deep Laws of the Cosmos. I haven't seen a copy, but from what I can gather, it looks like it is probably the best of the many books about "multiverse" ideas, but still not exactly my cup of tea. If you're interested in the "multiverse" and want to read a popular level exposition, you should try this one. But you should also pay attention and see if there's any experimental evidence (or reasonable hope of getting some) for the ideas being discussed. The book has very extensive more technical notes, and the Amazon site gives access to these. Brian also has an Op-Ed piece in today's New York Times drawn from the book, about the fact that in an accelerating universe, in the distant future less and less will be visible.
- It seems that there recently was a Physics of the Universe Summit, along the lines of last year's (see here). There's a web-site here, but about all you tell without a password is that the participants were staying at a very trendy hotel in West Hollywood.
- A film has been made about the geometer Shiing-shen Chern. The title is "Taking the Long View" and there's a web-site here.

Update: Two more.

- XKCD on extra dimensions.
- Matthew Chalmers has an interesting new article at Physics World entitled Reality check at the LHC.

Update: There's a review of the new Brian Greene book by George Ellis at Nature this week. It emphasizes the problem of lack of testability.

Comments

1. **Myke**
   January 17, 2011

   What do you think about Massive by Ian Sample?

   Myke.

2. **Kris Krogh**
   January 17, 2011

   Michel Berube's article does a good, honest job of putting Sokal's hoax into perspective for the broader academic community. When it comes to physics, I don't think it can hold a candle to Mara Beller's wonderful essay, The Sokal Hoax: At Whom Are We Laughing? A free version, without the George Gamow illustrations, is here.

3. **Peter Woit**
   January 17, 2011

   Myke,

   See

   http://www.math.columbia.edu/~woit/wordpress/?p=3266

4. **Peter G**
   January 17, 2011

   Back in '95 I interviewed Jean Bricmont about his book with Sokal for a leading newspaper in Belgium. I can assure you that he was very surprised by the fact that Sokal's hoax and Impostures Intellectuelles were described as some kind of 'war'. He had no intention to wage war against the humanities. For him, the book was more a Voltairian laugh.

   Michael Bérubé writes that "it turns out that the critique of scientific "objectivity" and the insistence on the inevitable "partiality" of knowledge can serve the purposes of climate-change deniers and young-Earth creationists quite nicely."

   That's a bit of revisionism. Already in the beginning of the 90s the "critique of scientific objectivity" was used by young-Earthers, evolution-doubters etc. to defend their views. I remember making a piece about it for a radio programme in '92 or '93. But I don't remember many people from the humanities taking a stance against it at that time. Bruno Latour's doubts ("Was I wrong to participate in the invention of this field known as science studies? Is it enough to say that we did not really mean what we meant?" etc.) came much later.

   There's a lot to be said about Impostures Intellectuelles and the hoax, but I think it was a necessary kick in the b!t.

5. **Peter G**
   January 17, 2011

   Mara Beller writes: "Yet physicists relate to Derrida's and Bohr's obscurities in fundamentally different ways: to Derrida's with contempt, to Bohr's with awe. Bohr's obscurity is attributed, time and again, to a "depth and subtlety" that mere mortals are not equipped to comprehend."

   That sound very far of the mark to me. I studied physics and I never ever heard a professional physicist attribute Bohr's obscurity to depth and subtlety etc. Quite to the contrary: all the physicists I know admitted that they didn't understand a word of Bohr's more esoterical writings and agreed that a good physicist can be a terrible philosopher. The man who thaught me QM once told me: "There's nothing more pitiful than a good scientist who turns into a bad philosopher."

6. **Paul**
   January 17, 2011

   This is kind of off topic, but why is Witten interested in Khovanov homology these days?

7. **Peter Woit**
   January 17, 2011

Paul,

The invariants in Chern-Simons-Witten theory are given by taking the Euler characteristic of Khovanov homology, so it's a generalization of the Chern-Simons story (which Witten won a Fields medal for). There's lots of reasons to be interested in the 3d Chern-Simons theory, and good reason to expect Khovanov homology to appear as part of an interesting 4d TQFT. Lots of people have tried to figure out what is going on here, and Witten has some new ideas, using path integrals over the complexification of the usual space. He ends up using yet another version of N=4 supersymmetric YM, the QFT that appears in geometric Langlands, as well as AdS/CFT. His paper with the details of how he gets Khovanov is not out yet. When it does come out, I'll probably try and spend some time with it, and write about the whole story here.

8. **Peter Woit**
January 17, 2011

Just after writing the above, I see that Witten's paper has appeared on the arXiv this evening. It's 147 pages long, so "spending time with it" may take a while….

See:

http://arxiv.org/abs/1101.3216

9. **Peter Woit**
January 17, 2011

Peter G and others,

Please stick to the Sokal/Berube topic….

10. **Peter Woit**
January 18, 2011

It seems that some unemployed guy in Pilsen who reads this blog thinks Brian Greene is my employer and is upset that Brian is not having me fired. For the record, my position as "Senior Lecturer" in the math department is not tenured, but I have a long-term contract and whether it gets renewed at some point in the distant future will have nothing to do with what Brian thinks about this blog, or with what I think about his books.

Actually, my impression is that if most string theorists could choose one well-known blog dealing with string theory to shut down, it wouldn't be this one…

11. **maros**
January 22, 2011

solve recent witten`s paper problem discussed here http://www.math.columbia.edu/~woit/wordpress/?p=104 ?

12. **Peter Woit**
January 22, 2011

Maros,

Witten' work does give an answer to that problem. Unfortunately it's a rather complicated one, and I'm still far from completely understanding it.

13. **Shantanu**
January 24, 2011

Peter, this maybe a bit off-topic, but have you discussed your concerns about string theory with Brian Greene and does he agree with you?
Also what about other particle theorists at Columbia? (I don't actually
who all work on string theory and who does not). What about Eric Weinberg? Does he agree with your pov?
Thanks

14. **Peter Woit**
January 24, 2011

Shantanu,

Brian and I tend mostly to discuss other things than string theory. If you look closely at what he says publicly and what I say, I think you'll find we don't disagree about any facts, just how we evaluate the likelihood that certain very speculative ideas will turn out to be successful.

I don't remember ever discussing string theory with Erick Weinberg, and don't want to speak for anyone over in the physics department here. But my impression is that it's accurate to characterize the Columbia department on average as not one that has ever been very enthusiastic about string theory.

News from Templeton and FQXi

January 18, 2011
Categories: Uncategorized

The Templeton Foundation has just released their "2010 Capabilities Report", a sort of bi-annual report. It shows that in 2009 they had assets of $1.5 billion, and spent $31.8 million on "Science and the Big Questions". For 2010 two of their funding priorities were Quantum Physics and the Nature of Reality and Foundational Questions in the Mathematical Sciences, but they have yet to report what grants they made in those categories.

The foundation is now being run by Jack Templeton, a surgeon who is devoting his efforts to spending his father's money according to his instructions. For a couple of recent articles explaining what is going on at Templeton these days from two very different perspectives, see God, Science and Philanthropy at the Nation, and Honoring his Father at World magazine. The Nation article reports that the Foundation should soon have $2.5 billion or more to spend as the father's estate is settled, and discusses how Jack Templeton's right-wing politics and the Foundation's goal of bringing science and religion together make many scientists uneasy. At the World on the other hand, they seem concerned that the Foundation is supporting the theory of evolution, for reasons that Jack Templeton spells out:

> Every five years, three independent analysts are to conduct a review to see if Jack Templeton (or his successor) is making grants consistent with Sir John's intent. If they find that Jack is giving 9 percent or more of the grants to causes inconsistent with paternal intent, he has one year to get back into line. If not, Jack and his top two officers will be fired.
>
> Nor can Foundation trustees make changes by themselves or choose new board members. Templeton family members, plus winners of the annual Templeton Prize, plus heads of several organizations Sir John respected (such as the Acton Institute) are honorary members: There are about 75 in all, and 95 percent of them must be in agreement for any substantive change in foundation goals and purposes to be made. Even to change the location of the board's annual meeting requires a 75 percent vote of the honorary members.
>
> The Foundation maintains Sir John's "core funding areas." The lead one, "Science & the Big Questions," includes questions about evolution. Other Templeton core areas are Character Development ("We can determine how to be the masters of our habits"), Exceptional Cognitive Talent & Genius (humans can be "helpers in the acceleration of divine creativity"), and Genetics (the Foundation is not yet accepting unsolicited proposals in that area). Jack Templeton would not discuss any differences from Sir John in those areas: His calling is to do the will of his father.
>
> The son clearly sees things the same way as his father in one other Core Area, Freedom & Free Enterprise. Jack recalls how Sir John "often spoke, year after year about 'people's capitalism' and what it would mean if the overwhelming majority of people in any country were shareholders themselves with the result that they would be much less likely to be envious and instead would focus much more persistently on 'the good of the whole.'

Besides science, Templeton has traditionally funded lots of activities related to religion, as well as ones promoting "Character Development" and "Freedom and Free Enterprise". Another core funding area is "Exceptional Cognitive Talent and Genius", where they try to identify and nurture "young people who demonstrate exceptional talent in mathematics and science." Their newest interest is in genetics, where they've just started to make grants, including one in support of the "Genetics of High Cognitive Abilities Consortium."

One of the Templeton Foundation's biggest grants, featured on the front-page of their web-site, was $8.8 million given to set up FQXi. They list this grant as having an end-date of December 2009, and the plan was for FQXi to get later funding elsewhere. FQXi is still in operation, either with leftover Templeton money or new funds from other sources. They announced today the award of $1.8 million dollars in grants for research into "The Nature of Time", based on this request for proposals, which asked for research "unlikely to be supported by conventional funding sources". The list of grants announced includes quite a few that satisfy that criterion, but winners also include some prominent theorists working on not exactly unconventional topics such as Andy Strominger on AdS space-time, Joe Polchinski on holography and AdS/CFT duality, Hiranya Peiris on analyzing WMAP data, and Berkeley's Raphael Bousso on the Multiverse (along these lines). Maybe the last one does qualify as "unlikely to be supported by conventional funding sources".

In further support of the cause of investigating the Nature of Time, FQXi will pay for an event entitled Setting Time Aright which will take attendees on a chartered cruise from Bergen to Copenhagen late this summer.

Comments

1. **id**
   January 19, 2011

   So – setting aside questions of 'is string theory science?' – does Templeton fund grants on Intelligent Design? Apparently not, back in 2007 –

   http://www.antievolution.org/cs/node/216

2. **QED**
   January 20, 2011

   what are some of the bigger research results to come out of the fqxi research?

3. **larry**
   January 22, 2011

   garrett lisi's e8

4. **larry**
   January 22, 2011

   i meant to write lee smolin's garret lisi juggernaut

5. **Peter Woit**
   January 22, 2011

   QED and larry,

   Garrett's initial work on E8 was done way before he got any funding from FQXi. If his newer work ends up being successful, I suppose FQXi should take some of the credit.

Maybe the relevant question though is to compare the results of FQXi funded research and DOE/NSF funded theoretical physics research. Neither has had much success in recent years, which you could take as due to the problems addressed being too hard, or to both of them funding the wrong things.

One area where FQXi beats DOE/NSF in terms of funding research results is in multiverse studies. In that case there's an argument to be had about whether more and bigger research results is a good thing. Does the world need big, high-impact pseudo-science results?

6. **QED**
January 22, 2011

well, garrett lisi may well be the next einstein as lee smolin proclaimed, and we may well live in a multiverse. both entities represent the pinnacle of fqxi's foundational research. and just like lisi did a lot of his research before the fqxi funds, so too was the multiverse conceived of and set forth before fqxi. 🙂

7. **QED**
January 22, 2011

Dear Peter,

After googlig some items this morning, it seems you are a fan of Garrett Lisi and Ed Witten, while you are not a huge fan of fqxi, the multiverse, and some string theorists' claims of the testability of their theory. Well, Ed Witten supports the the multiverse, and Lisi claims his theory can be tested, even though it cannot. Can you please elaborate on these inconsistencies?

Thanks for your time!

QED 🙂

8. **Peter Woit**
January 22, 2011

QED,

I guess I am a "fan" of Ed Witten's but no, I'm not a "fan" of Garrett Lisi. In any case though, in almost all cases I like some aspects of people's work, and not others.

In Witten's case, some of his work is just completely fantastic and far outclasses anything anyone else in the field has done (see my book for a chapter about this). But I also think he has made a mistake in not giving up in the face of things not working out on an idea (string theory unification) that he had some good reasons to get enthusiastic about back in 1984. He's in good company though, Einstein made the same sort of mistake. About the multiverse, several people have told me that he has been quite critical of the idea in private. His public statements on the subject are typically rather guarded. Recently he sometimes in public talks describes the string theory multiverse idea, then says something about how he's not yet convinced, but it might be true. I'd rather he adopt David Gross's full-throated denunciation.

About Lisi, he's obviously not in the same league as Witten. I'm sympathetic to some of the ideas he is working with and wish him well. What he's trying to do is worthwhile, but I don't think he's yet found a solution to the basic problems of the subject. He should be careful and precise in what he says in any claims of testability of his work. Caveats tend to get lost in the media.

Back to Templeton and FQXi, Witten has nothing to do with them as far as I know. One of my criticisms of the two organizations would be that they don't support research of the sort that Witten has had success with, at the intersection of mathematics and quantum field theory. Lisi's research is about as far as they go in that direction, and he seems to be somewhat of an anomaly in terms of what they support.

9. **Steve Dufourny**
January 31, 2011

Hi all,

Sorry for my english.I am belgian.
I am surprised.First FQXi is a wonderful platform where scientists can speak together in a total transparence.
For example ,I come from Belgium, I work about my theory of spherization, a GUT of spinning spheres.In resume,quantum spheres….cosmological spheres…UNIVERSAL SPHERE.
And FQXi has accepted my posts.There on FQXi you can see this year for ther contest, scientists from all over the world.It's wonderful.These scientists speak together in a total transparence.A real innovant platform, young with a fantastic and wonderful future.This net is revolutionnary indeed.
An other point dear scientists, the strings are purelly falses , a string is divisible, a sphere no.Mr Lisi and Mr Witten are falses simply in the whole and in the details.We can't play as we want with our constants, irreversibilities, coherences,….if you want really knowing FQXi….Read their threads or I have a better idea, you go there and we shall speak in total transparence together with our real name of course.
Dear Mr Woith, come on FQXi you shall be accepted and we shall speak.

On that

Best Regards

Steve

Number 999 or 1000

January 25, 2011
Categories: Uncategorized

According to the WordPress software, this is either post 999 or 1000 on this blog, depending on whether you count one I haven't gotten around to finishing. I'm not sure that number is reliable anyway, since there are various anomalies due to a long-ago transition from Movable Type to WordPress. Some other statistics: 27,089 approved comments since the beginning (March 2004), 84,259 spam comments since the latest spam filter was turned on a couple years ago, 510,530 page hits last month (mostly spam, robots), and 8,842 subscribers at Google Reader.

The blog has turned out to be far more of a success than I ever expected when it was first started. There were few similar blogs back then, with Jacques Distler's Musings having been around for a while, and Sean Carroll's *Preposterous Universe* just starting up. Lubos Motl's Reference Frame quickly followed, I gather somewhat in response to mine. These days, Musings seems to have gone dormant, but Sean and Lubos are still at it. I haven't kept track of physics blogs in general, but there are now quite a few that deal with particle physics in one way or another. For particle phenomenology, Resonaances is a great source of information, for experimental HEP Tommaso Dorigo has a wonderful blog, and Philip Gibbs at *viXra log* does a great job of keeping track of the state of the LHC (for the latest, see here).

There's also now a huge variety of research-level math blogs, including a very active blog by Fields Medalist Terry Tao. The new site Mathblogging.org is an exhaustive source of information and links. The big recent change in the math blogging world is the amazing phenomenon of MathOverflow, which features many of the best young mathematicians around carrying on the sort of conversations about research-level mathematics that traditionally go on in math common rooms. In an odd way, mathematics has often been somewhat of an oral tradition, since the impenetrability of much of the literature often meant that the only way to learn about something was to find an expert and get them to explain it to you. Now you can do this on-line, and this may significantly change how mathematics is done. Just as listening in to common room conversations was a great way to learn things, poking around the links on MathOverflow can provide quite an education. The moderation system somehow maintains a high level of discussion, although sometimes it lets its hair down to allow discussions like the ongoing one about Mathematical "Urban Legends". There one can learn that one's suspicions about string theorists educated at Princeton are correct, with Jeff Harvey contributing the following:

> Since the OP gave a physics example, here is another one, also at Princeton. Why are they always at Princeton? Student finishes his presentation on very mathematical aspects of string theory. An experimentalist on the committee asks him what he knows about the Higgs boson. He hems and haws and finally says "well, it was discovered a few years ago at Fermilab", Experimentalist: "Can you tell me the mass?" Student: "I think around 40 GeV."
>
> This was more than 20 years ago and actually happened. I was there. The student passed, but the next year all Ph.D students working on string theory were required to take a course on the phenomenology of particle physics.

There's now a physics version of mathoverflow starting up, but so far it seems to me much less successful, with far too much in the way of the high-school level topics and uninformed discussion that plagues most internet physics discussion forums. There are some examples of serious questions and well-informed people writing in, so maybe things will improve and it will turn into something very worthwhile, replacing much of what is now going on at blogs.

I'm surprised to still be doing this nearly seven years after starting, but now doesn't seem to be the time to stop. Particle theory has long been a rather intellectually dead topic, but whatever the news is from the LHC, it promises to shake the field up in one way or another, a process that should be interesting to follow. In coming weeks I may try and find time to learn some more about the features of WordPress, adding some features to the blog, or at least refreshing its rather tired look. Don't be surprised if its appearance starts to change, or at least become unstable…

Comments

1. **Noname**
   January 25, 2011

   "Particle theory has long been a rather intellectually dead topic"
   Ha, ha, ha… You mean to say the part of it you actually understand?

   [Initially, I deleted this, but finally decided to leave it. It does represent well part of the experience of blogging these past few years. No way to know who this is, other than that they're at CERN. Maybe it's the Princeton Ph.D. from Jeff Harvey's story….]

2. **Barbara**
   January 25, 2011

   I think you missed a detail. It's not just young mathematicians talking to each other on MathOverflow. In fact, I think its main strength is the range of users, going from grad students to retired professors.
   Other than that, it is totally awesome.

3. **Lee Brown Jr.**
   January 25, 2011

   Congratulations on #1000! Your blog has inspired me. I look forward to reading more.

4. **Chris Austin**
   January 25, 2011

   "In coming weeks I may try and find time to learn some more about the features of WordPress, adding some features to the blog, or at least refreshing its rather tired look."

   I hope it won't become necessary to download megabytes of superfluous data to read a post, as with some blogs. I find N.E.W. a very good source of quick information.

5. **El Cid**
   January 25, 2011

   "*Student finishes his presentation on very mathematical aspects of string theory. An experimentalist on the committee asks him what he knows about the Higgs boson. He hems and haws and finally says "well, it was discovered a few years ago at Fermilab", Experimentalist: "Can you tell me the mass?" Student: "I think around 40 GeV. … The student passed, but the next year all Ph.D students working on string theory were required to take a course on the phenomenology of particle physics."* "

   Peter, really do you think I am stupid? I can't believe it. Maybe, you think Princeton University is like Capillas's School.

6. **Justin Hilburn**
January 25, 2011

There is a proposal for a high level physics site on stack overflow. It would be mathoverflow to physics.SE's math.SE.

http://area51.stackexchange.com/proposals/23848/theoretical-physics

7. **Shantanu**
January 25, 2011

Peter and others, sci.physics.research is still good (even though
most scientists no longer post there).
I learned of many interesting papers and results (Which are usually
ignored in mainstream physics/astrophysics literature) through
that.

8. **D R Lunsford**
January 25, 2011

10 OPTION BASE 0

So 999=1000 anyway.

Happy anniversary

-drl

9. **nbutsomebody**
January 25, 2011

I agree that discussion of urban legends is a little diluting. However it is extremely entertaining, also for non-mathematicians.

10. **C. J. Mozzochi**
January 26, 2011

Keep up the good work, Peter!

11. **Domenic Denicola**
January 26, 2011

I came here to say exactly what Justin Hilburn said. Hopefully we'll have a good theoretical physics research site soon!

12. **Tim van Beek**
January 26, 2011

> Particle theory has long been a rather intellectually dead topic…

the most depressing feature among severy really depressing ones is – at least for me – that in the 21st century there is a need to discuss – with tenured physicists – what a "test" of a physical theory is, see e.g.

String Theory and the Real World by Gordon Kane.

This resets the whole topic to a pre-aristotelian level. Maybe we should just walk away and never look back…

13. **MO guy**
January 26, 2011

I'll have it known that if questions like the urban legends one become too common (called big-list questions), the NARQ (NotARealQuestion) squad will be there to MOp the floor with their smoldering remains (ooh, a mixed metaphor!).

14. **Chris Oakley**
January 26, 2011

1,000 posts? It seems like just yesterday when this blog and its followers were just a small (but mostly well-educated) group of malcontents and misfits … the days before you started filtering comments (what has happened to Quantoken, I wonder? – no, forget I asked). I thought that the principles of scientific enquiry were generally agreed upon, but a few hundred string theorists have proved me wrong. It is nice to have somewhere on the internet where they are nonetheless taken seriously (along with the basic rules of English grammar).

You were talking about improvements, for which I am grateful, especially since you are not being paid for any of this. One thing is the annoying "1y -12m" showing up in tooltips to express a recent time; another thing is that it would be quite useful if in the sidebar, or something, there was some explanation as to what is possible in terms of mathematical formulae, HTML, etc. in the comments section.

15. **Myke**
January 26, 2011

Peter nice work on reaching 1000, keep it up! Think very carefully before you change your blog's format! It works well and doesn't need fixing. Tired is perhaps a euphemism for lack of confidence, which shouldn't exist in considering your blog…

16. **Michael Gogins**
January 26, 2011

Thanks for your site. I'm not a scientist, but I enjoy reading about it and thinking about it. I've learned a lot from Not Even Wrong.

When I was a teenager, I wondered what would happen when physicists came up with theories so good they would very very hard to test. I had no idea that the Standard

Model, coming into existence at that time, was such a theory. But your blog and those who respond to it make me wonder even more…

What I wonder is this: if there is no practical way to test a theory, does this just sort of automatically turn people who in an earlier age would have been good empirical scientists into theologians and mythologists?

In an institutional and sociological sense, what can be done to enforce intersubjectivity?

Thanks,
Mike

17. **ohwilleke**
January 26, 2011

Congratulations! I particularly commend you for taking the opportunity to recognize other blogs rather than just your own at this milestone. My blog, started in July 2005, recently had its 5000th post and I can't claim to have been as gracious.

Your thousand post count also deserve special attention because you are in the camp of bloggers who post in multiple paragraphs, rather than single sentences or sentence fragments as many of the high post count bloggers do, something that makes your blog worth reading, especially in a relatively arcane field where analysis can add considerable insight.

The fact that your noise to signal ratio in the comments is 3-1 also indicates that a lot of people find this a worthwhile place to want their spam to be (I did a recent purge of years of comment spam and found that about one in ten of my comments was spam.)

Perhaps we should to the analysis of your posting timing that Résonaances did, which seems to indicate that the end of the blog (or maybe the end of the world or the end of particle physics) is due sometime in 2012, per the Mayan calendar. 😉 After all, if it is true at enough physics blogs, the sigma might get low enough to make it publication worthy as a metatheory of physics.

18. **Mean and Anomalous**
January 26, 2011

Thanks for the blog, and thanks for the book; keep it up.

19. **Jeff McGowan**
January 26, 2011

Congrats on the longevity Peter, fun to check in to the physics world once in a while. As for mathoverflow, great resource, funny story. I was at a math conference in the fall, and we were talking about it. One of Thurston's students, very well known person, ton of students etc., was saying how great it was that Thurston had been following/posting on it, but that it got kind of depressing because he would read Thurston's posts and just think "why do I even bother?"

20. **John Baez**
January 26, 2011

Congratulations on your 999.5±0.5th post, Peter!

Michael Goggins wrote:

> What I wonder is this: if there is no practical way to test a theory, does this just sort of automatically turn people who in an earlier age would have been good empirical scientists into theologians and mythologists?

Another option is for them to become mathematical physicists: that is, to study the theory as rigorously as possible. In the absence of experimental evidence, this another way to keep from "playing tennis with the net down". Witten is the most famous example of someone who has taken this tack.

21. **Arun**
January 27, 2011

Congrats on the 1000th post (at the 5sigma confidence level!) Here's to the next 1000! Will the Higgs have been discovered by #2000?

22. **Curious Wavefunction**
January 27, 2011

Thanks for the fish. Your blog and book have been thoroughly enjoyable and I have learnt a lot of interesting facts from them. One thing that I wish you would do more often is write book reviews since I really like them.

23. **Shantanu**
February 5, 2011

Peter and others,
I am still concerned that the only branch of experimental particle physics in which there has been a lot of progress since 1998 (viz neutrino physics) makes absolutely no connection with any theory or hyped extensions to standard model(string theory, ADS/CFT. supersymmetry etc).
See this webpage for quotes by various physicsts (including string theorists such as Pierre Ramond after super-k announced result for non-0 neutrino mass.
Maybe if string theorists are reading this, they could point out what will theta_13 be?

News From Chamonix

January 28, 2011
Categories: Experimental HEP News

The people responsible for the LHC are meeting in Chamonix this week to make plans for the upcoming run, slides of many talks are available here. The results of discussions there are:

- The recommendation will be to run at 3.5 TeV/beam, not increasing to 4 TeV/beam as widely expected.
- The long shutdown to fix splices and allow going to 7 TeV/beam will be delayed until 2013, with 2012 devoted to a physics run. The 2013 shutdown will likely last more than a year.
- Officially, the integrated luminosity goal for 2011 remains at 1 inverse femtobarn. However, unofficially, they expect to be able to do at least twice this, ending up with 2-3 inverse femtobarns by the end of the year

For a discussion of the possible physics that can be done with these parameters, see here. By the end of 2011 the LHC should be able to do better than the Tevatron on the Higgs search over most of the possible mass range, except for the low end of the range, where higher energy isn't much help, and the Tevatron's more than 10 inverse femtobarns and longer experience with the data analysis may give them an edge.

Comments

1. **Anon**
   January 28, 2011

   Particularly noteworthy is the plot on slide 19 of the talk linked above – the first public limits on SUSY from ATLAS, obtained from the same data-taking period as the CMS plot on the previous page. Results from the analysis of other channels still to come …

2. **Tommaso**
   January 28, 2011

   Hi Peter,

   I think CMS and ATLAS will actually win the race even in the range of masses where the Tevatron is advantaged. This stems from the recent re-evaluation of sensitivity performed by the two experiments. Also note that ATLAS and CMS might in principle decide to combine their limits or significances, something that the Tevatron does systematically.

   A few pictures are worth thousands of words: see the CMS expected 3-sigma reach and 95% exclusion reach in 2011 for some scenarios here.

   As you can see, 2/fb at 7 TeV will still allow to reach 3-sigma in a wide range of masses (130 GeV onwards), or to exclude a region down to 120 GeV or so at 95% CL. Combining with ATLAS, which has a similar sensitivity, will do the rest.

   Cheers,
   T.

3. **hg**
   January 31, 2011

   In all the talk about searching for the Higgs at the LHC, what happened to the LEP signal for a Higgs at 114 GeV?

4. **Rhys**
   January 31, 2011

   hg, nothing 'happened to it'. It's still talked about, and many people (especially those of us keen on supersymmetry) think the Higgs is probably quite light, by which I mean in the 110-130 GeV range.

5. **Peter Woit**
   January 31, 2011

   hg,

   I believe that after final analysis, the LEP "Higgs signal" was about 2 sigma, nothing to get excited about. Hopefully though within the next year or two we'll find out whether there was really something there. It's interesting that that Higgs mass is about the worst possible one for the LHC to try and see. Maybe they'll end their 2012 run with a 2 sigma signal….

Is the Multiverse Immoral?

January 29, 2011
Categories: Multiverse Mania

In the last week or so, I've run into two critiques of the currently fashionable multiverse mania that take an unusual angle on the subject, raising the question of the "morality" of the subject. The first of these was from Lee Smolin, who was here in New York last week talking at the Rubin Museum. I probably won't get this quite right, but from what I remember he said that discussions of a multiverse containing infinite numbers of copies of ourselves behaving slightly differently made him uneasy for moral reasons. The worry is that one might be led to stop caring that much about the implications of one's actions. After all, whatever mistake you make, in some other infinite number of universes, you didn't do it.

Over at Scientific American, yesterday they had John Horgan's Is speculation in multiverses as immoral as speculation in subprime mortgages?. There's more about this in a Bloggingheads conversation today with George Johnson, where Horgan describes his current reaction to multiverse mania as "I can't stand this shit."

I'm in agreement with Horgan there, but my own moral concerns about the issue are different than the ones he and Smolin describe. The morality of how people choose to live their everyday lives doesn't seem to me to have much to do with whatever the global structure of the universe might be. The world we are rapidly approaching in which a multiverse is held up as an integral part of the modern scientific world view isn't one in which many people are likely to behave differently than before, so I don't share Smolin's concerns. Horgan's exasperation with seeing the multiverse heavily promoted by famous physicists appears to have more to do with the idea that this is a retreat by physicists from engagement with the real world, something morally obtuse in an era of growing problems that scientists could help address. For what he would like to see instead, I guess a good model would be John Baez's recent decision to turn his talents towards real-world problems facing humanity, see his blog Azimuth for more about this. Personally, I'm not uncomfortable with the fact that many mathematicians and physicists find that they don't feel they are likely to be of much help if they go to work on the technology and science surrounding social problems. Instead, one can reasonably decide that one has some hope of making progress on fundamental issues in mathematics or physics and choose to work on that instead. One can try and justify this by hoping that new breakthroughs will somehow, someday help humanity, although this may be wishful thinking. Or one can argue that working towards a better understanding of the universe is inherently worthwhile, so pursuing this while taking some care to avoid worsening one's local corner of the world is a morally reasonable stance.

My own moral concerns about the multiverse have more to do with worry that pseudo-science is being heavily promoted to the public, leading to the danger that it will ultimately take over from science, first in the field of fundamental physics, then perhaps spreading to others. This concern is somewhat like the one that induced Alan Sokal to engage in his famous hoax. He felt that abandonment by prominent academics of the Enlightenment ideals exemplified by the scientific method threatens a move into a new Dark Ages, where power dominates over truth. Unfortunately, I don't think that revelation of a hoax paper would have much effect in multiverse studies, where some of the literature has already moved beyond the point where parody is possible.

For a while I was trying to keep track of multiverse-promoting books, and writing denunciatory reviews here. They've been appearing regularly for quite a few years now, with increasing frequency. Some typical examples that come to mind are Kaku's Parallel Worlds (2004), Susskind's The Cosmic Landscape (2005), and Vilenkin's Many Worlds in One (2006). Just the past year has seen Sean Carroll's From Eternity to Here, John Gribbin's In Search of the Multiverse, Hawking and Mlodinow's The Grand Design, and Brian Greene's new The Hidden Reality. In a couple weeks there will be Steven Manly's Visions of the Multiverse. Accompanying the flood of books is a much larger number of magazine articles and TV programs.

Several months ago a masochistic publisher sent me a copy of Gribbin's book hoping that I might give it some attention on the blog, but I didn't have the heart to write anything. There's nothing original in such books and thus nothing new to be said about why they are pseudo-science. The increasing number of them is just depressing and discouraging. More depressing still are the often laudatory reviews that these things are getting, often from prominent scientists who should know better. For a recent example, see Weinberg's new review of Hawking/Mlodinow in the New York Review of Books.

While most of the physicists and mathematicians I talk to tend towards the Horgan "I can't stand this shit" point of view on the multiverse, David Gross is about the only prominent theorist I can think of known to publicly take a similar stand. One of the lessons of superstring theory unification is that if a wrong idea is promoted for enough years, it gets into the textbooks and becomes part of the conventional wisdom about how the world works. This process is now well underway with multiverse pseudo-science, as some theorists who should know better choose to heavily promote it, and others abdicate their responsibility to fight pseudo-science as it gains traction in their field.

Comments

1. **Kea**
   January 29, 2011

   After I (the one copy of me) was physically denied entry to a multiverse conference in Oxford in 2009, I had a good laugh about it with a friendly stringer. Then he told me that the conference was secret so that the participants could debate (in secret) whether or not the multiverse was science. It seems they have still not made up their minds …

2. **John Romeo Alpha**
   January 29, 2011

   The universe we inhabit exhibits immorality in a narrow range, defined by the constant I, which is just immoral enough to allow for the possibility of free will, but not so immoral that we annihilated ourselves the moment we gained the technology to do so. Less immoral, and the inhabitants of the universe would bore themselves into petrified non-existence, more immorality, and they vanish in a flash of look-what-we-can-do. I present this as irrefutable evidence that Erdős's SF has planted us in the only or single universe with just the right amount of I to support intelligent life, teetering between determinism and destruction.

3. **Friend**
   January 29, 2011

   I think that multiverses are a misinterpretation of the Path Integral used in QFT, etc. Instead of it predicting the actual existence of alternative paths/universes, it really predicts that it takes ALL possibilities to make just one universe. Thus it is impossible for multiverses to exist.

   However, physics may have something to say about morality. Moral belief systems do consider cause and effect of deeds, which are actual physical events. Perhaps such belief systems can be considered to have some structure and entropy associated with them. Then it might be that maximum entropy limit theorems prevent instant dissipation of those structures so that they do have some physical effect. But then how would one measure belief?

4. **John Baez**
   January 29, 2011

   Maybe a branch of science is ripe for infection by pseudoscience whenever it stops making enough progress to satisfy the people in that field: as a substitute for real progress, they'll be tempted to turn to fake progress. One could expect this tendency to be proportional to the loftiness of the goals the field has set for itself… and to the difficulty its practitioners have in switching to nearby fields that are making more progress.

But is this really true? Does anyone know other examples, beside the current situation in fundamental physics?

5. **Shantanu**
January 29, 2011

Peter, there are many other critics of multi-verse, although they may no be very vocal. some other examples of critics include Steinhardt, Turok, Woodard, Polyakov, Krauss, Turner, Strominger (which you pointed out)

6. **Friend**
January 29, 2011

John Baez, how about the financial market;-) Unsatisfied with economic progress, they've invented extravagant financial theories of prime-lending rates and complicated security instruments. Funny, I've heard that some physicists have found work in the financial industry. Perhaps their theories work in some other universe.

7. **Casey Leedom**
January 30, 2011

I hesitate to offer a serious response here but …

As far as I've heard, all of the promoters of multiverses are using them to explain the "current universe" as just "an" arrangement of a bunch of fundamental parameters of much larger theories. But at the same time, also saying that all of the other universes are beyond our horizon. (The motivation for which seems to be to get out of the hole created by theories which have high degrees of freedom.)

The proposed multiverse models don't appear to offer us any ability to make predictions about this universe. Thus this doesn't give us any more insight into how this universe works.

So beliefe in this multiverse story is at best an amusing diversion along the lines of worrying about the number of angels which can dance on the head of a pin. At worst, that diversion gets in the way of people actually trying to understand what we see around us. Amoral? Meh.

I say: "Where's the beef?"

8. **Andrew L.**
January 30, 2011

This argument shows how desperately insecure these people are from a purely empirical viewpoint. They know there's no more empirical substance to thier grandoise hypotheses then that host of MIT engineering students who famously picketed a science fiction convention with a 200 page mathematical "proof" that Larry Niven's Ringworld was unstable. They HAVE to know-they're PHD's in physics,for heaven's sake! The problem is if they let people look behind the curtain,75% of the current funding in theoretical physics and mathematics in the Western World will vanish in a flash of insight. It's rather sad,truth be told.

9. **Tim van Beek**
January 30, 2011

Peter said:

> For what he would like to see instead, I guess a good model would be John Baez's recent decision to turn his talents towards real-world problems facing humanity…

Well, John switched to "real-world problems" from a topic that was and is seeing a lot of progress (n-categories), the switch from quantum gravity was earlier.

John said:

> Does anyone know other examples, beside the current situation in fundamental physics?

Well there are a lot of topics in fundamental physics that make a lot of progress, even in theoretical high energy physics. Besides that, I do think that the situation in string theory is quite unique:

a) Physicsists that spend their time at their desk, thinking strange thoughts, can become global celebrities, this phenomenon did not exist before the 20th century,

b) the growth of the scientific community led to the formation of a subgroup of a size so big that its members became unable to see beyond it. This, too, is new.

c) There is this unfortunate tendency that at least some physicists think that they are the smartest people on the planet and that everybody who does not appreciate what they do is simply unable to understand it. (Most of these guys are also smart enough to keep these thoughts to themselves, for which I am very thankful.)

To elaborate on the second point: An active researcher monitors the work of his collegues, how many may the average researcher be able to follow? 200? If a research community becomes too big, it usually splits into more specialized subgroups such that its members stand a chance to stay up to date with respect to the work of their collegues, and have time to communicate with their neighbors. It would seem that this did not work well for the string community.

Why do I think this? Because I heard a lot of criticism of string theory way before Peter published his book, from all kind of tenured and not-tenured physicists, both experimentalists and theorists (all outsiders to the string community, of course). Sometimes voiced with very unusual aggression. Yet most members of the string community seemed to be both completely oblivious of this (maybe not anymore), and immune to any criticism from outsiders.

Is there something we can learn from this? I don't know. Maybe funding should be cut in a way that never more than 100 people do research on the very same topic.

10. **Myke**
January 30, 2011

Friend restates Feynman's view, which seems better to me than the multiverse nonsense! Besides, just look up on a clear night and see what is a 'multiverse' of galaxies. The final theory might well show that the mix of galaxies is the 'multiverse' by another description. John's pseudoscience (as judged by his crackpot index) seems to have its epitome in the ramblings of Tomas Campbell, through his big toe, and multiverse mania seems just a small step behind…

11. **Bee**
January 30, 2011

Well, one may like or dislike the multiverse for scientific reasons, but there's no denying it's interesting from a philosophical point of view and it's a fairly new idea, so I don't find it very surprising it's getting its share of attention. Whether that's helpful for physics is a completely different question.

One shouldn't worry about the multimoraliverse, one just has to postulate that our universe is finetuned for moral. It has to be, because otherwise your blog wouldn't be here to discuss the issue 😉

In any case, the question of moral is moot because there's no such thing as free will, and what is moral if your decisions were predetermined already before you were born anyway? Now imagine what's going to happen if that became a popular concept. Then you should be worried…

12. **John Baez**
    January 30, 2011

    I wrote:

    Maybe a branch of science is ripe for infection by pseudoscience whenever it stops making enough progress to satisfy the people in that field But is this really true? Does anyone know other examples, beside the current situation in fundamental physics?

    Friend wrote:

    John Baez, how about the financial market;-)

    That's a nice analogy because it seems to have been caused by a desperate search for "high rates of return".

    But I'd really like examples of branches of science that "sank into pseudoscience" as certain portions of fundamental physics seem to be doing now. Or is this a historic first? That seems hard to believe.

13. **John Baez**
    January 30, 2011

    This just in: Brian Greene is interviewed by Terry Gross on her show "Fresh Air": A physicist explains why parallel universes may exist.

    By the way, just to lay my cards on the table: I think that parallel universes *do* exist. I just don't think they're worth writing papers about.

14. **simplicio**
    January 30, 2011

    Was Smolin discussing the multiverse from Sting Theory or the one from the Many Worlds interpretation? I thought only the latter had the "slightly different versions of yourself making slightly different decisions", but Peter's rant seems directed at the former.

    (also, do people that hold with both ideas think each universe from String Theory has its own near-inifinte set of "Many Worlds" universe. thats a lot of universes! I wonder if William of Occam exists in all of them)

15. **Anton Tykhyy**
    January 30, 2011

    @John Baez 8:45 — good insight!
    As a recent and important example, may I offer paleoclimatology? Also teaching methodology (?) — there are reams of research on how to teach children the three R's, but young adults have been getting progressively worse at them for decades. Philosophy, e.g. philosophy of science, also appears to offer many stellar examples and has been doing so at least since Kant's remark that

    > I do not wish to hide the fact that I can only look with repugnance … upon the puffed-up pretentiousness of all these volumes filled with wisdom, such as are fashionable nowadays. For I am fully satisfied that … the accepted methods must endlessly increase these follies and blunders, and that even the complete annihilation of all these fanciful achievements could not possibly be as harmful as this fictitious science with its accursed fertility.

16. **Peter Woit**
    January 30, 2011

    simplicio,

    I don't actually know which version of the multiverse Smolin had in mind. One of the things that annoys me about the subject is that discussions of it typically involve conflating all sorts of completely different ideas (it's not that Smolin was intentionally doing this, as some do, it's just that's the context in which these discussions are often held).

    You're right that my main problem is with the use of this to avoid the implications of the failure of string theory unification. Unfortunately I think that's the main motivation for some very smart people taking up and promoting a dumb idea.

17. **Giotis**
    January 30, 2011

    This is the main thing I don't like about Brian Greene's new book. He presumably mixes various notions of the multiverse together with some exotic concepts. This I think would obscure the multiverse idea derived from string theory and eternal inflation. This idea has strong theoretical support and it's not some wild ungrounded interpretation of QM. But let's wait to read the book first…

    Regarding Peter's commetns I think he forgets the main motivation for the introduction of the multiverse.

    With the observation of the small CC physicists were facing a big problem with the apparent fine tuning of the universe. It seemed indeed that the universe was fine tuned for life. Trying to make sense of this weird observation they found that the degeneracy of string vacua combined with eternal inflation give rise to the multiverse concept where such a extraordinary fine tuning (and the implied intelligent design) is not necessary. What's wrong with that? People are trying to give a theoretical scientific explanation to an observation. This is what science always does.

18. **Thingumbobesquire**
    January 30, 2011

This question of morality really belongs to the department of metaphysics. Leibniz soundly and cogently rejected the worldview that underlies the multiverse some 300 years ago. The rank pessimism of Voltaire's Candide as a satirical response is echoed by today's widely held "scientific" tenet that the fundamental law of the universe is irrationality. We meet this type of rather flagrant pessimism in this statement from Hawking: "But our genetic code still carries the selfish and aggressive instincts that were of survival advantage in the past."

19. **Peter Woit**
January 30, 2011

Giotis,

I'm not likely to forget the anthropic multiverse explanation of the CC. It's the only "evidence" for the multiverse, explained over and over and over again by those promoting it.

The problem is that I'm just not convinced. The string theory + eternal inflation + anthropic tautology pseudo-scientific ideology makes exactly the same predictions for observable physics as my personal theory of the CC (which is that I have absolutely no idea where it comes from, so its a priori probability distribution is flat).

And to some extent I agree with John Baez. Maybe there are parallel universes. But if you're going to write papers about them and promote the idea to the public as science you need to follow the scientific method. This means having a real theory you can calculate with, one that makes distinctive, falsifiable predictions. Right now, the "theory"being promoted is just a complex mess of failed ideas that "predicts" that a priori any value of the CC is equally likely.

20. **Joel Rice**
January 30, 2011

Giotis indicates that it looks like a Darwinian backlash against an implied 'intelligent design' and thus fine tuning might be irrelevant. But why should fine tuning be assumed due to intelligent design ? Both fine tuning and intelligent design may well be irrelevant. Suppose the design of the world requires the construction of atoms – the association of building blocks – how would one have such a nested hierarchy without the effect looking fine-tuned ? One might ask – do forces explain atoms, or do atomic structures determine the forces ? Normally one thinks forces explain – but maybe we have it all backwards, or upside down.

21. **Frank Quednau**
January 30, 2011

Is there actually an awareness of how we can move on with proper science? Which real world measurable phenomena are currently unexplained and require new models / methods ? What could become / falsifiable possible if we obtain an overarching grand theory?

22. **Emanuel Derman**
January 30, 2011

Re John Baez's post:
Maybe a branch of science is ripe for infection by pseudoscience whenever it stops making enough progress to satisfy the people in that field But is this really true? Does anyone know other examples, beside the current situation in fundamental physics?

The Efficient Market Model and the tendency of financial theory to become irresolutely axiomatic irrespective of whether the axioms describe the world seems a reasonable example.

In physics it used to be fairly easy to tell the crackpots
from the experts by the content of their writings, without having to know their academic pedigrees. In finance, as in nutrition, it's not easy at all. Perhaps physics is going that way too.

23. **Anton Tykhyy**
January 30, 2011

@Frank: that's just the problem. In fundamental physics, there is precious little to chew on — SM has enough free or underconstrained parameters to fit available experimental data; BB cosmology also rehashes the same few datasets — microwave background, elements' abundances etc. It appears that either we have entered the age of diminishing returns on research effort, or (and?) research has self-focused into areas which have rather small measure in the idea space. But whereas 200 years ago it was within the powers of a single individual of independent means to master the available data, theories and methods, come up with new ideas and test them, today it is nearly impossible for multiple interacting reasons.

24. **Arun**
January 30, 2011

In reply to John Baez:

Paul Krugman calls what has befallen on macroeconomics "The Great Ignorance".

http://krugman.blogs.nytimes.com/2009/06/26/a-thought-about-macroeconomics/

Not exactly the same phenomenon as certain portions of fundamental physics. But the lapse into pseudoscience is probably rare enough that the common causes, if any, are hard to perceive.

25. **Peter Woit**
January 30, 2011

Frank,

It's very well understood by people in the field exactly what the open problems are, as well as what a convincing solution to any of them would look like. These problems are hard though and have resisted solution for a long time. One reason for this is that much of the last 25 years the field has been dominated by investigation of one set of speculative ideas that don't work. The continuing problem is that instead of admitting this, prominent physicists go to the public with claims that these ideas do work, it's just that the "multiverse" makes them impossible to test. Thus my shared feelings with John Horgan…

26. **Chris Austin**
January 30, 2011

In my experience, the "moral" impact of taking the Everett-Wheeler "many worlds" picture seriously is the opposite of what Smolin suggests: you start becoming concerned about what happened on other paths. If by chance you narrowly escape a serious accident, you realize, "On another Everett path I got killed just then," which can be a frightening thought. And more generally you find yourself thinking, “What would have happened if … ,” and trying to think through increasing numbers of diverging paths, which can become a serious burden. Nowadays I try to follow a principle, “Make every path, including this one, as good as it possibly can be,” and not to concern myself about other paths.

27. **Low Math, Meekly Interacting**
January 30, 2011

While the observation of putative dark flow, and the proposed observation of evidence of bubble nucleation in the CMB intrigue me, what has been true of multiverse speculation appears to remain true. Specifically, I see no practical evidence that the field depends crucially on empirical observations of any kind, nor that it will do anything but continue indefinitely regardless of whether or not hints of other “universes” are ever seen beyond the chalkboard.

Regardless of what value judgement one puts on this state of affairs (I lean strongly toward the negative), there’s no question it’s a paradigm shift that we’re well in the midst of. “Science”, like it or not, has clearly changed. Whether one wishes to call the new era pre- or post-Baconian, empiricism appears now to be too quaint for a core discipline. It pioneered the scientific and technological successes of the modern era, and now a new course is being charted, for good or ill.

We’re past the point of arguing against it. I find that extremely sobering, to say the least.

28. **Yatima**
January 30, 2011

“Paul Krugman calls what has befallen on macroeconomics … ”

Oh…pullease. Not Paul Krugman; he always reminds me of the poor court alchemist trying to justify why repeated attempts to generate gold out of lesser materials has so far met with complete failure lest the well-funded speaking occasions dry up; sorry I meant to say lest the royal executioner pay a visit.

Keynesianism – Multiverse Theory for the economic profession. And look where it has lead us to.

29. **Peter Woit**
January 30, 2011

Please, informed analogies of economic to physical theory are welcome, but arguments over which economic ideology is best of the same quality as typical arguments over string theory and the multiverse aren’t.

30. **Fred Zarguna**
January 30, 2011

Tipler essentially made this argument going on seventeen years ago in *The Physcis of Immortality* wherein he claimed to have neatly disposed of the Problem of Evil by virtue of the Many Worlds interpretation. Possibly he was correct: unfortunately, if Many Worlds describes actual worlds it also does away with the “Problem of Good,” which most theologins and ethicists never saw as a problem in the first place.

Content yourself with an update to an ancient aphorism: “Sufficient to each path is the evil thereof.”

31. **Bugsy**
January 30, 2011

I am confused by what role a probability distribution is supposed to play (or not) in both of two “multiverse” ideas- the Many Worlds one and the recent CC one. If there is a probability measure on some uncountable set (like the unit interval) then one can think of “all” events (points) occurring, with the one observed (in this case, inhabited by us) distributed in that way- but with the other points also existing. Alternatively, only one occurs (the one we are in). For example, coin-tossing can be modeled by Lebesgue measure on the unit interval with the binary expansion of each point giving a sequence of 0’s and 1’s; alternatively you imagine a single randomly chosen infinite sequence, almost surely with the right statistics. Regarding “MW”, if different worlds are supposed to constantly branch off, then that is severely problematic to model, in either case. One could perhaps imagine branching occurring at all times something like the
direction changes of a Brownian motion path, but with only one path surviving and actually existing, like the coin-toss sample path. Ok, but if it is splitting at every moment at every location…???

And regarding “CC”, if these other universes “actually exist somewhere else” then where? There isn’t enough room in any n-space for uncountably many worlds. So maybe it is envisioned as a
fiber bundle over the probability space. But is any case is sounds very difficult to make any real sense of, to say the least….

32. **Brendan**
January 30, 2011

Peter, don’t discount the degree to which fundamental physics, and our collective understanding of it, inform culture. People do live their lives according to how they see the world. When I was a kid “radical!” was common slang. Today, “random!” is a more common exclamation. I think these are both examples of pop culture tracking science trends. So, I don’t think it’s out of line to consider the moral implications of a particular theory. But it’s an article in Rolling Stone at best. The outcome of considering the possible moral implications of a theory shouldn’t interfere with primary research. But the underlying portrayal and execution of “philosophy of science” is a worry here. I don’t like to think of the message being sent to kids when we have major theories which aren’t subject to much Popperian gravity – just floating around out there in the ether, waiting for the rest of us to have faith that “we’re on the right track, you’ll see. Just keep funding x…”.

I also agree that both finance and physics are “ripe for infection” There’s a lot of energy and diversity of opinion in physics right now. The pent up urst for a true discovery in physics is probably driving some unprotected research. But I’d point to the oft pooh-pooh’ed idea of “the edge of chaos” to suggest that that might not be a bad thing. I think both physics and finance are sailing close to the wind. In sailing terms, you can win the race sailing close to the wind, but sail too high and you luff the sails, you lose the race. Whether I’m comparing science to a boat race, or randy adolescent boys, the importance lies in competition and having a some “grown-ups” around to establish the boundaries.

33. **milkshake**
January 31, 2011

Examples for John Baez: My impression is that wishful thinking and pseudoscience have played an important role in psychiatry, psychology and management theory.

34. **Casey Leedom**
January 31, 2011

Let me be more brief.

I don't care if there are multiple universes or not. I care so little that I'll even grant it to the proponents just to get them to shut up.

And then I say: what about _this_ universes? Now that you've had your fun dodging the hard problems, can we get back to making concrete predictions about what we _can_ see?

35. **Joe Bob**
January 31, 2011

"But is this really true? Does anyone know other examples, beside the current situation in fundamental physics?"

Probably one can see things such as the research fraud committed by Marc Hauser as similar (this related to cognition in monkeys). In the biosciences there is more money and more opportunity for popular exposure, and so the degeneration is even greater, into outright fraud. Or, perhaps, the difference is that the fraud can be debunked, whereas metaphysical quasi-religious speculations are inherently more difficult to counteract.

Looking back to the nineteenth century, perhaps the degeneration into social darwinism was a similar phenomenon.

36. **Bee**
January 31, 2011

Regarding the question what infected economics, you might find this amusing:
http://www.nytimes.com/2011/01/31/opinion/l31atom.html

37. **John Benavides**
January 31, 2011

Dear Peter

It would be interesting to make a post to expose in a detailed way your objections about the multiverse, pointing the difference between the many world interpretation or the consistent histories approach to quantum mechanics (for example I would like to know what do you think about Deutsch's work and Omnès' ideas, where I think there is lot of ideas that should be take seriously), and the inflationary and string theory's multiverse (where I think there is a lot of pseudo science)

38. **Frank Quednau**
January 31, 2011

I think that a popular science book outlining the currently open subjects in physics would be a very nice subject that would be quite refreshing from the bold books that seem to be pouring out these days.

Such a book is obviously subjected to a high risk of becoming obselete quickly, but it would be of great service to the public. Or maybe there is one still accurate enough? Or maybe I am a minority…

39. **SteveB**
January 31, 2011

I listened to Brian Greene's interview on Fresh Air last week. Brian was reasonably fair about mentioning the speculative nature of his arguments and that many disagree with his ideas. What came across was his passion for the beauty of the mathematical outcomes that come from extrapolating from other mathematical theories that come from the Standard Model (QFT) which is itself a mathematical predictive tool that attempts to describe the real world.

And that is where I see the problem. Mathematics is a wonderful, beautiful endeavour. Extending mathematical knowledge is a great and worthy career choice. But as I see it, mathematics in Physics is a tool that is used to try to understand the real world and not an end in itself. While extending the mathematical tools used in Physics can lead to new insights and discoveries, they can also lead to nowhere (in the real world). I think Greene is mistaken that some beautiful mathematical result makes for a plausible theory of physics. Maybe Dirac did it that way once, but that does not prove it to be the best method to use. GR came from the elevator/gravity gedanken experiments and not from the Math.

As others have mentioned, one can argue that the scientific method itself is being abandoned when one goes the routes that have lead to the current multiverse ideas. I do not support that.

40. **Peter Woit**
January 31, 2011

Bee,

Thanks, that's great, I hadn't seen it. Maybe the argument that particle physicists need to be kept busy or they'll bring down the financial system will help get funding for the subject.

John,

As far as I can tell, the string theory multiverse and the many-worlds multiverse have nothing much to do with each other. The string theory multiverse I understand, and it's clearly pseudo-science. QM interpretational issues are something I'm just not expert in (I know basically nothing about the work of Deutsch or Omnes), so mostly try and avoid comment on. To the extent I have tried to follow the subject, there seem to be very interesting issues there, with most writers ignoring them and devoting themselves to arguing about things that are not interesting (at least to me…).

Frank,

Well, I did write a book with a chapter devoted to this (see chapters 8 and 9 of NEW).

SteveB,

I strongly object to blaming the multiverse on the pursuit of mathematical sophistication and beauty in physics. Most multiverse papers use nothing but high-school mathematics. The "string theory vacuum" constructions that motivate this are just hideously complex and ugly beyond belief. Anyone looking for mathematical beauty would give up on this line of research immediately after looking at these things. The "derivation" of these things from some supposedly beautiful "M-theory" suffers from all sorts of problems, not least of which is that no one knows what "M-theory" actually is, making claims for its supposed beauty to be taken with an immense grain of salt.

41. **Paul A. Houle**

January 31, 2011

Physics has been on the wrong side of the rabbit hole since the 1980's or so. The fundamental problem is that there's very little going on physics that grabs the public imagination, so science fiction starts to move in.

(You're just not going to get the public excited about, say, Spin Glasses)

I think of the way the Bell Inequality got popularized in the 1980's. It's real science, but it's a much less profound expression of quantum entanglement than the way that fermion character makes solid matter possible. And if you start taking it too seriously pretty soon you're Jack Safaratti or Clifford Stoll.

In the time I was in grad school, quantum gravity went from a holy grail to an embarrassment of riches: people invent new theories of quantum gravity every day, theories in which it's actually possible to calculate something. Now, "possible to calculate something" means unitarity, and unitarity means "no information loss in a black hole" which means that the 1970's classical picture of a black hole interior is obviously wrong, but on some level the study of black hole interiors is futile since we're never going to send a probe into a black hole that sends back pictures.

Cosmology has always been linked with concerns that are essentially theological. Einstein found an expanding universe distasteful, so he added a cosmological constant to keep it from expanding. Once we learned the universe was expanding, many cosmologists believed in a steady state universe that had neither beginning nor end, rather a process of continuous creation that creates new hydrogen in the spaces between the stars.

Then the CMB was discovered and it became clear that the universe must have been much smaller and very different in the past. Although cosmologists had little evidence that the universe was either closed or open, it seemed like most of them believed that the universe was closed (How could you have a beginning without an end?) until inflation came on the scene, and pointed to a universe that's exactly flat.

Today the multiverse, true or not, reflects a theological preference for 'continuous creation'. It's something that many of us (myself included) find emotionally appealing… in the same sense that some people find it emotionally appealing to believe that God created the world 6000 or so years ago.

42. **Joel Rice**
January 31, 2011

Peter, regarding the idea that it is well understood what the open problems are and what a convincing solution would look like, well, I often wonder if the present situation is not similar to the state physics was in just before Spin finally made sense. There was no lack of ideas, just that none of them actually worked. And Spin was a limited problem. And the answer was in quaternions. It appears that Nature is really good at making a lot with a little. The problem with this is that subtlety can be as big a problem as just being complicated. All of today's ideas might end up like Heisenberg's Core Model, if we are not noticing something that we ought to notice.

43. **Geoff**
January 31, 2011

"None of the so-called "unified theories" are considered in this book, as in our opinion their physical significance is inversely proportional to the number of variants advanced, now more than twenty."

From the preface to Petrov's Einstein Spaces

Perhaps physicists could clear up some of the confusion by not prefacing a chalkboard equation with the statement "Morally speaking, the following should be…."

44. **John Bresnahan**
January 31, 2011

Larry Niven wrote "All the Myriad Ways" with this as it's theme. The original copyright is 1968.

45. **thomas**
January 31, 2011

any reasonable person would do the same thing with or without a non-collapsing wavefunction, since the reasonable thing to do is to maximize your chances of success, and the non-collapsing wavefunction should still respect probability distributions, after all, we observe it to!

I guess what I'm trying to say is that stuff that by definition has no effect on our world might just as well not exist.

Anyway, for a science that has fallen into pseudoscience, we have economics, which Krugman says has been taken over by right-wing ideologues who don't have any awareness of the arguments that people used to have.

Psychology, which started with Freud's brand of pseudoscience, continues to be full of it. That's not a descent into pseudoscience, though.

Medicine maybe? About a decade ago, some people started taking certain alternative medicine theories seriously. That was never really mainstream, though.

It's worth mentioning that neither geology nor biology have fallen into pseudoscience, much as the right wishes it so.

I think that pretty much rounds out our collection of sciences!

46. **John Fro**
January 31, 2011

All cosmologists these days are caught up in the legacy of Einstein and the glorious notion that they can rewrite all of physics with just an idea, even one that is counter-intuitive. This notion is very mistaken. There are no multi-verses because there is no way to produce them. What you have are potentials, but after an event those potentials collapse. Don't try to be the next Einstein. He was solving a very narrow problem with a specifically tailored solution that fit the known facts. While this resulted in predicting unseen phenomena, the goal was not to discover something new, but to solve an old problem. The odds are that you are going to get it wrong.

47. **El Cid**
January 31, 2011

Is the Multiverse Immoral? what is immoral is this post, let me remind you, that your criticising against string theory is for money, too. WTF?

48. **felix**
January 31, 2011

I'm surprised that Greene didn't name the book "The Elegant Multiverse".

49. **Adam F**
January 31, 2011

Biology has certainly not fallen into pseudoscience, but we are running into situations where a disturbingly large proportion of studies (particularly in medicine) turn out to be false. The everyday upshot of this is the constantly changing ideas about what foods and drugs are healthy. It gets very difficult to tease one small cause-effect relationship (say, genes or environmental conditions) from a huge mass of other contributing factors, all of which interact with each other. There are still plenty of areas where relationships are strong and research can be usefully done, though.

50. **Steve Baker**
January 31, 2011

So the multiverse theory must be wrong because it causes immorality? Wow – cogent argument!

We can't prove or disprove the multiverse theory – and there are other alternative theories that we also can't prove or disprove. There is no special reason to believe or discard any of them.

However, the moral issue is a specious one. Would you also condemn the possibility that the universe is infinite on moral grounds? You need to because in an infinite universe, there are also infinite numbers of "parallel earths" in which decisions you make come out differently.

Really, we all need to simply talk about what is possible and what is not – and look for more clues to tell us which is true.

Personally, I'm becoming somewhat impressed by "The Simulation Hypothesis" (look it up on Wikipedia) – it explains an awful lot about some of the seemingly arbitary things about our universe.

However, we can't either prove or disprove that one either.

In the end, all we can do is to fall back on weak guidance from Occam's razor.

51. **Peter Woit**
January 31, 2011

Steve Baker,

I've not claimed either that the multiverse is wrong or that it is immoral or that one implies the other.

What I do claim is that the string theory multiverse is not legitimate science since it is inherently untestable. Note that I don't claim all multiverse theories are untestable, I'm specifically referring to the ones promoted as an implication of string theory. Also note that you need to examine exactly what these theories say to see what their problems are and just how unlikely it is that they can be overcome. This is a complicated subject, but what's not complicated is the current situation: these theories currently make no predictions at all that can be used to test them. It's up to proponents of the idea to come up with a plausible avenue to getting such tests. I don't think they can, and I haven't seen them do it when confronted with the question.

I don't share Smolin or Horgan's particular concerns about morality, I was just reporting them. I do have my own concerns about the morality of how the string theory multiverse is being promoted, but this is not about the "morality of the multiverse", it's a moral question about how science is done.

52. **Clark**
January 31, 2011

The moral argument is weird. It's akin to the argument I've heard some philosophers make that a block universe (i.e. taking GR seriously) must be false as there then isn't the kind of free will many philosophers think is necessary for responsibility. It's an odd kind of reasoning undermined further by the fact, as you note, that in practice no one is probably going to change their behavior anyways.

It's odd Smolin goes so much against the multiverse theory since his own quasi-evolutionary theory doesn't seem *that* different. Although it isn't quite saying that anything goes.

What I don't get is why folks like Greene don't just accept that they are doing philosophy rather than science. I suspect it's a remnant of the anti-philosophical tendency left over from Feynman's influence. But honestly philosophy can be interesting. It can even be rigorous. It just shouldn't be taken as a scientific statements we have relative confidence in.

53. **Brian Knoblauch**
February 1, 2011

While the multiverse theories may not currently (or ever) be able to be experimentally verified/debunked, I don't buy the morality argument. As long as the science being done is still sound, and the multiverse theory is not presented as "knowledge" (merely as speculation), I have not problem with it. Speculating on all kinds of weird and exotic things has been a source of inspiration for others to make REAL discoveries/developments. Dragging morality into it this is pure silliness. If you're doing science the right way, "morality" should not be a concern. It either is right or wrong scientifically, there is no wishy-washy emotional component. By dragging morality into it, you defeat the science.

54. **Peter Woit**
February 1, 2011

Brian,

The problem here is that pseudo-science is being passed off as legitimate science to the public. One reason this is being done is to evade the implications of the scientific failure of string theory unification. This situation seems to me to raise moral issues.

Just to give one example, this is from a recent article on the subject:

"since almost all contemporary physics accepts and even demands the reality of parallel worlds. Love it or hate it, the multiverse is here to stay."

55. **David A. Spitzley**
February 1, 2011

Perhaps I've got the wrong end of the stick here, but is the "multiverse" any different from the Many Worlds interpretation of standard quantum mechanics, which people have been kicking around for decades? If not, how exactly have things gotten any worse in the context of these new books?

56. **Peter Woit**
February 1, 2011

David,

The controversy over the multiverse is about something different, the idea that string theory implies a multitude of completely separate universes with different physical laws. This is quite different than many-worlds, which is an interpretation of standard quantum mechanics, with one fixed set of physical laws. The two very different ideas are getting mixed together often, I'm not sure why.

57. **Nick**
February 1, 2011

Why do they get confused? Even though I have a B.S. in Math and follow science regularly, I didn't know the distinction between multiverse theory and many-world theory until now.

Science journalism for the general public needs an upgrade. Reputable newspapers, etc. have a difficult discerning the legit. from the pseudo science, sometimes degrees in the specific field are required. Any journalist wanting to do serious science reporting should start with a college level statistics course.

58. **Peter Woit**
February 1, 2011

Nick,

I don't think this can be blamed on journalists. The physicists promoting the "multiverse" tend to talk about these things together, without making the distinction, confusing the issues. On the whole I've found that science journalists do a pretty good job of reporting what scientists tell them.

59. **Michael T.**
February 1, 2011

It should come as no surprise to learn that scientists are not immune from self-promotion. The multivese is clearly a metaphysical concept at best but it does sell and the public eats it up big time. You can get a book deal, a few interviews on prime time, maybe even your own TV show. It is the era of the "scilebrity" so why shouldn't they hustle a buck 😉

60. **Tay Owens**
February 1, 2011

Many concepts which are now accepted as scientific fact were once derided as unfalsifiable and hence unscientific. Just because they are unfalsifiable with today's technology and limited knowledge doesn't mean all multiverse theories will always be unfalsifiable, and are therefore pseudoscience. Indeed, Greene takes pains to point out which of the 8 kinds of multiverses he details may be testable in the future (and why) and which most likely will remain unfalsifiable. Perhaps then we can view some multiverse theories as scientific theories, and others as philosophical theories. I rather like that idea, particularly as it brings physics closer to its roots as Natural Philosophy–a heritage to be proud of, not ashamed of.

Indeed, modern physics has often been preoccupied with matters that are perhaps more philosophical than strictly scientific in a Popperian sense–or, at least they seem so until much later when the math or technology or observations finally catch up to the theorizing (philosophizing). The chase after Grand Unified Theories or Theories of Everything or any unification of currently separate physical idioms is as much about producing a philosophically pleasing merge of disjointed models as it is scientific–after all, nothing precludes a universe that obeys multiple sets of laws at differing scales or states; we just don't like the idea on philosophical grounds and seek a more elegant and parsimonious solution. We'll probably find it eventually, at which point it will really be pure science; but until then it's just philosophy in science's clothing. Sounds a lot like multiverse theories to me. Does that make every physicist seeking more unified theories than current models and maths and observations accommodate a pseudoscientist?

61. **srp**
February 3, 2011

I'm a little puzzled by the oft-made claim that the SM is so good at matching the data that theorists have nothing to do. Lederman says that the gap between the observed vacuum energy and what theory says it should be is off by many orders of magnitude. And I seem to recall hearing about some gross anomalies in transverse-spin polarized collisions (Krisch?). Then there's dark matter. So I guess I don't see why everyone chases unification when more tangible prey seems to be available.

62. **Peter Woit**
February 3, 2011

srp,

In the SM the vacuum energy is a free parameter, you can make it anything you want. It's only when you try and work with unified theories of the SM and quantum gravity that the CC can become in principle calculable and you have to find an explanation for why it is so small.

There's no observation at all of dark matter or anything related to it in a laboratory experiment. It's an astrophysical phenomenon. The fact that standard models of cosmology and galactic structure seem to point to some sort of gravitating matter that we don't understand is an interesting hint, but it's not a lot to go on. In any case though, there's a huge concentration of effort by theorists to pursue this hint.

The anomalies in polarized proton collisions are a topic I wish I knew more about. They probably do deserve a lot more attention. Betting though seems to be that they aren't evidence of a problem with the SM, but with the fact that our understanding of how to do non-perturbative calculations in QCD is still quite crude.

63. **Phrustrated Fizix Stoodint**
February 3, 2011

Peter, first thanks for your book and this weblog, it has saved me many hours of not pursuing dead ends.

From your book, I have a quick question. On the last page of your book, you mention two directions that quantum gravity can go in, involving diffeomorphisms. A brief update on progress towards those goals since you wrote the book would be appreciated.

From this weblog and your last reply, we'd be interested in knowing if dark matter phenomenology is still the hottest field in theoretical physics (it was last year, yes?) and whether or not in your humble opinion it would be worthwhile putting effort into it at the present time. I'm not a herd animal and wish to pursue something unique, is why I'm asking. Entering a crowded field is unappealing to me and my friends.

64. **Peter Woit**
February 3, 2011

Phrustrated,

Dark matter is still a very hot subject, but there's a good reason for that: not much else in the way of data that disagrees with the standard model. Everyone is hoping that the LHC will soon change this.

There hasn't much new learned about the problem I mention in the book, how to handle diffeomorphism and gauge symmetries in QFT. Personally I've spent a lot of time learning about how to think of Langlands and geometric Langlands in terms of representation theory, and hope that this might lead somewhere, but that's a long way off.

65. **cormac**
February 5, 2011

Hi Peter, how are things:
my understanding is that,even with ST, the basic concept of the multiverse arises out of cosmic inflaton i.e. the possibity of bubble universes is raised by many of the current models of cosmic inlation.Have I got this wrong?

66. **cormac**
February 5, 2011

ops, that should of read "without ST"

67. **Peter Woit**
February 5, 2011

Hi Cormac, things are good!

Sure, you get bubble universes in some models of inflation. But what's new with string theory is using this as an excuse for not being able to predict anything. People making models of inflation with bubble universes can argue that their model doesn't predict certain things because those things are different in each universe. But they have to predict something to be taken seriously.

String theory is a unified theory, so in principle supposed to explain everything about fundamental physics. It doesn't actually work, but instead of admitting this, string theorists try and use the multiverse to explain why they can't predict anything at all. To me, that's where there's a moral problem…

68. **Stefano**
February 6, 2011

Peter, I enjoyed your book and visit your blog often. Now, not sure this comment belongs to this posting, but anyway… what I don't understand is why you and Lubos are such "enemies". Sure he goes for string theory, but on the other hand he is pretty clear about that other great bs science of day, i.e. "climate change", just like you are about string theory. Now, I have no personal reasons to argue that climate may not be changing or even warming – but what I do see clearly is the bs articles popping up everywhere tying AGW to even the most unrelated topics. It's clear that researchers have understood where the money is and what they need to say to get access to it. New Scientist, Scientific American and even something relatively neutral like sciencedaily.com have become nauseating in that respect. It's just like string theory. Bottom line you and Lubos clearly disagree on a lot of things, but you both can only take so much hypocrisy and bs… I wish the two of you noticed that commonality. (Sorry for the ranting style – I think this is why I commented for this particular post.)

69. **Peter Woit**
February 6, 2011

Stefano,

Actually I do agree with Lubos about a lot. He's not much more in sympathy with the multiverse than I am.

About climate science though, I don't think it's much like string theory at all. Public discussion of the issue is even more politicized and dominated by ideologs than string theory. I'm doing my part to fight this by stopping discussion of the subject here.

70. **chris**
February 10, 2011

"Maybe a branch of science is ripe for infection by pseudoscience whenever it stops making enough progress to satisfy the people in that field But is this really true? Does anyone know other examples, beside the current situation in fundamental physics?"

I don't really know this too well, but astronomy after Ptolemy seems to fit the bill. Or actually Ptolemy can be viewed as part of the decline already.

Another example that pops up in my mind is the scholastic period in the 13th-14th century and the associated mutation of science into comments on Aristotele.

In modern context i'm really tempted to say economics (as a science) and psychology of the Freud and Adler kind. But i don't know how much one can count these as sciences to begin with.

71. **Allan Rosenberg**
February 11, 2011

If multiverse-based theory (or its promotion) is immoral in our universe, does that mean it's immoral in all other universes?

72. **Peter Woit**
February 11, 2011

Allan,

If the laws of physics are different in different parts of the multiverse, I don't see why laws of morality shouldn't vary too…

73. **Allan Rosenberg**
February 11, 2011

Great, we finally have a physical definition of the term "moral landscape." But I think we should reserve judgment on the morality of the multiverse in our universe until the string theorists can at least let us know which of the many universes we inhabit.

74. **Matt**
February 11, 2011

Peter–

I think in your next posting on the multiverse controversy, it might be worth taking a moment to distinguish clearly between the different notions in play. There's the very old notion of the "many worlds" interpretation of quantum mechanics, going back to the work of Everett and DeWitt, in which the different universes are branches of a wave function.

Then there's the more modern notion of a multiverse—the one you're referring to in this post—in which different universes are realized at different locations in physical space. But there are two theoretical underpinnings to the idea, one coming from cosmology and the other from string theory.

Even putting string theory aside, the most successful models of inflation, which are quantitatively consistent with observational evidence from fluctuations in the cosmic microwave background and resolve myriad inconsistencies of the old Big Bang model (horizon problem, flatness, relics, structure, etc.), require at least 60 e-foldings. That essentially inevitably produces a vast, vast space in which our entire observable universe is smaller than an atom in comparison.

There's no string theory here, and this simple consequence of inflationary cosmology that our observable universe really and truly is but a mere speck in a far more immense space immediately raises the question of what the heck else is out there. Is it just empty space, is it stuff that looks pretty much like our own universe, or is it totally different? Given that the early universe exhibited energies far beyond those at which the Standard Model breaks down, there's as much reason to believe the third possibility as there is to believe that ice crystals forming at isolated, separated locations in a giant vat of water will all line up in parallel.

Is it really unscientific to be asking these questions, or thinking about them, or looking for observational signatures (CMB patterns, evidence of bubble collisions, etc.)? What string theory brought to the conversation was merely a candidate theory of quantum gravity that leans to the third possibility, but that possibility is still hanging there whether string theory is correct or not. What do you say to that?

75. **Matt**
February 11, 2011

Typo— should read " … will all line up in different directions."

76. **chris**
February 11, 2011

Matt,

the cosmological "multiverse" you try to construct is really not a multiverse. if you just wait for long enough (and assuming that the pressure to energy ratio of dark energy is at least not <-1) you will see. our horizon is expanding constantly and nothing too shocking is expected to come into view.

why?

because all relevant cosmological transitions are not first order as it seems now. there are no bubbles or cosmic strings as far as we can tell – nothing that would resemble a multiverse at all. so unless the underlying physical laws themselves are spacetime dependent, i guess it is rather boring.

and by the way, what is the scale that the SM breaks down? last time i checked it can be somewhere beyond the Planck scale if the Higgs mass is around 150GeV.

77. **Peter Woit**
February 11, 2011

Matt,

Some of the books I mentioned treat both many-worlds and cosmological multiverses, and the authors seem to thing these two things have something to do with each other. I've never been able to understand this.

I've no problem with people looking for observational signatures of bubble collisions or whatnot in various inflationary models. But it seems unlikely that the size of such effects is exactly such that they're just barely visible in the CMB data, and there's no underlying model that has experimental support that gives any reason to believe this is something reasonable to expect. So, it seems likely that all claims about this have about as much to do with the structure of the universe as does the appearance of Hawking's initials in the data. If people want to do this fine, but they should also be a bit careful about participating in articles in the popular press about about "scientists find evidence for the multiverse."

String theory doesn't "lean to" a multiverse or anything else, it's compatible with essentially anything. Instead of admitting that this means it's a failure, the people backing it have adopted the morally dubious tactic of invoking a multiverse scenario carefully constructed to not predict anything. This is not science, but an attempt to use pseudo-science to prop up a failed enterprise.

78. **Matt**
February 11, 2011

In the interests of clarifying matters, let me ask you a series of questions:

1\. Do you agree with most cosmologists that inflation is the leading candidate for resolving the troubles with the old Big Bang model, and that its quantitative predictions about fluctuations in the CMB have been quite successful?

2\. Do you agree with most cosmologists who say that you generally need 60 e-foldings of inflation for the models to work?

If you say yes to both 1 and 2, then you are basically led to the conclusion that the true universe is really vastly larger than the region we can currently see. You might call this vast expanse the "megaverse."

3\. Now there are two possibilities for this megaverse—it's either empty, or it's not. Do you think it's likely to have lots of other stuff in it?

If you say yes, then you've already accepted that the megaverse is really a kind of a multiverse. Maybe those other regions are filled with matter similar to the stuff in our own observable region—galaxies, planets, etc.—and maybe no two regions are alike. So it's not an "everything"-verse. But if you regard as scientifically acceptable the previous propositions, then you basically accept that we live in some kind of a multiverse.

4. We can go further, of course. Generically, models with inflation never end—inflation is always going on somewhere, because inflationary space expands far faster than patches can freeze out and stop. This is eternal inflation. Where do you stand on this question?

You may or not be on board with that idea, but it would obviously mean that we'll never see most of the multiverse, no longer how long we wait. In that sense, most of it becomes unobservable.

5. Do you regard speaking about those other parts as unscientific and out of bounds?

6. Finally, there's a larger notion of a multiverse where all those hypothetical low-energy vacua of string theory are realized in various places in space. Again, however, this doesn't mean that there's a copy of you where everything possible happens. If the set of possibilities is infinite and unbounded, then even an infinite set of bubbles won't necessarily explore them all.

7. Lastly, there's the idea that every single possibility is literally realized somewhere in some bubble. That's where the multiverse becomes an "all-verse." I definitely know that you regard this last possibility as pseudoscience, and it's what Smolin and others are morally objecting to.

I think it's important not to conflate all these ideas. There are serious scientists who find themselves at various places in this list. Where precisely do you think is too far?

79. **Matt**
February 11, 2011

Peter–

And note that string theory only came up in question 6 on the list. The landscape is not the same thing as the multiverse, as most cosmologists will be very careful to explain.

80. **Cosmonut**
February 11, 2011

@Matt:

2. Do you agree with most cosmologists who say that you generally need 60 e-foldings of inflation for the models to work?

If you say yes to both 1 and 2, then you are basically led to the conclusion that the true universe is really vastly larger than the region we can currently see. You might call this vast expanse the "megaverse.
———————————————–

This is not really true from what I have read.
60 e-foldings during inflation would lead to a homogenous universe of the same size, roughly, as what we see.
That is how the "60 e-folds requirement" is obtained.

Now, number of e-foldings to go beyond 60 — no one knows how many more, because there are dozens of models with no and no idea which is right.

IF that happens, then our "inflationary bubble" could be much larger than the observable universe.

But there's no reason to believe that there would be different laws of physics operating in other parts of the bubble. The default case would be the same laws all through the bubble.

81. **Peter Woit**
February 12, 2011

Matt,

You don't appear to pay any attention to the answers I already gave you, so I don't see why I should spend time answering the same questions again.

82. **Matt**
February 12, 2011

Peter– My point was just to make clear where you stand, but I understand if you no longer wish to discuss the matter.

Cosmonut— No, 60 e-foldings is necessary for our observable region to have the correct properties, such as flatness, but the consequence is that it's then a tiny patch of a far larger space. Think of what you have to do to a balloon to make a tiny surface patch look look very flat–you need to blow up the balloon to a size far more tremendous than that patch.

One piece of advice to you folks that will help you make your case more successfully. (And I'm somewhat sympathetic to that case, by the way.) Critiques of the multiverse would be more convincing if people didn't conflate all the relevant issues, and made more clear exactly what they accept as reasonable and what they don't. Is that asking too much?

Take care everybody.

83. **Emile**
February 12, 2011

Dear Matt,

Science is nothing without the part where we test our hypotheses. There is nothing wrong with starting with observational evidence of the early Universe and trying to figure out the implications. You can then formulate new hypotheses that hopefully will eventually get tested. Until you have figured out how to test those ideas, they are just ideas: call it scientific speculation if you must (but it is speculation). What makes science so powerful is the testing part. I think people are doing damage to its good name when they confuse speculation with science. The lay public is on to us. Look at some comments in newspapers and elsewhere and you'll read sentences like this one:"Modern quantum theory has gone into the never, never land of all theory and no proof, to the point where it's become a religion unto itself, whereby its all based on faith and no fact". That comment was related to a story about religion (I've started to collect these comments). It's unfortunate that quantum theory (think of the anomalous magnetic moment of the electron) is getting dragged into this. I personally think that these ideas about multiverses (even the landscape) are quite likely. But what I think doesn't mean squat. I'll call it science when we have figured out how to test it. And if it is forever untestable, than it will never be science.

84. **Matt**
February 12, 2011

Emile–

Are you a practicing physicist who works in high energy theory and cosmology? I ask not because the opinions of lay persons are inferior in any way–in particular, you make very good points–but that lay persons simply don't get a chance to see what the people who actively work on this stuff actually think about everyday. Peter has a lot of personal bias, as do we all, and he won't tell you.

I can assure you from much professional experience that whatever Hawking or Greene or Susskind or Weinberg (none of whom are very active on the research side any more) say in public, the people who actively study this stuff are absolutely obsessed with observability. Tests are very much on their minds. People are constantly devising schemes to look for signatures from the CMB, or evidence to favor one measure over others for use in making probability statements about features of our observable universe. (See the measure problem.) And that's only scratching the surface.

We are led almost inexorably to many of the central ideas of the multiverse even without invoking string theory, as my list earlier attests, and I can assure you that many researchers were dragged along unwillingly by logic and strong indirect evidence from cosmology, which has become an extremely precise subject in that past decade. (Peter diminishes this fact because it detracts from his belief that string theory is to blame for everything he doesn't like about contemporary high-energy theoretical physics.) Weinberg made his first use of multiverse arguments way back in the eighties when he correctly ballparked the as-yet-unobserved cosmological constant through anthropic reasoning, ten years before it was first measured and without using any string theory. Linde and Vilenkin are not and never were string theorists.

There's an enormous diversity of opinions on even the notion of the multiverse itself, even before connecting it to the landscape of string theory, and top people fall in many different places on my list earlier. I was hoping Peter himself would take a precise stand.

We live in the Internet age now, so you don't have to take my or Peter's word for any of it. Go online and find some videos of research seminars by active people studying this stuff, and it'll give you a real picture of what's on their minds these days.

85. **Peter Woit**
February 12, 2011

Matt,

From the e-mail that "Emile" left, he's a professional high energy physicist. I gather that his opinion is that there may very well be a multiverse, but at the moment it's not a scientific issue, and my impression is that this is probably a majority opinion among working physicists.

Those who work on trying to figure out how to get observational evidence for a multiverse should keep in mind that most trained people in the subject are skeptical about what they are doing. This is not because they are ignorant, but because they've seen nothing much substantive come out of these efforts. There's only so much mileage you can get out of the anthropic argument for the CC. People who want to work on how to turn multiverse speculations into solid science are welcome to, even if the odds of success are long, but until they've made some progress they might want to stop engaging in high-powered hype aimed at the public.

86. **Matt**
February 12, 2011

Peter–

My point is simple, and I think we're talking past each other.

The basic reasons for the multiverse do not come from string theory or the landscape, but from cosmology. Unlike many ideas in contemporary high-energy physics that people are looking for—supersymmetry, small extra dimensions, KK modes, brane-worlds, microscopic black holes, etc.—the basic underpinnings of the idea of a multiverse were not generated in any way by string theory, but from many directions and much indirect evidence (some of which is too technical to make sense to the public, unless they know how differential geometry or Einstein's equation work) in cosmology and inflation.

The cosmic inflation models that we use and that agree excellently with the data generally have as a side-effect the generation of a multiverse of some kind. So writing, even in a public forum, that we're likely living in a giant multiverse is perfectly within the realms of science. That's what the data are telling us.

Where I agree with you is that we don't know very much about what else is in that giant multiverse. Here's where string theory finally comes in: It suggests something about the multiverse, namely, that the multiverse consists of regions that look very, very different from our own region, as they have frozen out into different low-energy solutions with different low-energy particle menus and effective laws of physics.

Some people go even a step further, and claim that the multiverse is an infinitely big space and that every conceivable (and inconceivable) possibility is realized there, with copies of you and me doing and living every possible way imaginable.

Now that's entirely speculative, and we have no evidence to support. If you believe that those speculations are not scientific, then I'm in agreement with you, at least until people find a way to test them.

I worry that you are conflating the entire notion of the multiverse, which has strong backing from many directions in cosmology, with string theory and the landscape, when they're two conceptually different (but connected) ideas. Writing publicly about the cosmic multiverse and how it follows very convincingly from what we know about cosmology (and the search for signatures like bubble collisions, etc.) is perfectly scientific. Inserting string theory and making other metaphysical claims about all possibilities being realized in the multiverse is not.

Is that fair?

87. **Marty**
February 12, 2011

Hi Matt,

(Am not sure if you're the Matt I think you are, but if you recognize my name you probably are. In that case, Hi!)

I'm sympathetic to your interest in hearing Peter's stand on some of your questions to him. I too would be interested… However, in partial defense of his response, I think he probably did state his overall position; it's just that his position kind of preempts your other questions, if I understand his point of view correctly. I *think* where he's coming from is this: It's perfectly legitimate to start with a set of assumptions and use them to make theoretical arguments for a multiverse, bubble collisions, and so on;

but, without feedback from Nature (i.e., unambiguous observations of predicted consequences of those theories), there is no compelling reason to accept that the original assumptions and the theory they imply are correct. Theoretical reasons alone aren't enough… Peter has made it clear over time that his great respect for the Standard Model comes from the huge number of predictions it makes that have been verified, and the lack of predictions it makes that have been observably shown to be false. I think he also finds certain elements of the SM to be elegant as well, but that is secondary. (Peter, if I have misrepresented you here, my apologies.)

If you are "that Matt" 🙂 you know I have done some work with bubble collisions, the unfortunate conclusion being that they probably can't be unambiguously observed except in very "atypical" scenarios. That was enough to discourage me from working further on the problem in any significant way. To me, it just doesn't feel enough like "good science" to rely on theoretical "necessity" and consistency arguments; in the end, there needs to be solid experimental confirmation (as I'm pretty sure you agree), something I'm not optimistic will happen here.

You mention,

> We are led almost inexorably to many of the central ideas of the multiverse even without invoking string theory, as my list earlier attests, and I can assure you that many researchers were dragged along unwillingly by logic and strong indirect evidence from cosmology […]

This seems to be the standard wisdom, but I'm going to give a different take on it. First to state my own position: I personally can't imagine how any process that generated our visible universe wouldn't also generate other "universes," and the number of such occurrences would almost certainly be infinite. Moreover, I find the evidence for an inflation-like era in the very early universe to be very compelling and that the idea is almost certainly correct. But… I personally don't find compelling the argument that the second viewpoint implies the first. It all comes down to a question of theoretical uniqueness: Does an inflation-like era in the early universe *imply* an early de Sitter phase caused by one or more scalar fields tarrying for awhile (e.g. in a local minimum of the potential), or are there alternative pictures that can also yield something that looks like inflation and is fully consistent with all observations? It isn't enough to dismiss this question by noting that no other promising candidates have come forth yet, since both uniqueness of the inflation picture or missing insights (or imagination) can equally well explain the lack of alternatives.

After all, it's not as though the traditional picture of inflation is elegant in its crucial details. You know well that it is extremely hard to come up with an inflaton potential that is at all "natural" (so hard that nobody has figured one out yet, as far as I know) — the ones that are natural don't work, and the ones that work are contrived. (Potentials that rely on the correctness of the idea of a string theory landscape for their existence fall into the "contrived" category, in my opinion.) There's also the issue of the inflaton itself; this particle must be postulated as the driver of inflation, but there is no other evidence for it except that it seems necessary to explain inflation. In fact, given that the search for the Higgs has turned up empty-handed thus far, we don't (yet, anyway) have experimental evidence that *any* fundamental scalar particles exist in Nature. Hence, the reheating phase is also speculative; if we can't observe the inflaton, then we must rely on our speculation that the phase even exists, and further speculate on what a QFT would look like for the inflaton. It seems apparent that none of these problems can be addressed by observation, so it's hard for me to see how there is any inevitability to our current models of inflation or their consequences in terms of an inflation-inspired multiverse.

My own viewpoint is that reliance on an unnatural scalar potential (or set of scalar potentials) to drive inflation in just the right way to give us the CMB we observe, as well as the postulation of scalar particle(s) whose sole purpose and consequence is to drive inflation, are significant conceptual problems. In fact, for the past couple of years or so I've been working hard on an alternative scenario that seems much more "natural" to me; in it, an inflation-like era is an inevitable part of a process of spacetime emergence, ending *because* spacetime becomes sufficiently homogeneous and isotropic and not because of certain characteristics of weird potentials. This blog isn't the appropriate place to discuss that or other alternatives; I only bring it up to make two points. First, it suggests that the current inflation picture isn't unique in the sense I discussed above. Second, questions like those related to the measure problem or "Boltzmann brains" aren't even well-posed questions in the alternative scenario, suggesting that at least some of our current thorny conceptual problems are artifacts of our current (still partly speculative and non-unique) early cosmological picture rather than inevitable difficulties that will arise in any plausible inflation-like scenario.

(Just as I was about to post this, I noticed that Matt responded to Peter. Sorry if what I just wrote doesn't take into account anything Matt just said.)

88. **Peter Woit**
February 13, 2011

Matt,

I mostly agree with your last comment. Hopefully we'll learn more about inflation in coming years, but I'm skeptical we'll learn much relevant to the question of whether inflatiion actually does produce multiple universes. Maybe someday we will, which would be a good time to write popular books on the subject, not now.

Many thanks to Marty. He has thought a lot more about this than me, and taken the time to write out a detailed analysis of reasons to be skeptical about some parts of inflationary theory. His seems to me a better-informed version of my own skepticism about some aspects of the subject.

89. **Matt**
February 13, 2011

Peter–

Who says arguing on the internet can't ever produce agreement? (Or quasi-agreement; I'll take it.)

Just a last point. Evidence for inflation and the more general notion of a multiverse is in a different category from string theory/the landscape. There is no actual physical evidence for the latter, nor does it make any hard predictions, but several pieces of evidence and verified predictions for the former. So the former is not in the same category as the latter, even if it's not on quite the same rigorous footing as the Standard Model. The multiverse is not the landscape, and that's an important thing to make clear. I worry when I hear people like Horgan, who don't seem to make that distinction.

We have evidence that our observable universe is just a small part of a far vaster space, which we call the multiverse, even if we don't know what the rest of it looks like. (And even if we don't yet know exactly how inflation works in detail.)

Would it be fair for a cosmologist to write a popular account of the logic and evidence that has led serious people to that conclusion, as long as they don't veer off into wild speculation about the contents of the rest of that multiverse, and don't make strong claims about the reality of string theory? Is that scientific?

90. **Emile**
February 13, 2011

Hi Matt,

Peter essentially answered for me. My point is also echoed in Marty's post above: "To me, it just doesn't feel enough like "good science" to rely on theoretical "necessity" and consistency arguments; in the end, there needs to be solid experimental confirmation."

91. **Peter Woit**
February 13, 2011

Matt,

If a cosmologist would write a popular book explaining exactly what the evidence relevant to inflation is, as well as what the models people are looking at are (including ones with some sort of multiverse) and how they match the evidence, that would be great, I'd definitely buy a copy. All the ones I've seen though tend to go on at great length about speculative ideas that are supposed to be "exciting", ignoring or minimising their testability problems, and making really dubious arguments in favor of these ideas.

In more technical expository articles aimed at physicists, there's a lot less of this, but still they're often written in a way that makes one feel that one is only being told one half of the story.

92. **Matt**
February 13, 2011

Peter–

Wait, so you're upset that cosmologists who write for the public are taking their more rigorous work as springboards and then also going off in speculative directions they find exciting and inspiring? Why do you think they went into science in the first place? What do you think inspires a lot of the kids to want to do physics and cosmology?

As long as they make very clear what's speculation and what's not, I think it's perfectly fair for them to start with what we're pretty sure about and then talk about exciting future possibilities. There's criticism, and then there's being a grinch. It's a fine line sometimes, but worth keeping in mind.

This is actually a larger problem I have with some of your own public outreach. Science needs criticism, maybe more so than it's been getting. I would certainly concede that, and raise you a nickel. Your points are quite valid.

But too much negativity can be extremely counterproductive, and turns off a lot of young people, not to mention potential allies. It really does. Rather than pointing students in better directions, or brining in people who are sympathetic to your point of view (as I am), it can have the effect of just driving them away from wanting to get involved altogether. (I presume that's not your intent.) I speak from personal experience, by the way. I've seen fellow students react to it while I was coming up through my physics schooling.

That's one reason why I got so upset when I saw this posting on your blog. You seemed to be tossing in a lot of cosmology—some of it speculative—together with your diatribe against string theory. Is there any way you can be a little less negative and vitriolic, a little less broad-brush, and maybe more positive and constructive? In words that are going around a lot these days in other areas of discourse, could you disagree without being so disagreeable?

I mean, you're entitled to your approach and your attitudes. These are just suggestions. Take them under consideration or not.

93. **Peter Woit**
February 13, 2011

Matt,

People can write about speculative ideas they're excited about, I just think that they need to do so in an intellectually honest way. The popular press multiverse stuff I see, string-related or not, contains massive amounts of mis-leading, over-hyped material, aimed at getting people "excited" about some ideas which have very little evidence supporting them. As for the argument that this is the way to get kid interested in physics, I don't accept it. It's a good way to confuse people (of any age) about the difference between science and science fiction, and a good way to drive away people with some ability to think critically, who quickly suspect that someone is selling them a bill of goods.

It's true that I've ended up being critical of cosmologists pushing multiverse ideas independently of string theory. One reason for this is a general distaste for hype in science, although there's plenty of that elsewhere and maybe it should just be ignored. But in this case, cosmologists hyping the multiverse have just about always joined in with string theory pseudo-science, co-promoting it and using it to justify what they are doing. I'm seriously concerned that this is doing a lot of damage to this subject, damage that may be permanent. We're well on our way to having the dominant paradigm of the subject be "string theory is a fantastically wonderful unified theory justified by the deepest mathematics, and it shows we live in a multiverse such that we can never calculate anything beyond what we already understand, and that's just the way the world works."
There are a lot of people pushing this, and a lot of cosmologists signing on for the ride. I think it's worth my time to keep pointing out that there's no evidence for this, that it's an ideology driven by refusal to admit that a certain speculative idea has failed.

94. **Matt**
February 13, 2011

Peter–

I made clear that I consider a lot of your points valid. My point was your delivery, which does you no favors and drives away a lot of people who would probably be sympathetic to your point of view. A lot of people dismiss you out of turn because of your sometimes-overwhelming negativity and the tone of your comments–some of which I've seen you make in person–and that ends up hurting your own cause of making physics better.

Negativity drives people to shore up their defenses and to be less willing to take up alternative viewpoints.

Sometimes when it seems like trends are leading to doomsday, then there's an inclination to believe that all's fair and there's no reason to hold back. What was it Goldwater said, "extremism in the defense of liberty is no vice"? Well, I would have to disagree. I think it can be a vice.

95. **Matt**
February 13, 2011

Woops, looking at those last few sentences again—please don't think I meant to call you an extremist! I was just taking an extreme example. My sincere apologies for seeming to construe otherwise.

96. **Peter Woit**
February 13, 2011

Matt,

I do appreciate the advice and will keep it in mind. Sometimes the thought occurs to me "Oh, no, I'm starting to sound like Lubos!".

The string theory multiverse issue though I find increasingly both depressing and highly frustrating. Before about 5 years or so ago I thought it was possible to have a worthwhile discussion with string theory optimists, since the issues I disagreed with them about could be clarified by discussing the technical problems of the subject. In recent years that's increasingly no longer the case, as the string theory multiverse ideology has been constructed to make the subject immune to the usual sort of scientific challenges. My going on about this has likely gone past the point of being useful.

97. **Matt**
February 13, 2011

Peter–

I adore how you put that.

Sure, I may disagree with how depressed and angry you sometimes sound (even though you're really no cynic), but you aren't even in the same bubble universe (sorry, couldn't help myself) as Lubos. He's beyond Serge Lang territory these days. As long as you retain your evident capacity for self-criticism and introspection, you don't need to worry. I just wish you'd lighten up once in a while!

But, honestly, what can I expect from a leftist feminazi communist university AGW idiot?

🙂

98. **Matt**
February 13, 2011

But, no, seriously, what I find depressing is walking around my department and seeing everybody on his blog. I think the last time I could bring myself to look at it, he was deriding the decision to hold a theoretical physics conference in Mexico, because, as he put it, Mexicans statistically have too low an average IQ to do physics.

Sigh.

99. **Cosmonut**
February 14, 2011

Cosmonut— No, 60 e-foldings is necessary for our observable region to have the correct properties, such as flatness, but the consequence is that it's then a tiny patch of a far larger space. Think of what you have to do to a balloon to make a tiny surface patch look look very flat–you need to blow up the balloon to a size far more tremendous than that patch.
————————————————-

That's what I used to think, but its not true.

60 (or maybe70) e-foldings are the minimum required to give a value of Omega of the *same order of magnitude* as 1.

So, for instance, if we had observed an Omega of 2 – that would indicate a (hyper)spherical universe of about the same size as the observable universe.
But even to explain *this*, we would need to either assume super fine-tuned initial conditions, or an inflation period with at least 60 to 70 e-foldings.

The idea is, that just after inflation ended, the observable horizon would indeed be a tiny patch of the whole universe. But then the horizon would expand until the size of horizon and universe became comparable.
(See Sean Carroll's Spacetime and Geometry for the technical aspects of this.)

Now the point is, it is easy to get *more* than 60 e-foldings in the models, which would lead to the "small patch" effect you mention, even today.

But since we have no idea which model is right, nobody knows if we actually had just only a few more than 60, or billions of e-folds.
The number mentioned in articles is pretty arbitrary and varies widely.

100. **Cosmonut**
February 14, 2011

@Marty:
Does an inflation-like era in the early universe imply an early de Sitter phase caused by one or more scalar fields tarrying for awhile (e.g. in a local minimum of the potential), or are there alternative pictures that can also yield something that looks like inflation and is fully consistent with all observations?
—————————————

There was an interesting post on this a few days back at Cosmic Variance.

http://blogs.discovermagazine.com/cosmicvariance/2011/02/07/do-you-think-inflation-probably-happened/

Sean makes the same point in the comments that while it is observationally clear that some kind of rapid expansion happened early on, we are not sure whether this corresponds to any inflation model currently around.

101. **Cosmonut**
February 14, 2011

@Matt:
Hope I didn't come across as condescending in my last reply. I think you may be a cosmologist yourself from the comments.

Its just that I got really interested in the question of how large our universe is compared to what we can observe.
I had initially thought that if inflation gives us 60 e-foldings, then maybe our universe is at least e^60 times larger than what we can see (a mind boggling thought !).

But then, from further readings, I came to the conclusion I just mentioned.

If you know of any lower bound to the size of our universe from observational evidence, I'd be very interested.

102. **Bugsy**
February 14, 2011

Just want to add my 25 cents… as a mathematician largely ignorant about real physics!…
Matt says (Feb 12):
"Some people go even a step further, and claim that the multiverse is an infinitely big space and that every conceivable (and inconceivable) possibility is realized there,

with copies of you and me doing and living every possible way imaginable.

Now that's entirely speculative, and we have no evidence to support…"

Of the multiverse ideas, this one seems to be the most completely problematic.
Yet to popular writers (and the general public) this is understandably the most attractive, as it can lead to all sorts of wonderful plots for sci-fi stories…There is NOTHING wrong with that, if it stays as sci-fi (it's not just Sheldon, Leonard et al who draw inspiration from wild ideas-many of us got inspiration to study math or physics with well-written, creative sci-fi). I do object to real (ex?)-scientists blurring the distinction, es[ecially if they are using that to sell books, as that does a disservice to the endlessly honest and infinitely deep beauty of reality…

On to the "parallel copies of me" theory, obviously attractive to the ego in each of us…

Maybe this came about partly because of a confusion about the very successful and basic models in probability theory or dynamical systems. To be overly pedantic in the service of having something precise to address, thus an infinite sequence of tosses of a fair coin can be thought of as either a single sequence with certain statistical properties (a "sample path") or as the space of all possible such sequences endowed with a probability measure. There is a standard way to pass from one to the other, due to Kolmogorov combined with the Ergodic Theorem. Ditto for Brownian motion. Additional clarity is added by realizing coin-tossing as the 2x(mod 1) map on the unit interval, with Lebesgue measure, coding points with their binary expansions. Then the infinite random choice of coin flips (made by "Tyche, the goddess of chance" in Billingsley's beautiful explanation), is equivalent to a single random choice (by Tyche!) of a point in the interval, and the dynamics of the map moves us on to the next flip. But though this model is extremely useful, and though the space of all outcomes exists mathematically, no one thinks of all outcomes "actually" existing at the same time except mathematically.

On to the parallel multiverse. Now if in this universe I flip a coin infinitely many times, then presumably in the parallel universes all other sequences are realized, with a probability measure that reflects the Lebesgue measure. But then the
coin-toss model has been realized as a (very very small) factor (i.e. measure-preserving homomorphic image) of the multiverse!

In other words, the advocates of this particular multiverse fantasy have indeed made the mistake of conflating a successful mathematical idea with the "real world". And this makes me question seriously their understanding of either. Here I have been talking only about extremely simple math ideas (which I do however know something about) and if there is that much confusion there, I hate to think how sloppy the thinking must be when the much more sophisticated math needed for partical physics or cosmology is involved….!!!

In short I second Peter's concerns about the necessity and primacy of intellectual honesty. The real world is beautiful and amazing enough
without conflating and inflating it with lazy speculations; it is still wonderful to be able to say, "I don't know!" And with that basis, we can honestly say "what if…!" and again begin to dream…

103. **Allan Rosenberg**
February 17, 2011

Peter,

I think the way you express yourself does do you favors. Science needs informed criticism to progress, and that criticism needs to be most vehement where there is the most hype.

I'm not qualified to evaluate technical issues in string theory, but I can imagine a biologist who submits a paper postulating that lions have sharp canines because there is a multiverse that includes universes with all possible biota, and we happen not to live in a dull-toothed lion universe. I don't think that would pass peer review. Maybe we biologists are just too closed minded.

Don't worry, you don't sound at all like Motl. He has a Czech accent. And you're FAR funnier than he is.

104. **Jon**
February 17, 2011

Well, ether (wrt speed of light) was accepted scientific fact for a long, long time. Likewise various forms of Lamarckism. Likewise Freudian nonsense. All well within the modern era; all had a long run. I guess the new thing with multiverses is the large body of scientists who know it's shit, but don't say so publicly.

105. **Eli Rabett**
February 19, 2011

Well, Eli could make a fair argument that the multiverse kills moral responsibility. If all choices are made, why bother thinking about consequences.

Lest you think such angel counting has no consequences, think about the revolutionary change brought in by the reformation with respect to moral responsibility.

The 4% Universe

January 31, 2011
Categories: Book Reviews

I've written a review of Richard Panek's quite good new book The 4% Universe, which has appeared at the Wall Street Journal. The main topic of the book is the supernova searches that led to what seems to be a non-zero value of the cosmological constant. It also discusses the astronomical evidence for dark matter, as well as on-going searches for a dark matter particle.

One of the most interesting themes of the book is that of the encounter between the two different cultures of particle physics and astronomy. Astronomers have begun to worry not only about a new culture of large collaborations, but about the danger of an over-emphasis on certain specific measurements of fundamental significance. For more about this, see the article by Simon White from a few years ago Fundamentalist Physics: why Dark Energy is bad for Astronomy. Now that cosmologists have their own highly successful Standard Model, they're starting to take a look at what happened after the arrival of the Standard Model in particle physics, and worry that they too may someday become victims of their own success.

Comments

1. **Thomas Larsson**
   January 31, 2011

   Medieval astronomers knew that the universe is a mechanical clockwork with at least 13 epicycles.

   The point is that Nature's answers depend on how the question is posed. If you ask her about epicycles, she will answer with epicycles, even if that has little to do with the correct dynamics. And if you ask her about dark matter and dark energy, she will answer in terms of dark matter and energy. Perhaps this is the right framework. But perhaps it is not.

2. **Noname**
   January 31, 2011

   "Now that cosmologists have their own highly successful Standard Model, they're starting to take a look at what happened after the arrival of the Standard Model in particle physics, and worry that they too may someday become victims of their own success."

   Really? I'd say this is rather your misinterpretation.

3. **Mitch Miller**
   January 31, 2011

   The White article is pretty interesting…. though I imagine some high energy experimentalists will be offended as he basically said that creative and talented people would not go into it.

   It pretty much comes down to the LHC it seems. If only a single Higgs field is found astrophysics could potentially be the only possible experimental input for high energy theorists for the foreseeable future and that will have a large impact on astrophysics whether the current practioners like it or not.

4. **Peter Woit**
   January 31, 2011

   noname,

   You're right, I could be wrong. It's quite possible that astronomers the world over are wetting their pants in anticipation of changing the way they work, on the experimental side joining collaborations of 3000 others to work on a $10 billion dollar piece of equipment that takes 20 years to build, and possibly measuring only one new number, while the more theoretically minded work on mind-numbingly complex and unsuccessful models, in between writing science fiction and trying to pass it off as science.

   Then again, maybe not.

5. **Marcus**
   January 31, 2011

   Peter congratulations on your WSJ book review–and on the broadening of scope it represents. It would be great if you applied your informed intelligence and judgment to a wider range of topics, and would enrich what you have to say on the home topic. I'm just stating the obvious, not news to you!

   This is a good paragraph–saying in concentrated irony what was the overall takeaway message of Simon White's excellent article:

   "…It's quite possible that astronomers the world over are wetting their pants in anticipation of changing the way they work, on the experimental side joining collaborations of 3000 others to work on a $10 billion dollar piece of equipment that takes 20 years to build, and possibly measuring only one new number, while the more theoretically minded work on mind-numbingly complex and unsuccessful models, in between writing science fiction and trying to pass it off as science."

   So congratulations on several things (around the time of NEW #1000) and keep on truckin'

   -Marcus

   PS. I think Nature is more generous and holds more astrophysics surprises than collider, so it mightn't come down to the grinding semi-stall of your ironical vision. But that's not the point.

6. **Peter Woit**
   February 1, 2011

   Thanks Marcus,

   Unfortunately I'm not so optimistic about astronomical evidence helping to solve the fundamental problems of particle physics. The big open issue to me seems to be understanding the origin of electroweak symmetry breaking and I don't see astrophysics helping there. Maybe this will change, but to be clear, right now the difficult path

HEP experimentalists are following seems to me the only viable one.

7. **Noname**
   February 1, 2011

   I can see the point of White but I don't understand your rant about huge collaborations working on very complex experiments. When you consider the LHC, your lack of vision and appreciation is simply astonishing. I wonder why you chose Particle Physics at all. You must have been very confused. If the time comes when Astrophysics is over (not any time soon anyway) too bad for astrophysicists and so much the better for science.

8. **Peter Woit**
   February 1, 2011

   Noname,

   Since you don't understand my "rant", I'll spell it out for you:

   The fantastic success of the Standard Model has made life extremely difficult for particle physics. Improving on the SM is very hard, and the LHC is the only plausible way forward. It's a fantastic project and what people are doing there is great. But I don't think other sciences are looking at what is going on and thinking "gee, I wish my science was in that situation with those constraints and our job was more like theirs".

   Another thing I can assure you about astronomers and others outside particle physics is that they read things like what you write here and think: "My, what a bunch of arrogant nitwits there are in particle physics, thank God my science so far has relatively few people like that."

9. **Noname**
   February 1, 2011

   Thanks for your clarifications. I don't think anybody considers the situation in your terms, though. If there is something people from other sciences might envy of particle physics is the content, not the means.

   Concerning your last remarks, I don't understand why anybody would interpret what I wrote like you did (unless they are affected by some kind of paranoia). I'd write exactly the same as above with astrophysics replaced by any other branch of science, particle physics included. You on the other hand seem to consider that the real use of science is to offer jobs to the scientists.

10. **steve newman**
    February 2, 2011

    i like what Thomas Larsson wrote-

    "Medieval astronomers knew that the universe is a mechanical clockwork with at least 13 epicycles.

    The point is that Nature's answers depend on how the question is posed. If you ask her about epicycles, she will answer with epicycles, even if that has little to do with the correct dynamics. And if you ask her about dark matter and dark energy, she will answer in terms of dark matter and energy. Perhaps this is the right framework. But perhaps it is not. "

    I think his remark is relevant to the the 2 big 'mysteries'- dark matter, dark energy.
    The analogy is Big Bang model = mechanical clockwork with epicycles.

    Dark matter is not mysterious because it is dark, (lots of matter doesnt shine like stars) — but because it is presumed to be non-baryonic. If it were baryonic, it would not be a mystery.
    The gravitational mass of spinning galaxies is deemed non-baryonic only because being baryonic would contradict the limit on baryonic mass imposed by the big bang theory of nucleogenesis.

    So for no other reason but to save the big bang 'standard model' , there needs to be 6 times as much non-baryon mass as ordinary matter..

    Likewise, to save the big bang standard model from its many 'problems' (horizon, flatness,…) requires accepting the Inflation 'Scenario' (scenario,since its not quite a theory).
    This in turn requires the density of the universe to be the 'critical' density (omega=1), and so 73% of the universe mass must be something beyond the 4% baryon and 24% dark stuff.

    so– as Larsson says
    "And if you ask her (nature) about dark matter and dark energy, she will answer in terms of dark matter and energy. Perhaps this is the right framework. But perhaps it is not."

    — — — — –
    Similar remarks about 'cultural' and 'foundational' obstacles to progress in physics and cosmology
    were made by P.M. commenting on your book review of Yau's "inner space".
    PM says:
    September 20, 2010 at 9:23 am
    ….

    best wishes and thanks for your reports and reviews.

11. **neo**
    February 2, 2011

    So what is it? "Wonderful" or "quite good"?

12. **cormac**
    February 4, 2011

    Nice taut review Peter, I must get the book. The only thing I might have done differently is to emphasise why there has been this astonishing convergence of the study of the world of the v large with that of the v small in recent years…but that's cos I love this theme!

IAS 80th Anniversary Talks

February 3, 2011
Categories: Uncategorized

This may be old news, but I just recently noticed that talks given at the IAS in Princeton last fall to celebrate its 80th anniversary are now available on-line [here](). They include talks by Voevodsky on the [foundations of mathematics](), Zaldarriaga on [cosmology](), Wilczek on [supersymmetry and quantum computing](), and Arkani-Hamed on [Fundamental Physics in the 21st Century]().

According to Arkani-Hamed, the 21st century will be all AdS/CFT, all the time, using it to justify:

> the slogan is that string theory/quantum gravity is particle physics…
>
> There is a less interesting but amusing sociological fact associated with this, that since this realization there aren't really different camps in theoretical physics any more. There aren't string theorists and particle theorists, there's one big structure "Good Ideas in Theoretical Physics", OK. That structure has many different facets and you can work on different parts of it, but it's all connected. You can really see it in the way the field developed since the late 90s, we're much, much more one big happy family than was the case in the 1980s. I should say of course there are still people that do bad theoretical physics, but they're not at the Institute. So all good ideas in theoretical physics are combined in one very big structure that no longer is there such a big difference between strings, QFT…

This puts into practice Nati Seiberg's 2005 [prediction]():

> "Most string theorists are very arrogant," says Seiberg with a smile. "If there is something , we will call it string theory."

Both Wilczek and Arkani-Hamed advertise supersymmetry, with Arkani-Hamed making the peculiar claim that physicists need to throw space-time out the window, but for some reason, before doing so it is important that they add supersymmetry to it. Wilczek states definitively that if superpartners don't show up at the LHC, as far as he is concerned the idea will be gone.

The minimal supersymmetric standard model is an important pillar of the "Good Ideas in Theoretical Physics" that those at the Institute cling to, despite efforts to use them to unify physics failing miserably over the last 25 years. If the LHC doesn't see supersymmetry and Wilczek and others give up on the idea, it will be interesting to see if the IAS faculty revise their point of view and start to develop more of an interest in what they now claim is "bad theoretical physics". I suppose though that when they do, they'll call whatever they change over to "string theory".

Update: By the way, for the latest on what initial results from the LHC are saying about extra dimensions and supersymmetry, see [this talk]() given today at CERN by Alessandro Strumia.

Comments

1. **crystal**
   February 3, 2011

   Crystal ball gazing is a popular pastime, no matter that it has regularly failed. The title of Stephen Hawking's inaugural lecture as Lucasian Professor (1979) was "Is the end in sight for theoretical physics?" (The reason being, of course, the imminent arrival of the Theory of Everything.) So Arkani-Hamed 2010 is no wider off the mark than Hawking 1979. Physics – including "bad theoretical physics" and the Theory of Everything – will move forward on its own merry way.

2. **Geoff**
   February 3, 2011

   I've always wondered whether or not pure mathematicians dismissively refer to string theory as mere applied algebraic geometry.

3. **Peter Woit**
   February 3, 2011

   Geoff,

   No. Mathematicians are well aware that, whatever string theory might be, it's not a subfield of algebraic geometry.

   However, some mathematicians undoubtedly enjoy this xkcd comic:

   http://xkcd.com/435/

4. **Shantanu**
   February 3, 2011

   Peter See nima's recent colloquium at Perimeter

5. **Yatima**
   February 4, 2011

   Yeah, yeah… H.264 encoder needed. IAS doesn't care …

   crying_indian.jpg

6. **Peter Woit**
   February 4, 2011

   Shantanu,

   Unfortunately at the IAS talk he never got to the much more interesting material of the Perimeter colloquium.

7. **neo**
   February 4, 2011

As a non-physicist, I do not understand why adS/CFT generates so much excitement given that the CC is known to be positive, not negative. Is there a simple explanation?

8. **Peter Woit**
February 4, 2011

neo,

Besides that AdS/CFT tells you about gravity with the wrong sign of the CC, you might also want to worry that it tells you about gravity in the wrong dimension…

AdS/CFT is certainly interesting, but I do find the claim that all good theoretical physics is related to it just bizarre.

9. **Yatima**
February 5, 2011

Frank Wilczek's talk (http://video.ias.edu/stream&ref=409) is as usual pretty good. He also talks about aliens straight from a Greg Egan novel!

Is he last man standing who likes Spin(10) GUT?

I wonder why the speakers need to start off at super-low level. What kind of audience do these talks have? Hell, give me an equation in the first minute, amaze me!

10. **John Rennie**
February 5, 2011

Assuming that Arkani-Hamed's talk is the same as the last one I saw, his point (stripped of the propaganda) is that AdS/CFT is a useful mathematical model for describing various physical situations. In fact he explicitly compared it to the simple harmonic oscillator, which (also?) doesn't exist in the real world.

Since he is modelling physics that is experimentally accessible, or at at least much more accessible than the Planck scale, presumably we'll know soon whether AdS/CFT is a useful mathematical model or not.

11. **Bob**
February 5, 2011

In some sense, string theory is a unique way towards the theory of everything if it really exists. There are numerous reasons for that. Here i list some of them.
1 String theorists are smart
2 String theorists are hard-working
3 String theorists are not stubborn, namely always ready to absorb any important insight or development from other desciplines into their toolbox.

On the other hand, just like GR, when you learn AdS/CFT more, you will be intrigued by its beauty and power. With SUSY, AdS/CFT is a very powerful tool at least.

12. **AdSCFTlover**
February 5, 2011

Peter wrote: Besides that AdS/CFT tells you about gravity with the wrong sign of the CC, you might also want to worry that it tells you about gravity in the wrong dimension…

I don't understand why you wrote this.
If you are interested in 4-dimensional AdS space then you need a 3-dimensional CFT. There are plenty of those known, and now we also know that some of them are exactly dual to 4-dimensional AdS times a 7-dimensional Einstein space, like a 7-sphere. They are Chern-Simons theories, so you should like them. There has been lots of work on this since 2008. We also know that the critical 3-dimensional O(N) model is dual to a higher spin theory in AdS_4.

13. **Peter Woit**
February 5, 2011

My main problem with AdS/CFT is that it's an extremely technical subject, with by now about 10,000 papers or so dealing with it. This makes it hard for anyone to understand exactly what the state of the subject is, and this is made much harder by its proponents, who engage in an absurd level of hype. If you believe everything you read or hear, you learn that:

1. It's the modern version of the harmonic oscillator, the central approximation method for solving physical problems that we teach all undergraduates.

2. It contains all good theoretical physics, theoretical physics not related to it is bad theoretical physics. Theorists at top institutions like the IAS are all in agreement about this.

3. It explains heavy-ion physics.

4. It explains high-Tc superconductivity.

5. According to people like Bob, those who work on it are hard-working geniuses, always open to new ideas from all sources. They have found the unique possible route to a theory of everything. It's not their fault that this theory of everything turns out to predict nothing.

6. According to Arkani-Hamed and AdSCFTlover, this solves the problem of a realistic 4d quantum theory of gravity, that's done and over now.

John Rennie has been led to believe that soon we'll have definitive experimental tests of this wonderful subject. But it seems to me that we're now nearly 15 years into this, and I've yet to see anything like a convincing way to confront these ideas with experiment. The most highly developed thing I've seen are the attempts to use AdS/CFT to do approximate calculations in QCD, but here it still looks to me like you have only a very crude approximation with little control of its accuracy.

14. **Bob**
February 5, 2011

To be honest, as to AdS/CFT, i do not think it is a HIGHLY technical object if one is assumed to be familiar with both GR and QFT, two pillars of fundamental physics in 20th century. In particular, the essential underlying idea is remarkably as simple as possible, but not simpler. So i do believe that an idiot can also understand it.

Concerning its contact with experiments, string theorists are trying their best to making it possible. But there is no doubt that this is a tough job. We should be patient with its progress along this direction.

In addition, i am not saying that string players are geniuses although definitely a few of them are.

PS: After i spent some time on studying AdS/CFT, i found there are many misunderstandings about string theory and AdS/CFT in Roger Penrose's Road to Reality book. With this sort of experience, i realize that it is very dangerous to criticize something you are not very familar with.

15. **Peter Woit**
February 5, 2011

Bob,

Sorry, but you're really not changing my opinion that AdS/CFT has a problem with its proponents being fountains of hype.

16. **Bob**
February 5, 2011

Maybe there is some sort of hype in popular scientific articles related to string theory and ads/cft. But similar hype may also happen to other subjects.

But when you read any serious research paper by string theorists,
you will find that none claims that ads/cft has solved QCD problem. On the contrary, they are always saying that there is a long way to go for real QCD….

In addition, I prefer Nima's slogan, namely physics makes progress by radical conservativists rather than conservative radicalists. String theorists belongs to the former.

17. **anon**
February 5, 2011

An example of the bad physics pursued by stupid people is described in hep-ph/0604254 where progress towards over-constraining the CKM parameters via a combination of experimental measurements and lattice QCD calculations is reviewed (no doubt out of date by now). Of course, only ignorant losers could possibly be interested in such an approach to determining new physics beyond the SM.

As usual, if ads/cft geniuses and other geniuses have anything interesting to say about physics, let them go publish it in Physical Review Letters.

Cheers from `amused', after a few too many beers in Taipei. Happy Chinese New Year everyone.

18. **Alex Mikunov**
February 6, 2011

V. Voevodsky's talk is outrageously shallow. He definitely didn't do his homework when analyzing Godel's incompleteness theorems and Gentzen's consistency proof. The fact that the formalized version of 2nd incompleteness theorem is provable in arithmetic itself has far reaching consequences. It led (together with other developments) to an important class of logics called Provability logics (http://en.wikipedia.org/wiki/Provability_logic)
Voevodsky's inability to answer E. Witten's question [about impact on real numbers and the notion of continuum] makes one wonder if he's even aware of the work on independence in set theory. For recent advances see Woodin's review:
http://www.ams.org/notices/200106/fea-woodin.pdf
http://www.ams.org/notices/200107/fea-woodin.pdf

19. **Tim van Beek**
February 6, 2011

> …all good ideas in theoretical physics are combined in one very big structure that no longer is there such a big difference between strings, QFT…

Let's assume that Mr. Arkani-Hamed is aware of the fact that not all theoretical physics is theoretical particle physics, so let's replace "theoretical physics" with "theoretical partical physics".

But then this still means that if you work in theoretical particle physics, you either work on string theory or you're a bad physicist. The way that Mr. Arkani-Hamed made this statement seems to indicate that it is not meant to be a joke.

Well, someone has to tell the non-stringy particle physicists that they'd better switch to string theory, if they are intellecutally capable to do so, that is. Maybe some of them can be redeemed.

20. **joestudent**
February 7, 2011

I have a question about the slides of Alessandro Strumia's talk mentionned in the post: the physics is over my head, but the tone of the last few slides seems to be that SUSY is now very unlikely to be correct.

So I was wondering how much more data is needed to rule it out altogether (e.g. the little green area not yet excluded on slide 16), and whether this could happen before the 2013 shutdown. Thank you…

21. **Peter Woit**
February 7, 2011

joestudent,

The LHC will never be able to completely rule out supersymmetry, since you can always push the masses of superpartners higher. However, if you accept the standard motivation for the subject ("solves the hierarchy problem"), then you get that the supersymmetry breaking scale should be of order 100 GeV. Making other assumptions, Strumia gets a probability distribution peaked there, falling off at higher energies. By this analysis, supersymmetry was already rather an unlikely hypothesis, and the LHC last year removed half of the remaining probability.

I don't have a reference at hand, but talks by Atlas/CMS people projecting the reach of their experiments under various assumptions shouldn't be hard to find. My very rough guess is that they should be able to get gluino mass limits in the range 1-1.5 TeV in the run beginning this year, would need more beam energy and/or a lot more data to get up to 2-3 TeV. Even with this though, there will always be some small amount of probability higher.

It's going to be very interesting over the next few years to see at what point supersymmetry enthusiasts throw in the towel. By Strumia's measure, already half of the possible places for supersymmetry to hide have disappeared. Probably half of the remaining half will vanish in the coming year. Based on my experience with string theory though, I'm wary of making any estimate for how much evidence it takes to convince someone a speculative idea they favor doesn't work.

22. **Rhys**
February 7, 2011

Those plots are for the CMSSM, which is a highly constrained version of the MSSM (although to be fair, the MSSM parameters are already highly constrained by experiment). But weak-scale SUSY does not even have to be the MSSM.

Quite simply, it is way too early to be trying to draw conclusions about the existence of supersymmetry at the weak scale. The next few years will tell. I for one, and I think many other people, will give up on low-energy SUSY if it is not found at the LHC, but the time to give up is not yet upon us!

23. **Peter Woit**
February 7, 2011

Rhys (or any other supersymmetry optimists who care to comment),

You have to make some assumptions on supersymmetry-breaking parameters to even make any understandable statements about this subject, since there are over a hundred of them in the minimal version. If you're not going to be convinced until the LHC nails down bounds on every corner of the parameter space, this could take a while.

What I'm really curious is, at what point would you be willing to give up on supersymmetry if the LHC sees nothing:

1. Late this year (say 3 inverse fb at 7 TeV)
2. Late 2012 (say 5-10 inverse fb at 7 or 8 TeV)
3. 2015? (say 10 inverse fb at 14 TeV)

or later? (will you wait for 100s of inverse fb at 14 TeV, and when nothing is seen then, say then thing to do is to wait for the higher luminosity of the S-LHC, or higher energies from higher field dipoles?).

24. **Eric**
February 7, 2011

Peter,

I think it's only reasonable to wait until after the next two years of running to come close to drawing any conclusions about supersymmetry. As you've said, the expected SUSY spectra are dependent on different models of supersymmetry breaking, and there is currently still a lot of viable parameter space in different models. If supersymmetry is there, it will be found. If not, then there should be some other interesting physics to be discovered. I think it's better to approach things with the optimism and excitement of potential discoveries rather than the pessimism and cynicism that is all too prevalent in your posts.

25. **Peter Woit**
February 7, 2011

Eric,

Well, I'm optimistic that the LHC will tell us something interesting, but think it will be about electroweak symmetry breaking and the Higgs.

Still, I would like to hear from those who expect to see supersymmetry about when (in terms of amount/energy of data) they're expecting to see it.

26. **Eric**
February 7, 2011

Peter,

I'll think we may learn something new about EWSB as well, and I'm very excited to see what is discovered at LHC. I think everyone should try to get past being aligned with and fighting over particular theories and models, as if they were religious doctrines. I do think that supersymmetry plus Higgs is the most likely possibility, however I will rethink this if nothing shows up by the end of 2012.

27. **Rhys**
February 8, 2011

Hi Peter,

I hope your question about when to give up is moot. If the LHC doesn't find sparticles, then I desperately hope it finds some other new dynamics around the TeV scale, otherwise I will probably give up on high energy particle physics altogether.

Eric's comments above are very sensible.

28. **tristes_tigres**
February 9, 2011

Wilczek in his talk presents a graph of 3 coupling constants, that intersect at high energy. To an uninformed person like me, it looked rather persuasive. Is that honest, or did they have to tweak free parameters to get those 3 lines to intersect?

29. **Eric**
February 9, 2011

Hi tristes_tigres,

The gauge coupling unification at high energies happens automatically (within experimental errors) in the minimal supersymmetric Standard Model. Whether or not this is a sign of supersymmetric grand unification or just a coincidence is currently unknown, but many people take it as a circumstantial bit of evidence in favor of TeV-scale SUSY.

30. **Peter Woit**
February 9, 2011

tristes_tigres,

The coupling constant unification argument Wilczek gives is probably the best evidence for supersymmetry, but it is still rather weak evidence. The situation is more complex than the way it is usually advertised, for a detailed explanation, one place to look is

http://www.physics.ohio-state.edu/~raby/pdg.guts.revised05.pdf

My understanding is that things work best in a one-loop calculation, going to a more accurate two-loop calculation makes the agreement less accurate. There are many ways to parametrize the size of the disagreement, Raby gives it as 3 sigma. Note that this kind of calculation typically assumes no new physics between the TeV scale and the GUT scale, which most people find hard to believe.

Wilczek has a personal reason for being enthusiastic about this idea, since he was one of the co-authors of the original calculation of this kind.

31. **chris**
February 10, 2011

"I think it's better to approach things with the optimism and excitement of potential discoveries"

i for once think it has always payed off to approach new theory with extreme scepticism unless they explain all current experimental findings and make definite predictions for future ones.

"I desperately hope it finds some other new dynamics around the TeV scale, otherwise I will probably give up on high energy particle physics altogether."

so you don't follow Churchills advise. that might be really good for you and your future life. i for once am much more interested in how nature works and not so much in some particular theoretical idea. i wouldn't dream of throwing the towel just because an experiment does not confirm my prejudices. but motivations for doing theoretical physics are different i guess.

32. **Rhys**
February 13, 2011

"i for once am much more interested in how nature works and not so much in some particular theoretical idea."

When did I say I was only interested in some particular idea?
If all the data from the LHC can be described perfectly well by the standard model with a fine-tuned Higgs, then it will be very difficult to motivate further experimental or theoretical work in high-energy particle physics.

That's different to saying that there will be nothing to do in theoretical physics…

33. **chris**
February 14, 2011

I think you seriously underestimate human inventiveness 🙂

Celebrity News

February 8, 2011
Categories: Uncategorized

A selection of celebrity math/physics news:

- Jane Fonda's blog has a report on My Meeting With Stephen Hawking. Hawking told her "You were my heart throb", admitting that Barbarella was what he had in mind.
- MIT has put online a video of a lecture given in December by Jim Simons. Simons tells some of the story of his remarkable career and gives some advice about doing math. The financial news this morning reports that Simons's Renaissance Technologies is now down in Brazil doing high-frequency trading. The story is illustrated with a photo of Gisele Bundchen super-imposed on what appears to be Simons lecturing on differential K-theory.
- Someone has come up with an Einstein Index to rank theoretical physicists using a new citation analysis. According to this index, Juan Maldacena outranks Steve Weinberg and Ed Witten by a sizable amount. Further down the list, Sean Carroll and Murray Gell-Mann get the same ranking.
- Brian Greene's new book on the Multiverse is getting a lot of attention, with mostly laudatory reviews. For an alternate take, see the review at Bookforum by Charles Seife.

Comments

1. **milkshake**
   February 8, 2011

   the Seife's review of Greene's book apparently conflates the Landscape multiverse with many-worlds interpretation of QM. I wonder if the distinction is made clear enough in the book.

2. **Steve**
   February 8, 2011

   The Einstein index does not look too bad for comparing young scientist within a subfield of physics. Having said that, there is no index that works when comparing scientists across a timespan of many decades.

   Peter, your Einstein index is a miserable 180. How come? 😉

3. **Peter Woit**
   February 8, 2011

   Steve,

   Well, I guess it's because I'm not the next Einstein…

4. **Cesar Laia**
   February 8, 2011

   PG de Gennes died some years ago…

5. **Rien**
   February 8, 2011

   I'd say that any index that doesn't put Weinberg on top is flawed 😉

6. **Thomas Larsson**
   February 8, 2011

   How would Einstein score on the Einstein index?

7. **Chris Oakley**
   February 8, 2011

   I cannot help thinking that Hawking "blew it" slightly … I mean, there he was, surrounded by gullible actors. He could have said anything he wanted and they would have lapped it up. Instead of these quips about getting out of teaching due to his disability and Hanoi Jane being "hot" in Barbarella (although the latter, I admit, is a perfectly sound observation), he should have spun some yarn about being in contact with beings from the 11th dimension and then started warning them about the compactification that was due if the Earthlings did not spend more money on theoretical physics …

8. **Fabio**
   February 8, 2011

   Ah, the latest silly ranking index. These are always fun. I think the results tell us more about the field itself over time than they do about the individuals being ranked. Clearly you can see citation inflation at work. Also you can see increased herding tendencies in a field starved for good ideas.

9. **noEinstein**
   February 8, 2011

   I don't get it. In making up the ranking, he did not include Weinberg's books, but he included Maldacena's review which is similar to a book. If Weinberg's books our counted, he jumps way ahead of everyone. Clearly, all books and reviews should be excluded from the ranking.

10. **Shantanu**
    February 8, 2011

    Peter, this maybe old news and could be on your website. but the talks of 20th anniversary of INT are
    here
    This includes a talk by Howard Georgi on history of QCD and particle physics from 50s to 80s (sans string theory)

11. **Anony-Mouse**

February 8, 2011

I was recently looking through some classic papers from the 40's and 50's, and one very striking thing is how few references they have. Most of them cite fewer than five other papers total. Nowdays, the norm is to cite anything that is even tangentially related to what you are doing. 50 or more references seems common. I wonder if anyone has studied when and why this change took place.

12. **neo**
February 8, 2011

Anony-Mouse,

Einstein's 1905 special relativity paper cited exactly ZERO papers. Maybe the Einstein index should depend on how few cites an author can accomplish in a published paper.

13. **thomas**
February 9, 2011

I posted this over there, but I'd like to inject into the discussion that one reason people would dislike citation indices is how obviously they get gamed- the pressure to publish, the jokes about the minimal publishable unit, the politics of who gets a cite or a coathorship, the random cites of papers that the authors haven't read or think are viewed as influential just to pad the bibliography- and the concern that taking citation indices seriously would lead to more noisy politics and get in the way of doing science

14. **the Dude**
February 9, 2011

A very interesting ranking of the top theoretical physicists was done in the Discover magazine a few years ago. "With Smolin's aid, DISCOVER has scoured the landscape and found six top candidates who show intriguing signs of that Einsteinian spark. Smolin is too modest to say so, but he might qualify as a seventh;" The results are published here:

http://discovermagazine.com/2008/mar/13-e-nste-n

15. **Chris Oakley**
February 9, 2011

Hi Dude,

Interesting list.

1. Lisi. Unification based on E8. Currently no contact with the real world.
2. Hawking. Radiation of black holes. Not currently measurable.
3. Milgrom. MOdified Newtonian Dynamics. Explains galactic rotation without invoking Dark Matter.
4. Amelino-Camelia. Doubly special relativity. No experimental evidence.
5. Markopoulou-Kalamara. Works on loop QG , for which there is no experimental evidence.
6. Witten. Promotes Superstrings, for which there is no experimental evidence.

It looks to me as though the only one of these who has a theory that actually explains real physical phenomena is Milgrom. Citation counts unfortunately, are a poor substitute for this attribute.

16. **Giotis**
February 9, 2011

Well I'm sorry to say that but putting Witten in the same category with some of the members of this group it's kind of a sacrilege. Some of them will be thrilled if they could just shake his hand.

I wouldn't say that Witten is the next Einstein but that Einstein was the Witten of his time.

He is many classes above anyone we know.

17. **Bugsy**
February 9, 2011

And sometimes a wrong paper is cited many times with all the recent
citations remarking that the proof had been found to be false (as it was based on another false but previously much-cited paper)
…won't name names, to save the guilty from further well-deserved embarassment!

18. **Geoff**
February 9, 2011

How does Randall's coauthor Sundrum not appear as high on the list?

19. **chris**
February 10, 2011

Giotis,

with all due respect, but this should be a list of top physicists. Witten did some pretty good physics, ok. but there are people like Weinberg, Glashow, Wilson, Gell-Mann, 't Hooft – please. What does Witten have to show in terms of physics that would come close to the standard model, the renormalisation group, quarks or the renormalizability proof of nonabelian gauge theories?

20. **El Cid**
February 10, 2011

I don't see John Ellis in the list WTF?

21. **Amber Zee**
February 16, 2011

“Einstein-indices for well-known theoretical physicists and cosmologists. The Einstein index is plotted vertically, and each scientist is listed by the search term used in Google Scholar. Nobel laureates are shown in amber, others are listed in blue.”
So,physics and cosmology blogger Sean Carroll has entered the bottom end of this level which, without any doubt, makes him the physics blogger with the highest Einstein index. At levels above 6,000 we see true giants emerging. Well-known names like Stephen Hawking reside at this level.

22. **anon**
February 17, 2011

Amber, Carroll only made the list because the author forgot to exclude review articles.

Budget News

February 14, 2011
Categories: Uncategorized

Almost five months into FY 2011, the US still has no budget for the year, operating on a continuing resolution that funds the government at FY 2010 levels until March 4. The House Republicans have come up with a proposal for huge budget cuts, which would arrive late in the fiscal year, probably requiring national labs like Fermilab to essentially shutdown for the remainder of the fiscal year. John Conway has more about this here. No one seems to seriously believe this proposal will pass into law.

This morning, the White House announced its budget proposal for FY 2012. The DOE Office of Science would get a healthy increase, to $5.416 billion from $4.964 billion in 2010. Similarly, the NSF would go from $6.873 billion to $7.768. However, the Administration's FY 2011 request ended up being pretty much irrelevant, and it's not clear that this one will fare any better. No information yet on how HEP and mathematics fare specifically in these requests.

So, bottom line is that no one really knows what this year or next year's budget numbers will be, with the House proposal a lower bound for this year, and I suspect the president's proposal will be an upper bound for next year.

Midday tomorrow, Fermilab director Oddone will give an all-hands talk at Fermilab to discuss the implications of all this for the lab.

Update: At the DOE, the FY2012 request for High Energy Physics is $797 million, versus $791 million in FY 2010 (last year at this time, the FY 2011 request was for $829 million). More details here.

Details of the NSF budget request are here. Mathematics research goes from $241 million in FY 2010 to $260 million in FY 2012. Physics from $290 million to $300 million. The NSF has pulled the plug on the DUSEL lab, freeing up $36 million/year, which is repurposed towards what they describe as their three priority areas: physics of the universe, quantum information science and the physics-biology interface.

Comments

1. **Dave Miller**
   February 16, 2011

   Peter,

   What is your take on the NSF priorities: "physics of the universe, quantum information science and the physics-biology interface"?

   My own take is that "physics of the universe" (cosmology, I assume) is pretty interesting, that "quantum information science" is amusing but unlikely to really lead anywhere, and that "the physics-biology interface" sounds really important, except that I am not quite sure what it means.

   Dave Miller in Sacramento.

2. **Peter Woit**
   February 16, 2011

   Dave,

   Well, at least it's "physics of the universe" (whatever that means, presumably astrophysics/cosmology), and not "physics of the multiverse".
   The other two choices for priority are hot topics of this millenium, so not surprising. I don't know enough about research in those areas to have an opinion about how well-justified choice of them is.

   What is more interesting to me is what is now not a priority: accelerator-based high energy physics.

3. **cath**
   February 16, 2011

   "What is more interesting to me is what is now not a priority: accelerator-based high energy physics."

   That is a very astute observation. Robert Wilson, the first director of Fermilab (originally just NAL), explicitly compared the large new synchrotrons to the large cathedrals of medieval Europe. Wilson Hall at Fermilab is intended to resemble a pair of hands in prayer, and inspired by a cathedral in Beauvais, France.
   http://www.nature.com/nature/journal/v404/n6776/full/404350a0.html

   Perhaps the large accelerators will go the way of the old large cathedrals. Constructing cathedrals no longer holds as central place in society as it once did. Cathedrals are still built (and still respected), but built using very different materials and techniques. Perhaps new large accelerators will continue to be built (occasionally?), but with new technologies.

4. **The Cosmist**
   February 16, 2011

   On the issue of accelerator-based HEP, isn't the issue one of severely diminishing returns? I hate to be so practical, but what is the potential impact of this kind of research on human scales? I think the observation about cathedrals is astute, but even theological enterprises have their limits.

   I'm rather dismayed by the fact that, though we can understand mathematically the behavior of elementary particles, model the evolution of the universe almost from its inception and can see galaxies 13 billion light years in the distance, we still haven't found a suitable replacement for burning dead plants to power our civilization! Aren't we guilty of "fiddling while Rome burns", since without energy to power our technology none of the big science required by physics will be possible in the future? I suppose string theory will still be possible in a powered-down post-fossil fuel civilization, but you can forget about any more super-colliders ever again! Maybe physicists need to expend more intellectual energy on applications that can revolutionize life on human scales such as new energy sources, propulsion systems, new modes of industry, computing, communication, etc. – something I haven't seen a lot of in recent decades!

5. **Eric**
   February 16, 2011

   Dear Cosmist,

I think that nuclear/particle physicists gave the world an alternative source of energy to fossil fuels a long time ago, nuclear power. That's what will power the accelerators in the future. Most likely, the LHC already gets most of its electricity from nuclear power plants.

On a different topic, I thought that it was DoE that basically funded the national labs and particle accelerators rather than NSF?

6. **Peter Woit**
   February 16, 2011

   In my comment about NSF priorities, I should have just said "particle physics", not specifying accelerator-based.

   Eric,

   DOE funds Fermilab and much of experimental HEP in the US, but the NSF also funds experimental HEP, through experiments at the LHC and the Tevatron. In the past they have operated accelerator centers (CESR at Cornell), although no longer. They've also just recently pulled out of funding the DUSEL underground lab, which has created a problem.

First LHC Winter Conference Results

February 17, 2011
Categories: Experimental HEP News

This week the Aspen Center for Physics is hosting one of the first of this year's "Winter Conferences" where results from last year's LHC run are being reported. Appropriately, the title of the conference is New Data from the Energy Frontier. The most dramatic result has to do with what is not being seen: any evidence of supersymmetry, with new limits reported today by ATLAS. The new ATLAS results rule out gluino masses up to 7-800 GeV, improving on the first limits of this kind from CMS which were about 600 GeV.

For more detailed discussion courtesy of the blogosphere, see Resonaances and Cosmic Variance. For some indication of what this means for string theory, Michael Dine's lecture notes for his talks on "What LHC might tell us about String Theory" at last summer's TASI summer school are now out, with the title Supersymmetry from the top down. These lecture notes start off with a section very unusual for this kind of thing, entitled "Reasons for Skepticism". and he notes:

> Our enthusiasm for supersymmetry, however, should be tempered by the realization that from existing data – including early LHC data – there are, as we will discuss, reasons for skepticism.

For some historical perspective about what pre-LHC expectations were, I happened to run across today a copy of Witten's lecture notes from a string theory conference at Hangzhou in 2002, where he gives the muon magnetic moment discrepancy as one piece of evidence for supersymmetry, and says:

> Assuming this discrepancy holds up, we would expect to interpret it in terms of new particles, but these are highly constrained; one explanation that does work is supersymmetry, with masses of new particles of order 200 – 300 GeV.

Of course, even the minimal supersymmetric extension of the Standard Model is ferociously complicated, with over a hundred unknown parameters, so all quoted limits make various simplifying assumptions. Relating LHC data to limits on supersymmetry will be a subject keeping many physicists busy for the next few years, for more about this, see this talk at Aspen by Jay Wacker. He doesn't expect this year's run to as dramatically increase limits on gluinos as last year's run did, describing early results as "full coverage up to 300 GeV, reach up to 600 GeV", increasing to "full coverage up to 375 GeV, reach up to 800 GeV" after an inverse femtobarn of data is analyzed (that's the official LHC goal for 2011, although it's hoped they can double or triple that).

The last sentence of his last slide refers to something that I've always worried about, but am not expert enough to know whether such a worry is serious. He describes the web-site http://LHCNewPhysics.org where simplified models based on supersymmetry and other BSM ideas are given, and notes:

> ATLAS studying 10 Simplified Models from 0 in August. Changing their triggers.

The worry I'm not so sure about is to what extent the LHC detector triggers are being optimized to look for supersymmetry, potentially missing un-expected non-Standard Model physics. Since there were always reasons to be skeptical of LHC-scale supersymmetry, and these have now become so compelling that even Michael Dine is writing about them, one hopes that the trigger designers will keep that in mind.

Meanwhile, back at the LHC, powering tests are finished, the ring is closed and will be put through full tests of its operational cycle the next couple of days. Official start of beams for this year is planned for Monday.

Update: More details about the latest on this at Resonannces.

Update: More from Tommaso Dorigo (LHC Excludes SUSY Theories, Theorists Clinch Hands), and a Physics World article by Kate McAlpine here. Tommaso links to a 2008 posting by Ben Allanach that discusses predictions for SUSY masses made (using various assumptions one can read about there) around that time. One of these, by a large group including John Ellis, predicted that 50 inverse picobarns at 10 TeV would be enough to explore most of the region they expected SUSY masses to be in, at 68% confidence level. The latest data, which is about that luminosity but at 7 TeV, does rule out much of that region, with the most likely SUSY mass right around the boundary of the region ruled out by ATLAS (although the tan(beta) values are different). According to the Physics World article:

> John Ellis of CERN and King's College London disagrees that the LHC results cause any new problems for supersymmetry. Because the LHC collides strongly interacting quarks and gluons inside the protons, it can most easily produce their strongly interacting counterparts, the squarks and gluinos. However, in many models the supersymmetric partners of the electrons, muons and photons are lighter, and their masses could still be near the electroweak scale, he says.

Comments

1. **Thomas Larsson**
   February 17, 2011

   If even Michael Dine admits that there are reasons for skepticism, the reasons must be very strong indeed. Now we are only waiting for Gordon Kane…

2. **DB**
   February 18, 2011

   When the LHC fails to find supersymmetry, then Split Supersymmetry will be invoked to explain this away. Just as the Multiverse is now used to explain away the Landscape Problem of String Theory.

   Fundamental theoretical physics has never been in such a parlous state as it is today. It looks increasingly less like science, and more like a branch of metaphysical speculation.

3. **An Ominous**
   February 18, 2011

   However, after SUSY particles start to be detected at the LHC, Supersymmetry will be considered the holy grail of Theoretical Physics and another jewel in the crown of theoretical speculation and ingenuity, a la par with General Relativity. And then you should feel ashamed, but that's petty and unimportant.

4. **Peter Woit**
   February 18, 2011

   DB,

In split supersymmetry, the gluinos are still supposed to be at a mass accessible to the LHC. To explain away a failure to see them, you have to invoke super split supersymmetry,

http://arxiv.org/abs/hep-th/0503249

5. **Giotis**
   February 18, 2011

   There are two sides of the story.

   If Low SUSY is ruled out then someone could see it as a failure of the SUSY idea (and thus String theory) but others as another triumph of anthropic reasoning and the landscape. Indeed such failure will leave EWSB unexplained (like CC) and thus susceptible to anthropic interpretation (like CC).

   The rule is that the more phenomena are left unexplained the more the landscape idea gains validity.

6. **lun**
   February 18, 2011

   Peter, the "trigger bias" is a huge issue.

   Experiments were always theory-laden, but this used to be compensated by an ensemble of experiments to match the ensemble of theories.
   In era where each experiment costs billions of dollars, and you can cound the number of experiments on the fingers of one hand, biases built in the experiment can literally set phenomenology back for decades.

   A "toy scenario" example relevant for SUSY would be if unitarity at the EW breaking scale was restored by a slight decrease in multi-particle processes, rather then a resonance (this is exactly how unitarity is restored in high energy QCD, via Froissart's bound. We do not know exactly how to derive in terms of partons, but we know its associated to low Bjorken x strongly coupled physics).
   If this was the case, the only way to get a grip on the new physics would be to precisely cound how many multi-W, multi-Z and multi-top events there are, and compare with expectation. Can ATLAS and CMS do this, given the trigger bias? Perhaps, but its a good question.

   Experimental timescale to adapt to changes in phenomenology has already had big consequences in heavy ion collisions: When the LHC experiments were being designed, the consensus was that most interesting physics indicating deconfinement would be hidden in correlations of very low momentum particles (the technical name is HBT interferometry), and jet physics, for example,such would have little or no relevance.
   Nowadays the consensus is entirely reversed, few people care about HBT and jets are where the physics is at. Because of this, "particle-optimized detectors" (CMS and ATLAS) actually might have a better shot at intesting physics than the detector optimized for heavy ion collisions (ALICE), at least if this consensus lasts and is justified. So its not a "worry". It happened already.

7. **Tristes_tigres**
   February 18, 2011

   Lun,

   Thank you for info on the subject I have wondered about for some time.

   To what extent the raw data from finished experiments is archived and available for re-analysis, in case someone later comes up with a new idea?

8. **Peter Woit**
   February 18, 2011

   Tristes_tigres,

   The data collected is archived, but the issue with "trigger bias" is much more serious. Data that doesn't pass the trigger is not collected at all. In these experiments, the detector is producing data far more quickly than it can be collected, so an elaborate trigger is necessary to decide which data to collect.

9. **Tristes_tigres**
   February 18, 2011

   Don't they ever consider collecting some amount of randomly sampled events? I suspect it could help to debug and validate the trigger, in addition to enabling tests for alternative theories.

10. **trig**
    February 18, 2011

    They do all sorts of tests and checks. Control expts to quantify backgrounds, tests to quantify cosmic ray events which decay in the detector, lots of things. Why not ask someone on a big HEP collaboration? It does not have to be LHC, the same is/was done at the Tevatron and LEP, SLC, B-factories, etc.

11. **Michael**
    February 18, 2011

    Hi,

    concerning Tristes_tigres' comment: yes, we devote a small fraction of the trigger bandwidth to recording min-bias and even zero-bias data, precisely for its value in calibration and checks of detector performance. Such data are also extremely useful for tuning models of the underlying event (ie, extra tracks and energy not directly related to the main interaction) which can spoil the construction of an "interesting" high-pT event. Aside from the zero- and min-bias data, we collect events with relaxed trigger requirements, in order to verify the main triggers. These so-called "monitoring" triggers are pre-scaled, which means we save some fraction 1/N of them, so as to keep the trigger rate under control.

    People sometimes propose to look for new physics in these auxiliary triggers. The problem is that they have been heavily prescaled, so that the effective luminosity will be really very low. The new physics signal would have to have a large cross section in order to be discovered this way, which is why we experimentalists work so hard to devise a wide variety of triggers to collect relevant, rather than irrelevant, events.

    regards,
    Michael

12. **Bernhard**
February 18, 2011

I think it´s pretty soon to make claims about supersymmetry not being seen, as the luminosity is still very low. But in any case, the complicated plots showed by ATLAS and CMS showing some new parameter space region being excluded must be handled with caution. Normally a constrained model like minimal supergravity is used to set those limits and it´s not clear at all e.g. if light gluinos are really being excluded… I think that best thing you can say is that light gluinos are being disfavored but the evidence is circumstantial in the sense that if they were there, sure you could have detected them. On the other hand SUSY models have so many parameters that I can really see already some new paper coming up saving light gluinos again. Eventually they will have to give up but although you can exclude more regions, the theory is poorly falsifiable with such low statistics.

In any case I believe also that one thing that should not be forgotten, ever, is that even if SUSY is found and string people will be delighted, we should never miss the point that we are still talking about point particle theory (even with Michio Kaku lying to children on TV saying he works with string theory, THE theory that predicts s-particles).

13. **ss**
February 18, 2011

Supersymmetry will never be disproved. Phlogiston and the luminiferous ether were never disproved. Instead something else came along which could do a better job of explaiing the facts. But the point is "the facts" — HEP is really hurting for expt data beyond the Standard Model. In the 1950s and 60s there was plenty of unexplained data, and new discoveries every year. This just isn't happening now. A "particle desert" at the LHC will make the futire of expt HEP really bleak. People will not give up on supersymmetry or string theory jusy becaise no sparticles are found. Something needs to be FOUND, and then we shall see how well the various theiries explain it.

14. **Claver**
February 19, 2011

Dear Peter,

You might find the book 'Laboratory Life' by Latour and Woolgar interesting.

I say this because I understand your 'worry' to mean that you are concerned that experiment is being used to construct facts rather than observe them. Something the authors discuss.

Perhaps in this case you feel that the process is overly constructive of facts by means of excessive selection of data.

Anway, my goal was to point out the literature and show its relation to the present discussion. I cannot possibly make a qualified comment.

15. **Claver**
February 19, 2011

Perhaps this link may be helpful (Cormac O'Raifeartaigh) http://coraifeartaigh.wordpress.com/

16. **Michael**
February 19, 2011

Let's not forget what has been pointed out many times already, namely, that a signal in CMS or ATLAS for physics beyond the standard model will be difficult or even impossible to ascribe to SUSY per se. The reconstruction of the final states is difficult and it will take a while before any pattern points to SUSY as opposed to extra dimensions or Little Higgs models, among others. Some versions of low-energy SUSY might be more-or-less ruled out by CMS and ATLAS, but they cannot be unambiguously discovered.

17. **tristes_tigres**
February 19, 2011

Michael –

I have no doubt that HE experimental teams are thoughtful and know what they are doing. What I was concerned with is the possibility that motivation to get results ASAP gets in the way of testing alternative physics ideas by some outsider later. If we indeed hit the "particle desert", I hope they will store some minimally filtered data, just in case. Although, I imagine, someone not involved with detector designers won't find it easy to make use of the raw data.

18. **Peter Woit**
February 19, 2011

Michael,

Thanks for the explanations. You make clear the problem: almost certainly not enough possible luminosity in "unbiased" data to find evidence for something new.

Claver,

Thanks, but I should make clear that the "selection of data" effect here is not "excessive", it's forced upon the experimentalists by the overall limits on how much data they can collect. They're well aware of the problem and devote a lot of effort to figuring out how to deal with this. I'm not expert in this subject, so I don't understand all the choices they're making or their implications. My "worry" is just that these choices may be being overly influenced by theoretical models (supersymmetry and large extra dimensions) that have gotten a lot more attention than they deserve.

tristes_tigres,

There's been some debate in the past about whether these kinds of experiments should make their data public for others to try and analyze. Given the complexities involved, I'm rather dubious that outsiders could do this in any reliable way. In any case, my guess is that the LHC is going to be the only experiment of this kind for a very long time, and also will run (with upgrades) for a very long time. The groups working there are extremely large and will be doing this for many, many years. After the first few years, unless they've got some very exciting new physics already to analyze, I'm pretty sure that many of the physicists there will be quite interested in pursuing any new ideas about how to find something unexpected in the data.

19. **nnn**
February 20, 2011

This just appeared on CNN

http://news.blogs.cnn.com/2011/02/20/early-universe-revealed-at-4-billion-degrees/?hpt=C2

I suppose the nattering nabobs of negativism will complain about the statement near the end "finding … the Higgs boson … would explain gravity" or that the scientists said they would be happier if the Higgs is not found (?). But really, it is an effort to convey what is happening in modern science (expt HEP anyway). The stuff about 4 trillion degrees (Celsius or Fahrenheit doesn't matter) and primordial soup/early Universe is not bad at all.

20. **Geoff**
    February 22, 2011

    I've always wondered – to what extent does Witten's proof of the positive mass theorem depend on supersymmetry being realized in the physical world?

21. **Peter Woit**
    February 22, 2011

    Geoff,

    The great thing about mathematical proofs is that they don't depend at all on the physical world. Witten's argument about the positive mass theorem has nothing to do with whether a supersymmetric theory describes nature.

    More generally, if the MSSM doesn't appear at the LHC, this doesn't affect in the slightest any of the many interesting mathematical applications of supersymmetry.

22. **Claver**
    February 28, 2011

    Thank you for your response Peter.

This Week's Hype

February 21, 2011
Categories: This Week's Hype

A session on results from the LHC at last week's AAAS meeting has generated some news reports about results from the heavy ion run, see here and here. Under the heading "String theory supported", MSNBC reports:

> Previous experiments conducted at another particle accelerator, the Relativistic Heavy-Ion Collider in New York, showed that quark-gluon plasma took on the form of a liquid. Some scientists expected the plasma to go to a gaseous state at the higher temperatures achieved by ALICE, but it didn't. Instead, it was a "perfect liquid, which flows without resistance and is completely opaque," Schutz said.
>
> That in itself was a big surprise. But Schutz told me that the results were consistent with what had been predicted by a particular variant of string theory known as AdS/CFT correspondence, which also addresses such mysteries as quantum gravity and extra dimensions. "I'm surprised that they can make a prediction and that it matches what we measured," Schutz said.
>
> String theory is a long-debated conception of the subatomic world that envisions matter as being composed of incredibly tiny strings or membranes that vibrate in an 11-dimensional universe. Skeptics have criticized the concept as being untestable and unfalsifiable, but if findings from the LHC can confirm some hypotheses and falsify others, that could increase string theory's acceptance.

The campaign to deal with the failure of string theory unification by confusing it with AdS/CFT as an approximate calculational method continues. No matter how successful or unsuccessful AdS/CFT is at describing heavy-ion collisions, this has nothing to do with string theory as a unified theory of gravity and the Standard Model. I am curious though about the question of how well AdS/CFT does work as an approximation for describing heavy-ion physics. Can anyone point me to distinctive AdS/CFT predictions about what the LHC should see that are now being tested? The news reports just seem to refer to evidence that at LHC energies the quark-gluon plasma seems to continue to exhibit the perfect liquid behavior seen at RHIC.

Update: See the comment section for an extensive discussion by someone expert in the field (Hans Juergen Pirner) relevant to the question I was raising.

Comments

1. **El Cid**
   February 21, 2011

   Peter,

   String Theory is the best paradigm to build a theory of quantum gravity. I don't know why, like you, but what I've read about ST, I can say, she has the potential to solve the unification problem in physics. ST models can unify the gravity force with the other three fundamental forces in an only consistent framework, namely, ST is the way to build the Theory of Everything. ST is the natural heir of quantum field theory. So far, ST is just a paradigm. A paradigm with the adequate features to describe all known fundamental forces. A paradigm that is compatible with special relativity and quantum mechanics. A paradigm where you can quantize the Einstein field and to obtain consistent results with the calculations. Theoretical Physicists who are working in string theory are also discovering new math tools useful in other branches of physics. How time do you think is enough to complete the TOE? I think a hundred of years would be optimistic ;-). By now I just can say, ST is spectacular.

2. **Peter Woit**
   February 21, 2011

   El Cid,

   Even if one were inclined to swallow all of your hype, that wouldn't change the fact that the data about heavy ion collisions collected by ALICE has nothing to do with it.

3. **Amused**
   February 21, 2011

   Peter,

   I think these results about the quark-gluon plasma rather behaving like a fluid than a gas are not new but discovered and reported about last year already…

   So, it seems that Your google alert works not perfectly well sometimes :-P, ha ha …

4. **Peter Woit**
   February 21, 2011

   Amused,

   The relevant LHC ALICE results may not be new (I first mentioned them here), but the MSNBC hype is fresh this week.

5. **Brathmore**
   February 21, 2011

   (Note: I'm a big fan of the blog and appreciate the often lonely battle you've waged against string theory hype.)

   Peter,

   I went to a popular talk about string theory today by Brian Greene, expecting that all I would hear was unabashed hype. To my surprise and delight, at several points in the talk he specifically said that string theory was speculative in the sense that it didn't yet have experimental support. I find it encouraging that, due to just criticism from people like you, some of the greatest popularizers of string theory (e.g., Brian Greene) have started conveying some of the theory's difficulties. For instance, he mentioned the 10^500 possible geometries, and said that some people had taken the "justifiable step" of quitting work on string theory, that some were still trying to figure out a way to make it unique, and that others had resorted to the multiverse interpretation, which he openly acknowledged was very speculative and controversial. On the subject of AdS/CFT, he mentioned the recent Relativistic Heavy-Ion Collider result as being an "encouraging" result for string theory, but he certainly didn't tout it as being proof that the theory is correct.

I don't mean to make this about any one popularizer of string theory, but I think it shows that your critiques are having an impact, and that the public might be beginning to get a more balanced view of the theory from some of its popularizers. Good work!

(Side note – given the lack of progress in string theory, I for one would love to hear any news you might have about progress of other approaches…non-commutative geometry, loop quantum gravity, etc. Thanks!)

6. **Bee**
February 22, 2011

Reg. heavy ion physics at LHC & AdS/CFT, here's one paper I know of

Di-Jet Conical Correlations Associated with Heavy Quark Jets in anti–de Sitter Space/Conformal Field Theory Correspondence
Jorge Noronha, Miklos Gyulassy, Giorgio Torrieri

http://arxiv.org/abs/0807.1038

Abstract: "We show that far zone Mach and diffusion wake "holograms" produced by supersonic strings in anti–de Sitter space/conformal field theory (AdS/CFT) correspondence do not lead to observable conical angular correlations in the strict $N_c\to\infty$ supergravity limit if Cooper-Frye hadronization is assumed. However, a special {\em nonequilibrium} "neck" zone near the jet is shown to produce an apparent sonic boom azimuthal angle distribution that is roughly independent of the heavy quark's velocity. Our results indicate that a measurement of the dependence of the away-side correlations on the velocity of associated identified heavy quark jets at the BNL Relativistic Heavy Ion Collider and CERN LHC will provide a direct test of the nonperturbative dynamics involved in the coupling between jets and the strongly-coupled Quark-Gluon Plasma (sQGP) implied by AdS/CFT correspondence."

There was also something about jet quenching and elliptic flow, but can't recall the details. Maybe this paper: http://arxiv.org/abs/1009.2286

7. **Chris Oakley**
February 22, 2011

Brathmore,

It is of course a good thing that (e.g.) Brian Greene is prepared to be honest about the failure of String Theory; what is retarded is that the conclusion of all of this seems not to be that one should give up on it and try something else.

8. **piscator**
February 22, 2011

I heard an AdS/CFT talk recently which claimed that the thermalisation time of the QGP should depend on the boost factor of the heavy ion (I forget the precise dependence, 1/gamma?), so there was an effective prediction for the functional dependence of the thermalisation time on the centre of mass energy of the initial state.

Not an expert so don't know to what extent this is correct, a particular feature of AdS/CFT, or a particular feature of the model used. It remains the case that N=4 SYM in the N_c -> infinity limit is not a systematic approximation to QCD.

9. **AdS/QCDnik**
February 22, 2011

Some proposed predictions by Casalderrey-Solana et al
http://arxiv.org/abs/0912.3717
http://arxiv.org/abs/0907.4012

Iancu, Mueller and Collaborators have come with a picture for energy loss and momentum broadening at strong coupling.

All this builds strongly on the work of Gubser and collaborators, to mention just another prominent name.

10. **neo**
February 22, 2011

I am currently reading Brian Greene's new book. In it, as in the talk Brathmore attended, he candidly admits there is no evidence for string theory, as yet. However, he still asserts that the evidence is around the corner at LHC. His view remains that string theory is a huge success on all fronts, except for that little matter of experimental evidence…

11. **Sam**
February 22, 2011

The fact that ADS/CFT has some fairly strong qualitative resembelance to nuclear physics seems remarkable to me. I don't understand why you insist that S.T. must provide a cannonical explanation of such a compicated interaction before it can at least be admitted that the correspondence provides some level of evidence that the theory is on the right track.

12. **Peter Woit**
February 22, 2011

Brathmore,

Thanks for the encouragement. I should point out that Brian has always taken pains to make sure to acknowledge the lack of experimental support for string theory. Unfortunately his caveats often get dropped in stories that make it into the press. My main disagreement with him has always been on the issue of whether optimism about this situation changing is justified.

Unfortunately I don't know of any news about anything promising coming out of non-commutative geometry or loop quantum gravity. Personally I'm excited by some ideas about representation theory coming out of the Langlands program that I think might go somewhere, but I'm still a ways away from understanding this well enough to start promoting these ideas here. Maybe soon though…

13. **Peter Woit**
February 22, 2011

Sam,

I don't insist string theory provide a canonical explanation of nuclear physics. I'm quite willing to believe that it provides a useful approximation scheme in this field. However, the ferocious level of hype in this subject is pretty discouraging and makes it very hard for a non-expert to figure out exactly what works and what doesn't work. Again, it would be very interesting to see exactly what AdS/CFT based approximations predicted about what the LHC would see and compare to what the experiments are finding. Maybe such a comparison is out there, but I haven't seen it.

All of this though has nothing at all to do with string theory's failure as an idea about unification. Arguments otherwise seem to me to be nothing more than attempts to mislead people and avoid owning up to this failure.

14. **hv**
February 22, 2011

PW, you keep bashing string theory for its failure to provide a unified model for gravity plus the gauge forces. Why don't you post an explanation of how, in your opinion, the correct model to describe such an unified reality is *excluded* from the scope of string theory?

15. **Peter Woit**
February 22, 2011

hv,

Because no one knows precisely what "string theory" is, you can't prove that there is no way it can lead to a successful unified theory. However, you can look carefully at what is understood about it, what the problems are keeping it from success so far, and what the history of attempts to overcome those problems is, and make an educated judgment based on those facts. That's what I've tried to do in the book I wrote and in many of the blog entries here.

If they want to, people are always going to be able to come up with some reason to hope that string theory's problems will be overcome, no matter how bad things look.

16. **Bernhard**
February 22, 2011

These claims about ADS/CFT predicting anything in these experiments sound very suspicious. Where are the monte carlo/detector simulations showing that? Does one really want to use vague or qualitative arguments to support this? Or did I miss the point? I doubt very much any studies on this direction exists at all.

17. **hv**
February 23, 2011

I'm sorry, but when you say that 'no one knows precisely what "string theory" is', you're wanting an exact and rigorous nonperturbative definition of string theory. However, you know exactly how that unknown theory looks like in several perturbative limits. In addition, duality relations between those limits confirm that the they are different points of view of the same physical formalism. But even if that wasn't the case, each perturbative limit of string theory is known and well-defined.

So, reformulating my question, how would you argue that a realistic 'gravity + standard model' model is *excluded* as a vacuum of any one of these perturbative string theories?

While you say that 'you can't prove that there is no way it can lead to a successful unified theory', I say that it is strong enough to show that no perturbative vacuum of the theory contains 'gravity + standard model' as a low energy limit. If you fail to prove this, it means that there is a possibility that the opposite statement is true. There's also the possibility that it is false. Our inability for technically proving an assertion or its negation is not synonymous of unprovability.

18. **Peter Woit**
February 23, 2011

hv,

You can't show that a realistic unified theory is excluded as a solution of string theory for two reasons:

1. Even if you assume that couplings are small and you're in a region where a perturbative expansion about some hopefully consistent background works, you can't actually compute what you want, since the number of such things is exponentially large, and each individual computation is too difficult (you need to be able to do things like have explicit Calabi-Yau metrics).

2. Assuming you can solve 1, you still would not have shown that a realistic solution to string theory is excluded, because you don't know what the theory is outside of limiting cases. If the realistic solution is not in a region close to one of these limits, you don't even know what it is you are supposed to compute.

19. **hv**
February 23, 2011

If perturbative string theories are quantum and consistently contain both gravity and gauge forces (as they do), and if, as you argued, one cannot exclude that they may contain a realistic unified model, that means that they *potentially* harbor one valid complete description of reality. And that's enough motivation to continue doing research in string theory. You only listed the technical difficulties in dealing with the theory. Difficulties that warrant a continued research program, not its abandonment.

IMO, the necessary condition for a valid realistic unified theory is to contain gravitational and gauge forces in one consistent framework. String theory does that. The sufficient condition is for it to contain at least one specific model that is consistent with the low-energy reality and is consistent with new experimental high-energy results.

Other proposals, like loop quantum gravity, fail already at the level of the necessary condition, let alone the sufficient condition. Which is why string theory is the only framework on the table to a complete description of reality.

Theorists are yet to find a realistic unified model within string theory, yes, but that only means that it is premature to ask for experimental predictions. But the fact that no one has ever found one realistic model doesn't mean that none is there. It might not be there, true. But potentially it is. And that's what matters.

For example, Yang-Mills theories are a good framework to describe non-gravitational forces. But it is only a framework, just like string theory is. The match with reality in Yang-Mills theory resides in choosing the correct gauge group that describes reality. Luckily, there aren't as many Lie groups as there are Calabi-Yaus, so it was relatively easy to figure out that SU(3) describes the strong interactions, for example.

20. **Peter Woit**
February 23, 2011

hv,

You're just repeating standard hype. The facts of the matter are that thousands of people have worked for more than a quarter century trying to do what you suggest, and what they have learned is that:

1. They can't extract any specific predictions out of this framework. Not one. Nada. Zip.

2. There are clear reasons why this is true (see my earlier comment), and no one has any reason other than wishful thinking to believe that these reasons will disappear and the theory will become predictive.

Comparing this situation to that of the Standard Model is just silly.

21. **hv**
February 23, 2011

PW, if I'm repeating anything, it is standard LORE, not HYPE. With respect to the predictability of string theory, I think you are mistaken:

1. A theory is *wrong* if it is predictive but at least one of its predictions does not fit experimental data.

2. A theory is *incomplete* if the prediction for some physical phenomenon that could/should be in its scope is empty.

3. A theory is *non-predictive* if the prediction for some physical phenomenon is multivalued.

Unless you can demonstrate any these points, you can't dismiss any theory! And since no one, including you, has ever shown the multivaluedness or emptyness of the generic predictions of string theory (on any vacuum of the theory, toy or realistic), you can't dismiss it as wrong. Nor as non-predictive. Nor as incomplete.

One thing is certain: the theory may be technically hard, but none of the points above is undecidable. And that's why research continues.

22. **Peter Woit**
February 23, 2011

hv,

Your definition of "non-predictive", predicts multiple things, is what I would call "inconsistent". My definition of "non-predictive" is that it's a vacuous idea that can't now be used to predict anything, and such that there are good arguments that it never will. String theory fits this definition well. There are lots and lots of vacuous speculative ideas out there that don't and can't lead to a predictive theory. String theory is just one more.

23. **Hans J. Pirner**
February 24, 2011

Dear Dr. Woit

You have asking for information on Ads/CFT and QCD. I have been following with our research the Ads/QCD and AdS/CFT debate. My preliminary conclusions are as follows:(Hopefully this is not too long)

It is very difficult to model AdS/QCD breaking conformal symmetry in such a way that it reproduces the equation of state of Lattice QCD and the Debye Mass as a function of temperature. We have tried with a dynamical dlilaton in a 5-d two-derivative gravity action and with a Nambu-Goto action for the string to reproduce QCD as accurately as possible. See our reference "Trouble finding the optimal Ads/QCD (Veschgini, Megias,Pirner) Phys Lett B 696: 495-498,2011, see also very extensive work by Kiritsis et al. before us.

In this paper we also describe further work on the optimization program. (See the papersQCD-Thermodynamics using 5-d gravity and Thermodynamics of Ads/QCD within the 5-d dilaton-gravity model)

Concerning the heavy ion physics there are two aspects to be investigated further:
1)low energy flow (parameter v2) which documents the strongly interacting Quark gluon plasma according to most theoreticians

2)Jet quenching which should also indicate the strong interaction of the parton with the plasma

We only worked on the second topic there is a paper in review
"Jet Evolution in the Quark Gluon Plasma from RHIC to LHC" by Domdey, Kopeliovich and Pirner where we calculate two scenarios:
1)A conventional QCD parton cross section with the plasma particles which adds to the DGLAP evolution and then at the critical cross over temperature an absorption mechanism for the freshly formed prehadrons in the resonance gas.
2)An enhanced (K-factor =8, purely hypothetical) cross section (a la strongly interacting plasma) and no final state prehadronic cross section

Note, however, perturbative QCD has a problem to choose the coupling scale, since the shower virtuality is high , but the temperature in the plasma is low. We can fit with both scenarios RHIC data. But at LHC scenario 1 is favoured. In our prediction we cannot reproduce the strong transverse momentum dependence seen in the ALICE data between 5 GeV and 20 GeV. Our prediction for pion suppression is above the data for charged particles between 5-10 GeV. In our opinion this necessitates a more careful absorption calculation. Our conclusion would be: from jet quenching there is no strong indication for an ADS/CFT like strongly interacting plasma with the partons. Anyhow AdS/QCD is so bad an approximation to QCD concerning the running coupling and the string tension and the equation of state and the Debye mass that in our opinion it is not worth to be discussed seriously.

One definitely needs to break conformal invariance. Further progrees depends on coupling the world sheet to the dilaton and perhaps increase the number of derivatives in the gravity dilaton action. In total I think low energy QCD up to 10 GeV is a really good candidate for string theory and 5 dimensions may help to do a good job as Polyakov already pointed out very early. The arguments for this theory have to bottom up and at the moment not so much top down, but more work on the relation between a nonconformal string theory and the corresponding gravity is urgently needed.

Best regards

Hans Juergen Pirner

Implications of Initial LHC Searches for Supersymmetry

February 22, 2011
Categories: Experimental HEP News

There's a new paper out this evening from a large collaboration entitled Implications of Initial LHC Searches for Supersymmetry. Instead of just adding it to the bottom of my recent posting, I thought it would be a good idea to start a new one, and add a bit more explanation of what is going on.

For a good news story from today by Kate McAlpine, see this at Physics World. For excellent more technical explanations, see the latest blog postings at Tommaso Dorigo's blog (today) and at Resonaances (yesterday). Physics World, Tommaso and the new arXiv preprint discuss published results from CMS and ATLAS, while Resonaances discusses even more stringent preliminary limits on SUSY from ATLAS made public last week at Aspen.

Tommaso also refers to a 2008 guest blog posting by Ben Allanach explaining how statistical predictions for SUSY masses were being made, adopting various simplifying assumptions (CMSSM) and assuming supersymmetry solves the problems it is advertised as solving (muon g-2 anomaly, dark matter, etc.). Allanach discusses the 2008 version of this kind of calculation by the same group that has just put out a new, 2/22/2011 version this evening.

The usual model for how science is done is that theorists make predictions before an experiment is done, then when the experimental results come in, they get compared to the predictions. That's not quite what is going on here, where as far as I can tell, the new paper doesn't directly compare the 2008 predictions to the new experimental results. Instead, the new experimental results are used to make new predictions. Since a large part of the parameter space favored in the 2008 predictions has now been ruled out, the new ones move the favored part of parameter space up to higher particle masses. The authors do make clear what is going on, showing on their plots a "snowflake" where the 2008 best-fit value was, and "stars" for where the new best-fit values are based on data from the two experiments. Note that the paper does not include the latest, stronger results from ATLAS announced last week, which presumably would move the "stars" up to even higher mass.

While the question this paper addresses about where supersymmetry might be given that it hasn't been seen yet is interesting, it leaves unaddressed the more conventional question: do the LHC experimental results show that the theoretical predictions about supersymmetry made in 2008 before the machine was turned on were wrong? This is a statistical question, so should have a statistical answer. Assuming that the LHC continues to not see supersymmetry as it collects more data, I'm interested in the question of how the experimental data will falsify the theory. Will its proponents just keep calculating statistical predictions of higher and higher masses as lower ones get ruled out? Most will undoubtedly at some point throw in the towel, although there will be some who will never, never, never, never give up (see here):

> SUSY may still be there even if it remains invisible to the LHC, indeed. And yes, I don't hide that I will be convinced that SUSY is there even if the LHC doesn't find it. The LHC will only confirm or exclude effects at particular regimes – usually low energy but it's not quite accurate a description of the regime that may be excluded.
>
> What I have been scared for several years is the pseudoscientific propaganda of your kind trying to claim – without any justification – that not seeing SUSY at the LHC should imply that physicists shouldn't be allowed to work on SUSY or believe that it is a key feature of our Universe. There are many reasons to think it's the case and theorists whom I consider any good will continue to treat SUSY as an essential feature whether or not it shows up at the LHC.

Update: See figure 1 of this evening's What if the LHC does not find supersymmetry in the sqrt(s)=7 TeV run? to see how how much of the predicted region of superpartner masses was ruled out by initial LHC results, and how much of the rest is likely to be ruled out during by the 2011-2 7 TeV run.

Update: There's a very new up-to-the-minute survey of LHC results concentrating on supersymmetry by John Ellis here. Unfortunately no figures that superimpose CMS/ATLAS exclusion regions on the statistically favored regions for supersymmetry that are discussed (based on assuming supersymmetry explains dark matter and the muon g-2 anomaly). It does look like this year's data should be able to convincingly rule out the idea that supersymmetry explains both of these phenomena.

Update: The ATLAS results providing the strongest limits so far on SUSY are now out, see the paper here.

Comments

1. **DB**
   February 23, 2011

   At this rate Tommaso will soon be able to lodge his bet as triple A rated security. It certainly looks a lot more solid than what most banks have in their vaults.

2. **Bernhard**
   February 23, 2011

   "The usual model for how science is done is that theorists make predictions before an experiment is done, then when the experimental results come in, they get compared to the predictions. "

   Although I agree things are getting ugly for SUSY you have to admit that the SM was completely tuned to data for each we have absolutely no idea from where parameters are coming from. So, whatever the beyond standard model will be, SUSY or not, it appears the same procedure will happen, so people tunning SUSY models to data are doing nothing new and if it was good for the SM I don't see why this should be a problem if it's BSM. I am not advocating anything for SUSY but your arguments this time sound less convincing.

   My point: was the SM proven right or wrong? You can make a case for predicting W's and Z (so in a sense it was proven right) but in the end it uses a "minimal particle content". If any SUSY model will someday fit data better than the SM that would be an amazing achievement no matter how high the masses turn out to be, if that's what Nature wants. If it indeed what it wants it's THE question and I agree we should abandon SUSY (or regard it as unfavoured candidate as opposed to mainstream subject) if it does not show at the LHC at all.

3. **Peter Woit**
   February 23, 2011

   Bernhard,

   The SM case was quite different, with no free parameters available to significantly change the W and Z mass predictions made before the SPS started looking for them. If the W and Z hadn't been found, you wouldn't have had a group publishing a paper about how this changed the probability distribution for where the W and Z were. Instead, everyone would have just acknowledged that the SM was wrong and moved on to figuring out a better theory.

   In this case, all we have are statistical predictions of superpartner masses. I'm curious to see some measure of how the new experimental results compare to those predictions (e.g., what fraction of the original probability distribution is now ruled out). There's no way you can completely rule out supersymmetry the way the SM could have been ruled out, but you can quantify what new results say about predictions made before the machine was turned on.

4. **piscator**
February 23, 2011

Peter,

This isn't a completely accurate historical account of the development of the SM. For example there was a developed history of topless models to account for the repeated non-observation of the top quark. After all, the top quark was a basic prediction of the Standard Model which wasn't observed – and wasn't observed again – and wasn't observed again….

Sometimes the right response to an expected discovery that doesn't show up is to go to higher energies, where it will. We don't know whether the LHC will discover SUSY or not, but what is surely clear is that finally after many many years there is a machine capable of probing the TeV scale fully.

5. **Peter Woit**
February 23, 2011

piscator,

The top (like the Higgs) is an example of something not tightly constrained by the model before the experiments that went looking for it. My point was just that the SM did make dramatic, tightly constrained predictions that could not be evaded (the W and Z masses). This is very different than supersymmetry.

6. **Thomas Larsson**
February 23, 2011

I liked the first sentence in the collaboration paper.

"The results of experiments at the LHC will be make-or-break for supersymmetry."

Something to quote in 2015.

7. **Bernhard**
February 23, 2011

Peter,

I agree the SM was way more robust than SUSY in providing evidence that could not be evaded. My point was that both models however share the fact that some aspects can adapted. The SM has a minimal particle content solution, which naturally makes the necessity of things not constrained by the model much lower. The price is to leave lots of things unexplained.

The prediction SUSY makes that should not be evaded, even if shared by other BSM scenarios are particles TeV scale accessible, reason I said one should abandon it the same way they would have done it if the W and the Z were not there, as you said. The question of model determination will be very hard but in the end this is the price for a hadron collider as opposed to a linear collider.

I would also like to see a probability distribution for spartner masses, guess the best way to get this is to bet with SUSY theorists they are not capable of doing that (joke).

8. **UnderlyingEvent**
February 23, 2011

It's an interesting question – should we ditch SUSY if no hints of it are seen at the LHC? The problem is that supersymmetry appears to be a very important concept in quantum field theory – and the Standard Model is a QFT. To my mind, it would be perverse of nature not to be supersymmetric in some sense.

On the other hand, it is, to say the least, unconvincing to just dismiss lack of evidence for superpartners by saying "they must be at higher energy". It's a perfectly true statement, but where does this leave us?

I don't think the null results so far are too much of a problem for susy though. But if nothing is found in 2-3 years, it will be very interesting indeed to see how the people working in the field react. It's times like this I'm glad I work in QCD – because we know for sure that exists!

9. **Roger**
February 23, 2011

> Bernhard says: I agree the SM was way more robust than SUSY …

This is like saying that Newtonian mechanics is more robust than astrology. SM has always had huge amounts of quantitative agreement with experiment. SUSY makes no quantitative predictions, and no aspect of it has ever had any confirmation.

10. **Anonymous**
February 23, 2011

On-shell N=4 SYM: recursively solved to all orders:
http://motls.blogspot.com/2010/08/on-shell-n4-sym-recursively-solved-to.html
There's no other 4d QFT that is understood so well analytically.
This alone will guarantee that SUSY remains one of the most important ideas in theoretical physics even if a Planck-energy collider fails to find it.

11. **UnderlyingEvent**
February 23, 2011

Roger: "SUSY makes no quantitative predictions"

What do you think the LHC results are being compared to? Maybe you are thinking of string theory.

12. **A.J.**
February 23, 2011

Eh, Lubos is right on this one.

The hypothesis the LHC might disprove is that particle physics at the TeV scale is well-described by a supersymmetric theory. If LHC doesn't find evidence for a supersymmetric theory, people will continue to wonder if physics at higher scales is supersymmetric. That's their right, until someone actually does an experiment. Other people will no doubt speculate along other lines.

I doubt the idea of susy will ever go away entirely. If nothing else, it's easy to imagine a QFT class 50 years from now starting with the professor saying, 'Well, these theories aren't realistic, but it's useful to study them first, because they behave like the SubStandard Model in some respects but are easier to understand.'

13. **Roger**
February 23, 2011

> UnderlyingEvent says: What do you think the LHC results are being compared to?

They are being compared to the SM. And successfully too, as far as we know. SUSY does predict new particles, without saying much about their properties. That is a qualitative prediction, not a quantitative prediction.

14. **Eric**
February 23, 2011

Roger,

The only detailed property of the superpartners that SUSY-theories do not predict is the exact mass spectrum. This is because the mechanism of SUSY-breaking is not understood in detail. However, the masses are expected to be TeV-scale if supersymmetry is responsible for stabilizing the electroweak scale. If the superpartners are found, it will be considered a tour de force of theoretical reasoning. If not, then there is likely some other other new physics such as technicolor which will be found. The thing that must be kept in mind is that nothing will show up until the energy is raised high enough and/or enough data is collected. It is extremely foolish to draw any conclusions until these criteria have been met.

15. **chris**
February 24, 2011

"If not, then there is likely some other other new physics"

the key word here is likely. likely as judged by arguments such as naturalness.

in the case of the W and Z that were mentioned above there was no "likely". something needed to be there to make the S-matrix unitary. The SM however can provide for unitarity far beyond the Planck scale provided the Higgs mass is in the correct region.

i'd say it's a really tough call. colliders of any currently imaginable size might just not give us any new particles. we'll see.

16. **Sven**
February 24, 2011

Hi there,
two things:
1) The SM is already experimentally ruled out by the observation
(by several independent experiments) of Dark Matter – which on the other hand is automatically explained by SUSY (if R-parity is conserved).
2) SUSY, well, let's better talk about the MSSM to be specific, does indeed not make any prediction for the SUSY mass scales. However,
2a) there are firm predictions for the mass of the lightest Higgs boson. If there is no Higgs below 135 GeV the MSSM can be considered as ruled out.
2b) there are predictions using the existing experimental data. Exactly these kind of predictions, as made in the paper that Peter discussed in the beginning of this blog (and of which I am co-author), tell us that we should expect relatively low SUSY mass scales. Consequently, the new bounds by CMS and ATLAS not only move up the best-fit points, but also in the case of ATLAS worsen the fit probabilities slightly. The chi^2/d.o.f. will be an interesting measure in the future for these theories.

However, one should also keep in mind that the GUT-based versions of the MSSM under investigation right now (CMSSM, NUHM1, VCMSSM and mSUGRA) are only one special subclass of the MSSM. Other realizations might look completely different. This has hardly been analyzed so far.

17. **Bernhard**
February 24, 2011

"Dark Matter – which on the other hand is automatically explained by SUSY (if R-parity is conserved)."

Strictly speaking even if R-parity conserving SUSY is discovered that would be strong circumstantial evidence of (one of the, maybe) the origin of dark matter, but not a proof.

18. **Peter Woit**
February 24, 2011

Sven,

Thanks for the interesting comment. Given though that there's no evidence that dark matter is actually a WIMP, that's a pretty weak argument for adding a huge number of new particles and 120 or more new parameters to the SM.

Any evidence of a Higgs sector will definitely be the big news coming out of the LHC. If it contains no evidence for supersymmetry and the limits on strongly interacting superpartners continue to move up, covering almost all of the pre-LHC predicted region, I find it hard to believe that many people will continue to find the MSSM or its extensions very interesting.

19. **Peter Woit**
February 24, 2011

AJ,

I don't think Lubos's concern that theorists won't be allowed to think about supersymmetry anymore will be justified. While there are a lot of interesting aspects of supersymmetry, and supersymmetric qfts worth thinking about, the MSSM and its extensions don't seem to me to qualify. The world might be better off in a future where students aren't encouraged to spend a lot of time learning how to do calculations in this particular framework. It's quite a bit harder than in the SM, not easier.

20. **chris**
February 24, 2011

Sven,

I agree that there is a huge consensus that your arguemnt 1 is true, but it is a bit formal. strictly speaking the SM was disproven by neutrino oscilations. now for DM there is e.g. the axion possibility that carries no further implications for higher energies. if you want to make a case for the SM to be really disproven in the sense that at higher energies some new physics will show up i think the strongest point is matter-antimatter asymmetry and the associated need for B-L violation.

21. **King Ray**
February 24, 2011

Bad theories never die. They just fade away to higher energies.

22. **bad**
February 24, 2011

Bad theories indeed never die. They get ignored because alternative theories come along which can explain the facts better (and make successful testable/falsifiable predictions). Then they "die". That is what happened with phlogiston, for example. People simply lost interest.

23. **neo**
February 24, 2011

SUSY won't die, at least not easily. Here is a theory that doubled the number of possible particles, without one of them detected at pre-LHC energies. Only one thing can make physicists consider such a theory–an enormous aesthetic appeal. That appeal will keep it in the race a while yet.

24. **KD**
February 25, 2011

Anonymous:

"On-shell N=4 SYM: recursively solved to all orders:" is nice.

The question is "what theory describes the universe", not "what theory can be solved to all orders".

KD

25. **Paolo Valtancoli**
February 25, 2011

I am an expert of 2+1 gravity, which is an exactly solvable model. But unfortunately we live in 3+1 gravity… I think the same is for the idea of supersymmetry, which enhances the integrability of the model. But it is not warranted that Nature is an integrable model, and I suspect that it isn't.

26. **El Cid**
February 25, 2011

To the readers of NEW. This guy, Peter Woit is an enemy of science.
He's just looking for glory or money.

If you love Physics please:

ATTACK THIS BLOG!

YES WE CAN!

STOP PETER DON'T DELETE THIS COMMEN!

27. **Geometrick**
February 25, 2011

Very interesting what's going on with this experimental data. Puts a lot of current grad students into a bind when deciding which area of mathematical physics to pursue…

28. **Peter Woit**
February 25, 2011

Geometrick,

If graduate students have been making their career choices based on assuming low-energy supersymmetry would be seen at the LHC, they've been making a mistake. I don't think this issue puts them much in a bind, they can pretty safely assume that the LHC won't be seeing superpartners. There never was a good reason to believe this, and this should just become more and more clear as results come in.

29. **Eric**
February 25, 2011

"…they can pretty safely assume that the LHC won't be seeing superpartners. There never was a good reason to believe this, and this should just become more and more clear as results come in."

I look forward to seeing you eat your words in a couple of years.

30. **Sakura-chan**
February 25, 2011

"I look forward to seeing you eat your words in a couple of years."

Peter has a good track record of admitting when he's wrong, something SUSY proponents seem to lack…

31. **Eric**
February 25, 2011

Dear Sakura-chan,

If SUSY is not discovered after the LHC has run for 1 or 2 years at maximum energy, I think that most proponents would admit that it is not relevant to the TeV-scale. However, this is far from the case at the present. There is a lot of parameter space available for the LHC to explore, and only very foolish people are willing to jump to conclusions. It is not possible to rule out anything yet, including ideas which are alternatives to SUSY for which data has apparently not shown up either, such as technicolor etc… For that matter, the last piece of the Standard Model has not been found yet either.

32. **Anonymous**
February 25, 2011

"If graduate students have been making their career choices based on assuming low-energy supersymmetry would be seen at the LHC, they've been making a mistake."

If based on the least optimistic assumption that LHC would find nothing significant beyond the SM, graduate students in particle physics should seriously consider alternative options, because particle physics would become a dead field, more dead than now. At least you should be mentally prepared to switch research fields in case funding for particle physics drains in future. However, this mainly applies to the phenomenology. People working on the mathematically interesting aspects of SUSY and strings would be less affected. But keep in mind that mathematical physics is always a niche in the physics department.

33. **P.**
February 25, 2011

Dear Peter,

concerning your last update, within the context of the MSSM it is no problem to accommodate neutralino dark matter and the muon g-2, and at the same time have a very heavy spectrum with TeV scale squarks and gluinos. I don't think TeV scale supersymmetry will be ruled out anytime soon. More predictive models like Msugra or the CMSSM are easier to constrain and rule out however.

In general, the results of the LEP experiments and of the Tevatron have made it clear that new physics is most likely at or close to the TeV scale, with only certain weakly interacting particles (e.g. neutralinos, sleptons) still allowed to have smaller masses. This not only applies to the MSSM but also to other models that attempt to solve the hierarchy problem and have a dark matter candidate, like composite and little Higgs models, walking technicolor, 5D gauge Higgs unification and so on. For most of these models it is somewhat challenging to find regions of parameter space that are easily accessible at the LHC and not yet ruled out either by precision constraints or Tevatron searches.

34. **Peter Woit**
February 25, 2011

P.

Thanks! My comment about muon g-2 was based on Figure 5. of the new Ellis paper that I linked to, where he shows as a pink band the region favored by the g-2 measurement. I really wish Ellis had superimposed on that figure the latest CMS/ATLAS results, but my attempt to eyeball it (and guess that the difference in tan(beta) isn't important) indicated that much of the pink region is now excluded. It's quite possible I've got this wrong. I really would love to see the pre-LHC plots of regions favored by assumptions about supersymmetry superimposed on the data.

35. **Shantanu**
February 26, 2011

Sven,
the Dark matter -> supersymmetry or vice-versa argument has been oversold. (WIMP miracle etc)
some facts :
if you assume dark matter is a non-thermal relic you can essentialy get any mass or cross-sections in order to be a dark matter candidate (axions to wimpzillas)
Also DM could be primordial black holes which also has almost nothing to do with particle physics.
Although this is not a watertightargument I claim that simple vanilla WIMP dark matter is almost close to been ruled out, given that DD experiments are reaching limits close to 10^{-43} cm^2 (which is precisly the cross-section for a particle to be weakly interacting)

36. **Sven**
February 26, 2011

Shantanu,
your arguments could be right if one would invoke SUSY just to explain DM.
However, SUSY was 'invented' for particle physics for completely different reasons, and it has many virtues. The fact that DM is easily explained, is just a free bonus.
Concerning the DD limits, also this has been analyzed in the context of the fits and those GUT based models, see, for instance, fig. 20 in http://arxiv.org/abs/0907.5568 or figs. 6,7 in http://arxiv.org/abs/1102.4585.
One can see that DD limits will be challenging in the near future, but are not very restrictive nowadays.
Another advantage of the models: they make clear and falsifiable 🙂 predictions.

37. **Bernhard**
February 26, 2011

Sven,

SUSY was invented in the framework of a theory that so far has nothing to do with reality (strings). The other big advantage it could have, i.e. to solve the hierarchy problem was killed by LEP, so the best one can say today is that it ameliorates it. This is an embarrassment for the theory, not an advantage.

The bonus of DM is very weak, as Peter pointed out.

I´m not sure about other advantages. I admit its falsifiable only if one takes the position that one has to find TeV scale particles in order to stabilize the electroweak scale. But there is no hard requirement on that too, reason why many proponents are already saying that will not give it up even if the LHC finds no clue of SUSY.

Did I forget something?

38. **Eric**

February 26, 2011

"The other big advantage it could have, i.e. to solve the hierarchy problem was killed by LEP.."

This is completely false statement.

39. **Eric**
February 26, 2011

Bernhard, I think you may be confusing the gauge hierarchy problem and the so-called little hierarchy problem. The LEP results in no way whatsoever killed the possibility of supersymmetry being responsible for stabilizing the electroweak scale, nor due the present results from LHC. However, the range of allowed Higgs mass from LEP is such that it creates a small amount of fine-tuning in the SUSY mass spectra.

40. **Bernhard**
February 26, 2011

I´m not confusing anything. The fact that LEP killed the possibility that s-particles could have the same mas as its SM cousins requires that SUSY must be broken. That introduced the little hierarchy problem which is the same thing as to say that the hierarchy problem cannot be solved, i.e., that although the electroweak scale can be stabilized (you misinterpreted me here, as I never said it could not, quite the contrary) it must do it by a little bit of fine tuning. So, that´s why I repeat. SUSY does not solve the hierarchy problem, it ameliorates it. You can call this the micro hierarchy problem if you will, but still a problem. If the theory were so great it should solve the problem by zero fine tuning not by a small amount.

41. **Eric**
February 26, 2011

Bernhard,

The hierarchy problem as I understand is equivalent to the problem of stabilizing the electroweak scale, in other words, preventing the Higgs mass from receiving large quantum corrections which push it to the Planck scale. The little hierarchy problem is due to the fact that LEP has constrained the mass of the Higgs to be > 114 GeV, whereas it has a more natural value of around 90 GeV. This causes there to be a small amount of fine-tuning in the SUSY mass spectra, but this amount of fine-tuning is not really a problem. In fact, if there ends up being a fourth-generation of fermions, the Higgs mass should be heavier.

42. **Bernhard**
February 26, 2011

Yes, I have a problem with this consensus that it is allowed to say you solve a problem by introducing a little one. I´m aware of this "understanding" people have, which for me it´s just cheating. But fine, this is polarizing into semantics not physics, since I agree with what you said. But contrary from you I believe that introducing a little bit of fine tuning for answering a problem of large fine tuning is far from great. Let´s forget how one calls it. My point: can SUSY stabilize the electroweak scale by no fine tuning and no fine tuning at all? If the answer for this is "yes", than I would retreat and admit I was wrong as this is not the information I have. If however the answer for this is "no" or "kinda", than sorry, stop calling this the little hierarchy problem and just admit the theory failed in solving the dam thing.

43. **Eric**
February 26, 2011

Bernhard,

The hierarchy between the TeV-scale and the Planck scale is sixteen order of magnitude. A model with TeV-scale SUSY cancels corrections to the Higgs mass of this order. You are complaining if the Higgs mass is roughly 25 GeV larger than the mass of the Z-boson. This seems extremely unreasonable to me. I don't know about you, but I think that a mass difference of the order of the Plack scale is much larger than 25 GeV. Just sayin'….

Equally unreasonable is Peter's claim that a theory should be able to predict the exact masses of unknown particle ahead of time, something which has almost never happened in the history of particle physics.

44. **Bernhard**
February 27, 2011

Eric,

That´s why I acknowledged SUSY ameliorates the problem (by a great deal, I can even add that if makes you happy), but if we are talking about a solution, than the 25 GeV difference is enough for complaining. You cannot expect someone to say a theory is great because it is off by only 25 GeV. What I would like to see is a real solution, something that solves the problem and afterwards you have the clear "aha" feeling that it was unavoidable. That´s not what´s happening here and everything you said, to my mind, reinforces that.

Of course there is still hope. If TeV scale SUSY is found than loop corrections to the Higgs mass will be saved almost naturally. Almost. But a real natural solution was discarded by LEP, my original point. Furthermore if on the other hand the LHC still does not find any signs of SUSY I really see not point on insisting with this theory.

45. **Peter Woit**
February 27, 2011

Eric,

The problem with the motivation for SUSY that it "stabilizes the weak scale" is that the weak scale is around 100 GeV, and the limits on superpartner masses and thus the supersymmetry breaking scale are getting close to 1 TeV. So, there's an order of magnitude problem here. This shows up in the Higgs mass as a requirement of of significant fine-tuning to get a mass above the LEP limit. I forget what the numbers are (ten percent, one percent?), but they're much larger than just 25 GeV/Higgs mass.

46. **Sven**
February 27, 2011

Fine-tuning can easily be regarded as ill-defined, since there is no measure on the parameter space. Yes, precise measurements require precise values of input parameters. This is called 'measurement'. 🙂

That the MSSM even in its most simple realizations (like the CMSSM) can easily produce a Higgs mass value high enough, was shown (again) in the paper discussed in the original post (and many others). To me it is really amazing that such simple models are in agreement with *all* experimental measurements, including (g-2)_mu and

DM.

Another good point for SUSY: in its most simple realizations (GUT based models such as the CMSSM) it correctly predicted the top quark mass many years before its discovery. The requirement of correct electroweak symmetry breaking placed m_top between 150 and 200 GeV, which is exactly correct as we know now. 🙂

47. **chris**
February 27, 2011

Sven,

"Fine-tuning can easily be regarded as ill-defined, since there is no measure on the parameter space."

how right you are! and now please tell us again why it is so great that SUSY is supposed to solve the fine tuning problem of the Higgs mass?

48. **Eric**
February 27, 2011

Peter,

If you look in any elementary textbook, you will see that the electroweak scale is stabilized by SUSY so long as the splitting between the SM particles and the superpartners is of the order of a TeV or less. Thus, your above argument is completely ill-informed and spurious. I suggest you do a better job of educating yourself on the subject.

Best,

Eric

49. **Peter Woit**
February 27, 2011

Eric,

One of many places I've learned about this is one that I went back to recently, Arkani-Hamed's talk at Strings 2005 on "HEP Circa 2010".

http://www.fields.utoronto.ca/audio/05-06/strings/arkani-hamed/

I suggest you listen to it, especially the part starting around 21 minutes in, where he discusses supersymmetry and the "little hierarchy problem". He explicitly states that the problem is that one expects the superpartner spectrum to be at LEP energies, not TeV energies.

By the way, he was expecting first collisions summer 07- winter 08 and arguing that by the end of 2008 one would certainly know if supersymmetry had any relevance to the hierarchy problem, claiming that only weeks or months of data would be needed to see squarks and gluinos if they were there and relevant to the hierarchy issue. He was far too much of an optimist about the LHC initial energy and luminosity, but still.

I suggest you contact Nima and explain to him why he's full of it and needs to better educate himself…

50. **Peter Woit**
February 27, 2011

Sven,

Interesting to see that you're abandoning the naturalness argument for supersymmetry. Arguing that it's remarkable that by doubling the number of degrees of freedom and adding 120 new parameters one can fit some conjectured new physics isn't exactly convincing.

Will your group put out a new analysis soon using the latest ATLAS paper, and showing how their results compare to your 2008 CMSSM predictions? It seems to me that your argument that CMSSM explains muon g-2 AND dark matter has likely already been blown out of the water by ATLAS. But maybe more data is needed, I'd love to see an appropriate plot.

51. **Arun**
February 27, 2011

Arkani-Hamed slides on "Approaches to the Hierarchy Problem" (PDF file, best I can tell, it is from July 2004)
http://www.sns.ias.edu/~arkani/pdfs/HierarchyAppr.pdf

52. **Sven**
February 27, 2011

Peter,
our simple realizations of the MSSM have 3 or four new parameters, not more.
Consequently, the chi^2/dof is excellent.
Our latest paper (the one that you discussed) includes already the latest official
ATLAS results (although not the one that will appear only next week 😉

53. **Peter Woit**
February 27, 2011

Sven,

The latest ATLAS paper is out this weekend, see the link I added to the posting. Get to work….

54. **Bernhard**
February 27, 2011

Sven,

You have a very selective way of arguing.

The test of a good theory is whether it can predicts. The only semi convincing point you gave in the earlier post was the prediction of the top-quark mass, but this is a half merit since the particle itself was predicted by the SM.

The cherry on top is abandoning naturalness and at the same time brag that the model can “easily produce a Higgs mass value high enough”.

55. **Eric**
February 28, 2011

Peter and Bernhard,

The current constraint on the Higgs mass in three-generation SUSY models is that it should be less than 130 GeV. A Higgs mass of around 120 GeV is well within this range, and so I believe it is completely incorrect to claim that this somehow makes a SUSY solution to the hierarchy problem unnatural. If the experimental Higgs mass limit ends up being pushed above 130 GeV, then perhaps this will be the case, but until then I think it would be a good idea for you to stop exaggerating the issue.

As for Arkani-Hamed, I believe that he was also exaggerating the problem in the talk you mentioned in order to sell his own alternative models. Other theorists such as Ellis would disagree with him, as do I.

56. **M. Wang**
February 28, 2011

I am a financial statistician. Just want to point out a problem in Sven’s latest comment here.

You seem to be saying that your latest model has only 3 or 4 parameters and therefore can produce quite decent statistical significance. This is actually a common folly in the trading business. People spend months looking for the “right” model that gives high R^2. They forget that their own efforts at excluding unsuccessful models should be taken into account. This is called the data-mining problem. Even in a theoretical framework with zero predictive power, careful structuring of the model can always generate nominal statistical significance to any degree you want, as long as you look hard enough.

57. **Anonymouse**
March 5, 2011

Sven,

You might not want to go too far down the path of arguing against naturalness considerations. The fact that (as you yourself argued above) even the MSSM has an upper limit on the Higgs mass is only because someone like you has chosen to apply an upper limit to the stop masses.

Make the stop masses the GUT scale, and you can get essentially any Higgs mass you like (within reason of a perturbative quartic). Then your favorite “prediction” of the MSSM fades away.

58. **Jaykov Foukzon**
March 5, 2011

SUSY has no reliable and clear physical motive. It has arisen only from desire some mad Russian mathematicians
http://ufn.ru/en/articles/2001/9/f/
to expand a class of renormalizable models of canonical QFT. But mathematic does not work if basic physics idea Not Even Wrong.

59. **Christopher Lester**
March 7, 2011

There are some comments in this blog asking to see the effect of the lastest ATLAS 0-lep susy exclusion data on the CMSSM fit … if you are still interested, see the first paper on that topic here:

http://arxiv.org/abs/1103.0969

(btw – I should declare a self interest as an author in the above)

60. **Peter Woit**
March 8, 2011

Thanks Christopher,

If I’m not mistaken, figure 3a in your paper answers the question I’ve been asking about comparing the ATLAS exclusion results to earlier estimates of SUSY parameters. It looks like very roughly half of the probability-weighted parameter space is now excluded (although a much smaller fraction of the size of parameter space). Presumably over the next year or so we’ll see results that cover much of the rest…

61. **purple**
March 8, 2011

Thanks for taking the time to discuss and share this with us

62. **Jaykov Foukzon**
March 11, 2011

Physicists joke again:

Extra dimensions of space, for instance, which are predicted in many forms of string theory (a variant called M theory requires 10 spatial dimensions rather than the familiar three), could be accessible at the Large Hadron Collider (LHC), said Barnard College physicist Janna Levin. In the LHC’s unprecedentedly high-energy experiments, some products of particle collisions could go missing, having vanished into those extra dimensions.

http://www.scientificamerican.com/ar…SA_DD_20110310

Space-time, Quantum Mechanics and the Large Hardon Collider

February 28, 2011
Categories: Favorite Old Posts, Uncategorized

The title of the posting is that of Nima Arkani-Hamed's public lecture last week at the IAS, with the spelling that of the title at the beginning of the video (available here, lower resolution version here).

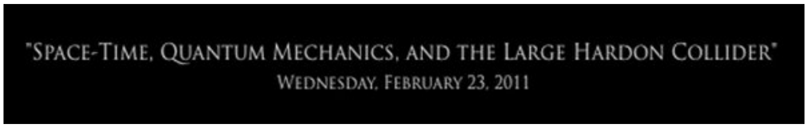

The bulk of the talk is devoted to expounding the idea that the central problems of fundamental physics are two hierarchy problems, that of the CC (why isn't it at the Planck scale?) and that of the Higgs mass (why isn't it also at the Planck scale?). Given that we don't understand quantum gravity, and don't know that the Higgs phenomenon is due to an elementary scalar, it's not clear to me that these are yet real problems. In any case, Arkani-Hamed gives the anthropic multiverse argument for the CC problem, and claims that if the LHC doesn't see supersymmetry or large extra dimensions, then we're stuck with the anthropic multiverse argument also for the electroweak scale.

The LHC only puts in an appearance in the last fifteen minutes of an hour and a half talk. Back in 2005 (see his talk at Strings 2005) Arkani-Hamed claimed that we would know whether supersymmetry solves the hierarchy problem within a year or so of first collisions at the LHC (then scheduled for summer 2007). Now that initial results from the LHC are in, showing no evidence of supersymmetry, his estimate is:

> We're going to have answers one way or another to this question on the time scale of 2020.

One of his slides estimates production of 1 squark/minute given 1 billion collisions/sec, which would mean about 50 squarks already produced in each detector. While it's true that the LHC won't be running at full energy until 2014, no explanation is given for why we need to wait until 2020 to find out about supersymmetry. Back in 2005, before the machine was turned on, enthusiastic predictions of quick results were being made. Now that the data is coming in, the story seems to have changed.

Update: Nature News has a new article up by Geoff Brumfiel: Beautiful theory collides with smashing particle data (also available here). While Arkani-Hamed is arguing that one will have to wait until 2020 (the sLHC perhaps?) before knowing whether supersymmetry is at LHC energies, John Ellis appears willing to give up much earlier, maybe the end of next year:

> "I'm wouldn't say I'm concerned," says John Ellis, a theorist at CERN, Europe's particle-physics lab near Geneva, who has worked on supersymmetry for decades. He says that he will wait until the end of 2012–once more runs at high energy have been completed–before abandoning SUSY. Falkowski, a long-time critic of the theory, thinks that the lack of detections already suggest that SUSY is dead.
>
> "Privately, a lot of people think that the situation is not good for SUSY," says Alessandro Strumia, a theorist at the University of Pisa in Italy, who recently produced a paper about the impact of the LHC's latest results on the fine-tuning problem. "This is a big political issue in our field," he adds. "For some great physicists, it is the difference between getting a Nobel prize and admitting they spent their lives on the wrong track." Ellis agrees: "I've been working on it for almost 30 years now, and I can imagine that some people might get a little bit nervous."

The article ends with a very sensible quote from experimentalist Chris Lester, who evidently doesn't share Arkani-Hamed's view that it's SUSY or the Multiverse:

> "Plenty of things will change if we fail to discover SUSY," says Lester. Theoretical physicists will have to go back to the drawing board and find an alternative way to solve the problems with the standard model. That's not necessarily a bad thing, he adds: "For particle physics as a whole it will be really exciting."

Update: It seems that the video files have been temporarily removed, presumably for editing. I fear that some poor tech person is having a bad morning…

Update: New video files with typo fixed are now available.

Comments

1. **Kea**
   February 28, 2011

   AAAARRRGH! Nima of all people should be taking the twistor geometry message seriously: ie. the right spaces for calculating twistor amplitudes depend on particle number and holographic principles, NOT on an a priori local spacetime with fairy fields attached.

2. **Chris Oakley**
   February 28, 2011

   Right – at first I thought that there was a Large Hardon Collider that fulfils all Nima's dreams/fantasies. Now I realise that it is probably a mis-spelling.

3. **anon**
   February 28, 2011

   Much in the same way that God implanted fake dinosaur bones in the ground as a test of faith, so too has SUSY arranged to trap the unfaithful. The likes of Ellis will abandon hope over the next few years, and will be left behind when the super-Rapture arrives in 2020.

4. **Thomas**
   February 28, 2011

   So SUSY or nothing for Arkani-Hamed?
   How many more physicists will give up on their field if there is no SUSY?
   What a waste of talent and experience it is, to just throw up your hands like that.
   What a perverse way of saying that you are wrong; to say that there is no answer.
   And what an arrogant way to do physics; to claim the unknown and call it yours.

5. **realistic**

February 28, 2011

Let's not get carried away with `Hardon'. Tim Bishop, a Congressman for Long Island, NY, USA, is fighting against budget cuts which might cost 930 jobs at BNL. He speaks of the negative impact this would have on the "Realistic" Heavy Ion Collider.

http://northshoresun.timesreview.com/2011/02/6698/breaking-news-bishop-says-gop-budget-cuts-would-cost-bnl-1000-jobs/

"It could also result in BNL shutting the doors on the National Synchrotron Light Source (NSLS) and Realistic Heavy Ion Collider (RHIC) machines, world class research facilities," according to a Bishop press release.

One can laugh, or scoff, but that will be small comfort to the 930 jobless at BNL. Even smaller comfort of you're one of them. And it *will* be a real loss for science, realistic or not.

6. **Peter Woit**
   February 28, 2011

   realistic,

   I'll try and write something here about the current situation of the US federal budget when it becomes a bit clearer what the implications for science are, and suggest comments on this topic should wait until then. At the moment, as far as I can tell, no one knows what will happen, with the extreme House budget presumably DOA, and legislators fighting over government funding not for next year or next month, but for this weekend. Strange way to run a society…

7. **Shantanu**
   March 1, 2011

   Peter or others, what about other implications for models such as technicolor, Little higgs models etc based on current LHC data?

8. **Geoff**
   March 1, 2011

   Shantanu, many little Higgs models predict W' and Z' bosons, and they have been ruled out up to fairly large mass values.

   The experimental situation is simply that there is no deviation from the standard model at all, whereas all theoretical models predict such deviations.

   Both the Higgs and supersymmetry were predicted because the standard model cannot be all there is. Now experiments are showing the opposite. The only thing one can do, if really nothing is found, is to go back to these predictions and check whether they are water-tight.

   In a sense, this is what Peter is doing, especially when he criticizes the supersymmetry fans.

9. **csrster**
   March 1, 2011

   In case anybody is interested:
   http://www.googlefight.com/index.php?lang=en_GB&word1=large+hadron+collider&word2=large+hardon+collider

10. **Bernhard**
    March 1, 2011

    Peter,

    Thanks for the news and the video. One thing I find worth discussing is Alessandro's comment "This is a big political issue in our field,"

    It's a interesting point because many people in power right now are SUSY or string proponents and I just hope more people will be willing to take Ellis's example and throw the towel if SUSY is not found at the LHC.

    At the same time what gives me really high hopes is that eventually these people will go into pension and the new generation will not swallow theories so experimentally discredited (if that really happens, of course). This is the same problem of the multiverse "revolution", a movement of old theorists giving up altogether to impose that if they couldn't make it than the answer is likely that there is no answer.

11. **Bernhard**
    March 1, 2011

    Shantanu,

    I think nothing really conclusive (at least not new) can yet be said about these models. Differently from SUSY, 35 pb-1 are not enough to improve limits in a significant way:

    Have a look:

    http://arxiv.org/abs/hep-ph/0402037

    http://cdsweb.cern.ch/record/1259930/files/

12. **fatdog@yale.edu**
    March 1, 2011

    Very smart people whose main concern is winning a Nobel Prize usually finish their unimaginative careers as deans.

13. **SUSY**
    March 1, 2011

    The news of my death have been greatly exaggerated.

14. **Giotis**
    March 1, 2011

It’s not a coincidence that both explanations (SUSY, anthropic) are provided by String theory. This is because String theory is currently the only consistent theoretical framework with enough explanatory power to provide potential answers to such deep questions.

15. **Jens**
March 1, 2011

Giotis,

Or perhaps String Theory has so little explanatory power that it’s consistent with anything.

16. **Tom**
March 1, 2011

I amnot a SUSY advocate & have worked on many BSM ideas over the years so will claim to be an agnostic as to what new physics exists beyond the SM. However it is fair to point out that SUSY is NOT mSUGRA (or even the MSSM). If you give up on the very simple mSUGRA idea there is plenty of parameter space still available where sparticles can be light & missed (so far) by the LHC. The problem is that (the 4-parameter) mSUGRA has been pushed so hard for so long by advocates that people forget about this.

17. **Peter Woit**
March 1, 2011

Tom,

The initial data has ruled out only a part of even the CMSSM region in parameter space promoted as most likely, so it’s certainly too early to announce the death of SUSY. But, presumably over the next couple years we’ll see much of the rest ruled out, as well as other parts of parameter space.

Since the MSSM parameter space is so large (and extensions of it even bigger), SUSY diehards will be free in coming years to keep pointing out that possible places for it to hide remain. While this is going on, it seems to me a good idea to look at what the same people were saying before LHC turn-on. I found the difference between Arkani-Hamed 2005 (answer 1 year after first collisions) and Arkani-Hamed 2011 (answer 10 years after first collisions) quite striking, and not fully explicable by the fact that the initial run is at half energy and luminosity is ramping up more slowly than some might have expected.

18. **chris**
March 1, 2011

the tension is mounting not because susy is under scrutiny – our understanding of physics is. regardless of what it might be, we did not see any sign of new physics at LHC yet – safe for some high multiplicity and heavy ion stuff.

the news is not so much that susy is still not found but that it takes a pretty darn large stick to beat old standard model dead at the energy frontier.

19. **SUSY**
March 1, 2011

People get a bit impacient, don’t they? SUSY is dead with this little integrated luminosity? Gimme a break!

20. **Bernhard**
March 1, 2011

I didn´t save Nima´s talk. Can someone share?

21. **Somdatta**
March 1, 2011

Susy will most definitely be found.

22. **Peter Woit**
March 1, 2011

Bernhard,

Sorry, but I don’t think I should use this blog to provide or exchange info about bootleg copies of something belonging to the IAS. I’m assuming that they’ll soon provide the full video again themselves, after a very small amount of editing…

23. **Bernhard**
March 1, 2011

I understand, Peter.

24. **Giotis**
March 1, 2011

If somebody is interested in the anthropic alternative (a la Bousso-Polchinski) to low SUSY for solving the hierarchy problem, Higgs mass etc, he could check this original paper by Silverstein http://arxiv.org/abs/hep-th/0407202. It is well known that low SUSY was never a prediction of String theory. It is just a reasonable hypothesis; a bonus if you like. SUSY could break at String scale for example as the paper explains.

25. **Bernhard**
March 1, 2011

Giotis,

If low SUSY was never a prediction from String it´s even worse. SUSY is a testable theory by the prediction of TeV scale particles. If you are not a fanatic neither pro or against it, one can even say that it can be ruled out. Part of the problem Peter is trying to draw attention to, I believe, is regarding the sudden efforts by some of its leaders to change their minds about this. But that´s point particle theoryl, where one can discuss, agree, disagree and make bets but in the and it will be up to the LHC to define, if SUSY, technicolor or something else.

With Strings there are no tests to be made, since the theory cannot be falsified. You can get any answer you want. If SUSY is there, good for Strings, if SUSY is not there good for Strings too.

26. **Chris Austin**
March 1, 2011

chris,

"safe for some high multiplicity … stuff"

Are you thinking of the right-hand graphs on pages 8 and 9 of [ATLAS-CONF-2010-088](), or is there something else at high multiplicity? And if so, where?

Thanks in advance

27. **Peter Woit**
March 1, 2011

Giotis/Bernhard,

For a while 6 or more years ago, Susskind, Douglas, Dine and others were quite excited about the idea that string theory could predict the SUSY breaking scale statistically: you count "string vacua" and take into account anthropics. It quickly became clear this doesn't work, see for example

http://www.math.columbia.edu/~woit/wordpress/?p=182

Like everything else, the SUSY breaking scale can be absolutely anything at all in string theory, with no way to decide among the possibilities. There is one thing predictable here though: if susy does show up at LHC energies string theorists will claim this was a prediction of string theory, but if it doesn't they'll claim that string theory made no such prediction.

28. **chris**
March 2, 2011

Chris Austin,

i was thinking about the CMS observation – it is e.g. the first reference in a paper by Shuryak arXiv:1009.4635.

most likely it is the first observation of a QGP in pp colissions, so nothing too exciting, but kind of new physics in some sense.

29. **Chris Austin**
March 2, 2011

Thanks

30. **Roger**
March 2, 2011

> Geoff says: Both the Higgs and supersymmetry were predicted because the standard model cannot be all there is.

That is true about Higgs, but not supersymmetry. Supersymmetry was predicted for entirely aesthetic reasons, and not because of any standard model shortcoming.

31. **Eric**
March 2, 2011

Roger,

Supersymmetry is predicted because it provides a solution to a big problem in the Higgs sector of the Standard Model. A Higgs which is a fundamental scalar receives large loop corrections which should push its mass to the Planck scale. These corrections may be cancelled by introducing supersymmetry partners. This is called the hierarchy problem and is one of the main topics mentioned in this post.

32. **Roger**
March 2, 2011

Yes, the post talks about the hierarchy problem, and beliefs that people have about it. But it is not a problem with the SM or with any experimental outcomes. It is only a problem with the beliefs that some people have. If I am wrong, please point me to something that shows that I am wrong.

33. **Eric**
March 3, 2011

Roger,

No, the hierarchy problem is not about belief. It is quantifiable, mathematical problem with the Standard Model + Higgs. SUSY provides one way of solving this problem. It is not the only solution, just the one that has been considered most likely. I think what you may be trying to say is that it is a theoretical problem rather than a problem with experiment.

34. **Roger**
March 3, 2011

I am not sure why you would rather call it a theoretical problem than a problem about beliefs. It is not even a theoretical problem unless you have certain unverified beliefs about how things ought to be. At any rate, I guess that you are agreeing that it is not a problem with any observed physics.

35. **Bernhard**
March 3, 2011

Eric,

As you already know some people disagree that SUSY offers a solution to the hierarchy problem, and as you also know I include myself among them. But in any case let's not go there, as we know each others arguments (but I believe you agree with me this is a clear object of dispute).

In any case: “not the only solution, just the one that has been considered most likely.”

Likely by whom? Are you talking about Ellis, Arkani-Hamed et al? Sure. Or are you talking about particle physics community? Then, not so sure.

By the way, what is the likelihood of this “solution” being right given the already amount of fine tuning needed? 10%? 50%? 90%?

Let me know, since you see this as a clear “quantifiable, mathematical problem”…

36. **Eric**
March 3, 2011

Bernhard,

Yes, I would assert that supersymmetry has long been considered by the particle physics community as a whole as the most likely solution to the hierarchy problem since at least the 1980’s. The only other serious contender is technicolor, but it has had problems satisfying precision electroweak measurements, as well as other issues. More recently, there have been solutions involving warped extra dimensions, and there is always the possibility of (drastic) fine-tuning with just the SM.

The degree of fine-tuning in minimal supersymmetric models required at present is rather mild, of the order of 10%. It is still very easy for SUSY models to satisfy all experimental constraints and produce a Higgs mass ~115 GeV. If there happens to be additional couplings which are ignored in the minimal models, even the 10% degree of fine-tuning can go away.

37. **Peter Woit**
March 3, 2011

What I’ve found surprising, for about 30 years now, is that the “hierarchy problem” has been considered something especially significant. It’s only a problem if you have a unified theory you believe in, with quantum gravity at the Planck scale, and electroweak symmetry breaking due to the dynamics of an elementary scalar at the 100 GeV scale. If you don’t think you understand quantum gravity and how it unifies with the SM, or if you think electroweak symmetry breaking may be due to something more interesting than an elementary scalar, there’s no problem. Put differently, if you don’t think you understand what is responsible for the electroweak scale or the Planck scale, that they’re very different is not a “problem”, it’s just part of what you don’t understand.

38. **Eric**
March 3, 2011

Peter,

The hierarchy problem is a problem in QFT with the Higgs being an elementary scalar. If this is the case, then the Higgs mass will receive large quantum corrections, regardless of whether or not we understand anything about quantum gravity. Unless there is some low-energy scale at which these corrections are cut-off, then there is no reason why the electroweak scale should be ~100 GeV, rather than some arbitrarily high energy.

39. **Peter Woit**
March 3, 2011

Eric,

Sure, the whole set-up of electroweak symmetry breaking via an elementary Higgs field, with weak-couplings that make perturbation theory valid, has a fine-tuning problem. This just seems to me an argument that probably something more interesting is causing electroweak symmetry breaking. Hopefully the LHC will soon start giving us hints about this.

40. **Seesaw**
March 3, 2011

A simple seesaw already shows clearly the hierarchy problem. No need of quantum gravity or GUTs.

41. **Peter Woit**
March 3, 2011

Seesaw,

Again, that’s a hierarchy problem you’ve introduced by introducing some speculative new physics at a speculative new energy scale. Until we have some better evidence for that new physics, it seems to me the “hierarchy problem” is with your speculation, not with the physics we know about.

42. **Bernhard**
March 3, 2011

“Supersymmetry
The most popular model beyond the Standard Model
is unquestionably supersymmetry. Its motivation is to
ameliorate, not solve, the gauge hierarchy problem.”

This is from Frampton in 1997:

http://arxiv.org/PS_cache/hep-ph/pdf/9706/9706237v1.pdf

I think that opinions changed drastically after LEP, so I strongly disagree with your vision about what people in general think about SUSY solving or not the hierarchy problem. But OK, this is not a very solid thing to discuss.

But Peter has as point. This is a problem only if you are sure that electroweak symmetry breaking is caused by some scalar field, that by the way, unless you are talking about bound states, was never seen in nature at all.

You create a solution to a problem (hierarchy), that solutions has another problem (large quantum corrections to the particle that its essential for your solution (Higgs) and to solve this you create a trick that should solve everything (supersymmetry). If the trick is true you should see evidence of it, but you don´t. So you accept the fact that the trick is not perfect (little hierarchy problem) and go on.

One begins to wonder if your original solution to the electroweak breaking was correct in the first place. Perhaps there is something else that you missed and makes the need for constantly repairing the theory unnecessary. I don´t have this solution (unfortunately) but it´s conceivable that the LHC data will gives us a good hint.

43. **Roger**
March 3, 2011

I looked at the above Frampton paper. He explicitly says that the motivation for susy is “not from the physics end. It does seem, on aesthetic grounds, that supersymmetry is likely to be used by Nature in Her fundamental theory.” From this I conclude that susy is only likely if you have certain mystical beliefs. If I am wrong, I would like to see the paper showing that I am wrong.

44. **Bernhard**
March 3, 2011

Roger,

I believe what Eric was trying to say is that if you assume that the cause of electroweak symmetry breaking is a scalar field, then you have a mathematical problem, for you expect a number and you get another one that is way larger than this expectation. This is not a mystical belief. It´s to solve this discrepancy that people invented supersymmetry and if it´s elegant or not, well depends who is looking at. Now, if you should assume that the origin of the electroweak symmetry breaking is indeed this scalar field in the first place is a whole different story, as this might well be the wrong assumption. But this assumption is not mystical in nature.

If you want to see mystical assumptions in physics talk with String theorists since their best argument seems to be a mystical feeling that the theory is right and that non believers in Witten are infidels 😀 .

45. **Eric**
March 3, 2011

Bernhard,

Yes, I agree with you that the argument that SUSY solves the hierarchy problem is on less solid ground since LEP and also with the latest results from the LHC. This undoubtedly causes some people to worry, probably more than they should at this point. My point is that SUSY still can solve the hierarchy problem, though not as naturally as before. However, it is still the best and most likely solution to the hierarchy problem.

You and Peter are absolutely correct that the hierarchy problem arises only if the Higgs is a fundamental scalar. Technicolor models attempt to avoid this by having the Higgs be a composite state of some strongly coupled sector. Other variants of this idea are top condensates.

I should also say that there is no requirement that SUSY must solve the hierarchy problem in a completely natural way. The important thing is that it can get the job done.

46. **Seesaw**
March 4, 2011

Eric says “From this I conclude that susy is only likely if you have certain mystical beliefs.” What you call mystical beliefs other people call beauty and theoretical ingenuity. It can fail but it can also bring glory. Examples abound and they should be familiar to all the readers of this blog.

47. **Seesaw**
March 4, 2011

Sorry Eric, I meant Roger!

48. **Seesaw**
March 4, 2011

By the way, it would be good that those impatient people who want to prematurely exclude SUSY remember the long story of failed expectations regarding the top mass value. Of course the theoretical need for the existence of the top affects the picture. Take that need out and many people would have considered the top as a purely mythical belief and bla bla bla. History repeats itself.

49. **Emile**
March 4, 2011

Hi Peter,

There is something I don’t get about what you wrote: “Again, that’s a hierarchy problem you’ve introduced by introducing some speculative new physics at a speculative new energy scale. Until we have some better evidence for that new physics, it seems to me the “hierarchy problem” is with your speculation, not with the physics we know about.”

Maybe it is just some distinction between the hierarchy problem and the naturalness problem.

As you know, the SM, something we’ve tested quite well, includes a scalar field. If we don’t find that scalar field, the SM is wrong. You also know that the corrections to its self-energy increase in proportion to the energy cutoff you use. I don’t need to introduce speculative new physics or hierarchies for this statement to be true. Nothing in the SM prevents me from doing calculations at high energies. Nothing in the SM says that I can only use it below 1 TeV or 100 TeV or whatever. So the SM has a theoretical problem in that the Higgs’ mass is “unnaturally” small. It could be that this is just the way the Univserse rolls… Of course, anthropic arguments will immediately explain the fine tuning 🙂

50. **Peter Woit**
March 4, 2011

Emile,

I’m not sure what your question is…

One point is that the “naturalness” problem is just one of several reasons to be unhappy with an elementary scalar Higgs (others are the renormalization group behavior of its couplings, and the way Yukawa couplings are arbitrary). It might be a good idea to wait and see if the LHC shows the Higgs to be an elementary scalar at TeV energies, or gives us a hint about something more interesting. If it still looks like an elementary scalar, I’ll still be unhappy about the state of the theory, with “naturalness” not the most serious problem to my mind.

If you instead want to look at the problem as a "hierarchy problem", i.e. why are two hugely different energy scales kept separate, you first need to be sure there's a second energy scale (i.e. we have no actual evidence for GUT or Planck scale physics).

51. **Bernhard**
March 4, 2011

"If we don't find that scalar field, the SM is wrong."

Emile,

That´s not true. Would mean only that we got wrong this specific sector of the model. It ´s like having U(1)-q X SU(WRONG). If SU(WRONG) is wrong does it mean that QED is wrong? Of course not. So of course it is conceivable to have the SM with another origin for electroweak symmetry breaking.

52. **Emile**
March 4, 2011

Bernhard,

I guess we have a different definition of what is commonly referred to as the Standard Model. Take most (any?) text books and what constitutes the SM is clearly defined: SU(3)xSU(2)LxU(1)Y, has 3 generations of fermions, a Higgs boson etc. If you want to extend this definition to the point where we can get the the gauge groups wrong, and still call it the SM then I guess it is up to you. Not finding the Higgs would mean that many text books would need to be revised… I really like the fact that the SM (according to my definition of what the SM is) has a scalar field in it and that I can go and look for it and possibly exclude it. That you could still say that the SM is alive and well even if we don't find the Higgs is precisely what I don't like about certain theories. You can't kill them…

53. **Emile**
March 4, 2011

Peter: I guess I was trying to make the point that there are theoretical issues with the SM even without the need to speculate about new physics or other energy scales. I thought what you wrote sounded as though we only ran into problems with the SM if we assumed physics beyond the SM.

54. **Bernhard**
March 4, 2011

Sigh..

I was really hoping here that you would not come with this "argument".. If you want to discuss the label SM fine. Then you are saying that if I come up if a model that has exactly the same fermion, quark, group sector, everything and has a different mechanism of electroweak symmetry breaking that actually INVALIDATES, i.e., makes it wrong (if you want to be picky about wording here it goes too) that other model identical to the first one in every single aspect but this sector? OK, then with this twisted way of seeing things and with a precious caution about how you name it and nothing else in mind but this, then I guess you´re right. My bad.

55. **Eric**
March 4, 2011

Dear Bernhard,

It might help if you actually specified the alternative to the Higgs mechanism that you are thinking about and said something about whether or not this can be fit into the minimal SM as is. Also, please enlighten us as to whether or not your alternative mechanism can do completely the same job as a fundamental scalar and still satisfy experimental constraints.

56. **Giotis**
March 4, 2011

Peter really, what do you mean when you say "speculative new physics at a speculative new energy scale"?

I don't suppose you imply that maybe the SM is all there is? Do you?Regularization is not a trick, is the admission of the SM that it holds up to a certain energy scale. Without this admission it wouldn't make any sense.

57. **Peter Woit**
March 4, 2011

Giotis,

My problem is with arguments like: "we must explain why the electroweak breaking scale is so much smaller than the GUT scale!" or "we must explain why the electroweak breaking scale is so much smaller than the Planck scale". We have no evidence at all for what physics is like at those scales, just speculation about their significance. Claiming that you've identified one of the most important problems in fundamental physics, but basing it on some purely speculative sector of the theory that you have no evidence for seems to me unconvincing.

The question of how high in energy we can extrapolate the SM before running into various problems is certainly interesting, but seems to me different than the usual "hierarchy problem".

58. **Bernhard**
March 4, 2011

Eric,

The SM is a model composed of parts, parts that fit together but even the anomaly cancellation is "trivial" (in the sense that they are applied separately for each family) and the Higss sector appears as an almost plug-in accessory that sophisticated and clever as it is, might be incorrect. I really don´t think that it is correct to affirm that not finding the Higgs makes all the success in explaining every single experiment so far a failure, because statements like "if we don´t find the Higgs the SM is automatically wrong" pass this idea. I think it is conceivable that this specific mechanism could be clever but wrong, and my original discussion with you was the need for SUSY, a theory with problems of its own, to correct it. So the SM Higgs create something already begging for physics BSM, and I´m just saying that this might be actually a longer shot away from the SM that is actually needed, but of course I don´t know that, SUSY, Higgs and cia might all be there and and if that happens to be confirmed by experimental evidence I will happily acknowledge it. I however I´m not so much without imagination to think that an alternative is impossible and even if it happens to be a solution less simpler than the Higgs I will not have a sudden epiphany that the SM is completely wrong since its precious Higgs was not found.

59. **Eric**
March 4, 2011

Hi Bernhard,

I certainly agree with you that there are alternatives to the simple Higgs mechanism which don't involve a fundamental scalar. The problem with these, generally, is they also introduce a lot of BSM physics which introduces problems of their own. For the most part, these problems are much more severe than a SUSY + Fundamental Higgs scalar solution. However, it is not at all impossible that LHC will find technifermions or something similar which will point directly to something besides SUSY. Personally, my opinion is that if the simplest ideas with a three generation SM + fundamental Higgs + SUSY doesn't pan out, then the most likely possibility is that in addition to the above there is a fourth generation. Because of the large Yukawa couplings required for the fourth generation, the fourth generation quarks become strongly coupled and can play a role in EWSB. In any case, what we know is that it's extremely unlikely that the SM is all there is, unless things are just extremely fine-tuned.

60. **Bernhard**
March 4, 2011

Eric,

"In any case, what we know is that it's extremely unlikely that the SM is all there is"

Agreed 100%!!! 🙂

61. **Roger**
March 4, 2011

Seesaw says that what I call mystical beliefs, other people call beauty and theoretical ingenuity. Maybe so, but I don't see any good reason to doubt that the SM is all there is.

62. **Anonymouse**
March 5, 2011

The Standard Model includes the Higgs. Like it or not, that is the definition. No Higgs means no Standard Model.

The hierarchy problem is nothing more than fine tuning. As Peter correctly has stated, it is not a problem unless there is some kind of high scale physics beyond the Standard Model. I personally think it is likely something like that exists, but there is no evidence as of yet.

The Standard Model includes exactly one dimensionful parameter, which is the Higgs mass parameter (or alternately, the VEV which determines the Z and W masses). There are no large quantum corrections to it. What you call large quantum corrections are just the renormalization required to move from an unphysical number in a Lagrangian to a physically measurable quantity (such as any of the masses listed above).

Even perturbative gravity is soft, and does not correct the Higgs mass parameter by a positive power of M_Planck. (Gravitational couplings are negative powers of M_Planck). Of course, there may be non-perturbative gravitational phenomena which upset this, so the best we can do is to say that there is no obvious contribution to the Higgs mass from quantum gravity. Without a verified quantum theory of gravity at small distance scales, there isn't much more one can say.

But once there is new physics with massive particles, such as super-particles, stringy states, GUT bosons, right-handed neutrinos for a seesaw, or anything else with a relevant interaction with the Higgs, the hierarchy problem exists. So it is fair to say that SUSY must first create the problem before it solves it.

If we see some clear sign of heavy physics, any at all, this situation will change. Neutrino masses don't work because there need not be any heavy new degrees of freedom.

63. **Bernhard**
March 5, 2011

Anonymous,

I agree with your point. I was imprecise to exclude the Higgs in the framework of the SM so I take it back. Emile, I acknowledge you also had a point. I guess I just wanted to say I didn´t want to have a pointless discussion about naming, since if want really to be picky neutrinos oscillations, even if they don´t include new degrees of freedom are enough to say we have physics BSM and that neither this nor an absence of a Higss sudden turn the

64. **Bernhard**
March 5, 2011

(sorry, sent too early)

SM wrong. I agree however that the absence of the Higgs would require solving a much harder puzzle to maintain the SM minimally affected. That is, a new mechanism of electroweak symmetry breaking that does not destroy the current structure is a a tough problem.

65. **Bernhard**
March 5, 2011

…and a just extending a little bit the last thing I want to say about this, neutrino oscillations for example already require a significant change in the SM since something that was supposed to be conserved (lepton family number) was violated. One can require total family number still to be conserved but that´s characteristic already of many BSM models, like the 331 model.

66. **OhDear**
March 5, 2011

"something that was supposed to be conserved (lepton family number) was violated"

Sure, but what do you mean by "supposed to be" ? Before we found neutrino oscillations it just happened to be true that lepton family number was conserved. Then it turned out it wasn't true, and I'm tempted to say "big deal". The essence of the standard model is the gauge group, the specification of the matter particles, and the higgs mechanism. All else is just detail. Neutrino masses can be accomodated in the same way as quark masses. Mind you, nobody really believes that the SM way of giving fermions masses is correct.

And to Roger, who sees no reason why the SM cannot be all there is – what about gravity? There is no gravity in the SM.

67. **Bernhard**
March 5, 2011

ohdear,

you should have followed the discussion, not my isolated comment before trying to be clever…

Sure I also don´t give a dime for lepton family conservation, but if you want to be precise about how the SM was DEFINED, then it was defined with a massless neutrino and a massive neutrino represents a violation of this definition. If it is to be precise and consistent I can agree, but then let´s be consistent to what the model defined not matter what.

68. **Bernhard**
March 5, 2011

and by the way, not giving a dime to lepton family number conservation can be very significant depending on the how you see it. If for example the decay "mu–> e + gamma" were to be observed you are forced to say you STILL haven´t seen any violation of the SM since you suddenly changed your mind that this principle can easily be thrown out of the window. I think most experimentalists would disagree with you on that.

69. **OhDear**
March 5, 2011

Bernhard, my point was that the SM is *not* defined to include lepton family number conservation. But you make a good point about "mu–> e + gamma". That would be a big result, but wouldn't violate my (perhaps too narrow) definition of the SM.

Short Items

March 6, 2011
Categories: Uncategorized

- There's a wonderful interview at the Notices with last year's Abel Prize winner John Tate (video here). He blames the fact that his name is on so many mathematical results and concepts on Serge Lang. The 2011 Abel Prize winner will be announced on March 23rd.
- Sir Michael Atiyah's February 1 talk at the College de France titled *A Geometer Explores the Universe* is now on-line.
- Through the intervention of mathoverflow.net, Barry Mazur managed to retrieve a copy of his 1963/64 unpublished paper that first promoted the idea of an analogy between prime numbers and knots in a 3d space.

> In 1963 or 1964 I wrote an article Remarks on the Alexander Polynomial) about the analogy between knots in the three-dimensional sphere and prime numbers (and, correspondingly, the relationship between the Alexander polynomial and Iwasawa Theory). I distributed some copies of my article but never published it, and I misplaced my own copy. In subsequent years I have had many requests for my article and would often try to search through my files to find it, but never did. A few weeks ago Minh-Tri Do asked me for my article, and when I said I had none, he very kindly went on the web and magically found a scanned copy of it. I'm extremely grateful to Minh-Tri Do for his efforts (and many thanks, too, to David Feldman who provided the lead).

For more about this fascinating topic, see a summary by Lieven le Bruyn here.

- LHC beam commissioning is now in progress, it is supposed to start colliding beams for physics again in another week or so.
- In the Dark Matter world, all eyes are on Xenon100, waiting to see what their results will be. Nature News has an update here. Next week Elena Aprile will be speaking at NEUTEL11 (which has a blog here) and revelations may occur.
- This year's Asimov debate is on the topic of string theory and whether there's any hope for a unified theory. I'll have to miss this, I'll be at local bookstore Book Culture introducing Richard Panek who is giving a talk there that evening about his recent book that I wrote a review of for the Wall Street Journal. I'll be curious though to hear from anyone who does go to the debate what they thought of it.
- Blogging may become more sporadic over the next couple weeks. If so it's because I'm on Spring Break in Paris.
- Things don't seem to have gone well for Raja of Invincible America John Hagelin and his Global Financial Capital of New York down on Wall Street. He has given up the mansion/headquarters building at 70 Broad Street, sold to a Chinese construction company. Nowadays he is President of the David Lynch Foundation and working on a much more conventional way to make a living, offering an on-line course on Quantum Field Theory, Superstring Theory, Inflationary Cosmology, and Higher States of Consciousness, $1400 if you take it for credit, $600 otherwise.

Update: A podcast of the Asimov debate is available here.

Comments

1. **Yatima**
   March 6, 2011

   QFT and Higher States of Consciousness?

   Do we really need a repeat of Scientology…

   What's with people trying to link that barely adequate debugging tool called "consciousness" to quantum whatever and claim that there are 'higher states'. For some reason consciousness being thrown overboard whole through meditation or laudanum is assigned to it being in a "higher state". It makes me nauseous.

2. **Yatima**
   March 7, 2011

   And while we are doing the laundry list:

   http://spectrum.ieee.org/aerospace/astrophysics/string-theory-made-easy

   "String Theory Made Easy: Two books tackle one of the most complex theories known to man — with surprisingly satisfactory results"

3. **pos**
   March 7, 2011

   Surely it is not necessary to focus only on the negative. I had not heard of the Asimov debates until now. Thanks to PW for bringing them to my attention. So this year's debate is about string theory (so what?) … looking at the list of debate topics in years past, there are some fascinating choices (and I missed them all, of course, and seats for this year are sold out). But anyway, Xenon100 is physics at its best, tackling a really difficult problem on the cutting edge of research, it will be fascinating to see how it all works out. My best to them.

4. **Shantanu**
   March 7, 2011

   If somone knows of a link to the webcast of the event, maybe they
   can point it out. At times I like this I wish was in NYC

5. **Bugsy**
   March 8, 2011

   I just listened to a bit of Hagelin on You Tube and find it very hard to take… it's to me unfortunate that he seems to find it necessary to forcibly "unify" two of his personal interests (physics and TM meditation), the effect of which could be to bring both into some ridicule. Physics and math are infinitely beautiful and so is human consciousness. So what? The fact that we use our reason and our intuition, our logic and our dreams, and that there are so many very different kinds of minds successfully pursuing science is fascinating. On the other hand meditation can be a valid practice for gaining deep insights into oneself or into life, and that can have profound beneficial effects for individuals who happen to find that such a practice fits them. And that could naturally include scientists (as well as artists, writers and dish washers). Why not? Poetry is also inspiring, so is listening to a bird sing or
   Brad Mehldau play the piano. But that doesn't mean we need to claim that string theory explains why Brad's or the bird's song is beautiful or why
   a proof of John Tate's is. Mashing these things together diminishes all of them, and misses the surprise of the smile that can be hidden in a line of prose, or the mystically marvelous taste of an apple pie, which could provide a wonderful moment to prove a truly new theorem, whether not not we happen to be levitating at the time.…Oh well.…

6. **Steven Colyer**
March 9, 2011

Clara Moskowitz, in her 3/8/11 article on the 3/7/11 11th Asimov Debate:

When filtered through the lens of string theory, general relativity and quantum mechanics can be made to get along.

From the article:

"The progress [in string theory] over the last 10 years has only solidified my confidence that this is a worthwhile direction to pursue."
… Brian Greene

"Are you [string theorists] chasing a ghost or is the collection of you just too stupid to figure this out?" Neil deGrasse Tyson teased, beginning a friendly banter that would continue throughout the night.

deGrasse Tyson later says if you're making progress, keep pursuing it.

7. **Peter Woit**
March 9, 2011

Thanks Steven,

I'd be curious to know what these "string theory predictions"are about large dimensions at the LHC and about structure in the CMB. As far as I know there are no such predictions, and for Freese to claim that they exist is intentionally misleading. Presumably what's involved are things that stretch the term "prediction" far away from its conventional usage.

I suspect that when Brian is referring to progress in string theory, he's thinking of things like AdS/CFT, not string theory unification. As far as unification goes, progress in the last decade has been negative (the landscape is not progress, quite the opposite).

It's funny that it looks like the organizers were only able to come up with one person willing to argue that the extra dimensions lead to deadly problems, and that's Jim Gates, who is a string theorist.

It also looks like the previously fashionable argument that "string theory predicts supersymmetry" has now vanished down the memory hole.

8. **Steven Colyer**
March 9, 2011

You're more than welcome Peter but is is we who thank you for a frank weblog about these important topics.

We have a second article up on the event, also very frank and not overstretching in the least, by participant Marcelo Gleiser at NPR Blogs: here.

9. **Geometrick**
March 9, 2011

As much as I love David Lynch, I'm a bit uncomfortable when spiritualists try to invoke quantum mechanics. I believe that Rhonda Byrne tried to use quantum physics in "The Secret." I would say more about her and her ilk, but I don't want to slander her.

10. **Peter Woit**
March 9, 2011

Steve,

Thanks again. I wrote a posting last year about Gleiser's recent book about this.

http://www.math.columbia.edu/~woit/wordpress/?p=2865

11. **Bernhard**
March 9, 2011

Peter,

Perhaps not exactly what you´re looking for but I saw a talk in Boston (SUSY 09) on "Strings at the LHC" that could interest you (if you don´t already know it):

http://nuweb.neu.edu/susy09/talks/Talk_599-L%C3%BCst.pdf

I remember Lüst said in the end "we could see something only if we are very very lucky".

Lüst has by the way some articles on the subject:

http://arxiv.org/abs/0807.3333

http://onlinelibrary.wiley.com/doi/10.1002/prop.201000024/abstract

And you can find more in hep-th.

12. **Peter Woit**
March 9, 2011

Bernhard,

I have seen that work, but I don't think anyone has ever taken it (the idea of strings with a TeV string scale) seriously or promoted it as a string "prediction" for the LHC.

13. **Coin**
March 9, 2011

Way back in 2000 when John Hagelin was running for President, the Natural Law Party campaign website had this absolutely mind-boggling diagram on it that– I am not sure I am remembering this exactly correctly– had “SUBATOMIC PARTICLES” at the bottom and “HUMAN RIGHTS” at the top. The diagram was built like a pyramid where somehow constitutional law rested on top of quantum field theory, or… something. Alas, I didn’t bother to save this masterpiece of incoherence when I first saw it, and I’ve been trying to locate a copy ever since to no avail. (The Natural Law Party website does not appear to have updated since about 2003, but somehow that diagram doesn’t seem to still be on it…)

14. **Bernhard**
March 10, 2011

Here more on the dabate:

http://www.scientificamerican.com/article.cfm?id=string-theory-debate

15. **@ Coin**
March 10, 2011

Not exactly the one you mentioned, but nearly equally hilarious (also with some Natural Law Party affiliation):

http://www.invincibledefense.org/popups/uf1.html

16. **Nigel**
March 11, 2011

“… working on a much more conventional way to make a living, offering an on-line course on Quantum Field Theory, Superstring Theory, Inflationary Cosmology, and Higher States of Consciousness, $1400 if you take it for credit, $600 otherwise.”

Just curious, but do you see stringy theory continuing its trip into pseudoscience if it fails at the LHC, just as the S-matrix continued with *The tao of physics* eastern mysticism after it was superseded by the standard model?

17. **Coin**
March 11, 2011

@: OMG, there may have been more layers in the version I saw, but that is so totally it! Except I don’t remember it being INVINCIBLE before.

18. **Shantanu**
March 12, 2011

The podcast is
http://www.amnh.org/news/tag/isaac-asimov-memorial-debate/

Peter: what do you think?

19. **Peter Woit**
March 12, 2011

Thanks Shantanu,

I’m on vacation right now, which seems like a good excuse for not listening to this at the moment…

20. **Peter Woit**
March 12, 2011

Nigel,

I think the string theorists who have gone down the route of anthropic multiverse pseudo-science are already on thin ice in terms of retaining any credibility with their colleagues. They’ll concentrate on trying to ahore up this situation by promoting the multiverse, and won’t engage in obvious nonsense like the consciousness stuff. This is not new for Hagelin, he’s been doing this for 25 years now.

21. **Giotis**
March 13, 2011

When somebody from the audience asked Marcello Gleiser about which experiment in his opinion would help most in the quest of a unification theory, he replied the detection of gravitons.

I completely agree.

There is a lot of speculation that gravity might not be a fundamental force but some emergent collective phenomenon. If this is true everything changes regarding unification and Quantum gravity.

If somehow we could verify that gravitons exist as mediators of the gravitational force then our confidence to String theory where gravitons find their natural place would increase substantially.

On the other hand if you could verify somehow that gravitons don’t exist you kill String theory automatically.

22. **Peter Woit**
March 13, 2011

Giotis,

The problem is that you can give a good order of magnitude estimate of the probability of detecting a graviton, using any kind of apparatus conceivable in you, my or our great-grandchildren’s lifetime. It’s essentially zero, so string theory remains immune from this “test” like all others. In any case, I disagree that detection of a graviton would be evidence for string theory, but that’s irrelevant since we know it’s not going to happen, whether or not they’re there.

23. **Anonymous**

March 13, 2011

Only 3 days to go before Xenon100 announces SUSY WIMP!

24. **Coin**
March 14, 2011

…dumb question… Given the kind of data produced by Xenon100/XMASS/whatever, if they got a signal would they actually be able to distinguish a SUSY/superpartner WIMP from a normal WIMP? If so, how?

25. **Shantanu**
March 14, 2011

Coin, no you cannot.
All you get from dd experiments is the mass and WIMP-nucleon cross-section.
The result is agnostic to whatever BSM theory produces this mass and
coupling constant

26. **martibal**
March 14, 2011

Peter,

why are you so pessimistic on graviton detection ? What about the detection of gravitational wave (several experiments are going on on that matter): couldn't it be related to the existence of graviton ? Or say differently, if gravity is an "emergent phenomenon" (whatever its means, I am still not sure I understand what people intend by that), would it mean that there is no gravitational wave ? Or simply that there are classical gravitational wave with no corresponding quanta ?

27. **Peter Woit**
March 14, 2011

martibal,

Classical GR effects, including classical gravitational waves, are certainly measurable, and experiments likely will see them. But seeing the quanta of any quantization of a theory with classical gravitational waves is a very different story.

28. **Paul**
March 21, 2011

Smolin's point was never really addressed in the debate. He said spacetime is dynamical, so any extra dimensions have to be dynamical too, but this leads to the extra dimensions blowing up to spacetime-size. Does anybody know how this is dealt with?

29. **Giotis**
March 21, 2011

Paul,

The overall size (volume) of the compactified dimensions is a modulus from 4D point of view i.e. a massless scalar field. This modulus is stabilized in small values by various techniques. In general a potential is obtained in 4D for the volume modulus which is then minimized in a stable/metastable vacuum and thus the field acquires an expectation value (a small size in this case). There are many geometric and other moduli in String theory compactifications; it is a very large subject called moduli stabilization.

This Week’s Hype

March 14, 2011
Categories: Experimental HEP News, This Week's Hype

The LHC is back in business, producing stable colliding beams for the first time this year, although still with a small number of bunches and thus a low luminosity. The number of bunches and luminosity will increase over the next couple weeks.

Reuters explains the significance of this, based on quotes from CERN scientists: they expect to find evidence of the multiverse as predicted in Stephen Hawking and Brian Greene’s books.

> Oliver Buchmueller, a leading physicist on the $10 billion project, said top priority in 2011 and 2012 would be finding evidence of super-symmetry, extra dimensions, dark matter, black hole production and the elusive Higgs boson.
>
> These concepts and ideas are at the new frontiers of science research as it pushes into the realms of what was once science fiction, giving a new impulse to cosmology and theorizing on whether the known universe is alone, or one of many.
>
> Cosmologists, like Briton Steven Hawking and U.S. physicist and mathematician Brian Greene, are looking to the LHC to turn up at least strong signs that there was another universe before the Big Bang or that others exist in parallel to our own

There’s no word on exactly how LHC data is going to provide evidence for the multiverse, I guess we’ll just have to wait and see.

Comments

1. **King Ray**
   March 14, 2011

   String theory is a Zombie Theory that won’t die, and keeps eating the brains of those silly enough to believe in it.

2. **UnderlyingEvent**
   March 14, 2011

   Hi Peter, Where exactly do you get your latest LHC news from? Like the fact that beams are colliding now, and what the luminosity is.

3. **Glenn**
   March 14, 2011

   Perhaps John G. Cramer’s prediction will come true after all?

   http://en.wikipedia.org/wiki/Einstein%27s_Bridge_(book)

   Of course, in that case, the proof would exist on a world-line inaccessible to any living observer.

4. **Gerrid Hargors**
   March 14, 2011

   is there a chance that the LHC might find absolutely nothing?

   when would we see this?

5. **Sakura-chan**
   March 14, 2011

   Even if it finds nothing that’s still something.

6. **Christian Takacs**
   March 15, 2011

   It is very possible to write a brilliant, internally consistant, story in the english language which is incredibly detailed and resembles reality in some ways, but only so much as the author desires and has the skill to imagine and impart. We call this body of work a fiction or fantasy, and no one seems surprised it is quite possible to make things up which don’t exist but have aspects of realism.

   It is very possible to write a brilliant, internally consistant speculation in the language of mathematics which is incredibly detailed and resembles reality in some ways, but only so much as the mathematician or physicist desires and has the skill to imagine and impart. It appears many in the mathematics and physics community are confused about what to call this growing ‘speculative body of work’, while others seem quite surprised (and unwilling) to consider it is quite possible to make things up which don’t exist but have aspects of realism…even in mathematics.

7. **DB**
   March 15, 2011

   I expect that your colleague Brian Greene will quickly and vocally distance himself from this nonsense. Right?
   After all, surely he doesn’t want to be remembered as the mass peddler of pseudo-scientific quackery.

8. **Peter Woit**
   March 15, 2011

   UnderlyingEvent,

   At some point I started not linking to LHC info at CERN, since they were making private previously public sources when they were linked to. But it now looks like they made a long-term decision to keep public these:

LHC vistars (see Page 1, Coordination and Operation)
http://op-webtools.web.cern.ch/op-webtools/vistar/vistars.php

Latest news from daily commissioning meetings:
http://lhc-commissioning.web.cern.ch/lhc-commissioning/news-2011/LHC-latest-news.html

Philip Gibbs does a good job of summarizing the situation at
http://blog.vixra.org/

9. **Peter Woit**
March 15, 2011

Gerrid,

The supersymmetry, extra dimensions, etc. is all a side show,these are not well-motivated ideas and they will just get slowly ruled out to higher and higher energy levels. The serious thing to watch is the Higgs. Either it will be found, or our understanding of electroweak symmetry breaking is wrong, and presumably some sort of evidence will be found for what the right theory is. This should start later this year…

10. **Peter Woit**
March 15, 2011

DB,

Brian does seem to often take pains to claim he's not a proponent of these multiverse ideas, but that generally gets lost.

More importantly, I think the CERN PR people should think twice about having those who speak publicly for them heavily promoting extra dimensions, etc., and encourage them to stick to real physics

11. **pr**
March 15, 2011

The PR office has to say *something*, and it can't be simply "the LHC is looking for the Higgs boson". The PR office *has* to say "the LHC is searching for new physics, beyond current knowledge" ~ and what is there to say? It can only be phrased in terms of the prevailing ideas/fashions of the day. That's not the fault of the PR office. SPEAR was not built to search for quarks. The AGS was not built to search for quarks (or CP violation) either. They were all built to explore the unkown, which turned out to be quite a bit different from the expectations of the day. It is a pity that nothing has been found (by accelerators) beyond the SM for 30-odd years, and perhaps the LHC will be the same, but the PR office can and must speak in terms of the ideas of the times. No matter if history shows that this is a very poor guide to successful predictions.

12. **Michael**
March 15, 2011

pr has a point. The easiest and most effective way to justify the LHC now is with these speculative theoretical ideas. However, doing so is also dangerous since the speculations cannot all be right and a malicious journalist could always write a lead article "CERN scientists fail to find Supersymmetry again!" or something like that. So the hype does cost us in the long term and makes our community seem unserious or even silly. Which we are not.

Oliver Buchmüller is a serious scientist and I regret that his words were twisted by Reuters. He is a leading figure in CMS and contributes at many levels. Don't judge him by this one twisted quote.

13. **Rhys**
March 15, 2011

I agree with both pr and Michael. Of course there *are* searches for black hole production, extra dimensions etc. being performed by the LHC experimental groups, but nobody would argue that they are the "top priority", and I'll bet that Buchmueller didn't say so.

14. **dir**
March 15, 2011

if lhc finds just the standard model higgs, and nothing else, then the best understanding of the stability of the weak scale is anthropic idea, this is especially the case if the higgs mass is 143 \pm 5 gev which implies a high scale susy breaking.

15. **Giotis**
March 15, 2011

I think there is a misunderstanding here. The article explains later the line of reasoning behind this assertion, that is if SUSY is discovered this would backup String theory and String theory offers backup for the multiverse idea. In fact this is consistent with Brian Greene's book which promotes the same thing i.e. that if we could estimate by circumstantial evidences (e.g. SUSY) that String theory is on the right path then the multiverse will gain validity and a concrete theoretical justification.

Also I'm not sure if this is an actual quotation of Oliver Buchmueller; maybe only the first paragraph is his.

PS. Peter the link is faulty…

16. **Mike**
March 15, 2011

Peter,

This is off-topic so feel free to delete.

The Japan Society has created a disaster relief fund to aid victims in Japan. 100% of your generous tax-deductible contributions will go to organization(s) that directly help victims recover from the devastating effects of the earthquake and tsunami that struck Japan on March 11, 2011.

See: http://www.japansociety.org

17. **neo**

March 15, 2011

I just finished reading Greene's multiverse book. He may not be a proponent, but he is far from agnostic about it. He is very sympathetic, and most of the book is an extended defense of his view that the multiverse (he considers NINE types) is science, not pseudo-science. No where does he claim, however, that the LHC will provide evidence for it, except indirectly, such as finding SUSY particles, which he argues would weakly affirm string theory, which would (even more weakly) affirm the landscape multiverse.

18. **Roger**
March 15, 2011

> Brian does seem to often take pains to claim he's not a proponent of these multiverse ideas, but that generally gets lost.

Greene just wrote a book that consists entirely of a promotion of nine different types of multiverses. He is out giving TV interviews promoting the multiverse. It appears to me that he is more of a proponent of multiverse ideas than anyone else. Where does he take pains to deny it?

19. **Thingumbobesquire**
March 16, 2011

I guess these types never weary of peddling this kind of dross: "Large Hadron Collider could be world's first time machine"

20. **rhofmann**
March 16, 2011

Some very personal thoughts on the LHC situation: The healthy part of the electroweak (ew) part of the SM, namely an SU(2)x(effective)U(1) gauge structure, which was confirmed so many times by LEP and predecessor machines, will be reconfirmed by LHC. The ugly part – the entire Higgs sector with its implications for all sorts s of model building – will go away to eventually make room for a much more fundamental understanding of ew symmetry breaking, fermion-mass emergence and effective chirality of the weak interactions, the grasp of the latter providing an EXPLANATION for the effective anomaly cancellation in the SM. This will require a sharp break with the perturbative approach to gauge theories, and we will have to go a long cumbersome way to reach the same level of SM accuracy that was confirmed by past ew precision experiments. Apart from not seeing the Higgs the LHC may provide hints in low-energy secondaries (multiplicities) and possibly by seeing additional heavy vector triplets.

21. **Susy WIMP**
March 16, 2011

What did Xenon100 announce this morning? They haven't made anything available online yet.

22. **Peter Woit**
March 16, 2011

Susy WIMP,

See the Neutel11 blog for info on this. Xenon100 announced only that their data is not yet unblinded. Nothing yet….

This Week’s Hype II

March 16, 2011
Categories: This Week's Hype

LHC-related hype is coming fast and furious this week during my vacation, with Vanderbilt University yesterday issuing a press release headlined Large Hadron Collider could be world’s first time machine. It’s based on this paper, and the Vanderbilt press release explains:

> Weiler and Ho’s theory is based on M-theory, a “theory of everything.”

The press release has been picked up by lots of other media outlets, including CBS News and UPI.

It’s rather impressive that these tests of M-theory at the LHC will not only provide evidence for other universes, but allow time travel in this one.

Comments

1. **causality**
   March 16, 2011

   “Causality-Violating … (anything)”. Oh dear, if you can violate causality then you can probably work your way around the Second Law of Thermodynamics. Make a perpetuum mobile!

2. **anonymous**
   March 16, 2011

   I bet there are a lot of people who would like to go back in time and change their theories right now. Maybe the Multiverse folks can use the LHC to go back in time and uninvent this concept (and if they are real generous, maybe they can plant some papers in a few journals detailing how mathematically ridiculous it is).

3. **David Bailey**
   March 17, 2011

   This is an example of the scourge of image consultants! God particles, recreation of the big bang, and now time machines!

   Non scientists are not stupid, and they know that vast amounts of money are spent on these projects, and that less and less comes out of them except hype!

   The science community should realise that resorting to image consultants is like taking cocaine – a short term ‘solution’, but a long term disaster. Think for a moment about people’s current disillusionment with politicians in general – part of this is because politicians have overdosed on image consultancy!

4. **Peter Woit**
   March 17, 2011

   Dave Bailey,

   I don’t think the multiverse or outrageous claims about the LHC are the fault of image consultants. It’s physicists themselves who have gotten used to getting attention for spouting nonsense and are happy to do it themselves without any need for encouragement. The press will always lap this stuff up if physicists are willing to provide it.

5. **Michael**
   March 17, 2011

   Peter,

   it depends on which physicists you are talking about. Speaking for at least a segment of the HEP experimental community, it is extremely frustrating when some outspoken colleagues try to grab some glamor by making inflated and extravagant claims. The rest of us who are more sober and less addicted to the limelight can only suffer these fools. The cocaine analogy is a good one – some people want a high but everyone pays the consequences.

   Another example is all the hype expounded a decade ago for the ILC. The result was a proposed machine that would not have delivered the physics needed and therefore would have led to the death of the field. Instead, we suffered the embarrassment of a project that was not supported by the whole community because many people resisted the hype and did not jump on the bandwagon led a noisy and boisterous subset of the community.

   Michael

6. **Peter Woit**
   March 17, 2011

   Michael,

   I had in mind theorists, too many of whom seem to think it’s a good idea to issue press releases promoting absurdly unlikely speculative ideas. This isn’t something that I’ve seen experimentalists engage in.

7. **cf**
   March 17, 2011

   I had in mind theorists, too many of whom seem to think it’s a good idea to issue press releases promoting absurdly unlikely speculative ideas. This isn’t something that I’ve seen experimentalists engage in.

   Cold fusion?

8. **TedUnger**
   March 17, 2011

   We have been promised Earth-consuming black holes, a new Big Bang, marauding aliens from another dimension, and enough antimatter to build our own USS Enterprise

– but there's been nothing from these CERN scientists except some lousy boring data on physics! They better at least give us some time travel or else!

You know that is what Joe Public is thinking.

9. **David Bailey**
March 17, 2011

I'd say the problem is wider than high energy physics. Terms like "quantum teleportation" seem pretty absurd, when compared to the Star Trek version, and even the term "quantum computer" makes me feel a little uncomfortable, because it is far from clear if there is a realistic chance of scaling the idea up to anything resembling a computer – certainly I'd be happier if they reserved the term for something with >1000 (say) q-bits coupled together. Then of course there is the invisibility cloak…..

10. **Roger**
March 17, 2011

Michael, can you tell me some names of these sober physicists, so I can credit them? Are they willing to be quoted? As far as I can see, they all make silly and extravagent claims.

11. **Joe Public**
March 17, 2011

I have no scientific background, but this is what bothers me. 3-Dimensionality is a model for understanding our "spacial reality". I don't have a problem with claiming some newly discovered plane to be the "4th dimension" (discounting time, obviously)- what bothers me is using mathematical models with no empirical basis to make such wild and ubiquitous claims about x many new dimensions, and then the even more specious subsequent claims that spew forth…time travel, parallel universes etc. Not just claims that properly belong in science fiction, but claims that seem to be forming the basis for some new scientific religion. A very lucrative mythology for those who profit from it.

Brian Greene and friends seem to be giving "realism" its most outrageous treatment since Plato. Btw- I'm all for science, and I believe it is the best tool we have to collectively model and understand our material reality, but…why can't they see how horribly they're abusing their own concepts?

12. **csrster**
March 18, 2011

"Large Hadron Collider could be world's first time machine"

or maybe it already has been! Did they think of that?

13. **TedUnger**
March 18, 2011

I know a lot of scientists have neither the skills nor the interest in sharing their findings and knowledge with the unwashed masses. But someone has to. After all, don't you want to inspire that next great scientist? Don't you want the public to at least appreciate and understand a bit of what you are doing?

The public may be ignorant and superstitious, but guess who foots the bill for most science, and guess who takes it away when they don't get what the guys in the white lab coats are doing up in their ivory towers. Brian Greene and Carl Sagan were are treated like lepers by many of their peers for daring to reveal some of the mysteries to the people.

Apollo and SSC went away in large part because the public did not see their value. How about some of you stop whining about the publicity machine and start doing something about it? Otherwise hello new Dark Age.

14. **Allan Rosenberg**
March 18, 2011

If Weiler and Ho are right, then future string theorists will no doubt use the effect to send messages with the experimental proof of string theory back in time. Once we have that data (and who knows, maybe we already have it), nobody will need to actually perform those experiments. So much for Woit's claim that we need experiments to justify string theory.

15. **Michael**
March 18, 2011

Hi Roger,

sure – you can look at the blogs written at US LHC, for example. There is the famous blog by Tommaso Dorigo, the one by Gordon Watts, John Conway at Cosmic Variance, and perhaps even my own obscure blog.

Hi Ted,

none of us is whining about the need to promote science. I agree that it is our duty and it should be our pleasure to share our positive and privileged experiences with non-experts. I don't look down on the public nor do any of my friends in HEP. On the contrary, efforts at genuine outreach increase every year and are far, far better than anything I saw as a student. My teenage interest in science stems from popular science articles like those in Scientific American and from science instruction in school, not from Star Trek or other entertaining but purely fictional productions. It never serves science to bait with science fiction and I would resist any tendency to do so, at CERN, at Fermilab or at my own university.

regards,
Michael

16. **Bugsy**
March 18, 2011

Michael writes:
My teenage interest in science stems from popular science articles like those in Scientific American and from science instruction in school, not from Star Trek or other entertaining but purely fictional productions.

I completely agree; while I found written sci fi inspiring (not so much the
watered-down movie or tv versions, with few exceptions) it needed to be backed up with the "real thing" or I never would have gotten interested in math and science. But

back then, Sci Am was far better than now:
meatier, without the fancy graphics and sensationalism, and with the incomparable Martin Gardner in every issue.

17. **Paul**
March 18, 2011

When a physics department issues a press release, don't a bunch of physicists in the department have to sign off on it?

How many physicists would have signed off on this time-travel release?

18. **jpd**
March 18, 2011

"Large Hadron Collider could be world's first time machine"

or maybe it already will have been! Will they thunk of that?

19. **Peter Woit**
March 19, 2011

Paul,

No, in American universities, typically tenured faculty members do this sort of thing without any involvement of their colleagues. It's between them and the university administration, which typically is very interested in seeing research of its faculty promoted in the media.

20. **nonstupid layman**
March 19, 2011

Everybody knows that one of the purposes of the LHC is to "look back in time"; meaning to investigate the state of the universe (note: no plural s here !) shortly after the big bang. And it is well known too that the media reporting about different subjects (politics, economy, science, …) tend to exagerate and amplify what they are told more and more these days to sell their stories.

It is just unfair and incomprehensible of You, Peter, to take the fact that the media make "time-travel" of things like "looking backward in time" as another pretext to get started again about scientist working in the fields (note: here is a plural s) You hate. Be sure that apart from certain exceptions the scientists feel uncomfortable too watching the media reporting claims which are neither scientifically true nor pronounced in this way by them.

Du You really think that we laymen are that stupid such that we are not capable of distinguishing a silly and exagerated headline and just laugh about it by ourselfs? It is really not necessary that You point out and term every bit of science knews You dont like (independent of it being exagerated or not, true or not true, etc) as a Hype!

You are getting more and more unfair, destructive, fanatic and obsessed by what You obviously think is Your "holy crusade" to protect the poor innocent laymen against the demonic scientists You see everywhere in Your hallucinations.

Get well soon!

21. **Roger Schlafly**
March 19, 2011

Michael, thanks for your suggestions. I did not immediately see any sober assessments of the "inflated and extravagant claims" that you complain about. On your own blog, you seem to avoid making such claims, but you don't comment on such claims either. I would suggest that if the fools really frustrate you so much, then you criticize them on your blog.

22. **Cents**
March 19, 2011

To nonstupid layman:

I am sorry that you feel Peter is not providing a useful service in pointing out the BS and hype that is part of today's magazines business model. Besides providing interesting and timely insight as to what is going on in various areas of Math and HEP he is acting as communicator to the lay public – getting current science info out and and pointing the speculation aspect to the stupid laymen (do you think everyone is just like you). There is no shortage of speculation out there as that what sells magazines. I have been lurking here for a couple years (since reading Greene, Randall, Hawking and Smolin's books that discuss multiuniverses and string theory and other speculative views), and really appreciate the service that Peter provides pointing out what is on the fringe versus what has solid experimental evidence. If I wanted speculative BS I would follow Michio Kaku or similar sci-fi-ish authors. Normally I would just keep lurking but I felt the comments by nonstupid were completely unfair. Since I won't comment again, I just want to thank Peter for his blog and ask when and if you will write another layman book. Your writing style is extremely approachable and I would enjoy reading any new layman related science book that you care to write. (Yes I admit I am a fanboy).
Nonstupid, no one is forcing you to read Peter's blog so if you don't like what he writes look for your Math/Physics fix somewhere else.

23. **Peter Woit**
March 19, 2011

Cents,

Thanks a lot! Providing a more accurate source of information about subjects I care about is certainly one thing I hope I'm doing, and glad to hear it's appreciated. No plans anytime soon for writing another book for layman. There are some expository writing projects I'd like to work on, but they would be at a more advanced level.

nonstupid layman,

You've completely misunderstood what this is about. The LHC as "looking backward in time" to the big bang is the usual kind of hype which isn't worth making too much of. This is about something different, a claim that using extra dimensions and M-theory, signals can literally travel backwards in time, in a fashion observable at the LHC. This kind of M-theory inspired nonsense, spread by university press offices, is the kind of thing "This Week's Hype" tries to point out. I think it's worth doing, and no one else is doing it. I'll be glad to stop when someone else starts (or, better, physicists stop doing this or tolerating it when their colleagues do it).

24. **PWfan**
March 19, 2011

"I think it's worth doing, and no one else is doing it. I'll be glad to stop when someone else starts (or, better, physicists stop doing this or tolerating it when their colleagues do it)."

You're a hero! 🙂

Thanks!

25. **Yatima**
March 20, 2011

Backwards-in-Time signalling would make the NP complexity class accessible. Weakly godlike computational power at your fingertips, even if backed by Big Science? I'm all for it!

http://www.scottaaronson.com/democritus/lec19.html:

"Yes. Once we set up the CTC [Closed Timelinke Curve Computer], its evolution has to be causally consistent to avoid grandfather paradoxes. But that means Nature has to solve a hard computational problem to make it consistent! That's the key idea that we're exploiting."

26. **nemo**
March 20, 2011

Peter,

did you see this:

http://today.msnbc.msn.com/id/41974768/ns/today-today_tech/

?

27. **Paul**
March 20, 2011

Peter, you might be very interested in the recent string debate at

http://www.amnh.org/news/tag/isaac-asimov-memorial-debate/

It's got Greene, Smolin, Freese, Gates, Levin, Gleiser, and Tyson debating.

28. **Bernhard**
March 20, 2011

There should be some kind of political action against these clowns, really. Society is paying for this time-travel garbage which has no chance of explaining fundamental theory, is not science, not even good sci-fi. Even when people promoting this are competent in their fields, something should be done do punish them. Perhaps go tell on them to some piss-off congressman who knows nothing about science and willing to not see tax-money spent on this crap. Than after one or too time loosing funding perhaps the Universities would consider themselves not allow this kind of crap to be promoted.

29. **Bernhard**
March 20, 2011

… anyway, this sounds a bit extreme, and it is I admit, but it pisses me off no end, see this kind of things on the papers. I see no reason why this people have so much power and recognition but in the end what´s really worse is that these are the people with job deciding powers (on the theoretical side)!

30. **Peter Woit**
March 20, 2011

Nemo and Paul,

See previous mention of this in "Short Items" posting.

Bernhard,

In this case and others, I think the people and institutions putting out dubious press releases don't have much influence in the field. Consider it just evidence of the odd nature of the 21st century American university (50 years ago, if one of a university's faculty members was working on time travel, the administration would probably not issue a press release about it).

It would be helpful if prominent physicists contacted by journalists about stories like this would be willing to make forceful and colorful statements. In this case though, I guess the stories were just based on the press release, with no real reporting. Also a sign of the times….

31. **CarlH**
March 21, 2011

While Peter and others should never give up the good fight, keep in mind that you are up against something that has gone on for a long time.

When Percival Lowell was using his wealth and influence to promote intelligent canal-building beings on Mars over a century ago, there were plenty of professional scientists who showed why there could be no such thing on the Red Planet.

Take a wild guess who the media focused on and gave disproportionate amount of publicity to. Plus, Lowell apparently gave some very enthralling lectures and was a very good popular writer. I have read two of Lowell's books on Mars and I have to admit, I can see how he could convince a lot of people there were ETI one planet over.

I think we are going to have to wait for the human species to become a different and more educated creature before we can expect the majority of people to logically gravitate towards views such as those being espoused here. It's going to be a long wait.

32. **Shantanu**
March 22, 2011

Peter, am still eagerly waiting for your comments on ASimov debate which btw is on youtube

33. **Peter Woit**
March 22, 2011

Shantanu,

I just got back from vacation, and there's a long list of things I need/want to do that seem a lot more appealing than watching the Asimov debate. Maybe I'll get around to it, maybe not. I'm much more interested in what other people think about it.

Also, I pretty much know what the debaters each are likely to say on the topic, and readers of this blog probably know what I'm likely to say about what they say. So the marginal utility of me watching and writing about it may not be very high…

34. **Zathras**
March 22, 2011

If the goal of this blog is to take down the massive amounts of science hype out there, the proper course is to do more than just a blog. How do movements get started in science? By articles and conference proceedings. Are there any articles on the arXiv that lay out the problem of hype in science? How about a session at an APS meeting which goes through what is hype and why it hurts science. With just a blog the believers will nod their heads, the trolls will troll, and everyone else will never get the message.

35. **Shantanu**
March 22, 2011

Peter the session was quite interesting and I did learn something new from the discussions and worth a look.
Jim Gates (JG) does not believe in extra dimensions. He was contradictied by Brian Greene who said that extra dimensions are instrinstic to string theory.
JG also said that this nobel prize winner found applications of string theory in graphene. He also said that everything is a matrix (which I don't think anyone else including Brian understood). Marcelo Gleiser pointed that he is skeptical about grand unifcation and even things like unification of electricity and magnetism works only in a limited sense (that is in vaccuum in absence of sources) and
he doesn't think electro weak theory is a true unification of electricity & magnetism.
Katie Freese pointed out that LHC and dark experiments may provide evidence for super-symmetry which might pave the way for string theory. At the end of the debate Neil Tyson (who btw did an excellent job in moderating the debate) grilled Brian a bit as to why "string theory has not made much progress". Brian's reply was that the questions we are addressing are much more profound and insisted that there had been progress(he mentioned applications to RHIC etc)
I think that's a short summary from my side.

36. **thanksgod**
March 22, 2011

Thanks god that in Europe all is well in contrast to what is going on in the US as it seems.
Evereybodyhere can investigate what he wants and what he is interested in, including quantum gravity or other fundamental questions, without being threatend by religious fanatics who want at any cost to eradicate certain research directions. Sorry but it is really insane what is going on and what some people are saying here …

37. **Peter Woit**
March 22, 2011

thanksgod,

Sorry to have to tell you this, but, based on the e-mail addresses they leave and the IP addresses they are coming from, as many as half of the more vocal commentators here come from Europe, including the "religious fanatics who want at any cost to eradicate certain research directions".

However, both in Europe and the US, I don't think there's any danger that those who comment on this blog or others will eradicate any particular line of research. Physicists issuing ridiculous press releases promoting their line of research might manage though to convince some of their colleagues that what they do is not to be taken seriously.

38. **Peter Woit**
March 22, 2011

Shantanu,

Jim Gates and I were once invited together to debate string theory, but disappointed our hosts by agreeing on most things. He (and others, e.g. Warren Siegel) don't think string theories in 10d give viable unification, they're interested in making sense of "non-critical" string theories in 4d. So far this doesn't give viable theories either, but you can argue it's a more promising thing to work on than 10d strings, which have been a failure.

It would have been interesting if Tyson had asked Freese whether, since supersymmetry at the LHC was evidence for string theory, wouldn't no supersymmetry at the LHC be evidence against it? And at what combination of luminosity/beam energy would she give up on supersymmetry if it wasn't seen by then? For Brian an interesting question would be whether he thinks there has been progress in the area of string theory unification. This raises the interesting issue: is the landscape progress or not?

39. **Shantanu**
March 22, 2011

Peter,
I think Marcelo pointed out that some early LHC results already rule out a lot of parameter space in SUSY, but K.Freese didn't quite agree. People also talked about the anticpated PLANCK results. but it was not obvious to me what that rules out or not.

40. **Shantanu**
March 22, 2011

a typo in my prev. posting
"he doesn't think electro weak theory is a true unification of electricity & magnetism".
I meant
"he doesn't think electro weak theory is a true unification of weak nuclear force and electromagnetism" , since both weak nuclear force and electromagnetism retain their individual identities.
also one interesting question asked by Neil was whether people worked on extra time dimensions and the answer was yes (I couldn't catch the name of the person who worked this, but apparently this is complicated). No one mentioned neutrino experiments anywhere in the debate.

41. **supsym**
March 22, 2011

The “establishment” will not give up on supersymmetry just because it is not found, at the LHC or anywhere else. People did not give up on the luminiferous aether just because it was “not found”. Instead they invented more and more contrived explanations. Remember that Lorentz-FitzGerald length contraction was actually invented to accomodate the null result of the Michelson-Morley expt. The aether theory was NOT abandoned, it was **modified**.

What really killed the aether theory was that Albert Einstein could explain all the observed phenomena with a simpler alternative scheme which was demonstrably more successful. New observations fitted well with relativity, **without** the need to tinker with the foundations of relativity.

So … bottom line … people just lost interest in the aether. The same happened with phlogiston — people just lost interest because there was a better alternative explanation for things.

Supersymmetry will only go away when “something else” comes along which can do a better job of explaining known things, and can explain new things without revision of its foundations. But that requires “new things” to be found (new particles at the LHC). The “non-finding” of superpartners is neither here nor there.

42. **younghun park**
March 23, 2011

supsym
good! I feel what you said is good, very good.

The problem we have is not that SUSY is wrong, but, there is not the alternative theory, the other creative idea against SUSY and String theory to explain the experiment.

43. **Bernhard**
March 23, 2011

thanksgod,

I do research in Europe and have certainly no problem with people investigating quantum gravity. It’s a really interesting subject. Actually even time-travel per-se as an (highly) theoretical speculative direction can has its value, like guys like Kip Thorne demonstrated. Asking the question on how much the laws of nature are allowed to be bent is part of the fun. On the other hand as a direction of research String Theory offers nothing. It failed in every single problem which aimed to solve and makes absolutely no prediciton. Of course if someone is willing to embark in such a sinking boat I have personally also nothing against it. But scientists must be responsible and not make really retarded public claims like leaving someone thinking that M-theory at the LHC will provide evidence for other universes and or time travel, which is complete crap. For this kind of think I have no patience or sympathy.

44. **Nathalie**
March 23, 2011

I wouldn’t worry about the Ho-Weiler paper. The LHC physicists can’t even imagine how they could experimentally observe that the secondary vertex appeared before the primary one.

supsym
I agree with your main message that people tend to lose interest … However, your historical account is not correct. Michelson didn’t know that he had proven the non-existence of the aether. If he was sure of having made such a great discovery he would have at least mentioned it in his Nobel lecture 1907. The great Lorentz believed in aether when he gave his Nobel lecture in 1902 (15 years after the Michelson-Morley experiment). For him the fact the “atoms” of aether were smaller than the ordinary atoms was not such a big deal.
It is never easy to abandon an idea that has fascinated you for years just because it happens to be wrong.

45. **mm**
March 23, 2011

Michelson (and Morley) never claimed to demonstrate the non-existence of the aether. They could not. They could only test a hypothesis and report a null result. MM performed many versions of their expt (rotating mirrors) to test (and rule out) various hypotheses. But it was up to the theorists to explain the data using the aether, or something else.
http://en.wikipedia.org/wiki/Michelson%E2%80%93Morley_experiment

The LHC, for its part, can only report null results of searches, but not that “supersymmetry does not exist”. But note the following:

a) The aether theory made concrete predictions (“solar wind”). These were tested (MM) and null results were found.
b) MSSM in that respect does not make any specific predictions. If LHC does not find superpartners, MSSM is not “ruled out”.
c) Things will only change if the LHC (or ILC or whatever) **finds something**, then the theorists will have to explain it. A continuation of “not finding anything” will not change the status quo. The more new physics found, the better. But even if new stuff is found, it is only if and when a competing explanation can do a better job, then MSSM will go away. And if MSSM explains the data … hah!

46. **Chris Austin**
March 23, 2011

shantanu,

“also one interesting question asked by Neil was whether people worked on extra time dimensions and the answer was yes (I couldn’t catch the name of the person who worked this, but apparently this is complicated).”

Two-time physics is developed by Cumrun Vafa in the context of F theory, which is something like type IIB string theory oxidized to 10 space dimensions and 2 time dimensions. F theory is being vigorously developed by Jonathan Heckman, who has published or co-authored 19 articles on it in the past 3 years. There is a separate development of “two-time physics” by Itzhak Bars.

47. **Kenneth Vatz**
March 25, 2011

For whatever it’s worth, I reviewed Brian Greene’s latest book, “The Hidden Reality….,” on Amazon, and it is easily found there under “one star reviews” of the book. What I said is entirely derivative, since I am not a physicist, and is based mainly upon the books I cited at the top of the review, including, most importantly, “Not Even Wrong.”

More people than not have found the review "helpful," although I did get one rather nasty comment calling it a "crank review," to which I responded as objectively and dispassionately as I could. Well, maybe I could have been nicer…. 🙂

In this review I was really expressing my own disappointment at having had the idea of string theory, multidimensionality, and parallel universes taken away from me after all these years of assuming this would be the ultimate GTE once it was proven experimentally. I guess it all has to do with the natural human desire and need for intellectual closure, even when it can't be achieved.

48. **Mich Zimmerman**
April 1, 2011

I know I'm jumping in way late here, but:

cf –

BA-ZING!

Things That Deserve (but won't get) Longer Blog Postings

March 23, 2011
Categories: Experimental HEP News, Langlands, Uncategorized

Here's a selection of news that deserves longer blog postings that, for one reason or another, I'm unable or unwilling to provide…

- This year's Abel Prize goes to John Milnor. With an excellent blog posting about this from Fields Medalist Tim Gowers, why should I try and compete?
- I've been waiting for the US budget situation to clarify before writing about its implications for physics and math research, but it looks like that isn't going to happen anytime soon. The US Congress is now engaged in a bizarre and irresponsible exercise of trying to run the country by each week fighting over not next year's budget, not next quarter's, not next month's, but next week's. At the moment there's a budget for the next week and a half, but no one seems to know what will happen after this. The president has issued a proposed FY2012 budget, but there's no reason to believe it will have anything to do with whatever the reality of funding later this year turns out to be. Trying to make plans and run a large laboratory like Fermilab under these conditions must be a nightmare. Last week there was a HEPAP meeting in Washington, with presentations that explain the current situation. A good excuse for not writing more about the future implications of federal funding decisions is that no one is actually making such decisions.
- Last week Langlands supposedly gave a talk at the IAS, On Functoriality; on the Correspondence; and on Their Relation, Part I (I'm not sure if or when there will be a Part II). I wasn't able to attend, but perhaps video will someday be available. Langlands provides a link to a document of "work in progress" entitled Functoriality and Reciprocity. In it, he gives his reflections on the current state of attempts to precisely formulate and understand the conjectures generally referred to as "Langlands functoriality" and the "Langlands Correspondence" (or "Langlands reciprocity"). These conjectures come in versions for algebraic number fields, function fields, and so-called "geometric Langlands" over the complex numbers, in each case in local and global versions.

Much of the document consists of Langland's description of his struggle to understand some issues in the geometric Langlands story, including the work of Witten and collaborators relating this to 4 and 6d quantum field theories. Another topic is that of the Abelian theory, and attempts to understand it locally. A very good reason to not write more about this is that I don't understand it very well, although, paradoxically, I find Langlands writing about what confuses him rather easier to follow than when he writes about what he has completely understood. Another good reason is that I'm busily learning more about some of this, and maybe someday I'll be less confused and able to write something more sensible here.

- Also from the IAS, there's video of a talk by Arkani-Hamed to the mathematicians available here, about work on scattering amplitudes. I'm curious to know what they made of it.
- Also on the Langlands front, again in a category of things I don't understand well enough to write more about, see this new Seminaire Bourbaki report on the Fundamental Lemma from Thomas Hales.

Update: There's a Newsday story about Milnor here, unfortunately only the first bit is free. He explains what he is going to do with the million bucks: buy more leg-room on airplane flights (he's 6'3″).

Comments

1. **fatdog**
   March 25, 2011

   A semi-serious meta-mathematical question: is it possible to write more pompously than Langlands does? Is Langlands as pompous and condescending in person as he is in print? While he somehow manages to write clearly, and he clearly writes deeply, his tone almost makes the readable intolerable. It's like a movie caricature of a self-important professor.

2. **dogfat**
   March 25, 2011

   Pomposity is a point of view. I am sure the perspective is different if one is offering the benefit of one's years of hard-won accumulated wisdom to the unwashed masses.

   I see that this post has undergone numerous deletions of comments. Add this and previous the list … asap!

3. **Peter Woit**
   March 25, 2011

   fatdog/dogfat,

   I've deleted various off-topic comments, these are at least on-topic, although I really don't like the use of anonymous comments to personally criticize people, and this comes awfully close to that. I probably should delete the exchange, but I do think the question of Langlands's somewhat unusual writing style is an interesting one.

   I don't think "pompous and condescending" are accurate descriptions of this. He often is giving opinions, in ways that are not conventional for mathematical writing. Usual standards require this sort of thing to be rigorously suppressed, for good reasons: in a scientific paper you want facts, not opinions. But Langlands has got the track record to make his opinions worth hearing, no matter how they come off.

   I've no personal experience interacting with him, and sometimes found his writing style odd, but not really off-putting. The one thing he does I find hard to justify is his choice to sometimes write not in his native language (he has quite a few papers written in French, and German, and is also known to write in Turkish). I found his extensive autobiographical comments you can find here

   http://publications.ias.edu/sites/default/files/ubc.pdf

   quite fascinating, and they also give some insight into how he came to love erudition and thus explain a bit of the style and use of other languages.

4. **Jeff**
   March 25, 2011

   Well, as a data point, I was once informed by the chair of a Research I math department that he had read a number of recommendation letters from Langlands, and in each of them the only thing Langlands talked about was how the applicant had contributed or not to the Langlands program. No general discussion of their mathematical ability at all.

5. **Kea**
   March 25, 2011

Well, I actually met Langlands once, some years ago now, and I thought he was interesting and friendly.

6. **Bugsy**
March 25, 2011

Peter, thanks as usual for all the links and observations. I have to say I found fatdog's comments equally stupid and offensive. Being not at all in Langland's area, I began with the autobiographical interview and proceeded to
the recent paper, skimming both, just to get an impression. What is that? Of a thoughtful, erudite, creative and intellectually curious mind, who is genuinely trying to communicate something of value.
I didn't pick up a mote of arrogance or condescension, to the contrary I found the style refreshingly literate for a mathematician. Just because someone (in this case from an older generation) does know how to use the English language doesn't mean we should feel bound to insult them. In other cultures age and concomitant deep experience are met with respect: our eyes open wider, and shine from the privilege of the encounter….

7. **Shantanu**
March 28, 2011

Peter or others, have you heard any assessment of reports on how the earthquake in Japan is going to affect ongoing HEP(mainly neutrino) experiments in Japan and implications for near term future?

8. **lun**
March 28, 2011

I would be very interested in your opinion of the colloqium today at Columbia University, if you happened to attend it.

It seems to concern several of the issues you most often write about in a very succint way

9. **Peter Woit**
March 28, 2011

lun,

Yes, I was there (Arkani-Hamed colloquium on scattering amplitudes here at Columbia, somewhat like the IAS talk mentioned in this posting).

Personally, I found it a mixed bag: about an hour of very interesting results and ideas about scattering amplitudes in N=4 super Yang-Mills, explained well, prefaced by a tedious half an hour of unconvincing arguments and ridiculous hype. He seemed to have planned to give at least an hour and a half talk, even though it was scheduled for an hour, which struck me as rather rude, especially since skipping the first half-hour would have improved the talk a lot.

As he said at the beginning, this is work in progress. This has been an active area of research for several years, and it's not clear where it's going. That will be really interesting to see in the future. The impressive results he described are only about N=4 SYM in the planar approximation. One question is how widely these techniques can be applied to other theories, the other is whether they will lead to a fundamentally different theory, and if so, what it will look like. I'm a fan of twistor geometry, and I think it's great to see people doing something new with it.

On the other hand, I don't see how this yet shows any signs of solving any of the fundamental problems of how to get beyond the standard model and how to unify it with gravity. What's there now, impressive as it is, just didn't seem to me to support the grandiose hype that made up a big part of the talk.

Change of Direction

April 1, 2011
Categories: Uncategorized

It probably won't surprise my regular readers to hear that recently I've been getting rather tired of the usual topics of this blog. String theory has been intellectually dead for a very long time now, and continuing to point this out is becoming more and more tedious. About the multiverse, surely no one takes that seriously anymore, so, the less said, the better. Recently, John Baez decided to move away from abstract mathematical physics to write about topics of more relevance to the real world, see his blog Azimuth. Like him, I think it's time to move on to subjects of wider interest. In the past I've very much restricted the topics I write about on this blog, but now have decided that I should share my views on a wide array of topics not just with my friends and colleagues, but with the wider world. This blog will be one way of doing this, but in the next days and weeks I'll also be entering the world of Web 2.0 in a big way. There's now a twitter feed, with much more to come.

As part of this new order, I intend to stop my previous somewhat fascist policy of deleting a large fraction of comments on various ill-defined grounds. I now encourage interaction with my readers, feel free to write about whatever's on your mind!

Comments

1. **Dave**
   April 1, 2011

   As a reader for a long time, I won't say the prior topics of the blog won't be missed, they surely will. However much luck and enjoyment on the new direction, judging by the politics post, still a blog of interest to follow. Thanks for all the hard work and time leading up to this.

   p.s. Is there a good book/blog that you can recommend that deals with refuting the multiverse?

2. **Matti Pitkanen**
   April 1, 2011

   April first joke?

3. **abbyyorker**
   April 1, 2011

   A welcome break from your whining. Of course, this opinion only lasts 24 hours.

4. **Robert Smart**
   April 1, 2011

   If, after studying the issue for 1000 years, we determine that the probability of intelligent life arising in this universe (the bit we're in causal contact with) is very low, then it seems that it will be impossible to distinguish between 3 ways of looking at it: (a) Our existence causes the universe to have had some fortuitous events in the past; (b) We are actually just a simulation in a higher being's computer and they fudged it to get us; (c) There are multiple "universes" and naturally we are in one that happens to have intelligent life. Which will you prefer on aesthetic grounds (it being agreed that Science can't distinguish)?

5. **Casey Leedom**
   April 1, 2011

   Wow. I _Really_ Should_, as per always, read my email — blogs comments — in reverse order. I'm Very Sorry Peter for not having done that tonight.

   My personal belief: High Energy Particle Physics is both dead and boring in the current world. I hope that some day we'll have the ___DATA___ to generate some new and interesting theories. But right now, not so much.

   So, it's time to focus on more productive paths. And I'm sorry, but physics isn't part of that. My own personal view is that: if I were a young person trying to figure out what's "interesting", "likely to mean something", etc. … it would be biology. I think that there's a very real possibility of making a large number of people's lives better. The data is there and growing. It's going to take people with strong mathematical skills to process, integrate into good models, and help develop new takes on things.

   Let's give this "Unified Field Theory" stuff a rest and try working in a domain that's actually interesting …

6. **chris**
   April 1, 2011

   april 1st?

7. **Peter Woit**
   April 1, 2011

   This is me posting from a parallel universe and I'm _very_ annoyed at me in this universe for giving up the good fight!

8. **joke**
   April 1, 2011

   to bad that it's april 1st 🙁

   A change to more interesting directions would be desirble …

9. **joke**
   April 1, 2011

   … and a stop of the randomly and absolutely unpredictible disappearance of posted comments into … 😛 (the Nirvana ?) would be a god thing too …

   But Alas! …

Happy April 1st :-/

10. **Frank Quednau**
April 1, 2011

Well, the bit about the Twitter account is true, April Fool's or not, why not? "Not even wrong" applies to so many areas in our modern life, that you'd never run out of subjects in line with the blog title.

11. **Shantanu**
April 1, 2011

Peter, I shall miss the science and physics posts from you (as well as related comments from the readers). hope you continue blogging about particle physics.

12. **Giotis**
April 1, 2011

Good decision; if you can't beat them, ignore them…

13. **alt**
April 1, 2011

"About the multiverse, surely no one takes that seriously anymore, so, the less said, the better."

what about Briane Greene "The Hidden Reality: Parallel Universes and the Deep Laws of the Cosmos" Jan 25, 2011 release date. Have you read it and do you have a review of it?

from Publisher's Weekly

But string theory opened up a new can of worms, hinting at the possible existence of multiple universes and other strange entities. The possibility of other universes existing alongside our own like holes in "a gigantic block of Swiss cheese" seems more likely every day. Beginning with relativity theory, the Big Bang, and our expanding universe, Greene introduces first the mind-blowing multiplicity of forms those parallel universes might take, from patchwork quilts or stretchy "branes" to landscapes and holograms riddled with black holes.

14. **Sung Lee**
April 1, 2011

Was NEW hacked by string proponents?, or Peter just went nuts? Hope it is really an April 1st joke and the site goes back to the way it has been tomorrow. This site has really been a great source of information on mathematics and theoretical physics, and I am shocked and appalled by what I am seeing now. Is this a sign that mathematics and theoretical physics are hitting a dead end?

Sorry, this is your blog but as a long-term reader of your blog, I am very disappointed if this is not a joke.

15. **Anonymous**
April 1, 2011

"String theory has been intellectually dead for a very long time now."
Particle phenomenology is a much more intellectually stimulating. It's all about supersymmetry, supersymmetry, supersymmetry… and thousands of non-SM Higgs models.

16. **The Cosmist**
April 1, 2011

Mark my words, the future of physics and cosmology is turtleverse theory!

17. **SieveMaria Lucianus**
April 1, 2011

April 1st right ?

18. **D R Lunsford**
April 2, 2011

Peter, it was a heroic effort and did enormous good. I was utterly depressed about the state of science when I stumbled across your manifesto on arxiv a long time and another city ago. I was immediately cheered and took up physics again with a renewed effort. So this may be Aprils Fools day, but I do detect some sincerity in your screed here. You can still write about physics and desserts and New York all at once. I like to see some "then and now" photography from the city, maybe some architecture tours – that'll give you some excuses for biking around.

-drl

19. **John Baez**
April 15, 2011

Grrr…

20. **Peter Woit**
April 15, 2011

I guess not everyone is amused by getting brought into my practical jokes…

21. **donald klein**
April 16, 2011

I appreciate your thoughtful dehyping book Since you intend to march on to other
super-hyped areas you might look at "personalized medicine" and
the need to complete every biological finding with the promise of new therapeutic horizons.
This all falls under the rubric of translational medicine.
Please un-translate using scalpel of predictive validity.

22. **John Baez**
April 17, 2011

That was supposed to be a somewhat ironic "Grrr…", but it's hard to convey tone of voice in this medium.

Obama Worse Than Bush

April 1, 2011
Categories: Uncategorized

I voted for Obama in the Democratic primary, because I figured Hillary Clinton was more likely to expand the war in Afghanistan and otherwise engage in the sort of misguided military adventure favored by the shrub. Look what happened. He appointed Clinton Secretary of State, and then sent even more troops into Afghanistan than Bush Jr. would have dared consider. Don't even get me started about his Mideast policy and spineless cave-in on Israeli settlements. Remember Guantanamo? He's commander-in-chief, could shut this illegal abomination down whenever he wants to, instead he intends to keep it open indefinitely. Again, W would have closed the place by now and moved on. The fact that Obama was given a Nobel Peace Prize is some sort of sick joke.

On the domestic front, let's face it: Obama has been a disaster for the country, moving it farther to the right than it has been at any time since perhaps a period of a few years sometime back in the 19th century. He has pursued policies more or less in line with those of Bush, confusing and neutering moderates and progressives (who don't dare criticize him). Based on his inspiring speeches, they thought they had elected a community organizer, but are slowly realizing that they've been had, with the White House now in the hands of a Bush clone interested not in fighting powerful interests but in playing golf with them. By doing this, he has pushed the Republican opposition so far to the right that they've descended into lunacy, and ensured that he'll should have no trouble winning re-election in 2012. The only threat to him is that of the rise of a populist/fascist movement, motivated by blind hatred and the (accurate) feeling that they are being driven into poverty by a ruthless Ivy-league-educated establishment with a lock on the political and financial system of the country. At the Harvard Club in midtown there's a huge new portrait of him set in a prominent place as you enter the building. The establishment lawyers and financial types who congregate there know that he's their man.

The military budget is now significantly higher than during the Bush years, and taxes on the wealthy even lower (taxes on large estates are lower than under Bush). While Bush expanded Medicare significantly to cover prescription drugs, Obama's health plan was written in partnership with those responsible for the problem (high costs): doctors, insurance and pharmaceutical companies. The great innovation seems to be to expand access to medical care by forcing people who can't afford it to buy insurance from rapacious insurance companies. Obama's choice for Fed Chairman: same guy as the one Bush had running his Council of Economic Advisers, before moving on to the Fed and presiding over the worst financial crisis since the Great Depression. If there's any difference between Obama's treasury secretary (Geithner) and Bush's (Paulson), I'm unable to see it and haven't met anyone who can. Geithner is now in charge of gutting the few minor reforms that were passed in the aftermath of the crisis, while institutionalizing a system of government backing for too-big-to-fail financial firms of sizes expanded since the Bush years. The organized looting of these firms by their employees that brought on the mess of 2008 is now back in full-swing.

Next year's presidential campaign is predicted to cost a billion dollars, which Obama has already started raising from the financial industry and other interest groups. He faces no progressive or moderate opposition at all, with the only question to be resolved that of exactly how extreme his Republican opponent will be. I'll be covering all this here on the blog, but on days when I don't get around to giving you my thoughts on what is happening, two other places you might want to consult are FireDogLake and Naked Capitalism

Comments

1. **MarkoB**
   April 1, 2011

   the prime axiom of neoclassical economics, and much else besides: when the rich are happy, great things happen. Corollary: when the poor wallow in misery, wonders never cease. I bet all the theorems of that other body of theory that is "not even wrong" are based on the prime axiom. Adam Smith himself called it the "vile maxim" of the "masters of mankind." There are clear parallels between string theory and neoclassical economics. Might make for an interesting research paper.

2. **Mikhail Obamachev**
   April 1, 2011

   April Fool, none of this actually happened… right?

3. **Casey Leedom**
   April 1, 2011

   Okay, sorry, moving my way through your posts in reverse order. My bad.

   Breath. Really and seriously. Politics is "The Art of the Possible." It's weird and absurdly true. As a Died-In-The-Freaking-Wool liberal I have many dashed hopes in this game. But as a Died-In-The-Wool realist, eehhh, I'm open to what comes next. It really is better than what came before. Really.

   And by the way, if you are ever in the South Bay, drop me a line. I'll organize a bike ride/hike and a party and we'll all have a grand time.

   In the mean time, yeah, we've all got to try to influence things in reasonable directions.

4. **Cliff**
   April 1, 2011

   I think I am a much bigger fan of Peter Woit the political commentator than Peter Woit the physics commentator ;D

   Seriously, you're right in just about everything you say here. I voted for Obama also. Nothing wrong with embracing the mistake. What I really can't stand is the completely irrational unwillingness of Obama supporters to realize that his nice-guy facade is a complete lie. People desperately want to believe he's some victim of the establishment rather than a part of it. Grow up people, you were deceived by a bunch of empty PR. Now we have to figure out what to do about it.

5. **chris**
   April 1, 2011

   very well said. i never thought that you political views would be so absolutely congruent with my own ones – although the tone of your budgetary comments was hinting at it.

   why did you not become a journalist, really? you 've got talent for it.

6. **Nigel**
   April 1, 2011

   Look on the bright side, Peter. At least President Obama has got America firing its cruise missiles and dropping its bombs in the Libyan Civil War. So it's not all bad news.

7. **eldavojohn**
April 1, 2011

OMG!!!1!1! I LUV you're knew blog format! Like, I had gotten really sick and tired of you're "blah blah blah string theory blah blah blah like math …" and that shit is hard! It's about time you switched to something worthwhile and I'm glad I stayed and kept reading.

This is such a HUGE coincidence. Just the other day, me and my one friend LaShawnda, were talking about how there just isn't enough like politic blogs on the 'Net. SRSLY! You can't find anything like this anywhere! I even searched on AOL and you're like the only person with enough balls to diss the president.

And you totally changed my mind. I used to be some sort of Obama parrot but after reading your blog post, it really made me think. And after I thought I had like this revelation and then I totally agree with you now.

8. **tomate**
April 1, 2011

I see this as refined mock of aggressive and vague political writing, raging over the internet. No surprise that more than one commentator liked it: for the information standards today, this is high journalism!

But jokes sometimes are a good way to say what one really thinks… is that so?

9. **Desperate scientis**
April 1, 2011

Well, I think you can relax because there is no way Obama will win over Palin in 2012. So our next president will be even better.

10. **chris**
April 1, 2011

if a president is not able to locate a country on a world map, does that imply that (s)he will not send troops there?

if this is so, i am all for palin.

11. **SieveMaria Lucianus**
April 1, 2011

Do you think really that Obama is his own man ? That he has any power ? I think no and thought then that he is a straw man and he is afraid. Where did he come from – who put him there ? clearly he is someone who obedient to the powers that be.
I was one of 300.000 who voted for Ralph.

12. **steve newman**
April 1, 2011

hi peter-
i've been coming regularly for the physics, but i like the new format.
i consider string theory, standard model cosmology (and probably a lot of the math, which i don't understand), a total dead end.
just like the political scene, its depressing that the wrong way is in
power and squashes any opposition. a miniature version of the wrongness of things, is your aricle about the police war on bicyclists!
I agree with the commenter who said that your new blog direction is probably an admission of (hopefully) temporary defeat in the physics wars.
i'm less enthusiastic about the food reviews, especially in the context of all the grim stuff. finally, i agree completely with your assessment of Obama.
looking forward to seeing how your blog develops. maybe you should rename it to "More than Even Wrong".

13. **Roger**
April 1, 2011

I think I get the joke. You've written a parody of an anti-Lubos blog!

14. **Claver**
April 1, 2011

If the wise play the fool, still the fool remains more foolish indeed!

Nice one Mr Woit.

15. **S. Molnar**
April 1, 2011

Satire or not, I agree with almost everything you have said in your 1 April posts. I differ in the matter of BBQ – I refrain from eating land animals so that my atmospheric methane contribution will be of the direct kind only – and I have one example of the Obama administration being better than Dubya's. The Transportation Secretary, who is an open Republican, unlike the crypto-Republican Obama, has actually done a respectable job of promoting cycling infrastructure, pedestrian access, and public transportation. He clearly comes from an alternative universe.

16. **anonymous**
April 1, 2011

This may be a great time to mention the new element that is speculated to be the bearer of all political mass in the universe:
Govermentium (Gov). Govermentium is normally stable and does not change under most circumstances (including new election cycles), has a transient state where it spews a great deal of negatively charged taxions. Governmentium has 1 neutron, 12 assistant neutrons, 75 deputy neutrons, and 224 assistant deputy neutrons, giving it an atomic mass of 312. These 312 particles are held together by forces called morons, which are surrounded by vast quantities of lepton-like particles called peons. Since governmentium has no electrons, it is inert. However, it can be detected as it impedes every reaction with which it comes into contact. A minute amount of governmentium causes one reaction to take over four days to complete when it would normally take less than a second. Governmentium has a normal half-life of three years; it does not decay, but instead undergoes a reorganization in which a portion of the assistant neutrons and deputy neutrons exchange places. In fact, governmentium's mass will actually increase over time, since each reorganization will cause some morons to become neutrons, forming isodopes.

17. **SieveMaria Lucianus**

April 1, 2011

The real worry is the brute hammer of censorship – good choices are not made with missing information. An educated, well informed public is imperative for the survival of democracy – We have now at this very moment in the internet an amazing tool for the freedom of information – or in a parallel world – a twinkling of the eye it is gone and more and more you will be finding we are living in a totalitarian regime. If you want to control people just make them feel stupid or better yet shut the book stores, control the web. In a few years most everyone will forget how to survive.

18. **Bobito**
April 2, 2011

Right on the money, so to speak.

19. **Josuah Chamberlain**
April 4, 2011

Glad to see that you come to your senses. Isn't being able to think independently,that is to say, being able to think for yourself a wonderfull thing!!!!

Being president of the US for Barack and Michelle Obama is all about being the coolest kid in the class….scaled up big time. That it requires the destruction of human beings in the Muslim world and the destruction of American teenagers economically coerced into carrying out imperial policy in the Middle East is of no consequence to these two sociopaths and the previous two socoipaths-Bush W and Bill Clinton…all three working on behalf of the oil companies and the hedge funds. It is all going to end very badly and in tears.

20. **Andrew L.**
April 5, 2011

I agree with most of this post except that he's worse then Bush. Bush's administration INSTITUTED most of the most horrific polices you complain about. The problem with Obama is that he's gone along with them after BSing the nation. He's not a real Democrat,as I've said many times. He's a corporate token. But the really frightening part is how the Corporations have decided to use his weakness to eliminate the middleman entirely and return us to the 18th century of gilded age psuedodemocracy and legalized slavery of non-wealthy people. They've done this by a sustained 2-pronged attack through the subversion of the Supreme Court that now is systematically handing the country over to them piece by piece beginning with the Citizens United decision-and the shift of the Republican party into lunatic policies that are almost comically pro-wealthy and anti-middle/lower class.The local attack on labor is the centerpiece of the latter. All is not completely bleak, though.The plutocrats' huge mistake was directly attacking the rights of workers in such a heavy handed and overt manner and not even bothering to conceal thier contempt for the suffering working poor. People are ANGRY and they see through the scam now.They tell me, "oh,the mindless rabble will forget this in a few years." I don't think so, this was too direct and vicious. Taking away workers rights,demonizing them and cutting thier pay while laughing is pretty hard to spin effectively. That was thier mistake-they're usually a lot more subtle then this. Right now,that anger is locally directed-it may take a few more years before that anger is directed upwards towards Washington. The real breakthrough will come when they see that Obama and "bluedogs" like him aren't just the "lesser of the two evils"-they're as big a part of the problem as the Conservatives are. Personally-I'm seriously considering moving to Canada when I get my PHD.

21. **william C wesley**
April 5, 2011

OK progressives/liberals time to give a culinary review, how was the crow? Hope you liked it because you'll be eating that almost exclusively for most of the foreseeable future. You thought that because your weapon is individual honesty you were immune to this, that this dish could only be for the Republicans who's main weapon is shared illusion. Nope. Anyone can be wrong, anyone can be had. Liberals are had when the truth is falsely employed while conservatives are had when illusion is falsely employed. Bush was a wolf in wolfs clothing while Obama is a wolf is sheep's clothing, and so yes, he's worse. I don't mind when obnoxious ignorance does the wrong thing compared to when supposed compassionate wisdom does the wrong thing. I can understand when denial and illusion turns out to be denial and illusion but I can't forgive when honesty and knowledge turns out to be illusion
Bush never claimed to be on the side of the powerless, unlike the current occupant who did. WE HAVE BEEN HAD. In the next election VOTE INDEPENDENT! Who? Whomever most represents the attitude that creativity and liberty will solve our problems because ONLY RESPECT FOR THE INDIVIDUAL WILL. Nothing of any value was ever invented by a committee, and it maters not whether the committee that rules us is a government committee or a corporate committee it is rule by closed committee that must end. Return the patent/copyright office to INDIVIDUALS and take it out of the hands of the MOBS THAT NOW RULE OUR LIVES whether they be the Ivy league gangs or the crime boss gangs the government gangs or the corporate gangs, educate children to be EMPLOYERS NOT EMPLOYEES, CREATIVE PRODUCERS NOT PASSIVE CONSUMERS. If we continue to rely on gangs to get us out of what only individual initiative can overcome WE ARE DOOMED

22. **Chris W.**
April 6, 2011

…educate children to be EMPLOYERS NOT EMPLOYEES, CREATIVE PRODUCERS NOT PASSIVE CONSUMERS.

Uh, excuse me for asking this, but who will these employers employ, and who will consume what these creative producers produce? Just wondering…

23. **Tim R**
April 9, 2011

Hilarious.

I used to be sorta naive too, back in grad school. At least I never had a lapse in judgement as large as yours.

24. **Dave Miller**
April 17, 2011

Peter,

I realize that this is a bit of an April Fools' spoof and should be taken with a grain of salt…

But, in my experience, most intelligent liberals AND conservatives would agree with most of what you wrote here. The two things we could reasonably hope for from Obama were an end to the senseless wars and an end to the bailouts of the fat cats.

I was never an Obamaniac, but I too thought Obama would be better than Hillary and hoped he would be better than McCain.

I now think we elected Hillary W. McCain in '08.

Dave Miller in Sacramento

25. **Matt**
April 18, 2011

Nothing is more offensive than a naive person who claims that everyone else is naive.

Where to begin? First, Afghanistan. Obama campaigned on increasing the fight in Afghanistan. He talked about it repeatedly, especially in the debates. That you are surprised of his decision to increase troop levels in Afghanistan is rather strange. Did you think he was lying about his AfPak policy during his campaign?

And Guantanamo. Okay, let's say President Obama, the Commander-in-Chief, unilaterally declares that he's closing Guantanamo. What happens to all the prisoners? Where do they go? No state will try them–just look at the fiasco in NYC when Holder attempted to hold the trial for one of the 9/11 conspirators there. Ten years later, they eventually just gave up.

And no state will agree to hold the prisoners, either. Nor any foreign non-torturing country. None of our European allies have agreed to take them in. So what is Obama supposed to do with all these people? If he releases them all, and one of them commits another major act of terrorism, the blowback would be far more devastating to the liberal cause than Guantanamo's existence. If you can think of a way for Obama to somehow make state or local American politicians have a spine and take in Guantanamo prisoners, please tell me. The mere fact that you think it's just as simple as the Commander-in-Chief declaring the end of Guantanamo (what, in some sort of executive order?) is preposterous. How do you physically carry out that policy?

And on it goes. Health Care Reform? The model is a carbon copy of the system we have here in Massachusetts, and you know that if Ted Kennedy were alive when the national plan had been passed, he'd have been jumping with joy. Here in MA, 98% of adults have health insurance today, and 99.7% of children. Have you seen the national rates? How can you possibly argue that nationalizing the MA plan wouldn't be a staggering improvement, especially considering that the national plan, unlike the MA, also includes countless additional trial programs for actually making health care delivery less expensive over time, like bundling, ACOs, revised payment schemes inspired by the Mayo Clinic, computerization, etc. If you or Jane Hamsher or Howard Dean had your way, we'd be stuck with the status quo, just like the GOP wanted. So tell me, do you really think a national version of the MA plan is worse than the status quo?

This whole making-the-perfect-the-enemy-of-the-good thing is what makes me so disgusted with the hard left. Do you know what the original incarnation of Social Security looked like, and how long it took before the program took its modern form? Would you have demanded that FDR veto the original Social Security plan?

The choice between the Dems and the GOP has become the choice between Coca-Cola and arsenic. On the one hand you have an ideologically blurry party with liberals, moderates, and conservatives represented, and on the other hand you have a party that's basically the John Birch society. If it hadn't been for the GOP's massive federal tax cuts, two decadal unfunded wars, and a Medicare prescription plan bigger than Obama's health care bill, all put entirely on the national credit card, there would be no short-term or medium-term debt crisis right now. And you think Obama is worse than Bush? Who would Bush have replaced Souter and Stevens with on the Supreme Court?

As for the Fed, the best possible thing the Fed could be doing is a massive quantitative easing program that pours a huge amount of money into the system. And that's precisely what they finally decided to do, and they're keeping at it, despite many right-wingers demanding that it's going to lead to too much inflation. You think another term of Bush would have them doing monetary stimulus on that scale?

Your post is ridiculous, pie-in-the-sky whining. Politics is very, very hard, and it's for grown-ups who have a grip on the real world. You fight hard and take what you can get. But this attitude of declaring that it's all a big sell-out and you're just going to take your marbles and go home is childish and immensely counter-productive. You think if Gore had won in 2000 we would have had Iraq and those massive tax cuts, let alone Alito and Roberts on the Supreme Court, who are responsible for Citizens United?

Here's a good idea: Find a way to eliminate filibusters and other anti-majoritarian parliamentary rules in the Senate. Nancy Pelosi and Obama got everything you wanted through the House, including a public option, cap and trade, a much bigger stimulus package, and over two hundred other bills. But they all died in the Senate. Was that Obama's fault too?

I'd like to see the Peter Woit plan. Compaining is easy, but productive suggestions are much more difficult.

26. **Marty**
April 18, 2011

Matt's comment is a good example of why I'm glad Peter avoids political commentary in his postings (except for April Fool's Day…). Why the *ad hominem* attacks? They add nothing useful or interesting. There seems to be something about political views, religion, and sports team rivalries that can bring out some of the worst in people. Well reasoned discussions that avoid ideology and strong emotions are enjoyable and thought provoking, but it seems too hard for some people to keep the discussion at that level…

27. **Peter Woit**
April 18, 2011

Thanks Marty,

You're quite right, Matt's comment does an excellent job of showing why I don't allow political discussions here except on April 1. The internet unleashes too many of people's worst characteristics, including that of vilifying anyone who disagrees with them. Only a self-destructive idiot would choose to spend his time moderating such discussions.

For the record, no, I don't think that "if Gore had won in 2000 we would have had Iraq and those massive tax cuts, let alone Alito and Roberts on the Supreme Court, who are responsible for Citizens United?" I actually was quite a fan of the Clinton/Gore administration, something which now seems to make me, according to Matt and other Obamaites, a member of the "hard left". My problem is just with Obama, and the collapse of moderate/progressive politics in the US that he has been (partly) responsible for enabling.

28. **Matt**
April 18, 2011

Perhaps you would be willing to tell me what specific things a hypothetical Bill Clinton 2008 presidency would have done differently? Clinton failed to pass health care reform, enacted Don't Ask Don't Tell and the Defense of Marriage Act, and shut down much of welfare, not to mention presided over most of the financial deregulation that led to the financial collapse of 2007.

It was much easier for Clinton to look good when the economy was roaring along. But Obama inherited the worst financial picture since the Great Depression. He somehow got nearly a trillion dollars in fiscal stimulus through (you should read through the list of separate items in that bill, which also included a huge boost to science research funding), plus at least as much monetary stimulus through the Fed. He saved the American auto industry and with it millions of working-class jobs. He finally ended the banking middle-men in student loans and the insurance middle-men in Medicare Advantage. He finally got DADT ended, got a federal hate crimes bill and the Lily Ledbetter act through, and dropped the federal defense of DOMA. He ended the global gag rule on foreign aid groups providing family planning services, too. And, refusing to give in to GOP demands, he is refusing to send ground forces into Libya, and letting the Europeans do the job (poorly, as we're now seeing). And he replaced two Supreme Court justices with liberals.

Please tell me how Bush would have handled all this. I just don't understand. Please explain it for everyone.

29. **Peter Woit**
April 18, 2011

Sorry Matt,

I don't do rants about how politicians I approve of do everything right, and ones about how politicians I disapprove of do everything wrong I only do on April Fool's Day. The world is very complicated, and it's also quite different than in the 1990s: militarism and inequality between the rich and the poor in the US has increased dramatically. Progressives and moderates should be holding Obama accountable for his failure to do anything about these problems, not making excuses for him about how it's impossible.

30. **Matt**
April 18, 2011

I notice that you're not willing to give specifics. I know you're disappointed, but please tell me concretely what you would have Obama physically be doing differently.

How does he get a bolder agenda through the buzz saw of the Senate, given the filibuster and other procedural roadblocks there? Look at all the bills they got through the House but that failed to get through the Senate, from a public option to cap and trade to a much bigger stimulus bill, etc.

How would Obama have gotten around that? I mean, look at what they had to do to get the health care bill through the Senate—they needed to do all kinds of crazy workarounds and loopholes, and even then it barely squeaked through after nearly a year of constant effort.

31. **Matt**
April 18, 2011

And, for goodness sake, Bush brought us Dick Cheney as VP, Donald Rumsfeld as Defense Secretary, and John Bolton as UN ambassador, just to name a few. These were all raging sociopaths. Was there ever a group of more horrible human beings in top US posts than these guys, who did more domestic and global damage? Were Obama's picks of Joe Biden, Bob Gates, and Susan Rice really worse?

I just don't understand.

32. **Peter Woit**
April 18, 2011

Matt,

I did give specifics in this posting:

1. Expansion of the war in Afghanistan. He's Commander-in-Chief.

2. Failure to close Guantanamo as promised. The place is a disgraceful violation of international law. He's Commander-in-Chief, personally directly responsible for the place, and could have flown its inmates to a legal military prison in the US.

3. If he wanted to decrease military spending, he could have proposed this, and vetoed legislation that increased such spending.

4. It was his choice to veto the UN resolution on illegal Israeli settlement activity.

5. Appointment and Reappointment of those responsible for the financial crisis (Bernanke + Geithner).

6. Bonus points for what he has done recently concerning the treatment of Bradley Manning.

You can argue back and forth about whether Obama could have gotten better legislation through the Senate (and now his problem is not the Senate, but that he and the Democrats politically did such a bad job during his first two years that they lost the House), but the record is clear on what he has done on his own.

About Bush Jr.: Yes, he and the people around him were awful and did a huge amount of damage to this country. The question now is whether Obama is going to fix any of it or continue along the same lines. Under Bush Jr., most Democrats were appalled by what was going on, now they're defending it.

33. **Matt**
April 18, 2011

>2. Failure to close Guantanamo as promised. The place is a disgraceful violation of international law. He's Commander-in-Chief, personally directly responsible for the place, and could have flown its inmates to a legal military prison in the US.

In what state? He needed Congressional approval and state approval to move them. Don't you remember all that wrangling going on in his first year, all the crazy US and state legislators screaming that these Guantanamo prisoners were supervillains who couldn't be contained even in supermax facilities, and refusing to vote to allow the prisoners to come in? Don't you remember all the jokes by Jon Stewart about how foolish those legislators were, given that we had man-eating serial killers in these prisons?

In the abstract, it all seems so simple. But the president doesn't have a magic wand. He's got the veto pen, the state of the union address, and he's commander of the armed forces. He doesn't get to vote in Congress, and he can't overturn Supreme Court decisions. Do you know of any off-mainland military prisons other than Gitmo where he could have sent the prisoners, and, importantly, that he could get Senate approval for?

The rest I basically agree with. But that's all small potatoes compared to what Bush did. The Israel UN vote? Who cares? Bradley Manning? Criminal treatment, but come on! Bush would have waterboarded him to death. Bush invaded Iraq for no good reason, and Obama has finally wound it down. Bush presided over the biggest economic collapse in modern history, and Obama turned it around. Bush tortured hundreds of innocent prisoners to death, and Obama's contribution was to keep those photos from getting out. Coca-Cola versus arsenic, again. But perhaps the McCain-Palin ticket would have been more to your liking.

The fact is that Bush was a reckless man, and Obama is not, and that's probably the single most important reason why Bush was far, far worse. And that's more generally true of the GOP versus the Democrats. So the lesson is never, ever to elect the GOP, because they'll put in place all kinds of horrible, horrible policies that the Democrats are too cautious to sharply reverse as soon as they get back into office. The lesson is not to say that the Democrats are worse than the GOP, and that nobody should care if the GOP wins the next election. I'm sure the Paul Ryan budget plan would not be to your liking.

34. **Matt**
April 18, 2011

I'm not making this up. Here are the results of a simple Google search on "guantanamo house vote":

"U.S. House votes to block Guantanamo transfer"
http://www.reuters.com/article/2009/10/01/us-usa-guantanamo-idUSTRE5905EQ20091001

"House votes to prohibit moving Gitmo detainees"
http://articles.chicagotribune.com/2010-05-30/news/ct-met-house-vote-gitmo-05-30-20100528_1_democrats-split-gitmo-center-in-guantanamo-bay

Here's a sampling, from http://entertainment.xin.msn.com/en/radio/938live/headlinesnews.aspx?cp-documentid=4510680

>>The US House of Representatives has approved legislation to stop the moving of terrorism suspects from Guantanamo Bay to American soil

It prevents the transfer of prisoners by prohibiting the government from spending any money to do so.

The vote is a blow to US President Barack Obama's efforts to prosecute prisoners in America.

The spending ban makes it impossible for Mr Obama to follow through on his campaign pledge to close the prison by re-locating the prisoners.

The Obama administration has condemned the tighter restriction.

It argued that Congress should not direct how the administration prosecutes such cases.

There are still 174 detainees at Guantanamo prison.
—

So Peter, what should Obama do about it?

35. **Matt**
April 18, 2011

What drives me most crazy about the Guantanamo fiasco is that Obama is getting it from both sides. The right wing is calling him a terrorist-loving, America-apologizing, naive Marxist for trying to close it down, and the left wing is calling him a capitulating, lying, worthless scum bag for keeping for being unable to get its closure through Congress. If you have any concrete ideas for how to get closure of Gitmo through Congress, please let me know.

36. **Peter Woit**
April 18, 2011

Matt,

One reason for emphasizing the Guantanamo story is that it's the clearest case where Obama had the power to do something and refused to use it. When he came into office, he clearly had the authority as commander-in-chief to issue orders to the military to shut the Guantanamo prison, send transports down there, pick up the prisoners, and fly them back to military prisons in the US. Had he had done this, I seriously doubt that Congress or the States could have gotten a court to issue an enforceable order that the prisoners be sent back to Guantanamo. This might be a bit trickier now due to the Congressional legislation, but I still think he should do this and suspect he would do very well in court making the case that this is not up to Congress, but up to the Commander-in-Chief. Do you really think a court is going to rule that a President leading a nation at war does not have the power to hold captured enemy soldiers on US soil if necessary?

In any case, looking at how he has handled the Manning case and continued a large number of other Bush-era violations of civil rights on national security grounds, it's pretty clear that the reason he didn't do this is that he just doesn't see the issue very differently than Bush did. Again, if we had Bush still as President, I strongly suspect that he would have emptied Guantanamo by now, through one means or another, and if he hadn't, the Democrats would be putting heavy pressure on him to do so. Instead they're rolling over and supporting Obama's disgraceful and illegal behavior on this issue.

As for the politics of it, he has caved in to the far-right and done what they want, but they're never going to love and support him. The "far-left" of people who believe in following conventional legal and ethical standards are not happy with him, but see no alternatives. So he's not making many people happy, but he is pursuing what will probably be a successful political strategy: govern like Bush, driving Republican opposition to the loony extreme right, while making sure no challenger from the center or the left emerges. To make this work he just needs to convince most people that it's not his fault that moderate or progressive policies are not being pursued. So far, this is working quite well for him.

37. **Matt**
April 18, 2011

Actually, Article I, Section 8 of the Constitution declares that only Congress shall have the power to "make Rules concerning Captures on Land and Water." To the extent that Bush made these sorts of decisions unilaterally, he was acting in an extrajudicial–illegal–way. Perhaps Obama should do so as well, but I sort of like the fact that he has decided not to keep breaking the law. Perhaps he was stupid to try to get Congress to act, but that was the legal approach. And his inability to get it done has nothing to do with his desire to get it done. Bush wanted to create Guantanamo, and Obama wants to get rid of it. And yet Obama is worse than Bush?

The Manning case is a travesty. But Bush did not do a single good thing for the country in his entire eight year presidency. (His only positive accomplishment was PEPFAR, but that was in Africa.) Every decision Bush made was a catastrophe and a screw-up that made our lives incalculably worse. He added literally trillions to the debt, and the cost was strangling many of the good things liberals wanted to get done when he left.

Has Obama really done nothing good in his two years in office so far? I listed a huge number. Saving us from total financial collapse has to go up on that list. So was the nearly a trillion dollars in stimulus. You should really read the contents of that bill in detail. It's apparent magnitude seems small only because it all got into one single bill. Read its Wikipedia page:

http://en.wikipedia.org/wiki/American_Recovery_and_Reinvestment_Act_of_2009

The unemployment rate had a major inflection point just when the stimulus package started:

http://en.wikipedia.org/wiki/File:Recovery-JobLossGain.png

Then came the year-long struggle to pass a health care bill, nationalizing the MA plan so that 30 million more people get access to health insurance. And he actually paid for the damn thing, despite having to raise all kinds of taxes and cut Medicare Advantage.

The financial bill was too weak, but at least it wasn't more de-regulation, like Bush favored. Obama saved the American auto industry, despite enormous criticism at the time and calls to let the auto makers collapse. (And take millions of jobs with them.) He ended DADT and stopped defending DOMA. If it hadn't been for the filibuster, he would have gotten cap and trade passed, too, as well as a public option and much more stimulus.

I also mentioned before the Lily Ledbetter Act, the new federal hate crimes bill, and the two Supreme Court replacements. Bush didn't do anything helpful, and Obama did many helpful things. The few bad things Obama has done (AfPak, Manning, etc.) are as bad as what Bush did, but Bush had nothing to balance that out. To say that four more years of Bush would have been better is just difficult to understand. Are you also saying we should all sit out the 2012 election and not vote for Obama?

38. **Peter Woit**
April 18, 2011

Matt,

Since Guantanamo is illegal, Obama is in violation of the law by keeping it open. Saying he is going to keep doing this in order to more carefully follow the law than Bush is just absurdism.

I'm not saying people should sit out the 2012 election. They should make clear that Obama's cave-to-the-right tactics are not acceptable, work to get a better Democratic candidate and vote for him or her in the primary. If this fails, barring some miraculous decision of the Republicans to nominate someone sensible, people won't have any choice and will have to vote for Obama, at least if they live in swing states.

39. **Matt**
April 18, 2011

Oh. That actually sounds like a pretty good plan. Why do you have to be so damn reasonable?

40. **chris**
April 19, 2011

Matt,

you make it sound like Obama is a string theorist. He does not produce results but excuses.

Looking at it from the outside it is just plain disappointing that with both chambers on his side he could get so little done in the first 2 years.

41. **Matt**
April 19, 2011

Chris, in the American system we have this thing called the filibuster. Look it up. Unless you have 60/100 seats in the Senate, there's no advantage in having a majority.

42. **Matt**
April 19, 2011

Again, to get a truer sense of what Obama and the Dems were trying to accomplish, look at what made it through the House. Unfortunately, except for a brief period after Spector switched parties, the Dems didn't have enough votes in the Senate to overcome the filibuster. It's a frustrating peculiarity of the American political system.

43. **chris**
April 20, 2011

matt,

if someone is too cowardly to bring in an initiative just because a stupid loophole would in principle allow to veto it is not really convincing.

i always hear a filibuster as an excuse by the dems, never have i heard of them threatening to do it against the other side. i would guess that after 2 years of constant filibusters voters might grow weary of such strategies, but all of the us seem to accept this extreme measure as bussiness as usual – like a nuke, that does not need to be employed, just its existence blocks change of any sort.

44. **Matt**
April 20, 2011

Voters do not "grow weary," because the vast, vast majority of voters have no idea what the filibuster is. Instead they just blame whoever has the majority.

To get a sense of the scale of this massive institutional problem, see

http://voices.washingtonpost.com/ezra-klein/2010/12/breaking_the_filibuster_in_one.html

Most of people's gripes with particular political parties are really problems with the system itself, problems that are insanely difficult to fix. (And outside the president's powers to fix, by the way.)

45. **jpd**
April 20, 2011

@Matt for historical accuracy, when Ted Kennedy was alive
the Dems had 60 (same number as with dem spector)

46. **vorpal**
April 20, 2011

@matt,

Obama didn't hesitate to skirt the Constitution when initiating a war in Libya, yet when it came to Gitmo he followed, and continues to follow, the "law" to a T.

I think the contrast in attitude Obama has shown in the above two policies shows where his heart lies.

47. **Matt**
April 20, 2011

jpd- As I said very clearly, the Dems had 60 votes in the Senate only for a very brief period, a few months, after Spector switched parties and before Ted Kennedy died and was replaced by Scott Brown. (Here I'm treating Lieberman as a quasi-Dem, which is frankly questionable.) But nothing can move through the Senate that fast, because of cloture votes, holds, and other parliamentary delaying tactics that the GOP gleefully employed.

Given your blatant error being brought to your attention, I'm sure you'll revisit your opinions, right? (Ha!)

vorpal- So far the president refused to declare war on Libya, despite both Dems like Kerry and Republicans like McCain pushing him to do so, and has sent no troops over, both acts that would have required Congressional approval. So far he's made menacing gestures and fired missiles, but left the heavy lifting in Europe's limp-wristed hands. As a NATO treay signatory, Article 6 of the Constitution makes certain demands of us.

Let me know when Obama changes his mind and decides to invade. Given his (and his Sec Def's) blatant distaste for getting involved in another Middle-Eastern conflict, I'm not holding my breath.

48. **Matt**
April 20, 2011

Indeed, without a declaration of war, the legal status of presidential authority in matters of no-fly zones is rather murky, especially when there's a situation of acute humanitarian crisis and UN approval (such as Libya, or Clinton's intervention in Kosovo, although the UN probably would have approved intervention in Rwanda and Darfur if Western powers had pushed for it).

See Wikipedia's entry on the War Powers Resolution of 1973, especially the section on its constitutionality:

http://en.wikipedia.org/wiki/War_Powers_Resolution

See also as this piece specifically on the Libyan no-fly zone by Michael Kelly, president of the U.S. National Chapter of L'association International du Droit Penal:

http://jurist.org/forum/2011/03/the-president-does-not-need-congressional-approval-for-libya-no-fly-zone-yet.php

Whether you approve of the Libyan intervention or not, Clinton's intervention in Kosovo was certainly as significant, so Obama is hardly an outlier among Democratic presidents on this one. Inasmuch as this is a debate over whether Obama is worse than Bush, who actually invaded two Middle Easter countries at a cost of trillions of dollars and thousands of American lives, I'm afraid I can't concede this point.

49. **Matt**
April 20, 2011

These last few comments really typify why this belly-aching gets on my nerves so much. These criticisms are mostly based on profound misunderstandings and ignorance about presidential powers and the basic structure of American's political institutions.

The system itself really sucks. It's poorly designed, to a large extent because the founders didn't understand the sociology of party politics. Hell, they thought the Senate was going to be a nonpartisan body of elders! (And the Senate is probably the single dumbest part of our system.)

The president exists inside this highly constrained, frustratingly obstacle-ridden system, and criticisms should be deployed with all these factors in mind. It's far, far easier for a reckless president to be destructive than for a cautious president to clean up the mess. That's the breaks, unless you all can figure out a concrete, well-though-out way to change the system.

Any ideas?

50. **jpd**
April 21, 2011

whoa, "blatant error"? i wasn't even involved in your discussion, just pointing out the Spector count was the same as the Kennedy count.
but now that you bring it up, why is it so easy for obama to invade yet another country but ie not close guantanimo?

51. **jpd**
April 21, 2011

rhetorical question, dont bother answering

52. **Matt**
April 21, 2011

Because the American political system makes it far easier to fire missiles than to relocate military prisoners. There needs to be a place that agrees to take the hundreds of them in. If you'll recall, Obama was pushing hard for several states and European countries to take them, but without much success. For a while Yemen was taking prisoners of Yemeni nationality, until the Yemeni Christmas underwear bomber fiasco made that untenable. Obama also found a place for the Uighurs.

I'm not going to repeat my arguments again. Read my previous comments. But it's clear Obama wants to close Gitmo, and has put much effort into doing so, no thanks to Congress or state governments or Europe. There's no simple solution. The lesson here is not to let the GOP come to power in the first place.

By the way, they just agreed to move Manning out of the hell hole he was in. We'll see what happens.

53. **vorpal**
April 21, 2011

@matt,

So Pearl Harbor was not an act of war since Japan didn't commit ground troops?

If Libya "just" shot missiles at Gitmo (to help protect the detainees), that would not be interpreted by the US as an initiation of war? Or do I have a profound misunderstanding of America's political institutions?

54. **Matt**
April 21, 2011

I didn't say act of war, I said declaration of war. You can drive a truck through that legal loophole.

Putting legal issues aside, if you can't see a moral distinction between Libya and Pearl Harbor, I'm not going to try to explain it you.

55. **Politically naive**
April 21, 2011

Clearly people who are not allowed to return to their home country count as refugees and thus eligible for specific treatment under treaties to which the US is a signatory? There are clear legal procedures for such an occasion.

As for Pearl Harbor vs. Odyssey Dawn, indeed, those are very different: the first was a miscalculated military response to an economic blockade as part of the maneuvering between two imperial powers, the other is an intervention in a foreign civil war for the purpose of maintaining secure access to cheap oil.

56. **Matt**
April 21, 2011

What treaties to which the US is a signatory would they be covered under?

And, oh, how original! A claim that it's a war for oil. You're quite a genius! Please shower us with more of your rarified wisdom.

Such lazy thinking. Thank you for reminding me why I could never find myself at home with the Left. (If only the Right weren't crazier…)

Given that there's obviously no way to prove a negative, I'll just say that letting the genocidal madman Qaddafi crush the rebel movement would be a far cheaper and more reliable way to keep Libya's oil flowing to the West. Turning against Qaddafi, with whom the US formerly had a stable relationship, and supporting the rebels in an indefinitely-long conflict would be the last thing you'd do if oil was the priority.

And Obama and Bob Gates resisted getting involved for weeks, almost till it was too late. You can read plenty of in-depth reporting online that got inside the White House during the crazy time when all the deliberations were going on. It was Hillary Clinton, Susan Rice, and Samantha Power who are most responsible for US backing of the UN resolution and US intervention. (And who's a more blood-thirsty imperialist hound for oil than Samantha Power, right?) Nobody wantes this to be another Rwanda; they were all hoping for Kosovo.

Do some reading, and then you can speak without looking foolish.

57. **Matt**
April 21, 2011

And I'll let slide your typical left-wing moral equivalency between Imperial Japan and the isolationist US in WWII.

58. **Matt**
April 21, 2011

You know what? There's no point in further discussing any of this with all of you. You've all made up your minds already, and no amount of further evidence is going to change any of your minds.

What I will say is that if you any of you think Obama is as bad as any of the current GOP contenders, and that your voting in 2012 is meaningless, then you're sadly misguided.

Obama/Biden 2012. Peace out.

59. **Politically naive**
April 21, 2011

As to refugees, there is
http://en.wikipedia.org/wiki/Convention_Relating_to_the_Status_of_Refugees
to the Protocol of which (the version which extends outside of Europe) the US is a signatory. Under that, a refugee is defined as

"A person who owing to a well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group or political opinion, is outside the country of his nationality and is unable or, owing to such fear, is unwilling to avail himself of the protection of that country; or who, not having a nationality and being outside the country of his former habitual residence as a result of such events, is unable or, owing to such fear, is unwilling to return to it.."

If those countries refuse to accept the detainees or if there is a real fear that they will be persecuted in their countries of origin, they seem to be covered by this convention. They will not be the first or only refugees living in the US, where in most cases the US is not directly responsible for them having become refugees. That is definitely no excuse to bar them from their basic human rights.

As for Libya, that truly sounds very noble until you look around the Middle East and note that the US does not even deign to pressure regimes with which it is in much closer contact when they butcher their civilians for protesting the government, e.g. in Yemen or Bahrain. It is clear that nobility has nothing to do with it; the danger of a civil war resulting in a conclusion that is bad for US and European oil interests in Libya seems a much likelier impetus, especially in light of other "comrades" already being deposed or removed.

Nevertheless, I am open to other suggestions; unfortunately, you have left the stage after bombarding us with further abuse and ad hominems which are presumably based on a background respect for the humanistic tendencies of particular members of the administration, as if that mattered. Finally you conclude with a campaign sticker. Marvelous.

New York, New York

April 1, 2011
Categories: Uncategorized

Another topic I hope to write about extensively is that of New York City, including discussing the wide range of cultural events going on, as well as the amazing restaurants. On the subject of food, I should give a plug for my friend Nathan Myhrvold's new book Modernist Cuisine. It's been getting rave reviews, and the first printing has sold out. I've been promised a copy from the second printing, and Nathan tells me that, "while it's not a coffee-table book, you could use it as a coffee table…" I'll report once the book arrives.

I've been in and out of New York City since the earliest times I can remember, which were in a suburb 25 miles north. My mother was born here and my father came here by himself as a 17-year old after the war. The place has changed quite a bit over the years, and some of the changes of the past few years are quite remarkable. These days, most of Manhattan is filled with new or renovated architecture, everything fixed up to a high level of gloss, and virtually crime-free. With one bedroom apartments going for a million dollars in many neighborhoods, if you trip on your shoelace you're likely to take down a couple millionaires. These people are not going to mug you, and any outsiders who might think of this are deterred by the intense police presence, especially since 9/11. The only exception is bicycle theft, which is rampant, and doesn't much interest the police. Last summer I came out of a store on Broadway mid Sunday afternoon to find a group of guys with bolt cutters freeing my bicycle from its chains. No one seemed to find their activities unusual or worth doing anything about.

Back in the 1980s there was a lot of talk of "gentrification", as poor people were displaced by well-educated young middle-class people. These days a new word is needed to describe what is going on, perhaps "plutocracification". Someone who lives in Tribeca described to me how 20 years ago the neighborhood changed as lawyers and doctors moved in, and artists moved out. Nowadays, the lawyers and doctors are getting pushed out as the hedge-funders and investment bankers arrive. It's hard to overstate the effect of the financial industry in Manhattan, where supposedly it provides half the personal income, with much of the rest of the economy based on catering to this new wealth. Bank branches are everywhere, often taking up four corners of an intersection, with long swathes of expensive commercial street frontage devoted to cubicles for not very well-paid bank employees, most of which are normally empty.

In late 2008, there was a blip there for a moment, and I even saw one bank branch get closed. That didn't last long though: apartments are selling again at high prices, new bank branches are opening, and you can't get a reservation at a long list of popular expensive restaurants. Midtown streets are impassible, filled with fleets of massive black SUVs, their bullet-proof windows tinted dark. Used to be that the rich favored limos, but no longer. No one knows how long this will last, but the city is partying like it's 2011. Huge cuts to the budgets for schools and the city university system have just been announced, but most Manhattanites are unconcerned, since they would never have their children educated in public institutions.

One side effect of having a lot of rich people from many different countries is that the restaurants in Manhattan tend to be spectacularly good. Some are trendy and rather expensive, but for not a ridiculous amount of money you can get a fantastic meal, and you have to go out of your way to find a bad one. I'll be writing extensively about some of my culinary obsessions, one of which is barbecue. At this point I might argue that New York has better barbecue than just about anywhere else in the world. Just down the street from here (108th and Broadway), Rack and Soul has some of the best ribs I've ever eaten. On 26th St., Hill Country has taken the best sausage and brisket available in Texas (Kreutz's in Lockhart), stolen it and brought it here to the city. Over in Williamsburg, you can get great barbecue with the best pork and beans I've ever seen at Fette Sau. The list goes on and on….

Recently opened near here just off 125th St. is Marcus Samuelson's Red Rooster Harlem, where I recently had a wonderful lunch. Getting a dinner reservation is not easy, and some days the restaurant is packed with the power elite. Last week Obama took over the place for a $31,000/head dinner with his friends from the hedge funds. Here's the menu. This is a typical story of the new New York. In what used to be pretty much a slum, now there's a beautiful restaurant with some of the world's best food. The wealthy may sometimes monopolize it, but if you're a New Yorker and play your cards right, you too can participate in the fun and get a fantastic meal in a gorgeous place, at a not unreasonable price.

Comments

1. **JSE**
   April 1, 2011

   Come now — NYC has a glorious array of great restaurants, but you hardly "have to go out of your way to find a bad one." In fact, I think the median is pretty bad. New York has tons of people who never cook, so mediocre restaurants can stay in business there in a way they never would in smaller cities.

2. **Casey Leedom**
   April 1, 2011

   And I'll offer that NYC is hardly, even close to Great BBQ. I do better here in Palo Alto. And if I take you to Flint's in Oakland, you're in trouble. And if we heads on down south, well, you gets whats you deserve.

   It's not that the "interpretations" of BBQ in NYC are bad. They're not. They are in fact Great and even Spectacular. But, in the end of the day, they're really simply "different," interesting, etc.

   When you hear Jimmy Hendrix's cover of Bob Dylan's All Along the Watchtower, it's not that's he's devalued what Bob did. He's just offered a completely different take. In the end of the day, there's still the validity of the original.

   And speaking of which, I feel a big desire to cook some BBQ this Saturday. And that ain't no April Fools Day joke!

3. **Looking for BBQ**
   April 1, 2011

   Casey,

   Where in Palo Alto?

4. **milkshake**
   April 1, 2011

   this reminds me of an excellent documentary "Cocaine Cowboys" about late 70s and 80s – half of the Miami downtown was built with drug money. Hedge fund gangsters at least wont spray you with bullets.

5. **Deane**

April 1, 2011

I agree with JSE that most New York restaurants are mediocre or worse. It was easier to find good food in Houston. The neighborhood around Columbia has very little to offer in good food. My family and I have tried Rack and Soul, but have never been extremely impressed. I'll have to try Hill Country some time, because I really miss good Texas barbecue. I was, however, very impressed by the food at Smoke Joint in Fort Greene (about two blocks from BAM), both the meat and the sides.

6. **John**
   April 2, 2011

   Just a factual note: those black vehicles in midtown are Lincoln Towncars, and I've never seen one that was bulletproofed.

   If you wanted to see armored black SUVs, Obama's motorcade on 5th ave a day or two ago was the place to be.

7. **frank**
   April 2, 2011

   I too have been disappointed trying to find good authentic BBQed Texas brisket in NYC or anywhere else outside of the Texas. Unless you are lucky enough to be a guest on a Texas ranch that slow smokes over mesquite for 8 to 12 hours briskets from 5 to 10 year old steers that have never tasted any grain and have a nice thick layer of fat then you should just 'forgetaboutit'! Real Texas BBQ does not involve sauce. The flavor comes from the fat and the smoke. The skill and patience of the cook turns the toughness of the brisket with its associated gristle into tender gold. With its omega3/6 ratio exceeding one, its fat is as healthy and nutritious as it is delicious.

Biking in New York

April 1, 2011
Categories: Uncategorized

I've been biking in and around New York City for many years, recently doing several thousand miles a year, and this should provide many topics for the blog. Look forward to, for example, an explanation of how to best get across the Passaic River to Newark on bike. Biking in Manhattan has always been a challenge, but things have gotten exciting recently. A few years ago the city started painting lines on some of the streets, announcing that these were "bike lanes". They're generally filled with double-parked cars or trucks, and pedestrians hailing cabs or waiting for a break to run between the traffic. The width is carefully chosen to coincide with the width of a car door, so if you ride inside the lane you're guaranteed to properly get "doored" by people leaving their parked cars. The act of painting the line has the added feature of making it illegal for bicyclists to ride outside of it, at a safe distance from the cars.

The latest news is that a few special, protected lanes have been created, with cars parked outside the lane. These lanes go for a few blocks, and are heavily favored by delivery people to store what they're working with, tourists taking pictures of each other, parents changing their baby's diapers, or basically any activity that pedestrians would complain about if it was done on the sidewalk. The new lanes have enraged some powerful New Yorkers, who are now on a "bikelash" campaign to get them removed. They've managed to enlist the police, who have a long history in Manhattan of fighting with bicyclists, and have started up a serious campaign of legal harassment.

I used to ride regularly in Central Park, which has a 6 mile long road winding through it, most of the time closed to traffic. A couple months ago the police started issuing $270 tickets to bicyclists for not stopping at any of the 50 or so traffic lights (it seems that when traffic is not allowed, bicyclists must obey the traffic lights anyway, runners or pedestrians no). This caused almost all bicyclists to stop riding in the park, but a few kept on anyway. The police then decided that the speed limit should be 15 mph for bicyclists, and set up a speed trap at the bottom of a hill early one morning, ticketing quite a few people. Later they changed their mind about this, and decided the law really was 25 mph. Teams of armed police were dispatched to appear at homes of the 15-25 mph ticketees in the evening and tell them a mistake was made, while continuing to make clear that if they didn't stop at traffic lights when there was no traffic, they would still be ticketed. And if they were going faster than 25 mph at the bottom of a hill, there would be trouble. I'm sure they found this very reassuring. Personally, I've stopped riding in the park. It turns out though that there's a platoon of undercover police throughout the city in unmarked cars waiting to start up sirens and go after any bicyclist who violates any rule in the hundreds of pages of regulations governing not just bicycles, but motor vehicles (their slogan: a bike is the same as a car!). Recently I ran afoul of one of these due to rolling very safely and slowly through an intersection, which got me not one, but two \$270 tickets. I'll appear in court on these charges some day soon, and I'm sure my readers will want to hear all the details of how this works out.

Update: A commenter correctly points out that I got the Passaic and Hackensack Rivers confused, it's the Hackensack that is difficult to cross by bike down around Newark.

Comments

1. **Casey Leedom**
   April 1, 2011

   Dude. I'm sure you've seen this already but, whatever. If you're a bicyclist in NYC, this is _mandatory_:

   http://www.youtube.com/watch?v=6nQs7u3fDXc

   That aside, I've been focused on getting back into biking shape recently so if you're ever in the San Francisco South Bay, drop me a line and I'll schedule a ride. Currently I'm focusing on getting in shape enough to do "The Coast Ride" again this summer (~62 miles, ~6,200 feet vertical elevation gain). It's more a question of perseverance than capability … 🙂 But it _is_ beautiful. And there's the "Terminator" on Stage Road between 84 and Highway 1 …

2. **Chris Oakley**
   April 1, 2011

   Peter, this change of tack of your blog is only going to viewed as a surrender in view of the announcements this morning, namely that Motl has been re-appointed – this time as a full professor – to Harvard, and that Distler has been nominated for the 2011 Nobel Prize in physics ("for contributions in showing the world that surf bums are surf bums, and will never be anything more").

3. **Agent314**
   April 1, 2011

   Even foolish April cyclists are welcome in San Francisco. Peter, why don't you move out west to our fair city of SF?

4. **Blaaaaaack Ice**
   April 1, 2011

   I'm tired, soooooo tired, of taking that long uphill walk to the methadome clinic. There's got to be an easier way. Now I suggest to you it takes 5 seconds, just fiiiiiiiive seconds, to put a morphine suppository all the way inside. Yeah!

5. **Christian Takacs**
   April 2, 2011

   The problem with riding a bicycle anywhere is that it will make you two tired.

6. **John Romeo Alpha**
   April 2, 2011

   Blog symmetry is preserved, as I have recently decided to become a cranky physicist.

7. **Ed**
   April 2, 2011

   A sophomoric pot shot at Clifford Johnson? That's pretty low.

8. **Peter Woit**
   April 3, 2011

   Ed,

????

I disagree with Clifford about string theory, not about biking.

9. **Ed**
   April 3, 2011

   So, you're telling me that your April 1st incursions into the world of inane bike and food related commentary were in no way spoofs of Asymptotia? My apologies then.

10. **Peter Woit**
    April 3, 2011

    Ed,

    Inane? Those were honestly my heart-felt opinions about bikes and food. I don't personally think it's a good idea to go on about them on my blog under usual circumstances, but the great thing about blogging is that everyone's blog is different. And the virtue of blog posts you're not interested in is that you can not read them…

11. **AL**
    April 4, 2011

    Contrary to the yelps from those who find this post unphysic(s)al, it seems to me quite relevant to any enlightened theoretical mindset and interesting to all who share the joys of bike riding in NYC, given that a bicycle is second only to the wok as a peak of the engineering of a tool's form to its use ie it is one of the simplest arrangements of simple components -basically two wheels and a frame, handlebars, pedals, chain and a saddle – to achieve maximum payoff in terms of usefulness and other benefits such as outdoors, sunlight, ability to get off and on anywhere instead of official locations, exercise of lower body muscles, etc. In other words, a physics problem (urban transport for the individual) perfectly solved.

    Be that as it may, one has to admit that the riders on the recommended video are foolishly ignoring the fact that most cars weigh enough to kill them in a collision and in New York City are usually driven as if they were beds and the drivers half asleep, at least to what is happening on either side of them towards the rear, for the reason US automobiles are very hard to see from, and anyone who cuts right through the narrow passage between two buses is asking for disaster. So one doubts that they are trained in elementary physics.

    But actually I came to comment mainly to make the point that the complainers are underresearched in the matter of whether blogs should stray from their primary topic. If they pick up a copy of this weeks New Yorker (April 11 2011) they will find that James Surowiecki their excellent Economics columnist quotes many studies which have shown that if you distract people from the main task they will perform better at it ie if you let them have the Internet at work they will do more and better work. The link is http://www.newyorker.com/talk/financial/2011/04/11/110411ta_talk_surowiecki). So blogging actually should be alleviated by wandering from the main topic from time to time, because it will increase the quality of everybody's attention when it gets back to it.

    Anyhow biking in New York is a very great cause which should be promoted until cars are largely replaced. The fact that it has such distinguished support is important news in its own right.

12. **jensph**
    April 5, 2011

    Too bad this topic was just an April Fool's diversion, because I was all set to explain that it's not the Passaic River that's difficult to cross on bike, it's the Hackensack River…

13. **Peter Woit**
    April 5, 2011

    jensph,

    Of course you're right, I got those confused, will fix. I've tried this once on Lincoln Highway (Rt. 9), once on Newark Turnpike (Rt. 7), neither of which was an experience I'd like to repeat (getting a flat in the middle of the Rt. 9 one didn't help..). Final conclusion was that one really has to go all the way North up to around the Teterboro airport to make this crossing sensibly.

14. **AL**
    April 5, 2011

    May I add that I for one am agog to hear how your $270 x 2 ticket fighting goes, though I suspect you will be let off. If it is any encouragement, a speed trap on the path crossing Central Park at 97st last autumn at 5pm ie at twilight caught me (actually it didn't, but the cop said "Oh no, we saw you get off the bike after you rode twenty feet into the path to talk to your friend who was warning you we were here!" That was when I walked up to them and asked how many tickets they had given out so far. They immediately added me to the list!

    As it turned out when I got to the court's payment windows downtown to say I was innocent they said Don't worry about it! the ticket had been cancelled! That was a relief, especially when I learned that it cost more to proceed with a second court date to have a hearing than it would to pay the $50 fine.

    Your stories sound as if they have ratcheted up the irationality since then, however.

15. **jensph**
    April 6, 2011

    Peter,

    I have also tried the Rt 7 and 9 crossings once. The Rt 7 bridge is particularly bad since you have to climb over the guard rail… Never again. There really is no safe crossing south of Teterboro. The only safe option is to take your bike on the PATH.

    There is talk of adding a bicycle crossing to the designs for the new Portal Bridge (which is the NE Corridor train crossing) just north of Rt 7. But I haven't heard much on that over the last year.

Dessert

April 1, 2011
Categories: Uncategorized

Another important topic that this blog will cover is that of baked goods. While every street-corner in Paris has a wonderful bakery, they're hard to find in the US. Luckily for me, there's Silver Moon at 105rd St. and Broadway, which could be the best bakery in the city, and often is the place where I start my day. Another related fine source of sugary goodness is the Wafels and Dinges food truck that spends Monday near Columbia, providing a wide array of waffle possibilities.

Until recently, a sad fact about life in New York City was that you couldn't get a religieuse. This situation has now been rectified, with La Bergamote at 20th St. and 9th Avenue an excellent source. On my last trip to Paris I was introduced to a French pastry treat I'd never had before, a Breton specialty called a Kouign Amann, which is available quite a few places there. Unfortunately, as far as I can tell, such a thing is not for sale in New York. I hope that this shocking situation will be rectified soon, and will report on any progress.

Comments

1. **D R Lunsford**
   April 1, 2011

   The only place I know of anywhere near, that has really good bread and lots of it, is New Orleans. Natives will say, one of the things they miss most when they get relocated, is the bread. Maybe multiverse mania is a side effect of processed bread.

   -drl

2. **Cents**
   April 1, 2011

   Is it after midnight already?
   It sure must be where you are!! 🙂

3. **Kea**
   April 1, 2011

   So which post is the April Fool's joke? They all sound pretty depressing to me – this one because I can't often afford to indulge in delectable pastries.

4. **Crackpotl**
   April 1, 2011

   I always love your April Fool's jokes.

5. **martibal**
   April 1, 2011

   Shame on me… being french (but not Breton), I do not know what a Kouig Amann is. Could you explain ?

6. **Casey Leedom**
   April 1, 2011

   Okay, when I was 19 — and many eons ago — I lived with a "sugar-holic" in a "retro-converted" Community Hospital building. E.g. Condemned in any other universe. So I was 19 and devoid of any free will other than that offered to me by the external universe. I.e. I ate what my house-mate ate without any questions. After five months of eating Chips-Ahoy-Chocolate Chip Cookies, Mountain Dew and Dinty Moore Beef Stew I've never been able to face sugar in the same way since. I now _hate_ all of the above and struggle to cope with anything even vaguely sugary.

   These days, I'm with an old friend of mine: another [savory] main dish would be my favorite dessert.

   And by the way, since we _are_ talking about NYC, I'm thinking Katz's and Peter Luger's.

7. **Casey Leedom**
   April 1, 2011

   So I've processed your posts in reverse chronological order as per my typically stupid email response pattern. But, that aside, here's my overall view:

   Life is great, NYC is … something unique that we're all "blessed" to have available … and, I repeat, go to Katz Deli, get [your favorite sandwich], grab a beer, sit out on the sidewalk and watch The People drift by. We may not be — and probably aren't — Special Creatures, but our lives are wonderful.

8. **JAC**
   April 1, 2011

   The dessert of God is threads of spun sugar vibrating in 10 dimensional spacetime.

9. **Breton from Paris**
   April 1, 2011

   The kouign-amann (kouign with an N) is indeed a delicious specialty

   Not very light, though -in breton language, kouign=cake and amann=butter- it is literally a "butter cake" 🙂

10. **Peter Woit**
    April 1, 2011

Thanks Breton and others, typos fixed: Kouign has its n back, and Silver Moon is at 105th St.

11. **Pablo**
April 1, 2011

Are you gonna also change the blog's name to 'not even wafles' or whatever? You got me in this one!! Thanks for the good laugh.

12. **wrep**
April 1, 2011

Perhaps the `Woit Report' ~ "We're On It Totally"? (The definition of `it' being subject to loose interpretation …)

13. **Peter Orland**
April 1, 2011

Oh good, a physics topic. Imagine my relief. What about that dessert between the standard model scale and the aptly-named GUT scale? Perhaps the LHC will find lots of particles (instead of the dessert) beyond the berryons, such as croutons or raisons.

14. **Tommaso**
April 1, 2011

Silver Moon – I think it's the one we went to last September, right Peter ? I confirm, delicious pastry!

Your April fool is much less dramatic than mine. I suggest to first read mine and then yours, to overcome the depression with some sugary thought.

cheers,
T.

15. **Deane**
April 1, 2011

Peter, I agree that Silver Moon is a great bakery. Even the bread is great. I love their rustic baguettes, which compare well against the better baguettes in Paris. We're very lucky to have it in our neighborhood, which otherwise has mostly rather mediocre food.

16. **Tom W.**
April 1, 2011

With three minutes left in April 1, maybe it's safe to add:

http://www.halfbakery.com/

17. **GT**
April 2, 2011

Seriously? Come on. I just started reading your book yesterday. I beseech thee, for humor wherefore thou presently posteth!

18. **anon**
April 2, 2011

there's two hours left over here suckas.
String theory rules!!!!1!!!!
i got your 11th dimension right here

19. **Christian Takacs**
April 2, 2011

Eat, drink and be merry, for tomorrow we may diet.

20. **Yoshi**
April 2, 2011

The best bakery ever is located in Montreal, 322 av Mont Royal, and it is called "Kouign Amman". They have Kouign Amman, and their croissants and their pains-au-chocolat are even better than in France !!

21. **MathPhys**
April 2, 2011

When a friend of mine, on a visit to some place in Florida, asked "Is there a place where one can get a serious espresso in this town?", he was told "No, but you can open one".

So, Peter, you want a serious French bakery? you can open one. You've probably discovered a hole in the market.

22. **John**
April 2, 2011

There are occasionally serious French bakeries in Manhattan, but they tend not to survive. Fauchon used to do an excellent kouign-amann.

Bouley makes something they call by that name, but it would be unrecognizable to a Breton.

Back to the Usual

April 2, 2011
Categories: Uncategorized

Since April 1 is over, choice of blog topics will revert to the usual, and at some point I'll get around to reverting the logo. There are plenty of other blogs covering desserts, biking, and what's wrong with Obama, so I think I better stick to my market niche.

In Langlands/String Theory/Media news, the last episode of The Big Bang Theory (The Zarnecki Incursion) starts off with a scene where the white-board in the background has a central object in Langlands theory. It's the representation of the upper-half-plane modulo SL(2,Z) as an adelic double coset, with a picture of a tree for the prime p=2. Next week, my colleague Brian Greene will make a guest appearance. For more Big Bang coverage, see Lubos, who really does seem to think that he's Sheldon.

Later today I'll post the usual tedious new review of a book about the multiverse. But, first I need to get breakfast at Silver Moon, and the weather is great for a bike ride…

Comments

1. **Chris Oakley**
   April 2, 2011

   No offers to appear yourself, reading from "Not Even Wrong"?

2. **Per**
   April 2, 2011

   The below was a very clever way to put something off topic, but still relevant, into the blog without having to defend your position for it 🙂

   Peter – Raders : 1 – 0

   P

3. **Per**
   April 2, 2011

   Raders should of course have been readers. Sigh.

4. **NE1**
   April 2, 2011

   Wow. You got me. I need to be less easily amused?

5. **Navneeth**
   April 2, 2011

   You almost had me remove your blog from my Google Reader subscriptions, Dr. Woit. And let me congratulate you for pulling off the only the April Fool's joke (IRL or on the interwebz) that actually fooled me.

6. **Nono**
   April 2, 2011

   Lubos has just publicly admitted that there actually was a global warming in the last two decades of the twentieth century. Unless it was his April fool's joke, things look really bad on the climate front. Now I'm sure we're all going to get fried in a few years.

7. **Anonymous**
   April 2, 2011

   Sheldon quit career a few times, but unlike Lubos, he always returned…

8. **Desperate Scientist**
   April 3, 2011

   It was a joke?
   Oh, dear….

9. **tulpoeid**
   April 3, 2011

   I at least hope that you won't also revert to the usual fascist (sic) policy about comments. Actually imho it's not fascist and this was on purpose exaggerated in order to make it appear milder; it's not fascist since it's your own blog, but it's definitely ill-defined.

10. **Bugsy**
    April 3, 2011

    Peter: welcome back! But actually I enjoyed the change so much I suggest you invite other-universe self to guest comment once a month…
    !!!

    As for Obama, what you say is so sad but true. On Libya he may for once be right (see Juan Cole's excellent blog), but I don't question for a moment his capacity to cave in to intrinsically arrogant neocon sidekicks who will screw things up once more. And what do they care? Their kids all go to expensive private schools; money can buy a mountain retreat if the beaches get flooded, so the future is assured; Koch is paying the bills, investments have been made, and Libya is a land far far away…the better to test out the latest ideological theories on…

11. **Peter Woit**
April 3, 2011

    Bugsy,

    Thanks, but part of back to the usual is that ruthless fascist deletion of all comments attempting to carry on discussions about politics will now resume. Same goes for biking and dessert discussions.

12. **cormac**
April 3, 2011

    'the usual tedious new review of a book about the multiverse'?
    Did you mean a review of a tedious new book about the multiverse?
    I've just finished a review of kaku's latest book for The Irish Times, not at all what I expected

13. **Low Math, Meekly Interacting**
April 3, 2011

    I chuckled through my tears, as it were. The irony of finding the words here was overwhelmed, somewhat, by the sadder irony of how reasonable I found them.

14. **Joe S**
April 11, 2011

    You don't realize how close I came to deleting the bookmark for this site. Good one!

Multivers: Mondes Possible de l'Astrophysique, de la Philosophie, et de l'Imaginaire

April 3, 2011
Categories: Book Reviews, Multiverse Mania

While I was in Paris recently I picked up several French books that aren't readily available in the US. One of these is entitled [Multivers: Mondes Possible de l'Astrophysique, de la Philosophie, et de l'Imaginaire](), and it takes the form of a conversation between theoretical physicists Aurélien Barrau and Jean-Philippe Uzan, as well as historian of science fiction Patrick Gyger and philosopher Max Kistler. Astrophysicist Isabelle Joncour acts as moderator. The conversation is often dominated by the provocative philosophical flights of fancy of physicist Barrau, with philosopher Kistler playing the role of providing sobriety and down-to-earth arguments.

The contrast with the typical Multiverse Mania books of the Anglo-Saxon world is striking. French intellectuals are seriously educated in philosophy, and think it natural to carry on arguments invoking the ideas of a wide range of philosophers, even in contexts such that similar Americans wouldn't see the point of raising philosophical issues. In this case, some of the discussion revolves around the ideas of American philosophers David Lewis and Nelson Goodman about "possible worlds". It's amusing to note that it would probably be extremely difficult to get together for a discussion a group of American physicists who had even heard of these two of their countrymen, much less be capable of seriously discussing their ideas. Maybe that's just as well though, as professional philosopher Kistler makes a good case that the "possible worlds" at issue in this sort of philosophy don't really have anything to do with the multiverse.

Barrau takes a position refreshingly agnostic about string theory and LQG, deploring the ideological warfare between them. Unlike most physicists though, who were interested in string theory when it might have predicted something and are now losing interest, he claims that the fact that it can't predict things is what got him to really like string theory:

> la theorie des cordes commence à m'intéresser à partir du moment où, précisément, elle prend ce tournant où l'on ne sait plus très bien où on va et où on change les règles du jeu au milieu de la partie. Ca devient très motivant!
>
> string theory starts to interest me precisely from the moment where it takes this turn; one doesn't much know where one is going and one changes the rules in the middle of the game. This starts to become appealing!

For Barrau, it's just when string theory starts to turn into pseudo-science that it interests him. In brief, he agrees that the string theory multiverse moves the field from physics into metaphysics, but thinks that's a *good* thing. He's in love with the idea of finally being free from many of the conventional constraints physicists labor under as they try to do science and the possibility of taking up again the overlap of some French philosophy with science that Alan Sokal very successfully made a joke of. He starts out:

> En philosophie francaise, je pense à Deleuze et à son rhizome, au "plus d'un" de Derrida, au(x) toucher(s) chez Nancy, au nominalisme de Foucault, a l'ontologie du multiple de Badiou…

and immediately realizes that the question of Sokal must be addressed if you're going to go on like that:

> Je crains que l'on soit encore dans une sorte de timidité généralisée qui est peut-être issue des contrecoups de la triste affaire Sokal. Il est tout à fait souhaitable d'enjoindre les gens a ne pas dire n'importe quoi. Cela ne se discute pas. Mais il serait dommage que cet excès de précaution leur interdise tout simplement de penser à partir des constructions scientifiques. La physique d'aujourdhui me semble fabuleusement propice à philosopher, il faut oser.
>
> I fear that we're still in a kind of generalized timidity that may have come about as a consequence of the sad Sokal business. It's completely desirable to insist that people not say just anything, that's not up for discussion. But it would be a shame if too much caution keeps them from thinking starting with scientific constructions. Contemporary physics seems to me fabulously propitious for philosophizing. One must be daring.

There's much abuse directed towards Popper, falsifiability, and of crude attempts to separate science from non-science. Barrau is very happy with the idea of not having any way of distinguishing the two, while Kistler tries to remind him that, tricky as it may be, there's an important distinction involved. What seems important to me here is maybe more of a sociological than philosophical point, and it's a bit like the one that motivated Sokal. If you don't have any standard at all for what is science and what isn't, you lose control of the powerful role of science in how we see the world, and put yourself at the mercy of socially stronger forces who will be happy to take on this role and grab control and power away from those who have disarmed themselves. In the specific local area of fundamental physics, if there's no way to recognize that ideas have failed, those with a vested interest in a set of failed ideas will never give up their control of the discourse. Instead of the philosophers listed by Barrau, someone like Michel Foucault might be more relevant…

Comments

1. **Kea**
   April 3, 2011

   A courageous, but futile new direction for NEW. It's not like the French only discovered philosophical rationalism yesterday. They have been mocking this kind of Anglo-Saxon idiocy since the revolution, which is probably why they are blind to the good neo-Popperian arguments.

2. **Dr. Philip Carey**
   April 3, 2011

   Peter, your comments regarding the familiarity of American physics professors with modern philosophy are wholly inconsistent with my experiences at several large American research universities. In my current department, for example, lively and sustained conversations about philosophy and physics are a daily treat, both with grad students and with faculty. Indeed, a number of professors in my department have masters in philosophy; one has a doctorate. Please avoid making such generalizations in the future!

3. **The Cosmist**
   April 3, 2011

   Doesn't the "philosophication" of physics represent the decline and fall of the field? Aren't you be back to the days of Zeno and Aristotle contemplating the universe abstractly without contact with reality? And to philosophize a bit myself, will anyone care about the laws of physics in a post-Singularity world of infinite reality simulations and arbitrary physical laws in the Dyson sphere computronium hive-mind of the future? 😉

4. **Peter Woit**
   April 3, 2011

   Philip Carey,

   American academia is a huge and wonderful place, and I don't doubt that somewhere it includes physicists able and willing to knowledgeably discuss David Lewis and

Nelson Goodman. But personally I don't seem to have run across these people. It is true that I spend less time amongst physicists than I used to, and maybe since the advent of the multiverse, such discussions of "possible worlds" now are common in American physics departments, who knows…

5. **Aaron**
April 3, 2011

Really? You've never met them?

http://scienceblogs.com/principles/2007/08/alone_in_the_multiverse_1.php

6. **Peter Woit**
April 3, 2011

Aaron,

OK, I have to admit I was wrong about this. It was kind of a mistaken attempt though to defend the honor of American theoretical physics. I should have known better.

7. **Bee**
April 4, 2011

That's interesting, thanks. I was wondering the other day what's the history of the multiverse-idea? I mean, most modern ideas turn out to have been touched upon much earlier. Is there anything in the books about it?

8. **Aurelien Barrau**
April 4, 2011

Dear Peter,
Thanks for your interest in our book. This is much appreciated.
If I can allow a few comments, I have not said that "string theory is a pseudo-science", nor that it is "metaphysics". What I think is that it is both impossible and uninteresting to try to define the borders of science. Which does not mean that "everything is the same", of course. Clearly, string theory plays with the limits of physics and I indeed find this interesting.
However, on this specific string theory point, I would like to emphasize that one can work on Loop Quantum Cosmology (this is the case for me) but still highly respect string theory and consider this approach as beautiful and meaningful. The World is highly diversified and so are our models !
Best regards,
Aurelien Barrau

9. **peterg**
April 4, 2011

"I think is that there is both impossible and uninteresting to try to define the borders of science."

The first part probably is correct, but I respectfully disagree with the second part. Maybe I'm missing the point, but it is as if you're saying that morality is not grounded in rational thought (I agree) and that therefore rational discussions about morality are uninteresting (wrong, in my view).

A long time ago I collaborated with physics researchers. I lost track of most of them, but I'm certain one of them (a mathematical physicist) has asked "Yes, it's nice, but is it physics?" when he learned about the more esoteric domains of string theory.

Perhaps more scientists should have asked that question, even if there is no straight answer.

10. **Peter Woit**
April 4, 2011

Aurelien,

Thanks for the clarifications. I should have made clear that the use of "pseudo-science" was my own editorializing, and shouldn't have been attributed to you.

The Bogdanov Equation

April 3, 2011
Categories: Book Reviews

Another book that I picked up in Paris is Lubos Motl's L'Équation Bogdanov: Le secret de l'origine de l'Univers?. It's a rather weird document, a mish-mash of defense of the Bogdanovs (partly by comparing their ideas favorably to loop quantum gravity), generalities about cosmology, and promotional material about string theory. Among the odd features of a book entitled "The Bogdanov Equation" is that there is no "Bogdanov Equation" in the book (or anywhere else, as far as I know). In a comment on his blog posting about the book Lubos writes

> If there is an equation written by the twins that can be shown to explain the origin of the Universe, you will read about it in the book. If there is none, you won't find such big statements. But I can't tell you and others the punch line here. Wink

I don't think it's hard to guess which alternative is the right one…

One of the great mysteries of the book is that of its authorship. Supposedly it was written by Lubos in English, then translated into French. I don't doubt that large parts of it were written by him, although in a style somewhat different from his blog, and then passed through the filter of translation. Some parts of it though, especially some of the details of the endless defense of the Bogdanovs I can't believe were written by him. For instance, pages 187-189 are taken up with a translation into French of this internet mailing list posting by "Osher Doctorow Ph.D.", and the author is described as "Professor Osher Doctorow, mathematician at the California State University", which appears to be misinformation of a Bogdanovian rather than Lubosian sort.

Another commenter on the same blog posting by Lubos gives a long and detailed list of dubious things in the book and states that "To make it short, I have the impression that you are not the sole author of the book.", asking him to clarify this issue. The response is

> Sorry but I have neither time, nor desire, not the full rights to answer ten kilobytes of such questions, some of which are well-informed observations but most of which are not.
>
> The book is created not only as a blog but also to satisfy a contract with the publisher. So I was okaying some proposals from the publisher. It is essentially good if you can identify these places.

Theoretical physics in recent years has produced some very odd things, this book is one of the most bizarre.

Comments

1. **Roger**
   April 3, 2011

   It is getting harder to separate the hoaxes from the non-hoaxes. This seems to be a serious post about a hoax book. Or maybe Lubos got tricked by some hoax research. Or maybe Lubos was trying retaliate against a publisher somehow in order to get out of a contractual obligation to deliver a book. Or maybe the Bogdanovs have discovered the secret to the universe.

2. **Mike**
   April 4, 2011

   what about this one?

   http://novel-theservant.blogspot.com

3. **Emanuel Derman**
   April 4, 2011

   One of the things that everyone wants, even in academic life, is to have their cake and eat it. If someone puts his name to something you should give him credit where credit is due and blame correspondingly. It doesn't seem right to get the upside and put the downside on someone else unnamed. (No more bailouts.)

4. **Nono**
   April 4, 2011

   I have not read the entire book, just some excerpts on the internet. Why not tell the truth about it, if everyone already knows it? It's a lot of bullshit.

   But I think you're being unfair to its author. I don't see why there should be any doubt that this book is exactly what its author wanted it to be, and exactly what he is capable of writing, no more and no less.

   The choice of topic, the crappy smokescreens to hide the fact that the book is completely empty and devoid of substance, the inclusion of ancillary material written by third persons (the Bogdanov brothers themselves, of course, who else?) just to fill pages. All that is what the author wanted it to be and nothing else.

   After all, if there was some kind of contractual agreement to be fulfilled, the author could have done a completely different thing. Just write a popularizing introduction to string theory, or to the many different approaches to quantum gravity, or to the history of those subjects. Or whatever that's not just useless rubbish like this book. But, as it happened, Motl chose to write this and nothing else.

5. **Peter Woit**
   April 4, 2011

   In case I wasn't clear enough. Yes, this book is a lot of bullshit. However, it is also an extremely weird example of bullshit…

6. **cormac**
   April 4, 2011

   BS is one thing, plagiarism in order to meet a publishing deadline quite another

7. **Peter Woit**
   April 4, 2011

cormac,

It’s not plagiarism, since “Osher Doctorow, Ph.D.” is properly quoted. However, this seems to me too weird even for Lubos. I don’t have conclusive evidence, but if I had to guess I’d say that parts of the book were written not by him but by the Bogdanovs. That’s not really plagiarism, more like a weird variant of ghost-writing.

8. **Chris Oakley**
April 4, 2011

People wonder about the motivation of the brothers, but I have had them figured them out for some time. Their aims are very similar to yours, Peter – to expose modern “fundamental physics” for the sham it really is. Getting theses they know to be bullshit passed, and, what’s more, published in a respectable journal; getting a former Harvard professor to endorse their “work” in a popular science book. They are lying low at the moment because they want to net a bigger fish – a Weinberg or a Witten, maybe. Once they have done that, or at least established that they cannot, then the public denunciations will begin in earnest.

9. **milkshake**
April 4, 2011

Nothing weird about this partnership. Lubos wants to write pop-sci books that reach wide audience. He would be delighted to be on TV. Re-opening the Bogdanov controversy works well with media. The brothers can help him: he provides them vindication, they introduce him in showbiz. And they flatter his genius. From the starting position that the brothers were unfairly treated by anti-string crusaders, and that they were basically on the right track (even if they were not the brightest contributors to ST – he reserves this place for someone else), he can then plug in any of his own favorite topics. Without Bogdanovs, his tirades would not get the interest of a publisher.

10. **chris**
April 5, 2011

“Osher Doctorow, Ph.D.” – this name alone is about enough to gauge thew IQ of the Bogdanoff brothers.

11. **Bobito**
April 5, 2011

The explanation for this weird book is simple, and gets to the true commitment to science that lies behind it: money.

12. **J.F. Moore**
April 5, 2011

Chris,

If that’s true, they’re playing a very long game with almost no leverage, since they’ve already been ‘exposed’. It seems much more likely that they’re just self-deluded.

Milkshake,

Good analysis, but I’d still call it weird or bizarre, even if there is some underlying reason involved.

13. **FrediFizzx**
April 6, 2011

Osher Doctorow is the handle of a person that posts a regular series of messages on the sci.physics.* UseNet newsgroups that no one really ever responds to. Some of what they post is quite involved and very bizzare sometimes. Do a googlegroup search if interested.

14. **YBM**
April 6, 2011

Did you know that *even* the forewords from Clovis de Matos (which is NOT a physicist as the book pretends) have been deeply rewritten by the Bogdanov’s ?

Précision: Science & Vie et l’équation Bogdanov.

> A moins que le préfacier de l’ouvrage, un dénommé Clovis de Matos, présenté comme ayant soutenu un doctorat sur la théorie des cordes bosoniques et physicien théoricien à l’Agence spatiale européenne (Esa), nous en apprenne davantage. Interrogé lui aussi par nos soins, il a surtout tenu a signaler, manifestement gêné, qu’il n’était pas docteur en physique (il a une maîtrise) et que l’Esa n’avait rien à voir avec sa préface. Quant au ton dithyrambique de son intervention, il serait le fait d’une profonde réécriture de son manuscrit, sans que les épreuves ne lui aient été envoyées pour relecture. Drôle d’équation !

About the book book itself, an interesting article : L’equation Bogdanov: The Salvador Dali school of physics.

Meanwhile, the two fraudsters are now sueing the french magazine Marianne (which published the CoNRS report in last october), asking for not less than… 750 000 € !

15. **Yatima**
April 6, 2011

Tout ça commence à bien faire!

16. **publius**
April 7, 2011

LOL at Chris Oakley theory.

It could be true! I did not realize until now the bogdanoff’s are fighting the good fight too! they are just doing it the french way, maybe subtler but also effective, while Peter is doing it the american-estonian way.

17. **In Hell's Kitchen (NYC)**
April 8, 2011

It’s hard to believe that the B’bros are running a long-winded
Sokal Hoax for the simple fact that EinMotlstein would have seen
through it in a femto-second 🙂

2011 Templeton Prize

April 6, 2011
Categories: Multiverse Mania

This years $1.6 million dollar Templeton Prize has been awarded to astronomer and cosmologist Sir Martin Rees. The Templeton Foundation has traditionally been largely devoted to promoting the intersection of science and religion, so one surprising aspect of this choice is that, while Rees is a very accomplished scientist, he doesn't believe in God (although he likes the music and architecture in churches):

> In fact, Rees has no religious beliefs, but considers himself a product of Christian culture and ethics, explaining, "I grew up in the traditions of the Anglican Church and those are 'the customs of my tribe.' I'm privileged to be embedded in its wonderful aesthetic and musical traditions and I want to do all I can to preserve and strengthen them."

Rees does seem to believe in something that the Templeton people are willing to take as a replacement for belief in God: belief in the Multiverse. He has been one of the leading figures promoting the Multiverse and anthropic explanations, even before the recent string theory landscape pseudo-science made this so popular. For more about his views, see a 2003 interview In the Matrix, which leads off with:

> All these multiverse ideas lead to a remarkable synthesis between cosmology and physics…But they also lead to the extraordinary consequence that we may not be the deepest reality, we may be a simulation. The possibility that we are creations of some supreme, or super-being, blurs the boundary between physics and idealist philosophy, between the natural and the supernatural, and between the relation of mind and multiverse and the possibility that we're in the matrix rather than the physics itself.

Something for future Templeton candidates to keep in mind: no need now to believe in a Christian God, belief in "The Matrix" is good enough.

Comments

1. **CarlH**
   April 6, 2011

   If this world is the ultimate reality, then I am all for finding an alternate universe and getting the heck out of here! Plus I admire Rees' honesty regarding religion. Almost everyone is a product of their culture which includes religion, so why pretend to be a stout atheist when we've all been indoctrinated since birth about the Big Man Upstairs and His Kid who we tortured and killed.

   BTW, go back to taking about physics. Bike riding in NYC isn't why I come here. There's enough of those kinds of blogs anyway. Do you want to hear about what I had for breakfast this morning? Neither do I.

2. **Bobby**
   April 6, 2011

   Peter, why not put a lid on the negativity and simply congratulate Rees on his achievement? Imagine how you'd feel if you just won 1.6 million dollars and some blogger was sniping at you from across the pond. Not cool. If you want to take on the substance of Rees views at an appropriate time and place then go ahead. But what you're doing now is not much different than slagging off a colleague at his funeral.

3. **Peter Woit**
   April 6, 2011

   Bobby,

   If you read the posting, my description of Rees was "a very accomplished scientist". His accomplishments as a scientist deserve recognition, but the Templeton Prize is not a prize given for scientific accomplishment. It's given for "an exceptional contribution to affirming life's spiritual dimension, whether through insight, discovery, or practical works." I do find it remarkable that Templeton is now giving this prize to atheist scientists, as far as I can tell that's the first time they've done this. If they turn it into a prize for purely scientific achievement that will be great. In the past it was a prize for bringing God into science, which personally I think is a bad thing, not worth encouraging. Replacing the God part though with the Matrix isn't an improvement.

   I don't see the analogy to "slagging off a colleague at his funeral". He's not my colleague, and getting $1.6 million dumped on you is not really like a funeral. Amidst all the adulatory praise and all the money he's getting this morning, I don't think a bit of sniping from a blogger across the pond is going to ruin his day.

4. **The Cosmist**
   April 6, 2011

   I don't understand your equating of "the Matrix" with monothestic notions of God — I certainly consider a multiversal Matrix of simulations an improvement over an omnipotent bearded guy in the clouds who takes a personal interest in human beings! And it's just strange enough that might even be true…

5. **DB**
   April 6, 2011

   I think Rees is a particularly good example of what happens when a top-flight physicist goes off the boil and enters his dotage. For reasons not very well understood, it tends to happen rather early in life to theoreticians, usually by the age of 45.

   They lose their hard edge and begin to wallow in intellectual mush. Giving out to them is like criticising granpa for his senior moments. They really can' t help it.

6. **Peter Woit**
   April 6, 2011

   The Cosmist,

   My interest in either the bearded guy or a "multiversal Matrix of simulations" is pretty much nonexistent. I just wish people would keep them out of science I care about, instead of encouraging bringing them into it with large cash prizes.

7. **Allan Rosenberg**

April 6, 2011

CarlH,

I live in New York, too, so I'm always looking for recommendations for good bakeries. BTW, if you ever visit, you have to try the cupcakes from Baked by Melissa, they're awesome. Seriously, these are the best cupcakes you'll ever eat. Keep the NYC blog entries coming, Peter!

8. **Peter Woit**
April 6, 2011

Allan/CarlH,

The postings about bikes and bakeries were a local April 1 phenomenon. No more. At least not in this particular branch of the multiverse…

9. **Roger Aune**
April 7, 2011

Congratulations. Thanks for getting some of our money back! M theory and similar science are certainly welcome compared to the unfunny joke that is religion. The fact that the templeton group is is honoring this is very suspect, though. Enjoy the $ indeed!

10. **Guillaume**
April 7, 2011

I went to a talk Martin Rees gave just a couple of weeks ago in London. The traditional Ouroboros cosmological kind of talk, nothing original but I remember being very annoyed by its distinctive Kantian flavour, which fits nicely within the Templeton framework. I find it sad that Rees does not have enough common sense to reject such an "honour"…

11. **honour**
April 7, 2011

Wait till someone slaps you in the face with a wad of unmarked $100 bills (or sterling) with random serial numbers to the tune of > $1M, and then see what you do!

12. **Bobito**
April 8, 2011

"Peter, why not put a lid on the negativity and simply congratulate Rees on his achievement? Imagine how you'd feel if you just won 1.6 million dollars and some blogger was sniping at you from across the pond. Not cool. If you want to take on the substance of Rees views at an appropriate time and place then go ahead. But what you're doing now is not much different than slagging off a colleague at his funeral."

This is wrong-headed in so many ways.
1. Winning a prize is not an 'achievement'. Perhaps some 'achievement' led to the prize being awarded, but let's not confuse the two.
2. What is 'not cool' about criticizing someone who just won 1.6 millions dollars? What would be 'not cool' would be criticizing some guy publishing ridiculous things that no one paid attention to, ever. It's not at all like 'slagging off a colleague at his funeral' – there's not a good analogy between dying and winning the lottery (which is, to some extent, what winning such a prize is).
3. The misuse of millions of dollars is something that merits criticism. It's reasonable to debate whether this is a misuse, but if it is, it certainly merits criticism, because the misuse of millions of dollars is socially indefensible. The libertarian sort will say – it's their money, they can do what they want with it – but beside being an indefensibly self-centered anti-communitarian point of view – that's just oblivious to the social context of money.

13. **miss use**
April 8, 2011

Will it be a misuse if Rees endows a scholarship(s), for example?

14. **oracle**
April 8, 2011

You Guys here are just unbelievable, really!

While it is obvious that the US Governement ist just giving up on all science like particla physics, cosmology, space research and other more fundamental than applied stuff (Fermilab will close, LISA canceled and surely more such good news will follow soon…) people on this blog are complaining about a non-governemental organization giving some money to a scientist!

There`s no need for being that impatient and trying to accelerate the process of dissipation of science. If all goes well, the US will continue to put all their money into war games pulling it out from science in particular and hopfully Europe and the rest of the world will follow their example as they usually do …

Be assured that You Peter, and all the others here, will soon be put out of the misery to smash everything down that is going on in particle physics and related subjects while pretending to care in the slightest for or being interested in science.

The april 1. change of direction of this blog was indeed a unusually wise and very clear-sighted moment. It seems time has come to look out for other things; soon there will be nothing left for You to fight. So why not write about things like politics, culture, food, cycling, etc …?

Have fun and enjoy youself as long as You can

cheers

15. **Paul**
April 8, 2011

There are some similarities between the status of monks and priests in the past, and the status of professors of fields that use technical jargon today. They have a claim to be purveyors of truth. But what is exactly the extent of this claim?

16. **Peter Woit**
April 8, 2011

oracle,

Please note that (unlike other blogs, e.g. Cosmic Variance and Pharyngula, have you written to them?) I'm not complaining about Templeton giving money to Rees, or him taking it. If I were him, I'd take the money, and it's their money, they can give it to whoever they feel like. I'm just reporting that they did it, and offering some commentary on how their choices seem to be changing (belief in God no longer necessary). We report, you decide… In this new world order where particle theory is more and more going to be funded by private sources, I don't see why I shouldn't point out that one side-effect is that going on about Multiverse pseudo-science is the sort of thing some private funding sources really love.

Sorry, I don't feel any personal responsibility for Nasa's budget problems that led to the cancellation of LISA, or the shutting down of the Tevatron. As you might have noticed from an April 1 posting, I share your opinions about the dreadful current state of the US government and its budgetary priorities. If this blog instead were devoted to how wonderful the multiverse idea is and how well string theory is doing, that would have no effect on those issues.

17. **oracle**
April 9, 2011

Ok, Peter

yes You just reported it, maybe I`ve overdone it a bit yesterday 😉

But I agree with Bobby that it would be god if You could hold yourself back a bit and stay generally more fair than is often the case.

These are really dreadful times for particle physics and all of the other sciences not providing results which can be turned immediately into money or weapons…
In principle we have the knowledge and technology to make progress in our scientific understanding as never before but it is all in vain as it seems now 🙁

It would just be cool to see that You can avoid kicking (almost) dead horses lying on the floor already

On the other side it would be interesting if You could invest more time into understanding the mathematicel topics You announced to not report about some time ago than in "dead-horse-kicking" which is uncool 🙂

best wishes

18. **Tim van Beek**
April 9, 2011

oracle wrote (with a hint of sarcasm):

> …hopfully Europe and the rest of the world will follow their example as they usually do…

There is no indication that European countries will cut the funding of CERN, DESY or other projects. There have been no cuts to funding of universites, no cuts of tenured positions either. Business as usual, for both string theorists and people interested in different approaches. While there are many controversial topics discussed in European politics, cutting funding to science, education, universieties etc. is not one of them.

19. **Giotis**
April 9, 2011

I wrote the following to another blog but I repeat it here because I think it fits perfectly with Rees' quotation in Peter's post too.

I wonder, why whenever the multiverse is discussed people start talking weird all of a sudden mixing various philosophical/metaphysical Mumbo Jumbo? There are so many beautiful physical concepts within the multiverse that someone could discuss; from quantum cosmology, eternal inflation and the dynamics of the landscape to the properties of the underlying fundamental theory. Why not focusing on these concepts for a change? The conditions for the appearance of the multiverse/landscape and its ability to explain measurements in *our* universe are highly non trivial with hard physics at their core.

Of course I understand that Peter choose the above excerpt to discredit the multiverse idea but still Rees should have known better.

PS. I bought Greene's book but I skipped all the "crazy" stuff regarding computer simulations etc. I'm only interested in the physics of the landscape/multiverse which is quite elegant, robust, theoretical motivated, mathematical consistent and with enormous explanatory power. These are enough I think to take the framework seriously. Regarding experimental (even circumstantial) evidence; well you never know where research, technological breakthroughs and the deeper theoretical understanding of these concepts may lead you in 10, 50 or 100 years from now. Even tomorrow some clever guy may come up with an extraordinary proposal that could put some of these ideas to the test.

20. **oracle**
April 10, 2011

Goitis,

would it not be better to constrain the number of solutions of the hard physical core further mathematically and physically to a reasonable value?
To just say now we have a multiverse gives one a good giggle; but to stop at this point and being happy with that looks like some kind of a lazybones-approach 😛

21. **Peter Woit**
April 10, 2011

Sorry Giotis, but you're just putting forward over-the-top hype and wishful thinking. Many people thought the initial 1984 models for string theory unification were elegant, hardly anyone thinks that about the complicated mess you get out of a conjectural anthropic string theory multiverse. "Enormous explanatory power" doesn't really go with "can't predict a single f—ing thing"….

22. **Phil**
April 10, 2011

I think it is a mistake to rely on the Big Bang or anthropic coincidence or any particular theory of physics as "proof" for the existence of God. In the same way, it would be foolish to use the multiverse theory as "proof" against God. While the Big Bang or anthropic coincidence may offer something like a hint about God's existence, the philosophical argument about God's existence is much older than those findings. The proper idea is not that God just started the big bang and moved on to something else. In this sense, perhaps the idea of a "first cause" is misleading. In Christian theology(Catholic theology, anyway), God is the underlying cause of the creation and the

continued existence of all things, regardless of whatever physical theory happens to be true. St. Thomas Aquinas famously argued that the universe would still need an ultimate cause even if it had no temporal beginning. The debate about all this will continue, but it would be more helpful if people on both sides framed it in a better context.
Christian belief is not dependent on the Big Bang.

23. **milkshake**
April 12, 2011

A large cash prize given to famous senior people is probably harmless. I would be concerned about Templeton directly-sponsored research – even modest grants can induce people in fledgling groups to spend their time on explaining how their research connects to mystic-friendly universe…

Suspicious Bump

April 6, 2011
Categories: Experimental HEP News

Last night a new preprint from CDF appeared at the arXiv, discussing a signal observed in their data, at about 3 sigma significance, that could in principle correspond to a new particle not seen or predicted before. This morning's New York Times has an article about this here. The Times does a pretty good job of getting quotes from relevant experts and explaining the situation, which is basically "if it's real that's very exciting, but it probably isn't".

I went to check Tommaso Dorigo's blog only to find that he had a short posting up explaining that a more detailed one was embargoed until the public talk this afternoon at Fermilab (live stream at 1600 CDT here). This seemed rather odd since the Times had clearly been given the story a few days ago, embargoed only until last midnight. He now has a full posting up, and you should go there for a detailed and authoritative look at what this all means (most likely not much, modeling the huge background you have to subtract is hard).

Update: For other blog postings about this well worth reading, try Michael Schmitt, Resonaances, Gordon Watts and Flip Tanedo.

Update: Took about 21 minutes from the time of release of this data to submission of a paper explaining it.

Comments

1. **Tommaso**
   April 6, 2011

   Thanks for the link and the kind words Peter. Indeed, I had promised somebody in CDF that I would not blog on this until it was public.
   What do I know… First of all Viviana Cavaliere's thesis contains all the information of the paper -actually ten times more, and that one is public since a while ago (note that it caused a theoretical paper hypothesizing a Z' signal in CDF to be produced by April 1st).
   Second, the NYT piece. And third, the paper is in the arxiv but the seminar hasn't started yet. Oh well.

   So, this is no higgs. This might be the Z', but it is orders of magnitude more likely a JES problem mixed in with a W+jets background simulation mismodeling…

   Yet it is nice that CDF is not afraid to publish these kinds of signals any more – ten years ago they would rather sit on such a thing and die than publish it.

   Cheers,
   T.

   Cheers,
   T.

2. **Michael**
   April 6, 2011

   Hi,

   I also applaud the CDF Collaboration for bringing this out reasonably quickly. It is not shody work, and so other collaborations (D0, CMS, ATLAS) should be motivated to check for a corresponding signal. Good since, for sure.

   In my opinion, if it is not real, then it is a statistical fluctuation rather than a problem with modeling the background or with the jet energy scale.

   Michael

3. **Kea**
   April 6, 2011

   Well, the 21 minute paper has an abstract quoting the 2010 thesis sigma of 3.2, as opposed to the new paper sigma of 3.3. This suggests that the paper was already prepared, and rushed onto the arxiv to beat competitors.

4. **lcs**
   April 6, 2011

   I would be more convinced if both electron and muon Mjj distributions showed equally convincing bumps. At least in Fig. 7.1 of Cavaliere's thesis this is not the case.

5. **abbyyorker**
   April 7, 2011

   Negative spin would say that it is hype driven by the imminent shutdown. Rather a nasty thing to say, I admit.

6. **AR**
   April 7, 2011

   The paper you cite was not the first paper explaining the data. There were two theory papers even before the CDF paper came out (arXiv:1103.6035 and arXiv:1104.0243). The analysis had already been published in a thesis.

7. **jo**
   April 10, 2011

   Off-topic: there's a pretty pompous-looking string theory conference next may at IHES, invoking "three generations" http://www.ihes.fr/jsp/site/Portal.jsp?page_id=62

   No Witten nor Maldacena though: it would be interesting to know if they can't attend, or don't want to attend…

8. **Peter Woit**
   April 10, 2011

jo,

I don't know about "pompous", lots of those people have done very interesting things, many of them with nothing to do with string theory, and if I wanted an excuse to go back to Paris, there's plenty of talks by that group I'd like to hear.

What is kind of funny is the "three generations" thing. If one wanted to, I suppose you could start with the people who first worked on string theory circa 1970, find their students, their student's student's etc, and have a conference on "six generations of string theory". Pretty impressive for an idea that doesn't work…

9. **Jurgis Rudkus**
   April 10, 2011

   Hey Peter, have you watched the wonderfully combative interview Brian Greene recntly did with Amir Aczel? http://www.booktv.org/Watch/12292/The+Hidden+Reality+Parallel+Universes+and+the+Deep+Laws+of+the+Cosmos.aspx Greene fared quite well, though he had to work for the win. In fact, Greene dispensed with most of the objections you've raised on your blog in a lucid, congenial, and complete fashion. I haven't felt this good about the multi-verse in years. Check it out!

10. **Peter Woit**
    April 10, 2011

    Jurgis,

    No, I hadn't seen that, thanks for pointing it out to anyone who wants to follow it. I have to admit though that I think I'm pretty well conversant with the arguments of Brian and other multiverse proponents (I've read about half a dozen books on the subject now, including his…). So, I'll skip this. If someone who does watch it hears a new argument not gone over endlessly already, let me know…

11. **Shantanu**
    April 11, 2011

    Peter, I thought you said Brian and you agree on most things.
    (looks like multiverse is not one of them)

12. **Peter Woit**
    April 11, 2011

    Shantanu,

    I don't think I disagree with Brian and most string theorists about what the actual state of the subject is, when you get down to precise questions. I've always disagreed with them about the likelihood of the kind of future progress needed to overcome the well-know problems they have. When it comes to the multiverse, this is even more true, and I suspect that actually the majority of string theorists are skeptical that the obvious problems with it can be overcome.

    But, enough about the multiverse here, surely a posting where it is more relevant will come along soon…

13. **kelly**
    April 12, 2011

    Now that the major media has taken this report and run with it (and oh what a run!..) all the good physics blogs have gone silent! Common guys, lets hear some follow up! Don't be afraid, the ball is already rolling, you can blame Tomaso, let's hear some speculation on what confirmation of a new boson or force could mean! And I don't mean technicolor strings!

14. **Peter Woit**
    April 12, 2011

    kelly,

    I'm not sure what follow-up is needed to reporting that there's almost surely nothing here. As a follow-up though, it still looks very likely there's nothing here…

15. **James**
    April 13, 2011

    Peter: I enjoy your blog and I'm reading "N.E.W." now. I wonder, is this article at all relevant to the Fermilab "bump"?

    http://arxiv.org/PS_cache/hep-ph/pdf/0002/0002292v2.pdf

16. **Peter Woit**
    April 13, 2011

    James,

    Glad you enjoy the blog. Looking at that paper, it "predicts" that the mass of such a Z' is above 30 TeV, beyond even the scale accessible at the LHC. So, the Tevatron bump can't be it.

More on Scattering Amplitudes

April 10, 2011
Categories: Uncategorized

Last week was the beginning of a program at the Santa Barbara KITP entitled [The Harmony of Scattering Amplitudes] which will focus on topics including recent advances in computing N=4 super Yang-Mills scattering amplitudes. Talks are available on-line [here]. There's a full schedule of talks in the program and related talks [here]. On Thursday, Nima Arkani-Hamed will give a talk on "The planar integrand of N=4 super-Yang-Mills theory", which some wag has scheduled as lasting from 1:30pm to 5am.

For a survey of some of these recent developments, a correspondent points me to the thesis of Arkani-Hamed's student Jacob Bourjaily, which has just appeared online [here].

Comments

1. **Kea**
   April 10, 2011

   Interesting that the Spradlin talk mentions motives and Hopf algebras in passing, since he has been working with the mathematician Goncharov (et al) to obtain a short expression for the 2 loop 6 pt amplitude in N=4 SYM. There was a bit too much talk about being in a 'cult' though.

2. **Anonymous**
   April 11, 2011

   The linked article says in the first paragraph "With the imminent arrival of the Large Hadron Collider (LHC) at CERN, it is clear that in the coming years scattering amplitudes and cross-sections will take on an even more prominent role in particle physics."

   I have a hard time believing their work on N=4 SYM and N=8 SUGRA would shed much light on QCD processes at LHC. Can someone explain to me to what extend will this be true?

3. **Anonymous**
   April 11, 2011

   Oops, there's no such thing as "imminent arrival". The LHC *has* arrived for some time.

4. **Kea**
   April 12, 2011

   Anon, as explained in the first (technical) talk, the original Parke Taylor MHV (tree) amplitude is just as valid for QCD as for N=4 SYM. For this reason alone N=4 SYM is worth pursuing, and one might also note that SUSY aspects are rather easy to separate out from the other degrees of freedom. That is, there is every reason to believe that real QCD has a similar non local formulation in terms of motivic Galois theory (whatever that is).

5. **¡Viva la twistor revolución!**
   April 12, 2011

   Remember that even the original Parke-Taylor QCD calculation was done by using susy arguments.

6. **Rhys**
   April 12, 2011

   I'm also skeptical about the application of all this stuff to theories with less SUSY. For example, a simple argument shows that (+++…+) and (-++…+) amplitudes vanish to all orders in N=4 SYM, but this is not true for theories with less supersymmetry. I saw Nima give a talk recently, and this was one of many crucial points which seemed to rely heavily on having N=4.

7. **Susy WIMP**
   April 13, 2011

   XENON 100 announces the result!
   http://arxiv.org/abs/1104.2549

8. **Proudmemberofthecult**
   April 14, 2011

   @anonymous #1,

   There are several ways in which information from amplitudes in supersymmetric theories can be used in practical QCD calculations up to one-loop. Let me mention three

   – At tree level susy and non-susy theories are not that different: fermion lines do not show up in the diagrams of purely bosonic amplitudes. There is a story for what happens to tree amplitudes with quarks (bottomline: you can extract the QCD quark amplitudes from the N=4 ones).

   – Tree amplitudes appear in unitarity cuts of one loop amplitudes, also in QCD. There are known ways to use this information to calculate the full loop amplitudes. This includes the amplitudes with helicity +++…+ configuration for instance.

   – last but not least: people write actual numerical code for calculating QCD amplitudes using methods inspired by all the N=4 type stuff (BLACKHAT, ROCKET, etc, etc).

Short Items

April 12, 2011
Categories: Uncategorized

- For a while now the situation with this year's budget for science in the US has been very unclear, with threats being made of huge cuts to be instituted in the middle of the fiscal year, requiring shutdowns of labs, etc. The recently negotiated budget agreement turns out to involve only relatively small cuts for both the NSF and the DOE Office of Science, more here and here. Details still have to be determined and the legislation has to be passed, but it looks like most physics and math research will escape any serious immediate cuts. The new fiscal year starts October 1, and fighting over that budget has not even begun. From the hearings already held, it looks like math and physics research has bipartisan support, but in the new environment of significant budget-cutting, focused on discretionary non-military spending, I'd guess that budget levels for the next few years will be flat at best.
- There's an interesting interview with Dennis Overbye of the New York Times here. He's noticed a problem with string theory:

> One pet peeve is press releases about papers that show that string theory is about to be experimentally tested. When you read the fine print that's never true. There was a press release that the large hadron collider was going to test string theory. It was kind of embarassing for them.
>
> Scientists and science journalists just take these shortcuts And I think they become enshrined as truth in the public mind.

He also has some comments about blogs:

> Science journalism is in a very interesting, very turbulent state I think. We still have newspapers. Some newspapers still have science reporters, like the Times. I feel like the blogs have risen up to become huge force in the coverage of science. I think the readership now is very fragmented. I think a lot of people get their information from blogs, where people can be more casual or more arcane if they want to be. I think even at my newspaper there's a difference between people who read the science times and the font page. There are a lot of these different layers of coverage going on.

- In the category of "string theory about to be experimentally tested" nonsense that Overbye refers to, no press releases this week, but we do have a special section of Science News with an array of over-hyped stories about Cosmic Questions, with the one on string theory assuring us that:

> Even then, the LHC will be far from powerful enough to re-create the single, unified force that physicists believe existed for a fraction of a second after the Big Bang — you'd need a collider as big as the universe itself for that. But the LHC might be able to test some of the predictions made by the leading theory that joins gravity and the other forces.

- In the category of something I just put on my list to try and find time to listen to, there's a Science Friday program featuring a discussion about Science and Art between Cormac McCarthy, Werner Herzog and Lawrence Krauss.
- In the category of talks I'd like to hear but can't, Graeme Segal will be giving the Felix Klein lectures in Bonn next month, on the topic of Three Roles of Quantum Field Theory.

Update: Two more

- According to a new preprint, CDF's observed suspicious bump that made the New York Times "is a generic feature of low mass string theory". No word yet on whether there's going to be a press release. I guess this also means that if D0 doesn't see the bump, that pretty much rules out low mass string theory since its generic feature is not observed, right?
- Langlands has written a very interesting review for Mathematical Reviews of Ngo's paper proving the fundamental lemma.

Comments

1. **Bee**
   April 13, 2011

   "you'd need a collider as big as the universe itself for that"
   Last time I read that I believe the collider was as big as the Milky way. Just out of curiosity, does anybody know of an estimate for that?

2. **chris**
   April 13, 2011

   i remember a seminar on planck scale colliders where the conclusion was that they can't be built. the thing that came closest was the Unruh collider – it consisted of two orbiting black holes. so i guess it is a bit up to your fantasy and to what you call a collider, but i doubt any serious size estimate can be given at all.

3. **petergreat**
   April 13, 2011

   When will D0 have enough data to confirm or disprove the CDF finding?

4. **Paolo Valtancoli**
   April 13, 2011

   I think that CDF bump reveals that physicists are only a bunch of fools.

5. **Peter Woit**
   April 13, 2011

   Bee,

   Assuming similar magnets, collider energy scales linearly with size. So, very roughly saying the Tevatron is 1km in radius and gives collision energies of 1TeV=10^3 GeV, you need a factor of 10^16 to get to the Planck Scale. If I believe Wikipedia the Milky Way is 10^17km or so in radius, so a Planck scale collider would fit nicely.

   One other problem is luminosity, since interesting cross-sections fall off quickly with energy. It might not be possible to get sufficient luminosity to produce a useful number of events. Then there's the minor problem that string theory doesn't actually predict anything about what will happen if you built such a machine…

6. **neo**
   April 13, 2011

One thing that gets ignored when people talk about milky way sized (or larger!) colliders is that it would take at least 300K years for a beam to circulate, so in our reference frame the data would be very slow coming in even if we were somehow to have one.

7. **neo**
   April 13, 2011

   Scratch that–I guess you could have a very large number of detectors.

8. **Yatima**
   April 13, 2011

   Well, I wonder what the business end of the collider (the thingamabob that corresponds to the Schroedinger Cat) would have to look like for Planck-Scale events to be detectable. Calorimeters made of thin slices of neutronium?

9. **A.J.**
   April 13, 2011

   Calorimeters made of thin slices of neutronium?

   You're also going to have to work pretty hard to shield the detector from background effects, like the occasional gamma ray burst. You think the TGV caused calibration problems…

10. **Yatima**
    April 14, 2011

    Under "believe it or not", Michio Kaku is being interviewed about the striken Fukushima 1 plant and manages to be even more catastrophist as the Japanese governement. It's very bizarre and unfortunately rather unhinged:

    http://www.alternet.org/story/150599/fukushima_reactors_are_a_%22ticking_time_bomb%2C%22_japanese_govt_in_denial

11. **Peter Woit**
    April 14, 2011

    Yatima,

    A quick read of what Kaku has to say doesn't seem to me that unreasonable. Accurate information about what is going on at that reactor seems to be hard to come by, and the Japanese government has every reason to minimize the dangers to stop panic. Kaku quite possibly is maximizing the dangers, for his own reasons. I wish it was clear that he had gone way beyond the realm of plausibility. If someone knows an authoritative source that shows this convincingly, that would be reassuring.

No WIMPs

April 13, 2011
Categories: Experimental HEP News

A commenter points to the long-awaited release of a preprint from the XENON100 experiment giving results from a 100-day run last year. This is the most sensitive dark matter experiment that has released data. The result: with an expected background of 1.8 +/- .6 events, they see 3 events (i.e. about what you'd expect if there's nothing there). For a WIMP mass of 50 GeV, this allows them to exclude certain WIMP cross-sections at the level of 7.0 x 10^{-45}cm^2. This pretty conclusively kills off some other claims by dark matter experiments to have seen something, especially the CDMS result from late 2009 (see here).

One motivation for supersymmetry has always been that it can provide a WIMP with the right properties to explain astrophysical dark matter observations. This new data rules out some (if you use the SUSY expectations plotted in the new paper), or most (if you use the expectations plotted in the CDMS paper, see here and here) of the possible parameter space where such a particle is expected, providing yet another nail in the SUSY coffin.

Update: More details available at Resonaances and Tommaso Dorigo's blog.

Update: For a detailed analysis of the implications of the XENON100 result for supersymmetry models, see here.

Comments

1. **Kea**
   April 13, 2011

   There may be many nails in Susy's coffin, but Susy is still running around, terrorising people as a zombie.

2. **Susy WIMP**
   April 14, 2011

   The New York Times reports the finding, with some collaborators posing for a photo:
   Particle Hunt Nets Almost Nothing; the Hunters Are Almost Thrilled

3. **Georges**
   April 14, 2011

   This is excellent news for all those theories that predict the absence of supersymmetry. That is only a tiny fraction of all candidate theories, but it is an important step that tells us where to continue searching. What a great day.

   And compliments for being faster than Resonaances on this news item!

   Peter, is there an overview of candidate theories that do not contain supersymmetry somewhere?

4. **Holycow**
   April 14, 2011

   Your take on this is really a disgrace. The result is not bad for SUSY and the most important point is that no WIMP of any kind is there (yet), SUSY or non-SUSY. To intrepret this just as "another nail in the SUSY coffin" is only another instance of your legendary theoretical shortsightedness. Paraphrasing the classics, "the dead that you kill, are in excellent health".

5. **Peter Woit**
   April 14, 2011

   Holycow,

   What's a disgrace is the refusal of SUSY proponents to admit that there is such a thing as experimental evidence against their pet idea. Imagine that XENON100 had seen a signal. You can be sure that SUSY advocates would be loudly proclaiming that this was exactly what they expected, that evidence for SUSY had been found.

   More specifically, the John Conway and Tommaso Dorigo 2008 postings that I linked to argued that the data then was already starting to cut deeply into the region favored by SUSY. Now that the limits have been moved down more than an order of magnitude, wiping out the "favored region" on the plot they show, why is this not evidence against SUSY?

6. **DB**
   April 14, 2011

   Peter,

   Susy is like Count Dracula. It will take more than a few nails in the coffin to put paid to him. He already has a few exit routes mapped out. But he is definitely on the run.

   I expect Tommaso Dorigo's credit rating to be upgraded by Moody's any day now.

7. **Georges**
   April 14, 2011

   Holycow,

   you come close to say that nothing can be bad for SUSY, which would mean that SUSY is not science. But I am sure you do not want to say that.

   So please tell us what else would be bad for SUSY – we are all listening.

8. **Rien**
   April 14, 2011

Actually, CDMS did not claim to have seen anything. They had two candidate events, but this was not significant.

9. **Peter Woit**
   April 14, 2011

   Rien,

   Many people did at the time interpret the CDMS two events as "seeing something", although not conclusively. One example from the many news stories at the time:

   From http://news.sciencemag.org/sciencenow/2009/12/17-01.html

   "Even so, Joseph Lykken, a theorist at Fermilab, says he's relieved that CDMS has finally seen something. WIMPs are predicted to exist by theories involving a principle called supersymmetry, which posits a heavy partner for every particle currently known. Had CDMS continued to see nothing, the results would have undermined those theories. So seeing something is better than seeing nothing, Lykken says."

10. **Holycow**
    April 14, 2011

    Clarification. What I find annoying in this post is that Peter seems to be so happy about the loss of another chunk of SUSY parameter space that forgets being worried by the absence of a positiva signal, of any kind (Or do you believe that WIMPs and SUSY are equivalent?). And that's the most important fact. You should get your priorities right.

    Then, SUSY can be disproved by LHC with more luminosity
    (certainly not with 35/pb).

11. **Rien**
    April 14, 2011

    Peter: yes, this is true, but it was over-eager theorists, not the experiment themselves that thought they were real. They even went back to check those two candidates again, IIRC, and concluded they were background.

12. **Paul Wells**
    April 14, 2011

    Dear Holycow,

    What Luninosity at what energy would be sufficient to disprove SUSY ? 1 fb at 7 Tev ?

    Thanks
    Paul

13. **Peter Woit**
    April 14, 2011

    Holycow,

    I'm happy that the XENON100 experiment is a success, accomplishing what it is supposed to do. That they get a null result tells us something new and non-trivial, which is great. A positive result would have been Nobel prize-worthy and revolutionized the field, but it's hard to call a null result very disappointing, since it's the one that seemed far and away the most likely.

    That the null result makes one of the main arguments for SUSY less compelling is just a fact that I'm reporting.

14. **Holycow**
    April 14, 2011

    Peter, can you also report on how Xenon results affect the very possible and relevant SUSY idea of gravitinos as DM? Or explain to other readers how it is enough to exclude some chunck of parameter space to be ready to give up on the best theoretical idea we've had in the last 30 years?

    Paul, say 30/fb at 7TeV should be quite good. Of course for you it would be much simpler: as soon as SUSY particles start to pop up you can join the celebration (and no problem with that).

15. **chris**
    April 14, 2011

    "What I find annoying in this post is that Peter seems to be so happy about the loss of another chunk of SUSY parameter space"

    i for once am very happy. but that might be due to the fact that i am in the business of cutting parameter spaces for SM extensions myself.

    the best thing you can get is evidence for new physics. the second best thing is a good weeding of obsolete theories.

16. **Eric**
    April 14, 2011

    I think what needs to be pointed out here is that SUSY can still be observed at low energies without the dark matter actually being a SUSY WIMP. First, the LSP is only stable if R-parity is conserved, which doesn't necessarily have to be so. Also, even if R-parity is conserved, the resulting relic density can be vanishingly small, or the LSP could be a gravitino. So, these direct detect experiments only constrain the hypothesis that dark matter is a SUSY WIMP. Superpartners could still be observed at the LHC without also needing to explain the dark matter.

17. **Peter Woit**
    April 14, 2011

    Holycow,

    OK, here's a report on what XENON100 has to say about the idea that, since the neutralino idea didn't work, let's save SUSY by postulating the gravitino as DM:

Nada, Zip, Nothing. This is an idea you can't test with this kind of experiment.

As for your description of supersymmetric extensions of the SM as "the best theoretical idea we've had in the last 30 years", I'm afraid I think it's more accurate to describe it as "a not very good idea that has done huge damage to the field for 30 years by dominating the attention of theorists, for no good reason".

18. **Holycow**
April 14, 2011

Dear Peter,

if you have proof that "the neutralino idea doesn't work" you better publish it as many people would be interested in knowing. If the lack of a signal in some region of parameter space is enough for you to give up on some idea, then this just goes to show you are a bad scientist (bad theorist and would also be a bad experimentalist) and that you let your prejudices rule over logic.

You write a blog that journalists read and you should be more careful with the way you project your prejudices (and personal frustrations) on your reports.

"Since the neutralino idea didn't work, let's save SUSY by postulating the gravitino as DM". Your naive simplistic point of view is wrong again. This idea has been circulating for years and has its own appeal. The point you fail to appreciate is that it is damn difficult to guess what the theory of nature is without experimental guidance. By your comments you also fail in discriminating the sensible theoretical ideas.

"Nada, Zip, Nothing. This is an idea you can't test with this kind of experiment." Correct, so your claim about coffins and nails is pure hot air.

"a not very good idea that has done huge damage to the field for 30 years by dominating the attention of theorists, for no good reason". Which as Lubos would say just goes to show your limitations as a theoretical physicist.

19. **Peter Woit**
April 14, 2011

Holycow,

I happen to think that the scientific accuracy of what I write here is quite a bit higher that that of what well-known theorists promoting SUSY/extra dimensions/string theory have to say to the press. To the extent journalists do pay any attention to this blog, that's one reason.

If a low opinion of SUSY extensions of the SM makes one a bad theoretical physicist, yes, I'm a bad theoretical physicist.

20. **Georges**
April 14, 2011

Holycow,

SUSY is by far not "best theoretical idea we've had in the last 30 years". I do not like string theory too much, but it is clear that strings themselves are a much better idea than supersymmetry. Strings were much more successful than supersymmetry by any measure you might want to use. And strings are not the best idea either, as Peter consistently showed.

Fact is that supersymmetry is wrong. Go back to the drawing board. And better be quick – better ideas, which agree with data, are already on the market.

21. **Anon**
April 14, 2011

Wow.

I highly advise anyone who is actually trying to learn about physics by reading this blog, to look briefly into the differences between gauge mediated and gravity mediated supersymmetry breaking. That will teach you a lot more about what susy does or does not predict, than reading oversimplified statements here will.

These distinctions became clear in the early 80s and are not a response to any recent experiment, as a brief perusal of the many thousands of papers will teach you.

22. **Holycow**
April 14, 2011

Georges, agreed, string theory is even better. I was just thinking of the TeV scale and the LHC.

"Fact is that supersymmetry is wrong."
Fact probably has a different meaning for you than for the rest of scientists.
Fact is, it's pretty hard work to come up with good theoretical ideas and even harder to find new stuff experimentally. You should show a bit more respect for this enterprise and present a more serious perspective of what this is all about. It's OK if you have your personal preferences for whatever you expect to show up at the LHC but don't misrepresent the meaning of experimental results for the theories you dislike. Be honest. I think that's a minimum to ask.

"better ideas, which agree with data, are already on the market." Really? Like what?

23. **Mike**
April 14, 2011

Anon,

As a laymen, I have some questions. Could you please say (simply please) what SUSY does predict? Then, could you please explain what experimental results would falsify such predictions? Finally, could you please state whether, in your opinion, current experimental set-ups, if any, are capable of falsifying such predictions? Thanks.

24. **Peter Woit**
April 14, 2011

To be clear, I don't agree with Georges that good ideas about how to get beyond the SM that agree with data exist.

Mike asks an excellent question, and I'm curious to hear what SUSY advocates have to say about this now, as well as to look at the record and see what they were saying earlier. So far the reaction I've seen to null results from the LHC and dark matter experiments has been "that's no problem at all for SUSY, although if it had come out the other way, it would have been strong evidence for SUSY", which strikes me as less than honest. Or, if SUSY is going to be pursued scientifically by the new standards developed to deal with the failure of string theory (experiment can never provide evidence against the idea, only evidence for it), that should be made explicit.

25. **Holycow**
April 14, 2011

So, even Peter should be able to understand this allegory: you go pick up your friends who went for a long walk in the forest and in your way to meet them you lose your car keys. You discover this when you find them, explain the situation and then you start your search. As time passes and no keys are found, your friend Peter starts ranting that there were no keys at all and that the null result of your search just proves it. Well, you know that this simply means you haven't covered the whole path. But then somebody comes along with a brand new metal detector and you continue your search much faster and happier. You cover 1/4 of the whole path you walked in no time and here comes Peter repeating again and again there are no keys because the whole idea is preposterous and there are no such things as keys in the world. The best you could do is to ignore him and patiently continue your search, till you find the keys, or some gold coin or nothing, because somebody took them and you'll be faced with a different problem to solve. As you see the analogy is not perfect, but really captures well the role of the chorus of naysayers in this business: Just a bloody pain in the ass.

26. **Mike**
April 14, 2011

I was asking a serious question, and perhaps my lack of grounding in the subject is why I don't follow your "allegory."

So, you lost your keys, you know already that you lost your keys –they're just misplaced.

However, unless I'm missing something, even those who think SUSY exists (I'm agnostic on this — I simply don't know), don't know for sure that it exists — they haven't "lost" it anywhere. What's the connection?

Perhaps you could help me understand. What SUSY does predict? What experimental results would falsify such predictions? In your opinion, are current experimental set-ups, if any, capable of falsifying such predictions? If not, what more is needed, other than time to keep searching? Thanks.

27. **Georges**
April 14, 2011

Peter, Holycow,

there are several ideas on the market that state that there is simply *nothing* beyond the standard model plus Higgs. Such ideas agree with all data. These ideas then have to explain the shortcomings of the standard model in a way or another; but when they do, they have fewer assumptions than all those ideas that use supersymmetry or higher dimensions.

28. **Math Student**
April 14, 2011

Mike,

A better analogy could be something like SETI. Everybody would agree that extraterrestrial intelligent life (or SUSY) exists with very little positive data but falsyfying it could require close to an infinite amount of data, and is thus impossible.

29. **Geometrick**
April 14, 2011

As a math PhD student myself, the previous commenter is way off the mark. The analogy they used is actually a horrendous analogy. It needs to be pointed out.

30. **Holycow**
April 14, 2011

Sorry, my analogy was devised to explain the chorus, not the search.

31. **Holycow**
April 14, 2011

Mike: one basic prediction of SUSY is that for every type of particle we know there is another with the same quantum numbers but different statistics (boson fermion) and mass. So LHC should find strongly interacting partners of the known quarks and gluons, among other things.

SUSY theories do more than this and help solve some of the problems of the Standard Model. For such solutions to be natural the mass scale of these new particles cannot be much higher than the TeV scale, which is the range that will get covered by LHC. If the limits on such masses are pushed well in the few TeV
regime then one should give up on this idea. But talking about the demise of SUSY now, is plain stupid.

32. **Mike**
April 14, 2011

Math Student,

Thanks, that does seem like a more apt analogy.

I'm agnostic on that question as well — but I admit to a bias that extraterrestrial life should exist, all based on my limited understanding of the nature of the physics and the scope of space and time.

One possible difference I see is that the answer to the ET question is either yes or no — in the sense that there is no other possibility. And, although the answer to the SUSY question is also yes or no –there may well be other competing possibilities (at least in theory) for answering the questions at hand regarding what, if anything, extends beyond the SM, and any such theory has some realistic hope of being falsified in some reasonable period of time.

On the other hand, perhaps that's a distinction without a difference — and I'm just moving the goal post in the middle of the game.

If so, rather than drop what I see to be an important aspect in the progress of science and the growth of knowledge — at least some hope of falsifiability — I suppose I would in the end have to say that SETI is a great and useful "hobby" for humans to undertake, but perhaps not "science" as such.

Thanks again.

33. **Mike**
April 14, 2011

Holycow,

Thanks for your comment as well.

I take it then, within current experimental design limits, if the LHC fails to observe strongly interacting partners of the known quarks and gluons, among other things, and absent any other new discoveries that would put it back in play, at least in your mind SUSY would be effectively falsified. Is that correct?

Thanks again for your comment. I appreciate people trying to deal seriously with my necessarily naive questions.

34. **Peter Woit**
April 14, 2011

Holycow,

As others pointed out, your extended analogy was rather bizarre. You have never seen the keys in question.

I take your answer to Mike to be that you'll give up on SUSY once limits on superpartners are "well in the few TeV" regime. Hopefully the ultimate reach of the LHC will be sufficient to satisfy you. Personally I don't see why you have to go all the way up into the few TeV regime to falsify the idea that supersymmetry explains the stabilization of the weak scale at around 100 GeV.

I don't expect SUSY proponents to be conclusively giving up now, but they might want to admit that these experimental results are not encouraging. For a long time, SUSY phenomenologists have been playing the game of producing elaborate plots showing what are supposedly the most likely ranges of SUSY parameters. As experiments start to rule out large sections of these ranges, the reaction seems to be to not admit that one's predictions are starting to fail, but to produce new predictions.

35. **Tommaso**
April 14, 2011

Hi all,

indeed, in 2008 the searches were over one order of magnitude less sensitive to the presence of SUSY wimps. Still, they were already clipping out regions of phase space for SUSY theories. What was true back then, is even more so now. A neutralino may be there -and I would be quite happy if we found it at the LHC, or even if one of these sit-and-wait experiments found it- but we are Bayesians by nature, at least somewhere close to our amygdala. And if the prior belief in SUSY was what it was four years ago, we have to acknowledge that it has shrunk quite sizably now.

As for my bet, those 1000$ will be invested in a party in a suitable place. The losers of the bet (Watts and Distler) will of course be invited. Reservations are open, RSVP.

Cheers,
T.

36. **The Cosmist**
April 14, 2011

If you don't mind another layman's (meta-)question, mine is this: how would you characterize the state of theoretical physics today? From the outside it sounds like there is huge disagreement and confusion about basic questions, as if the whole field is in disarray like never before. It's strange to hear physicists arguing about experimental results and interpreting them so differently. WTF is going on with physics these days?

37. **Mike**
April 14, 2011

The Cosmist,

"WTF is going on with physics these days?"

A layman's answer to a layman's question. Being an optimist and assuming, as I do, that new fundamental scientific explanations are almost always surprising in many ways, I would judge the current disorder as a good sign — for surely it is now just a matter of time (short I would hope) until a better fundamental explanation emerges.

Of course, this will only give rise to new and better questions, but that's the name of the game.

38. **Giotis**
April 14, 2011

There is a clear distinction between SUSY as a fundamental property of nature and its breaking scale. Low energy SUSY which LHC tries to find is just a convenient estimation of this breaking scale engineered to help people solve some SM problems; nothing more. It may well be that these problems may not be solved by Low SUSY after all but that doesn't mean SUSY doesn't exist as a fundamental property of nature. There are models where SUSY's breaking scale reaches the string scale. So SUSY cannot be falsified by LHC or any similar experiments.

39. **Mike**
April 14, 2011

Giotis,

Question: Do the models where SUSY's breaking scale reaches the string scale encompass and purport to solve the SM problems?

40. **Giotis**
April 14, 2011

Yes in some cases but by other means. These models are constructed by String theorists not particle/field theorists and String theorists have other alternatives (not just Low SUSY) to solve these problems. LoW SUSY is the most obvious way but not the only one.

41. **Mike**
April 14, 2011

Giotis,

Thanks. I assume that experiments at the string scale are far beyond our current and reasonably foreseeable capability. If that's the case, maybe Math Student's analogy to SETI is more apt than I imagined.

42. **Peter Woit**
April 14, 2011

The Cosmist,

The fundamental situation in particle physics these days is that we have an extremely successful theory, the SM, with only one sector of the theory untested. This is the Higgs sector, and it's the source of a lot of the less satisfying aspects of the theory. The thing to be excited about is that later this year we should start getting experimental data relevant to this, seeing a Higgs if it is there. This could get very interesting, less so if the Higgs exists and behaves as predicted by the SM.

Things like SUSY/strings/black holes/extra dimensions are a sideshow. These have never given any convincing explanation of anything about the SM or any serious reason to believe they exist. All that's going to happen on that front is that over the next few years there will be a lot of null results coming from the LHC and other experiments. Sooner or later some of the partisans of these ideas will give up on them, other ones will keep the faith, but no one else will take them seriously. The collapse of some speculative research programs that have been around for decades may be of sociological interest to watch, but there's not much of scientific interest there.

Mike: if the SUSY breaking scale is much higher than the 100 GeV electroweak scale, SUSY does nothing at all to solve problems of the SM, all it does is introduce new ones.

43. **Paul Wells**
April 14, 2011

Peter,

As you know there aren't that many valid ways to build a point particle Quantum Field Theory- and supersymmetry is one of them. It may or may not exist but isn't a search justified from a pure phenomenology point of view? I am thinking of the situation with Dark Energy a few years ago….

Paul Wells

44. **Peter Woit**
April 14, 2011

Paul,

I've nothing against people investigating supersymmetric QFTs. Actually, there is some fascinating mathematics behind the general idea of supersymmetry, and I wouldn't be surprised if it somehow turns out to be of fundamental importance.

The problem is that the supersymmetric extensions of the Standard Model that are known, once one includes supersymmetry breaking to agree with experiment, become quite complicated and don't actually explain anything that the SM doesn't. People like to go on about how SUSY relates fermions and bosons, but it doesn't relate any known fermion to any known boson. It relates fermions to unknown bosons and bosons to unknown fermions. This isn't convincing at all. But, sure, the LHC should look for it anyway, who knows what will be found?

45. **GradStudent**
April 14, 2011

The Cosmist,
"From the outside it sounds like there is huge disagreement and confusion about basic questions, as if the whole field is in disarray like never before."

Yes.

46. **imho**
April 14, 2011

chuckle… me thinks HolyCow is a first year grad student 🙂

These results and discussions are all signs of a healthy field, and as a cond mat guy I think this is all great. My question is where are the discussions about technicolor and its incipient resurgence???

47. **No Black Holes?**
April 14, 2011

Peter,

"Things like … black holes … These have never …. any serious reason to believe they exist."

Am I understanding correctly that you're saying there's no serious reason to believe that black holes exist?

48. **milkshake**
April 14, 2011

Another layman question: How likely is that LHC will miss new physics? My understanding is that LHC detectors have to perform extensive data filtering, which in turn depends on how the triggers are set. So, my question is, would they be able to notice unexplained collisions against the high background, especially if signs of new physics were subtle ? Thanks

49. **SpearMarktheSecond**
April 14, 2011

Wimps don't need SUSY, that dark matter has a weak interaction is sufficient. The existing ratio of dm to luminous matter is consistent with a weak interaction and thermal equilibrium in the big bang, but there are other solutions too. In any case mundane issues like hadronic uncertainties in the wimp-nucleon cross section are about an order of magnitude.

50. **Shantanu**
April 14, 2011

Peter, I know several people (including my former colleagues) who still
believe proton decay exists and continue to do thesis projects and searches for it, despite 30 years of null results. So don't expect people to stop believing in supersymmetry for weak-scale interaction strength based dark matter even if LHC or NLC finds nothing.

51. **Peter Woit**
April 14, 2011

NBH,

This is about the LHC. Sure, there's a black hole in the center of the galaxy, but none are going to be made in Geneva.

milkshake,

It's possible to come up with scenarios where there would be new physics missed by the LHC because of the detector triggers. I have no idea how to assign a probability to this. If the experimentalists are doing their job well, it should be low.

Shantanu,

I think people should keep searching, both for WIMPs and for proton decay, as long as they are able to keep increasing the sensitivity of these experiments. Doesn't really matter if the main motivation is misguided, you still should look where you've never looked before to see if something unexpected turns up.

By the way, I haven't followed that story recently, but last I heard, proton decay experiments were getting to the point where they too could start putting nails in the SUSY coffin…

52. **D R Lunsford**
April 14, 2011

Hey, UCLA is being positive

http://newsroom.ucla.edu/portal/ucla/dark-matter-search-one-step-closer-200546.aspx

"Now that we know there are no unicorns in Chicago, we are that much closer to finding them."

-drl

53. **Susy WIMP**
April 14, 2011

I've heard an interesting take on this. Since we are looking for dark matter interacting via Higgs exchange, could it be that the result is evidence against a single scaler Higgs, rather than against supersymmetry?
Does anyone know the status of Xenon-1t, XMass etc.? Are we expecting another order of magnitude increase in sensitivity within the next one or two years?

54. **M. Wang**
April 15, 2011

Tommaso,

"we are Bayesians by nature, at least somewhere close to our amygdala. And if the prior belief in SUSY was what it was four years ago, we have to acknowledge that it has shrunk quite sizably now."
This is the best comment from a physicist that I have read in years, and how relevant it is here! I wish more scientists could have the broad knowledge and common sense you demonstrated in one sentence. It is sad to see so many confusing narrow technical expertise with true understanding of a subject.

Mike,

"I would judge the current disorder as a good sign — for surely it is now just a matter of time (short I would hope) until a better fundamental explanation emerges."

I cannot share your sanguinity. In 30 years of wild goose chase, the majority of the institutions studying theoretical particle physics have been staffed mostly with worshipers of the false faith. Here I don't mean false theories but an entire system of anti-scientific attitude. Witness the total inability for someone like Holycow to appreciate the relevance of the failure of their previous predictions. People don't accept failure easily, particularly not when they are in their 40's or 50's and had their entire careers built on not just the failed ideas but also the false methodology. On top of this, they are many. Tommaso may be able to point out more specific research regarding how crowd psychology induces greater variations from rational behaviors.

Take the investment banking industry for example. For years, they spouted all kinds of theories that purported to show their great contributions to the efficient markets and therefore to the economy as the justification for looting the nation's wealth. The great recession proved them false, but have they stopped doing what they were doing? Once the regulators chose rescue over closure, their jobs were secure and they promptly went back to the old business of ripping off the nation. When the senate questioned Goldman Sachs regarding its behavior, the CEO simply lied and got away with it, even though now some senators publicly acknowledge that they have been lied to. The moral is, if you don't clean out the scums, don't expect them to behave differently if given a second chance just because they had created a big mess the first time around. Last time I check, most of the physics institutions still follow the tenure system…

55. **Thomas Larsson**
April 15, 2011

So crackpots don't care about the experimental verdict. What else is new?

56. **chris**
April 15, 2011

Mike,

if no Higgs is found below say 150GeV SUSY will start drifting into the fringe science direction and slowly die off similar to -say -the steady state model in the '70s and '80s.

57. **Holycow**
April 15, 2011

"if no Higgs is found below say 150GeV SUSY will start drifting into the fringe science direction and slowly die off similar to -say -the steady state model in the '70s and '80s." Wrong. A light Higgs is a prediction of the minimal versions of SUSY.

58. **Holycow**
April 15, 2011

M. Wang

"This is the best comment from a physicist that I have read in years". You have to read more.

"Witness the total inability for someone like Holycow to appreciate the relevance of the failure of their previous predictions." What is astonishing is the inability of some to understand the very basic fact that in the road to discovery your limits will be pushed up and chunks of parameter space will be discarded. How on earth can it be otherwise? I have no problem at all in giving up on a theory if it doesn't have anything to do with nature. (By the way, I'm not like Georgi who once wrote a paper titled "Why I would be sad if a Higgs boson is found".)
But I will not give up until it has been put in a hole by experiment, which is not the case for SUSY. (Not even close). If you think otherwise, that's your problem, but respect the work of others who still have to collect much more luminosity searching for this jewel.

You would have given up in the search for the top when the lower limit was at 90 GeV. (Don't reply saying this case it's not the same, because we knew it had to be there. I know. What I critize is the attitude of people putting down others worthier than them simply because they fail to understand the issues involved or because of their personal prejudices).

59. **ShrugOffNaysayers**
April 15, 2011

It seems that everyone here would get very happy if nothing is found. That's the difference to actual scientists.

60. **Rhys**
April 15, 2011

The WIMP miracle is a remarkable theoretical fact which now looks less and less likely to be the way nature actually works. This is completely logically independent of supersymmetry, but some SUSY models naturally provide stable WIMPs. So this is really only evidence against certain realisations of supersymmetry; there is plenty of scope for gravitino or axion dark matter, for example. (I have personally written exactly one paper about low-scale SUSY, and our model doesn't have stable WIMPs).

I agree with chris that the absence of a light Higgs would be much stronger evidence against SUSY. Of course, by the time a light Higgs can be ruled out, we should have found some superpartners anyway! If we haven't, I will probably already have given up on weak-scale SUSY.

Susy WIMP:
Somebody may correct me, but I don't believe the direct detection experiments have yet begun probing Higgs-exchange cross-sections.

imho:
In my opinion, 'realistic' technicolour models are a lot uglier than low-scale SUSY models (which are themselves rather messy).

61. **chris**
April 15, 2011

holycow asys:

"if no Higgs is found below say 150GeV SUSY will start drifting into the fringe science direction and slowly die off similar to -say -the steady state model in the '70s and '80s." Wrong. A light Higgs is a prediction of the minimal versions of SUSY.

that was exactly my point ^_^

62. **Holycow**
April 15, 2011

Chris, it's important to give the right message. If you say SUSY *predicts* a Higgs below 150 GeV you are misleading people. That prediction is not universal and can be violated. SUSY does predict superpartners and that's the key search, not the Higgs.

Rhys, thanks for your post. It's good to read reasonable people from time to time.

63. **Thomas Larsson**
April 15, 2011

Speaking about nails, how are the searches for permanent electric dipole moment going? In 2005, Chad Orzel wrote: "Experiments currently in production should lower that limit by another 2-4 orders of magnitude, which would either find a non-zero EDM, or rule out pretty much every theory now on the books. I'm told that the theorists who deal with this stuff are starting to sweat a little… "

But I guess the silence speaks for itself…

64. **chris**
April 15, 2011

"If you say SUSY *predicts* a Higgs below 150 GeV you are misleading people."

sure you would, i fully agree with you. it would be equally misleading as e.g. saying that SUSY is a fact of nature.

65. **Holycow**
April 15, 2011

"It would be equally misleading as e.g. saying that SUSY is a fact of nature." Not really. This might be true (we don't know yet). We know already that SUSY does not predict a Higgs below 150 GeV.

66. **Georges**
April 15, 2011

Supersymmetry is a fact of nature. At lest according to Warren Siegel, who writes since many years: “Thus, particle theory predicts supersymmetry.” (See his page http://insti.physics.sunysb.edu/~siegel/vs.html)

Siegel obviously has a special view of nature, one that many others do not share. But if you want to make a career in particle physics, you better follow Siegel and Motl instead of those many others who prefer fact to fiction.

67. **Holycow**
April 15, 2011

Time will tell what’s fiction and what’s fact. And the future will be of the daring. Zwicky’s name will be remembered and those who opposed his ideas are already forgotten.

68. **joke**
April 15, 2011

Ha ha,

is Siegel’ s whole homepage not just a joke-page?
He does not mean any stuff he writes there serious or does he?!

The joke-papers on it are really funny and give one a good laugh 🙂

69. **edg**
April 15, 2011

Maybe the dark halo is very different to the standard one.
See http://arxiv.org/abs/1103.6091: a WIMP with mass in the TeV range and a rotating dark disk could be a viable solution for DAMA and the recoils measured by the other experiments.

70. **Geometrick**
April 15, 2011

Supersymmetry is not a fact until we discover those predicted particles. That is the objective truth.

71. **M. Wang**
April 15, 2011

Holycow,

It is difficult to have a logical discussion with you because you consistently employ counterproductive debate tactics, but I will give it one last attempt.

Claiming the others are trying to stop research into SUSY is a plain straw man attack, because I have not seen anyone here making that argument. What everyone here is saying is, instead, that previous SUSY predictions have been largely ruled out, and therefore Bayesian logic dictates that SUSY should be treated as a far-out idea unlikely to have relevance to this universe. Research into such ideas is still part of science, but they should certainly not considered mainstream.

“You have to read more.””Your take on this is really a disgrace.””So, even Peter should be able to understand this allegory” These are all ad hominem attacks that serve only to degenerate the discussion into name calling.

And finally, when you are not using these attack tactics, I find your logic impossible to discern. For example, “‘It would be equally misleading as e.g. saying that SUSY is a fact of nature.’ Not really. This might be true (we don’t know yet).” How does “might-be-true” leads to “not really” on a statement like “misleading…saying that SUSY is a fact of nature”, regardless of its true merits? If it is not misleading saying that SUSY is a fact of nature, then there is no might, could, would or any uncertainty.

72. **Eric**
April 15, 2011

M. Wang,

The statement that SUSY has been mostly ruled out (even in the Bayesian sense) is completely false. I believe that what irritates Holy Cow is that yours and Peter’s statements are highly misleading in this regards. Obviously, this is related to your own biases and opinions, but I’m sure also it is related to fundamental ignorance about how science works.

73. **Peter Orland**
April 15, 2011

“Obviously, this is related to your own biases and opinions, but I’m sure also it is related to fundamental ignorance about how science works”.

No, such statements are what betray fundamental ignorance. Remarks like this remind of what is said of professional tennis players. They have to be smart enough to play tennis, but just dumb enough to think it really matters.

74. **Shantanu**
April 16, 2011

Peter, If you have a multi-purpose experiment with different physics goals (such a Super-K), then sure if you have some people working on proton decay
it is fine. But beyond a point, I am not sure its worth investing in building dedicated proton decay experiments, esp. if most well motivated theories continue to be ruled out
in the 80’s building a dedicated magnetic monopole search experiment would make sense, but not now.

75. **KD**
April 16, 2011

Just to state the obvious. SUSY is a general idea with many specific incarnations. Those specific incarnations (and those only) can be falsified experimentally.

Once a stage is reached where most popular incarnations are falsified one may start to doubt the general idea.

KD

76. **felix**
April 16, 2011

Among all the envisioned possibilities at LHC, SUSY and large extra dimensions are the few outcomes that will bring a paradigm shift to physics. Most other speculations are merely mundane QFT theories built upon the old paradigm of gauge theory and spontaneous symmetry breaking. They are nothing but simple generalizations within the good old SM framework. I dare say that if the new physics that's discovered is among this boring category, then the theorists behind it wouldn't even get a Nobel prize.

77. **Bobito**
April 16, 2011

Too many take too seriously the idea that what is science is what can be experimentally falsified.

What is science is what can be justified by experiments. This involves falsification, but there is more to it.

The ether was a good idea that did not correpsond to reality.

Right now there is not much (if any) experimental evidence that suggests that SUSY, in any form, is true (in the platonic sense). It's simply a bad scientific attitude to say that what has not been ruled out is still credible. What confirmed general relativity in the minds of professionals wasn't that it couldn't be falsified (that's not even clear yet). Rather it was that the predictions it made related to Mercury's orbit coincided with observation (not even yet experiment – that came later). The acceptance of the theory was based on positive confirmation – and it didn't come after years of null results.

The success of such mathematical theories as general relativity or the standard model has ruined a lot of physicists educated in recent decades. Those working in the 50's and 60's mostly had some direct experience in engineering type activities in a war, and were able to distinguish between theory for its own sake and theory that grapples with real physical questions. That's been lost to some extent. The success in mathematics of some theorists, e.g. Witten, has further confused the scene. However, as much as the geometrical-topological theory of higher dimensional manifolds is beautiful and deep, it has yet to find any (!) really deep application in genuine physics. Better to levitate a frog.

78. **Mitchell Porter**
April 16, 2011

felix says: "a paradigm shift to physics"

You can see a paradigm shift in theory happening at the KITP program discussed in a previous post:

http://www.math.columbia.edu/~woit/wordpress/?p=3611

There is an extraordinary convergence among gauge theory, string theory, and twistor theory taking place. It's as if they are all different ways of expressing the same thing. It's being understood first for highly supersymmetric field theories, but there's every reason to think that these new perspectives will extend all the way to the standard model, which is after all a gauge theory.

Even without the LHC, then, big things would be happening, but all this is happening at the same time that we're going to get the facts on electroweak symmetry breaking. But the point I would emphasize is that, even if neither Higgs nor supersymmetry is revealed at the LHC, even if every single beyond-standard-model phenomenological theory ever proposed is wrong, all the years of theory since mid-1970s have not been wasted, because they were indispensible in bringing us to this new convergence of mathematical perspectives, in which theories known for decades are proving to contain all these hidden properties.

AdS/CFT and the twistor string were the breakthrough ideas, and as Arkani-Hamed mentions in his talk, now connections are being made to a great body of mathematics which had not previously played a part in physics, such as the theory of motives. With respect to string theory, what may be happening is that strings and branes are showing up as universal structures in quantum field theory, with compact extra dimensions corresponding to the moduli space of degenerate ground states. (That's how it works in AdS/CFT.) It may turn out, not that string theory picks out a unique field theory as the theory of everything, nor that string theory has nothing to do with reality, but that string theory is one of several equivalent pictures available for a very large class of field theories, one of which describes our world.

79. **Peter Woit**
April 16, 2011

KD,

The problem I'm trying to point out is that what's a "popular incarnation" of SUSY keeps changing. For instance, it used to be that for a choice of SUSY parameters to be popular, it had to have a viable WIMP candidate to explain dark matter. Since a couple days ago, that may no longer be true. I'm sure that SUSY phenomenologists are hard at work on new plots of "favored" regions of parameter space, different than the old ones, since the old ones have been at least partly ruled out by XENON100.

80. **Peter Woit**
April 16, 2011

Mitchell Porter,

That's just nearly pure unadulterated hype. String theory has a long history of taking a very complex theoretical situation and claiming much more for it than there's any evidence for. It seems that nothing has been learned from the results of that.

81. **Benito Callas**
April 16, 2011

> SUSY and large extra dimensions are the few outcomes that will bring a paradigm shift to physics.

Well, no. There could be experimental results that do not fit into any known theoretical framework that force a paradigm shift. In that case the Nobel you mention would go not to theorists but to experimentalists.

On the other hand, I don't see how SUSY could possibly be considered a paradigm shift, since it has been the de-facto paradigm for the last three decades.

82. **Holycow**
April 16, 2011

M. Wang,

I don't even know what "counterproductive debate tactics" means. But I sure agree with you that it is very difficult to have a logical discussion with some of the people here.

I don't think I claimed there are people trying to stop research on SUSY. I'm not paranoid. I'm just sick of bloggers saying SUSY is dead, or nearly so, when they should know better, especially thinking of what journalist can make of it. Why I feel strongly about this?: first because I happen to think SUSY is the best BSM idea in the market. Second, because I hate inconsistent crappy logic. I said it before and I'll repeat it: experimental searches are bloody difficult, even when you know that the particle should be there. Remember the top. When the lower limit on its mass kept increasing and increasing who thought the top was a far out idea? There was a range for it to be found, we knew it should be below 200 GeV, and till the limit reached that point the idea was pretty much alive. So, Bayesian logic, my ass.

The same holds for the statuts of SUSY searches (only much more difficult this time). Till the limits reach into the few TeV masses the search is on, the idea is alive and it's in fact the most relevant goal for LHC besides the Higgs. If you don't like it, too bad, but don't discharge your frustration on the logic of scientific research.

"These are all ad hominem attacks that serve only to degenerate the discussion into name calling." Agreed.

"""It would be equally misleading as e.g. saying that SUSY is a fact of nature.' Not really. This might be true (we don't know yet)." How does "might-be-true" leads to "not really" on a statement like "misleading…saying that SUSY is a fact of nature", regardless of its true merits? If it is not misleading saying that SUSY is a fact of nature, then there is no might, could, would or any uncertainty."

OK. I didn't say I support the claim that SUSY is a fact of nature. What I wanted to say is that "SUSY predicts a Higgs below 150 GeV" is a false statement, while we don't know yet if "SUSY is a fact of nature" is true or false. Is that clear enough?

83. **Holycow**
April 16, 2011

Peter, you spend a lot of time fighting "hype" but then you fall into the hype-logic yourself. If some people wanted to promote their SUSY models or preferred regions of parameter space this shouldn't fool you into thinking they are right. You do well in fighting such hype, but fight the hyppers, not the theory.

I don't care what the "popular incarnation" of SUSY is.
Do you think somebody really thinks the CMSSM is the true BSM theory? It just serves a useful purpose to tune the experimental searches and the needed triggers, but SUSY is not over if some particular incarnations of it are ruled out. They have to be ruled out before we get to the real model.

But in your mind, SUSY doesn't exist, and I have the feeling that no amount of arguing will ever convince you that the discovery of SUSY is a logical possibility not ruled out at all.

84. **Peter Woit**
April 16, 2011

Holycow,

To make clear what I think (and continually specifying that here SUSY=any of the known SUSY extensions of the SM, not something more general).

1. The discovery of SUSY is a logical possibility not ruled out at all. But the world is full of implausible ideas that are logical possibilities, not ruled out.

2. From the beginning SUSY was not a very compelling idea, since it doesn't really explain anything about the SM.

3. The continuing lack of any evidence for SUSY at LEP, in precision electroweak results, proton decay searches, dark matter searches makes the idea quite implausible.

4. The last hope for SUSY is the LHC, which has already ruled out a lot of possible parameter space. There was some hope that dark matter searches would find a SUSY WIMP, but that had started to become unlikely, became even less so last week.

5. Over the next few years better dark matter experiments will put even tighter constraints on SUSY WIMPS, and the LHC will rule out more parameter space. Each time these new results come out, SUSY advocates will produce new plots showing that it's likely to be just around the corner, just a bit past the latest limits. I and other bloggers will accurately describe the situation as more and more nails firmly closing the coffin on SUSY. One notable exception to this will be Lubos.

6. Anonymous commenters here will attribute skepticism about SUSY to ignorance. A large part of the particle theory establishment will go to its grave still convinced that SUSY is a wonderful, beautiful idea, with experimental confirmation just around the corner, and will promote this idea to anyone who will listen. As years go by, fewer and fewer will listen.

85. **Holycow**
April 16, 2011

Good enough. I'll keep a copy of this for future reference. 🙂

One simple reply, as I'm tired:

#1 For you, everything is equally likely? Cultivate your taste.

2. The existence of a third generation doesn't solve any problem. Or does it? SUSY has the potential of solving several problems and explaining puzzles of nature. Go read some textbook on the subject.

Do you know of any other BSM scenario at the TeV scale for which the lack of experimental evidence is less strong than for SUSY?
#3. down

#4. SUSY WIMPs are still a healthy possibility. Just in case, study gravitinos as DM candidates, if you wantyour claims to be as strong as you say. And, sorry, experiments exclude things before discovery. It would be nice if you just turned the experiment up and lots of new exciting stuff would pop up. Maybe in a different universe. If you don't like this one, go somewhere else!

Do you have a sorcerer's ball? #5 irrelevant.

#6 what's the point?

86. **Peter Woit**

April 16, 2011

Holycow,

Where did I say all things are equally likely? I definitely have my tastes, although you don't like them. SUSY is more likely than some, less likely than others. On an absolute scale, I happen to think it's pretty unlikely. Maybe more likely than black hole production at the LHC…

I'm very well aware what the arguments are for SUSY, about which problems and puzzles of nature it is supposed to solve. I just happen to think those arguments are weak. If you can't find those arguments here, a large part of a chapter of the book I wrote deals with the issue. The "you should read a basic textbook on the subject" argument is just juvenile, I've noticed it's a favorite of those who don't have a real scientific argument of their own.

Sure, the best argument for SUSY is the lack of other good ideas. Doesn't mean it's a good one though.

#6 The point is that I'm showing off my predictive powers, successfully it seems…

87. **Georges**
April 17, 2011

Peter, here your efforts ("there is no susy!") are compared to those of El Baradei ("there are no weapons of mass destruction!") :
http://physicswithoutideology.blogspot.com/2011/04/woit-and-elbaradei-weapons-and-standard.html

All the best to you!

88. **Holycow**
April 17, 2011

Peter,

So, what do you think LHC will find besides the Higgs?

"If you can't find those arguments here, a large part of a chapter of the book I wrote deals with the issue."
Thanks, maybe I can find your book in some library. I'm not buying it 🙂

"The "you should read a basic textbook on the subject" argument is just juvenile, I've noticed it's a favorite of those who don't have a real scientific argument of their own." It's rather being tired of repeating the same well known arguments.

"Sure, the best argument for SUSY is the lack of other good ideas. Doesn't mean it's a good one though."

That's a matter of taste and in the end nature will tell. Coming back to Georgi's boutade ("Why I would be sad if a Higgs boson is found"), I'll be happy with whatever LHC brings up. I'm more interested in finding out how nature works than proving my option is realized in nature. Can you say the same? Would you be happy if SUSY turns out to be the way nature is?

89. **Yatima**
April 17, 2011

"So, what do you think LHC will find besides the Higgs?"

How about

"So, what do you think LHC will not find besides not the Higgs?"

Seriously, this is getting tiresome. I though popularity contests where for Italian Saturday Night TV.

90. **Peter Woit**
April 17, 2011

Holycow,

My general point of view is that the SM is a fantastically beautiful and compelling structure, with the most problematic part of it the electroweak symmetry breaking mechanism. So, if I get to choose, I'd like the LHC to find not a scalar Higgs with the expected properties, but some unexpected evidence that leads to finding a better electroweak symmetry breaking mechanism. Of course I don't get to choose, and will be very interested in whatever the LHC does find, even if it is SUSY. Actually I suppose I'd rather have some form of SUSY than the all too possible result that may come out of the LHC, a Higgs exactly as predicted and nothing else. Now, that's going to be depressing…

91. **Holycow**
April 17, 2011

Cheer up! It's gonna be SUSY all over the place! 🙂

92. **Georges**
April 18, 2011

Susy all over the place?

– Monopoles?
– Proton decay?
– Large electric dipole moments?
– Sparticles?
– Many Higgses?

In fact, even the wikipedia entry on supersymmetry does not list any experimental prediction. One gets the impression that the supersymmetry crowd is going the way of the string crowd, with the statement:

"We do not make predictions, but we know that we are right."

93. **Holycow**
April 18, 2011

Well, if the wikipedia doesn't list any prediction I should consider seriously giving up on SUSY. 🙂

94. **Younghun Park**
April 18, 2011

The gravitational wave is not found, but, no one gives up the general relativity. When will it be found? We don't know.
The gerneral relativity is perfect? No!

We can't give up SUSY and String theory even if that is not found in experiment. Some concepts in SUSY and String theory are useful, very useful to make the better physics.

We must not give up the all of SUSY and string theory.
We must use the some usefulness in SUSY and string theory
for the next advanced physics.

What is useful in SUSY and String theroy?
It' up to you!

95. **Peter Woit**
April 18, 2011

Yonghun Park,

One difference is that there actually is quite a lot of evidence for GR, while there is zero evidence for SUSY/string theory. Even on gravitational waves, the GR predictions have been indirectly tested (pulsars).

Theories that never pass experimental tests or predict nothing should be abandoned, ones that pass experimental tests are the ones you know are worth trying to build upon.

96. **Bernhard**
April 18, 2011

Peter,

I think one should not give the impression SUSY and Strings are the same thing. Of course you know that, but when you say "zero evidence for SUSY/string", someone could get the wrong idea.

Now I think the worst thing about SUSY is something I already discussed in this blog is that since LEP and with the LHC going to new parameters space, one of the most advertised things about SUSY, the "solution" to the hierarchy problem is looking less and less a "solution". But of course there is still some room for it. So I agree with those who say we should wait.

After 2012, as far as I know the situation should change. I am involved in an LHC experiment searching for SUSY or not SUSY. If SUSY is not there I want to be one of the people to rule it out.

In any case the absence of a WIMP is bad, but for people working with R-parity breaking SUSY perhaps not a big surprise.

In any case, SUSY is not dead at all, just looking less likely. The difference between the top quark history is that the heavier the sparticles the worse is the solution to the main motivation for SUSY exiting in the first place. The top quark could not have been too heavy but being light or heavy did not affect the motivation for its existence.

97. **on the way out**
April 18, 2011

SUSY will be abandoned only when something else comes along that does a better job of explaining things (and making successful falsifiable predictions). Just as the ether was abandoned only when relativity came along and did a better job of explaining things and making successful testable predictions. But for SUSY to be abandoned, the accelerators must find *something* BSM (suspicious bump? it will need more than one bump), and (i) SUSY must fail to explain it satisfactorily, and (ii) a "new theory" must do a better job of explaining things, and (iii) don't forget the testable falsifiable predictions, the new theory will win only when it passes this step. But SUSY per se will never be abandoned, it will get displaced. So it was with the ether.

Consider also the J/psi and other peaks (psi'). The quark model could explain all the peaks in a natural way (and predict the chi states and D mesons their properties ~ decays, selection rules, etc). Other models (S-matrix) could not do such a good job. One needs new bumps.

98. **Bobito**
April 19, 2011

Mitchell Porter:

It's very difficult to take seriously as physics any discussion that involves motives. This is taking something from the deepest reaches of number theory and algebraic geometry and imposing it on the physical world. It's the same error that macroeconomists made when they took dynamical models from physics and tried to use them to interpret economic markets.

Knowing too much can be a terrible impediment to understanding.

99. **Peter Woit**
April 19, 2011

Bobito and Mitchell Porter,

When Arkani-Hamed spoke at Columbia, he mentioned the connection to motives, referring to discussions with Goncharov. He also stated quite clearly that he had no idea what a motive is.

100. **physicsrob**
April 21, 2011

Regarding SUSY parameter priors, there are generally two categories:

1. Priors based off of theoretical or philosophical motivation

2. Priors chosen by/for experimentalists so that they have sensitivity

Okay, so #2 is not a prior, but it could be easily misinterpreted as such. Experimentalists choose to study theories that their experiment might have sensitivity to (not theories that they consider to be likely). If one were to misinterpret their choice of parameters as some sort of bayesian preference one would come to the misguided conclusion that the preferred versions of SUSY have been excluded. But that's nonsense.

If, however, the motivation for SUSY requires a certain choice of parameters and those parameters have been excluded that would be a strong case to abandon it at as plausible theory. But is that really the case? (I'm not a theorist so I don't have much to say about this)

Personally I have a completely flat prior for SUSY parameters (or its existence in the first place), so the whole argument that SUSY should be considered a fringe theory simply because a small set of parameter space has been excluded seems a bit ludicrous to me.

101. **Mark**
April 24, 2011

Check out the title of this post-Xenon100 talk by Dan Hooper: http://www.phys.psu.edu/seminars/index.html?event_id=2232;event_type_ids=0;span=
That said, I won't be terribly surprised if someone like Weiner, Hooper or Zurek will put out another paper in a few weeks, where they tweak a few parameters in one of their pet models and produce another "consistent" scenario 🙂

102. **Shantanu**
April 25, 2011

Peter, what do you think of this paper by 'Hooft?
http://arxiv.org/pdf/1104.4543v1

103. **felix**
April 26, 2011

My bold prediction: even if LHC finds new physics that is something other than susy, the new physics will cause new naturalness problems, which will be curable by susy. At that point, susy will regain momentum.

104. **Seb**
April 26, 2011

@Shantanu

I'm also curious about this paper by Hooft. Peter, will you write something?

105. **Peter Woit**
April 26, 2011

Well, first I have to find time to read and understand it. Too busy with other things right now, we'll see. From a very quick glance all I can tell is it's quite far from anything I've ever though much about.

This Week's Hype

April 18, 2011
Categories: This Week's Hype

This week's string theory hype is brought to you by a press release headlined Dark Matter and String Theory? from the Institut Laue-Langevin in Grenoble, and another one from the Vienna University of Technology. These have led to a BBC News report which is getting wide distribution, claiming that Neutrons could test Newton's gravity and string theory. According to the BBC, this is going to allow a search for:

> supersymmetric particles, part of some formulations of string theory that suggest that many extra dimensions exist over tiny length scales, which would require the precision that is only now possible with the team's approach.

The actual physics here is described in this paper. You'll need to find an atomic physicist to explain exactly what this is about, but the claim is that the author's new techniques in resonance spectroscopy can potentially be applied to measuring the gravitational potential at micrometer distance scales. This hasn't actually been done yet. As for what string theory predicts about how the gravitational potential will deviate from the Newtonian value at these distances, the story is the usual: no predictions at all one way or another. Such violations would be very interesting, but say nothing one way or another about string theory.

Update: The folks at Slashdot have started to get a clue, stripping the nonsense about string theory from this story before posting about it here.

Comments

1. **ucn**
   April 18, 2011

   Experiments with UCN (ultracold neutrons) have already reached the point where the change in gravitational potential energy of the neutrons makes a detectable change to the de Broglie wavelength of the neutrons. Yes, the neutrons are going so slow that they really do fall down measurably as they traverse a detector! I believe some Berry's phase experiments have been done but I am not sure of that. So now they can probe gravity down to micrometer length scales? Well done, I say. If the motivation to do this is to test dark matter and string theory, well that's too bad, but it is good physics in its own right.

2. **Peter Woit**
   April 18, 2011

   Thanks ucn,

   I am curious if anyone expert in this area can tell whether this really is likely to lead to a method for measuring the gravitational potential at short distance scales, and if so, what the limits on this technique would ultimately be. I don't see anything in the paper addressing this.

3. **Giotis**
   April 18, 2011

   When they say:

   "part of some formulations of string theory that suggest that many extra dimensions exist over tiny length scales…"

   they mean most probably the ADD paradigm of large extra dimensions which is not considered part of String theory per se.

   The title is misleading of course but such titles are common practice for the vast majority of journalists; so no surprise there.

4. **Peter Woit**
   April 18, 2011

   Giotis,

   As I keep pointing out in these cases, the misleading nonsense is not something the journalists came up with. It's in the press releases given to them, and in the actual Nature paper:

   "The experiments are linked to current ideas in string theories…"

   This can't be blamed on inept journalists, it's physicists who are putting this out since they think it will draw more attention to their research.

5. **Giotis**
   April 18, 2011

   Even so Peter don't you think there is a big difference between the above excerpt and BBC's title?

6. **Peter Woit**
   April 18, 2011

   Giotis,

   If you're a physicist who decides to participate in issuing a press release with a title making a misleading claim about the relation of your research to string theory, I don't see how you can complain when a journalist writes a story based on the press release and uses a misleading title involving string theory.

7. **Pawl**
   April 18, 2011

   I wonder if the physicists approved the title of the press release. (I know journalists don't generally get to choose their stories' titles.)

   It might be good for scientists, university and institute press/publicity offices, and media covering science stories to adopt some standards in these matters.

8. **tittle**

April 18, 2011

Curious isn't it, that everyone can only comment on the title, and nobody can find anything to say about the physics? Has anyone attempted to contact the authors, and/or friends at ILL (say), to ask about the science as opposed to the title?

9. **Bernhard**
April 19, 2011

Anything to do with experiments cannot, by definition, have anything to do with string theory.

10. **Bee**
April 19, 2011

I presently don't have access to Nature, so have only read the abstract. It sounds related to the experiments with bouncing neutrons in the gravitational field that we discussed on our blog some years back. Yes, one can get down to distances of some micrometers, yet I'm not sure how sensitive one is at this distance to Newton's law. For that I suppose one would have to actually read the paper. Needless to say, the connection to string theory is vague. Also, if one wanted to test extra-dimensional scenarios with that, those with a lowered Planck scale around a TeV, one would only test d=2 or maybe 3, and these are strongly disfavored by astrophysical measurements already. Details are in the Particle Data Book.

11. **Proudmemberofthecult**
April 19, 2011

One can discuss the phrasing of the press release but it should be obvious that any measurement of a deviation of Newton's law is indicative of radically new physics beyond what is presently known. If you were writing the press release though you would also put in something about what this would mean. The layman does not know ADD, but does know the word string theory, so this is what they write.

Big deal.

Although logically disconnected, any measured deviation *would* strongly favour string theory types of scenario. Even this blog's owner might agree with that.

Of course I do not believe they will find anything, but it's an interesting proposal for which they should get credit.

12. **Bernhard**
April 19, 2011

"Although logically disconnected, any measured deviation *would* strongly favour string theory types of scenario. Even this blog's owner might agree with that. "

This is no way true. Really far from it. It would indicate new physics. There is no way on this Earth this would favour a string scenario. Only if you accept Seiberg's claim that whatever new thing that appears you call it string theory. This is by the way likely to happen also with whatever new thing we find at the LHC. Somehow it will support string theory, but this is completely dishonest.

13. **Proudmemberofthecult**
April 19, 2011

Dear Bernhard, which other consistent physical theories do you know which support large extra dimensions? That is, theories which do not have a string theory (-inspired) description? Hence the 'favour'.

Note it's the same kind of 'favour' which fancies susy solutions to the hierarchy problem. Or inflation as a solution to cosmology problems. For both of these the 'generic' mechanism works although figuring out the exact one is beyond current understanding (how many xxxxxx-inflation type models exist, where the x's are some word?).

I tried to be careful to say that measuring deviations does not *prove* string theory in any sense, shape or form. Not with the current state of it. It would erect a huge sign in the sky pointing at string theory saying "look here".

14. **Peter Woit**
April 19, 2011

No, the blog owner doesn't believe that an observation of a violation of Newton's law would favor "string theory types of scenario" or "string theory (-inspired) descriptions". Just because string theory has turned out to be an empty idea you can use to get anything doesn't mean it's the thing to turn to if you observe something unexpected.

I'm somewhat amused by the way string theory partisans have adopted various favored weasel-words. A translation table to keep in mind would be:

"string theory description": almost anything
"string theory-inspired description": everything else

15. **Proudmemberofthecult**
April 19, 2011

If deviations of Newton's laws are observed at these scales and no explanation can be found within the known laws of physics, what would *your* explanation be?

Please note I am being careful with my words because I wanted to avoid being too 'partisan'… sorry to note I failed.

– string theory description: arises naturally as a well-defined low energy limit of a string theory setup (modulo all the usual things we do not understand about string theory (time-dependence, non-perturbative behavior, vacuum selection)).

– string theory-inspired description: incorporates some elements natural in string theory (extra dimensions, supersymmetry, grand unification, etc, etc.), but not all. Usually some form of effective action where the string theory inspiration bit provides some constraint to limit parameter space (usually also rather arbitrarily).

16. **Peter Woit**
April 19, 2011

If evidence is found for new physics at a distance scale of 10^-6 meters, I don't have any good ideas about what that physics might be. However, in terms of places to start looking, a theory based on new physics appearing at 10^{-35} meters wouldn't be an obvious place to begin.

17. **Eric**
April 19, 2011

Peter,

You've missed the entire point regarding large extra dimensions in string theory. If the extra dimensions are large, this lowers the string scale so that the new physics appears at larger distance scales than 10^{-35} meters. This is exactly why black holes could be produced at the LHC in models with large extra dimensions.

18. **Zathras**
April 19, 2011

Proudmemberofthecult,

Care to explain the logical leap between "radically new physics beyond what is presently known" and "which support large extra dimensions?" There's been a lot of radically new physics done in the past without inventing extra dimensions out of thin air.

19. **Peter Woit**
April 19, 2011

Eric,

I realize you can make "string theory-inspired" models (such as ADD) with observable effects an any distance scale you want. This doesn't seem to me a point in their favor. I'm just pointing out that the conventional thing to do when faced with new phenomena at a certain distance scale is start by looking for models where that specific distance scale appears naturally for some reason.

20. **Proudmemberofthecult**
April 19, 2011

Zathras,

If any deviations from Newton's law are measured this would be unexpected to say the least. You are right I should have stressed more I'm presuming these measurements would conform to the large extra dimensions scenario. As this is the only known class of physical theories accommodating such deviations in a natural way as far as I know (correct me if I'm wrong!), this is not such a big assumption.

Hence IF some non-trivial effect is measured it's a fair bet it will support large extra dimensions. This in turn would favour (not prove) a very specific type of background in string theory.

Let me stress again that I don't believe in this scenario, basically for the same reason as Peter W. mentioned. For me though it's not `natural' in string theory :). Not 'natural' does not mean impossible though: improving bounds by orders of magnitude as is proposed in the press release is a good thing.

21. **Higgs?**
April 21, 2011

Internal Note
Report number ATL-COM-PHYS-2011-415
Title Observation of a γγ resonance at a mass in the vicinity of 115 GeV/c2 at ATLAS and its Higgs interpretation
Author(s) Fang, Y (-) ; Flores Castillo, L R (-) ; Wang, H (-) ; Wu, S L (University of Wisconsin-Madison)
Imprint 21 Apr 2011. – mult. p.
Subject category Detectors and Experimental Techniques
Accelerator/Facility, Experiment CERN LHC ; ATLAS
Free keywords Diphoton ; Resonance ; EWEAK ; HIGGS ; SUSY ; EXOTICS ; EGAMMA
Abstract Motivated by the result of the Higgs boson candidates at LEP with a mass of about 115~GeV/c2, the observation given in ATLAS note ATL-COM-PHYS-2010-935 (November 18, 2010) and the publication "Production of isolated Higgs particle at the Large Hadron Collider Physics" (Letters B 683 2010 354-357), we studied the γγ invariant mass distribution over the range of 80 to 150 GeV/c2. With 37.5~pb–1 data from 2010 and 26.0~pb–1 from 2011, we observe a γγ resonance around 115~GeV/c2 with a significance of 4σ. The event rate for this resonance is about thirty times larger than the expectation from Higgs to γγ in the standard model. This channel H→γγ is of great importance because the presence of new heavy particles can enhance strongly both the Higgs production cross section and the decay branching ratio. This large enhancement over the standard model rate implies that the present result is the first definitive observation of physics beyond the standard model. Exciting new physics, including new particles, may be expected to be found in the very near future.

See: http://cdsweb.cern.ch/record/1346326?

22. **Brathmore**
April 21, 2011

Peter,

Quick questions from someone who is not a physicist but enjoys reading this blog: What is the state of the experimental evidence saying that gravity is quantized? Is the BBC correct when it says that in 2002, "gravity's quantum nature was proven"? How strong is this evidence? (I had thought– perhaps erroneously–that the quest to find a quantum theory of gravity was motivated largely by theoretical concerns).

Thanks

23. **Peter Woit**
April 21, 2011

Higgs?

That looks very, very interesting! It looks like one can't yet get a copy of the paper without a CERN login. If anyone can provide this or more information, that would be great.

24. **McNicholl**
April 21, 2011

After a quick read of the article: a sensitive resonance measurement of the energy levels of slow neutrons due to the gravitation potential from the earth (among other boundary interactions). The results agree at the current level of precision with a classical treatment of the gravitational potential. The authors propose to improve precision with a technique analogous to a well known resonant technique (separated oscillatory fields). Now I am not a theoretician so up to you guys but, for what it is worth: (1) this is not a high precision measurement of graviation per se–the precision is required due to the small mass of the neutrons; (2) much of the misleading hype (not sure how much due to the paper itself) comes from the misleading juxtaposition of the words “quantum states” and gravitation; (3) though the vertical (transverse) dimensional scale of the experiment is ~ 20 microns, one is not measuring graviational effects between two objects of this scale (a la Cavendish balls)–one of the objects is indeed the earth.

25. **Peter Woit**
April 21, 2011

Brathmore,

What’s observed is the effect of the classical gravitational potential on the wavefunction of a particle. This isn’t really “quantum gravity”, by which one normally means quantization of the gravitational degrees of freedom (e.g. the metric), which would show up as new phenomena such as a graviton (quantized excitation of the gravitational field). If this method works out, you would get more information about classical gravity at shorter distances, but still nothing about whether and how gravity gets quantized.

26. **McNicholl**
April 21, 2011

Still questionable that it provides information on classical gravity at small scales…if I intrpet scale as the range we are always inverting. I.e., the effective range we know from 101 is ~ radius of the earth.

27. **Arun**
April 21, 2011

Assuming Higgs? is not spoofing us:
http://arxiv.org/abs/1103.0631

Quoting from the abstract: “…the di-photon Higgs signal $pp->h->\gamma\gamma$ is a sensitive probe for new physics…….(i) In the MSSM the signal rate for the SM-like Higgs boson (the lightest CP-even Higgs boson) is suppressed compared to the SM prediction, and for many survived parameter samples the suppression can be over one order; (ii) For the SM-like Higgs boson in the NMSSM the signal rate can be either suppressed or enhanced by one order, depending on the parameter space; (iii) For the SM-like Higgs boson in the nMSSM the signal rate is much suppressed.”

28. **OhDear**
April 21, 2011

“Higgs?” more information please…

Why are only a few authors listed, and not the whole collaboration? Also I wouldn’t expect the abstract of an experimental paper to gush about new physics like that. So I’m a little sceptical and think you might be pulling our collective leg.

29. **Amos**
April 21, 2011

@OhDear:

Re “Why are only a few authors listed”? That seems to be how Atlas organizes its papers. See http://www-wisconsin.cern.ch/physics/papers.html .

And its the right names. But it still doesn’t feel right.

30. **Anonymous (but PW can see my details anyway)**
April 21, 2011

Higgs? is not pulling any leg, the paper is there. Of course the reason there are only few authors and such a strong claim is the same – it’s still an internal publication. And there seems to be some room for discussion about the photon identification methods, but after all that’s why internal notes are internal.

This Week's Rumor

April 21, 2011
Categories: Experimental HEP News, Favorite Old Posts

A commenter on the previous posting has helpfully given us the abstract of an internal ATLAS note claiming observation of a resonance at 115 GeV. It's the sort of thing you would expect to see if there were a Higgs at that mass, but the number of events seen is about 30 times more than the standard model would predict. Best guess seems to be that this is either a hoax, or something that will disappear on further analysis. But, since spreading well-sourced rumors is more or less in the mission statement of this blog, I think I'll promote this to its own posting. Here it is:

> Internal Note
> Report number ATL-COM-PHYS-2011-415
> Title Observation of a γγ resonance at a mass in the vicinity of 115 GeV/c2 at ATLAS and its Higgs interpretation
> Author(s) Fang, Y (-) ; Flores Castillo, L R (-) ; Wang, H (-) ; Wu, S L (University of Wisconsin-Madison)
> Imprint 21 Apr 2011. – mult. p.
> Subject category Detectors and Experimental Techniques
> Accelerator/Facility, Experiment CERN LHC ; ATLAS
> Free keywords Diphoton ; Resonance ; EWEAK ; HIGGS ; SUSY ; EXOTICS ; EGAMMA
> Abstract Motivated by the result of the Higgs boson candidates at LEP with a mass of about 115~GeV/c2, the observation given in ATLAS note ATL-COM-PHYS-2010-935 (November 18, 2010) and the publication "Production of isolated Higgs particle at the Large Hadron Collider Physics" (Letters B 683 2010 354-357), we studied the γγ invariant mass distribution over the range of 80 to 150 GeV/c2. With 37.5~pb−1 data from 2010 and 26.0~pb−1 from 2011, we observe a γγ resonance around 115~GeV/c2 with a significance of 4σ. The event rate for this resonance is about thirty times larger than the expectation from Higgs to γγ in the standard model. This channel H→γγ is of great importance because the presence of new heavy particles can enhance strongly both the Higgs production cross section and the decay branching ratio. This large enhancement over the standard model rate implies that the present result is the first definitive observation of physics beyond the standard model. Exciting new physics, including new particles, may be expected to be found in the very near future.
>
> See: http://cdsweb.cern.ch/record/1346326?

Update: Jester is up late with some comments here.

Update: Tommaso is skeptical here.

Update: It should be made clear that, while members of ATLAS work here at Columbia, I have no connection at all to them, and they had nothing to do with this. The source of the abstract posted here anonymously as a comment is completely unknown to me. The question has been raised of whether I should allow this kind of material to be posted to this blog and I think it's a serious one that I have mixed feelings about. On the one hand, ATLAS has legitimate reasons for keeping this kind of information private, on the other, it's the kind of information that traditionally has sooner or later circulated outside a collaboration in one form or another. As an example, in my graduate student days back in the early 80s, I remember Carlo Rubbia telling a large group of people at the departmental tea about how his experiment had the top quark "in the bag" (actually, they didn't…).

I've generally taken the point of view that it's not my job to stop rumors, but rather to put out accurate information about them when available to me. But blogs do raise all sorts of issues, and they're likely to keep coming up. I'm curious to hear if my readers have any wisdom to share about them.

Update: Via Slashdot, some more comment about this, including disclosure of another vector of information transfer out of ATLAS:

> Someone left a copy of the note on the printer in my office building. (I work on CDF at Fermilab, but there are others in the building who work on ATLAS at CERN.) The gist of the article is that they found a bump in the diphoton mass spectrum at a mass of ~115 GeV. If the Higgs exists, it is expected to produce a bump in that spectrum, and 115 GeV is a very probable value for the mass of the Higgs. (Experiments at LEP ruled out masses up to 114 GeV, but a mass as low as possible above that fits best with other measurements.)
>
> Now, the inconsistencies: The bump that they found is ~30 times as large as the Higgs mass peak is expected to be. However, due to field theory that I don't want to get into here, the Higgs peak in this spectrum could be larger than expected if there exist new, heavy particles that we haven't discovered yet. The latest published result from CDF sets a limit of about 30 times the expected rate at 115 GeV in the diphoton channel. (Yes, this means that, if you're optimistic enough, there's just enough wiggle room to fit a Higgs in there while accommodating both measurements.)
>
> The internal note is very preliminary and uses a crude background estimate; I'll have to see a more thorough analysis before I make any judgment on it. We shouldn't have to wait very long; I expect that after this leak, they'll be working overtime to push out a full published result as soon as possible.

Update: Since I don't traffic in rumors of dubious source, you'll have to go here to get the latest rumors from someone younger who knows about this whole Twitter kind of thingy…

Comments

1. **GJ Philip**
   April 21, 2011

   The note says that a revolutionary new particle will appear soon, something that happens maybe once every 20 years. It also says that the the standard Model is about to be superseded, something that has been touted for 20 years too.

   It's like saying "The world will end next week!" or "A new continent will be found in the Pacific Ocean tomorrow!"

   The flavour of it is similar to a pep-talk at a political rally just before an election.

2. **OhDear**
   April 21, 2011

   If someone has access to the note, perhaps they could send the pdf to peter and he can make it public via this blog.

3. **Yatima**
   April 21, 2011

   Harold Camping [http://en.wikipedia.org/wiki/Harold_Camping] predicts End Of The World Through Rapture for in exactly one month [21st May].

Coincidence? I think not!

4. **Peter Woit**
April 21, 2011

A question for experts: is this kind of signal consistent with Tevatron data? Looking at

http://moriond.in2p3.fr/QCD/2011/MondayMorning/Cooke.pdf

I see an overall combined limit at 115 GeV of 1.5 times SM, and in the specific channel mentioned here (115 GeV Higgs to gamma-gamma) independent limits of about 20 times SM from D0 and CDF. Can a higher production cross-section at LHC than at the Tevatron explain this?

5. **Sumar Ongi**
April 21, 2011

This is probably an April 21st fool's joke…

6. **Eric**
April 21, 2011

If it's true, it likely means that there is a fourth generation.

7. **lun**
April 21, 2011

Lets draft a press release that this is an experimental test for string theory! 🙂

8. **Tom**
April 21, 2011

I can't swear but I believe that when the different final states are combined to quote 'The SM Higgs Bound' the SM branching fraction weights are used. If for some reason the Higgs were SM-like except for the gamma-gamma final state (which I think is non-trivial especially as the glu-glu channel cannot be too much changed) then one needs to look only at the search in the specific gamma-gamma search channel. As Peter points out the bounds from Moriond for masses near 115 GeV are ~20x the SM value.
A Tevatron/LHC experimenter should comment further.

9. **OhDear**
April 21, 2011

Peter, according to the abstract the search is along the lines of that proposed in "Production of isolated Higgs particle at the Large Hadron Collider Physics" (Letters B 683 2010 354-357), which is "semi-exclusive" Higgs production. This is quite different from the searches so far at the Tevatron, which are inclusive, i.e they search for ppbar -> H + X, where X is anything. So this could explain why the previous exlusion limits do not apply.

Of course, we have no real details of the search or analysis…

10. **Peter Woit**
April 21, 2011

OhDear,

I'm no expert, but that paper they refer to strikes me as odd on several counts, including its promotion of the idea of "considering the LHC as a Pomeron collider", and the fact that it looks like it was never posted to the arXiv.

From the abstract, it certainly sounds like all they're doing is looking for two photons and plotting their invariant mass. If so, that's comparable to the Tevatron searches, right?

11. **Paul Wells**
April 21, 2011

If it coupled so strongly to gamma gamma wouldn't it have been seen at LEP ? Is this size signal consistent with the Higgs "hint" seen at LEP ?

I am afraid I think this is a cruel hoax.

Paul Wells

12. **Tim Tait**
April 21, 2011

Four generations aren't enough for a factor of 20. One could do it with five, but that would require something else as well to take care of the precision Electroweak measurements.

13. **Peter Woit**
April 21, 2011

Paul,

Good question. They're claiming to be at 115 GeV, not far at all above the LEP limit.

14. **Tom**
April 21, 2011

Tim: even 5 isn't enough as the Hgg coupling increases BUT there is a destructive interference with the WW contribution in the gamma gamma case shrinking it!

15. **Spencer Chang**

April 21, 2011

I'll fuel the controversy a little bit.

Looking at the Moriond talk by Markus Schumacher (ATLAS), it says that they have a limit of 40 times the expected Standard Model rate (see figure on bottom right of slide number 4). This is with the the 2010 data of 37.6 pb^-1.

http://indico.in2p3.fr/getFile.py/access?contribId=76&sessionId=1&resId=0&materialId=slides&confId=4403

However, if you look at the data (bottom left plot), it is much flatter than what they predict. So what they could be doing in a reanalysis is using a sideband, where you would estimate by extrapolating from outside the peak that the two bins around 115 should have 10 background events each (20 total). That leaves roughly 14 signal events which would give a significance of 14/sqrt(20) = 3.13. With the additional luminosity of 27 pb^-1, scaling up these numbers would give a significance that goes to sqrt(1+27/37.6) = 4.1, very close to the 4sigma the note claims.

16. **Tom**
April 21, 2011

Thanks for the quick analysis Spencer…I showed the same panel to a VERY famous , prize-winning experimenter some time ago 7 HE thought there was a bump there in the 35 pb^-1 data set

17. **Amos**
April 21, 2011

Assuming that it's right, what does this do for SUSY?

18. **Eric**
April 21, 2011

Amos,

It would be a big confirmation for SUSY, but might also imply the existence of additional matter besides the superpartners.

19. **JoAnne**
April 21, 2011

IF this is right, all it says is that there is a bump in the gamma gamma channel at ~115 GeV. It says nothing about SUSY and seems difficult to explain with a Higgs since the production rate is so high.

20. **Eric**
April 21, 2011

IF the bump is the Higgs, then it does say a great deal about SUSY since this is exactly where SUSY says it should be. The high production rate can be explained if there are extra heavy states which also couple to the Higgs.

21. **Alexey Petrov**
April 21, 2011

If it is a Higgs, the enhanced production cross section could just be an upward fluctuation. If I remember it correctly, top quark production cross section was also too high initially…

22. **Tom**
April 21, 2011

Well, MSSM SUSY needs the Higgs below ~130 GeV. If there really are a bunch of extra states (like a 4th generation or more) then this limit no long applies & one can't say that SUSY predicts ~115 GeV. Also the additional states would change the EW fits so that a light Higgs would then also not necessarily be prefered by the data.

23. **JoAnne**
April 21, 2011

@Eric: The problem is that CDF/D0 already limit the glu-glu-Higgs couplings from their searches in the WW* mode. You can't add extra heavy states willy-nilly without screwing this up.

And I contend that a potential observation of a light Higgs does not confirm SUSY. Could be a coincidence. The observation of a sparticle (and the precise measurements of the terms in the underlying Lagranian) confirm SUSY.

24. **Eric**
April 21, 2011

So, if there's a fourth generation, am I right in remembering that the enhanced production rate is about a factor of ~9 for 300-400 GeV fourth-generation quarks?

25. **JoAnne**
April 21, 2011

You get a factor of ~9 enhancement in glu-glu-Higgs with a 4th generation, but then you also get a factor of ~2 suppression in gamma-gamma-Higgs coupling.

26. **Daniel**
April 21, 2011

This is an internal, un-vetted, un-reviewed ATLAS document. It hasn't been released for public consumption precisely because it hasn't been reviewed or vetted.

You're wasting your time spinning your theoretical wheels on such tripe, and it doesn't serve any purpose other than the blogger's desire for hits.

27. **Peter Woit**
April 21, 2011

Daniel,

This blogger actually isn't very interested in hits, this isn't a commercial site. Rumor-mongering has its own rewards though, and this sort has a certain educational value (I learned some things about Higgs searches in general this evening).

I'm shocked to hear that ATLAS internal documents contain tripe!

28. **Daniel**
April 21, 2011

Like all internal, un-vetted documents, there is the significant possibility for errors. I imagine if I ruffled through a theorist's unfinished work I'd find some shocking results which turn out later to be wrong…

I'm all for openness, but then we need to be consistent, so you'd see how often ATLAS sees wiggles in the data which appear to be interesting, but are then quickly understood to be run-of-the-mill errors in background calculations….

29. **DonManuel**
April 22, 2011

Madame Wu is the best scientist in the world, and one of the most influent women in USA.
She discovered already twice the Higgs in LEP in ALEPH.
Her famous words when anybody tried to argue on the consistency of arguments and data were:
"What do you prefer, to have a signal or not?"
So she is now simply rediscovering her Higgs at 115 (that was 10x what expected also at that time).

30. **Steven**
April 22, 2011

Peter,
you rightly chide others for blogs and press releases which allow others to gain a distorted view of research findings. Given that this "Higgs discovery" seems based on an internal ATLAS note and that it is not an approved and well checked result, you're as guilty of hype dissemination as those you criticise.

Experiments spend a huge amount of time and effort in getting results right. They have the right to announce a result only after they've performed due diligence. Circumventing this with rumours needlessly damages their hard-earned reputations and does nothing for the advancement of science.

31. **Tommaso**
April 22, 2011

Hi all,

just to mention that I also covered this in my blog too, after reading Peter's post above. See
http://www.science20.com/quantum_diaries_survivor/did_atlas_just_see_higgs-78316 .

In any case, I think the signal is a fluctuation. Their significance is overblown by tuning cuts, and their estimated look-elsewhere effect (the trials factor due to not knowing in advance what to search for and where) is much smaller than what it actually is.

I also want to comment on the use of these "rumors". I think they are valuable for attracting the attention of outsiders. However, insiders should not be attracted into this game too much…

Cheers,
T.

32. **Visitor**
April 22, 2011

This is likely trigger effect. If the have any trigger cuts around 110-120 GeV, this is threshold behaviour of not very well simulated trigger. ATLAS is famous for its approximations in MC, when they are using average efficiencies for various kinds of particles across large eta-phi areas instead of full detailed MC. So, it is possible to gain whatever bump you want in the areas near the trigger thresholds – they just didn't simulate them properly.

33. **Chris Austin**
April 22, 2011

Tim: "Four generations aren't enough for a factor of 20."

Tom: "even 5 isn't enough"

Is this specifically for 300-400 GeV fourth-generation quarks? What if the 4th generation was heavier, e.g. 600 GeV?

34. **Steven**
April 22, 2011

@Visitor
Can you point me towards an ATLAS physics publication in which a detailed trigger simulation isn't used ?

@Tommaso
I don't share your view that this is all good publicity. The field has suffered from a number of hyped up non-discoveries in recent times, eg WIMP dark matter, dubious resonances etc. The public may well be getting sick of the particle physics community crying wolf. I certainly am.

35. **Tom**
April 22, 2011

Chris: Once the mass for the chiral quark is larger than of the top quark's, then to a good approximation the explicit value doesn't matter so 300 or 600 GeV makes little difference here. However, there is a perturbativity bound on the couplings of these heavy objects so they cannot be heavier than ~600 GeV.

36. **Anon-ish**
April 22, 2011

@Visitor: Oh please. ATLAS is, if anything, famous for using (slow) full simulation for everything. Perhaps you are confusing "efficiency" with "efficiency systematic uncertainty".

I find this discussion rather distasteful: publicizing a collaboration's internal documents, *with absolutely no context*, is a profoundly unprofessional thing to do – we're here to deliver quality science, not to entertain people with exciting rumors. Shockingly, while initially exploring and considering the data, we may use looser terms and standards when talking to each other than we would use for an external publication.

37. **Peter Woit**
April 22, 2011

Steven,

To "hype" something, you have to put out misleading information about it. My goal has been to provide accurate information about topics of interest, and I think that's what I did here, with the posting and subsequent discussion providing to anyone interested a pretty accurate picture of what is going on.

The question of what I should do with different categories of information that either gets sent to me or posted on my blog is a legitimate one. This may be a borderline case, but I don't think the problem here is "hype" as long as the information made available is accurate.

38. **Simplicio**
April 22, 2011

I think I'm "pro-rumor". As you say, they're educational, and it gives us something to discuss during the current drought of actual discoveries. If the rumor turns out to be bunk, I don't really see any harm done.

39. **Maxime**
April 22, 2011

I've seen the note. It is clear that they see something with cross sections largely above what is expected in SM. It is also clear that there is no tengible proof shown that this is not a fluctuation of the QCD background. The QCD with 2 prompt photons is not well known area. The estimations of the background is purely data driven based on the side bands estimation.

What is nice is that the analysis is quite simple, no Neural Network, no complicated cuts and games.

40. **Paul Wells**
April 22, 2011

I think as a taxpayer I have a right to know what is going on at an early stage. Peter and others are doing a valuable service that arguably should be done by CERN itself being more open. This type of blog shows that science is an exciting, risky, and difficult human activity where mistakes are often made. What is the big problem with that ? Please credit us taxpayers credit with enough intelligence to filter out the gold nuggets from the crud.

Paul

41. **Tommaso**
April 22, 2011

I would agree it's a "simple" analysis, but the devil is in the details. If they grew enamoured with the fluke they saw in 2010 data, they probably played around with cuts enough to get something out of nothing.

As for the bigger issue of whether this is good or bad, and despicable or reasonable, I have written at length on the topic, but let me point out that in this case indeed we are talking as if this was an atlas result when it really looks like the work of four people working in isolation. At least that is the impression I got.
I must say that if I were in ATLAS, I would not be very happy of the way things have turned out… But I would never blame the bloggers, who do nothing but report what is already out there. They should blame their own collaborators for leaking the material.

Best,
T.

42. **Steven**
April 22, 2011

@Paul Wells

As a taxpayer you have a right to the best science and value for money. Why does this mean that you need to read internal documentation and look at results as they are being produced/disarded/prepared for publication ? Don't you think that scientists should be allowed to have private discussions during data analysis ? To the best of my knowledge there has never been a climategate-style scandal in particle physics. There are no "secrets" but scientists rather like the right to talk openly at work without the world listening in. Furthermore, collaborations are usually in healthy competition with each other. They wish to develop their own techniques to, eg, suppress backgrounds to steal and edge on the opposition. There won't be too much of that in the true "open access model" which you propose.

43. **Steven**
April 22, 2011

Peter,

you put out information which suggests that ATLAS has found the Higgs and hosted the subsequent discussion/speculation. That pretty much fulfills the definition of hype:
"excessive publicity and the ensuing commotion".

44. **Chris Austin**
April 22, 2011

Tom, thanks.

45. **RealScientist@ATLAS**
April 22, 2011

This is NOT a PUBLISHED PAPER. It's a COM note.

Anything, especially garbage, can be published as a COM note. It's internal to the ATLAS collaboration, NOT REVIEWED by anyone, NOT APPROVED.

An approved result is signed by the entire collaboration, namely more than 2000 people.

Seeing this on a blog is a shame. The person who releases this paper is not worth being called a scientist. Makes me sick.

46. **Peter Woit**
April 22, 2011

Steven,

Read what I wrote introducing the topic:

"Best guess seems to be that this is either a hoax, or something that will disappear on further analysis."

I partly agree with your response to Paul Wells: scientists should largely be allowed to do their data analysis in peace, without the world looking over their shoulder. People need to have some privacy to make mistakes. But I don't buy the argument that scientists won't come up with better experimental methods if the competition is going to immediately get access. A better argument for secrecy would be that leakage of information between two experimental groups makes their results not completely independent, whereas that's a very desirable characteristic.

One thing that occurs to me is that the scale of the LHC experiments is something new. If everyone in ATLAS has access to this note, you're talking about 3000 people, no? Maybe a third of the world-wide experimental HEP community? If you've already got 3000 people looking over your shoulder, you're not exactly able to make mistakes in private. I'm not well-informed about ATLAS procedures, so it surprised me to hear that results from a small group of 4 people were being immediately broadcast to the entire collaboration, before a smaller, more specialized group had done some vetting.

47. **Steven**
April 22, 2011

As was mentioned earlier, anyone can write a com note and send it to the collaboration. A com note is unreviewed and is a very good way to quickly transmit ideas and techniques within the collaboration. A com note is not a result by any stretch of the imagination. Any ATLAS member can write a com note claiming anything.

I agree with "real scientist", whoever passed this on should be ashamed of themselves. Collaborations have internal peer review procedures for a reason.

48. **Nathalie**
April 22, 2011

A historical note: Sau Lan Wu and her collaborators had V-V events in their data while working in the ALEPH Collaboration at LEP (Large Electron Positron) Accelerator. In those days, the events were not considered to be significant. That was indeed ages ago as LEP was closed in the year 2000.

49. **Low Math, Meekly Interacting**
April 22, 2011

There's going to be a lot of this for the next few years, isn't there.

50. **AS**
April 22, 2011

keeping secret the work of 2000 persons in the age of internet is just impossible.

51. **Paul Wells**
April 22, 2011

I don't want to start a flame war so I will summarize my position then shut up.

I paid for this work so I think I should have free access to-

1. All Raw Data
2. Programs required to interpret raw data.
3. Project documentation to interpret 1. and 2.
4. Final publications WITHOUT having to pay for journal.

Agreed though that IF this was leaked without the author's permission it was wrong.

Thanks
Paul

52. **Peter Woit**
April 22, 2011

Paul,

What you're bringing up are issues not really related to the question at hand, that of information getting to the public about incomplete results that have not yet been fully checked. The people who designed and built this experiment have a legitimate interest in seeing that the data from it is responsibly analyzed and interpreted.

53. **Also a real scientist @ ATLAS**
April 22, 2011

The first commenter is correct. This cannot be considered an official result of the ATLAS experiment. It has not been reviewed by even a single internal reviewer. COM notes are, as noted by others, used to communicate information to the whole collaboration. The subject of such notes is information all collaboration members have access to already, but usually written in such a way that it is well-explained for members that don't necessarily participate in a given analysis. It's a 3000 person collaboration; without something like COM notes, it would be difficult to follow everything that goes on. You certainly can't go to all the meetings.

Here is a link (publicly available) explaining the categories of ATLAS documentation that can be found on the CERN document server (CDS): https://twiki.cern.ch/twiki/bin/viewauth/Atlas/CdsCategories

"ATLAS Communications (ATLAS-COM-*): ATLAS communications – no approval or vetting by ATLAS; also used as "discussion category" for documents to be approved (Proceedings, Slides, INT and PUB notes)"

54. **Peter Woit**
April 22, 2011

Thanks for the information about ATLAS documents, although the link you provide requires a CERN login when I try it.

I don't know if anyone from ATLAS or CMS is willing to discuss this, but I for one would be interested in hearing an outline of how things are supposed to work for searches like this one. This case seems anomalous. Surely the standard procedure can't be to have everyone off looking for whatever they think might be interesting, then when they think they see something writing a draft of a paper about how physics has just been revolutionized and putting it up for 3000 people to look at and discuss.

55. **OhDear**
April 22, 2011

yes yes, we all appreciate that this is not an official statement from the Atlas collaboration, and has in no way been vetted by anyone apart from those who produced it.

The question is, how often do sensational com notes like this one get circulated internally? How likely is it that the authors of this note have done something silly in the analysis? They are professional experimental physicists, and are members of Atlas, so they know what they are doing. Of course we all make mistakes, but I assume they would have checked this analysis quite thoroughly, to avoid looking silly in front of the rest of the collaboration.

I also wonder if/how the Atlas heirarchy are going to try to discover the source of the leak. I guess only a limited number of people would have clicked on the link to this document, and their identities may be available. (note that to access the document you need to login using your CERN login.)

56. **mark**
April 22, 2011

Paul,

All LHC publications are available from CERN CDS or arxiv free of charge (unlike most other scientific fields which require you to pay for expensive journals).

I suggest you read the talks and papers on the topic of (1)-(3) (google it) – its far from trivial to do due to various technical software reasons (what happens when the computers the computer code ran on cease to exist – think of a program on a 5 inch floppy disk from 30 years ago – what on earth would you do with it now if you wanted to use the programs and data on it….computer hardware does not last forever, and noone builds any device that could read it anymore), though I believe e.g. ALEPH has attempted this in recent years and lots of people spend lots of time thinking about how best to do this to preserve the data for future generations (obviously given the expense you cannot rerun these experiments easily if you lose the data).

57. **Anonymous**
April 22, 2011

Paul,

If everyone has access to all raw data immediately, what makes you think anyone would actually build these experiments? Just let someone else do the hard work and wait around to do an analysis.

As a former member of ATLAS I can tell you that the only reason most physicists work on these experiments is to have access to the data.

If anyone could do any analysis on any experiment's data then no experiment would actually get built.

58. **Also a real scientist @ ATLAS**
April 22, 2011

Sorry, I thought the link should be publicly available as I got to it from the public page. The important part is the definition of a COM note, which I anyway listed.

>yes yes, we all appreciate that this is not an official statement from the Atlas collaboration, and has in no way been vetted by anyone apart from those who produced it.

Good! 🙂

>The question is, how often do sensational com notes like this one get circulated internally?

I don't want to say "never"; the thing is, we have not been taking data that long…and how can you have a note making truly sensational claims with Monte Carlo simulation? I will say that I have seen some worthless COM notes, documenting studies that are not well done and are clearly not going anywhere. Sometimes a student is going to graduate, or a person is going to need to search for jobs, and they need to just document their work, put an internal number on it, and it looks like they accomplished something. Funding agencies also seem to like it when you can show that you have produced many of these notes. I'm not saying that's the motivation with this incident. But I have seen several throw-away COM notes that fit that bill.

>How likely is it that the authors of this note have done something silly in the analysis? They are professional experimental physicists, and are members of Atlas, so they know what they are doing. Of course we all make mistakes, but I assume they would have checked this analysis quite thoroughly, to avoid looking silly in front of the rest of the collaboration.

Here's the thing: most of the time when an important analysis–let's use a less controversial example, like the measurement of the Z cross section–is documented by a COM note, it is not a situation like this one. These analyses are very complicated and have many pieces. There is an entire working group, of usually around 30 people, behind the note. Each aspect of the analysis is carefully studied. In the case of the Z cross section, there were probably 2-3 people working on the electron identification criteria for that analysis. They presented their results to the working group, and those were discussed. After several iterations of this the group agreed on the criteria that seems to work the best. We went through this for other parts of the analysis…QCD background determination, electron efficiency measurements, etc. When the group has a complete analysis, it is written as a COM note. This is to document and communicate the work that has been done so far. The collaboration offers comments etc., which is helpful but does not count as review. An internal review board is assigned to the note, and only after this board signs off, and all comments from the collaboration have been addressed, can the document be made public.

In this path, the original, un-approved COM note, although not an "official" result, was stringently validated due to the work of many people in the working group and is probably not far off from the final approved version. The authors of the note behind the leaked abstract did not take this path, so while it is not guaranteed that their results or their approach are wrong, but a mistake somewhere is also much more likely.

As far as wanting to avoid looking silly in front of one's colleagues…Even when you are considering people who are professionals, wanting very badly for something to be true, really BELIEVING that something is true, can cloud one's judgement. Also consider: what does Sau Lan Wu have to lose? She has a position with tenure at a nice university. She has a proven track record of success and will continue to be able to get funding for the immediate future. If she makes sensational claims to the collaboration, people will roll their eyes, scrutinize the work, but if it's wrong it doesn't really cost her anything in the end. As for the graduate student and post-docs, I don't know if they have much of a choice whether to be listed as authors or not. I guess they could refuse, but that's tricky since they certainly are the ones who did the work.

>I also wonder if/how the Atlas heirarchy are going to try to discover the source of the leak. I guess only a limited number of people would have clicked on the link to this document, and their identities may be available. (note that to access the document you need to login using your CERN login.)

I am sure that almost everyone in ATLAS has read this document. Seeing who clicked on the link would not be a viable way of finding the leak. I've heard (completely unfounded) rumors that email tracing may be done, but my understanding is that the leak happened via a comment on a blog (this one?). In this case, I guess that the only way to find out would be if the blog owner could be persuaded to give the information. Even in that case, if I were going to leak it that way (and I didn't), I would use a fake email address. So probably we're not going to find out who did it.

59. **OhDear**
April 22, 2011

Hi "Also a real scientist @ ATLAS " – thanks for your detailed reply! Very interesting to see how large collaborations like this work. Us theorists are of course only working in small groups.

Regarding finding the leak, I had in mind that the timestamps of the requests for the document might be available. The culprit would likely be one of the first few to access it. Of course, by now everyone else will have accessed it, but these will all be at later times.

60. **asymptotea**
April 22, 2011

Peter, Here is some press for you.

http://www.wired.com/wiredscience/2011/04/higgs-rumor/

61. **Emile**
April 22, 2011

The tax payer argument does not work. That public funding pays for these experiments does not entitle everyone to listen in to private conversations, read private emails exchanged by colleagues, or get
to go inside the control and start to play with buttons. You don't get access to all internal documents produced by civil servants either (think of lawyers working on behalf of governments or internal documents written by public officials). Similarly, publicly funded artists should not be required to provide you with all their private recordings or sketches or everything that's unfinished or perhaps not yet good enough.
What you pay for is for good science to be performed. This involves necessary internal discussions and studies where scientists, who are experts on different aspects of the detector, trigger, and software, come to a consensus and are willing to stand behind the result. You pay for science to get done properly and releasing results that have not been checked and approved is not good for anybody. It's bad science. Results should be released when the experimentalists are satisfied that the results are scientifically sound. I'm disappointed that internal documentation of a scientific collaboration was posted on this site.
This is tabloid science. Hyping theories gets you into print and similarly, jumping on every bump will get you some attention too. But, given the trials factor associated with these large experiments, you'll
cry wolf often enough that people will not take you seriously. All
this also leads to armchair guessing about a result. This is speculation. It's not serious science. Taxpayers deserve better value for their tax money.

62. **barry taylor**
April 22, 2011

It is difficult to keep a secret in today's World. Some of us simple folks think this is all pretty exciting given the news from the Chicago area collider a few weeks back. A large public excitement over the GOD particle might generate enough interest to get more $$$ for the science bunch. The U.S. is in desperate need of some good news. Let the rumor run, and let's all have a good time and pay up if they don't gots it yet. More $, more research. More interest, more money.

63. **Jay**
April 22, 2011

The taxpayer argument works. There exists freedom of information legislation in over 85 countries. Additionally there are open meeting laws enacted in many countries. It would be interesting to be a fly on the wall in your HEP experiment meetings. The taxpayers who sponsor these experiments have a legal right to see the process that leads up to the good science that is produced.

In astronomy, there are many instrument builders who take great pride in their work. They are especially celebrated when astronomers use the data produced by their instruments to gain profound insights into nature.

64. **Steven**
April 23, 2011

@Jay
I'm sure that one could attempt to use FOI information to get hold of all sort of internal information. However, I have a suspicion that, since the LHC experiments are CERN-based and since CERN is an international organisation in a country which offers such organisations privileged status, I'm not altogether sure such information would eventually become available.

Also, I'm at a loss to see how science would improve even if the cupboard doors were flung open and every internal document was made available. If all techniques and all numbers (eg calorimeter scale uncertainties) were available as soon as they were calculated and documented then why should a country bother to take part in the experiment and do the messy bits i.e. build and maintain the detector. All the information anyway becomes available for free. Great news for science until the science stops happening. Its also not so great news for the PhD student who is measuring, eg, jet production but who also has spend much of their time making sure their bit of the detector works while another PhD student enjoys the fruit of the first student's labour by reading all the documentation on jets and publishing first.

Also, we should at the same time be making available theorists' early numbers. That theorist who has spent a year updating his/her Monte Carlo program in collaboration with a couple of others should be prepared to give it up to others to use before they have the chance to write their own publications with it.

Particle physics has been at the forefront of efforts to make freely available research findings through an open access model. Similarly, a lot of work is going on in making sure that experiments are able to document their data and techniques such that future analyses will be possible (and open to all). One can throw a spanner in the works with FOI requests (knocking down things is easy, building them up is hard) but I'm far from convinced that this would lead to better science being performed.

65. **Ryan**
April 23, 2011

I don't see what the big deal is with leaking this?

This is similar to a conference paper! A bit of info pre-publication. If it turns out to untrue, who cares! Some more publicity for the scientists. If it's true we get a sneak peak before it's on the cover of Nature.

66. **physicsrob**
April 23, 2011

@Steven
Exactly — But I think you should take it a step further. The analogy would be that theorists should open up their notebooks and share any incomplete idea they are working on.

@Jay
The analogy with telescopes doesn't completely work. ATLAS has roughly 3000 physicists. Every single one of them is required to complete "service work" to be a member of collaboration. If completing this work was not a requirement for analyzing the data then literally half of the physicists (more, actually) wouldn't bother being members of the collaboration or doing any of the necessary work.

67. **Also a real scientist @ ATLAS**
April 23, 2011

@Ryan:

>This is similar to a conference paper!

No it is *not*. That is precisely what all the fuss is about. This paper has not gone anywhere NEAR the level of internal checks that we do before putting something out as a conference paper.

>A bit of info pre-publication.

It is not necessarily info, it is quite possibly trash. The result has not been checked by the collaboration as is necessary to ensure that the analysis was properly done. As far as publication, this is not anywhere near being pre-publication. Others have indicated they understand this, but I feel I must emphasize it again for your benefit: this is *not* an official result of the ATLAS experiment, and you should not expect to see this any time soon as a publication.

>If it turns out to untrue, who cares! Some more publicity for the scientists. If it's true we get a sneak peak before it's on the cover of Nature.

You don't care I guess, but as a member of the collaboration I do. I feel that it reflects badly on me and my colleagues that this has been leaked. A large fraction of the general public seems to believe that this is a result that has been reached and approved after our usual rigorous checks…that it's just almost a result we can all stand behind, but not quite. You very much imply that you think that's exactly the case, but it couldn't be further from the truth.

As far as the authors, they may gain publicity, but I don't think I'd want it in 3 of their cases. Wu won't suffer, I think. She's too well established. The other 3 are a grad student and 2 post-docs. It's a small field. How are their job prospects, as you say, "if this turns out to be untrue?"

68. **Ryan**
April 23, 2011

@ Also a real scientist:

Hang on – you peer review your conference papers? Get real.

Not one organization of respect is claiming this as fact. Everyone has claimed this as just what it is – a rumor. If your worried about the morons in the general public taking this the wrong way you have bigger issues to worry about. And don't flatter yourself – the vast majority of the worlds population don't understand nor care what you are doing. I care – and understand some – but I'm in a minority.

As for the phd students – again, get real. As a recent phd graduate, if they drop the name of where they did their thesis studies the whole field will open their doors to them.

For a scientist, you are awfully melodramatic.

69. **Giotis**
April 23, 2011

Real scientists, I think you are exaggerating. An internal note about the Higgs hunt was leaked. So what? This kind of research does not have any impacts on society (like a prognosis of an upcoming earthquake or the cure of a disease would have for example) so I don't see any harm done. On the contrary such results often trigger public discussions which are always good and informative for many people. Also I don't see why you should worry about the reputation of the people involved and of ATLAS collaboration. Don't you think people are smart enough to understand that this (as stressed by Peter and others) is just an unvetted internal note?

70. **Steven**
April 23, 2011

@Ryan
Yes, referees are appointed for all conference contributions and they are rigorously reviewed. This is the true for all of the major collaborations.

Neither I nor "real scientist" are being particularly melodramatic.
The fact is that the LHC was built primarily to find the Standard Model Higgs or to disprove its existence. A lot of people have spent a lot of time making sure that the

ATLAS detector works and that results are sound and definitive. Circumventing a well structured review chain to spread rumours about unchecked results on a Higgs search is annoying and unscientific.

71. **Steven**
April 23, 2011

@Giotis

Over the past few years there have been a few "discoveries", eg hints for WIMP dark matter and various new particles. Unfortunately, these discoveries have tended to vanish upon receipt of more data.

It does the field little credit to continuously have rumours washing around but no actual verified discoveries.

72. **Also a real scientist @ ATLAS**
April 23, 2011

Thanks Steven, for articulating very well exactly what I also think about this.

@Ryan: As Steven said, yes we do internally peer review our conference notes. So do CMS and CDF and D0. Furthermore, we internally review a class of internal notes as well, known as INT notes. COM notes really are the furthest thing down on the ladder of peer review, a ladder that we take very seriously…they are the ONLY class of ATLAS documents on CDS that is not reviewed. This includes the actual slides shown at conferences, which must also be signed off on.

I don't think that people outside of my field are morons. I do realize that they provide my funding via taxes. That funding is for research that I'm convinced has a noble purpose, even if there is no immediately practical application. That noble purpose is the pursuit of further knowledge, just for the sake of it, conducted in a scientific manner. Leaks like this severely undermine that, and that is especially the case if the work is not rigorous enough to stand up to scrutiny. With that said, I actually do not have more important things to worry about. If this is what the general public grows to expect of us, we are going to be out of business.

As for my "melodrama" regarding the future job searches of the post-docs and grad student, I would venture to guess that you are not a part of this field. It really is a valid concern, especially for the post-docs. It may be of interest of you to see where Sau Lan's grad students end up: http://www-wisconsin.cern.ch/~wus/achievements.html Notice the trend towards grad students staying at Wisconsin for post-doc positions, something considered unusual and not particularly desirable in this field, or going onto less prestigious positions than earlier in her career. She paints a very rosy picture to the outside world, but is not as highly thought of within HEP as her website and interviews with the press would have you believe.

@Giotis: I guess you see no harm done because it probably doesn't impact your life as much as it does mine. Quite frankly, it is just *annoying* on one level. From what I've seen on message boards and blog comments, many people don't understand that this is not on behalf of the whole collaboration. It's annoying to have my name attached to that note in any way. It's annoying that if we do really discover the Higgs later, the result will be viewed by the public with sort of a contempt…many would question the discovery even if that result would be scrutinized to the highest level possible, just because we "cried wolf" earlier. Aside from the annoyance, I do also see it as harmful for the reason I told Ryan. There is already a large sort of mistrust towards scientist, and a reluctance to give us funding. Things like this just make that worse.

73. **Non-HEP PhD**
April 23, 2011

Not even wrong is my all time favorite phrase of condescension ;). Go Peter! …. I disagree with the writer who asserts rights to early access to data. … I hope for all concerned that this is a hoax

74. **David**
April 23, 2011

3 questions come to mind:
1- who has interest in leaking this info?
2- why did the leaker prefer the Columbia Uni site?
3-what will be gained or destroyed with this leak?

No matter what the answers are, this leak shows that there are some internal problems within the Atlas collaboration.

[*personal attack deleted*]

New people, but well connected to the management, can be appointed to key positions in ATLAS even before doing any service work for the collaboration. This has certainly got many people angry, and some of them have simply left ATLAS.

All the physics leaders are appointed by the management and the collaboration has no say. Yes, powerful people with a lot of $ certainly get involved in the process, but everything is done behind the scenes. Is this right in HEP? no idea? will this model work in a collaboration with 3,000 people? I don't know….

I Think CERN has to take this leak seriously and should try to understand why people are unhappy. Perhaps the solution will be to fire this management and appoint a new management which will encourage the creativity of people instead of shooting up everybody who does not belong to the close circle of the Queen. This may explain why ATLAS is always behind CMS. I think this leak is a wake-up call for CERN and will expose all the dirty-political games going on in ATLAS. Unhappy people do unpopular things.

Now coming back to the leak, my view is that the leaker is an enemy of Dr. Wu? atlas physicists should have some names…..but as I said, this is not the real issue. The real problem is the atlas management.

75. **Peter Woit**
April 23, 2011

David,

Please do not use this blog for anonymous attacks on people, that's really problematic.

I have no idea at all who posted the abstract, have seen suggestions both that it was an ally of Dr. Wu or that it was an enemy of hers. It seems even more likely to me that it was just some random ATLAS member who thought this was exciting news that deserved wide discussion, enough so that they were willing to break ATLAS policy on this. The choice of my blog was probably just due to the fact that it has quite a few readers interested in the topic.

76. **BBBShrewHarpy**
April 23, 2011

@giottis:
In addition to the problems of credibility for science in general and ATLAS in particular referred to by the real scientist@ATLAS, this leaking has serious implications for the continued effectiveness of the collaboration. I am not part of ATLAS, but I am part of a large collaboration, with rules that aim to ensure effective work practices and equitable rewards for those who perform the work and follow the rules. There is a lot of background slogging that goes on in large experiments, and as much close work that goes into evaluating results. Not all of this work receives press, or results in presentations at conferences, but all of it is vital for the running of the experiment. The tension between those who perform the hard work without recognition and those who receive the flashy rewards exists even when the rules are followed, but when the system breaks down, all the grievances and frustrations can cause severe problems for the working of the collaboration. Much of this is a matter of personalities and human nature, and conflict is inevitable, though we would all love to believe that our love for science is pure and our expectation of worldly rewards nonexistent. This facade can hold only if those of us who are mostly drudges and worker bees are not pushed beyond a certain limit by the queen bee and her attendants. In my view the rights of the people who are attempting to live within the strictures of the collaboration trump the vicarious thrill the rest of us are experiencing from the breakdown of the system.

77. **scientistOnCMS**
April 23, 2011

I'm a collaborator on CMS. I can tell you that notes that are unreviewed by the collaboration never see the light of day, and for good reason, on every major HEP collaboration. Thousands of scientists have reputations that rely on the results that come out of the experiment. So there's a peer review process for even single plots to be shown at APS section meetings. Nothing particularly prestigious about those meetings, but the results must be approved nonetheless.

As for people claiming rights to the data because they paid taxes? Give me a break. First, CERN is an international collaboration, and the bulk of funding comes from European countries. But even ignoring that, I could give you the raw data, and you wouldnt' be able to do a thing with it. You certainly don't have rights to our software. And even if you did, what would you do with it? Do you understand the physics you'd be looking for? If not, it's useless to you. Regardless, keeping the data confidential (even from scientists on rival experiments) is a very good idea from a science point of view. It keeps everybody honest. If one experiment finds something, the other can then look in their own dataset to find a confirmation of the effect. Independent collaborations are a good thing.

78. **physicsrob**
April 23, 2011

@scientistOnCMS
Very well put. I think many non-experimentalist underestimate just how much effort goes into taking the raw data and generating for example a diphoton mass plot.

79. **sciing**
April 23, 2011

What are you talking about? There are some rumors about the a discovery. Nice to know. No one takes it as real. But its nice for all the fans of HEP. And no one else is really interessted.
I have more concerns about the way CDF is working right now.
I think a note like this is not destroying the reputation of the particle scientiest but all the stuff that comes out right now from CDF (per reviewed from the whole collaboration). I get they feeling they make 1000 analysis and claim everything a hit thats probability to be nothing is less than a thousands. Is this science? CDF does not only claiming the results but also begins to speculate about new forces and so on. What is the motivation? At the end of the year LHC will get enough data to answer al this question. So why they can't wait?
On the other hand is it science if the carrier of young graduate is detroyed just for starting a disscussion of a new "result", observation?
That these note is leaked is a problem of ATLAS not the graduates.

80. **Peter Chase**
April 23, 2011

Much uncertainty. Can we say ATLAS shrugged?

(Gad, that was cheap.)

81. **Berger**
April 23, 2011

@ "David" :

> No matter what the answers are, this leak shows that there are some internal problems within the Atlas collaboration.

> All the physics leaders are appointed by the management and the collaboration has no say.

This statement is factually wrong. "Physics Leaders" (by which I assume you mean physics group conveners) are elected by the collaboration.

The rest of the post probably qualifies as "Not even wrong".

82. **NoNaMe**
April 24, 2011

To Paul Wells, who wrote
"I paid for this work so I think I should have free access to- 1. All Raw Data
2. Programs required to interpret raw data.
3. Project documentation to interpret 1. and 2.
4. Final publications WITHOUT having to pay for journal."
Wow. I guess with the amount of money *you* paid, you should be given access to a couple of events, five lines of code and a commemorative ball-pen.

83. **Georges**
April 24, 2011

@David: well written.

@Berger:

Your statement about physics conveners is wrong and David is right. The collaboration has no say. There is a call for nomination by the collaboration. The management makes the selection. There is no shortlist that is communicated to the collaboration, there is no discussion within the collaboration and the voices of the collaboration have always been ignored. People without (or not enough) service work and/or inactive people became physics leaders from nowhere. However they are good servant of the

Queen!

84. **woit**
April 24, 2011

I’m shutting off comments for now. The fraction of informed comments relevant to the posting has gone to almost zero, and dealing with the large number of other ones is wasting too much of my time.

Short Items

April 27, 2011
Categories: Experimental HEP News, Multiverse Mania

- Progress on increasing luminosity at the LHC has been going extremely well, with peak luminosity a few moments ago over $7\times10^{32}cm^{-2}s^{-1}$. So far integrated luminosity is over 200 pb^{-1}, well on the way to the extremely conservative nominal goal for the year of 1000 pb^{-1}. By fall, with the shutdown of the Tevatron at the end of FY 2011, the LHC experiments should be in a position to start overtaking the Tevatron and seeing evidence of a standard model Higgs if it is there.
- It's still very early to know how Fermilab will do in the US FY2012 budget, other than that the Tevatron will definitely not be there. However, the Obama administration is supportive in its budget proposal, and this document from the Republicans running the relevant House committee is encouraging for HEP research. Democrats and Republicans seem to agree that science research is a good thing in general, and HEP research is not one of the categories that annoys Republicans and that they suggest cutting (applied research that could be done by private companies, climate science research, environmental research, ITER). One member of the committee is freshman Republican Randy Hultgren, who represents the district that includes Fermilab, and he added his own addendum to the report, emphasizing support for HEP research. Hopefully the Republicans will want to help re-elect him by getting him anything he asks for…

With the bizarre US budgeting process of recent years though, whatever the appropriate Congressional Committee decides may turn out to be irrelevant, with last minute budget cuts appearing from mysterious sources to get things under whatever numbers end up being agreed to.

- The New Yorker has a profile this week of David Deutsch. I still can't figure out what his argument is that if a quantum computer works, that means there are multiple universes.
- Lots of people are asking me what I think of 't Hooft's new paper. The answer so far is just that I don't understand it. He's doing something unusual with how he handles conformal symmetry, and I think one needs an expert on that to weigh in.
- Mathoverflow continues to amaze me, providing the sort of high-quality discussion that the internet was always supposed to provide, but rarely did. For example, see this recent question, which asks about the relationship between two different ways of encoding the geometry of a manifold. One way to do this is to choose a metric, the other is to choose a connection on the frame bundle. For arbitrary bundles, there's an infinity of possible connections and they have nothing to do with the metric, but the frame bundle carries extra structure (the vierbeins, in physicist's language). Given a metric, this extra structure can be used to pick out a unique connection (called the Levi-Civita connection), which satisfies two conditions: orthogonality and zero torsion. The question asked is about whether one can go the other way: given a connection, is there a unique metric for which it is the Levi-Civita connection?

The answers given include one by Fields Medalist Bill Thurston, whose comments reflects his background as a topologist, another is by MSRI director Robert Bryant, whose answer is that of a geometer, one who has delved deeply into the subject, including its roots in the work of Elié Cartan. The fact of the matter is that the relationship between these two structures is not one-to-one, for reasons that are well explained. This may be of interest to physicists thinking about the quantization of gravity. In that subject, one basic question is that of which fundamental variable to pick to "quantize", and the conventional choice is the metric, even though in non-gravitational physics, the conventional choice is the connection. Philosophically though, the gauge symmetry involved in gravity is something like local translation symmetry, and the right analogy of a Yang-Mills connection might be not a connection on the frame bundle, but something like the vierbein, but that's a whole other story….

Comments

1. **Deane**
   April 27, 2011

   I'd like to echo how amazing and wonderful it is to see answers by Bill Thurston and Robert Bryant to a question on MathOverflow, not only because each one is a world class mathematician but because each brings a completely different perspective to the same question. It allows the rest of us, especially students, to see how differently the same question can be approached by different mathematicians.

2. **anon**
   April 27, 2011

   Support the new site: http://physics.stackexchange.com/
   The community has quite a few experts on hep, including a persistent criticizer of this blog.

3. **das higgs**
   April 27, 2011

   The author thanks ….R. Bousso, …P. Mannheim, … for discussions.
   huh.

   I can tell you it's going to be a controversial and not seriously taken paper without even reading it

4. **also anon**
   April 28, 2011

   @anon: so math has Fields medalists and phys has Lubos.
   Kind of sums up the sorry state of physics if you ask me.

5. **Octoploid**
   April 28, 2011

   Regarding 't Hooft's new paper:
   I always thought that this (local conformal symmetry)
   is the only direction in which further progress could be
   made on the theory side. (The whole mass/higgs and
   hierarchy problems are closely related to this)
   It's nice that he's working on that topic.

6. **Tim van Beek**
   April 28, 2011

   The physics stackexchange site allows homework level questions, too, and should rather be compared to the math stackexchange site than to math overflow. The success of math overflow can be attributed to the strict moderation, allowing high quality questions only – and of course the existence of a consensus about the meaning of "high quality" in the global math community in the first place. Is there a similar consensus in the physics community or even in the hep-th community? I don't thinkt so…

7. **John Baez**
   April 28, 2011

   If you support a certain glamorously worded version of the many-worlds interpretation of quantum mechanics, the ability for a quantum computer to put itself in a superposition of a bunch of states, do interesting things in each of those states, and then 'recombine' and cough up the answer to a problem would be evidence for 'many universes'. But so would various simpler applications of the superposition principle. I don't think it's really that big a deal. It's just a way of talking.

   If you don't like this way of talking, you can just translate it into an interpretation that you like better – if you do it right, you'll never disagree on any observed phenomena. That means you can argue endlessly about which way of talking is 'right' and never reach a conclusion. But it also means you don't have to.

8. **Moshe**
   April 28, 2011

   For various reasons the physics stack exchange site is not likely to ever have the same quality as Mathoverflow (I don't think it's the level of questions, but the quality of the answers which is the issue, but that's for another place). There is a proposal to have higher quality physics site devoted mainly to research questions here:

   http://area51.stackexchange.com/proposals/23848/theoretical-physics

   Once it has enough supporters we'll see exactly what is needed to have high quality discussion, but I understand the intention is to aggressively pursue something like the MO model. I suspect it won't be easy…

9. **Mike**
   April 28, 2011

   In a short interview Deutsch was once ask:

   "How does quantum computation shed light on the existence of many worlds?"

   He responded as follows:

   "Say we decide to factorise a 10,000-digit integer, the product of two very large primes. That number cannot be expressed as a product of factors by any conceivable classical computer. Even if you took all the matter in the observable universe and turned it into a computer and then ran that computer for the age of the universe, it wouldn't come close to scratching the surface of factorising that number. But a quantum computer could factorise that easily in seconds or minutes. How can that happen?

   Anyone who isn't a solipsist has to say the answer was produced by some physical process. We know there isn't enough computing power in this universe to obtain the answer, so something more is going on than what we can directly see. At that point, logically, we have already accepted the many-worlds structure. The way the quantum computer works is: the universe differentiates itself into multiple universes and each one performs a different sub-computation. The number of sub-computations is vastly more than the number of atoms in the visible universe. Then they pool their results to get the answer. Anyone who denies the existence of parallel universes has to explain how the factorisation process works."

   This was a simple response to a complicated question in the popular press. Of course, he goes into greater detail in his work, and especially in his two books.

   That may not satisfy you — but it gives a pretty good outline of how his argument runs.

10. **dan**
    April 28, 2011

    "Anyone who isn't a solipsist has to say the answer was produced by some physical process. " You know, like, maybe, the schrodinger equation in the observable universe?

    Not including quantum mechanics in your definition of "computing power" seems a little restrictive. Is there a precise definition/reason for sticking to classical physics?

11. **Peter Woit**
    April 28, 2011

    Mike,

    I did read that argument in the article, and it made no sense to me, still doesn't. All it seems to give is that there is more to the universe than classical physics, not that there are other universes.

12. **Moshe**
    April 28, 2011

    While I am here, seems to me the basic flaw in Deutsch argument is that it suggests a speedup in quantum computing far beyond what is actually attained. It is also strange that the argument does not need or make reference to decoherence, which is a necessary ingredient in any "many-world" view.

13. **Klil Neori**
    April 28, 2011

    @Mike:
    Deutsch's argument is fallacious. They key term is "classical computer." There do, in fact, exist physical processes working within this universe that can accomplish what he is stating: they are part of quantum mechanics. Unless you believe quantum mechanics requires multiple universes, this isn't a proof. It is circular reasoning.

    Moreover, he is forgetting that prime factorization has not been proven to be above polynomial – it is just that the asymptotically most efficient algorithm known is not polynomial. He would best stick to a fact that is well known, that simulation of a quantum system by a classical computer is, in fact, exponential in the number of quantum states.

    As for the article: from the abstract (I don't have access to the paper itself), I object to a few claims made in the New Yorker article itself. For example, "With one millionth of the hardware of an ordinary laptop, a quantum computer could store as many bits of information as there are particles in the universe." That requires a very odd definition of storage. Storage usually implies retrieval. You cannot retrieve more than N classical bits out of an N-qubit system. Or "Tells how Deutsch came to propose a universal computer based on quantum physics, which would have calculating powers that Turing's computer (even in theory) could not simulate." This is kind of sloppy, since in theory a Turing machine could in fact simulate any quantum system – it may just take more than the age of the universe/more than the matter available in the universe to do so.

Of course, these could be just mistakes by the writer rather than by Deutsch himself.

14. **Mike**
April 28, 2011

Peter,

I know you've seen at least part of the following as well. I think it's informative, but of course, if you wish to delete it I understand and am not interested in cluttering up your blog.

Dan,

The the Schrodinger equation, as applied to a quantum computer, certainly "predicts" the right results, yes — but it doesn't "explain" how they arise.

Deutsch has addressed this distinction by analogy to Einstein's general theory of relativity:

"Our best theory of planetary motions is Einstein's general theory of relativity, which, in the early twentieth century, superseded Newton's theories of gravity and motion. It correctly predicts, in principle, not only all planetary motions but also all other effects of gravity to the limits of accuracy of our best measurements. For a theory to predict something "in principle" means that as a matter of logic the predictions follow from the theory, even if in practice the amount of computation that would be needed to generate some of the predictions is too large to be technologically feasible, or even too large to be physically possible in the universe as we find it.

Being able to predict things, or to describe them, however accurately, is not at all the same thing as understanding them. Predictions and descriptions in physics are often expressed as mathematical formulae. Suppose that I memorise the formula from which I could, if I had the time and inclination, calculate any planetary position that has been recorded in the astronomical archives. What exactly have I gained, compared with memorising those archives directly? The formula is easier to remember – but then, looking a number up in the archives may be even easier than calculating it from the formula. The real advantage of the formula is that it can be used in an infinity of cases beyond the archived data, for instance to predict the results of future observations. It may also state the historical positions of the planets more accurately, because the archives contain observational errors. Yet, even though the formula summarises infinitely more facts than the archives do, it expresses no more understanding of the motions of the planets. Facts cannot be understood just by being summarised in a formula, any more than by being listed on paper or memorised in a brain. They can be understood only by being explained. Fortunately, our best theories contain deep explanations as well as accurate predictions. For example, the general theory of relativity explains gravity in terms of a new, four-dimensional geometry of curved space and time. It explains how, precisely and in complete generality, this geometry affects and is affected by matter. That explanation is the entire content of the theory. Predictions about planetary motions are merely some of the consequences that we can deduce from the explanation.

Moreover, what makes the general theory of relativity so important is not that it can predict planetary motions a shade more accurately than Newton's theory can. It is that it reveals and explains previously unsuspected aspects of reality, such as the curvature of space and time. This is typical of scientific explanation. Scientific theories explain the objects and phenomena of our experience in terms of an underlying reality which we do not experience directly. But the ability of a theory to explain what we experience is not its most valuable attribute. Its most valuable attribute is that it explains the fabric of reality itself. As we shall see, one of the most valuable, significant and also useful attributes of human thought generally, is its ability to reveal and explain the fabric of reality.

Yet some philosophers, and even some scientists, disparage the role of explanation in science. To them, the basic purpose of a scientific theory is not to explain anything, but to predict the outcomes of experiments: its entire content lies in its predictive formulae. They consider any consistent explanation that a theory may give for its predictions to be as good as any other, or as good as no explanation at all, so long as the predictions are true. This view is called instrumentalism (because it says that a theory is no more than an "instrument" for making predictions). To instrumentalists, the idea that science can enable us to understand the underlying reality that accounts for our observations, is a fallacy and a conceit. They do not see how anything that a scientific theory may say beyond predicting the outcomes of experiments can be more than empty words. Explanations, in particular, they regard as mere psychological props: a sort of fiction which we incorporate in theories to make them more memorable and entertaining. The Nobel prize-winning physicist Steven Weinberg was in an instrumentalist mood when he made the following extraordinary comment about Einstein's explanation of gravity:

"The important thing is to be able to make predictions about images on the astronomers' photographic plates, frequencies of spectral lines, and so on, and it simply doesn't matter whether we ascribe these predictions to the physical effects of gravitational fields on the motion of planets and photons [as in pre-Einsteinian physics] or to a curvature of space and time."

Weinberg and the other instrumentalists are mistaken. It does matter what we ascribe the images on astronomers' photographic plates to. And it matters not only to theoretical physicists like myself, whose very motivation for formulating and studying theories is the desire to understand the world better. (I am sure that this is Weinberg's motivation too: he is not really driven by an urge to predict images and spectra!) For even in purely practical applications, the explanatory power of a theory is paramount, and its predictive power only supplementary. If this seems surprising, imagine that an extraterrestrial scientist has visited the Earth and given us an ultra-high-technology "oracle" which can predict the outcome of any possible experiment but provides no explanations. According to the instrumentalists, once we had that oracle we should have no further use for scientific theories, except as a means of entertaining ourselves. But is that true? How would the oracle be used in practice? In some sense it would contain the knowledge necessary to build, say, an interstellar spaceship. But how exactly would that help us to build one? Or to build another oracle of the same kind? Or even a better mousetrap? The oracle only predicts the outcomes of experiments. Therefore, in order to use it at all, we must first know what experiments to ask it about. If we gave it the design of a spaceship, and the details of a proposed test flight, it could tell us how the spaceship would perform on such a flight. But it could not design the spaceship for us in the first place. And if it predicted that the spaceship we had designed would explode on takeoff, it could not tell us how to prevent such an explosion. That would still be for us to work out. And before we could work it out, before we could even begin to improve the design in any way, we should have to understand, among other things, how the spaceship was supposed to work. Only then could we have any chance of discovering what might cause an explosion on takeoff. Prediction – even perfect, universal prediction – is simply no substitute for explanation."

15. **Peter Woit**
April 28, 2011

Mike,

I don't buy that either. Quantum mechanics "explains" what is going on perfectly well in this case, it's a compelling model of physics, far more powerful and successful than the classical model. Insisting that anything that counts as an explanation must retain features of the classical model seems unreasonable. To me, multiple universes actually "explain" nothing in this case. They provide a way to look at things that some people might find attractive, but as far as I can tell they just introduce a lot of extraneous structure that raises more questions than it solves.

16. **Mike**
April 28, 2011

Peter,

I understand your view — I guess we just disagree on what consitutes a good “explanation”. Thanks for the chance to discuss.

17. **Klil Neori**
April 28, 2011

I think there is a false dichotomy here: it’s not “instrumentalists” vs. “many worlds interpreters”. It’s “many worlds interpreters” vs. people who do not find it compelling for whatever reason. It is disingenuous to present “many worlds” as the only viable interpretation or world-view.

18. **Mike**
April 28, 2011

“It is disingenuous to present “many worlds” as the only viable interpretation or world-view.”

No it isn’t. It might ultimately be wrong. But it’s certainly not disingenuous to compare the philosophical basis of the MWI to instrumentalism (only predictions are important) or solipsism (the calculations just happen in an abstract “black box” unrelated to any explanation of the actual physical entities and processes taking place).

I promise Peter — that will be my last comment on this point 🙂 Thanks again for allowing it to continue, probably much longer than you would like.

19. **Klil Neori**
April 28, 2011

You concede that it might ultimately be wrong, yet you persist in denying that there are other interpretations that do not fall into instrumentalism or solipsism: consistent histories, pilot wave, stochastic mechanics, objective collapse, etc. All have their faults (the latter actually makes predictions that have been falsified), but they are not all less viable than many worlds, nor are they all less established than many worlds in the foundations of physics community, and it is therefore disingenuous of you (as it is for Deutsch) to present many worlds as the only world view.

20. **Physicsphile**
April 28, 2011

I realize Peter does not want the comment section to turn into a debate on the interpretation of QM. But I can’t resist mentioning my own take on this. The many worlds interpretation seems to me to be to be the most direct way of interpreting the mathematical formalism of QM. As far as I understand it is just saying the ultimate reality is a wave function on configuration space. It is not adding anything new to the QM formalism it is just saying what it is. While pilot wave, stochastic mechanics, etc seems to me to be adding new physics which is not motivated by experiment.

21. **Peter Woit**
April 28, 2011

Physicsphile (and others),

Yes, unless it’s interesting and about David Deutsch, please don’t continue here with the usual sort of discussion of QM interpretational issues. Personally I find this kind of discussion just depressing, tedious and unenlightening. There are interesting issues here, but people seem devoted instead to over-simplified sloganeering. Enough.

22. **outside_math**
April 29, 2011

Moshe,

From my perspective as an outsider to both fields, there is a very large cultural difference between math and theoretical physics. Several Field’s medalists not only contribute to mathoverflow but blog about technical mathematics as well. There is nothing comparable in the physics world, heck you can’t even get physicists to comment with their real names on blogs! My guess is that a physicsoverflow site will not work until physicists at the very top level of research are willing to actively participate. It’s a shame, because I think that such a site would be very useful.

23. **also anon**
April 29, 2011

@outside_math I am pretty sure MathOverflow would not have worked well during the Grundlagenstreit with Hilbert and Brouwer going after each other.

And PhysOverflow will not work well as long as the community disagrees if string theory is physics or wishful thinking.
As long as you have Lubos and others behave the way they do, any sane person will either stay away from such websites or not post with their good name.

24. **Yatima**
April 29, 2011

If anyone of the quantum-computing-interested gentlemen or ladies is at CMU on April 29th at 1630 EDT (I won’t):

http://www.cmu.edu/mcs/news/pressreleases/2011/4_25_Buhl2011.html

MIT’s Scott Aaronson to Present 2011 Buhl Lecture – “Quantum Computing and the Limits of the Efficiently Computable”.

Should be interesting.

25. **Scott Aaronson**
April 29, 2011

Yatima: Thanks for the publicity! 🙂

I’ve been working on my own blog post about the New Yorker piece, but briefly: I agree with Peter that quantum computing has no *direct* implication for the Many-Worlds debate. (Ironically, Deutsch also agrees: he thinks that Many-Worlds is already an experimentally-established fact, regardless of whether quantum computers are built.)

Assuming the plausible conjecture that quantum computers can’t be efficiently simulated by classical ones (i.e., P≠BQP), a scalable quantum computer would show that Nature has “computing resources” that exponentially exceed anything described by classical physics in certain respects. But there’s then the further question of whether you want to describe those computing resources in terms of “parallel universes.”

(Incidentally, it's not just factoring that's not known to be hard for classical computers, but also simulating quantum mechanics in general.)

26. **Dave Miller**
    May 1, 2011

    Peter,

    I used to be on an e-mail list of David Deutsch's followers, to which the Big Man occasionally contributed himself. Most of his followers are, of course, non-physicists, and they did not much appreciate questions being raised about whether or not his views on physics can be justified.

    I eventually got bored, but it was sociologically interesting to see a physicist who had real, live, and *very* intense followers!

    I've also followed David's work on MWI for many years. At one point in his career he explicitly recognized some of the technical problems with MWI: e.g., the "preferred-basis" problem and the "probability-measure" problem. He eventually decided that these were not problems, after all, though I was never able to understand his published reasons as to why he changed his mind.

    Deutsch has written widely on politics, child-rearing (!), and, of course, various areas of science, and often has interesting, insightful comments on many of those subjects. However, I found myself a bit turned off by the constant implication that David had found the final answer to every question, answers that should not be questioned.

    Anyway, if the "String Wars" ever end, and you find yourself drawn to further explorations in the side-lanes and by-ways of the sociology of physics, you will find much to explore in the Deutsch phenomena.

    Dave Miller in Sacramento

27. **rrtucci**
    May 1, 2011

    Does "The New Yorker" magazine make you dumber?

    Yes. The article rhapsodic about Deutsch's views proves it.

28. **McNicholl**
    May 1, 2011

    (1) The multiverse implications of the capabilities of a quantum computer makes no sense…since the latter relies on (essentially) nothing that Bohr did not know, and (2) (the short subject that you all ignore) it is navel-looking (if not hypocritical) to hope for a Fermi-lab district congressman to save HEP funding. Where is Aristophanes when we need him?

29. **vorpal**
    May 2, 2011

    Thanks for the link to the discussion on connection coefficients. I really do wonder what the "right" quantity to quantize is.

30. **Shantanu**
    May 3, 2011

    Peter and anothers,
    See a ilnk to this dark matter conference at STSCI which is been webcast
    live. See in particular the talk on LHC (Which discussesthe rumor about Higgs result)
    http://www.stsci.edu/institute/conference/spring2011

This Week's Hype

April 28, 2011
Categories: This Week's Hype

This week's hype comes from an unusual source, John Baez and his ex-student John Huerta, who have a new article in Scientific American entitled [The Strangest Numbers in String Theory](#).

The expository article about octonions by John (Baez) that appeared in the AMS Bulletin (copy [here](#), a web-site [here](#)) is one of the best pieces of mathematical exposition that I have ever seen. The octonions can be thought of as a system of numbers generalizing the quaternions. As with the quaternions, multiplication does not commute, and things are even worse, it's not associative either. So, probably best not to try and think of these as "numbers", but they do give a very remarkable exotic algebraic structure, one that explains all sorts of other exotic structures occurring in different areas of mathematics. The article beautifully explains a lot of the intricate story of how octonions connect up surprising phenomena in algebra, geometry, group theory and topology.

If you're a mathematical physics mystic like myself, you're susceptible to the belief that anything this mathematically deep, showing up in seemingly unrelated places, must somehow have something to do with physics. The story of octonions is closely related to the story of Clifford algebras, which are definitely a crucial part of physics, but it seems to me we're still a long ways from truly understanding the role in physics of Clifford algebras, much less the more esoteric octonions. One thing that is fairly well understood is that the sequence of division algebras explains some of the structure of low-dimensional spin groups in Minkowski signature, through the isomorphisms:
SL(2,**R**)=Spin(2,1)
SL(2,**C**)=Spin(3,1)
SL(2,**H**)=Spin(5,1)
The octonion story is supposed to be the next in line, involving Spin(9,1), but made much trickier by the fact that SL(2,**O**) doesn't really exist, since the octonions are non-associative.

Back in 1982, a very nice paper by Kugo and Townsend, [Supersymmetry and the Division Algebras](#), explained some of this, ending up with some comments on the relation of octonions to d=10 super Yang-Mills and d=11 super-gravity. Baez and Huerta in 2009 wrote the very clear [Division Algebras and Supersymmetry I](#), which explains how the existence of supersymmetry relies on algebraic identities that follow from the existence of the division algebras. Kugo-Townsend don't mention string theory at all, and Baez-Huerta refers to superstrings just in passing, only really discussing supersymmetric QFT. There's also [Division Algebras and Supersymmetry II](#) by Baez and Huerta from last year, with intriguing speculation about Lie n-algebras and what these might have to do with relations between octonions and 10 and 11 dimensional supergravity. For a nice expository paper about this stuff, see their [An Invitation to Higher Gauge Theory](#).

In contrast to the tenuous or highly-speculative connections to string theory that appear in these sources, the Scientific American article engages in the all-too-familiar hype pattern. The headline argument is that octonions are important and interesting because they're "The Strangest Numbers in String Theory", even though they play only a minor role in the subject. It wouldn't surprise me at all if octonions someday do end up playing an important role in a unified theory, but the rather obscure connection to the calculation of the critical dimension of the superstring that seems to be the main point of the Scientific American article isn't a very convincing argument for such a role.

Somehow I suspect that those string theorists who were upset by Scientific American's decision to publish speculation by Garrett Lisi about E8 and [wrote in to complain](#), won't be similarly upset to find this highly speculative material about the octonions appearing in the magazine.

Comments

1. **Eric Weinstein**
   April 28, 2011

   Hi Peter,

   I don't think it is sporting to say that octonions have played only a minor role in string theory. Any area where maximal supersymmetry, large exceptional Lie-Groups (F_4 and E_6-8), triality, G_2, Exceptional Jordan algebra or a host of other constructs have cropped up is, in essence, an octonionic branch of mathematical or physical theory. Even Bott periodicity uses the octonions (in disguise) to cycle through the other three division algebras.

   While hardly a supporter of the string triumphalist movement, I think that string theorists have been at their best in calling the attention of the mathematics and traditional physics communities to the possibility of integrating octonionic mathematics with core geometric and physical theory.

   Octonionic mathematics used to be on the 'down low' but has now made the leap out of the closet and into mainstream research.

   So: Two cheers for string theory!

   Join me, won't you?

   Eric

2. **Peter Woit**
   April 28, 2011

   Hi Eric,

   It's kind of like with E8: I've got mixed feelings about all these exotic structures, feeling I can't be sure what's a beautiful, fundamental structure, and what's a complex piece of mathematical junk that managed to just barely hold on to some interesting structure for kind of random reasons. The string theorists also did a great publicity job for E8, although they seem to have lost interest in it.

   At least with Garrett's SciAm article on E8, you didn't have to put up with a sales job implying string theory was the reason to take an interest in the subject.

   I really did love John's expository article on Octonions. It lays out beautifully the attractive part of the mathematics and how it hangs together. If that could be connected to physics, I'd be interested, but the only connection advertised in the SciAm piece is the rather obscure one about getting the supersymmetry algebra to work out a certain way in a certain dimension. It's not even clear to me that this is what explains why 10 is the critical dimension, which is the claim repeatedly hammered home in the article.

3. **King Ray**
   April 28, 2011

Corinne Manogue, Tevian Dray, and others wrote some very illuminating papers where they spell out the properties of octonions and postulate about their applications to physics:

http://arxiv.org/find/all/1/all:+AND+manogue+octonion/0/1/0/all/0/1

The one below by Manogue and Schray was especially enlightening. They even define structure matrices for the octonions in Appendix A:

http://arxiv.org/abs/hep-th/9302044

4. **Thomas Larsson**
April 28, 2011

I never understood the big deal with octonions. Sure, it is the last division algebra, but if you relax your axioms a little the Cayley-Dickson construction gives an infinite tower of increasingly uninteresting algebras:
n=1: Reals.
n=2: Complex numbers.
n=4: Quaternions, not commutative.
n=8: Octonions, not associative.
n=16: Sedenions, not alternative but power associative.
n=32: 32-ions?
…
I can see why you need to give up commutativity – things can be done in different order – but why is the division algebra property important? There might be a reason that octonions have not made it into physics in 150 years.

5. **John Baez**
April 28, 2011

Peter wrote:

> The octonion story is supposed to be the next in line, involving Spin(9,1), but made much trickier by the fact that SL(2,O) doesn't really exist, since the octonions are non-associative.

PSL(2,O) does exist: you just have to be careful about it. The octonions aren't associative, but the subalgebra generated by any two octonions is. So, you just need to avoid recklessly multiplying lots of numbers when you don't really need to.

You can define the octonionic projective line and the group PSL(2,O) acting on it. You can even go ahead and define the octonionic projective plane and PSL(3,O), which turns out to be a certain real form of E6. But then the show stops: to define PSL(4,O) we'd really need the associative law. It's fun to see how one "hits the wall" here.

This is explained in my octonions paper, though I could make it all much clearer now.

> but the only connection advertised in the SciAm piece is the rather obscure one about getting the supersymmetry algebra to work out a certain way in a certain dimension.

It's not at all "obscure" — within the narrow confines of string theory at least — that classical superstring Lagrangians rely for their supersymmetry on a magical identity that holds only in spacetimes of dimensions 3, 4, 6 and 10. It's explained in Green, Schwarz and Witten's textbook, for example.

And it's been known for quite some time that these special dimensions are 2 more than the dimensions of the reals, complexes, quaternions and octonions. And in fact this is no coincidence: the existence of these number systems is the simplest explanation of what's going on here.

So, don't try to make it sound like an obscure yawn-inducing technicality about "some supersymmetry algebra working out a certain way in a certain dimension". It's a shocking and bizarre fact, which hits you in the face as soon as you start trying to learn about superstrings. It's a fact that I'd been curious about for years. So when John Huerta finally made it really clear, it seemed worth explaining — in detail in some math papers, and in a popularized way in *Scientific American*.

But of course, none of this has anything to do with whether superstring theory is *right* as a theory of physics. The article says quite clearly that superstring theory makes no testable predictions:

> At this point we should emphasize that string theory and M-theory have as of yet made no experimentally testable predictions. They are beautiful dreams — but so far only dreams. The universe we live in does not look 10- or 11-dimensional, and we have not seen any symmetry between matter and force particles. David Gross, one of the world's leading experts on string theory, currently puts the odds of seeing some evidence for supersymmetry at CERN's Large Hadron Collider at 50 percent. Skeptics say they are much less. Only time will tell.

I thought you'd quote that part. Oh well.

By way, I just won a case of scotch from Dave Ring: I'd bet him that the LHC wouldn't discover "strong evidence for supersymmetry" in its first year of operation.

> It's not even clear to me that this is what explains why 10 is the critical dimension, which is the claim repeatedly hammered home in the article.

I don't think the octonions explain why 10 dimensions is the critical dimension – at least, not now. It would be cool if they did. But so far, all I know is that classical superstrings favor dimensions 3, 4, 6 and 10, thanks to the four normed division algebras. Other considerations pick out the 10-dimensional case, which happens to be the octonionic one.

It's a bit odd to say we "repeatedly hammer home" something about the critical dimension – that was certainly not our intention, and we never even use the phrase "critical dimension". But maybe I can guess how you'd get that impression. It's probably worth comparing what John Huerta and I originally wrote, to what came out in the final version.

We originally wrote:

> For strings, when the number of extra directions is 1, 2, 4, or 8, we get supersymmetry. Why? Because then its vibrations can be described using numbers in a division algebra. But the total number of dimensions of space and time is 2 more than the number of extra dimensions. So, we get supersymmetry when the total number of dimensions is 3, 4, 6, or 10. One of these dimensions is time; the rest are space.
>
> Curiously, when we fully take quantum mechanics into account, it appears that only the 10-dimensional theory is consistent. This is the theory that uses octonions. So, if string theory is right, the octonions are not a useless curiosity: on the contrary, they play a fundamental role in understanding spacetime, matter, and the forces of nature!

Here it's pretty clear, I hope, that quantum considerations pick out the 10-dimensional theory—not some special fact about octonions. The "curiously" makes it clear that we don't understand the connection to the octonions.

The final version says:

> At any moment in time a string is a one-dimensional thing, like a curve or line. But this string traces out a two-dimensional surface as time passes. This evolution changes the dimensions in which supersymmetry naturally arises, by adding two — one for the string and one for time. Instead of supersymmetry in dimension one, two, four or eight, we get supersymmetry in dimension three, four, six or 10.
>
> Coincidentally string theorists have for years been saying that only 10-dimensional versions of the theory are self-consistent. The rest suffer from glitches called anomalies, where computing the same thing in two different ways gives different answers. In anything other than 10 dimensions, string theory breaks down. But 10-dimensional string theory is, as we have just seen, the version of the theory that uses octonions. So if string theory is right, the octonions are not a useless curiosity: on the contrary, they provide the deep reason why the universe must have 10 dimensions: in 10 dimensions, matter and force particles are embodied in the same type of numbers—the octonions.

This "provide the deep reason why the universe must have 10 dimensions" makes it sound as if some special fact about octonions explains why the universe has 10 dimensions. So yeah, that's bad. But if you read the beginning of the paragraph you'll see that's *not* what's going on: it's quantum considerations that pick out the number 10. And the "coincidentally" makes it clear that we don't understand the connection.

If you saw how much editing and counter-editing were involved in, perhaps you'll forgive us for letting that phrase slip past. I also don't like the title, but I couldn't think of a better one. I had to admit that our original proposed title, "The Octonions", would to most readers be about as appealing as "The Metaphraxis" or "The Sexadent".

Of course, it would be cool if we could find a link between the calculation that singles out the number 10 and the fact that the normed division algebras give an identity that lets you write down supersymmetric string theory Lagrangians in dimensions 3, 4, 6 and 10. It's hard to find real "coincidence" of this magnitude in math. So, I suspect it's just a matter of time before someone finds a deeper link. I've made some nice progress on this but not enough to talk about.

By the way, I have permission to put the Sci Am article on my website about a month after it comes out, and I'll also put up the various drafts, just for the amusement of people who wonder how this sort of editing process works.

6. **John Baez**
   April 29, 2011

   Thomas wrote:

   > … but why is the division algebra property important?

   The division algebra property is not important. What's important are two things.

   First, you get a *normed* division algebra in dimension n if and only if you can find an n-dimensional spinor representation of Spin(n). This starts the interplay between spinors and vectors, which provides the special features of Lorentzian geometry in dimension n+2: namely, dimensions 3, 4, 6 and 10.

   Second, a normed division algebra is *alternative*. The alternative law gives the spinor identity that makes supersymmetry work for super-Yang-Mills theory and classical superstrings in dimensions 3, 4, 6 and 10. It also gives the Jacobi identity for the exceptional Lie algebras F4, E6, E7 and E8 — which *contain* the Lorentz Lie algebras for dimensions 3, 4, 6 and 10. It also gives a 3-cocycle on the superPoincare groups in these special dimensions, which let John Huerta build 'categorified' Lie supergroups relevant to describing superstring theory as a higher gauge theory.

   So, a lot of math interacts in a marvelous way when you have a normed division algebra.

   None of this stuff works anymore when we move further up the Cayley-Dickson tower. I bet something interesting *does* work, and I'd love to know what it is, but I don't know and nobody seems to be working on it.

   Needless to say, I'm not talking about whether any of this stuff is useful for physics. I'm talking about math.

7. **John Baez**
   April 29, 2011

   By the way, John Huerta is not my "ex-student". His thesis defense is Wednesday May 18th and he should be working on his thesis *right now* — not wasting his time reading blogs.

8. **also anon**
   April 29, 2011

   @baez

   you wrote "The universe we live in does not look 10- or 11-dimensional"

   so what do octonions tell us about 11 and the M?

9. **kneemo**
   April 29, 2011

   The split-octonions have already been used by Ferrara and Duff et al. in the description of black hole and string charge vectors in toroidally compactified M-theory (N=8 supergravity). See [arXiv:1002.4223 [hep-th]](arXiv:1002.4223)

10. **Peter Woit**
    April 29, 2011

    John,

    Thanks a lot for the detailed explanations. I should have made clear that this is a quite different case than the usual editions of "This Week's Hype", which just about always involve a claim that "we've found a way to test string theory". Thankfully, there's nothing at all like that here, and the material in the article about the relationship of string theory/supersymmetry to the real world was accurate.

11. **neo**

April 29, 2011

I read the article and enjoyed it. From the SA headlines, it sounded hypey, but the article itself was not. I am guessing Baez and Huerta don't control how SA chooses to headline the story, which of course will be done in a way that catches the most attention.

12. **chris bolger**
April 29, 2011

I have done some reading of octonions, and I read John's paper on it. It is very good, but technical to me. From what I understand about octonions is that they are anti-associative for three oconions that are different (excluding a real scaling factor). Since, the ORDER of the octonions determines the value, has anyone tried to make a consistent theory of spacetime using octonions? The order would determine different possible outcomes, but nature would only choose one. Also since the order does determine output, this gives a before and after quality if we build a length of time by successive octonion multiplication.

13. **John Baez**
April 29, 2011

also anon wrote:

> so what do octonions tell us about 11 and the M?

Maybe you can try my blog article here… it's a bit technical, but a lot less technical than our actual paper.

But here's something really quick: the real numbers, complex numbers, quaternions and octonions don't just let you write down the (classical) theories of supersymmetric strings in 3, 4, 6, and 10 – they also let you write down (classical) theories of supersymmetric 2-branes in dimensions 4, 6, 7 and 11! And the 11-dimensional octonionic case is believed to be relevant to "M-theory", whatever that is.

Once you hear this, you should wonder about 3-branes in dimensions 5, 7, 8 and 12. But if you look at the old "brane scan" picture on that blog entry I'm pointing you to, you'll see it doesn't quite work as smoothly as that. In particular, John Huerta has done a bunch of calculations showing that the trick relating octonions to strings in 10 dimensions and 2-branes in 11 dimensions does not continue to work one dimension higher. Apparently the nonassociativity messes things up!

(It's long been known by string theorists that something "ends" at dimension 11. However, we are focusing on a limited portion of the math, not the whole story physicists are interested in. So, the calculations John Huerta did may be new, or at least a little different than the usual story.)

By the way, John Huerta has gotten a postdoc position with Peter Bouwknegt at Australian National University in Canberra. That's 'close' to where I'm working here in Singapore – meaning, only about a 12-hour flight. (In fact Australia isn't close to anything.) So, I hope to continue doing a bit of work with him on octonionic puzzles. There are not many people with a good intuition for the octonions, and he's one.

14. **Casey Leedom**
April 30, 2011

Okay, I know this is trite (and liable to get deleted) but I haven't found the answer after a bit of survey. So how is "octonions" pronounced? Is it "Oc-'tone-ions" or is it "'oct-'ung-ions"? (As in the In-and-Out: do you want a _lot_ of onions with that?)

15. **John Baez**
April 30, 2011

Everyone I know says "Oc-'tone-ions". I'm assuming that little accent thing means the second syllable is accented – that's what everyone does.

This is like "quaternions", which also has the second syllable accented.

But the really evil people, who need to be brutally mocked, are the ones who write "octonian", often in the same sentence as "quaternion". I have no idea why.

16. **Giotis**
April 30, 2011

Peter wrote:

"The string theorists also did a great publicity job for E8, although they seem to have lost interest in it."

This has nothing to do with the E8 per se as a mathematical structure. They were forced to focus on Heterotic E8 since at that time it was the only road connecting String theory to phenomenology. With the advent of D-branes and flux compactifications though they were able to construct realistic string vacua in IIB/F-theory with all the moduli stabilized and even to connect these models to cosmology i.e. dS vacua (via KKLT), or warped D-Brane inflation (via KKLMMT) etc. On the other hand Heterotic has some know problems with respect to moduli stabilization and wasn't able to compete with IIB/F-theory on all these fronts of phenomenology.

Of course this doesn't really matter since as is well know all these theories are connected via dualities. There is only one String theory.

17. **someone**
May 1, 2011

just heard from colleagues that Quillen is no more.

18. **Thomas**
May 3, 2011

(Posted here because answering is disabled in: http://www.math.columbia.edu/~woit/wordpress/?p=942)
H. Hironaka speaks about news on resolution of singularities in positive characteristics http://www.mpim-bonn.mpg.de/node/3357 in ca. two weeks.

19. **Zathras**
May 3, 2011

"If you're a mathematical physics mystic like myself, you're susceptible to the belief that anything this mathematically deep, showing up in seemingly unrelated places, must somehow have something to do with physics."

This point of view is absolutely pervasive with particle/GUT theorists right now. There is an absolute disdain for non-rigorous mathematics in theory. The problem is that non-rigorous mathematics has often been the tool to advance theory in the first instance. Newton's theories depended on a very non-rigorous theory of calculus. Advancements in quantum mechanics were made for decades before renormalization could be rigorously handled. Feynman's initial methods for QED were an absolute mess of ad hoc rules and tricks. Only later were the rules formalized.

To make better theory, we don't need better mathematics. We need better Physics. Sort out the math later.

20. **Peter Woit**
May 3, 2011

Zathras,

This has nothing to do with rigorous vs. non-rigorous. The theories being studied by particle/GUT theorists are not rigorously well-defined, and there's nothing wrong with that.

Many physicists believe that whatever mathematicians are working on, it's irrelevant to them: they just need the right "physical idea", with the only math needed maybe something like PDEs. Newton was not so foolish. He was at the cutting edge of developing both new mathematics and new physics, well aware that the kind of new ideas about mathematics being developed by Leibniz and others were going to be required to express the new ideas about physics that he was developing. If he had taken the attitude that "all these new-fangled ideas about math can't be needed for physics", surely the dynamics of particles can be expressed using basic algebra and the theory of functions (e.g. today's high school pre-calculus math), he would not have gotten far.

The problem with what has come out of string theory is not that it is based on too much abstract mathematics, but that it is based on a physical idea that turned out to be wrong.

21. **Zathras**
May 3, 2011

"The problem with what has come out of string theory is not that it is based on too much abstract mathematics, but that it is based on a physical idea that turned out to be wrong."

No, that *was* the problem with string theory. The problem *now* is that the people doing string theory are so raptured with the beauty of the mathematics in string theory, that they think the theory must be correct. Which brings us back to your quote. People have stopped looking for other physical analogies because they think their rigorous math has to be correct.

22. **Peter Woit**
May 3, 2011

Zathras,

String theory is not based on rigorous mathematics. No one has a consistent definition of the theory, even at a non-rigorous level. What people won't give up on is not a piece of rigorous mathematics, but a physical idea: space-time has 10/11 dimensions and strings/M-theory branes unify particle physics and gravity.

Some interesting mathematics has come out of string theory (e.g. mirror symmetry), but this happened not by physicists coming up with rigorous mathematics, but by physicists coming up with non-rigorous, conjectural ideas about mathematics, which mathematicians then used as inspiration to come up with rigorous versions (of small parts of the original ideas).

23. **younghun park**
May 3, 2011

Peter
Very good! I agree to your thought. I think you are right.
As you said, many physicists and mathematicians won't give up string theory right now because that gives the inspiration to them, especially mathematicians.

I think you too don't give up the string theory as mathematician
because that you and your people in mathematics can obtain the conjectural ideas from string theory.

Even if string theory is not good physics, it is good for mathematicians to long for getting conjectural ideas.

24. **Frank**
May 7, 2011

@Peter: To me it seems increasingly likely that string theory is the physics equivalent of what in markets we call a "speculative episode". I think theoretical physics has suffered from badly exagerated expected returns on investment of time in string theory. And as usual those who are stuck in the investment till their necks will not be the first ones to blow the whistle that a market correction is coming. But I think for the rest of us … we might start to think about how we're going to bail our colleagues out …

This Week’s Leak

May 4, 2011
Categories: Experimental HEP News

Recently there was a bit of a kerfuffle triggered by someone leaking here the abstract of an internal ATLAS document claiming to have found a Higgs signal as a bump in the gamma-gamma invariant mass distribution. After some initial discussion of this, I wrote:

> Best guess seems to be that this is either a hoax, or something that will disappear on further analysis.

It quickly became clear this was not a hoax, but now there’s a new leak, this one from CMS to New Scientist, which indicates that “disappear on further analysis” is where this is going:

> Now physicists working on the LHC’s other main detector, CMS, have come up empty in an initial search for a similar bump in their data, according to a document shown to New Scientist. So ATLAS’s bump may not be due to Higgs particles, after all, but instead down to something mundane, such as an error in the analysis.
>
> The internal CMS document has not been released to the public, so the result is still preliminary, as was the news of the original ATLAS bump, for that matter, which was leaked before it was reviewed or endorsed by the ATLAS collaboration.

Well, maybe first news of the Higgs won’t show up on a blog, but at a more standard journalistic outlet…

Update: Curiouser and curiouser. It seems that there are questions about the existence of the supposed CMS document leaked to New Scientist. In other rumors floating around, while there may not be such a CMS document shooting down this signal, there really is an ATLAS one, soon to see the light of day. In any case, there are no rumors I’m aware of that there’s any confirmation of the original signal.

Comments

1. **tulpoeid**
   May 4, 2011

   Ahem, http://resonaances.blogspot.com/2011/04/atlas-shrugged.html

2. **Peter Woit**
   May 4, 2011

   tulpoeid,

   Yes, but that was just a rumor, this is a leak…

3. **Mike**
   May 4, 2011

   Peter,

   The link in the update you’ve added “This Week’s Rumour” does not work

4. **Peter Woit**
   May 4, 2011

   Thanks Mike, fixed.

   That was a link to the Resonaances posting mentioned here by tulpoeid.

5. **tulpoeid**
   May 5, 2011

   Peter:
   Oh ok, to be honest I didn’t realize at first they claimed to have seen an official document. Now that it’s clear, I still believe it’s a rumor.

6. **Bernhard**
   May 5, 2011

   It seems nobody in CMS is willing to comment on this. I think it’s likely that either the journalist made it up or what he claims to be a “document” is not even a CMS memo, just a piece of paper that someone in CMS have showed to him, (if one wants to believe he is not being dishonest).

7. **DB**
   May 5, 2011

   Dorigo doesn’t pull any punches. He states: “I think that the New Scientist reporter made this up. Yep, that’s what I think, and I challenge him to prove otherwise.”

   As Dorigo is a member of CMS, that carries some weight.

8. **lun**
   May 5, 2011

   There is a very nice blog-post about this whole matter, and physics leaks in general. Unfortunately, it is in Italian, but perhaps google translate does an acceptable job:

   http://www.borborigmi.org/2011/04/27/integrita-scientifica/

9. **rummy**
   May 5, 2011

This is all really a pathetic display of how everybody and his pet dog has to report rumours and counter-rumours or else get left out of the feeding frenzy. Honest scientists painstakingly working on a difficult analysis cannot do their work properly without everyone peering over their shoulders and gossiping.

Like me.

10. **Peter Woit**
May 5, 2011

rummy,

Consider the feeding frenzy just as evidence that people are very interested in what you're doing, so it must be worthwhile. Much of the world goes to work every day and does difficult, painstaking things that no one cares about. So, enjoy it while it lasts….

11. **Shantanu**
May 5, 2011

Peter something else to blog about is the COGENT result present at the STSCI symposium (which I am watching live now) and it will soon be archived on the web. what do you and others think?

12. **Peter Woit**
May 5, 2011

Shantanu,

My impression is that the COGENT results are just marginally statistically significant, and possibly inconsistent with CDMS/XENON100, so not worth getting excited about. But I'm not a dark matter expert, so to get an informed opinion about this, best to go elsewhere. The people at Science magazine just told me about a a program by Adrian Cho talking to dark matter experts this afternoon, which might be interesting, see here:

http://news.sciencemag.org/sciencenow/2011/04/live-chat-unraveling-the-mysteri.html?ref=topst

13. **nessuno**
May 6, 2011

Could it be that the CMS document is too atrocious to be made public?

14. **chris**
May 6, 2011

Peter, I think rummy indicated that he is gosipping, not doing the hard work 🙂

15. **Bernhard**
May 6, 2011

In the meantime, Fermilab is willing to die fighting:

http://www.voanews.com/english/news/science-technology/At-FermiLab-end-of-Search-for-God-Particle-Nears-121350499.html

"As we look at these huge data sets that we've acquired over the 10 years, we're now putting out things that we've learned about that data," he said. "And so what you're seeing here is evidence for perhaps a new particle and there will be other things that will come out over the coming months that will be just as interesting as this."

I´m puzzled what evidence Roser is refering to (perhaps the 3-sigma CDF paper?).

16. **Valerie Jamieson**
May 6, 2011

New Scientist was sent a 46-page powerpoint presentation by the CMS Higgs group outlining the search for a Higgs to gamma gamma decay that was conducted 21-27 April.
Valerie Jamieson, New Scientist deputy features editor

17. **Bernhard**
May 6, 2011

Meaning the "document" was indeed NOT even a CMS memo, just an internal presentation, not reviewed nor endorsed by CMS, since we can assume this was not a public presentation. But OK, a genuine leak then.

18. **Valerie Jamieson**
May 6, 2011

I should clarify my comment above. The presentation by the CMS Higgs group was sent anonymously to New Scientist, and not by the CMS Higgs group.

19. **tommaso**
May 6, 2011

By writing
"The internal CMS document has not been released to the public"
in their piece, NS made it look like it was a paper draft. If these are powerpoint slides the matter is different. Although their stating in more detail what they were sent does not prove anything, I will admit that it makes them more credible, especially since the author is now backed up by the editor.

In the end, I am only concerned that now nobody will take my $1000 bet, alas.

Cheers,
T.

Not a Leak

May 8, 2011
Categories: Experimental HEP News

ATLAS this weekend has finally released their latest analysis of the gamma-gamma invariant mass spectrum, carried out in response to claims from within their collaboration that a 4 sigma Higgs signal had been observed in this channel. The result? Nada:

> The dominant background components are measured and found to be in agreement with the Standard Model predictions, both in terms of overall yield and invariant mass distribution. No excess is observed.

Comments

1. **ruomur**
   May 8, 2011

   Oh dear. What would happen if it took > 1y to do the full analysis to say the rumour was unfounded and there was no excess beyond SM? There would be a dozen published PRL, all the way from MSSM to 'rigorous evidence of the existence of the Multiverse'. It's a nuthouse.

This Week's Hype

May 8, 2011
Categories: This Week's Hype

Philip Gibbs points to an impressive piece of string theory hype from British Channel 4 news.

If you watch the clip, you get the latest news about string theory and the LHC: people were getting discouraged about string theory, but now some of its predictions are being confirmed by the LHC. For the extra dimensions to appear, we may have to wait a couple years for when the machine runs at design energy.

Not clear at all where they got this nonsense from.

Update: The source for this seems to be a story by Jonathan Leake in The Sunday Times, entitled Stand by, we may soon enter a new universe (subscription required, but a syndicated version is freely available here). The story has David Evans of Alice trying to promote his experiment with:

> The Alice experiment may soon be able to make experimental measurements which, for the first time, can be modelled using the techniques of string theory.
>
> Although the experimental results will not prove string theory to be correct, an accurate prediction would certainly show that the techniques work, could distinguish between different versions of the theory, and perhaps even show whether the theory is going in the right direction.

Given this kind of quote, one can see why the writer completely mixes up string theory unification and string theory as approximate calculational method in heavy-ion physics:

> The researchers, at Cern, the European centre for particle physics near Geneva, say results from the Large Hadron Collider suggest it could offer the first experimental test for some aspects of string theory.
>
> Formulated in the 1960s, this theory attempts to describe how all the fundamental forces of nature, such as gravity and electromagnetism, interact with matter.
>
> On paper, the theory has been highly successful, resolving many mathematical problems.
>
> In practice, however, there is no experimental evidence to support its predictions, including the idea that there could be as many as 11 dimensions – the three physical dimensions, time and seven others as yet undiscovered.
>
> At Cern, there are now hopes the LHC may be able to break this impasse.

Then, as usual, the headline writer takes things a step further:

> SCIENTISTS have devised the first experiment capable of giving insight into one of the universe's greatest mysteries: could there be more dimensions than we know about?

So, out-of-control promotional efforts for ALICE are at the bottom of this one.

Comments

1. **ch4qgp**
   May 8, 2011

   Umm … it wasn't as bad as all that, really. There was a reference to expt data at the ALICE detector of collisions of lead ions and the quark-gluon plasma. But others have been talking about 'applications of string theory' (or AdS-CFT?) to the QGP for a while. There was a clear statement that string theory has been worked on for years and has made no predictions at all, and this has prompted some scientists (hmm …?) to claim that string theory does not work. There was a statement by the interviewed physicist that 'if a theory cannot predict anything then it's not really physics, it's … (shrug) … philosophy' (gasp!!). There was quite a bit of 'data from the LHC may be on to something' with regard to string theory. It did say towards the end that when the LHC cranks up to top energy then perhaps some evidence for the extra dimensions may be observed (didn't say what that evidence would be). However, it is indeed not clear why Ch4 would choose to run such a news item now. There was no statement of any new or recent discovery. ALICE/QGP has been around for a while. It may be (speculating here!) a fallout from the ATLAS-CMS rumours that 'something is afoot at CERN, with the LHC'.

2. **Chris Oakley**
   May 9, 2011

   A theory that cannot even get the number of spacetime dimensions right is not generally in good shape, and consequently the only reason for mentioning String Theory when reporting on a scientific experiment is as a sociological phenomenon amongst theorists. The public will eventually realise that they have been misled and what worries me is that then *all* theorists – not just the String partisan who advised on this piece – will be tarred with the "bullshitter" brush.

3. **DB**
   May 9, 2011

   Probably the source is the most vocal, and prominent of string theory hypesters, your colleague, Brian Greene. In a recent interview (Jan 24) on NPR he gushes:

   "You almost can't avoid having some version of the multiverse in your studies if you push deeply enough in the mathematical descriptions of the physical universe," he says. "There are many of us thinking of one version of parallel universe theory or another. If it's all a lot of nonsense, then it's a lot of wasted effort going into this far-out idea. But if this idea is correct, it is a fantastic upheaval in our understanding."

   "There are a couple of multiverses that come out of our study of string theory," Greene says. "Within string theory, the strings that we're talking about are not the only entities that this theory allows. It also allows objects that look like large flying carpets, or membranes, which are two dimensional surfaces. And what that means, within string theory, is that we may be living on one of those gigantic surfaces, and there can be other surfaces floating out there in space."

   "If we are living on one of these giant membranes, then the following can happen: When you slam particles together — which is what happens at the LHC — some debris from those collisions can be ejected off of our membrane and be ejected into the greater cosmos in which our membrane floats," he says. "If that happens, that debris will take away some energy. So if we measure the amount of energy just before the protons collide and compare it with the amount of energy just after they collide, if there's a little less after — and it's less in just the right way — it would indicate that some had flown off, indicating that this membrane picture is correct."

4. **Peter Woit**

May 9, 2011

DB,

Brian has recently been the source of a lot of multiverse hype, but this is a somewhat different category. I've identified the source here, see update to posting, and it's not Brian.

5. **neo**
May 9, 2011

You should have titled this as "This Week's Leake".

6. **Bernhard**
May 9, 2011

Hi Peter,

I remember you excused experimentalists some time ago for not engaging in the string, multiverse and likes nonsense, but clearly this is not the case anymore. Experimentalists too, talk all kinds of nonsense to get attention. Just depressing. This is much worse than any false rumor.

7. **DGP**
May 10, 2011

I doubt if you have identified the source correctly. There just doesn't seem to be time for one reporter (I think that Channel 4 have precisely one science reporter) to have produced a 2 minute magazine article from scratch, even though other journalists probably saw the Sunday Times at 23:00 on Saturday.

It's more likely that this was picked up from the CERN Courier piece on May 3rd which probably had a parallel Press Release (or David Evans sent out his own Press Release, which seems likely if he is a bit of a self-publicist).

My media advisor says that the lead-in (the voice-over on the stock LHC film) was probably written by a sub-editor to link to previous LHC coverage, so it would mention things said previously. Since the public preception is that LHC is meant to find "Higgs" and "Strings" it's not surprising it was mainly about strings.

Overall, I thought it was fairly constructive. Clearly, some experimentalists think that string theories are now testable to some extent and Evans was clear about the priority of experiment over speculation in physics (although he offended my wife by implying that String Theory would be philosophy if it wasn't verified: she is a philosopher).

I chance to recollect that one of the more fully worked out string theories makes some predictions for the QGP which I think this experiment may well be at variance with.

So it looks like "Game On" for the experimentalists. If my recollection of being a theorist amongst experimentalists is accurate, many experimentalists will take great pleasure in kicking down any theoretical edifice. Looking in Particle Data Tables "Cross Sections" suggests that some people have been working hard at trying to extract testable predicitons from a variety of String theories.

Happy Days! I wish I was there.

8. **Bernhard**
May 10, 2011

DGP,

"Clearly, some experimentalists think that string theories are now testable to some extent and Evans was clear about the priority of experiment over speculation in physics"

If there are experimentalists thinking that, too bad for them. There is no such thing as testing String Theory at all.

9. **Peter Woit**
May 10, 2011

DGP,

The "public perception" that the LHC is meant to find "Strings" is the problem, since this is simply not true. Claims that heavy-ion physics experiments at the LHC will "test string theory" are heavily over-blown, and part of an effort to mislead the public by generating exactly this sort of completely false story in the press. The supposed application of AdS/CFT approximation methods to certain phenomena in heavy-ion physics has nothing at all to do with string theory unification. The physicists involved here are well aware of this, but happily help generate false news stories like this one.

It's too much of a waste of time to keep rewriting this same point every time a new bogus press story like this one appears. For some idea of the scale of the problem, click on my blog category "This Week's Hype" to get an idea of how long and intensively this dishonest campaign has been going on.

10. **Giotis**
May 10, 2011

"The supposed application of AdS/CFT approximation methods to certain phenomena in heavy-ion physics has nothing at all to do with string theory unification"

More importand ST is a theory of Quantum Gravity and AdS/CFT via ST is closely related to Quantum Gravity.

So AdS/CFT and it's verification is very important for ST as a theory of QG.

QGP is just another application…

11. **qpg**
May 10, 2011

"QGP is just another application…" — how many other applications are there?

It appears that this news item is an attempt by ALICE to gain a share of the spotlight. Rumours have circulated that ATLAS has "seen something" (and CMS is checking its data to confirm) and every false lead gets reported or blogged to fever pitch (yes?). And (horror!) if the "thing" should prove to be true, then ATLAS/CMS/CERN will grab ALL of the headlines. Possibly also all of the LHC beam time. Alice will traipse in Wonderland totally unnoticed. So better to call some attention to oneself. And so to string theory … who in the taxpaying public cares about turning lead ions into pancakes? It appears now that the current rumour from ATLAS is not true … there will be others.

12. **Bernhard**
May 10, 2011

Giotis,

the point is if AdS/CFT cannot make ST explain anything for its own, it´s a test of equivalence. It´s like if Einstein came with the theory of relativity and said that the only experimental test he has is that in particular cases his theory is equivalent to Newton´s… This is the same thing, so NOT a experimental test. A test would be something that QFT cannot make it and ST can but this is something by definition AdS/CFT will never be able to make it. So, the story so far is the same.

13. **Peter Woit**
May 10, 2011

Giotis,

To supplement Bernhard's comment:

We are pretty sure we know what the theory is that governs heavy-ion physics, QCD. Claims that ALICE is "testing" some theory aren't about testing the underlying theory, but about testing whether some string theory-AdS/CFT inspired model reproduces QCD approximately in some specific situation or not. If ALICE doesn't find agreement with the predictions of a particular string theory-AdS/CFT based model, it doesn't mean QCD is wrong, it doesn't mean string theory is wrong, and it doesn't even mean the AdS/CFT correspondence is wrong. This is not a "test of string theory". All it means is that, for the specific phenomenon checked, the model turns out to be useful (to some approximation), or not.

Claiming that you can get from this situation to using the LHC to test the idea of 11 dimensions and string theory unification, which has nothing to do with any of the above, is just absurd.

14. **Mike Reeves**
May 10, 2011

Why all of the hush, hush. I would think they would post everything there learning and attempting real time! It not like anyone else has the equipment to outdo or discover something first. Science is for all mankind.

15. **Peter Woit**
May 10, 2011

Mike Reeves,

I think you're referring to an earlier posting. But one thing to keep in mind is that there is a competition here. ATLAS is only one of two big general purpose detectors, very much in competition with the other one. One reason for keeping their internal documents out of the public eye is to keep their competitors from reading them. This competition is scientifically valuable since it means that they are under pressure both to get a result as soon as they can, AND to get it right.

16. **chris**
May 11, 2011

Mike,

all of the LHC experiments and all accelerator based experiments are under extreme pressure to discover *something*. because if they don't, LHC is probably the last of its kind and even running it to the end will be questionable.

it has become a multi-billion $ business and that is generally not healthy.

17. **Giotis**
May 11, 2011

Peter and Bernhard,

Nobody talks about testing string theory in the strict sense.

People are talking about circumstantial evidence from different areas of ST which in time could pile up and boost our confidence to the general framework.

I think the article has been written in this spirit. You could tell this from the title which is rather conservative.

I quote: 'SCIENTISTS have devised the first experiment capable of giving insight…'

This is quite an honest statement I think especially for a headline .

18. **Bernhard**
May 11, 2011

Giotis,

You remind me of Polchinski defending ST by saying that "String theorists have a strong sense that they are discovering something, not inventing it".

I'm sorry to say I am very much not convinced by string theorists feelings, nor do I agree with you that one gets ANY insight that there could be 11 dimensions because of a particular string theory-AdS/CFT based model might agree with QCD. I think there is no logical conclusion one can draw from this.

On the other hand, if you get such an insight, whatever you mean by that, than I guess we could spread the news that if this "test" fails you will get the insight that there is no such thing as 11 dimensions, right?

19. **Peter Woit**
May 11, 2011

chris,

It’s true that the LHC experiments are under a lot of pressure to discover something new. It’s also true that they’re under just as much pressure to not make a mistake, with the competition between detectors ensuring that possible mistakes will be vigorously pursued by someone with a huge interest in showing them wrong.

20. **Peter Woit**
May 11, 2011

It is true though that the pressures to find funding for these experiments might explain why the string theory hype campaigns sometimes conducted on their behalf.

21. **srp**
May 11, 2011

I heard one of the CERN management guys say that he hopes they find the Higgs, because he promised the politicians that they would. But it seems that not finding it would be bigger news–kind of a null Michelson-Morley-type result that would blow up everything. So I don’t think they should worry too much about anything but inconclusive findings.

Pricey Strings

May 11, 2011
Categories: Uncategorized

In recent years most of the conferences I've attended have been mathematics and mathematical physics ones, and I had noticed that, while modest registration fees were often a feature many years ago, these days most such conferences, especially in the US, have no registration fee at all. It seems that mathematicians tend to organize conferences at rather modest cost, and mathematics research is very well supported by the NSF, universities and private foundations. This morning I'd been idly considering the idea of taking a day-trip down to Philadelphia next month to visit friends and maybe attend a couple talks at String-Math 2011, which has a promising list of speakers, but not yet a schedule of talks. I noticed though that registration is $200, which is a bit high as a price to attend a couple talks, whatever source I might find to pay for it.

As one moves from mathematics topics to physics ones, it seems that things get a lot pricier. You might think that string theory not working out as hoped would lead to the availability and market-value of string theory talks heading downwards, but the opposite seems to be true. The big string theory conference this summer is Strings 2011 in Uppsala, where registration will cost you 5625 Swedish Krona (about \$900) . For that you get the talks, coffee, lunch and a reception. If you want to go to the conference dinner, that's \$112 extra. The conference series is advertised as "gathering more than 500 researchers in string theory", although in recent years, attendance at Strings 20XX conferences has been a bit lower. Last year's was anomalous, held in March (when academics often can't travel) in College Station, Texas (not exactly a major tourist destination), it attracted only 193 participants, despite a relatively low registration fee of only $350. For Strings 2011, they've got 208 people registered already. The list of speakers is here. There's an associated program of Public Lectures which seem likely to have little to do with string theory, instead concentrating on "mind-boggling questions" about the multiverse and the Big Bang. Those at least seem to be free.

I haven't added up the total cost, but if you've got significant funds available from a grant, university research support, or private wealth, you can spend pretty much the entire summer attending not just string theory talks, but even string phenomenology talks (see a list here). There's the String Vacuum Project meeting in Philadelphia starting May 23, from which one could head to String Phenomenology at Nordita from May 30 to June 25, then Strings 2011 in Uppsala until July 2, a workshop and conference in Spain from July 3-29, Les Houches for most of August, then String Phenomenology in Madison August 22-26 and SUSY11 at Fermilab keeping you busy until Labor Day.

Update: The hot topic these days is not string theory, but gauge theory amplitudes, using twistors. If you can't afford strings, the price of the twistor talks is still low: a correspondent points out to me that for a registration fee of 15 pounds, you can attend Twistors, Geometry and Physics, a meeting this summer in honor of Penrose's 80th birthday.

Comments

1. **Bee**
   May 11, 2011

   And we all know why the fees are so high. Because you pay for the flights of the invited speakers.

2. **chris**
   May 11, 2011

   curious – i always have to pay my own trip even as an "invited speaker".

3. **lun**
   May 11, 2011

   This is true for physics conferences in general: cost of upcoming Quark Matter conference, 600 Euro.

   From what I understand (Anyone correct me if I am wrong, I heard this from participants), the string conference series is invited speakers only.
   In practice, this and the number of attendants translate into an exercise of celebrities talking to each other and everybody else listening and hoping to get a word to someone edgewise.

   If this is true (see disclaimer in previous paragraph), I would not attend such a conference, for a whole slew of reasons, summarized in the equation conference!=oscars. 🙂

4. **confs**
   May 11, 2011

   Lots of confs, not just on strings, are pricey. And if you look beyond physics, to medical or financial confs, you may have to pay USD 900 just for a one-day meeting. Phys/math confs are pertty cheap by comparison. If 200 bucks gives you pause, you probably can't affort to gamble at the casinos and chug the cocktails either. What will the hookers think of you?

   Invited speakers may or may not have to pay their own way. A friend of mine was invited to speak at a conf, but had to pay his own airfare (and registration fee!). But I invited him to stay at my home, and took him around ~ tourism, which persuaded him to accept the invitation. On the other hand, I was invited to an overseas conf and all my expenses were paid.

   Numerous prestigious confs have invited speakers only. That does not detract from their quality.

5. **15p**
   May 11, 2011

   If the Penrose b'day registration fee is 15 pounds I doubt the tea, coffee and sandwiches will be any good.

6. **Henry Walloon**
   May 11, 2011

   Peter,

   Conference attendance costs amongst professional scientists?…Pity the amatuer who, arxiq aside, struggles to even read the typical academic paper for less than twenty dollars per download…

   Henry

7. **nbutsomebody**
May 11, 2011

It is quite pointless to participate in the Strings. Most talks will possibly be available online. On a generic ground it is hard to understand what one may gain by paying $900. There is not so many new results in string theory, I wonder who is going to attend this conference and for what reason ?

8. **Peter Woit**
May 11, 2011

nbutsomebody,

Like most conferences, the main reason to go is to get a chance to meet up with other people in your field and talk to them. I suspect that very few people will be paying the \$900 themselves, with it being covered in some cases by the conference organizers, in many cases by research grants and other funds available from one's home institution for this purpose. So, if there's the possibility of a summer trip to Sweden, with someone else paying the bill, and you'll get to hang out with friends and colleagues you haven't seen in a while, why not go? If there's money from some source to cover your flight and hotel, it will probably also cover the registration fee, even if it is \$900.

Finding 500 string theorists with this happy financial situation though might be a bit of a challenge, but maybe not. At least in the US, the budgetary situation for mathematicians and theoretical physicists has been pretty good recently (partly due to "stimulus" funding). Then there's an increasing amount of private foundation money available, for example the Simons Foundation has
https://simonsfoundation.org/funding-guidelines/current-funding-opportunities/collaboration-grants-for-mathematicians

9. **In Hell's Kitchen (NYC)**
May 11, 2011

I'll take 3 kilos of strings for $9.99…

You should see the registration fees in Applied Math and Engineering conferences…in the stratosphere !

10. **Jeff**
May 11, 2011

Wow, you physics types really get jerked, unless you get paid a lot more than us mathematicians. Even the big yearly AMS meeting is much cheaper than this, and targeted conferences often don't really have a registration. I ran a CBMS conference two summers ago, big name main speaker, bunch of well known invited talks, we were going to charge \$25 but ended up just forgetting about it. The banquet was less than \$50, wine included, at a really good restaurant.

11. **King Ray**
May 11, 2011

The truth is free, but falsehoods are usually charged for.

12. **Peter Woit**
May 11, 2011

Jeff,

Traditionally, particle theory groups are quite a bit better funded than mathematicians, with significantly more money available for things like paying conference fees.

13. **Jeff**
May 11, 2011

Peter,

Kind of figured that was the story, just moving the money around I guess. I mean, when you're spending millions on equipment, what's a few thousand on conference fees? The biggest expense for most mathematicians is probably a Mathematica license, or MathSciNet fees, peanuts in the particle physics community. I guess the fact that invited speakers have to pay there own way is for the same reason? For any sort of major speaker that would never happen in the math world – I mean if you're giving a talk at a session at an AMS meeting you probably pay your own way, but if you're one of the "invited speakers" you won't, and if you're giving a talk at a seminar somewhere you won't pay you're own way either usually.

14. **nbutsomebody**
May 11, 2011

Peter,

Thanks for the reply. Yes, it is indeed nice to meet friends and colleagues. For some people this is the motivation, however I do not think these kind of conference are good place to start new scientific collaborations. The reason being that young researchers do not talk in such conferences. Even if there is 3-5 min sessions for less well known people then it may be worth to go. One may talk to people personally but it is difficult to know beforehand who may be interested. Similarly it is difficult to know whose work I may be interested in.

Not to mention the fact, with the current situation of string theory, it is a clear wastage of time to fill ones ear with mostly useless talks and a fair-share of gibberish and promotions. Frankly it is difficult to see the point of arranging a global conference each year, there are not simply so many new results! A five yearly thing would possibly be more appropriate. So I am not going even if I have some NSF money.

15. **sanjay sood**
May 12, 2011

You are right about the hype being generated about ALICE with a likely boost by ALICE people. But one could try to understand their point of view as well.
Gauge/gravity duality or AdS5/CFT4 duality does have some relevance to the properties of quark gluon plasma even though it's a tenuous one. While QGP has neither the supersymmetry nor conformal symmetry, these two are essential in AdS5/CFT4 duality. On the other hand QGP has asymptotic freedom and running of gauge coupling which are missing in AdS5/CFT4 duality. Despite these major differences, AdS5/CFT4 has had some modest success in explaining the properties of QGP, especially the ratio of viscosity to entropy density bound.
One key component of AdS5/CFT4 duality is, by its very nature, a correspondence between a 4+1 dimensional space(AdS5) and a 3+1 space(Euclidean space). Since string theory is in fact the most important part of this duality(closed strings giving rise to quantum gravity in the AdS5 bulk) one could perhaps argue that this duality does in fact explains how space comes into existence. In other words how extra spatial dimensions come into being.

So even though one must be wary of any purported claim of string theory being able to unify all 4 forces, one could at least agree that string theory or Ads5/CFT4 duality has opened a new door that may lead to a new and very deep understanding of the nature of space.

16. **Bernhard**
May 12, 2011

Uau, there's a conference on String "phenomenology"? And from Monday to Friday?

Talking about what???

17. **Peter Woit**
May 12, 2011

sanjay sood,

I'm not seeing how a "tenuous" relation to gravity in AdS5 has anything much to do with gravity in 4d and unification, other than providing an opportunity to mislead people.

18. **Rien**
May 12, 2011

The high price of the Strings conference may have to do with the banana republic-ness of the dollar… Recently you'd get about 6 SEK per USD, while a few years ago, you'd get almost 9. That's quite a drop.

19. **Joe Minahan**
May 12, 2011

Peter,

Thanks for providing a link to the Strings2011 conference for which I am one of the organizers.

However, I would like to correct some of the misconceptions about the costs involved. The registration fee you quote, while technically correct, applies only for same day registrants. Those who register in advance pay 3500 SEK +VAT, which corresponds to 700 USD. We are the first to admit that this is still a lot of money, but this is mainly due to circumstances beyond our control. These are: 1) The weak dollar. Today it is 6.25 crowns per dollar, 2 years ago when we agreed to host the conference the exchange was more than 8 crowns per dollar. 2) The strong Swedish crown. Not only has the crown risen against the dollar, but it has also increased substantially against the euro. Since the majority of the participants will be coming from Europe, this will affect them fairly severely. 3) The VAT. Starting in 2010 the Swedish government instituted a mandatory VAT on conference fees paid by participants coming from outside of Sweden. We, nor any other potential conference host in Sweden saw this coming in 2009. And there seems to be no way to get around it. 4) Not much outside funding. Vetenskapraadet (the Swedish equivalent of the NSF) and KVA (The royal scientific academy, the group responsible for choosing the Nobel prize winners) have generously contributed the maximum amount they are allowed to fund conferences. But this still does not come close to covering our costs.

Let me now say what the conference fee pays for. 1) Local costs for invited speakers. This is a long tradition of the Strings 19xx and 20xx conferences. However, despite what Sabine wrote in her comment, we do not pay for the speakers airfare. 2) The lecture hall and accompanying personnel. This also includes the technical facilities so that you and others can watch the proceedings live (or taped if you are not normally up at 4am). 3) Subsidizing the costs for students. On the registration page you will notice that PhD students pay half price. This money has to come from somewhere. 4) As you mentioned, coffee, lunches and a reception are provided.

Let me add a remark on having invited speakers. It is true that many of them are "celebrities", but they are celebrities for a reason. I can almost guarantee that the most well attended talks will be by Maldacena and Witten, and they will surely give very interesting and exciting talks. Other speakers were chosen depending on their field and recent contributions. Many suggestions came from our International Advisory committee and we tried to round things out as best we could. Nevertheless, there are many deserving people who were regrettably not invited to speak because we did not have room.

Finally, while the number of participants is lower than in past years, we are still expecting to have a very exciting conference. This is the second strings conference I have helped organize. The first was the 1995 USC conference which only had 150 participants, and I think that one turned out okay.

Best Regards,
Joe Minahan

20. **Peter Woit**
May 12, 2011

Joe,

Thanks for the corrections and detailed explanations about the conference costs and funding. I certainly hope Witten or someone else has something up their sleeve that will match the 1995 USC conference, and look forward to free-loading by remotely watching some of the talks. Good luck with the conference!

21. **lun**
May 12, 2011

I do not doubt the ability of the celebrity speakers to give excellent and inspiring talks. But as peter seems to say, if this is all there is to a conference, why should someone (especially a student!) pay the fee rather than just watch the conference from home.
If the only talks are invited than, by definition, the conference will be about presenting results that there is a consensus are interesting, rather than discussing and deciding WHICH results are interesting and thought-provoking. The separation between talking celebrities and listening students will make it very difficult for a student to meaningfully participate in the discussion.

That might well work out for the best. However, sometimes the most interesting result of that year is one everyone on various international advisory committees would have happened to have missed. And in a less formal (in an organizing sense) conference where there are more submitted talks, such results might emerge and stimulate further work and discussion.

In the off-chance that this happens, I generally prefer smaller workshops to big conferences, for this reason.

22. **Amos**
May 12, 2011

The science of economics offers an explanation for the high cost of string theory workshops: rent seekng.

An economic rent is the ability to earn money without expending effort, like the way a landowner can (simplified, of course) sit back and receive a revenue stream by renting out his land.

"Rent seeking behavior" is the tendency of people to overspend in chasing rents, leading to a net economic loss. For example there must be ten thousand barristas in Los Angeles who could be starting productive careers but instead hope to become famous.

Tenure is also a rent. So is an academic reputation with salary-command potential. The great efforts spent to obatin these things, in which most contenders lose out, is economically wasteful rent seeking.

As the number of available string theory rents declines, rent-seekers will increase their expenditures in the hopes of obtaining the last few.

23. **conf_student**
May 12, 2011

I got my first job after graduation by going to a conference. My thesis supervisor did not attend, but he told me "Go speak to people, tell them that you are writing your thesis and expect to graduate in a few months, and are looking for a postdoc job, do they have any openings?" I was utterly terrified at this bare-faced way of doing things. (I later found out this is called "cold-calling" and it is a recognized technique and studies show that it is successful.) But anyway I took my life in my hands and approached some strange professor whom I had never heard of and made my pitch. As soon as I saw the big smile on his face and he put his friendly hand on my shoulder I knew that all would be ok. He didn't have a job opening, but he pointed me to someone who did. I went to that person and he scheduled an interview, and I got my first job. There's more to a conf than listening to talks via streaming video. Conferences are job fairs, too. And (some?) professors do not object to being approached by unknown grad students. I made many friends at that conf, friendships which have lasted many years. So I got my money's worth from my registration fee (which was paid by a grant, of course).

24. **Peter Woit**
May 12, 2011

Amos,

The problem with your argument is that no one here is making a profit or collecting rent. The money that comes in from registration fees goes back out to cover the expenses of organizing the conference. This is not something you do to make money, and it's a huge hassle. The rewards are intangible ones.

Joe Minahan did a good job of explaining here why their conference has pretty high fees. Most small math conferences I know about have many fewer people attending, and a quite healthy contribution from the NSF and some other sources. The small size means the organizational work can be done by the organizers themselves, and getting a venue at the university is easy and cheap. For a big conference you need staff and a large venue, and without a correspondingly larger financial contribution from grants, registration fees have to cover the difference.

25. **Peter Woit**
May 12, 2011

conf_student,

Thanks for the excellent explanation of why attending a conference is important for young scientists, even if they get nothing out of the talks. Conference organizers are well aware of this, it's one reason that they offer much lower registration fees for students.

26. **Christine**
May 12, 2011

For what is worth, in Brazil a postdoc position is not considered a job at all. It is even amusing for us to imagine it seen as a "job".

27. **pdoc**
May 12, 2011

Does a postdoc position (a non-job) pay a salary? Does a postdoc have to pay income taxes on that salary? (If yes, is "income" not correlated with "job"?) Is the postdoc expected to put in (regular?) office hours, if the position is not a job?

28. **Christine**
May 12, 2011

@pdoc: In Brazil, the answer is no to all your questions. Postdocs receive a fellowship (sort of) and are seen mostly as trainees. See e.g.: http://www.fapesp.br/en/5427

29. **Mitchell Porter**
May 15, 2011

Peter wrote:

"The hot topic these days is not string theory, but gauge theory amplitudes, using twistors."

But these aren't separate topics! It started with Witten's twistor string, and the theories being studied have string duals in AdS space.

30. **Bugsy**
May 15, 2011

Christine- a postdoc grant of 5,000 reais per month tax-free I suppose is not so bad in Brazil, for a single person. In a weird move, the Brazilian govt is trying to make up for abysmal primary/high school education by expanding the universities enormously, so if the person ends up wanting to stay in Brazil, it appears there are at the moment lots of open positions at new federal universities throughout Brazil, in math or physics, at which a posdoc who learns Portuguese has a good shot. So rarely in today's world, there are jobs. Now whether this is a good career move is a different question- you might "end up" in a small town someplace, with a nontrivial teaching load, or in a big city choked with cars like Sao Paulo or Rio.

31. **Peter Woit**
May 15, 2011

Mitchell Porter,

Just because you can find some connection between a topic and string theory, that doesn't mean the topic is string theory.

Nati Seiberg of course predicted this years ago, when he said that no matter what replaced string theory, string theorists would “call it string theory”.

32. **Mitchell Porter**
May 15, 2011

Peter – d=4 N=4 Yang-Mills theory, which is at the center of the twistor/gauge enthusiasm, is *equivalent* to Type IIB superstring theory on a certain background. (Or if, against all the evidence, it *is* inequivalent, then it is so close that the difference consists of a very subtle deformation.) And Type IIB is, uncontroversially, old-school string theory, it’s not some new topic which has been adventitiously appropriated by string theorists in order to remain relevant. So string theory was rediscovered in an unexpected place.

It *is* remotely conceivable that the string description will recede into the background conceptually, and people will prefer to think in terms of twistors, but I doubt it. A more reasonable question might be, does this mean that strings “mean” something different to what people thought in the 1980s? What I mean is that from the d=4 field-theoretic perspective, the AdS dimension, the compact dimensions, and the extended objects (strings and branes) all emerge from renormalization group flow and the structure of moduli space. It might be argued that strings and branes should therefore be conceived as abstract in some way, and one might wish to reserve the notion of physicality proper for the fields in four dimensions. I think *that* is a debate with a future. But if string theory is truly irrelevant to reality, then so is the twistor/gauge revolution.

33. **Peter Woit**
May 15, 2011

Mitchell,

As far as I can tell, the reasons twistors are useful in studying perturbative gauge theory amplitudes have little to nothing to do with string theory. But it’s an evolving story, we’ll see what the final result is when people really understand how to formulate these theories in twistor space. Maybe strings will play a central role, we’ll see. Until then, I think continually hyping the importance of strings in cases where they aren’t the center of attention is PR, not science.

34. **Christine**
May 16, 2011

@Bugsy: I didn’t say to be a postdoc in Brazil was *bad* (in terms of temporary earning), only that it is not seen as a *job*. The notion of job is simply different here. There are openings (jobs!) in federal universities/institutes, but they require disputed contests. I had to win over 86 candidates to get a permanent position in a related area (not my primary area) in a military institute, considering at the time personal constraints (city, family, etc). What I have *is* a job, but as you say, not the best career move. But I guess you’re right: there are probably more opportunities now in Brazil than in the USA/EU, and I wouldn’t want to live anywhere else anyway.

In any case there are no guarantees that a postdoc will end up exactly where she/he planned, in terms of career and city. That is why a postdoc is seen here as a trainee with a temporary grant, and for us this is not exactly a “real job”.

35. **Shantanu**
May 17, 2011

conf_student, I wish it is as rosy as you point out. I wanted to switch fields after ph.d and my thesis advisor and people who wrote letters for me did not know anyone among the jobs I applied and none of the conferences I went to ever helped me.

Recent NSF Grants

May 11, 2011
Categories: Multiverse Mania

In responding to a comment on the previous posting, I was curious if one could easily get some data on relative sizes of grants in mathematics and physics, so started to do a quick search on nsf.gov. Among the first few NSF grants that turned up, I noticed a couple rather odd things:

- Award 1056580 for a postdoc in "Dark Energy, Fine-Tuning, and the Multiverse: Testing Theories in Modern Cosmology" drew my eye, since my impression was that NSF physics panels weren't so likely to support Multiverse Mania research. Taking a look at the details of the award gave the explanation: this one is being funded not by the physics division (PHY) at NSF, but by the sociologists (SES, Division of Social and Economic Studies). So, now it seems that multiverse studies are part of sociology, which is much more appropriate than physics, and has the added advantage of opening up new funding opportunities.
- Trying to pick a typical theory group grant, I took a look at Award 0969020, for the string theorists at UT Austin. I was pleased to see that blogging is now a selling point on NSF grants:

> Professor Distler authors a blog which discusses and elucidates many of the important research papers which appear on the daily arXiv listings, and he plans to continue his activity.

The abstract was the usual sort of string theory promotional verbiage, beginning:

> For the past two decades, string theory has been one of the most intensely investigated areas of theoretical high-energy physics. This is true chiefly because string theory offers what is currently the most successful method of unifying gravity with the other fundamental forces (strong, weak, and electromagnetic).

The next one I took a look at was Award 1001296 to theorists at UPenn, whose abstract sounded kind of familiar, beginning:

> For the past two decades, string theory has been one of the most intensely investigated areas of theoretical high-energy physics. This is true chiefly because string theory offers what is currently the most successful method of unifying gravity with the other fundamental forces (strong, weak, and electromagnetic).

Comments

1. **Shecky R.**
   May 11, 2011

   Well, this is all sort of funny… or, NOT! (or maybe by mistake you just logged into the National Sociological Foundation grants and didn't realize it).

2. **JSE**
   May 11, 2011

   Re the first grant: studying the battles touched off by string theory over the philosophical question "what constitutes evidence for a theory?" seems to me a very interesting and important question in the philosophy and sociology of science. It seems especially suited to somebody like the PI, David Kaiser, a historian who holds Ph.Ds in both history and physics. He gave a very illuminating physics colloquium here last month about the history of Bell's theorem outside academic physics in the 1970s. Good granting, as far as I'm concerned.

3. **Peter Woit**
   May 11, 2011

   JSE,

   The multiverse business is certainly an interesting issue in sociology and philosophy of science, and I've no problem with it being studied as such. The funny thing is how much their abstract reads just like those of people claiming to be doing conventional physics these days.

   By the way, Kaiser has an interesting book about to appear on the topic you mention, called "How the Hippies Saved Physics". I've written a review of it for American Scientist, soon to appear, at which point I'll write something on that topic here.

4. **DLS**
   May 11, 2011

   Regarding the last two quotes, I've seen that sort of thing before. My understanding is that NSF program officers can sometimes insert a sentence or two in award descriptions that they feel need a little extra non-technical context, and these get re-used. So while it's possible that the PIs borrowed from each other, you can't take it to the bank.

5. **Peter Woit**
   May 11, 2011

   DLS,

   Since the awards were made at the same time, the hypothesis that someone at the NSF is responsible for this seems reasonable. Perhaps they'll save people some work from now on by just reusing the same hype on all grants in the field. Just imagine the amount of time that has been wasted over the past 25 years by thousands of physicists trying to come up with some new way of saying the same thing. Enough's enough.

6. **Marc**
   May 12, 2011

   The funny side is that Distler has not discussed physics or arxiv papers in months on his blog, but only posts about his bizarre software packages. His blog has become completely narcissistic and useless.

7. **Tim van Beek**
   May 12, 2011

   Peter said:

> The multiverse business is certainly an interesting issue in sociology and philosophy of science…

Yes, definitely! Someone could start to investigate the redefinition of *"successful"* in statements like

> …string theory offers what is currently the most *successful*method of unifying gravity with the other fundamental forces…

Also, I'm surprised that the fact that

> …string theory has been one of the most intensely investigated areas…

is supposed to be an argument **pro** more research. When I was a graduate student in Heidelberg, I remember a discussion about someone not getting funding for more solid state physics research, because the Heidelberg factulty already had a fair share of solid state physicists. (And these are people who produce tangible results every year.)

8. **JSE**
May 12, 2011

By the way, you can find mention of my blog in my latest grant, too:

http://www.nsf.gov/awardsearch/showAward.do?AwardNumber=1101267

I'm with Peter — blogging is "broader impact," and if you blog you should say so in your proposal.

9. **Mikhail shifman**
May 12, 2011

Excellent observation!

10. **Roman Oliynyk**
May 12, 2011

My first post here, although enjoyed reading the blog for a couple of months.
Excellent and sad observations. Will be interesting to see 5 years from now, hopefully the trend will change by then. Provided string theorists can find a different line of work and enough integrity to go a different way, when CERN and other experimental results will show no evidence of supersymmetry as stated and no heavy particles.
Well, I am preaching to the choir masters…

11. **Roman Oliynyk**
May 12, 2011

On second thought, one already have done it. Lubos Motl 🙂

12. **anon**
May 12, 2011

If SUSY is not found, SUSY experts will be among the first to figure out whatever new theory that is to be established. They have the formidable mathematical and theoretical skills that are needed for such tasks.

13. **ysus**
May 12, 2011

If SUSY is not found at the LHC, then SUSY will get modified, that is all. When experimental data showed there was no aether drag, the aether theory was not abandoned. Instead it was modified: the Lorentz-FitzGerald length contraction was invented. The aether theory was only abandoned when Einstein came up with a better alternative theory (= special relativity). (And as events showed, relativity incorporated the Lorentz-FitzGerald length contraction in a natural way.) SUSY will only be abandoned when something better comes along. But first there will need to be some data, which SUSY cannot explain easily. "No data" (at the LHC) is not a disproof. There is no other clearly superior alternative theory, and "no data" doesn't point the way to a better theory.

14. **Peter Woit**
May 13, 2011

anon and ysus,

Sometimes I just can't tell whether comments here are parodies or not.

There is a better, more powerful and simpler theory than SUSY. It's called the Standard Model.

15. **anon**
May 13, 2011

I don't totally disagree, but worshiping the standard model is really an activity for the 1970s. SUSY is proven to be the only possible extension of Poincare symmetry of the S-matrix, so its experimental discovery would be on par with special relativity. Given that GR exists in nature, the discovery of SUSY would automatically imply the existence of SUGRA, which may be the low energy limit of string theory. Even if SUGRA is to have a UV completion other than string theory, we are already half-way at finding a quantum theory of gravity. Besides, SUSY would guarantee the precise unification of gauge coupling constants, which would constitute the strongest evidence towards GUTs which are natural and appealing models.
Even though people are well aware that SUSY is too speculative, the potential pay-off is simply too big, so they can't resist the temptation to work on it. Why haven't criticizers come up with something that is more exciting and more addictive?

16. **chris**
May 13, 2011

"Given that GR exists in nature, the discovery of SUSY would automatically imply the existence of SUGRA, which may be the low energy limit of string theory."

do you really mean imply? not hint at or provide evidence in favor of?

i mean, we have no clue yet how to quantize gravity. there are some indications that some sugra models are preturbatively renormalizable (or even superrenormalizable), but they have a host of other difficulties.

in addition, we know for a fact that at least the vacuum state breaks susy. this is quite a bit different than in the case of the Poincare group. and just because we know how to promote this symmetry to a local one in the classical case does not at all imply that we can do the same with a spontaneously broken extension in the quantum case.

it is exactly this kind of sloppy thinking that poisons hep-theory these days. finding a vague analogy is often a good starting point, but unless you can establish every single argument in your chain of reasoning, consider a conjecture a conjecture and not more.

17. **Peter Woit**
May 13, 2011

Please all, enough of the SUSY arguing, please stick to the topic of the posting.

18. **Bertrand Gray**
May 13, 2011

I wonder how many times that NSF abstract is used in other multiverses?

Probably billions upon billions?

19. **Bertrand Gray**
May 13, 2011

Or, NSF could claim that the duplicate abstract in this universe is proof that not only does the multiverse exist, but chosen entities pass back and forth between them!

Also–am I reading DLS right? He writes, “Regarding the last two quotes, I’ve seen that sort of thing before. My understanding is that NSF program officers can sometimes insert a sentence or two in award descriptions that they feel need a little extra non-technical context, and these get re-used. So while it’s possible that the PIs borrowed from each other, you can’t take it to the bank.”

What? Really? Then what is the purpose of abstracts? Is he saying that the National SCIENCE Foundation is striving to be non-technical? For who? Aren’t the folks applying for grants and receiving them–the folks reading NSF Bulletins–the most technical folks in the world?

And is it not possible to be non-technical without repeating verbatim an abstract? Or is there only one non-technical abstract for all of string theory?

DLS’s ho-hum nonchalance and flippancy is as astounding as the identical abstracts! If not moreso! It’s like a vast car pileup/accident or big building going up in flames and someone standing on the street corner going, “nothing of interest here. Move along now.”

One must wonder how often Bohr, Einstein, and Heisenberg submitted IDENTICAL abstracts. lol!

20. **Peter Woit**
May 13, 2011

Bertrand Gray,

In general, the abstracts that appear on the NSF web-site are not necessarily the original ones written for the grant application, but may have been partially rewritten by someone at the NSF, with the goal of explaining what the grant is about to as many people (taxpayers, congresspersons, etc) as possible. So, in this case it’s not what it might originally appear to be (someone plagiarizing someone else’s language, which is a big No-No in academia), but someone at the NSF maybe self-plagiarizing and re-using language of their own.

Coming up with sensible language explaining why string theory research needs to be funded is not so easy to do once these days, having to do it dozens of times, year after year, is quite a task.

21. **cormac**
May 13, 2011

Hi Peter, I too am pleased to see that first award. Prof Kaiser is both an excellent physicist and historian. I’ve sat in on many of his lectures on the history of 20th century physics at MIT this year and they are superb.
More importantly, I’ve studied quite a few sociological studies of physics by academics who know a lot about sociology but less about physics, and they can reach some very strange conclusions indeed. I think studies like this one, by academics who walk in both worlds, are to be strongly encouraged.

22. **Bertrand Gray**
May 13, 2011

Thanks Peter,

Yes–I was just at my local Tea Party meeting, and thanks to the duplicate abstracts which were authored for us laymen, I think we all finally grasped the beauty and promise of string theory. Some people just need to hear things twice is all, and I think we’ll all be voting for more string theory funding now. 🙂

23. **anon2**
May 14, 2011

The highlights from the long interview by John Horgan which you gave in five years ago for *Not Even Wrong* are now on youtube: http://www.youtube.com/watch?v=ukNbQEq4kEE

24. **David Brown**
May 14, 2011

I have 2 questions about the academic sociology of string theory: Approximately how many string theorists are there? Approximately how many academic positions are there for string theorists?

25. **Roman Oliynyk**
May 14, 2011

Peter, just curious about your opinion on John Moffat’s Modified Gravity. This is one of many attempts on cosmological solution. But it does not pretend to be a theory of everything, and at current level of experimental data it feels to me like one that does not over-reach into wild fantasy land (string theory). Any comments?

Anon, I don't agree with the statement that 'SUSY experts will be among the first to figure out whatever new theory that is to be established'. They sure exercised in esoteric mathematics a lot, but your statement would be correct only if the new theory math was closely related to string theory, which is unlikely.

26. **Peter Woit**
May 14, 2011

David,

As a very rough guess at number of string theorists, about 500 or so have tended to show up at the big annual conference, and that's a sizable percentage of the total. A total number of 2000 is probably at least the right order of magnitude. One problem with this question is that it's often rather unclear these days what it means to be a "string theorist".

Someday I'll write about the latest data, but in recent years in the US very few string theorists have been getting tenure track positions. There are however quite a few postdocs in string theory, but these are temporary jobs. As far as permanent positions goes, I suspect the situation is better in Europe, where hiring is traditionally not so trendy (in the US, string theory is not the trend these days).

27. **Peter Woit**
May 14, 2011

Roman,

In general I'm not that interested in or expert about gravity questions. No opinion at all about Moffat's gravity theory.

There was a time when susy was the hot new mathematical idea and the smartest young theorists were the ones all over it. That was about 30 years ago though. By now, it's an extremely old, very conventional (but not successful) idea that is worked out in detail in all the textbooks. Becoming a "SUSY expert" just requires having spent some time with grad-student level material at some point during the last 30 years. This isn't necessarily an indication that you're going to be an innovator…

28. **Peter Woit**
May 14, 2011

anon2,

Thanks. I pretty much can't stand watching myself or listening to myself, but anyone who wants to hear me say the usual things, at a time of the height of the "String Wars", might want to take a look.

Much more interesting video is now available though, since a correpondent tells me that Graeme Segal lecturing on QFT at Bonn is starting to be available on video. I hope to find time soon to watch and may write more about this after that:

http://www.mpim-bonn.mpg.de/node/3365

29. **Shantanu**
May 17, 2011

BTW I looked at the NSF grants in theoretical gravitat and almost all of them are on binary black hole or black-neutron star simulations along with a few more on LQG.

The Big Bang

May 15, 2011
Categories: Film Reviews

There's a new film out this weekend with a particle physics theme (no string theory), called The Big Bang, starring Antonio Banderas. I figured that it's my duty to cover this kind of popular culture use of particle physics, so went to see the film last night. It took some effort to identify the one screen in New York where it was showing, and the theater contained about 10 people at the Saturday night 8:20 show. If it's showing in your area (only New York and LA I think) and you want to see it on a big screen, better go very soon.

I was going to write a review, and mention as many as possible of the various physics inside jokes that appear, but this has been done better here, where the film is aptly described as belonging to the genre "nerd noir", with a "particle physics fetish" sex scene. The film features an LHC-lookalike built underground in New Mexico, designed to search for the Higgs (God Particle). The sex scene mentioned in the review pairs Banderas with a woman with a bubble chamber event and uncertainty principle tatoos. In the throes of passion she discusses Heisenberg uncertainty, entanglement, and the Standard Model.

There's more about the film here, and Lubos has his take here. Rex Reed really didn't like it, and it's hard to disagree, unless you're a great fan of particle physics camp in movies.

Presumably this will be going more or less straight to DVD in a rather short time.

Comments

1. **Yatima**
   May 15, 2011

   I haven't seen it nor do I plan to but this is a pretty good review, sounds like something straight out of Michael Weldon's "Psychotronic Video Guide".

   [Explanation for that obscure term is here: http://en.wikipedia.org/wiki/Psychotronic_Video%5D

2. **Bernhard**
   May 16, 2011

   Hi Peter,

   Sorry norry really related to Antonio Banderas, but have a look at this:

   http://www.guardian.co.uk/science/2011/may/15/stephen-hawking-interview-there-is-no-heaven

   It contains a heavy amount of hype, multiverse mania, boosted with the traditional misleading statemenets like:

   "Evidence in support of M-theory might also come from the Large Hadron Collider (LHC) at Cern, the European particle physics laboratory near Geneva.

   One possibility predicted by M-theory is supersymmetry, an idea that says fundamental particles have heavy – and as yet undiscovered – twins, with curious names such as selectrons and squarks."

3. **Bernhard**
   May 16, 2011

   sorry, *not really*

4. **Syksy Räsänen**
   May 16, 2011

   Maybe you should add a tag for "Pop culture".

5. **milkshake**
   May 16, 2011

   No tarantino was observed in this experiment.

6. **Peter Woit**
   May 16, 2011

   Bernhard,

   Maybe it's a sign of the times that you can get more accurate information about fundamental physics from a naked young waitress with impressive tattoos while she is having sex than from one of the most famous theoretical physicists of our era.

7. **Bernhard**
   May 16, 2011

   🙂

   And in the meantime the guy is trying to dig some deeper meaning between the lines:

   http://www.guardian.co.uk/science/blog/2011/may/16/hawking-physics

   "To me, it feels as though he is referring to the idea that there are many possible universes and that we can use Darwinian ideas of natural selection to work out which might be most hospitable to life as we know it, and because they are habitable in some sense we value them more highly. That's my best guess, but I have minimal confidence in it being right."

8. **Low Math, Meekly Interacting**
   May 16, 2011

   Duty? Given the reviews, “cover(ing) this kind of popular culture use of particle physics” was apparently the aesthetic equivalent of taking a bullet for us.

9. **Orre**
   May 16, 2011

   Actors, actresses and directors do not age gracefully. They just decay.

   I remember the pain of seen giants like De Niro and Hoffmann in “Meet the Fokkers”, and great Al Pacino and Coppola making fools of themselves in “The Godfather III”.

   I guess a minor talent like Banderas must be in bad need of cash to co-star a movie with the SM… He’s definitely on the way out.

10. **Giotis**
    May 16, 2011

    Ok Hawking makes various controversial statements to attract attention most probably but you have to wonder why the elite of physicists thinks that String-M/theory is the most promising framework of unification and of Quantum Gravity? Hawking has no obligation or personal interest to support M-theory, he didn’t work on the field and has no significant contributions to it; on the contrary judging by his research history you would expect to support other ideas closer to the canonical quantization of GR.

11. **Peter Woit**
    May 16, 2011

    Giotis,

    There are plenty of the “elite of physicists” who don’t think string/M-theory is the most promising path to unification, and even those who say this often only mean that no one has a good idea, with string theory just the least bad.

    I certainly do wonder why Hawking and other very smart physicists have signed on to M-theory multiverse mania. Then again, some smart people do and say stupid things. I’m encouraged that the waitress with the tattoos at least seemed to have the sense to ignore this stuff…

12. **bjm**
    May 16, 2011

    @Orre
    Re: “…Banderas must be in bad need of cash to co-star a movie with the SM…”

    Trying to milk a commercial success (your first two examples) is a lot different than taking a chance with an original, independent production. Maybe the film didn’t work, but there’s no way to know without trying. Thank goodness for those who try.

Hawking and the Google Zeitgeist

May 16, 2011
Categories: Multiverse Mania

Today is the first day of this year's Google Zeitgeist gathering of high-powered world leaders and thinkers (described here, it seems that Google either doesn't believe in having a web presence, or it's a secret one. Anyway, my attempts to Google it have failed). One of the headline speakers is Stephen Hawking, and the Guardian reports that:

> His talk will focus on M-theory, a broad mathematical framework that encompasses string theory, which is regarded by many physicists as the best hope yet of developing a theory of everything.
>
> M-theory demands a universe with 11 dimensions, including a dimension of time and the three familiar spatial dimensions. The rest are curled up too small for us to see.
>
> Evidence in support of M-theory might also come from the Large Hadron Collider (LHC) at Cern, the European particle physics laboratory near Geneva.
>
> One possibility predicted by M-theory is supersymmetry, an idea that says fundamental particles have heavy – and as yet undiscovered – twins, with curious names such as selectrons and squarks.
>
> Confirmation of supersymmetry would be a shot in the arm for M-theory and help physicists explain how each force at work in the universe arose from one super-force at the dawn of time.

They got him to respond to some questions posed in advance, and one of them has the Guardian's Ian Sample puzzled. Hawking's answer to "What is the value in knowing "Why are we here?"" was:

> The universe is governed by science. But science tells us that we can't solve the equations, directly in the abstract. We need to use the effective theory of Darwinian natural selection of those societies most likely to survive. We assign them higher value.

Sample's reaction to this:

> On reading it I had one of those familiar, sinking moments of realisation that my brain is so spectacularly inferior to the interviewee's that all I can do is hold up my hands and say: "Huh?"
>
> At best I might have an inkling of what this means, but I am by no means sure. In this situation, it might take a while to clarify the answer, but other bright minds out there might well be able to unravel it for me and anyone else who might be interested. If you can help, post your thoughts below and put me out of my misery.
>
> To me, it feels as though he is referring to the idea that there are many possible universes and that we can use Darwinian ideas of natural selection to work out which might be most hospitable to life as we know it, and because they are habitable in some sense we value them more highly. That's my best guess, but I have minimal confidence in it being right.
>
> I will do my best to clarify the answer this week.

My contribution to explaining this is that I see two possibilities:

1. Hawking has signed on to Lee's Smolin's ideas about cosmological natural selection.

2. Hawking has realized that once you've decided to trade in science for pseudo-science and head down the Multiverse Mania path, there's no longer any point in worrying about whether what you say makes sense or not, and is behaving appropriately.

Comments

1. **Thomas Barton, JD**
   May 16, 2011

   Perhaps he is prepping in his mind for a laudatory speech to EU bankers and Eurozone types who believe they will survive their own self-inflicted wounds like some Darwinian super-beast. The man may be experiencing the onset of the structural changes in the brains of some elderly men which explains their rapid and vapid and vigorously angry old codger syndrome. I personally think he has been deified for so long that perhaps he believes his eternal consciousness resides in some supermassive black hole.

2. **Bernhard**
   May 16, 2011

   I think option number 2 is more likely. Hawking has thrown in the towel, probably has given up that someone will ever detect his radiation that (according to him) would presumably make him win a Nobel prize. What´s left is make misleading public statements about multiverses and M-"theory"as if they were traditional science. He is responsible for a huge scientific disservice, as people like Sample really think he still has something to say. Pathetic to say the least.

3. **DB**
   May 16, 2011

   History teaches us that spent theoretical physicists, i.e. virtually all over the age of 45 – engaging in speculative musings are rarely worth listening to. They may sell popular books to bamboozle an already bewildered public, but for science, their value for all practical purposes is nil. Indeed they are often Worse Than Useless, since they undermine their own prestige and that of the field they are undermining when they should be providing valuable and inspirational mentoring service to up-and-coming young talent – from where new discoveries invariably emerge.

   Although never as egregious example of this phenomenon as many String Theorists, Hawking has been slithering down the road towards WTU status for some years now.

4. **Chris Oakley**
   May 16, 2011

   I must admit, I cannot see what Sample's problem is. Hawking is losing it, going senile, going off the rails, losing his grip, losing the plot … whatever. What further explanation does he require?

5. **Jeff**
May 16, 2011

Well, my take on this when I read it in the Guardian was initially that option 2 was correct, but rereading it I think it parses like this – sentence 2 refers to "equations" that cannot be solved, I take this to be Hawking saying that science can't tell you why we are here, the equations describing the human condition cannot be solved. Sentence 3 about natural selection is referring to selection between different human societies. Sentence 4 says that people value societies that survive the most, hence those might somehow be the "answer" to why we are here.

Now, even parsed this way it's not like it makes much sense, and it's said in such a way as to make it impossible to tell anyway, so in some sense we're back to option 2.

6. **MarkoB**
May 16, 2011

What Hawking says makes perfect sense. He is talking about natural selection and evolution of human societies. Why are most countries around the world compelled to adopt "the Washington consensus?" One reason is because those who adopt social democratic policies get out competed by more lean and mean free market societies. That kinda remins me of natural selection. I don't see what the multiverse has to do with this quote specifically.

7. **Carlos**
May 16, 2011

Why we are here, in the google's zeitgeist? The universe is governed by science. Read: The universe is governed by scientists, us. But science tells us that we can't solve the equations, directly in the abstract. Read: But we can't solve your problems, directly in the abstract, we want cash. We need to use the effective theory of Darwinian natural selection of those societies most likely to survive. We assign them higher value. Read: The dollar is going the dinosaurs way, give us gold coins. We assign them higher value.

8. **Yaakov Baruch**
May 16, 2011

Carlos – that was funny!

9. **Anonymouse**
May 16, 2011

DB, that statement is worse than idiotic.

Theoretical physicists taken as a whole have had breakthroughs at all points in their careers. Your ageism is simply indefensible and senseless. I hope whatever field you belong to decides to treat its elder members better than you would seem to do, at least by the time you are one of them.

10. **Bourgeois Nerd**
May 16, 2011

Well, I suppose there is option 3: his voice synthesizer had a meltdown.

11. **Peter Woit**
May 16, 2011

To be fair to Hawking, the difficult nature of communication for him surely explains why no one seems able to figure out what he was trying to say in this case (was it about the multiverse, about the nature of human society?). If he could easily write more sentences, that would help disambiguate.

This doesn't explain though why he thinks it's a good idea to use his limited abilities to communicate to give talks to business and government leaders about how the answer to our fundamental questions about physics is the pseudo-science of the M-theory anthropic landscape.

12. **marrakesh**
May 16, 2011

I agree that Prof. Hawking has to be very concise in his answers, and that may make them rather cryptic at times.

On the other hand, it's clear that chemistry, and biology are effective theories of the underlying fundamental theory. Evolution theory could probably be considered as an effective theory of, say, molecular biology.

Obviously, trying to explain chemistry, biology or evolution directly from the SM would be impossible, so we use those effective theories instead.

So, the answer to "why are we here?" should be sought for in evolution theory, not the underlying fundamental theory. I think Hawking's point is pretty obvious.

He probably was trying to say something more about societies, rather than individuals, which seems to me to involve moving up one more rung in the effective theory ladder (maybe sociology is an effective theory of evolution theory?).

But I think that part was partly lost to the mandatory conciseness. Probably just formulating that simple answer took him a good half-hour.

13. **Harry Johnston**
May 16, 2011

The question is so vague as to be pretty much meaningless anyway. If we knew how he had interpreted it, perhaps we'd understand his answer. 🙂

14. **Kurt**
May 16, 2011

It should be obvious from the first sentence of his reply – "The universe is governed by science" – that he is way out of his depth on such subject matter. The universe isn't governed by science, science is our study and resulting knowledge of the universe.

Wasn't it made clear some time ago when Hawking started pontificating on things like aliens and theology that when answering questions outside of physics he has no idea what he's talking about?

15. **Bernhard**

May 17, 2011

Peter,

I think excusing Hawking for his communication limitation is a big problem. He's receiving much of attention and every single word he types has a direct influence on society and this is a responsibility. One could say is difficult for him to express himself clearly but if we assume this, we have to take from him this right of being someone people look up to or as an authority on science matters. If on the other hand we accept he is capable of making meaningful statements via his computer we have to right and to criticize him for what he says as anybody else. No excuses, and I think we even show him more respect doing so. If I believe the following is more or less accurate:

"In the talk, he will argue that tiny quantum fluctuations in the very early universe became the seeds from which galaxies, stars, and ultimately human life emerged. "Science predicts that many different kinds of universe will be spontaneously created out of nothing. It is a matter of chance which we are in," he said."

then I repeat, he's down the road. If he can use his limited speech capability to say such a thing (m-"theory" might predict multiverses, science certainly does not) I have no desire in trying to find any deep meaning in what he's saying. From the very beginning is clear he embarked in the pseudo-science journey, so farewell.

16. **John Baez**
May 17, 2011

I like this quote:

> One possibility predicted by M-theory is supersymmetry, an idea that says fundamental particles have heavy – and as yet undiscovered – twins, with curious names such as selectrons and squarks.

So M-theory makes a prediction after all! It predicts that these heavy and as yet undiscovered particles will have curious names.

17. **chris**
May 17, 2011

the universe is governed by science?

huh???

18. **zomarz**
May 17, 2011

I suppose Hawking is just reasoning in effective theory sense. I think he meant that it doesn't make sense to use particle physics to describe life (too complicate equations with too many variables), but rather one has to use the "effective theory" of Darwinism. In other terms, he found a new way out of the question "about (the meaning of) life?" a scientist is often asked, that cannot be asked within science itself.

And yes, the universe is ruled by science, because it tells you "how", while not "why".

19. **Bernhard**
May 17, 2011

At least now m-"theorists" are discussing in the proper arena (i.e. far away from physics):

http://www.irishcentral.com/news/Christians-angered-by-Stephen-Hawkings-heaven-is-a-fairy-story-comments-121991594.html

"Hawking is happy to discuss the M-theory, in which the universe is said to have 11 dimensions, why then could the universe not have a 12th spiritual dimension?'

This guy seems to be an advanced m-"theorist".

20. **chris**
May 17, 2011

"And yes, the universe is ruled by science"

yeah, about as much as the surface of earth is ruled by google maps.

21. **fairy story**
May 17, 2011

SWH is on CNN now "Heaven is a fairy story", reported by the CNN Religion Editor, no less.

http://religion.blogs.cnn.com/2011/05/17/heaven-is-a-fairy-story-scientist-stephen-hawking-says/?hpt=T2

Then again, back in the 19C, Napoleon Bonaparte famously inquired of the Marquis Pierre Simon de Laplace (who had presented the Emperor a copy of his book on Celestial Mechanics) that Laplace's book nowhere mentioned the Almighty. Laplace equally famously replied "Je n'avais pas besoin de cette hypothese". (I had no need of that hypothesis.)

22. **Peter Woit**
May 17, 2011

fairy story,

I guess it's a good thing that Hawking has decided to go on about religion. Everyone then just pays attention to this and ignores the business about M-theory.

23. **Bernhard**
May 17, 2011

Ignore the business of m-string-theory is unfortunately not going to happen soon. It seems now, ST is an alternative to the Higgs or something like that:

"Theories have been suggested as alternatives to the Standard Model that do not require a Higgs boson.

One of the leading ideas is string theory, which proposes that elementary particles are not 'points' but 'strings' with curled up higher dimensions."

http://www.dailymail.co.uk/sciencetech/article-1388086/God-particle-likely-end-year.html?ITO=1490

24. **Claudio Corbetta**
May 17, 2011

I was at the Zeitgeist this afternoon and listened to Professor Hawking presentation. I had not read his latest book in advance and it came quite as a shock/surprise to see that he unmistakenly said "I believe we have a final answer, it is M-Theory and I believe this theory is right". I am quite familiar with Lee Smolin's ideas and there was definitely no hint whatsoever to cosmological natural selection. By the way, by the sounds in the audience it was quite clear that there was more compassion than attention or interest in what Professor Hawking had to say. On the positive side there he praised data driven cosmology!

Professor Hawking video is not up yet but if the organisers publish it it will be found here http://www.youtube.com/user/zeitgeistminds

25. **Peter Woit**
May 17, 2011

Claudio,

Thanks, although that's rather depressing news.

While some string theorists embrace the M-theory anthropic landscape as an excuse for not admitting failure, I can't help thinking that in Hawking's case the unconscious motivation is that he wants to feel that he has found a final theory before he passes away. This is the ultimate final theory, it says that the laws of our universe are essentially random artifacts of an array of solutions so complicated we can't really analyze them and compare the results to what we see. Signing on to this is doing what David Gross accurately described quite a few years ago when this started: giving up. Maybe someone should read Churchill's speech to Hawking to give him some inspiration…

26. **imagine**
May 17, 2011

What did John Lennon/Imagine say? Who needs heaven?

"Imagine there's no Heaven
It's easy if you try
No hell below us
Above us only sky
Imagine all the people
Living for today "

27. **Cosmonut**
May 17, 2011

I agree with Claudio.

After two decades of making grandiose proclamations about being on the verge of "complete understanding of the Universe", Hawking seems to be having trouble digesting the fact that the unified theory is nowhere in sight, and when its found, his name won't be on the discovery.
So, he keeps pretending that it has either already been discovered, or it never will be.

Hawking's latest popular book, "The Grand Design" is a good example.

He starts off by saying that there *is* no unified theory – only multiple observer-dependent theories (whatever that means).
Then he contradicts himself by saying that
M-theory is the unified theory (never mind that nobody knows what M-theory really is).

Then he muddies the waters even further by saying that M-theory predicts that the Universe can spontaneously appear from Nothing
(No way. M-theory hasn't made a single testable prediction. The spontaneously appearing Universe is a pure speculation based on quantum field theory)

That, of course, allows him to bring in God, and make headlines, and also pretend that he has "Explained Everything".

With all due respect, I'd say its time to relegate Hawking to his rightful position of scientific museum relic.

28. **Paul**
May 18, 2011

When was the last time Hawking published a paper?

29. **Bernhard**
May 18, 2011

I just saw this:

http://www.telegraph.co.uk/technology/google/8520033/Stephen-Hawking-tells-Google-philosophy-is-dead.html

"Speaking to Google's Zeitgeist Conference in Hertfordshire, the author of 'A Brief History of Time' said that fundamental questions about the nature of the universe could not be resolved without hard data such as that currently being derived from the Large Hadron Collider and space research."

and at the same time:

"In a 40-minute speech, Prof Hawking said that the new "M Theory" of the universe was the "unified theory Einstein was hoping to find". He compared the idea to the computer programme Google Earth, saying it was a "map" of theories, but added that a new, bigger Hadron Collider the size of the Milky Way was needed to collect more data to prove it. "

So, at the same time that we need hard data and that philosophy is dead (here he's probably excused for not really knowing what he's talking about anyway) what comes to the rescue is a theory that makes no predictions and that we have no idea to extract anything meaningful or to get any of the hard data he himself claims is needed.

Hawking must be really sad to see he will die not knowing the truth but this way out is really depressing.

30. **Amateur Scientist**
May 18, 2011

Paul,

The last publication of Stephen Hawking was written by J. Hartle and T. Herzog, and it came out on the 8th Apr. this year, in PRL 106 141302 (2011).

31. **Amateur Scientist**
May 18, 2011

Oops! I meant, of course, that the paper was written WITH Hartle and Herzog, not "by" them. Sorry!

32. **tate**
May 18, 2011

i don't know m-theory from animal magnetism; on the other hand, i find the statements about using science to assign differential "value" to different societies to be extremely creepy. some here have suggested that there is a level of unrepresented nuance to his statement or that he is being imprecise, but i don't see any indication why we shouldn't take him at his word that he believes his own statement (except for wishful thinking). why should anyone read this as anything other than an explicit endorsement of eugenics?

also, if science was vested with the exclusive right to "value" certain societies over other societies or certain individuals over other individuals–based on whether they are "likely to survive"–would we have had a stephen hawking in the first place?

and does no one else here see the problem with hawking making such statements to elite members of government and the private sector?

33. **Nabla**
May 18, 2011

No.

Just wrong key. Hawking sent his answer number two, to Ian's question number three.

34. **ObsessiveMathsFreak**
May 18, 2011

Has anyone considered the possibility that Hawking is not in fact "writing" these speeches at all? He's 69 and in pretty bad shape anyway. There have been several cases of carers of highly disabled people effectively writing their words for them. I know nothing about the professors current condition, but the paragraph quoted seems too incoherent to be written by someone with or without their faculties.

35. **speeches**
May 19, 2011

Even if someone else writes the speeches, Hawking has to approve them. One can hardly proclaim "Heaven is a fairy story" to an international audience and not realize that it will grab headlines.

36. **Claudio Corbetta**
May 19, 2011

Hi again,

by the way the original video has been uploaded, so you can all see by yourself the presentation and the speech

http://www.youtube.com/watch?v=r4TO1iLZmcw&feature=player_embedded#at=124

Regards

Claudio

37. **Peter Woit**
May 19, 2011

Thanks Claudio,

I'm watching the video, it really is appalling. Hawking attacks philosophers as not knowing anything about modern science, then goes on to aggressively promote pseudo-science and make misleading claims about testing M-theory about at the LHC. Very sad.

38. **Peter Woit**
May 19, 2011

Just finished watching the video, and, as the video showed the audience I noticed something about the Google Zeitgeist: women aren't part of it. Besides the woman in charge of taking care of Hawking it looked there were just a handful of women in a room of a couple hundred people, with none of them in the front rows.

39. **Giotis**
May 19, 2011

Be careful Peter, by using the word appalling some may think that you refer to his physical appearance which I don't think is the case.

40. **Fallen Angel**
May 19, 2011

I don't know what's the big deal. Everybody know that heaven actually IS a fairy tale.

Now, are they going to forbid Hawking from entering the US like they did with Bertrand Russell ?

41. **spirit of russell**

May 19, 2011

When was Bertrand Russell denied entry to the USA? Russell was resident in the USA during WW2, and was appointed as a professor at City College in New York in 1940, but it was annulled on the grounds that he was "morally unfit". Look it up, it's a famous case.
Russell also participated an anti-Vietnam war marches in the 1960's. He was in his nineties when he did that.

Paul Erdos was denied a reentry visa to the USA.

42. **lun**
May 19, 2011

Actually, the M-theory multiverse, together with Poncaire cycles, makes the existence of the after-life inevitable precisely because our branes are computers with a finite amount of degrees of freedom.

All string theory needs to become a true religion is an afterlife, and Hawking might be trying to fill gaps 😀

43. **cycles**
May 19, 2011

If the Multiverse is indeed ruled by Poincare' cycles, then surely we must all return to our origins, and be reborn in eternal cycles? So Hawking may indeed be correct that "Heaven is a fairy story". Is Hawking also a Hindu?

44. **D R Lunsford**
May 19, 2011

Attacking philosophy this way was patented by Feynman, but apparently the patent has expired.

And you are right Peter – the only word is appalling. Any sympathy one feels is erased by contempt for the disservice done to what once was called "natural philosophy".

-drl

45. **Peter Woit**
May 19, 2011

drl,

If you've just discovered how to make sense of QED and do calculations in it, you quite reasonably could taunt philosophers for not keeping up with you. If on the other hand, you've just announced that an empty bit of pseudo-science is the grand-unified theory of everything, you might want to not abuse philosophers, who could teach you a thing or two.

46. **bonk**
May 19, 2011

To Hawking's credit, his Brief History of Time is much worth reading than any of latest books by Greene or Sussikind. It has a wide range of topics, with only the last 4 chapters being complete speculation (which still presents a very physical picture).

47. **willi**
May 28, 2011

I think it's bad if one of either stientist has more importance in science than other scientists. It's nothing bad done by Hawking, but it's not good for science. He can be manipulated. People look up to him and too much importance is given to his speech. Every word which is left from his mouth can change air in science world. For example Einstein has done great theories of relativity / and Hawking has made a lot of good theories too. Einstein "has a very forceful word" in science in that time as Hawking now. Einstein thought Quantum Theory is wrong and now it's one of the best teories we have. Hawking can do the mistake too and can be inteligent how you want…. Some people think that everything what Hawking said is holy right….that's bad!

48. **Anon**
May 29, 2011

Willi, Einstein did not say that Quantum Mechanics was "wrong". His objections regarding QM were way subtler than that, and really have never received a satisfactory answer. The best we have collectively been able to do so far, to quote Feynman, has been to admit that "…nobody understands quantum mechanics". To Einstein this state of affairs was, quite rightly, unsatisfactory, and any thinking Physicist today in his right mind would consider that to be to Einstein's credit. It is only certain science journalists who have no deeper understanding of the issue continue to criticize Einstein for it.

49. **willi**
May 29, 2011

Ok. Maybe I used a wrong parable, but my opinions on Hawking and his strong word in science still stay. Do you agree?

Particle Theory Job Market 2011

May 19, 2011
Categories: Uncategorized

By now the hiring season for tenure-track jobs is pretty much over, and for the field of particle theory some idea of the results is available at the Theoretical Particle Physics Jobs Rumor Mill. As has been usual for the last few years, most jobs are going to phenomenologists. Remarkably, it seems that no jobs at all are going to string theorists so far this year. The final number of jobs is yet to be determined, with ten people so far getting job offers. It looks like the total number of jobs in the field will remain at the low level typical of the last three years since the recession hit in 2008.

Erich Poppitz has been compiling statistics based on the Rumor Mill data, and has the results through 2010 here. Job numbers rose to a level of 20-25 jobs/year from 2000-2007, from a low level of 10 jobs/year back in the 1990s. This phenomenon is generally attributed to jobs becoming available as the generation that was tenured during the huge expansion of universities in the 1960s finally started to retire. The recession has brought those numbers down to 15 (2008), 9 (2009) and 14 (2010). In recent years (since about 2004) he counts about a fifth of the jobs as going to string theorists. If that number does go to zero this year, that would be the first time this has happened since the numbers became available, and I would guess all the way back to shortly after string theory first became popular in 1984-5.

Comments

1. **bonk**
   May 19, 2011

   Is it just my institution? Graduate students here are dying to do string theory rather than phenomenology. I hope the trends are not as misrepresented in other places.

2. **Peter Woit**
   May 19, 2011

   bonk,

   String theory has always been much more popular than phenomenology. Given the choice of working on the theory that unifies all physics and explains the big bang or computing grungy things about parton distribution functions, it's easy to see why this is. At least for the moment though, those willing to work on phenomenology have much brighter job prospects than the string theorists.

3. **phenom**
   May 19, 2011

   Actually almost *anything* (not only ST) is more exciting than phenomenology. Back in the days when I was looking for a PhD thesis topic, the exciting stuff was the magnetic monopole (which is NOT string theory). Phenomenology is (widely perceived, falsely or not) as bread-and-butter work. It's not the path to choose, to stick one's nose in the air and show the world that one is "exploring the unknown" on a "search and discovery" voyage.

   Even if ST is indeed all bunk. All that glitters may not be gold, but at least it glitters.

   FWIW I did not do a thesis on the monopole.

4. **Jeff Murugan**
   May 20, 2011

   http://inspirebeta.net/search?p=find+a+Bringoltz%2C+B

5. **Peter Woit**
   May 20, 2011

   Jeff,

   Yes, Bringoltz is a lattice gauge theorist.

6. **piscator**
   May 20, 2011

   A lot of 'phenomenology' hiring is model building hiring. Proposing sexed-up models of new weak scale physics is not remotely the same as studing PDFs or calculating NNLO processes. Physics where you get to invent the rules ain't grungy. Most of the recent hiring, at least in the US, seems to be in the former than the latter.

7. **nbutsomebody**
   May 20, 2011

   Peter,

   Let me thank you heartily for the job you are doing. Physics in general, high energy in particular, badly needed somebody like you who will expose, explain and highlight various soci-policitc-economic-al dynamics of the field. That's helpful to both physicists and lay people/tax payers equally. Regular journalism is not of much help, as somebody with a fine understanding of scientific issues is needed.

   Sorry for a very general comment !

8. **human mathematics**
   May 20, 2011

   Wow. I'm so glad I didn't go to grad school. TEN jobs?! It seems like every other person I know is trying to go to grad school. Because you can just do quantitative finance or something if you don't get an academic job. Except that's not really true (according to the people who know http://www.nuclearphynance.com/Show%20Post.aspx?PostIDKey=147527) either. It seems the only truth worth knowing for a twenty-something as far as this Big Decision is concerned is that grad school is a treadmill you run to benefit someone else.

9. **Anonymous**

May 20, 2011

I have to say, I agree (more, not completely) with piscator and disagree with nbutsomebody. Almost all of the hires in the last 10 years in the area of "phenomenology" have been "beyond-the-standard model general phenomenology", meaning that the BSM is the common denominator, but the actual expertise in phenomena ranges from collider to astroparticle/dark matter. Sure, Erich's data aren't sorted this way, but if you just peruse the archives of the rumor mill, it is evident.

Piscator is absolutely right that there is nothing very grungy, and certainly nothing bread and butter about the BSM enterprise. I think for many students, catching the LHC wave and being the bright young theorist who "figured out the LHC discoveries" is pretty sexy. Now if we would just make some of those…

10. **else where**
May 20, 2011

You are only looking at the US rumor mill..
if you look at the UK rumor mill, you'll find at least two people who are string theorists with jobs: franco and wecht.

http://pyweb.swan.ac.uk/~pybl/rumours/

11. **Shantanu**
May 20, 2011

Here is a very nice talk which every particle physicist grad student/postdoc must watch. This is heared towards astrophysics/cosmology, but same advice applies to particle physics
http://online.kitp.ucsb.edu/online/colloq/loeb1/

12. **bonk**
May 20, 2011

For a flavor about how current BSM research works, look at this recent comprehensive paper:
http://arxiv.org/PS_cache/arxiv/pdf/1105/1105.2838v1.pdf
Just look at the table of contents. 46 models, mostly highly speculative, are awaiting the verdict from the LHC.

13. **surikata**
May 21, 2011

> Phenomenology is (widely perceived, falsely or not) as bread-and-butter work. It's not the path to choose, to stick one's nose in the air and show the world that one is "exploring the unknown" on a "search and discovery" voyage.

And who says string theory is not bread-and-butter work? Especially after three decades of furious activity on it.

These days working in ST is more or less the same thing as working in any other well-established branch of physics: doing small variations on results already available in the literature.

14. **Physicsphile**
May 22, 2011

Shantanu, thanks for that link to Loeb's talk. It one of the best talks I have ever seen.

15. **Anon**
May 25, 2011

A word in support of what is being called here 'grungy physics' – i.e. Standard Model phenomenology.

It has been commented that

"…. catching the LHC wave and being the bright young theorist who "figured out the LHC discoveries" is pretty sexy. Now if we would just make some of those…"

Quite how anyone thinks we make any of those discoveries without relying on the PDFs and NNLO processes that people seem to find so boring is beyond me.

Look at it another way – as a 'grungy' phenomenologist you get to see your work used by and quoted in all the major experimental papers developing our understanding of the Standard Model and possibly discovering New Physics. BSM model builders however have to develop increasingly exotic models to get noticed, with a vanishingly small probability of being observed. They also have to become expert self-publicists.

Of course they can try the scatter gun approach (publicise lots of models in the hope that one will bear some relation to reality) or they can start ambulance-chasing the experimentalists – either way its not what I would call the makings of a satisfying or useful career. So you have to balance a guarantee of making a big impact on the field as a whole, against a tiny probability of winning a Nobel Prize. Of course if you have sufficient arrogance you will probably make the wrong choice ….

16. **grungy**
May 25, 2011

BSM, phenomenology, grungy, sexy etc ~ these are holdovers from the 70's and 80's. (Approx ~ one can make a case that the attitudes go back much further.) But back when the SM was brand-new, when it had just been put together and shown to be renormalizable, etc, and spectacular experimental confirmation of weak neutral currents, quarkonium, W and Z bosons etc were pouring in, there was tremendous optimism that high-energy physicists knew the way forward. The buzzword was 'unification' (the 'Theory of Everything'). For a graduate student searching for a thesis topic, BSM ideas abounded, and it was sexy to work on any one of them, and there was optimism and respect that one of those ideas might lead to the TOE. Phenomenology, by contrast, was verification of a model (SM) that was already accepted as correct in all its essentials. Not a glamorous path to follow for a thesis. And HEP was basking in (well-earned) glory in those days. There really was a feeling that the 'big breakthrough' (whatever it might be) was just around the corner. So SWH gave his famous inaugural lecture as Lucasian Professor "Is the End in Sight for Theoretical Physics?" ST was part of that wave of enthusiasm. Today, 30 or so years later, the attitudes or prejudices persist, even if the justification wears increasingly thin.

17. **Shantanu**
May 25, 2011

Peter or anyone else,
are there good blogs which discuss most interesting hep-th or hep-ph papers?
Most of the blogs linked on Peter 's blog (by high energy theorists) hardly discuss hep papers anymore. cosmicvariance once upon a time was good. but there are almost

no paper discussions.

18. **Peter Woit**
May 26, 2011

Shantanu,

It does seem odd to me that some of the blogs that used to discuss hep theory papers now rarely do (e.g. Distler, Cosmic Variance). The best that I know of is Resonaances. One suspects that the lack of anything very interesting happening to talk about may have something to do with this.

One blog specifically devoted to arXiv papers is here:

http://www.technologyreview.com/blog/arxiv/

but I have no idea who is writing it (they hide behind the name "KentuckyFC"), and the coverage of hep-th does things like take seriously Bousso-Susskind, see

http://www.technologyreview.com/blog/arxiv/26787/

19. **Tim van Beek**
May 26, 2011

Sometimes it's hard to tell if a post about hep-th is a hoax, a satire, or not. For example, having Peter Woit recommending a blog where people write this about the BS-paper:

> Still, what this new approach does have is a satisfying simplicity– it's neat and elegant that the many worlds and the multiverse are equivalent. William of Ockham would certainly be pleased and no doubt, many modern physicists will be too.

20. **Peter Woit**
May 26, 2011

Tim van Beek,

My comment wasn't really a recommendation, since I don't know anything about the blog author, and their taste in hep-th papers is pretty dubious. I found that blog and the posting I linked to from an arXiv trackback, so the blog is approved by the arXiv moderators, for what that's worth…

21. **rrtucci**
May 26, 2011

I think Susskind is also interested in complexity theory. It's only a matter of time before he extends his theorem from
Thm
multiverse=many worlds
to
Thm
(a)multiverse = many worlds
(b)(multiverse = many world) iff P!=NP

Cosmological Interpretations of Quantum Mechanics

May 19, 2011
Categories: Multiverse Mania

It seems that there's now a new burgeoning field bringing together multiverse studies and interpretational issues in quantum mechanics. Last year Aguirre, Tegmark and Layzer came out with with Born in an Infinite Universe: a Cosmological Interpretation of Quantum Mechanics, which claimed:

> This analysis unifies the classical and quantum levels of parallel universes that have been discussed in the literature, and has implications for several issues in quantum measurement theory… the analysis suggests a "cosmological interpretation" of quantum theory in which the wave function describes the actual spatial collection of identical quantum systems, and quantum uncertainty is attributable to the observer's inability to self-locate in this collection.

Last month there was Nomura's Physical Theories, Eternal Inflation, and Quantum Universe where "a picture that the entire multiverse is a fluctuation in the stationary, fractal "mega-multiverse," in which an infinite sequence of multiverse productions occurs" is invoked and:

> Our framework provides a fully unified treatment of quantum measurement processes and the multiverse. We conclude that the eternally inflating multiverse and many worlds in quantum mechanics are the same.

Most recently, tonight's arXiv listing has Bousso and Susskind's The Multiverse Interpretation of Quantum Mechanics:

> We argue that the many-worlds of quantum mechanics and the many worlds of the multiverse are the same thing, and that the multiverse is necessary to give exact operational meaning to probabilistic predictions from quantum mechanics.

I confess that I'm having trouble making sense of any of these papers. According to Bousso and Susskind, if I want to understand how quantum mechanics describes some simple, local physical system and what happens when I do measurements of it, I need to sign on to the theory of eternal inflation and the multiverse:

> We will offer some principles that we believe are necessary for a consistent interpretation of quantum mechanics, and we will argue that eternal inflation is the only cosmology which satisfies those principles.

In the case of many string theory papers, one's problems understanding their claims could often be attributed to the highly complex and sophisticated mathematical framework involved. These papers are mostly long sections of verbiage, sometimes with pictures. My inability to make sense of them must have some other source…

Update: Lubos has an explanation of the Bousso-Susskind paper: "they're on crack".

Update: I suppose one could have guessed that Sean Carroll would be a fan of this. In his book he argues that the way to understand the second law of thermodynamics and the arrow of time is to invoke cosmology and the multiverse, now he seems happy to do the same thing with the interpretation of quantum mechanics. The ideas seems to be that to understand some local quantum mechanical phenomenon, you need to use cosmology and think about the horizon that is part of the deSitter geometry. I don't find this argument any more plausible than the arrow of time one.

It does seem like this is now being promoted as the hot topic in theoretical physics, with Sean and others organizing a conference partially devoted to this at Perimeter this summer.

Comments

1. **hutom**
   May 19, 2011

   If you quote them correctly, you are lying about the universe.

2. **DT**
   May 19, 2011

   Interesting.

   In an inflationary multiverse, each universe is separated spatially (albeit over a very large distance) so this still theoretically allows one to travel from one bubble universe to another.

   Now since the many-worlds contains the ensemble of all possible scenarios, it is possible that there is a bubble universe where an advanced alien civilization exists and that has the technology to travel betwen bubble universes.

   Let's also assume that their leader is the abducted Elvis Presley. Theoretically Possible.

   Since anything is possible as long as consistent with physics, we should anticipate this alien civilization to travel to the milkyway and start an inter-bubble war that will soon involve planet earth triggering armageddon.

   Of course, one will argue that we may belong to the universe within the ensemble which the aliens never invaded. But since infinite quantities are now allowed in physics, nevermind everyone confuse quantity with the limit theorem, without a doubt this alien civilization will have the ability to replicate itself infinitely to invade all the infinite number of universes within the ensemble.

   But since this will assign a 100% probability to the invasion scenario, this is a complete violation of the ensemble wave function. A case of inifinity over infinity.

   Wow. The things that are revealed to us by the double-slit experiment.

3. **Bertrand Gray**
   May 19, 2011

   Where do I get the feeling that the Wall Street hedge funds funding fqxi are using the same logic, reason, and math as the folks at fqxi?

4. **DT**
   May 19, 2011

Now, with regards to unifying quantum measurement processes and the multiverse, its understanding is a simple derivation of the reformulated Schroedinger Cat.

Imagine an astronaut who is asleep in orbit around planet Earth, which is in a verge of annihilation care-of an anti-matter death-ray using quantum mechanical effects as trigger.

Since there is now a superposition of Earth-still-there, and Earth-blasted-into-smithereens, there is a superposition of gravity as well – a case of gravity being there and NOT there at the same time, which shouldn't confuse the austronaut since he is asleep and unconscious. But since this is not possible (gravity and no-gravity superposition), the gravity wave function will collapse even without direct observation.

This simple analogy with complex mathematical implications tells us that gravity, including quantum gravity is immune to quantum observational effects. Such a brilliant concept is now being applied to inflation theory.

Only Physicists who are confused by the mathematics, like Woit, won't figure this out.

5. **Anon**
May 19, 2011

The real tragedy is that these blathering idiots are occupying the resources that could be so much better spent on the many worthy young physicists who don't have jobs.

6. **also anon**
May 20, 2011

>> when I do measurements of it, I need to sign on to the theory of eternal inflation and the multiverse

as we have learned from Sean Carroll: if you scramble an egg you are a cosmologist.
and as we have learned from Lewis Carroll: We're all mad here.

7. **Guillaume**
May 20, 2011

And you could also mention the would-be modern Aristotle, crackpot David Deutsch and his multiverse-parallelized quantum computer… Seriously, I've just started his latest book (The Beginning of Infinity), it's quite something. And he blames sceptical physicists for having a "bad philosophy"! Hopefully you can find some time to write a review, Peter.

8. **Francis Cornish**
May 20, 2011

Peter, when are you going to review Penrose's fascinating new book, "Cycles of Time"?

9. **Tim van Beek**
May 20, 2011

I'd like to know if there is some consent in the String theory community about the Bousso-Susskind paper: How important is it and what should be done with it?

A kind of review at the nearing end of a long academic career, the kind I understand and really like to read, is this:

* Rudolf Haag: Questions in quantum physics: a personal view

10. **Christian Takacs**
May 20, 2011

I believe there should be a new classification of fallacy in math and physics called "Appeal to Complexity". The way it works is, an argument or theory is proposed that is so complicated that you can't understand it, but since you don't want to look stupid in front of your peers, you nod your head and say solemnly "hmmm, very interesting, very sophisticated, might be possible" which will cover your ass from actually making a judgement you can be accountable for, while at the same time presenting the pretense that you actually understand the argument, thus making you look clever. The only weakness of an "Appeal to Complexity" argument is if someone not ruled by his peer's favor (or a small child) does a sniff test and says "hmmm, It smells like bullshit".

11. **enc**
May 20, 2011

@Christian Takacs: "The Emperor's New Clothes". The story goes exactly as you say and the hero was indeed a small child, who uttered the famous line "The Emperor has no clothes".

12. **anon**
May 20, 2011

If the Lenny from 20 or 30 years ago could travel forward in time, I'm sure he'd tear the modern "multiverse" Lenny to shreds.

13. **Anon**
May 20, 2011

I think it deserves serious consideration whether someone like Susskind wouldn't be a better fit for a humanities department.

14. **Peter Woit**
May 20, 2011

Anon,

I think you're being unfair to humanists. Relatively few of them are on crack.

Francis Cornish,

I've written a review for a popular publication, will write something here when it appears.

15. **Bernhard**

May 20, 2011

I wonder if it bothers these guys at all they´re not doing physcis anymore. “the multiverse is necessary to give exact operational meaning to probabilistic predictions from quantum mechanics.” ???? Really, what is this? I certainly don´t need to know math as good as them to smell bullshit.

This kind of curiosity should not be put together with physics and I think anything related to these multiverses should start having their own space, their on arxiV… And leave the LHC and measurable things for “narrow minded” people who just want to make science in the old experimental fashion way… This is all going too far and I hope the new generations entering physics will understand that what these guys are doing is not bold and forward. Is just not science.

16. **Paul Wells**
May 20, 2011

I think the key question is if this idea makes any new predictions.

If, for example, there was a connection between the cosmological constant and a time-varying behavior of the fine-structure constant that would be interesting. Otherwise I think it is philosophy.

17. **Peter Woit**
May 20, 2011

Tim van Beek,

I suspect that Lubos’s “they’re on crack” reflects the attitude of much of the string theory community. They thank Witten (among others) for discussions about this, I’d be curious to know what he thinks.

18. **maciej**
May 20, 2011

Even in the Everett many world idea the “worlds” have the same physical laws and constants, e.g. the fine structure constant is the same in every “world” of Everett’s concept.

However in the multiverse concept one imagines that the separate “worlds” may have different constants.

Do they comment on that? (sorry for my ignorance not having read their paper).

19. **Mitchell Porter**
May 20, 2011

Bousso and Susskind’s paper became much more comprehensible to me once I arrived at Figure 13 (page 40). I don’t agree with any of these “cosmological interpretations of quantum mechanics”, but at least I can now understand something of how B & S’s cosmology is organized. In Figure 13, you have a big causal diamond on the left, which corresponds to a stable supersymmetric vacuum state in a flat space which lasts forever. Then you have all the little causal diamonds on the right, which correspond to metastable de Sitter geometries in which supersymmetry is broken, and which decay to the supersymmetric ground state in finite time. And the idea is that there is a novel holographic duality (“FRW/CFT”, see 0908.3844) connecting spacelike slices of the eternal supersymmetric geometry with entire histories in the de Sitter patches. So the formula for this paper is “FRW/CFT eternal-inflation cosmology + the dubious philosophy that QM has no meaning unless every experiment occurs infinitely often”.

20. **Anon**
May 21, 2011

Shall this be called “The BS interpretation of Quantum Mechanics”? 🙂

21. **anonymous**
May 21, 2011

It may now be an open question of whether the authors on arxiv write better fiction than those on vixra – it used to be a no brainer to decide this. I guess crack helps in writing fiction..

22. **Crackpotl**
May 21, 2011

Come on, they’re obviously not on crack. That’s a ghetto drug. More likely some sort of organically grown hashish.

23. **srp**
May 21, 2011

I’ve become more and more unhappy with the new marriage of physics and cosmology. They used to serve as a check on one another, but now it seems like whenever people get stuck on one side they pull something bizarre out of the other to move ahead. Trouble with the Big Bang flatness problem? Invent an inflaton field! Trouble with the time reversibility of physical law and the Second Law of Thermodynamics? Appeal to the low-entropy early universe!

24. **bonk**
May 21, 2011

That’s true. Inflation also serves as the solution to the “monopole problem”, a non-existent problem from particle physics.

25. **Zed's dead**
May 22, 2011

> They thank Witten (among others) for discussions about this, I’d be curious to know what he thinks.

Zed’s dead, baby.

http://www.youtube.com/watch?v=eEutI_JKr7A

3:30 – 5:15

26. **Hansi**
    May 22, 2011

    Arxiv should have some filter that papers with a certain number of words versus number of formuas ratio are automatically given to an administrator for refereeing or are automatically rejected.

    Typically, only crackpots publish dozents of pages without even one single, or only very few mathematical formulas.

    It is a typical criteria for crackpot papers not to contain interesting calculations.

    It is a shame, that people like Suesskind are publishing stuff like this.

27. **the next einstein**
    May 22, 2011

    Dear Hansi,

    The notebooks of Faraday and Bohr contained page after page of words, thoughts, and ideas, with nary an equation. Are you calling Bohr and Faraday crackpots?

28. **the next einstein**
    May 22, 2011

    p.s. I do agree that the current crop of Aguirre, Tegmark and Layzeritus papers constitute crackpottery, but it is because the ideas are completely crankish with no physical basis.

    On the other hand, Bohr and Faraday's words reflected a true, deeper physical reality.

    Einstein had a picture of Faraday hanging in his office, next to one of Newton and Maxwell, so Einstein held Faraday in high esteem.

29. **Hansi**
    May 22, 2011

    Certainly, many of the papers of Faraday and Bohr contained only words. And, according to modern science standards, these would be certainly not be mathematical physics papers.

    Remember: the section where postet their nonsense is

    High Energy Physics – Theory

    But a theory does at least imply some degree of mathematisation.

    If they would have postet their nonsense in physics/general or some private homepage, I would not care, because I do not read physics general.

    What Suesskind is doing here is simply to spam the hep/th archive with off topic nonsense..

30. **Anon**
    May 22, 2011

    I don't like the direction this discussion has taken. I think censoring the arxiv would be a very dangerous and unfortunate path to choose.

    I think it is good that this paper was hosted in hep-th. It just makes their embarrassment more public and humiliating in the circles where it counts, which ultimately will serve the interests of real Physics.

31. **Yatima**
    May 22, 2011

    @srp

    "Trouble with the time reversibility of physical law and the Second Law of Thermodynamics? Appeal to the low-entropy early universe!"

    Err… what?

    If you accuse someone of having to "appeal" to a low-entropy early universe you might as well accuse people of "appealing" to the heat pump if they find their fridge in a markedly off-equilibrium temperature upon opening it.

    And who has trouble with the "time reversibility of physical law" which clearly ain't … is this 1910 or what?

32. **Bernhard**
    May 23, 2011

    Anon,

    You have a point and this applies specially for someone like Süsskind who is already famous and one can say used to be a physicist. But I wonder if we want to allow any sort of crakpotish paper in the arXiv with no moderation. Going around the internet you find so many "awesome" theories that is hard to see that physics have anything to gain with all of them populating the arXiv…

33. **chris**
    May 23, 2011

    His name is Susskind.

34. **Anon**
    May 23, 2011

    There is already moderation.

35. **Bernhard**
May 23, 2011

Anon,

that there is already moderation was exactly my point. So hopefully the level of nonsense will not raise even more, otherwise it will start to be difficult to separate the usual nonsense from the sophisticated string nonsense.

36. **Cosmonut**
May 23, 2011

String theorists have long been trying to piggyback on eternal inflation by saying that the many bubble universes predicted by inflation correspond to string theory vacua. As far as I can see, the landscape of and multiverse are quite independent of each other.
The attitude seems to be “the multiverse can’t be osberved, and neither can the string landscape, so they must be the same”.

Now they are trying to go one step further and claim that the landscape, multiverse and many worlds of QM are one.
I can see an attempt at “unification” going on here – just very different from the unification that was promised in the 80’s !! 😉

Agree with Lubos that these guys are on crack. I think they mostly have been for the last decade or so.
The quest to unify physics is increasingly turning into a farce.

37. **Peter Woit**
May 24, 2011

Anon, Bernhard,

Susskind is one of the few respectable physicists I know who has had an arXiv submission rejected on grounds of sheer looniness, see

http://www.math.columbia.edu/~woit/wordpress/?p=63

In general though, if journals are publishing this stuff, prominent people are doing it and endorsing it, and it is the public face of the field (next month Bousso and Susskind are on a program at the World Science Festival here, funded by Templeton), the arXiv can’t reject such things. They reflect the standards of the field, can’t institute higher standards.

Note that one place where they are doing multiverse-related censorship is that they are censoring links to this blog, a policy that seems to have had something to do with me saying mean things about a Bousso/Polchinski article in Scientific American promoting the landscape.

38. **Anon**
May 24, 2011

Peter, that is exactly why discretionary censorship of the arxiv is a bad thing. Those who exercise this discretion have in various known instances shown themselves incapable of controlling their baser political urges. The dictatorial behavior of the emperor of the arxiv is already bad enough. We don’t need more of that kind of thing.

39. **Bernhard**
May 24, 2011

Anon, Peter

Agreed. Maybe the arXiv could create a new category for these kind of papers, because I still think hep-th does not apply. Perhaps hep-mv (for multiverse), since hep-non (for nonsense) would probably offend someone.

40. **Albert Zweistein**
May 24, 2011

How about hep-ut, for untestable. Or maybe hep-ps for pseudo-science? Or hep-pc for Platonic?

41. **oaf**
May 24, 2011

What makes anyone on this blog think hep has a monopoly on silliness?

42. **Albert Zweistein**
May 24, 2011

Of course hep does not, but the offerings at hep sure make the competition look stodgy.

43. **Guillaume**
May 25, 2011

What I find most disturbing is that arxiv seems to have become a substitute to publishing in decent peer-reviewed journals, which is not what it was supposed to be. Maybe it would be a good idea to only accept on arxiv papers that have already been submitted for publication somewhere? I’m sure a lot of garbage would immediately disappear…

44. **chris**
May 25, 2011

Guillaume,

at least in my field (hep-lat) this is not true. preprints with an arXiv number older than 2 years and no journal reference are generally frowned upon and regarded as proven junk. arXiv is very effective and useful just as it is in giving the community a chance to ‘referee’ a paper before it is submitted to a journal.

45. **ark**
May 25, 2011

The arXiv is supposed to be ‘preprints’ so articles are posted before submission to any journal. So one cannot insist that a post has already been submitted to a journal. But it is also true that in some cases the publishers **want** people to also post on the arXiv. For example the proceedings of the Rencontres de Moriond (or will you call them crackpots?). For example for the 2010 Electroweak meeting, the instructions to contributors state in part:

May we also suggest that you submit your contribution to the proceedings as an eprint to the arXiv.org repository.
http://arxiv.org/
This will ensure a long-term web persistence of your paper. Inform us of the submission number to AirXiv, by sending a mail to the secretary (specify that your proceedings are for the Moriond 2010 EW session)

See this link
http://indico.in2p3.fr/internalPage.py?pageId=2&confId=2065

46. **joenobody**
May 25, 2011

Off-topic: perhaps you’ve noticed it already, but there’s a recent paper in Nature about the measurement of the electron which appears to rule out some supersymmetric theories and, according to its authors, could conclusively rule in or out SUSY in general within the next few years, see this
http://www.guardian.co.uk/science/2011/may/25/electrons-round-cosmos

47. **Paul**
May 25, 2011

“Our formal argument hinges on properties of what we term the quantum confusion operator…”

There’s no way THAT could go wrong.

48. **Bernhard**
May 25, 2011

joenobody,

That´s interesting. I would really like to know how much this experiment can disfavor SUSY (and how much it already did), because I find it hard to believe it can “pretty conclusively rule in or out supersymmetry”.

49. **edm**
May 25, 2011

The paper is really about a measurement (or upper limit) of the electric dipole moment (EDM) of the electron. (There seem to be many ongoing or proposed EDM searches.) Many BSM theories yield much larger values for the EDM than the SM value. (Indeed the SM is almost unique in yielding very small values for EDMs of elementary particles.) Many theories of CP violation yield much larger values for EDMs than the SM, and many models were ruled out in the 1982 review by Norman Ramsey
http://iopscience.iop.org/0034-4885/45/1/003

So almost 30 years later, the search for EDMs continues, and the ruling out of BSM models continues. Of course, for SUSY one can tweak the SUSY parameters. But that will also constrain other things (sparticle masses?).

50. **edm**
May 26, 2011

A more precise statement is that the SM is almost unique in that it predicts nonzero CP violation and also small values for EDMs of fundamental particles. Most other models of CP violation also yield much larger EDMs. Experimental searches for nonzero EDMs have ruled out many such models.

51. **Bernhard**
May 27, 2011

I spent my morning trying to digest Sean’s post. Seems the universe is an inverse black-hole like place where light far away can not reach us (or whatever place you take as “central”) and that defines a cosmological horizon. With this you define a notion of “in and out” of this say pocket universe and just like in a black hole to have a complete description you need to know what’s actually inside the observable universe and this has to includes an amplitude for being in various possible states. Now why and how this possible states becomes the many worlds of quantum mechanics I have no idea. He then asks what the consequences are for cosmological initial conditions and the arrow of time. What I on the other hand would like to know is what would be the consequences of this for experimental cosmology, if any. A pure philosophical discussion with very little chances of becoming a discussion of physics.

52. **somebody**
May 27, 2011

“A pure philosophical discussion with very little chances of becoming a discussion of physics.”

The point however is that these are the ONLY chances we have for making progress in fundamental physics. So I would take them, even if they are slim.

Since this seems to be a popular objection to a lot of what goes for as high energy physics (including string theory), let me just clarify a point that I have often seen the proverbial “layman” to miss. This is not necessarily addressed to the author of the quote.

These questions are only “purlely philosophical” in as far as we are NOT able to do experiemnts/observations at high enough energies like the GUT or Planck scale. If we could (for example) do direct observations of gravitational waves from before big bang many of these questions/theories/models will have immediate answers. Even including things like multiverse. So these are possibly physical, yet at the current time technologically inaccessible (at least if we don’t get lucky), questions. You might be not interested in this distinction, but they do exist. If we had a Planck scale collider of course, these questions are DEFINITELY physical questions, but building one is likely technologically impossible at ANY time.

Finally, ANY fundamental question that we are puzzled by at this point in history, is BOUND to be open to this criticism. We understand the understood Universe amazingly well (this is lost oftenh on many people), so what is to be understood is going to quite difficult to experimentally reach! Despite the statements made on this blog, I am not sure there is a much better way to go about unraveling the remaining mysteris of the Universe than our current ones. We are all a bit frustrated we are not living in the haydays of quantum field theory in the late sixties or so when experiments were still accessible, but we got to keep trying.

Our best shot after LHC (Last High-energy Collider?) will be cosmology.

53. **Bernhard**
May 27, 2011

somebody,

I agree with the fact that such discussions have have a certain value, but I repeat that as of today they remain a philosophical discussion, and there is nothing wrong with that. This fact could change and multiverses could even become part of physics, but more than having the experimental apparatus would be needed in order to change this. The problem is not how crazy or not this all sounds. I'm sure when Einstein started to talk about curved space this was taken as crakpot talk by many. The difference is that GR made genuine predictions that could be tested. It's up to the proponents of a theory to suggest how can be tested or falsified.

I ask, even with the galactic collider or even with cosmology, how would anything Susskind and Bousso claim be falsifiable and become as you say a physical discussion? I cannot see how this would be possible, but maybe a just missed it. I'm open to hear the contrary and I it would be exciting to be wrong.

Also, I disagree with you that this is the only way to make progress. Progess will be made by people collecting data, making plots and theorists struggling to understand it. The hope is of course to see some real SM deviations at the LHC and this would be real progress in fundamental theory. I understand some people just can't wait for experiments to reach they favorite physics scenario but with no predictions, no experiments or even suggestions of a test, it remains a discussion beyond physics. The some physics can potentially emerge from this someday, I can't deny, but right now, this is something entirely different.

54. **Christine**
May 27, 2011

There is a simple, but fundamental detail. It is called the "scientifc method".

As a method, it consists of several items to proceed. One of them is experimental verification. The scientist should provide concrete statements on how to prove that a proposed theory will correctly describe observed data or predict certain phenomena. If that is so, the theory can be regarded as a scientific one. Otherwise, it is not a scientific theory. It is called "speculation". The end.

Now, there are only 2 possibilities: 1- people deliberately want to dismiss the scientific method altogether in order to promote their speculative endeavors as some kind of new "science". 2- They do not know what the scientific method is.

Either possibility is disturbing. Specially if they teach to the younger generation.

55. **Anon**
May 27, 2011

It is clear that Bousso and Susskind do not understand the relative state (many-worlds) interpretation. They do not appear to understand the purpose of it, and they /clearly/ do not understand its technicalities. I doubt they even bothered to read the original Everett paper.

56. **Christian Takacs**
May 27, 2011

May I propose that the problem with the Bousso and Susskind (known hereafter as B.S.) model is not of a philosophical kind, it is actually of a logical nature. By demonstration ; A is true because of B. B is not measureable, testible, or predictable until C. C is when a miracle occurs OR some unknown discovery at some unknown time allows B to be measureable, OR when pigs fly. Logic clearly shows that the B.S. model suffers from premature speculation without any kind of verification, and should be given the sniff test before further consideration.

57. **Anon**
May 27, 2011

Are we sure this paper is not a parody? It reminds me of this classic paper by V. Gates et al., maybe from 1985:

http://insti.physics.sunysb.edu/~siegel/parodies/sgs.html

58. **Peter Woit**
May 27, 2011

Anon,

It's more like this one:

http://insti.physics.sunysb.edu/~siegel/parodies/misanthrope.html

I'm pretty sure though that these papers are not parodies. For one thing, they're not funny…

59. **Jeffrey Dunham**
May 27, 2011

http://news.yahoo.com/s/afp/20110527/sc_afp/australiaastrophysicsscience – is this missing matter that is not part of dark matter, or is it saying they found the matter that dark matter was suppose to account for?

60. **Peter Woit**
May 27, 2011

As far as I know this has nothing to do with dark matter. But you really should consult an astrophysicist, not me.

61. **Cosmonut**
May 31, 2011

This seems to be part of a general pseudoscience principle:
X can't be observed/explained, neither can Y.

Hence, X is the same as Y, or X explains Y.

I hereby propose that the multiverse is the same as the many worlds of QM which is also the same as the various spirit worlds proposed by the religions of the world.

Cycles of Time

May 27, 2011
Categories: Book Reviews

Today's Wall Street Journal has a review I wrote of Sir Roger Penrose's new book Cycles of Time. The review is aimed at a much wider audience than this blog, and is the product of substantial editing to get its length down and make it as readable as possible for as many people as possible, so here are some supplementary remarks.

I should make it clear that I'm not at all convinced by what Penrose is proposing. He needs the distant future of the universe to be conformally invariant, and this requires all particles to be massless. As far as we know the electron is completely stable, with unchanging mass, and this will always ruin conformal invariance. Penrose himself notes the problem. For this to be overcome, whatever our ultimate understanding is of how particles get mass must change so that these masses go to zero in the future. It's also seems to me that the conformal anomaly of QCD will always be a problem, with quantization and the renormalization group always breaking conformal invariance and giving a mass scale, indefinitely far into the future.

The other main problem is the one shared by most "pre-big-bang" ideas: how do you ever test them? Penrose and a collaborator last year created a stir by claiming to see in the CMB patterns of the sort he argues might be expected from black hole decays late in an era before the Big Bang, but it's not clear there's a real prediction here, and others who have redone this analysis say they see nothing.

Attempts to get a Big Bang in our future as well as our past generally strike me as motivated by a very human desire to see in the global structure of the universe the same cyclic pattern of death and rebirth that govern human existence. To me though, deeper understanding of the universe leads to unexpected structures, fascinating precisely because of how alien they are to human concerns and experience. Just because we might find a cold, empty universe an unappealing future doesn't mean that that's not where things are headed.

The book is in many ways an unusual document. It includes an extensive appendix working out some of the details of the mathematics of his proposal. In some sense he has managed to get a trade publisher to put out a highly technical discussion of a speculative idea inside the covers of a popular book, instead of going the usual route of publishing this in a refereed journal. The only references I can find to other places where he has written some of this up are to chapters in this book and this one, as well as this contribution to a conference proceeding. The technical idea behind this, that the hypothesis of the vanishing of the Weyl curvature in the early universe leads to possible cosmological models that can be extended past the Big Bang singularity he attributes to this paper of K.P. Tod. There's a nice recent exposition of this by Tod here.

So, I'm not convinced by the speculation about the far future, and for an evaluation of the ideas about extending back through the big bang singularity you'll need someone more expert about cosmology than me. These topics are very clearly labeled in the book as speculative, without support from other physicists or any experimental evidence. The bulk of the book though is other material providing a background and context for the speculation, and it is this which I think makes it most valuable as a popular book. Penrose is a wonderful, elegant and clear writer, and he covers a lot of ground about physics beautifully here. Most remarkable are the illustrations, by far the best visual representations of a range of important ideas that I know of. Physicists and mathematicians work with lots of internal pictures in their minds representing important aspects of the concepts they are investigating, but very rarely do they have the technical skill to grasp some of the essence of these pictures and get them down on paper. Even more rarely do they make it into wide distribution in print, so I'm glad to see that happen here.

Comments

1. **manyoso**
   May 27, 2011

   Thanks for the review! This will definitely be going on my reading list!

2. **Rich C**
   May 27, 2011

   Penrose makes another testable prediction in the book though, doesn't he? He argues that the correlations in the microwave background should not extend past 60 degrees, for reasons that I sort of thought I understood when reading the book but can't articulate now. As there is a wealth of new data on the CMB coming in now and expected in the near future, wouldn't this also potentially provide either falsification or support for his speculations?

3. **Philip Gibbs**
   May 27, 2011

   I don't see why there would be electrons left. Negative and positive charges are perfectly balanced in the universe as far as we can tell, so if the electron and positron are the only stable charged particles then eventually they will come together and annihilate.

   The theory may have other problems, such as those you mention, but I think it is right for physicists to speculate in this way. It is only by thinking along the lines of such crazy ideas and seeing where they lead that we can figure out what are the consistent possibilities.

4. **Peter Woit**
   May 27, 2011

   Philip,

   First you have to get all your protons to decay, then, in an expanding universe I don't see why all electrons and positrons have to find each other. And, if they're just one electron left over, you're not conformally invariant. Besides, the same argument could be made with neutrinos.

   To make this work, it seems to me you need that whatever gives mass to fermions does so in a fashion that goes to zero at long times. Since we don't understand really the origin of these mass terms, in principle it's possible, but it doesn't seem likely.

5. **Yatima**
   May 27, 2011

   From a layperson's perspective, this review hits the mark.

   Public Service Announcement: Well-grounded discussions about what the universe is, is doing or will be doing shall have to be postponed for an indefinite time, though it's always fun to speculate.

6. **D R Lunsford**

May 27, 2011

Peter said “He needs the distant future of the universe to be conformally invariant, and this requires all particles to be massless. As far as we know the electron is completely stable, with unchanging mass, and this will always ruin conformal invariance.”

Well I think the idea is that is gets close enough to conformal invariance that the remaining masses are comparable to fluctuations in the conformal vacuum. It’s almost a way of realizing Mach’s principle. The fluctuation could be thought of as a new Big Bang. I don’t know if this is what Penrose intends, but that was how I rationalized it to myself.

-drl

7. **D R Lunsford**
May 27, 2011

The other aspect of this, is that at the time scales involved, there is not enough happening to define the scale (that’s the conformal scale) without it being disturbed by fluctuations.

-drl

8. **the next einstein**
May 27, 2011

^^^^
“Penrose is a wonderful, elegant and clear writer, and he covers a lot of ground about physics beautifully here. Most remarkable are the illustrations, by far the best visual representations of a range of important ideas that I know of. ”

“There is nothing worse than a sharp image of a fuzzy concept.”
— Ansel Adams

9. **abomb**
May 28, 2011

How it can it be hard to buy the concept that electrons aren’t infinitely stable, .i.e they are only “meta-stable”. We’re already trying to cope with quadratic renormalization problems with theories like super symmetry. How it can be that electrons are genuinely believed to infinitely stable?

Regardless, given infinite amounts of time, are these electrons really a problem?

10. **ZZZ**
May 28, 2011

I find Penrose’s idea interesting. If the lambda-CDM model holds, each remaining electron positron or neutrino would become isolated in its own private observable universe and the total volume of the universe will become exponentially dominated by voids with no matter. Since physical laws and statements are accurate to only finite number of digits, at some point the universe will become empty, i.e. can be described by models with no particles. So I don’t see the electron that would not die as the fatal flaw in this idea. Rather I don’t know whether vacuum fluctuations continue to supply a time scale. The problem with setting the clock by the vacuum is, what’s that clock for?

Also Lunsford makes a valid point in that, if the universe is finally cold, i.e. nothing happens any more, then what defines time? If the time between particle interactions increase exponentially as things fly apart and cool down, then the total number of interactions is finite. So then the universe really does have a rather definitive end. Or is the ticking of the clock really a counter of interactions because a clock must have physical reality? Then, provided that the cooling down happens faster than the flying apart, distances would actually seem to shrink, and the universe would collapse at the final tick.

11. **Chris Oakley**
May 28, 2011

The lack of conformal invariance of the real world is also the reason that Penrose’s Twistors have not made serious inroads into physics. The twistor is a fundamental vector in SU(2,2), a double cover of C(3,1), the conformal group in 3+1 dimensions. Interesting mathematically – and more so than the ISO(3,1) subgroup – but the latter, the Poincare Group is at least a known symmetry of nature.

12. **Marius Buliga**
May 28, 2011

“Physicists and mathematicians work with lots of internal pictures in their minds representing important aspects of the concepts they are investigating, but very rarely do they have the technical skill to grasp some of the essence of these pictures and get them down on paper.”

Almost any paper by Louis Kauffman (btw, one of the first mathematicians exploring topological quantum computing) is amazing in this respect. See
http://www.math.uic.edu/~kauffman/

Another excellent communicator is Jean-Marie Souriau (geometric quantization and many others), see his latest book “Grammaire de la Nature” (in french) here
http://www.jmsouriau.com/

13. **apostolos syropoulos**
May 28, 2011

Intriguing review of an intriguing book!

14. **Rael**
May 28, 2011

But even if electrons completely annihilated with positrons (what positrons, by the way?), as long as there is a gluon field in the vacuum, wouldn’t its dynamics (also known as QCD) dynamically set a mass scale?

We don’t need to have protons in order to have a natural mass scale of ~1 GeV, only gluons and massless quarks are enough for that.

15. **GeorgeDorn**

May 28, 2011

Hi Peter. Could you comment on this perhaps?

http://arxiv.org/abs/1105.5283

16. **cormac**
May 28, 2011

Nice review Peter, just the right balance.
Here's a morsel of trivia: Mum claims that Penrose seriously considered a permanent position at DIAS in the 1970s. He came over, took on look at Dublin traffic and decided no thanks!

17. **Anon**
May 28, 2011

This book perhaps says more about humans than the universe, specifically our panic, as social primates, at the desolation and loneliness of a wound-down universe.

To avoid this prediction, we will do what we can to try to force a square peg into a round hole, which is unfortunately what this looks like to me. Something like Kepler's attempt to relate the orbits of the planets to the platonic solids.

18. **bjm**
May 28, 2011

@Rael: (what positrons, by the way?)

The ones that would come from proton decay. (See Peter's post above).

19. **jpd, MD,PhD,MfA,**
May 28, 2011

its not a sphere, the electric dipole moment was found to be very
very small. for some reason many articles describe this as "spherical electrons"

20. **Philip Gibbs**
May 29, 2011

Peter, Rael, even if the proton is stable at the GUT scale, it is predicted to be unstable through the non-perturbative effects of the sphaleron solution in electro-weak theory. We are talking about very long timescales so any instability is relevant. Another mechanism is the proton falling into a blackhole and any excess charge being reradiated with electrons and positrons most favoured. By whatever means protons will ultimately decay into positrons in equal number of the electrons and the vast majority (if not all) will pair off and annihilate. I think most (or all) neutrinos will be captured in black holes too.

It is true that electrons could become isolated in regions separated from positrons due to the horizon effect of the cosmological constant. This will be rare because once the particles slow down they will tend to attract. Isolated regions will be quite large with a scale determined by the cosmological constant. In this case there will still eventually be other isolated regions of this size in which there is nothing massive, not-even a neutrino.

Does the Penrose theory really require the whole universe to be absent of massive particles or is a causally disconnected region sufficient?

21. **Markus Maute**
May 29, 2011

What do you think about the following statement by Lenny Susskind:

"One of the deepest lessons that we have learned over the past decade is that there is no fundamental difference between elementary particles and black holes. As repeatedly emphasized by 't Hooft, black holes are the natural extension of the elementary particle spectrum."

Doesn't that point to a possible resolution of the problem of the "decay of all masses" ?

22. **Peter Woit**
May 29, 2011

George Dorn,

Looks like the dozens of other bogus "We've found a way to test string theory!" papers chronicled here in "This Week's Hype". I'll be interested to see if the media pick it up, in this case it looks like the author's institutions at least haven't so far issued press releases. My impression is that science journalists mostly now realize that they've been had far too many times by these claims and may now start just ignoring them. Perhaps that's the best thing to do here too…

23. **Yatima**
May 29, 2011

>Does the Penrose theory really require the whole universe to be absent of massive particles or is a causally disconnected region sufficient?

Well, the new C^3 universe doesn't "take off" at the exact moment everything is living on the light-cone. The new C^3 universe is "just" a remapping of the extremely large, fast-expanding and empty old one. If I understand correctly, once only conformally mappable fields living on the light-cone remain the remapping makes *mathematical* sense, but in effect the current universe, right now is already the next one moving towards its Big Bang. So ok, this idea needs some work.

24. **july**
May 30, 2011

A quick question for anyone understanding Penrose's idea. Why does the space is remapped but not the time? In other words, for what reason does Penrose stipulates a series of eons instead of single recycling eon?

25. **Rael**
May 30, 2011

bjm, phillips, OK, there are enough positrons then. I have no trouble with that.

(I'm not sure sphalerons transitions would play a role in a very cold universe, though. They may have played a role in baryogenesis when the temperature was of the order of the electroweak scale or higher, but as the universe cools down they become irrelevant.)

But even if there are no particles left, besides very cold photons getting even colder as the universe expands, the electron mass and the QCD scale are still present in vacuum fluctuations and they would be important at the very low energies involved in a very cold universe. In other words, QED and QCD are far from scale invariant at low energies.

26. **Jess Riedel**
May 31, 2011

I really don't think it's fair to attribute interest in cyclic universes to "a very human desire to see in the global structure of the universe the same cyclic pattern of death and rebirth that govern human existence," any more than it's fair to attribute interest in big-bang-big-crunch universes to, say, ' a desire to see in the global structure of the universe the creation and ultimate destruction of existence as suggested in the bible'. (If you have actual evidence for any non-scientific motivations of Penrose or others, please share it.) I have no particular fondness for any cyclic universe model, but in the abstract they are pleasing simply for the chance that they could remove mathematical discontinuities/singularities.

27. **lun**
May 31, 2011

For your next book review, I highly recommend
"Higher Speculations: Grand Theories and Failed Revolutions in Physics and Cosmology" by Helge Kragh

It is right up your alley, plus a historical dimension; eg, at the turn of the20th century we also had a "theory of everything" which everyone considered correct because it sort of managed to include all observed forces and objects of the world (from electromagnetism to atomic spectra) and was highly mathematically elegant (helping the development of knot theory): The theory of atoms as ether vortices.

28. **Peter Woit**
May 31, 2011

Jess,

"If you have actual evidence for any non-scientific motivations of Penrose or others, please share it."

Actually, all the scientists I've ever met are human beings and have non-scientific motivations of all sorts.

Removing singularities doesn't have anything to do with cyclicity. If you turn the Big Bang into a Big Bounce to avoid a singularity, that doesn't imply the universe will recontract and do it again.

lun,

Many thanks for the suggestion. I'd read his book on Dirac long ago, which was quite good, and look forward to reading this new one.

29. **Jess Riedel**
May 31, 2011

Peter,

Thanks for the response.

"Actually, all the scientists I've ever met are human beings and have non-scientific motivations of all sorts."

Yes, of course. What I mean is: do you have good reason to believe that cyclic-universe proponents are *significantly more* swayed by such motivations than proponents of mainstream cosmologies…other than just the fact that cyclic models aren't the dominant paradigm?

"Removing singularities doesn't have anything to do with cyclicity. If you turn the Big Bang into a Big Bounce to avoid a singularity, that doesn't imply the universe will recontract and do it again."

Well, fine, cyclic cosmologies are a subset of those which avoid big-bang singularities through a big bounce. How's this for a cyclic-specific benefit: cyclic universe may remove singularities *without* producing infinite spacetime volumes (by making time periodic), thereby avoiding problems choosing measures with which to compute observation probabilities.

Again, I'm not arguing for cyclic models. I'm just offering off-the-cuff reasons why someone might reasonably prefer cyclicity to a big bang.

30. **Peter Woit**
May 31, 2011

Jess,

My comment about the aesthetic appeal of cyclic models was really nothing more than that, an observation that many people seem to find them aesthetically appealing (from what I remember, Bojowald's recent book had some explicit discussion of this). This isn't an argument for or against such models (including Penrose's), or a claim that some people are subject to unscientific behavior and others aren't.

31. **Allan Rosenberg**
May 31, 2011

Peter,

I thought the way you suggested that Sir Roger has, perhaps, progressed past his use by date was very well done. Anyway, I don't blame him for the wild speculation, I blame the microtubules in his brain.

32. **july**
May 31, 2011

>A quick question for anyone understanding Penrose's idea

It seems no one here fits the description. ^^

33. **Lee Flight**
May 31, 2011

Penrose covered much of the material in the book in a recent talk at the PI: http://pirsa.org/11040063/

I saw him give much the same talk live a few weeks ago, he was very open about some of the issues with his approach, the fate of matter fields "you can take it or leave it". There is also his willingness to lose information in black holes which seemed a bit odd to me as a main driver in his book was to reflect on the second law of thermodynamics and it seemed like the evolution of the left-hand side of the Boltzmann entropy formula (S) was of paramount importance to his thinking and yet the right-hand side (logW) not so much. Any how I thought the book worth the price just to read him on conformal diagrams.

34. **Peter Woit**
May 31, 2011

Allan,

I'm sorry to hear that you interpreted my review that way, it's not at all what I think or intended to convey. The idea put forward in Penrose's book is quite speculative and has problems, and he says as much himself. But it's far more lucid and sensible than the kind of speculative nonsense involving multiverses that is is now highly popular among prominent physicists less than half Penrose's age. If I wanted to classify books about fundamental physics aimed at the public in recent years by degree of apparent brain damage of the author, Penrose's is definitely on the low end of the neurological problem scale.

35. **Anon**
May 31, 2011

Just to add into this conversation, Penrose also ties his CCC ideas into his Orch-OR ideas in his latest paper on consciousness with Hameroff:

http://journalofcosmology.com/Consciousness160.html

From towards the end:

"The recently proposed cosmological scheme of conformal cyclic cosmology (CCC) (Penrose 2010) also has some relevance to these issues. CCC posits that what we presently regard as the entire history of our universe, from its Big-Bang origin (but without inflation) to its indefinitely expanding future, is but one aeon in an unending succession of similar such aeons, where the infinite future of each matches to the big bang of the next via an infinite change of scale. A question arises whether the dimensionless constants of the aeon prior to ours, in the CCC scheme, are the same as those in our own aeon, and this relates to the question of whether sentient life could exist in that aeon as well as in our own. These questions are in principle answerable by observation, and again they would have a bearing on the extent or validity of the Orch OR proposal. If Orch OR turns out to be correct, in it essentials, as a physical basis for consciousness, then it opens up the possibility that many questions may become answerable, such as whether life could have come about in an aeon prior to our own, that would have previously seemed to be far beyond the reaches of science.

Moreover, Orch OR places the phenomenon of consciousness at a very central place in the physical nature of our universe, whether or not this 'universe' includes aeons other than just our own. It is our belief that, quite apart from detailed aspects of the physical mechanisms that are involved in the production of consciousness in human brains, quantum mechanics is an incomplete theory. Some completion is needed, and the DP proposal for an OR scheme underlying quantum theory's R-process would be a definite possibility. If such a scheme as this is indeed respected by Nature, then there is a fundamental additional ingredient to our presently understood laws of Nature which plays an important role at the Planck-scale level of space-time structure. The Orch OR proposal takes advantage of this, suggesting that conscious experience itself plays such a role in the operation of the laws of the universe."

36. **Peter Woit**
May 31, 2011

Anon,

Thanks, that journal looks pretty peculiar and crank-ridden. I've no idea what Penrose is up to with getting involved with them or what his current ideas about consciousness are, he keeps that quite separate from his cosmology work. I don't recall seeing anything at all about any of this in either his book or the talk on the subject I attended a while back.

37. **ArchBishopofCanterbury**
June 1, 2011

You are the first to report about all the negative things, reports about the alleged rise of string dictatorship etc etc etc. However, when there is anything interesting going on the field like the fermi lab anomaly you dont have much concrete to say. This makes me really sad and also suspicious of you. I wonder if you really have anything to say about anything that matters.

38. **Peter Woit**
June 1, 2011

ABC,

Well, I'm right in the middle of writing a new posting, and was just writing about the CDF news. Not to get anyone's hopes up, mainly I was just pointing to the postings by Jester and Tommaso Dorigo. In general, when a topic is being well covered by other bloggers, I don't see much point in my going on about the same thing. Very often, on the latest news about some experimental data, the two of them do an excellent job of covering the topic from both the experimentalist and particle theory phenomenologist point of view. I wish I could compete and provide something better than them, but that's rarely the case.

39. **John Baez**
June 2, 2011

Jess wrote:

> I really don't think it's fair to attribute interest in cyclic universes to "a very human desire to see in the global structure of the universe the same cyclic pattern of death and rebirth that govern human existence," any more than it's fair to attribute interest in big-bang-big-crunch universes to, say, ' a desire to see in the global structure of the universe the creation and ultimate destruction of existence as suggested in the bible'. (If you have actual evidence for any non-scientific motivations of Penrose or others, please share it.)

Since nobody else is doing so, I'll come out and admit that the idea of a cyclic universe always appealed to me, just because it seems depressing to think the universe will just fizzle out in a heat death. The idea of a phoenix-like "death followed by rebirth", and another chance at a universe with no Sarah Palin, seems a lot more pleasing.

However, like plenty of scientists, I'm sensible enough to recognize that the universe isn't necessarily the way we'd like. Right now the evidence seems to favor an eternal and increasingly boring and chilly future – a scenario I explore in my page on [The End of the Universe](). Anyone who places a lot of stock in perpetual progress, or the notion that the future makes our present actions worthwhile, has got to think about this scenario and consider recalibrating their value system.

However, it's also important to remember that there's a lot we don't know about physics, and predictions of what will happen in, say, 10^(10^26) years are incredibly un-robust: changes in our understanding, or even more careful calculations based on the physics we already know, could change these predictions dramatically.

(10^(10^26) years is one estimate I've seen for how long it will take for the Earth to quantum-tunnel into becoming a black hole.)

So, besides avoiding a value system where the worth of our actions today depends heavily on their consequences in the far future, it's probably also good to avoid a value system where the worth of our actions depends heavily on calculations that could easily be wrong.

40. **Rael**
June 2, 2011

> (10^(10^26) years is one estimate I've seen for how long it will take for the Earth to quantum-tunnel into becoming a black hole.)

But, by then, the Earth will have long ceased existing.

> Anyone who places a lot of stock in perpetual progress, or the notion that the future makes our present actions worthwhile, has got to think about this scenario and consider recalibrating their value system.

I think you're mixing two completely different time scales. Our values are based on a human time scale (decades, centuries, or even a few milennia), and that's as it must be. The notion that we should recallibrate our values because the Universe will be cold and lonely in 10^(whatever>9) years seems to me a form of millenialism.

41. **John Baez**
June 6, 2011

Rael wrote:

> Our values are based on a human time scale (decades, centuries, or even a few millennia), and that's as it must be. The notion that we should recalibrate our values because the Universe will be cold and lonely in 10^(whatever>9) years seems to me a form of millennialism.

Some people seem to think that it's important that things keep getting better *forever*. I'm saying that there's no reason to think that, so we should avoid a value system where the worth of our actions today depends heavily on their consequences in the very far future.

Maybe I should give you an example of the people I'm talking about. To take an extreme case, there's [Frank Tipler]():

> According to Tipler's Omega Point cosmology, for the known laws of physics to be mutually consistent it is required that intelligent life take over all matter in the universe and eventually force the collapse of the universe.

42. **Rael**
June 6, 2011

Oh well, in that context your point seems perfectly sensible to me. But, boy, what a context!!

43. **Tony Smith**
June 6, 2011

John Baez said
"… we should avoid a value system where the worth of our actions today depends heavily on their consequences in the very far future …"

which reminds me of the "corporate finance" guy who, when evaluating projects for building civilization, asked
"… what is the net present value of these projects? …"

in Abstruse Goose at abstrusegoose.com/363

Tony

44. **John Baez**
June 7, 2011

Hi, Tony. it's interesting to think about the whole idea of how the future matters less and less to us as it recedes into the distance… and how what counts as the "far future" depends on who you're talking to. For some "corporate finance guys", the far future might be 5 years from now. But for me, if I had to pick, I'd say it begins when the human race is about twice as old as it is now. We should at least try to do things to make it likely that we'll be around as long as we've *been* around.

The idea of [hyperbolic discounting]() is one attempt to inject some sanity into discussions of how much we value the future.

Quick Links

June 1, 2011
Categories: Experimental HEP News, Uncategorized

- The big news of the past couple days has been the release of more data by CDF which continues to show a bump in the invariant mass of two jets produced with a W. Resonaances gives an excellent description of this and its possible significance. Tommaso Dorigo remains a skeptic.

  I can't do better than the two of them on this story, but here's my summary take on the situation:

  With the new data, this can no longer be written off as a statistical fluke. 3 sigma you can argue away as such a fluke, but not 5 sigma.

  The main reason to be skeptical though hasn't been the statistical significance, but the possibility that this is due to bad modeling of the background. The signal is being extracted from a huge background, so a small misunderstanding of the background could be its cause. If this is the case, the new data changes nothing, you expect to continue to see the effect as more data is analyzed.

  The fact that Tommaso is a skeptic carries a lot of weight, since he works on the CDF experiment and understands the problems well. In general, experimentalists want the experiments they work on to make great discoveries, so tend to be optimists about their own results. When someone is skeptical about a result of their own experiment, that should give one pause.

  What would really make the case for new physics here more compelling would be if the result is confirmed by one of the other experiments (DO at Fermilab, CMS or ATLAS at the LHC) that should be able to see the same effect if it is there. These groups have a certain motivation to not just confirm their competition's discovery (raising the question of why they didn't find this first), but to convincingly shoot it down. This posting by Pauline Gagnon of ATLAS says that they see nothing in their 2010 data. One expects that D0 is hard at work and should soon release whatever they have found. ATLAS and CMS should also be hard at work looking at the much larger 2011 data samples. We'll know soon the results, but the public comments of Dorigo and Gagnon don't sound to me like those they would be making if they knew their experiments had preliminary confidential results confirming the CDF anomaly.

  Finally, while there are lots of theory papers out already with supposed models explaining this, none are really compelling. This is not an experimental result with an obviously attractive theoretical explanation.

- Abstruse Goose has commentary on SUSY here.
- In gossip of the mathematics world, it looks like Princeton (the IAS) has stolen away number theorist Richard Taylor from Harvard.
- Video of Graeme Segal's Felix Klein lectures this spring at Bonn on quantum field theory are now available, and well worth watching.
- Other interesting video available is Greg Moore's lectures on geometry, topology and QFT at Rutgers last fall.
- For pictures from this years Physics of the Universe Summit, see here. Any info beyond the transparencies caught in the pictures seems to be private.
- The journal Foundations of Physics will be putting out an issue on "Forty Years of String Theory". So far articles intended for this have appeared on the arXiv from Dean Rickles, Steven Gubser, and, last night, Steven Giddings. The Giddings contribution is entitled Is string theory a theory of quantum gravity?, and provides an unusually hype-free discussion of the relevance of gauge/gravity duality to hopes to use string theory to understand quantum gravity, writing:

  > While string theory addresses some problems of quantum gravity, its ability to resolve these remains unclear. Answers may require new mechanisms and constructs, whether within string theory, or in another framework.

Comments

1. **X**
   June 1, 2011

   I'm not following the phenomenology too closely, but I liked the technirho: http://arxiv.org/abs/1104.0976 Do you know what the major criticisms of that suggestion are that make it not "an obviously attractive theoretical explanation"? It would also explain the absence of the Higgs and presence of EWSB in terms of (relatively) known mechanisms.

2. **Peter Woit**
   June 1, 2011

   X,

   I'm no expert on this, would love to hear from someone who is. In particular, I'm curious to know if this kind of technicolor signature is something that was investigated before the CDF result. My uninformed impression is that this wasn't an obvious thing to expect to see with this kind of cross-section.

3. **P.**
   June 1, 2011

   Well, such technicolor inspired resonances are being searched for. They have their own entries in PDG, and also on p. 138 in Viviana Cavaliere's thesis you can find a related plot. I think Peter is right in that one would expect somewhat smaller cross sections for this channel (actually the cross section obtained in 1104.0976 is on the lower end of what is compatible with the CDF excess).

   One reason technicolor is not considered an "obviously attractive theoretical explanation" is that technicolor models tend to be in conflict with a lot of other experimental constraints, in particular flavor physics and electroweak precision tests. But many of these things are very hard to actually calculate, so maybe it does work.

   Cheers

4. **anon.**
   June 2, 2011

   I think that if you had asked technicolor advocates, at almost any point in the last 10 years, at what mass scale they would expect techni-resonances to show up, they would have tended to give you answers much larger than 150 GeV. Even if this state is supposed to be lighter because it's a techni-pion, it would be coming from something at 300 GeV or so, which is *still* lighter than any TC advocate would have expected. This scenario — an s-channel resonance decaying to a W plus another resonance — would definitely be a "who ordered that?" moment for the field.

   Electroweak precision and precision flavor physics results are still incredibly difficult to reconcile with technicolor, and most attempts to do so amount to putting lipstick on a pig. Even if the result holds up and proves to be a new s-channel resonance, interpreting it as being related to technicolor would be a real stretch, one not (so far) justified in any way by the data itself.

5. **Bernhard**
   June 2, 2011

   Peter,

   About Giddings article… One sentence on the very beginning caught my attention:

   “Specifically, string theory naturally regulates the infinite proliferation of ultraviolet divergences in the loop expansion, and, order-by-order in perturbation theory, apparently gives UV finite scattering amplitudes.”

   I´m interested in the “apparently”… Do you know how certain anyone is of this claim? It´s hard to believe one can get “nice”cross-sections out of ST, but perhaps I´m wrong…

6. **Peter Woit**
   June 2, 2011

   Bernhard,

   The situation with finiteness of multi-loop superstring amplitudes is kind of murky, and during the “String Wars” for some reason I ended up getting into long arguments about it, which you could probably find on my blog, Jacques Distler’s or Clifford Johnson’s. One of my colleagues at Columbia, Phong, is one of the experts in the subject. He and Eric d’Hoker (who I went to grad school with) have shown finiteness at two-loops, in a calculational tour-de-force. The technicalities involved are fearsome. As far as I know, neither they nor anyone else has conclusively shown finiteness at 3 loops or above. One reason this story is complicated is that there are several different possible formalisms to use in the superstring quantization, with different technical problems. The claims made from the early days that string had to be finite at any number of loops could be described perhaps as plausibility arguments.

   To be fair to string theory, you certainly can compute amplitudes perturbatively at the first couple of orders, with very pretty and interesting results (which have little to do with HEP as far as we know…). Of course, the full perturbation series is divergent, you need some non-perturbative formulation to get something fully consistent.

   In any case, what Giddings is claiming is that this is all irrelevant, that the real problem is not UV finiteness of perturbation theory, but the infrared and non-perturbative behavior.

7. **Igor Khavkine**
   June 2, 2011

   Question for anyone who has watched Graeme Segal’s lectures. Is there anything particular in his lectures that is worth paying attention to? For instance, which, if any, of his comments are particularly new or particularly clear summaries of what’s already known? For the record, having watched the lectures, I found it hard to figure out an answer to these questions. I do have to admit a certain bias in my theoretical interests, which have so far stayed clear of 2-dimensional, topological, Euclidean QFTs, which might have been a drawback.

8. **Peter Woit**
   June 2, 2011

   Igor,

   One thing worth paying attention to is Segal’s treatment of the relation between the Minkowski and Euclidean formalisms of QFT (he treats this by doing analytic continuation on a space of metrics, not in physical or momentum space as is conventional). Even if you think you’ve never deal with Euclidean QFTs, you probably have, since in order to make calculations well-defined you generally have to do this. This is a fundamental issue about QFTs, and Segal is one of the few people I know who has tried to think it through clearly.

This Week's Hype

June 1, 2011
Categories: Multiverse Mania, This Week's Hype

The latest New Scientist has a much larger dose of M-theory/multiverse hype than I've seen in one place in quite a while. There's a four-part series on M-theory (here, here, here and here) by Mike Duff. It tells the story of the progress of modern physics over the past century according to the dominant ideology: general relativity, Kaluza-Klein extra dimensions, super-symmetry, superstrings, branes, ending in the apotheosis of M-theory more than fifteen years ago. For the current state of affairs, Duff describes his "M-theory" predictions about the real world (that 4 qubits can be entangled 31 different ways, something discussed here). He ends with the M-theory multiverse and the following comments on whether this can ever be tested:

> So is M-theory the final theory of everything? In common with rival attempts, falsifiable predictions are hard to come by. Some generic features such as supersymmetry or extra dimensions might show up at collider experiments or in astrophysical observations, but the variety of possibilities offered by the multiverse makes precise predictions difficult.
>
> Are all the laws of nature we observe derivable from fundamental theory? Or are some mere accidents? The jury is still out.
>
> In my opinion, many of the key issues will remain unresolved for quite some time. Finding a theory of everything is perhaps the most ambitious scientific undertaking in history. No one said it would be easy.

Here he makes it clear that, at least while he's still around and enjoying academic prominence because of M-theory, there's no danger it will face any sort of test it might fail. He answers critics of M-theory by claiming that its failures don't matter. It's the dominant paradigm, and will reign as such until someone comes up with a different theory of everything that isn't a failure.

Elsewhere in the magazine, there's a fawning article about the recent Bousso-Susskind paper (see here):

> TWO of the strangest ideas in modern physics – that the cosmos constantly splits into parallel universes in which every conceivable outcome of every event happens, and the notion that our universe is part of a larger multiverse – have been unified into a single theory. This solves a bizarre but fundamental problem in cosmology and has set physics circles buzzing with excitement, as well as some bewilderment.

No critics of the idea were located by the writer, with the discussion on blogs described as:

> The paper has caused flurry of excitement on physics blogs and in the broader physics community. "It's a very interesting paper that puts forward a lot of new ideas," says Don Page, a theoretical physicist at the University of Alberta in Edmonton, Canada. Sean Carroll, a cosmologist at the California Institute of Technology in Pasadena and author of the Cosmic Variance blog, thinks the idea has some merit. "I've gone from a confused skeptic to a tentative believer," he wrote on his blog. "I realized that these ideas fit very well with other ideas I've been thinking about myself!"

Somehow Lubos's "they're on crack" take on the subject was missed.

Finally, the significance of all of this is summarized in an editorial which argues that Bousso-Susskind finally pulls the plug on religion and replaces it with science:

> Cosmologists can now begin to take God seriously, precisely because they can explain him (or her) away.

Comments

1. **Anon**
   June 1, 2011

   I think you mean Bousso-Susskind, not Bousso-Polchinski.

2. **Peter Woit**
   June 1, 2011

   Anon,

   Oops, many thanks for the correction, now fixed. My apologies to Polchinski…

3. **Bernhard**
   June 1, 2011

   There was also some little hype here: http://bigthink.com/ideas/38684 (not surprising though, since this is a hype specialized blog).

   I wonder if this new wave of hype is just related to Buosso-Susskind´s paper.

   And I´m surprised nobody yet suggested their paper "predictions" will also be tested at the LHC…

4. **Peter Woit**
   June 1, 2011

   Bernhard,

   It's not completely hype-specialized since they have an interview with me on their site…

   That seems to just be a link to a not-very-good article by an author who got the nonsense from popular science writer Amir Aczel, who got it from physicists who should know better (those promoting the usual "string theory can be tested since there are string theory models with extra dimensions visible at the LHC" hype which has been around since the dawn of time).

5. **Bernhard**
   June 1, 2011

   Really? OK, didn't know that. I got this impression because I often see things like this: http://bigthink.com/ideas/38513

But good to know it was just my bad luck.

6. **Cosmonut**
June 1, 2011

I wonder why they always keep claiming that the multiverse explains God away.
After all if there's an unobservable multiverse out there, there can also be an unobservable God who creates the entire multiverse and keeps creating more !

7. **Peter Woit**
June 1, 2011

Cosmonut,

What I can't figure out is why people who want to fight the science/religion war on the side of science think that giving up on the scientific method and promoting an untestable pseudo-scientific unified theory of the "multiverse" is the way to do it. The behavior of many of these physicists looks every bit as irrational as that of the most fervent religious believer. I don't see how you make a convincing argument to someone that they should trade in their Bible for Bousso-Susskind.

8. **Eric habegger**
June 1, 2011

This newest hype is just another good reason to vote with your wallet and discontinue one's subscription to NS when it comes up for renewal. I did and I've never looked back, plus my blood pressure has been lower ever since. I feel sorry for you Peter that you have to read it just to keep up with the zeitgeist of physics in popular culture.

9. **Allan Rosenberg**
June 1, 2011

Very cute, Bernhard. No doubt their predictions will be tested in some eigenLHC in some eigenuniverse. We just don't know which one it will be.

10. **Peter Woit**
June 1, 2011

Eric,

Stopping having anything to do with popular science media that features multiverse nonsense would mean dropping pretty much all of it. Might be a good idea, but I do need things to write about on the blog.

The bulk of the hype in the latest New Scientist isn't written by them or by any journalist, but by one of the leading British academics in the field. Can't blame journalism for this…

11. **Sean Strange**
June 1, 2011

Thank you Peter Woit. It's nice to hear a physicist point out the crazy hubris of these mathematical theologians. They seem to be trying a little too hard to discredit religion, and the way they pull new theories out of their heads and treat them as scientific facts strikes me as pure mysticism. A true skeptic should reject any group which attempts to be the self-appointed arbiters of truth without evidence, and that includes these multiverse mystics of the scientific priesthood. I don't have a problem with wild speculations about multiverses, as long as they don't call it science or insist that any of it is true!

12. **Anon**
June 1, 2011

What really bothers me is that friends and family members who are laypeople but interested in science keep wanting to talk to me about these "great new multiverse findings", and then I have to be the bad guy and burst the bubble on their sense of wonder.

Oh for the good old days when I was a kid and we had Carl Sagan. We had it good then. We didn't know just how good we had it.

13. **Bee**
June 2, 2011

Ah, but you can now have LQG with SUSY and extra dimensions too: http://arxiv.org/abs/1105.3709

14. **M. Wang**
June 2, 2011

I, too, have noticed the declining quality of popular science media, but I cannot discount the possibility that they have always been filled with Hypes; we may simply have grown old and acquired enough knowledge and wisdom to tell the difference.

Take the latest issue of Scientific American for example. The article, "Planning for the Black Swan" by Adam Piore, claims that the Westinghouse's AP1000 is a Gen III+ design like the pebble bed reactors and will be immune to a Fukushima-type disaster. This is pure baloney. AP1000 is a Gen III reactor, which is basically a Gen II (like the Fukushima) with additional cooling back-up that gives additional 3 days of grace period under blackout before a meltdown. Pebble beds, on the other hand, is a true Gen IV reactor, which is totally immune to melt-down even in principle. Mr. Piore is probably a paid consultant to Westinghouse, but ten years ago I did not know enough to tell. In fact, ten years ago, I probably would have read all the multiverse propaganda with just the faintest unease. Woit's and Smolin's books make all the difference.

15. **Guillaume**
June 2, 2011

So the Busso-Susskind paper has just hit the arxiv, hasn't been published in a journal yet, hasn't even been properly reviewed, but the media are nonetheless showcasing it like it's going to be the next Nobel Prize… Somehow the Rapture craziness doesn't seem so crazy now…

16. **Guillaume**
June 2, 2011

NS "report that cosmologists claim to have found a way to rid themselves of the need for a God-like observer."

Well that's a false problem anyway, as long as you replace the metaphysics of Copenhagen by a materialist interpretation of QM like Bohm/de Broglie/Bell's pilot wave theory.

17. **Aleksandar Mikovic**
June 2, 2011

The reason why many reputable scientist use “unscientific” methods in order to explain everything is due to the fact that scientific method is not sufficient for obtaining/explaining all the features of reality. Simply, there are truths which we obtain without science, and mathematics is the most familiar example. The domain of science are the phenomena which can be tested by experimental tools, and it is clear that this domain is limited, unless you are diehard scientific fundamentalist. The history of the scientific approach to the Universe was to postulate a theory and then to try to test it, but as the domain of the theory grew larger, it was more difficult to test it. Eventually one is forced to use the theories where most of the features cannot be experimentally tested, but this is OK for me, since one is entering in the metaphysics domain, which philosophers have been exploring since Plato.

18. **Anon**
June 2, 2011

You should stop bashing these magazines. They publish hype because hype sells. They are in business and their goal is to make mucho dinero. In any event, real pragmatic scientists like WOit here have not produced any science worth writing about in a long time. The last discovery was the top quark in 1994, I believe. So produce new stuff or shut up. By your reasoning all these mags should have been out of business for the last 16 years or so.

19. **Christine**
June 2, 2011

The history of the scientific approach to the Universe was to postulate a theory and then to try to test it

History shows the contrary.

From *observed* phenomena, elaborate a theory that correctly describes the phenomena. Eventually, the theory may lead to the prediction of new phenomena.

The only theory that I know of which predicted something unobserved until then, from pure thought, was Dirac’s.

Quantum gravity goes in that same direction, though, since it originates from the pure theoretical need to conciliate quantum field theory with gravity.

Nevertheless, any proposed theory in this area should provide means to be tested, at least in principle. Otherwise, it is a speculation and any honest scientist would label it accordingly.

20. **SteveB**
June 2, 2011

Concerning the popular scientific publications. I was once a simultaneous subscriber to Science News, New Scientist, and Scientific American.

I dropped SA because many of the articles were nothing more than thinly veiled advertisements for some product or company. I do miss Mirsky and “100 years ago”, though.

I dropped SN because its content became weaker and NS had the same current news with more details.

I am keeping NS because the writing is excellent, and the coverage is very good. I don’t always agree with each article, but you may get an opposite point of view in the next issue. It doesn’t seem like their staff has too many agendas they are promoting. The worst thing about NS is that their editors leave subtle British slang in many articles which is difficult for an American to translate.

21. **Bernhard**
June 2, 2011

Anon,

Bashing these magazines and newspapers and comment the latest hypes is what I believe is part of the mission of this blog and the reason many of us like to read it. This is not just to simply mock string theorists for no reason. What society gets convinced is important can ultimately even affect our funding. All the crap that is published everyday needs some balance and I have to say I feel kind of a relief when after seeing some ridiculous article about Susskind latest crackpot paper I can read Peter shooting it down merciless.

22. **cormac**
June 2, 2011

I don’t agree with Anon at all. I think NS in particular has become quire sloppy in its physics coverage (can’t judge the rest), mainly because it does not delineate clearly between relatively well-established science and speculation.
I have thought this for some time, so I was interested to observe during the year that NS is not admired amongst the rather prominent physicists I have met at Harvard and MIT – to put it mildly.

23. **milkshake**
June 2, 2011

I don’t know if its is relevant, but I remember reading pop sci articles about multiverse in 80s that were not all that different from what one finds in New Scientist these days. It really helped me during a dreaded oral exam in marxism-leninism philosophy, in 1988, which of course was a mandatory part of the university curriculum in Eastern Europe. So I sailed from historic materialism to anthropic principle and multiverse, and finally I disposed with God. The stern marxism professor was visibly delighted, she even asked me for article reference so that she could use it in her class, and she did not molest me during the following year. (We had several semesters of this stuff). So you can see that multiverse has a practical explanatory power and can become useful if you are cornered by a merciless marxist.

24. **Peter Woit**
June 2, 2011

cormac,

I’m also curious what those you met in Cambridge thought of the sources of the New Scientist stories. New Scientist isn’t just making up this multiverse stuff themselves…

milkshake,

The great thing is that you can use the multiverse to justify either atheism or theism. You made good use of it, but see here for someone who is making good use of it to get paid from religious hedge fund source:

http://www.oxfordtempletonfellows.com/winners201112.html

"Professor Kraay hopes to develop and defend the view that if theism is true, the world God selects for actualization is a multiverse comprising all and only those universes worthy of being created and sustained by God. He further hopes to show that, if theism is true, this multiverse is the only possible world. He will explore the consequences of these views for theism, and will consider the connections between these views and various multiverse theories in contemporary physics and cosmology."

25. **the next einstein**
June 3, 2011

didn't ed witten start the whole m-theory thing?

and isn't brian greene hyping the multiverse?

is not new scientist just following the lead of prominent physicists?

26. **chris**
June 3, 2011

"In any event, real pragmatic scientists like WOit here have not produced any science worth writing about in a long time. The last discovery was the top quark in 1994, I believe. So produce new stuff or shut up. By your reasoning all these mags should have been out of business for the last 16 years or so."

yea, right. you missed the CC? CMB anomalies? neutrino masses and mixings?

and if you want speculation, there is plenty of good one. just this week, heard of the Wjj CDF anomaly?
or, also recenly featured, PAMELA and all other DM searches? The ALICE bumps in high multiplicity events?

lots and lots of new, genuinely interesting stuff. all of which was totally unforseen by the prevalent ToE of today.

27. **Jim**
June 3, 2011

Bernhard,

Thank you for your comparison of the science magazines. I too have subscribed to all three in the past. However I remain faithful to SA. It's like a family faith tradition for me because I began reading my dad's subscription while I was a child-like object. Like every other source, they require critical analysis, only more so.

Thanks also for your mention of Big Think and for eliciting Dr. Woit's reply. From its name I might well have suspected it to be as hype-driven as you were suggesting. Then I found this: Peter Woit interview at http://bigthink.com/peterwoit
It is a video interview with our good Dr. Woit telling it like it is And it is excellently done. Thank you Dr. Woit! I get to hear how to say his name correctly, too.

I believe I can now rank Big Think above New Scientist.
(Please forgive my clumsy attempt at html.)(no bold text was intended)

28. **Jim**
June 3, 2011

my apologies :

I was agreeing with SteveB's assessments of the mags.
I do agree with Bernhard's valuation of this web log.

29. **David Bailey**
June 3, 2011

I used to love reading NS as a kid in the 1960's. The articles were written in an authoritative style without hype, and included such things as relevant chemical formulae, which pushed me into reading more chemistry. NS must have helped to push many kids into science.

Now, I only buy the occasional copy of NS if there is an exceptional article. Far too many articles seem to be hype of one kind or another , or to be written by journalists who have little understanding of the subject matter.

Does hype about infinitely bifurcating universes (a concept which surely goes back to Everett in the 1950's) really sell magazines – maybe, but I'll bet it doesn't encourage many kids to actually enter science!

30. **Cosmonut**
June 3, 2011

@Peter:
I don't see how you make a convincing argument to someone that they should trade in their Bible for Bousso-Susskind
————————-

Well, Hindu cosmology has a beautiful story about how an infinite number of universes are continually emanating from the mind of Brahman.
So, if people are getting tired of evidence-based science, we have a much better deal for them than the Bousso-Russkind multiverse.
No convoluted math jargon, just pleasant Sanskrit shlokas. 🙂

31. **Cosmonut**
June 3, 2011

But honestly, I think the whole landscape/multiverse fiasco shows that the dream of unifying physics purely through mathematical consistency – much hyped by Stephen Hawking – is dead.

Mathematical consistency is not strong enough to give us a unique theory unifying GR and the standard model. We need more data.

32. **Nige Cook**
June 4, 2011

> It's the dominant paradigm, and will reign as such until someone comes up with a different theory of everything that isn't a failure.

The different theory will be a failure until it is developed far enough to not be a theory. String theory, like weeds, will starve off any new born alternative ideas long before they can be developed to the stage of providing a serious challenge to string theory. The intimidation (fear of abuse from string theorists) will in fact probably deter anybody from working on alternatives.

The reason why a lot of progress occurred very fast in quantum mechanics in the mid and late 1920s was because people could make progress *quickly*, without being bogged down in something speculative for decades! String theory is the exact opposite: attracting the opposite kind of people, who want long term stability in their careers. Non-falsifiability is turned to a career advantage, offering stability to physics.

33. **grad_student**
June 4, 2011

@Cosmonut
"But honestly, I think the whole landscape/multiverse fiasco shows that the dream of unifying physics purely through mathematical consistency – much hyped by Stephen Hawking – is dead."

This is the same thing as saying string theory is the 'only game in town'. How do you know that unification based on pure mathematical consistency is dead because of one failed attempt?

34. **Peter Woit**
June 4, 2011

Blaming the multiverse on putting too much faith in mathematical consistency is completely backwards. If you look at papers like Bousso-Susskind, or anything from Hawking in recent years, you find very little mathematics, just a lot of verbiage and a few trivial equations. This is what you get when you ignore the lessons of the deep connections between physics and sophisticated mathematics, and decide that you are going to pursue "physical ideas" uninformed by mathematics.

The reason string theory failed was not that it used too much mathematics, but that it was a wrong physical idea about fundamental physics.

35. **Cosmonut**
June 4, 2011

@Peter, @grad_student

Blaming the multiverse on putting too much faith in mathematical consistency is completely backwards.
——————————————

Ok, here's my understanding of the situation.

In the 1980's, the big hope – much hyped by the likes of Stephen Hawking – was that unifying GR and QFT would prove to be so mathematically stringent, that we would get one unique Theory of Everything which would predict the values of the fundamental constants, predict the masses of the particles (and know the Mind of God as a bonus 🙂).

But now, string theory claims to already be a quantum theory of gravity (see my question here: http://physics.stackexchange.com/questions/10754/is-string-theory-a-quantum-theory-of-gravity), which is consistent with both GR and QFT.

Yet, it allows 10^500 solutions at least, leading to the multiverse nonsense.

Hence, my claim that mathematical consistency with GR and QFT is not strong enough to give us an unique theory.
If string theorists are to be believed, there are 10^500 "theories" consistent with both !

Am I missing something here ?
For instance, is string theory not actually consistent with GR and QFT ?

36. **Cosmonut**
June 4, 2011

Just to clarify, I am not saying that mathematical consistency can never lead to an unified theory.

Its just that the string theory flop show suggests to me that our attempts at this point may be premature (just like Einstein's attempts were).

Maybe we need to create a few more layers of sub-theories using the old fashioned experiment-observation-theory route before purely mathematical constraints can give us the TOE.

37. **Bernhard**
June 4, 2011

Cosmonut,

The proper marriage of QM and GR, when it happens, should lead to a profound understanding of fundamental theory and when it happens I bet with you is going to be old fashioned in terms of mathematical rigor. The problem with ST, as Peter said, is that it is based on the wrong physical idea. The mathematical nonsenses coming out of the theory should actually serve as an alert that this is the case, but on the contrary, it made the proponents to abandon math and start the pseudo-science multiverse campaign. With this kind of attitude you can build any theory you want, since when you get in trouble you can advocate that, the crap solutions you´re getting out of your math, are in other universes.

38. **David Rod**
June 6, 2011

Highly recommend the recent book by Helge Kragh – Higher Speculations: Grand Theories and Failed Revolutions in Physics and Cosmology (Oxford University Press, 2011). It has excellent chapters covering many of the ideas expressed above.

39. **Zathras**
June 6, 2011

I second David's recommendation of Kragh's book. It really puts the ST enterprise, the multiverse, and anthropic principles in their proper historical perspective.

40. **John Baez**
June 7, 2011

I hope Peter writes a review of Kragh's book. I'm too cheap to fork over $63 for it right now, but I've long dreamt of *writing* a book about the history of "grand theories", and I hope this book covers the ground well enough so I can drop that ambition. For example, I hope it talks about Schrodinger's attempt at a grand unified theory, and also Einstein's various attempts. It would be nice to have a detailed analysis of these.

41. **Tim van Beek**
June 7, 2011

John wrote:

> I'm too cheap to fork over $63 for it right now, but I've long dreamt of writing a book about the history of "grand theories", and I hope this book covers the ground well enough so I can drop that ambition.

Until someone writes a review of the book, there is an article covering the period of 1914 – 1933 here:

Hubert Goenner: On the History of Unified Field Theories, living reviews.

42. **Greg Egan**
June 7, 2011

The Amazon page for Kragh's book has a look-inside function that enables you to browse the index and table of contents. There are certainly entries in the index for both Einstein and Schrödinger.

43. **Shantanu**
June 8, 2011

So john, you don't think proton decay will ever be detected?

44. **Bernhard**
June 8, 2011

Peter,

Did you review this book already?

http://www.theglobeandmail.com/news/arts/books/the-hidden-reality-parallel-universes-and-the-deep-laws-of-the-cosmos-by-brian-greene/article2050835/

45. **Peter Woit**
June 8, 2011

Bernhard,

I wrote a little bit about it here:

http://www.math.columbia.edu/~woit/wordpress/?p=3389

based on looking at the extensive "technical notes" available free on Amazon, and then took a bit more of a look at it when it came out. Capsule review: the usual pseudo-science and lack of any possible way to test it as every other one of the endless number of multiverse books, but better writing and presentation.

I haven't read Brian's book (or some of the other recent such book like Gribbin's and Manly's) carefully and written a detailed review partly because I'm too bored and discouraged by these things to put more time into thinking or writing something substantive about them. By now I've repeated time and again the same obvious arguments about why these things are problematic pseudo-science, for a recent example, mentioning the book, see here:

http://www.math.columbia.edu/~woit/wordpress/?p=3419

There's another similar book coming out soon, Barrow's "The Book of Universes". When I see a copy of that one, maybe I'll write something, maybe not.

These things are atrocious, but one consolation is that I think the whole "multiverse" thing is now getting over-exposed. Publishers want to sell something "new" and this stuff is getting old fast.

46. **DGP**
June 9, 2011

This is rather belated I know, but I wasn't going to read the NS "Instant Expert" item about String Theory until I saw the comments about Duff's rather polemical last page. I am surprised NS let that through since "Instant Expert" is supposed to be informative not controversial.

I asked a mathematician friend for his opinion. He said he had stopped reading papers and articles about String Theory. He did take a look, however, and here is his reply:
http://xkcd.com/171/
Outside the string coterie, there seems to be increased cynicism about their claims.

Duff seems to have reverted to full adherence to the String Party line recently. His opinions seem to have been more tolerant of criticism. In September 2010, in an article on his work on entanglement he said:
"We have nothing to say about whether string theory is the theory of everything." (From http://www.sciencenews.org/
Home / News / September 25th, 2010; Vol.178 #7 / Article String theory entangled .)
Admittedly, that was in the particular context. Even so, the change of tone is remarkable.

DGP.

47. **John Baez**
June 11, 2011

Tim wrote:

> Until someone writes a review of the book, there is an article covering the period of 1914 – 1933 here:
>
> Hubert Goenner: On the History of Unified Field Theories, *Living Reviews*.

Yes, I really liked this article. I should reread it! For people who have followed the recent struggles to find a unified theory of physics, it's very thought-provoking to look back at these earlier attempts. For example, the problems with media hype today remind me of Einstein's criticism of the premature publicity Schrödinger engaged in. I'm not finding the exact quote now, but it was quite biting. I'm only finding this:

> In January 1947 he believed he had made a major breakthrough in Unified Field Theory. He read a paper at the Royal Irish Academy on 27 January entitled The Final Affine Laws. Schrödinger presented it to the Academy and to the Irish press as an epoch-making advance. The Irish Times carried an interview with Schrödinger the next day in which he said:-
>
>> This is the generalization. Now the Einstein Theory becomes simply a special case… I believe I am right, I shall look an awful fool if I am wrong.
>
> Einstein, however, realised immediately that there was nothing of merit in Schrödinger's 'new theory'
>
> Before he received Einstein's opinion, he was attempting to offer an explanation for the 'exaggerated' newspaper reports. Soon afterwards, Einstein stopped corresponding on the subject and no communication between the two was made at all for three years.

Shantanu wrote:

> So john, you don't think proton decay will ever be detected?

I have no idea. I wish something like the SO(10) GUT would turn out to be true, because it so neatly packages all the fermions in each generation into a single irrep. But I'll be happy if people make *any* sort of significant progress on understanding elementary particles.

The Quantum Story

June 8, 2011
Categories: Book Reviews

Jim Baggott's The Quantum Story: A History in 40 Moments is now out here and I've been starting to see it in bookstores. I read most of it a year or two ago when he sent me a draft of the manuscript asking if I'd take a look at it, and very much enjoyed getting the chance to see it then. If you're looking for an excellent popular level physics book to read, I highly recommend that you consider this one, which should be accessible to just about anyone, no matter what their background.

The topic of the book is the story of quantum physics in general, told historically with a structure of 40 vignettes. The first three chapters cover mainly events of the dramatic period of the mid-1920s to early-1930s during which physicists uncovered the basic structures of quantum theory and struggled to make some sense of them. The next two take on the late-40s to mid-70s during which a long succession of discoveries about elementary particle physics drove theoretical progress on quantum field theory and gauge theory, culminating in the Standard Model falling into place in 1973. The material Baggott works with here has been the topic of many other books, but he does a wonderful job of putting it together in a fast-paced but very clear and entertaining narrative. Along the way, the individual stories he tells often contain fascinating details I'd never heard before, even though I thought this was a subject I knew all too well.

The next to last chapter starts with the 1950s and David Bohm, picking up the thread of later debates and discoveries related to the general problem of the interpretation of quantum mechanics. It brings this story up to date, explaining some of the current questions that are still being debated. The final chapter gives an appropriately short discussion of speculative ideas in quantum gravity and string theory that have dominated theoretical research for the past few decades, making clear that they're a long ways yet from the solid science that's the main topic of the book.

The LHC and the search for the Higgs make up a final epilogue or 41st vignette, accurately describing the high expectations and drama that surrounds the final period of the long wait for new data that is finally coming to an end this year. The story of quantum theory is not a finished one, and we all hope that very soon we'll get some clues as to where it will go next.

Comments

1. **Steve**
   June 11, 2011

   Thank you for recommending this book!

2. **Eric habegger**
   June 13, 2011

   This is a sensational book and i'm about 2/3s through it right now. Even though it doesn't focus in and concentrate on narrow subject matter within the history of quantum physics it still describes those individual discoveries better than 90 percent of more narrow focus books. One really comes away with the feeling of an author who not only writes well but also knows how physics well.

3. **Andrew Daw**
   June 28, 2011

   Having read Jim Baggott's excellent *Beyond Measure: Modern Physics, Philosophy and the Meaning of Quantum Theory* I'd better read this one too.

   (Long live the nonlocal real world revolution!)

This Week's Rumor

June 8, 2011
Categories: Experimental HEP News

A couple months ago CDF made the New York Times by releasing results claiming to see a resonance in the invariant mass spectrum of two jets produced together with a W. Last week they released a new analysis with twice as much data, claiming the signal was still there, now at a statistical significance of nearly 5 sigma.

I recently wrote about this here, explaining the reasons for being skeptical, despite the high statistical significance. One very good reason for being a skeptic is that CDF's Tommaso Dorigo doesn't believe this is real, going so far as to put his money where his mouth is, offering a $100 wager to back up his arguments. The crucial question in everyone's mind has been whether D0, CDF's competition and sister detector at Fermilab, would see the same thing in its data. If there's really something there, D0 should see it.

This Friday there will be a Wine and Cheese talk at Fermilab, where the D0 results will be unveiled, and you can watch this as a live video stream here. But, as one might expect, now that the D0 result is ready to be revealed, people do things like leave print jobs on printers, etc., causing well-sourced rumors to spread. Blogs such as this one seem to be a place where such information tends to end up, so I can report a rumor (based on excellent sources) that Tommaso is right. D0 will report on Friday that there's nothing there, that they find no evidence for a dijet resonance in the region from 110-170 GeV. They reject the CDF hypothesis of a resonance with a cross section of 4 pb at a significance level of over 4 sigma.

In other news, the LHC is running very well, with the official goal of this week being to reach an integrated luminosity of 1 fb^{-1}, something that had been the official goal for the entire year (although, unofficially, 2-3 fb^{-1} is more like it). Right now, they're around .8 fb^{-1}. This sort of luminosity should finally start to allow in coming months results that either rule out a Higgs in the region it is expected or see first indications if it is there.

At the KITP, this week marks the start of a program on The First Year of the LHC. Unfortunately for theorists, the only result of data from the first year of the LHC has been to shoot down some of their favorite models, ruling out for instance a large amount of the parameter space where supersymmetry was expected to be found, making the most popular theoretical idea of the last thirty years significantly less popular. The first talk held at the KITP program was this afternoon, and it dealt not with the LHC data, but with the supposed CDF resonance (it appears that news of the D0 result hadn't yet made it to Santa Barbara).

Update: The KITP talk is now available here.

Update: The D0 PRL submission that has been circulating privately for the past few days is supposed to be available at 9am Friday Fermilab time here. See here for other material to be released publicly today.

Update: Now that the D0 results are officially out, as usual your best bet for informed explanation is to check out what Jester and Tommaso have to say.

Comments

1. **Anonymous**
   June 9, 2011

   As always, the devil will be in the details. When D0 concludes no resonance, one should ask compared to what. An interesting D0 thesis on W+2 jets is http://www.slac.stanford.edu/spires/find/hep/www?r=fermilab-thesis-2010-48.
   Same luminosity as in D0 talk on friday, different cuts.
   Starting at pg 101, a procedure called reweighting the theory prediction is explained. Of particular interest is fig 44 (the dijet invariant mass before and after reweighting). The crucial thing to look for during Friday's D0 talk is how does the dijet mass distribution compares to background estimates (theory prediction+detector simulation) before reweighting. Only then do we compare apples to apples. Everything after that, including reweighing, is interpretation and will be open to discussion…..

2. **anon.**
   June 9, 2011

   The first talk held at the KITP program was this afternoon, and it dealt not with the LHC data, but with the supposed CDF resonance (it appears that news of the D0 result hadn't yet made it to Santa Barbara).

   Everyone expects D0 will say they see nothing. This doesn't mean we shouldn't be talking about the CDF resonance. It's raised a lot of questions — how do we know when to believe signals that come from jet physics? How can you successfully make a case that a peak is real if it's a wiggle on a steeply falling background? What can we do to convince ourselves something is new physics, if even high statistical significance isn't a good guide? (See also: pentaquarks.) Even if D0 rules it out so decisively that everyone is convinced, we'll still have the puzzle of what went wrong in the CDF analysis, and how to keep it from going wrong again. None of the potential mismodeling explanations seem entirely convincing.

   The LHC is doing amazing work slicing away parameter space using missing ET or leptons, but if new physics shows up in very jetty channels, the same issues raised by the CDF analysis are going to be important. Whether or not the CDF bump is real (and even the most rumor-happy phenomenologists in the world tend to think it isn't), getting theorists and experimentalists to talk seriously (hopefully to each other) about jets is an important thing.

   Having watched the video of the KITP talk, it's very much in the vein of "let's try to figure out what the data are saying and what could be causing this," not a premature celebration of new physics.

3. **factotum**
   June 9, 2011

   CDF measurement has been scrutinized for 2 months. Why should we take D0's word as final without subjecting it to the same level of scrutiny?

4. **anon2**
   June 9, 2011

   @factotum

   The D0 analysis is not new. They have been looking at this data as long as CDF has. This is, at least, what I've heard from a D0 member directly involved in this analysis.

   @Anonymous

CDF does not need to tweak the Monte Carlo generator knobs to match the data control distributions because they make more strict kinematics cuts than D0. If D0 presents the results with an analysis strategy similar to CDF's, the effect of the so-called "reweighing" (which is a really bad name) will be negligible.

5. **Anonymous**
   June 9, 2011

   @anon2: then let's hope D0 leaves the kinematic adjustment (=reweighing) issue out of this analysis. This will give us a clean comparison between CDF and D0.

6. **Bernhard**
   June 9, 2011

   @ anon.

   "What can we do to convince ourselves something is new physics, if even high statistical significance isn't a good guide?"

   Now that D0 is refuting the peak, I see two possibilities on the table. Either D0 screwed it up and is not seeing the peak because it overestimated the systematics and erased the signal in a sensitive high-background region or CDF is suffering from a subtle detector effect, so a subtle systematic effect that D0 does not suffer from and so does not see the peak. Either way, there is no chance (or a extremely tiny one) that the much better ATLAS and CMS detectors with better resolution eta coverage etc will suffer from both. Given enough data, it will be clear if there is something there, but to be sure this will take more than 1fb-1 I'm afraid. And imagine that both ATLAS and CMS could suffer from the same supposed systematic effect that CDF could be suffering from at the same time is very unlikely. So if they both see the peak, case closed. The apocalyptic scenario would be ATLAS seeing the peak and CMS not but I see no reason to imagine that such a bizarre scenario like that would happen.

7. **tommaso**
   June 9, 2011

   I like a lot this thread -very informed. Interesting points are raised. In particular, the reweighting technique is indeed the crucial point IMO.

   Now all please stop and think. What do we do when we put a Monte Carlo simulation under our data ? We are comparing previously tuned simulations to newer data, nothing else. The parton shower simulation that is used to go from matrix element calculations to final state observables contains knobs that are turned to match the simulation output to large datasets of well-understood physics processes. Every time something matches poorly, one asks oneself if NP is the cause or a mismodeling issue; once the NP interpretation is disproven, the simulation receives a further tuning.

   In this case, what I think I can say is that W+jets production is a rather well-known and well-understood process, but its detailed kinematics had not been tested to this level of statistical precision before. So we are in a regime where mismodeling issues may arise. And they do. If a reweighting appears to be all what's required to match the data, Occam's razor comes a-slashing the pet signal that CDF published.

   Peter, thanks for the mention of the bet -but I guess that by now nobody else will take it. On the other hand, it is not a single $100 bet: I made available 20 tickets. 19 are still there, so if you believe the CDF signal will eventually survive, take one. Say so in my blog.

   Cheers,
   T.

8. **Bernhard**
   June 9, 2011

   Tommaso,

   If this is the case, one should expect different mismodeling effects from different Monte Carlos, don't you agree? I mean even if you have all generators needing some reweighting in that region to fit the SM one could expect the tunings not to be the same. And the shape and even how much the peak is pronounced would probably be generator dependent as well, right?

   Also, I'm not sure I understand for example how could CDF and D0 apply such a different reweighting… The reweighting factors should be taken from a control region (not where the peak is or is not) where one would expect both detectors to agree very well so the final tuning (for each generator) would be similar. How can this, in the end cause, such a dramatic difference? Is hard to believe CDF would make such a fuzz on something that can be corrected by a simple reweight factor. What they are claiming is a genuine peak, something that cannot be corrected by scaling some curve up. So, a subtle detector effect seems more likely IMHO.

   What did I miss?

9. **Ken Lie**
   June 9, 2011

   I want to share with you a rumor which is discussed in an informal way in Munich. The source of this information is in the theory group of CERN.
   The LHC has seen a particle of energy 150 GEV which is not Higgs (!!)
   Apparently, the result will be confirmed in 2-3 weeks.

10. **Bernhard**
    June 9, 2011

    Ken Lie,

    "The LHC" didn't see anything .

    Which experiment?

11. **DGP**
    June 9, 2011

    I used to think I knew something about experiment design but I am awed by the difficulty of the background subtractions involved in these latest experiments.

    I assume that all proper precautions have been taken to avoid a systematic error in the background: I know that more than one Monte Carlo code was used but are the algorithms and especially the pseudorandom number algrothims fully independent? And what dependence is there on assumptions about QCD? QCD is very good but it's not quite as solid as QED.

    I will be watching the contributions from the other experiments with great interest.

DGP.

12. **Walter**
June 9, 2011

@tommaso: exactly! However, at this point the question is not new physics or not.
Understanding this potential mismodeling in terms of the theory predictions will extend beyond CDF and D0. After all ATLAS and CMS use the same tools.
This point was very well made in anon's first comment. If the deviations from the theory expectation are subtle, how can we conclude there is new physics? The CDF bump, no matter its origin, is a perfect case against which this can be discussed.

13. **chris**
June 9, 2011

DGP,

random number generators have matured substantially over the last 2 decades. with all the modelling of e.g. hadronization in there, RNGs are the last thing i would be concerned about.

14. **DGP**
June 9, 2011

Chris,

Thanks. I know RNGs have moved on since I modelled scattering but there was always a tendency for everyone to use the best, i.e. latest, RNG so independence was always an issue. I thought that might still happen.

I assume that by "hadronization" modelling you mean the sort of consideration I meant when I referred to the assumptions in QCD.

Regards,

DGP

15. **Peter Woit**
June 9, 2011

anon.,

I didn't intend to suggest that phenomenologists at the KITP or elsewhere were optimistic about the CDF signal, just thought it was an interesting sign of the times (the times being this month…) that at a workshop supposedly devoted to the LHC results, the hot topic was a Tevatron topic. I did listen to some of the talk last night, and noticed that if the speaker or those in the audience knew what the D0 result was about to be, they were keeping quiet about it…

Ken Lie,

Are you sure this isn't a garbled version of the Tevatron story about a supposed 150 GeV state discussed here? I haven't heard any rumors about either of the LHC experiments seeing a 150 GeV state. If there are more details though, pray tell…

16. **factotum**
June 9, 2011

@anon2: CDF looked internally for over a year, then their result has been subject to 2 months of public scrutiny

Sure, D0 may have had the internal studies for a long time. But they should be subject to the same questioning and testing from the broader community, just as CDF has, and not just be taken immediately at their word..

17. **Philip Gibbs**
June 9, 2011

Peter, the new LHC results are being presented at PLHC2011 this week. There are ten talks from ATLAS and one from CMS that cover new results using 2011 data. All the plots appeared in conference notes last week so we already know they just show new exclusions, not new physics. D0 are also presenting Wjj there on Saturday.

18. **tommaso**
June 9, 2011

Dear Bernhard,
not necessarily different MC would give different outputs. If you take Alpgen matrix element calculations and add a Herwig parton shower evolution, and then take Sherpa and add a Herwig parton shower evolution, the parton shower is the same.
Further, the reweighting issue involves quite a few choices that require craftmanship. Experimentalists are proud of this part of their job, which is in fact what distinguishes them from a piece of computer code (a simulation of the typical particle experimentalist is not so hard to write, anyway). I think this is enough to understand why different experiments can see and do different things.
As for a detector artifact, I doubt it. The detectors are very, very well understood by now.
Cheers,
T.

19. **Peter Woit**
June 9, 2011

Philip,

I noticed that the most interesting of these talks, the Friday ones with some results from 2011 data, have now been posted, see

http://indico.cern.ch/conferenceOtherViews.py?view=standard&confId=100963

The bottom line: SUSY and extra dimensions are in even more trouble. No hint of the Higgs yet, but they're getting closer. If it's there it could easily start to show up in the data they're analyzing now, trying to get ready for conferences in late July.

20. **Bernhard**
June 10, 2011

Dear Tommaso,

Thanks. I agree with you that if one has the same parton shower, the output can be almost the same.

As for reweighting, you are probably talking about something more sophisticated than I thought. I naively thought you were just talking about the procedure of choosing a control region (let´s say a dijet QCD region for a final state with no leptons) that you know well the MC should reproduce it and if the data gives say 100 events and your MC just 85, one multiplies this MC and all other curves by a factor of 1.17… This should be fairly simple to do…

As for the detector effect, you maybe right, but we never know for sure… 🙂

21. **anon**
June 10, 2011

to ken lie :
i can say that there are quite a lot analyses which have some sort of large disagreement with expectation. mc modeling sometimes is not reliable which people might not realize in some special cases. 2 or 3 sigma is easy to see, what's interesting is if this is a modeling problem, people would see significance growing as more data come in, which resembles what people expect if there's a real signal. if this happens to a *benchmark* analysis, rumor may spread easily.

22. **model and sim**
June 10, 2011

I was curious, for the modellers out there, as the means and distributions are changed in the monte carlo model, I assume that someone is recording those changes in assumptions. Has anyone done analysis on those changes as part of a dual solution check?

23. **M**
June 10, 2011

there is a new rumor from LHC…

24. **Anon**
June 11, 2011

Tomasso says: Further, the reweighting issue involves quite a few choices that require craftmanship. Experimentalists are proud of this part of their job, which is in fact what distinguishes them from a piece of computer code (a simulation of the typical particle experimentalist is not so hard to write, anyway). I think this is enough to understand why different experiments can see and do different things.

Hmm, as a theoretician reading the last sentence, it doesn't sound like something to be that proud of 🙂

25. **Peter Woit**
June 11, 2011

M,

I'm hoping there's finally enough luminosity for rumors to start about seeing something with a plausible cross-section to actually be the Higgs. Or, at least there should be a supersymmetry signal rumor, I'm surprised there haven't been any of those yet. Let's hear some more details…

26. **tulpoeid**
June 12, 2011

Oooooh, this is starting to border begging, guys…

27. **d0 compared to cdf**
June 16, 2011

This is still a little prelimary, but after pulling apart the distributions, the principle reason why cdf sees a particle and d0 does not appears to be correlated with cdf favorings top quark events over w events. Because the top event distribution has a different peak than the w event distribution, the difference in proportionality appears to cause a deficit in the region were cdf claims to see a new particle. There also appears to be a shift in distributions related to choice of bin size which is also having a lesser effect.

The underlying distributions for the different types of events appear to be largely in agreement between the two teams, but CDF definitely appears to favors more events as being top quark events as compared to D0

28. **Peter Woit**
June 16, 2011

d0 compared to cdf,

Thanks for the informative comment. People interested in this should also look at Michael Schmitt's blog entry here

http://muon.wordpress.com/2011/06/12/top-background-shapes-and-the-cdf-mjj-anomaly/

29. **d0 compared to cdf**
June 16, 2011

Thanks,
One of the things I am hoping to figure out is why there is a deficit at 65 GeV in the D0 analysis that appears to have harmonics. One can enhance the harmonics and transform into the t-domain and see two distinct events each half cycle. The same deficit at 65 GeV is apparent in the CDF data, but the CDF error interferes with the harmonics. If I can isolate the nature of the error, it might be possible to see if the harmonics where actually enhanced in the 7.3 fb^-1 data.
I am sure there is a logical explanation for the harmonics, it could just be a fluke.

New Look

June 9, 2011
Categories: Uncategorized

I've updated the blog a bit, to a newer, widgetized theme. Functionality should be the same as before. The only issue I'm aware of now is that the "Archives" widget adds an annoying character below its header which I can't get rid of. This widget may also be the source of problems some people are having (due to some combination of Javascript and incompatibility with latest WordPress version), it may get replaced…

Please let me know of any other issues.

Comments

1. **Anon**
   June 9, 2011

   Slick! I like it.

2. **Mike**
   June 9, 2011

   Like the new look!

3. **Chris Austin**
   June 9, 2011

   What is meant to be below the Archives header? I am using Konqueror in KDE 3.5 (Debian Lenny), admittedly a bit dated now, and there is just a short vertical line under the left of the A, and then nothing until the Links header, just below that.

4. **Peter Woit**
   June 9, 2011

   Chris,

   You should be seeing links to old postings, by year. I just changed the settings so that this should work if you don't have javascript, does it work now?

5. **Chris Austin**
   June 9, 2011

   No, it's still the same, (after several refreshes). However I can see the links to the archives in Iceweasel, with nice + boxes to click on to expand the menus. And the menus correctly hide again when you click the – boxes in Iceweasel. I shouldn't worry about the issue with Konqueror, which still has some Javascript issues in KDE 3.5. I had similar issues trying to get a similar show / hide effect to work in Konqueror with Javascript on a website. Getting it to work in Konqueror requires arranging the Javascript code in very particular ways that should not be required according to the Javascript definition. The Javascript implementation in Iceweasel is much more robust.

6. **Chris Austin**
   June 9, 2011

   I should have added that Iceweasel is simply Debian's name for Firefox.

7. **UnderlyingEvent**
   June 9, 2011

   Great, the same content as before except now I have to wait a finite time for it to load.

8. **Peter Woit**
   June 9, 2011

   I've reverted the javascript behavior change, since that didn't help Chris, perhaps this will improve performance.

   As far as I can tell, the new theme/widgetization shouldn't affect the time to load signifcantly, since you're getting pretty much the same data, processed the same way. I'm guessing the problem is wp-dtree that handles the archives, and comes in a rewritten version. If anyone has a suggestion for a replacement for that, I'd be interested to hear about it.

   And if people complain too much, I'll contact Lubos and get the info from him about how to install all the widgets he's got running on his blog…

9. **Lukasz Grabowski**
   June 10, 2011

   For me the top picture is now annoyingly huge.

10. **jharvey**
    June 11, 2011

    I do read the comments. Would it be possible for you to number them? That would make it easier for me to keep track of where the previous last one was. Thanks.

11. **Aaron Davies**
    June 11, 2011

    @chris: i think the pipe on the line after "Archives" and before 2011 and its plus/minus box is the "annoying character" in question.ah, i see what's going on–that pipe is supposed to separate two buttons which aren't being rendered properly:

 |

note the two empty <a> tags.(what you should do about this, i have no idea, but hopefully this will set you on the right track.)

12. **Peter Woit**
June 11, 2011

Thanks Aaron, that explains it, by default the there are "Open All" and "Close All" button, but with blank text. Added text, at least now it makes sense.

jharvey,

I'll look into that, but one problem is that comments get added/deleted a lot for various reasons, making any numbering system not necessarily stable.

13. **Visitor**
June 12, 2011

The text column of the comments is now wider than previously; so wide, in fact, that it is far less comfortable to read.

Math and Physics, Summer 2011

June 9, 2011
Categories: Langlands

This week in Philadelphia the String-Math 2011 conference is going on, planned as the first of a series, with String-Math 2012 next summer in Bonn. Slides of the talks are appearing here. There's also supposed to be video, but the saved video seems to require some sort of UPenn login, and I've not been able to get the streaming video to work. The public talk by Cumrun Vafa puts out the classic message that strings have come to the rescue of physics, unifying QM and gravity, and that:

> Smooth geometry of strings seems to explain all known interactions (at least in principle)

The techinical talks cover a lot of ground, much of it having little to do with string theory. Michael Douglas's talk surveys problems related to finding non-perturbative formulations of quantum field theory that one might hope to say something precise about, but it contains a lot more questions than answers. I'm most curious about David Ben-Zvi's talk tomorrow, so hope that slides or video of that will be available.

The circle of ideas relating gauge theories, geometric Langlands, TQFTs and representation theory will be getting even more attention than the mathematics of string theory this summer. In a couple weeks will begin a two-part program at Luminy and then Cargese on Double Affine Hecke Algebras, the Langlands Program, Affine Flag Varieties, Conformal Field Theory, Super Yang-Mills Theory. I don't know who the author is, but some person or group has written up for the occasion a wonderful summary of the current activity in these and related fields of mathematics, see here. Next month, the KITP will be hosting a program on Nonperturbative Effects and Dualities in QFT and Integrable Systems that will cover some of the same topics.

In some other unrelated news, if you understand French, you can listen to an interesting set of interviews with Pierre Cartier here. Finally, it was announced recently that my colleague Richard Hamilton is sharing this years $1 million Shaw Prize for Mathematics with Demetrios Christodoulou. Congratulations Richard!

Comments

1. **Igor Khavkine**
   June 10, 2011

   Hmm, Michael Douglas's talk is interesting. However, from his remarks about cluster expansions, I wonder if he is overlooking potential interaction with the work on position-space renormalization that has been continuously improved since the contributions of Epstein and Glaser in the '70s.

2. **Daniel L. Burnstein**
   June 10, 2011

   Just a note to let you know that the video works fine from my computer.

   DLB

3. **Peter Woit**
   June 10, 2011

   Thanks DLB,

   I'm also now able to access the archived video as well as the stream (using VLC for the latter).

4. **Fabien Besnard**
   June 10, 2011

   Thanks for the link to Cartier's interview. The first part is missing on the link you provided, but it can be found here.

5. **Fabien Besnard**
   June 11, 2011

   Sorry about the preceding link : it says it is the first part of Cartier's interview but it has nothing to do with it ! I'll see what I can do with the people at France Culture.

6. **Thingumbobesquire**
   June 11, 2011

   Bye the bye more hype from the stringers today: http://www.ia.ucsb.edu/pa/display.aspx?pkey=2509

7. **Peter Woit**
   June 11, 2011

   Thingumbobesquire,

   Well, at least that's the first string theory hype press release I've seen in a while that doesn't claim discovery of a way to test string theory…

8. **Tim van Beek**
   June 12, 2011

   A nice talk by Michael Douglas. I think it is understandable that it is biased somewhat towards the work that he himself is involved in. I'd liked to hear a little bit more about axiomatic quantum field theory, and also about the work on making Feynman integrals rigorous. BTW, people in AQFT usually work with the Haag-Kastler axioms, not the Osterwalder-Schrader axioms as Douglas implied. Also, the axioms have been fully generalized to Lorentzian manifolds quite some time ago, see for example this paper on the arXiv.

$6.5 Million for Entropic Gravity

June 12, 2011
Categories: Uncategorized

One of this year's Spinoza Prizes goes to Erik Verlinde. It comes with 2.5 million euros to fund the prize-winner's research. Last fall Verlinde received a 2 million euro ERC Advanced Grant to fund his research program, so that's a total this past year of 4.5 million euros, or about $6.5 million.

Verlinde's current research focuses on ideas about "emergent gravity" (see here and here). According to Wikipedia his work explains the observed value of the cosmological constant.

I've no idea how Verlinde will spend the money, but it looks like emergent gravity research will be extremely well financed. $6.5 million I'd estimate corresponds to about 100 postdoc-years. In a couple weeks Verlinde will unveil his latest work at Strings 2011. Since that's among the most expensive conferences around (see here), perhaps he could chip in to fund it. I'd estimate he should be able to single-handedly fund Strings 20XX through at least 2050.

Comments

1. **anon**
   June 12, 2011

   A scientist that already has a very good, well payed, tenured job, has no need for that type of money. Doesn't Holland want to improve the world and its own country and science? This certainly is not a very efficient way of doing it. If this prize is supposed to increase the prestige of Holland, it has the opposite effect. Now I think the Dutch have their head up their ass.

2. **Anon**
   June 13, 2011

   Verlinde solved the CC problem!? News to me!

   He does very interesting work in more speculative directions – the Dutch seem to be good at original thinking in Physics – but really, I have to agree with anon. I doubt this kind of investment in one well-established secure person is justified.

3. **Octoploid**
   June 13, 2011

   Really, all you guys just sound jealous.
   For me it's a sign of civilization to be able to spend this amount
   of money on a guy whose work is purely theoretical.
   I may not agree with his results, but I think he deserves congratulations
   and not snippy remarks.

4. **Fritz**
   June 13, 2011

   Read the announcement two days ago at the hammock physicist (a Dutch tax payer I presume). He is critical about the way the Spinoza prize committee motivates their choice, but enthusiastic about the choice itself: http://www.science20.com/hammock_physicist/blog/entropic_gravity_snatches_spinoza_prize-79902

5. **Tim van Beek**
   June 13, 2011

   > $6.5 million I'd estimate corresponds to about 100 postdoc-years.

   Verlinde will hardly be able to mentor that many postdocs, but if it turns out that the money is spent on postdocs, that will result in roughly 300 papers, I'd guess? The problem is, that postdocs will have to publish wether they have something to say or not, adding a lot of noise to a topic that already shows severe stress due to overpopulation.

   If Verlinde would like to spent the money on a software project instead, 4.5 million Euros are worth roughly 23 man years (good software developers seem to be more expensive than physics postdocs). That would be enough to start a second Twitter or Facebook. But the problem is the same, of course: One needs a good, a very good idea for people to work on 🙂

6. **Yatima**
   June 13, 2011

   >One needs a good, a very good idea for people to work on 🙂

   But it doesn't work that way.

   In research, as in real life, you at first work on something, and then it may turn out to be "good" along a few of several dimensions. As usual, luck is 80% of it. Facebook or Twitter might never have made it for thousands of reasons; they still might flounder in fact once the hype about advertisement-driven revenue lets up.

7. **Bugsy**
   June 13, 2011

   I agree with Yatima.
   Though it is good to see money go to research in any form, the money would be well spent for instance by the recipient not on research postdocs focussed on one possibly valid idea, but rather on more general support for struggling, honest, independent-minded, creative, and not well known researchers, at times overwhelmed by the stress of trying to job-search, teach, and have some sort of family life in the midst of an often commercial, superficial and ruthlessly competitive world. That is, to fund real science…. and real, stable positions which provide some level of security and some time just to happily think.

8. **chris**
June 14, 2011

oh, come on. 4.5ME is not that much. it's equivalent to about 3 permanent positions. if the work is computer intensive though it will just pay you a few years decent computer time and a few postdocs.

9. **Anonymous Coward**
June 14, 2011

Let me clarify this one bit: the Spinoza prize is both an accomplishment and an encouraging award. Its purpose is to provide academic freedom and financial means to the best, not necessarily Dutch, scientists in the Netherlands. Recipients are next to completely free to spend it as they see fit. For instance, Verlinde's Amsterdam colleague Robbert Dijkgraaf set up an educational project for school children (http://proefjes.nl/).

10. **Peter Woit**
June 14, 2011

chris,

From what I can tell, the main funding for the most computationally intensive area of particle theory (lattice gauge theory) for the entire US comes to about $2.2 million/year

http://www.scidac.gov/physics/quarks.html

so Verlinde should definitely be able to afford some decent computer time. Maybe his new work changes things, but it seemed to me that his previous work on this didn't allow any possibility of computing anything, so costs of computer time are pretty irrelevant to him.

11. **Anon**
June 14, 2011

I am sorry, but I cannot agree with irresponsible throwing money around. Theoretical Physics is so criminally underfunded that to give the money away to some project that has little if any chance of ever advancing fundamental Physics at all (Dijkgraaf's schoolchildren project) just because your own position is nice and cushy, compounds the crime. It is certainly a subversion of the intent with which the money was given.

12. **Pachu**
June 14, 2011

Im available as software architect/developer for Verlinde 🙂

13. **anon**
June 14, 2011

Only problem is that Verlinde's work involves no software. Also, string theorists and computer programmers (e.g., ipad application developers) aren't exactly dying to collaborate with each other

14. **Michael McGuigan**
June 14, 2011

There is a small intersection between string theory and computing, for example to test the AdS/CFT correspondence numerically at intermediate coupling.
An upcoming workshop:
Novel Numerical Methods for Strongly Coupled Quantum Field Theory and Quantum Gravity
http://www.kitp.ucsb.edu/activities/dbdetails?acro=novelnum12
has a description that says:
"Conformal or nearly-conformal large N quantum field theories, supersymmetric lattice gauge theories, out-of-equilibrium QFT, formation and evaporation of black holes, spacetime foam, and ideas for emergent gravity, all illustrate the importance of the development of new or improved methods for studying strong coupled theories. As computers steadily become more powerful, the efforts of many people using numerical techniques to study these problems are becoming increasingly fruitful."

15. **Patrick**
June 15, 2011

Not being a a "theoretical" physicist, I disagree with Anon's remark. There is nothing "criminal" about funding levels in general and certainly nothing "criminal" about the percentage spent on "theoretical" areas. The quotes above are because it is not clear (a) what is "criminal" nor (b) what speculative ideas (to be polite) ideas can be considered to be "theoretical". Is the argument to be that we should fund every crackpot idea especially if it cannot be falsified? If an individual with sufficient funds wishes to do so…well it is his money. As a taxpayer, I expect public funds to be held
hostage by conscensus.

16. **Anon**
June 15, 2011

Well, if you can come up with a better adjective than "criminal" when comparing the amount of money spent on research (of all kinds, but including theoretical) with the amounts spent on the military, for example, I am all ears. And I am using Theoretical Physics in the old sense, not as a "String speculations". Maybe if we hadn't lost the majority of young Ph.D.'s over the past 20 years due to lack of funds, someone might have come up with better ideas by now. I personally know a number of these lost Physics Ph.D.s, some of them of great caliber, whose loss to the field is a crime, I can promise you that.

Also, I agree that private foundations can spend money on advancing whatever speculative ideas they want. What I called "criminal" was Dijkgraaf's subverting this intent by giving the money away.

17. **Anonymous Coward**
June 16, 2011

I think this thread has exceeded its usefulness, but assigning the adjective "criminal" to Robbert Dijkgraaf is absolutely silly. A long-time popularizer of science in general in the Netherlands, he is currently doing a great job as president of the Royal Dutch Academy of Sciences, constituting a pillar of reason and enthusiasm in times of major budget cuts. He's done more good for science than the next young post-doc probably ever will.

Besides, the Spinoza prize doesn't carry a label "money for theoretical physics"; if Dijkgraaf or Verlinde wouldn't have gotten it, they would have given the prize to a historian, medical researcher or whatever.

18. **Peter Lynds**
June 16, 2011

    Just crazy.

19. **maciej**
June 17, 2011

    Wow! Quite a lot of money considering the fact that the argument (that gravity might emerge from some more fundamental statistical system) does not even belong to Verlinde but to T. Jacobson (starting from the 95′ paper http://arxiv.org/abs/gr-qc/9504004).

    Yeah, but Jacobson does not publish papers about string theory unlike Verlinde – this explains the artificial hype.

How the Hippies Saved Physics

June 13, 2011
Categories: Book Reviews

A review that I wrote of David Kaiser's [How the Hippies Saved Physics] is now available at [American Scientist]. A quick summary is that I think it's a marvelous book, telling in well-researched and entertaining fashion a story I've always wanted to know more about. I'm not convinced though by the main argument of the title, that this group of people "saved physics", rescuing it from an oppressive "shut up and calculate" ideology by showing the way towards the importance of Bell's theorem and helping start the field of quantum information theory. Perhaps the author though is just emulating his subjects, known for their playful outlandishness.

There are quite a few interesting things I learned from the book that didn't make it into the review. One example is the story of Werner (of EST fame) Erhard's theoretical physics conferences of the late 70s and early 80s, organized in collaboration with Sydney Coleman and Roman Jackiw. Among the factors that brought these events to an end was the advent of string theory: it was felt that no string theory conference without Witten attending would be taken seriously, and by then Witten wanted nothing to do with EST and its founder (although he had attended, with the likes of Feynman and Weinberg, the earliest conference in the series back in 1977).

If you find this subject at all interesting, I highly recommend the book.

For another take on the same subject, from one of its main participants, Jack Sarfatti's memoir *Star Gate* is available for free these days in a pre-publication version here.

I'm afraid that my own description of where the physicists described in Kaiser's book ended up would not be the field of quantum information theory, but the much larger swamp of dubious claims about quantum physics that is still very influential. For example, this week at the AAAS meeting in San Diego there's a session on Quantum Retrocausation, see this listing from the World of Parapsychology.

Update: I should also mention that Chad Orzel discusses the book here and here.

Comments

1. **Dave Miller**
   June 13, 2011

   Peter,

   EST has metamorphosed into "Landmark Education," which seems to be basically the Rosenberg family business and which has developed a rather unique "business model." ("Werner Erhard" was really Jack Rosenberg – supposedly the "Werner" came from Werner Heisenberg and the "Erhard" from Ludwig Erhard.) I won't go into details about Landmark (I have some friends deeply involved in it) except to say that it is not to my taste nor, I suspect, to the tastes of most of your readers and that Googling Landmark turns up a lot of interesting sociological information: it's a hobby of mine to follow groups like Landmark, the David-Deutsch movement, etc.

   I myself am old enough to remember the transition when people started to recognize that John Bell was right and that von Neumann had made an error in his "proof" of the "impossibility" of hidden-variables theories: I wrote a term paper on the topic during the '74-'75 academic year. I suppose that all this did help open the floodgates to all the "quantum" nonsense (can we do anything about Deepak Chopra, please?), but the underlying physics is interesting and it was good to correct von Neumann's error, even if almost none of us are really convinced that any hidden-variables theory is correct. (I've done some unpublished work in the field, and, no, I do not have any pet theory that I believe is true.)

   Do you know if there is anything to the "retrocausation" stuff that is even remotely physics or is it all just gobbledygook? Bell's theorem does seem to hint at faster-than-light connections of some sort, which of course should violate relativity, except that in all the hidden-variables theories the very dramatic violations of relativity are guaranteed to be completely undetectable experimentally. I've long wondered if some sort of retrocausation could be used to model this – faster-than-light should of course allow signaling into the past according to relativity. However, I've never seen anyone come up with any serious proposals along those lines, nor have I come up with anything myself.

   Yet, I still have this nagging feeling that "Bell's theorem" is trying to tell us all something we do not yet get.

   Of course, a major problem is that the signal-to-noise ratio on this subject is so low that it is hardly worth following even the papers in the arXiv on the subject. But, I'm still not convinced that "shut up and calculate" is the final word.

   Thanks for your post: I guess I need to get the book.

   Dave Miller in Sacramento

2. **Yatima**
   June 13, 2011

   They are not taking prisoners here..

   genuine retrocausal phenomenon

   But these claims are never operational in an engineering sense…

   I would think at the claimed significances, world and dog would know about this and the NSA would be working around the clock to open up the unrivalled frontiers of closed timelike loop computation, faster-than-light signalling and massive entropy reduction going forward in time. The abstract alone raises doubts about the authors being able to keep QM "retrocausation" (certain to be misinterpretation of mathematics) and "foreknowledge of future events" (quite likely to be misinterpretation of statistics and psychology) well apart. Sigh.

3. **Roger**
   June 14, 2011

   You are way to easy on this book. It is crackpot stuff.

4. **Chris Oakley**
   June 14, 2011

The S-matrix approach is a rather mundane attempt to get something for nothing, and arguably the least trippy thing to come out of the theoretical particle physics mainstream in the last 60 years. Had these hippy physicists had Superstrings and 11-dimensional Calabi-Yau manifolds to contemplate with the aid of proscribed substances, their brains would probably have exploded.

5. **martibal**
June 14, 2011

Nice review Peter,

could you – or somebody else – explain why believing in the S-matrix approach aims at claiming something like the standard model could not work (or maybe there is a precedent post about that) ?
That S-matrix and SM are two different views on qft is one thing, but I did not realize they were mutually exclusive.

6. **Tim van Beek**
June 14, 2011

Dave Miller said:

> Bell's theorem does seem to hint at faster-than-light connections of some sort, which of course should violate relativity, except that in all the hidden-variables theories the very dramatic violations of relativity are guaranteed to be completely undetectable experimentally. I've long wondered if some sort of retrocausation could be used to model this – faster-than-light should of course allow signaling into the past according to relativity. However, I've never seen anyone come up with any serious proposals along those lines, nor have I come up with anything myself.

It is possible to show that correlations that are described by Bell's inequalities do not violate relativistic causality, in axiomatic quantum field theory.
See, for example:

* Stephen J. Summers: "Yet More Ado About Nothing: The Remarkable Relativistic Vacuum State" (<a href="http://de.arxiv.org/abs/0802.1854v2";).

An example of a theorem that describes "action at a distance" is the Reeh-Schlieder theorem (see the nLab page). People have written papers about how the Reeh-Schlieder theorem shows that the axiom of locality in AQFT is wrong and needs to be replaced, without noticing that this axiom is not used in the proof of the theorem (ouch!).

Anyway, "action at a distance" aka correlations of states that are spacelike separated, does not necessarily imply that Einstein causalitiy is violated.

7. **Chris Oakley**
June 14, 2011

Martibal,

They are not mutually exclusive. Far from it. S-matrix theory takes the operator that connects initial and final states in a scattering experiment (the S-matrix) and infers a surprising amount about it just from very general considerations (relativity, etc.) Its conclusions are not invalidated by quantum field theory it is just that it does not tell one enough in order to classify as a replacement.

8. **Emanuel Derman**
June 14, 2011

I've read only the first couple of chapters but I agree that so far it's a marvelous book.

I graduated with a PhD in 1973, the year in which the production of physics PhD students maxed out, and the author's description of the sociology of physics and its job market in those days is very accurate. And yes, "Shut up and calculate" was the strict attitude of faculty. I knew grad students who wanted to do research on quantum mechanics rather than on particle physics, and they were strongly discouraged. One prof at Columbia, a nice guy, said cynically "Leave that for when you're old."

I'm always struck, in conversations with biologists and amateur and professional neuroscientists, how naive they are about physics and matter. Spinoza wrote that the body can do many things which the mind doesn't understand at all, and yet many people put their faith in the idea that matter is simple, and that everything follows from that.

9. **Peter Woit**
June 14, 2011

martibal,

I wrote quite a bit about this in my book. The S-matrix philosophy was that symmetry arguments, quantum fields and a specific choice of Lagrangian to determine the dynamics could never explain the strong interactions. The discovery of QCD and asymptotic freedom conclusively showed this was wrong in 1973. In 1975 Fritjof Capra, one of the members of the group discussed in this book, published "The Tao of Physics", promoting exactly this philosophy, just after the point at which it had conclusively failed.

Of course the S-matrix and its properties is a crucial thing to study in particle physics. What Chew and others had hoped though, that general properties of the S-matrix would be enough to determine the theory, turns out to just not be true.

10. **ObsessiveMathsFreak**
June 14, 2011

So, String Theory came out of LSD trips?

More seriously, is the book claiming that modern physics as we know it is a product of the social revolutions of the 1960s? Is it the case that physics underwent a radical break from its past in the 1970s, as profound as that seen in other social and cultural spheres? If so this is a very substantial claim, which may have had profound influences on the way physics is researched and understood in the present day.

11. **Peter Woit**
June 14, 2011

OMF,

The book isn't making claims about most of modern physics, including the Standard Model, string theory, etc. That's just not its topic.

The book is mainly about the story of a group of "countercultural" physicists active in the mid-seventies at Berkeley, and their unorthodox claims about quantum mechanics (that it might have something to do with para-psychology, for instance). They were quite interested in issues like entanglement and Bell's Theorem, topics ignored by the conventional physics establishment at the time, but which now have become quite hot topics in the field of quantum information theory (NOT in string theory or particle physics). One could argue about what effect their interest and activities had on the later movement of these topics into the mainstream.

12. **Eu**
June 14, 2011

Offtopic:

Emanuel Derman, thanks for writing "My life as a quant".

13. **David Bailey**
June 14, 2011

Perhaps the best retrocausation experiment, is that devised by Dean Radin himself, known as 'presentiment'.

Subjects are shown a succession of calm images on a computer (landscapes, etc), with the occasional violent or erotic image randomly inserted. The subjects are monitored using skin conductance to measure their state of arousal. The claim is that the subjects begin to show a response to the disturbing images for about 1 second before they have been displayed, and indeed before they have been selected by the computer! The effect achieves statistical significance over a relatively short test period.

This experiment has been around for many years, with a number of independent confirmations.

I'd say it is time someone determined exactly where this anomalous result arises – it just seems wrong to me that such a bizarre result is just left hanging in the air.

14. **Peter Woit**
June 14, 2011

David,

I just remembered that I've been exposed to Dean Radin before, see here

http://www.math.columbia.edu/~woit/wordpress/?p=342

He's behind the "Institute for Noetic Sciences", and makes an appearance in "Down the Rabbit Hole", the extended Director's Cut version of the appalling "What the Bleep".

I'm afraid he is a good example of what was wrong with the "counter-cultural physicists" and their interest in parapsychology and pseudo-science. The same nonsense about parapsychology and quantum physics that they were pursuing 35 years ago continues to be pursued by a huge group of people, with not the slightest sign of anything you could call scientific progress. I'm sure they'll be at this long after I'm dead. In the meantime, paying attention to them is just a waste of time, so please don't encourage it here. Those who want to discuss this kind of thing can find dozens of other places devoted to it.

15. **Bugsy**
June 14, 2011

There's nothing wrong with mysticism per se, in fact I think that mixing science and mysticism can do a disservice to both.

That said, at their best, what both can have in common is to help awaken our sense of wonder.

Then again, for some people what does the trick is fresh pesto on pasta…

16. **Anon**
June 14, 2011

David, if this were a real phenomenon, surely someone would have used it already to make a fortune on the stock market by converting, e.g., upticks into erotic and downticks into neutral images. A second of foreknowledge is a long time in today's markets. I would like to see someone put their actual money where their mouth is.

There is no such thing as retrocausation in known Physics (despite some speculation on future boundary conditions in gravity). There is certainly no such thing in Quantum Mechanics, and a Physicist pretending that there is some such thing is either ignorant or dishonest.

17. **Roger**
June 14, 2011

Advocating retrocausation is either ignorant or dishonest? I am inclined to agree, but surely the same is true about the hippie physics of this book.

18. **Thomas Barton, JD**
June 14, 2011

Perhaps you could write a blog entry on the irony of the Berkeley hippies giving rise through their idiosyncratic views to the now burgeoning orthodoxy you now find resident in the redwoods of Stanford.

19. **Peter Woit**
June 14, 2011

Thomas,

I don't think multiverse mania can be blamed on the Berkeley hippies. They were guilty of all sorts of nonsense, but not that one. The string theory establishment came up with it on their own, with little to no help from hippies, hot tubs and psychedelic drugs..

20. **Jack Sarfatti**
June 15, 2011

I am reporting from the AAARP USD Retrocausality Conference.
Intellectually honest theoretical physicists will first read Bem's paper before casting stones – especially if they are string theorists.

Here are a few scattered notes from the meeting.

From: Dean Radin
Date: June 14, 2011 8:42:39 PM PDT
To: JACK SARFATTI
Subject: Re: AAAS Retrocausality Meeting at University of San Diego

So Art dreamt that he fired a revolver, you dreamt that a revolver was aimed at you, and I spontaneously decided to tell that story and show the photo of the gun to illustrate presentiment in real life? A genuine three-way entanglement through time. That's a first even for me!

Best wishes,

Dean Radin
Senior Scientist
Institute of Noetic Sciences

On Jun 14, 2011, at 8:33 PM, JACK SARFATTI wrote:

I was with Henry Stapp today and he and I see eye to eye on the same page.
Henry said he changed his mind in past few months because of Daryl Bem's data.
Henry never even heard of Antony Valentini BTW – he independently now says that Bem's data (and Radin's, Bierman's …) show violation of "orthodox quantum theory" i.e. what I call "signal nonlocality" and that messages can be decoded nonlocally in the past as I have been arguing.
Do! ug Hofstader wrote that Bem's results turn physics upside down. Bem today said he was not the Copernicus of the new paradigm alluding to Bierman, Radin, Libet … but I say he is the Michael Faraday of the new paradigm.
Also Russell Targ & Fred Alan Wolf as well as A. Elitzur.
Elitzur agrees with me that Hawking was right the first time and should not have caved in to Gerardus 'Hooft and Lenny Susskind. Like me, Elitzur does not believe in unitarity as absolute. Indeed, I told Stapp and Elitzur at lunch today at USD that "unitarity prohibits novelty" i.e. "novelty" as in Henri Bergson as used by Henry Stapp. Elitzur concurred saying "I'll buy that."
Ibison and I had good discussion on the de Sitter boundary issue – we are converging more.
Before I forget – the Bologna cold fusion and Moddell-Haisch zero point battery patent do not look good – this from a reliable source.
Bem should collect the $1,000,0000 from James Randi who I am told keeps changing the rules and is not honest about his "prize."

Now here is the kicker. I woke up this AM with a dream that I had been shot in the face with a revolver. I almost missed Dean Radin's talk. I came in on the end. Dean told the following story shortly after I came in. "My friend collected guns. He was loading a double action revolver. Something told him not to load the sixth bullet – leaving one empty chamber. Several weeks later one of his drunk friends went berserk and picked up that revolver on the table – pointed it at Dean's friend's face at close range pulled the trigger and of course the gun did not fire since the barrel turned to the empty chamber – that's Novikov loop in time inside a Novikov loop in time. Also, Art Altschuler a retired physics teacher told me and Fred Alan Wolf that he dreamed last night that he fired a revolver at close range into someone's face – i.e. a loop within a loop within a loop.

Andrew Jordan (AJ) University of Rochester
mentions negative pressure from Phys Rev editors re: retro-casual interpretation
Tollaksen is here BTW but Nauenberg did not make it – too bad.
Menas Kafatos also here
ABL 1964 both past and future boundary condition of pre & post-selection – time symmetric
weak value have both pre and post selection constraints.
See my Journal of Cosmology Vol 14 April 2011 paper for the math – free online (edited by Penrose and Hameroff)
weak value WV can exceed eigenvalue range (i.e. negative probabilities weighting the eigenvalues) and need not not be real number, can be a complex number
Stern-Gerlach test of WV actually carried out at University of Rochester with DARPA & NSF funding.
pre-selection
weak splitting in z
strong splitting in x
post-selection in x
record the z-delection

/ = weak

If —> 0 you exceed eigenvalue range but with small probability

weak measurement experiments in solid state quantum hall effect relevant to quantum computers
also optical systems

Optical telecom for super internet can use weak measurements with value added.

There is technological spin-off not just idle theory. Obama gave Yakir Aharonov a medal for this work.

Spin Hall Effect of Light via Weak Measurements – spintronics

Weak measurement technology/protocol way of measuring tiny signals in large noise fields.

Ultrasensitive beam deflection measurement via interferometric weak value amplification.

Nature 463 Feb 2010 Aephraim Steinberg "Light Touch"

Sagnac effect weak measurement – which path photon took CW or CCW on rotating interferometer?

this can measure very small shifts in optical parameters using both pre and post-selection technique.

560 femto-radian deflection signal i.e width of human hair on Moon can be seen from Earth

So for GPS Drones in Pakistan and Afghanistan – amazing resolution for military operations against terrorists for example.

Also for seeing exo-planets in 100 year Star Ship Study (NASA/DARPA)

Weak value precision phase measurements! For Kip Thorne's gravity LISA/LIGO detectors of big bang inflation physics.

pico-radian measurements with only few hours of integration time.

They have a patent!

Measurement Contextual Values (MCV aka POVM) – new interpretation of QM. PRL 104, 240401 (2010) avoids negative probabilities – more palatable for orthodox thinkers

get averages of operator moments, also find conditioned averages

include post-selection and any measurement interaction strength

weak measurement is a practical tool for precision measurements in optical, solid state et-al systems

!

21. **Yatima**
June 15, 2011

> I am reporting from the AAARP USD Retrocausality Conference.

Jesus Christ! This is like reading random dispatches from free association experiments.

On the same page with Stapp: http://www.mth.kcl.ac.uk/~streater/stapp.html

22. **David Bailey**
June 15, 2011

"I'm afraid he is a good example of what was wrong with the "counter-cultural physicists" and their interest in parapsychology and pseudo-science."

Finding out exactly how those experiments produce the result they do, would be a far more effective repost than calling them pseudo-science.

At the very least such research would expose a problem in an experimental technique that is widely used in conventional psychological experiments.

At the very most…………

23. **Giotis**
June 15, 2011

"The string theory establishment came up with it on their own"

Peter, if you accept inflation (which by now is incorporated in the standard model of Cosmology) then you must admit that eternal inflation and thus the multiverse is at least very plausible regardless if String theory is valid or not. So no String theory 'establishment' came up with this idea. In fact it is quite old. But it happens these two major developments of modern Physics from different starting points to combine and point in the same direction. This deserves people's attention to say the least.

24. **Shantanu**
June 15, 2011

Peter, first result from T2K
See
http://jnusrv01.kek.jp/public/t2k/sites/default/files/t2k-nue1st.pdf

25. **Peter Woit**
June 15, 2011

Shantanu,

This result can be accommodated with a non-zero mixing angle theta_{13}, somewhat below the previous experimental bounds, right? If so, very interesting and impressive measurement, but the Standard model still remains unchallenged.

26. **Mark**
June 16, 2011

Jack Sarfatti,

With respect to Bem's study, every single attempt to replicate his results has failed. The analysis of his data has been heavily criticized by Wagenmakers et al. and there is clear, unequivocal evidence that Bem performed more trials than he reported, which brings his entire credibility into question.

27. **Mark Hillery**
June 18, 2011

For a nice, short history of the no-cloning theorem, see the beginning of the review article by V. Scarani, S. Iblisdir, and N. Gisin quant-ph/0511088 or Rev. Mod. Phys. 77, 1225 (2005). It includes a nice discussion of some missed-opportunities that could have resulted in the theorem being discovered earlier.

28. **Sebastian Thaler**
June 18, 2011

George Johnson just reviewed the Kaiser book in The New York Times: http://www.nytimes.com/2011/06/19/books/review/book-review-how-the-hippies-saved-physics-by-david-kaiser.html?_r=1&ref=books

29. **Emanuel Derman**
June 18, 2011

One totally unexpected thing I learned from this book was how closely straight-and-narrow physicists like Jackiw and Coleman were involved with the Erhard meetings. I never knew about it at the time. Was it opportunism, open-mindness, or both?

30. **Henry**
June 20, 2011

Leonard Susskind came up with the no-cloning theorem independently while thinking about black hole complementarity. However, he also attended the EST seminars and had a similar background…

31. **David Gladstone**
June 22, 2011

Peter Woit-"The same nonsense about parapsychology and quantum physics that they were pursuing 35 years ago continues to be pursued by a huge group of people, with not the slightest sign of anything you could call scientific progress."
Your comments reveal your negative bias as well as your ignorance of what is scientific progress, which is so easily available to any reader in the content of your missive is laughable at this point, given Bell, Clauser, Aspect, etal.
The S-Matrix theory was an idea worth exploring then, the fact that we know it's not a theory now, just as the Standard Model is not a theory now, just a model, means at worst they are equally fallacious. The Standard Model of physics has no explanation for its own success and no clue about consciousness either. It seems clear that Johnson in his review in the Times, wants to hold retroactive thought crimes trials for these people, and saves his final barb for the sexual enthusiasms of some, which is mere prudishness, but that doesn't hold water when discussing the search for truth. As for retro causality, the anecdotal evidence is astounding at least, compared with the same kind of evidence for UFOs or God and that should eliminate at least the public hectoring of researchers who try to find the there there.

32. **Peter Woit**
June 22, 2011

David Gladstone,

The Standard Model doesn't need to have "an explanation for its success", it just needs to be a success and it is (unlike the S-matrix program).

Like a large number of people, I'm on Jack Sarfatti's mailing list, so can see for myself that, bless his soul, he's still trying to do the same kind of things this group was trying to do in the 70s. Claims for experimental evidence about things like retrocausality remain, 35 years later, at the same "anecdotal" level of that time.

As for Johnson's "physics porn" comment, I've some sympathy for Nick Herbert's point of view that that's not necessarily a bad thing. But Johnson is making a more serious point that this stuff is in the category of "exciting" but not real, solid science, and that's an accurate point to make.

33. **David Gladstone**
June 22, 2011

Two things, Peter, need clarifying.
First given the problems with the Standard model already well-known and articulated, should Johnson have made it it such a key point of his attack in his review? It seems he was just looking for a club to use as a weapon. Understandable, perhaps, but not logical or compelling.
The physics porn comment was nothing but a cheap shot, larded with nastiness. Admittedly the title of the book is tendentious, purposely so, it seems clear from Kaiser's video talks. I can see why Johnson trots out all the old buzz words people use when the word hippie is uncloaked. It's just so easy to respond that way if you don't challenge yourself, as Johnson clearly didn't, and it certainly doesn't shed any light. Yes, we can all see the derision literally dripping off the pages of the Times, but that's about all that's there and readers should and do expect more from the old 'Gray Lady'!

34. **Peter Woit**
June 22, 2011

David,

Actually I thought it was a good review, and Johnson an excellent choice of reviewer. He's someone with a deep knowledge of the history of this subject, as well as a real sympathy for non-hard-nosed, more mystical take on it. I just took the use of "porn" as in "cheap thrill". Nothing wrong with cheap thrills, but there is something more satisfying out there…

35. **David Gladstone**
June 22, 2011

Clearly you thought it was a good review, he agrees with you and vice versa! Quel Surprise!:] He is clearly holding a disapproving mirror up to these guys (and more importantly to us, the public masses), as if saying 'you cannot be serious enough to think you have anything new to say! ' We professionals all have things perfectly well in hand, thank you very much , now move along…! It seems like many people, have a love-hate relationship with this new physics stuff and all the spooky action at a distance effects Einstein himself was afraid of decades ago. The eminent scientist Heinz Pagels, was someone was so enraged by the temerity of people like Jack (actually it's more honest to say it was just Jack who could drive him to distraction), he'd literally chew the carpet and yet he felt compelled to listen to what he had to say (through me) and contest every thought he'd ever have with incredible personal emotional heat. You'd think we were talking about global warming. Wheeler would just throw Jack's mimeos and letters in the circular file, he (JAW) told me that himself, btw.
But I see some of what's wrong with physics right here on this blog in the attitudes of a number of people, a pretty fair number btw, who will never accept this stuff and what it really means till the Angel Gabriel blows his horn on American Idol.
The Palestinians and Israelis will kiss and make up before that ever happens, imo.

36. **David Gladstone**
June 22, 2011

Peter, allow me to say one more thing about this:
But I do believe there are greater concerns at work here, I do not believe it's a theoretical discussion that has no temporal or mundane urgency, it has. The very existence of of just one of the spooky effects, say telepathy, poses grave questions concerning our national security, and our near term futures, that few have addressed, with the exception of people like Martin Reese and Stephen Hawking. The fact is if just one of those things is real, they're all real, because related and what they mean for humanity has hardly ever been addressed and the withholding of recognition, just like in the middle east, will continue to keep us from delving farther into what we're facing, which is a grave mistake, imo. We are a species in denial.

.

37. **Dave Miller**
June 24, 2011

Tim van Beek wrote to me:
>It is possible to show that correlations that are described by Bell's inequalities do not violate relativistic causality, in axiomatic quantum field theory.

Yes, I think we all know this: it is a very, very old result. In fact, I first saw it proved by Lenny Susskind (!) in a class back in the mid-70s.

But that is not what is bothering a lot of us: *of course* the predicted and observed violations of Bell's inequalities do not result in observed violations of relativity. Nevertheless, Bell's inequalities (which do not assume determinism) do seem to follow necessarily from "locality" in the common-sense meaning of the term. Their (expected) violation by quantum mechanics does seem to me to be telling us there is something here we do not understand.

In fact, a similar point can be made more easily in QFT: the VEV of a product of fields with space-like separation often does not vanish. I find that puzzling, even though I of course know that, as long as the appropriate (anti) commutators vanish for space-like separations, then no violation of relativity will be observed experimentally.

I have no axe to grind here: I am not an advocate of any particular alternative to orthodox QM, not even such now-popular alternatives as decoherence or many-worlds.

I am merely puzzled. It seems to me that sometimes a willingness to be puzzled is a good thing.

Dave Miller

This Week’s Hype

June 15, 2011
Categories: This Week's Hype

Last month’s Quark Matter 2011 conference was a venue for discussion of new results from the first heavy-ion run at LHC energies last fall. I’ve looked a bit at the slides of the talks, but this is an area far from my expertise. One thing I’ve been wondering about is whether the heavily-promoted application of AdS/CFT to studying heavy-ion physics could possibly be tested at the LHC. Does AdS/CFT make any distinctive predictions about how things will change as one goes from RHIC energies to LHC energies, and have these been checked? Looking at the slides, there seem to be all sorts of interesting things being learned about heavy-ion physics, but little mention of AdS/CFT modeling of such phenomena. Perhaps an expert can help by pointing to pre-LHC predictions, and explaining whether they’ve been tested already, or may be in the future.

Symmetry Breaking magazine today does cover Quark Matter 11, with String theory may hold answers about quark gluon plasma, which appears to mostly contain the same hype about string theory and heavy ion physics that has been current for the last half-dozen years now:

> Now, scientists have begun to see striking similarities between the properties of the early universe and a theory that aims to unite gravity with quantum mechanics, a long-standing goal for physicists.

Unfortunately there’s nothing in the article about any LHC test of these ideas. The closest we get to that is this from Krishna Rajogopal (his talk is here):

> “String theory is like a gift to us,” Rajagopal said. “We’re challenged with understanding the quark-gluon plasma as a liquid, and while string theory doesn’t give us precision, it can help us get a feel for the shape of the subject.”

So, I gather that AdS/CFT makes no precise, testable predictions, with the best case to be made for it that “it can help us get a feel for the shape of the subject”, whatever that means. A question for experts: if “String theory may hold answers about quark-gluon plasma”, what are the questions for which string theory is giving answers, and what does the LHC data have to say about these questions?

Update: David Mateos has posted a write-up of his Quark Matter 2011 talk here. In it, he explains what the problems are with using AdS/CFT to say anything about QCD. In terms of the question of LHC predictions, he gives an example: the dispersion relation of heavy quarkonium mesons moving through the quark-gluon plasma. Unfortunately, this doesn’t look like much of a prediction:

> I emphasize that whether one obtains a visible peak, simply a statistical enhancement or an unobservable effect depends sensitively on many parameters related to the in-medium J/Psi physics. The latter is not sufficiently well understood to make a precise prediction, so all one should take away from figure 3(right) is that there could be an observable effect for some values of the parameters within the acceptable range.

Comments

1. **Derek Teaney**
   June 16, 2011

   Peter,

   Testable predictions seems to be asking for too much — as the correspondence
   (at its best) is for maximally supersymmetric theories.

   But, it would be certainly unfair to claim that the AdS/CFT has not to heavy ion physics (and vice versa) . For instance, the most striking observations at the Quark Matter conference were the remarkable wealth of new data on hydrodynamic flow (higher harmonics) . In simulating RHIC and LHC events we use a second order hydro formalism which was fully worked out (through work in AdS) by Baier, Romatschke, Son, Stephanov, Starinets. Steve Gubser, recently found an analytic solution to the hydro equations using methods inspired by AdS which will be useful to even the most phenomenological heavy ion smashers, especially those concerned with higher harmonics (myself included). It is also interesting that traditional heavy ion people spoke about work in AdS/CFT. For example, in my case, instead of speaking about flow, I spoke about a new approach to hawking radiation in non-equlibrium geometries which drew heavily upon experience with non-equilbrium field theory (based upon the 2PI formalism).
   I view the AdS/CFT as an extreme limit which can be useful when confused by
   the new data. For example, while it the shear viscosity to entropy ratio may not
   be 1/(4*pi), the new data are placing new constraints which suggest a range (1-3)/4\pi . Berndt Mueller (who could have spoken on any number of current heavy
   ion topics) also gave an interesting talk on thermalization in AdS/CFT
   here.

   In summary, while the correspondence may not provide precise quantitative predictions, it can hopefully provide guidance and insight to complex physics.

2. **Conformal**
   June 16, 2011

   Whatever the status of AdS/CFT as physics, it has inspired some deep mathematics, e.g. in differential geometry. That means that it has not been a waste intellectually.

3. **Yatima**
   June 16, 2011

   Could anyone point me to a succint explanation of where the “liquid” or “hydro-” methapor comes from in this case and what idea it is supposed to encapsulate?

4. **Bernhard**
   June 16, 2011

   Derek Teaney,

   What is very clear from what you are saying is that traditional criteria to handle a certain theory and test it, somehow does not apply to AdS/CFT since it does not provide precise quantitative predictions. “Provide guidance and insight to complex physics” is not enough since one cannot be sure if the theory is correct in the first place, so this guidance could be wrong. Asking for testable predictions is NOT asking too much, is asking for what makes science to actually work. What I would like to see if whether anything being done in heavy ion physics can say if AdS/CFT or ST is correct. Clearly this is not the case, if and when results don’t match with AdS/CFT it does not mean its wrong and even when it does “give guidance” , one is at best confirming a test of equivalence between two theories so no real experimental test was done.

5. **Derek Teaney**
June 16, 2011

Dear Bernhard,

Regarding "What is very clear from what you are saying is that traditional criteria to handle a certain theory and test it, somehow does not apply to AdS/CFT since it does not provide precise quantitative predictions. "

This statement more less discounts models for providing qualitative insight to complex physics. Think back to QCD for how wrong this is — for confinement and asymptotic freedom the Gross-Neveu model; for chiral symmetry breaking we have the linear sigma model and the nambu jona lasinio model; for small x physics (important to heavy ions) the McLerran Venugopalan Model; in energy loss in QCD (important to heavy ions) the Gyulassy-Wang model… None of these models provide "precise quantitative predictions", but all have been extremely useful at capturing one or more aspects of complicated physics

Of course AdS/CFT does make precise quantitative predictions for N=4 SYM theory with a large number of colors and strong coupling. So if "only" the N=4 theory could be simulated on the lattice then the predictions of the correspondence could be tested.

6. **A.**
June 16, 2011

At LC2011 last month there were several talks about AdS/CFT and AdS/QCD. The bulk of questions from the audience were from disgruntled older physicists asking, slightly more aggresively than I would have guessed, "But what does this have to do with real physics?" to which there were some refreshingly honest answers, such as "AdS/CFT has probably taught us nothing about real QCD."

I hadn't seen this kind of attack/surrender pattern before.

7. **Bernhard**
June 16, 2011

Dear Derek Teaney,

Thanks for the answer, that was very interesting. You have a good point that qualitative insight is indeed useful and the examples you gave are very good. But for asymptotic freedom even if the prediction is not all mathematically precise it is certainly not ambiguous and highly testable.

What I find most interesting is your statement:

"So if "only" the N=4 theory could be simulated on the lattice then the predictions of the correspondence could be tested."

If you care to enlighten my ignorance a bit more, are you saying that if this simulation were possible one could test AdS/CFT in the sense that it could also proved to be wrong? I know this would not test ST in any sense, but it would indeed be an exceptional achievement.

8. **Peter Woit**
June 16, 2011

Derek,

Thanks for the detailed and informative content. This still leaves me with the same general impression I got from the slides I was looking at: AdS/CFT provides an interesting toy model to play with, but it's far from being the sort of thing that is in any sense experimentally testable. The descriptions of the situation provided by some physicists to the press are pretty outrageously over-hyped.

It would be useful if people kept separate the different sorts of issues involved here about confronting QCD with experiment which your last comment to some extent mixes up. By my count, there's

1. purely toy models, like Gross-Neveu (which is a 1+1d model), which can't in any sense be confronted with experiment.

2. sigma models, which provide a good approximation to the low energy behavior of the theory in certain sectors. These can be confronted with experiment in some regimes.

3. the McLerran Venugopalan and Gyulassy-Wang models you mention, which I know nothing about.

4. the comparison of AdS/CFT-based calculational methods with other non-perturbative calculational methods in N=4 QCD (e.g. the lattice), which provide checks on the validity of the methods, but have nothing to do with confrontation with experiment.

9. **J**
June 16, 2011

Derek: I am very happy to see you here making nice explanations about quark-gluon plasma physics.

And guys, let's be fair and honest.

Does string theory or more narrowly the AdS/CFT provide precise, quantitative, and testable predictions for QGP physics? I think the answer is NO. Further I think most people in the field of QGP physics hold the same awareness, despite whether they would like to speak this atitude out or not.

Does this tool AdS/CFT useful for the QGP physics? YES, actually very much. Again I believe a lot of people (maybe not the majority, but certainly a fair fraction) of QGP field share this opinion. Look, physics is different from mathematics in many aspects: one distinction of course is the experimental test; yet another distinction is that physics often gets boost from conceptually useful ideas and models even some of those are not the complete story or even wrong. I think playing wild cards a bit is healthy and good for the development of physics, and it is fair to say the application of AdS/CFT for QGP physics has produced many intellectually useful developments.

Of course, over-selling (on purpose or not) is not good. But over-killing is probably another extreme. The only thing that matters, I guess, is that the people who are doing QGP physics know very well the boundary of the usefullness and bullshit-ness for applying AdS/CFT to QGP.

10. **Peter Woit**
June 16, 2011

J,

Thanks for the comment. I assume most experts in the field do know what's useful and what's bullshit in this story. Unfortunately, the press coverage has been uniformly misleading about this.

11. **srp**
June 16, 2011

As a complete outsider, it's kind of interesting how "wild" the theory of the strong force still is, even after it's been incorporated into standard physics. It sounds like it's still very hard to figure out exactly what the theory predicts in any specific case, but I'm a bit confused about just where we are. For example, would it be correct to assume that the proton is the model system where everything can be calculated pretty well from first principles? How about something like a helium nucleus? Is there a quark-gluon derivation of hydrogen to helium fusion?

Naively, if people are trying to study heavy-ion collisions with zillions of pieces flying around you'd think that the simpler cases had long been solved, but I'm not sure because I'm aware that sometimes messy things have to go first experimentally. In any case, it sounds like there's lots of important work to do.

12. **Derek Teaney**
June 16, 2011

This is my last (long) post on this

Dear Peter,

There are models which are somewhere in between toy models such as Gross Neveu and more rigorous approaches such as Chiral Perturabtion theory. For instance, the Nambu-Jona Lasino model does provide a picture of chiral symmetry breaking and does have a phase transition at finite temperature to a chirally symmetric phase. This qualitative feature of NJL models was of course confirmed by lattice (i.e. real QCD) only (much) later.

Lets talk about the Gyulassy Wang model: in this case the medium is replaced by random static scattering centers — this is certainly not the real quark gluon plasma. Nevertheless, the Gyulassy Wang model can be used to study the radiative energy loss of high energy partons propagating through random static scattering centers (as opposed to QCD). (Much) later when people worked out real QCD (at weak coupling) it turned out that the Gyulassy Wang model captured almost all the essential physics, e.g. the probe energy and path length dependence.

One of the qualitative lessons from the strongly coupled N=4 theory is the absence of quasi particles. This leads to qualitative predictions for spectral densities (i.e. photon production rates) which are markedly different from weakly coupled expectations. I (and others) wrote about this here . (See figure 5.)
While no one would reasonably compare the N=4 theory to real QCD spectral densities it is interesting wether this qualitative result survives.
At the quark matter conference, there was, what I would describe as the first serious effort to extract the current-curent spectral density here. The lattice spectral functions (which are not without systematic effects) are disasterously inbetween the quasi-particle theory and the strong coupling theory

Another lesson from the N=4 theory is the appearance at strong coupling distinct inversion of scale, Temperature=T << \sqrt{\lambda} T. lambda = g^2Nc is the 't Hootft coupling. In weak coupling non-abelian plasmas the scales are exactly reversed \sqrt{\lambda} T << T. The not particularly smalln Debye mass (which is proportional to \sqrt(\lambda)) for real numbers forces perturbation theory to go to very high orders and to treat m_D as variational parameter. This was discussed at quark matter here , by Nan Su.
In fact naive first order application of perturbation theory in QCD the Debye mass is several times the temperature. All of this makes the N=4 theory and interesting theoretical foil to the weak coupling calculations.
In particular, the scale inversion implies that the dynamics of high energy probes are distinctly different depending on how the energy compares to these scales, ie. T << E << \sqrt{lambda} T. Or T<< \sqrt{\lambda} T << E . It is interesting to look for such inversion of scales in the heavy ion data.

Peter and Bernhard,

Regarding hypothetical simulations of N=4 theory at finite temperature (which are basically impossible, but who knows…). It would provide numerical evidence for an amazing web of conjectures starting with the Black Hole entropy and arriving that the pressure of N=4 SYM at Large N is 3/4 of the steffan Boltzman pressure. It also would strongly suggest (What do you think Peter) that a non-perturbative formulation of the string theory exsists.

13. **Peter Woit**
June 16, 2011

srp,

Yes, calculations in QCD are amazingly difficult, and for many aspects of it we still have no good calculational methods. The proton itself is still imperfectly understood, and calculating nuclear physics quantities (or heavy-ion physics ones) from first principles is currently pretty hopeless. The amazing thing though is that when something can be reliably calculated (perturbatively, lattice calculations, symmetry arguments) there is agreement with experiment.

14. **Peter Woit**
June 16, 2011

Derek,

Thanks for putting the effort into explaining more about some of these issues.

In principle, calculations at arbitrary couplings and number of colors of N=4 SYM could show that AdS/CFT works, and that you can approximate the theory in some regimes with strings or with supergravity. Even if you did this though, the only well-defined theory you have is a quantized Yang-Mills theory, a QFT. You still can't start from the other end of the duality and write down a well-defined theory whose fundamental variables are strings. What one would really like is a version of the duality that allowed you to start from either end, writing down a complete theory in terms of either strings or gauge fields. Right now you've only got the QFT half of this.

One aspect of this is that you can (in principle!) define precisely any QFT, in any background. AdS/CFT is only going to give you a precise theory you may want to call a "string theory" in one specific background. You can generalize this with more general gauge/gravity dualities, but I still don't see anyone making progress on the question of what string theory in general backgrounds really is non-perturbatively, i.e., what is M-theory?

15. **Peter Orland**
June 17, 2011

In the last few years, I made a few comments on this blog about AdS/QCD. I said something along the lines of:

The real strong-coupling theory is found by a renormalization group. Take QCD with a cut-off up at the Planck mass. Integrate out all the degrees of freedom with wavelength bigger than 0.5 Fermi. There is no universality (the strong-coupling action depends on the cut-off method) and tons of irrelevant operators in the action. THAT'S the right strong-coupling theory. I was metaphorically excoriated as ignorant or worse, as a result.

Anyway, strong coupling is an old story. In the 1970's, Hamiltonian lattice theorists (some of whom later turned to string theory) got a rough caricature of the hadron spectrum, by working at very strong coupling (total confinement without any real gluon dynamics). Their predictions were not that different from the quark model (and they did not get good results for mesons, like the quark model w/o current algebra). Something better came along, namely Monte-Carlo simulations (though it took a long while for them to be much better).

This just means AdS/QCD is a model and not the actual strong-coupling limit. I gather most comments above are saying precisely this.

16. **chris**
June 17, 2011

As a relative outsider to QGP physics i hope not to offend people in the field, but my general impression is that heavy ion physics and quark matter is basically still a mess. and it's no wonder why this is so: strongly coupled multi-particle systems are notoriously difficult from the theoretical side. and (non-equilibrium) thermodynamics of a short-lived collision product of hundreds of strongly coupled particles is a really tough experimental job. experimentalists frankly admit that they can't determine the (pseudo-)critical temperature even. and theorists are fighting about the correct value for years now (on top of which the hydro is really difficult).

in this general setting, things like AdS/QCD are extremely welcome. i have repeatedly witnessed senior heavy ion people trying to understand the lifetime or equilibration time of the QGP in terms of dual black holes. these people know that the model has its limitations but it seems to me at least that the competition is not doing so much better in terms of rigor. the advantage of string duality methods is of course the relative ease. the competition is lattice gauge theory and hydrodynamic simulations – both really tough and computationally intensive. once the field matures – and that may take a very long time – models will hopefully be obsoleted and replaced by ab-initio understanding. but as it stands, AdS/QCD seems to be one model among many and probably not the worst at the moment.

17. **Wolfgang**
June 18, 2011

"… the only well-defined theory you have is a quantized Yang-Mills theory, a QFT. You still can't start from the other end of the duality and write down a well-defined theory whose fundamental variables are strings."

I don't think that's the right way to think about this.

N=4 super Yang-Mills should be seen as the nonperturbative definition of Type IIB string theory on AdS5xS5. That definition implies a prediction: that the large-N, large 't Hooft coupling limit of N=4 SYM is described by classical supergravity in AdS5xS5.

That is a surprising and unexpected prediction. (Not totally-unexpected: it was long-conjectured that large-N Yang-Mills is a classical theory; it's just that no one suspected that the "master field" was a higher-dimensional supergravity.) But it really is a prediction, and it has lots of consequences that have been checked by explicit calculations.

Furthermore, if you supplement the above with an additional hypothesis: "Strongly-coupled gauge theory plasmas have certain universal features of their behavior." then this prediction about N=4 SYM has experimental consequences for the QGP.

Of course, "certain universal features" is a bit vague. That's why AdS/CFT is, at best, a model for the QGP. Still, it seems to capture a lot of the important features of the QGP, which is why the nuclear theorists are excited.

18. **Peter Orland**
June 19, 2011

"Furthermore, if you supplement the above with an additional hypothesis: "Strongly-coupled gauge theory plasmas have certain universal features of their behavior." then this prediction about N=4 SYM has experimental consequences for the QGP."

Right, this is what people say, and I am certain it is plain wrong. There is nothing universal about BARE strong-coupling theories, where the cut-off is large. So, knowing someone will call me an ignoramus or just plain evil, I will elaborate a bit on my comment above.

In field theory and critical phenomena there is nothing universal in a random point of the phase diagram (including the coupling as well as the other parameters) of a cut-off theory. Thus, the strongly-coupled theory with a cut-off has NO universal properties. Universality is something that applies to theories near a critical point (where the cut-off is gone!), which in the case of gauge theories means small bare coupling. The renormalized coupling can be large.

I am convinced this bit of conventional wisdom some AdS people say about the QGP is false. These AdS/QCD theory are models. You can do phenomenology, perhaps even good phenomenology with them. That doesn't make them universal.

It is a technical point, but it is important.

19. **Wolfgang**
June 19, 2011

"There is nothing universal about BARE strong-coupling theories, where the cut-off is large."

You are right that there is nothing "universal" about the UV behavior of QFTs. QCD is weakly-coupled in the UV.

But QCD is strongly-coupled in the IR, and it is that IR behavior (at finite temperature) that we are discussing.

20. **Peter Orland**
    June 19, 2011

    No, I did not mean UV behavior. I meant IR behavior.

    In asymptotically-free theories, the real bare coupling (not the renormalized coupling) goes to zero as the cut-off is removed. It is the properties of the theory with no cut-off that are universal. If you have a large gauge coupling in a cut-off (but otherwise local) field theory, nothing is universal. Not viscosity divided by entropy, not parameters of elliptic flow, not anything!

    If the bare coupling is small, so that the theory is near the continuum limit, you have universality in a quantum field theory. That is not the case here.

21. **Peter Orland**
    June 19, 2011

    I should add that often it seems there is much confusion concerning the phrase "strong coupling". There is a difference between a cut-off theory with a large bare coupling and the strong-coupling (IR) limit of a gauge theory. The strong-bare-coupling theory is just a model. It it the strong-renormalized-coupling theory which is unique, i.e. universal. In this theory, the bare coupling is infinitesimal, not big.

    The bare coupling is not physical. It must be taken to zero with physical quantities fixed (this defines the r.g. equation). This is true for any observable, large Wilson loops or other probes of IR included.

22. **Wolfgang**
    June 19, 2011

    "If the bare coupling is small, so that the theory is near the continuum limit, you have universality in a quantum field theory. That is not the case here."

    I assume by "here," you mean N=4 SYM at large 't Hooft coupling (since, for QCD, the bare coupling obviously is small). N=4 SYM is conformally-invariant. So you are always near the continuum, at any value of the bare coupling.

    "The bare coupling is not physical."

    True in QCD, but not true in N=4 SYM.

23. **Peter Orland**
    June 19, 2011

    But heavy ion physics is not N=4 YM!

24. **Wolfgang**
    June 19, 2011

    In QCD, the bare coupling is unphysical, and is tuned to zero to obtain the continuum limit. In N=4 SYM, the theory is conformal, the bare coupling is physical, and you are near the continuum, for any value of the bare coupling.

    In neither case do your comments, above, seem to be relevant. When you said

    "If the bare coupling is small, so that the theory is near the continuum limit, you have universality in a quantum field theory. That is not the case here."

    what did you mean by "here"?

25. **Peter Orland**
    June 19, 2011

    "Here" is heavy-ion physics, described by (renormalized) QCD. Again, I am not criticizing using AdS methods as models. I just think this talk about universality has no basis.

    If you claim universality, you need a reason. I don't see why AdS models should give universal answers. There is nothing universal about a strong-bare coupling AdS/QCD model. Certainly nothing universal about an entirely different theory like N=4 Yang-Mills. People say it's so, but they don't provide a reason.

26. **K. Kajantie**
    June 20, 2011

    I think one could draw some parallels to QCD around 1970. Then there was an extremely successful constituent quark model, counting rules, parton model, etc, but really no idea why they worked so well. Now we have some very suggestive observational facts (p=3/4 p_ideal, shear viscosity, pattern of quark energy loss) but again no quantitative theory, only rough idea. Then around 1972-3 came QCD Lagrangian, asymptotic freedom and quantitative predictions for logarithmic scale violations in deep inelastic scattering. It took a few years to verify these and convince everybody of the fact that QCD is the correct theory. This is the stage we are missing today and until a QCD-string theory dual permitting quantitative predictions is there, there is no way to convince everybody. This is, of course, a completely trivial statement. Unfortunately this dual seems extremely remote and also there is much less help from experiment to guide us there.

27. **Wolfgang**
    June 20, 2011

    ""Here" is heavy-ion physics, described by (renormalized) QCD."

    Then the bare coupling has been taken to zero, and I don't see what you are going on about.

    "There is nothing universal about a strong-bare coupling AdS/QCD model."

    Perhaps I don't know what you mean by "AdS/QCD", but in N=4 SYM, the theory is conformal, the bare coupling is physical, and the strong bare coupling behavior IS universal (in the commonly-used sense of the term "universal").

You seem to want to apply either the intuition from QCD (where the bare coupling is unphysical) to N=4 SYM (where the bare coupling is physical, and your intuition is incorrect), or you seem to want to work in QCD and take the bare coupling to be strong (which is NOT a sensible thing to do).

I can't figure out which of them you have in mind, but neither of them seems correct.

28. **Peter Orland**
June 20, 2011

Wolfgang,

I made no comments on the validity of calculations for N=4 theories, just for QCD.

I said only one thing: I see no justification in claims of universal predictions for heavy-ion physics. It is a pretty clear statement, even if you don't agree.

29. **Wolfgang**
June 20, 2011

"I said only one thing: I see no justification in claims of universal predictions"

Your statement was about the strongly-coupled lattice gauge theory of QCD. But there is no justification for using the strongly-coupled lattice QCD. To the contrary, one wants to tune the bare lattice gauge coupling to zero.

Whether or not there is some sort of "universality" in the behavior of strongly-coupled gauge theory plasmas, your statement seems to have no bearing on the question.

But perhaps I have misunderstood your argument. Why is the strongly-coupled lattice QCD relevant to heavy ion physics?

30. **Peter Woit**
June 20, 2011

Wolfgang,

Your discussion with Peter Orland I fear is going nowhere. My understanding is that he's simply objecting to your claim about "universal behavior" in N=4 SYM and asking for justification. What exactly does this mean and what is the argument? Presumably it can't be the same kind of universality argument one can make for asymptotically free theories by putting them on a lattice, and defining the continuum limit by going to the critical point at zero coupling.

His reference to strong-coupling lattice theory I take as an analogy: there's a strong-coupling limit in lattice gauge theory, but there's nothing universal about it.

As for your response to my posting: of course AdS/CFT makes a prediction, a very interesting one, but it's a prediction about a specific QFT (N=4 SYM in a certain limit). Again, what's well-defined here at all couplings is a QFT and statements can be made about that. You don't have a method for defining string theory at any coupling. All you can do is, in a very specific background, define away the problem of "what is non-perturbative string theory" by saying that whatever it is, it's the same thing as a well-defined QFT.

31. **Peter Orland**
June 20, 2011

Wolfgang,

OK, I'll try one more time. Here is what I said:

No arbitrary theory with a cut-off and large bare coupling (which includes both lattice and modified AdS approaches. The latter go by the name of AdS/QCD – not standard N=4 susy YM) can give universal results. These are strongly-coupled theories, but they are the wrong strongly-coupled theories.

I wasn't commenting on theories with vanishing beta functions, though it is hard to see why these should produce results in the same universality class either. They have no QCD-type scale, and completely different matter content. There is no reason to think their infrared behavior resembles QCD.

Summary – I don't believe AdS methods give solid predictions for heavy-ion physics, though I don't doubt some good phenomenology can be done.

I tried to give some justification for this statement in terms of the renormalization group.

I don't know what I can add to make the above points clearer.

32. **Peter Orland**
June 20, 2011

Wolfgang: When I posted the last comment, I did not see Peter W.'s clarification. He summarized it very well.

33. **Wolfgang**
June 20, 2011

"… he's simply objecting to your claim about "universal behavior" in N=4 SYM and asking for justification"

The only justification is a semi-empirical one: there are a number of 4d theories (not just N=4 SYM), for which one can form an AdS/CFT correspondence. They have different amounts of supersymmetry, different matter content, etc. Those properties (and only those properties) of the corresponding gauge theory plasmas, which agree between these different theories, have some claim to being "universal."

"They have no QCD-type scale, and completely different matter content. There is no reason to think their infrared behavior resembles QCD."

Their zero-temperature behavior is completely different from QCD. The claim is about (certain features of) their finite-temperature behavior.

"All you can do is, in a very specific background, define away the problem of "what is non-perturbative string theory" by saying that whatever it is, it's the same thing as a well-defined QFT."

To the extent that you can do computations in that QFT (by whatever techniques you, as a field theorist have available), you are computing the behavior of the corresponding string theory in an AdS background.

You surely didn't expect that determining the behavior of the string theory required no computation whatsoever! So the fact that the computation has been reduced to a field theory computation (in lower dimension) is about as simple a result as one could possibly hope for.

And that computation carries with it lots of predictions about the field theory, too: in certain limits, certain "known" features of the string theory ought to emerge from the field theory calculation (the supergravity limit was one such limit, the BMN limit is another). So one learns not just about string theory in AdS, but about (suprising!) features of field theory.

34. **Peter Woit**
June 20, 2011

Wolfgang,

My objection here is one of language, not about the science. When you write "quantum field theory" this is a well-defined term: at least conjecturally, for the relevant class of theories, we know what this means (i.e. how to non-perturbatively define the theory). When you write "string theory", you don't: what is really meant is "something (M-theory?) we don't even have a good conjectural definition of that has a specific limiting behavior that can be written down as a quantization of strings". I don't believe that the fact that certain qft's appear to have strong-coupling limits that can be identified with weakly coupled strings changes this and makes "string theory" a well-defined term.

35. **Peter Orland**
June 21, 2011

Universality is a very strong statement. It means that there are universal numbers.

36. **Wolfgang**
June 21, 2011

"My objection here is one of language, not about the science."

You do have a point.

People speak of AdS/CFT as a duality between the string theory in the bulk and a field theory on the boundary. Usually, when we speak about a "duality", we have two independently-defined theories, which we claim are equivalent. Here, as you say, the field theory, on the boundary, has an independent definition, but the bulk theory does not.

At best, it has a set of properties it should satisfy. In the IIB case:

* It should reduce, at low energies, to type IIB supergravity.
* It should have an S-duality symmetry, which acts on the axio-dilaton by fractional linear transformations
* It should have a certain set of BPS extended objects (branes), permuted by the S-duality symmetry.
* etc.

The "right" way to think about AdS/CFT is to take it to be the DEFINITION of the bulk theory. Then these properties become predictions about the field theory on the boundary.

It's certainly true that it would be nicer if there were independent definitions for both sides of the duality. Then one would get testable predictions in both directions (not just in one direction).

But, in the absence of such an independent definition, it is still true that AdS/CFT provides a nonperturbative definition of string theory in AdS. And it is nontrivial that this definition is consistent with all of the properties that we expect string theory to possess

37. **sanjay sood**
June 21, 2011

Dear Yatima,
The "hydro" refers to the hydrodynamics of strongly coupled quark gluon plasma that one is interested in studying. AdS/CFT allows one to calculate certain properties of this plasma such as viscosity to entropy density ratio of QGP in the IR region of QCD, where it is now known to be strongly coupled. The asymptotic freedom in QCD means that gauge coupling would decrease exponentially at lambdaQCD limit. This is what makes LHC such an interesting machine. In a few years, when gets up to full power, it would be able to push beyond the lambdaQCD limit and (hopefully) one would then observe the hydrodynamics of weakly coupled plasma! Since Ads/CFT only applies for the strong coupling case, it would not be suitable (in its current form) to study this new plasma. There is some conjecture though, that AdS/CFT applies for weak coupling as well. This is what's known as the 'weak' statement for AdS/CFT.

38. **Charles**
June 29, 2011

Rumor has it that Atlas has a "Higgs"-like excess at slightly more than 160 GeV. It has not hit the rumormill, but will do so soon. Also an excess of leptonic events at 950 GeV…..something like a Z'.

39. **Peter Woit**
June 29, 2011

Thanks Charles!

It would be pretty funny if ATLAS finds the Higgs right where the Tevatron experiments claim they have excluded it…

Anyone from ATLAS who feels like posting the abstract of any internal documents about this is encouraged to go right ahead.

40. **Shantanu**
June 30, 2011

Peter,
the 2011 TASI videos are online. See
http://physicslearning2.colorado.edu/tasi/tasi_2011/tasi_2011.htm

Quick Links

June 17, 2011
Categories: Uncategorized

- The House committee responsible for the DOE budget has passed a FY2012 appropriations bill, details here. Total funding for DOE Science is down .9% from FY2011 at $4.8 billion. HEP gets a .2 percent increase, Biological and Environmental Research is whacked %10.6, with the committee opposed to climate and atmospheric research being funded by DOE. The language about DUSEL argues against it becoming a DOE lab, but money is made available to keep options open. There is support for Fermilab's Project X (the "intensity frontier"), but a warning that it may not be possible to continue funding both the "intensity frontier" and the LHC (the "energy frontier").

Fermilab this week announced a program to offer a "voluntary separation program" under which they hope 100 employees will voluntarily leave. They're clearly trying to better position the lab for tight budgetary conditions ahead.

- Over in the Czech Republic, Lubos Motl is hanging out with President Vaclav Klaus and is one of the contributors to his 70th birthday Festschrift. Lubos may have a career ahead in Czech politics, too bad he left the US just before the Tea Party movement got going. He would have fit in quite well with them, but I guess at least in the Czech Republic, he can legally become President some day.
- From Physics World, it seems that Lawrence Krauss will be joining with 13 other very prominent academics to teach at New College for the Humanities, a new private university in London. The new university has caused quite a stir in Britain, since it's unlike anything else there. Tuition will be set at US private college levels, $29,000/year, twice what other British universities charge. The business plan is not public, but Wikipedia says 10 million pounds in funding for the first two years is coming from private investors, with the 14 senior academics getting a 1/3 equity stake in the venture. It's unclear how much teaching they'll each be doing, since most will retain their current positions elsewhere and just give anything from one to 20 lectures per year.
- The LHC is doing quite well, with over an inverse femtobarn delivered to the experiments already. For the latest, take a look at the slides of the talks here. At the KITP, there was a very interesting talk by Tim Nelson. He addresses the question of whether the LHC detectors, once their searches aimed at standard speculative ideas such as supersymmetry and extra dimensions turn up empty, can be reconfigured to look for other sorts of exotic possibilities, ones that the current triggers are not sensitive to.
- There's an article here about filmmaker Errol Morris, whose new film "Tabloid" is coming out later this year. I saw it a few months ago at a showing in New York, and highly recommend it. It's one of the most surprising and amazing documentaries I've ever seen. Real life is much stranger than fiction. In the article, Morris describes his early career, which included having Thomas Kuhn throw an ashtray at him and have him kicked out of the graduate program in philosophy at Princeton. He moved on to Berkeley, where he hung out with Dan Friedan:

> "I felt that he had destroyed my life," said Morris. It left him reeling for years to come: He still remembers sitting in a coffee shop at Berkeley with Daniel Friedan, a fellow Princeton exile and the son of feminist icon Betty, and commiserating over the frustrating time they'd had out East.
>
> "I'm talking about all these problems that I had with Kuhn, which was a constant refrain, and he's telling me about all the problems he'd had in the physics department," Morris recalls. "He said, you know, 'They just could not appreciate me. I had discovered a new kind of physics!' And I thought, 'Oh, no. This looks bad. This looks very, very, very bad. This is not going to turn out well. We're both going to the nuthouse.' "
>
> Of course, they didn't. Friedan would go on to win a Macarthur Fellowship, and be recognized for his pioneering work on string theory. Morris, meanwhile, left academia behind once and for all to make a movie about a pet cemetery, called "Gates of Heaven," which became a cult classic, and which Roger Ebert described as one of the 10 greatest films ever made.

- There's a conference going on this week and next at the ETH in Zurich on quantum gravity, with slides appearing here. My long held belief about quantum gravity is that it's a problematic subject unless some way can be found to connect it to unification with the rest of physics, and thus some sort of testability or good reason to believe one is on the right track. Matthias Blau promotes string theory by arguing that it should be judged:

> not by, say, its failure to (so far?) provide specific predictions for BSM physics, or disgust with some of the hype and overblown claims regarding string theory (I may share your feelings . . .)

Among other things, he explains some of the problems with M-theory, then notes that Tom Banks has a highly mystifying recent proposal about this:

> For very recent proposal for how to deal with (some of) these issues, see T. Banks, Fuzzy Geometry via the Spinor Bundle, with Applications to Holographic Space-time and Matrix Theory, arXiv:1106.1179 (and then please explain it to me . . .)

His talk, together with the recent preprint Is string theory a theory of quantum gravity?, provides a good understanding of what the problems are facing attempts to use string theory to quantize gravity, from the point of view of a string-enthusiast.

For the latest from the LQG camp, see Carlo Rovelli's talk here.

Comments

1. **anonymous**
   June 18, 2011

   The LHC has been doing extremely well, not "quite well".

2. **anonymous**
   June 18, 2011

   Would it be better to go to higher energy (1.e. 14 TeV) or try to improve luminosity or work on a detector upgrade? From Nelson's slides it looks like the improved luminosity outweighs any detector upgrades and if there is confidence in new splices then an energy upgrade does not present a risk? I can see this going on for many years..

3. **Yatima**
   June 18, 2011

   Lewandowski: Canonical LQG: soluble models and other advances

   "The canonical LQG provides more and more soluble models of quantum gravity with all the local degrees of freedom. The first model was LQG coupled to dust (Giesel-Thiemann). The current second model describes gravity coupled to massless scalar field. It is an exact generalization of the cosmological models of Loop Quantum Cosmology to the full theory with the local gravitational degrees of freedom."

   http://www.conferences.itp.phys.ethz.ch/lib/exe/fetch.php?media=qg11:lqgrecentadvanceszurich.pdf

4. **abbyyorker**
June 18, 2011

On the new college for …

It's just a new education scam. I did not know Mr Krauss was so cynical. Unless he really thinks he's going to help with his "1-20 lectures a year".

5. **Jon Lennox**
June 19, 2011

anonymous #1: I'm guessing your dialect of English is a UK one? In American usage, "quite" means "very", not "somewhat", so Peter agrees with you.

6. **OhDear**
June 19, 2011

As a Brit living in the states, I would say that "quite" and "very" mean the same on both sides of the Atlantic. We used to have a thing for understatement, but not these days.

7. **Aleksandar Mikovic**
June 19, 2011

There is a growing number of physicist, including the string theorists, who realize that the problem of quantum gravity is not just the problem of finiteness, but it also involves the problem of the fundamental degrees of freedom and the problem of applying the standard quantum mechanics to the whole universe. Obtaining a finite or renormalizible quantum gravity theory with the right classical limit, after all, is not that difficult: for example, take the Horava gravity. However, many people believe that the structure of the spacetime changes at the Planck length, so that string theory and loop quantum gravity (LQG) have many more adherents. When comparing string theory to LQG, both have a non-trivial quantum geometry (strings vs. spin foams) and both are finite and have the right classical limit. String theory offers a natural unification of interactions, while LQG can address more naturally the quantum cosmology. String theory allows a multiuniverse, and LQG as well, through the group-field theory approach, which is a type of the third quantization. Still, neither theory has nothing to say about the problem of applying quantum mechanics to the universe, and even less to the case of the multiuniverse. It is clear that one needs to modify the quantum mechanics, and this is the hard part of the quantum gravity problem.

8. **Peter Woit**
June 19, 2011

Yes, I meant "quite" as in "very".

If you look at the projections made earlier this year, typically they were for around 3 inverse femtobarns for 2011. It seems to me that they're on track for this, which is great.

If they do 5 inverse femtobarns, that might merit "extremely", we'll see what happens, maybe that's possible, maybe not. As a general rule though, this subject is hype-ridden enough, I don't see the point in adding to the problem.

9. **noname**
June 20, 2011

OK, would you agree on the following? If by the end of 2011 each experiment collects 3/fb you'll write "the LHC is doing great". If they collect between 3 and 5/fb you'll write "the LHC is doing extremely well". Anything beyond 5/fb (which I believe is what will happen) will merit your "the LHC is doing outrageously well". Deal?

10. **Peter Woit**
June 20, 2011

noname,

Doing just above 3 inverse femtobarn would still be more or less according to plan, not exactly an extreme situation. I certainly hope you're right, and they get above 5 inverse femtobarns, which would get into a more extreme situation. For "outrageous" I think they need to go to 10…

11. **anon**
June 20, 2011

this one goes to eleven.

12. **Caramel**
June 20, 2011

Am I correct in reading Giddings' paper as saying that no-one no how to predict particle-particle scattering at the Planck regime using string theory? How does that square with all the claims I've heard along the lines of "if we only had a Galaxy-sized particle accelerator, we'd *definitely* observe a tower of excited string states"?

13. **Caramel**
June 20, 2011

"…that no-one knows how…" rather.

14. **Peter Woit**
June 20, 2011

Caramel,

I've often pointed out that the argument for those claims isn't very solid. It assumes that the string dynamics at that scale is weakly coupled and that perturbation theory is valid. There never was a good argument that this had to be true. Giddings explains in detail how non-perturbative effects are important even in scattering processes at energies far above the Planck scale.

15. **Casey Leedom**
June 21, 2011

Wow. I know that this is a "physics blog" but no one else is concerned about the 10.6% decrease in Biological and Environmental Research? High Energy Particle Physics is either “hopeless” or “doing great” depending on which side of the String Theory Divide you fall on but Biological Research is doing amazing things these days and promises both fundamental new knowledge and relevant medical breakthroughs. Very sad if that’s taking a hit.

Casey

16. **Peter Woit**
June 21, 2011

Casey,

This is just the DOE budget. Most biological research is funded elsewhere (NIH?). Also, the budget process is a long ways from completion. I don’t think this necessarily reflects how such research will do overall in the budget.

17. **Casey Leedom**
June 22, 2011

Ah, good point. But still, when I worked at Lawrence Livermore National Laboratory there was a big biology effort (breast cancer, human genome, etc.) and my friends at Lawrence Berkeley Laboratory do a ton of basic biology research.

18. **Mark Decker**
June 22, 2011

Well good for Krauss. Maybe some of the lectures will wind up on youtube so some of the Brits (and us) who can’t afford can still watch. In any event, it will be sad to see him go. He is one of the few Physicists left in this country who still has his sanity.

19. **John Baez**
June 24, 2011

I’m at that Zurich conference. You can see some very fragmentary notes on it starting here.

Bad Boys of Physics

June 22, 2011
Categories: Multiverse Mania

Scientific American is running a Bad Boy of Physics story (also see here) in the July issue, about Lenny Susskind. Here's the "nut graph":

> Physicists seeking to understand the deepest levels of reality now work within a framework largely of Susskind's making. But a funny thing has happened along the way. Susskind now wonders whether physicists can understand reality.

In the interview, Susskind explains that he was a bad boy as a youth, but "just so much better than anybody else, including the professor." In recent years he has been the most prominent promoter of the string theory multiverse, and now claims that this pseudo-science convincingly dominates the field (SciAm seems to agree…), with the situation just like in the early days of QCD:

> A large fraction of the physics community has abandoned trying to explain our world as unique, as mathematically the only possible world. Right now the multiverse is the only game in town. Not everybody is working on it, but there is no coherent, sharp argument against it.
>
> In 1974 I had an interesting experience about how scientific consensus forms. People were working on the as yet untested theory of hadrons , which is called quantum chromodynamics, or QCD. At a physics conference I asked, "You people, I want to know your belief about the probability that QCD is the right theory of hadrons." I took a poll. Nobody gave it more than 5 percent. Then I asked, "What are you working on?" QCD, QCD, QCD. They were all working on QCD. The consensus was formed, but for some odd reason, people wanted to show their skeptical side. They wanted to be hard-nosed. There's an element of the same thing around the multiverse idea. A lot of physicists don't want to simply fess up and say, "Look, we don't know any other alternative."

Susskind had a distinguished career as a theorist for many years, and has managed to do quite well with his multiverse campaign for quite a while now. There has been a lot of coverage of this story on this blog, for some high points, see here, here, here and here.

In other news, the media has been full of stories about another physicist who has been a bad boy, David Flory. He started his career as an HEP theorist back in the late 1960s, as a student at Yeshiva University, and collaborator there with Susskind. Like a huge number of other people, he got his permanent academic job in 1969, and has been at Fairleigh Dickinson University ever since.

Comments

1. **anon**
   June 22, 2011

   maybe Flory was working on G-string theory.
   oh, that joke is bad.

2. **Lee Brown Jr.**
   June 22, 2011

   Susskind now wonders whether physicists can understand reality.

   So, Susskind thinks that if he can't understand it, then nobody can.?

   BTW: SciAm also celebrates the main sequence of stars in this issue. Great article. In fact, if you look closely enough at the depiction of the "periodic table of the cosmos" you can see Susskind's ego grouped in with the supergiants.

3. **In Hell's Kitchen (NYC)**
   June 22, 2011

   obviously, working at FDU for 30+ years isn't good for you !

4. **Bernhard**
   June 22, 2011

   The way Susskind sees the history of QCD can be put into context using David Gross´s Nobel lecture introduction:

   "The progress of science is much more muddled than is depicted in most
   history books. This is especially true of theoretical physics, partly because
   history is written by the victorious. Consequently, historians of science often
   ignore the many alternate paths that people wandered down, the many false
   clues they followed, the many misconceptions they had. These alternate
   points of view are less clearly developed than the final theories, harder to
   understand and easier to forget, especially as these are viewed years later,
   when it all really does make sense."

   Of course QCD does make sense and we now do know it is correct. But we know it because (after asymptotic freedom rescued it) we could be guided by experimental data. This is the only reason there is any consensus among physicists, same can be said about the Big Bang or even more basically quantum mechanics. I´m appalled to see Susskind trying to push the argument that consensus is just a sociological phenomenon among scientists: "The consensus was formed, but for some odd reason, people wanted to show their skeptical side" Really? Meaning what? We don´t want to swallow the untestable multiverse "theory" just to play hard ball? Give me a break.

5. **Chris Oakley**
   June 22, 2011

   David Flory seems to have run a web site similar to http://www.ratemyprofessors.com, except rating whores instead of professors (incidentally there *is* a http://www.ratemywhore.com – I just checked – but this is not obviously connected with David Flory). Curiously, Professor Flory is not well reviewed is http://www.ratemyprofessors.com – maybe he had more important things to think about than teaching physics.

6. **Peter Woit**

June 22, 2011

Bernhard,

I think Susskind's comparison to QCD is absurd, and evidence that the man really has lost his marbles. From the beginning QCD made all sorts of precise predictions, and some were tested decisively early on. Unlike the string theory multiverse, it was a well-defined theory from the beginning, very closely related to the rest of the standard model. In 1974 it was very new and people were just starting to understand how to deal with such an unusual theory, weakly coupled in the UV, strongly coupled in the IR (and, to this day there's a lot we don't understand). The fact that, months after its discovery, people were not sure that a new unusual theory was the final answer is neither surprising, nor has anything at all to do with the string theory multiverse (which now has been around a decade or so, with no sign of prospects for predicting anything at all).

7. **Eric habegger**
   June 22, 2011

   "Flory told police he did not make money off of the (prostitution) website and instead saw it as a hobby"

   He obviously was aware that he was one of the more fortunate professors in being tenured and seemingly financially secure. All men need a good hobby.

8. **Sean the Mystic**
   June 22, 2011

   So I guess H.P. Lovecraft was right then? The multiverse is incomprehensibly alien and vast, far beyond our puny attempts to rationally understand it. Is this is where the Enlightenment ends, and the new dark age of Cthulhu cults and hyper-dimensional string theories begins? Why do I have this strange image of physicists like Susskind wearing black robes and supplicating beneath the stars in some black rite of submission before the vast, inscrutable Cosmos? Oh never mind, I'm probably just crazy. Hahahahhahaahahhahahahahahahahah….

9. **abbyyorker**
   June 22, 2011

   L. Susskind. Guilty by association (40 years ago)? Please say that's not what you mean….

10. **Peter Woit**
    June 22, 2011

    abbyyorker,

    No, obviously the fact that Susskind collaborated with and/or supervised as a student Flory 40 years ago has zero to do with Flory's recent activities.

    After 40 years the two of them ended up in very different places. I suppose you could argue about what is more morally noxious, promoting pseudo-science or promoting prostitution…

11. **Mystic**
    June 23, 2011

    Physics is dead! Long live the new physics! Physics has died, and reincarnated as religion! This is the dawn of a New Age!

    Welcome to Reality!

12. **anon**
    June 23, 2011

    Do you think Flory asked a post doc to write the software, or he programmed it himself? It sounds very similar to math-stack to me.

13. **anon**
    June 23, 2011

    Maybe it should be titled "Senile Boy of Physics"

14. **anonymous**
    June 23, 2011

    LHC will blow them out of sight and into oblivion.

15. **Emanuel Derman**
    June 23, 2011

    Off topic and in bad taste:
    The Susskind – Flory first paper was on Hadronic Currents. Kind of says it all. Second word close to what he was offering to rent, first word close to what it took to rent it. Published in 1969. There is a tide to the affairs of men which taken at the flood …

16. **ateixeira**
    June 23, 2011

    "I suppose you could argue about what is more morally noxious, promoting pseudo-science or promoting prostitution…"

    Really?! WOW!

    Susskind has really lost his marbles… Too bad, I guess.

17. **Fred Tucker**
    June 23, 2011

    Peter, Flory hasn't been convicted of a thing. He could well be innocent of all charges. Doesn't seem right to provide a forum for people to spread nasty rumors about a respected physicist.

18. **tristan stefan**
June 23, 2011

After seeing this blog entry I looked at some stories relating to the investigation, it seems pretty unlikely he's innocent in the sense of caught by mistake. On the other hand, is he guilty of an actual crime, ethically? It looks like he was rating prostitutes online and sharing info about them. In my opinion promoting pseudo-science is just as bad.

19. **Joe Ruski**
June 23, 2011

The new look is acceptable. I want credit for having suggested the archive widget. You owe me, forever.

I've been following the lulzsec hackings. You might want to get rid of your meta "login" link and change the name of the php login file. In general, I've been thinking about passwords, answers to security questions, etc., thinking about security in general.

I've thought of a use for twitter for someone who mainly only wants to blog. To promote a blog post on twitter, you would tweet something like this for every blog post:

"Bad Boys of Physics" [link provided of course] is up on the blog. Comments by twitter are welcome.

You might also link to the tweet in your blog post so a person could go to twitter.com and reply to your tweet from there.

There would be several reasons to do this: 1) to allow people to use twitter to get notified of new posts, 2) to allow comments on twitter that you might not want on your blog, and 3) to give people another way to give you feedback; some people might tweet you, but not email you or comment on your blog.

Twitter comments have some advantages. A person's comment can only get so stupid when limited to 140 characters, plus your twitter account won't display comments from people you're not following. It's a happy medium. It lets people comment, but you don't have to host their comments. To find replies to someone's tweets, people have to do a search like this: http://twitter.com/#!/search/%40lulzsec

Here's a guy who uses twitter to promote his blog posts:
http://gerard.cc/
http://twitter.com/gerardvroomen

On his blog, he has a "Tweet" icon below each post, but those links do retweets and searches based on the blog post URL rather than on @gerardvroomen; thus my suggestion above about linking to the tweet in the blog post, so that the twitter replies are synced to the appropriate tweet.

I've had no use for twitter, but people are using it more all the time. It's taken me a while to see the purpose in people tweeting cryptic messages, but I've started to see that some people use it for more than useless and trivial details about their life.

20. **Joe Ruski**
June 23, 2011

I don't tweet yet, so I don't know all the ways twitter forces a tweeter to promote those who reply to a tweet. One way is that if you click on the arrow next to a tweet, it'll show you the replies to that tweet, although I think a person might be able to suppress certain replies on his or her twitter page.

In case you care.

It's not all upside. You wouldn't want me riding on your coattails, would you?

21. **Peter Orland**
June 24, 2011

I am kind of disturbed by some of the negative comments about Susskind here. I don't
care for multiverse ideas, but Susskind made very important contributions to field theory and particle physics (strong-interaction strings, parton models, U(1) problem, Hamiltonian lattice QCD, technicolor). You can have a strong difference of opinion without getting nasty…

22. **Peter Woit**
June 24, 2011

Joe,

It does seem there's a lot happening now on twitter or other social media, moving away from blogs. For now though, since I'm already spending too much time on the internet, I think I'll continue not getting involved in that. It seems quite likely that twitter will either evolve into something interesting or die off within a couple years. I'll try and wait that out and see what happens…

Peter O.,

When people like Susskind, who have a history of real accomplishments, start saying completely idiotic things (like his comparison of QCD and the multiverse), it does create a problem. Do you accurately describe what they are saying as idiotic, or do you politely avoid doing so in recognition of their past history? Susskind unfortunately has gotten and is getting a lot of positive attention for his nonsense, one reason being that most people in the community think it best to politely ignore what he is up to.

In general my own policy is to stick to serious arguments and avoid personal characterizations of anyone I disagree with, and to encourage the same from commenters. In Susskind's case, given his history of launching nasty personal attacks on me and on others who disagree with him, I'm a bit less motivated to worry about whether he is getting the proper respect he is due.

In his case as in quite a few others in recent years as string theory has collapsed, I'm continually suprised not only by the kind of behavior and argument engaged in, but also by the rest of the community's tolerance of it.

23. **Chris Austin**
June 24, 2011

I thought Lenny Susskind's story about the informal poll he conducted in 1974 was an interesting historical anecdote.

But David Gross seems to have given QCD more than a 5 percent chance in 1974: see page 20 of hep-th/9809060, “Like an atheist who has just received a message from a burning bush, I became an immediate true believer.”

24. **Anon**
June 24, 2011

The quality of Scientific American is unfortunately not nearly what it used to be in, say, the 70s or 80s. The caliber of journalism in this publication has become, overall, very poor. Even just the first paragraph is illustrative of this depressing downwards trend:

“And he helped to develop the modern conception of parallel universes, based on what he dubbed the “landscape” of string theory. It spoiled physicists’ dream to explain the universe as the unique outcome of basic principles.”

Both sentences are strictly speaking false, yet marketed as accepted truth. It exhibits the kind of intellectual laziness you would expect to find in a mediocre high school essay.

The writers of this magazine should be forced to read the New Yorker for examples of beautifully constructed and intellectually coherent writing.

25. **Peter Woit**
June 24, 2011

Anon,

I don’t think it’s fair to blame the journalists. They’re just reporting the claims made by some of the most prominent and respected figures in the field of theoretical physics. The two sentences you quote are ones that I’m sure Susskind would describe to journalists as accurate. And he’s far from the only one who would do so.

People seem to expect that science journalists will do the job that other physicists should be doing but aren’t: pointing out the reasons why this is pseudo-science. You can’t expect journalists to have better critical scientific skills than the people they are covering.

26. **Anon**
June 24, 2011

Other physicists are pointing out that this is pseudoscience.

If the New Yorker, which is not a science magazine, can have articles of this quality (written by non-science journalists):

http://www.newyorker.com/archive/2006/10/02/061002crat_atlarge
http://www.newyorker.com/reporting/2008/07/21/080721fa_fact_wallacewells
http://www.newyorker.com/archive/2006/08/28/060828fa_fact2

why shouldn’t we expect better from SciAm?

27. **Leonard Lerner**
June 24, 2011

I have just read “Not even wrong”, a lot of what it says I suspected but did not know for sure. I also belong to the generation of 1980’s PhDs in particle physics, and recall the great the triumph with which we were told at the 1986 summer school in Durham that string theory has proved that the world is ten-dimensional, because only in that case do its infinities cancel.

I wanted to comment concretely on what I think is a more general problem, from which much of what the book discusses flows.

The sharp drop of honesty in physics. And much of this has to do funnily enough with money. On the one hand theres too little, so that only 10% of PhD in particle physics can get a job in what they were trained (as mentioned in the book) on the other hand, there are 20 or so conferences a year in “nice places” for string theorists to choose from. SO money is there, but it is being misspent by those at the top, because of their desire for a certain ‘lifestyle’. They then have to justify the lifestyle by a huge output of papers from their subordinates. This generates a certain lack of honesty, but I will come back to the concrete case I wanted to mention.

Physics Review Letters has been a premium journal in physics, everyone is proud if their article is accepted there. It has a comments and reply section, by now absent in most other journals, where readers are invited to comment on regular articles published in that journal. If you look at the comments you will find that if they do raise some controversial issues about the original article, there is always a ‘collegial’ responce in the reply, which gives further food for thought to both parties. There is never a comment pointing out a major error in a published article to which the authors have no response. Yet there are rules in PRL stating that if the response can not pass the refereeing process, the comment will be published without a response. Yet no comments pointing out major shortcoming in articles in PRL with no response have ever been published.

A few years ago I noticed an article in PRL on some work I did a few years previous, and which I put aside considering it of no promise. The PRL article however manipulating the same formulas got a different result, gave it a ‘big’ interpretation, and this constituted ground for PRL publishing it. I re-analysed the formulas, saw the mistake they made (it was to do with treatment of singularities), and then wrote a comment explaining how these singularities should be dealt with, explicitly drawing terms from a sum the aticles authors claimed was irrelevant to their caclualtions, and showing how it spoiled the overall conclusion. I submitted this to PRL and wondered how it would go. The authors submitted a response which was close to nonsense, and went close to calling me names. The first referee of both comment and reply took 3 months to produce 4 pages restating my comment and the authors reply, and then said he had not the qualifications to decide in the matter as it involved complicated mathematics. The editor asked me to review my comment if I wanted it considered further as refereeing the matter was clearly problematic. I did as the editor asked (which enlareged the comment) and asked the editor to go for a second review. This time he disappeared. I waited 4 months, then wrote several times and got no response. I rang several times, his secretary would answer and then would tell me he is ‘not in’. After several such replies she admitted he was ‘in the building’, but not here, he will return my call, or write to me. After 6 months I did get a message from him stating he is very busy, he is not sure about the matter of my Comment, it is not suitable for publication as is, but Im ‘free to submit it elsewhere’. I raised the matter on appeal, which PRL expressly sets out the rules for. After 4 months I got a response. There were two reviews. The first from 6 months ago stating the comment demonstrates serious problems with the published article, but the reply is unacceptable and should not be published. This was followed by a second review which was generated after my appeal. The reviewer stated that what I raised was all very well, but he would really like to see another problem addressed in the same comment. The editor added his verdict, as my comment was already on the borderline in terms of space it has now run out of both space and time, and the matter is closed. After waisting 18 months of my time and effort, I had the article published in a European (also prestigeous) journal. But what this really demonstrates is what I had commented on at the start of my email.

Thanks for the book

Leonard

28. **Shantanu**
June 25, 2011

Interesting post, Leonard. Can you point to a copy of the article you are referring to?

29. **David Bailey**
June 25, 2011

"I don't think it's fair to blame the journalists. They're just reporting the claims made by some of the most prominent and respected figures in the field of theoretical physics."

Blame is possibly not the right word, because journalists are only responding to their editors, who are in turn trying to market a magazine.

I see the problem – which I am sure goes much wider than physics – is that magazines like New Scientist and Scientific American have tried to sell themselves to a larger and larger audience. One way to do that (and it may not be an explicit conscious decision), is to pick incredibly grandiose theories, and present them in a shallow way. Explaining that other physicists don't agree, and quibbling over the details, would just spoil the story!

30. **Roger**
June 25, 2011

Anon, you might like the writing style of the New Yorker, but those science and math articles are pretty horrible for their content.

31. **Jack Lothian**
June 25, 2011

I agree with Anon. I subscribe to SciAmer & have for 30 years. The quality of the writing & editing started to declined in the late 80s & the quality of the articles has dramatically plunged in the last 10 years. I pay a lot for the subscription & each year I debate about dropping it because of the lack of interesting articles. I read a lot of math journals as well & in the last few years I have seen a decline in quality. Scientific & math publishing is becoming exclusively a profit business & appealing to the masses brings in more profits. I do not see this trend reversing. I disagree with Peter on this one, it is the owners of journals who are steering the editors towards mass appeal articles. There are still lots of good physists & mathematician's out there but they lack mass appeal.

32. **peterg**
June 26, 2011

I do think it's fair to blame 'the journalists'.

Even before Peter published his book, a decent journalist could and should have known that String Theory as a description of measurable reality runs into huge problems. Around 2000-2001 I met a friend who worked in High Energy Physics in the 70s and 80s and who turned to ST somewhere in the 90s. "So, how do you like String Theory?" I asked him. He replied: "It's great! But it's not physics and I have no idea how we could turn it into physics."

Not to mention that little fact is bad journalism. Susskind apparently says that "there is no coherent, sharp argument against (ST)". "It's not physics" seems to be a pretty sharp and coherent argument to me.

However, having been a journalist myself, I think I understand what's happening.

1) ST is the only game in town, and you're flooded with news about it. You can't keep quiet about it, because everybody else will be writing about it.
2) Many Great Names have put their weight behind it. You need some courage as a journalist to criticize people like Susskind. You know that your editor is going to ask: "Who the f!ck are you?" or "So, how many Nobel prizes did your friend win?"(*)
3) Claims about ST are very sexy and impressive for a lay audience.

Added to this are a few other factors.

4) There's a tendency to limit journalism to 'reporting', without providing much context. The idea is that readers are mature enough to sort it out by themselves.
5) Commercial pressure and lack of time.

If you can't live with this situation – and no good journalist can, in my opinion – you're weeded out. Only the mediocre and the bad ones are left at the end. So yes, I think it's fair to criticize the journalists. You probably won't be criticizing the good ones.

(*) It actually happened to me when I was discussing Prigogine with someone. I mentioned a curious phenomenon: the better scientists knew what his ideas were about, the more critical they were. Reply: "So, how many Nobel prizes do these scientists have?" I was lucky and could mention Anderson.

33. **Cosmonut**
June 27, 2011

Regarding SciAmer – Does anybody else get the feeling that all their physics-related articles nowadays seem to be rehashes of things published in the 80's and 90's ?

Apart from a 1998 article on the accelerating expansion of the universe, there seems to be very little which is new and authentic (as opposed to vague speculation).

I also notice that SciAmer is switching towards more biology/enviornment related articles. This is just an impression – I don't have any stats.

34. **chris**
June 27, 2011

Leonard,

i think your story precisely hits the point. these days, there definitely is a lack of Paulis or Feynmans who declare crap to be crap. wrong or misleading work of a large collaboration can survive long enough to support a few nice careers and generate millions in funding money. respected people are forced into situations where it is socially unacceptable to say they were wrong – too many real life things depend on them being correct. so they act accordingly and crap does not get weeded out at the pace it should.

this is very frustrating, especially for the younger ones. they are held in precarious situations long enough to make them align to the main stream and shut up. it takes some serious nerves today to declare some wrong ideas wrong.

35. **Math Student**
June 27, 2011

Peter Woit wrote

"It seems quite likely that twitter will either evolve into something interesting or die off within a couple years. I'll try and wait that out and see what happens…"

Wasn't that your reaction to the first string theory revolution? Looking forward to your anti-twitter book in 20 years 🙂

36. **Peter Woit**
June 27, 2011

Math Student,

Good point. One major difference though is that, about the fate of twitter, I couldn't care less….

37. **Chris Austin**
June 27, 2011

chris,

"wrong or misleading work of a large collaboration can survive long enough … "

That is a very startling assertion, and a little bit alarming. Are you thinking of lattice gauge theory or HEP experiment? It really doesn't seem likely that something like that could happen when you have two competing experiments like CDF and D0, or ATLAS and CMS. Do you know something to the contrary?

38. **G.E. Hahne**
June 27, 2011

Hi Peter,
Susskind's beliefs on humans' inability to solve some current physics quandaries have at least one historical precedent: I can't recall a citation, but remember reading that at the Berlin Colloquium in the early 1920's, both Einstein and von Laue manifested scepticism that physicists were smart enough to develop a successful theory of atomic structure.

39. **Peter Woit**
June 27, 2011

G.E. Hahne,

Interesting. Whether human beings will get stuck at some point in their ability to understand fundamental physics is an interesting question. One sure way to get stuck is to react to an idea not working out not by abandoning it and trying something different, but by starting to work on an elaborate pseudo-scientific apparatus to explain why your idea was right even though it couldn't ever predict anything. I don't think Einstein or on Laue ever went in that direction…

40. **Leonard Lerner**
June 29, 2011

Here's the reference to the article. B. Elattari, S.A. Gurvitz, Phys. Rev. Lett. 84 (2000) 2047;. The editor of PRL Comments if anyone is interested was and is George Basbas, who played this funny game with me. I did have my article published, in Physics Letters, but that is not how it should be. Firstly without thoroughly reading my article you would not understand the EG article is flawed. Secondly as it was not now a comment it had to be a positive article – I had to write about quantum dots and the Zeno effect, which is a misconception I would never have liked to write about in the first place. So why did I bother? Well with Basbas help I wasted 18 months and I wanted to recoup my time somehow. But this is not at all how it should be.

On the same point, there was a flourish of articles (including PRL) a few years ago about hadronization in the light cone gauge. Now as anyone would know QCD is gauge invariant, so you dont expect to find hadrons in the LCG but free particles in another gauge. But with no progress, people are desparet these days, so this continued for a while. I certainly learnt not to pick up pen and paper and write a comment, and I think so have most people.

I dont think the string theorists whom Peter is rightly attacking, are hearing anything they don't already know. They would be out of a job if what they regard as a cool activity suddenly had to be judged by real world standards. Then articles could be rejected like Peter Higgs article once (unfairly) was, on the grounds that this has no relevance to physics. They would then have to publish in mathematics journals and abide by much higher standards. The truth is that physics has a tradition of accepting lower standards of rigour than mathematics not because physicists care less if something is wrong, but on the tacit assumption that physics is harder than maths beacuse unlike the former you are working with nature where you do not see the whole system. So people publishing in physics on the pretence that this has something to do with the real world, when they make no attempt to make sure it does are cheating this.

41. **coolstar**
July 8, 2011

I agree with anon above along with several others about the decline of SciAm. I don't remember when their last big editorial change was, probably pre 1998, but that seems to be when the slope became pretty extreme. I stopped buying and reading it soon after. They have one editor I find particularly noxious for his arrogant, right-wing views. I find Discover to have much better science and to be much better written. In the U.K., I've found the BBC magazine Focus to be much better than SciAM and I recently discovered Eureka, and it also seems to be better than SciAm, though I think the writing in Discover is still better than in those two.

42. **Anon**
July 8, 2011

I agree with coolstar. I almost always feel that I wasted my time with SciAm, which has become mostly empty fluff targeted at 12 year old ADHD sufferers (what with all those intrusive little summary boxes, lest anyone be expected to concentrate for more than 20 seconds at a time), but I do still read Discover, which most of the time (though not always) tends to have better quality content.

43. **Eric Baird**
July 9, 2011

Back when we were waiting for the LHC to be completed, I saw a number of physicists online complaining bitterly about the low standards of reporting, and criticising articles that frequently used inane headline-grabbing terms like "Big Bang Day" and the "Big Bang Machine".

In fact, "BBD" and "BBM" didn't seem to be media inventions, they were promoted at the media by Brian Cox, who worked at the ATLAS project at LHC (and then got himself a TV presenting gig for the BBC). Cox seemed to be on a one-man mission to turn "Switch-On Day" into a full-blown media event, and if anything, the news reports were a fairly muted version of some of his statements. But the journalists got the blame.

IMO, a number of scientists have worked out how to “game” the media by building “headline” phrases into their press briefings, and it’s then difficult for a reporter to decide //not// to use the obvious headline that they’ve been fed, on the grounds that they reckon that the scientist was exaggerating. A news editor would say that it’s not the job of a reporter to “correct” a scientist’s statements to make them more scientifically accurate, or less cheesy.

“Gaming the system” works over the short term, and generates the headlines and media column inches, but unfortunately the readership eventually gets tired of being constantly hit with exaggerated news stories and stops listening. I’ve heard a number of non-scientists complaining about wide-eyed articles on modern cosmology, because, as one of them put it “These guys just make this ____ up”.

Same thing happened with string theory – to a lot of the general public, string theory is now regarded as a bit of a joke. They may not understand the first thing about what string theory //is//, but they understand how to spot probable B.S., and some of the statements about ST that drifted into the popular press sometimes strayed too far into unintentional self-parody. If a group of scientists make those sorts of grandiose statements, the public tends to give them a few years to come up with something concrete to back those statements up, and if they don’t manage to turn them into something more concrete, tends to regard the whole field as a sham from that point on.

I think that the public used to give scientists the benefit of the doubt a lot more, but too many groups abused the trust that was given them to get more funding. After decades of too-cheap-to-meter fusion power being always “just around the corner”, and biotech companies promising that that they’d be able to solve world hunger any year now if we just gave them special legal concessions, people increasingly stopped listening.

Increasingly, I think the general public are beginning to see scientists as just another self-serving lobbist group after public money, who’ll say anything to get funded. I think that sometimes these researchers think that what they’re doing is playing a harmless and neccesary game to get funds, and they don’t always appreciate the damage that they’re doing.

Strings 2011

June 27, 2011
Categories: Strings 2XXX

Strings 2011 started today in Uppsala, with attendance quite a bit lower than in the past (259 registered participants, versus 500 or so at some of the past such conferences). One reason for this may be the high conference cost ([discussed here]), another may be that excellent [video of the talks] is available, so why bother traveling to Uppsala?

The opening talk was by David Gross, who tried to address the question "Where do we stand?" for string theory. He claimed the field is "extremely healthy", "vibrant and exciting", "making enormous progress in a variety of areas'", with "stupendous progress" in N=4 planar SYM. At the same time, he acknowledged that it was "very sobering" that string theory was 43 years old.

In the past, Strings XXXX conferences often featured a call for progress towards making predictions that could be tested at the LHC. With LHC data now coming in, Gross acknowledged that this had been a failure: there are no string theory LHC predictions. He put a positive spin on this by noting that the lack of any BSM signal at the LHC so far is not a worry for string theory, since string theory can't be tested at the LHC. As for the lack of any supersymmetry signal so far, he says that "I personally am not yet worried", while acknowledging that some people are becoming pessimistic. While no SUSY is not a worry for string theory, he feels that "it would be awfully nice for string theory if SUSY appeared". Supposedly he has made bets on SUSY at the LHC, but he gave no indication of when he would start to worry (or pay off the bets) if SUSY continues to not be there.

The main area of progress he sees is the usual gauge-gravity duality that has dominated the field for years, together with progress on N=4 SYM amplitudes. He sees Verlinde's "Entropic Gravity" as an "exciting development I find enormously interesting". Evidently later this week Verlinde will discuss his latest ideas about this which supposedly include an explanation of dark energy and dark matter.

Gross went over quickly the questions about string theory he first raised in a similar talk 26 years ago, which mostly remain unanswered, including the basic one of "What is String Theory?". The additional questions raised by attempts to understand the emergence of spacetime in a deSitter background were one factor that inspired him to end with the quote that:

> The most important product of knowledge is ignorance.

To which he added "After 43 years of string theory , it would be nice to have some answers."

Surprisingly, not a word from Gross about anthropics or the multiverse. I assume he's still an opponent, but perhaps feels that there's no point in beating a dying horse. Susskind isn't there and oddly, the only multiverse-related talks are from the two speakers brought in to do public lectures (Brian Greene and Andrei Linde, Hawking's health has kept him from a planned appearance). So the multiverse is a huge part of the public profile of the conference, but pretty well suppressed at the scientific sections. Also pretty well suppressed is "string phenomenology", or any attempt to use string theory to do unification. Out of 35 or so talks I see only a couple related to this, which is still the main advertised goal of string theory.

I'm looking forward to the talks of Witten, Gaiotto and Gukov, which I hope will provide a gentle introduction to their intriguing recent long papers on the arXiv. To the extent I find time to watch talks this week and have any comments about them, I'll try and add updates to this posting.

Update: After looking at most of the talks online, the most remarkable thing about Strings 2011 is how little there is about string theory. One of the speakers, Chris Hull, started off his talk with the comment:

> At lunch today one of the organizers was observing that my talk was unusual in being one of the few talks actually about string theory. It would be interesting to speculate on what that might mean about the state of the field, but it would be invidious to do so here.

One of the main themes of the conference so far has been study of mathematically interesting supersymmetric QFTs in 3,4,5 and 6 dimensions, often obtained from a specific class of 6d theories, which themselves remain poorly understood (what is known about them was reviewed by Greg Moore). Witten gave an overview of his work relating Khovanov homology and QFT, which involves a chain of various 6d, 5d, 4d, 3d and 2d QFTs. Nati Seiberg reviewed the technology used for constructing these theories on various special backgrounds, noting that this was all about "rigid" SUSY theories, with supergravity and string theory making no appearance.

Update: The videos of the talks are now all up. I took a look at the Verlinde talk, and the ideas he is putting forward still strike me as pretty much empty of any significant content. In Jeff Harvey's summary of the conference, he notes that many people have remarked that there hasn't been much string theory at the conference. About the landscape, his comment is that "personally I think it's unlikely to be possible to do science this way." He describes the situation of string theory unification as like the Monty Python parrot "No, he's not dead, he's resting." while expressing some hope that a miracle will occur at the LHC or in the study of string vacua, reviving the parrot.

That the summary speaker at the main conference for a field would compare the state of the main public motivation for the field as similar to that of the parrot in the Monty Python sketch is pretty remarkable. In the sketch, the whole joke is the parrot's seller's unwillingness, no matter what, to admit that what he was selling was a dead parrot. It's a good analogy, but surprising that Harvey would use it.

Comments

1. **Marcus**
   June 27, 2011

   The talk that Verlinde is scheduled to give later this week at Uppsala has the same title as one he gave last week (22 June) at Perimeter. Anyone interested can watch the Perimeter video of the talk, which got some lively reactions from the audience.

   http://pirsa.org/11060065/
   The Hidden Phase Space of our Universe
   Erik Verlinde
   By combining insights from black holes and string theory we argue for the existence of a hidden phase space associated with an underlying fast dynamical system, which is largely invisible from a macroscopic point of view. The dynamical system is influenced by slow macroscopic observables, such as positions of objects. This leads to a collection of reaction forces, whose leading order Born Oppenheimer force is determined by the general principle that the phase space volume of the underlying system is preserved. We propose that this adiabatic force is responsible for inertia and gravity. This fact allows us to calculate the hidden phase space volume from the known laws of inertia and gravity. We find that in a cosmological setting the appearance of dark energy is naturally explained by the finite temperature of the underlying system. The adiabatic approximation that leads to the usual laws of inertia and gravity breaks down in the neighborhood of horizons. In this regime the reaction force degenerates into an entropic force, and the laws of inertia and gravity receive corrections due to thermal effects. A simple estimate of these effects leads to the conclusion that they coincide with observed phenomena attributed to dark matter.
   Date: 22/06/2011 – 9:00 am

2. **Bernhard**
June 28, 2011

"While no SUSY is not a worry for string theory, he feels that "it would be awfully nice for string theory if SUSY appeared"."

I confess I keep being puzzled about this situation. As I understand the situation with string theory without SUSY is pretty ugly, isn't it? I mean, what is left is the bosonic ST, so why people like Gross keep on insisting no SUSY is not necessarily bad for ST? It seems to me that more than half of their business is gone without SUSY.

3. **anon**
June 28, 2011

If the Higgs but no Susy is found by the LHC , I would say it would be a fatal blow to String Theory's reputation. It's kind of exciting that after 43 years, "the day of judgment" for all my friends that are String Theorists is less than 2 years away.

4. **chris**
June 28, 2011

Bernhard,

strings without SUSY are pretty ugly. but the point is that at string scales SUSY can be restored in a lot of models.

what makes SUSY "appealing" for the rest of us is the potential to explain or soften the hierarchy and naturalness problems and to provide a DM candidate. for this you need a relatively low SUSY breaking scale in the region and somehow connected to EW symmetry breaking. but for string theory this is irrelevant. the SUSY breaking scale might be anywhere and it might not explain a single thing at TeV scales or below and yet it is good enough for strings.

5. **Ian Agol**
June 28, 2011

We have some slides from Witten's talk in Berkeley at the Michael Freedman 60th birthday conference available here (as well as from a few other talks):
http://web.math.berkeley.edu/~matthias/conference/abstracts.html

6. **King Ray**
June 28, 2011

Peter, what do you think of this?

http://arxiv.org/abs/arXiv:1009.5414

(Gravity is not an entropic force, Archil Kobakhidze: We argue that experiments with ultra-cold neutrons in the gravitational field of Earth disprove recent speculations on the entropic origin of gravitation.)

7. **Peter Woit**
June 28, 2011

Ian,

Thanks. I watched Witten's talk today, it's a very general overview of what he's doing. The slides you link to give a more detail about one piece of this.

King Ray,

My quick attempt to understand Verlinde's derivation of Newton's law left me suspicious that there was nothing to it but dimensional analysis, in which case, you can't prove it wrong. I don't know if the paper you link to refutes Verlinde in any sense. If Verlinde now claims to explain dark energy and dark matter, that's more substantive. I presume that it will soon get a lot of attention, personally I'll wait for experts to sort out what is going on with these ideas.

8. **Marc**
June 30, 2011

A few years ago, Gell-Mann said in an interview that the biggest failure of string theorists was the failure to clearly specify its principles. After several decades of research into the field still nobody came up to the challenge. I recently did a literature search (both papers and books) and there is indeed nothing on the principles of string theory. If we recall that most string theorists are part-time mathematicians, and mathematics is built on axiomatic systems of some sort, the lack of writing down axioms and principles is an important sign.

Together with the remark that almost nobody is giving talks about string theory anymore, this is a strong hint that people are finding, maybe only unconsciously so far, that the emperor is naked after all.

9. **Proudmemberofthecult**
June 30, 2011

@marc

In the same vein you can ask "What are the principles of Yang-Mills theory?". There's many results about perturbative Yang-Mills in four dimensions, but we lack a complete understanding beyond that. For string theory the situation is about as good or bad depending on your point of view. Definition-wise that is. For pure mathematicians string theory is a black box which generates conjectures.

10. **Peter Woit**
June 30, 2011

Proudmemberofthecult,
That's just ridiculous. We have a rigorous definition of non-perturbative quantum YM in 4d, even if it is an open problem to prove that it has all the properties that it appears to have (for which there is lots of evidence, numerical and otherwise). At every major conference he speaks at, Gross makes the point repeatedly that the big question is "what is string theory?". No one has ever stood up at a major conference and said that the problem with quantum YM is that we don't know what it is.

General conjectures about the properties hoped for in string theory have in the past led to interesting mathematical conjectures. The interesting thing about Strings 2011 is that most of the speakers have stopped trying to do this, instead sticking to QFT itself.

11. **manyoso**
June 30, 2011

> 'As for the lack of any supersymmetry signal so far, he says that "I personally am not yet worried", while acknowledging that some people are becoming pessimistic. While no SUSY is not a worry for string theory, he feels that "it would be awfully nice for string theory if SUSY appeared". '

Forgive me, but how is this possible? It has always been my understanding that String Theory _depends_ upon SUSY. That without SUSY String Theory fails completely as a theory of gravity or unification. If the LHC does not see SUSY or rules out a large parameter space how is this not staggeringly bad for the proponents of String Theory?

I'm just a layman so I'd really appreciate any answers as this has me baffled.

12. **anony**
June 30, 2011

Since string theory is dual to YM theory, there is no reason to discuss the ill-defined string theory anymore, we can stick to things we understand, and this is what most people are doing.

13. **Peter Woit**
June 30, 2011

manyoso,

String theory never has been able to say anything about what causes supersymmetry breaking, in particular, it doesn't even say anything about what the scale of such breaking should be. So, it could be an energies high above where the LHC could reach. It's a generic phenomenon: string theory predicts absolutely nothing, and in particular it predicts nothing at all about what the LHC will or won't see.

anony,

Great. So, Strings 2012 is being renamed "QFT 2012"?

14. **peaceful termination**
June 30, 2011

So, if they really rename it and do just QFT from now on, Peter could peacfully terminate this blog because it`s aim would finally be achieved 🙂

Cheers

15. **anony**
June 30, 2011

The kind of QFT discussed this year is really bizarre (i.e. maximally far from things relevant to nature, except maybe for 1 or 2 talks). So the most suitable name is probably "bizarre QFT 2012."

16. **somebody**
July 1, 2011

If we magically knew that there was no supersymmetry at any energies, I think it is a pretty strong argument against string theory. Unfortunately LHC will check the existence (or not) of SUSY only upto a very low energies ("TeV") compared to Planck scale. This is the sense in which it is "encouraging" for string theorists if LHC finds SUSY, but admittedly nothing more.

As to the reason why some supersymmetric QFTs are being studied at Strings 2012 is because many of them are interesting because of their nuemrous connections to 2D CFTs, string theories, etc. Many string theorists including myself, love to berate our field (who wouldn't like to make a testable prediction?), but that should always be taken with a grain of salt. Sorry to speak truth to the power here, but anyone who claims in this day and age that N=2 (in 4D and analogous things in other dimensions) supersymmetric quantum field theories have nothing to do with string/M-theory is agenda-driven. Seiberg-Witten solution which looks magical from 4D is AUTOMATIC from a brane-engineering point of view. Gaiotto's construction of his N=2 theories which started the recent interest came DIRECTLY from M2-branes wrapping Riemann surfaces. The AGT conjecture whch is another focal point of the current interest immediately relates these theories to 2D CFTs. Not to mention the are numerous connections these things have with topological strings, matrix models, etc.

This message only serves a public service point, Peter of course has been well known on this blog to describe anything interesting to come out of string theory, as NOT string theory. 🙂

17. **peaceful termination**
July 1, 2011

Interesting somebody,

thanks for these explanations 😉

18. **Thomas Larsson**
July 1, 2011

Hm. The claim I saw is that N=2 SUSY has nothing to do with nature ("bizarre QFT"). Does not N=2 have a number of peculiar properties (e.g. non-renormalization theorems) that are manifestly false for the SM.

19. **Peter Woit**
July 1, 2011

peaceful termination,

The disappearance of string theory would certainly mean that this blog would get a lot less attention, not necessarily that it would disappear…

Somebody,

One can make as much hype as one wants claiming that all good ideas in the hot topics of today (amplitudes, N=2 SUSY, various applications of gauge-gravity dualities, etc.) historically come from string theory, but the undeniable fact of the matter is that if you watch the talks at Strings 2011, virtually no one is talking about string theory itself or using string theory anymore to do anything. A student who wants to work on any of the hot topics has no reason to bother to learn string theory anymore.

What's remarkable is that this seems to be true even in those areas where string theory has had some success, far away from the heavily promoted ones. Besides virtually no talks about string theory and unification, there's also almost nothing about string theory as a theory of quantum gravity. The hot topic of recent years, the idea that string theory would explain heavy ion physics, seems to have completely disappeared.

Thomas,

Personally I find some of the work being discussed about these SUSY gauge theories quite fascinating. There's some beautiful mathematics, and techniques being used (e.g. "SUSY localization") that may some day have important applications to physical theories. These theories are not physical, but they are tantalizingly close to physical theories. That said, much of what is being done is "topological quantum field theory", which inherently uses techniques special to topological, not physical, observables.

20. **somebody**
July 1, 2011

Of course I disagree with Peter's claims despite his vehemence. But we already knew that. 🙂

The real reason for this comment is that I wrote "M2-branes" wrapping Riemann surfaces, when I meant "M5-branes".

21. **manyoso**
July 1, 2011

I don't see any vehemence in Peter's post. I think too often we read too much into conversations on the internet. Imagining emphasis and body language that is not inherently conveyed by the printed words on the page. That emphasis and body language we imagine is all too often just a manifestation of our own minds and not actual communication.

Close your eyes and see if you can see the posts above coming in a jovial manner from two respected colleagues frankly disagreeing, but not in a disagreeable way.

22. **Wolfgang**
July 1, 2011

"… virtually no one is talking about string theory itself or using string theory anymore to do anything. "

That's like saying that people doing AdS/CFT are "just doing field theory" (since the string theory side of the duality "isn't defined").

To pick one example, consider Witten's talk on Khovanov Homology. He starts with the D3-NS5 system in Type IIB, S-dualizes to the D3-D5 system, T-dualizes on a transverse circle, to get the D4-D6 system in Type IIA and then, finally, lifts the whole construction to M-theory (where he, finally, obtains a description in terms of the 6D (2,0) theory).

I suppose one could characterize this work as "just field theory" (a "followup on his work on Chern-Simons theory"). But his paper (and most of the talks, however "field theory"-oriented, at Strings 2011) would be completely incomprehensible if you don't know a good deal about string theory.

In fact, what you need to know is "modern" string theory. In his concluding remarks, Jeff Harvey noted that most of the talks would be utterly incomprehensible to even a well-trained string theorist who, Rip Van Winkle-like, had slept through the past 15 years.

23. **Peter Woit**
July 1, 2011

Wolfgang,

The terminology Witten uses in his talk is based on string theory, but what he actually does doesn't use string theory. Those names for dualities and boundary conditions correspond to constructions that are quite independent of string theory. If you look at the more detailed description of some of the same material that Witten gives when talking to mathematicians (see the slides Ian Agol links to), you'll see no reference to string theory at all, with string theory/M-theory terminology replaced by explicit boundary conditions. It's true that the way Witten and others found those boundary conditions was from studying string theory, but that quite possibly is just an historical accident, you could imagine coming upon them from other starting points.

Better yet, look at the main paper of Witten's that his talk is an overview of. All the calculations are in field theory, not string theory. Here's how Witten himself describes the situation (see pg. 12 of the "Five-branes and knots" paper):

"One goal is to give a gauge theory definition of Khovanov homology (as opposed to a definition that requires a full knowledge of string/M-theory). String theory and branes will be used as clues, but the results can be expressed as a gauge theory construction."

My understanding is that he thinks the fundamental object here is the 6-dimensional (0.2) field theory and he would argue that, since that isn't well-defined, much of what we know about it comes from string/M-theory. It seems to me that we'll have to see what the future holds as this theory is better understood. It's quite possible that it will have some good definition and way of studying it that has nothing to do with string/M-theory.

What Witten is finding here (and I think it's true in a lot of the other "physical mathematics" work described at the conference) is that the huge amount of knowledge developed over the last 20 years relating string theory and gauge theory is quite useful in providing hints as to what to look for, but at the end of the day, the interesting results don't require string theory calculations to get to, or string theory ideas to understand.

Jeff Harvey noted that many people at the conference were asking "where did the strings go?", and Chris Hull made much the same point. I don't think the people they were talking to who were asking this question were people who have been asleep for the past 15 years. Quite the opposite: I think they're people who are very aware of what the state of the field is, and are seeing a quick swing away from string/M-theory to return to more conventional field theory ideas and methods.

24. **Cosmonut**
July 1, 2011

The relative absence of "real" string theory in the recent conference has been noticed elsewhere.
Lubos Motl is ascribing it to string theorists trying not to arouse the envy of their non-stringy colleagues. Always entertaining, this Lubos. 🙂

25. **Marcus**

July 1, 2011

"The relative absence of "real" string theory in the recent conference has been noticed elsewhere. Lubos Motl is ascribing it to string theorists trying not to arouse the envy of their non-stringy colleagues."

That could be funny. Do you have a link? I think the relative absence of string (and the relative presence of what Weinberg in a talk at CERN called "good old QFT") needs to be seen in the context of declining string jobs compared with first-time faculty hires in particle theory/cosmology as a whole. Peter reported on that earlier.

26. **Marcus**
July 1, 2011

"Quite the opposite: I think they're people who are very aware of what the state of the field is, and are seeing a quick swing away from string/M-theory to return to more conventional field theory ideas and methods."

You reported on Weinberg's CERN talk almost exactly 2 years ago:
http://www.math.columbia.edu/~woit/wordpress/?p=2199
He plotted the "stock price" of conventional QFT over the past 80 or so years and extended the curve with a dotted line where he expected it to go. Basically he predicted this "quick swing" or QFT resurgence—and described a general pattern or mechanism using several historical examples. The talk video is online and could be worth watching a second time.

27. **Wolfgang**
July 1, 2011

"If you look at the more detailed description of some of the same material that Witten gives when talking to mathematicians (see the slides Ian Agol links to), you'll see no reference to string theory at all …"

Nor to quantum field theory. In the end, the problem comes down to solving certain PDEs. And you can explain those PDEs to mathematicians, without any reference to string/field theory.

It would, however, be impossible to deduce those equation without using string theory (and field theory) as a guide. And, if you read Witten's paper (and the previous papers with Gaiotto), the same can clearly be said of the relations between the various field theories (in 3, 4, 5 and 6 dimensions) that he uses.

It is all very well to fantasize about an alternative universe where a purely field-theoretic explanation of those dualities between field theories was discovered. Perhaps, somewhere in the multiverse, that universe exists. But it's not our universe …

28. **Marcus**
July 2, 2011

The video of Frank Wilczek's talk is no longer online.

I especially liked that talk and would like to watch it again. Does anybody know of an alternative link? I don't see that any of the other talks have been taken offline.
What to expect?
It used to be number 24.
Now you get some kind of message in Swedish.

29. **Yatima**
July 3, 2011

Frank Wilczek's talk is up. The sound is not well synchronized with the image though:

http://media.medfarm.uu.se/flvplayer/strings2011/video24

What did he mean with the Portals though? I suspect a Wheatley joke was planned.

30. **Mark**
July 3, 2011

Wilczek's "I certainly don't recognize it" response to Witten's very insightful comment was, to say the least, puzzling. How can he NOT be aware that low scale SUSY + a QCD axion implies a Polonyi-type problem? Wow!

31. **Dead Parot´s friends**
July 7, 2011

For your benefit:
here is the parot´s joke:
http://www.youtube.com/watch?v=e6Lq771TVm4

and here is the suggest theme song of strings2011
http://www.youtube.com/watch?v=AQcmBUc0OQc

Local Blogs

July 1, 2011
Categories: Uncategorized

There are now several excellent blogs somehow related to mathematics being run by local people, including a couple new ones, so I thought it would be a good idea to mention these here:

- Andrew Gelman of the Columbia Statistics department runs the very active Statistical Modeling, Causal Inference and Social Science blog, which features a wealth of all sorts of different topics, from technical ones about statistics, to social science applications.
- Emanuel Derman, who started his career as an HEP theorist, was one of the early migrants to the financial industry, and now is teaching here at Columbia in the Financial Engineering program, has a new blog at Reuters. His last book was the very interesting My Life as a Quant, this fall he has a new one coming out entitled Models Behaving Badly.
- Cathy O'Neil, a mathematician who taught here for a while before changing career path, starting with a job at the hedge fund D.E. Shaw, has recently started the wonderful Mathbabe blog.
- I think I mentioned this already, but one of my colleagues, Johan de Jong (Cathy's husband) also has a blog, the Stacks Project Blog. If your metric to evaluate blogs is something like "quality of information" x "degree of abstraction and technicality", his has to be the best blog in the world.

If you have comments on these blogs, I encourage you to post them there rather than here. I would be interested in hearing about any other local math/physics related blogs that I'm unaware of.

Update: Another local math/physics-related blog has made its debut today, Davide Castelvecchi's Degrees of Freedom. It's part of a network of new blogs being launched today by Scientific American, which is based here in New York.

Comments

1. **John Baez**
   July 2, 2011

   "Stacks Project Blog"? Cool. But I want "Stack Overflow".

2. **Han Yan**
   July 2, 2011

   I think the US LHC Blog is a great one. Now it has became part of Quantum Diaries.

3. **off topic**
   July 6, 2011

   It seems that Lubos just discovered supersymmetry in Obama's birthcertificate.
   Quite amazing!

Higher Speculations

July 8, 2011
Categories: Book Reviews

Some commenters here a while ago made the excellent suggestion that I should take a look at a book published this spring, Helge Kragh's [Higher Speculations: Grand Theories and Failed Revolutions in Physics and Cosmology](). I've always wondered what historians of science would make of the increasing dominance of research in fundamental physics by unsuccessful highly speculative research programs, and have also often wondered if there are any relevant historical parallels to this situation. This book does a great job of addressing those questions, and it's pretty much unique in doing so.

Kragh spends the first half of the book on history, the second half on currently popular (of varying degrees of popularity…) topics including varying constants of nature, cyclic cosmological models, anthropics, the multiverse and string theory. He doesn't explicitly make any attempt to evaluate how successful these current efforts are, but they are discussed in the context of previous failures and parallels are drawn. I didn't know much about the history of "vortex theory" in nineteenth century physics, and this turns out to be possibly the best historical parallel to the story of string theory. Here's an extract from the extensive and enlightening discussion of that bit of scientific history:

> From its beginnings in 1867 to its end at about 1900, the theory was frequently justified on methodological and aesthetic grounds rather than its ability to explain and predict physical phenomena. In an 1883 review of ether physics, Lodge described the vortex atom theory as 'beautiful' and 'the simplest conception of the material universe which has yet occurred to man'. He added, just as Michelson would do twenty years later, that it was a 'theory about which one many almost dare to say that it deserves to be true'.
>
> The audience listening to William Hicks' address at the 1895 meeting of the British Association for the Advancement of Science would not suspect that the vortex theory of atoms was dying. Without paying much attention to the theory's disappointing record with regard to empirical physics, Hicks reviewed in an optimistic tone the theory of various vortex objects such as rings, spheres and sponges. He realized that relatively little progress had been made over the years in the mathematical development of the theory, and that progress was even more lacking in the theory's contact with experiments. However, these problems he deftly turned into a defence of the theory, for the undeveloped mathematical framework meant that the theory could not be rigorously tested. Hicks was convinced that the road towards progress would be to develop still more advanced mathematical models. The vortex theory, he said 'is at present a subject in which the mathematicians must lead the attack'.

Surely many physicists of the day would have described vortex theory as "our best hope for a unified theory", and one wonders if any of them thought of it as a "part of 20th century physics that fell by chance into the 19th century."

Kragh's book does something really remarkable and valuable: it starts to put some aspects of the last 30 years of fundamental physical theory into a plausible historical context. The future of the subject remains a mystery though, but one can hope that on the vortex theory timeline we're about to hit the analog of 1900, with successful rather than failed revolutions ahead of us.

Comments

1. **wolfgang**
   July 8, 2011

   Vortex theory was a major motivation to study knot theory – a topic advanced much later by Ed Witten.

2. **Chris Oakley**
   July 8, 2011

   From the Wikipedia article "History of knot theory":

   > In 1867 after observing Scottish physicist Peter Tait's experiments involving smoke rings, Thomson came to the idea that atoms were knots of swirling vortices in the æther. Chemical elements would thus correspond to knots and links. Tait's experiments were inspired by a paper of Helmholtz's on vortex-rings in incompressible fluids. Thomson and Tait believed that an understanding and classification of all possible knots would explain why atoms absorb and emit light at only the discrete wavelengths that they do. For example, Thomson thought that sodium could be the Hopf link due to its two lines of spectra. (Sossinsky 2002, p. 3–10)

   …

   > When the luminiferous æther was not detected in the Michelson–Morley experiment, vortex theory became completely obsolete, and knot theory ceased to be of great scientific interest.

   It is just me, or does the first sentence sound a bit like "all matter is comprised of tiny vibrating strings".

3. **Daniel**
   July 8, 2011

   History repeating itself? But doesn't the book discuss the human aspects of the debacle of the vortex theory? I'm curious to know how the vortex theorists reacted and/or adapted.

4. **Peter Woit**
   July 8, 2011

   Daniel,

   The book doesn't have much on the topic of vortex theorists admitting the idea didn't work and moving on. Perhaps this was something that Max Planck had in mind in his famous quote: "science advances one funeral at a time".

5. **Roger**
   July 8, 2011

   The vortex theory was not so stupid. It was before the discovery of electrons, and when so little was known about atoms that some people could still doubt them. The vortex theory was not any worse than other theories. The [1902 Encyclopedia Britannica explained]():

> These properties of vortex rings suggested to Sir William Thomson[5] the possibility of founding on them a new form of the atomic theory. The conditions which must be satisfied by an atom are permanence in magnitude, capability of internal motion or vibration, and a sufficient amount of possible characteristics to account for the difference between atoms of different kinds.
>
> The small hard body imagined by Lucretius, and adopted by Newton, was invented for the express purpose of ac counting for the permanence of the properties of bodies. But it fails to account for the vibrations of a molecule as revealed by the spectroscope. We may indeed suppose the atom elastic, but this is to endow it with the very property for the explanation of which, as exhibited in aggregate bodies, the atomic constitution was originally assumed. …
>
> On the other hand, the vortex ring of Helmholtz, imagined as the true form of the atom by Thomson, satisfies more of the conditions than any atom hitherto imagined. In the first place, it is quantitatively permanent, as regards its volume and its strength, two independent quantities. It is also qualitatively permanent as regards its degree of implication, whether "knottedness" on itself or "linkedness" with other vortex rings. At the same time, it is capable of infinite changes of form, and may execute vibrations of different periods, as we know that molecules do. And the number of essentially different implications of vortex rings may be very great without supposing the degree of implication of any of them very high.

But then it goes on to give an argument that sounds a lot like the string theorists of today:

> But the greatest recommendation of this theory, from a philosophical point of view, is that its success in explaining phenomena does not depend on the ingenuity with which its contrivers "save appearances," by introducing first one hypothetical force and then another. When the vortex atom is once set in motion, all its properties are absolutely fixed and determined by the laws of motion of the primitive fluid, which are fully expressed in the fundamental equations. The disciple of Lucretius may cut and carve his solid atoms in the hope of getting them to combine into worlds; the follower of Boscovich may imagine new laws of force to meet the requirements of each new phenomenon; but he who dares to plant his feet in the path opened up by Helmholtz and Thomson has no such resources. His primitive fluid has no other properties than inertia, invariable density, and perfect mobility, and the method by which the motion of this fluid is to be traced is pure mathematical analysis. The difficulties of this method are enormous, but the glory of surmounting them would be unique.

In other words, it aimed to derive all of atomic theory from a few numbers and mathematical principles, without need for experiment.

6. **Wolfgang**
   July 8, 2011

   Aside from Lord Kelvin, James Clerk Maxwell, J.J. Thompson and Peter Tait, all of whom seem to have survived with their reputations intact, who else worked on "vortex theory"?

7. **Peter Woit**
   July 8, 2011

   Wolfgang,

   Kragh discusses many others who made the case for "vortex theory", including

   Gerald Francis Fitzgerald (Irish physicist)
   George Johnstone Stoney (Irish physicist)
   William M. Hicks (British physicist)
   Donald MacAlister (British physicist)
   Balfour Stewart (British physicist)
   Francis Venable (American chemist)
   Joseph Larmor (British physicist)
   Silas Holman (American physicist)

   So, proponents of the theory include the long-forgotten, as well as physicists who did important work on other subjects.

   There were also skeptics (including mathematician William Clifford), and non-scientist fans (Madame Blavatsky and the Theosophists)

8. **Matti Pitkanen**
   July 8, 2011

   Vortex theory inspired the theory of knots and braid theory is applied in quantum context to topological quantum computation. For the physical space-time dimension D=4 the orbits of space-like 1-knots are 2-knots-knotted string world sheets. If one accepts holography and the space-time correlate for finite measurement resolution as a discretization replacing 3-D space-time hologram with braids, one ends up with 2-knotted string world sheets as basic objects and we have string theory like structure in D=4. Maybe the intersection of string theory and vortex theory- actually developed by Witten- could give us the long waited physics.

9. **Bee**
   July 9, 2011

   Interesting. Can you say a few words on whether the book is well written and worth spending the time on?

10. **Peter Woit**
    July 9, 2011

    Bee,

    The book is well written, and definitely worth the time for anyone interested in these issues. It's more of an intellectually serious, scholarly work, with a lot of research behind it, rather than something aimed purely at a popular audience (the publisher is Oxford University Press, not a trade publisher). From my point of view that's a plus. It's not full of pictures, gee-whiz language, or heavily over-simplified attempts to explain physics by analogy.

11. **Martin Ouwehand**
    July 9, 2011

    > one can hope that on the vortex theory timeline we're about to hit the analog of 1900

    I'm not sure that this is what you want, because that's when the next *unsuccessful highly speculative research program* started, viz. the "electromagnetic view of Nature"…

12. **model and sim**

July 9, 2011

I would agree with Peter that there are some items shared by theories that make the differences between the theories more a matter of semantics. Whether we label solutions as vortexes or strings I think is purely semanitics. The question is more about the governing equations, and the nature of the thing that characterized by those equations. From that point of view, equations in string theory are much more nuanced, for better or for worse. Modern understandings of algebras are much more advanced than at any other time in history, and those algebras are fairly immune from the objects representing them. It is just disheartening that the last decade was more focused on semantics than substance. I just hope that we see something this summer that will finally move us forward.

13. **Wolfgang**
July 10, 2011

Maybe I'm too literal-minded, but this seems apropos of any discussion of "vortex theory."

14. **FrankH**
July 10, 2011

My response is so what? Vortex theory was a reasonable approach to try. It was very difficult to do the calculations and it turned out not to work, or rather a better more calculable theory was developed, but I hold nothing against the physicists who advocated vortex theory. Similarly, string theory is a reasonable approach to try. It is very difficult to do the calculations, but they have been able to calculate a lot more than vortex theory ever did. String theory may or may not be the appropriate theory to explain the elementary particle zoo that we have discovered, but until a better theory comes along, there is nothing wrong with getting as much as we can out of string theory, in my humble opinion…

15. **Peter Woit**
July 10, 2011

Wolfgang,

One can point to lots of ideas that a small number of people have tried to pursue that haven't been any more successful than vortex theory. String theory is different though: it's an example of an idea that dominated the whole field, including getting the attention of the major figures in the subject, for several decades, while not going anywhere.

Actually, back in 1984/85, string theory was a much more specific proposal than vortex theory, with a much more highly developed and detailed structure. As things haven't worked out, it has migrated towards vaguer and vaguer proposals. The latest hot topics, the multiverse and Verlinde's entropic gravity, are even more content-free than vortex theory ever was.

16. **Wolfgang**
July 10, 2011

"The latest hot topics, the multiverse and Verlinde's entropic gravity …"

From the complete absence of multiverse-related talks at Strings 2011, and from the frosty reception that Erik Verlinde's talk received there, one might question how "hot" those topics are.

17. **N. Nakanishi**
July 11, 2011

The vortex model of atom was discussed in comparison with superstring by the famous science writer, Martin Gardner in "The New Ambidextrous Universe" , 3rd ed. (1990).

18. **plum**
July 11, 2011

The vortex theory failed to explain atoms, but let's not jump to conclusions that the "electromagnetic theory" of atoms got it right from the beginning. J J Thomson discovered the electron in 1897, but he was of the opinion that the positive charge in an atom was smeared out — the so-called `plum pudding' model of the atom — the electrons were like raisins, points of negative charge, in a background mush of positive charge. In 1909 Ernest Rutherford discovered the atomic nucleus and proposed the `solar system' model of the atom. But Rutherford's model had immediate problems, which the plum pudding model did NOT have. It was known by 1909 that a point charge moving in a circle will radiate, which meant that Rutherford's proposed solar-system model of the atom was unstable. In 1913 Neils Bohr proposed a solution (the Bohr model of the atom), but it was hardly a rigorously formulated theory. The Bohr theory was really a suspension of belief, with ad hoc postulates to fit the observed data. The Bohr-Sommerfeld quantization just slapped on ad hoc `quantization rules' on top of classical physics, the "old quantum theory". It was hardly a rigorous theory. So who is to say that the vortex theory of atoms was a ridiculous idea to pursue, circa 1900?

19. **Arun**
July 11, 2011

If I remember correctly, there is a lot about vortex theory in Sir Edmund Whittaker's A History of the theories of aether and electricity, specifically the first volume. It would be interesting to collide Helge Kragh's work against it.

20. **D R Lunsford**
July 11, 2011

Focusing of vortex theory in particular is barking down the wrong whirlpool. The point was that the idea of matter in the vacuum, in the light (haha) of Ampere, Faraday, and Maxwell, had become extremely complicated when compared to the sane world of Newtonian continuum mechanics. Vorticity in continuum mechanics itself obeys a sort of conservation law and so naturally, people who were attempting to construct fundamental mechanical models of matter in the vacuum fell back on this fact. The main issue was a wrong-headed and premature attempt to model the vacuum when too little was known about the actual state of matter.

-drl

21. **Nige Cook**
July 13, 2011

The analogy to string theory is deep. The toroidal vortex or smoke ring in an ideal fluid doesn't disperse, so it seemed like a good model for the Greek concept of the atom. It represented atoms of matter as just a disturbance in the Maxwell model of space, a vortex in a fluid in which "displacement current" can flow (his addition to Ampere's current, to make his equations postdict light). When Michelson and Morley failed to detect Maxwell's postulated light-carrying vacuum medium, and when radioactivity was discovered, Lord Kelvin simply dismissed the MM experiment and radioactivity as experimentally bogus because they conflicted with his beautiful model of stable vortex atoms. You can expect the same denialist reaction to the failure of experimentalists to find supersymmetric particles, etc. Einstein is famous for saying (when light deflection was observed in 1919) that general relativity didn't need testing because it is a beautiful theory, so it must be right. (It then turned out he was confused about the role of the cosmological constant in his "beautiful" theory.)

22. **aelight**
July 13, 2011

Einstein – is that so? Einstein wrote to Edwin Hubble to ask his opinion if the deflection of light could be measured by observations of stellar positions **in daylight** (emphasis by Einstein ~ I read this maybe in Physics Today ~ maybe in 2005 celebrations of Einstein annus mirabilis). Hubble replied that there was no hope of doing so, but that measurements during a total solar eclipse might be possible. A suitable solar eclipse took place in 1919, Eddington made the observations, and the rest is history. So possibly Einstein said different things before and after the test of GR proved successful. Dear Uncle Albert was only human?

23. **John Duffield**
July 16, 2011

Pay careful attention to "vortex theory". There are hands-on experimentalists doing work in this area, see for example http://www.physorg.com/news182957628.html, as Roger said the people involved in vortex theory during the 19th Century didn't know about the electron, and it was Thomson and Tait who gave us "spherical harmonics".

24. **CPV**
July 16, 2011

It's obviously not clear which ideas will be successful ahead of time, so Monday morning quarterbacking of failed ideas is not a particularly noble or useful enterprise. I suspect the main point of this blog is the percentage of resources devoted to any one idea, not the concept that there shouldn't or won't be failed ideas, or that people shouldn't be encouraged to work on wildly speculative ideas. Usually experimental evidence will break logjams and sort things out. We just don't have any interesting evidence right now.

25. **Peter Woit**
July 16, 2011

CPV,

In the case of string theory unification, the difference with "Monday morning quarterbacking" is that in the football case, the team that lost on Sunday had to acknowledge it. The problem with string theory unification research has in recent years evolved from a problem about relative allocation of resources into a more disturbing one of pseudo-science being promoted in order to allow failure to not be acknowledged.

In this circumstance, a comparison of the current situation to previous ones where the failure is now clear to everyone may very well be enlightening.

26. **cormac**
July 18, 2011

Peter and Bee: of course it's well-written!
Helge Kragh is the author of Cosmology and Controversy, a highly regarded book on 20th century cosmology that focuses on the debate between the steady-state and BB models. It is a most unusual book in that it is considered historically accurate, scientifically accurate and highly readable. It is cited in almost every major cosmology book I have, whether textbook, popular book or historical analysis!

This Week's Leak

July 12, 2011
Categories: Experimental HEP News

Everyone in the HEP community is breathlessly awaiting the release of results from the 2011 LHC run, expected to come at the EPS-HEP 2011 conference in Grenoble starting July 21. A public press conference has been announced for July 25. Presumably the new results will further tighten limits on supersymmetric particles, extra-dimensional models and other exotica, but the real excitement surrounds the question of what the news about the Higgs will be. The latest LHC data should finally allow competition on this front with the Tevatron.

Philip Gibbs at viXra log has posted here what looks like the bottom line for CMS. They are not yet able to exclude a Higgs at lower masses, including the range where the Tevatron has an exclusion region, but are able to exclude (at 95% confidence level) a SM Higgs in a higher mass region (about 275-425 GeV). This sort of result is not quite what it looks like, since precision electroweak measurements already rule out such a SM Higgs, and recall that the Higgs self-coupling increases with Higgs mass, meaning that one is entering into a region where one is not sure that perturbation theory applies. If the Higgs is not a weakly coupled field, life becomes much more complicated.

The source of the plot is variously described as "shown at a seminar which as far as I know was public", from "a public part of the CERN repository", and "not yet public but was made accessible on a Fermilab site".

ATLAS, the competition for CMS, presumably has a similar plot up its sleeve just about ready for release at EPS-HEP 2011. Once the two experiments have made public their independent results at this conference, they intend to immediately get to work producing a combined plot, with goal of releasing it at Lepton-Photon 2011, which will take place in Mumbai August 22-27.

Comments

1. **DB**
   July 12, 2011

   Minor typo Peter, the EPS-HEP conference begins on July 21st.

   I think it's too early to expect anything really interesting out of the LHC this time around. I suspect that the inexorable closing off of the SUSY parameter space is a bit of a sideshow for most. It's like dutifully visiting a terminally ill relative you never really got along with.

   This is the year to celebrate the machine's performance after the initial disastrous startup. ICHEP 2012 in Melbourne, or especially EPS-HEP 2013 in Stockholm should be another story altogether.

2. **Peter Woit**
   July 12, 2011

   Thanks DB,

   Absent surprises, the most interesting thing should be whether a Higgs is seen in the expected region of 114-158 GeV. Do you really think it will take that long for ATLAS/CMS to be able to exclude or start to see something in this region?

3. **DB**
   July 12, 2011

   I'm a Higgs agnostic myself, Peter, and I won't be hugely surprised if they don't see it, which is why I think it will take until 2013 before we know one way or another.

4. **GS**
   July 12, 2011

   I really enjoy the high quality of the discussions in this blog. Even though my comment may not be living up to the level of the rest, I just wanted to mention–as a (very speculative) side remark–the CERN announcement for the press conference http://www.interactions.org/cms/?pid=1030888
   Notice topic three: "The LHC and other projects: what strategy should Europe adopt for particle physics? by Michel Spiro, President of the CERN Council and CNRS Scientific Director for the Alpes region." This may be exaggerated, but it sounds suspiciously like they are preparing to present no revolutionary results there.

   Best,
   GS

5. **P.**
   July 12, 2011

   CDF has seen B_s -> mu+mu-

   Talk on friday:
   http://theory.fnal.gov/jetp/

6. **Peter Woit**
   July 12, 2011

   P.

   Thanks, very interesting! I'm assuming for them to see this, the signal must be too large to be compatible with the SM, right?

   At the KITP, they were guessing that this seminar would be "possible…D0 ttbar asymmetry measurement", see

   http://lhc11.wikispaces.com/Talks+%26+Discussions

7. **Peter Woit**
   July 12, 2011

P.

The CDF paper is out tonight:

http://arxiv.org/abs/1107.2304

They do claim to see an excess of B_s -> mu+mu- above expected background + SM value, with 1.9% probability of such an excess if the Standard Model is correct. They conclude

“Although of moderate statistical significance, this is the first indication of a B_s -> mu+mu- signal”

Not yet a convincing violation of the SM, but intriguing. Can D0 also see this?

8. **PA**
July 13, 2011

A seriously garbled html version of the CMS presentation remains in the Google cache. It’s very preliminary, so who knows what will really be the result presented. The bottom line is that although “strong statements” about the Higgs are now possible, the upward fluctuations seen in Phil G.’s plot are fully consistent with statistical expectations. So as of now, it looks like CMS will firmly exclude some models (SM Higgs + 4th generation), but a little more patience will be needed for the vanilla Higgs.

9. **anonymous**
July 17, 2011

“This sort of result is not quite what it looks like, since precision electroweak measurements already rule out such a SM Higgs, and recall that the Higgs self-coupling increases with Higgs mass, meaning that one is entering into a region where one is not sure that perturbation theory applies.”

Does this suggest that we may not be able to confidently detect a more massive Higgs if it exists? Does the vanilla Higgs show a better signature over all mass regions?

10. **chris**
July 19, 2011

no, it just means that this was primarily ruling out technicolor Higgses, which are not so popular anyways.

11. **Peter Woit**
July 19, 2011

chris,

It’s not clear to me that ruling out a high-mass Higgs described by SM Feynman rules actually tells you anything about a Higgs in a technicolor theory, which will have a different behavior.

Questions About the Multiverse

July 19, 2011
Categories: Multiverse Mania

The August issue of Scientific American has the multiverse on the cover, with a skeptical feature article on the topic by George F. R. Ellis, Does the Multiverse Really Exist?, which argues that heavily promoted multiverse research isn't really testable and can't explain much of anything. Vilenkin and Tegmark respond with The Case for Parallel Universes.

I just took a look at some of the earliest postings on this blog about the multiverse from as far back as seven years ago (e.g. here and here). Things haven't changed at all. One might be tempted to criticize Scientific American for keeping this alive, but they just reflect the fact that this pseudo-science continues to have significant influence at the highest levels of the physics establishment. The Perimeter Institute recently ran a conference on Challenges for Early Universe Cosmology, which was dominated by multiverse mania. Unlike the case at SciAm, multiverse skepticism didn't get prominent play at Perimeter.

Update: For those of you who just can't get enough multiverse, the Sci-Fi film Another Earth opens Friday. Click on "Parallel Worlds" for an explanation of "the theoretical physics behind the film."

Comments

1. **Sean the Mystic**
   July 19, 2011

   Brilliant article by Max Tegmark. I'm not a physicist, but I find his arguments compelling and perfectly rational. What about Tegmark's arguments do people here object to?

   My only disagreement with Tegmark is his last statement: "The price we have to pay is becoming more humble—which will probably do us good—but in return we may find ourselves inhabiting a reality grander than our ancestors dreamed of in their wildest dreams." Actually, if science continues to humble us in this manner, I fear we may do what Lovecraft warned about in the face of our mind-boggling cosmic insignificance, and "flee from the light into the peace and safety of a new dark age."

2. **Peter Woit**
   July 19, 2011

   Sean the Mystic,

   Unlike mysticism, where there may be other ways to convince people you have interesting insight into something, in science there's traditionally just one way to do this: use your insight to make a convincing testable prediction and then check it. This is what neither Vilenkin nor Tegmark can do, and they've made no progress towards doing this in the years and years that they have now spent promoting multiverse studies.

   There may very well be a multiverse, of levels I, II, III, IV or whatever. But at this point all the years of work on this have led to nothing but:

   1. claims that if everything worked out just right, there'd be a signal visible in the CMB, of exactly the right size to now be just beyond the level detectable. This appears to be just wishful thinking, and even Tegmark says he doesn't believe it:
   "I totally share his skepticism to these claims."

   2. claims that string theory will allow statistical predictions by analyzing the landscape of string theory solutions. This has been a complete failure and there are strong arguments that it is guaranteed to fail.

   As for the argument that skepticism about multiverse studies is some sort of hubris, I think the opposite is true: the hubris of multiverse maniacs with their conviction that we don't need conventional standards of science any more and can happily go on about level IV multiverses is just staggering.

3. **Sean the Mystic**
   July 19, 2011

   Thank you for this reply. Maybe there should be a new field of study then, call it "multiverse studies" or something, so these multiversalists can pursue their ideas separately from the more traditional empirical scientists? Maybe these theories are more pure math or even art than science, but that doesn't mean they aren't worth exploring.

4. **Peter Woit**
   July 19, 2011

   Sean,

   The burgeoning field of "Multiverse Studies" definitely isn't pure math. The papers in this subject typically just use high school algebra. Categorizing the subject as an art form and not a science would be a step forward.

5. **Hakeem Shabazz**
   July 19, 2011

   Let them do all the multiverse studies they want, on their own nickel.
   Just cut off uncle sam's gravy, although that may thin out the crowd milking a nice conference.

   BTW, is this not a corollary, in headline form, of the theory of the infinite mutiverse, where anything consistent with physical laws will happen:
   President Obama and Palin Have Love Child in Alternative Universe.
   'He cheated with that racist dingbat', sobs Michelle.

6. **Peter Woit**
   July 19, 2011

   Hakeem,

There’s very little of Uncle Sam’s money in multiverse studies, since NSF and DOE panels tend to not support this. Private money (Templeton Foundation for Tegmark’s FQXI, Blackberry for Perimeter) is funding most multiverse mania.

And, by the way, multiverse jokes have also gotten really old by now…

7. **Sean the Mystic**
   July 19, 2011

   So is that what this is about? A power struggle for government funds? I suppose you could argue that if the public finds multiverse studies more interesting than experimental particle physics, and wants to spend their tax money on it, then that’s their perogative. My impression is that we’re fully in the postmodern era now, so the pursuit of objective truth for its own sake no longer carries much weight. Multiverse studies fits perfectly with postmodern thinking, whereas traditional physics just seems like intellectual tyranny. These are just the random thoughts of a mystic who used to study physics, feel free to ignore them 😉

8. **Eric habegger**
   July 20, 2011

   Sean,

   “My impression is that we’re fully in the postmodern era now, so the pursuit of objective truth for its own sake no longer carries much weight. Multiverse studies fits perfectly with postmodern thinking, whereas traditional physics just seems like intellectual tyranny.”

   I think you are being too kind by half by describing the current mood as post-modern. If this new era continues as it has been in physics for the last thirty years and isn’t just a temporary blip I think it would be better called the second dark age. Or better yet, in the spirit of the times we should now call the dark ages the first post modern era. That would then make everyone feel better about the second post modern era we may be entering. Anyone interested in eye of newt to put into your latest physics experiment? I have them at rock bottom prices.

9. **Markus Schwarz**
   July 20, 2011

   Dear Sean,

   this “traditional intellectual tyranny” has brought to you such things as electricity, cell phones, GPS systems… In chemistry it led to things like plastics, and in medicine to antibiotics… The list is still growing. If your multiverse/postmodern thinking could produce things like warpdrives or teleporters, or even improve known technologies, I am sure it would have a justified place in natural science. However, they have not contributed anything so far, and my impression is that they don’t even try. Instead, they invent new rules and still want to be part of the natural sciences. To me, they sound like soccer players who pick up the ball by punching other players and, when the referee shows them the red penalty card, argue that this is allowed in rugby. If someone wants to pursue these ideas, he should indeed do so in the humanities department. But why should I abandon the successful traditional way of thinking?

10. **Bobito the Payaso**
    July 20, 2011

    “more traditional empirical scientists”

    This made me laugh. And cry.

11. **Ignatz Thugg**
    July 20, 2011

    Perhaps Alan Sokal can revise his first book (Fashionable Nonsense)
    by replacing the literary post modernists by the mystics and new-agers
    within the Physics community. Not much would have to be changed –
    appropriate quotations of course, but Sokal’s commentary could
    (almost) be left intact.

12. **Aleksandar Mikovic**
    July 20, 2011

    “If someone wants to pursue these ideas, he should indeed do so in the humanities department. But why should I abandon the successful traditional way of thinking?”

    The reason why many physicist are working on the multiverse ideas is that any attempt to extend the current fundamental physical theory, i.e. the Standard Model of particle physics and cosmology, without an experimental input, boils down to the problem of formulating a mathematical theory with certain properties. The main property is to recover the Standard Model in a certain limit, and the other requirements are based on the authors philosophical inclinations. Hence this type of research cannot be done in the humanities departments, since the people there do not know enough physics and math to do it or to evaluate it. Although this type of research is not a traditional science, I think that it is useful, if it is done in an adequate proportion to the usual science research, since it opens new vistas and provides new ideas. There are several episodes in the history of science when such an approach was fruitful, for example: General Relativity and Dirac’s equation.

13. **Alexander Kruel**
    July 20, 2011

    Have you ever heard about Eliezer Yudkowsky and his take on the multiverse? I am curious about your opinion:
    http://lesswrong.com/lw/r8/and_the_winner_is_manyworlds/

14. **Igor Khavkine**
    July 20, 2011

    Alexander, this is a case of one word with multiple distinct meanings. The ‘multiverse’ discussed on Yudkowsky’s blog is logically distinct from the notion of ‘multiverse’ discussed in the recent SciAm issue. The former consists of a certain way of looking at a mathematically precise, empirically well tested theory of quantum mechanics, while the latter consists of a loosely grouped collection of speculative ideas about how to explain some features of our universe, though without any empirical way of separating good ideas from bad ones.

15. **Sean the Mystic**
    July 20, 2011

Markus, who wants you to abandon a successful way of thinking? I just suggested a new field of study, so the different camps don't have to fight each other. I think it would be great if multiversalists joined artists, writers, mystics and other creative people who like to speculate about possible worlds. Maybe it would all be nonsense, but it would be a lot of fun and they might even discover something "important". I just find it fascinating that creatives had ideas about parallel universes a long time ago that sound very similar to what physicists are saying now. (See http://en.wikipedia.org/wiki/Parallel_universe_%28fiction%29 for some examples).

I think you're on a slippery slope if your justification for science is its practical applications; we might as well abolish many areas of higher math, stop building space telescopes and forget about astronomy and cosmology with that attitude. For that matter, what is the practical value of the LHC? I like to think that anything the human mind can imagine is real in a larger multiverse of ideas, and that exploring that realm is a form of empirical science — but then, I am a bit of a crackpot. 🙂

16. **Bob Levine**
July 20, 2011

Sean, I think you've missed the point of Markus's post. If I read him right, he was pointing out a substantial number of technological developments that count as existence proofs of a particular connection between science and something normally labeled 'reality' that corresponds to how the world works, and was suggesting that the multiverse has no such evidence to favor it. The old question, 'If it were wrong, how would we know?' is relevant here. If the multiverse, on your or anyone else's conception were wrong, how would we know. Markus' and Peter's point, and that of a lot of the other contributors to this site, is that hype notwithstanding, there isn't any way we would know. So whether you enjoy 'exploring' the multiverse or not, it's unconnected to science, period.

17. **Christian Takacs**
July 21, 2011

Dear Sean,
Perhaps you need to reassess your definitions of science… Like I did. When I was growing up I read a great deal of Science Fiction and watched Star Trek, Babylon 5, and other Sci-Fi oriented programming. I read The Time Machine, Permutation City, The Broken God, and many other fine books. When I started in great excitement to look into how such things as Warp Drive, Time Travel, Alternate Universe Travel, Teleportation might work, what I discovered was that there was not so much "science" as "plot devices" at work. I began to realize that my love of good fictional stories with technological plot devices was clouding my understanding of what science actually was. While there is truth to be found in fiction, it is usually in the form/message of good and bad human choices and consequences, and not how scientifically accurate or valid the plot devices are. When you read an Aesop Fable like The Fox and the Crow, or The Tortoise and the Hare, you aren't supposed to take away that animals talk, have foot races, or are genetically predesposed to have philosophical discussions, Or that they actually exist in an alternate universe; You ARE supposed to consider the concepts and implications behind the character's actions, masks, and words.

When you study Science/ and or Physics, you are going after "what is it and how does it work?" and to do this you follow very strict rules of definition, methodology, heirarchy, logic and confirmational experimentation. These "rules" are not to be mistaken for "plot devices" and vice versa. These "rules of science" are also designed to be resistant (not fool proof) to sophistry and deceitful manipulation.
As a good general rule, when anyone claiming to be doing science/physics avoids or denies "confirmational experimentation" they should be met with a healthy dose of skepticism, then slapped with a ruler on both palms before being made to write "I shall not evade confirmational experimentation while in THIS particular universe" on a white board for as many times as there are theoretical unproven multiverses, or until they repent, or just renounce all their government funding and start quacking like a duck.

18. **Paul D.**
July 21, 2011

And, by the way, multiverse jokes have also gotten really old by now…

I'm sure there are universes where the jokes are still funny.

Just … not this one.

19. **lun**
July 21, 2011

As a general point, this discussion has been going in circular form in many places, physical and virtual. I think the question is very simple:
Can the multiverse make experimentally verifiable predictions?
Since there have been at least two proposals in the affirmative ((a) cosmological signals of neighbouring universes and (b) correlations among physical constants), dismissing the multiverse idea as unscientific a priori is wrong.

ON THE OTHER HAND, claiming that the multiverse must exist because string theory "must be right" but no other solutions exist for problems like the cosmological constant and vacuum selection is also wrong. I believe I can not explain why.

What gets me about modern string theory is not so much that they do research on the multiverse, as I said to me its a potentially legitimate and productive area of investigation. Its that SOME string theorists make claims of results ("we know string theory is the theory of everything!" "We know the multiverse is predictive") well before results are on the horizon (indeed, well before the results will be on the horizon).

The fault here is not with string theorists, ALL fields of science, today, make wildly overhyped claims because so much of their funding depends on their PR ability.
In the long-term, this corrupts the science. Feynman explained this in
http://www.lhup.edu/~DSIMANEK/cargocul.htm much better than I can here.
String theory, because of the fuzziness of its fundamental picture and the
lack of experimental verification, is more susceptible to this corruption than other fields, thats all.

It is not just with the multiverse: I saw a seminar where an eminent AdS/Condensed matter person and his collegues had a near-shouting match with AdS/condensed matter skeptics on whether AdS/CFT has anything to say about condensed matter.
And the crucial question, "is there a way to tell that a certain system can be described by a classical supergravity dual", was not even addressed!
It is a question which we could answer for some systems. For QCD, for example, it probably is no because of asymptotic freedom and the fact that the number of colors is about the same as the number of flavors.

Which brings me to the second problem of modern science to which string theory is susceptible: the fact that young people have to publish on very short time-scales. Both the multiverse and AdS/whatever provide a good way to publish many papers in a short-time, so they are "popular", even if everyone knows these ideas are probably profounds flawed. Again, this is not because string theory is particularly evil, its just the economics of modern research.

20. **lun**
July 21, 2011

ps: Igor, there have been attempts to unify the two pictures by string theorists:

http://arxiv.org/abs/1105.3796

21. **Condensed Matter Theorist**
July 22, 2011

To all the non-physicist readers out there I would like to point out one thing regarding the potential demise of physics or the burgeoning second dark-age being discussed in the comments. The largest swath of physicists in the American Physical Society are categorized as condensed matter physicists. Virtually EVERY cmt physicist I know thinks the multiverse business is completely without merit and ridiculous. Further, almost all cmt physicists I know also think that string theory is complete pseudo-science and likely to be looked at as the greatest hoax ever played on physics.

Just wanted to assure people that MOST physicists are stuck solidly to the scientific method where theorists are only taken seriously if they make testable predictions and nothing is considered to be close to reality until it is experimentally demonstrated beyond a reasonable doubt.

22. **Giotis**
July 22, 2011

Condensed Matter Theorist you are not qualified to reject HEP/QG theories using such dismissive language. Have you ever studied String theory in depth?

In my mind your opinion about String theory or the Multiverse has the same weight as the opinion of a TAXI driver.

Be modest and don't make pompous statements for things you have no idea about.

23. **Condensed Matter Theorist**
July 22, 2011

Giotis, you're joking, right? Taxi driver? Really? Even most taxi drivers are able to dismiss string theory due to it's lack of adherence to the scientific method. Experimentally verifiable. It's very simple. Taxi drivers….come on, who's pompous?

…I should have guessed I would get a "condensed matter theorists are intellectually inferior to string theorists" type response in the comments of this blog. Most theorists I know actually are quite knowledgeable about "HEP/QC" theories–as you call them–and astonishingly to you–still think they are ridiculous and choose to work on something else. I know it's probably hard to believe but the complement of the set of all string theorists does not equal the set of all theorists too stupid to be string theorists. That may hurt your ego (and I understand that the ego is one of the only thing most string theorists have left) but it's true.

24. **Condensed Matter Theorist**
July 23, 2011

Giotis, one more thing. You've set up such a standard that ONLY a world-renowned and string-theory-establishment-certified expert's rejection of string theory would have any meaning for you. Any other rejection would necessarily come from someone who is "unqualified". That's very convenient for you. I suppose until Witten turns his back on string theory, how dare any other mere mortal to have a judgement. Although, I imagine that if Witten jumped ship, you all would convince yourselves that you threw him overboard due to his loosing his marbles and being no longer credible.

25. **Peter Woit**
July 23, 2011

Giotis,

CMT makes the obvious points about this, but in addition it's worth pointing out that most multiverse papers use very little mathematics (and not that much physics). A bright (or even not so bright) high school student could read and follow some of them (as well as figure out why it wasn't science…)

26. **gregor berkens**
July 24, 2011

Michael Faraday also used very little mathematics, and yet Einstein had a picture of the great Faraday hanging in his office.

This blog also uses very little mathematics, so perhaps this blog isn't really about physics. 🙂

27. **Thomas Curran**
August 3, 2011

The article " Does the Multiverse Really Exiist" states astronomers are able to see to a distance of 42 billion light years……
My question is what do they see past 13.7 billion light years?

First International Spring School on Particle Physics and Philosophy

July 19, 2011
Categories: Uncategorized

From an article in the CERN Courier I recently learned about a program that brought together physicists and philosophers of science earlier this year around the topic of philosophy and particle physics. This was the First International Spring School on Particle Physics and Philosophy held last March in Germany, and I gather that there are plans for a second one in two years.

Unlike many "physics and philosophy" efforts, which often revolve around rather sterile debates, the central topics of this school were very real issues currently at the heart of fundamental physics. In particular, the questions of gauge symmetry and the Higgs mechanism played an appropriately large role, with the experimental situation an important part of the discussion. In a few days (at EPS-HEP2011) we're likely to hear the first significant results about the Higgs coming from the LHC. This will mark the beginning of a new era likely lasting for a while which will be dominated by news coming from the LHC on this topic, and a major re-orientation of theoretical research in response. New ideas will hopefully emerge, and models that have held theorists attention for decades will likely fall by the way-side (in his talk on supersymmetry, Michael Kraemer expresses the opinion that if it doesn't show up in the 2011/12 run, it's all over for weak scale supersymmetry).

The speaker's slides for the conference unfortunately aren't now publicly available since the organizers haven't gotten permission from their authors, but perhaps they'll be made available at some point, somewhere in some form.

Comments

1. **lun**
   July 20, 2011

   Maybe I am too cynical, but 99% of activity at such "interdisciplinary" meetings ends up consisting in people talking past each other.
   The timetable and titles of the talk tends to reinforce my cynicism.
   Perhaps someone present at the conference could report on it…

   As for philosophy of physics… I sat in a graduate class on philosophy of quantum mechanics at a major research university and was stunned that the professor did not know of things like the Schrodinger and Heisenberg pictures. That you can derive the whole of QM just from Heisenberg's uncertainity relations. That the Kochen-Specker theorem precludes ANY consistent hidden-variable description fully agreeing with QM.

   An eminent philosopher once said that the job of philosophers is to define questions in a sufficiently rigorous way that they can be investigated by other disciplines.
   Its a good definition, and by this mark, physicists seem to be doing this job already.

   Again, I would very much like a direct report from the conference, but I would be willing to bet money that the speaker on gauge theory has no idea that in string theory one can easily transform gauge into global symmetries.
   (Not saying that string theory is necessarily the correct description of nature. Just that, before linking gauge symmetry to "structural realism", perhaps one should see if a more concrete understanding of them is available. And fort that one needs to be a physicist at this point).

Results from EPS-HEP 2011

July 21, 2011
Categories: Experimental HEP News

Results from the EPS-HEP 2011 conference that began today are starting to appear. These include the first results making use of most of the 2011 LHC run data. This is a factor of 30 or so more data than that from the 2010 run, which was the source of almost all previous results released by the LHC experiments. Some of the news so far:

- ATLAS pretty much says here that there are no squarks or gluinos below 1 TeV (see page 9). Comparing to analyses of the regions considered mostly likely (see for example here, figure 7) pre-LHC, significantly more than half of the region in which supersymmetry was supposed to appear is now ruled out. Another factor of 10 or so in data should come in during the rest of the 2011/2012 run, which should allow limits to be pushed a bit higher. At this point, it looks like SUSY is on its way out. It will be interesting to see if die-hards insist that the factor of 2 in energy at the next (starting in 2014-5) run will make a difference.
- For results relevant to strings, black holes, extra dimensions, split supersymmetry, and other exotica, CMS has them appearing here, for ATLAS they're here. No such objects are being seen, with limits being pushed up dramatically from those coming from the 2010 data. Again, it's going to be very hard to argue that there's a significant probability that such things will be seen in the rest of this run, or even later ones at full energy.
- CDF results available here say no Higgs between 156 and 175 GeV, D0 exclusion (here) looks like it covers about 160-170 GeV. Fermilab has issued a press release about this, advertising the release of the combined numbers at a July 27 talk. This should also include low mass searches which might provide exclusion above the 114 GeV LEP limit. The press release mentions a "most likely" range of 114-137 GeV for the Higgs mass, and links to earlier Tevatron exclusion limits, but I suspect the 137 number comes from a different source, not a Tevatron direct search result.
- CMS and ATLAS results on the Higgs are to be announced tomorrow afternoon (an early version of the CMS results leaked here). A combination of results from the two camps will be done after the conference, planned to be announced at Lepton Photon 2011 in late August, although a rough guess as to what that will look like should be available just from seeing the two independent results.
- Philip Gibbs is keeping a close eye on this at viXra log.

Update: Tommaso Dorigo has some more news here: CMS is not seeing the SM violating forward-backward top pair production asymmetry seen at the Tevatron (more about it here).

Update: ATLAS results on the Higgs are 95% exclusion 155-190 GeV and 295-450 GeV. They see a 2.8 sigma excess of events in the 120-140 GeV range.

Update: I just noticed that Matt Strassler now has a blog and is blogging from Grenoble.

Update: Matt Strassler reports from the CMS Higgs combination talk that they exclude 145-480 GeV at the 90% confidence level. Some excess 120-145 GeV, smaller than ATLAS.

So, in summary, it looks like the LHC + Tevatron have pretty much excluded a high mass Higgs, narrowed the possible mass range down to 114-150 GeV or so. No evidence at all of anything but the SM. The big story of the next few months will be to watch and see if a Higgs signal emerges in the last non-excluded region. Or not….

Comments

1. **SA**
   July 21, 2011

   Actually, the new ATLAS data does not exclude much of the parameter space not already ruled out by other experiments (in the case of mSUGRA/CMSSM).

2. **Peter Woit**
   July 21, 2011

   Well, the new results rule out a significantly larger region of possible squark and gluino masses than the 2010 data, one that includes the points that various groups were advertising as "most likely". It's very easy to find papers arguing that the LHC was likely to see supersymmetry in this region, here's a couple more:

   http://arxiv.org/abs/0907.2589

   http://arxiv.org/abs/1011.1246

   I'd be curious to see a review of the pre-LHC literature on this, for comparison with arguments likely to be made now about why these masses have to be much higher.

3. **asdf**
   July 21, 2011

   So Susy turns out to be crap after all. Makes sense. It cut too deeply against Ocam. I suspect "diehards" will never give up. Not even after 10 years of operation at full power. I think Planck said that progress in physics is made one funeral at the time.

   I suspect the Higgs will be excluded. Chiral symmetry breaking and supeconductivity were both explained as scalars initially. Scalars are probably just something we resort to when we can't understand the microphysics.

   You'll be sure to update tomorow's Higgs limits Peter?

4. **KD**
   July 22, 2011

   I can't help thinking supersymmetry is so popular because of the name. I mean, who wants to work on ordinary symmetry if we have supersymmetry? If it had been named more modestly, like Fermi-Bose symmetry, perhaps it would have been placed more appropriately.

   Same with string theory. "String theory": boring. "Superstring theory"? Wow! Must be super interesting.

   KD

5. **dir**
   July 22, 2011

just wander how fermilab disfavors 137 – 156 gev higgs

6. **piscator**
July 22, 2011

Seriously. The problem is not that there is no sign of susy, the problem is that so far there is no sign of *any* physics beyond the Standard Model. If a light Higgs is found, then the hierarchy problem remains as a real problem, and something needs to address it. The underlying argument for susy would remain unaffected – and a 1 part in 10^x fine tuning is a lot better than the 1 in 10^32 fine tuning of the Standard Model. This is why the crappest possible outcome would be a light mass Higgs with no signs of new physics – the best way to kill low-energy susy would be a positive signal for technicolour.

7. **younghun park**
July 22, 2011

piscator
I agree with you. Not finding higgs and SUSY paritcles will threaten the quatum field theory and Gauge theory. Now quantum field theory needs higgs or something like higgs. If we find nothing and no new physics, many physicists will doubt QCD and Electro-Weak theroy. Finding nothing will not only threaten SUSY and superstring theory, but also gauge theory.

8. **piscator**
July 22, 2011

QCD and electroweak theory seem in pretty good shape, one would hope so given the number of Nobel prizes already awarded for them….and the correctness or otherwise of strings as a theory of quantum gravity will not be determined by results from a TeV scale collider. But for the question of microscopic structure beyond the Standard Model, the LHC is the best hope there is, and we should hope something new does show up.

9. **Thomas Larsson**
July 22, 2011

My understanding is that the absense of something Higgsish at LHC would disprove QFT – Sean Carroll talks about a no-lose theorem. But the replacement (call it QG) must of course reduce to QFT and the SM in the low-energy limit. This is itself hardly shocking, since neither string theory nor LQG is equivalent to QFT. However, QG must reduce to QFT in such a way that the no-lose theorem is evaded, and this I think is not a property of string theory (and I doubt that LQG has a QFT limit at all).

10. **Alex**
July 22, 2011

Why would not finding a Higgsy thing disprove gauge theory, or even QFT? My take on that always was that it would simply mean that we lose perturbative control around the TeV scale, and it would be hard to explain all the nice electroweak precision observables. But the claim that QFT itself would have to be wrong because of that sounds way too strong to me.
Nonlinear realizations, fine with me.

asdf,

"So Susy turns out to be crap after all. Makes sense. It cut too deeply against Ocam."

It may not be realized in nature, but why does it violate Occams Razor so much? The susy breaking one needs to introduce in order to get a good light spectrum (if one insists) is sometimes, but not always, a little messy, but the concept of SUSY itself is very simple…

KD,

"I can't help thinking supersymmetry is so popular because of the name."

I can help you with that: no, that's nonsense.

Supersymmetry is popular because it's a simple concept, can be integrated in renormalizable theories, solves the hierarchy problem, can solve the dark matter problem. Or do you also think gauge theory is so popular because people like pressurized air so much? 🙂

11. **Peter Woit**
July 22, 2011

Thomas,

No Higgs doesn't mean QFT is disproved (and LQG or string theory are irrelevant), it just means that the simplest (and in many ways unsatisfactory) way that we know to describe electro-weak symmetry breaking is wrong. Every HEP physicist should be fervently hoping this is the case. If the LHC results showing the standard model works perfectly up into the multiple TeV range are joined with results showing the Higgs mechanism also works perfectly, the future of the subject is in big trouble (i.e. a victim of its own success).

Alex,

Supersymmetry actually is not that simple a concept, the MSSM is a lot more complicated than the SM. Many people have always found it an unattractively complicated idea that doesn't explain very much, and the LHC is in the process of justifying this point of view….

12. **Thomas Larsson**
July 22, 2011

Perhaps gauge theories are popular because some of them make predictions which are confirmed by experiments? Nah, that's so 1970ish.

Actually, we already know that QFT is wrong, since it most likely is incompatible with gravity. 🙂 But what I really would like to know if you can explain away the no-lose theorem by just hand-waving a bit about non-perturbative effects. A generation or more of theorists has said that the LHC is guaranteed to find something, or else WW scattering is non-unitary. Were they all mistaken?

Not that I would bet against a Higgs boson at LHC, at least not at even odds. Perhaps at 20:1.

For some reason my link to Sean's post did not work. Here it is in plain text: http://blogs.discovermagazine.com/cosmicvariance/2011/06/14/why-we-need-the-higgs-or-

something-like-it/

13. **Alex**
July 22, 2011

Peter,

No-one sane would disagree that the MSSM is much more complicated than the SM (I think…), but that's because of the modesty of the MSSM, right? – it remains willfully ignorant of the source and mechanism of spontaneous supersymmetry breaking. You can't deny that SUSY as a concept is simple, right? It's about as complicated as BRST, {Q,Q*} ~ 2P. Now you can gripe about how ugly SUSY breaking for "realistic" TeV scale SUSY is, fine, but I think the distinction should always be made for the sake of clarity.

14. **Thomas Larsson**
July 22, 2011

Peter, our posts crossed. Above response was directed to Alex.

15. **Alex**
July 22, 2011

Thomas Larsson,

I wasn't explaining it away, isn't that basically the no-lose theorem? We either observe something "higgsy", *or* we observe (though a lot less clearly) the loss of perturbarive unitarity, however that may look. I wasn't claiming that one wouldn't see anything, just that it wouldn't deserve the label "higgsy", but rather "messy" :).

16. **Paolo Valtancoli**
July 22, 2011

In my opinion, the searches for Higgs and supersymmetry are hopeless. The truth is probably in an orthogonal direction that no one has been able to see.

17. **M**
July 22, 2011

News from EPS: a higgs at 144 GeV and a anti-Higgs at 350 GeV

18. **Peter Woit**
July 22, 2011

Alex,

The very general mathematical idea of "supersymmetry" does lead to some amazing things and probably has something to do with reality (in a way we don't at all understand). But the supersymmetric extension of the SM, even without SUSY breaking, is not at all compelling, it just makes the SM more complicated, with minimal gain. And SUSY breaking is just a disaster. Claiming that "I have this wonderful beautiful symmetry, only problem is it is badly broken and I don't know how" is really not at all convincing.

19. **Alex**
July 22, 2011

Peter,

Of course we can argue all day, in the end nature doesn't care :), and I'm not here to fight the fight for SUSY, really. Just about your last comment, I do not understand your claim that that the supersymmetric version of the SM before SUSY breaking is so complicated. Beyond the unique supersymmetrization of everything, one only needs a second Higgs doublet for three independent reasons (but no second Higgs mass parameter!) and in return we even get rid of the quartic coupling, and the potential quadratic divergence, and that's it. Do I miss something?

20. **Peter Woit**
July 22, 2011

Alex,

You've doubled the number of degrees of freedom and, at a formal level, supersymmetric field theories are just more complicated beasts to write down precisely and do calculations in. I don't buy the argument that the MSSM Higgs sector improves the Higgs situation.

If this actually led anywhere, sure, it would be worth it. But all it leads to is the necessity of totally making a worthless mess because you need SUSY breaking.

21. **David Pennell**
July 22, 2011

http://www.nature.com/news/2011/110722/full/news.2011.435.html "Collider sees tantalizing hint of Higgs" did you already discuss this?

22. **Henry**
July 22, 2011

Peter,

I am sorry to disappoint you but SUSY is not dead at all. I should remind you that SUSY is not just mSUGRA or some simplified model that's been used by ATLAS/CMS to extract gluino/squark exclusion limits. You can have for example a compressed spectrum resulting in soft jets / leptons which will easily lower those exclusion curves. Besides 1 TeV is not too high…

23. **SA**
July 22, 2011

(In reply to comment #2)

The question about being “most likely” comes from naturalness. The papers you have linked are candidates for detection in the first round of combined LHC-7, XENON-100 data. Specifically, they found models where both the gluino (which the LHC is sensitive to) and the neutralino (which XENON is sensitive) are light. This means that in the case of a null result, they would be the first to be ruled out.

However, the naturalness/fine-tuning parameter relating the (high energy) higgs-mixing mass to the Z pole, can remain small for very large sparticle masses. This means that you should not look to squark-gluino or m_0–m_{1/2} exclusion curves, but instead exclusions in the Higgs sector to rule out SUSY. It is extremely unlikely to have SUSY with Higgs > 140 GeV, whereas gluino and squark masses can be very heavy.

24. **Benni**
July 22, 2011

I think I remember some private talking to some reowned person working at LHC. He/she laimed that LHC will have good exclusion data for the higgs in the range of 145-120Gev at the end of 2012… but this is just what their group expects…

25. **Alexandre Fauré**
July 22, 2011

Hi Peter,

Thanks for this very useful post.

It seems that this 2.8 sigma excess could be just background … Let’s see if there is an other end with more stat.
For the SUSY parts, perhaps we should remind ourselves about a low-mass Higgs predicted by SUSY too.

Feel free to answer to me If I have some parts I misunderstand.

Thanks again.
AF

26. **Sebastian Thaler**
July 22, 2011

Hype from the Guardian:
http://www.guardian.co.uk/science/2011/jul/22/cern-higgs-boson-god-particle

27. **Jobs**
July 22, 2011

“at a formal level, supersymmetric field theories are just more complicated beasts to write down precisely and do calculations in.”

While I agree with most of what you write here, Peter, the above statement is wrong. Something that has become apparent in the last few years is that, contrary to expectations, supersymmetric theories are significantly *easier* to perform calculations in. In fact the more SUSY a theory has, the “neater” it is, and the easier it is to calculate. Take a look at arXiv:0808.1446, a very instructive and interesting paper.

This doesn’t affect your argument about broken SUSY and the MSSM, which I think is just as messy as it appears. But perhaps we have no right to expect simplicity at low energies.

28. **Peter Woit**
July 23, 2011

Jobs,

Another way of saying what I was trying to say is that, to calculate things in the SM, you need to read volumes I and II of Weinberg. For any supersymmetric extension of the SM, you need this, and volume III. When there are lots of supersymmetries (N=2 and N=4 theories) you get interesting new ways to do calculations. That’s much less true of N=1, which is the experimentally viable case.

Is calculating g-2 of the electron easier in N=1 super Yang Mills than in the standard model?

Sebastian,

That article is fairly accurate and hype-free from what I can see.

Benni,

I think we’re not going to have to wait till late 2012, maybe more like late 2011…

Henry and SA,

Sure, you can push up the gluino and squark masses as much as you want to evade non-detection at the LHC. But, if you want SUSY to solve various problems it was supposed to solve, you get the sort of predictions that have now been shown to be wrong. For a more general argument, watch Arkani-Hamed’s talk at Strings 2005. He states quite clearly and vigorously that within a year or so of LHC turn-on, we’ll know whether SUSY is relevant to the electroweak scale (exactly because squarks and gluinos should be easy to see in the early data). His prediction has come true and the answer is now in.

One thing I’m curious about is whether Gross and others who were taking bets on SUSY are still willing to take such bets. I suspect they’re not yet willing to pay off, but they’re not about to take any new bets…

29. **Mitchell Porter**
July 23, 2011

A reminder of why supersymmetry is so popular in physics might be in order. Supersymmetric grand unification produces gauge coupling unification; supersymmetry can protect a light Higgs mass without finetuning; supersymmetry provides dark matter candidates; supersymmetry shows up in unified theories that include gravity. It’s a fairly impressive résumé, so it’s natural that so much effort has been expended in exploring the space of supersymmetric theories, looking for models which realize all the features above.

30. **Tobias**
July 23, 2011

Isn't it also true, that sometimes SUSY is introduced to make calculations easier if the result doesn't actually depend on the SUSY?

31. **piscator**
July 23, 2011

Jobs:

`apparent in the last few years'? Apparent in the last thirty years would be more apt. It has been known for a long time that the more supersymmetry present the more special the theory is, and I don't think the paper you refer to adds to this.

Just to be explict for Peter: there are *lots* of computations that are extremely easy to do in theories with N=1 supersymmetry that are *impossible* to do in theories with N=0 supersymmetry. For example, the question 'does a vacuum of the theory with exactly vanishing vacuum energy exist?' can be answered in N=1 theories to arbitrary order in perturbation theory with very little work. But I'm sure you know this, so I am puzzled why you are trying to argue to the contrary.

32. **Peter Woit**
July 23, 2011

piscator,

My argument is really motivated by an allergy to SUSY hype. Sure, there are calculations you can do in SUSY theories you can't do in non-SUSY theories. But, the technical baggage that comes with SUSY is substantial, and for experimentally relevant calculations in the SM, it's typically either not useful or less than useful. I'm legitimately curious about whether one can point to uses of SUSY to help with loop calculations in the SM like the electron g-2.

One simple example: one main source of reliable non-perturbative calculations in the SM is lattice gauge theory. There SUSY is worth than useless, adding SUSY to the SM generally changes things from difficult to impossible.

33. **Eric**
July 23, 2011

Peter,

Yes, supersymmetry helps with loop calculations, the primary example being the loop corrections to the Higgs mass. The effect that supersymmetry has on loop corrections is, in fact, one of the main reasons for theoretical interest in it.

34. **Eric**
July 23, 2011

Also, supersymmetry can have a strong effect on g-2 of the muon. Corrections to g-2 of the muon have long been looked at as possible evidence for supersymmetry.

35. **anon.**
July 23, 2011

It's just not accurate to say that the LHC is excluding gluinos and squarks below 1 TeV. What they are excluding are SUSY spectra in which gluinos and squarks always decay directly to a light neutralino plus jets. Whatever your prejudices are about SUSY models and mechanisms of SUSY breaking, you should appreciate that this is a very specific assumption. If you assume, for instance, that every decay chain always has one extra step — gluino decays to jets plus a neutralino, which in turn decays to another particle or particles and a lighter neutralino, for instance — you will find that the typical limits on gluino masses are lower by at least a couple hundred GeV, and often more, depending on details.

The limits are very strong; the LHC detectors are working remarkably well and the collaborations are making good use of the data. But it is not yet the case that the results exclude new strongly-interacting particles below 1 TeV. The easiest results are getting out first, and these results are not representative of the bulk of SUSY parameter space.

36. **Low Math, Meekly Interacting**
July 23, 2011

Hi, all,

Without wishing to create a tangent to the argument about the pros or cons of SUSY, SUGRA, or what-have-you, I just have a simple question: Are we now much closer to or at the point that the MSSM has been ruled out?

Thanks!

37. **Peter Woit**
July 23, 2011

anon.,
But where is the "bulk of SUSY parameter space"? By some measures, it's off beyond the LHC reach. I've been pointing to some of the pre-LHC work I'm aware of by groups trying to give an answer to that question in terms of assumptions that SUSY does various things it is supposed to do, as well as general statements such as Arkani-Hamed's one at Strings 2005, and these seem to me starting to be in serious conflict with the data. This is not just my opinion, see the last slide of the talk by Rogerson on SUSY fits: "Air is starting to become very thin for these constrained models of SUSY". If you start relaxing these constraints generically, do you evade the LHC results? Or do you need to start picking specific directions to go in to do this? As an example, is adding steps to the decay chain generic or not?

LMMI,

The problem is that the MSSM has 120 or so extra parameters. You can't ever rule it out, all you can rule out are parameter ranges, specifically the parameter ranges corresponding to various motivations for the MSSM. If your only motivation is that you like the idea of SUSY, that you like the expected CC to be off by 60 instead of 120 orders of magnitude, or some such, there's no way to rule this out.

38. **anon.**
July 23, 2011

see the last slide of the talk by Rogerson on SUSY fits: "Air is starting to become very thin for these constrained models of SUSY".

The key there being "these constrained models." In fact mSUGRA and the other similar constrained models being fitted are not even *models* in any real sense, they're ansätze chosen to evade flavor problems but not really consistent with any reasonable UV-completion I know of. (Of course, I don't know any real SUSY model that's *completely* free of problems; but at least I know some that are honest models.)

If you start relaxing these constraints generically, do you evade the LHC results? Or do you need to start picking specific directions to go in to do this? As an example, is adding steps to the decay chain generic or not?

If the gravitino is light enough (e.g., in low-scale gauge or gaugino mediation), there is always at least one extra step in the decay chain, and generically this weakens limits quite a bit (although more specific targeted searches recover part of the difference). Another fairly common scenario that can arise in high-scale SUSY breaking models is that instead of getting light quark jets in decay chains, you can get a substantial fraction of tops. Because the top mass is bigger and it has more decay products, these events tend to fail the jets+Met cuts (less missing E_T, lower energy jets). Again, though, targeted top-related searches might still exclude part of this parameter space. More generally, it's pretty generic to have extra steps, but the decay considered in the ATLAS search, for instance, still happens with some branching fraction. So the limit is weakened by a model-dependent amount.

It's very hard to make completely general claims. The constraints are very strong and getting stronger; I don't mean to suggest otherwise. I think there's cause for at least a little concern (not just about SUSY, but about finding new physics beyond the Higgs at the LHC at all!). Certainly I was hoping there would be some kind of unambiguous new physics showing up by the time we had 1 fb^-1. But, there *are* still sensible models where the gluino can be in the neighborhood of 500 to 600 GeV without being ruled out, and somewhat more contrived models where it can be quite a bit lighter even than that.

39. **Benni**
July 23, 2011

yes peter, it was a typo. My source ment late 2011. The sentence should be

He/she laimed that LHC will have good exclusion data for the higgs in the range of 145-120Gev at the end of 2011… but this is just what their group expects…

40. **Jobs**
July 23, 2011

"My argument is really motivated by an allergy to SUSY hype. "

This is admirable, but anti-SUSY hype is just as bad as SUSY hype. We should try and be objective.

Regarding supersymmetry being used in SM computations, I can testify that it provides an extremely useful organisation of loop calculations. Indeed, you can separate one-loop amplitudes in QCD into "SUSY parts" and "non-SUSY" parts, and this division is a significant aid to calculation. This says nothing about whether low energy susy exists, but it does point to QFT and SUSY being natural bedfellows.

41. **Low Math, Meekly Interacting**
July 23, 2011

Hi, Peter,

That's kind of what I thought, but thanks for clarifying.

42. **anonomous**
July 23, 2011

What are the chances of finding an attractive alternative to Higgs/SUSY through phenomonology by the end of the year? Is anybody suggesting new settings for the detectors? I am just thinking that if the Higgs is excluded by the end of 2011 there won't be much data in other areas as the LHC shuts down for the next year.

43. **Peter Woit**
July 23, 2011

anonomous,

The LHC will run not just through the end of this year, but through the end of next year. The Higgs should keep them busy, either conclusively ruling it out, or studying a real signal.

If the Higgs is ruled out at all masses, there will be a lot of attention on the implications of this, and the question of where the LHC can look that might provide a clue as to what is going on. Maybe different kinds of triggers and searches will get attention, but I'd guess that this would wait until after the machine turns on again at higher energy in late 2014/early 2015.

44. **Garrett**
July 23, 2011

"One thing I'm curious about is whether Gross and others who were taking bets on SUSY are still willing to take such bets. I suspect they're not yet willing to pay off, but they're not about to take any new bets…"

At the last FQXi conference, Frank Wilczek expressed a lot of confidence in SUSY. During the questions after his talk I suggested we place a bet, and he took me up on it. (He's a good guy, and I respect him a great deal, but we differ in our SUSY confidence.) I figured it would be a typical $1 physicist bet, but he suggested $1K and a six year deadline. I agreed. So now we have a public bet that superparticles will or won't be seen before July 8, 2015. Max Tegmark is our referee.

There has to be a better way to make money than betting against nobel laureates, and I may lose, but it's certainly fun.

45. **piscator**
July 24, 2011

LMMI:

IMO we are a lot closer to the point where the MSSM is ruled out, but not any closer – in fact perhaps further – from the point where low-energy susy is ruled out.

For the MSSM, the required fine-tuning in the Higgs mass looked contrived before the LHC turn on and looks even more contrived now. If the hints of broad excesses in

120-150 GeV in the Higgs search turn out to be true, then things will be even trickier, as to further raise the Higgs mass in the MSSM requires either very heavy sparticles or large trilinear terms, and this all looks terribly ugly and fine-tuned.

However, for supersymmetry in general, this does not hold. From a top down perspective there is diddly squat reason to think that the only new particles nature can have is one Higgs doublet. In models beyond the MSSM it is not hard to raise the Higgs mass and so a Higgs of (say) 135 GeV is not a good reason to reject low energy supersymmetry per se.

The way I see it, the hierarchy problem is real and there is something that solves it. The only two good candidates for this are susy and technicolour (or its modern Randall-Sundrum avatar). If a light Higgs exists – and the EPS results give preliminary hints that it does – then I regard weakly coupled solutions to the hierarchy problem (like susy) as preferred, if not definitively, to strongly coupled solutions like technicolour.

IMO anti-susy partisans need an intelligible opinion on the hierarchy problem – the Higgs mass *is* quadratically divergent in the Standard Model. You either think this isn't a problem (and if so why not?) or if you do think this is a problem, then you must describe how you will solve it. If you think susy is bad, study the alternatives….

46. **Unknown**
July 24, 2011

Dear Peter,

You complain about the doubling of degrees of freedom in SUSY but you fail to realize that multiplying by 2 is not the same as adding 50 new particles. After all, doesn't antiparticles double the number of degrees of freedom? Does that bother you?

Then you say that SUSY QFTs are more complicated. How could this be? They are
as good QFTs as any other but with more symmetry (which means less independent couplings, more predictive power, unexpected cancellations an so on). How could this be more complicated?
If you "don't buy the argument that the MSSM Higgs sector improves the Higgs situation", then I'm afraid is hopeless to argue with you at all.

47. **Peter Woit**
July 24, 2011

Unknown,

This is just ridiculous. Adding SUSY to the SM give you the MSSM (or worse), which has 120+ more parameters than the SM and is vastly more complicated. After 30 years of failure, believing the problem of SUSY breaking can be solved in some simple way that gives a simple, predictive theory is just wishful thinking. With no symmetry breaking, all you have is a theory in violent disagreement with reality, with lots of new symmetries, all of which do nothing but relate degrees of freedom we know about to ones that can't possibly exist.

Piscator,

The Higgs mechanism seems to me to have more serious problems than the hierarchy problem. Problems with the Planck scale are QG problems we really don't understand. Before worrying about whether another scale (e.g. the GUT scale) can be stabilized with respect to the weak scale, one should first have evidence for its existence.

Besides the problem of whether a non-asymptotically free theory like the Higgs really makes sense at high energies, the really serious problem is that the Higgs makes it impossible to ever calculate most of the SM parameters. This is really ugly, unlike the rest of the SM, which is a fantastically powerful and beautiful mathematical structure. SUSY doesn't solve this problem, instead makes it worse.

In any case, first of all the question is whether the Higgs really is there, and it's very exciting that the next few months might see an answer to this question after so many years.

48. **Unknown**
July 24, 2011

I guess I'm losing my time arguing with you, but maybe some readers of this blog will benefit after being exposed to your misconceptions.

"…new symmetries, all of which do nothing but relate degrees of freedom we know about to ones that can't possibly exist." I beg your pardon, maybe you published some proof of the inconsistency of SUSY?

"Adding SUSY to the SM give you the MSSM (or worse), which has 120+ more parameters than the SM and is vastly more complicated."
The trees don't let you see the forest. What you get out of SUSY is the ability of explore experimentally, through the SUSY spectrum, physics at extremely high energies, and it's difficult to imagine how the hell could you get a similar ability in any other theoretical framework.

"After 30 years of failure," 30 years of failure are fine as soon as the first superpartner turns up. Let's talk about failures then.

"The Higgs mechanism seems to me to have more serious problems than the hierarchy problem. Problems with the Planck scale are QG problems we really don't understand." Do you find the see-saw mechanism attractive? If you do, you have the Higgs hierarchy problem exploding in your face.

"Besides the problem of whether a non-asymptotically free theory like the Higgs really makes sense at high energies,…" Next time I hear this often repeated B.S. I'm gonna throw up. You're really concerned about QED then?

"the really serious problem is that the Higgs makes it impossible to ever calculate most of the SM parameters." Excuse me?

"This is really ugly, unlike the rest of the SM, which is a fantastically powerful and beautiful mathematical structure." Yeah, like the flavour sector.

Sometimes one wonders if you're really a particle theorist.

49. **MF**
July 24, 2011

Btw, QED is beautiful.
The SM and gauge theories are not, there is a lot of ugliness that people tend to forget about. Like ghosts, BRST and so on…

And who cares about the Planck scale? The Higgs sector has problems even without looking at such an high scale. EWPT? Little hierarchy problem? On the other hand if we forget about the Higgs boson there is loss of unitarity in W scattering. I don't think i have to go on with such famous old stories (even if it seems people forget them too).

50. **Alex**
July 24, 2011

PW: "…new symmetries, all of which do nothing but relate degrees of freedom we know about to ones that can't possibly exist."

Is spontaneous breaking against the law now?

Unknown: "You're really concerned about QED then?"

Or, more to the point, the U(1) of hypercharge…

Unknown: "Yeah, like the flavour sector."

I think Peter meant exactly that in the comment before when talking about the Higgs taking away our inability to calculate most parameters. That phrasing is maybe a bit obscure. I suppose he hopes for an alternative which tells us all about flavor at the TeV scale where we can observe the mechanisms, as do I. Wouldn't that be nice indeed. Unfortunately, we do not have any hard reason to think that nature is this nice to us , not theoretical nor phenomenological, apart maybe from the hierarchy problem itself, which he however discusses away.

Repeating the slogan "susy has 120 parameters" over and over again is not good style. I'm also not in favor of hyping anything, but throwing wrong statements around is not the way to respond to a hype. A little more nuance is all I would be asking for. There are more adequate ways to criticize the idea of TeV scale supersymmetry than repeating simplistic half-truths like that, there really are. Talk about how gruesome difficult many models of SUSY breaking are, and how msugra is nonsense and why.

51. **Alex**
July 24, 2011

"taking away our inability"

erm, I meant "taking away our ability" of course.

52. **Peter Woit**
July 24, 2011

MF,

Personally I think the ghosts/BRST issue does involve beautiful mathematics. The problem is that we don't completely understand it (which makes it more interesting, and it's why I work on it..). Mathematically, this is about using Lie algebra cohomology to understand fundamental issues in representation theory. The question of exactly how "beautiful" this is does await a fuller understanding.

Going from QED to the SM – Higgs is just going from U(1) to slightly larger Lie groups, so the "beauty" issue doesn't change much. The fact that this requires a more sophisticated way to deal with imposing gauge symmetry may be a feature, not a bug.

53. **Peter Woit**
July 24, 2011

Alex,

The problem of the 120 extra parameters and the problems of models of spontaneous SUSY breaking are functionally the same. I don't see anything wrong with avoiding a long description of the various ugly and complex ways people have found to spontaneously break SUSY without contradicting experiment, and just referring to the bottom line: lots of undetermined parameters. In general, I think it's misleading for anyone to go on about the beautiful physics of their favorite symmetry, when they have to introduce a lot of ugliness to avoid contradicting experiment.

54. **Peter Woit**
July 24, 2011

Unknown,

"…new symmetries, all of which do nothing but relate degrees of freedom we know about to ones that can't possibly exist." I beg your pardon, maybe you published some proof of the inconsistency of SUSY?

You deleted the first part of this sentence "with no symmetry breaking", which is the point. Without SUSY breaking, your symmetry relates each particle to one with the same quantum numbers and the same mass, something that is known not to exist.

"What you get out of SUSY is the ability of explore experimentally, through the SUSY spectrum, physics at extremely high energies" This is just unadulterated hype. So far SUSY has told you zero about physics at the GeV/TeV scale. Conviction that it's going to not only appear, but in a form that tells you about "extremely high energies", is nothing but wishful thinking.

No, I'm not a huge fan of the see-saw mechanism. Introducing yet another higher energy scale as an explanation isn't very convincing. Maybe it's right, who knows, but the bottom line is that we now really don't know anything about where fermion mass matrices come from.

The problem of the SM U(1) being non-asymptotically free can be evaded with GUT unification. The cost of course is adding yet more scalars, so the problem with scalars like the Higgs has to be dealt with somehow.

"Excuse me?" Maybe you've heard that Higgs Yukawa couplings give fermions masses? These are unconstrained in the SM. I suppose the standard ideology about this these days though is that we're supposed to give up, and just bow down to the glory of the multiverse, which picks such couplings out at random.

The flavour sector is a beautiful structure (the Dirac equation coupled to gauge fields) IF you throw out the Higgs couplings.

"Sometimes one wonders if you're really a particle theorist."
Besides being a mathematician, I am a trained particle theorist, although my training (late 70s-early 80s) was in a period when supersymmetry was around, but it had not hardened into a hype-filled ideology. My concern is that the standard training of many theorists over the past 25 years has been within an ideological framework based on some wrong assumptions. The LHC is in the process of blowing up some of the underpinnings of this ideology. The interesting question is whether many people will admit this is what has happened or hold fast to their ideology in the face of contradiction by experiment. I'm afraid that even post 2015-6, when LHC results at the highest

energies are in, SUSY die-hards will be repeating the same mantras, unwilling to give up on the idea, no matter what. You can already see that happening with string theory….

55. **Peter Woit**
July 24, 2011

Garrett,

Thanks for that piece of information. I'm curious whether Wilczek is still willing to take bets on the same odds post this summer's LHC results. If you hear that he is, let me know…

56. **Low Math, Meekly Interacting**
July 24, 2011

Thanks for your perspective, piscator.

57. **Alex**
July 24, 2011

"With no symmetry breaking"

D'oh, one should always go to the primary literature!

I see your point that merely talking about the beauty of supersymmetry misrepresents the difficulty of constructing a realistic low energy susy model. That being said, there may be one that is simple

58. **piscator**
July 24, 2011

Peter,

>>In any case, first of all the question is whether the Higgs really is there, and it's >>very exciting that the next few months might see an answer to this question after >>so many years.

Amen to that.

LMMI,

Cheers, no problem.

59. **Yatima**
July 24, 2011

To misquote a movie villain:

"This bickering is pointless. LHC will provide us with the location of the Higgs mass by the time this year is over. We will then crush the doubters with one swift stroke!"

(Or not)

60. **Low Math, Meekly Interacting**
July 24, 2011

I have to say, it's truly, truly wonderful to start seeing the first hints of theoretical people arguing about…lots of data! Nothing at all against theorists of any kind, but I've felt the field of HEP has suffered long enough without some fresh meat to chew on. As a huge fan of physics, especially that golden era of the 20th century when the SM was born and matured, the idea that we might be on the verge of seeing something utterly new is exciting.

61. **chris**
July 25, 2011

Higgs at ~120GeV and nothing else. looks like i'll win by bet about what LHC is going to find 🙂

62. **DGP**
July 28, 2011

SUSY isn't dead yet but it's certainly sick. I suspect that the experimentalists are going to lose interest in it completely if the LSSP is not found under ~1.2TeV, for one simple reason: the best pointer to new physics we have is the high mass of the top quark. If something isn't found that can explain that then there will be plenty of work for theorists – but not on supersymmetry and string theory (superstrings were originally attractive because they could save supersymmetry from its internal contradictions: that won't be necessary if SUSY is no go).
Searches for supersymmetry will probably continue even if a blank is drawn but really they will be searches for new physics (and a reason for the top quark mass).

I personally wasn't expecting evidence for supersymmetric particles below ~0.9 TeV since I think that there would be some evidence in the form of effects on lower-energy physics which would have already been puzzling us so the current results haven't changed my opinion (that SUSY is unlikely) either way.

The measurements I am looking forward to are the ones of the various coupling "constants" (alphas) at increasing energies. The most reasonable interpretation of the current data is that Unification does not happen. If there isn't some evidence of a tendency for the coupling "constants" to head for a common asymptope by 14 TeV, Grand Unification hypotheses will look far less tenable. As it is, I am amazed by how many people seem to be taking the Unification Energy as a data point.

63. **echo**
July 30, 2011

But even if only a single, SM-like Higgs is found, its dynamics should be pretty interesting. Without SUSY (if it's actually not there) the naturalness problem remains unsolved. And it is quite doubtful that a simple quartic potential put by hand into the lagrangian just for the purpose of breaking the symmetry is the end of the story. More statistics, and eventually more energy, are going to reveal a lot of interesting stuff.

String Theorists Throw SUSY Under the Bus

July 25, 2011
Categories: Favorite Old Posts, Uncategorized

Over the past few days the results of the 2011 LHC run have been revealed at the EPS-HEP 2011 conference in Grenoble, where a press conference today marked the beginning of the next part of the conference, featuring summary talks. For some discussion of these results see for example here, here, here, here and here. The bottom line is much stronger results ruling out supersymmetry, extra dimensions, black holes and other exotica, restriction of the possible mass range of the Higgs to about 114-150 GeV, and a tantalizingly small and not yet statistically significant excess of possible Higgs events in the mass range 120-145 GeV.

The big surprise here is that the experiments have done a fantastic job of getting these analyses of the data done at record speed. Before the LHC turn-on, estimates based on experience at the Tevatron tended to be that it would be 2012 before we saw completed analyses of a significant amount of the 2011 data. A lot of people have been working long hours and going without a summer vacation… The bottom line though is not a surprise, but rather pretty much what many people (including myself) expected. The unconvincing popular theoretical models of the last few decades have finally been confronted with experiment, which is falsifying them, to the extent that they can be falsified. It's an inspiring example of the scientific method working as it should. The remaining mass range for the Higgs is the expected one, and, as expected, this is the hardest place to separate the Higgs from the background. If it's really there, the data collected during the rest of this year should be enough to give a statistically significant signal. So, within a few months we should finally have an answer to the question that has been plaguing the subject for decades: "Higgs or something else?". This is very exciting.

For more than a quarter-century, supersymmetry has been advertised as the most significant prediction of string theory. Back in 1996 Gross and Witten responded to John Horgan's skeptical take on string theory in *The End of Science* with an article in the Wall Street Journal where they claimed:

> There is a high probability that supersymmetry, if it plays the role physicists suspect, will be confirmed in the next decade. The existing accelerators that have a chance of doing so are the proton collider at the Department of Energy's Fermi Lab in Batavia, Ill., and the electron collider at the European Center for Nuclear Research (CERN) in Geneva. Last year's final run at Fermi Lab, during which the top quark was discovered, gave tantalizing hints of supersymmetry. The situation should be clarified when this machine is upgraded in 1999. (A further upgrade, which would cost the Department of Energy about $300 million, should be seriously considered.) As for the CERN electron collider, its energy is being increased by 35% in the next few months. The results could be dramatic, since electron colliders, though their energy is generally much lower than that of proton colliders, are rather thorough and swift in exploring certain phenomena.
>
> If supersymmetry is out of reach of these existing colliders then it is very likely to be discovered at the Large Hadron Collider, which will begin operation at CERN in about a decade…
>
> Wherever it occurs, the confirmation of supersymmetry would open up one of the golden ages of experimental physics. It could provide us with essential insights about the unification of the four major forces; that is, a theory that would describe gravity, the strong nuclear force, the weak atomic force and the electromagnetic force as varying expressions of a single phenomenon. And it would give a big boost to the development of a remarkably rich new theoretical framework known as string theory. For supersymmetry is one of the basic predications of string theory.

The next year Physics Today published Gordon Kane's String Theory is Testable, Even Supertestable, which included a plot showing gluinos and squarks as having expected masses in the range of 200-300 GeV (the latest results rule them out in typical SUSY models up to about 1000 GeV).

Today, the most prominent active string theory bloggers have blog entries reacting to the weekend's news. Clifford Johnson has Living in Interesting Times, where he writes:

> One of those hoped for stories is called Supersymmetry, which would imply the existence of several more particles besides just the Higgs. Now, the cool thing is that the simplest models of supersymmetry could be in danger as well if we do not see something in the coming several months. Wouldn't it be interesting if both the Standard Model Higgs and the simplest models of Supersymmetry were ruled out? (I'm not saying that they are – it's all to soon to tell – but it is a possible outcome.)

When the LHC turned on, Lubos Motl was blogging about Why supersymmetry should be seen at the Large Hadron Collider, giving the probability of the LHC seeing SUSY as "90% or higher". After the results of the last few days, he's done a 180 degree turn, with a new blog entry attacking phenomenologists and arguing that the LHC results just show that HEP theorists should be doing string theory, not phenomenology:

> No hep-th theorist has ever claimed or boasted that the bulk of his work had too much in common with the data produced by the next-generation collider so of course, the hep-th work isn't really affected by the "null" results from the LHC. Many theorists and many string theorists – but not all – would feel more excited if the LHC were generating totally new phenomena and their phenomenological friends would be really thrilled. However, it's still true that the theorists don't care as much as the phenomenologists do.
>
> What I really want to say is that most of the phenomenological work has been a waste of human resources and time. Instead of producing 1,000 models that could be relevant for the sub-TeV observations, those people could have just waited for a few years and let Nature speak. And it seems that Nature has spoken – and it may still speak in an ever clearer language – and so far, the answer is that the right model of these phenomena is called the Standard Model…
>
> So I hope that instead of shifting the energy scales from 200 GeV to 1,400 GeV and continuing in random guessing, many phenomenologists will buy some string theory textbooks and begin to think about the Universe at a slightly deeper and less sensationalist level.

Update: Lubos clarifies here: he's only throwing some SUSY models under the bus, not all of them. It's no longer above 90%, but he still thinks there's a 50% chance that the LHC will see supersymmetry. And all the bogus claims for "tests of string theory" are my fault, since I created a hostile environment for string theorists where they felt they had to do this kind of thing.

Update: The MasterCode Project has moved up to higher masses its "best-fit" points for SUSY now that 2011 LHC results have ruled out previous "best-fit" points, see here. Now the "best-fit" for SUSY is not even a very good fit… Tommaso Dorigo explains and comments here.

Update: In his talk concluding the conference, David Gross throws just the CMSSM under the bus, saying it is now "on life support". He argues though that this is just one possible SUSY model, and one can't conclude much from the death of the CMSSM. Much of his talk was an advertisement for N=4 SSYM and AdS/CFT. He's sticking to his prediction of last year that SUSY particles will appear within 10 years, no word on when he'll give up if the LHC continues to see nothing. Near the bottom of his list of predictions was "string theory will start to be a THEORY, with predictions", which drew laughter from the crowd. He acknowledged that it was next to last on a list ordered by plausibility, but insisted "Some day…"

Update: Pauline Gagnon reports on what theorists are up to in response to all this:

> This summer, I had the opportunity to spend a week at a theory workshop. Being the only experimentalist there, I spent plenty of time discussing what was going on in their camp. Clearly, they are not sitting idle while we are frantically searching our recently collected data for signs of new physics or the Higgs boson. On the contrary, many of them were already hard at work trying to find excuses for supersymmetry and reasons why it has not shown up yet as anticipated.

At Cosmic Variance John Conway summarizes the situation, and draws flak from Matt Strassler, who explains more here, and has a new paper out about how to evade the LHC results:

> This is a key job of particle theorists; make sure **all** the ground gets covered by the experimentalists before they give up and move on!

Given the huge number of possibilities and parameters for how to implement SUSY, insisting that all of it gets tested by experiment will ensure that SUSY phenomenology will be with us for a very long time. Ideas like SUSY can never be completely ruled out, they can just be made so unlikely that they're not worth people's time anymore, and the argument over how much more unlikely the LHC results make SUSY will continue…

Comments

1. **chris bolger**
   July 25, 2011

   If supersymmetry is not found, what is the best explanation of the running of the coupling constants meeting precisely at the GUT level, as found in the early 90's, in its absense?

2. **Peter Woit**
   July 25, 2011

   Chris,

   They don't actually meet "precisely", although the agreement is better than in the non-SUSY SM. The relative strengths of the SM couplings is just something we fundamentally don't understand. GUT explanations haven't worked out well, with their generic observable prediction (proton decay) not being observed. Note that to believe the SUSY GUT calculation of meeting of couplings, you have to believe that there is no physics affecting their running, from 1 TeV scale up to the GUT scale (the "desert hypothesis"). Most physicists have always found this difficult to accept.

3. **John Baez**
   July 25, 2011

   I've never believed supersymmetry was true in our universe. Earlier this spring I won a case of scotch from the particle physicist Dave Ring, on an old bet about whether the LHC would see "strong evidence for supersymmetry" after one year of operation.

   A true gentleman, he sent me an email saying "I believe I owe you a case of scotch. I knew they'd go over schedule at the LHC, but not by this much!" And he even got me 18-year-old Laphroaig.

   I would feel guilty if I won on a technicality. So, I'm glad they're not seeing evidence of supersymmetry.

4. **Eric**
   July 25, 2011

   Might it be pointed out that the remaining low-energy range (115-135 GeV) allowed is precisely where the Higgs should be if TeV-scale SUSY exists?

   Furthermore, the energy range above 450 GeV is still allowed, which is where the Higgs should be if there turns out to be a heavy fourth generation.

5. **neo**
   July 25, 2011

   I am confused. I read elsewhere that at Higgs found between 115-140 GeV could be a SM Higgs or a SUSY Higgs. Why do you think the current range is ruling out SUSY?

6. **Alex**
   July 25, 2011

   Dang! I should have placed bets with Laphroaig. But wait! there is still time!
   Who is willing to bet a bottle of Laphroaig against the idea that there are particles that look like color-charged superpartners between 1200 and 1500 GeV? 🙂

   neo,

   Such a light Higgs is not only compatible with the SUSY, it is almost a necessity to have a Higgs in this region unless you add further stuff like a fourth generation, or make the superpartners very very heavy, to pull up the higgs using large loop corrections. Many models of SUSY being ruled out is simply for the lack of a signal in the usual SUSY discovery channels, like jets or leptons with missing energy (For R-Parity conserving SUSY), or corresponding observables for R-Parity violating SUSY.

   Eric,
   "which is where the Higgs should be if there turns out to be a heavy fourth generation."
   Not too long ago, a standard fourth generation could maybe barely hide there, the trouble is that this would not only generically predict a heavy higgs, but also a production cross section that is a factor 5-9 higher than the SM one, because all of those new particles contribute to the coupling of gluons to the higgs. The ways to circumvent that is 1) to have a fourth generation and a fourth antigeneration, which would not have to talk to the higgs to be massive. But that's not really a proper fourth generation, or 2) Have the higgs decay invisibly, which is of course not very compelling if only done for that reason.

   chris bolger,

   Making the couplings kind of meet without SUSY is possible by adding further fields, like for example a handful of scalar doublets. But then if you really postulate some kind of GUT at the resulting unification scale, you have to engineer away proton decay, and the hierarchy problem really shows up explicitely in your theory then because you have introduced explicitely a very high scale which turns up in the radiative corrections to the Higgs potential.

7. **Peter Woit**
   July 25, 2011

   neo,

   The Higgs results don't rule out SUSY. What is causing trouble for SUSY is separate searches for strongly interacting superpartners which have turned up nothing.

8. **DB**
July 25, 2011

“a tantalizingly small and not yet statistically significant excess of possible Higgs events in the mass range 120-145 GeV.”

In the hands of idiot journalists, this becomes
“Scientists in the US announced they may have detected the elusive and potentially universe-changing Higgs boson particle yesterday, just two days after rivals in Switzerland signalled that they, too, have caught their first sight of it. “

The Independent is considered to be a quality UK newspaper.

9. **Peter Woit**
July 25, 2011

DB,

As usual, in this case it’s not only journalists who are to blame, see

http://blogs.nature.com/news/2011/07/hep2011_a_whiff_of_higgs_at_th.html

10. **Peter Woit**
July 25, 2011

For a more problematic journalistic take on the situation, see

http://www.newscientist.com/article/dn20729-should-we-worry-about-what-the-lhc-is-not-finding.html

where we learn that:

“there is no sign yet of gravitons – particles that transmit gravity and are essential for a quantum theory of the force – below an energy of 2 TeV”

11. **Alex**
July 25, 2011

“For a more problematic journalistic take on the situation, see”

Apart from giving Supersymmetry, which you dislike, a prominent position and attributing it a tiny bit more than it can actually do for us, I thought it’s not that terrible an article. The 2 TeV Graviton, that obviously came out of a mingled statement about Randall Sundrum I suppose. I’ve seen much worse in official LHC public relations, like the whole recreating the big bang business…

12. **lun**
July 25, 2011

I think one needs to be careful about confusing “string theorists” with famous bloggers who write on string theory and more or less accepted spokespeople of the string theory community.

Some string theorists have “trown supersymmetry under the bus” well before the LHC gave them a reason to do so:
http://arxiv.org/abs/0804.4718
http://arxiv.org/abs/hep-ph/0104274
while these papers were ignored by bloggers and the scientific press, this does not necessarily make them invalid. This, in fact, illustrates one of the main problems of the way string research is done: The extreme faddishness of the field.

13. **David Bailey**
July 25, 2011

Given the colossal pressure on CERN to come up with some results, can we be 100% certain that they would not fudge the evidence for the Higgs?

14. **Peter Woit**
July 25, 2011

David,

It’s not even clear which way you would want to fudge this. If they find the Higgs, they have an important discovery to crow about. If they rule out the Higgs, that’s an equally important (and much more interesting…) discovery.

Remember that there are two major independent experiments looking for the Higgs. Each one of them is under a lot of pressure to not only make a discovery, but get it right. If they don’t get it right, their competition is sure to take advantage of this. Also remember that there are 3000+ physicists on each experiment. If some subset of them tried to fake the most important result they were looking for, their colleagues would not stand for it. Getting 3000+ physicists to agree to anything is nearly impossible, much less getting them to agree to a faked result.

15. **Alex**
July 25, 2011

@David Bailey,
It’s not CERN’s job to analyze any data. They just provide the bang for the buck so to speak. Two collaborations of thousands of people who run the two main Higgs search experiments independently, do that. That does not mean that the people doing the analysis for the collaborations didn’t put some prejudice in these early results about our indirect expectations of a light Higgs, but that will quickly fade away with more statistics. You can be certain that, barring a sheer criminial conspiracy, an eventual Higgs discovery claim will be held under great scrutiny by a large number of people.

16. **Yatima**
July 25, 2011

I sure am glad that Lubos is an outlier. This mindset is appalling, tending towards occultism. A lifetime study of Kabbalistic texts can’t be far away.

17. **anonymous**
July 25, 2011

People have realized that string theorists are out of touch with reality, as evidenced by the near-absence of string theorists in this year's Particle Rumor Mill.
The LHC might soon prove that "phenomenologists" are out of touch with reality as well, as their models get decimated.
How about we simply disband the whole hep enterprise? 30 years ago smart people already knew that particle physics was a dying field.

18. **Bee**
July 26, 2011

I'd agree that "Instead of producing 1,000 models that could be relevant for the sub-TeV observations, those people" SHOULD "have just waited for a few years and let Nature speak." (Funny enough, I've said almost exactly the same in my talks during the last years to explain why I stopped working on models with large extra dimensions 6 years back…) But whether they COULD have waited is a different question. I believe I've commented here several times saying that this trend towards phenomenology seems unreasonable to me because many of the models are very poorly motivated (if motivated at all). And I say that as a phenomenologist myself. So why the paper flood? I guess the reason is simply too many people, too few jobs, pressure to publish and then in the late 90s everybody started yelling 'falsifiability' which did the rest. It doesn't really matter which way you shove scientists, it always has a backlash. However, since that is unlikely to change anytime soon the question is now where will the next bubble be? I actually think we already see it coming, and it will be in cosmology.

19. **Neil Harris**
July 26, 2011

I don't understand why Lubos Motl has to be so rude about Peter Woit, but then I'm southern English.
This quote from Motl is worrying: "No hep-th theorist has ever claimed or boasted that the bulk of his work had too much in common with the data produced by the next-generation collider so of course, the hep-th work isn't really affected by the "null" results from the LHC." It begs the question: what experimental evidence does affect hep-th work?
It cannot have escaped the notice of all physicists that the world seems to be running out of money (next week, in the case of the US federal government). It may be that the CERN LHC is the last particle collider, or that any successor will be a long way into the future. This has implications for experimental and theoretical physicists. There again, if experimental evidence is irrelevant to theorists like Motl, he'll be able to carry on.
As a mere science documentary film-maker I cannot contribute at a technical level, though I was honoured to film an interview with Peter Higgs back in 1995. He's a charming, modest man and strove mightily to explain a difficult subject to a general audience. I am crossing my fingers for his sake that the LHC will resolve some of the theoretical predictions made by him and others getting on for nearly 50 years ago. I know he at least respects experimental evidence.

20. **rhofmann**
July 26, 2011

Like Neil Harris I also would like to thank Peter Woit for his constant efforts in exposing the desolate state of a failed research program in high-energy particle theory in spite of humiliation and aggressive action by some of its followers. It takes a presently rare-to-find strength of character, intelligence, conviction, wit, and integrity to stand as solid as Peter did over the last seven or so years.

21. **chris**
July 26, 2011

i don't quite get it why Motls opinion is still discussed. he is out of the business for several years now and it shows in all his comments.

22. **Arun**
July 26, 2011

…resulting in the Superbus!
http://www.superbusproject.com/

23. **CM Theorist**
July 26, 2011

Firstly, Motls' opinion….I agree…who gives a crap. He's just a typical internet blowhard trying to maintain some reputation of expertise by constantly giving his un-asked-for opinion.

Regarding the coupling constants meeting at the GUT level? My problem with this has always been that one has to be confident in a linear extrapolation of many many many orders of magnitude. This sort of extrapolation almost never works. It is tantalizing…don't get me wrong…but it's completely absurd to take as some sort of evidence for supersymmetry.

24. **Bernhard**
July 26, 2011

Nature, on the Higgs:

http://www.nature.com/news/2011/110725/full/475434a.html?WT.ec_id=NEWS-20110726

25. **Peter Woit**
July 26, 2011

rhofmann,

Thanks, but being witty and staying amused by it all is made very easy by Lubos.

Actually I agree a bit with the point made by him and Bee that the emphasis on "phenomenology" in recent years is not necessarily healthy. The subject needs a good balance between phenomenology and formal theory. The dominance of formal theory by a failed research program, whose proponents refuse to admit this failure, is what has caused the rest of the community to lose patience. The multiverse nonsense and the spectacle of string-theory fanaticism that Lubos is the poster-boy for haven't helped. If you're going to work on ideas that aren't ready for experimental test, you have to be honest with yourself when they aren't working out.

26. **Phenner**
July 26, 2011

It should be pointed out that "phenomenology" is much wider than the caricature being portrayed here. The field involves a lot of SM (especially QCD) predictions, vital for separating new physics from backgrounds at the LHC. All the simulation programs – Pythia, Herwig, Sherpa etc – are written by phenomenologists. Deciding which observables can give the best chance to discover physics (i.e. where to look, what to plot, how to interpret it) is also part of this field. It's true there are a lot of papers, especially recently, which make spurious attempts to explain ~2sigma deviations in terms of fanciful models, but I don't see this as representative.

27. **Yousuf**
July 26, 2011

If SUSY, and by implication Superstring Theory are going down the toilet, then what's the next best theory down the line that needs to be falsified? Technicolor? Loop Quantum Gravity? Etc.?

28. **Peter Woit**
July 26, 2011

Yousuf,

Superstring theory can't really be falsified since it doesn't predict anything. In particular, it doesn't predict SUSY at LHC energies.

The LHC will have nothing to say about LQG. It should have something to say about technicolor, which won't survive if a Standard Model Higgs is found.

So far, the LHC is finding nothing that disagrees with the Standard Model, already falsifying various models of extra dimensions, low energy strings, and much much more. If this continues, ALL models that make predictions about LHC scale physics different than the SM will be falsified.

29. **abbyyorker**
July 26, 2011

Maybe a little too celebratory here. SUSY is an appealing theory for many reasons and I am personally sad that there's no evidence for it. It looks like LHC is decisively showing that the popular versions of SUSY are not correct in their details. But I secretly suspect that those versions were tilted towards those with low energy predictions, to take advantage of the presence of the LHC. So, as many have said, SUSY will never go away because it cannot be disproved, and un-disproved versions will likely be promoted and will sound good to many.
My guess is that people will soon become unable to do high energy physics, because of the expense, unless there is some astonishing breakthrough in the method.

30. **Yousuf**
July 26, 2011

Peter, if even a Standard Model Higgs isn't found amongst all of the noise of the range of energies that they are now looking for at LHC, then is the Higgs dead, or do they continue looking for it at ever higher energy ranges? Also if SM Higgs isn't found, then does Technicolor then automatically get promoted into the Standard Model in its place? I.e. if there's no Higgs then it's Technicolor, or if there is Higgs then there's no Technicolor.

31. **Justin Hilburn**
July 26, 2011

I found this post by Urs Schreiber at the n-category cafe on the difference between local and global supersymmetry to be very enlightening.

32. **KD**
July 26, 2011

Chris:

Is there empirical evidence that "the running coupling constants meet precisely at the GUT level"?

33. **chris**
July 27, 2011

if they meet they meet at the GUT scale, because that's how the GUT scale is defined.

34. **rhofmann**
July 27, 2011

Peter,

I completely agree with you in seeing the phenomenological industry as a necessary consequence of the hyping spear head of "deep" theory. Trying to make a connection to nature of "deep" theoretical concepts certainly was pleasing some of the formalists until it was universally realized how obviously embarrassing this industry became. (The story about black holes at LHC in connection with extra dimensions that developed its own life after being received by broader society, …)

Keep on pushing, Peter. And thank you again.

35. **Peter Woit**
July 27, 2011

Justin,

I think that counts Urs as another string theorist now throwing SUSY under the bus. He does make clear that what is at issue here is space-time SUSY, which is what the LHC can test. There's also world-sheet SUSY , SUSY in the 1+1 d world-sheet of the string, which goes back to the earliest days of SUSY. That's a different issue. He also makes the good point that there's SUSY QM, the 0+1 d case, where a form of SUSY show up in the simple theory of a spinning particle. That's a very important and deep idea, but it's not 4d space-time SUSY, which is what the LHC is looking for.

36. **Peter Woit**
July 27, 2011

abbyyorker,

There are always people willing to swallow hype about untestable ideas. I think they are a minority though among professional scientists, even if some of them get a lot of exposure in the media. If they deal with the LHC ruling out their favorite ideas by changing them to make them untestable, they're not going to have much credibility with their colleagues. Whether the popular media continues to give them prominent attention will be interesting to see.

37. **somebody**
July 27, 2011

Among the physics bloggers, Lubos is probably in the top one or two when it comes to his grasp of the subject matter. No two ways about it. That doesn't mean that his OPINIONS, even about physics, aren't crazy sometimes. What I always find amusing is when people -including him- say there is a 90% (or 50% or 0.001% or whatever) chance of finding SUSY. Either we find it or we don't. Percentage a-priori probablities are emotional nonsense.

I also don't understand what it means when people say the LHC is "ruling out" parameter space for SUSY. Do they mean the soft-breaking parameters of the MSSM? If that is the case, I think it is important to keep in mind that weak-scale SUSY is a much more general thing than our sociologicakl obsession with the minimal supersymmetric extension of the standard model. Weak scale SUSY is a nice idea – MSSM is just the simplest realization of it. Even weak scale SUSY could be wrong, but it is not going to be so easy to rule in/out SUSY without some serious time and hard work, it seems to me.

And finally, as Peter correctly points out, string theory does not make the prediction of weak-scale SUSY. It will be good to find susy somewhere because string theory is ultimately supersymmetric, but there is no reason why it should already be there at minscule energies like the weak scale.

38. **Technicolor**
July 27, 2011

I am also wondering about the technicolor question posted by Yousuf. If the Higgs and SUSY is not found is the only other theory technicolor (which I have heard is a very ugly theory)? What theories get promoted if we find nothing?

39. **manyoso**
July 27, 2011

Technicolor is not the only alternative to Higgs for explaining electroweak symmetry breaking: http://en.wikipedia.org/wiki/Higgsless_model

40. **Dan**
July 27, 2011

Peter, I disagree on technicolor. Recently a lot of composite Higgs models showed how difficult it is to distinguish some of these composite states from a Standard Model elementary Higgs.

41. **Shantanu**
July 27, 2011

so if no supersymmetry is found at LHC accessible energies are all proton decay
models ruled out or can one have proton decay but no supersymmetry?
shantanu

42. **multigoal**
July 27, 2011

Peter
Two questions: 1) What do we mean by SUSY being ruled out?… it's actually CMSSM that is getting ruled out, right?
2) What beyond standard model we can learn from LHC if they find 114-150 GeV Higgs and nothing else?

Multigoal

43. **Neg**
July 28, 2011

PW,
Would the data the LHC is collecting from now to the year's end be enough to either find or r/o higgs in the interesting region of 114-140? And SUSY?

I understand the LHC will power down to save on money. Then restart, again, but it will be another 3-5 years before it reaches design beam collision energy of 14 TEV?

What additional physics is the higher 7/beam 14 TEV is supposed to find over the original 3.5/7Tev?

Any word on when SuperCDMS and the Xenon100 dark matter detection ?

44. **Peter Woit**
July 28, 2011

Shantanu,

Sure, you get proton decay in non-SUSY GUTs. Actually, this is one of the arguments for SUSY: non-SUSY GUTs have lower unification scales, so higher proton decay rates, mostly already ruled out by experiment.

multigoal,

You can't completely rule SUSY out by experiment, since one can always find regions in its huge parameter space that are inaccessible to experiment. You can make it a lot less plausible by showing that the sorts of regions of parameter space intensively studied by theorists as the most likely place for it to be are ruled out. The CMSSM involves a particularly simple choice of parameters, but I don't know that moving away from that simple choice to a random generic one changes the conclusion (the LHC results would still rule it out). Expect to see lots of frantic back-pedaling by SUSY theorists over the coming days and months, saying that the analyses they published pre-LHC are now to be ignored, instead you should pay attention to their latest effort to find some SUSY models that aren't ruled out by the LHC.

The Higgs is the most dubious part of the SM, best bet for falsifying the SM is to either rule it out, or observe it and see that it behaves differently than expected. If the SM Higgs works perfectly, it may be possible there is no observable violation of the SM at LHC energies.

Neg.

I suspect this year's data may be enough to see the Higgs if it is there. If not, the 2012 data should do it. The exclusion of more and more SUSY models should continue as more analyses are done, but I don't think you'll see another big jump in energy scale of SUSY exclusions until the machine energy is raised. They're shutting down the machine in 2013/4 not to save money, but to fix the magnet interconnections. This is a huge job, they're a lot of them. It is supposed to come back on early 2015, presumably mid-late 2015 will see first higher energy results.

45. **Leonard Lerner**
July 28, 2011

I do not understand whar all this Higgs excitement is about. Most of the excess at 120-130 is driven by the WW channel, while the othetr channels show nothing extraordinaty in this range. Its a differential signal in all channels that would indicate a Higgs, an integral over the channels, where most of the excess comes from one of them is more likely a background error

46. **DGP**
July 28, 2011

While I might agree with "Somebody" that Lubos Motl is still reasonably on the ball as far as physics is concerned (without giving support to his fanaticism about strings and supersymmetry or endorsing his view that worries about the validity of renormalisation schemes are evidence of a weak mind), I observe that the physics postings in his blog now get very few comments compared to the global warming denial and general right-wing politics ones. It wasn't surprising that he has been accused of being a holocaust denier since he seems to have adopted most of the other baggage of the unsavoury wing of European right-wing politics. It looks like most physicists have given him up.

Does anyone know how he is funded now? I assumed he had taken an academic appointment in his native Czech Republic but that doesn't seem to be the case.

His view of phenomenologists is on a par with his other prejudices. One would hardly gather from his diatribe that HEP experiments could not proceed without good hands-dirty theoreticians to help design the experiments and compute the backgrounds. I have said before that I am awed by the difficulty of the background subtractions in modern experiments.

47. **DB**
July 28, 2011

@Leonard.
It's pretty straightforward. It's just competitive human nature. The Tevatron, about to be shut down, is engaged in the same sort of "I think I see a hint of something there" that LEP2 was up to just before they were shut down (to make way for the LHC). In response, the LHC has uncovered "hints" of their own.
The competition between American and European accelerator communities has been the very lifeblood of particle physics since the field's inception and is a very healthy thing. It's a great pity that the US will have effectively exited experimental particle physics once the Tevatron shuts down. Who's going to keep those pampered fois gras munching Europeans on their toes now? (I jest, I jest)
Unfortunately, naive, gullible journalists with little understanding of the field's history can't see this for what it is. A harmless outbreak of I-wanna-be-first-itis.

48. **Henry**
July 28, 2011

CMMSM or CMSSM?

49. **Peter Woit**
July 28, 2011

Henry,

Thanks, fixed. It's Constrained Minimal Supersymmetric Standard Model.

50. **Neg**
July 28, 2011

"I suspect this year's data may be enough to see the Higgs if it is there. "

About when should the results for the end of 2011 Higgs/SUSY LHC results be available? Dec 2011 or Jan 2012?
I'll mark it in my calendar.

51. **Peter Woit**
July 28, 2011

Neg,

The normal schedule would be to release results at one of the so-called "Winter Conferences" in early 2012. In this case though, if one or both of the experiments has a statistically significant Higgs signal, I find it hard to believe they'll keep it under wraps waiting for an appropriate conference date. Maybe someone will post the news to a blog…

52. **mtarifi**
July 28, 2011

Hello Peter. Just to make sure I understand: if "somehow" we can prove that super-symmetry is false, does this rule out String Theory?

53. **Peter Woit**
July 28, 2011

mtarifi,

You can't prove that SUSY is false, all you can do is show that certain versions of it that have been advertised as solving certain problems disagree with experiment. Similarly, you can't rule out string theory, it's compatible with just about anything. You can however, like SUSY, show that certain heavily advertised versions of string theory are falsified by experiment, and that's what's happening. Versions of string theory which have SUSY discoverable at LHC energies have always been the been string theorist's best bet for a version that has some observable consequences.

54. **Matt Strassler**
July 28, 2011

Dear Peter,

It isn't really appropriate to suggest that collider theorists are now backpedaling, and looking around for supersymmetric models that would evade current searches at the LHC, in some desperate effort to save supersymmetry. Many of us have been saying for a long time that the LHC experiments' reliance on a missing-energy-based search strategy for supersymmetry (and supersymmetry-like models) would lead to large classes of very reasonable models being initially undetectable. (There are papers going back at least to the 1990s and probably earlier.) Many of us are also on record since the 1990s saying that it would take many years for LHC data to convincingly disfavor supersymmetry.

The current news from the LHC is neither a disaster for supersymmetry, nor a shock to those who like supersymmetry, nor forcing theorists to backpedal. In fact, rather than backpedaling, what many theorists are actually doing is pulling out old work they did years ago, knowing this situation was likely, and making their old results more precise.

Just as an example, look at a 1999 paper by collider theorists Konstantin Matchev and Scott Thomas, on gauge mediated supersymmetry with decays of the next-to-lightest superpartner to a Z or a Higgs plus a gravitino. This OLD class of models, which has a large parameter space, would not easily be observed with the strategies shown at the EPS conference last week — the missing energy signature is simply too small. And this is just one of many examples.

best regards,

Matt Strassler

55. **Peter Woit**
July 28, 2011

Hi Matt,

Thanks for the comments and interesting added details about this. To an outsider, it certainly seemed that pre-LHC, the kind of signals that are now ruled out were ones that were being promoted as typical expectations. The idea that it was going to take many years to see if SUSY was there as expected wasn't something I remember seeing advertised. Instead, the typical kind of thing I remember hearing was Arkani-Hamed's remarks at Strings 2005:

"If weak scale supersymmetry is right, then we will know by spring 2008 [e.g. a few months after expected start of LHC physics]. I think this is really a very sharp statement."

From context, he was talking about roughly the sort of integrated luminosity now available. It's true he was expecting an extra factor of two in energy, and 1-2 TeV mass particles, so he could be right if the gluino is 1.5 TeV. He definitely wasn't saying anything though about it taking years to find a hard-to-see signal.

56. **Neg**
July 28, 2011

"The normal schedule would be to release results at one of the so-called "Winter Conferences" in early 2012. In this case though, if one or both of the experiments has a statistically significant Higgs signal, I find it hard to believe they'll keep it under wraps waiting for an appropriate conference date. Maybe someone will post the news to a blog…"

Based on LHC current luminosity, would the data in the Jan 2012 LHC would be able to either provide evidence (i.e 2-sigma) or rule out in the more interesting "Higgs events in the mass range 120-145 GeV"?

57. **Peter Woit**
July 28, 2011

Matt,

One more comment: pre-LHC I remember often hearing that the Higgs would be hard, SUSY much easier. Now we're almost at the point of seeing a Higgs or ruling it out…

Neg,

No one is sure how well the LHC will do the rest of the year, but typical estimates are that it will produce a factor of 3-5 more data than analyzed at this latest conference (and there should be further refinements and improvements in the analysis). By some measures, the hint of a Higgs signal already seen is already getting to be around 2 sigma, it doesn't seem unreasonable to expect a 3 sigma or more signal (depends on mass, much harder at lower masses).

58. **Peter Woit**
July 28, 2011

The Higgs Hunting 2011 conference is now going on, see this presentation

http://indico2.lal.in2p3.fr/indico/getFile.py/access?contribId=25&sessionId=10&resId=0&materialId=slides&confId=1507

for details on prospects for the near future and 2012. By my reading, if there's a 140 GeV Higgs, both CMS and ATLAS expect to see a 5 sigma signal (if they get 5 inverse femtobarns) by the end of the year.

59. **Thomas Larsson**
July 28, 2011

It might be sobering to recall some statements from Frank Wilczek's Future Summary from a decade ago, http://arxiv.org/abs/hep-ph/0101187 :

5.1. Small Effects Among Known Particles
[…] The modern experimental limits on deviations from the Standard Model in each of these processes puts very significant pressure on the supersymmetric parameter space already [i.e. 10 years ago].

5.2. Proton Decay
[…] but it will not be easy to reconcile limits tau_proton > 10^34 years with straightforward models.

5.5. Produce the New Particles!
Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new R-odd particles. Even in the focus point scenario, **there must be several accessible to the LHC.**

60. **Neg**

July 28, 2011

http://indico2.lal.in2p3.fr/indico/getFile.py/access?contribId=25&sessionId=10&resId=0&materialId=slides&confId=1507
page 9

I see that if the Higgs does NOT exist, then the 5 inverse femtobarns is enough data to exclude it even in the low-mass range. So apparently by Q1 2012, if the LHC collects 5 inverse femtobarns as expected, if the Higgs does NOT exist, the LHC 5 inverse femtobarns would be enough to exclude it over the 115-150 Gev range.

Is that a correct reading of page 9?

Do you think that the LHC with 5+ inverse femtobarns excluding the Higgs, (if it doesn't exist) even in the low mass range, would be like the Michelson–Morley experiment null result on the luminous aether?

61. **Peter Woit**
July 28, 2011

Neg,

That's also my reading of that document. If there is a 5 sigma exclusion of the SM Higgs late this year, that will have a huge impact on the field, maybe the Michelson-Morley null result would be a good analogy. I don't know if this was the case with Michelson-Morley, but here I think most theorists are rooting for the null result, which would be a lot more interesting than an observation of a Higgs with SM behavior.

62. **Neg**
July 28, 2011

There's no mention of technicolor in those slides. How does say 5 or 10 inverse femtobarns (at the end of 2012) data collection have to say about technicolor models?

63. **Peter Woit**
July 28, 2011

Neg,

I haven't seen any results from the LHC yet specifically about technicolor.

64. **Neg**
July 28, 2011

"Given the huge number of possibilities and parameters for how to implement SUSY, insisting that all of it gets tested by experiment will ensure that SUSY phenomenology will be with us for a very long time. Ideas like SUSY can never be completely ruled out, they can just be made so unlikely that they're not worth people's time anymore, and the argument over how much more unlikely the LHC results make SUSY will continue…"

If LHC does NOT find evidence of higgs or technicolor, and no evidence of SUSY, would SUSY still be viable? If the LHC rules out the Higgs, would that also rule out SUSY?

65. **Peter Woit**
July 28, 2011

Neg,

The question of SUSY and the question of electroweak symmetry breaking (Higg or no Higgs? Technicolor?) are pretty much independent questions.

As far as electroweak symmetry breaking goes, SUSY and the SM aren't very different. They both have electroweak breaking via a Higgs, only difference is that in the SUSY case you need at least two Higgs fields, unlike the SM, where you can get away with just one. If a Higgs is found, one big question will be whether it behaves like the SM Higgs, or like one of the two minimal supersymmetry Higgses.

66. **mtarifi**
July 28, 2011

Thank you, Peter, for taking the time to respond to my comment.

67. **Neg**
July 28, 2011

"As far as electroweak symmetry breaking goes, SUSY and the SM aren't very different. They both have electroweak breaking via a Higgs, only difference is that in the SUSY case you need at least two Higgs fields, unlike the SM, where you can get away with just one. If a Higgs is found, one big question will be whether it behaves like the SM Higgs, or like one of the two minimal supersymmetry Higgses."

But if come Jan 2012, the LHC rules out the higgs, it would also rule out two minimal supersymmetry Higgses. If it doesn't even find a single Higgs, much less 2. Or to put it another way, if the LHC rules out electroweak breaking via a Higgs, wouldn't it also falsify both the SM single higgs field, and the SUSY 2 Higgs fields at the same time?

thanks

68. **Leonard Lerner**
July 28, 2011

Thanks DB, its good to see someone agrees with me. I absolutely discount these claims of a 3sig signal already been observed. This is very misleading because it suggest theres a 95% chance we have seen the Higgs. In reality there data more conclusively excludes the Higgs than vice versa. Why? Because these combined channle statistics make no sense in light of what the LHC has been seeing.

Take a look at the way the WW background behaves over the energy spectrum. Our SM expectation (this is loose becuase a whole pile of assumtpions are needed to related the SM predictions to what ATLAS actually SHOULD see) has about a 2sig deviation from what ATLAS actually is seeing over the entire 100-170GeV energy range. Thats not suprising, because of the W mass the branching ratio is very low there. Yet almost the entire of the present 3sig from the Higgs comes from these handful of events.

In this light we really have to look at the three main channels separately. Lets list the events as: expected background, expected background + SM Higgs, observed. I will sum events over the expected Higgs width in each channel (you can see the width in the ATLAS histograms). We have

In the WW channel a strong signal for a 150GeV Higgs: 43back/64 back+H/64 observed. So the data is spot on with a SM Higgs, at a 3sig CL. BUT caution, over the rest of the WW spectrum 20 events are expected versus 29 observed, a 2sig devitation, and also positive.

In the gamma_gamma channel for a 150GeV Higgs: 31oback/320back+H/380 expected. So we would have expected almost a 4sig Higgs signal, but got almost nothing. The Higgs is fairly narrow in this channel (only about 5 GeV, see ATLAS plot). So it makes sense to separate the spectrum. If we repeat the analysis at 130GeV, we have 310/340/370, that is 2sig expected vs 1 sig observed.

In the H-bb channel at 130 GeV: 700/700/820. So a 0sig deviation vs 4sig expected. (I neglect the bb via ZH since its muich smaller than via WH)

I think the data speak more clearly for no Higgs than anything else, I would be glad if someone can show where the above is wrong.

From the ATLAS presentation, almost all luminosity came in the last month. So in 3 months we will have 4 times the integrated luminosity, and so all the sigmas above will be multiplied by 2, and if the above trnd continues, this will exclude the Higgs at something like 5sig.

69. **Leonard Lerner**
July 29, 2011

Im sorry for the spelling mistakes above. I should have checked it, I had too little time.

70. **andhor thjorjend**
July 29, 2011

When you refer to “black holes” as “exotica”, are you suggesting that black holes in general don’t exist? I had thought black holes’ existence was accepted as deriving from general relativity. Is this incorrect?

71. **Einstein's Bastard Son**
July 29, 2011

andhor thjorjend,
He’s referring to hypothetical micro black holes that are supposedly produced in high energy particle collisions, not to the astrophysical black holes of general relativity.

72. **Martin**
July 30, 2011

> And all the bogus claims for “tests of string theory” are my fault, since I created a hostile environment for string theorists where they felt they had to do this kind of thing.

So now it is official. Peter, you’re to blame for all these “string theory is testable” claims. You’ve been really mean to these guys! I hope your feeling sorry for poor Lubos and his pals.

73. **William Nelson**
August 3, 2011

One of the arguments string theorists have made over the decades is that string theory is the “only game in town” when it comes to unification. Certainly it would seem to have been the most *interesting* game, given that other approaches have less mathematical interest and have also not had support from experimental data. Do you think these new findings (or lack thereof) change this situation at all? In other words, do the new findings provide a compelling reason for a would-be high energy theorist to work on anything *besides* string theory?

News From Simons Center

July 28, 2011
Categories: Uncategorized

The Simons Center for Geometry and Physics at Stony Brook has a new web-site, and this week their annual summer workshop got underway, talks available in very high quality video here. Luca Mazzucato, a postdoc there, has started putting together an Outreach section of the web-site, which now includes some wonderful interviews with various theorists, often covering topics well-known on this blog. The last of the interviews looks the most intriguing, it promises "the formula of love". Unfortunately, when you try and get access to this intriguing formula, for now you find that it is password-protected…

I'll be spending much of the next two weeks on vacation in Scotland. Blogging may be light to non-existent.

Comments

1. **Fred Tucker**
   July 30, 2011

   There's a new bloggingheads.tv episode on String Theory. Perhaps you should watch it while unwinding after an aggressive day hiking in the rain-swept Scottish highlands?

2. **Peter Woit**
   July 30, 2011

   Thanks Fred,

   The weather in Edinburgh is unusually sunny, warm and pleasant. Noticed the Bloggingheads thing a little while ago. Both John Horgan and George Musser are well-informed on the string theory controversy, with John a skeptic and George much more pro-string theory. Worth watching if you want an introduction to the controversy, probably nothing much new if you've been reading this blog….

3. **Casey Leedom**
   August 2, 2011

   Or you should just enjoy the latest Abstruse Goose:

   http://abstrusegoose.com/384

4. **Shantanu**
   August 4, 2011

   Peter, here is something which is an intersting and am hoping you wil blog when youa re back.
   http://arxiv.org/pdf/1108.0868v1

5. **Peter Woit**
   August 13, 2011

   Shantanu,

   I don't have anything really new to say about that. Lee Smolin's article back in 2003

   http://arxiv.org/abs/hep-th/0303185

   seems to me to have done much the same thing, quite a bit more thoroughly, and I don't see that much has changed since then.

This Week’s Hype

August 5, 2011
Categories: Multiverse Mania, This Week's Hype

I noticed today that BBC News has a story headlined ‘Multiverse’ theory suggested by microwave background that assures us that:

> The idea that other universes – as well as our own – lie within “bubbles” of space and time has received a boost.

After taking a look at the PRL and PRD papers that are behind this, it’s clear that a more accurate title for the story would have been “‘Multiverse’ theory suggested by microwave background – NOT”. As usual, the source of the problem here is a misleading university press release, one from University College London entitled First observational test of the ‘multiverse’. Somehow the press release neglected to mention something one might think was an important detail, the fact that this “First observational test” had a null result.

It’s well-known that one can find Stephen Hawking’s initials, and just about any other pattern one can think of somewhere in the CMB data. The authors of the PRL and PRD papers first put out preprints last December (see here and here). In these preprints they essentially claimed to have found four specific features in the CMB where the hypothesis that they were due to bubble collisions was statistically preferred. A guest post by Matthew Johnson at Cosmic Variance explained more about the preprints. I didn’t understand their statistical measure, so asked about it in the comment section, where Matthew explained that, by more conventional measure, the statistical significance was “near 3 sigma“.

It turns out that the PRL and PRD papers differ significantly from the preprint versions. In the acknowledgements section of the PRD paper we read that:

> A preprint version of this paper presented only evidence ratios confined to patches. We thank an anonymous referee who encouraged us to develop this algorithm into a full-sky formalism.

and the result of the new analysis asked for by the referee is summarized in the conclusion of the paper:

> The posterior evaluated using the WMAP 7-year data is maximized at $N_s = 0$, and constrains $N_s < 1.6$ at 68% confidence. We therefore conclude that this data set does not favor the bubble collision hypothesis for any value of N_s. In light of this null detection, comparing with the simulated bubble collisions...

So, the bottom line is that they see nothing, but a press release has been issued about how wonderful it is that they have looked for evidence of a Multiverse, without mentioning that they found nothing. As one would expect, this kind of behavior leads to BBC stories about how the Multiverse has “received a boost”, exactly the opposite of what the scientific evidence shows.

Update: The FQXI web-site has an article about this. In it, the authors seem far more interested in promoting their PRL paper as “first test of the multiverse” than in acknowledging that a referee made them do a better test of the idea and they got a null result. There’s no mention of the null result in the article.

Update: News stories based on this keep on coming. The latest: Proof of a multiverse discovered?

Comments

1. **Daniel L. Burnstein**
   August 5, 2011

   After following your blog for the last several months, I think it will soon be time for a new book, Peter (I read the first one). 2012, when the Higgs and other “predictions” have been ruled out would be a good timing for an update on the state of theoretical physics. I’d buy that book. You working on it?

2. **neo**
   August 5, 2011

   University press releases are always suspect If my institution is any example, such offices are paid to make the big splash, putting them one step below the tabloids.

3. **neo**
   August 5, 2011

   That would be above the tabloids. I think.

4. **Bee**
   August 6, 2011

   I also wrote about the Feeney et al paper here.

5. **Peter Woit**
   August 6, 2011

   Daniel,

   No plans for a new book. So far, the situation described in the old one hasn’t much changed (except that string theory has become a lot less popular). I hope new physics worth writing a book about comes out of the LHC, but if so there likely will be many people doing a better job than me of writing such a book.

6. **Giotis**
   August 6, 2011

   “…exactly the opposite of what the scientific evidence shows.”

   Scientific evidence?? Peter until now your mantra was that the multiverse hypothesis is pseudo science because it can’t be tested; now you are saying that it can? Then surely is not pseudo science…

7. **Davide Castelvecchi**
   August 6, 2011

   Peter:

I agree that over-hyped press releases are a big problem, compounded by the fact that certain web sites aggregate press releases making them look like they are journalistic reports, as you have pointed out in the past. Do you agree, though, that some universities have more reliable press offices than others? And ultimately, when a supposedly reliable publication covers something, it's that publication's responsibility to see the science (if there is any) through the hype.

8. **kyrilluk**
August 7, 2011

@Giotis: The fact that they tested the idea of the existence of Multiverse is not a proof that this idea is a valid scientific theory. It would have been a valid scientific theory and I'm sure that Peter would have been happy to recognized that he was wrong, if after testing their theory and finding nothing, the scientist would have conceded that their theory had been refuted.
The issue with the Multiverse ideas is that they can't be refuted. We cannot create a test that can disprove such theories. There's always going to be way of saving this theory.

9. **martibal**
August 7, 2011

Peter:
a blog technical question about the pingback
According to wikipedia, a pingback is added to your blog because this other blog on Intelligent Design has quoted your post. But does it require your approval ? (i.e. do you think that ID is an interesting answer to multiverse ?).

10. **John Baez**
August 7, 2011

Someone should start a blog or wiki or weekly column all about university press releases: the good, the bad and the ugly. It might eventually exert some discipline on the people who write those releases, if word got back to them.

11. **Peter Woit**
August 7, 2011

Davide and John,

I suppose the quality of university press offices varies, but the amount of hype they put out depends largely on what researchers are willing to feed them. A lot of these over-hyped press releases are the fault of PRL, which evidently has a policy of encouraging press releases when they publish papers. I find it hard to believe that many university press offices are issuing press releases over the objections of the researchers involved, or without letting them see the text of the release and vet it before it goes out. Few faculty that I know would tolerate their institution issuing without their approval a press release that they felt misrepresented their research.

If you look at the collected "This Week's Hype" postings about university press releases I think there's a pattern. They're mostly about how someone at the university has made progress towards solving the problem of some well-known speculative theory being untestable. There's every reason to believe that the researchers themselves think they have done this, and are on-board with the idea of a press release promoting their work. These press releases are often carefully worded with crucial caveats placed so as to be technically accurate, but highly misleading in that they don't mention problems with the claims being made. When various news sources pick them up, it's not surprising they often take them at face value, are successfully misled, and miss the significance of the caveats. Good news organizations aren't fooled, do their own checking, and then decide to ignore the misleading press release, weaker ones are fooled.

You can blame the news organizations for getting fooled, or the press offices for spreading hype about topics they don't really understand, but I think the most significant blame should go to the researchers themselves, who are either misrepresenting their own research or standing by and letting their employers do so.

12. **Peter Woit**
August 7, 2011

martibal,

pingbacks appear here automatically, without needing to be approved by me. If they're spam, or from someone trying to introduce links to off-topic material here, I delete them when I get a chance. If they are legitimate postings of some kind that happen to refer to something here, I leave them, no matter what I think of the posting.

I happen to think that ID is worthless pseudo-science, and also that arguing with people over anything related to religion is a waste of my time and energy. One of the worst aspects of multiverse pseudo-science is that it delegitimizes real science, the best tool available for fighting irrationality. I'm glad that some people are willing to spend time making the argument against ID, mystified that some of them think it's a good idea to sign on to multiverse pseudo-science as part of this argument. This seems to me ultimately self-destructive behavior. You can go ahead and say that the string theory anthropic multiverse deals with the argument from design for the existence of a deity, but you're then stuck without a legitimate answer when IDers point out to you that their "science" is as good or better than yours.

13. **Peter Woit**
August 7, 2011

Giotis,

Kyrilluk does a good job of explaining the point: there may very well be a multiverse, but current heavily advertised multiverse theories are often inherently unfalsifiable. If you have a theory compatible with almost everything, you can't ever show it is wrong.

The authors here did a careful job of measuring something and achieved a real scientific result: they looked for evidence in the CMB of effects of certain kinds of bubble collisions and showed that no statistically significant such effects are visible in the WMAP data. Then, for unclear reasons, they allowed a press release to be issued implying that they had figured out how to "test" multiverse theories, but not mentioning that the "test" had failed. Perhaps mentioning that the "test" had led to a null result would have made it necessary to explain that this wasn't the sort of "test" of multiverse theory that one might think it was, since failing the "test" wasn't a problem for the theory.

To be clear: I don't think studying models of the early universe involving bubble collisions is pseudo-science. You can make predictive, scientific such models, although I don't personally think you're ever going to find evidence for them. You're going to get null results like in this work. What is pseudo-science is the use of the multiverse as an excuse for the failure of string theory unification. It is the heavily overhyped and oversold string theory landscape multiverse framework that is inherently unpredictive, untestable and unscientific. If you want to choose specific cosmological models with a multiverse and examine their observational implications, that's not inherently unpredictive, untestable or unscientific. So far though, there's no evidence for such models, so such work is highly speculative and likely to lead to nothing but null results, a good reason not to issue press releases about it.

14. **martibal**

August 8, 2011

Peter: thanks for the explanation.

15. **Emile**
August 8, 2011

A propos of delegitimizing real science: here's a comment that Kenneth Miller, a biologist from Brown U. who makes the case in his book that science and religion are compatible:

"Believers … are right to remind skeptics and agnostics that one of their favored explanations for the nature of our existence involves an element of the imagination as wild as any tale in a sacred book: namely, the existence of countless parallel simultaneous universes with which we can never communicate and whose existence we cannot even test. Such belief also requires an extraordinary level of "faith" and the nonreligious would do well to admit as much."

Hard to argue with the logic. The premise though is that theories "…whose existence we cannot even test." is science…

16. **Kenneth Cohen**
August 8, 2011

When these papers refer to a collision between universes, in what space are the universes thought to be moving and colliding? And with what laws of motion?

17. **Bernhard**
August 9, 2011

Here comments from "Physics":

http://physics.aps.org/synopsis-for/10.1103/PhysRevLett.107.071301

18. **Anon**
August 9, 2011

A bit off topic, for sure, but I am surely not the only one who bemoans the effect the multiverse ideas have on science fiction. True, they were writing multiverse stories long before the current popular mania, but now it is out of control. If I have to read yet another friggin' intellectually lazy multiverse story, I might just go mad.

19. **chris**
August 10, 2011

so much about the obsolescence and inadequacy of the refereeing process…

20. **Anon**
August 12, 2011

Here is another popular article on this same issue:

"Weird! Our Universe May Be a 'Multiverse,' Scientists Say"

http://old.news.yahoo.com/s/livescience/20110812/sc_livescience/weirdouruniversemaybeamultiversescientistssay

The way these people misrepresent their own work is outrageous.

Talks at the KITP

August 12, 2011
Categories: Langlands, Uncategorized

Back now from vacation, and found that there have been quite a few interesting talks at the KITP in Santa Barbara this week which are now available on-line:

- Since the EPS-HEP conference last month, the "First Year of the LHC" program has some interesting results to discuss. Yesterday Matt Reece gave a talk on Assessing SUSY after 1 fb $^{-1}$, on the hot topic of how worried SUSY proponents should be that no sign of SUSY has been found at the LHC so far. He takes the point of view that the failure of direct collider searches to see anything is much less of a problem than the pre-LHC failure of SUSY to show up indirectly in flavor physics or in cosmology. While it's true that SUSY was in trouble pre-LHC, there's psychologically a big difference between indirect effects not showing up, and directly looking for something and finding it's just not there. The discussion with the audience is quite interesting, with some audience members a lot more worried about SUSY. One of them reminded people that SUSY is supposed to solve naturality problems, so relatively light squarks were expected, but now "those models are being screwed." Someone else (Lisa Randall, I think) reacted to Reece's mentioning R-parity violating models as one way to evade the LHC limits with "Is there any good reason to think about R-parity violation?" All in all, the discussion gives a good indication of what prominent theorists are thinking now that the initial results from the LHC are in.

About a year ago on this blog, I had the following exchange with a well-informed phenomenologist on this blog:

> *If there's no sign of supersymmetry in this year's LHC data, how discouraging will this be for those who expect to see supersymmetry at this energy scale?*
>
> In 2010 data? Not discouraging at all. In 2011 data? Fairly discouraging. In 2014 data? Enormously depressing.

The LHC has now gathered as much data as expected for all of 2011, so I think that with the negative results, "fairly discouraged" is where SUSY proponents would have expected to be and are now. "Enormously depressed" is on the agenda for late 2014, early 2015, after the LHC reaches design energy.

- Adam Falkowski, the Jester of Resonaances fame, also gave an interesting talk this week, on Higgsless theories.
- On the mathematical end of things, Ed Frenkel gave a very nice expository "Blackboard Lunch" talk on What do Fermat's Last Theorem and Electro-magnetic Duality Have in Common?, explaining to physicists a bit about the Langlands program and the connection between geometric Langlands and QFT pioneered by Witten and developed by him and others over the past few years. For something more technical with newer ideas about the relationships between TQFT, gauge theory and representation, see David Ben-Zvi's talk on Geometric Character Theory.

Comments

1. **Eric**
   August 12, 2011

   Regarding R-parity violation, I think the important thing that must be kept in mind that this is only introduced to solve the problem of rapid proton decay through dimension-4 operators in the MSSM. However, if the MSSM is extended such that baryon and lepton number conservation result from local gauge symmetries, then R-parity really isn't necessary. So, in such a scenario, there is no problem with R-parity violation, which makes detecting superpartners much more difficult.

2. **Shantanu**
   August 13, 2011

   Peter, are people worried about finding no evidence for extra dimensions (thinks like Randall-Sundrum models etc)?

3. **Peter Woit**
   August 13, 2011

   Shantanu,

   I don't think anyone seriously thought the LHC would find evidence for extra dimensions. This was always just a talking point for the media, and a class of models that some people found interesting to study for various reasons, but not because there was any serious reason to believe they would actually show up at the LHC.

   Supersymmetry is different. In that case, quite a few people did expect it to be seen at the LHC.

4. **Bernhard**
   August 13, 2011

   Peter,
   what do you think will happen to string theory if supersymmetry does not show up at the LHC at all? I know what string theorists are going to say, i.e., string theory never predicted it would, etc, but I wonder what will happen in terms of their funding, in terms of presence in the media, respect from the rest of the community. I cannot believe (or I don´t want to) that the strange situation we have now, that a theory with no predictions will go on receiving so much attention. Do you agree?

5. **Peter Woit**
   August 13, 2011

   Bernhard,

   I think string theory in the last few years has already been seriously affected along the lines you ask about, partly because of its failure to make any real LHC predictions. It has been clear for a while to most people in HEP that string theory is a failure at saying anything at all about TeV scale physics, and that has had an impact. Finding supersymmetry was kind of the last hope along those lines, and no supersymmetry will just continue current trends.

   One thing I've learned is that it's just about impossible to get people to give up on an idea once they have a certain amount invested in it. Many string theorists will go to their graves insisting string unification is a great idea, not a failure, and they'll be prominent in the media, as well as having influence on funding. Their colleagues will just take them less and less seriously. Instead of being "cutting edge" they will be seen as has-beens.

   The situation of supersymmetry itself may be a more dramatic story. There's a large community of people who have invested 20-30 year careers in TeV-scale supersymmetry phenomenology, and by 2015 or so, that may be a really dead subject. How they will deal with this will be interesting, it's already kind of fascinating to see how this is starting to play out.

6. **Clara**

August 16, 2011

Peter,

is there any recent news on what Ed Witten is thinking, now that Susy looks so bad?

7. **gradstudent**
   August 16, 2011

   I have a general (factual) question, coming from a math student who doesn't know modern physics, but is trying to follow. When you and others say string theory makes no predictions, does this mean more precisely that, "it does predict things like supersymmetry, but leaves open a wide (perhaps infinite?) range of energies at which the first SUSY phenomenon would be detectable, which in particular means it can never be absolutely invalidated by a (finite) number of experiments?"
   ,
   Thanks! I've always been confused about this point. And another similarly fundamental and ignorant question: what do people mean by the "landscape"? Is it related to the above point about an infinite range of predictions?

8. **Peter Woit**
   August 16, 2011

   Clara,

   I don't know what Witten thinks about the latest discouraging news for SUSY. In the past he has given talks about SUSY that acknowledged that SUSY effects should have already showed up, at least indirectly, so it was not at all guaranteed to show up at the LHC (this is somewhat the same at what Matt Reece starts off by pointing out).

   gradstudent,

   The problem with supersymmetry has always been that it has to be a broken symmetry, and the scale of the breaking is undetermined. There have been arguments (the "hierarchy problem") made that the scale should be around the electroweak breaking scale (100 GeV), and that is what is now being ruled out by experiment. In string theory, normally you need supersymmetry, but it predicts nothing about its breaking (or about much of anything else..). By the way, I wrote a book about this, where things are explained carefully, in a way that should be accessible to math students…

9. **Dave**
   August 17, 2011

   So with string theory being thrown under the bus, what is the new theory of everything? I've heard LQG has even more problems and is also not a leading contender.

10. **Peter Woit**
    August 17, 2011

    Dave,

    It's SUSY that string theorists are throwing under the bus. It may be a while before string theorists throw themselves (i.e. string theory) under the bus, although that might happen. LQG is not something that has ever had a way of giving a unified theory.

    As far as unification goes, I think the situation is that it's pretty clear right now that no one has a good idea. Not an unusual circumstance in face of a hard problem.

11. **HoldYourHorses**
    August 17, 2011

    It's worth pointing out that TeV-scale SUSY has not *yet* been ruled out. It has not appeared in the most obvious places, and for sure my degree of belief is rather less now than it was 6 months ago, but let's not react prematurely.

    Peter, if it is discovered, which I consider possible but not probable, you're going to have some back-tracking to do…

The Fabric of the Cosmos on PBS

August 15, 2011
Categories: Uncategorized

A four-part NOVA series based upon Brian Greene's The Fabric of the Cosmos is coming to PBS this fall, starting November 2. In some sense this is a follow-on to his wildly successful The Elegant Universe NOVA series from 2003, which was largely devoted to promoting string theory. From the program description and preview it appears that the new shows don't emphasize string theory, although the fourth of the series promotes the Multiverse (Clifford Johnson joins the effort here), along the lines of Brian's latest book The Hidden Reality.

Comments

1. **Roger**
   August 15, 2011

   The episodes are: (1) microscopic forces can generate whole universes, our universe is just a hologram, (2) wormholes, time travel, (3) goofy speculations about the meaning of quantum mechanics, (4) multiverse fantasies. I think that I'd rather watch Michio Kaku on the Discovery Channel.

2. **Mel B.**
   August 15, 2011

   Dear Prof Woit,

   What portion of Brian Greene's work is actually based on testable and falsifiable science? Are his books based on religion-like, unprovable fantasy concepts?

   Thank you

   Mel

3. **Peter Woit**
   August 15, 2011

   Mel B.,

   Much of theoretical physics research has always been not testable/falsifiable. The point is that when you start investigating a new idea, you typically don't understand it well enough to know exactly what its implications are. You can't just say "since this is not testable, it's not science", when people are still struggling to see if they can come up with a test. That struggle is a legitimate part of science. The real question is whether they are getting anywhere: are they making any progress towards coming up with a test, or is it looking increasingly unlikely that this is possible?

   I haven't seen the new programs, but I'd guess that they're very much a mix of solid, tested science, together with a range of speculative ideas, including some that are extremely unlikely to lead anywhere. As long as one makes clear what is solid and what is speculative, with some clear indication when one has entered the realm of the "wouldn't this be cool, even if it's very unlikely", I don't see a problem.

   Personally I thought "The Elegant Universe" programs didn't do enough to make clear how speculative some of the ideas there were, because many theorists were very optimistic about them at the time. In retrospect a decade later I suspect that some things in that program might be much more carefully hedged if redone today. There's a similar over-enthusiasm these days among some people for the idea of a string-theory based multiverse. I hope the new program does better in making clear that there are good reasons for skepticism about this.

4. **Roger**
   August 15, 2011

   No part of Greene's work is testable. His last TV show started with him saying, "One thing that is certain is that string theory is already showing us that the universe may be a lot stranger than any of us ever imagined." No, string theory has not shown anything about the universe. Greene occasionally says that experimental confirmation would be a good thing, but he makes reckless and untestable statements anyway.

5. **Peter Woit**
   August 15, 2011

   Roger,

   "string theory is already showing us that the universe MAY be a lot stranger…"

   Note the carefully placed caveat. Unfortunately I think these often don't get noticed by the viewer since they're not much emphasized, but they are there.

6. **Bernhard**
   August 15, 2011

   Peter,

   I see simply no reason to make such documentaries, they´re a disservice to making the general public about science. The caveats you mention, are noticed by nobody and the public impression is not that their talking about established accepted science. A documentary to talk about speculations in HEP could be even made, but the point of it should to inform the public of some of the really many scenarios thought by theorists. String theory could of course be there with the multiverse hand by hand, provided it was explained this is something less and less taken seriously by the scientific community.

7. **Bernhard**
   August 15, 2011

   sorry, *they´re a disservice to making the general public aware about science.*

8. **Jason**
   August 15, 2011

Brian Green – he is a good writer.
He can deliver those ideas of particle physics to a large audience.
Much like Martin Gardner, also a good writer, and the author of this blog.
I believe good writers can bring much enthusiasm to a field of interest, whether
or not some of the ideas in that field are still questionable.
To bring the story up front, to generalize the technical details.
Not even wrong
Would you agree?

9. **Peter Woit**
August 15, 2011

Bernhard and Jason,

It's by now an old argument about whether these kinds of enthusiastic and potentially misleading promotions of highly speculative theoretical ideas are a good thing because they get people interested in physics, or a bad thing because they misinform people about physics. I don't know of any data quantifying the effect one way or another.

One can say though that this kind of TV program has been popular for quite a while now, and I don't see any evidence that they have inspired a huge cohort of brilliant young theorists into getting physics Ph.D.s and revolutionizing the field.

10. **Casey Leedom**
August 16, 2011

Hhmmm, I don't know. I think that it would be hard to find people who would argue against the value of Carl Sagen's "Cosmos" series in 1980. It was absolutely a popularization of physics (general, not HEP) and it absolutely had to cut corners (in terms of details) for the general audience. But it did a great job explaining science. And I'll bet there are a bunch of people who ended up in science because of that series.

The point is: it *is* possible to generalize science for popular consumption and not have to tell fairy tales.

11. **John Baez**
August 16, 2011

If people wanted to popularize physics that actually matters, they could talk about climate physics, starting with the laws of thermodynamics, the laws of radiation, atmospheric physics, the theory of ice ages and other climate cycles, and other basics. There are lots of people who know nothing of this, and are easily fooled by any second-rate "climate skeptic" who comes along. Meanwhile physicists are trying to teach them about wormholes, time travel and the multiverse. You can't say we don't deserve what's coming.

12. **Peter Woit**
August 16, 2011

John Baez makes a good point about the nature of science programs on TV, but as I feared, his comment immediately led to a host of comments from people who want to engage in ideological debate about global warming here. Don't even think of it, not on this blog. The internet is full of plenty of other places for that (including John's blog if you want to argue with him…).

13. **chris**
August 16, 2011

The physics behind electric energy generation would be another thing that a lot of people would hugely profit from i guess. The whole nuclear vs. renewable vs. fossil energy debate could use some good physics input.

14. **Trulo**
August 16, 2011

I think a good program about the LHC, and its first year of physics results would be terribly interesting to me. And it would certainly be popularizing real-world physics.

But I guess it'd be way too much to ask for. These days, even particle physics blogs barely mention it.

15. **Giotis**
August 16, 2011

I don't agree with some of the comments here regarding Brian Greene. Brian Greene desperately wants to connect String theory to the "real world". His research shows that; from Calabi Yau compactifications during the early Heterotic era to topological transitions, String cosmology and now transitions between flux vacua.

Initially he hoped that our world could be derived from String theory in a unique way and my understanding is that he is not very happy with the multiverse but since the theory points to that direction he feels (due to his strong belief that the theory deep down is correct) obliged to examine this possibility seriously.

16. **M**
August 16, 2011

actually, there is some TV here at CERN.

17. **Bernhard**
August 16, 2011

Giotis,

Greene and others tried to connect string theory to the "real world", but he and everybody who tried failed miserably. Furthermore, it is clear that they are still not going anywhere and the multiverse, landscape and other things are not really much a direction, but simply they giving up. Connection with the "real world" would be to make a prediction for the LHC. No, I take it back, it would be to make any falsifiable prediction at all.

18. **Jason**
August 16, 2011

All I'm suggesting is it takes a lot of editing power to successfully encompass a large subject, such as the study and history of physics, into a smooth few hundred pages while keeping it original in perspective. I'm not talking about marketing a theory, I'm talking about telling a story from begining to end. Asimov was the master.

19. **Anon**
 August 16, 2011

 Carl Sagan was indeed a great popularizer who wasn't afraid of addressing controversial topics such as the nuclear arms race in his series. We need more people like him.

 As for Green, I bought his new book and if that is what this program is going to be talking about, I don't have high hopes. His book is poorly written, and full of rather ridiculous claims such as the statement that, if the universe is infinite in extent, there must be exact copies of us far away. This is as absurd as saying, for example, that any infinite sequence of integers /must/ contain the number 42 more than once.

20. **Mitchell Porter**
 August 16, 2011

 Anon said

 "His book is poorly written, and full of rather ridiculous claims such as the statement that, if the universe is infinite in extent, there must be exact copies of us far away. This is as absurd as saying, for example, that any infinite sequence of integers /must/ contain the number 42 more than once."

 No it's not, because Greene would be assuming standard ideas about how physics works. It would require some unusual hypotheses about initial conditions and/or dynamics for such copies to *not* exist in an infinite universe.

21. **MP**
 August 17, 2011

 I think these programs are great. As a layman myself who doesn't grasp the complex mathematics required to truly understand these theories I love to read books, blogs, etc and watch shows that bring the complexity down a notch. I have my own opinions on what theories I think are a waste of time; what is insulting to me is that there are (educated) people who believe that the people who pay for this would not be interested in the results or capable of separating speculative from hard science.

 Bernhard, saying that the unwashed masses should remain unwashed has historically never worked. I am astonished to read comments like you made.

22. **Peter Woit**
 August 17, 2011

 MP,

 I don't think Bernhard is calling for keeping the public unenlightened about science, but rather for providing them with information about science that will be enlightening rather than just dazzling. You may be able to separate speculative from hard science, but there's a real danger that these programs give people completely misleading ideas about science, while doing little to explain what the real thing is.

23. **D R Lunsford**
 August 17, 2011

 John Baez made the essential point above, that there is a great deal of hard science involved in the climate change scenario and that would be a lot more interesting to people than endless, pointless speculations about ephemera.

 There is hard data that this sort of "fuzzics" is turning people off. Many media portals, newspaper sites etc. don't even bother to have a dedicated science section -it gets lumped under some amorphous "tech" section. Why? There is just no interest in the same old crowing about how many dimensions are curled up into cosmic dust bunnies.

 -drl

24. **Anon**
 August 17, 2011

 Mitchell Porter, I don't think any of us know what the hell we are talking about when we discuss the initial conditions of the universe. In fact, "standard ideas about physics" are at still pretty much at a loss in this regard, given our current ignorance regarding quantum gravity. Even in the absence of gravity, the existence of copies is not a solid mathematical prediction of any quantum field theory in infinite space as far as I know, unless you posit some /very/ special initial conditions. If you have references to the contrary , I'd be all ears. So to claim the existence of infinite numbers of copies, as he does, is science fiction dressed up as fact.

 I am so tired of my doctor, my brother in law, my freshman students, and others, wanting to talk to me about some nonsense from his books because I am a physicist, and having to be the bad guy who shatters their illusions. I am just tired of it.

25. **Mitchell Porter**
 August 18, 2011

 Anon:

 "the existence of copies is not a solid mathematical prediction of any quantum field theory in infinite space as far as I know, unless you posit some /very/ special initial conditions. "

 Let's consider, for the purposes of argument, that we are talking about our solar system within the orbit of Pluto. Now consider the whole past light-cone of that region, back to the initial conditions, however you choose to think of them. The important point is that the cosmological initial conditions will be defined across an infinite spatial volume, for a universe that is spatially infinite later on. But the portion of the initial conditions relevant for our existence are only going to take up a finite part of these infinitely extended cosmological initial conditions.

 So we know that our own existence is an outcome of finite probability, given the existence of a finite initial region in a particular initial state. But the full cosmological initial conditions are spatially infinite, therefore contain infinitely many finite regions. If the local initial conditions that led to us are repeated infinitely many times across the overall initial conditions, then copies of us should be realized infinitely many times, because our existence is a finite-probability outcome for any one of those duplicates of the local initial conditions. So to avoid this outcome, you have to postulate that the local initial conditions which gave rise to us occur only finitely many times throughout the whole infinity of the cosmological initial conditions.

 As I said, it requires an unusual hypothesis about initial conditions or about dynamics to avoid Greene's conclusion.

26. **Luke**
August 18, 2011

I have to agree with a point John Baez made and a point I've heard Feynman make in a video of his. I'd much rather, being a mathematical physicist, watch a scientific documentary on thermodynamics (say global warming) or perhaps some interesting optical phenomena. Heck, even a documentary on some fascinating results from fluid mechanics would be interesting because you have all sorts of cool demonstrations and whatnot to show. Heck, I'd say that those branches of physics are more popular then quantum gravity in terms of thought and effort put into them. But people don't want to see or hear that. They want to learn about quantum gravity and hear about cool things like time travel or wormholes. It's very unfortunate.

27. **DB**
August 18, 2011

Peter, you might be interested in the following as an illustration of the seige mentality which often accompanies the cult-like behaviour of those caught up in the multiverse idea:

http://www.science20.com/alpha_meme/internet_study_guide_nonlocal_quantum_physics_and_solution_einstein_podolsky_rosen_paradox-81760

After announcing that "The proof against local realism is humankind's the (sic) most relevant finding in the realm of science and philosophy" the author assures us "I will ensure the comment thread here is going to be helpful for readers, so pseudo-scientists and religious anti-multiversers please stay away or get deleted."

So no criticism allowed while he tries to explain his non peer-reviewed paper for the benefit of gullible undergraduates.

I wouldn't mind but this guy holds down a postdoc in a large university.

28. **Peter Woit**
August 18, 2011

DB,

I think that author is overly optimistic about the level of anyone's interest in that kind of philosophical discussion of of "realism" and the many-worlds interpretation of QM. To get "religious anti-multiversers" to overwhelm his blog comment section I think he's going to have to expand his interests from QM to cosmology and TOEs.

29. **Anon**
August 20, 2011

"**If the local initial conditions that led to us are repeated infinitely many times** across the overall initial conditions, then copies of us should be realized infinitely many times, …

As I said, it requires an unusual hypothesis about initial conditions or about dynamics to avoid Greene's conclusion."

Aren't you *assuming* an unusual hypothesis about initial conditions in your argument (namely, the part I bolded)?

30. **srp**
August 21, 2011

I read The Fabric of the Cosmos, and it is primarily about non-speculative ideas in physics, such as special and general relativity and quantum mechanics, along with mainstream cosmological theory. Greene is pretty careful to say when something is speculative or that others disagree with his opinions (often giving a citation so the reader can check it out). The one thing in there that was new to me was a thermodynamic argument for the Big Bang–that without the whole universe starting out in a lower-entropy state, reversible micro-physical laws force us into paradoxical conclusions about the world-lines of melting ice cubes. I'd be curious to see if that rather subtle argument makes it into the show.

31. **Mitchell Porter**
August 22, 2011

Anon: "Aren't you assuming an unusual hypothesis about initial conditions in your argument"

No; other possibilities would be of measure zero in the set of initial conditions.

That is not a *refutation* of the idea of unusual cosmological initial conditions, but it means that you need some new physical principle or other argument that specifies the unusual starting point you have in mind.

Does String Theory Predict Low Energy Supersymmetry?

August 16, 2011
Categories: Uncategorized

It used to be that string theorists would respond to arguments that string theory predicted nothing with the claim that it predicted supersymmetry. For example, in an interview with Witten done for the PBS *Elegant Universe* series, one sees:

> **NOVA:** It seems like the standard criticism of string theory is that it isn't testable. How do you respond to that criticism?
>
> **Witten:** One very important aspect of string theory is definitely testable. That was the prediction of supersymmetry, which emerged from string theory in the early '70s. Experimentalists are still trying to test it. It hasn't been proved that supersymmetry is right. But there is a very precise relationship among the interaction rates of different kinds of particles which follows from supersymmetry and which has been tested successfully. Because of that and a variety of other clues, many physicists do suspect that our present decade is the decade when supersymmetry will be discovered. Supersymmetry is a very big prediction; it would be interesting to delve into history and try to see any theory that ever made as big a prediction as that.

Of course the problem with this was always that supersymmetry had to be broken somehow, and string theory said nothing about how to break it, not even the scale of the breaking. Back in 2004 when the anthropic landscape business began, Susskind was enthusiastic about the idea that it could be used to predict the scale of supersymmetry breaking, and Michael Douglas started working on computations counting string vacua that were supposed to say something about this (I've followed this story in several blog postings, an early one was here). The bottom line quickly became clear: a host of problems make this impossible, string theory remains incapable of predicting anything about this.

Today at the Simons Center, Douglas gave a talk entitled *Does String Theory Predict Low Energy Supersymmetry?* (video available here), and not surprisingly the conclusion is still that string theory predicts nothing about this. Amusingly, someone in the audience took exception to Douglas saying that string theory doesn't now make predictions, and one gets to hear Douglas try and explain to his fellow string theorist what a real prediction is. The video quality is great, but the sound doesn't work so well when two people are loudly trying to talk over each other.

This particular talk was held indoors, for a report on what the outdoor ones have been like, see here.

Comments

1. **Anon**
   August 16, 2011

   I am surprised that someone of Witten's calibre would commit this elementary logical error so publicly. To claim, as Witten did, that the hunt for experimental supersymmetry is a "test of string theory" is an example of the fallacy known as "affirming the consequent", given the existence of supersymmetric quantum field theories.

2. **Peter Woit**
   August 16, 2011

   Anon,

   I suspect that, given the opportunity to elaborate, Witten would have explained that what he meant was just that discovering supersymmetry would have provided support for the ideas behind string theory (but not proved string theory, since you can have supersymmetry without string theory). To pin him down, the interviewer should have asked about the falsifiability question: would a failure to find supersymmetry provide evidence against string theory?

3. **Mitchell Porter**
   August 16, 2011

   Anon, your diagnosis of Witten's error is wrong. If theories A and B both predict unobserved phenomenon X, the search for X is still a test of A, even though B also predicts it.

4. **Peter Shor**
   August 17, 2011

   Does anybody know what Witten mean when he said *"But there is a very precise relationship among the interaction rates of different kinds of particles which follows from supersymmetry and which has been tested successfully?"*

5. **AV**
   August 17, 2011

   Isn't supersymmetry an assumption of string theory as opposed to a prediction? If this is the case it seems like no physicists care to make this distinction any more even though there is a clear logical difference between the two, basically what Anon is saying I think.

6. **piscator**
   August 17, 2011

   I guess it must mean gauge coupling unification, and what that says about the relative size of the three gauge couplings at the weak scale.

7. **piscator**
   August 17, 2011

   @AV: since consistent string theories appear to require supersymmetry for their internal validity, I think it is fair to say that supersymmetry *at some energy scale* is a prediction of string theory. Supersymmetry at the TeV scale is clearly not a prediction of string theory – its absence does not falsify the subject – although its presence would suggest that string theory is on the right track.

8. **Peter Woit**
   August 17, 2011

   Peter Shor,

As piscator comments, what Witten is referring to is a supersymmetric GUT calculation, where coupling constant unification works better than in non-supersymmetric GUTS. This assumes, among other things, a "desert hypothesis" of no new relevant physics between the weak and GUT scales. While supersymmetric GUTs push up the GUT scale and thus make the proton lifetime longer, some classes of these models are ruled out by proton lifetime limits. Of course, as usual, there are lots and lots of models and choices to be made, so one can evade experimental limits.

9. **Giotis**
   August 17, 2011

   Douglas correctly says that the importance of Low SUSY for String theory has been diminished after the latest theoretical developments, mainly because the theory has now other mechanisms to solve the hierarchy problem like warped compactifications, large extra dimensions or even anthropic reasoning via the string landscape. Low SUSY was very important in the early days of String theory when only the Heterotic model could produce the standard model and Low SUSY was the only available mechanism to solve the hierarchy problem. Now on the other hand there is no such restriction you can get the standard model from D-branes in IIB or IIA where the aforementioned mechanisms could be used to deal with the hierarchy problem and thus do not need Low SUSY (they do not preclude it though). In this case SUSY could break at larger scales since it doesn't have to solve the hierarchy problem anymore.

10. **Giotis**
    August 17, 2011

    Just to add something to my previous comment.

    Of course large hierarchies (large enouph to solve the hierarchy problem) produced by large extra dimensions or strong warping, means new physics at TeV scale that should be detected by LHC. So far though this doesn't seem to be the case.

    So if they found just a light Higgs at LHC and nothing else at TeV scales (no new physics) the stringy landscape may be the only explanation for the stability of the EW breaking (unless of course someone comes up with a better idea).

11. **Roger**
    August 17, 2011

    The LHC is discovering that there is no supersymmetry — at least none of the sort that had been widely predicted. The string theorists are like a doomsday cult that desperately needs an alternate story to cover up their failed predictions.

12. **Anon**
    August 17, 2011

    Mitchell Porter, I guess it depends on the semantics of "test of string theory" that Witten could have reasonably taken his audience to understand. In the context of the interview question, it is clear to me that the average educated listener would have taken this to mean "something that can distinguish string theory from other serious proposals such a field theories". In this sense, even if you may be right in your statement that Witten did not commit an error of logic in a strict technical sense, in my opinion he certainly did so given the context. I am pretty sure that he was bloody well aware of the loophole at the time and made a conscious decision to take advantage of it for political reasons, which I find somewhat immoral.

    In any case, you will no get many physicists today still seriously claiming that the LHC is testing string theory. Why do you think that is?

13. **DB**
    August 18, 2011

    "In this case SUSY could break at larger scales since it doesn't have to solve the hierarchy problem anymore." – Giottis

    Now that the hierarchy problem has been magically airbrushed from the scene don't forget about Split Supersymmetry. Or my own personal favourite, Supersplit Supersymmetry.

$100 Million From Simons and Simonyi for the IAS

August 18, 2011
Categories: Uncategorized

The Institute for Advanced Study in Princeton announced today that Jim Simons and Charles Simonyi will donate \$100 million to the Institute, in the form of matching funds for a \$200 million campaign mainly aimed at increasing the endowment. For some idea of previous fund-raising by the IAS, see here.

Simons and Simonyi have donated significant sums to the IAS in the past, including \$6 million from Simonyi to endow a professorship for Witten. The IAS has about 25 permanent professors, with salaries reaching above \$300K/year. To get some idea of the scale of the new endowment funds, if they all went to new permanent professorships (unlikely), the number could be doubled or more. This kind of sizable increase of resources for prestigious pure research positions in math and physics, funded by huge fortunes made in the technology and financial industries, is part of a trend, with Perimeter and the Simons Center at Stony Brook two other noteworthy examples.

Comments

1. **donations**
   August 18, 2011

   That is good. One hopes the money will be put to good use. Note that endowing a chair for Witten in years past also meant (to some extent) endowing a chair for (the pursuit of research into) string theory. See previous post about Witten interview and "string theory predicts supersymmetry". So don't complain. It's a mixed bag.

2. **Peter Woit**
   August 18, 2011

   donations,

   Who is complaining? Not me. Witten's as good a choice (if not better) than anyone for an endowed chair. That a sizable chunk of $200 million in new money will be available in the future to support theoretical physics and math research is a good thing. For the physics part of it, it will be interesting to see which direction the IAS takes.

   The continuing trend away from public funding for math/physics research in favor of funding from the great fortunes of the wealthy is interesting and in some sense a reversion to the pattern of many centuries ago. At least in the form of an endowment, such funding may be more stable than public funding, which is at the mercy of the politics of science and university state and federal budgets. One might worry that the wealthy sources of the funding will have too much influence, and take research in dubious directions. Besides the Templeton multiverse funding, that doesn't seem to me to have been a problem, with people like Simons, Simonyi and Lazaridis arguably making as good or better choices than DOE/NSF peer-review panels.

3. **Mitchell Porter**
   August 18, 2011

   If they haven't explained the particle masses within five years, Simons and Simonyi should insist on a refund.

4. **Amitabha**
   August 19, 2011

   Is it better (for the future of research) to put a lot of money in a top theory place that is already reasonably well-funded, or to spread the money over several mid-ranked places so that each can attract one or two very good theorists?

5. **Guillaume**
   August 19, 2011

   Peter: Isn't the endowment revenue highly dependent on the state of the stock market, and therefore subject to large fluctuations? I'm not sure that's better than even the currently poor state of public funding.

6. **Peter Woit**
   August 19, 2011

   Guillaume,

   In the long term, endowment revenue does depend on how well the endowment is invested. In the shorter term, endowment pay-outs typically depend on the average value of the endowment over several years, not what happens in any one year. If revenue is down, you can always take more out of the endowment to keep funding whatever you want (and hope that either investments will turn in your favor, or your patrons will come up with more money. Note that $50 million is less than 1% of the Simons net worth…).

7. **Douglas Natelson**
   August 20, 2011

   That's quite an investment, particularly for a theory-focused institution. I'd love to see that kind of growth in support for experimental work….

8. **abacus salam**
   August 23, 2011

   that sounds pretty good; i hope more hedge fund managers will follow suit.

String Theorists Suggest Space Wormholes Possible

August 18, 2011
Categories: This Week's Hype

I was just out for a bike ride, during which an idle thought came to me about a rule of thumb that might deserve publicity. This rule of thumb is that the mention of wormholes in a popular science book, TV program, etc., indicates that real science is not what's being discussed. When I got back to my office, I found that USA Today has a new story: [String theorists suggest space wormholes possible](). The source of the story is [this preprint]().

Via Twitter, the story's author did get the obvious response to this claim: this isn't news since everything is possible in string theory.

Comments

1. **Shantanu**
   August 18, 2011

   Peter there is no mention about string theory in the paper. (wormholes
   is an old concept in relativity and there are papers on it from 30's onwards)
   shantanu

2. **Cliff**
   August 18, 2011

   I dont suppose there is any way to map this 'rule of thumb' into a meaningful statement about whether string theory is relevant to understanding the world or not?

   After all, according to the rule general relativity isn't "real science" either.

   Interestingly, you seem to advocate the opposite demand than those of e.g. Smolin. Instead of insisting that string theory be formulated with manifest background independence, it is now apparently required to be unable to deal with certain backgrounds. Is this basically what you're saying?

3. **Jeff**
   August 18, 2011

   Well, I work with a physicist (who ended up in the math department for various reasons) who's a GR guy, and has written several papers on this stuff. In particular he has done work on limitations on negative energy, which is what you need to keep a wormhole stable. Together with collaborators they found limits on the duration of any localized negative energy, these are based on the uncertainty principle. The upshot is that you can't get stable wormholes in standard GR. From what I can deduce from the paper here, they are trying to get around this, and it does use string theory – they get the extra curvature they need from the compactification from 10 dimensions down to 4 (I thought M theory was 11 dimensional 🙂

4. **Peter Woit**
   August 18, 2011

   Cliff,

   This rule of thumb is not about string theory, it's about TV programs…

   There may very well have once been a TV program that discussed wormholes and GR in a scientifically solid way. I missed that one though.

5. **GR student**
   August 18, 2011

   It would be sad if the public internalized your rule of thumb and then denied themselves the pleasure of reading "Black Holes and Time Warps: Einstein's Outrageous Legacy" by Kip Thorne because they thought "real science" was not being discussed.

6. **Natron**
   August 18, 2011

   @Shantanu,
   Yes, the paper mentions string theory 4 times. Also, Peter was just reporting on what the article is claiming based off this paper.

7. **Peter Woit**
   August 18, 2011

   GR Student,

   Well, it's just a rule of thumb, doubtless there are exceptions. I think it works better for TV shows, and has become more and more accurate as time goes on. Less accurate for books from more than 15 years ago.

8. **Casey Leedom**
   August 19, 2011

   Peter, I think you're "re-coining" a rule of thumb that's already been covered in "Jumping the shark." Isn't that what the entire multiverse thing is? Once you run out of other ideas …

   Casey

9. **Shantanu**
   August 19, 2011

   Natron, the paper says that Gauss-bonnet gravity is motivated from heterotic string theory. but again G-B gravity has a much longer history than string theory.

btw, see this paper on wormholes
http://arxiv.org/pdf/0902.1994v3

10. **DB**
August 19, 2011

Enjoyable paper all the same, especially if read from the perspective: "what corrections to the GR Lagrangian would permit stable traversable wormholes?" Although it uses a particular string theory lagrangian to derive those corrections as its starting point, I would be more interested in a research program that tried to explore what the most general class of such corrections were and then seek constraints on them.
Also interesting that the most recent paper to be referenced was 1988.
Aside from all the string theory hype, the fact remains that without some means of faster than light travel and communication we are stuck in this solar system for eternity, so I consider research efforts in this utterly neglected area worthwhile and deserving of the title "fundamental", provided always that they take GR as their starting point.

11. **DB**
August 19, 2011

That should of course have read "string theory action" and not "string theory Lagrangian"

Sunday Night Higgs?

August 18, 2011
Categories: Experimental HEP News

Lepton-Photon 2011 begins Monday morning, the schedule is here. It should start off with a bang, with the latest Higgs search results from ATLAS and CMS presented starting at 11:20am local time, the middle of Sunday night here. There will be a press conference on Wednesday.

If the hints of a Higgs signal seen in the data presented last month at EPS-HEP 2011 are real, they should be more pronounced in the new data (the experiments have now collected about twice as much data as that used in the analyses presented at EPS-HEP 2011). The Higgs Combination Group should by now have produced a combined analysis using last month's data from the ATLAS + CMS and presumably that will also be released on Monday or soon thereafter. They have just today released a new document giving the details of how the combination is done: Procedure for the LHC Higgs boson search combination in summer 2011. Still holding out on us though in terms of the real data, that document just shows toy data…

Update: The latest rumor I'm hearing is that the only analyses updated with new data (nearly twice as much) since EPS-HEP that will be available Monday will be from individual channels. Analyses combining the different channels won't be ready for another 2 to 3 weeks. I still think though that we should see the CMS+ATLAS combination of the old data shown at EPS-HEP. So, if the Higgs is there, a definitive signal may still not quite yet be available. These people do need to take a vacation sometime in the summer…

Update: The news is that CMS and ATLAS have produced new combinations (although the combination of older ATLAS + CMS data has not been released, and I'd love to know why…). The bottom line is that the hints of a Higgs around 140 GeV have weakened with the addition of more data. A simplified summary of the current situation would be:

- No Higgs above 145 GeV
- In the region 135-145 GeV, both experiments are seeing somewhat more events than expected from background, but less than expected if there really was a Higgs there.
- Not enough data to say anything about 115-135 GeV, the Higgs could still be hiding there. If so, a malicious deity has carefully chosen the Higgs mass to make it as hard as possible for physicists to study it.

More details available on the conference slides that should be available here. Tommaso Dorigo and Matt Strassler have commentary.

Update: Still no word on why no CMS+ATLAS combination has appeared. Philip Gibbs has hacked together an unofficial version (see here and here). Comparing the EPS data to the latest, one sees clearly that a marginally significant signal consistent with a Higgs has weakened quite a bit with the new data (and thus, there was little to no evidence for such a Higgs in the new data). Also worth reading, commentary from Jester here.

Comments

1. **M**
   August 19, 2011

   The rumor is different

2. **Charles**
   August 19, 2011

   What is the rumor this round?

3. **Peter Woit**
   August 19, 2011

   Charles,

   If I had heard any believable rumors, I'd post them here. Maybe M can share more details with us. Or, anonymous posting by the well-informed is encouraged…

4. **null**
   August 19, 2011

   Would there be any reason for "Lepton-Photon 2011" and "Higgs Combination Group" to announce a null result (if there is one)? So if they do plan an announcement, presumably they found some evidence for a higgs?

   How many fb-1 have been collected to date?

5. **SA**
   August 19, 2011

   Over 2/fb

6. **bonk**
   August 19, 2011

   There's no point in keeping announcing null results. BUT, since many people have nothing better to do other than wait for Higgs results, suddenly there is a point.

7. **Peter Woit**
   August 19, 2011

   null, bonk,

   With this amount of data, the result (even a null result) is guaranteed to be interesting. If you really don't see anything, you can rule out the possible existence of a Higgs on more and more of the allowable mass range. If this can be done all the way to the LEP limit (this is still a ways away), you've shown there is no SM Higgs and shown there is something dramatically wrong with our understanding of the SM. If you do see a signal, that's of course big news and the beginning of a research program to understand exactly how the Higgs behaves.

The least interesting thing is a signal of marginal statistical significance, but as data accumulates this starts to become impossible. Either you see the thing or you show that it isn't there.

8. **Emile**
   August 19, 2011

   Bonk: since there is no point in announcing null results, the two experiments will not tell the rest of the world in what mass range the Higgs has been excluded. And, they won't bother to tell the world if/when the whole mass range has been excluded either.
   CDF and DO would also like to apologize for releasing null results.

9. **Michael Schmitt**
   August 20, 2011

   Hi,

   I think "bonk" must be joking. 😉 CMS and ALTAS are testing a wide range of possible Higgs masses and any statement that there is no evidence for a standard model Higgs boson within that range is an important scientific result. There is an hypothesis, and then there is the experimental test. This is not really grounds for cynicism, right? 😉

   About 2 fb-1 has been collected by each experiment, but you can expect some fraction, say 20%, not to ready yet for analysis. Since the increase in the data samples is rapid, the need to combine ATLAS and CMS results is not so strong. I'm sure each experiment will show updated combined results, but I would not necessarily expect to see an ATLAS+CMS combination.

   bye

10. **Peter Woit**
    August 20, 2011

    Hi Michael,

    I'm hearing from other sources the opposite: that individual ATLAS and CMS combinations of new data aren't yet ready (although some individual channels are). Also, the LHC Higgs Combination Group was supposed to have an ATLAS+CMS combination based on analyses released at EPS-HEP ready a week or two ago for release at Lepton-Photon. Is there any reason that won't happen?

11. **M**
    August 21, 2011

    The rumor is that the combinations of different channels within single experiments will be shown at 1.5 fb-1 (but no ATLAS + CMS combinations) and that unfortunately the hint at 140 GeV XXXXXXXXXXXX, partly because the background XXXXXXXXXX.

12. **Anonymous**
    August 21, 2011

    Do you personally believe that they're going to find the SM Higgs boson, Peter?

13. **Peter Woit**
    August 21, 2011

    Anonymous,

    I have no idea whether there's an SM Higgs, or something more interesting is causing electroweak symmetry breaking. I've been wanting to know this for more than 30 years, so very much looking forward to finding out….

14. **bonk**
    August 21, 2011

    In the Tuesday talk "BSM results from LHC", we will also get an update on SUSY with over 1.5 fb^-1. Is that right?

15. **bonk**
    August 22, 2011

    Just watched ATLAS and CMS talks. Moderate enlargement of last month's exclusion regions. They expressed disinterest in combining data, because data are being produced faster than can be combined. Also, last month's excess has decreased in significance.

16. **Thomas Larsson**
    August 22, 2011

    Does decreased significance mean probably no Higgs? If the signal were real, we would expect significance to increase with more statistics, right?

17. **Fred**
    August 22, 2011

    There is no Higgs and probably no new physics. Wow.
    Sorry Higgs, Guralnik, Hagen, Kibble, Brout, Englert, Anderson, Weinberg, and Salam, but you were wrong all along. Good effort though.

18. **DB**
    August 22, 2011

    The UK's guardian newspaper is reporting from the Lepton-Photon conference in Mumbai that there's nothing to see here, the signals have all but vanished.

    http://www.guardian.co.uk/science/2011/aug/22/higgs-boson-signals-fade

19. **Bernhard**

August 22, 2011

CERN press-release on this:

http://press.web.cern.ch/press/PressReleases/Releases2011/PR14.11E.html

20. **jon-student**
August 22, 2011

Suppose there's no Higgs. From wikipedia, there appears to be several proposed Higgless models: are some of them testable with LHC data accumulated so far? If not, how much more data would be needed: end of 2012? or the 7 Tev run after 2013? or even more? Thanks, exciting times!

21. **Thomas Larsson**
August 22, 2011

Don't Higgsless models also require something like a Higgs at the LHC, even if it is composite. E.g. a bound state of technifermions?

22. **m**
August 22, 2011

Wow, some people are fast at concluding there's no Higgs! The limits do not cover the whole mass range; the Higgs could still be light and compatible with SM predictions if it exists!

23. **Peter Woit**
August 22, 2011

jon-student,

The search for the SM Higgs is very straightforward in the sense that you have a very precise model to test, with everything completely determined except the value of one parameter. In terms of testability/predictivity, it almost never gets any better than that.

If there is no Higgs, and something else is responsible for electroweak symmetry breaking, there are lots of possibilities that have been suggested, but none that are really compelling. These have lots of undetermined parameters. In many cases evidence should be visible at the LHC if you do the right analysis, but you can also come up with models that would not have effects visible at the LHC. For now I suspect the experiments will remain focused on looking for the Higgs, but if that gets ruled out, attention will move to other possibilities, and in some sense the subject will get a lot more interesting…

24. **Alex**
August 22, 2011

Slow down, what are you people talking about? Looking at the ATLAS/CMS exclusion plots, they are still pretty much what one would expect to see right now if there was a Higgs at above 140 GeV for example. Any data points that go beyond the sigma/sigma_SM =1 line are bound to become smaller if it is a SM like Higgs. Large peaks beyond that line are not expected to prevail if the SM Higgs is realized in nature.

@Thomas Larsson,

The family of so-called Warped Higgsless Models which became famous around 2003, do not have to have any fundamental scalar resonances, in their range of perturbative validity, which extends up to about 5 TeV. (apart maybe from the radion which represents fluctuations of the size of the extra dimension).
This is in contrast with so-called composite-Higgs models. It is conceivable that if the radion is light, it might get some effective nonrenormalizable somewhat higgs-like couplings, but not necessarily comparable to what a Higgs would do.

25. **Alex**
August 22, 2011

In my last comment it was supposed to read

"The family of so-called Warped Higgsless Models which became famous around 2003, do not have to have any —————- scalar resonances, in their range of perturbative validity, which extends up to about 5 TeV."

26. **felix**
August 22, 2011

95% CL exclusion:
ATLAS
146~232, 256~282, 296~466
CMS
145~216, 226~288, 310~400
Tevatron
100~109, 156~177
I don't understand why people take this as a sign that the Higgs might not exist. Since precision electroweak constraints favor a light Higgs, ruling out larger Higgs masses only makes us more confident in the SM.

27. **Charles**
August 22, 2011

The "Higgs" also could be composite – consistent with the general proof of some of the theorists back in 1964.

28. **Harry Johnston**
August 22, 2011

Out of curiosity, do there happen to be any theoretical models which predict that the Higgs should be exactly where it is most difficult to find?

29. **chris**
August 23, 2011

Harry Johnston,

yes, in fact one extremely compelling one. if there is the SM Higgs in the region of 130~160 GeV then unitarity of the SM is guaranteed up to beyond the Planck scale. so if you think the SM is all there is up to gravity and if you disregard all naturalness-based arguments, the Higgs is 'expected' to lie in that region.

if it will turn out to be a 130GeV SM Higgs – well then very probably goodbye to all accelerator-based signals of BSM physics for probably the rest of our and our grandchildrens lifes (although one has to be careful with this sort of statements).

30. **chris**
August 23, 2011

the SUSY summary was just given: no signal again.

31. **Eric**
August 23, 2011

"Not enough data to say anything about 115-135 GeV, the Higgs could still be hiding there. If so, a malicious deity has carefully chosen the Higgs mass to make it as hard as possible for physicists to study it."

Isn't this precisely the Higgs mass range expected in the MSSM (< 130 GeV)? Apparently, the malicious deity goes by the name, Susy.

32. **Peter Woit**
August 23, 2011

Eric,

Minimal susy (as well as precision electroweak fits) most naturally leads to lower values of the Higgs mass. It's the LEP bound that gives you the 115 GeV. Already a malicious deity seems to have been at work, pushing the Higgs mass just above where LEP could see it.

Maybe the LHC will find no superpartners, just the Higgs of the MSSM. Hard to see how that would work, but I'm sure susy modelers can find a way…

33. **Eric**
August 23, 2011

Peter,

You need to be careful about what you mean by "natural". Natural is whatever nature does, not what you think it should be based on your personal judgment. In the case of minimal supersymmetry, the Higgs mass can be as great as 130 GeV. In fact, taking into account all current constraints, the "most likely" Higgs mass is around 118 GeV. If this is the case, then no Higgs signal should have shown up yet, but it should show up by the end of 2012.

In the case of the observation superpartners, you are essentially looking at the situation with a very naive and uninformed point of view. As has been pointed out to you by other commenters, the current contraints are based on simplified models which make certain assumptions. The main assumption of these models is that squarks and gluinos decay directly to the lightest neutralino, and thus produce large missing energy signatures. This definitely does not have to be the case, which would cause the signals of the superpartners to be much more difficult to see. Similar considerations apply if R-parity is violated. Besides these considerations, the squarks and gauginos easily be as heavy as 2 TeV, meaning that it would not be possible to see them until the second higher energy LHC run after the year-long shutdown/upgrade.

34. **Tom**
August 24, 2011

The CMS speaker himself said it would be worthless to present a combination plot at this point as it would be outdated by the time it appears since the data has been coming in so quickly. Since pp data taking ceases at the end of Oct. it is reasonable to expect a combination to appear before the end of the year holidays.

35. **Peter Woit**
August 24, 2011

Tom,

From what I hear, while there was some concern about the value of a combined CMS+ATLAS result when the individual experiments already had more data and better results, a deciding fact was that at the last minute a mistake was found in one of the input channels to the combined result. So, an excellent reason not to release it publicly at the conference…

36. **D R Lunsford**
August 26, 2011

Blast from the NEW past

http://www.math.columbia.edu/~woit/wordpress/?p=163

-drl

37. **Peter Woit**
August 26, 2011

Thanks drl,

Looks like Tommaso's prediction was pretty good. By the time all Tevatron data is analyzed next year, their limits may be about what he was expecting. The LHC schedule however, didn't work out as expected….

38. **Funes the Memorious**
August 27, 2011

Another quote from that same thread, showing not-so-accurate predictions:

> My personal preferred guess (30%) is that the Higgs is at the 115 GeV level, and it's one of the light Higgs scenarios that are natural in SUSY.

> But the discovery of Higgs will be a few-day celebration only. It's a trivial thing. We know that something like the Higgs must be there to make the WW WW scattering unitary, and the fundamental scalar is simply preferred by precision measurements.
>
> The real question to answer is SUSY below TeV, and other potential new physics.

Ah! Those were the days when "SUSY below TeV" seemed to be just within reach…

39. **steve newman**
August 27, 2011

"Not enough data to say anything about 115-135 GeV, the Higgs could still be hiding there. If so, a malicious deity has carefully chosen the Higgs mass to make it as hard as possible for physicists to study it."

.
Could someone explain what makes one energy range easier or harder than another for Higgs detection.
thanks.

40. **Bill K**
August 27, 2011

What makes the Higgs boson easier or harder to detect is which decay channels are available, what their branching ratios are, and how easily they can be distinguished from the QCD background. See for example http://www.hep.lu.se/atlas/thesis/egede/thesis-node14.html. At low mass the leading decay is H -> b-bbar, very hard to identify. But the H -> WW is easy, and note how rapidly its branching ratio rises with increasing Higgs mass.

41. **Allan Rosenberg**
August 27, 2011

I used to think Lederman called the Higgs the God particle just to sell books. I didn't give him enough credit. Now I realize that he called it the God particle as a sly prediction that it doesn't exist.

42. **Chris Oakley**
August 27, 2011

Lederman has covered the possibility of the Higgs being composite, too, since God, assuming he exists, is also composite, comprising the Father, Son and Holy Spirit.

This Week’s Hype, Part II

August 21, 2011
Categories: This Week's Hype

String theory hype is still coming fast and furious, so much so that the latest edition of *This Week’s Hype* needs to be a double issue. Today we learn that Black holes and pulsars could reveal extra dimensions, solving that thorny problem of testing string theory:

> String theory, which attempts to unify all the known forces, calls for extra spatial dimensions beyond the three we experience. Testing the theory has proved difficult, however.
>
> Now John Simonetti of Virginia Tech in Blacksburg and colleagues say black holes orbited by neutron stars called pulsars could do just that – if cosmic surveys can locate such pairings. “The universe contains ‘experimental’ setups we cannot produce on Earth,” he says.

The source of the hype isn’t really new though, they were featured a few years ago in an earlier edition of *This Week’s Hype*.

Comments

1. **Bernhard**
   August 21, 2011

   I have to say this sort of hype worries me a bit. Since string theory is a theory of anything at all, it won´t take long until any random astronomical signal to be consider evidence of string theory. Is the black-hole radiation loss going to be precisely predicted by the theory so that in the who-knows chance some experimental group see this it could be confronted? No, of course not. So, if we see nothing, situation stays as it is, string-theory is the marvelous theory of zero predictions. If on the other hand we see anything (at all) we can´t really precisely fit to general relativity, sure enough that´s string theory being “confirmed”.

2. **abbyyorker**
   August 21, 2011

   Peter,

   It seems like a simple idea that could provide a test although there is some “work” to be done in discovering a system with the appropriate properties. As a layman, I cannot see the difficulties. Where does the idea break down?

3. **KanzasTabasco**
   August 21, 2011

   This would be a fascinating development if true (about the black holes and pulsars), but it won’t do much for string theory. We can have extra spatial dimensions without any string theory.

4. **Peter Woit**
   August 21, 2011

   abbyyorker,

   It’s the same problem as every other “test” of string theory. String theory predicts nothing about these supposed extra dimensions, they could be any size whatsover and have all sorts of different properties. So, even if there are extra dimensions, and you manage to get yourself a pulsar and a black hole with just the right properties to see evidence for them, this in no sense “tests” string theory.

5. **Brandon Greggs**
   August 22, 2011

   Dear Peter,

   I just started following your blog.

   In two recent posts you showed how a) Ed Witten stated that String Theory is testable, and that b) Ed Witten deserves an endowed chair.

   So why is it that you are surprised when other physicists hype string theory as testable?

   Is only Ed Witten allowed to state that string theory is testable, and if so, why? What rules/laws are you following?

6. **Peter Woit**
   August 22, 2011

   Brandon,

   There’s no contradiction between someone being a great physicist, deserving of an endowed chair, and at the same time being over-enthusiastic about some idea, to the point of making claims for it that are not really supportable. If believing or saying something not quite true disqualified one from an endowed chair, we would have extremely few professors occupying such chairs.

   In this particular case, I’d suspect that Witten, given a chance to revise and extend his remarks, might have something a bit different to say today about this (and even back then probably would have liked to add appropriate caveats). But, no matter what, nothing can change the fact of the matter that he has a fantastic list of achievements to his name, and is a scientist of the highest accomplishment.

7. **Shantanu**
   August 23, 2011

   Note also that we don’t have a single known example of pulsar-black hole binary.

This Week's Hype, Part III

August 25, 2011
Categories: This Week's Hype

Today's [Wisconsin State Journal] covers the [String Phenomenology 2011] conference going on in Madison this week, where, according to the organizers, about 100 scientists are discussing how to "test string theory":

> The Madison conference is something of a milestone in the study of string theory, Shiu said, because it represents 10 years of thought and advances. "It means the field is moving forward, that interesting things are going on," he said.
>
> Kane agreed and said much of the conference focuses on the predictive powers of string theory. If the theory can predict the existence of certain particles or behaviors, Kane said, and those are then borne out by successful experiments at projects such as the Large Hadron Collider in Europe, string theory would become an accepted explanation for the workings of the universe.

Kane has a long history of making "predictions" based on string theory, including a 1997 Physics Today article String Theory is Testable, Even Supertestable, which gave a plot showing the masses of all superpartners, in the range of 50-300 GeV. His latest "generic predictions" from the conference are here(see page 22). These days most of the superpartners have for some reason moved up to 50 TeV, well beyond any hope of observation at the LHC. There's a gluino though at a bit above current bounds of around 500 GeV, and claims that, with the right sort of analysis, this will be visible. Once this analysis gets done, one suspects the gluino will go join its friends at much higher masses. There's also a "prediction" of the range of the Higgs mass, which happens to be within the range not yet ruled out.

Another conference going on at the moment is at Les Houches. There Luis Alvarez-Gaumé gave a survey talk about string theory, and in his conclusion he makes quite clear what he thinks of efforts like Kane's:

> One cannot make LHC-accessible predictions.

Update: After posting this, I remembered that I'd once read a much more interesting story about theoretical physics in the Wisconsin State Journal. This was from when Dirac, not string phenomenologists, came to town, and gave the paper an interview.

Comments

1. **Frank Sharkany**
   August 25, 2011

   Dirac interview was pretty funny! Keep up the good work.

2. **kane1997**
   August 26, 2011

   Technically, the 1997 Kane article in Physics Today displayed a "possible spectrum of superpartners". Kane didn't say that his graph was a firm prediction of sparticles. Nevertheless, it is a valid ponit that the spectrum of possible superparticles has always been just beyond the reach of then-currently accessible energies. So supersymmetry has always been just around the corner. The Wisconsin State Journal article is well known, but retains its humor after all these years.

3. **Thomas R Love**
   August 26, 2011

   Dirac was capable of longer sentences. After he gave a talk, I asked him if he would sign my copy of his book. He replied "I don't do that."

4. **Dathan**
   August 26, 2011

   Please take care this weekend when the hurricane hits NYC!

5. **fgh**
   August 27, 2011

   Peter, you may want to have a look at this http://www.bbc.co.uk/news/science-environment-14680570

6. **Peter Woit**
   August 27, 2011

   Dathan and fgh,

   Thanks. New York should be fine, but I don't think the field of supersymmetry is going to ever recover from the devastating effect of the recent LHC results…

7. **Shantanu**
   August 27, 2011

   Peter, just a warm-up to another possible rumor. At the taup meeting next week,
   there is apparently a press release from CRESST which claims to have found dark matter. See
   http://taup2011.mpp.mpg.de/?pg=Press

8. **anonymous**
   August 28, 2011

   Supersymmetry is a very interesting possibility to test, regardless of the fact that some researchers have oversold it. The LHC will test weak scale SUSY, assuming the machine continues to work well enough to do so, and the results will be very useful to know, whichever way it goes. This mode of argumentation — picking out extreme statements by some individuals and then making snide comments about the field based on them — is not useful. The posts on news of recent developments of various sorts are useful.

9. **Peter Woit**

August 28, 2011

Irene also turned out to be seriously overhyped. Went out this morning at what was supposed to be the height of the hurricane: found modest amounts of rain, wind gusts going all the way up to 20-25 mph. It was high tide, and the Hudson was about a foot higher than usual. Traffic was moving fine on the West Side Highway in both directions around 100th St, whereas it normally floods up here at the drop of a hat. Bought a newspaper and some ice cream, then came home. At home, turned on the TV news, which was full of frantic warnings not to go outside, 4-8 foot storm surges, 65 mph winds, etc. Also learned from the TV news that the West Side Highway was closed from 96th St. to 125th St.…

10. **Peter Woit**
    August 28, 2011

    anonymous,

    A more accurate statement than "the LHC will test weak-scale SUSY" would be that weak-scale SUSY already was tested by LEP and Tevatron null searches for superpartners, as well as null results for various indirect effects. The last hope was the LHC, and the null results it has found pretty conclusively finish this off. So, the LHC HAS tested weak-scale SUSY, and this is (or should be) over, no matter how strenuous the efforts of SUSY partisans to deny reality. Pointing out the huge differences between SUSY partisans's past and present arguments isn't just being snide.

LHC results put supersymmetry theory 'on the spot'

August 28, 2011
Categories: Uncategorized

The HEP theory community is atwitter over a BBC News story LHC results put supersymmetry theory 'on the spot' that reports from the Lepton-Photon 2011 conference in Mumbai, where more null results relevant to supersymmetry were reported. According to the story:

> Results from the Large Hadron Collider (LHC) have all but killed the simplest version of an enticing theory of sub-atomic physics.
>
> Researchers failed to find evidence of so-called "supersymmetric" particles, which many physicists had hoped would plug holes in the current theory.
>
> Theorists working in the field have told BBC News that they may have to come up with a completely new idea.

Joe Lykken, an organizer of the SUSY11 conference about to start at Fermilab, is getting worried:

> "There's a certain amount of worry that's creeping into our discussions," he told BBC News.
>
> The worry is that the basic idea of supersymmetry might be wrong.
>
> "It's a beautiful idea. It explains dark matter, it explains the Higgs boson, it explains some aspects of cosmology; but that doesn't mean it's right.
>
> "It could be that this whole framework has some fundamental flaws and we have to start over again and figure out a new direction," he said.

On Twitter, there's Carlo Rovelli gloating here, Matt Strassler (here and here) and Lisa Randall (here) claiming all is not lost. In an exchange here, Strassler notes that he's fighting to prevent the risk of "no money for your research". It's unclear if he's referring to funding for the LHC experiments or for SUSY theory. There is a real long-term danger to HEP experimental funding once the public realizes that they're not getting the extra dimensions some have promised them, but the time to fight that risk was the many years during which hype about the LHC was rampant.

Both Strassler and Kane now seem to attach great importance to the point that, in some SUSY variants, gluino mass bounds are lower than the 1 TeV of the most popular models, more like 500 GeV. Kane goes so far as to claim that the gluino will be found, at masses below 1 TeV:

> The current limit on gluino masses is not above 500 GeV. Whether the squarks are indeed so heavy is not the issue, the point is that if they are the limits on gluino masses are smaller than is often stated. I and others expect this decay to tops and bottoms is the signature by which gluinos will be found, with masses well below a TeV.

Presumably LHC searches are underway for signatures of gluinos in this mass range in these versions of SUSY. I'd be very curious to hear what the status of those searches is. If they come up negative, will SUSY proponents finally give up? New results relevant to SUSY are appearing rapidly, see the latest from CMS here and here.

For some historical perspective, something I ran across recently was a 1993 New York Times report 315 Physicists Report Failure In Search for Supersymmetry, which described null results from early days of the Tevatron. One very funny thing about the article is that much of its emphasis was on the unwieldy nature of the CDF detector, with its $65 million budget and huge number of 315 physicists.

Update: SUSY11 opens tomorrow with a talk by Murayama that incorporates the BBC News story and describes evidence against superpartners as "impressive, worrisome, but not quite there yet". No indication of when it will get there. The title of the talk: Why do SUSY in 2011?

Update: Quite interesting reading is Michael Peskin's summary talk at Lepton Photon. On the topic of this posting, he writes:

> Before the start of LHC, I expected early discovery of supersymmetry in the jets+MET signature. Many other theorists also had this belief. But, it was not correct.

and he explains why this was (large amount of fine-tuning required if superpartner masses are even as large as 1 TeV). He also explains possible ways to construct SUSY models that evade current experimental bounds while keeping superpartner masses relevant to the fine-tuning problem from getting much too large.

This week at CERN there's a workshop on Implications of LHC results for TeV-scale physics, which should have many interesting talks.

Update: Yet another technical talk about the state of SUSY searches that begins by reproducing the BBC story is today's talk at CERN by John Ellis. Ellis gives an overview of SUSY fits. The regions identified by these (pre-LHC) as the most likely place for SUSY to show up have in many cases now been ruled out. With the latest LHC data, the "most likely" region moves out to higher and higher masses, with less and less of a good fit. Ellis concludes:

> LHC data putting pressure on popular models.

Update: Another review of the SUSY situation is here (from the Physics in Collision conference). A quote from Altarelli:

> It is not time to desperate yet… but maybe it is time for depression already."

Comments

1. **Giotis**
   August 28, 2011

   Why on earth Rovelli is gloating and why these results are good news for LQG? Did LQG make a prediction about SUSY at LHC that I'm not aware of? LQG has nothing to say about SUSY, for or against (especially about SUSY at LHC).

   It's really a pity; I thought Rovelli was a modest guy and a serious scientist but he jumped at the opportunity to mislead the public and his pet theory for no good reason.

2. **Giotis**
   August 28, 2011

   "and his pet theory for no good reason."

   It should be:

“and to hype his pet theory for no good reason.”

3. **Maury Markowitz**
August 28, 2011

““It’s a beautiful idea. It explains dark matter, it explains the Higgs boson, it explains some aspects of cosmology; but that doesn’t mean it’s right.”

Forgive this comment from the laity, but as I understood it, nothing in this quote is, at first glance, correct. That being the case, I’m wondering if someone can help point me in the right direction for further reading, as I assume the problem is mine.

1) “It explains dark matter”: I was under the impression that it was *possible* that one of the super partners might have the right mass and properties to serve as a WIMP suitable for DM. I have not heard of anything more direct that that, however, along the lines of “with supersymmetry, DM is not an issue in the first place”, or, less difficult, “in order to work, susy must have a [insert super partner here] with mass of xx TeV that would exactly explain DM”.

2) “it explains the Higgs boson”: I assume he is referring to the problems with the hierarchy problem of Higgs mass. If that is what he’s talking about, I was under the impression that susy simply moved the goalposts on this problem, as it does not explain the μ problem. But is this what he is saying?

3) “it explains some aspects of cosmology”: well so does practically everything – notably the DM that was already mentioned. This is definitely something I am weak on, what does susy talk about in cosmology that isn’t part of DM?

4. **Bernhard**
August 28, 2011

That BBC article was not so good, but the journalist was probably only passing on what he was being told, there´s no way for him to judge how the LHCb results affect SUSY. Besides, people can´t complain about a little bit of hype against SUSY since in the pre-LHC era some people abused from it in the other direction.

I agree with those who say that SUSY is not ruled out and that is rather premature to make those claims. However, there is no way the LHC or even the super LHC could rule out the complete SUSY parameter space, which means that arguments such as “the strategy to find SUSY at the LHC rest upon assumptions” can go on forever.

What we should be checking is how the LHC results are messing up with SUSY motivations. I´m sure right now it´s easy to evade experimental limits in terms of affecting the SUSY “solution” to the hierarchy problem, but would it be after 2012? After, 5 10 years of LHC running?

5. **anonymous**
August 28, 2011

“There is a real long-term danger to HEP experimental funding once the public realizes that they’re not getting the extra dimensions some have promised them, but the time to fight that risk was the many years during which hype about the LHC was rampant”

Some of us have been saying this for a long time – it’s a bad coincidence (or maybe fate) that the current government funding crisis is happening at the same time that many of our theories (which have been made popular through broadcast science channels) are being proven only to be fantasies. Biology is starting to look pretty interesting …

6. **Peter Woit**
August 28, 2011

Maury,

I also thought this was fairly outrageous hype. Your explanations of 1 and 2 I think are correct, and I also have no idea what he was referring to about SUSY explaining cosmology.

Bernhard,

The LHCb results are relevant, although they’re just one more example of this problem for SUSY of it not showing up in precision electroweak measurements. Matt Reece, in his recent KITP talk, claims that these are actually a more serious problem for SUSY than not finding superpartners at the LHC. If you had seen 1 TeV squarks or gluinos at the LHC, it would be very hard to understand why they didn’t contribute measurably to cause deviations from SM predictions in the electroweak sector.

It has become increasingly difficult to come up with a plausible explanation for why experiments that should be sensitive to SUSY see nothing. With the missing superpartners at the LHC, and the new LHCb result, this may have reached the tipping point, where even long-time SUSY proponents like Joe Lykken are getting ready to throw in the towel.

7. **Emile**
August 28, 2011

Anonymous: before you enroll in biology courses, you should wait to see if a Higgs is found and if anything else shows up. Will there be a Higgs and nothing else? Too soon to bail out… (besides, have you ever seen Joe Lykken top ten list of why physicists are better than biologists?)

8. **Shantanu**
August 28, 2011

Maury, I agree you should take all such claims with a grain of salt.
in 1998, P. Ramond claimed that the Super-K results for non-0 neutrino mass point evidence for low-energy supersymmetry.

9. **anonomous**
August 28, 2011

Emile: Just saw the list and liked #3:

Particle Physics promises:
– superstrings, supersymmetry, supercolliders.
Biology promises:
– supermice, supertomatoes.

I guess we will have to settle for the big tomatoes!! What will string theorists do with their free time?

10. **Roger**
August 28, 2011

A 2006 NY Times article said, “Physicists are a bit frustrated that their results keep agreeing with the Standard Model and so far show no hint of supersymmetry.”

11. **Maynard Handley**
August 28, 2011

“impressive, worrisome, but not quite there yet”
“There’s a certain amount of worry that’s creeping into our discussions,”

Am I the only one extremely irritated by this language? “What do you mean, WE, white man”.

The discomfort these people are feeling seems fully justified by the fact that they have no qualms about speaking for the whole of physics.
Perhaps they’d be mocked a little less by other physicists if they used phrases like “this is worrying for OUR theory” in their statements? Until they learn that level of humility, personally I say to Carlo Rovelli et al “good work and keep at it”.

12. **Matt Reece**
August 28, 2011

I’m going to violate my personal rule against writing blog comments that can be easily found by Googling my name, because I want to take issue with this:

> Matt Reece, in his recent KITP talk, claims that these are actually a more serious problem for SUSY than not finding superpartners at the LHC. If you had seen 1 TeV squarks or gluinos at the LHC, it would be very hard to understand why they didn’t contribute measurably to cause deviations from SM predictions in the electroweak sector.

Rather, what I said in my KITP talk is that *flavor* is still the strongest constraint that we worry about when we think about SUSY models. I don’t agree that precision electroweak physics is a strong constraint on supersymmetry. Flavor is a strong constraint on *any* physics beyond the Standard Model: check out Table 1 of this paper by Isidori, Nir, and Perez to see just how bad. Because the bloggy audience might not know about this aspect of high-energy physics, I’ll briefly summarize. If we add generic higher-dimension operators to the Standard Model, we find that some of them associated with flavor-changing neutral currents are suppressed by a scale of 10,000 TeV (or even more if we allow them to violate CP). This means that if we had new particles that couple strongly and in a completely generic way to the Standard Model, they have to be heavier than 10,000 TeV, *way* beyond the reach of the LHC. The constraints in sensible models are *not* this bad, because the new physics isn’t completely generic: it couples weakly and often at least approximately respects the Standard Model’s flavor structure, both of which make the constraint more mild. But compared to precision electroweak, it’s a much bigger worry, and absolutely any model of new physics near the TeV scale has, as a zero-order task, to explain how it avoids causing problems with these constraints.

So how bad is this for SUSY? It’s not necessarily bad at all. One well-motivated way of breaking SUSY in the Standard Model, gauge mediation, has a special structure that’s automatically free of flavor problems. (However, it has some cosmological problems, especially if you try to embed it in a more UV-complete framework, which were second on my list of things to worry about but far beyond the scope of this comment box.) Other approaches to SUSY breaking are somewhat more susceptible to flavor problems, but are also less well-understood, depending more on high-scale physics. I gave a brief, but I think reasonably complete, list of such scenarios in the KITP talk (and Yael Shadmi pointed out that other models with somewhat more interesting possible flavor signals are still allowed). So if we see squarks, I agree that we will need “to understand why they didn’t contribute measurably to cause deviations from SM predictions,” but in the flavor sector, not in the electroweak sector — and the answer might turn out to be a mechanism we already understand, like gauge mediation.

Precision electroweak really doesn’t bother me at all for SUSY. It’s a somewhat more serious problem for technicolor (or Randall-Sundrum), which as far as we know *always* has dangerous contributions to electroweak observables, but a mild tuning can get around it, so we could just be unlucky that nature didn’t give us that hint. Flavor is the biggest constraint on technicolor/RS, just as much as on SUSY, if not more so (indeed, the most serious attempt I know to really confront flavor in technicolor is probably Markus Luty’s work on using SUSY to solve the problems!).

Anyway, let’s be patient and wait for more data and more thorough analysis of existing data. The LHC is doing amazing things, but so far we’re only seeing the first-pass results coming out. A lot more is on the way, and I for one hope for surprises….

13. **Jeff**
August 29, 2011

Matt Strassler describes in more detail his position on funding concerns, as written on the comments section of his webpage (http://profmattstrassler.com/2011/08/19/current-lhc-data-and-supersymmetry-is-supersymmetry-in-trouble).

He says:
“… a 9 billion dollar experimental facility’s long-term future [is] on the line. There are consequences to making incorrect or overoptimistic statements to the press. In my view, we need to be quite precise about what we say; the public, and the politicians that provide the money, want to know what is going on, and we will pay a price if we say one thing and then have to backtrack. And if we disagree with each other, that’s fine; it is best that the public and politicians know that there is no scientific consensus yet.”

14. **DB**
August 29, 2011

@Jeff
Human nature being what it is, I expect experimentalists and SUSY advocates to come closer together and not engage in the dogfights that Strassler fears. Why?, well, there is only one positive outcome on the cards and it plays to the interests of both camps: a discovery of a “low” energy Higgs in the 115-128Gev range. No Higgs and a simple confirmation of the standard model would be a disaster for both camps: the premature shutdown of the LHC and all that that would entail.
A low energy Higgs discovery – the only one still just about on the cards – keeps SUSY alive as the search strategies are changed to hunt for every conceivable variation in the 100+ parameter SUSY universe, and so stretch out the life of the LHC.
Bad news for those who would hate to see experimental physics go down the road of hype and false promise, but hey, folks gotta eat. And the alternative looks like no machine at all.
But lurking always in the background is the data from precision electroweak data which points to a Higgs of c.72 Gev, already ruled out by LEP2. One could argue with some justification that the LHC was built on the basis of a solid dose of hype all of its own. Tick tock, tick tock.

15. **Peter Woit**
August 29, 2011

Matt,

Many thanks for the helpful explanations about this.

16. **Maury Markowitz**
August 29, 2011

DB:

"And the alternative looks like no machine at all."

That's true, but the "no machine" case was either going to happen at LHC or the one after it. SSC's cancellation wasn't entirely surprising in that respect, and that LHC is alive is a testimonial to the very interesting funding system CERN's arranged (took notes from NASA it seems 🙂

Looking to the future, I suspect that this is the last new high-end accelerator I'm likely to live to see. There's nothing particularly interesting beyond LHC except "bad" higgs, and I suspect that funding for such a machine is unlikely to be forthcoming.

But on that note, I don't think I'll live to see man on Mars either, so maybe I'm just a spoilsport. And who knows, someone might get a surfatron working!

Peter:

Darn, I was hoping I simply didn't understand the issues well enough. I'm always looking for an excuse to pick up a new read.

17. **Bobito the Payaso**
August 29, 2011

Perhaps soon deans will count twitters when deciding on promotion.

18. **AlBme**
August 29, 2011

I can see how some researchers might be concerned over future funding over an idea that may very well fail to produce empirical evidence. On the other hand, it's an opportunity for new ideas to be considered and seriously examined. These scientists shouldn't need to be reminded that even a negative experimental result advances our knowledge of nature. The LHC is producing more data probably than any other experiment preceding it. If they bother to look, I'll bet they'll find plenty that isn't explained completely by even the Standard Model. From that, new ideas can be proposed and examined.

19. **Bernhard**
August 29, 2011

I can´t access Peskin´s talk. I´m trying it for days already but just loads forever and nothing.

20. **Peter Woit**
August 29, 2011

Bernhard,

The talk is there, it's about 6.5 MB, although downloading and opening in my firefox browser was slow and sometimes flaky. One might want to try a different browser…

21. **Jack Levitt**
August 29, 2011

Let's wait to hear what Witten has to say before we panic, okay?

22. **anon**
August 29, 2011

I can't help but feel that SUSY is in a deeply oversold condition at the moment. I'd be tempted to buy shares in anticipation of a short-term snapback rally in the fall.

23. **null**
August 29, 2011

Would not finding a Higgs rule out SUSY?

24. **Peter Woit**
August 29, 2011

null,

No. SUSY also has a Higgs, actually at least two of them. One question everyone will be asking once a Higgs is found is whether it is a SM Higgs or a SUSY Higgs.

25. **Marcus**
August 29, 2011

Giotis' first comment (with the snide omitted):
"why these results are good news for LQG? Did LQG make a prediction about SUSY at LHC that I'm not aware of? "

Yes and you are evidently not aware of it. Finding signs of SUSY would be a considerable setback for the Loop researchers. It would make it incumbent on them to incorporate SUGRA which would mean back to the drawing board for some.

The current LQG does not do supergravity. This was pointed out by Thomas Thiemann at the recent loops conference in May, where he presented a seminal paper on how to formulate LQSG. He discussed the problems to be overcome. (Also with extra dimensions.) The motivation he gave was basically to have "insurance". Suppose the LHC detects signs of supergravity and or extra dimensions, just in case let's see how the theory could adapt.

One would presumably have to abandon the results of the past few years–I'm guessing it might amount to going back to how things were around 2005 or 2006.

Thiemann and two of his PhD students have come out with several papers on this subject in the past 3 or 4 months.

So from a practical standpoint it is certainly good not to see SUSY and this is a simple observation, not "gloating", which any one familiar with the situation can make.

Here are 3 papers if you want to read up:
1. arXiv:1106.1103 [pdf, ps, other]
Towards Loop Quantum Supergravity (LQSG)
Norbert Bodendorfer, Thomas Thiemann, Andreas Thurn
Comments: 12 pages
Subjects: General Relativity and Quantum Cosmology (gr-qc); High Energy Physics – Theory (hep-th); Mathematical Physics (math-ph)
2. arXiv:1105.3710 [pdf, ps, other]
Towards Loop Quantum Supergravity (LQSG) II. p-Form Sector
Norbert Bodendorfer, Thomas Thiemann, Andreas Thurn
Comments: 12 pages
Subjects: General Relativity and Quantum Cosmology (gr-qc); High Energy Physics – Theory (hep-th); Mathematical Physics (math-ph)
3. arXiv:1105.3709 [pdf, ps, other]
Towards Loop Quantum Supergravity (LQSG) I. Rarita-Schwinger Sector
Norbert Bodendorfer, Thomas Thiemann, Andreas Thurn
Comments: 43 pages
Subjects: General Relativ…

26. **Chris Austin**
August 29, 2011

Matt Reece, thanks for the clarifications. Could you comment on Lunghi and Soni's claim that "the CKM-paradigm now appears to be in serious conflict with the data"?

27. **Shantanu**
August 30, 2011

So Marcus, how then does LQG address the dark matter problem? or is the dark matter have nothing to do with electroweak scale according to LQG?

28. **Giotis**
August 30, 2011

Marcus,

It is widely accepted that LQG is not incompatible with SUSY/SUGRA and the papers you mentioned just prove this point. This is the reason why no one in this field ever said that finding SUSY at LHC would mean that LQG is wrong.

No, Rovelli is not gloating due to these technical implications you mentioned. He is gloating because he believes that not finding SUSY at LHC will be a blow for String theory at least at psychological level and LQG (as a rival) will benefit from that.

LQG has nothing to say about physics at LHC; it can't even derive GR at the classical limit.

29. **Peter Woit**
August 30, 2011

All,

Please, enough LQG/string warfare, this really isn't very relevant to the topic of SUSY.

30. **Marcus**
August 30, 2011

Shantanu and Giotis,
My reply to your recent comments addressed to me is here:
http://physicsforums.com/showthread.php?p=3476917#post3476917
I responded in some detail. You are cordially invited to continue discussion there, if you wish.

31. **null**
August 30, 2011

null,

No. SUSY also has a Higgs, actually at least two of them. One question everyone will be asking once a Higgs is found is whether it is a SM Higgs or a SUSY Higgs.

PW, to clarify, my question was what would be the ramifications to SUSY and SUSY extensions of the SM like MSSM if future LHC results rule out Higgs in the 115-145 range? So instead of finding 1 as predicted in SM or 2 as predicted in MSSM, LHC rules out the Higgs.

32. **Peter Woit**
August 30, 2011

null,

The main thing the LHC is looking for is a SM Higgs, with only one unknown parameter. If that is ruled out, it will mean only that that specific theory is ruled out. SUSY extensions of the SM contain something like a SM Higgs, as well as other Higgs fields. There are more parameters, so completely ruling out SUSY Higgs fields is trickier, but they need to have something that plays the role of the SM Higgs, so you should be able to see that.

This still wouldn't rule out the general idea of SUSY though, just SUSY extensions of the SM (ruling out Higgs, you've ruled out the SM). Whatever replaces the Higgs field and causes electroweak symmetry breaking, you can imagine a SUSY extension of that new theory existing. So, no, ruling out the Higgs does not rule out SUSY in general.

33. **null**
August 30, 2011

You answered my question thanks.

34. **Shantanu**
August 31, 2011

Peter, another article in New scientist

35. **Shantanu**
August 31, 2011

Sorry the url is
http://www.newscientist.com/article/dn20838-dont-panic-about-the-missing-higgs–for-now.html?DCMP=OTC-rss&nsref=online-news

36. **Bernhard**
August 31, 2011

Here is an article on a perhaps not most reliable magazine for scientific matters, but anyway, it´s interesting to see the SUSY hype is now going "counter stringwise":

http://www.pcmag.com/article2/0,2817,2392094,00.asp

37. **Peter Woit**
August 31, 2011

Thanks Bernhard. The comment section of that article is priceless…

38. **SpearMarktheSecond**
August 31, 2011

Perhaps the most impressive thing is we aren't replaying the mid-1980's, when UA1 (mostly) and UA2 (a little) had false SUSY signals for a while, which were really background. CMS and ATLAS are way better run with better background estimates.

The flavor sector constraints on SUSY have long been quite strong… Haim Harari among others long emphasized this point. But contemporary SUSY models all were engineered to evade the constraints from the lighter flavors, although the Bs results keep squeezing the parameter space more.

In the early 1990s all the greats testified to Congress that LHC at 14 TeV couldn't make a discovery if the Higgs got heavy… therefore the SSC with 40 TeV in the center of mass was mandatory. Although we've had an interregnum where the Higgs has been light, with good justification, maybe the multiple blurry pictures from loops of the Higgs were over-interpreted. And so the SSC scenario might be coming true.

For sure the SUSY and extra dimensions stuff was oversold. But when Columbus argued for his ships, he portrayed the riches of Asia, which he never actually obtained. So the real point is the machine builders at CERN and in the old days at the SSC CDG knew they were shooting for the highest energies possible for pure exploration. Mel Schwartz was so clear in his testimonies on this point. The outer circles of HEP experimentalists and theorists pitch SUSY and extra dimensions.

Perhaps the deeper point is… if nothing knew is seen at the LHC, even no Higgs, what next? Give up? Or build the next hadron collider in…. China? In Texas under President Perry? But a linear collider doesn't seem wise.

39. **Peter Woit**
August 31, 2011

SpearMarktheSecond,

I'm also surprised that we haven't had some mistaken "discoveries" of supersymmetry so far.

Sometime soon I should write about the question of the "next collider". I don't think extra dimensions and supersymmetry can be used to sell it if they don't show up at the LHC. Unless the LHC comes up with something that falsifies the SM, the case for a new machine is going to be difficult. Best bet I think is no Higgs, but some alternative origin of electroweak symmetry breaking. The argument has always been that the LHC is a "no-lose proposition", either it finds the Higgs or has to find something else. We're going to find out if that argument was right…

In practical terms, the most viable higher energy "next collider" seems to be a revamped LHC with higher field magnets, more than doubling the energy. But even for that, the case won't be easy to make unless the LHC gives some reason to believe that there's something to be seen at 33 TeV that can't be seen at 14 TeV. Maybe that old testimony needs to be dusted off…

40. **SpearMarktheSecond**
August 31, 2011

I think the old argument was that W's and Z's become strongly interacting under a very heavy Higgs… so you start getting W-W- hard scattering, and jets of W's and Z's like we now have jets of quarks and gluons. The weak become strong.

Good enough to justify $30 billion or something like that in today's dollars? Don't know.

Maybe an LHC upgrade is the only way to go. We've seen how hard even getting to 7 TeV in the center of mass is. But last time I checked, Europe had some money problems too. Maybe China and India can become full member states at CERN…

41. **Lee Brown Jr.**
August 31, 2011

The argument has always been that the LHC is a "no-lose proposition", either it finds the Higgs or has to find something else.

Maybe it's a bit premature, but you addressed precisely what I was wondering, "Is it possible for the LHC to find nothing?" How would physics move forward?

42. **Bernhard**
August 31, 2011

Lee Brown Jr.,

Of course this is possible, although many of us hope it won´t happen, me included.

Other fields of physics will move forward for sure, just not sure how particle physics would without new experimental evidence. Would be near to impossible to justify building another billion dollar accelerator, so in my opinion the only hope in this scenario would be to wait for technological advance to a point that to create TeV collisions you don´t need a 27 km accelerator and expensive magnets. And this possibility is in a really distant future.

43. **SpearMarktheSecond**
August 31, 2011

I think the advocates of the SSC circa 1990 most definitely argued that the LHC could find nothing. That's how the justified 40 TeV in the center of mass for the SSC… by saying that energy was necessary to confidently project observation of the new physics deemed necessary if no Higgs (or SUSY) were detected.

As far as I remember, that physics is, at high momentum transfer, W's and Z's behaving as though they had effectively strong interactions, due to new effective terms in the lagrangian necessary to forestall infinities.

Back in 1990 there was a fair amount of skepticism about achieving the luminosities now actually achieved at the LHC. Center of mass energy seemed like the bet more reliable than luminosity. So unless the Higgs pops out between 115 GeV and 145 GeV, a lot of scrutiny of the LHC's ability to see high Q^2 W/Z scattering will happen… likely interesting info is already buried in all the studies.

Maybe the achieved high luminosities prove the early 1990's arguments wrong.

If no Higgs, too hard to tell if LHC will be the high water mark of HEP, like the Apollo program was for NASA. Could be, but there are huge differences…

44. **HoldYourHorses**
August 31, 2011

Guys, the LHC was designed to run for *decades*, and after one year with no superpartners everyone is losing their minds. I get the impression people are thinking "so the LHC had its shot, found nothing, so what's next". This is nonsense.

People used to think in the 80s the top quark would be found at 30 GeV, then it was "always around the corner" until it was discovered at 175 GeV in the nineties. The moral is that we (and I speak as a theorist) are actually quite bad at predicting what nature will do.

45. **Model and Sim**
August 31, 2011

I think the what happened to SSC will go down as one of the great tragedies of our era. Its still too early to tell with LHC, but I agree with Peter that if we don't see something it will be very difficult to go to the next level. The margins simply are not there to justify such a large expenditure. High energy searches in space might be the only option in the near future.

46. **younghun park**
August 31, 2011

It is possible to think the next collider without finding something valuable at LHC
in the case of no money problem in Europe and USA. Now, Europe and USA have big money problem. Other people won't agree to bulid the expensive collider.
So, our friends in CERN have to find higgs or something else.

47. **Peter Woit**
August 31, 2011

HYH, Model and Sim,

There are two distinct issues here. The first is supersymmetric extensions of the standard model, which never had much to recommend them. Here the on-going piling up of negative evidence is finally causing believers to rethink, and that process will just continue for the next few years. To the extent that these were used as a big selling point for the LHC, that was a decision bound to incur some future cost, we'll see how large. Even more so for extra dimensional models, which virtually no one takes seriously. Using these as part of the LHC sales pitch was guaranteed to cost something in credibility later.

The real issue is the Higgs, and there the time-frame for something interesting happening is months not years, with evidence for or against a SM Higgs likely to start showing up soon, although the current situation is highly ambiguous. If there is a Higgs, the sales job for a new machine is going to be about the importance of being able to study the thing, with a serious problem the argument that "the SM Higgs theory seems to work, why spend lots of money checking it?" If there is no SM Higgs, there's going to need to be a serious effort to explain to the public why this is a major discovery, opening up whole new areas of research. There will undoubtedly be a lot of analysis of issues like the W/Z scattering one explained by SpearMarktheSecond. Unfortunately, absent the kind of breakthrough mentioned by Bernhard, viable options for making progress are limited (in particular, going into space doesn't help). Increasing the LHC energy with new magnets looks like something that could address the physics issues, at tolerable cost.

Anyway, I'm guessing that by next year it will be clear which way this is going to go with respect to the Higgs.

48. **null**
August 31, 2011

PW,
"The real issue is the Higgs, and there the time-frame for something interesting happening is months not years, with evidence for or against a SM Higgs likely to start showing up soon, although the current situation is highly ambiguous. "

do you have any idea how soon the Higgs issue will be decided at current rate of data accumulation?
It's my understanding the recently published exclusion of 145 gev and above was based on an accumulated 2 fb-1 data set? 95% confidence

Is 95% exclusion really a strong exclusion? I am aware that "discovery" is 5-sigma, so exclusion is a lower 2-sigma?

1-how many fb-1 per month is being collected and analyzed by LHC
2- how many fb-1 to find evidence or rule out Higgs in the 115-145 range? 95%

Is a 95% exclusion of Higgs in 115-145GEV acceptable to researchers to give up on the Higgs or is it more stringent? If excess events are reported at 3-sigma at 115GEV, this would be reported as a possible Higgs discovery?

49. **Peter Woit**
August 31, 2011

null,

The LHC will run with protons until the end of October. Estimates are that they'll accumulate 5 inverse femtobarns in each experiment. Latest public release of data was based on 1-2 inverse femtobarns (and no combination of data from the two experiments). There are old projections made last year about how much data it would take to exclude at 95% level all the way down to the LEP limit, I believe it was about 5 inverse femtobarns or so.

Yes, typical standards for exclusion are different than for a discovery claim. If the Higgs isn't there, there seems to be a good chance of having a 95% exclusion result, possibly using both experiment's data. If it is there, the 5 sigma standard may not get met, but a 3-4 sigma signal would start making people think something was there.

Anyway, we'll see what the data says, sooner or later….

50. **null**
August 31, 2011

PW
you answered my question. What happens after October? Do they shut down or switch to heavy nuclei, or retool for 4TEV (8 TEV total)?

Does combining the data from both channel change results?

How soon after they collect 5 fb-1 will they report their finding?

51. **Peter Woit**
August 31, 2011

null,

They switch to heavy ions in early November. After a shutdown, early next year it's back to protons, with an energy increase possible depending on the results of measurements made during the shutdown.

In principle the data gathered by the two experiments is independent, so it should make sense to combine it and improve the statistical significance of any signal. As far as I know, they have a mechanism in place to do this kind of combination, and I don't see why they wouldn't do it, after each experiment has independently reported its own combined result. I have no idea what the timing of this all will be. End of year? Earlier if one of the experiments feels they have a convincing signal and wants to announce it?

52. **Paolo Valtancoli**
September 1, 2011

I remember that Hawking doesn't believe to the Higgs boson, but believes in supersymmetry and M-Theory. I think that the idea of supersymmetry will survive for many years to come even though LHC finds absolutely null results.

53. **Peter Woit**
September 1, 2011

The latest projections about the Higgs are consistent with my comments above, and given in detail here:

http://indico.cern.ch/getFile.py/access?contribId=54&sessionId=13&resId=1&materialId=slides&confId=141983

54. **chris bolger**
September 1, 2011

Can the W+, W- and Z be considered as supersymmetric to the e+, e- and neutrino. If not, why not?

55. **Eric**
September 1, 2011

Chris,

The W's and Z are in the adjoint representation of SU(2), while the electrons and neutrinos are in the fundamental representation. Supersymmetry does not act on the group representations, and so there is no way for the W's and Z's to be transformed into e's and neutrinos by supersymmetric transformations.

56. **null**
September 1, 2011

"On the topic of this posting, he writes:

Before the start of LHC, I expected early discovery of supersymmetry in the jets+MET signature. Many other theorists also had this belief. But, it was not correct.

and he explains why this was (large amount of fine-tuning required if superpartner masses are even as large as 1 TeV). He also explains possible ways to construct SUSY models that evade current experimental bounds while keeping superpartner masses relevant to the fine-tuning problem from getting much too large."

What is the upper bound for SUSY masses while keeping superpartner masses relevant to the fine-tuning problem , and is it within LHC full design energy 14TEV, or does it evade it?

57. **Peter Woit**
September 1, 2011

null,

The problem is that there is no well-defined point at which the necessary fine-tuning is "too much". 1%? .1%, .01%???

Many people did argue before the LHC was turned on that the LEP and Tevatron bounds on superpartners (and bounds on the Higgs mass) already forced a degree of fine-tuning that made supersymmetry unlikely. In simplified terms, the scale of electroweak symmetry breaking is of order 100 GeV, and if supersymmetry breaking is responsible for this, it should be of the same order, whereas this is ruled out by Tevatron/LEP bounds.

The problem for SUSY enthusiasts is that bounds are now moving up to 1 TeV and above. They have to either give up on the fine-tuning argument, or say that 1 TeV is not a problem. Once you say 1 TeV is not a problem, there's no reason 2, 3, 5 TeV should be a problem, so it really doesn't matter how much the LHC bounds get pushed up.

The argument Strassler and some others are trying to make is a bit different. They argue that until you rule out all the different kinds of signatures (not just the simplest ones) you can get from different patterns of masses, you really haven't ruled out SUSY below 1 TeV.

58. **Bernhard**
September 1, 2011

Yes, some people are definitely giving up on the fine-tuning problem as SUSY motivation:

http://www.math.columbia.edu/~woit/wordpress/?p=3479&cpage=1#comment-81529

59. **Eric**
September 1, 2011

Peter,

Low-energy supersymmetry was formulated to stabilize the Higgs mass against radiative corrections, rather to provide an explanation of EWSB as you state above. Thus, there is absolutely no reason to expect that the superparner masses should be the same as the electroweak scale ~100 GeV. In order to make the Higgs radiatively stable, the superpartners only need to be of the order of a few TeV, not 100 GeV. When you quote this number, it gives people the wrong impression about the current degree of fine-tuning necessary for supersymmetry to solve the hierarchy problem.

60. **Model and Sim**
September 1, 2011

Thanks for the clarification. I must defer to your better opinion on the matter. I suppose null results for luminiferous aether in 1887 did lead to a revolution in physics, although it all seems a little discouraging at the moment.

61. **Marcus**
September 2, 2011

I'm not a physicist; I'm a professor of philosophy, and all I know about this stuff is the (considerable) amount of pop physics I've read, blogs like this one, and a tiny bit of online self-education. So, having drawn attention to my ignorance, I have a simple conceptual question that has been bugging me. The question is this: how is it even coherent to say (1) that the SM Higgs is supposed to give other particles their mass, and then say that (2) the SM gives no account of gravity (which is true), when we also know that (3) an object's mass is what generates its gravitational field. This looks plainly incoherent. If the SM cannot account for gravity, how COULD there be a SM Higgs (given that gravity is a matter of something's mass)? I'm sure I'm making a mistake somewhere, but where?

62. **SpearMarktheSecond**
September 2, 2011

Marcus… the Higgs gives certain fundamental particles their mass. But there are other sources of mass… most of the mass of our kind of matter is caused by energy in the gluon field between the quarks in neutrons and protons, which is not the Higgs at all. However, the masses of neutrons and protons are not quite as fundamental as the masses of neutrinos, or electrons, or quarks (the fermions), and it has been the fundamental questions that have attracted people.

Classical gravity doesn't care what the origin of the mass is… given some mass of any origin classical gravity can take it from there.

Of course everyone would like a complete unified quantum theory of it all, but we don't have that yet…. or perhaps we have too many and don't know which to pick . In any case it is unsettled.

On the no lose theorem… here is an old paper in favor of the SSC and against a center of mass energy lower than 20 TeV…

http://www.osti.gov/bridge/servlets/purl/6397394-oQaPRo/6397394.pdf

63. **Florian**
September 3, 2011

I don't particularly like SpearMarktheSecond's explanation to Marcus's question, so I offer another one.

Mass is not only associated to gravitation. Mass also measures how inert a body is. The masses one talks about in the context of the Standard Model refer to this understanding of mass.

Well, actually the masses in the SM are just parameters in the theory. We call them masses, because in some experiments particles appear to be inert (e.g. electrons in a synchrotron). Now if you want to formulate the SM in a certain way (gauge invariant), it seem to be necessary to introduce one more particle – the Higgs boson. (Otherwise putting masses for some particles in by hand would destroy gauge invariance.)

64. **Igor Khavkine**
September 3, 2011

@Marcus: There is a hidden equivocation in your logic on the term "mass". Physicists can easily spot the change in the meaning of that word from context, as partially explained by SpearMarktheSecond.

In general, *mass*, *energy* and *momentum* are distinct but related concepts and quantities. When used colloquially, the term "mass" may stand for any one or combination of them. Here are some different context where it can appear.

Inertial mass is an empirical notion which can be determined from experiments comparing the force acting on on object and its acceleration. Inertial mass is combined in a specific with an objects energy and momentum into an *energy momentum tensor* (which just reduces to gravitational mass in Newtonian gravity), which is entirely responsible for the object's interaction with gravity in General Relativity. For a *composite* object, its inertial mass is determined in a specific way from the energies, momenta and inertial masses of its *constituents*. For *elementary* (non-composite) particles, the inertial masses are determined in a specific way by parameters that enter into the mathematical formulation of the Standard Model. These parameters can be roughly split into two classes: the *particle masses* and the *interaction strengths* (aka coupling constants). Oddly enough, the Standard Model sets all particle masses (except for the Higgs boson) to zero. This means that the inertial masses of the elementary particles that we observe are entirely due to the strength of their interaction with the Higgs boson. In particular if a certain property of the Higgs boson known as its *vacuum expectation value* increases by a factor of 10, the inertial masses of all other elementary particles universally increase by 10 as well.

It is in the following sense that the Higgs boson gives other particles their masses: the strength of the interaction of an elementary particle with the Higgs boson determines its inertial mass. Since inertial mass does not necessarily involve gravity in its definition or measurement, the points (1) and (3) in your reasoning are actually logically disconnected.

65. **Anon**
September 3, 2011

Could someone explain to this naive reader what is keeping us from doing these kinds of searches with high energy cosmic rays, which commonly have much higher energies than those achievable at the LHC? Not enough supply?

66. **Peter Woit**
September 3, 2011

Everyone,

Enough about mass, this isn't a forum for general physics discussion, I can't moderate such a thing. As general advice to anyone trying to learn more about basic topics in physics, internet comment sections are not a good place for this. You're much better off getting hold of a real book, with equations, where someone competent has put a lot of effort into writing something long, careful, with equations. This is what you need to understand a basic concept like mass.

Anon,

For short questions like yours, you can get the answer from a comment section, although I really wish people would stick to the topic of the posting. Again, a general discussion area is too hard to moderate. In this case though, the simple answer is luminosity. At the LHC you have many high energy collisions every nanosecond and you may need a year's data to get what you want. If you instrument some area of the earth or space and wait for LHC energy collisions, they will be very rare events.

67. **Bernhard**
September 4, 2011

Seems the focus for the Higgs mass searches are now between 114-145 GeV by all accounts, but Bill Murray´s slide 6 points to the possibility that in fact its mas could be say 600 GeV. I thought this was almost excluded by electroweak measurements. How much not excluded is this > 460 GeV region? And of course, what does it mean for SUSY? Are there any SUSY models floating around with such a heavy Higgs?

68. **Eric**
September 4, 2011

Bernhard,

A superheavy Higgs (m> 600 GeV) would be natural in a four-generation SUSY model, since in such a case the Higgs receives large radiative corrections.

Knocking on Heaven’s Door

September 4, 2011
Categories: Book Reviews

Lisa Randall’s new book is about to come out, it’s entitled Knocking on Heaven’s Door: How Physics and Scientific Thinking Illuminate the Universe and the Modern World. It turns out that it’s really two books in one, both of which are much better and more clearly written than her previous effort, Warped Passages. One reason might be help from well-known novelist Cormac McCarthy who is thanked (together with Lubos Motl) for extensive feedback during the book’s writing.

One of the books here is not surprising, it’s somewhat of an update of the earlier book, emphasizing the story of the LHC. This includes a very detailed explanation of the history of the LHC and how it works, together with a wonderful and clear examination of the design of the ATLAS and CMS detectors, as well as the physics they are looking for. All in all, this is about the best popular-level explanation of what is going on at the LHC that I’ve seen, up-to-date as of a few months ago.

The second book inside the book is of much wider scope. Randall’s prominence as a scientist has brought her into contact with a wide range of people (Bill Clinton’s endorsement is on the book’s cover), including artists, government officials, financiers, technologists, and a wide range of thinkers of different sorts. She has taken on the role of a public face of physics, and has written a book which is in part a very general defense of science and the materialist, rationalist world-view that modern science is based on. Her experiences with non-scientists are reflected in how she writes about a range of topics, including the notion of beauty in science, the question of how to analyze risk, the relation of religion and science and much more. Her discussions of these topics are uniformly sensible, although rather conventional and unsurprising.

In the end though, the book left me somewhat uncomfortable. Understandably, Randall is overly enthusiastic about the prospects of Randall-Sundrum models, describing them as “an idea that probably stands as good a chance as any of being right” (most theorists would assign a much higher probability to SUSY). She writes that, if correct, the LHC is expected to see KK gravitons at a mass of around 1 TeV. Recently limits on masses of such particles have been pushed up to nearly 2 TeV. These extra-dimensional models were considered interesting but not especially plausible by most theorists pre-LHC. Like SUSY, they’re starting to be ruled out by the LHC, a process which may take a while until their defenders finally admit that the expected signals just aren’t there.

The time period of Randall’s career roughly corresponds to my own (she’s a few years younger), and, as she acknowledges, her field of model-building throughout this career has been dominated by string theory-inspired SUSY and extra dimensional models. These were never very convincing, and they are now biting the dust. From the experimental side, this is an inspirational story of the triumph of the scientific method and the huge achievements of the LHC machine and detectors, but from the theoretical side, the story of this period is darker and much less inspirational. It’s not something that makes the best topic for a defense of how science is conducted.

One odd thing about the book is the title, which for Randall carries a positive meaning that she acknowledges doesn’t correspond to the very dark one of the Bob Dylan song from the soundtrack of the Sam Peckinpah film. It’s a beautiful song, but one not about finding truth, but about getting shot in the gut and facing death, hopefully not relevant to particle physics in the LHC era:

> Mama, put my guns in the ground
> I can’t shoot them anymore.
> That long black cloud is comin’ down
> I feel like I’m knockin’ on heaven’s door.

Comments

1. **Proudmemberofthecult**
   September 5, 2011

   > Like SUSY, they’re starting to be ruled out by the LHC, a process which may take a while until their defenders finally admit that the expected signals just aren’t there.

   “Science advances one death at a time”: Models only ever really go away once their proponents have died.

2. **Bernhard**
   September 5, 2011

   “but about getting shot and facing death, hopefully not relevant to particle physics in the LHC era”

   Well, getting shot and facing death is basically what will happen with all this extra dimension models…

3. **Henry Bolden**
   September 5, 2011

   Peter, you’re wrong about string theory not being good for anything: it’s good for making money by selling popular books to the public. Fiction writing is still a profitable business for a select group of authors. Brian Greene and Lisa Randall are essentially part of the entertainment industry. Look for more cameos on “The Big Bang Theory”.

4. **Hamid Gupta**
   September 5, 2011

   This is a reply to Henry Bolden’s comment.

   Why you should not expect a Lisa Randall cameo on “The Big Bang Theory”.

   It has been alleged that Dr. Elizabeth Plimpton character in the “The Big Bang Theory” episode “The Plimpton Stimulation” (Season 3, number 21) is modeled on Lisa Randall. If there is any truth to this rumor then expect lawsuits rather than cameos. The fictional character is a Princeton lady cosmologist, not from Harvard, so perhaps that’s how the producers of the show might defend themselves against possible legal action. One person connected to the show allegedly said that the original script mentioned “Plimpton-Humdrum models” but this was deleted on advice by lawyers. Of course all this is probably just more internet disinformation only slightly more likely to verified than the physics of a Randall-Sundrum model.

5. **Mitchell Porter**
   September 5, 2011

   Lisa Randall already appeared on The Big Bang Theory.

6. **abbyyorker**
   September 6, 2011

   Do y'all watch TBBT? I watched one episode and it was terrible.

7. **Peter Woit**
   September 6, 2011

   abbyyorker,

   I'm a fan. Any TV show with a main character pretty much based on Lubos is bound to be a hoot.

   Sorry though, discussing the pros and cons of TV shows is really off-topic here, the discussion is fluffy enough as it is…

8. **Anne Marie Thomas**
   September 7, 2011

   Given that string theory and multiverse theories have slid from speculative physics into a form of entertainment, perhaps the discussion of TBBT is justified. By the way, which episode of TBBT features a guest appearance by Lisa Randall? I couldn't find her in Wikipedia's list of cameo appearances for the show:

   http://en.wikipedia.org/wiki/List_of_The_Big_Bang_Theory_characters

   Now, if we could only get a Peter Woit cameo on the show, that would be way cool. Readers of this blog lobby the producers for it!

9. **martibal**
   September 9, 2011

   Anne Marie: great idea ! Peter on TBBT!!
   How can we lobby the producers for it ?

10. **Sebastian Thaler**
    September 12, 2011

    Peter,

    Speaking of interesting new books, you might like the upcoming title PHYSICS ON THE FRINGE by Margaret Wertheim. Most of her book is devoted to the motivations of crank physicists and their bizarre theories, but chapter 11 includes a bit on string cosmology and multiverses. I'm pretty sure the string community will not be happy to see itself mentioned in a book like this…

11. **Peter Woit**
    September 12, 2011

    Thanks Sebastian,

    I look forward to seeing that book. Over the years I've received large numbers of documents from cranks. It has struck me that it's unclear how to distinguish a lot of multiverse papers by respectable scientists from the crank literature.

12. **Ulrich Mohrhoff**
    September 21, 2011

    "These were never very convincing, and they are now biting the dust." So why do people keep doing string theory? I found a possible answer in the April 2011 issue of *Discover* Magazine, in an interview with Lynn Margulis :

    > Population geneticist Richard Lewontin gave a talk here at UMass Amherst about six years ago, and he mathematized all of it — changes in the population, random mutation, sexual selection, cost and benefit. At the end of his talk he said, "You know, we've tried to test these ideas in the field and the lab, and there are really no measurements that match the quantities I've told you about." This just appalled me. So I said, "Richard Lewontin, you are a great lecturer to have the courage to say it's gotten you nowhere. But then why do you continue to do this work?" And he looked around and said, "It's the only thing I know how to do, and if don't do it I won't get my grant money." So he's an honest man, and that's an honest answer.

Imagine There's No God Particle

September 5, 2011
Categories: Favorite Old Posts, Uncategorized

It's easy if you try (as John Lennon would say).

The LHC is back in business after a technical stop, getting ready to collide protons for the next couple months, perhaps reaching an integrated luminosity of about 5 inverse femtobarns. This is a factor of four higher than the luminosity used in most analyses that have been made public so far, and the latest projections are that this should allow an exclusion of a Higgs over the entire expected mass range at 95% confidence level, if such a particle really doesn't exist.

My pre-LHC predictions (see here) of five years ago have held up well, and nothing yet has changed my view that a Higgs particle scenario and a no-Higgs scenario are equally likely. The best argument for a Higgs in the mass range of 114-145 GeV is that it's the simplest way anyone has found of making the Standard Model work, and explains a range of precision electroweak measurements.

The best argument against the Higgs is that elementary linear scalar fields are problematic (since not asymptotically free) and esthetically displeasing (not geometrical and constrained by symmetries, so lead to lots of undetermined parameters, mainly for the Yukawas that determine the masses of all fermions). By analogy with the theory of superconductivity though, one can imagine that the Higgs makes a good low-energy effective theory (a la Landau-Ginzburg), even if there's a more interesting fundamental theory, which may require going to a smaller distance scale (a la BCS theory). As the allowed Higgs mass range has narrowed though, I'm starting to think that there may be something to the argument that it's implausible that the mass would end up being in the hardest mass range for colliders to examine. More likely it's just not there, and the hardest range is the last one to fall to experiment.

By the way, I was interviewed about this on a Wired podcast (see here), not sure how it turned out. I don't think I said anything surprising or controversial.

The imminent arrival of an experimental result deciding the issue of the SM Higgs has focused attention on what the implications will be, and here's what I've been thinking:

If the SM Higgs is found, there will be rejoicing at first at CERN and within the physics community, and an appropriately proud announcement to the public. Debate will begin on who gets the Nobel: experimentalists? which of the 6000+ people at LHC/CMS/ATLAS? or theorists? Anderson/Higgs/Englert/Brout/Guralnik/Hagen/Kibble, or ? I gather Brout is no longer with us, maybe this will have to wait until the list gets down to three by attrition. Probably the best case would be for Weinberg/Salam, but they already were rewarded for the SM. Maybe the Swedes could make Weinberg's a double. The LHC experimentalists would have an active research program for many years trying to measure the Higgs properties. Theorists though would face the gloomy prospect that these would just agree with the SM. We'd be stuck pretty much where we have been for thirty years: no clues as to how to do better than the SM.

What though if the SM Higgs gets ruled out? CERN may consider this an embarassment, but it's actually a far more exciting result, one even more worthy of the Nobel than finding the long-sought particle. SUSY enthusiasts will claim this means it's a SUSY Higgs, and model builders will get to work on constructing more complicated models designed to explain the result by making the Higgs even harder to see (Matt Strassler is starting to write about such models here). My guess would be though that no Higgs means the argument from esthetics was right, so adding in more scalar fields in some complex pattern isn't a very plausible explanation of the null result.

A commenter here pointed out that this possibility was discussed during the debate over the SSC, when it was argued that, in the case of no Higgs, you would need a 40 TeV machine to look at W/Z scattering, to get information about what was really going on. The LHC should be capable of quite high luminosity, which may compensate for its lower energy in such searches, see a recent discussion here.

My own very vague favorite idea has always been that, non-perturbatively, there's something important we're missing in our understanding of gauge symmetry in chiral gauge theories and that this may hold the secret to the mystery of electroweak symmetry breaking. While this idea has been a motivation for research I've been pursuing in recent years, I can't claim to have made any progress on it. My second real blog posting here was about this, back in 2004, leading to a torrent of abuse. Maybe if there's no Higgs, SUSY and extra dimensions are gone, this could become a legitimate question in the eyes of mainstream theorists.

> You-hoo-oo-oo-oo, you may say I'm a dreamer
> But I'm not the only one…

Update: It seems that I'm definitely not the only one inspired by John Lennon recently, with CIP beating me to this a while ago.

Update: On the topic of this posting, see Slava Rychkov's talk that just appeared on the arXiv. From the summary:

> We have seen many impressive new physics limits set at this conference. But, have we ever truly believed in the models that are being pushed away? Z-prime, CMSSM, split SUSY, to name a few? I myself certainly never believed in these. Take Z-prime. In spite of what you may have heard, this is a completely unmotivated extension of the SM. It solves nothing of its problems and has nothing to do with Naturalness. Same for split SUSY, anathema to Naturalness. CMSSM is the only victim on the list for which I feel sorry, but we can't give up on SUSY just because this straightjacketed version of it failed.
>
> Another early casualty has been the Large Extra Dimensions scenario. But again, this was hardly a bona fide solution to the hierarchy problem. The mechanism which cuts off the Higgs mass quadratic divergence has not been concretely specified. It's only because the idea was so original that we ever gave it the benefit of the doubt. Now with LHC limits on the (4+n)-dimensional Planck scale already a factor two above the Tevatron limits, it's basically gone. The truth is, apart from SUSY, there are only two other motivated scenarios for TeV-scale physics: strong EWSB and Composite Higgs. I mentioned some of the signals expected in these models. Unlike CMSSM, they typically require much higher luminosity to be seen.

Comments

1. **Paolo Valtancoli**
   September 5, 2011

   I studied in my thesis the quantization of chiral symmetry in gauge theory in presence of anomalies. For what I remember the main problem is that the anomalies ruin unitarity. Now that I have more experience with noncommutative geometry, it is pretty natural that anomalies, being nonlocal, enter in conflict with unitarity. Maybe one should try to extend the notion of quantization, by allowing that pure states go into mixed states, i.e. introducing a microscopic arrow of time in quantum field theory.

2. **Amitabha**
   September 5, 2011

   While it is not polite to plug one's own work here, I would simply like to mention that it **is** possible to get massive vector bosons even in the absence of a Higgs particle.

3. **chris**

September 5, 2011

Paolo,

t'Hooft has been working on this for over a decade now.

4. **ru**
September 5, 2011

So I read Matt Strassler's Higgs FAQ, and he says that whether or not there are Higgs particles, there is definitely a Higgs *field*, "essentially by definition".

Do you (plural) agree with this assertion?

5. **Thomas Larsson**
September 5, 2011

No Higgs: spontanous breaking of EW symmetry is wrong.
Dynamic symmetry breaking: no expert, but know that technicolor models are already in trouble, and a Cooper pair of technifermions should effectively look like a Higgs, so probably wrong.
Explicit symmetry breaking: ouch, too ugly!!!
So if all other modes of EW symmetry breaking is wrong, how could the electro-weak symmetry be broken. Easy, by symmetry breaking 🙂

6. **Marton Trencseni**
September 5, 2011

Can someone point to a good paper that discusses/re-examines the state of EW symmetry breaking assuming no Higgs is found? Thank you.

7. **DB**
September 5, 2011

"The best argument for a Higgs in the mass range of 114-145 GeV is that it's the simplest way anyone has found of making the Standard Model work, and explains a range of precision electroweak measurements. "

Let's put on our sceptic's hat for a moment.

The second argument is iffy at best. The Review of Particle Physics (Higgs Bosons: theory and Searches, May 2010, p.12) quotes the best global fit to precision electroweak data giving the mass of the SM Higgs at 87(+35/-26)GeV. So the heaviest an SM Higgs could be, and remain consistent with precision EW is 122GeV.

This tension has been around since LEPII ruled out Higgs below 114GeV leading to the waving of many hands in the interim, not least the "reworking" of the EW fits to "incorporate" the direct search result of LEPII.

The other problem posed by Higgs is the implication of the scalar Higgs field for cosmology. Under reasonable assumptions, the vacuum energy contribution of the Higgs field results in a cosmological constant fifty times larger than the one actually measured, contributing to some of the anthropic fine-tuning nonsense, notably that peddled by a certain Steven Weinberg as far back as 1987.

Another Nobel Laureate, Martinus Veltman, has been openly derided for expressing long-held scepticism towards the Higg mechanism. It will be some irony if he is vindicated, and a catastrophe for the HEP establishement which pitched the LHC to funding politicians on the basis that the Higgs was a slam dunk.

Back 2000, Nobel Laurate Martinus Veltman was on record expressing scepticism on the existence of the Higgs Boson.

8. **Yatima**
September 5, 2011

> a catastrophe for the HEP establishement which pitched the LHC to funding politicians on the basis that the Higgs was a slam dunk

Really, why? It's not as if people holding the purse strings were promised lucite blocks with an embedded Higgs that they could show off at home. Or antigravity devices to revive the (flagging, money-printing, laden-by-social-promises-that-can't-and-won't-be-kept, arbitrarily warfaring) national economies.

There was a decision by the respective national funding agencies to go ahead and contribute to a project for which the outcome was open-ended, maybe get some redistribution effects for industry, parking space for PhDs, work for university departments as well as the occasion to do international collaboration and speeches in front of worthy audiences.

LHC did a lot more than the international space station, and no-one is embarrassed about the 150 billion USD or so that went into it.

9. **Bernhard**
September 5, 2011

"Debate will begin on who gets the Nobel: experimentalists? which of the 6000+ people at LHC/CMS/ATLAS? or theorists? Anderson/Higgs/Englert/Brout/Guralnik/Hagen/Kibble, or ?"

From the experimental side, LHC collaborations as a whole should start getting Nobel prizes with CERN director going to Stockholm to get it, representing them, not simply give to the spokesperson of an experiment. The prize should go to the whole collaboration and although I agree 10M SEK is not really much and can't pay the collaboration electricity bill, it's the correc thing to do.

From theorists, tough call…

10. **Bernhard**
September 5, 2011

But to be clear, I believe it should be a shared prize between theory and experiment, I'm just not sure which theorist should get.

11. **Chris Oakley**
September 5, 2011

At first I detested the term “God particle”, feeling that it was merely an invention of those too ignorant to understand that a particle with zero rest mass could still exist. However, given the possibilities for humour and/or relating to popular culture I am slowly coming round to it.

12. **Igor Khavkine**
September 5, 2011

    @Amitabha, about your [arXiv:1107.1501]. It’s likely that your construction is equivalent to the Stückelberg mechanism, where gauge bosons are derivatively coupled to auxiliary scalar fields instead of 2-forms (2-forms in 4d are in fact equivalent to scalar fields [Weinberg, v.I, 8.8]). However, an analysis by Dütsch & Scharf [arXiv:hep-th/9612091] shows that a Higgs field is then still required if both *renormalizability* and *freedom from gauge anomalies* are required. They are probably not the first to get this result, but their analysis is quite careful. This makes me skeptical about the viability of your construction. 🙁

13. **DB**
September 5, 2011

    @Yatima
Perhaps catastrophe is putting it too strongly, but the HEP establishment will lose a great deal of credibility with funding politicians if the Higgs doesn’t turn up, or, at least, a discovery of comparable importance is not forthcoming within a reasonable timeframe. It was seen as a pretty safe bet and sold as one. CERN, don’t forget, has been outmanoevered by the US on more than one occasion, and the prestige to Europe that would accrue from discovering the Higgs is what these politicians are really paying out for, and the other benefits that you mention are viewed by them as mere cream on the strawberries.

14. **Igor Khavkine**
September 5, 2011

    A follow up on my last comment. It seems to me that the case for a Higgs-like mechanism is solidly backed up by the requirement of simultaneous *renormalizability* and *freedom from gauge anomalies*. According to Dütsch & Scharf, allowing the theory to be gauge anomalous eliminates the need for a Higgs field and results in a model that’s apparently due to Curci & Ferrari.

    Naturally, I wonder: is known whether a Higgs field is still needed if one drops the renormalizability requirement instead? After all, to quote Weinberg: “Non-renormalizable theories are just as renormalizable(*) as renormalizable ones.” Of course, he uses the term “renormalizable” in two somewhat different technical senses. I believe that the (*) sense is the more important one.

15. **Thomas Larsson**
September 5, 2011

    Btw, Brout will not receive a Nobel for Higgs. It is not awarded posthumously.

16. **bonk**
September 5, 2011

    DB: “The Review of Particle Physics (Higgs Bosons: theory and Searches, May 2010, p.12) quotes the best global fit to precision electroweak data giving the mass of the SM Higgs at 87(+35/-26)GeV. So the heaviest an SM Higgs could be, and remain consistent with precision EW is 122GeV.”

    That’s exactly why I think that ruling out heavier higgs mass at the LHC is *evidence* for the standard model, rather than a warning that something might be wrong. But somehow many people including Peter don’t share this opinion.

17. **anonomous**
September 5, 2011

    “The LHC is back in business after a technical stop, getting ready to collide protons for the next couple months”

    This is what I fear the most – not proving or disproving the existence of the Higgs but not getting enough data by the end of the year because of a major technical outage like the one in 2008. My experience is that Murphy’s law is much like the uncertainty principle – the less you pay attention to it the larger it’s momentum gets!

18. **frank**
September 6, 2011

    Peter,

    did you reread that 2006 comment of Lubos in your blog where he answers “B” saying that the LHC “must” find something, and that it was “againt logic” that it found nothing? He wrote that the LHC “had to” find something at 1 TeV (we are already beyond that). Lets wait till Christmas, and then he should get depressed while eating his own words.

    By the way, Lubos is now claiming in his blog that the possibility that nothing happens at 1 TeV scale “is in the literature since the 1980s” – a statement which is completely false. I claim to have read every review on the standard model since that date and have not found a single mention of this option. In contrast, the general, unanimous opinion is that standard model has to break down at 1 or 2 TeV. Now that the LHC is telling us otherwise, everybody who has ever written a review with a title such as “beyond the standard model” should get a red face.

    What would be even more interesting is to have a discussion on the arguments that led people claiming that the standard model is “incomplete” or even “wrong” at 1 or 2 TeV. There are a number of such arguments, and obviously they are all wrong. The whole issue shows that nobody from the quantum field theory “experts” who abused you so much in 2006, from Distler to Lubos to Srednicki, said anything correct about the limitations of the standard model from the time they are in charge of teaching about it. (Remember how they questioned your qualifications, as if truth in physics depended on the qualification of the messenger?) I wonder how these people are doing now.

19. **Amitabha**
September 6, 2011

    Igor, this discussion is not strictly on-topic, so Peter is unlikely to encourage its continuation, but I would like to mention that the recent paper is only a part of the story — you may want to look at Phys. Rev. D63 (2001) 105002 regarding renormalizability, and bring the discussion to email if you wish.

    And this is not the Stuckelberg mechanism — the two-form is not dual to a scalar. Weinberg’s statement about equivalence is not very useful when the fields have cubic or higher couplings — even though there is only one degree of freedom (per gauge index), which you can think of as a scalar, it is not possible to write the theory as a local field theory in terms of that single degree of freedom. But you probably knew that already.

If a theorem claims that a Higgs field or similar is `required', it usually assumes that all degrees of freedom appear as local fields in the action. Even though the two-form action is local, the actual degree of freedom does not appear in the action in a local combination. IIRC that is why these theorems are not applicable to this case.

20. **OhDear**
September 6, 2011

frank, your comment is way premature. The LHC has not shown there is nothing going on at the TeV scale. We are not "beyond" 1 TeV (only cMSSM and other constrained variants are ruled out). The time may well come for red faces among BSM physicists, but that time is not now.

21. **mark**
September 6, 2011

+1 to OhDear

All thats been ruled out are the simplest SUSY models surely? We are hardly at the point where you have to concoct absurdly contrived models to explain its absence (yet).

22. **Paolo Valtancoli**
September 6, 2011

I think that physicist should learn from the failure of LHC a simple pratical principle, i.e. that Nature is essential, nothing that you see is superfluous. That's why I am convinced that all the models proposing a river of extra particles to be detected have
a lot of chances to be wrong.

23. **Bernhard**
September 6, 2011

Peter,

I wonder if the Higgs is not discovered (and this seems more and more plausible) if people will shift the holy grail of physics research to electroweak symmetry breaking, as you have been discussing for years now. Personally I think it's also a fascinating but more tangible question, perhaps one we really need to answer properly before worrying too much with gravity. In any case, would be ironic to see the very same people who fought against you some years ago to engage in the activity you pointed out as the most important one… 😀

24. **Charles**
September 6, 2011

I think the big question is what if it is determined there is no "boson" (it could be a composite particle) but there is a mechanism? I am not sure that is being clearly stated or addressed here. Or I am not getting it (quite possible).

What does LHC do?
What does the Nobel Academy do? (PH gets a lot of credit for this "sentence" on the boson)
What happens to the SM?

Thanks.

25. **DB**
September 6, 2011

A curious PR release from the Tevatron to Reuters claims that it will be in a position "to rule out the existence of a Higgs boson with a mass within the most likely range" before is shuts down for good on September 30th. Is this a last attempt at an end-run around the LHC:? "we discovered the non-existence of the Higgs before you did"!

http://www.reuters.com/article/2011/09/05/us-science-higgs-idUSTRE78445C20110905

Of course, the phrase "with a mass within the most likely range" can be interpreted many ways.

26. **AcademicLurker**
September 6, 2011

CERN, don't forget, has been outmanoevered by the US on more than one occasion, and the prestige to Europe that would accrue from discovering the Higgs is what these politicians are really paying out for

Hasn't Europe in a sense already won on the prestige issue? With the U.S. powering down its colliders, CERN is now the only game in town*, in terms of experimental HEP.

*to use a phrase that's much disliked around here…

27. **Peter Woit**
September 6, 2011

DB,

I think the caveat here is that, while a fully complete, combined D0+
CDF analysis with all the Tevatron data (up to shutdown at end of Sept) might be able to rule out a Higgs in the interesting region above 114 GeV, my understanding is that the schedule for availability of this is the summer 2012 conferences. The LHC experiments have huge numbers of people to throw into these analyses, not so at all at the Tevatron these days. So, even if this works, it may come out after the LHC has already put out results about this. A good thing to have such very independent results though…

28. **Igor Khavkine**
September 6, 2011

Amitabha, I see now that you are right about the 2-form field not being equivalent to a scalar via a local field redefinition. I'm still skeptical about both renormalizability and non-anomalous gaug invariance being satisfied, but I'll look into it. In any case, not being an expert in this area, I'm now happy to know another method (other than Stückelberg's) of representing a system with second class constraints (Proca's massive vector bosons) as one with only first class constraints. At the very least, your paper points to some interesting literature.

29. **Bernhard**
September 6, 2011

AcademicLurker,

true, but only if the experimental activity "here" in Europe lead to a discovery, otherwise we are all going down.

30. **Peter Woit**
September 6, 2011

frank,

Despite what one might think from the various hype fed the public, my impression of the situation has always been that virtually no one ever believed extra dimensions would show up at the LHC. As for SUSY, while it's the most popular of "BSM" ideas, I still suspect that the median particle theorist would always have assigned it a probability of showing up at the LHC of somewhat less than 50%. So, the median theorist is probably not too surprised at no extra dims and no SUSY, but may have expected something unexpected to show up by 1 TeV, if not SUSY.

Most theorists are aware of the argument that, if no Higgs, something else must show up to make WW scattering unitary, and that's part of what people typically are thinking of when they say: if no Higgs, must be something around 1 TeV. This argument is going to get vastly more attention if the SM Higgs gets ruled out, which will be a very good thing. The basic problem for years is that theorists like to work on relatively easy problems in well-understood frameworks (like SUSY), not spend time banging their heads on a problem for which no one has a promising idea.

31. **Peter Woit**
September 6, 2011

OhDear,

Besides SUSY, as I mentioned, limits on RS KK gravitons are already getting towards 2 TeV, and there's lots of other exotic physics searches also ruling things out to that energy scale. From trying to follow the arguments about the state of SUSY searches, my impression of the situation is that they are already filling in holes in parameter space pointed out by model builders, and describing the situation as "no strongly interacting superpartners below 1 TeV" is a reasonable description of the generic situation, although of course special classes of models can always be found for which the bound is lower. Something else I don't understand though is why anyone would believe there's a large probability that the gluino will show up precisely above 500 GeV but below 1 TeV, making it worth their time to argue much about this.

32. **Mark**
September 6, 2011

"Something else I don't understand though is why anyone would believe there's a large probability that the gluino will show up precisely above 500 GeV but below 1 TeV, making it worth their time to argue much about this."

My best guess as to why Kane would make such a claim comes from this paper:
http://arxiv.org/abs/1105.3765

In particular, in eq. 2, where the first two terms on the right hand side almost cancel out, the next term denoted by R(t) contains a product A0*M3 (see the expression above eq. 3) where A0~O(10-50) TeV and M3 is directly related to the gluino mass. So, I suspect that for such models with very heavy scalars and trilinears Radiative Electroweak Symmetry Breaking , where the Higgs vev is of O(100) GeV, becomes much harder to achieve when M3 goes above 1 TeV because of the A0*M3 contribution. Again, this is just my guess.

33. **VP**
September 6, 2011

Looks like CERN might be preparing for the possibility of "No God Particle". In the agenda of last June's Scientific Policy Committee meeting there is a talk with the title:
"Interim report on: The scientific significance of the possible exclusion of the SM Higgs boson in the mass range 114-600 GeV and how it should be best communicated."

34. **Peter Woit**
September 6, 2011

Mark,

I see that that's a carefully constructed example of a model with this feature, I still don't understand why anyone would believe it….

35. **Charles**
September 6, 2011

"CERN, don't forget, has been outmanoevered by the US on more than one occasion, and the prestige to Europe that would accrue from discovering the Higgs is what these politicians are really paying out for"

This outcome is one of the sub-plots of the story. It is not just CERN vs. FermiLab but making the "Higgs" a true European story from theory to discovery. Folks are working together to make this happen at CERN and ultimately in Stockholm. Guralnik made this point in a historical survey from a theorist perspective (*quoted on this blog months back*). I'm also told t'Hooft also feels **strongly** the "Higgs" is an opportunity to make good on previous Stockholm snubs for European Nobels in favor for US scientists. The Veltman/t'Hooft Nobel paper listed all the 1964 theorists and referred to it as the "Higgs-Kibble mechanism". Since being buttressed by the Nobel, it is the BEH Mechanism whenever he speaks or writes on the topic.

Time will show if this broader European strategy is successful. Recent results from LHC certainly make it more interesting.

36. **Peter Woit**
September 6, 2011

Charles,

Thanks. I haven't followed the twists and turns of the names used over the years, but I did do my own historical research last year on the early literature, results were in this posting:

http://www.math.columbia.edu/~woit/wordpress/?p=3282

37. **Rick Ryals**
September 6, 2011

Nobody has an answer for "ru"… ?

38. **Peter Woit**
September 6, 2011

Rick Ryals,

I left that comment there in case it attracted an interesting response, but I'm not surprised it hasn't. It's not really well-defined (what exactly do you mean by "Higgs field" and what exactly do you mean by "by definition). Even choosing ways to make the question well-defined, it's a complicated issue. And, all in all, I think trying to start discussions here of material on someone else's blog is a bad idea. Better to discuss it over there, where the text explaining what the author means is in place, and he can clarify it as needed.

39. **Mark**
September 6, 2011

"I see that that's a carefully constructed example of a model with this feature, I still don't understand why anyone would believe it.…"

To be fair, Kane has been heavily advertising such a model during the past 4 or 5 years, while most SUSY folks, especially gauge mediators, were expecting light squarks and sleptons to show up, and I'm not surprised that as the CMSSM gets ruled out these ideas will draw more attention. Models with a split spectrum have been around for a while but the amount of splitting and the scale of SUSY breaking in bottom up approaches was always kind of arbitrary. On the other hand, Kane's example is actually rooted in a top-down approach (see his work with Acharya et al), where the gaugino-scalar splitting is highly constrained. I personally don't know how general such a spectrum really is but I do know that gaugino mass suppression relative to the gravitino mass is rather generic in many string compactifications, while scalar mass suppression is much "less generic".

40. **Rick Ryals**
September 6, 2011

"I left that comment there in case it attracted an interesting response…"

Okay, Peter, that's what I was hoping for too, since I've already been to that blog and had some conversation with the author. Anyway, thanks for the reply.

41. **SpearMarktheSecond**
September 6, 2011

Thanks for finding the Govoni talk, Peter. Looks to me like page 23 indicates the LHC is not very promising for like-sign W's. But maybe page 17 indicates W/Z scattering is hopeful in distinction from background… the raw number of events is awfully low, however.

If the Higgs really is discovered between 115 GeV and 145 GeV, that will be amazing and should make heroes out of everyone involved. It is too easy to let the hype detract.

Sure, there is always hype. Without hype there would have been no machine. The only guaranteed route to a totally predictable outcome is to do nothing… then for sure nothing gets discovered. If scientific funding had been easier for the last 20 years, the hype could have been dialed down. So I'm definitely in favor of copious forgiveness for the hype, particularly if the Higgs is 115-145 GeV.

If no light Higgs, and the SSC was the right machine, darn, darn, darn.

42. **Thomas Larsson**
September 7, 2011

Looking back at my previous comment, I realize that it makes little sense. Fear of Peter's censorship made me mutilate my comment into incomprensibility. So let me write a comment which is both on topic and does not explicitly mention my own work. A reference for the statements below is chapter 4 of Pressley-Segal: Loop groups.

If the LHC does not see a Higgs, spontanous symmetry breaking is wrong (modulo more epicycles), and EW must be broken in some other way. The mechanism which has not been beaten to death by generations of physicists is anomalous symmetry breaking.

To be precise, by a gauge anomaly I mean an extension of the group of gauge transformations, as in "the conformal anomaly is the central charge in the Virasoro algebra". The group of gauge transformations in Yang-Mills theory has two types of extensions: the Mickelsson-Faddeev group, which is responsible for the anomalies found in QFT, and the central extension. The former has no unitary reps of quantum type (Pickrell 1989) and can hence not be used in physics. Anomalies of MF type must cancel, as they do in the SM.

In contrast, the central extension does have unitary reps – in fact, the unitary irreps are classified in PS – and hence it may provide a mechanism for anomalous symmetry breaking. However, this extension does not arise in QFT, so if it is responsible for EW symmetry breaking, one must go beyond QFT. Unfortunately, I did not manage to turn this observation into useful physics, but my shortcomings do not change the mathematical facts above.

43. **Giotis**
September 7, 2011

Peter,

Do you doubt that there is a field with a non zero VEV that breaks EW ?(*regardless* of its nature, properties and particle manifestation)

44. **Bernhard**
September 7, 2011

"Take Z-prime. In spite of what you may have heard, this is a completely unmotivated extension of the SM. It solves nothing of its problems and has nothing to do with Naturalness."

I strongly disagree with such a claim. Z´are neutral bosons that appear in many extensions of the SM, also in the most conservative, strongly motivated and only relying in model building rules for renormalizable gauge theories, such as the 331 models. It´s not correct to say these models don´t solve SM problems. I quote the generation problem, the solution of the strong CP problem and the explanation of the heavy top quark mass as a few. Not to say I am because of that a strongly believer, but the statement is incorrect.

45. **Bernhard**
September 7, 2011

sorry, *strong believer*

46. **DB**
September 7, 2011

@Peter
"while a fully complete, combined D0+ CDF analysis with all the Tevatron data (up to shutdown at end of Sept) might be able to rule out a Higgs in the interesting region above 114 GeV, my understanding is that the schedule for availability of this is the summer 2012 conferences."

Dmitri Denisov of DZERO is on record in July as saying "We should be able to exclude the Higgs particle or see first hints of its existence in early 2012".

A key factor is that the Tevatron can detect the dominant mode of Higgs decay into quark anti-quark pairs, whereas due to its high QCD background the LHC is forced to look for the much rarer Higgs decay to pairs of W bosons decaying to two photons and therefore is at a disadvantage in the lower end of the remaining 114-145GeV range.

The LHC has more bodies yet the Tevatron has more experience. Reminds me of the tortoise and the hare.

That the Tevatron intends to race the LHC to the finish line is definitely the sense I get from reading the Fermilab PR release. Time will tell if it's just bravado but I hope they make a real race out of it.

47. **Peter Woit**
September 7, 2011

Giotis,

If there is no Higgs field, I think we really just don't understand what is going on. One possibility is that there's some other field (elementary or composite), with some other dynamics, that does what the Higgs field does. But there may be something else going on. How gauge degrees of freedom get handled in chiral non-abelian gauge theory is very tricky, maybe there's some subtlety there we're missing.

48. **null**
September 7, 2011

DB "A key factor is that the Tevatron can detect the dominant mode of Higgs decay into quark anti-quark pairs, whereas due to its high QCD background the LHC is forced to look for the much rarer Higgs decay to pairs of W bosons decaying to two photons and therefore is at a disadvantage in the lower end of the remaining 114-145GeV range.

The LHC has more bodies yet the Tevatron has more experience. Reminds me of the tortoise and the hare."

Since Tevatron intends to shut down at the end of Sept, 2011, will it be able to present results in the interesting region of 115-145Gev? How long after Sept 2011 will Tevatron announce its results?

49. **SpearMarktheSecond**
September 7, 2011

Or maybe it is simply that the Higgs field is there, but the width of the Higgs is a whole lot larger than we expected, due to couplings to (as yet) unknown particles. I'm not aware of limits on the width of the Higgs from the precision electroweak, although maybe someone has looked into that. If only SM couplings, then we'd know the width up to QCD. But perhaps the width should be viewed as a parameter to be measured, in which case, maybe the Higgs can be so broad as to be everywhere, smeared out.

50. **Chris Austin**
September 7, 2011

DB:

"the vacuum energy contribution of the Higgs field results in a cosmological constant fifty times larger than the one actually measured,"

I think you mean about 10^{55} times larger than measured. The observed cc corresponds to a vacuum energy density $ \Lambda / (8 \pi G_N) $ of about (2.3 x 10^{-3} eV)^4, while the Higgs vev gives in order of magnitude about (100 GeV)^4.

51. **Joe S**
September 7, 2011

If the Higgs is 'found', I predict people will begin naming their firtborn male child by the particles name.

52. **Anonymous**
September 7, 2011

I once read in an article (of which I don't seem to remember the name or author(s)) that a pseudo-Riemannian metric or frame field may act as a Higgs field. The article dealt with the mechanics and its consistency with the electroweak Higgs mechanism. This, albeit just like a scalar Higgs field, would shed light to the problematicity and inelegance of elementary scalar fields unlike the scalar Higgs, and would explain the origin of the Higgs and why it occupies every point in space, since the metric/frame field is simply a property of space-time, and every point in space has a metric. Also when it is switched from Einstein frame to Jordan frame, the scalar Higgs mechanism is recovered. Thus this would be a more than decent argument to resort to whether the Higgs is found or not.

53. **Peter Woit**
September 7, 2011

Anonymous,

I've always had some fondness for the idea that the Higgs field has something to do with the choice of a time direction. But the problem with any of the many ideas around that interpret the Higgs field as some element of space-time geometry is that you need to come up with a real consistent and complete theory, i.e. at least a

Lagrangian or something. You'll need to explain where the Yukawa couplings come from, and thus you'll explain all particle masses. Since you're unifying a crucial element of the electroweak theory and the space-time geometry, set by gravity, you probably also need to figure out how to quantize gravity and unify it with the Standard Model….

54. **DB**
September 8, 2011

@Chris Austin
Thanks Chris, I meant, of course, fifty orders of magnitude.

@SpearMarktheSecond

Actually the decay width of the SM Higgs is only 10MeV for a Higgs mass < 135GeV and is much too narrow to be measured by the LHC. The very narrow width is governed by the dominant decay to quark anti-quark pairs in this energy range and is not related to precision electroweak fits, it's a theoretical calculation. The RPP reference I gave above has a nice plot of SM Higgs width vs mass on page 9 (figure 4). For a Higgs mass above 145GeV the width expands dramatically but ironically the LHC has ruled those masses out. If the Higgs exists in the range still open to it, the LHC is not well-placed to elucidate its properties; you really need a muon anti-muon collider to achieve the precision necessary to measure such narrow widths. Fermilab is planning to build one but will they get the money?

@null
Who knows? If the Tevatron shut down six months from now it would make little difference to the huge dataset they already have, since they have virtually maximized the data taking capability already, whereas the LHC needs to and is aggressively accumulating data and is at a disadvantage anyway in the lower end of the 115-145 GeV range. It's also not clear to me whether DZero and CDF are now pooling their resources compared to Atlas and CMS which, AFAIK, are still operating as independent competing teams. Morale might also be an issue at the Tevatron. And there is the complicating factor that quite a few researchers have a foot in both the Tevatron and LHC camps.

55. **Peter Woit**
September 8, 2011

DB,

Do you really need a muon-anti-muon collider to measure the SM Higgs width? Why couldn't it be done with the ILC or CLIC? Besides money issues, my understanding is that it's still not at all clear if the technology for a muon-anti-muon collider (and detectors…) is even feasible. One surprising problem that comes up is a radiation hazard from so many neutrinos…

56. **Bernhard**
September 8, 2011

And are we also going to be sure about the "Higgs" spin at the LHC if it has a low mass?

57. **Rhys**
September 8, 2011

@DB
I think the point about the Higgs width was that hidden sector fields can have renormalisable couplings to the operator |H|^2, where H is the Higgs. This won't affect its production rate at a collider, but will certainly increase its width if it is kinematically allowed to decay to such hidden sector states.

58. **Bernhard**
September 8, 2011

This article http://arxiv.org/abs/hep-ph/0212396 for example seems to implicitly indicate this is going to be at least inconclusive.

59. **SpearMarktheSecond**
September 8, 2011

Sure, DB, if the Higgs couples exclusively to the SM particles. But maybe, as Rhys points out, there are other as yet undiscovered fields that the Higgs couples to that broaden it. Maybe the Higgs is so broad that it is under the LHC backgrounds.

A limit plot that treated the Higgs width as an unknown parameter would be an interesting CMS/ATLAS output.

I think one of the well-known members of the I-missed-the-J/Psi club had a nice plot in their thesis that included resonance width… at that time no-one expected the *narrowness* of the J/Psi, and so scan points skipped over the resonance. But a proper limit was still made, where the limit admitted the possibility that a narrow resonance would have been passed over.

Maybe a *wide* Higgs would be passed over with current searches.

60. **DB**
September 8, 2011

@Peter
Given a Higgs width in the 10 MeV range a muon antimuon collider is your only option. This is because the production of Higgs at high rates in the s-channel is a unique feature of muon colliders – whence the moniker "Higgs Factory". (s-channel is the scattering mode that describes the gluon-gluon fusion leading to Higgs as an intermediate particle which then decays to a bottom antibottom quark pair – this largely dominates if Higgs is around 120GeV and unlike the LHC, it's what the Tevatron is tuned to observe). Next, you have very small radiative losses (vs electron colliders) which allow you to achieve extremely narrow beam energy spreads. Plus, you can determine the beam energy very precisely using the time-dependent asymmetry from the polarized muons in the beam itself. Also, because the Higgs couples to particles based on their mass, an electron-positron collider will generate too few Higgs vs its much heavier cousin, the muon. In turn, this enhanced coupling of the Higgs boson to the muon makes for an ideal opportunity to perform ultra-high precision measurements of the Higgs mass. Finally, because of the greatly reduced radiative losses (bremsstrahlung) you can design circular muon colliders in lieu of the traditional linear ep colliders. It's this unique combination of features that gives a muon collider its preferred status as a high-precision probe of *low* mass Higgs properties.
That the LHC would need help in measuring Higgs properties is no surprise. Electron-positron colliders have long been been the backbone of the high-precision measurements of Standard Model parameters, the LEP2 studies of the Z boson being a prime example. Lepton colliders are just better suited to high precision measurement than their hadron equivalents.
As to the high levels of neutrinos from decaying muons, this is a considerable bonus, because of the high luminosity of the resulting beam and the fact that its flavour content is precisely known makes it an excellent source for neutrino spectroscopy. In addition, it's expected that muon decay within a storage ring can be greatly reduced

using ionization cooling so that decay can be tuned as and when secondary neutrino experiments require. This is currently the object of study of the International Muon Ionization Cooling Experiment (MICE), which recently became operational and is due to complete its work in 2015 or so.
Ultimately, a great deal depends on precisely where the Higgs boson is found. If it's below 120 GeV then you simply need a muon collider if you want precision measurements. As we move above 120GeV, and especially above 130GeV, the charged vector boson decay process rapidly takes over from the s-channel as the decay width expands dramatically. Then traditional linear collider alternatives such as CLIC become competitive. After all, they have two orders of magnitude higher luminosity than a muon collider and there is less technological uncertainty associated with them, and since the Higgs width is now much wider, and the s-channel no longer dominant, the unique precision of the muon collider is less relevant.
But in any event, the recent exclusion of Higgs above 145GeV has already done serious damage to the rationale and justification for CLIC and will do the muon collider case no harm at all, assuming, that is, that we find the Higgs.

(For more detail see Berger's article in Lee (ed.) Search for the Higgs Boson, p.85-99, Nova Science 2006)

@Rhys, Spearmark the Second
I simply don't buy into such open-ended speculative scenarios and was just responding to the query re Higgs width.

61. **jpsi**
September 8, 2011

There are many people who missed the J/psi. Leon Lederman was prominent amongst them. Lederman ran an expt at the AGS at BNL, and found something which came to be called "Lederman's schoulder".

http://www.science20.com/quantum_diaries_survivor/blog/say_week_ledermans_shoulder

With the benefit of hindsight this is now known to be due to production of the J/psi. The BNL management wanted someone to propose an experiment to investigate the Lederman shoulder, but years went by. In the meantime Lederman went to CERN and ran an experiment at the ISR, but so much `background noise' was coming from the mass range of 3.1 GeV/c^2 that they cut it out. Also at the ADONE ring at Frascati, Italy, they found background noise from the mass range of 3.1 GeV/c^2 and they cut it out. At the same time the CEA (Cambridge Electron Accelerator ~ MIT/Harvard) reported an increase in R (ratio of e+e- –> hadrons/e+e- –> mu+mu-) consistent with the creation of a new quark. But nobody believed CEA because it had lost credibility because of prior publication of false claims. Finally Sam Ting proposed an experiment (at the AGS at BNL) which did the job. (Although Ting did not specifically propose his experiment to investigate the Lederman shoulder.) Ting's experiment was sufficiently detailed that it proved the existence of a new particle of width less than 10 MeV/c^2 (the J). Then it becomes the well-known story of how Ting delayed publication, and Richter's team at SPEAR (using the Mark II detector) discovered the psi, and there was no doubt of the existence of a narrow resonance, with width less than 1 MeV/c^2 (the psi). It was only after that, that the various puzzling results were recognized as the J/psi.

So one hopes that there is not a similar glossing over of backgrounds (or false assumptions about what constitutes `noise') or whatever, especially given that this time around people are specifically searching for the Higgs.

62. **jon**
September 8, 2011

To expand on what VP mentionned, what I've noticed is that:
1) last june there has been such an interim report (see this page) presented as "Given the level of public investment and interest in the LHC and implications of the LHC's discovery potential for the natural and human science fields, the Council *underlined* the importance of a policy on communicating discoveries at the LHC that was geared to providing information accessible to politicians, the general public and other scientific disciplines and not just to the particle physics community"
2) the final report will apparently be delivered on september 15 by the Chairman of the Scientific Policy Committee (see http://indico.cern.ch/conferenceDisplay.py?confId=152955).

But not sure what to make of that: that's his job after all to plan for every possible outcome, just in case, not necessarily anything to do with as yet undisclosed data.

63. **DB**
September 8, 2011

"gluon-gluon fusion" should of course read "muon antimuon interaction"

64. **Chris Austin**
September 8, 2011

DB, thanks for the fascinating info on muon colliders. Am I correct in thinking that "gluon-gluon fusion" here is a typo for "muon-antimuon fusion" via the Yukawa coupling? The idea being that you would tune the beam energy to the Higgs peak, as they did with the Z peak at LEP2?

65. **SpearMarktheSecond**
September 9, 2011

@DB, perhaps the muon collider, and also all the SUSY scenarios involve speculation too… in the old days one had to measure a particle's mass, spin, width/lifetime, parity, and C (when pertinent) to declare it a particle. It seems a bit less than open ended to work out Higgs limit plots with a variable width, but that is just my opinion.

@jpsi, great stuff, great stories. There are so, so many with the J/Psi. Let's pray for a rerun with something new!

66. **DB**
September 9, 2011

@Chris
For a suite of Feynman diagrams that show the various ways muon-antimuon interactions can generate Higgs see the Higgs/Scalar Physics section of http://home.fnal.gov/~rruiz/FeynLib/

You're correct, in a Higgs Factory, muon collider centre-of-mass energy is set equal to the Higgs mass. Then this centre-of-mass energy is varied over a narrow range so as to scan over the Higgs resonance. One of the benefits of LEP2 having excluded the Higgsbottom anti-bottom pairs which would have compromised a muon collider's sensitivity to Higgs. So if there is a Higgs around 120 GeV, then a muon collider is really in the sweet spot as far as not only precisely determining Higgs parameters, but also in distinguishing between SM and SUSY versions of Higgs.

One further point about Peter's neutrino shielding issues: Using a conventional accelerator as a beam source of neutrinos, as originally proposed in the late fifties by Pontecorvo and Schwarz and implemented at CERN in the early sixties, does require massive shielding, because here you smash protons into a target, generating a large number of pions which have to then decay over a distance into muons and neutrinos. Similarly if you are using a nuclear reactor as a neutrino source where you are restricted to antineutrinos and cannot generate collimated beams. But muons just decay into electrons/positrons which are easy to shield.

67. **DB**
September 9, 2011

That should have read: One of the benefits of LEP2 having excluded the Higgs below115GeV is that there is very significant background at the Z-pole via Z–>bottom anti-bottom pairs which would have compromised a muon collider's sensitivity to Higgs.

68. **Peter Woit**
September 9, 2011

DB,

What I was referring to is a different potential problem, the intensity of the neutrino beam from the muon decays being high enough to create an unshieldable radiation hazard off-site, see for instance here:

http://www.hep.princeton.edu/~mcdonald/mumu/johnson/neutrino.pdf

69. **Ric - ½ g tr(Ric) = 8πT**
September 9, 2011

Now if the Higgs is discovered it would be horrible. The tabloids will be screaming the variants of "Physicists have found God" for the next few weeks or even months.
–
Also, for Anonymous's comment, here are some articles that might be of interest:
– I. Kirsch, A Higgs mechanism for gravity, Phys. Rev. D72 (2005) 024001; arXiv: hep-th/0503024
– M. Leclerk, The Higgs sector of gravitational gauge theories, Annals of Physics 321 (2006) 708
– D.Ivanenko, G.Sardanashvily, The gauge treatment of gravity, Physics Reports 94 (1983) 1
– L. Nikolova, V. Rizov, Geometrical approach to the reduction of gauge theories with spontaneous broken symmetries, Reports on Mathematical Physics 20 (1984) 287
– G. Sardanashvily, O.Zakharov, Gauge Gravitation Theory (World Scientific, Singapore, 1992)
Interesting indeed.

70. **Peter Woit**
September 9, 2011

Ric,

It should be made clear that those articles are about the idea of using gauge symmetry and spontaneous symmetry breakdown in GR, and this, until somebody has a really good idea about unifying quantum gravity and the SM, has nothing to do with gauge symmetry and spontaneous symmetry breakdown in the the electroweak theory, which is what the LHC results are relevant to.

71. **DB**
September 9, 2011

@Peter

To be able to examine a 120GeV Higgs with a muon collider, the Fermilab studies concluded one would only need a centre of mass energy sqrt(s) in the range 100-500GeV, commonly known as the FMC or First Muon Collider. The paper you cite only considers high energy muon colliders in the 1-4TeV range where the neutrino radiation begins to become a serious concern. However, even the authors conclude "For CoM energies *exceeding 4 TeV* (my emphasis) some countermeasure must be adopted to limit the radiation dose". Furthermore it takes no account of muon ionization cooling which would be expected to contribute significantly to reduced muon decay and lowered neutrino radiation levels. Incidentally a 4TeV version is considered the ideal machine to fully explore strong vector boson scattering – a far cry from the 40TeV estimate required for the SSC.

In any case, it's difficult to imagine building a 4TeV collider without having prototyped the various novel technologies with an FMC in the first place.

72. **null**
September 10, 2011

Db,

What do you think the likelihood of finding or not finding a Higgs by Dec 2011, given that Tevatron is shutting down Sept 2011 and LHC will complete its run by Oct, 2011?

If there is no Higgs, then does the 10^55 cosmological constant/energy density no longer a problem (i.e QFT calculation for particle physics for cc can be close to the astronomical cc)

73. **Sakura-chan**
September 11, 2011

Happy B-day Peter!

74. **Shantanu**
September 12, 2011

Peter slightly OT, but given that the nobel prize will soon be announced, what do you think of http://arxiv.org/pdf/1109.1972v1

75. **DB**
September 12, 2011

@null
For a good analysis of this issue, see Richard Ruiz's Sep. 1st updated posting on Quantum Diaries, especially Figure C:
http://www.quantumdiaries.org/2011/08/29/presusy-2011-natures-little-secrets/

I'll just recast that table:
Exclusion of SM Higgs: Excluding an SM Higgs Boson at 95% confidence level down to 114 GeV requires *significantly less data than discovering same to 5 sigma over the same range*. As I'm unsure as to whether Ruiz's numbers are correct for the 95% confidence exclusion I won't quote them here but in any event both LHC and Tevatron should have enough data already to do this.

Discovery of SM Higgs: For a 5 sigma detection of a SM Higgs based on a joint analysis of Atlas and CMS data *their simulations* project that they need the following:
Down to 140GeV: 1 fb-1 for each experiment separately and subsequently combined
Down to 128GeV: 2.5 fb-1 ditto
Down to 117Gev: 5 fb-1 ditto
Down to 114GeV: 7.5 fb-1 ditto

On June 17th the LHC had collected 1 fb-1 for each experiment.On August 5th, 2 fb-1. They expect to achieve 5 fb-1 by end October which, when combined (joint analysis), tells us that by then they should have accumulated enough data to do the discovery job down to 117GeV. End 2011 for a preliminary result one way or the other is quite realistic.

The reason we are hearing from the Tevatron now is that, although they may not be in a position to discover a SM Higgs boson to 5 sigma levels, they have enough data to exclude one at 95% confidence level.

It will be quite a bitter pill for CERN to swallow if Fermilab gets to the exclusion finish line first.

76. **null**
September 12, 2011

Db,

thanks. also thanks for the link. So when should we hear an announcement from Fermilab? How long after collecting the data does it take for the analysis and number crunching to occur?

77. **Peter Woit**
September 12, 2011

null,

From here:
http://www.sciencenews.org/view/feature/id/334164/title/Last_Words
"We expect to complete our Higgs analysis by March," says Fermilab's Rob Roser.

Thanks Shantanu,

I had never really looked into the story of Salam's role in Weinberg-Salam. That's quite interesting.

78. **Amos**
September 12, 2011

So, how negative is the mood in the HEP community at this point? I don't mean with the proponents of any one theory or another, but the community as a whole?

If the LHC finds *nothing* …

79. **Anon**
September 12, 2011

@null: "If there is no Higgs, then does the 10^55 cosmological constant/energy density no longer a problem (i.e QFT calculation for particle physics for cc can be close to the astronomical cc)"

I don't think so. You still have to somehow resolve the problem of the vacuum energy of all the other fields in the Standard Model. Formally, the contribution of each field to the vacuum energy is infinite, although these can typically be renormalized to pretty much any value you like by what amounts to a simple redefinition of the measure of the path integral. The signs of the vacuum contributions may be different for different fields, but it is difficult to make an argument that they would all cancel (never mind "almost" cancel, which is probably even more difficult) without some symmetry principle, lacking in the Standard Model.

80. **Peter Woit**
September 12, 2011

Amos,

I think the mood in the HEP community as a whole is very positive, with a lot of excitement that the answer to the question "is there a SM Higgs?" may finally be close.

The mood among SUSY enthusiasts may not be so good (they're still in the first stage of grief, denial). As for the mood among those who seriously thought the LHC would see extra dimensions, that also would not be good, but I'm not sure there's more than a vanishingly small number of such people.

81. **Anon**
September 13, 2011

Dombey's paper is disgraceful. What an evil, vindictive little gossip.

82. **Anon**
September 13, 2011

Sorry, wrong thread. Don't know how that happened.

83. **VP**
September 14, 2011

DB,

"One further point about Peter's neutrino shielding issues: Using a conventional accelerator as a beam source of neutrinos, as originally proposed in the late fifties by Pontecorvo and Schwarz and implemented at CERN in the early sixties,…."

Actually the first accelerator source of neutrinos for an experiment was used by Mel Schawartz himself (along with Leon Lederman, Jack Steinberger, et al) in the early sixties at the AGS at Brookhaven and not CERN. It led to the discovery of the muon neutrino and a Nobel prize for the three mentioned above a quarter century later. As

Jack once advised: “Do your good work early and live long enough”.

84. **DB**
September 16, 2011

@null
Professor Guido Tonelli, the spokesman for CMS, told the BBC on Sep.1st: “We could discover the Standard Model version of the Higgs Boson or exclude it earlier than expected. Could we discover it by Christmas? In principle, yes,”

In fact, both Fermilab and the LHC should be in a position to exclude the Higgs by Christmas, if it doesn’t exist. If it exists, discovery to 5 sigma will take longer and only the LHC has a realistic chance of achieving this. The closer the Higgs is to 114 GeV, the harder it is for the LHC to see it and the longer it will take.

@VP
I was wondering if anyone was awake. 🙂 Actually CERN was ready to go before Brookhaven, but an outsider, Guy von Dardel was brought in to verify the neutrino flux calculations and he argued that the team (wrongly it turns out) had made important errors, as a result CERN postponed startup until June 1963, too late to do anything but verify the Brookhaven results.

85. **null**
September 19, 2011

thanks DB. Is the fact they’ve not found any signal above background 115-145GEV mass range with the data thus far collected and analyzed telling?

86. **Peter Woit**
September 19, 2011

null,

There is signal above background at various places in this mass range, just not enough give a statistically significant signal. And, with the amount of data available, you shouldn’t see a statistically significant signal (at least not in the lower part of this range). So, for the moment, all is consistent with either Higgs or no Higgs in this region. It will take more data to resolve this, everyone’s waiting…

87. **null**
September 19, 2011

So by the end of Oct 2011, there should be enough inverse femtobarns to decide the issue one way or the other 95% confidence from both LHC and Fermilab?

How to Win the Nobel Prize

September 12, 2011
Categories: Uncategorized

I'm too busy to write much on the blog just this moment, and besides, there's nothing of great interest I can think of that need's writing about. So, I'll take up commenter Shantanu's suggestion and try and stir up a little trouble with two quick topics related to the Nobel Prize.

- Norman Dombey recently posted on the arXiv [Abdus Salam: A Reappraisal. PART I. How to Win the Nobel Prize]() which more or less seems to argue that Salam didn't deserve his 1979 Nobel. He describes a lot of history I didn't know, but I'm not completely convinced. Part of the argument seems to be that he stole the idea from Weinberg, and didn't even know the importance of what he had stolen, but my impression was that no one, not even Weinberg, thought very much of the unified electroweak theory at the time. A quick look at the paper in his collected papers that I take to be the 1968 one that justified the Nobel to him appears to discuss the crucial points: a gauge theory with Higgs mechanism.

Unfortunately I don't have more time now to look into this history carefully. If someone expert on this history has comments on the Dombey claims, that would be interesting.

- One way to win the prize is to do revolutionary work. This year's prize will be announced October 4, and for the past few years I haven't had much in the way of thoughts about obvious candidates. After reading Richard Panek's *The 4% Universe* early this year and learning more of the story of the discovery of the acceleration of the universe, I'm pretty sure that sooner or later there will be a Nobel Prize for that, maybe this year. Those better informed than me can speculate about what the exact names will be that will go on the prize.

Comments

1. **noprize**
   September 12, 2011

   There are many who deserve a Nobel Prize and didn't get one. Conversely I suppose some people feel that there are those who did win but didn't deserve it. A pointless debate, unless one can prove fraud or something like that. Consider:

   – Weinberg's 1967 is titled "A Model of Leptons" Leptons only! No hadrons! And yet it is one the classic papers of HEP theory. Weinberg explicitly admitted that he did not know if his model was renormalizable. Is that a valid unification?

   – Glashow 1961. Weinberg 1967 stated that his model was an extension of Glashow's 1961 "partial" model of the weak interactions (see title of Glashow's paper). Dombey states that by 1979 Glashow's model had been almost forgotten, but Gell-Mann stepped in at the last moment to speak up for Glashow. Glashow's model was not renormalizable. The Higgs mechanism had not been proposed yet, in 1961. Is that a valid unification?

   – Salam 1968. Salam explicitly emphasized that gauge invariance was absolutely necessary to obtain a renormalizable theory, and to this end the bare masses of the gauge bosons (called"mesons") must be zero, in a nonabelian theory. Salam stated that a possible way to generate mass and preserve gauge invariance was to have a coupling to a scalar boson, which would exhibit spontaneous symmetry breaking, and the Higgs mechanism would transform away the Goldstone bosons. All well and good, but known by others. Salam did NOT relate the strengths of the weak charged and neutral currents (which Weinberg 1967 did). Salam claimed that the original unbroken theory was renormalizable because it was gauge invariant. Salam could not prove that the symmetry breaking did not destroy renormalizability. Salam talked nonsense at the end about extending the model to hadrons, because he knew only of the Cabibbo angle and three quarks. Is that a valid unification?

   Glashow (not in 1961 but later ~ GIM) proposed the existence of a fourth quark to explain in a natural way the absence of flavor changing neutral currents (the quark was so named because it solved so many problems that it worked like a charm). So did Glashow not deserve his Nobel? Do GIM deserve a separate Nobel? Cabibbo did NOT share the Nobel Proze with Kobayashi and Maskawa, even though the matrix is universally called CKM and Cabibbo was still alive at the time of the award to KM. Is that unfair?

2. **physicsphile**
   September 13, 2011

   For the accelerating Universe prize, Perlmutter would certainly be one of the names.

3. **chris**
   September 13, 2011

   I do not konw the story of Salam and I have heard other people doubt his contribution (notably Veltman), but Dombeys paper is a disgrace. one just needs to read the first paragraph do draw some conclusion regarding his character.

4. **John**
   September 13, 2011

   Only one question: Why did Dombey not raise these issues when the prize was being given out?

5. **Will**
   September 13, 2011

   "Only one question: Why did Dombey not raise these issues when the prize was being given out?"

   Or object to writing his own reference…

6. **hrk**
   September 13, 2011

   "I do not konw the story of Salam and I have heard other people doubt his contribution (notably Veltman), but Dombeys paper is a disgrace. one just needs to read the first paragraph do draw some conclusion regarding his character."

   Agreed.

   Its also funny because at least according to some people, the Nobel for 't Hooft and Veltman, should really have only gone to 't Hooft. Salam and 't Hooft and Weinberg and Glashow all have many other contributions named after them other than what they are cited for in the Nobel prize. What does Veltman have? It is not impossible that his biggest discovery was a spectacular graduate student named ‘t Hooft.

7. **MathPhys**
September 13, 2011

Dombey’s paper doesn’t contain any scientific information that isn’t already known, and I found it in bad taste. Salam wished to help him, trusted him, and told him in confidence “Write the letter and I will sign it”. It’s disgraceful to reveal that in public after the man’s died.

Why he didn’t he write this paper while Salam was alive? Salam had many friends, particularly in the UK.

8. **MathPhys**
September 13, 2011

Veltman stayed with quantum field theory when no else did. He stayed with gauge theory and the very well defined question of renormalizability. He gave ‘t Hooft a concrete problem to solve. A precise definition of what the problem to solve is, is half the way towards a solution. He fully deserves his half of the Nobel prize.

9. **Per**
September 13, 2011

I find it a bit tasteless. In the first paragraph of the paper its mentioned that Salam was the first muslin to receive the noble prize, which is a rather strange comment to make. But then, looking at the publishing date 9th September 2011, the reference to 9/11 seem almost to obvious to be just a mere coincidence.

In any case, just my two cents. Besides, who is this Dombey and why is his opinion of interest?

10. **MathPhys**
September 13, 2011

Per,

Yes, I wondered why this paper? and why now? Could it be the anniversary of 9.11? Well, that would be worse than in bad taste.

Dombey wrote a book with Bailin on weak interactions. That was about 30 years ago.

11. **Arun**
September 13, 2011

Norman Dombey’s article on Abdus Salam seems to be a wee bit prejudiced, from terming him a “Moslem” (really oldfashioned) , to referring to a series of lucky coincidences that enabled Salam to study physics as a miracle (somehow violating the laws of physics?).

Dombey starts off with how the New York Times described Salam’s contribution:

> So did Salam do what the New York Times (presumably based on a briefing by the Nobel Committee) had claimed that he had done; namely propose a theory in 1967 which predicted the size of the parity-violation in electron scattering off hydrogen nuclei as observed in 1979. If he didn’t why was he awarded the Nobel prize? I knew all three prize winners and NATURE had asked me to write about the award of the prize in November 19797. So I would like to explain how Salam won the prize even though he was unable to predict the result of the 1979 experiment.”

There’s a problem in this framing of the question:

As an example of the Nobel citation,
http://www.nobelprize.org/nobel_prizes/physics/laureates/1979/presentation-speech.html
or
http://www.nobelprize.org/nobel_prizes/physics/laureates/1979/#

Once you understand that

The Nobel Prize in Physics 1979 was awarded jointly to Sheldon Lee Glashow, Abdus Salam and Steven Weinberg “for their contributions to the theory of the unified weak and electromagnetic interaction between elementary particles, including, inter alia, the prediction of the weak neutral current”.

then one can look at the totality of Salam’s contributions to the theory.

12. **dopey_john**
September 13, 2011

The comments from some people here objecting over Abdus Salam being described as a muslim are rather silly. He was the *first muslim* to win a Noble prize in physics which is an important point. The funny thing is that whereas Abdus may be paraded as the first muslim to win this prize by some in the muslim community, the majority brand him as a heretic because he was a member of the Ahmadiyya Muslim Community and therefore not an “orthodox” muslim.

13. **Trulo**
September 13, 2011

> So the prize was awarded on the basis of a non-peer-reviewed
> publication which quotes an unpublished lecture. Yet the name
> Weinberg-Salam model stuck because almost everyone used it. Weinberg
> was happy with that arrangement: he knew that he could only benefit
> from association with Salam.

I’m rather puzzled by this statement. What kind of benefit could Weinberg obtain from being (fairly or not) associated with Salam? Trips to Trieste?

14. **Jack Levitt**
September 13, 2011

Given the tone of Dombey’s article, I have no choice but to wonder to what extent racism and anti-muslim feelings are behind the attack on Salam. Sad, but not without precedent in the field of theoretical physics.

15. **Math Student**
September 13, 2011

Trulo,

That part that made no sense to me as well. I don't see any reason why Weinberg and Glashow would just go along with Salam getting credit if it was obvious he didn't deserve it.

16. **Hugh Osborn**
September 13, 2011

It is too easy to ascribe racism or other prejudices to views one does not like. Norman Dombey wrote an article saying similar things many years ago, I don't have the reference. The reason for the current article appears to be that he has had access to papers not previously available. It is a fact of life that people do campaign behind the scenes for Nobel prizes. It is also true that, perhaps rarely, they are given in small part at least for political rather than purely scientific considerations.

17. **M. Wang**
September 13, 2011

Dombey's writing may exude the appearance of prejudice, but that does not necessarily imply that he is lying. Whether or not he tells the truth is the real issue and should be evaluated based on independent facts. Sadly I don't seem to find any discussions along that direction here. Does anyone have something solid to contribute?

18. **Arun**
September 13, 2011

M. Wang,

Dombey's paper is titled "Part I". I'd wait for Part II to see what else he says.

The events of 1960-1968 might be beyond most people here's personal memory, but one could examine, e.g, if Antonio Zichichi has written anything – as per Salam, he attended the 1967 lectures where Salam expounded on the model. One can examine the literature to see how the model got named. One can examine why P.A.M Dirac nominated Salam for the Nobel – would Dirac do so as a political favor?

If none of the readers here happen to know it off the top, and no other write-ups appear, then I'll do some of this work to examine the factual basis of Norman Dombey's claims; but there is no point in starting just now.

-Arun

19. **Arun**
September 13, 2011

From M.J. Duff's tribute to Salam:
http://155.198.210.128/~mduff/talks/salam.html

"One of my greatest regrets is that as a student in the Theory Group at Imperial from 1969 to 1972, a group that included not only Abdus Salam but also Tom Kibble, no-one suggested that weak interaction physics would be an interesting topic of research. In fact I did not learn about spontaneous symmetry breaking until after I got my PhD! The reason, of course, is that neither Weinberg nor Salam (nor anybody else) fully realized the importance of their model until t'Hooft proved its renormalizability in 1972 and until the discovery of neutral currents at CERN."

20. **J**
September 14, 2011

Regarding the 1979 Nobel Prize: my personal feeling is that Weinberg deserves more credits than the other two. The most striking of the 1967 paper, I guess, is the concrete prediction of a neutral current in weak interaction (W+- exchanges were known for long time by then) and a specific angle of mixing (Weinberg angle).
Nobel committee often favors an original theoretical proposal solving old problems and predicting new things that are verified.

21. **Chris Oakley**
September 14, 2011

> One can examine why P.A.M Dirac nominated Salam for the Nobel – would Dirac do so as a political favor?

One has to assume so, as Dirac did not buy into the actual theory.

22. **MathPhys**
September 14, 2011

Salam lobbied for his Nobel prize. So what?

Salam wrote to Dombey explaining to him why it should be the Weinberg-Salam model. Weinberg called up those who didn't cite his paper, explaining to them why they should. What's the difference?

23. **d nom**
September 14, 2011

Dirac may have nominated Salam out of personal friendship and admiration, even if Dirac did not agree with the idea of renormalisation. The electroweak unification was still an elegant model, and other ideas Salam was working on. What political favours would Dirac possibly have received or cared about (in view of his attitudes throughout his life), by 1971 and later?

24. **SpearMarktheSecond**
September 14, 2011

There are always shoving matches and lobbying over Nobel Prizes. Ho hum.

I'd have given one to Prescott for the polarized electron scattering at SLAC in the late 1970's that turned the tide on people believing in the Z^0. Up until that time the atomic parity violation experiments were confusing, as were the neutral current experiments.

But surely politics prevents *4* Nobels for work at SLAC in part (J/Psi, tau, DIS are first 3).

25. **np**
September 14, 2011

Prescott deserves a Nobel Prize. Three Nobel prizes (in physics) were also awarded for experiments at the AGS at BNL (J/psi, CP violation, multiple neutrino families). But no Nobel prize was awarded for the discovery of the Omega- particle (an experiment also done at the AGS). Note that Ray Davis won a Nobel (2002), for the solar neutrino problem. Davis was on the staff at BNL, although his experiment was done off-site.

So who knows how the politics works out. At Columbia in the 1950s, a Nobel was being won almost every year. Some of the students (I think Lederman amongst them) minted buttons "I have not a Nobel Prize". PW should be able to confirm this.

26. **mo**
September 15, 2011

Here are some interesting stats (author impact factor) from SPIRES-HEP:

Partial Symmetries of Weak Interactions.
S.L. Glashow, (Copenhagen U.) . 1961.
Published in Nucl.Phys.22:579-588,1961.
Cited 4120 times

Weak and Electromagnetic Interactions.
Abdus Salam, (Imperial Coll., London & ICTP, Trieste) . May 1968. 11pp.
In the Proceedings of 8th Nobel Symposium, Lerum, Sweden, 19-25 May 1968, pp 367-377.
Cited 751 times

A Model of Leptons.
Steven Weinberg, (MIT, LNS) . Nov 1967. 3pp.
Published in Phys.Rev.Lett.19:1264-1266,1967.
Cited 7450 times

27. **Some Guy**
September 15, 2011

It's disappointing to see somebody so critical of string theory hype being so gullible when it comes to cosmology hype. Try http://arxiv.org/abs/0710.5307 for a start.

28. **SpearMarktheSecond**
September 15, 2011

And the experimental paper that triggered the 1979 Nobel Prize…

Parity Nonconservation in Inelastic Electron Scattering.
C.Y. Prescott, W.B. Atwood, R.Leslie Cottrell, H.C. DeStaebler, Edward L. Garwin, A. Gonidec, Roger H. Miller, L.S. Rochester, T. Sato, D. Sherden (SLAC) et al..
SLAC-PUB-2148. Jul 1978. 20 pp.
Published in Phys.Lett. B77 (1978) 347-352
Cited by 607 records

So citation count doesn't accurately measure impact.

29. **july**
September 15, 2011

Peter, don't you think Michel Mayor is an obvious candidate?

30. **Shantanu**
September 16, 2011

Someguy that paper was from 2007. I think its fairly robust now with new evidence
from BAO, LSS .
Anyhow probably it will given to Reiss, Perlmutter and Schmidt.

31. **Some Guy**
September 16, 2011

Shantanu, all that "evidence" was known already, and discussed in the paper.

32. **Peter Woit**
September 16, 2011

Some Guy,

Arguments over what the supernova observations mean for theoretical cosmology are irrelevant here. The Nobel prize would be awarded for the observational achievement, which found something unexpected.

33. **Some Guy**
September 16, 2011

Fair enough, a Nobel prize for finding that high-z SNe Ia are not as bright as expected (as opposed to accelerated expansion) might be justified. But it would be unusual to award a Nobel prize for an unexplained observation. Offhand, I can't recall a precedent. Then again, the committee has done strange things before…

34. **piscator**
September 16, 2011

While on the theme of Nobel prizes that should have been awarded, surely the observation of weak nuclear currents at Gargamelle is right up there?

35. **weak neutral currents**
September 16, 2011

The case of weak neutral currents is a sad story, and an embarrassment to CERN. The discovery richly merited a Nobel Prize. But the Fermilab experiment (HPW ~ Harvard-Penn-Wisconsin ~ Carlo Rubbia, David Cline, Alfred Mann) successfully muddied the waters with their claims of what are now called (tongue in cheek) `alternating neutral currents'. The CERN management failed to stand by the excellent work of their own people. Later, when the existence of weak neutral currents was generally accepted, CERN realized too late that it had let a Nobel Prize slip through. Sadly, Paul Musset, Andre Lagarrigue and Andre Rousset are all dead, so there is no hope of a Nobel Prize for the discovery of weak neutral currents.

36. **Charlie**
September 16, 2011

My perpetual nominee: Mitchell Feigenbaum. It's not every day that someone discovers a few new constants of nature.

37. **Shantanu**
September 16, 2011

weak neutral currents, HW stood for Harvard-Purdue-Wisconsin (not Penn)

38. **weak neutral currrents**
September 16, 2011

Shantanu – HPW in the 1970's was the Harvard-Penn-Wisconsin experiment at Fermilab. It was a fixed-target experiment. Carlo Rubbia was from Harvard, Alfred Mann from U Pennsylvania and David Cline from Wisconsin.

However, you prompted me to look up HPW on Google and I discovered that HPW also refers to a later proton decay experiment (big underground tank of water), and that is indeed Harvard-Purdue-Wisconsin.

So you are correct and I am not even wrong.

39. **CWJ**
September 16, 2011

I agree with Charlie–Feigenbaum is a good candidate for a Nobel and it's a shame Ed Lorenz didn't get it before he died.

40. **Shantanu**
September 17, 2011

Yes and Carlo Rubbia was also the PI of Harvard-Purdue Wisconsin water Cherenkov experiment.

41. **zafhore**
September 17, 2011

Salam did no less than Glashow and Weinberg to merit this award, and probably more. His first attempts at unifying forces were in 1955 (with Polkinghorne), and continued to 1973 (with Pati). None of the others hung in there for so long, and meanwhile created an international centre on the side. After publishing his disgraceful piece, Dombey has the audacity to thank ICTP management after they let him sift through Salam's archived papers.

42. **Euler**
September 17, 2011

This is not a research article on history of physics, it only expresses doubtable opinions of the author, made without a serious and in-depth scientific discussion of ideas, theories, experiments and literature of the time. Its place should not be the historical section of arXiv.

43. **MathPhys**
September 17, 2011

No one takes Salam's work with Pati seriously.

44. **weak neutral currrents**
September 17, 2011

Shantanu (and anyone else) – read these interesting and informative articles from the CERN Courier about the discovery of weak neutral currents, and more generally the genesis of the Gargamelle collaboration. (And if you don't know that Gargamelle was the mother of Gargantua … well now you do.) BTW it was HPWF at Fermilab (Harvard-Penn-Wisconsin-Fermilab).

http://cerncourier.com/cws/article/cern/29168

http://cerncourier.com/cws/article/cern/28875

The second article is by Donald Perkins and gives more detail about the mistakes by HPWF, and the care with which the CERN analysis was performed. The articles also tell you how the CERN results were greeted with skepticism. They allude to how timid the CERN management was. They indirectly say how the HPWF collaboration boldly announced all of its bogus claims. By the time weak neutral currents were widely accepted, the damage had been done. CERN realized too late that it had let a Nobel Prize slip through its fingers.

Andre Lagarrigue died in 1975. Paul Musset was killed in a mountaineering accident in 1985. Andre Rousset died in 2001. No Nobel Prize for weak neutral currents.

The HPWF experiment was a bad experiment. It was responsible for many false results. I recall the acronym as just HPW. I am reading between the lines here, but I suspect that the acronym HPW = Harvard-Purdue-Wisconsin was deliberately chosen so that people would forget about Harvard-Penn-Wisconsin. Certainly Shantanu thought so (and clearly did not know about HPWF), and saw fit to correct me. I am guessing Shatanu is young.

45. **weak neutral currents**
September 17, 2011

Towards the end of Don Perkins' article he mentions Willi Jentschke. Willibald Jentschke was the DG of CERN at the time (1971-75).
http://cerncourier.com/cws/article/cern/28878

Previously Willibald Jentschke was the DG of DESY.
http://cerncourier.com/cws/article/cern/28880

Being DG of DESY was a stepping stone to becoming DG of CERN. This tribute to Jentschke was written by Herwig Schopper, who was himself DG of DESY and later DG of CERN.
http://cerncourier.com/cws/article/cern/28873

Actually Jentschke founded DESY, much as Robert Wilson founded Fermilab. Jentschke was the first employee of DESY, and had to build a lab and his staff. His first employee was Klaus Steffen, who later built DORIS. I never met Jentschke (or maybe I saw him from a distance once), but I knew Klaus Steffen, who was very nice to me (and who was already old when I met him as a freshman graduate student). I also recall another senior DESY person (maybe employee #3) who told me he named his daughters Deli and Desi, because it had not yet been decided if the new lab would build a linear accelerator or a synchrotron, so he covered both bets.

And if anyone objects that this is all off-topic, let me just say that they all assured me that they were all fiercely anti-string theory. Back in the 1950s. Before I was born.

46. **anonomous**
September 18, 2011

@Charlie:

I agree – Feigenbaum deserves the prize for discovering a very important constant in nature and his paper was dismissed for years as irrelevant. For as many people working in the field of dynamical systems today, you would think he would have been awarded already, but maybe the trend is towards experimentalists.The sad thing is that Feigenbaum's constant has been experimentally verified, but the topic doesn't qualify under the glamorous physics that the award committees are looking for.

47. **IM**
September 20, 2011

If there is a Nobel for HEP, part of it can be given to Faddeev. When the Prize was given to 't Hooft and Veltman, Faddeev also deserved it. Perhaps Polyakov can also be awarded for his work on topological objects and on CFT (2-D CFT being now important in condensed matter).

48. **MathPhys**
September 20, 2011

Topological objects first appeared in the paper of Nielsen and Olesen on vortex lines as models for strings. Based on that, there were the papers of 't Hooft and Polyakov on monopoles, followed by Belavin et al. on instantons. Faddeev's work on gauge theory was joint with Popov and with Slavnov.

49. **Thomas Larsson**
September 20, 2011

IM, a Nobel prize for CFT for its application in 2D statphys would be appropriate, and something I have argued for over the last 20 years. It would go to BPZ, not just to Polyakov himself (it was the other Zamolodchikov twin that died, right?). I have long suspected that the reason why Dotsenko and Fateev were not included in the seminal BPZ paper was that only three people can share a Nobel.

But from a condensed matter perspective, the restriction to 2D is serious. Yes, 2D materials can be manufactured in the lab, but most real materials are 3D.

50. **Trulo**
September 20, 2011

> Topological objects first appeared in the paper of Nielsen and Olesen on vortex lines

As a topological theory, the Skyrme model is much older and more relevant to our world, me thinks.

51. **MathPhys**
September 21, 2011

Trulo, You are perfectly correct. But as far as I know, teh gentlemen that I mentioned were not aware of Skyrme's work. Who re discovered Skyrme's work? Was it Witten?

Thomas L,

Dotsenko and Fateev's work is based on an earlier paper by Dotsenko alone. The latter is based on the BPZ paper and distinct from it, in content as well as in time.

52. **Trulo**
September 21, 2011

> Who re discovered Skyrme's work? Was it Witten?

I know Balachandran worked on it, and I believe it was him who suggested Witten to look into it. But the truth is I only know that story at the level of unsubstantiated rumors.

By the way, isn't Dirac's monopole theory a topological theory as well? Probably the first one in the context of QFT.

53. **MathPhys**
September 21, 2011

Trulo,

Yes, I believe that you are right again. Witten's work is based on Balachandran, Nair, Rajeev and Stern.

54. **abbyyorker**

September 21, 2011

Does anyone know at what energy unitarity kicks in to demand a Higgs boson? Or perturbative effects on other processes?

55. **IM**
September 22, 2011

1. Polyakov has another contribution to CFT : He was the first to say that critical phenomena have not only scale invariance, but also conformal invariance.
2. I think that the Dotsenko paper MathPhys refers to is
Critical behaviour and associated conformal algebra of the Z3 Potts model:
Nuclear Physics B, Volume 235, 1984, Pages 54-74.
(This is the earliest paper by
Dotsenko or Fateev in the collection edited by Itzykson, Saleur and Zuber).
Yes, the Introduction of this paper clearly acknowledges that the new development in 2D CFT was initiated by BPZ.

56. **Andrew**
September 22, 2011

Dear Peter,
Love your blog and hope to get around to reading your book in full one of these years.
I was wondering,since you're infinitely better qualified to judge this then I am-have you heard of this and if so, what do you think?

http://www.npr.org/blogs/thetwo-way/2011/09/22/140713791/scientists-report-breaking-the-speed-of-light-but-can-it-be-true?sc=tw

Personally,I think it's Bell's incompleteness theorem all over again-a lot of wild speculation that ultimately goes nowhere in terms of a real challenge to general relativity's ontology.

Andrew L .

57. **Charles**
September 30, 2011

When I first read this I had to double check that it was not written by Frank Close at Oxford. He is in the process of writing a book which will dive deeper into this topic and has been probing many theorists who were around Imperial College London and the "Higgs" phenomenon in the 1960's. On a further review I noticed Dombey has connected with Dr. Close as indicated by the paper's notes.

I am grateful to Frank Close for early sight [38] of some material from his book [39] which presents an independent summary of Salam's role in the 1979 Nobel prize: in particular the memos from Salam to Matthews [33] in which Salam writes his own nomination letter.

I (*and many others, such as a bloggers here*) put Close, Dombey, and John Ellis in similar light…on the peripheral from the front line of theoretical physics with clearly some sort of axe to grind or motive behind their positions or fronts.

No News

September 15, 2011
Categories: Experimental HEP News

I've been hearing no interesting news from the LHC recently, about all I've learned is that CMS/ATLAS haven't even decided whether it's worth combining their latest public data (probably not, what is much more interesting is the large amount of data they are now analyzing separately). So my plan for next week is to travel to Antwerp, where I'll try and get Tommaso Dorigo drunk and see what I can find out. We'll both be at TEDx, he's got more of the story here.

Adrian Cho has a wonderful long piece in Science (and podcast here) about the sociology of the two big experiments at the LHC. It gives some insight into the process by which a Higgs result is likely to emerge, including the steps being taken to make sure that some group doesn't "parachute in" at the last moment to try and capture glory. I'm still trying to figure out who gets a Nobel prize if the Higgs is found.

For some other reading material, there's John Ellis's 65th birthday colloquium, an interview with Bianca Dittrich, and yet more evidence that MathOverflow rules.

Comments

1. **bruno**
   September 16, 2011

   I'm still trying to figure out who gets a Nobel prize if the Higgs is found.

   Higgs?

2. **Bobito**
   September 16, 2011

   I love math overflow, but why has Lurie's answer gotten 68 votes? Many much more informative answers get only 5 or 10 votes. The answer to my question is that the answer was by Lurie.

3. **Octoploid**
   September 16, 2011

   > I'm still trying to figure out who gets a Nobel prize if the Higgs is found.

   Lyn Evans would deserve it…

   BTW there will be an interview with Lisa Randall on Charlie Rose tonight.

4. **DB**
   September 16, 2011

   Cho makes the interesting point that with groups the size of CMS and Atlas with around 3,000 physicists in each, the issue of competition *within* groups is a phenomenon which may need to be reckoned with, as compared with the straightforward traditional competition *between* groups, illustrating it via the story of how, earlier this year, the University of Winsconsin was accused of trying to steal a march on "discovering" the Higgs boson diphoton decay, a decay mode memorably described by Tommaso Dorigo as: "two angry gamma rays, each roughly carrying the energy of a 2 milligram mosquito launched at the whooping speed of four inches per second toward your buttocks."

5. **Jack Levitt**
   September 16, 2011

   Peter, the obvious answer re the Nobel question, while the discovery of the Higgs is certainly worthy of the prize, no person or group of persons have done enough (in comparison to their colleagues) to warrant the award. Hence, no Nobel for the Higgs should be awarded. Simple, huh?

   By the way, please take care in Antwerp. I hear the city has gotten more violent in recent years.

6. **Yatima**
   September 16, 2011

   That should be "angry gamma quanta", I guess.

   Slow news day? Slow news day. But we have this:

   http://arxiv.org/pdf/1109.0702v1

   "Results from 730 kg days of the CRESST-II Dark Matter Search"

   64 events maybe.

7. **Peter Woit**
   September 16, 2011

   Yatima,

   Somehow, ambiguous claims to see dark matter that conflict with other experiments no longer seem newsworthy…

8. **weichi**
   September 16, 2011

   bobito – but isn't Lurie's answer the only answer?

9. **Garrett**
September 16, 2011

Hey, cool — have fun at TEDx.

The nobel committee will no doubt struggle to outdo their previous curious decisions.

10. **Dan L**
September 16, 2011

Actually, your link is one more example of why MathOverflow appears to be more interesting than it really is. The stated purpose of the site is for researchers to answer research questions posed by other researchers. The link is to a question that a (presumptive) graduate student asked that could have been answered simply by looking at the Preface of the book. This is one of many examples of what I would call "lazy grad student" questions on MO.

Frankly, I have no idea how successful MO is with its stated goal. That is because its greatest successes are too technical to be understood by a broad audience. If an answer to a question gets 50+ votes, then maybe it's a great little exposition of some piece of mathematics, but I seriously doubt that it was an answer to true research question, because real research questions are typically only understandable by a few dozen people in the country (if that). The main exceptions occur only when you need something from outside your field of expertise.

11. **Dopey john**
September 16, 2011

Maths Overflow is an amazing success, probably because f**kwits like me can gawp at the brain exploding questions being asked there, and check out the profiles of today's rising stars and Fields medal winners. There's now a high-brow theoretical physics version currently private, but going public in 6 days time: http://theoreticalphysics.stackexchange.com/ Who knows, even Witten may post there someday if it's as successful as Maths Overflow. On the other hand, maths seems to be far more popular than physics so it may not flourish.

12. **Bernhard**
September 16, 2011

If the Nobel committee follow what they did in the past, the answer is easy. They will pick up the spokesperson(s) of the experiment(s) . This used to make sense I guess, in the time of Rubia and Van der Meer. Now these would be a much less logical decision. First they have decide in ATLAS for example between F. Gianotti, current spokesperson, or P. Jenni, who was the spokesperson of ATLAS for years and in this sense who put things to work. In an experiment involving the LHC I´m afraid that´s not good enough, why not give some credit to the accelerator guys? So, best solution, give it to CERN, and let Rolf Heuer go to Stockholm to get it, in the same way that UN got a piece prize. Otherwise my suggestion is to give the prize to Tommaso Dorigo, because even people working in experimental collaborations learn more reading his blog than actually working, so this should have an influence in the discovery itself 🙂

13. **null**
September 16, 2011

Would a Nobel be awarded for establishing the Higgs does NOT exist?

14. **Anon**
September 16, 2011

I think one would do well not to start counting Nobels (or Higgses) before they are hatched.

15. **Alex**
September 16, 2011

LHC and collider physics are clearly dead…check out the new OPERA results next week: it seems that neutrinos are tachyons !!!!

16. **Artie**
September 16, 2011

Dan L: I agree that the link is a pretty terrible example of what MO is good for, but I disagree with your overall premise. There's a lot of material on MO that isn't research-level but is fascinating nevertheless (irrespective of the site's stated goal). And there are plenty of fascinating things that are research-level, but are not "too technical to be understood by a broad audience", for example this question by Bjorn Poonen (which, I admit, is an outlier). I don't expect anyone to come along and solve Poonen's problem, but that doesn't make the question any less interesting.

17. **Truly Anomalous**
September 16, 2011

I say give the Nobel to Tommaso D…

18. **Jeff M**
September 16, 2011

As a mathematician I'd have to agree with Artie about MO. Actually, one of the great things about MO is that as a researcher you can ask questions about areas outside your field, and get expert input. I'm a geometric analyst, but I've recently been using graph theory to get results about Riemann surfaces. I just posted a question to try and get unstuck on something that has been driving me crazy for a while now, it's much easier than asking all my analysis friends if they happen to know a nice graph theorist I can bother 🙂

19. **johnR**
September 16, 2011

Octoploid I agree
Lyn Evans would highly deserve it.
Instead, I would hardly figure out which is the point to have some high level burocrat going to Stockholm to get the prize.
Is Lyn still involved with the machine operation?

20. **anonymous**
September 16, 2011

I think we have a long ways to go before somebody gets a Nobel for HEP – this year's Nobel will probably go for dark energy (Riess or Perlmutter is my guess), but I am thinking the Higgs is on it's last leg (wouldn't we hear some leaks by now)?

21. **lun**
September 17, 2011

Actually, the news that CRESST has found dark matter is accompanied by the news dark matter is incompatible with galaxy formation
http://www.bbc.co.uk/news/science-environment-14948730

22. **Thomas Larsson**
September 17, 2011

"Would a Nobel be awarded for establishing the Higgs does NOT exist?"
Michelson won a Nobel prize, Morley not, so who knows.

23. **null**
September 18, 2011

"Would a Nobel be awarded for establishing the Higgs does NOT exist?"
Michelson won a Nobel prize, Morley not, so who knows.

It's funny you say this b/c I've wondered whether the Higgs (and SUSY, GUT, string theory) is the"luminiferous aether". of 21 century.

24. **visitor@CERN**
September 18, 2011

Here at CERN, My colleagues tell me that in a recent Scientific Policy Council meeting DG Rolf-Dieter Heuer was asking advice how to best tell European political leaders that there is no Higgs.

25. **not@CERN**
September 18, 2011

That is simply gossip and hearsay. The DG will necessarily have to prepare speeches for multiple eventualities. How can anyone positively conclude that there is "no Higgs" so early in the game? One can say that the task is very difficult, and there is no conclusive signal as yet, but a lot more – and painstaking – work remains to be done. The LHC has not even reached its top design energy of sqrt{s} = 7 TeV yet. Too much idle gossip.

Blood, toil, tears and sweat?

26. **not@CERN**
September 18, 2011

E_max = 7 TeV/beam, sqrt{s} = 14 TeV

27. **topper**
September 18, 2011

The Tevatron came online in 1985, with adequate energy to produce top quarks, but the top quark itself was discovered only in 1995 (approx). It required a lot of integrated luminosity and also enormous data analysis to discern the top in all of the accumulated data. (The Tevatron devoted part of its time to fixed-target experiments. The LHC devotes part of its time to heavy ions. Then there is downtime for maintenance. This all takes time.) So no reason to panic about `no Higgs' just yet.

28. **Peter Woit**
September 18, 2011

topper + people@CERN,

The Standard Model makes very precise predictions about the Higgs, and, if all goes well, the LHC experiments should have the necessary luminosity to test the prediction, letting us finally know whether there is a SM Higgs or not. Waiting for higher energy or much higher luminosities should not be necessary to decide the question of whether the SM is right. It's true that, for Higgs masses near the lower bound, the statistics will be near the edge of what is necessary for 95% exclusion, and one may end up with an ambiguous result. But, even in that case, the luminosity available in the first part of next year should decide the issue.

Coming up with a CERN statement for the case of no Higgs has been an agenda item for quite a while at their Scientific Policy Council, so that's not necessarily indicative of anything. It's an interesting question whether the DG is being fed inside rumors from the experiments about what the data is saying. Quite possibly not, in which case he knows no more than the rest of us about the Higgs existence question.

29. **Yatima**
September 18, 2011

"DG Rolf-Dieter Heuer was asking advice how to best tell European political leaders that there is no Higgs."

That's pretty simple, innit "THERE IS NO STANDARD MODEL HIGGS and here is where we go from here: … ".

Why do I get the feeling that some think the situation is that of Admiral Piett having to tell Darth Vader that Han Solo hast just slipped through the fingers of the combined might of the imperial expeditionary force.

Please stop it!

I find it actually refreshing that some things in this universe do not bent to incessant spin, political hemming and hawing or Jedi mind tricks.

30. **OhDear**
September 19, 2011

"I find it actually refreshing that some things in this universe do not bent to incessant spin, political hemming and hawing or Jedi mind tricks."

Me too, but unfortunately government funding of scientific research is not one of them.

31. **Shantanu**
September 19, 2011

Peter, maybe you could blog about Avi Loeb's talk which is a must watch for particle physicists (than astrophysicists) esp. now that there is no evidence for supersymmetry, extra dimensions etc

32. **Marcus**
September 19, 2011

Shantanu, which Avi Loeb talk is that? Is there an abstract, or slides pdf, or online video? I searched and could not find any recent talk by Loeb.

33. **Shantanu**
September 19, 2011

Marcus see
http://online.kitp.ucsb.edu/online/colloq/loeb1/
He also gave a similar talk at CFA, but I can't find a link right now.

34. **Peter Woit**
September 19, 2011

Shantanu,

By Loeb's investment analogy, I'd say most particle theorists right now have moved to cash (most risk-free investment), waiting for ongoing market crashes to get sorted out. SUSY and extra dimensions are in free-fall, with people trying to get everything they've invested in them out before the market value is zero. Depending on the Higgs/no Higgs question, the field could look very different in a few months, but I have no idea which way this is going to go, so no idea what happens next.

35. **Peter Lee**
September 19, 2011

Is there any comment to be had concerning the current and ongoing (??) debates within the Gran Sasso neutrino experiment (including the views of the Lyon data analysis team) about the potential (but possibly/probably false?) signal of supra-luminal neutrinos in that experiment?

36. **Peter Woit**
September 19, 2011

Peter Lee,

Someone wrote a comment about this here 3 days ago.

The rumor seems to be a "6 sigma" observation of timings indicating faster than light propagation of neutrinos. This seems almost certain to be a mistake; to believe it, you would need overwhelming evidence. Supposedly details to be released later this week, we'll see. If it is real, presumably it should also be observable by other experiments in other conditions. My rule of thumb is don't believe something like this until it's confirmed by a second experiment: people on the first experiment want to believe they've found something revolutionary, people on the second experiment want to shoot down the result of the first experiment…

37. **none**
September 19, 2011

Shantanu,
Thanks for the Loeb link. In addition to the paper linked there
(Taking "The Road Not Taken": On the Benefits of Diversifying Your Academic Portfolio
http://arxiv.org/abs/1008.1586), there is also a recent paper
(Rating Growth of Scientific Knowledge and Risk from Theory Bubbles
http://arxiv.org/abs/1108.5282) with abstract:
In physics the value of a theory is measured by its agreement with experimental data. But how should the physics community gauge the value of an emerging theory that has not been tested experimentally as of yet? With no reality check, a hypothesis like string theory may linger for a while before physicists will know its actual value in describing nature. In this short article, I advocate the need for a website operated by graduate students that will use various measures of publicly available data (such as the growth rate of newly funded experiments, research grants, publications, and faculty jobs) to gauge the future dividends of various research frontiers. The analysis can benefit from past experience (e.g. in research areas that suffered from limited experimental data over long periods of time) and aim to alert the community of the risk from future theory bubbles.

38. **Zathras**
September 20, 2011

Regarding superluminal neutrinos, this is not the first time the possibility has come up. In the early 90s, there was a nuclear experiment (sorry, can't remember the name) which produced a Curie plot for neutrinos indicating that the square of the neutrino mass was negative. Such a result is consistent with superluminal neutrinos.

39. **Reply to Zathras**
September 20, 2011

http://arxiv.org/abs/hep-ph/0009291
lists some of the experiments with negative neutrino mass-squared.

40. **SpearMarktheSecond**
September 21, 2011

I doubt their will be any Nobel for a Higgs or no Higgs. Too many cooks. Unless there is a twist that some clever group figures out and sees the Higgs in an unexpected manner.

Peter Woit is right… dark matter has lots of claims and counterclaims, and is just a mess right now.

No Higgs? The DG should just say, `Waxahachie'.

As for superluminal neutrinos, I thought Telegdi ruled that out years ago for electron antineutrinos, and then someone else did it for muon neutrinos. But, who bothers to read the literature anymore? Perhaps I'm misremembering though.

41. **Charles**
September 21, 2011

Pretty safe bet that there will not be a Higgs Nobel on October 4, 2011 for Theory or Experimental. Awarding would bring too many questions and undermine final steps in confirmation of the theory.

42. **Bernhard**
September 22, 2011

The shut-down of the Tevatron in nine days or so is perhaps a new subject to be discussed (on the absence of something more exciting).

43. **kristo**
September 22, 2011

(@Jack Levitt)
I will be volunteering at TEDx Antwerp. There is nothing unsafe about walking around in Antwerp – at any hour. True in certain quarters there has been more 'uproar' in recent years, but this is restricted to some streets in certain area's a normal visitor of Antwerp will not stroll into.

European (Belgian) cities are nothing like what I hear from US main cities, where walking around in the evening or at night is indeed dangerous. Quite the contrary here. US visitors are amased when you say during or after a night out that you're walking to another place or homewards. BTW I live in Ghent, you should visit.

44. **M. Wang**
September 22, 2011

Dr. Woit, the superluminal neutrino story has hit the Economist. Will you do a more detailed blog entry when more info is released in the next few days?

45. **Bernhard**
September 22, 2011

Not only the Economist, the buzz is growing exponentially and will hit the official peak tomorrow when CERN will make the announcement.

http://indico.cern.ch/conferenceDisplay.py?confId=155620

46. **Low Math, Meekly Interacting**
September 22, 2011

Seriously: WTF is up with the superluminal neutrinos???

47. **Lau**
September 22, 2011

http://www.reuters.com/article/2011/09/22/us-science-light-idUSTRE78L4FH20110922

48. **Peter Woit**
September 22, 2011

kristo,

I've spent a day in Antwerp and enjoyed it a lot, maybe I'll run into you. No sign of anything dangerous. It's also true that in Manhattan these days, you would have to go to a fair amount of trouble to find a place that wasn't safe to walk around anytime of day or night. Both New York and major European cities are much safer now than what I remember several decades ago. Several people have told me I must see Ghent, and maybe a plan to visit Bruges will be modified to add a short trip to Ghent.

49. **Peter Woit**
September 22, 2011

About the superluminal neutrinos. My initial reaction is that this has to be a problem with the experiment, and making a big public announcement of this is a really bad idea, even if you do this together with explaining that it's almost surely an experimental problem. If I get time and there's something more interesting to the story, I'll try and write a posting on the topic.

50. **Andrew Foland**
September 22, 2011

If ever there were an experimental result that deserved the tagline "not even wrong", surely it would be superluminal neutrinos.

51. **Low Math, Meekly Interacting**
September 22, 2011

Well, it's already in the NY Times now, so if this is spurious (and pretty much everyone thinks it is, apparently), it does seem to have the potential to be a rather ignominious claim.

52. **abubakar**
September 22, 2011

hey … some new cern data is saying (but not totally confirmed) that speed of light maybe exceeded by neutrinos http://www.bbc.co.uk/news/science-environment-15017484 .. plz comment. Thnkx

53. **chris**
September 23, 2011

what has the world come to? a rumor hits the NYT before any result has been announced.

54. **Bernhard**

September 23, 2011

As usual, if it were true, it would be confirmation of string theory:

"Zichichi speculates that the "superluminal" neutrinos detected by OPERA could be slipping through extra dimensions in space, as predicted by theories such as string theory."

http://www.scientificamerican.com/article.cfm?id=particles-found-to-travel

55. **Paolo Valtancoli**
September 23, 2011

I think that Zichichi should be fired immediately from all universities.

56. **martibal**
September 23, 2011

Well… I cannot believe nobody has posted comments yet, on the CERN presentation of Opera experiment. So Michael, these people obviously have done a really serious job, they seem to have taken into account many of the effects one can think of (e.g. gravitational effect). Still, there is a discrepancy in the speed of the neutrino of order 10^-5 of the speed of light. They do not how to explain this as a statistical bias. Many people will try to find other explanations than faster-than-light neutrinos. Before theoretician start to propose hundreds of models with tachyonic neutrinos, maybe it would be worth letting the experimentalist confirm – or not – these result.

57. **Bitboy**
September 23, 2011

The cynic in me thinks "hmmm, no Higgs yet. No SUSY. What a great way to hold over CERN's political and financial handlers."

I understand that such extraordinary claims as superluminal neutrinos require peer review and independent verification but I wonder whether under other circumstances these wouldn't be conducted without such fanfare.

58. **martibal**
September 23, 2011

@bitboy: I did not find the CERN presentation was done with fanfare. And in their preprint, the Opera collaboration is very prudent, insisting that they do not drive any phenomenological or theoretical conclusions from their result.
Now, it is true that there has been a lot of fanfare in the media. I am not sure this was a desire from CERN or opera.

59. **Vince**
September 23, 2011

Neutrinos from the 1987 supernova were clocked in at precisely the speed of light. How? The supernova was around 150,000 light years away, so even if neutrinos travel faster than light by even a tiny amount, they would have arrived months before the light from the supernova was detected. Instead, due to the dynamics of supernovas, the neutrinos were detected up to 3 hours before the light was first observed. (Neutrinos are generated from the collapse of the core, with the emission of light happening a little afterwards, when the star is blown apart.) Also, more robust experiments show that neutrinos have a very tiny mass, which means they should travel a teeny weeny bit slower than light.

For more, look at Matt Strassler's posting on this:

http://profmattstrassler.com/2011/09/20/supernovas-and-neutrinos/

60. **Fabien Besnard**
September 23, 2011

How ironical that the title of this entry is "No news" 😉

It almost makes me believe that there is something real there.

Vince : thanks for that link. Maybe
this one could be helpful too.

Now my two cents (sorry I can't resist) : the definition of time of flight for a quantum particle is quite tricky. Claims of FTL transmission of information through tunnel effect have been made in the past. Apart from a general relativity effect not taken properly into account, this would be my second best bet for where the error comes from… if there is one !

61. **Peter Woit**
September 23, 2011

All,

I'm afraid I think the "No News" heading is quite appropriate for the neutrino story. Even if I weren't traveling I'd probably not write a posting about it here. It seems nearly certain that there is some subtle problem with the experiment, so the only story here is what that problem might be, and virtually no one submitting comments here appears to have the kind of expertise necessary to make a good guess about that (I certainly don't). So, please, if you want to discuss this, find a blog where the owner is interested in the topic and willing to moderate a discussion. Matt Strassler and Tommaso Dorigo are good bets along those lines, and surely there are others.

62. **martibal**
September 23, 2011

Just a last word (if Peter allows): maybe the topic could move to "this week hype" instead. Just heard from the news on France Culture (a well informed radio, with good scientific programs in addition) a brief interview of a well established particle physicist, claiming that the explanation could be that neutrino have shorter distance to travel, because they travel in extra-dimension.

63. **Kent Traverson**
September 23, 2011

Peter, it might be wise for you to make some kind of answer about the FTL neutrinos, otherwise the media and the masses will keep pestering you about it. This is human nature.

64. **Peter Woit**
September 23, 2011

martibal,

One thing this story does show is that string theory, since it predicts nothing, can be invoked as an explanation of anything.

Kent,
To make this absolutely clear: I'm convinced this has to be due to some sort of error in the experimental analysis, and I'm pretty sure this is the majority opinion of knowledgeable people in this subject. To find the error though requires a real expert, and I'm certainly not that.

65. **Stephen**
September 23, 2011

No News = Headlines on all major media outlets announcing "Einstein was wrong" , how ironic.

66. **Chris W.**
September 23, 2011

The Washington Post's coverage of this story (FTL [?] neutrinos) is pretty good, making clear that a definitive evaluation of the experiment and its results will take quite a while to complete.

67. **MathPhys**
September 24, 2011

This is not the first time that I hear that the speed of light was violated at CERN.

Peter W, When was the first time that you heard that the speed of light was violated at CERN?

68. **M. Wang**
September 24, 2011

I find it ironic that string theorists are trying to jump on this FTL neutrino bandwagon, because isn't string the only quantum gravity theory that insists on preserving Lorentz invariance down to Planck scale? In other theories (including LQG?) Lorentz symmetry is emergent, and small violations can manifest themselves at low energy scale as tiny corrections to various particles' propagators. Effectively, each elementary particle sees a slightly different c. (I am simply quoting Coleman & Glashow 1999.)

I am not saying that the latest result is likely true, just possibly so. After all, 10^-5 is quite large. Not to mention the 1987a supernova experiment, which seems to have already set an upper bound of 10^-8…

69. **Mike**
September 26, 2011

Just heard a great joke:

Neutrino. Who's there? Knock knock.

😉

70. **Mike**
September 26, 2011

Someone just pointed out to me that we already knew that neutrinos pass through air faster than sound, so I guess even if true, FTL neutrinos won't add anything to our humor knowledge base. Ah well . . . . 🙁

71. **Marc**
September 29, 2011

@Charles (from Sept 6)

You are correct in your comment…"*Folks are working together to make this happen at CERN and ultimately in Stockholm*."

At best, John Ellis is the main front-man for CERN and the effort to make this case for a Euro-centric Nobel. At worst, he thinks he should get part of a Higgs Nobel.

His most recent comments in Telegraph in UK make me think more and more it is the later. Pretty remarkable as he has no role in this other than being a prop.

TEDxFlanders

September 25, 2011
Categories: Uncategorized

Tommaso Dorigo and I put on a bit of a show yesterday here in Antwerp at TEDxFlanders, and the results are already available on YouTube (and Tommaso has blog postings here and here). Doing this sort of thing for 1000 people in a venue like the Antwerp Opera House is not at all the sort of thing I'm used to, so I'm glad that it seemed to come out reasonably well.

Much of this was due to the incredible all-volunteer staff, which put on an ambitious day-long program on a shoe-string (+ crucial help from some sponsors) and pulled it off with only the most minor hitches. Christophe Cop was the "curator" and founding father of TEDxFlanders, and Thomas Goorden led the production team to victory. It was a great pleasure to meet them, many of the other volunteers, and some of the other speakers, as well as to get a chance to enjoy some time in the beautiful city of Antwerp.

Back to New York (and maybe somewhat more regular blogging) on Tuesday. Hoping to make Dick Gross's second Eilenberg lecture on Local Langlands at the Columbia math department in the afternoon…

Comments

1. **Bernhard**
   September 26, 2011

   Very enjoyable. I kind off lost a bit what you were saying at 13:31 as the camera decided to show a really beautiful woman instead of you, but the rest I´ve got it :-).

2. **P**
   September 27, 2011

   is it true you don't own a car?

3. **keinkar**
   September 27, 2011

   George Washington didn't own a car either.

4. **Peter Woit**
   September 27, 2011

   P,

   Yes, no car. This isn't exactly unusual though for people living in Manhattan, where it makes a lot more sense to rent a car when one needs to go out of town and rely on public transportation, taxis (and a bike..) in the city.

5. **Lau**
   September 27, 2011

   Great talk, both you and Tommaso!

   I'm shocked to see Lubos' comment on Tommaso's blog about it. I hope Tommaso doesn't delete it, so everyone can see how impolite (I don't want to use a stronger word) Lubos is…

6. **Peter Woit**
   September 27, 2011

   Lau,

   It's not really anything new for Lubos, he's been at this for years. That string theory skepticism has been getting a lot more attention than string theory advocacy in recent years really drives him nuts, with the results that you see….

7. **Quantumburrito**
   September 28, 2011

   Great talk, and not much to add about Lubos Motl whose comment was disgusting. For all his apparent erudition in swearing, Motl is nothing but a failed physicist (who was denied tenure at Harvard) who has crashed and burned and is simply jealous that others are getting more attention than his lunatic, hateful ravings. He hates the fact that he will never make an important contribution to physics and he hates the fact that criticism of string theory is now mainstream; all he can do is foam about it. Face it Lubos, you will probably end up a jealous, cussing old geezer who will never make it even to the footnotes of the physics books. Enjoy your wretched life.

8. **Peter Woit**
   September 28, 2011

   Quantumburrito,

   I do wonder what Lubos will end up as, maybe a major political figure in the Czech Republic?

   For the sake of accuracy, it's my impression that he wasn't denied tenure at Harvard, but (like many if not most of their junior faculty) left before a tenure decision came up. In some places like Harvard, it's assumed that typical junior faculty will do this, with most going on to very respectable positions elsewhere. I don't know exactly why Lubos left academia totally, but string theory crashing and burning was probably part of it (not that he would admit this). Maybe a larger part was realizing that if he wanted a tenured position somewhere he'd have to stop ranting public attacks on people he disagreed with.

9. **King Ray**
   September 29, 2011

   This comment is a little off topic, but might be illuminating. I read Understanding Other People by Stuart Palmer, since it was recommended in a book about writing fiction, to help you understand your characters and develop their motivations. Palmer's thesis is that everyone is motivated by the need to gain approval and/or the need to avoid disapproval. If someone is frustrated in these needs, and they are not properly socialized, then they turn to aggression against others. Someone who is too socialized to aggress against others may end up aggressing against themselves, e.g., suicide. An example given in the book is that of a hermit. A hermit doesn't have a strong need for approval, but has a very strong need to avoid disapproval, so they avoid other people.

10. **Rane Bowen**
    October 3, 2011

    I seem to have taken too long to get about watching this – the youtube video is now private, and I can't see it!

This and That

September 27, 2011
Categories: Uncategorized

- Since everyone wants to hear about the faster-than-light neutrinos, here's some additional information about why I don't believe it. Jon Butterworth explains here the problem with timing the neutrinos at the CERN end. In a postscript, a senior member of OPERA points out that he and four other senior members of the collaboration kept their names off the paper. Their reasoning seems to have been that this is a very preliminary, likely wrong, result, being sold as more robust than it is. Tommaso Dorigo had a similar analysis to Butterworth's up on his blog early on, but was induced to take it down because the release to the press and the associated hullabaloo had not yet taken place.
- I had been wondering what had happened to the million dollars from the Millenium Prize that Perelman turned down. The Clay Mathematics Institute has recently announced that the money will go to fund, for the next five years, a postdoctoral position at the Institut Henri Poincaré, to be called the Poincaré Chair.
- A sign of the times: today's HEP seminar at the IAS was titled "Is SUSY still alive?". I wasn't there, so don't know what the answer was, but clearly the question is now being asked.
- The Tevatron will shut down on Friday for good, ending an era. There's an article about this in Science magazine here. Gordon Kane was expecting SUSY to be discovered by this machine, but that didn't work out, and he's no fan of Fermilab management:

> But Kane argues that the Tevatron underperformed all along because of weak management at the lab and the Department of Energy, which funds Fermilab. "It could have performed much better and done much more," he says.

Reaction to this from Nicholas Samios was:

> "I would not trust a theorist to talk about management," Samios says.

Comments

1. **MathPhys**
   September 27, 2011

   http://arxiv.org/abs/1109.5687

2. **Tom**
   September 27, 2011

   A million bucks only pays for five years of a postdoc? Mathematics must pay a heck of a lot better than experimental HEP does.

3. **chris**
   September 28, 2011

   it should have been named Perelman Chair at the very least.

4. **mo**
   September 28, 2011

   > A million bucks only pays for five years of a postdoc? Mathematics must pay a heck of a lot better than experimental HEP does.

   I guess they meant an endowed chair funded by interest income. A million dollar endowment yields these days about $40k in income which is barely enough provided the administrative overhead at IHP is very low.

5. **Piérick**
   September 28, 2011

   chris says: *"it should have been named Perelman Chair at the very least."*

   I do not want to offend the work of Perelman but, IMHO, he has not yet accomplished a sufficient work so that we can claim to substitute its name with that of Henri Poincaré…

6. **Peter Woit**
   September 28, 2011

   mo,

   It doesn't appear to be an endowment, since it is not permanent, but only for five years. This does seem to be a lot of money to pay for a postdoc for five years.

7. **Bobito**
   September 28, 2011

   Why working in a Mediterannean, crisis-affected country is a bad idea: a million dollars would pay my salary for the rest of my life, and retirement too (remember, no need to adjust for inflation, since austerity measures cut my pay yearly).

8. **martin**
   September 28, 2011

   They probably mean many postdoc positions.

9. **EscherBach**
   September 28, 2011

   aha! the long awaited experimental proof of string theory.

   Of course, ST is also compatible with this result being hokum (phew!) 😉

10. **EscherBach**
September 28, 2011

referring to MathPhys' link above

11. **Steven Colyer**
September 28, 2011

Peter,

The cost of an employee is huge, sometimes more than twice sometimes of what the salary is, thanks to government regulations and internal bureaucracy. For example: benefits, insurance, travel, etc. Payroll is often 60% of expenses, so a $100,000 salary at Poincare seems reasonable.

12. **francois**
September 28, 2011

Regarding the seminar at IAS, there is a popular saying that states that:

When the title of a talk is a question, then the answer is most likely NO…

13. **Peter Woit**
September 28, 2011

Steven,

It's still unclear though how this can add up to 200K/year to fund one postdoc, unless the postdoc is extremely well paid. Maybe there is more than one position, or maybe not just the postdoc is getting funded.

14. **mgr**
September 28, 2011

A theorist can be a good manager, for example Neils Bohr and his institute of theoretical physics (now named after him). Oppenheimer was a theorist and manager of the Manhattan Project. Isaac Newton was Master of the Mint and by all accounts he ran the place well. (But was Newton a theorist or a mathematician?) Experimentalists can also be good managers. Both theorists and experimentalists can also be outrageously bad managers. Being a good manager is a skill unto itself. What could the Fermilab management have done better, with the Tevatron? They could have operated it to higher energy. The Tevatron magnets run at 7.7 Tesla (approx) to reach a beam energy of 1 TeV. Operation at higher energy would have required a higher magnetic field, also higher operating costs. There was also a scheduling problem for collider vs. fixed-target operations. Remember that the Tevatron also operated as a fixed-target proton synchrotron, not exclusively as a p-pbar collider. The Fermilab management eventually built the Main Injector, which could deliver beam for neutrino physics in parallel with Tevatron collider operations. The integrated luminosity could perhaps have been higher, but it is not clear how that could have been done any better than it was. It is not so simple to figure out what more the Tevatron could have done.

15. **Coin**
September 28, 2011

"it should have been named Perelman Chair at the very least."

I would question whether Perelman himself would want that.

16. **EscherBach**
September 28, 2011

No doubt Perelman is fine with "Poincare" name for the Chair, for obvious reasons.

$200,000 per year might include expenses and travel for organising annual seminars. Also $1million dollars don't get you as many euros as it used to, and the trend could be downward (depending on which currency has the biggest crisis over the next few years)

17. **BJM**
September 28, 2011

Maybe million dollars is not to be all spent and the plan is to revisit what to do with the remaining sum after five years.

18. **Bernhard**
September 28, 2011

I find a bit curious the claims that the top quark "had to be there". Had to be there like what? The Higgs? SUSY? I think Kane put in SUSY the same kinf of faith, perhaps even more faith. I remember when I was in a school for which David Gross was the director. The question raised for the audience was "what was the last big discovery in HEP?". Someone shouted "the top quark!". Dave Gross turned around and said loudly with a grumpy voice "iiit was a confirmation!". Perhaps. What is the difference compared to the Higgs tough? Easy to say something had to be right after you know the answer…

19. **Reply to Bernhard**
September 28, 2011

Wasn't the top required to be there for triangle anomaly cancellation?

20. **Bernhard**
September 29, 2011

Reply to Bernhard,

Sure. There were indeed strong reasons to believe the top quark would be discovered. These reasons were proved to be correct, but I don't think they are stronger than the reasons we have to believe the SM Higgs should also be there. My point is that you can't be sure until you really detect it. This attitude of "of course it will be there because my model said so" is what put us in this situation of over confidence with SUSY. Guys like Kane were not really worried about discovering SUSY, but simply confirm it, since of course the s-partners would be there to "solve" the hierarchy problem, explain dark-matter etc etc. The success of the top-quark prediction is undeniable but could have been proved wrong too, for some reason. We were right and that's nice but we won't be always right.

21. **besnard**
September 29, 2011

I've counted 12 papers already (with 2 replacements) about OPERA in the arXiv, just for today !

22. **J**
September 30, 2011

"francois says:
September 28, 2011 at 11:45 am
Regarding the seminar at IAS, there is a popular saying that states that:

When the title of a talk is a question, then the answer is most likely NO…"

FYI: there was actually a talk a bit earlier at BNL with the title "Is SUSY dead?" I think although this title is a question the intended answer was most likely YES 🙂

Peter: I like the amusing comment from Nick.

23. **Craig Dukes**
September 30, 2011

mgr:
The Tevatron magnets run at 4.2 T, not 7.7 T, half the LHC magnet design field of 8.4 T. At the present the LHC magnets are at about the same field as the Tevatron magnets.

24. **James Matterson**
October 1, 2011

Every time I keep hearing about this FTL neutrino nonsense, I think of [this recent xkcd comic]!

25. **Reply to Bernhard**
October 2, 2011

The reasons to expect the top quark were much stronger than the reasons to expect the standard model Higgs. A triangle anomaly renders the theory fundamentally inconsistent. No Higgs or other-than-standard-model Higgs doesn't violate consistency, it only violates the Standard Model.

26. **Bernhard**
October 2, 2011

Reply to Bernhard,

Triangle anomalies are solved within models (e.g. in the SM is solved family by family separately while other models solve them between families) . It is perfectly possible to build a QFT without a top quark making an arrangement to cancel it out. Sure you can´t find many of such models since model building in HEP starts with the SM, but a top quark does not make QFT inconsistent it makes the SM and other models inconsistent. I´m not saying the top quark solution was not a expected one, quite the contrary, but I´m not convinced it makes the "theory" (which by the way?) inconsistent. Within a certain framework, like the SM, but not necessarily, is unavoidable, but this is not to say is unavoidable as a general rule. It´s like you are saying "model building rules for renormalizable QFT depends on the existence of a top quark". That ´s not true.

27. **Reply to Bernhard**
October 2, 2011

All I can say is that an alternative-to-the-top anomaly canceling solution would be revolutionary.

28. **Bernhard**
October 2, 2011

I still think you´re talking about the SM.

29. **Reply to Bernhard**
October 2, 2011

Use the particles known prior to the top consistent with the then known facts, and tell us how anomaly cancellation does not require the top or why we don't care to have an anomaly-free gauge theory.

Over and out.

30. **Bernhard**
October 3, 2011

Sigh… Anomaly cancelation where my friend? In the SM, I have no idea how to do that. But if I'm allowed to throw in the theroy whaetver mirror particles (with high mass) that I want mass that's not difficult.

31. **Reply to Bernhard**
October 3, 2011

Sigh, you're mixing up "known experimental facts" and "standard model". If prior to top discovery, you were able to throw something together with no top **and** no anomalies that was compatible with the then-state of experimental knowledge, that would have been a valid competitor(then) to the standard model. Either such a model was proposed but is hidden behind the veil of time, or physicists were singularly unimaginative back then. Or the existence of the top was postulated on a very sound basis. Contrast with the Higgs. Right now, there are plenty of alternatives to the standard model Higgs, none of them yet ruled out by experiment.

32. **Eric**
October 3, 2011

Dear Bernhard,

It is well-known that the anomalies from the the quarks are canceled by the anomalies from the leptons in the SM, separately within each generation. Once the bottom quark and the tau lepton were discovered in the 70's, it was clear that the top quark should exist in order to satisfy the anomaly cancellation. Since the anomaly cancellation is within each generation, one could add any number of generations of quarks and leptons to the SM and still cancel anomalies.

33. **Peter Woit**
October 3, 2011

Eric, Bernhard, Reply to Bernhard,

Discussion of this not-very-interesting off-topic question has gone deep into pointlessness. Enough of this.

This Week's Hype

September 28, 2011
Categories: This Week's Hype

It had to happen. New Scientist managed to find a physicist willing to describe the OPERA result as "evidence for string theory":

> So if OPERA's results hold up, they could provide support for the existence of sterile neutrinos, extra dimensions and perhaps string theory. Such theories could also explain why gravity is so weak compared with the other fundamental forces. The theoretical particles that mediate gravity, known as gravitons, may also be closed loops of string that leak off into the bulk. "If, in the end, nobody sees anything wrong and other people reproduce OPERA's results, then I think it's evidence for string theory, in that string theory is what makes extra dimensions credible in the first place," Weiler says.

Update: hep-ph is chock-a-block with papers purporting to explain the OPERA results, using theoretical models of varying degrees of absurdity. There is however one much more sensible paper this evening, from Cohen and Glashow, which points out that superluminal neutrinos would produce electron-positron pairs via bremsstrahlung, and lose energy, which is not observed. This is also incompatible with Super-Kamionkande and IceCube data. No matter what sort of extra dimensions you introduce for the neutrinos to travel in, the OPERA claim seems to be in violent disagreement with other observations.

Comments

1. **Ben Martin**
   September 28, 2011

   I remember when New Scientist was a decent, reputable publication. This make me feel very, very old.

2. **Peter Woit**
   September 28, 2011

   Ben Martin,

   And I can remember when the only people the press could find to make absurd claims like this were obvious crackpots, not respectable physicists…

3. **Bernhard**
   September 28, 2011

   Was easy to see this one coming. String theory is the explanation for anything new, right or wrong.

4. **Proudmemberofthecult**
   September 28, 2011

   Compared to the current flood of papers on hep-th,hep-ph, gr-qc with the word superluminal this is but a small trickle. Sometimes it seems physicists are like locusts in search of food.

5. **MathPhys**
   September 29, 2011

   When I posted that link to Gubser's paper, I thought it was the only one on the subject. How silly of me.

6. **Phil**
   September 29, 2011

   I'm more sorrowful for the decline of Scientific American… as I remember it New Scientist was always sensationalist, albeit often in an entertaining sort of way. Anyhow I wouldn't beat them up too bad for this – *if* the OPERA results hold up it *will* point to *something* very unexpected and bizarre. Can't argue with that. Given how unlikely it is they'll hold up, it's admittedly a surreal discussion…

7. **Misha**
   September 29, 2011

   Who is this Weiler, after all? Is he a reputable physicist? I have not heard of him. The superluminal hype will inevitably continue for some time, since many theorists are indeed hungry and in search of ideas, but then it will fizzle out …

8. **I wonder ...**
   September 29, 2011

   If string theory explains the OPERA result, why didn't any string theorist cite their work before it came out?

9. **chris**
   September 29, 2011

   so… does that mean that when the opera result doesn't hold string theory is falsified?

10. **MathPhys**
    September 29, 2011

    Misha, I expect that Tom Weiler hasn't heard of you either.

11. **Yatima**
    September 29, 2011

    Neutrinos Faster Than Light, or Artifacts of the FPGA?: Comments About the FPGA Platform Used in the Data Acquisition System of the OPERA Experiment"

    > After devouring everything I could read about the experiment, I speculate ("gut feeling") that the explanation of these unexplainable numbers are variable

timing delays introduced by the FPGA-based data acquisition system (DAQ), for the reasons stated below.

I guess that's a possible explanation.

12. **Shantanu**
September 29, 2011

Tom Weiler is a very respected physicist at Vanderbilt who has worked on neutrinos
including astrophysical aspects.

13. **Noah Smith**
September 29, 2011

Hi Peter, this may only be on-topic regarding hype occurring this week. I'm curious how you interpret this press release. It seems to almost ignore the possibility there is no Higgs.

http://www.spacedaily.com/reports/Could_the_Higgs_boson_explain_the_size_of_the_Universe_999.html

14. **Peter Woit**
September 29, 2011

Noah,

I took a very quick look at their paper. It looks like they're trying to put together the Higgs field with another scalar field (the "dilaton"), and get an underlying scale-invariant theory with known mass scales picked out by dynamical symmetry breaking. This certainly does require a conventional Higgs field, so if no Higgs shows up at the LHC, it rules this idea out. Like most ideas, it's quite speculative, and not particularly convincing. They are trying to use it to make predictions about inflation, maybe this goes somewhere, but to evaluate that you need a cosmologist, not me. It doesn't say anything new about LHC-scale physics.

The story involves just the usual amount of hype always there whenever theorists try and promote ideas they're working on. I suppose it's worth noting sociologically that maybe now it's considered much more promising to relate one's ideas about a dilaton field in cosmology to the Higgs than to string theory…

15. **Noah Smith**
September 29, 2011

Thanks Peter, the sociology seems interesting. When I see this type of press release I wonder about the types of people involved. Last weeks neutrino news felt like a similar case. I wonder if I'm just noticing these things more often, or the frequency is actually increasing…:)

16. **Peter Woit**
September 29, 2011

Noah,

The Higgs/dilaton thing is something very common, an isolated story about some speculative theoretical idea, one that isn't getting a lot of attention from anyone except the authors.

The neutrino thing is quite different, there a major experimental collaboration is releasing an experimental result claiming to overturn fundamental principles of physics. If this were solid, it would be huge, and deserve the attention it got in the press. The problem here though is that it's a very implausible result, and not that solid. Many experiments at one time or another appear to be giving exciting data that violates fundamental principles, but this is essentially always a mistake in the experiment. So experimentalists typically are more wary than the OPERA people seem to have been about going public, since the likely end is that this will be shown to be mistaken, in which case the fewer people who see your mistake, the better…

Once the experimental result is out there, I guess it's not surprising that lots of theorists try and jump on the bandwagon and get attention for speculative ideas of their own. That anyone these days puts out a story about "evidence for string theory" with a straight face is perhaps a bit surprising.

17. **Chris Austin**
September 29, 2011

This is the list of Weiler's articles on arXiv.

18. **Daniel**
September 29, 2011

There seems to be a number of scientists who will be coming up with a posteriori explanations of the OPERA results, but that's a little too easy imho. Too many theories can be retrofitted.

My question is, are there any theories that predicted superluminal speeds, and in particular, superluminal neutrinos?

19. **Bernhard**
September 29, 2011

Daniel,

Perhaps this might be of interest to you:

http://www.sciencedirect.com/science/article/pii/0370269385904605

20. **Coin**
September 29, 2011

Yatima: … hm. Having worked with FPGAs in the commerical engineering space, I would be * really * surprised if this turned out to be the problem, just because this is the sort of thing you'd normally expect any competent FPGA engineer to get right. FPGA engineers are usually– basically have to be– highly sensitive to timing issues, and it would seem that the kinds of (highly plausible) error the blogger proposes would be very easy to test and calibrate for without requiring actual neutrinos in the system. I'm sensitive to the argument an experiment run by physicists might not think to worry about some of these issues even where it would be the first thing engineers would think of, but I'd assume if an experiment like this is hiring someone to design this sort of thing for them then that person is basically being paid to think of these sorts of things…

I mean, I do think the blog article author raises some excellent questions that deserve answers but I'm not going to expect this to be the source of error.

21. **Daniel L. Burnstein**
September 29, 2011

Thanks Bernhard. Very interesting indeed.

22. **M**
September 30, 2011

Notice that already http://arxiv.org/abs/1109.5682 had an inconsistency argument against the OPERA anomaly:
"We point out that, quite generically, electroweak quantum corrections transfer the information of superluminal neutrino properties into Lorentz violations in the electron and muon sector, in apparent conflict with experimental data."

The argument from Cohen and Glashow is probably more clean.

23. **Shantanu**
September 30, 2011

Peter, this maybe offtopic , but what happened to the unparticle by Georgi a few years ago? Is it completely ruled out?
Thanks

24. **Sascha Vongehr**
September 30, 2011

Actually, one can in hind sight say that the results were expected:
http://www.science20.com/alpha_meme/expect_tachyonic_neutrinos_have_their_higgs_and_smoke_it-83157
Anyway, why so negative?

25. **Anon**
September 30, 2011

One thing that bothers me, is that if neutrinos are tachyons, why should all extant measurements of their speed give a result so close to the speed of light, instead of being distributed over all possible velocities on their purported tachyonic mass hyperboloid? In an experiment like this one, maybe there is a kinematic reason, but what about the supernova observations? Would there also be a kinematic reason?

26. **somebody**
September 30, 2011

Me and none of my string theory colleagues have ever heard of this Weiler character. There are plenty of fossils with tenure in various universities. Not suggesting that Weiler is one such, but it also doesn't mean that his opinions are the opinions of the string "community".

Gubser paper says in fact that it is not easy to explain superluminal neutrinos even with extra dimensions, if you only have ordinary matter. Anybody who wants some attention from the media just has to make some statement (positive or negative) about string theory. Shrug.

27. **J**
September 30, 2011

There is actually a paper focusing on the experimental issues

http://arxiv.org/abs/1109.6160

which seems to suggest "the effect of the synchronisation convention is not properly taken into account in the OPERA analysis and may well invalidate their interpretation of superluminal neutrino velocity".

28. **M. Wang**
September 30, 2011

Thanks for the link to Cohen & Glashow. It is most pertinent. Now theorists can conclusively say that the OPERA result has to be wrong.

Too bad that Sidney is no longer with us, but I am glad to see that Sheldon is there to cut through the clutter in 3 simple pages. They don't seem to make physicists like them any more.

29. **DPB**
September 30, 2011

@M. Wang

While I agree that superluminal neutrinos are incredibly unlikely I have to disagree on one major point. It isn't the job of theorists to conclusively say that experimentalists are wrong. Experiments must always have the final say, eventually.

30. **abbyyorker**
September 30, 2011

DPB

According to Glashow and Cohen, the experimental evidence IS there, but the experimentalists could not parse it. Not dumping on them but theorists have a bigger role than you imply.

31. **high school physics teacher**
September 30, 2011

Sir,
Having Glashow's tour de force "refutation" existing under the main heading of This Week's Hype seems a bit amiss. I'm just saying…

32. **MathPhys**
October 1, 2011

"Me and none of my string theory colleagues have ever heard of this Weiler character. "

If your string theory is as good as your grammar, I don't think my colleagues and I will ever hear of you.

33. **D R Lunsford**
October 1, 2011

Glashow seems to keep a pretty low profile, probably because he's just a nice man and doesn't want to get involved in controversies. In this case, I get the strong feeling that "enough was enough", and the world's great expert on neutrinos had to say something correct 🙂

-drl

34. **Shantanu**
October 1, 2011

Its also nice to see 3 of my papers cited in Cohen and Glashow reference (ref 8 to 10) 🙂

35. **Aaron Sheldon**
October 1, 2011

As a statistician, after reviewing the OPERA paper, the analysis conducted is seriously flawed. They fitted only a single MLE for a time offset against the empirical proton distribution; that is they fitted assuming all the neutrinos have a uniform time of flight. They should have fit additional MLEs for the dispersal in neutrino velocities. This oversight should have sunk the paper out of the gate, as it makes the numerical analysis is fatally flawed. It is very likely that including MLEs for velocity dispersal would have reduced the time offset MLE to within the range of the systematic uncertainties and biases.

Overall it is a bad sign for the quality of the editorial statistical review in the literature.

36. **Aaron Sheldon**
October 1, 2011

Just to be clear how the dispersal MLE would work. One would fit for the parameters of a normal distribution that is convoluted with the empirical proton distribution, where the mean is the time offset and the standard deviation is the velocity dispersal along the flight path.

37. **doctor physics**
October 1, 2011

would i be far off, in saying, that this seems to be not-even-wrong theories seeking out faulty experimental results as life-blood?

38. **David Nataf**
October 1, 2011

Aaron,

Would you care to elaborate on why that would matter?

Are the mean and variance of a Gaussian not supposed to be uncorrelated?

39. **Aaron Sheldon**
October 2, 2011

Basically the opposite of over fitting the data, you are under fitting the data. In the OPERA article they assumed they knew more about the velocity distribution then they actually did. If you assume you know less about how the velocity is distributed, then your uncertainty in the time offset is going to be much greater. Really they assumed there is much less variance (and co-variance) in the data then their actually is.

The dirty little secret of statistics is that p-value, and sigma counts are really just indicators of likelihood of convergence of the MLE to a particular value, not the validity of the model. If you choose the wrong distributions to fit to the data, the MLE will in most cases still converge (as long as the Kullback-Leibler divergence of the fitted distribution against the actual, unknown, distribution is finite), but the answer will not be meaning full.

The best test would be to use the likelihood ratio test to compare the convolution with the normal hypothesis to the linear shift hypothesis, because the linear shift is a sub-space of the convolution hypothesis (normal with zero variance, i.e. the Dirac linear shift functional).

It will take a solid week to work out the differences formally, but basically it comes down to work on a couple theorems like this:

1. Given a family of probability density functions f(x – \theta) parameterized by \theta, if we find the MLE of the family of probability density functions parameterized by a convolution with the normal distribution n({x – \mu} over \sigma) \star f(x) then the distribution of the MLE of \mu and \sigma are…

2. (dual) Given a family of probability density functions parameterized by a convolution with the normal distribution n({x – \mu} \over \sigma) \star f(x), if we find the MLE of the family of probability density functions f(x – \theta) then the distribution of the MLE of \theta is…

the physics community should take some solace that this is an incredible common mistake in the life sciences, my particular haunt, and hence I've become particularly attuned to this error.

40. **Aaron Sheldon**
October 2, 2011

PS,

A literature search on my name will not turn up much of anything meaningful.

Because I am a senior analyst for a large health care provider much of the work I do is either privileged within the organization and its oversight body, or is restricted by the prohibition against the secondary use patient information laid out in health information laws in the jurisdiction in which I work.

41. **Reply to Zathras**
October 2, 2011

OPERA assumed that all neutrinos travel at the same speed. Theoretically there was no a priori reason to believe that neutrinos with an average of 17 GeV energy travel with any significant speed dispersion.

42. **Reply to Zathras**
October 2, 2011

One more point – it is difficult to be sure from the OPERA paper, but it does appear that the time interval from the earliest to last detected neutrino events matches the duration of the proton pulse – i.e., there is no spreading. It bothers me – physical processes tend to spread pulses. A 60 ns spread in a 10,000 ns pulse is only 6 parts per 1000, and yet might swallow up the 60 ns that they found. On the other hand it is difficult to think of a known physical process that would cause so much spread. So as far as we know there is no spreading of the pulse; and so, at least to me, it seems there can be no speed dispersion of significance.

43. **Aaron Sheldon**
October 2, 2011

Unfortunately the Law of Large Numbers is not as kind to such assumptions as one would assume, and in the case of the OPERA results, all the 6-sigma level tells me was that the N was more than large enough to get good convergence of the MLE, but little else.

As I stated before a statistical hypothesis that understates the sources of variability will over state the statistical confidence of the MLE. Without writing a single equation down I can tell you with certainty that if you do not include dispersion in your statistical fit of the MLE you will underestimate the variance in the fitted MLE.

In information theoretic terms, adding the dispersion “uses up” part of the information in the data set, and so the variance in the time offset will be larger because you have less information to constrain its value.

44. **David Nataf**
October 2, 2011

Zathras,

1) You have a fantastic screen name for someone on a time travel discussion thread.
2) If the neutrino has a mass, real or imaginary, does that not mean that an energy dispersion implies an velocity dispersion?

In light of the very small effect they “measure”, even an energy dispersion of say… 1%, could give the velocity dispersion sufficient to yield the systematic effects mentioned by Aaron.

45. **Reply to Zathras**
October 2, 2011

David,
Wiki says the muon neutrino has a mass less than 170 KeV. In conventional Einstein theory, at 1700 KeV, already the neutrino is within 0.005% of the speed of light, and we are talking here of neutrinos with an average energy of 17 GeV = 17,000,000 KeV. There is no room for a 1% speed dispersion here, if the speed of light is the upper limit of speed. Note also that the superluminal speed that OPERA finds is about 0.0025% faster than light.

The second point, on which I would like clarity, if anyone has it, is what I pointed out already – there seems to be no spreading of the pulse, and that would suggest no speed dispersion, even if the neutrinos were superluminal.

Unless there is something new to say, I’ll be silent now; Peter does not like repetitions.

46. **Aaron Sheldon**
October 2, 2011

Actually, according to the paper they have sufficient N to detect a dispersion of about 1 part in a thousand, but the cost would be that the 6-sigma intervals would be much larger (roughly your MLE distribution is over area instead of length).

Interestingly, this is also why political polls are so unreliable, because they underestimate the variance, which is calculated for simple binary choices, when in fact political polls typically have four or more choices depending on the number of candidates. It comes down to the problem of volume, if you want 90% of a centimetre you only need 9 millimetres, but an area of 9 X 9 millimetres is only 89% of the square centimetre, you need ~9.48 X 9.48 millimeters to get 90% of a square centimetre…the logic continues inductively, remembering that probability is just a fancy way of saying volume.

47. **Aaron Sheldon**
October 2, 2011

About the restrictions due to being close to the speed of light, that actually adds one more hypothesis with one more unknown parameter that needs to be fit. To enumerate:

1. A simple uniform time shift with a single parameter, the time shift.

2. Convolution with the normal distribution with two parameters, the time shift and the dispersion

3. Convolution with the normal distribution where the underlying variable is first Lorentz transformed by some unknown speed (which destroys the symmetry of the dispersion), this has three parameters, the time shift, the dispersion, and the speed of the Lorentz transformation.

Hypothesis 2 is a sub-space of hypothesis 1 in the limit that the Lorentz speed parameter goes to 0.

Hypothesis 1 is a sub-space of hypothesis 2 in the limit that the dispersion goes to 0.

This makes all of these hypotheses ripe for the likelihood ratio test.

And that is pretty much how a statistician thinks.

48. **Aaron Sheldon**
October 2, 2011

Opps hypothesis 2 is a sub-space of hypothesis 3…

I think you can see where I was going

49. **Bob McElrath**
October 2, 2011

Aaron Sheldon,

First, the assumption of velocity dispersion is, not reasonable from a physics perspective. To have a delta-v large enough to affect the pdf, the energy of the neutrino would be so low that it would not be detected. One can easily calculate the expected dispersion from several sources and it is vanishingly small. Second, from a statistics perspective, the assumption of velocity dispersion would broaden the observed distribution of arrival times, as well as possibly bias it toward later arrival times. A careful look at Figs. 11 and 12 in the paper shows that this is not supported by the data.

Second I think you are misinterpreting the "six sigma" quoted as a statistical p-value, and as that p-value is very tiny, you're attributing disbelief in that small value to an error in fitting. It is not a p-value. The experiment clearly does not have enough statistics to report such a p-value (they only have ~16000 events). In physics the distribution of errors on a counting experiment such as this is generally unknowable, and is generally not Gaussian. One can only know the actual arrival time error function as well as the 16000 events will allow you. So what happens instead is that a fit is done, and a central value for that fit, with 1-sigma error bars is reported. When the 1-sigma error bar is separated from the expected value by a factor of six, we report "six sigma". That is six times the 68% confidence interval, *not* a statistical p-value of 2e-9. e.g. the unobserved tails of the arrival time pdf are assumed to be gaussian. They never are. They are always much flatter than gaussian far away from the central value. The 5-sigma used in physics to report a "discovery" is a rule of thumb, forced upon us by the fact that we cannot reasonably determine the pdf of the measurement to the precision required. In most cases we can simulate the pdf using a model of the detector and backgrounds, but simulating the tails to the required precision to report the p-values of a discovery is out of the question computationally. Furthermore, the tails tend to be dominated by extremely rare physics that is not accounted for in the simulation. There are a very large number of possible sources of 0.1% errors, most of which are not known, and to chase them all down in order to improve the simulation is impractical from a manpower perspective, and usually impossible physically. In our field, we see three and four "sigma" results disappear on a regular basis, while any reasonable statistician would agree that the p-value corresponding to three sigma is sufficient to claim a discovery. Often the cause of an erroneous measurement is never actually discovered, but subsequent experiments fail to confirm it, and the older experiment is dismissed.

50. **Bernhard**
October 2, 2011

Off-topic: Witten will be Ireland talking about "The Quantum Theory of Knots.":

http://www.irishtimes.com/newspaper/sciencetoday/2011/0929/1224304927736.html

51. **Daniel L. Burnstein**
October 2, 2011

http://muon.wordpress.com/2011/10/01/theories-about-superluminal-neutrinos/

A list of theories about OPERA results.

52. **Reply to Bernhard**
October 2, 2011

J mentioned: http://arxiv.org/abs/1109.6160
The story seems to be that a portable atomic clock was synchronized with the OPERA clock, and then transported to CERN, and used to measure the offset with the clock there. There are obviously all kinds of problems – special and general relativistic effects would make that measurement useless.

Fortunately, the "Portable Time Transfer Device" that was used does not such thing. OPERA used highly accurate GPS based clock synchronization. The common view time transfer mode is explained here:
http://tf.nist.gov/time/commonviewgps.htm

The one problem is to measure accurately the offset between the GPS signal and the local clock, and the "Portable Time Transfer Device" enables the use of exactly the same device at both CERN and OPERA, so that uncertainties introduced by the use of different devices is eliminated.

53. **ZZZ**
October 2, 2011

The Cohen and Glashow paper does not prove the OPERA result wrong, but instead suggest a way to reconcile OPERA with the 1987 supernova observations.

OPERA showed that the average velocity of the neutrinos over their first 730km of travel was higher than c. C&G suggests that neutrinos can slow down to c via pair production. Therefore their average velocity from the supernova to Earth can remain very close to c even if their initial velocity was higher.

This deceleration might also explain why OPERA observes neutrinos with different energies to have average velocities that differ only slightly.

54. **Reply to Bernhard**
October 2, 2011

ZZZ, OPERA reports seeing neutrinos of 42.9 GeV or higher. Cohen and Glashow say that if neutrinos are superluminal, even travelling over just 730 kilometers, they shed energy so rapidly that whatever their initial energy, few if any should have reached OPERA with energies greater than 12.5 GeV. "The observation of neutrinos with energies in excess of 12.5 GeV cannot be reconciled with the claimed superluminal neutrino velocity measurement." If C&G are correct, OPERA is wrong.

55. **Aaron Sheldon**
October 2, 2011

Bob,

First I'm not any reasonable statistician, I'm in fact a quite unreasonable one, who is more of an extremely diligent probability theorist disguised as a statistician.

What it comes down to is very simple, they fit a flawed statistical model. The attractively simple model of a linear time shift, assumes a prior that any and every time shift is possible. All the 6-sigma indicates is that there is good convergence in the MLE, not that the finding is correct. Bluntly MLEs do not test hypotheses.

Another possible attack they could have used is to propose a model with a parameter controlling the lower bound of time shifts allowed, and that limits toward the simple linear shift model. Then fit the MLEs of each model and use the likelihood ratio test to compare the two models.

56. **Mitchell Porter**
October 3, 2011

I bet that in the coming weeks, the assumptions behind the derivation of superluminal bremsstrahlung will be a point of contention. There will be a need to return to first principles. For those who want to play, http://arxiv.org/abs/hep-th/0611263 revisits Wigner's classification of representations of the Poincare group and includes the tachyonic cases…

57. **Mitchell Porter**
October 3, 2011

There's also a large literature on tachyon physics, like Feinberg's original paper, and the work of Dawe and Hines, which so far isn't playing any role in the discussion about OPERA. (Dawe and Hines raise the issue of tachyonic bremsstrahlung in their second paper.) So far the model builders are all trying to "save causality", e.g. with extra dimension models in which the true relativistic speed limit is only realized off our braneworld. But the next step has to be an examination of models in which there are closed timelike curves and retrocausal influences. There have been various attempts to explain quantum mechanics itself as arising from such effects (e.g. http://arxiv.org/abs/gr-qc/0703150) or to employ quantum mechanics in order to neutralize their paradoxical threat (e.g. http://arxiv.org/abs/1005.2219). Hopefully one side effect of OPERA's announcement will be a closer examination of these issues.

58. **Chris Oakley**
October 3, 2011

Mitchell,

The Bekaert/Boulanger paper points out (near the end) that tachyonic unitary irreps cannot have finite-dimensional spin, whereas the neutrino has spin 1/2, which is finite (NB: I look at tachyonic ISO(3,1) myself on page 12 here: http://www.cgoakley.demon.co.uk/qft/RQM.pdf) so in the unlikely event that there is anything superluminal here, Unitary Irreps of the Poincare group are probably going to be an early casualty.

59. **Igor Khavkine**
October 3, 2011

For those entertaining thoughts of using tachyonic representations of the Poincaré group to explain superluminal neutrino propagation, isn't anyone bother by the fact that the wave equation whose solutions furnish such representations is essentially the Klein-Gordon equation with the sign of the mass term flipped? The reason someone should be bothered by this is that a general theory of hyperbolic PDEs shows that the speed of signal propagation (say as characterized by the support of the retarded Green function) entirely depends on the highest derivative terms, which are unaffected by the sign flip. In other words, the speed of signal propagation by Poincaré invariant "tachyonic" wave equations remains squarely ≤ c.

60. **Chris Oakley**
October 3, 2011

Igor,

Not following you here. Tachyonic Poincare irreps have E = √((pc)^2-(μc^2)^2), so the dispersion relation of the de Broglie waves is ω=√((kc)^2-(μc^2/hbar)^2) and hence group velocity ∂ω/∂k = kc/√(k^2-(μc/hbar)^2) which is greater than c.

61. **Marcus**
October 3, 2011

Remarkable paper by Dmitri Nanopoulos and co-author where he gets string to explain stuff going all different speeds in agreement with various observations:
http://arxiv.org/abs/1110.0451
Background Dependent Lorentz Violation from String Theory

I know there are scads of "string theory predicts FTL neutrino" papers, but this could just be a cut above the rest. It explains how they can go FTL thru rock here on earth and yet slower than light out in space between galaxies. It takes the cake.

62. **Igor Khavkine**
October 4, 2011

Chris,

However fast either group or phase velocities are, the information contained in a pulse (be it encoded in a peak or some kind of modulation) cannot outrun the discontinuous edge of the same pulse.

If you model signal propagation with a **retarded** solution of the "tachyonic" wave equation *box φ+(m^2) φ = f* , where *f* is a source with support in a bounded region *S*. Then, interpreting *&phi* as carrying the news about what happened in *S*, the retarded boundary conditions ensure that *φ* is exactly zero everywhere outside the domain of influence of *S* (the region reached by **null** or **timelike** rays emanating from *S*). Hence, the news of what happened in *S* does not reach anywhere faster than light (null rays). This conclusion holds independently of the sign of the *(m^2)φ* term.

If one disallows generation of signals through discontinuous (just non-analytic is enough, actually) processes, then it becomes very difficult to draw the distinction between communication and a priori conspiracy. Then the notion of the speed of a signal becomes significantly muddier.

63. **Anon**
October 7, 2011

Ehrlich dismisses Cohen and Glashow's objection (and some other objections) here: http://arxiv.org/abs/1110.0736v2

64. **Reply to Zathras**
October 7, 2011

Ehrlich argues that the high energy neutrino events logged at OPERA were caused by subluminal neutrinos, and the overall velocity determination is by a mix of super- and sub-luminal neutrinos. Interesting idea, but this would justify Aaron Sheldon's point on the previous page that speed dispersion needs to be part of the statistical analysis.

65. **Henry Bolden**
October 7, 2011

A good, old-fashioned clock synchronization problem may the source of the apparent FTL neutrinos according to:

“The OPERA neutrino velocity result and the synchronisation of clocks” by Carlo R. Contaldi

http://arxiv.org/abs/1109.6160

66. **Reply to Bernhard**
October 8, 2011

Contaldi wrote: “In an effort to go beyond this accuracy thresh- old, the OPERA experiment employed a travelling Time- Transfer Device (TTD) to calibrate the difference in time signals at each receiver. We assume this device to be a transportable atomic clock of sufficient accuracy.”

Unfortunately his assumption is wrong. The Travelling Time Transfer Device was not a transportable atomic clock. It was yet another portable clock synchronized by GPS and carrying a time difference counter. The TTD was used at CERN and at Gran Sasso to measure an offset between the GPS signal and the local clock. To the extent the TTD remained physically identical at CERN and Gran Sasso, its antenna, antenna cable, electronics delays remained identical, and hence the difference of offsets it measured gives the difference in the two local clocks with high accuracy.

67. **Jeff Moreland**
October 11, 2011

Did anyone predict superluminal particles based on string theory? That would be much more impressive than an after-the-fact rationalisation.

68. **Tim van Beek**
October 19, 2011

For those interested in a review of relativistic effects in GPS, have a look at this:

* Neil Ashby: “Relativity in the Global Positioning System”, online at “living reviews” here.

New Pursuit of Schrodinger's Cat

October 3, 2011
Categories: Uncategorized

Prospect magazine has an excellent new article by Philip Ball on recent developments in the fundamental problem of the interpretation of quantum mechanics: why don't we see superpositions? Most popular discussions of this seem to me to be stuck back in debates from the 1930s, and ignore the main question. QM is a simple, beautiful mathematical structure that works perfectly experimentally, the confusing question is that of how classical behavior emerges during a measurement process (typically involving huge numbers of degrees of freedom, making analysis difficult).

At the end of the article, Ball mentions one particularly intriguing set of ideas, due to Wojciech Zurek, about "quantum Darwinism". For more about this, see Zurek's survey here.

There's a new preprint out by Steven Weinberg on Collapse of the State Vector. Weinberg claims that "There is now no entirely satisfactory interpretation of quantum mechanics", and refers for more detail about this claim to Section 3.7 of his *Lectures on Quantum Mechanics*, a manuscript that is "to be published". I'm definitely looking forward to this book when it comes out. The sort of thing that Weinberg examines in the preprint though, modifying QM to take into account wave-function collapse, is the kind of idea I've never found promising. Why modify QM, it works perfectly and is mathematically extremely aesthetically compelling? Better to keep QM as is, and closely examine one's understanding of what the problem really is.

Comments

1. **Mike**
   October 3, 2011

   Peter,

   I agree that modifying QM does not look promising. As Zurek notes in the paper to which you link, his mathematical approach looks a lot like Everett — which just takes the equations as they come — without making any ontological judgements, which Zurek calls a "virtue". Not sure I agree on that point, but I do agree with you that we probably shouldn't go around changing a very successful theory just to find an "acceptable" foundational interpretation — if that's the point you were making.

2. **Joel Rice**
   October 3, 2011

   The Ball article did not mention Feynman's approach in 'QED: the strange theory…', which seems quite a bit less spooky than the Schrodinger wave approach – though the two are supposed to be equivalent. The possible paths go thru both slits, but nothing like a photon somehow going through both in some spooky fashion.

3. **Peter Woit**
   October 3, 2011

   Joel Rice,

   The Feynman path integral formalism is supposed to be completely equivalent to the Schrodinger equation formalism for QM, and has nothing to say about the measurement problem that Ball was writing about. You can't invoke it to solve the problem.

4. **Anon**
   October 3, 2011

   A "measurement" (at least in human terms) always involves, even if implicitly, space position and time. So it's not unfathomable if the final answer would be given by unified quantum theory of space-time as suggested by R.Penrose. It just may be that classical notions of space-time are logically incompatible with the quantum world that is supposed to live in it. That's why asking these questions isn't useless and may lead, in the eventuality, to a valuable insights.

5. **Peter Woit**
   October 3, 2011

   Anon,

   Quantizing space and time variables brings in a much more difficult set of problems. They're important and interesting, but I've never seen much evidence for the idea that they have something to do with the measurement problem. It seems to me that what Zurek is doing is much more promising, concentrating on trying to understand the (difficult enough) problem at hand (QM in a fixed space-time geometry). One should be wary of trying to solve a hard problem by embedding it in an insanely hard one, especially if one hasn't first completely understood what the hard problem itself actually is.

6. **Derek**
   October 3, 2011

   Could someone comment on whether the Couder experiment (Single-particle interference observed for macroscopic objects) is, in any way, relevant to quantum theory?

   Somewhat embarrassingly, I learned of both this experiment and the de Broglie–Bohm theory / Pilot wave theory via an episode of Through the Wormhole (section begins at 16:30).

7. **Peter Woit**
   October 3, 2011

   A request to all: please limit comments to the topics of this post (articles by Ball, Zurek, Weinberg). I've no more time for moderating a general discussion of quantum mechanics.

   Derek,

   I hadn't heard of the Couder experiment before. From a quick look, it seems like it doesn't actually have much to do with QM, but rather is an experimental set-up that is supposed to behave deterministically according to the pilot-wave equations. It's typical that this and de Broglie-Bohm are the sort of thing you "learn" about from "Through the Wormhole", which completely mixes up real physics issues with hogwash. "Pilot wave theory" may or may not succeed as an awkward way to rewrite single-particle QM, but mathematically it is much more shallow than QM. Furthermore it doesn't generalize well to a true fundamental theory, which needs to be a relativistic quantum theory of fields and incorporate the symmetries and structures of the Standard Model.

8. **Mitchell Porter**
   October 3, 2011

   I agree with Mike that what Zurek writes sounds a lot like the many-worlds interpretation. Except that for some reason he doesn't want to go there, and so he writes gibberish like the paragraph at the end of section III.

   For example, he says there that "Quantum states acquire objective existence when reproduced in many copies." What on earth does that mean? Would he apply this criterion of existence to the Andromeda galaxy as well? You know, there only seems to be one Andromeda galaxy, and yet we don't consider it any less real for that reason.

9. **Peter Woit**
   October 3, 2011

   Mitchell,

   The difference is that Zurek is trying to actually understand how classical behavior emerges from QM, this is not part of the standard Many Worlds Interpretation as far as I know.

10. **neo**
    October 3, 2011

    I question Ball's assertion that decoherence can be suppressed with an "isolated" Schrodinger cat experiment. If he means an actual cat, there is no way it can be isolated. If he means a laboratory buckyball as a metaphoric cat, then the essence of the paradox (quantum uncertainty applying to a familiar macroscopic entity) is lost.

11. **Bernhard**
    October 4, 2011

    Peter,

    Zurek article is interesting, but I think it goes into the category "philosophically interesting". I´m not sure how can one go forward from Zurek´s ideas. Can one suggest new experiments, new tests that will shed new light on what we already know about QM? Otherwise I´m afraid the debate will end up going to where you don ´t want it to go, .i.e., a general debate about QM interpretation, that will be appealing to some but not others with no way of deciding who is right.

    For example concerning how classical behavior emerges during a measurement process, how do decide if quantum darwinism is any better then what we learn at undergraduate school if not by philosophical taste?

12. **CWJ**
    October 4, 2011

    Off topic, I know, but congratulations for successfully predicting the Nobel this year. (Comments appear to have been turned off in the relevant post.)

13. **Allan Rosenberg**
    October 4, 2011

    I find it refreshing to see a science journalist suggesting that we address interpretational questions experimentally. Can anyone point me to any reviews of the real experiments that explore the classical-quantum boundary (or whatever it is)? I thought Haroche and Raimond did a pretty good job of this in Exploring the Quantum (2006).

14. **new fan**
    October 4, 2011

    Hi Peter,

    I enjoyed your book and I'm a fan of your blog. Regarding the Ball article, many discussions of the "measurement problem" neglect the fact that classical and quantum-mechanical measurement theories have much in common. See, for example, A.E. Glassgold and D. Holliday, Phys. Rev. 155, 1431-1437 (1967). In particular, note the discussion of the limit hbar –> 0.

    Keep up the good work!

15. **Anon**
    October 7, 2011

    With due respect, as best I can tell, Weinberg's paper is contentless.

16. **George Purdy**
    October 20, 2011

    I agree with Peter. Modifying a theory that has never been wrong seems like a bad idea. There are some things worth changing. Throw out those things in the Copenhagen Interpretation that aren't actually part of QM–for example the idea that a measurement must disturb A, or must disturb it by a certain amount, which is disproved by Aharonov's weak measurement.

Two for Two

October 4, 2011
Categories: Uncategorized

Back in 2004 I made my first venture into Nobel Prize predictions, then decided to retire from that business. This year I came out of retirement with another prediction. After the posting, I consulted with experts who assured me that the right names were Perlmutter, Riess and Schmidt, something I thought I mentioned in a comment, but it appears that I didn't, instead leaving this to Shantanu.

Congratulations to Perlmutter, Riess and Schmidt. The theoretical significance of their tour de force observational work remains still controversial, but it richly deserves the Nobel prize.

Comments

1. **Some Guy**
   October 4, 2011

   Yes, Shantanu nailed it. Riess' presence at http://agenda.albanova.se/conferenceDisplay.py?confId=1125 this spring was a strong hint. What I find surprising is the unequivocal motivation, "for the discovery of the accelerating expansion of the Universe through observations of distant supernovae". No room left at all for all the ongoing work on dark flows, anisotropies etc. The "scientific background" document, http://www.nobelprize.org/nobel_prizes/physics/laureates/2011/sciback_fy_en_11.pdf , is also quite unequivocal, starting from the title. I may have to write something about the politics of it all… somewhere else, under my own name.

2. **SpearMarktheSecond**
   October 4, 2011

   Bravo, although perhaps the winners might have preferred a satellite devoted to dark energy measurements. Doubtful that the US political class will reverse the current course and give it to them based upon Scandinavian laurels.

   Now perhaps dark matter can get a price. Vera Rubin?

3. **Christopher Nubbles**
   October 4, 2011

   I really thought Guth was going to win it this year.

4. **Math Student**
   October 4, 2011

   No share for Weinberg for the prediction? 🙂

5. **Shantanu**
   October 5, 2011

   Christopher,
   We still don't have any smoking gun evidence that inflation happened or if it did, what caused inflation. Inflation also has a whole set of conceptual problems (cd Steinhardt article from Sci. Am in 2011).

6. **John**
   October 5, 2011

   In the scientific justification of Nobel Prize there is the sentence "None of the alternative models … extra dimensions or modifications of general relativity, seem to convincingly account for all observations." At the announcement video I can see Lars Brink one of the godfathers of string theory, he also seems to be a member of the Nobel Prize Committee. Does this mean he is getting doubts?

P-adic Numbers and Cosmology

October 4, 2011
Categories: Multiverse Mania

The next math department colloquium at Stanford will feature Lenny Susskind lecturing on p-adic numbers and cosmology, here's the abstract:

> The biggest conceptual problem of cosmology is called the measure problem. It has to do with the assignment of probabilities in an exponentially inflating universe, which falls apart into separate causally-disconnected regions. Neither I nor my friends had ever intended to learn about p-adic numbers until we realized how similar such a universe is to an endlessly growing tree-graph. The result has been some new insights from p-adic number theory into the measure problem and other puzzles of eternal inflation. Within the constraints of a one-hour lecture, I will explain as much of this as I can.

I've no idea what this is about, but I'm guessing that Susskind is somehow drawing inspiration from two facts:

- p-adic integers can be represented using a "tree" diagram vaguely remniscent of the logo for the Stanford theoretical physics group on their web-site.
- The p-adic integers, unlike the usual integers, are compact, so you can put a finite measure on them.

It's hard to believe that any of the special features of these mathematical structures will make the problems of eternal inflation go away, but who knows…

Coincidentally, I've spent a lot of time recently learning about the p-adics, with a very different motivation. The way these things come up in mathematics is that you can think of number theory as being about a space, the space of prime numbers. The p-adics appear naturally when you decide to ask what happens locally near one point (i.e. at one prime). P-adic integers correspond to power series expansions, p-adic numbers to Laurent series. Various people have thought about analogies between conformal field theories on a Riemann surface, where one also wants to focus on what happens at a point and use representation theory methods, and the Langlands program which does something similar in number theory. This is part of the geometric Langlands story, and has roots in a remarkable paper of Witten's from 1988 entitled Quantum field theory, Grassmannians, and algebraic curves.

As I've mentioned before, this semester here at Columbia we have Harvard's Dick Gross as Eilenberg lecturer, and he's giving a wonderful series of lectures starting with local Langlands. I'm hoping at some point to put together what I've been learning about this and possible connections to QFT in some readable form, but at the moment things are still too speculative and hazy. In any case, no sign that these ideas are going to solve the problems of cosmology…

Update: The Susskind et al. paper on this topic is now out at the arXiv. A p-adic model is studied, but no reason is given to believe that it has anything to do with eternal inflation and cosmology.

Comments

1. **Octoploid**
   October 4, 2011

   The link to the Witten paper is wrong (missing w in www):
   http://ww.springerlink.com/content/k30v44524276r854/

   Why not link to the free version instead?
   http://projecteuclid.org/DPubS?service=UI&version=1.0&verb=Display&handle=euclid.cmp/1104160350

2. **Peter Woit**
   October 4, 2011

   Octoploid,

   Thanks, will fix and point to the Euclid version.

3. **Shantanu**
   October 4, 2011

   Peter when is the last time you met (or corresponded with) Lenny?
   Or let us know the talk and your meeting with him goes

4. **Chris Austin**
   October 4, 2011

   I would guess Lenny might be trying to deal with the problem that if eternal inflation is past eternal as well as future eternal, and the 3 extended spatial dimensions are infinite flat space, and the bubble nucleation rate \Gamma_{tot} is always less than R_{de Sitter}^{-4}, so that bubbles don't collide, then every bubble is contained in an infinite tower of bubbles extending into the past, and every bubble has an infinite nest of descendant bubbles, which does indeed look like an infinitely branching tree.

   Using spatially flat FRW coordinates, the worldsheet of each bubble stops expanding relative to the spatial coordinates after about the de Sitter time and tends to become a tube of fixed coordinate radius, inside which the worldsheets of its descendant bubbles develop into tubes of ever smaller coordinate radius.

   The vacuum energy density of each child bubble has to be less than the vacuum energy density of its parent by a non-zero amount, so if you want eternal inflation to be some sort of "steady state" model for which you can define time-independent probabilities for finding yourself inside a bubble with given characteristics, you face the problem that since the branching tree containing our bubble is not a steady state because the vacuum energies are decreasing, your "steady state" model also has to contain an infinite number of *other* infinite branching trees of bubbles, shifted backwards and forwards in time relative to "our" infinite branching tree, by all possible amounts.

   But since the infinite tower of bubbles in the past of "our" infinite branching tree contains bubbles whose coordinate radii presumably get ever larger without limit as we go to earlier times, there is no "room" in the usual infinite flat extended spatial dimensions to put those other infinite branching trees of bubbles. So perhaps Lenny is trying to use the compactness property of p-adic numbers to get around this. I would guess that the resulting "enlarged" spatial dimensions might look something like infinite numbers from Non-standard analysis.

   In my opinion, a simpler resolution of the problem would be to give up the idea of having a "steady state" picture of eternal inflation, with time-independent probabilities, and the corresponding requirement that eternal inflation be past infinite.

5. **Bernhard**
October 4, 2011

I just hope Susskind will stay with cosmology problems and not try to use p-adic numbers to make nonsensical claims about multiverses.

6. **Peter Woit**
October 4, 2011

Shantanu,

My only interaction directly with Susskind was asking him a question when questions were called for after a colloquium talk he gave here many years ago promoting the anthropic multiverse. He basically politely refused to answer. While we've never met, he does seem to have various opinions about me, positive and negative, which he made clear publicly during the "string wars" period, see for instance

http://www.math.columbia.edu/~woit/wordpress/?p=454

and

http://www.math.columbia.edu/~woit/wordpress/?p=437

I have no idea what he's trying to do with p-adics nowadays, but don't see how they could help him deal with the measure problem. The general problem of most of the multiverse people seems to me that they are trying to sum over "all universes" without any non-trivial input as to what the set of "all universes" looks like. Unless you start with non-trivial structure, I don't see how you get anything non-trivial out. But, who knows… I'll be curious to hear what this is about from anyone who attends the talk at Stanford.

7. **plm**
October 5, 2011

Perhaps you can explain a little more what you thought when writing:

"The p-adic integers, unlike the usual integers, are compact, so you can put a finite measure on them."

You can put finite measures on any measurable space. Any absolutely convergent series gives one on Z, any absolutely convergent integral on R, etc.

Thanks.

8. **Peter Woit**
October 5, 2011

plm,

To be more accurate I should have specified something like "translation invariant". The point is that on the p-adic integers the Haar measure you get from thinking of them as an additive group can be normalized to get a finite result, something you can't do for the usual integers.

9. **jpd**
October 5, 2011

so its bubbles all the way down?

10. **Chris Austin**
October 5, 2011

Correction to my comment above: the infinite number of branching trees of bubbles, shifted backwards and forwards in time by all possible amounts, that you need for a true "steady state" model of eternal inflation, actually *can* live side by side in the spatially flat FRW coordinates of what Harlow, Shenker, Stanford, and Susskind (HSSS) call the noncompact case, (page 4). The point being that the physical 4-volume of a region bounded by a fixed spatial coordinate sphere and extending from a fixed FRW time into the infinite past is finite, due to the exponential time dependence of the FRW R(t). So in the noncompact case, just like in the compact case, there can reasonably be a first, or outermost, bubble in the history of any bubble.

It seems to me that the numbering of the descendant bubbles of a given outermost bubble by the p-adic integers, (Fig 6 on page 16), is simply the natural base p decimal numbering of the reals >= 0 and less than 1, reflected in the decimal point. And the p-adic numbers used for distinguishing descendant bubbles of different outermost bubbles, (eqn (3.7) on page 17), are the reals written in base p and reflected in the decimal point.

The HSSS scheme usefully answers the following question: in the noncompact case, how should we label the outermost bubbles that are present at a fixed FRW time t? The answer: number the outermost bubbles such that their number roughly increases with their distance from the origin of the spatial coordinates, but such that *the larger the coordinate radius of an outer bubble, the more powers of p its number is divisible by*.

The scheme then reproduces itself: one de Sitter time later, the outermost bubbles that were present at the earlier time all have almost the same coordinate radii as before, and for each of them, approximately p – 1 new outermost bubbles have appeared between them, all with approximately the same coordinate radius, 1/e times smaller than the previous minimum coordinate radius. Multiply the numbers assigned to the outermost bubbles present at the earlier time by p; these are then those outermost bubbles' new numbers. And label each new outermost bubble by the new number of its nearest neighbour old bubble, plus a unique number from 1 to p – 1.

So I think the HSSS scheme certainly helps with clarification and visualization. I am not able to judge, from a quick look through the article, whether the use of p-adic numbers gives you anything beyond what you would get by working with the real numbers obtained by reflecting the p-adic numbers in the decimal point.

11. **plm**
October 5, 2011

"the Haar measure you get from thinking of them as an additive group can be normalized to get a finite result"

First, I do not see the importance of having a (multi)universe with finite measure. Or at least I do not really understand you mean.

Second, the integers are amenable, they have a finitely additive translation-invariant probability measure (though not countably additive, and not the Haar measure).

Third, Haar measure is unique up to multiplication by a nonzero real number. We "get a finite result" from all Haar measures on p-adic integers. Also, if you had an nonfinite Haar measure on your space (say Z) normalization would not help.

12. **Peter Woit**
October 5, 2011

plm,

The basic problem is that the multiverse people want to make statistical predictions, but have no real theory of what the space of universes should be or how they should be statistically weighted. So, they decide to weight everything equally. Even for a toy model where the integers parametrize your universes, this doesn't work (since you can't normalize such a weighting). If you take as your toy model the p-adic integers, then you can make this work. Again, I haven't carefully looked at the paper, but I'm guessing this is what is going on.

13. **plm**
October 6, 2011

Thanks, I looked at the paper.

Actually the p-adic integers do not seem to play any particular role. The p-adic numbers (rationals) are used in places in the paper, and infinite branches of the p-adic tree, i.e. p-adic integers which are not standard integers, are not really necessary for much of the discussion.

What matters seems to be the p-adic distance itself, between vertices of the causal tree/multiverse cells/universes in the multiverse, arising from the assumption that all edges of the causal tree with p branches at each vertex have the same length (p^{-u}/2 at time u),.

So compactness of the p-adic integers does not seem to be basic, neither translation invariance, rather the "homogeneous distance"/ultrametric assumption (which can be restricted to Z).

In any case thanks for the blog post and replies to my comments. It was all interesting to think about.

14. **plm**
October 6, 2011

Correction to my comment: the assumption is that all vertices causally separated at time u are at distance p^{-u}.

15. **Shantanu**
October 15, 2011

Peter,
how was the colloquium and did you get a chance to interact?

16. **Peter Woit**
October 16, 2011

Shantanu,

The talk was at Stanford on the West Coast, I'm in New York….

17. **Shantanu**
October 16, 2011

oops, I thought its at columbia

18. **Jonathan Langdale**
October 17, 2011

Was this recorded and put up online?

What's That at the Top of This Page?

October 5, 2011
Categories: Uncategorized

The graphic chosen years ago as the header for this blog is an event display from the UA1 detector in 1982, of historical importance since it was the first event found with a W candidate. To be honest, the reason it's there is that I was looking for something quick to use at the interactions.org Imagebank, figuring these were graphics I could steal without getting sued. What I ended up with is a cropped, lower-resolution version of the much better image available here.

Even stripped of identifying info, UA1 experimentalist Jim Rohlf of course recognized it, and recently wrote me to tell me some more about it. He also tells me that he will soon be blogging at Quantum Diaries, and I look forward to seeing that. So, here's the story behind that image:

> The collision is Run 2958, Event 1279 and was the very first W candidate that was found. It was recorded in the Fall of 1982 with UA1. As a newly minted junior faculty member and CERN scientific associate, I was resident at CERN and the first round of the W event selection and analysis was completed during the CERN holiday shutdown. On 23 January 1983 we submitted the W discovery paper for publication (Phys. Lett. 122 B, 103 (1983)). The details of the events were given in this paper. Within a few days, I got a letter from Lev Okun who had become a good friend of mine due to his frequent visits to CERN and his great interest in the working details of our experiment. In this letter which was several pages long he referred to this event as a "monster" because it decayed in the "wrong direction" and asked if we could have made a measurement error. Then the obvious hit me instantly- nobody had thought of this before- we don't measure the longitudinal momentum of the neutrino due to the singularity in the direction of beam pipe but we can solve it to a quadratic ambiguity knowing the W mass. Furthermore, I saw that the kinematics of a 80 GeV object being produced with a relatively low cm energy of 540 GeV gave a remarkable result: often one of the 2 solutions was kinematically forbidden and when it wasn't, the two solutions were often close together. Therefore, we could solve for the longitudinal momentum of the neutrino and be able to transform to the rest frame of the W. Since the W was polarized because it was produced in proton-antiproton collisions, we could measure the angle of the decay wrt the spin direction. Very simple idea, but be the first to do it and it becomes interesting and fun. I immediately wrote this up as a UA1 internal note in which I acknowledged the contribution of Okun. This technique subsequently became a standard at the Tevatron and now at the LHC.
> In the following months, we collected more data and the next international conference to come along was at Fermilab and I was told by Rubbia to give the talk which was published (J. Rohlf, "Physics at the Proton-Antiproton Collider," Proceedings of the 12th International Conference on High Energy Accelerators, Fermilab, 619 (1983). I reported the first measurement direct observation of parity violation in (real) W decay and measurement of the spin. I attach a slide from a talk I gave at Fermilab 20 years later in 2003 where I pulled up some of my 1983 slides. (Notice I fit the W mass to 2% and got the right answer.) You can see the "monster" event in the bin at cos(theta) =-1. This W decayed in the wrong direction. We went on to collect about 300 W events in UA1. We never saw another one go in the wrong direction. We also could not find anything wrong with that original event 1279. So you see the event was "not even wrong".

Update: A copy of the talk slide that Jim Rohlf refers to is here.

Update: Jim Rohlf's blog at Quantum Diaries is now up here. His first blog entry is great, it's about, independent of the Higgs issue, the fundamental problem the LHC hopes to investigate: what is causing electroweak symmetry breaking? He emphasizes that one way to study this is to try and see the self-interactions of Ws and Zs, which become strong at the TeV scale.

Comments

1. **Chris Austin**
   October 5, 2011

   With reference to solving for the longitudinal momentum of the neutrino up to a quadratic ambiguity, I raised this recently here after Tommaso said that we only know the transverse component of the neutrino momentum. But from Jim Rohlf's message, apparently the longitudinal momentum of the neutrino *is* sometimes measured this way. Is it possible to clarify when this can be done?

2. **B Experiment At Small Theta**
   October 5, 2011

   Very interesting! I recognized the image as one of the UA1 W events too, although I did so from having previously dug around on CDS for images from the infamous megatek
   (I wasn't born when that event was read out!). You can see the beautifully rendered event displays these things produced here: http://cdsweb.cern.ch/record/1049887

   would it be possible for you to host the slide Jim sent you (pending his permission?) I'd love to see more.

3. **Peter Woit**
   October 5, 2011

   B experiment,

   He didn't send me the image, it's always been hosted at Imagebank, and you can access it there. There may be some other source of UA1 event display images, but I don't know anything about this.

4. **Jim Rohlf**
   October 5, 2011

   Chris:
   Directly we measure only the transverse component of missing energy because hundreds of GeV can escape down the uninstrumented beam pipe. However, for the case of W –> e nu, mass of the parent particle is known and we have m**2 = E**2 – p**2 (where E is electron plus neutrino energy and p is electron plus neutrino vector momentum). This is a quadratic equation for p_z , the unknown longitudinal momentum of the neutrino which has TWO solutions. Now visualize where the neutrino can go to see the two solutions conceptually. If the electron has energy nearly half the W mass and is produced nearly at right angles to the colliding beams, then the neutrino must recoil opposite the electron and have very little p_z. If on the other hand the electron is going forward (like the monster event), then to make the W mass the neutrino could be also going forward and have large energy so that p_z plus the electron p_z exceed the proton energy (kinematically forbidden) or it can go at large angles and have smaller energy.

   Peter:
   I think B Experiment was asking about the W angular distribution I sent you which you may post if you want.

   B Experiment:
   I have tons of events displays because I never throw anything out. I am at Fermilab now but when I return to Boston I may find some time to dig up some more pictures of

this famous event.

5. **Chris Austin**
October 6, 2011

Jim, thanks very much for the explanations. To summarize when the two solutions coincide, (I think you described a special case for simplicity), if M is the W mass, p and q are the neutrino and electron transverse momenta, and c is the cosine of the angle between the neutrino and electron transverse momentum 2-vectors, the solutions coincide when M^2 = 2 p q (1 – c). I guess with enough statistics, "lucky" events, where this is approximately satisfied, could be selected to improve the mass resolution of some of the Higgs searches.

6. **Peter Orland**
October 6, 2011

Hi Peter,

You should substitute "what's" for "what".

Regards,
Peter

7. **Peter Woit**
October 6, 2011

Thanks Peter! Fixed.

8. **Bernhard**
October 7, 2011

This story is really interesting, is nice to know more about how it happened and the REAL persons involved with it. I had enormous trouble reading "The God particle" from Leo Lederman, because he gives the impossible impression he almost did everything by himself, had all the ideas, and therefore got the Nobel. I immediately thought "but who was the postdoc (or "newly minted junior faculty member ") who really did the work anyway?". Same thing with the W discovery. Only now, I know who did it. Not to say John did it alone too, but reminds us how giving Nobel prizes to individuals for experimental collaboration work is totatally bogus.

9. **Bernhard**
October 7, 2011

Sorry, I meant Jim, not John.

10. **SpearMarktheSecond**
October 7, 2011

Great to hear from Jim Rohlf, he is a gem.

`Nobel Dreams' by Taubes describes the subsequent discovery of Supersymmetry by UA1 and UA2.

http://www.nytimes.com/1987/01/29/books/books-of-the-times-556387.html

11. **Jim Rohlf**
October 7, 2011

Chris:
What Higgs? Only joking (well, sort of). At the LHC, Higgs searches in fact do select "lucky" decays by asking for the charged lepton + neutrino transverse mass to be an appreciable fraction of the W mass. These lucky decays BTW are not so improbable because a 2-body decay makes the so-called Jacobian peak which is just a solid-angle effect. This is the reason the W mass can be determined so well from only measuring the charged lepton. I was truly thrilled when I first understood this!

12. **Jim Rohlf**
October 7, 2011

Spear:
The problem with the Taubes book is he arrived way too late to document the W and Z… at least I got to be the Prince. For the record, UA2 was not part of the SUSY false alarm which was largely propagated by theorists. I stand behind the two major talks I gave on the subject at ICHEP Leipzig 1984 and APS Eugene 1985.

13. **Anon**
October 11, 2011

The header image contains a good unintentional example of the parallel lines illusion http://www.moillusions.com/2006/04/parallel-lines-illusion.html

News From Europe

October 7, 2011
Categories: Experimental HEP News, Uncategorized

A few items with a European flavor:

- The news from Dublin is that Witten will be in town soon to give the Hamilton Lecture, with the Irish Times reporting that

> Witten's Hamilton Lecture will abandon string theory, however, in favour of knots, with a talk entitled: The Quantum Theory of Knots.

He may be there the previous day, when they hold the annual Hamilton Walk, commemorating Hamilton's discovery of quaternions and inscription of the quaternion relations on a bridge.

In other mathematical physics coverage by the Irish Times, one of their columnists describes the interaction of the Irish revolutionary leader de Valera with Schrodinger and Dirac, speculating (humorously) that the three of them might have come up with an Irish "unified field theory."

- From a meeting today in Madrid, here's an overview of theoretical particle physics in Spain. There's the same pattern reported as has been going on in the US for a while: "moving from more formal and mathematical developments to phenomenology and also astroparticle/cosmology", as well as trying to get theorists more involved in LHC physics. Another similar pattern to the US, the threat of "decreasing funding support for basic science in difficult economic times."
- The question of what future facilities for particle physics should be is not just a European one, but I fear that in practice a higher energy machine is not likely to be built anywhere except at CERN. This week at CERN there was an ICFA Seminar on Future Perspectives in HEP, which gives a good overview of the state of the field and prospects for the future. The question of what to build next to get information at the energy frontier is very unsettled, pretty much completely up in the air waiting to find out if there's a Higgs particle or not. The SUSY and extra dimensional models used as partial selling points for the LHC are dying and won't be convincing arguments for what the next generation should do.

Comments

1. **Paul Collins**
   October 7, 2011

   For a fictional treatment of Schrödinger's time in Ireland, see Neil Belton's "A Game With Sharpened Knives".

2. **martibal**
   October 8, 2011

   A bit off topic, but still it is news from Europe. For those who understand italian, you should have a look at:
   http://www.youtube.com/watch?v=FcUDjxXu0pc&sns=fb

   For those who don't here is the context: a few hours after the announcement of the FTL neutrino, the ministry of italian research (Mrs Gelmini) has published an official comment, where she explained this was an historical discovery, to which Italy had greatly contributed by spending, at least, 40 something millions in the construction of a tunnel between Italy and CERN ! She has not dismissed, but her spokesman yes.
   And in this very funny audio-piece, some people (I do not know who, but there are very talented), report an accident that happened in the Gelmini-tunnel, between Gran Sasso and CERN, when a neutrino tried to overcome a photon. The neutrino warn the photon with "light-signal" (sorry, I do not know the exact english term), but since he was going faster, of course, he bumped into the photon before its signal, and this made a lot of mess in the tunnel, with protons abandoned on the side of the road etc etc

3. **Bernhard**
   October 8, 2011

   martibal,

   great story :-).

   Peter,

   I wonder if the reason for the choice of Witten´s talk has more to do with the occasion or if it could be that not even Witten is willing to talk directly about string theory these days.

4. **Peter Woit**
   October 8, 2011

   Bernhard,

   During the past year or so Witten's papers have mostly been about his research on QFT and knot theory (especially Khovanov homology), so it's a natural topic for him to talk about. I don't think he has given up on string theory as a unified theory, but I suspect that even he would acknowledge that there has been little progress in recent years, making it a less viable topic for the talk.

   Witten's recent research extensively uses various dualities among QFTs. He might point out that even though these can be understood as QFT phenomena, string theory research has been responsible for insight into these in the past, so in that sense his current research still has a connection to string theory.

5. **DB**
   October 8, 2011

   If you are ever in Dublin, you can visit Kilmainham Jail, where DeValera – who founded the Dublin Institute of Advanced Studies – was imprisoned after the 1916 Easter Rising, narrowly escaping execution, and where his graffiti of Hamilton's quaterion equation can still be seen on the wall of the cell he occupied. A video of his visit to his old cell is available here.
   http://www.youtube.com/watch?v=-I-SSTN5Nko

   As to what accelerator should be built next, if there is a Higgs in the energy range not yet excluded it is unlikely to be CLIC but more probably a muon collider, once the technical problems are resolved. This we should know within months.
   If no Higgs, it will depend on what clues the LHC turns up regarding dynamical symmetry breaking among other things. It might motivate CLIC or something else

entirely. But nothing is likely to happen until the LHC is much further down the road and discovers something really important, because in the absence of Higgs and SUSY, funding politicians may feel they were sold a pig in a poke. Once bitten…

6. **Sheldon Pherris**
October 8, 2011

Does this mean that Peter will soon start a new blog titled: Knot Even Wrong?

7. **cormac**
October 9, 2011

It's a good Irish Times column, but it doesn't explain *why* Dirac was so surprised to see de Valera at the conference. At a lecture in Dublin a few years ago, Professor Goddard of IAS Princeton explained that Dirac said he ' was amazed that the Irish Prime Minister could take a few days out to attend a mathematics conference!'

8. **DB**
October 9, 2011

Unless the story is apocryphal, such a lengthy response from Dirac to a question must surely be something of a record.

9. **Chris Oakley**
October 9, 2011

DB,

No necessarily. It could have gone something like this:

Reporter: Are you amazed that the Irish Prime Minister can take a few days out to attend a mathematics conference?

Dirac: Yes.

De Valera's passions, BTW, were (i) hatred of the British and (ii) mathematics, in that order. The odd combination of schools (i.e. Celtic studies and theoretical physics) at the Dublin IAS, which he founded, more or less reflect this.

10. **Stephen**
October 9, 2011

Peter I just read the USA Today article concerning the OPERA claims. Glad a non-stringy expert is called upon when expert advise is needed. Your comments were great.

11. **Peter Woit**
October 10, 2011

Stephen is referring to this:

http://content.usatoday.com/communities/sciencefair/post/2011/10/pundits-puzzled-by-physics/1

12. **SpearMarktheSecond**
October 10, 2011

If no Higgs in the remaining allowed interval (115-145 GeV or so), I think we'll know the SSC was indeed the correct machine and not the LHC. The Republicans supported the SSC (aka, the Ronald Reagan Lab); it was the Clinton administration and democratic congress that killed it.

Newt Gingrich was a big SSC supporter.

All uncomfortable truths, those. But maybe a light Higgs will emerge and they won't be truths at all.

13. **RRL**
October 10, 2011

The SSC was born as a panic-stricken reaction to the failure of ISABELLE at BNL (=USA) and the discovery of the W (and Z) by the SppS at CERN (=Europe, aka "not USA"). An important part of the SSC mandate was to rub it into people's faces that "America has the biggest accelerator in the world". Yes indeed the SSC was supported (enthusiastically, I might add) by Ronald Reagan. The (papa) Bush administration continued the SSC. Yes indeed the SSC was cancelled by the Clinton administration. But it is also true that DOE micromanaged the SSC from the start, and it is also true, in these modern times, that every new administration cancels the projects of its predecessor (if from the opposing party). The SSC was part of US-HEP, so it gets mentioned on a blog like this. But other non-HEP special interest groups will point to their own pet programs, which got cut by some incoming administration, Democrat or Republican.

14. **Eric**
October 10, 2011

Actually, it is not true that the Clinton administration canceled the SSC. The Clinton administration actually supported the SSC, though not as enthusiastically as Bush/Reagan. It was really opposition in Congress that killed it, due largely over political jealousy from other states over Texas having two big ticket items, the SSC and the space station. If the SSC had been designated for some other state, say Nevada or Illinois, then it would have been built. Also, there was a lot of opposition from physicists in other research areas who were upset that high-energy physics was receiving so much funding.

15. **ssc**
October 10, 2011

Most of that is true. But if the SSC had been designated for some other state, such as Nevada or Illinois, the SSC proposal might not have been approved at all. There was a lot of jealousy from non-HEP physicists. They thought that if the SSC was cancelled, the funding would go into their pockets, e.g. solid-state. This proved to be both greedy and wrong. The cancellation of the SSC led to a general reduction in physics funding overall. However it is also true that the SSC was mismanaged. The estimated cost kept going up and up, and it was clear that the proponents were just telling lies, and it eventually became too much for Congress.

16. **RayGun**
October 10, 2011

As I recall, Reagan was also enthusiastic about the atomic-bomb driven space-based X-ray laser as part of his Star Wars anti-missile fantasies; I wonder if he somehow saw the SSC as linked to this?

17. **Ilya**
October 10, 2011

"Comments that just add noise and/or hostility are not." Does the post by RayGun above belong to the aforementioned category?

18. **Peter Woit**
October 10, 2011

Ilya,

I'm not sure that Raygun's contribution was any more objectionable than others but I'll agree that the "who to blame for the SSC clusterf–k" discussion is off-topic anyway, and political arguments about it generally contain more noise than signal. There was plenty of blame to go around on that one. Enough about it here.

19. **cormac**
October 10, 2011

DB and Chris:
Dirac's comment on Dev was not verbal, but contained in a letter (to his wife, I think) describing the DIAS conference and Dublin in general

20. **piscator**
October 10, 2011

In regards to the DIAS and the presence of both Celtic studies and theoretical physics, I think it is appropriate to repeat the waggish Irish Catholic comment that the purpose of the Institute was to prove that there was no God and two Saint Patricks.

21. **D R Lunsford**
October 11, 2011

When I was visiting Dublin in the mid-80s, of course one of my prime destinations was the Broom Bridge. I was surprised to find in surrounded by a typical US-style suburb. The Royal Canal had some sad-looking water in it, not at all reminiscent of a pastoral scene involving W.R. and Lady Hamilton. The bridge itself is very narrow and small cars were zipping by as I examined the stone railing. "This is dangerous!" I thought. Just about then a guy slowed down as he passed me and stuck his head and shoulders out the window…

He smiled and shouted at me – "Ya wooon't find it! Ya wooon't!!"

I gave up 🙂

-drl

22. **Artie**
October 11, 2011

DR Lunsford: "typical US-style suburb"? My wife, who moved from the US to Cabra (the suburb in question) about ten years ago, would be amused by that one…

I agree, though, that the surroundings are less than idyllic. As you mention, the bridge doesn't even have a footpath! I've risked being run over a couple of times to bring visitors there, only to see their faces fall at the sad, worn-down little plaque under the bridge. (Not to mention the shopping trolleys and traffic cones floating down the canal.)

Weinberg on Symmetry

October 10, 2011
Categories: Uncategorized

The latest New York Review of Books has an article by Steven Weinberg entitled [Symmetry: A 'Key to Nature's Secrets'](). It's a bit unusual for the NYRB, since it is both scientifically more technical than usual for them (coming from a write-up of Weinberg's talk at [this conference]()), and doesn't review any books. The printed version tells readers to go to the web version for footnotes, but some of these just note that things are being over-simplified. One of the footnotes is worse than useless: the editors have replaced x^3=x as an example of an equation with solutions that break a symmetry (x goes to -x) by "x 3 equals x", an equation with the same symmetry but only a symmetric solution (x=0). The idea seems to have been to remove or replace any symbols in the equation that might upset people.

Weinberg tells the conventional story of how the Standard Model emerged during the 60s and early 70s out of the realization that non-abelian gauge symmetries were important and an understanding of what happens when symmetries are spontaneously broken. He tries to do some much more ambitious things, explaining the idea of "accidental symmetries" that are due to the limited number of possible renormalizable terms you can build out of a specified list of fields, but I'm not sure the typical reader of the NYRB is going to get much out of this.

The question of how to explain the notion of "symmetry" is an interesting one, and I thought a lot about it when writing *Not Even Wrong*, the book. To my mind, most such explanations mix up two conceptually distinct things: the group of symmetries (a group), and the action of the group on some other mathematical object (the representation: mathematically a homomorphism from the group to the group of automorphisms of something). It's both the group and the representation that are important in the use of symmetries in physics, although often what is important is the trivial representation. From a mathematician's point of view, the simplest representations to look at are unitary representations on a complex vector space, so the mathematical structure of quantum mechanics is very natural. To each symmetry generator you get a conserved quantity, and it appears in quantum mechanics as the thing you exponentiate (a self-adjoint operator) to get a unitary representation. In Weinberg's piece, which aims at sophisticated issues in particle theory, the question of the basic relation of symmetries and conservation laws is relegated to a footnote which says only "For reasons that are difficult to explain without mathematics…".

Weinberg ends with a landscape sort of picture, involving symmetries emerging only when a specific ground state emerges out of an initial chaotic inflation state. Philosophically this is a popular view of the future of the subject these days, but one that has so far led nowhere, and one that I think even in principle can never lead anywhere. Much more interesting would be to try and draw lessons from what has worked well in the past: exactly the gauge symmetries and spontaneous symmetry breaking phenomena that led to the standard model. We may very well soon find out there is no Higgs particle, turning this whole subject into a wide-open one. Future progress may come from exactly the same place as in the past: new ideas about how to exploit the mathematical structures inherent in quantum mechanical symmetries.

Update: The missing exponentiation in the on-line footnote has been fixed.

Comments

1. **peterg**
   October 10, 2011

   "To my mind, most such explanations mix up two conceptually distinct things: the group of symmetries (a group), and the action of the group on some other mathematical object (…)"

   Very true, but it's incredibly difficult to explain the difference to a layman.

2. **Peter Woit**
   October 10, 2011

   peterg,

   I agree. I'll also admit that I was confused about this myself for an embarrassingly long time as a student, studying the topic from a physicist's view-point. Part of the problem is that if you think of groups as groups of matrices, your very definition of what a group is comes packaged with a representation.

3. **neo**
   October 10, 2011

   It is not at all unusual that this NYRB article does not review books. Many of their articles do not review books. Kind of strange for a book review periodical.

4. **udi**
   October 11, 2011

   Can someone explain why Weinberg makes a statement, which seems to me to be wrong:

   "these symmetries, known as lepton and baryon conservation, would dictate that neutrinos (particles that feel only the weak and gravitational forces) have no mass"

   Neutrinos can have a Dirac mass term, which is renormalizable and invariant under all local symmetries.

5. **Emanuel Derman**
   October 11, 2011

   Not being in physics anymore, I find it a little depressing to see this evolutionary psychology approach to physics being espoused by Weinberg himself. What fraction of physicists ascribe to this?

6. **Peter Woit**
   October 11, 2011

   Emanuel,

   I think Weinberg has more sympathy towards this kind of thing than you might expect, since he's credited with the earliest argument that the anthropic "explanation" for the size of the cosmological constant gives a bound at least the right order of magnitude to correspond to the observed value.

   I'm curious to hear what other's impressions are of how this kind of empty pre-big bang scenario is thought of among physicists in general. From what I've seen, I'd describe physicists who are not theorists as mostly quite hostile to the idea as not being science. Within the theory community there's a vocal group actively pushing it, but perhaps a silent majority that sees it as kind of an embarassment, best ignored, hoping it will go away.

7. **Giotis**
   October 11, 2011

   Strictly speaking Weinberg, for the prediction of the (bound of the) value of the CC, would deserve to share this year's Nobel prize.

   Why not? He is a physicist and he made a prediction based on physics.

8. **Jon**
   October 11, 2011

   For Higgs stuff, it looks like the last 2011 update will happen in Paris on wedenesday november 16 http://hcp2011.lpnhe.in2p3.fr/hcp-2011_program.pdf (at least I couldn't find a december conference).

   What puzzles me is that the sections on the Higgs that day consist only on reports on experimental results, except the last one (on "combinations and beyond") which is planed to end with a talk entitled "Electroweak Symmetry Breaking without SM (or SUSY) Higgs" by C.Grojean: might it be a hint that still nothing has been found? Is that what you are alluding to, Peter?

9. **Peter Woit**
   October 11, 2011

   Giotis,

   If the Nobel Prize committee had done anything that idiotic, I suspect that Weinberg himself would have turned down the prize, and a bunch of other physics medalists would have mailed theirs back…

   Jon,

   As far as I know, the Paris conference will be the next time CMS and ATLAS announce new results about the Higgs (and a combined CMS/ATLAS analysis using this summer's results also appears). I don't now how much data they will have analyzed by then, at the moment they've collected something like 3 times the amount used in the summer results. This should be very interesting…

   The fact that a theory talk about models without a Higgs is scheduled isn't surprising, and doesn't reflect any inside knowledge about the new data. There's no point to a theory talk about the Higgs, everyone has known for decades exactly what the theory says there. The failure to find the Higgs so far is leading to more and more interest in Higgsless scenarios.

10. **Eric**
    October 11, 2011

    Lubos Motl is hinting on his blog that he has inside information that a Higgs signal has emerged and he knows its mass….

11. **Chris Austin**
    October 11, 2011

    An observational test of Weinberg's anthropic explanation of the small value of the cosmological constant has been proposed by [Abraham Loeb](). The idea is to search for exo-planets in nearby dwarf galaxies that formed at redshifts up to 10. Discovery of such planets would provide evidence that planets could form even if the cosmological constant was larger than the observed value by a factor of up to 1000. Loeb said that the search could be carried out with 2006 technology, but no results appear to have been reported so far.

12. **Peter Woit**
    October 11, 2011

    Eric,

    One would expect some news to start leaking out of CMS/ATLAS soon, on the other hand, the idea that Lubos would keep something he knows quiet isn't all that plausible. Anyway, we'll see soon….

13. **mo**
    October 12, 2011

    > udi says:
    > October 11, 2011 at 12:19 am
    > Can someone explain why Weinberg makes a statement, which seems to me to be wrong:
    >
    > "these symmetries, known as lepton and baryon conservation, would dictate that neutrinos (particles that feel only the weak and gravitational forces) have no mass"

    Weinberg explains it himself in footnote 9.

14. **DB**
    October 12, 2011

    @Peter "we'll see soon…"

    In the valuable link you provided to the ICFA seminar in CERN, Terry Wyatt from Manchester appeals for a 5-sigma standard for *exclusion* on the Higgs (slide 30), effectively excluding the Tevatron from the exclusion process. He states "The gold standard for SM Higgs discovery or exclusion is the same 5σ" That's a first for me and was understandably challenged by a questioner after the presentation. He doesn't indicate how he proposes to rejig the LEP2 results which only reach the 95% confidence level, especially given that LEP2 originally eliminated most of the range originally predicted by precision electroweak data.
    He continues: "Excluding the SM should be regarded (*and presented to the outside world* – my emphasis) as a discovery of equal or greater significance."
    Yeah right….

15. **x3**

October 13, 2011

There may be more typos. Cna't b too quick to judge.

16. **ZZZ**
October 13, 2011

Re: old planets in dwarf galaxies. There was some report last year regarding finding some very old planet around a star that most likely came from a dwarf galactic neighbor. Another point is that many of them would have been swallowed by their stars by now, so there is a built-in selection bias for lower mass long burning stars.

17. **S. Molnar**
October 13, 2011

I'm a bit late on this, but the missing exponentiation in the footnote was clearly an HTML-type error and not an intentional change to the text. I'm unhappy with the continuing decline of editorial rigor in the New York Review of Books (an article recently referred to a film actor who played Dracula as "Bella Lugosi", apparently confused by the fact that Béla Lugosi appeared in "Glen or Glenda"), but let's apply Hanlon's razor when appropriate.

18. **Chris W.**
October 16, 2011

Footnote 3 of Weinberg's NYRB piece might catch the eye of a few people. Consider how it would be phrased if one replaces Galilean invariance by Lorentz invariance, and adopts the perspective of general relativity; the Earth's orbit is locally inertial. There is no need to talk about gravity balancing the effects of the centrifugal force caused by the earth's curved motion. 🙂

19. **anonymous**
October 17, 2011

Eric,

some atlas and cms people seem to believe that there is a higgs signal at 120GeV. others don't. this is the reason why there is no new update for so long. let's be patient a bit longer and let the experimenters do their job, then we'll know.

20. **Lau**
October 21, 2011

I've recently come across this discussion between Weinberg and Dawkins and thought of sharing it, in case anyone would like to watch. It is 3 years old, so you may have seen it before.

Starting approximately at the end of part 2, they discuss the multiverse.

http://richarddawkins.net/rdf_productions/steven_weinberg

21. **Georges MELKI**
October 27, 2011

I don't really get the difference between x^3=x and x3 equals x, and how the latter upsets people less than the former! Usually, when the number is at the right of the letter, it's supposed to be an exponent, not a coefficient…unless the reader is completely illiterate, and he/she wouldn't be reading an article by S.Weinberg!

Welcome to the Multiverse

October 19, 2011
Categories: Multiverse Mania

The October issue of Discover magazine has a new feature, a column by Sean Carroll, whose inaugural effort is now on-line as Welcome to the Multiverse. Sean makes the argument that opposition to multiverse mania is due to people having too naive an idea about what science is. They don't realize that testing those parts of a theory you can directly observe allows you to draw conclusions about those parts you can't directly observe:

> A lot of people, both inside and outside the scientific community, are viscerally opposed to the idea of other universes, for the simple reason that we can't observe them—at least as far as we know. It's possible that another universe bumped into ours early on and left a detectable signature in the cosmic background radiation; cosmologists are actively looking. But the multiverse might be impossible to test directly. Even if such a theory were true, the worry goes, how would we ever know? Is it scientific to even talk about it?
>
> These concerns stem from an overly simple demarcation between science and nonscience. Science depends on being able to observe something, but not necessarily everything, predicted by a theory. It's a mistake to think of the multiverse as a theory, invented by desperate physicists at the end of their imaginative ropes. The multiverse is a prediction of certain theories—most notably, of inflation plus string theory. The question is not whether we will ever be able to see other universes; it's whether we will ever be able to test the theories that predict they exist.

Sean makes quite clear that multiverse mania is driven by string theory. Inflation is part of the story, but it's not a fundamental theory by itself. All it can tell you is that your fundamental theory should have an inflaton field of some kind, with a potential satisfying certain properties. The big idea that justifies the multiverse is that:

> In short, string theory predicts that the laws of physics can take on an enormous variety of forms, and inflation can create an infinite number of pocket universes. So the different laws of physics predicted by string theory might not be just hypothetical. They might really be out there somewhere among the countless parts of the multiverse. This is not a situation that cosmologists dreamed up in a flight of fancy; it is something we were led to by trying to solve problems right here in the universe we observe.

The problem that Sean doesn't mention is one of circularity. Since you can't observe anything about it directly, the multiverse must be justified in terms of another theory that can be tested and this is string theory. But if you talk to string theorists these days about how they're going to test the unified theory that string theory is supposed to provide, their answer is that, alas, there is no way to do this, because of the multiverse. You see, the multiverse implies that all the things you would think that string theory might be able to predict turn out to be unpredictable local environmental accidents.

So, the multiverse can't be tested, but we should believe in it since it's an implication of string theory, but string theory can't be tested because of the multiverse.

Until recently, string theorists would sometimes hold out hope that the LHC would see low-energy strings, extra dimensions, or supersymmetry, and that these discoveries would somehow pick out a predictive version of string theory from the landscape of the multiverse. This year's data from the LHC has pretty much destroyed such hopes.

Sean ends with the inspirational admonition:

> The proper scientific approach is to take every reasonable possibility seriously, no matter how heretical it may seem, and to work as hard as we can to match our theoretical speculations to the cold data of our experiments.

What's going on in this story though is not a concerted effort to match theoretical speculation to experimental data, but something very different, a concerted effort to build a theoretical framework perfectly insulated from testability, and sell it to the rest of the physics community and the public, hoping no one notices the circularity.

Update: Besides the usual spam, this topic seems to attract mostly empty comments supposedly agreeing with me, and comment moderation is unusually annoying. I think I'll turn off comments on this posting, and encourage people who want to discuss the topic to do so over at Cosmic Variance, where Sean has a posting devoted to this.

Comments

1. **Roberto**
   October 19, 2011

   agree with you: what Sean C. does with the multiverse happens, somehow interestingly, elsewhere: e.g.; evolutionary biology, where some unverifiable ideas (and not good either) are deemed dogma, for example: the "selfish gene', the "meme' and so forth.

String Theory Finds a Bench Mate

October 19, 2011
Categories: Uncategorized

There's a nice article this week in Nature about AdS/CMT, entitled [String Theory Finds a Bench Mate](). According to the article, the whole thing is (partly) my fault:

> But in 2006, string theory took a public battering in two popular books: *Not Even Wrong* by Peter Woit, a mathematician at Columbia, and *The Trouble With Physics* by Lee Smolin, a physicist at the Perimeter Institute for Theoretical Physics in Waterloo, Canada. Both books excoriated the theory's isolation from experiment.
>
> "It's hard to say whether the interest in condensed-matter applications is a direct response to those books because that's really a psychological question," says Joseph Polchinski, a string theorist at the Kavli Institute for Theoretical Physics in Santa Barbara. "But certainly string theorists started to long for some connection to reality."

The main point of the story is to tell about what is probably the hottest topic in hep-th these days, attempts to use AdS/CFT to say something about some models in condensed matter physics. For some idea of what this is all about, see the review article What can gauge-gravity duality teach us about condensed matter physics? by Subir Sachdev, and take a look at the online talks from the KITP workshop Holographic Duality and Condensed Matter Physics.

The article does go into the history of this in some detail, including its roots in efforts to use AdS/CFT to say something about heavy-ion physics phenomena observed at RHIC (for the string theory promotional campaign surrounding this, see e.g. here). I had expected to see a lot about this topic when higher energy results from heavy-ion collisions at the LHC were released earlier this year, but it seems to have gone quiet, perhaps because of the kind of comparison of data with AdS/CFT predictions that Sabine Hossenfelder points out here:

> As the saying goes, a picture speaks a thousand words, but since links and image sources have a tendency to deteriorate over time, let me spell it out for you: The AdS/CFT scaling does not agree with the data at all.

My knowledge of condensed matter theory is minimal, and the hype level surrounding string theory makes it hard to know whether to take at face value many of the claims being made. On general principles, this looks a bit more promising than the heavy-ion case, since there are many different kinds of systems one might look at, and the connections are more to QFT than to string theory. Experts quoted in the Nature article give opinions ranging from:

> Polchinski admits that the condensed-matter sceptics have a point. "I don't think that string theorists have yet come up with anything that condensed-matter theorists don't already know," he says. The quantitative results tend to be re-derivations of answers that condensed-matter theorists had already calculated using more mundane methods.

to condensed matter theorist Andrew Green's:

> "Maybe string theory is not a unique theory of reality, but something deeper — a set of mathematical principles that can be used to relate all physical theories," says Green. "Maybe string theory is the new calculus."

Time will tell whether this suffers the same fate as in the case of heavy ions.

Comments

1. **Nige Cook**
   October 19, 2011

   Here in England, tonight BBC2 TV just screened a "Faster than Light" program with Michael Duff giving some string theory hype to explain the alleged 60 ns "faster than light" neutrinos. Duff stated that the results could be explained by neutrinos leaving our 4-d brane, taking a super-fast short-cut through the 11-d bulk, and then appearing again on the 4-d brane nearer the detector. Eventually, after claims about the experiment being "proof" of string, they showed Duff making a more sensible comment that he *wasn't* actually hyping the experiment as proof of superstring or even confirmation of a falsifiable prediction from string theory. But it left the viewer in no doubt that superstring offers the only "real" good explanation. Duff illustrated the 4-d brane with a slice of bread, using a loaf for the 11-d superstring bulk.

2. **SteveB**
   October 19, 2011

   Gerard 't Hooft has updated his web site. Similar to your quote from Andrew Green, in the quantum gravity section 't Hooft now says:

   > …many of my colleagues are convinced that "string theory solves the problem". But why does this happen? How does string theory resolve the paradox? Curiously, string theorists themselves do not quite understand this. But I think I might understand this now. String theory is just an instrument to do calculations in regions of a theory that are otherwise inaccessible.

3. **Peter Woit**
   October 19, 2011

   Nige,
   Duff can really be relied upon when string theory hype is needed. There's a page about this program here:

   http://www.bbc.co.uk/programmes/b016bys2

   It seems to have been produced in record time.

   SteveB,

   't Hooft is referring to AdS/CFT's supposed role in resolving the information paradox about black holes, which is a somewhat different topic. Whether it does this is independent of whether it provides a useful approximation method for certain condensed matter systems.

4. **Spinons**
   October 20, 2011

   As a condensed-matter theorist, I don't really appreciate the AdS/CFT papers. Although they might be useful in the vague sense of being a phenomenological model, they don't really solve anything fundamental. For example, this Gauge/Gravity paper on Science: Faulkner et al., 329 (5995): 1043-1047 talks about their findings of a class of non-Fermi liquids through the AdS/CFT correspondence, with a tantalizing application for the normal state of high-Tc superconductors. However, such approach couldn't give any clue at all about the mechanism of the high-Tc superconductor, or even what makes the electrons non-Fermi liquid. It is beyond their capability.

With the arXiv flooded with this kind of take-the-easy-route papers, I was wondering whether this correspondence between AdS and CFT is really well established as rigorous mathematical theorems ? Probably not…

5. **lun**
October 20, 2011

Spinons, while I am skeptical of AdS/phenomenology in general, I do not think your argument works: With the same reasoning, you would have also had to ditch the Ginzburg-Landau theory of superconductivity, since it also offered no clue as to the origin of the scalar field responsible for spontaneusly breaking U(1).
Your namesake origin, spin-charge separation became famous well-before any microscopic explanation was even proposed, if I remember correctly.

The 10^n dollar question is what are the necessary requirements for a physical system to be described by a holographic theory. How do we know if a generic field theory is dual to SOME string theory?
For QCD, the "obvious" answer is that classical gravity is only good for SU(N), and hence one either needs to quantize gravity or concentrate on things that dont change between N=3 and N=infinity. I would be interested to know if something similar exists in condensed matter.

6. **Spinons**
October 20, 2011

lun, but Ginzburg-Landau theory is indeed a phenomenological theory, and is indeed very useful. So as I said, the AdS/CFT might be useful in this sense if this gravity/gauge equivalence is really well established.

The GL theory finds even more applications than superconductivity. But as far as the (old) superconductivity problem is concerned, only BCS theory provides the true explanation and advances our understanding of superconductivity (electron-phonon coupling, cooper instability, etc).

As you must have known, the GL theory can be derived from BCS theory. Without this connection, I believe the GL approach is not much different from what engineers are doing.

I guess what GL approach also symbolizes is the "universality" aspect of physical systems. Different microscopic models share the same field theory at the critical point. The AdS/CFT is also similar in this spirit. But again, it makes more sense to talk about universality when you have a bunch of explicit model systems which show similar behavior.

I'm not so sure about what you said "spin-charge separation became famous well-before …" But in 1D this is well known theoretically with explicit lattice models long ago. Although there are a lot of talks about fractionalized excitations in high dimensions, but without at least some lattice models or (better) experiments, it is indeed not much different from stringy speculations in my opinion (fortunately, we have several such models already)

7. **lun**
October 20, 2011

Agree with spinon. Having said all that, for Peter, see my comment
[at your link]()

While I am convinced QCD is fundamentally different from any theory with a classical gravity dual, I think ruling out AdS/CFT as phenomenology for heavy ions is premature,

8. **Derek Teaney**
October 22, 2011

Dear Peter,

I posted this remark at Bee's web site.

I think this post of Bee is off the mark, when it comes to
heavy ion collisions.
In particular the remarkable aspect of the
new data is the success of hydrodynamics
in describing the new data on the higher
harmonics flow when the shear
viscosity is of order 1-3/4\pi . This was the truly
new (and remarkable) aspect of the new data from
the LHC. It is certainly very difficult
to reconcile this with weak coupling . I spoke
about the higher harmonic flow at the
AGS Users meeting:

http://www.bnl.gov/aum/content/plenary/ps1.asp

(see Current Theory Perspective, D. Teaney)

Certainly when the momentum becomes
large p_T >> Lambda QCD \sim T it
becomes increasingly dubious to use strong
coupling methods. So, although there
are many reasons to be skeptical of the
AdS/CFT this example (which Bee used) does not change
my opinion one way or the other.

9. **David Brown**
October 23, 2011

My translation of "Confutatis maledictis, flammis acribus, voca me cum benedictus" is "When the cursed sinners are condemned to the hot flames, may the Lord call me among the blessed."

This Week’s Hype

October 19, 2011
Categories: This Week's Hype

A couple people have written to tell me about the new BBC Faster Than the Speed of Light? documentary on superluminal neutrinos which evidently featured trademark hype from string theorist Mike Duff about how string theory could explain this. For better or worse, I don’t think I have access to the show from the US, although I’m sure that sooner or later it will arrive on our shores.

Update: Philip Gibbs has more here.

Update: The BBC program is now on Youtube, see here.

Comments

1. **Matt Leifer**
   October 19, 2011

   I watched this tonight and can confirm that it did include stringy hype. The entire last segment was given over to a speculation that neutrinos might travel in the bulk in a braneworld scenario and thus appear to be travelling faster than light to those of us who are stuck on a brane. I’ve never heard anyone propose that anything other than gravity might operate in the bulk, so this seems like an extreme example of opportunism to me. Of course, I am not an expert on braneworld scenarios, so I could be wrong about this. Nevertheless, to claim that the OPERA result might be evidence for string theory seems quite a stretch to me.

   To be fair, the documentary did include a reasonable amount of discussion of the possible errors in analysis of the OPERA result, but, apart from some vague talk about tachyons, this stringy explanation was the only serious alternative mentioned. There was not even any discussion of the possibility that Lorentz invariance might be violated.

2. **may c j**
   October 19, 2011

   The presenter claims that the mass changes if the speed of the body is increased – moreover he claims that this follows from Special Relativity. He is a mathematician so perhaps this is why he does not realise how false that statement is. A mass is a Lorentz invariant – it cannot be changed by Lorentz transformations (like a boost) – what changes is the 4-momentum.

3. **Roger**
   October 20, 2011

   Not everyone equates “mass” with “rest mass”. It is okay to say that speed changes mass.

4. **Philip Gibbs**
   October 20, 2011

   Don’t criticize this program if you have not seen it. It gives a very balanced view of the theory and experiment. Mike Duff explains in hypothetical terms the idea of how extra dimensions could explain the effect and then makes a very clear statement that he does not believe that this is the right explanation. There is no hype.

5. **may c j**
   October 20, 2011

   Roger:

   no, it is not ok to say “the speed changes mass”. The numerical equivalence between the gravitational mass and the inertial mass is the reason why “M times \gamma” has no physical meaning i.e. cannot be called a mass. You will not increase the gravitational field of a body by increasing its velocity.

6. **Roger**
   October 20, 2011

   Do you similarly object to people saying that speed changes length, or speed changes time?

   I do not wish to argue it here, except to point out that both views are common and tenable, as explained by John Baez and Wikipedia. It is silly to complain about the BBC, when many physicists and textbooks say the same thing.

7. **Eric**
   October 20, 2011

   Back in the day, people did tend to refer to the “relativistic mass” rather than the relativistic momentum. This is because the momentum for a particle with non-zero mass is given by

   p = gamma*m_0*v

   so sometimes people would consider the combination gamma*m_0 to be the relativistic mass as opposed to the invariant mass m_0. From a practical point of view, it’s just semantics. From the theoretical point of view, it is more technically correct to speak of the relativistic momentum rather than the relativistic mass.

8. **may c j**
   October 21, 2011

   No Roger, I am not objecting to length contraction and time delays.

   Gravitational mass m_g is equal to m_0(rest mass, inertial) and not to m_0*gamma therefore the statement “the speed changes mass” is not true (as it would imply that m_g is speed dependent which is clearly not the case). Note that the equality m_g=m_0 is empirical therefore the statement “the speed changes mass” would contradict the (Eötvös) experiment.

9. **Roger**
   October 21, 2011

   may c j, what you have is an argument for a definition that some textbooks use, and some don't. The concept of relativistic mass is just as legitimate as the FitzGerald contraction. You could likewise argue that the lengths do not really contract, but it appears that way when measured in another frame. There are some advantages to saying that the mass changes, but you ignore those.

10. **Henry Bolden**
    October 21, 2011

    Better hype: Hints of supersymmetry seen at the LHC, according to Matt Strassler:

    http://profmattstrassler.com/2011/10/19/something-curious-at-the-large-hadron-collider/

11. **Peter Woit**
    October 21, 2011

    Please, enough about relativistic mass.

12. **Bob Levine**
    October 22, 2011

    @ Henry Bolden

    What Strassler actually *says* is,

    "There's absolutely no evidence at this point that this has anything to do with supersymmetry. ZERO. So keep that in mind."

    So it seems a little unfair to attribute SUSY hype to him, on this score at least.

13. **Celestial Toymaker**
    October 22, 2011

    In fact, Mike Duff said that, having worked on the concept of extra Dimensions for 30 years, he'd be very pleased if the Opera result provided experimental confirmation for them.
    But he also said that he didn't think this was the case.

    So there wasn't really any hype involved; no more so than any other theoretical conjectures that would arise were it confirmed that neutrinos were indeed superluminal particles. Which would be pretty paradigm shifting, if true.

    Marcus du Sautoy pointed out that none of the attempts to find measurement errors in the results had produced a quantitiative explanation for the excess speed calculated. But I think it was made before Ronald van Elburg's paper on the subject, which does seem to provide such an explanation – ironically one that confirms Einstein's theory rather than refuting it.
    http://www.technologyreview.com/blog/arxiv/27260/?ref=rss

Solvay Centenary

October 19, 2011
Categories: Uncategorized

The first Solvay conference was in 1911 (at the Hotel Metropole in Brussels, where I stayed one night of my recent trip to Belgium, without knowing the history), attended by the great men of the early days of quantum theory, and one woman (Marie Curie). For more about the 1911 conference, see this [recent paper by Norbert Straumann](). Today the [25th Solvay conference]() got underway in Brussels City Hall, celebrating the 100th anniversary of the first conference.

Like the first one, this conference is by invitation only, and it upholds a policy of confidentiality that goes back to 1911, with not even a schedule or list of attendees for the scientific session publicly available that I can see (just the statement that they are "most of the prominent physicists working on the subject). We do however have Lisa Randall reporting on [Twitter]() about the proceedings. Evidently she's the only woman there :

> Seems ratio of x to y chromosomes hasn't changed in 100 years since first Solvay conference in 1911…

The only other source of info on the internet seems to be [this Cal Tech news item](), which lists the Solvay chair as David Gross and rapporteurs as:
John Preskill (Quantum Computation)
Anthony Leggett (Quantum Foundations)
Ignacio Cirac and Steven Girvin (Control of Quantum Systems)
Frank Wilczek (Particles and Fields)
Edward Witten (String Theory)
Alan Guth (Cosmology)

The gender distribution may have stayed the same, but it looks like the age distribution is somehat different. Today the average age of the chair and Rapporteurs is about 61, back then it was about 46.

Comments

1. **Kea**
   October 19, 2011

   How embarrassing. But then, they aren't embarrassed, are they.

2. **Peter Morgan**
   October 19, 2011

   Even in that social class, people died younger a century ago.

3. **M**
   October 20, 2011

   100 years ago Einstein and Curie did not discuss about feminism and string theory.

4. **Johntheman**
   October 20, 2011

   I found this concluding passage in the Strauumann article interesting:

   "I conclude with personal remarks, that look totally disconnected with what was said in this historical account, and may just reflect my advanced age. One often hears that the present day situation in fundamental physics (string theory, loop gravity) has some similarity with the early years of quantum theory, before the great breakthrough – mostly by a young generation – in 1925-26. I find this analogy totally wrong. Without the precision experiments by the Berlin group (Kurlbaum, Rubens, etc.) and the difficult measurements of the specific heat of molecular hydrogen and other diatomic gases at low temperatures, that demonstrated the freezing out of the rotational degrees of freedom, as well as the low temperature measurements of the specific heat of solids by Nernst, Lindemann and others, it is hard to imagine that quantum theory could have been developed. This is, of course, not new, but it may not be inappropriate to be recalled in an article for this journal."

5. **anonymous**
   October 20, 2011

   any conference on the foundations of physics that does not invite Weinberg can't be taken seriously.

6. **Bernhard**
   October 20, 2011

   The gender distribution of this conference roughly reflects the actual gender distribution in physics, as far as I know. Then why is the distribution like this is not an easy question but I would not say that is for the lack of initiatives. At least in Sweden they are everywhere, to the blowing point of being unfair with men. Besides Randall which other woman comes to mind that is in the same caliber as Witten? Right now I can' t think of any. Not to say that would not be nice to have more women in the field, would be very nice and healthy, but the situation as it is, seems to a result of a mixture of things. The most decisive point is a lack of interest of women to go to graduate school, even tough the rate women/men seems to raise drastically from undergratuate to graduate school (because of the many initiatives that are taken, this is a key point in all European Union applications, for example). The rate is still not in a satisfying proportion. It' s beyond me tough what else could be done to change this situation.

7. **Bernhard**
   October 20, 2011

One action that could be taken of course is to try to get more women in the filed at undergraduate level not leave the problem to graduate school, because then one already has too few choices. But this is a complicated task. How to force someone to choose to do physics?

8. **justanotheranon**
October 20, 2011

I agree with the anonymus above- how could they not invite Steven Weinberg (not to say Wilczek is undeserving- but still Weinberg is a living legend- the greatest physicist alive and probably in history – how could you not invite him to such a prestigious conference (well maybe they did but he declined?)
Any news/dates(!) on his "Lectures on Quantum Mechanics"- really looking forward to this one…

9. **Lisa Randall**
October 20, 2011

I'm not the only woman here. Eva is participating too. But there are more than twice as many participants which is why I mentioned the ratio.

10. **Peter Woit**
October 20, 2011

Thanks Lisa, correction added.

11. **Łukasz Grabowski**
October 20, 2011

nitpick: Marie Skłodowska-Curie.

12. **Bernhard**
October 20, 2011

Łukasz,

Living people, please.

13. **fem**
October 20, 2011

Weinberg might have been invited and could not attend. In later years, Einstein was invited but was not always able to attend the Solvay conferences.

"100 years ago Einstein and Curie did not discuss about feminism and string theory."

By 1911 Einstein was married to his first wife and embarking (or would soon embark) on an affair with his (eventual) second wife, while Marie Curie was a widow (Pierre died in 1906 in a street accident in Paris) and embarking on an affair with Paul Langevin. Feminism, anyone?

14. **Charles**
October 20, 2011

Two things are likely given the focus and sponsors…the conference invited Weinberg…and they are paying many tributes to Brout (who unfortunately passed early May) and Englert (in attendance) as the primary folks behind the Higgs Field and Boson (excluding PH and GHK).

15. **Peter Woit**
October 20, 2011

Bernhard,
I suspect that Lukasz was nitpicking about my not giving Marie Curie's full name, not your comments about the small number of female physicists. The question of why there are so few women physicists at the top ranks of the profession is a complicated one, and discussion of it a whole other topic.

In this case, the standard shouldn't be Witten though, since in many ways there aren't even any other male theorists in his league.

16. **Bernhard**
October 20, 2011

Hi Peter,

I agree, and sorry if I went a bit off-topic, surely I know you don' t want a general debate about women in physics. But going back to the Solvay conference I agree Witten is putting the bar too high, but a fair bar is to ask for a woman in the same level of all the other guests, including Lisa Randall. Perhaps people can can up with one other name, I could not come with one tough.

17. **Paulus**
October 20, 2011

Eva who?

18. **Rapunzel**
October 20, 2011

Kea, why should "they" be embarrassed?

19. **Kea**
October 20, 2011

Rapunzel, unlike the dudes here, who feel entitled to wax lyrical about women in physics, I actually know about the research that proves without any doubt whatsoever that the reason for few women is Discrimination Discrimination Discrimination. Bernhard's inability to read even one paragraph of this research is a good demonstration of his misogyny. Fact: women leave physics AFTER getting PhDs, and sometimes very good ones, for a number of reasons, but mostly because of Discrimination Discrimination Discrimination.

20. **Peter Woit**
October 20, 2011

Paulus,

Eva Silverstein

21. **anon.**
October 20, 2011

Weinberg's absence could just be due to limited funds — he never goes anywhere unless whoever invites him pays for a first-class airline ticket. (I can't say I blame him, as I think he's earned that sort of thing; but it is very expensive.)

22. **SpearMarktheSecond**
October 20, 2011

I hope they all have a nice time but the chances are slim that anything innovative will result. Laying the groundwork, both theoretical and experimental, for a post-no-Higgs situation might be nice. But hasn't the trend, like graphene and quasicrystals, been away from the mainstream? Even Dark Energy, although the greats leapt in front of that parade and hypnotized us into neglecting that they totally missed predicting Dark Energy.

23. **Jeff**
October 20, 2011

@spear – hey, didn't Einstein get "dark enegy?" Of course, he didn't come up with a clever name for it 🙂 Still remember working out the field equations for GR as an undergrad at Hampshire and getting into the CC discussion with Herb Bernstein.

24. **Edgar Loesel**
October 21, 2011

There are no female physicists giving public lectures or participating on the panel discussion no woman:

25. **Bernhard**
October 21, 2011

Kea,

Nothing I said justifies what you are implying. I asked a simple question, and perhaps there is an answer to it, but so far nobody, including you, actually gave me good examples of women in the level of Randall to attend this meeting. I suppose there could be such persons, I am just not aware of them. If you however are, please share your knowledge. I am with my mind open and I am glad to recognize I was wrong if you or anybody proves me wrong. But please, do not simply rotulate me as misogyny without actually answering the question seriously. And in any case even if the problem were that I am not aware of them it is not because I have something against them. There could be a not famous other Lisa Randall, but if there is, then is not my fault not knowing her, since the hypothesis is that she is not famous enough for me to know her in the first place.

As for why women leave physics and when I don ´t think you are right, but in any case this is not the place to discuss it. I am however open to new information and if there is something ("the research") I missed please send me a link.

This is my last reply to this, otherwise this will become an annoying debate for Peter to moderate.

26. **Peter Woit**
October 21, 2011

Bernhard,

Eva Silverstein immediately came to mind when I started to think of prominent women theorists that could have been invited to Solvay besides Lisa Randall. Turns out she is there though, so not an example of someone passed over or ignored.

What Kea addresses is kind of a different, not why Solvay is passing over prominent women, but why women don't rise to prominence. Again, that issue is a complicated one that is kind of off-topic, and I'm not up to moderating a discussion of it here and now.

27. **Bernhard**
October 21, 2011

Thanks Peter, agreed.

28. **lun**
October 21, 2011

I hope Peter allows me to link this talk from his collegue, as it answers comprehensively why the X chromosome ratio moved so little.

29. **jg**
October 21, 2011

The most famous Solvay Conference of all (1927) is analysed in detail in the book

'Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference'
by Guido Bacciagaluppi, Antony Valentini

which is available in draft version on arxiv (all 553 pages)

http://arxiv.org/abs/quant-ph/0609184

30. **cormac**
October 21, 2011

Looking forward to curling up on the couch and reading that Straumann paper tonight, he's an excellent historian of physics….wish more physicists would do this. Many thanks for the link Peter

31. **Łukasz Grabowski**
October 22, 2011

"I suspect that Lukasz was nitpicking about my not giving Marie Curie's full name"

Yes, or rather as I'd put it, your not giving her correct name 🙂 (my understanding is that if you stick with what she called herself you should use Marie Skłodowska-Curie, if you prefer the legal state of matters after her marriage with Pierre Curie, it's probably Maria (not Marie) Curie, unless she changed her name. But this seems to me very unreasonable, see for example Michel Thomas on wikipedia – no one called him anything other than that, presumably because that's the way he wanted it to be, so why not granting Marie S-C the same luxury?)

32. **Giotis**
October 22, 2011

If Silvertsein was there then why not Kallosh or Becker&Becker for example? If they wanted more females participants they could easily invited them and others.

But I guess who you know makes a huge difference. If you know Witten I think you have better chances.

33. **Giotis**
October 22, 2011

If you know Witten and Gross even better

34. **anonymous**
October 22, 2011

Most people in high energy physics know Witten and Gross, and most people
go to plenty of conferences. No doubt there are many men and women who could have been invited, as with any conference.

35. **Kent Traverson**
October 25, 2011

If Madam Curie's husband was deceased, how could it be said that she was having an affair with anyone?

36. **Eric**
October 25, 2011

Kent,

The man she was involved with, Paul Langevin, was married at the time.

37. **Umesh**
October 31, 2011

Am sorry, but I have to point out that nobody compares to Edward Witten, male (or) female. Besides, he's 'The Martian'!! On a more serious note, it's not for nothing that he's said to posses superhuman intelligence, which puts him beyond anyone currently involved in the field. So please stop throwing around comparisons with this great man so freely.

The Status of SUSY

October 22, 2011
Categories: Experimental HEP News, This Week's Hype

You may have seen by now claims from various sources about evidence for SUSY coming from CMS, for instance Hints of New Physics Crop Up at LHC, A Lifeline for Supersymmetry?, and CMS sees SUSY-like trilepton excesses. This nonsense is all due to Matt Strassler, who for some reason thought it was a good idea to post a blog entry Something Curious at the Large Hadron Collider that starts off:

> Finally, something at the Large Hadron Collider (LHC) that does not seem to agree that well with the predictions of the equations of the Standard Model of particle physics.

followed by various caveats, which include though the advice:

> But this is clearly something to watch closely over the coming months.

As one could easily have predicted, this got picked up by the media and various blogs, mostly dropping the caveats. In a later more detailed posting, Matt carefully three times in italicized red explains that "The excess will probably disappear". He does continue to claim that "particle physicists are paying close attention" to this statistically insignificant discrepancy between data and theory, something I suspect was true before his blog posting for an equally statistically insignificant number of particle physicists.

During this past week or so, there has been a lot of various news about SUSY at the LHC, all of it bad. For some background, one should look at Mike Peskin's write-up of his summary talk at LP2011, which he posted last week to the arXiv. See pages 37-41 for his discussion of the state of SUSY. He explains why one would expect that all SUSY mass terms are of the order of a few hundred GeV, with the Tevatron bounds on gluino and squark masses (around 300 GeV) already making one suspicious. Similar LHC bounds are already around 1000 GeV, getting close to the limit (around 1200 GeV) of what can be produced at current beam energy. When the LHC comes back on-line with higher beam energy in 2014, these bounds should then go up to 2000 GeV or more. Much has been and is being made of the fact that one can find SUSY models that evade these bounds, with LHC results then giving lower limits in the range 500 GeV and above.

Peskin writes:

> As the LHC experiments become sensitive to hypothetical new particles with TeV masses, we are reminded of the phrase from the Latin Requiem Mass:
>
> Confutatis maledictis, flammis acribus addictis, voca me cum benedictus.
>
> A loose translation is: Thousands of theory papers are being tossed into the furnace. Please, Lord, not mine!
>
> …
>
> Before the startup of the LHC, I expected early discovery of events with the jets + missing transverse energy siignature of supersymetry. It did not happen. A particularly striking comparison is shown in Fig. 33. On the left I show the expectation given in 2008 by De Roeck, Ellis, and their collaborators for the preferred region of the parameter space of the constrained Minimal Supersymmetric Standard Model (the cMSSM, also know as MSUGRA). The red region is the 95% confidence expectation. On the right, I show the 95% confidence *excluded region* from one of the many supersymmetry search analyses presented by CMS at LP11. No reasonable person could view these figures together without concluding that we need to change our perspective.

Peskin goes on to argue though that the thing to do is not to abandon SUSY since it hasn't shown up where it was supposed to, but to "acknowledge that, to test SUSY, we must search over the full parameter space of the the model". The obvious problem with this is that the "full parameter space of the model" is huge, containing all sorts of corners that will never be accessible to the LHC, or that can be made arbitrarily difficult to rule out, requiring intensive effort from LHC experimenters for decades to come.

For details on what has been going on, various recent sources to consult include Anyes Taffard's FNAL talk on ATLAS SUSY searches ("SUSY was NOT 'just around the corner' … must be hiding well … Or may be … need to go back to the drawing board") and the many talks at the Berkeley Workshop on Searches for Supersymmetry at the LHC which included a huge array of negative SUSY results, including the one that for some reason got Matt so excited. Besides the kinds of models that Peskin expected to see at the LHC, lots of other more obscure ones are being ruled out by new LHC analyses. These include some that had gotten a lot of popular attention, such as split supersymmetry and F-theory models. These predicted things like long-lived gluinos or staus, which have now been searched for and ruled out in regions where they were supposed to show up. For example see here for more about F-theory and the stable staus, which CMS now says are not there where they were supposed to be (below 300 GeV).

For some other recent news, see the talks at the BNL conference running the past couple days, A First Glimpse of the Tera Scale.

Finally, for the best in recent HEP news, see this from Warren Siegel.

Comments

1. **Phil**
   October 22, 2011

   Dear Peter,

   I would very much appreciate your thoughts in response to my comment (#60) over at Sean's post. My comment was in response to your response (#59) to an earlier comment I had made (#19). Here is the link to Sean's post:
   http://blogs.discovermagazine.com/cosmicvariance/2011/10/18/column-welcome-to-the-multiverse/#comments

   You'll also find a comment (#62) from "somebody" (not me, I assure you) and I am also very interested in your response to him/her. Thank you!

2. **Bernhard**
   October 22, 2011

   I don´t think it is absurd to ask SUSY to be tested over a larger parameter space, since who knows, it could be there and Nature not necessarily care about our experimental difficulties. What I would however like to see is how likely some regions of the SUSY parameter space is with respect to its motivations, most notably the "solution" of the hierarchy problem. Some interesting plots would be to show different regions of the SUSY space with a calculation of the percentage of extra fine tuning one needs to "solve" the hierarchy problem. I guess one could go on searching for SUSY forever but the community would slowly start to understand it is probably not there if the remaining parameter space to be searched for were a theoretically already disfavored one.

3. **Giotis**
October 22, 2011

Phil, of course Peter knows better but I can tell you my opinion about your comment i.e. how QFT is different from String theory in that respect.

QFT is not a theory of everything and thus it doesn't have to explain everything e.g. the values of the various constants it contains. It waits for a more fundamental theory to explain these things i.e. to explain QFT, GR and the Standard model itself.

ST on the other hand claims that it is a TOE and consequently must explain everything. Specifically it must certainly explain why we live in this particular low energy effective world with this particle/force content and with these constants.

Now if the String vacua picture is true then ST obviously can't do that. In this picture there is no reason whatsoever why from these enormous number vacua only our vacuum, with these particular properties, is realized. So far nothing indicates that there are probability distributions highly peaked over our kind of vacuum.

Faced with this problem ST theory must admit her failure or else claim that indeed all these vacua can be realized using eternal inflation as the mechanism to populate them. This is how the multiverse picture emerges.

So Peter objection (as I understand it at least) is that ST instead of admitting its inability to predict anything regarding our world (and thus to admit her failure as a TOE) it justifies this inability by adapting the multiverse paradigm. According to this perception this situation resemebles a drowning man clutching at a straw.

4. **Joel Rice**
October 22, 2011

I had not seen Warren's parodies – still rolling around on the floor trying to catch my breath – he should team up with Weird Al Yankovic and make some videos.

5. **Eric**
October 22, 2011

Dear Giotis,

Your comments in regards to string theory basically reflect the exact same attitude that many physicists had towards QFT in the 1950's and 60's. It is a myopic way of looking at the real situation as it is today. Just because our current understanding of string theory is limited doesn't mean that the theory itself is limited.

Bernhard: Any supersupersymmetry parameter space with superpartners less than around 2 TeV can naturally solve the hierarchy problem. The present experimental limits are not even close to this.

6. **Bernhard**
October 22, 2011

Eric,

I would like to see this for the future. At 14 TeV we will eventually get there and at some point the amount of fine tuning will start to get troubling (do you agree?). I´m not sure when this will start to happen to you, my understanding of this is that what come out is already worrying. So in any case, I would like to see a more detailed prediction on this, for when we get there I know exactly where we are standing in terms of the likelihood of SUSY being correct.

7. **Phil**
October 22, 2011

Giotis,

Isn't it true that theorists haven't been able to use QCD to calculate the proton's mass? From my understanding, this is due to the failure of perturbative QCD and only non-perturbative QCD (which we don't know very well) can allow us to calculate the proton's mass. Isn't this similar to our lack of knowledge of string theory non-perturbatively?

Maybe if we understand string theory non-perturbatively, we will be able to find our low energy universe in the theory and rule out the rest of the landscape? But we don't know if such a thing is possible, hence the continuing research in string theory.

Also, one can argue that string theory is NOT a TOE because it is only a perturbative theory of strings that postulates what the degrees of freedom are beyond the standard model and towards the Planck scale. Perhaps M-theory, or whatever the nonperturbative formulation of string theory is, is the real TOE.

If QCD is the theory of the strong interactions, why haven't we been able to use the theory to calculate the proton mass? Because we don't understand the theory very well non-perturbatively. Perturbative QCD, like perturbative string theory, cannot explain everything (i.e the proton mass), but a more fundamental formulation (i.e. non-perturbative QCD) can. The same may be true with string theory.

8. **Roger**
October 22, 2011

Phil, I thought that lattice QCD could calculate the proton mass from the masses of the up and down quarks, and they have values for those masses.

9. **Eric**
October 22, 2011

Bernhard,

If superpartners are not observed below ~ 2 TeV, then supersymmetry cannot solve the hierarchy problem. Thus, a major motivation for the expectation of observing supersymmetry at the LHC would go away. At the moment, we are not there. It will be a few years into the second LHC run before this scenario might be realized. However, I think it is probable that signals of the superpartners will show up by the end of next year, if they haven't already i.e. the observed trilepton excesses. Also, I think it is likely that in the next few months, the first evidence for the Higgs will be anounced, and it will in fact be in the region favored by the MSSM.

10. **Peter Woit**
October 22, 2011

Phil,

QCD does have a precise non-perturbative definition, there is a lot you can say based on it, and this all agrees precisely with experiment.

Trying to say that the QFT framework which is the most predictive and successful theoretical framework ever developed by human beings is the same as the completely unpredictive string theory framework is really absurd. What you're doing is saying that black is just like white since they're both shades of gray.

11. **Peter Woit**
October 22, 2011

Eric,

If you look at the Peskin paper I linked to, you'll see that his argument is that for SUSY to solve the hierarchy problem it should have superpartners at the mass scale of 100s of GeV, not 2 TeV.

12. **anon**
October 22, 2011

Siegel's frog parody is funny, but it's impossible because the total energy of the protons that ever collided in the tevatron is far less than the rest mass energy of a frog.

13. **Eric**
October 22, 2011

Peter,

If you read Peskin's paper carefully, you will see that what he actually says is that the simplest possibility is for the superpartner masses to be in the few hundred GeV range, not that this is where they have to be in order to solve the hierarchy problem. The superpartners can be heavier than this, and masses from ~100 GeV to 2 TeV can do this.

14. **Phil**
October 23, 2011

Peter,

There are many quantum field theories possible, corresponding to the many possible choices one can make for the underlying gauge groups. Similarly, there are many possible perturbative string theories, corresponding to the many possible ways of compactifying C-Y manifolds, etc. So what is the difference between these two?

The only difference I see is that we are able to perform experiments that show us the way towards the gauge groups of the standard model, but we do not yet have the technology to conduct experiments that show us what physics looks like far beyond the standard model or near the Planck scale. When string theorists get better at building stringy models that match the standard model they will have many models that not only contain the standard model, but also have different possibilities for what lies beyond the standard model and near the Planck scale. When or if our technology improves to the point which allows us to conduct much higher energy experiments, nature will guide us to the correct model, just like how nature guided us to the correct SU(3)XSU(2)XU(1) quantum field theory after we performed lots of particle physics experiments.

Do you agree with the above?

If it is shown that no stringy model contains the standard model (which hasn't been done yet) or the results of beyond standard model experiment, then string theory will be shown to be wrong. If such a thing is not shown, then string theorists will keep on looking.

15. **Bernhard**
October 23, 2011

Eric,

OK then, bets are on :-).

As for the Higgs, I think we would already be hearing rumors by now if it were true and things are really quiet, but who knows…

16. **Martin**
October 23, 2011

I was so fascinated by your latin quote that I used Google's translation tool.

> Confutatis maledictis, flammis acribus addictis, voca me cum benedictus.

translated to

> Confuted curses, devoted to acrid flames, call me with the blessed"

I think you did better. But then I assume Latin is merely like another Italian dialect to you.

Shame about SUSY. I have always been a great fan.

Cheers,
Martin
PS I did find this official translation, although I still prefer yours

> When the accused are confounded,
> and doomed to flames of woe,
> call me among the blessed.

17. **JE**
October 23, 2011

Bernard said, "As for the Higgs, I think we would already be hearing rumors by now if it were true and things are really quiet, but who knows…"

Referring to the Higgs, Tommasso commented on his blog a couple of days ago that "… CMS and ATLAS experiments are now seeing an excess exactly of the right size at the most probable mass".

So let's wait and see if this turns out to be a hint of a true signal…

18. **Bernhard**
October 23, 2011

JE,

Really?? OK, I missed that. But as you said, let´s wait and see.

19. **Peter Woit**
October 23, 2011

Phil,

You're trying to argue that black is just like white by starting with "first assume that black somehow turns into light grey..". This is a waste of time.

Eric,

Why 2 TeV? Why not 4, 10, 100?

Martin,

The translation is not mine, but Peskin's.

20. **Eric**
October 23, 2011

Peter,

You can do find this calculation in any QFT book which discusses supersymmetry. It is a simple calculation of the loop corrections to the Higgs mass. For exact supersymmetry, the correction is zero. As the mass difference between the SM particles and their superpartners gets bigger, then the corrections to the Higgs mass get bigger. At some point (~1-2 TeV), the Higgs mass becomes unstable. I would suggest actually performing the calculation yourself so that actually know what you are talking about for once.

21. **Artyom**
October 23, 2011

Dear Peter

I recently started reading your blog, thanks, it`s very interesting.

What do you think is it possible that theorists do not recognize the failure of SUSY and simply close their eyes on LHC results?

False theories have taken place before but in SUSY too much invested… Academic degrees, thousands of papers, grants etc. Do scientific community able to recognize the crash of SUSY, strings and other "great theories"?

Sorry for my English, I am from Russia.

22. **Phil**
October 23, 2011

"You?re trying to argue that black is just like white by starting with ?first assume that black somehow turns into light grey..?. This is a waste of time."

Peter, I really don't understand your point here. Can you please clarify?

You say QFT is predictive. Does QFT predict the SU(3) X SU(2) X U(1) group structure as the one that describes our world? No. Experiments were needed to elucidate this fact about our low energy world. Likewise, experiments are needed to elucidate the correct compactification for the extra dimensions, assuming extra dimensions and string theory describe our world. Again, experiments are needed to determine whether or not string theory and compactified extra dimensions describe our world, just as experiments were necessary to show us that group theory and the formalism of quantum field theory describe our world.

So, in that sense, QFT is just as unpredictive as perturbative string theory.

Do you agree with this? Why or why not, and please, for the purposes of enlightening and persuading me to adopt your point of view, refrain from using the "black", "white", and "grey" "argument", since I have absolutely no idea what any of that means. Thanks!!

23. **ha!**
October 23, 2011

Phil,

the point is that simple groups like SU(3), SU(2) and U(1) are the very first thing one would think of. They are quite the simplest thing to try. Calabi-Yau compactifications, however, are mind-numbingly complicated, and there appear to be so many of them that if ever one of them is found that reduces to the standard model in the low engery limit, it would hard to claim that that is a "prediction" of string theory. This is why string theorists are reduced to nonsense like the landscape, and spurious statistical arguments based on it.

24. **Bernhard**
October 23, 2011

Phil,

QFT as an effective theory cannot predict a bunch of things, like masses. But once you used it there is a clear predictive power to it, once you use e.g. the rules for renormalizable gauge theories you are very be able to compare the model you built with experiment. The models based on QFT´s are very predictive each one separately, reason way we can start discussing which models will be excluded at the LHC. That is in this sense a big difference between SUSY and string theory (ST), even if SUSY was born inspired by ST we can really discover or rule out SUSY. That´s very exciting and interesting. ST comparatively have nothing even remotely close to that. There are is no way you could use ST to make different models as predictive as QFT and there has been no advances on how to solve this in the last years. Even if you had a gigantic accelerator there would be no way you could say "right, that´s the correct ST model", because there are no precise quantitative predictions to compare to. QFT is a powerful framework, ST on the other hand is… well I´m not sure what it is.

25. **Phil**
October 23, 2011

ha! and Bernard,

Thank you very much for your helpful responses! I'm not a string theory expert by any means (my field is in a different area of physics), and I'm pretty neutral about all this, but I just want the straight dope about all this. Here are my responses.

ha!, True the simplest groups turned out to be the right ones, but isn't it true that Nature doesn't care what is simple or not? Her laws are what they are. If Nature cared about simplicity, then why don't we simply live in a "billiard ball" universe controlled only by Newton's laws of motion?

Bernard,

So are you saying that, as of now, if a string theorist produced a particular compactified calabi-yau manifold, it wouldn't lead to definite predictions for what our low energy universe should look like according to the model (and, therefore, to see if it matches the standard model), or what physics beyond our low energy universe should look like? How come? I always assumed that once you choose the parameters for the calabi-yau, you will get a definite theory of particle interactions by using the rules of string interactions. Are there no definite rules for string interactions? I always thought strings join together and break apart and that there were clear expressions for amplitudes for these processes. Why is building a string theory model, based on a particular calabi-yau manifold, capable of telling us what the particle interactions at different scales should look like, such a hard problem?

26. **Peter Woit**
October 23, 2011

Eric,

In my advanced old age, I have trouble performing loop calculations in the MSSM, especially keeping track of the extra 105 undetermined parameters. And then, I never know whether I should really be using the NMSSM or some other extension of the MSSM, and sometimes I'm not sure if I need to go to higher loops or not. So, maybe you can help me out by pointing to where someone has done this calculation. And, by the way, is it 1 TeV or 2 TeV? If it's 1, the subject is just about done.

27. **Peter Woit**
October 23, 2011

Artyom,

Theorists aren't closing their eyes to LHC results, they're paying close attention. Right now, the question is, what do you do if these results rule out the generic picture of how SUSY appears that you had been advertising for a long time as the one to be expected? One attitude (mine) would be that there is already a long list of reasons not to expect SUSY, so this should finish the subject off as a popular one to work on. Lacking a better idea, experimentalists might want to keep doing searches for SUSY variants, but should be under no illusion that they're likely to see anything.

If you have been spending the last 20-30 years of your professional career investing your time in developing expertise in the ins and outs of the intricate details of SUSY models, or you have loudly advertised SUSY as an implication of another subject you are deeply invested in (e.g. string theory), you might not be willing to give up so easily. In this case you would start going on about all the special cases of SUSY you could think of that might be such as to evade the current LHC limits. There may be some limit though to how long you can keep doing this, and still have people pay any attention to you.

28. **may c j**
October 23, 2011

I am always surprised when people say that Supersymmetry was born inspired by String Theory (as Bernhard says). Miyazawa, in papers from 1966 and 1968, introduced Supersymmetry a few years before the superstring hype. Of course no strings are mentioned in Miyazawa papers. The superstring was introduced in 1971.

29. **Peter Woit**
October 23, 2011

Phil,

I see that part of the problem here is that you don't actually understand what is involved in constructing a realistic string theory model. To start with: you can't just "pick a Calabi-Yau and calculate". Calabi-Yau's come in a very large number (possibly infinite) of families, each family locally parametrized by a space of high dimension. If you pick a point in such a space, that does pick out a Calabi-Yau, but you basically know almost nothing about its metric, and that's the first thing you would need to start doing calculations. Oh, and this actually is only approximately true: you really don't want Ricci-flatness, but a more complicated condition with higher order terms.

Let's say you actually could do this. Then you'd have a theory that was clearly wrong, it would have the "moduli" problem. The parameters defining your Calabi-Yau would behave like massless fields, giving you lots of long-range forces of gravitational strength, violating the equivalence principle. So, you need to add a bunch of non-perturbative structures in by hand to "stabilize moduli". Take a look at the KKLT construction which is supposed to do this, which even its advocates refer to as a "Rube Goldberg construction".

Let's say you've done all this in some specific case. Then you have the problem that vacuum energy is completely wrong. Etc, etc, etc. What's going on here is nothing at all like QFT, where you can just sit down and calculate. Here anything you can calculate comes out wrong, so you are forced into more and more complicated constructions, just to avoid being ruled out by experiment. This is black where doing the calculation and getting an answer that agrees with the real world to 10 decimals is white. String theorists who go on about how they really can calculate things in principle, there are just some minor technical problems are being intentionally misleading.

30. **Bernhard**
October 23, 2011

may c j,

Miyazawa proposed a proto-SUSY (meson-baryon) thing but as far as I know was not exactly the same SUSY (fermion-boson) that we have today. But in any case, I´m fine to give credit to Miyazawa, makes no difference to me.

31. **Eric**

October 23, 2011

Peter,

Once again, you are throwing in spurious arguments that have nothing to do with the actual point. I presume you are doing so with the intent of being deliberately misleading. Whatever extra parameters (SUSY soft terms) there are in the MSSM, they have no relevance in regards to the loop corrections to the Higgs mass. The loop corrections are only sensitive to the scale of the mass splittings between the SM particles and their superpartners, not on the detailed superpartner spectra which are determined by the soft terms.

32. **Mark**
October 23, 2011

Phil,
Just to make what Peter is talking about more explicit, here is a reference http://www.springerlink.com/content/2845m53jmpw5754h/fulltext.pdf , where the authors first considered a general case with an arbitrary Kahler metric to obtain some general expressions but then picked a particular Calabi Yau and looked at stabilizing a subclass of the moduli (so called Kahler moduli) – the scalar fields that describe the deformations of the internal metric that correspond to the volumes of two- and four-cycles and also control the overall volume of the compactification. You don't have to look through all the details but I would like to draw your attention to the expressions in eq. 3.24. Here \tau_i describe the volumes of four-cycles, t_i describe the volumes of two-cycles and V_X is the volume of the Calabi-Yau manifold used in this example. If you examine eq. 3.24 you will notice immediately that all these volumes are controlled by a single parameter, denoted by \tau_D while all the numerical coefficients are completely fixed by the topology of the Calabi-Yau. In Type IIB compactifications, Kahler moduli control the values of gauge couplings, e.g. the value of the unified gauge coupling \alpha_GUT=1/25 is literally equal to the inverse volume of the four-cycle wrapped by the visible sector D7-brane stack, so if you assume the standard gauge coupling unification and disregard the small threshold corrections, the corresponding volume \tau_GUT=25. Now, if you have a realistic compactification, you could use this as input to determine the value of \tau_D and therefore automatically determine the numerical values of all the t_i, \tau_i as well as the overall volume V_X. So, once you pick a Calabi Yau, your model would be extremely predictive because you would be able to immediately constrain all the physical quantities whose values are controlled by Kahler moduli.

33. **Jim Rohlf**
October 23, 2011

People are quick to forget that LEP took a BIG bite out of supersymmetry pushing it to an odd corner of parameter space for survival. The LHC is only mop-up.

34. **Shantanu**
October 24, 2011

Phil/Eric, if you are so sanguine about supersymmetry.string theory can you tell me what it predicts for theta_13? This is anumber we shall measure very accurately in next few years.
shantanu

35. **anonymous**
October 24, 2011

Phil,

http://www.sciencemag.org/content/322/5905/1224.abstract

36. **anon**
October 24, 2011

Phil/Eric, if you are so sanguine about supersymmetry.string theory can you tell me what it predicts for theta_13?

Non-zero. Because SUSY etc. doesn't have any symmetry principle that requires it to be zero.

37. **Kish J**
October 24, 2011

@anon,

A frog genome weighs roughly 5pg. LHC collisions can accumulate this rest mass in less than an hour, I guess.

38. **Phil**
October 24, 2011

Shantanu,

If you read my comment carefully you will have noticed that I am not sanguine about string theory. Like I said, I'm no expert in string theory (my field, though in physics, is something completely different). I am merely after the truth about string theory and its prospects and how the situation with string theory's predictive power (or lack thereof) is different from quantum field theory's predictive power. No bias, just the "straight dope". So, in answer to your question, I have absolutely no idea. I'm not even so sure of what a theta angle is. I think it's something to do with neutrino masses.

39. **Shantanu**
October 25, 2011

anon,
I want a prediction for its value with error bars. we sort of already know its non-0(I think 3 sigma from T2K results). If string theory is a theory of everything it should be able to predict its value.

40. **aa**
October 25, 2011

i think string theory doesn't require it to be 42 either.
thats my prediction, its not 42.

41. **Denis Boers**
October 26, 2011

If SUSY goes, does that imply that inflation has to go too ?

42. **Peter Woit**
October 26, 2011

Denis,

SUSY and inflation have pretty much nothing at all to do with each other.

43. **HoldYourHorses**
November 1, 2011

Peter, what do you think of this: Natural SUSY Endures, arXiv:1110.6926 ? One of the points it makes is that one must distinguish between sparticles which must be low-mass in order for the theory to be `natural', and others who can be as heavy as they like.

44. **Peter Woit**
November 1, 2011

HoldYourHorses,

Was thinking of writing a post about this, not sure if I'll have time. It's interesting to see that people are focusing on this one sort of scenario, kind of a last hope of avoiding fine-tuning. Interesting question it whether this (and next year's) data will be enough to test it, or if one will have to wait until 2014-5. And when it gets shot down, will SUSY advocates give up?

Besides the paper you mention, there's lots about this at

http://indico.cern.ch/conferenceOtherViews.py?view=standard&confId=157244

Especially pithy is Arkani-Hamed's presentation, see

http://indico.cern.ch/getFile.py/access?contribId=7&sessionId=2&resId=0&materialId=slides&confId=157244

Higgs Non-News

October 24, 2011
Categories: Experimental HEP News

The two main LHC experiments have now recorded just about 5 inverse femtobarns each of data this year (CMS here, ATLAS here). This is the last week of the proton run, so that number will be near the total available for analysis until next spring when the next proton run gets started.

For some idea of what this means for the Higgs search, see for example Tommaso Dorigo's discussion here of last year's ATLAS projections about this. 5 inverse femtobarns was at the high end of what was expected, and the ATLAS projection was that this would be just barely enough to expect to be able to rule out a Higgs at 95% confidence level, all the way down to the LEP limit at 114 GeV. Of course, this expectation is statistical. If the Higgs is not there, and one is lucky (downward statistical fluctuation in number of events), then one can exclude at better than 95%. If one is unlucky (upward fluctuation), or the Higgs is really there, the 95% exclusion will not be achievable.

The LHC Higgs Combination Group has now combined the ATLAS + CMS Higgs results released at the summer conferences, and plans to release this in time for the HCP 2011 conference next month. Because of the efforts of Phil Gibbs, about all that needs to be said about the LHC-HCG plot is that it looks an awful lot like his, which is available here. An SM Higgs is excluded at 95% for masses below about 480 GeV, down to a lower limit of around 135 GeV (the data is actually very flat and close to the right value to exclude the SM from 135 GeV to 145 GeV).

The huge question of course is now whether there is a Higgs with mass between 135 GeV and the LEP limit (114 GeV). The current LHC data shows no definite sign of the Higgs, but the statistics is still too low to really say anything, especially for the lower part of the region. The crucial thing to watch now is the Higgs to gamma-gamma channel, which is the only one sensitive enough to hope to rule out or see a Higgs in the 115-125 GeV region, for the current amount of data collected. I don't know when the experiments expect to release new data in this channel, just that their goal has been to each have some sort of result in December. Perhaps they'll release something at HCP 2011, more likely not. The only rumor I've heard is from someone who has seen a recent plot of the ATLAS data for this channel, and he tells me he doesn't see any bump in this region. But work on this data is on-going, and I have no idea what CMS is seeing or not seeing (my efforts to get Tommaso Dorigo drunk in Antwerp last month didn't yield much).

At last month's CERN Council meeting, there was a report submitted to the Council on "The scientific significance of the possible exclusion of the SM Higgs boson in the mass range 114-600 GeV and how it should be best communicated." The report is based on the summer 2011 data, and it emphasizes excluding the Higgs at not just 95% (2 sigma), but at 5 sigma, something that will require (if the Higgs is not there) combining the 10 inverse femtobarns of data from each of the two Tevatron experiments and a similar amount from each of the two LHC experiments, something that won't be possible until sometime after mid 2012.

Update: One more related item. At Berkeley starting today, ATLAS is holding an Analysis Jamboree on Higgs Searches. First two days is the good stuff, open only to ATLAS members, but the last day there will be an open session with theorists allowed.

Comments

1. **Anon**
   October 24, 2011

   Your link to Tommaso Dorigo's post starts http://http//.

2. **dir**
   October 24, 2011

   to fix the higgs mass, zz*–>4l and h–>2gamma channels are important. it seems to me that philip gibbs's combination of these channels does hint a 140 gev higgs, see http://blog.vixra.org/2011/09/18/higgs-days-at-santander/

3. **Peter Woit**
   October 24, 2011

   Anon,

   Thanks, fixed.

   dir,

   I guess the kind of question raised by Phil Gibbs in that posting explains why CERN is looking for better than 95% exclusion in the combined channels, just to make sure.

4. **chris**
   October 25, 2011

   where are all the spies if one needs them 🙂

5. **Paolo Valtancoli**
   October 25, 2011

   If the Higgs is not found, phenomenologists are ready with their fancy higgless models…

6. **Bernhard**
   October 25, 2011

   Paolo,

   the discussion is about the SM Higgs for the moment. It will take a huge amount of data to discard just any Higgs.

7. **Tony Smith**
   October 25, 2011

   As to the Higgs to gamma-gamma Channel,
   Eilam Gross in Slide 57 of his 29 August 2011 presentation
   "Higgs Searches at the LHC"

described CMS results at around 1.7/fb saying:

"… A local moderate (yet genuine) 2.3 sigma excess
is seen in CMS around 120 … LEE washes out the excess
…
There is 2.8 sigma [around 140] with Higgs to gamma-gamma
which is reduced to 1.7 sigma with the LEE …".

So, whether or not use of LEE (the Look Elsewhere Effect) is
a decision with significant consequences
that needs to be taken carefully in analyzing the 5/fb Halloween data.

Tony Smith

PS – His slide 22 shows the connection between
the 140 peak on the CMS Brazil Band plot
and
the same 140 peak as a bump on the CMS data histogram.

8. **Peter Woit**
October 25, 2011

Tony,

The released gamma-gamma data from this summer is just not enough to have any realistic hope of seeing a SM Higgs signal, at 120 or 140 GeV. To get an idea of the significance of any excess you see anywhere in it, you definitely need to include the look elsewhere effect, since you are looking for an excess anywhere in a large region. Statistically, you're sure to find some excess somewhere.

By now CMS and ATLAS should have much more data analyzed, approaching the amount needed to actually see something. Whether they have enough to say something definitive will be very interesting to see. Maybe we'll find out in December…

9. **null**
October 25, 2011

Has the Tevatron collected enough fb-1 to decide the issue in the interesting region 114-135GEV?

10. **ohwilleke**
October 25, 2011

IIRC, the issue with the 135-145 GeV range was that given the power of LHC to see a signal there, there possible signal was much weaker than one would expect from a SM Higgs even if it had some statistical signficance – we should have see twice as many sigmas of significance as we did at last publication. In contrast, at 115-125 GeV where there was also some hint of a signal, the power of the LHC to see anything in that mass range was weak enough to not be inconsistent with a SM Higgs.

11. **Shantanu**
October 25, 2011

Peter, curious about your comments on the discussion about eternal inflation on cosmicvariance between Tom Banks and Sean Carroll.

12. **younghun park**
October 25, 2011

Peter
As null said, many people including me want to know about the analytic result of Tevatron. Do you know something on Tevatron? If you have heard, I hope to get good information from you.
Cheers

13. **Peter Woit**
October 25, 2011

null, yonghun park,

For the details of the Higgs story at the Tevatron, see here:

http://indico.cern.ch/getFile.py/access?contribId=60&sessionId=0&resId=1&materialId=slides&confId=141983

They have reported results this past summer based on up to 8.6 inverse femtobarns (per experiment), now have about 10 inverse femtobarns. Speculation is that they might report results at Moriond (March), but definitely by next summer's conferences. They should be competitive with this year's LHC results at the low end of the interesting mass range (just above 114 GeV), less so as the mass gets larger. In any case though, they should be able to just barely see or exclude a Higgs in the interesting region. Whatever their result is, if it's consistent with the LHC results (either an exclusion or positive evidence) that would provide confirmation (or the LHC would provide confirmation to them depending on how you look at it…).

So far though, they're not seeing anything in the interesting mass range, but so far with statistics too low to claim exclusion. But they're almost there…

14. **Peter Woit**
October 25, 2011

Shantanu,

I haven't followed the details of their discussion, but from what I did read and what I've heard from Banks in the past, I tend to agree with his argument that effective field theory arguments being used in this area are dubious, so there's no particular reason to believe in eternal inflation. But I'm equally dubious that he has any believable alternative. This is an area where physics has gotten too close to metaphysics for my taste. There's neither experimental observation nor solid theoretical argument available to decide the issues they are discussing, so this kind of discussion can go on eternally without getting anywhere….

11/11/11, Portal to Another Universe?

October 25, 2011
Categories: Uncategorized

According to World News Forecast, 11:11am on 11/11/11 could, if Uri Geller is right, be a portal to another universe. This is from Geller's web-page on the subject:

> **String theory** is said to be the theory of everything. It is a way of describing every force and matter regardless of how large or small or weak or strong it is. There are a few eleven's that have been found in string theory.
>
> I find this to be interesting since this theory is supposed to explain the universe! The first eleven that was noticed is that string theory has to have 11 parallel universes (discussed in the beginning of the "11.11" article) and without including these universes, the theory does not work.
>
> The second is that **Brian Greene** has 11 letters in his name. For those of you who do not know, he is a physicist as well as the author of The Elegant Universe, which is a book explaining string theory. (His book was later made into a mini series that he hosted.) Another interesting find is that Isaac Newton (who's ideas kicked off string theory many years later) has 11 letters in his name as well as **John Schwarz**. Schwarz was one of the two men who worked out the anomalies in the theory. Plus, 1 person + 1 person = 2 people = equality.

Whether or not a portal to another universe does open up, there will be a film opening that day about the topic, see here. In possibly related news, Brian Greene's *Fabric of the Cosmos* series will start appearing on broadcast TV 11/2/11, with the first episode already available here if you are an iperson.

Comments

1. **anonomous**
   October 25, 2011

   Amazing! So if there are 11 characters in a phrase then it's proof of string theory? How about "W+h+a+t + a+n + i+d+i+o+t" = 11 ?!!

2. **Mitchell Porter**
   October 25, 2011

   Also, there are 4 letters in John and 7 letters in Schwarz, and Schwarz's ideas about 3-dimensional superconformal field theories (hep-th/0411077) paved the way for ABJM to figure out the dual theory for AdS_4 x S^7 (arXiv:0806.1218).

   And while we're on the topic, the period between the discovery of the Veneziano amplitude and the Green-Schwarz second superstring revolution was 1968-84; and 196884 is the coefficient in the j-function which led to the discovery of monstrous moonshine.

   Obviously, we're all living in the M(atrix), and string theory is not a theory of physics, it's a set of cheat codes for the corny MMORPG we all happen to be trapped in.

3. **Bee**
   October 26, 2011

   Best post I've read lately! (I'm not a spambot.) In Germany, Nov 11th (at 11:11am) is the begin of the carnival season. If you're not German, I suppose it may appear like a portal to another universe 😉 Widely used paper streamers are clearly a connection to string theory.

4. **apeleytheros**
   October 26, 2011

   Interesting is also, that this guy is reading a sting theory related article and the most he can say about is something about counting the letters. I think kids over 1rst class elementary school can do better.
   Terrifying is that there is still people considering him a kind of a genius or something.

5. **Giotis**
   October 26, 2011

   When a scientific theory becomes part of the popular culture it means that a paradigm shift indeed took place in the public eye.

6. **chris**
   October 26, 2011

   I knew that Ed Witten was a traitor ^_^

7. **Avattoir**
   October 26, 2011

   I think your point is well taken, particularly as a thread enhancer (or, given this one deteriorated pretty quickly, and perhaps predictably so, in this instance more of a savior), but I suspect Gellar doesn't regard himself as part of pop culture (whether his self awareness is important being maybe another sort of thread enhancer); indeed, his periodic encounters with James Randy would suggest he's perfectly aware he's something more like the vulture in the famous cartoon telling his companion that sometimes he just feels like going out there and actually killing something.

8. **Avattoir**
   October 26, 2011

   Ach: I meant to make clear I was addressing my earlier comment to Giotis.

9. **Alejandro Rivero**
   October 26, 2011

   Perhaps we are going to close the time loop reported in http://insti.physics.sunysb.edu/~siegel/parodies/sam/sam.html "The Nobel Prize Singularity": "the time of the work for which they were awarded has long since converged to 1973; It is not yet clear exactly what happened that year: perhaps a time loop or phase transition"

10. **Bernhard**
October 26, 2011

I hope the dimensional portal will be open 11:11 am GMT otherwise it will be a mess of portals opening all over the planet at different times and places. Well if no GMT, people in Australia will be one of the first to enter the portal. I'm afraid the portal in New York will be one the last ones to open, unfortunetly for you Peter.

11. **TR**
October 26, 2011

I don't see anything about string theory on the "iperson" link.

12. **Coin**
October 26, 2011

There's also a new Tarsem Singh (director of "The Fall") movie coming out 11/11. I think I am more excited about that.

Hey, what's the next big prophesied-doomsday date after 2012's past? Do we just start worrying about the Year 2038 Problem?

13. **ja carl**
October 26, 2011

WTF is this pathetic clap trap doing in your blog?

14. **Peter Woit**
October 26, 2011

TR,

The link just advertises the "PBS" app, if you install that you can then download many of their programs, including the first episode of the Nova series.

ja carl,

Either trying to provide entertainment, or warning that you really should watch out for that portal at 11:11 on 11/11/11.

15. **martibal**
October 26, 2011

11:11 am US time ? East coast or west coast ?
I know Newton did a lot of weird stuff, including alchemy, but who could imagine his ideas kicked off string theory many years later ?

16. **may c j**
October 26, 2011

Peter,

considering the fact that your blog is quite popular, I think that by accident you just made an advertisement of Brian Greene's Fabric of the Cosmos series. Perhaps he should thank you 😉

17. **crazy yeti**
October 26, 2011

Did anybody catch this amusing detail:

"HOWEVER, if you count the fingers on your hands in unary arithmetic – no zeros – then 10 fingers, in unary mathematics, = 11111111111 – EXACTLY 11 ONES"

What, does Uri Geller have an extra finger or something?? 😉 Or is he just a bit confused about "unary mathematics"?

18. **derek**
October 26, 2011

Do people like Geller actually believe their own ramblings?
That's not a rhetorical question – I am genuinely curious. I see two possibilities:
1. For reasons unknown (Attention? Psychological games with the naive?) they make absurd statements, but don't actually believe the nonsense.
2. They truly believe in what they say (A much scarier scenario.) One wonders how many numerological calculations were thrown out when they failed to support the hypothesis. Calling such behavior delusional is an understatement.
–
If only I knew how to enter the multiverse's Konami code… I could transport to a universe anthropically tuned for less stupidity.

19. **Thomas R Love**
October 26, 2011

I think 12/12/12 is much more likely to be important. After that, the next triple date is 1/1/1; 89 years later, and I don't plan to be here for that.

20. **Seth Thatcher**
October 26, 2011

This dude is seriously on to something! Barack Obama has eleven letters in his name. Coincidence? i think not!

21. **Philip Gibbs**
October 27, 2011

"Do people like Geller actually believe their own ramblings?"
Do you think there is even the slightest possibility that he believes it? Do you think he might believe that he uses magic to bend spoons too? I think he has amazing powers. Powers of persuasion that is.

22. **Marc**
October 27, 2011

Don't forget that the number of generators of E8 x E8 is 496, which, when multiplied by the number of large spacetime dimensions, gives 1984, the year of the first superstring revolution!

23. **Lau**
October 27, 2011

I actually think it makes sense that to draw attention to himself, he chose the most sci-fi topic.

24. **Coin**
October 27, 2011

Thomas R Love: I think, when the date 13/13/13 comes, THAT is when we need to be really concerned

25. **S. Molnar**
October 27, 2011

Those 11-lettered guys might be pretty good, but Edward Witten goes to 12.

26. **Chris W.**
October 27, 2011

You would think that after all his years in this game Geller could produce something more than fourth-rate numerology.

Check out his appearance (many years ago) on the Tonight Show with Johnny Carson, which can be found on YouTube. Carson (a good amateur magician himself) consulted with James Randi beforehand, and made hash of Geller, and he did it in the nicest, least confrontational way one can imagine.

27. **mandy**
October 28, 2011

its a really interesting perception you have there, but at the end of the day, 11/11/11 is still an ordinary day like any other. It is just my birthday.

28. **jpd**
October 28, 2011

wow, you are going to be born next month?

29. **Andy**
October 28, 2011

The stupid, it burns!

30. **derek**
October 28, 2011

Nigel Tufnel: The numbers all go to eleven. Look, right across the board, eleven, eleven, eleven and…
Marty DiBergi: Oh, I see. And most amps go up to ten?
Nigel Tufnel: Exactly.
Marty DiBergi: Does that mean it's louder? Is it any louder?
Nigel Tufnel: Well, it's one louder, isn't it? It's not ten. You see, most blokes, you know, will be playing at ten. You're on ten here, all the way up, all the way up, all the way up, you're on ten on your guitar. Where can you go from there? Where?
Marty DiBergi: I don't know.
Nigel Tufnel: Nowhere. Exactly. What we do is, if we need that extra push over the cliff, you know what we do?
Marty DiBergi: Put it up to eleven.
Nigel Tufnel: Eleven. Exactly. One louder.

31. **monoceros4**
October 29, 2011

Well, 11 o'clock on 11 Nov *was* the original Armistice Day (and hour) so you can't quite say it's just another day.

32. **Richard**
October 29, 2011

In important scientist sartorial news, [11|11|11 is The Date that Most Closely Resembles Corduroy, Ever].

33. **Henry Bolden**
October 31, 2011

"The second is that Brian Greene has 11 letters in his name. …"

Lisa Randall also has 11 letters in her name. This proves that the universe has 11 dimensions and either that God is androgynous or that God loves show business.

Surely this insight is worth a Templeton Prize!

34. **JAC**
October 31, 2011

crazy yeti: The "unary" he was referring to is probably the one from automaton theory. In the usual formulation, a Turing machine can only store two distinct values on its tape: marks (or 1s) and blanks (or 0s). So the usual method of representing the natural numbers is as a sequence of marks separated by blanks and since having an empty sequence of marks represent a value is inconvenient, zero is usually represented by a single mark and the representation of N+1 is the representation of N followed by another mark.

What this has to do with portals to other universes isn't clear, unless he thinks life is like *The Matrix* only with hardware even more impractical than that shown in the movies.

35. **Uwe**
October 31, 2011

I think Uri Geller is right with his addiction to the 11 at least in one respect: In Germany the carnival season starts at the 11th of November at 11:11h which means you can expect all sorts of foolish behaviour on that date.

http://en.wikipedia.org/wiki/Carnival#Germany.2C_Switzerland_and_Austria
http://en.wikipedia.org/wiki/Cologne_Carnival

36. **Kent Traverson**
November 1, 2011

So what happened on 10/10/10?

37. **Bernhard**
November 11, 2011

Is today Peter, you still have a chance to see it in New York.

No portals opened in Europe unfortunately.

38. **Peter Woit**
November 11, 2011

Here in New York we seem to have survived…

39. **Sakura-chan**
November 11, 2011

Everything is OK in the Pacific Northwest. That's ok, there will be other portals to look forward to.

Templeton Frontiers Program

October 28, 2011
Categories: Uncategorized

The Perimeter Institute announced yesterday a new partnership with the Templeton Foundation, in the form of something to be called the Templeton Frontiers Program. The research areas to be supported are “quantum foundations and information, foundational questions in cosmology, and the emergence of spacetime.” A $2 million grant from Templeton will pay for three postdocs, as well as other programs in this area.

The previous major Templeton effort in this area was the $8.8 million dollars in grants a few years ago that funded FQXI. I’m not aware whether FQXI is still getting money from Templeton, or if it has successfully found other sources of funding.

Update: I hadn’t realized this, but over the last year, the Templeton Foundation has awarded an even larger sum of money in direct individual grants (for details, see here). They’ve made about $2.4 million in grants for research in the area of foundations of quantum theory, and another $1.1 million in grants in mathematics/logic, emphasizing foundational results on the limits of mathematics. These are quite large sums relative to the previously available research funding in these particular areas.

Comments

1. **moneyyyyyy**
   October 28, 2011

   Why exactly should FQXI be either/or wrt Templeton and other sources of funding? Why not get Templeton money AND funding from elsewhere too?

2. **Peter Woit**
   October 28, 2011

   moneyyyyy,

   My understanding was always that FQXI was always trying to get funding from both Templeton and elsewhere, I just don’t know how much success they’ve had.

3. **Bernhard**
   October 28, 2011

   Supporting research for emergence of space-time (EST) is an idea I´m in favor of. Kind of thing Lee Smolin is advertising for years. I´m curious tough how projects for EST are judged. The funding agencies I know here in Europe would never have something so specific. In any case, as usual, money goes to postdoctoral positions, not jobs.

4. **Allen Massey**
   October 28, 2011

   How does this fit with unbiased science? The Templeton foundation seems to insist on linking all science to God and religion.

5. **Richard Séguin**
   October 28, 2011

   In addition to the religious aspect of their foundation, John Templeton has also been contributing money to the extreme right wing politics of the day. A recent example is a contribution to fund the reelection of a Wisconsin supreme court judge who was probably crucial to maintaining a supreme court favorable to program of governor Walker. I believe they also contribute to the Heritage Foundation.

6. **Avattoir**
   October 29, 2011

   So, the first temptation is to think Perimeter has made some deal with the devil in taking money from Templeton, implying this compromises Perimeter in pursuing science in an areligous way, but I actually think most here would see this as something more subtle; after all, not just is Smolin there but the joint is run by Neil Turok, both of whom have written more than enough for us to judge them on this, and each comes out looking fine.

   This, I think, and again I’m sure I’m not alone in this, is John Templeton purchasing tolerance for his activities in support of the right by covering his areligious left flank. He and his apologists will be able to point to this funding of Perimeter in an effort to blunt criticism for the inexcusable bigotry and ignorance they will continue to fund elsewhere. I think it was Martin Rees who earlier this year tried to erect some sort of wall between the prize money Templeton ‘gave’ him and his science chops, and that sort of thing, a general gift to a person, gets way too obvious because then Rees had to talk and talk about that dang wall. This is shrewder: both Templeton and Perimeter can point to the specificity of the grant, and no one is going to be going after Turok or Smolin as having volunteered to wear a scarlet R.

   It also suggests to me John Templeton is very likely anticipating doing some awfully nasty things in the 2012 election.

7. **may c j**
   October 29, 2011

   As far as I remember this 2M is exactly (or close to) the amount Verlinde got for that entropic thing he is doing. Funny coincidence.

8. **Harry Dale Huffman**
   October 29, 2011

   Just another instance of “The World is in the Hands of Children”, as I see it. Money, money, money, money, money, money, MONEY — it’s a gas (as Pink Floyd used to sing). Put that together with “the emergence of spacetime” in the same sentence, and it becomes clear, to me, that this is a circus announcement, meant to thrill the geeks (the paying customers/supporters; the academic establishment first and the public last). It’s Not Even Wrong, and It Is To Laugh, or Cry.

9. **Yatima**
   October 29, 2011

   I don’t know what the problem is here (and no problem was mentioned in Peter Woit’s post, either). Cries of “right-wing” this and that and bemoaning of money changing hands seems to be a hankering for some bizarre Marxist position that should have been abandoned when people promoted from high school.

Or to put it more succintly:

"If all the researchers would work for free, all of this could have been avoided."

10. **Peter Woit**
October 29, 2011

For more about the complicated issue of what the Templeton Foundation funds and how it is run, see a previous posting

http://www.math.columbia.edu/~woit/wordpress/?p=3396

and the links contained there.

11. **Albert Z**
October 29, 2011

Rumor has it that Don Page is going to be appointed the Archbishop of Perimeter.

12. **Allen Massey**
October 29, 2011

Of course research needs funding (and lots of it), but taking money from a group that tries to bend science to support their view of creationism seems like a bad idea.

Would the PI be comfortable accepting money from the flat earth society? Especially if the PI knew the flat earth society would advertise that the PI and the society were working together?

All I am saying is that taking money from people that want to turn back the clock a few thousand years may not be the best idea.

13. **Jim Akerlund**
October 29, 2011

may c j,

According to Peter's post of June 2011, Verlinde got 6.5 M. http://www.math.columbia.edu/~woit/wordpress/?p=3781
Just wanted to let you know.

14. **Peter Woit**
October 29, 2011

Allen Massey,

The Templeton people are not creationists, Flat Earthites, or multiple thousand year clock-turn-backers. They are in favor of both religion and science, which is not exactly an unusual combination among people in the US and Canada today. From what I have seen they generally keep religion out of their physics research funding. Except perhaps in the case of their fondness for multiverse studies, but there the danger to science is coming not from without, but from within…

15. **Avattoir**
October 30, 2011

Peter, I found your take-down of Sean Carroll's inaugural feature in Discover on the multiverse compelling for the latter materially depending on string "theory", but then a few days later I read this:

http://scienceblogs.com/startswithabang/2011/10/why_we_think_theres_a_multiver.php

which purports to rely entirely on concepts both consistent with the Standard Model and not at all on string "theory".

I wonder if you'd consider letting us know your view on whether that purport is accurate, and secondarily your take on the argument as a whole.

16. **Peter Woit**
October 30, 2011

Avattoir,

That blog posting is mostly just about inflation, with eternal inflation mentioned at the end. Not everyone agrees about inflation implying eternal inflation, see the Tom Banks guest post at Sean's blog. There's certainly no relevant experimental evidence, and the theory is highly speculative.

As the blogger notes at the end, the eternal inflation argument gives something different than what Sean and the string theorists are promoting. All the extra disconnected universes you get have the same physics, this isn't a multiverse with different physical laws in different places. You can't use it to try and justify not being able to explain anything. It really tells you nothing about fundamental theory, as I wrote:

"Inflation is part of the story, but it's not a fundamental theory by itself. All it can tell you is that your fundamental theory should have an inflaton field of some kind, with a potential satisfying certain properties. "

17. **Giotis**
October 30, 2011

"the eternal inflation argument gives something different than what Sean and the string theorists are promoting. All the extra disconnected universes you get have the same physics, this isn't a multiverse with different physical laws in different places. You can't use it to try and justify not being able to explain anything. It really tells you nothing about fundamental theory"

Peter, the eternal inflation described in the aforementioned blog is one type of eternal inflation. The string landscape picture fits with the false vacuum driven eternal inflation where part of a universe in a inflating dS false vacuum may tunnel to another lower dS vacuum while the parent false vacuum keeps inflating. As you know the properties of the compact extra dimensions changes in the new vacuum because the moduli fields (which constitute the landscape) change and thus in general you may have different physical laws e.g. a different standard model.

Of course these things are well known but for some reason you avoid to mention them in your reply and thus your statement may be misinterpreted by the uninitiated.

18. **Peter Woit**
October 30, 2011

Giotis,

The eternal inflation argument made in the blog entry that I was being asked about is explicitly one where you have a simple dilaton field. This doesn't affect at all the SM. The author states this quite clearly and notes that the string theory landscape, with its multiple moduli fields, and moduli stabilization problem is something quite different and much more complicated, for which there is not the slightest evidence (unlike inflation itself).

What's misleading is the often-made argument by string theory multiverse proponents that evidence for inflation based on a single scalar with a specific potential chosen to make inflation work is somehow evidence for the string theory landscape picture with its (hundreds…) of moduli fields, determining all low energy physics.

19. **Jess Riedel**
October 30, 2011

I'm a grad student advised by Wojciech Zurek, and our work is partially funded by the Templeton Foundation. Here's my impression, for what it's worth.

I have not gotten the slightest hint from the foundation that we should connect our research with religion in any way whatsoever. Wojciech's grant proposal to the foundation made no mention of it, and the conference we attended back in July with the other grant recipients was similarly completely secular. (The description of [Wojciech's grant] and those of the other grant winners are available on the Templeton website.)

As you can expect, the topic of Templeton bias came up during dinner conversation. Among the presenters I spoke with, none claimed to be theists or expressed any sympathy with religious ideas. I also had a chance to speak with the two representatives of the Templeton foundation who were personally chosen (along with others) by John Templeton to help direct the foundation after his death. They seemed very genuine in their belief, which they said was shared by John Templeton, that truths about science and religion would arise naturally from objective research. I didn't get the sense that they had any interest in trying to influence anyone at all.

Of course, just choosing who to fund unavoidably influences the direction of scientific research. And the Templeton Foundation unabashedly funds scientists sympathetic to religious ideas. But, to my knowledge, they do so with grants which are explicit about this. (Presumably, they think these ideas aren't given adequate consideration from other funding sources.) The idea that they are cleverly trying to steal the reputability of Perimeter Institute or of the physics community at large is pretty laughable.

And yes, even genuine, nice people can do harm if they are convinced of sufficiently destructive ideas. But I really don't think this is true of the Templeton foundation. The impact on physics of the foundation's funding will be grossly positive.

20. **Peter Woit**
October 30, 2011

Thanks Jess,

I agree that from what I've seen, when the Templeton people fund something related to religion, they are explicit about it, when they fund science they leave religion out of it.

I hadn't realized that this year they have funded a variety of physics related grants, including the one you're associated with, with information about them at their web-site at:

http://www.templeton.org/what-we-fund/grant-search/results/taxonomy%3A2?page=1

I'll add a note about this to the posting.

21. **Anon**
November 2, 2011

Avattoir, just wanted to mention that John Templeton has been dead since 2008.

22. **Peter Woit**
November 2, 2011

Anon,

He means the son, John M. Templeton, Jr. (Jack), who is now in charge of the Foundation.

23. **Bobito**
November 3, 2011

Yatima: When one takes money from the Templeton Foundation or the NSA or the DOD one is at least implicitly, and often explicitly, participating in the agenda of the funder. In the case of the NSA or the DOD the legitimacy of the funder, in a purely scientific sense, is not much questioned, whereas in the case of the Templeton Foundation, some regard its agenda as even anti-scientific. It's not a question of right or left wing. It's a question of whether the goals of the agency funding one's research act in favor or to the detriment of science and understanding. In the case of the NSA or the DOD those goals might even be to the detriment of humanity (how one sees this depends on how one views activities such as building atomic bombs or cloaking devices for robotic aircraft rather than on ideology per se – comparing the Obama and Bush administrations, one sees that these goals are not much dependent on political ideology, at least in the US, both being fully supported by all important political actors).

What also happens is that all these organizations fund genuinely basic research that apparently has no direct contact with their explicit agenda. They do this for at least two reasons. The two I have in mind are: 1. they understand that basic research, directed by curiosity, is at least as likely to produce results useful to them as is heavily focused research. 2. they see funding basic research as a way to counteract the sort of criticisms I implicitly made in the previous paragraph. For example, the Templeton Foundation can defend itself by saying that it funds much research that has nothing to do with religion, and those who accept that money can salve their consciences by believing that what they do does nothing to promote its agenda, although their acceptance of its funds gives it credibility.

24. **Anon**
November 3, 2011

Bobito, you have raised a good point. Those among us (almost all in the U.S., I think) who have accepted funding from the NSA or the DOD have no moral standing to condemn receivers of Templeton funds.

25. **patfla**
November 3, 2011

Is PI's funding particularly problematic given the declining fortunes of RIM (as in the Blackberry)?

PI was founded (in the money sense) by one of the founders of RIM, Mike Lazaridis, but I don't know the extent to which PI has remained a beneficiary of either Lazaridis or RIM.

If so, then your iPhones (I'm a programmer but haven't sprung for an iPhone) are contributing to PI's difficulties.

26. **Avattoir**
November 3, 2011

Patfla, I cannot see why that would be. Just going off the disclosure on the Perimeter Institute website, it looks like not just Lazaridis but anyone connected to RIM last provided large funding in 2009, with the bigger donations from that group coming even earlier, and at about the same time that roughly comparable funds were coming in from the governments of Ontario and Canada. A lot of all that would have been construction, and just from having been involved in a few of these sorts of things in the past, it looks like the PI is working on an endowment model for its basic work, with the new projects like the one raised in this post being the ones that need new targeted funds.

27. **Bobito**
November 4, 2011

Anon: I think your "almost all" is too much, as I know physicists who for many decades have had funding, never once from the DOD, DOE, NSA, or anything similar. In the 60s and 70s there was more consciousness about this, I think, than there is now. There is always the NSF. One can argue that it's the same government, but I think that is not entirely fair. On the other hand, I agree with what may be implicit in what you say – I'd perhaps go further – while I object to the Templeton Foundation's mission, I find that its mission is far less problematic than that of the DOD or the NSA, for instance, simply because it never intends to do any concrete harm to anyone, whereas those institutions certainly do.

28. **Anon**
November 4, 2011

Bobito, it is my impression that the majority of important high energy physics departments in the U.S. get a large part of their funding from DOE grants and that it would be almost impossible to avoid benefiting from this. Even if you are not the grantholder, you may be his student and get paid from his grant, your postdoc may be funded by such a grant, you may share lab equipment, computers, or secretaries paid for with DOE money, your expenses or your office as a visiting scholar to a department may be paid from such a grant, or you may attend a conference sponsored by the DOE.

Recollections of Rudolf Haag

November 3, 2011
Categories: Uncategorized

Thomas Love pointed me to a wonderful article from last year by Rudolf Haag. It's more or less a short memoir of his scientific career, entitled Some people and some problems met in half a century of commitment to mathematical physics. There's a lot about the history of mathematical physics related to quantum field theory that I learned from the article, which covers the second half of the twentieth century. Haag started out his career heavily influenced by Wigner and his work on representations of the Poincaré group, investigating what this had to do with quantum field theory. He has been one of the leaders of the operator algebra approach to formulating QFT.

His comments about the Witten and string theory bring back memories of the late eighties, when several people told me of similar experiences. Haag writes:

> I had been asked to give a physics colloquium talk about my views on quantum gravity and hoped to have some discussion with Ed Witten. Next morning he greeted me by saying: "Your talk was very interesting but I would really advise you to work on string theory". When he saw the somewhat incredulous look on my face he added "I really mean it. I shall send you the manuscript of the first chapters of our book". This ended our discussion. Back in Hamburg I received the manuscript but it did not convert me to string theory. I remained a heathen to this day and regret that meanwhile most physics departments believe that they must have a string theory group and have filled their vacant positions with string theorists. To be precise: It is good that people with vision like Ed Witten spend time trying to develop a revolutionary theory. But it is not healthy if a whole generation of young theorists is engaged in speculative work with only superficial grounding in traditional knowledge. In many popularised presentations the starting point of string theory is explained as the replacement of the fundamental notion of "particles" with its classical picture of a point in space or a world line in space-time by a string in space respectively a two-dimensional worldsheet in space-time. This, I think, is a misunderstanding of existing wisdom. First of all, paraphrasing Heisenberg, one may say "Particles are the roof of the theory, not its foundation".

Comments

1. **younghun park**
   November 3, 2011

   Thanks for introducing haag's writing.

   In his writing, I notice these sentences that First of all, paraphrasing Heisenberg, one may say "Particles are the roof of the theory, not its foundation".

   Could anyone say what this means in detail?

2. **Peter Woit**
   November 3, 2011

   younghun,

   See the more detailed comments about Heisenberg and S-matrix theory at the bottom of page 272 and top of page 273.

3. **Thomas R Love**
   November 3, 2011

   IMHO, Haag's most powerful statement comes a few lines later:

   String theory is hailed as the most promising among present endeavours. But it is
   an overstatement to call it a theory. It has not settled down to a well defined formalism
   nor has it explained any existing puzzle nor can I see that it can make contact with
   any observable phenomenon in the foreseeable future.

4. **Peter Woit**
   November 3, 2011

   Thanks Thomas,

   I should have included that quote too…

5. **Peren Woondeen**
   November 4, 2011

   > "This had induced Heisenberg 14 years earlier to search for the truly observable quantities in elementary particle physics. It led him to the concept of the S-matrix and the … " (p. 272)

   I thought the S-matrix was invented by Wheeler in 1937. Did Heisenberg actually anticipate Wheeler?

6. **Thomas R Love**
   November 4, 2011

   Peren, According to Wikipedia:

   "S-matrix theory was proposed as a principle of particle interactions by Werner Heisenberg in 1941, following John Archibald Wheeler's 1937 introduction of the S-matrix." So Haag's memory is faulty.

7. **David Brown**
   November 5, 2011

   Haag won the 1970 Max Planck medal. Has anyone compiled a list of criticisms of string theory from Planck medal winners?

8. **Maynard Handley**
   November 10, 2011

A link to SpringerLink? This (not even a paper, just a reminiscence) is stored behind a paywall, not at arxiv? Not cool, Haag, and not cool for linking to it, Peter.

Columbia and other US institutions may get free access to everything on SpringerLink, but the rest of us are not so lucky. A great advantage of physics over other fields is that we have rather more arxiv and rather less SpringerLink, and this is maintained in part by letting those who use SpringerLink rather than arxiv know just how much scorn and contempt we have for them.

9. **Tim van Beek**
November 11, 2011

younghun park said:

> First of all, paraphrasing Heisenberg, one may say "Particles are the roof of the theory, not its foundation".
> Could anyone say what this means in detail?

A simplified and short explanation: The starting point of AQFT is a net of operator algebras which contain all observables of a given QFT as selfadjoint elements fulfilling a certain set of axioms. This is the only input to the theory, so you could say that AQFT is quantum field theory without fields. Fields may be used as an auxilliary device to construct the net of observables, but they are no necessary ingredient.

The philosophy of AQFT is to derive every other concept from the net of observables, including the particle content. The concept of a particle as a point in space or as a worldline in spacetime does not enter the theory here. Instead, it is a theorem that the algebra of observables of a point of a spacetime is necessarily trivial, which means that AQFT already says that the concept of a pointwise localised particle is oversimplified.

Furthermore, the Reeh-Schlieder theorem says that there cannot be a localized particle number operator, which means that the question "how many particles where in a certain time interval in a certain box" is meaningless in AQFT.

For more information I would like to recommend the nLab as a starting point:

http://ncatlab.org/nlab/show/AQFT

10. **Mario**
November 15, 2011

Alas it is behind a paywall.

Solvay Update

November 3, 2011
Categories: Uncategorized

With the Solvay Centenary conference now finished for over a week, some information has been posted about it on the Solvay Institute web-site, where you can see a group picture of the attendees on the main page. Lisa Randall's comment comparing the gender balance to that at the first conference is actually rather restrained, given that the fraction of women (2.9%, 2 out of 69, Randall and Silverstein) is smaller now than back in 1911 (4.1%, 1 out of 24, Skłodowska-Curie). Oddly, only women working in extra dimensions seem to have made the cut.

The schedule of talks is here. Perhaps someday we'll see proceedings of the conference and learn what was in the talks and discussions. For now, this page has links to some outlines of the rapporteur talks. There were two separate sessions related to quantum computing (here and here), as well as sessions on condensed matter, particle physics, quantum gravity and string theory.

The string theory talks seem to have just been about possible applications to condensed matter and to quantum gravity, rather than about using string theory to get a unified theory. Witten attended, but appears not to have given a talk of any kind.

Comments

1. **Kea**
   November 3, 2011

   Oddly, only women working in extra dimensions seem to have made the cut.

   Not at all odd. To have one's existence acknowledged, it is often a prerequisite for intelligent women to be working on Safe Topics.

2. **Peter Woit**
   November 3, 2011

   Kea,

   Well, in that case it's odd that speculating about extra dimensions is a "Safe Topic", whereas doing condensed matter physics or quantum computing isn't…

3. **Lee Brown Jr.**
   November 3, 2011

   But she was talking about the number of chromosomes, in which case, this year there were 71 X's and 67 Y's, a ratio of ~ 1.06 . At the original Solvay, 25 X's 23 Y's ~ 1.08.

   So, she was right. Not much change in the ratio, considering it's been 100 years.

4. **Kea**
   November 3, 2011

   I meant Safe from the dangerous possibility of being phenomenologically important. That is, I would not blame Gross here. It is the CMTs and QITs who *don't* believe String Theory who are at fault for neglecting women.

5. **Shantanu**
   November 4, 2011

   Peter or anyone else, any talks on LQG or background independent approaches to quantum gravity at this meeting?

6. **manybodycpa**
   November 4, 2011

   Are the talks really just five minutes long, as suggested by the schedule?

7. **chris**
   November 4, 2011

   come on, this institution is obviously past its prime.

8. **Aleksandar Mikovic**
   November 4, 2011

   The link outlining the talks of the Solvay conference rapporteurs was quite interesting to read. For me the most interesting was the quantum gravity outline by Maldacena, since I do research in quantum gravity. Although not being able to see the
   complete content of the talk, the outline was quite indicative. Maldacena´s view of QG
   is heavilly biased by string theory and AdS/CFT correspondence. The other approaches to QG, like Loop Quantum Gravity, seem to have been completely ignored, and I consider this as a serious flaw in his rapport, since a rapporteur should review the field and not just give a rapport of his own work.

9. **Anonymous**
   November 4, 2011

   > Oddly, only women working in extra dimensions seem to have made the cut.

   Do physicists prefer women with extra dimensions?

   (apologies to everyone, I just couldn't resist it)

10. **Kea**

November 4, 2011

Anon, no, I assure you. Extra dimensions count against you.

11. **anon2**
November 4, 2011

One possible objection to AdS/CMT is that a practically minded physicist cares about whether the investment into a theory is worth the output. In the theory of critical phenomenon, mean field theory and Landau's theory are extremely simple, and provide some qualitative insight into phase transitions despite being a toy model. On the other hand, the exact solutions of the Ising model and other integrable models are (were) mathematically very challenging, but provide more detailed information than mean field theory.
In contrast, AdS/CFT is by all accounts very mathematically involved, yet only gives you a crude *toy* model, and attempts to construct more realistic models soon get more complicated and ugly.
It's good that a small number of single-minded people are pursuing this research direction, but the above cost/reward analysis means that the majority of condensed matter theorists who are result-oriented are justified to ignore this topic.

This Week's Hype

November 3, 2011
Categories: This Week's Hype

Now that the LHC has turned out to be dud, producing no black holes or extra dimensions, the latest news is that physicists are planning a new machine, "to follow in the footsteps of the Large Hadron Collider". This one will be based on "A laser powerful enough to tear apart the fabric of space", able to "rip a hole in spacetime", and it will do this much more cheaply than the LHC ($1.6 billion).

For details, see for instance here, here and here. The new laser will also explain what dark matter is, and provide new treatments for cancer.

It's unclear to me who is responsible for the extra-dimensional hype, which appears to be inspired by ADD models that were popular 10 years ago (and that became much less so once the LHC turned on and saw nothing).

Comments

1. **anonymous**
   November 3, 2011

   So Europe is going to fund this with their bailout money? I can't believe anybody would pay ten cents towards it. I guess this is what happens in the ultimate fantasy – after all dimensions and black holes have been exhausted we will just through billions of $$ of cash into the furnace.

2. **Nat Singleton**
   November 3, 2011

   Seems to me that the military will find this new project of particular interest.

3. **Z**
   November 4, 2011

   I think the NIF lasers at Livermore has higher radiative flux (and energy) per pulse than this proposed laser, although the purpose is different. There were some nice news focus articles last week in Science on the NIF.

4. **Dave**
   November 4, 2011

   The LHC is far from a dud. The experiments are doing exactly what they were designed to do – hunt for the Higgs. Its also doing this in a careful, non-sensationalist way and the results (positive or negative) will be landmark measurements.

   In addition to that, the LHC experiments are steadily falsifying a number of models which purport to describe new physics at the TeV scale and making precision measurements of SM processes in the electroweak and strong sectors which take place at the smallest measurable distance scales .

5. **Armin Nikkhah Shirazi**
   November 4, 2011

   If the laser can be used to measure the gravitational field of a laser beam, then that would seem to me to constitute the first direct experimental test of the assumption that gravity must in fact be described by a quantum theory. As this fundamental assumption underlies basically all current theories of quantum gravity, it seems to me that this would make it a very important experiment. In fact, if it finds something totally unexpected, it could become the MM experiment of the 21st century.

   I really hope that they are considering this type of experiment.

6. **chris**
   November 4, 2011

   Armin,

   this assumption has been tested by neutron interference in a gravitational field almost 2 decades ago.

7. **Armin Nikkhah Shirazi**
   November 4, 2011

   Hi Chris,

   You are probably referring to Staudenmann et. al 1980 (although there are probably others) where Bragg reflected neutrons were observed to accelerate at g. In fact, when you get down to it, the gravitational bending of light, Einstein Rings etc. have already tested the same assumption that you speak of.

   What characterizes these kinds of tests (and all others that are tests of gravity involving quantum objects that I am aware of) is that strictly speaking, they involve testing the passive gravitational charge of quantum objects.

   The test I mentioned would be a test of the active gravitational charge of a quantum object (here, a beam of photons). Now, I agree that it is eminently reasonable to assume that since we have already confirmed that they obey the equivalence principle as applied to passive gravitational charge, then we should also expect that it applies to the EP as applied to gravitational charge. If it didn't, it would appear that conservation of momentum would be violated, as a first of a host of major consequences…

   …but still, should we not at least make an effort to test our assumptions by experiment? If you know of a direct test of the gravity field produced by a quantum object, please do let me know, as I am unaware of any, despite searching for it. So, to the best of my knowledge this is still an empirically unverified assumption, as plausible and reasonable as it is.

   I made the comparison with MM consciously. If anyone had suggested a null result prior to the performance of the actual experiment, they would have been laughed at for making such an obviously ridiculous suggestion.

   However, it was a null result, and as result the physics of the 20 century looks completely different from the physics of the 19th century.

8. **Bernhard**
November 4, 2011

Apart from the confusing hype of probing extra-dimensions, does anyone know what is the real physical potential of this experiment and what is it really trying to measure?

9. **Anon**
November 4, 2011

I am not sure why the negativity. I would have thought proposed expenditure on foundational experimental HEP were a good thing. What am I missing?

10. **mesasola**
November 4, 2011

Hello from Greece
here in Europe we do not have that kind of money just for theories.

11. **Rhys**
November 4, 2011

Never mind the brief mention of extra dimensions; the whole Telegraph article is awful. I don't know whether it is entirely the journalist's fault, or whether somebody has fed him the nonsense line "this laser will tear apart the fabric of space", and misled him into suggesting it will allow scientists to observe the "ghost particles" which usually "pop in and out of existence too fast to be seen". "Ghosts" are a technical feature of certain QFTs, but certainly can't ever be observed, even in principle. And did a physicist really say "even a true vacuum is filled with pairs of molecules…"?

The Daily Mail is, unsurprisingly, even worse:
"Scientists say it will be so powerful they will be able to boil the very fabric of space and create a vacuum."
FFS, to use the vernacular of the age…
In fact, this article also says "tear apart the fabric of space", so that has obviously come from somebody who should know better, rather than the journalists themselves.

Bad reporting of science, whether caused by the journalists or the researchers whom they interview, is at least partly to blame for the stupidity of people like "mesasola" and the first anonymous commenter in this very thread.

12. **anon.**
November 4, 2011

It seems pretty clear that what they're actually proposing is to test the Schwinger effect….

13. **jg**
November 4, 2011

At the offical site there is no obvious hype about extra dimensions etc
http://www.extreme-light-infrastructure.eu/what-is-eli.php

And in fact there may be practical benefits:

> ELI will have a large societal benefit in medicine with new radiography and hadron therapy methods. It will also considerably contribute to material science with the possibility to unravel and slow down the aging process in nuclear reactors and in the environment by offering new ways to treat nuclear wastes.

14. **Julien Sorel**
November 4, 2011

Peter, please don't judge the ELI Ultra High Field Laser until it has had a chance to produce results. This machine has great potential. The physics community should put its whole support behind this project.

15. **AI**
November 4, 2011

So far LHC is a dud, it didn't lead to any new leaps in our understanding of reality. Cutting down empty speculations is far from enough. What we need to justify it's costs is new actionable knowledge to further fuel technological progress of human race. Progress which when it comes to fundamental physics has stalled for decades.

The reality is that you physicists are failing us all. Whether it is due to your personal shortcomings or the fact that remaining problems are "objectively" intractable at current levels of human intelligence and technology is in the end irrelevant. If all you can come up with is speculations and hype don't expect the money to keep coming.

16. **strong field physicist**
November 4, 2011

While generally sympathetic to the aims of your crusade, you can really bend the stick too far – this article being a case in point. The physics behing ELI is really quite simple, to produce an electromagnetic field strong enough to polarise the vacuum and pull virtual particles onto the mass shell. Its nothing more complicated than basic QED.

Yes everyone knows the high energy laser facilities have spin offs that include medical applications. That's one of the selling points of FELs and Synchrotrons around the world.

Are you against selling points for the funding of physics facilities? Whose side are you on?

17. **Peter Woit**
November 4, 2011

Dave,

The "LHC is a dud" comment was tongue in cheek, it only made sense if you believed the hype about the LHC being designed to look for extra dimensions.

Others,

This in no way is intended as a criticism of the ELI project, which seems likely to do interesting science with practical applications, including medical ones. The "tearing apart the fabric of space-time" hype though I think is a big mistake. This kind of intense EM field may do all sorts of interesting things, but it's not going to do anything to space-time degrees of freedom. I can't see exactly where the reporters got this from in this case, but physicists need to put a stop to selling everything with a helping of extra-dimensional BS. This may work for a while, but sooner or later is going to do significant damage to the credibility of the field (this may already have happened).

18. **jpd**
November 4, 2011

"ADD" = attention deficit disorder?

19. **Andy**
November 4, 2011

I think this is just an endemic problem with science communication. Scientists have a duty to talk to the public about these sorts of big iron projects (we are, after all, spending their tax money) but having to go through the media is really hurting the message.

Of course the media is going to muddle the details and sensationalise; they're the media, that's what they do. I wouldn't be surprised (if this was some sort of press conference rather than a press release, they don't seem to reference their sources…) if the scientists were fed the question about extra dimensions, answered it in a typically scientific way with caveats (e.g. "I suppose the possibility exists that extra dimensions could be seen under such and such a condition" etc.) and the media spins it into "Experiment looking for extra dimensions!".

Likewise things like "the vacuum is full of pairs of molecules" is almost certainly a misquote. No physicist would be daft enough to say that on purpose.

That said, I don't know what the solution is, other than training people who come in contact with the media to consider them as the enemy.

20. **SpearMarktheSecond**
November 4, 2011

At first I thought that link might have something to do with old proposals to backscatter lasers off of electrons beams, and do gamma + gamma > Higgs. But looking at the link, it seems to be (as strong field physicist notes) a QED experiment. I liked Uranium + Uranium for strong field QED better.

What I'm hearing is a`absence' of an LHC Higgs might be interpreted as a suppression of the rate of Higgs ->gamma gamma via loops from new physics that destructively interfere with SM or SM+SUSY loops. Anyone got info on that?

21. **Doug**
November 4, 2011

I think the negative consequences of the thoughtless hyping of speculative physics are quite right here in this thread. The commenters anonymous in the first post, mesasola, and AI. People who are not informed enough to know what is actually going on and just get their info from sound bytes and press releases (although one would hope that people coming to a blog like this would know better) and make erroneous comments like "Progress which when it comes to fundamental physics has stalled for decades." AI is right about one thing though: if the public feels like they've been misled by the physics community they will not decide to fund future important projects on the level of the LHC or ELI (and many smaller projects that will be shut out of the smaller monetary pool).

I do think it is perhaps a problem of irresponsible journalism: I would wager that whoever made the "tear a hole in spacetime" comment was being purposely hyperbolic, and that any speculations are couched in realistic views about what can be achieved (unless the interviewee was Kaku or Greene). The journalist probably just decided to only include the "sexy" bits because that's what sells papers.

Which goes to illustrate: unless you know the periodical publishing your interview is scientifically reputable, you should treat them as a tabloid and give them the bare minimum of speculation to work with.

22. **Doug**
November 4, 2011

*quite apparent

23. **MP**
November 4, 2011

Actually AI is right. Scientists *are* failing us. The amount of money sunk in theoretical models that are basically a bunch of BS are hurting the community and it's creditability. The LHC certainly is a dud in that respect. Never mind it is doing fantastic work. It isn't working as advertised. No SUSY, no extra dimensions, no Higgs, no no no. Just confirmation of what we already knew. Woit and Smolin are dead on. The string mafia is keeping valuable money out of alternative research. The amount of money sunk into to that BS in incredible and there are no signs of letting up. Variety is the spice of life; there *needs* to be more research done in something else than string.

24. **Peter Woit**
November 4, 2011

SpearMarktheSecond,

I don't know anything about the particular scenario you mention, but if the Higgs is really not there, part of the way it will play out is with theorists coming up with a sequence of models, more and more complicated, trying to evade the no-Higgs conclusion. Lowest masses around 120 GeV will be the last to be ruled out, mainly by the H->gamma gamma data, so evading that SM prediction will be the first order of business….

25. **Peter Woit**
November 4, 2011

MP,

I don't think there's a lot of money (on the scale on which HEP is financed) being sunk into string theory, extra dimensional models, the multiverse, etc. these days. There is a huge attention given to them in the press though, and that's going to cost a lot to the credibility of theorists. The LHC has cost and is spending a lot of money, which is money well spent: the only way to find out if there is a Higgs or not and to make progress is to look for it with something like the LHC. I think it was a mistake for people to try and justify the LHC with extra dimensional models, but already people are stopping doing that, as the models fail. Hopefully that mistake won't have long-term consequences.

26. **anonomous**
November 4, 2011

AI is dead on – the consequences (from a public perspective) of the LHC are damaging. Many exotic objects were promised – we all know good science comes from ruling out models but not getting positive evidence on any model is bad news from a funding perspective. In other words, we believe in science but the era of big budget science funding is likely go the way of Apollo and Orion missions of NASA. Once this happens it will take quite a while to get some public trust back. Our criticism of ELI is not based on the science (which would be very interesting) but on the hype that is surrounding it to try and get it funded.

27. **Christian Takacs**
November 5, 2011

Peter,
String Theory and a great deal of physics hype and speculation for the last fifty years may quite possibly be ruled out by the LHC quite soon. Before more money is spent on the next shiny new toy, should not the physics community do some house cleaning? I am curious at this point how far back the wayback machine is going to have to be set to put physics on level footing with testible reality again…back to QCD? earlier perhaps? It would be wise in the name of enlightened self interest for the high energy physics folk to reacquaint themselves with the formula HYPE = BULLSHIT = BAD FOR FUNDING.

28. **Eye**
November 5, 2011

AI and MP are happily using the web (which was given for free to the world as a result of developments that ocurred at CERN) to show us their ignorance. Shame on you. Those who say the LHC is failing us don't seem to understand very basic things. One is that doing science is not a Hollywood movie, full of easy discoveries every two months. It is a painful endeavour. What the LHC guys are doing is extremely difficult and they are doing a superb job of it. What comes out of the LHC is whatever nature hides there for us to find and the LHC finds will remain forever in the history of science as a treasure for all humankind, long after nobody remembers who Obama was.

29. **Jeff**
November 5, 2011

Eye

I'm not going to comment on the lhc, as a mathematician I have the luxury of not worrying about heavy duty funding. The notion that the web came frome CERN however is just silly, HTML came from CERN, but HTML is just an Implementation of hypertext, an idea which goes way back (the first Mac came with a program "HyperCard" which had clickable links and allowed you to build your own decks). The nuts and bolts of the web go back well before that. It is certainly true that the web came from public funding though.

30. **MP**
November 5, 2011

EYE,

You are misunderstanding me. The LHC is doing great and fantastic work and will be for the foreseeable future. The folks who work there are advancing science the way science should be advanced.

The failing part is selling machines to the public that don't produce the predicted speculative models. The public quickly starts thinking that the science community are a bunch of clowns that simply make (complicated) stuff up.

If the LHC was sold as a "advance science" machine then it would be exceeding expectations. Speculative science is great and needs to be part of the eco-system but it is called speculative for a reason.

Again, LHC = good, selling lies to the public who pay for it = bad. I have nothing but admiration for the hard working folks at the LHC and I sure hope that speculative science community didn't blow it for these types of experiments.

31. **Eye**
November 5, 2011

Sure, the web didn't come out of the blue and any given breakthrough, in technology, science or otherwise is stepped on the work of others. The crucial step forward for the web was taken at CERN as everybody knows.

Then, describing the output of LHC so far as "Just confirmation of what we already knew" is plain wrong. One example among many: so, you knew already the SM Higgs didn't weight 160 GeV? Hey, you should've told everybody!

You are being mislead by the likes of Woit or Dorigo who can't wait to bury Supersymmetry even if the surface of those theories has been barely scratched. Searching for new physics is a tough business and the attitude of some bloggers that should know better is just shameful.

32. **Peter Woit**
November 5, 2011

Eye,

"the surface of those theories has been barely scratched"

I understand the tactic of doubling-down on the hype when faced with failure, but don't think it's ultimately going to work out for the people trying it…

33. **Eye**
November 5, 2011

Peter, there is no tactic and there is no hype, at least in what I'm writing. I can understand that some people dislike some particular theory, for whatever the reason. But they should at least share with the rest some standards on what it means to check a theory against experiment in a serious way. You are not helping science when you say SUSY is dead, because nobody can help science by propagating falsehoods. I actually sympathize with your criticisms of those claiming string theory will be tested at the LHC (not with your conclusion that string theory is a failure) but you should be more careful when criticizing the hard work of many people (theorists and experimentalists) who are trying to test the best motivated models beyond the Standard Model, simply because you happen to dislike those proposals. That's not very serious and in my opinion is bad service to the popularization of the scientific endeavour.

34. **jpd**

November 5, 2011

and roger ebert must hate movies because he criticizes so many

35. **MP**
November 5, 2011

For now Woit and Dorigo are more right than string, SUSY, multiverse etc speculators. 30 years in and not a single shred of evidence or testability. Serious science can only be advanced when grounded in reality and experiments.

36. **Eye**
November 5, 2011

MP, to be "right for now" is very cheap. What do you guys propose as a meaningful search goal to the LHC experimentalists? Do you have any theoretical insight about what to expect for physics beyond the Standard Model? If not, maybe it would be smarter to shut up and let people do their job.

What does your 30 years time span mean? The greek were right about the atomic structure of matter and experimental confirmation had to wait for centuries. Who told you the pace of scientific discovery is fixed?

Why don't you stop whining and advance some positive idea?

37. **Peter Woit**
November 5, 2011

Eye,

The claim that for SUSY, "the surface of those theories has been barely scratched" is just hype. This has nothing to do with whether I 'like" SUSY models or not. You know very well that the main motivation given for low-energy SUSY, stabilization of the weak-scale, implies a SUSY-breaking scale low enough that it was hard to understand why SUSY effects were not being seen pre-LHC, and even more difficult now. The LEP + Tevatron + flavor physics + LHC results are not "a scratch on the surface" in any honest description of the current situation, they're a huge problem for the whole idea.

Besides the Higgs work, it IS hard to come up with well-motivated suggestions for what LHC searches should be looking for. I trust the experimentalists to have pretty good judgment about what the LHC + their detectors can do, and to work hard at viable analysis targets. Theorists of course have an important role to play, but I don't think they're doing anything helpful if they engage in hype, dramatically over-estimating the chances of extra dimensions, black holes, or superpartners being found.

As far as what the most useful thing physicists can do in terms of presenting the situation to the public, I think stopping the "LHC is looking for really cool stuff like extra dimensions, or super-this and super-that" hype would be a good idea, instead getting to work on explaining that ruling out a SM Higgs would not be a failure, but the most exciting thing to happen in high energy physics in more than 30 years.

38. **Eye**
November 5, 2011

Peter,
what's the LHC limit for neutralinos or for stops? We can discuss about how dead SUSY is after you tell me this.

"Besides the Higgs work, it IS hard to come up with well-motivated suggestions for what LHC searches should be looking for."

They need to know what to trigger on, don't they?

There is a similarity between overselling ideas, which I dislike as much as you and
overselling how bad the situation is, which you are very good at.

39. **Peter Woit**
November 5, 2011

Eye,

"They need to know what to trigger on, don't they"

Exactly. If a load of hype is being used to convince them to trigger on obscure SUSY signatures and dump everything else, that's a problem that deserves pointing out.

Sure, one can reasonably point out that ruling out gluinos and two-thirds of the squarks up to around 1 TeV still allows an stop at a mass worth looking for. However, describing this situation as "barely scratching the surface" is just absurd.

40. **Eric**
November 5, 2011

Peter,

It is not true that gluinos and two-thirds of squarks haven been ruled out up to 1 TeV. This is only true for certain benchmark points in the mSUGRA parameter space which feature large missing transverse energy signals. The reason these points were chosen as benchmarks is that they have well-defined signals that are easy to see. The majority of the SUSY parameter space is not the same as these benchmark points, and for these points in the parameter space it is much more difficult to identify a signal. So, the true statement is that most of the supersymmetry parameter space has not been tested yet. You should really please stop attempting to mislead your readers, although I am aware that you and Dorigo have been making a planned and concerted effort to do so.

41. **Peter Woit**
November 5, 2011

Eric,

The claim that the only thing ruled out is specially chosen benchmark points is just not true. See for example

http://www.slac.stanford.edu/th/lectures/RIZZO/CERN-Scans_8-31-11.pdf which reports that more than 90% of 2 million randomly chosen points in parameter space have now been ruled out.

42. **Bernhard**
November 5, 2011

Eric,

The majority of the SUSY parameter space is a really large thing, so sure, the LHC will have a hard time scanning it. But what Peter is saying is that for the part that was sold in the pre-LHC era as likely to show up and natural, this has been killed. One has to admit this is not a strach in the surface, this is a serious wound. That´s why you hear depressing comments from SUSY enthusiasts like John Ellis. Of course, for the die-hards (like you) the limits will have to be much stronger and that´s fine. But the limits Peter is quoting, however particular are a serious injury the idea of SUSY. It might still be correct but it got unquestionably less popular and less compared to pre-LHC era. If you have been in particle physics congresses (at least here in Europe) in the last months like I did, you for sure heard that from people who worked and were cheering for SUSY their whole carrier lifes.

43. **Eric**
November 5, 2011

Bernhard,

Sure, what was advertised in the pre-LHC era was the most optimistic possibility based on the simplest assumptions, namely universal soft supersymmetry breaking soft terms as in mSUGRA/CMSSM. Now, it may be true the this simplest possibility has been nearly ruled out, the majority of the supersymmetry parameter space has not. Obviously, the simplest version of supersymmetry with the cleanest and most identifiable signals were used to sell the idea of supersymmetry to those making decisions on funding etc.. However, just because this simplest and most highly optimistic possibility may not be correct does not invalidate or lessen the motivation for expecting superpartners to show up at the LHC. It simply means that it will be much more difficult to find them since the signals are not as clean. It may take some time to establish discovery, however in the next year we should expect at least a three-sigma Higgs signal in the region favored by the MSSM, and there should be anomalies such as trilepton events (which have already been seen) which are predicted by generic SUSY models.

@Peter: You should have a look at the comment section of the Resonaances blog.

44. **Eye**
November 6, 2011

Peter,
"If a load of hype is being used to convince them to trigger on obscure SUSY signatures and dump everything else, that's a problem that deserves pointing out."
It doesn't need to be pointed out: everyone involved in the searches knows the importance of this issue. Now, it's very cheap to call the best motivated BSM theory that has been proposed obscure or overhyped. By pointing this out without offering alternative BSM signatures or theories you're just being a pain in the ass.
So, experimentalists are eager to know what other alternatives they could be looking for. Please contribute to this if you have anything worth saying. Otherwise let more competent people do their jobs.

45. **Jim Rohlf**
November 6, 2011

The LHC triggers are dominated by electroweak interactions in order to look beyond the 100 mb strong-interaction garbage pail. These triggers are first-principle simple: single muon, single electron, single tau, MET, single photon, and then combinations of dileptons, diphotons, and then leptons plus jets, etc. The only reason to make combination triggers is to lower the thresholds, but these are still very generic. The LHC is pretty much a lepton and photon game. The exception is inclusive jet which was foreseen to be explored very early, indeed the jet spectra are very well measured out to a few TeV. The inclusive electroweak net catches W, Z, top, onia, rare B decays, etc. So there really is no "SUSY", "higgs", "black hole", "put your favorite theory here" trigger as these things are (very importantly) caught by generic triggers. All it costs us to make limits on SUSY, etc. is analysis manpower.

46. **Jim Rohlf**
November 6, 2011

BTW a nice what I would call "even handed" talk was given by Joe Lykken at the US LHC users organization on Friday, Nov. 4 on the status of SUSY at the LHC. It is available here:
https://twindico.hep.anl.gov/indico/getFile.py/access?contribId=46&resId=0&materialId=slides&confId=641
An interesting political issue is whether limit setting will be outsourced to theorists. It is currently being discussed by the experiments.
I have only ever heard 2 really motivational talks for SUSY: one by Steve Weinberg in the early 1980s and another by Ed Witten 7 years ago. They were enough to make a person believe that there might be something to it.
I personally think LEP took a very big byte out of SUSY. People are quick to forget this.

47. **emile**
November 6, 2011

For the non-specialists reading this blog:

The main motivation for the LHC (and the SSC) and the main selling point was that it was going to find something: either the Higgs, or if there is no Higgs, the new physics that prevents WW scattering from violating unitarity. A second motivation is that we could understand the new physics that solves the naturalness problem: if there is a Higgs, then we would like to know what allows the the Higgs mass to stay at the electroweak scale. If there is no Higgs, then we are back to the first motivation above. Some potential solutions to the naturalness problem have received a lot of publicity and some will have been over-hyped. One possible answer is that our world is fined-tuned (to some unknown amount) or maybe we just don't understand the naturalness problem (theoretical physics has bigger problems with the smallness of the cosmological constant). Bottomline is that the LHC was sold to find the Higgs or the physics that replaces it. It will do that. It was not built or funded to find extra dimensions.

P.S. to Rolf above: you forgot the incl. etmiss trigger

48. **SpearMarktheSecond**
November 6, 2011

Rizzo has posted a pretty good Higgs summary… don't know where he spoke…
http://www.slac.stanford.edu/th/lectures/RIZZO/BSM_Higgs.pdf

Hilarious that Lykken characterizes the need for good SUSY searches as a `fiduciary responsibility to the taxpayer'. Odd language, perhaps all the money spent on theorists salaries is the taxpayer investment, and if there is no Higgs, no SUSY, at the LHC, that would be the financial loss like a Greek bond.

But among experimentalists, merely getting good luminosity at the high energy frontier has always been the point. Mel Schwartz was always the most direct about that, and he was right. Promising great discoveries is and was hype, but getting the machine and experiments built always has great value: without experiments, we would never advance.

Jim, you've heard 2 more inspirational SUSY talks than I ever have. I lost it at the complexities introduced to suppress flavor changing.

Why not outsource limit setting to theorists? If SUSY is present and important, it should not be so subtle to discover. I doubt, however, that theorists and phenomenologists will ever be convinced by mere experimentalists, so let them dig through the data (after the straightforward analyses have been done) and convince themselves.

49. **Peter Woit**
November 6, 2011

Jim Rohlf,

Thanks for the reassuring comments about triggers…

I think the Witten talk Jim was referring to might have been something like this one:

http://conferences.fnal.gov/lp2003/program/papers/witten.pdf

while Witten is quite fond of SUSY, he's careful there to explain the problems.

50. **Jim Rohlf**
November 6, 2011

I don't think Lykken meant that we have a fiduciary responsibility to make a discovery but rather that we have such responsibility to extract as much as we can quantitatively about SUSY and other limits. I hate setting limits. Nobody will ever care that we did not see something and the only worthwhile thing about not seeing something is to help tell us where to look next. I agree there is absolutely no possibility that a theorist could discover SUSY because the signatures are not subtle.

I agree totally with your comment about Mel Schwartz. We used to joke that our friends Jack and Leon may never get the prize because then they would have to give it to Mel too, which was thought to be politically incorrect because he left the field, unlike the other 2 who made sustained, important, high-visibility contributions throughout their careers. They had to wait a long time, from 1962 to 1988, considering the central importance of the result (2 neutrinos). It was a great prize. Whenever I teach particle physics, I always spend a whole class going over the 1960 paper of Schwartz on how to make a neutrino beam with enough intensity to study the weak interaction. The actual beam made in 1962 was very much according to the vision of Schwartz. Of course, Bruno Pontecorvo also had the idea but was not in a position to do the experiment. It is absolutely amazing that from conception to construction to discovery took only 2 years. This was as far as I can tell in a large part due to the drive of Schwartz.

51. **Jim Rohlf**
November 6, 2011

emile:
MET = missing transverse energy

52. **Jim Rohlf**
November 6, 2011

emile:
MET = missing transverse energy

Peter:
It was this one
https://indico.cern.ch/getFile.py/access?contribId=s7t2&sessionId=s7&resId=0&materialId=0&confId=a041794

53. **Bernhard**
November 6, 2011

Jim,

This is a protected link.

54. **A.**
November 7, 2011

I seem to have come late to this party. I'm working on ELI stuff. I'm shocked by how bad the Telegraph article is and I agree with some comments above that "we" are not doing ourselves any favours by promoting the machine in this way.

ELI will not get closer than two orders of magnitude *below* the Schwinger limit (at least not for the foreseeable future). This doesn't mean that there is no chance of seeing the Schwinger effect, but it is not the first experiment they'll be doing. I think the attosecond science, medical imaging, radiography cases for ELI are perhaps stronger, but that's not my area and it seems the higher-ups want to keep the "fundamental physics" aspects in the public eye, and that seems to require this ridiculous sensationalism. Quite how committed they are to doing QED experiments… well, time will tell.

Also depressing is the list of comments in the Telegraph article which reveals both the woeful levels of scientific education in the UK and the abstract terror which such press releases can engender in the public eye. People are actually _scared_ that ELI will kill us all. Of course you can't stand up and say "oh come on, it's just a big laser pointer" without jeopardising both your funding and your prospects in the field.

55. **Bernhard**
November 7, 2011

Off-topic, but fresh hype:

http://www.newscientist.com/article/mg21228374.500-nuclear-clock-could-steal-atomic-clocks-crown.html

"Such clocks could shed light on string theory. The frequency of the jumps in a nuclear clock will depend on the strong nuclear force, while the jumps by electrons in atomic clocks depend on a different fundamental force. So together they could reveal if the relative strength of the forces changes, as string theory has it."

Interview With Sidney Coleman

November 4, 2011
Categories: Uncategorized

Via [Steve Hsu], at the AIP Center for History of Physics, there's a transcript of an [interview with Sidney Coleman] from 1977. It's provocative and amusing, like the man himself, as well as informative history. Go and read the whole thing, but here are some excerpts:

> **But you do enjoy working with students or do you?**
>
> No. I hate it. You do it as part of the job. Well, that's of course false…or maybe more true than false when I say I hate it. Occasionally there's a student who is a joy to work with. But I certainly would be just as happy if I had no graduate students…
>
> **I guess your remark means then that you would like to avoid teaching undergraduate courses or even required graduate courses…**
>
> Or even special topics courses. Teaching is unpleasant work. No question about it. It has its rewards. One feels happy about having a job well done. Washing the dishes, waxing the floors (things I also do on a regular basis since I'm a bachelor) have their rewards. I am pleased when I have done a good job waxing the floor and I've taken an enormous pile of dirty dishes and reduced them to sparkling clean ones. On the other hand, if I didn't have to, I would never engage in waxing the floors, although I'm good at it. I'm also good at teaching; I'm considered very good at teaching, both by myself and others. And I'm also terrifically good at washing dishes, in fact. On the other hand, I certainly would never make a bunch of dirty dishes just for the joy of washing them and I would not teach a course just for the joy of teaching a course…
>
> **So I guess really you would be happier with the format of an institute of theoretical physics? Rather than a teaching institution like a university?**
>
> Well no. That makes it too abstract. Because that means, would you like to have a position at, say, the Institute for Advanced Studies? And then all sorts of other things would enter the picture. Like you'd have to live in Princeton which is truly an awful experience.
>
> **I was there as a young bride a long time ago.**
>
> Young brides get the worst of it. They're even worse off than the people who are at the University or the Institute because at least the people at the University or the Institute can fill their days by engaging in their professional interests from the moment they wake up until the moment they go to sleep. But if you don't have that, there is really nothing. Nothing. It's a terrible place. Dullest place in the world. No I wouldn't say that, but certainly the dullest place at which decent science or decent scholarship is done in the world today. The only advantage to Princeton is that it's close to Princeton Junction.

Personally I'm quite glad that Harvard was wealthy enough to support Coleman, while forcing him to teach, since I benefited quite a bit from his teaching, right around the time of this interview. I had heard before that he didn't enjoy his time in Princeton. Once at lunch at the IAS, a senior physicist visiting there told me about the advice Coleman had given him when he told him he was going to Princeton for a year. The advice was "Be sure to bring with you everything you need." The senior physicist then told me that: "I recently realized what Sidney was trying to tell me: there are no women here so I should have brought someone with me…"

Comments

1. **erice**
   November 4, 2011

   Taking all of the above at face value, one wonders why Sidney taught at the Erice summer schools (some of the most brilliant lectures they have had). Harvard could force him to lecture at Harvard, but nobody could force him to lecture at Erice. Zichichi must have had fantastic powers of persuasion.

2. **Kea**
   November 4, 2011

   I was in Princeton for a few months in '96 (studying mathematics) and I loved it precisely because it was so quiet. One can sit at lunch at the IAS without being bothered by passing traffic, work in a quiet space, and watch squirrels in peace. I have never seen a better working environment anywhere.

3. **Bob Levine**
   November 4, 2011

   Coleman's dislike of teaching reminds me of something that the great G. H. Hardy said in his A Mathematician's Apology: that he disliked teaching but loved lecturing. I suspect that quite a few of the professoriate, if they were being completely candid, would have something similar to say.

4. **Peren Woondeen**
   November 4, 2011

   I would happily wash dishes and wax floors for a one-week trip to Erice, all expenses paid. Sicily is a beautiful place, you know.

5. **Shantanu**
   November 4, 2011

   Peter, thanks for the link. very nice interview. Btw do (or anyone else) know whether Harvard think of forming a relativity group during the heydays in 70s?
   Do you recollect the attitude of Harvard folks in 70s towards relativity?

6. **Chris Oakley**
   November 4, 2011

   Erice was rumoured to have connections with the Mafia. Maybe they threatened to kneecap Coleman if he refused to give lectures there.

7. **Thomas R Love**
   November 4, 2011

   I sincerely doubt that Princeton is the "Dullest place in the world", outside the Lab, it has to be Los Alamos.

8. **Peter Woit**

November 4, 2011

Shantanu,

When I was there (75-79), the November revolution had just happened and the Standard Model was becoming widely accepted, with new confirming results coming in regularly. The investigation of QCD had really just begun, with people trying to extract as much from perturbation theory as possible (jets, charmonium), and beginning to try to understand its non-perturbative behavior. With all of these dramatic developments, gravity was not something that many people were thinking about. I'm sure there was no discussion of starting a new relativity group then. Elsewhere, supergravity was getting off the ground. Lee Smolin was a graduate student, working on gravity, nominally with Coleman, but I think mostly with Stanley Deser at Brandeis. I vaguely remember that he discusses this period in his book.

9. **Franklin Chen**
November 5, 2011

I took Quantum Field Theory from Sidney Coleman way back. I remember how he had planned his lectures down to the jokes. I also remember how he would sometimes respond to students in the class, "Ask a stupid question, get a stupid answer."

10. **Edward Pickman Derby**
November 5, 2011

I don't get it; if Princeton is such a sh*t hole then why would Witten, who could get a lucrative job at any university in the world at a moment's notice, choose to work there?

11. **Richard**
November 5, 2011

It's always worth reminding people of the the splendid Coleman [QFT lecture notes] from 1985, transcribed from [Brian Hill]'s records.

12. **MathPhys**
November 5, 2011

I knew Coleman a bit and from the little that I knew, Princeton was too quiet for him.

13. **Jim Rohlf**
November 5, 2011

I knew Sidney pretty well when I was at Harvard. Two of the funniest things I ever heard him say were:
1) at a poker game in the middle of the night in Aspen (keep in mind we played poker often in Cambridge) a prominent Cornell physicist exclaimed after as I recall was a very strange situation, "Professor Coleman why did you fold?" and Sidney without a missing beat said "I don't know, I'm a poker player not an analyst."
2) for those that know David Cline (also a friend of mine) who was calculated to travel at an average speed of 43 mph because he flew so much, in a chance encounter of the 3 of us at the CERN cafeteria, I said to Sydney, "You've met Dave Cline, I presume."
Coleman: "No. The flux is large but the cross section is very small."

14. **MathPhys**
November 6, 2011

I was told by no less authority than (okay, let's not get into dropping names) that Sidney C did not know a word of French, but he was an expert on reading the menu in French restaurants.

15. **M. Wang**
November 7, 2011

My favorite story about Sidney is that when he was asked to give a talk at 10am, he replied, "I don't think I can stay up that late."

16. **MathPhys**
November 7, 2011

When lecturing on magnetic monoploes, Sidney would make a circle with the thumb and index finger of one hand to represent closed magnetic flux lines, use the index finger of the other hand to represent charges flowing through it, then look at the audience and say "I shouldn't be making these obscene gestures". He was great fun.

17. **chris**
November 7, 2011

M Wang,

that was Pauli to Heissenberg…

18. **JadedIdealist**
November 7, 2011

A recent newscientist article argued (well enough to give the idea credit in my eyes) that part of the reason for the backlash against science funding in the american right wing was due to poor engagement with a) the public and especially b) the rich – ironically due in part to secure science funding.

This interview fits right in with that 70's zeitgeist of "we don't want to and don't need to talk to ignorant people", I hope we can see now that in the long run that attitude is a mistake.

19. **Steve Christensen**
November 7, 2011

I was at Harvard 1977-79 in Physics and at the Center for Astrophysics. Also there doing relativity related work were Bill Press and Larry Smarr. Most of the other gravity related work was being done by Mike Duff, Stan Deser, Marc Grisaru, Burt Ovrut, and others at Brandeis where I spend most of my time working with Mike. My discussions with Sidney at the time were all about science fiction due to his writings in Fantasy and Science Fiction magazine.

20. **Chris Oakley**
November 7, 2011

> This interview fits right in with that 70′s zeitgeist of "we don't want to and don't need to talk to ignorant people", I hope we can see now that in the long run that attitude is a mistake.

Sure, but if, when journalists telephone, scientists start prattling on about extra dimensions, supersymmetry, wormholes, parallel universes, strings and other stuff that has no experimental support whatever then *they* are the ignorant people.

21. **Patrice Ayme**
November 8, 2011

JadedIdealist and Oakley have it exactly right. The most important function of scientists is not to pose as if they had made great strides in magic, but to explain humbly and thoroughly real science, and how it progressed through more exact thinking, and the general interest this has for society, thus insuring the social support of science and the powers that be, for a long time to come.
Certainly presenting as facts fancy flights of the imagination is long term disastrous for the support of thinking by this civilization. Because when those turn out completely silly, scientists, and, therefore science, lose all credibility. We should be modest, because our theories are modest, and so doing other decision makers can take heed.

22. **aa**
November 8, 2011

re the staying up that late joke,
i have heard that attributed to Maxwell

"Aye, I suppose I could stay up that late."
Maxwell, on being told on his arrival at Cambridge University that there would be a compulsory 6 a.m. church service, as quoted in Spice in Science : The Best of Science Funnies (2006) by K. Krishna Murty

23. **Chris Austin**
November 8, 2011

The Sidney Coleman lecture notes taken by Brian Hill are now available on arXiv.

24. **Thomas**
November 9, 2011

Yes, and the abstract points to these wonderful , where Coleman looks rather happy and completely at ease. Oh my what a chain smoker.

25. **Pich Hambroy**
November 9, 2011

> The Sidney Coleman lecture notes taken by Brian Hill are now available on arXiv.

I think I know who Sidney Coleman is, having read his book (Aspects of symmetry) and several of his papers. But who is this guy Brian Hill who has been mentioned already twice in this thread? What is he famous for?

26. **Peter Woit**
November 9, 2011

Pich Hambroy,

In this context, he's famous for writing up notes for the Coleman course…

But, for more about him, you can try

http://www.linkedin.com/in/brianhill

27. **John Baez**
November 14, 2011

> "Washing the dishes, waxing the floors (things I also do on a regular basis since I'm a bachelor".

And if he weren't, he wouldn't? Interesting assumption about the natural order of things.

Extortion

November 6, 2011
Categories: Uncategorized

An explanation of something that readers of this and other related blogs may have run into recently:

An ex-boyfriend of a close personal friend of mine has been posting here and elsewhere repulsive and delusional material about the two of us. A few weeks ago he wrote to her to demand money, telling her that if she didn't pay up he would go on a campaign to broadcast information discreditable to me and embarrassing to her. She didn't pay him and what you may have seen is the result of him making good on his threat.

I've never met this person, and his accusations are simply untrue. If you're owner of a blog where this is posted, please delete this material, although it might be a good idea to save a copy in case it becomes evidence in a criminal case. Otherwise please just ignore it completely, unless you'd like to help me out by contacting the blog owner to tell them what is going on and ask them to delete it. Thanks.

Note added: I appreciate the intention, but please do not post comments or advice about this here on the blog.

No Comments

This Week’s Hype

November 7, 2011
Categories: This Week's Hype

A couple people have pointed me to an article at *New Scientist* that requires another edition of This Week’s Hype. According to Nuclear clock could steal atomic clock’s crown:

> Such clocks could shed light on string theory. The frequency of the jumps in a nuclear clock will depend on the strong nuclear force, while the jumps by electrons in atomic clocks depend on a different fundamental force. So together they could reveal if the relative strength of the forces changes, as string theory has it.

The amusing thing about the string theory “prediction” that relative strengths of forces change over time is that many string theorists promote as a “prediction” exactly the opposite: according to this argument, change in these strengths would imply large changes in the vacuum energy, which we don’t see, so a prediction of the landscape is constancy. See for example here.

Comments

1. **Carl**
   November 7, 2011

   “It’s not that this experiment contradicts string theory, it’s just that you’re looking at the wrong universe.” — All string theory defenders, ever.

2. **Bernhard**
   November 7, 2011

   Peter,

   Not sure I follow. Why would the landscape imply that? Perhaps a bit mathematically formulated would be easier to understand.

3. **Peter Woit**
   November 7, 2011

   Bernhard,

   You’d have to ask Michael Douglas for the details of the argument, it’s his. But I gather the idea is just based on the fact that your changing couplings and your vacuum energy are both determined by the details of the potential function for moduli fields. If your universe moves in the moduli parameters, the energy will also change, on a scale that ruins your extremely low, anthropically tuned CC.

4. **SpearMarktheSecond**
   November 8, 2011

   Hmmm… doing the highest possible precision test of the time-stability of the strong interaction strength relative to electromagnetic strength is simply a good idea!

   Don’t need a string theorist or string theory to know which way the wind blows in this case. Sadly, though, experimental groups feel compelled (maybe even justifiably) to tie what are excellent ideas to the latest fashion.

   I guess Columbus had to argue that he’d improve trade to Japan in 1490 or so too.

   It is rather remarkable to have a low-lying nuclear level that is accessible with UV or optical photons. Never knew that before.

   Now out there somewhere in the literature are studies, I’m sure, of the time stability of the mass of nuclei/atoms measured with electromagnetic mass spectrometers. Another test of the time-constancy of strong/em. Mass spectrometry is darned sensitive, but in the end optical methods probably win in sensitivity.

   Good for these guys!

Higgs Rumor Roundup

November 10, 2011
Categories: Experimental HEP News

As far as I've been able to tell, there's still nothing definitive one way or the other about the SM Higgs, as the experiments continue to analyze data from the now-finished 2011 pp run. Starting Monday is the HCP 2011 conference which at one point seemed to be a possible venue for announcement of confirmation of hints from early this summer of a Higgs around 140 GeV or so. Those hints disappeared later in the summer, so conventional wisdom recently has been that not much new will come out next week in Paris. A new blog entry from one of the organizers refers to this disappointment, leading to worries about conference attendance, but adds some dramatic and mysterious news at the end. It seems that some experimental collaboration requested a last-minute slot at the conference to unveil a new result that might be the highlight of the conference. They're on for 15 minutes on Monday, still not announced which collaboration this is, who the speaker is, or what their title is. This may very well have nothing to do with the Higgs: maybe something else travels faster than the speed of light…

At HCP2011, ATLAS will have new results from the H->ZZ->llnunu channel (already released, see here) and from the H->ZZ->llqq channel. Unfortunately neither of these are relevant to the low mass region where the Higgs is believed to be hiding. I don't know what CMS has up its sleeve. One other thing that will be released is the combination of ATLAS and CMS summer conference data, which will exclude the Higgs at 95% confidence level from 141-476 GeV (and come very close to this exclusion down to about 135 GeV).

It looks like release of new data in the channels that are sensitive to a low mass Higgs will wait longer, until the experiments have had a chance to do some analysis on the entire 2011 data set. Mid-December has been rumored as a date, and a logical bet would be that the CERN Council week would be the time for this, in particular at the Scientific Policy Committee meeting on December 12-13. Rumors going around about this are that there's still nothing definitive in the crucial H->gammagamma channel, and that in the H->ZZ->llll "golden channel" (very low background), one experiment is seeing no excess at low mass, the other is seeing an excess. Higher quality, better informed rumors are encouraged…

Nature reports here on the discussion at CERN about what to do in the (wonderfully exciting…) event that the SM Higgs is not seen. The first action they're taking is semantic: if no Higgs is seen at the 95% confidence level, instead of saying that this "excludes" the Higgs, they will announce that it "disfavours" it. So, the first reaction will not be jumping for joy, but a defensive one about how it might still be there.

Whatever comes out in December, I hear the plan is to wait for the Moriond conference at the beginning of March to release a result combining data from all channels (separately for each experiment). The Tevatron may have a result to release then too. Philip Gibbs will then swing into action for the full combination.

Update: Via Philip Gibbs, this talk includes the information that "The CERN DG has requested updates for December council". Not clear to me if the plan is to release these publicly, or to try and keep them confidential (which may not be easy…).

Update: The mystery new result is from LHCb, a 3.5 sigma observation of CP violation in an unexpected place. Details here, Jester has analysis here.

Comments

1. **this conference's hype**
   November 10, 2011

   "A new blog entry from one of the organizers refers to this disappointment, leading to worries about conference attendance, but adds some dramatic and mysterious news at the end."

   There is your answer in a nutshell: "… worries about conference attendance, …" And so it is a self-fulfilling prophesy that conference organizers **must** promise the prospective audience some sensational news. "… but adds some dramatic and mysterious news at the end." Et voila!

   Don't blame string theory for any of this. And don't restrict the responsibility for this state of affairs to HEP.

2. **Coin**
   November 10, 2011

   "Higher quality, better informed rumors are encouraged…"

   In other words you are looking for something like a 5-sigma rumor to distinguish the event from the noise floor? Or if we collect a sufficient number of 3-sigma rumors is that adequate…

3. **emile**
   November 10, 2011

   I think rumors and hype are not so distant cousins… There is a problem with honesty in hyping results or spreading false rumors. If someone if spreading "true rumors", you need to realize that that person has agreed not to discuss unapproved results but is doing it anyway after saying that they would not do that. This is not about whether collaborations should or should not impose a ban on discussions of internal results. Simply put: if you agree to something, abide by it. Don't encourage hype and don't encourage the spreading of rumors. Science does not profit from either.

4. **icebug**
   November 10, 2011

   Apologies for OT comment, but I just received an email about this new particle physics textbook from Princeton, purporting to be "a cutting-edge introduction to the field, preparing first-year graduate students and advanced undergraduates to understand and work in LHC physics at the dawn of what promises to be an era of experimental and theoretical breakthroughs." Do you have any thoughts on the timing? I suppose it is timely in one sense, but I wonder how they decided how much speculation/prognosticating to put in or leave out…

5. **Peter Woit**
   November 10, 2011

   icebug,

   That's a serious book, from the experimental view-point, with only a minimal level of theoretical speculation. The LHC will be operating for many years, and most of what's in the book will continue to be relevant. The author has chosen to go ahead and publish even though some of the material may be out of date soon. Nothing wrong with that, if a field is moving fast, doesn't mean people shouldn't try and write textbooks about it.

6. **Peter Woit**
November 10, 2011

emile,

I won't get into the debate about discussing rumors about unapproved results, other than to note that I try to do so accurately, and only when I have some good reason to believe the rumor (note that information leaks out in many ways…). I don't think this damages the progress of science.

Also note that much of the information in this posting that may be confidential is not unapproved analysis of data. As far as I know, this summer's ATLAS/CMS combination is approved and final. Sometimes people have results that have gone through the proper vetting procedure, and they're just keeping them confidential for non-scientific reasons, e.g. waiting for some conference or press release. Other information in this posting is rumor/speculation about tentative scheduling of future releases of data, and I actually don't see any good reason for that to be confidential.

7. **M**
November 10, 2011

…or maybe something else violates CP at 3.5 sigma level.

8. **Greg**
November 10, 2011

It seems that some experimental collaboration requested a last-minute slot at the conference to unveil a new result that might be the highlight of the conference. They're on for 15 minutes on Monday, still not announced which collaboration this is, who the speaker is, or what their title is.

…

W mass from CDF?

9. **Jester**
November 11, 2011

According to my sources, it's going to be the LHCb measurement of a large CP asymmetry difference between D -> K K and D -> pi pi decays.

10. **M**
November 11, 2011

Jester, your sources are right. Can we start discussing what is the Standard Model expectation for that?

11. **David Brown**
November 12, 2011

Do the CERN physicists have a Higgs betting pool?

12. **Nige Cook**
November 12, 2011

"The first action they're taking is semantic: if no Higgs is seen at the 95% confidence level, instead of saying that this "excludes" the Higgs, they will announce that it "disfavours" it."

How far will they have to go to "exclude" it? Is it just that they want perpetual funding to go on searching and have job security forever? Not only does the Higgs not predict a certain energy, but the LHC experiments can't definitely exclude it?? If these people were searching for ESP phenomena, would they be similarly cautious?

13. **Peter Woit**
November 12, 2011

Nige,

I think they want to reserve the word "exclusion" for a higher standard than 95% sure it's not there, e.g. 5 sigma. In terms of communicating with the public, this is not unreasonable, since in this business analysis of systematic errors is tricky and "95% sure" has been known to be wrong more often than expected. However, "disfavored" sounds to me misleadingly weak…

In any case, I'm sure they'll keep reporting 95% results, whatever they call them, and if the SM Higgs is excluded at that level, most physicists will start assuming it's quite likely that it's not there.

14. **cc**
November 13, 2011

Indeed non-leptonic D decay seems to be it. Difficult to think of a tougher nut to crack, re. a decently clean SM prediction.

What about the Dec 16 CERN announcement buzz, though?

15. **Neo**
November 13, 2011

I am not a physicist, just an interested observer. What impresses me is that if the SM Higgs is not found, there appears no particular direction to go. Yes, the kluge-assisted standard model is wrong, but so what? Technicolor seems doomed. String theory can retreat to any level of energy to avoid being refuted. What to do?

16. **Peter Woit**
November 13, 2011

neo,

A situation where there's no good theoretical idea, and experimentalists are hard at work producing results that might provide important hints sounds to me like a pretty good situation for a theorist. Far preferable to the LHC data just confirming the SM or some other well-known theoretical model. If the SM Higgs doesn't show up, for the first time you'll have an experimental result in radical disagreement with the SM, and that's what we need in order to start figuring out how to do better than the SM.

17. **Peter Woit**
November 14, 2011

cc,

What is the "Dec. 16 CERN announcement buzz"? Does this have to do with a plan to publicly update the Higgs search results at the CERN Council week?

18. **CPV**
November 14, 2011

Direct link to the HCP talk about CPV in the charm sector at LHCb.

19. **Shantanu**
November 15, 2011

Peter, or anyone else:
Do you know what happened about the muon (g-2) anomaly. what's the latest
theoretical and experimental consensus?

20. **BJM**
November 17, 2011

I remember when it was exciting to think that a final theory was within reach. Now it's exciting to think that perhaps everything we know is wrong and we need to start over again. Sheesh!

Models.Behaving.Badly

November 14, 2011
Categories: Book Reviews

Emanuel Derman has a fascinating new book out, Models.Behaving.Badly, which I've been intending to write about here for a while now. One of the problems that has kept delaying this is that every time I start to write something I notice that a new review of the book is out, and it seems to me quite a bit better than anything I have to say. An example that comes to mind is this review from Cathy O'Neil at mathbabe a while ago, another is a new one out today at Forbes. Any book that attracts so many thoughtful reviews has to be worth reading as well as capturing something important about what is going on in the world.

There is a somewhat frivolous virtue of the book that I haven't noticed other reviews discussing: the wonderful title. I can't help but enjoy the double meaning linking two of the out-of-control groups that are features of downtown life here in New York. One might think that drunken fashion models in downtown clubs and quants at financial firms don't have a lot to do with each other, and yet…

Derman's first book, My Life as a Quant (which I wrote a little bit about here in the early days of this blog) tells the story of his move from high energy theory into financial modeling. He was one of the first to do this, and that book reflects a time when the financial industry was riding high, with the role of quants and their models relatively uncontroversial. With his new book, he's also one of the first, this time in his disillusioned but serious look from the inside at where we are today:

> I am deeply disillusioned by the West's response to the recent financial crisis. Though chance doesn't treat everyone fairly, what makes the intrinsic brutalities of capitalism tolerable is the principle that links risks and return: if you want to have a shot at the up side, you must be willing to suffer the down. In the past few years that principle has been violated.

His focus is on the role that models inspired by physics have played in this debacle, arguing that they have been used in a fundamentally misconceived way, and explaining the evolution of his own understanding:

> … I began to believe it was possible to apply the methods of physics successfully to economics and finance, perhaps even to build a grand unified theory of securities.
>
> After twenty years on Wall Street I'm a disbeliever. The similarity of physics and finance lies more in their syntax than their semantics. In physics you're playing against God, and He doesn't change His laws very often. In finance you're playing against God's creatures, agents who value assets based on their ephemeral opinions. The truth therefore is that there is no grand unified **theory** of everything in finance. There are only **models** of specific things.

Much of the book is devoted to explicating his views about the importance of distinguishing between "theories" that are supposed to accurately capture phenomena, and "models", which are metaphors which which only approximately capture some aspects of phenomena. As examples of theories, he discusses not just QED, the quintessential accurate theory of the physical world, but also Spinoza's theory of the emotions. Besides the financial models that are the focus of the book, he also covers a wide range of other failed models. The book begins with a short memoir of growing up in South Africa, where he was a member of a Zionist youth organization, and its failed models as well as the racial ones of apartheid played a role in his coming of age.

If you're part of the modern world which generally finds actual books too long and time-consuming to read, Derman has an often enjoyable blog, and for the truly ADD-afflicted, he also has one of the very few twitter feeds I've seen worth following.

Comments

1. **Tim van Beek**
   November 14, 2011

   FWIW I faced the career choice ten years ago of wether I should try to get a PhD, become a quant or do something entirely different (choosing the latter). After working on models in classical statistical physics, the necessary mathematical machinery for doing mathematical finance isn't hard to learn.

   But I was and am convinced that while the abstraction of fast varying degrees of freedom of 10^23 particles following static rules of dynamics may be of some value, transferring the modelling techniques to 10^4 or 10^5 interacting humans can lead to catastrophic failure only.

   Several years later, in about 2005, I very much regretted my career choice, while watching the financial markets and the earnings of quants. In 2008 I was very much surprised how much I did not regret my choice then.

   Meanwhile I start to regret the choice again, as I see how virtually all quants continue their work, and get a lot of money for it, after causing a global economic meltdown. I wonder what will happen during the next catastrophy, which almost surely will take place within the next decade (I know this, I have a sophisticated model) 🙂

2. **M. Wang**
   November 14, 2011

   I first met Dr. Derman nearly 20 years. I always considered him lucky in the sense that his job was quite academic by bank standard and he did not have to get his hands dirty as often as traders had to do.

   _
   I am glad that he has also reached the conclusion that I believe every financial professional with a conscience should have suspected by 2000 and be thoroughly convinced by 2008: Modern finance as practiced in the US is a game rigged for the powerful insiders. Contrary to what business schools have trumpeted over the years, no high level personnel in investment banks care a bit about optimal allocation of resources. The biggest rewards inevitably go to those who are the most nasty and shameless in ripping off first their customers and later the taxpayers.

   _
   While this kleptocratic arrangement goes all the way to every branch of government and therefore is a much bigger problem than finance alone, it was made significantly worse by the willing and enthusiastic participation of the financial academia. It is so easy and common to buy off a few big-shot professors and have them pump out fact-warping papers to push for more deregulation or excuse blatant thefts of public goods, all without the slight bit of disclosure, that even when caught red-handed, no one will suffer any damage either legally or in reputation.

   _
   All this was a little hard to stomach for an ex-physicist like myself. I have escaped a field dominated by the nonsense that was string theory only to land among an immense group of shameless thieves. Although the string theorists have wasted 30 years of public resources pursuing an increasing pointless venue, the waste pales completely in comparison to the willful and wide-spread wrong-doings by financial academics and practitioners and their incredible damage to this nation and the world.

   _
   Those of you who had the fortune of not having to bear witness to the avarice and debauchery prevalent in the US banking industry of the past decade, consider yourself

blessed.

3. **Visitor**
November 14, 2011

I wonder if is Derman as sanctimonious as the quotes make him appear to be – because he sounds *very* sanctimonious indeed. (i.e. He is full of mea culpas but will be keeping the money.)

4. **M. Wang**
November 14, 2011

Why do you think Derman should give up his salary? To everyone's best knowledge, he has never participated in ripping off clients or the taxpayers. He was hired to produce research papers on pricing options and he did a decent job of it.

Thousands of string theorists have produced no results relevant to this universe. Don't they owe the public their salary back?

5. **Chris W.**
November 14, 2011

So, Visitor, if you observed the corruption from the inside, you're culpable like everyone else, so you should STFU.

On the other hand, if you observed it from the outside, you're ignorant and naive about how finance works, so you should STFU.

Enough venting on accusations of sanctimoniousness. On the question of linking risks and return, the real problem is that the risk takers can put other people—sometimes millions of other people—at risk besides themselves. When risk-taking is allowed to become systemic in that sense, it becomes very hard to just say "let 'em burn". Of course, the risk takers *wanted* to pull as many suckers into the game as possible, and they convinced our elected representatives to help them do it—not that some of them needed much convincing.

If a gambler wants to indulge his proclivities, let him do so, but require that he not have any dependents. The goal is to protect the dependents—in particular, from being held hostage to ensure a bailout—and not to punish the gambler, who will be punished by his bad bets.

The point is all pretty much moot where the U.S. government is concerned. The 2008 bailouts were a one-shot deal; we can't possibly afford to do them again. If we forget that, I think Japan and China (and much of Europe) will remind us.

6. **Peter Woit**
November 14, 2011

Sorry folks, but I'm not about to try and moderate an open forum for everyone to explain what they think about economics or the financial system. If it's not about Derman's book, it's off-topic here. There are lots of blogs out there devoted to economics and finance in general, try one of them.

7. **Yatima**
November 14, 2011

Young ones finding back the wisdom of those who went before?

As Ludwig von Mises said in The Ultimate Foundation of Economic Science

The study of economics has been again and again led astray by the vain idea that economics must proceed according to the pattern of other sciences. The mischief done by such misconstructions cannot be avoided by admonishing the economist to stop casting longing glances upon other fields of knowledge or even to ignore them entirely. Ignorance, whatever subject it may concern, is in no case a quality that could be useful in the search for truth. What is needed to prevent a scholar from garbling economic studies by resorting to the methods of mathematics, physics, biology, history or jurisprudence is not slighting and neglecting these sciences, but, on the contrary, trying to comprehend and to master them. He who wants to achieve anything in praxeology must be conversant with mathematics, physics, biology, history, and jurisprudence, lest he confuse the tasks and the methods of the theory of human action with the tasks and the methods of any of these other branches of knowledge. What was wrong with the various Historical Schools of economics was first of all that their adepts were merely dilettantes in the field of history. No competent mathematician can fail to see through the fundamental fallacies of all varieties of what is called mathematical economics and especially of econometrics. No biologist was ever fooled by the rather amateurish organicism of such authors as Paul de Lilienfeld.

When I once expressed this opinion in a lecture, a young man in the audience objected. "You are asking too much of an economist," he observed; "nobody can force me to employ my time in studying all these sciences." My answer was: "Nobody asks or forces you to become an economist."

8. **David Brown**
November 15, 2011

According to Derman, "In finance, you can't predict the future to even one decimal place."
http://www.youtube.com/watch?v=PnK3CKtuL_k Emanuel Derman: Valuation and Its Discontents

9. **Bernhard**
November 15, 2011

"As examples of theories, he discusses not just QED, the quintessential accurate theory of the physical world"

I almost feel bad about making this comment since it is on QED, which is as far as I know the most successful thing ever made IMHO in physics, but isn't fair to say QED is a model based on a theory (QFT) just as the SM, just as SM extensions…?

10. **Christopher Faille**
November 15, 2011

Allow me to advertise my own review of Derman's fascinating book here:

http://allaboutalpha.com/blog/2011/11/10/derman-why-all-models-are-toys/

Thanks.

11. **Peter Woit**

November 15, 2011

Bernhard,

I think Derman is trying to make a useful distinction, and using the terms “theory” and “model” to name it. The same terms do get used in different ways though. For instance, “string theory” refers to all sorts of very different things. The Standard “Model” is the name given to the most extreme case of a “theory” we have, in Derman’s usage. The general question of how mathematical structures like QED and the SM relate to the “real” world is a very interesting and intricate one, but I don’t think that’s what Derman is trying to get at. It seems to me that he’s mainly interested in examining why the models used in finance are of a different nature than things like the Standard Model.

12. **Suraj**
December 3, 2011

@Bernhard.. “As examples of theories, he discusses not just QED, the quintessential accurate theory of the physical world”

Thanks for this.

Dijkgraaf Next Director of the Institute for Advanced Study

November 14, 2011
Categories: Uncategorized

The IAS in Princeton announced today that Robbert Dijkgraaf will take over from Peter Goddard as director starting next summer.

Like Goddard, Dijkgraaf has devoted much of his career to string theory, more specifically the formal side of the subject, including conformal quantum field theories, topological quantum field theories, and their manifold interesting relationships to mathematical issues. Unlike Goddard, he's from a later generation, getting his Ph.D. in 1989 and entering theoretical physics after string theory had begun to play a dominant role. His cohort of theorists who entered the subject as Young Turk revolutionaries riding the wave of string theory is now settling into the role of Grand Old Men.

Dijkgraaf is known as a masterful expositor, with pretty much any survey article by him you can find sure to be lucid and very much worth reading. He also has world-class political skills, recently overseeing the review of the IPCC, a topic putting him at the center of the religious war over climate change. His background makes him an ideal choice to lead an institution like the IAS, one with a great history in theoretical physics and mathematics, and an important ongoing role to play in keeping those subjects healthy.

Comments

1. **Marcus**
   November 15, 2011

   Here is a video
   http://loops05.aei.mpg.de/index_files/abstract_dijkgraaf.html
   of the invited plenary talk that Dijkgraaf gave at the Loops 2005 conference at the Einstein Institute (Potsdam MPI.)
   http://loops05.aei.mpg.de/
   The title of the talk was "Quantum geometry and topological strings".

   I think this video illustrates what Peter said about excellent expository skill and it also gives a sense of the person (who may also be highly able politically, as was suggested.)

2. **Edward Pickman Derby**
   November 15, 2011

   Peter, the importance of the IAS has declined greatly in recent years. Why is the announcement of its next director news? Do you actually believe he can right the ship?

3. **cthulhu**
   November 15, 2011

   Declined in favor to what?

4. **Marcus**
   November 15, 2011

   *Peter, the importance of the IAS has declined greatly in recent years. Why is the announcement of its next director news? *

   I don't know what Peter will say to this EPD, and I'm interested to hear his answer. But your question contains a non sequitur: it is precisely when such a notable institution's importance has declined that the announcement of a new director is news.

   There may be a broader problem, not limited to the IAS. I see several areas of fundamental physics where the importance of US research has declined relative to Europe and Canada (happily not in cosmology though!) and I attribute this to a US leadership that is set in its ways and lacks fresh vision in these areas. Changes in leadership could be part of the solution.

5. **Peter Woit**
   November 15, 2011

   Edward,

   I don't see the importance of the IAS as changing much at all in recent years. In math they've just hired away from Harvard Richard Taylor, who is about the best there is in number theory, and more generally they remain one of the great centers of math research. In physics they recently hired Arkani-Hamed, who arguably is the most influential theorist in his generation. They're pretty heavily loaded down with string theorists these days (even more so bringing in Dijkgraaf, keeping Goddard as a professor), but the string theorists there are the best in the world, with no sympathy for nonsense like the landscape, and increasingly moving from string theory to other topics. Like everyone else in particle theory, I think they're trying to find their way and waiting to see what the LHC says. But, like in math, the IAS has been and remains extremely influential in particle theory.

6. **Zakok Allen**
   November 15, 2011

   Peter, hate to interrupt the discussion (although this is not really off topic) but video of Witten's talk on Knots and Quantum theory is up on the IAS website. Would love to hear your take on it.

7. **lun**
   November 15, 2011

   regarding the no sympathy for landscape nonsense….

8. **Peter Woit**
   November 15, 2011

   lun,

What I wrote was carefully worded:

"the string theorists there are the best in the world, with no sympathy for nonsense like the landscape"

Arkani-Hamed is not a string theorist…

9. **Peter Woit**
   November 15, 2011

   Zakok,

   Thanks for pointing out the Witten talk. I think maybe it deserves its own posting, however short…

10. **Bobito**
    November 16, 2011

    Pickman Derby: to paraphrase Mark Twain: "the reports of my demise are greatly exaggerated".

Knots and Quantum Theory

November 15, 2011
Categories: Langlands

A commenter on the last posting pointed to the new video available at the IAS site of Witten's recent public talk there on Knots and Quantum Theory. The talk is aimed at a general audience, including supporters of the IAS, so it's rather non-technical. For the technical details behind what Witten is talking about (his recent work on Khovanov homology and QFT), see this survey for mathematicians, a survey for physicists at Strings 2011, and this paper.

For me an interesting part of Witten's talk was how he described the evolution of his ideas about this topic, and the relationship to geometric Langlands. He also had interesting comments about number theory and the Langlands program, denying any real knowledge of the subject, but arguing that sooner or later (probably later, after his career is over), there would be some convergence of number theory Langlands and physics. He finds the coincidence of geometric Langlands showing up in QFT so remarkable as to indicate that there are deep connections there still to be explored. I suspect that he sees the likely path of information going more from physics to math, with QFT ideas giving insight into number theory. While I agree with him about the existence of deep connections, I suspect the influence might go the other way, with the powerful ideas behind the Langlands program in number theory someday providing some clues about QFT useful to physicists.

Also on the Langlands program topic, this semester we're having a wonderful series of lectures on the topic by Dick Gross. He's a fantastically gifted lecturer, and this series is pitched at just the right level for me, explicating many of the parts of the subject I've been trying to learn in recent years but have found quite confusing. It's a beautiful, very deep, but rather intricate subject, bringing together a range of remarkable ideas about mathematics. In the end though, the Langlands program is really mostly about new ideas in representation theory, and since I'm convinced that deeper understanding of QFT will require new ideas about how to handle symmetries, which is the same thing as representation theory, perhaps finding connections between the subjects won't have to await Witten's retirement.

Comments

1. **martin**
   November 16, 2011

   Of course there is a big difference between Geometric Langlands Program and Langlands Program in the sense that Langlands uses the term. When you say that Witten expects a connection between number theory and physics does he mean Geometric Langlands or number theory?

2. **Peter Woit**
   November 16, 2011

   martin,

   Witten has already done extensive work on the connection between geometric Langlands and physics (and there was such a connection, to conformal field theory, there from the beginning). What he was speculating about was the possibility of a connection to the number theory case.

3. **martin**
   November 16, 2011

   Peter Woit,

   Thakns, so he did mean number theory. I've just finished watching the talk, and I saw the part where he said that about the Langlands program, but it was only one sentence in passing. Has he explained somewhere why he thinks so? Or is ti really just a speculation, he did say that he was not very familiar with number theory.

4. **Peter Woit**
   November 16, 2011

   martin,

   As far as I know he doesn't have anything specific in mind, this is just speculation. Philip Gibbs at vixra log transcribed the relevant part of his comments:

   "I had in mind something a little bit more ambitious like whether physics could affect number theory at a really serious structural level like shedding light on the Langlands program. I'm only going to give you a physicists answer but personally I think it is unlikely that it is an accident that Geometric Langlands has a natural description in terms of quantum physics, and I am confident that that description is natural even though Ithink it mught take a long time for the math world to properly understand it. So I think there is a very large gap between these fields of maths and physics. I think if anything the gap is larger than most people appreciate and therefore I think that the pieces we actually see are only fragements of a much bigger totality."

5. **Sakura-chan**
   November 16, 2011

   I really enjoyed Gross's lectures on undergraduate abstract algebra. He is a great speaker.

6. **aa**
   November 16, 2011

   Knot even wrong?

7. **Chris Austin**
   November 16, 2011

   On the topic of possible applications of the Langlands program to physics, there is a famous conjecture of Atle Selberg about the lowest nonzero eigenvalue of the Laplace-Beltrami operator on 2-dimensional cusped hyperbolic manifolds obtained by quotienting 2-dimensional hyperbolic space, with sectional curvature -1, by a k principal congruence subgroup of SL(2,Z), k >= 3, which has no torsion. Selberg conjectured that the lowest nonzero eigenvalue is 1/4, and proved it is >= 3/16. Selberg's lower bound was improved to 171/784 = 0.2181… by Luo, Rudnick, and Sarnak, and has been increased to 975/4096 = 0.238… by Kim and Sarnak. The analogous conjecture for the lowest non-zero eigenvalue on cusped principal congruence quotients of n-dimensional hyperbolic space H^n is that it is (n – 1)^2/4, which is where the spectrum starts on H^n. Sarnak has obtained a lower bound of 21/25 = 0.84 for a family of cusped hyperbolic 3-manifolds called Bianchi manifolds.

   I understand from page 5 of this article by Peter Sarnak that Selberg's conjecture, and from pages 14 to 16, possibly also the above generalization to cusped principal congruence quotients of H^n, are now understood to be part of Generalized Ramanujan Conjectures that are part of the (number theory) Langlands program.

If I have got that correct, (and I have to admit that there is a lot in Sarnak's article that I don't understand), does Ngo's proof of the Fundamental Lemma of the Langlands program mean that Selberg's conjecture, and its generalization to higher dimensional cusped hyperbolic manifolds as above, are now proved?

8. **Peter Woit**
November 16, 2011

Chris,

The proof of the Fundamental lemma only proves a small piece of the conjectures that make up the Langlands program, and as far as I know doesn't tell you anything about these Generalized Ramanujan conjectures.

For an interesting connection between physics and number theory that Sarnak has worked on, see his lectures about "Arithmetic Quantum Chaos" here http://publications.ias.edu/sites/default/files/Arithmetic%20Quantum%20Chaos.pdf

There are evidently examples of chaotic dynamical systems such that the problem of quantizing them and finding their spectrum is related to problems like the one you mention. This however I think is very different than what Witten has in mind, which involves not just QM, but quantum field theory.

9. **Chris Austin**
November 16, 2011

Thanks, Peter, I'll take a look.

10. **Jeff M**
November 17, 2011

I guess I should educate you physics types 🙂 The history of the Selberg conjecture and related issues is fascinating, McKean in the early 70s published a "proof" of the fact that the first eigenvalue was > 1/4 for ANY surface, thing was there was a mistake in the proof. Burt Randol (who taught me complex and real analysis) showed this by coming up with examples of surfaces with first eigenvalue going to zero (Randol is both a great mathematician and the best teacher I ever had, he just retired). The reason that 1/4 is so important is that it is the bottom of the L^2 spectrum of the Laplacian for the hyperbolic plane, and is related to the zeros of the zeta function (that is in fact how Selberg got his 3/16 result, he used Weil's theorem on zeta functions over finite fields). This sort of thing is why I ended up in geometric analysis, somehow everything seems to come together in strange and interesting ways…

11. **Chris Austin**
November 17, 2011

Thanks, Jeff. Randol's article appears as ref [26] of this review by Peter Sarnak. The examples with 1st eigenvalue going to 0 on the 2nd page here use a thin neck such that the length of a closed geodesic going round it goes to 0. You can't have examples like that in n > 2 dimensions because you can't put a hyperbolic metric on the "neck" of a connected sum for n > 2, and furthermore a finite volume hyperbolic structure has no shape moduli for n > 2 by Mostow rigidity. Which raises the question of whether the result claimed by McKean, (which I had not previously heard of), might be valid at least for n >= 4, with 1/4 replaced by (n – 1)^2/4. For n = 3 you could perhaps look for examples with large Dehn surgery coefficients in Thurston's construction.

12. **Jeff M**
November 17, 2011

Chris,

Yes, n=2 is a special case. Basically for hyperbolic manifolds, there are 3 different cases, n=2, n=3 and n>3. In dimension two you can have continuous deformation, in dimension 3 you can still shrink a geodesic to length 0 to get a cusp, but the deformation isn't continuous, and in dimension 4 and higher you can't even do that, there is a direct relation between the length of the shortest geodesic and the volume. My thesis was the n=3 case for the Laplacian on differential forms, and I studied the accumulation of eigenvalues near the bottom of the essential spectrum as the length of the cusp goes to infinity. For functions the bottom of the essential spectrum is (n-1)^2/4, but there can be isolated eigenvalues below that. The relevant theorem is due to Buser, Colbois, and Dodziuk, and says that for n>2 with pinched negative curvature, the number of eigenvalues below the bottom of the essential spectrum is a constant times the volume, and the constant depends only on n and the curvature bound. I have a paper with Ruth Gornet which discusses the bounded curvature case for forms, it's in Contemporary Math 237, and if I remember right it lists at least most of the relevant references in case you're interested.

Peter – hope it's OK to mention a paper of mine here, it was the best place I could think of for a reasonably complete list of interesting references for Chris.

13. **Chris Austin**
November 17, 2011

Thanks a lot Jeff, I'm reading your article, "Small eigenvalues of the Hodge Laplacian for three-manifolds with pinched negative curvature," in Contemporary Mathematics 237, now.

To find examples with small eigenvalues for all n >= 2, section 2.8.C in Gromov – Piatetski-Shapiro gives examples for all n >= 2 of finite-volume hyperbolic n-manifolds X, both compact and cusped, that contain a 2-sided non-separating embedded closed hypersurface S. If we take 2N copies of X, cut each along S, and join side b of copy 1 to side a of copy 2, side b of copy 2 to side a of copy 3, … , side b of copy 2N to side a of copy 1, we get a hyperbolic n-manifold that is a 2N-fold cover of X, and if we choose the function f on page 2 of the Sarnak review to be 1 on copies 1 to N and -1 on copies N + 1 to 2N, with smooth transitions across the two copies of S where f changes value, we get an upper bound on the first non-zero eigenvalue that is reduced by a factor 1/N, so can be made arbitrarily small by choosing N large.

Peter, I'm sorry if this has got off-topic.

14. **Jon**
November 18, 2011

Off-topic but highly interesting: a recent paper, with lots of apparently serious people backing it, claims the wavefunction has no statistical interpretation, it is real: see this Nature description…

15. **lun**
November 18, 2011

Honestly, I have watched this presentation and was disappointed.

He failed according to Feynman's criterion, "You do not really understand something unless you can explain it to your grandmother", and a lot of Witten's classic papers are classic precisely because they succeed in this respect, in the sense that they are remarkably clear in their arguments, both in stating the conclusion, supporting it, and elucidating the consequences.
I understand of course that this is a public talk on a specialized subject, but I failed to get even an inkling of what he was trying to accomplish, and why this worthwhile to do, and this is what the talk should have been about.

16. **hello!**
November 18, 2011

Witten's old paper on Free Fermions on an Algebraic Curve has some connection to number field Langlands, as well as geometric Langlands (that it predates).

17. **mbn**
November 19, 2011

hello!,

could you tell us which paper. Thanks.

18. **Peter Woit**
November 19, 2011

mbn,

The paper "Free Fermions on an Algebraic Curve" is in

The mathematical heritage of Hermann Weyl (Durham, NC, 1987), Proc. Sympos. Pure Math. 48, Amer. Math. Soc., Providence, RI, 1988, pp. 329-344. 16. Garland, H. and Zuckerman, G.: R

A more extensive version is

Quantum Field theory, Grassmannians, and algebraic curves,
Comm. Math. Phys. 113 (1988), 529-600.

For a more recent paper along similar lines, see Leon Takhtajan's
http://arxiv.org/abs/0812.0169

19. **David Ben-Zvi**
November 21, 2011

I think it's fair to say that ideas from the geometric Langlands program have recently started having an impact in number theory. The starting point is Ngo's celebrated work on the Fundamental Lemma, which doesn't explicitly use geometric Langlands but uses ideas that are very much part of that world. Since then this interaction has expanded significantly, primarily (in my not completely informed view) thanks to work of Ngo, Zhiwei Yun (MIT) and Xinwen Zhu (Harvard). In a spectacular recent preprint on his website, following up on ideas of Frenkel-Gross and his prior joint work on Kloosterman sheaves with Heinloth and Ngo, Yun used geometric Langlands ideas to construct motives with exceptional groups as Galois groups (in particular solving the inverse Galois problem for a large family of exceptional finite groups of Lie type). Other exciting developments in this direction include Zhu's resolution of a conjecture of Pappas and Rapoport on Shimura varieties and a variety of works deepening our understanding of various fundamental lemmas and their geometric origin. Behind all of this at some basic level is the realization of the motivic or geometric nature of many quantities of arithmetic interest, such as the constituents of the trace formula (see eg Nadler's BAMS article on the Fundamental Lemma), which enables arithmetic questions to be deduced out of more structured questions that can be approached in a function field setting using geometric ideas.

(Let me note that some of this work, eg Yun's work on global Springer theory, EXPLICITLY acknowledges a debt to physics: the influence of Kapustin-Witten on our understanding of the geometric Langlands program shouldn't be underestimated.)

20. **David Ben-Zvi**
November 21, 2011

I should also mention the fascinating work of Frenkel-Langlands-Ngo, proposing a strategy to understand Langlands functoriality based again on ideas of geometric (and physical) origin.

21. **Peter Woit**
November 21, 2011

David,

Thanks for your comments. I should note that, following up on Dick Gross this fall, Edward Frenkel will be the Eilenberg lecturer here this spring. I'm hoping we'll be hearing more about some of the topics you mention from him.

22. **Lee Flight**
November 24, 2011

Gross' Eilenberg lecture series is being made available:

http://www.math.columbia.edu/~staff/EilenbergVideos/Gross/index.html

kudos to Columbia for doing this.

23. **Ignatz Thugg**
November 25, 2011

Kudos to Columbia???? – what is SliverLite?

Generic problem: science for the rest of us should not be dependent
on on the fact that (a) not everyone uses Windows, (b) not everyone
has access to a university library – e.g. it costs ~$30 to read an article
published in Nature or a Springer or IOP journal.

24. **Peter Woit**
November 25, 2011

Lee and Ignatz,

The videos were produced, with no help from the university or department, for free by some of our grad students (especially Alex Waldron and Ioan Filip, maybe others I don't know about). They definitely deserve thanks from the whole math community for doing this on their own. One of our staff members, Nathan Schweer, helped put them up on the web server.

As for the format, it works fine on Macs (I just tried it out). Yes, you do need to install a "Silverlight" browser plugin from Microsoft. If you're allergic to them, in this day of Apple and Google dominating the internet, you should try and get over it and get outraged about something more up-to-date. Under Linux, there seems to be a Silverlight firefox addon, which, as usual in these things, doesn't work with the version of firefox I have, so I gave up. If we were paying somebody for this video, I suppose I'd look into demanding that they use a Linux-friendly format, but, you get what you pay for in the free software and content world….

As for the Nature/Springer/IOP problems, yes, in many cases it's outrageous that content produced by scientists that should be in the public domain isn't. In some cases though, I'm linking to material produced by professional science journalists, and someone has to pay them…

25. **Lee Flight**
November 25, 2011

Well I confess I did not notice the dependency when I watched using Chrome on whatever my netbook was booted as at the time, of course barriers are offensive just as it's offensive that there are millions who have no access to the Internet at all.
As one of the fortunate it astonishes me that material like this is there (thanks to grad students in this case); if I had been told 25 years ago when I was a student that material like this and some of the open courseware, lecture notes and also videos of the quality found on SCGP,KITP and PIRSA and elsewhere would be "available" it would have seemed to me the stuff of science fiction.

Higgs Non-News

November 18, 2011
Categories: Experimental HEP News

The combination of summer ATLAS and CMS Higgs results has finally appeared today (see here and here). This was originally supposed to be ready back in August, and has been circulating in various versions for quite a while. The bottom line (95% exclusion for 141-476 GeV) was mentioned here last week. They also quote limits using a much more stringent standard (99% exclusion for 146-443 GeV, excepting three small regions). Also worth mentioning is the 90% exclusion result, which reaches down to 132 GeV, leaving a SM Higgs possible only within the region 114-132 GeV.

What everyone really wants to know is when the experiments will release results based on the much larger full 2011 data set. Today's HCP 2011 talk just says:

> LHC experiments will analyze the x3 data already collected before 2012 Winter Conferences.
>
> Tevatron will provide the final results on 10 fb^{-1} by the 2012 Summer Conferencences.
>
> On the same time scale, there will be a combination LHC + Tevatron.

On this schedule, a possible 95% Higgs exclusion would not happen before next summer. However… I've seen comments from Fermilab that they should have results ready for Moriond in early March, and they expect to be able to rule out the Higgs at 95% (if it isn't there), over the relevant mass region. More immediately, the LHC experiments have been tasked to provide updates of their Higgs results, including per/experiment combinations, for the CERN Council Week (December 12-16). Rumors from the two experiments indicate that one experiment is seeing no excesses that could be attributed to the Higgs, the other only a very small number of events in one channel (ZZ->4l). It seems not impossible that the results available (publicly or not…) mid-December will come within striking distance of ruling out the Higgs (at 90% or 95% level) over the relevant low mass range.

One interesting aspect of today's data release is that it agrees closely with what Philip Gibbs put together back in September. For more about this, see here, especially this plot. In the past, many have speculated that the first observation of the Higgs would be reported on a blog. Now, it's looking not unlikely that a possible exclusion of the Higgs will be first reported at viXra log…

Update: CMS has released a video including footage of their internal discussions back in August when they decided not to release the ATLAS/CMS combination. There's no real explanation of what changed, but by November people's concerns had been addressed and they decided to release the combination.

Comments

1. **Kid Icarus**
   November 18, 2011

   But Peter, isn't news that the FTL neutrino experiment has been duplicated at CERN worthy of comment? Everybody's talking about it….

2. **Bernhard**
   November 18, 2011

   Kid Icarus,

   The experiment was not duplicated. It is the same OPERA experiment that did the measurement not another independent experiment. What they did was to repeat the measurement with very short beam pulses from CERN, which allowed the extraction time of the protons that lead to the neutrino beam. Since this more precise measurement did not change the conclusion the only thing one can say is that this specific source of systematic error was not responsible for the anomaly. Does not mean that there aren´t others. Until another experiment confirms it (most likely disprove it) the best bet is still a unknown source of systematics.

3. **Peter Woit**
   November 18, 2011

   Kid Icarus,

   As Bernhard points out, this is just the same experiment, with one of many sources of error removed. I still don't believe the result, and think it's getting way, way, way too much attention for something that is almost certainly wrong. Then, there's the added fact that I don't know anything much about the experiment, so it would be silly for me to write much about it here. As in many cases like this, I urge anyone interested in HEP to also follow the Resonaances blog, as well as Tommaso Dorigo's. They actually know something, and in this case I think Tommaso knows some of the people involve and has spent some time trying to understand the result. He has a bunch of postings up about it, I'm not going to compete with that…

   On the other hand, the Higgs story seems to me incredibly exciting, I can't figure out why people would rather talk about superluminal neutrinos at this moment in history…

4. **Bernhard**
   November 18, 2011

   And back to the Higgs, if the SM Higgs is excluded we are probably going to see a huge increase of interest in WW scattering.

5. **Amitabha**
   November 18, 2011

   Bernhard,

   Some of us expected some increase of interest in WW scattering already, but alas, few actually seem to care.

6. **Anonymouse**
   November 19, 2011

   Just remember to catch the bear (SM Higgs exclusion) before selling its skin (strong WW scattering). And please relax: this is science, not football.

7. **Anonyrat**

November 19, 2011

Tommaso Dorigo reads the same charts and thinks the Higgs will show up at 119 GeV.
http://www.science20.com/quantum_diaries_survivor/lhc_combination_higgs_limits_mh141_gev-84800

"In the lower part of the figure you see (black curve with blue "one-sigma" band overlaid) the "best fit" Higgs cross section, again in SM units. Here you see that while the 135-150 GeV fluctuation is only consistent with a Higgs boson that has a cross section much smaller than SM predicts, the 120 GeV one is in the right range: in other words, a 120 GeV Higgs (or rather, 119 GeV Higgs, as I predicted some time ago) would fit the bill quite nicely.

At the 2012 winter conferences ATLAS and CMS will present their search results employing three-times more statistics -all the data collected in 2011. By then we will know more about the possibility that the 120-GeV fluke is the real thing. "

8. **Anonyrat**
November 19, 2011

Meanwhile an antagonist chooses to highlight this: the scent of SUSY wafting from CERN:
http://arxiv.org/abs/1111.4204

9. **Peter Woit**
November 19, 2011

anonyrat,

I agree with those authors that there is a smell coming from SUSY these days, I just wouldn't describe it as a "delicate perfume"…

10. **Anonymouse**
November 19, 2011

Any respectable theorist, whether working on SUSY or not, knows what it means that somebody says SUSY is nearly killed. Being a polite mouse I will spare our host that knowledge.

11. **Low Math, Meekly Interacting**
November 20, 2011

I don't know if this is something anyone should worry about or not, but I figured I could ask here. I also know that consensus isn't a gold standard of anything in science, but there appear to be very few serious physicists who wouldn't agree that something simply must be going on at the TeV to break electroweak symmetry. The odds in favor of nothing showing up in the LHC appear to be pegged at about the same quantity.

So, fine, if the Higgs is about 120 GeV, that's a fiendishly difficult range for a hadron collider to probe, and it will take a lot more data. Still, some folks are apparently starting to get a little worried. Let's say that fluctuation vanishes. Let's say we see nothing else either.

Out of necessity, only a tiny fraction of collision data is captured, and those events are screened according to some entirely justifiable, but undoubtedly biased, criteria. When do we have to start wondering about missing something? Could a real signal be thrown away?

12. **Peter Woit**
November 20, 2011

LMMI,

I'm sure the current triggering choices being made at the LHC are designed to be optimal for looking for a SM Higgs signal. If it turns out there is no SM Higgs signal, then the question of what the LHC should be looking for to understand electroweak symmetry breaking will become a very important one. One answer will be "some SUSY or multiple Higgs model in which the Higgs is harder to see", and if triggers start being designed to look for these, one might worry about that.

In general though, I think the LHC experimentalists can be relied upon to not pay too much attention to theorists, and design triggers most optimal for picking out interesting classes of events. But in principle the possibility does exist that electroweak symmetry breaking comes from some source we haven't thought of, one that the LHC triggers aren't sensitive to

13. **David Brown**
November 20, 2011

… "some SUSY or multiple Higgs model in which the Higgs is harder to see"… Are there leading candidate theories (references?) for such SUSY or multiple Higgs models?

14. **Peter Woit**
November 20, 2011

For a good overview of many possible such models, see Matt Strassler's page

http://profmattstrassler.com/articles-and-posts/the-higgs-particle/implications-of-higgs-searches-as-of-92011

The problem is that none of these models are well-motivated (i.e. no evidence at all indicating them, and they're significantly more complicated than the SM: you're adding lots of new particles for no good reason other than to avoid conflict with experiment).

15. **Anonymouse**
November 20, 2011

I feel obliged to correct our kind host on this very common misunderstanding he repeats time after time: SUSY versions of the SM are not more complicated because you have to add lots of new particles which are not well motivated. The spectrum is doubled due to a fundamental hypothetical new symmetry of the world, which is something altogether different. I guess our kind host in his daily life multiplies 2×13 by summing one by one from 13 to 26.

16. **ZZZ**
November 20, 2011

Anonymouse, are you saying when you double a quantity, you are not adding anything to it?

17. **Chris Oakley**
November 20, 2011

[The Supersymmetric Black Knight](#) "Come back here! I'll bite your legs off!"

18. **Dave**
November 20, 2011

@Anonymouse
Its not a case of adding a fundamental new symmetry, rather a broken fundamental new symmetry. One needs to explain how the breaking occurs and this adds complications.

19. **Peter Woit**
November 20, 2011

Anonymouse,

I was specifically answering a question about the Higgs, so, in the context of supersymmetry, the point is that no one has seen evidence of any of the multiple Higgs particles needed in that theory. So, one has to go to some trouble to hide the things, a problem that will get worse if a SM-like Higgs is ruled out.

More generally, as Dave points out, what makes supersymmetric extensions of the SM complicated is not the doubling of degrees of freedom, but the huge number (>100) or parameters that must be introduced to describe supersymmetry breaking. In the world of SUSY-dreams, some new physics will be found to dynamically break SUSY and determine those parameters, but even in SUSY-dreamland, the new physics that does this will make for a significantly more complicated theory than the SM.

20. **ohwilleke**
November 20, 2011

@ Anonymouse

Most of the SUSY spectrum not already excluded requires a light Higgs just as much as the SM does. A Higgs exclusion greatly squeezes the parameter space of these models as well, arguably more, since its mass spectrum is more tightly constrained theoretically.

21. **N. Nakanishi**
November 21, 2011

I would like to note that four years ago I proposed a modification of SM in which the Higgs becomes unobservable. In this modification, in which a Weyl gauge field is introduced, manifest covariance, renormalizability, and unitarity are maintained. See, N.N., Prog. Theor. Phys. 118 (2007), 913.

22. **Bernhard**
November 21, 2011

N. Nakanishi,

Are you saying your idea is to have the SM with a mechanism to avoid a possible conflict with experimental data? With no disrespect but that sounds not too impressive. Unless this extension actually produces a measurable difference that can be cached, either than one that cannot, this does not seem to improve our understanding of what´s going on.

23. **N. Nakanishi**
November 21, 2011

Bernhard,
Have you read my paper (arXiv: 0704.2645v2 [hep-th])?
If the Higgs is not found experimentally at all, do you have any idea rescuing SM?

24. **Anti Ant**
November 21, 2011

ZZZ:
What the mouse says is that claiming SUSY adds too many new particles misrepresents the idea.

Chris Oakley:
Nice clip. Love those guys. But life may improve over art: keep posted.

Dave:
You're right, that's the ugly side of it, not the doubling of the spectrum. But this will turn out to be an opportunity to learn, once we have experimental access to the details of the spectrum.

Peter:
You're now completing your previous misleading reply, but you cannot help
your dislike of SUSY showing through. People don't try to "hide things". They try to figure out how the theory might be guided by experiment. There is a world of difference between these opposite ways of putting things. You are transmitting the message that scientists are cheating and fooling themselves while I know many honests physicists who are just trying hard to figure out things. You do a disfavour to science.

Concerning the huge number of parameters related to SUSY breaking that's just the low-energy point of view, which would be wrong to use if you want to gauge the complexity of your theory. You can't say much about how complicated SUSY breaking is till you have experimental information about the spectrum. People also forget that SUSY could've been dead long ago, e.g. if it predicted that scalars should be lighter than fermions. Or disfavored if it predicted the wrong running of gauge couplings.

25. **Bernhard**
November 21, 2011

N. Nakanishi ,

Now I did, nice paper. I was indeed expecting that any model that would make the Higgs unobservable clearly predicted something else and your model cleraly does that:

"in addition to the physical absence of the Higgs boson, slight violationof Lorentz invariance and the existence of a new massive gauge boson."

So that is fine, your first comment made me jump to the wrong conclusion but I anyway should have looked with more attention before criticizing it.

26. **Paolo Valtancoli**
    November 22, 2011

    The requirement of unitary and renormalizability always implies something extra to be discovered beyond the SM. If LHC doesn't find anything new, this means that these requirements are too strong. In fact quantum gravity (and Nature) is in conflict with these requirements and works fine without them.

Still Waiting for Supersymmetry

November 21, 2011
Categories: This Week's Hype

The headline story at the APS Physics site is Still Waiting for Supersymmetry, by Sven Heinemeyer, which reports on a PRL article from CMS reporting no evidence for supersymmetry.

According to Heinemeyer:

> It's important to realize that CMS's results do not exclude supersymmetric theories. Rather, they only conclusively say one of two things. One possibility is that the CMSSM (the specialized version of the MSSM) is realized in nature, but the supersymmetric partner particles, the gluinos and squarks, are relatively heavy—too heavy to be produced in large numbers at the LHC so far…
>
> The other interpretation is even simpler: while supersymmetry is realized in nature, it might not take the form described by the CMSSM, but possibly that of any one of the many (GUT scale) models. Different versions of supersymmetry make different predictions for the outcomes of high-energy proton-proton collisions. Many of these outcomes are more complicated than what is shown in Fig. 1, and to see them would require experiments to investigate many more collisions (and to study them for a longer time). Consequently, in these other models, it will only be possible to place much weaker bounds on the new particle masses (so far, however, no such dedicated analysis has been performed).

I would have thought that there's an even simpler third alternative: no supersymmetry in nature at all, but I'm not a SUSY phenomenologist…

Comments

1. **neo**
November 21, 2011

Einstein was once asked what he would conclude had the evidence of the deflection of light by the sun not supported the theory of genral relativity. He answered something along the lines of "that would be too bad for the evidence, because general relativity theory is too beautiful not to be true." Is SUSY too beautiful not to be true?

2. **M. Wang**
November 21, 2011

"It is difficult to get a man to understand something, when his salary depends upon his not understanding it!" — Upton Sinclair

3. **Peter Woit**
November 21, 2011

neo,

No.

4. **N. Nakanishi**
November 22, 2011

Neo

General relativity was quantitatively confirmed by the perihekion shift of Mercury before the observation of the deflection of light. On the other hand, SUSY has absolutely no experimental support.

5. **Henry Bolden**
November 22, 2011

The supersymmetry debacle was predicted by the The Everly Brothers:

Wake up, little SUSY, wake up
Wake up, little SUSY, wake up
We've both been sound asleep, wake up, little SUSY, and weep
The movie's over, it's four o'clock, and we're in trouble deep
Wake up little SUSY
Wake up little SUSY, well

6. **Bernhard**
November 22, 2011

That Sven wants to give SUSY the benefit of doubt is fine of course. Technically speaking he is correct (SUSY is indeed not excluded). But he is being deliberately misleading in not talking about the third possibility that SUSY might simply not be realized in nature and to say this out loud. All in all I think it is not nice APS asks a SUSY fanatic to talk about an article that is clearly not good for the SUSY idea without asking also an anti-SUSY person to make a counter comment in the name of keeping the discussion two-sided. This is not healthy.

7. **Cesar Laia**
November 22, 2011

Comments here are too harsh, I think. I am a layman, but I clearly see "…suggests that if the theory is correct, it may be more complicated to discover these particles than previously thought."

So, he states "if the theory is correct" and then goes on. I don't see the problem with this.

8. **Bernhard**
November 22, 2011

Cesar Laia,

Of course Sven did not forget the usual caveats, but this is in my opinion not putting all possibilities with the same weight. If we add the fact that the particular article he is talking about should lean to the possibility he does not explicitly discuss this is even worse.

9. **William Tell**
November 22, 2011

Time will tell and put everybody in place.

10. **Peter Woit**
November 22, 2011

Cesar Laia,

What you're quoting is from the headline summary of the article, something which is normally not written by the author, but by an editor. The normal complaint is that physicists write something accurate, and the editor then hypes the story in the headline. Here, the physicist is writing inaccurate hype, and the editor is correcting the story in the headline.

To be clear, I don't think Heinemeyer is unaware of the logical possibility of no SUSY, but it's psychologically interesting that he can't bring himself to mention this in print.

11. **CIP**
November 22, 2011

Or perhaps the squark, like the snark*, is a boojum.

*with apologies to Lewis Carroll

12. **Philip Gibbs**
November 22, 2011

Peter, he is not giving a list of all the logical possibilities. He is giving a list of the possible things that can be said "conclusively" from the CMS results. In the first sentence he makes this clear and he says explicitly that excluding supersymmetry is not on that list.

13. **Peter Woit**
November 22, 2011

Philip,

I'm sure one can come up with a strained reading of the text involving unmentioned other "inconclusive" possibilities, and, again, of course Heinemeyer is aware of the non-SUSY case and would not deny it is possible.

Still, I think a straight-forward reading of the text as written is that there are only two interpretations of the data he is willing to refer to explicitly and discuss: one is that SUSY partners are too heavy to see, the other is that they are hiding.

14. **Cesar Laia**
November 22, 2011

I made the remark because that headline looked to me as an abstract written by the author, not a feature added by the editor. And to me it is an useful statement about the point of view discussed further in the article. It looks like a good paper, anyway, although I am not a physicist so I miss details. The editors of that journal should consider to give an opportunity for other to express non-SUSY alternatives, no doubt. But that is not the problem of the author, I think, he expresses only is point of view.

15. **Philip Gibbs**
November 22, 2011

Peter, I am just reading it as it is written. It is your reading that is the strained one because you are always looking for your particular angle which is that the theorists are not willing to accept that supersymmetry has failed (which it has not yet).

That angle is not there in this story. Nowhere does it imply that supersymmetry must be found. The bold line below the heading uses the phrase "if the theory is correct" . I can see nothing unbalanced there at all.

16. **Peter Woit**
November 22, 2011

Philip,

People can read the thing and make up their own minds whose reading is strained. The only place you or anyone else has pointed to in the article that mentions the possibility that SUSY may not be realized in nature is the headline, which usually is written not by the author, but by an editor.

17. **SpearMarktheSecond**
November 22, 2011

Someone used to say… SUSY has no experimental support, but it has withstood the test of time.

There are lots of contrived aspects of the current SUSY models… not one of them is as beautiful as General Relativity. But the non-GR part of the Standard Model is pretty darned ugly too… where does the ratio of the mass of the top quark to the mass of the up quark come from?

The excitement of the LHC is like what Columbus and his crew must have felt when they sailed westward. And that is sufficient!

What is distressing about the hype is that it has setup a sense of disappointment if SUSY is not there. I don't care, the great value is in *knowing*, even if that is knowing that SUSY is absent at LHC mass scales.

18. **emile**

November 22, 2011

"It's important to realize that CMS's results do not exclude supersymmetric theories."
What if he had said: "I think it is important to realize that SUSY theories cannot be excluded." I think that would have been more helpful to the reader…

19. **flog**
November 22, 2011

Flogging a dead horse? SUSY will never be disproved. Epicycles were never explicitly disproved. The luminiferous ether was never explicitly disproved. In both cases they just kept getting more and more contrived as new data came in. Also in both cases, an alternative theory came along which could do a better job of explaining the facts, and which did not require fine-tuning every time more data was found. People just lost interest in the outmoded clumsy way of doing things. SUSY will only go away when a new theory comes along that can explain things better. But there is no compelling alternative which does a better job.

20. **srp**
November 22, 2011

Do ellipses have fewer free parameters than epicycles (and equants and eccentrics?) That sort of inductivist criterion, while fine for assessing rival models within known-to-be-stationary, common data generation processes (such as pattern-recognizing neural nets), flops for comparing theories with non-stationary or overlapping but distinct predictive domains. The theory that the farmer loves the chicken and that his morning visits are for feeding purposes parsimoniously explains the data right up until the chicken is fat enough and gets slaughtered. Then the more complex theory that the farmer has extrinsic reasons for feeding the chicken looks a lot better, even though it has a lot of free parameters about how fat the chicken should be depending on prices for feed and for chicken parts.

I'm sympathetic with our host, but a lot of non-systematizable judgment is needed in order to decide when more theoretical complexity is called for.

21. **N. Nakanishi**
November 22, 2011

Certainly, SUSY may be beautiful mathematically, but as a physics, it is evidently violated very badly. There is no Nambu-Goldstone fermion. If one wishes to use the super-Higgs mechanism, one has to introduce supergravity. However, quantum supergravity is a very ugly theory! There is no SUSY-invariant gauge fixing. The SUSY anticommutation relation for global super-charges no longer holds because of index mismatch (Note that gamma matrix is a local quantity; there is no global quantity having the same set of indices as gamma matrix in the framework of supergravity).

22. **Shantanu**
November 23, 2011

Peter, is there a similar sentiment on extra-dimension models or people have resigned
to the fact these are all ruled out.
(I asked this question to Lisa Randall when she did a guest post on cosmicvariance, but didn't receive any response),

23. **Sven**
November 23, 2011

Hi,
I feel I should also say a few words here…

The journal asked me to write a layman explanation of the CMS paper, which deals
with the search for SUSY. They did not see any excess and set limits. The real question is how these limits are to be interpreted. Interestingly, many people draw wrong conclusions from this kind of limits. I tried to convey what these limits really mean. Nothing more, nothing less.

Any negative search that sets a limit can of course be interpreted as an indication that the 'object' searched for does not exist. However, this was not the question here. On top of that, the consequence would be to consider that nothing beyond what
we know for sure so far exists: no Higgs, no SUSY, no extra dimensions, … because the LHC did not see anything new so far. Not a very helpful approach in my opinion.

On the other hand, I would be interested whether I succeeded to explain the consequences of the SUSY searches published by CMS in a comprehensible way.

24. **Peter Woit**
November 23, 2011

Thanks Sven,

It seemed to me your article avoids mentioning the elephant in the room: not finding SUSY evidence of the sort many people were expecting does throw more doubt on the whole idea of TeV-scale SUSY. There really are three possibilities here worth mentioning, not just two…

25. **Anon**
November 23, 2011

From logic there can be only two definitive resolutions on supersymmetry: 1) it exists (direct or indirect observation of behavior predicted by or indicative of it) or 2) it does not (every single variant and flavour of it has been disproven experimantally. Which would be an immensly daring task indeed). Everything in between would be little more than ideology exchanges between theorists' camps (at least for a long long time before not yet disproven parameter space of supersymmetry would start drying up).
Want something more productive? Sure, just come up with an alternative theory that would achieve at least the same results as SUSY with superior observability and testability in experiment!

26. **Peter Woit**
November 23, 2011

Shantanu,

In the extra dimensions case I think very few people ever seriously thought these would be seen at the LHC. So the LHC results just move the bounds up, and in a few years when the LHC runs at full energy, there will be another jump in the bounds. This is all just what was expected, so no one is talking about it. One can of course keep going working on such ideas, saying the scale is just higher up, but there's likely to be less interest in this.

27. **helvio**

November 24, 2011

Gauge coupling unification, which can be extrapolated from current data to be an exact match at some very high energy (unlike the prediction from the Standard Model as it stands), is experimental evidence albeit indirect. Explain that without SUSY, in a simpler way.

28. **archytas**
November 24, 2011

Here is something that might amuse you all.

I was re-watching the Elegant Universe the other day, and I noticed that Brian Green explicitly states that supersymmetry is a "central prediction of string theory."

If you want to find the quote, it is in the latter part of the program when they talk CERN.

29. **Eric**
November 24, 2011

Supersymmetry is a prediction of string theory since it is inconsistent without it. However, this does not necessarily mean low-energy supersymmetry which is observable at the LHC or other colliders. As far as string theory is concerned, supersymmetry can be broken at any scale below the string scale. So, if SUSY is not observed for LHC, this has absolutely no implications regarding whether or not string theory is correct.

The reason for believing in low-scale supersymmetry is that it may allow several phenomenological issues relevant to the Standard Model to be elegantly resolved. These issues are the stability of the Higgs mass against quantum corrections, the apparent unification of the gauge couplings, and a natural dark matter candidate. The existence of a light Higgs boson which is an elementary scalar and low-energy supersymmetry are really tied together.

30. **Beelzebud**
November 25, 2011

It just strikes me as rather convenient that people declare SUSY as a central prediction of string theory, and then also say that if SUSY isn't found that is has absolutely no implications for string theory.

I don't have to be a genius to see the problems with this. After awhile it just looks like a classic case of moving the goal posts around, every time you don't get the results you hoped for.

31. **Eric**
November 25, 2011

Beelzebud,

You seem to be missing the fact that, although supersymmetry is a necessary component of string theory, the scale at which it is broken is not fixed. Supersymmetry may only be observed at the LHC if it is broken at a relatively low scale. If it broken at higher scales, it will not be seen. As I stated, there are phenomenological reasons for believing in low-scale SUSY which have nothing to do with string theory. If SUSY is observed at the LHC, then this will provide support for string theory, but does not prove that it is correct. One can have supersymmetric point particle theories without any connections to strings. Conversely, the non-observation of supersymmetry at the LHC would only show that SUSY is broken at a higher mass scale, if it exists.

32. **Peter Woit**
November 25, 2011

Eric,

The problem is that, in places like Brian's NOVA series, the public was told something like "string theory does make predictions: it predicts supersymmetry, and this will be tested at the LHC". One can be sure that if SUSY had (or does…) show up at the LHC, string theory enthusiasts would be vigorously claiming it as a confirmation of a string theory prediction. Not telling the public that no SUSY at the LHC was fine for string theory, since string theory really makes no predictions of the usual sort, was kind of misleading. I think the people doing it did believe that SUSY was likely to show up at the LHC and they would be vindicated. Now they're trying to get away with claiming it wasn't a real prediction. There's an obvious attempt here to have it both ways…

33. **Eric**
November 25, 2011

Peter,

The statement, "string theory does make predictions: it predicts supersymmetry, and this will be tested at the LHC" is technically correct. It does predict supersymmetry and this can be tested at the LHC, so I really don't see your problem. You seem to be incorrectly interpreting this statement as saying that string theory predicts that supersymmetry should be observed at the LHC. If string theory is correct, then supersymmetry might be observed at the LHC, which is much different than saying that it predicts that it should be seen.

If supersymmetry is found at the LHC, then this would indeed constitute evidence in favor of string theory, although it does not prove it is correct. However, you cannot claim that the a non-observation of supersymmetry disproves string theory since string theory minimally only requires unbroken supersymmetry at the string scale.

34. **Eric**
November 25, 2011

Also, it should really be pointed out that the expectation of seeing supersymmetry at the LHC comes from particle phenomenology rather than string theory. I do think that you have a tendency to give people the wrong impression about this.

35. **Bob Levine**
November 25, 2011

@Eric

"The statement, "string theory does make predictions: it predicts supersymmetry, and this will be tested at the LHC" is technically correct. It does predict supersymmetry and this can be tested at the LHC, so I really don't see your problem. You seem to be incorrectly interpreting this statement as saying that string theory predicts that supersymmetry should be observed at the LHC. If string theory is correct, then supersymmetry might be observed at the LHC, which is much different than saying that it predicts that it should be seen."

————————————————
I really *don't* think so, Eric. Putting an hypothesis in an experimental situation where in principle its *in*validity cannot be tested—where it can be confirmed if it's true, but not ruled out under any of the experimental protocols in question—is not what most of us understand as a 'test' of that hypothesis. It's precisely this sort of disingenuousness that makes statements like Greene's so suspect. If the lab conditions involved don't make it possible to disconfirm, please *don't* pretend you're "testing" the proposal. And note also that it doesn't help that Greene said, not 'can/might be tested' as per your gloss, but *will* be tested.

36. **Eric**
November 25, 2011

Dear Bob Levine,

As in Peter's case, I believe that you are misinterpreting Brian Greene's statement, which has two points:

1) String theory predicts supersymmetry
2) Supersymmetry will be tested at the LHC.

Each point separately is completely correct. However, you are combining these two points into a single point claiming that string theory will be tested at the LHC. This does not follow logically.

As I clearly stated, the observation or non-observation of SUSY at LHC does not prove or disprove string theory. However, the observation of SUSY would, in fact, provide experimental support for string theory. This is all that Brian Greene is saying, nothing more, nothing less.

37. **Bob Levine**
November 25, 2011

No, Eric, I'm misinterpreting nothing. I was not talking about string theory at all, and I wonder why you thought I was. The defense of SUSY against its no-show at the LHC is, as you yourself have expressed it, that the energy regime of the breaking scale may be higher than what can be tested at the LHC, so that nothing that happens at the LHC can disprove SUSY; all that can happen at the LHC is that observations might confirm SUSY. So in what sense will SUSY actually be *tested*?? And that's the problem with both what Greene says and what you say: string theory or no string theory, Greene is making an unjustifiable claim when he says that 'this ([i.e. supersymmetry] will be tested at the LHC', and you are making the same unjustifiable claim in defending his statement. An experiment which cannot in principle exclude a particular family of hypothesis can in no sense be said to *test* that hypothesis. Why is this conclusion even slightly contentious?

38. **EDR**
November 26, 2011

Dear Bob Levine and Eric,

I agree with Bob's criticism that the most natural reading of Brian Greene's statement is misleading.

This does, however, raise the question of how best to make a brief statement that suggests something more like: "If the LHC were to find evidence supporting supersymmetry, this would give a significant (though utterly inconclusive) measure of support to string theory."

The word 'test' is misleading in Greene's context. What word or phrase would be better?

39. **Eric**
November 26, 2011

Dear Bob,

What is being tested at the LHC is low-energy supersymmetry. More specifically, the theory that low-energy SUSY solves the hierarchy problem, provides the dark matter, and allows the gauge couplings to be unified is what is being tested. Yes, the scale of SUSY can be pushed higher if the superpartners are not observed at the LHC. However, in this case, SUSY cannot solve the above problems.

40. **Bernhard**
November 26, 2011

Eric,

The statement Greene should have made is that SUSY is a testable theory and it will be tested at the LHC. Discovering SUSY would be supportive of string theory but in no way a test of it since of string theory is not a testable theory. Not finding SUSY or not finding anything at the LHC would not contradict string theory. You expect people to consider the two statements of Greene separately but for crying out loud this is the definition of a misleading way of arguing even if it were to agree with you that it is logically correct. Mind you also that even some physicists not very much into SUSY or strings might get the wrong idea, what to say the layman, which is Greene ´s target.

The Infinity Puzzle

November 22, 2011
Categories: Book Reviews

There's a fascinating new book by Frank Close out this week about the history of the Standard Model, called The Infinity Puzzle. Until now I've always recommended The Second Creation, by Crease and Mann, as the best popular book for this history, but Close's new book gives that one a run for its money. While Crease and Mann is a comprehensive overview, covering theory and experiment, as well as a longer time-frame, Close gives an insider's look focused on the decade or so that led up to the Standard Model coming together around 1973.

Knowing the history of a subject has always seemed to me an integral part of really understanding it, so I'd argue that anyone who wants to really understand modern particle physics should spend some time with a book like this. In addition, there's an outside chance we may soon be seeing the collapse of one of the central pillars of the Standard Model, the Higgs field, and if this happens, an understanding of where the Higgs came from may very well be relevant to anyone who wants to think about how to live without it.

About a year ago I spent some time looking into the history of what I think is best called the Anderson-Higgs mechanism, writing a long posting about it here. Particle physicists have long overlooked the fact that it was condensed matter theorist Philip Anderson who not only first understood the basic physics that was going on, but even wrote a paper aimed at explaining it to particle theorists (which they ignored). Anderson's insights grew out of his work on the BCS theory of superconductivity, a subject in which the role of gauge invariance was not so easily understood. If the Higgs field needs to be replaced, the analogy with BCS theory might provide a clue about what could replace it. Another book I've been reading recently is a collection of Anderson's essays, called More and Different: notes from a thoughtful curmudgeon. Many of the people and topics he discusses there are much less familiar to me, but I confess to enjoying the curmudgeonly tone, and wishing I knew more about the history and physics behind the superconductivity research that he describes. Included in his collection is a review of my book I was very pleased by. His prediction about what the LHC will see is one I'm very sympathetic to: no supersymmetry, and "we will probably discover unexpected complexity in the Higgs phenomenon."

Anderson is justifiably scornful that the APS awarded the ~~Dannie Heinemann prize~~ for work on the Higgs to no less than ~~seven~~ people, managing to leave out Anderson. Close gives Anderson his due, but also gives by far the most detailed and well-researched account available of the work of Higgs, Brout, Englert, Guralnik, Hagen and Kibble in this area. He puts the most dramatic revelation from his research in a footnote (page 388):

> A bizarre coincidence is that on Monday, October 5, just a week before Guralnik, Hagen, and Kibble's paper was received by the editor of *Physical Review Letters* in New York, and hence around the time that it would have been submitted to the journal, Peter Higgs gave a seminar about his mechanism at Imperial College. Neither Guralnik nor Kibble has any memory of this, and extensive correspondence between us has failed to shed light on this.

If the Higgs particle does show up at the LHC and the Nobel committee starts debating who should get the prize, this may become relevant. Another thing that I learned from Close that argues for Higgs in this context is that he was the first (in 1966) to write down a model ~~with Yukawas giving masses to the fermions~~ .

Close knows especially well the British cast of characters in this story, and one issue he devotes attention to is the unusual story of J. C. Ward's eventful career and the question of why Ward and Kibble weren't the ones to come up with the Weinberg-Salam model. Ward and Salam had worked on unified electroweak theory, minus the Higgs, and Kibble was very much involved in the Higgs story. One of the factors at play according to Close's account was Ward's rather paranoid nature, which made him unwilling to share ideas.

Another Nobel-related part of the book that will likely be controversial is the discussion of Salam's case for sharing the Nobel with Weinberg and Glashow. This topic recently was raised by Norman Dombey in a preprint on the arXiv (discussed a bit here), which refers to Close's book. Close gives a detailed description of Salam's activities around the time he was supposedly doing the Nobel Prize winning work, raising the possibility that he may not have had the right idea independently of Weinberg. One thing that is clear about this particular story though is that no one involved, including Weinberg and Salam, understood the significance of the Weinberg-Salam model at the time.

An argument might be made that the book has quite a lot of "inside baseball", about who exactly did what, and what people's relative cases for recognition might be. If you really detest this sort of thing and want nothing but the physics, maybe you should stick to The Second Creation. But, if like me, you're fascinated by this history and want to learn something new about it, go out and get a copy soon.

Update: I should make it clear that what I wrote here about Salam is my own interpretation of the story, not that of Close. He explains that Salam learned about the Higgs mechanism from Kibble, and had a unified electroweak theory with Ward, so it makes perfect sense that he would come up with Weinberg-Salam, independently of Weinberg, and he was lecturing about something. Still, the lack of any written record of exactly what Salam had pre-Weinberg makes one wonder…

Update: For the first-hand case that Salam did lecture on the Weinberg-Salam model pre-Weinberg (which is also described in Close's book) here's this from Robert Delbourgo:

> Dear Peter
>
> There have been murmurs on your blog-site, following Dombey's article I think, which cast doubt on Salam's worthiness for the prize. I wish to refute the innuendos and aspersions which are circulating.
>
> I was indeed present at the talks given by Salam on SSB for weak interactions where the famous model was described. Paul Matthews also attended, but being Oct that year, Tom Kibble was away on sabbatical at Rochester. I am prepared to take an oath on that.
>
> It was more than one lecture, but I cannot remember whether it was two or three talks which he gave as it quite long ago. Then I went to the library and spotted Weinberg's paper, newly arrived, and pointed it out to Salam and urged him to write up his own independent discovery ASAP. Matthews also encouraged him to do so and the first opportunity was the Nobel Symposium. That is the long and short of it.
>
> I hope that ends the the rumours and controversy!!!
>
> Bob Delbourgo

Comments

1. **Dr. David Edwards**
   November 22, 2011

   I've read that Einstein considered The Einstein-Maxwell-Dirac System ~1930. Do you know who first considered The Einstein-Yang-Mills-Dirac System?

2. **Charles**

November 22, 2011

Like Dombey, Frank Close takes liberties with memories and diaries in several areas from a quick scan of this book and footnotes.

For example, the IC seminar by Peter Higgs that you note may have taken place but not in October 1964. Not possible GHK would have skipped this as they were all there at this time – Kibble is particular would recall this. A person named Ray Streater organized the seminar at IC and he is unable to confirm the date of the session. This likely took place in 1965 – Hagen would have been back to Rochester by then but Guralnick and Kibble were still at IC.

Just one example, where Close should be more careful with the facts – or at least how they are represented.

3. **William Tell**
November 23, 2011

"Knowing the history of a subject has always seemed to me an integral part of really understanding it, so I'd argue that anyone who wants to really understand modern particle physics should spend some time with a book like this."
You mean to say that extraterrestrial scientists will never really understand the SM physics?

4. **Peter Woit**
November 23, 2011

Charles,

The information about Higgs' talk is from Higgs's diary, which Close has access to. Close notes in a postscript to the book that he found memories of the period sometimes in conflict with the written record, which is much more reliable. He encourages anyone with evidence that conflicts with what he has written to contact him.

5. **Peter Woit**
November 23, 2011

William Tell,

Extraterrestrials will have their own history of how they found the Standard Model (or something like it…), and use that history to better understand it. They would undoubtedly benefit from access to our history, just as we would learn a lot if we knew theirs. If you can get ahold of an extraterrestrial history of how they developed their Standard Model, I for one really, really want to read it.

6. **Vladimir Kalitvianski**
November 23, 2011

There is a video-interview of Murray Gell-Mann about it. It is worth watching episodes 150-152, I think. http://www.webofstories.com/play/52253?o=MS

7. **Bernhard**
November 23, 2011

Vladimir,

This was very interesting.

Off-topic: By accident I saw also Gell-Mann´s comment on string theory. I knew he was sympathetic but when you hear him saying that Glashow "helped to spread the entirely wrong idea that superstring theory can never be tested" one really feels like asking him what is his exact idea on how to do that…

http://www.webofstories.com/play/52254?o=R

8. **Peter Woit**
November 23, 2011

Bernhard,

Gell-Mann's comments about Ward and Salam are quite interesting and relevant to the book. As for string theory, the video is from almost 15 years ago, at a time when enthusiasm was much higher. He may by now be more sympathetic to Glashow's point of view. Enough about string theory though, a topic mercifully irrelevant to the book.

9. **Bernhard**
November 23, 2011

Sorry Peter, I couldn't resist. But back to the point what I am a bit uncomfortable is that it seems these late accusations of Salam not being responsible for Salam-Weinberg is the fact that Salam is not here to respond these attacks. If this video is really from 15 years, than is from the time Salam died. I wonder if people confronted Salam with this when he had a chance to fight back.

10. **Peter Woit**
November 23, 2011

Bernhard,

The video says it is from 1997, Salam died in 1996. So, yes, he couldn't respond to Gell-Mann.

Close explains in his book that people's recollections of this era often diverge significantly from the written evidence he turned up. Although Salam and many others are not around, Salam's papers are and I'm sure he kept whatever could document his case for a piece of Weinberg-Salam. Evidently the Nobel committee was convinced by what was sent to them, and at some point (50 years after the prize?) the nomination materials become public I think. Dombey and Close both had access to the nomination from Matthews, written by Salam. This one is a case for the historians of the future to work on, but my impression is that Close gives a fair account of the evidence he turned up, and he doesn't seem to have an ax to grind here.

11. **Avattoir**
November 23, 2011

Can I just say, Peter, as someone who's a professional with a number of academic degrees but quite limited capacities in math and interest but only incidental exposure to physics, the earlier post you link to her on the subject of who might the Nobel prize go to if the Higgs were to be found was the most fun one I've found here in the year I've been visiting; and it and this one too finally got me off my duff to order your book.

So much for the fanboy part – but, though it's an imposition that doesn't actually demand a response, I wonder if you've read Louisa Gilder's book on Entanglement, and if so what your thoughts are.

12. **Peter Woit**
November 23, 2011

Avattoir,

Thanks, I hope you'll enjoy my book. If you liked the kind of thing I was writing about the Higgs mechanism history, there's a lot more of that in Close's book.

I haven't read the Gilder book, although probably should have. It looks good, with an historical take on the subject that would be interesting. My interest in interpretational issues in quantum mechanics though is somewhat limited, and life is just too short to read everything one would like to.

13. **Charles**
November 24, 2011

Happy Thanksgiving.

Source is understood but likely not correct from place or topic which is unfortunate for the story and the book. More broadly, this footnote that you highlighted struck me as odd/interesting and seems to have a similar tone to Dombey's article on Salam. In two papers in the last few years, Guralnik has stated how he learned of the other PRL submissions (and PA) when submitting the document for publication.

http://arxiv.org/abs/0907.3466

http://arxiv.org/abs/1110.2253

As these links point out, after seeing pre-prints at IC of Higgs and Brout/Englert, GHK famously referenced them and also referred to a theory that was "partially solved" by BE and H (as you pointed out in related blog they did not reference Anderson). So again, I was just confused by what Close was getting at with this footnote.

I have not completed the book but the other oddity is the selection of other "facts" and "contributions" that Close brings forward on this clearly charged topic of the "Higgs" mechanism. Some initial thoughts include…

1) Again, for the footnote on page 388….is Close and Higgs trying to imply PH gave GHK the idea and in a matter of two days they wrote a paper that was considerably more complete than PH or BE – improving the concept by showing both the degrees of freedom and explicitly how the Goldstone theorem is avoided? Guralnik was working on topic with Gilbert back in Cambridge, MA in 1962 and 1963 and published an earlier paper (Phys. Rev. Lett. 13, 295 (1964)) which has the basis of the GHK approach to the mechanism (submission date precedes the others) so that is a hard case to make (if he is trying to put that implicitly in the reader's head).

2) The 1966 PH paper that FC puts great stock in, writes down some terms that contribute to higher order corrections to the leading approximation given in the PRL papers. This is fairly trivial and represents little new insight or any surprise to anyone who does quantum field theory. Not completely sure Close's background or understanding on these points. Prior to this '66 PH paper, there was a similar contribution by GG who gave a talk in 1965 containing detail beyond that contained in any of the PRL submissions (see http://arxiv.org/abs/1107.4592v1).

3) Similarly, the follow-up paper by TK (Phys Rev 155, 1554 (1967)) is considered by most (even TK) to be a minor extension to the 1964 GHK paper and FC puts a lot of weight in that contribution also – more than the larger physics community.

4) When discussing the naming of the mechanism and Nobel politics he does not mention t'Hooft and his role. In their Nobel winning paper, t'Hooft and Veltman reference all three PRL papers and even refer to the mechanism as the "Higgs-Kibble mechanism" (http://igitur-archive.library.uu.nl/phys/2005-0622-155148/13877.pdf). Since winning the Nobel, t'Hooft has removed credit from the GHK team and pushed hard for a BEH award…and I am sure written letters to the Swedish Academy. As I have posted here before, t'Hooft feels many Europeans have been ignored for Nobel Prizes and sees the "Higgs" story as an opportunity to partially fix past snubs.

So as I read a footnote that seems to be false, it was odd that important items from this story that can be more easily verified by papers and lectures were omitted. It is difficult to deal in half-truths on this topic. Not being able to get the organizer (Streater) of the seminar at IC to confirm the date or clarify the point of the footnote leads me to believe it was somewhat careless (at best) or intentionally false (at worst) in order to help PH's or someone else's case in this "game of credit". FC may have felt he lost the personal side if he relied more on the papers but these two extremes could have probably been better combined better for this topic. Again, the similar in tone (as you said:" curmudgeon") to Dombey and Salam is notable.

Will circle back after I complete book and do more research .

Again, Happy Thanksgiving

14. **O Zapata**
November 25, 2011

Dear William Tell,

Peter's opinion coincides with that of Aristotle; don't forget, the most important physicist for almost two millennia:

"If you would understand anything, observe its beginning and its development."
Aristotle

This was valid for the physics of the Presocratics as it is today to understand the fundamental issues behind modern theoretical physics ("what if there is no Higgs?").
Another quote, more controversial, this time by a modern particle physicist:

"Finally, learn something about the history of science,or at a minimum the history of your own branch of science. The least important reason for this is that the history may actually be of some use to you in your own scientific work. For instance, now and then scientists are hampered by believing one of the oversimplified models of science that have been proposed by philosophers from Francis Bacon to Thomas Kuhn and Karl Popper. The best antidote to the philosophy of science is a knowledge of the history of science."
Weinberg.

Best.

15. **teeth**
November 25, 2011

Another quote from Aristotle worth remembering, to gauge the measure of his wisdom: “Women have more teeth than men because they talk more.”

16. **iwanna prize**
November 27, 2011

Scornful of the award of the Dannie Heineman Prize for Mathematical Physics to seven people, leaving out Anderson?

The web page of the DH Prize says no such thing
http://www.aip.org/history/acap/institutions/heineman.jsp

Wiki even tells the 2012 winner to be Jona-Lasinio
http://en.wikipedia.org/wiki/Dannie_Heineman_Prize_for_Mathematical_Physics

Perhaps Anderson is scornful of the J J Sakurai Prize for Theoretical Particle Physics.
The award in 2010 was to six people (not seven samurai), but no Anderson in any case.
And the 2012 prizewinners are listed by Wiki
http://en.wikipedia.org/wiki/Sakurai_Prize

Anderson is being hypocritical. He wants a piece of a prize for theoretical particle physics? Anderson has long opposed HEP in toto. Anderson wrote a NY Times op-ed article opposing the SSC. Anderson may have written a book review of NEW, but I have heard Anderson lecture before, and what he would really like is NEHEP.

17. **spin**
November 27, 2011

Hey “iwanna prize”:

Like it or not, it was a condensed-matter theorist who first figured out the now so-called “Higgs” mechanism. Who says that he cannot win a prize for theoretical particle physics. He’s probably smarter than most of them.

I can’t help thinking that if the high-energy people gave Anderson his due credit at that time, maybe later on he would not so oppose the SSC and everything related.

18. **Peter Woit**
November 27, 2011

iwanna prize,

Thanks for the corrections re Anderson’s non-prize, he has this wrong in his book, but it’s clear he’s referring to the Sakurai prize.

19. **prizewinning**
November 27, 2011

Awarding prizes is a very subjective thing. Many if not most prize awards are controversial. The memory of the award of the Nobel Prize to Kobayashi and Maskawa and omitting Cabibbo is probably still fresh in people’s minds. And this was not even cross-disciplinary HEP/condensed matter. Any award of a prize for the “Higgs” (choose any other name you like) mechanism, and/or discovery, will almost certainly be heavily criticized. BTW Anderson already won the Dannie Heineman prize in 1975.

http://www.nndb.com/people/911/000099614/

However, it was the DH prize awarded by the Gottingen Academy of Science. It is awarded every two years and is a sister prize to the APS award, which is the Dannie Heineman Prize for Mathematical Physics. As the web page also states “Since winning his Nobel honors, Anderson has used his public platform to speak out against the “Star Wars” military missile program, and against federal funding for a proposed $8B superconducting supercollider in Texas.” It is a matter of record that Anderson was one of the most prominent condensed matter physicists who thought that termination of the SSC would cause the $8B to flow into funding for condensed matter physics. The truth is that after the SSC was terminated, there were across-the-board cuts in many areas of science funding. Basically, “if the HEP community can absorb an $8B loss to their program, then you can also absorb a sacrifice of $1B (or whatever) to your program”.

As for Salam, the whole discussion seems silly. The award of the Nobel to Salam did not deprive anyone else. Awarding the prize to Salam did not detract from the merits of Weinberg. Murray Gell-Mann put in a good word for Glashow, and Glashow deserves his share of the prize, too.

20. **J**
November 27, 2011

A side note to Charles’s comment about t’Hooft’s role in the Higgs crediting:

indeed, I recently attended a public lecture by t’Hooft on particle physics and standard model (for the general public). The ppt page on the Higgs mechnism listed only three names, i.e. Higgs, and Brout-Englert.

21. **anonymous**
December 3, 2011

Related link from National Post highlighting the Nobel Prize issues and The Infinity Puzzle book by Frank Close discussed here.

http://news.nationalpost.com/2011/12/03/large-hadron-collider-the-god-particle-six-creators-one-nobel-prize/

22. **Anonyrat**
December 7, 2011

One possible lesson from Frank Close’s narration is that the ideas that prove to be correct are often born with no seeming connection with experimental reality – it can take decades for that to happen.

Another possible lesson from the same book is that those ideas languish in minor modest obscurity and are not hyped, until their day comes, when they do connect up to reality.

If string theory is as successful some day as Glashow-Weinberg-Salam-ABEGHHK'tH etc, presumably it will be because they stop making TV programs about it.

The Ultimate Guide to the Multiverse

November 28, 2011
Categories: Multiverse Mania

Yet another cover story about the Multiverse can be found this week at *New Scientist*, which calls it The Ultimate Guide to the Multiverse. As just one more in a long line of such stories over the last decade, a trend that shows no signs of slowing down, one can be pretty sure that this is not yet the "ultimate" one, nor even the penultimate one.

The content is the usual: absolutely zero skepticism about the idea, and lots of outrageous hype from the usual suspects (Bousso, Tegmark, Susskind, etc.) We're told that scientists are now performing tests of the idea, even at the LHC. The LHC test has been a great success: Laura Mersini-Houghton used the multiverse to predict that the LHC would not see supersymmetry, and that prediction has worked out very well so far. There's a companion editorial Neutrinos and multiverses: a new cosmology beckons, which tells us that the multiverse is now orthodoxy, backed by "almost everything in modern physics":

> The widest crack of all concerns a theory once considered outlandish but now reluctantly accepted as the orthodoxy. Almost everything in modern physics, from standard cosmology and quantum mechanics to string theory, points to the existence of multiple universes – maybe 10^{500} of them, maybe an infinite number.
>
> If our universe is just one of many, that solves the "fine-tuning" problem at a stroke: we find ourselves in a universe whose laws are compatible with life because it couldn't be any other way. And that would just be the start of a multiverse-fuelled knowledge revolution…
>
> These are exciting, possibly epoch-making, times.

This past week also saw the premiere of the Multiverse episode of Brian Greene's *Fabric of the Cosmos* series on PBS. It's more or less an hour-long infomercial for the Multiverse, with the argument against it pretty much restricted to some short grumpy comments by David Gross about how he didn't like it. Brian's pro-multiverse argument was that many new advances in physics are all pointing to a multiverse, and he showed support for the idea as resting on a three-legged structure. One of the legs was string theory, and I've described elsewhere recently how circular reasoning makes this one very shaky.

The multiverse propaganda machine has now been going full-blast for more than eight years, since at least 2003 or so, and I'm beginning to wonder "what's next?". Once your ideas about theoretical physics reach the point of having a theory that says nothing at all, there's no way to take this any farther. You can debate the "measure problem" endlessly in academic journals, but the cover stories about how you have revolutionized physics can only go on so long before they reach their natural end of shelf-life. This has gone on longer than I'd ever have guessed, but surely it has to end sooner or later, and I have no idea what rough beast will slouch onto future covers of *New Scientist* and episodes of Nova a few years down the road.

Comments

1. **Quantumburrito**
   November 28, 2011

   I don't think the multiverse will end any time soon since people will always be suckers for fairy tales.

2. **Peter Woit**
   November 28, 2011

   Quantumburrito,

   Yes, but even most children get bored if you keep telling them the same fairy tale every night, and sooner or later start demanding a new one.

3. **Chris Oakley**
   November 28, 2011

   It could be that Susskind, et al have been told the answers to the "ultimate questions" by a race of hyper-advanced aliens, but have also been told to keep it quiet or risk being annihilated. String theory, 11 dimensions, multiverses and all the other B.S. is just their way of throwing the rest of mankind off the scent.

4. **grant davis**
   November 28, 2011

   Well, as long as Ed Witten and all the best and brightest at the Institute for Advanced Study pursue and exalt the untestable String Theory, instead of speaking out against it, expect the multiverse mania to continue.

5. **Giotis**
   November 28, 2011

   Inflation is now part of Standard model of Cosmology (supported by an increasing corpus of evidence) and almost all models of inflation predict eternal inflation and thus the multiverse. Thus the multiverse stands on solid theoretical ground regardless if String theory is correct or not. String theory though gave a boost to the multiverse idea because String theorists are the dominant group of the top American Universities and these Universities more or less determine what is orthodoxy and what is not. So indeed in the last decade we had a paradigm shift.

6. **Beelzebud**
   November 28, 2011

   I started watching that Nova episode, and turned it off before it was over. When he starts going on about how there is an exact copy of himself in an alternate universe, it just made the whole thing too silly to even take seriously.

   I'm open to the idea of "pocket universes", etc, but even if that's the case I don't see how anyone could draw the conclusion that they have copies of themselves in multiple universes. Is there some mathematics they are using to make this guess, because the show is so light on technical details I felt like a toddler he was talking to? Please tell me that these conclusion are arrived at based on something tangible. I just don't see why the universe would be making exact copies of human beings, or anything else for that matter.

   It's not hard to see why physics hasn't gone anywhere in 20 years, after watching part of that episode. I'm not even sure if he talked about one thing that can be verified with observation or experiment. He might as well be trying to prove that the bible is true.

7. **aa**
November 28, 2011

almost all models of fairys include leperchauns.

8. **Aidyan**
November 28, 2011

I’m not particularly in love with multiverse theories, tend more to be an agnostic. However, as far as I could see from some papers there are ideas to test the hypothesis and something has been already done (see e.g. http://arxiv.org/abs/1012.1995). So far data seem to dismiss it, but I wouldn’t say that the theory is (or will forever) be impossible to test.

9. **Peter Woit**
November 28, 2011

Aidyan,

I wrote extensively about that heavily-hyped “test” of the multiverse here

http://www.math.columbia.edu/~woit/wordpress/?p=3879

10. **the aethereal multiverse**
November 28, 2011

Omitting the internet and blogs etc., the aether theory was taken very seriously (hyped?) by distinguished physicists at the turn of the 19-20th centuries. Sir Joseph Larmor, the Lucasian Professor of Mathematics who preceded Dirac, wrote a monograph “Aether and Matter” in 1900
http://books.google.com/books/about/Aether_and_matter.html?id=jvZImrMMUcIC

But in 1905 Einstein came along and offered an alternative explanation of the observed phenomena, viz. the (Special) Theory Of Relativity, which did not require the aether hypothesis for the propagation of electromagnetic waves. And as events proved, the theory of relativity could do a better job of explaining things than could the aether theory. The aether theory did not go away of its own accord. It went away only when something better came along. Furthermore the aether theory was never explicitly disproved. It was simply abandoned. But Larmor was not a fool; he is remembered today for Larmor precession, if nothing else.

So today the multiverse is hyped, and there is tv and the internet etc. The multiverse is taken seriously by talented minds. It is also disbelieved by talented minds. But the ultimate arbiter will be the advent of an alternative theory, which can do a better job of explaining observed phenomena. Until that happens, the multiverse will not go away.

11. **grant davis**
November 28, 2011

dear aethereal multiverse,

but the multiverse explains nothing and cannot be tested. thus some other theory cannot out-perform it in the realm of explaining nothing and untestability, unless it is also not science.

there is no need, as you argue, to disprove non-science with science. simply put, the multiverse is not science. we do not need any additional science to somehow displace non-science. rather, the non-science of the multiverse ought be outed and then ignored because scientists are supposed to be moral, rational, logical, honest creatures.

🙂

12. **Peter Woit**
November 28, 2011

the aethereal multiverse,

I think there is a real danger that the people pushing the multiverse will be successful at what they’re trying to accomplish: getting people to give up on conventional scientific standards, and turning this into the new orthodoxy. One motivation for this is to avoid admitting the failure of string theory unification. If they’re successful, young people who enter the field will be taught that there’s no alternative to string theory unification, the problem has been solved by showing that it is inherently impossible to do better. As this becomes more and more entrenched the path for someone looking to do something better (and better than pseudo-science is not much..) becomes harder and harder. This is not like the beginning of the last century when there were a lot of experimental hints pointing in promising directions.

Interestingly, despite his dedication to string theory, David Gross is someone who clearly finds this too high a price to pay, at least so far. The disturbing trend I see is more and more physicists who should know better falling into line here.

But this is an argument about what the scientific orthodoxy will be. It may very well end up being the string theory multiverse. If so though, there are only so many years that science magazines can keep putting it on the cover as a new revolutionary idea. At some point there will have to be something new for the cover, and I’m wondering what it will be.

13. **Nathalie**
November 28, 2011

Dear aethereal multiverse,

I can’t resist adding to your statement “Larmor was not a fool …” that moreover Larmor had Lorentz transformations before Lorentz and some physicists in Leiden, including Lorentz himself, knew that.

14. **Beelzebud**
November 28, 2011

the aethereal multiverse,

“So today the multiverse is hyped, and there is tv and the internet etc. The multiverse is taken seriously by talented minds. It is also disbelieved by talented minds. But the ultimate arbiter will be the advent of an alternative theory, which can do a better job of explaining observed phenomena. Until that happens, the multiverse will not go away.”

What observed phenomena does a multiverse hypothesis describe? Honest question. I'm by no means an expert.

15. **bcs**
November 28, 2011

It seems many distinguished theorists committed themselves to the multiverse idea. Some are willing to bet their house or dog's life on it, some are only willing to bet his friend's house and dog's life. Whether they really believe it, or just try to find something to go beyond standard model… I don't know.

It is well known that some high-energy theorists consider condensed-matter physics probably not much better than chemistry, economics, or biology. There has been a long list of such people: Dirac (implicitly), Gell-mann, Pomeranchuk, etc. Of course, I myself don't think so.

However, at this particular juncture, I consider myself lucky that I can still do physics research the old honest way, guided by the experiments.

16. **Bernhard**
November 28, 2011

This was very depressing, as usual. I hope this will go away, I simply can´t believe HEP (and cosmology) are taking this course, we are risking going from respectful fundamental science to the laughable part of physics (if we already didn't), a new astrology.

My hope is that the at some point people will just stop funding this thing or at least to fund only exceptional cases. To make this mainstream is absurd. Pointless speculation after speculation, hype after hype these guys just won´t stop. At least it is clear the purpose they serve: show business and nothing at all to do with science or the truth. Makes me sick.

17. **Giotis**
November 28, 2011

Beelzebud

It explains the observed value of the cosmological constant and in general why the universe seems fine tuned for life.

There is no other explanation for that at the moment (and according to many there will never be one). The alternative is to believe in God…

18. **the aethereal multiverse**
November 28, 2011

Beelzebud – "What observed phenomena does a multiverse hypothesis describe?" None that I know of. The aether theory of 19C also failed to explain any observed phenomena. All attempts to detect the aether wind failed. (The Lorentz-Fitzgerald length contraction was born as an attempt to reconcile the null result of the Michelson-Morley experiment.) And yet the aether theory remained the orthodoxy.

grant davis – "… because scientists are supposed to be moral, rational, logical, honest creatures." That is the (Platonic?) ideal, which is clearly not happening, on a large scale. A lot of it has to do with lining one's pocket with salaries.

PW really hits the nail on the head (or punches the head on the nose, if you prefer) – "This is not like the beginning of the last century when there were a lot of experimental hints pointing in promising directions." They key is *experimental hints*. That is the really lacking ingredient in these modern times. And that leads to the danger of a change (or corruption) of the scientific orthodoxy. But all of that will only change for the better when new data (BSM) comes along, and an alternative theory which can do a better job of explaining it.

19. **DB**
November 28, 2011

Until prominent physicists step into the limelight to ridicule and debunk these charlatans the situation will only deteriorate. The likes of Greene, Kaku et al. have free reign because they encounter precious little pushback from professionals of far greater eminence than they.
There seems to be a culture of omerta, where one does not speak out against one's "colleagues" in public. Dirty linen is to be washed only in private. The fear seems to be that such arguments aired in public would damage physics funding generally by creating the impression that physics was wracked by disagreement and division.
From this perspective it seems that allowing the likes of Greene to peddle their nonsense is viewed as the lesser of two evils.

20. **a**
November 28, 2011

The problem is not the hype.
The problem is that this anthropic stuff could be true.

We should at least try to get some physics out of it, see e.g.

http://arxiv.org/abs/1111.2551
"Anthropic solution to the magnetic muon anomaly: the charged see-saw"

21. **Bernhard**
November 28, 2011

a,

This paper assumes that "the only scalar existing at the weak scale is the Higgs" and that the explanation for this lies in anthropic reasoning.

Even if I were to swallow the assumption for the sake of seeing what phenomenology comes out of it I absolutely not accept that the "explanation" for this should in any way be anthropic related.

22. **Aidyan**
November 28, 2011

Didn't they tell us that the atomistic hypothesis is mere metaphysics since we won't never have a microscope showing us atoms? (E. Mach) Who said that we will never be able to know the star's chemistry because they are too far away? (A. Comte) And what about that famous guy who told us that he "never frames hypothesis" but now we know from his manuscripts that he did so continuously? I think that history has shown how rejecting too quickly a theory only on the assumption that it will never be testable or because too speculative (here multiverses) is not reasonable.

23. **Peter Woit**
November 28, 2011

Aidyan,

I don't think you can argue away multiverse arguments trivially as in "we'll never observe other universes", but if you look carefully at the multiverse arguments being made (see for instance the link I gave you earlier) you find that they are very weak, full of holes, and outrageously overhyped. Before going to magazines and TV to claim a new scientific orthodoxy, multiverse advocates should have something more solid than "it's not impossible that someday we might somehow figure out how to get evidence for this, even though things don't look promising right now", which is where they are today.

24. **Aidyan**
November 28, 2011

Peter, ok this sounds better… 😉 As to the motives which stand behind the "outrageously overhyped" arguments I tend to subscribe to what Smolin describes in the (much less discussed and taken into consideration) last chapter of his book "the trouble with physics". Just an excerpt (you certainly already know, but also for others):

"Put simply, the physics community is structured in such a way that large research programs that promote themselves aggressively have an advantage over smaller programs that make more cautious claims. Therefore, young academic scientists [note: S. Feeney is a PhD student] have the best chance of succeeding if they impress older scientists with technically sweet solutions to long-standing problems posed by dominant research programs. To do the opposite – to think deeply and independently and try to formulate one's own ideas – is a poor strategy for success. Physics thus finds itself unable to solve its key problems. It is time to reverse course – to encourage small, risky new research programs and discourage the entrenched approaches. We ought to be giving the advantage to the Einsteins – people who think for themselves and ignore the established ideas of powerful senior scientists."

Sorry, if I advertise another book… 😉 But in my opinion he got to the root of the problem, which is not scientific but political/ideologic/sociologic. As long as the (unwritten) rules on which the academic system is based will not change it will remain a pious illusion that these things can change either.

25. **Peter Woit**
November 28, 2011

Aidyan,

What Smolin wrote applies well to the string theory story, but the multiverse one is much stranger. The multiverse remains quite unpopular among physics departments, with only a small number likely to hire someone working in this area (and lots of NSF panels likely to refuse to fund multiverse grant proposals). It seems to me that what is happening is that multiverse proponents are trying to do what string theorists used to accuse me of doing: going to the press and TV to try and win an argument they have lost with their colleagues. If they can get the public sphere to believe that this is now "scientific orthodoxy", they may break down the unwillingness of physics departments to put up with this kind of pseudo-science. I suspect this isn't going to work, that young multiversers will find that this is no way to get a permanent job. But, we'll see, the campaign continues, with multiverse mania still dominating the media, and only David Gross willing to try and do battle on those grounds.

26. **pravda**
November 28, 2011

Look at the situation from the other point of view ~ what's in it for anyone (including David Gross) to do battle with the multiversers? (or string theorists, or anyone else promoting hype …) Probably they just open themselves to a heap of abuse, and receive no real reward from "the establishment". How many physics departments (or govt labs ~ Fermilab? CERN?) encourage their staff to make public statements against "people who promote hype"? "Standing up for scientific merit" isn't exactly rewarded. Lots of people grumble about the hype, but to speak out is to label oneself as a troublemaker.

27. **Puzzled**
November 28, 2011

The "multiverse" PR campaign puzzles me, for a much simpler reason than most of the issues raised here… the issue is "what problem does it resolve?" I can't see any problem for which "multiverse" provides an "aha" moment. Most truly powerful scientific or mathematical ideas create that sense of "now I get it" when something truly paradoxical suddenly makes sense.

Maybe I misssed something in my 60 years of mathematical and scientific life that requires a multiverse (and/or the "anthropic" principles that beguile the media) to resolve. Otherwise, it's another "just so" story – plausible, entertaining, and just clever enough to impress the rubes.

While it's kind of sad that these things sell books and get grants, science is really not defined by the well-funded and most visible. Intellectual integrity ultimately defines science for those who do science. The best and worst part of science is that anyone can say they are a scientist, but in the end, the "aha" is the only real measure of science. (not the "coolness factor" and the folks that go to the best parties).

28. **Einstein's Bastard Son**
November 29, 2011

String theory first started to enter the public consciousness in the mid 80s. Your book and Smolin's weren't published until 2006. So I'm guessing that this new multiuniverse craze might last at least 20 years.

29. **Einstein's Bastard Son**
November 29, 2011

*multiverse

30. **Bobito**
November 29, 2011

The "multiverse" is basically a religious argument.

"God" is also a hypothesis that "explains" the observed character of the universe. Lack of explanatory power is not the reason that "God" is a bad answer to questions about the nature of the universe. Rather, it is a bad answer because such an explanation provides nothing, in operational terms, that was not available before.

The "multiverse" as explained by many is not so different from "God".

31. **John**
November 29, 2011

It is important to point the differences between the different kinds of multiverse, for example in this blog I have not seen a clear position respect to the work of David Deutsch on the quantum multiverse, Deutsch have two long books (one publish this year) and many papers on this subject but strangely there is not yet a post about this, I think this is a strange gap in the multiverse mania series. I think Deutsch ideas have not been consider seriously yet, mainly because ideas that include the word multiverse are rejected even before being considered, maybe would be useful to use constructions as strings-multiverse, inflation-multiverse, quantum multiverse to be more clear.

32. **Guillaume**
November 29, 2011

Yes, I'd be interested to hear what Peter thinks of Deutsch's quantum multiverse too. I've already suggested that a while ago.

Having skimmed through his latest book, my own opinion on that particular topic is that his version of the quantum multiverse requires an *uncountable infinity* of universes, and that's just unphysical.

I could say a lot more about his book, which contains some interesting stuff and a lot of outrageous crap, especially when he ventures out of pure science into the real of human affaires.

33. **besnard**
November 29, 2011

I find this multiverse bashing a bit sad. As Peter sais in response to Aydan, multiverse arguments are not trivially wrong. Generally speaking, a theory is not unscientific because it sounds crazy to some people. In fact, a single infinite universe already contains the same strange things as the multiverse. One has to remember also that most, if not all, physical theories make claims about entities that cannot be observed.

Finally, I agree that the claim "there is a multiverse" is by itself impossible to disprove. But the situation is entirely different if a theory predicts A, B, C, and D : "there is a multiverse", and A, B, C are usual falsifiable predictions. If it turns out that A, B, C are observed, then you have to believe in D until someone comes up with a better theory that explains A, B, C but does not imply D. The problem with multiverse hype is that there is at the moment no such a solid theory which happens to predict a multiverse (although I think a case could be made for eternal inflation).

This is why I think critics should concentrate on the theories, and not on the multiverse idea per se.

34. **Peter Woit**
November 29, 2011

John and Guillaume,

The "multiverse" of various interpretations of QM is a completely different issue than the string theory landscape pseudo-science one. In the QM case, you may be able to perfectly sensibly talk about a "multiverse", but you can also describe exactly the same theory and same physics without invoking "other universes". Because of this I confess to a distinct lack of interest in this kind of debate over interpretation of QM, it seems to me to just avoid the interesting issues in favor of empty arguments over language. So, I haven't read Deutsch's books and so don't have anything to say about them. I did read a recent New Yorker profile of him and wrote a bit about it here:

http://www.math.columbia.edu/~woit/wordpress/?p=3656

My reaction there was that his argument that the existence of a quantum computer meant you had to have a multiverse didn't make sense to me. This is still true, but, again, my main problem is that I'm not convinced it's a non-empty issue worth thinking much about.

35. **SteveB**
November 29, 2011

One aspect of this problem is the economic one. There seem to be more new books being published than new, interesting and testable ideas. When a popular, well spoken physicist writes a new book they make the circuit of the radio shows (Fresh Aire, Science Friday, etc.), also the magazines, and eventually Nova TV. All of these need popularity to survive. Popularity is dependent on the public's "gee whiz" factor — and in my opinion — more important than Science, truth, or usefulness.

I found Greene's Nova series to be totally lame, with no real content, but just cute. unnecessary CGI . What a waste of 4 hours of potential science programming. New scientist is producing more and more of these silly articles and they are always highlighted on the cover. Almost every week! What a waste! (We have heard from a NS contributor here before, maybe she will provide some inside info about what the heck is going on there.)

Give me some dinosaurs or underwater archaeology anyday. I'd like some TV shows on new astronomical observations, on solid-state physics, on the electrical grid.

These just don't have the popularity for the American public who do believe fairy tales and vote for advowed anti-science candidates.

Sad times….

36. **Beelzebud**
November 29, 2011

Giotis,

That sounds an awful lot like just trading one myth for another one that you prefer more.

37. **Trevor Turton**
November 29, 2011

Lots of good, strong comment here. Is the take-home message Multiverse == astrology 2?
This is of course a math forum debating some deep physics questions, and one could be forgiven for thinking that the math guys are just sour because they haven't been made an offer by the physics funding machine.

38. **Joel Rice**
November 29, 2011

The trouble with the LHC is that it has not put a nice juicy Discrepancy on the table, like a Lamb Shift to cast doubt on Dirac, so there would be a real problem to work on. It is curious that they want to speculate about multiverses rather than why muons exist.

39. **why muons exist**
November 29, 2011

Joel Rice – Thanksgiving is over, but at Christmas dinner I suggest you engage the family in a discussion "why are there multiple families of leptons?" Don't blame anyone but yourself if you are dunked in a pot of boiling oil.

Trevor Turton – "… one could be forgiven for thinking that the math guys are just sour because they haven't been made an offer by the physics funding machine." The vaunted "physics funding machine" is regularly starving for money, so if the math guys are sour, I can only imagine they receive negative funding.

40. **Puzzled**
November 29, 2011

Just a quick comment… afaik "epicycle" models of planetary orbits were never proven "wrong", per se. In fact, it's a small exercise in Fourier analysis to develop a "sum of complex exponentials" (e.g. circular orbits) that would approximate any ellipse to any degree of accuracy you want. That ellipses are simpler solutions to a differential equation formulated according the invers-square gravitation than sums of complex exponentials is nice for planetary orbit calculations.

But one could also use the Fourier decomposition as a more general way to solve a much larger class of theories about gravitation (including some problems in GR). So maybe there are two "aha" ideas here: Fourier analysis, which is a truly great idea (and more or less the same as epicycles) and Kepler's Laws, which is a truly great idea about observed physical reality. But one could keep epicycles and still have Newton's Law… and you still need a version of "epicycles" calledFourier analysis to do good solutions to large classes of differential equations.

Which is the "truth"? There is no "law of the excluded middle" here… both are simultaneously likely to be true. I'd suggest forgetting about truth, and focus on what a theory gives us as ways of knowing. There multiverses and string theory both are pretty barren deserts.
Falsifiability is not enough, nor is

41. **Coin**
November 29, 2011

Bernhard: "the only scalar existing at the weak scale is the Higgs"

It would be awfully interesting to compile a list of anthropic-reasoning arguments whose conclusions are, or sometime after being made turn out to be, in conflict with experiment. It seems like you should be able to find plenty of anthropic arguments which are structurally as sound as the ones multiverse advocates commonly cite, but produce factually incorrect results…

TAM: "'This is not like the beginning of the last century when there were a lot of experimental hints pointing in promising directions.' They key is *experimental hints*. That is the really lacking ingredient in these modern times."

You know, this is an oft-repeated thing, and I understand what you and Peter mean by it, but I don't really feel like it's *true* and it seems to get less and less true all the time. It seems like we keep stumbling over all kinds of fascinating experimental hints all over the place, but it's not convenient or doesn't come in the form physicists know how to easily consume it (ie a scattering diagram) so we scratch our heads and go "huh, that's funny, well let's see what the LHC finds". Dark energy is the biggest, most burningly difficult to ignore example here. Colliders might not have found any new particles in decades, but astronomy *has* (my understanding is that the bullet cluster observation can only be explained by the existence of a WIMP-like particle). Every time we poke at neutrinos we find more strange things about them– the OPERA result is probably BS, but "how fast do neutrinos move and what is their rest mass?" seems like an incredibly tantalizing question which no one seemed very interested in until the OPERA BS result threatened Einstein. And in areas I don't understand so well we seem to be learning more about inflation all the time, UHECRs are a thing, quantum computers are coming online…

And a lot of these "hinty" areas of physics, it seems to me we could be pushing a LOT harder than we are, there are whole experimental avenues that could be full of even stranger things but which we're just not really exploring. In particular look at all the astrophysics probes that have been cancelled since 2006 (Google "beyond Einstein program", it's sad) climaxing with LISA finally biting the bullet just a few months ago– astronomy has been one of the most productive sources of new experimental physics data in the last ten, fifteen years but there seems to be very little interest in or pressure toward funding astrophysics.

I'm not sure I'm qualified to make a statement like thus, but– I really feel like it's possible someone a hundred years from now could look at this period and see not a period devoid of experimental hints, but a period when we were awash in hints we just didn't understand what to do with and leads it didn't occur to us to explore.

42. **Peter Woit**
November 29, 2011

All,

A reminder: this isn't a general physics discussion forum, there are good reasons why I refuse to moderate such a thing. The topic might be kind of dumb and tedious, but if what you want to write about is something else, please restrain yourself…

43. **archytas**
November 29, 2011

Peter,

I read Deutsch's "Fabric of Reality" (not to be confused with "Fabric of the Cosmos"). As I recall, one of his main arguments for the many worlds interpretation of qm had to do with a photo's behavior in a Mach–Zehnder interferometer. He argued that the photon was interfering with itself in another universe. I am probably not doing it justice without an exact quote. I only mention this, because I know there was more to his argument than just the computational power of a quantum computer.

It was an interesting read, but one cannot help but be bothered by the ontological cost the MWI, which is infinite by my estimate.

44. **Bernhard**
November 30, 2011

Coin,

The problem with these kind of arguments is that they are never unique and cannot be made to disprove the theory. You can, as you said, come up with hypothesis with verifiable consequences and claim they each one of them is anthropic inspired. But what's the point? What is happening is clearly that only one part of the argument is really being tested. If a multiverse could be tested by simply saying "if anthropic reasoning is right than there must be a single Higgs scalar and the consequences are A, B and C. If not, the multiverse idea must be abandoned", than I'm all for it. But that is clearly not what's happening. If this supposed anthropic argument fails, you just come up with another one, if fails, with another one and so for. Eventually I can see someone making the correct assumption but I doubt there is any reason to believe it would be correct because of anthropic reasoning, even if one can trivially find a connection with it.

45. **Sascha Vongehr**
November 30, 2011

You may like to consider that many worlds and multiverses are much much older than those guys you apparently do not like.
More hype for the multiverse “fary tale” right here:
http://www.science20.com/alpha_meme/modest_agnostics_expect_multiverse_and_hold_many_worlds_true-85038

46. **Joel Rice**
November 30, 2011

What bothers me about the sorts of things Brian Greene says is the implication that there is no particular necessity that the universe we observe be this way, other than being in a Bang where the fine tuning goes our way – as if that is the only reasonable way to look at fine tuning. Where is the necessity of having a particular kind of collection of particles? If that structure of matter is determined by some algebra then all universes ought to be equivalent – the necessity of this universe having electrons is the same necessity in any universe. It is a structural thing rather than a matter of coupling constants. Of course, if particles are stringy then one has a lot more flexibility – the question is whether there is too much flexibility. I always wonder if we are not skipping over algebraically elementary stuff when we are trying to explain physically elementary stuff. Things might be fine tuned for reasons that have nothing to do with coupling constants – if algebra determines what associates with what, and coupling is just a phenomenological device for description. The Multiverse says you do not get the big picture from looking at this universe. Is that an algebraically reasonable thing to assert ? It seems to follow from their assumptions. One might say we don’t see supersymmetry because of the multiverse, but I am more comfortable saying we don’t see it because it isn’t there.

47. **dane**
December 3, 2011

thanks for sharing this! im gonna bookmark your site!!

48. **lun**
December 5, 2011

One of the “usual suspects” is currently giving a talk in Columbia,essentially about this paper
While being skeptic,I have to admit this IS kind of thought-provoking, and he does have a kind of a non-trivial post-diction (he calls it a prediction). If it was really true that one can relate the cosmological constant “coincidence” to the (in principle calculable) number of states in the landscape,it would be something approaching a phenomenology.

49. **anonymous**
December 7, 2011

The big revelation next week will not be a discovery or signal claim or anything close to it. Only an elimination of an energy range for the boson.

More Higgs Non-News

November 30, 2011
Categories: Experimental HEP News

The latest Higgs non-news is that there is news about when there will be news. The Scientific Policy Committee at CERN will meet on December 12 and 13, with the agenda for December 13 featuring a 15 min presentation by the CERN Director-General on "CERN plans for communications on the Higgs boson search at the LHC" in the morning. This will be followed in the afternoon by a public event including half-hour updates on the SM Higgs searches from each of the experiments, and a "joint public discussion" about what it all means.

Before the LHC I tended to be 50/50 on the odds for a SM Higgs vs. no SM Higgs. As data has come out during the past year, and rumors have arrived in recent months, my take on these odds has gone back and forth. First it looked like maybe there was a 140 GeV Higgs, then not. Then I was hearing that nothing was being seen by either experiment, followed by rumors about something being seen by one, but not the other, making a SM Higgs start to look unlikely. Lately though, I'm starting to hear that maybe both experiments are seeing something in the Higgs to gamma-gamma channel. Is it at the same mass? What's the statistical significance if you combine the results? Looks like we'll hear about this on December 13 (unless someone leaks the news to a blogger first…).

For now, I'm back to 50/50. According to the latest Higgs coverage in the New York Times, back in 2005 Frank Wilczek was willing to give 10/1 odds in favor of the Higgs (although he wants a SUSY version), at least if the stakes were in Nobel chocolate coins.

Update: This month's Physics World has a long article by Matthew Chalmers (not available free online I think, see here) about the search for SUSY. In includes details about David Gross's SUSY bet (with Ken Lane):

> SUSY is "alive and well" according to the Nobel-prize-winning physicist David Gross of the Kavli Institute for Theoretical Physics in Santa Barbara, who helped to create quantum chromodynamics – the theory of the strong
> force. "People shouldn't pay too much attention to the bounds now because it's signals that matter," he told Physics World. "When will I give up on SUSY? I have a serious bet with Ken Lane that it will be found after 50
> inverse femtobarns ," he says.

The LHC won't accumulate that amount of data until quite a while after it comes back up at or near design energy in 2014. So, it looks like Gross won't have to pay off his gambling debts until 2015 or so.

Comments

1. **chris**
   December 1, 2011

   begging for rumors, eh? well, I'm dying to hear them, too of course.

2. **Frank Quednau**
   December 1, 2011

   I've been looking for a betting shop where you can bet on the Higgs boson being found within a period of x or not, but couldn't find one. Anyone knows?

3. **Peter Woit**
   December 1, 2011

   Frank,

   If such a thing exists, one obvious problem is insider trading. There are 6000 or so physicists with some sort of access to internal CMS/ATLAS data, smaller groups with more detailed access to the latest analyses of data, and these people all have friends…

4. **Philip Gibbs**
   December 1, 2011

   From the title is looks like we now have news of a date when they will give us news about how the news will be communicated when they have it. If it helps, an anonymous commenter at viXra says CMS and ATLAS will each give 30 minute talks about new Higgs results after the council meeting, but I don't see that on indico yet and it is not certian that they will be public even if they really happen

5. **a**
   December 1, 2011

   ATLAS and CMS do not speak among themselves, and experimentalists at CERN are very strict. So, the people that already know what ATLAS and CMS will announce are theorists working away from CERN.

6. **Philip Gibbs**
   December 1, 2011

   The seminars are real and public, http://indico.cern.ch/conferenceDisplay.py?confId=164890

7. **aa**
   December 1, 2011

   intrade has several higgs markets
   observed before dec 31 is currently 17%

8. **Blendletan**
   December 1, 2011

   If you are interested in betting on Higgs, http://www.intrade.com/v4/markets/?searchQuery=higgs

The odds being given are kinda strange if you ask me, btw…

9. **Chris Oakley**
December 1, 2011

Thanks, aa and Blendletan for the link to intrade. I just signed up and sold 29 x December 2011 Higgs. This cannot be called insider trading as I left HEP in July 1987 (I would have happily put the same trade on then, BTW). It is not irreligious either as, in my humble one, calling it the "God" particle is one of the stupidest things ever.

10. **DB**
December 1, 2011

Tomasso Dorigo believes that the hints of a Higgs at 119 GeV (+/- 2) as seen last summer by CMS , of which he is a member, will be vindicated in due course. He offered me 3/1 but while I may be sceptical, those odds were not good enough to bet against.
Initially, ATLAS didn't see these hints, but there have been discussions between both groups and it may be that after some adjustments to their processing they now also see something.
In any case, it would be unreasonable, given that the LHC is poorly positioned to find a low mass Higgs, to expect an early decisive conclusion.
But if ATLAS is now seeing something similar, and if the additional data since the summer conferences show even a slight improvement on the initial signal, then this would be compatible with the sort of evolving discovery pattern one might expect from the LHC in the low mass regime.
An early result could be provided by the Tevatron data – which has better resolution in the gamma-gamma channel – concerning a 95% exclusion of Higgs. The Tevatron dataset will never be able to confirm a Higgs to 5 sigma, but it can exclude one @95%. But all's quiet on that Western front. A couple of months ago some at CERN tried to argue that a 95% exclusion on the Higgs would not be enough, but they failed to explain why the LEP2 exclusions below 114 GeV, which were also only 95%, would not then also need to be revisited. But this distraction seems to have taken a backseat for now.

11. **Thomas Larsson**
December 1, 2011

Exactly what does "Observation of the Higgs Boson Particle" mean at intrade? 5-sigma? Published? Announced at some conference? Announced by whom? (ok, probably a spokes-person for atlas or cms).

I'm willing to bet against 5-sigma by Dec 31, 2011.

12. **SD**
December 1, 2011

@ Thomas Larsson: from the rules tab in
http://www.intrade.com/v4/markets/contract/?contractId=700241

"Clarification (Jan 5th 2009): for the Higgs Boson particle to be "observed" there must be a "five sigma discovery" of the particle."

There are 126 shares at $0.5/share (where $10 ($0) means (no) discovery): I think you are not the only one willing to bet against it. 😉

13. **Chris Oakley**
December 1, 2011

SD,

It is trading at $1.60 now, so if you "sell" then you get $1.60 if it is not found, but have to pay out $8.40 if it is; reverse these if you choose to buy. The inferred probability of finding the Higgs before 2012 is therefore 16%.

14. **ohwilleke**
December 2, 2011

The kind of disclosure format that is in the works sounds more like an exclusion than a find.

15. **Frank Quednau**
December 2, 2011

well, thanks for the link to the "intrade" platform, didn't know that one yet 🙂

16. **SD**
December 2, 2011

Chris,

If you want to sell (buy) the price is $0.5 ($1.59), so the average probability is 10.5%. The spread reflects the uncertainty of this prediction, as quantified by "the market". In practice if you want to make money by betting against the Higgs being discovered by 2011, you would earn $1 for every $20 that you invest (but I think that there are also rather large commissions, so you might actually get no money at all). So I think that, though I'm 99% sure of no 2011 Higgs discovery, I'll keep that money in my wallet, ready to pay (part of) a nice dinner.

17. **Kent Traverson**
December 2, 2011

In 1997, when the SETI Institute was conducting a scan of the sky for any alien signals, a strange one was detected. While the staff quietly tried to figure out what it might be, one of the workers there ran to call his girlfriend that they may have found The Signal. She spread the news from there and in a matter of mere hours the rumor grew exponentially.

The signal turned out to be from a satellite of terrestrial origin. But the moral of the story is, humans are a bunch of big gossips, so you should be getting your Higgs rumor news in no time. And you will share, won't you?

18. **Eric**
December 2, 2011

“So, it looks like Gross won’t have to pay off his gambling debts until 2015 or so.”

Well, if the Higgs mass turns out to be 119 GeV or so as rumors suggest, then I think that Gross will most likely be winning his bets. This is exactly the mass range predicted by the MSSM.

19. **Peter Woit**
December 2, 2011

Eric,

Since 119 GeV is exactly the mass predicted by the MSSM, I guess if the latest rumors about a 125-126 GeV Higgs turn out to be right, that’s it for the MSSM, no?

20. **Vince**
December 2, 2011

Eric said “This is exactly the mass range predicted by the MSSM.” The key words are “mass range”, not “119 GeV is exactly the mass predicted by the MSSM.”

21. **Eric**
December 2, 2011

Peter,

As Vince points out, I did not say that the MSSM predicts exactly 119 GeV for the Higgs mass, but rather that this is in the range predicted in the MSSM. This range is essentially less than 130 GeV down to 114 GeV, including the LEP limit. A 125 GeV Higgs would also favor SUSY.

22. **Peter Woit**
December 2, 2011

Eric,

I see, the “exact” prediction of the MSSM is that the Higgs has a mass not already ruled out by experiment…

23. **Eric**
December 2, 2011

Peter,

The upper limit on the Higgs mass (130-135 GeV) in the MSSM has been known for a long time, and it is no accident that the remaining viable mass range for the Higgs is consistent with this. If the observed Higgs mass is within this range, which may very well be the case, then this implies that superpartners will be observed. So the point I am making here is not that SUSY predicts the exact mass of the Higgs, but that observation of the Higgs in the mass range preferred by SUSY implies that low-energy SUSY exists and will be observed at the LHC.

24. **Anonyrat**
December 2, 2011

UK Telegraph coverage of the Higgs search:
http://www.telegraph.co.uk/science/large-hadron-collider/8928575/Search-for-God-Particle-is-nearly-over-as-CERN-prepares-to-announce-findings.html

25. **Mitchell Porter**
December 3, 2011

Eric said

“The upper limit on the Higgs mass (130-135 GeV) in the MSSM has been known for a long time, and it is no accident that the remaining viable mass range for the Higgs is consistent with this.”

In what sense is it *not* an accident? Isn’t “the remaining viable mass range” a result of LHC search strategies, not of theory?

26. **Martin**
December 3, 2011

> Before the LHC I tended to be 50/50 on the odds for a SM Higgs vs. no SM Higgs. As data has come out during the past year, and rumors have arrived in recent months, my take on these odds has gone back and forth.

Hi Peter,
I have always thought you were a SUSY sceptic. Now you seem to be positioning yourself as undecided. Or does “no SM Higgs” mean no Higgs at all rather than a SUSY Higgs instead?
Cheers,
Martin

27. **Peter Woit**
December 3, 2011

Martin,

There I meant no Higgs at all (i.e. no elementary scalar field playing the role of the Higgs). I’m still a SUSY skeptic, even if there is a Higgs at 125 GeV…

28. **crandles**
December 5, 2011

Re: Betting at intrade

What would you like to see in the way of scientific betting?

Intrade wrote

https://www.intrade.com/forum/
"What would be the time span involved in this market? Mid-2012, end of 2012?
How would the existence of Dark Matter be confirmed? What criteria would you suggest?

We haven't had a great record with scientific markets in the past, but if we have a short-medium term markets (no longer than a year out) that will generate interest and trading volume we'd consider it. "

Maybe they need scientists to ensure questions are well posed.
Is it too late for: Confirmation of detection of WIMP in Earth based laboratory?
Does confirmation mean peer reviewed paper published?

I don't think intrades fees at $5 per month regardless of number of contract traded is much, but bank charges for conversion to US$ and transfer to Ireland and then back again mean it is probably not worth looking for a certain 5% return before fees unless you are going to leave the money there for other opportunities. For that you may need or want more scientific contracts. So suggesting more contracts when intrade asks might make sense.

A 125-126 GeV Higgs?

December 2, 2011
Categories: Experimental HEP News, Favorite Old Posts

Some more detail on Higgs rumors I've been hearing recently. Evidently the latest ATLAS data shows an excess in the gamma-gamma channel around 126 GeV, of the size expected if the Higgs is there, and CMS is also seeing an excess (2 sigma?) around 125 GeV in the same channel. I haven't heard anything about confirmation of this in other channels. Independently, someone has posted a [similar rumor at viXra log](), and Philip Gibbs is writing about it [here](). This looks to be still not a conclusive Higgs signal, but the closest thing yet. More details may or may not emerge before the public talks on December 13.

Update: Latest rumor is that the significance of the ATLAS gamma gamma bump is almost 3 sigma.

Update: This morning's rumors are a 3.5 sigma 126 GeV excess at ATLAS in the ATLAS-only combination, and 2.5 sigma at 124 GeV for CMS. Heuer's message to all CERN personnel says the December 13 announcements will be "significant progress in the search for the Higgs boson, but not enough to make any conclusive statement on the existence or non-existence of the Higgs." Presumably they're waiting for 5 sigma before claiming conclusive proof.

Comments

1. **Jester**
   December 2, 2011

   That would be weird, given that in the summer dataset both ATLAS and CMS had a deficit in gamma-gamma around 125 GeV…

2. **Mark**
   December 2, 2011

   According to Motl, this is very close to Kane's favorite value 127 GeV !

3. **Amos**
   December 2, 2011

   Has to be close to *someone's* favorite value.

   Gibbs says "at this mass the standard model has problems with vacuum stability that are likely to require supersymmetry or something similar to stabilize."

   Can someone explain that?

4. **Marc Sher**
   December 2, 2011

   Amos—if the Higgs mass is 125 GeV, then the quartic self-coupling of the Higgs, which (like all couplings) changes as the energy scale changes, will become negative at an energy scale well below the Planck or unification scale. That means the vacuum becomes unstable. Supersymmetry would cure this, and so would many other theories. Since the energy scale at which the self-coupling goes negative is well above the reach of accelerators, it is possible that whatever new physics solves the problem can't be seen in accelerators. Of course, we all hope that the new physics is at the TeV scale.

5. **Cliff**
   December 2, 2011

   Gordon Kanes work has been really interesting to me even before the rumors of a 125-127 GeV Higgs… So this result has me a little bit excited.

   You can confirm from his presentation here that 127 GeV is indeed the preferred value (see the conclusions page of the slide):
   http://conferencing.uwex.edu/conferences/stringpheno2011/documents/kane.pdf

   I actually downloaded this a while ago, so I can confirm that he didnt just make a different pdf for each possible mass haha

6. **anonymous**
   December 2, 2011

   Take a look at this one: The Standard Model (SM) plus gravity could be valid up to arbitrarily high energies. Supposing that this is indeed the case and assuming that there are no intermediate energy scales between the Fermi and Planck scales we address the question of whether the mass of the Higgs boson m_H can be predicted.
   http://arxiv.org/abs/0912.0208

7. **Henry Bolden**
   December 2, 2011

   Wired magazine has an article about impending Higgs news (or non-news):

   http://www.wired.com/wiredscience/2011/12/higgs-lhc-anticipated/

   Disclaimer: No Brian Greene has been harmed in the making of this message.

8. **Paolo Valtancoli**
   December 3, 2011

   I remember that an indian guy applied the four colour theorem to the SM finding that the best value for Higgs mass is (2 M_W + M_Z) / 2, i.e. a 125 GeV Higgs mass!

9. **Frank**
   December 3, 2011

   Doesn't the primary decay of a 4th quark family b' quark also involve a photon? Then a b'-b'bar would produce gamma-gamma signal also, confusing the interpretation until a complete analysis determines the spin of the original particle.

10. **Peter Woit**
December 3, 2011

Frank,

In principle lots of things could produce a gamma-gamma signal. But remember that the SM Higgs predicts exactly the width and size of such a signal, and my understanding is that this is consistent with what the experiments are seeing.

11. **Bernhard**
December 3, 2011

Width ans size may be and of course since it is consistent it will be tempting to interpret the signal as a SM Higgs, but until its spin is measured, which will take a lot of data, even this consistency can be put into question.

12. **Peter Orland**
December 3, 2011

Hi Marc Sher (who I used to know at Santa Cruz many years ago),

Though the basic thrust of your argument is right (i.e. that scalar fields are problematic at high energies), it is not true that the quartic Higgs coupling becomes negative. I am pretty sure you are thinking about Landau's pole.

I hope Peter W. will indulge my discussing the technical issues, so to avoid causing too much trouble, I'll be brief. He may permit my comment because they directly concern the Higgs mechanism.

Landau's formula for the effective coupling is only good for small couplings. It breaks down at high energies, where it predicts the sign change. The pole/sign change is fictitious.

The real problem is that if the coupling at high energy (say with a Planck-scale cut-off) is not fine-tuned to some enormous value, the coupling at the TeV scale is nearly zero. This can checked by the renormalization group in 4-epsilon dimensions, by 1/n-expansions and by numerical simulations. It is also strongly indicated by some rigorous results about triviality, but these don't quite work in four dimensions. There is no change in the sign of the coupling.

Some field-theory books present the Landau pole as gospel, even though it is completely unphysical. The real problem is triviality/fine-tuning.

13. **emile**
December 3, 2011

Bernard,

How long it takes depends on the mass of the Higgs (assuming it exists). With a WW signal, the spin can be checked without that more data IF there is a Higgs and that the mass is not too far from 130. In the case of WW->lnln analyses, both experiments use a delta(phi) cut between the two leptons which assumes a spin 0 particle decaying to two vectors. That distribution can be measured too. If there is a Higgs and if the mass is in the 115-120 range, then it would be a lot harder and would indeed take a lot more time.

14. **anon**
December 3, 2011

What I heard: CMS's 119GeV peak in gammagamma is still strong, but another at 124GeV appears.

15. **Thomas**
December 3, 2011

Peter O:

I believe Marc does mean what he wrote (Higgs self coupling goes negative). This is due to the contribution from Yukawa couplings (primarily the top), which will drive the Higgs coupling negative if m_top/m_H is too large. This is known as the vacuum stability bound (and it has been checked in lattice calculations).

16. **Peter Orland**
December 3, 2011

Thomas,

This is something I should know about. Thanks, I'll look into it.

17. **Younghun Park**
December 4, 2011

Peter and everyone
If the signal remains at 3~4 sigma level until 6th 1212, what will CERN conclude?
They will conclude that Higgs exists or not?
If the level of the signal doesn't come up to 5 sigma, what will happen?

18. **Younghun Park**
December 4, 2011

I'm sorry. 6th 1212 -> june 2012 or december 2012

19. **Amos**
December 4, 2011

Peter:

Thomas' comment was enough to locate this article : http://arxiv.org/abs/hep-ph/9307342v1 which seems to sum it up.

20. **Marc Sher**
December 4, 2011

Peter–Thomas and Amos are correct, although the reference Amos gives is quite outdated–the papers of Quiros and others are more updated with the proper top quark mass (http://arxiv.org/abs/hep-ph/9703412 is a good summary). It has nothing to do with the Landau pole. The quartic coupling goes "negative" above some scale, or if you wish to not think about negative couplings, solve the RGE for the Higgs potential and there will be an instability at high scales. But frankly, since few people believe it's just the SM up to huge energy scales, it doesn't matter that much.

21. **Jeff M**
December 4, 2011

Pardon me for asking, but aren't 124 GeV and 126 GeV miles apart? I mean even physicists wouldn't claim to have found a particle with a mass of "about 125GeV," would they? Is someone expecting one or the other of these numbers to move a percent or two when more data comes in? I don't see how you could possibly combine these results to claim you have found something, if anything it would indicate exactly the opposite.

22. **Cesar Laia**
December 4, 2011

I guess the peaks come out as distributions centered at 124GeV and 126GeV. So if you combine both experiments we get a larger and broader peak with a 125GeV mean value.

23. **Jeff M**
December 4, 2011

@Cesar,

Yes, I'm sure that is the case, and I'm sure the statisticians have ways of combining that sort of data, but I have issues with that even in more "normal" cases like population sampling. Guess it's just that I'm a mathematician, and I just don't trust statistics about things like this, too much flexibility with your assumptions. Could just be that I never liked the SM, elegant as it is, too many parameters…

24. **Peter Woit**
December 4, 2011

Jeff,

You can look up the expected width of the bump, I think it's very roughly 2 GeV or so. Remember, right now the bumps are near the limit of statistical significance, not something well-resolved where you can precisely locate a peak. The numbers being quoted are also not as precise as possible. It could very well be that the ATLAS peak is near 125.5, the CMS one near 124.5. So, the small discrepancy in mass value is probably not significant.

Yonghun Park,

As you accumulate better statistics, if there's a Higgs there, the statistical significance should increase. If there's nothing there, it should decrease. Both statements are probabilistic, but with very high probability if substantially more data is accumulated.

25. **anonomous**
December 4, 2011

Are there other channels besides the WW signal to verify the results or is this the only data that will verify the Higgs existence? What does the Tevatron data say about this channel? How much more data collection (and how long) is required going into 2012 to get to the 5-sigma level?

26. **SpearMarktheSecond**
December 5, 2011

I think the natural width of a Standard Model Higgs at M_h=125 MeV or so is only… 5 MeV.

http://www-library.desy.de/preparch/desy/proc/proc02-02/Proceedings/pl.1a/haber_pr.pdf

I think detector resolution broadens that observed full width to maybe 3-4 GeV, or a sigma of about 1.5 GeV.

http://hal.in2p3.fr/docs/00/36/46/31/PDF/LAPP-EXP-2008-09.pdf

27. **SpearMarktheSecond**
December 5, 2011

Whoops M_h=125 GeV.

28. **Jack Lothian**
December 5, 2011

Being both a statistician & former physist 124 & 126 could be the same point – depends how many points were measured in the empirical distributions, how smeared out the empirical peaks are in the two distributions & the standard deviation of the underlying distributions. I am sure somebody has figured this out at CERN. It is such an obvious point that I suspect that they are checking this point closely.

29. **The show must go on**
December 6, 2011

STRING/M SUSY theory PREDICTS the HIGGS mass to be 126 ± 2 GeV
http://arxiv.org/pdf/1112.1059
in full agreement with the fully correct rumors.

By the way, can I post the main plots that will be presented on Dec 13?

30. **Mark**
    December 6, 2011

    @The show must go on, in case you did not know, Kane had made this exact point about the Higgs mass, including the plot, a while ago. See the corresponding post on Motl's blog.

31. **Bernhard**
    December 6, 2011

    The only prediction that was confirmed up to now is that whatever one finds at the LHC is is of course a prediction of string theory, despite the fact that is this very thread people pointed out to other articles (e.g. Shaposhnikov and Wetterich) that make a even more accurate prediction regarding the Higgs mass (I'm not saying the model is correct) and very importantly in 2009. Kane's article was sent on Monday December 5 when the whole world already heard the rumors, really, what kind of "prediction" is that? It may be that Kane wrote about it before, but THIS specific article cannot be used as a serious prediction. Best case scenario is a postdiction.

32. **Eduardo**
    December 6, 2011

    It is interesting to note that from global analysis of low energy data~(electroweak data, with high statistics but very low energy),
    e.g., In Fig 1 of arXiv:1110.5807 [hep-ph]~(and references therein) , always appears an small "bump" between 120-130
    (there is a shark peak between 115-120, but that is due the Lep2 constrains).
    The bumps coming from global data analysis have different meaning~(They are not so crazy as the colliders or any Single experiment. It remains for years and are very difficult to wash away)

33. **brazilian**
    December 6, 2011

    Hello guyz, I'm not a scientist, I just like reading about science, and I don't know if it's my english that is really bad (I'm brazilian), but I can't until now undertand the implications of string theory and SUSY in the search for the higgs boson, can someone help me? I was reading about it, some blogs, comments, etc, and I got some understandings…

    What I did understand is that string theory (M theory included) predicts SUSY, but if SUSY is not found, that's OK, string theory can survive without it, correct? Then, I read that SUSY predicts some particles to be found at LHC, but if you don't find, that's OK, because it can be hiding somewhere else, I got it right? Then, about the higgs mass, if what I understood is correct, SUSY accept a really good range for it's mass, but if the higgs is about 125GeV, even if it minimize the need for SUSY to explain somethings, that would be a prediction from SUSY, if you find it in some other areas, you'll need SUSY to stabilize the system, and if you don't find it at all, then you need SUSY again!

    That's correct? I mean, or my english is really bad or SUSY is such a lank theory that can answer just about everything and cannot be proven wrong, that's really weird.

34. **Ronan**
    December 6, 2011

    Here is @iansample 's blog entry from today's Guardian in the UK

    http://www.guardian.co.uk/science/blog/2011/dec/06/is-higgs-boson-real

    It includes some reactions from Glashow, Wilczek, Randall, Veltman, t'Hooft ….etc

35. **Peter Woit**
    December 6, 2011

    brazilian,

    String theory has nothing at all to say about the Higgs, it makes no prediction about this at all. So, discussion of the pros and cons of string theory is off-topic here.

    The show must go on,

    You certainly can send me copies, and I'll summarize for our readers.

36. **Predictor**
    December 6, 2011

    It's interesting that somebody compared the Kane "prediction" with the "prediction" of Shaposhnikov et al. They're are pretty much on the same footing: pure wishful thinking.
    I mean, it's OK to play that kind of game, but you shouldn't call that a prediction. In the case of Kane, because of the huge amount of implicit or explicit assumptions. In the case of Shaposhnikov et al, because of the lack of any theory behind the whole construction.

37. **Jeff McGowan**
    December 6, 2011

    I just love it when I get people on a *physics* blog trying to teach me elementary mathematics 🙂 My issue about 124 vs 126 GeV is my surprise that the HEP peaks are wide enough to allow something like that, combined with my mathematicians inherent mistrust of things like the assumptions that go into statistical calculations like combining peaks. Actually, I'm kind of curious, for things like particle signatures, what are the theoretical rationales for what distribution you expect to see?

38. **Peter Woit**
    December 6, 2011

    Readers of other blogs who wonder about some disturbing material about me they might see there should refer to

    http://www.math.columbia.edu/~woit/wordpress/?p=4127

    and ignore it.

39. **Bernhard**
    December 6, 2011

Peter,

off-topic: will you comment on this paper: http://arxiv.org/abs/1112.0788 at some point?

40. **Coin**
December 6, 2011

So if there is a 3.5 sigma at 126 GeV with the current ATLAS analysis, and a 2.5 sigma at 124 GeV for the current CMS analysis, then when the combined analysis of both ATLAS and CMS is done next year which has been mentioned here previously– if the signals are real, what sigma would one estimate the combined analysis could probably get up to with this existing data? (Is this even a sensible question?)

41. **Peter Woit**
December 6, 2011

Bernhard,

Traveling today, will probably write something about that tomorrow.

Coin,

Philip Gibbs is the expert on this….

42. **JollyJoker**
December 6, 2011

Coin, about 4.3 sigma.

43. **David Ewan Kahana**
December 7, 2011

Interesting rumours, indeed, and I am confident
the Higgs will be found in the range 122-132 GeV, having
predicted its mass together with the top quark mass
in a composite model, before either top or Higgs were
detected. But I'ld point out there were similar rumours
of an excess in the b anti-b channel indicating a Higgs
in the range of 130-140 GeV just a couple of months back
(personal communication from W. Marciano). It was a similar
deal, a couple of sigma each in CMS and Atlas, which added
to a bit more than 3 sigma. It went away of course.

My last look at the 2 gamma data, with about
half of the total data set analysed showed points above
and below the theoretical continuum, and no
sign of a bump whatever. The SM Higgs width at this
mass is so small that I don't even remember the number
but I believe it's on the order of 1 MeV. So in this case
the width of any bump in the 2 gamma mass spectrum
will be determined by detector resolution, on the order
of 5 GeV. There was an extra factor of two available
in the integrated flux not analysed at that time, but
that only gives 40% better resolution of any bump
at 125GeV. So I can't believe this will be conclusive.

A SM Higgs this light just escapes the vacuum
stability and metastibility (due to finite temperature
effects in the early universe) if new physics only
appears near the Planck scale.

So it's premature for Kane and the supersymmetricians
to be rejoicing, I think. They should rather be worrying
about the absence of supersymmetry at 95% confidence
level, below about 1 TeV. Exciting times!

about 50%

44. **Jozef Šima**
December 7, 2011

The Higgs mass (125 GeV) was calculated, along with other parameters of the Universe, based on the model of Expansive Nondecelerative Universe, see our paper J. Šima, M. Súkeník, Nondecelerative Cosmology – Background and Outcomes, published in Pacific Journal of Science and Technology 12(1), 214-236 (2011) and included references.

45. **David Ewan Kahana**
December 7, 2011

I suppose it's only polite to leave references that can be checked, when one claims to have predicted something, so here are links to my various predictions of the top and Higgs masses.

I seem to remember that the major uncertainties at the time these predictions
were made was the value of the strong coupling at the Z mass. And we were using one loop beta functions for the evolution. But no theorists I talked to at the time believed in a top mass as a high as 160 GeV.

Standard-model bosons as composite particles

Phys. Rev. D 43, 2361–2368 (1991)
http://prd.aps.org/abstract/PRD/v43/i7/p2361_1

Prediction: Higgs ~140 GeV, Top ~ 160 GeV

Preprint (1993):
(Top and Higgs Masses in Dynamical Symmetry Breaking
http://arxiv.org/abs/hep-ph/9312316)
Prediction: Higgs ~ 125 GeV, Top 175 GeV

Publication:
http://prd.aps.org/abstract/PRD/v52/i5/p3065_1
Prediction: Top~180(185) GeV and Higgs ~130(135) GeV.

46. **pfogle@tiscali.co.uk**
December 7, 2011

sorry if this is a well known fact, but how correlated would the errors from Atlas and CMS be? Presumably there are shared system errors?

thanks.

47. **democrito**
December 7, 2011

Hi Pfogle,

the uncertainties of the two experiments are mainly statistical at this stage, and completely independent. There is, in truth, some strange common choice for background shape parameterizations (both experiments are using Bernstein polynomials for the background in their H->gamma gamma searches, which is odd at the very least, since the choice is not particularly bright), but most of the stuff is completely orthogonal. In particular, backgrounds in the gamma-gamma are data-driven, so no MC dependence is there.

48. **David Summers**
December 7, 2011

Well I guess now that I'm out of academia, I can comment without stepping on anyones toes – as I've had not direct contact with experimenters working in the field in 10 years!

Anyway the photon-photon channel is interesting, because only spin 0 particles can decay into two photons. So if a bump is seen in the photon photon mass – then it must be some kind of spin 0 particle, e.g. the Higgs.

So its just how big is the bump over the photon photon background. Alass don't have the code I once had, so can't do some simple calcuations of the various cross sections. Nice thing is though that for signals like this the significance of results builds quickly. So its at 3 sd now – give it another 6 months or so (or rather the next run), and it should be a very significant signal then.

Interesting though to hear about my PhD subject coming back to life …

49. **David Summers**
December 8, 2011

Oh yes – what I also should have said, is whats interesting about this channel is it only demonstrates a higgs in the electro-weak channel; e.g. responsible for mass of the W and Z; but non necessarily responsible for generating quark masses.

To establish the latter they would need to look for the b bbar branching ratio of the Higgs, and that will be a very messy signal down at 125GeV – probably can't be seen in all the muck.

So does make it an interesting time for high energy physics …

50. **Suzie**
December 8, 2011

Sorry to contradict you, David. How do you think the Higgs is being produced in the first place?

51. **Sascha Vongehr**
December 9, 2011

Hope you don't mind my extensively quoting from your blog here:
http://www.science20.com/alpha_meme/light_higgs_discovered_and_about_destroy_universe-85357

52. **Yatima**
December 11, 2011

Sascha Vongehr writes

> Low higgs max expected
> Universe about to decay – possibly due to experiments in the LHC
> Nobody should care because Many Worlds theory

I must confess I have rarely read anything weirder outside the most psychotronic layers of the Internet Subconcious' Alluvials.

freud.jpg

53. **Bernhard**
December 11, 2011

http://www.nature.com/news/where-s-my-higgs-1.9609

An interview with Joe Lykken, with the cherry-on-top comment to that "brilliant" article from Kane: "I would say it would be an example of a successful connection between string theory and experiment."

54. **McPhee**
December 11, 2011

Tuesday will be mostly a confirmation that there is enough data to narrow search to the 115 to 130 range with some hints of a signal (likely around 125). These hints will be less than 3 sigma.

55. **David Summers**
December 13, 2011

Suzie,

Only just check back here – taking my mind back 15 years to when this was fresh …

Higgs of about this mass at pp colliders are mainly produced by a pair of W or Z bremed of quarks in the t channel – these then merge to form the Higgs. So this is sensitive to WWH and ZZH couplings.

There is also a contribution from gluon fusion, via a top loop. That is sensitive to H t tbar coupling, and so mass generation for quarks. From what I recall though at these masses its t channel vector boson fusion which dominates – my memory though could be wrong, so I'd be happy to be contradicted ….

David.

String and M-theory: answering the critics

December 7, 2011
Categories: This Week's Hype

Mike Duff has a new preprint out, a contribution to the forthcoming *Foundations of Physics* special issue on "Forty Years of String Theory" entitled [String and M-theory: answering the critics](). Much of it is the usual case string theorists are trying to make these days, but it also includes vigorous ad hominem attacks on Lee Smolin and me (I'm described as having an "unerring gift for inaccuracy", and we're compared to people who campaign against vaccination "in the face of mainstream scientific opinion"). One section consists of a rather strange 3-page rant about Garrett Lisi's work and the attention it has gotten, a topic that has just about nothing to do with string theory.

Duff explains that his motivation for answering the critics is that we have been successful on the public relations front, supposedly responsible for the British EPSRC "office rejecting" without peer review grant proposals on string theory. I know nothing of this, but I think it's clear to everyone that the perception of string theory among physicists has changed, and not for the better, over the past decade. One dramatic way to see this is to notice that at this point, US physics departments have essentially stopped hiring string theorists for permanent appointments (i.e. at the tenure-track level).

String theorists have a problem not just with the public, but with their colleagues. The main reason for this is not Smolin or me, but the failure of the string theory research program. Duff's take on whether the landscape is pseudo-science is that string theory can't even tell whether there is a landscape, and he is "doubtful whether the kind of issues we are considering here will be resolved any time soon." On the question of the time scale for possible progress, he invokes the two millennia it took to get from Democritus in 400 BC to quantum theory early last century. His list of greatest achievements of string theory in recent years has just two items: applications to fluid mechanics and his own work on entanglement in quantum information theory. Given this, it's hard to see why he's surprised the EPSRC is cutting back on support for string theory.

While Duff has detailed complaints about exactly what Smolin wrote in *The Trouble With Physics*, he mentions my book without saying anything about what is in it (one suspects his policy of how to deal with it is that of Clifford Johnson and some other string theorists: refuse to read it). He does have some specific complaints about material from my blog:

- According to Duff:

> he wrongly credits me with having told author Ian McEwan about the Bagger-Lambert-Gustavsson model in M-theory, which he then proceeds to criticise.

This is based on a [book review]() about Ian McEwan's novel *Solar*, where I wrote about M-theory references in the book that "McEwan seems to have gotten this from Mike Duff, who is thanked in the acknowledgments". Since Duff is an expert on these topics and the only particle theorist thanked, this was an obvious guess, worded as such. In this review I wasn't criticising M-theory, just noting an interesting occurrence of it in popular culture. My only criticism was of McEwan, for the minor anachronism of a topic from 2007 showing up in a book set in 2000. In a segment from the novel that I quoted, one character is expressing opinions about M-theory research which you could call critical, but this material was written by the novelist, not by me (and I'm still wondering where McEwan would have gotten this from, other than from Duff).

- In two cases, Duff claims that I misrepresented his words on the blog. Both are cases where I wrote the blog entry based on information from someone who had heard him talk, since I didn't have access to his words themselves when I first wrote the blog entry. In general I try to be very careful about what I quote, making sure it is accurate and in context. In these cases, what was reported here was clearly labeled as someone else's impression of his talk, and Duff has some reason to be annoyed at not being quoted accurately, although it wasn't me doing it.

The first case was a posting about the [debate in 2007 between Duff and Smolin]() (see also [Clifford Johnson's blog](), which includes comments from Smolin), where one attendee [described]() the scene following Smolin's talk as:

> Smolin sat down. Duff stood up. It got nasty.
>
> The trouble with physics, Duff began, is with people like Smolin.

The transcript actually shows:

> Good evening everyone. The trouble with physics, Ladies and Gentlemen, is that there is not one Lee Smolin but two.

followed by an extensive description of Smolin as deceptive and two-faced, saying completely different things at the debate and in his book. From the transcript, I'd describe the "Duff stood up. It got nasty" part as completely accurate, the "The trouble with physics, Duff began, is with people like Smolin" much less so.

- The second case has to do with a [posting]() about a recent BBC program on superluminal neutrinos, where Duff discussed string theory explanations for this. Based on two e-mails people had sent me who had watched the program, I wrote that it "evidently featured trademark hype from string theorist Mike Duff about how string theory could explain this." The first commenter, who had also seen the program wrote in "I watched this tonight and can confirm that it did include stringy hype." Duff complains that

> I said that, although superluminal travel is in principle possible in the "braneworld" picture of string theory, in my opinion this was NOT the explanation for the claims

It still seems to me that going on a TV program to claim string theory as a possible explanation for this kind of experimental result can accurately be described as "hype", even if, since no one believes the experimental result, you express the opinion that string theory isn't the right explanation in this case.

Duff is much less interested in the virtues of accuracy when he describes my words. I guess I've joined Smolin on his list of targets because of what I've had to say on the blog (see [here](), [here]() and [here]()) concerning his publicity campaign claiming a "prediction of string theory" about qubits (recall that he thinks this is one of the two main advances in string theory of this decade). He claims that "falsifiability of string theory is the single issue of Peter Woit's 'single-issue protest group'", and that my argument about the qubit business "may be summarised as (1)It's wrong (2)It's trivial (3)Mathematicians thought of it first." One can read the postings and decide for oneself, but I'd summarise the argument quite differently: Duff has nothing that can possibly be described as a "prediction of string theory" and it's misleading hype to issue press releases claiming otherwise. The experimentally testable "prediction" is that "four qubits can be entangled in 31 different ways", but if experimentalists make measurements of four qubits that show something different, one can be sure that the headlines will not be "string theory shown to be wrong in a lab".

Duff's article contains an appendix about this, in the form of a "FAQ", where he explains that he approved the text of the press release headlined "Researchers discover how to conduct first test of 'untestable' string theory" which is misleading hype by any standard. Initially someone who was successfully misled in the Imperial media team added the subtitle "New study suggests researchers can now test the 'theory of everything'", which was later removed. Duff claims that Shelly Glashow, Edward Witten and Jim Gates told journalists that they didn't agree with this because of the "theory of everything" subtitle, implying that otherwise they were fine with the "first test of 'untestable' string theory" business (except for Gates noting that in any case this is just supergravity, not string theory). It would be interesting to hear from the three of them if they're really on-board with this "first test of 'untestable' string theory".

What Duff and some other string theorists don't seem to understand is that this sort of "answering the critics" is exactly what has gone a long way to creating the situation at the EPSRC that he is worried about. Unfortunately it has damaged not just the credibility of string theory, but of mathematically sophisticated work on particle theory in general. According to Duff

Just recently, in fact, EPSRC completely abolished its Mathematical Physics portfolio.

Update: Matin Durrani at Physics World (also a target of Duff's ire) has a blog entry about this here.

Update: Lee Smolin sent me the following comments on the Duff article:

> Maybe it would help if I provide some context for the debate Mike Duff took part in with Nancy Cartwright and myself in London in 2007. The occasion for the debate was the publication of my book TTWP in the UK and the reason for the debate was that I had insisted that, as the point of the book was to explore the role of disagreement and competing research programs in science, the best way to illustrate it was to have a debate. String theory was discussed in the book as a case study illustrating the issues and so it seemed appropriate to have a debate with a string theorist. I also insisted that in each of these debates a philosopher of science would be included to highlight the fact that the main themes of the book were longstanding issues in philosophy of science, having to do with how consensus forms within a scientific community on issues on which there is initially wide disagreement.
>
> There were two such debates in the UK, the other was at Oxford with Philip Candelas and Simon Saunders. That went very well, as Philip gave a strong defence of string theory that stayed focused on the scientific issues.
>
> Duff's construction of two me's is, so far as I can tell, a debating tactic to avoid addressing the key issues my book raises. He starts with
>
> "Who can dispute that the ultimate goal of a scientific theory is to make experimentally testable predictions? Who will challenge the need to keep an open mind and listen to unorthodox views? Who can disagree with the assertion that our current understanding is only partial and that the ultimate truth has yet to be uncovered? What Lee Smolin said in the London debate was so uncontroversial that, had I confined my response to these remarks, the evening would have would have fizzled out in a bland exchange of truisms."
>
> Indeed the constant theme of my book is the development of those "truisms." What Duff does not explore is that in spite of the agreement there may be over these "truisms", they have strong consequences for the evaluation of research programs in fundamental physics. Apparently we disagree about the implications for string theory. What Duff could have done is acknowledged these disagreements and explored the reasons for them. Instead he claims to attack my book, but it is striking that he does so, not by criticizing the text I actually wrote-but by attacking first the publicity blurb on the cover and then responses from journalists. As I have stated many times, the material on the cover was neither my text nor my choice and is more strongly worded than anything in the actual book. I hope it is obvious also that you cannot attack a book by pointing out inaccuracies in reviews.
>
> When he finally does get around to quoting from the book, he makes a few good points mixed in with distortions gotten by quoting out of context. Had he stuck to the good points he had we could have had a useful debate that would have shown the audience the role of disagreement among scientists faced with difficult questions. Had he done that, there would have been no need to construct a fiction of two me's. I am happy to leave it to readers of my book to judge whether its text is or isn't completely consistent with the "truisms" he asserts we agree about.
>
> There is one aspect of Duff's rant which deserves correction, which is his attack on me related to Garrett Lisi. What Duff says is, "So when Lee Smolin described him as the next Einstein, the publicity juggernaut moved into overdrive". There are several untruths in this short sentence.
>
> First, this refers to a Discover article of March 2008 which says, "With Smolin's aid, DISCOVER has scoured the landscape and found six top candidates who show intriguing signs of that Einsteinian spark" of whom Lisi is one. This was, so far as I recall, based on a phone call with an editor at Discover following a piece I had written for Physics Today on the challenges faced by those who do high risk-high payoff research. I think anyone who looks up the full list of six will see that the editors were aiming to illustrate a wide range of approaches to fundamental research, of which Lisi is at one pole. And as they make clear-the choice of the list was theirs and not mine.
>
> Furthermore, the media attention on Lisi had begun and peaked already in November of 2007, sparked by a New Scientist article, following immediately the posting of his article on arxiv.org. And while there was a very exaggerated media response-which I and others did our best to advise against-there was no "publicity juggernaut" ie no attempts by Lisi or anyone to seek publicity for him, no press releases, no publicist, no calls to journalists except to strongly advise the story was premature. I told everyone who asked not to write a story on Lisi because the preprint had just been uploaded and there had not been time for experts to evaluate it. Indeed, New Scientist had quoted me very much out of context, ignoring emails I sent them advising them not to write a story on Lisi's paper before the experts could evaluate it. So the reality was the opposite of the impression created by Duff's sentence.
>
> None of this is new, none of it is said for the first time. It is depressing to revisit these debates from five years ago. Most of us have moved on. At least I have, as readers of my next books, as well as the article I was invited to write for the same special issue, will, I hope, see.

Update: The following is Garrett Lisi's response to the Duff article. I should note that I'm not complaining about Duff's listing of my titles. If you want to make up your mind who is right based on titles, Duff's your man in this argument.

> Michael Duff's article is full of deceptive half-truths. To attack the commentary on Lee's book, while avoiding Lee's actual arguments, is just one example of this fundamentally dishonest tactic. A similar example is his reference to Peter as "Computer Administrator and Senior Lecturer in Discipline," as if Peter was not also a very knowledgable mathematical physicist. Duff then launches an attack on my work, once again focusing on a large volume of commentary by others rather than on my actual arguments. Also, Duff refers only to my first paper, saying it's never been peer-reviewed and published, avoiding the fact that I've since published papers on the theory, including "An Explicit Embedding of Gravity and the Standard Model in E8."
>
> Scouring Duff's rhetoric, baseless statements, and ad hominem attacks in search of some factual argument supporting his attack on my work, I can find only this:
>
> "Nature (and the standard model of particle physics) has three chiral families of quarks and leptons. 'Chiral' means they distinguish between left and right, as they must to account for such asymmetry in the weak nuclear force. But as rigourously proved by Jacques Distler and Skip Garibaldi, Lisi's construction permits only one non-chiral family."
>
> This is, once again, a misleading half-truth, avoiding the fact that a chiral family of quarks and leptons can be part of a non-chiral representation space, as is the case in E8. I cannot credit Duff alone for this deception, as its source is Jacques Distler — a master of the half-truth — but I can blame Duff for supporting it.
>
> For anyone who actually cares about the state of E8 theory, I would recommend my recent papers. Apparently Duff considers the work sufficiently threatening to the string program that he needs to attack it in this dishonest manner. If string theory models are as twisted and misleading as the statements in Michael Duff's paper, it's no wonder they're dying.

Comments

1. **Ms Quote**
   December 7, 2011

   I suggest you delete the last quote "According to Duff … EPSRC completely abolished its Mathematical Physics portfolio." This is a ripe candidate for misquotes later on.

Find out from someone in the EPSRC (if possible) to confirm this (and then cite them). Mathematical physics is a big discipline. It is unlikely to be cancelled in toto.

2. **Peter Woit**
December 7, 2011

Anyone who gets to the bottom of that post is likely to realize that "According to Duff" indicates high probability of inaccuracy in whatever follows. I'm guessing his claim does have some sort of relation to reality, presumably to this issue (in which case this is not just about mathematical physics, but mathematics in general)

http://www.dpmms.cam.ac.uk/~bt219/epsrc.html

or more specifically to this statement from EPSRC

http://www.cms.ac.uk/summaries/EPSRCreply.pdf

If any readers know more about this and can point to authoritative information, I'll add an update about this to the posting.

3. **Frank**
December 7, 2011

Would you like to comment this article:

http://physicsworld.com/cws/article/news/48045

and

http://arxiv.org/abs/1108.1540

4. **Peter Woit**
December 7, 2011

Frank,

Not really. Life is too short to pay much attention to claims about string cosmology. From what I remember there were plenty of claims made 10-20 years ago about how string theory explained 3 space dimensions.

5. **LHB Jr**
December 7, 2011

In the paper, he says that Smolin called Lisi "the next Einstein." He fails to point out that Ed Witten is mentioned as a candidate for "the next Einstein" in the very same article, along with 4 other people with diverse backgrounds and viewpoints.

Seems to me that calling somebody "the next Einstein," and naming somebody, along with a handful of others, as a possible candidate for being "the next Einstein" are two different things.

6. **Clara**
December 8, 2011

Peter,

Duff's paper is so full of vague phrases and so empty of physics that to a casual reader it sounds like the rant of a typical unemployed who is jealous that other people have a job.

40 years without results is a track record that no theory in physics has ever achieved, especially with so many and so smart people.

Why are these people so obsessed to ride a dead horse? The obsession is barely understandable. There seems to be some religious drive behind it. You speak to many string theorists. Why are they doing this? What collective vision drives this considerable boy's network into the wrong direction?

7. **Bee**
December 8, 2011

First I was thinking how awful this thing got published in a journal I previously considered to have a pretty good reputation, probably entirely without peer review. Now I'm thinking, this will make an interesting piece for the history of science 100 years from now. I'm sure future students will have something to laugh about that one.

8. **felix**
December 8, 2011

It's incorrect to attribute the lack of job positions for mathematical physics to the influence of string theory. With or without string theory, mathematical physics is always a niche. For example, there is this beautiful research field of integrable models in statistical mechanics, started by people such as Onsager and Baxter. In addition to its obvious relevance to physics, it has connections with a wide range of mathematics including quantum groups, graph theory, topological quantum field theory, elliptic curves and so on. However, if you do a PhD in such a field, your chance of landing a job is even slimmer than a string theorist in this post-string hiring environment.

In fact, string theory *is* serious mathematical physics, as long as you ignore the ultra-speculative part such as the multiverse. There are too many examples, one of which is the AdS/CFT correspondence. People can argue about how precisely real systems can be approximated by AdS/CFT, but if you take a mathematical physicist's point of view and don't insist on immediate physical applicability, AdS/CFT is definitely one of the most striking achievements in modern mathematical physics.

By any reasonable standard, string theory has only *created* jobs opportunities for people pursuing mathematical physics, a field that's often overlooked by funding agencies. The real sad part is that you have to rely on hype about Theory of Everything to get funding for mathematical physics. Just look at what we have got into after string theorists were no longer getting many job positions. The jobs are now going to "phenomenologists" who are never tired of writing a 10,000th paper on MSSM or inventing a new variant of a new model for dark matter every day. This is a group of people whose vision and whose standard of work, overall speaking, are infinitely worse than string theorists.

9. **Crackpotl**
December 8, 2011

I can remember good old pre-Woit/Smolin days, when 100% of popular articles on string theory were fawning and uncritical. And then Woit/Smolin started their vicious smear campaign, the result of which is that 90% of popular articles are fawning and uncritical of string theory, and 10% voice skepticism to varying degrees.

10. **piscator**
December 8, 2011

The comments on EPSRC are off-base, there was a much bigger issue with EPSRC than simply a narrow restriction on not funding string theory or mathematical physics. In its forthcoming (now current?) fellowships round, EPSRC at one point announced that the they would only consider mathematics applicants doing applied probability (I think that was it). That is number theory, combinatorics, analysis, algebraic geometry etc (and yes mathematical physics) – if you were a young postdoctoral/junior faculty mathematician doing any of these areas, you were de facto ineligible for funding.

Unsurprisingly this caused a major rumpus between the research council that funds mathematics and the UK mathematics community. I haven't followed it all, and I don't know to what extent this policy was bad communication and to what extent a not-so-secret desire to fund only 'practical' areas. I think if you dig around on Tim Gowers' blog you can find the details.

11. **piscator**
December 8, 2011

In particular try this

12. **Nigel**
December 8, 2011

The Horizon "faster than light" BBC2 Horizon program used a political trick (maybe due to BBC editing, not necessarily completely controlled by Duff) of Duff first hyping superstring theory for ages using a loaf of bread to illustrate how a neutrino might take a short cut and arrive faster than light, and then at the end including a brief statement by Duff that he a rigorous scientist and wasn't hyping anything.

Horizon on BBC2 did the same trick using Sir Paul Nurse earlier this year in a different documentary which tried to lynch James Delingpole for asking questions in a different science. The trick is used by politicians over here. You first give a long subjective argument full of one-sided biased hype, then just when everyone is brainwashed, bored and changed channels, you inject a brief disclaimer to look objective.

It's the two-way bet used extensively by politicians, lawyers and the media here. Whatever happens, you claim credit. If you're right, you're right. If your hype is wrong, you highlight the brief disclaimer as "proof of rigorous objectivity".

Duff should stick to mathematical physics or defending non-mainstream ideas, not overhyped groupthink religion (disclaimer: this is just a suggestion, not a threat).

13. **Bobito**
December 8, 2011

Whatever one thinks of Lisi's preprint, no article considering him a potential Einstein (whatever that means) should be taken seriously, as such a claim is not well-founded in anything he has written and distributed publicly.

14. **Carl**
December 8, 2011

First they ignore you, then they ridicule you, then they fight you, then you win.

15. **Carl**
December 8, 2011

@Clara: One possible explanation is our old friend cognitive dissonance. When someone has sunk their career and their reputation, and in some cases the careers of their graduate students, into a field of study that increasingly looks to be petering out, that person is likely to do everything in their power to avoid acknowledging the reality.

Or as Hans Christian Andersen might have put it, when a child shouts out that the emperor has no clothes, it's a lot easier to have the child tossed into a dungeon than to admit that you've been parading around naked in public.

16. **Mike**
December 8, 2011

Here is a link to Gordan Kane's (and others) new paper talking about a predilection for the Higgs derived, it is said, from String Theory:

http://arxiv.org/abs/arXiv:1112.1059

I was curious whether you thought this was significant, or whether you thought that it could simply be a function of "backing into" the mass. By that I mean, noting the chatter about the potential predicted Higgs in the 124 to 126 range, and then, given String Theory's notorious "flexibility", choosing specific constraints and inputs that arriving at the projected Higgs mass?

17. **Joel Rice**
December 8, 2011

I've been reading Schweber's "QED and the men who made it." What a contrast to the speculation going on these days.

18. **Peter Woit**
December 8, 2011

Mike,

That's another topic to be addressed another day. Surely we're going to see in coming days/months a long list of claims by people that their model predicts the right Higgs mass, whatever it is. Kane has been promoting a long sequence of susy/string theory models over the years that supposedly make "predictions", evolving to fit whatever the latest data shows. If the Higgs really is at 125 GeV, it's right in the middle of a range that for 10 years or more has been by far the most likely one (from LEP and precision electroweak constraints). Everyone making "predictions" has concentrated on finding models with a Higgs in this range.

Anyway, this is off-topic. In Duff's favor, unlike Kane he admits that string theory isn't now able to make real predictions about the Higgs mass.

19. **Pheelios**
December 8, 2011

I am PhD student in conformal field (just finishing) in a UK mathematics department, I heard about the ESPRC cutting funding to string theory months ago from my supervisor who is worried about the funding implications it will have for him. The reason I gathered for the cuts was because prominent pure mathematicians in or related to the ESPRC hated string theory and thought the idea of a string theorist being let anywhere near a mathematics student was unacceptable. Personally I don't see why string theory shouldn't be funded from the pure mathematics budget, the only real benefits that either discipline confer to the average taxpayer is the mental training they give to their undergraduate and graduate students who then go on to apply it in the real world. As long as the level of teaching coming from a string theorist is the same as the level of teaching from a pure mathematician I don't see why the average taxpayer should be too concerned about the level of rigour that either discipline applies to it's own highly esoteric and detached research schemes.

20. **Peter Woit**
December 8, 2011

Pheelios,

I don't know what's going on in the UK, but my impression is that in the US, while many physics departments have become unhappy with string theory and are not hiring in that area, the situation in math departments hasn't changed much. Mathematicians have always had some interest in the math coming out of string theory, and are unconcerned about whether it works as a theory of everything. However, for a math department to hire someone, they need to be convinced that what they are doing has interesting mathematical implications, and most string theory work doesn't meet that standard.

21. **Bob Levine**
December 8, 2011

@Pheelios,

Do you actually find it plausible that objections from a few 'prominent pure mathematicians' would have so much influence with the ESPRC, when we've just been given a huge body of evidence (available in the links provided in this thread) that ESPRC has ignored virtually the entire mathematics research community in the course of gutting their fellowship programs so that only statistics and applied probability theory candidates are eligible for funding? Just about everyone who is anyone in the British mathematics community has signed one or another strong protests to the ESPRC about this radical new approach to setting funding priorities. It's quite clear that what ESPRC intends to support exclusively are branches of mathematics which are perceived as having immediately industrial applicability, with support eliminated *across the board* for highly theoretical foundational research. This policy shift seems more than sufficient to explain whatever defunding of work in string theory has gone on, quite apart from ST's failed track record.

Interestingly, one of the chief complaints in the mathematicians' response to the ESPRC fellowship cuts is that decisions at the agency are being made by people with no expertise whatever in the field; indeed, Delby seems to think that this is a *good* thing. It seems highly unlikely, given the the culture of the agency, that guidance from pure mathematicians on *any* point of policy would be taken seriously. So it might serve you well to dig a little deeper into the information you've received on this point before accepting it uncritically.

22. **Curious Wavefunction**
December 8, 2011

One dramatic way to see this is to notice that at this point, US physics departments have essentially stopped hiring string theorists for permanent appointments (i.e. at the tenure-track level).

Yes, but couldn't this also be because of the bad job market in which *nobody* is really getting hired? I guess my question is, if they are not hiring string theorists, who *are* they hiring then?

23. **chris**
December 8, 2011

LHC phenomenologists.

24. **Peter Woit**
December 8, 2011

Curious Wavefunction,

Hiring is definitely down across the board, but physics departments are still hiring theorists, just mostly phenomenologists. Dark matter or LHC phenomenology is popular. See

http://www.physics.utoronto.ca/~poppitz/Jobs94-08.pdf

and

http://particle.physics.ucdavis.edu/rumor/doku.php

for data.

25. **Marcus**
December 8, 2011

Chris, you might want to check that. What you say (LHC phenom'sts) doesn't accurately represent what I see here:
http://particle.physics.ucdavis.edu/rumor/doku.php?id=archive:2011

I see one offer to a string theorist (Baylor U. offer to Antonino Flachi) and a total of 10 offers marked "accepted" with two others apparently undecided because not marked "declined".

So 12 outstanding offers in HEP-theory as a whole. When I looked down the list I remember seeing quite a few cosmology/astrophysics people. You might want to make your own count. It seemed to me that these people were doing varied and interesting research, which could not all be lumped into "LHC phenomenology."
I'd actually be very interested to know how you or a knowledgeable HEP-theorist would sort the list of 12 out. We might learn something we didn't automatically expect by looking at how North American physics departments are behaving.

As a comparison, if you look back at the three-year period 1999-2001 the annual rate of firsttime HEP-theory faculty hires was about 18 per year, of which string

represented about 9 per year—essentially half of first-time faculty hires in US and Canada. The change from 9/18 down to 1/10 or 1/12 is remarkable—makes me wonder if there is something wrong with the statistics. But I am relying on what a HEP theorist at U Toronto has posted here:
http://www.physics.utoronto.ca/~poppitz/Jobs94-08
You can see his reservations and caveats by visiting his site and form your own impression. (I have a pretty high opinion, but check it out.)

26. **jg**
December 8, 2011

Mathematical Physics is still funded by the STFC (Science and Technology Facilities Council), funding for Standard Model and Beyond the Standard Model physics are 2nd and 3rd highest funded on the current list (~£84 million), Theory has another £19 million awarded

I think 2012 will be tough year financially for the UK, it'll be amazing if the Olympics break even. Come 2013, with better financial prospects and maybe LHC inspired promotion for particle physics the funding agencies may get generous again

27. **JC**
December 8, 2011

felix,

I guess one of the problems with string theory research is that its supporters make far too much claims about the real world physics based on a bunch of speculative ideas sugared with sophisticated mathematics.

I very much agree with what Phil Anderson said in his review about Peter's book:
"One may particularly cavil at the high level of hype around string theory, to the point of monopolising popular attention, and that the gigadollars of a number of philanthropists, as well as numerous physics department employment slots, are being farmed out to what is really an esoteric branch of algebraic geometry."

28. **strong field physicist**
December 8, 2011

Yes, its all very well to attack the sociology of physics and physicists – as if physicists can stop being people or as if the mechanism Lakatos described long ago is some new found foe to smite, and its another to allow your name to be put to a complete debasing of perfectly respectable physicists like M Duffy.

The thing is that you cant abstract your attempts to "correct" physics and physicists from the real life world of funding priorities we live in. We all know that that politicians prefer to fund banker bonuses and war machines (otherwise the politicians are quickly out of a job) than Science. So perhaps you could temper your sword so that it also strikes against the enemies of Science as well as the practioners of it

29. **Pechorin**
December 8, 2011

Peter, would you be willing to publicly debate Duff? I think the entire physics community would get a kick out watching you two beat up on each other. It might be educational, too.

30. **abbyyorker**
December 8, 2011

Well, Duff does make some good points. It's not all rubbish.

31. **Peter Woit**
December 8, 2011

Pechorin,

I don't see the point of trying to argue about scientific issues with someone who engages in ad hominem attacks, so, no, unless someone wants to pay me a huge sum of the money.

I have done public debates with string theorists in the past (one example was with Jim Gates in Florida a few years ago). People who attend such debates expecting fireworks tend to be very disappointed. Most string theorists and I actually disagree about far less than people think.

32. **Garrett**
December 8, 2011

Wow, if string theorists are that worried about me, they must be in even worse trouble than I thought.

33. **José Figueroa-O'Farrill**
December 9, 2011

There are some imprecisions in the comments, which I would like to clear up. I work in the UK and know the situtation fairly well.

The situation with the EPSRC and mathematical physics transcends String Theory. It so happens that the UK has a healthy hep-th community, many of whom work in stringy related topics and hence this decision does affect the stringy community in the UK, but it affects others as well.

It should be mentioned that the hep-th community in the UK is to be found both in Physics and Maths departments. This traditional blurring between mathematics and physics has been one of the main features in UK science; think Newton, Maxwell, Dirac,… This makes the EPSRC decision not to fund mathematical physics even harder to explain. The EPSRC has decided to stop funding mathematical physics, but will still fund Mathematics proposals inspired by Physics. In other words, it will not fund a research proposal about the use of Mathematics (however sophisticated) to solve a Physics problem (and Physics, for the EPSRC, includes the formal end of hep-th as well), but it will fund a proposal where ideas from Physics are used to solve a mathematical problem. The distinction is crucial and it amounts to EPSRC not viewing Mathematical Physics as part of Mathematics, contradicting centuries of UK scientific tradition. To be sure, annecdotal evidence suggests that there are mathematicians who agree with this decision of the EPSRC, but equally there are many who do not.

The situtation with STFC is that they have traditionally funded particle physics and in the absence of additional funding, they will not expand its remit to include mathematical physics per se.

It is naive to think that the fault for this lies in the string theory research community, just as it is naive to think that the fault lies in its detractors. It's a complex situation. EPSRC has re-invented its role from funders of research to sponsors of research. In other words, they wish to play an active role in deciding what research gets funded, a role which until recently was played by the scientists themselves.

34. **Shantanu**
December 9, 2011

Peter or others,
someone should ask him about his reponse to other critics of string theory such as Krauss, Woodard, T'hooft, and many others.
shantanu

35. **aa**
December 9, 2011

also this blog already seems like as public a debate as you can get

36. **Peter Woit**
December 9, 2011

Thanks José,

That's really a shame. As someone who thinks that the area in between math and physics is an exceptionally fruitful one, I was always pleased to notice that in Britain there seemed to be more overlap between the subjects than elsewhere, with people working on physics applications of mathematics often in math departments. A decision to try and reverse this in favor or enforcing rigid disciplinary boundaries is quite unfortunate.

37. **Peter Woit**
December 9, 2011

I've added to the posting a response to Duff's article from Lee Smolin.

38. **Henry Bolden**
December 9, 2011

More about the Mike Duff controversy in the article by Matin Durrani:

"String theorist sparks a spat"

http://physicsworld.com/blog/2011/12/string_theorist_sparks_a_spat.html

Disclaimer: No Brian Greene or Lisa Randall has been harmed in the making of this message.

39. **Anonyrat**
December 9, 2011

A future Frank Close writing the history of these times will have access to Duff's preprint. But will the material on this blog still be available thirty years from now?

40. **Peter Woit**
December 9, 2011

Anonyrat,

An interesting question, which amusingly enough I was discussing with Frank Close himself earlier this week (I was in the Bay Area at the same time as him and we met up). Perhaps some standard way of archiving blogs will emerge.

41. **Anonyrat**
December 10, 2011

Given that Columbia IAS has already lost a fairly widely cited published-online conference paper from 2002 – the webmaster searched, but sent me her regrets – I'm really concerned about the potential loss of information.

42. **Bernhard**
December 10, 2011

Peter,

perhaps you could make a selection of articles with potential historical interest in a book that you can either distribute in pdf in this blog or even find an interested publisher.

43. **John Duffield**
December 10, 2011

I see that Lee Smolin is on the editorial board of Foundations of Physics:

http://www.springer.com/physics/history+%26+philosophical+foundations+of+physics/journal/10701?detailsPage=editorialBoard

44. **james**
December 11, 2011

Duff does make a good point about how theres a dearth of any progress in quantum gravity, not just the popular theory. String theory is most definitely barking up the wrong tree, but, when it comes to uncovering the deeper mathematical model of the universe, who isnt these days. Anything new come out of LQG recently? Fundamental physics research thats destined to be wrong isnt neccesarily pointless. Maybe by barking up the wrong tree theyll tear down a branch. Who knows. It gives people something to do while we wait around for the next 1 in a 100 years einstein to pop out. Like Duff, I don't really get the String hostility in this way. Is it really a battle over funding and prestige? It seems to me the next einstein wont need too much by funding. He'll probably uncover the inexorable symmetry underlying the universe in a patent office with a pen paper.

45. **Aidyan**

December 11, 2011

There is nothing wrong in working on a theory that might turn out to be wrong. That's history of science. When you don't know what the truth is the only way is just to give a chance to this or that approach, theory or speculation. I've done this too for some time with so called non-extensive thermodynamics, and don't regret it. But sooner or later you have to admit (especially to your self) that, if after decades and generations of physicists the concrete results are still lacking, then you are probably going in the wrong direction and that you must move to something else. String theorists have to be blamed, not for the theory in itself, but for their decade long insistence on something to the exclusion of almost everything else and with great waste of human resources. Duff"s comparison of the development of string theory with the history of Laplace's black hole theory or Democritus' atomism is fallacious. The latter were relatively simple ideas subsequently forgotten for centuries with nobody working on it, the former is a theory on which hundreds, if not thousands, of physicists have worked now on for decades without tangible results. If strings will succeed nevertheless that would be a unique historical case. My own rule of thumb for considering a theory worth of attention is to set a limit of a dozen, max 15 years, for it to proof itself to be at least partially true. Otherwise it is time to close down the puppet show and fold up the makeshift stage.

46. **Albert Z**
December 11, 2011

James says: "It seems to me the next einstein wont need too much by funding. He'll probably uncover the inexorable symmetry underlying the universe in a patent office with a pen paper."

Perhaps he already has, but will anybody listen to a new paradigm that is radically different from prevailing assumptions?

If everyone thinks they know the "one true path" to unification, will anybody consider an untried route identitied by a "nobody"?

47. **harryb**
December 11, 2011

Following up a few of the references in the main piece and responses, I note the summary quote (lifted directly from the article "Stringscape" in PhysicsWorld Sep 2007, by Matthew Chalmers) by Ed Witten:

"There is an incredible amount that is understood,
an unfathomable number of details. I can't think
of any simple way of summarizing this that
will help your readers."

This reminds me of the UK comedy act Armstrong & Miller Physics Special (see youtube) where the professor refuses to attempt to simplify his new take on string theory. The sketch was supposed to be ironic, but Onion-like, it seems parody is only a few paces of absurd realities.

I think Peter's site here is a refreshing skeptical zone in the arcane world of modern physics. If String Theory is indeed fundamentally correct, then its should have no problem with educated questioning.

To the interested layman, Witten's words are quite concerning – it's as if to say there comes a point where those not fully involved in the deep details of string theory need no further explanation, but to take it on trust that a rarefied few (male) experts can handle it: the meaning of everything, the basis of the universe.

The parallels are too blunt to spell out.

In any event, questioning string theory or quantum loop gravity or whatever is the basis of normal science surely? I re-read Feynman's Value of Science essay (1955) where he ends it with the plea: "to teach how doubt is not to be feared but welcomed and discussed"

If only.

Still, I think this site helps keep that spirit alive and well thank goodness.

48. **Albert Z**
December 11, 2011

Nature, with the help of prudently skeptical experimentalists, keeps theoretical pipe dreams from wandering off too far into the swamp of pseudo-science. In the long run (and sometimes the very long run), that is.

49. **John Merryman**
December 11, 2011

Interesting comments on the nature of progress and skepticism. It seems, just like nature, we need to keep building out those large, unwieldy, unstable structures in order for them to collapse and see what hard little nuggets of content remain. On the other hand, maybe those large gaseous fields are every bit as natural as the little bits of mass into which they collapse. Lots of heat and some light are generated.

50. **Jon Lennox**
December 11, 2011

Anonyrat / Peter: The archive.org Wayback Machine archives of this site are pretty thorough (with the usual caveat that they run about six months behind).

For long-term storage, that's a single (organizational) point of failure, though.

51. **Chris Austin**
December 11, 2011

@ harryb,

I was surprised and disappointed by that unfortunate quote from Ed Witten; perhaps he had his mind on something else at the time, like Paul Dirac in that wonderful interview that Peter linked to recently.

Here's a one-page article from Witten's IAS website, where he does a better job of explaining why superstring theory is interesting.

52. **Peter Woit**
December 11, 2011

I've added as an update a response from Garrett Lisi to Duff's article.

53. **Dan Burgess**
December 12, 2011

Apparently even Google's a critic. A search for 'Not Even Wrong' brings up this result for Google books:
Not even wrong: the failure of … – Peter Woit

That's tough 🙂

54. **harryb**
December 12, 2011

@Chris Austin

Thanks for the link – agree it is a lot more accessible / humble on string theory's strengths / limitations. Leaves little room for any other line of thought though, with the suggestion that should any emerge, they will be reshaped as just a yet undiscovered element of string theory. But clear article.

I'm reminded again of Feynman's comment when asked if he had to leave one sentence behind to aid other civilisations: he noted it would need to be "Everything is made of atoms".

I guess Witten would rejoin "Everything is made of strings". Would this help or hinder these hapless civilisations I wonder.

55. **Hendrik**
December 12, 2011

Maybe not too far off-topic, is a piece by John Horgan in Scientific American, how in physics sometimes cranks are indistinguishable from experts. Based on a book by Margaret Wertheim, link is here. Of course strings are mentioned.

56. **Peter Woit**
December 12, 2011

Hendrik,

I ordered a copy of the book a few days ago, should arrive tomorrow, then review to follow shortly.

57. **Shantanu**
December 17, 2011

Peter or anyone else, does anyone know if smolin and Duff actually went for a drink or dinner after the debate?

58. **Dan**
December 26, 2011

Dear Peter,

even though, I might agree with what you say in some points of your article. The response you published from Lisi really needs a comment. Lisi makes comments about half-truths when he's the first one using them.

Lisi mentions the fact that his peer reviewed paper (in a conference proceedings) wasn't mentioned by Duff, but Lisi fails to say that this paper doesn't say that he succeeds in the unifications, it just provides an embedding for the "one family" standard model, something already present in Distler's paper.

About it, also, Lisi wants to make the readers thing that it's possible to include a chiral generation in a non-chiral representation. This is almost outrageous. Everybody knows that this sentence either makes no sense, or it states the obvious. Distler and Garibaldi's point isn't that the one generation isn't possible, they say that "because of the presence of an antigeneration, that generation will never be chiral". But, most importantly, they prove that three generations are impossible to get out of E8. And Lisi doesn't have any answer to this. Of course.

About Lisi's work being threatening to string theory, let's just have a laugh. What Duff is clearly upset at certainly isn't Lisi's theory, he's upset at who still doesn't clearly state that Lisi's was an interesting attempt, but the theory has so many holes and it's so incomplete that there isn't really a reason to give him so much resonance in the news. And when they prove his mistakes (he wrote several "wrong" statements, both physically and mathematically), then the press forgets and they don't give reasonable updates.

59. **Peter Woit**
December 26, 2011

Dan,

About Lisi vs. Distler/Garibaldi, I think both sides have had a chance to make their case, people can make up their own minds about this. The bottom line I think is that there are problems with Garrett's unification proposal, just like there are problems with all unification proposals out there. He's pursuing his and may someday overcome them, others may find some inspiration in what he has done for their own ideas. If you don't think his proposal can go anywhere, work on something else. It's not like there's a problem with the field being dominated by work on his ideas, crowding out others.

As to the complaint from string theorists that Garrett got too much positive media attention, and Duff's belief that this is such a big problem for string theory that he had to devote several pages to it of his short article defending string theory, try the following thought experiment. Imagine that Garrett's ideas fit into the string theory program, that he had some unification proposal he was pushing involving string theory (with similar problems to all the string theory unification proposals). The media would have found it just as attractive to write about the surfing outsider physicist with his revolutionary ideas about how to make string theory work. Do you really think Distler or Duff would have mustered the same outrage and gone on an anti-Lisi campaign? Would Duff have devoted pages of his article to the Lisi problem?

What's funny about both Duff and Distler is that they clearly have no problem with outrageous, dishonest media hype. They have both had their universities issue absurd press releases about their own work supposedly "testing string theory", when they had nothing remotely approaching such a thing, see

http://www.math.columbia.edu/~woit/wordpress/?p=510

and

http://www.math.columbia.edu/~woit/wordpress/?p=3127

It’s only when a string theory critic gets media attention that they get completely bent out of shape.

60. **Dan**
December 26, 2011

Peter, thanks for your answer.

I think you missed my point. Lisi “himself” says clearly that his theory isn’t currently able to reproduce the standard model, with one, two or three generations. He has no particle masses and doesn’t have the correct quantum numbers. This is clear to everybody who understands the physics. But Lisi’s answer here and in popular magazine is deceptive because he doesn’t always present this statement clearly.

There is a big different between the Pati Salam model, say, or other unifications models that are not complete, but that can at least explain the standard model, and Lisi’s model, that cannot even explain why the electron has a different mass than a muon. Or even if he can actually have a muon (second generation problem).

That is a huge incompleteness, much much bigger than not being able to prove, for example, whether or not N=8 supergravity is finite.

In general, forgetting about string theorists, there isn’t a hate towards E8, which is in fact used in many fields. The annoyance is towards some interviews or presentation of facts, that don’t say clearly that in Lisi’s theory: still we don’t know how to actually get this fermionic degrees of freedom (the unconventional BRST isn’t ever defined), still we don’t have a way to get chiral fermions in the one generation case (there is only a comment about it), still there is no way to get the three generations (not a real word about it, that says something mathematically meaningful), still we don’t have a proper symmetry breaking mechanism, still we don’t know if really the Coleman-Mandula theorem is valid or not in those limits, still we don’t know why it would be a finite theory… And I could keep going. Of course everybody is welcome to work on the theory they want. And the good thing about nature is that it doesn’t care about what theory we like. Eventually the right theory will become evident. What matters though, is to be honest with the readers, especially the ones who can’t understand most things we are saying. And to them we owe the truth, which is that Lisi’s model is not more wrong or less wrong than a million other models, but also that it is way way far from being a complete theory, given that it doesn’t even reproduce all the known results of the standard model, which most other “good” attempts to theories of everything instead can reproduce. This difference is important.

The other point that I was making wasn’t supportive of Distler, Duff or Motl or anyone else. I was just saying that Duff was upset because what I said above isn’t stated clearly enough. And the press makes it look like Lisi’s theory is as probably and as developed as lots of actually “working” theories (in the sense of at least reproducing known results and producing some predictions). Which is obviously not the case. Duff certainly isn’t upset or scared that Lisi’s theory will be creating problems to string theory, certainly not at this premature definition of the theory, does is just slightly more than some numerological correspondance between Lie groups and quantum numbers.

Think of a theory like the one that Nima Arkani-Hamed is working on. Do you think it is as developed as Lisi’s. Less worth? More worth? Do they have the same media attention? Do they deserve that compared to each other?

I’m not even saying anything bad about Lisi or his research. I’m just saying that the theory is in such a preliminary state that calling it theory of everything is almost a joke. Maybe, an work in progress, stil incomplete, attempt towards a theory of everything could have been better. Especially when people present it to the main public that isn’t capable of distinguishing between theories.

61. **Peter Woit**
December 26, 2011

Dan,

I just disagree with you about why Duff is upset. He has shown conclusively that he has no problem with misleading hype appearing in the media or making untrue statements himself when it’s part of a campaign that aligns with his interests. The idea that his problem with Lisi is just that he’s offended by inaccuracy in the media is not plausible.

62. **Dan**
December 26, 2011

OK, I understand your point of view. But again, I wasn’t supporting Duff’s point. I was just saying that the literal interpretation was about that. Then everybody is free to have their own interpretations. But it was misleading of Lisi to present his words in a deceptive way and changing the literal meaning of them. Which is always the problem with every statement that Lisi makes. He rarely talks any physics or math but very often gives deceptive and misleading answers. That’s all.

And even if Duff is wrong, or hypocritical and all that. What the media do with Lisi isn’t something to be proud of. It damages good physics. Maybe also Duff does, but it’s not with one bad thing that we cancel another bad thing.

Thanks for offering this space of confrontation.

63. **Peter Woit**
December 26, 2011

Dan,

My perception is different. Lisi’s public claims about his ideas seem to have about the average accuracy level you expect from someone enthusiastic about his own research. On the scale of the Distler and Duff press releases, not to mention the average popular book about string theory, he’s a stickler for accuracy. And, while the damage done by string theory hype to particle theory is huge and on-going, I just don’t see any significant damage from the media Lisi coverage.

I’m not sure what work of Arkani-Hamed’s you’re referring to, and quite possibly I don’t know what he’s up to most recently. But whatever it is, I’m sure it will get quite a lot of attention from other theorists, and media attention if it pans out. If he quits his job at the IAS, moves to Hawaii and takes up surfing, he’ll do much better on the media front.

64. **Dan**
December 26, 2011

Ok, let’s ignore for a second string theory. I believe myself that it has made its damages. I don’t even like string theory, and for a while we all know all the jobs where there. Similar thing with supersymmetry. We aren’t finding it, and still we will have to deal with some fine tuning problems to solve if the Higgs is confirmed at 125 GeV. Also, is it really a Higgs? Is it elementary? Is it composite? How does it couples, in case, with gravity and so on… all questions to answer. Fortunately at least in particle physics we will be busy enough with this.

But about Lisi, allow me to strongly disagree. I studied very carefully both Distler’s paper and all Lisi’s papers. And even if it’s true that he’s very excited about his theory it seems to me that he's not even trying to have progress in it, just to defend it for the status he's got in the media.

What did Lisi really publish about his E8 stuff after the first paper? His paper with the explicit matrix representation doesn't add anything to the initial structure. The result was even not only compatible, but in agreement with Distler and Garibaldi's paper. His paper with Smolin and Speziale is barely related to the unification part but there is no E8 and the fermions (his main innovation in the other theory) are treated in a completely different way. Has he published any mathematical work on the unconventional BRST theory that he said he wants to use? Has he published any results about whether or not he's safe from the extensions of Coleman-Mandula theorem? Has he found any way to get rid of the non-chirality and at least have a one generation standard model? Has he said anything specific about how he wants to recover the three generations out of fields with the wrong quantum numbers? Has he given any important math about this stuff in the last few years? Has he studied whether or not his theory is anomaly free, finite, renormalizable… etc? The paper with Smolin and Speziale certainly is not exhaustive about the topic.

So, at the end, why does he give lots of interviews saying that Distler and Garibaldi made unnecessary assumptions in proving his theory wrong, but never actually says why those assumptions are wrong, what they are, and how he thinks of actually progress?

To me this doesn't seem a very accurate. He seems, although, very lucid about responding to these questions without really giving an answer. So we, physicists, know that he gave no answer, while the general audience believes he gave a valid answer. I don't find this very encouraging…

Thanks!

Higgs Predictions and Results

December 12, 2011
Categories: Experimental HEP News

Tomorrow at 14:00 CET CERN will start to unveil the results of this year's LHC Higgs search, see here. Jester has just posted details consistent with what I've been hearing for the past couple weeks, although his numbers are slightly different. The big news is that both experiments are seeing something that looks exactly like a Standard Model Higgs in the Higgs -> gamma-gamma channel around 125 GeV. Some details about this signal:

- I've heard that ATLAS sees this with significance a bit less than 3 sigma, 3.5 if you combine results from other channels. To take into account the look-elsewhere effect, the significance of seeing such a fluctuation anywhere in the range studied is 2.2 sigma. Jester has

> The combined significance is around 3 sigma, the precise number depending on statistical methods used, in particular on how one includes the look-elsewhere-effect.

- What makes this something to take very seriously is that CMS is also seeing much the same thing in the gamma-gamma channel around the same mass. I've heard their significance is 2.5 sigma. Jester has

> All in all, the significance at 125 GeV in CMS is only around 2 sigma.

- Another channel that might start to have some sensitivity to a Higgs of this mass is the "golden channel", where you look for 4 leptons reconstructed as coming from 2 Z's. Here Jester says that ATLAS has three events near 125 GeV, contributing to the higher significance in the combined result. CMS also sees an excess involving three events, but they see this in the range 117-121 GeV, which should be too low to come from a 125 GeV Higgs. They also see a 2.1 sigma bump in ~~gamma-gamma~~ their combination at 119 GeV .

In this business, for each experiment a 2 sigma bump is not worth taking seriously, around 3 sigma is where it gets serious (and 5 sigma is the conventional standard to claim a discovery, they're definitely not there yet). But a 3 sigma bump from one experiment and a 2.5 sigma bump from the other, at the same place, is serious evidence indeed. It is still quite conceivable though that this kind of signal could disappear with more data (for this we'll have to wait until mid-2012). I think Jester has it about right:

> There is a good chance we're finally looking at the real thing, I'd say 50% based on the data alone and 80% adding our sincere convictions that Higgs must really be in that mass range.

One thing that can be predicted with certainty is a flood of papers from theorists claiming that their favorite model predicts this particular Higgs mass. Something to keep in mind when evaluating such claims is that for more than ten years we have known from LEP that the Higgs mass is above 114 GeV, and from precision electroweak measurements that it can't be too much above that value. Preliminary data early this summer showed some indications of a signal around 140 GeV (leading some to claim this as vindication of the multiverse, see here). By the end of the summer this was gone, with the combined CMS+ATLAS data excluding a Higgs down to 140 GeV or so at 95% confidence level, around 130 at 90% (tomorrow's data should have each experiment excluding down to 130 GeV at 95%, with a Philip Gibbs combination result sure to follow soon). Rumors of the gamma-gamma signal at 125 GeV have been circulating for at least the past two weeks.

We'll soon move from rumors to results, and I'll add anything new or different we hear tomorrow to this posting. Surely there will be a press release and a lot of versions of the results coming out of CERN, covered by the media and many bloggers. For some good recent blog postings explaining what to look for tomorrow, see Matt Strassler and Tommaso Dorigo.

Update: One correction. Besides the 124-125 bump in gamma-gamma, there's another bump in the CMS combination at low mass, around 119 GeV, but this is due just to the "golden channel" excess there, they see nothing there in gamma-gamma.

Comments

1. **petergreat**
   December 12, 2011

   I read that a SUSY Higgs can be indistinguishable from a SM Higgs, at least during the initial stage of discovery. Is the same true for composite Higgs models?

2. **Bob S**
   December 12, 2011

   http://news.cnet.com/8301-11386_3-57341543-76/has-higgs-been-discovered-rumors-of-watershed-news-build/

   Nice props for "blogger" Peter Woit. Heck, the article even states string theory is "contraversial"

This Week's Hype

December 12, 2011
Categories: This Week's Hype

The announcement at CERN tomorrow of a likely-looking signal for a 125 GeV mass Standard Model Higgs will probably unleash a flood of hype from theorists claiming this as evidence for their favorite Beyond the Standard Model scenario. One obvious problem with any such claim is that the CERN results correspond well so far to the Standard Model with no additions whatsoever, so spinning them as providing support for things like supersymmetry and string theory will require some work.

For the last decade we have known that the Higgs mass is above 114 GeV (from LEP) and unlikely to be very much higher than that (from precision electroweak results). This summer's LHC results disfavored masses above about 130 GeV, so for the last few months we've known that if the Standard Model Higgs is there, it should be between 114 and about 130 GeV. For a couple weeks news has been circulating widely from ATLAS and CMS that they are both seeing something around 125 GeV.

First out of the gate in the hype derby is Gordy Kane, who is quoted by Davide Castelvecchi at Scientific American claiming that string theory predicts the Higgs mass to be between 122 and 129 GeV:

> "If it's in that range it's an incredible success for connecting string theory to the real world," Kane says. He says he is confident that the upcoming LHC announcements, if they pan out as predicted, will constitute evidence for string theory. "I don't think my wife will let us bet our house, but I'll come close," he says.

It's unclear exactly what he's willing to bet the house on. If it's just that the Higgs is in that range, this might have something to do with the plots from the experiments widely circulating privately the last few days. In a remarkable coincidence, after more than 25 years of unsuccessfully trying to extract a definite experimental prediction from string theory, Kane and collaborators were able to achieve the holy grail of the subject (a prediction of the one unknown parameter in the SM, the Higgs mass) just a week before the CERN announcement. They submitted their paper to the arXiv the evening of Monday December 5, a few days after rumors of a 125 GeV Higgs were posted on blogs Friday December 2.

The paper deals with a "prediction" you get based on a host of assumptions about which particular class of string theory compactifications to look at. The main result is that in this particular class of models, you can relate the Higgs mass to the parameter tan(β) that occurs in SUSY extensions of the SM. As you increase tan(β) from around 2, the Higgs mass lies in a band, increasing from 105 GeV and a maximum of about 129 GeV:

> We will demonstrate that, with some broad and mild assumptions motivated by cosmological constraints, generic compactified string/M -theories with stabilized moduli and low-scale supersymmetry imply a Standard Model-like single Higgs boson with a mass 105 GeV < M_h < 129 GeV if the matter and gauge spectrum surviving below the compactification scale is that of the MSSM, as seen from Figure 1. For an extended gauge and/or matter spectrum, there can be additional contributions to M_h.

This conclusion and Figure 1 correspond closely to what is in the slides of Kane's talk at String Phenomenology 2011 this past August. The plot of Higgs masses as a function of tan(β) is there, giving a range of 108 GeV to 127 GeV. There is an intriguing comment on the conclusion slide:

> Single light Higgs boson, mass about 127 GeV unless gauge group extended.

I can't tell where the number 127 came from. Since 127 GeV is the top limit in the figure, and the wording "unless gauge group extended" is used, one guess would be that Kane meant that 127 GeV was the upper bound on the Higgs mass in this class of models.

There's nothing in Kane's August talk about a 122-129 GeV range for the Higgs mass, but in the December 5 paper it appears explicitly three times:

- In the abstract there's:

> When the matter and gauge content below the compactification scale is that of the MSSM, it is possible to make precise predictions. In this case, we predict that there will be a single Standard Model-like Higgs boson with a calculable mass 105 GeV < M_h < 129 GeV depending on tan β (the ratio of the Higgs vevs in the MSSM). For tan β > 7, the prediction is : 122 GeV < M_h < 129 GeV.

I don't see where the tan β > 7 comes from, presumably it's in one of their other papers.

- In the introductory paragraph there's:

> Furthermore, in G2-MSSM models we find that the range of possible Higgs masses is apparently much smaller, 122 GeV < M_h <129 GeV.

- G2-MSSM models are mentioned only briefly again in the paper, at the third occurrence of this particular mass range:

> For instance, in G2-MSSM models arising from M theory, Witten's solution to the doublet-triplet splitting problem results in μ being suppressed by about an order of magnitude. Hence, in these vacua, the Higgs mass sits in the range 122 GeV < M_h < 129 GeV.

Kane has chosen Lubos Motl's blog as the place to guest post and promote this string theory "prediction", concluding there:

> If generic compactified string theories with stabilized moduli correctly predict there is effectively a single Higgs boson and correctly predict its mass, it will be a huge success for the main directions of particle physics beyond the Standard Model, for supersymmetry and for string theory, both of which are crucial for the prediction. It will be a huge success for LHC and the accelerator physicists and experimenters who made the collider and the detectors and the analysis work. It will put us firmly on the path to understanding our own string vacuum, and toward the ultimate underlying theory. The value of the Higgs boson mass not only confirms the approach that predicts it, remarkably depending on its numerical value it may allow an approximate measurement of tan β, the μ parameter, the squark and gravitino masses, that the gauge group and matter content of the theory below the string scale is that of the MSSM, and that light (TeV scale) gluinos and dark matter are likely.

I don't quite see how finding a SM Higgs at 125 GeV is going to give all these different numbers and pieces of information. Kane claims that these string theory predictions also predict gluinos visible at the LHC within months from now:

> Then the gluino should be detected at LHC, and because of the heavy scalars the gluino decays are different from the ones usually discussed, being dominantly to third family quarks, top and bottom quarks. I won't explain that here because of space and time; we can return to it as the gluinos are being detected in coming months. They have not yet been systematically searched for.

In addition, there's a dark matter prediction that should be tested in "1-2 years".

If I were Kane, I wouldn't bet my house (or go to the press claiming the 125 GeV Higgs as a "huge success" for string theory) just yet. Assuming he's right, within months the gluinos will be there, and his ticket to Stockholm will be assured.

Update: My prediction in the first paragraph is coming true faster than I thought. In tonight's hep-ph listings one finds:

http://arxiv.org/abs/1112.2415 (m_H= 127 +/- 5 GeV)

http://arxiv.org/abs/1112.2462 (m_H < 128 GeV)

http://arxiv.org/abs/1112.2659 (m_H =126 +/- 3.5 GeV)

http://arxiv.org/abs/1112.2696 (m_H > 120 GeV)

There are going to be a lot of these…

Update: Over at a Nature live chat, Kane is giving the public some interesting explanations:

"for a higgs to be meaningful it must be part of a supersymmetric theory, so the superpartners should be found. The form it takes implies that gluinos should be found with masses around a TeV, maybe less, by summer, and decaying mainly into topquarks and bottom quarks."

"Recently we have published string theory calculations that imply the higgs boson mass is 125 GeV so if it is there are strong implications for connecting string theory to the real world, and for what the higgs discovery implies. we did that before the data."

"string theories are now well enough understood to predict higgs physics"

There will be another live chat involving Kane tomorrow, this one at Science magazine.

Update: Just took a look at the Science chat. Kane seems to have completely lost touch with reality, somehow deciding that the two experiments have reached the 5 sigma level needed to claim a discovery. As far as I know, he's the only one in the world to think this.

> Gordy Kane:
> YES. an experimenter from one experiment can't say that, but theorists see that two different experiments both saw a signal at about the same mass, and also saw additional channels, so it's a discovery!
>
> …
>
> Gordy Kane:
> The 5 sigma is a criteria people have chosen. i think as soon as the data are combined from two detectors, which is entirely legitimate, then the signal will indeed be over 5 sigma.

The following claims make about the same amount of sense as the discovery one:

> Comment From Tom
> Does the existence of a 125 Gev Higgs give any support to supersymmetry?
>
> Gordy Kane:
> Yes. first, for a long time it has been known that the lightest higgs boson of supersymmetry should be lighter than about 135 GeV (actually closer to 140 GeV but people make assumptions), so this is consistent. Then the supersymmetric string theories as i mentioned do predict the 125 number and it is a supersymmetric lightest higgs boson.

Update: The hype goes on, with a column today at Nature.

Comments

1. **glukanos**
   December 12, 2011

   Well, Kane did at least make a definitive statement (even if in a blog post, not a journal paper) about BSM particles, viz. gluinos will be produced at LHC and discovered in a few months. (The timescale will of course depend on the LHC operating schedule, a few months could easily become more than a year.) But, anyway, you have your definitive string theory/MSSM prediction of BSM. Consider it a Christmas present from Kane to you.

2. **Peter Woit**
   December 12, 2011

   glukanos,

   From his comments at the blog, the problem isn't not enough data, but the experiments not having done the right analyses (they were wasting their time looking for "unmotivated sugra"). He seems to think that they are now doing the right analyses, thus the "months" rather than "years" estimate. Presumably he's hoping for a Christmas present…

3. **Mark**
   December 12, 2011

   Since I've looked into the G2-MSSM scenario, it is probably worth mentioning that after moduli stabilization and SUSY breaking, the G2-MSSM scenario of Kane et al contains only *four* GUT-scale input parameters so it will be relatively easy to rule it out.

4. **Eric**
   December 12, 2011

   I think it's worth pointing out once again that the Standard Model with such a low-mass Higgs isn't really stable. This implies new physics to provide the stabilization, and supersymmetry is the best motivated option for this new physics. Furthermore, the Higgs mass has an upper bound in the MSSM of around 130 GeV. So, it is likely that supersymmetry will eventually be observed at the LHC. Then, models such as Kane's can then really be put to the test.

5. **Mark**
   December 12, 2011

Also, in contrast to the flux compactifications such as the KKLT scenario where one has no good reason to expect low-scale SUSY and has to fine-tune the tree-level flux superpotential to 15 orders of magnitude to get TeV scale SUSY, the non-perturbative M-theory vacua that result in the G2-MSSM scenario generate low SUSY breaking scale dynamically via the mechanism of dimensional transmutation (strong gauge dynamics).

6. **emile**
December 12, 2011

Eric, speaking of the SM being unstable, can you comment on this?
http://arxiv.org/pdf/1112.2150

7. **Anonyrat**
December 12, 2011

emile, remarkable if true!

8. **younghun park**
December 12, 2011

We must not trust the signal which amounts to the 2~3 sigma.
That signal may disappear in the next analyzing the remained data.

I don't understand why CERN will announce as it is the real signal.
Why?
I doubt if we trust the result of experiment done in CERN.

We must not believe any result until that proves true

9. **Urk**
December 12, 2011

I am no fan of this site, but I will say two things:

A) Kane knows less string theory than an unborn baby (less, because he knows many things that are not demonstrably true).

B) String theory is in no state to predict the Higgs mass. This is simply impossible, at this time, given whats known about the theory.

Don't take my word for it. If you're anywhere near a top 10 physics department, ask your local string theorist. Other issues (AdS/QCD? AdS/CMT? The controversial "Landscape"?) will bring nuanced responses, with uncertainty from good theorists. Not one will say string theory can now, in any reasonable sense of the word, "predict" the Higgs mass. They may say "random particular model X has N light Higgses!". This is very very different.

10. **Albert Z**
December 12, 2011

It is very disconcerting to see certain regulars at this blog acting as if the forthcoming LHC data unquestionably heralds the advent of the putative Standard Model Higgs boson. It seems close to proseletizing in the eyes of one who has a more skeptical view of the heuristic foundations of particle physics.

This wishful thinking: seeing only the positive "hints"; ignoring the various contradictions and uncertainties; downplaying the very shaky "predictability" issues, etc., seems uncomfortably close to the behavior of string/brane theorists who you rail against?

Troubling contradiction? Or am I too much of scientific skeptic? Or perhaps just too dim to receive the wisdom accessible to the fully initiated and sufficiently refined?

Albert Z

11. **pfogle@tiscali.co.uk**
December 13, 2011

from a layman… surely Kane is not 'predicting' M_h, but rather it sounds like using the phenomena to tune ST parameters, and show that MSSM is 'capable' of producing a credible explanation. It's the gluino mass that's the prediction here, and I'll await that result with bated breath!

12. **Urk**
December 13, 2011

I'll await the data with bated breath. Kane is making a strong argument for mandatory retirement with his statements.

13. **Z**
December 13, 2011

What a curious state of affairs we have in physics, where a "3 sigma" detection significance is taken by physicists to mean not a 99.9% confidence that the Higgs has been found, but something far lower. There are amusing statements on many blogs along the lines of "there's a 50% chance these 3-sigma results will hold up". Do people really not trust the analysis and fear systematics of the data from these large collaborations? Why doesn't 3-sigma mean 3-sigma?

I know HE astronomers that would be ecstatic about 3 sigma detections of pulsations or some such phenomena in their data.

14. **Phil**
December 13, 2011

In my experience, high energy astrophysicists are *frequently* ecstatic about their 3 sigma "discoveries", some of which indeed last long enough to appear in Nature 🙂 I'm more amazed the LHC folks understand their systematics as well as they do, and I'll forgive them a sigma one way or the other.

15. **Daniel**
December 13, 2011

Z: It doesnt mean "there is a 99.9% chance this is the Higgs" it means "if this was just random data the chance against this being nothing 99.9%".

Which sounds interesting until you think if you look at 1000 things you will have a 50% chance of getting a 3 sigma result. Im not sure how they take into account the look elsewhere effect with this analysis however.

There is no need to be impatient, if its real, its not going anywhere 🙂

16. **Bernhard**
December 13, 2011

String theory is really an amazing theory. It is capable of predicting the Higgs mass almost fours days after the result is publicly known, what a triumph.

Seriously, I wonder if Kane is being deliberately scientific dishonest with this Higgs story or just forgot what predictive and explanatory power of a theory is.

On the other hand if he keeps the some standards for the possible non-observation of gluinos in the next moths and announce he now thinks the whole string idea is likely wrong, this would make me satisfied.

17. **John Duffield**
December 13, 2011

I took careful note of Albert's comments, and I've been paying attention to recent developments. Sorry, but it doesn't feel as if it's just the BSM guys unleashing a flood of hype here.

18. **Breaking rumors**
December 13, 2011

The rumor is that after re-calibrations the CMS and ATLAS peaks do not coincide

19. **Bernhard**
December 13, 2011

Breaking rumors,

oh no! That would be so bad for string theory, I hope you're wrong.

20. **Albert Z**
December 13, 2011

The explanation is as follows.

In astrophysics, a 3 sigma result usually is vindicated by more data.

In particle physics, a 3 sigma event often-as-not "disappears".

See the difference?

Albert Z

21. **Mithras**
December 13, 2011

@younghun park:

I've actually had someone, on another site, tell me that "Well, yes. 3 sigma data has disappeared in further analysis, but never when there's been a theory there to predict it".

Being a layman at all this (I read physics for pleasure – you can back away slowly, I won't be offended), I didn't have a good citation to refute that at hand; the only thing I could think of (and haven't used yet) was solar neutrinos, where there was 3+ sigma of evidence that "we've got something wrong here", although that's likely not the way to word it.

Any thoughts?

22. **Quantumburrito**
December 13, 2011

I fear that the overzealous will go out of their way and swamp all media communication to convince an ignorant public that the Higgs now provides a new reason why string theory deserves all the support it can get. You may have to write a sequel to "Not Even Wrong".

23. **Cliff**
December 13, 2011

Whatever someone thinks of Kane's work, it is demonstrably not true that he adjusted his predictions in any significant way as a result of the links. Nice job muddying that up though.

Is it really so hard to identify the logic favoring a large tan-beta from his august presentation? Its because the gravitino mass naturally wants to be close to that of the lightest the moduli, which must be heavier than around 25 TeV due to the cosmological constraints. So the 127 GeV mass is favored because thats where it stabilizes for large tan-beta. So its a real prediction.

You didn't mention the reasons that G2 models were well-motivated in the first place, which is that they can naturally generate an ~electroweak mass scale while stabilizing moduli.

But I understand theorists aren't allowed to use any data or logic besides "string theory is true" according to the exchange we had over at cosmic variance a while ago.

24. **Peter Woit**
December 13, 2011

Cliff,

I don't see what you're complaining about in what I wrote. I wrote that I didn't understand exactly where the 127 came from. If you're telling me that the prediction of these theories is large tan(beta), and that as a result the prediction Kane is making for the Higgs mass is 127 GeV, that's fine, but it's not what he's saying in the paper. For the first calculation of the Higgs mass directly from string theory, solving the biggest problem of the subject, it would be a good idea if he explained more clearly what exactly he is doing.

As for the exchange at Cosmic Variance, I have no idea what you're talking about. I don't know who you are, or whether you're talking about an exchange over there weeks, months, or many years ago.

25. **Mark**
December 13, 2011

@Cliff, actually, you have it kind of backwards with regard to the gravitino mass, the constraint on the gravitino mass M_{3/2} less than O(10-100) TeV in these vacua does not come from cosmology but instead it comes from setting the tree-level vacuum energy to be small. Then, when they compute the moduli masses they find that the masses of the lightest moduli turn out to be close to the gravitino mass but less than twice the gravitino mass (the moduli cannot decay into the gravitinos by the kinematics). Then they note that this mass range is perfectly consistent with cosmology since the moduli are heavy enough to decay before BBN but the relic density must be non-thermal, etc, etc. What Kane is doing now is trying to extrapolate this result obtained for the non-perturbative G2 vacua to a "generic" string vacuum but I and many others don't think that his reasoning is justified because there are well known counterexamples where scalars can be much lighter than the gravitino. However, what I would definitely agree on with Kane is that moduli stabilization in a generic vacuum combined with cosmological constraints greatly disfavors low scale gauge mediation scenarios.

26. **Mark**
December 13, 2011

Well, I actually just reread what Kane said in the paper and on Motl's blog and the comments there and I guess I agree that in generic string vacua SUSY breaking is not sequestered, which translates into squarks and sleptons being as heavy as the gravitino. The known counterexample I had in mind was the KKLT scenario, where strong warping results in sequestering and light scalars and I admit that it is indeed kind of non-generic.

27. **chris**
December 14, 2011

Eric,

125 is right at the vacuum stability edge. if that turns out to be m_H, then there is work for a generation of lattice gauge theory to figure out whether the new physics scale in this scenario can be as high as m_planck.

but even if it might turn out to not be ok all the way up, the SM new physics scale will be well above 10^10GeV, maybe at about the GUT scale. so while there may be aesthetic reasons for low energy susy (fine tuning et al), there is no need. or, in other words, if m_H~125GeV and nothing else is found at the LHC, then any new collider below GUT scale energies will be a gamble: there is no strong reason to believe that it will find anything new.

28. **Eric**
December 14, 2011

Hi Chris,

Yes, I agree with you that a Higgs mass around 125 GeV is right on the borderline between being stable and unstable. Clearly, we'll need more data before the picture becomes completely clear.

29. **Mark**
December 14, 2011

First of all, I think that Kane is grossly exaggerating what he and his collaborators have demonstrated. String theory at this stage can not make a firm prediction about the Higgs mass and I think that he is doing a great disservice to the community by this type of hype. That said, there is definitely something in their work that's very intriguing and I'd like to highlight a few points below.

I think that it would be fair to say that string vacua that can be described in terms of N=1 D=4 supergravity with a low scale of SUSY breaking *generically* contain heavy sfermions and trilinear couplings but light gauginos (KKLT with warped sequestering is a non-generic counterexample). In addition, the masses of the lightest moduli are *generically* of the order the gravitino mass. These statementes would not be controversial among string theorists who know about moduli stabilization. On top of that, cosmological constraints imply that the moduli must be very heavy >O(10-100) TeV to preserve the success of the BBN. When you combine all of the above you may conclude that in such vacua even with low scale SUSY the lightest sfermions must be very heavy. What that means for the lightest Higgs in the context of the MSSM is that its mass will receive large radiative corrections mainly due to the heavy stop and will get pushed into the range above 120 GeV but below 130 GeV. Now, the exact value does, of course, depend on tan beta. With regard to tan beta being O(10) I'd recommend reading this paper: http://arxiv.org/abs/1105.3765 . Basically, tan beta ~ O(10) is needed to reduce the so-called little hierarchy problem when the scalars and trilinears are heavy, as can be seen from eq. 6 in that paper. So, the value of 127 GeV seems rather natural is such a scenario. Kane's optimism about the gluino presumably stems from the same considerations, namely, in eq. 2, there is a quantity denoted as R(t), that depends on M3 (related to the gluino mass) as you can see in the expression in the paragraph below eq. 2. So, the relatively light gluino is needed if one wants to keep the little hierarchy problem under control.
So I think that this is all definitely very interesting and in my mind compelling.

As for the G2-MSSM, this is definitely the simplest model of moduli stabilization out there – a strongly coupled hidden sector (SQCD with a single family of vector like matter) dynamically generates a non-perturbative potential that stabilizes all the moduli and generates a hierarchically small scale (there are no fluxes, no warping, no antibranes). And these guys did it for the most general Kahler potential compatible with G2 holonomy with an arbitrary number of moduli, found closed expressions for the moduli and derived soft terms in the MSSM largangian!

30. **Allan Rosenberg**
December 16, 2011

Peter,

Yes, the "prediction" business appears disingenuous (and your post is one of the best I've read, which is saying a lot). But, in all fairness to Kane, there's nothing wrong with a paper that identifies a subset of string theoretical models that accords with an experimental result or publishing it first. Just think of the vast swath of the multiverse and the numerous false vacua we can rule ourselves out of if we can identify a Higgs boson.

Wouldn't you be impressed if string theorists were able to present at least one vacuum that resolves assymptotically to general relativity and the standard model? I don't claim that string "theory" will get there, but isn't constraining the "theory" using experimental results the best way out of the "string theory proves everything" hysteria?

31. **Bernhard**
December 16, 2011

Kane´s campaign still at full speed:

http://www.nature.com/news/particle-physics-is-at-a-turning-point-1.9675

32. **Peter Woit**
December 16, 2011

Thanks Bernhard,

At least someone seems to have told him that there's no way this is 5 sigma…

I like how he claims that he published his "significantly more precise prediction" "just days before the CERN data were reported", without mentioning that it was four days after the CERN numbers were accurately reported on this blog and others.

Allan,

I just don't see that the new Higgs result (if confirmed) does anything much to constrain string theory vacua. You still can get almost anything you want. After claiming that the 125 GeV Higgs shows his string theory arguments are right, he does go on to claim that this implies gluinos showing up in the data any day now, definitely by summer. When there are no gluinos next summer though, he's just going to move on to other "string theory inspired" models. He has been engaging in this kind of nonsense for more than 25 years (see e.g his "String theory is testable, even supertestable" in Physics Today, back in 1997), with nobody calling him on it, no reason for him to believe they'll start now.

33. **Allan Rosenberg**
December 16, 2011

Ah, I didn't put Kane's name together with that article. I see your point. Perhaps the best way to do string theory would be to publish papers in entangled pairs–for every prediction a lab makes, it should publish another paper predicting the exact opposite. That way they can accumulate a string (sorry) of predictions well in advance of the rumors.

34. **Mark**
December 16, 2011

So, I was looking at slide 24 (gluino production cross section vs gluino mass) in Gordy's stringpheno 2011 talk:
http://conferencing.uwex.edu/conferences/stringpheno2011/documents/kane.pdf
and I'm not sure if the LHC will actually collect enough data by the summer to really test for gluinos. Let's say they collect 10fb^-1. From the plot, if the gluino mass is at, say 900 GeV, the production Xsection is 10 fb, which gives 100 events. However, taking into account the detector efficiency (at most few % according to the slide) is it realistic to expect some signal?

Today's Higgs Results

December 13, 2011
Categories: Experimental HEP News

The discussion after the talks is going on at CERN now, and the results that were presented agree well with what was posted here over the past week or so. This looks a lot like a Higgs near 125 GeV. Hiccups in the streaming make it difficult to impossible to follow the discussion. Caught Heuer at the end urging caution: "intriguing hints". This looks to me like a lot more than "intriguing hints": it's about what you would expect if a Higgs was there at 125 GeV, highly unlikely to see if there is no Higgs there.

The ATLAS results are here

Higgs to gamma gamma: 2.8 sigma bump at 126 GeV

Higgs to ZZ to 4l: 2.1 sigma (3 events near 125 GeV)

Higgs to WW to l nu l nu: Data not fully analyzed, 1.4 sigma excess at 126 GeV

Combination: 3.6 sigma excess at 126 GeV.

The CMS results are here

Higgs to gamma gamma: 2.34 sigma bump at 123.5 GeV.

Higgs to ZZ to 4l: 2 events seen near 126 GeV (expect .5 background)

Combination: 2.4 sigma excess at 124 GeV.

I see Tommaso Dorigo is posting a detailed analysis here under the title "Firm Evidence of a Higgs Boson at Last!". He's likely to be the best source around for a discussion of the details.

Update: Go to the blog of Philip Gibbs now to take a look at his (highly unofficial) plots of the combined ATLAS+CMS+Tevatron results on the Higgs. You might also want to check out Matt Strassler's blog entry about this, which wins the award for being downbeat ("Inconclusive, As Expected"). For some reason he is incensed by Tommaso Dorigo's "Firm Evidence".

Comments

1. **anon**
   December 13, 2011

   Though still too early to say, congratulations to Prof. Peter Higgs!

2. **Albert Z**
   December 13, 2011

   I would not be surprised in the least if the wheels fell off this latest Greatest Bump Hope within weeks.

   Then again, I am much less skeptical of a Higgs boson than the ad hoc "WIMPs", since the standard model, while quite heuristic, at least has considerable empirical support.

   Albert Z

3. **Foster Boondoggle**
   December 13, 2011

   What's the SM width of a 125 GeV higgs?

4. **Douglas Natelson**
   December 13, 2011

   I find your response interesting, Peter. Matt Strassler seems considerably more cautious, based in part on look-elsewhere concerns and whether the CMS number (123-ish) is compatible w/ the Atlas number (126-ish). As someone with no dog in this hunt, it'll be fun to see how this plays out, and particularly the media reaction in the short term.

5. **Peter Woit**
   December 13, 2011

   Foster Boondoggle,

   See this comment

   http://www.math.columbia.edu/~woit/wordpress/?p=4212&cpage=1#comment-101006

   The SM width is very narrow, so the observed width is due to the resolution of the detectors. Commenter thinks 3-4 GeV. The compatibility of the two gamma-gamma signals is a very interesting issue I'd love to see a detailed analysis of, much more relevant than the "look-elsewhere" effect you need to worry about if you only had data from one experiment. My impression is that, at this level of statistics, taken together they're reasonably compatible with a 125 GeV SM Higgs (and seriously incompatible with nothing being there at all…)

   Doug,

   Just as you can accurately describe the same glass as "half-full" or "half-empty", you can accurately describe this as "inconclusive" or "firm evidence". No one is claiming that this yet reaches the conventional 5 sigma level for claiming a discovery. An accurate description of the situation would be that the data shown is, given the statistics, consistent with a 125 GeV SM Higgs. It is fairly seriously inconsistent with the no Higgs hypothesis. More expertise in statistics than I have would be required to properly quantify the relative probability of these two hypotheses, perhaps that's something that will get done publicly soon.

6. **Trulo**
December 13, 2011

> the standard model, while quite heuristic, at least has considerable empirical support.

Albert, in this context "considerable empirical support" is the mother of all understatements.

7. **Roger**
December 13, 2011

I assume that you are preparing to write a rebuttal when Kane publishes an article about how this result confirms string theory.

8. **Peter Woit**
December 13, 2011

Roger,

Already done (on both sides), see the previous posting:

http://www.math.columbia.edu/~woit/wordpress/?p=4262

9. **VP**
December 13, 2011

Anyone have any comments on the fact that the ATLAS bump is 126-ish and the
CMS exclusion lower limit at 127 GeV?

10. **Peter Woit**
December 13, 2011

VP,

Other than that they're consistent?

What I'd really be interested in seeing is some attempt to sensibly combine the two gamma-gamma datasets. Would the result have a bump statistically consistent in width and size with the SM Higgs? As far as I know the answer is yes, but I'd love to know for sure.

11. **anon**
December 13, 2011

What's the implication for possible funding of ILC?

12. **Richard**
December 13, 2011

The significance of this announcement is that it is insignificant.
The press conference was almost certainly called to defend against funding cuts that are likely in a time of austerity and Euro-stress. The HEP community has gotten quite sophisticated at PR. Big physics needs big money. With the French under pressure to cut spending, the LHC folks need some smoke and mirrors to keep the Euros flowing.

Call me when you get to five sigma.

13. **Low Math, Meekly Interacting**
December 13, 2011

Assuming this is the prelude to a firm discovery, and the mass is just about what the result indicates, anyone care to comment on the potential for probing new physics at the LHC? I've seen some rather pessimistic assessments, i.e. this might be the harbinger of a desert all the way up to the GUT scale. A reasonable conclusion?

14. **Peter Woit**
December 13, 2011

anon, Low Math, Meekly Interacting

Knowing the Higgs mass means one can calculate exactly how well it could be studied by an ILC with different design parameters. If the LHC finds the Higgs is really there and behaves just like the SM says, and finds no other new physics, the case for not only the ILC, but any other expensive energy-frontier collider is going to be hard to make.

In that scenario, there's a real danger that what we saw today was the crowning achievement of this kind of experimentation, with prospects for the future rather dismal. For that reason, I and many others would have been a lot more excited if what we heard was that a SM Higgs was excluded or nearly excluded over the low mass region.

The LHC really needs to find SOMETHING that disagrees with the SM, or there will be trouble ahead…

15. **H125**
December 13, 2011

I feel the Irish Times newspaper gave us the most balanced headline for this 'discovery'

'God particle' may or may not exist.

That headline seems to summarise the situation for me.

Irish Times

16. **Michael Schmitt**
December 13, 2011

Hi Peter,

it is a bit too early to voice disappointment that this is “only” a SM-like Higgs boson. After all, we do not have discovery of anything yet. Secondly, we do not know whether it is a Higgs boson – does it have spin zero, can we show it couples according to mass by measuring some relative branching ratios, and is the production cross section the SM one? I agree whole heartedly that we need to break the SM (I have been hoping to do that for nearly 20 years now!) but perhaps we could wait and see what we have here – if anything at all. 😉

Michael

17. **Anon**
December 13, 2011

@H125, while it may be balanced, I find the terminology “god particle” simply detestable and misleading to the public as to what Physics is all about. As Laplace famously said: “Je n’avais pas besoin de cette hypothese-la.”

18. **Allan**
December 14, 2011

The date element of the link appears to have changed. The new, improved permalink is now:
[Higgs Boson Live Blog: Analysis of the CERN announcement]
Also (for now) is available from the main page.

A certain kind of statistician might ask “what is the largest bump reversal one would expect in a search of this nature?” The false alarm expectation might have been 3 sigmas, though the variance in this kind of meta statistic is often ludicrously high. In any case, I wasn’t all that impressed by the argument on one of the blogs that we’ve already seen an anomaly nearly as big. That could be normal on the path to discovery.

19. **Peter Woit**
December 14, 2011

Allan,

Thanks, fixed the link.

20. **ESB**
December 14, 2011

“The LHC really needs to find SOMETHING that disagrees with the SM, or there will be trouble ahead…”

Excuse me Peter, but if the LHC simply confirms the SM, what phenomena through which experiments, in your opinion, should the next generation of particle colliders study ?

This is a question which any proponent of higher energy colliders beyond the LHC can be expected to address.

21. **Samuel Prime**
December 14, 2011

I don’t have the time to read those papers to get an answer to this, but do I understand that the evidence so far obtained points to the SM Higgs and/or to a muti-Higgs scenario (where one can have as many as 5 Higgs particles)? (Or perhaps I’m uninformed about the number of Higgs in the Standard Model.)

Secondly, is the evidence supportive or consistent with non-SM Higgs?

22. **David Summers**
December 14, 2011

Michael,

Re a Higgs being spin zero.

if we see a resonance in the photon-photon channel, then we have the indication that some object is decaying to two photons.

Now there is a theorem (is it Yangs theorem – I forget) that only spin 2 or spin 0 particles can decay to two photons – its impossible for spin 1 to do this.

So if a resonance is decaying into two photons, is probably spin 0 (e.g. a higgs) – or something totally unknown (spin 2).

David.

23. **Bernhard**
December 14, 2011

Does anyone know if the seminar was recorded and posted somewhere? The transmission was so crappy I gave up in the middle and would be nice to see it wihout the hiccups.

24. **Anon**
December 14, 2011

Bernhard, isn’t it ironic? It’s the 21st century, and though we still don’t have flying cars, we are quite possibly on the verge of finding the Anderson-Higgs. Yet we cannot manage to broadcast a intelligible public announcement of this magnitude, which 20 years ago might have been done perfectly well by TV signal.

25. **tulpoeid**
December 14, 2011

The phrase “inconclusive, as expected” is not “downbeat”; it’s a very objective description of the results and of what >95% of cern experimentalists think right now (95 is a subjective but rather well-informed number).
Honestly, if lhc shut down *today*, would you be completely honest with yourself in believing that higgs is there? What’s with the hurry people? Find something else to play with for a few more months and then everything will be fine.

26. **Peter Woit**
December 14, 2011

tulpoeid,

I think “inconclusive, as expected” would have been a good headline for this past summer’s Higgs results. The latest results deserve something different and better.

27. **Peter Woit**
December 14, 2011

Samuel Prime,

The evidence presented is still not sufficient to claim discovery of a SM Higgs, although it is consistent with such a thing. It’s also consistent with all sorts of other possible Higgs sectors, especially if they’re not that different than a 125 GeV SM Higgs.

I’d summarize by saying that until now the experiments were just seeing random haze, but the latest results show something emerging from the haze, in exactly the way you’d expect if there were a 125 GeV SM Higgs. However, this is still a ways from being completely sure there’s something there, a long ways from seeing exactly what it is and whether it matches closely with what the SM says it should look like.

28. **VP**
December 14, 2011

If you look at Philip Gibbs’ “unofficial” combined exclusion plots for both November and December, you see that the 95% CL line (black line) is consistently at the upper edge of the two standard deviation band for the mass range of 115-170 or so GeV. To me it says that there is something at low masses (backgrounds?) we don’t understand. Yes, I know, different channels have different backgrounds but still, there is something funny. Wouldn’t this significantly dilute the statistical significance of any bump/excess? Wouldn’t that explain a higher likelihood of excess events in several channels?
http://blog.vixra.org/2011/12/13/the-higgs-boson-live-from-cern

29. **mp**
December 14, 2011

I am confused as to why everyone is hoping for physics beyond the SM. Why isn’t it good enough for the LHC to confirm the SM?

Is this purely a funding argument?
A, more fun, argument?

30. **Peter Woit**
December 14, 2011

VP,

I don’t think one can take seriously 1-2 sigma size deviations from expectations. Having systematic problems of this size in the understanding of backgrounds wouldn’t be unusual at all. That’s why you don’t start talking about “evidence” until you get to 3 sigma.

mp,

There’s a list (fairly short) of ways in which the SM is unsatisfactory. To my mind the most important one is that it’s incomplete: there’s a list of questions you would expect a fundamental theory to answer which it doesn’t (e.g. what determines the different values of the masses and mixing angles of elementary particles?). People who chose to work in HEP are rarely doing it for the money, more likely because they want to know the answer to such questions. Having an experimental result inconsistent with the SM would help a lot to give a hint as to where to look for an improvement of the SM. Without this, you can consistently take the attitude that the SM, incomplete though it may be, is the end of the story. The multiverse is often used to try and provide an intellectual underpinning for this, but, looking at the history of our increasing understanding of fundamental physics, I don’t see a good reason to believe that this pattern of progress has to stop here.

31. **Damasceno**
December 14, 2011

From a live chat in Nature.com happening now.

4:22 Gordon Kane: Recently we have published string theory calculations that imply the higgs boson mass is 125 GeV so if it is there are strong implications for connecting string theory to the real world, and for what the higgs discovery implies. we did that before the data.

Any surprise in the comment?

32. **Peter Woit**
December 14, 2011

Damasceno,

The Kane hype is just completely outrageous. I wrote about the source of it in the previous posting

http://www.math.columbia.edu/~woit/wordpress/?p=4262

and just added his comments from the Nature chat as an update.

33. **Peter Woit**
December 14, 2011

By the way, Science magazine tomorrow is running a similar live chat, this one with Rob Roser and Kane, see http://news.sciencemag.org/sciencenow/2011/12/live-chat-the-higgs-boson-at-las.html?ref=hp

34. **mp**
December 14, 2011

Well obvious I didn't mean "let's stop doing science because we confirmed the SM".

There are all kinds of unknowns in the SM that need to be figured out. In fact I believe there are decades worth of work ahead of us and strengthening our understanding of the SM and filling in the gaps sounds like pretty serious work.

I get that it would be more rewarding to find the next revolution for some physicists however not at all costs (I am talking to you string, multiverse, SUSY etc). I believe there should be a healthy balance between speculative and experimental science.

35. **jdm**
December 14, 2011

Peter,

First, thanks for making your reactions available. They're quite helpful in clarifying the implications for those of us who aren't in the loop.

Now, a nitpick: I don't believe mp was implying that high energy physicists are "in it for the money," but rather that the large-scale governmental/inter-governmental support that keeps very-high-energy physics chugging along would be seriously imperiled by a result that has long been touted as the missing piece in an almost complete theory (which appears to be the essence of your reply to anon, Low Math, and Meekly Interacting). This would seem to me to be a powerful and legitimate motivation for wanting to see the SM fail, and one not at all incompatible with equally legitimate intellectual factors you listed.

Presuming (however prematurely) that an SM Higgs is what CMS and ATLAS are seeing, that funding question would seem to me to loom large over physicists reactions to the results. I would even argue that the proximity of such funding questions probably has a chilling effect on the reception of "standard" results, which might otherwise generate the same kind of excitement the top quark did, despite some dissatisfaction with the SM as it stands.

-jdm

36. **Samuel Prime**
December 14, 2011

Thanks for your response, Peter.

37. **Peter Woit**
December 14, 2011

jdm,

Actually my impression is that things are the other way around. The people who run CERN (and have to be very concerned about their funding if they want to stay in business) have been worrying about not finding a Higgs. The worry is that the public and government officials deciding on funding would think: "these guys have been saying for years that they're spending all this money to find the Higgs, now it turns out they can't find it. Why should we give them any more money?" Once there's 5 sigma significance for a Higgs, I'm sure CERN will be appropriately and justifiably crowing about the discovery. Partly because they want the money sources to think of them as a success worthy of future support, but also because, well, they are a success and deserve to be proud such an achievement.

38. **jdm**
December 14, 2011

Peter,

Interesting. Thanks for that insight. Within the CERN context that makes a lot of sense. I'd still be surprised though if others, particularly in the United States, didn't look upon a no strings attached Standard Model result as reason to worry over the prospect of securing future funding, especially in the longer term. At any rate, it will be interesting to watch those dynamics as the play out.

-jdm

39. **Z**
December 14, 2011

There are still plenty of mysteries in HEP, like the anomalous magnetic moment of the muon, neutrino oscillations, dark matter, QCD in general…
The LHC will probably be funded for the next 15 years, if not longer. The ILC may be superseded by a muon collider design for the future, since a muon collider may be cheaper with existing infrastructure.

If nothing mysterious is found with the decades-long LHC run in the GeV/TeV sector, HE astrophysics may benefit since Chrenkov telescopes and space telescopes routinely see remnants of particles with energies several orders of magnitude higher than the LHC produces – and thus, they would be the only hope for seeing something beyond the SM. An grand-observatory class X-ray telescope studying neutron stars could also highly constraint QCD equations of state.

40. **VP**
December 14, 2011

Peter,
"I don't think one can take seriously 1-2 sigma size deviations….."

Perhaps I didn't make myself clear enough. My point is that if I consistently measure something at 2 standard deviations above the "no Higgs" hypothesis , then an one SD fluctuation would appear as a 3 SD effect. As appears in Gibbs' plots the measurements of both experiments for the mass range ~115-170 GeV are consistently 2 or so SD above the "no Higgs" hypothesis.

41. **Peter Woit**
December 14, 2011

VP,

In my reading of the plot, I don't see "consistently 2 sigma" excess, maybe 1-1.5 sigma. In any case, it looks to me like whatever is locally happening around 125 GeV is 1-2 sigma above other fluctuations and systematic effects. So, 68-95% chance it's real. I like Jester's rough guess of 80% (which takes into account the sort of thing this is: looking for something we have reason to expect to be there, not something that shouldn't be there). One thing that definitely is true is that this is not a genuine 3 sigma effect, in the sense of a 99.7% sure-thing.

42. **Anonyrat**
December 15, 2011

Brian Greene has an op-ed in the New York Times about the Higgs.
http://www.nytimes.com/2011/12/15/opinion/waiting-for-the-higgs-particle.html

43. **tulpoeid**
December 15, 2011

' ' I think "inconclusive, as expected" would have been a good headline for this past summer's Higgs results. The latest results deserve something different and better. ' '

Why?

They are (i) not perfectly understood yet, and they are (ii) consistent with both assumptions. Moreover, this was expected and well calclulated given the current integrated luminosity.
Also, have you said the same about all previous 3-sigma hep results? Would you mind writing a post with the history of all 3-sigma results past?
God, I'm really glad cern people are proving to be better than the crowd commenting on blogs these days and they are staying unaffected for the biggest part (the management might not be the best example of what I'm talking about at the moment). I think you are exhibiting a very uncharacteristic case of string-theorist-like way of thinking during this last week.

44. **Paolo Valtancoli**
December 15, 2011

In my opinion the standard model is valid up the grand unification scale! In any case since the four colour theorem proved to be useful to derive the right Higgs mass, one should extract all its consequences for the MSSM model…

45. **Bernhard**
December 15, 2011

tulpoeid,

the CERN people, here and include both analysis leaders + teams are very excited about this. See http://www.nature.com/news/live-qa-the-hunt-for-the-higgs-1.9642

Murray says: " We have no proof, but I polled 10 tof the leaders of the ATLAS search and my wife last night if they thought we had found it and they ALL said yes. But that is not proof, it could be over-excitement."

Got the picture? Nobody is claiming a discovery but the feeling of optimism among the experimental folks that this result will hold up is very high. They might be wrong and acknowledge that, but "inconclusive as expected" does not even begin to draw the correct picture of what people REALLY THINK about this result. That Strassler wants to be cautious that's fine, but again what a downbeat title.

46. **vmarko**
December 15, 2011

I remember seeing the CMS result in the H->4l channel to have observed 13 events, as opposed to the expected 9.5 at that peak. Is there any clear data published about the total **count** of excess events observed by CMS and ATLAS?

I mean, the luminosity of 5 inverse fb basically means they have produced 10^{14} or so collisions (essentially a very large number). Out of all that, they have filtered out only a **dozen** of events, and found 13 instead of expected 9 (in that channel). I guess that the total number of events (all channels summed) does not go above 100 or so. How much can you trust the statistics of that?

I could flip a coin 20 times, and get 13 tails plus 7 heads, instead of 10 plus 10. Does that mean that I am seeing some real signal about the coin fairness?

I believe that people could have a better intuition about how much to trust that 3.5 sigma result, if they knew the **actual number of observed events**, rather than looking at a fancy graph with a "peak that goes above the green and yellow strips". Is this data available anywhere on the net in a clear easy-to-read form?

Best, 🙂
Marko

47. **Peter Woit**
December 15, 2011

Marko,

You're just talking about 1 channel and 1 experiment. The interesting thing here is that there are 3 channels/experiment, no one giving much of a signal on its own, but they are all showing something. In the ZZ -> 4l channel, what's important is that you're seeing several events close to 125 GeV, with expected background at that energy very low.

48. **anonyrat**
December 15, 2011

Sorry, looks like comments on the old thread are closed, so posting it here. Part II of Norman Dombey's series on Abdus Salam is on arxiv.org.
http://arxiv.org/abs/1112.2266
"Abdus Salam: A Reappraisal. Part II Salam's Part in the Pakistani Nuclear Weapon Programme"

Abstract: Salam's biographies claim that he was opposed to Pakistan's nuclear weapon programme. This is somewhat strange given that he was the senior Science Advisor to the Pakistan government for at least some of the period between 1972 when the programme was initiated and 1998 when a successful nuclear weapon test was carried out. I look at the evidence for his participation in the programme.

49. **Peter Woit**
December 15, 2011

I realized I didn't respond to the question about numbers of events. Look at the CMS and ATLAS papers for each channel. For ZZ -> 4l they plot number of events vs energy, with the number of events in each bin 0, 1, or 2, expecting to see o in each bin. For gamma-gamma, they also give a plot of number of events vs energy, here the numbers in each bin are large (several 100/ GeV) and what you are looking for is a bump in the distribution.

50. **Bernhard**
December 16, 2011

Outrageous hype from M. Kaku at the Wall Street Journal:

"So far, one of the leading candidates to explain dark matter is string theory, which claims that all the subatomic particles of the Standard Model are just vibrations of a tiny string, or rubber band. "

http://online.wsj.com/article/SB10001424052970204026804577098382660789136.html

Apparently, Kaku already made a complete symbiosis of SUSY with string theory.

51. **Anand**
December 16, 2011

Looks like they will find the "Higgs.

Let the politics Nobel begin – by all three groups. First opportunity will be this weekend for Englert to work Nobel Committee member Lars Brink. Past performances were great on this topic.

http://cgc.physics.miami.edu/Miami2011.html

52. **Z**
December 16, 2011

If the Higgs is going to be confirmed, this is also good opportunity for the Nobel Foundation to change the archaic rules limiting the prize to three people before next year's prize. Even on the theory side, more than three people (including Peter Higgs) deserve the prize.

Perhaps, if selecting between an arbitrary number of people is too distasteful, awkward, or against the constitution/charter, the committee could also simply give the prize to the "CMS Collaboration" and the "Atlas Collaboration", letting those groups decide on how to split the prize money inside them. If corporations being people is good enough for the Supreme Court of the US, surely a scientific collaboration is a good enough person?

53. **simona**
December 16, 2011

Figures 1 and 2 in the ATLAS note show even a greater excess at ~102 GeV; however this mass range is left out in the statistical analysis in Figures 7 and 8 where the range begins at 110GeV. Any ideas on that?

54. **Peter Woit**
December 16, 2011

simona,

I don't know, but there likely is some innocuous reason, known to the ATLAS people. It's also possible they don't know, but figuring it out has been a relatively low priority. Whatever it is, LEP has ruled out the possibility that it's the Higgs, so ignoring it in these notes makes sense. The results presented this week in any case aren't the final analysis of this data, but whatever they could get done by the deadline of being ready to present on this date.

55. **Chris Austin**
December 17, 2011

LEP excluded the Standard Model Higgs below 114 GeV, but apparently more complicated Higgs sectors can still have some Higgs bosons with mass below 114 GeV, and be consistent with LEP. See for example [0811.3537](0811.3537).

56. **Chris Austin**
December 17, 2011

Jester wrote a great post on how a lighter Higgs could have escaped discovery at LEP.

The fact that ATLAS don't discuss the narrow excess at about 101 GeV in Figs 1, 2, and 5 (top right) of their Dec 11 note might simply be because the title is, "Search for the *Standard Model* Higgs boson". Although if you allow for the slightly shorter error bars at around 126 GeV, and that the excess there is in 2 neighbouring data points, the excess at about 126 GeV is perhaps a bit bigger than that at about 101 GeV.

57. **pah**
December 17, 2011

Chris Austin,

I see only a single-bin fluctuation, totally insignificant. It's debatable if there's a significant excess at 125 GeV, but what you're seeing at 101 GeV is nothing.

58. **Chris Austin**
December 18, 2011

In my second comment I should have said "more significant" rather than "bigger". I agree that the excess at 101 GeV is not significant.

59. **Ric - ½ g tr(Ric) = 8πT**
December 30, 2011

I think, after this is settled a discovery, we should focus on why the mass is so close to the half of the VEV (246 GeV). My thoughts is that the coupling constant is as that, yet anomalies and RG corrections drive it up to ~125 GeV.

Physics on the Fringe: Smoke Rings, Circlons and Alternative Theories of Everything

December 16, 2011
Categories: Book Reviews

There's a new very thought-provoking book out from Margaret Wertheim, entitled Physics on the Fringe: Smoke Rings, Circlons and Alternative Theories of Everything. Much of the book is the beautifully told story of "outsider" physicist Jim Carter, who has spent much of his life developing an alternative fundamental physics theory based on objects he calls "circlons".

I have to confess that, while respecting the impetus that leads people to develop such theories, I have essentially zero sympathy for this kind of thing as science. As Wertheim explains

> In Jim's theory of the universe, *everything* is mechanical; like INCOBO , the world he imagines is made up of simple mechanically interlocking parts. As with his engine, none of the parts are complicated and you don't need much mathematics to understand how it works…
>
> One way to think about what Jim Carter is doing is that he insists on a universe he can comprehend. As with the old Chryslers and Cadillacs that grace his front yard, Jim demands a cosmos he can figure out for himself.

One way in which I'm very different than Jim Carter is that I've never been one for insisting on ideas that I can figure out for myself. I'm grateful for and fascinated by the fact that there's a huge amount of knowledge about the universe out there discovered over centuries by a collaboration of a long list of brilliant people, and many places to try and learn about it. This kind of learning is a joy, and not being willing to engage with and try and appreciate the accumulated wisdom of the human race to me makes no sense. When I got to the point of learning about quantum theory, it became clear to me that this was something of great power and beauty, carrying the lesson that at a fundamental level the world is very different than the mechanical picture we derive from our human-scale intuitions. At the same time, fundamental physics and mathematics are deeply intertwined, with the deepest ideas in mathematics showing up when one tries to understand the deepest questions about physics.

The basic problem with efforts like Carter's is that the tools and ideas he is using just aren't powerful enough. There's no way they can be used to understand the universe (and test one's understanding by calculating from theory and comparing to experiment) with anything like the power of the Standard Model or general relativity. Anyone who wants to do better than the Standard Model or GR has to come up with equally powerful ideas. It seems unlikely that this can be done by any means other than understanding well the ideas behind these theories, as well as their weaknesses, as a starting point to look for something new.

Wertheim discusses a range of other failed and "outsider" ideas about physics. She sees an analog of Carter's vision in the the 19th century work of prominent scientists like Tait and Thomson, who studied smoke rings as a phenomenon that might model physics at the atomic scale. More recently, Steven Wolfram's *A New Kind of Science* featured claims that conventional fundamental physics could be replaced with ideas about cellular automata. Wolfram is a MacArthur winner, and Ph.D. in particle theory, so it's not by credentials alone that one can identify "outsiders" barking up an unpromising tree.

Remarkably, Wertheim explains that her motivation for writing the book came from attending a 2003 conference on string cosmology at the Santa Barbara KITP. This was at the beginning of the "string theory anthropic multiverse" madness which has afflicted the field since that time. In 1998 she had attended with Carter an annual meeting of the NPA (National Philosophy Alliance), a group of "outsider" physicists, and she was shocked to find the KITP hosting something that seemed not obviously different:

> That string cosmology conference I attended was by far the most surreal physics event I have been to, more bizarre than any NPA event for the very reason that this was not a fringe affair but a star-studded proceeding involving some of the most famous names in science…
>
> After two days, I couldn't decide if the atmosphere was more like a children's birthday party or the Mad Hatter's tea party – in either case, everyone was high…
>
> … the attitude among the string cosmologists seemed to be that anything that wasn't logically disallowed *must be out there somewhere*. Even things that weren't allowed couldn't be ruled out, because you never knew when the laws of nature might be bent or overruled. This wasn't student fantasizing in some late night beer-fueled frenzy, it was the leaders of theoretical physics speaking at one of the most prestigious university campuses in the world.

Besides the difference in credentials, there is an important difference between most recent mainstream theoretical work, even when in multiverse madness mode, and that of the "outsiders" of the NPA. Mainstream theorists recognize that they need to be compatible with the SM and GR. What they are doing is working within a conjectural extension (call it "M-theory") of the SM and GR, claiming to preserve the successes of those theories. In principle, working this way should provide a very tight constraint on what you can do. The problem though is that one doesn't know exactly what "M-theory" is. All one has is a list of conjectured characteristics, and these seem to be weak enough to allow a vast array of "vacua", of such complexity as to effectively remove much in the way of constraints on what you can observe at low energy. Since the conference Wertheim attended in 2003, there have been huge efforts made to extract some non-trivial implications out of this "landscape" scenario, with no success. In practice things have in many cases degenerated to the level of "outsider" physics: anything goes, and one ends up with a group of people making ambitious claims about their wonderful theory, with no conceivable way for such claims to be tested or backed up.

All in all, I think this is an important book, one which raises in an interesting way fundamental issues about how people think about and conduct research into fundamental theoretical physics. We're at an unusual point in the history of the subject, one where the foundations of how this kind of science has traditionally been done are being questioned. Wertheim's contribution to this questioning is worth paying attention to.

For some other recent reviews, see John Horgan here, and Michael Shermer here.

Comments

1. **J. Caleb Wherry**
   December 16, 2011

   When you say "…I have essentially zero sympathy for this kind of thing as science" are you talking specifically about Carter's work or do you mean this more generally? For instance, do you have the same feelings towards Penrose's Twister Theory? I would not consider Penrose (and others) "outsiders" as Carter is but am just curious if your statement encompassed people (and theories) such as Penrose. Thanks!

2. **Peter Woit**
   December 16, 2011

   J. Caleb Wherry,

   As I try and explain in the next paragraph, my lack of sympathy is for the idea of new foundations of physics built out of simple mechanical ideas, requiring little mathematics or anything beyond everyday intuition to comprehend. Penrose's twistors are very different.

3. **Anonymous**
December 16, 2011

I used to be very much in favor of alternative theories to physics. I still am, but after studying physics I only endorse the efforts of folks that are at least trying to maintain some consistency with what we already know. But it really gets me when people try the angle of trying to take the math out of physics. It reminds me of Sarah Palin trying to say we need a regular person in the white house and not a politician. I think it would be great if everyone could understand all the intricate details of physics, but the lazy approach or the idea that we can "dumb down" physics is completely absurd.

I have people tell me that sacred geometry leads to a theory of everything with almost no math. But these sorts of things are far from rigorous. So again, I understand science is driven by many alternate approaches, but like you said, there are some avenues that are just silly to go down.

4. **anon**
December 17, 2011

The crackpots who disregard mathematics are easy to identify. But on the other hand, there are crackpots who know much more sophisticated math than most physicists. This category even includes some professional mathematicians who stray into physics. Their problem is that they think whatever beautiful math must lead to promising theory in fundamental physics, with total ignorance of phenomenology and experiments.

5. **Bee**
December 17, 2011

I think the issue has two causes. First, there is an ever widening gap between what's going on in modern theoretical physics and what the average person has learned. Second, the average person has no clue how large that gap really is. So there is the occasional know-it-all who believes he has solved all problems without even understanding what the problems are to begin with. I believe this is supported by an abundance of terribly dumbed-down popular science books that leave the reader with the justified feeling like they can do better, without providing a guide how to, or without at least saying it's dumbed-down.

I have no clue what that particular string cosmology conference is she is talking about, but I suspect her impression is partly fed by her own lack of knowledge. I am not very fond of all the bubble-stuff, but some aspects of string cosmology are actually quite interesting, esp when it comes to CMB polarization. I am hoping we'll be able to sort out some models in the next years, and that will be a good thing.

6. **Paulibus**
December 17, 2011

Peter : you're quite right; Margaret Wertheim's book is thought provoking, especially for those frustrated by the sterility of lots of today's theoretical physics. Your first quote is particularly apt:

…what Jim Carter (her special crackpot) is doing is (insisting) on a universe he can comprehend. As with the old Chryslers and Cadillacs that grace his front yard, Jim demands a cosmos he can figure out for himself.

That's just what string theorists seem to have been (collectively) doing for the last forty years or so. They accepted Einstein's optimistic aphorism about the surprising comprehensibility (for us) of the universe. It's been brave of them, but sadly they've not had the restraining benefit of making confirmed predictions to keep them on the rails.

Perhaps, though, they've taught us (as Margaret Wertheim seems to be suggesting) that such optimism may just be anthro'centric hubris — like Einstein's happy thought, so well justified in that case by his geometrical description of gravity. But here in the 21st Century biologists and paleoantologists have pretty well confirmed for us Darwin's opinion, namely that the world is packed with us and our animal cousins, all quite similarly driven by quite similar DNA. Some, like my cat and the vervet monkeys in my trees, (probably) lack the capacity for understanding the universe fully. Perhaps we are also so limited, despite our wonderful discovery/invention of the Platonic world/language of mathematics.

Thanks for reviewing this book.

7. **Christian Takacs**
December 17, 2011

It's simply scandalous that anyone should try return physics to the understanding of the mechanical underpinnings of how the universe functions, instead of the far superior methodology of faith in anthropic principals, imaginary multiverses, virtual messenger photons, Higgs Bosons (31 flavors don't cha know!!), renormalization of infinite answers from finite quantities, and logical paradoxes loaded high with contradictions galore lined up like added features in a car salesman's pitch. After all, EVERYONE knows that all REAL understanding has ALWAYS come from the established academic elite, not common inventors, astronomy buffs, bicycle mechanics and patent clerks who presumed to try something without the blessing and peer review of their betters. Please for the love of truth, put the Hubris away Peter. From the debates that have gone on in your own site, it is quite apparent to even the lowly layman that esoteric theoretical mathematics have run rampant, wild, and free for decades… and made a hash of comprehending physics as anything other than verbose numerical onanism, or science fiction. Take your pick. Either way, someone is coming to change things . That someone is not going to be coming from inside your august academic ranks, or speak in the buzz words and tongues preferred by the HEP mathematical priesthood… that much is painfully obvious.

8. **Dave**
December 17, 2011

@Christian
We are on the verge of discovering a particle which allow us to confirm the correctness of a theory explaining how a unified force is observed as two separate forces. This is quite a feat of theoretical and experimental physics and is truly Nobel prize winning. Along the way it will also the falsification (or confirmation of the correctness) of a number of models of symmetry breaking. All of this came from the particle physics mainstream. How does that fit into your worldview ?

Also, how do you know the next big thing will come from outside of the pp mainstream ? You write that this is "painfully obvious" – can you share your soothsaying secrets and lay down the chain of logic which allows you to make such a declaratory statement devoid of uncertainty ?

9. **james**
December 17, 2011

Great review. I think in between the outsider crackpots and the fading string theorists there's reason to be optimistic. A new generation can start fresh. I dont think the foundations of science will change. String theory failed to predict the physical constants of our universe, la la la, so string theorists give up with the possibility of multiverses with a wide array of constants. Kind of embarassing, but understandable. They can work on this stuff in their later years.. Fresh minds come out of school. There'll be more people working on a consistent mathematical theory that has the standard model and its 40+ parameters as a unique prediction.

10. **Bobito**
December 17, 2011

The basic failure of the best, most imaginative physics crackpots is to forget that physics is an empirical science, and that what does not explain observed phenomena in a way useful for later building instruments and devices is really quite useless, and, moreover, not physics.

The psychological failure that leads to this is often related to a deeply entrenched arrogance. It is simply impossible that one person can reproduce the cumulative efforts of the hardest working, most imaginative scientists over several centuries.

11. **Yatima**
December 17, 2011

@Takacs

You write sarcastically: "all REAL understanding has ALWAYS come from the established academic elite, not common inventors, astronomy buffs, bicycle mechanics and patent clerks"

Please remind yourself of the fact that the fêted "patent clerk" had a prior good grounding in / knack for mathematical physics and could base his understanding on the previous work of Maxwell, Hertz, Lorentz and Poincaré (with the equations already fully formed) as well as the experiments of Michelson and Morley and "just" had to look at the ideas in a new way. And even then, Minkovsky had to come in to clean up the formalism. The next step (1915) came after the use of *more* amazing math combined with persistency and hard work over 10 years.

"That someone is not going to be coming from inside your august academic ranks, or speak in the buzz words and tongues preferred by the HEP mathematical priesthood… that much is painfully obvious."

The Chinese Cookie you will open today says "You are wrong". Now go and start working on Group Theory.

12. **Danny Black**
December 17, 2011

Yatima,

one should also remember that special relativity was not really a major contribution as it was "in the air", if Einstein hadn't come up with it someone else would have. As you pointed out Lorenz – from the "established elite" – had already come up with the key transforms and Poincare was close to establishing the same facts. Einstein essentially let the equations speak for themselves.

By the time Einstein DID come up with something original and unexpected – General Relativity – he was well established.

I am struggling to think of a major maths or physics breakthrough that was made by some "crank" outside the "academic elite" in the last 200 years, especially as one can argue the term "academic elite" only really gathered force in the late 19th century.

13. **Peter Woit**
December 17, 2011

Bee,

Wertheim has a physics degree and some experience among physicists. The string cosmology conference was in 2003 at the KITP, probably one of the first conference dominated by "the landscape", so at the height of enthusiasm for that idea. More recent conferences on the topic might have struck Wertheim differently, with the landscape proponents having eight years of going nowhere making for a lot less enthusiasm.

14. **Joel Rice**
December 17, 2011

There may be a lot of dumbed down popularized stuff out there, but there is also Veltman's book, not to mention Feynman's QED . If these folks want to see what they are really up against they should be reading Schweber's QED and the men who made it, and contemplating what it was like when Dirac was pestering Feynman with "but is it unitary" ! It is unconscionable to have the bad books driving out the good.

15. **Giotis**
December 17, 2011

"Even things that weren't allowed couldn't be ruled out, because you never knew when the laws of nature might be bent or overruled."

This isn't true. The fundamental principles of nature cannot be bent or overruled in the multiverse picture.

No String theorist or cosmologist could have made such assertion. Obviously the author hasn't the slightest idea about String theory or Cosmology. Her reaction is the naive reaction of an outsider.

16. **Jack Lothian**
December 17, 2011

Giotis,

Outsider-insider?? Just what do these terms mean. If she has a physics degree, she has to be in the top fractional percent of people who might be capable of understanding this type of complex math/physics. Are you saying only string-theory practitioners can understand this stuff and everyone else is a dilettante and incapable of offering valid criticism?
Her comment: "Even things that weren't allowed couldn't be ruled out, because you never knew when the laws of nature might be bent or overruled." May be a bit over-the-top & phrased in a way that I am sure no true believer would phrase it but the comment is a not an unreasonable précis of many comments that I have read here.

17. **Peter Woit**
December 17, 2011

Giotis,

If you interpret "Laws of Nature = Lagrangian of SM + GR", what she is saying here makes perfect sense.

About this time, possibly in reaction to the same conference, David Gross in his conference talks started quoting Churchill's words during the Nazi bombardment of London ("never, never…. give up!") as a call to arms to particle theorists to resist multiverse madness. It wasn't only "outsiders" who were appalled by this…

18. **Giotis**
December 17, 2011

Peter,

When we talk about laws of nature we mean the fundamental principles. Fundamental laws are the principles of QM for example and these you can't bend.

The content of SM is a derived concept, not a fundamental law/principle.

Regarding GR, the low energy limit of String theory is just supergravity which fully respects GR as an effective description of Gravity.

The author (due to ignorance I believe) gives the false impression that everything goes in the multiverse. This isn't true. There are fundamental physical laws which must be respected and deep physical principles you can't bend.

Jack Lothian,

"Are you saying only string-theory practitioners can understand this stuff and everyone else is a dilettante and incapable of offering valid criticism?"

No, but of course you must have studied the theory at least in some depth to have an opinion. This could take months or years depending on your background. The author obviously hasn't done that and a Physics degree doesn't mean anything in that respect.

19. **D R Lunsford**
December 17, 2011

Peter wrote (beautifully)

"One way in which I'm very different than Jim Carter is that I've never been one for insisting on ideas that I can figure out for myself. I'm grateful for and fascinated by the fact that there's a huge amount of knowledge about the universe out there discovered over centuries by a collaboration of a long list of brilliant people, and many places to try and learn about it. This kind of learning is a joy, and not being willing to engage with and try and appreciate the accumulated wisdom of the human race to me makes no sense. "

Yes that's about the entire matter right there -it's really a cultural matter. As long as the culture is respected, you can stand up for whatever you want to – but you have to make some effort to take in what has been done before. If you don't at least try that, then you don't get a ticket to enter the contest.

And what's really frustrating, is to see that culture threatened by unphysical ideas, and there's not much you can do to change it. At some point you lose patience and become Christopher Hitchens.

-drl

20. **Marco Masi**
December 17, 2011

Woit says: "When I got to the point of learning about quantum theory, it became clear to me that this was something of great power and beauty, carrying the lesson that at a fundamental level the world is very different than the mechanical picture we derive from our human-scale intuitions."

And this is the main reason I don't believe in string theory. A world which is too 'classical', intuitive, and made of 'objects' like strings or membranes, D-branes, etc. which please homo sapiens' belief system and his human-scale perceptual mind made of forms in space and time, but which is far from the very different story QM is trying to tell us. What we know about is a micro-world where forces are exchanged indeterminstically, showing up as particles and yet behaving like waves, in a nonlocal context where things appear entangled in space (and possibly also in time), etc. Where is there any place left for 'objects' like vibrating strings having tension and extension, or sharp N-dim curled up spacetime manifolds? There is nothing like that 'down there', neither more no less than there are colors, tastes or smells. These are only our anthropocentric reifications. And unfortunately this is what most 'outsiders' have in common with those who they claim to challenge with their 'new' theory when they think in terms of 'circlons' type of stuff.

So, what about going back to basics? But we have been told "shut up and calculate". In fact it worked for some time egregiously and that gave us the SM. String theorists where encouraged by this and now want to 'save the appearances' at all costs. But epicycles and defernts did that for a long time too, and yet turned out to be the wrong worldview nevertheless. I would not be surprised to see history condemning string theorists as the new Aristotelians.

21. **Giotis**
December 17, 2011

I forgot to respond about David Gross,

Gross reacted this way for completely different reasons. He never said that the theoretical structure/reasoning which leads to the Multiverse picture is crazy or unfounded. He just believes that is too early to make such claims since the theory is not mature enough to assert that the landscape is indeed its definitive prediction.

22. **LHB Jr**
December 17, 2011

Outsiders can be useful in breaking GroupThink. One excellent example of this is Yuri Knorozov. He was a pioneer in deciphering Mayan script. He lived in the Soviet Union, isolated from Western educational institutions, and brought a fresh (and ultimately correct) view of how to read Mayan script.

Interestingly enough, he did this when progress in decipherment had stagnated.

Given the vast democratization of information resources provided by the Internet, I don't see why a particularly clever person, with relatively modest training compared to today's elite, couldn't spark an insight that leads to a major advance in theoretical physics. Maybe it will require an outsider. As Yuri's Wikipedia page says:

He(Yuri) was perhaps shielded to some extent from the ramifications of peer disputation, since his position and standing at the institute was not adversely influenced by criticism from Western academics.

23. **Peter Orland**
December 17, 2011

Giotis,

You say the standard model is a “derived concept”, but the rules of quantum mechanics are “fundamental”. By that standard, all Lagrangians are derived concepts (if I understand your meaning).

I have seen similar arguments before by landscape advocates, trying to say that the standard model is not a theory of nature, hence we have to something more “fundamental”. This is philosophy, not physics. Furthermore, it is not good philosophy.

The principles of quantum mechanics aren’t very useful as form. They require content. I don’t care whether the content meets somene’s standard of fundamental.

This wordplay distinguishing “derived concepts” from “fundamental laws” ones is not meaningful. There is no physics without the former, such as a force law or a Lagrangian. By itself, the latter is hot air.

24. **anonomous**
December 17, 2011

If somebody found a way to find more symmetry in the SM with fewer equations that reduces the number of free parameters, then that should be considered a breakthrough. This is not the same as dumbing down the math, but simplifying it to within a greater symmetry of known results. Of course the new model would have to match all known experimental results and predict new ones – but clearly the current path between GR and the SM is not working, so we need to try something new.

25. **Peter Woit**
December 17, 2011

Giotis,

I don’t think Gross (or Churchill) was saying that “it’s too early” to give up. They were both saying “Never, Never, Never, etc. give up”, which is kind of different. It would be interesting to ask Gross what he would do in the event that string/M-theory is completely understood, but shown to have so many vacua of such complexity that you could never predict anything. I suspect he’d say that one should still not give up, but look for an alternate, better theory that was predictive. But, you’d have to ask him…

26. **Daniel L. Burnstein**
December 17, 2011

There is no reason why the likes of Fermat, Ramanujan, Faraday or Pascal not to exist today.

I know the common belief is that science is now a collective effort,. Yet there’s also no example of revolutions of science that have not been brought upon by individuals who thought outside the box, and often outside institutions. There is a reason for that. The kind of synthesis of knowledge and intellectual leaps required can only be done within the individual mind.

27. **Hendrik**
December 18, 2011

Thank you Peter, that’s a great review.

The fascination with outsider science springs maybe from the fact that we all have a bit of the outsider scientist in us,- after all, to solve a problem with substance requires thinking outside of the box. (Of course there are degrees of how far out an outsider is.)
According to Kuhn, scientific revolutions originate with the outsiders (the “seers” of Lee Smolin?), so it is good to have them around.

But perhaps the new element illustrated with string theory, is that an outsider theory can become mainstream, not by surpassing the experimental & predictive successes of its predecessors, but by sociological forces alone. Maybe an example that an outsider theory can be detrimental to science. An indication that the mechanisms of science (or physics) may be in disrepair.
–

28. **Robert D. French**
December 18, 2011

Carl Sagan addressed this in an episode of Cosmos. There was a psychiatrist who tried to introduce a crackpot theory for the creation of the solar system. His hypothesis was pretty easy to dispute, but he was censored by mainstream scientists. Sagan’s point was that we don’t know where the next big insight will come from, so we shouldn’t reject ideas out of hand just because they don’t meet a certain aesthetic. He also said that the suppression of ideas has no place in science.

29. **G Alex**
December 18, 2011

Yes, everybody officially welcomes ‘outsiders’. If you ask academics if they appreciate free thinking and original ideas everyone will tell you “of course I do, I even encourage it”. But that’s easier said than done and truth is different. The present academic system on the contrary accurately rejects from the outset those who dare to come up with their own research project based on unconventional ideas, especially if it does not fit with their ideological background. I found many of these then complaining that their students, despite being brilliant and well versed in the subject, for some reason they can’t understand, lack nevertheless of novel insights. But that’s no wonder… And when I hear about how ‘suppression of ideas has no place in science’ I can only lough about it. If not suppressed they are certainly discouraged to say the least. It is something intrinsic and deeply ingraved in our academic mindset itself.

30. **Bob Levine**
December 18, 2011

I’m afraid that some of the commenters here are forgetting a basic principle of scientific innovation: at *some* point, the novel approach must account at least equally well for the full range of facts as the conventional approach(es) it seeks to replace. People who complain about the rejection-out-of-hand of new or highly unorthodox hypotheses (or, at a more fundamental level, theories) should ask themselves if those hypotheses have come anywhere close to duplicating the predictive/retrodictive successes of the current ‘mainstream’ treatments, whatever they happen to be. That’s what the notion ‘burden of proof’ is all about. As far as I can tell, having *that* engraved in the ‘academic mindset’ is the reason why progress in science occurs is even possible.

31. **G Alex**
December 18, 2011

Yes precisely, at *some* point. But how can something get at that point if it is rejected out-of-hand? While the ‘mainstream’ approaches are frequently seen to be allowed to go far beyond that point, even if they failed in producing testable results.

32. **Fabien Besnard**
December 18, 2011

I think that the important, and difficult thing to do, is to distinguish between people with very unorthodox ideas (call them "outsiders" if you want, but they might also be found in academia), and crackpots (they can sometimes be found in academia too). Some have insisted here on the point that new ideas must always be compatible with known facts : this is certainly true but it is sometimes a difficult thing to judge (a striking example here is string theory). So it can't be taken against a new idea that it does not fit at once with all the known phenomema, since it might be a very long process to derive these phenomena from a theory. It is even possible that a incomplete, but in the end right, theory be for some time in violent disagreement with observation (think of the heliocentric theory which was unable to explain why we are not flung in outer space by the rotation of the Earth until Galileo came with a new theory of motion).
Perhaps a demarcation line could be found between reactionnary ideas and ideas which are only unorthodox, even wild, speculations.
Crackpots often tend to hold reactionnary ideas. By this I mean that they want to go back to an outdated world view : whatever the next revolution in physics will be, it will certainly not be to go back to some incarnation of Newtonian mechanics.

33. **skeptical**
December 18, 2011

People here seem to be suggesting that professional physicists should carefully consider every idea thrown at them in blog comments or on websites. Obviously, if they were to do this they would not have time to do anything else. The unfortunate fact is that the vast, vast majority of ideas from outside academia are nonsense (some ideas from inside academia are also nonsense, of course). I know it was true in the past that plucky outsiders with a different viewpoint could make a big impact, but that is simply not possible today. To see how to go beyond our current best theories you absolutely must be well-versed in them, and that is well-nigh impossible if you're not a professional. It's a pity that this is the case, but there you are.

34. **Mark Bennet**
December 18, 2011

Answering skeptical: there is a tension between being so inducted into the current model that you can't think differently, and knowing enough about the current model that you are confident/foolish/bold (same thing?) in thinking differently.

If a different model from the current one is required – think relativity or quantum mechanics -v- what went before – the person who makes the next great advance will have to be versed in the right material which is radically different from the Standard Model. They will inevitably also have to know why the Standard Model is so compelling over the range of physical experiment for which it "works" – so will need to have that aspect tied up: and that will no longer be possible for an amateur observer like me: probably.

35. **anonymous**
December 18, 2011

What happens is that the constant rejection of all ideas that seem crazy (like Plate Tectonics, which very slowly gained acceptance even though it had quite a bit of evidence), slows down growth in the field and creates a very disheartened attitude from the public, some of whom think about becoming scientists and then wonder if they can handle the bureaucracy. Slow and careful, but not so slow and careful that the field dies an early death.

36. **Bob Levine**
December 18, 2011

"What happens is that the constant rejection of all ideas that seem crazy (like Plate Tectonics, which very slowly gained acceptance even though it had quite a bit of evidence), slows down growth in the field and creates a very disheartened attitude from the public, some of whom think about becoming scientists and then wonder if they can handle the bureaucracy. Slow and careful, but not so slow and careful that the field dies an early death."

Wait a second. Plate tectonics was rejected at first, then became increasingly favored as paleontological and stratigraphic evidence accumulated, and is now the fundamental framework of geology. And geology is a thriving scientific field. How does the example plate tectonics serve to counter the claim that if an idea is robust, and succeeds in motivating a variety of independent lines of evidence, it *will* survive in the face of severe scrutiny? Isn't plate tectonics in fact a poster child for the vitality of ideas that really *are* correct, and for the position that the bar needs to be set high enough that only such ideas survive, and then only after proving themselves on multiple empirical fronts?

And are you really suggesting that people don't go into science because they're discouraged by the fact that very high standards of explanatory and descriptive adequacy must be met before being accepted into the body of scientific knowledge?

37. **G Alex**
December 19, 2011

Fabien, and skeptical I'm not talking about crackpots or people who self-learned outside the academia but of professionals and students inside of it. I have seen several trying to advance novel ideas and research lines but saw it rejected, not on the basis of being 'crackpotish', but because they did not fit with the selecting committee/supervisor agenda and ideological approaches on how to solve a problem or on what is "interesting" and what is not. Those students and researchers who have original ideas but are not that good in politics and in pleasing the hierarchy (and perhaps are the shy introvert guy), even don't dare to come up with something which is not strictly in line with the mainstream paradigm. Because after some attempts of having done so they learn quickly how it will end. And this state of affairs became even worse in the last two decades or so also in fields traditionally open to free thinking and which underwent a centralization based on the ideal of the 'big science' where reserach is conceived as in an assembly line and has to obey the diktats of few 'science top managers' (who in turn are also heavily dependent from the diktats of outside fundings). It is a matter of culture, it is the idea of how to conceive and organize research and education which has gone astray. In this climate the new Einstein for sure will soon leave or even never grow up.

38. **Casey Leedom**
December 19, 2011

People keep bringing up Einstein as an example of an "outsider who actually came up with something new." Hogwash. Einstein built his ideas on the evidence and the ideas of all who had come before him. If he had existed 200 years earlier he might have been bright enough to have come up with some revolutionary idea about mechanics. But he wouldn't have come up with Special- and then later General-Relativity. He needed the input of Maxwell and a whole host of others to build his ideas upon. People who wander off the beaten path and come up with something new are the ones who _do_ pay attention to the work that's gone before them and then find new insights from that information.

39. **Bobito**
December 19, 2011

There is a strong psychological tendency to celebrate the outsider, or the supposed outside (to label Einstein a "patent clerk" and try to portray him as an outsider is to exhibit a complete ignorance of the circumstances of his education and employment – he was highly educated in the classical sense and conversant with the relevant literature). The typical profile of the successful physicist is more like that of Chandrasekhar than that of Ramanujan – child of intellectuals, nephew of Raman, and supremely educated in the classics of both the east and west, in physics and outside of it too.

40. **Proudmemberofthecult**
December 19, 2011

Why apply the 'outsider' label only to string cosmologists? There's more communities like this out there. AdS/CMT is heading this way. Loop quantum gravity is here already…

41. **Robert Plant**
December 19, 2011

With regard to Carter's work – I can only add: In my thoughts I have seen, rings of smoke through the trees – and the voices of those who stand looking.

42. **srp**
December 19, 2011

One must distinguish between theoretical and empirical "crackpots." Arp was the consummate insider until his inconvenient observations and interpretations got him tossed from mainstream astronomy. Frank and the late Sigwarth paid a huge professional price for their empirical work. Krisch seems to have come through by soft-peddling (judging from his U.Mich bio page) the importance of his anomalous findings, but neither do I sense any groundswell by theorists to either show that those findings can be accommodated in the SM or to use them to go beyond it.

Read Benford's entertaining novel Timescape for an insider account of how professionals feel about discovering revolutionary empirical facts. Hint: Their initial impulse is often to try to bury them to avoid trouble. I bet there was a strong faction that wanted to do that on the recent superluminal neutrino claim, and I tip my hat to the team for releasing the controversial findings.

43. **emile**
December 19, 2011

When circlon theory can get the first 10 digits of the gyromagnetic ratio of the electron right, I promise I will then pay attention.

44. **Coin**
December 19, 2011

Giotis: "This isn't true. The fundamental principles of nature cannot be bent or overruled in the multiverse picture."

It seems to me like you can't really say this because there is more than one choice of multiverse. Certainly in a Tegmarkian multiverse (MUH!) "everything goes". How do we meaningfully determine whether we are in the Tegmarkian everything-goes multiverse, or in the constrained m-theory multiverse, or maybe even a non-string chaotic-inflation multiverse (which is also constrained but in a potentially different way from in m-theory)?

And even if we pick the m-theory multiverse– can you really tell me that there is exactly one way to *formulate* the m-theory multiverse such that string multiversers would all agree on which physical ideas are fixed and which vary between different points in the multiverse?

Once you've decided to start walling off physical principles as chance whims of the multiverse, I don't really see what principle we have to guide us as to when to *stop*. Why *not* go full Tegmark and suggest the "fundamental principles" of the m-theory multiverse (like QM) are just due to local multiverse conditions as well?

45. **anonomous**
December 19, 2011

Bob,

Here is a good read on how plate tectonics was shot down by physicist Scheiddiger (who's arguments were strictly theoretical) in 1953, some 41 years after Wegener (who was not a geologist) proposed the theory in 1912 after seeing how the continential pieces might fit together:

http://en.wikipedia.org/wiki/Continental_drift

Yes, there was very good evidence in 1968, but there was quite a bit of circumstantial evidence for 40 years prior and the extreme rejection of an idea that was considered fairly simple (the pieces fit together on a globe) may play out again in physics.

46. **Trulo**
December 20, 2011

> how professionals feel about discovering revolutionary empirical facts. Hint: Their initial impulse is often to try to bury them to avoid trouble.

Funny you would say so, when the entire particle physics community has been begging for decades for at least a single shred of a tiny departure of empirical data from the SM.
Revolutionary empirical facts is what everyone would love to see. The Higgs boson empirically excluded with 95% confidence from 115GeV all the way to 1TeV, for example, would have been pretty cool. Alas, it seems nature chose to stick to the SM. It's not the theoreticians' fault, just as it is not a government conspiracy that cold fusion was obviously wrong from the beginning, or that Opera needs to do a lot more work before their observation of superluminal neutrinos can be remotely believable.

Transgression may be valuable by itself in poetry, but science does not work that way and it never has.

47. **srp**
December 20, 2011

I agree that particle physics has been so starved for new findings that the situation is different at present. (BTW, I haven't been following the rumors closely, but aren't you jumping the gun on the Higgs?) And a systematic error at OPERA is also my best guess about what is going on with the neutrino claim. On the other hand, I don't sense great community excitement about the apparent huge proton transverse-spin anomalies, which appear to be well-verified. And I've seen little buzz about claimed anomalies in radioactive decay during solar flares–perhaps these are also assumed to be obvious experimental errors.

I don't think not finding the Higgs counts as an "anomaly" in the same sense because lots of people anticipated the possibility. When the organisms under your microscope spell out "take me to your leader" that's an anomaly. (Check your colleagues' propensity for calligraphically daubing growth medium on your slides before you call the newspapers.)

48. **Zathras**
December 21, 2011

Trulo: *"… the entire particle physics community has been begging for decades for at least a single shred of a tiny departure of empirical data from the SM."*

True. The question then has to be asked, why does there have to be anything beyond the SM. What necessity is driving a need to unify gravity with the SM? Maybe gravity is just different.

49. **Trulo**
December 21, 2011

@srp:

> but aren't you jumping the gun on the Higgs?

It might be so, that's why I said "it seems" there's a Higgs. More data and further analysis are needed to confirm the signal and, if it remains there, to find out what exactly it is.

@Zathras:

> Maybe gravity is just different.

Personally, I fully agree. But it is also true that I have no clue how that would work, so, as far as I'm concerned, it's not a terribly productive idea.

50. **Clay Farris Naff**
December 22, 2011

I'm probably chiming in too late with too little, but happily talk is cheap. I only want to say that I've been struck for quite some time about how loony much of the conjecture in theoretical physics has become. Worse yet, it worries me that theorists all too often seem to forget that they are merely conjecturing, albeit with the tools of math, and that experiment has yet to confirm gravity waves let alone any hint of the multiverse.

It led me to devise an extension of Occam's Razor, which I call "Clay's Clipper." It states that any proposition that tends to impair reason should not be accepted as true in the absence of compelling evidence. Everett's Many Worlds interpretation of QM is one example. It *may* be true, but its implications are such that we should not presume it to be true unless and until some evidence is produced. (Unfortunately, the one experiment that could convincingly establish Many Worlds only works for the person conducting it, and involves a gun, a lottery ticket, and the violent deaths of at least five versions of the experimenter.)

Clay Farris Naff
Science & Religion Writer

51. **Margaret Wertheim**
December 22, 2011

Dear Peter

Many thanks for the splendid and thoughtful piece about my book. Your own book, and Lee Smolin's, were important touchstones in the development of my thinking about the far fringes of science. As someone trained in physics I have been astounded by the rise of string theory, which has seemed to me for a long time to be little more than magical speculation. There is of course a role for magical speculation in out lives, the question in my book is does it belong in science?

You may be interested to hear that since the book was published I have received quite a few comments from outsider theorists who are distressed at my representation of themselves. They have defended their community as one that has shared concerns and methods – just like the insiders. Its rather strange that on the one had they are calling for a revolution, yet on the other hand they also crave some kind of continuity with the very thing they claim to be rejecting….

52. **Margaret Wertheim**
December 22, 2011

Peter

It occurs to me that your audience may be interested to know that I am currently putting together an exhibition of outsider physics theories. Some of these are graphically stunning – the show will be a visual version of my book. The exhibition will be on display at the Institute For Figuring in Los Angeles. Opening date will be in early March 2012. More information about the show can be seen on the Exhibition Page of the website for my book: physicsonthefringe.com

Let me know if you come to LA and I'd be delighted to show you around the exhibition and my collection of outsider theories.

all best
margaretw

53. **Peter Woit**
December 22, 2011

Thank Margaret,

Probably won't make it to LA next year, but if so, will definitely take you up on your offer.

It's interesting to hear about the "reaction" of the "outsiders" to your book. Sounds like you managed to get both "insiders" and "outsiders" distressed, a good sign…

54. **Margaret Wertheim**
December 23, 2011

Peter

Yes this book has proved to be a source of equal opportunity upset. On the other hand many people – especially nonscientists – have understood that the core of the book is the questions it raises. So many people are bamboozled and intimidated by contemporary theoretical physics, and not knowing where to turn for help. When I started working on the project I planned to write a much more academic book that addressed in depth the sociological issues raised by outsiders; but in the end I decided to, in a sense, stay true to the spirit of my subject and let my subjects speak for themselves. At one point I wanted to call the book "a scientific fairytale" – like many fairytales the story hinges around both a hero and a moral dilemma.

For me the appeal of physics was always its magical beauty. In this sense I agree with your remarks that outsiders are missing out on something really wonderful. That is a failure of their education I believe – one of the sad facts of our time is that so few people can gain the kind of education needed to appreciate aesthetic brilliance of general relativity. I wrote a chapter (that I didn't end up including) in which I proposed that Einstein is the Leonardo of physics and that Jim Carter could be seen as it Heironymous Bosch.

I hope to be coming to NY next year for an event about the book. Perhaps we could meet up then. Who knows, the LHC might have found something we both could celebrate.

all best margaretw

55. **harryb**
December 28, 2011

Peter, I also enjoyed your review, and when the book is available as an ebook on amazon, I'll promise to buy it. A trawl through arXiv recently shows at least some continued skepticism re String Theory – eg Hedrich's: String Theory – From Physics to Metaphysics – arXiv:physics/0604171

Admittedly from 2006, and (to me) a confusing paper – never sure if pro / anti the String Theory position – but extensive use of Lee Smolin's arguments.

A number of recent papers also give the Multiverse/Anthropic Princple/String (M) Theory axis short shrift also. Although you sense a wariness on being tough on string theory, whilst hammering away at the other two (odd).

I tend to agree with you re bias for building on the blocks of previous science – maverick ideas even on settled science surely still welcome, but most (all?) historical "mavericks" seemed to work productively against the common grain rather than bypassing it.

What Margaret Wertheim's book appears to offer (and I will read it fully) is that the deep efforts on String Theory are becoming more fantastical and magical the more it leaves behind the shackles of empiricism and measurement – hence are they any more worthy of detailed consideration than Circlons?

That is a very bad fate for a leading and prominent scientific theory, and it continues to have worrying implications – eg snuffing out diversity of ideas, disengaging the wider populace. On top of this, the approach seems destined for a final theory of unfalsifiable metaphysics (multiverse / AP) coupled with unskeptical and highly defensive approaches to any challenge. All the hallmarks of cultism that Wertheim's book seems to identify.

A poor end for fundamental physics potentially. Where to now – back up the hierarchy to the quantum / classical cut and start again? Top-down v bottom-up?

Short Items

December 21, 2011
Categories: Book Reviews, Experimental HEP News, Multiverse Mania, Uncategorized

A few short items:

- The Multiverse propaganda campaign continues this month, with a piece by Alan Lightman in Harpers entitled The Accidental Universe: Science's Crisis of Faith. The content is pretty much the usual: string theory implies an untestable multiverse, and the multiverse explains why string theory is untestable. The whole thing is wonderfully consistent, coherent, and justifies the world-view developed by leading theoretical physicists over the last 30 years. The main person quoted in the article is Alan Guth:

> Guth started his physics career in this sunny scientific world. Now sixty-four years old and a professor at MIT, he was in his early thirties when he proposed a major revision to the Big Bang theory, something called inflation…
>
> He wears aviator-style eyeglasses, keeps his hair long, and chain-drinks Diet Cokes. "The reason I went into theoretical physics," Guth tells me, "is that I liked the idea that we could understand everything—i.e., the universe—in terms of mathematics and logic." He gives a bitter laugh. We have been talking about the multiverse…
>
> "We had a lot more confidence in our intuition before the discovery of dark energy and the multiverse idea," says Guth. "There will still be a lot for us to understand, but we will miss out on the fun of figuring everything out from first principles."
>
> One wonders whether a young Alan Guth, considering a career in science today, would choose theoretical physics.

The only hint anywhere in article that some physicists might feel that there is something wrong with this picture is the passing remark that some physicists "remain skeptical of the anthropic principle and the reliance on multiple universes to explain the values of the fundamental parameters of physics."

- The FY2012 DOE budget has now been finally agreed upon (see here), remarkably not even that far into FY2012. HEP does pretty well, at $791.7 million, versus $795.4 million for FY2011, a small decrease, considering that the Tevatron was running all of FY2011, is off now. More from Fermilab director Oddone here, who describes the budget as "very good news".
- The recent evidence for the Higgs particle found at the LHC kicks up a notch the question of who might get a Nobel prize for this discovery. Whether there's something there at 125 GeV won't be confirmed until too late in 2012 for a 2012 experimental prize, maybe that will be on the agenda for 2013, with the main question being who would get the prize. ATLAS and CMS are looking at very similar data, with similar analyses, releasing results in a coordinated fashion, so they should both get the prize, but there's no obvious particular scientists to give it to.

A theoretical Higgs prize would likely take much longer, since it will take a while to be sure that whatever is found behaves the way a Higgs should. Guralnik continues his campaign for the prize here.

- Erik Verlinde's claims made nearly two years ago that gravity is an entropic force have gotten a lot of attention. He doesn't seem to have written up any elaboration of these ideas, but he comments very recently on Twitter:

> For those who wonder: the fact that the Higgs has (perhaps) been found has no influence on my ideas on gravity. These ideas remain correct.

- Some people send me books that I enjoy in one way or another, but don't have the time or energy to write about here. Maybe you should consider them as last-minute holiday gifts:

> A First Course in Loop Quantum Gravity, by Rodolfo Gambini and Jorge Pullin. This book explains the ideas behind loop quantum gravity at an introductory level, suitable for undergraduates, or anyone wanting as non-technical as possible of an introduction to the subject. Maybe it can be sold in a package with Barton Zwiebach's A First Course in String Theory.
>
> Fascinating Mathematical People edited by Albers and Alexanderson. This contains a wonderful interview with my colleague Dusa McDuff. There's also this exchange in the interview with Ahlfors:
>
>> **Mathematical People**: How about physicists?
>> **Ahlfors**: Well, I don't believe in physics!
>> **Mathematical People**: You don't believe in physics? Why not?
>> **Ahlfors**: Physicists are so close to physics, but they don't know mathematics.
>> **Mathematical People**: … There's also a great deal of mathematics used by string theorists.
>> **Ahlfors**: But it's the wrong theory. I like the knot theory aspects, especially the knot theory applied to string theory. The strings are knots now, and there are these ready-made knot theorems that can be applied. That appeals to me.
>> Probably physicists are important for mathematics, but they cannot be important for me in any sense. I don't think that mathematicians should take their inspiration from physics.
>
> Magical Mathematics: The Mathematical Ideas That Animate Great Magic, by Ron Graham and Persi Diaconis. ~~Comes with its own deck of playing cards~~. Diaconis is a magician as well as a mathematician, so this is written by experts.
>
> Division Algebras, Lattices, Physics, Windmill Tilting, by Geoffrey Dixon. Dixon tells his personal story about pursuing research in particle physics, trying to connect it to the mathematics of the division algebras over the reals. I'm sympathetic to the idea that this kind of algebra has something to do with the patterns we see in the SM symmetries and quantum numbers. Unfortunately I still think no one yet knows the right way to understand this.

Update: One more. See here for an explanation of why string theory is useful:

> Dr. Kaku explains that time machines do not violate Einstein's laws of physics, and that – difficult though it might be – future humans would be wise to build one and slip through a wormhole to one of the alternate dimensions proposed by string theory before the cooling universe extinguishes all known life.

Update: The Times has an article and series of letters about the Higgs/Nobel issue, see here.

Comments

1. **JC**
   December 21, 2011

Suppose there will be a Nobel Prize for the theory of Higgs particle and everything, I really don't see how Guralnik and his company can win over H, E, or B. They beat the GHK team in print. If no more than 3 people can share the prize, the HEB are the obvious choice.

Besides, none of them proposed the final theory for how electroweak symmetry breaking works in reality. They only provided a mechanism. It doesn't matter who did a better job (Guralnik thinks the GHK team had a more complete and rigorous paper).

I think the best Guralnik can achieve is that no one will win it for the theoretical part unless the number of candidate is reduced to 3. We'll see.

2. **Peter Woit**
   December 21, 2011

   JC,

   Brout is no longer alive, so that's one less. If those goes on long enough, mortality will reduce the number of contending candidates to 3.

   I should also point out that Philip Anderson actually has a better case than any of these people for a piece of such a prize, see
   http://www.math.columbia.edu/~woit/wordpress/?p=3282

3. **Shannon Starr**
   December 21, 2011

   I bought Magical Mathematics a little while ago. It is a beautiful book, also with many beautiful pictures of Graham juggling, as well as explanations of card tricks. But I am pretty sure my copy that I ordered from Amazon did not contain a deck of cards.

4. **Bernhard**
   December 21, 2011

   If the Nobel prize is disputed between 5 in theory, between how many is in experiment? I think a Nobel prize for the Higgs definitely should go to at least two theorists, or even 3 and leave the experimental Nobel prize for something that was not predicted that the experiments happen to find (if). And even in this case the most neutral thing to do is to give the prize to CERN (therefore to Rolf Heuer as its representative) , also because people forget there is a not so easy accelerator effort called LHC involved in this whole process and we would not a a Higgs without it. Tough call, but doable if the Nobel committee wants to be less unfair as possible (to some degree a Higgs Nobel prize is doomed to be unfair).

   A last comment is that I am not so sure I agree that it should take that long to give the prize for a Higgs. As I understand the prize should be given to those who conceived the idea of a Higgs field, not to some random model builder who happened to use the idea and hit the jackpot. So even without knowing its exact properties, we will know if it is some kind of Higgs or not and this should be enough.

5. **Peter Woit**
   December 21, 2011

   Shannon Starr,

   Thanks for letting us know. It's entirely possible the deck of cards was just a promotional bribe included in the package from my friends at PUP to get me to mention the book. Anyway, it's a fun book, even if you don't get an extra pack of cards with it…

6. **anon**
   December 21, 2011

   IMO any Nobel prize for the Higgs should go to theorists alone. Experimentalists already obtained Nobel prize for discovering W and Z particles in the electroweak sector using hadron colliders. I don't think they deserve another prize for discovering the Higgs simply by using a more powerful hadron collider, without revolutionary change in experimental method. If they discovered something groundbreaking using a future muon collider, that might be more Nobel-worthy.

7. **Bernhard**
   December 21, 2011

   One more comment before Christmas.

   Kaku multiverse and string theory. It´s incredible how these guys simply manage to sell the same story every week and still get attention. Kaku is the kind of author I like to read to relax because he has some fascinating stories (the lecture is almost certainly based on his "Physics of the impossible") that has nothing much to do with real physics problems that real physicists have like calculating parton density functions, so it´s really a relaxing "not thinking about my job" thing. Kind of read that is nice to tell my little kid about and that has the same value of us seeing Star Trek.

   Guth´s comments on the multiverse by the way gives me just yet another hint that they all already completely departed to a different reality that is not really connected with physics anymore. It´s hilarious to say "we will miss out on the fun of figuring everything out from first principles". Who is going to miss it? I think physicist will keep trying, also because most experimentalists (the only ones capable of making the real decisions where the filed will head to) are not even aware of the level of madness things went in string theory. The only thing that is unfortunately missing is to acknowledge that the multiverse thing is not theoretical physics and start an independent field with an independent name, maybe connected to the philosophy departments (I hope not to offend any philosopher here).

8. **Avattoir**
   December 21, 2011

   Peter, isn't the claim for Anderson rendered moot by his already having won in 1977?

   On the main point discussed in this thread (so far), I like the idea of distinct awards for the theorists & experimentalists, partly because in this case justice seems to demand it; but also because it allows for the possibility of the sort of 'politicized' award given in the case of the Peace prize involving Gore. An award specifically identified with CERN, such as to Heuer, maybe coupling it with Oddone (since Robert Wilson is long dead & Lederman's got one), might make for an effective slap at Congress for having cancelled the Supercollider, thereby exporting discoveries that link so strongly to the history of physics in this country.

9. **Peter Woit**
   December 21, 2011

   Avattoir,

   The Nobel is for specific discoveries, you can get it more than once (John Bardeen is an example, I don't know if there are others). What Anderson got the Nobel for in

1977 was a completely different topic, work on disordered materials.

10. **neo**
December 21, 2011

Has there been any update in SUSY @ LHC? Has SUSY been excluded to higher energies from the summer's previous announcement?

11. **Foster Boondoggle**
December 21, 2011

Don't you get tired of bashing popular hype? I mean, what's the point? No one takes Kaku seriously, if they ever did. It's almost like going after Fritjof Capra…

Regarding Guth: the thing is, inflation seems to be a compelling explanation for a number of observations – in particular, it's at least consistent with the concordance Lambda-CDM model and WMAP data. And there's really nothing else around to do the job. But once you take inflation seriously, you've got eternal inflation, and then you've got multiple universes. So even though they're unobservable (like "parallel earth" 10^20 light years away in this universe), this model that seems likely to be true also entails them. So why complain when smart & informed people draw out that extrapolation?

12. **Peter Woit**
December 21, 2011

FB,

Who's bashing Kaku or taking him seriously?

And, it's hard to believe, but it looks like I haven't written enough blog postings explaining why evidence for inflation doesn't imply the string theory multiverse, with different low energy physics in every universe.

13. **Foster Boondoggle**
December 21, 2011

re Kaku: OK, if I say "mock" instead of "bash" does that help? I just mean why waste the pixels?

On inflation & the multiverse, I suppose you are referring to a post like this one: http://www.math.columbia.edu/~woit/wordpress/?p=4043. I understand that inflation by itself doesn't imply the landscape. But eternal inflation is a pretty straightforward inference, and that gives multiple "pocket universes" as the lingo has it. Whether they're the ones implied by the landscape is another matter. Maybe they all look like this one. Why complain about this kind of speculation? Is it really that different from talking about the likely existence of parallel Woit all those many light years away?

14. **HI**
December 22, 2011

I'm not a physicist, but I wonder if the prize to Nambu could be considered a prize for the Higgs mechanism. Perhaps they could justify awarding the prize before the experimental confirmation of the Higgs boson because Nambu's work was general enough. (And perhaps they didn't want to wait too long – another prominent Japanese physicist (Yoji Totsuka) had passed away without winning the Nobel Prize earlier that year (2008).) But isn't Nambu's work important in large part because it lead to the Higgs mechanism and the Standard Model?

I won't be too surprised if no Nobel Prize is awarded for the theory of the Higgs boson. A Nobel Prize can be shared by only up to three people. There is no posthumous prize. It is too difficult to choose when there are six or seven contributors of which one is already dead (Brout) and one have already won a Nobel Prize albeit for a different topic (Anderson). And prizes have been awarded for related topics (Weinberg and Salam, 't Hooft and Veltman, and Nambu) already.

15. **db**
December 22, 2011

There is one person who will most definitely get the nobel for Higgs and that is Peter Higgs.
Could you just imagine the public ridicule the Nobel Committee would face if they didn't honour the man after whom the particle is named?

16. **Shantanu**
December 22, 2011

Foster, inflation has a whole bunch of conceptual problems. See talks by Brandenberger,Steinhardt, Turok, Penrose (many of which you can find online at KITP or PI). Plus I think a Harrison-Zeldovich spectrum + lambda cdm model is consistent with observations.

17. **Bobito**
December 22, 2011

My copy of the Diaconis/Graham book did not come with a deck of cards.

It's a nice book anyway.

18. **Oliver**
December 22, 2011

Good timing of this blog Peter.

Last week an interesting article ran over here in the Times by John Richardson on the topic Nobel issues and options. It was followed by two letters from 1) Frank Close (*of course*) and 2) Guralnik, Hagen, and Kibble (*ditto*).

GHK must be feeling the heat due to last week's results and Frank's comments. Close seems to be positioning himself as referee on all things "Higgs" – of course I thought we had John Ellis for that.

The Times
British physicist could be in line for Nobel Prize
John Richardson

December 14 2011

If the LHC experiments find the Higgs boson, the theoretical physicists who first suggested it are expected to win a Nobel Prize. There is a hitch, however: each Prize has a strict limit of three recipients each year, but six people suggested the particle's existence at almost exactly the same time.

Peter Higgs proposed the particle that came to bear his name in 1964, to explain why matter has mass. But a pair of Belgians, François Englert and Robert Brout, and three Imperial College London physicists, Gerald Guralnik, Carl Hagen and Tom Kibble, published papers containing the same idea within a few months of Higgs, in the same journal.

It is now accepted that all six conceived the Higgs simultaneously and independently, and all were awarded the prestigious J. J. Sakurai Prize for Theoretical Particle Physics in 2010.

The Nobel Prize's rules were laid down by Alfred Nobel a hundred years ago, when science was a much smaller and less international enterprise than today, and Erik Huss, of the Nobel Foundation, thinks they are unlikely to be changed. "You can't just rewrite something that's over a hundred years old," he said.

However, a compromise may be possible. Last year's Nobel Prize for Physics was awarded to three physicists, but at the announcement, the committee honoured the teams of which each was a member — though their colleagues did not receive any of the prize money.

There may be pressure on the committee to make an award sooner rather than later, to ensure all the potential recipients – aged between 74 and 82 years old — receive any recognition they deserve. The Nobel Prize cannot be awarded posthumously.

The Times
Letters to the Editor
December 16 2011

The problems faced by the administrators of the Nobel Prizes, and the huge possibilities opened up by the Higgs boson

Sir, You said (report, Dec 14) that six people suggested the Higgs boson's existence at the same time and that this is a problem for Nobel Prizes, which can be shared by at most three. As this assertion was adjacent to a commentary by me and is false, it needs expunging.

Peter Higgs uniquely drew attention to the boson, which now bears his name. While it is true that Higgs is but one of six (at least) who independently discovered how to give mass to fundamental particles, it is by studying the properties of the eponymous boson that the whole idea can be tested, as my book The Infinity Puzzle explains.

If the mass mechanism is regarded as key, then there is indeed an overabundance of candidates. However, if the boson is key to a Nobel Prize, there is no problem with the limit of three winners.

Professor Frank Close
Professor of Theoretical Physics, University of Oxford

The Times
Letters to the Editor
December 21 2011

Boson issue

Sir, while we are enjoying Professor Frank Close's recent book The Infinity Puzzle, we take issue with his letter (Dec 16) on some significant points. In this letter he implies that our paper "GHK" did not have the "eponymous boson". Our 1964 Physical Review Letters paper, written while all three of us were at Imperial College London, has the boson and describes it with what amounts to the same equation as that derived by Peter Higgs.

Additionally, the GHK paper was unique in that it explained in detail how the mass mechanism is consistent with the laws of causality and charge conservation along with other essential insights not presented elsewhere.

Gerald Guralnik, Brown University
C. R. Hagen, University of Rochester
Tom Kibble, Imperial College London

19. **Peter Woit**
December 22, 2011

Oliver,

Thanks a lot for pointing this out. Someone else should join in the fun and write to The Times about Anderson.

20. **Anonyrat**
December 22, 2011

Since number of Nobels have come up, here is from the web:

"…the International Committee of the Red Cross has won the most Nobel Prizes. The Red Cross received the Nobel Peace Prize in 1917, 1944, and 1963.

The only other people or organizations that have received more than one Nobel Prize are Marie Sklodowska Curie (for physics in 1903 and chemistry in 1911), Linus Pauling (for chemistry in 1954 and peace in 1962), John Bardeen (for physics in 1956 and 1972), Frederick Sanger (for chemistry in 1958 and 1980), and the Office of the United Nations High Commissioner for Refugees (for peace in 1954 and 1981)."

21. **anon**
December 22, 2011

I second Neo's question. When will LHC publish new SUSY results based on this year's data?

22. **Peter Woit**

December 22, 2011

anon and neo,

Maybe you should try the question at Matt Strassler's blog and see what he has to say about it. For the first ATLAS/CMS SUSY searches, the ones that were widely advertised before LHC turn-on as the best bet for finding SUSY, nothing showed up in the first 2 inverse fb or so. The reach of these searches won't go up very much with the analysis of the full 5 inverse fb of this year, so no one expects anything there.

SUSY proponents have been claiming that these first searches weren't the right ones, that it's only later, more difficult ones where they now think SUSY will show up. I'm also curious exactly which searches they're willing to claim as decisive ones, and what the state of those searches is. Maybe Matt or someone else can be encouraged to write about this. In general, I thought that the experiments weren't planning much in the way of release of new data until the winter conferences (e.g. Aspen in February).

23. **TimG**
December 22, 2011

True, the Nobel is for specific achievements, but even those who have multiple Nobel worthy accomplishments have historically not won two Nobels. (Einstein never won for either version of the theory of relativity, for example.) John Bardeen is the only exception (as far as winning two physics prizes is concerned). I assume this is because he worked in collaboration; the Nobel committee couldn't very well award Cooper and Schrieffer and ignore their co-author. But given that they already have too many possible winners for the Higgs mechanism, it seems especially unlikely that they'd take the essentially unprecedented step of giving a share of the award to a past laureate.

24. **Brathmore**
December 23, 2011

Peter,

As a somewhat informed layman trying to explain the fanciful idea of the multiverse to a bunch of smart humanities professors who know little about science, I found myself unable to answer a few of their basic questions, and I wondered if you could chime in. It goes without saying that like you, these professors and I think the multiverse idea is far-fetched. Still, we recognize that you and other readers of this forum, while not supporting the multiverse hypothesis, are likely knowledgeable enough about it to answer our questions.

1) If there are multiple expanding universes, do they collide? For instance, could a different universe's expansion collide with ours and, presumably, lead to all manner of destruction as stars from different universes run into each other? Wouldn't these events be observable in principle? If so, wouldn't that make the multiverse a testable idea, and assuming a lack of evidence for it, one that conflicts with, or lacks, experimental evidence?

2) I was asked whether space extends forever, and answered (hopefully correctly) that it does NOT — that the frontier of space in our universe is the outer reaches of expanding space caused by the big bang. This then lead to 2 questions:

(a) What is "beyond" that frontier (e.g., what if you're on the most distant object of the universe and throw a baseball outward…Does that act create new space?) Relatedly,

(b) What is the "stuff" that supposedly separates different universes of the multiverse? "Space," or something else? Given (by definition) that this "stuff" isn't part of any of the different universes, what is it? If the concepts of distance and location apply to such a substance (e.g., the notion of "between" different universes), then it seems unsatisfying to me to simply say that nothing exists there.

3) Are any/all of the different universes of the multiverse supposed to have originated from the same Big Bang as our universe's (and if so, why would they constitute a different universe than ours)?

4) One hears from the same writers who promote the multiverse that, in light of the accelerating expansion of our universe due to dark energy, one day our universe will be essentially "empty." Yet if there are 10^500 other universes that could potentially be expanding into ours, wouldn't the "stuff" from those universes potentially make our universe much more interesting than the barren wasteland depicted by the aforementioned writers?

I recognize you and many of your readers are not proponents of the multiverse (neither am I), but I think that discussing issues like these has value for either discrediting the multiverse or for finding ways to learn more about it. Thanks.

25. **EDR**
December 23, 2011

Brathmore,

The key to understanding many of these issues is to recognize that space does not need a meta-space (and then meta-meta-space, and then…) in which to exist.

In particular, expansion of space need not be expansion *into* anything else. Not also that the metric curvature of space is measured inside space itself; there's no notion of curving "up" or "down" in another dimension.

26. **Marty Tysanner**
December 23, 2011

Brathmore,

I'm not Peter, but I can try to answer some of your questions. I'll break it up into two comments — this one will try to paint a general picture of inflation and the multiverse, and with that context I'll talk about your specific questions in a second comment.

The general topic you are talking about is the creation of "bubble" ("pocket") universes within a total Universe, in the context of something called *eternal inflation*. Eternal inflation seems to be a generic prediction of most models of inflation. What I'll discuss assumes "false vacuum" eternal inflation, but the general issues that arise should be there for other ways currently considered for modeling inflation.

The inflation idea is that there is a "scalar field" that permeates all space, and it is in a state of high, essentially constant potential energy. (The origin of the potential energy, or more accurately potential energy *density*, is a "potential" which is a property of the scalar field itself.) According to Einstein's general relativity, under this circumstance of constant positive energy density space will expand exponentially fast. Obviously you won't get anything interesting if space expands exponentially forever, so inflation needs to end somehow. That is where quantum mechanics (and hence probability) comes in. The scalar field quantum mechanically "tunnels" out of the "false vacuum" (elevated potential energy) state in which it is trapped, then slowly decreases in potential energy until it reaches the minimum of the potential (the "true vacuum" state). But the tunneling event that leads to an end of inflation doesn't occur everywhere at once; it is *localized* to a region of space, i.e., it follows a probability distribution in each (exponentially expanding) region of space. It would be great if it ended everywhere at once, but that isn't the way quantum mechanics works.

When inflation ends locally this way, the event corresponds to formation of a “bubble” (“pocket”) universe. Because tunneling follows a probability distribution, bubble universes form at a characteristic average rate. This is analogous to the decay of radioactive atoms — a given radioactive isotope has a characteristic decay rate, but the decay process itself is random so you never know when or where the next decay will occur.

Maybe you can now see how eternal inflation comes about in this picture. Bubbles form at a particular rate, and as a consequence of the “equation of motion” for the scalar field they continue to expand afterward. Meanwhile space outside the bubble continues to expand exponentially at a *faster* rate than the bubble, so the total volume of empty space (i.e., not contained inside bubble universes) within the total Universe increases exponentially faster than the fraction of the total Universe that is occupied by bubble universes. This will continue to be true forever — because the probability of a tunneling event occurring within a given “box” is proportional to the volume of the box, and the volume of such boxes is increasing exponentially fast, there can never be enough tunneling events to end this runaway effect. Hence if inflation starts it is eternal to the future.

You can also see where the so-called multiverse comes about in this picture. Just as the volume of inflating space grows exponentially, so does the number of bubble universes in them.

I’ll make a couple of general comments about this picture. First, it should be clear that the inflation idea doesn’t depend at all on string theory — it comes from mixing scalar fields, quantum mechanics and general relativity. What string theory does in an inflation picture is vastly complicate what form the potential takes, so that it is possible for tunneling plus field evolution to occur in a huge number of different ways. You’ve probably heard of the string theory landscape of perhaps 10^500 different vacua — inflation gives a way to “populate” all those different vacua, at least in principle.

Second, since eternal inflation appears to be a generic feature of inflation models that are “up to the job” of accomplishing what inflation is “designed” to do, it is a very small step to conclude that the multiverse is a prediction of inflation. Except perhaps for string theory landscape enthusiasts, this proliferation of bubble universes is not desirable from a theory standpoint because it likely compromises the predictivity of the theory, at least somewhat. There doesn’t appear to be an easy way to escape the prediction that with inflation you get a vast number of bubble universes, i.e., a multiverse.

On the other hand, according to the most “natural” way of computing probabilities, i.e., the volume measure, we almost certainly don’t live in the universe we observe (!). The volume measure is a weighting by relative volume; for example, the probability of finding ourselves in a universe *at least* 13.7 billion years old is the total volume of all bubble universe aged 13.7 billion years or more, divided by the total volume occupied by *all* bubble universes. As you might be able to see from the discussion above, the volume fraction of universes much younger than ours is exponentially greater than the volume fraction of universes our age or older. This means that the natural probability measure predicts that with near certainty our universe should be much younger than we observe. This is a dramatic failure of an important prediction of (eternal) inflation. There are some very smart people actively trying to find a different way of computing probabilities that is also “natural” but that doesn’t predict our observable universe doesn’t exist. We’ll see, there certainly is good justification for skepticism…

27. **Brathmore**
December 23, 2011

Marty,

Thanks for the thoughtful reply. I admit that these issues are way over my head, so some of the questions I still have may not make sense, or may have been answered by your previous response…but here goes nothing:

If I understood your post correctly, bubble universes are contained within the larger universe (which began expanding about 14 billion years ago), and the bubble universes are expanding at a slower rate than the larger universe. If so, won’t there be objects in the bubble universe that will collide with objects in the larger universe? Wouldn’t we be able to observe this?

For instance, I imagine the larger universe like an ever-expanding solid balloon, and a bubble universe to be a small bubble within that balloon that itself begins to expand outward in all directions. But if so, won’t there be situations where the bubble universe is expanding in the opposing direction of the larger universe? (e.g., In a cartesian coordinate system, suppose the center of the larger universe is located at (0,0,0), the left-most edge of the larger universe is located at (-100,0,0), and the center of a bubble universe is at (-50,0,0) and has currently has radius 10. I would expect that the point (-40,0,0), the bubble universe is traveling in the positive X-direction, whereas at that same point the larger universe is traveling in the negative X-direction).

Shouldn’t we be able to observe these bubble universes? Wouldn’t some of them be expanding towards us and be observable as such (e.g., we see a pocket of stuff corresponding to the bubble universe that is blue-shifted compared to its surroundings?). Couldn’t there be collisions between the bubble universe and larger universe that are observable?

28. **Marty Tysanner**
December 23, 2011

Brathmore,

Now, on to your specific questions…

> 1. If there are multiple expanding universes, do they collide? Wouldn’t these events be observable in principle?

Yes, with an emphasis on “in principle.” If two bubbles formed closely enough to each other, their expansion could “outrun” the exponential expansion of space between them and the bubble walls would collide. This should create a big mess with lots of energy dissipated in the collision. However, almost all such events would occur so far away from Earth that the huge disturbances in the CMB or matter distribution would essentially be point-like, making them unobservable for all practical purposes. There is a very small but still finite probability that the aftermath of such a collision would be detectable by us. See this post by Matt Johnson on Cosmic Variance for more information:
Observing the Multiverse.

> 2) I was asked whether space extends forever, and answered (hopefully correctly) that it does NOT — that the frontier of space in our universe is the outer reaches of expanding space caused by the big bang.
> (a) What is “beyond” that frontier (e.g., what if you’re on the most distant object of the universe and throw a baseball outward…Does that act create new space?)

You are touching on what I think is one of the most non-intuitive aspects of the Big Bang model. A “bang” conjures up an image of an initial explosion somewhere in a larger space, and the debris flies outward into the larger space (otherwise, what would it fly into?). That isn’t the “real deal” in the Big Bang model. One of the central tenets of cosmology is that on large enough scales (say, several hundred megaparsecs), the universe is homogeneous and isotropic. (Obviously a lump of matter like a star or galaxy is an inhomogeneity; that’s why it’s important to average over much larger volumes.) Our observable universe started out homogeneous and isotropic, and it has remained homogeneous and isotropic since then.

There is a unique kinematical description of an expanding homogeneous universe — it is the Hubble law. It says no matter where you are in the observable universe, when looking outward *at sufficiently large distance scales* everything is moving away from you as though you were at the center of an expanding sphere. (To understand this better, read about the Hubble law.) But again, that's only a consequence of a homogeneous and isotropic expanding universe. This situation of uniformly expanding space can be modeled as space expanding according to a time-dependent *scale factor a(t)*, which governs how the volume of the universe increases with time. Roughly, think of $V * a(t – t\_0)$ as the the volume of the universe after a time $t – t\_0$ has elapsed, assuming you measured its volume as V at the time t_0.

A natural follow-on question is, "Then why does *a(t)* increase? What makes space expand if we aren't adding new energy/matter to the universe?" That one is harder and there is disagreement about the best way to answer that question. I don't think there is any consensus even among the most accomplished cosmologists. Some will argue that space really is expanding, whereas others will say the idea of "expanding space" is nonsense and a source of confusion.

> (b) What is the "stuff" that supposedly separates different universes of the multiverse? "Space," or something else?

Am not sure if I answered that satisfactorily in my previous comment. In the eternal inflation picture, you can think of the space between bubbles as space filled with a scalar field which is trapped in a "false vacuum" state.

> 3) Are any/all of the different universes of the multiverse supposed to have originated from the same Big Bang as our universe's (and if so, why would they constitute a different universe than ours)?

The Big Bang cosmology describes the evolution of the interior of "our bubble" universe, whereas the multiverse corresponds to the evolution of an eternally inflating Universe.

> 4) […] Yet if there are 10^500 other universes that could potentially be expanding into ours, wouldn't the "stuff" from those universes potentially make our universe much more interesting than the barren wasteland depicted by the aforementioned writers?

It seems you are referring to collisions between bubble universes. If the (eternal) inflation picture is correct, then presumably there have been a huge number of collisions of our bubble with other bubbles that "nucleated" nearby. However, according to the same picture even just our own bubble universe should be incredibly huge, far far larger than what we observe. Most such collisions (unless they occurred very early in our universe) would affect only an extremely tiny fraction of our bubble, so the amount of potentially interesting (i.e., probably catastrophic) mixing is very limited. Moreover, there would be a very limited region surrounding our bubble in which other bubbles could nucleate and eventually collide with ours. The thickness of that region depends on how rapidly the walls of our bubble and nearby bubbles accelerate toward each other after nucleation (this acceleration need not be the same for every bubble). But still, it is clear that if no bubble wall can move faster than the speed of light within the exponentially expanding "outside" space there is going to be a limit to how far away a bubble can nucleate and eventually collide with ours. This means that almost all bubble universes would never collide with ours.

If our universe were a bubble universe, by now the wall of our bubble would be far far beyond our Hubble horizon, and any collisions happening "now" would never affect us nor ever become visible. The accelerated expansion within our own universe would insulate us even more, since any effects of a collision would need to outrun the accelerated expansion. So you're almost certainly safe…

29. **Marty Tysanner**
December 23, 2011

Hi Brathmore,

Sorry I didn't see your follow-up comment before posting my second comment. Anyway, if I understand your follow-up questions I *think* I may have answered them in that second comment. As far as things being over your head, that's fine and expected if you haven't studied this stuff before. It just means you will probably have to think about the answers and do a bit of follow-up study to get the main ideas. (Wikipedia might be a start, but in my opinion it's a mixed bag because some articles can feel a bit technical if all you're looking for is a conceptual picture.)

I just want to make one clarification. You said

> If I understood your post correctly, bubble universes are contained within the larger universe (which began expanding about 14 billion years ago) […]

The 14 billion years ago would apply to our bubble and not the total Universe (or multiverse). We would have no way of knowing when eternal inflation started; all such information would lie well beyond our horizon, outside our "protective" bubble.

30. **Brathmore**
December 23, 2011

Marty,

Thanks so much for your extensive posts. I think a key source of my questions in my second post was thinking, incorrectly, that our universe was the "total universe" and that the other "bubble" universes were contained in ours. Now its clear to me that what is meant is that our universe is just one bubble in an ever-expanding total universe that we don't observe. Thanks for the clarifications.

31. **Andy**
December 24, 2011

Marty, the volume measure is in no way natural. It's terrible – if you use a volume measure on an infinite volume you get nonsense results – the most likely place to be, for example, is inside a black hole. Gibbons and Turok wrote a paper on this, using the Liouville measure which in a sense is the only 'natural' measure available to the theory. Again, they found bad news for inflation, but using a much more mathematically rigorous way of defining measure.

Of course, all this discussion has been about eternal, vacuum tunnelling inflation instead of the now normal slow-roll paradigm which seems to answer a lot of these questions quite easily. It's still got bad probability results, but I think some people have been looking at this in other contexts (Ashtekar and Corichi?) to show it becomes probable in modified gravity.

32. **Anonyrat**
December 24, 2011

Correct me if I'm wrong, but in eternal inflation without string theory, the bubble universes all have the same laws of physics (e.g., the same values of parameters in the standard model), but the string landscape problem is that each of the bubble universes is in principle different, one could have two species of light fermions, and another could have six. ?? Thanks in advance.

33. **Peter Woit**
December 24, 2011

Anonyrat,

Yes, that's the problem with the inflation implies unpredictive multiverse argument. Inflationary theory just uses a single scalar field, one that has nothing to do with particle physics. If the eternal inflation universe exists, it's a rather boring one, endless similar universes with the same physics.

To ruin predictivity, you need string theory, where the single inflaton field is replaced by some huge number of moduli fields governing the size, shape, and other aspects of the complicated "string vacuum" configuration. These are required to have scalar fields that give different low energy physics. Whatever you think of the eternal inflation multiverse, there's no argument from it that gives the kind of multiverse promoted as an excuse for string theory's failure.

34. **Marty Tysanner**
December 26, 2011

Andy,

I agree with you that the canonical measure worked out by Gibbons and Turok is more rigorous and deserving of being called the "natural" measure. I probably should have called the volume measure the "simplest" or "most straightforward" measure. I thought the volume measure conceptually easier to understand at the technical level of the rest of my response, since my goal was to make a simple point. (I probably shouldn't have even brought the subject up…)

However, if you assume that eternal inflation began at some finite time in the past rather than being eternal to the past, then the present-day volume of the total Universe would not be infinite (unimaginably huge, yes; infinite, no). So if one bases a volume measure on these finite volumes and takes this imperfectly defined measure to be representative of the measure you would obtain for an ensemble of pocket universes, then I think what I wrote is reasonable, i.e., that it predicts our observable universe should almost certainly be somewhat younger than it is. This conclusion can be modified by changing how relative volumes are computed. Anyway, like I said, I probably shouldn't have brought it up; the subject of measures on an eternally inflating Universe is complicated and controversial, and I am certainly no expert on it.

As you noted (and as I mentioned at the beginning of my initial comment), my discussion was in the context of false vacuum eternal inflation. You mentioned a "now normal slow-roll paradigm which seems to answer a lot of these questions quite easily," but I'm not sure what you mean in particular. Are you referring to chaotic inflation? You need slow roll in any workable inflation model I'm aware of in order to get the right spectrum of CMB perturbations, i.e., [tex]\delta \varphi/\/\varphi \sim 10^{-5}[/tex]. For example, in the "open inflation" variant of false vacuum eternal inflation, right after a bubble nucleates the scalar field slowly rolls down the potential for a "long enough" period to give the desired number of e-folds of inflation *inside* the bubble; this is a *second* stage of inflation (the first stage being the metastable local minimum where eternal inflation occurs).

35. **Andy**
December 26, 2011

Marty,

First a seemingly technical but actually important point: the volume of the universe could still be infinite even if inflation is not eternal. It depends on the topology of the spatial slice on which you perform the calculation – if you pick your space-time manifold to be R^4 with spatial slices R^3 you will always have infinite total volume. You then define volume with respect to a fiducial cell volume at a given time, and test the evolution of this cell to show expansion etc. In a closed model, or a T^3 X R model, you certainly do have finite volume, so the volume measure could make sense, but in the R^4 case you don't. Now at first this appears to be just splitting hairs, but in constructing a measure on an infinite space, the measure itself will depend crucially on how the limit of infinite volume is taken, and you can effectively sneak in any prejudices you like by taking the limits in different ways, something I think you're hinting at when you talk about how the relative volumes are computed.

When talking about slow roll I'm really meaning an inflaton modelled as a scalar field with a pure quadratic potential, which is what G+T talk about (admittedly a lot of my understanding comes from their paper, so I'm probably biased). Here you do see that although relative volumes do depend heavily on initial conditions, every point in the phase space does come to an end of inflation (or rather crosses \phi=0 at which point inflation is by hand brought to a halt as the inflaton couples to other fields here – this part always seems hazy). Likewise, the space-time has a singularity in the infinite past – there is no eternal inflation. I never really understood why people seemed to add the vacuum tunnelling part to this – just set a scalar field with a quadratic potential and inflation occurs. Of course, here I must admit my ignorance as there may well be a good reason for it, but for all I can tell you can't observationally distinguish between the models and the pure quadratic potential (ie just making the particle massive) seems the simplest to my mind.

36. **Marty Tysanner**
December 28, 2011

Hi Andy,

I agree with you about obtaining an infinite volume spatial slice in [tex]\mathbb{R}^4[/tex]. In fact, the example I mentioned of open inflation inside a bubble does just that. Open inflation is somewhat non-intuitive in that you have a finite bubble when volume is computed with respect to the exponentially expanding space outside the bubble, (i.e., where you have a scale factor which goes as [tex]e^{Ht}[/tex] with *t* the "cosmic time"), but by appropriate choice of slicing *inside* the bubble (e.g., as [tex]\mathbb{R}^3[/tex]) you obtain an infinite volume inside the bubble. The slicing is chosen so that just after nucleation and the *second* era of inflation occurs inside the bubble, the spacelike surfaces are hypersurfaces of equal scalar potential (homogeneous and isotropic); the later time hypersurfaces are the time evolution of those early-time hypersurfaces. Thus, the cosmic time inside the bubble is different than the cosmic time outside the bubble.

Outside the bubble, when I mentioned finite (but huge) volume for the total Universe, I was thinking of a particular kind of slicing, i.e., slices of constant cosmic time where the bubble volumes and the eternally inflating region are both finite for finite cosmic time; *t*=0 marks the start of inflation. I admit this isn't very general.

Slow roll inflation with the kinds of potentials that are usual in particle physics (e.g., [tex]V(\varphi) \propto \varphi^2[/tex] or [tex]V(\varphi) \propto \varphi^4[/tex]) don't seem to work well, in that they don't give the right amount and kind of inflation required for inflation to do the job it is designed to do — inflation typically ends too early and you don't get the right spectrum of CMB perturbations. Potentials that work better apparently need to be of the "hand crafted" variety, containing a long, gentle slope. (You also need [tex]\dot \varphi[/tex] initially small, which requires some way of obtaining that condition. Tunneling from a false vacuum provides one such way, as in the instanton formalism.)

Take the case of a slow-roll inflation model with no false vacuum or tunneling, but just a long gentle slope of the potential. I think this is what you were envisioning. Since the inflaton is a quantum field by assumption, it can behave differently in different regions. As the inflaton slowly rolls down the gentle slope, vacuum fluctuations can act on it stochastically, nudging it back up the slope in some regions (increasing the energy density) and thereby causing that region to tarry in its descent toward the end of inflation there. Inflation lasts longer in such a region than in one where the inflaton continues to descend normally or is nudged down the slope. Since the regions where the inflaton tarries will inflate more, the total volume of the Universe becomes dominated by them rather than by regions where inflation ends. Thus, you still get eternal inflation in this picture even though there is no false vacuum with tunneling.

37. **Anon_B**
December 31, 2011

Frank Close responded to the GHK letter in the London Times posted above. Point is around a "massive" vs. massless boson. Frank says GHK was massless (which is correct). GHK has stated such but claims the boson gets mass through leading order approximations to the 4 physical degrees of freedom.

GHK has not responded but certainly it will look like that below from Guralnik over the past summer.

The Times
Letters to the Editor
December 22 2011

Massive Boson

Sir, My letter specifically (Dec 16) referred to the "Massive" (sic) boson which bears Higgs's name.

GHK, the work of Gerald Guralnik, C. R. Hagen and Tom Kibble (letter, Dec 21), contains an equation for a boson without mass. While mathematically this might "amount to the same equation [as Higgs]", for physics the difference is … massive, in all senses of the word.

Only a massive boson can decay, and it is the decays that can prove the mass mechanism. Higgs first wrote the relevant equation for this decay in 1966. If GHK, or anyone else, made published mention of decays of the massive boson before that, I shall correct my book The Infinity Puzzle, where these arguments are outlined in greater detail. In the meantime I hope they continue to enjoy it.

Frank Close
Professor of Theoretical Physics
University of Oxford

Again, while we will have to wait to see if GHK responds, one can bet it will sound a bit like this below from Guralnik's recent APS talk/notes this past summer. It will be based on the fact that fundamentally the difference between massive and massless at lowest order is insignificant.

The Beginnings of Spontaneous Symmetry Breaking in Particle Physics — Derived From My on the Spot "Intellectual Battlefield Impressions"
Authors: G. S. Guralnik
(Submitted on 11 Oct 2011)

http://arxiv.org/abs/1110.2253

p. 9

…

Recently it has been claimed that the GHK paper does not have the "Higgs boson". This claim astonishes us. We, far more than any of the other groups, keep very careful track of the degrees of freedom of our scalar electrodynamics model. On the bottom of the right column of page 586 of the GHK paper are the three equations for the leading order approximations to the 4 physical degrees of freedom. We observe that the two degrees of freedom of the vector field combine with one scalar boson to form the three degrees of freedom of a massive spin one vector field. There is one remaining scalar field, 2 in our notation, which in our approximation has zero mass. That this mass is zero has absolutely nothing to do with any dynamical constraint including the Goldstone theorem. The Goldstone theorem, if valid here, would only constrain the mass of 1. The zero mass is an artifact of how we pick the explicit action and the leading order approximation. This is different from the Higgs paper in that he puts in an explicit pure scalar interaction. In a 4 dimensional renormalizable theory that interaction is limited to being pure quartic. As was our practice mirroring that commonly used by Schwinger and associates, we did not put in this explicit quartic term in scalar electrodynamics but were fully aware that such a term is generated in higher approximations. Ultimately because, of renormalization, the GHK choice of the action is operationally identical to the one used by Higgs.

In summary, our purpose was to show that the Goldstone theorem did not constrain physical mass in scalar gauge theories. We demonstrated this generally and in a specific example. The mass of 2 happens to be zero in leading order, but as was obvious to us and every other experienced field theorist of that time, this would change order by order as the theory was iterated in a manner closely related to how it changes in unbroken scalar electromagnetism.

…

Hagen also spoke about this briefly in the Sakurai lectures.

http://www.youtube.com/view_play_list?p=BDA16F52CA3C9B1D

Finally, this was found on ABC Radio. An interview with Lord Bob May of Oxford – not sure if he and Close know each other as they are both at Oxford. He comments a bit on his time at Harvard and with GHK.

ABC Radio National
December 24, 2011
Australian scientific superstars No.1 – Interview with Lord Robert May of Oxford
Robert May has achieved the pinnacle of scientific success: President Of The Royal Society, Chief Scientist in the UK, Order of Merit, the equivalent of three Nobel Prizes – yet he could have been a lawyer in Sydney like his dad, instead of Member of the House of Lords. This is the first of a series of interviews with top Australian scientists.

http://www.abc.net.au/radionational/programs/scienceshow/australian-scientific-superstars-no1—robert-may/3745700

….

But secondly I had this group of graduate student friends because they were more my age than the faculty people, and in particular a chap called Gerald Guralnik who, interestingly, along with Tom Kibble here in the UK and Higgs of the Higgs Boson, two years ago the American Physical Society gave its award essentially for the ideas of the Higgs Boson which were simultaneously arrived at by three different groups, the first of whom was not Higgs but was Kibble, Guralnik and Hagen. The other two groups proved the result in a special gauge. Kibble, Guralnik and Hagen had done it two years earlier, but Kibble is a really modest, meticulous person. He said, 'We're not publishing it until we have proved it with gauged generality.' So if you go to the website of people comparing this unusual…six people get the prize that it is thought may go to no more than three for a Nobel for the discussion, well, if it's going to go to any three, which three?

…

This Week's Hype

December 22, 2011
Categories: This Week's Hype

The Japanese are getting in on the string theory hype business, with KEK issuing a press release today with the title: The mechanism that explains why our universe was born with 3 dimensions: a 40-year-old puzzle of superstring theory solved by supercomputer. As usual for this kind of press release, the claim is that researchers at the institution issuing the press release have finally solved the age-old problem of string theory predicting nothing. In this case the prediction is that there are 3 dimensions of space (I think that, technically, this is a "post-diction"). According to the press release:

> A group of three researchers from KEK, Shizuoka University and Osaka University has for the first time revealed the way our universe was born with 3 spatial dimensions from 10-dimensional superstring theory in which spacetime has 9 spatial directions and 1 temporal direction. This result was obtained by numerical simulation on a supercomputer…
>
> … it is expected that superstring theory allows the investigation of the birth of the universe. However, actual calculation has been intractable because the interaction between strings is strong, so all investigation thus far has been restricted to discussing various models or scenarios…
>
> … It is almost 40 years since superstring theory was proposed as the theory of everything, extending the general theory of relativity to the scale of elementary particles. However, its validity and its usefulness remained unclear due to the difficulty of performing actual calculations. The newly obtained solution to the space-time dimensionality puzzle strongly supports the validity of the theory.
>
> Furthermore, the establishment of a new method to analyze superstring theory using computers opens up the possibility of applying this theory to various problems. For instance, it should now be possible to provide a theoretical understanding of the inflation that is believed to have taken place in the early universe, and also the accelerating expansion of the universe, whose discovery earned the Nobel Prize in Physics this year. It is expected that superstring theory will develop further and play an important role in solving such puzzles in particle physics as the existence of the dark matter that is suggested by cosmological observations, and the Higgs particle, which is expected to be discovered by LHC experiments.

This goes back to the pre-arXiv days, before many of our current graduate students were even born, but some of us are old enough to remember similar claims being made back in the late 1980s. For example there's the 1989 Brandenberger-Vafa paper claiming that string theory predicts 3 dimensions, using a "string gas" cosmology. I don't remember if there was a "finally, physicists find a way to make a prediction based on string theory" press release back in 1989 or not.

Comments

1. **Bee**
   December 22, 2011

   and then there's the Randall and Karch paper and the Momen and Rahman paper. And you know what? They all found we live in 3+1 dimensions!

2. **Anonyrat**
   December 22, 2011

   I suppose a continuation of this pre-print?
   http://arxiv.org/abs/1108.1540

   "Expanding (3+1)-dimensional universe from a Lorentzian matrix model for superstring theory in (9+1)-dimensions
   Sang-Woo Kim, Jun Nishimura, Asato Tsuchiya
   (Submitted on 7 Aug 2011 (v1), last revised 7 Sep 2011 (this version, v2))

   We reconsider the matrix model formulation of type IIB superstring theory in (9+1)-dimensional space-time. Unlike the previous proposal in which the Wick rotation was used to make the model well-defined, we regularize the Lorentzian model by introducing infrared cutoffs in both the spatial and temporal directions. Monte Carlo studies reveal that the two cutoffs can be removed in the large-N limit and that the theory thus obtained has no parameters other than one scale parameter. Moreover, we find that three out of nine spatial directions start to expand at some "critical time", after which the space has SO(3) symmetry instead of SO(9). "

3. **MathPhys**
   December 22, 2011

   Here is a physics-trivia-type question to think about over the Holiday Season. Name the earliest possible scientific paper, written by a bona fide physicist, that aims to explain why we live in 3+1 dimensions.

   Merry Christmas.

4. **Mitchell Porter**
   December 22, 2011

   Ehrenfest 1918?

   http://www.dwc.knaw.nl/DL/publications/PU00012213.pdf

5. **Chris W.**
   December 22, 2011

   Rather than say that such a result explains why space is 3-dimensional in some absolute sense, one should merely say that it explains how space can be 3-dimensional in this particular theory, which prima facie implies that it is n-dimensional with n ≠ 3 — that is, implies that the observed dimensionality must be a dynamical consequence of the theory, and may be highly contingent.

   Of course we are not accustomed to theories in which the observed dimensionality must be a dynamical consequence of the theory — or rather, we are accustomed to having considerable difficulty recovering the observed 3-dimensionality of space in such theories, and therefore tend to be skeptical of all of them on this basis alone. From that point of view this is an interesting result, whatever its place in the sprawling and profligate theoretical mess that "string" theory has become.

   If a result like this — and others that the authors would like to pursue* in this framework — turn out to be generic, it might point to a path out of the morass known as the string theory landscape. I admit that is a faint hope, but given prior experience it will always seem like a faint hope until the path is actually found.

(* See their conclusion: "The next step would be to show that the four fundamental interactions and the matter fields appear in our universe at later time. For that we need to change the scale from the Planck scale to the TeV scale, which may require constructing a framework similar to the renormalization group or the low energy effective theory.")

6. **Peter Woit**
December 22, 2011

Chris W.,

You can come up with string theory models with any large number of spatial dimensions from 1 to 9 (10 w/M-theory). A large number of people over the last 25 years have come up with "arguments" to pick out 3, Brandenberger-Vafa is one example, Bee gives some others. I don't see what's supposed to be interesting about finding yet another one, since there's no more reason to take it seriously than any of the others.

As for their "The next step would be…", that is just bizarre. What do they think thousands of very smart people have been trying to do for the past couple decades? Exactly this of course, with zero success. These authors write as if string theory were some new idea, and they're the first to suggest that one should try and connect it to reality.

7. **lun**
December 22, 2011

To be fair, the Brandenberger-Vafa paper is one of the most beautiful papers in the history of string theory. Its main results, including the 3d explanation, can be "explained to your grandmother". There is very little in string theory, old or new, that meets this criterion.

8. **MathPhys**
December 22, 2011

Mitchell Porter,

That's indeed the paper that I had in mind. I held it the original publication in my hands in a library many years ago, and I'm glad to know that it's now available online. Thanks for that and best wishes on the Holiday Season.

9. **Thomas Larsson**
December 23, 2011

Some 20 years ago I saw a paper where someone had measured the dimensionality of space, with the result D = 3+epsilon where |epsilon| is less than 10^-20 or so. What they did measure was the power in Coloumb's law, but the result was phrased as a measurement of dimensionality.

10. **Proudmemberofthecult**
December 23, 2011

@ Thomas Larsson

If you permit measuring Coulomb's law exponent as a measurement of space dimensions, then you go to about 1769 or so:

http://en.wikipedia.org/wiki/John_Robison_%28physicist%29

Incidentally, you can make sense of non-integer dimensions: see "fractal dimension". This arises in some triangulation models for quantum gravity.

11. **plm**
December 23, 2011

Thanks alot for the post Peter, the research looks awesome, groundbreaking.

The article to be published is cited on PRL with the same title as the preprint linked to by Anonyrat:
http://arxiv.org/abs/1108.1540
They do not seem to have posted the official version, accepted november 21, 2011.

The conclusion is quite gripping: they propose investigating nonperturbatively the appearance of a local field theory on commutative 4-dimensional spacetime at low-energy (which is the only parameter of their simulation it seems), and then appearance of matter and the 4 fundamental forces, or some gauge theory at least I guess. That is: researching consequences of type IIB strings from nonperturbative simulations in the IKKT model -I think.

Perhaps the most exciting aspect is trying to explain these observations theoretically, mathematically. There is research into relating various models of quantum gravity (Rivasseau has a recent arXiv article), and these computational observations may definitely give us motivation to relate perturbative and nonperturbative formulations, to find limitations of perturbative string theory, and perhaps relate string theory to loop quantum gravity, and eventually understand something about quantum gravity. (We may optimistically take cues from lattice QCD to inspire us.)

Of course these are pipe-dreams, and I am pretty much ignorant of the little knowledge actual researchers may have of what I mention -I plan to learn. But this is honest work for theoretical physicists and mathematicians to do, and we could be grateful for that, in the current situation.

12. **Chris W.**
December 23, 2011

Also see section 7 of Wikipedia's article Spacetime for a discussion of spacetime's dimensionality, with references to two papers by Ehrenfest and one by Tegmark. (The latter employs a partially anthropic argument.)

The PDF document linked in Mitchell Porter's comment slightly misstates the title of Ehrenfest's *Proceedings of the Amsterdam Academy* paper.

13. **Plecostimous**
December 23, 2011

In *The Road To Reality*, Penrose writes that classical N+1 GR spacetimes with N>3 are unstable under a theorem he proved together with Hawking. Taking this together with T-duality wouldn't one then expect a spacetime with 3 large dimensions and 6 with Planck-scale ones to be dynamically stable?

The classical instability would presumably cause one of the existing small dimensions to collapse in on itself if it spontaneously became much larger than the Planck length, and by T-duality one might expect a natural corollary of this to be that it can't get much smaller than the Planck length either, as that would be dual to some other geometry with with more than 3 spacial dimensions and would likewise be classically unstable.

(One might further expect spacetimes with *fewer* than 3 large space dimensions to be vulnerable to the spontaneous expansion (or contraction) of one of their Planck-scale dimensions due to the absence of this instability.)

14. **abbyyorker**
December 24, 2011

    Layman question:

    Does anyone know whether this result, or its antecedents, would hold for all of the 10^500 false vacua? Or is that huge ambiguity just a perturbative artefact? If so, why so much dismay about the unscientific nature of the anthropic "landscape" if it will go away when when we do non-perturbative calculations?

15. **Peter Woit**
December 24, 2011

    abbyyorker,

    The IKKT matrix model the authors are using dates back to 1996, one of a class of proposals for a fundamental non-perturbative definition of string theory from around that time. These proposals don't seem to lead to low-energy physics that looks anything like the real world, so most people abandoned them after a few years.

    The landscape models of the last decade use a different philosophy. They don't start from a fundamental non-perturbative theory, but just assume the existence of such a thing, with various properties, and then try to construct "string vacua", which are supposed to be consistent low-energy solutions to the unknown fundamental theory, whatever it is. One can certainly take the point of view (Gross and many others do), that until you actually find the fundamental theory, claims about the "landscape" of solutions to it shouldn't be taken too seriously. The problem is though that if you take that point of view, you can no longer say much of anything at all about how string theory is supposed to connect to reality.

16. **Plecostimous**
December 24, 2011

    ^^^ meant to write "a spacetime with 3 large dimensions and 6 Planck-scale ones"

17. **Cesar Laia**
December 26, 2011

    About Kaku, funny response here: http://bigthink.com/ideas/41681

    It has some gems, such as "or finding experimental deviations from Newton's inverse square law which may prove the existence of parallel universes".

18. **Peter Woit**
December 26, 2011

    Thanks Cesar,

    I also like Kaku's definition of a superpartner: "higher vibration of the string".

19. **Bernhard**
December 26, 2011

    Would be interesting to know who, according to Kaku, would win the bet if SUSY were discovered, since he seems to make no distinction between it and strings.

2011: A Banner Year for Hype

December 30, 2011
Categories: This Week's Hype

Since every blogger seems to feel it necessary to have a year-in-review posting, I thought it appropriate to point out that 2011 has been a banner year for string theory and related hype, with about twice as many editions of "This Week's Hype" as in previous years. One reason for this is the LHC. When talking to journalists, string theorists are rarely willing to admit that the hopes of the past couple decades that string theory would make some predictions about LHC energy scale physics turned out to be a dismal failure, and this tends to lead to confused headlines. Besides the LHC though, there's a huge on-going effort to promote other bogus "tests of string theory". This has been going on since string theory's lack of testability problem first started to get a lot of attention a few years ago. I see no reason for either of these two driving forces to weaken in 2012, so expect more editions of "This Week's Hype" next year.

String theory supported by early LHC heavy ion results

Cosmologists expect the LHC to turn up evidence for the multiverse

M-theory shows that the LHC will be the world's first time machine

Neutrons could test string theory

Octonions explain string theory

The LHC tests string theory (more heavy ions)

M-theory is a big success, predicts behavior of 4 qubits

String theory and heavy ions, yet again

Multiverse observed in the CMB – Not

String theorists suggest space wormholes possible

String theory testable with black holes and pulsars

String phenomenologists come up with predictions testable at the LHC

Superluminal neutrinos evidence for string theory

Superluminal neutrinos could be explained by string theory

CMS multi-leptons provide evidence for SUSY

A new laser will tear apart the fabric of space

A nuclear clock will test string theory

The LHC will decide between two versions of SUSY

String theory research is going well, only problems are Garrett Lisi, Lee Smolin and Peter Woit.

Gordy Kane predicts the mass of the Higgs using string theory, just days before the announcement

Superstring theory predicts three space dimensions.

Comments

1. **chi_b(3P)**
   December 30, 2011

   Then again, one could blog something about the chi_b(3P) aka real physics.

2. **Yatima**
   December 30, 2011

   Let me take this occasion to wish everyone in Physics and Math community, whether their soul is laden by a large ego or not, a successful 2012, with lots of sigmas on the experimental front and important insights on the theoretical one.

   Hopefully the fiat money economy won't have collapsed, another few unnecessary wars won't have been kicked off and various civil liberties that one thought written in stone won't have been flushed down the drain.

   Godspeed – and Good Luck!

3. **Quantumburrito**
   December 30, 2011

   To me it seems pretty clear that string theorists are getting desperate and are sparing no efforts and platforms to peddle their wares (As you noted, Gordon Kane for instance has had at least three articles about the apparent "testability" of string theory in the last month or so alone). The question is how long this attempt at PR will last and how willingly a credulous public will buy the claims. Sadly, almost anything found by the LHC will be seen as a successful prediction of string theory and the public will have scant capabilities to call out the shenanigans.

4. **BJM**

December 31, 2011

chi_b(3P),

The title of this blog announces a different mission than just blogging about real physics.

While I enjoy reading popular accounts of cutting edge physics here and elsewhere, this blog provides a valuable service in pointing out "physics" that goes well beyond the edge.

5. **Zarrax**
December 31, 2011

Why do you have nothing to contribute to physics other than dragging down string theory? I am not saying there are not grounds to object to string theory, but as a physicist shouldn't you have some worthwhile research of your own to help the field in some way? We get it, string theory is untestable… but why on earth do you have to say the same thing over and over and over again, day after day, month after month, year after year? Don't you have any interesting ideas to share with others? It really is disturbing to see that there's someone out there who tried to be a successful theoretical physicist, failed (in the sense you don't seem to be adding any substantial research of your own), and now just lives to drag down string theory.

6. **KFP**
December 31, 2011

Zarrax,

Someone has to do this job such that the string theory research won't such in all the money and attention.

Also, why should we tolerate people like Brian Greene promoting a speculative idea like string-theory and multiverse as the only sensible answer to the everything.

I am glad somebody has the courage to do it.

7. **Brian Dennehy**
December 31, 2011

Zarrax,

It is called a `culture of criticism'! In a typical fluff piece about string theory you will seldom find much questioning of some of the zany claims some of these guys make. Regardless of what you think about Peter Woit's research record I think that this blog performs a very valuable function. Your use of the word`failed' in regard to him really does mark you out as someone who is involved in some of the stuff criticized here and ruffled by what you've read. You should learn to be able to accept criticism of your research and be able to engage with it. It is one of the most valuable parts of science.

8. **DaveC**
December 31, 2011

Someone (Zarrax) had a bad year.

Hype is in overdrive throughout science nowadays. I wish there were someone as intelligent, well informed, balanced and independent as Peter collecting examples of hype in condensed matter physics, for example. But there isn't and there probably never will be, because we all have out-yell our competitors in front of the program managers and editors to get funded and published, and those famous guys who are masters of hype are the reviewers of our manuscripts which will never get into Science or Nature if we don't suck up.

Happy New Year to my favorite blog!

9. **Peter Woit**
December 31, 2011

Zarrax,

I've been working for quite a while now on trying to understand relations between quantum field theory and the Langlands program in number theory. This has been going quite well and I'm in the middle of writing a paper about it, which will likely be titled "Automorphic Representations and Quantum Field Theory". A crucial part of the story involves the ideas about BRST and Dirac Cohomology which I've written about here

http://www.math.columbia.edu/~woit/brstdirac.pdf

Now that I can see where this project is going, I'll soon finish that paper finally. You may not like any of the ideas I'm investigating, but I do spend most of my time on them, very little thinking about the multiverse or string theory.

I agree that "string theory is untestable" is a boring idea to keep repeating. However, until string theorists stop issuing press releases claiming to have found a test for string theory, having some place which explains what is wrong with these press releases seems to me to be worthwhile. The day someone else takes up that task, I'll happily stop doing it myself. Even better would be for string theorists to stop issuing the bogus press releases, or for the science media and their colleagues to start laughing in their faces when they try and do this. I think we're getting to the latter, if not the former.

10. **Peter Woit**
December 31, 2011

chi_b(3P),

I try to stick to blogging about topics where I think I have something to contribute that isn't done better somewhere else. For lots of particle physics news, as in this case, I don't have anything insightful or interesting to say, and I expect that interested readers are also reading blogs that cover these topics very well (three good examples are Resonaances, Matt Strassler's blog, and Tommaso Dorigo's blog).

11. **Peter Woit**
December 31, 2011

Yatima (and all),

Best wishes for the New Year!

One of my New Year's resolutions will be to finally update my home page, that should get done very soon. Another will be to finish the BRST paper, and get the new paper I'm writing in some reasonable shape for others to read, maybe before classes start.

I'm very much looking forward to 2012. The ideas I've been working on for the last couple years are coming together enough to begin writing about them. Last fall our visiting Eilenberg lecturer was Dick Gross, and I learned a huge amount about representation theory and number theory from him. This spring, Edward Frenkel will be here, lecturing on the Langlands program and QFT, and I'll be teaching again our graduate course in representation theory. We should finally find out for sure about the Higgs. A very promising year indeed.

12. **Zarrax**
December 31, 2011

It's not really that believable that you will suddenly produce some major research after all of this time. Of course only time will tell how significant "Automorphic Representations and Quantum Field Theory" will be. But as they say, one's past research record is often the best indicator of one's future accomplishments. The fact that this work seems to have been dragging on for years without you producing much else is not encouraging.

On the other hand, you have kept up this blog full-throttle since 2004. You are highly motivated about some things at least. It is sad that this is what you care about the most, dragging down successful physicists because they may not deserve it. I don't think that lowering others raises you. Even if their success is unjustified. Maybe you should try to do something more constructive with your time, as this blog really does make you look like you're embittered over your position in the physics world and are trying to take others down who you don't think deserve it. Surely you can do something better with your time (and I don't mean gradually writing a math paper over a decade or more).

13. **Zarrax**
December 31, 2011

By the way, I don't agree with this idea that you need to counteract the publicity people like Brian Greene or Michio Kaku get… in the math and physics world many people do understand the limitations of string theory in the current form, and it has reached the mainstream media to some extent too. Does it really matter so much that Brian Greene can show up on TV wearing a trenchcoat and BS about the universe? I mean, I too roll my eyes when I see that kind of thing but I am not going to make a mission out of tearing him down.

14. **Brian Dennehy**
December 31, 2011

Zarrax,

You're coming across as rather bitter, nasty, and petulant. Your comments are terribly gauche, it makes for bad reading.

15. **Peter Woit**
December 31, 2011

Zarrax,

I don't know who you are, but if you think I'm "embittered", you definitely don't know me. Since you're hiding behind anonymity, we can't examine the record of your research and how it's going, but you sure sound like a string theorist embittered by the failure of the research program you devoted your life to, upset that this failure is becoming publicly recognized. If you want "embittered", take a look at Mike Duff's rant about me, Lee Smolin and Garrett Lisi.

It's New Year's Eve, and I'm not anonymously posting personal attacks on people I disagree with, but reflecting on how lucky I've been in life, rewarded professionally, personally and financially in all sorts of ways that I deserve much less than many others. Life is very, very good, right now, has been for many years, and there isn't anyone in the physics community I'd willingly change places with. To the extent anyone has managed to get professional or financial rewards from multiverse mania or making bogus claims about string theory, that's fine with me. My arguments are with their science, nothing else.

16. **Douglas Natelson**
December 31, 2011

DaveC, I try, though I tend to focus more on nano-hype.

17. **Henry Bolden**
December 31, 2011

You can tell it's the holiday season – Peter is generously allowing the nasty (and hence entertaining) comments to be posted.

18. **Zarrax**
December 31, 2011

I'm actually a mathematician in a field not related to string theory… but I think your wishful thinking does go to your true motives… you want to gloat in the failure of string theorists. This is your right, I suppose, but it doesn't really engender much respect.

Anyhow, one point I do agree with, is that it's getting a little late in the day of New Year's Eve to be arguing about this stuff. So have a happy holiday, and have an even better 2012…..

19. **Anonymous**
December 31, 2011

Zarrax:

If string theory were currently a worthwhile, productive research program, I don't believe one lone blogger criticizing it would really have attracted much attention, especially when the lone blogger was a not particularly famous mathematician/physicist. I know a few physicists who have held very dim views of string theory for some time, but for various political reasons have kept relatively quiet about this. Peter Woit has merely been playing the role of the child in the story of the *Emperor's New Clothes*, pointing out something that many people believed but were not particularly eager to be the first to point out.

20. **David Berman**
January 1, 2012

Its a new year so I can break my resolution (of last year) and make a post, something I don't intend to do often since I always end up getting frustrated. I for one feel very happy with progress over the last few years in string theory (more on this later). I suppose my string theory colleges are always a bit mystified by Peter's unwillingness to acknowledge progress that perhaps we subscribe his motivation for the Blog to other reasons. I won't speculate and like some many welcome some genuine work. But will Peter acknowledge how the attention of the relationship of the Langlands programme to quantum field theory came about through M-theory. In particular Witten's work which was clearly inspired by deriving S-duality from in quantum field theories through fivebrane compactifications. I suppose this is what is annoying, the insights into quantum field theory that keep rolling on are obviously valuable and come from string theory; our understanding of quantum gravity with holography and other developments continues; the applications to things like condensed matter systems, quark gluon plasmas and a whole host of things. Yes string theory deserves the headlines and progress contunes. I have no idea what weird eureka insights readers of this blog want but do people imagine that all those string theory papers really have no value? Do people imagine that the strings conference is like a meeting of the illuminati to discuss world domination through a conspiracy or perhaps actually many people are doing interesting work. If they or their universities publicise it then well and good, in an environment where society may values engagement with the sciences then communicating the enthusiasm we have for science if good for everyone. I congratulate my colleges for engaging with the press positively.

Happy New Year to all and looking forward to another productive one

David

21. **d4string**
January 1, 2012

Zarrax is very wrong ! Having more skeptical people like Peter Woit is necessary. New graduate students should become familiar with these sort of failures of string theory, and they should not believe all the hype. In the end it should help channel the research to more meaningful direction even within a string theory approach.

22. **MathPhys**
January 1, 2012

Happy New Year, Peter and all!

23. **CIP**
January 1, 2012

David Berman,

Did you look at the list of links in Peter's post? Do you really think all that hype was justified by this year's (or decade's) progress in string theory? I don't hear Peter saying that string theorists have produced nothing of value – but I do hear him pointing out (again and again) examples of claims unjustified by the facts. If guys like Kane and Duff would stop spewing BS, Peter would quite likely feel free to stop saying "BS." Figuratively speaking.

24. **BJM**
January 1, 2012

Re:
Zarrax : " Does it really matter so much that Brian Greene can show up on TV wearing a trenchcoat and BS about the universe?"

I am a consumer of these types of shows, and nowhere near knowledgeable enough to sort out the wheat from the chaff. This blog helps me enjoy the theatrical "what if's" while keeping my feet on the ground. I and apparently many others find this blog useful. There are other blogs out there for you.

25. **Peter Woit**
January 1, 2012

David,

You're making up straw-man arguments that I never make, and you're completely ignoring the ones I do make. Do you really think that the material linked to in this blog entry can be justified as an honest and accurate portrayal of string theory research? Is it honest or accurate to go to the press and claim to have found a "test of string theory", or is that a way of trying to mislead people about the fact that string theory unification is an idea that appears to have failed, as evidenced by its inability to predict anything about anything? String theory was sold to the public as a way to unify physics, but that's a project that has devolved into the pseudo-science of the landscape. I think you and most of your colleagues know this very well, and it would be healthy if you were to admit that and move forward, instead of complaining that I'm pointing this out.

I'm a fan of Witten's work that relates various QFTs to Khovanov homology and geometric Langlands (and often write about it on the blog), and sure, he uses M-theory and string theory ideas in that work. The exotic 6d QFT behind a lot of this plays a crucial role in M-theory, but it's also an important question to understand to what extent it can be understood independently of M-theory. While Witten does make an attempt to popularize some of this material (see his public talks on knot theory), and retains hopes for string theory unification despite its problems, he doesn't descend to misleading claims that what he is doing provides a "test of string theory".

Sure "string theorists" are often doing interesting things, although more and more often they have little to do with string theory. I noticed that several participants at Strings 2011 pointed out that few of the talks actually were about string theory. People doing good work in this area should think hard about the kind of hype that's in these links and whether it's really a good idea for them to try to defend the indefensible because it's coming from their "side". Duff is quite right that "string theory" is getting a bad name among the public and granting agencies, but it's largely because of the hype, and his idea that the way to fight this is with more hype is completely backwards.

26. **Ryan Budney**
January 1, 2012

Happy new-year Peter. I always enjoy reading your blog. The anonymous trolls are a bit annoying at times but I suppose having a small number of them about isn't unhealthy. That's the closest to a positive I have to say about them.

27. **Bernhard**
January 2, 2012

Peter,

Happy new year. This blog is one of the best services for the HEP community I know. As far as I understand your critics of string theory as not yet "old story" since, as you said, the string-hype goes on non-stop and a healthy critic counter-weight to all the lies being written everyday about bogus tests for string theory is still extremely valuable. You make many string theorists outraged and make enemies on the way, but critical and skeptical scientists (like they all should be!) support you and enjoy reading your views.

I´m looking forward to read your paper. One thing however that is predictable to me is that no matter how good you can make it, wait for unreasonable critics to it from the people that are angry with you.

Looking forward to 2012´s first entry.

28. **chris**
January 2, 2012

Peter,

keep up the good work. the hep literature is full of junk and you are one person dedicated to weeding it out. that is much more helpful than contributing to accelerate the growth rate of phys.rev. volumes until it reaches the speed of light 🙂

happy new year.

29. **Anonyrat**
January 2, 2012

Zarrax: It is my guess that if there was a headline "5-brane compactification in string theory leads to mathematical breakthrough" Peter Woit would likely not take issue with it. He might even promote it. Look at the two dozen headlines featured in this blog post, and compare them with this hypothetical one, however.

30. **Mike**
January 2, 2012

I have a feeling this will be the year that physics gets a lot of the experimental data it needs to sort out many brain twisters – we know the LHC will be the one to watch but I think we will see data from other simpler experiments come in that help us understand the universe better. Reminds me of a talk James Van Allen gave in the early 90s at a small satellite conference – "How to do science for (much) less than a million dollars". Thanks to Peter for providing such a fascinating discussion with this blog!

31. **Eric**
January 2, 2012

Peter,

Up until the end of last year, your position was that the Higgs probably did not exist and you promoted this idea relentlessly on your blog. Yet, it seems that it does exists and has been observed. You have had a similar position in regards to low-scale supersymmetry. How will you react whenever the first signs of SUSY show up at LHC? Will you attempt to dismiss this as "more hype"?

32. **Rob R.**
January 2, 2012

> Re:
> Zarrax : " Does it really matter so much that Brian Greene can show up on TV wearing a trenchcoat and BS about the universe?"
>
> BJM: I am a consumer of these types of shows, and nowhere near knowledgeable enough to sort out the wheat from the chaff. This blog helps me enjoy the theatrical "what if's" while keeping my feet on the ground. I and apparently many others find this blog useful. There are other blogs out there for you.

Ditto, my fellow layman! Happy New Year and keep up the good work, Peter!

33. **Peter Woit**
January 2, 2012

Eric,

That's nonsense. My position has never been that "the Higgs probably did not exist". For a typical posting about this from last year, see (from after the summer release of data)

http://www.math.columbia.edu/~woit/wordpress/?p=3960

where I write: "nothing yet has changed my view that a Higgs particle scenario and a no-Higgs scenario are equally likely. " It's definitely true that I've often pointed out the the no-Higgs scenario would be a lot more exciting for particle physics, and was often hopeful last year that this was what the data would show. Unfortunately, it definitely now looks like it's going the other way.

I continue to strongly doubt that the LHC will see anything like conventional SUSY. If something new does show up, that will be fantastic. And, sure, whatever it is, my prediction is that early ambiguous signals will be promoted as evidence for SUSY, whether it turns out to be that or something else…

34. **Eric**
January 2, 2012

Peter,

When all is said and done, and the LHC has made its discoveries in the coming years, I think there is a high probability that your so-called skepticism will be revealed to be nothing more that close-mindedness. Now, it really should go without saying that the job of a theorist is to consider all possibilities in nature, and not to simply reject certain ideas just because you don't happen to like them (whatever your motivations happen to be).

Given that the Higgs exists in the mass range 120-125 GeV, there are really only two possibilities: 1) Supersymmetry (or something like it) exists and solves the hierarchy problem, or 2) The Higgs mass is fine-tuned. Possibilities such as technicolor and warped extra dimensions would appear to be ruled out. So really, I don't think that stories about supersymmetry should be considered hype. Granted, if the Higgs mass is 125 GeV, the MSSM is a bit strained to obtain such a Higgs mass. However, this just means that the MSSM should be extended in some way.

It would serve you and your readers better to have real discussions about real physics rather than continually droning on and on about how bad all of those theorists are for being excited about their ideas.

35. **Sheldon Pherris**
January 2, 2012

Peter,
Happy NEW Year!!!! (N for Not, E for even, W for Wrong). All of the opinions and links available on this blog have been and continue to be an incredible resource!! Thank you.

36. **derek**
January 2, 2012

Happy New Year all!

To PW's outspoken critics: I am genuinely curious… Why the (perceived) obsession and anger? Some claim PW's arguments lack merit; some resort to personal attacks? Why? If that were the whole story then why feel threatened? Indeed, why pay any attention whatsoever?
It astounds me to see so many highly educated scientists lashing out at a handful of critics. Perhaps they could benefit from a high school level general science course. I always thought of science as a melting pot of ideas and criticisms, and experiment as the ultimate arbiter. Stop complaining and prove

37. **Zarrax**
January 2, 2012

Derek, I'm not especially angry or obsessed… I just am witnessing a blog going back to 2004 which says the same thing over and over again. So I wonder if the source of Woit's obsession (and it is clearly an obsession) is some bitterness over his career. Maybe it is, maybe it isn't. But you have got to admit it's a little weird for anyone to beat this same drum thousands of times. There has to be some very serious motivation here, and to the outsider it's not so clear why anyone would be so obsessed with this one issue.

38. **Bob Levine**
January 2, 2012

from Zarrax: "There has to be some very serious motivation here, and to the outsider it's not so clear…"

Well, for starters, how about the enormous lack of intellectual candor and the embarrassing drop in standards of integrity in theoretical physics which have accompanied the dominance of string theory (sociologically, at least) for much of the past four decades? Peter adduces massive evidence (in the form of the hype-list 'Wall of Shame' he provides in the OP for this thread) that a sizable chunk of the string theory community has made grotesquely exaggerated claims for the framework's predictive success, and your response is that it's most likely career bitterness?? The field reflects serious intellectual corruption and duplicitousness, so blame the messenger and make snide insinuations about what his motives could be… *excellent* reasoning, Zarrax.

39. **John**
January 2, 2012

Zarrax, why are you intent on casting a reasoned objection as a pathological obsession? You might have a point if Peter just engaged in empty rhetoric against ST, but that clearly isn't true. Maybe you should engage the actual argument rather than casting aspersions…

40. **Anonymous**
January 2, 2012

Zarrax says:

But you have got to admit it's a little weird for anyone to beat this same drum thousands of times. There has to be some very serious motivation here, and to the outsider it's not so clear why anyone would be so obsessed with this one issue.

I don't think it's strange at all. The string theory hype has not stopped, so Peter Woit is not about ready to stop his campaign against it.

41. **Eric**
January 2, 2012

I'm curious, why is it such a problem if string theory gets a lot of attention and/or hype? I think this would happen anyway, if not with string theory then with some other area of physics. Up until about 15 years ago, most of the hype went to quantum mechanics and relativity. String theory fulfills a public niche for those who are interested in physics "on the edge". At the very least, this serves to get people interested in physics. On the other hand, blogs such as this one* and Smolin's books tend to create a negative impression for physics and physicists in general. If this infighting is left unchecked, this will ultimately have a very negative impact on scientific funding.

* I should say that this only applies to some of the posts. Many of the other posts are actually quite good and provide a forum for discussing real problems in physics.

42. **Bob Levine**
January 2, 2012

OK, Eric, try this on for size: the real problem with the Enron catastrophe wasn't the depth of the dishonesty in the financial sector's business practices or the corruption in the political system of this country that allowed it to happen in the first place; it was all those damned Wall Street Journal investigative journalists who kept nosing deeper and deeper into the story until the horror stories about Skilling and other executive felons emerged. It really made the American financial sector look awful, didn't it—-shook up our faith in the system and put the economy at risk. Does this interpretation sound absurd to you? Well, read over your own rationale in your previous note and tell me exactly how your spin on string theory/landscape hype differs in principle from that take on the financial crisis of a decade ago. Your story sounds *exactly* like the analysis of the Enron fiasco I just presented.

If string theorists go on making ludicrous claims, pretending that they really *can* predict anything substantial about the physical world, continue ad nauseum about how ST is still the 'only game in town' for unifying electroweak/QCD and gravity in a single formalism, and crafting press releases which in some cases border on (and into) fraudulence—when the truth is that there has been exactly zero progress on any of these fronts—then Lee Smolin and Peter are the baddies for spoiling people's profitable credulousness? Are you *serious*??

43. **Eric**
January 3, 2012

Dear Bob,

As a point of fact, string theory is the only known way to unite quantum field theory with gravity. In addition, it is possible for specific string vacua to make predictions. There is absolutely no dishonesty in any of the claims made by string theorists. In fact, this constant suggestion that string theorists somehow control some evil empire bent on deceiving the public and controlling science funding is one of the reasons that most string theorists find this blog and Smolin's books highly insulting. String theory and string theorists have been under continual unjustified assault and attempted character assassination now for many years. As best as can be ascertained, these attacks originate from those who have some sort of inferiority complex or feelings of jealousy in regards to high energy theorists.

44. **lun**
January 3, 2012

In 2012 it looks like loop quantum gravity has the edge on string theory
[on over-inflated misleading press releases]

Only 3 days into the year and LQG is already experimentally testable by "measuring Hawking radiation precisely".

45. **Peter Woit**
January 3, 2012

lun,

You're right, this seems to be an attempt to have LQG compete with string theory on the hype front. I'm curious if there's an actual press release behind this, if anyone has seen such a thing, let me know.

46. **Lee Smolin**
January 3, 2012

Dear Eric,

Since you mention my books, plural, I can't help but ask you a question:
Please tell me exactly which passages of my first two books in your view "tend to create a negative impression for physics and physicists in general."

I have been impressed that none of the people who read my third book as critical of string theory have ever expressed any appreciation for the very hopeful and positive picture of string theory drawn in my second book. Nor do most credit the first book for its introduction of the concept of the landscape.

Then lets go back to the third book, and please tell me exactly which passages in your view create a negative impression of physics or physicists in general.

Exact quotes would be appreciated.

The reason I ask these questions is that my third book has been brutally misquoted and mis-characterized by people claiming to disagree with it. I often find that people who think they object to something they think I wrote are actually responding to a rumor or a misquotation. I am happy to defend what I actually wrote, but I am very tired of being challenged to defend things I didn't write. Now it seems that the mis-characterizations of my third book are being sloppily attributed to all my books.

I can add that I have piles of emails and many reviews thanking me for writing books that give a positive and hopeful characterization of physics and physicists.

Thanks,

Lee

47. **Zarrax**
January 3, 2012

Lee: Even using the title "The Trouble with Physics" can create a negative impression of physics and physicists. I read your book (admittedly a while ago), and you really did sound very downbeat about the state of physics at that time. I don't remember passages anymore, but after I read the book it seemed clear that you felt physicists were on the wrong path.

Also, since the first two books aren't prerequisites for the third, and there are many like myself who only have read the third book as it was more widely distributed, it would be an easy error for people to make to assume your other books had a similar viewpoint.

48. **Zarrax**
January 3, 2012

Lee: One thing I do remember about the (third) book was you mentioning that people were looking for the next Einstein as if they were waiting for the coming of the Messiah. This really does convey an air of desperation. And the way so many people gravitated towards Garrett Lisi when he came out with his theory of everything just confirmed that impression for me.

49. **Peter Woit**
January 3, 2012

Zarrax,

Einstein is a huge model of success for theoretical physicists, so, hoping for the "next Einstein" is something people have always been doing. I've not noticed more of this now than at any other period. Also, I don't know why you think "so many people gravitated towards Garrett Lisi when he came out with his theory of everything". That's not true if you're counting theoretical physicists.

Fundamental physics made huge advances in the years leading up to the mid-seventies, much less since then, and this is not a controversial statement. If you're going to write honestly about the state of the subject, it's part of the story.

50. **Giotis**
January 3, 2012

Lee said:

"Then lets go back to the third book, and please tell me exactly which passages in your view create a negative impression of physics or physicists in general."

"What we are dealing with is a sociological phenomenon in the world of academic science. I do think that the ethics of science have been to some degree corrupted by the kind of group think explored in chapter 16, but not solely by the string theory community."

and later

"It may seem strange to be discussing academic politics in a book for the general public, but you, the public, individually and collectively, are our patrons. If the science you pay for is not getting done, it is up to you to hold our feet to the fire and make us do our job."

The public should make you do your job?! Well I can’t think of anything else that could create a more negative impression for physicists.

51. **Peter Woit**
January 3, 2012

Giotis,

Lee’s comment here was directed at Eric’s complaint about his “books” and specifically asked for an example of what Eric was complaining about from either of his first two books, to justify the plural. You’re completely ignoring this.

If Eric wants to provide such examples, or Lee wants to respond to you or Eric, they can do so. But the argument over Lee’s controversial book is really not only off-topic, but very old by now, so please don’t anyone try and carry it on here. I really doubt anyone has anything new to say.

About this topic, I do want to say though that when I wrote my own book (mostly in 2002), I tried to stick to the science and avoid what could be interpreted as personal criticism of string theorists (perhaps not always successfully, but still…). If I were to write an updated version (not damn likely), I would be sorely tempted to go on at length describing some of the huge number of examples of grotesque, unprofessional behavior that I’ve seen from string theorists in more recent years. My current view is that if there’s any criticism of string theorist’s behavior in Lee’s third book, it’s way too mild.

52. **Eric**
January 3, 2012

Dear Lee,

The books I am referring to are “Three Roads to Quantum Gravity” and “The Trouble with Physics”, both of which contain attacks on string theory. The attacks in the first book aremore subtle and more-or-less attempt to shade string theory in a bad light in comparison to alternatives, while the attacks in the infamous second book are more direct. The chapter on the “Sociology of Science” is particularly bad. I think it’s fair to say that the second book is basically a reworking of the first.

Best,

Eric

53. **Eric**
January 3, 2012

Also, I should emphasize that my point is not to continue the string theory vs. alternatives fight, but rather that the attacks on both sides of this debate are counterproductive to the credibility of the physics community. This also includes Lubos Motl counterattacks. The physics community is all better served if we attempt to work together towards common goals rather than fighting over whose approach is better.

54. **Peter Woit**
January 3, 2012

Eric,

Note that you’re not providing any of the evidence for your argument that Lee asked for. About his first two books, your only complaint is that one of them makes string theory look worse (as an idea about quantum gravity) than alternatives. This has nothing to do with creating “a negative impression for physics and physicists in general.” Evaluating which scientific ideas are working is a standard part of doing science, and it’s not criticism of scientists to do this.

55. **Bob Levine**
January 3, 2012

@ Eric: “The physics community is all better served if we attempt to work together towards common goals rather than fighting over whose approach is better.”

Eric, I am having a *very* hard time understanding your conception of science. There is an empirical domain, and there are competing models of it, and if one of them is correct, then either (i) the others are wrong or (ii) some subset of the competing approaches are in effect mathematical variants of each other (think Heisenberg vs. Schroedinger in QM, or Feynman/Schwinger as reconciled in the work of Freeman Dyson). But the conflict between plum-pudding model of the atom vs. the ‘orbital’ (energy-level shell) model, or the analysis of cosmic rays as high energy photons (Milliken) vs. charged particles (Compton) wound up with the defeat of one model and the victory of another, as determined by the tribunal of predictive success. QM displaced classical physics the same way, and there were plenty of classical physicists who viewed the ascension of QM not as progress, but as a bitter defeat. Unfortunately, it is a fact that some approaches will be better than others, at the end of the day, and science’s job is to attempt to find the truth about the material world, not to be a big tent where everyone has to make nice with everyone else. Not everyone can be right, alas. And when one side has presented enormously far-reaching claims with virtually no hard results to back up those claims (look again at Peter’s hype list), the other side(s) can hardly be faulted for pointing out that no, you haven’t even come close to justifying those claims. Debating (civilly but *fiercely*) about ‘whose approach is better’ is a necessary *part* of working together towards common goals.

56. **Lee Smolin**
January 3, 2012

Zarrax,

“The trouble with X” is a recurrent trope in literary and popular culture that carries overtones of affection and bemusement. It goes back at least to a 1955 Hitchcock film “The Trouble with Harry”. I am sorry if you missed the allusion, but many readers got it. In any case I insist that the characterization of physics I gave was balanced. There was a whole part of the book devoted to new directions. I’ve gotten lots of email from young physicists saying my books-and that one in particular-inspired them to go into physics.

I don’t know how to answer the rest of your messages because its hard to argue with people who insist on their vague misunderstandings. I am pretty sure I never said anything like, “people were looking for the next Einstein as if they were waiting for the coming of the Messiah”.

And how does this answer Eric: “it would be an easy error for people to make to assume your other books (which they had not read) had a similar viewpoint.” Yes, and would you assume all of Herman Melville’s books were about whales?

Similarly, no one I know of “gravitated to Lisi”. Since you think many did, please name one.

Thanks,

Lee

57. **Zarrax**
January 3, 2012

Lee: In the book jacket of the edition I have it says “Smolin not only tells us who and what to watch in the coming years, he offers novel solutions for seeking out and nurturing the best new talent- giving us a chance, at long last, of finding the next Einstein”. While technically you may not have written the book jacket, you can’t really blame the reader for thinking as I do since the book jacket is supposed to summarize what the book is about. And I don’t really have the time to go through the book looking for other corroborating comments.

And here’s something from viii of the introduction: “To put in bluntly, we have failed. We inherited a science, physics, that had been progressing so fast for so long that it was often taken as the model for how other kinds of science should be done. For more than two centuries, until the present period, our understanding of the laws of nature expanded rapidly. But today, despite our best efforts, what we know for certain about these laws is no more than what we knew in the 1970s.”

Quotes such as these (which I found within five minutes) do convey a negative impression of theoretical physics and physicists, namely that they are stuck and don’t know where to proceed, and are desperately seeking the next Einstein. (And to Woit: Note that a string theorist apologist wouldn’t say the kinds of things I am saying.)

Lisi did have plenty supporters. I am not a physicist so I don’t know the inside scoop but I did see many quotations at the time from scientists who at least took him quite seriously. I think a google search should still find them.

58. **Peter Woit**
January 3, 2012

Zarrax,

You clearly have absolutely no idea what you are talking about re Lisi, or the problems of theoretical physics, and the only thing you can find to back up your “next Einstein” business is something you know Lee didn’t write. If you want to launch stupid personal attacks on me, that’s one thing, but it was my mistake to not follow my usual policy of deleting anonymous attacks on others.

It’s now been a very long while since there was an on-topic comment here, so I’ll close comments on this posting.

New For 2012

January 2, 2012
Categories: Langlands

To celebrate the new year, I've finally gotten around to updating my home page, and have updated the blog theme to the latest wordpress default. I've added an "FAQ" feature, which is still under construction and should get additions as I find time. Please let me know of anything that doesn't work.

This coming semester I'll be teaching the second half of our graduate course on Lie groups and representations, following on from Andrei Okounkov's first semester. A tentative syllabus is here, and I hope to find time to update some of my older lecture notes as well as write some new ones.

The time I've spent the last couple years trying to learn about the Langlands program has finally gotten me to the point of thinking I understand enough to write something sensible about what the relationship might be between automorphic representations and quantum field theory. I'll be working on that and preparing the graduate course for the next couple weeks.

One thing that was extremely helpful was Dick Gross's lecture series here last semester. Video of the lectures is on YouTube here. Gross is an incredibly good lecturer, and this series was aimed at explaining exactly many of the things I've been having trouble understanding about this subject. Anyone who seriously wants to understand the representation theory point of view on number theory and the Langlands program should find these lectures helpful.

This spring the Eilenberg lecturer will be Edward Frenkel, who will be lecturing on geometric Langlands and quantum field theory. In some sense he'll be picking up where Gross left off. He was at Gross's last lecture, and there was a ceremonial handing over of the chalk. Of course I've very much looking forward to these lectures, and I expect that there will again be video available.

Comments

1. **pah**
   January 2, 2012

   my two cents: the banner graphic is way too big now.

2. **Peter Woit**
   January 2, 2012

   pah,

   I kind of agree, but the header height doesn't seem to be easily adjustable. For now I'm going to try just getting rid of the header. Should find a new one anyway…

3. **jorge**
   January 3, 2012

   My 2 cents:Congrats, great updates, but text font of this theme is not as easy to read as was before (arial/verdana)

4. **Bee**
   January 3, 2012

   Which banner graphic? I can't see one (using Google Chrome, 1280×800). It looks pretty awful altogether with my browser. All the spacing is far too large. There is also no sidebar (should there be one?).

5. **Bee**
   January 3, 2012

   Ah, correction, there is a sidebar on the main page.

6. **Jim**
   January 3, 2012

   Clean and sleek, if spare.
   Uncluttered, yes. Sparse, perhaps.

   I shall miss your classic masthead graphic.
   This post *What's That at the Top of This Page?* might not make sense without it.

7. **plm**
   January 3, 2012

   I like the new design, thank you. There is alot of empty space though, in many places, too much I find.

   Regarding ideas on ARep/QFT can you say a bit more -about yours and others' if related, perhaps with references?

   Thanks.

8. **cormac**
   January 3, 2012

   Sounds like a frightening teaching assignment! All of my own teaching is at undergrad level, thank God, plenty of time to work on other stuff. Though it must be great to have such a convergence of teaching and research.
   Re new design, I'm puzzled by the WordPress idea of putting all the links, headings on the homepage only – a lot of first-time readers may never make it to the homepage
   Best wishes for the NY, Cormac

9. **Joel Rice**
   January 3, 2012

Just a tad surprised at not seeing a reference to Pertti Lounesto's work on Spinors and Clifford Algebras, speaking of which, a long time ago somebody swore that the curvature of spacetime can actually be seen around Chevalley's book on theTheory of spinors.

10. **nitpicker**
January 3, 2012

A tad puzzled why you say "Spin(2n) as a double cover of SO(2n)". For example Spin(3) = SU(2) is the double cover of SO(3) is the classic example of spin angular momentum.

11. **Peter Woit**
January 3, 2012

I may yet try out some other wordpress themes for the blog. This one does seem to have way too much white-space, and no way to include a reasonably sized header graphic. We'll see, don't be surprised if you see some odd appearance tests over the next few days or so…

Joel Rice, nitpicker,
I don't want to get into the intricacies of spinors here. In the course I'll certainly discuss the relationship between SO(3) and Spin(3)=SU(2) and their reps, but for the general case of SO(n) and Spin(n), even and odd n behave somewhat differently. In the even case there's a beautiful parallelism with the symplectic group which I want to discuss, so that's the case I'll work out in detail. If you take a look at the old lecture notes linked to, maybe you can see what I'm doing.

The Lounesto book is nice, but this course isn't aimed at physicists, so I'm not covering the Lorentz group, or anything about spin groups and spinors in the indefinite metric case, instead just sticking to the case of compact Lie groups and their finite-dim reps.

12. **Peter Woit**
January 3, 2012

Reverted to older default theme. The new one seemed to not be an improvement.

13. **Peter Woit**
January 3, 2012

plm,

The kind of automorphic representation/QFT relationship I have in mind was first discussed by Witten in his 1988 paper "Quantum field theory, Grassmanians and algebraic curves" (the QFTs in question are 2d CFTs). It's something that clearly has inspired many of the people working on geometric Langlands (but it doesn't have anything obviously to do with Witten's work in recent years on geometric Langlands, higher-dim supersymmetric QFTs, and S-duality). Before trying to write more about this on the blog though, I'd first try and like to get something detailed down on paper. It's a kind of long story, so requires much more than a blog entry…

14. **Low Math, Meekly Interacting**
January 3, 2012

I like spare more and more with every passing year. Perhaps internet-induced ADD has left me so impaired I'm incapable of resisting distractions, but there's something almost comforting now about a white space filled with nothing but clean black text.

15. **SteveB**
January 4, 2012

FWIW. The old (now reverted back to) scheme constantly pops up an IE8 warning about a macro running that may be slowing down my browsing experience. I don't notice any slow down however. The temporary large-white-space scheme did not have that. I do like having your links available on more than just the home page, which the old scheme accomodates.

16. **Peter Woit**
January 4, 2012

Thanks SteveB,

The Windows machines I have access to run IE9, and that seems to work very well with the blog. If someone knows more about the IE8 problem and whether something can be done about it, let me know.

17. **Coin**
January 4, 2012

"there was a ceremonial handing over of the chalk"

Frivolous question: Y'all still using chalkboards in New York? I thought everybody had moved to whiteboards by now.

18. **Peter Woit**
January 4, 2012

Coin,

We've moved beyond whiteboards, to a much better technology: chalk. 20 years ago someone had a whiteboard installed in our lounge. 10 years later, one of the most popular decisions of the dept. chair was to have it ripped out and replaced with a chalkboard. At this point, they'll have to pry the chalk from our cold, dead fingers….

19. **Douglas Natelson**
January 4, 2012

Peter, your comment about chalk put a smile on my face. When we moved into our new physics building down here, I specifically asked for a chalkboard in my office. A little dusty, but totally worth it.

20. **Chris W.**
January 4, 2012

SteveB and Peter,

I am using IE8, and I see the slow-running script warning only after *refreshing* the page after initially accessing the site. I also get it on the search results pages, so the problem appears to originate in a page template.

See this [Microsoft Support KnowledgeBase article] (ID# 175500), which offers an IE configuration fix (with the expected caveats).

21. **Chris W.**
    January 4, 2012

    PS: See [this detailed discussion] of the long-running script issue by web developer [Nicholas Zakas].

22. **Richard**
    January 4, 2012

    If a web site consisting of plain text and a few diagrams needs to *run scripts on the computers of every visitor who stops by to read that text*, stealing valuable computrons from unsuspecting bystanders, then something is horribly wrong. It doesn't matter if the script runs for a second or 20 milliseconds: anything more than zero indicates something is profoundly broken.

    Stop the insanity!

    PS Yes, comment previewing – a nice feature – is something that needs more than static HTML, for unfortunately historical reasons. But 99.99% of visitors aren't commenting.

23. **Peter Woit**
    January 4, 2012

    I found a machine with an older IE, and think I identified the problem. It's the WP-dTree plugin, which puts the archive listings in a nice hierarchical format. I turned off the feature that allows it to list all postings in a given month, and that seems to fix things. It looks like it was building a list of all 1000+ postings on the blog whenever the sidebar was loaded. Maybe there's someway to make it more intelligent, and only generate listings of posts when you actually click on the month. Unless that gets figured out though, for now I'll leave it as is, which just gives access to links allowing you to go to month-by-month archives, but no list of the postings in each month on the sidebar.

24. **Bernhard**
    January 5, 2012

    Off-topic. I read this article thinking would be of little interest more than a curiosity about Hawking´s birthday, but it managed to find time for hype:

    "The Large Hadron Collider at Cern, the European particle physics laboratory near Geneva, could do more than anything else to revolutionise scientists' understanding of the universe, Hawking said. The machine could find "supersymmetric" particles, which are partners of the more familiar subatomic particles. Such a finding would be "strong evidence" for M-theory, a version of string theory that describes gravity and the other forces of nature in an 11-dimensional universe."

    http://www.guardian.co.uk/science/2012/jan/04/stephen-hawking-women-complete-mystery?newsfeed=true

25. **Peter Woit**
    January 5, 2012

    Thanks Bernhard,

    I saw that, was mainly impress by Hawking's claim that the thing he spends the most time thinking about is women.

    If TeV scale SUSY would be "strong evidence" for M-theory, I guess that means no SUSY at the LHC will be strong evidence against M-theory, right?

26. **Giotis**
    January 5, 2012

    " I guess that means no SUSY at the LHC will be strong evidence against M-theory, right?"

    Peter let me answer with a metaphor. God could perform a miracle and split the red sea. If the red sea splits in front of your eyes then this of course would be strong evidence for the existence of God. If the read sea does not split in front of your eyes that would be strong evidence against the existence of God? Of course not.

    This is just a metaphor, so please don't reply that String theory is like a religion belief.

27. **Peter Woit**
    January 5, 2012

    OK Giotis,

    Bu you're the one comparing our ability to test string theory with our ability to understand the ways of God, not me…

28. **Jeff M**
    January 5, 2012

    Giotis,

    Sorry, but in a relevant version of the metaphor, it would have to be that god (or someone) told you that to prove it's existence it would part the red sea. Then, if the red sea doesn't part, it *is* evidence for the non-existence of god. You're version of the metaphor doesn't have a prediction in it, hence has no relation to the actual issue.

    Peace

29. **swh bday**
    January 5, 2012

    Hawking birthday and LHC and "hype". Once again, such comments have to be taken in context. It's a celebration of Hawking's 70th birthday, of course the press will be there, many eminent physicists (Lord Rees? Saul Permutter? What do *they* say about the multiverse/string theory?), many others. Hawking will naturally be asked about the LHC, and he can hardly say "it will be boring" or "it will find nothing of interest to me". He will naturally say something with a spin of "optimism for great discoveries". So what if it's optimism about M-theory, whatever. If anyone here wants to get carried away with "hype", that's a reflection on you, not SWH.

Wish SWH a happy birthday. He's made it to 70. How many thought he would make it to 30?

30. **Mihai Pomarlan**
January 5, 2012

Uhm, what's wrong with whiteboards? Is this another of those vynil vs. CD silly shibboleths?

31. **aa**
January 5, 2012

my whiteboard complaints:
the whiteboard in my office has stuff on there for so long
it requires scrubbing to get it off. i believe it was the correct markers,
but we didn't have the fluid and left it there and now it seems cemented on.

the markers i find are always f-ng dried out and not working

i hate the smell

32. **Jeff M**
January 5, 2012

Let me second aa on everything about whiteboards. Even as chair, I can't get blackboards put in anywhere, only whiteboards are allowed. It was explained to me that this was because blackboard dust could damage computer equipment, which strikes me as ridiculous nowadays.

33. **Mihai Pomarlan**
January 5, 2012

Yeah well. All the blackboards I've encountered in (high)school and university had chalk caked into them, I hate the chalk dust, and trying to eke out the last few lines out of a crumpet of chalk is soo-oh delightful. I guess all of these complaints go both ways.

What I didn't know is that this is an actual issue at all ("save our blackboards" vs "whiteboard power"). A more relevant issue, at least in my background (engineering) was {white/black}board vs. projector. Any course that is heavy on math should, imo, rely heavily on {white/black}board, as seeing the flow of a proof being written in front of you is much better than just taking glances at slides. Especially when the presenter, who was an otherwise decent prof, had a monotone droney voice that discouraged wakefulness. Alas, progress marches on and where I studied, most courses that were not math pure and simple succumbed to projection.

34. **Peter Shor**
January 5, 2012

There were, at some point (shortly after the introduction of whiteboards) whiteboards that actually worked—they would be erasable for years and years, and you didn't have to worry about markers drying on them and becoming permanent. We had some of those installed at Bell Labs. Unfortunately, the company that made them—I forget its name—seems to have been driven out of business long ago by companies who made cheaper whiteboards which don't work.

35. **srp**
January 6, 2012

Blackboards are great when they're completely pristine and you have a big piece of chalk. Once you start erasing, they become cloudy and messy. Whiteboards require some forethought or strong community norms to assure a supply of good markers, but they are so superior in terms of erasing (especially if you use purple) and allowing multiple colors that I have no nostalgia for the northeastern US preference for chalk.

36. **ateixeira**
January 6, 2012

" I guess that means no SUSY at the LHC will be strong evidence against M-theory, right?"

By sheer logic the answer is a resounding yes.

37. **swh bday**
January 6, 2012

" I guess that means no SUSY at the LHC will be strong evidence against M-theory, right?"

By sheer logic the answer is a resounding HB to SWH, regardless of what the blogosphere has to say about SUSY and the LHC.

38. **Attitude_Check**
January 6, 2012

A "fix" for a bad whiteboard is Pledge spray wax. Spray it on and wipe off. It will remove old marks and improve resistance to future permanence.

39. **Artie**
January 6, 2012

> srp says:
> January 6, 2012 at 4:29 am
>
> Blackboards are great when they're completely pristine and you have a big piece of chalk. Once you start erasing, they become cloudy and messy. Whiteboards require some forethought or strong community norms to assure a supply of good markers, but they are so superior in terms of erasing (especially if you use purple) and allowing multiple colors that I have no nostalgia for the northeastern US preference for chalk.

The Germans have a good solution to the first problem you mention: every lecture room has a sink in it, and instead of an eraser you bring a squeegee with you. When the board is full, you wash it, and voila! it's pristine again. It sounds absurd, but once you get used to it, it works rather well.

My major objection to whiteboards is encapsulated in your third sentence: in my experience those "strong community norms" are almost impossible to enforce. A stick of chalk never spontaneously vanishes while you're writing with it.

40. **Chris Oakley**
January 7, 2012

I cannot believe that such archaic communication devices are being promoted on a blog that purports to deal with cutting-edge physics. Surely the modern solution to the problem is having neural implants in the cerebral cortices of both the lecturer and the audience. These can communicate via an A-to-D converter and WiFi to the lecturer's smartphone which uses Kinect to report his/her gestures, gestures which are then recorded and re-broadcast by an appropriate smartphone App ("iLecture", let us call it). This scheme would have the additional advantage that one could integrate images from arXiv, or any other digital source, without further processing.

41. **Mark Bennet**
January 8, 2012

I think there is something about cognitive processing which is present when a lecturer has to make notes on a blackboard – even if they are in three different colours on three different boards just to draw attention to the three different notations – and there are lines between them which get displaced when the boards are shuffled – even so – and students have to make notes:

… which is a very different learning experience to downloading the contents of a whiteboard onto your iPad – which is a route of engagement which does not necessarily pass through the brain.

In other words there are some deep questions about the best use of technology which have not been thought through.

But once upon a time paper did eventually take over from clay tablets as the note-taking medium of choice. So technological changes are possible, even in the 21st century.

42. **Nick M.**
January 8, 2012

> **"**If TeV scale SUSY would be "strong evidence" for M-theory, I guess that means no SUSY at the LHC will be strong evidence against M-theory, right?**"**

Unfortunately, no SUSY at the LHC does not necessarily mean no SUSY:

See the recent January 6th, 2012 New Scientist article Higgs result means elegant universe is back in vogue.
Oh well, the beat just goes on.

43. **pig with lipstick**
January 9, 2012

Physicists (or HEP theorists at least, but it's really a much bigger set than that) have been searching for "elegance" and claiming "elegance" in their various theories/solutions since the birth of QM and probably much before that. So what else can one reasonably expect from a science magazine article?

Emerging Trends

January 9, 2012
Categories: Uncategorized

Most of the lectures from this year's Jerusalem Winter School in Theoretical Physics are now available online. David Gross was the main organizer, and the choice of topics reflects his point of view on what is interesting these days in theoretical physics. The landscape and anthropics were pretty much completely suppressed (although Michael Dine did manage to slip in a mention, his slides are here). The idea of "string phenomenology", i.e. getting the standard model out of a unified string theory and saying something about particle physics has fallen by the way-side.

In his summary talk (given at the beginning of the conference), Gross described the same point of view he has promoted for many years now. He thinks something is missing in our understanding of string theory ("we don't know what string theory is") which will somehow fix the failure of ideas about string theory unification. This failure though doesn't matter anyway, since he now claims that string theory = QFT, based on gauge-gravity duality, and thus "string theory cannot be killed". It's very unclear how this equivalence claim fits together with the "we don't know what string theory is" claim, since we do know what QFT is (we have a very good understanding of the Standard Model). It seems that Gross would like to define away much of the troubles surrounding string theory.

One idea that Gross has favored for a very long time is that string theory is telling us that we must give up our usual notions of space and time, only recovering them in some limit. Two of the series of lectures at the school were about rather grandiose attempts to do something along these lines. There were three lectures by Erik Verlinde on his ideas about "emergent gravity". I continue to not be able to make much sense of this program. He invokes the reasonable idea that gravity is an effective long-distance force due to unknown fundamental degrees of freedom, which may very well be true. But he doesn't seem to have anything specific to say about the fundamental degrees of freedom strong enough to get anything new out of this. There were some ideas at the end about dark matter and dark energy, but it was unclear to me whether these go anywhere.

Much more interesting was Nima Arkani-Hamed's series of lectures on *Scattering Amplitudes and the Positive Grassmannian*, which he said could have been titled "How you get spacetime from the permutation group". The first lecture started with a philosophical introduction that he said he would limit to 5 minutes, but which went on for 40. Only the first two lectures are now online.

Unlike Verlinde's ideas, here there are very specific calculations involved. The starting point is recent progress in understanding perturbative amplitudes for N=4 SYM (and for more general gauge theories). These involve working in twistor space, and more generally working with variables such that locality and unitarity are no longer manifest. The basic mathematics and geometrical principles at work are quite different than the standard formulation of gauge theories in terms of local variables and gauge symmetry. As usual, Arkani-Hamed makes wildly enthusiastic claims. In this case, he claims to have finally found a remarkable new understanding of the subject, based on some combinatorial objects and dramatic new mathematical ideas. He does note that this still hasn't been written up, and that it's the third time in the past year that he has thought he had things understood, with the last two times not working out.

I'm quite curious to see where this all goes, although I confess that I'm planning on waiting a while to try and follow the details, since this is clearly very complicated work in progress, and a 4th or 5th iteration of the fundamentals may very well be on its way over the next few months. Arkani-Hamed is talking to mathematicians, including his colleague Pierre Deligne at the IAS, and says that he is moving from the old-style of Atiyah mathematics to a new-style of Grothendieck-based mathematics. I'm not sure what this means, but suspect that Atiyah's ideas are still in there (Grassmanians, twistors and toric varieties are subjects he has been very much involved with), and that the Grothendieck business may be an artifact of talking to Deligne, who comes from that tradition. Grothendieck was the master of generality, so his ideas can be applied to a very large fraction of mathematics.

Also lecturing on amplitudes, from a much more down-to-earth point of view, was Zvi Bern. For more from both Bern and Arkani-Hamed, see the program of the recent Amplitudes 2011 conference in Michigan. Slides from Arkani-Hamed's talk give a better idea of what he is up to, and Bern's slides contain his summary comments about the state of the subject. He emphasizes the connections between amplitude calculations and other fields and seems to be arguing for more attention to practical results, less to "symmetry, beauty and aesthetics". He worries that while "Today our field is one of the hottest ones around", the long-term future is less clear: "Is our field just another (albeit long lasting) fad?", so attention to results relevant to the rest of physics is important for long-term health.

Lots more "Amplitudes" conferences on the way, including Amplitudes 2012 in March, a conference at the Newton Institute in April, and one at the IHES in December.

Comments

1. **JollyJoker**
   January 9, 2012

   "He does note that this still hasn't been written up, and that it's the third time in the past year that he has thought he had things understood, with the last two times not working out."

   I happened to check the Arxiv yesterday to see if he had published anything lately. I was wondering if he took 2011 off. Hopefully the third (or fourth) attempt at understanding the twistor approach works out.

2. **Proudmemberofthecult**
   January 9, 2012

   Of course, both Nima and Zvi are just returning to the Analytic S-matrix program… whose biggest success has always been string theory 🙂

3. **anon**
   January 10, 2012

   It's very unclear how this equivalence claim fits together with the "we don't know what string theory is" claim, since we do know what QFT is.

   There is a class of conjectured correspondences between an established theory and a conjectured theory. Is that so hard to swallow?

   Since AdS/CFT was invented, we have discovered a large class of new conformal field theories which have no known Lagrangians, but are defined solely in bulk space via a gravitational action coupled with certain matter fields. In this case, our understanding of the particular QFT is limited, but it becomes transparent when we look at it from the gravity/string side, in stark contrast to what you said above.

4. **Peter Woit**
   January 10, 2012

   anon,

I’m well aware of the conjectured set of correspondences. My point is just that Gross is engaging in a bit of sophistry here. He’s trying to avoid acknowledging the failure of unified string theories (“string theory can’t be killed”) by saying that now “string theory” is identical with a well-established, successful QFT, the Standard Model. But if you ask what’s the string theory equivalent to the SM, you find it’s one that “we don’t know what string theory is” applies to. You can legitimately make the argument that “string theory” is now a set of ideas deeply related to our ideas about QFT, but the controversial question is whether these ideas provide a fundamental theory or not, and Gross is playing word games with this question.

5. **Mimi**
January 10, 2012

This was just a winter school. It is not necesarily relevant for the development of String theory.

6. **piscator**
January 10, 2012

@anon:

it is statements like this that make me queasy about the whole applied AdS/CFT business. AdS/CFT is well defined (and well checked) for certain, mostly highly supersymmetric, examples. I do not accept that writing down (say) the Einstein-Hilbert action plus a U(1) gauge field plus a scalar field with an AdS minimum defines a CFT. We generally don’t know whether the bulk theory exists as a limit of string theory, whether there are any subtle instabilities, whether the naive rules of AdS/CFT work here, etc etc….

7. **Anonyrat**
January 11, 2012

In the last two minutes or so of the video of David Gross’ opening talk, Gross answers a question, arguing that string theory is the ultraviolet completion of quantum field theory. He says that our modern understanding of QFT is in the Wilsonian sense; and that is understood only in the infrared.

8. **Peter Woit**
January 11, 2012

Anonyrat,

This kind of argument by Gross is much trickier than he is making it sound. He won a Nobel prize for showing that non-abelian gauge theories are asymptotically free, so extremely well-behaved in the ultraviolet (it’s in the infrared that they’re hard to understand). There is no need for string theory as an ultraviolet “completion” of QCD; if it’s of any use in QCD, it would be in the the infrared.

The “ultraviolet completion of a QFT” argument only makes sense if you insist on coupling your QFT to gravity and quantizing gravity. Then you have non-renormalizability problems (unless Zvi Bern shows some version of supergravity is finite and you can use that…), which the string theory “ultraviolet completion” might solve for you. But, as Gross well knows, string theory unification opens up multiple cans of worms, ones he hopes to argue away with “we don’t know what string theory is”. This is not a rock-solid argument…

9. **pah**
January 11, 2012

Peter, just to clarify: There is no problem with QCD in the infra-red, all IR divergences cancel when you calculate a cross section or other physical observable. There is no need for help from string theory, or any other new physics, here. There are UV divergences, which are removed by renormalisation of course. We don’t worry about this, because we know that QCD is just a low-energy effective theory, and this is how low-energy effective theories behave. It is an open question as to what is really going on at high energy. This doesn’t matter for day-to-day QCD calculations, thanks to renormalisability.

I’m sure you are in agreement with this, just thought I’d spell it out.

10. **Peter Woit**
January 11, 2012

pah,

Actually I’m not in agreement at all. QCD is asymptotically free, which means that its ultraviolet behavior is perfectly under control. Put it on the lattice and you know exactly how to define the limit as the lattice spacing goes to zero. There are no problematic divergences (non-asymptotically free theories like QED are a different story). In the infrared, there’s no problem defining the theory, but it is a long-standing hope that some sort of string theory provides a useful effective description.

11. **pah**
January 12, 2012

Right, you’re saying that string theory (or whatever) might help us understand low-energy, non-perturbative QCD, which is otherwise well-nigh impossible to calculate with. And that would be great. But it would still just be the QCD Lagrangian, so nothing would have changed in that respect, it would “just” be an effective description of QCD in this domain.

At high energy the theory (QCD) actually breaks down and requires renormalisation to make any sense. The property of asymptotic safety does not change the fact that scattering amplitudes are UV divergent. Renormalisation can be thought of as taking from experiment (in the form of the measured coupling and masses at a certain scale) the parts of the calculation that your theory does not predict – basically the parts which depend on what is going on at very high energies. It is here where new physics must come in, hence all this talk of UV-completion.

12. **Peter Woit**
January 12, 2012

pah,

No. QCD is not “asymptotically safe”, it is asymptotically free. The theory does not break down at high energy, quite the opposite: it becomes more and more weakly coupled and under better and better control. Any divergences in your perturbation theory calculations are artifacts of how you do the calculation and we understand how to deal with them. You need to learn more about the renormalisation group.

Think about how lattice gauge theory calculations in QCD are done. You take the continuum limit by taking the coupling to zero as you take the lattice spacing to zero, according to the renormalisation group, and this gives a well-defined limit, with non-trivial infrared behavior, but trivial ultraviolet behavior. This is not yet rigorously mathematically proved (that’s one of the Clay million dollar problems), but all numerical and other evidence is that this is how the theory works.

13. **MathPhys**
January 12, 2012

In my humble, uneducated opinion, the line of thought of Arkani-Hamed, Cachazo, Hodges, Mason, Skinner and friends is very exciting indeed, and I expect it to be more so over the next couple of years.

Galois Conference Videos

January 9, 2012
Categories: Uncategorized

Last October there was a conference held in Paris to celebrate the 200th anniversary of the birth of Galois. Some of the talks were quite interesting, giving an overview of the current state of areas of mathematics in which Galois theory plays a role, in particular the Langlands conjectures. The videos of the talks are now available, see here.

The closing talk, by Alain Connes, has a certain amount of wild-eyed speculation about the "cosmic Galois group", of the sort that I believe has inspired Arkani-Hamed recently (see the previous posting).

Roy Lisker reports (here, here and here) about his visit to Paris to attend the conference, as well as take a look at Galois's manuscripts.

Update: It appears the Connes video is no longer there. But on his web-site you can find the slides for the conference talk, as well as another talk about Galois for a more general audience.

Comments

1. **theoreticalminimum**
   January 10, 2012

   Don't get me wrong, but we're living in 2012, and although Paris is still a major centre for mathematics research, it is intriguing to see Connes give a talk in French during what was advertised, and what indeed turned out to be, an international colloquium. I am fluent in French, but I am convinced this very interesting talk by Connes would have interested more people were it delivered in English. Just my 2 euro cents 😉

2. **fp**
   January 11, 2012

   Sadly, it seems that the closing talk has disappeared!

3. **Peter Woit**
   January 11, 2012

   fp,

   That's odd. I watched the first part of that…

   Connes has put on his web-site his slides for the conference talk and another more popular talk. The first set of slides is the ones he was using in the video. I'll add links to the posting for these.

4. **Felipe Zaldivar**
   January 11, 2012

   May I suggest, as related reading, the monograph by P. Newmann "The Mathematical Writings of Évariste Galois" (EMS, 2011) that in addition to English translations of Galois publications and manuscripts contains transcriptions of the original French articles, with commentaries and appropriate discussions of them. I just got a copy of it and I am enjoying it.

5. **Thomas**
   January 12, 2012

   @theoreticalminimum
   I don't see what 2012 has to do with that?
   I think France is in a sufficiently strong position in mathematics to allow the use of french for french speakers in an international conference. One which is, moreover, taking place in Paris. Interested people only have to read the transcript (or better, learn french ! un merveilleux langage.).
   In addition, maybe the quality of the talk would have suffered from Connes giving it in english. His ideas might flow better in french, and this is what we are really interested in.

6. **theoreticalminimum**
   January 12, 2012

   To answer Thomas:

   I meant to point out that we're living in times (2012 is the present year in the Gregorian calendar, as you are probably aware) when it would be understood that a talk in an international conference would be normally delivered in English. These days, most people learn sciences in English, and write papers in sciences in English.

   I have spent many years learning French (I even studied mathematics in French at the ENS), so I don't need you to tell me how nice a language French is. It is, in fact, my favourite. Besides, your "un merveilleux langage" is wrong; one says "une langue merveilleuse". You should probably consider fixing your French ;-]

   I have heard Connes give talks of the same topic in both English and French, and I can tell you, as far as he is concerned, the message never fails to come across. Anyway, my comment had to do with the fact that many of my mathematical friends would have liked to understand the content of the talk (which unfortunately looks to have disappeared off the webpage), but do not know French.

7. **Thomas**
   January 12, 2012

   @tm
   Let's be constructive. Apparently you know and love both maths and french, and you also appreciate Connes' work. So: you should really write down the trasncript. This would please your interested-but-non-french speaking friends much more than to pester on this blog, and it would even help them learn french.

This Week’s Rumor

January 10, 2012
Categories: Experimental HEP News

The start of the LHC 2012 physics run is still a while off, scheduled for around the beginning of April, with beam energy likely raised a bit, to 8 TeV total in the center of mass. So, it’s going to be quite a few more months before the LHC experiments have enough new data to analyze that will allow a conclusive determination of whether the evidence seen for a Higgs around 125 GeV is confirmed, with a significance high enough to claim discovery.

The results announced on December 13 were preliminary, and more complete analyses are underway, with results to be announced relatively soon. This week’s rumor is that the full CMS Higgs to gamma-gamma analysis is showing a stronger signal than the preliminary version. The bump has moved up a bit, from 123.5 GeV to 124 GeV, and the local significance is up from 2.3 to 3 sigma, with look elsewhere effect up from .8 sigma to 2.0 sigma. This strengthens a bit the evidence for a Higgs around 125 GeV. However, the best fit size of the bump is, as with ATLAS, about twice what the SM predicts. The errors are large, so quite possibly both experiments just got a bit lucky, in which case the first few months of 2012 data may not quickly add much to the significance of the signal.

For detailed discussion of issues surrounding the Higgs analyses, see this week’s workshop in Zurich: Higgs search confronts theory.

Comments

1. **Mottled Lobush**
   January 10, 2012

   You meant 8 Tev c.m. energy.

2. **Peter Woit**
   January 10, 2012

   Ooops, fixed.

3. **A Physicist**
   January 10, 2012

   It sounds bit like that in the rush to include all the Higgs channels, CMS did not do a good enough job of the important ones …

4. **Paolo Valtancoli**
   January 11, 2012

   Is it possible experimentally to distinguish between a fundamental or a composite Higgs Boson ?

5. **Peter Woit**
   January 11, 2012

   Paolo,

   Theories where the Higgs is not the quantum of an elementary field will often have limits in which it behaves like one, so, generically you should be able to tell the difference in principle, but in practice you may just be able to put bounds on some compositeness scale. If there really is something at 125 GeV, the focus for many years will be on measuring its properties to see if they’re precisely those of an SM Higgs (or a SUSY variant). Hopefully some difference will be found, otherwise life will get very boring.

6. **Trulo**
   January 11, 2012

   Although the Higgs boson is obviously the star now, and for some time to come, I guess the collaborations are still working hard on putting bounds on SUSY parameters. Are there any news on that front?

7. **Peter Woit**
   January 11, 2012

   Trulo,

   I haven’t heard anything, but the time to expect announcements is probably Moriond in early March.

   My understanding is that the negative results of the initial SUSY searches won’t be strengthened very much by doing the same analyses with more data (since the cross-sections they were looking for were strongly-interacting, so large). So, now the question is about different analyses that look for SUSY in different ways, but I don’t know much about this. We’ll probably hear from Matt Strassler more about this as results get released.

8. **harryb**
   January 13, 2012

   Although most associated with the US Presidential Election betting, as we start the year I thought I would have a look at what Intrade markets put the odds at on the Higgs boson turning up in 2012 (“discovery published in major scientific journal” original criterion, now 5 sigma, peer-reviewed etc being added to the rules).

   Currently the market gives it a 40% chance this year, down from highs of about 70% in December (volatility presumably due to all the mid Dec announcements).

   Interestingly, there is also a market on SUSY particles being discovered. It has been inactive for a year, and the starting price offered is 25% chance.

   So if our String Theory colleagues want to fill their boots I assume they will now rush off to Intrade and buy heavily into those shares….except the market so far has seen no activity at all. Hmm, seems the iron faith in ST is not translating into even one adherent parting with a single buck.

   Even unbelievers ought to dive in at this level- if nothing else as an emotional hedge against ST actually pulling off a SUSY triumph. At least we would have money in our pockets as a consolation.

Of course, alternatively Mr Woit and colleagues ought to sell heavily at 25% – still good money if we are true to our convictions…

Be worth watching these markets from time to time in 2012 in line with hype / discoveries.

9. **Proudmemberofthecult**
   January 13, 2012

   @ harryb

   or the string theorists are too busy doing science to look for internet sites where one can bet…

10. **harryb**
    January 13, 2012

    @Proudmemberofthecult

    The comment re betting was not meant to be flippant – but I take your point.

    It was interesting to note that there is a live market on Higgs outcome which is remarkable given its potential obscurity. So Higgs acceptance or expectation appears to have entered to a certain extent the mainstream – so much so people will actually bet their own money on it being found.

    SUSY particles remain esoterica, but there is a market for them which you can argue is credibility of a sort.

    Genuinely intrigued that erstwhile obscure particle physics has already attained a status where likelihood of occurence or major announcement is being subjected to mainstream economic probablistics using buyer-seller dynamics ("betting"). However, markets remain thin which makes them of limited value – for now.

    Any odds offered on a solution to the Fermi paradox after the exo-planet discoveries of this week?

11. **Jason**
    January 13, 2012

    As an economist and peruser of physics blogs I thought Higgs at 40% for 2012 was a great bet a few weeks ago when I first saw it, but decided against wagering when I realized the total number of contracts available at that price was so small that it wasn't worth my time to click (even were you certain of a published 5-sigma result in 2012 and bought all possible shares you would stand to gain about $250, and the average share price would be around 70%).

    The lesson here is that you should look at the number of contracts offered before inferring that the listed Intrade price represents the market's assessment of the probability that the event will occur.

12. **Jeff M**
    January 13, 2012

    As for the Fermi paradox, not really sure why exactly this is a paradox. Assuming 1,000,000 advanced civilizations in the Milky Way right now, and assuming that only 1/2 the Milky Way is habitable (over compensating for the black hole at the center), a seat of the pants estimate gets me that the civilizations are separated by more than 300 light years on average. Can't see that the odds are very high we would notice anything. Boy that light speed limit is annoying 🙂

    Me, I figure there really are a whole lot of planets with life out there, and a whole lot with "intelligent" life too, since we now know that planets are ubiquitous.

13. **Peter Woit**
    January 13, 2012

    I better stop this Fermi paradox thing right here. It's really, really far off-topic…

14. **Jeff M**
    January 13, 2012

    Sorry Peter, couldn't resist, promise to stay on topic in the future.

What is your favorite deep, elegant, or beautiful explanation?

January 15, 2012
Categories: Uncategorized

Science publishing impresario John Brockman's Edge web-site each year runs a "Question of the year" feature, with short pieces from a wide range of people providing their answer to the question. The past few years I've passed on their invitation to submit something, but this year the question was one that I couldn't resist. It was "What is your favorite deep, elegant, or beautiful explanation?" and you can read people's answers here.

There are quite a few answers from various physicists, with General Relativity, inflation and the multiverse getting a lot of attention. To me though, the most satisfying answer to the question involves the remarkable role of symmetry principles at the foundations of both our everyday laws of mechanics and our deepest ideas about quantum mechanics. Far more so than in classical mechanics, in quantum mechanics these principles are built into the fundamental structure of the theory. This makes it clear why quantum mechanics works the way it does, and indicates that the structure of quantum mechanics is likely to always be fundamental to our understanding of the physical world, not some approximation like the classical picture. In addition, it links together fundamental physical principles and a fundamental set of ideas that occur throughout modern mathematics, a veritable grand unification of the two subjects.

Here's what I sent in:

> Any first course in physics teaches students that the basic quantities one uses to describe a physical system include energy, momentum, angular momentum and charge. What isn't explained in such a course is the deep, elegant and beautiful reason why these are important quantities to consider, and why they satisfy conservation laws. It turns out that there's a general principle at work: for any symmetry of a physical system, you can define an associated observable quantity that comes with a conservation law:
>
> 1. The symmetry of time translation gives energy
> 2. The symmetries of spatial translation give momentum
> 3. Rotational symmetry gives angular momentum
> 4. Phase transformation symmetry gives charge
>
> In classical physics, a piece of mathematics known as Noether's theorem (named after the mathematician Emmy Noether) associates such observable quantities to symmetries. The arguments involved are non-trivial, which is why one doesn't see them in an elementary physics course. Remarkably, in quantum mechanics the analog of Noether's theorem follows immediately from the very definition of what a quantum theory is. This definition is subtle and requires some mathematical sophistication, but once one has it in hand, it is obvious that symmetries are behind the basic observables. Here's an outline of how this works, (maybe best skipped if you haven't studied linear algebra…) Quantum mechanics describes the possible states of the world by vectors, and observable quantities by operators that act on these vectors (one can explicitly write these as matrices). A transformation on the state vectors coming from a symmetry of the world has the property of "unitarity": it preserves lengths. Simple linear algebra shows that a matrix with this length-preserving property must come from exponentiating a matrix with the special property of being "self-adjoint" (the complex conjugate of the matrix is the transposed matrix). So, to any symmetry, one gets a self-adjoint operator called the "infinitesimal generator" of the symmetry and taking its exponential gives a symmetry transformation.
>
> One of the most mysterious basic aspects of quantum mechanics is that observable quantities correspond precisely to such self-adjoint operators, so these infinitesimal generators are observables. Energy is the operator that infinitesimally generates time translations (this is one way of stating Schrodinger's equation), momentum operators generate spatial translations, angular momentum operators generate rotations, and the charge operator generates phase transformations on the states.
>
> The mathematics at work here is known as "representation theory", which is a subject that shows up as a unifying principle throughout disparate area of mathematics, from geometry to number theory. This mysterious coherence between fundamental physics and mathematics is a fascinating phenomenon of great elegance and beauty, the depth of which we still have yet to sound.

Comments

1. **plm**
   January 15, 2012

   In mathematics we have a classic anthology: Proofs from the Book, by Aigner and Ziegler, http://www.springer.com/mathematics/book/978-3-642-00855-9 .

   I guess we can pick many proofs from that book.

   I may select Cantor's diagonal argument, or a similar logic or computer science reasoning, like Gödel's liar paradox proof of incompleteness, or Turing's proof of undecidability of Halting.

2. **Hendrik**
   January 15, 2012

   This is a very nice proposal, but as your post on Weinberg's symmetry views shows, it is a bit idealized. The elegance is a bit less in broken symmetries, supersymmetry, and quantum local gauge symmetry is not even mathematically rigorous yet. There is also plenty of confusion in physics literature on what precisely constitutes a symmetry e.g. what is the difference between physical transformations and symmetries, must symmetries be automorphisms of the observables, or is it enough to be just derivations, and what about superderivations?

3. **Jeff M**
   January 15, 2012

   Well, as a geometric analyst, I should say the proof of the Selberg trace formula, which relates the lengths of closed geodesics, a geometric quantity, to the eigenvalues of the Laplacian, an analytic quantity. Not like there is any easy way to explain the proof though. Another would be the Hodge decomposition. But for pure elegance I'm going with plm, Turing's proof of Goedel's theorem is truly beautiful and shockingly clever.

4. **Peter Woit**
   January 15, 2012

   Hendrik,

   I think we're far from completely understanding many things about representation theory, as well as far from completely understanding fundamental physics. All that's clear is that the two subjects are closely related. I hope that in the future we'll understand this relationship better, allowing new ideas to flow both ways between math and physics.

5. **AcademicLurker**
   January 15, 2012

   Interesting coincidence with the timing of this post. I just started a book, "Emmy Noether's Wonderful Theorem" (Dwight Neuenschwander, 2011). It's not a popularization. Instead, it works through the proof and then explores some of it's consequences. It seems to be pitched at the level of an undergraduate physics major.

6. **JB**
   January 15, 2012

   Peter, can you recommend a good textbook on Representation Theory?

7. **Sascha Vongehr**
   January 15, 2012

   Symmetry is of course the underlying elegant reason for pretty much anything, say the tautological truth of many world modal descriptions (which you seem to actually not like). To reduce it almost mechanistically to something that is indeed more "representation" rather than explanation, is not so good. Especially not if it involves time/energy (as that very symmetry is not given in a general relativistic context) and phase transition/charges. A fundamental explanation kind of symmetry be better fundamental, and not these apparent and emergent symmetries you are talking about. With those, a mathematical modeling/representation/mapping into another language is not an explanation.

8. **King Ray**
   January 15, 2012

   Peter, you mean U = exp(iH), not exp(H), where H is self-adjoint aka Hermitian. iH is skew-adjoint.

9. **Peter Woit**
   January 15, 2012

   King Ray,

   I was trying to be concise and avoid equations, so at the level of precision of that text, you should take h-bar=i=-1=1.

10. **Bob Levine**
    January 16, 2012

    For my money, the prize has to go to Maxwell's incorporation in the expression for the magnetic field circulation of a term expressing the time rate of change in the electrical field flux, purely on the basis of mathematical parallelism with the relationship between the electrical field circulation and the time derivative of the magnetic field flux. There was absolutely no 'phenomenological' motivation for this modification in the original Ampere law, not one bit of experimental evidence for a 'displacement current' at the time; it was a matter of pure formal symmetry, yielding the wave equation for the propagation of orthogonal E and B fields that established the wave nature of light on the basis of the coefficient 1/c^2 in a wave equation that could *only* have been derived on the basis of that modification.

    We're used to this kind of thinking in the era of modern physics. But what Maxwell did was, so far as I can tell, virtually unprecedented in the context of 19th century physics—-and probably the greatest breakthrough in physics since Newton's original work. The key point is that Maxwell was guided purely on the basis of the principle that, as Dirac was famous for saying, 'it is more important to have beauty in one's equations that to have them fit experiment'—but followed that principle just short of a century before Dirac wrote those words. In context, his innovation ranks with the very best that Newton and Einstein achieved.

11. **Abbyyorker**
    January 16, 2012

    In the answers I liked:

    Natural selection
    Boltzmann explaining entropy
    Geometrization of the quantum

12. **Proudmemberofthecult**
    January 16, 2012

    @ Peter Woit

    I've heard that Seiberg refers to this as "string theory units" 🙂

    (for computational simplicity include 2=\pi=1)

13. **Frank Meulenaar**
    January 16, 2012

    Why call it "mysterious'? It might be 'deep' or 'unexpected' but I would never call it 'mysterious'.

14. **Chris Oakley**
    January 16, 2012

    Bob,

    I do not agree that Maxwell's displacement current was just plucked from thin air. It is, after all, the simplest extension of Ampere's law that makes the equation consistent with charge/current conservation.

15. **plm**
    January 16, 2012

    Peter, I find it interesting that you chose to underline how fundamental physical theories are selected by stability/symmetry among possible theories, while you are very critical of string theory.

It seems to me that the main argument in favor of string theory is that it adds degrees of freedom to quantum field theories (or so it seemed), which are then pruned by stability/symmetry conditions (conformal invariance) with mysterious (more or less attractive) consequences. Like for instance special relativity takes a point (or field or wave-function) in 4-space and asks for hamiltonians invariant under the transformations you mention.

I think this is also what makes string theory a little more attractive than canonically quantizing general relativity. (This may also suggest that some form of canonical GR may be recovered by either a generalization of string theory or even some existing formulation.)

So we have failed alot to find experimental evidence in favor of string theory, but it is mysterious and embeds this symmetry principle workhorse of theoretical physicists that you promote…
Then how do we weigh those things, what is the right bayesian framework? How does this mix up with the social structure of scholarship -e.g. personal interests?
I try to understand your attitude, and others'.

And thank you for the post, it was very interesting.

16. **plm**
January 16, 2012

I should add that quantizing GR is also based on the symmetry workhorse insofar as quantization is. We add probabilistic fuzziness to our objects and reduce by requiring (biological) observers. This results in projections onto preferred (superselected) states, like eigenstates of conserved observables, or the next best thing, coherent states. And decoherence is a mysterious mathematical ingredient, which affects how much we need to project.

(My perspective on the interpretation of quantum mechanics has been that experiment supports quantum physics and classical biology so I start with quantum theories for the quantum experiment and try mentally to model a simple quantum neural network coupled with the observed system, and then I ask that what happens is an observation, a neuron either firing or not, which technically can only be made projecting on some subspace of states, and this with some mathematical unraveling, explaining decoherence mostly, should be equivalent to projecting on just eigenspaces. So I usually tell myself "Why should we humans be all of physical reality and not just part of it, projections of states?". And I think a bayesian view of physics supports this, our experiments give ever-increasing support to QM and we just continually observe consciousness which we can formalize in models of neural networks -from single neurons, to perceptrons with feedback, to realistic full brain models as begin to appear- and which boils down to Born selection on complete sets of observables.)

With this in mind canonical quantum general relativity may be as attractive as string theory on the bayesian grounds I mention. I don't know.

17. **Bob Levine**
January 16, 2012

@ Chris—

I didn't mean to imply (and don't think my comment should be read as saying) that Maxwell's extension was 'plucked from thin air' (at least as I think of what that expression suggests) or had no physical basis. But, at least as I've read the history, his main concern was to make the E and B field equations as close to mirror images as possible in the context of the evident lack of magnetic monopoles corresponding to free electrical charges. Proceeding in that fashion, without substantial experimental support, was a completely different style of physics from standard practice and, I think one could argue, anticipates a strikingly contemporary approach—much closer to Dirac's approach to nature than Faraday's.

18. **Noah Smith**
January 16, 2012

Thanks for this Peter, it seems to summarize some of the most interesting ideas in your book. Years after reading it, these ideas are what I remember – interesting no? This may be a good opportunity to try asking a question, if it's possible to pose it properly. This is a sincere question and I'm genuinely interested in your thoughts. How about this: do you see a route by which these ideas will spread, so that they appear in undergrad physics and engineering courses? One example I think of is Lagrangian and Hamiltonian dynamics. In aerospace, it's interesting to see where and how they appear in very practical applications. Thanks.

19. **Joel Rice**
January 16, 2012

From a historical perspective, there was a big bang in physics and mathematics when Hamilton discovered quaternion algebra. I read somewhere that Maxwell did his thinking in quaternions, and later translated this into matrices. And where would Einstein have gone without Maxwell ? Pascual Jordan mentioned quaternions in an early paper that led to Pauli dealing with spin, without which atoms make no sense at all. All in all, it seems that a lot rests on Hamilton's discovery.

20. **Peter Woit**
January 16, 2012

JB,

I've listed some textbooks on the web-page for my course, and will be adding some more material there soon.
http://www.math.columbia.edu/~woit/LieGroups-2012/

JB and Noah,

I'm hoping to at some point teach an undergraduate course on this material, and would write something up then. Unfortunately I don't know of a good source now at this level (I'm thinking of "Hermann Weyl for undergrads"). There's a long history since the early days of QM of interaction between the physics and the math of representation theory. In practice though, most physicists end up just learning some very special cases that they need to do certain calculations, and nothing about the general story. I'm not sure what will ever change this, maybe if I end up writing something like a book about this it would have a small effect.

plm,

My argument is about representation theory, which does have to do with symmetry, not with stability. Discussing the various ways in which symmetry shows up in string theory is a huge topic. The bottom line though is that symmetry arguments get used everywhere in physics, but just because your idea uses a symmetry argument doesn't mean that the idea does what you want it to.

All,

Please try and stick to some semblance of the topic. This one does lend itself to people deciding to write here about whatever they want, but, as usual, I can't moderate a general physics discussion forum.

21. **Tim May**
January 16, 2012

Jumping in late, I'd say the "constancy of the speed of light" has been crucial to our understanding of how things work. General relativity and quantized gravity (and the various approaches, some wrong, some not even wrong (!), maybe one of them right?) get the attention in the comments in the Edge comments, but the combination of the constancy of c and quantum mechanics in the 1905-1930 period really had profound implications.

Dirac's effective predictions of anti-particles, based on light cone reasoning, must've been astounding to those at the time.

And at another level, the whole view of causality and light cones that comes from the constancy of c has implications about simultaneity, the impossibility of omniscience (in our space-time), and the inability to ever, ever, communicate with distant objects in any meaningful sense.

22. **harryb**
January 16, 2012

Peter

First – excellent piece you started, and fascinating (192) entries I trawled – quickly – through. @Abbyyorker has a good point re Evolution, and Boltzmann's entropy – I always think the Second Law of Thermodynamics ought to win these fests just because of its name.

Still – the entries on elegance and symmetry not being true or useful also rang home. Jamshed Bharucha's piece – The Beauty and Tragedy in the Mathematics of Music – had some interesting insights on the asymmetry or irrationality of the ratio of music (on strings, ho ho)

One insight – the elegant ratios are "Close but unequal. If you tune by octaves, the fifths are out-of-tune, and vice versa."

So I worry about the equation : symmetry = beauty, elegance, depth.

I see depth is the fact mathematics underpins so much of what we know.

The deep theory of evolution, for example, in Dawkin's entry, is offered in mathematical language:

"The ratio of the huge amount that it explains (everything about life: its complexity, diversity and illusion of crafted design) divided by the little that it needs to postulate (non-random survival of randomly varying genes through geological time) is gigantic."

I guess I am noting that asymmetry may have an elegance of its own, and a reality too. To exclude asymmetry from deep truths may be to fall into the dead space we talk a lot of in these blogs re strings and so on.

Daniel Kahneman (nobel laureate in Economics) discusses "theory-induced blindness" and over-adherence to "inside views" as ways in which groups stunt their progress. He also famously crafted an elegant theory on "loss-aversion" in economics which showed as humans we have an asymmetric view between losses and gains (reacting to losses typically twice as much as equivalent gains).

Its elegant, and asymmetric.

Close but unequal.

So, excellent blog forcing me to look at alternative deep thoughts out there – truly insightful.

23. **Chris W.**
January 16, 2012

1. The symmetry of time translation gives energy*
2. The symmetries of spatial translation gives momentum*
3. Rotational symmetry gives angular momentum*
4. **Phase transformation symmetry gives charge***

[* … conservation]

Whereas #1 – #3 can be approached in a strictly classical way, #4 has always struck me as the most interesting and mysterious. Why should quantum mechanical phase, a notion which is not bound prima facie to any assumptions about the *forces* affecting the evolution (propagation) of particle probability amplitudes, have anything to do with electric charge or charge conservation? The way the connection is made—through the global gauge invariance of the electromagnetic field—is clear enough, but the outcome is still strange; from early on the electromagnetic field seemed to have a particularly intimate connection to the structure of quantum mechanics. One feels there must be more to this story….

24. **Mike J.**
January 16, 2012

I am a non-physic under-graduate. Can you introduce a book/paper/article etc. that describes these symmetry in a more understandable way. Thanks.

25. **Peter Woit**
January 16, 2012

Mike J.,

My book "Not Even Wrong" has a chapter on this. But, honestly, if you haven't studied physics a bit seriously, these ideas aren't going to make much sense.

26. **Thomas Larsson**
January 16, 2012

Conformal field theory – using the representation theory of the Virasoro algebra to explain the spectrum of critical exponents in 2D. QED is often hailed as the most successful theory, because it predicts a handful of quantities to ten decimal places or so. CFT correctly predicts infinitely many quantities to infinite precision. 2D phase transitions is admittedly not a an extremely important field, but within its limited domain of validity, CFT is the ultimate theory.

27. **Geoff**
January 17, 2012

I'm profoundly biased in this regard as my first and foremost love has been and will always be general relativity. It is a never ending source of fascination that when translated literally the field equations simply say that physics is geometry and geometry is physics. I find it incredible that a mathematical discipline originally tasked with drawing maps of curved surfaces on flat pieces of paper has been so successful in describing the universe at large. Of all the theorems that Gauss proved, the one that he named Theorema Egregium turns out to be critical to the whole theory. (A beautiful article by Kuchar explaining some of this can be found here –)

http://arxiv.org/pdf/gr-qc/9304012

In a completely different field I always enjoyed listening to the recordings of Sidney Coleman where he would repeatedly and passionately say that if the PCT Theorem was wrong he'd have to go back to day one and start over. I must confess I haven't appreciated that fact in any meaningful way just yet.

Peter, as a suggestion perhaps you could give us some of the history behind the development of Noether's Theorem. It's my limited understanding that it came up in the context of General Relativity, specifically trying to understand gravitational radiation.

28. **Per**
January 17, 2012

Thomas Larsson:

Wouldn't you say that's a consequence of the symmetry argument? The reason CFT is so successful is of course because of its symmetries (which survives quantization).

29. **Fabien Besnard**
January 17, 2012

I quickly browsed the list of answers, and not surprisingly, the atomic theory (in one form or another), natural selection, Maxwell's theory of em field, and GR come several times. It's an opportunity to remember how much we owe to nineteenth century science regarding the explanations of natural phenomena.

I'm a bit surprised though that Kaluza-Klein theory does not appear on the list. To me that's still the best idea one ever had about unification. It is also at the root of the set of ideas that eventually led to gauge theories.

30. **Chris W.**
January 17, 2012

Following up on Fabien's comment, see arXiv:gr-qc/0012054 ("On Pauli's invention of non-abelian Kaluza-Klein Theory in 1953"), which seems to be widely cited.

I seem to recall reading somewhere that Pauli offered some rather strong criticisms of the Kaluza-Klein approach over a decade before the work described in this paper. I didn't find exactly what I had read previously, but I did find this paper [PDF, at cdsweb.cern.ch], containing excerpts from a correspondence between Einstein and Pauli in the late 1930s. Pauli and Einstein came to agree on the futility of the Kaluza-Klein approach in finding a classical basis for quantum discreteness. Pauli's 1953 work, and most subsequent work inspired by the Kaluza-Klein approach, had a rather different motivation.

31. **D R Lunsford**
January 17, 2012

Chris W,

Pauli began his career in the early 20s working with Veblen – or at least contemporaneously with him – on something that was known as "projective relativity", based on the "projective geometry of paths", where the geodesics in a Riemann space are given a sort of projective structure, and one works in a sort of system of homogeneous coordinates. He later showed that Kaluza's ansatz was just PR in a particular coordinate system. Since he had abandoned PR some time earlier, this reduced the Kaluza ansatz to the realm of the non-dynamical, and really, invalidated it as a road to unification. He also pointed out, in addition to this weighty argument, that the particular form of the action in Kaluza's work was arbitrary. These arguments, as far as I can tell, have never been superseded by more subtle ones.

-drl

32. **D R Lunsford**
January 17, 2012

Fabien B,

Kaluza theory and gauge theory could not possibly have had any more different origin. Kaluza was non-dynamical and was based on Riemannian geometry with a restricted (cylinder) geometry. Gauge theory emerged from Weyl's gauge invariant extension of Riemannian geometry, in which calibration changes enter alongside coordinate changes as logically independent operations. These two things have really nothing in common. In particular, the Weyl ansatz is completely local and needs no global assumptions, such as the cylinder ansatz.

-drl

33. **Thomas Larsson**
January 17, 2012

Per: Of course CFT is all about symmetry, but I would not say that the symmetries survive quantization, for two reasons.

1) The quantum theory (Hilbert space, correlation functions) is constructed directly, without classical crutches. So there is no quantization.

2) If there were a classical theory, it would presumably have zero central charge. So the classical form of the symmetry has not survived.

I like both of these aspects.

34. **Chris W.**
January 18, 2012

DRL,
Much thanks for that. On reflection, I think I was recalling a comment by you posted a long time ago on another N.E.W. post. (I tried locating it just now, without success.)

35. **jorge**
January 19, 2012

"…string theory is the first science in hundreds of years to be pursued in pre-Baconian fashion…"

Philip W. Anderson
Edge 2005: WHAT DO YOU BELIEVE IS TRUE EVEN THOUGH YOU CANNOT PROVE IT?

36. **Jess Riedel**
January 19, 2012

> One of the most mysterious basic aspects of quantum mechanics is that observable quantities correspond precisely to such self-adjoint operators, so these infinitesimal generators are observables.

One of the reasons I decided to come work for Wojciech Zurek is because he more-or-less solved this mystery. More precisely, he showed [PRA, ArXiv] that what is often taken to be a very mysterious postulate

> Observables are represented by Hermitian operators

can be derived from a much, much more natural postulate

> Outcomes are identified with vector components of the global wavefunction

along with the natural definition that an outcome must be amplified to count as a measurement. By the linearity of quantum mechanics, only orthogonal vectors can be amplified. If we label each vector with a real number, we are left with a Hermitian operator being the natural mathematical object to identify with an observation, i.e. the catalog of all possible measurement outcomes.

(Actually, from this we can see that it is better to identify observables with the *normal* operators, which include Hermitian operators as a subset. Indeed, the sensible operator when we measure the amplitude and phase of an electromagnetic wave is normal but not Hermitian.)

Incidentally, this argument would undoubtably be *my* choice for most elegant explanation in physics.

37. **Fabien Besnard**
January 19, 2012

D R L, of course you are right to stress the importance of Weyl's idea. Nonetheless dimensional reduction did play a role in this story, in particular in the works of Klein himself and also Pauli. You can consult O'Raifeartaigh's book "the dawning of gauge theory" about this, notably chapters 3, 6 and 7.

38. **harryb**
January 19, 2012

A good try, but no cigar?

http://imgs.xkcd.com/comics/string_theory.png

Picked up from Lawrence Krauss's latest work – A Universe from Nothing – interested in what you think of the book.

39. **Alberto**
January 20, 2012

As a first year student in physics in the seventies, I discovered the relation of symmetry and conservation laws in the first pages of the first volume of Laudau's physics course. An instructor told me to look for it (in those times, in Europe, the books of the famous Soviet physicists and mathematicians circulated widely among penniless students, in the translations into French printed by the state publisher MIR). I thought this was the closest one could get to seeing the hand of god in action.

40. **D R Lunsford**
January 20, 2012

There are two amazing things that come from the same person, Felix Klein, and I can't imagine their strange mystery or depth ever being exceeded.

One was the association of Platonic solids with the solution of equations of the fifth degree.

The other was the mathematical theory of the top, in which the best treatment necessitated the introduction of a 4d space of indefinite metric. Klein however pointed out that the required non-Euclidean geometry should not be taken seriously (!)

-drl

41. **Peter Woit**
January 21, 2012

harryb,

That drawing pretty much does sum it up, and it has become very famous.

I confess that the cosmological "why did something come from nothing" question never interested me at all (for one thing, the big bang singularity doesn't seem like "nothing" to me), and the arguments about religion really don't interest me either. So, I think I'm going to pass on reading that book carefully or writing about it.

42. **harryb**
January 22, 2012

Peter

Understood – but just to be clear Krauss's works including this latest one are very much negative on any religious, quasi-religious or even anthropic arguments (with a – supportive – Afterword by Richard Dawkins you can see where he is coming from.)

He is negative on string theory too as you can imagine from the cartoon for its lack of scientific rigour or evidence – a theory of anything, which is not science.

His recent biography on Feynman – Quantum Man – may be a better option.

43. **jorge**
January 22, 2012

Why do we need to postulate an infinite number of universes with all sorts of different properties just to explain our one?

Paul Steinhardt
Edge 2005: What do you belive is true even though you cannot prove it?

44. **chiz**
January 28, 2012

Geoff – Noether's original paper is here and it includes some discussion of the non-conservation of energy in GR.

Mathematical Foundations of Quantum Field Theory

January 17, 2012
Categories: Uncategorized

The Simons Center for Geometry and Physics at Stony Brook is having a workshop this week on Mathematical Foundations of Quantum Field Theory. I was hoping to find time to go out there and hear some of the talks, but the beginning of classes has kept me here today and tomorrow, and later in the week I need to make a short trip to Toronto. But luckily there are high-quality videos, and today Witten gave an interesting talk on What one can hope to prove about three-dimensional gauge theory. What struck me most though was how little we still know about even simple questions about 3 and 4 dimensional gauge theory. Witten expressed hope that studying these questions is something that will get re-invigorated and draw new attention. I hope he's right. Also worth watching is Arthur Jaffe's summary of the history and state of the art of constructive field theory.

Also in the category of talks that I'd love to hear, but they're a bit too far afield to get to this week are Jacob Lurie's Whittemore Lectures at Yale, dealing with the Siegel Mass Formula, Tamagawa numbers and Non-abelian duality. I fear they don't have video, but if anyone knows of a source of information dealing with what Lurie talked about, I'd love to hear about it.

Comments

1. **David Roberts**
   January 18, 2012

   Peter,

   Do you have a link/source/description/abstract for Lurie's lectures?

2. **Igor Khavkine**
   January 18, 2012

   I hope that video recording was not turned on only for Witten and Jaffe's sake and that videos for Monday's (and other day's) talks will appear eventually.

3. **Peter Woit**
   January 18, 2012

   David,

   There are some abstracts for the talks on the Yale math dept. calendar listings at

   http://math.yale.edu/mathematics-calendar

4. **Tim van Beek**
   January 18, 2012

   > I hope that video recording was not turned on only for Witten and Jaffe's sake and that videos for Monday's (and other day's) talks will appear eventually.

   Dito, hopefully the talk of Planck medaillist Detlev Buchholz will be put online, too. Maybe some of the AQFT specialists have something new to say about the connection of quantized gauge theories and Haag-Kastler nets.

5. **Marcus**
   January 18, 2012

   I hope that video recording was not turned on only for Witten and Jaffe's sake and that videos for Monday's (and other day's) talks will appear eventually.
   Igor,
   The site says Monday's talks were not recorded (because MLK holiday).
   But all Tuesday's are shown as recorded and the rest will eventually be available from the looks of it.

6. **Igor Khavkine**
   January 18, 2012

   Marcus, thanks for pointing that out. Still, that's an unfortunate, though clearly accidental, anti-AQFT bias.

An Introduction to Group Therapy for Particle Physics

January 24, 2012
Categories: Uncategorized

The latest CERN Courier book review section is out here. Besides a long review of Frank Close's The Infinity Puzzle, there are some short reviews, including one for Stephen Heywood's *Symmetries and Conservation Laws in Particle Physics: An Introduction to Group Therapy for Particle Physics*. That's one I really want to see: I'm all for symmetries and conservation laws (see here), and *Group Therapy for Particle Physics* (at least for particle theorists) seems like an excellent idea.

This semester I'm not doing Group Therapy, but I am teaching group theory and representation theory. The class has started and I'm trying to write up lecture notes. One discouraging/encouraging thing is that looking around the web one finds several places other people have done this better, links are slowly getting added on the class web-page. The course is mainly aimed at mathematicians, hoping to provide our graduate students the background they need for several different areas, including number theory. It will however have a physics flavor, with more concentration on topics like spinors, geometric quantization, the Heisenberg algebra and oscillator representation than usual. The Dirac operator may even put in an appearance, we'll see…

Update: Turns out there are more books on group therapy in particle physics. See here for J.F. Cornwell's *Group Therapy in Physics, Vol. 1*. John Gribbin's promotional *In search of superstrings* includes an appropriate appendix on Group Therapy for Beginners. Then there's Terry Tomboulis's Renormalization Group Therapy, which is something different.

Comments

1. **theoreticalminimum**
   January 24, 2012

   Pierre Ramond's "Group Theory: A Physicist's Survey" is probably a book you'd like.

2. **jpd**
   January 24, 2012

   live by word completion, die by word competition

3. **Mithras**
   January 24, 2012

   I picked up the Infinity Puzzle on your recommendation; I suppose I'll now have to pick up Heywood's book as well.

4. **AS**
   January 24, 2012

   What's the best opposite, say "Particle Physics for people who already know about groups and representation theory, and aren't afraid of functors" ?

5. **?**
   January 24, 2012

   Isn't hosting a link to an illegal download going to get you in trouble with Columbia? The link is definitely illegal.

6. **Barry Adams**
   January 24, 2012

   I liked S. Sternberg's, Group Theory and Physics, was on my reading list as an undergrad and its one I often return to.

7. **SteveB**
   January 24, 2012

   This is quite funny. But now I have to point out your typo in the Update. A new particle the: Renormaliz-a-ton, which is akin to the instan-ton or par-ton, I suppose. Sorry…couldn't resist.

8. **Peter Woit**
   January 24, 2012

   AS,

   I don't know a good answer for that, curious if anyone else does. I've always intended someday to try and write something like that, basically an explicit mathematical description of the standard model, aimed at mathematicians completely familiar with the technology of differential geometry.

   Not particle physics, but some of what I'll be doing in my class and hopefully writing notes about is basic quantum mechanics, expressed in the language of representation theory. This won't get really to particle physics, where the interesting groups are infinite-dimensional.

9. **Peter Woit**
   January 24, 2012

   SteveB,

   Thanks! Fixed. Typos are hard to avoid…

   ?
   The illegal download links for that turned up first, I didn't realize that the legal sites had the same title. I'm removing that link, putting in a legitimate one.

10. **Tim van Beek**
    January 25, 2012

AS asked:

> What's the best opposite, say "Particle Physics for people who already know about groups and representation theory, and aren't afraid of functors" ?

The slightly more general question [Quantum Field Theory from a mathematical point of view] has been discussed both on the theoretical physics stackexchange site (see link) and on mathoverflow (the link to mathoverflow is in the second comment under the question).

Peter wrote:

> I've always intended someday to try and write something like that, basically an explicit mathematical description of the standard model, aimed at mathematicians completely familiar with the technology of differential geometry.

Don't underestimate the amount of physics one needs to understand (Hamiltonian mechanics, classical electromagnetism, quantum mechanics, particle physics).

IMHO there is no easy way for mathematicians to understand the standard model.
But I would recommend to every mathematician to read the classic "Spin, Statistics and all that" (Wightman/Streater) in order to understand what a *QFT is, actually*, before trying to understand Lagrangian QFT heuristics.

(And next Schottenloher: "A mathematical introduction to conformal field theory").

11. **Chris W.**
January 25, 2012

I remember my first day in an undergraduate course in group theory and quantum mechanics many years ago, taught by a condensed matter physicist. A few minutes into the class, one of the students stood up (while looking quite embarrassed), collected her stuff, and quickly left the room.

The student never came back, so I assumed at the time that she had initially misunderstood the course's subject matter, and dropped it when she realized her mistake. My guess was that the term "group theory" had implied something else to her that she was more familiar with, but I never got a chance to confirm that. 🙂

12. **Peter Woit**
January 25, 2012

Tim,

Thanks for the reference. Looking at the mathoverflow version

http://mathoverflow.net/questions/57656/standard-model-of-particle-physics-for-mathematicians

I realize that I sent in a response there which is relevant.

I don't think giving a mathematician a fairly precise definition of the Standard Model in a high-level mathematical language is going to cause them to "understand" it, since there are all sorts of ideas implicitly packaged in such a definition that you really need more basic training in physics to understand. However, it might give them an entry-point to starting to learn more about the subject.

Streater and Wightman style axiomatic QFT is quite problematic for this purpose. It is very much dependent on exploiting Minkowski space symmetry, and it really can't handle gauge symmetry. There's also no path integral there. If you want to try and start understanding the SM from a geometric, coordinate-invariant perspective, the path integral is a much better place to start.

13. **cormac**
January 25, 2012

Hi Peter, Dad has a book on group theory for particle physicists called 'The Group Structure of Gauge Theories' (OUP) . It gets a lot of respect though I gather it's at quite an advanced level

14. **Andrzej Derdzinski**
January 25, 2012

"Geometry of the standard model of elementary particles" is a book I wrote over 20 years ago. It deals with the classical level only.

http://books.google.com/books/about/Geometry_of_the_standard_model_of_elemen.html?id=uNTvAAAAMAAJ

15. **Thingumbobesquire**
January 28, 2012

It would seem that spellchecker is sometimes too much of a good thing. Once I discovered a reply on a political blog from a self proclaimed English professor that used the phrase "out of sink." (sigh)

16. **Belastingvrije auto's**
January 28, 2012

Hi, J.F. Cornwell's Group Therapy in Physics, Vol. 1 is essential imo to read for anyone interested in group theryapy & theory in physics. Although you probably have to read it more than once. 🙂

17. **Chirag**
January 29, 2012

Does anyone have any recommendations for an elementary discussion of phase transitions and group theory? From what I understand Morse theory is also important. I would be interested in knowing if there are any elementary survey papers on this circle of ideas.

18. **Jeff M**
January 29, 2012

@Chirag,

While I have no idea what the physics reference would be for phase transitions and group theory, for Morse Theory the great reference is “Morse Theory” by John Milnor. Truly beautiful book, really thin and you will not only learn Morse theory you will get a wonderful writeup of everything you need to know about differential geometry.

19. **former mathematician**
    January 30, 2012

    Fraser’s review omits any reference to Close’s discussion of Bjorken’s work. Strange, since Close’s table-pounding about Bjorken’s contribution was the single strongest impression I got from the book.

    The only other book by Close I have read is “Neutrino,” which has similar table-pounding for Bahcall. It seems that non-recognition of theoreticians in experimental Nobels is a hobbyhorse of his.

20. **Jon**
    January 30, 2012

    @Jeff M@Chirag

    There are online copies of Milnor’s Morse Theory available here and here. Not sure if they’re completely legit, but they’re hosted on university servers.

Short Items

January 31, 2012
Categories: Uncategorized

A few short items:

- No Higgs news on the LHC front, but on the BSM front today's CERN talk Update on Searches for New Physics in CMS provides more evidence against the various exotic scenarios heavily advertised over the last twenty years. No sign of extra dimensions, SUSY, or other exotics. For more, see Resonaances. SUSY proponents now seem to be somewhere between the first stage of grief ("denial") and the second ("anger", see e.g. here) and on their way to the third (Bargaining – "I'll do anything for a few more years").
- From Sean Carroll's Twitter feed I learned about this long description of a recent workshop bringing together philosophers and quantum field theorists. I guess my take on the "heuristic" vs. "mathematical" quantum field theory debate is that we need both, since there is still a huge amount we fundamentally don't understand about QFT.
- For a finite number of degrees of freedom, quantum mechanics itself is, unlike QFT, rather well understood. For a nice recent review of some topics in the mathematics of quantization, see Ivan Todorov's Quantization is a mystery.
- It's not only string theorists producing over-hyped university press releases, there's things like this. See more discussion here.

Comments

1. **Lee**
   January 31, 2012

   After browsing Todorov's text, I was reminded how such short shrift is given to the probabilistic and statistical nature of quantum theory. The virtues of group theory, symplectic spaces are extolled by virtually every mathematical physicist, yet, quantum theory is equally an exercise in probability theory. Looking back on grad school, I am surprised how little probability and statistics I was taught.

2. **Enrico**
   January 31, 2012

   Hey Peter, when are you going to review Lawrence Krauss's new popular science book?

3. **Peter Woit**
   January 31, 2012

   Enrico,
   See
   http://www.math.columbia.edu/~woit/wordpress/?p=4385#comment-103275

4. **Shantanu**
   January 31, 2012

   Peter, there continue to be talks/hype on extra dimensions.
   See
   http://pirsa.org/12010130/
   by C. Burgess

5. **Peter Woit**
   January 31, 2012

   Shantanu,

   I suspect that many if not most of the people who have been promoting this kind of thing their entire professional lives will stay stuck in the denial phase forever. The attempt by Burgess (and others) to claim the LHC not seeing anything as a success for their theory is pretty amusing.

   My TOE is that everything is turtles, very small ones, of size about (100 TeV)^-1. So, not visible at the LHC, thus the latest data there is a huge success for my theory.

6. **Low Math, Meekly Interacting**
   January 31, 2012

   When I read the abstract, I thought that Dr. Andrulis' theory could very well be a hoax. Then I scanned the manuscript, and am convinced that my first impression was wrong. Nobody would go to that much trouble to be a joker, no matter how deranged. Perusing his other publications, it's all quite solid and uncranky. I don't know what to make of it.

7. **Low Math, Meekly Interacting**
   January 31, 2012

   OK, having read a bit more, I do know what to make of it: the guy is nuts. Certifiable. It's monomaniacal crankery of the first water. No matter how coked up the guy might have been to hasten production, this thing must have taken many months of painstaking research, however bizarre the inferences he drew from his hundreds of references. He practically invented a new language just to convey the details of this…I don't even know what to call it; "theory" just isn't up to the task. It looks to be the mother of all crackpot opera, and somewhat disturbing to read.

8. **Avattoir**
   February 1, 2012

   low math, etc. – Could be a crying need for a journal for TOEs short on T, where O = 0, long on E, with peer-review standards fixed by xkcd; call it Doodles Review.

9. **Cesar Laia**
   February 1, 2012

   Andrulis' theory is not a theory, it is a completely useless language that tries to put everything under some kind of common ground. I have to say it: it is weird beyond imagination.

10. **harryb**
February 1, 2012

Interesting stuff . Add Gyres to Circlons – Margaret Wertheim would be proud of this stuff / guff.

Is this a harbinger of non-science becoming more difficult to combat due to a rapid veneer of internet credibility? I don't think so. Hyped for a moment, blogs like this refute it pretty quickly.

The pace of theory generation may have increased – but the testability / prediction filter now seems even more precious.

The comments on the CMS show also how String Ideology seems more and more a zombie theory – still going, unkillable, yet lifeless.

11. **harryb**
February 1, 2012

@Enrico

I did indeed suggest this book as Peter points out. I am afraid the invocation of multiverses is used quite a bit, and though I think Lawrence Krauss a charismatic author, this preference for multiverses continues to vex me. Its a giant hand-waving answer when more grounded unresolved basics such as quantum pressure seem to have much more life in them.

12. **Nick M.**
February 1, 2012

To quote from the article on the ARS Technica website that Peter has a link to above:

> **"**He's serious about the "not limited to" part; one of the sections describes how gyres could cause the Moon to form.**"**

So perhaps we will finally have an answer to the question of whether the Moon is there, or not, when nobody is looking!!

13. **Low Math, Meekly Interacting**
February 1, 2012

I just checked out Pharyngula, and the discussion there seems to confirm it: Dr. Andrulis is probably not well. Perhaps it's best I retract some of my harsher statements and hope he gets the help he needs.

So, better to focus on the real issue here, then (which I think was Peter's intent anyway): How? How in the bloody HELL did this get past Case Western's filters, or do they have none?

14. **Yatima**
February 1, 2012

>quantum theory is equally an exercise in probability theory

Along the lines of Lucien Hardy (http://arxiv.org/abs/quant-ph/0101012) and Ray Streater (http://arxiv.org/abs/math-ph/0002049) I suppose? Ray is way over my head here though.

15. **AcademicLurker**
February 1, 2012

LMMI:
How in the bloody HELL did this get past Case Western's filters

At least in my experience, academic institutions don't generally have filters that would prevent something like this. I've certainly always submitted directly to the journals, nothing was ever approved beforehand by some university level official (and a good thing too).

CWRU's screw up was in issuing the press release, assuming they're the one's that issued it. If that's what you meant, then I agree. *Someone's* alarm bells should have been set off before they reached the point of publicizing this stuff.

(stupid unrelated question: why do none of the standard methods for inserting line breaks seem to be working?)

16. **AcademicLurker**
February 1, 2012

Never mind…

17. **Low Math, Meekly Interacting**
February 1, 2012

Yes, I meant the hyping. It is not possible to avoid overstating the importance of Dr. Andrulis' work because, from a scientific perspective, it is utterly bereft. Anyone who reads even the abstract ought to be able to recognize that it is in the interests of all concerned to avoid public acknowledgement that it exists.

18. **Chris W.**
February 1, 2012

Low Math, Meekly Interacting,

As an RPI undergraduate many years ago, I recall coming across some curious little volumes among their physics books, the contents of which were quite similar to Dr. Andrulis' writing. I've sampled quite a few crackpot musings on the web since. They all seem fundamentally quite similar—a certain kind of barely coherent word salad that conveys a feeling that the author imagines himself in possession of a vast and indubitable insight into the nature of reality. I'll bet reams of this stuff get written every year, albeit rarely with the visibility granted to faculty at reputable institutions.

19. **Low Math, Meekly Interacting**

February 2, 2012

I guess it's old news on the interwebs, but a few of Life's editors, who were reportedly unaware of the paper until after publication, have resigned in disgust. Wonder if any heads will roll at Case.

20. **Shantanu**
February 2, 2012

Peter have you seen this?
Any comments?
http://blogs.nature.com/news/2012/01/us-physicists-call-for-underground-neutrino-facility.html

21. **Chris W.**
February 2, 2012

In my last comment I meant to say:

"As an RPI undergraduate many years ago, *browsing the university library*, I recall coming across some curious little volumes among …"

22. **Peter Woit**
February 2, 2012

Shantanu,

Interesting to see the strong endorsement from so many theorists. I haven't been following the LBNE story, had assumed that closing the Tevatron while keeping the US HEP budget fairly constant would free up funds for it.

23. **Shantanu**
February 3, 2012

Agree. however am surprised that the theorists in the letter still believe proton decay will be seen. Also I think we discussed here, non-0 neutrino mass really doesn't tell you much about physics beyond standard model.

24. **Tim van Beek**
February 3, 2012

Peter wrote:

> From Sean Carroll's Twitter feed I learned about this long description of a recent workshop bringing together philosophers and quantum field theorists.

Thanks for the tip, now I understand where the idea that "AQFT is a failed research program" comes from that I read about at several places in the blogosphere, it is from

*David Wallace: Taking Particle Physics Seriously: a Critique of the Algebraic Approach to Quantum Field Theory

While I *could* explain what Wallace gets wrong, in the spirit of the "string wars" I will instead mention that Ed Witten chose to use the framework to make some of his earlier ideas mathematically precise, see the arXiv. See?

While I agree with Wallace that philosophers of science should not limit themselves to theories that are mathematically precise (ignoring Lagrangian QFT and concentrating on AQFT, in this case), I get the impression that he (as several other theoretical physicists) actually don't understand what "mathematically precise" means – to mathematicians: He writes that Lagrangian QFT is, in this regard, on the same footing as QM, i.e. molecular and nuclear physics etc. From his paper:

> …the level of mathematical rigor in CQFT is basically the same as elsewhere in theoretical physics. So applying this standard even-handedly would invalidate practically all of the theoretical physics of the twentieth century: atomic physics, non-relativistic scattering theory, astrophysics, nuclear physics, etc, etc.

It would seem that this misunderstanding is also behind a lot of back and forth regarding the statement that "string theory is mathematically consistent".

25. **CU Phil**
February 3, 2012

Hi Tim,

I agree with you (in disagreeing with Wallace) that AQFT should not be considered a failed research program, and with Peter that people interested in understanding QFT should marshal any resources available. (As an aside, I was actually at the conference in question, and that tended to be the consensus view). However, I don't think the "failed research program" claim really originates with Wallace — I've heard it from physicists more than a few times, including some who I consider more concerned with rigor than most, as well as some wouldn't be caught dead reading a paper published in a philosophy journal. So my experience suggests that Wallace's claim is probably a symptom, rather than a cause, of the attitude you report seeing in the blogosphere.

26. **Chris Oakley**
February 3, 2012

Insofar as after more than 50 years one cannot calculate cross sections with AQFT, it is certainly a failed research program.

A bit more honesty about conventional QFT by Wallace, though, would be welcome.

QFT, as originally formulated, does not work when interactions are introduced. We therefore do not use it for this purpose. Instead our interacting theory comprises a set of invented fitting functions. Suggesting that these derive from quantum field theory is a nonsense as this would involve extracting unique finite values from the difference of two infinite ones. This is impossible, as all should know.

27. **Tim van Beek**
February 4, 2012

CU Phil said:

> However, I don't think the "failed research program" claim really originates with Wallace — I've heard it from physicists more than a few times, including some who I consider more concerned with rigor than most, as well as some wouldn't be caught dead reading a paper published in a philosophy journal. So my experience suggests that Wallace's claim is probably a symptom, rather than a cause, of the attitude you report seeing in the blogosphere.

Of course I do not know where this claim originates, but since Wallace does not cite anyone else, we could simply stick to discuss what he himself wrote.

Of course a research program can fail only in relation to a specific goal (some people seem to use the phrase to express their *dislike*, like in "AQFT is a failed research program, I don't like it because I have developed a metaphysical allergy against operator algebras". I hope it is possible to keep that out of the discussion).

Wallace claims that the goal of AQFT was to clear up the mess with infinities in QFT and especially the need of renormalization. That may have been the motivation of *some* researchers, but the core motivation for the program was and is to have a *mathematically precise* formulation of what a QFT is. One where you can actually *prove theorems*. As I said before, I now doubt that Wallace even understands what that means.

Then, if the goal is to "calculate cross sections before the year 2000" then the program has failed, too. Wallace keeps pointing out that there is no interacting 4D QFT that fulfills the Haag-Kastler axioms yet, and that he does not see any reason to believe that there ever will be. Well, the advanatage of the axiomatic approach is that you can go back to the drawing board and learn what was wrong with the axioms, in such a case.
I would really like to learn if people who say that the Haag-Kastler axioms don't describe (the Heisenberg picture of) realistic QFTs have specific ideas about what is wrong with the axioms?

28. **Peter Orland**
February 4, 2012

Hi Chris,

You said, "QFT, as originally formulated, does not work when interactions are introduced. We therefore do not use it for this purpose. Instead our interacting theory comprises a set of invented fitting functions. Suggesting that these derive from quantum field theory is a nonsense as this would involve extracting unique finite values from the difference of two infinite ones. This is impossible, as all should know."

No field theorist believes in QFT as it was originally formulated. The original formulation, as a Lagrangian or Hamiltonian with finite interactions is simply wrong. That's why regularization (is this what you mean by "fitting functions"?) must be introduced, then removed. If you do this in the right way, there are no infinities (see below) and the result is unique (we believe). The modern "conventional" viewpoint is that this IS the QFT – the limit of a regularized theory. Most field theorists are very comfortable with this.

A regularized QFT in a finite volume has an interaction representation. There is a "Haag's theorem" ruining the interaction representation, when crossing critical couplings, but we work away from these couplings, approaching them only as we remove the regulator. One never encounters anything infinite, provided renormalization is done this way. I want to stress that this is not the way Feynman, Schwinger or Tomonaga thought about QFT in the 1940s – that point of view is long obsolete.

My impression is that people who don't like regularization are repelled esthetically. They feel regularizations are somehow dirty. I don't know if this is the best analogy, but I think it's like hating Newtonian sums as way of doing integrals, wanting some "purer" method e.g. Lebesgue integration. That's great, if you know how to use the purer method to do calculations or prove theorems. Unfortunately, that's a tall order for algebraic/axiomatic methods.

I think that the point of axiomatic or algebraic approaches is to avoid regularization and start with the theory which already has the (renormalized) S-matrix and Green's functions. But the conventional approach has the same goal.

We have some understanding why results don't depend on the regularization. We define the theory as a limit of the regularized theory, in which we take couplings to zero (in the case of QCD). There are different ways of doing this, but there is numerical and analytic evidence (though not a proof) that the result is unique.

Some algebraic field theorists are looking at conformal or massive integrable theories in 1+1 dimensions. I suspect that's the best place for them to make progress, at least in the short term. These theories can be "constructed" (I am not sure that is the right term) with no regularization, but we know their S-matrices and something about their Green's functions.

29. **martibal**
February 6, 2012

@Chris Oackley:
"this would involve extracting unique finite values from the difference of two infinite ones. This is impossible, as all should know."

What about renormalization as a Birkhoff decomposition ? (work of Connes and Kreimer in the years 2000) ?

30. **Jeff McGowan**
February 6, 2012

@peter orland

As a mathematician who has aesthetic issues with regularization I have a question – does the Riemann sum/Lebasgue analogy hold in that there are cases in the QFT's you can't handle, but perhaps you could handle with a better formulation? The Lebasgue integral is clearly better than the Riemann one, since it works for a (much) wider variety of functions, is there some hope one might get a formulation of QFT which did the same thing?

31. **Peter Woit**
February 6, 2012

Jeff,
Maybe some new magical idea will appear, but the problem here doesn't seem to be with the kind of integral you are using, but with the fact that the effective behavior of the fundamental degrees of freedom is changing as you vary the distance scale.

For asymptotically free theories like QCD, what is happening is that the behavior is approaching that of a free theory on distance scales going to zero, so in principle under better and better control as you go to zero distance. We believe there is a well-defined limit for these theories and have a conjectural precise statement about how it works (and the way it works isn't just a technicality of properly defining a measure). Proving these conjectures though is hard (worth a million dollars from Clay), but the difficulty is not at short distances, but that you have to also understand what happens at large distances, where the physics is strongly coupled and perturbation theory is useless.

For non-asympotically free theories like QED, or the Higgs theory, and for non-renormalizable theories like gravity, the degrees of freedom are not weakly coupled as you go to short distances. So, understanding the continuum theory requires some precise understanding of the theory non-perturbatively, and we have very little to go on there. Maybe some new notion of "integration" will solve this, but it looks like it would have to be something much more powerful than a simple technical trick. People have tried many technical tricks, but I think the reason nothing has really worked is that your "trick" needs to essentially solve exactly a non-trivial quantum theory with infinite number of degrees of freedom, away from any limit we understand.

32. **Peter Orland**
    February 6, 2012

    Jeff,

    This is just to add to what Peter W. has said:

    To answer your question as to whether one can do better than the standard approach – it isn't clear. There are (non-rigorous) exact calculations in some field theories with no regularization at all. These exist only in one space and one time dimension. There is no powerful general method, except in the conformally-invariant case. In the massive case (sine-Gordon, nonlinear sigma models, Gross-Neveu models), a lot of guesswork is involved, though the results are remarkable.

    Such techniques don't generally work in higher dimensions, though there is good reason to be optimistic for conformally-invariant supersymmetric gauge theories.

33. **Jeff M**
    February 6, 2012

    Thanks Peters. First Peter, I wasn't thinking some clever new integral might work, just using it as an analogy, my thought being that perhaps one might come up with a new way of looking at things (as Lebasgue did) to eliminate the problems. Second Peter, I guess from what you say that this can be done, but only in 1+1 dimensions, which I would think are quite far from being in any way generalizable. At least in my field 2 dimensions is very very different from 3, which is very different from 4+…

34. **thomas**
    February 7, 2012

    I was watching Arthur Jaffe's Simons Center talk the other day. He motivated work on QFT by the enormous success of QED in predicting g-2, and then went on to lament the lack of effort devoted to rigorous algebraic results. To me, this seems like a rather strange argument. We strongly believe that QED is just an effective field theory, so presumably there will never be a rigorous algebraic construction of QED.

    What also strikes me as slightly odd is that we do think that Wilson successfully "constructed" QCD, as the continuum (and infinite volume) limit of the lattice regularized version. It would seem like an interesting research program for mathematically inclined folks to show that the limit exists, and that is satisfies certain axioms. (In fact, it seems to me that this would be the most promising avenue towards proving confinement, because we already know that the theory is confining at strong coupling). But somehow this is not emphasized all that much, possibly because it does not really fit in very well with the emphasis on algebraic methods.

35. **Peter Woit**
    February 7, 2012

    thomas,

    What you suggest is exactly what many mathematical physicists have tried to do, and I'd think it's considered the only known path to getting the million dollars. The reason you don't see papers about it is that no one has a good idea about how to do this. On a fixed finite lattice you have good control over the weak and strong coupling limits, but this isn't good enough. The problem seems to be that no one understands well the physics of how the weakly coupled behavior crosses over to the strongly coupled. There are ideas about large N limits, but for fixed N you just don't have a good idea. Lacking any real "physical" argument for what is going on, finding a rigorous mathematical argument is probably hopeless.

    One example of this kind of thing is the Tomboulis paper discussed back in 2007 here

    http://www.math.columbia.edu/~woit/wordpress/?p=576

    and here's a later critique of it

    http://arxiv.org/abs/0901.4246

36. **thomas**
    February 8, 2012

    Work like the Tomboulis paper is indeed what I had in mind. I was mainly commenting on the fact that the Simons people organize a meeting on the Foundations of QFT and it appears that this sort of thing is not even discussed (they did, admittedly, have a talk by Kaplan on lattice fermions and lattice SUSY) .

37. **harryb**
    February 8, 2012

    Peter

    On following your link to CERN book reviews, I downloaded John Marburger's Constructing Reality on the basis of the review. For the non-physicist a demanding but very engaging overview on QT and particle physics. Interestingly, in a long book he only devotes two or three paragraphs to ST, and in chapter 6 – The Machinery of Particle Discovery – he references ST explicitly, but the footnotes take the reader only to Greene's The Elegant Universe, and to yourself and Lee Smolin who "approach string theory with much insight and considerably less enthusiasm".

    Balance at last?

38. **Peter Woit**
    February 8, 2012

    harryb,

    Thanks, I haven't seen that book. On the whole discussion of string theory has been much more balanced the last few years, for whatever reason…

39. **Allan Rosenberg**
    February 9, 2012

    If philosophers can't solve the technical problems of QFT, why not let the lawyers have a crack at it?

40. **Thomas**
February 10, 2012

Do physicist not become very nervеous because of the open consistency questions in quantum field theory? Friends whom I had asked compared with the (for ca. a 1.5 centuries) unsettled similar problems with analysis in the past, but the intensity and time frame would put it more properly (and more uneasy) near the centuries of unclear foundational issues in geometry in the past. And until those open issues are not really solved, does physics not risk to run into a dead end, as one needs enough theory to learn what data tell – when I read about waiting for accelerator's or astrophysical data, I wonder if the situation could be actually worse. (I.e. even if current theories fit reality, only with missing better theory one can find that out). The "practical standpoint" one reads about sounds to me wrong, e.g. it happened science history in the disputes between ptolemaic and kopernican astronomy, where real insight and progress came with the "unpractical theory" only.

41. **Chris Oakley**
February 10, 2012

> The "practical standpoint" one reads about sounds to me wrong, e.g. it happened science history in the disputes between ptolemaic and kopernican astronomy, where real insight and progress came with the "unpractical theory" only.

I totally agree. If you are going to botch it (and I am not necessarily demeaning that activity), it should be with the simplest thing available, which is why I am not in favour of (e.g.) the Epstein-Glaser technique or Hopf algebras. I challenge anyone who thinks that "regularizing" pathologically divergent integrals is an acceptable activity to formulate their theory in a way that avoids mention of divergent integrals at all. Introducing extra, spurious parameters that were not present at the outset is not allowed.

42. **Thomas**
February 10, 2012

@Chris – But is renormalization not very nicely in the process of becomming clear, in the work of Connes, Kreimer, Marcolli?

43. **Thomas**
February 10, 2012

@Peter Woit: "non-renormalizable theories like gravity" – what do yo think about Kreimer's idea that it may be renormalizable?

44. **Peter Woit**
February 10, 2012

thomas,

I don't know what idea of Kreimer's about quantum gravity you're talking about, and I don't think recent work by Connes, Kreimer, Marcolli resolves the problems with consistency of QFT that are serious worries. They're all studying the perturbation expansion as far as I know, and I don't see how you can get a fully consistent theory even for QED that way.

45. **Thomas**
February 11, 2012

Hi Peter,

yes, that work on renormalization fits probably better into the process of extracting interesting "platonic ideas" from quantum field theory and where they arise in pure mathematics, but not into those consistency questions. My impression is that one can say that such use of ideas from physics just extends what the antique greek geometers did with intuitions about space coming from our biology for doing geometry and even Kant struggled much later with distinguishing mathematics, "platonic ideas", physics (I just remember that this confusion extends to Bertrand Russel too, his PhD-thesis was about "proving" from first, nessessary principles of space-concept constraints for the physical universe).

Kreimer's remarks on quantum gravity are linked to in his website: http://www2.mathematik.hu-berlin.de/~kreimer/index.php?section=research&lang=en

And the arxiv has some articles by Jack Morava on physics, I would like to know what physicists think about it (in mathematics, his genius is so highly esteemed that I can't imagine that his idas on physics are not deep and interesting too): e.g. http://arxiv.org/abs/math/9908006 , http://arxiv.org/abs/math/0407104 , http://arxiv.org/abs/1001.0965

46. **Igor Khavkine**
February 11, 2012

@Chris Oakley wrote:
"I challenge anyone who thinks that "regularizing" pathologically divergent integrals is an acceptable activity to formulate their theory in a way that avoids mention of divergent integrals at all."

The Epstein-Glaser method precisely meets your challenge. To see that, one need only study it. Other, more conventional methods, whether involving divergent integrals, also lead to the exact same result. Though, if extensive discussions on spr did not convince you of the soundness of renormalization, a few blog comments won't either. However, the false implication made in your comment is worth correcting.

47. **Chris Oakley**
February 11, 2012

Ivor,

As I understand it, the Epstein-Glaser method involves inserting c-number functions which generalise the otherwise-badly-behaved time-ordered product expressions that appear in the perturbation expansion. A limit is taken with these functions to obtain a finite "physical" result.
The renormalization I was taught (in Cambridge, 30 years ago) on the other hand typically involves cutting off an integral, and the limit one takes later is with respect to this cutoff parameter.
Whilst I cannot argue that the Epstein-Glaser approach is more highbrow, it still amounts to the same thing: introducing extra degrees of freedom midstream, and taking some kind of limit. Although one may *think* one can get a unique answer, the fact is that by applying a little creativity one can get any answer one wants. This makes the exercise little more than the application of invented fitting functions. I see no axiomatic basis.

48. **Peter Woit**
February 11, 2012

Enough about renormalization, please. At this level of arguing about whether perturbative divergences in general are

1. A serious problem indicating the theory is ill-defined
2. Things that can be eliminated with mathematical machine X
3. Not there if you use the renormalization group properly

the discussion is stuck in a 40-50 year old time warp. I don't think that endlessly repeating geriatric arguments is fruitful.

49. **Anon**
February 16, 2012

This is probably very naive, but aren't we blurring the boundaries here between showing existence of a QFT and actually solving the QFT? For QCD, isn't existence pretty much a consequence of the property that the theory has a well-defined continuum limit (which is a consequence of asymptotic freedom)? Do we have to actually have to require, as you state, Peter, that someone solve the collective degrees of freedom of the low energy regime to know that the theory is well-constructed? And if so, why draw the line at the energies at which low energy particle experiments are done, as opposed to, say, requiring a first principles construction of, say, fluid mechanics, or the folding of hemoglobin, from QCD, before we will accept it as mathematically rigorous?

50. **Peter Woit**
February 16, 2012

Anon,

The problem is that you have to show that when you take the continuum limit you get a non-trivial theory. If you don't know anything at all about the infrared behavior when you take the continuum limit it's possible that in the limit you get a trivial theory in which all physical degrees of freedom have masses that go to infinity. Or all masses are zero (this is what is supposed to happen for the N=4 supersymmetric case).

The Clay problem is carefully worded, just asking for proof of a finite mass gap. This is pretty much the minimal information you would need to see that you have something non-trivial.

The Langlands Program and Quantum Field Theory

February 1, 2012
Categories: Langlands

Edward Frenkel is here this semester in the math department at Columbia, and he's giving a series of lectures on a topic dear to my heart. Video of his lectures on The Langlands Program and Quantum Field Theory is starting to be available, courtesy of our graduate students Alex Waldron and Ioan Filip, as well as our staff member Nathan Schweer.

The first lecture last week was an overview, outlining the general picture of the Langlands program in the number field, function field and geometric cases, as well as two sorts of connections to QFT (to certain 2d conformal field theories, and to S-duality in 4d super Yang-Mills). This week he started to get more specific, giving some details about how the Langlands program works in the function field case, in preparation for moving next week to the geometric analog where a curve over a finite field gets replaced by a Riemann surface. As an indication of references covering much of the material to be discussed in lectures, Frenkel suggests this survey article and this Seminaire Bourbaki report.

Frenkel is also working on some other different but quite interesting projects. With Ngo and Langlands he has a program to "geometrize" the trace formula, for details see his very recent AMS Colloquium Lectures. With Losev and Nekrasov he has a fascinating program for studying certain field theories using instantons in a very different limit than the usual semi-classical one. See here, here, and here.

Comments

1. **Some Physics Bro**
   February 2, 2012

   Interesting links!

   I was reading through and on the 6th page of the first Frenkel, Losev, Nekrasov paper they rather off-handedly state (without reference) that "It is generally believed state that non-supersymmetric quantum field theories should be understood as non-supersymmetric phases of supersymmetric ones". Is this a common piece of wisdom? I have never heard anything along these lines before. What does this even mean? Is there any sort of talk or paper that actually lays out an argument about this? Or is this just some personal conviction the authors decided to slip in without having to explain?

2. **Peter Woit**
   February 2, 2012

   SPB,

   You're dropping the adjective "realistic" in that quote, it doesn't sound like they are talking about a general phenomenon in QFT. I'm not sure what the authors had in mind, but one possibility is that they are just referring to the idea that the SM is really part of a SUSY theory with broken supersymmetry. This certainly has been a popular idea among theorists, perhaps less so after the past year of LHC results…

3. **Ossicle**
   February 3, 2012

   To what extent does his work intersect (or whatever verb you like) with you own ongoing efforts? Is his presence at Columbia this semester potentially fruitful for your work?

4. **Peter Woit**
   February 3, 2012

   Ossicle,

   There's quite a lot of intersection of Frenkel's work with what I've been trying to do, and having him here is great for me. We've already talked a bit, that has been very helpful and I'm sure this will continue. The next few weeks he'll be covering in his lectures the topics closest to my interests (how 2d QFT gets used in geometric Langlands). The combination of the lectures and being able to talk to him about this should help clarify a lot for me and hopefully will lead to some significant progress (and to finally getting some of this written up…).

5. **Ossicle**
   February 6, 2012

   That's terrific, Peter, good luck. I'm rooting for you to accomplish great things! 😀

6. **Mike**
   February 9, 2012

   Peter,

   I was wondering if you'd seen the discussion at Sean Carroll's blog about Lagrangian QFT.

   Here is the link:
   http://blogs.discovermagazine.com/cosmicvariance/2012/02/07/how-to-think-about-quantum-field-theory/#more-7957

7. **Peter Woit**
   February 9, 2012

   Mike,

   I wrote about this here:

   http://www.math.columbia.edu/~woit/wordpress/?p=4408

   To my mind it's hard to overemphasize how poorly we understand QFT, and so "heuristic" as well as "rigorous" approaches are both worth pursuing.

Latest from the LHC

February 7, 2012
Categories: Experimental HEP News

CMS and ATLAS have just released final versions of their Higgs analyses for the 2011 data (the new CMS gamma-gamma analysis was previously discussed here). The preliminary versions of these were what was released last December, and the final versions don't differ in a major way. The bottom line is still the same: there's evidence for a Higgs around 125 GeV, of the sort that you would expect with this amount of data if it were really there, but the evidence is still too weak to claim discovery. The CMS papers are here, here and here, with the combination here. The ATLAS papers are here and here, combination here.
Check out Philip Gibbs to see if he updates his unofficial overall combination. Rumor is that the official CMS+ATLAS combination, along with the latest Tevatron Higgs results, will be released at Moriond (first week of March).

This week the people responsible for operating the LHC machine are meeting at Chamonix, slides here. Current plans are to start recommissioning the machine March 14, and run at 4 TeV/beam, a slight increase over the previous 3.5 TeV/beam. Projected integrated luminosity is 3-4 times that of 2011 (15-20 inverse femtobarns). After the end of the pp run in October, it will be quite a long time until proton collisions start up again (late 2014 or 2015?), since there will be a long shutdown to fix magnet interconnections and allow the machine to operate at or near design energy (7 TeV/beam).

On some other blogs you can find rumors of evidence for observation of an stop squark. I've heard nothing of the sort, but who knows? Informed rumors are encouraged. One of the things about the LHC results that has surprised me is the lack of any such claims over the past year or so. With all the searches being done, you'd think that someone, somewhere would find a fluctuation big enough to get SUSY enthusiasts excited, whether or not there was anything actually there.

Update: For more about this, see Tommaso Dorigo, Matt Strassler and Jester.

Update: A new unofficial combination from Philip Gibbs is now up here.

Update: Chamonix summary is here. The long shutdown starting this fall should last until at least September 2014. After this the hope is to run the machine at 6.5 TeV/beam. There seems to be little hope any longer of running at full design energy of 7 TeV/beam.

Comments

1. **Bernhard**
   February 11, 2012

   So, we will probably discover the Higgs this year and if nothing else shows up, be until 2015 without data again and still having absolutely no good idea what lies beyond the Standard Model. After the Higgs hangover passes, will be a depressing scenario.

2. **neo**
   February 15, 2012

   re "he long shutdown starting this fall should last until at least September 2014. After this the hope is to run the machine at 6.5 TeV/beam. There seems to be little hope any longer of running at full design energy of 7 TeV/beam."

   What are the ramifications to BSM physics for this?

3. **Peter Woit**
   February 15, 2012

   neo,

   The implications of the lower energy are not that large. Shouldn't significantly affect the study of the Higgs (if it really is around 125 GeV). The bounds on SUSY and other exotic BSM phenomena will be slightly weaker, but that's not of great significance for physics.

SUSY Still in Hiding

February 14, 2012
Categories: Experimental HEP News

Recent rumors supposedly coming from theorists at Harvard indicating that today would be the day that an announcement would be made of first evidence for a superpartner of a top quark have just been shot down. The talk at CERN on recent ATLAS searches for such a signal shows that nothing was found. An example of new limits is that if stops are produced via gluinos, the gluino has to have mass greater that 650 GeV and the stop a mass greater than 450 GeV.

Over the past year the LHC has conclusively falsified pre-LHC predictions that strongly interacting superpartners would easily be seen in the early data, with typical bounds on gluino masses now up to 1 TeV or so. One way to evade this conclusion has been to argue that the first two generations of squarks are quite heavy, with only the sbottoms and stops accessible to the LHC. A typical example of analysis of scenarios of this kind can be found here, where the conclusion is that naturalness requires that the mass of an stop be less than 400 GeV, and the mass of a gluino less than twice the mass of the stop. This is now starting to be in significant disagreement with the data.

The ATLAS analysis uses 2 fb^{-1} of data, with the promise of updated results using the full 4-5 fb^{-1} coming soon. The details of the new analyses were made public today here, here and here. For some background, see the latest posting at Resonaances. I hear that similar analyses now completed by CMS, with the full 2011 dataset, also show nothing. This week the earliest of the Winter conferences is going on, at Aspen, and tomorrow there will be talks updating the LHC SUSY situation from ATLAS, CMS, and theorist Matt Reece.

The LHC has done an impressive job of investigating and leaving in tatters the SUSY/extra-dimensional speculative universe that has dominated particle theory for much of the last thirty years, and this is likely to be one of its main legacies. These fields will undoubtedly continue to play a large role in particle theory, no matter how bad the experimental situation gets, as their advocates argue “Never, never, never give up!”, but fewer and fewer people will take them seriously. As always seemed likely, the big mystery the LHC will solve will be that of the Higgs: is it really there, and if so does it behave as the Standard Model predicts, or does it do something more interesting? Unfortunately we’re going to have to wait a while longer for more news on that front.

Comments

1. **manyoso**
   February 14, 2012

   With so many prominent theorists betting on SUSY over the last few decades and the continued null signal at the LHC… if this continues couldn’t you almost call it ‘New Physics’ discovered by the LHC in that so many expected to see SUSY?

2. **Harry**
   February 14, 2012

   “Never, never, never give up!”.. Is this supposed to echo Churchill’s “never, never, never […] give in except to convictions of honour and good sense” ? 🙂

3. **physicsphile**
   February 14, 2012

   Having spoken to someone working at the LHC, apparently crazy seeming results are always turning up but they have always gone away on closer examination. So it would probably be better if the rumor mongers rather wait to get the official word from CERN.

4. **MathPhys**
   February 14, 2012

   In my humble opinion, the belief that there is susy and it solves many problems, but we don’t see it because it’s softly broken, and if we go to high enough energy, we’ll see it again, is one of the most simple-minded ideas in modern physics.

   I have to believe that God is more imaginative than that.

5. **Peter Woit**
   February 14, 2012

   physicsphile,

   Or they could get higher-quality rumors from more reliable blogs…

   MathPhys,

   What’s odd is that since following this logic leads you quickly to something quite ugly that explains very little (105 extra parameters???). I’ve never understood why people found that route attractive. There’s some fascinating mathematical structure going on, and I can see why people want to take that seriously, but there’s also clearly some big ideas missing about how to relate this to the real world.

6. **MathPhys**
   February 14, 2012

   Peter,

   In my very humble opinion, people take the susy road for two reasons

   1. The mathematically-minded types find supersymmetric quantum field theories extremely attractive because you can do some stunning things in them.

   In QCD, you work so hard for so long to compute some radiative correction to low order in perturbation theory, then you go to super YM and use relatively simple arguments to obtain some deeply non perturbative results. It’ hypnotic.

   2. To the rest, it’s a good way to generate a steady stream of papers.

   I think that things are getting really exciting in high energy physics now. I am very curious how things will be like 5 years from now.

Can such a wonderful, elegant and extremely useful mathematical structure turn out to be physically totally irrelevant? Did the "unreasonable effective of mathematics in the physical world" finally let us down? Very exciting times ahead.

7. **MathPhys**
February 14, 2012

I went to a talk on some topic in a hyperbolic universe. The speaker started by saying that of course we know that the universe is not hyperbolic, but it's still important to learn as much as we can about that topic regardless of the parameters.

I think that that's how people will continue to work on many topics long after LHC discredits them. My point of view is that, if it's harmless and makes you happy, go for it.

8. **Cesar Laia**
February 15, 2012

I don't understand why they used only 2 fb-1 of data, when they have already 5 fb-1. If the 5 fb-1 data will come soon, I guess they already have an idea of it's coming. So… would it make any sense to announce now they saw notting just to say a few months later that actually there is something new?

9. **Peter Woit**
February 15, 2012

Cesar,

It takes a lot of work to analyze this data. They need to completely understand the behavior of their detector and all the possible sources of background. And the data was gathered over 2011 under changing conditions (luminosity was increasing, triggers changing). So, possibly quite a lot more work is needed to analyze the rest of the data set.

Right now ATLAS has the best bounds on SUSY of this kind, which is presumably why they announced now. They're in a competition with CMS, which provides pressure to release results once they have confidence in them, not wait for something better.

10. **Angel Naydenov**
February 15, 2012

Dear Peter,
I am currently reading Chapter 12 of your book and to be honest, I was hesitating to continue once the stop rumour surfaced.
However, it looks as if apologies are in order for letting you down. All seems so logical when you explain it, it's fascinating that your view is not shared by the majority of scientists.
Do you believe a paradigm shift is on its way? Which direction shall we choose to make a step forward?
Thanks, and please blog more! 🙂

11. **Eric**
February 15, 2012

There are actually two separate issues which are being confused here, the rumors regarding the stop having been seen at the LHC and the speculation of one blogger that these rumors were to be addressed in a seminar this past Tuesday. The speculation regarding the Tuesday seminar did not pan out, however it does not then follow that there is nothing to the rumors. Be patient and let the experimenters complete their analyses.

12. **Anonyrat**
February 15, 2012

Eric, Tuesday's seminar set more stringent bounds on sTops, so……

13. **Eric**
February 15, 2012

Anonyrat, this is only with 2 fb-1 of data, whereas 5 fb-1 of data has been collected, so…

14. **Anonyrat**
February 15, 2012

sigh, if the 2 fb-1 showed some hint of something, then its strengthening in 3 additional fb-1 of data would be plausible.

15. **MathPhys**
February 15, 2012

Is there a way to put money on whether susy will or will not discovered, let's say by end of 2012?

I want to bet $100.00 that there will be NO evidence of susy by end of 2012. `Evidence' here means an official announcement from LHC. Rumours do not count.

16. **Peter Woit**
February 15, 2012

Angel,

Thanks, though I should point out though that my views about SUSY, string theory, extra dimensions, etc. have never really been minority ones, and definitely aren't now. I think MathPhys is going to have trouble finding anyone willing to bet $100 on SUSY this year, or maybe even on SUSY this decade.

One big lesson of the LHC I think is going to be that the Standard Model is just too good. None of the ideas of dramatically adding lots of complicated structure to it are having any success. It would be a good idea to concentrate on what we don't understand about the Standard Model itself (one example: how to formulate chiral gauge theories non-perturbatively). Personally I'm enthusiastic about new ideas I'm working on about how to handle gauge symmetry with a variant of BRST called Dirac cohomology, as well as related ideas about the relationship of QFT and Langlands theory in math. But, people should work on whatever they enjoy and think is promising. It's getting harder and harder though to claim that SUSY ideas like the MSSM are in any sense "promising" these days.

17. **anon.**

February 16, 2012

Two brief comments:

* The original rumors *I* heard, at least, were about CMS, not ATLAS. Further attempts to ferret out what's going on haven't turned up anything, and the rumors exist in several contradictory strains, so I doubt there was any substance to them.

* Tuesday's seminar didn't set more stringent limits on stops, it set limits on *gluinos* decaying to stops, and not really more stringent ones than previous same-sign dilepton analyses had set.

18. **sUP**
February 16, 2012

Dear Peter,

if there is no SUSY out there, what would be most promissing explanations for so called dark matter/energy puzzle?

19. **Peter Woit**
February 16, 2012

anon.,

I'm still curious about where these supposed Harvard-originated rumors came from. I've seen one soon-to-be-released CMS SUSY analysis very similar to the new ATLAS ones, and they see nada.

It always seems to be though that as new ATLAS/CMS analyses are released, we hear "move along, nothing new to see here". Is there an analysis to be released at Moriond that SUSY proponents can point to as likely to provide a signal according to one of their favorite scenarios? If not, what is the time scale for such an analysis: Summer 2012? Winter 2013? Winter 2016?

sUP,

SUSY never gave an explanation of dark energy. For dark matter, I know of no explanation that could be called "promising", SUSY or otherwise.

20. **Shantanu**
February 16, 2012

suP, dark energy could be a plain cold cosmological constant or just back reaction
due to density inhomogeneties (which has nothing to do with SUSY)

Also dark matter could be axion or primordial black holes (nothing to do with
SUSY) or massive graviton and many other such candidates nothing to do with SUSY.
none of these are ruled out.
Or maybe dark matter also could be due to modified gravity (yes, people
always cite bullet cluster as a counter-evidence against this, but that is oversold
and bullet cluster observations pose problems for dark matter).

21. **anon.**
February 16, 2012

Rumors I heard originated with CERN theorists.

A few results so far that were *not* "nothing to see here," in my idiosyncratic opinion:
* First 1/fb jets+MET results
* First 1/fb same-sign dilepton results
* ATLAS limit on sbottom -> bottom + invisible
* Limits on R-hadrons
* The potential Higgs signal

All of these wiped out huge chunks of parameter space. I don't know if Moriond will give us anything as dramatic or not. Direct stop searches are more difficult, but will be very interesting, whenever they start to appear.

22. **harryb**
February 16, 2012

@MathPhys

"Is there a way to put money on whether susy will or will not discovered, let's say by end of 2012?"

Yes. Go to Intrade.com and you will find a potential market there. I raised this before in this blog a month ago, and was chastised for leading people to a market that is too thin for real gains. Whatever, acorns, oaks etc.

I still believe that if there was an active "market" for current theories then as in sports that were latterly opened up to proper markets, interesting bets might take place. (Who would believe a left tackle is the second highest paid position in American football etc).

If capitalism / market theory (flawed or nor depending on your political flavour) is essentially a human endeavour, as is science, then a consilience between the two ought to be at least explored. If nothing else it would take esoterica more mainstream and if String Theory stock fell miserably, we all might feel a bit more comforted (and if it rose, then we would have explaining to do).

Anyway – I think the market test you propose is very good. Good luck at Intrade.

23. **SpearMarktheSecond**
February 16, 2012

Well, if there is thermal equilibrium between quarks/leptons and the dark matter, a weak interaction cross section between quarks/leptons and the dark matter is indicated.

Hence the promising WIMP conjecture.

That SUSY implementations got this right along with detailed Big Bang modeling was also promising.

But SUSY has lots of nutty adaptations in it… like 5 squarks degenerate in mass. Also… left handed scalars and right handed scalars. Lots of people have trashed SUSY for a long, long time.

24. **MathPhys**
February 17, 2012

@SpearMarkTheSecond,

Can you give us more details of how susy model building was abused? This has always been my impression as well, but I don't know enough phenomenology to say something meaningful. I go to their talks and I hear what I consider to be unimaginative science fiction, but everyone else sits there nodding.

@harryb

Thank you. I will.

25. **Cesar Laia**
February 17, 2012

I would like to make a question. Everyone seems focused on the LHC results, but what are the chances in the near future that other experiments could give hints about SUSY and other theories? E.g., I wonder what relevance the results from the Planck Telescope will have, but being a laymen I am not aware of other promising experiments. It seems to me that what you need are clever experiments right now.

26. **Shantanu**
February 17, 2012

Cesar, short answer is no. Planck can only potentially detect tensor modes, from
which you can potentially say something about inflaton potential. but susy says nothing about it.
In theory if proton decay is detected that could be an indication for SUSY. but again
the current limits have already ruled out many promising models and I doubt even the SUSY enthusiasts who post on this blog are sanguine about proton decay been detected.

27. **anonymous**
February 17, 2012

Planck is likely to nail 2 important and largely unknown parameters, N_eff and n_s, and is likely to observe f_NL. N_eff is the effective number of neutrino species, which is 3.04 in the standard model. Other CMB experiments (ACT/SPT/WMAP) have been reporting a higher value, albeit with large error bars. Higher N_eff could be evidence for sterile neutrinos. The other two parameters, n_s and f_NL, are important for understanding cosmic inflation or alternative theories. Planck is probably not sensitive to the other inflation parameter, r, although BICEP2/Keck Array or some of their up-and-coming competitors may have something to say about it in a couple years. Cosmic inflation is thought to occur around the GUT scale — a lot of new physics to learn there.

Direct dark matter searches often say they're in the business of looking for SUSY. These experiments usually postulate the existence of a WIMP and try to observe it scattering off a nucleus. Ones that use aggressive cuts to make sure that all they ever see is WIMPs tend to report negative results (e.g. Xenon-100). Ones that just look for seasonal variations in signal tend to report positive results (e.g. DAMA). Some theories say dark matter annihilation can give excess background in the Fermi telescope, which looks at gamma rays although you shouldn't take a result based on a too-high background all that seriously.

One of the more interesting but ignored experiments I see coming together soon is AEGIS, which wants to look at the effect of gravity on antihydrogen. Any surprises there would tell us a lot about the equivalence principle and basic assumptions of particle physics.

28. **Shantanu**
February 17, 2012

anonymous, does super-symmetry actually predict the existence of a sterile neutrino?
(and conversely if no sterile neutrino is found, does that mean SUSY is ruled out?).
from what I understand non-0 neutino mass, whether sterile neutrinos exist,
whether theta_13 is zero or not is completely decoupled from TeV scale physics
and models for EW symmetry breaking.
Also I disagree with
"Cosmic inflation is thought to occur around the GUT scale — a lot of new physics to learn there."
This may have been true before WMAP results. but current limits on energy scale of inflation have already ruled out GUT models as I understand.

29. **Rick Ryals**
February 17, 2012

MathPhys,

If harryb's market doesn't do it for you, try Longbets, "The Arena For Accountable Predictions":

http://longbets.org/

You may or may not find a taker, but it'll be on record, regardless. John Horgan, and more notably, Martin Reese, have predictions there. So do I:

http://longbets.org/476/

30. **anonymous**
February 17, 2012

Yes, those properties of neutrinos should be independent of most SUSY theories. However, neutrino oscillation is the one piece of "Beyond the Standard Model" physics that has been observed concretely.

As for the GUT comment, nobody really knows for sure. There's a lot of parameter space that experiments are actively exploring. The state of the art is the

SPT+WMAP+H_0+BAO result at http://arxiv.org/abs/1105.3182 , which has a solid 3.6 sigma evidence of n_s < 1 and the best published upper limit on the tensor-to-scalar ratio (r < 0.21), with figure 8 showing how some of the more optimistic models are getting ruled out. You need to measure r, though, if you really want to learn the energy scale of inflation. With r~0.2, the energy scale of inflation still could be as high as ~10^15 to 10^16 GeV (very approximately), which SUSY people will tell you is the GUT scale. These SPT+WMAP attempts to measure r through CMB temperature only (as opposed to polarization) are running out of steam because cosmic variance ultimately gets in the way (http://arxiv.org/abs/astro-ph/9407037). If r<0.01, then polarization experiments will also start running into lensing and galactic foregrounds. Hopefully an exciting signal gets discovered before then…

31. **Shantanu**
February 18, 2012

Anonymous, you said

"However, neutrino oscillation is the one piece of "Beyond the Standard Model" physics that has been observed concretely."
This is what everyone says in any neutrino talk, but that's incorrect.
PDG lists it as part of standard model.

See the discussion between me, Tomasso and Andrea here on this in the comment section. (At that time this was news to me also).

Plus as you yourself agree models of neutrino mass such as seesaw mechanism are completely decoupled from BSM physics.

32. **MathPhys**
February 18, 2012

Okay, so even neutrino oscillations are part and parcel of the standard model. Is it safe to say that we don't know of anything, anything at all, that requires 'beyond standard model' physics?

33. **Thomas Larsson**
February 18, 2012

I gave an undergradutate seminar about neutrino oscillations in the spring of 1981, as part of course which nominally was about nuclear physics, but which we students generally called CERN physics because of the trip that ended the course (Cernphysik statt Kernphysik, same thing in Swedish). Assuming that I did not originally came up with the idea of neutrino oscillations, it must have been pretty mainstream at that time.

34. **Ravi K**
February 18, 2012

Shantanu — I think there is no "standard" model for the neutrinos yet. If there is one what is it?

35. **Ravi K**
February 18, 2012

Cesar — I am looking forward to the neutron edm experiments — for a good recent update please see this http://www.nature.com/news/dipole-hunt-stuck-in-neutral-1.9943 I hope they will provide clues as to how P and CP symmetries got violated in nature…..

36. **Shantanu**
February 18, 2012

Ravi, for some reason the link I pointed to did not come out well.
See the comment section of
http://dorigo.wordpress.com/2008/10/31/cdf-publishes-multi-muons/

esp. comments by Tomasso and Andrea Giammanco
Also as mentioned above seesaw mechanism (or whatever other mehanisms
you invoke to give neutrino mass) is completely decoupled from TeV scale physics,
ew symmetry breaking.

37. **Ravi K**
February 18, 2012

Hi Shantanu,

I think the argument by Andrea in that link that neutrino mass is like any other particle's small mass, only smaller, is not quite right.

Basically there is new physics because the standard model has only one mass scale — the Higgs VEV. I dont think you can get the neutrino mass from this single mass scale by multiplying it with a small dimensionless number. The problem is not that the number is too small — the problem is that such a procedure will also make the right handed neutrino light. We need to introduce a second mass scale — large or small — to explain the neutrino mass.

The mechanism may be decoupled from EW symmetry breaking — but so what? why should that not make it BSM. However nothing really is decoupled from anything as we have the hierarchy problem…..

38. **Shantanu**
February 18, 2012

Ravi, I disagree, but let's agree to disagree. I have yet to see a single prediction from
any of the BSM theories about theta_13, whether sterile neutrinos exist or not etc.
(Note also that by this logic do you consider existence of gravity
is also beyond the standard model physics).

39. **Ravi K**
February 18, 2012

Agreed! — and anyway whether it is SM or BSM its just a name.

You dont have a single more or less agreed upon prediction because there is no consensus on the model with neutrinos. But each model by itself would have some predictions for example on whether sterile neutrinos exist or not. Also some of these models have gotten ruled out (or constrained) because of their predictions.

But what is the standard model prediction for neutrino mass/mixing scale — that is a question that should have an agreed upon answer, and any deviation from it can be attributed to BSM.

Quantum treatment of gravity would be BSM in my view — If gravitons are found would that be considered SM physics? Currently there are two parallel standard models – one for the quantized forces and one for gravity.

40. **David Broadhurst**
February 19, 2012

> neutrino oscillation is the one piece of "Beyond the
> Standard Model" physics that has been observed concretely
Neutrino oscillations were envisaged before the standard model.
Bruno Pontecorvo [JETP 33 (1957) 549; 34 (1958) 247]
originally suggested oscillation between \nu{e} and
\bar{\nu}e. Later he suggested [JETP 53 (1967) 1717]
oscillations between \nu{e} and \nu{\mu}, before Ray Davis
had firmly established a deficit in the solar neutrino flux.
This was elaborated with Gribov [Phys. Lett. B28 (1969) 493].
Reading this as student, I found it natural to assume
that all quarks and leptons are massive and that neutrinos,
like quarks, may mix. Growing up along side the SM, I saw
no reason to exclude that possibility and, when I started
teaching the SM, I used to remind students that the number
of its free parameters had (then) been underestimated.
I find it odd to read, nearly 40 years on, that neutrino
mixing is now supposed to be BSM.
David Broadhurst

41. **Ravi K**
February 19, 2012

I guess what I am trying to say is that there are two ways of extending the standard model to accommodate the observed neutrino physics. One way is by adding 2 or 3 right handed singlet neutrinos to the standard model quark and leptons and providing them with a very heavy mass — maybe even like at the GUT scale — which will in turn see-saw into the observed small masses of the already known neutrinos. The other way is to not add any right-handed neutrinos keeping the usual fermion content of the standard model with only the left-handed neutrino and try to accommodate the neutrino masses.

Until one of these ways is experimentally ruled out it may be difficult to claim there is a standard way of dealing with the neutrinos though theoretically the see-saw way seems to be the more standard way.

However in either way of doing things a new mass scale for quantized forces is implicated by the neutrino data.

42. **David Broadhurst**
February 20, 2012

Ravi K wrote, on February 19, 2012 at 11:40 pm:
> a new mass scale for quantized forces is implicated
> by the neutrino data
Yes, I tend to agree with that, if one regards the SM as an
effective theory. Yet, if one adopts the R-G viewpoint that
no term in the Lagrangian density can have its coefficient
specified a priori; rather it must be taken from experiment
(as in pure QED), then the SM may still be regarded as
formally complete. I guess that we all hope that the SM is
merely an "effective theory", with something (as yet utterly
unknown) lying above it, at some (as yet utterly unknown)
energy scale. But that is, at present, pure hope. All we
know, at present, is that, within the rigid formalism of
renormalizable QFT and the discipline of experiment, the
standard model is theoretically and empirically flawless.
It seems to me that only experiment can modify that strange
success. Else, I can only agree with Hamlet that "there is
nothing either good or bad, but thinking makes it so".

43. **Mark**
February 22, 2012

I just went through Matt Reece's slides and was a bit surprised to see the claim that in Type IIB the tree-level gauginos are down by the Log[M_Pl/m_32] relative to the scalars, which is not the conclusion of the paper he is referring to. Has he actually checked the reference http://arxiv.org/abs/hep-th/0610129 that he is citing? At the bottom of page 24 it clearly states that because parameter p=1 for bifundamental matter, which is what the MSSM scalars are, "the scalar masses are comparable to the gaugino masses rather than logarithmically larger". There is even a sample spectrum for the this Large Volume scenario on page 30 and for the KKLT type scenarios on pages 31 and 32 where one clearly sees that in these Type IIB vacua the squarks and sleptons are as light as the gauginos. It's disappointing to see incorrect information presented in such important review talks.

44. **Mark**
February 22, 2012

I just realised that Matt is actually referring to an earlier paper of Conlon and Quevedo, http://arxiv.org/abs/hep-th/0605141 where the computation of the scalar masses was estimated before the authors found the form of the Kahler potential for bifundamentals. Sorry Matt, I guess you were a bit misled by that earlier paper, but I hope you'll take the time to read the subsequent paper as well.

45. **Henry Bolden**

February 22, 2012

At least we finally have definitive news on the Higgs:

[Dilbert discovers the Higgs.]

46. **Matt Reece**
February 22, 2012

Sorry for the sloppiness, Mark. I'm not an expert on string constructions. A later reference, 0906.3297, also makes the claim that there may be "a minor version of split supersymmetry," and I was trying to grab the earliest such claim in the literature. Looks like I picked the wrong one. (The '06 references, as far as I understand, all suffer from the chirality vs moduli stabilization problem pointed out in 0711.3389, so I probably should have stuck with just citing the later one.)

Anyway, the point wasn't to single out a specific construction so much as to observe that these small hierarchies often fall out of models, and that we should maybe understand a little better precisely when it does or doesn't happen. Your later reference reinforces that point, I think—it seems to me that there should be a 4d effective field theory explanation of why, on doing a more careful calculation, they found the expected leading term ~m_{3/2} canceled.

47. **Joseph Conlon**
February 23, 2012

Matt,

One comment to make here is that many of these cancellations of terms of order M_{3/2 } relate to an underlying no-scale susy breaking structure. No-scale has many neat features: it cancels both tree-level and loop corrections to soft masses (such as arise in anomaly mediation). The fact that no-scale also arises naturally in string constructions further marks it out as a very special structure.

There should be a right way of looking at supergravity so that the no-scale cancellations all become obvious. Unfortunately I don't know what that way is, but there is manifestly a structure present in no-scale models that is obscured by the conventional presentation of supergravity, where these cancellations appear just as a series of terms that happen to add up to zero.

48. **Marko**
February 23, 2012

Thank you for your reply, Matt! I see Joe has already tried to answer your question. As far as I understand, in the KKLT case the suppression of the scalar masses relative to m_3/2 is due to warped sequestering, which does have a 4D interpretation as conformal sequestering, as explained here: http://arxiv.org/abs/hep-th/0703105 . In the LV scenario of Joe et al, the no-scale structure does play the crucial role and specifically arises in Type IIB due to the particular scaling of the Kahler potential. However, the LV scenario is also unique because SUSY breaking there is moduli-dominated. So, these two properties, when combined, lead to the above cancellation. This remarkable property disappears in, e.g. G2 vacua, where the scaling of the Kahler potential wrt to the moduli is completely different and even if one could construct a G2 analogue of Joe's LV vacua with moduli-dominated SUSY breaking, the corresponding scalar masses would look like m_i^2=[(1-7/3)+\epsilon_i^2]*m_32 so the scalars would remain as heavy as m_3/2. As for the reference for string models with slightly split spectra, check the papers of Acharya et al.

49. **Marko**
February 23, 2012

the m_32 should be m_32^2

50. **Mark**
February 23, 2012

@ Matt, for stringy models with split spectra I'd recommend checking out this paper http://arxiv.org/abs/hep-th/0701034 or the more general version of the same construction http://arxiv.org/abs/0810.3285

51. **Mark**
February 25, 2012

@Matt, I must correct one of my previous statements. After a more careful checking, it turns out that for moduli-dominated SUSY breaking when the moduli F-term contributions nearly cancel the -3M_{3/2}^2M_{Planck}^2 term in the scalar potential, the cancellation of the leading contribution to the scalar mass squareds a la LV scenario of Joe et al does happen for the G2 case as well! In Type IIB, the moduli Kahler potential is K=-2Ln(V) and the bifundamental chiral matter Kahler potential scales as 1/V^(2/3), where V is the CY volume, and these two things, when combined with moduli-domination and CC=0 constraint, lead to parameter p=1. In G2 compactification case, K=-3Ln(V) while the Kahler potential for fundamental chiral matter scales as 1/V, where V is the volume of the G2 manifold, and when combined with the assumption of moduli-dominated SUSY breaking and CC=0, also lead to p=1 and the suppression of the scalar masses! These cancellations look completely mysterious from the 4D point of view though. That said, the G2 vacua of Acharya et al do not have this feature and do exhibit a split spectrum because in that case SUSY breaking is not moduli-dominated.

52. **John Baez**
February 26, 2012

MathPhys wrote:

> Okay, so even neutrino oscillations are part and parcel of the standard model.

The original Standard Model had massless neutrinos, and thus no neutrino oscillations. The new Standard Model has neutrinos whose masses and oscillations are described by the [Pontecorvo-Maki-Nakagawa-Sakata matrix]. I'm not sure how convinced the experts are that neutrino physics is adequately captured by this new Standard Model – I haven't been keeping up with that.

> Is it safe to say that we don't know of anything, anything at all, that requires 'beyond standard model' physics?

No, that'd be going too far. Many astrophysicists believe there's 'cold dark matter' made of some particle or particles not included in the Standard Model. Many believe in 'dark energy', which can be modeled simply by including a cosmological constant in Einstein's equations, but might arise from some particles we don't know. And many believe in 'inflation', which requires some physics beyond the Standard Model and general relativity.

In short, astrophysics and cosmology seem to require physics beyond the Standard Model and general relativity.

53. **Eric**

February 26, 2012

In addition to the excellent points made by John, one shouldn't forget that a certain amount of CP violation is required in order to generate baryogenesis/leptogenesis. The Standard Model does not provide enough CP violation, and so presumably this is generated by new physics.

54. **Shantanu**
February 27, 2012

Eric,
Note that one thing which is not usually well known/advertised among particle physics/HEP audience is that there are modifications/extensions to GR which can potentially explain baryogenesis. Two examples are Einstein-Cartan-Kibble-Sciama theory(see http://arxiv.org/abs/1101.4012) and Chern-Simmons theory (see http://arxiv.org/abs/gr-qc/0308071).
so in that case you don't need beyond SM physics, but we do need physics beyond SM+GR.

55. **Vince**
February 27, 2012

Eric,

Judging from the abstract from http://arxiv.org/abs/1101.4012, this is only a classical theory and the theory is supposed to apply at the very early universe, for which you absolutely need a quantum theory of this theory that also incorporates the SM, as well as any other possible interactions between this scale and the SM scale.

56. **Marty Tysanner**
February 28, 2012

> the theory is supposed to apply at the very early universe, for which you absolutely need a quantum theory of this theory […]

Just to be clear, this is the conventional wisdom and it may very well be true, but it isn't an established fact. We don't have a "real" quantum gravity theory yet, and don't know what will ultimately describe the very early universe…

String Theory Skeptics and Multiverse Mania

February 21, 2012
Categories: Multiverse Mania

My endless rants here about the hot field of multiverse studies are mainly motivated by concern about the effect this is having on particle theory. Multiverse scenarios all too often function as an excuse for not admitting that string theory/extra-dimensional ideas about unification have failed. Such an admission would encourage people to move on to more promising ideas, but instead hep-th is stuck in an endless doldrums with the high profile public face of the subject dominated by excited claims about what a wonderful discovery this region is.

Independently of the string theory problem, I'm personally a skeptic that multiverse studies have any promise, simply due to the fact that the subject lacks a viable theory, any experimental evidence, and any plausible prospects for getting either. Others feel differently though, and very recently two of my fellow string theory skeptics have written about the subject much more positively.

The first is Lee Smolin, who has written an essay for the *Foundations of Physics* "Forty Years of String Theory" volume with the title [A perspective on the landscape problem](). Smolin's interest in multiverse models goes way back, to long before the current string-theory-based mania. He's got a good argument that he was the originator of the term "landscape" itself, which he wrote about back in his 1997 book [The Life of the Cosmos](). If you're interested in the multiverse at all, Smolin's article is well-worth reading. I very much agree with his emphasis on the principle that one has to be careful to stick to ideas that can legitimately count as science, by conventional standards of testability. He is pursuing "cosmological natural selection" scenarios which he argues do have testable consequences. I'm not convinced there's enough there to ever lead to solid evidence for such a scenario, although there may be enough structure there to sooner or later make it clear if the idea is simply falsified by one fact or other about the universe.

Today's New York Times has an [article by Dennis Overbye]() about Lawrence Krauss and his new book [A Universe From Nothing](). Much of the book is an excellent discussion of cosmology and the physics of the vacuum, but it also devotes a lot of effort to discussing the meaningless question of "Why is there something rather than nothing?" and arguing against the invocation of a deity in order to answer it. Krauss is no fan of string theory, which he regards as overhyped, but he seems to have developed an attraction to multiverse studies recently, perhaps motivated by their use in arguments with those who see the Big Bang as a place for God to hang out.

Personally I've no interest in arguments about the existence of God, which epitomize to me an empty waste of time. Given the real dangers of religious fundamentalism in the US though, I'm glad that others like Krauss make the effort to answer some of these arguments. I'm less happy to see him and others adopting the multiverse as their weapon of choice in this battle, since it's a lousy one and not going to convince anyone. In the New York Times piece we're told:

> "Maybe in the true eternal multiverse there are truly no laws," Dr. Krauss said in an e-mail. "Maybe indeed randomness is all there is and everything that can happen happens somewhere."

Given the choice between this vision of fundamental science and "God did it" as explanations for the nature of the universe, one can't be surprised if people go for the man in the white robes…

Comments

1. **Low Math, Meekly Interacting**
   February 21, 2012

   This one also troubles me. There appears to be an urge among skeptics to fill the gap of universal origins, where the theists God resides, with something else, rather than admitting it must remain empty. I seems somehow unacceptable to have no answer but an admission of ignorance when confronted with claims that the ultimate hows and whys of existence are the purview of religion and not science. For me it's perfectly acceptable to reject mythology whilst having no clue myself what the "truth" of the matter is. When engaging in such debates, it's clearly better to be ignorant than wrong, because the theist's argument cannot be disproven, while the scientist's can. The theist holds the scientist to such rigorous standards, while needing none for himself. All the worse when the scientist proposes ideas that are not even wrong. If God is untestable and the multiverse is untestable, then claiming superiority as part of the scientific enterprise and using abstruse equations to make the case for untestable hypotheses cannot exonerate one from accusations of hypocrisy.

2. **Nige**
   February 21, 2012

   "There appears to be an urge among skeptics to fill the gap of universal origins, where the theists God resides, with something else, rather than admitting it must remain empty."

   Premature uncheckable speculations which harden into an orthodoxy have founded some of the world's greatest religions. The replacement of a single God with a multiverse of them goes against Occam's Razor, and it isn't new either.

3. **JE**
   February 21, 2012

   Interesting post, right to the meat of the matter… in this pseudo-obscurantist era? All in all the multiverse and the man in the white robes provide similar circular answers to the same old question. Ironically, most scientists wear white aprons, as opposed to black soutanes, and some seem to be taking the analogy too far. Either the LHC sheds light on the landscape of particle physics, with the God particle explaining EWSB and reinforcing the SM as the framework into which to try to accomodate deviations (or from which to build a larger framework that can accomodate escapologist deviations and unsolved issues), with SUSY being largely ruled out and black robes and circular thinkers being gradually ousted from the scientific arena, or this neverending multiverstring idea may end up synergizing with God to last another 2,000 years. Unless, please, someone comes out with a better idea.

4. **emile**
   February 21, 2012

   I like the multiverse idea. It may well be true. A priori, I don't see why there should be only one universe. In general, I would be careful about just bashing this idea: it's not as if it does not make any sense at all. Granted, it is clear that, at this point, this type of speculation is not science and may never will be. And of course, scientists should be careful if/when they discuss this idea. They should make it very clear that this is not science until we know how to test it. The public is already on to scientists who make claims without evidence: they turn around and say they can also make similar claims about the supernatural world.

5. **Peter Lynds**
   February 21, 2012

   Hi Peter,

Good post. One thing puzzled me though. Why do you think the question "why is there something rather than nothing" is meaningless?

Also, I think I should note that, unless one believes that a universe can be magic'ed from truly nothing a finite time in the past (whether by a non-physically existing, timeless, eternal God, or by non-physically existing, timeless, eternal laws of physics, with both being to said to somehow magically bridge to the physical to create a universe out of nothing), eternal physical existence is the only other option, whether it be in the form of an eternal multiverse, eternal cyclic universe, or a finite universe in which time is cyclic. That is, and although it is very difficult to see how any of these models could realistically be confirmed empirically, they are still on a much firmer footing than any theory that posits physical existence had a beginning a finite time in the past. Also, multiverse obviously need not mean string theory.

Best wishes

Peter

6. **Anonyrat**
   February 21, 2012

   Why "Why is there something rather than nothing" is meaningless:- how do you formulate it as a scientific question which can have an experimental or observational test?

7. **David Metzler**
   February 21, 2012

   @Peter Lynds: I think you may be confused about what a universe with a finite past means. It doesn't mean that there was nothing, then there was suddenly something, in some dynamical process—that would involve a notion of time that went back before the big bang. I think the error is the common one of assuming that any space or spacetime must be embedded in something larger (and usually more familiar)—what mathematicians call the extrinsic point of view. But from the intrinsic point of view, a universe "starting with a big bang" is completely self-consistent, and need refer to no notion of "how it got started".

   (To use a pure-mathematical example, a finite open interval is just as natural a Riemannian manifold as the real line is, and neither needs to be embedded in something larger to be made sense of.)

   Pardon me if that misrepresents what you were saying, but I know that it is a major point of confusion with the lay public.

   Now, the likely quantum-gravitational nature of the initial few Planck times is still a huge source of uncertainty, so quite possibly the pure classical relativistic notion of a singularity is not realistic. But I feel that that is a different issue from the resistance many people have to the very notion of a finite (in time) universe.

8. **Peter Woit**
   February 21, 2012

   All,

   Please avoid the temptation to turn this into an opportunity to discuss your favorite ideas about the big bang and pre-big bang physics. I'm in a bad enough mood already. If it's not about what Krauss or Smolin have to say, it's probably off topic…

9. **Peter Lynds**
   February 21, 2012

   Hi Anonyrat,

   Yes, I agree that it cannot be formulated as a scientific question that can be experimentally or observationally tested, but as long as nothing rather than something is a possibility, it is still a meaningful question and in need of an answer. If one perhaps disagrees with this, one would need to show why nothing isn't a valid possibility (and in doing so, they would also actually provide an answer to the question). I've got my own ideas on this, but I'll keep them to myself here.

   Hi David,

   Yes, of course, but almost every modern cosmologist would agree that the big bang needs an explanation. What caused it? If the universe arose from truly nothing, we have 2 options (laws of physics or God). As I explained earlier, both require magic.

   Best wishes

   Peter

10. **Giorgio**
    February 22, 2012

    Peter,

    a simple question about the multiverse shows how nonsensical the idea is (I got it from a physics book for free download somewhere):

    Why do you believe in only ONE multiverse?

    Real men believe in many.

11. **Marcus**
    February 22, 2012

    Peter L, anonyrat, David, I don't think inflation has been mentioned in the current discussion. Part of the impetus towards multiverse thinking comes from the inflation mechanisms people dream up.
    As Peter L said cosmologists confronted with visible features of the early universe generally agree there should be an explanation for the big bang. That's not an impossible job there are a bunch of models—but the catch is that to get results that look right a lot of cosmologists think a period of exponentially rapid expansion has to be included. Once you think up and include a mechanism it becomes a headache, it keeps happening, or you need to fine tune it so it lasts long enough but then stops etc etc. So far most of the imagined inflation mechanisms tend to drag multiverse in on their coat-tails. Try to think of an explanation not just for the start of expansion but for the usually assumed amount of inflation, and try to make it not give you a multiverse.

12. **Marcus**
    February 22, 2012

I think too often people confuse the job of explaining the big bang with deep/ultimate questions like why do we have these physical laws and why does existence exist.

Explaining the big bang is just an obvious next step in gradual growth of understanding. Get rid of the singularity, go back in time from there, fit the available data. It's physics business as usual, no need to get philosophical and confuse it with ultimate questions.

13. **Sascha Vongehr**
February 22, 2012

What Krauss holds is similar to the reason why I have always expected the multiverse. Simply, a modest agnostic will. Apart from that, multiple world models are anyway tautologically true. This I have explained both here and in links:
http://www.science20.com/alpha_meme/modest_agnostics_expect_multiverse_and_hold_many_worlds_true-85038
The multiverse/MWI cannot excuse everything and string theory must not be beyond the reach of criticism, but it is time that those who poo poo the multiverse at every opportunity also address fundamental arguments that were never based on their pet peeve.

14. **Bane**
February 22, 2012

Is Krauss still a physicist of the first rank? Christopher Hitchens (his friend) used to say Krauss was the greatest living physicist. Not sure if he was right…

15. **Anon**
February 22, 2012

Krauss: "Maybe […] everything that can happen happens somewhere."

This is the kind of statement that makes my blood boil. Seriously, nobody calling himself a physicist should be allowed to poison the minds of the public with this nonsense. I have started hiding the fact that I am a physicist from new acquaintances from sheer exasperation at the futility of having to try to unteach them these kinds of ideas that they pick up from the media and believe to be mainstream.

16. **hdz**
February 22, 2012

Peter, I share your reservations regarding the many-worlds-hype (as far as it is a hype), but you have to differentiate:
Max Tegmark's level 1 appears trivial, since any possible state must exist within any given approximation at sufficient distance in a presumed infinite universe. However, proposed numbers for distances are usually quite unrealistic, since they are based on mere chance configurations (such as "Boltzmann brains") and do not consider an evolutionary universe of given age. (I have never seen realistic estimates for the rate of evolution of specific life forms per volume, for example, but I don't actually care for such trivial doppelgängers.)
If you give up homogeneity (as you do in inhomogeneous inflationary models, usually presented at level 2), you may speculate about all kinds of "landscapes" and ages, but any estimates must depend on your specific speculation – so here is the true hype.
The original many worlds concept is given by level 3 (Everett). They do not exist somewhere in space and time, but somewhere else in what we classically call configuration space. In contrast to all other levels, these many worlds are NOT science fiction, since they are solely based on the empirically well founded Schrödinger equation. (I would instead regard collapse theories or hidden variables, when used to avoid Everett's conclusion, as science fiction.) Unfortunately, David Deutsch introduced considerable confusion, when he turned Everett's proposal into science fiction by considering time travel between different "worlds" (in conflict with Schrödinger and decoherence, for example), or when he regarded quantum computers as calculating in parallel worlds. This parallelism would be no more than the superposition principle. If quasi-classical "worlds" split according to decoherence, quantum computers have to remain in one and the same world in order to be able to produce results that may be used in our world.
Tegmark's level 4, finally, seems to be based on a confusion between the concepts of physical existence (to be based on observations and experience) and mathematical existence (which means no more than consistency of a definition – usually within a given axiomatic system). This level does not seem to be relevant for physics at all (except that inconsistent formal concepts cannot be used in physics either).

17. **Peter Woit**
February 22, 2012

hdz,

Thanks for the clear outline of the various "multiverses", which seems quite sensible to me.

One of the more annoying aspects of multiverse mania is the tendency to throw some very different things all together. In particular, there's

1. The "multiple worlds" of decohered quantum phenomena, which are an interesting and very real topic we know a lot about theoretically and experimentally.

2. The cosmological "multiverse" of causally separated parts of what used to be called the universe. These may exist, but require a serious theory, since we have no direct experimental evidence. These are the ones that get exploited by string theorists, giving them whatever properties (different values for anything string theory should be able to explain but can't) they find convenient.

3. Different laws of physics. Once we understand what the fundamental consistent mathematical structure is behind the laws of physics, we may very well find out that it contains pieces disconnected from ours (with different values of some constant, different numbers of dimensions, different gauge groups, etc.). Then if one wants to think of these pieces as "existing", I suppose one can. But we're a long way away from this…

18. **hdz**
February 22, 2012

Peter: so we seem to agree!
I guess when you talk about the "laws of physics and the consistent mathematical structure behind them", you mean the empirically founded laws (that we hypothetically and consistently extrapolate beyond what we have observed). We should also be aware that some parameters in what we regard as laws may have come into existence by some "symmetry breaking". They would then not form fundamental laws, but characterize either specific Everett worlds or, if locally different, just regions of a "landscape" (without necessarily requiring string theory). Yes – we are very far from understanding it!

19. **Peter Woit**
February 22, 2012

hdz,

I agree. We have no idea now what determines the parameters of the Standard Model. Most of them just appear as mass/mixing matrices in the Yukawa terms coupling the Higgs to fermions. These may be calculable from first principles in a better version of the theory, or they may be characteristics of one particular state of the fundamental theory that we happen to locally be in. In either case though, you need a better theory than anything we have now.

20. **Kent Traverson**
February 22, 2012

According to this piece, one may be able to detect neutrons from another universe, thus a testable hypothesis. Is there any validity to it?

http://www.technologyreview.com/blog/arxiv/27517/

21. **Vince**
February 22, 2012

Sorry, this is off topic, but I think you should know that the supposed faster-than-light neutrinos are no more:

http://news.sciencemag.org/scienceinsider/2012/02/breaking-news-error-undoes-faster.html

Many pet theories have now been ruled out!

22. **Peter Woit**
February 22, 2012

Vince,
Thanks for posting the news!

Kent,

If one feels like it, one can certainly devote one's life to carefully watching particles, and if one suddenly disappears, you can then announce that it jumped into another universe. The problem of course is that you need to have some plausible consistent theory that would predict that this would happen. I don't think there is such a thing here.

You can easily come up with absurd theories that are completely implausible and explain nothing, then make a big deal that they're testable because they predict something bizarre will happen someday (even though such a thing has never happened before). I don't think this can really be called doing science though.

23. **Kasuha**
February 22, 2012

Personally I don't need God or Multiverse to explain our origin. Anthropic principle in the form "we see our universe because we are here to see it" makes me perfectly happy. And regarding arguments about fine tuning of fundamental constants – imagine a Julia set, the classic one. It is driven by just two constants. Change them just a little and you get a very different picture – may be similar overall for tiny changes but the better look you take at it the more fundamental differences you find. If fundamental constants were different, the universe would probably be very different as well but that does not mean it would not support its "fractal edge" on which we are all living. The edge would just be very different from what we are used to and there would be some totally different life forms contemplating how comes their universe's fundamental constants are just so exactly fine tuned.

24. **Bourgeois Nerd**
February 22, 2012

Having read Krauss's book, I do have to stick up for him a bit and say that he's neither convinced of, nor happy about, the possibility that all is randomness and there are no fundamental laws. He's simply resigned to the possibility.

As someone who is haunted by the question "Why is there something rather than nothing?" I'm curious why you think it a meaningless one, Peter. I also think the multiverse answer that Krauss and others give not terribly convincing. That one universe somewhere might be "nothing" seems rather besides the point.

25. **Shantanu**
February 23, 2012

Vince, thanks for the link.
Someone should ask all those who claimed that this is evidence for string theory,
whether now they think string theory is ruled out.

26. **Thomas Larsson**
February 23, 2012

The anthropic explanation of the Michelson-Morley experiment: a non-zero ether wind is incompatible with human life.

27. **harryb**
February 23, 2012

Peter

Back to topic.

I read Smolin's article and I'd agree its is much more balanced than most in this arena. Krauss's book, which I mentioned a few weeks ago on this blog, is also good stuff, but I also agree when he gets to the multiverse issue, its all arm-waving fluff, which is disappointing given the solidity he shows when discussing more standard physics.

Smolin appears to hinge his arguments on a philosophical maxim that he describes as "Laws must evolve to be explained". This allows him the space to therefore discuss landscape theories such as Linear Cyclic Cosmology, Eternal Inflation etc on the basis that our (current?) laws may merely be the latest manifestation of previous ones that were in existence in "deep time".

He finishes the piece with:

" The main lesson which can be drawn from the successes and failures of at-
tempts to resolve the landscape problem surveyed here is that theories which embrace the evolution of laws have a better chance to make falsifiable predictions than do theories which try to hold onto to the notion that law is eternal. "

I must admit I felt after this in a bid of a bad mood too. The obvious question of – well, what was the original law / theory? in deep time seems to be given over to String or M theory (or something else) by Smolin. At least he only discusses this and posits no others in detail.

True , Smolin points out that S/M theory is still unproven and not really going anywhere, and true he demands testability (eg he seems to rule out cosmological natural selection on observed data).

But although much more rigorous than Krauss, it still seems to be, in summary, something along the lines of – only a multiverse argument will provide the answers to deep principles, and those deep principles may well have something to do with string theory followed by evolution of its laws to create us.

I am sure Smolin would say that is too simplistic, but it sort of comes through no matter how many subtle caveats are used.

I can buy the argument we need to be more thoughtful re how we look at the early universe and what may have changed since then, or at that point. The current answers posed seem a long way from being a graspable deep truth however: Unless we embrace the multiverse, string vacua and symmetry breaking, and evolution via endless cycles.

Some may find this uplifting. For whatever reason, not sure why, I do not. There must be better answers than this. On a day when many grand, fluffy theories attached to faster than light-speed neutrinos have been brushed aside by faulty wiring, I remain in the camp of hoping for more straight-forward solutions.

28. **Paolo**
February 23, 2012

Thanks for recommending Lee Smolin' paper. I found it very clear and useful, in particular I liked the comparison between the various cosmological scenarios and the observations about AdS/CFT (By the way, I noticed a few trivial typos, nothing worth mentioning)

29. **Joel Rice**
February 23, 2012

That profusion of speculative hypotheses would be more convincing if they had a theory that dealt with what we observe. One can complain about the Anthropic Principle, but at least we are pretty certain that we exist, and it does point to curious properties of Carbon, but going on about possible different regions or epochs having different laws sounds like giving up on the idea that Nature knows more math than we do. GR explains the redshift of the CBR but not the spectrum, and QM explains the Planck spectrum but not expansion. That one gets from this predicament some need for different laws sounds like one is desperate to save the bacon. Are they willing to throw out Arithmetic ?

30. **Dan Winslow**
February 23, 2012

Doesn't what we think of as 'knowledge' have to run out at some point? Since no matter what explanations we derive, it's always possible to then say 'Yes, but what explains *that?*

It seems to me that we must inevitably arrive at the end of what is possible for us to know. That point might look somewhat like what we are seeing now. I personally don't like the idea, and I'm not suggesting we've reached it, but I think it needs to be acknowledged that we might at some point.

31. **JC**
February 23, 2012

The problem with all these BS from so-called high-energy/cosmology experts is the lack of any experimental guidance. I'm so happy that I work in a different area of physics where fascinating emergent phenomena can be studied both theoretically and experimentally.

I totally agree with the suggestion that we should put multiverse, landscape, … in the same category of the ideas of God(s), or the world is supported by infinite elephants, turtles, etc.

And I also think really smart math-oriented people should just do mathematics.

32. **Peter Woit**
February 23, 2012

JC,

I'll just repeat my usual comment that you can't blame the multiverse on mathematical physics. I don't think there are any equations in Smolin or Krauss, and that's par for the course in multiverse studies. The people who find the multiverse idea appealing tend not to be those fond of using sophisticated mathematics in physics, but those looking for "physical" ideas they can easily explain to their grandmothers. While I share skepticism about string theory with Smolin and Krauss, I suspect their attitude towards the relationship of math and physics is quite different than mine.

By the way, I don't completely disagree with the idea that if you really understand something you should be able to explain it to your grandmother. It's just that the explanation of some ideas would require said grandmother to sit still and pay close attention for quite a while. Others, like the multiverse, can be explained in minutes, with perhaps a few hits from a bong to help the understanding along.

33. **IT**
February 24, 2012

"Its much too a wild a night to travel in."
"Wild nights are my glory," Mrs. Whatsit said, "I shall just sit down for a moment and pop on my boots and then I'll be on my way. Speaking of ways, pet, by the way, there is such a thing as a tesseract."

But then Peter Woit appeared in the Murrys' kitchen, proclaimed "No there isn't" and shot Mrs. Whatsit dead.

34. **harryb**
February 24, 2012

Peter

How the web works…

Here is a cut and paste of your comments above already showing up in pro-Christian faith websites. It really is depressing to see Multiverse speculation undermining science in this way. Playing into the theologists hands it seems. No wonder you felt in a bad mood.

http://christian-apologetics.org/2012/peter-woit-on-the-multiverse-as-a-weapon-against-religion/

For interest here is the Dawkins / Krauss talk on the "something from nothing" debate from a few weeks ago. More nuanced.

http://richarddawkins.net/videos/644930-dawkins-krauss-discussion-from-asu-4-feb

Maybe you are right. Multiverse / string etc now urgently need data and equations. Otherwise its tennis with the net down, and priests and mullahs hitting it back easily from the other side.

35. **Anon**
February 24, 2012

hdz says: "Max Tegmark's level 1 appears trivial, since any possible state must exist within any given approximation at sufficient distance in a presumed infinite universe. "

This is not so trivial as it is often made out to be, since it depends on speculative assumptions regarding ergodicity, mixed in with speculative assumptions of infinite volume and even more speculative assumptions regarding QM and quantum gravity as applied to the cosmos as a whole.

36. **Allan Rosenberg**
February 24, 2012

Vince, all we can say with confidence is that the FTL neutrinos haven't been discovered in THIS universe.

37. **Nancy Reagan**
February 26, 2012

Thanks Peter,
I have been trying to get kids to: "Just say no to the Multiverse" for years. Maybe your message will help.

38. **pierre**
March 7, 2012

I too like the poster above am haunted by the question 'why is there something rather than nothing' and don't find it a meaningless question to ask. It seems to show the limitation of what we're capable of, in the sense that not only do we not know, but we can't even imagine an answer. I suspect if we were ever told the answer by some higher intelligence , we would look at them like a dog being shown a card trick, to quote the late Bill Hicks.

The problem I suspect is that the answer must be 'logical' for us to accept it, and we're confined to just one system of logic. We're trapped in logic flatland.

39. **Hansl**
March 9, 2012

Why is there something rather than nothing?
– Because the Man In The White Robe had a higgup
(sorry Peter, I had to get this out)

40. **Marcus**
March 9, 2012

Most apt, Hansl. The hiccup analogy underlies multiversal reasoning: once Andrei has imagined one hiccup he cannot think how the hiccups will stop. So the hiccups must go on forever—producing a multiverse.

Various and Sundry

February 27, 2012
Categories: Uncategorized

- Lots of people seem to be unhappy with my characterization of Lawrence Krauss's question "why is there something rather than nothing?" as meaningless. I'm well aware that one can give this question a non-trivial meaning, I just don't think Krauss does, nor do the many commenters here on the topic whose comments I've deleted. Happily for those of you who want to discuss this topic, the Templeton Foundation has funded a whole new institution, the Rutgers Templeton Project in Philosophy of Cosmology , and they now have a blog, called What There Is and Why There Is Anything. They give a long list of questions they want to address which are pretty much orthogonal to ones I find interesting, ending with

  > 13) Why is there something rather than nothing?
  >
  > I imagine that all of these will be discussed during the course of our project. However, I suggest holding off definitively answering question 13 until our grant has expired.

  So, go right ahead and help them out, but hold off on your definitive answer to this question for at least 3 years (if not more, they might want a grant renewal).

- Another new website is the all-new, shiny, WordPress-based website for the Columbia Math department. We needed a new site since the university software running the old one ("Hypercontent") was about to die. The new university plan, involving Drupal, didn't seem ideal to me, so I convinced our staff that WordPress was the way to go. Web designer Matthew Kressel did a great job setting up the site for us, and our staff member Nathan Schweer has turned it into a huge improvement over the old one.
- In other Columbia news, tomorrow there will be a panel discussion on Recent Developments in Access to Research, which will discuss the Elsevier boycott amongst other things. I'll be on the panel, not sure how much I'll have to contribute, we'll see.
- A correspondent sent me a link to this wonderful piece centering around Fred Hoyle and film.
- For interesting video to watch, I recommend this interview with Yuri Manin at the Simons Foundation, and videos from the Clay 2010 conference in Paris about the Poincare conjecture proof.

Update: I hadn't realized that "Why is there something rather than nothing?" studies is now a burgeoning field, with heavy Templeton funding. Besides the Rutgers Templeton Project in Philosophy of Cosmology, this past fall Yale hosted the Templeton + Yale Divinity School funded "WITA" (Why is there Anything?) conference (see whyisthereanything.org), which has its own blog here. As Multiverse Mania gets to be old-hat, perhaps WITA studies will take over as the cutting edge of this kind of science.

Update: This news from a "Cambridge University spokesman":

> It is not true that Professor Hawking is a "regular" visitor to the club in question.
>
> 'This report is greatly exaggerated. He visited once a few years ago with friends while on a visit to California.'

Comments

1. **Dave B**
   February 28, 2012

   Adam Curtis makes some superficially interesting documentaries, but they always leave me with the feeling that he is making the same mistake as internet conspiracy theorists.

   His films tend to establish tenuous sets of connections between interesting events, turn them into a narrative of "what really happened" and ignore everything else that might have been going on at the time. Cherry picking + over simplification essentially.

   The films are interesting, and contain plenty of fascinating trivia but after a while it becomes obvious that the visual style is mostly designed to distract the viewer so they don't notice the unfounded assertions and flawed logic.

   Having said all that I did like the quote about fish comprehending Yarmouth. Having been there I suspect that they might be able to 🙂

2. **CU Phil**
   February 28, 2012

   A group of philosophers/physicists at Oxbridge got a similar Templeton grant for philosophy of Cosmology:

   http://www.philosophy.ox.ac.uk/news__events/news/philosophy_of_cosmology_-_new_field_of_study

   I think the Rutgers grant and the Oxford grant are each in the neighborhood of a million dollars.

3. **Peter Woit**
   February 28, 2012

   Thanks Phil.

   By the way, I just noticed this:

   http://rationallyspeaking.blogspot.com/2011/11/templeton-foundation.html

   Not all our local philosophers are interested in Templeton funding….

4. **Bob Levine**
   February 28, 2012

   Peter notes:

   "As Multiverse Mania gets to be old-hat, perhaps WITA studies will take over as the cutting edge of this kind of science."

I'm not sure that the hardcore MM enthusiasts will let that happen. An obvious option would be to *subsume* WITA under MM by asserting that the MM predicts that there are universes within the multiverse where there is nothing, rather than something. (The question of whether or not there can be more than one such universe is left as an exercise for the reader) An elementary application of the Anthropic Principle then yields the observed state of affairs. The WITA problem thus simply goes away… a wonderful demonstration of the far-reaching explanatory power of cutting edge science, eh?

Don't laugh. I'm sure at least one of the MM heavies has already thought of this one. We'll know how bad things are when arxiv papers exploring this 'solution' start appearing.

5. **Guillaume**
   February 28, 2012

   I didn't know this Adan Curtis; he's got some pretty fascinating stuff on his blog! Dave, History his all about cherry-picking and interpretation, so unless you can show that Curtis' choice of facts and interpretation doesn't stand up, your criticism doesn't go very far…

6. **Dave B**
   February 28, 2012

   Watch the films and make your own mind up. I'm not saying he is definitely wrong, but while I do find the films interesting and entertaining, I can't really take the conclusions seriously. The connections are just too tenuous and far fetched for me.

7. **Christian Takacs**
   February 28, 2012

   When someone studies or examines "Why not something instead of nothing?" it is called Existential Angst, and can be cured by growing up and getting a good job. When someone studies or examines "Why not nothing instead of something?" it is called Nihlism, and can't really lead to anything (by its own definition), except perhaps a fatal overdose of drugs or self inflicted gunshot to the head (or foot). If a contemporary Mathematician wanted to model either of these "Why not" scenarios, all they would need to do is take the possible number of speculations in this universe and multiply by the number of multiverses, then multiply by (n+1) dimensions in an infinite series, divide by the number of papers that could be produced with "X" amount of funding in "T" amound of time, then multiply by zero. This formula should provide an extremely predictive and accurate answer.

   I must disclose that this experiment has actually already been performed by the very well funded "Deep Thought" Project. Unfortunatly, their results diverge from mine by exactly 42, and took… slightly longer to calculate. It has already been proposed that with even greater government funding and resources a third research venture might take place that will settle the discrepency between the two answers and maybe snag a nobel prize in the process!

   P.S.
   It saddens me that physics has come to the point where Douglas Adams begins to sound more visionary and less absurd than current theory.

8. **Giotis**
   February 28, 2012

   This is indeed a great initiative by Templeton foundation.

   I can't imagine a nobler human activity than to ponder about these fundamental questions exploring the origin of the Cosmos.

   This is what defines an advanced civilization.

9. **Bernhard**
   February 28, 2012

   Peter,

   this is a really nice response of yours to the critics and thanks a lot for the links to the proper places to discuss the question. As always, you do a a better job then asked for.

10. **harryb**
    February 28, 2012

    The Templeton funding issues has been around a while – plenty controversies eg between Richard Dawkins and Paul Davies because of the latter's Templeton funding and his view science is never free of "faith". MM and ST does not help Dawkins' view.

11. **Steven M.**
    February 28, 2012

    Regarding your involvement in "Recent Developments in Access to Research":

    As a Ph.D. physicist who works in applied science and not associated with any university or major industrial lab, I cannot access most journals unless I am willing to pay ~$25 per article. Needless to say, this hinders me from doing serious research (for free… my day job pays the bills). The question I have is why do journals need to charge at all? Reviewers (I am one) work for free. The editing is mostly done by the authors—for free. Publication is electronic and automated and costs almost nothing. Does anyone use printed journals anymore? So where is the cost that justifies the high price? This is a wall that prevent people on the "outside" from participating in scientific progress.

12. **Peter Woit**
    February 28, 2012

    Steven M.,

    This is what is behind the Elsevier boycott, and this has been an issue among physicists and mathematicians for a long time.

    In more and more fields, people are putting everything on the arXiv, making this literature freely available. To the extent they aren't, you also need to ask the scientists themselves why they aren't doing this.

    My impression, at least in math, is that mathematicians are moving toward non-commercial journals, and these are going to be cheaper, but not free. There is a significant amount of work involved in running a journal, even on the cheap, and someone has to pay for it. Note that there never was an ideal age in which journals were free. People's time is an expensive item, and even when journals are "free", someone somehow is paying for it. The existence of an arXiv of unrefereed, unedited papers is the one way you're going to get close to really "free" (although someone has to pick up the tab for the arXiv itself).

13. **Bane**
February 28, 2012

Hey Peter, your colleague, Briane Greene, gave a scintillating talk on the multiverse at TED in Long Beach this morning. You hear him speak?

14. **Peter Woit**
February 28, 2012

Bane,
Missed that one. For one thing I was here in New York with other things to do. I see there's a report here:
http://blog.ted.com/2012/02/28/the-multiverse-in-three-parts-brian-greene-at-ted2012/

15. **Geoff**
February 29, 2012

Brian Greene was scheduled to speak here tonight at the University of Utah. A massive storm rolled just prior to his talk so I'm not sure if it was cancelled. Regardless, and despite my scepticism of string theory, he took time out this morning to speak to the physics undergraduates which I thought was pretty classy.

John Morgan was out a few weeks ago and gave a talk to the math department. Out of curiosity does he ever need notes when he lectures or does he simply carry everything around in his head? It was pretty amazing to watch.

I'm grateful your colleagues took time to visit our little corner of the world.

16. **anonymous**
February 29, 2012

Briane spoke at the University of Utah tonight – sort of a lecture on general science (http://www.physics.utah.edu/index.php/component/content/article/105-2012/477-the-nature-of-things-keynote-lecture-dr-brian-greene) – no multiverse in this talk but I am guessing he worked in a special lecture for the physics department that covers more detail.

17. **Nicky Nichols**
February 29, 2012

There are other places on the internet where you can talk about speculative concepts in a rigorous way without being made to feel foolish. But not WITA which appears to be as blinkered as anything else I've ever seen. They won't want to get to the answers too quickly.

If you want to see something really impressive take a look at the incredible efforts of the amateur mathematicians over at the Xkcd bigger numbers discussion.

18. **Geoff**
March 2, 2012

I spoke entirely too soon. The first line of the student newspaper describing Greene's visit reads: "We are all holograms existing in multiple universes simultaneously, said Brian Greene, a renowned theoretical physicist, at a lecture Wednesday night".

19. **jean**
March 3, 2012

"When someone studies or examines "Why not something instead of nothing?" it is called Existential Angst, and can be cured by growing up and getting a good job."

Hum, Leibniz would have been glad to know the name of his illness. And to discover your (easy) treatment. Thanks Doctor.

20. **uair01**
March 3, 2012

Totally unrelated to the above, but I could not resist:
http://9gag.com/gag/3103879

21. **Sascha Vongehr**
March 5, 2012

Thanks for ignoring
http://www.science20.com/alpha_meme/why_there_something_rather_nothing_studies_now_burgeoning_field-87641

22. **ajkem**
March 5, 2012

@Sascha
How terrible for you that no one should notice your article in the 2 hours after you posted it.

23. **Sascha Vongehr**
March 5, 2012

@ajkem
You misunderstood. My meaning is that PW always ignores any argument I make that on fundamental grounds (not string hype) criticizes his consistent pooping on multiverse and many world concepts. So here I thanked him in advance for what he will surely do, namely ignoring. Which is fine; he will have his reasons; just that I will henceforth also ignore him more. Nice links here often, but it sometimes sounds way too much like creepy crackpots' "oh those establishment physicists with their obviously wrong nonsense left common sense behind".

24. **Peter Woit**
March 5, 2012

Sascha,

Yes, I'm really just not at all interested in the sort of multiverse argumentation that you are, hope you find useful the links to other places on the web run by people that are interested.

I’m not attacking here “establishment physics”, since my impression is that it’s a majority opinion among “establishment physicists” that most arguments about the multiverse are a waste of time and not really science. Lenny Susskind may disagree with me, but I’ll bet David Gross and Edward Witten are closer to my point of view on this than Susskind’s. You don’t know very much about “establishment physicists” if you think Susskind is more of one than Gross and Witten.

I can also assure you that the Templeton Foundation is definitely not the scientific establishment. No matter who they get to agree to take their money, it’s a well-funded fringe organization trying its best to muddy the distinction between science and non-science, for its own ideological purposes.

25. **Sascha Vongehr**
March 7, 2012

Wow – a reply.
“the sort of multiverse argumentation that you are”
If you knew(understood) *my* argumentation, you would either reject it with arguments or become much more receptive to the concept.
“I’m not attacking here “establishment physics”, …”
I wrote: “sometimes *sounds way too much like* creepy crackpots’ “oh those establishment phys…”
Thanks for lecturing me about Templeton and who Lenny and Ed are as if I don’t know them personally, all proving that you do not know “the sort of multiverse argumentation that” I am interested in (for example my harsh critic against Lenny and Max Teg).
I am out of here – your answer was arrogant.

26. **ajkem**
March 7, 2012

@Sascha
People who live in glass houses …
Try not to behave like a petulant child and others might be more receptive to you.

Moriond 2012

March 4, 2012
Categories: Experimental HEP News

The LHC will next week enter a Machine Checkout phase for the 2012 run at 4 TeV/beam, with beam commissioning scheduled to start March 14, the physics run April 7. Meanwhile, the LHC experiments have been for months targeting the Moriond conference which starts today as the time to release their latest analyses of the 2011 LHC data. There is likely to be not much new on the Higgs front from the LHC, since the Higgs results were fast-tracked and released back in December. One thing to expect is further evidence that supersymmetry is hiding very effectively.

The big news is likely to come from the Tevatron, with D0 and CDF releasing their combined Higgs results based upon the full Tevatron data set (the machine was shut down for good last September). The Tevatron data is not enough to provide convincing evidence for a Higgs at the 125 GeV mass now expected based on LHC results. The most likely result is something much like the last one (the Summer 2011 combination is here, and they only have 25% more data since then). Some excess would provide a bit more support to the possibility of the Higgs at 125 GeV. More interesting would be the much less likely result that the Tevatron could rule out a 125 GeV Higgs, in some contradiction with the LHC results, although the Tevatron is mainly sensitive to a different channel than the LHC.

The initial schedule had the big news this morning, SUSY tomorrow, but a revised schedule has put off the most newsworthy announcements until Wednesday (Tevatron Higgs) and Thursday (SUSY).

For some reason, no one has seen fit to leak to me the Tevatron results. If this changes soon, rumors will appear here. Otherwise, since on Wednesday I'm heading off for a 10 day spring break vacation in Paris and Iceland, your best bets for Moriond news will be the usual reliable locations: Resonaances, Tommaso Dorigo, Matt Strassler and Philip Gibbs.

Update: Moriond slides are here. LHCb has a new result constraining CP violation in Bs decays to close to the SM value, see here, press release here. Jester reports on some details from last week about new CDF Higgs results, indicating that maybe the Tevatron will report an excess as expected. Matt Strassler also discusses results from last week, these from CMS, reporting that the multilepton events he got so excited about last year weren't anything to get excited about.

A reliable rumor-mongering commenter here warns us to take a look at the new LHC fermiophobic Higgs results coming this week.

Comments

1. **M**
   March 5, 2012

   LHC analyses about fermiophobic higgs could be interesting…

2. **ohwilleke**
   March 5, 2012

   "Paris and Iceland"- weird travel agent? looking for variety? some crpytic link? Paris is spring is famous, but maybe you'll have volcano weather too.

3. **Peter Woit**
   March 5, 2012

   ohwilleke,

   Iceland is on the way back home from Paris, courtesy of Icelandair, and I've never been there. Hoping to see Northern lights, a volcano, geysers, glaciers and waterfall, ideally all at the same time, while trying tasty putrefied whale meat.

4. **Anonyrat**
   March 5, 2012

   Some Iceland in photos.
   http://www.parrikar.com/blog/category/iceland/

   Hope you have a great time!

5. **Anonyrat**
   March 5, 2012

   (Those photos are by a friend).

6. **pakri**
   March 5, 2012

   What is a fermiophobic Higgs ?

7. **Anon**
   March 5, 2012

   You've missed the : after http in the link to the slides.

8. **Peter Woit**
   March 5, 2012

   Anon,

   Thanks, fixed.

   pakri,

In my not well-informed understanding, in “fermiophobic” models, the Higgs couples to itself and gauge fields to give electroweak symmetry breaking, but it doesn’t couple to fermions (no Yukawas). Maybe an expert can say more, I’m definitely not one, and it’s not clear to me that it makes sense to set Yukawas to zero once you have something coupling like a Higgs to gauge fields. We’ll probably hear a lot more if there’s an unexpected LHC result about this.

9. **jon**
   March 8, 2012

   About fermiophobic Higgs: there is this new ATLAS paper where with 4.9 fb-1 of data they see a 1.6 sigma excess at 125.5 GeV. So while not at all definite I’m quite curious to see what will happen with more data…

10. **jon**
    March 11, 2012

    Apparently CMS in a recent paper about their own fermiophobic searches have looked at 4.8fb-1 of data, and saw a 1.2 sigma excess at about 126 GeV. Very similar to what ATLAS sees then, but of course these are small excesses.

    Still, I wonder what experts think. And also, how many sigmas the combination would provide (hopefully someone like Phil Gibbs may be interested in doing a quick unofficial one…).

Tevatron Higgs Results

March 5, 2012
Categories: Experimental HEP News

The combined D0 + CDF Tevatron results on the Higgs are scheduled to be announced Wednesday, but it looks like this web-page may have jumped the gun a bit, listing the new results (based on "up to 10 inverse fb") as:

> SM Higgs is excluded between 147 and 179 GeV there is a greater than 2-sigma excess observed at low mass.

The summer 2011 combination excluded at 95% the mass range 156-177 GeV, so the new results extend this slightly higher and ~~11~~ 9 GeV lower. The most intriguing aspect is the "2-sigma excess at low mass", which is about what you might expect them to be seeing if there really is a 125-6 GeV Higgs as the LHC data suggests. In the H->b bbar channel that the Tevatron is most sensitive to (and that the LHC is not sensitive to) the expected signal is very wide, not allowing much of a fix on the Higgs mass (see Resonaances for more of an explanation of this).

Details to appear Wednesday, at

http://tevnphwg.fnal.gov/results/SM_Higgs_Winter_12/

Update: Matt Strassler has written a long posting to provide context and caveats for these results here.

Update: Results described here a couple days ago now official (2.2 sigma). Details at the usual recommended places (Dorigo, Gibbs, Jester, Strassler). I'm teaching a class, then off on a plane…

Comments

1. **Nex**
   March 6, 2012

   If more LHC data eventually rules out Higgs around 125 GeV the fact that pretty much every experiment is seeing some bump there will need good explanation.

2. **E.S.**
   March 6, 2012

   156-147 = 9 …

3. **Peter Woit**
   March 6, 2012

   E.S.,

   Oops, I supposed I should proof-read these things… Fixed.

4. **Craig**
   March 6, 2012

   Nex,
   Re a good explanation. There were a bundle of experiments that reported observation of a pentaquark a few years back. Eventually some negative searches were reported, from Fermilab experiments and elsewhere, and the pentaquark disappeared. I don't recall any of the experiments that reported positive sightings providing good explanations, or any explanations, of why they saw something. Of course, although the pentaquark "discovery" did make the news, it's not quite the Higgs!

5. **JRPS**
   March 8, 2012

   To Craig:

   You are right not all experiments for the pentaquark gave detailed explanations of why they saw something. But some people closely related to the experimentalists did, despite initially supporting the pentaquark . Some of these papers appeared in nucl-th and may have been unnoticed to people reading hep-th or even hep-ph. For instance I would suggest Phys.Rev.Lett. 105 (2010) 092001, e-Print: arXiv:1008.4978 [nucl-th] (To my view that was the last nail in the coffin, but came out too late, when basically just one experiment still supported the pentaquark). Of course, showing why a result which nobody believes anymore is wrong, does not get much attention. But that kind of works are done. I agree with you it would be nicer if the experimentalists had done it themselves. Some of them do and some don't.

   This is an example of why I give little significance to the "confidence level" claims. But there are many others. Quite often in Particle Physics we have seen "many sigma" and "99% C.L." claims that go away due to some systematic effect, which is well explained later… or not (the pentaquark, R_b, FTL neutrinos, and so on…). You can make a fantastic job with statistics, that is useless if you do not understand your systematics.

6. **guest**
   March 8, 2012

   Hype about strings=BAD. Hype about the Tevatron=GOOD.

7. **guest**
   March 8, 2012

   Or in other words: when will you tell your blog readers that the latest splash on Higgs searches at Moriond came from ATLAS results?

8. **jpd**
   March 8, 2012

   results from strings=NONE. results from the Tevatron=INTERESTING.

9. **Peter Woit**
March 8, 2012

guest,

1. I'm on vacation.
2. As usual I suggested other places that do cover these things much better than I do, including the ATLAS results in detail.
3. I try to post news here that is not available elsewhere. The Tevatron news was a good example
4. It's great that ATLAS is able to rule out lower mass ranges, but that now is not where people are expecting to see something. The big question is what is going on at 125 GeV, is it the Higgs? And ATLAS has nothing much new to say there.
5. The Tevatron seeing a marginal signal in a new channel, consistent with 125 GeV Higgs, is news.
6. This is probably the last major Tevatron result we'll ever hear. The victors at CERN who have put it out of business should be gracious.
7. I don't think there's any danger that ATLAS and CMS results at the LHC will receive insufficient attention.

Back to vacation…

10. **guest**
March 8, 2012

Thanks for the detailed answer!

1. Apologies.
2. Yep.
3. OK
4. Strongly disagree. Among other reasons ruling out that extra chunk increases the significance of the bumps, by reducing the LEE. And ATLAS had also the potential of directly reducing the significance by enlarging the dataset analyzed.
5. Yes, it's news. Marginal news.
6. On this I agree.
7. Right.

Thanks in any case for the info.

11. **anonymous**
March 8, 2012

Who cares about puny 2-sigma Higgs results? Have you seen Daya Bay's very convincing 5-sigma neutrino result? That just killed the tribimaximal mixing theory dead and gone and opens the door to studying CP violation in the leptons!

12. **guest**
March 9, 2012

Who cares? Anyone with the right understanding on what's more fundamentally important.

13. **A.**
March 16, 2012

Totally off-topic, sorry, but I just saw this postdoc ad. for a position in "the philosophy of cosmology" in DAMTP. Is this just a very silly choice of title or has Cambridge really gone down hill?

http://www.damtp.cam.ac.uk/vacancy/#RAPhilosophyCosmology

14. **Chris Oakley**
March 16, 2012

A,

I would opt for the latter explanation.

15. **Andym**
March 16, 2012

Definitely, downhill all the way.

Templeton Millions

March 16, 2012
Categories: Multiverse Mania

I'm still on vacation for a few days, but will take a quick break from sitting in a hot tub watching the Northern Lights here in Iceland for a short blog entry.

The Templeton Foundation has just announced a plan to honor the centenary of the birth of Sir John Templeton by giving $5.6 million dollars to physicists and astronomers willing to work on four "Big Questions" of a philosophical sort about cosmology. The multiverse is of course one of them. This program will be run out of the University of Chicago and led by astronomer Donald York, who surely was chosen for this partly because he's an evangelical Christian:

> …plenty of scientists are religious. Take Donald York, PhD'71, the Horace B. Horton professor in astronomy & astrophysics, the Enrico Fermi Institute, and the College. Founding director of both the Sloan Digital Sky Survey and the Apache Point Observatory, York is also an evangelical Christian who served as Intervarsity's faculty sponsor from the mid-'80s through mid-'90s. "I don't try to make the literal resolution" between science and Christianity, he says. "We're always changing and growing, and some things are acceptable at different times." To him, "Science is a story just like the religious stories."

A commenter points out here that DAMTP at Cambridge has just posted a job ad for Templeton-funded hiring in "Philosophy of Cosmology". Note that this hiring is not in the Philosophy department but in the physics department. The announcement says that there will also be a similar job at Oxford. The "Philosophy of Cosmology" grants used to fund this and similar positions in the US seem to involve at least a couple million dollars, more here and in my earlier blog entry about this.

Normally I try and avoid editorializing directly about news like this, but this time I'll make an exception. I think what is going on here is very dangerous. The Templeton Foundation's agenda is not the advancement of science, it is the advancement of a particular religious point of view about what science is and how it should be done. They are very cleverly putting large sums of money into backing theology and pseudo-scientific research at the most prestigious academic institutions in the world. One reason that these places are happily taking the money is because public funding is drying up. The organization is extremely wealthy, and now led by Templeton's son, who when he isn't spending his father's money on this is spending it on promoting Rick Santorum's political career or other far-right causes (see here for example).

At least in physics, some of those who can usually be counted on to do battle with the forces of religion have gone quiet. See for example Sean Carroll's posting about this recent funding, where he discourages commenters from criticizing the source of this money, since it's being spent on something he approves of. Seems to me that people in this field need to start seriously talking about the implications of this large new funding stream and its source, not suppressing such discussion.

Update: I'd be curious to hear from anyone at the University of Chicago who knows what the university's involvement with this actually is. The main page claims it is a project "led by the University of Chicago" and their logo is all over the site, but Donald York is the only University of Chicago affiliate listed (actually, he seems to be the only person listed, others are just "honorary"). Who at Chicago would have had to approve this, and is the university getting part of the grant funds from Templeton?

Comments

1. **Deon Garrett**
   March 16, 2012

   Part of my problem with this sort of thing is that I know that it's very purpose is to promote things I think are ultimately harmful. That's the major issue I have, and it's enough for me to oppose this sort of thing on its own. But even if you take Carroll's approach, there's such an overt agenda here that even if all intentions are honorable, it just invites skepticism. If you're going to do a real study showing that tobacco use isn't correlated with cancer risks, you can't work for a cigarette manufacturer.

   By the way, how are you enjoying Iceland? I've been here about 1.5 years, and I'm always interested in other people's impressions of the place…

2. **Nex**
   March 16, 2012

   I don't have the problem with this as long as I am getting my share of the money 😛

   No, but seriously, it does invite skepticism but I still think the balance is positive. I mean it's not like those funded by templeton will be able to sneak some false premises past their peers or falsify some data, all their work can be easily scrutinized by everyone else.

   The only valid risk I see is in promoting nonsense to the public, but that has been going on even without the templeton.

   Also I am genuinely interested if anything sensible can be said about some of the topics they fund and I don't see anyone else paying for that that kind of research.

3. **birth cry**
   March 16, 2012

   Robert Millikan was a devout Christian (Baptist I think). He honestly believed that cosmic rays were a signal of the `birth cry of the Universe', from the Book of Genesis. There is a Wiki page about this
   http://en.wikipedia.org/wiki/Birth_cries_of_atoms
   Millikan was quite serious in his belief, and went to Chicago (his hometown) many times to give public lectures to raise money for his cosmic ray experiments (this was in the 1920s or so). Whatever his motivations, and the source of his funding, Millikan produced serious science on cosmic rays with the money. Eventually Carl Anderson and others at Caltech had to distance themselves from Millikan. Millikan eventually recognized the indisputable scientific results from his work. He produced serious science whatever his personal religious beliefs and motivations.

   I suppose the big difference is that Millikan set his own agenda (of science) and found people to fund it. The difference today is that the funding agency (Templeton foundation) sets the agenda. It remains to be seen what actual science is produced, and if it does not support any evidence for the Templeton foundations religious point(s) of view. Yes this is all very much a product of the drying up of public funding. The science labs/institutes have to get money from somewhere. In earlier years (pre WW2?) eminent scientists did get funding from religious sources.

4. **Bernhard**
   March 16, 2012

   Following Sean Carrol's logic we should accept science being funded with drug money as well. If we continue on this path we will end up in the dark ages again.

5. **Foster Boondoggle**
   March 16, 2012

I think the main issue for scientists or other academics is the provenance of the funds, not what other things the Templeton foundation might support. The $ came from building a mutual fund company – not from selling addictive drugs or some other nefarious business.

And the funds aren't being used in some lame attempt to bribe practicing scientists to change their beliefs. No thoughtful person is going to decide to become a fundamentalist Christian just because they get offered a grant – or if they do, they're unlikely to retain the respect of their peers, and they also don't care what you think about them. From googling around about what science Templeton has spent $ on to date, it seems definitely biased towards origins and foundational questions, but hardly for trying to convert people.

Religion seems to be a basic feature of human society – you find it everywhere and in all cultural histories. Atheist though I am, I see no real problem with scientists taking money from believers to do their (the scientists') work, on the modest condition of showing up at conferences to talk across the divide about the philosophical implications of their studies.

6. **Peter Morgan**
   March 16, 2012

   Although you imply that the "point of view about what science is and how it should be done" is a "particular religious" point of view, I take ideas such as Donald York's, that "Science is a story", to be culturally more widely held than merely religious. Comparable ideas are, to limit it to my first hand experience, fairly common amongst academics in the humanities. Some of the claims one hears for various mathematical models are fabulous enough to be generously open to critique by a rhetorician.

7. **John Romeo Alpha**
   March 16, 2012

   In economic terms, it's probably a positive thing that these funds are being circulated from the religious sub-economy into the scientific, rather than the foundation just sitting on them, or spending them strictly within their own sect in a "buy-" mindset. If there's a net cash flow from religion to science, too, that seems like a better direction for the deficit than the other way. When scientific institutions and foundations start financing religious research to the extent that the net flow goes the other way, we should worry more. Like all that money Steve Forbes spent on presidential campaigns, rational reflection recognizes it to be a waste in terms of first order return on investment, but all the baristas and bus drivers and advertisers on the second and extended orders of economic activity supported by those funds benefited.

8. **Alp**
   March 16, 2012

   Surprisingly, I find that the scientific goals of such scientists might often align with the rest. For instance, from the movie Religulous:

   > Maher meets with both a Vatican astronomer and a priest, both of whom challenge the stereotype of Catholics. Maher asks why the Vatican would have an astronomer or be interested in science and the priest says that "Well, I can tell you that we are not here trying to find other planets just so we can get to them and convert everyone — and beat the Mormons to it.

   Even though I am not religious and I would be happier it did not happen, I have no right to impose my views on what others should choose as worthwhile. I acknowledge that people have every right to support *any* kind of research with their own funds. After all what good would wealth do if one cannot support the ideas one believes in? This is also true for funding of political candidates. I see this as a consequence of sacred economic liberties. On the other hand it's quite dubious if people have any right to spend others' wealth for a particular kind of research (i.e. taxpayer funding).

   Although it's sad that appointments might've been made not entirely by merit, that is often the case in many academic research positions. It's easy to see the hypocrisy of liberal academic establishment when they choose to ignore certain elements deciding research. Politics, just like religion, play a tremendous role in research. Just look at what motivates the so called climate science.

9. **Brathmore**
   March 16, 2012

   Peter,

   Normally I'm a staunch supporter of this blog and your viewpoints, but this time I think you're over the top. It almost sounds like you'd like to blacklist any scientist with a specific religious belief, which seems at odds given that you're upset about the blacklisting of physicists who criticize string theory.

   You refer to the "dangerous…advancement of a particular religious point of view." And what, precisely, is that "particular" point of view? Evangelical Christianity? Christianity more broadly? Theism? Any belief in God? It's hard for me to believe that the Templeton Foundation is really now a front for Evangelical Christianity. (Surely you recognize that there are widely different strains of Christianity, right? To equate an evangelical christian with a Catholic, for instance, is as absurd as a lay person equating a physicist and a biologist, simply because both are scientists).

   In my view, dogmaticism itself is an evil – whether it be found amoung the religious believers or scientists. One should be careful that in attempting to replace religious dogmaticism, one doesn't simply replace it with dogmaticism of a different kind. Your most recent post seems to do just that.

10. **TCSF**
    March 16, 2012

    I'm sorry to comment in a thread that's rapidly decaying (we already have the usual attempts to deflect the slightest criticism of religion with accusations of dogmatism, and it didn't even take 10 posts for what looks like climate denialism to show up).
    However, I think Peter Woit is spot on. Templeton millions pumped in is going to make it less likely people will feel free to speak their minds if they are critical of religion. The Templeton people have an agenda, and there's no question that buying some scientists is meant to favor that agenda–whether or not there are explicit strings attached. Now they (Templeton) are free to do what they want with their money, but can't people be free to criticize them–and their agenda?

11. **Dan D.**
    March 16, 2012

    I also very much enjoy this blog, but I also disagree with Woit's stance on this. I don't see how the mere funding of physics research by a religious institution is dangerous in and of itself, no more than any government funding for applied research where a certain end goal is envisioned is dangerous.

    Actually, I was about to say that I appreciated some of the comments that were defending religious believers, or at least not outright attacking them. I'm more used to science blogs these days taking potshots at them whenever they can, and the comments section being a cesspool of irrational anti-religious rage. I found this thread to be on the whole balanced by those who both agree and disagree with the idea of Templeton funding physics research (though I agree with TCSF regarding the climate denialism).

    I myself am a scientist who is also a religious believer. Now I'll be the first to admit that there is a fundamentalist anti-science strain in Western Christianity these days, and I'll link arms with my non-believing colleagues to promote the cause of science over and against these influences. But, I part ways when I start hearing non-religious scientists imply (note: NOT saying that Woit is doing this) that to be a “pure” scientist, one has to eschew religion, or even that being religious and a scientist at the same time is grounds for suspicion of one’s scientific integrity (though, I sometimes get the opposite from certain religious circles!). I would think that folks would welcome religious institutions becoming interested in science again and wanting to fund it, or at least not immediately cry foul. After all, for the vast majority of the history of science, this was often the case. There are other models for the relationship between religion and science other than just warfare. Not every religious institution that wants to get involved in science is doing it for nefarious reasons. Maybe Templeton is, maybe it isn’t, I don’t know. I guess it remains to be seen.

My $0.02.

12. **Low Math, Meekly Interacting**
March 16, 2012

If the researchers can ignore the source of funding, it’s not dangerous. If they can’t, it is. The latter possibility is a legitimate criticism of research being funded by pharmaceutical companies, and the intense scrutiny such research is subjected to is warranted. I figure the same standard should be applied to any funding agency which has other mandates besides the advancement of pure science. Whether it be a profit motive to satisfy shareholders or a philosophical motive to satisfy a particular religious viewpoint, vigilance is hopefully a reasonable countermeasure. I’d stop short of advising people to refuse the money. If one can get both the money and assurances that the research needn’t make the funding entity happy, then there’s reason to be optimistic a healthy balance can be struck. In essence, I’m saying if the recipients of these funds can be good at gaming the funders, why not take the money?

13. **Roger**
March 16, 2012

How can it be bad for Templeton to spend money on multiverse research when the NSF and others are spending 10x or 100x as much on it. Do you want the govt to be funding the foolish research for some ideological reason?

14. **harryb**
March 16, 2012

I have to say I am with Peter on this one. And in many ways the concerns this blog has with String Theory is the concerns it should have with Templeton-Science (I suggest we distinguish it clearly).

Freeman Dyson commenting in this months New York Review of Books on Margaret Wertheim’s Physics on the Fringe notes that the weirdness of the theories proposed (including in Wertheim’s view ST) is “what happens when imagination loses touch with observation”.

When this happens religion and other faith-based obsessions eg circlons, ethers, etc are as good as any science, because neither has to provide observable evidence.

Religious belief and Multiverse theories are thus inter-changeable, and the Templeton funding, clearly recognising this, will provide the impetus for unconstrained conjecture via “faith-based” science. Watch this space.

I believe ST, Templeton-funded, and other faith-based science should be clearly labelled as such.

One will argue that great science (as Dyson does in the article above) especially in frontier fields, often uses imagination and creativity about things yet unknown and unseen. Fine.

But those pursuing those answers should at least declare up front – I believe in a traditional Christian position (or other equivalent religious story) about ultimate truth which underpins my over-arching theory about life, and the universe, and my personal moral and intellectual belief-system, and/or am funded by a trust so disposed –

– before we read your arXiv paper.

Or,

I am not religious, do not believe in a Christian or any other diety, and am not funded by any religious or quasi-religious group, and have carefully, objectively, as far as is possible on this earth, produced this paper for your consideration.

Now read on…

15. **Alp**
March 16, 2012

harryb said:

> I believe ST, Templeton-funded, and other faith-based science should be clearly labelled as such.

But the entire problem is who decides what is faith based and what is not. Some would say ST is a religion and some would say the same for Keynesianism or warming activism.

This appointment in question is an entirely voluntary. If Mr. York was not happy with the strings attached, he has every right to seek alternative employment.

Surely one can pass judgment on the credibility of the research. But people have a right to research whatever they want to, especially if somebody but the taxpayer is willing to fund it.

16. **harryb**
March 16, 2012

@Alp

I don’t disagree – of course – that you are free to research what you will.

My concern is more basic.

If Templeton millions are passed into faith-based science, then it is very likely that such science will grow in importance in the public debate, which is in America at least , pre-disposed to that way of thinking.

Your examples are interesting:

String theory – indeed, I made the same point and I am very depressed by ST having lead us to this predicament – the string theorists have let imagination, images of mathematical "beauty", and so on take the fundamental of physics into a meta-physical minefield. It used be said that if a scientist from the 1700s walked into a science lecture today, he would be utterly bemused, but a philosopher from that era would see the same debates underway in today's lectures. I think now Saint Thomas Aquinas may be very comfortable attending Strings 2012.

– Keynesianism – fair enough, but no-one is invoking biblical stories in economics (much)

– Climate activism – now there is the problem. Chip away at particle physics, and you can have a go at it all. Super-luminal neutrinos – hah, the scientists got it wrong again! Oh wait, its been corrected – but anyway, they're probably wrong again so why believe in light's speed anyway. Evolution, same thing. And so it quickly goes.

No-one seems to argue about eg the tensile strength of steel, and inorganic chemistry. These seem to be value-neutral science pursuits. Albeit they rely on particle theories.

But take cancer therapy – that's still hotly debated, and in the Emperor of All Maladies Siddhartha Mukherjee notes how the ideology of radical surgery pursued by physicians possibly held progress back for decades due to their unswerving faith in the procedure, resistance to data and so on

So I agree we should sniff out biased (faith-induced) thinking wherever we see it.

My point is the Templeton approach just exacerbates and supports that style of bad science – data-free, issue-fixated – in potentially a confrontational way.

And propels it towards an American public already clearly predisposed to hear that science that conflicts with their moral / life-style belief system is not worth listening to and shoul be actively resisted eg climate change, evolution, stem-cell therapy, (and vice-versa – if creation myths are as good as or better than Big Bang – says a scholarly article funded by Templeton – and, even better, written by a declared Christian, why then should the believing public at large resist and question it? Its better than that string nonsense…)

We created the science method surely to stand outside that situation and it has brought us many gains. But, as David Deutsch notes in the Beginning of Infinity, Enlightenments in science and thought are by no means certain to survive, and historically all previous instances were eventually snuffed out by pessimistic ideologies.

It may be an exaggeration to state that ST and Templeton-Science could start the process whereby our present scientific advances back inside the ideological tent, and stifle progress once more.

But it may not, and our sensitivity to even the chance of it should remain high.

Let's see where the $5.6m dollars-worth of articles and conferences etc takes us in the coming months and years. I hope to be proved clearly wrong in my cynicism – but the balance of proof is with Templeton-Science for now.

17. **Eric Habegger**
March 16, 2012

Bait and switch is a time honored tactic not just in advertising, but in all human affairs. Many of the tactics written about by Machiavelli for overcoming ones adversaries were simply variations on this theme. While many people claim reasonable people can come together on important topics such as science it should be noted that people both fundamentalist scientists and atheist scientists who investigate and theorize about the multiverse have a lot in common: they have both given up the ground that science must ultimately be about things that have the potential to be observed and measured.

Who is to say that some time in the future after everyone has been cajoled into believing that the multiverse is science that both fundamentalist and atheist sides agree that both heaven and hell are legitimate subjects of science to investigate as part of the multiverse environment.

What's next? How many angels are there on the head of a pin?

18. **Alp**
March 16, 2012

@harryb

Good points and I definitely understand what you are getting at. I gave the examples ST, Keynesianism, climate debate just because I see credible people on both sides of the argument — despite some academics would disagree. For the same debates, I also see examples of data fudging/truth distorting to fit the desired intentions. All of these people would claim they were being as objective as possible as well as the racial scientists of world war 2 era.

Even though I'd like to agree there must be good criterion for testability as to what good science is, I am more worried if we push in this direction we may end up with a future where science is decided by the government or elites and debate is suppressed. This is not as farfetched a nightmare as I often see academic bullies who want to suppress contrary opinions on ST/Keynesianism/warming debates. I always thought Peter has been a victim of such behavior by others and that is why his post surprised me.

As I mentioned before I am not religious. But we are all human and being entirely objective is often impossible. Should the government have warned Einstein when he said "God does not throw dice" because he was letting religion cloud his judgment? Every researcher comes with their own baggage of bias. And often times such bias might've been instrumental in their reaching unique results. Shouldn't we let the results speak for themselves? Anything else would be self righteous.

19. **Typhoon**
March 16, 2012

Grant-whoring has a long and venerable tradition in the sciences.

The Templeton Foundation is simply making good use of this reality.

20. **Ryan Budney**
March 16, 2012

Rather than looking at this through the eyes of using grant money to endow a particular religion or religious perspective with the approval of a research institution, you could turn your perspective on its head, Peter.

This could be an admission that the multiverse as an idea is inherently religious. The acceptance of religious funding frees them from the constraint of living up to scientific standards, in a certain formal sense.

21. **Peter Woit**
March 16, 2012

A few quick comments from Reykjavik.

I don't have any particular problem in general with religion or with religious people as scientists or funders of science. I do think though that it is very much in the interest of scientists to keep religion and science separate, and this is why an organization spending as much as hundreds of millions of dollars on a well-targeted campaign to erase distinctions between the two seems to me problematic.

Hi Alp,
I'm not calling for suppression of the ex-Sir John Templeton's right to have his money spent on whatever research activities he wanted. I'm just exercising my own right to criticize what he and his heir are doing and encourage others to think about the issues involved.

Roger,

On the whole, NSF/DOE panels mostly refuse to fund multiverse grants, because most physicists think this is not science. With Templeton funding of multiverse studies probably long ago passing the $10 million mark, it's Templeton that is putting up a large fraction of the money promoting this. The goal of course is to make this subject academically respectable and well-entrenched at leading institutions. Once that happens, the NSF/DOE panels will change their behavior. So, this is seed money targeted to changing the facts on the ground.

Deon Garrett,

It's a spectacularly beautiful place (and the hot tub thing was not a joke, pretty amazing experience…) Just got into Reykjavik this evening, will see more of it tomorrow.

22. **z**
March 16, 2012

About Iceland — is it true what they say about the water and showers – does it stink?

23. **Peter Woit**
March 16, 2012

z,

At a high-end hotel in the middle of nowhere, no (excellent water conditions in the hot tub). In Reykjavik, yes.

24. **paddy**
March 16, 2012

Tis the stink of satanic sulfur….and I meant it humorously.

25. **Joy**
March 16, 2012

As other major science funding sources include DOD, DOE, DARPA, DHS … I am hard pressed to consider Templeton as being any more toxic to humanity or science than the others.

@Bernhard
Re: "science being funded with drug money" Just ask the researchers at Columbia University Medical Center who pays their bills. I suspect you meant to say illegal drugs. However, according to the U.S. Centers for Disease Control and Prevention, annual US deaths due to prescription drugs now exceed deaths due to motor vehicle accidents. Not an innocent funding source either.

26. **Chris**
March 17, 2012

@Peter Woit
„*I don't have any particular problem in general with religion or with religious people as scientists or funders of science*…. but you would prefer the money to come from the taxpayer via the government with the choices being made by atheists whose views on what is acceptable science conform with your own.

27. **Paulibus**
March 17, 2012

I note that to Donald York, who will run the Templeton scheme, "Science is a story just like the religious stories." Science is indeed a story (that's what evolution has conditioned us to do; talk stories, endlessly). But science is a quite different story to religion. Except for outbreaks of la haute poppicocquerie (as the French might call string theory), science is based on the Baconian tradition of observing nature and making verified predictions about it. Religion makes some predictions about the physical world (often that it will end soon) but generally shuns this risky occupation. Perhaps that's why religion is fragmented, while science trends towards unification.

Better the two cultures each tend to their own knitting. Financally as well as morally.

28. **Peter Woit**
March 17, 2012

Chris,

I don't care much about the source of science funding, and not at all about whether atheists or believers are making funding choices. I do care if pseudo-science is funded and promoted as science, with the goal of institutionalizing it as such.

29. **harryb**
March 17, 2012

@Chris

You suggest funding from "atheist taxpayers" would be as problematic as Templeton funding. OK, as a thought experiment, let's say I win a major lottery and come into $100million. I decide to set up a 10 year funding of $10million a year on the following basis:

– no declared follower of an organised faith is acceptable – so Donald York, sorry no, and indeed LeMaitre in years gone by, but there you go, I am sure he would have got Templeton-esque funding, science would have got by

– no multiverse or ST allowed – well-funded already (see above) – unless it meets the next condition
– only Baconian principles of at least testifiability in some form in the near future – (this will be judgmental for sure)
– declared atheists acceptable / encouraged to apply – no need to be coy about it. People of uncertainty in religion fine as well, otherwise we have entered Orwellian territory.

Now. No doubt much to antagonise folk here – but let me note that as a card-carrying non-religious person, I would personally feel uncomfortable taking Templeton funding and wondering what type of peer review I would actually be coming under from Mr York et al. Its not that I would try to block its funding – but I am within rights surely to attempt to impose an alternative world-view as well.

One will argue that there is no such thing as objectivity and hence any ideology above is just as constrictive as any religion.

That is indeed a real problem the experiment exposes. But I would argue the key difference is this fund starts out with a belief in Nature that is objectively testable. And I would argue assessing criteria for fund money will constantly be a hot debate and so it should be – any faith-based person can go to Templeton with stories if they want. This fund ought to constantly test its own principles to ensure its not slipping down an ideological slope – hence testifiable / potentially testifiable ST is in.

It does not attempt as a starting point that ST, multiverses, or for example, Christian religious teachings, are received wisdoms, and hence undeniable. And it does not set the bar so low that any untestable multiversey landscapey billion-dimension stuff gets dollars.

I actually take a stronger position than Peter on this, because I think overtly religious funding with such low criteria will just expand bad science – watch next for a flurry of intelligent design arXiv papers. Almost by definition religion / faith is bad science and I have always had a problem with the attempts to reconcile the Two Magisteriums as Stephen Jay Gould tried.

I see I am heading towards Dawkins territory – so be it. I think both religion and science can handle such a fund being created.

The fund runs of course the risk of missing great strides in ST – but again Science sees no such risk as Templeton etc funding will allow ST to carry on.

Anyway – young physicists, whose million do you take ?

30. **Giotis**
March 17, 2012

But the multiverse refutes the apparent intelligent design as a mere illusion.

Normally Templeton should fund anti-multiverse research.

So Peter’s allegations don’t make much sense to me.

Question: FQXi is controlled by Templeton?

31. **Peter Woit**
March 17, 2012

Giotis,

Not all religious people reject evolution and embrace intelligent design. In particular, the Templeton Foundation explicitly supports evolution research and rejects intelligent design. Their agenda is to institutionalize not fundamentalist religions, but their “theology and science are all the same thing” philosophy (which is fine with the multiverse and evolution). They’re having great success at getting even scientists like Sean who don’t like theology to get into bed with them. Enough money will do that…

FQXI was started with Templeton money and seems to have been mostly funded by that in recent years. I don’t know what the current situation of FQXI’s funding is, and would be curious to hear from someone who does know.

32. **BJM**
March 17, 2012

Re:
Peter Woit says:
“I don’t care much about the source of science funding, and not at all about whether atheists or believers are making funding choices. I do care if pseudo-science is funded and promoted as science, with the goal of institutionalizing it as such.”

This seems at odds with what I thought was your previous concern that believers making funding choices could muzzle scientists on the subject of religion. If your real problem is any funding of pseudo-science, this is not how it was presented in your blog.

33. **Danny**
March 17, 2012

There’s a difference between Templeton funding researchers and Templeton funding Oxford/Cambridge researchers. As a result of smoking-causes-cancer, climate change, and a zillion poorly-excuted-but-overstated psychology experiments, the public is skeptical of scientists; however, the top academic brand names retain respect. I’m disappointed the heads of Oxford, Cambridge, and the other universities are willing to allow their brand to be attached to the sort of papers and headlines we’ll surely be seeing.

“Oxford physicists prove existence of God”, and so forth.

34. **harryb**
March 17, 2012

I read the pingback reference on Templeton funding to Jerry Coyne’s blog at whyevolutionistrue web site. In the reference he points to the Metanexus foundation website, part-funded by Templeton. If this website is the future (well-funded) face of science, then oh dear.

Chilling mix of spiritualism, brief riffs of scientific comment and then back to religious poetic sentiment. What to do with all that? – But many solid names have signed up, and no doubt will sign up.

Lot to be said for contrarianism.

35. **Peter Woit**
March 17, 2012

BJM,

I don't see what was unclear about what I wrote about this:

"I think what is going on here is very dangerous. The Templeton Foundation's agenda is not the advancement of science, it is the advancement of a particular religious point of view about what science is and how it should be done. They are very cleverly putting large sums of money into backing theology and pseudo-scientific research at the most prestigious academic institutions in the world."

As anyone who has followed my blog for a while can tell, I have zero interest in arguments about the existence of God, or who believes what on that topic.

36. **pakri**
March 17, 2012

Whatever the agenda maybe, surely the people who attend these meetings, and I assume they are scientists, will have to decide for themselves what they want to achieve in their life as researchers in science and what else in their more personal religious life. Whether the foundation continues to receive their support will depend on the scientists only.

37. **Dave Miller in Sacramento**
March 18, 2012

Peter,

I am a much more rabid atheist than you are: I grew up attending a fundamentalist church and am still angry about being subjected to years of threats of eternal torture in Hell (I never did agree to join the church as a child, but I did have ongoing nightmares due to the constant threats of Hellfire).

Nonetheless, I am much less concerned than you about the Templeton matter for a very simple reason: they are going to fail. I myself am a bit fond of occasionally musing over questions such as "Why is there something rather than nothing?", but we all know that such questions have pretty much nothing to do with serious, contemporary science.

All that Templeton is going to do is to add a small amount to the meaningless nonsense already published on such subjects, not just in the popular media but also coming from many academics in philosophy, theology, literary theory, etc.

Templeton will get some scientists to announce that God might exist? Old news – anyone who looks into the issue already knows that some scientists think God might exist. Maybe most scientists think God *might* exist, even many of us who think he probably does not.

The major war between science and religion is already over: old-time, literalist religions is dead for good among intelligent, educated people. Templeton cannot change that (and does not even seem to want to). All Templeton can do is encourage vapidly pointless declarations of the possibility of some higher "spirituality."

If I understand you correctly, your main fear seems to be that this will divert scientists from real, productive scientific work. First, the Templeton spiritualism really should not take much time, energy, or effort: playing soccer twice a week should hardly interfere much with scientific productivity, and it is hard to see why churning out some vague nonsense for Templeton should be much more demanding. And, any scientist who really has a good idea (not that common!) should find it so consuming that Templeton is unlikely to really distract him much.

I have trouble seeing Templeton as anything more than a very tiny addition to the cultural pollution in which everyone is already immersed.

Dave Miller in Sacramento

38. **Friend**
March 18, 2012

Peter,

I think you may be a little too dismissive. It's entirely possible that Templeton's religious views may inform them to seek the truth, whatever that may be, with the expectation that it will ultimately justify their faith at least in some part. That's their prerogative and does not interfere with scientific truth. It's difficult to prove the existence of something that is not well defined. The concept of "God" is not well defined. Some think in anthropomorphic terms which may seem silly to scientist. And others may think that God is some overriding principle of reason that governs how the universe works, which may seem like the antithesis to some religious views. So I don't think one can prove or disprove the existence of God. Ultimately we will have to judge by what is predicted by whatever theory is proposed by science or religion. Let's wait until we have all the facts or until we have mathematical proof one way or the other. Thanks.

39. **paddy**
March 18, 2012

To sum up my opinion: Galileo Galilei. He took his "grant money" and took his chances with more a potential downside than not being funded next year. But (in line with what I think D. Miller implied), Galileo (perhaps apocryphally) died saying "and still it moves". This is always the choice of a scientist…to take patronage but to remain independent nonetheless. And we pardon those who cannot sacrifice themselves…and those like Galileo who compromise a wee bit. Because they have been better than we probably could ever have been.

40. **Marion Delgado**
March 18, 2012

I posted a link to this on Chris Mooney et al.'s "Intersection" Blog at scienceprogress (was at scienceblogs, then Discover) He got a more proper Templeton grant, but it seems it's time to bring them up again.

41. **Sammy Coleridge**
March 19, 2012

Hello Peter:

There are many decent people who are concerned that the public schools seem to be teaching their children that they are no more than sentient robots existing briefly in a pointless universe. Scientists need to find neutral ground on which to engage these people in civilized debate. Analytical philosphy, which addresses words like the first person singular nominative pronoun and, again, 'universe' is that ground.

Great website, thanks.

SC

42. **chris**
March 19, 2012

wow.

the blog comments are a true testament of how far right the us has moved recently.

thanks peter for keeping it up and don't worry, there are readers who understand that perfectly simple and valid point you are making.

43. **Christian Takacs**
March 19, 2012

Hello Peter,

Your latest blog entry is a bit puzzling for multiple reasons.

#1. Newton, Descartes, Einstein, Bernhard Riemann, Pascal, Yes… even Galileo, Maxwell, Faraday, Cantor, Planck, etc… all of them and many, many, more believed in Religion and God. You would be wise to refresh yourself with the beliefs of a few of your predecessors in the fine sciences of Mathematics, Physics, Biology, Chemistry, Astronomy, etc. if only to gain some perspective of whose shoulders you are presently standing on, and perhaps realize that many before you found their Belief a source of inspiration, not an irrational impediment to understanding as you would entertain.

#2. Your elitist disdain of religion is quite evident, borderlining on contempt… But Peter, you are yourself a follower of a set of beliefs/premises based mostly in the completely abstract numerical reasoning you favor that entertains creation ex nihilo or Big Bangs from timeless voids, point particles that have zero extension but are assigned infinite mass, treat infinity like a mere number that can be operated on, and sing praises about quantum mechanics which requires renormalization in order to even function, a process it's own creators admitted was 'dippy' or wasn't mathematically valid.

#3 Your reaction to research being funded by the Templeton Foundation reveals your dog in the hunt, or political bias. Yes, you too have an axe to grind Peter. I am well aware of what politics pervades most universities, and how they depend from their mostly governmental sponsors. (You should review the farewell speech of President Eisenhower when he warned of the intellectual stagnation and group-think nepotism which would result from government funding of research.) You dislike the idea of anyone you are politically/philosophically opposed to competing with you for favor in your field. I'm kind of amused by this considering the irony of you going on at some length in your book about 'diversity' as being a possible solution to the present impasse in the field of physics. Perhaps you should have defined 'diversity' differently to keep the riff-raff out of your ivory tower.

#4. You have written a book about how ridiculous and wacko things are getting in the house of physics. Endless multiplying Multiverses, time travelling nonsense, logical paradoxes and contradictions galore, extra-dimensional loop de loops tied up with super strings… This was all done with your fellow peers in peer reviewed approval in your 'correctly' funded research, just the way you like it.

For you to look down on other people who are scientists, physicists, philosophers, and happen to have religious beliefs as corrupting and 'pseudo-scientific' when your own non-religious peers believe in things they can't prove, and want to redefine science so they don't have to, and fund it with the taxes of people 'clinging to their guns and bibles' whom it would both you and your peers have contempt for … it boggles my mind how you can't see the hubris and hypocrisy of your own argument.

44. **Peter Woit**
March 19, 2012

Christian,

I don't know how many times I need to keep repeating this: unlike many scientist bloggers I've nothing against religion in general or religious people, or religious belief. My attitude about theological issues is very simple: I just don't care at all. Other people do, with variable results.

I do care though about what people's religious beliefs cause them to do that affects our society in significant ways. The culture war going on in the US driven by fundamentalist religion is a big problem and one look at this year's US presidential campaign should convince anyone of that. About Templeton, I don't care at all what they believe, I do care about what effects their beliefs have on a field that I care about, and I've explained what my concerns are repeatedly here.

And, obviously, I'm well aware that Templeton is not the only problem here. There are very unfortunate tendencies in modern theoretical physics, that I go on and on about here and elsewhere. The Templeton problem is that they're effectively putting their money to work to make things worse.

45. **Friend**
March 19, 2012

Peter, it might help if you could point to at least one example where the influence of the Templeton foundation has lead to scientific error. Thanks.

46. **Peter Woit**
March 19, 2012

Friend,

The problem isn't "wrong" science, it's "not even wrong" science. More specifically, pseudo-scientific studies of the "Multiverse", masquerading as science, something that I'd guess Templeton has spent over $10 million dollars supporting in recent years. It's hard to quantify how much effect Templeton money has had, but the rise in Multiverse Mania has been significant, and the Templeton money behind it is part of the story. This topic has been exhaustively (some would say, obsessively, way beyond what is necessary…) discussed on this blog.

47. **person**
March 19, 2012

@ Friend:
Here's one example where Templeton funding has led to scientific error.

http://newsinfo.iu.edu/news/page/normal/14990.html

In this case the error stems from a misunderstanding of statistics and of the scientific method. Templeton apparently funding motivated someone with no understanding of science to "try their hand at it". It gives an example of what garbage can result when a science funding organization has a non-scientific agenda.

48. **Spencer Tracy Jr.**
March 19, 2012

Times sure have changed.
I used to say: "Show me someone trying to bridge the gap between science and religion and I will show you a religious person."
Now we have to add physicists shamelessly looking for a payday to the list.

49. **harryb**
March 19, 2012

Person,

Thanks for the post, depressing though it is. Takes us right back to the fundamental point of this blog and Peter's book.

Not even wrong – and I blame string theorists for letting this metaphysical claptrap get a more serious outing (and funding) – as they have played into its hands. The more Templeton funds ST receives, the more they should be viewed as contributors to the metaphysical / spiritual realm – Not Even Physics.

As a partial antidote I read Ian Stewart's 17 Equations that Changed the World. The familiar ones are all there – but the point he makes it that these equations (either relating two quantities, or providing information regarding an unknown quantity) really have impacted the world – for good, sometimes ill. Some writers suggest plausibly over 30% of US GDP is now indebted to QM.

Point is – where is all this well-funded conjecture / spiritualism type research taking us? I am not advocating a simplistic show the immediate pay-back type approach. But I worry the other extreme could take hold – well funded introspective guff about spirituality and hidden meanings. And whilst they fiddle about on this, research into other areas such as condensed matter that could improve people's daily lives may go abegging. So if Templeton wants a moral high ground, they ought to be careful of their preachy graciousness in this context.

They have no equations.

50. **Shantanu**
March 19, 2012

Peter, forget all this. Haven't seen any comments form you on Daya Bay result.
(Is it because neutrino physics doesn't excite you?) maybe someone should see if string theorists regard this as evidence for string theory 🙂

51. **Peter Woit**
March 19, 2012

Shantanu,

Well, I was on vacation, sitting in that hot tub…

More importantly, I just don't know that much about this, prefer to leave writing about it to those who know more. It's a significant advance in the subject, but I don't have anything of much interest to add.

Dyson on Fringe Physics, String Cosmology and Hermann Weyl

March 19, 2012
Categories: Uncategorized

The latest New York Review of Books has a review by Freeman Dyson of Margaret Wertheim's recent book Physics on the Fringe (which I wrote about here).

Dyson is much more sympathetic than most physicists to "fringe physicists" like Jim Carter who is the main figure in Wertheim's book. He compares Carter to William Thomson and Peter Tait, well-known 19th century scientific figures, while making clear that Carter's "Circlon" theory is not worth taking seriously. He then goes on to discuss two cases of "fringe physics" that he had personal experience with:

> In my career as a scientist, I twice had the good fortune to be a personal friend of a famous dissident. One dissident, Sir Arthur Eddington, was an insider like Thomson and Tait. The other, Immanuel Velikovsky, was an outsider like Carter. Both of them were tragic figures, intellectually brilliant and morally courageous, with the same fatal flaw as Carter. Both of them were possessed by fantasies that people with ordinary common sense could recognize as nonsense. I made it clear to both that I did not believe their fantasies, but I admired them as human beings and as imaginative artists. I admired them most of all for their stubborn refusal to remain silent. With the whole world against them, they remained true to their beliefs. I could not pretend to agree with them, but I could give them my moral support.

About the later speculative work which he was exposed to as a student in Eddington's class at Cambridge, Dyson writes:

> Two facts were clear. First, Eddington was talking nonsense. Second, in spite of the nonsense, he was still a great man. For the small class of students, it was a privilege to come faithfully to his lectures and to share his pain. Two years later he was dead.

This sympathy for a great physicist who headed down a wrong path in his later years is easy to understand, but the case of Velikovsky is less so. Velikovsky was a well-known author of crackpot best-sellers starting in the 1950s (lots got explained by Venus and Mars moving out of their orbits and colliding with the Earth a few thousand years ago), and a neighbor of Dyson's in Princeton. Here's what he wrote as a proposed blurb for Velikovsky in 1977:

> First, as a scientist, I disagree profoundly with many of the statements in your books. Second, as your friend, I disagree even more profoundly with those scientists who have tried to silence your voice. To me, you are no reincarnation of Copernicus or Galileo. You are a prophet in the tradition of William Blake, a man reviled and ridiculed by his contemporaries but now recognized as one of the greatest of English poets. A hundred and seventy years ago, Blake wrote: "The Enquiry in England is not whether a Man has Talents and Genius, but whether he is Passive and Polite and a Virtuous Ass and obedient to Noblemen's Opinions in Art and Science. If he is, he is a Good Man. If not, he must be starved." So you stand in good company. Blake, a buffoon to his enemies and an embarrassment to his friends, saw Earth and Heaven more clearly than any of them. Your poetic visions are as large as his and as deeply rooted in human experience. I am proud to be numbered among your friends.

He goes on to explain:

> Why do I value so highly the memory of Eddington and Velikovsky, and why does Margaret Wertheim treasure the memory of William Thomson and Jim Carter? We honor them because science is only a small part of human capability. We gain knowledge of our place in the universe not only from science but also from history, art, and literature. Science is a creative interaction of observation with imagination. "Physics at the Fringe" is what happens when imagination loses touch with observation. Imagination by itself can still enlarge our vision when observation fails. The mythologies of Carter and Velikovsky fail to be science, but they are works of art and high imagining. As William Blake told us long ago, "You never know what is enough unless you know what is more than enough."

Dyson's sympathy for mystics, even ones spouting nonsense, is of a piece with his views on religion and science (which helped win him the Templeton Prize for 2000). These views are hard to do justice to here, if interested to know more, his 2002 review in the NYRB of a book on theology by physicist John Polkinghorne is a good place to look.

The review goes on to address a different sort of "fringe physics", the somewhat mainstream topic of "string cosmology", which Wertheim compared to the work of Jim Carter.

> Over most of the territory of physics, theorists and experimenters are engaged in a common enterprise, and theories are tested rigorously by experiment. The theorists listen to the voice of nature speaking through experimental tools. This was true for the great theorists of the early twentieth century, Einstein and Heisenberg and Schrödinger, whose revolutionary theories of relativity and quantum mechanics were tested by precise experiments and found to fit the facts of nature. The new mathematical abstractions fit the facts, while the old mechanical models did not.
>
> String cosmology is different. String cosmology is a part of theoretical physics that has become detached from experiments. String cosmologists are free to imagine universes and multiverses, guided by intuition and aesthetic judgment alone. Their creations must be logically consistent and mathematically elegant, but they are otherwise unconstrained. That is why Wertheim found the official string cosmology conference disconcertingly similar to the unofficial Natural Philosophy conference. The insiders and the outsiders seem to be following the same rules. Both groups are telling stories of imagined worlds, and neither has an assured way of deciding who is right. If the title *Physics on the Fringe* fits the natural philosophers, the same title also fits the string cosmologists.
>
> The fringe of physics is not a sharp boundary with truth on one side and fantasy on the other. All of science is uncertain and subject to revision. The glory of science is to imagine more than we can prove. The fringe is the unexplored territory where truth and fantasy are not yet disentangled. Hermann Weyl, who was one of the main architects of the relativity and quantum revolutions, said to me once, "I always try to combine the true with the beautiful, but when I have to choose one or the other, I usually choose the beautiful." Following Weyl's good example, our string cosmologists are making the same choice.

I strongly disagree with Dyson that "string cosmology" is beautiful, and suspect that he hasn't bothered to look closely into it. Even the people most enthusiastic about the anthropic string theory landscape don't generally characterize it as beautiful. Brian Greene's characterization of string theory as "Elegant" concerns the idea of a highly predictive unified theory based on a Calabi-Yau, but I don't think he has tried to characterize the Multiverse in this way. There's lots to say about the problem of "beauty" and string theory, at one point I wrote a whole book chapter about it, so won't say more here.

The Hermann Weyl quote is very famous, and I had always assumed that it was something that Weyl wrote somewhere. It turns out that the source is not Weyl, but Dyson himself, who wrote after Weyl's death in the March 10, 1956 issue of Nature:

> Characteristic of Weyl was an aesthetic sense which dominated his thinking on all subjects. He once said to me, half joking, 'My work always tried to unite the true with the beautiful; but when I had to choose one or the other, I usually chose the beautiful'. This remark sums up his personality perfectly. It shows his profound faith in an ultimate harmony of Nature, in which the laws should inevitably express themselves in a mathematically beautiful form. It shows also his recognition of human frailty, and his humor, which always stopped him short of being pompous.

The "half-joking" and "his humor" part of this quote just about always gets left off, making Weyl sound, well, kind of pompous.

The Institute for Advanced Study now has some new web-pages devoted to Weyl and his work, the main one is here.

Comments

1. **Curious Wavefunction**
   March 19, 2012

   Have you seen Ira Flatow's documentary "Big Ideas" about the IAS? He interviews several string theorists including Witten, and Dyson emerges as the lone dissenting voice on string theory. He says that he thinks that string theory might end up being like Lie groups, a very interesting and elegant piece of mathematics that may find unexpected applications in physics a hundred years from now.

2. **Peter Woit**
   March 19, 2012

   Curious,

   Yes, I did see that documentary, and noticed that Dyson was the only one at the IAS expressing skepticism about string theory. This was nearly 10 years ago, it would be interesting to see the results if Flatow went back and asked what people think now. I suspect Dyson wouldn't have changed his views, other people might have…

3. **Bee**
   March 20, 2012

   Here's what Heisenberg had to say about Weyl's book…

   I'm not sure though Dyson is being fair to string cosmology. He seems to be talking about the multiverse, but for me that isn't really the interesting part. String cosmology is imo presently the most promising area to make contact between string theory and experiment, see eg this paper. For a very brief summary see also this paper, section 2.4.8 and 3.3.

4. **Nige Cook**
   March 20, 2012

   On mathematical physics "crackpotism", can I point out that Weyl invented the key idea of first quantization quantum mechanics in his quantum gravity gauge theory of 1915. Weyl scaled the metric for curvature in general relativity by exp(iX) where X is a function of the electromagnetic field (the periodic real plane solutions to the complex exponent do the quantization). [Weyl, "Gravitation und Elektrizitat", Sitzungsberichte der Preussischen Akademie der Wissenschaften, v. 26 (1918), pp. 465-480.]

   After Einstein dismissed this Weyl quantization of gravity, Schroedinger applied Weyl's exp(iX) in 1922 to model the quantized energy levels in the Bohr atom [Schroedinger, "On a Remarkable Property of the Quantum-Orbits of a single Electron", Zeitschrift f. Physik, v. 12 (1922), pp. 13-23], from which he derived the Schroedinger wave equation – the solution to which shows that the wavefunction is proportional to exp(-iHt), simply Weyl quantization – in 1926 to explain de Broglie's work on wave-particle duality.

   Naturally, nobody dismisses Weyl as a crackpot who first misapplied gauge theory to the metric rather than the wavefunction, because the wavefunction didn't exist in 1915. This is typical real world science: people do the best they can, sometimes make mistakes, and others correct those mistakes, ignoring the errors and trying to build on what is useful. There is nothing "professional" about subjectively picking out straw man trivia to "ridicule" someone for making errors. That is just paranoid.

5. **chris**
   March 20, 2012

   speaking of Dyson and the fringe, i think it is worthwhile to point to one of his former projects

   http://en.wikipedia.org/wiki/Project_Orion_%28nuclear_propulsion%29

6. **Peter Woit**
   March 20, 2012

   Bee,

   From what I've seen, the only possible "contact between string theory and experiment" coming out of forseeable cosmology experiment is results like "this class of string theory scenarios is ruled out, this class isn't". In a situation where a huge variety of string theory models can be constructed, matching any plausible experimental result, falsifiability and distinctive predictions are gone, so you're not doing conventional science. All you're doing is parametrizing experimental results in a rather baroque way, chosen to keep a certain speculative framework alive rather than admit it doesn't really tell you anything.

   More specifically, Planck results are probably already known to some (who aren't telling). When they're announced, what will they tell us about string theory? I don't see how it can be anything significant.

7. **Bane**
   March 20, 2012

   Speaking of all things Dyson, Peter, have you read, and will you review, "Turing's Cathedral" by George Dyson, son of Freeman? I think you might enjoy it quite a bit.

8. **Tom**
   March 20, 2012

   Opening paragraph – "John Carter" ?

9. **Tom**
   March 20, 2012

   Oops, hit enter too fast!
   "John Carter" is the protagonist in the Edgar Rice Burroughs "Barsoom" books, and the title of a recent Disney scifi film based on the books.
   Dyson's book review refers to "Jim Carter" !

10. **Peter Woit**
    March 20, 2012

Thanks Tom!
Fixed. I must have had that film name somewhere in the back of my mind…

11. **harryb**
March 20, 2012

Re Dyson's review in NYRB – I felt he was soft on string theory at the end: having mauled it with the "imagination detached from experiments" line, he then seems to imply its search for beauty saves it, suggesting it may actually be Visionary. Frustrating fence-sitting.

12. **Peter Woit**
March 20, 2012

Bane,

Probably worth reading, but life is short and I don't think I've got time to really ever learn about computability…

13. **harryb**
March 21, 2012

Bane / Peter

I started the book, and persevered for several chapters, but – sorry – gave up. Its central story was interesting, fair enough – but its digressions and historical asides were bizarrely long and irrelevant. For example before getting to some material on computer development at Princeton NJ we get a whole chapter on its history from native American indians onwards. Why? How many books were being written here?

Life is indeed too short I think for this book – TL:DR as 4chan would have it (too long / didn't read)

14. **Margaret Wertheim**
March 25, 2012

Hi Peter

I am utterly delighted by Freeman Dyson's poetic review of my book in the current NYRB and am charmed by his anecdotes about his own encounters with physics dissidents, especially his story of his friendship with Immanuel Velikovsky. What I like so much about Freeman's piece is the sympathy he shows for people who have "aberrant ideas" but who are so passionately concerned with finding meaning in the world around them. Dyson compares Velikovsky to the poet William Blake: "Your poetic visions are as large as his and as deeply rooted in human experience."Though Dyson "profoundly disagrees" with much of Velikovsky's science, he nonetheless writes that "I am proud to be numbered among your friends."

What a beautiful jewel of kinship is on display here – one of the great physicists of the past century and one of the great outsider science visionaries, profoundly disagreeing yet capable still of affection for one another.

In an age when theoretical physics has sometimes come to seem like an elite competitive sport, Freeman Dyson reminds us of the enterprise's intrinsic links to poetry. My book also aimed to foreground the aesthetic roots of this science. My intention was not to adjudicate the rightness of wrongness of any particular ideas but rather to raise the question of how we, as a society, decide which theories, and which experts, to listen to. I am deeply grateful and proud to class Dyson among my reviewers.

15. **harryb**
March 27, 2012

Margaret

That's all very poetic and conciliatory, but such an approach might be the death of progressive science. Your view seems to be to find all theories worthy irrespective of their predictive and empirical power. I find that a very pessimistic approach to science, assuming there are no theories "better" than others (ie more predictive, more insightful, more useable and so on).

I love poetry – and view it somewhere strangely between fiction and fact – but I love science too, and want to lean heavily on its predictive reality as far as we can. Blending science with poetry and philosophy can create a great consilience, but only if we avoid rounding science down.

I don't believe that physics is now an "elite competitive sport". Blogs like this developed from Peter's book challenge any elite String Theorists for example – the Solvay group who spawned QM were surely more of an elite.

But let me try to get half way – I do agree that if physics for example tried to be a bit more polymathic these days and embraced other arts such as biology or economics it might create some new BSM insights and bright leaps.

However – that is a far cry for going further down the unidimensional dead-ends of strings or circlons – where is the – true – poetry in ersatz mainstream physics? I'd love more for the grafting Woits or Deutschs's of this world to have a Eureka moment after watching the Hunger Games or reading a magical realism novel.

And why isn't your book in e-form yet?

16. **srp**
March 28, 2012

From an economic theory point of view, so long as individual seekers of truth and recognition decide which other work to pay attention to solely on the basis of what will best advance their own personal quests for discoveries and citations, all the incentives line up perfectly well. Everyone has an incentive to be sincere in a personal assessment of others' work–useful, sloppy, genius, crackpot–because that work is only relevant to him as an input into his own project of inquiry and publication. His attention allocation to different sources of information is dictated by his best guess about what will aid his research. The impact of a decision to ignore another's work as crackpottery is borne by the individual making it. (Similar incentive compatibility works for such issues as how much to disclose about one's methods in a publication–each individual worker is led, as if by an invisible hand, to pick a level of detail and openness that collectively maximizes the rate of discovery.)

This perfect incentive structure breaks down when an individual seeker becomes concerned with the behavior of third parties–funders, policy makers, the public–who are not themselves part of the scientific process. Now the sincerity principle fails to hold, because the individual seeker's incentives include not only the desire to maximize his own rate of discovery and citation, but to influence these third parties. In a battle for resources, for example, getting a competitor's funding cut off by calling him a crackpot, even if you're not sure about whether he's right, becomes a possible motivation. In a battle for public recognition, it may "pay" to disparage others' work beyond one's sincerely-held beliefs. (It may also pay to feign greater agreement than one actually has with some other ideas.)

Short of returning to the days of wealthy, self-financing scientists whose only audience was a small number of cognoscenti, these imperfections in the “market” for scientific opinion will be with us always. Unquestionably, the expansion of science beyond the narrow aristocracy that once sustained it has been a huge net boon. But there has been a price to pay for that expansion, one aspect of which is the incentive distortion in assessing others’ work, leading to bigger problems with getting the crackpot/visionary boundary demarcated correctly.

Particle Theorist in Argentine Jail

March 21, 2012
Categories: Uncategorized

Particle theorist Paul Frampton of the University of North Carolina was arrested in Buenos Aires January 23rd on charges of attempting to smuggle two kilos of cocaine out of the country. He denies the charges, but is in jail in Argentina, and UNC has suspended his pay since he could not return to teach his spring semester class. More about this here.

I don't know Frampton personally, but he has commented on the blog here in the past, and is well-known in the particle theory community. He is the author of a standard textbook in the subject Gauge Field Theories.

Update: From reports with more information, like this one, it seems clear that Frampton was the victim of a scam. Hopefully friends and colleagues will be able to help him regain his freedom.

Comments

1. **Bernhard**
   March 21, 2012

   This is a complete absurd, it is more than obvious that this was someone framing Frampton. What is extremely concerning is the attitude of the North Carolina University which is doing absolutely nothing to help him, as one can read here:

   http://www.foxnews.com/world/2012/03/20/decorated-north-carolina-physics-professor-in-argentine-jail-on-drug-charges/

2. **UNCgrad**
   March 21, 2012

   While I am sure that Dr. Frampton did not know about the cocaine in his luggage, I am annoyed that people like Bernhard are getting the wrong idea about this story. Since the faculty is under orders to respond with "no comment" and no graduate students have been interviewed that are not dependent on Frampton for funding/advising, the articles about this case have pretty much presented Frampton's viewpoint unchallenged. The truth is that the University HAS helped Frampton as much as could be reasonably expected. I know for a fact that it was arranged to have money sent down to Frampton, and that an associate dean of the university spoke with a judge in Buenos Aires to try to get Frampton out.

   [*rest of comment deleted. Please, anonymous personal attacks not welcome here*]

3. **Another UNCgrad**
   March 21, 2012

   I don't think I am qualified to comment on the quality of Dr. Frampton's professional work, but on a personal level he was not well liked by many graduate students (and some, if not most, faculty) in the department. There were several who were quite pleased with the prospect that Dr. Frampton might not be returning to teach in the fall (or ever). If there are more comments from grad students at UNC it will be hard to find some that do not include "anonymous personal attacks".

   This is a difficult situation since it places his two graduate students in a tough spot. It also has the possibility to make the department look bad, even if they really did do all they can do. From the news articles it seems that the decision to stop Dr. Frampton's pay was taken on the university level, and not at the departmental level. I think for many of the faculty they really don't know how to respond to this one way or the other. Consider this, how would your department and university respond if this happened to a noted professor in the department?

4. **Trulo**
   March 21, 2012

   From the link:

   > "I have never been in prison before, so I have no way of making an accurate comparison," he said.

   This statement seems to be inaccurate, according to this link

5. **Trulo**
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6. **Trulo**
   March 21, 2012

   In any case, and regardless any mistakes he may or may not have made, I hope he'll be released as soon as possible. If he was arrested on Jan. 23, as the article says, he's been in Hell for two months already. That's terribly bad. I wish him good luck.

7. **Interested Observer**
   March 21, 2012

   It has been obvious for quite some time that Frampton has been losing his grip on reality. For example, he devotes quite some time in his paper arXiv:1004.1910 to argue why he has accomplished more than Isaac Newton. The argument appears to be serious and does not seem to have been made in jest.

   It appears likely that someone tricked him into carrying something in his luggage and his warning bells probably did not go off owing to his diminishing grip on reality.

8. **Yet another UNC Grad**
   March 21, 2012

Firstly, I would like to address the “discontinuation of pay” concerns, which should be called “unpaid leave”. UNC is a public institution, funded by taxpayer dollars. If the governor, the legislature, the people learn that UNC is paying $100K/year to someone is in an Argentinian jail awaiting trial for drug trafficking, is it unreasonable to believe that Chancellor Thorp will be dragged through every oversight committee in the UNC system? Is it unreasonable to believe that the legislature might cite such incident in its decision to further decrease UNC’s funding? Is it unreasonable to think that Chancellor Thorp will be forced to resign if Dr. Frampton is found guilty? How can UNC justify keeping on staff and paying someone who is accused of smuggling cocaine, is abroad for an undetermined amount of time, and cannot explain the purpose of his trip? If he was on an official UNC business or a conference, that would be another matter. The University chose to act this way to minimize the damage to its reputation and funding. I believe it is a correct thing to do.

Secondly, addressing “UNC does nothing to help him” assertions. Again, UNC is a public institution. To accuse it of not helping him is the same as to accuse The State Department of not helping him. UNC must maintain neutrality in much the same way The State Department must.

I agree with the first UNCgrad: I see not a single interview with the staff, graduate students, former faculty, etc. The only comments are form Dr. Frampton, his former wife, friends, and a graduate student whose career currently depends on Dr. Frampton. Many former and current graduate students are willing to speak on condition of anonymity, and a few will put their name on their words.

9. **Yet another UNC Grad**
   March 21, 2012

   It would be nice if the media obtained the police report.

10. **abbyyorker**
    March 21, 2012

    He’s been in the field for many years. Cant some of his friends/colleagues fly down there to help?

11. **Steve**
    March 21, 2012

    Innocent until proven guilty of course but why would drug smugglers stuff cocaine into a random tourist’s luggage if they could not collect it States side? And what was he doing down there anyway on a private trip? It’s not looking good for him esp. with his money cut off, and their courts and criminal justice system don’t have the rigor and efficiency of their US equivalent. Section 3 of Arxiv/1004.1910 where he talks about his brilliance as a student, and claims to be smarter than Newton, actually does look like it was written by someone “coked up” or someone who is losing it. If he is innocent then I do hope it all gets sorted out. Unfortunately, sh–t sticks, even If one is exonerated and proved innocent. Someone that smart is simply not expected to get themselves into a horrendous mess like that. Whenever you lose your integrity and reputation in high professional circles you are finished.

12. **Interested Observer**
    March 21, 2012

    From what I understand (through a somewhat informed source), Frampton was in Argentina to meet a “model”. And apparently she asked him to take one of her bags back with him to the US. One can speculate why a “model” might want to meet with a 68 year old physicist earning only about a $100k a year, but she may have been impressed by his physics accomplishments that have surely dwarfed Newton.

13. **A.J.**
    March 21, 2012

    Steve,

    Trashing someone’s reputation is a public forum — when that person is in jail in a foreign country and likely unable to respond — and then saying that they should be expected to protect themselves or they might lose their reputation…. Does this not induce cognitive dissonance?

14. **Yatima**
    March 21, 2012

    More seriously, why should anyone object about someone else carrying cocaine or not?

    Oh wait, we are in the era of the “War on Drugs” psychosis created by a state employees looking for politically easy enemies to justify their paychecks.

    Ok, carry on, then.

15. **Steve**
    March 21, 2012

    A. J. You misinterpret me. I much respect the prof and his achievements. It is not a personal attack and whether someone takes/uses/smuggles cocaine is not something I judge. It is their decision but there very are serious consequences to that esp. abroad. I’m not even saying he did that. But I’m looking at it purely from the point of view of how the law, the legal/justice process, and the media will see it. Law, like science, goes on the weight of evidence and it does not look good unless some good defense lawyer(s) has some good evidence to clear him. UNC are cutting themselves off from him, or so it seems so how does he pay for that? He is in a very serious legal situation. And I’m afraid sh-t does stick as far as reputation goes. Any enemies he does have–certainly not me, I don’t know him–will use it. It’s not right but that’s how it goes.

16. **Steve**
    March 21, 2012

    Let me say I think this is a horrible situation I would not wish on anyone, and I really hope it gets sorted out for him and he can return home.

17. **anton nymos**
    March 22, 2012

    >> he talks about his brilliance as a student, and claims to be smarter than Newton,
    >> actually does look like it was written by someone “coked up” or someone who is
    >> losing it

    this sounds just like e.g. Lubos Motl and some others, it seems to be a string theorist thing so I would not hold it against Prof. Frampton.

18. **Anonyrat**

March 22, 2012

Let's quote fom 1004.1910 since others have mentioned it:

It would be a wonderful to have lunch, may be at L'Atelier de Jo¨el Robuchon in Roppongi Hills, with Murray Gell-Mann, Isaac Newton, and Grigori Perelman to compare notes on personal fulfillment. What does Grigori Perelman mean, when he tells journalist, in turning down a million dollars, I have all I want. I'm not interested in money or fame? This seems to baffle some americans, whose idea of happiness, as an inalienable right, is a three-comma net worth. Yet, a two-comma net worth suffices, for all practical purposes.

Fame can hardly exceed that of the singer and entertainer, Elvis Presley (1935-1977), whose name, from my non-scientific studies in public transportation, is still recognizable by one billion people. He died, when he was only fourty-two, so his fame was not very useful.

—– speaks to lack of motive.

19. **Peter Woit**
    March 22, 2012

    anton nymos,

    The conviction that one is smarter than everyone else isn't exactly unusual in academia, not just among string theorists. It's also the kind of delusion that can make a good target for a scam, which is what may have happened here.

20. **Anonyrat**
    March 22, 2012

    UK Telegraph
    http://www.telegraph.co.uk/news/uknews/law-and-order/9160448/Distinguished-British-scientist-faces-16-years-in-Argentine-jail-after-being-caught-with-suicase-of-cocaine.html

    Quote:

    But respected Argentine newspaper Clarin reported he has told investigators he was set up after flying to the country to meet a woman he been romancing over the Internet.

    He is said to have made a statement to investigating judge Juan Galvan Grenway after initially refusing to answer questions.

    He claimed in his statement, leaked to the Argentine press: "The reason for my trip to south America was to meet a female friend, who is a well-known model, but I wasn't able to meet her.

    "I believe my friend's representative, who was the one who gave me the flight tickets, is probably the person responsible for the drugs found in the suitcase."

    Mr Frampton, who arrived in Argentina on January 21 from Bolivia, says he agreed to check the suitcase in on his return flight to the States on the understanding it belonged to his friend.

    Investigators who confirmed his prison remand order last week justified their decision saying: "It is improbable and it wouldn't be likely that a 68-year-old man with a solid university education, has come to the country to meet up with a female friend, and despite not having had contact with her, has agreed to carry a suitcase apparently belonging to her with him."

    University friends of Mr Frampton are now fighting to get him freed from prison while the investigation into his alleged wrongdoing continues.

    Former colleague David Stallard said: "I knew Paul professionally and socially for 17 years.

    "He never showed any interest in drugs and it is inconceivable to me that he intentionally smuggled cocaine. He must have been duped.

    "I fervently hope that he will be exonerated and then reinstated in his university."

    Retired lawyer John Bird, a former neighbour of Mr Frampton, said: "There's no-one in the world more improbable who would smuggle cocaine.

    "He got set up. I would bet my life on it. It would be contrary to everything in his background."

    End quote.

21. **Bernhard**
    March 22, 2012

    "It is improbable and it wouldn't be likely that a 68-year-old man with a solid university education, has come to the country to meet up with a female friend, and despite not having had contact with her, has agreed to carry a suitcase apparently belonging to her with him."

    And how probable it is that "a 68-year-old man with a solid university education, has come to the country" to smug cocaine? Really.

22. **Bernhard**
    March 22, 2012

    smuggle

23. **Anonyrat**
    March 22, 2012

    This page in Spanish from clarin.com is relevant.
    http://movil.clarin.com/policiales/narcotrafico/reconocido-fisico-ingles-cocaina-Ezeiza_0_668333271.html

    The google translation of an excerpt:

    "For starters, Frampton is a recognized and respected theoretical physicist, professor at the American University of North Carolina, who even signed several papers with Nobel laureate Sheldon Glashow (see worked …). But also, as he explained in his defense, the suitcase with the cocaine would come into his possession as a last step in a long series of deceptions of those who says he was victim: a woman who posed as a model contacted him via the Internet with the months came to him love, and he was following his trail to meet her in person to get to South America.

So far, his alibi for Justice is unlikely. So Frampton-prisoner for two months in the prison of Devoto-and has a preventive imprisonment pending trial (more than 1,350,000 pesos in foreclosure), confirmed by the Hall B of the House Economic Criminal.

The funny thing is that your story is strikingly similar to that made at the time Sharon Mae Armstrong (54), a former official of the Government of New Zealand Maori-language expert, who was arrested at Ezeiza the April 13, 2011 with 5 kilos of cocaine in his suitcase when he went to take a flight to Spain.

In the case of Armstrong, but ended up being convicted, the Court took it for granted that he had been deceived by a man who fell in love via the Internet, offered her marriage and, with an excuse, made travel to Argentina with the promise that after would meet in London to meet in person. Because judges believed in her story, Sharon was imposed almost the minimum penalty for the crime of drug smuggling (see The Case of New Zealand). "

—
Looking up Sharon Mae Armstrong, whose story is similar to Frampton's
http://www.stuff.co.nz/dominion-post/news/6589959/Family-eases-strain-for-jailed-Kiwi

She's been in jail since April 13, 2011. Her not guilty plea was entertained Nov 10, 2011.
http://www.3news.co.nz/Alleged-drug-mule-Sharon-Armstrong-pleads-not-guilty/tabid/423/articleID/232343/Default.aspx

Quote: "Mr Piripi said advisers warned her to plead guilty, as "with her circumstances and mitigating factors she might get three years".

"But she just could not plead guilty, she couldn't, which means she's looking at two to three years waiting for a trial and absolutely no leniency if convicted," he said."

—– If Professor Frampton's case follows the same course, things don't look good.

24. **Anonyrat**
March 22, 2012

This last – I hope I am not abusing our host's hospitality –
http://www.argentinaindependent.com/currentaffairs/newsfromargentina/caught-with-cocaine-drug-trafficking-in-argentina/

on Sharon Mae Armstrong –

Quote:

"The courts will assess her innocence said Claudio Izaguirre, president of the Argentine Anti-Drugs Association, a non-governmental group in Buenos Aires.

"It depends on the judge's mood, the attorney's mood, her personal background, her previous lifestyle. Quite a few things will have to be evaluated," he said. "But the reality is she was trying to leave the country with five kilos of cocaine.""

25. **Trulo**
March 22, 2012

> And how probable it is that "a 68-year-old man with a solid university education, has come to the country" to smug cocaine? Really.

It could be very plausible, if the man in question is a cocaine user, and it is cheaper and of higher purity in that country than in the US. As an example of a mature man with a solid university education who was known to be a consuetudinary cocaine user you can take Sigmund Freud.

To be sure, I have no idea whether Prof. Frampton was duped as he says or not. But his story seems to be so weak from a legal point of view. If that's all he has to say in his defense, he will probably be found guilty and given a term in prison, hopefully the minimum. Which, by the time he is sentenced, he will have probably already served.

A very sad story, indeed.

26. **Anonyrat**
March 22, 2012

viXra log weighs in: http://blog.vixra.org/2012/03/21/paul-frampton-drug-case-could-take-years-to-resolve/

"Everyone passing through international airports will know that they must pack their own bags and be responsible for the contents. Travellers are continually warned and asked about it. It is easy to be befriended especially in honeypot traps. The details of how Frampton may have been tricked are not yet known but similar stories are well-known. Cases have even been turned into films such as Bangkok Hilton. It will be hard for an intelligent professor to persuade his prosecutors that he was naive enough to innocently accept to use a suitcase with cocaine stuffed into the padding. We wish him luck."

27. **Bernhard**
March 22, 2012

Trulo,

it is however even more plausible that he got himself involved in a scam, which the authorities are contesting the plausibility of. If the story Frampton is telling sounds unconvincing, the alternative that he is lying and was indeed smuggling drugs seems to be worse, at least to me.

28. **Trulo**
March 22, 2012

Bernhard, Prof. Frampton was caught in the act of committing a crime. As far as I understand the legal system, which is not much, the fact that he was unaware of what he was doing may constitute a mitigating circumstance, if the judge believes him, but it cannot exonerate him. Mitigating circumstances can help reduce the term he is sentenced to serve. But trying to go across an international border with a significant amount of an illegal drug is legally a serious crime, regardless of what you and I think about it, and I'm afraid even the minimum could be a few years. Let's hope I'm wrong.

29. **Bernhard**
March 22, 2012

I unfortunately agree with you about this.

30. **Peter Woit**
March 22, 2012

Trulo,

I'm no more of a lawyer than you are, but I'd guess that not suspecting at all about the cocaine would be not just a mitigating factor, but would make one innocent of the crime. Unfortunately for Frampton, he may have trouble convincing the authorities of this, and they may want to make an example of people caught up in drug smuggling, even if they are the victim of a scam.

31. **Trulo**
March 22, 2012

Peter,
you may be right, but I doubt it. If that were true, then carrying bags and boxes full of stolen paintings, fake dollar bills, and even military-grade plutonium through international borders would be a trivial matter: It would just require a temporary suspension of disbelief. As long as you don't suspect anything you'd be innocent.

32. **Bugsy**
March 23, 2012

From what everyone says, he's probably the victim of a scam. But if they were clever,
it might be very hard for him to identify them; and if he could- well, I suppose his life might be in danger.

It reminds me of the famous Nigerian email scams-
http://en.wikipedia.org/wiki/Advance-fee_fraud
which nearly sucked in a friend of mine- a very smart guy who fits the profile (the overconfidence of arrogance). Finally he swallowed hard and called the FBI and told them all; it must have been embarrassing to admit how stupid he had been, and also frightening thinking he might be in trouble for the various small, embarrassing or illegal things they had got him to do along the way. In fact
they just gave him sound advice (under NO circumstances meet with them; lives have been lost, these people can be very dangerous) and didn't blame him at all.

BUT…what was he doing in Bolivia before that, which is more or less cocaine central?

33. **paleface**
March 23, 2012

Imagine that a Colombian physicist tried to enter the US through La Guardia Airport with a suitcased loaded with four pounds of cleverly concealed cocaine. And imagine that, once caught, he would declare that he was the innocent victim of a scammer taking advantage of the fact that he was madly in love with Britney Spears. I wonder if anyone would believe him. And I wonder how the US judiciary system would react to that. Would they try to use him to set an example? Or would they just apply the law?

34. **Bernhard**
March 23, 2012

paleface,

if this Colombian physicist were a 68 year old world renowned theorist with a extreme solid career in Academia and no record of involvement with illicit drugs and complete lack of motive to out of the blue start smuggling cocaine, I believe they could believe him. Needles to say he would still be in a lot of trouble as Frampton unfortunately also is.

35. **Bernhard**
March 23, 2012

What surprises me is that, so far, I believe the HEP community is not yet very much engaged in helping him. There was a dedicated conference in his honor in 2003 (his 60th´s birthday) where Nobel prize´s like G Hooft attended. I hope that it is only that I am not yet aware of the efforts as I cannot believe the majority of these people can think Frampton is really guilty. See for example:

http://server.physics.miami.edu/~cgc/2003CGC.html

And what is even more curious is that the banquet photos are suddenly unavailable…

36. **Interested Observer**
March 23, 2012

The difficulty with the law is that once they do something stupid, such as sentence the unfortunate Sharon Armstrong to prison even after accepting her version of events, they have to continue doing that same stupid thing. Otherwise they risk appearing stupid by reversing course on their previous stupid action. This is unfortunate fact about the law, where precedence has more traction than reason, is endemic to many legal systems. While not a lawyer myself, I suspect that if Frampton's lawyers pursued the theory that he was tricked by this model's agent, he would find himself in prison for sometime (with perhaps some allowance made for his ill health).

In my opinion, they ought to pursue the claim that Frampton is mentally incompetent . This will at least give the Argentinian Judiciary a legal excuse to allow them to set him free without establishing a legal precedence that they will be uncomfortable with. And in fact, considering many of Frampton's recent actions, his mental incompetence can be plausibly argued.

37. **Steve**
March 23, 2012

Peter, I appreciate this is a touchy subject, that deletions are justified, and I won't attempt to post any more opinions . However, the following news story this week about an extradition block of a UK citizen to Argentina on drug charges, on the grounds that her human rights would be violated, is very relevant. He therefore may be refused bail. Also, the political situation between the UK and Argentina is unfortunately bad and he is firstly a UK citizen. I am hoping he gets a fair and unbiased hearing/trial and humane/respectful treatment even if sentenced, and the rest of the community should at least insist on that too.

http://www.guardian.co.uk/law/2012/mar/20/pregnant-briton-lucy-wright-wins-extradition-appeal?newsfeed=true

38. **Mike Vaughn**

March 23, 2012

Bernhard :"What surprises me is that, so far, I believe the HEP community is not yet very much engaged in helping him." Since the story has become public only since March 21, as nearly as I can tell, I wonder what Bernhard thinks the HEP community is supposed to do? We cannot organize a SEAL team to go to Argentina and free him. According to published reports, he has legal counsel and seems to be confident that he will emerge in due course . He may not be fully aware of the deliberate pace of Argentine justice (not that American justice is exactly speedy). I certainly hope the matter can be cleared up soon, but the issue is not political and we should not try to make it so.

I send him my best wishes.

39. **paleface**
March 24, 2012

The very similar case of Sharon Armstrong reported at the end of that Telegraph article looks like a very bad precedent. Left to his own means, Prof. Frampton would probably end up in the same situation as Armstrong. Let's hope the most influential academics in the US are trying to get their government to support Prof. Frampton at high diplomatic levels.

40. **BJM**
March 24, 2012

The Professor was carrying a bag given to him by a stranger. A definite no-no and specifically screened for by questioning in airports. The results could have been much worse than just a drug bust of one person.

41. **Trulo**
March 24, 2012

From the Telegraph article linked to in the update, referring to Sharon Armstrong,

> Judges convicted her despite accepting her claim she had been duped by a man she met over the Internet.

This is exactly what I meant in my posts above. If you try to board a plane carrying illegal stuff and you get caught, you're roast. If you are technically innocent because you didn't know about the stuff, then you're tender roast, but roast nonetheless.

Having ten million academic citations will help you about as much as having a cat named "Fuzz". Now, having friends very high up in government will make things extremely smooth. If Hillary took an interest in the case, Prof. Frampton would be out for lunch with the judge in no time, eating the best barbecue money can buy in Buenos Aires, all expenses paid by the court.

42. **AnotherUNCer**
March 24, 2012

[From a UNC student, first part removed. I've had all too many experiences with character assassination by anonymous comment to not be sensitive to why it's a really bad idea to allow it on blogs. Please keep this is mind.]

My friends and I figured that he was taken in by a sex-related scam well before Prof. Frampton's defense became public. We came to that conclusion based on several factors. Number one, regardless of whether or not one considers Paul Frampton to be a good physicist, it is hard to deny that he happens to be a singularly clueless individual when it comes to street smarts and common sense. The idea that he could somehow get involved in cocaine smuggling seemed ludicrous for the simple reason that he appears to lack the wherewithal. Secondly, Prof. Frampton's relationship to the opposite sex is well known within the department, and the idea that he could be taken advantage of in this way is, sadly, almost overwhelmingly reasonable.

All in all, I would say that while none of us wish this upon Prof. Frampton, few of us are surprised that it happened.

43. **Chris Oakley**
March 25, 2012

From Prof. Frampton's web site:

> The three most important exams were one in 1954 and two in 1961.
> In 1954 I sat for the 11+ exam and achieved the highest mark out
> of 250,000.
> In Spring 1961, I sat for GCE A-level exams and received the
> highest A-level marks ever at King Charles I School.

He is going to bring the British educational system into disrepute.

44. **Anonyrat**
March 25, 2012

A. Is Professor Frampton an American citizen?
B. If yes, will there not be approachable high level members in the US government who can be approached for help?
C. Can some big name physicist or other academic start the ball rolling?

45. **Bernhard**
March 25, 2012

Mike,

"I wonder what Bernhard thinks the HEP community is supposed to do?"

I agree with you that the news came not so long ago but public opinion in his favor specially from powerful friends could have an effect in his favor. So far, I have seen only mild explicit declarations to support him which indicates either they don´t want to get involved with it or believe he might be actually guilty. It is also possible they just did not have time to act, but time is passing…

46. **Visitor**
March 25, 2012

“Now, having friends very high up in government will make things extremely smooth. “If Hillary took an interest in the case, Prof. Frampton would be out for lunch with the judge in no time, eating the best barbecue money can buy in Buenos Aires, all expenses paid by the court.”

Not necessarily. While I might not know the specifics, it is possible to imagine a situation in which Argentine public opinion reacts negatively to American political interference in a judicial process involving an American who, wittingly or unwittingly, _was_ carrying two kilograms of narcotics. And an upshot of this might be, not that Frampton would be liberated, but that he would received a harsher sentence than he might otherwise have received.

47. **Martin**
March 25, 2012

There is an article in an argentinian newspaper. The comments that people are making about the professor are quite bad. I even read someone saying “He should go to jail because he is british”. The Falkland/Malvinas thing will only make things worse.

I’m sure he didn’t mean to do this and I hope he is released as soon as possible.

Here is the link of the article if any of you wants to check it out:

http://www.clarin.com/policiales/narcotrafico/reconocido-fisico-ingles-cocaina-Ezeiza_0_668333271.html

48. **Bernhard**
March 26, 2012

It´s good to see that he is anyway still very positive:

“Frampton said he will likely leave prison in a week thanks to evidence in his favor, though he will still be detained in Argentina.”

http://www.dailytarheel.com/index.php/article/2012/03/drugs_prof

Hope he is right…

49. **Mike Vaughn**
March 26, 2012

Bernhard,

This not a political case where public posturing might be helpful. It is a case that appears highly likely to be a setup, and quiet work through the Argentine legal system seems to be the best course to follow. Some of the stories suggest that such work is going on and we can only hope that it will be effective.

50. **Bernhard**
March 27, 2012

Mike,

I still have my doubts about this, but perhaps you are right and I really hope you are. To a certain degree my reaction is also driven by the frustration I feel when I hear the comments from the Argentinian investigators (“It is improbable and it wouldn’t be likely that a 68-year-old man with a solid university education, has come to the country to meet up with a female friend, and despite not having had contact with her, has agreed to carry a suitcase apparently belonging to her with him.”). It is clear to from this statement they do not yet understand how actually probable this is. Understanding Frampton´s character and position, I would think, could help them with this. But again, I might be wrong and political interference might have a negative impact on the whole case and I certainly don’t want that. So, in the end I agree with you we should wait for the moment and let the case follow its course.

51. **Rocketgal**
March 27, 2012

This may seem like an obvious question to some…

*HAS ANYONE CHECKED HIS PERSONAL COMPUTER FOR EVIDENCE???

Seems like an easy enough thing to do. If the Professor IS TELLING THE TRUTH (and I tend to believe he is), then I would think his family or friends would fly down to Argentina to give it to his personal lawyer immediately to get him out of there!

Just sayin!

52. **MathPhys**
March 27, 2012

A man exercised a serious error of judgement (as many of us do on a regular basis) and will regret that for the rest of his life. Let’s leave it at that.

53. **mesolimbic reward pathway**
March 28, 2012

Completely off-topic, but a search for “cocaine” in arxiv revealed this… interesting “paper”: http://arxiv.org/abs/astro-ph/0204013

54. **Jack B**
March 29, 2012

This is nuts. Good luck to him.

55. **King Ray**
March 29, 2012

As Heraclitus of Ephesus said, “Character determines destiny.”

56. **Bernhard**
April 3, 2012

I believe these are good news:

http://www.stuff.co.nz/world/americas/6635954/Drug-courier-may-be-freed-next-year

The bad news is that he is likely wrong he will be freed by the summer…

57. **Trulo**
April 5, 2012

Interestingly, there's a third case of a foreigner duped with the same trick, presumably by the same criminal gang in Argentina. Besides Sharon Armstrong and Paul Frampton, the nurse Catherine Blackhawk (that's a cinematic name, isn't it?), a US citizen, was caught on June 29 2011 in Buenos Aires airport (Ezeiza) trying to board a plane to London with eight pounds of cocaine. The story is more or less the same as in the other two cases. She met a supposedly English guy on the Internet, whom she was supposedly going to marry, and who asked her to meet him in London. But before that she was to make a stop in Argentina to pick up documents related to an inheritance he had received. Those documents were in a suitcase she was to take to London… the rest of the story is obvious.

58. **Trulo**
April 5, 2012

Both women, the New Zealander Armstrong and the Texan Blackhawk, traveled to Buenos Aires at the request of their supposed twin souls with all expenses paid. Both were lodged in the same hotel in the Once neighborhood of Buenos Aires, and both were contacted by a woman named Esperanza, apparently the same woman in both cases. The judge(s?) in those cases have already begun an investigation based on those leads.

Blackhawk (I still think there might be a mistake in the name quoted by the Clarin newspaper) is still awaiting trial. Armstrong has been sentenced to four years and ten months, but apparently there's the possibility that after serving one year she'll be expelled from the country.

2012 Abel Prize

March 21, 2012
Categories: Uncategorized

The 2012 Abel Prize was awarded this morning to Endre Szemerédi. I know nothing about him or his work, but there's a webcast going on right now with Tim Gowers providing explanation.

Update: The written version of the Gowers talk is here.

Comments

1. **Sakura-chan**
   March 21, 2012

   A very deserving recipient!

2. **paddy**
   March 21, 2012

   All I can say is that the Gowers discussion of Szemerédi works (which you provided link to) was fascinating. TY

3. **M. Roachcock**
   March 22, 2012

   Should have gone here instead!

F-theory Phenomenology

March 23, 2012
Categories: Uncategorized

In the years before the LHC start-up, one heavily promoted claim that "yes, string theory can too make predictions, and here's what it predicts the LHC will see" was based on a class of models known as "F-theory". Detailed superpartner mass spectra were produced and shown around the world at conferences and departmental colloquia. For an example, take a look at figures 3 and 29 of this paper.

Early LHC results showed that nothing like these mass spectra corresponds to reality. For the latest such results, see Resonaances, which describes new SUSY limits from ATLAS, now wildly out of agreement with the F-theory "predictions".

At this week's F-theory Workshop, there seems to have been little acknowledgement of this failure. I didn't notice either any reference to the fate of the "predictions", or even an attempt to come up with new, updated ones. The closest I could find was this comment by Michael Dine in a discussion of the state of F-theory phenomenology":

> A lot of us I think are resigned to the idea that maybe there's supersymmetry and it's going to look tuned, or maybe there's not low energy supersymmetry. I think a challenge I've always said for string theory is to try and think about theories without supersymmetry and that has proven to be hard. But you know, that's certainly a direction which maybe we're being confronted with.

So, the long-standing ideology that supersymmetry stabilizes the weak scale, and seeing its effects will finally give evidence for string theory unification looks like it is crumbling. With this hope gone, string theory unification becomes a completely unpredictive subject, with no hope of connection to experiment. One has an infinite array of mathematically highly complex models one can spend time studying, but it's hard to characterize doing so as any recognizable form of physical science.

This situation hasn't slowed down string phenomenologists, who will follow up the F-theory workshop with a summer school for graduate students to train them in the failed techniques of the subject. I have a hard time understanding why any sensible graduate student would want to attend such a thing, or why any responsible advisor would encourage them to do so.

Comments

1. **Marcus**
   March 23, 2012

   In case anyone's curious that quote from Michael Dine begins shortly after minute 52:30 of this video discussion:
   http://media.scgp.stonybrook.edu/video/video.php?f=20120322_1_qtp.mp4
   If you drag the button to minute 53 you miss the start of it.

   This was the Thursday 22 March 10:15-11:15 am session with Dine, Moore, Seiberg,
   titled "A Critical Look at Phenomenological Issues in F-theory"
   that you can find the link to in the overall program here:
   http://scgp.stonybrook.edu/archives/1493

2. **Shantanu**
   March 23, 2012

   sigh, such a sad situation. how do these guys get funding! Someone should really attend
   some of these talks and ask these hard questions.
   OTOH I know a colleague who has been looking for jobs for last seven years without luck and only because the work he does is not trendy.

3. **abbyyorker**
   March 23, 2012

   Great post IMHO. I am surprised at the level of dismay – after all, susy proponents could just claim we need higher energy (but maybe it is absolutely essential to see things at LHC scale? I don't know – I'm kind of ignorant). I have to be honest: I really like SUSY except for the experimental non-validation.
   But, to be frank, you have been puzzled for 20 years as to behaviour of responsible advisors. I mean that in a neutral, not negative way.

4. **Giotis**
   March 23, 2012

   "One has an infinite array of mathematically highly complex models one can spend time studying, but it's hard to characterize doing so as any recognizable form of physical science."

   "We seem to be at a critical point in the history of science, in which we must alter our conception of goals and of what makes a physical theory acceptable."

   Stephen Hawking, THE GRAND DESIGN.

   You may reach a point where your physical theories cannot be verified directly via experiment due to their very nature and the nature of their general predictions. What do you do then? You abandon the theory? Of course not, the theory could be right. Then you need new criteria to decide when a theory is acceptable as scientific. These criteria are set by the people who are doing the actual scientific work i.e. the physics community. You can't live in the 19th or 20th century forever following blindly a strict dogma of what science is and what isn't. This just stalls progress.

5. **emile**
   March 23, 2012

   Giotis,

   Science without experimental verification isn't science. Period. It may very well be that some theories can't be tested with current (or future) technology, but then don't call those scientific theories. Call it math, call it natural philosophy, or whatever. But don't call it science.

6. **Chris W.**
   March 23, 2012

Giotis: *You can't live in the 19th or 20th century forever following blindly a strict dogma of what science is and what isn't. This just stalls progress.*

Progress? Progress in what, exactly?

Of course not, the theory could be right.

Could be right? In what sense? Have you utterly forgotten that the whole point of testability is to have a way to determine from empirical evidence that a theory is *wrong*, and that **no amount** of confirmation, if it could be had, cold ever definitively establish that a theory is "right"? If "right" means anything, it means "has been subjected to many empirical tests, and has passed them". That's all you can hope for, and string theory hasn't even found its way to the examination room. However, it sounds like F-theory has, and it flunked. I guess that would be something, if the grade wasn't being disputed or ignored. 🙂

7. **Peter Woit**
   March 24, 2012

   abbyyorker,

   You can just claim that SUSY breaking is at a higher mass scale, but then you give up the main argument used for why you want SUSY. It starts to become just one more unmotivated, complicated extension of the SM which doesn't explain anything. Why spend any time on such things?

   Giotis,

   You're joking, right?

8. **Peter Shor**
   March 24, 2012

   There is an important theoretical question to be answered, which is: how is it possible to reconcile quantum field theory and general relativity? Some string theorists claim that string theory answers this, but judging from the number of people working on how information gets out of black holes, for the most part, they seem to have either given up on the question or declared victory and moved on.

9. **Peter Shor**
   March 24, 2012

   And maybe, before we start working on the question of how to mathematically reconcile quantum field theory with general relativity, we should really try to answer the question of how to mathematically reconcile quantum field theory with itself.

10. **A.**
    March 24, 2012

    Re: Giotis' comment, a senior mathematician/string theorist once said almost exactly the same thing to me, following it up with "The definition of `science' was invented to satisfy Marxism. It has nothing to do with modern physics."

    I responded with "or you could say that modern physics has nothing to do with science" and _that_ was met with genuine shock. And a protracted silence.

11. **Giotis**
    March 24, 2012

    I just want to make a clear distinction between constructive criticism which is a healthy endeavour and a Taliban-like preaching in the name of some scientific orthodoxy which demands the theoretical research to stop if it doesn't follow some strict definitions and dogmas. These rules were made by the physics community and could be bent or re-adjusted by the same community if necessary in order to face the new difficult challenges of Planck scale physics.

12. **P**
    March 24, 2012

    To play devil's advocate and also to ask a question . . .

    Pretty much everyone here is being critical of F-theory, which is respected and worked on by many of the brightest in the scientific community. It has made a resurgence since 2008, at which point many good people who hadn't worked on it started working on it because of apparent promise. This all happened *after* Peter's book.

    Who of you critics can actually state cleanly what F-theory is, what the status of the field is, and why people believe it is promising compared to (say) the heterotic or type II superstring.

    I'm interested to hear your responses.

13. **Peter Woit**
    March 24, 2012

    P.,

    Personally I don't see any point in arguments over the relative merits of heterotic/type II/F-theory models since they've all now failed more or less equally miserably, and all show no hope of leading anywhere.

    I claim no expertise in F-theory, beyond that coming from sitting through several promotional talks (the topic is a favorite at math/physics conferences), and spending some time carefully looking at the supposed "LHC predictions" of the theory before writing about them here. At one of such talks, a prominent string theorist I know who was sitting next to me after the talk explained to me in detail how it showed that I was just wrong: string theory does make predictions about LHC physics, and the LHC results would change my mind. I was polite.

    The resurgence of F-theory since 2008 was based to a large degree on the strong and explicit claims being made for LHC predictions based on F-theory models. I'm still waiting to hear the people who made those claims acknowledge what has now happened.

14. **P**
    March 24, 2012

    Peter,

It’s a bit irresponsible of a prominent string theorist to claim that F-theory definitely makes LHC predictions, in my opinion. On the other hand, there are definite reasons to believe that it gives rise to more promising models for particle physics (GUTs in particular) than the weakly coupled type II string, so it’s simply wrong to say that they’ve all failed “equally miserably.” Regarding the failure of the authors to address what their papers said in light of experimental data, I can’t speak to why they haven’t. I did take a look at Heckman’s talk, though, and it’s interesting to notice that F-theory did give rise to the correct Theta_13 angle that was recently measured in the PMNS matrix.

I took a look at some of the other videos and recent abstracts of speakers, and I think it’s important to point out that most people in the field are currently working on very hard problems having to do with the global aspects of F-theory compactifications. Many of the properties of the “local” GUTs studied over the last few years have things to say about GUT physics, but of course GUT physics isn’t everything and there are other effects in the vacua which must be taken into account. This is probably why the workshop at Simons didn’t seem as phenomenological as similar programs in the past.

15. **Peter Woit**
March 24, 2012

P.

“It’s a bit irresponsible of a prominent string theorist to claim that F-theory definitely makes LHC predictions…. F-theory did give rise to the correct Theta_13 angle that was recently measured…”

Right. Glad to hear you’re opposed to irresponsible claims about F-theory.

As for why “the workshop at Simons didn’t seem as phenomenological as similar programs in the past”, I think it would be a reasonable conjecture that maybe the unmentioned LHC results that torpedoed and sank all the F-theory “predictions” of recent years might have something to do with this. Then again, it was a Math/Physics program, not a pure physics one.

16. **Shantanu**
March 24, 2012

P, can you point me to the F-theory paper which made the correct prediction about theta_13? I am curious. Also you said that F-theory predicts GUTs. So what does it predict for proton lifetime and which modes should one look for a signature?

17. **Anonyrat**
March 25, 2012

Offtopic, but the Krauss thread is closed. If you have access to the New York Times, philosopher David Albert takes on Krauss.
http://www.nytimes.com/2012/03/25/books/review/a-universe-from-nothing-by-lawrence-m-krauss.html

18. **P**
March 25, 2012

@Peter:

Right, I am opposed to those claims about F-theory that are, in fact, irresponsible. Given the current state of the field, I would put “definite LHC predictions” on that list.

The problem is that the overall situation is much more nuanced, particularly as the field is still developing (again, F-theory for particle physics only since 2008), and to make blanket statements is not appropriate and doesn’t do justice to the dozens of very bright people working on the subject.

As I’m familiar with some of the literature, it’s worth correcting your conjecture about the mathematical (not obviously phenomenological) nature of the Simons workshop.
Most of the phenomenological scenarios of 2008 and on were so-called “local” models that focus on the physics near a stack of GUT 7-branes, where (say) the SU(5) gauge theory lives. This approach has its merits, as much of the GUT physics is described locally (GUT matter, Yukawas, and so on), but there are important “global” issues of the physics and of the compactification which are missed by the local picture. This isn’t surprising: the local picture isn’t a full blown F-theory compactification, but instead is a subset of the mathematical data which controls the GUT physics.

The reason for the more mathematical and “global” Simons workshop seems to have been to report on progress in understanding these global issues, including the description of chirality inducing G-fluxes. It’s only by understanding these global issues that one might hope to address moduli stabilization and (ideally) vacuum selection. It’s not that people have abandoned hope in the role of F-theory for phenomenology. It’s that they’re studying hard mathematical aspects which give rise to a deeper understanding.

@Shantanu:

I’m not sure, it’s likely a paper by Heckman and Vafa, though, and has neutrino or PMNS somewhere in the abstract. If you can’t find it I’ll let me know and I’ll google around a bit.

19. **Mark**
March 25, 2012

@ Shantanu, see http://arxiv.org/abs/0904.1419 for \theta_{13} prediction

20. **Peter Woit**
March 25, 2012

Anonyrat,

Thanks. I read that over my coffee this morning and just wrote a short posting about it.

21. **chris**
March 26, 2012

P,

it is by no means irresponsible to produce concrete LHC predictions. on the contrary, that is the essence of the theory being predictive.

what is irresponsible is the failure to acknowledge that these predictions and, consequently, the underlying model is now falsified.

22. **P**
March 26, 2012

Chris,

Sorry, I wasn't clear enough. You missed the point.

The local F-theory models that produced LHC "predictions" are "local models", not globally defined F-theory compactifications. This approach has it's advantages, though, as many aspects of GUT physics can be understood, but global effects can change the physics and are completely missed by the F-theory models you're referring to. People are making progress in understanding the global aspects, and they matter.

I completely agree that making concrete LHC predictions from a very well understood theory is of crucial importance and is absolutely the responsible thing to do! The point is that F-theory is still getting there (yet again, concrete particle aspects only since 2008) . . . and the authors of the mentioned predictions tried to understand as much of the low energy physics as they could from the local picture. But we know the local picture isn't the whole story.

P

23. **Bugsy**
March 26, 2012

As a mathematician (not in math physics) I wonder how it feels to invest enormous amounts of time working on theoretical physics models which may end up being complete failures.

Math people have the advantage of using pure esthetics (and correctness) as the main criteria of interest- so anything you do which is interesting to you (if you have reasonably good taste) will not only be of interest (eventually!) to the community but will remain so in perpetuity.

Maybe math physics on the speculative fringe should give up on the supposed connections to the real world and ALL of the hype and just try to do good work instead?

Question is, whether it still makes sense to do it in a "physics way"- without precise definitions and proofs….standards are way way different. But if the hype were removed it would help clear the air of junk. Maybe that's part of what Peter is about: trying to ridicule the hype out of HEP.

24. **Peter Woit**
March 26, 2012

Bugsy,

That's about right (although I think I reserve ridicule for only the ridiculous cases…)

F-theory is a good example. Lots of algebraic geometers have gotten involved in it, because questions about algebraic geometry arise in the subject. The hype about bogus progress can encourage them to spend their time and effort on questions of little real mathematical interest, whereas this time and effort would be better spent pursuing issues of mathematical interest that come up. This is independent of the question of rigor and proof. That's another question, one where most mathematicians are aware of the dangers of getting too far away from what is precisely understood.

25. **P**
March 26, 2012

Peter,

Again, you're not being specific enough. What precisely do you mean about the "the hype about bogus progress"? If you mean specific, completely unique LHC predictions, then I agree.

But from the point of view of the relationship between gauge theories in F-theory and mathematics, the relationship is deep, which is why the algebraic geometers work on it. For example, the heterotic F-theory duality gives an equivalence of moduli spaces of vector bundles on Calabi-Yau manifolds and elliptic K3 fibrations (roughly). The study of such moduli spaces actually governs gauge theories, including both the gauge symmetry and matter content in four dimensions. If string theory has anything to say about the real world, these things are clearly of great importance.

Mathematicians aren't studying F-theory because of grandiose claims about local models. It is interesting to them because well-defined geometric moduli spaces correspond to concrete things in low energy gauge theories.

P

26. **Peter Woit**
March 26, 2012

P,

I don't disagree. I just think all mathematicians who get involved in this area need to realize that some of the talks they're exposed to on this subject need to be consumed with a massive grain of salt. Especially ones that claim F-theory "predictions" about observable physics.

27. **SA**
March 26, 2012

I really don't see the point in the especially caustic rhetoric denouncing the mainstream BSM theories. You really should start complaining if/after the LHC significantly disagrees with the SM in an important way and this can be explained by theories simpler than SUSY.

28. **Peter Woit**
March 26, 2012

SA,

If and when the LHC data significantly disagrees with the SM, I and everyone else will be too excited to complain about anything.

Right now though, I don't see what's wrong with pointing out which "mainstream BSM" models promoted as making LHC predictions have been shown to be wrong by experiment. When this happens, maybe they should stop being "mainstream" (if they ever were..), don't you think? Or, if the predictions were bogus, maybe the question of why that was so should be examined. I understand that people whose models have recently died may still be in one of the earlier stages of grief and not yet at acceptance and ready to move on, but they should be encouraged to do so.

29. **P**
March 27, 2012

Peter,

Agreed, and this is where it is the responsibility of physicists to be honest with them: string vacua do make low energy predictions (and even sometimes stringy ones depending on M_string), but the big "predictivity" problem involves not individual vacua but instead the landscape and vacuum selection. Hopefully there is some mechanism. Of course, this is an extremely difficult but important problem and is even a problem in quantum field theory – effectively the same as asking "why the standard model?" from an enormous class of possible quantum field theories.

The role of mathematicians in this process includes helping to understand string vacua and the low energy predictions of certain vacua or classes of vacua. To say that a certain vacuum or class of vacua look something like the world (e.g. F-theory vacua can at certain regions in their moduli space) is not the same as addressing the landscape problem.

Do you agree?

P

30. **Peter Woit**
March 27, 2012

P,

Sorry, but I think the problem is much worse than you describe it. I just don't believe that constructing ever more complicated "string vacua", designed to evade obvious conflict with experiment and using poorly controlled approximations to claim "predictions" about physics is a worthwhile activity. It's been going on for nearly 30 years, and the results are clear. I'm quite serious that I think training graduate students to do this is irresponsible. The hope that somehow a "vacuum selection principle" will be found that will change the situation is based on nothing but wishful thinking.

The people doing this had a shot at showing they could get somewhere. They just needed to say something, anything, about what the LHC would see. That's gone now, time to fold tent and give up.

31. **P**
March 27, 2012

Peter,

I think this is where we fundamentally disagree, and that's fine.

For the last 30 years, every 3-5 years (or even shorter) there is a very serious development in the understanding of string theory that improves the our understanding of string vacua and the associated low energy physics. So it's not just that people have been beating their heads against the wall for thirty years. There has been progress in understanding (I can cite papers and ideas, if you wish), and to me the irresponsible thing is to ignore this progress and completely stop the subject.

That being said, I agree that training hordes of graduate students in these techniques probably isn't the way to go. The real progress in the subject over the last 15 years has been made by a relatively small number of people (order 100-200 worldwide?) and if the problem is to be solved, I don't think it's just by adding bodies. There are other interesting things to study as well, particularly with the data coming from LHC.

Cheers,
P

32. **Peter Woit**
March 27, 2012

P,

My impression from following the field is that, yes, every 3-5 years there's a new idea, but that these new ideas all go in the wrong direction: they show you some new sort of "string vacuum" to look at. The whole field started out in 84-5 with some very tight constraints on what a string vacuum could be, and since then the possibilities just keep multiplying. This is not a good thing…

In any case, there's no danger that people will be forced to stop doing this, based on what I or anyone else thinks. It will likely be a major industry for quite a while, until an actual good idea about BSM physics and unification appears, at which point it will get dropped like a hot potato…

33. **SA**
March 27, 2012

Peter,

Insofar as the only problems with the LHC are theoretical in nature, it makes sense to develop theories that mitigate these issues and explore their consequences. The most obvious extensions of the SM have been considered already: fourth generation, technicolor, more trivial extensions of the gauge group, more dimensions, or SUSY.

In the case of SUSY, all of the predictions of course depend on the mass spectrum, and the current limits on superpartner masses simply do not disqualify even the simplest SUSY models from solving the theoretical concerns that motivated them.

34. **SA**
March 27, 2012

Er, sorry by LHC I meant SM.

35. **Peter Woit**
March 28, 2012

SA,

"the current limits on superpartner masses simply do not disqualify even the simplest SUSY models from solving the theoretical concerns that motivated them."

I just don't think that is true. The main motivation given for SUSY pre-LHC was that it would stabilize the weak scale. Assuming this, you can go back and listen to talks from people like Arkani-Hamed explaining that SUSY should already have been seen at the Tevatron, so it was a sure thing that it would be there in the early LHC data. Now that it's not, people are trying desperately to find ways around their earlier predictions. I don't think it's going to work. The argument that "well, we didn't really mean it when we said SUSY had to show up at the LHC if it was going to stabilize the electroweak scale", or "so what if SUSY doesn't stabilize the electroweak scale" is going to be tried by SUSY proponents, but I don't think it's going to fly.

36. **SA**
March 28, 2012

Peter,

This is not really a matter of opinion. We can be specific here. If the light CP-even Higgs has a mass of about 125 GeV, then even in the case of minimal supergravity, one can stabilize the electroweak scale while being totally consistent with other experiments. The task is even simpler if we allow the gaugino fields to have different masses at the high scale.

Indeed, we have no evidence to believe that SUSY is correct. However, the only accurate statement to make at this point is that if SUSY is correct, the scalars must be very heavy, in most models. It would of course be useless to keep studying SUSY if additionally we need the spinors to be very massive also. This is not the case yet, and certainly hasn't been so due to Tevatron data.

I am not sure exactly what statement you are referring to by Nima, but it's possible that he is referring to a very specific model or he is considering other qualities such as naturalness.

37. **SA**
March 28, 2012

I would like to add that the main issue that I see with the minimal forms of local SUSY is that there is a tension between the heavy scalars implied by both the apparent Higgs mass and indirect limits from B-physics, and the need for larger corrections to g_\mu.

38. **Peter Woit**
March 28, 2012

SA,

See for instance his talk at Strings 2005 (link and discussion here:

http://www.math.columbia.edu/~woit/wordpress/?p=3492

It seems to me that the definition of "minimal SUSY" you are using is a new one, which actually means "obscure corner of the large SUSY parameter space that is the hardest for the experiments to rule out, so they haven't got there yet".

39. **SA**
March 28, 2012

Peter,

No. Look at the mSUGRA model. Suppose that the Higgs is 125 GeV. Then, you are in the region to which you are referring. Coincidentally, the other constraints are satisfied.

40. **Peter Woit**
March 28, 2012

SA,

I see. But I guess according to Arkani-Hamed, these would just be models too fine-tuned for SUSY to explain what is stabilizing the electroweak scale, so the answer is that SUSY doesn't do that.

It's unclear to me then what SUSY actually does for you other than make your model much much more complicated than the SM. I suppose if the LHC sees no hint of SUSY, these models will still live, but there won't be much motivation for them.

41. **SA**
March 29, 2012

Yes, exactly!

Nothingness Smackdown

March 25, 2012
Categories: Uncategorized

Theoretical physics, as practiced in the mainstream media, seems to be moving from a mania about multiverses to a religious battle over nothingness. On one side we have physicist Lawrence Krauss, with his best-selling new entry into the atheism book sweepstakes: A Universe from Nothing. Krauss is backed by Richard Dawkins, who compares the book's devastating effect on religion to that of Darwin's.

On the other side we have philosopher David Albert, backed by a million dollars from the Templeton Foundation (see more here), who has a review out this morning in the New York Times characterizing Krauss as "pale, small, silly, nerdy" (his ideas, not him, I think).

The big debate here is over what one means by "nothingness", which seems to me characterizable as nothing of interest. I guess though that there is a lot of money to be made in the nothingness business. The next opportunity for a big payday from nothingness will be on Thursday, 11 AM GMT, when Templeton will announce who gets this year's $1.7 million dollar prize. I've no idea who will get it, just that it won't be Krauss.

Comments

1. **uair01**
   March 25, 2012

   I have never risen above the technical level of the "Not even wrong" book but I seem to remember that "nothing" does not really exist in modern physics. Isn't vacuum a seething mass of appearing and annihilating particles? And isn't empty space not some kind of weird background-generating quantum foam? So is "nothing" a viable concept? If some expert could clarify this for me in simple language 🙂

2. **Giotis**
   March 25, 2012

   Contrary to what some people might think little progress has been made in the area. Krauss to answer the question uses Hatrle's Hawking good old 'No boundary proposal' and Vilenkin's tunneling wave function which by the way don't agree with each other. These proposals however are 30 years old and not much have been found since. In fact spontaneous creation of a Universe from nothing (no space-time) is poorly understood and these proposals are semiclassical approximations.

   Of course we have learned some things, for example that eternal inflation can't be past eternal and thus a beginning is needed even in a eternally inflating Universe. LQC guys have found that their models predict a bounce instead of a singularity for FRW metrics at least but they don't have a clue on what triggered the Universe at the very beginning. String theory on the other hand has little to say about cosmological singularities.

   Lacking a theory of Quantum Gravity physicists can't brag that they have answers (or even hints) for these profound questions.

3. **Marcus**
   March 25, 2012

   Giotis: "LQC guys have found that their models predict a bounce instead of a singularity for FRW metrics at least but they don't have a clue on what triggered the Universe at the very beginning."

   What do you mean by "at the very beginning"? We have no reason to imagine a "very beginning", certainly not on the basis of a conjecture like "eternal inflation".

   What the LQC guys aim to do in this respect is simply to resolve the cosmological singularity in a testable way–that predicts features of the cosmic background radiation that can be looked for–and provide for adequate one-time inflation. So the goal is not to explain "why existence exists" :-D.

   I think it's naive and premature to ask "why existence exists" at this point. A more reasonable goal is simply to model what nature was doing shortly before and after the start of expansion, in a way that makes predictions. It's important to be able to judge when a question is ready to be asked.

4. **Giotis**
   March 25, 2012

   I meant the simple fact that a bounce presupposes a collapsing Universe but what gave birth to this Universe? They don't have a clue but since their 'difference equation' approximates the Wheeler de Wit equation and the Harltle-Hawking wave function is a solution of the Wheeler de Wit equation I guess they have to to resort to the usual method to answer the question regarding the birth of the Universe. I know though that they are on trying to answer such questions.

5. **emile**
   March 25, 2012

   That last sentence of the review that includes the "nerdy, pale, small, …" should have been left out. It undermines the review, which I thought made some good points, and makes the author sound like he is insecure. In any case, I'm kind of partial to Steven Weinberg's comment on the unreasonable ineffectiveness of philosophy in physics (to be contrasted with the unreasonable effectiveness of mathematics in the physical sciences of Wigner). If you are going to blur the line between science and scientific speculation and then philosophy or even religion, you'll have to argue with philosophers without solid experimental evidence to back you up. Good luck…

6. **Mike**
   March 25, 2012

   It seems like we should exorcise the mysticism in physics before hammering out issues with the religious philosophers – when we are battling things like F-theory (because the mathematics is "beautiful") and multiverse mania where experimental evidence is seen as a 19th century thing, we don't stand much chance proposing the use of logical reasoning.

7. **Joe**
   March 25, 2012

   I saw this review on Friday already, and left this post on my FB page:

   "This is possibly the WORST review EVER in the NYT — He completely trashes the book simply because he disagrees with the premise; and the last sentence is virtually incomprehensible (from a Columbia Philosophy Professor !!!). NYT, you're scraping the bottom of the barrel with this one."

The Templeton connection at least now explains the idiotic review. What remains unexplained, is why the NYT would publish such claptrap, much less pay someone good money for it, especially since the politico-religious leanings of the NYT is surely far more toward the viewpoints of the author, rather than this inane critic.

8. **Bernhard**
March 25, 2012

"Richard Dawkins, who compares the book's devastating effect on religion to that of Darwin's."

That is an awfully silly comparison to make an Dawkins should know better. Darwin had a solid theory backed-up with overwhelming amount of scientific evidence while what Krauss is discussing, while could be interesting in a philosophical level, it seems to make no harm at all to religion as any arguments he might have, are not backed-up by any evidence (as far as I know, someone can correct me here, but I find it hard..). I believe this is the part that actually matters about Darwin, i.e., a scientific theory that actually does tell something about who we are with evidence to corroborate it. What Krauss is doing refers to very old philosophical debate, which will make no smart religious person lose the appetite.

9. **former mathematician**
March 25, 2012

The language "pale, small, etc." did not refer to Krauss. Nor did it refer to his ideas, exactly.

It referred to an attack on religion because, as the reviewer paraphrased, it can be dumb. Rather than, say, cruel, oppressive, destructive, or any of the various big things that anticlericals used to cite.

The reviewer attacks Krauss's notion that the absence of material particles is a reasonable definition of "nothing." I found the attack convincing. Am I missing something?

10. **Peter Woit**
March 25, 2012

former mathematician,

The exact quote is

"all that gets offered to us now, by guys like these, in books like this, is the pale, small, silly, nerdy accusation…"

which does explicitly refer to Krauss's ideas, not his physique. However, it is a fact that Krauss is physically rather short, rather pale, and, like lots of us, rather nerdy. In addition, Albert makes it pretty clear that "silly" is what he thinks of Krauss in general. I don't think it's a coincidence that the four terms he uses could be applied to Krauss as a physical person. In particular, the term "nerdy" actually makes little sense except as a personal comment. Who has ever heard an academic criticize a scholarly argument because it is "nerdy"? All scholarly arguments are "nerdy", by the definition of "nerdy" common among people I know.

As for the argument about whether it's accurate to refer to a physical state with "nothing" in it as "nothing", that seems to me about as intellectually empty as the vacuum is physically empty.

11. **CU Phil**
March 25, 2012

I think "former mathematician" has read Albert's review entirely correctly — in particular, the significance of the "pale, small, etc." comment — while some of the other misreadings on display here are somewhat startling. Albert simply thought Krauss's argument was a bad one, which is worlds apart from defending the religious viewpoint that Krauss is attacking.

Peter's describing the review as part of a "religious battle over nothingness" and mentioning Albert's Templeton funding strikes me as extremely disingenuous. Albert himself is an atheist, and has spoken in the past at conferences such as Beyond Belief. Again, Albert simply thought that Krauss's argument was a bad one, and that many "new-atheist" criticisms of religion seem to be misdirected, ignoring the most pernicious aspects of religion by focusing on the fact that it can be dumb. Short of willful misreading, I'm not really sure how one could understand the review to be saying much more than that.

12. **Brathmore**
March 25, 2012

Peter,

This blog used to be critical of polemical, imprecise attacks on people, ideas, etc. When it comes to your recent discussions addressing religion, however, it seems that you don't hold yourself to this same high standard. Quoting fragments out of context? Come on. Please. Here's the full, final paragraph of the article, so that people can read the full context of the reviewer's words.

"And I guess it ought to be mentioned, quite apart from the question of whether anything Krauss says turns out to be true or false, that the whole business of approaching the struggle with religion as if it were a card game, or a horse race, or some kind of battle of wits, just feels all wrong — or it does, at any rate, to me. When I was growing up, where I was growing up, there was a critique of religion according to which religion was cruel, and a lie, and a mechanism of enslavement, and something full of loathing and contempt for everything essentially human. Maybe that was true and maybe it wasn't, but it had to do with important things — it had to do, that is, with history, and with suffering, and with the hope of a better world — and it seems like a pity, and more than a pity, and worse than a pity, with all that in the back of one's head, to think that all that gets offered to us now, by guys like these, in books like this, is the pale, small, silly, nerdy accusation that religion is, I don't know, dumb."

13. **Anonymous Atheist**
March 25, 2012

I think the idea was that Darwin gave us an alternative explanation of how the astounding complexity of life could have come into existence *without* an intelligent creator crafting biological machines, and a universe from nothing likewise is supposed to provide an alternative route to the creation of energy and matter in the first place.

The difference is Darwinism is accessible to controlled experiment and observational confirmation and has proven to be a central organizing principle of all biology. It provides a framework of understanding that advances biological science.

It is hard to imagine any tome on how the universe came to be having a similar impact, I believe it is in essence an untestable assertion, and there is no price to pay for the religionists simply continuing to claim that God initiated the whole shebang.

14. **Peter Woit**

March 25, 2012

Brathmore,

I should perhaps make clear, if it isn't already, that I have equally little sympathy for or interest in either side of this argument, and plead guilty to making fun here of both of them because of this. As far as I'm concerned, theoretical and experimental cosmology is an interesting science, but trying to drag arguments about religion into it just leads to wasting your time on meaningless nonsense. This is true whether you are trying to sell books with your arguments about God, or review said books by going on about whether their theological arguments are sufficiently colorful, large, serious and manly.

15. **Peter Shor**
March 25, 2012

Couldn't we agree that the question "why is there something rather than nothing?" is outside the purview of science? Physicists who start trying to answer it are only going to end up making fools of themselves and giving the creationists and other religious extremists more ammunition.

16. **abbyyorker**
March 25, 2012

I'd like to read the Krauss book and see if it says anything im interested in. But i look on amazon and the kindle version is more expensive than hardcover. I just cant buy it! Guess ill be rethinking my kindle addiction.

17. **Christian Takacs**
March 26, 2012

@Anonymous Atheist,
Your moniker reveals your own faith, which is just as strained as any other belief to prove something in the absence of evidence. Your belief depends from something you can not confirm, so it is strange you hold such disdain for others who share your justifications. As to what Charles Darwin said, or tried to demonstrate, please try reading what the man wrote before making such statements. Darwin never said his theory explained the orgin of life or sought to say it could, it attempted to explain how EXISTING life might change through natural selection, not how it came to be in the first place. To this day Evolutionists really do not know how life started, and have speculated life might not have originated on this planet but traveled here on meteroites or ???… this only keeps kicking the question down the hall of course and is of course based entirely on speculation.
@Peter,
I think you should think about what you have written in your own book before you make light of what is in other peoples books. Considering some of the amazing things people who call themselves Physicists are saying these days, and using the most tortured of logics to support their theories, or claiming they no longer have to even provide testible evidence, or claiming computer mathematical modeling predictions are equal to or more valid than physical experimentation, I have to wonder, do even you know now where the line is between mathematical abstraction and testible reality?? Since quantum mechanics and maybe even before, physics has tried it's best to jettison all mechanical underpinnings to the massless point where not much is based on anything anymore, except perhaps the 'interpretation' of a few men in a very small shibboleth who told their devout followers to 'pay no attentioon to the man behind the curtain!' and later to "shut up and calculate" . You should not say one word about religion being meaningless nonsense Peter, or looking down your nose at those like Albert Eisnstein who even humbly admitted "Everything that can be counted does not necessarily count; everything that counts cannot necessarily be counted." You may know your math, but you seem to not know a great deal of your mathematical history or philosophy concerning those who came before you and what they believed. You presently stand on the shoulders of many far greater than you who you would sneer at. If you really want to help physics Peter, try cleaning up the hubris that seems to have infected it. The smug and arrogant are not ever likely to catch their own mistakes or be capable of making corrections. People such as yourself depend on the good graces of funding, from many many people… Most of them who believe in something you have shown disdain for. I don't think I need a large hadron collider or a quantum computer fiasco to see a large part of the physics community has become detatched from the people who would fund it through their taxes. Money for research into how many superstrings can dance on the head of esoteric singularities in the nth dimension is going to be in short supply very soon, so I suspect some financial re-evaluating will soon take place that will bring the HEP fabulists back to earth in their own unique version of "The Hunger Games". It would be nice if when this 'big crunch' occurs, someone like youself has some suggestions on where to start over and try again from.

18. **Ray**
March 26, 2012

I love the title of the article.

19. **Anonymous Atheist**
March 26, 2012

Christian Takacs:

Your moniker reveals your own faith, which is just as strained as any other belief to prove something in the absence of evidence.

There is no strain. As a scientist, in the absence of evidence I do not believe anything; including claims of the existence of something for which I believe there is no evidence whatsoever, like a creator. I am comfortable with the stance that we do not know how the universe began.

Your belief depends from something you can not confirm,

You have this backwards. Atheism is a **lack** of belief in magical beings. I refuse to believe in something that cannot be confirmed. it is religionists that believe in something they cannot confirm, the essence of religion is "faith," which is a belief immune to evidence, even contradicting evidence or logic.

As to what Charles Darwin said, or tried to demonstrate, please try reading what the man wrote before making such statements.

I have read "On the Origin of Species" cover-to-cover twice.

Darwin never said his theory explained the origin of life or sought to say it could,

Neither did I. I said he gave us an explanation for how the *complexity* of life could have come into being, which I think refuted the vast majority of arguments for God that depended on what is now called "intelligent design." IMO very few scientists of any stripe remain that think "functional complexity" demands any intelligence to design at all, not even something as functionally complex as intelligence itself: The human mind.
As it happens, however, as with many scientific discoveries, Darwin's principles would indeed provide a route to the origin of life, because all we really need is the random assembly of a barely functional and laughably inefficient Rube Goldberg self-replicator, somewhere in the vastness of the universe, to get natural selection started. IMO, that in turn reduced the majesty of the "creation of life" considerably. Because of Darwin, one can imagine life started as a simple microscopic entity as small as a virus. That puts the "creation of life" firmly into the realm of a chance chemical reaction.

To this day Evolutionists really do not know how life started, and have speculated life might not have originated on this planet but traveled here on meteroites or ???…

By analogy, to this day, physicists have not quite figured out quantum gravity either. That does not mean these problems are insoluble for all time, or that God gave us gravity and we should not question it. Science advances by testable speculation, and testable speculations evolve from less testable speculations.

It is not based entirely on speculation, it is informed speculation based on observation. Various chemical compounds necessary to the amino acid bases of RNA and DNA are found (by spectral analysis) in distant interstellar clouds. Some are unexpectedly complex. So life may have had more than just a few billion years on Earth to evolve; the ingredients of life, the chemical basis of RNA replicators, may well have been cooking for billions of years before that. That is speculation based on evidence, and informs the debate by doubling or tripling the time line within which complexity could have evolved and matured.

20. **Peter Woit**
March 26, 2012

Atheists and Believers alike,

There are a thousand other web-sites devoted to endless debates about religion/evolution etc, please take these to one of them, and stick to the topic here.

21. **Anonymous Atheist**
March 26, 2012

@Peter: I apologize; I was originally responding to the hyperbolic praise of Krauss' book by Dawkins, which was in your post.

22. **Nihilist**
March 26, 2012

if you want to know what "nothing" means in physics, you should read this paper

http://arxiv.org/abs/1111.0301

23. **The Vlad**
March 26, 2012

Nihilist,

Thanks, that made my day! I thought the paper was a joke when I read the following line in the abstract:

"bubble solutions that are close to this limit, bubbles of next-to-
nothing, give us a controlled setting in which to understand nothing"

But then I skimmed the manuscript and realized the authors were serious. Wow, has theoretical physics really come to this? You guys really need new data, bad!

24. **cormac**
March 26, 2012

Read the book recently, enjoyed it very much. Simple ideas, simply explained. I don't agree with all of it but it is beautifully written.
Just read the review. Poorly written, full of sciency jargon and not very clear. I've never heard of this philosopher but I don't think he has a good understanding of the area, nor does he express himself very well. Are you sure he's not a novelist?

25. **pakri**
March 27, 2012

Why can't we just accept the present evidence of a universe that is expanding
at an accelarating rate and move on from there? Of what use is it to the study of
Physics to ask whether it all came from nothing ?
The questions which seem to be troubling experienced, serious physicists
were asked more than 2500 years ago. The unequivocal answer was that (without
the use of our complex maths) such questions were profitless.

26. **Jadine**
March 27, 2012

I don't think Karuss' efforts should be belittled. I would even say that they're admirable.

There are, as far I can see, two reasons why some would describe this discussion as pointless:

1- Thinking that the specific question is silly.

Well, I agree. And to be fair I think so does Krauss (one can certainly criticize his book if this much wasn't stated clearly). When one cannot speak meaningfully about something, then one should not speak at all. But one still ought to explain why certain things are meaningless and propose more meaningfull questions. The way I see what Krauss is saying is this: "Look, it's completely meaningless to speak about 'something coming form nothing (as in physical non-existence)', we don't know whether such a thing as 'nothingness' exists
and it's not clear whether it's even a possible abstraction. However, I can tell you how something which is pretty darn close to nothing can possibly give rise to an entire universe".

This is no different than Darwin saying that he can't explain to you how non-life becomes life, but that he can illuminate how something pretty darn close to being inanimate can evolve into millions and millions of species. Sure, Darwin had a testable, predictive theory, and Krauss doesn't. But theories start as speculation, and all that Krauss is doing is explaining that all of this is possible (I'll leave it to a physicist to comment on the validity of the science); initiating a more meaningful discourse in the public about a view that should not be discounted.

2- Thinking that the whole subject matter (religon vs skepticism) is pointless

As silly as both parts of the debate can be, it's not pointless. We might think it's an issue which has been settled a long time ago, but obviously some don't agree. And they will use everything in their arsenal, including butchering science, to attempt to justify their claims. These claims have a real impact in the world, from public policy to people brainwashing children. Certainly scientists, at least those interested in educating people, ought to say "Here's what science has to say about that bit" every once in a while. Dismayed as they may be for putting impircal science behind them for a bit to venture into lands of philosophical speculation, it is ultimately something that will educate people on how the cutting-edge science of their time informs other areas. I see no harm arising from this, only good when scientists are clear about what is based on evidence and what is speculative.

27. **Danny**
March 27, 2012

That seemed more like a spell-checked Youtube posting than a NYT book review. In the third paragraph, David Albert seems oblivious to Bell's work; in the fourth and fifth, he seems to reject the meaningfulness or even usefulness of canonical formalism (Hilbert space, states, operators, etc); and throughout, he's got an axe to grind — consider the first sentence of each paragraph.

Positivist atheist scientists have been picking a lot of fights lately, between the battle over intelligent design in the USA, the "new atheism", these sorts of cosmological statements, the abundance of underemployed math/physics guys trying to apply their techniques to the humanities and financial marks, and the closer ties to philosophy departments. Unfortunately, theologians and philosophers are quite well-trained in rhetoric, and "pale, small, silly, nerdy" scientists don't exactly have the public on their side. If we're doing research that doesn't have direct applications, we'd better not be seen as ridiculous and expendable, either in the classroom, in academia, or in the public sphere.

28. **Bernhard**
March 28, 2012

Peter,

off-topic: I just read this review on "Echoes of The Big Bang", looks quite interesting. The best is that the guy seems unsympathetic to the multiverse thing, so potentially a serious book…

http://online.wsj.com/article/SB10001424052702304459804577285901885706224.html

29. **Phil**
March 28, 2012

The vacuum and the laws of physics undelying it are sort of like a bank account with no money in it(or perhaps in the quantum case one which has tiny daily deposits and withdrawals that average out to zero), which of course still has a bank and all the underlying banking rules supporting it. Trying to posit the vacuum as "nothing" and positing the ultimate existence of everything out of it it really about as silly as trying to "explain" the existence of the bank as arising from the empty bank account.

30. **xristy**
March 31, 2012

I don't think that the big debate is over the meaning of "nothingness." The big debate is over using scientific credentials to lend authenticity to absurd claims.

I read Krauss' book and in parts it was a workable overview of some of the current thinking in cosmological theory. On the other hand Albert is completely correct in questioning the futility of trying to chase the indefinite regress of how it comes to be that there are laws or that there are unstable vacuum states or whatever. Krauss doesn't offer an answer to this and I agree with Woit that there is little point in trying to argue one side or the other of such origins.

On the whole I thought Albert's review was reasonable. There's really no benefit to most of us in this battle of pseudo answers to ineffective questions.

31. **TB**
April 2, 2012

Pakri said: "Why can't we just accept the present evidence of a universe that is expanding at an accelarating rate and move on from there? Of what use is it to the study of Physics to ask whether it all came from nothing ?"

There is utility in speculation – it can point us in directions we might not think to go. But I kind of agree with Anonymous Atheist when he says "it is religionists that believe in something they cannot confirm, the essence of religion is "faith," which is a belief immune to evidence, even contradicting evidence or logic."

The question I have is when does speculation cross the line to religion? One thing I disagree with AA is in his definition of faith. It doesn't have to be immune to evidence or contradictions. It can be belief with an absence of evidence, and I think we need to be careful about defining things based on how people misuse it. After all, people put their faith in many earthly things now and are not disappointed.

To get this back in topic, my concern is when the rate of adopting a speculative idea is inverse to the likelihood that there can be evidence to support/refute that idea. When does the adopting become faith? At what point can we say the multiverse stands in for god in this new religion?

32. **Jamie Portsmouth**
April 5, 2012

> As for the argument about whether it's accurate to refer to a physical state with "nothing" in it as "nothing", that seems to me about as intellectually empty as the vacuum is physically empty.

Peter,

Are you saying that in your view, the vacuum ("a physical state with nothing in it") should be regarded as self-evidently identical to "nothing"? You seem to imply that arguing against that assertion is "intellectually empty".

It seems to me that Albert is quite correct in pointing out that the vacuum state of a relativistic quantum field is manifestly *not* nothing, but a particular state of a well-defined physical entity, which obeys some non-trivial mathematical rules (the origin of which we are completely ignorant of). The quantum field is also embedded in and evolving with a background spacetime with its own dynamics, and since we don't have a compelling theory of quantum gravity/spacetime, we don't really understand in detail how the interaction between spacetime and quantum fields works (except in semi-classical approximation). It seems likely that spacetime and the quantum fields therein both arise from some deeper theory which we don't currently have access to (but it isn't string theory, I'm sure you would agree).

So to argue that since the vacuum is unstable in relativistic QFT implies that we fully understand how the universe might arise from literally nothing (which is how Albert characterizes Krauss' core argument) is indeed pretty silly isn't it?

Anyway, in my view, Albert's critique is an interesting and important one, not remotely "intellectually empty".

33. **Peter Woit**
April 5, 2012

Jamie,

Krauss is using the word “nothing” in one perfectly legitimate way according to the dictionary (“nothing”= no thing there = perfectly good way to define the vacuum), Albert is using it in another. There doesn’t seem to me to be even a slightly interesting question at stake in this argument over this word. If others think this is an interesting and non-trivial argument, that’s fine. I just don’t.

34. **Bob Levine**
April 5, 2012

Peter—

I think you’re doing your *own* point of view a disservice by conflating Krauss’ and Albert’s discussions of ‘nothing’ and rendering a symmetrically negative judgment on both their houses. So far as I can see, you and Albert are on the same line of the same page. He’s doing what any good analytic philosopher does: attempt to eliminate spurious philosophical (or, as in this case, putatively scientific) problems by locating the source of these non-problems in the category errors people make in the way they talk about the issues in question. What Albert sees as Krauss’ bogus solution arises from conflating (i) the vacuum state of a universe loaded with fields, fluctuations, curved spacetime etc. on the one hand with (ii) the absence of any entities whatever. The result of this error is that Krauss appears to be making the question itself one which has scientific content; Albert’s point—which is exactly yours, no?—is that the question has no scientific content whatever, and that this is apparent once one sees that Krauss has confused the physicist’s vacuum state with the complete absence of anything, anything at all, which is what the theologians and metaphysicians are talking about when *they* pose the question. So, like Jamie, I’m surprised that you regard both Krauss (who propagates the error) and Albert (who tries to show the source of the error, and that the question, once Krauss’ confusion is eliminated, has no scientific denotation) as equally irrelevant from your own perspective….

35. **Peter Woit**
April 6, 2012

Bob Levine,

I just don’t think there’s an “error” here. Krauss is talking about one thing, Albert about another. Neither of them is interesting.

36. **Science_is_Testability**
April 9, 2012

Peter –

you are of course fully entitled to find this issue uninteresting, just as others find it uninteresting whether there is a multiverse or not, or whether string theory is good science or not. But what is at issue here is that Kraus is claiming to give scientific proof for things that are not scientifically provable: indeed he is claiming to solve philosophical problems that are thousands of years old by scientific methodology. This category error falls smack in to the “Not even wrong” category, claiming to give scientific proof when in fact he is offering philosophical speculation. Albert’s book review nails this pretension beautifully.

37. **Peter Woit**
April 9, 2012

Science_is_Testability,

I just can’t get interested in the philosophical/theological problem Krauss is claiming to solve, so don’t really care about the argument between him and Albert over whether Krauss has anything worthwhile to say about it.

I do care about whether the anthropic multiverse models Krauss is promoting as a solution to a scientific problem actually work (I think they don’t). If Albert and other “philosophers of cosmology” would address the question of whether these models are science or pseudo-science, that would be much more worthwhile.

Implications of LHC Results

March 28, 2012
Categories: Experimental HEP News

The winter conferences are now come and gone, with any dramatic new LHC results now likely to wait until more data is on hand. First attempt to collide beams at 4 TeV/beam is now scheduled for Friday morning, with stable beams for physics a week or so later. Results from the 2012 data should first start to arrive at the summer conferences.

This week at CERN there's a workshop on [Implications of LHC results for TeV-scale physics]. Lots of detailed information in the slides about the latest LHC bounds on non-SM physics. For a summary of the situation with the Higgs, SUSY and what it all means, you could do worse than take a look at the slides of [Alessandro Strumia], which include the sobering:

> Implications for European Strategy for Particle Physics: The Higgs could be the last particle. Carpe diem.

He describes the SUSY situation as "the naturalness motivation for weak scale SUSY is mostly gone", with the one loophole not yet ruled out a stop particle at accessible energies. This scenario has now been dubbed "natural SUSY" and will be a major focus of searches going forward.

There was a similar workshop organized last week at the University of Maryland, slides are [here]. Matt Strassler reports [here], [here] and [here]. Evidently there was a final panel discussion for which video may appear at some point.

Update: One more thing on the same topic, a very recent review of the implications of LHC results for SUSY phenomenology is [here].

Comments

1. **Bernhard**
   March 29, 2012

   Peter,

   the link for Alessandro´s talk is not working (you forgot an "i").

   But now, with a likely Higgs at 125 GeV what are the best options to stabilize the electroweak scale? When people finally throw in the towel on SUSY, I am not sure what the best options are. The problem is now really there, before we could always conjecture the whole Higgs idea was incorrect in the first place.

2. **Peter Woit**
   March 29, 2012

   Bernhard,

   Thanks, fixed.

   It's never been clear to me that "stabilizing the electroweak scale" is actually a real problem. Yes, the ratio of electroweak scale to the GUT scale and the Planck scale is a small number you would have to explain, but you don't know there even is a GUT scale, and who knows what the significance of the Planck scale is.

   To me the main problem with the Higgs is that it looks like an effective field for something more interesting we don't understand, and this shows up as a long list of undetermined parameters (most of them the Yukawas). The fact that the LHC is starting to actually see the Higgs and measure its couplings is what is really exciting, hopefully something non-SM will turn up there.

3. **Dave Miller in Sacramento**
   March 30, 2012

   Peter,

   I'd be very curious to hear you elaborate some time on Strumia's point: "Implications for European Strategy for Particle Physics: The Higgs could be the last particle. Carpe diem."

   It seems pretty clear that much of the wackiness in theory in recent years has been due to a lack of new experimental data to chew on. Assuming the Higgs really has been found, and behaves much as it should, Strumia's point would seem to imply that we are in for many, many decades of repeating recent experience.

   Do you agree? What are your constructive suggestions?

   My own guess is that the best we can hope for is a long period, a half-century or more, of digesting everything that has happened since 1900: everything from figuring out how to do better calculations in QCD to figuring out better ways to explain, present, and understand renormalization, symmetries, general relativity, the "weirdness" of quantum mechanics, etc. Continuing work, including experiments, can continue on topics such as quantum computing, quantum teleportation, entanglement, etc., but almost certainly all that will again and again confirm the predictions of quantum mechanics.

   Of course, there are times when reflection and consolidation can be of value.

   And at worst? Well, let's just say the "landscape" might be just the beginning!

   Do you see a brighter prospect?

   Of course, nobody can predict the coming of a Newton or an Einstein, but, on the other hand, surely Newton and Einstein arrived at times when their genius could be applied to productive ends.

   Dave Miller in Sacramento

4. **Shantanu**
   March 30, 2012

   Dave, there are alternatives. Unfortunately people working on alternative ideas are considered mavericks and usually get no funding/jobs (with a few exceptions).

Even institutes like PI, which originally encouraged non-mainstream thinking are now focussing mainly on trendy stuff such as branes etc.for a few random examples, look up Einstein-Sciama-Kibble gravity, non-commutative geometry etc.
See BJ Bjorken's article at last year's Fermilab conference on this.

5. **no**
   March 30, 2012

   "the best we can hope for is a long period, a half-century or more, of digesting everything that has happened since 1900."
   Holy cow, I hope you're not doing research!

6. **Peter Woit**
   March 30, 2012

   The "SM Higgs and nothing else" LHC scenario has been looming on the horizon for a while, and its probability has been increasing, so Strumia's concern I think is on many people's minds. I'd prefer though to hear if people have any substantive comments on the discussions at these workshops. It's not like there's not enough discussion on this blog of the problems with current popular BSM scenarios or the need to encourage alternatives…

7. **Peter Woit**
   March 30, 2012

   From what I can tell, first collisions at 8 TeV center of mass energy are happening right about now. Still a few days at least though before stable beams and physics.

8. **Eric**
   March 31, 2012

   http://www.science20.com/quantum_diaries_survivor/unmistakable_gluino_signal_atlas-88569#comment-103930

   Time to eat crow, my friend.

9. **Eric**
   March 31, 2012

   On second thought, perhaps this is an April Fool's joke.

10. **Peter Woit**
    March 31, 2012

    Eric,

    Very exciting. I guess I was all wrong. Oh well.

    Can't possibly be an April Fools joke. It's only 11:48 March 31 in Geneva.

The Darth Vader Theory

March 30, 2012
Categories: Uncategorized

This week at Caltech there's a workshop celebrating the 35th anniversary of N=4 Super Yang-Mills theory. George Musser of Scientific American is covering the workshop here . He reports that N=4 Super Yang-Mills is being describe as the "Darth Vader theory", I guess by Nima Arkani-Hamed. The conjectural 6d (2,0) superconformal theory gets called "the Emperor Palpatine of theories".

Perhaps slides from the talks will be posted at some point. Witten will close the workshop tomorrow with a talk not about Darth Vader or the Emperor Palpatine, but about "Superstring perturbation theory revisited".

Update: Clifford Johnson reports on the conference here.

Update: Someone at the conference confirms that the "Darth Vader" description came from Arkani-Hamed, who in his talk said something like:

> "The relation between 4D N=4 SYM and the 6D (2, 0) theory is just like that between Darth Vader and the Emperor. You see Darth Vader and you think "Isn't he just great? How can anyone be greater than that? No way'.Then you meet the Emperor".

Update: A couple reports from the conference banquet. During his presentation Dan Freedman unveiled his new textbook on Supergravity (see here) and offered to sell copies to those attending the conference at 20% off. Stephen Hawking was there. He's in Pasadena for his yearly visit and to appear in an episode of "The Big Bang Theory". The show's writers and producers are very excited about the "Darth Vader/Emperor Palatine" thing and planning on working it into the show's script. Afterwards Hawking invited many people to join him and some of the cast of the show on a trip out to his favorite club in San Bernadino.

Update: Given that the previous update was written on April 1, readers might want to have some suspicions about whether it is completely accurate.

Update: Some slides from the talks are now available here.

Comments

1. **Anonyrat**
   March 30, 2012

   "Perhaps most intriguingly, (2,0) theory is irreducibly quantum."

   What does that mean? Does it mean it cannot exist in classical space-time, or indeed any classical manifold?

2. **A.J.**
   March 30, 2012

   Nope. It means that the (2,0) theory has no small parameters in flat 5+1 dimensional spacetime. It can't be approximated in the usual, using perturbation theory around a classical limit, in this setting. But the spacetime is classical.

3. **Anonyrat**
   March 30, 2012

   ^^^ What is so strange then about being "irreducibly quantum"?

4. **Giotis**
   March 31, 2012

   I think it should be emphasized why these top physicists are so interested for these local Quantum field theories.

   The N=4 Super Yang-Mills is the QFT which appears on the world volume of multiple D3 branes in 10D String theory. This theory gave birth to the notorious AdS/CFT correspondence. Similarly the (2,0) is conjectured to live on the world volume of multiple M5 branes in 11D M-theory. String theorists have already found the QFT living on the world volume of multiple M2 branes, the famous ABJM theory. It is amazing that string theory predicts the existence of certain consistent highly non trivial QFTs and then the theorists go on and prove its existence and consistency by explicitly constructing its action.

   This can't be a coincidence and this is one of the main reasons the elite of the physics community believe in String theory as the ultimate TOE.

5. **Bernhard**
   March 31, 2012

   Giotis,

   the notorious AdS/CFT correspondence seems to be more and more in tension with data.

   As for "String theorists have already found the QFT living on the world volume of multiple M2 branes, the famous ABJM theory" , this means only that string theory is able, from time to time, to connect with QFT in order to be alive, seems it has nothing else, no single prediction all. Well bravo for ST! The mathematical connection with QFT is an obligation not a merit and says absolutely nothing about its correctness. As far as I know, is ST´s only business now. I have my doubts on a TOE which the only power is to find increasingly difficult ways to connect to the theory that actually works (QFT).

   And last "theorists go on and prove its existence and consistency by explicitly constructing its action. " This is your idea of proving the existence of a theory? Construction of an an action?

6. **Peter Woit**
   March 31, 2012

   Anonyrat,

The “irreducibly quantum” business is often used as an explanation for why this theory is poorly understood (as AJ explains, there’s no usable semi-classical approximation).

It’s not really strange though. Another example would be the world’s simplest quantum system, a 2-d spin.

7. **Dan**
March 31, 2012

Recently I heard Nima describe N=4 SYM as “the harmonic oscillator of the 21st century”, which is probably my new favorite tongue-in-cheek hep-th joke…

8. **Anonyrat**
March 31, 2012

The Darth Vader and Emperor names given to those theories by Arkani-Hamed are really apt, for apart from string theory aficionados, everyone else is waiting for the Return of the Jedi.

Peter, exactly what I thought, but wasn’t sure – the “irreducible quantumness” of a theory hardly makes it more profound. Thanks!

9. **Giotis**
April 1, 2012

Bernard,

The prediction of the existence of certain highly non trivial QFTs does not prove of course that the theory is the correct paradigm of Nature, this is extremely difficult to achieve especially for theories in these extreme energy regimes; it does prove though that the theory is internally consistent and cohesive. This is an extremely important aspect of a TOE and characterises its theoretical quality.

10. **Thingumbobesquire**
April 1, 2012

I think you might have posted this a day early…

11. **Thingumbobesquire**
April 1, 2012

Oops, I should have said this was published two days early…

12. **Peter Woit**
April 1, 2012

Thingumbobesquire,

Yes, it is very hard to tell parody from reality in this business. However, I stick to absolute accuracy all the time here, with some exceptions one day a year. Here the posting was not written on that one day, although the updates were written later so I can’t vouch for them completely corresponding to reality.

13. **Giotis**
April 2, 2012

I’m confused…

Could you clarify the false parts of this post?

It seems that you have mixed truths with lies and this makes it difficult to find where the lies are.

14. **Peter Woit**
April 2, 2012

Giotis,

That’s funny. It appears that the goings on at string theory conferences are so bizarre that real events and outrageous fantasy can’t be distinguished…

15. **John Doe**
April 2, 2012

Are there any comments on the talk by Witten? It’s always interesting to have a clue as to what he’s up to…

16. **PerfectDigit**
April 2, 2012

@John Doe,
Here are slides of an earlier version of this talk by Witten:
http://www.princeton.edu/~masahito/confs/2011/Witten_PCTS2011.pdf

17. **John Doe**
April 3, 2012

PerfectDigit, thanks. Those slides are pretty detailed…

18. **gigel**
April 6, 2012

Spin systems aren’t “irreducibly quantum”. You can start from a classical system with degrees of freedom living on an internal sphere and quantize to get any spin you want.

19. **Peter Woit**

April 6, 2012

gigel,

The classical limit is when you take the spin quantum number to infinity, that's where you'll get an algebra of observables that looks like functions on a phase-space of a sphere. For the spin-1/2 rep, and the algebra of 2 by 2 matrices acting on it, you're in a situation not at all like the classical one, not in any sense a perturbation of it.

Nothingness in LA on April Fool's Day

April 1, 2012
Categories: Multiverse Mania

The media and blogosphere today are full of April Fool's Day jokes of various degrees of funniness. Then there's the Los Angeles Times, which used the date to publish a piece by Lawrence Krauss entitled [A Universe Without Purpose](). It promotes the argument that the multiverse is science's answer to religion, with in this case backing coming even from the LHC:

> Out of this radically new image of the universe at large scale have also come new ideas about physics at a small scale. The Large Hadron Collider has given tantalizing hints that the origin of mass, and therefore of all that we can see, is a kind of cosmic accident. Experiments in the collider bolster evidence of the existence of the "Higgs field," which apparently just happened to form throughout space in our universe; it is only because all elementary particles interact with this field that they have the mass we observe today.

Maybe it's just my defective sense of humor, but I'm not finding this funny.

Comments

1. **Low Math, Meekly Interacting**
   April 1, 2012

   Then there's this quote:

   "Perhaps most remarkable of all, not only is it now plausible, in a scientific sense, that our universe came from nothing, if we ask what properties a universe created from nothing would have, it appears that these properties resemble precisely the universe we live in."

   If we're the products of an accident in the multiverse, these properties are, at best, a parochial curiosity among a vast ensemble of "universes" with unimaginably diverse properties, right? Does this not imply that if we ask what properties a universe created from nothing would have, it would appear that anything goes? How does anything remotely resembling "precision", beyond perhaps a basic need for mathematical consistency, follow?

2. **billy the skeptic**
   April 1, 2012

   Okay – let me see if I've got this right:

   In order for us to explain our universe as NOT being part of some grand design or intended creation with a purpose – we have to assume that there are many of them? Meaning that some athiests are conceding that "how" our universe popped into existence is so unlikely that there must be many because if this is the only one, then it could possibly be due to purposeful creation? Really???
   How many other universes would need to exist in the multiverse in order to find our random but good fortune statistically plausable?
   I myself am an agnostic, which means the religious folk don't like me because I'm a nonbeliever, and the athiests don't like me because I have a fear of commitment.

   If you believe this multiverse nonsense, then I assume you must also believe we are far luckier than those 3 Powerball winners this weekend!

3. **Yatima**
   April 1, 2012

   >I'm not finding this funny.

   That's because it's not an April Fool's joke, I reckon.
   But I don't see where the problem is. Only a person deeply encumbered by religion would find anything to object here.

4. **Anonyrat**
   April 1, 2012

   Lawrence Krauss writes: "Asking why we live in a universe of something rather than nothing may be no more meaningful than asking why some flowers are red and others blue."

   In order to come to this conclusion, he has to invoke the latest in cosmology and speculative particle physics theory.

   But I would say that "asking why we live in a universe of something rather than nothing" is a priori without meaning. It did not have and does not have any meaning within a scientific framework. The question is meaningful in the framework of one particular theology which, however, is hardly universal.

5. **Aidyan**
   April 1, 2012

   "…. all that we can see, is a kind of cosmic accident."

   And what is an "accident"? What is in the eyes of a tiny ant the world outside of its anthill if not an unintelligible stream of random "accidents"?

6. **paddy**
   April 1, 2012

   Pardon me for waxing a wee bit pop-ish on this april fools day..but:
   (A) Just when did the jester steal the thorny crown of physics?
   (B) When did some physicists think it was okay to jump the shark?
   (C) How valid are their opinions in the face of the "Julie Andrews assertion": i.e., "Nothing comes from nothing, nothing ever could".

7. **SpearMarktheSecond**
   April 1, 2012

   Krauss has always hopped on the publicity machine… \`The Physics of Star Trek'. For a publicity guy, he knows a lot of physics, but he doesn't care to mention that most of the mass in our own matter has nothing to do with the Higgs, but originates in dimensional transmutation in QCD, and that we have no idea whether or not the Higgs has something to do with the mass in dark matter.

N-rays, polywater, faster-than-light-neutrinos, cold fusion… hype and hokum are always out there and always will be.

Meanwhile new interesting physics always breaks out in areas overlooked by the publicitarians…. 100 or so earth like planets! Now that is real and exciting. Good that SETI got some money to point at them!

8. **Peter Woit**
April 1, 2012

SpearMarktheSecond,

Yes, but even giving Krauss the simplification of ignoring QCD and dark matter mass contributions, my problem is with the remainder. I don't see what the justification is for claiming LHC Higgs results as evidence that the Higgs and its Yukawa couplings are "a cosmic accident", "which apparently just happened to form throughout space in our universe". The question of what the Higgs really is and where its Yukawas come from is a deep mystery we don't understand. The multiverse philosophy is to sweep everything we don't understand under the rug as a "cosmic accident". You can do this if you want, but I don't think you should claim experimental backing for this pseudo-scientific procedure.

9. **pfogle@tiscali.co.uk**
April 1, 2012

there's science and there's speculation… each can give rise to the other. Maybe all speculative ideas should be written in red type, just to remind us that they're not science!

10. **SC**
April 1, 2012

"Only a person deeply encumbered by religion would find anything to object here."

I don't think it's the case that only religious people care about abuses of science. If someone runs around saying "science shows" what science doesn't actually show (and may, in fact, be unable to show even in principle), anyone who values science should be annoyed. Even if the sentence is "science shows God doesn't exist!"

It's as bad when an atheist does it as it is when a creationist does it. Perhaps worse, since at least one can easily identify the creationist's bias.

11. **Bee**
April 2, 2012

I've figured it out: We live in a computer simulation, and it has a loose cable.

12. **Christian Takacs**
April 2, 2012

I am not a believer in creationist Mythology, but I certainly am not a believer in the Big Bang Theory ex nihilo ad nauseam either, because quite frankly, knowing with certainty anything that happened fourteen or more billion years ago is quite speculative to the point of absurdity, and claiming it popped out of nothing is "Credo quia absurdum". Are the leaders of the scientific community really so scared of "I honestly don't know, we still have to find out" that they debase themselves with the inaccurate bravado of "Oh yes, we know almost everything, we just have a few last calculations to work out" ? Think of it this way, if you can't admit the limits of your knowledge, how are you going to actually to learn anything that will allow you to extend this limit? I think many potential scientists are discouraged from going into the sciences and making real discoveries because of the arrogance and hubris of the present scientific community that makes wild claims to knowledge they don't possess, Much like Bohr telling Einstein "it is the best we can do and the best we can ever do"… Hogwash, pure and simple, it's the fallacy of the appeal to (scientific) authority telling future generations not to look for answers anywhere that might challange todays dogmas.

13. **Emanuel Derman**
April 2, 2012

It is deeply dishonest to pretend he understands the mysteries of the Higgs, even more so to claim it follows from the multiverse. Probably it's just PR.

But, you don't have to be a believer to see the wisdom of the following two statements:

> "When people stop believing in God, they don't believe in nothing — they believe in anything." – G.K. Chesterton.

And, from Saul Bellow:

> The philosopher Morris R. Cohen was once asked by a student, "Professor, how do I know that I exist?"
>
> "So?" Cohen replied. "And who is asking?"
>
> Thanks to Professor Cohen I feel that I stand on firmer ground, and can do what I have done all my life: i.e., to fall back instinctively on my first consciousness, which has always seemed to me to be most real and easily accessible. For people who have no access to any such core consciousness, no mysteries exist. Linguistic analysts aim to clear away all mysteries—alleged mysteries, they would say. Facts, however, must be respected, and the fact is that for reasons I can't explain, my own first consciousness has had a long unbroken history. I wouldn't know how to defend my faithful attachment to it. All I can say is that it is a fact and I wonder why anyone should feel it necessary to put its reality in doubt. But our meddling mental world puts all such realities in doubt. This world of truly modern, educated, advanced consciousness suspects the core consciousness that I take to be a fact of being inauthentic and probably delusive.

14. **BlizzardOfOz**
April 2, 2012

"The Large Hadron Collider has given tantalizing hints that the origin of mass, and therefore of all that we can see, is a kind of cosmic accident."

Yes, I'm sure the experiment "hints" "tantalizingly" at something he had already decided years ago. What a wanker.

15. **TB**
April 2, 2012

Peter said: “The multiverse philosophy is to sweep everything we don’t understand under the rug as a “cosmic accident”. You can do this if you want, but I don’t think you should claim experimental backing for this pseudo-scientific procedure.”

That’s interesting as that’s the same (valid) criticism of religion when they shrug their shoulders and say “god did it.” I suspect the rush to a multiverse has more political motivations than scientific.

16. **TB**
April 2, 2012

SC said: “It’s as bad when an atheist does it as it is when a creationist does it. Perhaps worse, since at least one can easily identify the creationist’s bias.”

Agreed. But that’s what seems to have changed in the past few years.

17. **Allan Rosenberg**
April 2, 2012

I’m a little disappointed by the four slash one post. I was hoping for some more NYC bakery recommendations or maybe a discussion of snide anti-string theory bumper stickers (“String Theorists Do It In Their Pants”). Maybe next year…

18. **Peter Woit**
April 2, 2012

Sorry Allan,

Not much news on the NYC bakery or biking fronts I fear. After extensive investigation, I can report though that an excellent Kouign Amann can be found in Paris at the boulangerie/patisserie on the square where Rue Monge and Boulevard St. Germain come together (i.e. at the Maubert-Mutualite Metro stop).

No one seems to have noticed this year’s much too subtle attempt at 4/1 humor, I guess no one reads the updates…

19. **Giotis**
April 2, 2012

Can’t you see why he is making this assertion Peter?

Currently in LHC we have light Higgs but no SUSY. If these results remain it means that extreme fine tuning is needed similar to CC. This means accident and this means multiverse.

20. **Peter Woit**
April 2, 2012

Giotis,

Yes, I understand that argument very well. I just don’t agree with it. That the ratio of the electroweak scale to the Planck scale is a very small number is an observed fact that we don’t understand. Saying that “the multiverse did it” just isn’t a scientific explanation unless you have a viable theory of the multiverse, testable in some way, even indirectly (which you don’t). At the moment, “the multiverse did it” is just a pseudo-scientific statement, being used to claim you have a scientific explanation, when you really just don’t know what is going on. This is a bad idea for lots of reasons, one of which is the one David Gross emphasizes: promoting a non-explanation for something you don’t understand discourages people from continuing to do the hard work of trying to find a real explanation. If you’re the sort who wants to argue religion, this doesn’t seem to me a good way to go: arguing against one non-explanation with another non-explanation is not going to convince anyone.

21. **Low Math, Meekly Interacting**
April 2, 2012

It’s not clear to me that the multiverse has any predictive value whatsoever, even in the complete absence of theoretical alternatives. If I read the history of the subject correctly, originally evidence of fine tuning in the value of the CC and phenomena at the electroweak scale, in combination with certain assumptions about what conditions were required to get flat universes with such-and-such matter content, complex chemistry, etc., left us with a stark choice between God or the multiverse. But those assumptions turned out to be far too narrow. Perhaps intelligent life and many other gross features of the universe we observe can be accommodated by a far greater range of constants, etc. than previously assumed. So it’s not even clear if we can make good estimates of probabilities of required conditions. Meanwhile, there’s probably no good way to estimate probabilities of finding some-or-other physical features in the Landscape. Apparently there are no solid constraints on, well, much of anything in the multiverse. You can simply posit its existence, twiddle the available knobs however you like, and satisfy yourself that what you get bears some resemblance to what we see.

If I’m totally or partially wrong about the above, I’m all ears. But, again, I haven’t the foggiest idea where the notion of “precision”, or anything remotely like a veritable “prediction” comes into any of this.

22. **Juan Ramón González Álvarez**
April 2, 2012

I think you missed this funny one: [First evidence for string theory at the Large Hadron Collider]

23. **Dan Winslow**
April 2, 2012

I’ve always wondered what ‘random’ means. People throw it around a lot in relation to quantum thingies and cosmology…Krauss uses it a lot. Lots of phrases like ‘…and then through random fluctuations of … ‘. But I’m not sure that the term random has any absolute meaning. I think it just means ‘for reasons unknown’…right? I think there’s a paradox about randomness, in that if you can define rules for deciding whats random then it’s no longer random, or something along those lines. Can any of the high powered folks here elucidate?

24. **paddy**
April 2, 2012

For my own April fools day exercize I attempted to calculate the transition probability from something to nothing and vice versa. That pesky density of states on the “nothing” side unfortunately kept coming up zero (e.g., nada, null, and–doh!–nothing). Not wishing to admit defeat, I am currently (a la a recent arxiv paper cited somewhere in this blog) attempting to sequentially approach “nothing” with diminishing state densities (as well as diminishing returns). Once again though I have a problem: trying to be careful about uniform convergence issues I keep losing track of my epsilons and deltas (physicist you see not a mathematician). Should someone find these characters (subscripted with “nothing”) I would be happy if they returned them.

25. **Peter Woit**
April 2, 2012

Dan,

The problem with current multiverse theories is that there is no viable underlying theory which would explain exactly what the different possible effective laws of physics would be, and allow computation of probability of any particular set of effective laws.

So, instead, people just talk about the “measure problem”, and say things are “random”, giving equal weight to all values of various parameters, for no reason other than that they don’t know what else to do. This has exactly the same implications as my “theory”, which is: I have no idea what determines the value of that parameter, so, a priori could be anything. This kind of “multiverse theory” is identical to complete ignorance. More honest if you ask me to just call it what it is.

26. **Don Murphy**
April 2, 2012

Peter

Nothing is wrong with your sense of humor.

Don

27. **Casey Leedom**
April 3, 2012

Okay, since you didn’t find that funny, try A Quantum Theory of Mitt Romney The New York Times author managed to find a way to very humorously integrate modern physics — including a multiverse reference — into the current year’s political landscape.

Note that I am explicitly **not** posting this as a “political” comment, simply as a humorous use of “physics speak” in the popular media. It would have been just as funny if it was about Obama, etc.

28. **N.**
April 3, 2012

@Christian Takacs:

You see, you get no funding for saying “I don’t know”…

It’s much wiser to say “I know almost everything about it, just need a little (better: a lot) more money”.

That’s it.

:))n.

29. **Adrian_H**
April 4, 2012

I’m not a great fan of Albert — his pugnacious style is off-putting to me, and he tends to weigh in too heavily on particular sides of debates. But I have to say that I completely agree with his review of Krauss. He nails it. OK there is still too much rhetoric, too much “this, and that, and tother, and your mother” rankling through the prose, but he is right about the way that Krauss is wrong.

Father of String Theory on LHC Funding

April 3, 2012
Categories: Uncategorized

A report from India:

> **'CERN need not spend so much on LHC experiment'**
>
> Father of String Theory and noted physics scientist Holger Bech Neilsen of Denmark has said that contributions from the Large Hadron Collider (LHC) at CERN are over-rated and that there is no need for spending so much on the experiment.
>
> Interacting with the students and faculty of National Institute of Technology (NIT), Warangal, here on Monday, Prof. Neilsen said that he discouraged spending huge funds on such research projects.
>
> When asked how he would justify the need for unification, considering the fact that individually theories such as quantum mechanics and gravitation have been showing good results, he said that there was no ardent need for a unified theory of everything but that such a theory would bring about new perspectives of understanding the world around us.
>
> Prof. Neilsen who was nominated twice for the Nobel Prize was here at NIT on a two-day visit at the invitation of the students.

The picture with the article show a blackboard where Nielsen has been explaining superstring theory to the students. The talk supposedly was on April 2, so presumably this wasn't an April 1 performance. I guess he's right: since string theory says nothing about LHC physics, the machine is kind of pointless.

Update: A commenter points to this video interview with Nielsen made during his trip to India.

Comments

1. **John Doe**
   April 3, 2012

   Somebody should tell them it's "Nielsen", not "Neilsen".

2. **SA**
   April 3, 2012

   This is embarrassing, disappointing, and I would claim also highly atypical. The LHC is the most crucial aspect of particle physics at the moment.

3. **Dan Winslow**
   April 3, 2012

   "he said that there was no ardent need for a unified theory of everything but that such a theory would bring about new perspectives of understanding the world around us."

   Wow, now *there's* an observation worth waiting for.

4. **Curious Wavefunction**
   April 3, 2012

   The article seems to have been deleted from the website.

5. **Zac**
   April 3, 2012

   @Curious Wavefunction

   No, Peter messed up the link. The article is here.

6. **Bernhard**
   April 3, 2012

   An how the heck do they know he was appointed twice for the Nobel prize? Do they really meant Nobel or was it the Oscar? Information about the Nobel nominations cannot be revealed until 50 years later.

7. **Peter Woit**
   April 3, 2012

   Link fixed.

8. **ComeOn**
   April 3, 2012

   Peter, this guy is a crank and he's been a crank for ages. The fact he did important work in the past is irrelevant. You shouldn't give publicity to this kind of stupidity.

9. **Arjun**
   April 3, 2012

   Didn't know there was a "father" of string theory. As to Bernhard's question about Nobel prizes, it is fashionable in Indian news media to mention Nobel prizes in various science related articles.

10. **Peter Woit**
    April 3, 2012

Arjun,

Nielsen doesn't actually have a Nobel prize (and this kind of thing won't help him get one). Being "nominated for a Nobel prize" is a rather low bar these days.

ComeOn,

Problem is that if I stop covering cranky behavior by theorists promoting string theory, there will be significantly fewer blog postings here. Hard to know where to draw the line…

11. **Student@NielsBohrInstitute**
April 3, 2012

I think I need to defend Holger Bech Nielsen here. I don't know what he has said in India, but I cannot recognize this picture of him. He is actually VERY excited about LHC, he has said that several times on danish television and radio shows and popular talks given to the public. People who have talked to him in person cannot recognize this attitude towards LHC, at least I can't.
I don't understand why Peter characterize him as a "cranky theorist, promoting string theory". Holger is famous for saying, during almost all popular talks, that he is one of the "fathers" of string theory (together with Nambu and Susskind) , but he is a bad father since he doesn't believe in his own child (meaning that he doesn't believe in string theory). I don't know why people think he is promoting string theory, nothing I have ever noticed.

He is an old guy now days, I think he is formally retired. So he works on more crazy ideas sometimes, nothing he is taking seriously himself. Sometime he is having fun and mentions for example "god" while explaining certain ideas, this has attracted some media attention and leading to people thinking that he is being serious. Talking to him it is clear that he is just having fun. Actually he is very bad at selling his crazy ideas since while giving talks and interviews about them, he usually emphasizes that they are probably all wrong and doesn't sincerely believe in them himself.

Finally, I don't know where they got any information about Nobel prize nominations since they are usually kept secret. Holger have many important contributions in theoretical physics (for example the Nielsen-Ninomiya no-go theorem and Niels-Olesen vortices), but I don't think any of them are Nobel prize worthy. I doubt he has been nominated twice.

I think this is more about bad and shallow journalism from "The Hindu", rather than about Holger's true beliefs.

12. **Peter Woit**
April 3, 2012

Student@NBI,

Thanks a lot for the information about Nielsen. It's probably true that the news report is a mangled version of whatever he was saying. As for whether he was promoting string theory at the talk, I was making assumptions based on the article and what little I could see of the board in the picture, but from what you say those assumptions are likely wrong.

As for whether he's a "cranky theorist" these days, I'm not sure he would disagree…

13. **Anonyrat**
April 3, 2012

Here is another report, with a little more than what the Hindu carried.
http://www.thehansindia.info/News/Article.asp?category=5&subCategory=5&ContentId=50228

14. **Anonyrat**
April 3, 2012

Nielsen is on a tour of India. Here is a 19:31 interview with Nielsen at IIT Kharagpur, by the college newspaper, Scholar's Avenue (scholarsavenue.org).

http://www.youtube.com/watch?v=h8LxN4fARU4

15. **Orwin O'Dowd**
April 4, 2012

No-go theorems are the critical experiments of the theory game, and any author deserves ba place in the Skeptics' Hall of Fame. Here's a view of what no-go theorems, and Nielsen's work in particular, meant for mainstream QCD: http://cerncourier.com/cws/article/cern/28271.

That was a decade ago, and at the physical limits of observation and computation, thoughts turn to pure logic and thus philosophy. Here's a no-go that takes out the whole Weyl/Wigner group theory stream: you can't make a universe of groups because the universe of groups is defined as the theory of groups (Lawere Theory)!

Time for theory of theory? But that's philosophy!

16. **emile**
April 4, 2012

I do not know any string theorists (or any particle theorists) who are not supportive of the LHC. There can be a big gap between what is written in the press and what someone actually thinks.

17. **somebody**
April 4, 2012

Holger is just having fun. He loves being crazy a bit, but he doesn't take himself too seriously.

His kind of personality is inevitable, and perhaps necessary, in a field like high energy theoretical physics. People who are outraged by this- sorry if I hurt your feelings, but yours is the imcomprehension that mediocrity has towards creativity. Theoretical physics is not accounting, it also has some poetry in it.

He has paid his dues by doing good science in his youth, and earned his right to think about interpretation of quantum mechanics or complex actions or whatever whimsical idea that he wants to think about these days.

Let him be.

18. **J**
April 4, 2012

Had some personal experience with him and agreed with some commenters that number 1 the journalist report could not be a good mirror image of him and number 2 Holger is a “far-from-equilibrium” person as compared with routine physicists you meet and talk with everyday, so whatever he said could not be taken as serious evidence for/against anything anyway…

19. **jg**
April 4, 2012

Holger Nielsen doesn’t need the lhc since he has pretty much worked out the structure of a theory of everything using pure thought with his random dynamics philosophy:

http://www.nbi.dk/~kleppe/random/trapp/u.html

(looks like a dodgy staircase)

Is it just me or does ‘fathers of string theory’ sound a bit feeble compared to ‘fathers of quantum mechanics’?

20. **Sadley**
April 4, 2012

Everyone very keen to discredit Nielsen, just because reportedly having said something that we do not like to hear.

s

21. **namenotfound**
April 5, 2012

You do a disservice to the field and to serious science in general by giving more publicity to this.

April Fools at the APS Meeting in Atlanta

April 4, 2012
Categories: This Week's Hype

From the latest Science News:

> **String theory weighs in on Higgs**
>
> **ATLANTA** – Physicists working on big experiments at particle colliders aren't the only ones who have something to say about the mass of the elusive Higgs boson. A theorist has now thrown his hat into the ring. Theoretical physicist Gordon Kane of the University of Michigan in Ann Arbor reported April 1 that he and colleagues have calculated the mass of the Higgs from the principles of string theory, with no additional inputs. In the standard model of particle physics, the Higgs boson is required for other particles to have mass. Kane's team, which also reported the calculation online last December at arXiv.org, put the mass at between 105 billion and 129 billion electron volts. The proposed mass is consistent with hints of a Higgs at around 125 billion electron volts, reported later that same month by both the Atlas and CMS teams at the Large Hadron Collider near Geneva. "This is the first string theory prediction for the mass of the Higgs — ever," Kane said.

For some background on this, see here.

Update: It seems that this joke is far more elaborate than I had realized. The APS this year awarded Kane the Julius Edgar Lilienfeld Prize, and then scheduled him to deliver the Prize speech on April Fools day. His speech abstract is:

> **The Higgs Boson, String Theory, and the Real World**
>
> In this talk I'll describe how string theory is exciting because it can address most, perhaps all, of the questions we hope to understand about our world: why quarks and leptons make up our world, what forces form our world, cosmology, parity violation, and much more. I'll explain why string theory is testable in basically the same ways as the rest of physics, and why much of what is written about that is misleading. String theory is already or soon being tested in several ways, including correctly predicting the recently observed Higgs boson properties and mass, and predictions for dark matter, LHC physics, cosmological history, and more, from work in the increasingly active subfield "string phenomenology."

His presentation advertises in large red letters:

> **First String/M-theory tested prediction for new physics — predicted 125 Gev (August)**

and claims that you shouldn't believe arguments that string theory is untestable, even when they come from string theorists:

> If your impression of string theory came from some popular books and articles and blogs (or from formal string theorists) you might be suspicious of taking string theory explanations seriously.

He has many slides explaining the supposed "125 GeV Higgs Mass Prediction", but I can't see an argument that gives 125 GeV, and it's a prediction that suspiciously comes without error bars. The closest thing to a bottom line seems to be page 30, where the "Blue dots are favored prediction", and these blue dots span a Higgs mass range of about 121-128 GeV, so maybe he means 125 +/- 4 or something like that. There are also a lot of red dots from 105 GeV to 121 GeV, which the theory "disfavors", "but doesn't yet rule out".

The other LHC predictions he makes are that the squarks are up around 30 TeV, so unobservable at the LHC, and that the gluino is light enough to be seen at the LHC. His "generic LHC predictions" plot has a gluino around 600 GeV, at a value that has already been ruled out by LHC results. Back in December, he was predicting "a few months" until he was vindicated by observation of a light gluino. If 4 is a "few", his time is up.

Comments

1. **Bernhard**
   April 4, 2012

   "with no additional inputs"

   Either then already knowing the results, of course.

   "reported later that same month"

   What a shameful campaign, what a big liar Kane is. The guy makes a postdiction and sell it as a prediction. Shameful, shameful.

2. **Peter Woit**
   April 4, 2012

   Bernhard,

   When, after decades of trying to find a string theory prediction for the Higgs mass, Kane came up with one just days before the number was publicly announced (although also days after it was described here…), I thought that was pretty strange and hard to understand. However, it is now clear what was going on: he was just setting up an April Fool's joke! It's definitely about the most successful one I've seen, had lots of people fooled.

3. **harryb**
   April 4, 2012

   It does show however the fine line that ST continues to run, and how it erodes away at science – its very obscurity makes it ever harder to discern ST from its own ironic counterpart

4. **Aspirin**
   April 4, 2012

   Kane is of course the King of String Theory Hype so this is hardly surprising. At this point he is just sullying the reputation of arXiv. I am surprised people like Witten haven't called him out as an embarrassment to the community.

5. **Mike**
April 4, 2012

Sadly, the April fool's joke is on the world, for being beguiled for many years by false predictions from string theorists. I think we laid some of this to rest last year with no evidence of SUSY and no gluinos. How long will people continue to ignore experimental evidence? If the excess at 125 GeV starts going down, all of the postdiction-prophets showing their theory's prediction at this energy should be looking for new jobs.

6. **P**
April 5, 2012

Groan groan groan groan groan. Some of the comments on this blog make people's biases extremely clear. Some of you are so anti-string-hype (I'm anti-string-hype as well, though not anti-string) that you haven't done your homework or even considered the fact that Kane et al did a field theory computation that had assumptions and consequences.

First, let me be clear: I'm not criticizing Peter on this issue, as he seems to have done some of his homework in the previous post on Kane's scenario, even though this post of his a bit of a rant. Second, I'm not a fan of hyping Kane's scenario as a unique prediction of string theory. It's not.

Criticisms:

@ Bernhard: Do your homework! You imply it's a "postdiction" because you think Kane only made these claims after the Higgs rumors came out. That's manifestly not the truth. Peter links to Kane's slides where he is clear that he was talking about this Higgs result at a conference in August. You can go to those slides from Madison in August and see that his 122-129 GeV range was discussed months before the Higgs rumors came out.

@Peter: Okay, one criticism of Peter. In these comments you seem to imply that Kane's claims were only made two weeks before, when (I think) you know he made these claims months before the Higgs rumors came out.

@ Aspirin: I don't disagree with you. He's overhyping and perhaps a big shot like Witten should make a clear statement about these things.

@Mike: Same comment as with Bernhard. Even though it's not a unique prediction of string theory, as he would like to hype, it's a bold-faced lie to say it came after the Higgs rumors came out.

Can we not talk honestly about science anymore? Are we really reduced to just attacking some person because he overhypes something without us really understanding the assumptions he made?

Make no mistake about it – Kane's scenario is not a "unique prediction" of string theory – but he and his collaborators actually make field theoretic assumptions and did a computation, and the consequences of the assumed field theoretic structure (that he claims is generic in string theory) is a 122-129 GeV SM-like Higgs if tan beta >= 7. Regardless of hype, this is a computation! In field theory! With a Lagrangian! They run coupling constants and impose cosmological constraints! And no one on here has even mentioned that yet. Particle physicists have taken SUGRA very seriously for thirty years and this is precisely the framework for Kane's paper. It just so happens that string theory gives rise to SUGRA, but SUGRA can be studied independently as well.

All of this is to say: regardless of claims about string theory, if a field theory calculation they did with some assumptions about moduli masses due to honest cosmological bounds (amongst other things) gave m_H in a given range, this is an interesting result.

Now I ask you all a question: Peter raised in the previous blog post that he doesn't know why Kane emphasizes tan beta > 7. I don't either, though Kane in his slides claims its phenomenologically preferred over low tan beta. Does anyone else know why this is?

Sorry for the rant. I just really hope we can raise the bar above blind criticism without understanding what he and collaborators did. That's not how we should discuss science.

Hype? Criticize away . . .

7. **Mark**
April 5, 2012

@P, if \tan\beta ~O(1) then the Higgsinos (the mu-term) are too heavy and the "little hierarchy" problem gets worse. So, having an intermediate range \tan\beta alleviates the little hierarchy problem in the split SUSY scenario of Kane et.al.

8. **Clara**
April 5, 2012

Peter,

who is on the Prize committee? Is it possible to find out?

9. **Bernhard**
April 5, 2012

P,

"@ Bernhard: ….
Peter links to Kane's slides where he is clear that he was talking about this Higgs result at a conference in August. You can go to those slides from Madison in August and see that his 122-129 GeV range was discussed months before the Higgs rumors came out."

I did. Here´s what is written "There's nothing in Kane's August talk about a 122-129 GeV range for the Higgs mass, but in the December 5 paper it appears explicitly three times"

In any case don´t worry about the Higgs, since the gluinos he predicted were not found , I now believe Kane 100%, I think "string theory is testable in basically the same ways as the rest of physics" and that because of it, is ruled out.

10. **Igor Khavkine**
April 5, 2012

Looking through the prize citations of past Lilienfeld Prize winners, it seems that what they mostly have in common is being active popularizers or science.

11. **Bernhard**
April 5, 2012

P,

sorry. What you mentioned was the slides themselves. I of course did look at the time, and it is exactly what Peter said.

One more thing, who took or takes SUGRA seriously? In my opinion, this is a framework experimentalists love for the low number of parameters, not that people were really taking it seriously.

12. **Ralph**
April 5, 2012

I predict that the Higg's mass is within 20% of the hints seen from LHC last year.
Please post the Nobel medal to me at my home address.
Ta.
R.
PS. I know what next weeks winning lottery numbers are. I'll prove that to you in May. wink. wink.

13. **P**
April 5, 2012

@ Mark: thanks! Simple enough answer . . . sounds good.

@Bernhard: I understand your objection now. For some reason, you're upset that he put error bars in his December paper, saying 122-129 GeV rather than "about 127 GeV". True, "about" is not a precise error bar, but are you really going to take issue with the fact that he said 127 GeV and "about", while the supposed observed value is 125.5GeV? I don't take issue with this. These were conference slides, and he was loose about it.

Regarding your remarks about gluinos: can show me the paper which makes the same assumptions as Kane's field theory model and rules out gluino? I just took at look at all ATLAS papers with the word gluino in the title and found nothing of that sort. Perhaps there is something from CMS or LHCb that definitively rules out light gluinos (below a TeV?), period. If so, show me. If not, we should still be very conservative about what we say about the LHC and SUSY. It's still in the early days of the experiment and they make very strong assumptions about their analyses that only apply to a fraction of SUSY parameter space. In my opinion, the joy of experiment is that it's not worth philosophizing too much now based on early analyses, but in a number of years we'll definitely know the answers.

Regarding SUGRA – I'm no supergravity expert, but I think you're mixing up SUGRA and mSUGRA. The latter has a small number of parameters and does some very interesting things, including the dynamic generation of an ESWB Higgs potential. SUGRA itself . . . I believe really anyone who takes SUSY seriously takes SUGRA seriously. And there are many, many non string-theorists who take SUSY seriously.

14. **P**
April 5, 2012

Also, for those that haven't looked back at the previous post on this subject, Mark's comments are particularly good and are worth a read. He seems to have really tried to understand what Kane et al actually did, as opposed to just getting angry at the hype.

http://www.math.columbia.edu/~woit/wordpress/?p=4262

15. **Peter Woit**
April 5, 2012

Aspirin,

Of course I think it would be a good idea if the elder statesmen of HEP thought it worth their while to speak up in cases like this. Pretty uniformly though, people seem to think that it's best to just try and ignore this behavior and hope it will go away on its own. Another example of someone who should weigh in on this is Matt Strassler, who claims to be serious about countering mis-information about HEP phenomenology. He's made a big point of going after people for taking too seriously the 3-sigma Higgs evidence, but my attempts to get him to do anything about Kane led nowhere. It is true here that Kane has had little success in getting science journalists to take him seriously and report his claims. I suspect that any journalist who thinks of writing a "string theorist predicts Higgs mass" story quickly finds after calling up a couple people that this is obviously a "phenomenologist makes unsupportable claim" story, about as worth covering as "dog bites man". From Kane's comments about "formal string theorists", I gather that he has heard from people like Witten and Gross what they think about this.

One place where authorities do weigh in is when Kane tries to publish this in a reputable journal. It seems likely that he would have immediately sent this to PRL at the beginning of December. As far as I can tell, it is still unpublished. When and if it is published somewhere, it may be interesting to compare the published version of the paper to the arXiv version, to see what changes the referees insist on. There's a history of string theorists (see the Distler et al story back in 2007) having referees refuse to let them publish their "string theory predictions", but then going ahead and issuing press releases about them anyway.

16. **Peter Woit**
April 5, 2012

Clara,

The prize committee is on the web-page I linked to. To be fair to them, they decided on Kane and announced the prize before he pulled this stunt in December. Maybe the later decision to schedule his talk for April 1 wasn't a coincidence…

17. **Bernhard**
April 5, 2012

P,

about SUGRA and mSUGRA, point taken, for some reason I assumed you were talking about mSUGRA since ATLAS and CMS are continuously using it. My mistake.

About gluinos, you should listen to what Kane himself said about it. According to his own predictions we should have seen some light gluinos already. If I believe parameters can be easily played with to adapt, sure, but that is the whole point with the non predictability of the theory.

About the hype. Well, this is my SINGLE problem. Papers like Kane´s that play with parameters and make Higgs predictions are many. In the old thread you can see many posted much older papers that make the same- I take it back, much better prediction about the Higgs mass but before a larger fraction of the parameter space being ruled out. Kane made a much stronger case about the exact mass value after the rumors and in any case even if we ignore this, and I´m willing to, there is absolutely no point in making the circus he is doing around this, otherwise every single phenomenologist that got the mass Higgs right should immediately get the Nobel, specially considering their predictions beat Kane´s in years.

All in all, I still think string theory says nothing, zip, nada, about the real world.

18. **Peter Woit**
April 5, 2012

P,

As I think I've made clear elsewhere, all I can see that Kane really has is a complicated calculation in a specific model implying the Higgs mass should be below about 130 GeV, something which was experimentally indicated already when he started making these claims last August. The "122-129" business just appears in the early December paper, after the 125 number was known. The public claims he has been making about having a prediction of "125" before the results were known are just outrageous.

Also in the August slides, there's some sort of "prediction" of a 600 GeV gluino, which I believe has been ruled out by now. Kane conveniently doesn't mention that.

He has been promoting "string theory predictions" of masses of new particles for 20 or more years (see his famous "supertestable" article in Physics Today back in 1997). These "predictions" keep changing, moving up as experimental results come in showing them to be wrong. I guess one easy prediction for anyone to make a few years ago would be that even if the LHC found no superpartners, for the one SM thing it would find (the Higgs), Kane would be there with string theory "predictions" covering the allowable range as it shrunk to the right value.

19. **P**
April 5, 2012

@ Bernhard:

Thanks for the thoughtful response. I think we see eye to eye on SUGRA vs mSUGRA now. Not to say that mSUGRA isn't interesting in it's own right.

Regarding gluinos: which Kane comments are you referring to? I'll take a look at whatever source you send me. Also, in the most conservative experimental sense, everything we can say about not having seen gluinos is in those papers, and the analyses seem to be in very limited frameworks, most of which don't apply to Kane's comments. (I haven't seen one, please show me if there is one!). Though rumors are exciting, I try to limit my thoughts about the existence or non-existence of hypothetical particles to what the experiments have actually said.

Regarding pheno papers: do you have examples from many years ago of serious phenomenologists who strongly pushed for 120-130 GeV SM-like Higgs in a SUSY framework? I'd be curious to see this. SUSY theorists like to talk about light Higgs, and I had really first heard about the stated m_H scenario in press releases (ugh) from Kane et al.

Regardless, hype – yes, it's an enormous disservice to both the string and the non-string community.

Regarding strings – depending on how nuanced you make your statement, our opinions probably differ. Saying that strings will definitely tell you something about scattering amplitudes at low energies (see: our experiments :)) that a field theory model COULDN'T is . . . well, a hell of a claim. There is good work, though, studying stringy scattering amplitudes at the string scale, and they consider M_s ~ O(TeV). It likely is much higher.

The reason for hope, I believe, is that compactified string theory DOES generically give rise to all of the ingredients we see in our world: non-abelian gauge theories, GR as a limit of quantum gravity, and scalar fields which could be of cosmological importance. True, your coming criticism that it hasn't yet singled out OUR gauge theory or OUR cosmology is a good one (and it may never), but of course field theory doesn't do this either . . . and moreover nothing in field theory requires the existence of gauge theories , GR, or scalar fields – they're input. In string theory, they're output. This compelling, and I think that if there were any other framework for physics that naturally gave rise to these three things as the output of fundamental principles, string theorists would take it seriously.

20. **Thomas Larsson**
April 5, 2012

Actually, it is possible to use string theory to make falsifiable predictions about the LHC. I made one back in 2007.

I'm especially proud that I already in 2007 could predict that poor Lubos would lose his experimental-susy-by-2006 bet, considering the he evidently hasn't realized it himself yet.

21. **Anon**
April 5, 2012

I know it sounds like a cliche, but "history will determine the truth".

You can't enforce truth by effective policing.

22. **Shantanu**
April 5, 2012

Peter, have you look at Susskind's talks at the KITP conference on bits/branes/black holes?

23. **Peter Woit**
April 5, 2012

Shantanu,

Not my kind of thing, but others might enjoy. Sounds like he's getting into the "philosophy of cosmology" business. If he'd stop saying mean things about religion, he'd be a natural for the Templeton Prize.

24. **P**
April 5, 2012

@ Peter:

Sorry, I didn't see your post from earlier in response to me. Clearly you're not a fan of Kane, and I'm also not a fan of the hype.

So I'm still not sure what you're really taking issue with regarding the slides from August. He clearly says 127GeV in those slides, and if you want to nitpick about 125.5 GeV and 127 GeV, that's fine, and you can take issue with that. In my mind, the thing that's interesting about the current location of the bump isn't that it's 125.5 and not 127 or 125 – it's that it's significantly about the 115-118 GeV often preferred by SUSY theorists (who might have told you SUSY like it below 100 pre-LEP).

25. **Peter Woit**
April 6, 2012

P.,

The "127" is not explained in the slides, and it doesn't appear in the paper. If "127" is the first actual prediction from string theory, it would be nice if Kane would explain the 127 in his paper. It would also be nice if he wouldn't call "127" "125". Again, the bottom line in his paper appears to me to be a graph of points covering the Higgs mass range from 105 to 128 GeV. If the LHC had found a signal at 118 GeV, I have no doubt Kane would have produced a paper about how that number was a "string theory prediction".

26. **P**
April 6, 2012

Peter,

Indeed, in his short talk he spends time explaining his setup and assumptions, discussing the masses of various particles that go into his RGE analysis. Then, at the end, he states the conclusions of the scenarios: heavy scalars, light gluinos, and a single light higgs boson with mass around 127 GeV. Now, if you want to be cynical and say the last number was complete lie and not the result of any analysis, that's fine. To me it seems more plausible that it is the result of an actual analysis, details not included in the paper because of time spent motivating the setup.

Do you think the "about 127" is actually a lie, not the result of an analysis that he didn't present in the slides?

Also, regarding your remarks about the gluinos of his scenario being ruled out: show me the paper that makes the same assumptions as his model. Some ATLAS papers clearly do not make the same assumptions. They may very well have ruled out his model, but I haven't seen it.

Cheers,
P

27. **P**
April 6, 2012

Also, sorry to be a stickler about this, but again, the surprising thing about "about 127" or the "125.5 +- error bars" is that it's a heavier Higgs than expected by most SUSY theorists.

For historical comparison: the ever useful Wikipedia (someone should check PDG) tells me that the error bars on the top quark mass are +- 1.5 GeV. Even if you want to ignore the "about" in "about 127", a 1.5 GeV discrepancy is not uncommon. In fact, though CMS was originally worried about being 2 GeV or so lower than ATLAS for m_H, this caused some worry but not enough to make strong statements. So I don't understand why you're harping on this.

Regarding claims that it is generic in string theory, I'm not sure, and in fact I'm skeptical and definitely think he shouldn't be hyping it. But I don't question the results of a field theory analysis.

28. **Peter Woit**
April 6, 2012

P,

I simply don't know what the argument for the 127 is. The situation here is that you are claiming that Kane had a good argument solving the biggest problem in particle theory, fixing the Higgs mass at 127 back in August using string theory, but didn't bother to give details of it in his talk or publish it. Then, several days after the 125 news came out, he puts out a paper with no 127 in it, but with an argument for 105-129, with above 122 "preferred". He then uses this to launch a publicity campaign claiming that he predicted from string theory the "125" long before it was announced. People can draw conclusions from those facts.

I also have no idea what his "string theory predictions" for gluinos are. He has a plot showing what appears to be about 600 GeV for the mass, was publicly claiming back in December that confirmation of his predictions was imminent, now acknowledges in his slides that "current limit for gluinos from string/M theory about 700 GeV" (page 33).

Yes, it's a heavier Higgs than you get from other popular string theory models. If the LHC had found a lower value, Kane would be giving talks about how string theory predicted the Higgs mass using those models.

I'm not arguing about what a reasonable size of error bars is here, either experimentally or theoretically, just pointing out that he never gives any error bars on his "string theory predictions".

29. **anon.**
April 6, 2012

CMS PAS SUS-11-020 excludes gluinos decaying through top quarks up to somewhat more than 800 GeV. ATLAS CONF 2012-004 sets ~ 700 GeV limits on gluinos decaying to stop/chargino, which is a similar topology to what you could get in the model under discussion. Standard jets + missing ET searches also set limits. Gluinos decaying to b+bbar+neutralino are excluded to ~900 GeV by 1203.6193 from ATLAS. Depending on branching ratios for gluino -> t+tbar+neutralino versus gluino -> t+bbar+chargino, you might not find an official CMS or ATLAS result excluding this scenario, but the gluino mass bound is probably ~ 800 GeV or higher, independent of all these details.

It would be a real stretch to say there's a sharp prediction of the gluino mass in these models, so you can't really say this rules them out. It's above what Kane was predicting a year or so ago, though.

Aside from the various overly strong claims, there are some basic facts that are worth bearing in mind:
* A Higgs mass of 125 GeV in the MSSM requires large mass parameters in the stop sector. This could be a very large A-term, with somewhat light stops. But it seems increasingly plausible that if the MSSM is right, some variation on split SUSY is the right model to be thinking about, with scalars in the range from ~ 10 TeV to 1000 TeV. The upper end of that range only fits a 125 GeV Higgs if tan beta is quite close to 1.
* The one input string theory really provides is that it suggests the existence of moduli, i.e. of scalar fields coupling with gravitational strength. With that ingredient,

standard effective field theory tells you that they probably have a mass near the gravitino mass and that this is a cosmological disaster unless the gravitino mass is ~ 30 TeV or higher.
* As observed by Randall & Sundrum in 1998, anomaly mediated SUSY breaking can make a 30 TeV gravitino consistent with ~ 1 TeV gauginos, and moduli decays can then produce dark matter (as explained by Moroi & Randall).
* And, as Giudice, Luty, Murayama, & Rattazzi noted in their simultaneous work on anomaly mediation, such a scenario can plausibly lead to scalars a loop factor heavier than gauginos. This kind of scenario was also studied by James Wells, and then Arkani-Hamed & Dimopoulos.

So, if we relax our idea that electroweak symmetry breaking should be completely natural, we have a supersymmetric scenario consistent with the measured Higgs mass and the observed fact that gauge couplings in the MSSM unify. The scalars are quite heavy but the gauginos may be in reach of the LHC. The moduli problem is solved, and flavor isn't quite as bad as in standard scenarios.

This basic idea was already around in 1998/99. Kane has essentially advocated a specific version of it, tied to a particular model of moduli stabilization. It's not quite identical to the preexisting split SUSY idea, since he has contributions to gaugino masses not coming from anomaly mediation, but of the same order. But it's basically a(n idiosyncratic, overhyped) variation on an idea that has been recognized as important for a decade.

30. **Peter Woit**
April 6, 2012

anon,

Thanks for the very helpful explanations.

31. **alsoanon**
April 6, 2012

An addition to anon. above. Even the large threshold corrections to the anomaly mediated gaugino masses is not a novel feature of the Kane model. Arkani-Hamed, Giudice and Delgado introduced the "simplest split susy" model in Jan 06, with one-loop splitting between scalars and gauginos, but also higgsinos around the same scale as the scalars, which completely changes the anomaly-mediated gaugino spectrum. Another point: as can be seen from the plots in Giudice and Strumia 's paper, http://arxiv.org/pdf/1108.6077.pdf, figure 3, stops at a few hundred TeV (as expected from the one-loop splitting) give a higgs near 125 GeV for a reasonable tan beta around 2-3. Taking tan beta close to 1 allows scalars nearer to 10,000 TeV.

32. **Mark**
April 6, 2012

I have studied the papers of Acharya et al and have heard several talks on the topic and I must say that to me the Higgs mass is the least interesting part of the whole story. To me, the most appealing feature of their construction is its simplicity and that it solves many problems simultaneously and in a natural way, the biggest of the problems addressed being the Hierarchy problem. Kane mentions some of those things in his slides so I'm not going to repeat them. However, I wanted to correct the previous poster that in these G2 vacua the gravitino mass scale is constrained to O(10-100) TeV *not* by the relation between the moduli masses and cosmology but instead by the requirement that the leading tree-level contribution to the vacuum energy is tuned to be of the same order of magnitude as the leading one-loop contributions so they could mutually cancel, combined with the constraint on the value of the seven-dimensional volume, which comes from the relation between \alpha_GUT, M_Planck and M_11. In these vacua, when one combines those two together one gets the above constraint on the gravitino mass. I think that it is quite non-trivial that the constraint on the cosmological constant plays a key role in constraining the gravitino mass and it should not be overlooked. Regarding the gaugino masses, just to be more precise, the anomaly mediated terms, which are typically discussed in the literature do not include the Konishi anomaly contribution since people always assumed some sort of sequestering. In the G2-MSSM there is no sequestering and the Konishi anomaly contribution to the gaugino masses is quite large and is directly proportional to the (large) tree-level reduced trilinear terms. So apart from the heavy scalars and trilinears, there is some non-trivial interplay between the trilinear couplings and the gaugino mass spectrum.

33. **anonymous**
April 6, 2012

Good science cares about explanation. Whether an explanation is a prediction or a "postdiction" is just a historical accident. There are plenty of postdictions that are good science, BCS theory of superconductivity being the first example that comes to my mind. The problem with Kane's paper isn't the timing (although that's maybe reason to be suspicious) as much as it is what Peter notes, that the paper pulls 127 GeV out of thin air, not really explaining anything.

By the way, Peter, I heard Erik Verlinde give a well-attended talk at Caltech yesterday. I know you've posted here about his entropic gravity before, so perhaps you would like an update. He said gravity as entropic force would come from string theory, but I took that to mean any underlying microscopic degrees of freedom from unknown physics. The more interesting new contribution was for dark matter. We know how Unruh temperature and Hawking temperature are proportional to accelerations and related to Rindler and black hole horizons, respectively. Verlinde applied the same logic to the cosmological horizon to get a temperature/acceleration. He didn't show all the details, but apparently the acceleration matched the a_0 of MOND. Someone asked how to cope with the Bullet Cluster, and he managed to brush it aside as not in thermal equilibrium. He also at the very end wrote on the board with no derivation a relation between baryonic matter and the inferred dark matter:
Omega_b=(pi/4)*Omega_DM^2
Someone else, Sean Carroll I think, asked how this relation could be time-independent. This question seemed to take Verlinde by surprise. The abstract for the talk advertised dark energy as well, but this was not really covered in the talk.

34. **Peter Woit**
April 6, 2012

anonymous,

Thanks for the update on Verlinde. I'm curious whether much of the audience found him convincing.

In a case where one has a completely clear argument, with assumptions stated explicitly and justification given for them, then of course even a postdiction can be completely convincing. Probably the main reason for being skeptical of string phenomenology models is not that they don't make solid predictions, but that they don't even make any convincing postdictions. If there was even one of the numbers characterizing the SM that had a convincing string theory explanation, that would carry a lot of weight.

Unfortunately, human minds being what they are, it's all too easy to come up with a bogus "explanation" of a number once you know the number. Thus the weight assigned to predictions over postdictions.

35. **Chris Austin**
April 6, 2012

Does anyone know how Kane et al propose to fit the small value of the cosmological constant, given that there are no fluxes in their G2-MSSM scenario?

36. **Peter Woit**
April 6, 2012

Chris,

Looking at page 39 of his APS presentation, the answer seems to be that he proposes to just set the CC to zero and ignore the problem.

37. **S**
April 6, 2012

Chris,
This exact question was brought up at the KITP String Phenomenology meeting back in 2010. A plausible mechanism, where the CC scan is due contributions from gauge degrees of freedom in multiple hidden sectors, was briefly discussed in this talk (watch from 41:50 to 43:30):
http://online.kitp.ucsb.edu/online/spheno_m10/bobkov/rm/flashtv.html

38. **Anonyrat**
April 7, 2012

Is the instability/"fine-tuning" problem in QFT an artifact of perturbation theory, or is it a genuine physical problem?

39. **Peter Woit**
April 7, 2012

Anonyrat,

The explanation of the "fine-tuning" problem in terms of quadratic perturbation theory divergences does look like it might be an artifact of perturbation theory. Another version of the problem though doesn't depend on perturbation theory: the ratio of the Higgs masss (or electroweak breaking scale) to either the GUT or Planck scale is a very small number, with no symmetry to explain it being almost zero. That's one motivation for SUSY, where SUSY is supposed to relate the Higgs to fermions with masses small because of chiral symmetry (this argument is potentially ruined by the SUSY breaking scale being too high).

My problem with this argument has always been that it's not clear why one should take it seriously: we don't know there even is a GUT, or a GUT scale for GUT symmetry breaking, and we have no idea what the relevance of the Planck scale or quantum gravity really is to the Higgs until we have a unified theory. Much of the argument for why the Higgs mass is "unnatural" seems to depend upon unification schemes for which there is no evidence.

40. **Shantanu**
April 7, 2012

Peter, interesting that string theory also predicts MOND. (I thought it predicted
supersymmetry and dark matter)

41. **Chris Austin**
April 7, 2012

Peter and S, thanks.

42. **Bernhard**
April 11, 2012

A bit old news, so perhaps already pointed out already. Sounds like a boring code problem somewhere, but who knows.

http://www.guardian.co.uk/science/life-and-physics/2012/apr/01/1

43. **Marcus**
April 13, 2012

I noticed this on the arxiv today:
http://arxiv.org/abs/1204.2795
Compactified String Theories — Generic Predictions for Particle Physics
Bobby Samir Acharya, Gordon Kane, Piyush Kumar
(Submitted on 12 Apr 2012)
In recent years it has been realized that in string/M theories compactified to four dimensions which satisfy cosmological constraints, it is possible to make some generic predictions for particle physics and dark matter: a non-thermal cosmological history before primordial nucleosynthesis, a scale of supersymmetry breaking which is "high" as in gravity mediation, scalar superpartners too heavy to be produced at the LHC (although gluino production is expected in many cases), and a significant fraction of dark matter in the form of axions. When the matter and gauge spectrum below the compactification scale is that of the MSSM, a robust prediction of about 125 GeV for the Higgs boson mass, predictions for various aspects of dark matter physics, as well as predictions for future precision measurements, can be made. As a prototypical example, M theory compactified on a manifold of G_2 holonomy leads to a good candidate for our "string vacuum", with the TeV scale emerging from the Planck scale, a de Sitter vacuum, robust electroweak symmetry breaking, and solutions of the weak and strong CP problems. In this article we review how these and other results were derived, from the key theoretical ideas to the final phenomenological predictions.
Comments: 30 pages, 1 figure. Invited Review for International Journal of Modern Physics A

44. **Luke**
April 13, 2012

Thanks for the reference, Marcus! This is a very nice review article and contains a good summary of what's been known for some time about generic string vacua that result in N=1 D=4 supergravity. The main lesson being that the moduli masses and the gravitino mass are extremely hard to decouple, hence the non-thermal pre BBN cosmological history of the universe with moduli-domination at late times should take over after the radiation domination era, contrary to the standard cosmological picture, hence the axion overabundance problem for f_pq~M_GUT is naturally solved by the entropy dilution due to the moduli decays. Also, the absence of sequestering in string theory coupled with the limits on the gravitino mass from cosmology implies heavy scalars together with the Higgs mass somewhere above 120GeV, if one assumes the MSSM spectrum.

45. **Peter Woit**
April 13, 2012

Marcus,

I took a fairly close look at that last night. The Higgs mass “prediction” is based on the same plot as before, with, after various assumptions, masses between 105 and 129 GeV, with those above 121 GeV “favored”. According to Kane, string theory now predicts only one non-SM effect observable at the LHC, gluinos, and he’s cagey about them. In 1997 he was predicting 250 GeV gluinos based on string theory, last year they were supposed to be at 600 GeV, by December their observation was a “few months” away. Now they seem to be at “less than or about 1 GeV” and “should be observable with 2012 data”. Once the 2012 data is in, I’m sure an explanation will be found for why string theory “predicts” that their mass is above the LHC bounds.

46. **guest**
April 15, 2012

I really like how they simultaneously solved both strong CP and supersymmetric (weak) CP problems in one stoke. I was unaware of this connection before and I think it’s pretty neat that they found it.

47. **Vince**
April 17, 2012

Peter,

Jester has posted about a possible dark matter signal from FERMI. Lubos has posted about it as well, and claims that Gordon Kane can explain it with stringy physics. I wonder: If this does turn out to be a genuine dark matter signal, does this new particle show up in Kane’s earlier paper about how M-theory implies the right range for the Higgs mass (according to the LHC’s results)? If not, does this new particle imply that M-theory is wrong?

48. **Peter Woit**
April 17, 2012

Vince,

I recommend Jester’s (Resonaances blog) article for anyone interested in this. I’m not convinced by the signal, would be more convincing if this were coming from the experiment itself, will be interesting to see what they have to say.

About Gordon Kane’s claims to explain this with string theory, he can explain anything with string theory. You can’t falsify a theory that predicts anything you want.

Theory Bubbles

April 11, 2012
Categories: Uncategorized

In this week's *Nature*, Abraham Loeb, the chair of the Harvard astronomy department, has a column proposing the creation of a web-site that would act as a sort of "ratings agency", implementing some mathematical model that would measure the health of various subfields of physics. This would provide young scientists with more objective information about what subfields are doing well and worth getting involved with, as opposed to those which are lingering on despite a lack of progress. Guess what Loeb's main example is of the "lingering on" category?

> In physics, the value of a theory is measured by how well it agrees with experimental data. But how should the physics community gauge the value of an emerging theory that cannot yet be tested experimentally? With no reality check, a less than rigorous hypothesis such as string theory may linger on, even though physicists have been unable to work out its actual value in describing nature…
>
> **Theory bubbles**
>
> The study of the cosmic microwave background provides an example of how theory and data can generate opportunities for young scientists. As soon as NASA's Cosmic Background Explorer satellite reported conclusive evidence for the cosmic microwave background temperature fluctuations across the sky in 1992, the subsequent experimental work generated many opportunities for young theorists and observers who joined this field. By contrast, a hypothesis such as string theory, which attempts to unify quantum mechanics with Albert Einstein's general theory of relativity, has so far not been tested critically by experimental data, even over a time span equivalent to a physicist's career.

The problem of course is that of deciding who gets to make evaluations of what's a healthy field and what isn't. People with a lot invested in a dying or dead subject have strong incentives to misrepresent the situation (see, for example, the famous Monty Python Dead Parrot sketch). Loeb implicitly compares the current situation with string theory to that following the financial crisis, which was worsened by the ratings agencies assigning AAA ratings to debt not far from default.

> Senior scientists might seem the people best suited to rate the promise of research frontiers. But too many of these physicists are already invested in evaluating the promise of these speculative theories, implying that they could have a conflict of interest or be wishful thinkers. Having these senior scientists rate future promise would be akin to the 'AAA' rating that financial agencies gave to the very debt securities from which they benefited. This unseemly situation contributed to the last recession, and a long-lived bias of this type in the physics world could lead to similarly devastating consequences — such as an extended period of intellectual stagnation and a community of talented physicists investing time in research ventures unlikely to elucidate our understanding of nature — a theory 'bubble', to borrow from the financial world.

The problem of how to get scientists and academics to rigorously evaluate what works and what doesn't is a difficult one. In particle physics, success has led to making progress harder to come by, so just noticing a lack of progress at the rate of earlier times is not enough. String theorists are right to point out that developing ideas to the point that the theory can be compared usefully to experiment could be a difficult goal that may take a long time to get to. They're wrong though not to acknowledge the fact that they're not getting closer, but rather farther and farther away. And misrepresentations about the state of a subject can victimize young students and researchers, induced to devote crucial parts of their lives to something not worthwhile.

I'm rather skeptical about Loeb's faith in mathematical models to provide objective guidance. The AAA ratings assigned to dubious mortgate-backed securities were the product of mathematical models, defective ones. If you let me design the model, I can come up with one that will justify whatever conclusion I want. In the end, outcomes will depend on the quality of the judgment and decisions of those the community chooses as its leaders. Throughout academia, bad ideas live on, and good ones don't get the recognition they deserve. At the same time, in many fields those put in positions of responsibility live up to them and often do a remarkable job of countering the forces promoting stagnation as well as providing a positive vision that drives real progess.

As for Loeb's idea about a web-site where young scientists could go to get information about the health of a field, I remain skeptical about prospects for one that implements a mathematical model. However, a website devoted to honest and informed discussion about what is going on in a field and whether it is healthy, providing a place for students and others to listen to and participate in debate, helping them make up their own minds, seems to me an excellent idea…

Update: I just noticed that Loeb had a paper on the arXiv last year spelling out his proposal in more detail.

Comments

1. **CPV**
   April 11, 2012

   There is an interesting analogy here between analyzing stocks based on recent price movement (technical analysis), or based on earnings (fundamental analysis). In the long run fundamental analysis is what works, but in the short run in may not.

   It seems like funding and publications are like short term price movements while validation by experiment is more like earnings. I'm not sure about hiring and promotions!

2. **chris bolger**
   April 11, 2012

   Evaluating the health of a research field? I agree with you, how do you do that. It seems to me great advancements in history were made when some egg head told somebody else "This can't done." Indeed, the best advice might be to tell a young physicist to go into the field that such a website says is dying.

3. **piscator**
   April 11, 2012

   The Nature article is behind a paywall, but from your quotes and summary there seems to be not inconsiderable chutzpah for the chair of the Harvard astronomy department to say both that senior scientists are not the best judges of research frontiers and by the way string theory is overfunded.

4. **Peter Woit**
   April 11, 2012

   piscator,

   The "chutzpah" label has been previously applied to Loeb in this context:

http://www.bautforum.com/showthread.php/120202-Physics-Chutzpah

For another sort of argument from him, about encouraging people to also spend time on some riskier non-mainstream work, see

http://arxiv.org/abs/1008.1586

5. **abbyyorker**
April 11, 2012

I think it is ridiculous to think that young scientists need help in ranking fields of study. Typically, they're 20+ and have been thinking a long time on it. Grad students I have known, including future string theorists, know perfectly well the prospects. Many go into string theory anyway, for other reasons (one being that it interests them). Maybe a bad decision but I don't see a coverup of the truth.

6. **anton nymos**
April 12, 2012

"I advocate the need for a website operated by graduate students that will use various measures of publicly available data … to gauge the future dividends of various research frontiers. The analysis can … aim to alert the community of the risk from future theory bubbles. "

Peter, you are not a grad student but I think you have already implemented to some extent his proposal for string theory.

7. **Bee**
April 12, 2012

I've discussed the problem of bubbles of nothing in academia here and here for example. If Loeb had read what I wrote he might have understood that his proposal isn't going to solve the problem. The main problem is not that we need a better way to rate on the promise of a research field or project. The problem is that presently scientists have insufficient incentive to accurately rate it to begin with. I referred to this as "external pressures" that essentially prevent an accurate judgement. As a result, the system, as it is presently organized, doesn't optimize for "most promising research."

To see what I mean, consider the following example that I personally find the most troublesome, though there are other problems: It is hard if not impossible for a scientist to change fields after, say, the 2nd postdoc. At that time, you'll be desperately looking for a permanent position and trying to get in grants. Both will only work if you have a track-record in some field, and that field is in addition considered promising. Taken together, this means researchers will go around and advertise their research field because they're stuck on it. You can't sell your publication's keywords and buy new ones like you buy stocks. It's your history and the way the system presently operates it basically *forces* people to create bubbles of nothing because otherwise they'll end up unemployed!

You can find more details and other examples in my posts. More recently, in my post On the importance of being wrong. we discussed the problem that admitting on having misjudged the promise of a research project is not only not rewarded but actively punished in the community, and wishful thinking is one of the most common biases affecting science.

A "rating agency" isn't going to do anything about what's the actual origin of the problem. In fact, if people came to believe the ratings it would make the problem even worse, because it contributes to a rich-get-richer trend (if there are many people who work in a field there are many people who have all reasons to rate it up).

8. **Rhys**
April 12, 2012

I think Bee makes a very good point. New graduate students have the freedom to choose their field, but tend to be relatively naïve when they do so (I know I was; I couldn't possibly accurately evaluate the worth of, say, string theory, at the time). It's practically impossible to change fields after a year or two of graduate study, because one must write a thesis before running out of funding!

There may then be some freedom to wander during the first postdoc or two, but there are overheads in time and effort associated with branching out, and one is always concerned about writing enough papers, lest the next job not be offered.

9. **Steven Chan**
April 12, 2012

Speaking from a researcher from another field, I had my concerns who string theory develop. I think the comparison with an economic bubble is quite appropriate. One of the biggest problem faced by younger scientists are the need to publish and to be seen "doing"/"producing" something. The string theory "bubble" is self-reinforcing; the bigger the bubble gets, the urge to even make it bigger is even higher due to career pressure.

The biggest difference with an economic bubble is how the bubble could be burst. Real economic bubble burst as balance sheets and debt obligations implode, and people and institutions running out of money. I wonder what mechanism can burst an "academic bubble". I am not sure research grant cuts can actually burst an "academic bubble" – in the ends… it is experimental/field work that cost the most, not theoretical and computer model work. The folks inside the bubble field won't burst the bubble themselves, of course – for the interest of their own career.

What I think may burst the bubble is a major breakthrough in experimental physics that allow a highly competitive and creditable theoretical work to develop.

10. **Peter Woit**
April 12, 2012

Bee,

Thanks for the links to a lot of good discussions of this topic. I agree that young scientists are in their most vulnerable position, with a lot invested in a particular field, and leaving it very dangerous, at the time they don't yet have a permanent position, but need to get one soon. This is the time in people's career that they are least likely to be able to change research topic, so it's not surprising that few do.

What is more surprising is how few people are willing to give up on a failed research program and try something else AFTER they have a permanent position and (at least typically in the US), lifetime tenure. The whole point of the tenure system is to make it possible for people to make this kind of change, but few take advantage of this.

11. **Belizean**
April 12, 2012

> The whole point of the tenure system is to make it possible for people to make this kind of change, but few take advantage of this."

This shouldn't be too surprising. The tenure granting process actively selects those who are either not particularly curious about topics irrelevant to their immediate research or are sufficiently disciplined to suppress any such curiosity. A young condensed matter theorist who devotes a considerable fraction of her time to work on quantum gravity is unlikely to obtain tenure. If a newly tenured professor had in fact been suppressing a burning extracurricular curiosity for years, it is likely that this habit would have become permanently become ingrained.

12. **Jeff**
April 12, 2012

A better analogy IMO would be Technology Adoption and Meme Adoption. Both involve beliefs of what is true as well and they have many dynamics in common, specifically in term terms of human networks and in terms of nonlinear thresholds or tipping points of believe or "paradigm" acceptance.

Rather than "control" these, which generally isn't possible, it may be enough (or better) to simply characterize and comprehend where an idea or theory is in its adoption curve. Not all technologies are adopted and so too not a ideas are adopted. And also some technologies simply are "ready" adoption.

In technology there is the "20 year rule" which basically is the empirically observed time between discovery of an idea and its broad acceptance. When a technology isn't "ready" it typically takes a generational replacement to bring it up for consideration again so often technologies take 20, 40, 60, etc. adoption intervals.

I suspect theories in science follow a similar pattern for similar reasons.

13. **Shantanu**
April 12, 2012

Peter, Avi gave two talks one at KITP and another at CFA talking about these ideas in detail. Anyhow I know from personal experience that its very hard to work on independent ideas (without any sort of encouragement/feedback). I know someone who
has been working seven years tirelessly on a modification of GR, having refereed
publications and inspire of not getting paid a cent for his research, or getting
seminars/conference invitations

14. **Lucy-Lee de Cortez**
April 12, 2012

The irony is remarkable

Tenure is supposed to encourage independent thinking.
But anyone sould see what would happen.
That it would coerce people to think the way the tenure granters tell them to.

Was it always thus?
And was that the purpose all along?
And now, how do you get rid of a system that is so corrupting?

15. **N.**
April 12, 2012

Most researchers do not recognise the simple fact that to admit "I was wrong" means that other fellows might not follow the same path, thus lots of funds, energy, time, etc. could be spared.

I was even considering setting up a "Wrong Nobel" fundation – rewarding those who have the courage and honesty to say aloud "I was WRONG"!!

Unfortunately, the EGO is stronger than any award… (especially with matematicians – sorry Peter :)))

16. **arjun**
April 13, 2012

Bee said: "It is hard if not impossible for a scientist to change fields after, say, the 2nd postdoc. "

I think it is hard to change fields even after a PhD (and this includes subfields of physics), unless one is going into the industry (then one can do whatever one wants to). For one, there is a steep learning curve, and two, there are already many PhDs in the new field who will be in the queue for post-docs.
The only way I can think of someone changing fields (or a subfield) is by getting a second PhD and this time in an area chosen with better judgement—born-again academic.

17. **srp**
April 16, 2012

The obvious answer is that science must go back to recruiting the independently wealthy types like Boyle, Darwin, Noyes, etc. who have little need for external funding. Theorizing is cheap, so someone with a good inheritance could do good work without any need to deal with these institutional problems. Selection from such a narrow group would of course restrict the talent available, but a) you could keep the existing system alongside it and b) those wealthy pseudo-amateurs did a pretty remarkable job over a long period of time.

The implementation barrier is that being an independent scientific theorist is no longer a "cool" thing for the idle rich to aspire to.

Emerging Grant Opportunity

April 12, 2012
Categories: Uncategorized

I just noticed that the Templeton Foundation has a competition for $5 million in grants in the area of "strong emergence", submission deadline very soon (April 16). I'm not sure I understand their distinction between "weak emergence" and "strong emergence" (classical phase transitions are weakly emergent, quantum ones strongly emergent), but they seem to intend to support real physics research, and they're inspired by Philip Anderson's wonderful paper More is Different. This is a refreshing contrast to some of their other ventures I've written about here that tend towards encouraging pseudo-science, so I hope this one is a success and they do more things like it.

Their other current funding opportunity, also with a deadline of April 16, is Breaking New Ground in Science and Religion, which is more the usual kind of thing for them. They don't give a total number for what they intend to spend in this area, but it appears to be much less than the $5 million going to emergence.

Comments

1. **Don Murphy**
   April 13, 2012

   Peter

   Thank you for the reference: More is Different. Indeed one of the best papers ever written on the subject.

   Don

2. **Chris Austin**
   April 13, 2012

   Thanks for pointing out the Physics of Emergence competition. I've just submitted an entry; I thought the submission procedure was very well designed and organized.

Testing the Holographic Principle

April 12, 2012
Categories: Uncategorized

Adrian Cho at Science magazine this week has an article about Craig Hogan's project to build a "holometer" and somehow test the "holographic principle". Since this promises some sort of experimental test of fashionable ideas about quantum gravity, it has gotten a lot of attention, including a cover story in the February Scientific American (also available here and maybe elsewhere).

This kind of thing often gets promoted as a "test of string theory", but in this case, at least from certain quarters, that definitely isn't happening. Cho quotes Raphael Bousso:

> But some experts on the holographic principle think the experiment is completely off-target. "There is no relationship between the argument is making and the holographic principle," Bousso says. "None whatsoever. Zero." The problem lies not in Hogan's interpretation of the uncertainty relationship, but rather in "the first step of his analysis," Bousso contends.
>
> Bousso notes that a premise of special relativity called Lorentz invariance says the rules of physics should be the same for all observers, regardless of how they are moving relative to one another. The holographic principle maintains Lorentz invariance, Bousso says. But Hogan's uncertainty formula does not, he argues: An observer standing in the lab and another zipping past would not agree on how much an interferometer's beam splitter jitters. So Hogan's uncertainty relationship cannot follow from the holographic principle, Bousso argues.
>
> The experiment can do no good in testing the holographic principle, Bousso says, but running it could do plenty of harm. The holometer has garnered an inordinate amount of attention in the blogosphere and in press accounts, he says, raising unrealistic expectations. "They're not going to have a signal and then there is going to be a backlash saying that the holographic principle isn't valid, and we'll look like we're on the defensive," Bousso says. "That's why I'm trying to get the word out without appearing to make excuses."

There's also the following from Lenny Susskind:

> Not everyone cheers the effort, however. In fact, Leonard Susskind, a theorist at Stanford University in Palo Alto, California, and co-inventor of the holographic principle, says the experiment has nothing to do with his brainchild. "The idea that this tests anything of interest is silly," he says, before refusing to elaborate and abruptly hanging up the phone.

Comments

1. **Shantanu**
   April 12, 2012

   Peter, two swallows don't make a summer. am sure you will find some string theorists who will claim this as evidence for string theory.
   Over the years I have found that string theory agrees with even contradictory facts.
   o string theory is consistent with both equivalence principle and violations of equivalence principle
   o string theory predicts supersymmetric dark matter as well as MOND.
   o string theory agrees with superluminal neutrino as well as neutrinos moving at c.
   someone should add more such examples.

2. **Dan Winslow**
   April 12, 2012

   Well, hype aside; whether it tests the holographic principle or not it does seem to me (as a layman) to be an interesting test. Wouldn't having some quantization of space time affect things like renormalization and other infinity-plagued calculations? Forgive me in advance if I;m not making sense.

3. **Peter Woit**
   April 12, 2012

   Shantanu,

   If Hogan's experiment actually sees something, I have no doubt it will be claimed as "evidence for string theory".

   Dan,
   A quantization of space/time is a holy grail for many theorists, with the fact that it would provide a natural cut-off for QFT calculations just one of many reasons to be interested. The argument here is over whether Hogan's experiment actually is sensitive to such a quantization. It seems overwhelmingly likely that it won't see anything, so Bousso/Susskind are sensibly trying to distance themselves from having their favorite theoretical idea associated with it.

4. **Low Math, Meekly Interacting**
   April 12, 2012

   http://backreaction.blogspot.com/2012/02/hoganmeter.html

5. **Bee**
   April 13, 2012

   I explained the issue with Lorentz-invariance years ago here. One has to be grateful, I think, to Bousso and Susskind to speak at least a few clear words before hanging up the phone.

6. **Sesh**
   April 13, 2012

   Peter, your first link is missing a semicolon.

7. **Sesh**
   April 13, 2012

Sorry, I meant a colon.

8. **Rhys**
April 13, 2012

I think it's important to emphasise that the holographic principle is independent of string theory; AdS/CFT (of the 'hard' variety) is just the most concrete realisation that has been found. If there is one thing which is thought to be 'known' about quantum gravity, it is the relationship between horizons and entropy.

But as Bousso said, there doesn't appear to be any link between what Hogan is doing and the holographic principle. The "Scientific Bibliography" on the holometer website is extraordinary, by the way:
http://holometer.fnal.gov/scientific-bibliography.html

9. **Peter Woit**
April 13, 2012

Sesh,

Thanks, fixed.

10. **Visitor**
April 13, 2012

"In fact, Leonard Susskind, a theorist at Stanford University in Palo Alto, California, and co-inventor of the holographic principle, says the experiment has nothing to do with his brainchild. 'The idea that this tests anything of interest is silly,' he says, before refusing to elaborate and abruptly hanging up the phone. "

Considering how much of his time and effort is spent in creating and proselytizing for baseless speculations that masquerade as theory, and his appeals to their utility as a philosophical foundation of atheism as a reason to support those speculations, Susskind seems to me to be pretty nearly the last person to be making such a criticism, and this would have severely diminished my opinion of him, had I any positive opinion of him in the first place.

11. **Owen Patterson**
April 17, 2012

While I am sure that Dr. Susskind is a very busy and important man, perhaps if he had taken a few moments to explain things a bit to the interested pleibians huddled outside his door, perhaps we would all be better enlightened and then would not bother the great man with so many such puny questions and theories in the future.

Short Items

April 13, 2012
Categories: Experimental HEP News, Multiverse Mania

- The LHC is back in business, with the experiments collecting data at 4 TeV/beam, marginally higher than last year's 3.5 TeV/beam. They are ramping up the number of bunches in each beam, already this afternoon achieving a higher initial luminosity than the best of late last year. This should be a record luminosity for a collider. One place to follow the amount of data being accumulated is this CMS page.
- Also in Switzerland, another hard to comprehend publicly funded experiment is going on, see details here.

> The Swiss boson is a hypothetical condition which is supposed to account for why the Swiss franc has 'mass' when all other neighbouring currencies don't.
>
> A multi billion-euro experiment, operated by BERN (but funded outright by tax payers), is currently under way on the borders of Switzerland and the Eurozone to try and stamp out the asymmetries, ideally by creating something known as the 'anti-franc'.
>
> As part of the experiment, highly skilled practitioners smash billions of Swiss francs against the euro currency daily, with the explicit aim of blowing apart the franc.
>
> Experiments to date suggest the boson is probably hiding somewhere in the 1.20-1.22 field. Though some say there's a chance of finding it at the 1.25 mark.
>
> Yet as the experiment continues, fears grow that a black hole could unwittingly be created in the current account of the nation — a singularity known as the "ever depreciating euro asset" phenomenon.

- Jean-Pierre Serre has a web-page at the Collège de France where one can download copies of many of his recent manuscripts. There's also a wonderful interview with him here.
- Michio Kaku, the "co-founder of the superstring version of string theory", gave a talk about the future recently in Yakima, Washington. Clifford Johnson reports on a recent phone conversation he had:

> *Michio Kaku says that the universe is full of many things and all you have to do is ask for something and you'll get it. How do you go about doing that?*
>
> Well… I am not sure what he had in mind. It might be…. might be best to ask him…. But maybe what he meant is that the universe is a very big place, with lots of things going on, and maybe he meant that there are all sorts of things you could find out there because it is so big and diverse… But perhaps he did not have in mind that a particular person could go out and get any of those things… but you might want to ask him. I can't say for sure.

Perhaps Clifford should have clarified things for his caller by explaining that it's only string theorists for whom "the universe is full of many things and all you have to do is ask for something and you'll get it".

Comments

1. **Visitor**
   April 13, 2012

   The "Swiss boson" article seems to be satirical in nature, Peter. Possibly a belated April Fools' item.

2. **Peter Woit**
   April 13, 2012

   Visitor,

   It's not an April Fool's joke, but yes, it's a satirical comparison of what is going on with the Swiss franc and stories about the Higgs boson. I thought it interesting to see that discussion of the Higgs has gotten so much attention that financial journalists find it a worthwhile topic to use in a satire. Unless you're pretty well informed about the Higgs, you're not going to get the joke and find it funny. That the Financial Times thinks much of its readership will get the joke is kind of remarkable.

3. **MathPhys**
   April 13, 2012

   I usually have negative things to say about M Kaku, but I have just watched the video clip that Peter linked and I must say that he spoke very well in favor of supporting science. He gave a level-headed impression, used the right words and metaphors (no matter how oversimplified) that can make sense to the layman and, hopefully, make a difference.

4. **anonymoo**
   April 14, 2012

   Peter,

   Read the FT piece through to the comments and you'll see that pulling strings behind the facade of "reality" is taken to be the prerogative of the policy wonk. The text-book is Thomas Hobbes' and that should not surprise Ye denizens of Ecclesiastical Polity. This is Scientific Materialism as also preached by Galileo from Tertullian.

5. **Shantanu**
   April 14, 2012

   Peter, see Rabi Mohapatra's talk at PI. e believes SO(10) has to be right.
   http://pirsa.org/12040080/

6. **Joel Rice**
   April 14, 2012

   I have not seen anything so funny on a physics blog in … well, ever.

7. **Joy**
   April 15, 2012

Ok, so electrons collectively interacting in condensed matter can function on a quantum level as a sort of virtual “Majorana particle” (not the public’s idea of a particle, but never mind that)
Prediction: This 1930s concept will be said to be evidence for string theory by midweek

8. **luminosity**
April 17, 2012

It is curious that almost all the posts are to laugh at Kaku/string theory but nobody has anything to say about the LHC and the outstanding work at CERN. When claiming a record luminosity, be careful to restrict to hadron colliders. Or else check what the e+e- colliders achieved, for example at the B factories (including CESR). It’s so easy to laugh at or put down other people. It’s so much harder to come up with a positive contribution of one’s own.

9. **Peter Woit**
April 17, 2012

luminosity,

Thanks for the correction. You’re right, the LHC has not yet quite reached the luminosity levels achieved at the e+/e- B-factories (they are above CESR, but not PEP-II). I agree with your comment that the progress at the LHC is remarkable and this deserves to be noted and appreciated. Unfortunately, as long as things are going well, the LHC likely won’t produce dramatic news until early July. So, to keep people entertained, the continuing antics of Kaku and the string theorists are one of the few possibilities here.

Quantum Gravity at Scientific American

April 17, 2012
Categories: Uncategorized

Scientific American is doing a good job this month of putting out stories related to quantum gravity that actually make sense, steering clear of the multiverse and other pseudo-science. This month's magazine has a very nice article by Steven Carlip about quantum gravity in 2+1 dimensions. For a more technical introduction to the subject, Carlip's book and review article are good places to start.

I haven't seen the new May issue yet, but from their web-site, it seems that their cover story on new ideas about "A Unified Physics" is what looks to be an interesting article from Zvi Bern, Lance Dixon and David Kosower: Quantum "Graviton" Particles May Resemble Ordinary Particles of Force, summarized as

> Maybe unifying the forces of nature isn't quite as hard as physicists thought it would be.

I'm curious to see the full article, but I assume it's about the intriguing work on amplitudes of recent years that has shown that supergravity theories have fewer divergences than people thought, for reasons that are still unclear. There's presumably some new symmetry structure here, and understanding it may offer a way around the old argument that "you can't put quantum mechanics and general relativity together, the quantum fluctuations at short distances are just too violent." Maybe you don't need strings, M-theory, the multiverse, and all the other baggage theorists have been weighed down by for the last quarter-century. There has been quite a bit of discussion about this topic here, the earliest posting is this one from 2005. For another take on how these ideas might lead to a new way to handle quantum gravity, the latest visionary talk by Nima Arkani-Hamed from last week at the Simons Center is available here.

Also at Scientific American, George Musser has been producing some interesting blog entries on these topics. There's a video here about the Carlip piece, a story about Darth Vader and the Emperor Palpatine that was discussed here, and a recent nice explanation of work on higher spin theories here.

Update: Also in the May issue, from Davide Castelvecchi, there is a shorter article, *Is Supersymmetry Dead?* with summary

> The grand scheme, a stepping-stone to string theory, is still high on physicists' wish lists. But if no solid evidence surfaces soon, it could begin to have a serious PR problem.

Peskin is still a believer though:

> "It is the next step up toward the ultimate view of the world, where we make everything symmetric and beautiful," says Michael Peskin, a theorist at SLAC National Accelerator Laboratory…
>
> Many are still hopeful. "There are still very viable ways of building supersymmetry models," Peskin says. Expecting to see new physics after just a year of data taking was unrealistic, says Joseph Lykken, a theorist on the CMS team.

Comments

1. **Vince**
   April 17, 2012

   I'm not, by any stretch of the imagination, an expert at any of this, but I heard it said that supergravity still needs a "UV completion" even if it is finite/renormalizable. And string theory is supposed to provide that UV completion. Is this necessary?

   Also, is this allegedly finite supergravity theory a supergravity theory that may describe our universe? That is, is this supergravity theory based on 4D spacetime, etc.?

2. **Proudmemberofthecult**
   April 17, 2012

   @ Vince

   There are two issues here:

   – does N=8 supergravity make sense as a perturbative quantum field theory? (is it finite or renormalizable)
   – what is the complete non-perturbative structure of this theory?

   Bern et.al. have something to say about the first question. The second question is very interesting as it has relevance for black hole physics and many other things. If your interest is in question 2, then it seems at present you need superstring theory to give a well-defined answer. However, we do not know the (full) answer to the second question even for Yang-Mills theories! As far as *perturbative* finiteness is concerned superstring theory is irrelevant.

   Moreover, if N=8 would make sense perturbatively (be finite or renormalizable), there's no reason why it shouldn't make sense also non-perturbatively. This however is very much a handwaving argument.

3. **Peter Woit**
   April 17, 2012

   Vince,

   Yes, this is 4d supergravity. In principle it could describe our universe. It doesn't obviously provide an interesting unified theory though (in the sense that it doesn't explain the standard model or make solid LHC predictions). What's interesting about this though is that it raises the possibility that there are new ways of dealing with QG and making unified theories, you don't need completely new, different degrees of freedom.

   I've repeatedly seen the argument made that "OK, maybe supergravity is alright perturbatively, but there's problem X non-perturbatively, which is solved by string theory". But we know little about supergravity non-perturbatively, and definitely don't have a viable non-perturbative string theory, so any arguments of this kind seem to me less than solid. But, again, even if N=8 supergravity has some problem as a viable unified theory non-perturbatively, the interesting thing is that its unexpected perturbative consistency raises all sorts of new possibilities, opening back up an area of research that had been considered closed off.

4. **Speculative**
   April 18, 2012

I'm no expert so I'm only speaking from a standpoint of analyzing possible reactions, but if N=8 Supergravity is finite, it probably won't dampen string theory research. In my opinion it will probably be a boon to ST because supergravity is a low energy limit of ST. Correct me if I'm wrong, but if a supergravity theory is finite, wouldn't the ST UV completion also be finite? Besides string theory does still hold the promise of a unified theory in a way that sugra doesn't.

I'm not making any judgements on the merits of the theories themselves, I just think this will be a more likely reaction among the HET/String community.

5. **Peter Woit**
April 18, 2012

Speculative,

If N=8 supergravity is finite, that doesn't fix any problem with string theory that anyone was worried about, quite the opposite. It removes the whole "ultraviolet completion" motivation for string theory, since it says that N=8 supergravity already is "ultraviolet complete". Yes, N=8 supergravity does not now provide a viable unified model, but the interesting thing here is that we may be learning something new which could affect our understanding of the possibilities for such models. I go on far too much about this, but I think it has been clear for a while that string theory is a failure on the unification front. The possibilities inherent in string theory for unification have been investigated to death for more than a quarter century, leading to the dismal current state of affairs.

At this point though, having the main motivation for string theory removed isn't likely to convince many string theorists to head for the lifeboats. If the landscape didn't do that for someone, there's no reason not to believe they're not willing to go down with the ship.

6. **Giotis**
April 19, 2012

Peter,

I didn't know that you are promoting N=8 supergravity these days. This is a pleasant surprise from someone who don't believe in SUSY.

7. **Peter Woit**
April 19, 2012

Giotis,

I "don't believe in SUSY" in the sense that I don't believe that the standard version of SUSY is a symmetry that appears in nature. It's not completely mathematically compelling, one doesn't see it explicitly, and breaking it, even spontaneously, leads to a useless mess. However, there are some very interesting mathematical structures involved in SUSY, which likely do have something to do with the real world. An example of the kind of thing I have in mind is Witten's use of "twisted N=2 SUSY" to produce topological quantum theories. Maybe there's some other way of "twisting" SUSY to get something more interesting.

N=8 supergravity doesn't give a viable unified theory, but, if it shows that there is a class of QFT's which give quantized gravity without non-renormalizability problems, I'd love to know why this happens, and how large the class of such QFTs is.

8. **SA**
April 19, 2012

I am surprised that you have not discussed the recent article from Kane, attempting to shift the perception of string theory towards it being a theoretical framework, like vanilla QFT and not necessarily a theory of particle physics.

9. **Peter Woit**
April 19, 2012

SA,

I've discussed Kane's claims quite a lot here, probably more than is a good idea since no one takes them seriously. The "string theory is just a framework, like QFT" excuse for failure has been around a long time, also often discussed here.

10. **Giotis**
April 20, 2012

OK Peter but the thing is that you are facing a contradiction here. All these novel approaches (like those in Musser's posts) which apparently you find very interesting are more or less related to String theory research. They are investigated and promoted by String theorists working within the general context of String theory and become important due to that.

Amazingly String theory incorporates every promising idea in high energy theoretical physics and soon you won't be able to talk about anything without referring to the general String theoretic framework.

In fact this general framework would become so large that practically would be synonymous to high energy theoretical physics.

This ship is unsinkable…

11. **Peter Woit**
April 20, 2012

Giotis,

"This ship is unsinkable…"

I don't doubt that whatever idea has some success, string theorists will insist it is "string theory", since there will be some connection of some sort between it and at least one of the tens of thousands of "string theory" papers written over the last 30 years. At this point, Strings 20XX mostly has nothing much to do with string theory. Where this was going was clear back at the time of the following quote, way back in 2005

"Most string theorists are very arrogant," says Seiberg with a smile. "If there is something [beyond string theory], we will call it string theory."

Whatever. If a great discovery is made that gets us beyond the SM that will be wonderful, no matter how many string theorists will insist on saying it is "string theory" to make themselves feel better about the wasted last quarter-century.

12. **Aleksandar Mikovic**
April 21, 2012

String theory is not the only known theory which provides an UV completion of quantum General Relativity. Spin foam models of quantum gravity are another example, and it has been recently shown that the EPRL-FK class of spin foam models has representatives whose classical limit is General Relativity, see
arXiv:1104.1384, Effective action and semiclassical limit of spin foam models,
A. Mikovic and M. Vojinovic,
Journal-ref: Class. Quant. Grav. 28, 225004 (2011).

13. **sheepdip1**
April 24, 2012

> I haven't seen the new May issue yet, but from their web-site, it seems that their cover story on new ideas about "A Unified Physics" is what looks to be an interesting article from Zvi Bern, Lance Dixon and David Kosower:

The article is out now:
http://www.scientificamerican.com/article.cfm?id=search-for-new-physics

Any comments?

Adventures in Peer Review

April 18, 2012
Categories: Uncategorized

Yesterday's New York Times had an article by Carl Zimmer about increasing numbers of retracted papers in the biological sciences. Physics and Mathematics weren't part of the story and I don't know of any evidence of retractions increasing in these fields (although maybe they should, given the Bogdanov and other scandals).

There's a blog called Retraction Watch where they follow these things, and they have come upon a mathematics example. A couple years ago the Elsevier publication *Computers and Mathematics with Applications* published the article "A computer application in mathematics". It's less than a page long, one author has a yahoo.com e-mail address, the other a budweiser.com address. Last week Elsevier finally got around to acknowledging that something was up, publishing a retraction notice that explained:

> This article has been retracted at the request of the Publisher, as the article contains no scientific content and was accepted because of an administrative error. Apologies are offered to readers of the journal that this was not detected during the submission process.

I gather that this is behind a paywall, so you need to be at a place like Columbia that pays Elsevier a lot of money, otherwise you can't read the retraction. That's also true of the original paper, but if you want to violate all sorts of intellectual property laws, you could click here.

Comments

1. **Mean and Anomalous**
   April 18, 2012

   Thanks. Sean Carrol at Cosmic Variance is exploring this same topic, but he did not mention the Bogdanov affair (last I checked).

2. **Rhys**
   April 19, 2012

   My favourite bit have to be this:

   "In brief an impossible proposition was proved as possible. This is a problematic problem."

   Yes, it is!
   Obviously something went badly wrong here, but the whole peer-review system is a bit shaky, as it depends on referees being conscientious, with no tangible rewards if they are so, or serious repercussions if they are not.

3. **DaniH**
   April 19, 2012

   Dear Peter,

   In relation to the Elsevier paper, the "bad habits" of Elsevier are well known in part of the physics community and most of the mathematical community. It is well-known, for example, the case of the journal Chaos, Solitons and Fractals where the editor-in-chief, El Naschie, published tens of papers without any peer review.
   Some more cases in mathematics can be found at Math2.0
   The peer review model has serious problems and some are discussing alternatives there.

   Offtopic: I am still astonished to see that most of the physics blogs have not said a word about the ongoing Elsevier boycott. I invite everybody interested to visit The Cost of Knowledge

4. **Cesar Laia**
   April 19, 2012

   That paper is a funny (and extreme) example of how things go wrong, but I think it is impossible to avoid these sort of things. I wonder how many papers are being overlooked or hyped, and how to many papers are being ignored or refused because they don't meet certain obscure criteria of editors, readers or reviewers… it's a sociological pain.

5. **Aidyan**
   April 20, 2012

   A quite natural consequence of the "publish or die" and "impact factor" philosophy of our actual academic system. Goedel published only a handful of papers. De Broglie did not much other significant work beyond his particle wave relation paper. A giant like Feynman published about 120 papers but half of it were reviews, reflections, philosophical commentaries. While I find those who boast about their "more than 200 papers in peer reviewed journals" (and are perhaps only 40 years old…) as laughable little midgets on stilts. Most of these are at best good science managers, politicians, academic barons who have under their feet several little soldiers who work for them and sign their papers. A system where a Sadi Carnot or a Gregor Mendel would have no chance, as they in fact didn't have, or a new semi-autistic Dirac would be discarded from the outset because of his scarce PR qualities (or because being a former engineer…). A system so obsessed by presenting to the audience its "renowned physicist" being 'brilliant' in talking and joking during their PPT presentation, instead of looking for new ideas. A system obsessed by deadlines and of "over secrecy, and rushing out their papers to beat their competitors" that it obviously had to produce 'faster than light neutrinos'. Papers retracted? Maybe, to understand what really stands behind it, it would be worth to take a look also at some of the papers rejected.

Spring in the Virgin Islands

April 19, 2012
Categories: Uncategorized

One thing that a career in math or physics research can get you, courtesy of financial industry wealth, is a nice trip to the Virgin Islands. A couple current possibilities are:

- The Simons Foundation funds week-long Simons Symposia, at Caneel Bay, on St. John. The next one is next week, on Knot Homologies and BPS States. These are serious, invitation only, family members discouraged, events. To get this trip you better be a top expert in a specific field of the Symposium. The Simons Foundation plans to accept proposals for next years Symposia in the fall, see here.
- If you're a "renowned physicist", preferably one with a Nobel prize, then you're eligible for a trip to financier Jeffrey Epstein's own Virgin Island, Little Saint James. Epstein (described by Wikipedia as "an American financier and science philanthropist, and convicted sex offender") a couple weeks ago "Convened a Conference of Nobel Laureates to Define Gravity", according to this press release from his foundation. The event was organized by Lawrence Krauss, who is quoted as describing the situation as follows:

  > "Right now we're floundering," Krauss admits. "We're floundering, in a lot of different areas."

Comments

1. **jpd**
   April 19, 2012

   "an American financier and science philanthropist, and convicted sex offender"
   i am not sure which is most disgusting

2. **Yatima**
   April 20, 2012

   Financier – Provides jobs and shifts resources where they are needed
   Science philanthropist – Provides money for people to perform tasks that do not need to show a ROI next quarter
   Convicted sex offender – For all we know, bonged a slightly underage beauty early in his life to the great horror of the "no fun allowed" puritanicals around

   À priori, I see nothing to be disgusted about.

3. **Peter Woit**
   April 20, 2012

   I think the two comments above more than enough cover the range of reactions to Epstein's sexual behavior. Please, that's enough on that topic.

4. **Tim Duckley**
   April 20, 2012

   How should we construe the lack of Witten at the conference? Did he refuse to go or was he refused an invitation? Were you invited Peter and if so why did you refuse to attend?

5. **Peter Woit**
   April 20, 2012

   Tim Duckley,

   Since attendees at Epstein's symposium are supposed to be physicists with Nobel prizes or equivalent scientific achievements, it would be absurd to invite me. If instead of "renowned physicists", he decides to have a symposium of "renowned physics bloggers", maybe I'll get invited. I have no objection as a matter of principle to helping wealthy financiers spend their money.

   I have no idea if Witten was invited, his achievements certainly qualify him for an invitation. Unlike me though, Witten is a very serious person, I suspect not much interested in the entertainment value of a week of hob-nobbing and snorkeling with illustrious people. So, I wouldn't be surprised if he turned down this travel opportunity (and many of the others he gets offered). For a somewhat related story, I was amused to read in David Kaiser's "How the Hippies Saved Physics" about how a yearly series of Werner Erhard-funded theoretical physics conferences came to an end once string theory took off, so Witten's presence would be expected, but Witten (quite sensibly…) wanted nothing to do with that sort of thing. See my review of the book here:

   http://www.math.columbia.edu/~woit/wordpress/?p=3785

6. **Shantanu**
   April 22, 2012

   Peter or anyone else, is there a website for this conference? Note that there was a similar one in 2006 http://www.phys.cwru.edu/events/grav_ws.php

7. **Don Murphy**
   April 24, 2012

   Shantanu:

   I sent Krauss and email asking if there is a website for the conference and if there will be any results published. I am awaiting a reply. I found nothing on his website.

   Don

8. **Wojciech Langer**
   May 9, 2012

   It is hard to reply to this very well 'camouflaged' information – so many restrictions placed by the author 🙂

I found this site looking desperately for any reviews or comments by scientific community, regarding recent Krauss' book "A Universe from Nothing".

I read Krauss books 7 years ago: "Quintessence" and "Hiding in the Mirror", both quite good. Man , oh man, how he has 'evolved' since then!
Epstein's recent 'conference' info can be found googling 'Epstein Foundation'.
The conference was IMO essentially meaningless to the scientific world (as per Krauss quote), but snorkeling must have been nice! What money can do!

Weinberg on the Crisis of Big Science

April 21, 2012
Categories: Uncategorized

Steven Weinberg has a new article in *The New York Review of Books* on The Crisis of Big Science, which is based on a talk he gave this past January at the American Astronomical Society meeting in Austin (for some discussion of this, see here and here).

Weinberg is rather gloomy about prospects for particle physics, seeing dim prospects for a new generation of particle accelerators, especially in the US. He goes over the sorry story of the SSC, which he was deeply involved in, and worries that the same thing is happening to the James Webb Space Telescope project. He argues that progress is particle physics will be difficult without going to higher energies:

> The discovery of the Higgs boson would be a gratifying verification of present theory, but it will not point the way to a more comprehensive future theory. We can hope, as was the case with the Bevatron, that the most exciting thing to be discovered at the LHC will be something quite unexpected. Whatever it is, it's hard to see how it could take us all the way to a final theory, including gravitation. So in the next decade, physicists are probably going to ask their governments for support for whatever new and more powerful accelerator we then think will be needed…
>
> That is going to be a very hard sell. My pessimism comes partly from my experience in the 1980s and 1990s in trying to get funding for another large accelerator….
>
> During the debate over the SSC, I was on the Larry King radio show with a congressman who opposed it. He said that he wasn't against spending on science, but that we had to set priorities. I explained that the SSC was going to help us learn the laws of nature, and I asked if that didn't deserve a high priority. I remember every word of his answer. It was "No."…
>
> All these problems will emerge again when physicists go to their governments for the next accelerator beyond the LHC. But it will be worse, because the next accelerator will probably have to be an international collaboration. We saw recently how a project to build a laboratory for the development of controlled thermonuclear power, ITER, was nearly killed by the competition between France and Japan to be the laboratory's site.
>
> There are things that can be done in fundamental physics without building a new generation of accelerators. We will go on looking for rare processes, like an extremely slow conjectured radioactive decay of protons. There is much to do in studying the properties of neutrinos. We get some useful information from astronomers. But I do not believe that we can make significant progress without also pushing back the frontier of high energy. So in the next decade we may see the search for the laws of nature slow to a halt, not to be resumed again in our lifetimes.

He has similar worries about cosmology:

> But cosmology is in danger of becoming stuck, in much the same sense as elementary particle physics has been stuck for decades. The discovery in 1998 that the expansion of the universe is now accelerating can be accommodated in various theories, but we don't have observations that would point to the right theory. The observations of microwave radiation left over from the early universe have confirmed the general idea of an early era of inflation, but do not give detailed information about the physical processes involved in the expansion. New satellite observatories will be needed, but will they be funded?

I'm not well-informed about what is going on with large projects in astronomy like the JWST, but do see news reports about cancellation or possible cancellation of important and valuable instruments which people have been working on for years. It's likely Weinberg's arguments are highly relevant in these cases. About particle physics though, I fear he neglects to mention the underlying scientific and technological difficulties of going to higher energies. A major reason why things look gloomy for another generation of colliders is that it's not clear what to build. Electron-positron colliders like ILC/CLIC would be very expensive, and not necessarily get to energy levels above those reached by the LHC. They would be excellent tools for studying TeV-scale physics, but if the LHC has shown there's no new physics there, the case for building them will be hard to make. Probably the best bet for going to higher energy is the HE-LHC, an LHC upgraded with higher field magnets. The technological limits on such magnets though will make it hard to go to dramatically higher energies. If no new physics besides the Higgs shows up at the LHC, there won't be a good reason to expect it at HE-LHC energies. The case for the LHC was a slam-dunk, because we knew that the Higgs or something doing the same job had to be accessible at LHC energies. What there will be for an HE-LHC to study is less clear.

An HE-LHC would of course be built in Europe, so prospects for a new collider in the US are very dim indeed. Weinberg attributes the problem to a failure of the US to support scientific research, and the public good in general (please, take discussion of his political arguments elsewhere, I'm sick of this already, and November is a long ways away…). About support for science I think he's a bit disingenuous though, arguing:

> Funding is a problem for all fields of science. In the past decade, the National Science Foundation has seen the fraction of grant proposals that it can fund drop from 33 percent to 23 percent.

without noting that the NSF has seen sizable budget increases over the past decade. The fact that the number of Ph.Ds in the subject is increasing much faster than funding for them to do research is another problem…

Comments

1. **uair01**
   April 21, 2012

   I hate to be obnoxious but isn't there some truth in what the congressman says? At least something to take seriously? I'm not a scientist so for me the fundamental research into neutrinos and Higgs bosons is a beautiful cultural enterprise, akin with art and poetry, beautiful, inspiring and life-value-adding but not something that "puts bread on the table". And if I have to spend my tax money on fundamental research then I would prefer to invest it in research on nuclear fusion – that scientists have been promising us for 50 years already – because that would solve a lot of our energy and climate problems. I don't see deep particle physics or cosmology doing that – let alone all the money that is spent on string theory.

2. **Peter Woit**
   April 21, 2012

   uair01,

   Money spent on string theory is negligible compared to everything else being discussed. Similarly, funding for deep particle physics and cosmology research is small compared to energy and climate research. The usual argument that we can't do particle physics because we need the money for X isn't very compelling since typically money already spent on X is 1-2 orders of magnitude higher.

   To me, the question is whether the particle physics community can come up with a compelling plan that can be funded at a level around the historical budget fractions devoted to fundamental research of this sort. You're always going to have people with different views on what society should fund, and maybe things will shift and those opposed to spending even a small fraction of the budget on fundamental research will impose their will. But no one should be under any illusion that eliminating particle physics funding is going to make a dent in overall budget problems.

3. **SteveM**
   April 21, 2012

   Increasing vision is increasingly expensive: peering deeper into space or the structure of matter takes bigger telescopes and accelerators, which simply cost more and more until there is a cut off. If the LHC finds nothing except the Higgs then it will probably be a last big accelerator project. But the congressman was flat wrong. The status of the US as a superpower depends in part on it being able and willing to pursue projects like the SSC. The purpose of fundamental research is that we don't know what the long-term potential applications could be. I guess in the 20s the emerging field of quantum mechanics must have looked pretty useless but it eventually spawned the transistor, laser and microchip, leading to vast new industries and sources of wealth. The problem the US is facing in big science right now is that it is not losing out in just one area but that it is losing its edge, to Europe and Asia, right across the board: in particle physics, space/aerospace, fusion, biology etc. The space shuttle is now in a museum and pretty soon Fermilab will probably become a science museum. Some of these issues are also discussed in this video featuring a distinguished panel with James Simons, Michio Kaku, biologist Craig Ventor and others..

   http://www.youtube.com/watch?v=i8lKGdDsHfg

4. **Anonyrat**
   April 21, 2012

   In a country where about 40% of the electorate subscribes to the notion that taxation is theft, it is hard to gain support for large government-funded science projects. This is true even if overall spending on science is rising. The big ticket item still serves as a lightning rod.

5. **sam coleridge**
   April 21, 2012

   By far the most important system for in situ (under water) observation of the ocean for climate is NOAA's fantastic TAO array which spans the entire tropical Pacific.

   http://www.pmel.noaa.gov/tao/

   Its annual running cost is of the order of $10m (that's m for million) yet the lead scientists are endlessly defending it. The array should be extended into the subtropical N and S Pacific, at the cost of a few more $10m. Fat chance. Folks should not blame the shortage of funds for HEP on climate research.

   By The Way whenever I have told taxi drivers about the TAO array they would go totally ape. They always said: "THAT is what we should be spending tax dollars on".

   BTW(2) I am not funded by TAO nor NOAA.

6. **Peter Woit**
   April 21, 2012

   sam coleridge,

   No one is blaming the shortage of funds for HEP (to the extent there is one; as I tried to make clear, a big problem is that HEP is facing difficulties no amount of money can solve) on climate research. It's a good idea to keep in mind though that if there's a fight over funding for some particular HEP project and it gets canceled, I'm quite sure that the money is not going to be redirected to the TAO.

7. **Thomas Larsson**
   April 21, 2012

   SteveM. Physics discoveries like EM or QM may have been esoteric in their days, but they were esoteric on terrestial energy scales. TeV physics is relevant in supernovas or in the Big Bang, but will never be relevant on earth. You cannot build a nifty gadget if you need to carry around an LHC to create the relevant state of matter.

8. **Peter Woit**
   April 21, 2012

   Thomas,

   Please avoid the unpleasant use of ad hominem arguments, they're really unhelpful, so I've edited your comment to remove them.

   I actually agree that it seems unlikely that better understanding of TeV scale physics will lead to practical applications. However, in the early part of the last century it might have seemed obvious that nuclear energy scales could never be practically relevant. Probably a lot of people believed this up until the day Hiroshima was annihilated.

   The interesting discoveries at the LHC will be the ones that tell us about the Higgs mechanism. It is not inconceivable (although I agree, not likely) that understanding the Higgs mechanism could in the distant future have some sort of application.

9. **Trulo**
   April 21, 2012

   Thomas, I agree with you on general grounds, but with one important caveat. If in 1980, say, I'd have asked you what use is GR on human or even terrestrial scale, I'm sure you'd have told me no use at all. Yet, today you probably have a GPS device in your car.

   The moral of all this is that natural laws must be there at our disposal when we need them. Much like books in a library, most of which hardly anyone ever reads. But still they must be there in case we need them. For that to be possible we need to discover them in the first place.

10. **SteveM**
    April 21, 2012

    Thomas, not sure what I said to merit an ad hominem response. (Now edited, before I read it.) But I really meant fundamental research in general, across the board in all areas, and not just in TeV-scale particle physics, which I strongly support nonetheless. Maybe I'm an idealist but I think fundamental big science research and the quest for pure knowledge–whether it can ultimately be applied or not–is the best investment there is, esp. for the US. Certainly money better spent than the trillions wasted on wars, "wars on terror", and bailing out certain dysfunctional and dated financial firms.

11. **Z**
April 21, 2012

What are the prospects for a muon collier in the next 20-30 years? There was a lot of talk about such an idea a few years ago, with the principal reason being much higher energies, or lower cost compared to the ILC.

But I haven't heard much of this idea recently. What are the technical or economical problems with it?

12. **ChuckO**
April 21, 2012

If physicists would really like the government to fund the next-generation collider in the US, all they would have to do is convince Congress and the Pentagon that it would lead to the development of some sort of super weapon.

13. **uair01**
April 21, 2012

The discussion raises interesting questions for me (as an outsider). I'm convinced by Peter Woit that the amount of money spent on "esoteric" physics is negligible compared to the rest of the research budget. But what about the intellectual energy? Would it be possible to redirect it in a fruitful way? For example, could people switch fruitfully from deep particle physics to nuclear fusion? And could young scientists be motivated to choose one field over another? And should we meddle with the mechanism or should we leave it to the "free scientific market" and could a "scientific plan economy" work at all? Thinking about all the serendipitous discoveries we should probably not try to steer the process, but then Woit & Smolin have already demonstrated that the process is not fruitfully self-regulating.

14. **Tony Smith**
April 21, 2012

Steven Weinberg said (New York Review of Books 2012) about The Crisis of Big Science:
"… What really motivates elementary particle physicists is a sense of how the world is ordered – it is, they believe, a world governed by simple universal principles that we are capable of discovering.
…
But not everyone feels the importance of this.
…
What does motivate legislators is the immediate economic interests of their constituents
…
Perhaps if the SSC had cost more, it would not have been canceled. …".

What if the USA physics community (prominent figures like Steven Weinberg, and associations such as APS)
proposed a $1 trillion project to understand more about fundamental high-energy physics
and
to create jobs for subcontractors which jobs would be required to be USA jobs spread out over the entire USA
?
The $1 trillion would allow for at least 3 projects:
1 – reconstruct the Texas SSC and raise its energy to 100 TeV
2 – a Linear Collider (maybe on a midwest site)
3 – a Muon Collider (maybe in the Pacific area on a site far from cities to minimize radiation hazard, say in eastern Oregon or Washington)
Subcontractor jobs could be spread all over the USA.
The projects should be built to a gold-standard with no cost cutting
in order to guarantee spending all of the $1 trillion and maximize job creation.

The $1 trillion could be directly funded by the USA government as part of the Quantitative Easing (effectively money-printing without raising any specific tax rates) program
that has already put multi-trillion dollar amounts into propping up the Derivative Casino of the Big 5 Banks.

Maybe the APS and prominent physicists might make a clear and loud case
that understanding fundamental physics is more important to human civilization than any Derivative Banking Casino
and
that a revived SSC will give more jobs/money to the bulk of the population
than does putting trillions in the pockets of Derivative Casino Bankers
and
will at $1 trillion actually cost LESS than the multi-trillion dollar QE bailouts already spent on the Big Bank Derivative Casino.

Tony

PS – I would like to add another Steven Weinberg quote, from a 28 June 2011 Bloomberg article by Zinta Lundborg:
"… the students who really are so good that they can feel they are going to be part of the effort of discovering the fundamental principles of nature … stay in the field.
It's the students who are not so sure of that who migrate into Wall Street.
I don't know of any cases where a student was doing really first-rate work who then moved into Wall Street. …".

That seems to me to be yet another reason for the projects of the first-rate students to get at least $1 trillion from the Quantitative Easing program that has already given multi-trillion to the Wall Street of the second-raters.

PPS – To those faint-hearted souls who might say that such a project might not succeed in the political climate of today's USA, I say that
you will never find out unless you try: I CAN'T NEVER DID.

15. **Roger**
April 21, 2012

No amount of money is going to "take us all the way to a final theory", as Weinberg advocates. As long as he states his goals in terms of these unachievable fantasies, the congressman is right to say "No".

16. **Steven C**
April 21, 2012

Given current political environment, very expensive and high profile science projects are going to be hard political sell. Even overall science spending is relatively small in US budget, science bearing part of the cost of US fiscal problems is expected. I do have to agree that under current political environment, prioritizing projects are definitely necessary.

If there is one thing I am against the tone of Dr Weinberg, what science folks should do is to sell/market/talk to the public the coolness, applications, and benefits of science. That would probably manage the expected science funding cut. I dislike the gloomy and negative tone (especially when some of the gloominess is not based on current physics possibilities as some poster stated already – you don't build a particle collider for its own sake – one does need to show possibility of new physics from whatever new collider). Instead of talking about fear that we may fall behind, we may better off selling projects that we know it may work, and make the projects sexy to the public.

17. **Peter Woit**
April 21, 2012

Tony,

The only problem with this plan is that if the Fed could be convinced that helicopter drops of cash should be directed elsewhere than the large banks, HEP physicists wouldn't be the only ones with their hands out. We could however possibly convince Simons and Epstein to stop wasting their money on directly supporting science, and instead start pouring their billions into electing officials who would make sure HEP gets the lion's share of any further QE. After all, that's what other interest groups are doing…

18. **Aidyan**
April 21, 2012

People are getting more and more skeptical of the so called 'big science'. And rightly so. The genome project promised us miraculous cures for genetic diseases, but now we know that it miserably failed. The ISS was advertised as a great revolution for material science, but so far not much came out of it. It is now half a century that they are telling us that controlled nuclear fusion would save us from future energy crises, but it is still unclear if it is possible even in principle to build the plasma containing walls. Astronauts were send to the moon but today everyone realizes that it was only about the cold war and politics, certainly not about science (and where is the 'giant leap for humankind.'..?). The Space Shuttle was supposed to become cheap and reusable, instead it turned out to be a bottomless pit. Another half of a century on cancer research produced equally very partial and disappointing results. It is now at least three decades that I hear about the coming bio-engineering revolution that should save the world. And we are still waiting…. And responsible of this state of affairs are to a good extent not only politicians but also men of science (I remember Carl Sagan lobbying for sending men on Mars… oh, no please not….).

But, what about funding several small science projects instead of a single big science one? Does it really make sense to divert all the funds, efforts and skills of people into the n-th mammoth project? It has become easier to get mi-bi-tri-zillions for conventionally accepted lines of research than for small and cheap original projects. I saw how it is sometimes almost impossible to get only 50.000 dollars for a post-doc working on a little but novel and original non-mainstream line of research just because it is new (i.e. risky), original (i.e. of uncertain effectiveness), and non-mainstream (read: who will take responsibility if the 'black sheep' will not bleat with the flock?). Of course, I know, I know…. some discoveries will never come from little projects, and the times of the lonely genius working in the patent office are over. But maybe we should reconsider this obsession for stratospheric projects. It is not about abolishing big science, it is about rediscovering the potentiality of the small scale one.

19. **Peter Orland**
April 21, 2012

Much of what I am seeing here has little to do with Weinberg's original points about high-energy physics. Most of science, including most of physics (but perhaps excluding particle and h-e nuclear physics), has a bright present and a bright future. For every example above of a failure there is also a success.

Some programs have not lived up to their technological promise – ventures always bring risks. People often conflate discoveries (or non-discoveries) hyped through the media with real progress- then become cynical when their expectations aren't realized. So what?

Who said science and engineering were easy?

20. **armin**
April 21, 2012

Nowadays, any big science project that spans several election cycles severely risks falling victim to the vagaries of politics.

Why can't we invest at least some resources to try to find entirely new and different answers to the technical question of how to precisely deliver a large amount of energy to single elementary particles? Sure, this will require going back to the drawing board, possibly even revisiting some 80-90 year old engineering concepts that underlie modern particle accelerators, but, given (1) the staggering costs of any future particle accelerators, (2) the enormous risk that they will not be completed and (3) the absence of exotic discoveries with the LHC so far that would further motivate the construction of such projects, this seems to me a worthwhile line of research to invest in. Arguing against this strikes me a little like arguing against modes of personal transportation that are entirely new and different from the latest high-end model horse carriage.

BTW I don't know of nor advocate any particular "alternative" approach to accelerating particles, but I do believe in out-of-the-box thinking.

21. **Ben**
April 21, 2012

Re: Tony Smith's idea is naive. Also, in regard to Weinberg quote "I don't know of any cases where a student was doing really first-rate work who then moved into Wall Street" We have a high profile example in Jim Simons, of Cern-Simons fame

22. **Clara**
April 22, 2012

Weinberg hides a failure. The failure of big science is first of all a failure of theoretical physics. We lack a good theoretical model. One that is so compelling that it excites everybody to test it with a big experiment. If somebody came up with a great theory, a theory that would get everybody excited in the style of "yes, now we really have a candidate of a TOE" and "Finally a TOE without nonsense, without strings and without a multiverse" and "Finally a TOE that is not obviously not even wrong" THEN the trillion dollar experiment would be no problem.

Nobody will spend huge amounts of money if the chances for result are so low.

The failure is one of theoreticians. They do not need big money. But they have produced only "Not even wrong" models since 40 years. They have focused on supersymmetry – because Weinberg promoted it for 40 years, among others – and now are finding out that they were chasing a fantasy.

The real failure is people like Weinberg, who drove most theoreticians and experimentalists to search for supersymmetry. And this was a mistake. It was, first of all, Weinberg's personal mistake. If Weinberg had not created such a "lemming climate" in theoretical physics, we all would have had a much better chance to have his "final theory" already on the table.

The whole article is that of an old bitter man who tries to divert from the fact that he is one of the main culprits for the present sad situation.

23. **harryb**
April 22, 2012

Some authors claim up to 30% of current US GDP can be directly linked to applications of QM – transistors, lasers etc and their technological offshoots. A century earlier than that you could make the same argument I guess re Maxwell's Equations.

The cheapness of theoretical physics, chalk and PCs, suggests its economic leverage is potentially vast, and sad to see it come under such cost scrutiny.

But, when taken down dead-ends, that credibility is undermined. The over-focus on ST has surely contributed to the lack of options now open to HEP.

Its a crisis of ideology and theory as much as Big Science.

24. **David Bailey**
April 22, 2012

If nobody had known about General Relativity, I somehow doubt that this would have prevented satellite navigation. I think the engineers would have discovered the discrepancy and added an ad hoc correction to fix the error. Anyway, developing and testing GR had a much lower cost than anything being discussed here.

I think Aiden makes a very strong point that (sadly) big science has not delivered in recent years.

The problem seems to be that science just blurs into politics at this exalted level!

More generally, unless the human race can expand into space and command much more enery than it can on earth, the quest for larger accelerators must end sometime – politics is just bringing that day forward!

25. **emile**
April 22, 2012

Clara: as you probably know, Weinberg came up with the theory that is currently being put to the test at the LHC (A model of leptons). To blame him for the current situation just sounds strange: he came up with a theory that we can test. Second, searching for Supersymmetry on the experimental is not a waste of time: it covers such a vast amount of final states that it is essentially a generic search for physics beyond the Standard Model.

26. **jpd**
April 22, 2012

this claim:
"The genome project promised us miraculous cures for genetic diseases, but now we know that it miserably failed."
is wildly ignorant

27. **Anonyrat**
April 22, 2012

It may be interesting to plot for each year from 1890-2010, the age of the chief mover/shaker in theoretical physics that year. If there were several, include them all.

Such a plot might show that physics is becoming more and more a field where long-established authority reigns.

One data point is below
{1905, 26}
🙂

28. **SteveM**
April 22, 2012

There are often useful technological spin offs and side applications that arise from big science projects not necessarily directly connected with the science itself: for example, advances in computing, engineering, supercomputing, software engineering, data mining/analysis etc., developed for particle physics and genome research; indeed the WWW originated at CERN. There were many technological advances and spin offs and new industries in the 70s, arising from the Apollo moon program. Cancer has not been cured as such but there have been considerable strides in treating and managing it, in everything from nuclear medicine to new drugs. Science and engineering are also difficult and often progress slowly but they do progress because ultimately the scientific method works; and some things just have to be done on a large scale and require huge effort. Clara: Weinberg predicted weak neutral currents and the W, Z bosons and these things turned up just like he described them. Supersymmetry was (and is) a compelling and powerful idea, compatible with both particle physics and Einstein gravity–it made total sense to pursue it, along with superstring theory, even if ultimately they do not describe nature as it really is. The jury is still out on susy and evidence of it could still turn up–a scientific discovery as significant as finding life elsewhere in the universe. (Personal opinion.) One should not describe this great physicist as a "bitter old man" but I suspect he is expressing a certain amount of frustration in that he would really like to know certain things within his life time.

29. **Aidyan**
April 22, 2012

Clara: "If Weinberg had not created such a "lemming climate" in theoretical physics, we all would have had a much better chance to have his "final theory" already on the table."

Agreed. But I don't think we can make only Weinberg responsible for this. If few central figures like him could create a flock-like mentality among theoretical physicists it is only because they found a large number of them willing to behave like sheep in the first place.

30. **cormac**
April 22, 2012

Re "The observations of microwave radiation left over from the early universe have confirmed the general idea of an early era of inflation, but do not give detailed information about the physical processes involved in the expansion "
it seems a pity not to mention that we may currently be on the cusp of a breakthrough; E-mode polarization has already been detected in the CMB, and if the PLANCK

satellite resolves B-modes, that will surely go some way towards pointing us in the right direction

31. **Neto**
April 22, 2012

I'm not a physicist, and I don't want to get in a political talk, but the fact is that humanity (and governments) have limited resources. I think we are getting in a time where HE experiments are very expensive. It's not about being pro or against science, but we can't make all the Science at the same time. Of course, particle physicist – and myself, and I'm not a scientist – want to know the real truth about the nature, but the question should be, the billions and billions that would be used to build a new particle accelerator could be used in another science areas that could bring more sooner and direct benefits to humanity? I mean, we need to think like a business and make ROI analyses to decide what the priority is in science. We have some real problems getting closer, water polution, food to feed billions and billions of people, etc. We need to be reasonable.

32. **Peter Woit**
April 22, 2012

Neto,

Since humanity is not a business,there's no reason this question should be addressed by "thinking like a business". It may be that understanding the fundamental nature of the physical universe is not something that ever is going to make anyone a buck (other than popular science writers, and that's not a very well-paid profession…) or solve any social or environmental problem. Historically though, many human beings are interested in such understanding and society has devoted (a small fraction of its) resources to pursuing this understanding. To me, wanting the answers to such questions is a part of being human, and to decide we're no longer interested enough to put any effort into this search would be a change in human nature, not necessarily one for the better.

I think anyone proposing to change the historical fraction of society's resources devoted to this research needs to either claim that it was a mistake to do this in the past, or explain what has changed. Weinberg is arguing I think that since the problems have gotten harder, physicists and astronomers need even more than in the past. I'm not sure this argument is going to convince people, and the lack of a very scientifically compelling argument for a higher energy collider is a huge problem for HEP. On the other hand, those who argue for cutting HEP funding also need to explain what has changed. The GDP or major nations is as big as ever, it's just not true that all of a sudden they can't afford this small fraction of their resources.

33. **Neto**
April 22, 2012

Peter,

It's not about the humanity being a business, but about rational use of the resources.

I'm as curious about the our nature as you are, that's why I keep visiting your blog and a lot others even without being a physicist. Actually, I spend a lot of my time looking for answers about the big questions, and I really would like to see great improvements in my lifetime.

But, I live in a third world country, and maybe because of this I can see that there are some really urgent areas that we should be investing into now, not only enviromental ones, I don't wanna look like an enviromentalist, but health, energy, and so on.

I'm not favorable to cut any resource that is being used in HEP physics today, I have a moderated view, I just think that before making new huge investiments in the big questions now, we should look for some big problems first.

If I could choose, I would give up being alive when the big questions get answered by sooner improvements in peoples lives around the world, I wouldn't hesitate.

Maybe if my life was dependent of HE physics, if I was a physicist myself, my view would be different.

If what you're saying is that the next HE particle accelerator would consume relatively as much as LHC, then I think it's justifiable.

34. **Anon**
April 22, 2012

@Neto, what is so wrong with looking like an environmentalist? Indeed, if I had had the opportunity to answer that congressman, I would have said that a century or two from now only two things about our era are going to truly matter to our descendants. The first is how we treat the environment, and the second is what we have achieved in fundamental research. Unfortunately, experience has shown that our political system is not remotely able to address either of these issues, and that makes me deeply pessimistic about the future.

35. **Mike**
April 22, 2012

Sounds like a good time to get into molecular biology or bioinformatics – it doesn't require a lot of capital expenses (like large machines) and the results are applicable to the human condition (= more funding). That being said, there is a surplus of PhDs in the life sciences as well.

36. **Neto**
April 22, 2012

@Anon,

Nothing wrong about it. I just didn't want to look like a radical, maybe I was not clear.

I think that there are more things that will matter to our descendants. A more equal, more safe world, for example.

Fundamental answers are… Fundamental. But I don't think it's the only way to measure humanity evolution. At least not this time in history.

37. **Einstein_kid**
April 23, 2012

Hi everyone!
I'm new to posting but not to the blog itself. I feel this debate is a good one for me to start contributing because it does not deal with high energy physics details (of which I am not an expert) but with the public face of sciences in general and physics in particular, where I think I may have my 2 cents to share (I am indeed a physicist).
Discussing about public money calls for addressing public needs: the very first one among those, I believe, is accurate information. The public is curious: just look at all the "LHC will create a black hole" thing! And the public is reactive and powerful: take the mobilization behind the last servicing mission of the Hubble Space Telescope

in 2009. It might sound too simple and obvious but if there is no public engagement toward theoretical physics, there is not going to be much funding for it in the future, which to me means not only no HE-LHC or NASA satellites but also no students, no professors and no universities. "Engagement toward theoretical physics? are you crazy" no, I'm not: there is a lot of things that can be told (and done), starting from emphasizing the word "Hadron" in the LHC acronym, which goes hand-in-hand with hadron-therapy, a technique that might have cured some listeners' friends/relatives' tumor.
For these reasons I think that public engagement has to become a funding priority for every scientific project or collaboration. I like to think of this problem "quantum-mechanically": if there is no efficient scientific communication, your project's case does not exist; same holds true for the results already achieved by an experiment: if these results only live inside the ivory towers of universities and research institutions, they do not exist for the public.
I truly believe that the scientific community has to become more actively involved in scientific communication in order to stay alive.

38. **william c wesley**
April 23, 2012

Ahh….reality check time. We spend 10 times more on sports than on science. Think about that. Bet we spend more on pornography than on science, ditto fashion and entertainment . Our military budget is the largest expenditure in history and I'm sure what is spent on religion is incalculable, like what the Egyptians spent on pyramids. No one EVER questions these other expenditures, why? So lets see, how many economic spin offs have come from ALL these other expenditures combined….ahhh, near Z E R O!
Now how many industries and jobs have spun off from scientific investments? NEARLY ALL OF THEM. Most of us virtually owe our existence to scientific investment. People who don't think shouldn't control investment nor should they be given a platform on which to offer opinions about investment ether.

39. **Serge**
April 23, 2012

@Peter Woit
Couldn't TeV energy physics become practically relevant in unpleasant sense if something heavy hit nearby black hole in 1600 ly from us? Or more remote black hole which happen to point axis on us, or would point as result of collision? Does gamma ray burst modelling not require any new physics?

40. **Peter Woit**
April 23, 2012

Serge,

TeV energy scales are so high that there aren't known astrophysical objects that probe them. The things that we still don't understand about possible TeV scale physics are typically very weakly-interacting phenomena, such as the Higgs or possible WIMPs. Such phenomena would be responsible for a vanishingly small fraction of whatever potentially dangerous radiation would be coming from some nearby astrophysical object.

41. **Satan Claws**
April 23, 2012

"One day sir, you may tax it."

> Michael Faraday's reply to William Gladstone, then British Chancellor of the Exchequer (Minister of Finance), when asked of the **practical value of electricity** (1850), as quoted in "The Harvest of a Quiet Eye: A Selection of Scientific Quotations" (1977), p. 56

Can you use something like that?

42. **Zathras**
April 23, 2012

The issue with funding particle physics is that there is not much left to do. It's been slow for 40 years, and there is no indication that this is going to change. The next energy gap to predictions is just too big. The only argument for a bigger collider is maybe we will see something new, but selling predictions of null results will be very, very difficult. There is no point in even trying to sell a next generation collider unless the LHC finds something unexpected.

43. **science rules**
April 23, 2012

uair01, fundamental research can do more than put bread on your table. It already put the web on your hands, so that you can discuss here and do much more with it. This has changed the way we live and on top it was given to the world for free. There are countless examples of money badly spent by governments everywhere everyday. There are few enterprises more useful, rewarding and honorable than fundamental research.

44. **Another Peter**
April 23, 2012

It seems a bit strange to me to take as granted that the general public will constantly support fundamental research. We do what we do mainly because we are interested in it. Most of the time we don't think of applications and many regard questions of "applicability" as being close to insulting. Unless we teach, we are satisfying our curiousity using tax money. The general public will support this if it makes people **feel** great (or superior) and if they can afford the luxury to pay for this feeling. I think we have to understand that we are being lucky that we are paid at all (again, teaching excluded). Financially speaking, it is natural at a time of crisis to restructure your "investment portfolio" by shrinking the high-risk component (and gambling) and by getting rid of the "little indulgencies" that you may otherwise have.

45. **Peter Woit**
April 23, 2012

All,

Please, repetitive, tendentious argumentation not carrying any new information is not appreciated here. This is not Slashdot, which now has a new posting about this, so take such arguments there:

http://news.slashdot.org/story/12/04/23/1235202/the-crisis-of-government-funded-science

46. **vorpal**
April 23, 2012

It would seem to me that in the 21st century, China would be the appropriate place to build a next generation high energy exploration instrument. They are a big country, with lots of money and hightly motivated to establish themselves among the technologically elite nations.

The U.S. is a nation in decline, mostly because it's interests have become synonymous with the interests of large corporations. I just don't see a lot of corporate lobbyists pleading U.S. representatives for a new facility so they can get some supercollider contracts.

Besides, even as a theoretical physicist, I would rather see the money go to the NIH for basic research into aging, cancer, stem cells etc.

47. **Noah Smith**
April 23, 2012

Only tangentially related, but:
http://noahpinionblog.blogspot.com/2012/04/down-with-particle-physics-up-with-big.html

48. **Owen Patterson**
April 25, 2012

Does every thing that humanity delves into have to have some immediately tangible benefit? I can think of a lot of wasteful junk that people spend lots of money and time on that does little more than immediate, temporary pleasure and is even often harmful. Our "souls" need nourishment too, otherwise let's just go back to the caves and grunt around the fire.

As for ignorant lawyer-trained politicians – to heck with them.

49. **Art Brown**
April 25, 2012

Dear Physicists and Rocket Engineers,
In case you didn't notice, we won the cold war. Congrats and our deepest gratitude for your contributions: we got to the moon first, and while you didn't develop any bigger bombs since the '50s, you showed convincingly that the other side wouldn't either. As a bonus, the Standard Model looks quite serviceable for all our future conceivable needs.

So again, hurrah for you: we contemplated a ticker-tape parade, but after Feynman passed, there just wouldn't be much star power there, would there? Going forward, the obvious new threats are biological in nature, so we'll be directing our resources that way. We hope the graduated reduction in your funding gives you all a chance to adapt.
Best regards,
America

50. **markob**
April 26, 2012

Why spend the money searching for the ultimate laws of nature when, according to Weinberg and many others, that ultimate description of nature, string theory, has already been discovered out of considerations for beauty and elegance alone? Why not just wait until more accessible means of experimental verification of the ultimate theory become available? When mathematical elegance becomes the true arbiter of a theory it really does become that much harder for the HEP community to make the case for bigger and bigger machines. I think, also, Jeff Hughes had a book on the Manhattan Project and Big Science where he argued that science has slowly evolved away from "big science" and that big science is, in his words, "pathological science."

51. **arjun**
April 26, 2012

Given the current economic climate where India and China are growing rapidly, I wouldn't be surprised if India or China might end up building the next generation of colliders. Already India has pledged an estimated $250 million for the INO project:

http://en.wikipedia.org/wiki/India-based_Neutrino_Observatory

52. **Shantanu**
April 27, 2012

So Weinberg is not excited by non-0 evidence for theta_13?

53. **Craig**
April 27, 2012

It's all comes down to cost and (likely) benefit. I'm sorry, but there you have it. If a new generation of accelerators cost 19.95 plus shipping, and you could run them with a couple of grad students, then we'd do it. When you hit twelve billion dollars, we've got to have a serious conversation about making choices.

Dr. Woit gets it exactly right here–it's not quite clear what we would be doing with a big new collider. On the other hand, astronomy in general is working on a thousand fascinating problems, from dark matter to extrasolar planets. Frankly, I'd much rather have a big new telescope. I'd probably much rather have a _lot_ of different things. Let's sit down and make a list of scientific projects we could spend twelve billion dollars on. Now, show of hands: who gets the feel that some fantastic new advance is "just out of reach" of the LHC?

If all this means that a certain kind of high-energy physics has run its course, then that's what it means. This is not about the Seekers of Truth versus the philistines, and I'm tired of seeing arguments framed that way. It is about priorities, and making limited resourses count.

Something and Nothing

April 23, 2012
Categories: Multiverse Mania, Uncategorized

- In the something of interest category, last week at Columbia there was a panel discussion held as part of the World Leader's Forum, introduced by our president Lee Bollinger, on the topic What If We Find the Higgs Particle and What if We Don't.

  Columbia's Michael Tuts and Brian Greene gave an excellent discussion of the topic, to a large and attentive audience. Probably nothing new to readers of this blog, but I think they did a great job of it, and was interested to notice that Brian expressed skepticism about Kane's claims to derive the Higgs mass from string theory. Dennis Overbye of the New York Times seemed rather wary of hype about HEP, since he's a veteran of seeing the Times burned by this sort of thing. It's now been quite a while since they've made the mistake of putting up LHC headlines like Physicists Finally Find a Way to Test Superstring Theory.

  Maybe there's a better source for the video linked above, in the version I'm looking at, everyone is blue…

  In other "something" news, Brian's World Science Festival has just announced its schedule, available here.

- On the Krauss/Albert debate over nothingness front, yesterday there was a piece on the Huffington Post by Victor Stenger taking up the fight on Krauss's side. Over at Scientific American today, John Horgan comes into the ring on Albert's side.

  Like Horgan, I've recently got ahold of a copy of a pre-publication copy of a much more interesting take on the something/nothing business, Jim Holt's Why Does the World Exist?. I look forward to writing something about it here soon.

Update: Thanks to commenter Zathras for pointing to the latest punches returned by Krauss (see here):

> Well, I read a moronic philosopher who did a review of my book in the New York Times who somehow said that having particles and no particles is the same thing, and it's not.

Update: When checking out John Horgan's SciAm piece on this, don't miss the comment section, where he and Krauss are going at it.

Update: Via commenter Billy Hudson, Krauss's fighting words about philosophers and philosophy seem to have brought the philosophy community into the fight on Albert's side, see Massimo Pigliucci's latest.

Comments

1. **Zathras**
   April 23, 2012

   The Atlantic also has an extended interview with Krauss, touching upon the multiverse quite a bit:

   http://www.theatlantic.com/technology/archive/2012/04/has-physics-made-philosophy-and-religion-obsolete/256203/

2. **Marcus**
   April 23, 2012

   You may have meant "it's now been quite a while since"
   and mistyped "it's not been quite a while…"

   It's been a while since I thanked you. You do a great job in something that really matters. Thanks.

3. **Peter Woit**
   April 23, 2012

   Zathras,

   Thanks for pointing that out. Krauss responds to "pale, small, silly, nerdy", with "moronic". I added the link as an update to the main text.

   Marcus,

   Thanks! Fixed.

4. **jg**
   April 24, 2012

   the 'blue people' problem I think is a recent flash bug.

   if you have html5 enabled browser there are no visual problems.

   try opting in to the html5 trial at youtube and restarting your browser.

   http://www.youtube.com/html5

   (you have to select a link at the bottom of the page to opt in to the trial)

5. **CarlN**
   April 24, 2012

   Yes, this nothing business is a lot ado about nothing..as it should 🙂

   Regarding where reality ultimately comes from, we cannot base any explanation on anything that needs further explanation of course. In particular we cannot base any explanation on any eternal "things" as there would be questions on why this eternal thing (a law of physics or a god) have a particular form or particular properties (that would explain everything else) instead of other conceivable properties. Thus no real explanation can be obtained. Of course the concept of eternal things has other logical problems as well.

We need to start with absolute Nothing (no spacetime, no laws, no gods etc) as this point of departure does not need any explanation at all.

Fortunately this is simple 🙂

There are no laws “when” there is Nothing, hence there is no law of causation (“things” may happen without causation, Big Bang anyone?) , no laws of conservation (of energy,charge etc).

In short, “when” there is Nothing there exist no “things” (since a true Nothing contains no “things” like laws) that can prevent the creation of (self-consistent) “things” from Nothing.

So logically the “road” is totally open for creation from absolute Nothing. But only for things that are self-consistent of course.

CarlN

6. **Bourgeois Nerd**
April 24, 2012

I’m very much, and very impatiently, looking forward to Jim Holt’s book in the summer. Hope you say more about it soon!

7. **Sebastian Thaler**
April 24, 2012

The John Templeton Foundation is a “Founding Benefactor” of the World Science Festival? *choke*

8. **billy hudson**
April 25, 2012

Massimo Pigliucci has a post about the Kraus/Albert dust-up:
http://rationallyspeaking.blogspot.com/2012/04/lawrence-krauss-another-physicist-with.html

9. **Peter Woit**
April 25, 2012

Thanks billy,

I’ll add a link to the main posting. Looks like Krauss picked a fight with the entire philosophy community…

10. **JC**
April 25, 2012

This book is really nothing new. It is just a re-package of all these unsubstantiated craps (landscape, multiverse, … etc) under the new hype of “something from nothing”.

This generation of “particle” physicists is really pathetic. They might be good as second-class mathematicians (since really good ones could just work on hard-core mathematical problems). They achieve nothing comparable to the those of the great minds in the past. At the same time, they are trying very hard to make themselves look good and smart.

11. **abbyyorker**
April 25, 2012

I’m with the philosophers on this one, if only because I don’t like mediocrities running around calling people they disagree with morons. And the pointless “muscular atheism” of Dawkins et al disgusts me (though I am atheist myself). But I sure like Dawkins books!

12. **David Kordahl**
April 25, 2012

Has anyone else here actually read the works of David Z Albert? I read his books while a physics undergraduate, and I found them to be very worthwhile, especially “Quantum Mechanics and Experience,” which is a simple but cogent introduction to the measurement problem. Krauss’s behaviour of late seems to be embarrassing at best, and not quite the sort of thing that makes one proud to be either atheist or physicist. For Krauss to write a book of philosophical claims and yet to say that philosophers of science have nothing of interest to say to anyone but themselves is quite the whiplash display of specious logic. The desire to attack things as unimportant simply because one does not and does not wish to understand them seems to me as good a definition of a “nerd” (in the pejorative sense) as any, and as this squabble continues my respect for Albert has only increased–regardless of the Templeton thing.

13. **Dan D.**
April 25, 2012

As a theist (and scientist) myself, I’m heartened to see that not every atheist is against us on this point at least: namely that science does not get to determine what “progress” and “knowledge” means for every other field of inquiry, and that mere dismissals of entire fields of inquiry that have existed for thousands of years do not count as arguments.

14. **MathPhys**
April 26, 2012

As a theist (and a scientist) myself, I never understood what the fight is about. Anyone who has 1. solved an algebraic equation, and 2. fell in love, must realize that the fully-rational and the totally-irrational compartments of our brains coexist, perfectly peacefully, right next to each other, and that they often operate at the same time.

15. **hdz**
April 26, 2012

Let me quote a passage from the Kraus interview in The Atlantic on which you provide a link:

“Is there an empirical frontier for this? How do we observe a multiverse?

Krauss: Right. How do you tell that there's a multiverse if the rest of the universes are outside your causal horizon? It sounds like philosophy. At best. But imagine that we had a fundamental particle theory that explained why there are three generations of fundamental particles, and why the proton is two thousand times heavier than the electron, and why there are four forces of nature, etc. And it also predicted a period of inflation in the early universe, and it predicts everything that we see and you can follow it through the entire evolution of the early universe to see how we got here. Such a theory might, in addition to predicting everything we see, also predict a host of universes that we don't see. If we had such a theory, the accurate predictions it makes about what we can see would also make its predictions about what we can't see extremely likely. And so I could see empirical evidence internal to this universe validating the existence of a multiverse, even if we could never see it directly."

Well – as yet it requires some imagination to have such a theory that would convince us of the existence of these multiverses on empirical grounds.

But would it? Imagine further there were then a bunch of influential conservative physicists who insist that "… this theory is not made for the universe, but only a tool to calculate certain laboratory data." And that "we cannot say anything about physical reality but only about our knowledge."

This is in fact what permanently happens to Everett's interpretation, since here we do have a perfect and extremely well confirmed theory for more than 80 years on which his multiverse is built: the Schroedinger equation.

16. **PS**
April 26, 2012

JC: many of "this generation" of particle physicists are really very, very smart people. The question of why they haven't accomplished that much really has to be answered not by dismissing them all as second-class mathematicians, but by looking at the sociology of the field of string theory and particle physics. I think something really wrong has happened in the culture of this field. However, I can't quite put my finger on what and I don't know whether it'll correct itself if the LHC starts producing interesting data.

17. **cormac**
April 26, 2012

I don't understand all the hostility to the Krauss book. I found it a clear and enetertaining read, intorducing the public to a simple concept that has been known to physicists for many years; that a universe does not necessarily need a cause, according to quantum physics.
He does not say this is what happened, he simply explains the possibility with admirable clarity. I found both the review by Albert quite poor and rather biased; and Horgan doesn't really engage with the material at all

18. **Foster Boondoggle**
April 26, 2012

Albert has earned a certain amount of skepticism for his views on physics by lending his academic credentials to the cult pseudo-science of "What the Bleep…" a few years back. That might go some way towards accounting for the level of vitriol from Krauss. It's a bit harder to understand the tone of Albert's review of Krauss. Albert seems to be saying that you can never make a philosopher stop asking "why" at the end of a chain of explanation. But so what? Krauss' point is that, where 40 years ago we had no idea how the Big Bang got going (we couldn't go back before Weinberg's "First 3 Minutes"), now we have speculations (admittedly) that are at least grounded in models based on currently viable fundamental theories, and that explain the emergence of "everything" – i.e., the observable universe – from "nothing", i.e., the physicist's vacuum. Yeah, the philosopher can keep asking "why this, why that", but that seems ultimately sterile, and a losing battle for mindshare. (It's the "god-of-the-gaps" all over again.) More explanatory power is better than less.

19. **Bernhard**
April 26, 2012

@cormac
"I don't understand all the hostility to the Krauss book. I found it a clear and enetertaining read, intorducing the public to a simple concept that has been known to physicists for many years; that a universe does not necessarily need a cause, according to quantum physics"

The problem begins with Krauss and his unnecessary hostility towards philosophy. If that was Krauss´s only point he could probably have sold it to some people, but not satisfied with that, he began a silly debate about nothingness. He is even backed up by incredibly silly comments from Dawkins (who probably got more impressed then he should because he is himself not a physicist and because it fits his anti-religion agenda).

When Peter first said this debate over nothingness was silly I complained, but I regret, this debate is not only silly but pointless and the blame is on Krauss´s side. He wanted to make the headlines by basically saying physics has the answer to the philosophical nothingness question. When confronted with the problem of quantum laws themselves being something he used "brilliant" arguments like "if the 'nothing' of reality is full of stuff, then I'll go with that." Really, what is he talking about? He should have made an argument of physics and stayed there and not gotten into stuff he clearly demonstrates he is ignorant about (philosophy). He wrote a popular book, not a philosophical essay nor a physics article. That he wants to use it to attack philosophy is simply ridiculous.

In the end I think he is trying to promote the book by making this cretinous noise and is being successful.

20. **JC**
April 26, 2012

Here is the first sentence from the preface of the "nothing" book:

"In the interests of full disclosure right at the outset I must admit that I am not sympathetic to the conviction that creation requires a creator, which is at the basis of all of the world's religions. "

This arrogant clown claims that "a creator" is at the basis of ALL of the world's religions. First, take Papua New Guinea for example, there are roughly 840 indigenous languages. I guess probably there are more than 500 indigenous religions too. Did this clown check all of them before he made this claim ?
Not to mention that many branches of Buddhism do not proclaim the need of an omnipotent creator deity.

This is just another example that Krauss should just shut up about those things that he is ignorant.

21. **Aidyan**
April 27, 2012

As a theist (and scientist) myself, who found Dawkins, Hawking, Krauss et al. atheistic arguments always quite primitive and unreflective, I must however say that if the debate has fallen to these lows it is also because of the incompetence of the other side, that of the minority of theist scientists and philosophers who could not offer serious alternatives, or worst aligned with the new-age pseudo-science. Most of us are still stuck in even more untenable conceptions which refuse to give up anti-Darwinian semi-creationists biblical interpretations and more or less implicit anthropocentric views of our place in the Universe. Obviously, a viewpoint that is easy to disprove first and ridicule then. This forced theist intellectuals to silence especially in the last years (where are they?), and gave ample space to a 'scientistic' militant atheist movement which, once it believed it has won its crusade, now targets what it considers the last remanent of ‘time-wasters’: all philosophers, theists and atheists without distinctions. But I think that, if the former would once and forever accept Darwin, acknowledge homo sapiens as a species as others, reconsider the unwarranted ontological meaning assigned to categories as ‘chance’ and ‘randomness’ in biology and physics, and drop their religious dogmatic interpretations of reality, it would become easy to debunk the naive arguments of the latter.

22. **BDennehy**
April 27, 2012

I’m not sure if I speak for anyone else here but Peter could you please put a stop to yet more people mouthing off about their particular take on religion and spirituality!

23. **Peter Woit**
April 27, 2012

BDennehy,

I agree completely. Been planning to take action, but spent the morning bike-riding along the Hudson river, saw the Space Shuttle fly by three times on its way to landing at JFK.

All,

Please help keep this blog free of religion, both pro and anti, of any stripe. There’s plenty of other places to debate such issues, and rarely is such debate interesting.

Off to lunch, then some more updates to this posting…

24. **Allan Rosenberg**
April 27, 2012

I think Krause is being totally irresponsible in his attacks on philosophers. If you physicists aren’t careful, the philosophers of physics will go on strike, and then you’ll really be screwed.

25. **cormac**
April 27, 2012

Peter: I agree, and Bernhard also has a point. I think it was a great pity to have an endorsement from Dawkins at the end of the book, it introduced a completely unnecessary religious/anti-religious element to the book

26. **SpearMarktheSecond**
April 28, 2012

The great part about the forum is getting Michael Tuts, a real scientist and not a pettifogging fop, up on stage. He is worth 10 or 100 of the Krauses and the Greenes.

27. **OMF**
April 29, 2012

What an embarrassingly public waste of time and effort.

If I was a public representative, I can’t say I’d be inclined to fund an SSC or other projects after reading the likes of this.

28. **Bob Levine**
April 29, 2012

@OMF:

Sorry, but I cannot see the logic behind your comment. The SSC represented the best chance the world ever had to probe energy scales at a range likely to yield robust data that could have shed light on BSM physics; the chance for a real, empirically robust breakthrough was there, and was lost. The comments in this thread reflect pseudoscientific issue that reflect the lack of any new findings to drive responsible, creative theorizing—nothingness and multiverse studies are what we have, arguably, *because* the SSC wasn’t built.

There were some interesting sensory deprivation experiments in the sixties that showed that people deprive of all stimuli (total darkness, deadened sound, aqueous suspension etc.) start to hallucinate after being left in that state long enough. What we’re see now, on a variety of ‘not even wrong’ fronts, seems like a different version of the same thing. Canceling projects designed to gather new data is a good chunk of what lead us to this state—not the whole story, certainly. Given the problem, how would your prescription be a constructive response?

29. **Peter Woit**
April 29, 2012

Bob Levine,

The standard argument for tolerating multiverse/pre-big-bang nonsense by prominent theorists is that it may not be science, but it gets the public interested in and excited about science, and hopefully they’ll then go on to the real thing. The problem is that if the nonsense goes too far, people will realize this and the field will get discredited. I think OMF is pointing to that danger. I have no idea how serious it really is, but physicists in general might want to think about the desirability of speaking up against nonsense promoted by their colleagues. There are now a lot of prominent physicists out there spouting nonsense about the multiverse, very few speaking up with a more sensible viewpoint.

30. **OMF**
April 29, 2012

> The problem is that if the nonsense goes too far, people will realize this and the field will get discredited. I think OMF is pointing to that danger.

I think this danger has long since been realised. Frankly, given the state of the field, I don’t think big physics projects should get any funding, or at least, not before big astronomy, or chemical, or engineering projects, etc.

I wouldn’t give funding to a field whose top representatives spend most of their time in cat fights with creationists, and giving interviews for CGI-ridden cable documentaries. It might sound harsh, but giving these people funding is actually part of the problem.

31. **Peter Woit**
April 29, 2012

OMF,

This might be a good argument for defunding some subfields of theoretical physics, but the experimentalists building and using tools like the LHC really have nothing at all to do with the multiverse-maniacs. If you take away funding for things like the LHC, you won't affect multiverse-mania, since those engaged in it aren't funded from there. Worse, if you got rid of LHC funding, you'd get rid of the people in the field doing actual science, leaving only the pseudo-science in place.

32. **OMF**
April 29, 2012

In that case, I think it is time for experimental(and other) physicists to begin disassociating their fields from the practices exemplified by this particular scandal.

33. **srp**
May 2, 2012

The pity is that Krauss could have been almost as provocative without overreaching. Proposed re-titling of his book: "You Ain't Seen Nothin' Yet."

The argument is that modern physics makes the constraints on "nothing" a lot more stringent than the casual thinker (and possibly the uninformed philosopher) realize. In the interview, Krauss claims to go beyond saying that empty space in our universe is unstable and will spawn particles and such. He claims to show that space and time themselves will emerge from quantum fields, so that a no-space, no-time, no-stuff field is unstable, too.

So Albert is correct–you can't stop asking "where did that come from?" because the fields' existence is taken as given–but Krauss has a point (assuming he's got the physics right) that logically banning creation ex nihilo requires a much more nihilistic nihilo than it used to.

Much Ado About Nothing

April 27, 2012
Categories: Favorite Old Posts, Multiverse Mania

I suppose I'm posting too much about this, but the ongoing fight over nothing between prominent physicists and philosophers strikes me as perhaps marking some kind of end-point in the multiverse-mania-driven decline of part of theoretical physics from a difficult, serious subject to a trivial and kind of ludicrous undertaking. How can you get any sillier than arguing over nothing? Will this be the end of it, or is there somewhere lower to go that I can't yet imagine? There's also a *Three Stooges* sort of entertainment value to following this fighting. It's kind of like a segment of Dumb (a multiverse explains everything!) vs. Dumber (bringing religion into it, "pale, small, silly, nerdy").

If you haven't been following the story so far, to start see links here, here, and here. Lots of gems there, one I just noticed is the moderated discussion at the Templeton-funded "Philosophy of Cosmology" blog, where the proprietor writes that:

> Krauss is a crybaby.

and then goes on to complain that Krauss hasn't taken him up on his request that he explain himself at the Templeton blog.

In this morning's developments, we have prominent skeptic Michael Shermer, in Much Ado About Nothing, making the case that the Multiverse finishes off that "God" business, using "multiverse hypotheses predicted from mathematics and physics". His authority here is the Hawking/Mlodinow popular book, but he's also convinced that WMAP and LIGO are somehow going to provide evidence for multiverses, something that even the most far-out theorists in this field aren't claiming. In addition:

> Maybe gravity is such a relatively weak force (compared with electromagnetism and the nuclear forces) because some of it "leaks" out to other universes.

Nobody seems to have told Shermer that this is not an idea taken seriously by a significant number of theorists, or that LHC data has shot down the hopes of the one or two such theorists.

Also this morning, with The Consolation of Philosophy, Krauss tries to extract himself from the trouble he got himself into with philosophers with his recent comments about them and their profession. He sticks to his criticism that it's physicists who have interesting things to say about fundamental issues of physics, not philosophers, but admits that at least they're not as bad as theologians:

> To be fair, I regret sometimes lumping all philosophers in with theologians because theology, aside from those parts that involve true historical or linguistic scholarship, is not credible field of modern scholarship.

Will now go get some popcorn to await further episodes of this comedy…

Update: Two more links. Sean Carroll has a long posting about this, with bottom line that he thinks Krauss is right, but shouldn't have said mean things about philosophers. David Albert responds to being called "moronic" by accusing Krauss (whose name he has trouble spelling) of being incompetent:

> …the business of pontificating about why there is something rather than nothing without bothering to get crucial pieces of the physics right, or to think about them carefully, or to present them honestly, strikes me as something of a scandal.

Update: Brian Leiter, at the well-known philosophy blog Leiter Reports, joins the fight, of course on the philosopher's side. In response to the Krauss attack on philosophers in general, he has this to say about physicists:

> Of course, it was not always so with physicists, but the current generation (at least those who try to speak to the broader public) does seem remarkably inept in logical and rational thought, and unembarrassed to display that to the world. Which raises the question: why? My best guess is that the culture so celebrates physics, that physicists have come to believe the "PR" about them.

Update: The fist-fight between Krauss and the philosophers continues in various venues. Surprisingly, today Leiter's blog has a philosopher (Justin Fisher) throwing punches on Krauss's side:

> …Albert is clearly just being snide for the sake of being snide.
>
> So Albert published a review that was needlessly uncharitable and snide, berating a good work in popularizing science for not solving philosophical puzzles that it openly acknowledges it doesn't solve. Albert was a jerk and then (as we all know) Krauss was a jerk back. It's all very entertaining drama. But why have you picked sides?
>
> My own view is that Albert's review was an embarrassment to our profession, and a setback for all philosophers of science who want our work to be taken seriously by scientists. When a prominent philosopher publishes a careless snide review like this – and in the NYT, no less! – it should be no surprise that many scientists react as Krauss did, by suspecting that philosophers generally behave as Albert did in this review: shedding much noise and little light. And, you're not helping when you, as a prominent philosophical opinion-shaper, uncritically take Albert's side. So I urge you to consider at least staking a more moderate stance, if not actively admonishing Albert for publishing a pointlessly snide review that reflected poorly on all of us.

Comments

1. **Deane**
   April 27, 2012

   I also liked "if God were to have existed she would have sent Lawrence Krauss to earth to give atheism a bad name"

2. **Thingumbob**
   April 27, 2012

   "Will this be the end of it, or is there somewhere lower to go that I can't yet imagine?"

   I think the next step in this Plutonic dialog about nothing must needs be whether there will be a big end of nothing or a little end of nothing.

3. **Don Murphy**
   April 27, 2012

   Peter Wrote:

Will now go get some popcorn to await further episodes of this comedy…

Don says:
Let's not give comedy a bad name as well.

4. **Bob Levine**
April 27, 2012

Krauss' problems with the philosophers seem to be fairly straightforward. He's written a popular science book taking some well-established ideas from QFT,about the vacuum state and the sources of vacuum fluctuations, which have some very speculative applications to quantum cosmology, and (in my view, somewhat disingenuously) titled the book so that it appears to have a bearing on the hoary (and possibly incoherent) metaphysical question of what motivates existence, as vs . nonexistence, thereby tapping into people's curiosity about Ultimate Things. The philosophers have been pointing out that what he's writing about has nothing to do with the latter point, which jeopardizes some of the marketing appeal of the book. So LK is pissed off, and responses with abusive nonsequiturs. He's close to admitted as much in the interview, in the parts that Massimo Pigliucci discusses at

http://rationallyspeaking.blogspot.com/2012/04/lawrence-krauss-another-physicist-with.html

Follow the (scent of) money, and you won't go far wrong, sums it up, I think.

5. **JC**
April 27, 2012

I think much more rectifying work has to be done by people like Peter. It is difficult for laypeople to understand that the "nothingness" of quantum vacuum is not really nothing at all.

I guess because of this, Krauss might continue to have some success in selling his crap and fooling the general populace.

6. **Peter Woit**
April 27, 2012

JC,

You really mistake my point of view on this. I've nothing against Krauss's definition of nothing. The problem here isn't the general populace getting fooled, since they almost all have enough common sense to see that there's nothing interesting at stake in this argument.

With the entertainment value a positive, the only negatives I see here are skeptics like Shermer discrediting their whole endeavor by swallowing whole nonsense they've read from Multiverse-maniacs, and physicists and philosophers turning their profession into a laughing-stock.

7. **Julian English**
April 27, 2012

I'm not sure if Krauss ever did good physics but I'm sure he doesn't have time to do any of it now; he's too busy promoting his book and being a public intellectual. Pop science is valuable but should be written by seasoned grad students unable to get tenure track jobs, not professors paid to do research.

8. **Peter Woit**
April 27, 2012

Julian English,

I'm not sure that the problem of tenured researchers not having time for research since they have taken up public debate is any larger than that of tenured researchers not having time for research since they have, say, taken up yoga, a new love affair, or decided to renovate their basement. The problem is what they do in this public debate, which may be valuable, neutral or damaging.

9. **Bernhard**
April 27, 2012

Now let´s hope some theologians get in the circus and take offense so this gets even more laughable.

10. **CWJ**
April 27, 2012

I wonder if Krauss could even name a modern theologian, such as Borg or Moltmann.

11. **Nex**
April 27, 2012

I agree its time to end multiverse-mania, we should instead focus on a more general and beautiful entity – hyperverse – a grand collection which besides all the mundane members of the multiverse (ie universes with different physical laws, or realizing different branches of wavefunction, or causally separated, etc) also contains all the really exotic universes like those to which no laws of physics can be applied at all (every hypothetical law one might propose in such an exotic universe has infinitely many exceptions violating it) and those in which mathematics is completely useless as a description of reality (whether those two classes are separate is the fist important question that needs to be answered).

12. **Sean the Mystic**
April 27, 2012

Don't you think New Atheist ideologues like Shermer and Krauss have become as hubristic as many theologians? The idea that the latest fashionable theories of cosmology and physics have *just now* rendered age-old questions about the cosmos null and void seems rather presumptuous, does it not? Where is humility and skepticism? Aren't these so-called skeptics trying a little too hard to find "just so" stories from the most speculative areas of modern science to discredit their theistic foes? I'm really struck by the ideologically-charged nature of science these days, and wonder how intellectual integrity will be maintained if science is reduced to a bludgeon in a war of worldviews.

13. **Dan Winslow**
April 27, 2012

Nex – Not to mention the hyperverse universe in which multiverses (multiversi?) are impossible….

14. **paddy**
April 27, 2012

Peter: Unfortunately, many*–even here–disappoint in not seeing the humor. This is funny in and of itself.

*exception for Don Murphy to whom I might paraphrasically whisper: "Apparently physics has become easy while comedy yet remains hard."

15. **Friend**
April 27, 2012

It might be that this argument about nothing is being fuled by equivocation of terms. What is nothing? Even according to GR the Big Bang started from a singularity, a single point. Yet, what dynamics can occur in a single point? None! For all practical purposes a singularity is equal to nothing. And we're all talking about the same thing.

16. **Avattoir**
April 27, 2012

Julian English: "I'm not sure if Krauss ever did good physics …"

Do you write that after or before having reviewing the first 101 publications on this list?

http://genesis1.asu.edu/Publications.htm

I'm not judging; I have no idea whether "Krauss ever did good physics". But he sure seems to have had lots & lots of peer-reviewed articles published in credible journals that are associated with good physics standards. I suspect our host tried to express a view in his response, but it's not exactly direct.

I think our host was a bit unfair on Schermer. Yes, that bit about gravity leakage is cringe-worthy; but it only pops up in the context of his attempt at a sketchy sketch on various ideas in the area, & in any event doesn't, as our host more than merely implies, purport to squash theology with the multiverse stick. Instead, Schermer consistently uses phrases like "may have been" & "may be just" in referring to the multiverse; nor does he stray from that standard on any of the other ideas; nor even does any part of his sketchy sketch detract from his point:

"… why turn to the supernatural when our understanding of the natural is still in its incipient stages? We would be wise to heed this skeptical principle: before you say something is out of this world, first make sure that it is not in this world."

Also, it seems to me Krauss is learning that semantics is a tricky game to play with semanticists.

17. **Peter Woit**
April 27, 2012

Avattoir,

Krauss is a perfectly respectable theorist. I don't think his research has led to any dramatic progress in recent years, but then again this is true for virtually everyone in this business. My dismay about him and Schermer is based on my belief that they're both smart, well-informed, and not easily bamboozled people who should know better than to get involved with multiverse-mania. They're careful to add lots of "maybe" caveats, but they should realize that people tend to not hear those, and that in any case, better to steer clear of dubious speculation than to engage in it while trying to protect oneself with weasel-word clauses.

18. **Neto**
April 27, 2012

To me it's very clear, you just have to look who is in Krauss side. Some people just don't care about religion, some feel they have to show religion is wrong at all costs, and will eventually give up good skepticism just to please their egos. Krauss, Dawkings and Shermer are really insterested in debunk religion, and debunk now. But they need some possible explanations to our reality that we don't have at this time. So they will accept a lot more easily some dubious possibilities because, you know, they are just like every other animal and they need to satisfy their egos. We do that all the time. Maybe we're more moderated in this subject because we don't really care about religion, and we can keep our skepticism while we wait for some new evidences.

19. **Anonyrat**
April 27, 2012

People grinding their ideological axes with highly speculative theories that superficially resemble physics – and some respectable physicists are aiding them, (inadvertently?) – either weep or break out the popcorn.

20. **Lee Smolin**
April 27, 2012

Dear Peter,

Krauss's hostility to philosophy is hardly new, it is the perspective within which our generation of physicists was educated. It is the defining polemic of a tradition of pragmatically oriented theoretical physicists whose dominance began with the generation of Feynman and Dyson. They took over leadership of science from the previous generation, who were heavily influenced by, and respectful of the tradition of philosophy, led by Einstein, Poincare, Bohr etc. The transition was marked by Dyson who noted that he was part of a generation of young conservatives who took over from a generation of old revolutionaries, who were seen as having exhausted themselves on fruitless philosophical quests such as the interpretation of quantum theory.

The triumph of that generation was quantum field theory and it is certainly ironic that the exhaustion of their pragmatic style is marked by the over-extension of quantum field theory to address questions it can't possibly answer such as the origin of the universe. Only someone whose understanding of the world comes entirely from within the pragmatic tradition could imagine that the description of a phase transition from one quantum field theoretic vacuum to another-both existing in a background spacetime-addresses the metaphysical-and probably unsolvable-question of why something exists rather than nothing.

And sadly, among the things you throw out when you discard philosophy is the work of Popper and others who established the demarcation between science and metaphysical nonsense.

I believe that the pendulum is swinging back because many of us have learned that an engagement with philosophy does greatly aid a serious assault on the key questions physics faces such as quantum gravity, the foundations of quantum theory and questions as to the choice of laws and cosmological initial conditions. Krauss wouldn't know it because he hangs out in the wrong circles, but there is a healthy dialogue between physicists and philosophers about these issues, which has greatly stimulated both sides. This engagement has been fruitful to a large extent because of a new generation of philosophers, such as David Albert, who are very well educated in physics. And as we see from Albert's review, they can certainly hold up their side of a debate because they understand contemporary physics far better than Krauss understands contemporary philosophy.

It is also disturbing to see that this fake issue is taken as somehow proxy for the conflict between science and religion. For one thing, it is hardly the case that Albert or other contemporary philosophers of physics defend the claims of religion or the medieval arguments about the why something rather than nothing query. Quite the opposite, they tend to employ the same piercing logic Albert used to puncture Krauss's arguments to destroy the claims of theologians and metaphysicians. Among the several ironies of this story is that the people Krauss is attacking are able to more powerful arguments against the claims of religion because they have a sophisticated education in the history of science and philosophy. To see how effectively a philosophically sophisticated physicist with a good classical education can make a case against religion that goes far deeper than Krauss's into the historical roots of the conflict with science, you might look at Carlo Rovelli's The First Scientist: Anaximander and his Legacy.

21. **Visitor**
April 27, 2012

Well I am not a scientist or mathematician, but I was struck by Krauss' claim that "science is meant to make people uncomfortable" because I have never heard this particular claim before. Being, it seems, very naive, *I* would have thought that, very loosely speaking, "science is meant" to make falsifiable statements describing observable facts. His statement is not an uncommon one, though, among attention seekers who think it excuses any kind of public or intellectual dishonesty. To me, it looks very much as though Krauss is saying that being a scientist gives him a license to lie. (Which is out-and-out Schneiderism but that's another subject.)

The antics of the current crop of loudmouth atheists brings to mind some of the more depressing and bleak sections of Ortega y Gasset's "The Revolt Of The Masses" because, in spite of their academic credentials, these people seem to be profoundly ignorant, and such people – very highly educated in certain narrow areas and completely lacking any knowledge of other areas – occupied a significant amount of Ortega's reflections.

22. **Roger**
April 27, 2012

Part of the confusion here is to equate nothing = aether = quantum vacuum state. Krauss's title says "Why There Is Something Rather Than Nothing", but there are always complicated quantum states present. He cannot say what happens when there is truly nothing.

23. **Peter Woit**
April 27, 2012

Hi Lee,

Thanks for writing. I agree that Krauss's broad-brush dismissal of philosophy is unfortunate. When Feynman, Gell-Mann and others were making great strides in understanding particle theory, one could understand their hard-nosed lack of interest in philosophy. These days I don't think theorists can afford to be so arrogant, since their track record of the last few decades is rather poor. Perhaps they need some help from another quarter.

I don't however think Albert's review was an impressive example of the power of philosophy. That there's other "nothings" than the vacuum state is rather obvious and didn't require a long bombastic argument. The "nerdy" business seemed to me just unprofessional and embarrassing. It came off as, well, something Feynman or Gell-Mann might pull.

If "philosophy of cosmology" would help keep straight the difference between science and metaphysical nonsense, that would be great. I haven't seen much of this though, and being funded by an organization devoted to blurring that line isn't really confidence inspiring.

24. **Anna**
April 28, 2012

Has science ever gained any insights from the musings of philosophy? The whole'nothing' debate is reminiscent of the 'how many angels on the head of a pin' discussions. If it is not measurable, who cares? Personally, I think that if the LHC confirms the Higgs and finds no hints of new physics, we will be left in a very uncomfortable place – two mutually inconsistant theories (QFT and GR), a swathe of apparently arbitrary parameters, some unknown symmetry breaking potential (the Higgs) … at least a null result or new physics would mean we need to rethink our models.

25. **Bugsy**
April 28, 2012

The easiest (and laziest) approach to something you haven't really studied is to belittle it. I see this tendency in myself concerning mathematicians- an analyst may privately
think set theory or category theory is a waste of time, a probabilist may feel that way about algebraic geometry and vice versa. Observing these tendencies in myself, the only thing I know is that when I really look into a new area, I am invariably surprised to find
something really interesting there. Don't know much about philosophy, for instance, but
I found Carlo-Rota's musings in his book "Indisrete Thoughts" very readable and interesting. Philosophy should not be mistaken for physics, however, nor vice-versa. For one thing, the general erudition and ability to use the language to express thoughts is expected to be higher in the humanities.

Where we get into trouble is when people in any area are motivated by personal greed, whether for money, power or fame, to over-hype low-quality work, and when intellectually lazy and arrogant people criticize what they do not want to try to understand.

On a lighter note, following Nex, I suggest the multiverse be expanded to something called the merriverse, which would bring the poets and real comedians (not unintentional ones) into the fray…

26. **theoreticalminimum**
April 28, 2012

Peter,
Have you read the latest news about *l'affaire Bogdanov*? Probably something you'd like to write about in a post?

27. **Lee Smolin**
April 28, 2012

Dear Peter,

I am normally a big fan of Krauss's writing defending science and I should say I haven't read the book. But if he said what Albert indicated he said then this time he over-reached.

The reason to prefer science to religion is not that science offers the better story to explain enigmas like why the world exists or consciousness. To the contrary, science offers nothing to compete with the certainties of religious dogmas on such questions. The reason is that science has a far higher standard for belief and this standard results in knowledge that is limited in scope and always provisional. But it is the best knowledge we can have, if by knowledge we mean provisional understanding that can be established and defended by rational argument from public evidence. Therefor it is a mistake to compete with religion on explaining how or why the universe began because it is a fight we can only contest by giving up the methodologies and standards to which we owe our entire success.

Anna asks for examples of contributions from philosophy to progress in physics. Here are a few that come quickly to mind:

-Leibniz's thinking on the need for space and time to be relational, a consequence of his principle of sufficient reason, inspired Mach to invent Mach's principle, which in turn inspired Einstein in his invention of general relativity.

-'t Hoofts work on foundational problems in quantum theory led to his postulation of the holographic principle which led to AdS/CFT.

-Turing and Godel's work on foundations of mathematics and logic directly inspired von Neumann and his colleagues in his invention of the standard architecture of computers. (as detailed in George Dyson's recent book.)

-David Deutch's thoughts on foundational problems in mathematics led him to invent quantum computation. The field of quantum information remains a lively point of interchange among physicists, computer scientists, and philosophers.

-Julian Barbour's extensions of Leibniz's and Mach's critiques led him to a deeper understanding of the role of active diffeomorphisms as the gauge symmetry of general relativity. This was a formative influence on the invention of background independent models of quantum gravity including loop quantum gravity. This also led him and younger colleagues to a reformulation of general relativity called shape dynamics, which in turn appears to explain the AdS/CFT correspondence as a property of general relativity.

-The best work on the many worlds interpretation of quantum theory is by philosophers of physics in Oxford, Simon Saunders, Hillary Greaves and David Wallace. I must say I am not convinced, for reasons best argued by David Albert, but they made a far better case for the cogency of the interpretation than had been given by physicists.

28. **Emanuel Derman**
April 28, 2012

I was present at a cosmology seminar at Columbia in the late Sixties in Pupin when Ed Tryon http://en.wikipedia.org/wiki/Edward_Tryon, in the audience, an assistant prof there, asked the speaker whether the universe might be a vacuum fluctuation. IT sounded like a joke at the time. He eventually published his idea as a paper: http://www.nature.com/nature/journal/v246/n5433/abs/246396a0.html. He once years later asked me to corroborate that I heard him ask that question, and I did.
—–
Science/physics is about studying details and deducing generalities: "To see the world in a grain of sand." I'm not a practicing anything, but I'm always struck by how narrowly Krauss and some of the new atheists interpret "the world."

29. **Jeff M**
April 28, 2012

Nice to see that Lee Smolin still thinks like a Hampshire student 🙂 Also nice to know that all those times Herb Bernstein told me "you'll never be as good as Lee if you don't get your act together" he knew what he was saying. Of course I decided I liked the math better anyway…
. I certainly agree that physics has no business discussing origins, since it just cheapens everything. My own intuition is that fundamentally the issue is "Godelian," in that I think there are things you can't say about a system unless you go outside that system, which in this case of course we can't do. From my experience there are very few philosophers who discuss origins for pretty much exactly this reason. You would think obviously intelligent people might realize how stupid they look when they're arguing about the existence of god or the concept of "nothing."

Then again, you can get some great jokes out of it. My favorite will always be from the Hitchhiker's Guide to the Galaxy, when Adams mentions Oolong Kaluphid's latest blockbuster, "Well That About Wraps it up for God." 🙂

30. **Peter Shor**
April 28, 2012

Peter: I believe Feynman was actually quite interested in philosophy. His work on the physics of computation owes something to philosophy in its nature, and he thought very hard about getting around Bell's proof of non-locality by using negative probabilities (an effort which ultimately was unsuccessful; I heard a talk he gave about it while I was at Caltech around 1980—see his article in *Quantum Implications: Essays in Honour of David Bohm*). There is a famous quote where he advises students not to pursue the philosophy of quantum mechanics—I think this was (and is) excellent advice, but I also think that it should not be taken to reflect his own opinion of the subject.

31. **Anonyrat**
April 28, 2012

I don't think Feynman would have any better opinion of some modern what-passes–for-physics than he had of some what-passes-for-philosophy.

32. **Peter Woit**
April 28, 2012

theoreticalminimum,

Thanks I hadn't seen that. Maybe I'll write about it here if I can learn more about what has been going on. Seems very odd that someone was taken to court for publicizing what I would have assumed was a public document (Igor Bogdanoff's thesis).

Given the recent trend towards nothingness/pre-big-bang/God studies in theoretical physics, perhaps the Bogdanovs should be recognized as true visionaries. They were pioneers in this field more than a decade ago. Their "zero-point" origin of the universe is true nothingness beyond Krauss's feeble attempt at nothingness, and might even satisfy Albert as really being nothing.

33. **pah**
April 28, 2012

It's weird… I am a theoretical particle physicist, and I never hear about studies on nothingness, pre-Big-Bang, multiverse etc, except through blogs such as this one. Guess I'm too busy working on LHC physics. Readers of this blog must have a pretty skewed idea of what people are working on in the field.

34. **Peter Woit**
April 28, 2012

pah,

You must also not watch TV (probably a good idea), not frequent bookstores (increasingly easy as they all go out of business), and avoid a segment of the hep-th arXiv. It's a valid point though that the high public profile of multiverse-mania does not at all correspond to the fact that most physicists think it is best ignored and act accordingly.

35. **Giotis**
April 28, 2012

"I am a theoretical particle physicist, and I never hear about studies on nothingness, pre-Big-Bang, multiverse etc, except through blogs such as this one. Guess I'm too busy working on LHC physics."

Good for you but not all people have to roll in the mud of the Standard model and of LHC physics. There are other issues of high theoretical importance which need to be explored. The fact that you are ignorant about these topics doesn't give you the right to belittle the people who are working on these fields.

36. **Peter Woit**
April 28, 2012

Giotis,

I find it wonderful that sometimes I can't tell whether you're serious or parodying a point of view…

37. **Mitchell Porter**
April 29, 2012

Nothing should be left to chance in this discussion. We can take nothing for granted.

38. **rshaw**
April 29, 2012

This entire discussion is wholly vacuous.

39. **pah**
April 29, 2012

Giotis, I meant no disrespect to anyone. There are indeed issues of high theoretical importance. However, I believe the only way to address them is by raking through the mud.

40. **Sebastian Thaler**
April 29, 2012

Lubos Motl has just chimed in as well on his blog.

41. **Marcus**
April 29, 2012

The logician R. Smullyan has considered the question "Which is better: eternal happiness or a ham sandwich?" and has argued as follows.
*
Well, nothing is better than eternal happiness.
And a ham sandwich is better than nothing.
Therefore a ham sandwich is better than eternal happiness.

42. **Peter Woit**
April 29, 2012

Sebastian,

In the Krauss/Albert case the insults flying everywhere are better than Lubos can muster (best he can do is describe Krauss as a "jerk"). He does however give an accurate account of a recent preprint by Lenny Susskind, the father of string theory:

"It's a philosophical ideology with a few equations remotely resembling elementary science that is being promoted by folks who found out that they feel very happy if they're sloppy and if they have a big mouth. Some of them are on crack."

43. **Thomas**
April 29, 2012

For those who can understand french (can't find an english version, sorry), here is the link to Carlo Rovelli's talk.

44. **The Lone Haranguer**
April 30, 2012

*Krauss (whose name he has trouble spelling) *

Maybe he's confusing him with someone else:
http://www.hellshaw.com/flann/deselby.html

45. **Marcus**
April 30, 2012

Thomas, in Lee's 8PM post on 27 April he mentioned this new book:
"… you might look at Carlo Rovelli's The First Scientist: Anaximander and his Legacy."

And then you kindly posted a link which according to the title is to a talk by Rovelli on Anaximander and the roots of the scientific tradition (in French).

However this link may contain an error. When I try it I get a lecture by a French astrophysicist named Kunth about some distant galaxies which he has observed.
Is the link to a two-part lecture, with the second half by Rovelli? I tried, but was unable, to fast-forward.

Rovelli gave a talk last week at Princeton about Anaximander and the scientific tradition–seminar in the Philosophy department organized by Halvorson, but AFAIK there is no recording online.

46. **Peter Woit**
April 30, 2012

Marcus,

The link works correctly for me, giving Rovelli's talk. Check to see that you're correctly following the link.

47. **Allan Rosenberg**
April 30, 2012

Before we get too comfortable with the idea of there being something rather than nothing, don't we need to define nothing more precisely? For example, can we say that nothing could have had an actual existence in its own right, or would nothing have been defined only relationally, as the absence of everything other than nothing? Had there been nothing, would there have been just a single nothing, or might we have had an entire multinullity in which–perhaps–every thing that could have existed would have had an independent nonexistence? As much as I wish I could come out on Krauss's side in this, I think philosphers are much better equipped to explore these kinds of empty questions than are scientists.

48. **paddy**
April 30, 2012

Allan R.: Yup, philosophers are much better equipped to deal with questions which are meaningless in science/physics.

49. **Thomas**
April 30, 2012

Marcus, Indeed Kunth rambled on for ages before Rovelli – I should have mentioned that.

I thought that was a deep and fascinating talk about a lesser known paradigm shift, the earth/sky vs up/down paradigm shift.

But particularly enjoyable is the argument Rovelli makes, that this paradigm shift was also some kind of a meta-paradigm shift: the birth of the "constructive criticism" that is essential to science. Brilliant.

I would like to put two questions, then, to this sophisticated readership (sorry if this brings us somewhat out of the Krauss thing):

1. is there a learnable method to accelerate the findings of paradigm shifts? (not just more budget)
2. is there a meta-paradigm shift that will be to science what science was to previous modes of thoughts? (found by a new Anaximander, in a move that would superseed science itself?)

50. **Neto**
April 30, 2012

@paddy

Actually, I could argue that physicists are much better to given the "meaningless" status to a question they can't answer.

Every question that is correctly formulated is meaningful, the question "why there's something rather than nothing" is perfectly meaningful, and it can have a lot of possible answers, between then the simplest "just because".

In fact, all this aggression between Krauss and philosophers is very naive, silly. Even this kind of post. As I'm not a physicist myself and have no reputation to lose, I can give me the please of commenting.

Of course philosophy is very important to our understanding and lead our thinking to new frontiers. Just because it sometimes can discuss topics that we cannot answer, or even give the wrong direction, it don't make it less useful. Indeed, that poor philosofical basis that some physicists may have makes them not even understand philosophical assumptions and propositions that themself made, what is embarassing.

I think the concept of nothing that sometimes philosophers are refering to is very conceivable and even is part of current models. As far as we know, this universe is expanding from a point the size of an atom to a finit size that it have now. What is inside this universe, despite being "something" (matter, stuff), is the Krauss' nothing, that is, vacuum full of properties and interactions between photons and so on. What is outside the boundaries of this finit universe is – possibly – the philosophers' nothing, the "meaningless no-existence". So, the meaningless no-existence is perfectly conceivable with our current theories, there's no reason to think the only no-existence that can exist is the Krauss' one.

Even this "existence of no-existence" talk is sometimes tricky and people want to use it to fuzzy the discution. But the questions still, and maybe, just maybe, sometime in the future we may have answers that will address them a lot better than now. But this questions should work as motivators, not as insult.

People like Krauss should note that being doubtful and intellectually honest won't make them looks any dumber, but the opposite. The most respectable and smart people that I know have no fear to say they don't have a clue about anything they don't know. It's all about intellectual honesty.

51. **Anonyrat**
April 30, 2012

"Every question that is correctly formulated is meaningful…"

The test of a question being correctly formulated is, is it meaningful?

52. **Aidyan**
May 1, 2012

"As much as I wish I could come out on Krauss's side in this, I think philosphers are much better equipped to explore these kinds of empty questions than are scientists."

Empty questions because the simple attempt to grasp 'nothingness' amounts to objectification. But objectifying is a cognitive act that refers always more or less implicitly to 'existence', and the very notion of 'existence' is rooted in some form of experiential conscious state of 'being', and which is 'something', leading to contradiction. Therefore the whole argument becomes circular and undecidable, at least for human mind. Taking second quantization's ground state levels or any definitions of quantum vacuum state (or 'quantum fluctuations' mixed up with or without 'virtual particles', an already doubtful fiction of our classical physics mind interpreting the math of QFT) as 'nothingness', can be a convenient physical mathematical working tool, but has no future as an ontological and universally accepted definition of nothingness, and even less can lead to any theological interpretations.

53. **Neto**
May 1, 2012

@Anonyrat

English is not my first language, so I can sound confusing. What I'm trying to say is that any question that respects language constructions and coherent semantics have a meaning. The "Why there's something rather than nothing" (WTISRTN) question have a perfectly clear and have meaning, but it may not have an perfectly clear answer, yet. Maybe someday we'll get in the time where one answer will be just "it is because it is", but that don't make the question meaningless, and I really hope this is not the case for the WTISRTN question.

Give the meaningless status to an reasonable question sounds a lot more like an Ostrich tactic than a real reflection about it.

If we consider that science is not about the truth, but about the facts that can be reach trough the scientific method, the question WTISRTN may not be a scientific question right now, but it don't make it meaningless. It may be someday.

54. **Peter Woit**
May 1, 2012

All,

Enough discussion of nothing, please. Please restrain your philosophical impulses. This is neither interesting nor entertaining.

55. **jg**
May 1, 2012

I meant to post this comment here (accidentally posted it on the other thread)

I'm not sure Einstein was so respectful of philosophical tradition as Lee Smolin (for example) suggests, in fact on page 2 of his 1922 'The Meaning of Relativity' he writes:

> ……………………………………………………. I am convinced that the philosophers have had a harmful effect upon the progress of scientific thinking in removing certain fundamental concepts from the domain of empiricism, where they are under our control, to the intangible heights of the a priori. For even if it should appear that the universe of ideas cannot be deduced from experience by logical means, but is, in a sense, a creation of the human mind, without which no science is possible, nevertheless this universe of ideas is just as little independent of the nature of our experiences as clothes are of the form of the human body.

http://www.gutenberg.org/files/36276/36276-pdf.pdf

56. **D R Lunsford**
May 3, 2012

Odd – Penrose had a "something from nothing" idea a while back that was actually interesting. As the universe empties out in the heat death of expansion, the world goes over into conformal invariance (nothing but free radiation) and the time scale becomes undefined. In this world without a clock, a vacuum fluctuation could be seen as a new Big Bang. The current discussion seems to have forgotten or ignored this relatively recent work.

-drl

57. **Nige Cook**
May 4, 2012

You've modified Penrose's "cycles of time" very slightly: he argued that the instant when blackholes have converted all matter into radiation which is redshifted so much it *cannot* cause any quantum event (wavefunction collapse), time ceases to exist and – with it – space (space can't exist without time). So at the precise instant that photons cease to be able to do work, space disappears and universe becomes a singularity, a new big bang. The big bang arises not because of a quantum fluctuation, but because such any fluctuation is impossible. Very Zen.

58. **Allan Rosenberg**
May 7, 2012

"Enough discussion of nothing, please. Please restrain your philosophical impulses. This is neither interesting nor entertaining."

So I guess you don't want to speculate on whether, had there been nothing rather than something, this blog would have been called Not Even There?

A Prediction About a Prediction

May 1, 2012
Categories: Uncategorized

In the years leading up to the LHC, string phenomenologists were vocal about their hopes to use string theory to make predictions about what the LHC would see, despite a history of a quarter-century of failure on the prediction front. For example, in late 2007 Michael Dine was writing in Physics Today:

> A few years ago, there seemed little hope that string theory could make definitive statements about the physics of the LHC. The development of the landscape has radically altered that situation. An optimist can hope that theorists will soon understand enough about the landscape and its statistics to say that supersymmetry or large extra dimensions or technicolor will emerge as a prediction and to specify some detailed features.

The main target for a landscape prediction has always been what appears to be the simplest possible question about BSM physics that the landscape could hope to address: is the supersymmetry breaking scale likely to be high (GUT/Planck scale) or low (electroweak scale)? By the time the LHC data started to arrive (showing no supersymmetry), these hopes for a landscape prediction had failed, as it became clear there was no way to get a clear answer about this (or any other question…) out of landscapeology. Landscape proponents have still not given up though, with Michael Douglas yesterday putting out a survey of work on the SUSY question, The string landscape and low energy supersymmetry. He has no string theory predictions, but he has a (very tentative) prediction about a (sort of) prediction:

> I am going to go out on a limb and argue that
>
> *String/M theory will predict that our universe has supersymmetry, broken at the 30 – 100 TeV scale. If at the lower values, we may see gluinos at LHC, while if at the higher values, it will be very hard to see any evidence for supersymmetry.*
>
> This is a somewhat pessimistic claim which far outruns our ability to actually make predictions from string theory. Nevertheless I am going to set out the argument, fully realizing that many of the assumptions as well as the supporting evidence might not stand the test of time.

As for the time scale and reliability of this prediction of a prediction, he writes:

> My guess at present is that twenty years or more will be needed, taking us beyond the LHC era. Even then, it is likely that such predictions would depend on hypotheses about quantum cosmology which could not be directly tested and might admit alternatives. It is entirely reasonable that sceptics of the landscape should reject this entire direction and look for other ways to understand string theory, or for other theories of quantum gravity. At present we do not know enough to be confident that they are wrong. Nevertheless the evidence at hand leads me to think that they are wrong and that this difficult path must be explored.

So, optimistically, if all goes well, long after the LHC is shut down, maybe we'll see a landscape prediction about whether the LHC should have seen SUSY. This prediction will depend upon assumptions about quantum cosmology that can't be tested, so if it disagrees with what the LHC saw, that won't falsify the landscape anyway.

Meanwhile, Gordy Kane is promoting the idea that string theory already has made a prediction: a 125 GeV Higgs mass, spectacularly in agreement with the latest data, and gluinos detected "by summer", "with masses around a TeV, maybe less" (see here). He's giving a talk today at the Simons Center with the title "String theory, the real world, and the prediction of the Higgs boson mass". As far as I can tell though, no one except possibly his collaborators believes him. At a public talk on the Higgs recently here at Columbia, Brian Greene was very skeptical, joking that if the LHC had seen something at 142 GeV, that would be Kane's "prediction" (for more about this, see Lubos's outraged coverage here).

Matt Strassler recently weighed in on the Kane prediction, which so outraged him that he has stripped Kane of his professional title. Matt is careful to put "Professor" before his own name and those of others who deserve the title, but Kane is now "Mr. Kane":

> The level of garbage and propaganda surrounding the Higgs is getting pretty ridiculous.
>
> You realize, yes, that by August 2011 the window for the Standard Model Higgs was down to 115 to 140 GeV, right? So your chances of getting within 5 GeV of the right answer is 15%. Many theories before Mr. Kane predicted a range that included 125 also. I'm completely unimpressed both by the science and the propaganda. Most of my friends who are experts in compactification (which Kane is not — he relies on one of his collaborators — and I am not an expert either) are not convinced of the assumptions on which they base their arguments. It all sounds good. But is it really? I've heard lots of arguments that sounded good over the years… and most of them are now known to be wrong. None of them are known to be right.
>
> Do not judge science on the ability of the scientist (who wants his or her Nobel prize and is trying his or her best to convince you) to present a compelling argument. A great salesperson can create a terrific argument; a great physicist does not need one.

So, Kane seems to be finding that his "string theory prediction of the Higgs mass" is being met with scorn, from all segments of the particle theory community. I'm curious what has happened to his paper from early December, which I'd guess was intended for PRL, but has yet to appear. He has about a month and a half for the gluinos to show up "before summer" and vindicate his "prediction".

The Strassler comment was at a useful posting about the state of SUSY searches (see here). Matt's time estimate for how long it will take the LHC to rule out SUSY: "This will take a while, probably a decade." A more mainstream time estimate might be that it has already happened. For Tommaso Dorigo's take on this, see SUSY and the Silence of the (Roasted) Lamb.

Starting tomorrow Brookhaven will host a workshop on the state of SUSY topic. For latest developments, look at the slides as they appear here.

Update: The video for Kane's talk at the Simons Center is now available. Instead of gluinos "by summer", he's now changed his tune, and he expects "discovery during 2012" (if the expected luminosity goals are met). The mass of the things has moved up from less than about 1 TeV to less than about 1.3 TeV.

Update: The Kane et al. paper with the Higgs mass "prediction" has just appeared at Phys. Rev. D. The preprint went to the arXiv on December 5, the Phys. Rev. D submission date is February 13. One guess would be that this more than two month delay might be due to the paper being rejected (or a referee insisting on the "string theory prediction" nonsense being removed) wherever it was first submitted, perhaps PRL.

Comments

1. **Shantanu**
   May 1, 2012

   Great article, Peter. Btw what's the status of models like Randall-Sundrum or ADD?
   From what I understand there is no evidence for these in LHC data. But some people such as Cliff Burgess(from his talk at PI) still seems sanguine. I asked this question

to L. Randall when had a post on cosmic variance, but didn't get a reply.

2. **Peter Woit**
   May 1, 2012

   Shantanu,

   LHC data has ruled out RS and ADD models at currently accessible energy scales at the LHC. Despite the huge publicity campaign for these things pre-LHC, hardly anyone ever seriously thought they would show up at the LHC. Now no one does, and it's thought best to just try and forget the publicity campaign. In principle in a few years when the LHC goes to near design energy, there's a new energy range to check. I seriously doubt though that even the biggest enthusiasts for these things would bet any money that something will show up, even with 10-1 odds in their favor.

3. **Allan Rosenberg**
   May 1, 2012

   "You realize, yes, that by August 2011 the window for the Standard Model Higgs was down to 115 to 140 GeV, right? So your chances of getting within 5 GeV of the right answer is 15%."

   Last time I checked, 10/(140-115) = 40%.

4. **Bernhard**
   May 1, 2012

   Wow, so lots of people putting Gordy Kane in a tight corner. I think Witten should speak up too, that would probably put and end on his infamous campaign.

5. **P**
   May 1, 2012

   Hi Peter,

   Two questions, but first a comment.

   Mike Douglas is better situated to say things about the landscape than just about anyone, including predictions of individual vacua (they exist!) but also whether or not the landscape as a whole prefers certain types of physics over others (which would be interesting if it did, because quantum field theory doesn't.) You make it seem that no one who is serious takes Kane seriously, but Douglas directly quotes Kane's work as having influenced his thoughts on the subject.

   Which leads to my questions:

   Did you actually read the Douglas article or did you just cherry-pick the first section regarding "predictions", bringing up the sorts of issues you usually do? That's a fine approach to some articles, though still unfair, but Douglas is a real expert and he wouldn't put his name on it without serious thought. There are very few people on the world that have his level of knowledge on string vacua and the landscape.

   And a more playful question:

   It seems you're not a fan of low energy SUSY. Do you believe that Higgs boson exists, and if so, what is your favorite mechanism for solving the hierarchy problem and why? Many theorists would invoke SUSY for precisely that reason.

   Cheers,
   P

6. **Slacker**
   May 1, 2012

   The paper of Kane et al clearly states all the assumptions and caveats, e.g. the MSSM spectrum below the GUT scale, N=1 D=4 supergravity framework, etc, so I see no reason for Mr. Strassler to belittle this work and refer to is as 'garbage'. Btw, both Acharya and Kane were twice acknowledged in the Douglas paper, so string phenomenologists do pay close attention to what they have to say. If you read their most recent review and compare it to the Douglas paper you'll notice a common thread. The set of arguments presented in their review, which also includes the Higgs mass discussion, are not at all controversial and pretty much agreed upon by most string phenomenologists who understand the details of moduli stabilization and SUSY breaking in N=1 D=4 supergravity.

7. **Peter Woit**
   May 1, 2012

   Slacker,

   Perhaps you can take up Strassler's and Greene's evaluation of Kane's claim with them. I'd be curious to know if you can find a single prominent theorist (not a collaborator of Kane) willing to say publicly that he thinks Kane really has done what he claims: predict a Higgs mass of 125 GeV from string theory.

   Slacker and P,

   I don't doubt that Douglas has paid attention to what Kane is trying to do, and, as he says, that line of thought has influenced his hopes of getting predictions out of string theory. I also note that he doesn't reference Kane's December paper claiming the 125 GeV prediction, or mention this prediction. He's quite explicit about what he sees as prospects for getting predictions out of string theory, and what he says about this is completely inconsistent with Kane's claims. He is clearly implicitly saying he doesn't believe that Kane has a string theory prediction of the Higgs mass.

   Slacker,

   I read parts of the paper carefully, skimmed others enough to see what arguments Douglas was making. Note that I've attended several talks by him on this topic, read the slides of others, as well as several of his previous papers. With this background, I looked at his paper to see what was new. The most striking new thing to me was the much more pessimistic evaluation of prospects for this program, so that's what I wrote about. I've never heard him before say that it would be at least twenty years before this led to any sort of prediction, and that even then it wouldn't be a real one (i.e. you would always have to put in assumptions about string cosmology).

   Kane I think is really out of his mind, making absurd and outrageous claims. Strassler's "garbage" quote is apt. Douglas is quite different. He's not claiming to have solved any problems or to have something he doesn't have. He even acknowledges that others looking at the same set of facts as him will quite reasonably conclude this is a doomed project.

I've repeatedly written about the "hierarchy" problem here. My basic point of view is that I'm unconvinced that it's a problem. One way of stating the problem is to say that we have no explanation for why the electroweak scale is so small compared to the GUT or Planck scale. Here I think it's a good idea to keep in mind that we don't even know if there is a GUT scale, and don't know enough about quantum gravity to understand the significance of the Planck scale. Put differently, the hierarchy problem is largely a problem set inside the GUT/string scenario of what a unified theory looks like. Such scenarios seem to so far be failures, so why worry about a "problem" that comes with them?

The Higgs field does pose serious problems, but they seem to me different than the "hierarchy" problem. Matter and gauge fields have a geometrical structure that tightly constrains their behavior. A scalar Higgs doesn't, introducing for instance the highly undesirable feature of completely unconstrained Yukawa couplings and thus arbitrary fermion masses and mixing angles. This to me is what really cries out for explanation, not the fact that the Higgs mass is small compared to the Planck scale. If SUSY had anything interesting to say about that, then I'd be a fan.

8. **Shantanu**
May 2, 2012

Peter or others, I think someone mentioned on physicsforums that Wetterich and Shaposhnikov had a paper on Higgs mass of 125 Gev 2 years before CERN result (see http://arxiv.org/abs/0912.0208). What do you think of this
Why doesn't anyone refer to this or mention it as a prediction of "string theory"?

9. **Peter Woit**
May 2, 2012

Shantanu,

I know nothing about that particular calculation, but it is remarkable that the Higgs is in the range where the SM holds out to very high energies. But, since this has nothing to do with string theory, it doesn't get that much publicity.

10. **theoreticalminimum**
May 2, 2012

I think it might be somehow interesting to some to read some of the other articles to figure in the memorial volume, which are linked to here.

11. **Andy**
May 2, 2012

Higgs-mass predictions, by Thomas Schucker
http://arxiv.org/pdf/0708.3344v8
gives a list of 96 Higgs-mass predictions, with references.

12. **M**
May 2, 2012

It is beautiful to see how well these pre-dictions change when new data arrive.
We should admire how flexible is string theory in adapting to whatever experimental result, improving from old-fashioned Heterotic Strings to modern Elastic Strings.

13. **theoreticalminimum**
May 3, 2012

I am currently watching the linked Kane talk. First of all, I am very surprised that Kane spent so much time painfully elaborating so much on what would be considered very elementary to a SCGP audience. Did he realise he was talking to experts in theoretical physics? Just something that caught me off-guard: I'm not sure I understand what Kane meant by F=ma not being testable. He used the word "tested" many times in his talk, but didn't seem to be bothered to define what he understands by something being testable or not. Can anyone enlighten me?

14. **piscator**
May 3, 2012

The simplest objection to the Kane etc work is that G2 manifolds are not, unlike Calabi-Yaus, actually constructed. The `G2-MSSM' does not involve an actual construction of the MSSM and does not involve an actual G2 manifold.

So given that in the UV you don't have the manifold and you don't have the model, many (including myself) think a little humility is needed on the the extent to which any of this work represents string theory predictions for the Higgs mass.

15. **Bernhard**
May 3, 2012

Peter,

you mean "about 1 *TeV* to less than about 1.3 *TeV*".

16. **Peter Woit**
May 3, 2012

Thanks Bernhard, fixed.

17. **Bernhard**
May 3, 2012

theoreticalminimum,

I also don´t get it. Kane say other strange things like "the Standard Model is the most theoretical theory that´s ever been, maybe that ever will be", but none of them is more nonsense than

"last summer we did a string theory, a honest string theory calculation of predicting the Higgs boson mass and the one they reported in December was the one we predicted they should report".

18. **Peter Woit**
May 3, 2012

theoreticalminimum,

This is now standard issue string theory ideology: string theory (which predicts absolutely nothing) is just as good as the quantum field theory (which gives us the Standard Model, the most successful theory in the history of science). The argument is that both are just "frameworks", just like "F=ma" is a "framework" (you need to pick a QFT, and pick an F, just like you need to pick a "string theory vacuum").

Arguing that the biggest failure in history of science is the same as the biggest success is obviously sophistry, but rather popular sophistry these days.

19. **Mark**
May 3, 2012

piscator, indeed, the G2-MSSM does not involve a specific singular G2 "manifold" (explicit smooth examples have been constructed by Joyce and Kovalev and the singular ones should exist by the arguments involving dualities) but instead, it uses the most general *known* properties of G2 manifolds. The scaling dimension of the 7-dim volume wrt to the moduli is known, the form of the moduli Kahler potential is known from dim reduction, likewise, the general functional form of the gauge kinetic function in terms of the moduli and the general dependence of the Yukawa couplings on the moduli are known, and the general scaling properties the Kahler metric for bifundamental chiral matter is also known from locality. It turns out that one can robustly demonstrate how to stabilise the moduli in this very general case without referring to a specific G2 construction, for instance, without knowing the precise moduli dependence of the volume in the Kahler potential but only its scaling property. Also, surprisingly, it turns out that the soft breaking terms in these vacua "forget" about most of these microscopic details as these computations involve a bunch of contractions, so the explicit moduli vevs drop out of the final result. In the end, the only moduli dependence appears through the 7-dim volume, which is considered a parameter, but which becomes fixed from bottom-up by the relation between alpha_GUT and m_planck. The specific properties of G2 manifolds that directly affect the numerical coefficients in the final results for the soft terms are the scaling dimensions, which are known. Be assured that Bobby Acharya, who is a world expert in these matters, would not be putting his name on these papers if there was something dodgy.

20. **Mark**
May 3, 2012

piscator, here is the G2-MSSM reference I found to be the most useful: http://www.springerlink.com/content/g6288v55tj184588/?MUD=MP

21. **P**
May 3, 2012

Peter,

Forget the F=ma stuff.

You state that you don't the like argument but you don't actually argue against it, which particularly frustrates me because I've espoused this view multiple times.

String theory and quantum field theory make predictions about the sorts of objects that exist in those frameworks. Neither, as frameworks, makes precise predictions about precise structures.

A string vacuum or a particular quantum field theory require introducing more input. Each makes precise predictions (matter content / gauge symmetry and scattering amplitudes, e.g.) that either are like the world or are not like the world. Most string vacua look nothing like the standard model. Most field theories look nothing like the standard model.

It is dishonest and an outright lie to pretend like an individual string vacuum doesn't make precise predictions about things like gauge symmetry and matter content. The ST FT and string vacuum particular QFT analogy is a good one for the reasons just mentioned. The issue, as I say over and over, is whether or not with LOW ENERGY EXPERIMENTS we can tell the difference. After all, at low energies, string vacua just look like effective field theories.

It's not that there aren't issues – there are – but pick the right ones, please. The landscape and the vacuum selection problem are the issues, not whether individual vacua make predictions.

P

P.S. Your previously comment about the Higgs mass hierarchy problem misses the point that it doesn't require GUTs or strings. The problem is the fine-tuning associated with the quadratic divergence of the Higgs boson mass, which has nothing to do with WHAT the UV completion is.

22. **P**
May 3, 2012

I second what Mark says.

Agreed, the big drawback of the G2-MSSM scenario is that no one has constructed an actual global MSSM model of M-theory on a G2 manifold.

Their scenario takes a very different approach, as discussed by Mark.

23. **P**
May 3, 2012

Sorry to harp, Peter, and sorry for three postings in five minutes.

Just to remind everyone, by the "biggest failure in the history of science", Peter is referring to string theory for the purposes of unification and a theory of everything.

Though I find his arguments flawed, there, as do many, I think that he and I would both agree that string theory as a subject has made tremendous contributions to pure mathematics and our understanding of quantum field theory, regardless of any statement about unification of fundamental physics.

Cheers,
P

24. **Peter Woit**
May 3, 2012

P,

I've repeatedly, many time, here explained what I see as the difference between the Standard Model situation and the string theory vacuum situation. In one case, among the simplest possible choices leads to an extremely restrictive and predictive model, one that makes absurdly precise predictions, which agree exactly with experiment. In the other case, all you are doing is cooking up more and more complicated models, carefully designed to avoid falsification.

Trying to argue that the QFT/SM situation is "the same" as the string theory/string vacuum situation is like trying to claim that black=white, since they're both shades of grey. It's a waste of time to try and have a discussion with someone who wants to tell you why black=white.

About the hierarchy problem: the quadratic divergence may just be an artifact of perturbation theory. I state it the way I did (which is quite conventional) because it makes sense non-perturbatively.

25. **Bernhard**
May 3, 2012

    P,

    I think the problem is in in the framework itself. To put differently, what do you need to construct a realistic theory of nature starting from QFT and starting from string theory? Peter made some interesting comments e.g. here: http://www.math.columbia.edu/~woit/wordpress/?p=4065&cpage=1#comment-98564

    And after we deal with this very ugly construction, what does it predicts? At low energies you say it looks like QFT, so I certainly don´t need it.

    At high energies (GUT scale? Planck?) what does it say? Would one be able to compare string theory with experiment in the same way the standard model is, even if we had a galactic accelerator?

    I don´t think so, put please correct me if I´m wrong.

26. **piscator**
May 3, 2012

    Mark, P

    Thanks for the comments. I do know this model and these papers (in fact I was the referee for the paper Mark referred to).

    It is a perfectly well-defined and honest task to take a 4d supergravity model, with assumptions that are more or less inspired by string theory, and study its properties and analyse its phenomenology. The model considered has some interesting features and it is perfectly reasonable to study it.

    However this gives absolutely no justification for saying this is a string theory prediction of the Higgs mass. I haven't watched the video, so I don't know whether the quote is accurate, but it is indefensible to call this an `honest string theory calculation'.
    This is not string theory. At best it is a 4d supergravity theory with properties inspired by string theory – which is fine, but is not string theory.

    String theory is special because of its ultraviolet behaviour. Look at the ultraviolet for these models – there is no actual manifold there, there is no actual construction of the MSSM, there is not even the worldsheet picture as you are in the M-theory regime.
    I'm not that down on this, because the subject is hard and you have to do what you can and not what you can't. But let us be honest about the difference between computing something and postulating it.

27. **P**
May 3, 2012

    Peter,

    Simplest possible choices!? The representation theory and couplings of the standard model are far from simple.

    And remember, I'm not comparing vacua with the SM, i.e. a single field theory. I'm comparing vacua with particular QFTs. The point is, given a particular quantum field theory or a particular string vacuum, in each case we can ask "does this match what we see in the world?". We have a particular quantum field theory, the standard model, that experiment tells us correctly describes particle interactions below the TeV scale or so. It has a tremendous amount of unpredicted, empirically determined input. Tell me, now, what predicts the tremendously detailed, complicated structure of the standard model? What predicts the gauge symmetry, matter content, and chiral structure? Because string vacua predict these things, remember . . .

    Re the hierachy: okay, I'll agree with that, but it's still not a GUT / string issue. It's a "high scale" issue. Any non-perturbative physics which would solve the hierarchy problem would have to kick in at a very low scale (TeV), though. And your point is that it would alleviate the quadratic divergences and cut off the running?

    Bernhard,

    Thanks for the comment. Your question "why do I need it?" depends on your taste. If you're just interested in modeling nature at low energies and are fine with unexplained mass hiearchies (note: different from previously discussed hierarchy problem), e.g., then effective field theory will model it just fine. String vacua give many mechanisms for explaining detailed structure and also predict things that QFTs don't (as far as I know): gauge symmetry, matter content, and chiral structure.

    At high energies, there definitely are stringy signatures that are clearly distinguishable from field theory. Technically this occurs at the string scale, but this is typically a high scale near the Planck scale. There, it is possible to excite very massive string modes which form an infinite tower, and there are papers on how the collider signatures could determine that it is a string mode and not some other type of tower of modes, such as a KK mode. It's worth pointing out that, along with gauge symmetry (almost always spontaneously broken) and general relativity at long distances, such modes are generic predictions of all string compactifications (that I know of). It is on the details where particular vacua differ, such as the precise structure of the gauge theory, and these are very important.

28. **P**
May 3, 2012

    Piscator,

    The one addendum is that their "string-inspiredness" regarding moduli masses really is applicable to all string compactifications. How broadly their claim about m_3/2 holds in M-theory, I don't know.

    I couldn't agree more. Very well said. Thanks for the comment.

P

29. **Peter Woit**
May 3, 2012

P,
In the case of string theory you're invoking a non-existent fairy land, where all the many problems of string theory and string theory "vacua" have vanished. In the case of the SM, you're describing a theory that can be written on a t-shirt as "tremendously detailed, complicated structure", and the trivial and defining representations of SU(3) and SU(2) as "far from simple". This kind of argument allows you to equate black and white, but it's kind of absurd.

Again, I'm not convinced there is a hierarchy problem that needs to be solved. The quadratic divergences are not obviously relevant. Until you have some specific kind of new physics at a higher scale, you just don't know whether the fact that the electroweak scale is much smaller than that scale is something that requires any explanation.

30. **Mark**
May 4, 2012

piscator, I agree, these are effective N=1 D=4 sugra computations where W, K and f are dictated by the known properties of G2 manifolds/compactifications. I'd also add that apart from toroidal orbifolds where explicit worldsheet computations have been done to compute, e.g the moduli dependence of the Yukawa couplings, Kahler metric etc, a lot of string phenomenology comes down to an effective field theory analysis combined with methods from algebraic geometry. I'm convinced that a lot more progress would be possible if explicit global G2 examples were constructed, independent of whether we have a complete formulation of M-theory. So yes, I think that for Gordy Kane to claim that they did explicit string theory computations is very misleading but. having seen his talk, I'd say go easy on him, he is not a string theorist so maybe he just does not know the difference, but his enthusiasm is certainly inspiring.

31. **P**
May 5, 2012

@ Peter:

Name a particle in the standard model that is only in the fundamental of SU(3) or SU(2) and transforms trivially under all other groups. Give me an example of some trivial structure of couplings or mixing angles that appears in the standard model.

The only reason we have identified the standard model as a good description of nature is because low energy experiments are good at distinguishing between particular quantum fields theories in the continuously infinite class of theories. The real problem with string vacua isn't that they don't make predictions. The problem is the landscape, and the fact that at TeV scale energies string vacua almost always look like standard quantum field theories. Sufficiently high energy experiments would be able to not only distinguish between the effective theories of different vacua, but also differentiate between whether nature is described by a string theory or a quantum field theory which is UV completed in a different way.

You're not really even responding to my arguments and instead are just saying the names of colors and invoking fairies. Please, please, please: just acknowledge the real problem. A string vacuum makes predictions, e.g. a low energy gauge theory. Almost none of them look like the standard model, but some do. With low energy experiments, it may be impossible to decipher which UV completion gives rise to the effective theory that describes nature. That is the problem, not that a string vacuum doesn't make predictions. If you acknowledge this, I'm happy to stop harassing you, but I feel the need to respond as long as there are false things stated in these comments!

You are not a string theory expert and have written a popular book and gained a voice in the community – and there should be dissenting voices, including yours – but progress of science requires honest dialogue about what the problems actually are in any given field. You seem to miss the actual problems, sometimes. Notice that I'm not one of those string zealots who is trying to say there aren't problems. I'm just trying to be clear about what they are . . . and I've stated them here.

Q: Do you acknowledge that a given string vacuum predicts the low energy gauge group and matter content? You continually avoid this question and imply that a string vacuum makes no predictions. Maybe you're just not aware, but I'm curious.

Regarding the hierarchy, I'm confused by what you're saying. Are you arguing that there isn't a fine-tuning problem between the bare mass and the lambda^2 contribution? Surely that must not be it – I've never heard a physicist take this viewpoint – but if so, please explain. The fine-tuning problem is independent of what new physics might come in at the cutoff.

@ Mark: right on.

32. **Peter Woit**
May 5, 2012

P,

Yes, there are 3 couplings in the standard model we don't understand the origin of, as well as an interesting, simple, pattern of representations of SU(2), SU(3) and U(1), involving nothing more complicated than the fundamental representations in the case of SU(2) and SU(3). This is a very small amount of basic data we don't understand and have no idea what explains it (best effort is the SO(10) GUT). There's also a matrix of masses and mixing angles, and two couplings, all due to the Higgs field. That's a huge problem we have no idea about.

For the case of string theory: we don't even know what string theory=M-theory is, and we can get almost anything we want out of it. No one has any way of even saying what a "generic" string theory/M-theory vacuum would be, or, given best guesses of such a thing, no way to reliably calculate what it predicts. The one "M-theory vacuum" that has been extensively studied outside of perturbation theory (AdS5) looks nothing at all like the real world: wrong dimension, none of the SM at all). All "string theory vacuum predictions" are based upon going to some extreme limit of parameters where you hope string perturbation theory will be reliable and various assumptions about branes will work out, and engineering something to fit the simple data that define the SM. When you do this, all you ever get is what you put into it.

I have no idea who you are and whether you have any idea what you are going on about. The description I gave above of the situation of string theory is not just my opinion (based on 30 years in this business), but now conventional wisdom among particle theorists. Even most string theorists these days agree that string theory can't be used to predict anything and think activity like Kane's is a bad joke (see the Strominger talk linked to in the next posting for example).

Answers to your questions: as explained above, you only get reliable calculations of the low energy field content when you drive the theory to a corner of its parameter space, and make some optimistic assumptions. As for the hierarchy problem, for the N'th time, the problems with the perturbation expansion is not obviously a physical problem. I don't have time to find a good reference that explains the standard lore about this, but 5 seconds of Googling turned up this paper

http://arxiv.org/pdf/1201.0857.pdf

which explains the issue from the point of view I've been talking about, one which is completely conventional.

33. **P**
May 5, 2012

Peter,

We're in complete agreement, then, about how the standard model is complicated, particularly at the level of Yukawa couplings which all involve the Higgs field, and how the matter content representations fit nicely into the 16 of SO(10). We're also in agreement that this is a huge problem, and you would probably agree with me that it would be tremendous progress in particle physics if one were able to derive them from some more fundamental consideration.

Since you mentioned how SO(10), for example, can nicely explain the standard model representations, I'll mention how this might arise in one type of string vacuum, though similar considerations hold elsewhere. Consider heterotic M-theory on CY3 x interval. This is not simply a "region" of "parameter" space (which doesn't even make sense, we should be talking about moduli space). A stable holomorphic bundle with structure group SU(4) will break the E8 (which must exist at 1 end of the interval for anomaly cancellation) directly to SO(10). Simple discrete choices of bundle-defining parameters can give rise to 16's of SO(10). As you implied by your mention of SO(10), all of the standard model fermions can fit into this representation. Furthermore, a good deal is known about non-perturbative effects in heterotic M-theory (worldsheet instantons and the like), contrary to what you implied. So getting things something like the standard model reps are definitely NOT just in miniscule regions of parameter space where all we have is perturbation theory and we don't know anything else.

Rest assured that I am well educated in both particle physics and string compactification and am familiar with the pitfalls and subtleties in both subjects. I am someone who sometimes feels the need to correct misinformation about these subjects on the web, regardless of where it may be. Often times I see it on this blog. Regarding particle theorists, many dislike string theory for the sake of unification. Sometimes that dislike is out of knowledge (as I've said, there are good criticisms!), and sometimes out of ignorance. Then there are also serious particle physicists who like string unification and have worked on it or related topics.

Regarding string theorists themselves, many would probably be wary (as would I) of the ability of low energy experiments to determine the UV completion of the effective theory we see at low energies – i.e. to test the stringiness of string theory that is not present in QFT. But 95% of string theorists would tell you 1) that string vacua predict certain things at low energies such as gauge symmetry and matter content (as evidenced by quality of people in the last ten years who have worked on these things) and 2) at high energies (string scale) there are clear stringy signatures that would allow one to distinguish between string theory and quantum field theory. This is a limitation of our experiments, not of our theory.

Regarding hype, we're in total agreement. It has no place in any science of any kind, and honest dialogue about science is the only proper thing to replace it.

Regarding the hierarchy – perhaps there is something I'm missing and I should look at this paper closely. But as an expert it worries me that I've never heard of the people who wrote that paper, and moreover I've never heard any particle theorist try to dismiss the hierarchy problem in the way you are. An alternative Google search "why the hierarchy problem is a problem" returns two Wikipedia articles first, and then some slides by Joe Lykken on solving the hierarchy problem. He, a very very well known particle theorist, as you know, seems to take it quite seriously.

34. **P**
May 5, 2012

clarification: on the last paragraph, I mean that as an expert in high energy theory (not the hierarchy problem) who very often talks to people who focus solely on particle physics, I've never heard anyone try to dismiss the hierarchy problem in the way you are. Humbly, I haven't thought about it too seriously beyond the common objections stated by particle theorists. There may be more things to take into account that I'm missing, but my point is that very very good people take the problem very seriously and don't just talk it away. I'll take a look at the paper you posted this week, though.

35. **Peter Woit**
May 5, 2012

P.

Yes, I know you can get SO(10) and the spinor rep (as well as just about anything else…). The big problem though with an SO(10) GUT (besides proton decay, or course…) is that you don't have a good idea about how to break it down to the SM. Adding more Higgs fields just adds a new set of parameters you don't understand in addition to the SM ones. If string theory provided any insight into these problems it would be interesting, but it doesn't. The problem with string theory, right back to 1984-5, is that it doesn't tell you anything about the SM you didn't already know. This was the reason I (and many others) were skeptical about the whole idea from the beginning. Nearly 30 years of effort hasn't improved the situation, quite the opposite.

The paper I linked to was randomly chosen, I don't know the authors. Their introductory text though makes clear that they are just repeating conventional wisdom you can find anywhere. This is the way the "hierarchy problem" looks if you try and state it non-perturbatively. As for my point of view on the "hierarchy problem", maybe it is a minority one, but I think if you ask around you won't have trouble finding theorists who were never impressed by the "hierarchy problem" argument, or see it as a relatively weak one, tied to our flimsy ideas about GUT-scale physics. The idea that there is a GUT/Planck scale and that you should worry about it is certainly part of the standard lore of how to think about BSM physics, but this lore has been highly unsuccessful in terms of leading to anything.

Much of the attention to the "hierarchy problem" was always because of its use as an argument for why the LHC should see BSM physics. Now that this physics is not showing up, the "hierarchy problem" argument will start to get ignored.

36. **Anonyrat**
May 5, 2012

1. If String Theory cannot tell me anything about how the effective QFT GUT (e.g., SO(10)) that it yields further flows down to the Standard Model, then the solution of the mystery of the Yukawa couplings and of the hierarchy problem (if there is one) cannot lie in String Theory.

2. For String Theory to be convincing as a lead I would want to follow, at a minimum, I would expect the stringy SO(10) GUT to be very strongly theoretically constrained. It should pick a very tiny part of the space of SO(10) GUTs that can plausibly flow down to a low energy theory that looks like the Standard Model. If it does that, even if I can't practically calculate anything, I'd be strongly inclined to take another serious look at String Theory.

37. **P**
May 9, 2012

Hi Peter,

Sorry for the delay in writing back, it's been a busy week.

Regarding your SO(10) comments – I don't disagree *quite* as much with these, though I'm certainly no expert on Higgsing it to the SM (126-dim rep, right?). There has been some talk of whether these reps are even possible in string vacua. A rough argument against them is that in many types of string vacua, a 248 of E8 is the highest rep, so you're not going to get a 126 of SO(10).

A string vacuum can and does predict things that QFT does not, e.g. particular matter reps, etc. If one vacuum was uniquely selected by some mechanism (hopeful), this would be a prediction (or postdiction, you might say) of something that is input in the standard model. But barring that, it is likely very difficult or impossible at the TeV scale to differentiate whether the effective field theory we observe is lying in a string vacuum or some other UV completion. Though you might not be happy with the first two sentences, I bet you'd happily agree with the last.

Anonyrat,

Certain it is well known that string vacua do give mechanisms for accounting for Yukawa hierarchies and the like. Small Yukawas can be generated radiatively from bulk effects or from any of the myriad of instanton effects, for example. There are also various rank arguments in F-theory.

The space of SO(10) GUTs is enormous, but gauge theories in string compactifications are typically more constrained than the space of all gauge theories due to something known as tadpole cancellation. For example, in QFT you could take SO(10) GUT with N 16 dim spinor reps. Quantities like N are typically bounded in string compactifications. In QFT they are not. Of course, from data observed below a TeV, N=3 seems like the right number, but we do not know for sure.

Cheers,
P

Neutrinos to Give High-Frequency Traders the Millisecond Edge

May 1, 2012
Categories: Experimental HEP News

Recently US plans for the LBNE next-generation neutrino experiment have run into trouble finding room in projected HEP budgets. Today (via Emanuel Derman's twitter feed), I learn of a promising new source of funding. A Forbes columnist reports here on prospects for using neutrino-based communication through the earth to do high-frequency trading, arbitraging prices in markets on opposite sides of the globe.

To actually do this, I'd guess that financial firms would have to site machines like Fermilab's proposed Project X and detectors like the LBNE one close to the servers running the markets. When they do this, maybe they'll let physicists use them on weekends to do physics. It has been unclear whether the US government could afford to build Project X/LBNE, but surely Goldman Sachs and other major investment firms would have no problem coming up with the billions needed. Yes, I'm well aware that this is a completely insane and ridiculous idea, but that hasn't been an obstruction to Wall Street innovations in recent years.

Update: For the latest on LBNE, see this from Nature, out today.

Comments

1. **Michael Hutchings**
   May 1, 2012

   Was this article posted exactly one month late by accident?

2. **Peter Woit**
   May 1, 2012

   Michael Hutchings,

   No, but it shows the problem one has trying to come up with April 1 postings. Truth is stranger than fiction. Actually, in this case I'm pretty sure this idea is unrealistic. But, I may be wrong…

3. **Edward**
   May 1, 2012

   Markets on the opposite side of the world are typically not open at the same time, which makes this less useful than it might be.

4. **paddy**
   May 1, 2012

   Let us calculate the bit rate available per Watt…..oops..never mind.

5. **Yatima**
   May 1, 2012

   I don't understand. Why would the computers (seller or buyer and the exchange) be on opposite sides of the globe so that the need to communicate with each other with low latency becomes a concern? Typically, one would set up seller and buyer computers in the same housing company building as the exchange computer. No other communication would be needed or useful, so there will just be a couple of routers and firewalls and a few metre of twisted pair cabling to pass. Maybe I'm being naive.

   Off-topic, but there recently was this fun article on HFT.

6. **Peter Woit**
   May 1, 2012

   Yatima,

   I think the point is that you want to be making a decision to buy or sell based on information that is 44 milliseconds ahead of your competitors trying to make the same decision (since your information channel goes straight at the speed of light, theirs goes around the globe). Of course, once everybody installs an accelerator, neutrino beam and neutrino detector in their office building, the things would kind of become worthless…

7. **Anonyrat**
   May 1, 2012

   The time to modulate a neutrino signal (even just on-off) is probably longer than the difference between the light travel time around the globe and through the globe.

8. **anonymous**
   May 1, 2012

   Maybe a few physicists who work on the project will get royalty contracts and become extremely wealthy (another quant bubble? Sign me up!). Wall Street could use some scientific involvement, though – maybe they could even learn the real calculus instead of that watered-down version they have to take.

9. **Z**
   May 2, 2012

   There was a March Physics World article on neutrino communication: http://physicsworld.com/cws/article/news/2012/mar/19/neutrino-based-communication-is-a-first

   There is also another, military application, for covert communication to submarines.

   The gist of it is that with current detector design and error correction techniques, the bit rate is about 0.1 bits/sec for a neutrino beam. This is a far cry from even the 160 bit/s that Voyager had, which is a celebrated example in information theory.

10. **S**
May 2, 2012

I think it's pretty clear they're hoping FTL neutrinos were true. Think of the money they could make then!

11. **M. Wang**
May 2, 2012

Two years ago, when a new firm raised $300m to dig a STRAIGHT-line trench for a new fibre optics line between Chicago and New York, I had joked with a colleague that the next step would be neutrino beams, which was the only thing capable of avoiding the earth's curvature. I guess someone else has now come up with the same idea, but not as a joke.
–
The reason why these investment makes sense (to varying degrees) is the physical separation of trading activities in two sets of financial assets: stocks and the stock-index-based futures. The former trades in New York, but the latter trades in Chicago. Usually, the big pension funds or mutual funds find it cheaper to enter/exit a big position by buying/selling futures than the corresponding stocks. Therefore, the futures price would move first. The competition to use this information to make money on the slower stock markets is intense. The aforementioned new fibre line saves 2ms in communication time, for which the firm charges $1m per month. Only the leading arbitrageurs in the country, typically with $500m or more in annual revenue can justify the expense.
–

It is important to stop this senseless race of shaving off another nano-second; there is no increased productivity once the refresh lag drops below human reaction time, roughly 200ms, a milestone that was passed in 2004. On the contrary, there is actually tremendous downside in this crazy speed race, as witnessed two years ago in the "flash crash". Actually, there are plenty of simple and natural remedies, such as charging for rapid cancellations of orders on the book for less time than human can react to, but the US equity market is "self-regulated", i.e. the SEC does not set the detailed rules; the exchanges do. The root cause to all this silliness can therefore be traced to the fragmentation of the equity (i.e. stocks) markets in the US. As each of the four major exchanges compete for order flows, they become beholden to the largest traders, and nobody can do more volumes than high-frequency firms.
–

Finally, just two weeks, the NASDAQ (in my opinion, the most professional and sensible exchange in the US) decided to take a small step in the right direction. A small fee will be charged on frivolous order placements/cancellations. So, maybe speed will lose its allure and no one will be funding the neutrino idea…

12. **Chris Oakley**
May 2, 2012

In 1994, when I worked on the Swaps Desk at UBS in London we planned to set up a neutrino communication link with with Sydney office. Originally protons were to be accelerated to 99.99% of the speed of light and smashed into a Nazi gold target, but to cut costs, it was decided to use Argentinian government bonds instead of protons. Unfortunately, this did not work as the bonds would default before reaching the target.

13. **Anon**
May 3, 2012

Well, if neutrinos are FTL, that could provide a whole new mechanism of shorting a stock. 🙂

14. **Ted Baxter**
May 4, 2012

Dare I ask what a Nazi gold target is?

15. **Chris Oakley**
May 4, 2012

Ted,

I don't wish to labour this but UBS = Union Bank of Switzerland. A lot of gold is stored in vaults in Zurich by Swiss banks including, allegedly, that originally stolen by the Nazis. I doubt whether, in reality, they would allow it to be used in a scientific experiment, especially if it ends up being radioactive. The chiefs would often play lip service to globalization but the reality was that the various centres (London, Zurich, New York, Tokyo and Sydney) would do their own thing. The only time that fast communication would have made a difference for our group was when Zurich & London had a video conference with New York – we actually had to fly to Zurich to join it and as well as a huge delay, the picture quality was poor (this was 1994/5).

Going back to the original topic, I doubt that program traders would benefit from a few tens of milliseconds speed advantage, and if they did, it is questionable whether such trades should be allowed as it would almost certainly be because someone was trying to game the system.

16. **Owen Patterson**
May 4, 2012

Let Wall Street and its global coven think they need neutrino communications. They will pay for all of it and then the science community can take it off their hands and get some real work done.

17. **AnonToo**
May 5, 2012

M.Wang, although your comment is well-informed (which I very much appreciate given all the misinformation out there on finance), and I agree with much of what you say, I'll have to disagree with this:

> It is important to stop this senseless race of shaving off another nano-second; there is no increased productivity once the refresh lag drops below human reaction time, roughly 200ms, a milestone that was passed in 2004. On the contrary, there is actually tremendous downside in this crazy speed race, as witnessed two years ago in the "flash crash"

First, the flash crash was a confluence of many causes, including an extremely large price-insensitive sell order, and I've never seen a report (speculation, sure, but nothing formal) that indicated HFTs were to blame. Of course HFT behavior is important, since it's a huge fraction of the market. But I don't know of a theory that says HFT caused the crash. If anything, it was HFT pulling out that caused the liquidity to disappear. In fact, the SEC was even considering adding rules that HFT can't "turn off" like they did!

Second, the flash crash was not a "tremendous downside". It was something like 20 minutes. It had no consequence to most investors, and errorneous trades were busted.

A few people learned that stop-loss orders with no limit are a bad idea (which was true before the flash crash, too), but overall there were few stories of real pain and there was no domino effect.

Third, I think you are vastly underestimating the benefits of HFT. There most certainly are benefits to sub-200ms trading. The liquidity in stocks with HFT participants is tremendous–as measured by bid-ask spread and cost of executing large orders–compared to stocks without HFT participants or stocks 30 years ago. We've replaced a large number of slow humans taking huge bid-ask spreads with a smaller number of very fast computers fighting for a few pennies. Needless to say, the latter is much more efficient (in the sense of market efficiency AND use of human talent) and is a better deal for investors.

Sure, it's silly people can make a lot of money reducing their network ping. But it's diminishing returns, so it won't last forever. Over the next 20 years (uneducated guess) things will stabilize as traders pull out the last bits of arbitrage that are still left.

We might as well accept the silliness for now, and be happy there are people investing in communication technology, no?

18. **Bugsy**
May 8, 2012

Z:
Interesting about the submarine communications. So, you need 1o seconds to transmit the one bit "fire missiles" and a bit more to transmit the more complicated "Oops-scratch that!!!"

Correction

May 3, 2012
Categories: Uncategorized

A while ago I wrote here about a recent "conference of Nobel Laureates" convened by Jeffrey Epstein in the Virgin Islands. This was based upon stories in boston.com (Boston Globe) and marketwatch.com (Wall Street Journal), which were based upon this press release from Epstein's foundation. The foundation also has stories about this on their web-site (see for instance here).

Looking into it more carefully, it appears that everything in the press release refers to something that happened not this spring, but back in 2006. More details about the 2006 event are in a piece by Lawrence Krauss at Edge.org. The pictures and quotes are the same as in the 2012 press release. Still available on-line here is a schedule of talks from the 2006 conference.

When I saw this I was wondering why it didn't give a specific date for the conference, and how Epstein had gotten the same prominent people as in 2006 to return this year, despite his well-known problems with the law in the interim. I have no idea why the Epstein Foundation recently issued this peculiar press release.

Epstein is a rather curious story, for some background, see this New York Magazine profile from 2002, and this Harvard Crimson piece about him when he donated $30 million to establish a Program for Evolutionary Dynamics in the Harvard math department. At the time of the 2006 conference, Epstein was under investigation by the police for having hired under-aged women for sex, and he ended up serving 13 months in prison as a result. He was arrested soon after the conference. Some recipients of donations from Epstein returned the money, but not Harvard.

Update: About the Program in Evolutionary Dynamics, I should point out that after losing Epstein to the penal system, it has landed on its feet, with a $10.5 million grant from Templeton.

For another article about Epstein, from 2003, see this at Vanity Fair.

Update: Another strange Epstein press release has appeared: Jeffrey Epstein's Involvement With the Edge Foundation. It's an endorsement of John Brockman and his Edge Foundation, with text that was also posted on all sorts of odd internet sites back in 2010. Very strange…

Comments

1. **Bob**
   May 3, 2012

   And I thought quantum mechanics was strange and unexplainable . . .

2. **Mike**
   May 3, 2012

   Stay away from gossip Peter, it will only get you in trouble . . .

3. **Peter Woit**
   May 3, 2012

   Mike,

   No gossip here, just what's in the public record. The intersection of moneyed individuals and math/physics research is a legitimate topic of interest, one that may be becoming more and more significant for how this research is funded and conducted.

4. **Shantanu**
   May 5, 2012

   Peter, this is probably OT, but couldn't find a place for it.
   anyhow wanted to point you to Andy Strominger's colloquium at
   Berkeley (which was almost same as the Harvard talk you blogged about a year ago).
   See his reply when someone asked him about loop quantum gravity.
   http://physics.berkeley.edu/events/Colloquia/movies/col.streaming.3-12-12.mov

5. **Peter Woit**
   May 5, 2012

   Thanks Shantanu,

   I took a look at the talk, interesting to see what Strominger's point of view is, but there seemed to be nothing new. The hype level was rather high (the way the failure of "string theory is the physics of the 21st century" has been morphed into "this idea from string theory is the harmonic oscillator of the 21st century" is idiotic. People should have learned to dial back the unsupportable hype, not recycle it). The message seems to be that string theory failed at what it was supposed to do, but we should all keep doing it anyway. As for the string theory/LQG fight, again, nothing new.

   Anyone who wants to argue these old tired issues, please resist the temptation.

6. **Wojciech Langer**
   May 9, 2012

   If not recent Krauss' book "A Universe from Nothing" I would not have a clue about Epstein. It is definitely easier to make "A $ from Nothing" 🙂
   BTW: do you have any opinion about this book?

7. **Peter Woit**
   May 9, 2012

   Wojciech,

I haven't read the book, just flipped through it in a book store. I'm just not very interested in that sort of speculative cosmology. That Krauss seemed to be discussing multiverse scenarios as an answer to the idea of God as starting it all didn't look very wise to me. But, again, I haven't read the book. Life is short, I'm way behind on other things, including writing my next posting, about a better book on the same question…

Half Hour to Midnight

May 5, 2012
Categories: Uncategorized

Matt Strassler posts here about a recent panel discussion of phenomenologists talking about the implications of the latest results from the LHC. You can listen to the thing for yourself, and see what Matt has to say at his blog, but here are some things that I noticed from watching the discussion:

- I don't recall string theory even getting mentioned once. The extent to which string theory is now agreed to be thoroughly irrelevant to LHC physics is kind of striking. The few people like Kane claiming otherwise are being ignored as an embarrassment. If evidence for SUSY or extra dimensions had shown up, this would be very, very different.
- Arkani-Hamed is probably the dominant personality in this field, and as Matt mentions, he embodies the conventional wisdom of the subject, expressing it at length and with brio. Back in 2005 he was claiming we would know whether SUSY solves the hierarchy problem within a year of LHC turn-on. Somewhat more than a year after LHC turn-on, in February 2011, he was saying that we'd have to wait until 2020. Now he's putting it differently: it's the "eleven and a halfth hour" for the idea of SUSY solving the hierarchy problem.

  The only remaining hope for this is that there's a light stop, which has so far escaped detection, and gluinos just above the current bounds. He sounds willing to bet against this, and is arguing that the idea may soon be toast, to be finally put to bed as results from better stop searches come in over the next few months. If there's no sign of SUSY in the 2012 data set, it sounds like he's willing to concede that SUSY can't be what stabilized the weak scale.

- On the other hand, he argues that a 125 GeV mass for the Higgs is evidence for SUSY. Here the argument is that such a low-mass Higgs must be an elementary scalar, not the sort of thing you get in technicolor or extra-dimensional models. "SUSY" is here equated with the SM, without comment. I'm not sure what the reason for this is other than the sociological reason that it's the dominant remaining paradigm for BSM physics, I don't see a positive scientific argument.

  The 125 GeV value is also described as uncomfortably inconclusive for the idea of SUSY explaining the hierarchy. It's somewhat too high for this, but not so high as to make it impossible.

- If the SM continues unvanquished at LHC energies, it sounds like conventional wisdom will move to "it still has to be SUSY, even though our main motivation for SUSY is gone, since we don't have any better ideas." Best guess for the SUSY breaking scale will move up to be just high enough to be unobservable at the LHC.
- Clearly a lot of theorists are looking at the failure of the last quarter century of BSM ideas and trying to figure out what else they can work on. The idea of "back to working on QCD" was repeatedly mentioned. Arkani-Hamed has over the past few years dropped BSM work and moved to a radical speculative program about new ideas for QFT based on a different point of view about amplitudes. One of the speakers jokingly accused him of becoming a mathematician. Maybe that's where things are going…

Comments

1. **Interested Observer**
   May 5, 2012

   There are still several SUSY scenarios that have not been explored at the LHC. These include violations of R-parity, spectra where the NLSP decays predominantly to the top quark, degradation of the "missing energy" signals through longer cascades etc. These require more data and effort before anything conclusive can be said about them.

   Of course, these models are not the "simplest" avatars of SUSY – but I doubt anyone would write down the standard model with its hideous yukawa structure if they were only motivated by "beauty" and unknown "top-down" philosophy. BSM physics may well exhibit similar complexity and if so, it would require effort to extract it from the dirty hadron environment.

   The important thing is that the LHC, as a machine, is performing very well and with time and effort, it should be able to probe several of these scenarios. And if anything, the first year of the LHC is a stern reminder of the importance of experiments in physics. For much of the past 30 years, we have unfortunately spent way too much time pursuing ideas solely on the basis of mathematical beauty and consistency. The LHC tells us that this armchair philosophy has not been useful in guiding us to describe physics at 1 TeV, a paltry order of magnitude above the 100 GeV scale explored at LEP. And yet, there are those who insist on extending these philosophies all the way to physics at the Planck scale, without the guidance of experiment. God be with them.

2. **Peter Woit**
   May 5, 2012

   Interested Observer,

   Fine to pursue searches for things like R-parity violating SUSY, as long as one acknowledges there's no good motivation for such things (such models are hideously ugly and explain nothing), and this doesn't interfere with actually important and well-motivated searches. I didn't mention the obvious point repeatedly made by everyone on the panel: anything at all about the Higgs and its decays, as well as anything probing electroweak symmetry breaking in new ways, is incredibly important and should be the highest priority.

   At this point, I fear the best argument for obscure SUSY searches is just that no one has better ideas about where to look for something unexpected. If there are such ideas out there, I hope they're not languishing because of higher priority being given to SUSY searches.

3. **Interested Observer**
   May 6, 2012

   I am not sure what you mean by stating that R-parity violation is "hideously ugly" or that "it explains nothing". R-parity itself isnt particularly natural – it was imposed as a way to protect the proton. At low energies, this is guaranteed as long as either baryon number or lepton number is conserved. There are very reasonable, straightforward ways of breaking lepton number at low energies. Breaking baryon number is harder, but again, not impossible.

   Indeed, R-parity preserving models have the virtue of stable dark matter candidates etc. But, it may well be that dark matter has nothing to do with the hierarchy problem (could easily be axions), in which case R breaking models would be natural and dedicated searches should be done in their aid.

   I am not particularly trying to defend R-parity breaking, but I just want to reiterate that it is now time for people to do the broadest possible searches since the governing theology of the past 30 years ie: the package of the MSSM + dark matter is very much at odds with experiment.

4. **Garbage**
   May 6, 2012

CMB fluctuations were detected five seconds before midnight. There also a theory: inflation, was proposed to solve a 'naturalness' problem (e.g. flatness), and we're waiting for PLANCK to give us some hints to nail it. Perhaps the LHC is like COBE, if the branching ratios stay (believable) away from SM; and like Nima says, we'll need ILC, etc (WMAP…PLANCK) to be able to probe SUSY at the high scale.

SUSY at the 100TeV scale won't be fully natural, neither is inflation for that matter (ask Steinhardt). But truly is the *only game (left) in town*. Yes, this smells like string theory as a theory of QG. Perhaps the universe is fine tuned and that's about it (read CC), or perhaps there's some aspects of tuning we yet don't understand.

I'm not sure I buy Nima's argument that m_H ~ m_Z = SUSY-like. The Higgs was designed to break EW symmetry. We knew G_F, hence the vev. One could have naively guessed: m^2_H ~ 1/G_F, which is obviously too high, so toss in some weak-coupling constant (\lambda) and voila, 100-ish GeV. (In fact, that's a very educated guess, considering e~0.3) It is true SUSY gives us a 'natural' mechanism for m_H at~ m_Z (or smaller, ahem…) but the Higgs still is where we expected it to be, SUSY hasn't 'predicted' anything here so far… All we hope for now is a confirmation the Higgs was found and significant departures from SM-like decay rates. Even a 300-ish GeV 2HDM could do at this point, so there's perhaps *new physics* to be found…

By the way, I'm not sure what you think of Matt S. attitude: "let's try everything". That's not how physics works…

5. **Peter Woit**
May 6, 2012

Garbage,

I just don't see any serious motivation at all for 100 TeV scale SUSY, from string theory or anything else, other than that a large group of theorists have careers invested in SUSY phenomenology and don't want to acknowledge that this hasn't worked out.

Matt S. seems to me obsessed with making sure to be extremely cautious and not acknowledge what has almost surely happened until it is 99.999% all locked up. For instance, he was paying attention to the OPERA FTL neutrinos long after it was clear there was no point to this. I suspect he'll also be working hard on obscure SUSY searches long after this is also a waste of time. The real question is whether there's something more promising the LHC experiments could be looking for. To my mind, the sooner phenomenologists admit that SUSY is a failed idea and devote their energies to other things, the better.

6. **john McAllison**
May 7, 2012

Peter, great that you've posted the link to the audio talk of Arkani-Hamed in 2005, but I'm sure there's an important public talk he gave before 2010 on the expected LHC milestones. It's fascinating because I'm sure he talks about susy being discovered within two years – maybe even before the Higgs. I'd love to be able to see it again.

7. **Peter Woit**
May 7, 2012

John McAllison,

I'm not sure what talk you're referring to, Arkani-Hamed is a favorite invited speaker for talks of this sort, so there have been lots of them, although most not available on-line. The idea that SUSY would be an early discovery, the Higgs much harder and later was always conventional wisdom. The simple reason is that SUSY was supposed to be visible via strongly-interacting partners (gluinos and squarks), which would be copiously produced, even at low luminosities, whereas Higgs production is a weak process. This was borne out, as even the earliest LHC results at low luminosity provided impressive limits on SUSY, nothing about the Higgs until later.

8. **MathPhys**
May 7, 2012

Well, look on the bright side. We surely have quite a bit to learn over the next 5 years. Exciting times in high energy physics.

9. **Kevin Lahey**
May 7, 2012

I just wanted to say I appreciate the summation and straight talk about latest results from the LHC data. Keep up the good work.

10. **Cesar Uliana**
May 7, 2012

Peter,

Could you comment on why Peskin said that the Higgs implies BSM? The talk seemed to me that if the Higgs is at 125 GeV then SUSY is not much of a help.

On a side note, could you explain why the Physical Review Letters has published an article saying that Lorentz force is wrong? It's on the arxiv http://arxiv.org/abs/1205.0096 as well as the PRl page. Really amusing.

Thank you

11. **Peter Woit**
May 7, 2012

Cesar,

I don't remember exactly what Peskin said, and I don't know what he had in mind. Not having a Higgs would definitely be BSM physics, having one is just the SM (if it behaves like an SM Higgs should, that's what everyone wants to find out from the LHC).

I heard about the Lorentz force controversy, but I confess to a lack of either expertise or interest in the subject. There's a nice story about it here

http://www.sciencemag.org/content/336/6080/404.full

but you need another site for an informed discussion.

12. **neo**
May 7, 2012

"MathPhys says:

May 7, 2012 at 10:56 am

Well, look on the bright side. We surely have quite a bit to learn over the next 5 years. Exciting times in high energy physics."

What that be true if the SM holds with no deviations?

13. **Peter Woit**
May 7, 2012

neo,

Even in the nightmare scenario of no SM deviations, we'll learn something. I think that one thing the field is already starting to learn is that the whole SUSY scenario that so much effort and attention went into was a mistake. The LHC's big accomplishments may be confirming the Higgs sector and shooting down some bad ideas which have dominated the subject for too long. Of course, will be even better if it provides hints for where to look for better ideas.

14. **MathPhys**
May 7, 2012

If SM holds with no corrections, then we'll learn that ideas such as naturalness, and our views about radiative corrections, what is large and what is small in perturbative renormalization theory, etc, are too simple minded.

On the other hand, rejecting supersymmetry altogether is too radical to contemplate at this stage (too many papers on the subject, too many good names on the line), and anyone who advocates that viewpoint puts himself in the same position as those who advocate supersymmetry since the late 1970's.

15. **God**
May 7, 2012

The Standard Model works and is fantastic. Its *main* problem is the hierarchy problem (followed by no dark matter, no baryogenesis, inflaton, no grand unification, or including gravity), so if nothing is found at LHC, theorists will have to scratch their heads as to what is going on to keep the EW scale so low. If no low energy SUSY is found (and I agree with Peter, its just about dead already), then I think we have 2 possibilities:
i) anthropics to explain the smallness of EW scale
ii) dynamical explanation that no-one has come up with after all these decades of trying
I think its a little too easy for Peter to just say that theorists should try other ideas…they do all the time…only SUSY seems to fit together nicely. But nature doesn't care about that. thank goodness experiment is pointing the way forward by killing it. so maybe we are left with i)…yikes…

16. **HypeWatch**
May 7, 2012

Oh Lord: hunt is on for 5 God particles
Jonathan Leake, Science Editor
The Sunday Times, 6 May 2012

Rolf-Dieter Heuer, director of the laboratory, has admitted there could be a family of Higgs particles and this could keep his team in work for 20 more years: "There could actually be a number of Higgs particles. In fact there could be up to five."

After smashing 640 trillion protons together, the Large Hadron Collider, Cern's giant particle accelerator, has generated 80-100 possible Higgs bosons. But the number of candidates is slightly more than predicted.

"One possible reason is that there is more than one Higgs," said Heuer. "And if there is more than one, then theory suggests there could be up to five."

This hypothesis is known as "supersymmetry", which predicts each known sub-atomic particle could be partnered by another "superparticle", although these remain undetected.

Heuer remains confident that Cern scientists will soon confirm at least one Higgs. "We will know if the Higgs exists at all by the end of this year. That I can pledge," he said.

17. **Peter Woit**
May 7, 2012

God,

I'm still not convinced that the big problem of the SM is "what keeps the electroweak scale so low?". Low compared to what? It's actually a high-moderate scale compared to any energy scale we actually understand. Without seeing a proton decay, there's no evidence for GUTs and a high GUT scale, and about quantum gravity we know even less.

The Higgs field and the Higgs mechanism for EW symmetry breaking is the most dubious part of the standard model, crying for someone to find a better alternative (and yes, I know people have been trying, hard, for a long time..). But I'm not convinced that the supposed hierarchy problem is the key to anything. It's going to get used by anthropicists as an argument for giving up, but it's not a good one.

18. **Peter Woit**
May 7, 2012

Hypewatch,

Thanks. But why stop at 5? Surely one can find SUSY models consistent with experiment with more than 5 Higgs particles…

19. **God**
May 7, 2012

I don't get your answer. I can only take from your answer that you don't really understand what the problem is; the problem is that the EW scale is sensitive to quadratically divergent corrections. So the SM taken at face value, assumed to be valid up to arbitrarily high energies requires infinite fine tuning. If it is taken to be valid up to, say, the Planck scale, then it has 1 part in 10^30 fine tuning, etc. So it indicates that something should intervene to keep the scale low.
Your point that we don't know much about the GUT or quantum gravity scale doesn't alleviate the problem at all. If the GUT scale is 10^15 GeV or so, as is usually assumed, then we have a 1 part in 10^26 fine tuning if the SM is valid up to this scale. If you are alternatively suggesting that we know so little about GUT physics that the

"true" GUT scale might turn out to be a TeV or so, well great; so that is the wild new physics that we are hunting for! Either way, the existence of the hierarchy problem tells us that crazy fine tuning is required or new dynamics is present. What about this do you not understand?

20. **Peter Woit**
May 7, 2012

God,

My point is that we not only don't know what the right GUT is, we don't even know if there is a GUT or a GUT scale. We really know nothing much for scales above 1 TeV or so. Describing the hierarchy problem as not knowing how to keep the EW scale very small compared to the GUT or Planck scale seems to me a robust way of characterizing the hierarchy problem, but it requires a high GUT or Planck scale, and these things are huge mysteries in themselves.

The quadratic divergence problem comes from applying perturbation theory and a momentum cutoff to the dynamics of the Higgs field at energy scales one knows nothing about. This kind of behavior of elementary scalar fields does look like an indication of something pathological about them at short distances, but there are other reasons anyway to be unhappy with elementary scalars.

Maybe one way to explain my point of view is that I see the bad high energy behavior and sensitivity to the cutoff of elementary scalar fields as maybe part of the problem, but that's not the same as "why is the EW scale so low?" unless you postulate some unknown BSM physics at a scale that acts as an effective momentum cutoff for the perturbative Higgs calculations. We're just barely starting to be able to observe the effects of tree-level physical Higgs field processes. Is there really any indication that Higgs loop processes are there and behaving like elementary scalars? I'm no expert on higher-order SM calculations, and I'd be curious to hear from someone who is, but I'd assume that if these calculations were significantly sensitive to physical Higgs loops, we'd have had a much better fix on the Higgs mass than we did pre-LHC.

21. **Wilson's ghost**
May 7, 2012

I think a lot of the discussion has to come down to understanding renormalization. The SM is renormalizable in the "old" sense and in principle none of this matters as long as the Higgs mass resides within a certain window. On the other hand from the Wilsonian point of view, clearly a Higgs behaves differently. Peter, I'm not clear as to what you mean about higher-order SM calculations. The problem arises from the mass renormalization of the Higgs, not the effect of the Higgs entering into other calculations (e.g. unitarity of WW scattering in the SM). One last thing to point out is there isn't any vodoo associated with the modern view of wilsonian renormalization and effective field theories, we use it all the time often to describe pretty difficult situations in QCD that are naively strongly coupled.

Now if there were no other scales beyond the TeV scale, we could just throw our hands up and say sure, there is no problem with fine tuning. Ironically this is akin to the original prescription for large extra dimensions which bring the Planck scale down to the TeV scale. However, this of course has consequences experimentally which we have seen none of. To do so without a model (more along the lines of what Petr is suggesting) means we should have an infinite set of higher dimension operators suppressed by the TeV scale. Of course we've searched for these things and found nothing of the kind. Now of course one can claim there is no higher scale, but the point is then there are a whole host of OTHER problems that become more pressing. This is also similar to the field theoretic understanding of the fact that if the Higgs weren't elementary there should be a whole host of higher dimensional operators at the TeV scale associated with strong dynamics that we don't see. This really isn't a matter of understanding perturbation theory, it's understanding if there is some pink unicorn theory that solves all the outstanding problems of the SM and leaves no traces elsewhere. There are certainly methods to understanding the implications of strong dynamics, and they all tell us we should be seeing something more if it's not an elementary Higgs. Additionally there definitely are much higher scales associated with the problems in the SM (listed by God) otherwise we would have seen experimental effects associated with flavor and CP violation popping up all over the place.

22. **Ravi K**
May 7, 2012

Hi Peter,

Considering that neutrino masses define a new mass scale we naturally now have high mass scales such as M_W (M_W/m_nu) in the theory. I don't think the argument that we don't know whether large mass scales exist or not in the quantum field theory can be made.

However whether the hierarchy problem needs a solution or not may still be a relevant issue, even if there are large mass scales. If nothing new other than the std. model Higgs is discovered at LHC, I guess we would have to conclude that it does not need one.

23. **MathPhys**
May 8, 2012

Peter Woit writes "The quadratic divergence problem comes from applying perturbation theory and a momentum cutoff to the dynamics of the Higgs field at energy scales one knows nothing about. This kind of behavior of elementary scalar fields does look like an indication of something pathological about them at short distances."

Or maybe there is something pathological about using a perturbative expansion where terms diverge one by one, we know that there are no divergences (the electron has finite mass and charge) so we subtract the divergences, then we complain that the subtracted terms must be finely tuned.

Maybe it is this way of doing things that is pathological in the presence of elementary scalars.

24. **Peter Woit**
May 8, 2012

Ravi K,

The problem of neutrino masses you mention is "why is the electroweak scale so large?, a different one (if you get neutrino masses by a see-saw mechanism you do get a new large scale, but there's no evidence that you need to get them that way). I agree that the problem of fermion masses, with their widely different scales, is about the biggest mystery around. By the way, there's something about this that has always bothered me that maybe someone can explain: supposedly the Yukawa coupling to the top is very close to 1 (an accident or a hint?). Doesn't this ruin one's ability to compute Higgs physics in perturbation theory (since the coupling is not small)?.

25. **Peter Woit**
May 8, 2012

Wilson's ghost,

My point was just that the significance of a one-loop perturbative mass renormalization calculation by itself is unclear: is this just a technical problem with perturbation theory? I'd be more inclined to take it seriously if we were in the situation of QED, where you can physically measure effects (g-2, for instance) that show that loop calculations are correctly giving you something physically observable.

I agree that getting the Higgs out of unknown strong dynamics at the TeV scale introduces its own problems, and certainly don't claim to have any good idea about how to resolve the problems introduced by the Higgs. It just seems like a good idea though to stay clear on exactly what the problems are and whether one comes from something you have solid knowledge of, or from some speculative framework that you understand even less (e.g. I'm not going to worry so much that the properties of the inflaton cause this problem with the Higgs).

26. **Ravi K**
May 8, 2012

Peter,

The neutrino masses break B-L symmetry and there is a fundamental new mass scale associated with this breaking — I think whether it is larger or smaller than M_W is immaterial as the hierarchy argument can always be made for the Higgs sector of the smallest scale.

If the B-L breaking scale is around the weak scale then it would have observable consequences at the LHC….for example a B-L breaking Higgs.

27. **YBM**
May 8, 2012

Some (unrelated to the article) news on the Bodanov Affair:

[Researchers and the threat Bogdanov]

The french scientific community reacted strongly:

[Affaire Bogdanov : lettre ouverte de 170 scientifiques]

By the way, as you know, their main "protection" in the political world has just lost his job.

28. **Ravi K**
May 8, 2012

Is there a renormalizable theory for the observed neutrino masses without having either a high mass scale (like see-saw) or a new light Higgs particle?

29. **Peter Woit**
May 8, 2012

Ravi K.,

I don't know, maybe someone more expert on neutrino masses can comment. But if you do this with a right-handed neutrino and another Higgs field, I don't see why the new Higgs can't be at the electroweak scale. Yes, you wouldn't know why Yukawa couplings were so small, but we already have that problem in the rest of the SM.

I guess that to me it's the Yukawas that are the huge mystery here, it's a huge array of numbers we understand not at all, even though we can measure them precisely.

30. **Ravi K**
May 8, 2012

The new Higgs can be at the electroweak scale — thats what I meant by the light Higgs — EW or lighter — but then it would have observable consequences at LHC and at some point will be ruled out.

The Yukawas hierarchy can be at least understood as being due to chiral symmetries that get restored as the fermion masses go to zero. For example if the electron mass is zero then there is an additional chiral symmetry that will explain this.

This is the 'tHooft criterion for naturalness (electron, up and down quark masses satisfy it) : At any energy scale mu, a physical parameter or set of physical parameters alphai(mu) is allowed to be very small only if the replacement alphai(mu)=0 would increase the symmetry of the system.

31. **God**
May 8, 2012

Peter you still seem confused about basic QFT of scalars. Scalars suffer quadratic corrections to their mass, unless protected by a symmetry. Hence the SM, which does not carry any such symmetry, by itself has infinite fine-tuning. One either accepts this, OR there must be new physics at some scale of the order of a TeV or so. That's what the realization that scalars have quadratic divergences leads to. Its a profound conclusion, and by itself has nothing to do with GUTs, etc. Hence, we should see all sorts of new physics at < 1 TeV, or there is fine tuning. What is it you don't get?

32. **Peter Woit**
May 8, 2012

God,
You seem to just ignore everything I write, in favor of the idea that I must not understand the conventional calculation and the conventional arguments about what it means. For your benefit:

I DO KNOW HOW TO COMPUTE ONE-LOOP FEYNMAN DIAGRAMS AND THAT YOU GET A CONTRIBUTION TO THE HIGGS MASS QUADRATICALLY DIVERGENT IN THE MOMENTUM CUT-OFF.

OK? The question is what to make of this. Is it (and the necessity of "fine-tuning") just an artifact of perturbation theory? This kind of sensitivity to short distances is one of several legitimate reasons to be suspicious that something funny we don't understand is going on with the idea of the Higgs as an elementary scalar. We're just starting to probe the behavior of the physical Higgs field at its lowest excitation energy. I don't see that it's useful to formulate the main problem of the Higgs as "fine-tuning is necessary to keep the Higgs mass small compared to some speculative physics at some high energy scale we know nothing about". That certainly hasn't been a fruitful avenue to pursue so far.

33. **JR**
May 8, 2012

Not an expert but: If you do the one loop calculation with dimensional regularization you get a 1/epsilon pole instead of the quadratic divergence. Is that not a hint that the fine tuning argument is a red herring?

34. **Florian**
May 8, 2012

Hello,

tomorrow's Sueddeutsche Zeitung, a German, high quality newspaper based in Munich is running an interview with Lisa Randall in its Wednesday issue. The occasion is some book of hers (knockin on heaven's door) appearing in German. I'd like to share the highlights of the interview. Since I am translating back something somebody has translated from English this will probably not be literal. Here goes.
[…]
SZ: You are trying to find pictures for these invisible worlds and you have developed a theory after which the universe has 4 space dimensions. Accordingly there could be parallel universes, which are very close to us.
LR: They are probably only 10-31 cm away. But these extra dimensions cannot be observed directly, because they are very small and rolled up.[…]
[…]
SZ: You attach a great importance to the fact, that your theories can be experimentally tested. Your calculations may be tested at the LHC. Can we expect new discoveries?
LR: After initial difficulties the LHC works now incredibly well, so I hope so.
SZ: I would have expected a clear "Yes" here.
LR: The reason why I am not sure is connected to the energies the LHC is able to reach. They might not be high enough to find the particles we are looking for. The once planned American supercollider (SSC) would have reached energies 3 times as high as the LHC, that would have made me more confident. At least we are now seeing clues of the Higgs Boson. But there are a few fascinating theories, like extra-dimensions or Super-Symmetry, in which heavy particles play a role. We could be lucky and see these particles at the LHC. But it could also be that we nearly miss these energies, which can give us the right answers.
[…]
There is other stuff, which is amusing, but I'm too lazy to translate that. The 10-31cm and the almost caught particles are the best part anyway. It would be amusing, if it wasn't so sad. Sorry for being slightly off-topic, Peter.

35. **JustDisWonce**
May 8, 2012

@God,

you are parroting the standard story used to sell SUSY for decades. That this story is so ingrained in the community may reflect great marketing more than great physics. While this story may turn out to be correct, the data thus far suggests otherwise. If nothing is found to stabilize the weak scale, "the community" is presumably thinking incorrectly about the matter. Time will tell. (These last statements are not controversial.)

From your comments, my guess is that you have not studied the corrections to the Higgs mass in detail (as I find with a number of my "expert" colleagues), as the matter is not as trivial as you imply. Study and figure out your own answers to questions like the following: How does this "problem" manifest itself in different regularization schemes? What is the physical meaning/origin of this difference (if any)? Which regularization scheme is "better" — does it even matter? Why are some schemes better in eg SUSY models; ie why do we choose regularization schemes in SUSY models that preserve the SUSY (an obvious one)? Does the answer to this question have any implications for the regularization of the SM (or implementation thereof)? What about if you send the bare Higgs mass to zero — does the way in which scales manifest via the trace anomaly alter your answer (see Bardeen's work)? Does the answer to these questions depend on whether there is a UV scale describing QFT-able physics like M_GUT? If there is no M_GUT, does the answer depend on the nature of quantum gravity (this one is harder)?

If you can answer these (except the last one, of course), you will understand what Peter is saying —- if not it would take more time to explain than any self-respecting person would have to spare.

An aside: if more people spent time pursuing original research directions in the '90s, rather than telling everyone else that SUSY was the only solution to the hierarchy problem, there is no apriori reason why things like ADD, RS and Little Higgs could not have been discovered earlier (no offense meant to the very insightful folks who eventually did make these discoveries). These statements alone are a good reason to be wary of anyone who tells you string theory/SUSY is the only hope, rather than encouraging diverse research programs to explore as many possibilities as are conceivable.

@Peter,

I congratulate your efforts and applaud your patience in responding to these same comments over and over again. Though no doubt tiresome, it is a valuable service.

@Ravi

There are a number of eg radiative models that generate nu mass without invoking high (beyond ~ TeV) scales; these are renormalizable and easily accommodate the data. The problem with neutrino mass is not that its hard to explain (nor that the existence of the scale m_nu implies a hierarchy problem), rather it's just too easy to write a renormalizable theory that explains the data, is completely natural, and has new physics at (almost) any scale you care to mention. This is what makes nu theory somewhat boring (along the lines of Jester's recent post); only experiment can shed light on the matter, but the scales may or may not be accessible and the couplings may or may not be large enough to allow detection. Also note that nonzero m_nu does not imply that B-L is broken; neutrinos can just as easily be Dirac particles and nu mass will be competely natural.

If one adopts the view that the SM Higgs mass is tuned, then this, at least, is a much more difficult problem to solve (thus the high number of original nu-mass-models compared to the number of original solutions to the hierarchy problem).

36. **pah**
May 8, 2012

I'd also be interested to hear an answer to JR's question above.

37. **Anonyrat**
May 8, 2012

pah, JR,

My memory is failing – I was sure I had a copy of this 1975 book: "Dimensional Regularization and the Renormalization of Quantum Field Theory", John C. Collins, but I can't find it. I'm about just as sure that in this book J.C. Collins worked out an example of a toy model with two scalars, one light and one heavy, and showed that even with dimensional regulation, the light mass was inexorably drawn to the heavy scale unless there was fine-tuning. But my memory is suspect.

38. **Anonyrat**
May 8, 2012

I'd appreciate being reassured that my memory is fine. 🙂 Anyway, if there is a GUT, then we have a hierarchy problem.

http://arxiv.org/pdf/0708.3550v1.pdf

"In fact, the structure of divergences does depend on
the regularisation scheme. One can use also the scale- independent regularisation, such as the dimensional reg- ularisation of 't Hooft and Veltman [48]. In this scheme the renormalization of parameters is the multiplicative one and thus there is no difference in removing the divergences from dimensionless parameters such as gauge coupling or dimensionfull parameters such as the mass of the Higgs boson. However, inspite of these specific features of the dimensional regularisation the conclusion about fine tunings remains intact [49]: even in this scheme to have a field theory GUT with two or more well separated scales one has to tune a number (varying from 1 in SUSY GUTs to 14 in non-SUSY GUTs) of terms to achieve the hierarchy of masses."

Reference 49 is:
[49] S. Weinberg, Phys. Lett. B 91 (1980) 51.

39. **God**
May 8, 2012

@JusDisWonce,
I am not parroting anything about SUSY, etc. Personally, I doubt low energy SUSY is right – i said that in my first post, see above, so your discussion is off target.

@Peter,
Sorry, but what you are saying doesn't make sense. The basic principles of effective field theory guarantees that the SM extrapolated to high energies is highly fine-tuned. If you are disputing this, you are disputing the core principles of EFT. If so, then please state so. You will be wrong, but you should at least state so clearly, so I understand the claim you are making. The alternative to this fine tuning is some form of new physics; I do not know what form the new physics will take. These arguments are completely general, they have nothing to do with GUT theories, etc.

40. **MathPhys**
May 8, 2012

Anonyrat,

Your memory is okay.

In fact, the first paper that gives an explicit computation in which the 'technical fune tuning problem' shows up is by E Gildner (a PhD student of S Coleman) from the late 70's, and there he uses dimensional regularization.

41. **MathPhys**
May 8, 2012

On a different matter, can an expert confirm or deny that one can compute scattering amplitudes, even for massive scalars, and that the results are finite and no divergences show up anywhere in the intermediate steps?

42. **Garbage**
May 9, 2012

"I think that one thing the field is already starting to learn is that the whole SUSY scenario that so much effort and attention went into was a mistake."

I'm not a big fun of susy at the EW scale myself either; I do believe though susy is too pretty for nature to pass on it at the GUT scale. However, susy at 100TeV is still partially motivated. Nature may have a split spectrum whether we like it or not. If the departures form the SM decay rates withstand further scrutiny, then high-scale susy will be by far one of the most natural options to look for an explanation. Now, if we find *just* the Higgs with 1-2 sigma-ish deviations from SM here and there, then is another story… Nima will tell you we need to measure the electron's dipole moment, gotta get down to 10^{-31} (current bounds at 10^{-28}). The cool thing is that there's still a lot of physics to be done and understand before we accept the universe is fine tuned for good…. (we have almost done it with lambda…)

43. **SpearMarktheSecond**
May 10, 2012

How about mentioning the theorists who long ago (even prior to, say, 1997) became disenchanted with SUSY at accessible energies, and sort of moved on? I can think of two easily. It would kind of compensate the negative undertones.

One of the two is Tini Veltman…. although the quote is from 2004, I can remember him saying this stuff through the 1990's….

http://www.timeshighereducation.co.uk/story.asp?storyCode=187373&sectioncode=1

"Anyone who thinks that string theory has, in the meantime, shown the way that particle physics is going gets short shrift from Veltman. Supersymmetry and string theory are "figments of the theoretical mind", he writes."

44. **MathPhys**
May 11, 2012

SpearMarktheSecond,

Veltman says these things, and in stronger terms, since the early 80's.

45. **Anonyrat**
May 11, 2012

MathPhys,
What were Veltman's stated reasons in the early 80s for giving short shrift to supersymmetry?
Thanks in advance!

46. **Peter Woit**
May 11, 2012

Veltman was far from the only one to be skeptical about supersymmetry. Among the other Nobelists I can think of who never showed much interest would be Glashow and 't Hooft. Actually I suspect that faith in SUSY has always been a minority viewpoint among theorists (although the minority has been much louder than the majority). For some evidence about this, see how people lined up back in 2000:

http://www.staff.science.uu.nl/~hooft101/susy_wager_2000.gif

47. **MathPhys**
May 11, 2012

Anonyrat,

I didn't hear him give any reasons. I only heard him say

"Gordy, if they discover supersymmetry, I'll eat my hat",

then he mimicked eating his hat.

48. **MathPhys**
May 11, 2012

That's a significant historical document you've linked to above, Peter. I haven't seen that before.

49. **Ravi K**
May 11, 2012

@JustDisWonce

I will try and give a more general argument than what I gave before. For masses of charged leptons such as the electron you just need the Yukawa couplings and no new term in the Lagrangian of mass dimensions (need only the usual std model Higgs mass term). However for the neutrino mass you can't get from Yukawa terms alone and the std model Higgs — you need to introduce some other term with mass dimensions no matter what the model. If this mass term is much more than the electro-weak scale then even in Peter's sense there would be a hierarchy problem as his artifact argument crucially depends on there being only one mass scale in the QFT. On the other hand if it is at the weak or TeV scale or lower then it would inevitably lead to new physics that the LHC or a future generation of colliders should see. If there is no new physics and only a std model Higgs then you will essentially end up with two different mass scales in the QFT — one to explain the neutrino masses and the other the weak scale. Even as theories like SUSY getting constrained so are all other new physics theories at or near the weak/TeV scale. If nothing new is found then fine-tuning must exist even in the sense Peter is talking about where you regard the cut-off as an artifact.

50. **Trulo**
May 12, 2012

Peter, the document you link to says that "The party of winners organizes a meeting of all involved in this wager not later than in June 2011" in which the cognac bought by the losers of the bet will be drank. I'm curious to know if that meeting has already taken place, or if it has at least been announced.

51. **Peter Woit**
May 13, 2012

Trulo,

The LHC turn-on ended up being several years later than expected back in 2000, so I think this bet may have been renegotiated to extend the time-line. No cognac purchased by losers yet as far as I know.

Phenomenology 2012

May 8, 2012
Categories: Uncategorized

This week at the University of Pittsburgh the Phenomenology 2012 Symposium has talks reviewing the current situation in particle physics phenomenology. Not much new, but there is one plenary talk on string phenomenology, Cumrun Vafa's Stringy Predictions for Particle Physics. Mostly this deals with Vafa's ideas about F-theory "predictions" for the fermion mass and mixing matrices. Quite a few assumptions and rather little string theory (the predictions are "stringy") goes into this, and except for the one recently measured neutrino mixing angle, these are all postdictions, not predictions. I suspect that most string theorists are no more sold on these as predictions of string theory than they are on Kane's Higgs mass prediction. For example, Vafa's colleague Andy Strominger in recent talks (and not so recent ones) gives string theory an "F" in the area of making unambiguous testable predictions, and I assume he's well aware of Vafa's work.

Pre-LHC, Vafa had been claiming F-theory predictions for SUSY at the LHC (see for example here, here and here). The most dramatic one, the focus of the Harvard Gazette story, was for a stable stau. One of the papers linked to has various detailed calculations for what such an stau would look like, with typical masses around 200 GeV.

At the conference on Monday, this talk gives recent CMS results relevant to such an stau, listing new mass limits of 314 GeV for a "cascade-decay" scenario, 223 GeV for a "pair-produced" one. Vafa only briefly mentions SUSY at the end of his talk, with his final slide "We will wait to see if SUSY plays any role at the weak scale!" I'm guessing he's getting resigned to the idea that the answer is probably No.

Update: Also on the No SUSY news front this evening, there's a preprint entitled Should we still believe in constrained supersymmetry? analyzing the current situation with one popular version of SUSY, the CMSSM, which concludes:

> We find that LEP and the LHC strongly shatter our trust in the CMSSM (with M_0 and M_{1/2} below 2 TeV) reducing its posterior odds by a factor of approximately three orders of magnitude.

Comments

1. **M**
   May 9, 2012

   Similar statements about the naturalness problem of CMSSM SUSY had been made already after LEP, see

   http://arxiv.org/abs/hep-ph/9904247
   "The Bayes theorem says how much one must reduce its confidence in SUSY after knowing the LEP2 results"

   After LEP the confidence dropped to 5%. Now down to 0.1%.

   Nowadays pointing out that SUSY & strings have problems is like shooting on the red cross.

2. **paddy**
   May 9, 2012

   a)Theorists putting such effort into a Bayesian analysis to quantify what we "know" may be quite a sign of the times–and not a good one.
   b)After reviewing the C. Vafa cartoonical slides, I must say that I prefer xkcd.
   c)In some other universe there is an S. Glashow who is not quite as nice as the one in this bubble (who decades ago taught stupid me Group Theory without making me feel stupid). This not-so-nice Glashow multiverse-twin is rolling his eyes and saying I told you so.

Why Does the World Exist?

May 9, 2012
Categories: Book Reviews

With a lot of attention these days (see here for instance) going to an argument between philosophers and physicists about the "Why is there Something rather than Nothing?" question, this is the perfect time for Jim Holt's new book Why Does the World Exist? An Existential Detective Story. While the argument between Krauss, Albert and their fellow combatants was mind-numbingly dumb, boring, narrow, petty and ill-mannered, Holt's discussion of the topic is brilliant, entertaining, and wide-ranging as well as generous in spirit to all points of view. The only unfortunate thing here is that the book won't be out until July. I've checked with him though, and he doesn't mind if I write about it now, since I just read an advance copy. In July I'll try to remember to repost this.

Holt first sets the stage by explaining some of the history of the question "Why is there Something rather than Nothing?" and why it's one he finds compelling (as well as explaining why one might reasonably think otherwise…). He first ran across the question as a high school student fascinated by Existentialism and trying to read Heidegger's *Introduction to Metaphysics*.

(Personal digression:

Around the same time I was also trying to read that book as a college freshman. I just found my old copy, where I underlined lots of things that seemed of significance at the time, as well as putting exclamation points around the paranoid nationalist ravings that appear at one point. I soon decided Heidegger wasn't for me, and some years later learned of a personal reason to dislike him, see this from the Wikipedia entry on Heidegger and Nazism:

> *Heidegger also denounced or demoted several colleagues for being insufficiently committed to the Nazi cause.*
>
> *On September 29, 1933, Heidegger leaked information to the local minister of education that the chemist Hermann Staudinger had been a pacifist during World War I. Heidegger knew this would cost Staudinger his job. The Gestapo investigated the matter and confirmed Heidegger's tip. Asked for his recommendation as rector of the university, Heidegger secretly urged the ministry to fire Staudinger without a pension.*

Hermann Staudinger was my great-uncle (on my father's side of the family). Despite Heidegger's efforts, he managed to keep his job, survived the war, and went on to win a Nobel prize. I never really got to know him since he died when I was rather young, but got to know very well his widow, my great-aunt Magda. Decades later, she was still quite upset by the Heidegger business.

End of personal digression.)

Holt then moves on to contemporary thinker's takes on the subject, including entertaining descriptions of his trips to visit some of them, starting with some philosophers. These include Adolf Grünbaum who takes the position (to which I'm sympathetic…) that this is a pseudo-problem, and derisively refers to worries about Nothingness as the "ontopathological syndrome". Another visit is to Richard Swinburne at Oxford, who goes for the "God did it" explanation.

The first physicist he visits is David Deutsch, also at Oxford, and Holt's description of the experience and account of their conversation is quite wonderful. As you might expect, lots about the deep significance of quantum physics and Many-Worlds. After a discussion of Robert Nozick and his principle of fecundity ("all possible worlds are real"), it's on to Alexander Vilenkin and the cosmological multiverse mania that has gotten so much attention from physicists in recent years. Here the sort of "something from nothing" that Krauss was discussing in his recent book comes into play.

The most intellectually powerful figure Holt talks to might be Steven Weinberg, who has this to say about the multiverse:

> "Vilenkin is a really clever guy, and these are fascinating conjectures," Weinberg said. "The problem is that we have no way, at present, of deciding whether they're *true* or not. It's not just that we don't have the observational data – we don't even have the *theory*."

and this about string theory:

> When I brought up string theory, a melancholy strain became detectable in Weinberg's voice.
> "I was hoping that with string theory things would fall into place much more rapidly than they have," he said. "But it's been rather disappointing. I'm not one of those people who bad-mouth string theory. I still think it's the best effort we've made to step beyond what we already know, but it hasn't worked out the way we were expecting it would."

About Susskind's claims that the Many-Worlds and string theory multiverses may be one and the same, Weinberg is having none of it, describing the two ideas as "completely perpendicular" and saying:

> "I found it puzzling too," he said. "I've spoken to other people about it, and they don't understand it either"… "I don't agree with Susskind on that," Weinberg told me, "and I don't know why he said it."

The discussion with Weinberg brings up the whole question of a "Final theory", a truly unified fundamental theory of physics, and what its significance for the question of existence might be. After Weinberg, Holt describes a meeting with Sir Roger Penrose, and explains Penrose's "Platonism", the philosophical point of view that mathematical objects actually are real things that exist (in some sense…). Penrose attempts to also bring the question of consciousness into this, which seems to me a mistake, but the questions raised here about the relation of mathematics and our fundamental ideas about the physical world are dear to my heart. To the extent that to me there's a sensible question behind "Why is there Something rather than Nothing?", it's bound up with this great mystery of the origin of the fundamental laws of physics. Mathematics and physics have a completely unexpected and still not understood congruence at their deepest levels, and this mystery seems to me not only a very real one, but one that we can hope to further elucidate. I realized from Holt's discussion that maybe my own mystical views on this subject are best described as "Pythagorean". While he does a reasonable job of raising some of these issues, to me he dismisses them too quickly in favor of moving on to other topics much less worth taking seriously. But I would think that, wouldn't I?

The later part of the book reverts to the philosophers. For his discussion with John Leslie about "axiarchism", you can watch the two of them here on Bloggingheads. His final philosophical encounter is with Derek Parfit, in the imposing venue of All Souls at Oxford. Novelist John Updike is his last interviewee, and Updike also isn't so happy with string theory:

> "But this whole string theory business… There's never any evidence, just mathematical formulas, right? There are men spending their whole careers working on a theory of something that might not even exist."

Holt ends the book on a personal note, telling the story of the death of his mother and his return to the place he grew up. Throughout the book, he weaves in accounts of time spent in Paris, reading at Sartre's Cafe de Flore, and wandering the city contemplating aspects of his great philosophical question, as well as life in general. Some might find this distracting and not so serious, but I enjoyed those parts of the book a lot. Of course, this may largely be due to the fact that I'm a sucker for Paris and know well and love the locations he was describing.

If you have even the slightest interest in the "Why is there Something rather than Nothing?" question, be sure to get yourself of a copy of this wonderful book when it comes out.

My interest in the question has always been rather minimal (I got grief from some of my commenters recently for my dismissive attitude on the topic), but this didn't keep me from getting a lot out of the book. It's philosophy of a high level, pursued in an unusual and personal manner, and it's a pleasure to follow along with the author as he tells a fascinating and thought-provoking story.

Comments

1. **pakri**
   May 9, 2012

   I believe this is nothing but a word-play. None of the writers,
   including Sartre, has had deep insight. Great physicists and
   mathematicians should shun the subject.

2. **Peter Woit**
   May 9, 2012

   pakri,

   That's a reasonable take on the question, but I'll just point out that Weinberg and Penrose disagree with you and thought the subject worth discussing.

3. **Bobito**
   May 10, 2012

   "Why Does the World Exist?"

   Bad question because it suppose an ill-defined alternative.

4. **anton ymous**
   May 10, 2012

   >> Heidegger knew this would cost Staudinger his job.
   >> [..]
   >> Despite Heidegger's efforts, he managed to keep his job

   This does not really fit together?

5. **Neto**
   May 10, 2012

   "Mathematics and physics have a completely unexpected and still not understood congruence at their deepest levels"

   You can use math to model anything that is scientific reachable. How the use of math to describe natural physics can be "completely unexpected"?

6. **Aidyan**
   May 10, 2012

   Everyone asks why things 'exist' taking for granted that we know what 'existence' is. But a honest thought about it would reveal that we really don't know.

7. **Peter Woit**
   May 10, 2012

   anton ymous,

   I forget the details, but from what I remember Staudinger was forced to resign his position because Heidegger denounced him to the Gestapo and suggested he be forced out of his job and have his pension taken away. Staudinger did manage to get reinstated, after Heidegger and others in power realized that this would make the university look very bad internationally.

   Neto,

   This is just the usual "unreasonable effectiveness" argument of Wigner, which has accumulated much more evidence for it since his time.

8. **Jeff McGowan**
   May 10, 2012

   I must say I find it amusing that this is what physicists are busy talking about. Nothing wrong with philosophical discussions, or literary ones, but physics? Perhaps it comes from some deep seated desire for a theory of everything, which unfortunately (or maybe fortunately) Godel proved was clearly impossible quite some time ago. You would think that someone as seemingly smart as Weinberg would understand something that basic. And Peter, gotta say I think you're wrong and Penrose is right, consciousness is at the heart of it somehow. Check out Newcomb's paradox.

9. **Igor Khavkine**
   May 10, 2012

   About this "unreasonable effectiveness". Some time ago, I watched this video lecture by cognitive scientist (and apparently ex-logician) George Lakoff, recorded at the Fields Institute. In it, he briefly addresses this observation in the question period. His take on it was interesting enough for me to get a copy of his book on the cognitive science of mathematical thought, which is unfortunately still collecting dust in my to-read book pile.

   In my imperfect recollection and rendition of his position, they key to taking the mystery out of this question is the observation that mathematical thought is carried out by the brain, which resides in the body, both of which are physical systems following physical laws. The application of the "(unreasonable) effectiveness" to particular physical process then simply corresponds to the existence of a translation from this process to the physical processes of mathematical thought. These translations Lakoff calls *metaphors*, I believe. And they are supposed to be documented in detail in his book.

   The above argument would answer a "how" question. "Why" questions are always trickier. However, I think some light could be shed on the corresponding "why" question by answering a related one: When is it possible for any part of a physical system (the universe) to be mapped to (be simulated by) another part of the same physical system (the brain)? Personally, I have no idea what the answer to this question would be, but it seems to me already somewhat better defined than Wigner's original remark. It even has shades of Turing universality, which has been studied in some detail.

10. **Peter Woit**
May 10, 2012

Jeff and Igor,

I don't think either Godel's or Lakoff's arguments are relevant. We're actually very close to a unified theory, with the questions we don't understand seeming to have no connection to the problems Godel's theorem raises. The fact that a very non-obvious to the human mind structure (the Dirac equation) explains so much of reality, at the same time that it appears non-obviously in the deepest parts of mathematics (e.g. it's the fundamental class in K-homology) seems to me to show that something mysterious is going on, and it's something that has nothing to do with the structure of the human brain, for which both parts of the story are quite alien. This stuff is very difficult for the human mind to understand, it takes quite an effort, and only is successful due to the high adaptability of human thought processes. It's quite possible, and Holt mentions this possibility, that there is some deep structure going on here, that is as incomprehensible to our minds as calculus is to the mind of a dog. But we have somewhat better abilities than the dog to learn new things.

This is getting kind of off-topic though…

11. **Jeff McGowan**
May 10, 2012

Peter,

No question one might get a "unified" theory, my point was that a "complete" theory won't ever happen. "Theory of everything" implies complete, at least to me. Now I'll stop…

12. **billy hudson**
May 10, 2012

@JeffMcGowan

If you haven't already seen it, you may be interested in the book "Godel's Theorem: An Incomplete Guide to Its Use and Abuse" by the late Torkel Franzén.

13. **Igor Khavkine**
May 10, 2012

@Jeff, I'll second billy's suggestion.

@Peter: The question you seem to be posing is I think different from the original one posed by Wigner. In that case, I agree that Lakoff's argument is not particularly relevant.

14. **Jeff**
May 10, 2012

Billy and Igor,

While I'm sure the book is enjoyable, if I want to revisit Godel I'll dig out my notes on Turing's proof, which I always thought was one of the prettiest things in mathematics (despite being a geometric analyst myself…) And Lakoff was never a logician, his Ph.D. is in Linguistics.

15. **Allan Rosenberg**
May 10, 2012

I'm not so sure that the philosphers are entirely convinced that there is something rather than nothing. True, Descartes did argue, cogiter ergo sum, but that leaves open the precise definition of the following words: cogito, ergo, and sum, all disputations subjects in contemporary philosophy. Until those definitional issues are resolved, it's hard to see how we'll reach a philosophical consensus as to whether anything exists.

16. **Christian Takacs**
May 10, 2012

"Why does the world (universe) exist?" is begging the question. Kinda like, When did you stop beating your wife? The question presupposes that the universe didn't exist before it did, and unless you can prove former first, you have no latter 'creation ex nihilo' to debate. This is a good argument for why philosophy matters, if physicists would employ basic philosophy and logic before running off with silly 'interpretations' of limited data, (e.g. Copenhagen intrepretation) fewer such houses of cards would be assembled… and have so much prestigious math piled upon them. Heisenberg himself was actually very philosophical (not in a good way)… and a Nazi, as well as working for the German's on the atomic bomb. His colleague and friend Niels Bohr went to some effort to hide this glaring little truth, probably so his own career wouldn't be tarnished. So much for the glory of quantum mechanics.

17. **billy hudson**
May 10, 2012

Jeff,

Do your notes analyze the applicability of the Incompleteness Theorem to "The Theory of Everything"? Section 4.4 of has a nice analysis. From page 88: "The basic equations of physics, whatever they may be, cannot indeed decide every arithmetical statement, but whether or not they are complete considered as a description of the physical world, and what completeness might mean in such a case, is not something that the incompleteness theorem tells us anything about."

18. **Yatima**
May 10, 2012

> silly 'interpretations' of limited data, (e.g. Copenhagen intrepretation)

What am I reading. It just becomes silly if esoteric weirdos start to confuse "observer" (which can be an LHC detector) with "conscious observer" (an ill-defined, slightly icky concept if there ever was one). Other than that, it apparently remains the only one that makes consistent sense. For people who accept that probability theory can be extended, that is. Others write endless philosophical drivel on this, which may be necessary to bring in the bacon, granted.

http://arxiv.org/abs/math-ph/0002049 "It took some time before it was understood that quantum theory is a generalisation of probability, rather than a modification of the laws of mechanics. This was not helped by the term quantum mechanics; more, the Copenhagen interpretation is given in terms of probability, meaning as understood at the time. Bohr has said that the interpretation of microscopic measurements must be done in classical terms, because the measuring instruments are large, and are therefore described by classical laws. It is true, that the springs and cogs making up a measuring instrument themselves obey classical laws; but this does not mean that the information held on the instrument, in the numbers indicated by the dials, obey classical statistics. If the instrument faithfully measures an atomic observable, then the numbers indicated by
the dials should be analysed by quantum probability, however large the instrument
is."

19. **Jeff M**
May 10, 2012

Billy,

That statement strikes me as a bit of a cop out. "What completeness might mean in such a case" allows for pretty much anything, no? Not saying he's wrong, since I have no doubt that different people might mean very different things by "completeness" in relation to a physical theory, but to a mathematician completeness is very clear. Perhaps that was really my point – when people start talking about "final theories" it isn't at all clear what they mean. Then again, perhaps I'm just being a nit-picky mathematician 🙂

20. **paddy**
May 10, 2012

And perhaps I am being a nit-picky physicist: "shut up and calculate".

21. **Anonyrat**
May 10, 2012

"Why is there something instead of nothing?" is an existential question that is meaningful only in the context of religion. This can be seen by thinking about what would constitute an acceptable answer. The prototypical answer to an existential question requires a God that is outside of and independent of the universe, and then "there is nothing" has meaning. Religion seems to have faded away, but it still lingers on powerfully. This shown by the number of people who think "why is there something instead of nothing?" is still meaningful.

22. **billy hudson**
May 10, 2012

Jeff,

I'm sympathetic with paddy's statement. In my experience, mathematicians (outside of the field of logic) are no more concerned with completeness than physicists. All of our calculations take place in an incomplete theory, but this doesn't affect anyone's practice of mathematics/physics (I'm of the camp that has a hard time distinguishing between the two). In any case, we've strayed off topic so I'll leave it there. Cheers . . . 🙂

23. **Friend**
May 10, 2012

"Why is there something rather than nothing?" Perhaps this is just another way to ask what is the purpose of the universe? Or, what is the most general principle that it displays as a whole? Then the answer to those question might answer the "why" of its existence. For example, one might venture to say that the most general thing that the universe does is display a set of consistent facts. Then that could be "why" it exists… to display a consistent set of facts.

24. **Bourgeois Nerd**
May 10, 2012

Thank you, Peter, for the review. The book sounds as great as I'd hoped! Can't wait for July!

25. **Fabien Besnard**
May 11, 2012

Thanks for the very interesting review, Peter.

26. **Peter Burnett**
May 12, 2012

Consider the intellectual distance between the knowledge you possess and the problem you're trying to solve. 'Why is there anything rather than nothing' is the question with the greatest distance. But let's not give up or ignore it just because it's a long trip. There must be a rational answer to this problem, if not, we might as well give up physics and spend the rest of our lives with our hands down our shorts.
The best comment I've come across is that there are three possibilities: either there is something, or there is nothing, or they are both the same.

27. **Tony Smith**
May 12, 2012

Since I do not have an advance copy of the Jim Holt book,
my question is whether/how the book might differ with respect to a couple of points in his November 1994 Harper's article "Nothing Ventured – A bold leap into the ontological void" in which he said:

1 – about Hegelian dialectic:
"… At the beginning of Hegel's famous dialectic is the assumption that
the Absolute is Pure Being.
But Pre Being is totally indefinite; it has no qualities; it is utterly empty. It is the same as Pure Nothing.
You can't have one without the other; they are dialectical twins.
And yet, inasmuch as they are also contraries, they can't coexist very happily.
Something new must be found that reconciles and supersedes them.
And that turns out to be: Becoming!
Becoming is what happens when Being is on the verge of passing into Nothing–or vice versa.
Thus does the Hegelian dialectic get merrily underway, eventually yielding up human history and culture in all their variegated splendor. …".

2 – Jim Holt's personal argument, thought of while shaving:
"… If the laws of physics come into being along with the universe, then they can't explain it.
If they exist prior to the universe, then there is nothing to account for their existence …
That is the dilemma of the nothing theorists.
… there is no need to get impaled on its horns …
a … way of showing why there is something rather than nothing … goes like this. Suppose, for the sake of argument, that nothing existed.
Then, in particular, there would be no laws. …
If there were no laws, then everything would be permitted.
If everything were permitted, then nothing would be forbidden.
Therefore, if nothing existed, nothing would be forbidden.
Therefore, nothing, if it existed, would forbid itself.
Therefore there must be something. …".

Tony

28. **Peter Woit**
May 12, 2012

Tony,

Sounds like the sort of thing that is in the book, in similar form. Holt is no fan of Hegel, but write a bit about him, along the lines of what you quote.

29. **Friend**
May 12, 2012

Peter,

Since you have an advanced copy, are you allowed to write a review on amazon.com?

30. **Tony Smith**
May 12, 2012

Just for fun, here is a silly thought about the Hegel quote:
Absolute Something = PURE BEING
Nothing = PRE BEING
Absolute Something – Nothing = PURE BEING – PRE BEING = U
So, U = YOU are what distinguishes Something from Nothing.
(?a version of cogito ergo sum?)

Tony

31. **Peter Woit**
May 13, 2012

Friend,

I think Amazon has a program to get people advance copies of books in exchange for writing reviews. I'm not part of that, my copy didn't come from them. Whether or not I could post a review there, I think I'll stick to just doing this on the blog.

32. **arjun**
May 13, 2012

Dear Peter,

Could you please expand on this statement that you make:

"The fact that a very non-obvious to the human mind structure (the Dirac equation) explains so much of reality, at the same time that it appears non-obviously in the deepest parts of mathematics (e.g. it's the fundamental class in K-homology) seems to me to show that something mysterious is going on …"

33. **Peter Woit**
May 13, 2012

arjun,

This is a very long story. I think there is quite a bit about it in my book.

An oversimplified way of stating it is that the most powerful theorem telling you about the relation of analysis, geometry and topology is the Atiyah-Singer index theorem (and you can abstractly express it using K-theory). In some sense it says that the Dirac equation is a "generator", everything else can be written in terms of that. Atiyah and Singer actually rediscovered the Dirac equation for themselves in the course of their work on the index theorem.

34. **Tom Whicker**
May 14, 2012

I think we have a strong aversion to the contemplation of non-existence, because
contained there-in must be the admission of our own personal non-existence. It is
very real and soon to happen to each of us. We try to laugh or to say it is uninteresting, to say there is no useful math or physics there, which is true. But
the topic itself is deeply disturbing.

35. **Anonyrat**
May 14, 2012

Luboš Motl:

"We use the term string theory for everything that is connected by equations to what we previously called string theory. That's why we will simply use the term "string theory" for every consistent corner of quantum gravity we may discover in the future. With this convention – which was adopted in recent decades – the uniqueness of string theory as a consistent theory of quantum gravity is a tautology. "

Elevated from the comments [here.](#)

36. **Peter Woit**
May 14, 2012

Anonyrat,

Thanks, but the philosophical issues raised by the existence of Lubos Motl and the fact that prominent string theorists guest post on his blog and put his endorsement on their books are greater mysteries than that of existence in general, and best left for another time…

37. **paddy**
May 14, 2012

PW:
Your LM post left me laughing til I went into a coughing fit. 'Twould be indeed better if LM were capable of seeing the humour.

38. **prianikoff**
May 15, 2012

Hegel was an idealist.
A materialist would define nothing as the anwer to the question.
"What's a Greek urn?"

39. **clauderains**
May 15, 2012

Why does anything exist? Because it's easier than not existing. And we exist within it to satisfy a statistical outlier on the universe's ability to 'spontaneously' create toasters…

40. **tulpoeid**
May 16, 2012

A little bit off-topic (but only a little bit), this is a chance for a comment I've meant to leave here for some time now. In case you (or any readers) enjoy good science-fiction, you might want to have a try at Neal Stephenson's Anathem. You'll find it touches on issues related to this blog!

41. **Dave Miller in Sacramento**
May 18, 2012

Peter,

Can you suggest any readable but mathematically detailed discussions of your point about the Atiyah-Singer Index Theorem?

I managed to figure out for myself (maybe "guessed by myself" would be more accurate!) this idea that the Dirac equation is at the base of the Index Theorem.

But, I cannot find a discussion of the theorem that actually explains this clearly and in detail. I am in the frustrating position that I understand (I think) the details of various proofs (Sobolev spaces, Chern characters, etc.) without really understanding the proofs.

And, thanks for the book review. Unlike you, I suspect the question is meaningful, but I must admit to a sneaking suspicion that it is not. Needless to say, I found Weinberg's comments the most sensible among those you quoted.

Dave

42. **Peter Woit**
May 18, 2012

Dave,

One good reference is these lectures by Nigel Higson and John Roe
http://folk.uio.no/rognes/higson/Book.pdf

Something much more general is this

http://web.me.com/ndh2/math/Papers_files/Higson,%20Roe%20-%202006%20-%20The%20Atiyah-Singer%20index%20theorem.pdf

43. **Dan**
May 18, 2012

The idea of origin brings with it a simple but profound bit of baggage: causality. Simply put, "something" must exist, and be transformable into "something else". In this context, the problem arises because we must now substitute "nothing" for "something" with the absurd transformation of "nothing" into "something else". How is "nothing" transformable? It is simply not logical.

There are various ways to try to beat this problem:

1. There never was a state of "nothing". The universe (as a transformable substrate) has unbroken temporal existence. It is either "temporally closed" (going in circles) or "infinite" (going going going …..)
2. The transformation of "nothing" into "something" is a false dilemma because you just ain't smart enough to understand such an abstract transformation. Now shut up and calculate.
3. God did it. And he needs some money. (of course, don't ask where God came from as this might invalidate the solution)

Please add to this list if you can think of any other options.

44. **Grep Agni**
May 20, 2012

On a much lighter note, this SMBC comic is apropos.

Igor Frenkel 60th Birthday Conference

May 14, 2012
Categories: Uncategorized

This week Yale is hosting a conference on Perspectives in Representation Theory, in honor of Igor Frenkel's 60th birthday. I'm planning to take the train up there and attend some of the talks tomorrow and Wednesday. Frenkel has been a pioneer in the field of representation theory, especially in the area of infinite-dimensional algebras whose representations are significant for understanding low-dimensional QFT, string theory and topological QFT. He started his career in the early 1980s, with many important results relevant to understanding affine Lie algebras. These became central in the explosion of interest among physicists in 2d conformal QFTs after 1984 due to their importance in string theory. Frenkel's later work has covered a wide variety of topics, with one theme that of trying to understand higher-dimensional generalizations of affine Lie algebras and their potential application to QFTs in higher space-time dimensions than 2. He has also promoted the themes of "categorification" and geometric incarnation of representations that are now central to much research in this area.

Pavel Etingof and other students and collaborators of Frenkel have put together a wonderful document, On the work of Igor Frenkel, which gives much more detail about the many topics of his mathematical research.

Update: Videos of the talks are now available here.

Comments

1. **Thomas Larsson**
   May 20, 2012

   Sigh! I had a look at Pavel Etingof's paper, and I got seriously depressed, due the following quote:

   "In spite of this progress, however, it is still not clear what the representation theory of central extensions of double loop groups should be like. Perhaps we don't yet have enough imagination to understand what kind of representations (or maybe analogous but more sophisticated objects) we should consider, and this is a problem for future generations of mathematicians."

   On the Lie algebra level, the answer has been around for 15 years or more. Geometrically, the off-shell representations of the multi-dimensional affine algebra (the MF extension is not central) act on trajectories in the space of g-valued p-jets, where g is the finite Lie algebra. The representations of the multi-dimensional Virasoro algebra are completely analogous.

   OK, I understand if people ignore an crackpot amateur physicist, but perhaps one should bother to have a look at the work of professional mathematicians, like Billig, Rao or Moody.

The Smell of SUSY

May 16, 2012
Categories: This Week's Hype

The implications of the failure to find SUSY at the LHC are beginning to sink into the particle physics community: the paradigm that dominated the subject for the past 30 years has collapsed in the face of experimental (non)-evidence, threatening to take down the life's work of hundreds if not thousands of theorists. For some recent attempts to quantify what is going on, from some people with much more statistical expertise than me, see Philip Gibbs and Tommaso Dorigo. By now a significant number of SUSY analyses of the full 2011 dataset have been completed, with negative results. By the end of the year there will be more data, but just a factor of 2-3 more, at about 14% higher energy. To believe that these sorts of increases will turn no signal into a signal requires a willingness to engage in a rather large amount of wishful thinking. The 62% jump in 2014 to 6.5 TeV/beam is more significant, but it's hard to see an argument for why this should do the trick, and the wait for these results will be discouragingly long, probably until 2015. How many SUSY enthusiasts will keep the faith?

A small number of theorists though still claim all is well, with one group producing a new paper claiming the full 5 inverse femtobarn results show Chanel No5 (fb^-1): The Sweet Fragrance of SUSY. Last year the same authors were claiming to detect Profumo di SUSY in the first inverse femtobarn, and argued that 5 times more data would be conclusive. To quantify the SUSY smells they are advertising, one can plot as a function of time their published predictions for the parameter $M_{1/2}$ which determines the gluino mass.

arXiv:1007.5100 455 GeV ("Golden Point")
arXiv:1009.2981 455-481 GeV ("Golden Strip")
arXiv:1111.0236 512 GeV ("Universe F-U2")
arXiv:1111.4204 518 GeV ("Profumo di SUSY")
arXiv:1203.1918 610 GeV ("Aroma of Stops and Gluinos")
arXiv:1205.3052 708 GeV ("The Sweet Fragrance of SUSY")

It's rather easy to extrapolate to the future what these authors will be claiming the SUSY masses are, harder to extrapolate how they'll be describing the smell. The rest of the particle physics community I suspect is already using very different terms for this.

Update: According to this report from Pheno 2012

> As pointed out by Rahmat Rahmat (yes, that's his name—in my notes I list him as Rahmat^2) and Csaba Csaki, respectively from University of Mississippi and Cornell University, the LHC should have detected some signature of SUSY by now, especially if the MSSM is correct. As Csaki said, "SUSY is a wonderful woman who does not return my letters. It makes you wonder if she even exists!"

Comments

1. **Aaron Davies**
   May 16, 2012

   > Last year the same authors were claiming to detect Profumo di SUSY in the first inverse femtobarn

   It's the Profumo Affair!

2. **rrtucci**
   May 16, 2012

   Maybe you should have titled it:
   "The Smell of Dead Susy"

3. **Frank**
   May 17, 2012

   Peter, some people are starting to play a well-known game with supersymmetry: whatever negative result comes in, they answer that it does not refute supersymmetry. This behavior begs the following question, which you know well enough from a similar field: is there any definite, testable prediction of supersymmetry at all? Or even more precisely: is there any definite, testable prediction of *broken* supersymmetry?

   To interested people it seems that none of the concepts introduced by supersymmetry, from sparticles to super-gauge fields, has any evidence to back it up. This begs a second question: Why have people been so convinced about supersymmetry to start with?

4. **Speculative**
   May 17, 2012

   Frank,

   1) Unlike String Theory, SUSY is certainly testable. The problem with discovering or ruling out SUSY is not its testability but its parameter space. That is there it will take a lot of experimental work to really rule it out. It is probably impossible to rule it out completely, rather if a large enough parameter space is ruled out theorists and experimentalists may just move on. Theories usually not outright disproven, but rather proven untenable.

   2) SUSY is (or was) appealing because it seemed to solve so many problems at once. It makes working with the math nicer, it fixes the gauge coupling at higher energies (so they all converge at the hoped for GUT scale), and it gave possible candidates for Dark Matter. Of course this doesn't matter if it doesn't actually exist but the search for it is certainly not without merit.

   My feelings coincide with Professor Strassler. We need to wait and see and not jump into any conclusions. Science is a slow process and there's no need to rush it or jump to conclusions. That being said, the results coming out of the LHC aren't encouraging, but you never know how things can change.

5. **Hun Hunahpu, PhD**
   May 17, 2012

   This accelerating increase in the gluino mass indicates that one of the hidden dimensions is rapidly decompactifying. My calculations indicate that the transition to a genuinely five-dimensional macroscopic world should occur a little over seven months from now.

6. **don**
May 17, 2012

I think both sides of the SUSY debate have jumped to conclusions too quickly. Those who worked on it believed it would be found soon after the LHC started up and now those who were skeptical of the idea believe SUSY is dead. What is happening now reminds me of the state of the Higgs search about nine months ago when nothing had been found yet and people were declaring the Higgs dead. SUSY might be right or it might be wrong, let's wait until we have more data before we jump to conclusions. This search is, after all, one of the reasons the LHC was built, let's let it do its job. And let us not forget this point: we will not have evidence of something until we have evidence of something. This is what science is all about: we have a problem, we come up with an explanation that attempts to solve the problem, then we test it.

7. **Peter Woit**
May 17, 2012

don,

I don't think there's much parallel to the Higgs case, where I don't remember anyone declaring it dead, just some speculation about what it would mean if it were ruled out.

In the SM Higgs case the predictions are very solid, with one free parameter, so it has always been very clear exactly what is ruled out (some range of the one parameter at various confidence levels). For SUSY, there are vast numbers of parameters to describe even the simplest SUSY extension of the SM, and it will never be possible to say "SUSY is ruled out at X confidence level". So, it's worth getting clear on exactly what the case for SUSY is, what exactly you expect to see at the LHC if that case is valid, and exactly what the analyses show. I'd argue that pre-LHC the case for SUSY was weak, with many arguments for it leading to the conclusion that it should have already shown up. The only remaining hopes for those arguments required SUSY to show up early in the LHC data, and it didn't. We've now seen quite a few analyses of the full 2011 data set, and there's no good reason to believe the 2012 data set will make large increases in the exclusions already available. So, now is a good time to start drawing conclusions. You can try and argue to wait for 2015-6 and higher energies, but I don't think there's a good argument there, just an attempt to stall for a few more years.

8. **don**
May 17, 2012

I think I am being fair when I say that while SUSY may not be true it is at least a reasonable theory that very well could be true. I think it is also fair to say that we are nowhere close to having excluded over 95% of the possible reasonably likely iterations of SUSY. Let's search and then draw conclusions after we have at least a year of full power LHC. It's not as if we are wasting our time on a crackpot theory that has almost no chance of being correct or that we even have very many other sensible theories at this time that could possibly go beyond the Standard Model. Perhaps you will argue that we should move on now, cut our losses, and try to come up with something new. I'm all for alternatives to SUSY, but I also believe that we can multitask.

9. **Peter Woit**
May 17, 2012

don,

My point was more about what theorists should do, not what the LHC experiments should do. It's long past the time that theorists should admit that things like the MSSM are a failed idea. I see no argument for waiting until 2015-6 for this.

As far as the LHC experiments are concerned, they certainly shouldn't stop SUSY analyses, and in any case there's no danger of that. It would however be a good idea for the people making decisions about which analyses to do (or how to design triggers) to have a realistic point of view about the prospects for SUSY, not an overhyped one.

10. **Visitor**
May 18, 2012

"As Csaki said, 'SUSY is a wonderful woman who does not return my letters. It makes you wonder if she even exists!'"

Beware non-existent women! Heed the troubles of Paul Frampton, and let it be a cautionary tale for you!

11. **neo**
May 19, 2012

"My point was more about what theorists should do, not what the LHC experiments should do. It's long past the time that theorists should admit that things like the MSSM are a failed idea. I see no argument for waiting until 2015-6 for this.

As far as the LHC experiments are concerned, they certainly shouldn't stop SUSY analyses, and in any case there's no danger of that. It would however be a good idea for the people making decisions about which analyses to do (or how to design triggers) to have a realistic point of view about the prospects for SUSY, not an overhyped one. "

"long past the time that theorists should admit that things like the MSSM are a failed idea. I see no argument for waiting until 2015-6 for this"
PW,

How does the failure to find SUSY in 2011 data set @ 7TEV energies translate to not likely finding evidence for SUSY at 14TEV (or 13TEV)

do you think there will be new physics, such as SUSY, in a proposed future upgrade to a higher-energy LHC ("HE-LHC") with about 16.5 TeV beam energy (33TEV)?

12. **Peter Woit**
May 19, 2012

neo,

The main argument for SUSY at LHC energies has always been that it would solve the hierarchy problem and stabilize the weak scale. The problem with this argument was always that if so, you should have already seen evidence for SUSY pre-LHC. One could argue though, that it was just beyond the pre-LHC range. That argument is now dead. I don't see a good argument for expecting SUSY either at 13 TeV or 33 TeV. Hopefully the LHC will see something non-SM, which will justify building an HE-LHC to investigate. Otherwise, the main argument for the HE-LHC will just be to check the SM at higher energies. That would be enough for me, but it will make selling the machine harder.

13. **neo**
May 20, 2012

A certain string partisan, whose name I won't mention, debated this with Tommaso Dorigo on his blog, that in his view, the situation is similar to the hunt for the Higgs –

Higher energies + enough data takes time to collect and analyze, and there are many versions which have not been ruled out.

14. **Peter Woit**
May 21, 2012

    neo,

    The MSSM adds more than 100 parameters to the SM, you will always be able to find corners of parameter space that the LHC can't rule out, or just push the SUSY breaking scale high enough. Some fraction of SUSY diehards will never give up on the idea, no matter what. If you look at Lubos's long posting about this, it boils down to his accurate argument that if your prior probability is 99.99% that SUSY exists, the LHC exclusions don't change your beliefs by much. Or, less technically, if you're a fanatic, experimental evidence is pretty much irrelevant. Most people however are not fanatics, and opinions about SUSY in the mainstream are definitely changing substantially in response to the LHC results.

    Interestingly, even Lubos doesn't believe the "Smell of SUSY" argument of Nanopoulos et al., which is remarkable.

    Why the SUSY search with its vast number of free parameters is just like the Higgs search, with its one parameter, escapes me.

15. **neo**
May 21, 2012

    I didn't mention the name yet you correctly guess. Haha. His claim "Your analogy with the Higgs allowed intervals is very appropriate. When we're approaching a discovery, we're inevitably able to eliminate increasingly large portions of the regions that were possibilities just a year ago. This just means we're learning something about the details of the model. For Higgs, the discovery could actually take place well before the exclusions of the most of the parameter space because its signals are sharp as a function of the Higgs mass. But this didn't occur. For SUSY, the signals are much more widespread as functions of the superpartner masses so we must clearly expect to exclude an even higher fraction of the parameter space before the discovery – at least one able to find out the masses of the new particles – can actually be made."

    In his opinion, the current LHC bounds only exclude a small fraction of the SUSY parameter space, MSSM and still resolve the hierarchy problem and stabilize the weak scale.

16. **Peter Woit**
May 21, 2012

    neo,

    In Lubos-land, it doesn't matter how much parameter space the LHC rules out, or how much fine-tuning is required for the MSSM to "resolve the hierarchy problem and stabilize the weak scale". 5 years from now when the LHC has gone to higher energies and luminosities, he'll be saying the same thing, although by then he'll have gone from being in a minority of theorists (the situation today) to being in a very small minority of true-believers.

17. **neo**
May 22, 2012

    –PW Possible, but that's assuming continued null result, which is entirely plausible. If the LHC continues to provide null results even at 13-14TEV design energies and sufficient fb-1 & data analysis, what do you predict will happen to string theory research and interest? Do you think Witten, Susskind, Greene, Kaku, Hawking, et al will continue to work on it and promote it to a popular lay audience?

18. **Peter Woit**
May 22, 2012

    neo,

    Most of those you mention already have stopped working on string theory, and have likely already given up on the LHC vindicating string theory. On the other hand, people who have devoted 20-30 years to promoting something rarely publicly admit this was a mistake. I'd guess what will see will be a continuing refusal to publicly admit failure, coupled with half-hearted endorsements of string theory unification, while working on other things. Kind of like the situation now…

Latest from the Rumor Mill

May 16, 2012
Categories: Uncategorized

There's at least one thing about string theory that has changed dramatically since my book was written back in 2002 or so. At the time I accumulated various numbers showing the way hiring in particle theory at leading institutions in the US had been dominated by string theory hires. Overall, at that time about 20 people/year were getting tenure-track positions, roughly half in string theory half in phenomenology. This data came from the [Theoretical Particle Physics Jobs Rumor Mill](), and Erich Poppitz has done an excellent job of putting some statistics together based on this data (see [here]()). In recent years Erich's data shows a much lower number of such positions (10-15/year), due to some combination of the bad economy and lack of enthusiasm for particle theory by other physicists. The number of string theorists getting positions had come down to about 2/year, then down to only one last year.

The hiring season is not yet over and not all the data is in, but so far the Rumor Mill shows no job offers to string theorists at all. Job offers are going pretty exclusively to phenomenologists and cosmologists, with phenomenologists allowed to stray into formal theory if they work on topics related to N=4 SYM and its superconformal invariance (including the hot topic of amplitudes). Marcus at [PhysicsForums]() has patched some of the Rumor Mill links for better accuracy.

One thing hasn't changed though since 2002: there's a much larger number of talented and accomplished candidates than there are jobs, and departments are playing it safe, offering the few jobs available only to people working in a small number of areas that are conventionally agreed to be "hot". As always, if you're working on some idea that's not in the narrow mainstream, there's no chance you'll get hired into a permanent position at a US institution.

Update: There is one string theorist now with a job offer it seems, with Princeton making an offer to Simone Giombi, who works on the hot topic of higher spins. So, at least Princeton has not given up….

Comments

1. **P**
   May 16, 2012

   "One thing hasn't changed though since 2002: there's a much larger number of talented and accomplished candidates than there are jobs, and departments are playing it safe, offering the few jobs available only to people working in a small number of areas that are conventionally agreed to be "hot". As always, if you're working on some idea that's not in the narrow mainstream, there's no chance you'll get hired into a permanent position at a US institution."

   Agreed, and the last sentence is particularly sad. In the absence of earth-shattering theoretical papers (compared to papers in the 1990's, e.g.), departments seem to be hiring purely based on what is "hot" (as you point out), but this is usually defined in a given 2-3 year time span.

   What do you think happens to faculty hires in HEP theory in five years if we have Higgs + nothing at LHC, no tensor modes from Planck, and no direct dark matter? The phenomenology of these fields are "hot" now, and rightfully so, but it will be interesting to see what happens in the next few years.

2. **Peter Woit**
   May 16, 2012

   P.,

   That's the "nightmare scenario" as far as business as usual in particle theory is concerned, and I don't know what will happen, although I suspect it will involve physics departments hiring fewer theorists overall.

   One thing one can be sure of: if Planck/LHC/direct dark matter experiments actually see something unexpected, if you want a job you're going to have to be working on that…

3. **Christian Takacs**
   May 17, 2012

   Peter,
   For quite some time now you have been speaking out about how physics should not have put all it's eggs in one basket, particularly a basket woven of superstrings and multiverses stuck together with supersymmetry glue. Ok. Given. How far back are you expecting the wayback machine to be set before physics has something it can testibly prove again? Pre superstring? Pre QED?? Pre SM??? Pre General/Special Relativity???? When things don't work out, you usually have to start questioning your assumptions, as logically this means some unrecognized error has taken place. This rethinking of a field of science has happened before, it's reality's way of saying "you're looking the wrong direction", and If you could even suggest such a place where the physics community might start looking for where they went wrong, as opposed to Not Even Wrong, a great service will have been rendered.
   I wouldn't worry so much about how many phsyicists get tenured, as sheer quantity isn't going to solve the trouble with physics as long physicists are looking to follow someone elses lead. I would be more concerned about how any physicist who could actually find a working solution could get any traction in your community to put things back on track, as the past history of physics has shown quite clearly, its not the group thinkers looking for approval from their peers or seekers of job security or conformity that makes for discovery. It's usually someone willing to challange the status quo, pecking order, or established assumptions, usually at personal cost to themselves.

4. **Peter Woit**
   May 17, 2012

   Christian Takacs,

   The experimental evidence is clear that the SM is a fantastically successful idea, supersymmetric and extra dimensional extensions of it are failures, which makes it clear where you want to roll-back to. There are aspects of the SM that remain poorly understood, for instance: confinement of QCD, non-perturbative behavior of chiral gauge theory, BRST treatment of gauge symmetry (outside of the perturbation expansion). These are difficult problems, and no one is likely to solve them quickly. If an ambitious young theorist in the US devoted their post-doc years to working on one of them, making some modest progress, what would their chances be of appearing on the Rumor Mill with a job offer? Perhaps the biggest obstruction to progress is the ingrained attitude in the theory community that only someone stuck in the past and too stupid to understand the standard textbooks would worry about such issues. Real men quickly learn the trivial SM and move on to the cutting edge of SUSY models, extra dimensions, strings and branes.

   What I'm wondering is how long you can keep claiming to be the "cutting edge" when your ideas have failed and you aren't coming up with new ones.

5. **Bernhard**
May 17, 2012

Peter,

this is not restricted to the US, in Europe too, you have no chances at all of working on your own ideas and get tenure. I would say the situation in some European countries is even worse, as one depends on grants for the rest of his life. In the US as far as I understand, after tenure you could in principle work on whatever you want. European funding is strongly driven to competition with the US and if you write a proposal on something that is not hot enough, be prepared for comments like "excellent proposal, but focused on rather exotic theoretical models" (as you may suspect I´m talking about my own experience). An "exotic model" is by definition almost exclusively something that is not SUSY, although with stronger limits I have no idea how is this going to be evaluated. If you look at the highly prestigious ERC grants from 2011 (PE2 is where HEP competes):

http://erc.europa.eu/sites/default/files/document/file/erc_2011_stg_results_all_domains.pdf

you see that the few HEP project granted are either dark-matter or more very down-to-earth experimental projects like using diboson final states or measuring the W mass. There is not a single project that (at least the subject) is not something that smells awfully traditional or mainstream. Oh yes, from the starting grants for 2011 you see no string theory projects, in 2010 two were granted. No idea what will happen for 2012 but this will be out in the middle of the year.

The way out I found in the past, was to find a way to include SUSY (in whatever way I could) in all projects I was writing. I always got grant this way. Now the new trick is to include "Higgs" in whatever you write and so we (sadly) go.

6. **Mike**
May 17, 2012

One thing I always wonder is that given universities seem to hire tenure-track people annually, does this mean a significant fraction never get tenure and are kicked out after 4 or 5 years? I can't believe departments are expanding indefinitely, and I suspect not many people leave the academic world voluntarily.

7. **Peter Woit**
May 17, 2012

Bernhard,

Too bad to hear about how similar things are all over the world. It used to be that the existence of local cultures of particle theory with different points of view allowed for a variety of viewpoints. Now we have a not necessarily good form of globalization.

Mike,

At least in the US, most tenure-track jobs carry a fairly high probability of leading to tenure and a permanent position (with a small number of exceptions, e.g. Harvard). The main source of new positions in the system is retirements these days, as the large number of people who got tenure in the 60s retire. When someone retires though, for budgetary reasons the university may not allow the department to replace them. The department may also decide to change fields, replacing a particle theorist with someone from a different field that they see as more promising.

There are also a small number of new institutions that are hiring all new staff. Main examples are Perimeter and the Simons Center.

8. **Marcus**
May 17, 2012

Mike,
"One thing I always wonder is that given universities seem to hire tenure-track people annually, does this mean a significant fraction never get tenure and are kicked out after 4 or 5 years? I can't believe departments are expanding indefinitely, and I suspect not many people leave the academic world voluntarily."

I imagine that someone hired tenure-track at a first-rate Ivy who then does NOT win tenure after 4 or 5 years is likely to make faculty or find a secure research position somewhere less celebrated to the south or west. So such folks do not necessarily leave academia.

9. **Aidyan**
May 18, 2012

"As always, if you're working on some idea that's not in the narrow mainstream, there's no chance you'll get hired into a permanent position at a US institution."

Indeed. But it is not only in the US but also in Europe (and i suspect a worldwide trend), and not only in theoretical physics. When you propose something which is not mainstream, say also in down to earth applied physics like nanophotonics (my stuff), it is usually something people don't know about precisely, which means also that those who have to take up responsibility (for funding, or mentoring, or represent the research group, etc.) begin too panic. The reptile brain "what if" reaction that begins to foresee possible dangers in case of failure (and that will always find some) dominates and mesmerizes every thought pattern. And at that point the typical response you get is "I'm afraid this is too far afield from…". The problem is that we lack of people who have e bit of spine. Most of these people are managers, politicians, bureaucrats who have a degree in science, but not scientists who manage any more.

10. **lun**
May 18, 2012

With a few exceptions, senior theoretical physicists, even senior ones, are in my experience open-minded to mainstream ideas being wrong, so the picture of an inquisition keeping down modern-day Galileos is simply not true.
The exclusion of non-mainstream ideas happens in a rather more subtle way: Physicists are ranked according to how good they are at solving "useful" and "timely" problems. These, by definition, are mainstream problems.
Hence, a physicist who is developing non-mainstream ideas, BY THAT DEFINITION, is not a good physicist.

This effect is systemic, not particular to string theory or any other mainstream idea.
And it seems much more efficient, at stamping out non-mainstream ideas,than outward hostility.

11. **srp**
May 18, 2012

There seems to be a general malaise across a broad range of natural sciences (for example, in biomedical research as well as particle physics) emanating from

1. Funding mechanisms that require a) endless grant-writing at the expense of doing actual research, b) conservative, closed-ended proposals rather than innovative, open-

ended proposals (sometimes to the extent of "promising" results that have already been obtained but not yet published), c) overproduction of new grant-seekers, leading to declining success rates over time, thereby exacerbating a).

2. A surge of questionable publications that make it hard to find actual nuggets of useful stuff in the literature. Said surge being induced by the funder and institutional pressure for lots of "high-impact" papers.

3. Strangling of new ideas by peer review abuses and by the need for junior faculty to placate senior colleagues.

Scientists are very institutionally conservative, so no matter how much they complain about 1-3 they will never support fundamental reforms out of fear that what comes after will be even worse. And since most incumbent scientists succeeded in an earlier, more functional, incarnation of the system, they are emotionally comfortable with its basic contours.

I am led to the conclusion that only alternative institutions, funded and governed independently of the existing system, will be able to remedy these problems. The original scientific revolution in Europe entailed just such a movement away from traditional institutional structures, so at least there's precedent.

12. **Peter Woit**
May 18, 2012

srp,

There are generic problems with modern academic scientific research, but it strikes me that the situation in mathematics is quite different than in particle theory. Math is a very similar style of research to theoretical physics, no labs, but the results are very different. Math has its problems, but in general it's a pretty healthy subject. It's an interesting question why the results are so different than in particle theory.

13. **DB**
May 19, 2012

Peter,

As an outsider with physics degree, which I haven't used for around 15 years I would hazard a guess that the difference between physics and mathematics is that maths has no requirement to meet the real world. So long as it is rigorous it is pretty much all good and any future application is a bonus.

Once you add the requirement that your mathematics has to model a universe then you add whole new ways for elegant, self consistent, rigorous work to be wrong.

In other words it is a lot easier for perfectly competent physicists to waste their careers, which makes the stakes somewhat higher.

14. **Peter Woit**
May 19, 2012

DB,

That's one difference, but that has always been a difference, and it didn't stop particle theory from being healthy pre-1970s. One thing that change around 1970 is that the job market in particle theory got awful and has stayed awful since then, in a way that is very different than mathematics and other sciences. I suspect that having a job market where you only have 1 in 10 chance of getting a permanent job, and that chance requires that you work on the most fashionable topic, is a big part of the story.

15. **Peter Shor**
May 19, 2012

It seems to me that one difference between math and theoretical physics is that there is a lot more unsubstantiated dogma in physics. There are a few leaders in the field, and everybody tends to believe what they say. For example, I've heard comments to the effect "if there's something to this non-commutative geometry stuff, why isn't Witten working on it?" and "Susskind showed that if the universe wasn't unitary, experiments would have detected it by now, so we know it must be unitary (this without having any idea of what was in Susskind's paper, and whether it was invalidated by the discovery of topological error correction)."

In math, if the leaders in a field have a proof, they are generally believed (sometimes incorrectly), but if they have a conjecture, it's not taken as solid evidence for anything.

16. **Shantanu**
May 20, 2012

Peter, do you have any data on the trends in topics covered in HEP or particle seminar series at various universities (over the years). That may give another idea of what's hot (or not) or how trends have changed over the years.
shantanu

17. **Peter Woit**
May 20, 2012

Peter Shor,

Interesting comment. One major difference between math and theoretical physics is that mathematicians are very focused on precisely drawing the boundary between what they completely understand (thus have a "proof"), and what they don't. This provides for a fruitful research environment, where people can try to push the boundary, even if only a little bit. In QFT, the distinction between what is completely understood and what isn't often is lost or highly obscure. People rely on the best minds of the subject (e.g. Witten) to make the distinction, but even Witten is not infallible (and relying on someone like Susskind for this is really kind of nuts).

This also leads to an environment where people assume that everything about QFT is basically understood, so anyone who works on a range of topics must be a second-rate intellect, unable to handle thinking about the acknowledged areas where we don't know what is going on.

Shantanu,

I have no such data, nor any good idea even how one would put it together. An interesting idea though, looking at seminar topics at major research institutions does give a good idea of what the current "hot" topics are.

18. **Maynard Handley**
May 20, 2012

(a) Regarding supposed malaise in science in general: do we have any real evidence of this. To an outsider, it certainly looks like we're discovering more faster than at any point in human history.
People seem to have a desire to claim to be living in the worst of all possible worlds (cf the on-going nonsense we hear about how violent our society, or our world, currently is — claims confidently made by people with absolutely zero knowledge of history or anthropology). Discussion of a general malaise in science strikes me as very much in that vein.

(b) Regarding particle theoretical physics in particular, my humble suggestion is that it is long past time that we throughly revise the curriculum and the textbooks. We have a situation right now where the amount of material that must be learned to reach competence is so large that it is way beyond the capabilities of most mortals.

That's fine if your goal is to be a very exclusive club of Sheldon Coopers; but as a general principle keeping out large swathes of the population is not conducive to intellectual progress. You can argue that progress in this field will only be made by those dedicated enough and smart enough to walk this trail — but we've been running the experiment for about thirty years and the results have not been positive, which suggests that perhaps we need to cast a wider net.

What would I suggest? In order of, perhaps, controversiality
(a) theoretical physicists should learn much more probability (and not bother with the little statistics that they do learn). Mathematicians have a robust and powerful vocabulary of probabilistic ideas culminating in stochastic processes, while physicists think of probability using the ideas of vocabulary of someone like Bernoulli.

(b) perhaps we need to split into theoretical and experimental physics tracks even at the undergrad level. The theoretical track could omit, for example electronics and much of optics. Also, as an undergrad, I did not (could not) take only physics courses, so I filled in the time with, for example, chemistry, which, IMHO, was a waste of time. (Not a slight against chemists — my point is that there is a LOT of physics to learn, and I would have been better off if I'd been able to take more physics courses, if the way the curriculum was sliced and ordered allowed for that.)

(c) we have to get realistic and sensible about how we teach the math. Yes, in an ideal world every student would work through, understand, be able to perform, every proof of every theorem of every branch of mathematics that is required. But we don't live in that world. IMHO we spend too much time teaching what is easily taught (or at least easily written down, easily tested, easily claimed to be rigorous) and too little time teaching an understanding of why this stuff matters.

Similar to "Physics for poets" type courses, I'd like to see the construction (and especially the accompanying textbook) of a "Math for scientists" type course. This would be a survey course of the whole shebang — analysis up to at least Measure and Integral, algebra up to at least Lie groups+algebras, Riemannian geometry, probability up to Stochastic Processes.

But here's the catch — no rigorous proofs. This is not meant to be a trivial course. I want to see rigorous definitions, the rigorous statement of theorems, and reasonable "explanations" for why the theorems are true. But we have a lot to cover, which means the point is to show you the sweep of what's available and how it all fits together, at a rather higher level than you can get from the popular science books at your local bookstore.

This book is a reasonable very first stab at what I have in mind
http://www.amazon.com/All-Mathematics-You-Missed-Graduate/dp/0521792851
but it's too biased in the direction of what the mathematicians need rather than physicists, and it doesn't go as far as I would like in a number of directions.

(d) the physics community has got to get its act together and improve the teaching of QM and the QFT. What is being today is a freaking travesty. We don't teach mechanics using Newton's original methods, and we don't teach EM as Maxwell's mechanical models, but we teach modern physics by its history. We use a set of ideas and language that were outdated by 1940.

My particular bete noire is that we not only insist on using Hamiltonian vocabulary to teach QM, we insist on wasting a semester teaching classical mechanics so that this Hamiltonian vocabulary is vaguely familiar. This is so ass-backwards it isn't funny — we might as well be teaching fluxions and quaternions.
Then, after insisting on forcing QM into the Hamiltonian procrustrean bed, we make learning QFT far harder than it has to be by trying to fit IT into Hamiltonian vocabulary until, eventually, we give up and admit there is a better way. WTF is all this achieving apart from wasting a year and a half of precious learning time?

And this is not even to mention minor idiocies like the handling of negative frequency solutions for the Dirac equation. For crying out loud — it is 2012. And still, most textbooks begin with weeping and wailing about how these are "negative energy" solutions and how will we interpret them, followed by some nonsense about holes and filled vacuums. Then we get the same thing all over again when it's time to calculate densities and currents and now the weeping and wailing is over how we can generate negative "probability densities". We need textbooks that spend ZERO time rehashing the surprises (and obsolete vocabulary and ideas) of the past and that, on every page, tell us how things ARE and how they fit together, not how things might have could have been if we interpret them using ideas from 1920 and how the results are then so puzzling.

To give another example of — not as egregious, but still a grievous waste of time. Why is time spent "deriving" the Pauli version of the Schrodinger equation? What on earth is the value of this exercise? Why not start by saying "this is the way the world is — this is the Dirac equation" as soon as you want to introduce spin, and deriving the Pauli equation from that. Follow logic, not history.

19. **David Nataf**
May 20, 2012

Maynard Handley,

Your post reminds me of a discussion I had with friends when taking a freshman-level undergraduate physics course, several years ago. The physics textbook we had for that course would not just discuss the physical concepts, but would include numerous blurbs about the physicists responsible for those concepts, and the intellectual and sociological conditions in which they worked. As you are against "history", you would have been one of the ones against the historical anecdotes.

I however, was for them, I thought they improved understanding, and in general I think it's valid and useful, for every scientist, to know the history of why some ideas took a long time to develop even though they were just around the corner, since scientific mini-revolutions are taking place all the time in many fields of science. It's 100% relevant, imo, that Boltzmann, as brilliant as he was, could not crack quantum mechanics. It's also interesting that Einstein took 10 years to put he equations of GR together, and the failed starts he had in between.

You also confuse the mathematics you need in your own work with the mathematics scientists need. Mathematical education should be broad, as you can't predict what type of work a first-year undergraduate will encounter. Further, good luck predicting, with any accuracy, whether a first-year undergrad will be a theorist or an experimentalist. I'm sure 90% of physics majors think they're going to be theorists when they're starting out, just like 90% of first-year law students think they're going to be human rights lawyers.

With that said, there is a lot of specialization in later years. In my program, particle physics, nuclear physics, general relativity, string theory, various labs, optics, advanced statistical mechanics, astrophysics, et cetera were all available electives for senior-year undergraduates. I suspect this is standard, and consider that optimal.

20. **Maynard Handley**
May 20, 2012

David, I am not against history. Most of my pleasure reading is history.

What I am against is teaching physics as it was historically discovered. Look at the list of examples I gave in my comment for precisely my point. We don't teach chemistry by starting with caloric, explaining why that doesn't work, then going on phlogiston, explaining why that sucks, THEN arriving at oxygen.

Look, the problem I am describing — too much physics, not enough time — is REAL. All the pretense, all the complaining about how things would be in a perfect world, won't change that. We can admit the problem and deal with it, or we can have another thirty years as oh-so-fruitful for theoretical particle physics as the past thirty.

21. **David Nataf**
May 20, 2012

Maynard,

Even though I think we started off physics with the same goals, I am now an astronomer, and from what I gather you are a theoretical physicist. You say your education was missing: Point set topology, complex analysis, differential forms, the curvature of surfaces, the axiom of choice, Lebesgue integration, Fourier analysis, algorithms, and differential equations (from the Amazon link you posted), whereas I'd say mine was missing numerical methods, fluids, tensor algebra, and computer science. It's very hard to envision a modified curriculum that would have been ideally suited to both of us. Can you?

It is true that it is bad for science if people can't push the boundaries until their late 20s. However, I'll point out that many intelligent individuals are never challenged by our education system until their early to mid 20s. I do think this waste of talent is bad for science, though I don't know that it ties in to any of Peter's specific points.

22. **Peter Woit**
May 20, 2012

Maynard Handley and David Nataf,

The discussion about teaching physics is interesting, but off-topic. Soon I will be writing about something related to this, maybe best to resume it then.

In an environment where ten times more people are getting phds in particle theory than there are jobs, I don't believe the solution to the problems of the subject is to figure out how to change the educational system so one can expand the population trained in particle theory.

23. **thomas**
May 20, 2012

I think the difference between the job market in math and physics is just a numbers game. At large US universities math departments do more service teaching than physics departments, and the number of faculty is larger. Grants are smaller, and there are no research projects that consume a lot of manpower, so the number of graduate students is smaller than in physics (even in absolute terms). This makes for a much better job market. I grew up in Germany, where Math departments don't do a lot of service teaching. Math departments are smaller than Physics departments, and the job market is just as tight.

We are worried about physics at the smallest and the largest scales, but most of physics is somewhere in the middle, and doing just fine. We hear about progress in math when difficult theorems are proved, but most successful math nowadays lives at the interface with computer science, finance, statistics, and biology.

24. **Jeff McGowan**
May 21, 2012

As a mathematician I thought I should weigh in. I think there is truth on both sides, leaning towards the "math is healthier" side, but with caveats. My caveats are that there is some of the "gods of math" thing which goes on, but it is seriously tempered by the fact that even the gods have to eventually come up with a proof. Peer review can also be a serious issue, but there are enough good outlets that there are ways around it. And while it is certainly true that the job market in math is much much better than the particle physics market, it's not that it's good or anything….

25. **Shantanu**
May 21, 2012

Peter to answer my own question, I have some data at my own alma-mater, BU (which has
no string theorists BTW) Between 2000-03, most of the talks were on braneworlds, RS,ADD models, deconstruction models, little higgs models etc. Then next few years, the trend shifted towards dark matter
searches (both direct and indirect) and I think last few years most of the talks were about dark matter. In fact I was surprised by the large no of talks on DAMA
expt, even though I myself don't believe it. (OTOH around 1999, many particle physicists didn't even know/believe about the "WIMP miracle" or that solution to galactic rotation
curves has something to with EW symmetry breaking).
Maybe others can chime in about the trend in their univs. (Probably someone shoud

26. **Henry Bolden**
May 25, 2012

Peter: Is there any data to suggest that the string theorists from that hiring wave have to turned to other kinds of physics?

Higgs Update

May 21, 2012
Categories: Experimental HEP News

The announcement of new Higgs results from the LHC is now scheduled for about a month and a half from now, July 7th, 9:30 and 10am Melbourne time, at ICHEP2012. The LHC is performing well, with nearly 2.5 fb^{-1} of integrated luminosity/experiment. With only 2-3 weeks more of data collection before the cutoff for what can be analyzed in time to be made public July 7, one can expect the July announcements to be based on perhaps 4-5 fb^{-1} per experiment. Rough estimates show that, combining last year's 5 fb^{-1} at 7 TeV and this year's expected data, if the SM Higgs really is there at 125 GeV, each experiment should see a signal of 4 sigma significance. This is not quite the 5 sigma significance traditionally set for a discovery claim, but very close.

It thus looks possible that a discovery claim will require combining the results from CMS and ATLAS. The LHC Higgs Combination Group has been hard at work for the last couple years, developing methods for combining results from the two experiments. This time, they will be ready to quickly combine all the data from the two experiments, and maybe this will be what gives the 5 sigma needed for CERN to claim discovery. I don't know what the plan is for when they will be provided with the CMS/ATLAS data, or when they plan to announce a result.

The LHC-HCG is competing with Philip Gibbs, who today released a Higgs Combination Java Applet which allows anyone to produce their own data combinations. If the LHC-HCG can't produce a combination by 10:30am July 7, CERN should perhaps consider getting Philip's applet properly set up and having DG Heuer publicly press the right button, allowing CERN to claim the Higgs discovery before Philip does.

Comments

1. **Rob R.**
   May 21, 2012

   > This is not quite the 5 sigma significance traditionally set for a discovery claim, but very close.

   [re: "very close"]Forgive the noob question, but, I thought, statistically speaking, there was a big difference between 4 and 5. As a layman, I thought it was akin to the (logarithmic) Richter scale (i.e., while 5 sigma is a somewhat arbitrary cut-off it was still much larger than a 4 sigma.) Is a 4 sigma really a big deal? Dumb question? Not Even Wrong, even?

2. **Peter Woit**
   May 21, 2012

   Rob R.,

   My "very close" maybe is overly optimistic, but Not Even Wrong, since I haven't specified the metric. To give some more numbers to justify it though, the same calculation I've seen that has an expected significance of 4 sigma at 125 GeV gives an expected significance of 5 sigma at 130 GeV. Also, this is just the expected significance: if they get lucky and get a statistical fluctuation of more events than expected, say of size 1 sigma (where the sigma now is a different one…), they would get to 5 sigma.

3. **HHG**
   May 21, 2012

   There will be no official combination until each experiment will have reached 5 sigma alone – for "political" reasons.

4. **Peter Woit**
   May 21, 2012

   HHG,

   Thanks! But if CERN in early July is looking at CMS and ATLAS 4 sigma announcements, do they have a plan for dealing with the "political" problem of the Higgs discovery being announced not by them, but on viXra log?

5. **Bernhard**
   May 21, 2012

   HHG,

   that´s a bit strange statement. Two experiments getting 4 sigma, would be more than enough to claim a discovery. Not so time ago one the the spokespersons of one experiment was actually claiming that ATLAS and CMS getting a bit more than 3 sigma would allow them to claim. 5-sigma alone EACH is overkill.

6. **Peter Woit**
   May 21, 2012

   Bernhard,

   Maybe CERN would just go ahead and announce "discovery", on the grounds that it really doesn't matter how you do the combination, the conventional standard has clearly been met. Pretty odd though if the only quantitative combination result available is that from Philip.

7. **Anonyrat**
   May 21, 2012

   Since CERN has no competition, in my opinion, there is no need to hurry up the claim of discovery of the Higgs. Whether two months or six months from now, it will be the CERN collaborations that discovered the Higgs. So let them do it as carefully and properly as they can.

8. **emile**
   May 21, 2012

   I think it could be a mistake for CERN and the experiments not to combine their results. **IF** the two experiments had over 4 sigma each at ICHEP, everyone would consider the Higgs discovered, and unofficial combinations would provide the "discovery threshold" of 5 sigmas. When CERN did eventually release results of one or both experiments with more than 5 sigmas, the cat would already be out of the bag, and the level of interest will just not be there: people will say that they have not announced anything they did not already know. They (CERN) should be prepared to go public with an official combination **IF** the two experiments together meet the discovery threshold or they will lose control of the PR game. This kind of event happens very, very rarely. You have to milk this for all it's worth. You can't mess things up because of "political reasons".

9. **BJM**
   May 21, 2012

   It seems to me that announcing discovery while both experiments are at 4 sigma alone but together reach 5 sigma is ideal from the standpoint of equal sharing of credit. If the announcement awaits both experiments to reach 5 sigma, one will inevitably reach it first and claim (and perhaps be due) sole credit. Is that desirable?

10. **strong field physicist**
    May 22, 2012

    BJM,

    In my opinion not. Scientifically, its good to have two independent experiments which may eventually confirm a discovery. But there is no valid reason why official combinations would detract from experimental independence. This political rubbish, though perhaps inevitable, is not justified. I dont think any particular experiment should see this as "their" discovery. Since funding is in a vast part from the public purse, its more correct to see it as *humanity's* discovery.

    I'm all for Phil's efforts (and his applet). It plays an important part in bringing science to the people – so to speak. Its just a bit sad that we are reduced to digitising data from webpages

11. **Nex**
    May 22, 2012

    I played with the applet and it looks to me that there is only a distinctive bump in the gamma gamma channel at ATLAS and CMS, while other experiments and channels are also above expectations there they are very flat and very broad and look to me like an underestimation of the background in the 110 – 140 GeV/c2 region.

    Isn't the fact that the bump is only in one channel a reason for skepticism?

    Personally I doubt the Higgs boson is real and the data as presented by the applet doesn't look convincing at all. The most likely explanation of the data IMHO is the underestimation of background in the 110-140 range combined with 3 upward fluctuations (CMS actually sees 2 similar bumps one at 125 and one at 135) in the gamma gamma channels, two of which happened to overlap.

12. **Bernhard**
    May 22, 2012

    Nex,

    you have to remember that the Tevatron results (2.2 sigma) are also consistent with the interpretation of a signal there. I agree one should be careful and has the right to be skeptical, but things are looking more and more in the direction of a signal rather than a statistical fluctuation.

13. **David Derbes**
    May 29, 2012

    There is one sort of political reason for announcing the discovery in July (if they have the data from both ATLAS and CMS that, responsibly combined, give 5 sigma): the age of the theorists involved. There are a lot of individuals whose work contributed to this; the Sakurai Prize was split six ways. But it was done circa 1964, nearly fifty years ago. Robert Brout has died. Peter Higgs whose health, as far as I know is fine, is 82. I don't know how the Nobel committee will assign credit, but you can't win it posthumously. It would not in my opinion reflect well on anyone if Englert or Higgs or any of the other four survivors died between, say, July and late September, when the evidence will presumably be more definitive. (Disclaimer: Peter Higgs was my thesis supervisor, and I admit to wanting to see him win. Who else additionally wins is fine by me. :-))

Welcome to the Multiverse

May 21, 2012
Categories: Multiverse Mania

Multiverse Mania makes the big time this week, with a cover story Welcome to the Multiverse by Brian Greene in Newsweek. While the title indicates that the Multiverse is here and part of our scientific world-view, the subtitle is a bit cagier: "The latest developments in cosmology point toward the possibility that our universe is merely one of billions."

The article is pretty uniformly a promotional piece for multiverse mania, although buried fairly deep in the piece is something a bit more skeptical:

> because the proposal is unquestionably tentative, we must approach it with healthy skepticism and invoke its explanatory framework judiciously.
>
> Imagine that when the apple fell on Newton's head, he wasn't inspired to develop the law of gravity, but instead reasoned that some apples fall down, others fall up, and we observe the downward variety simply because the upward ones have long since departed for outer space. The example is facetious but the point serious: used indiscriminately, the multiverse can be a cop-out that diverts scientists from seeking deeper explanations. On the other hand, failure to consider the multiverse can place scientists on a Keplerian treadmill in which they furiously chase answers to unanswerable questions.
>
> Which is all just to say that the multiverse falls squarely in the domain of high-risk science. There are numerous developments that could weaken the motivation for considering it, from scientists finally calculating the correct dark-energy value, or confirming a version of inflationary cosmology that only yields a single universe, or discovering that string theory no longer supports a cornucopia of possible universes. And so on.

I don't see how we're anywhere near finding such a version of inflation or getting rid of the string theory landscape, so the only hope of getting any evidence against the multiverse seems to be to calculate the cosmological constant. The multiverse thus looks to be pretty much impregnable and immune to any conceivable scientific challenge. A few years ago, pieces like this would hold out hope that the LHC would discover something encouraging for the multiverse, but now the LHC isn't even mentioned. The only possible positive evidence suggested is seeing remnants of bubble collisions in the CMB, but the very likely eventuality of not seeing such a thing doesn't count as evidence against the multiverse idea.

So, I fear Brian is right: Welcome to the Multiverse, physics is going to be stuck with it for a very long time…

Comments

1. **Anonyrat**
   May 21, 2012

   On the other hand, failure to consider the multiverse can place scientists on a Keplerian treadmill in which they furiously chase answers to unanswerable questions.

   Kepler to Vincenzo Bianchi, 17 February 1619: 'Don't sentence me completely to the treadmill of mathematical calculations,…leave me time for philosophical speculations, my sole delight". (from "Music and Science in the Age of Galileo, by Victor Coelho).

   The book then goes on to describe Kepler's attempt to relate musical scales to the fastest and slowest speeds of planetary motions.

   So **that** is the Keplerian treadmill! I fail to see how the multiverse rescues scientists from the Keplerian treadmill. The multiverse sounds to me more like the music of the spheres…

2. **Bernhard**
   May 21, 2012

   The only purpose of the multiverse seems again to sell books and make headlines. I never went to a conference where I saw a speaker talking seriously about it. It does not explain anything and is completely shielded from experiment. Brian Greene used to promote string theory, but with no advances, no chances of telling if the theory is write or wrong, and the cherry on top: no signs of SUSY, the multiverse is what is left. The only thing I am amazed is how long are these guys going to repeat the same story again and again and still find suckers that are "fascinated" with it.

3. **Chris Austin**
   May 21, 2012

   According to Kleban and Schillo and Guth and Nomura, observation of positive spatial curvature at \Omega_k < -10^{-4} would falsify the eternally inflating multiverse.

   Moreover, if we live inside a bubble, we would not in general expect to be exactly at the centre of the bubble, so there would be a cosmically preferred direction pointing towards the centre of the bubble. Thus searches of this type can also favor or disfavor the multiverse.

4. **Spencer Tracy Jr.**
   May 21, 2012

   I wonder if in any of the other universes their scientists realize how ridiculous this whole multiverse thing is?

5. **Peter Woit**
   May 21, 2012

   Chris,

   Somehow, I suspect that if spatial curvature of that magnitude and sign is found, some version of eternal inflation can be found to deal with the problem.

   As for whether searches of the type you point to can "favor or disfavor" the multiverse, I gather they've already been done, with negative results, so the multiverse is already disfavored, no?

6. **Aspirin**
   May 21, 2012

   I think Brian Greene has reached the apogee of bad science popularization. His recent TED talk with the gratuitous use of multimedia makes him look less like a scientist and more like the pastor of a mega-church pitching the most fashionable current faith-based doctrine with slick animation and rock-star gimmicks but little to no substance.

7. **Peter Woit**
May 21, 2012

Aspirin,

To be fair to Brian, I don't know about TED, but my experience at TEDx was that "gratuitous use of multimedia" was part of the whole concept of the thing, and strongly encouraged. Personally I don't have anything against him or others using gimmicks, multimedia or otherwise, to get people's attention, but I do think it would be better to stick to topics with more solid content.

8. **srp**
May 21, 2012

There really is a difficult question about what can be explained according to a broad covering law (e.g. Kepler's phenomenological laws of planetary orbits being derived from Newtonian gravitation) and what is "accidental" (or "environmental" or "path dependent"), such as the particular spacing of planetary orbits in the solar system. Up until very recently, we had no ability to observe other star systems and detect their planets, yet scientists were still convinced that the particular size distribution and spacing of the planets in our system was an accident of history. Hence Kepler's attempt to derive these spacings using Platonic solids was perceived to be wrong-headed. (Although everyone hasn't given up: http://arxiv.org/abs/0903.1732)

I haven't seen anyone trumpeting observations of extra-solar planets as verifying this conviction–it's long been taken as an article of faith or common sense rather than a point of controversy that the particular configuration of our solar system is a unique combination of historical circumstance. It isn't clear why a similar type of judgment could not be applied to, say, the masses of the fundamental particles, similarly without needing justification by empirical observation of other universes.

Solar systems are taken to be individuals rather than what philosophers call "natural kinds." In biology, there was a significant change when Michael Ghiselin argued that species are best thought of as individuals rather than natural kinds–his argument has made considerable headway there. A minimal multiverse "theory" would simply claim that some physical constants, based on internal logical and observational grounds, don't look to be derivable from more-fundamental laws but are brute facts that "could have been different." That's not obviously crazy, although it may be massively premature.

9. **Peter Woit**
May 21, 2012

srp,

The problem with current string/multiverse "theory"(i.e. we're in some random "string vacuum", no way to figure out how we got there) is that it's not a theory at all, just an excuse. In particular, it doesn't tell you which things "could have been different" and which "couldn't have been different".

10. **srp**
May 21, 2012

Peter:

I could really use clarification on what you believe is the methodological difference between a) the string theory "excuse" for not having a unique solution to our universe and b) the conviction that you can't explain the solar system's configuration without reference to historical accidents. I suspect I am not alone in this lack of clear understanding, at least among your lay readers.

It may well be premature surrender to say that, for example, we can't derive the relative masses of the various particles from general principles just because string theory looks like it can't do it. There might well be alternatives that could do the job. But someone who doesn't care about string theory at all might still agree with the intuition that particle masses, say, are "arbitrary" parameters, and that people working on trying to derive them from higher symmetry principles are as misguided as Kepler was in trying to fit planetary distances to the Platonic solids.

Not sure if I'm missing your point or just communicating my point badly.

11. **Beelzebud**
May 21, 2012

As an outsider looking in, I'm simply astounded that something as unscientific as this, is being taken seriously. If we had even a shred of evidence to think this way, that would be one thing, but as far as I can tell we don't.

I hope the data from the LHC will return physics to a more reality-based science, because it's going to be hard to fund something like that again, if the public thinks the money is going to nonsense.

12. **lun**
May 21, 2012

srp: Dont know about Peter, but for us two accept the "anthropic" explanation for solar system dynamics two things had to happen:

a) A clear understanding of the theory of gravity. In particular, that it really has a continuum of solutions and no fundamental way of choosing between them
b) An quantitative experimental validation of the theory of gravity, both for problems independent of the planetary one and for the "phenomenology" of planets (deriving Kepler's laws and such).

Once (a) and (b) were satisfied, we could confortably accept an anthropic explanation for planetary motion, and this explanation became fully scientific.
Any chance of anything remotely comparable happening with the multiverse, both w.r.t. (a) and (b)?

13. **Peter Woit**
May 21, 2012

srp,

It's possible some of particle masses, mixing angles, coupling constants, theta-angles, gauge groups, gauge group representations are "environmental", but to have evidence for this you need (as lun points out in the analogy) a well-defined theory which comes with a testable explanation of which ones are "environmental" and which ones aren't. String theory doesn't have that, it basically is being used to "explain" that everything we don't understand can't be understood, and this "explanation" can't be tested.

To make the Keplerian analogy, it's as if Kepler was being told there was no point to investigating planetary motion at all, since whatever laws governed it were "enviromental", and different everywhere. No point in trying to relate Jupiter's behavior to Saturn's since they were operating under mysteriously different physical principles, due to the unknown different ways they came into being.

The problem with the string theory multiverse is circularity, string theory predicts the multiverse and the multiverse predicts that string theory can't predict anything you can test. What's being put together here is not a scientific theory, but a bunch of excuses for failure, carefully constructed to be untestable and immune to challenge.

14. **srp**
May 21, 2012

lun:

Thanks for your response. Here's my problem:

Could we not say the SM gives us a good understanding of all the observable interactions and so all the remaining parameters are plausibly environmental? My point is not that I believe this (I'm not qualified to have much of an opinion, for one thing), but that these things are judgment calls. In the solar system case, there are people playing around with the Bode-Titius law for example, but most people seem not to take this very seriously. Is the community giving up too quickly? Or are the enthusiasts on a wild goose chase?

Peter,

I totally get your complaint about using the concept of a string theory multiverse to "explain" the purportedly environmental nature of all the stuff that's still not derivable from more basic principles. It smacks of special pleading. But my question is more basic and would hold even if there were no string/multiverse idea to swat at:

Kepler practically killed himself teasing out his three kinematic laws from Brahe's data, but it worked. He also tried very hard to deduce mathematical principles for the spacing of the orbits, which most people now think was a wild goose chase. What was the a priori difference? If you could go back in time, could you advise Kepler, based on principles of theoretical methodology (NOT on the basis of knowing by hindsight how the science later developed), which of these two endeavors would be fruitful? And if so, is the argument you would make to Kepler also applicable to today's high-energy particle theorists?

My guess is that that kind of question cannot be systematically answered. In the end, judgment or guessing is required and there is not way to be sure about who is right. But perhaps I'm missing some canon of method that can distinguish these cases.

15. **Peter Woit**
May 21, 2012

srp,

I don't think there's any way to know in advance what will be fruitful things to look at because they have a simple, fundamental explanation, and which things will turn out to have a complicated, possibly "environmental" explanation. We have no idea what sort of explanation is needed to understand what determines particle masses. But whatever explanation one suggests, one has to come up with some way to test it, some way to show it's not just something one cooked up to make the problem go away.

16. **Bugsy**
May 22, 2012

First: I recall hearing a math lecture where the speaker gave a very convincing dynamical argument for the spacing of asteroid belts; sorry I don't have the reference, but basically if the rocks are in the wrong place resonances cause them to eventually be ejected from the solar system. So
sometimes weird numerical "coincidences" do have an explanation.

Second: If I understand part of srp's point, it is that just because there is a valid stochastic model for something (for instance evolution of the species) it doesn't mean that worlds actually exist where all the other paths were taken. On the face of it, that is ridiculous. Nevertheless, there is a stupid anthropomorphic argument that
I am here because I happen to be here. The only reason I am writing these lines is because my parents' DNA happened to have met up in just that way. Similarly,
any small change way back when and the humans might have been wiped out, leaving only Neanderthals left to attempt to have this discussion. But so what?
We are lucky to be here. That could increase our sense of marvel. But the universe could care less, and will wipe us out in the near or distant future anyway. What makes us think we are so important? Only the fact that we are we, the ants don't care either, unless we leave crumbs for them to eat.

But this evolution conversation happens within a given, fixed system (ours) and what seems ridiculous on the face of it is to postulate that: (1) changing the basic laws or constants can be treated in the same way; (2) we can then jump to saying that all sample paths actually "occur".

17. **Peter Shor**
May 22, 2012

Even an "anthropic" origin of the solar system should be able to explain some facts: why do all the planets orbit the sun in the same direction, and roughly in the same plane?

Similarly, there are some facts about the Standard Model that any good "anthropic" theory should explain: why are there several different generations of particles, that are identical except for their masses? If we had a truly random theory of physics, it seems very unlikely that the fermions would be organized into three generations the way they are. How does the multiverse theory explain that?

18. **D R Lunsford**
May 22, 2012

I really wish you would stay away from these depressing topics and stick to things that are interesting. Write something about K-theory. You should change the direction of this blog away from the war you've already won, and just ignore these idiotic articles. Even lay people I talk to now with interest in science have realized that all this nonsense is passe'.

-drl

19. **Chris Oakley**
May 22, 2012

I have to concur with DRL – in the sinking Titanic of theoretical physics, the Multiversers are the third-class passengers.

<aside>I notice that your book has appeared in Czech but not, for example, German or Spanish. Indirectly, you probably have Lubos to thank for that. I hope that you are sharing some of the royalties.</aside>

20. **ScentOfViolets**
May 22, 2012

There's what, 19 parameters in the Standard Model? What's wrong with that number, and why would 5 be better than 19 and 1 better than 5? In the absence of any other compelling motivation, this seems to come down to some sort of numerological argument. Is this really the present case, or am I being unfair?

Same thing with the actual values of those parameters – given that they exist, why are people so uncomfortable with those particular values? After all, they have to be some particular number; indeed, given the range of numbers available, one could just as well express bemusement at how close they are to each other – within a factor of 10^50!

Finally, is there some sort of test or principle that would tell you where the regression stops? E8 has been a contender for the algebraic structure that ties everything together. Assume for the sake of argument that this is true and a complete theory of everything emerges which gives predictions in accord with reality down to the limits of testability. Wouldn't you then have to ask why E8 and not, say F4 or G2? Or would you be able to supply compelling reasons for why these sorts of questions need not be pursued any further?

21. **vmarko**
May 22, 2012

why do all the planets orbit the sun in the same direction, and roughly in the same plane?

Ignoring all the complexities of the models dealing with solar system formation, this can basically be explained via the angular momentum conservation. Think like this: the whole solar system started out as a big lump of gas, which gradually collapsed under its own weight to become coarser and eventually create the Sun and the planets. The initial lump had some nonzero total angular momentum, and since it is a conserved quantity, the solar system as we know it today still has it. This angular momentum determines both the plane and the direction of the rotation of planets.

In fact, it would be very weird if one of the planets were rotating in the opposite direction or in a different plane. I'm not even sure that this could be a stable configuration in the long term, given the attractive gravitational force among the planets themselves.

HTH, 🙂

22. **vmarko**
May 22, 2012

There's what, 19 parameters in the Standard Model? What's wrong with that number, and why would 5 be better than 19 and 1 better than 5? In the absence of any other compelling motivation, this seems to come down to some sort of numerological argument. Is this really the present case, or am I being unfair?

Having 19 or so parameters is considered too complicated. By your argument, one could have stopped at the periodic table of elements and be content that there are cca 110 (or so) "elementary atoms", without ever asking if they have any structure or not. Today, we have cca 100 "elementary particles" which can be arranged into a similar "periodic table", so it stands to reason to ask about some deeper explanation for that as well.

The number of experimental parameters in a given physical theory is just a measure of our theoretical ignorance about the physics in question. The less experimental parameters, the better the theory, and thus the greater the understanding.

The multiverse idea is a premise that denies us the ability to ask this sort of question. And furthermore, that premise is even untestable. By some definitions, such a framework can be called a religion, rather than science.

23. **Thomas Larsson**
May 22, 2012

The anthropic explanation of the Michelson-Morley experiment: a non-zero ether wind is incompatible with human life.

24. **ScentOfViolets**
May 22, 2012

Having 19 or so parameters is considered too complicated. By your argument, one could have stopped at the periodic table of elements and be content that there are cca 110 (or so) "elementary atoms", without ever asking if they have any structure or not.

By my argument? Well, no, I'm perfectly happy with having 19 parameters if that's what it takes to have the explanatory power of the Standard Model. Less than 19 doesn't give enough freedom to satisfactorily explain observable phenomena and more than 19 is unnecessary.

Contrariwise, the motivation for finding a deeper structure to atoms wasn't because there were "too many" of them; it was because the theory of indivisible atoms did not accord with observable reality (iirc, J. J. Thomson's discovery of the electron.)

By all means, speculate on why there are three spacial dimensions instead of four or four hundred, or why we live in a world based on the L2-norm instead of the L1-norm. But istm that if your only reason for doing so is that three and two are unsatisfactory numbers then your motivation seems to be more numerology than science.

25. **Peter Woit**
May 22, 2012

ScentofViolets,

Actually, 19 is just the number if all neutrinos were massless, so you need a few more for neutrino masses and mixing angles.

One can happily decide to just accept the SM as is, and have no interest in attempts to improve it. But, it's a structure that sure looks like something we don't completely understand, so (some) people are motivated to try and do better. Such a better understanding may lead to the possibility of computing some of these parameters, or just to knowing why one can't.

26. **ScentOfViolets**
May 22, 2012

The multiverse idea is a premise that denies us the ability to ask this sort of question. And furthermore, that premise is even untestable. By some definitions, such a framework can be called a religion, rather than science.

In that case, why stop at the multiverse? Why not extend the scope to include a class of universes whose basic structure is an infinite-dimensional Hilbert space, for example? One could even populate it with hypothetical beings who use anthropic arguments to reason that *of course* since life needs an infinite number of dimensions to exist, that must be why the universe is observed to have an infinite number of dimensions.

Iow, the Landscape idea seems to be little more Tegmark's notion that all mathematical structures are realized "somewhere" and throwing in a few restrictions to make it less ridiculous as a scientific theory.

27. **uair01**
May 22, 2012

Funny to mention Brian Greene. As a layperson I read his "The Fabric of the Cosmos" and I still remember vividly how my feelings about the book changed at the break between part 3 and 4:
Part 1: Reality's Arena
Part 2: Time and Experience
Part 3: Spacetime and Cosmology
========= break =======
Part 4: Origins and Unification
Part 5: Reality and Imagination
Before the break it was an inspiring description of exciting (thought and real) experiments and weird surprising outcomes. It taught me a lot about modern physics.
After the break the whole atmosphere changed and somehow I found the whole thing depressing.
Much later I realized that it was the lack of "feet on the ground" in parts 4 and 5 that made this part of the book so unappealing.

28. **ScentOfViolets**
May 22, 2012

Actually, 19 is just the number if all neutrinos were massless, so you need a few more for neutrino masses and mixing angles.

One can happily decide to just accept the SM as is, and have no interest in attempts to improve it.

But isn't this just how science proceeds? Up to the point where neutrinos were found to have mass, 19 was enough. It is the updating of the facts that changes the number from 19 to whatever, not that 19 is an unsatisfactory number, therefore there must be something like a nonzero mass for the neutrino. imho.

But now I'm repeating myself.

29. **harryb**
May 22, 2012

I share Peter's worry about the strength of the Multiverse attractiveness. Feynman noted in the The Character of Physical Laws "you cannot prove a vague theory wrong". He preferred imagination "in a terrible strait-jacket" to derive new laws.
But in the final section of the book he goes very bleak: "some of my colleagues say that this fundamental aspect of our science will go on: but I think there will certainly not be perpetual novelty for a thousand years. This thing cannot keep going on so that we are always going to discover more and more new laws."
Ultimately he foresaw " the philosophers who are always on the outside making stupid remarks will be able to close in"
If we run out of testable new laws the stupid remarks may win.
Very precise carbon energy levels may suggest billions of other universes, or sheer luck in the only one that exists that creates self-aware neurons.
But I think Peter's blog continues to raise a key point – fundamental physics has entered a strange phase of dual impenetrability – String Theory's increasing esoteric nonsense, and the Multiverse's low science but 1960s-sci-fi appeal.
The latter will be ascendant for a while. Feynman's outlook doesn't give too much optimism it will be usurped soon. New testable laws required.

30. **Tammie Lurleen Sandoval**
May 22, 2012

This is not a joke. It is a disaster.

And its NOT about discrediting science.with the public. Horrid as that is.
Its about science destroying itself.

With nothing to show for 40 years, the big names like Greene have just left physics behind. So when the top guys spout fantasy, and the mainstream press promotes it, how do you stop this train wreck. Who has the prestige to stop them before they ruin the whole scientific endeavor?

31. **Anonyrat**
May 22, 2012

No, not nothing to show for 40 years – I disagree – we have a whole set of approaches that had to be tried in order to be ruled out, there was no a priori way of knowing that these ideas would not work (I'm assuming of course that the current trend of these ideas not working continues.) It is on the shoulders of the unsung giants who cleared away all the unworkable underbrush that the future Newton might stand.

32. **Tammie Lurleen Sandoval**
May 22, 2012

Please accept my applogy.
I didn't mean to imply that physicists have not been working or learning for 40 years.

But it is my understanding that that the results of their work is negative. In other words they have shown what wont work rather than what does. If I'm wrong on this I'd be grateful to be told.

I have the highest respect for science. CP Snow said that science must be ruthless in surpressing those who state a aflsehood, even innocently. Otherwise the public has no rational basis to trust what it cant understand. This multiverse fantasy stikes me as far worse than what Snow feared. And it appears there is no will, either among individuals or institutions, to condemn those who advance it.
If so, this wont have a happy ending

33. **David Nataf**
May 22, 2012

srp,

The specific masses of the planets in the solar can't be predicted from theory, but you can predict probability distributions for these planets, derive them from reasonable rigorous theories (eventually), and compare these to planetary distributions in other star systems, and even to unbound planets in free space.

For example, note that the Jupiter mass is a characteristic mass for masses of the largest planets around stars with planets… the largest planet is typically 0.5-2.0 jupiter masses. Thus, while the exact value can't be derived from any specific model, good models of the probability distribution can be derived from gas and accretion physics, and are testable with real-world observations.

34. **CPV**
May 22, 2012

The less inherent quality a product has, the more important the advertising campaign. I think theoretical physics tolerates the Brian Greene's of the world to help perpetuate funding in times when the cupboard is plainly bare.

35. **Mitchell Porter**
May 23, 2012

Tammie, the standard model – the 40-year-old theory which explains almost everything we know about – has a lot of properties which in turn demand to be explained. Physicists have accumulated numerous ideas about how to explain them – e.g. grand unification to explain the patterns of fermions, and weak-scale supersymmetry to explain the "hierarchy problem". Model-building in physics draws on these ideas but isn't defined by them. It's the difference between saying "Let's have a simple symmetry group that incorporates all the separate symmetries" and "Let's use SO(10) symmetry with the following superfields in the specified representations". The true advance beyond the standard model will come when someone hits upon exactly the right combination – or when the LHC or some other experiment forces decisive new data upon us. Until then it's trial and error, and we *are* learning, both by the accumulation of ideas and by the accumulation of models that are still consistent with the data, as well as by the falsification of those models which do become incompatible with experiment. Anthropic multiverse reasoning is a *very* small part of what goes on. Try the PDF at http://arxiv.org/abs/hep-ph/0207124 for a glimpse of what has actually been happening. It's a technical account, but hopefully you will at least see that it still looks like science.

36. **vmarko**
May 23, 2012

I'm perfectly happy with having 19 parameters if that's what it takes to have the explanatory power of the Standard Model. Less than 19 doesn't give enough freedom to satisfactorily explain observable phenomena and more than 19 is unnecessary.

How can you be sure that 19 is the minimal number of parameters that one needs in order to build the Standard Model? Maybe the same SM can be worked out as an effective model of some other theory containing only 2 or 3 parameters. In that case, 19 would be totally unnecessary. And in addition, this theory would provide us with deeper understanding of the mathematical structure of particles and interactions we observe in experiments. If all coupling constants and particle masses could be calculated out of some first principles and a couple of experimental parameters, it would be a massive gain in terms of "describe more by using less".

That is the idea behind the program of unification of interactions. Of course, you are welcome to be content with the SM as it is today and its 19 parameters, and free to not look for a more profound theory. But claiming that people who do look for it are numerologists is a bit too much. Seeking out a more powerful model with less parameters is IMO a perfectly valid scientific question.

The fact that so far we have no experiments to force us into looking for a stronger theory is no excuse to claim that there isn't one. Maybe the experimental data is just inaccessible for our current technological level. Imagine that Thompson did not find the electron (for example, if all subatomic particles were in principle harder to find experimentally). Would you be content with the periodic table of atoms as basic structures in Nature, given no experimental evidence to the contrary? The very fact that the periodic table is actually *periodic* is a hint that there is some simpler underlying structure. People who are (and were) looking for this strucure are certainly not numerologists.

Patterns do not emerge randomly. If your experimental data shows a pattern, it is quite reasonable and scientific to look for a rule that explains the pattern.

HTH, 🙂

37. **Trevor Turton**
May 23, 2012

I love Chris Oakley's view, "in the sinking Titanic of theoretical physics, the Multiversers are the third-class passengers." Very timely.
vmarko says: "The multiverse idea is a premise that denies us the ability to ask this sort of question. And furthermore, that premise is even untestable. By some definitions, such a framework can be called a religion, rather than science." Hebrews 11:1 says: "Now faith is being sure of what we hope for and certain of what we do not see." (NIV) Multiverse theory falls pretty neatly into that categorisation.

38. **Igor Khavkine**
May 23, 2012

vmarko wrote: *The fact that so far we have no experiments to force us into looking for a stronger theory is no excuse to claim that there isn't one. Maybe the experimental data is just inaccessible for our current technological level.*

Lets take this argument to its logical conclusion. Just like any pattern that we see in the Standard Model requires an explanation by a deeper theory, it is "obvious" that any event requires the will of an intelligent agent (or agents) as a cause. The Big Bang is an event, so lets call the agents that caused it, say, Magical Pixies. Of course, having no experimental evidence that forces us to posit the existence of these Pixies is no excuse for not doing so. After all, their existence explains so much and the needed evidence could be just around the corner, say when the LHC pumps up its energy!

Both these arguments rest on a faulty premise. In one case that is more obvious than in the other.

Would you be content with the periodic table of atoms as basic structures in Nature, given no experimental evidence to the contrary?

Contentment can be learned, while the universe is the way it is whether wer are content or not. It's also worth remembering that identifying and documenting patterns in physical phenomena is useful in itself. This activity is not required to be a stepping stone to any more fundamental theory. Consider for instance the number of ways the periodic table has been rearranged. Each rearrangement, arguably useful for whatever is the intended practical purpose, is attempted despite that fact that we *already know* the underlying fundamental theory of electron shells.

39. **Peter Woit**
May 23, 2012

Mitchell Porter,

The Witten article that you quote does an excellent job of explaining the case for SUSY/GUTs/strings. However, note that it is ten years old, and mostly about ideas that were nearly 20 years old when it was written, which haven't received experimental confirmation. Quite the opposite actually, there's:

"Supersymmetric particles should be in reach of the LHC – and maybe of Fermilab – since the supersymmetric approach to the hierarchy problem does not make sense if they are too heavy."

The SUSY/GUTs/strings paradigm has so far been a failure, and it is this failure that has led to multiverse mania on the cover of Newsweek. Trying to claim this situation is not a problem for the field is not a good idea it seems to me…

40. **Giotis**
May 23, 2012

Well judging by the title of his public lecture in Strings 2012 apparently Witten still believes in the Universe.

http://wwwth.mpp.mpg.de/members/strings/strings2012/strings_files/public/public.html

My feeling though is that again he will wash his hands like another Pontius Pilate.

But anyway let's wait to hear what the big man has to say about this Multiverse controversy…

41. **Peter Woit**
May 23, 2012

Thanks Giotis,

Very interesting, of course I'm curious what he'll have to say. Funny that the title, which a few years ago would have seemed unremarkable, now sounds kind of polemical…

42. **Allan Rosenberg**
May 23, 2012

Beelzebud,

I agree that the hype is pointless outside selling books, and, were I a physicist, the multiverse would be right at the bottom of my list of research topics. But do you really think that the multiverse is unscientific? There's nothing inherently unscientific about raising a hypothesis. It only goes beyond science when someone makes claims about the truth or falsity of the hypothesis in the absence of evidence, and I don't think Greene has done that here.

More to the point is what difference does it make whether there is a multiverse? String theory's central problem is finding at least one string vacuum that reproduces relativity and the standard model in the low energy limit, not explaining why it can't find one. Would knowing that there is a multiverse bring us any closer to finding a useful string theoretic model?

43. **Curious Wavefunction**
May 23, 2012

I wonder if Greene has milked the last drop out of his writings. I wonder about what he could possibly write again that would have the same PR potential as his existing books. I have a post on my blog lamenting the sad state of the popular physics literature partly brought about by writers like Greene.

44. **srp**
May 23, 2012

David Nataf:

Thanks for the update on solar system modeling and theory. It makes sense that not just anything can happen when a star system forms, or at least that certain things are not very probable.

But note that the sole empirical test of these theories comes from extrasolar planetary observations. My point was that long before these observations were available, many hypothetical "laws" of the solar system were considered to be mere accidental patterns found in contingent events. The non-lawful nature of the solar planets themselves, with their weird, wonderful, and largely unpredicted features lends extra force to the notion that much of what we observe came from historical "accidents."

But maybe we are giving up too soon, and it will turn out that there is a class of star systems with inner, rocky planets, a couple of gas giants around Jupiter scale, and then some smaller gas giants, some of which have peculiar inclinations of their rotational axes relative to the ecliptic. We will learn this from empirical data if it is true, though, not from first principles of mathematical symmetry.

45. **Shantanu**
May 23, 2012

Peter, I agree with Danny and Chris. No one is paying attention to multiverse.
Maybe you could talk about conformal universe conference at PI
http://www.pirsa.org/C12027

which had talks by thooft

46. **Peter Woit**
May 23, 2012

Shantanu,

Glad to hear that no one is paying attention. The conformal universe conference talks look interesting. A good reason for me not to talk about them though is that I know hardly anything about that subject…

47. **Tim van Beek**
May 24, 2012

> Well judging by the title of his public lecture in Strings 2012 apparently Witten still believes in the Universe.

…

Thanks for the heads-up, I almost missed the fact that Strings 2012 takes place in Munich, Germany (it is not widely advertised, for reasons unkown, at least not in Munich itself 🙂 Although I'm sure that Peter intends to cover it, right?

I will see to it that I make it to that public lecture.

48. **Kyrilluk**
May 24, 2012

*Which is all just to say that the multiverse falls squarely in the domain of **high-risk science.***
High-risk science: is it the politicaly correct way of saying "pseudo-science"?

49. **Tammie Sandoval**
May 24, 2012

I thank Dr Porter and Dr Woit for their responses.

Regarding Dr Porter's recommendation , I agree that I should read that article by Dr Witten. At best however, it would be a huge amount of work to understand it. For that reason I'm secretly hoping that string theory falls, letting me off the hook.

I especially thank Dr Jogalekar for reminding me that most of physics is a quiet and serious endeavor. Its a fact that people care most about advancing their religion (or their lack of it). So things like string theory and the multiverse will always, and justifiably, grab the most attention. By contrast, chaos, turbulence, superconductors, and adaptive systems don't say much about our existence, but as sciences they are truly beautifu, and are making great progress.

Finally, I find the phrase "high-risk science" to be disgustingly false. Dr Green would do well to disown it. Whatever one can say about string theory, tenured professors working away in their specialty is not "high risk". What would be high risk would be trying something new. Take the military. It needs diligent people of many talents. But it is ridiculous when a procurement officer poses as a war hero.

Please note, this has nothing to do with the merits of Dr Greene's string theory, and whether he is right in persuing his specialty instead of abandoning it. But he should spare us the silliness.

50. **Bob Levine**
May 24, 2012

Multiverse 'studies' may indeed have hit the big time, as Peter suggests, in popular culture terms. But the thing to watch for is just how much of an impact they (will) have in academic physics, and I think the right metric here is not how many news releases or conference presentations by Susskind, Greene et al. we see over the next few years, but how many doctoral dissertations are written and defended, in major departments, which take the multiverse as the central topic, or even a crucial component. Speculation is free, but if I were betting I'd put (a LOT of) my money on that number being on the rather low side…

51. **ComeOn**
May 24, 2012

Completely agree with Bob Levine above. In academia you can go years without hearing anyone mention the word "multiverse". But in pop science, and blog science, it's infinitely more prominent.

52. **jg**
May 24, 2012

The Witten Lecture 'String Theory and the Universe' due at String 2012 has been given by him various times in the previous couple of years, you can watch one version online at IOP site:

http://www.iop.org/resources/videos/lectures/page_44292.html

53. **Giotis**
May 26, 2012

Yeah I've watched this lecture before; I wasn't quite remembering the title though. But even if his 2012 public lecture is exactly the same to this one there would be a crucial difference; audience's questions. Even if Witten does not want to elaborate or take sides the multiverse controversy would be certainly brought up by the audience and we may witness some interesting answers.

Although as I said I'm not very optimistic about it. My impression so far is that Witten is more or less agnostic about the subject. Like in this lecture he is very cautious and often says that we need more clues and more work needs to be done in order to decide whether the multiverse picture is really a prediction of the theory.

Nobody could disagree with that of course taking into account that M-theory is poorly understood. Yet again at this point it's hard to imagine how the multiverse paradigm shift could be evaded within the context of String theory. But you never know…

54. **Truth**
May 26, 2012

As Brian Greene points out, the multiverse idea appears in numerous directions of research. The properties of GR, quantum mechanics, inflation, string theory, dark energy, etc, all point in the same direction and provide independent circumstantial pieces of evidence for it. Right now it is our best idea about the world, given the information available to us. It is conceivable that it is wrong, but at the moment, it is more likely to be right than wrong.
Anyone who shows hostility to it are really just people who lack some level of imagination and want to just cling on to old archaic ideas.

55. **David Nataf**
May 26, 2012

Truth,

How do the properties of general relativity suggest the multiverse?

56. **Peter Woit**
May 26, 2012

David,

Presumably “Truth” is referring to eternal inflation models, which in his or her mind are now part of GR.

Truth,

If you’re representing Brian’s argument as it’s “more likely to be right than wrong”, I think you’re seriously misrepresenting what he has to say, which includes

“That there are ways, long shots to be sure, to test the multiverse proposal reflects its origin in careful mathematical analysis. Nevertheless, because the proposal is unquestionably tentative, we must approach it with healthy skepticism and invoke its explanatory framework judiciously….

But as with all rational bets, high risk comes with the potential for high reward…

The multiverse proposal might be wrong. But it might also be the next step in this journey, unveiling a breathtaking panorama of universes populating a vast cosmic landscape. For some scientists, including me, that possibility makes the risk well worth taking.”

Saying an idea is “high risk” means it’s more likely to be wrong, not more likely to be right. Also unlike you, I suspect that Brian’s point of view on the majority of the physics community is not that they

“are really just people who lack some level of imagination and want to just cling on to old archaic ideas.”

but that they have perfectly good reasons for being skeptical of a very speculative idea there is little evidence for, no matter how many articles about it appear in the popular press.

57. **Truth**
May 26, 2012

Peter, my first comment was my summary of Brian’s view. But from “Right now it is…” it was my own opinions.

As to your final comment that there are “perfectly good reasons” for thinking that there is only a single universe, I have never heard it. Given that the laws of physics were evidently sufficient to create at least one universe, I dare say it could create multiple, if not infinitely many. I suspect it is much harder to show that there is only one universe than many, especially when, GR, quantum mechanics, inflation, etc, are pointing in the other way.

58. **Truth**
May 26, 2012

Actually, I would add that in fact you are seriously misrepresenting Brian’s view. He does not say that the multiverse idea is likely wrong, he says that there are a few possible ways to test it directly (such as bubble collisions in the CMB) and it is unlikely that we will see that direct evidence. So, according to Brian, it is high-risk to pursue a proposal for direct detection, which may not materialize. But he is certainly not saying that the multiverse idea itself is unlikely to be true, it just may be difficult to test.

59. **Peter Woit**
May 26, 2012

Truth,

I didn’t say there are good reasons for believing there is only one universe. I said there are good reasons for being skeptical of the scientific multiverse proposal Brian is describing (and I suspect he might agree). The “high risk” in such proposals, which I think is what he is referring to, is that they may be inherently untestable in any way, and thus not science. A large majority of physicists see discussing the multiverse as an empty activity that is not science, since it is not testable, and claims to believe in either one or multiple universes are pseudoscience absent a plausible way to deal with the testability problem.

60. **Pravda**
May 26, 2012

“Truth” is, himself, proof of the existence of alternate universes, as he is obviously posting from one.

61. **Truth**
May 27, 2012

Certainly there are ways to test it, such as CMB data. So that kills your idea right there.

Secondly, even without direct evidence of that sort, the multiverse is an extrapolation of other well tested theories. Extrapolation is not only part of science, it is THE WHOLE POINT of it, as stated very eloquently by Feynman.

Anyone who goes around saying it is not science is being silly, and not engaging in the real issues at all.

62. **Bernhard**
May 27, 2012

Truth,

the multiverse is a speculation, not a theory. Speculations are a part of the scientific processes but not sufficient to be consider science. A scientific theory is something more rigorous and ultimately it needs to meet the criteria of testability and it must be falsifiable. Of couse one has the right to speculate about multiverses, say time is multidimensional and pretty much whatever you want, but without a theory and solid quantitatively predictions this stays in the realm of speculations. Some may find fascinating but others will find it boring, but anyway there is ever reason to be completely skeptical about it. Could be, could not be, without solid predictions, there is little to discuss.

The point you raise is that on the contrary, bubble collisions in the CMB are a real prediction of it and therefore it can be tested. This is certainly a scientific point you are raising. More specifically I believe you are talking about things like this paper: http://arxiv.org/abs/1012.1995

If you are read this paper carefully you will see there are many caveats and in particular their argument rests on azimuthally symmetric temperature modulations, which as the authors observe “are not unique to bubble collisions”. The paper is at best inconclusive. So, it might be that the multiverse picture will evolve into a robust scientific theory, but until then selling the idea as scientific consensus and making noise in popular articles confusing the non-scientists is not the best idea to do this, regardless if one thinks one or 20 universes are “more likely”.

63. **Truth**
May 27, 2012

Just because the CMB thing is inconclusive right now, certainly does not make it unscientific. There are numerous inconclusive things in the world. I dunno…GUTs, Higgs, what killed the dinosaurs, the origin of life, you name it…it is absolute garbage to suggest that work on these topics is unscientific because these issues remain inconclusive. The WHOLE POINT of science is to work on inconclusive things. That's what scientific RESEARCH is. We don't know the answers today, for instance, if there is a multiverse, if a meteor killed the dinosaurs, how the pyramids were built, etc, but we can work as hard as we possibly can to try and find out. Maybe we'll know in 1, 10, 100 years from now. That's science.

Sometimes the evidence fir things can be fairly direct and sometimes it is rather indirect, for instance, if it comes from extrapolating the predictions of a well tested theory. As I said before, that is the current situation for the multiverse proposal. It comes from extrapolating the predictions of GR, quantum mechanics, inflation, etc. extrapolation is what science is all about. To call it unscientific is simply wrong.

64. **Peter Woit**
May 27, 2012

Truth,

We've seen this misuse of the term "test" endlessly with string theory. In the conventional meaning of the term, a "test" is something you can fail, with negative consequences for your theory. Even the most enthusiastic multiverse supporters don't think there's much of a chance that new CMB data will actually vindicate multiverse theory. When Planck data comes in, sometime within the next year, I predict it's not going to contain evidence of bubble collisions, but no multiverse proponent is going to give up on the idea because of that.

You seem to misunderstand what the word "extrapolation" means, in Feynman's usage. He was referring to applying a tested theory to new domains, not a new theory. A multiverse with different laws of physics is not an extrapolation of any tested law of physics.

65. **Truth**
May 27, 2012

And of course, the multiverse did make a prediction. It predicted a small, but non-zero cosmological constant of the same order of magnitude that was later observed. Ok. So stop saying it does not make predictions. It did. It was observed. It is our only understanding of it. You may not accept this evidence as enough. Fine. Some people do. Some people don't. That is science. Get it?

66. **Truth**
May 27, 2012

100% wrong Peter. The multiverse is an extrapolation of inflation, which combines quantum mechanics with general relativity and some scalar field source. It is a tested theory.

67. **Peter Woit**
May 27, 2012

Truth,

The theory of inflation you are talking about involves a choice of potential for a single scalar field. If you extrapolate it way beyond what there is experimental evidence relevant to, you can get bubble universes, all with exactly the same physics as ours. That there's an infinity of bubble universes out there with exactly our physics is possible, but probably untestable, as well as not being very interesting since it explains nothing about our own. If you want to claim that the CC and other parameters are different in every different universe, and then use anthropics to "predict" the CC or anything else, (yes, lots of people think anthropic "predictions" don't deserve to be called "predictions", for good reason…) you need to invoke a new theory, like string theory, not extrapolate a conventional, tested one.

68. **Henry Bolden**
May 27, 2012

Multiverse theory predicts the existence of a universe where multiverse theory has been proven to be incorrect. Greene's paradox?

69. **MultipleMulti**
May 27, 2012

Re Witten, does anybody know why he's working on Khovanov homology now? What do knots have to do with String theory? Do the strings form knots? Do the Feynman histories form knots?

70. **Truth**
May 27, 2012

Well it seems we have made some progress Peter. At least you have agreed that by extrapolating inflation we get a multitude of universes. Ok, so that much is settled. It is an extrapolation and it is part of science.

Here your complaint is that the laws of physics are still the same though. Well, indeed there are different levels of the multiverse. Inflation forces on us a level, where there are many universes, although the laws can be the same. The universes do differ, however, in their distribution of matter. Eternal inflation generates every consistent distribution of matter. For instance, nc,uding universes with different values of the amplitude of density fluctuations.

As to having different laws of physics, for instance with different values of the CC, we indeed need a potential with multiple minima, which is a much more severe extrapolation. But it is an extrapolation, nevertheless.

71. **Peter Woit**
May 27, 2012

Truth,

You seem to know enough to know very well that the 10^500 different minima necessary to make the anthropic CC argument work don't come from any tested theory, even one as little tested as inflation. The KKLT mechanism and any of its variants invoke a huge array of speculative ideas from string theory, none of which have any experimental evidence for them. KKLT is an extrapolation of our current theory only in the same sense that any complicated, untestable, speculative idea for a theory of everything is an "extrapolation". I suspect Feynman is rolling over in his grave to have his name invoked in this way, it's pretty clear what he would think of your claims.

72. **Truth**
May 27, 2012

Again, I am glad that you admitted in your previous comment that inflation implies a multiverse from extrapolation.

In my most recent comment, I simply said that multiple minima are needed for different laws of physics. I didn't say anything about KKLT, etc. It is a straw man argument for you to criticize KKLT as a way of attacking my quite reasonable, and largely separate, comment.

73. **vmarko**
May 28, 2012

I am glad that you admitted in your previous comment that inflation implies a multiverse from extrapolation.

Khm, inflation does not *imply* a multiverse, at best it only *suggests* a multiverse. Namely, one can have inflation without multiverses. In loop quantum cosmology, for example, one doesn't even need an inflaton field to produce inflation. And one stays in a single universe after the inflationary period. You can take a look at gr-qc/0206054 for some details.

HTH, 🙂

74. **Marty**
May 28, 2012

"Truth",

You apparently believe that inflation is correct in its current form, a form which *does* appear to imply "many universes." This form may or may not ultimately be correct, but it is certainly not a "slam dunk" case. Here are a few links to talks and papers that you might find enlightening; they may help temper your confidence a bit:

Paper by Roger Penrose (1989; behind a paywall, unfortunately)
Penrose talks at Princeton, three total (inflation is #3; look at October 2003)
Steinhardt talk at Perimeter Institute, 2011
Turok talk at PI, 2011
The Inflation Debate (Sci. Am. article on Steinhardt's Princeton site; free)

There is very strong observational evidence for inflation, but as you will learn if you read and watch the talks the evidence is for the much simpler picture that emerged in the early 1980s. Since then, some severe *theoretical* difficulties with the inflation idea (not observational problems) have become apparent, and it isn't obvious how to fix them in spite of lots of effort. These problems have nothing to do with string theory, by the way — they are problems with inflation models themselves.

There is essentially zero observational evidence for eternal inflation, however. It is only implied by the inflation idea if general relativity plus quantum mechanics is the right framework to use prior to the Big Bang. If you study the theoretical problems, you may agree that it doesn't really make sense to use general relativity in a regime where it presumably breaks down, i.e., at and beyond the Planck scale.

Regarding observational tests of bubble collisions, it is extremely optimistic to think they will ever be successful. If bubble universes are real (questionable!), almost all such collisions that would be visible today would need to have happened at very early times, and their effects would generally be restricted to an extremely small angle in the sky, far too small to be discernible, much less provide useful observational evidence (the "look elsewhere effect" …). The only real hope is that relatively recent collisions occurred and left visible traces, but those are highly unlikely.

75. **Peter Woit**
May 28, 2012

vmarko and Marty,

The situation of experimental evidence for inflation is complicated, and multiverse maniacs typically start from some outrageous claim about how well established the theory is. It's very rarely pointed out though that even if you accept the most optimistic versions of the inflation story, that actually has nothing to do with the "extrapolation" that "Truth" and others are advertising. Even eternal inflation doesn't give you "different laws of physics" in different universes, it just gives you lots of universes with the same laws of physics. To get "different laws of physics: you need to introduce something like the hundreds of moduli fields of string compactification schemes, the exact hideous mess that has turned string theory unification into a failure.

76. **Truth**
May 28, 2012

Vmsrko and Marty,

Most of what Peter says here is wrong. As I mentioned before, eternal inflation gives you different universes with different properties, e.g., different amplitude of density fluctuations, different spectral index, curvature, etc. These universes would look quite different. It is a type of multiverse.

To get different microscopic physics, you do not need 100s of stringy moduli fields as Peter is advocating. A potential with just 2 local minima would suffice to have 2 different kinds of laws; that is enough for one to talk of a multiverse. And it does not rely on the validity of string theory at all, only quantum mechanics and GR. The only place where string theory enters the discussion is that it re-affirms this view with many, many fields and minima.

77. **Peter Woit**
May 28, 2012

Truth,

You know very well that cooking up an inflaton potential in standard inflation theory to have extra minima is not what is being advertised. This gives no anthropic explanation of the CC for instance, which is the main "evidence" given for this scenario..

78. **Truth**
May 28, 2012

Peter, I have addressed all these issues clearly and correctly in my previous posts. Inflation is well tested and when extrapolated it predicts a basic type of multiverse which look different. A potential with 2 minima gives different microscopic laws and is a more crude extrapolation. While many, many minima of the string theory type is a much more severe extrapolation, which is less likely to be true, but comes from extrapolating wuantum mechanics and GR to the extreme, as it is the only known consistent picture. But even if you hate the latter, a more basic type of multiverse does come from extrapolation. That is the point I am making. If others wish to focus on other issues, that is their business.

79. **Peter Woit**
May 28, 2012

Truth,

You just keep repeatedly abusing the term "extrapolation" (5 times in your latest comment). Repeated explanation from my why this is abuse of the English language and the memory of Richard Feynman would be pointless.

80. **Bernhard**
May 28, 2012

Truth,

can you point out one (or more) reference (s) that corroborates your claims? I think your use of the word "extrapolation" is not entirely clear, at least to me.

81. **ScentOfViolets**
May 28, 2012

I suspect it is much harder to show that there is only one universe than many, especially when, GR, quantum mechanics, inflation, etc, are pointing in the other way.

Looking at these last exchanges, it appears that "truth" is substituting some , er, nonstandard definitions of common terms for the usual ones and not telling anyone. The particular term I'm drawing attention to here is "skeptic" or "skeptical", which "Truth" seems to think is a synonym for "disagree". It's not.

For example, I'm skeptical of any sort of theorization which relies on the existence of a multiverse to make it work, but that doesn't mean that I disagree with the notion that there are multiple universes instead of just the one. It merely means that I'm agnostic wrt the number of universes.

Science being what it is, being a skeptic means that I don't have anything to prove; the burden of proof in this particular instance is on "Truth". And so far, he's not being very convincing. Name-calling is seldom an effective way to get people to agree with you 😉

82. **Truth**
May 28, 2012

Peter, above you admitted that when inflation is extrapolated you get many universes. These universes have qualitatively different properties, such as the curvature or amplitude of density fluctuatuions. So even you have conceded that the extrapolation of inflation leads to a multiverse. Here even you have used the term "extrapolated" in the same way as me, and indeed in the same way as Feynman. It is ridiculous for you to then mangle the issue by sneering at KKLT, or other such things, as though that were the basis of my argument which I have clarified repeatedly and consistently.

83. **Truth**
May 28, 2012

Scentofviolets, I never used the term "skeptic", and I never did any name-calling.

84. **harryb**
May 28, 2012

Truth et al,

May I quote from Feynman directly – The Character Of Physical Law, Chapter 7, Seeking New Laws. I quote at length because the original text it is well written and compact:

"In general we look for a new law by the following process. First we guess it. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation, to see if it works. If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is – if it disagrees with experiment it is wrong. That is all there is to it."
It seems the WHOLE POINT of science – according to Feynman – is for any smart guess to agree with experiment. The Multiverse may be a smart guess – but we need an experiment that rules it in or out.
The Multiverse sounds smart, and may well be correct. But we have to be smarter that just posing the guess – the hard work as Feynman knew – is in devising the experiment that will obviously – to many – make it more than just a good guess. Guesses are cheap – experiments to verify them are, very, hard work.

85. **Anonyrat**
May 29, 2012

Since inflation was proposed around 1980, and Feynman died around 1988, surely we must have some of Feynman's opinion of the idea instead of having to rely on extrapolation.

86. **Igor Khavkine**
May 29, 2012

@Anonyrat: Something slightly relevant. Here's Feynman weighing in on the somewhat larger question of the initial conditions of the universe. My interpretation: curiosity with extreme caution.

Feynman: Take the world from another point of view (3/4)
http://www.youtube.com/watch?v=uNOghidK2TY (from 7:10)
Feynman: Take the world from another point of view (4/4)
http://www.youtube.com/watch?v=mvqwm6RbxcQ (until 1:18)

87. **Tim van Beek**
May 29, 2012

jg wrote:

> The Witten Lecture ‘String Theory and the Universe’ due at String 2012 has been given by him various times in the previous couple of years, you can watch one version online at IOP site:
>
> http://www.iop.org/resources/videos/lectures/page_44292.html

Thanks for the tip. That is the standard history and motivation of string theory, so not very interesting to me, but probably for everyone who never heard the story before. I think it is safe to assume that Witten won’t invest much time to update the talk for Strings 2012 (no reason to).

Nevertheless, if I go to the strings 2012 talk and get the chance to ask a question, I’ll probably ask about what he thinks a young PhD should specialize in, or what he is currently working on and why, or something like that. But I’m taking suggestions 🙂

88. **Bernhard**
May 29, 2012

Tim van Beek,

I would be very curious to know Witten’s answer. Years ago (2004 if I’m not mistaken) a friend of mine asked him a similar question for which he answered (I’m quoting it this from memory so it’s more or less this): “Don’t know, but something with string theory”. Would even Witten give such an advice today?

89. **Chris Austin**
May 29, 2012

The British Astronomer Royal Sir Martin Rees, who seems to favor a multiverse picture, stated on page 115 of his 1999 book “Just Six Numbers” that a value of the relative amplitude Q = (delta rho)/rho of the primordial fluctuations substantially larger than 10^{-5} would make the universe too violent for life. This appears to contradict Paul J. Steinhardt’s statement, on page 43 of the Scientific American article by linked by Marty, that the universe is smoother than it needs to be to support life, so that anthropic selection cannot explain the value of Q. Is Rees’s statement now discredited, or is there no consensus on the matter?

90. **Peter Woit**
May 29, 2012

Tim van Beek,

There is something new since 2010, the fact that no SUSY (or anything else relevant to string theory) has shown up at the LHC. For years Witten and other string theorists have held out hope that the LHC would find something that would vindicate string theory (at least low energy SUSY). I’m curious to know at what point Witten will give up on this hope, and if that happens how it will change his view of string theory.

91. **Shantanu**
June 4, 2012

Peter and others, there is some discussion of multiverse in the panel debate on emergent spacetime at KITP conference.
http://online.kitp.ucsb.edu/online/bitbranes-c12/emergence/

92. **Peter Woit**
June 4, 2012

Shantanu,

There’s not much about the multiverse there, but interesting if you want to keep up on the latest thinking about emergent gravity. I gave up after watching for a while, seemed to me there was nothing new going on in this subject, in particular nothing new about the obvious question: “emerges from what?”

93. **Nick M.**
June 8, 2012

Let me preface my post by saying that although there have been over ninety responses to *“Welcome to the Multiverse”*, it has all been a very interesting and lively read.

> **“**So, I fear Brian is right: Welcome to the Multiverse, physics is going to be stuck with it for a very long time…**“**

A long time indeed, as it looks as though Stephen Hawking (along with James Hartle and Thomas Hertog) aren’t quite ready to give up on the Multiverse/String Theory paradigm anytime soon either; see version 2 of the this paper posted in arXiv on May 30th titled “Accelerated Expansion from Negative Lambda”. Incidently, I found this link via the New Scientist article “Hawking’s ‘Escher-verse’ could be a theory of everything”. (Note that you will have to be a registered user to completely read the New Scientist article.)

Friedrich Hirzebruch 1927-2012

May 29, 2012
Categories: Obituaries

The German mathematician Friedrich (Fritz) Hirzebruch passed away a couple days ago, at the age of 84. Hirzebruch was perhaps the most important mathematician in the Germany of the postwar period, responsible for the founding of the Max Planck Institute in Bonn, as well as the yearly Bonn Arbeitstagung conference. The Mathematics Genealogy Project lists him as having 52 Ph.D. students and 368 descendants. There's a wonderful interview and article about him at the Simons Foundation web-site.

Hirzebruch's first great mathematical achievement was the proof in 1954 of the generalization of the classical Riemann-Roch theorem to higher dimensional complex manifolds, now known as the Hirzebruch-Riemann-Roch theorem. This used the new techniques of sheaf cohomology and was one of the centerpieces of the explosion of new results in geometry and topology during the 1950s. Further generalization of this led to the Grothendieck-Riemann-Roch theorem, and the Atiyah-Singer index theorem. Hirzebruch's monograph on the subject *Topological Methods in Algebraic Geometry* was the essential textbook in this area for many years.

The last time I heard Hirzebruch talk was at the celebration of Atiyah's 80th birthday in Edinburgh, where Hirzebruch gave a talk about his interactions with Atiyah. He displayed some of their correspondence from this period, which makes fascinating reading and is now available here.

With the loss of Raoul Bott a few years ago, and now Fritz Hirzebruch, the math and physics communities are deprived of two of the great figures who built parts of modern mathematics that appear crucially in the structure of the Standard Model. Much of this connection between math and physics remains a mystery, and it's too bad they won't be around to help make progress unraveling it.

Update: The New York Times has a very good obituary of Hirzebruch here.

Comments

1. **Chris Austin**
   May 29, 2012

   Could you elaborate a little on the contributions of Bott and Hirzebruch to the mathematics of the Standard Model?

2. **Peter Woit**
   May 29, 2012

   Chris,

   This is a long story, and I wrote a significant amount about it in my book. The basic objects in the SM are the Dirac operator on spinors, coupled to a connection. These are exactly the same basic objects that appear in the Atiyah-Singer index theorem. The Hirzebruch-Riemann-Roch theorem is in some sense the crucial case of the index theorem, using the Dolbeault operator instead of the closely related Dirac operator. Bott's work made possible the general topological K-theory behind the index theorem. From the late 70s on, both Bott and Hirzebruch were very active in promoting the interaction of mathematics and physics, often giving beautiful expository lectures trying to explain this material to physicists, and relate the points of view of the two subjects.

3. **Chris Austin**
   May 29, 2012

   Thanks, Peter

4. **David Brown**
   May 30, 2012

   2 questions: (1) Is the problem mentioned at the end of Hirzebruch's Atiyah80 video/lecture still open? (2) Of the mathematicians who are roughly of the generation of Hirzebruch & Atiyah, which of them made the most significant contributions to the mathematics of the Standard Model of particle physics?

5. **Peter Woit**
   May 30, 2012

   David,

   I have no idea if anyone took Hirzebruch up on that challenge (he was asking for an embedding of E6/(Spin(10)xU(1)) in dimension 61, or, failing that, 62 or 63, hopefully showing that the non-embedding result in dimension 60 is sharp).

   Atiyah, Singer, Bott and Hirzebruch all have interacted strongly with physicists around issues raised by Yang-Mills theory, the Standard Model and attempts to extend it. Since the Standard Model was in place back in 1973, before they got involved, this isn't activity that has actually changed the Standard Model, rather led to a better understanding of it, especially issues related to anomalies and instantons. Atiyah and Singer have both been extremely active in working with physicists, with Bott only a bit less so, Hirzebruch significantly less.

6. **jg**
   May 30, 2012

   Hi Peter,

   no disrespect to a great mathematician, no doubt he made a beautiful contribution to pure mathematics, but isn't it a bit much to suggest the intricacies of the mathematical arguments Hirzebruch (et al) developed are relevant to Nature/Physics?

   I mean isn't this site devoted to the idea that over-elaborate mathematical arguments are not the way to understand Nature?

7. **Peter Woit**
   May 30, 2012

   jg,

   My problem is not with the use of mathematics to describe nature, quite the opposite. I believe that experience shows it is the deepest ideas in mathematics that are most likely to give us insight into fundamental physics at a deep level. The Hirzebruch-Riemann-Roch theorem is a very deep idea about mathematics, with close connections to things we have impressive physical evidence for (the Dirac operator, gauge fields), so this seems to me to be worth a lot of attention.

Yes, you can also use the HRR theorem to do some computations in horrifically complicated and ugly constructions used in failed attempts to get string theory unification, but there the problem is the construction, not the theorem. The HRR theorem ends up being useful in that case because it is so powerful and so general that it's one of the few tools that can say something about such a complicated mess.

8. **jg**
May 30, 2012

ah ok,

I personally don't believe the breakthrough requires a deep result in pure mathematics, but I admire your position on this 🙂

fxqi's 2012 essay question:

Which of Our Basic Physical Assumptions Are Wrong?
http://fqxi.org/community/essay

9. **P**
May 31, 2012

Peter,

HRR is very nice indeed, but perhaps your dislike of string compactification is causing you to leave out his surfaces, which are some of the simplest surfaces in complex algebraic geometry. 🙂

http://en.wikipedia.org/wiki/Hirzebruch_surface

Cheers,
P

25 Years of Topological Quantum Field Theory

May 31, 2012
Categories: Favorite Old Posts, Uncategorized

It occurred to me today that right about now is the time someone should have chosen as the date for a celebration of the 25th anniversary of the birth of the idea of "Topological Quantum Field Theory", as well as some much less well-known ideas about the relationship of QFT and mathematics that still await full investigation.

Just about 25 years ago, from May 12-16 1987, there was a remarkable conference that I attended at Duke, to celebrate the "Mathematical Heritage of Hermann Weyl", two years after the centenary of his birth. The proceedings were published a year or so later. At this conference, Michael Atiyah gave an amazing talk with the title *New invariants for manifolds of dimensions 3 and 4*. In it he unveiled a vista of new ideas about topology that would dominate the subject for years to come. For symplectic manifolds he described Andreas Floer's unpublished new ideas about what came to be known as "Floer Homology" and how these gave new invariants of such manifolds and their Lagrangian submanifolds, invariants related to very recent work of Gromov (now known as "Gromov-Witten invariants"). Replacing 1d (Lagrangian paths) and 2d (pseudo-holomorphic curves) objects in a symplectic manifold by 3d (flat connections) and 4d (instantons) objects in a space of connections on a 4d manifold gave yet another whole new world of mathematics. This is the subject of Floer Homology and Donaldson invariants for 4d manifolds, possibly with boundary, (and was based on work of Floer and Donaldson that was still unpublished). Finally, the Euler characteristic of Floer Homology was identified with a new invariant due to Casson (also unpublished, it seemed like nothing Atiyah was talking about was yet written up), which was a 3d invariant that fit beautifully into the whole picture.

There's a copy of Atiyah's write-up of the talk online here (perhaps the AMS will ignore any intellectual property issues here for the greater good). I see that, increasingly like everything else in the world, electronic access to the book is controlled by Google, see Google Play, which I didn't even know existed.

An inspiration for Floer had been Witten's ground-breaking paper "Supersymmetry and Morse Theory", which dealt with the relationship between Morse theory and some supersymmetric quantum mechanics models. Atiyah explained some of these ideas in the talk and towards the end conjectured that quantum field theories were part of the story. His conjectural QFTs would have Floer homology as their ground states and would turn out to be the basic examples of TQFTs. After repeated prodding from Atiyah, Witten a year later produced such theories as twisted N=2 supersymmetric QFTs: a sigma model for the symplectic manifold case, and a supersymmetric Yang-Mills theory for the 4d case. In his final remarks, Atiyah raised the issue of knot invariants and the Jones polynomial, suggesting that this too would have a QFT interpretation, something that came to fruition a couple years later with Witten's Chern-Simons theory that won him a Fields medal. Witten was at the talk, and I recall him coming down to the podium to ask Atiyah some questions about the Jones polynomial immediately after the lecture.

The Duke conference was also significant to me for personal reasons. At the time I was a postdoc at the ITP in Stony Brook, looking for a job and trying to figure out my future. It was becoming clear that physics departments didn't want to hear from any young theorists interested in mathematics who weren't doing string theory. I had been spending a lot of time at Stony Brook learning more mathematics and talking to some of the geometers there, who were housed on the floor below the ITP. My trip to the conference at Duke was motivated partly by a desire to visit my grandparents who were in North Carolina for the summer, as well as a plan to investigate prospects for a career change into mathematics. The Atiyah talk bowled me over, convincing me that the intersection of mathematics and QFT had an exciting future. Getting to know a bit more about the mathematical community showed me it could be a great place to work, in many ways much more welcoming and open to new ideas than the physics community. I soon moved up to Cambridge for a year, where the Harvard Physics department let me use a desk, and found a part-time job teaching calculus at Tufts.

What's remarkable to me now looking at the conference volume is how much exciting material was being discussed, in addition to the fantastic Atiyah talk. Raoul Bott gave a wonderful talk on Borel-Weil-Bott (and its relation to quantization), David Vogan on representation theory in general (and its relation to quantization). Roger Howe has a contribution also about deep connections between quantum mechanics and what he calls the "oscillator representation". Jim Lepowsky was talking about Kac-Moody Lie algebras, vertex operators and the Monster group, Is Singer about quantizing gauge theory and string theories, and there were a host of other wonderful talks on topology and geometry.

One topic that I didn't really appreciate at all at the time was that of Langlands theory. Langlands himself was there, talking about Shimura varieties, and James Arthur talked about the Trace formula and its applications to Langlands theory. I think I may have missed Witten's talk, since I don't remember it, but his contribution to the conference proceedings is about how to abstractly think about the theory of 2-d free fermions, in a form that makes sense on an arbitrary curve. A few weeks later (June 23), his amazing paper Quantum Field Theory, Grassmanians and Algebraic Curves was submitted to *Communications in Mathematical Physics*. If I had to point to a paper that truly looks like 21st century work that fell by accident into the 20th century, this would be it. It gives some strikingly different ways of thinking about QFT in 2d, including tantalizing connections to the structures ("automorphic representations") that show up in Langlands theory, and has provided inspiration to many people over the years, including the geometric Langlands program. Atiyah's lecture pointed to new ideas relating QFT to cutting edge geometry and topology, ideas that quickly led to lots of progress, while Witten's ideas related QFT to representation theory and Langlands theory, in ways that we still have yet to fathom.

Comments

1. **Jeff**
   May 31, 2012

   Memories. Wasn't at the conference, I was a brand new grad student, but a few years later my orals were to present that Witten paper. Took some work :-). There's a great book by John Roe which helped a lot. Of course, I was only interested in the math side…

2. **theoreticalminimum**
   June 1, 2012

   Thank you for this informative post, Peter.

   It's sad to realise that Andreas Floer would tragically end his life 4 years after that Duke conference, at the age of 35 and at a time when he was at the peak of his creativity.

   I would recommend the Floer memorial volume (Hofer et al.), which is the best collection of articles I can think of that captures the effervescence of the developments at that time in symplectic geometry and quantum field theories.

3. **Florin**
   June 1, 2012

   Hello,

   I've been quietly following with great interest your website for a couple of years now and although I don't have a formal training in mathematics I grew very interested in topics such as Langlands program or any other attempt to reveal stronger connections between mathematics and reality/physics. And while I somehow have some grasp on Noether's theorems and I have some kind of intuitive understanding of the importance of representation theory for say the quark model, the mathematical apparatus for even having a hint about the Langlands program seems to me very abstract and inaccessible.

So I was wondering, what would be the mathematical requirements for starting to approach this subject and also could you please indicate to a less mathematical introduction to the Langlands program?

Thank you, your answer would be much appreciated!

Regards,
Florin

4. **Peter Woit**
June 1, 2012

Florin,

The Langlands program brings together several fundamental areas of mathematics, so to really understand and appreciate it, you need some serious background in modern mathematics. Edward Frenkel's lectures this past semester at Columbia provide a good introduction and explain the links to affine Lie algebra representations and conformal field theory.

You could also take a look at this survey of the subject from Langlands himself

http://publications.ias.edu/sites/default/files/gibbs-ps.pdf

A good place to start might be the popular book by Ash and Gross, called ~~"Fearful Symmetry"~~.

[Oops, the correct title is Fearless Symmetry, thanks to all for the correction]

5. **Ossicle**
June 1, 2012

I believe these are the Frenkel lectures Peter refers to:

http://www.youtube.com/playlist?list=PLA391DE72F863E030&feature=plpp

6. **MathPhys**
June 2, 2012

Jeff,

Which of John Roe's books were you refering to? The CBMS write-up? or the longer version? Which of these two in more readable?

7. **Jeff M**
June 2, 2012

MathPhys,

It's "Elliptic Operators, Topology, and Asymptotic Methods" from the Pitman Research Notes, 1988, No. 179. My orals predate his CBMS lectures 🙂 It's basically his lecture notes from a course he gave at Oxford.

8. **paddy**
June 4, 2012

Though I am also mystified about the connection between mathematics and physics:
(a)It is arguably a trusism that mathematics provides a framework for physics;
(b)it is certainly arguable that this framework has always seem to come after the fact;
(c)what delimits and leads physics forward is observation and experiment;
and (d) "shut up and calculate".

9. **Anonyrat**
June 7, 2012

Off-topic: Feynman's FBI files
http://www.muckrock.com/news/archives/2012/jun/06/feynman-files-professors-invitation-past-iron-curt/

10. **SA**
June 7, 2012

This is off-topic but I just thought it was pretty funny: http://arxiv.org/pdf/hep-th/0503249.pdf

11. **Peter Woit**
June 7, 2012

SA,

What's funny is that many theorists don't seem to think this is a joke, see

http://inspirehep.net/search?ln=en&p=refersto%3Arecid%3A679450

More comments maybe in an imminent posting…

12. **Florin**
June 7, 2012

Hello Mr. Woit,

Just wanted to say thank you for all the resources you posted in your comment, much appreciated. Also Ossicle, thanks for the lectures, watching the first videos really rendered me the scale of the program.

Not sure if I'll ever make it to approach and actually understand all the topics involved here, but I plan to go on with my calculus manual so that I can access general relativity and quantum field theory and in parallel maybe start learning some group theory, Lie algebra… Too bad I started this late..

13. **SA**
June 8, 2012

Peter,

Indeed that would be funny (and sad). However, if you take a look at those citing papers, you will see that most are citing tongue-in-cheek and the 1 or 2 other papers are citing it obviously based on a superficial keyword search.

High Drama

June 7, 2012
Categories: Experimental HEP News

It's exactly a month until new LHC Higgs results are to be unveiled at ICHEP. The machine has been running well, and right about now should be the cut-off time after which new data will arrive too late for analysis before ICHEP. So the integrated luminosity available for each experiment to analyze will be about 4.5 inverse femtobarns. See Tommaso Dorigo for an explanation of what this means. Very roughly, if the Higgs is there with a mass around 125 GeV, each experiment should see tentative signal similar to last year's, with the combination of data from both years and both experiments likely reaching the 5 sigma standard necessary to declare discovery. If no signal similar to last year's is seen, this will seriously re-open the possibility of no Higgs (or a very different Higgs than the SM one). Either way, should be very exciting.

For the other story people are following, the death of SUSY, see Nima Arkani-Hamed's recent talk. He lists as "High Drama for 2012" not just the Higgs, but also SUSY results on stops, gluinos and multileptons. Here I think most people have given up hope that evidence for SUSY will be found, so the high drama is the human one of how SUSY advocates will react as remaining possible SUSY hiding places are mopped up. The conference was in honor of Savas Dimopoulos, who has been working on SUSY models for more than 30 years, so Arkani-Hamed's talk didn't even acknowledge the possibility of no SUSY. He described the 2 alternatives available to physics as "natural SUSY", which is just about ruled out, and various versions of "split supersymmetry", where superpartner masses can be pushed arbitrarily high, out of the reach of the LHC or any conceivable experiment. He didn't mention the version one commenter here recently brought up, "super-split supersymmetry", which was an April Fool's joke, but may be the direction the field is headed.

Tommaso notes that the new data has been blinded by the experiments, meaning that even those working with it don't know anything about what the final result will be until the last stage of the analysis. Whenever that might be, I'm hoping reliable rumors will soon ensue.

I'm heading out on vacation tomorrow, to the bottom of the Grand Canyon, out of reach of the internet until about June 18th. During this time I'll have to shut off comments here. So, hold your reliable rumors until I get back….

Update: Seems that some of these reliable rumors just won't wait. Whatever Tommaso says about CMS blinding its data, either that's not the case at ATLAS, or some of it is now unblinded. I hear that the first chunk of gamma-gamma data is showing some signs (2 sigma) of a signal at about the same place as last year's data. Analysis of more data is proceeding, and very soon people at ATLAS will know whether there's a signal there. High drama….

Comments

1. **King Ray**
   June 7, 2012

   Peter, have a great and well-deserved vacation!

   One thing that occurred to me that others may find funny, in the nature of lolcat jokes:

   ICHEP = "I Can Haz Extra Particles?"

2. **Shantanu**
   June 7, 2012

   Peter or anyone, any exciting news from CIPANP 2012?
   Unfortunately I don't see any slides yet from it.

3. **Peter Woit**
   June 7, 2012

   Shantanu,

   At least as far as LHC results go, the likelihood is that we've already seen whatever is really exciting about the 2011 data, and the earliest results from 2012 data are targeted for ICHEP. So, I'd be surprised if there's any big news from the LHC at any conference before ICHEP next month.

4. **Hamish Johnston**
   June 8, 2012

   Is it realistic to expect that the "discovery announcement" will be made half-way around the world in Oz. Surely CERN will want a tightly-orchestrated press event in Geneva?

   H

Too Much Ain't Enough Langlands

June 7, 2012
Categories: Langlands

I should be packing for my trip, but couldn't resist one last blog posting, since I've recently a run across a lot of interesting Langlands-related material, including:

- A Symposium this fall at the Fields Institute, in honor of Ngo's Fields Medal winning work, on Fundamentals of the Langlands Program. They have a symposium blog, and including a video with Jim Arthur who gives a little bit of historical background to the Langlands Program.
- This past semester the Fields Institute has had a program on Galois Represesentations, with an instructional workshop, lecture series by Michael Harris and Christophe Breuil, and lots more. Some notes are on the instructional workshop page, and lots of audio of the talks are available here (so you can see what trying to learn math will be like when you go blind).
- To hear from the man himself, there's something old here (notes here), something from last year here.
- In recent years Matt Emerton has written some wonderful expository pieces, often on Langlands-related topics, as the answers to questions on MathOverflow. He has collected links to them here.

No Comments

The Higgs Discovery

June 17, 2012
Categories: Experimental HEP News, Favorite Old Posts

Just got out of 8 days in the Grand Canyon which was spectacular,

Reliable rumors couldn't wait, and they indicate that the experiments are seeing much the same thing as last year in this year's new data: strong hints of a Higgs around 125 GeV. The main channel investigated is the gamma-gamma channel where they are each seeing about a 4 sigma signal.

More later when I reach civilization.

Update: Back in civilization, or at least New York City. The above was the first posting I've ever written on an iphone, late at night. Now I have a real keyboard, so I can write a bit more. The "4 sigma signal" refers to the combined 2011 and new 2012 data. To oversimplify the situation, last year both experiments were seeing roughly a 3 sigma excess in gamma-gamma around 125 GeV. This was enough to convince many people that it was highly likely that this was the Higgs. However, that size excess is not completely convincing, it is not unheard of for there to be statistical flukes of such size.

The 2012 data that is being analyzed for ICHEP is of a similar size to the 2011 data. If 2011 was a fluke, you expect to see nothing much around 125 GeV in the 2012 data. If the 2011 signal really was the Higgs you expect the signal to strengthen. What I'm hearing from both experiments is that they are seeing an excess in the new data, strengthening the significance of the signal.

Exactly how much data they'll have analyzed by ICHEP and exactly what the significance of the signal in the gamma-gamma channel will be (as well as what other channels will show) is still to be seen. CERN will soon have to decide how to spin this: will they announce discovery of the Higgs, or will they wait for some overwhelmingly convincing standard to be met, such as 5 sigma in at least one channel of one experiment? The bottom line though is now clear: there's something there which looks like a Higgs is supposed to look. Attention will soon move to seeing if this signal is exactly what the SM predicts (e.g. will the excesses in different channels agree with SM predictions?).

More details about this from Philip Gibbs (who is speculating about what will be announced), and from Tommaso Dorigo (who is keeping quiet about what he knows, but providing context for what the ICHEP announcements will mean).

Update: Matt Strassler has more about this here. He provides about 20 links to his own blog, no link to the source of his information (this posting). It appears that this is because I'm a "non-particle-physicist blogger" engaged in a conspiratorial plot with some of the 6000+ people who know this latest news to "subvert the scientific process" by sharing it with others.

Update: There are stories about this at Wired, New Scientist and the New York Times. The New York Times article emphasizes that the Higgs results are now "Shrouded in Secrecy", with the spokeswoman for ATLAS pleading "Please do not believe the blogs".

According to Matt Strassler "the experimentalists can't possibly have their data in presentable form yet, so the rumors can't be correct in every detail". To clarify any confusion

"Exactly how much data they'll have analyzed by ICHEP and exactly what the significance of the signal in the gamma-gamma channel will be (as well as what other channels will show) is still to be seen"

means that the above rumors were based on just part of the data (significantly less than half in the ATLAS case, somewhat more than half in the CMS case).

Update: I think I'm too old to ever really understand Twitter, but it seems that #HiggsRumors is a "Trending Topic", whatever that means. More explanation available from Jennifer Ouellette, and sensible commentary from Chad Orzel.

Comments

1. **N.**
   June 17, 2012

   Good for Higgs, finally.

   Not so good for SUSY, as I understand.

   Congrats on the Grand Vacations!

   :)n.

2. **Sven**
   June 17, 2012

   Hi N,

   125 < 135, so a 125 GeV Higgs is perfectly compatible with the MSSM (not talking about more extended models).

   Cheers, Sven

3. **Gennaro**
   June 17, 2012

   Four sigma? Is that higher evidence than in 2011? Or less evidence? Please let us know…

4. **Chris**
   June 17, 2012

   4 sigma is significantly higher than last year. Still not enough to really claim discovery (5 sigma is the standard), but if they really are at 4 each then the combination would almost certainly do it.

5. **Susan Mones**
   June 17, 2012

Any plot supporting those rummors ?

6. **Truth**
June 17, 2012

The Higgs was discovered in the 70's when the standard model was put together; this is just the latest confirmation of the standard model. It once again disputes all the naysayers, who think the heirarchy problem is fake. No. The higgs is real. The standard model is real. Its properties, such as the heirarchy problem, are real. Nature has spoken, we have listened.

7. **OMF**
June 17, 2012

> No. The higgs is real. The standard model is real. Its properties, such as the heirarchy problem, are real. Nature has spoken, we have listened.

Be careful what you wish for! A 100% confirmation of the standard model by the LHC would in fact be the worst possible outcome for theoretical particle physics as a whole. Everyone would basically have to pack up their things and go home(and weep presumably).

Better to hope for a multichromatic Higgs or the like. Fingers crossed!

8. **Henry Bolden**
June 17, 2012

Where's the picture of Peter standing in front of the Grand Canyon?

9. **Brian**
June 17, 2012

@ Truth that is completely laughable viewpoint. You can put together a 1000 wrong theories that make predictions. Just because you have a theory that predicts something not yet been seen does not mean you have discovered something. The Higgs was predicted in the 70s and that is all. Theory only supplies a framework to predict outcomes in reality. Reality can turn a theory to ashes or lend it creditability, but that is about it.

10. **David Nataf**
June 17, 2012

Brian,

The same standard model that predicted the Higgs predicted many other properties and particles that have been borne out by experiment.

11. **anon**
June 17, 2012

Peter, when will you reach to civilization?

12. **Roger**
June 17, 2012

I agree with Truth. The Higgs mechanism is an essential part of the Standard Model and has been quantitatively confirmed since the 1970s. The value of the mass was unknown, and there is always the possibility of some other explanation for the data, but the Higgs was discovered in the 1970s.

13. **Noah Smith**
June 17, 2012

The Higgs was discovered in the 70's when the standard model was put together

the Higgs was discovered in the 1970s

Scientific method FAIL

14. **Dan D.**
June 17, 2012

Sorry, but I have to agree with Brian and Noah. The idea that the Higgs boson was *discovered* in the 1970s when the *theory* was fleshed out is ridiculous. It's enough of a triumph of the scientific method that a theory *predicted* the existence of such a particle 50+ years before it was finally (potentially) discovered, then to go overboard and claim a *prediction* is the same thing as a discovery. One may have high confidence in a prediction, but no matter which way you slice it, that's no substitute for experimental discovery. Lots of people in cosmology had high confidence that the Universe was slowing in its expansion, too, and we know how that turned out…

15. **Beelzebud**
June 17, 2012

I'm no scientist, but wouldn't it be correct to say that the Higgs was theorized in the 70's and is still awaiting discovery? Does experimental validation matter at all to some of you?

I think this is the sort of mindset Not Even Wrong and The Trouble with Physics was calling out. For something to be discovered, you must confirm it with experiments.

16. **Eric**
June 17, 2012

I am amused about these comments that infer the 'strength' of the standard model. If the model were so strong, it would have needed dozens of refinements over the years.

And please pardon my skepticism, but an sd of 4 sigmas is hardly a solid confirmation. There are 5 sigma+ level events that occur on earth regularly by random circumstance.

Why is the higgs pegged at 124 GeV in one very recent study, and 126 in another? If the resolution of the detectors is < 2 GeV, which I believe it is, then how can two ~3-sigma level findings 2 GeV apart mean the existence of a 125 GeV particle? The error exists on both the low-side of 124, and the high-side of 126. And the probability

DROPS OFF in between. If anything, attention needs to be paid to possibilities including: 2 particles 124/126, 3 particles 124/125/126, or more likely, zero particles anywhere near 125 because this is due to fluctuation.

17. **Eric**
June 17, 2012

The model "WOUDN'T" have needed dozens of refinements over the years, is what I meant to say.

And furthermore, I don't see much evidence that people are thinking about what other possibilities could exist at 125 GeV OTHER than a higgs particle. The existence is completely inferred by the detection of indirect mechanisms in the first place, and the inference is fairly weak when you consider there may be multiple explanations for the behavior in the detectors including a completely new physical mechanism.

18. **Anonyrat**
June 17, 2012

Civilization no longer exists. Peter may have a ETA for the urban jungle.

19. **tommaso dorigo**
June 17, 2012

To Eric above (on 124 vs 126):

I think your skepticism, although healthy, is a bit misguided.

– planes crash on almost a daily basis so 5-sigma events do occur, true – but we do not look for a new particle a million times a day. The 5-sigma bar was set in HEP by common wisdom, and it has been surpassed very rarely by things that later boiled down to be false. The 5-sigma criterion is imperfect of course, but it is a good hunch. Note that it incorporates the famous "look-elsewhere effect", in the sense that experiments are allowed to claim observation of a new particle based on local significance, i.e. not accounting for the trials factor.

– the issue of 124 vs 126 has been put forth many times by people with handwaving arguments. If you know a bit of Statistics it won't take you long to realize that they are fully compatible, and that the weighted average of two signals can then be taken. The exercise of 124+-2 oplus 126+-2 gives the maximum likelihood estimate of 125+-1.4; but you need to stop there since for the significance things are not as straightforward, so you should rather look at Peter Gibbs combinations (vixra log), which show that the global effect is of the order of 4-sigma for the Winter 2012 data (which had 3-sigmaish effects in CMS and ATLAS). The trick is that while the most sensitive channels have resolutions in the 1-2 GeV ballpark, the combined results of each experiment benefits from channels with lower resolution, so that the p-values have broad distributions.

Cheers,
T.

20. **anonymous**
June 18, 2012

Is there a vacuum instability issue with an SM Higgs at 125 GeV?

21. **Mitchell Porter**
June 18, 2012

Eric: the standard model "needed dozens of refinements over the years"

Could you list just one dozen refinements that it needed?

22. **ScientistfromMars**
June 18, 2012

Calm down, guys. The Higgs Boson had been discovered long time ago. Why all the noise now? I know CERN has to come up with results after eating up billions of taxpayer funds. But for what: I have one Higgs Boson in my freezer and another one stored in a vault. Both are really pretty, but a bit small tough. Now, I need to walk my dogs.

23. **Eric**
June 18, 2012

Hi Mitchell — Isn't every run of every experiment to find the Higgs a 'refinement'? The model doesn't predict the mass. I don't know how else you can look at it.

Hi Tommaso — Thank you for your insight. But can you further clarify what you mean by '[physicists] don't look for the Higgs particle a million times a day'? As above, isn't every run of every experiment a trial? And is it wrong to break it down further by counting every single quanta of energy hitting the detectors as a separate 'trial', such that you really are looking for it "a million" times a day?

Second, please forgive me for referencing a web page instead of working through the math myself, but I like 'handwaving' 🙂 :). Please read what Sean Carroll has to say about probability in terms of football coin flips, of all things:

http://blogs.discovermagazine.com/cosmicvariance/2012/02/04/a-3-8-sigma-anomaly

Finally, could you please offer some advice on what the true resolution(s) of the detectors are? I would like to put together a thought experiment, if I can find the time, to show that maybe there's a different way to look at these results. The fact that you mentioned the experiment relies on data from detectors with even lower resolutions than 2GeV is both interesting and worrisome to me. I do know a fair bit amount statistics (although not as much as you 'particle' guys. You guys know your stats like nobody's business!). Anyway, have you thought about applying the error in the detectors to model an explanation to describe a new/different physical mechanism at play here? Just curious.

– Eric

24. **Eric**
June 18, 2012

Tommaso et. al. Please don't take my skepticism in the wrong way. I am not trying to be rude, for sure. I'm just very interested in the experiments, and I think you guys are indeed doing a tremendous job with tremendously complicated equipment (which you built to perfection from scratch), faced with looking for a needle in a trillion haystacks.

25. **Truth**
June 18, 2012

Dear anonymous,

Thank you for asking a relevant and important question, while most others here seem to be confused about the nature of science and evidence.

To answer your question about vacuum instability: as is the case for EVERY value of the higgs mass, something bad happens at suficiently high energies, whether it is vacuum stability for light higgs, or a landau pole for heavy higgs. For a 125 GeV higgs, there is vacuum instability at an energy around 10^12 GeV. There is also an associated timescale for our vacuum to decay, but this turns out to be longer then the present age of the universe. So the standard model with 125 GeV higgs is META-stable. This is probably a bad thing that is fixed by new physics entering before 10^12 GeV, although perhaps meta-stability is okay, we don't know.

So the standard model is yet again holding true. It's heirarchy problem is, in my opinion, its strangest feature, that most people think imdicate that the new physics will enter far low than 10^12 GeV. Or the heirarchy is anthropic. Those are the only possibilities.

26. **emile**
June 18, 2012

Truth: you wrote: "The Higgs was discovered in the 70′s when the standard model was put together; " . Then you add: "while most others here seem to be confused about the nature of science and evidence.".

You are the one who seems confused. The Higgs has not been experimentally discovered yet. Maybe you are the only one for whom a "discovery" of a particle does not involve experimental observation. Seeing neutral currents in the 70s is not a Higgs discovery. Maybe "discovery" means something different to you (and you alone, because I don't anyone in this field who would support your statement above).

Let me blunt: if a student in experimental HEP said something like this during a PhD examination, he/she would not walk away from my University with a PhD if I'm on the committee. This is so "Scientific Method 101" that a "fail" is the only grade one can give.

27. **Peter Woit**
June 18, 2012

All,

I'm back in the office, can now try and get a grip on moderating this discussion. Unless you have something interesting to contribute about the latest news here, please refrain.

Truth,

"Those are the only possibilities"

Actually, no. For the obvious reason that at the edge of scientific research, we don't understand what is going on well enough to know the "only possibilities", and in this particular case, because of the subtleties of the "hierarchy problem". But that subject is both off-topic and already beaten to death at another posting. No more here.

28. **tommaso dorigo**
June 18, 2012

My answer to Eric above:

"But can you further clarify what you mean by '[physicists] don't look for the Higgs particle a million times a day'? As above, isn't every run of every experiment a trial? And is it wrong to break it down further by counting every single quanta of energy hitting the detectors as a separate 'trial', such that you really are looking for it "a million" times a day? "

Not really. The analysis is done once per year or so. And if you broke the data down in bits, you would have no chance of saying anything in any given bit. It would be stupid, and meaningless.

About what Sean Carroll writes in his blog on probabilities: after three years working side by side with statistics experts who have published groundbreaking papers on statistics for HEP, I believe S.C. does not qualify as a source for me. And that's just another topic on which he doesn't, after his showing his bigotry in the issue of my comments to Lisa Randall's physical appearance 😉

[Sorry for my attitude in the paragraph above, I must be late in delivering my weekly dose of venom.]

About resolution: mass resolution depends on the objects you use to reconstruct the decaying object. Of course we use that information in all our analyses. I am not sure I understand your suggestions, but please consider that routinely we extract results from a likelihood $L(m;x,\theta)$ which is a product of probability density functions of all the observed events. The latter are written as $P(m;x,\theta)$ where m is the measured mass of an event, x is a vector of additional parameters measured in that event, and θ a vector of nuisance parameters (for which we usually have additional priors from subsidiary measurements $\Pi(\theta)$ which multiply L).

Now, in θ are contained systematic effects, but x is a measured vector of event characteristics, from which the mass resolution is deducible. So really P() contains that information; for instance if your measurement of m is gaussian, you could write P as $G(m,\sigma(x))$. I hope this is clear enough.

Cheers,
T.

29. **Zathras**
June 18, 2012

Does anyone know something about the structural correlation between last year's experiment and this year's? Or can we just add the sigmas because they are structural independent?

30. **Peter Woit**
June 18, 2012

Zathras,

As far as I know, for the most important channels for the Higgs search, the errors are dominated by statistics. So, to a reasonable accuracy you should be able to combine the 2011 and 2012 analyses as statistically independent.

This is a complicated business though, and I'm no expert, so surely there are subtleties about exactly how to do this that I'm missing. The experiments definitely are both doing combinations of the data from the two different runs (which were at different energies). I don't know the details of how they do this. Maybe someone else here does…

31. **N.**
June 18, 2012

@ Sven:

I meant Sir Peter H. , not the particle 🙂

As for SUSY, no signs so far (as I understand it)?

n.

32. **Gennaro**
June 18, 2012

Peter,

in Germany, your blog announcement about the Higgs made its to the internet news (spiegel.de) and into radio today (monday). You are now a celebrity …

33. **Anonyrat**
June 18, 2012

A relevant physics question, I think. Matt Strassler wrote at the link provided in the update above, regarding combining 2011 and 2012 results: "So in combining them you are making a theoretical assumption about how the production rate for Higgs particles changes as you change the energy from 7 TeV to 8 TeV. "

Actually, at 7 TeV or 5 TeV or 8 TeV, the constituents of the protons are what are colliding, and these constituents have some energy distribution with non-trivial spread, and the physicists are making a theoretical assumption that they know the production rate along this energy spectrum. So I'm not sure what great additional assumption we are making in combining 7 TeV and 8 TeV results.

34. **Peter Woit**
June 18, 2012

All,

I don't seem to have made this clear enough: pointless arguments about the scientific method, what's a discovery, etc. don't belong here. Stop submitting them.

35. **tommaso dorigo**
June 19, 2012

Hi anonyrat,

indeed, the PDF of the proton constituents have to be used in determining the production cross section of a hard subprocess, in what is called a "factorization integral": this is (pardon my laziness of not using latex), say for producing a H particle inclusively ("+X", where X is "whatever else")

σ(pp–>H+X) = Σ_(ij) Integral [f_i(x_i) f_j(x_j) σ_point(x_i x_j s, H) dx_i dx_j]

where x_i and x_j are the momentum fraction carried by the partons which produce the hard subprocess, and f_i, f_j are their PDF; s is the squared proton-proton CM energy. As you see in σ_point enter only the partons and their energy, so this part "factorizes" from the mess of the proton-proton collision.

The above does not mean that one has trouble combining data at different values of sqrt(s). Indeed, one has already taken assumptions about her capability to compute the pdf at a given scale, when using the factorization integral for a given s. Adding sigmas for different s does not cause a further headache, since it only means a minor "evolution" of the PDFs from one scale of hard process energy to a contiguous one. In other words, one is not sampling the PDFs in widely different ranges of momentum fraction in changing from 7 to 8 TeV.

There is one further point to make. LHC experiments searching for the Higgs are basing their background predictions on data (and so do not care about sqrt(s) at which data is taken) or, in specific cases, on electroweak calculations (say for instance the ZZ background in the H–>ZZ searches) which are very well known and computed to NNLO accuracy.

Finally, even within each experiment the methodology of combining the results should be that of extracting them separately from the two datasets (7 and 8 TeV), and then combining with the same tools LHC has used since last Summer to combine ATLAS and CMS data on the Higgs search. In a sense there is no difference in combining H->ZZ, H->γγ, H->WW, H->ττ results together and combining the same set twice, one for each sqrt(s) point.

Cheers,
T.

36. **Juan Ramón González Álvarez**
June 19, 2012

SLAC claims that BaBar Data Hint at Cracks in the Standard Model. The excess decays has to be still confirmed, but they claim that data already rules out the Two Higgs Doublet Model.

https://news.slac.stanford.edu/press-release/babar-data-hint-cracks-standard-model

37. **jon**
June 19, 2012

About the last part of Peter's post ("will the excesses in different channels agree with SM predictions?") is there enough data already (5/fb of 2011 + 5/fb of 2012) to, in any case, answer it (i.e a summer annoucement possible) ? Or is the full 2012 run needed to beat any statistical bad luck (i.e. a spring 2013 announcement) ? Thanks!

38. **Peter Woit**
June 19, 2012

jon,

This depends of course on whether reality disagrees with the SM predictions, and by how much. The uncertainty in the size of the signal will be large at ICHEP, smaller by early 2013. If the SM prediction is dramatically wrong, there may be evidence of this at ICHEP, although likely not statistically convincing.

The size of the signal depends on the Higgs production cross-section x branching ratio. The SM branching ratio should be accurately known, the production cross-section much less so (I've seen estimates of theoretical accuracy of 10-20 percent for this). Looking at the ratio of signals in two different channels (e.g. gamma-gamma and ZZ goes to 4 leptons) gets rid of the production cross-section uncertainty, but adds a lot of statistical uncertainty as you take the ratio.

39. **Yuri Gershtein**
June 19, 2012

Dear Peter,

I can not comment on the rumors themselves, but I can comment on the "Update" that you posted. Indeed, leaking the information (or making up stories) from a 3000+ experiment is a problem for a scientific process. We do not hide answers from you because we are such teases. We just want to figure out what the data tells us first, and it is only people working on experiment who are qualified to making the pronouncement.

Since your postings are followed by people who may not appreciate the difference: the official word from the experiment is the reality. Everything else is just hot air and narcissism…

regards,
Yuri

40. **Peter Woit**
June 19, 2012

Yuri,

I understand that it's in the interest of the experiments not to have public announcements coming from them until the job of getting the most reliable possible result is finished.

At the same time, I just don't see the supposed problem posed by an accurate characterization of preliminary results appearing on this blog. Yes, it's in principle possible that some reader here may not understand that these results are preliminary, but there's a huge amount of misunderstanding of particle physics by the public, with very little of it caused by accurate information being posted here.

Let me repost here a comment I wrote on Michael Schmitt's blog, which is relevant:

" The bottom line here is that over the last couple weeks, 6000+ particle physicists, the majority of the particle physics community worldwide, have seen or heard about preliminary analyses of data from their experiments which pretty conclusively confirm the existence of the Higgs. This is huge, historical news, and I don't happen to see why it shouldn't be shared at this point with the rest of the particle physics community. Notice that I'm not posting plots, or much in the way of detail. We all look forward to the day not too far from now when the best results possible from the new data are released, and we can all give heartfelt thanks to those like you who worked hard to get there.

Blogs introduce a new vector for the spread of rumors, but surely you're aware that historically the news of a big result has circulated fairly widely among physicists in the days and weeks before an announcement, often in highly inaccurate form. The current official policy that no one on an HEP experiment should breath a word about results before the public announcement doesn't correspond to the historical reality of the field. For instance, I remember going to tea at Princeton one day as a grad student back in the 80s, where I joined a group listening to Carlo Rubbia explain to everyone, with details, that his group had the top quark "in the bag". That's obviously not much of a good model for how to do things either, but the model of "no one will say anything at all to anyone" is neither realistic nor grounded in past practice."

41. **Yuri Gershtein**
June 19, 2012

Peter,

while I love the Rubbia comment, Rubbia was may be the only person in existence to be able to present wrong results in Nobel lecture and keep his credibility. I don't think we have people like that at the LHC. It was also a much different time, when our funding came from the echoes of the Cold War.

Yes, "accurate characterization of preliminary results appearing on this blog" would not be a problem. This however is NOT what was posted on this blog, since there are no preliminary results out. And if you think for a moment that what you hear from the rumor mongers is THE TRUTH, just remember ATLAS'es 115 GeV higgs signal around Easter of last year.

Yuri

42. **LHC security kernel**
June 19, 2012

Attention Everyone!
Scientific process has been subverted!
All LHC data servers are now undergoing mandatory reformatting.
Access to network has been restricted.
Please report to your supervisors for further instructions.

43. **Anon**
June 19, 2012

I think the blogs carrying these rumors have given a chance for many of us to feel the same "pre-we-know-for-sure" excitement before an official announcement that those in the collaborations and their friends with whom they share unofficially in the natural course of doing good science may be feeling. As long as the rumors are clearly labelled as rumors what is the harm that is done to science? In fact sharing a rumor on the internet as opposed to by word of mouth (that anyway happens) has the advantage that we know someone with a name and a face has originated it (on the internet)…..unless of course someone like Prof Matt doesn't reference the internet source in which case s/he becomes an independent source of rumors.

44. **Peter Woit**
June 19, 2012

Some more details about the Rubbia story, since I just did a little research, and many people may not know this history.

On July 4, 1984 CERN announced the discovery of the top quark by the UA1 experiment led by Rubbia, see here

http://cdsweb.cern.ch/record/714187/files/presscut-1984-011.pdf

The Princeton tea I remember must have been a couple months earlier, spring 1984, since I left Princeton summer 1984 and teas would have ended with the end of the spring semester. Rubbia wasn't just talking to physicists about this, see this May 9 story in the Christian Science Monitor:

http://www.csmonitor.com/1984/0509/050935.html

Of course it turned out that Rubbia's 40 GeV top quark was a mistake, with the real thing at around 173 GeV, and actual discovery waiting until eleven years later at the Tevatron. So, I'm not claiming Rubbia's behavior in this case as a good model for what the leaders of ATLAS and CMS should be doing…

45. **Thomas Wheeler**
June 19, 2012

Please keep sharing. I love your blog.

46. **paddy**
June 19, 2012

I second Thomas Wheeler's comments: thank you P. Woit. I need not, I think, go into why "blogs" provide some element of transparency which otherwise these days is not there. I also thank T. Dorigo for his insights (and will deny he has ever revealed anything he shouldn't have).

47. **Richard**
June 20, 2012

This "subvert the scientific process" prissiness is just hilarious.

The only people who *really* care about the results already know, via their own professional rumour mills, pretty much what is going on.

The only other people in the world who care are people like me: amateur science enthusiasts, washed-out graduate students, and technically inclined bystanders of the type any academic professional who cared about the political realities of funding his/her field would go out of their way to engage with; engage even to the awful, awful, awful, unprecedented level of letting slip bits of excitement and scandalous, terrible, non-peer-revived rumours.

Faster than light neutrinos were *really good* for physics. Higgs rumours are good for particle physicist's employment and for their grad students' prospects and for the public funding of incomprehensibly complex endeavours that no lay member of the public will ever comprehend except at the human drama level. Anything that gets anybody outside the experiments to give a damn about the way the universe works — and how science *really* works, warts and all — is good for humanity. These are massive, expensive, huge, public-funded collaborations, and human members of the public are interested in more than what a consortium Press Office approves for staged revelation.

As far as I know there's no neck-and-neck race on against the North Korean Muon Collider for publishing precedence and valuable electro-weak patent rights.

Pursed-lip tut-tutting about proper processes and the sanctity of Phys Rev B does nothing for anybody.

Save the secrecy and the dramatic revelations for blinded data analyses, where it actually does have any effect upon The March of Science.

48. **Anonyrat**
June 20, 2012

The New York Times: New Data on Elusive Particle Shrouded in Secrecy
http://www.nytimes.com/2012/06/20/science/new-data-on-higgs-boson-is-shrouded-in-secrecy-at-cern.html

Quote: Nobody who has seen the new data is talking, except to say not to believe the blogs, where a rumor of an enhanced signal has ricocheted around, and to warn that even if the signal is real, it may require much more data and analysis to establish that it actually acts like the Higgs boson and not an impostor.

"Please do not believe the blogs," Fabiola Gianotti, the spokeswoman for the team known as Atlas, after its huge detector, pleaded in an e-mail.

End quote.

49. **David Roberts**
June 20, 2012

Your New Scientist link is pointing to Wired's story.

50. **Peter Woit**
June 20, 2012

Thanks David, fixed.

51. **David Folsom**
June 20, 2012

Some great points Richard. I only half agree that super luminal neutrinos were good for science though. There are quite a few with influence in communities, churches, educational positions and politics that latch onto these things and say "See, they don't know what they're even talking about." The angle that those reporting the anomalous results were at least on some level asking for help in finding the error in their experiment is lost on that crowd (or even before that, at the media editor level).

All those people vote and have impact on science funding. The half agree part of me reminds myself that, as they say, there is no such thing as bad publicity. It's a thin crowd here in the "technically inclined bystander" demographic, but in reading your post, I'm reminded that I'm not alone.
.
Peter, I suppose the irony of "subvert(ing) the scientific process" by pointing out the lack of scientific rigor in string theory, etc. isn't lost on you. I hope you can at least find some humor in it. You're an inspiration. Thank you.

52. **Peter Woit**
June 20, 2012

Thanks David!

I agree that we could have all done fine without the OPERA business. I tried to ignore it on this blog….

Don't worry, I find the many accusations I've gotten over the years about how I'm ruining science quite a bit more amusing than upsetting.

53. **Anonyrat**
June 20, 2012

"You are subverting science but I cannot tell you just how, that too is a secret" – Strassler is all stressed out. And he cannot let go of his absurd position. One would think the effect of a blog post is to change the measurement! If the result is so fragile, the public needs to know that it funded a boondoggle.

54. **Anonyrat**
June 20, 2012

Peter, this risks going off topic, but the effect of having the 2011 results available is a far greater potential source of bias than any number of rumors. CERN really needs data analysts that know nothing about 2011 results, if the 2012 results are so susceptible to contamination. I read Strassler as not liking you, and so making up some reason or other to bash you. And if Strassler is in any way right, then this is a huge scientific scandal, billions spent on what is a very fragile result.

55. **Peter Woit**
June 20, 2012

Anonyrat,

I don't see any reason to take seriously Strassler's claims about the Higgs data analysis being compromised by my blog posting. The idea that it is that fragile is just kind of absurd.

You can be sure though that the experiments are doing the best they can with this very hard to see signal. It's been a topic of intense study for decades, and the situation of barely seeing a signal in one run, and trying to confirm it in the next is not exactly an unusual experimental design situation. I've heard a lot more this year about blinding and "opening the box". It's possible they are doing some things differently this year precisely because of the 2011 data, but I don't know. Tommaso Dorigo has been on the CMS statistics committee, so surely knows a lot about this, maybe he'll blog about it (after the results are made public).

56. **anon.**
June 21, 2012

This exchange has been remarkable and revealing. It's just about convinced me to completely stop reading one blog, and I don't mean this one….

57. **David Brown**
June 21, 2012

I see the Woit versus Strassler debate as a dispute over institutional control of information — both sides have some good points.

58. **Christian**
June 21, 2012

The notion that posting rumours of results can in any possible way subvert the scientific process is just absurdly precious.

They'd have to be an apology for working scientists that're working on the data if a bit of hype about what's been seen so far has any kind of affect on the outstanding work.

Heaven forbid the general public are allowed to get a teensy bit excited about the work (they funded) before the final results are in, eh?

59. **David Nataf**
June 21, 2012

Remember the Eot-Wash experiment?

There were rumors, sometime around 2006, that they had found deviations from the inverse square law at short distances. That didn't stop them from doing a competent analysis, and in the end their results were vanilla.

In that case I think the rumors were good for science. I was an undergraduate at the time and I thought it was really cool that these physicists were exploring fundamental science by basically building a super-sophisticated pendulum, aka applying classical mechanics. It certainly picqued my interest.

60. **Douglas Natelson**
June 21, 2012

Peter, your link to Chad points to a post of his from 2010. You want this one: http://scienceblogs.com/principles/2012/06/21/higgs-rumors-are-the-price-of-success/

61. **Peter Woit**
June 21, 2012

Thanks Doug, fixed.

62. **sasqwatch**
June 21, 2012

Only discovered your blog moments ago (through Coyne's "WEIT website posting"), though I thoroughly enjoyed "Not Even Wrong" a few months back. I also recommend Lee Smolin's popular works to readers here, having just finished
"Three Roads to Quantum Gravity". Thanks for continuing to make this stuff accessible. It is much appreciated!

63. **Yatima**
June 21, 2012

Woah this Higgs Discovery Meme is about to die of overexposure any minute now. But soon there will be another Greek Default or Lindsay Lohan story and things will calm down again.

I can only agree wholeheartedly with the sentiment that "pursed-lip tut-tutting about proper processes and the sanctity of Phys Rev B does nothing for anybody". Indeed. Getting stuffy in the ivory tower and making mysterious noises will just cause people to think one is in hock with the Synarchic Knights of Templar Rebirth to generate antimatter to kill the pope.

I'm off to quaff some beer while reading about quantum computing.

64. **Former CDFer**
June 21, 2012

Once upon a time I was a physics professor doing experimental particle physics in a large collaboration. My basic take is: an experimenter who finds that these rumors hamper their ability to search correctly for the Higgs is an experimenter who needs to turn in their badge and gun.

65. **M**
June 22, 2012

Actually the most important rumor/news is about the central value of the gamma gamma rates: in agreement with 2011 data they are ***(self-censorship)*** than what predicted by the Standard Model Higgs, both in CMS and in ATLAS!

66. **Neil Bates**
June 22, 2012

For a humorous interlude, here's my offering of what Dirty Higgsy had to say about all this:

I know what you're thinking: "Did we find five sigma, or only four?" Well, to tell you the truth, in all this excitement, I've kinda lost track myself. But being this is the LHC, the most powerful collider in the world, and would blow your mind clean off, you've got to ask yourself one question: "Do I feel lucky?" Well do ya, punk?

67. **Peter Woit**
June 22, 2012

M,

What are the uncertainties in the Higgs production cross-section? Could they be a fraction like **self-censorship** -1?

68. **jpd**
June 22, 2012

its the quantum theory of blogging:
posting a blog about an upcoming event changes the outcome of the event.

69. **lib**
June 25, 2012

Thanks for the post.
If the Higgs is found, how long will experiments continue at CERN in the field of dark matter research?

70. **Peter Woit**
June 25, 2012

lib,

The Higgs question very little if anything to do with the dark matter question. Research into dark matter will not be affected at all by what is learned about the Higgs at the LHC.

Higgs Discovery Announcement July 4

June 22, 2012
Categories: Experimental HEP News

I learned via [Physics World] that CERN will hold a press conference on Wednesday July 4 to give an "Update on the search for the Higgs Boson". More information has just [appeared] (including a press release [here]), showing that there will be a 2 hour seminar on the results starting at 9am Geneva time, followed by a press conference at 11am.

Reports from the experiments indicate that at least one of them, if not both, will reach the 5 sigma level of significance for the Higgs signal, when they combine 2011 and 2012 data and the most sensitive channels. So, this will definitely be the long-awaited Higgs discovery announcement, and party-time for HEP physicists.

One could note that the last major announcement of the discovery of a new elementary particle at CERN was also made on a Wednesday, July 4, back in 1984. That one didn't work out so well, but things are very different now, with results from two independent experiments and a high standard of evidence.

Comments

1. **Stan**
   June 22, 2012

   What particle was announced on July 4, 1984?

2. **Peter Woit**
   June 22, 2012

   The top quark, with a mass of about 40 GeV. Only problem is that the top quark really has a mass of about 173 GeV….

3. **Alfons Hoogervorst**
   June 22, 2012

   Long-time follower of HEP/Not Even Wrong here: A bit ahead of the news, unless you know more, Peter! (Because the press "ad" really says "update on Higgs search".)

4. **Peter Woit**
   June 22, 2012

   Thanks Alfons,

   There are some blogs out there with a policy of only discussing news approved by the relevant authorities. Not this one….

5. **Christian Takacs**
   June 22, 2012

   Please don't clobber me with criticism if I sound uninformed about this… but… though I do see all these articles, charts and graphs indicating something (large particle?) may have been found, I do not see anything (outside of a desire to find the Higgs) to indicate this IS the Higgs particle, nor do I see any explanation of how this will be demonstrated if a particle IS discovered. I would also ask, Isn't the Higgs boson/particle's existence based on the Standard Model's assumption that mass is granted, imparted, or virtually assigned by said particle? I just seem to be seeing lots of "There are indications of something there, if confirmed, it's the Higgs particle" statements. I see no mention of "What if it's just a newly discovered particle which is not the Higgs". I would just have thought they would first want to confirm that something was found, THEN go about some method to find out if the particle passes some falsifiable testing procedure for confirming it works as advertised.

6. **Peter Woit**
   June 22, 2012

   Christian,

   The Standard Model makes extremely detailed predictions about exactly what Higgs decays should look like, and the LHC experiments are carefully tuned to look for exactly these predicted signals. What they are seeing is exactly what they were looking for (with the interesting caveat that the production rate may be higher than expected, but that calculation is hard).

   So, either this is the Higgs, or if it's something different, you have to explain why it is doing precisely what the Higgs was supposed to do. Anyway, the big effort from now on will be trying to more precisely measure the properties of this signal to compare to the SM prediction.

7. **Anonyrat**
   June 22, 2012

   Continuing to subvert science, I see 🙂

8. **Eastender**
   June 22, 2012

   So who gets the Nobel prize ………

9. **SilverSB**
   June 22, 2012

   What will happen 4th of Jully is just the HEP will become less interesting. LHC is built to catch a Higgs, everything else would be a bonus (if the Nature is kind enough to throw at us hints for supersymmetry or dark matter at home-made energies – something I really doubt). So, yeah, the higgs is there. We have just saw the final battle in the first 5 minutes of the movie. How anticlimatic…

10. **emile**
    June 23, 2012

SilverSB: your are not 5 min. into the movie. The movie has been going on for decades… If a Higgs is confirmed, then I don't blame you from thinking that this would be anticlimactic. But if I've learned anything, it's that Nature doesn't care what we think.

11. **crandles**
June 23, 2012

So is pay $7 now to get $10 back if/when a paper is published in 2012 claiming a 5 sigma discovery a good bet?

Or is there too much chance that paper will wait for more information from different decay channels, and others hints of consistency with being a Higgs Boson and then be subjected to much scrutiny in peer review so that paper may not be published until 2013?

Or is there too much risk that judge will read a paper saying there is 5 sigma discovery of particle that is consistent with Higgs Boson as not being sufficient to say particle is the Higgs Boson? (If such a paper isn't sufficient, will anything ever be sufficient? and is that sufficient to ensure judge will decide that such a paper is sufficient?)

(Rules say "Confirmation of the Higgs Boson particle having been observed must be published in a major scientific journal for this contract to be expired.

Clarification (Jan 5th 2009): for the Higgs Boson particle to be "observed" there must be a "five sigma discovery" of the particle.")

BTW, there isn't much liquidity on this bet at intrade.com: 37 * US $7 is only pay US $ 259 to get US $370 less US $5 per month in fees less and bank/other fees for money transfers. Also by the time someone new to intrade gets that money into their intrade account, the opportunity might be gone. So probably not worth effort and risk for someone new to intrade.

(Now why do I suspect that Christian Takacs is an intrade trader?)

12. **SilverSB**
June 23, 2012

emile,
The LHC movie was decades in making, but only two years in playing. The Higgs is coming too soon, but the things are what they are.
Sure, Nature doesn't care what we think and what we want – it's not something we learned. It's something most people should understand.

13. **christian M**
June 24, 2012

Hi there,
Nice and happy news.

Just one comment/question.
We find allmost everywhere the misleading information that the Higgs boson gives mass. That is untrue in my opinion.
The Higgs boson is the trace left by something which gives mass. But not the boson itself. The scalar field responsible for giving mass has four degrees of freedom, three of which give mass to the W and Z. The fourth degree does not do anything. That's the Higgs boson. It is the remnant of this process. It's just an excitation mode of the scalar field, but it is the latter which gives mass.
That does not mean that it is not important to find the Higgs. It's like finding a trace left in the sand by a dinosaur : it proves that the dinosaurs existed.

14. **Henry Bolden**
June 24, 2012

[A rival Higgs discovery claim.]

15. **DB**
June 24, 2012

The Higgs will be the first fundamental boson discovered whose spin is not equal to 1. And the mass of 125GeV makes the building of a muon collider to probe the properties of the Higgs in fine detail a no-brainer. It also raises serious questions over the need for the CLIC upgrades to the LHC.

16. **Henry Bolden**
June 25, 2012

I'm hearing from someone (who does not wish to be named) who heard from someone else at the Perimeter Institute (whose name was not revealed to me) that the announcement on July 4 involves a Higgs which is NOT a Standard Model Higgs. Anyone else hearing this?

17. **Speculative**
June 26, 2012

Henry,

I'd be wary of anyone who is saying right now that we know the Higgs they're seeing is beyond the Standard Model. It will take a lot of careful measurements before we know for sure. If there is something about this particle that distinguishes it significantly from the SM Higgs that would certainly be very interesting and it might not even be the Higgs. My attitude is that we'll just have to wait and see until July 4th when more official data is released.

18. **Anonyrat**
June 26, 2012

It is time for corporate sponsorship, e.g., just like the MetLife Stadium, or the Citi Bank Arena, we could have corporations have naming rights on particles, such as the Disney Strange, the Dow-Jones Up, the Balenciaga Top, the Huggies Higgs. The proceeds of the sponsorships would go to support impecunious Superstringers, who promise to bring a whole lot of new particles and hence sponsorship opportunities, to the table. At least it would give some motivation for research. Some names might be already taken, such as Selectron Technologies' Selectron. Political parties might jump into the fray, such as the G.O.P. Dilaton. Social groups might enter the bidding too, such as the LGBT Spartner. The possibilities are limitless, we could further distinguish particles in different superstring vacua. With 10^500 possibilities, there will be enough for sponsorship by all the conceivable corporations the visible universe will ever contain. Hell, we can give each corporation sponsorship of an entire universe, why just a measly elementary particle?

19. **Tony Smith**
June 26, 2012

If the thing at 125 GeV “is NOT a Standard Model Higgs”
then
can they distinguish it from a non-Higgs particle such as for example
a technipion like that proposed by Eichten, Lane, Martin, and Pilon
in arXiv 1206.0186 (in the context of the CDF Wjj bump) ?

If it is not so distinguishable (and therefore not clearly any kind of Higgs),
then what should they call it ?

Tony

20. **Peter Woit**
June 26, 2012

Henry,

The signal seen in 2011 was already larger than the SM prediction (with large errors). The rumor that this year’s gamma-gamma signal is of similar size indicates that when they announce discovery next week, the size of the signal seen will not only be more than 5 sigma away from null, but also larger than the SM prediction.

There will be signals though in multiple channels: gamma-gamma, as well as ZZ to 4 leptons. The size of the signal is the product of the Higgs cross section x branching ratio. Whatever is observed, undoubtedly there will be dozens of theory papers promoting models supposedly explaining it. I’d love to hear from a Higgs phenomenologist about how good the SM Higgs cross-section calculation is, and what to look for in terms of deviations of the the signal sizes from the nominal SM predictions.

Maybe someone can convince Matt Strassler to stop complaining about what the recent NY Times article said about what nothing being seen would mean (which is irrelevant), and write instead about this.

21. **paddy**
June 26, 2012

Peter,
Not unusually you have hit the nail on the head–the things I have been wondering–and 3 times I might add:
(1) there are apparently hints of SM discrepancies in branching ratios–when does this become signficant?;
(2) uninitiated hep-ph folks like I would like to know the SM calculational “error bars” for such [fully of course realizing that the experimental notion of an error bar does not really apply]; and
(3) [at the risk of an understandably deletable ad hominem comment] I for one cannot fathom what particular axe MS has chosen to grind (this time).

22. **Anonyrat**
June 27, 2012

http://arxiv.org/abs/1012.0530

23. **Anonyrat**
June 27, 2012

See table 6 on page 27 of the above.

24. **Seth Thatcher**
June 27, 2012

A Higgs boson walks into a bar….mass exodus.

25. **Martin**
July 3, 2012

Yay, we’ve got a particle! Let’s just hope, it won’t turn into a difraction pattern when we stop talking about it 🙂

Higgs Update

June 28, 2012
Categories: Experimental HEP News

The Higgs discovery announcement will be at 9am next Wednesday. This is close enough that I can't reasonably be accused of "subverting the scientific process" and ruining the LHC Higgs analyses by reporting the results here. Unfortunately, no source has provided me with these results yet, so that won't happen anyway, at least not right now. However, I have learned the following, which may be of interest:

- On Monday at 9am Fermilab will try and steal a little bit of the LHC's thunder by announcing some new evidence for the Higgs from the Tevatron data. This uses the channel of a Higgs produced with a W or Z, the Higgs then decaying to pairs of b-quarks. This is a channel where the Tevatron is sensitive to a Higgs signal, but the LHC isn't (at the higher LHC energies backgrounds are too large).
- ATLAS and CMS each collected about 6 inverse femtobarns of data before the technical stop on June 18th, and they are rushing to get as much of it analyzed as possible. They are concentrating on the two most sensitive channels: H->gamma+gamma and H->ZZ->4l and are likely to have over 5 inverse femtobarns of 2012 8 TeV data analyzed in these two channels to present at ICHEP.
- There may not be any 2012 Higgs data from other channels presented at ICHEP. ATLAS will have a H->WW->lvlv analysis, but likely not ready for public release.
- To get the statistical significance necessary to claim a Higgs discovery, the experiments will be producing a combination of their best analysis of the 2011 data in all channels and the 2012 data in the H->gamma+gamma and H->ZZ->4l channels.
- There will be no CERN combination of ATLAS and CMS results publicly released. This is not because such a thing is hard to do (and I believe it is actually being done, just not released), but because of political reasons. I don't much understand these, but this blog entry gives some of the kind of reasoning being used.
- With no CERN combination, attention will focus on Philip Gibbs at viXra log who in the past has produced reliable unofficial combinations of data, and is likely to do so again.
- With the discovery a done deal, the attention of physicists will focus on the question of whether the signal being seen is compatible with SM predictions, or whether this new particle has unexpected properties. Here the main two numbers to look for are the ATLAS + CMS signal size in each of the two most sensitive channels. To get these, you can do your own combination of the separate ATLAS and CMS numbers, or wait for Philip. The signal size is a product of the Higgs production cross-section and the branching ratio for the channel. I've seen estimates of the reliability of the SM prediction of the cross-section varying from 15% to 25% (see more here). The branching ratios are much more accurately known.
- Probably nothing new about SUSY at ICHEP. New SUSY analyses are being targeted for the SUSY2012 conference in August.

Update: Resonaances has more here, including the news that CMS will report 2012 data about the H->WW->lvlv channel (about the significance of this, see the June 29 posting at viXra log), and possibly others. Whether the 5 sigma significance level will be reached by a single experiment remains unclear…

Update: Finally confirmation from a reliable media outlet… The Daily Mail reports God particle is 'found'. One evidence for this is that supposedly "Five leading theoretical physicists have been invited to the event on Wednesday". This may mean Englert, Higgs, Guralnik, Kibble and Hagen, with Anderson getting dissed as usual.

Update: Tommaso Dorigo is providing background to the imminent Tevatron announcement here, and I assume will be discussing the actual results immediately upon release. The papers with the results will be released here this morning.

Update: The interesting bottom line from the Tevatron is that they see an excess in the bb channel that the LHC is not sensitive to, of size 2 +/- .7 times that predicted by the SM for a Higgs of mass 125 GeV. So, a marginally significant signal, of size consistent with the SM. The LHC should soon report the sizes of such signals in 3 other channels. In a couple of days we'll have excesses in four channels, of sizes enough to claim discovery of a Higgs (or something very much like it, depending on how consistent the signal sizes are with the SM).

Update: The Tevatron paper on the Higgs combination is here. Most important number is the fit for the signal size for H->bb, for a 125 GeV Higgs. It's 1.97 +.74/-.68 (where the SM prediction is 1).

Comments

1. **Michael**
   June 29, 2012

   Dear Peter,

   nice summary of current rumors, that might be in the right ballpark. The CERN seminar will be very revealing, for sure. Concerning the ATLAS+CMS attitude toward combining results, Aidan's blog post has it right. It is very important that ATLAS and CMS establish independent evidence for a new particle of phenomenon and only combine once everyone is convinced that the discovery is real. There is a subtle but crucial distinction in the minds of experimental physicists when they know that their competitors might or might not confirm their results, and when they assume that they will and one will simply combine the results. Aidan and most collider physicists want to retain that distinction – and they are absolutely right in my opinion. Personally, I have no objection to Philip's unofficial combinations. But I don't agree with you that the reasons why ATLAS+CMS (not "CERN" by the way) are political.

   regards,
   Michael

2. **Peter Woit**
   June 29, 2012

   Thanks Michael,

   I see the argument about wanting independent discovery evidence. Still though, if neither experiment were to quite reach the discovery threshold, but the combination was well above it, few would take seriously official claims that the thing hadn't yet really been found.

   As the question moves to whether the cross-sections for different channels agree with the SM, it's going to be the combined values that people will look at. One could argue that providing the best combination is something ATLAS+CMS should be doing and not leaving up to Philip. In this case though, I guess anyone can add two numbers and divide by two….

3. **David Nataf**
   June 29, 2012

   Michael,

You wrote:

“There is a subtle but crucial distinction in the minds of experimental physicists when they know that their competitors might or might not confirm their results, and when they assume that they will and one will simply combine the results.”

Would you care to elaborate?

4. **Michael**
June 29, 2012

Hi Peter,

yes, for sure someone not on CMS and ATLAS very legitimately wants to see the combination. And an official combination will eventually be made. But first we need to see to what degree ATLAS and CMS agree, and that won’t happen in a serious way until the public announcement. This is why the combination comes some time after ICHEP (I honestly don’t know when). So there is no conflict between Philip’s unofficial combination and the unwillingness of ATLAS & CMS to make an official combination until well after ICHEP.

Hi David,

well, it is a question of wanting to avoid staking one’s reputation on a result that is later shown to be wrong. Think of the CDF di-jet anomaly, of muon bundles at D0, or of the OPERA anomaly for that matter. If you know you are measuring something that really exists, then you want to do the best possible measurement. That is challenging and stressful, for sure. But claiming to find new physics is the ultimate experience and no one wants to make a false claim and damage his/her career. So the emotions, mentality, fears and hopes are rather different when measuring something known and when claiming evidence for something new.

regards,
Michael

5. **OMF**
June 29, 2012

Two bucks says that the mainstream media makes an absolute hames of reporting the results and mass public confusion and ultimately annoyance ensues by Friday.

It’s the uncertainty principle of science reporting: (Significance of Story) x (Accuracy of reporting) < (Average Twitter post)

6. **Peter Woit**
June 29, 2012

OMF,

Actually I think the mainstream media has done a pretty good job on the Higgs, and will continue to do so next week. Part of this depends on CERN and the message they put out. In the interesting possible scenario of CMS and ATLAS independently having 4+ sigma but neither having quite 5 sigma significance, I hope CERN doesn’t try and add caveats, but sticks to claiming discovery.

Many of the problems in the past with bad media coverage of physics have been the fault not of the media, but of the scientists themselves. In this case there won’t be lots of theorists running around making ridiculous claims about string theory, etc. All the media has to do here is figure out that they should ignore Gordy Kane.

7. **Owen Patterson**
June 29, 2012

Could the Large Hadron Collider Discover the Particle Underlying Both Mass and Cosmic Inflation?

If the LHC discovers the Higgs boson or other theoretical particles, their existence could help explain inflation, one of the universe’s great mysteries

Full article here:

http://www.scientificamerican.com/article.cfm?id=could-lhc-discover-higgs-boson

8. **Peter Woit**
June 29, 2012

Owen Patterson,

A good example of theorists making grandiose claims in the press guaranteed to cause confusion. If you’re a journalist reading this, no, the Higgs discovery will not tell us about inflation and the Big Bang. If you’re a commenter who wants to discuss cosmology, sorry, that’s off-topic.

9. **Syksy Räsänen**
June 29, 2012

Peter:

This is an instance where your scepticism is misplaced. The Higgs field with a non-minimal coupling to gravity is a viable candidate for the inflaton, and in this case measuring the Higgs mass does tell us about inflation. (I am not commenting on the SUSY model also mentioned in the article, just a Standard model Higgs will do.)

10. **Peter Woit**
June 29, 2012

Syksy,

As far as I can tell, the various versions of this proposal discussed in the SciAm article have all sorts of problems, and hardly anyone takes them seriously. Polluting a serious discussion of real physics with implausible speculation about some relationship to the big bang is something theorists insist on doing far too often. Getting a story like this in SciAm to coincide with the discovery announcement of the Higgs does nothing to encourage the public understanding of this science, quite the opposite.

11. **Syksy Räsänen**
June 29, 2012

Peter:

I think you're straying a bit from your field of expertise! The original Higgs inflation model is taken seriously by the cosmology community (Wilczek, among others, has worked on it). I wouldn't recommend the above popular article for learning about the model. It's true that many people feel that the model has problems with ultraviolet completion and stability as an effective field theory, but there are no conclusive arguments.

12. **Peter Woit**
June 29, 2012

Syksy,

My point is just that theorists would do well to take a vacation on July 4th from promoting their favorite speculative ideas for which there is no evidence (such as Higgs=inflaton). The public and press deserve a break from having theorists trying to confuse them about what is solid science and what isn't. A headline about "source of the Big Bang discovered at CERN" is not what the field needs next week.

13. **piscator**
June 29, 2012

Agree with Peter completely. There are a million half-baked models of inflation out there and they don't become sensible just because a Nobel laureate worked on them. Next week is about results not speculation.

14. **gluino**
June 29, 2012

"I believe [the LHC combination] is actually being done, just not released"

I sincerely hope not. The CMS collaboration was only shown the combination this week. Personally I'd be pretty unhappy if ATLAS gets to see our results before I do!

15. **kbot**
June 29, 2012

Hi Peter,

The combination will eventually be done. The issue is that it requires the two collaborations to talk to each other to do the combination. The results are so new (putting all the latest data together) it won't happen in time for ICHEP. Of course if the two collaborations shared data it could be done faster – but the point is that you want the analysis to remain independent until they are finalized. That is one of the whole points of having two experiments in the first place. If you share data and information before you are finished with the analysis it ruins the independent confirmation of two separate results…

16. **Peter Woit**
June 29, 2012

gluino and kbot,

I may be wrong, but I was under the impression that the capability of doing a CMS/ATLAS combination was in place, just needing a limited amount of information from each experiment, and if one was willing to cut some minor corners, able to produce such a combination rather quickly. Philip Gibbs can do his version in a few hours if not less….

Now, getting both experiments to sign off on the result, that I understand can be time-consuming.

17. **Brian**
June 30, 2012

the Higgs is showing a small cross section into WW, which is quite puzzling.

18. **Christian**
July 1, 2012

Peter: Such a combination is, in fact, trivial if and only if one is willing to 'cut corners' as you say. These corners includes half-baked combinations of systematical error, which may be OK for Gibbs (I enjoy his page quite a bit), but certainly not OK for the ATLAS and CMS collaborations.

Regarding the cosmology: As long as the Higgs-Inflation model cannot make eg. a solid prediction of m_H = 125 GeV, even Wilczek should take a day off and enjoy the show.

19. **David Roberts**
July 1, 2012

The Daily Mail is being cited as a 'reliable media outlet'?? You're lucky they didn't claim the Higgs boson causes and/or cures cancer…
(evidence of their capability in this ridiculous article about scientific publishing: http://www.dailymail.co.uk/money/news/article-2160753/)

20. **Mithras**
July 2, 2012

Peter,

I haven't followed all of this for nearly as long as you have, but I was wondering about the "Anderson getting disssed as usual". Could you explain?

Also, I've made it through most of your book at this point, and have also finished The Infinity Puzzle, which you recommended. Is there another book or two you would recommend for those who wish to read about various aspects of particle physics?

Thanks!

21. **Jim Martindale**
July 2, 2012

Mithras,

On the right side of this web page there is a list of categories of links. Under the category "Categories" there is the category "Book Reviews". They're great. There are 26 reviews so far. I've read 'Massive' and 'Shape of Inner Space'. Both are excellent, as is 'Not Even Wrong'. Lee Smolin's 'The Trouble with Physics' is also great.

22. **Dave**
July 2, 2012

Anderson first came up with an early version of what is now known as the Higgs mechanism. This was (I believe) a non-relativistic approach in the context of solid state physics.

To leave him out of the festivities may well be unfair. Then again, he has been a vocal critic of particle physics so perhaps it makes sense not to invite the party pooper to the party.

23. **Peter Woit**
July 2, 2012

Mithras,

About Anderson, see

http://www.math.columbia.edu/~woit/wordpress/?p=3282

For other books, an older favorite is Crease and Mann's "The Second Creation". As Jim Martindale points out, much better than consulting my memory is to get the full list of book reviews I've posted here the past few years, which includes all the recent books on this topic that I've read through and enjoyed.

24. **David Derbes**
July 2, 2012

Not that it matters, but I was a research student of Peter Higgs's 1975-79. I have heard him speak about the history of his work more than once, and spoken with him about it many times. He invariably refers to about six or seven people, most especially Y. Nambu, G. Jona-Lasinio, J. Goldstone, P. Anderson and the other five guys recognized by the Sakurai Prize. (OK, that's nine.) He's a very modest person. For the longest time (and certainly for the four years I was in Edinburgh) he referred to "the well-known anonymous scalar" rather than a particle with his name on it.

It might be worthwhile reading the relevant papers anew. As far as I understand them, the Guralnik-Hagen-Kibble paper talks about giving mass to a vector, but there is a disconnected zero mass boson still about. That's not what seems to be going on in the data. To be blunt, there are a lot of people who deserve credit for the Higgs mechanism: the Sakurai Six, Phil Anderson and all the rest. But very frankly, the Higgs boson really seems to me to belong squarely to Peter Higgs. He may be embarrassed to have something named after him, but he deserves it.

25. **Anonymous**
July 2, 2012

@David Derbes

Things may get very interesting after Wednesday. According to articles it looks like many of those names will be at CERN over the next couple days.

Few points….

Until the Physics World Interview below (from a few days back), I never heard Higgs mention GHK – seemingly demonstrating a good understanding of Nobel math. I have seen the "Life of a Boson" speech in video and text a few times (recently and well after Sakurai Prize).

http://physicsworld.com/cws/article/indepth/2012/jun/28/peter-higgs-in-the-spotlight

Guralnik compares the papers in the GHK history posted on arxiv…clearly his view.

The History of the Guralnik, Hagen and Kibble development of the Theory of Spontaneous Symmetry Breaking and Gauge Particles

http://arxiv.org/abs/0907.3466

Additionally, the notion of zero mass boson in the GHK paper was posted on this blog in the comments section in the below link. Guralnik explains boson and how this differed from the Higgs paper (also inserted text directly from the above link).

http://www.math.columbia.edu/~woit/wordpress/?p=4306

The Beginnings of Spontaneous Symmetry Breaking in Particle Physics — Derived From My on the Spot "Intellectual Battlefield Impressions"
Authors: G. S. Guralnik
(Submitted on 11 Oct 2011)

http://arxiv.org/abs/1110.2253

p. 9

…

Recently it has been claimed that the GHK paper does not have the "Higgs boson". This claim astonishes us. We, far more than any of the other groups, keep very careful track of the degrees of freedom of our scalar electrodynamics model. On the bottom of the right column of page 586 of the GHK paper are the three equations for the leading order approximations to the 4 physical degrees of freedom. We observe that the two degrees of freedom of the vector field combine with one scalar boson to form the three degrees of freedom of a massive spin one vector field. There is one remaining scalar field, 2 in our notation, which in our approximation has zero mass. That this mass is zero has absolutely nothing to do with any dynamical constraint including the Goldstone theorem. The Goldstone theorem, if valid here, would only constrain the mass of 1. The zero mass is an artifact of how we pick the explicit action and the leading order approximation. This is different from the Higgs paper in that he puts in an explicit pure scalar interaction. In a 4 dimensional renormalizable theory that interaction is limited to being pure quartic. As was our practice mirroring that commonly used by Schwinger and associates, we did not put in this explicit quartic term in scalar electrodynamics but were fully aware that such a term is generated in higher approximations. Ultimately because, of renormalization, the GHK choice of the action is operationally identical to the one used by Higgs.

In summary, our purpose was to show that the Goldstone theorem did not constrain physical mass in scalar gauge theories. We demonstrated this generally and in a specific example. The mass of 2 happens to be zero in leading order, but as was obvious to us and every other experienced field theorist of that time, this would change order by order as the theory was iterated in a manner closely related to how it changes in unbroken scalar electromagnetism.

…

Hagen also spoke about this briefly in the Sakurai lectures.

http://www.youtube.com/view_play_list?p=BDA16F52CA3C9B1D

26. **David Derbes**
July 2, 2012

@Anonymous:

I have a friend who wrote a biography of Einstein (Walter Isaacson). A few years ago he was in Chicago because his daughter was looking at colleges, and my family and his family had dinner together. Aware of the "Nobel math" I asked Walter: Is the limit to 3 in the Nobel will? Said he: It is not. How did he know? "Because I've read the will." Historically and by tradition they never give the Physics prize to more than 3. But it ain't in the will, if Walter knew what he was talking about. I think the Sakurai folks did the right thing.

To be honest, I don't care personally if the Nobel folks wind up giving the prize to all of the living eight (R. Brout regrettably has died.) And I doubt Peter Higgs would care, either. He doesn't need the money (he doesn't care about money to tell the truth.)

As it happens 🙂 I have the first GHK paper in front of me. The Nambu-Goldstone potential is not present at all. I quote: (bottom of page 586, PRL 13 (20) 16 November 1964): "While one sees by inspection that there is a massless particle in the theory, it is easily seen that it is completely decoupled from the other (massive) excitations, and has nothing to do with the Goldstone theorem." It is also evident that their massive scalar has exactly the same mass as the vector. As far as I am aware, the to-be-announced scalar does not have the same mass as the W or Z.

I reiterate: Many people, including but not limited to Peter Higgs, came up, I believe independently, with the mechanism (by whatever name you want to give it.) I heard Peter use many names in connection with the work, and if I work at it I suspect I can find the relevant talks in which, prior to 1980, he gives GHK full credit for their work. That said, in my opinion, the scalar to be revealed in 36 hours or so belongs to Peter Higgs (if the data support this.)

27. **David Nataf**
July 2, 2012

Isn't Peter Higgs 98 years old? And he's getting on a plane? wow !

28. **Peter Woit**
July 2, 2012

David,

No, he's 83 and doing fine as far as I know.

29. **David Nataf**
July 2, 2012

"98 years-old" was said in reference to Peter Higgs in a talk I went to, by someone very informed, but now in hindsight I think what was meant is that they shouldn't wait the usual 15-20 years to award the Nobel prize, otherwise he'll be 98 years-old when they give it out.

30. **Anonymous**
July 2, 2012

David Derbes

You (and Walter) are smart and correct – it is not in the will (I knew that also). It is in the statutes.
>>>>>>>>>>>>>>>>>>>>>>>>
http://www.nobelprize.org/nobel_organizations/nobelfoundation/statutes.html

§ 4.
A prize amount may be equally divided between two works, each of which is considered to merit a prize. If a work that is being rewarded has been produced by two or three persons, the prize shall be awarded to them jointly. In no case may a prize amount be divided between more than three persons.
>>>>>>>>>>>>>>>>>>>>>>>>

I don't agree with you on the boson. It is clearly in GHK (but not stressed) and gains mass as it progresses in their model. They should have put in a sentence that stated and stressed "an essential feature of this theory is the boson" and it would have been more clear. 🙂

Will be an interesting Press Conference to watch with all those folks there…Englert can say boson was "obvious" and GHK can try to explain the massless vs. massive and degrees of freedom.

31. **David Derbes**
July 2, 2012

@ Anonymous:

Thanks for the clarification about the statutes, new information to me (and maybe also to Walter, who I will tell when I see him next.)

I clearly have no idea what the Nobel committee will decide. I am sorry that these statutes are in play (I would have liked to have seen N. Cabibbo get part of the Nambu award, for example, and Dicke/Peebles and Gamow part of the Penzias-Wilson award.) I think the Sakurai folks got it right, and would be happy to see all six (including the late Brout) split it. But it ain't up to us.

32. **Anonymous**
July 2, 2012

Pretty cool you know Walter Isaacson. I read his book 6 months back and thought it was great.

33. **Anonyrat**
July 3, 2012

Humorist Andy Borowitz has already had an interview with Higgs (the boson).
http://www.borowitzreport.com/2012/07/03/5-questions-for-the-higgs-boson-particle/

~~Proof~~ Evidence of "God Particle" Found

July 2, 2012
Categories: Experimental HEP News

Besides the Daily Mail, the AP is now reporting Proof of "God Particle" Found. They include the caveat:

> But after decades of work and billions of dollars spent, researchers at the European Organization for Nuclear Research, or CERN, aren't quite ready to say they've "discovered" the particle…
>
> Senior CERN scientists say that the two independent teams of physicists who plan to present their work at CERN's vast complex on the Swiss-French border on July 4 are about as close as you can get to a discovery without actually calling it one…
>
> Rob Roser, who leads the search for the Higgs boson at the Fermilab in Chicago, said: "Particle physicists have a very high standard for what it takes to be a discovery," and he thinks it is a hair's breadth away.

which suggests that neither CMS nor ATLAS have quite managed to reach the 5 sigma threshold, and CERN remains dedicated to not discussing the obvious result of combining the data.

The AP report also has:

> CERN spokesman James Gillies said Monday, however, that he would be "very cautious" about unofficial combinations of ATLAS and CMS data. "Combining the data from two experiments is a complex task, which is why it takes time, and why no combination will be presented on Wednesday," he told AP.

From everything I've heard, my impression is that the reason no official combination is being produced is not because it would be technically impossible to do so on a time-scale of days, but because the decision not to do such a combination for ICHEP was made for reasons described here. The problem with this is that it may lead to a lot of confusing explanations like this in the AP report, which muddles how particle physics experiments are done and the obscure issue of 5 sigma/experiment or in combination:

> experts familiar with the research at CERN's vast complex on the Swiss-French border say that the massive data they have obtained will essentially show the footprint of the key particle known as the Higgs boson — all but proving it exists — but doesn't allow them to say it has actually been glimpsed…
>
> Roser compared the results that scientists are preparing to announce Wednesday to finding the fossilized imprint of a dinosaur: "You see the footprints and the shadow of the object, but you don't actually see it."

Better for CERN to just announce discovery and break open the champagne…

Update: Weird. The AP seems to have changed their title from "Proof" to "Evidence". This may be the first time in history that a media headline about particle physics is incorrectly pessimistic ("Evidence" usually means a 3-sigma signal, which existed last December, "Proof" would be a better way to describe a 5+ sigma signal, if that's what the combined CMS/ATLAS data shows).

Update: Curiouser and curiouser. I'm hearing that per-experiment combinations are around 5 sigma or above. Very unclear why the AP report is indicating no completely conclusive discovery announcement. Maybe the CERN administration is playing a game with us, downplaying expectations…

Update: As pointed out in a comment, Matthew Chalmers at Nature has

> The ATLAS and CMS experiments are each seeing signals between 4.5 and 5 sigma, just a whisker away from a solid discovery claim.

Update: The Atlantic covers the best blogs you should be reading to follow the Higgs story. They miss Resonaances and a few others. About me, they have:

> If the Higgs boson was a dead celebrity, Woit would be your TMZ — first to the scene, first to break it, and have it be right.

Comments

1. **Dan D.**
   July 2, 2012

   I have a bad feeling that this is just going to frustrate and confuse the public even more. I understand the arguments for and against officially combining the data to claim a discovery, and I see the merits of both sides, but given all the media buzz, it's going to seem now like a real let-down to much of the public if they come out speaking of it as simply "strong" evidence. Perhaps a compromise wording such as "we found it beyond a reasonable doubt, but we are going to continue to refine our analysis" might be in order. Of course, I'm not a particle physicist, and all this might be moot anyway, depending on how accurate this and other stories are.

   Of course, all this might be moot

2. **DB**
   July 2, 2012

   After the faster-than-light shambles CERN is playing it safe, and rightly so. It's taken nearly fifty years to get from prediction to validation, and it would be poor champagne indeed that couldn't wait a few more months to be uncorked.

   Let the media stew. When mankind looks back at 2012 five hundred years hence, the discovery of the Higgs Boson is all they'll care about.

3. **Dan D.**
   July 2, 2012

   DB,

   True enough. By the way, I found this article from Discovery News that is more clearly written (IMO) than the AP story about this issue:

   http://news.discovery.com/space/higgs-boson-discovered-120702.html

4. **Trulo**

July 2, 2012

The argument that there must be at least two experiments so that one can act as check to the other is understandable. In this case, however, I think it may as well be turned around. They won't combine CMS and ATLAS data yet, because they want to have a winner (like UA1 with the W boson), and the loser relegated to the role of verification (like UA2). Whatever. If both experiments see a 4+ signal separately, as far as I'm concerned is a discovery. And a simultaneous one, so a technical draw.

5. **Henry Bolden**
July 2, 2012

Here's what the journal Nature has to say:

http://www.nature.com/news/physicists-find-new-particle-but-is-it-the-higgs-1.10932

6. **Pumpkin Seeds**
July 2, 2012

Putting a Higgs "discovery" at exactly 5 sigma is a little arbitrary, isn't it?

7. **Peter Woit**
July 2, 2012

Pumpkin Seeds,

Well, you have to pick some number, and any such choice will be kind of arbitrary. The 5 sigma convention comes from wanting to pick a number so high that you're sure it can't possibly be a mistake. What CERN may be doing here though is taking that standard as a starting point and insisting on something much stronger: the 5 sigma can't come from combinations of the various different channels and different experiments.

If a single experiment were to report a 4 sigma signal at a certain mass in a certain channel, one might reasonably say that this wasn't absolutely certain to be a new particle. If you see several signals this size in two completely distinct experiments, all at exactly the same mass, then it's absurd to claim that doubt remains.

8. **Alp**
July 2, 2012

Could someone explain in simple terms how we know that this particle is Higgs (the same question Nature seems to be asking)? Is it due to very particular decay characteristics? How long will it take to confirm that it behaves as Higgs predicted?

9. **Peter Woit**
July 2, 2012

Hi Alp,

The standard model gives very specific predictions for the behavior of the Higgs, which are fixed by the distinctive properties of the Higgs field (spontaneous breaking of the SU(2) gauge symmetry fixes how it interacts with gauge fields, giving mass to fermions fixes its interaction with them). These predictions have for a long long time shown that if the Higgs mass is 125 GeV, it could not be observed until the energies and luminosities of the LHC were available, and then it should be seen with certain specific signal sizes in certain specific decay channels.

What the LHC experiments are seeing is, after decades of no such signal in these and similar channels, signals appearing in just the right channels, with roughly the right signal size. If this isn't a Higgs, it's something very like it.

The big question now will be seeing if the signal sizes in the various channels agree with the SM predictions. Initially the measurements of these sizes will be crude, but they'll get better with more data. The Tevatron signal today was 2 +/- .7 times the prediction. On Wednesday we should see numbers for 3 different channels from the LHC. Rumors are that some of them are larger than expected, one (WW) is smaller. These signal sizes though are just getting big enough to be sure they are there, so we're a ways away from a precise measurement of their amplitude. If these numbers don't agree with the SM, then you've got something that acts a lot like a Higgs, but is doing something somewhat different than expected, which would be quite exciting to find.

10. **Belizean**
July 2, 2012

In medicine if treatment is shown a couple of trials work to an accuracy of 3 sigma (< 0.3% chance of it being a statistical fluke), its unquestionably and uncontroversially regarded to be the discovery of a cure.

A 4-sigma signal would have a chance of less than about 6 in 100 thousand of being a statistical fluke, while a 5-sigma signal would have a chance of less than 6 in 10 million.

Peter is right in describing the press' uncharacteristic pessimissm as "weird".

11. **Peter Woit**
July 2, 2012

Belizean,

The particle physicist's use of statistics here may be somewhat over-optimistic, neglecting things like the "look-elsewhere" effect. But that's why the insistence on 5 sigma, otherwise 3 sigma really would be enough to claim discovery. In any case though, the numbers I'm hearing from the experiments correspond to a very, very high degree of certainty that they're seeing something at 125 GeV.

What's especially weird is the change in the AP headline. It looks like someone got upset at the initial "proof" headline (OK, "proof" is only really in math, but 5 sigma is a pretty close analog in science). I'm curious who decided to change it to "evidence", which is kind of seriously wrong here, since "evidence" is the standard term used for a 3 sigma signal, and we're well beyond that.

12. **Anonyrat**
July 2, 2012

In the annals of physics, what is the highest sigma signal that turned out to be spurious?

13. **Peter Woit**
July 2, 2012

Anonyrat,

Probably the best known example in HEP is the "pentaquark", where there were claims of 4-5 sigma observations, and this is probably the sort of thing CERN is worried about. I'm no expert on that story, but it seems to me somewhat different. For one thing, I don't think there's ever been a reliable theory predicting pentaquarks or how they should behave. So, if you start looking for the things, there's a monster "look-elsewhere" effect since you can look for them in all sorts of types of collisions and all sorts of channels. The scale of effort going into the LHC experiments and care taken in the analyses is way beyond that of the much smaller scale experiments that claimed to see pentaquarks.

By the way, I looked for the 1984 Rubbia top quark discovery paper to see what he was claiming. It's in Physics Letters B vol 147, page 493 (1984). I gather by the time they submitted the paper in October, their evidence had weakened from the July 4 time of the CERN announcement and press release. There's a reference to "a clear signal" but a caveat that "more statistics are needed to confirm these conclusions and the true nature of the effect observed". No quantitative statistical significance is quoted. It's clear that a statistical measure would be kind of irrelevant anyway, since they have a very small number of events and the problem is clearly whether they've actually understood those events, not exactly how many they are. All in all, a very different kind of analysis than the modern ones at CMS and ATLAS.

14. **Henry Bolden**
July 3, 2012

The Empire (Fermilab) strikes back, according to the New York Times:

http://www.nytimes.com/2012/07/03/science/physicists-inch-closer-to-proof-that-higgs-boson-particle-exists.html

15. **piscator**
July 3, 2012

In terms of sigma, remember DAMA have a continuing claim to have 10-sigma evidence for dark matter detection which essentially no-one believes. And recently OPERA had 6-sigma-ish evidence for faster than light neutrinos.

However the thing to remember with counting sigma is that all these experiments are big complicated beasts with lots of people working on them and lots of systematics that may or may not be understood. So when evaluating number of sigma we need more than just statistics – there also needs to be a sanity check about whether there are hidden systematics that may not be understood, or a bug in the code that leads to rubbish being spewed out.

So with DAMA, there is a 10-sigma evidence for annual modulation in their event rates – and this isn't doubted. However sceptics will point out that lots of potential systematics (such as the temperature) also modulate annually. So the significance of the claim relates to the ability of the experiment to control all annual modulations – and the judgement on this has nothing to do with `statistical' significance.

With the Higgs, (one of) the main channels is Higgs to 2 photons. The signal here appears as a bump on an otherwise smooth and falling distribution. It is hard to see how systematics could reproduce such a bump – particularly across two experiments and also with other supporting decay channels – making the `number of sigma' significance credible.

16. **piscator**
July 3, 2012

For further clarification: the bump in Higgs to photon-photon should be sharp and narrow, as the Higgs has small width. Such a sharp spike on an otherwise smooth distribution is very hard to reproduce by a systematic.

17. **SeanM**
July 3, 2012

What are the implications of a 125 GeV higgs for Susy? Does the LHC have enough energy to potentially find its Susy partner?

18. **Sven**
July 3, 2012

Hi SeanM,

a Higgs below 135 Gev was a firm prediction of the MSSM. So a 125 GeV Higgs (with somehow SM-like behavior) fits perfectly. However, it s basically impossible to derive the masses of SUSY particles, in particular scalar tops, from it. Anything between about 200 GeV up to (in principle) multi-TeV or even higher remains possible.

19. **SeanM**
July 3, 2012

Thank you Sven.

I thought because the higgs is relatively light it has to have a massive partner.

20. **Peter Woit**
July 3, 2012

SeanM,

To supplement Sven's answer, the problem with SUSY is that you have to break it somehow, and you can get a huge range of possibilities depending on how you do this. So, even once you know the mass of the Higgs, there are no definite predictions about other particles related to it by SUSY.

The interesting thing we'll learn soon is, at least crudely, what the cross-section x branching ratios are for several channels of Higgs decay. If these differ from the SM values, that will be very interesting, and surely there will be attempts to explain this in terms of SUSY. Note that, as far as I know, there are no SUSY predictions one way or the other about this.

21. **SpearMarktheSecond**
July 3, 2012

The Split A1 was an effect over 5 sigma.

It was most likely the result of experimenter bias.

Although the look elsewhere effect will now be greatly reduced, because the interesting region was already defined prior to accumulation of the new data.

But now the danger once again experimenter bias. It will be interested to hear how the experiments dealt with that problem… whether they were blinded and whether or not they applied changes after unblinding.

Looks like the Tevatron `hint' is all in H>b bbar. Curious, because the LHC saw better `hints' in H>gamma gamma… must be the terrific LHC em calorimeters.

22. **tdb**
July 3, 2012

SpearMarktheSecond,

The difference in channels between the Tevatron and LHC experiments really comes down to the fact that the LHC is pp, while the Tevatron is ppbar. In a ppbar collider the dominant Higgs production is qqbar->W(Z)->H W(Z) where the W(Z) essentially radiates a Higgs. These events are easy to trigger on because of the high energy leptons or missing energy associated to the W(Z) decay. A light SM Higgs primarily decays to bbbar, so this an easy thing to look for. Events with one or two leptons, lots of missing energy, and two b-tagged jets.

At the LHC where you have pp collisions, the dominant Higgs production is gg->H, where you get the Higgs and nothing else. The Higgs will still primarily decay to bbbar, but there's also ALOT of other QCD processes that will give you two b-tagged jets, so it's hard to "pick out" these Higgs events from the background processes. A light Higgs rarely decays to gamma gamma, but despite the low branching ratio this channel still gives the best signal to background discrimination at the LHC (at least in early data).

23. **CB**
July 3, 2012

Leaked CERN video confirming a new particle:
"News: Cern Higgs boson announcement: we have observed a new particle"
http://www.telegraph.co.uk/science/large-hadron-collider/9373653/Cern-Higgs-boson-announcement-we-have-observed-a-new-particle.html

24. **Redly Spent**
July 3, 2012

I love the Atlantic article. Right on the button. Well done Alexander Abad-Santos.

25. **paddy**
July 3, 2012

I havn't stopped laughing since reading the Atlantic article. I have already renamed some of my links as….well you know if you have read the article.

26. **David Kahana**
July 3, 2012

@Anonyrat

The signal for the `sub-millisecond pulsar' (1989B) that was `discovered' in the remnant of SN1987A by Pennypacker et al., being a simple frequency measurement, had an enormously high statistical significance. It was between 11 and 37 sigma depending on when the observations were made: it was an absolutely beautiful peak, way above the noise.

I remember that hundreds of suspect theoretical papers were written after the fact explaining how such an object could possibly exist. All of the ex post facto theory was very dubious because rotational velocities at the surface of such a quickly rotating neutron star would need to be very significant fractions of the velocity of light, and it was almost possible to imagine a high density equation of state soft enough to allow the thing to exist, and also allow for the supernova explosion. But that didn't stop the theoretical astrophysicists!

http://www.cbat.eps.harvard.edu/iauc/04700/04735.html

"The frequency of the pulsar during the Jan. 18 observation was tracked by dividing the data into independent half-hour runs; the statistical significance during these runs ranged from 11 to 37 standard deviations. The frequency exhibits a sinusoidal modulation; the 15 frequency measurements were within 5 percent (rms) of a sine function with a central value of 1968.629 Hz (barycentric), amplitude 3 x 10E-3 Hz (peak-to-peak) and period of 8 hr."

All theory aside, the `signal' turned out to be due to a resonance in a faulty radio camera: the experiment had two cameras: camera A and camera B, and it turned out the pulsar was seen in camera A but not in camera B. Pennypacker had to publicly retract the observation, but I don't seem to recall many retractions having been made by the theorists who explained it!

27. **David Kahana**
July 3, 2012

But the Higgs observation isn't likely to be in the same ballpark: the signal can't be misproduced so easily at CMS and ATLAS.

28. **SpearMarktheSecond**
July 3, 2012

Nice answer, thanks, tdb. Why is Higgstrahllung is not yet discernible at the LHC?

29. **tdb**
July 3, 2012

@SpearMarktheSecond

I'm far from an expert on the Higgs analyses, but I believe associated production, or Higgstrahllung, is about ~100 times smaller production cross section than gluon fusion; but bbbar decay for a ~125 GeV Higgs is ~1000 times more likely than gamma gamma decay. Of course then you have very different experimental uncertainties (b-tag efficiency vs. photon ID efficiency, etc.). To be honest I'm not sure how much data is needed for a Higgstrahllung channel to become a viable analysis at the LHC, but it's certainly more than the ~10 fb^-1 being analyzed now.

CERN: We Have Observed a New Particle

July 3, 2012
Categories: Experimental HEP News

A commenter here reports that a CERN video announcing that "We have observed a new particle" was released early, and is available here. Note that the language used refers to "observation" NOT "discovery", indicating that CERN has decided on a version of the 5 sigma discovery criterion that has not yet been met. "Observation" generally means a lower standard of evidence such as 3 sigma. However, it appears that they are sensibly playing this down, with nothing in the video mentioning the word "discovery" or their decision not to use that word. Most physicists likely will however use "discovery" to describe these results, since the combined CMS/ATLAS results should be way beyond 5 sigma (look to Philip Gibbs tomorrow for exact numbers).

In the video, there is reference to "very very strong evidence" for a narrow peak in gamma-gamma (presumably above 4 sigma, close to 5 sigma in each experiment), as well as "also evidence" for ZZ (4 sigma?) and "less conclusive" evidence in other channels.

Immediate Update: The word here is that CERN is claiming that this is just one of multiple videos made to cover all eventualities. Maybe tomorrow's version will substitute "discovery" for "observation"…

Update: As commenter Tim points out, this is a CMS video, not a CERN one, so it just refers to CMS results. Evidently CMS is not claiming "discovery", but that doesn't mean ATLAS doesn't have slightly better results and will make a discovery claim. Also, it doesn't show what CERN will say about the joint results of the two experiments.

Comments

1. **Tim**
   July 3, 2012

   Do note that Joe only speaks for CMS in this video.

2. **abbyyorker**
   July 3, 2012

   The Higgs boson is a solid prediction of the standard model no doubt: gauge invariance and renormalizability predict this esoteric particle and it has now been observed. It's great, if expensive, science.

   But let's face it: the hype is WAY overblown. It's just not that huge of a deal to merit multiple announcements. Last year we had celebrations of a "bump". Now we have an "observation" and not a "discovery". I am getting past giving a shit, to be honest, and I have a little physics background. I can't imagine what the average joe thinks about this hoopla. It's a sign of the decline that we spend so much bandwidth on this particle.

3. **King Ray**
   July 3, 2012

   Any word or details on possible variations from the Standard Model Higgs, such as in the gamma-gamma and WW channels?

4. **King Ray**
   July 3, 2012

   I think it's very important whether the Higgs is a Standard Model Higgs or something else. If it's something else, then that means that there's new physics ahead. Plus the search for the Higgs has eliminated a lot of speculative theories that many have worked decades on, now fruitlessly.

   Experiment makes theory bleed, and keeps it honest.

5. **David Derbes**
   July 4, 2012

   @abbyyorker:

   I completely disagree. (Disclaimer: Peter Higgs was my thesis supervisor, so I'm not unbiased.) This is the greatest prediction and confirmation since the 1919 eclipse by Frank Dyson and Arthur Eddington.

   That the taxpayers of Europe spent ten billion dollars on what amounts to a postcard of equations by a guy in 1964 is breathtaking. Not claiming Higgs is the equivalent of Einstein (that would be silly) but this prediction to me is as astounding as the bending of light (by the right amount.) There is nothing like the Higgs that we know of; a fundamental scalar? Wild. (Maybe it isn't fundamental, but who knows yet?)

   People have been chasing this beast for forty years. Anticipation alone justifies quite a bit of hype. Add to that the immense labor, frustration, investment of time and money of so many for so long, and it's not hard to see that this is really one hell of a day.

6. **Anonyrat**
   July 4, 2012

   This is the greatest prediction and confirmation since the 1919 eclipse by Frank Dyson and Arthur Eddington.

   I'd amend that that this is the greatest prediction and confirmation since the observation of the Z boson.

7. **David Nataf**
   July 4, 2012

   I'd be hesitant to say the Higgs is a more fundamental confirmation of theory than say, the Dirac equation's prediction of antimatter, or Schwarzschild's prediction of black holes, or Friedmann's prediction of an expanding universe, but yes, this is cool.

8. **BJM**
   July 4, 2012

abbyyorker says:
"…It's a sign of the decline that we spend so much bandwidth on this particle."

You're kidding!!!

In the general scheme of things, news on the Higgs is a blip amid the flood of meaningless celebrity gossip and mendacious political propaganda that comprises what is laughingly referred to as "news" these days. The "average Joe" will remain almost totally unaware of the Higgs "hoopla".

Happy Higgs Day

July 3, 2012
Categories: Experimental HEP News

I hear reports that mobs of possibly violent physics live-bloggers have massed outside the CERN auditorium where the Higgs results will be discussed tomorrow morning. I'm going to sleep through this, then wake up late tomorrow (it's a vacation day here…), have a leisurely breakfast and check to see where the numbers ended up, then try out Philip Gibbs's applet.

I don't know exactly what numbers the experiments will be reporting, but basically both CMS and ATLAS should each have 4 sigma-ish or better evidence for the Higgs in two separate channels, gamma-gamma and ZZ. So, that's four independent measurements of a narrow resonance, any one of which would be strong evidence for the Higgs. Best bet for one of these coming in at over 5 sigma is probably the ATLAS gamma-gamma result. Or, just combine any two out of four of these results using Philip's software.

Things to look for if you're following the talks and press conference:

- The "D" word. Will it be used? Kind of a silly question though. July 4, 2012 will go down in history as the date of the announcement of the discovery of the Higgs, no matter what people say tomorrow.
- What are the signal sizes in the two channels? You should be able to use Philip's applet to combine the CMS and ATLAS numbers, and get a combined gamma-gamma number and ZZ number. Are these consistent with the SM prediction? Already tonight, hep-ph is starting to overflow with phenomenology papers describing models where gamma-gamma is enhanced with respect to the SM. I guess that indicates that tomorrow's numbers will be higher than the SM prediction.

I hope there will be plenty of champagne involved!

Update: Today so far I've been mostly on vacation, celebrating Higgs/Independence Day by sleeping late, doing a short piece on TV for Al Jazeera, going out for an excellent lunch, and lying around in the air conditioning checking out the news from other sources (it's brutally hot out there…). Later maybe a movie, dinner and fireworks.

The news was pretty much as expected: a strong signal from both experiments in two channels, very close to a 5 sigma level when combined. CERN did the right thing by simply claiming discovery, avoiding the situation suggested by the early AP story, where they seemed to be trying to say that they weren't quite at the discovery level. For details, the slides are here, and the usual suspects (Philip Gibbs, Tommaso Dorigo, Resonaances, Matt Strassler) all did an excellent job of providing details in real time as they came available. Probably also other bloggers I haven't had time to look at.

I'm still trying to get together combined numbers for the signal size in the various channels. It looks though that in the ZZ channel the size is close to the SM prediction, 2-3 sigma too high in gamma-gamma (nearly twice the expected size). So, still compatible with the SM, but the gamma-gamma excess is intriguing. Theorists with even better information than me have already started yesterday flooding hep-ph with papers supposedly explaining it.

Now, back to vacation….

Comments

1. **Q. I**
   July 3, 2012

   Peter, you don't seem to be happy about the announcement.

2. **Peter Woit**
   July 3, 2012

   Q. I,

   Not at all. It's a fantastic achievement, and amazing after all these years to finally be exploring experimentally the Higgs phenomenon. I'm looking forward to seeing what the numbers say, whether they agree with the SM or not. Either way though, it's an important day and a time for the field of HEP to celebrate (experimental) progress.

3. **fred**
   July 4, 2012

   although this is great work by the experimentalists, lets also remember the great work of theorists predicting this phenomena about 40 years ago, back when it was impossible to directly probe anywhere near this kind of energy scale! A great triumph to the power of theory, logic, extrapolation, etc. All things that are currently used by theorists today to look ahead to the new challenges

4. **Chris Oakley**
   July 4, 2012

   It is curious that they should choose July 4 as the date to make the announcement. Maybe it was to give Americans something more meaningful to celebrate than disloyalty to the British crown.

5. **Yuval Sanders**
   July 4, 2012

   I was watching the announcement today and I wanted to comment here, since your blog is how I obtain a lot of particle physics news. CMS announced a 4.9 sigma confidence of the existence of a new boson that has decay channels identical to what would be expected of a scalar Higgs. ATLAS announced 5.0 sigma. They are careful not to claim discovery of a scalar Higgs, but they are announcing the discovery of a new boson.

6. **jg**
   July 4, 2012

   Both teams are being ultra cautious, but at the end of the ATLAS presentation of the 5.0 sigma result CERN Director General Rolf Heuer says "I think we got it, you agree?" and the audience go wild! (Well, Peter Higgs was a bit more restrained, but he did finally applaud too)

7. **A.**
   July 4, 2012

Both Incandela and Gianotti gave nice talks, even if they did try to cram too many details in. Clearly excited by the results.

Heurer's "I think we got it, do you agree?" fell rather flat. Clearly scripted to be the "one small step" moment of the conference, did anyone else notice the awkward pause before the applause started, during which the audience realised they couldn't really do anything but agree?

8. **csa**
July 4, 2012

http://cdn.memegenerator.net/instances/250×250/22942101.jpg

In addition to the whole "comic sans" discussion.

9. **csvargas**
July 4, 2012

Sorry, I'm not a physicist but as I understand, "what gives mass to the Higgs boson" is the Higgs field. In fact, the field would be the really important discovery and the boson would be just its proof.

Am I getting the picture right?

10. **TB**
July 4, 2012

Thanks for blogging, I've lurked for a long time, and I appreciate the clarity you bring.

A lot of talk about whether this is a standard Higgs or not. This non-physicist isn't sure what that means and what it's implications are. Seems people invested in multiverse theory are very excited about that, but I could be misunderstanding?

11. **MomentCaptor**
July 4, 2012

I am such a huge fan of this weblog; however, when are you going to respond? It's July Fourth in America already!!! I've been anxiously awaiting to hear your response now that the talks are over and the champagne has be popped. Let's have it!

12. **SpearMarktheSecond**
July 4, 2012

The bump at 125 GeV in gamma gamma definitely persisted in the new data, and in 2 experiments. It is impossible that the bump is a statistical fluctuation.

Didn't stay up for the talks, but the slides don't invest much effort in,`What else could this be?'. But the S/B is so low their aren't a whole lot of useful plots (like distribution of the events in time, space, etc) that could help. If it is a systematic problem, only clever graduate students and postdocs in the huge collaborations could know.

I think CMS's shifting of bin widths and final plot weighted by S/B are very odd, a bit of unwarranted massaging. Thought Atlas seemed more consistent.

The 4lepton signals seem fairly convincing too, and are quite convincing evidence that the gamma gamma peak is not a systematic effect. Liked CMS analysis better… used kinematics better.

All the rest is smush, except, maybe, the Tevatron.

13. **anonomous**
July 4, 2012

I should have gone to sleep but I couldn't help it and stayed up to watch the live webcast. What a great presentation – the excitement of watching it live was well worth it (especially the applause when Peter Higgs entered the room!). This has to be the biggest day in physics in the last 100 years!

14. **neo**
July 4, 2012

What are the implications of a *single* higgs @125-126GEV to SUSY extensions of SM? If the SUSY exists, should the LHC have detected multiple Higgs and not just one?

15. **Fred D.**
July 4, 2012

What is the actual probability that the discovered boson is something other than the Higgs? I would think that probability is very small.

16. **Casey Leedom**
July 4, 2012

So now that we "know" that there's "something" at 125GeV, would it be possible to construct a specific experiment/set of equipment to specifically look at this mass range in order to get more details more quickly?

17. **Sciency Sciencer**
July 4, 2012

Maybe they chose independence day because the US stopped funding their collider project and left it to a collaborative international effort?

18. **Peter Woit**
July 4, 2012

Fred,

Whatever they are seeing, its properties are close to those of the SM Higgs. From now on, attention will focus on measuring "how close". Quite possibly the result will end up being "it behaves exactly like the SM Higgs is supposed to, to within the accuracy we can measure". Much more interesting would be: "it behaves like the SM

Higgs, except it does X differently", which would be an exciting clue about how to do better than the Standard Model.

Casey,

There has been talk of muon-antimuon or photon-photon collider "Higg Factories", and now we know the energy they would have to be designed for. The problem with muons is their short life-time, but people are working on the concept. I don't know much about photon-photon colliders, but such a thing might be possible to build for less than the cost of the ILC.

19. **Obs**
July 4, 2012

As for the date, there's no need for conspiracy theories here — the annual International Conference for High Energy Physics (ICHEP) opened today in Melbourne, and the CERN seminar was synced with the opening reception (and not just the seminar — the LHC run schedule for 2012 was intentionally designed to produce as much data as possible before the conference.)

20. **Tony Smith**
July 4, 2012

Peter, as to muon colliders,
it is not the "… problem with muons is their short life-time …"
because relativistic time dilation takes care of that nicely.

The most serious problem is neutrino radiation.
See hep-ex/0005006 by Bruce King of BNL for details.

At first thought, neutrino radiation looks not harmful,
but in a muon collider the neutrino flux is so great that it irradiates
a lot of the surrounding land making it radioactive,
which secondary radioactivity is really serious.

If it were not for the neutrino radiation, it would be a no-brainer
to build a muon collider on the Fermilab site,
but the radiation would seriously endanger Chicago.

Maybe the Chinese Western Desert would be a good place to build it,
but a lot of development work could be done at Fermilab.

Tony

21. **Callum**
July 5, 2012

I have been looking for a comment on the difference between the CMS and ATLAS results – 1 GEV sounds a lot to me. Is this a result of experimental or analytical differences?
(sorry if this has been covered before…)

22. **piscator**
July 5, 2012

Callum: 1 Gev is not so big compared to 125 GeV. The error on the quoted results is about +- 0.6GeV for so a 1GeV difference is within 2 standard deviations so nothing to get excited about. If it persists as the error decreases then the experiments would need to look for systematic differences.

23. **Peter Woit**
July 5, 2012

Evidently at the Lindau meeting yesterday Rubbia made the case for a muon-antimuon Higgs factory, see here

http://physicsworld.com/cws/article/news/2012/jul/05/new-boson-sparks-call-for-higgs-factory

and here

http://www.newscientist.com/article/dn22021-physicists-propose-factory-to-spew-out-higgs-particles.html

Higgs, Higgs, Higgs

July 5, 2012
Categories: Experimental HEP News

I'm trying to get over my Higgs obsession, and move on to other topics, but one last posting about this for now…

The first thing to say is that this is the biggest thing to happen in fundamental physics in about 30 years (i.e. since the discovery of the W and Z). It's a remarkable event and huge success for high energy physics, vindicating at the same time the colossal efforts that have gone into making the LHC and its detectors work, as well as the theoretical framework of the electroweak part of the Standard Model. Today's New York Times has a front-page story by Dennis Overbye, above the fold, which is very well done. In general the press reports that I've seen have been quite good, with minimal speculative nonsense thrown in. According to Overbye, CERN DG Heuer made the right decision to go ahead and simply claim discovery only on Tuesday afternoon. All in all, CERN has done an excellent job of communicating this story to the public (except perhaps for the "don't believe the bloggers" business, but what else could they do…).

Attention will turn now to who gets rewarded for all this, in particular, who gets a Nobel Prize? Personally I think the experimentalists are first in line, and with no obvious figureheads, a prize for three groups: ATLAS, CMS and the CERN accelerator engineers and physicists would be highly appropriate. If it's not too late in their process, maybe this could even be done in time for this year's prize, announced October 9.

As far as theorists go, Frank Close has posted something about this here. With the restriction to three people, he argues for Englert, Higgs, Kibble. Personally I think Anderson deserves a piece of it, see here. There's also a good argument to be made that what has just been validated is not the older work on the Higgs mechanism, but the Weinberg-Salam model of 1967 (extended to quarks), and that has already been rewarded with a Nobel.

I've been trying to get accurate numbers for the signal sizes seen by CMS and ATLAS in the various channels, but the only information out there now is the slides from the two talks. Resonaances includes the crucial plot from each experiment giving the signal sizes normalized to the SM, and eyeballing these and averaging, one gets 1.0 in the ZZ channel, 1.75 in gamma-gamma channel, about .75 in the WW channel (only CMS reports 2012 data). In the bb and tau-tau channels, no significant signal is seen, but the expected signal size there is very small. The errors per experiment are something like +/- .4, which you can make your own judgement about how to reduce for the combination. The bottom line is that, within errors, everything is consistent with the SM predictions. The gamma-gamma channel is the one to watch, it is about 2 sigma high.

The DG also announced a new LHC schedule, extending this year's proton-proton run by two months, to mid-December. This will hopefully allow the experiments to each accumulate another 20 inverse fb of data, finishing this run and going into a two year shutdown with a total of 30 inverse fb to analyze and use to improve the results on the Higgs.

While this announcement is a great triumph for physics, unfortunately it significantly increases the probability of what has become known as the "Nightmare Scenario": a SM Higgs discovery and nothing else at LHC energies. Before the LHC results started to come in, this scenario and its consequences was easy to ignore, but we may be getting closer to the point where it needs to be taken very seriously.

Update: For a rather complete analysis of the data about the different Higgs decay channels, see this new preprint.

Comments

1. **Marc**
   July 5, 2012

   Alas, Nobel prizes (other than peace) can't be awarded to groups, so it can't go to the collaborations (if so, would they each get $200?). Any thoughts on what individuals would get it for the experimental discovery?

2. **David Derbes**
   July 5, 2012

   Peter, you're exactly right that the ATLAS and CMS teams deserve recognition, and it wouldn't be surprising if the next Nobel honored them first as opposed to the large numbers of folk behind the well-known mechanism. For one thing, there may be residual doubts about the Higgs-ness of the new particle (which I do not share, but very cautious people may have 'em.)

   All that said, had Brout not died, I think that the next Nobel should have gone to Higgs, Englert and Brout. I'm sorry that the Nobel statutes limit it to three; I have said here before that I think the Sakurai Prize committee did the right thing (though, now that I hear about the brilliant Migdal and Polyakov having had similar ideas, maybe they were short two.) I don't feel so strongly about Anderson who (a) has one, well deserved, and (b) did not, as far as I am aware, ever write down a model, not even a non-relativistic model. Were Nobels given for brilliant insights alone, Gamow would have won three (well, at least two in physics and maybe one in medicine/biology.) Frank Close's *Infinity Puzzle* is really excellent (and not only on weak interactions, either.) If he thinks Tom Kibble should get a third, the Nobel folks ought perhaps to listen to him. (Disclaimer: I almost went to study with Kibble and/or Salam, but very happily Peter Higgs took me on.)

   There are however two other avenues for a Higgs theory prize: give it only to Higgs and Englert; or give it to Higgs and Englert and a third not directly connected to the scalar, to be named later. I can think of several who might reasonably fit here. My only concern, very frankly, is that it come soon, as none of the founding fathers of this stuff are getting any younger, and they don't award 'em posthumously.

3. **Peter Woit**
   July 5, 2012

   Marc,

   If the obstruction to awarding the prize to groups can be overcome, this is the time to do it. And this case makes clear why it should be done. As far as I know, there's no sensible way to pick out specific people in CMS/ATLAS to award the prize to. On the other hand, if any experimental discovery is worth a Nobel prize, this one is. Effectively making HEP experimentalists ineligible for the prize, while instead devoting a lot of time to picking out theorists for their often somewhat obscure and marginally relevant contributions (the models BEHGHK worked with are far from the SM Higgs model that has just been experimentally validated) seems to me a really bad idea.

4. **Roger**
   July 5, 2012

   If the prize goes to Higgs-Englert-Kibble, then the tag line is going to be that the LHC did not find any new physics so they gave the prize for work done 50 years ago.

5. **Michael**
   July 5, 2012

   Hi Peter,

   I really like this particular post. Nice have something nice to talk about, eh? 😉

   Let me remind everyone that the Tevatron experiments showed their update just five days ago, on Monday. While their signals are not strong enough to claim evidence for a new state, and don't match the significance of the CMS and ATLAS results, the fairly significant excess in a bb final state (showing up in a the associate production VH channel) is an important piece of information. Apparently this Higgs boson does decay to bb at a rate close to what the SM predicts.

   I am puzzled that the CMS tau results show a deficit, but the statistical power of the tautau analyses is not strong enough for this to be important yet (especially since we do have the bb signal at the Tevatron). As you say, gammagamma is the interesting channel at present…

   regards,
   Michael

6. **Peter Woit**
   July 5, 2012

   Thanks Michael,

   You're right that I should have included the Tevatron bb result in the summary of signal sizes here. It's 1.97 +.74/-.68 (where 1 is the SM), so, a bit high, but not very significantly so.

7. **emile**
   July 5, 2012

   I would suggest that the prize be divided like this: one half to Higgs and Englert, one half to CERN (that would cover the LHC scientists, and the ATLAS, and CMS collaborations). They should find a way to make this happen, and make it happen this year before the theorists pass away.

8. **Beelzebud**
   July 5, 2012

   I'm curious about what happens if this "nightmare scenario" comes to pass. I'm a layman, so forgive my ignorance. If this plays out and nothing beyond the standard model is found, is it possible that the LHC could be upgraded to probe deeper, or would this pretty much require a whole new system to be built?

9. **neo**
   July 5, 2012

   "While this announcement is a great triumph for physics, unfortunately it significantly increases the probability of what has become known as the "Nighmare Scenario": a SM Higgs discovery and nothing else at LHC energies. Before the LHC results started to come in, this scenario and its consequences was easy to ignore, but we may be getting closer to the point where it needs to be taken very seriously."

   How does this finding "increases the probability of what has become known as the "Nighmare Scenario": a SM Higgs discovery and nothing else at LHC energies."

   125GEV Higgs is compatible with MSSM/SUSY and there is still the hierarchy problem

10. **Bob Levine**
    July 5, 2012

    I think the Higgs breakthrough signals not just a potential 'end of an era' in discovery in physics, but the end of the 'Nobel prize' approach to discovery itself. The heroic model of breakthroughs driven by individual genius, on the model of Einstein, Dirac and Rutherford is, as the whole LHC experience shows, a thing of the past. The first great advance in forty years required the work of literally thousands of experimentalists and, arguably, a dozen or so so prominent theorists at the very least, and the fact that there's so much room for debate about who deserves a share of the Nobel suggests that the award criteria for the prize should be rethought from the ground up.

11. **Marc**
    July 5, 2012

    Peter, emile –Alas, the restriction to individuals is explicitly in Nobel's will, so it can't be given to ATLAS/CMS/CERN. I agree that if there were ever a time to change it, this is the time, but it was Nobel's money, and his right to put those conditions on it.

    It would have been fun if Kibble's paper had been a little earlier. Then we would be talking about how vector bosons eat Kibbles to get mass.

12. **Seth Thatcher**
    July 5, 2012

    Peter, what do you mean that the excess in gamma gamma channel is intriguing and what could it mean that the signal is 2 sigma higher than expected. Any informed speculation for us physics junkies?

13. **Eli Rabett**
    July 5, 2012

    There is lots of precedent for giving the Nobel Peace Prize to groups (IPCC, IAEA, UN, Médecins Sans Frontières, etc., International Campaign to Ban Landmines) the Swedes merely have to get with the program

14. **Casey Leedom**
    July 5, 2012

    And in a somewhat fitting commentary on the entire announcement event, we have [What if Steve Jobs Had Discovered the Higgs Boson?](#) It's fun …

15. **Eric**
    July 5, 2012

At some point, there should also be a Nobel Prize for Ellis, Gaillard, and Nanopoulos for

J. R. Ellis, M. K. Gaillard and D. V. Nanopoulos, Nucl. Phys. B 106 (1976) 292

which basically told the experimentalists how to produce and search for the Higgs.

16. **cormac**
July 5, 2012

Nice post, but I disagree about the Nobel.
Q; Who cares? This discovery is bigger than any prize. It is too important to be reduced to 'who gets the prize'. For example, in the case of Higgs himself, having an important particle named after you is far more important (though it is a little unfair on the others)

I have an idea; perhaps physicists around the world should use this occassion as an opportunity to persuade the Swedish Academy of Science to relax their self-imposed rule. The three-body rule is an anachronism that has no relevance to the way science is done today, at least for experimentalists

17. **jg**
July 5, 2012

In Nobel's time Physics wasn't done by huge collaborations, and discoveries were often the result of the brilliant insight of a single individual or experimental work by a small group . Similarly for Biology and Chemistry.

I am sure that Nobel himself would have wished the rules be changed to reflect changing times.

Give the award to CERN/ATLAS/CMS, maybe donate the prize money to charity.

Higgs, Englert, Kibble etc have already had plenty of recognition (and honours), I'm sure they wouldn't begrudge the experimentalists their recognition.

18. **Peter Woit**
July 5, 2012

Beelzebub,

The LHC can be upgraded to produce more collisions, and that plan is already in place. In principle it can also be upgraded to go to higher energies (you need to design and build higher field magnets). There are also proposals for other machines that could get to higher energies. The problem with the "nightmare scenario" is that it suggests that if you do build a higher energy machine, you'll see nothing new, i.e. no new phenomena will appear unless you go to some astronomically high energy scale like the Planck scale, and that is way beyond any conceivable technology.

neo,

Finding a Higgs particle with exactly the right SM couplings is not evidence for SUSY, no matter how many people say that "125 GeV is good for SUSY" or something similar. It's evidence that for the non-SUSY SM.

As for the "hierarchy problem" argument, the popular version of it: "to stabilize the Higgs mass (125 GeV) or EW breaking scale (246 GeV), you need new physics at the TeV scale" has already fallen flat on its face. As far as SUSY goes gluinos and squarks limits are at 1 TeV and above (yes, the stop limit is lower, but there's no good reason for a light stop and every other squark heavy). I've argued endless here that the "hierarchy problem" is not so obvious unless you insist that you know what physics looks like at GUT or Planck scales, and want to keep those separate from the electroweak scale.

19. **Peter Woit**
July 5, 2012

Seth,

For informed speculation about what would enhance decays in the gamma-gamma channel, I suggest Matt Strassler's blog or Adam Falkowski's (Resonaances). They're phenomenologists who know lots more than I do about the possibilities. My general impression is that there are lots of models you can imagine, but nothing really motivates any particular one.

20. **yyy**
July 5, 2012

@Beelzebud
> If this plays out and nothing beyond the standard model is found, is it possible that the
> LHC could be upgraded to probe deeper, or would this pretty much require a whole
> new system to be built?
It would be very hard to increase LHC's energy: theoretically it is possible to change LHC's NbTi magnets to Nb3Sn ones and have 2-times stronger magnetic fields and double energy (28 TeV instead of 14 TeV) but the cost would be quite high.
Nevertheless properties of Higgs boson couldn't be measured precisely in the proton collider – high energy lepton collider must be built to do that and this would be next step. Currently 3 options are considered here: linear e+e- colliders (ILC or CLIC) and the recirculating mu+mu- collider (Muon Collider).

21. **Beelzebud**
July 5, 2012

Thanks for the information, Peter and yyy. I appreciate it.

22. **ohwilleke**
July 5, 2012

It seems that the only game in town based on the data to date to avert a "Nightmare Scenario" is the possibility that the diphoton excess relative to the Standard Model expectation will continue to appear and not fade into the realm of a statistical fluke.

If it does, you have some new physics (SUSY or a composite Higgs, perhaps) and good clues about where to look for it. If the excess fades as more data becomes available, then we are in the particle physics "desert" and it is likely there there will be "no new physics" at the LHC other than the discovery of a SM Higgs.

23. **Jim Rohlf**

July 5, 2012

Peter says "this is the biggest thing to happen in fundamental physics in about 30 years (i.e. since the discovery of the W and Z)" and I agree as I was there in the middle of it doing the analysis. The first Z event (after the W was in the bag) was found at midnight and I examined it for 7 hours before I called Carlo Rubbia at home and told him we discovered the Z. With 4 events we wrote the paper (see http://www.symmetrymagazine.org/cms/?pid=1000630). Now I am on CMS. This analysis is infinitely more complicated. There is a lot to do to show that this is the higgs. The lack of tau signal is a big deal because the Z -> tau tau is cleanly measured. In my opinion this (proving the higgs has been seen) will be difficult to do this this year and I have made two bets last December to that effect, one with my grad student and another with a CMS colleague. I hope I am wrong.

24. **neo**
July 5, 2012

For whatever it's worth Sean Carroll over at CV says "Preliminary thought #1: There is a "nightmare scenario" that particle physicists have worried about for years. Namely: find exactly the Standard Model Higgs and nothing else at the LHC. I personally assign the nightmare scenario very low probability."

25. **Thomas Wheeler**
July 5, 2012

Please don't get over your obsession with the Higgs. We are all obsessed with the Higgs
and will be for a long time. Any thoughts or information about the Higgs that you can share will be greatly appreciated .

26. **Peter Woit**
July 5, 2012

neo,

The arguments that Sean gives aren't very strong, and he admits that some of them, e.g. the idea that the Higgs is a "portal" to dark matter is just a "wishful hope". I also don't see how the idea that the LHC will solve the dark energy, strong CP or baryogenesis problems is much more than wishful thinking. About the "hierarchy problem" argument, that just hasn't worked out.

Maybe dark matter is a phenomenon that will be explained by the LHC. Could be, but I don't see a reason to assign this very high probability. And the other arguments he gives just don't seem to me to add up to a very high probability either. So, I think the "nightmare scenario" gets quite a bit more than "very low probability".

27. **SpearMarktheSecond**
July 5, 2012

Hooray for Jim Rolph. He is a gem!

If this particle is the Higgs… sure it will take a while to prove it. Not just with decays to b-bbar and tautau, but also no signals in ee and mumu (is that already done??) and c-cbar and 0-life jets. But if it is the Higgs it is super interesting what keeps it so darned light! SUSY is a good candidate… but whatever it is, it seems to have something to do with the Weak interaction, which is real interesting.

If this particle is not the Higgs… yoo-hoo, that is an even bigger deal.

28. **D R Lunsford**
July 6, 2012

What a great post, Peter, and don't we all have smiles on our faces? 🙂

Attention will turn to the excess in the yy channel, let's hear what you think.

A long distance since "String Theory, an Evaluation".

-drl

29. **Allan Rosenberg**
July 6, 2012

Peter, if you were the dictator of CERN, where would you suggest they focus their detectors, outside exploring the properties of the new boson?

30. **Peter Woit**
July 6, 2012

Allan,

My knowledge of experimental HEP is good enough to know I have no business advising people at the LHC how to do their jobs. As far as I can tell, they're doing just fine without me. About the only not very useful comment I can make that might be relevant is that they might want to not take so seriously what some theorists tell them about SUSY models…

31. **WW**
July 6, 2012

Hello,
Is it unreasonable to consider the 7 and 8 GeV CMS and ATLAS data as 4 independent experiments? In all of them an excess of about 2 has been observed in the 2 gamma channel. There is then a 1:16 chance for this to happen if it is only for statistical reasons.
For this and other oddities, I think it is really premature to discuss who of the theorists should get a
Nobel prize. If we eventually know better, this is clearly the merit of the experimentalists. Leaving them out would put the whole thing upside down.
[P.Higgs original paper was one page!]

WW

32. **David Nataf**
July 6, 2012

Peter,

When you have time, I'd love to hear your thoughts on the 4-sigma signal of a gamma-ray emission line (that could be a dark matter annihilation line) toward the Galactic center at an energy 130 GeV,

Discovery paper:
A Tentative Gamma-Ray Line from Dark Matter Annihilation at the Fermi Large Area Telescope
Christoph Weniger
http://arxiv.org/abs/1204.2797

Best analysis to date:
Strong Evidence for Gamma-ray Line Emission from the Inner Galaxy
Meng Su, Douglas P. Finkbeiner
http://arxiv.org/abs/1206.1616

The first version of the abstract of the second paper comments on how the energy is very close to that of the Higgs, I think they suggest the dark matter particle might decay into the Higgs. That is removed in the abstract of the second version.

33. **Peter Woit**
July 6, 2012

David,

My main thought about that is that I'm not an astrophysicist, and know pretty much nothing about the tricky subject of trying to extract some sort of dark matter annihilation signal from astrophysical sources. So, for this topic you have to go elsewhere…

34. **fred**
July 6, 2012

woits claims that the hierarchy problem requires us to known what happens at GUT or Planck scales is wrong. If the SM is correct to very high energies it is fine tuned. If it breaks down at a lower scale there is a chance to avoid the fine tuning. Those are just facts. we don't know which way it will go. will it be a fine tuned universe, or new physics? hopefully we'll be able to find out.

35. **truth**
July 6, 2012

what has so far been missed in almost all the discussions of the Higgs boson is the most central issue of all. Namely, the purpose of the Higgs boson:

THE ENTIRE PURPOSE OF THE HIGGS BOSON IS TO RESTORE UNITARITY!

The Goldstone models couple to the W, Z bosons to give them mass and the vev gives mass to the fermions. None of that requires the extra degree of freedom which is the Higgs boson. The only reason we have to add this extra degree of freedom is to ensure the theory is unitary at high energies.

So what the LHC has discovered is that unitarity is respected by nature. This is the real content of the discovery. It is quite interesting to me that unitarity is the guiding principle of string theory, i.e., string theory is the only known consistent theory of gravity that exactly respects unitarity. This is extremely interesting.

36. **A.**
July 6, 2012

Purely in terms of securing funding, I'm not sure the nightmare scenario is something to be feared. I think there's been sufficient publicity surrounding CERN and the Higgs that it's become part of the public and – more "importantly" – media conciousness, at least in certain European countries. Given the sickening power that the media has these days, such awareness will be all that's needed to make The Higher Ups sign off on the next accelerator proposal. (Scientifically, well, that's another matter, but if no-one tells them we've no clue then we'll probably get away with it.)

37. **Peter Shor**
July 6, 2012

Isn't the "nightmare scenario", that the LHC doesn't find anything other than the Standard Model, exactly what Occam's razor predicts? (Although I have been told that you can easily add sterile heavy right-handed neutrinos to the Standard Model, and that these could both explain dark matter and the low mass of the left-handed neutrinos [using the see-saw mechanism], so maybe Occam's razor actually predicts the Standard Model with added heavy sterile neutrinos. I don't know whether these could be observed by the LHC.)

38. **CWJ**
July 6, 2012

While I agree this is big, I think the discovery of massive neutrino was a bigger discovery of the last 30 years. The Higgs was long anticipated, and "merely" completed the Standard Model. Neutrino masses were the first, and so far only, extension beyond the original standard model. At the very least massive neutrinos compete with Higgs as a major discovery of the last 30 years.

To be sure, this is big…

39. **truth**
July 6, 2012

Peter I don't get your philosophy.

On the one hand you are saying that there is a good chance of the "nightmare scenario"; this is the scenario where there is a great desert between electroweak physics and whatever is the physics that relates the SM to gravity, and the other mysteries we have. Here you seem to be attacking phenomenologists for suggesting new physics would should up at the low electroweak scales.

On the other hand you dislike most research into quantum gravity, especially string theory. Here theorists speculate on physics at the highest of energy scales, possible related to the Planck scale, etc.

So you seem to be attacking both the PHENOMENOLOGISTS, who look at hints from the world around us, such as dark matter or hierarchy problems etc, to speculate on

"low energy physics" (meaning directly accessible energy scales).
But you are also attacking quantum-gravity THEORISTS, who use logical consistency and mathematics etc, to speculate on "fundamental high energy physics" (meaning not-directly accessible, as it might be Planckian, etc).

So you seem to be having your cake and eating it too. If someone thinks that new physics will show up just around the corner, you say that there is a nightmare-scenario that means there will be nothing. If someone thinks that new physics will come in at the Planck scale and formulates a theory of this, you say that this is just wild crazy speculation. So all groups of theoretical physicists interested in expanding our knowledge of the fundamental laws seem to garner your attacks. It seems rather illogical and inconsistent.

40. **truth**
July 6, 2012

Peter Shor,
I'm not sure that's what Occam's razor would say here. The Occam razor would say that the simplest explanation consistent with all the facts is probably the right one. Well, the Standard Model is not consistent with dark matter, unification, gravity, baryogenesis, inflation, hierarchy of scales, smallness of \theta angle, etc, i.e., various facts about the world. So Occam would prefer the simplest theory that encompasses all this, which cannot be the Standard Model, since it doesn't. After all, that's why we built the LHC.
Now I'm not saying that these things will show up at LHC. Some of them might, some might not. Its just that its unclear which way Occam's razor points with regards to the scales the LHC can probe.

41. **Peter Woit**
July 6, 2012

truth,

How about celebrating the experimental discovery of the Higgs with a one-week moratorium on hype about string theory?

As for my "philosophy", it's pretty much that hyping ideas that don't work is not a good idea, during Higgs week or any other.

42. **truth**
July 6, 2012

Of course I am celebrating the great experimental work. I am also celebrating the great theoretical work from almost 50 years ago to predict this. Quite amazing. Around 50 years ago theorists started putting together a highly abstract theory built on consistency, logic, and extrapolation of the principles of quantum field theory to understand the weak interaction. This was put together despite the fact that there was no direct experimental evidence for the Higgs whatsoever. Back then there were skeptics calling into question the audacity of such theorists to speculate on such high energy physics without the necessary experimental input, and physics was faced with a tremendously daunting task to ever sort it out experimentally. In fact we could even go back to the first observations of the weak interaction and the Fermi theory in the early part of the century. Well here we are, many decades later, finally seeing direct evidence that a consistent QFT describes the weak interaction (first the W and Z bosons were introduced to make the theory renormalizable, and then the Higgs to make it unitary; It was not some ad hoc set of ingredients, but the minimal set of ingredients necessary to describe a short-ranged interaction that mediates decays). Those so-called theoretical "speculations" were actually built on consistency and fundamental principles, and they proved to be entirely correct. This is the great accomplishment of theory and of course the follow up by experiment and observation many decades later. I would argue that theorists who study quantum gravity are engaged in the same qualitative exercise, even if there are skeptics who won't admit it. It is yet another great triumph of the scientific method.

43. **Roger**
July 7, 2012

't Hooft wrote an interesting letter criticising an attempt to expand the Nobel prize to include other areas (http://www.staff.science.uu.nl/~hooft101/NobelPrizesExtend_09.html). In some of these disciplines institutions would need to be acknowledged and not individuals. He's pretty much against it in those cases. I wonder what his attitude is to particle physics collaborations.

44. **The Fly**
July 7, 2012

I'm surprised to see that none of you paid attention to the most important thing Peter mentioned in his post: the two month extension of this year's run. Don't you wonder why this is? Ask a knowledgeable CMS experimentalist if you can find one.

45. **C.**
July 7, 2012

The ~2 month extension of the pp running at the LHC is to allow the experiments to gather as much data as possible on the Higgs decays before the long shutdown starting in early 2013. The schedule has been announced. The pp run will end December 17, at which point the machine will go into a "technical stop" until January 7. At that point operations will resume to prepare for the heavy ion run, actually p+Pb, which will go from January 17 to February 11. After that the LHC goes into the shutdown for the upgrades needed to double to the design collision energy.

46. **Chris W.**
July 7, 2012

In the latest issue of Symmetry Magazine, Joe Incandela draws an analogy with the discovery and *gradual* excavation of King Tutankhamen's tomb.
http://www.symmetrymagazine.org/cms/?pid=1000971

47. **someone2**
July 7, 2012

c.,

so p+Pb is the data what we need for precise obsevation for a certain model?

48. **C.**
July 7, 2012

The p+Pb running is not related to the Higgs physics. It does serve as a baseline ("cold nuclear matter effect") for the heavy ion physics program at the LHC where there are Pb+Pb collisions which happened in 2010 and 2011. The heavy ion physics program studies the Quark Gluon Plasma, an extended region of nuclear matter which has become so hot that the constituent protons and neutrons have melted into quarks and gluons and many more pairs of quarks and antiquarks have been produced. While the

ICHEP 2012 is the big international conference celebrating the pp type high energy physics, the Quark Matter 2012 conference in August at Washington DC (http://qm2012.bnl.gov/) is the premier conference in that physics. During the shorter heavy ion runs, the CMS, ATLAS, and ALICE collaborators are focused on acquiring that data.

49. **Mano Philips**
July 8, 2012

On the origin of mass; the hype seems to be that the Higgs field imbues all elementary particles with mass. Frank Wilczek (http://lanl.arxiv.org/abs/1206.7114) and Tom Kibble (http://www.scholarpedia.org/article/Englert-Brout-Higgs-Guralnik-Hagen-Kibble_mechanism)
tell a different story.
Kibble – "It is sometimes said that the Higgs field gives masses to all other particles, but that is not strictly correct. It is important to note that most of the mass of the nucleon in particular does not arise in this way. Only the masses of the quarks come from the Englert-Brout-Higgs-Guralnik-Hagen-Kibble mechanism. The larger part of the nucleon mass comes from a mechanism along the lines sketched out earlier by Nambu (see Englert-Brout-Higgs-Guralnik-Hagen-Kibble mechanism (history))."
Can someone explain.

50. **Peter Woit**
July 8, 2012

Mano,

In QCD the proton and neutron are massive, even in the limit as you take quark masses to zero. You can think of the mass as coming from the gluon field and from the phenomenon of confinement. This mass has nothing to do with the Higgs, would be there even without a Higgs. So, the statement that "all particle masses" come from the Higgs is inaccurate. You could claim that all "elementary particle masses" come from the Higgs. The proton and neutron are not elementary but composite states of quarks and gluons.

51. **Gilbert Weinstein**
July 8, 2012

"In general the press reports that I've seen have been quite good, with minimal speculative nonsense thrown in."

OK, this might be the exception that proves the rule, but I couldn't resist: http://on.msnbc.com/LI11Es

52. **chiz**
July 9, 2012

Nobel nominations have to be in by the end of January. The committee can add in extra names themselves, but, as I understand it, they have to do so before consideration begins.

53. **Juan Ramón González Álvarez**
July 12, 2012

And we can already find some people speculating that what was observed is not the Higgs, but a scalar top supersymmetric quark: "The Fermilab pair thinks that it could be emerging evidence for SUSY". Not that this would be considered seriously, but it made me laugh

http://physicsworld.com/blog/2012/07/the_higgs_seven_days_on_what_h.html

Court Judgement in Nature/El Naschie Case

July 7, 2012
Categories: Uncategorized

Nature has finally won its court case against Mohamed El Naschie, see here. This was based on a 2008 Nature story by Quirin Schiermeier, which during the case was removed from the Net, but now is back up. The court found that this article was accurate, not libelous. I had talked to Schiermeier and was accurately quoted in the article. Over the past couple years, I've heard a few times from *Nature*'s lawyers that the case was in progress, but didn't know the details. The court judgement has full details, and is kind of interesting reading, it's available here (thanks to Hamish Johnston for pointing me to this).

I first came across El Naschie when a commenter back in May 2005 mentioned his papers here. It was immediately clear that the journal El Naschie was editing for Elsevier was highly problematic, and surprising that they hadn't done anything about it long ago. From the court documents it seems that they finally realized how much damage it was doing to their reputation and decided to shut it down, giving notice to El Naschie in June 2007. By then, much of the damage was done. If you talk to mathematicians who support the Elsevier boycott, the story of this journal is one that gets mentioned often as evidence for just how bad Elsevier's policies have been. It was the Elsevier problem that immediately caught my attention when I took a look at the journal and responded to the 2005 comment.

Neil Turok was brought in for the job of evaluating El Naschie's papers, and you can read the results in the court judgement. Perhaps the most striking thing about all this is not the weird El Naschie story or the problematic Elsevier story, but that it brings serious discredit to the British court system. This is a case that should have quickly been thrown out by any reasonable judicial system. Instead the defendant was forced to devote huge resources in terms of money and time to mount a defense. Many are pointing out that only a large corporate organization like *Nature* can afford to do this.

Update: There's an excellent piece in Nature this week by Quirin Schiermeier, the reporter who wrote the article about El Naschie that led to the lawsuit. While supported by Nature, he had to devote a great deal of time and energy to the suit, and he makes clear the intimidating effect on accurate reporting that the British libel system imposes, with effects reaching well beyond British borders.

Comments

1. **Jurisper**
   July 7, 2012

   Think there's an error in link to judgement – this works for me:

   http://www.taylorwessing.com/fileadmin/files/docs/El-_Naschie_handed_down.pdf

2. **Peter Woit**
   July 7, 2012

   Jurisper,

   Thanks. Fixed.

3. **Matt Leifer**
   July 7, 2012

   The problems with British libel law have been recognised by the government after a successful campaign by free-speech and science-related interest groups. Actually, this has been a much debated issue in the UK ever since the British Chiropractic Association attempted to sue Simon Singh for stating scientific facts about Chiropractic in a newspaper column a few years ago. A libel reform bill is due to be debated in parliament this session (see http://www.independent.co.uk/news/uk/politics/new-bill-will-reform-libel-laws-7728516.html) and I am optimistic that there will be cross-party support. Hopefully, the El Naschie libel case will be the last of this sort.

4. **Anonymous Coward**
   July 8, 2012

   For anyone interested in the British libel issues, Ben Goldacre has written at length abouth this:

   http://www.badscience.net/category/libel/

   And the story about his book chapter is particularly chilling:

   http://www.badscience.net/2009/04/matthias-rath-steal-this-chapter/

5. **Roger**
   July 9, 2012

   That these people exist isn't a surprise. What is surprising to the extent to which he gained a foothold in the academic world. Why did Elsevier (a) appoint him in the first place and (b) carry out no meaningful oversight of the practices of the journal ?

   I guess we'll never know even though it would be useful information to prevent future problems popping up.

   I did a bit of internet digging on this story. There are quite a few pictures of El Naschie with Nobel laureates. He looks like a fan asking a star to pose with him. One picture, showing a large number of participants at an event, has been photoshopped such that it appears only he, Wilczek, Gross and t'Hooft were present and posing together. The original and manipulated picture are both available.

   The written judgement is interesting in that it shows by just how much El Naschie was driven by prestige. He demanded a one year "grace period" to step down from his editorship to avoid it looking like he was pushed.

   The details of the case (and of the man himself) become a little boring after a while since we all feel as if we know someone like him. The mystery is how he ever came to an edit an (apparently) respectable journal.

6. **Hamish**
   July 9, 2012

The answer to how he managed to maintain his charade is simple — he has (had) lots of money, which he used to both ingratiate himself within the academic community and perhaps more importantly, to bully his critics into silence.

7. **Roger**
July 10, 2012

Money can only take someone so far. This gentleman ended up founding and editing an apparently mainstream journal with no (or wholly inadequate) oversight.

8. **Bobito**
July 10, 2012

@Roger: Why these people exist, and how they exist is easy to explain. In my department there is a fellow who has published many of his papers in Chaos, Solitons, and Fractals. (These articles are without content). It had a decent impact factor, and so his curriculum was improved – in my country the curriculum is scored in a rigid manner in part by a paper count weighted by impact factor. People like this fellow have a vested interest in supporting people like El Naschie. They need overpriced incompetently edited journals – it gives them a place to publish articles which no one will read (if anyone read them, they might be discovered for what they are). Meanwhile, Elsevier makes money off the journal.

In countries with poorly functioning university systems, this sort of behavior can take a man to the top – his curriculum impresses outsiders – he is an editor, author of many papers with fancy titles, etc. – and with this he secures many grants – which he uses to further consolidate his position. When a system has within it few researchers of high scientific quality, and these people are struggling to find stable employment (as is often the case), there is no oversight or control worth the name, because those tasked with oversight and control lack the relevant scientific criteria. Charlatans can dominate the system. This situation is hard to understand if one has spent one's entire professional life in a typical US or UK university, where things work much better. One has to imagine that the a***** scumbag operators that are present in every department are all there is.

9. **StevenC**
July 11, 2012

For Elsevier, this could simply be just a simple oversight – the rubbish journal appeared to earn money, and Elsevier did not have enough resource to monitor every single journal that is under them… when eventually Elsevier did discover the journal was rubbish, they toke action, and El Naschie fought back.

I think the real punchline that we should be glad that the court did the right thing. I think there are more severe versions of the same problem in our society – rubbish journals may only affect careers of some, … and then we have Scientology and intelligent design which pretty use similar strategies to deal with critics (laugh – sorry if I get somewhat off topic or into politics).

One thing is that more respected university and insitutes did pay more attention will notice folks that publish only in small journals. I think it is impossible to weed out all the questionable elements in academia. I think many posters and readers of this blog will understand that just by looking at their own university or research insitute , and there always some folks that produce rubbish science who try whatever way to game to system to keep their career alive.

10. **OA_SOC_3=1**
July 12, 2012

Disclaimer: I was not involved in this case, although I know some of the principles.

The judgment by Mrs. Justice Sharp in El-Naschie v Macmillan impressed me as a paradoxical mixture of clarity, indirection, and oh-so-decorous evisceration of the Claimant, Mohamed El Naschie. A minor masterpiece of its genre, in fact.

The judgment is almost 100 pages long and contains much information not previously known to those not directly involved in the case. It constitutes a complete vindication of the public interest reporting by the journal Nature and their senior reporter, Quirin Schiermeier— and implicitly, of John Baez. I raise my glass to the victors!

But according to groups like Sense about Science and Index on Censorship which are working to effect long-overdue reform of the libel laws in the UK, the judgment does little to protect persons who criticize pseudoscience in blogs or other public discussion forums, or academics who debunk pseudo-scientific screeds or who in the public interest draw attention to apparent academic misconduct by their peers. It does nothing to protect "anonymous" referees reporting their scholarly opinion to a journal editor. And it does nothing to help people like Tim Gowers who are working to eliminate Elsevier's practice of forcing academic libraries to purchase subscriptions of dozens of minor low-quality journals in order to obtain a subscription to a major high-quality journal.

The judgment does set a precedent helpful to would-be reformers of UK libel law, but its only immediate effect may be to permit Nature to once again make available the Nov 2008 article by Quirin Schiermeier (which they did within minutes of the decision being handed down).

Because El Naschie has relocated outside the UK, it appears likely that Nature will not be reimbursed for its the legal fees arising from the three-year ordeal, which are said to run into the millions of pounds. The judgment probably does make it unlikely that El Naschie will be able to file further defamation lawsuits in a UK court unless he pays Nature's lawyers. But until the UK reforms its libel laws, other individuals will continue to threaten scientists, journals,and universities with defamation lawsuits arising from scholarly debate and public discussion of controversial issues in the public interest.

During the pre-trial discovery process, Elsevier took the unusual decision to voluntarily share with the defense team internal emails between El Naschie and his "supervisors" at Elsevier, and the judgment includes some excerpts. These snippets show that Elsevier had been concerned about the unusual editorial practices at CS&F for years, and by November 2008 they had in fact been trying for more than a year to remove El Naschie as editor. I had not known that.

In studying the judgment, I found it useful to construct a timeline (far too lengthy to post here) using this document and other sources. After doing this, I felt that I understood for the first time the hysterical tone of the many, many, many suspected sockpuppet posts in various discussion forums which took up the case of El Naschie. One of the Cafe bloggers assured me in Nov 2008 that there was much more to the story than was so far known to the general public, and now I think I know what he meant.

But I am also left with some unanswered questions, and hope that someone here can offer some insights.

I have long wondered why the universities did not band together years ago to sue Elsevier for effectively forcing them to pay thousands annually for such a completely worthless journal as CS&F became under El Naschie (the judgment makes it clear that this is the opinion not just of myself but of Mrs. Justice Sharp). Could the voluntary disclosure by Elsevier been intended to dissuade the universities from just such a lawsuit? By showing that they were in fact attempting to clean up CS&F, despite appearances to the contrary? The reputation of this company in academic circles probably does not make it likely that we will easily accept the more straightforward premise that Elsevier simply wanted to do the right thing.

The internal emails disclosed by Elsevier do however suggest that at least some Elsevier employees were genuinely concerned with quality control and wanted very much to remove El Naschie. The emails show that El Naschie responded to repeated demands for his resignation with (to paraphrase Mrs. Justice Sharp) an odd mixture of praise for the publisher, emotional pleas for mercy, and legal threats. This raises the question of whether Elsevier, which is said to be one of the wealthiest multinational corporations in the world, was actually deterred by legal threats from summarily firing their rogue editor. If so, this is just one more illustration of how current UK libel law makes it utterly impractical for all but a handful of publications (like Nature) or private individuals (like Simon Singh) to fend off even the most baseless of defamation suits. It is necessary to have not only the money but also the will; Elsevier clearly did not lack the first but may have lacked the second.

I learned from the judgment that Elsevier apparently bought CS&F and some other journals from Pergamon, and there are hints that they inherited from Pergamon a contract which was in some way unusual. I wonder whether this may have resulted in some strange legal obstacle bearing on the enduring mystery of why it took them so agonizingly long to fire El Naschie.

Another remaining mystery is the question of why Greiner and some other respectable people seemed to offer El Naschie "honors" which he plainly did not merit. A hint of one possible explanation might lie in an pre-Baez-blog email quoted in the judgment in which El Naschie pleads with Elsevier for a "face-saving" exit.

In the end I found the judgment to be quite depressing, despite the complete vindication of Nature, Schiermeier, and Baez. The reason is that it emerges that El Naschie and his close associates not only succeeded in creating and maintaining for decades a walled garden of pseudoscience under the imprimatur of a "respectable" publisher, but drew several non-scientists into the web of deception. And all for what? The impression I obtained from El Naschie's emails, quoted in the judgment, is that the ultimate motive may have been to support, not with money but with the academic appurtenances of a "respectable man", an elegant lifestyle which included good dining with a circle of loyal admirers of the self-styled "genius".

Are there any lessons to be learned from this shabby affair? I suggest these:

1. Scientific journals must have public quality-control measures in place in order to prevent abuses, which must have teeth, and which must be enforced,
2. Publishers of such journals MUST maintain the financial and legal resources AND THE WILL to remove rogue editors "for cause".
3. Parliament MUST act decisively during this session to reform UK libel law along the lines suggested by organizations like Sense about Science.
4. Congress should enact its own laws preventing US bloggers from threats of libel actions in the UK, Egypt, Turkey, and other nations whose laws fail to address the need for open debate on issues in the public interest, including scholarly debate.
5. Prominent scientists should not set up as the editor of a new journal a young scholar lacking a scientific reputation established upon unquestionably solid research, even if they claim to consider him a "genius". Putting so much power into the hands of someone of unknown scientific aptitude, untried character, and untested judgment is simply unwise, and risks embarrassment for the profession and personal disaster not just for the editor, but for his family and employees.

One minor footnote: there was at least one well-informed comment criticizing El Naschie's so-called "E-infinity" so-called "theory" even earlier than the 2005 thread in this blog
http://www.math.columbia.edu/~woit/wordpress/?p=192
and the 2008 thread in N-Category cafe which resulted in Schiermeier's news story. A 2004 thread in physicsforums.com featured mostly uncritical praise of El Naschie's "E-infinity theory" [sic]— in hindsight, one might say that these posts have a familiar ring. But the thread also included one critical comment from an established poster, Chronos, who drew attention to the 2000 incident in which arXiv editors removed the claim of DAMTP affiliation from a preprint co-authored by El Naschie; see #17 in
http://www.physicsforums.com/showthread.php?t=53682

11. **Peter Woit**
July 12, 2012

OA_SOC_3=1,

Thanks for the excellent summary. To me, the questions about Elsevier are

1. When did they first realize the problem with El Naschie, and why didn't they take action earlier?

2. It's easy to remove an editor who goes willingly, but in this case they had to be willing to act forcefully and go to court if necessary. It seems that for quite a while they were well aware that they were putting out and charging money for a disastrously bad journal, but didn't want to go through the trouble necessary to deal with the situation. They have paid a heavy price of their reputation for this choice.

12. **OA_SOC_3=1**
July 12, 2012

According to my timeline, it appears that some Elsevier employees were very mindful of the damage which El Naschie's antics were causing to their reputation, and by Oct 2006 at the latest, they were trying to have him removed:

18 Oct 2006:
Charon Duermeijer (Elsevier's quality-control person) emails her own supervisor, Martin Tanke, expressing concerns about the strange referee practices at CS&F, and naming 30 Jun 2007 as the "next opportunity" to remove El-N; a three-year contractual cycle, mentioned elsewhere in the judgment, may suggest that a previous effort in June 2003 had failed.

13 Jun 2007:
First letter terminating El-N as editor effective 30 June 2008, signed by several persons including David Clark, Publishing Director at Elsevier

21 Jul 2007:
El-N writes Elsevier complaining about changes to board of CS&F
…
4 Dec 2007:
Elsevier letter to El-N explaining reasons why they insist that El-N retire as editor
…
23 Jan 2008:
Letter from El-N to Elsevier insisting upon condition that something "does not touch my status as Editor-in-Chief"

26 Mar 2008:
Elsevier internal email states ""We are increasingly concerned that lax or inconsistent peer reviewing, plus questionable editorial standards, have severely damaged the reputation of the journal and threaten Elsevier's reputation for upholding scientific standards".
…
19 May 2008:
Email from Elsevier to El-N

21 May 2008:
Email from El-N to Martin Tanke boasting that CS&F was "without a trace of doubt the best journal publisher in the history of the profession"

29 May 2008:
Email from El-N to Martin Tanke pleading ""all I really want is a return to the immaculate relationship which has always prevailed between me and my publisher."

5 Jun 2008:
El-N meets with Martin Tanke, who insists that El-N retire.

6 Jun 2008:
El-N email to Martin Tanke pleads "I want an exit and an honorable one. I am 65 and intended to resign in 3 years anyway… After working for so long…I should not be kicked out like that, and I will not be, come what may… I need a minimum of one year grace period for an exit…"

…
29 Oct 2008:
El-N email to Martin Tanke requesting 4 months grace period

5 Nov 2008:
El-N email to Martin Tanke requesting 4-6 months grace period "so that I do not loose face"

9 Nov 2008:
John Baez, "The Case of M. S. El Naschie", N-Category Cafe

18 Nov 2008:
El-N email to Martin Tanke finally accepting his retirement

26 Nov 2008:
Email from El-N to Martin Tanke

27 Nov 2008:
Quirin Schiermeier, "Self-publishing editor set to retire", Nature

A few more links for the curious:

Permalink to an html version of the judgment, 6 July 2012:
http://www.bailii.org/ew/cases/EWHC/QB/2012/1809.html

The 2000 arXiv eprint coauthored with Carlos Castro, with a note from the arXiv staff saying "The incorrect affiliation of M. S. El Naschie was removed" (the falsely claimed affiliation was "DAMTP, Cambridge University, Cambridge" as you can see from v.2):
http://arxiv.org/abs/hep-th/0008056

13. **Peter Woit**
July 12, 2012

OA_SOC_3=1,

Besides the question of why it took until 2006 for them to be aware of the problem (if it really was that long), the next question is why El Naschie wasn't fired for cause in 2006, with Elsevier instead negotiating with him about his retirement into 2008. It's hard to believe that their contract with him allowed the kind of behavior he was engaged in .

14. **OA_SOC_3=1**
July 12, 2012

Yes, Elsevier certainly should have been well aware by 2000 at the latest that El Naschie was behaving badly:

15 Aug 2000:
arXiv staff remove false claim of DAMTP affiliation from eprint coauthored by El-N; email quoted in judgment indicates El-N thought this was due to a complaint from Michael Green

9 Apr 2001:
Acting on complaints from 5 genuine DAMTP faculty members, Prof Timothy Pedley, Department of Applied Mathematics and Theoretical Physics, DAMTP, Cambridge University, requests El-N to stop claiming academic affiliation with DAMTP
According to judgment, El-N responds with legal threats
…

15. **Mathematician**
July 14, 2012

I read the judgement out of curiosity. I was surprised to learn that El-Naschie chose to represent himself early on? (he had a law firm representing him at the beginning, but then he dropped them it seems?)

16. **Impartial Commenter?**
July 18, 2012

Please allow me to make what in my opinion looks as an impartial comment.
I have been following this case for some time now on both its scientific and non-scientific fronts, and I can add the following points:
1- To be more more accurate, it should be stated here that El Naschie was in fact the founding editor of the journal Chaos, Solitons & Fractals. It was first published by Pergamon then bought by Elsevier. So, he was not appointed by Elsevier as the editor of this journal.
2- The court judgement dealt exculisevely with the non-scientific side of the case. This has been made very clear by the new judge Mrs Justice Sharp, who replaced the first judge Mr Justice Eady for undeclared and, hence, unknown reasons. Mrs Sharp also repeated in her judgement that the scientific aspects of the case were left for judgement by standard scientific debate in standard scientific circles. Also, the "expert witness" to the defendants, Prof Turok, stated it clearly in his witness statement that he is not an expert on the pure scientific aspects of the matter, and that his witness statements dealt only with the other aspects of the matter such as the excessive number of papers published, their refereeing if any, their clarity and completeness, misprints and misnomers in them, etc.
3- I haven't personally seen so far a detailed scientific discussion of the scientific aspects of El Naschie's work published in scientific circles. His work hasn't been so far proved to be partially wrong, completely wrong or "Not Even Wrong". He claims that his work is based on the use of "transfinite sets", "fractal geometry", "Cantor sets", etc and their application to quantum physics in a new way. I know that these concepts are new to physics and have also been controversial in the mathematics community for some time. So, one cannot exclude that there may exist some misunderstanding of his scientific ideas which many see for this reason as pure numerics of no scientific

value. This last point shouldn't be taken as implying that his scientific ideas are correct, but it could also imply that they shouldn't be also decided as completely incorrect unless an impartial scientific debate is done and finalized. It also shouldn't be understood as implying that his editorial behavior, which is another matter that has been judged by the court, is justified. On the contrary, the court judgement may have implied that the misunderstanding of the scientific aspects of his work could be understood as being partially due to his incoherence and other presentation problems in his publications.

In the light of the above remarks, may I call for an impartial pure scientific debate about the scientific ideas of El Naschie to put the scientific aspect of his case to rest, as it hasn't been finalized by the court judgement in my opinion. This would be more in the interest of science than anything else that may have been exclusively the job of the court and was finalized.

Thank you.

17. **Impartial Commenter?**
July 18, 2012

P.S.
OA_SOC_3=1 on
July 12, 2012 at 5:55 pm says:
"… there was at least one well-informed comment criticizing El Naschie's so-called "E-infinity" so-called "theory" even earlier than the 2005 thread in this blog
http://www.math.columbia.edu/~woit/wordpress/?p=192
and the 2008 thread in N-Category cafe which resulted in Schiermeier's news story. …, Chronos, who drew attention … ."

I, as a humble mainstream physicist, have accurately and repeatedly read what has been written in 2005 on the webpage indicated above, and also what has been discussed in the 2008 thread in the N-Category cafe website, and cannot be able to see in them a detailed pure scientific discussion which can qualify as the "standard scientific discussion in standard scientific circles" referred to by Mrs Justice Sharp in the court judgement. This criticism applies more to the comment made earlier in 2004 by commenter, Chronos, on the physicsforums.com website which touched only on El Naschie's possible misuse of affiliations. We know, as scientists, that any scientific research paper is usually, and in most standard cases, refereed by at least three independent impartial experts to qualify for publication in a respectable scientific journal. Then, how can any one be convinced that these type of comments and opinions can be considered as final decisions on the pure scientific aspects of the case? I make this additional comment with the a priori understanding that the court judgement have indicated that some of El Naschie's papers may have not been properly refereed, and with all due respect to the people who have made the comments referred to above and their scientific standards and integrity.
To my knowledge, there are no other places where one can find the required detailed scientific discussion, but I may be wrong.

18. **Peter Woit**
July 18, 2012

"Impartial Commenter?" appears to be an El Naschie sock-puppet, writing from Cairo. Any reading of the court judgment will make clear that his claims are nonsense (see for instance point 120 of the judge's decision). Everyone who writes nonsensical papers about physics making grandiose claims wants to get other scientists to waste a lot of time reading their papers and trying to extract something intelligible from them. In this case, due to the dysfunctional nature of the British legal system, a highly competent physicist, Neil Turok, spent a lot of time on this, and the results are available in the court judgment. I strongly discourage anyone from wasting more of people's time indulging El Naschie (or his sock-puppet) in this regard.

19. **OA_SOC_3=1**
July 21, 2012

Anyone interested in this case who lacks the time to read Justice Sharp's clear but lengthy judgment, cited above, may wish to study instead the able summary by the UK-based legal writer Gervase de Wilde
https://inforrm.wordpress.com/2012/07/18/case-law-el-naschie-v-macmillan-claim-against-scientific-publisher-dismissed-gervase-de-wilde/
He sums up the case by saying: "The Claimant's defeat could hardly have been more comprehensive, with all of the … defenses [by Nature] succeeding and [El Naschie's] own integrity, as a scientist and as an individual, questioned by [Justice Sharp] throughout her judgment. If [El Naschie's] intention was to suppress questioning or criticism of his work then the strategy backfired spectacularly."

The substance of the comments criticizing the conduct of El Naschie and the dubious scientific merits of his CS&F papers, comments which appeared in on-line forums such as a Physics Forums thread in 2004, Peter's comments in this blog in 2005, various discussions of the work of Garrett Lisi which were hijacked by suspected El Naschie socks, the N-Category Cafe post by John Baez in 2008, and dozens more which appeared after the publication of the Nature article by Schiermeier, were found by Justice Sharp to be entirely correct:

1. The criticisms leveled by Baez, and echoed in court by Nature's expert witness, Neil Turok, that El Naschie's papers exhibit "a failure to define terminology and concepts, conclusions unsupported by reasoning, and meaningless, obscure and simply wrong statements", were all found to be justified on the facts.

2. The assessment by Baez and others that these papers lack any discernible scientific merit was found to be a reasonable conclusion.

3. The suspicion expressed by Baez and others, that CS&F appeared not to be following accepted practices of editorial review, was found to be fully justified: Justice Sharp concluded that El Naschie failed to produce any documentation of any CS&F referee reports and that in any case, the papers were of such poor quality that they would never have been published by any journal which did employ adequate review.

4. The opinion expressed by Baez and others that CS&F's high impact factor was the result of manipulations by El Naschie was fully justified.

5. This manipulation, and the publication of the worthless papers of El Naschie without proper review, were found to constitute "an abuse of [El Naschie]'s position" as editor.

6. The opinion expressed by "Chronos", Baez, and others, that the publicly available evidence suggests that El Naschie had made a practice since at least 2000 of falsely claiming several academic affiliations he did not in fact possess, was justified on the facts.

7. The opinion expressed by a half-dozen blog editors and forum moderators (including Peter) that emails making legal threats to Baez, Nature, and others, supposedly written by "legal advisers" to El Naschie, as well as hundreds of posts supporting El Naschie's claims, were made by sock-puppets of a very small number of personal associates of El Naschie, was found to be justified on the facts. Justice Sharp even named, ever so obliquely, the person she concluded was responsible for most of these writings, and added that she considered this serial sock-puppetry the most "bizarre" aspect of the case. Elsewhere in the judgment she dryly noted that if the person whom she concluded had written the legal threats had actually been a lawyer, that person would be subject to severe sanctions for professional misconduct. She even specified a stylometric trait characteristic of the sock-puppet screeds. (Neither Peter nor I want this discussion to degenerate into another Sockfest, so anyone who wants to know more about this aspect of the case should read the judgment.)

While Justice Sharp did attempt to draw a sharp legal distinction between her rather narrow and carefully worded findings and her opinion of El Naschie's scientific status, I believe that the above account is an accurate summary both of her findings and of de Wilde's own summary of her findings, in so far as these bear on the wider issue I want to discuss: institutional failures which can allow someone like El Naschie to engage in serious misconduct for many years.

Near the end of his report, de Wilde says that he feels that the British legal system has proven adequate to the challenge posed by unmerited libel actions such as the one brought by El Naschie, since in this case the defense of truth was upheld. This might appear to be true if one looks only at the final result, but I feel that the enormous legal costs carried by Nature (which they are unlikely to recover from the Claimant) and the three years of anguish to which a reputable and careful science reporter, Quirin Schiermeier, was unjustly subjected, are simply unacceptable. In my view, no editorialist, science reporter, or scholarly blogger should be vulnerable to the credible threat of such wildly unmerited actions, particularly when the costs of mounting a defense generally exceed one million pounds. As de Wilde admits: "The real issue here, and in similar cases, appears to lie not in the law itself but in the cost of litigation. Large media companies with deep pockets can afford the robust approach which leads to them defending claims in the High Court over the course of several years, others cannot."

To be fair, de Wilde's comments occur in the context of the phone hacking scandal, which involves shocking revelations tending to support the view that certain "tabloids" based in the UK have engaged for years in objectionable practices which exceed what most people would regard as responsible journalism. The resulting furore (which has resulted in a dozen official inquiries in the UK) is in my view justifiable, but unfortunately it has tended to distract attention from the merits of the arguments presented by groups such as Sense About Science and Index on Censorship which hope to enact badly needed reforms in UK libel laws. I hope this will not prove to be a case of two laudable reform movements canceling each other out, since I believe that it is possible and urgently necessary to enact provisions strongly discouraging both unmerited libel actions targeting responsible bloggers like John Baez, and improper "investigative methods" targeting relatives of crime victims and other innocents who were allegedly victimized for profit by the Murdoch machine.

To reiterate some points I made earlier: the decision by Justice Sharp constitutes a complete vindication of the criticisms of Baez and the reporting of Schiermeier, and will no doubt be cited by every court which hears a similar case in the future. But while the decision may embolden a handful of major publications like New Scientist and Nature to once again speak out against future cases of uncovered scientific misconduct, it does little to protect whistle-blowers, not even tenured professors like John Baez whom many believe to enjoy a legally recognized privilege of scholarly speech. In fact, it appears that no such privilege is generally recognized in the law, and I think that is a problem. Even those nations whose legal statutes apparently do offer some protection to "scholarly speech", such as the USA, have defined this so narrowly that most bloggers lack legal recognition as professional "journalists" or "scholars". And the notion of free speech generally has endured an unrelenting attack during the past decade. Furthermore, I see nothing in Justice Sharp's decision which might tend to mitigate against the obnoxious practice of "libel tourism", which appears to be used with increasing frequency by persons hoping to suppress scholarly criticism of dubious scientific claims.

John Timmer has just published a provocative (and funny!) essay in Ars Technica, which I found timely and germane:
http://arstechnica.com/science/2012/07/epic-fraud-how-to-succeed-in-science-without-doing-any/
Timmer's essay was prompted by the recent dismissal of a Japanese anesthesiologist, Yoshitaka Fujii, who published hundreds of obscure papers which upon investigation were found to be based upon faked data. See the Wikipedia article
https://secure.wikimedia.org/wikipedia/en/w/index.php?title=Scientific_misconduct&oldid=501219123
for some further examples of widely publicized cases of scientific misconduct.

I think we need to coin a term for the comparatively small number of dishonest persons hiding among the population of honest scientists, who cause damage disproportionate to their number or "intellectual weight". A term denoting those who engage in scholarly misconduct such as unethical experimentation on human subjects, undisclosed conflicts of interest such as corporate sponsorship of dubious "medical research", suppression of genuine data, concoction of fraudulent data, intellectually dishonest statistical manipulation of data, plagiarism, gift and ghost authorship, malicious impersonation of other academics, misrepresentation of academic affiliations, editorial misconduct, state and corporate sponsored trolling, publication of "propaganda journals" and "marketing journals", and other insidious distortions of the scientific process. I propose to name this noxious invasive weed Mendax mendax.

Anyone who has followed the woeful careers of such icons of mendaxity as El Naschie and Fujii has surely noticed numerous features common to the genus Mendax, and Timmer enumerates some of the warning signs. I'll pass over his list in order to discuss another feature common to many scientific scandals: whenever a Mendax infestation is discovered, people often ask "How could this have gone un-noticed for so long?". The answer offered by Timmer is simple: in most cases, it was noticed fairly quickly, but "nobody took any responsibility for investigating the prospect of fraud, despite requests made by other researchers who suspected something was amiss."

To which I would add: when well-founded suspicions are raised, they are often ignored, at least initially, and when they are not, the resulting investigations are often excessively timorous, and if they result in academic sanctions, all too often the charges, the investigation, and the sanctions are all covered up in order to protect the journal, the university, or scientific body which published, employed, or funded the miscreant from legal action. This suggests that the fairly small number of highly publicized cases in which people have been dismissed for cause may represent only the tip of a substantial iceberg. Some observers believe that various kinds of scholarly misconduct are running rampant in China, India, South Asia, and parts of the Near East, and such misconduct may be increasing in North America and the Eurozone, particularly in the lucrative field of medical research, with universities and scientific institutions increasingly distracted by worsening financial problems which in some cases threaten their very existence.

Decisive and timely intervention is necessary to prevent Mendax infestations from getting out of control, but prompt, firm, and public action is rarely taken before serious damage has been done to institutional reputations. All too often, the result has been that an individual miscreant simply moves on to another journal or another university in order to resume his scientific abuses. It seems to me that it should not be difficult to insert into employment contracts, funding grants, and paper submission forms language protecting institutions such as journals, universities, granting agencies, and professional societies, which are responsible for helping to ensure the integrity of scientific publications, from legal threats such as those allegedly made by the (allegedly nonexistent) "legal advisers" of El Naschie when Elsevier attempted to remove him. However, such measures would do nothing to protect whistle-blowers like Baez from retaliation, and legal reforms protecting whistle-blowers generally may be needed to address this point.

Curiously, with the possible exception under some circumstances of improper medical experimentation resulting in death (which according to some physicians happens more often than most people realize), even the most severe scholarly misconduct does not appear to be considered felonious under the law. Possibly it should be.

Timmer adds: "Investigating scientific fraud is a slow and painful process. A false accusation can ruin someone's career, so these investigations tend to be careful, thorough, and done completely in private, in case the accusations are off base". No-one could object to that. The problem is, "nobody really wants to be the one on the hook for running [such an investigation]." This is an inherent human weakness which can and must be addressed by suitable institutional measures which ensure that someone takes ownership of a suspected problem and addresses it effectively, before it gets out of hand.

Scholarly misconduct is an international problem, for reasons which include but are not limited to points touched upon by both Timmer and de Wilde. I applaud everyone who puts themselves at risk by fighting corruption and scholarly misconduct. Particularly in countries like China where the consequences for speaking out can be especially severe.

Can anything positive come of such widely publicized instances of mendaxity as the recently terminated careers of El Naschie and Fujii?

If these cases stir serious discussion inside faculty rooms and professional societies, discussion which results in measures effective in suppressing future cases of scholarly misconduct, then something valuable will have been achieved. If not, all the toil and trouble over the voluminous but worthless writings of El Naschie will remain just what it is today: utterly pointless.

Short Items, Higgs-free

July 7, 2012
Categories: Uncategorized

- The Simons Foundation has some more wonderful interviews with mathematicians. There's one with Pierre Deligne, and another with Robert MacPherson. The MacPherson piece describes not just mathematics, but also the unusual personal and professional collaboration of MacPherson and his student Mark Goresky.
- James Milne has a wonderful article explaining John Tate's mathematical achievements, for the Abel Prize volume.
- Science Watch has an interview of Nima Arkani-Hamed by Gary Taubes, about supersymmetry, with Arkani-Hamed rather defensive on the topic. These Science Watch pieces are built around the researcher's most highly-cited papers. In this case these would be not about supersymmetry, but about extra dimensions, and it would have been interesting to hear discussion of LHC results relevant to those. While extra dimensions at the TeV scale got a lot of attention from 2000 on, the topic disappeared from view once LHC results started arriving.
- Jim Holt's book Why Does the World Exist? was reviewed on this blog here, and is now available. If you're interested in Nothingness, you must read this book.
- OK, I can't resist one Higgs-related item. One explanation for why Gordy Kane's claims to have predicted the Higgs mass from string theory haven't made it into recent media coverage of the Higgs is that not only do physicists not take it seriously (Matt Strassler characterizes this as "garbage and propaganda"), but even string phenomenologists feel "animosity" towards these claims. For more, see this report from a recent string phenomenology conference.

Update: One more. Oisin McGuinness pointed me to a new web-site at the IAS run by Dennis Hejhal, which has various hard-to-find material relevant to Atle Selberg. It includes an unpublished interview of Selberg by Betsy Devine (who is Frank Wilczek's wife).

Update: Yet one more. A couple weeks ago the KITP hosted a talk by Nova's Paula Apsell,their Journalist in Residence, entitled Controversy in Science. She covered the topics of Evolution, Climate Change, and the Multiverse. Go to about 43 minutes into the program for the segment on the multiverse, which dealt with Brian Greene's hour-long program on the subject. David Gross objected strenuously to the program and how it was made, criticizing it for not distinguishing solid science from speculation, being manipulative and not seriously presenting the arguments of opponents. Gross explained that he had been interviewed for four hours for the program, but what went on the air was virtually all Brian's point of view, with only a short bit from him which he felt didn't represent his arguments. Joe Polchinski however thought it was just fine…

Comments

1. **Cliff**
   July 7, 2012

   Gordy Cane's prediction of the Higgs boson mass is a historical fact. How significant that prediction is, and whether he predicted it for the right reasons are debatable. I have left comments making this clear several times on this blog, and given the views you advocate I think you have some obligation to report this fact correctly, even if you dont believe he did it for the right reason. If you want string theorists to be more focused on experimental predictions then why don't you give credit when they do exactly that? Let alone when the predictions are proved *correct*.

   The way you continue to frame this is bordering on dishonesty.

2. **Peter Woit**
   July 7, 2012

   Cliff,

   I'm just reporting the views of Matt Strassler and Kane's fellow string phenomenologists. You can try arguing the case with them, although first you might want to figure out how to spell Kane's name correctly.

   The problem is not that string theorists don't want to use string theory to make legitimate scientific predictions, it's that it's a failed idea so can't possibly be used for this purpose.

3. **Shantanu**
   July 8, 2012

   Cliff , what do you think of Shaposhnikov and Wetterich's claim of 125 Gev Higgs boson
   http://arxiv.org/abs/0912.0208
   that was almost three years ago.

4. **Speculative**
   July 8, 2012

   I think most of this controversy over predictions of the Higgs is quite ridiculous. The 126 GeV Higgs is compatible with many models: MSSM, SM without SUSY, some ST models and apparently an asymptotic safety model. Unfortunately we don't know which one is right yet, but we will soon. Right now, until something new is discovered the only thing we can determine is what models are compatible with results. Kane's idea is compatible with the results, that should be granted, but I'd hesitate to call it a prediction. Same with the asymptotic safety. We need a lot more data. Until then I'm not pinning my hopes on any particular idea.

5. **Peter Woit**
   July 8, 2012

   My mistake for not keeping this completely Higgs-free. But please, no more about this here except in the highly unlikely event that someone has something new and interesting to report about the Kane "prediction".

6. **Chris W.**
   July 8, 2012

   As a complement to the Arkani-Hamed interview, see this talk (on YouTube) he gave last January in Jerusalem. It starts with an extended and illuminating discussion of the point of view motivating much of his current work in which the work of Zvi Bern and others plays a central role.

   (I stopped at ~45 minutes. To be continued…)

7. **Anonymous**
   July 8, 2012

You know, the whole Goresky-MacPherson thing makes me uncomfortable. There are serious ethical problems with having affairs with PhD students. Most universities have explicit policies to this effect — at mine, you could lose tenure for doing so.

My feeling is that they are given a pass for two reasons. First, they are both top-notch mathematicians, and MacPherson especially has built up lots of good will (and rightly so). Second and even more importantly, however, is the homophobia thing. No one wants to openly criticize perhaps the most prominent gay mathematician for his love life (including me — there's a reason I'm anonymous). But I'd like to think that everyone should be held to the same ethical standards, gay or not.

8. **milkshaken**
   July 9, 2012

   anon 9:08: if you listen to MacPherson interview, he says they were not romantically involved while he was Goresky's thesis adviser, it developed later after their separation by distance.

9. **Yatima**
   July 9, 2012

   > Most universities have explicit policies to this effect — at mine, you could lose tenure for doing so.

   But this policy is just written down to prevent and counter abusive behaviour on the part of the supervisor. Once the supervisor (who is supposed to have the better judgement) lowers his guard (so to say) and there is no problem on either side, then that policy should in no case be taken out of toolbag. Indeed, in my book, someone threatening to do so would be in need of some after-hour attitude adjustment.

10. **Anonymous**
    July 9, 2012

    To anon 9:08 I agree with you there… if it happened today. Giving this sort of thing a pass legitimizes this kind of behavior for other professors, for whom it might not work out as well as it did for MacPherson and Goresky. I think many of us have heard of straight versions of that.

    But during the 70s so many people considered all gay relationships to be morally wrong, so you could argue that back then they had no real reason to abide by the rules and mores society had for them. Also it was much harder for gay people to date normally, so for a shy gay mathematician this would be a way to find someone.

    But I do think nowadays it's very different, and the standards people have for straight people should apply.. even if occasionally it works out, more often it wouldn't and it's best just not to go there.

    And to Milkshaken: we'll probably never know when it got romantic between them. They might be hesitant to tell the truth not just because of the professor-student issue but also because they were both married at the time, and may also have other reasons to hide what was going on.

11. **Peter Woit**
    July 9, 2012

    Please, I understand that the way of the internet is to take any interesting story and turn it into an uninteresting debate about one of a small number of hot-button topics that everyone seems to care about (e.g. sex between students and teachers). Enough here though about this particular debate.

12. **Eric Habegger**
    July 9, 2012

    I watched the Black Board Lunch video at KITP on the topic of controversies in science. I found Paula Apsell's defense of the program on multiverses quite disturbing and uninformed. She really came across as misunderstanding what the science community (and herself also) don't know yet. Nobody on either side seemed to bring up the fact that there was a distinct difference between Nova's handling of evolution and global warming and how it handled the subject of multiverses.

    In the case of evolution and global warming there was an unspoken but tacit goal in the Nova programs that the scientific evidence is in. Nova then tried to explain the evidence to the lay public showing why they are true, despite high profile opponents.
    In the case of multiverses, which has no physical evidence, the program attempted to convince the audience, that despite it's lack of physical evidence, it also was a good theory. At one point Ms. Apsell admitted that the dirty little secret was that programs are motivated by the creators interest in the subject. I really see that statement as admitting in the case of multiverses as having the same motivation that creationists and GW deniers have. I wish she understood that.

13. **paddy**
    July 9, 2012

    On the KITP lunch video: I find it disturbing that the speaker by content raises Multiverse theories to the level of evolution of climate science.

14. **Cliff**
    July 9, 2012

    Right, that would be Gordy Kane, apologies. Im just leaving his August 2011 presentation pdf here in my name-link, so hopefully no one will waste time arguing over whether those claims were manufactured the day after the rumor appeared again.

    To Shantanu, I dont find the asymptotic safety scenario a convincing possibility, but as long as someone disagrees then its very good to derive these predictions. On its own it doesn't mean much to me, but the task should always be to account for as many of these implications as we can. If I already was impressed and had a high prior for AS, then I would be much more excited about it, i.e. if it was joining other solid motivations. On the other hand I have very high theoretical priors for string theory, and I dont agree that Kane's scenario is completely "generic", but it is an interesting baseline case. So its interesting to keep that scenario and its other implications in mind. Like for example, in case a stop squark were observed, then if string theory is correct one of his assumptions would have to be violated, and that would have implications for what kinds of scenarios are physically possible. If there is some flaw in his reasoning then I would very much like that case to be made in a paper, so I can assess it. Matt didn't have any arguments to back up what he said other than his unspecified "friends" and their completely mysterious arguments.

    So Peter, if you see that case made in a specific fashion anywhere, by all means post it for me.

15. **Beelzebud**
    July 9, 2012

    paddy: It helps to keep in mind the context of the presentation, that being science in the media. For the layman, the multiverse theory is what is presented to us on television as current physics. If you watch science shows on TV it's presented right along with those other topics.

16. **Tim May**

July 9, 2012

I got my start in physics as a teenager in the 1960s. I read a lot of speculative stuff in "Science News" about tachyons, wormholes, etc. But I also knew this stuff was wildly speculative, and likely not to be either true or of any significance in the likely future.

But I didn't see stuff on tachyons, black holes, etc. on t.v. (I wished I had, I suppose.) I got my speculative fix in SN and my solid, stolid articles in "Scientific American." (SA has since slipped closer to "New Scientist," to my regret.)

And even as I was reading the fluff about time travel and wormholes and tachyons in "Science News," I was struggling as a 17-year-old kid to make sense of Feynman's wonderful Nobel Prize acceptance paper. I Xeroxed a copy at my local library and pored over it again and again.

Today, I tune in to a "Nova" show, expecting to see science, and instead see M. Kaku ranting on about the 10^500 universes that co-exist with us. Baaa! I read the first Brian Greene book (skimmed the second and third ones) and wasn't much impressed.

But, as a result partly of this site, I've looked at a lot more of the stuff from Nima Arkani-Hamed, Ed Witten, and others. AdS/CFT, scattering, the hierarchy problem. Familiar stuff to many of you, but new to me. I particularly enjoyed the 5 lectures N A-H gave at Cornell as the Messenger Lectures.

The math is intriguing. Who'da thunk Grothendieck would appear? (Topos theory is one of my major interests, though not so much from the physics side. Still, what Chris Isham is attempting is interesting.)

I saw recently a quote from Nima saying something like "Whatever we're talking about, it ain't about strings!" (I may've made his "ain't" more colloquial than it was.)

Of course, we may be many orders of magnitude in energy away from testable and confirmable/falsifiable predictions of scattering. But, then, I pretty much thought this was the case with GR/black holes in the mid-70s, and was part of why I moved instead into solid state and went to work for Intel. Turns out I was wrong, that a whole bunch of interesting stuff thatt I'll loosely call "black hole phenomenology" really hit the big time these past couple of decades. Lots of predictions, lots of observations. Wow.

The jump closer toward the Planck scale may take a lot more decades (centuries?).

I thank Peter for the heterodoxy of this site, which has helped to lessen the hype and maybe to help sharpen the arguments on all sides.

17. **Truth**
July 10, 2012

One of many reasons why Woit's opinion on string theory gets ignored by physicists is the following illogical position:

Headline: LHC sees no supersymmetry.
Woit: String theory predicts low energy supersymmetry, so it is falsified!
Headline: String theorists correctly predict mh=125 GeV.
Woit: String theory makes no predictions, so it is not falsifiable!

18. **Jon**
July 10, 2012

Another non-Higgs short item you may have missed: this article in Libération about Grothendieck, and how french mathematicians are actively trying to have five boxes of archives held in Montpellier classified as National Treasure, thereby allowing to bypass Grothendieck's interdiction to have a look at them, and digitize it all.

19. **Peter Woit**
July 10, 2012

Truth,

See my response to Cliff. No one in the field except perhaps Kane himself believes that he has a prediction of the Higgs mass from string theory. I'm not arguing the case here, just reporting this fact. We report, you decide…

20. **Peter Woit**
July 10, 2012

Jon,

Thanks, that's fascinating. Very intriguing to know what is in those documents and whether they'll ever see the light of day.

21. **Truth**
July 10, 2012

Peter, thank you for proving my point. You wish to argue that Kane did not make a real prediction because you believe string theory makes no predictions and is not falsifiable. But when it suits your goal, you switch and say it is falsified, such as the lack of evidence for supersymmetry at LHC. This is 100% contradictory.
So which is it, do you think string theory is falsifiable or not?

22. **Peter Woit**
July 10, 2012

Truth,
Again, I'm not the one claiming here that Kane did not make a string theory prediction. That's Matt Strassler, the participants in the String Phenomenology conference, and many others (e.g. Brian Greene). Go argue with them.

As for SUSY, of course string theory predicts nothing about that either, since you can have any scale of SUSY breaking you want. String theorists over the years have often claimed that "SUSY is a prediction of string theory", so seeing SUSY at the LHC would be evidence for string theory. I confess to pointing out every so often for many, many years that, if this is true, finding no SUSY at the LHC would be evidence against string theory. String theorists however seem to use their own idiosyncratic definition of what a scientific prediction is, one in which if the prediction fails to work, that doesn't count against the theory. Besides the 125 GeV Higgs, Kane also claimed that string theory "predicted" that gluinos were there below a TeV, and would be found in "a few months" after last December. The fact that this prediction didn't work somehow doesn't count against string theory either.

23. **paddy**

July 10, 2012

Save your breath Peter. For objective physicists–whether hep or nonhep–SUSY will remain not a matter of "truth" but of whether experimental observation supports it or not. The rest is midrash.

24. **truth**
July 10, 2012

Peter, why avoid the question? I am not asking you to give me your versions of what you think Kane, or Strassler, or Greene, etc believe. I am asking a straightforward question:
In your view, is string theory falsifiable or not?

25. **Peter Woit**
July 10, 2012

truth,

No string theory is not falsifiable, in any conventional sense of the word, since it makes no conventional scientific predictions.

This has gotten completely off-topic, and I've already wasted far too much of my time in the past arguing about the subtleties of what "falsifiability" means. I'm not going to start on that again here. Unless you've got something new to contribute to the topic of Kane's bogus string theory "prediction", enough already.

Please stop submitting endless tendentious argumentative comments here that have nothing to do with the posting.

26. **Stefan**
July 11, 2012

The Simons' Foundation videos aren't working at all for me… am I alone in this?

27. **theoreticalminimum**
July 11, 2012

Jon & Peter:

Thanks for the bringing the article to our attention, Jon. Peter, I bought Holt's book, and it's a fascinating read so far!

Sorry, my question will be off topic. Does any of you have any idea why they say the following about the use of motives: "Les physiciens qui traquent le boson de Higgs … l'utilisent pour essayer de rapprocher leurs observations et leurs calculs." Does this even make sense?

28. **Peter Woit**
July 11, 2012

theoreticalminimum,

There has been a lot of work recently on the relationship of scattering amplitudes and things like grassmanians and polylogarithms, which also show up in one corner of the mathematics of motives. For some examples of the kind of work going on, see the program of last week's workshop:

http://www.icmat.es/congresos/periods-and-motives/program.html

For an actual explanation of these topics though, you need someone other than me…

29. **David Metzler**
July 11, 2012

Peter, thanks very much for the link to the MacPherson interview. I didn't realize that it would be Robert Bryant doing the interviewing…it was a treat to see two of my favorite people in the world having such an interesting conversation.

30. **Anon**
July 11, 2012

Those who are involved in TV programs presenting their wildly speculative multiverse theories as if they were fact have to take some of the blame, in my opinion, for the difficulty the scientific community has in convincing the public of real scientific facts such as evolution or anthropogenic climate change.

31. **Cliff**
July 11, 2012

There is a much better framing to adopt for considering Kane et al's work than just "Is 125 GeV Higgs a prediction of string theory or not".

All scientific predictions are conditional, and Kane's is no exception. He explicitly makes several assumptions: that supergravity is the first valid field theory description, that SUSY breaking occurs by gravity mediation, that the matter content is only that of the MSSM, and also of course his predictions are advertised as applying only to compactified string theories. It seems pretty obvious to me that this kind of thing can be valuable, assuming none of his reasoning is demonstrated to be invalid, because if any of his predictions are violated that would mean that his whole large class of scenarios is excluded, and then we'd learn that one of these very general assumptions is violated. Its not like this is pie-in-the-sky stuff. The Higgs could have easily turned up outside the region 120-130 GeV, or we also could easily still find a scalar superpartner, either of which would falsify his setup. The dark matter experiments could as well. (the remark about the gluinos isn't right though, see the charts from the presentation or his papers. I would check the wording of your quote.)

If I were the type of person who lost sleep every night because theoretical physics is too disconnected from experimental predictions I would be jumping for joy at this kind of work. And moreover I think its an appropriate example of the kind of predictive work that is possible; to make predictions you don't necessarily have to prove that something is absolutely mathematically certain, its enough to show that a result holds in the overwhelming majority of cases unless you pathologically tune things to avoid it. This really just brings the meaning of "prediction" closer to the one understood in most of science. Theoretical physics is somewhat unique in its expectation for mathematical certainty.

32. **Peter Woit**
July 11, 2012

Cliff,

"the remark about the gluinos isn't right though"
Actually, it is. Kane last December described the time-scale for finding gluinos as "months", see for instance his posting on Lubos Motl's blog. Or, at Nature (see my posting from last December for the link), he had this to say:
"gluinos should be found with masses around a TeV, maybe less, by summer"
It's summer now, time is up. His string theory prediction is wrong, so I guess string theory is wrong.

Your remarks about Kane's "prediction" are pretty much unadulterated hype, with statements like

"This really just brings the meaning of "prediction" closer to the one understood in most of science."

sheer nonsense. No one is saying anything remotely relevant to "mathematical precision" here.

Again, before you keep spreading nonsense about this, I think you should take a minute and think seriously about what it means that one of Kane's friends who has every reason to be sympathetic towards him (Strassler), refers publicly to this particular claim as "garbage and propaganda". This is not me, this is the conventional wisdom among Kane's colleagues.

33. **theoreticalminimum**
July 12, 2012

Thanks for the link, Peter. A friend pointed me to this preprint.

34. **Cliff**
July 12, 2012

Wow, so "Truth" really is right, then. You really cannot decide if you believe string theory is unfalsifiable or falsified.

Assuming you're right, and string theory is falsified because gluinos haven't been detected as of July 2012, can you please point me to the gluino search documents from the two experiments using 8 TeV data? Because I would be extremely interested to see them if you have access to them!

Im not impressed by your managing to locate a single memorable quotation from one non-string theorist. Forgive me for having zero confidence in your assertion about what the string community thinks of this work, despite you inability to identify even one technical argument against it.

Ive checked both of Kane's articles. The Nature one says "Particles such as gluinos — superpartners to gluons, which mediate the strong force — have not yet been searched for explicitly in the decay modes predicted by the string theories, mainly decay to top and bottom quarks. They COULD be found in these modes by the middle of next year." The word "summer" does not appear anywhere on this page. On TRF he says gluinos should be detected at the LHC "in the coming months". Well its still the coming months.

I am amazed you're trying so hard to massage Kane's words to mean something else, and what for? So that the outcome of the 8 TeV run doesn't matter? That's clearly the implication of what you're saying, which is pretty richly ironic.

35. **Peter Woit**
July 12, 2012

Cliff,

The Kane "summer" quote is at
http://www.nature.com/news/live-qa-the-hunt-for-the-higgs-1.9642
at 4:18. We're going to be in the "coming months" for the rest of our lives…

CMS reported 8 TeV SUSY exclusion results at ICHEP. Note that Kane was predicting gluinos not at the summer conferences but "before".

Strassler has worked on string theory. I've given on this blog references to quotes from Brian Greene about this, as well as the report from the string phenomenology conference. How about you find me a single quote from a string theorist not a collaborator of Kane saying he or she agrees that Kane has predicted the Higgs mass from string theory?

36. **Rhys**
July 12, 2012

There have indeed been some 8 TeV analyses released by CMS; see for example the overview talk here (from today). It's far from clear to me what the implications are for the scenario advocated by Kane et al. (the LHC experiments can only produce exclusion plots for certain benchmark models, and I don't think they have looked at the case of very heavy squarks).

In any case, all this tit-for-tat about time scales for discovery and 'falsifiability' is really childish.

Implications of LHC Results

July 16, 2012
Categories: Experimental HEP News

The past few days at CERN there has been a workshop on Implications of LHC results for TeV-scale physics. This is the third in a series of these workshops, which have a goal of evaluating the implications of LHC results for choosing what new HEP facilities to design and fund.

One can argue about the implications, but the LHC results so far are in some sense very simple

- the Higgs has been discovered, with properties consistent with SM predictions, more detailed tests of this consistency to come.
- no evidence has been found for non-SM phenomena. The LHC has produced stringent bounds on extra dimensions and strongly interacting superpartners. The only remaining hope for a strongly interacting superpartner in the current data is for the stop, but evidence against that is accumulating, see for instance here.

The hope that the LHC would see extra dimensions was always quite a stretch, but the idea that it would see strongly interacting superpartners has been conventional wisdom for a very long time. It seems to me that many theorists who have spent the majority of their careers arguing for this conventional wisdom are having trouble admitting what has happened.

For some perspective on this, I recently ran across a 1997 Physics Today essay contest, which asked for submissions that would reflect what a "Search and Discovery" piece from the future might look like. The winner was Gordy Kane's Experimental Evidence for More Dimensions Reported. It's supposed to be from May 2011, and assumes that GUTs and supersymmetry were discovered long ago, even

> fully accepted in 2000 after the discovery at Fermilab of the needed supersymmetric partners.

According to Kane, 2011 would see discovery of extra dimensions at the LHC, through observation of a 950 GeV KK state.

Michael Peskin also submitted something similar to the Physics Today contest, purporting to be an October 2016 Search and Discovery column entitled Do Squark Generations Show Geometry? In Peskin's account, the first superpartner was found at LEP in 1999 when it got up to a center of mass energy of 200 GeV, By 2008 a large number of superpartners had been discovered, with ATLAS reporting precise values for four squark masses. Like Kane, he not only conjectures that by now we'd have a huge, well-tested SUSY phenomenology, but that our decade will see the discovery of extra dimensions, of a sort predicted by string theory. For Peskin, the discovery of extra dimensions comes about in 2016 from an electron-positron linear collider operating at a center of mass energy of 1.7 TeV.

Today Peskin gave a talk entitled Will there be Supersymmetry at the ILC?. He starts off by explaining his motivation as follows:

> One often hears:
>
> "If SUSY is not found at the LHC before the shutdown, then we will know that SUSY will not be found at the ILC."
>
> People attending this workshop know that this is incorrect. I hope that this will be explained clearly in the report to the European Strategy Study.

Despite the negative LHC results, Peskin is still trying to argue that one can expect to find supersymmetry at the ILC (which operates at a much lower center of mass energy than the LHC). He asks the question "Are light SUSY particles excluded at the LHC?" and answers it with:

> I will first give some sociological evidence against this
> statement:
>
> 1. No theorist who believed in SUSY before 2009 has renounced SUSY in the light of the LHC exclusions. (*)
>
> 2. Model builders are still building models with 200 GeV charginos.
>
> (* Gordy Kane might be considered an exception.)

I'm assuming the remark about Kane is a joke…

He goes on to argue that surely at least one strongly interacting superpartner (the gluino) will be found after the long shutdown, when the LHC operates at or near design energy:

> So, when we eventually reach the gluino at LHC 14 TeV, the
> generic jet+MET observables will begin to work and SUSY will be discovered unambiguously.
>
> The light SUSY sector will still be hard to explore at the LHC. We
> will feel lucky that we are already constructing the ILC !

After the long history of LHC SUSY predictions that haven't worked out, I'm not sure how seriously this will be taken as an argument for funding the ILC. There's a draft of a section of the report on the implications of the negative LHC results here.

I suspect that arguments about whether to build the ILC over the next few years will revolve around its capabilities in terms of doing a much better job than the LHC to study the properties of the Higgs. Attempts like Peskin's to argue that it should be built in order to look for supersymmetry are not likely to be taken very seriously by anyone outside the community of those who have been devoting the last few decades to thinking that SUSY is right around the corner, and still are unwilling to give up on this.

Bonus Higgs section: The last couple weeks have seen about a hundred Higgs-related things I could have linked to. For a random sampling, see this interview with Higgs, this from the Daily News and this survey of atrocities.

Bonus culture section: From last night's first episode of the new season of *Breaking Bad*:

> "We're living in a time of string theories and God particles. Feasible, doable, why not?"

Update: Geoff Brumfiel at Nature has some quotes from various theorists, including

- From Joe Lykken:

  > Under the weight of the LHC's hard evidence, SUSY and other beloved theories are feeling the strain. "There's going to be a huge massacre of theoretical ideas in the next couple of years," predicts Joe Lykken, a theoretical physicist at Fermilab in Batavia, Illinois.…

> And hopes of finding extra dimensions that would mysteriously swallow up energy from collisions in the LHC are evaporating faster than the postulated microscopic black holes that also failed to make an appearance. "I was one of the people who pushed the idea of extra dimensions that we could see in our lifetime," says Lykken. "Now that we have data, I'm becoming much more conservative."

- Frank Wilczek is hanging in there:

  > It is too soon to write off SUSY, agrees Frank Wilczek, a physicist at the Massachusetts Institute of Technology in Cambridge who was awarded the Nobel Prize in Physics in 2004 for his work on the standard model. "The last man standing, as far as ambitious ideas beyond the standard model go, is supersymmetry."

- Last year Gordon Kane was predicting SUSY discovery (gluinos) this summer. Now:

  > It will take years' more data to test some of the most promising ideas, says Gordon Kane, a theorist at the University of Michigan, Ann Arbor, and a longtime SUSY champion.

Update: At Berkeley they had an event to explain the implications of the Higgs to the public, which learned that we need to go beyond the "three known multiple universes":

> …the Standard Model Higgs has problems. To fix them, alternatives have been proposed that involve a composite Higgs – one composed of other matter particles – that has extra spatial dimensions beyond our three known multiple universes and something called supersymmetry.

Comments

1. **articles**
   July 16, 2012

   The statements about the articles by Kane and Peskin are mean-spirited. They are not to be compared to Kane's recent "prediction of the Higgs mass at 125 GeV" which rightly deserves the scorn heaped upon it. I recall reading the articles in Physics Today, and it was openly a contest to write science-fiction articles about "what might physics be 50 years hence." (I quote from memory.) So the authors were explicitly encouraged to make up stuff. So they made updates and discoveries. So what? I do not recall if Daniel Kleppner wrote an article about physics 50 years hence. He did write an invited article on something of the kind (in Physics Today), although perhaps not in 1997 and not as part of a contest. If the statements in such articles do not pan out, as the years roll by, what of it? Many people (not all string/SUSY theorists) were anticipating that BSM physics would appear at TeV scales. Perhaps SUSY, perhaps not. To date, new physics has appeared at new decades of energy ~ there has been new physics at scales of eV. MeV, GeV … why not TeV? The actual energy scales do not follow a strict pattern (arithmetic or geometric), but why not new (BSM) physics at TeV scales?

   Mankind has been waiting for the Second Coming of Christ far longer than mankind has been waiting for the appearance of SUSY.

2. **Peter Woit**
   July 16, 2012

   articles,

   I don't think my comments about the Kane/Peskin articles (which were projecting not 50 years into the future, but 13 and 18 respectively) were mean-spirited. Note that I just commented that that these provide "some perspective" on what they were arguing for back in 1997-8. Both of them thought superpartners were likely to appear at LEP or at the Tevatron, and if not there, then were a sure thing for the LHC. You can find many other locations where they made such predictions. Yes, they were less sure about extra dimensions, and that's why they made these the topics of their speculative Physics Today pieces.

   The long history of arguments for SUSY that haven't worked out is highly relevant to evaluating current arguments for SUSY by the same people. In this particular case, I think Peskin has devoted a lot of effort over the years to the SUSY argument for the ILC, with the laudable motivation that the ILC is the only viable idea for putting the US back at the energy frontier in HEP research any time soon. But this has always run up against the argument that one should wait and see what the LHC had to say about whether there really were SUSY particles and what their masses were. Peskin needs to acknowledge that LHC results are in and have blown a huge hole in his SUSY argument for the ILC.

   I think there's probably a good case to be made for some kind of lower energy "Higgs factory" machine, maybe some version of the ILC, maybe something else. Unless something else turns up at the LHC though, this case needs to be made in terms of studying the Higgs. The failure of popular arguments for SUSY at LHC scales is a major implication of LHC results and can't just be ignored. The "OK we didn't see SUSY at the LHC, but we think it has to be there at lower energies at the ILC" argument is just not going to fly. Maybe superpartners will soon appear and save the day for him, but it's much more likely that things will just get worse as SUSY exclusions get stronger. Making a big public case that the reason to build the ILC is SUSY is not going to end well in 2015-6 if that's the time frame for a decision by some government to finance the project as well as when the design energy LHC results come in, finally completely closing the door on TeV-scale SUSY.

3. **articles**
   July 16, 2012

   They were asked to write speculative articles, and the postulated timelines and discoveries in those articles don't mean anything. On the other hand, talks (Peskin at Lepton Photon or whatever) on SUSY at LHC and/or ILC energies are indeed to be rebutted.

4. **future 1**
   July 16, 2012

   Lubos contemplates "ATLAS: a 2.5-sigma stop squark excess".

5. **Peter Woit**
   July 16, 2012

   future 1,

   Besides the "stop excess", in other recent blog posts Lubos has argued that CMS is suppressing evidence for SUSY because of Tommaso Dorigo, and that there is something fraudulent about Barack Obama's Social Security number. I suggest you stick to more reliable sources of information, e.g. the slides of today's talk that I linked to, which give the latest from the ATLAS collaboration about stops. Enough about Lubos, please.

6. **Thomas Larsson**
   July 17, 2012

"Mankind has been waiting for the Second Coming of Christ far longer than mankind has been waiting for the appearance of SUSY."

Are you suggesting that these events are in the same category?

7. **Nex**
July 17, 2012

"Mankind has been waiting for the Second Coming of Christ far longer than mankind has been waiting for the appearance of SUSY."

Well, SCOC had a head start on SUSY, but the difference is bound to become insignificant.

8. **Jess Riedel**
July 17, 2012

Hi Peter,

For what it's worth, I agree with 'articles' that your main post does not make it clear enough that the essay contest was supposed to be fun and speculative sci-fi. (I certainly don't find this mean-spirited, it just gives the wrong impression.) Maybe you could clarify by quoting the description given by Physics Today?

If Peskin et al. have been arguing in a serious setting for the likelihood of SUSY at the Tevatron or the LHC, these talks shouldn't be too hard to find and highlight. As you say, that would be very useful for people deciding whether to listen to them when justifying potential LHC successors.

9. **Peter Woit**
July 17, 2012

Jess,

Here's the description of the contest:

"Physics Today, has announced an essay contest, "Can You Write Physics Tomorrow?" in celebration of the magazine's 50th anniversary in 1998. This is an invitation to write an imagined "Search and Discovery" story about a future discovery, advance in physics, or new technology. "

They ran at the same time a separate request for short humorous pieces. The Kane and Peskin pieces are now quite humorous, but I'm pretty sure that was not the intention of the writers, or of Physics Today in c0mmissioning them.

I'm not claiming that these pieces represent Kane and Peskin's 1997 solid predictions for the future, what they represent is their hopes for the future (experimental study of extra dimensions). In both pieces SUSY is in the background as a given. They didn't think it was worth promoting SUSY as a speculative discovery 15 years in the future, because they thought it was quite possible it would be discovered very soon (the LEP beam energy had just been increased).

To find predictions about SUSY at LEP/Tevatron you have to go back to before they were turned on, late 1980s/early 90s, which takes some effort since this was before everything was easily available on-line. For LHC SUSY predictions, 15 seconds of Googling turns up for instance this from 2007:

http://www.umich.edu/~mctp/SciPrgPgs/events/2007/kanefest/peskin.pdf

which has, in red:

"So the discovery of supersymmetry is not just the hope for this machine; it is the expectation."

I've heard many talks like this over the past decade, often making claims like this one, that the problem with the LHC will be such a complicated pattern of SM violations that it will be hard to disentangle them and figure out all the SUSY particles, their masses and couplings (remember the "LHC Olympics"?).

For those willing to do some searching, I'd be curious if anyone can find any example of a SUSY proponent arguing pre-LHC that maybe the LHC wouldn't see anything, but the ILC would. Personally I think trying to make such a case now doesn't pass the laugh test, but maybe someone can point to such an argument.

10. **David Nataf**
July 17, 2012

I don't really understand the arguing behind the arguments (metarguments) for the LHC successor.

From the perspective of particle physics, an LHC successor should be built regardless of what the LHC finds. Different physical concepts will likely influence what kind of successor, but there should be a successor period. The standard model is not a complete description of nature: it doesn't include dark matter, dark energy, and gravity among other proven concepts. I'm not sure if its consistent with matter's domination over antimatter. It would be irrational, in my opinion, to stop building particle accelerators after the first accelerator that has less success than the prior accelerators, there should be a more objective criteria.

There's a 4-sigma significance for a dark matter annihilation line toward the Galactic center at E ~ 130 GeV, with some arguing its two lines. We will soon know if its real. If it holds up it will in and of itself justify several experiments — from a scientific perspective. I think you would want to be build an ILC just to know if it produces this particle.

Now, with all of that said, it's hard for me to imagine that government civil servants care about any of these arguments. I'm just having a hard time seeing a government bureaucrat turn down the money for an ILC on the basis that supersymmetry wasn't found at the LHC. These scientific megaprojects are built to promote national prestige, like the space shuttle, or the olympics for that matter.

11. **Peter Woit**
July 17, 2012

David,

I agree there should be a successor to the LHC, the question is going to be of what kind. Such arguments need to be scientifically solid though. I think you seriously underestimate the competence of "government bureaucrats" who will have to be sold on such a project and get it funded. Many of them are trained physicists who can easily tell the difference between a good argument and a bogus one.

The possible dark matter signal you mention is a good example. First it has to hold up. Then it could be a good part of a justification for a linear collider, but you need some serious arguments about why it could not be seen in the LHC data, but would be visible at an LC. A crucial question about an LC that your arguments must address is what energy it should run at, which dramatically affects the cost.

12. **successor**
July 17, 2012

Indeed there should be an LHC successor regardless of what the LHC finds. That is to say, a search-and-discovery successor machine. Recall that after the b quark was found (at Fermilab in 1977), it was immediately postulated that there must be a partner top quark. The structure of the SM said so, and the SM by then was widely believed to be correct. But the SM did not give a mass for the top quark. So it was a case of go out and search. PEP, PETRA and TRISTAN were all built to find the top quark. Nature was unkind in that respect, the top quark was beyond the reach of those machines. But PETRA did produce good physics of 3 jet events and the discovery of gluons, also violations of scaling, vindicating predictions from QCD. An LHC successor will simply have to go "out there" and explore.

A Higgs factory is a separate issue, although also a successor to the LHC. Clearly the immediate interest will be to study the properties of the Higgs, although eventually the machine will push to higher energies. In that respect, LEP operated initially as a Z factory, but it did eventually push on to sqrt{s} = 209 GeV.

13. **Peter Woit**
July 17, 2012

successor,

Yes, but the question will be specifically whether the 250-500 GeV/beam ILC design makes sense as a search and discovery machine, after you've already operated the LHC at high luminosity and 7 TeV/beam. Is there really plausible physics that would be invisible at the LHC, but could be discovered at the ILC? Personally I don't think SUSY models cooked up to fit this description qualify.

14. **anon.**
July 17, 2012

If the 130 GeV gamma ray line holds up you would *absolutely* have a case for a linear collider since it's almost airtight that it implies the existence of new charged states and making charge states is what a linear collider does. You could imagine that at the LHC these states would be buried in QCD.

Similarly, if the Higgs really does have an enhanced branching ratio to two photons, you have a no-lose case for a linear collider.

Similarly, if an electron EDM is detected you have a case for a linear collider.

But let's be honest: right now, there is no compelling justification to spend the money on a linear collider. In a year or so that may change.

15. **successor**
July 17, 2012

SUSY has nothing to do with it. CERN had already produced the W and Z at the SppbarS, and for that matter CERN (or Rubbia anyway) had already discovered the top quark at 44 GeV/c^2 and monojets and supersymmetry, but CERN built LEP anyway. LEP produced good physics, but no BSM. In the 1970-80s, Fermilab was building the Tevatron, having already operated the Main Ring for years. Yet the NSF funded the construction of CESR at Cornell, even though CESR could not possibly match the physics reach of the Tevatron. (BTW note that CESR was not a DOE machine.) And then the SCOC (ok, the b quark) was discovered at Fermilab, at precisely the energy range of CESR, and the mission of CESR changed completely, and CESR became a dramatically successful Upsilon spectroscope.

Factories are well and good (nay, excellent), but the concept of a factory implies that the object of study has already been discovered. Search and discovery is something else.

The real problem is ultimately economic, that the machines are simply so expensive to build and operate. So the funding agencies (govts, ultimately) want to see some prospect of a return on investment. And it has to be a short term return. ("right now, there is no compelling justification to spend the money on a linear collider." Indeed.) It is the sheer cost that is the stumbling block. Pure search and discovery is just too difficult a concept to sell, at such a high price tag.

16. **s n d**
July 17, 2012

None of this would be an issue of the successor machine cost 1% (say) of what the actual successor machine is likely to cost. Let us say (very approximately) $10B for a linear collider or muon collider. So suppose instead that the successor machine cost $100M. That is still a lot, but the US could fund such a machine all by itself, without outside contributions. There could be two such machines in the world (or even in the US), say an electron linear collider and a muon collider. (Gloss over the radiation problems, etc.) Credible SUSY models would not matter, although something of the kind would in practice be offered as a justification to build the machines. The machines would be built, and the search for new physics would be pursued. Powerful senators would of course have to be cultivated, but such individuals have existed, in California, Illinois, New York and Texas. They are not an extinct breed. And if BSM physics was found, it would not matter if it was SUSY or not. All that would matter is that it is BSM.

Ultimately, the way forward probably resides in a breakthrough in materials science and technology. If 100T magnets can be mass produced, by industry, at a fraction of the cost of existing LHC magnets, then compact next-generation accelerators can be proposed at reasonable size and cost. (Even protons would emit synchrotron radiation at such energies and small bending radii, but let us say that the heat load in sc magnets can be dealt with. After all, this is all hypothetical …) The stumbling block is simply the cost, not the lack of plausible physics scenarios.

17. **strong field physicist**
July 17, 2012

"The real problem is ultimately economic, that the machines are simply so expensive to build and operate"

The cost of the ILC is about that of 2-3 stealth bombers. Lets see… fundamental knowledge of benefit to all of humanity versus mass murder of villagers… hmm…

or to put it another way… you could have 2 ILC's for the price of the bonuses City of London bankers paid to each other last year from public bailouts

18. **particle bombs**
July 17, 2012

"The cost of the ILC is about that of 2-3 stealth bombers." It doesn't work that way. It never has. Money poured in to science in the years after WW2 very much because nuclear physicists made atomic bombs. There was a ticker tape parade for the nuclear physicists in New York city, they were celebrated as heroes. Bonuses to City of London bankers … I imagine the New York thieves will take umbrage that you chose London over NY as your example.

19. **Peter Woit**
July 17, 2012

All discussions of the cost of HEP research tend to quickly degenerate into the "they cost no more than socially undesirable X" argument, and from there into the idiocy of internet political discussion. Just stop right here.

I will exercise my right as czar here to comment that the comparison to a few bombers makes me think "hey, that's not so much money, it's easily affordable", whereas the comparison to half the City of London bonues makes me think "wow, that's a lot of money.."

20. **martibal**
July 17, 2012

Off topic (Peter please apologize): I know you do not feel like to moderate a discussion on the possible dark matter annihilation line toward the Galactic center at E ~ 130 GeV, but could anybody indicate some links to read about this topic (a kind of "Not Even Wrong" devoted to astrophysics) ?

21. **David Nataf**
July 17, 2012

martibal,

This is the most recent analysis:
http://arxiv.org/abs/1206.1616

This is the discovery paper:
http://arxiv.org/abs/1204.2797

22. **martibal**
July 17, 2012

Thanks David.

23. **King Ray**
July 18, 2012

David, thanks also for the links.

24. **Thomas Larsson**
July 19, 2012

It might be sobering to recall what Wilczek wrote in his Future Summary from 2001:

"Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new R-odd particles. Even in the focus point scenario, there must be several accessible to the LHC."

25. **S. Molnar**
July 19, 2012

The two missing commas in the "three known multiple universes" quotation is faintly amusing, but not as good as the two extra commas in "eats, shoots, and leaves". Still, it provides evidence that a comma conservation law holds in our universe(s).

26. **Peter Woit**
July 19, 2012

S. Molnar,

Two extra commas would help, but I'm not convinced they can be placed so as to turn that particular word salad into something grammatically correct that makes sense.

27. **S. Molnar**
July 19, 2012

Surely the intent was to say "To fix them, alternatives have been proposed that involve a composite Higgs … that has extra spatial dimensions beyond our three known, multiple universes, and something called supersymmetry." Awkward, I grant you, but not total rubbish. I actually thought you knew that and just found the error entertaining. Am I being naive?

28. **Peter Woit**
July 19, 2012

S. Molnar,

It's clear what input the theorists involved provided. I suspect though that the incoherence of the end-result in the article is not due just to typos, but expresses the writer's failed attempt to make sense of what he was hearing (which, yes, is rather entertaining…).

29. **Xezlec**
July 22, 2012

"Despite the negative LHC results, Peskin is still trying to argue that one can expect to find supersymmetry at the ILC (which operates at a much lower center of mass energy than the LHC)."

Wait… are you implying that it's the center-of-mass energy alone that determines what can be discovered, and whether it's a pure electron-positron collider like the ILC or a hadron collider like the LHC doesn't matter much? I'm not a physicist, but that goes against everything I thought I knew about what a "parton distribution function" was and how it mattered.

I'm actually not sure there's a missing comma in that Berkeley quote. Reading your blog, Lubos' blog, and Flip Tanedo's blog, I feel like the three of you must surely live in different universes.

30. **Peter Woit**
July 23, 2012

Xezlec,

You can crudely think of proton-proton colliders as colliding bags of partons together, and a proton as mostly 3 quarks. Then the LHC is providing parton-parton collisions with about 1.3 TeV/parton (although with a very wide energy spread). As you can see from Flip Tanedo's latest posting, Z-primes are getting excluded up to about 2.5 TeV, so this crude estimate is reasonable. In 2015 the LHC energies will nearly double.

The initial ILC design is for 250 GeV + 250 GeV, very much below the 1.3 TeV + 1.3 TeV rough estimate above of what the LHC is doing. The LHC partons are colored, which gives it much higher cross-sections for producing colored particles, but, as the Higgs discovery shows, it also can discover new color-less particles. The main advantage of the ILC is that the fact that the initial state is not strongly interacting makes for a much simpler environment and much lower backgrounds. A Higgs discovery at the ILC would have been much much easier, and for getting detailed info about the Higgs it certainly would be a huge improvement over the LHC.

I'm sure one can come up with SUSY models with no new particle states observable at the LHC, although some that could be seen at the ILC at lower energy, but such things are rather contrived and I think few people will take them seriously. The argument for the ILC has always been that it would be the right tool not for discovery of particles at the energy frontier, but for the detailed study of things first seen much more crudely at the LHC.

Personally I think I'm in the same universe as Flip, but you're right, Lubos is evidence for the existence of another one.

31. **Xezlec**
    July 23, 2012

    Thanks for the explanation.

Two New Experimental Results

July 18, 2012
Categories: Experimental HEP News

Today brings news of two new experimental results, both consistent with the Standard Model:

- At the Higgs Hunting 2012 conference starting today, ATLAS reports results from the WW decay channel for the Higgs. At the July 4 joint announcement, CMS had reported results in this channel, but not ATLAS. Analyses of last year's data had indicated fewer excess events in this channel than expected from a 125 GeV SM Higgs (see here). The 2012 data from CMS and ATLAS now show an excess in this channel of a size quite compatible with an SM Higgs. For more about this, and a nice summary of the latest combined data for various Higgs channels, see viXra log. The gamma-gamma channel Higgs signal is high, the tau-tau channel is low, others close to expected, but deviations from the SM predictions are not especially significant.
- At DarkAttack2012, Columbia's Elena Aprile gave a talk this morning presenting new results from Xenon100. These show no evidence for a dark matter detection and provide the strongest exclusions yet of conjectural high mass WIMPs such as SUSY is supposed to provide. The Xenon100 results now rule out most of the region where pre-LHC CMSSM SUSY model fits showed a dark matter WIMP was supposed to be (see for instance slide 31 here).

Update: There's a press release about the Xenon100 result here.

Update: At Higgs Hunting 2012, two excellent summaries today of the theory implications of the Higgs results, from Matt Strassler and Michael Peskin.

Comments

1. **anonymous**
   July 18, 2012

   For those interested, DarkAttack2012 will be presenting some of the indirect search results (like Fermi LAT) tomorrow as well. Stay tuned.

2. **Yatima**
   July 19, 2012

   Probably the most aesthetic PowerPoint presentation I have seen:

   http://www.itp.uzh.ch/events/darkattack/talks/Evans.pdf

   Comic Sans users take note!

3. **Artie**
   July 19, 2012

   Yatima: really? We must have very different taste. I find Slide 20 particularly vile…

4. **David Nataf**
   July 19, 2012

   Slide 20 has some information on it that is likely to be incorrect (it implies Monoceros is a stream) but other than that how is it vile?

   Though personally I don't think the PP has great style. I think it's bad style to have 100 words per power point slide. I was taught to focus on figures.

   David

5. **Anonyrat**
   July 19, 2012

   I find the font very pleasing, but am unable to find out what the name of the font is, lacking Adobe Acrobat Professional. Using this website, http://www.identifont.com/ , with "Fonts by Appearance" did not help. If anyone knows the name of the font, would appreciate it.

6. **anon**
   July 19, 2012

   It's not powerpoint, it's a keynote presentation, to be precise it's done using the "Showroom" template. The font is Gill Sans.

   The pure text slides could use some highlights… overall I would say it's on the better side of presentations, but nothing extraordinary

7. **Artie**
   July 19, 2012

   David Nataf: "vile" was an exaggeration for comic effect. My objection to Slide 20 is the crudeness (and seemingly random colouring) of the annotations on the figure.

   By contrast, I don't find the slides too text-heavy at all. (If you want to see overloaded slides, look at any presentation by a mathematician…)

8. **Peter Woit**
   July 19, 2012

   slides about indirect searches now up at

   http://www.itp.uzh.ch/events/darkattack/programme.html

   which can be examined by experts for new physics or for excellent fonts.

   I'm no expert, but about the 130 GeV gamma line supposedly in the Fermi LAT data, see claims of such a line discussed here http://www.itp.uzh.ch/events/darkattack/talks/Hektor.pdf

and the Fermi LAT presentation is here

http://www.itp.uzh.ch/events/darkattack/talks/Murgia.pdf

which just says
“Comprehensive Fermi LAT team analysis on line searches based on 4 years of data (Pass 7) ongoing”

9. **Dan**
July 19, 2012

Looking at the presentations Peter links to, I think we have a new winner in the aesthetics stakes, the title slide in particular is quite special.

10. **Anonyrat**
July 21, 2012

So Peskin thinks the ILC is justified because the LHC has ruled out heavier particles; the way forward is high precision measurements of the Higgs?

11. **Peter Woit**
July 21, 2012

anonyrat,

In an earlier posting I wrote about his recent talk making the case that it still made sense to search for SUSY at ILC energies despite negative LHC results. That seems to me kind of implausible. The precision study of the Higgs does seem to be the best argument for a linear collider. Now that the Higgs mass is known, I wonder if the ILC design will be redone to optimize it for studying the Higgs. Perhaps a lower-cost design is possible if one is not trying to go for higher CM energies.

How the Higgs can lead us to the dark universe

July 22, 2012
Categories: This Week's Hype

The media frenzy surrounding the Higgs discovery announcement has on the whole consisted of stories that reasonably accurately deal with the scientific implications. Journalists have for instance by now learned that "string theory predictions" are a good thing to ignore. As usual though, theoretical physicists themselves can be counted on to inject some misleading hype into the press coverage when they get a chance.

Sean Carroll is doing his part, with a new piece at CNN entitled How the Higgs can lead us to the dark universe, which begins:

> The incredible discovery of the Higgs boson will open up new ways of probing the part of the universe that is invisible to our everyday senses: beyond ordinary matter, into the extraordinary world of dark matter.

Since most people just read the title and first paragraph of stories like this, CNN's readers will likely go away believing that Carroll's favorite speculative hypothesis, one which hasn't been working out very well, is the important significance of the Higgs discovery. What he's referring to are "Higgs-portal" models of WIMP dark matter. For examples of some recent papers discussing what the LHC has to say about such models, see here and here. In recent years experimental results have not been kind to these models. Negative recent results from direct detection experiments like Xenon100 haven't helped, nor have negative results from monoject searches at the LHC. The significance of the Higgs discovery for the Higgs-portal to dark matter idea is not that it provides evidence for this, but quite the opposite. Seeing signal sizes in various channels that roughly agree with the SM puts new limits on this kind of idea (because if it were true the branching ratios would be non-SM, as the Higgs had a new and potentially large possible decay channel to dark matter particles). Since one can construct a wide range of possible models of dark matter of this kind, many with behavior indistinguishable from the SM, there's no way to rule them out completely. It's of course possible that detailed future studies of the Higgs will find non-SM branching ratios that give evidence for a coupling to dark matter. My impression though is that most theorists find this rather unlikely, and I'd be curious to know what probability Carroll assigns to the idea that he is promoting. Back in 2008, he gave 15% as the probability for any kind of evidence of dark matter at the LHC, and the negative results about SUSY (which he assigned 60% probability) rule out many of the most popular models with LHC-visible dark matter.

Carroll has a new book coming out about the Higgs in November, The Particle at the End of the Universe: How the Hunt for the Higgs Boson Leads Us to the Edge of a New World. The table of contents and the description of the book here look quite promising, but unfortunately he seems to have decided that the way to market a book about the Higgs story is with the dark matter hype:

> A doorway is opening into the mind boggling, somewhat frightening world of dark matter. We only discovered the electron just over a hundred years ago and considering where that took us—from nuclear energy to quantum computing—the implications of the Higgs discovery hold the potential of changing the world.

I'm somewhat curious to know why dark matter is "frightening". In Carroll's last book the big speculative idea being marketed was the multiverse, it's interesting to see that he's chosen to move away from that particular mania to much more solid physics, although keeping it hype-free seems to be too much to ask.

First out of the gate post-discovery with a book about the Higgs won't be Carroll, but maybe Lisa Randall, with an e-book entitled Higgs Discovery: The Power of Empty Space, which I know nothing about, other than that it's supposed to be available Tuesday. Presumably it's an update of material in her recent Knocking on Heaven's Door, where, like Carroll, she moved away from the highly speculative material about extra dimensions of her first book, Warped Passages.

I have seen an early version of one quite good new book about the Higgs, Jim Baggott's Higgs: The Invention and Discovery of the 'God Particle', which is scheduled to be released August 13 in the UK~~, September 6 in the US~~ and the US. It will come with a foreword by Steven Weinberg, which is already available here.

Update: Over at Resonaances, Jester, who is an expert on this topic, comments:

> Finally, a simple and neat theory of dark matter that annihilates or scatters via a Higgs exchange, the so-called Higgs portal dark matter, is getting disfavored because Higgs would have a large invisible branching fraction, and thus a suppressed rate of visible decays.

Comments

1. **Yatima**
   July 23, 2012

   > dark matter is "frightening"

   You wouldn't be relaxed if a solar-mass neutralino ball were wandering into the Solar System! 😉

2. **higgs+bsm**
   July 23, 2012

   Many books and articles tell about the Higgs and the SM, essentially the search for the Higgs as the culmination of the quest to find all the pieces of the SM. That is well and good, to explain the SM to the lay public. But to do so it is at the same time also to look backwards, at what has already been accomplished, not to look forwards to ask 'what next'? So Sean Carroll has his take, and perhaps Kaku and Kane, etc. have even more outrageous pet ideas to peddle. Consider the hype that was promoted after WW2: everyone assumed that nuclear power was the unquestioned energy source of the future. And indeed millions (of not billions) were spent on nuclear power plants. Now that was real hype. Go to CNN and broadcast an interview "The Higgs is the culmination of a decades-long search to find all the pieces of the SM. Now we have (almost certainly) found it. It is a magnificent accomplishment, and it provides us with no clues at to what comes next, it says nothing about dark matter, or dark energy, and provides no pointers as to where to look for the next level of particle physics." See what happens.

3. **Tammie Sandoval**
   July 23, 2012

   I have a question on dark matter.

   When certain experiments first detected dark matter, an alternative theory was offered: That theory held that dark matter is an illusion. Instead, the experiments are explained if the simple law of gravity is incorrect, at least at long distances.

   Has this alternative theory been falsified?

4. **Peter Woit**
   July 23, 2012

Tammie,

The whole issue of “dark matter” is quite complex, with many alternative sorts of explanations of the phenomenon. I’m no expert, so not competent to moderate a discussion of this large topic here. Look elsewhere for debates about “MOND”, a modification of GR to accomodate the effects ascribed to dark matter. My impression is that such GR modifications both look rather ad hoc and don’t agree with some astrophysical evidence. But you really need to consult an expert…

5. **JR**
   July 23, 2012

   Tammie, This book provides one point of view on your question: http://www.amazon.ca/Reinventing-Gravity-Physicist-Beyond-Einstein/dp/0887622828

   there is a review of it here:
   http://www.eetimes.com/electronics-blogs/engineers-bookshelf-blog/4208974/Book-Review–Reinventing-Gravity-by-John-Moffat

   JR

6. **Will**
   July 23, 2012

   Carroll’s most recent blog post is about the Xenon100 results: http://blogs.discovermagazine.com/cosmicvariance/2012/07/20/dark-matter-still-hiding

   He certainly admits it’s evidence against the “provocative hints” he though previous experiments showed, and although I don’t understand any dark matter physics well enough to say whether this was much of an oversight, he makes no connection to the Higgs or his theories about it’s dark matter interactions.

7. **SpearMarktheSecond**
   July 23, 2012

   Or, maybe the dark matter particle doesn’t couple to the Higgs. I thought one of the components of the neutralino (the Bino?) did exactly that.

8. **David Nataf**
   July 23, 2012

   The Higgs has to couple to dark matter as dark matter particles necessarily have mass. If there’s no coupling of the Higgs to dark matter then it’s not a standard model Higgs.

   Peter, MOND is a modification to newtonian gravity not to GR, hence the name “modified newtonian dynamics”. The relativistic extension of MOND is TeVeS, I don’t know that it’s the unique extension.

9. **truth**
   July 23, 2012

   Daniel Nataf, that is not true.
   The Higgs is part of the Standard Model; it must give mass to the Standard Model particles.
   How the dark matter acquires mass is unknown; that requires an understanding of physics beyond the Standard Model.
   It is conceivable that its mass comes from the Higgs, but quite possible for it not to also – for instance, the axion acquires mass from QCD effects separate from the Higgs.

10. **In Hell's Kitchen (NYC)**
    July 24, 2012

    IMO, Carroll is the quintessential blog scientist…as a sci-fi fan I wouldn’t count anything he writes for more than its entertainment value or its value as a seed idea for a good novel.

11. **Tmark48**
    July 24, 2012

    > higgss+bsm wrote : “It is a magnificent accomplishment, and it provides us with no clues at to what comes next, it says nothing about dark matter, or dark energy, and provides no pointers as to where to look for the next level of particle physics. See what happens.”

    Frankly who cares if the field of HEP comes to a halt (except for high energy physicists that is) ? The LHC by itself will be productive for a good decade even if no new physics is discovered beyond the SM. Is this the end of physics ? Not by a long shot, physics even fundamental or theoretical physics is not reduced to HEP.

    ——

    People that believe this have a very distorted view of physics. Progress in astronomy will continue to be made, LHC or no LHC. As for dark matter, building better detectors in astronomy could provide an answer as to its nature. Why not neutrinos as dark matter ? They are massive and interact very weakly with ordinary matter. Progress in quantum mechanics will continue to be made, wether or not we nail down a consistent theory of quantum gravity if this is even possible at all. The same for many other branches of physics.

    ——

    If the field of HEP becomes stagnant because we don’t have the resources to takle whatever energy level is required to target supersymmetry (assuming it stops being a moving target) then so be it. No need to be delusional or hysteric about it. Invest the hundreds of millions, or billions of € on fields that are not stagnant.
    In 3000 years when we have the technology to probe space at the Planck scale, by all means construct that super duper particle accelerator that will give our descendants insights into quantum gravity and maybe string theory if it isn’t forgotten by then 😉 .

12. **Pravda**
    July 24, 2012

    Peter, is this article as foolish as it seems to me (“me” being a non-scientist)?
    http://www.theregister.co.uk/2012/07/24/what_next_for_higgs/

13. **Peter Woit**
    July 24, 2012

Hell’s Kitchen

I don’t think you’re fair to either Sean or “blog scientists”. There’s a huge range of quality of material put out on blogs from scientists, much of it I think better coverage of science news than you’ll find anywhere else. Yes, most of the best scientists in the world aren’t blogging or letting anyone know what they think, and if you can talk to them privately, they’re your best source of info, but for most people that’s not possible.

As for Sean, in general I think the content on his blog is accurate and of high quality. To the extent that I see a problem with it, it has to do with the way he writes about very speculative ideas that there are excellent reasons to be highly skeptical about. He is presenting himself as the voice of the scientific establishment, but not always making clear the distinction between when he is talking about settled science where most experts agree, and when he is talking about something highly speculative that some prominent people are enthusiastic about, but which most of their colleagues don’t take seriously (e.g. the multiverse). For a good example of the problem, follow the pingback above to a fanatical intelligent design site to see what kind of trouble Sean’s behavior is creating for the cause of serious science.

14. **Peter Woit**
July 24, 2012

Pravda,

The Register is fond of over-the-top British tabloid ways of expressing things, but I don’t think that’s a particulary inaccurate story. One of the main goals of the LHC is to look for SUSY (“mirror universe theory” I suppose) and extra dimensions. They’re just quoting one LHC physicist about how this is what they’re looking for. One can argue with some details, but the gist of the story is kind of right.

Would be more accurate of course to have the main point of the story be “LHC not seeing any SUSY or extra dimensions, as most people always expected”, but that’s a hard story to sell to the press.

15. **Jim Clarage**
July 24, 2012

Peter, thanks for pointing out Baggott’s book, and Weinberg’s review. Reading Weinberg’s review reinforced a confusion I’ve always had on the Higgs mechanism and electroweak symmetry breaking: as he says the mechanism gives masses to force carriers (Ws and Z), but is it an extrapolation to say it gives *all* particles mass (e.g., electron, quark)? In popular writing it seems the LHC Higgs is responsible for all known particle masses.

16. **Peter Woit**
July 24, 2012

Jim,

What I’d call the “Anderson-Higgs” mechanism discovered in 1963-4 gives masses to gauge fields (the W and Z). These mass terms come from the kinetic energy terms for the Higgs, since in gauge theory these use covariant derivatives involving the gauge field.

For fermion fields, things are a bit different, and weren’t discussed in the 63-64 papers. There you get masses from the Yukawa interaction terms coupling the fermion field to the Higgs. I’m not sure who first wrote these down, by 1967 Weinberg certainly did, but maybe someone did this earlier.

As often mentioned, there’s another source of mass: strong coupling in QCD, which provides most of the mass of protons and neutrons.

Simons Investigators

July 26, 2012
Categories: Uncategorized

The Simons Foundation has announced the surprise selection of 7 mathematicians and 9 theoretical physicists as [Simons Investigators](). Those selected will get \$100,000/year for 5 years, renewable for another 5, their departments \$10,000/year, their institutions \$22,000/year.

According to a [Washington Post story](), this is just the beginning of the program, which will continue to make these $1 million no-strings-attached awards to prominent mathematicians, theoretical physicists and theoretical computers scientists every one to two years.

This isn't something you can apply for, the Simons Foundation has a panel which made the selections. These awards are being compared to the [MacArthur Foundation "Genius Grants"](), which provide the same unrestricted \$100,000/year size grants, but only for five years. When the MacArthur program started back in the early eighties, particle theorists and mathematicians were often chosen (Witten and Wilczek were among the earliest choices), but in recent years that has been very uncommon. Two of the seven mathematicians chosen (Terry Tao and Horng-Tzer Yau) were also MacArthur Fellows.

The goal of the Simons program is to provde "a stable base of support for outstanding scientists, enabling them to undertake long-term study of fundamental questions." I guess this means the idea is to make it possible for them to work on longer-term more ambitious projects without worrying about the NSF cutting off their grants. It's interesting that the Simons Foundation sees this as a problem to be addressed, given that these are about the most prominent people in math and theoretical physics, among those least likely to ever have a grant application turned down.

In other Simons Foundation news, Yuri Tschinkel, an algebraic geometry from NYU, will take over from David Eisenbud as Director of the Division for Mathematics and Physical Sciences. Eisenbud is returning to MSRI in Berkeley for a second stint as Director there.

Comments

1. **Matthew 13:12**
   July 26, 2012

   For whoever has, to him more shall be given, and he will have an abundance; but whoever does not have, even what he has shall be taken away from him.

2. **mark callaghan**
   July 26, 2012

   I like the idea in principle, but surely Terry Tao couldn't possibly get any more productive,as wonderful as that would be, and i'm sure the same goes for the other recipients.To be charitable, so to speak, i think they have missed the target.

3. **theoreticalminimum**
   July 26, 2012

   I think the "big names" were chosen to give some legitimacy and credit to the money awarded (money is not a big deal for Simons after all). I am happy to see Chris Hirata on that list. He's a fantastic young researcher! He's also recently won a Presidential Early Career Award.

4. **Michał Kotowski**
   July 26, 2012

   I guess it's alright to give such awards to big shots if the money is in turn used to support postdocs, graduate students etc. – ultimately the younger scientists benefit from such an arrangement. But it's a good point that "the Matthew effect" may be dangerous and it is likely to increase (instead of bridging) the gap between the very top places and other institutions.

5. **Christopher Long**
   July 27, 2012

   I agree, all deserving people, but it's the piling of resources on people that already have everything they need to be productive.

6. **Bugsy**
   July 27, 2012

   One of the problems afflicting the world of today is the "star system" whereby, say in professional sports, a few people are paid millions (those are the ones we hear about); 10 times as many are paid 10 times less, and so on, until the bottom rung of pro players are paid barely enough to survive. Now it can be argued that a star is worth it because he/she is making the goals/solving famous open problems. But they are standing on the shoulders of so many others, and couldn't shine as brightly without that framework. So it is vital to keep that framework alive and happy. This is a parallel to the income inequality discussion of the 1% or .01% versus the rest of us, where one of the central points is that a truly healthy society needs a large and healthy middle class.

   In academics we are much luckier than athletes or jazz musicians in that there is at least some basic framework of support; however in many places in recent times that system has been seriously damaged, so that many very deserving individuals (sometimes even those who have done key work in a given area) struggle to find a reasonable job.

   Sometimes there is an ultimate happy success story, but perhaps more often enthusiasm turns into disappointment or bitterness over time, as the realization slowly dawns that certain dreams will likely never be realized.

   I wish granting foundations could find an effective way to contribute to the overall health of the scientific/academic enterprise in this sense. One possibility might be to give many smaller awards, say for great papers by people without already all the prizes and benefits of the Ronaldinhos and Beckhams…

   The CNRS system in France gave one way of doing that, but perhaps the framwork of permanent research positions could be replaced by say a rotating 5-year grant system coupled with permanent jobs (a sort of post-postdoc system).

   What do people basically need? A short answer is simply more money, whether to buy books or a house with or to pay for a child's education. But beyond that there is a huge need for serious time off from teaching obligations to do research. Of course it might be difficult to get institutions to go along with that: there would definitely have to be some sweetening of the pot for the employers.

It is easy to think that the “best” have already floated to the top but undeniably, those who have had large advantages of location, salary, better students, grants, and reduced teaching loads have had all the conditions which foster more productivity, while the opposite has the opposite effect (maybe Grothendieck after leaving IHES is an extreme example) so there are feedback loops at play, and such a grant system might break some of the negative downward spirals and contribute greatly to the overall health and well-being of the fields involved.

Short Items

July 26, 2012
Categories: Multiverse Mania, Uncategorized

A few short items:

- To compare and contrast to the activities of the Simons Foundation, there's the Templeton Foundation, which has a $1.7 billion or so endowment to spend:

  They have a new Big Questions Online site, which asks Does Quantum Physics Make it Easier to Believe in God?

  At Oxford and Cambridge, Templeton is putting $1 million into Establishing the Philosophy of Cosmology, by, among other things, having a conference in January on Is God Explanatory?

  > This miniseries will explore the theological and, by extension, metaphysical questions that pertain to cosmology. The origin and order of the cosmos have helped inspire belief in a "Supreme Being" or "First Cause" for millennia; but what bearing, if any, does the modern scientific approach to studying cosmology have on such beliefs? Does introducing God into the discussion add anything?

  This week this Foundation is funding a Workshop on Philosophy and Physics in Tuscany, with blogging from Sean Carroll and here.

  An apt quotation from Carroll a few years ago would be this one:

  > The problem with the Templeton Foundation is not that they coerce scientists into repudiating their beliefs through the promise of piles of cash; it's that, by providing easy money to promote certain kinds of discussions, those discussions begin to seem more prominent and important than they really are.

- Sometimes you'll see trackbacks in the comment section to Intelligent Design blogs which have become my fans since I'm critical of the multiverse. Often these get identified by the spam filter as spam, but when they get through I tend to leave them, partly because I've had my own problems with trackback censorship (see here), partly because they provide some insight into how the Intelligent Design people are using multiverse mania for their own ends. From one of these links I learned about a recent article in the Skeptic Society newsletter by Michael Schermer. At some point I wrote to him to warn him that claims of scientific testability for the multiverse were bogus, so he should consider avoiding this as an argument with IDers. After some e-mails back and forth it wasn't clear if I had made any headway with him. The new article shows that he hasn't given up on this, but maybe my arguments had some effect.
- For an update on the sad story of Paul Frampton, who earlier this year was the victim of a scam that ended putting him in jail in Argentina on drug-smuggling charges, see here, here, here, here and here. A website to provide support for him has been set up here. It includes letters of support from various people including Edward Witten.

Update: There's an article in the Telegraph about the Frampton story here.

Comments

1. **God**
   July 26, 2012

   QM doesn't make it easier to believe in me, but it does prove that I have a sense of humor.

2. **Hansi**
   July 26, 2012

   Peter, why do you not comment on strings 2012 which ends tomorrow?: http://strings2012.mpp.mpg.de/

3. **Peter Woit**
   July 26, 2012

   Hansi,

   I'm curious to see what the concluding talks there will have to say, probably will write something after those appear.

4. **imho**
   July 27, 2012

   "…Williams said Frampton thought he was meeting a young woman from the Internet, but instead an agent asked him to take a suitcase to the United States…"

   Why should we support him? How do we know his intentions? Why do we trust him over the Argentinian authorities?

5. **Anonyrat**
   July 27, 2012

   Obviously, you support Frampton only if you find him credible.

6. **quotes**
   July 27, 2012

   Today you cite an 'apt quote' from Sean Carroll, but a few posts ago you criticized the same for advertising the Higgs (on a CNN interview) as a `portal to dark matter' (or something like that). Unlike God, Carroll is only human (ok, I don't know that for a fact) … it really wasn't so bad, what Carroll said about the Higgs. Realize that it will be the same with many other people. Prof Sir Martin Rees peddles the Multiverse these days, I think. Perhaps his lordship will speak at the conf in Jan? What then?

7. **Peter Woit**
   July 27, 2012

   quotes,
   I don't understand your point. I agree with most of what Sean Carroll or Lord Rees has to say, disagree with them on certain points (promoting Higgs portals or the multiverse).

8. **Name Here**
   July 27, 2012

   Peter, Nicolai concludes about non-string quantum gravity efforts: "Confirmation or refutation by experiment/observation even more of a challenge than for string theory!" (last page of his .pdf).

   What do you think? And does this mean quantum gravity research is not science?

9. **Peter Woit**
   July 28, 2012

   Name Here,

   Seems to me Nicolai is arguing "well, as an idea about unification, or as something that can be connected to experiment, as a theory of quantum gravity string theory sucks less than the alternatives". That's kind of the main argument for string unification right now: "OK, it's a failure, but so is everything else". May be accurate, but not necessarily a good argument for pursuing the idea.

10. **Hansi**
    July 28, 2012

    @Name Here:
    Of course, especially in gravity, there are many things which are solid science even if they can not be experimentally verified. For example, the Hawking Penrose singularity theorems are certainly not something that can be experimentally verified. But they can be proven from general relativity. Similarly, research on quantum processes in gravity is most solid, when it is solely based on mathematical proofs. For example, Hawking radiation is derived from curved spacetime geometry and quantum field theory. If a theory of quantum gravity could be similarly derived, it would be very very strong science. Unfortunately, no one has found a way for such a derivation yet. From the phenomenological viewpoint, quantum gravity is always problematic, as it will always be difficult, if not impossible to test such a theory. In practice, it can be seen as a relative success in this field, if one comes up with something, that does not disagree with known physics.

11. **BH**
    August 5, 2012

    Looks like John Templeton Jr. has been secretly bankrolling anti-Obama evangelical political organizations. The story is in today's (8/5) Huffington Post.

12. **Peter Woit**
    August 5, 2012

    For those who want to follow up what BH is talking about, the reference is to

    http://www.huffingtonpost.com/2012/08/05/mega-donors-bankrolling-religious-right_n_1735337.html

    which mentions the funding from John Templeton Jr. of "Let Freedom Ring", a right-wing political organization. I don't know that there's any way of finding out the current size of his financial contributions to this.

    For discussion of this topic though, please use the Huffington Post….

Strings 2012

July 28, 2012
Categories: Strings 2XXX

The big yearly string theory conference was held this year in Munich over the past week. Strings 2012 was the latest in a series of conferences that started more than 20 years ago. I've now written something about so many of these things that I've added a category for them, so you can review the last eight years of the history of these conferences by clicking here.

This year the conference drew 385 participants, a bit lower than the 400-500 that showed up at many of these things when held in Europe in the past, but higher than last year's 259 (conference was quite expensive) or 2010's 193 (conference was in the middle of nowhere in Texas, off-season). The week before Strings 2012 there was String-Math 2012, which brought nearly 200 mathematicians and physicists to Bonn. This is the second in a series, which seems intended to supplement or rival Strings 2XXX, with plans already in place for String-Math 2013 (Stony Brook) and String-Math 2014 (Alberta). Unfortunately the String-Math talks have not been posted yet, although I hear there are plans to do this.

One important aspect of Strings 2XXX conferences in recent years has been their role as PR events designed to promote string theory to the public and the media, and fight the perception of a failed subject. This year a press conference was scheduled last Tuesday, but there seems to be no publicly available record of it. About the only Strings 2012 story in the press that a quick search turned up was this one, which had nothing from the press conference, but Thomas Grimm explaining how everything is fine with string theory and maybe the LHC will find extra dimensions.

Another part of the PR activity at Strings 2XXX is promotional talks for the public, which this year included one from Witten about String Theory and the Universe. In honor of the Higgs discovery Witten said that he would add material at the beginning of the talk about particles rather than strings. He is still holding out hope for SUSY at the LHC, although now down-playing the fine-tuning argument and pointing to split supersymmetry as the thing to hope for, with answers to come "within a few years".

The question session was unusually skeptical and challenging, beginning with a very hostile and long-winded question about whether he wasn't worried that he had led physics down a 30-year path of failure. Unfortunately the questioner was intent on making a hostile speech, and much time was wasted trying to get him to shut up so that Witten could address the question. His answer was basically that 30 years wasn't so long, the Higgs discovery had taken 50, and he gave other such examples. I don't think any of his examples addressed the real issue, which is not that practical tests of string theory are far away, but that it makes no predictions, even if you had the technology to test it. To defend the falsifiability of string theory he gave the dubious argument that if table-top experiments showed quantum mechanics to be wrong, that would show string theory was wrong.

Mathematician Michael Hutchings was there, and he blogs about the public talks here, including a description of the question period:

> The most interesting part was the question period afterwards. The first questioner launched into a very aggressive rant about how Witten was abusing his scientific responsibility by leading thousands of people to waste their intelligence on a theory for which there is no experimental evidence. The chairman basically needed to shut him up (and should have done so earlier)….
>
> Anyway I was kind of shocked to see such an agressive attack from the general public. I'm glad I don't have questioners attacking me because my work does not have enough real-world applications or whatever.

This was the first Strings 2XXX post-conclusive LHC evidence ruling out the discovery of SUSY in the form expected from arguments about "naturalness" and the "hierarchy problem". Even the talks that tried to make some contact with the real world mostly ignored the SUSY problem, but the talk of Savas Dimopoulos on What Has the LHC Done to Theory? did address this head-on. On "naturalness" he quoted Samuel Beckett ("I'd wait till it was black night before I gave up"), arguing that one should hang in there with this until the bitter end, which he saw as coming late this year or early next year after the data from the 2012 run is analyzed. His basic point of view was that there are only two choices: versions of SUSY that solve the fine-tuning or naturalness problem (which are about to be ruled out), and versions of SUSY that don't (e.g. split SUSY), which imply the multiverse to deal with fine-tuning. The only other option discussed was "high-scale SUSY" (SUSY broken up near the Planck scale). I guess the concept of SUSY extensions of the SM just being wrong is not within the realm of conceivability, given that they are part of the standard ideology of how to connect string theory with particle physics.

The slides from the talks are available here. I didn't notice anything really new, just much the same topics as have been popular in recent years (e.g. adS/CFT and connections to condensed matter, amplitudes, higher spins). At Strings 2011 there was a lot of comment that few of the talks involved strings and string phenomenologists were shut out. This year's conference had more stringy talks, as well as some on string phenomenology, possibly because it was organized by Dieter Lüst and his group in Munich, which does string phenomenology. Only one multiverse talk, Ben Freivogel on Predictions from Eternal Inflation, which, not surprisingly, had no predictions (but he did ask anyone who had one to get in touch with him, since his future employment would require some).

Hiroshi Ooguri's summary talk reviewed his summary talks from 2004 and 2008, which featured many of the same topics and much the same story. The LHC results were completely ignored, and one of his slides seemed to me just delusional:

> Significant progress has been made in understanding how to derive the Standard Model of Particle Physics from Superstring Theory.

Claims of such progress have been made at every one of these conferences for more than 20 years, with actual string theory predictions getting farther and farther away. There's a reasonable case to be made for continuing interest in string theory, but I find it hard to believe that even many string theorists seriously believe there has been progress in recent years towards using string unification to predict anything.

The one talk that hasn't yet been posted is David Gross's Outlook and Vision. He has given such talks at a large fraction of these conferences, so one knows pretty much what to expect. I do wonder though if he'll address the negative LHC results about SUSY, which at some point are going to cost him money, since he has made bets on this.

Update: I suppose I should ignore Lubos, but his reaction to the questions at Witten's talk is pretty amazing. It seems that they are somehow all my fault (and Lee Smolin's), and he gets into the spirit of Munich of a bygone era with his "endorsement of the creation of gas chambers for this scum" . The suggested way for Strings 2XXX conferences of the future to deal with this problem is to have all questions submitted in advance to make sure there aren't any ones like this year's.

Update: Video of David Gross's "Outlook and Vision talk" is now available. It struck me as much more defensive and hype-ridden than versions of this from past years. We're told that there is "every reason to be optimistic" that the LHC will discover how forces unify and how things fit into the string framework, with the standard arguments for LHC-scale SUSY given, no mention of the negative experimental results. About string theory, Gross claims "unbelievable progress every year", and it includes everything that is "nice" and "consistent" about fundamental physics, including all consistent QFTs.

He echoes Witten's argument that string theory is falsifiable since testing quantum mechanics tests string theory with his own claim that evidence for the SM is evidence for string theory (the SM is the "foundation" of string theory). He describes the press conference held last Tuesday as involving a lot of journalists complaining about the lack of testability of string theory, and Maldacena coming up with the argument that the LHC has successfully tested string theory since it hasn't found anything incompatible with it.

The one substantive remark was that he thinks work on higher spin symmetries may provide a hint about what he sees as the fundamental problem with string theory: no one knows what the theory is, or what symmetry principle it should be built upon.

Comments

1. **Hansi**
   July 28, 2012

   I would find it a bit more interesting (and funny) to know what Lubos has to say on the talks which say that the MSSM would perhaps be ruled out by the LHC in the end of 2012.

   What does Lubos want to do with lecturers writing such plasphemic things, and what does Lubos want do with the LHC after it has ruled out the MSSM?

2. **Yatima**
   July 28, 2012

   Cough. Peter, that's not Munich, that's Wannsee. Officious bureaucrats and psychopaths setting up a "social improvement" scheme for the good of everyone.

3. **Klaus**
   July 28, 2012

   It is hard to recognize a person from his voice only, but the first questioner might be a known German physicist who wrote a book which is so critical and nasty about string theory that compared to it, "Not Even Wrong" is a laudatio of the string idea. He appears regularly in the media.

4. **Peter Woit**
   July 29, 2012

   Hansi,

   It's not the MSSM that is getting ruled out, it's the idea that supersymmetry "stabilizes" the weak scale, that it provides a "natural" way to avoid the "fine-tuning" problem introduced by the Higgs. That whole set of ideas is more general than the MSSM, and has always been given as one of the main motivations for SUSY. "Unnatural" versions of SUSY like split SUSY are still versions of the MSSM.

5. **Alexander Unzicker**
   July 29, 2012

   To avoid too many interpretations without the facts, here is what I actualy said after Witten's public lecture:

   This was an impressive talk. I am sure many people are impressed. I am also sure that with your work you are making the best out of your extraordinary capabilities.

   I am not quite sure however that you are fully aware of the responsibility towards science, the search for the laws of nature…. It was Isaac Newton who said Nature created everything by number, weight and measure. Thus it is the theorist's business to predict numbers the experimentalit's business to measure these numbers. And as everybody can see supersymmetry and string theory in the past thirty years did not devliver a single result which Isaac Newton had called physics.

   So, aren't you afraid of being the scientific leader of an entire epoch of physics that might lead to nowhere? Aren't you afraid of misguiding the concentrated intelligence of seven billion people on a planet? (chairman tries to interrupt)

   Of course, nobody can prove you wrong, but it's the history of science which gives a clear indication: the real revolutions of physics have always been pushed forward by skeptic individuals, never by the euphoria of the many. And this is where your problem lies.

   Your risk is that you are playing mathematical games of with their link to reality is still a promise, concepts of which the link to the physical realiy you do not understand. Thus I'd like to urge you to reflect upon your role in science. Please, get back to reality.

6. **Hansi**
   July 29, 2012

   Peter, maybe this is hairsplitting, but I thought MSSM means "Minimal Supersymmetric SM". The name "minimal" would imply, I think, that split Supersymmetry is distinct from the MSSM. This naming convention is also the one in wikipedia:

   http://en.wikipedia.org/wiki/Split_supersymmetry

   "Split supersymmetry makes predictions that are distinct from […] the Minimal Supersymmetric Standard Model "

   To me, this split susy model looks more a bit like a joke than a serious proposal anyway.

   And regarding to these "attacking questions":

   Well, germans are often perceived not to be as "friendly" as people in other countries. For example, one almost can not! expect friendly questions in such a discussion in germany. I have seen Luest being asked similar questions (although shorter of course, and not only for self-distinguishing) by experimental physicists, when Luest hold an introductionary lecture at the technical university in munich.

   There are some experimentalists, especially experimental solid state physicists, who hold the viewpoint that mathematical physics (that is, to use physical ideas for doing mathematics) is not useful at all, and everything that does not lead immediately to an experiment should be abandoned. So, there is some hostility in germany against theorists. Especially against mathematical physicists.

   Historically, this may have to do with the early influence of Phillip Lennard and others in the 1930. They criticised the theory of relativity as "against common sense". Because of Lennard and others, the official Nazi doctrine, to forbid those new theoretical ideas as "jewish physics" had a fruitful basis in german academia.

   Then, after 1945, most theorists were gone, and one had to staff the majority of physics chairs with experimentalists. In many physics departments, the experimental physicists successfully block the installation of new chairs for theoretical physics since those early days. As a result, the rare theoretical physicists are often watched with suspicion and hostillity in germany. In many of germany's physics departments, experimentalists are quite fast to raise the question, whether a theorist uses money that could be better invested for some new experiment. And if a german theoretical physics professor retires, experimentalits often try to convert the chair into an experimental one. That is the situation in germany.

That some persons in the general public have, like some of the more practically minded experimantal colleagues, very hostile views against mathematical physics, this is not that surprising, unfortunately.

7. **Hansi**
July 29, 2012

@Mr Unzicker:
Physics does not only predict numbers. As physics uses mathematical modelling, it contains mathematical proofs. Mathematical physics is a scientific subject which takes physical ideas and does mathematics with it. This is most of what string theorists do. That is especially true for the research of Edward Witten, who won a fields medal in mathematics.

By writing "you are playing mathematical games" you are saying that you, Mr Unzicker, do not like mathematics.

Without doubt mathematics and mathematical physics is a wonderful science that proves interesting things. It is not just "some stupid game".

For this reason, Mr. Unzicker, nobody in the universe cares about whether you do not like mathematics. This is your own problem and so you should simply refrain from telling mathematicians, fields medal winners, and mathematical physicists that they should abandon mathematics.

Also, as you seem to have problems to understand those mathematical Ideas, it seems it is you, Mr Unzicker, and not Witten who should get a grasp of reality. The reason is that reality obviously contains mathematics.

8. **vmarko**
July 29, 2012

@Hansi:

Doing research in mathematics is not the problem. Selling this research as something that has physically observable predictions is the problem. String theory just doesn't have any predictions. Spending 30 years researching this is ok when done by a moderate amount of people, but recruiting almost 90% of young and knowledgeable people to do this research is just unproportional to the possibility of string theory delivering those promised predictions. And Ed Witten, as an important figure in both physics and mathematics, a Fields medalist, helped in this recruitment.

I believe Mr. Unzicker was just asking Witten to acknowledge his share of responsibility for this. Some others should too.

9. **P**
July 29, 2012

"I don't think any of his examples addressed the real issue, which is not that practical tests of string theory are far away, but that it makes no predictions, even if you had the technology to test it."

It's fine that you're once again stating what you think the real issue is, Peter – and one can have a hearty debate about the ability to distinguish between string vacua and effective field theories at the TeV scale- but you've clearly spoken too strongly here, with the assertion that it makes not predictions even if one had the technology to test it.

This is manifestly NOT true. ALL four dimensional string compactifications have a compactification scale and one will start exciting KK modes at energies above that scale. Moreover, probing with energies near the string scale one can begin exciting the infinite tower of string modes and these have observational signatures that have been studied in detail. They are also generic, as all string theories have them.

So if one had the technology to access these scales, one would definitively be testing string theory.

10. **Hansi**
July 29, 2012

vmarko wrote:
but recruiting almost 90% of young and knowledgeable people to do this research
end quote.

In germany, i bet around 70-80% of the physicists go into experimental physics, perhaps 10-15% in theoretical solid state physics, quantum physics and statistical physics. High energy theorists make in germany perhaps around 5% of all physicists. And they are mostly phenomenologists. Only a very very small fraction of the theoretical physicists in germany goes into relativity or quantum gravity research. The german chairs that are occupied with gravity or some form of quantum gravity or string theory can be counted with your fingers. And one must see this compared to dozends of solid state physics chairs.

And no, the string theorists in germany do not sell anything. Selling is something that german professors traditionally do not do. However, as this Unzicker guy adressed this to Ed Witten: Well, Witten indeed has not much string phenomenology papers. However, Witten has articles on the geometric langlands program. Everyone who reads Witten's papers immediately sees Witten's research his is mostly of (very high) mathematical value, and not something that could be used for constructing a measurement device. Arguing, like Unzicker, that this kind of research would be "only a game" and therefore of no value is just stupid. Nobody cares if Unzicker does not like mathematics.

11. **vmarko**
July 29, 2012

@P:

So if one had the technology to access these scales, one would definitively be testing string theory.

I think that Peter is not talking about testing one particular variant of string theory, but about the fact that there will always exist *some* version of ST that will be compatible with any experimental data at any given energy scale.

The problem is not in string theory having predictions, but in the fact that there are as many string theories as there are vacuua in the landscape.

String theory is not a single theory, but a framework for theories. That is why it has no predictions. The similar situation is with QFT — it is a generic framework, and becomes a concrete theory only after one writes down some particular Langrangian (like QED, SM, MSSM, or otherwise). Only then one can give falsifiable predictions.

So first go ahead and choose one particular vacuum from the landscape, one particular compactification scheme for extra dimensions, etc., and then you might get a testable string theory. And after it is tested against (hypothetical) Planck-scale experimental data, two things can happen:

(1) the theory could be correct, or
(2) the theory could be incorrect.

But then in case (2) you could just change your initial choices about vacua etc., and create a new string theory (a model different from the tested one), which would be in accord with experiment.

That is the lack of predictability of string theory framework. It is too general, you can fit it to any experimental data, just like QFT. There is no added value in it.

12. **vmarko**
July 29, 2012

@Hansi:

I wasn't talking about 90% of physicists in general, but 90% of hep-th/gr-qc physicists. You are indeed correct that the number of such chairs in Germany is single-digit or so. But even in Germany most of those are string theory (although statistics is lousy on a small sample, one should look planet-wide instead just Germany).

As for Ed Witten, I am not disputing the importance of his work in mathematics. Nor am I saying that math should not be done. But still, selling pure math as physics (N.B. "selling as" = "advertizing") does not always have merit, especially on such an unproportional scale.

People often get confused by the concept called "mathematical physics", and think that it is somehow a fusion between math and physics which sort-of creates a new scientific discipline. This is wrong. As a researcher in that area, I can tell you that there indeed is a fusion between the two, but in a very specific way (that I don't want to elaborate here). And no new discipline gets created — mathematicians stay mathematicians, physicists stay physicists. We just interact a bit more than usual, to obtain some knowledge, ideas and tools (and sometimes manpower) from the other side.

So, it is completely ok if Witten does research in math that may (or may not) be relevant to physics. But claiming that there is no difference between the two is not ok, IMNSHO. 🙂

13. **MathPhys**
July 29, 2012

Mr Unzicker,
Some of us may disagree with E Witten on this or on that, but we all admire not only his intellect, but also his personal and scientific integrity.
To achieve your 15 minutes of fame, you have attempted to corner and embarrass a very respectable man.

You have also done damage to your cause. There were many better ways to do this. You chose the worst.

14. **Roger**
July 29, 2012

@MathPhys

I think the question that was put to him was a valid one i.e. whether or not he should feel responsible for leading thousands of scientists down what may be a blind alley ? The problem was that the question was posed in a very confrontational way. This is a pity since a good discussion of the question can address useful topics, such as how a community of nominally free thinkers operates in practice and the roles of "leaders".

15. **DB**
July 29, 2012

I like the idea of high-scale SUSY, close to the Planck scale. It will save me having to listen to promises that SUSY is just around the next corner, nonsense I've been listening to for thirty years.

16. **Robert McNees**
July 29, 2012

Peter, let me start by saying that I'm more sympathetic towards some of your arguments than you'd probably expect. But you could do without statements like the following:

I don't think any of his examples addressed the real issue, which is not that practical tests of string theory are far away, but that it makes no predictions, even if you had the technology to test it.

If you had the technology to perform scattering experiments at the string scale you would see soft scattering. That's a basic prediction made by every string model, and there's no way around it. Do I think it's likely that we'll develop that technology? I doubt it; the string scale is probably too high.

The claim that there are no "in principle" testable predictions of strings is wrong. The ongoing debate about strings isn't advanced by presenting a misconception as if it were healthy skepticism.

17. **Bernhard**
July 29, 2012

Robert McNees,

pardon my ignorance, but could you kindly point out any article (since this is so basic) where one can see the details of how a "soft scattering experiment at string scale" would look like? Are the possible phenomenological consequences of such an experiment unique to string theory? Are you talking about definite quantitative comparison or qualitative comparison?

18. **Peter Woit**
July 29, 2012

Hi Bob (this is also a response to P and Bernhard),

I suppose I should get around to putting this question in the FAQ for the blog I started but haven't gotten back to…

You and I discussed this point almost exactly 7 years ago, on Sean Carroll's blog, see here http://blogs.discovermagazine.com/cosmicvariance/2005/07/21/two-cheers-for-string-theory/

and after a while even Sean seems to have agreed with me, see

http://blogs.discovermagazine.com/cosmicvariance/2005/07/21/two-cheers-for-string-theory/#comment-260

For those who don't want to wade through the endless discussion back then, the point is that the "prediction" of soft scattering amplitudes once you get higher than the string scale suffers from the problems that:

1. You don't know what the string scale is.
2. This prediction comes from a perturbative string theory calculation, but this is only reliable if the string coupling is small. Generically in M-theory this won't be true and you don't know what scattering amplitudes will be like.

19. **critical thinking**
July 29, 2012

I find quite bizarre the tone often adopted in discussions about the nature of string theory. I don't necessarily agree with the line of argument by Unzicker (especially the social responsability arguments he advanced here), but I don't see why skepticism about string theory should be considered out of the norm from a scientific perspective. Simplifying a lot, but I would like to confine myself to a few lines. Once there is widespread experimental confirmation of the predictions of a given theory, discussion normally ceases and the theory becomes — by most reasonable and informed people — acepted. Until then, skepticism is the norm, theories are work in progress and/or working hypotheses. In the end, it's that simple. It can well be that Witten et al. intuition is outstanding, and that "his" theoretical program will prove the most fruitful. But we have no way of knowing it in advance. Furthermore I suppose no one contests the quality of his mathematical contributions. So I find all this outcry to Unzicker's questioning as much ado about a completley expected scientific controversy. Unless someone thinks that dissent from the dominant theoretical approach should a priori be discouraged.

20. **Bob Jones**
July 29, 2012

Alexander Unzicker,

I don't know how much background you have in math and physics, but your comments at Witten's talk suggest that you don't completely understand what string theory is about. Like most critics of string theory, you focus on its failure to produce testable predictions, and you believe that scientists are wasting their talent on a failed idea. At one point, you even suggested that Witten's ideas might "lead to nowhere".

I think you should seriously consider Witten's response to your comments, especially the part where he explained how string theory has helped us better understand theories we already have. This is an aspect of string theory that has not been very well communicated to the public, but it's one of the major reasons why there's been such a sustained interest in the theory. In addition to providing a candidate for a unified theory of physics, string theory has provided valuable insights into other parts of physics and mathematics.

In physics, the biggest success of the theory was probably the discovery of the AdS/CFT correspondence. This result provides a concrete realization of the "holographic principle" which appears to be a very generic feature of quantum theories of gravity. The correspondence shows that quantum field theory and string theory can provide equivalent descriptions of the same physics, and it can be used to understand qualitative features of certain condensed matter and QCD systems.

In mathematics, the impact of string theory has been enormous. Ideas from string theory have led to important developments in mathematics like mirror symmetry, Gromov-Witten invariants, and the proof of the monstrous moonshine conjectures. In each of these subjects, string theory provides crucial physical intuition which has guided the development of the mathematics and led to new conjectures. Since you also mentioned supersymmetry in your comments, it's worth pointing out that a certain supersymmetric field theory called Seiberg-Witten theory has led to revolutionary developments in the topology of 4-manifolds.

Those are just a few of the applications of string theory, but it should be enough to convince you that Witten's ideas have not led to nowhere…

21. **Peter Woit**
July 29, 2012

Bob Jones,

I pretty much agree with you, that there's a case to be made for string theory research based on other things it has led to, even though the unification idea has failed. If you look at what most "string theorists" are working on, it is typically such spin-offs, rarely string theory itself (Witten's recent work is kind of an exception). "String phenomenologists" are now somewhat of a marginalized subfield, sometimes like last year even shut-out of the most prestigious string theory conference.

That said, it's remarkable that leaders of the subject keep trying to sell it with a hard-sell for string unification, ignoring the signs of failure of the idea. It is reasonable for members of an audience being sold this to question it, but too bad that Unzicker chose to speechify, since a real question expressed politely might have gotten a more interesting response from Witten, who surely is aware there's a difficult issue for him here.

22. **M**
July 29, 2012

String theory degenerated into carpet selling when, 15 years ago, the most influential string theorists failed to acknowledge that the brane revolution ruined the predictive power of the theory. Hiring of string theorists could have been stopped at that stage. On the contrary, some famous institutions hired only string theorists, with the result that now they are almost outside from physics.

I think that this a real responsibility.

23. **David Nataf**
July 29, 2012

I find it so unbelievably condescending that anybody would imply Witten is responsible for hundreds of young physicists going into string theory. It implies these young people are sheep with no internal direction, they're simply feeling the wind and thinking "Witten, he's the smartest, I'm going to go do whatever he's doing!!"

24. **Robert McNees**
July 29, 2012

Hi Peter,

Thanks for reminding me — I knew this discussion sounded familiar. Here is my take on the issue: I agree with both of the points you list, but I don't agree with your interpretation of them.

First, it's true that we don't know what the string scale is. For the sake of argument I am willing to accept a scenario where the string scale is taken to be near the Planck scale. If you don't see some indication of soft scattering as you approach the Planck scale, then it isn't there.

Second, it's true that I'm referring to the results of a perturbative calculation (As a note to Bernhard, you should be able to find this calculation in most string theory text books). But I don't see this as nullifying falsifiability. If string theory is weakly coupled then near the string scale you begin to see soft scattering. If it is strongly coupled then it's true that I don't know what the physics looks like. But I know what it doesn't look like: the same QFT that was used to describe the physics at lower energies. In a stringy framework the predictions eventually have to take a turn from the low-energy description, and if you don't see that then the framework is falsified.

For what it's worth, I think everyone should be skeptical of strings. I just don't agree with the claim that there are no in-principle falsifiable claims.

P.S. Sorry it took so long to reply. Today was our first case of "vomiting five month old baby".

25. **anonomous**
July 30, 2012

I think Strings 2012 falls into the category of high (human) drama that you mentioned several weeks ago, where everybody is looking for something to keep this field alive. Personally, if I had anything to do with string theory I don't think I would have shown up at this conference – it sounds more like a consolation party.

26. **Anonyrat**
July 30, 2012

A 10^19 GeV collision can produce a huge number of say, 125 GeV higgses; I find it hard to imagine that such collisions will be dominated by the few particle amplitudes that are typically calculated in introductory texts. Therefore even in a weakly coupled string what should start appearing at the string scale is not clear to me.

27. **M**
July 30, 2012

Anoyrat, suppose you build a proton-proton collider at 10^19 GeV. As you need a radius as big as the Galaxy, turning it on would require at least 100,000 years. Before being able of computing string predictions, you need to find a string model that contains protons and all the SM physics. At the moment no example is known, and this might be as difficult as building the Planck collider. But presumably there are about 10^400 such models, that will give you about 10^400 different string predictions. In practice this means no prediction.

PS: David, young people have internal directions, but in practice they need a stable position before they can work on what they like independently of the community.

28. **piscator**
July 30, 2012

M:

Exactly wrong. The number of different realisations of low energy physics is irrelevant for what happens at high energy scales. Once you hit the high scale, you start hitting string excitations, which have a basic general structure – softness of scattering amplitudes, exponential growth in number of states with energy, Regge excitations, etc.

Also note that soft scattering comes from the extended nature of the string – so the answer to Peter's comment is that as long as your particle-like excitations become replaced by extended objects, which holds in all limits of string theory that I know, you should have soft scattering at high enough energies.

29. **Tmark48**
July 30, 2012

Unzicker's question is right. After 30 years people have the right to question the "social" and scientific cost of having had an entire generation of theoretical physicists waste their time on such a hopeless quest (unification of physics). And this was true after each "string revolution", we went from bad to worse. And the situation right now is that String Theory (but should we be calling it String conjectures ?) is not in any sensible way a physical theory. By its very nature it cannot predict anything (it's like having a logical model that is inconsistent, you can derive without any problem contradictory theorems).
And who are more responsable for this situation than the leaders of this field ? A very unsettling question to be sure, but one which the powers that be should thruthfully answer.
—
Now having some theorists work on ST is a good thing, as is a good thing having theoretical physicists work on other "speculative models of the universe". But putting all your eggs in same basket is never a good thing. And sometimes fields in physics just die. I think ST is one of those. You simply cannot keep alive such a framework that has nothing to show for it after decades and decades. Saying that ST has helped to understand "superconductivity" etc… is all fine and dandy, but let's get serious.
The raison d'être of ST was to unify physics, and in this respect is has been a colossal failure.
—
Witten was awared the Field medals for his mathematical acomplishments using ST. Good, so he is a very talented mathematician. Has he or will he ever be awarded the Nobel prize in physics ? That I think will never happen. So as a theoretical physicist he hasn't done such a good job after all.
—
Leave physics (experimental and theoretical) to physicists, and mathematics to mathematicians (or mathematical physicists).

30. **Bernhard**
July 30, 2012

Robert McNees,

but then let us assume string theory is strongly coupled and that I don´t observe soft scattering at say, the Planck scale. Since we know nothing about Planck scale, saying how it does NOT look it´s not really very convincing. I have no trouble seeing QFT would break down there, but my understanding of your argument is the following: at the Planck scale we should see soft scattering (if string theory is weakly coupled) or ANYTHING at all that is not QFT, and this last "prediction" could be used to accommodate the non observation of soft scattering. If the case you were making was: no soft scattering = no string theory, OK, I agree this would be in principle falsifiable, but there seems to be, as always, a caveat that could be used to evade negative results.

31. **Tmark48**
July 30, 2012

> Hansi wrote :

> "For this reason, Mr. Unzicker, nobody in the universe cares about whether you do not like mathematics. This is your own problem and so you should simply refrain from telling mathematicians, fields medal winners, and mathematical physicists that they should abandon mathematics.

—
It is ironic because no one is hitting Witten on the head for his mathematical accomplishments. But as you say, we shouldn't ask mathematicians to abandon mathematics, but by the same token we sure as hell should ask or demand that mathematicians abandon physics research. Somehow theoretical physics departments have been inundated by wannabes mathematicians. And this is a disaster for physics and physicists.

A mathematician has no problem dealing with a "physical theory" that offers 10^400 possibile solutions. He doesn't care about physical predictions so anything is fair game. But try to pass this kind of "theory" to a real theoretical physicist (and if he isn't senile or deluded) he will throw it out the window together with the person the presented it.

Right now what theoretical physics is lacking are good PHYSICISTS.

32. **AcademicLurker**
July 30, 2012

Both Peter Woit and Lee Smolin have chapters in their respective books describing how hiring works in HEP theory and how this has had the effect of chocking off alternate, non-ST approaches.

An interesting question now that ST seems no longer to be in the invincible position it once held is: will things change for the better? That is, if the sociology of the field remains the same – "Whatever the hot thing is today, that's what everyone MUST work on if they want any chance at a job." – will it matter if the hot thing is no longer ST but something else?

33. **Anonymous**
July 30, 2012

Tmark48:
" "social" and scientific cost of having had an entire generation of theoretical physicists waste their time on such a hopeless quest (unification of physics)"

You should better ask about the social and scientific cost for wasting a whole generation of PhDs as quants in the financial bogus industry.

34. **MathPhys**
July 30, 2012

You know why string theory is a very attractive subject to work on, or even only to follow? It's because, by and large, the smartest people in the physical sciences, including some of the brightest young talent, work on the subject, and it's a great pleasure to listen and to talk to these people.

I go to stringy meetings and to non-stringy meetings and the difference in average IQ, and in breadth and depth of knowledge of physics and mathematics, is palpable.

Forget about the hype (which is infinitely less today than, let's say in the mid 80's) and forget about all pretensions to compute the mass of the Higgs and all that (very few people still talk this way, and they _are_ not string theorists). Just follow the intrinsic logic of the _theory_. It's hypnotic.

35. **Peter Woit**
July 30, 2012

Hi Bob,

Good luck with the baby!

About the "string theory predicts soft scattering at high enough energy" prediction: I think here the bottom line is pretty clear. I'd describe the situation as an empty glass, Bob and others want to argue that there's still a smidgen of moisture at the bottom (at some unknown scale, amplitudes will fall off exponentially in some unknown way).

Tmark48,

I think it's incredibly unfair and unreasonable to blame the string theory debacle on mathematics and mathematicians. Of the large number of speakers at the conference none are mathematicians. Quickly scanning the list of nearly 400 attendees I don't see any names that would be recognized by people in the math community other than Witten's. The problem with string theory is not that it uses math or mathematical standards, but that it is based on a wrong physical idea about unification. The people pursuing this are physicists, trained in physics departments, hired by other physicists, funded by physics grant panels. If you listen to the arguments being made for string theory at this conference, they are about physics, not mathematics. There's a healthy and interesting area of research going on at the boundaries of math and physics, inspired often by things that came up in string theory, but people doing this are not getting hired by physics departments.

AcademicLurker,

The problem of very few jobs in particle theory, leading to hiring only of people working on the "hot" topic, is independent of string theory. Lee Smolin made this very explicit in his book, I hope this point was clear in mine. For quite a few years now, physics departments in the US have stopped hiring string theorists, but instead only hired "phenomenologists" working on things like dark matter or BSM LHC physics. With nothing of this kind turning up at the LHC, this may be as much of a dead end as string theory. The backlash against string theory has also led to a trend of not hiring anyone doing anything mathematically sophisticated of any kind. This I think will turn out to be a big mistake, a complete misinterpretation of the string debacle as due to mathematics.

36. **AcademicLurker**
July 30, 2012

Peter,

Both your and Smolin's books were pretty clear that the "sociology" of HEP theory had origins independent of ST.

I was just wondering whether the prospect that 90+ percent of the brightest folks in the field might have spent the last 30 years chasing a single idea into a dead end might result in a greater appreciation of the benefits of intellectual diversity.

It sounds like the answer to that question may be "no".

37. **Peter Woit**
July 30, 2012

AcademicLurker,

Yes, the answer is a pretty definite "no". The lesson physics departments are taking away from the failure of group-think about one trendy subject is just to change to a different trendy subject.

38. **M**
July 30, 2012

piscator,
Planck-scale string states are just many particles.
You don't know how they couple to SM particles, unless you have a model of SM particles. All quantitative predictions depend on the unknown compactification.
You can only have qualitative expectations, such as the presence of black holes and other effects that can be understood in semi-classical approximation.

Notice that some people considered the possibility of extra dimensions and strings and the TeV scale: string theorists did not provide any solid prediction.

39. **Friedrich**
July 30, 2012

The problem of string theory is not that it makes no predictions; it is that string theory has no clear principles. The issue is usually avoided. But which alternative theory does? None. So the real failure is that hundreds of people with high IQ are not able, not willing, or not allowed to think about the principles for a unified theory.

Yes, string conferences are full of really bright people. But they do not talk about the real issues. They squander their IQ. Every single day.

40. **Peter Woit**
July 30, 2012

Friedrich,
Gross acknowledges explicitly the problem of no clear principles, and points to work on higher spin theory as an attempt to get some new insight into possible principles. Witten and many others have devoted a lot of effort to finding some foundational principles for string theory.

The problem may be not the failure to acknowledge the problem, but that the problem may be insoluble. The conjecture that there is some wonderful M-theory based on some unknown symmetry principles that gives known string theories in various limits and can be used to unify physics may simply be a wrong conjecture. I'd argue that there's now 20 years of evidence for this.

41. **Jack Lothian**
July 30, 2012

Last week I bought Smolin's book for a $1 at rummage sale. It was a great buy – the first time I read it – it really helped me understand the debate on this blog. If there is another illiterate on this board other than myself, I strongly recommend it.

As to Alexander's intervention – I think the reaction was a bit over-the-top. Sure he was confrontational but I have been to dozens of symposiums/conferences & I have heard several extreme confrontational exchanges – often between well-known academics. Sure he unfairly lays the fault on the one person who happens to be most physicists' choice for the greatest living physicists. But I bet Einstein also received some equally bad & unfair public attacks. We live in a free society & with freedom comes the right to say unpleasant things. While I recognize there are real limits to how uncivilized a person can act in a formal setting; I do not think Alexander really crossed that line. Witten's purported response suggests that he accepted the factual content within the remarks & ignored the emotions – good for him.

42. **Peter Shor**
July 30, 2012

Tmark48: Mathematicians are generally kept down to earth by the need for rigorous proof. Theoretical physicists have generally been kept down to earth by the need to match experimental findings. When you remove both of these constraints, …

43. **Alexander Unzicker**
July 30, 2012

The comments on my question led me to reflect upon some points I'd like to clarify.

I see that some people felt my comment was quite long. Actually I could not hear the chairman's words but I had – and still have – good reasons to assume that it was the content which annoyed him, rather than the duration. For that, I continued.

I don't quite understand why some people saw a hostility on my side which I don't feel. I didn't question Witten's integrity nor his mathematical intelligence. But maybe for that very reason, I think he is not interested in history or sociology of science, which I think are essential if one wants to evaluate the actual state of physics. And likely, for the same reason, he is plain uninformed about physics, e.g. about the missing evidence of gravitational waves (arxiv:0909.3583).

That might all be fine, and I have no problem if string theorists do math in their
meetings. But this was a public and highly publicized event which on purpose
created the impression that the big science experiments – Heuers talk was just before – meet the top theories of physics. But string theory is not physics. It is a problem that string theory is not falsifiable, another problem that its reasoning is entirely metaphorical (see e.g. Bert Schroer's papers), but the biggest problem is that it is still labelled as science.

Yet I do not even blame Witten in first place for that false declaration. But as everybody else on the planet, he has to reflect upon the consequences of what he is doing. I am sorry that those who complained about inappropriate words havn't been
able to tell it to their friend in a nicer way. But that had to be said.

44. **Bob Jones**
July 30, 2012

Tmark48,

"And sometimes fields in physics just die. I think ST is one of those."

I hate to disappoint you, but I think string theory is here to stay. For one thing, we know that it's equivalent to interesting quantum field theories like the N=4 super Yang-Mills theory. In addition to equivalences such as this one, we know that string theory subsumes a wealth of interesting effective quantum field theories related by dualities. For these reasons, string theory has become an important part of the mathematical formalism of quantum field theory.

"The raison d'être of ST was to unify physics, and in this respect is has been a colossal failure."

You realize that string theory was originally a theory of the strong force, right? Nowadays, many string theorists view their subject as a mathematical framework whose role in physics is not yet clear. Although unification was always the main advertized goal of the theory, I think it's oversimplifying to say that unification is the "raison d'être" of string theory.

"Witten was awared the Field medals for his mathematical acomplishments using ST. Good, so he is a very talented mathematician."

Here I think you're oversimplifying the relationship between string theory and mathematics. None of the results that made Witten famous are "mathematics" in the usual sense. The concepts that string theorists work with are fundamentally nonrigorous because they are based on the notion of a Feynman integral, which doesn't have any rigorous meaning. They are physical concepts, and I don't think mathematicians would have discovered them without help from the physicists.

45. **Bob Jones**
July 30, 2012

"he is plain uninformed about physics, e.g. about the missing evidence of gravitational waves (arxiv:0909.3583)."

Are you serious?? You think Edward Witten is uninformed about physics?

46. **Peter Woit**
July 30, 2012

Bob Jones/Tmark48,

It's simply not true that Witten won the Fields Medal for "mathematical accomplishments using string theory", although this has been repeated so often that everyone seems to believe it. I've written about what actually happened here:

http://www.math.columbia.edu/~woit/wordpress/?p=99

Alexander Unzicker,

You are behaving like a crank. The point of a question session after a public talk is not for everyone who feels like it to stand up and comment on what the speaker said. By doing this at length and refusing to shut up and let Witten speak you were rude to him and to the rest of the audience, who were there to hear what he thought, not your thoughts. You could have made your point with a concise question rather than a speech, which would have been a lot more interesting for everyone.

By going on about gravitational waves and Witten not being informed about the absence of their detection, you remove all doubt in the minds of those suspicious about whether or not you are a crank. If you think you are doing the world a service by your behavior, saving us from string theory and its excesses, please be aware that you are doing the exact opposite, making people think that those skeptical of string theory are cranks who don't know what they are talking about.

47. **Bob Jones**
July 30, 2012

Peter,

Yes, you're right. I didn't mean to imply that Witten got his Fields Medal for work on string theory (though Chern-Simons theory is certainly related to string theory).

48. **Peter Woit**
July 30, 2012

Note to all,

I'm not going to host any more discussion between Unzicker and other commenters here, that kind of thing is a crackpot magnet I want no part of.

49. **amused**
July 31, 2012

MathPhys wrote:

"You know why string theory is a very attractive subject to work on, or even only to follow? It's because, by and large, the smartest people in the physical sciences, including some of the brightest young talent, work on the subject, and it's a great pleasure to listen and to talk to these people.
I go to stringy meetings and to non-stringy meetings and the difference in average IQ, and in breadth and depth of knowledge of physics and mathematics, is palpable."

This is a familiar tune.
If any of these brilliant young string theorists have anything interesting to say about physics, let them go publish it in Physical Review Letters.

MathPhys, so that you can more fully appreciate the efforts the organizers of those high IQ meetings make to keep out the rabble, here's a little story:
Some years ago there was a Simons workshop on Geometry and Physics (or something like that). String theory was listed as one of the topics to be covered, but it was supposedly also open to other math-phys QFT-related topics. Young people who wanted to attend had to put in an application, listing 3 recent publications, and get a recommendation letter from a senior person. I decided to give it a shot, so I listed 3 of my papers on mathematical aspects of chiral gauge theories on the lattice, 2 in NPB and 1 in PRL, all single-author, and got a recommendation from a big-shot theoretical physicist at one of the major US universities with 7000+ citations (or maybe it was only 6000+ at that time) who was one of the main people working on that topic at the time.
My application to attend was of course turned down. The organizer wrote "Sorry, that fact that you are not working on string theory shows that you are ignorant and have a low IQ, so we don't want you at our meeting."
(OK that's not what he really wrote, but I'm sure it's what he was thinking 😉)
Afterwards I looked up the list of participants and they were all string theorists or mathematicians doing ST-related stuff. This of course just reflects the fact you mentioned that the smartest folks do string theory.

50. **piscator**
July 31, 2012

M: you are missing the physics of soft scattering. It is not something mysterious that requires pages of mathematics to understand, it is a feature of extended objects. Scattering at high energies probes structure at the corresponding inverse length scale, and if you have no structure at that scale – ie if all your objects are extended – then you cannot scatter hard. This is why Rutherford was surprised by hard scattering of alpha-particles, he thought the atom was some extended object rather than having a hard pointlike centre.

With regard to models of TeV scale strings, it wasn't string theorists who were responsible for the gross overhyping of these ill-defined and poorly motivated models.

51. **Henry Bolden**
July 31, 2012

It turns out the speculative physics = big money. So there is an application after all. Read about it the New York Times:

http://www.nytimes.com/2012/07/31/science/9-scientists-win-yuri-milners-fundamental-physics-prize.html

52. **Yatima**
July 31, 2012

"Milner Prize"?

"Where anthropic principle and philanthropic capitalism meet"?

I can only approve.

53. **Raisonator**
August 1, 2012

Just found that, A. Connes on string theory, min. 95:30-

http://www.newton.ac.uk/programmes/NCG/seminars/090415301.mp3

54. **Marcus**
August 2, 2012

In case anyone is curious about that September 2006 A. Connes talk for other reasons besides the quote just mentioned:
http://www.newton.ac.uk/programmes/NCG/seminars/090415301.html

And a followup talk that October
http://www.newton.ac.uk/programmes/NCG/seminars/100214002.html

Fundamental Physics Prize

July 31, 2012
Categories: Favorite Old Posts, Uncategorized

String theory may not be doing so well in the popular press or among physicists, but at least a fabulously wealthy Russian investor is a fan. Yuri Milner recently deposited \$3 million each in the bank accounts of 5 string theorists (basically the theorists at the IAS and Ashoke Sen) and four others, choosing them himself as recipients of the "Fundamental Physics Prize". It seems he intends to keep doing this in the future, making "Fundamental Physics" a very lucrative business to be in.

Update: Now that I'm awake, I noticed what is odd about this prize, after realizing that the winners are kind of a list of the most prominent people in the field who haven't won a Nobel Prize. What this does is turn the Nobel Prize on its head; you get it for doing work that is untestable or wrong, but that has a high profile:

> Unlike the Nobel in physics, the Fundamental Physics Prize can be awarded to scientists whose ideas have not yet been verified by experiments, which often occurs decades later. Sometimes a radical new idea "really deserves recognition right away because it expands our understanding of at least what is possible," Mr. Milner said.

Peter Higgs's ideas from 50 years ago have finally been verified by experiment, and as a result, if he can hang in there, he may share (probably 1/3) a Nobel Prize of nearly ~~\$1.5 million~~ \$1.2 million (reduced recently from \$1.5 million). The Fundamental Physics Prize winners get about ~~six~~ 7.5 times more for ideas that have gotten a lot of hype, but no experimental test (or at least not enough to satisfy the Nobel Committee of physicists). Even better, you get the prize for your over-hyped ideas even if experiment does show them to be wrong:

> Dr. Arkani-Hamed, for example, has worked on theories about the origin of the Higgs boson, the particle recently discovered at the Large Hadron Collider in Switzerland, and about how that collider could discover new dimensions. None of his theories have been proven yet. He said several were "under strain" because of the new data.

One wonders about the implications of this for the future of theoretical physics: why should young theorists work on unpopular ideas and/or try hard to find testable ones? That will get you only ~~\$500K~~ \$400K, and there's \$3 million to be had if you work instead on a speculative and untestable idea that you see on TV.

Update: The Fundamental Physics Prize Foundation has a website here. The board consists of Yuri Milner and Steven Weinberg (although it is specified that only Milner chose the prize recipients). The goal of the prize is to "bring long overdue recognition" to its recipients and "more freedom and opportunity to pursue even greater future accomplishments". It's not quite clear why the particle physics professors at the Institute for Advanced Study (all of whom got a prize) have been suffering from a lack of freedom and opportunity to purse their research.

Update: For a profile of Yuri Milner by Michael Wolff at Wired, see here.

Update: Geoff Brumfiel at Nature has a story about this here. Ian Sample covers the story for the Guardian here.

Update: Adrian Cho at Science reports this story as Russian Gazillionaire Lobs Money at Theoretical Physicists:

> David Lee Roth, the sometimes singer for the legendary rock band Van Halen, supposedly once remarked: "Money can't buy you happiness, but it can buy you a yacht big enough to pull up right alongside it." If so, then nine theoretical physicists can now afford to join the next-to-happiness flotilla, thanks to the generosity of Russian billionaire Yuri Milner.

Update: Another article about Yuri Milner is here. It seems that he has had a dramatic effect on the venture capital business in Silicon Valley, with his tactics there somewhat analogous to his tactics in setting up this prize. Where Jim Simons has put a lot of effort into making carefully targeted investments of different sizes in math/physics research, Milner has just dumped large sums of Russian money indiscriminately on the main figures in the "hot" area of the subject with no-strings-attached, which is somewhat the same as his investment philosophy in Silicon Valley. He had a lot of success there with investments in things like Facebook, but it's still to be seen whether this was a bubble that will burst. One big difference with physics though is that in the business world you're ultimately judged on whether you make money or not. In physics you're supposed to be judged on whether your experimental predictions turn out, but his investments in physics are structured to evade exposure to that problem.

Update: There's an article about Sen getting the prize here. Note the headline: this is now referred to as "Physics highest honour".

Update: Another article about this, from Luca Mazzucato, Fundamental Physics Prize: A Russian money shot for string theory which explains:

> Every physics student's wet dream when they join grad school is to ascend one day to the Olympus of Nobel Laureates, up there in the clouds with Einstein, Feynman and the like. And, of course, Barack Obama and the Secretary of Energy Steven Chu. But most grad students who score the highest points, like the proverbial fly to honey, get inevitably attracted to string theory – that is, the ones who ditch Goldman Sachs job interview. And their Nobel Prize aspirations will never have a chance of materializing – just like that dream house in the Hamptons. That's because string theory, a.k.a. The Theory of Everything, despite its appalling beauty and tremendous fascination, is not going to come close to the real world any time soon. And since the Nobel Prize may only be awarded to those scientific predictions that pass the merciless test of experiment, that brightest students' wet dream – alas, among many others – stands no chance of being fulfilled.
>
> This was the status of string theory up until a week ago, when Yuri Milner – Russian tycoon, Facebook shareholder, and former theoretical physicist himself – dropped the bomb: nine overnight wire transfers to as many physicists' bank accounts, that instantly turned the reclusive scientists into millionaires.

Update: There's an interview at the Times of India with Sen about the prize, which includes the question and answer

> **How does the discovery of the Higgs boson impact your research?**
>
> It's one of the great discoveries of our time. Its discovery has been eagerly awaited since the time Peter Higgs, the British theoretical physicist, proposed the Higgs boson 50 years ago. It tells us that standard model and string theory are correct and that I and every other theoretical physicist who has been working under the assumption that it exists are not on the wrong path after all.

This echoes David Gross and Juan Maldacena's similar claims at Strings 2012 that evidence for the SM is evidence for string theory.

Comments

1. **Anonyrat**
   July 31, 2012

   " Mr. Milner personally selected the inaugural group, but future recipients of the Fundamental Physics Prize, to be awarded annually, **will be decided by previous**

winners.
….
According to the rules, the prize in future years may be split among multiple winners, and a researcher will be able to win more than once. Mr. Milner also announced that **there would be a $100,000 prize to honor promising young researchers.**

Unlike the Nobel in physics, the Fundamental Physics Prize can be awarded to scientists whose ideas have not yet been verified by experiments, which often occurs decades later. Sometimes a radical new idea "really deserves recognition right away because it expands our understanding of at least what is possible," Mr. Milner said.
—-
This gives the current recipients a lot more clout – have any prior group of physicists had the power to confer instant millionairedom (or 100Kdom) on a fellow physicist? – and it might encourage clique-ishness among physicists.

2. **Clayton Pickering**
July 31, 2012

How does one go about submitting one's research to the "Fundamental Physics Prize?"

3. **MathPhys**
July 31, 2012

Wow!!!!!!!!!!!

4. **AcademicLurker**
July 31, 2012

Having just started reading *How the Hippies Saved Physics* a few days ago, it occurs to me that this prize would be much more awesome if it were called the "Fundamental Fysiks" prize.

5. **q**
July 31, 2012

I wonder if Michael Faraday would qualify for the price, shortly after his claim, that "magnetism can produce electricity", which claim as we all know – has been ridiculed by the top "mainstream physicists" of his time, but BEFORE electromagnetic induction discovery?
My impression is, that he wouldn't qualify both BEFORE and AFTER the discovery. Taking into account present, 21 century standards his discovery would be called for sure: "highly dangerous speculation", that has nothing in common with real, experimentally confirmed physics, like String Theory.

6. **Martin**
July 31, 2012

Just a minor correction, Peter: the Nobel Prize has recently been lowered to around $ 1.2 million. So 1/3 of that amount would be $ 400k and the ratio of the "payouts" for proven vs. highly speculative ideas is even worse.

Btw, I very much enjoy your site – I attended several talks at Strings 2012 in Munich (without registration as there was no verification procedure at the entrance to the lecture hall) and it was very interesting to read your comments on the talks and the overall coverage here on your blog. I was also able to attend the Nobel Laureate Meeting 2012 in Lindau at Lake Constance and talk to Laureate David Gross. I can assure you he is still totally convinced of Supersymmetry and String Theory describing Reality.

Even being among so many Nobel Laureates and approx. 550 international young researchers from various fields of Physics the very same week of the Higgs discovery announcement (there was live broadcast of the press conference and a panel discussion with CERN scientists at the meeting), your site was actually my main source of information on everything Higgs-related. You were the first to report the news and they have been very reliable. Thank you very much for this website!

7. **lun**
July 31, 2012

Some considerations:

i) The guy is a crook. He is did not make his money by being a "venture capitalist" (you need lots of money to become one), but by being the CEO of a crooked bank in the 90s, a bank that made money by ripping off Russian pensioners and the like. Unlike Khodorkovsky&co Milner even managed to stay friends with Putin after his involvement in that. This means he is not just a crook, but a crook^2.
The facebook response is hilarious: A few months ago physicists were all enthusiastic about occupy Wall Street. Now you are salivating that a guy who makes the worst Wall Street CEO look like Robin Hood is giving physicists 0.0001% of what he took.

ii) In the last few years there were all kinds of enthusiastic debates about whether modern theoretical physics, with its lack of experimental contact and mathematical rigor, was really science.
It seems the debate is settled: What defines science is the ability to impress a very rich guy with limited specialist training. Throughout history, this was a great indication of pseudo-science, but I guess homeopathy, astrology, Kabbalah, reincarnation, perpetual motion machines and mediums were underrated: Each has its legion of very rich supporters who swore up and down it was true.

iii) As the prize also extends to maths, it is revealing that two people who _unquestionably_ changed mathematics and are still alive and economically far from wealthy, Alexaner Grothendieck and Grigori Perelman, did not get anything (Grigori Perelman lives on his mother's 60 Euro pension, which by the way is 60 Euro because of the stuff Milner&co did in the 90s). Of course both are crazy, but the fact that their "PR dept" is no match of that of the winners also plays a role. It would also be amusing to just give them the money as a surprise, because they refused such prizes before.

8. **piscator**
July 31, 2012

I have to say this seems like a terrible idea. Three million dollars (or even a hundred thousand dollars) as personal cash is a lot, and giving the decision rights on it to a small set of big shots seems to embed cronyism into the subject. Furthermore, history shows that the big names of one generation have not in general been good at picking the winners of the next.

9. **Umesh**
July 31, 2012

I really cannot understand your problem with the winners of this generous gesture from a wealthy individual. Just reading the names would make it clear that everyone in that group, from the great Prof. Witten to Prof. Sen have contributed much much more to the understanding of not only strings, but quantum field theory and so on which are ‘fundamental’ in every sense of the word.

10. **Peter Woit**
July 31, 2012

Thanks Martin!

I’ve updated the post with the more accurate Nobel information. I don’t doubt Gross is still convinced about SUSY, although I wonder if this will still be true after he has to pay off bets he has made about this at some point in the next few years.

11. **prizes**
July 31, 2012

Anyone can endow any prize for whatever reason. A large part of the reason may be to bring reflected glory on Milner himself. (Had you ever heard of Milner till now?) Complain if you wish. As Liberace would say “I cried all the way to the bank.” Yes the stated criteria for the prize do encourage the wrong kind of attitude. Yes it is bad. (BTW the Templeton Prize is intentionally more than the Nobel Prize. It says so.) Then again, why should a bright young PhD want to pursue “honest” physics? (In this context physics means theoretical HEP.) It’s already more lucrative to enter the financial industry, fresh diploma and glowing testimonials in hand, and become a thief.

12. **Peter Woit**
July 31, 2012

Umesh,

I should make clear that those getting the money have significant accomplishments, some of them spectacularly so (e.g. Witten). My comments in the posting should make clear though what I see of concern here.

13. **Thomas Larsson**
July 31, 2012

The prestige of the Nobel prize does not only stem from the fact that it is very old and very big (although I am not aware of any prize that beats Nobel on both age and size), but also from the fact that the Nobel committees have almost never given the prize to somebody unworthy. Well, not in the sciences anyway. I doubt that the world will care much about a prize that a Russian robber baron sets up explicitly to award people who don’t live up to the high criteria of the Nobel prize.

14. **Peter Woit**
July 31, 2012

lun,

The Michael Wolff article says nothing about Milner making his money as “CEO of a crooked bank in the 90s”. If you can document that, please do. Otherwise, don’t use anonymity here to slander someone.

15. **Marty**
July 31, 2012

I guess the Higgs theorists not eligible? The prize site does say “Should recognize major achievements, with special attention to recent developments.” How “recent” is recent?

The boson’s zero-spin has not been determined so to say on this site that “ideas from 50 years ago have finally been verified by experiment” appears aggressive.

Seems like Higgs, Englert, Guralnik, Hagen, Kibble (and some experimentalists) could have been included…unless this is focused solely on “new” physics or young physicists.
Those geezers are certainly not young I guess.

Anyway, very interesting prize.

16. **Peter Woit**
July 31, 2012

Marty,

Guth’s work on inflation was from 1980, 32 years ago. Higgs was 1964, 48 years. So, I guess the cut-off for “recent” is somewhere between 32 and 48 years ago.

17. **Anonyrat**
July 31, 2012

Quote: “Milner, 49, is not an oligarch. He never wangled an oil company or iron mine from the Kremlin at knockdown prices or formed a private army to protect his family. But he has been close to two of the richest and toughest of them. In the 1990s he worked for Mikhail Khodorkovsky, who acquired Yukos Oil Co. and later gained fame as Russian business’ most famous martyr to then-President Vladimir Putin. (Khodorkovsky received a second long jail sentence while Google was courting Groupon last December.) Milner’s financial partner now is Usmanov, who served six years himself in an Uzbek prison on a conviction that was later overturned. Usmanov then built a Russian metals empire worth an estimated $17.7 billion while simultaneously heading an arm of OAO Gazprom, the state-owned natural gas monopoly.

Read more: Close connections – The Deal Pipeline http://www.thedeal.com/magazine/ID/038812/2011/close-connections.php#ixzz22DKKkJNL

18. **lun**
July 31, 2012

Pardon my unnecessary cynicism, Milner obviously made billions in 90s Russia by honest hard work and inventiveness. In particular, he was the deputy CEO of investment in Menatep, I will leave the anonymous writer of Wikipedia to do the slandering.
http://en.wikipedia.org/wiki/Menatep

19. **Quantum Computer Scientist**
July 31, 2012

Alexei Kitaev didn’t start doing his best work (all connected with quantum computing) until the late 1990s. So “recent” can certainly mean in the last 15 years.

20. **JollyJoker**
    July 31, 2012

    Are there any recipients that are not doing fairly prominent work right now? Looking at the list I assumed the idea is to support ongoing work.

21. **Trulo**
    July 31, 2012

    From the NYT article:

    > The \$3 million has already appeared in Dr. Guth's bank account, one that had had a balance of \$200. "Suddenly, it said, \$3,000,200," he said. "The bank charged a \$12 wire transfer fee, but that was easily affordable."

    I wonder what it feels like, waking up one morning and suddenly finding three million in your bank account. It goes without saying that this is all unmitigated madness…

22. **Peter Woit**
    July 31, 2012

    Trulo,

    When I read that I thought that perhaps I missed my chance at the prize. Various e-mails in recent months have promised me millions if I just send them my bank account information, but I deleted those without paying close attention to whether they came from a Russian billionaire.

23. **MathPhys**
    July 31, 2012

    My concern is how this sudden influx of personal wealth will affect the research of those who will get it.

    Next year's (or next time's) winners will be decided by this year's winners, so some of us have a good idea who these will be. But all of these accomplished people are already great professors with prestigious positions that already offers them very good salaries and plenty of freedom. They are already very well-recognized and rewarded.

    I also wonder about the effect of power that this year's winners will have on prospective candidates. They had better be really nice to them from now on.

24. **Peter Woit**
    July 31, 2012

    MathPhys,

    Used to be that giving a talk in Sweden was a cherished opportunity to put oneself before people who would decide whether you get the $400K. Now that $3 million is on the line, the IAS should have little trouble recruiting prominent speakers.

25. **MathPhys**
    July 31, 2012

    Peter,

    I think you have a pretty good idea who the next winners will be. They don't need to give talks at IAS.

26. **David Nataf**
    July 31, 2012

    I don't see how this is different from the Nobel Prize, Shaw Prize, Templeton Prize, etc. It was inevitable that somebody would set up an alternative prize scheme to the problematic Nobel prize, and indeed many groups have done so.

    Peter, think back to the reverence in your words when you discussed how the CMS and Atlas collaborations should get the nobel prize, and how the rules should be amended. You wrote of prize-giving as if it were something objective. It's not.

    Within popular media, the Nobel has been lionized as the greatest achievement in the sciences, which is ridiculous. See Lise Meitner, Rosalind Franklin, John Bahcall, Fritz Zwicky (discovered dark matter), et cetera. Alternative prizes have been set up, and there will be many more.

27. **Peter Woit**
    July 31, 2012

    David Nataf,

    I have no problem in general with rich people setting up prizes for scientists, including to compete with, supplement or even duplicate the Nobel. The Nobel was just discussed in this context because it's the best example of a long-lived prize that has kept very high scientific standards in its awards. You won't see me saying anything critical about the Kavli Prize, the Shaw Prize, or a long list of others. I do think there's a problem though with the Templeton Prize (rewarding those who bring religion into science) and this prize (which to some extent rewards work on over-hyped failed ideas).

28. **kdl**
    July 31, 2012

    This new prize from nowhere is quite odd. The proper approach to make such a prize or award creditable and long term commitment should be a foundation or trust setup in the name of Milner or his choice with certain amount of allocated fund set aside. Many wealthy people who support fundamental research in general would follow such approach either setup a Prize, an Institution or both. Like Shaw, Kavli, Simons, and Lazaridis did in the past, just name a few.

    The impression is that the award is given by Mr. Milner personally, from his bank wired to the recipients' bank accounts based on the various reports link. Very unprofessional done on the surface. Although 27 million is quite large amount of money, to spend it in such manner, it's more like a cheap shot for instant fame of the giver. No guarantee of long term availability.

29. **Tmark48**
    July 31, 2012

Thomas Larsson is spot on. The value of the Nobel Prize is not just the money, but the acknowledgement from your peers that you have done something worthy in science. This kind of scientific recognition CANNOT be bought.
So what if some crook russian guy donates milions of $ to "theoretical physicists" that haven't furthered their field. Their peers will continue to have a mediocre opinion of them, money or no money.

30. **Hack**
July 31, 2012

Wow, the theoretical physics field is crazy, now a bunch of 'top' physicists in string theory and other areas with untestable theories get 3 million dollars each for ostensibly over hyping their discoveries? It seems you should be a better PR guy than physicist now a days and you'll be more successful. Plus as several people have stated earlier in posts, this just reinforces the old guard. They get to choose who gets next years prizes? Wow.

As an aside, how can physicists who champion untestable, unproven ideas past any reasonable time frame remain so revered? Seriously, doesn't that indicate that they are lacking intelligence in certain areas? Say like common sense? I'm not trying to be insulting or inflammatory, I am asking a serious question.

31. **Allan Rosenberg**
July 31, 2012

Peter,

Anyone who has a serious shot at winning the FuPP could easily find a job at a quant fund that would pay more than \$3 million a year, so I wouldn't worry too much about billionaires perverting science–Witten isn't in it primarily for the cash. If a billionaire wants to back string theory research, let him–it makes it that much easier for funding agencies to move their support to other research programs.

If it will make you feel better, I know a couple of billionaires, and I'll ask them if they're interested in awarding some prizes in the application of representation theory to multiverse studies. 8)

32. **Anonyrat**
July 31, 2012

I think this blog, among others, has championed the idea that it is bad for physics if everyone simply plays follow the leader. Well, now the leaders get to decide what is interesting not just by sheer intellectual prowess but also by large money prizes. And this helps the "follow the leader" problem just how???????

33. **MathPhys**
July 31, 2012

You really think that this year's winners will continue to do research as if nothing has happened? And given their financial power over the rest of their colleagues, you think their relationships will stay as natural?

If someone had an incredible amount of money and wanted to sabbotage a subject, you think there is a more effective way?

Mr Milner could have started a new, well-funded institute dedicated to fundamental research in physics, along the lines of the Perimeter Institute, but this time in a different continent. He could have subsidized the research of a very large number of young, talented scientists (including many in Russia who live hand to mouth). But he decided to take the easy way and splash incredible amounts of cash on those who need it least.

34. **Bernhard**
July 31, 2012

Allan Rosenberg,

one cannot compare someone´s year salary with a prize that is deposited at once in your bank account. No idea if and how much taxes are payed, but still, this is a LOT of money even for Ed Witten. But this prize is really a joke as its contenders were chosen by Milner himself, who is nobody in condition to judge scientific merit, no matter how a genius he is as an investor. He should have arranged a serious and as neutral as possible scientific committee, then this would be a total different story.

Now the problem for the future is obvious. Witten, Maldacena, Arkani-Hamed et al are of course people of the highest scientific standard, but means also this will become a string theory prize (or a "hype" prize, who knows).

I´m sure they are all aware this prize means absolutely NOTHING from a scientific point of view, but again, they are also aware they are world leaders in the field and since the money is really good, it is really hard to say no…

35. **piscator**
July 31, 2012

Hack: in some cases – clearly Witten, Maldacena, Sen and to some extent Seiberg – the main achievements are in mathematical physics. Such results are clearly `true' and as such have an intrinsic importance independent of whether or not the world at sufficiently short distance scales is made up of vibrating strings. So the answer to your question is that the results are eternal and impressive, and are justly respected.

Sure, anyone can set up a prize for whatever reason. However the more I think about it the more I think that this is just terrible for the subject. There is the sheer arbitrariness of the selection – for example awarding inflationary prizes to Linde and Guth but not Mukhanov. There is a lack of regard for what really makes the best physics – Arkani-Hamed gets a prize much more valuable than the one Feynman, Gell-Man, tHooft, Weinberg etc got? As several have pointed out, it makes the problem of follow-my-leader physics worse. As it is there are too many young people whose work is based on what is fashionable at Princeton, and the prospect of a 100k/3M dollar carrot will just make this worse.

The commenter who suggested this is a secret plan to destroy particle theory may be on the money…

36. **fp**
July 31, 2012

I'm interested to hear how young people in the field feel about this. I can only speak for myself, a graduate student, but reading this news (and the news last week about the Simons Foundation grants) makes me less excited about doing research, not more. The merits of research should be decided gradually over time by the consensus of the scientific community. A billionaire giving awards to his favorite people feels arbitrary and, ironically, it cheapens the whole endeavor. And it feels immoral to give millions of dollars to IAS professors who already have everything they need. A far better way to tap into unused potential would be to donate the money to support physics education in underprivileged areas. Find the creative, hard working kids who's talents are going unused. Again, I'd be interested to hear whether this announcement has a motivating or chilling effect on other young researchers.

37. **David Nataf**
July 31, 2012

    Well, this is probably just the beginning of prize proliferation.

    If some of you are unhappy about this, you may be suffocating very soon.

38. **Peter Woit**
July 31, 2012

    Tmark48,

    I'm afraid that at least in the US, money does talk, quite loudly, especially for university administrations. The \$27 million Mr. Milner has just spent and the millions more to come will buy him and the people he has given it to a lot of influence.

    One aspect of this influence will be that you will see very little public criticism of this prize coming from academics. My impression is that most particle theorists who have done well enough to have some sort of permanent career are convinced that only the minor matter of experiments being too difficult is what keeps their work from being rewarded with a Nobel prize. They're not going to jeopardize their shot at \$3 million by shooting their mouths off in public…

39. **Chris Herzog**
July 31, 2012

    I hope that the winners spend the prize money on their research. Heck, if Arkani-Hamed, Seiberg, Maldacena and Witten pool their winnings, they ought to be able to endow a few new positions at the IAS. Maybe they can even reduce their tax burden with such an approach.

40. **Trulo**
July 31, 2012

    On second thoughts, funny how nobody seems to have rejected the prize. It seems Perelman is one of a kind.

    On the other hand, what on Earth is Weinberg doing in the Board together with this guy (appropriately described above as a "Russian robber baron"). I strongly suspect that if I invited SW to give a talk at my Department he would politely decline… could it be true that money can do the trick?

41. **Peter Woit**
July 31, 2012

    Trulo,

    We don't actually know if anyone turned down the prize. If they did, I doubt this fact would have been publicly announced. Maybe there were originally supposed to be 10 prizes. Hard to think though of someone who would naturally fit in with the other 9, but would turn down \$3 million.

    If you were to call up Weinberg to tell him you needed help spending \$20-30 million and you were sending your private jet to pick him up, you might have better luck getting his attention.

42. **anonymous**
July 31, 2012

    I had predicted Kitaev would win a Nobel prize if someone ever makes a qubit out of a non-abelian anyon, which might happen within a reasonable time scale, maybe a couple decades. Now that the Nobel committee will see he doesn't need any more money, I may have to cancel the prediction. Congratulations in any case! This prize, like his previous MacArthur Fellowship, is very well deserved.

    For the rest of you worried about cronyism, just chill out. There's no precedent. We should give this year's winners the benefit of the doubt that they'll do the right thing.

43. **Alp**
July 31, 2012

    I think this is welcome news. Nobel was more and more starting to become a self congratulation by the liberal establishment — influenced more by politics than merit. Even the president got it with hardly anything credible.

    If anything, it is more commendable when people choose to support ideas with their own money than with others'. I doubt any of the negative commenters would turn that reward down if they were chosen as one of the recipients.

44. **Peter Woit**
July 31, 2012

    Alp,

    I freely admit that I, for one, would not turn down \$3 million if some eccentric Russian gazillionaire wanted to deposit it in my bank account. Eccentric Russian gazillionaires should be free to do what every they want with their gazillions, to the extent that they are legally obtained gazillions.

    Spare us though the political commentary about "liberals", and I'll ruthlessly delete any further comments that try and enter into the usual tedious political discussion that you want to bring up. One wonderful thing about the Physics Nobel, as well as the arguments about string theory and related issues is that they have nothing at all to do with this.

45. **Henry Bolden**
July 31, 2012

    This has got to be disastrously bad for the future of theoretical physics. Now we've got a clique of cronies, the initial prize winners, who get to decide which of their friends are going to be rich. The scheme might be sufficient to corrupt even Witten which I would have thought to be impossible.

    The prize winners are certainly a talented bunch and well worthy of accolades, pats on the back and fancy plaques for their office walls, maybe even a marching band with 70 tubas, but giving them a whole lot of cash seems pointless. The IAS permanent members are well paid and already have the freedom to do whatever research they like.

    What's next? Maybe Tony Soprano will sponsor a big prize in arithmetic algebraic geometry? That will get the subject going. Or else.

46. **David Roberts**
July 31, 2012

So is Kontsevich a physicist now? There's a big difference between theoretical physicists who work on string theory and mathematicians who work on string theory. One of them can claim proof of their work.

47. **anonymous**
July 31, 2012

Kontsevich's work isn't just string theory. His deformation theory stuff is pretty cool. The phase space formulation of quantum mechanics, for which deformation theory provides some foundation, happens to be my personal favorite. The Kontsevich quantization formula sets up some fun generalizations. Sure, it has difficulties making the transition into relativity, but maybe this prize will encourage some brilliant graduate students to revisit this line of attack for, say, quantum gravity?

I'm really surprised by all the negativity in these comments. This prize should be an inspiration for promising young theorists to take some risks in their work (like the idea that Avi Loeb has been promoting) without having to worry about the volatility of public funding. They can use the money to attract new students, pass the funds along to experimental research that may corroborate or rule out their theories, or donate it to a charity.

And I'll reiterate that you all shouldn't assume the worst in people. These winners are not likely going to be corrupt and resort to cronyism. These guys already got their prize, so they're bound to pass around the torch to different branches of physics. Jeez.

48. **Robert Rehbock**
July 31, 2012

No strings were attached these awards, right?
These recipients are all top scientists. They get to choose what to do with the money. I am confident that they will use it more wisely than many persons who possess and use their money.

49. **Henry Bolden**
July 31, 2012

Here's a way that the Yuri Milner lottery winners, at least the IAS members among them, can show solidarity with the math/physics community. They can donate half their winnings to the IAS and that will be matched by the Simons/Simonyi $100 Million Challenge Grant:

http://www.ias.edu/news/press-releases/2011/08/18/challenge-grant

50. **Giotis**
August 1, 2012

Finally justice was served…

We were tired to see mediocre physicists winning the Nobel prize just for being lucky (a characteristic example is Penzias and Wilson but there are more) or for some unimportant shallow discoveries while the true giants of theoretical physics and their revolutionary frontier research remained largely unappreciated by the general public.

The Milner's prize certainly fills this gap.

51. **Bernhard**
August 1, 2012

"Hard to think though of someone who would naturally fit in with the other 9, but would turn down \$3 million."

Since this is just a high-profile prize, what about Lisa Randall? Well, she will end up winning anyway, now that is up to the stringy guys to choose the next winners.

52. **Bernhard**
August 1, 2012

Sorry, I misread, I don't think Lisa would (will) turn down 3 million, my comment is restricted to "someone who would naturally fit in with the other 9".

53. **M**
August 1, 2012

I propose a prize to Ereditato for superluminal neutrinos!

Speaking seriously, while awarding a prize for inflation is reasonable, giving a prize for speculations not confirmed by LHC is less reasonable.

54. **Anonyrat**
August 1, 2012

In reply to Giotis, physics is about the world, and not about theoretical achievements, and not about IQ. Penzias and Wilson accomplished more for physics than a lot of IAS physicists – they actually discovered something.

55. **Thingumbob**
August 1, 2012

Modest suggestion. Prize should be renamed physics of the fundament. Thank you.

56. **gurplesnork**
August 1, 2012

Hawking and Penrose would seem to be good candidates for this prize.

57. **Tmark48**
August 1, 2012

I think the correct term for this prize should be "Milner's Prize for speculative thinking". The general public will have a distorted (if it isn't already the case) of what

theoretical physicists do. This is what happens when a generation of theoretical physicists the caliber of Feynman and Wheeler (among others) passed away with no replacement in sight. Theoretical physics reduced to speculative thinking has nothing to do with physics.
I wouldn't have awarded the prize to anybody. Maybe put it on the backburner for Alan Guth waiting for experimental confirmation of cosmic inflation. As for the others no.

58. **Tmark48**
August 1, 2012

> Robert Rehbock wrote : "No strings were attached these awards, right?"

—
I'd say ONLY strings were attached to this prize. ^_^

59. **P**
August 1, 2012

Peter, Tmark, and other people with similar sentiments,

I'm a bit surprised by the anti-string talk regarding this award. You often distinguish, Peter, between the use of strings for attempts at unification (on which we disagree), but acknowledge that strings have made very important contributions to our understanding of mathematics and quantum field theory. I'm confused, then, by the attitude in

"String theory may not be doing so well in the popular press or among physicists, but at least a fabulously wealthy Russian investor is a fan"

for example. First, it doesn't really matter how string theory is doing in the popular press, because we physicists tell the non-expert public what they should think, anyways. Second, the issue that some physicists take with strings is (or should be, if one takes issue with anything) entirely related to unification, not the formal work which sheds light on other subjects. How does one take issue with this award, then, given that all of the recipients who work on string theory have used it for important not-unification-related purposes?

This is a serious question. Even if we disagree on unification, it's important to delineate between the different types of work done by string theorists. In my opinion, the animosity in the comments is unfair to direct towards these men.

And, if anyone thinks these men aren't deserving, please name who you think has done more significant work! I'm curious to hear ideas.

Cheers,
P

60. **Peter Woit**
August 1, 2012

P,

You're completely ignoring every argument I made here, so it's hard to see much point in repeating any of them. Again though, the basic point is that this is the largest award ever given to scientists, and it is being given explicitly for work that can't be experimentally tested, or has been tested and failed. The nine people given this award are a mixed bag, with very different accomplishments, but I think it's clear that if you gathered together a reasonable group of the most respected physicists in the world, and asked them to put together a list of the nine most important people in the subject, you wouldn't get six out of nine with a connection to string theory.

As for the animus about string theory, look at the previous posting. Last week, Maldacena and Witten were featured at a press conference promoting string theory in Munich. Did they acknowledge that string unification is not working out? No, instead they said all is fine, the field is making great progress, and made the absurd claim that string unification is testable because quantum mechanics and the SM are testable, and that's what the theory is based on. This kind of nonsense has been going on for 20 years, it has been really effective in misleading people about the true situation of string theory and making sure that string theorists get rewarded far beyond their due. If you want to know where the animus is coming from, that's it.

61. **MathPhys**
August 1, 2012

And, if anyone thinks these men aren't deserving, please name who you think has done more significant work! I'm curious to hear ideas.

The collective powerhouse of bright young people with no tenure, or with heavy teaching workloads, would greatly benefit from a fraction of 3 million dollars each, to allow them the opportunity to think in peace and to get some research done.

The great men of IAS and IHES have all the job security and the opportunity that they need.

62. **ex**
August 1, 2012

We just needed this prize, now that with some experimental data the community was starting to heal from decades of deviant thinking, of demagogic push for untenable arguments, of trivial or useless model building, of ignorance for the number of phenomenological open problems, of bad influence on students, of aggressive lobbying in career selection..

I guess that this prize is exactly what it is announced to be – a money prize for those who will never win a nobel physics prize. Let's hope they get their boat, and leave public recognition, personal satisfaction and happiness to Higgs.

63. **ex**
August 1, 2012

And it would be really nice to hear what Weinberg has to say about the selection and the fact that he is funnily on the Board, while not participating in it.

Weinberg has never been a critic of string theory, I guess for fear of being isolated by the community.

64. **David Derbes**
August 1, 2012

I want to disagree a little bit with the claim that the winners have no experimental evidence to back up their ideas. That may be true of the string folks, but it seems to me not true with respect to Alan Guth. Guth's work on inflation (in my opinion) has been confirmed by the size of the anisotropies measured by the Wilkinson MAP and by the harmonics predicted by Wayne Hu et al. I'm sorry that Alan Guth hasn't yet won a Nobel, and this Milner prize is nice consolation, well deserved. (For the record I had a undergraduate advanced mechanics class from Guth in 1973, during the all too short time he was at Princeton, and thought him one of the best teachers I'd ever seen. Forty years later that's still true.)

65. **anon**
August 1, 2012

It's interesting to compare the behavior of Simons and Milner. Simons gives out lots of small grants to young researchers in math — e.g. his 5000 travel grants are smaller than anything the NSF will give out, and are very useful for postocs. Although he has invested far more, and far more usefully, than Milner in science, he has (arguably) achieved less publicity. For example, Milner's prize was a prominent story in the NYT, whereas Simons had to take out an ad there to announce the Simons Fellows winners.

Milner's investment will probably not change science in any way: Witten, Kontsevich, etc. will continue doing the same great research but with a fatter bank balance (unless they decide themselves to donate some of this money to other, more needy, researchers). Simons has given many young researchers the time and money to pursue their research.

That said, I'm also surprised by the negative comments about the prize winners. If the title of the prize was changed to "mathematical and fundamental physics" no-one could possibly object to awarding it to Witten, Seiberg, Konstevich, etc. all of whom have made incredible contributions. All of their contributions are still great if you subtract anything to do with string theory.

66. **Bob Jones**
August 1, 2012

"there's \$3 million to be had if you work instead on a speculative and untestable idea that you see on TV."

"I think the correct term for this prize should be 'Milner's Prize for speculative thinking'."

I hope you guys aren't referring to the IAS physicists when you talk about "speculative" ideas. One of the things I admire about these physicists is that so much of their work is not speculative. As others have pointed out, much of their work is mathematical in nature and doesn't postulate anything about the real word. If you look at the citations on the website for the prize, you'll see that it's recognizing the laureates for a lot of perfectly legitimate things like

"the exploration of new mathematical structures in gauge theory scattering amplitudes"

"insights into a range of problems from high temperature nuclear matter to high temperature superconductors"

"exact analysis of supersymmetric quantum field theories"

"non perturbative duality symmetries"

I wouldn't say that any of these ideas are "speculative", and it makes no sense to say that they're "untestable". All of the IAS scientists have made outstanding contributions to science, and honestly, I think the sort of work they do is far more valuable than most of the testable phenomenology that other people work on. If theoretical physicists were just about coming up with testable hypotheses, it would be nothing more than a bunch of speculative and poorly motivated models, almost all of them wrong.

67. **Peter Woit**
August 1, 2012

Bob Jones,

There's no denying these 9 people have made significant contributions to science. These include however some of the most over-hyped work in the subject (among your examples, insights into "high temperature nuclear matter" is an example, going through all the things you left out of your list would provide many more). They have also benefited greatly from every reward academia has to offer, to some degree because of the huge hype level. Given this, the question is whether an order of magnitude jump in the rewards reaped for this sort of work is a healthy thing or not…

68. **ex**
August 1, 2012

Bob Jones – I think you have it in front of your eyes.

Higgs – Anderson – Weinberg – Glashow – Feynman – Wilson – Pontecorvo – Occhialini – Dirac – Majorana – Fermi – Bohr – Heisenberg – Einstein – Oort – Chandrasekar – Maxwell – Newton – Galileo .. a long and partial list.. All people leaving milestones in the comprehension of some physical phenomenon. Not technical analyses, but understanding of the dynamics, with a world of new phenomena coming out.

Please speculate like they did, but soon get reconnected like them to experiment, not to some unreachable very high energy frontier, which inevitably becomes obscure and demagogic. In this respect also inflation is at the boundary of being physics – we will never test it – we will never see the inflaton and its potential, with its 10^-20 small slope. Instead – we saw the higgs, we see the W, neutrino oscillations, Fermi interactions and so on.

69. **Sammy**
August 1, 2012

It is better to be 25, unknown and broke than 65, famous and worth $3m.

70. **Peter Woit**
August 1, 2012

I should also point out that I'm all in favor of work on speculative ideas, well aware that good ideas are often untestable for a long time if ever, and very much of the opinion that progress in physics needs more mathematics not less. The problem is the hype level surrounding string theory, which has brought huge attention to a very narrow range of ideas, coupled with a vigorous campaign to deny obvious failures. Hep-th has been full for nearly twenty years with papers on "non perturbative duality symmetries", leading to some results that are interesting, but often very much oversold, and no acknowledgement of the massive failure of the research program still used in the marketing of this work.

There's a universe of fascinating and poorly understood topics on the borders of fundamental physics and mathematics, with very little of it getting any attention, and with likely career suicide awaiting anyone who tries to work on many of these topics. Witten himself is responsible for a wide range of such ideas and he fully deserves the recognition and a huge prize, but the industry that developed around one aspect of his work doesn't. Young physicists need to be provided with financial support to try something new, not with millions of dollars dangled in front of them to encourage them to work in the same narrow, overhyped area.

71. **Tmark48**
August 1, 2012

@ Bob Jones :

—
I think a physics prize should reward physics accomplishments. Theoretical physicists have in past been awarded the Nobel Prize, so they arent' in any way penalised with respect to the rest of the physics community.
—
To be clear, I think we need theoretical physicists in all branches of physics (quantum optics, high energy physics, astronomy, astrophysics, condensed matter physics and so on…) and we also need a small number of theoretical physicists working on what I would call "highly speculative and non testable models" of reality.
But this second group has to know that while the community at large will recognise their accomplishments (if they have any), they don't deserve a physics prize.
Maybe a maths prize if the work they've done warrants it, but certainly not a physics prize. Nobody will say that Penrose or Hawking haven't done important discoveries in theoretical physics 50 years ago. Do I think they deserve the Nobel Prize in physics ? Not by a long shot. Unless someone comes up with an experiment that confirms that black holes actually evaporate.

72. **Raznol**
August 1, 2012

@Peter Woit: You say it's career suicide to work on these other topics.. how sure are you of this? Do you know of individuals who have done good work in less popular areas whose careers were ruined? I personally think it's more that people are afraid to try riskier routes, and moreover it's easier to travel down the paved road than to branch out into the wilderness, so the chance of success is lower and that deters people.
I'd prefer to stay anonymous here, but I've worked in pretty nonconventional areas (of math) and I didn't get much resistance. I don't think it hurt me careerwise. It always seemed to me that if you did good stuff in a different way, you'd get respected… just that most people were either unable to, or more commonly, not inclined to really try to.

73. **Bright Matter**
August 1, 2012

Yuri Milner approach to encourage fundamental physics is wrong because it fundamentally distorts the motivation and incentive for doing such research.

Fundamental physics is NOT a Silicon Valley tech startup. Where venture capitalists dump big money on big ideas to fund risky initiatives. Hoping for a big return from the marketplace.

Yuri Milner thinks by paying huge financial rewards for hot ideas, scientific discoveries will happen. No. Scientific discoveries happen because of two things: the ingenuity and dedication of the researchers, and the experiments to prove the theories. Paying for ideas, or even theories, without proof, is NOT science.

Researchers are already well-paid. Their reward is success of theories and recognitions. I can tell you none of them entered this most difficult field because of the money.

74. **Peter Woit**
August 1, 2012

Raznol,

I should have made clear that I was talking about careers in physics departments. Math departments are a very different story, with a different culture, and a better ratio between the number of smart people needing jobs and the number of jobs.

These days in physics, the job market for anyone doing anything involving sophisticated mathematics is dismal. If you apply for a post-doc or tenure-track job and are working on a short list of the hot topics pursued at the IAS (basically amplitudes, higher-spin gauge theory, or exact results in N=2 4d gauge theory), you have a slim chance of finding a job. Go through Witten's papers and pick any one of a large number of interesting topics that deserves to be followed up, then submit a job application saying that's what you want to work on. I think you'll then understand the "career suicide" remark.

75. **Sammy**
August 1, 2012

What is truly dismal is that PhD physicists cannot find jobs in high schools.

76. **Bob Jones**
August 1, 2012

Peter,

Fair enough. Do you think there's anyone else who deserves $3 million, or are you saying the whole idea of the prize is misguided?

77. **Bob Jones**
August 1, 2012

ex,

Your list might seem like a long one, but when you think about how many theoretical physicists there are, you realize that it's actually quite sort. Most theories of physics turn out to be wrong. My point was that if everyone in physics were doing phenomenological work, the subject would be more speculative, not less.

78. **Bob Jones**
August 1, 2012

Tmark48,

"But this second group has to know that while the community at large will recognise their accomplishments (if they have any), they don't deserve a physics prize. Maybe a maths prize if the work they've done warrants it, but certainly not a physics prize."

You may disagree with the choices that Milner made, but he can do whatever he wants with his money. If he wants to give a prize to a bunch of mathematical physicists, he's free to do it. If he wants to call it a physics prize, he's free to do that too.

"Do I think they deserve the Nobel Prize in physics ? Not by a long shot. Unless someone comes up with an experiment that confirms that black holes actually evaporate."

Should Hawking get the Nobel prize? Again, the prize committee can do whatever it wants. It doesn't matter what we think. Does Hawking's work deserve to be recognized as some of the most important physics work of the last 50 years? Absolutely. It makes no difference if we can't observe Hawking radiation because, like all of the other results I mentioned, Hawking radiation is a formal consequence of well tested theories of physics. It's a statement about what our theories predict, and such results are interesting and important even if they don't describe the real world.

79. **Peter Woit**
August 1, 2012

Bob Jones,

I think the whole idea of giving multi-million dollar prizes to the most prominent people in math and/or physics is misguided. There's no reason it should help with their work, and academia is already enough of a star system as it is. The sizable number of current $1 million-scale prizes (Nobel, Kavli, MacArthur, Crafoord, Kyoto, Shaw, Abel, Millenium, maybe others) seems more than enough, with the people who get them not exactly languishing in obscurity before they get the prize, or doing better work after getting it. Going to \$3 million seems unnecessary, other than to emphasize how wealthy the financier putting up the money is. He should stick to using his \$100 million house for this purpose.

80. **John Preskill**
August 1, 2012

@David Derbes: I also took that classical mechanics class from Alan Guth in 1973 … it was fabulous! He was the best physics lecturer I ever had aside from Sidney Coleman.

The criticism that the recipients already have all the freedom they need applies to any high profile prize doesn't it? Prize winners usually have very successful careers before they win their prizes.

One can always point to other worthy candidates, but these nine are very great scientists, and I'm sure they'll take seriously the responsibility of choosing the next winner. If someone is to chair the selection committee, Alan would be a good choice because he is so organized and conscientious.

81. **Tmark48**
August 1, 2012

@ Bob Jones :

Milner is abosolutely free to do as he pleases with his money. But I am free to criticise those that receive this "physics" prize. Now as a matter of principle I'm not opposed to awarding monetary prizes to theoretical physicists. But and this is were we have differing opinions, you think that any speculative idea is worthy of a prize, I on the other hand do not.

As for Hawking lets get real. Black hole radiation has been derived within a semi-classical model. And for good reason, no quantum theory of gravity exists at all. So what is in principle a quantum gravity effect has been derived by neglecting the important part. So we don't know wether this effet is real or not. General Relativity assumes space time is a 4 dimensional differentiable manifold. How can we assume that space at the planck scale is continuos ? It's a pretty ardous proposition with nothing to back it up.

82. **q**
August 1, 2012

Yuri Milner should give his prize to CMS and ATLAS people. As far as I know, there are 3000+ members in each team, so 27M divided by 6000 it is still a lot of money, all heavily working experimentalists absolutely deserve.

83. **Bernhard**
August 1, 2012

@ John Preskill:

"take seriously the responsibility of choosing the next winner. "

This is perhaps true, but although they are all, undeniably, excellent scientists, they are still humans and still in favor of their programs , which is only natural (after all one good reason they pursue their line of work is because they believe in it). You cannot choose 6 string theorists to decide "who did a great speculative job in physics" and expect the winners will not lean strongly to string theory. Physics has tons of models of physics in particle theory too, the vast majority of them (probably all) just as wrong as string theory. Again, this was not a prize for ideas, if it was, only the scientific community was in position to decide which ideas deserved a prize. Milner excluded from the equation the only thing that could make a scientific prize to have any credibility, the scientists.

84. **ex**
August 1, 2012

Bob Jones,
yes my list is short, exactly the ones I may think deserve any prize for "fundamental" physics.

On your point, I don;t know you should probably clarify what you mean by speculative, and whether it has negative meaning.

When Majorana was suggesting to Fermi new nuclear particles, neutrons, instead of a sum of proton+electron, do you think this was a useful or a useless speculation? It was really tied to ongoing experiments.

And when he was introducing neutral fermions being their own antiparticles, do you think this was useless speculation? To him, that was really a pure speculation, but in the same year ('37) it was recognized as possibly giving rise to neutrinoless double beta decay (measurable today).

The lesson I get is that you should speculate, but not escape in exotic constructions with weak or no motivation, which for its sustainment ends up needing mediatic overhype on the partial results, military occupation of academy, elimination of critic voices, etc – this reminds me of cultural fascism. It happens to societies, it happened in science.

I was discussing with collegues: the only sad question is why Weinberg had to follow them. He had other choices. He was a respected physicist with an impressive record of physics… I just compare with Anderson or Glashow – who did keep their mind clear.

85. **Anna**
August 1, 2012

First of all, it is worth noting that it is his money to do with as he pleases and it would be wonderful if more seriously wealthy people used their funds this way. Most of the complaints sound a bit like sour grapes. All contributions and rewards for research should be applauded – these are after all credible and serious researchers, not cranks.

Secondly, established funding mechanisms (government money decided by scientific peer review) are pretty good at excluding the patently crazy, but poor at picking out the high risk high return research which would largely be driven by very young researchers – decided as it is by what is effectively a gerontocracy. And so funding largely goes to the safe established middle. 'Big science' is making it harder for young people to do anything except join 'established' teams – it is no longer possible to come up with a revolutionary theory while sitting in a Swiss patent office and government funding is risk averse further pushing the safe middle option. Maybe the venture capital model is exactly the stimulus needed at this point in time for some radical thinking in the field.

Finally, there is space for speculative fundamental research – it is how great leaps forward occur, and whatever the success of the LHC in confirming the Higgs, no-one can deny that such a rethinking is required – unless Nature really is perverse. A null result is as important as a positive when that work has a feasible basis, is intellectually demanding and makes significant contributions to other fields. And whether a researcher picks a correct model or not is often a matter of luck and serendipity.

(P.S. I am not a string theorist.)

86. **John Preskill**
August 1, 2012

@ Bernhard: I concede that the prize might have more credibility if the initial winners had been chosen by a panel of scientists instead of by Yuri Milner. Regarding future winners, I'm inclined to give the selection committee the benefit of the doubt. I know all this year's recipients except for Kontsevich … not only are they extraordinarily deep thinkers, they are also pretty broad scientists with knowledge of and appreciation for physics outside of their specialties. Of course everyone has prejudices, but they will do their best to be fair.

87. **anonymous**
August 1, 2012

Nobody will say that Penrose or Hawking haven't done important discoveries in theoretical physics 50 years ago. Do I think they deserve the Nobel Prize in physics ? Not by a long shot. Unless someone comes up with an experiment that confirms that black holes actually evaporate."

I had predicted Penrose could share a Nobel prize with Schechtman and maybe one other person for their work on quasicrystals. When the Nobel committee award the prize to Schechtman alone this last year by taking the chemistry angle, they missed their opportunity. Oh well.

As for Hawking, the Nobel committee could lump him together with some of the [UBC team] or similar for a shared prize. These kinds of "fun" Nobel prizes aren't without precedent.

88. **Peter Shor**
August 1, 2012

Even in math departments, working in unpopular areas can be close to career suicide. I know at least one case of a good mathematician whose career nearly went down the tubes because he had a very good thesis in a very unpopular area (he managed to find a job, do some great work in a better known area after his thesis, and is doing very well now). However, in math, a much broader range of areas will get you a good job.

89. **anonII**
August 1, 2012

@piscator "Furthermore, history shows that the big names of one generation have not in general been good at picking the winners of the next."

Good point, I just read how Heisenberg told Hagen and Guralnik the Higgs Theory was "junk". Clearly he couldn't pick 'em.

Also, physics prizes in general will get a lot more attention in the next couple years as this prize announces its second set of winners and the Nobel Prize has to deal with how to handle the 5 remaining Higgs theorists (E-H-G-H-K).

90. **Truth**
August 1, 2012

A wonderful prize and a wonderful selection of truly worthy winners…deep, creative, imaginative thinkers. They have taught us a lot about deep properties of general relativity and quantum mechanics in strange and interesting ways. Some of their ideas are definitely correct, such as quantum computing, others have been rather well tested, such as inflation, others are surely connected to our world in one way or another, such as gauge/gravity duality, other ideas generate beautiful connections between physics and mathematics, such as formal string theory, and other ideas are just rather speculative, such as large extra dimensions.

"Imagination is more important than knowledge" Albert Einstein.

The only odd thing in my mind is that he decided to give \$27 million to 9 physicists. Why not \$27 million to 100 physicists, or so? Share the love…Wonderful nonetheless.

91. **Anonyrat**
August 1, 2012

The real worry is not about these 9 recipients, it is about the effect of this prize on every other physicist.

92. **Peter Woit**
August 1, 2012

Thanks to all for the unusually high quality of comments here. Some even are from people (John Preskill and Peter Shor) of high accomplishment who seem to me viable candidates to join the "next-to-happiness flotilla" if the committee does as good a job as one would expect.

Perhaps there are other such here among those who prefer to stay anonymous. I'm guessing for example "Truth" might be in a category where this would work out if the numbers were expanded as he (or she?) suggests. In any case, good luck to all of us in this new world we've entered where, as one venture capitalist described the situation to me "financiers are handing out \$3 million checks to scientists like so much candy." As our economy evolves into one dominated by the activities of a sizable group of multi-billionaires, often with math/physics background, they can't spend it all on yachts, mansions and jets, so surely even more will come our way.

93. **Low Math, Meekly Interacting**
August 1, 2012

Interesting thing about the Nobel: Almost no one talks about the money. As for the Milner Prize, money is all anyone is talking about.

94. **Michael Kovarik**
August 1, 2012

I don't believe that this is going to negatively impact the physics community. It won't combat the fact that the American job prospects for those who research quantum gravity has never been worse and it is given to such an exclusive club that it won't propel any research in any direction (no one does physics research for exorbitant prize monies). Perhaps if Yuri had split that \$5 million into 1,000 micro-grants, it might be a problem.

95. **Bob Jones**
August 1, 2012

ex,

When I say a theory is speculative, I mean there's a good chance it's wrong. For example, I would say that most of the theories predicting BSM physics at the LHC are highly speculative. On the other hand, the sort of formal string theory that people at the IAS are doing is not speculative at all because it's not the sort of thing that requires testing. Such ideas are known to be correct because they have been proven mathematically (at least by physicists' standards).

I would prefer to see more well motivated mathematical work, rather than millions of badly motivated predictions of LHC physics.

96. **Joydip Ghosh**
August 2, 2012

Alexei Kitaev — For the theoretical idea of implementing robust quantum memories and fault-tolerant quantum computation using topological quantum phases with anyons and unpaired Majorana modes.

@ John Preskill: Happy to see Kitaev as one of the laureates. Since you are perhaps the best person to talk about his research, I have a question for you: It's not clear to me from the above if he got that for topological quantum computation (with non-abelian anyons) or topological quantum error correction (with surface code).

97. **Raisonator**
August 2, 2012

I can't help myself, but there seems to be a certain bias towards people from English- or Russian speaking countries – or at least they work in such countries.
Yet I do fully appreciate the work of these 9 people and think that they have well deserved the prize.

98. **ex**
August 2, 2012

..On the other hand, the sort of formal string theory that people at the IAS are doing is not speculative at all because it's not the sort of thing that requires testing. ..

I would prefer to see more well motivated mathematical work, rather than millions of badly motivated predictions of LHC physics.

Ok, this is the point – very well, but I believe that may be mathematics or mathematical physics, but it is not physics, and it is not fundamental physics.

Thus, it should not be sold as such, let aside the purported "final theories", "elegant universes" or whatever. All these things already said, of course.

99. **piscator**
August 2, 2012

To those who think this is good news:

The issue is not that the awardees are not worthy of recognition. They are all good people who have done good work. Some I think are truly outstanding who have contributed continually across many areas, some I think were in the right place at the right time, some have extraordinary personal charisma and salesmanship. But they are all good people.

The big issue I have is when visualising the following type of scenario. Laureate X is on a committee handing out postdoctoral fellowships with Y who is not a laureate but could credibly be one. One of the fellowship candidates A is a student of X. Does the existence of this award make it more or less likely that Y will speak honestly about A?

Furthermore, it is clear from the history of science that brilliant people can also have ideas that are cranky or just plain wrong. The scientific ideal – again, hard to realise because scientists are people too – is that no matter who they are, other people can tell them that they are talking bollox. The existence of such a prize with this recurring structure only serves to act against and not for this.

It is of course worth reiterating that 3M is a lot of money for academics, even those with tenure at the IAS. The IAS is private, but if anyone doubts this you can look up online salaries of comparable people at public institutions, eg David Gross in the UC system.

100. **Rene Benthien**
August 2, 2012

This harkens back the days in the renaissance when the wealthy elite of the Italian city states, the Medici and the Sforzas, were commissioning the great works of Bernini and Michelangelo. To the time when the Dutch Royals and the merchants of Amsterdam patronised the works of Rembrandt or Van Dyke. An era where the Kings, nobles and bishops felt it necessary to fund the work of Copernicus, Brahe and other great philosophers.

There was a time when the reputation of a man, his pedigree and refinement, was based on his appreciation for and the encouragement of the arts and the philosophies.

In today's world where it seems the only thing that matters is the balance in someone's bank account, I think it is great that someone feels that the value of scientists and the work that they do be held at a greater esteem in society than it is currently.

101. **Clayton Pickering**
August 2, 2012

Low Math, Meekly Interacting said:

"Interesting thing about the Nobel: Almost no one talks about the money. As for the Milner Prize, money is all anyone is talking about."

Apropos, Low Math. Perhaps, as a suggestion, those who have won the three million dollar prize should do what Milner has done. Take 100, 000 dollars and find a big idea by some unknown. Now that's a 9 to 1 ratio whereby you folks empower yourselves.

102. **Peter Woit**
August 2, 2012

Clayton,

The four winners from the IAS already completely control about 20 research positions at the IAS, which typically pay more than 100K over the time the person is there. To the extent that there are "unknowns" with a "big idea" that meet their approval, they've always had plenty of jobs to hand out to them (although I guess one big advantage of a 100k check versus the job is that you wouldn't have to live in Princeton…)

103. **Peter Woit**
August 2, 2012

Rene,

I'm missing how depositing \$3 million checks in the bank accounts of successful academics fights the attitude that the only thing that matters is the size of one's bank account. Seems to me it reinforces that attitude.

And the idea of reverting to the world of the Medicis, with hedge funds and Russian oligarchs playing their role, also is not obviously progress.

104. **John Preskill**
August 2, 2012

@Joydip Ghosh: Kitaev's 1997 paper discussed too related ideas — using surface codes (systems supporting abelian anyons) for robust quantum memory, and using systems supporting nonabelian anyons for both robust quantum memory and fault-tolerant quantum computing. Both are great ideas and have had a huge impact on quantum computation research.

I think the nonabelian version was the deeper and more original idea, but surface codes might wind up being more useful for future quantum technologies. My reading of the prize citation is that the proposal to exploit nonabelian anyons is being recognized, but I agree it is not completely clear.

Actually, both ideas are quite timely, in view of the evidence for Majorana fermions in quantum wires reported this year by the Delft group, and the recent excitement about realizing surface codes using superconducting circuits (e.g. work by the UCSB and IBM groups).

105. **Low Math, Meekly Interacting**
August 2, 2012

But is it "the value of science", or the vainglorious act of a prestige-hungry Russian oligarch who likes to swing his fiduciary dick around. I myself don't know the answer to that question, but I think the answer matters.

106. **Low Math, Meekly Interacting**
August 2, 2012

I should say I hold the ricipients blameless for Milner's failings, real or perceived. And I don't think they should have refused the cash. There's controversy to this day about the value of the Nobel, despite its venerable status. No reason to use kid gloves on the Milner Prize, such as it is.

107. **Clayton Pickering**
August 2, 2012

"The four winners from the IAS already completely control about 20 research positions at the IAS, which typically pay more than 100K over the time the person is there. To the extent that there are "unknowns" with a "big idea" that meet their approval, they've always had plenty of jobs to hand out to them (although I guess one big advantage of a 100k check versus the job is that you wouldn't have to live in Princeton…)"

Understood, Peter. However, are these monies approved and overseen by the university? The largesse given by Milner is private. Correct? Does Milner have oversight with the monies allocated? The whole point of the three million dollar gift is for those recipients to have a degree of autonomy to explore new concepts by fostering independent research through THEIR own allocation. As you are aware many of funds and grants for R & D are drying up. My point is that physicists can pick up the ball and run with projects outside the purview of universities. Is this done?

108. **Trulo**
August 2, 2012

It's going to be interesting to see who's going to get the prize in the next ten years or so. If six or seven prizes, say, are awarded every year, the number of candidates with achievements even remotely comparable to Witten's is going to decrease exponentially. Will they have repeat laureates then?

It could be also interesting to see how many of the laureates in the next ten years (perhaps a bunch of between 50 and 100 people) will take the opportunity to quit working. Once a laureate has pocketed the money, there's no reason why he/she could not decide to retire and live as a rentier.

Finally, I assume there's a small crowd of people who may now feel the temptation to max out all of their credit cards, knowing that next year they will be rich. Seen who's in the committee, it's not hard to imagine that D-branes, certain types of extra-dimensional models, F-theory, and the like, are going to get a lot of monetary appreciation in the next few years.

109. **Trulo**
August 2, 2012

Sorry about the typo. It should be "Seeing who is…" not "Seen who is…"

110. **Peter Woit**
August 2, 2012

Clayton,

The IAS is not a university, it's a private research institution. The four IAS particle theory professors determine who the institution hires, and they can hire anyone they want. They have a lot of positions, and each year they spend a lot of time sitting down and deciding who to choose. I don't see any reason at all why they should choose different people to give Milner money to versus the IAS money they are responsible for distributing. The only difference is that the IAS money comes with a requirement that you actually spend your time in Princeton.

111. **Peter Woit**
August 2, 2012

Trulo,

It seems the plan is to award typically only one \$3 million prize each year, the nine awards at once was just to get the thing started.

112. **Trulo**
August 2, 2012

> It seems the plan is to award typically only one \$3 million prize each year,

Yes, I think you're right. The rules don't say so explicitly, as far as I can tell, but the statement "can be shared by any number of people" suggests that's how it's going to work in the future.

113. **Clayton Pickering**
August 2, 2012

Yes, Peter. I am well aware that IAS is not a university. However, your point is that IAS money requires that a person spends it time at Princeton. The university is still the anchor. Private money frees the professors from this obligation. Milner's prize does the same thing as far as I am aware.

114. **Michel Audiard**
August 2, 2012

120 commentaires !
Quand on parle pognon, à partir d'un certain chiffre, tout le monde écoute.

115. **Peter Woit**
August 2, 2012

Michel,

Yes, if Milner had set up an award with a medal and \$10,000, no one would have paid any attention at all.

I just added a link to the original posting to a news story which refers to the prize as the "Physics highest honour", which I guess is how the Milner Prize will often be described and thought of.

116. **R**
August 2, 2012

I work on an experiment which is currently being built to try and answer some (testable) questions in "fundamental" particle physics. (Though our research once got called "uninteresting" by one of the Serious Men on this list.) We're facing a budget shortfall somewhere in the range of \\$3 million dollars. I guess the only solution is to make friends with some more grad school dropout Russian oligarchs…

117. **Bernhard**
August 2, 2012

Peter,

I don´t believe (or perhaps don´t want to) people (at least not physicists) will compare the honor of getting a Nobel prize to this "tons of cash prize chosen by some rich guy". I think the Indian newspaper was simply pushing it in a nationalistic gesture.

I think the Milner prize will become a string theorist prize (time will tell), with very rare exceptions, which is maybe a good idea since string theory, being separated from science, indeed deserves its own separate prize.

118. **Peter Woit**
August 2, 2012

Bernhard,

Well, in future years, the Milner prize will have the imprimatur of a group of very prestigious physicists, and I'm sure they will choose other very prominent people, not always string theorists. It won't have the history and name recognition of the Nobel, but will be something more like the Shaw prize, except x 3. I think Milner knew exactly what he was doing when he set the prize level above any other prize, significantly so. That will make sure his prize gets reported and paid attention to, with "highest prize/honor in science" often the way it is reported.

The only thing that will change this will be when some other financier or oligarch institutes a \\$5 million prize….

119. **Z**
August 2, 2012

I can see this prize being a positive for other theory areas. Now funding agencies have another excuse to shift fundamental science grant money away from string theory.

120. **Jiav**
August 2, 2012

Congrats to the winners and many thanks to Yuri Milner for making famous several outstanding scientists.

121. **srp**
August 2, 2012

Maybe the committee of Milner winners can branch out a little and ask their colleagues in other physics fields if there are any worthy recipients. Given the relative lack of publicity for condensed-matter and nuclear physicists, for all I know there could be more-obscure deserving folks whom their peers see as water-walkers.

122. **perelmanfan**
August 3, 2012

Time for a Perelman prize awarded to scientists who refuse prizes above let's say 100K.

123. **David Rod**
August 3, 2012

There is an interesting article in Canada's The Globe and Mail (Friday, August 2), Report on Business Section, Page B2, by Chrystia Freeland (editor, Thomson Reuters Digital) about Yuri Milner's bequests. It is titled "Beyond the Big Bang: A Russian billionaire's investment". This article probably appears in other newspapers. Worth reading.

124. **MathPhys**
August 4, 2012

There was a time when we were all in awe of the Clay Inst prizes. Now that's small money. Don't think about the millennium problems, young man. Go for the real thing.

125. **Bob Levine**
August 4, 2012

I may be missing something, but I don't really see the logic of this excerpt from Mazzucato's piece. It's *still* the status of of string theory that, Milner or no Milner, it is not a Nobel-able field of research, for exactly the reasons he's stated, and, 3M or no 3M, the Nobel is still, and almost certainly evermore shall be, the highest aspiration of theoretical physicists—we're not going to talk about 'Milner Laureates', I suspect, no matter how many zillions he winds up giving away. Winning a Milner will never let you claim that you walk in the ultra-exalted company that LM himself alludes to: Planck, Einstein, Born, Feynman and the other immortals, and the 'wet dream' of all those grad students, in LM's ugly phrase, is exactly that walk, not the bucks that go with that prize, or with anything else (here's a thought experiment: imagine asking 100 physics grad students whether they'd rather (i) win a Nobel prize, or (ii) get a university position that, surreally, paid them five times annually what the Nobel pays—which set of responses would you want to bet on as the majority vote here?) Three million dollars makes you a multimillionaire; it *doesn't* make you a titan in the history of physics, which is I strongly suspect is what any healthily ambitious young physicist really wants.

Of *course* there's plenty to complain about so far as the Nobel is concerned: the names Jocelyn Bell and Rosalind Franklin are a good place to start, and let's not even get into the issue of the Peace prize. But the arguments take place precisely because of the cachet that the Nobel, unique among science prizes, has earned (a large chunk of which comes from the admittedly imperfect way in which Nobel laureates are themselves selected)—ultimately, from the epochal stature in the various Nobel disciplines that, by consensus, is conferred by each award. I can't see the Milner cash dump ever earning anything like that halo.

126. **Mitchell Porter**
August 5, 2012

perelmanfan: "Time for a Perelman prize awarded to scientists who refuse prizes above let's say 100K."

This is a cute idea… A Perelman prize can be a stipend of 60 euros a month (see lun's comment at the start of the thread). Milner's \$3 million per year would then translate to over 3000 Perelman prizes. Milner could support everyone who posts to vixra and still have plenty left over.

127. **Lufgang**
August 5, 2012

I happen to know Yuri Milner well and wanted to make a few comments.

1. He did work for Khodorkovsky in the 90s for a couple of years, but only as one of the managers and never made significant money at that time.

2. Yuri first significant capital came from Mail.ru, the largest Russian Internet portal that he founded in 2000, but not before he took it public in London in 2010.

3. In addition to that, he made a few early bets on companies like Facebook, Groupon and Zynga as an investor in 2009-2010. All these companies also went public in 2011-2012.

Peter, thank you for keeping the conversation fair.

128. **Tmark48**
August 5, 2012

What happens when one of the speculative ideas that has been awarded is falsified through experiment in 10, 20 or 30 years* ? Will they give the prize back ? This is why I think it makes no sense to award speculative ideas in physics. This problem doesn't exist with the Nobel Prize because the Nobel committee makes damn sure that the prize goes to a real physical discovery.
Lets be honest and call the Milner Prize a mathematics prize, in this sense wether or not physics turns out to be correct, the mathematics still stands on its own.
I don't know, I have the impression its all a big joke.

* or maybe it becomes dogma so no one shall ever question its experimental valdity.

129. **Clayton Pickering**
August 5, 2012

"R" wrote:

"I work on an experiment which is currently being built to try and answer some (testable) questions in "fundamental" particle physics. (Though our research once got called "uninteresting" by one of the Serious Men on this list.) We're facing a budget shortfall somewhere in the range of \$3 million dollars. I guess the only solution is to make friends with some more grad school dropout Russian oligarchs…"

Who would ever have thought years after the Cold War that a Russian oligarch would be awarding monies to some Americans and the institutions they are associated with. I wonder if Michio Kaku would call this Type I economic largess?

130. **loopy**
August 5, 2012

re: Fundamental Physics Prize

Since several string theorists have received the prize, do you think Loop quantum gravity theorists (Abay Ashketar), noncommutive geometry (Alain Connes) should also receive a prize, in future years?

131. **Peter Woit**
August 5, 2012

loopy,

I think by publishing “The Trouble with Physics” Lee Smolin ensured that the field of LQG research won’t be in line for any prizes where decisions about awards are made by leading string theorists.

It will be interesting to see which direction the prize committee goes in the future. Since the prize is new, and its description is fairly broad, including mathematics as well as physics, they have a large number of plausible candidates to consider, with Connes one possibility.

132. **Bob Jones**
August 5, 2012

I’m sure that Alain Connes will get this prize eventually. I was actually surprised that he was not one of this year’s recipients…

133. **Bernhard**
August 5, 2012

“It tells us that standard model and string theory are correct”

OMFG, these guys have really absolutely no respect for anybody else´s intelligence? Are they serious?

My only consolation to these absolutely ridiculous claims is that this is being recorded for history and that future physicists will know how much full of crap these guys are. Sorry to spit my guts here, but the audacity of these guys never end and I just can´t understand why is not all community laughing about this.

Kane is not alone, after all.

134. **M**
August 6, 2012

“It tells us that standard model and string theory are correct”.

Ironic science needs a good sense of humor.

135. **Tmark48**
August 6, 2012

“It tells us that standard model and **string theory are correct”**.
—

Ok, maybe we should award these guys some prize. What about the Ig Nobel ?
Maybe not, even the Ig Nobel requires a scientific achievement, as trivial as it is. ^_^

136. **john smith**
August 8, 2012

I think that giving a three- million-dollar prize for string theory research undervalues what science is all about. It would have been much better if this money had been spent in raising the quality of education in our elementary schools and high schools where our real “fresh minds” are, and are in need of such money.
Giving such a prize to IAS professors (or any other group of well-established scholars) will not foster more fundamental research; rather it will distract them to think how they should spend such a large sum of cash.
There was a reason why Perelman didn’t accept the Fields medal or the Clay millennium prize. Think about it.

137. **john smith**
August 8, 2012

I just read this in the nature article. So I guess I’m right.

> Andrei Linde, a cosmologist at Stanford University in California, found out about his good fortune only late last week. How will he spend his money? “This is a problem that is much more complicated than the physics problems I’m trying to solve,” he says.

138. **Bugsy**
August 8, 2012

Though the money could arguably have been better spent, ala Simons, I’m not too worried. My guess (and hope) is the winners will use the main part of the money where it can really do some good scientifically (as in, supporting promising young researchers in whatever part of physics or math, not necessarily just in their specialty). However funding more post-docs really won’t help; what is needed are permanent positions, and also grants which give a few years of teaching relief for folks (young or not so) doing good work and already with positions somewhere but overloaded with obligations. Also research grants could ease financial burdens on those struggling on that account. I think the winners will feel embarrassed if they use it only to buy a big house or a yacht with a submarine for themselves, as everyone will be watching what they do. In that sense it’s not really free money; since it is a very public prize, unlike with lesser grants, there will be peer pressure to do the right thing. Besides, these folks already have all they need in terms of working conditions; if they use the money for extraneous things it will only divert their time and energy from what they care about most, which is their work.

139. **David Roberts**
August 8, 2012

@Tmark48:

“Lets be honest and call the Milner Prize a mathematics prize…”

the problem is, most people on that list are not doing mathematics. When Witten got the Fields medal it was a small scandal as he was doing mathematical physics – I’m reading his relevant late-80’s paper on Chern-Simons theory and knots, and it is definitely not a mathematics paper, it’s all theoretical physics intuition with bits and pieces taken from mathematics (and, I freely admit, a great achievement).

140. **Peter Woit**
August 8, 2012

David,

I agree. Kontsevich writes math papers, and Witten is sui generis, but the other seven haven’t done much of anything that mathematicians would recognize as significant mathematical advances. In the case of Witten’s Chern-Simons paper, it may not have looked like a math paper, but unexpected and amazing relationships between quite different areas of mathematics (knot invariants, gauge theory, representations of affine Lie algebras, algebraic geometry of moduli spaces) could readily be extracted from what he was saying. Not everyone was convinced at the time that this deserved a Fields medal, but a few years later, when he wrote down the Seiberg-Witten equations, revolutionizing the subject of 4-manifold topology, the doubters were quieted.

141. **Tmark48**
August 10, 2012

@ David Roberts : I think it’s safe to say that mathematical physics tends more towards the mathematical side than the physics side. Physics can guide you, but in the end if you’re to make clear and mathematically unambigous and rigorous the operations (tools, equations etc..) in question you’re going to have to resort to what mathematicians do.
Theoretical physicists don’t really concern themselves with such trivialities. Can you imagine physicists going into metaphysical mode about wether or not the Dirac function is a well definied concept ? Or the Feynman path integral ? It works, it gives us the means to formulate predictions that can be verified. It passes the test. Who cares wether you can really do infinite dimensional integrals in infinite dimensional spaces ? Mathematical physicists care, mathematicians may care, theoretical physicists by and large don’t.
The scandal that you’re mentioning has more to do with a certain “narrow mindedness” of the mathematical community. Mathematical physics is like a fish out of its element. Not totally in the mathematical camp, not totally in the physics camp.

142. **Bernhard**
August 20, 2012

This interview was a bit interesting:

http://ibnlive.in.com/news/science-neatly-strung-together/283719-60-121.html

Among other things, Ashoke Sen says:

“To this I should add that while string theory naturally combines gravity with quantum mechanics, one of its goals is to also explain the theory of other forces and other matter. For this one needs to establish that for the kind of energies which the present accelerators can produce, string theory can be approximated by the standard model of particle physics. There are strong indications that this might be possible, but this has not been proven. This is at present one of the most active areas of research in string theory (and is commonly described as string phenomenology). If successful, this program will automatically explain the origin of the Higgs boson in string theory.”

143. **Peter Woit**
August 20, 2012

Bernhard,

Well that’s better than the previous claim that the Higgs discovery “tells us that standard model and string theory are correct”. He’s right that “if successful”, string theory unification would explain everything, although he’s neglecting to mention that it has so far been a dismal failure…

Interview(s) with Vladimir Voevodsky

August 3, 2012
Categories: Uncategorized

Vladimir Voevodsky is a mathematics professor at the IAS in Princeton, most famous for his proof of the Bloch-Kato conjecture, work which won him a Fields Medal in 2002. This conjecture relates the K-theory of fields and their étale cohomology (note that there are other, different, Bloch-Kato conjectures on special values of L-functions). For a description of Voevodsky's ideas from 2002, see this by Soulé. The proof of Bloch-Kato was only finished later, including work by other people, for more about this see Weibel's lectures on the proof, or Voevodsky's talk at the IHES conference honoring Grothendieck. For a popular talk by Voevodsky, see "An Intuitive Introduction to Motivic Homotopy Theory", video here, write-up here.

Voevodsky has had a somewhat unusual career, for an interview from 2002 where he discusses his early years in Moscow and at Harvard, see here. A recent interview with him by Roman Mikhailov in two parts has appeared (in Russian, I'm relying on Google Translate to get the gist of it) here and here. He describes what appear to be various delusional episodes, especially during a period in 2006 and 2007 when he was unable to work.

In recent years he has moved away from his work on K-theory, towards topics in applied math (for a while he was investigating population genetics) and foundations of mathematics. This year the IAS will run a year-long program he is organizing on what he calls Univalent Foundations of Mathematics. Back in 2010 he gave a popular talk at the IAS, entitled What if Current Foundations of Mathematics are Inconsistent?

Comments

1. **Yuri Danoyan**
   August 3, 2012

   Interview with Vladimir Voevodsky in Russian contain some mystical stuff
   http://baaltii1.livejournal.com/200269.html
   Very interesting…

2. **Alex R**
   August 3, 2012

   Anyone interested in Voedvosky's foundational work, or who has viewed the video of his talk in 2010, should also take a look at the discussions on the FOM email list in May of last year, which included much discussion of this talk and his foundational views. One can start with this post, if one likes:
   http://www.cs.nyu.edu/pipermail/fom/2011-May/015407.html

3. **P.**
   August 3, 2012

   What's the advantage of basing the foundations on homotopy theory?

4. **plm**
   August 3, 2012

   I think it is more type theory itself which has applications in automated theorem proving. The main general theorem proving environment is Coq, I think, based on type theory, and which can interface well with human-style proofs, because of the ease of making definitions.

   I also think the connection to homotopy theory is not very useful in applications yet, it is more something interesting to explore mathematically. And it seems to provide a natural framework for the connection between logic and algebraic geometry/topology. So perhaps we can say it is useful psychologically, it clarifies our ideas of what is possible.

   This is a quote from Awodey's survey on the subject:
   "The homotopy interpretation of Martin-Löf type theory into Quillen model categories, and the related results on type-theoretic construc- tions of higher groupoids, are analogous to the basic results inter- preting extensional type theory and higher-order logic in (1-) toposes. They clearly indicate that the logic of higher toposes—i.e., the logic of homotopy—is, rather remarkably, a form of intensional type theory."

   (I only comment this because nobody replied. If someone can make a real answer instead, please do.)

5. **Igor Khavkine**
   August 4, 2012

   The mystical bent of the two part interview with Mikhailov is somewhat shocking. For those who might be curious, Voevodsky himself takes part in the extensive discussion attached to the interviews under the screen name vividha. On, the other hand, from what I can tell, the mystical and mathematical parts appear to be disjoint for Voevodsky himself.

6. **wesolyromek**
   August 5, 2012

   http://www.wired.com/business/2012/08/ff_wallstreet_trading/all/

   Deserves a separate note.

7. **Urs Schreiber**
   August 12, 2012

   P. asked: "What's the advantage of basing the foundations on homotopy theory?"

   From the point of view of formal logic it makes the whole theory more coherent to regard also the notion equality "constructively", hence to consider intensional identity types and thus homotopy type theory. See

   http://ncatlab.org/nlab/show/homotopy%20type%20theory for more. What is remarkable, and this was Voevodsky’s insight, is that with this assumed the formal logic is automatically one that describes homotopy theory in its modern guise of “infinity-category theory”. This is remarkable since the latter is seen more and more to be of fundamental relevance in many areas of mathematics.

And of fundamental physics. Adding one extra axiom to Voevodsky’s homotopy type theory, that of “cohesion”

http://ncatlab.org/nlab/show/cohesive%20homotopy%20type%20theory

makes a large chunk of aspects of quantum field theory appear pretty straightforwardly from the formal logic itself, see

http://ncatlab.org/schreiber/show/differential+cohomology+in+a+cohesive+topos .

8. **Urs Schreiber**
August 20, 2012

Some more dedicated discussion of the relation between _Quantum gauge field theory in Cohesive homotopy type theory_ is now here

http://ncatlab.org/schreiber/show/Quantum+gauge+field+theory+in+Cohesive+homotopy+type+theory

Grigori Perelman, the Movie

August 5, 2012
Categories: Uncategorized

I've checked the date on this, and it's not April 1, so maybe this is actually true. According to the website of the Russian television news network RT, [James Cameron to produce story of reclusive Russian genius](#):

> Celebrated Russian mathematician Grigory Perelman, the man who solved a century-old problem then turned down a $1 million prize, is to be the subject of a Hollywood movie produced by James Cameron.
>
> Throughout his career, Perelman has made several breakthroughs in mathematics, geometry and topology. And though Perelman is known for leading a secluded life and avoiding journalists, he agreed to participate in the project dedicated to his milestone achievements. Avatar and Titanic creator James Cameron is rumored to be producing the movie.
>
> Israeli producer Aleksandr Zabrovsky told KP Daily that it took him three years to convince Perelman to sign on to the project. He then approached Cameron with the idea, who was reportedly enthusiastic about it.
>
> The film is due to be shot in the US, with an as-yet-unnamed professional actor covering the role of Perelman.

I'm finding it hard to figure out how Hollywood will dramatize the story of reclusively thinking about the Ricci-flow equations for seven years or so. I guess it will all be in the special effects, for which Cameron is famous.

One should perhaps take this with a large grain of salt, considering the following (from the [Wikipedia Perelman entry](#)):

> In April 2011 Aleksandr Zabrovsky, producer of "President-Film" studio, claimed to have held an interview with Perelman and agreed to shoot a film about him, under the tentative title The Formula of the Universe. Zabrovsky says that in the interview, Perelman explained why he rejected the one million dollar prize.
>
> A number of journalists believe that Zabrovky's interview is most likely a fake, pointing to contradictions in statements supposedly made by Perelman.

Comments

1. **Richard**
   August 5, 2012

   I'm still waiting for a movie about E. Galois!

2. **Armin Nikkhah Shirazi**
   August 6, 2012

   I don't think this is very well known, but James Cameron studied physics before he dropped out and went his own way. And if one thinks that the life of a mathematician thinking about highly abstract issues cannot be effectively dramatized, just remember 'A Beautiful Mind'.

   In fact, if this is true, it is really refreshing to have a movie about someone like that than the gazillion movies about artists, sports figures, business people and politicians. There are not very many people in this and related fields known to the general public, and think what one will about Perelman's idiosyncrasies, the ideals that he is trying to uphold and the significance of his work are worth knowing by more people.

   And yes, Galois' life would definitely be extremely well-suited for dramatization.

3. **Armin Nikkhah Shirazi**
   August 6, 2012

   Also, I just noticed an amusing sentence on the same RT page you linked:

   "The CERN laboratory in Switzerland has found a flaw in an experiment that was set to prove Albert Einstein's social theory of relativity wrong."

4. **Giotis**
   August 6, 2012

   I think his life has already been dramatized:

   http://www.youtube.com/watch?v=DVpmkRwVRfs

   Press cc for english subtitles…

5. **Tmark48**
   August 6, 2012

   @ Richard : Spot on. Galois is such an amazing character, would love to see a modern film about him. I'd prefer some french director to lead the project, there is no need to overdramatize his life which in fact was something surreal. Hollywood might just go too far and jump the shark. Oh and what about Fourier or even Grothendieck ?

6. **Thomas Larsson**
   August 6, 2012

   Some other possibilities:
   Majorana – a mystery.
   Planck – a tragedy.
   Beurling – a thriller.
   Teichmüller – a villain.

7. **Stefano**

August 6, 2012

James Cameron defamed the heroic William McMaster Murdoch in his Titanic movie, then as reparation offered a few THOUSAND dollars to a charitable prize in Murdoch's name, see http://en.wikipedia.org/wiki/William_McMaster_Murdoch for more information. Any movies by Cameron, on any topic, should be shunned by people of conscience.

8. **Navneeth**
August 6, 2012

And if one thinks that the life of a mathematician thinking about highly abstract issues cannot be effectively dramatized, just remember 'A Beautiful Mind'.

But John Nash's life had a nice "curve" (and a detailed biography) which could be further bent and shaped to fit the scaffolding of a Hollywood movie.

Here you have Perelman: a great mathematical achievement under his belt, yes; but too much with which to trouble the general public. You then knock on the door to his mother's apartment to find out more about the man (which is what will be central to the film) and he doesn't open it.

9. **Cesar Laia**
August 6, 2012

It's my opinion, but 'A Beautiful Mind' (even winning several oscars) is not a good movie.

James Cameron did good movies many years ago, and I still regard "Terminator" as is best. Probably wants to show that he can also do non-action movies… who knows, he still is a great director, even considering that "Avatar" sucks. If I was a film director, I would go for Majorana. Or Heisenberg and Bohr (it would be a great plot).

10. **Trulo**
August 6, 2012

Cesar, I agree… Having read the extremely detailed and well documented biography of Nash before watching the movie, I found the latter shallow and boring. Personally, I'd not be interested in a movie on Perelman at all. Now, a biography by Sylvia Nassar or someone equally talented, that would be most interesting. But I guess Perelman is still too young for a biography, which would probably need to be added several chapters twenty years from now.

11. **tristes_tigres**
August 6, 2012

The real question is who's badas enough to play the lead: Chuck Norris, Rutger Hauer or Harrison Ford?

12. **Blendletan**
August 6, 2012

I always thought the life of Grothendieck would make a more interesting move….

13. **Peter Woit**
August 6, 2012

Trulo,

I've been reminded that there is a biography of Perelman, Masha Gessen's book Perfect Rigor. For more about the book, see

http://www.math.columbia.edu/~woit/wordpress/?p=2435

14. **Rikki**
August 6, 2012

I would definitely pay to see this film. However, I doubt it will happen. While we're suggesting potentially great subjects for biographical films, I can look no further than the life of J. Robert Oppenheimer.

15. **Tim May**
August 7, 2012

There've been at least two films or miniseries about Oppenheimer. One was within the past 20 years, the other was with Sam Waterston ("Law and Order") as RO in a long 1980 miniseries.

Personally, I found neither of them very compelling. Nuclear scientist, conflicts about warfare, conflicts about loyalty to U.S. versus loyalty to world peace and communism, blah blah.

I liked "A Beautiful Mind" because of its abstract focus, along with the psycho. aspects. It was well-handled by R. Howard, but most directors would go for the money shots.

A film about Grothendieck would probably have mostly scenes of him teaching in Hanoi suburbs as bombs fell, perhaps with him (fictionally) meeting Jane Fonda in a gun emplacement. Blech.

A film about Perelman would, my guess, be mostly a riff on "Rain Man."

Not to sound too negative, nor too sympathetic toward strings, but I'd really rather watch Nima Arkani-Hamed's 5-part Messenger Lectures for a third time than sit through a political drama about Oppenheimer, or Teller, or Grothendieck or even Perelman.

And I can only imagine what other folks would want to sit through. (Probably not N. A-H., but also probably not Yet Another Oppenheimer biopic, etc.)

16. **srp**
August 7, 2012

OK, I must get out my two pet peeves about A Beautiful Mind:

1) They get Nash equilibrium wrong. It's a simple and conceptually elegant idea and for no good reason the scene in the bar gives the exact wrong intuition and result.

2) They did that Hollywood thing of making Princeton University out to be a formal and stuffy place. The actual introductory lecture to incoming doctoral students described in the book was pretty much the opposite of the one Judd Hirsch's character gives in the film. In the book, Lefschetz banged his wooden hands on a table and said that at Princeton we don't care about baby stuff like whether you go to class or dress nicely, but only care whether you create important mathematics.

And what was up with that hallucinogenic presentation of fountain pens to Nash at the faculty club? Was Ron Howard channeling his bar mitzvah experience?

17. **Ilya Ferapontov**
August 7, 2012

Source of the news is russian version of Onion news – Fognews (http://fognews.ru/gipoteza-perelmana-ot-dzhejmsa-kemerona.html). It is fake news 100%

18. **anon.**
August 7, 2012

English Translation of Fognews spoof news on Perelman film contract: http://www.microsofttranslator.com/bv.aspx?from=ru&to=en&a=http://fognews.ru/gipoteza-perelmana-ot-dzhejmsa-kemerona.html

Perelman hypothesized from James Cameron

Author: Stuart Cain

James Cameron announced that he would make a film about the Russian scientist Gregori Perelman.

The plot of the future masterpiece is kept secret, but when a famous Director in St. Petersburg, he coaxed a scientist filming visited our editorial chat and we were able to learn about the little-known details of the new project.

The movie itself will be built in a period of relative obscurity scientist opposed to his fundamental discoveries and a short period of time immediately after the award of the prize of the Fields.

Cameron said that it was important to show how the scientist's quiet life invasion begins. Grigori Perelman, proving the Poincaré conjecture, suddenly became interested in the scientific world.

This world, Kèmeronu, using a known mathematical problems solved, wants to make himself popular, despite the fact that it criticises the power within him and Perelman. Thus, the Director intends to show how, kommercializuâs', success is not only individual but also national traits.

The second line will be built around it, will be a renunciation of the Russian narration mathematics from 1,000,000 dollars. Cameron hopes to demonstrate the true causes of this Act. Based on the testimony of major General of Justice of St. Petersburg, the Oscar winner will deploy peRed zritelem epic picture of the battle state machines and little man.

In the years in which the Clay Mathematics Institute wanted to transfer dollars to the account of Perelman, St. Petersburg had strict instructions of Valentina Ivanovna the doubling of the budget, and naturally a major taxable income immediately drew the attention of the tax inspection of large taxpayers.

At the time of inspection, as told by Kèmeronu source, fulfilled the plan for the quarter, revenue from Gazprom, Rosneft were not as high as expected, so it was decided to take 13% of the million of Perelman before the end of the reporting period to finish the year with double overfulfilled the task. The Director was able to talk to the man who had appointed the Coordinator of operation "Perelman.

The operation was carried out on a grand scale: there were bikers who lit up at night in the apartment of the scientist, alluding to buy housing in another area were special raids with the fivefold increase in prices in stores where purchases Perelman were huge puppets-pig who accompanied Gregory in shops and on the streets.

Selected people, similar to his old friends and acquaintances whose task it was to ask the debt, telling about their own nesčast'h and the troubles, and the entrance of the House was obkleem advertisement charities, many real and imaginary persons with disabilities were planted on the pavement near the House of a scientist. Planned even a kidnapping, but … Perelman, not noticed.

Immersed in the world of theoretical mathematics, it's indifference forced power wasted throw 220 thousand dollars trying to exert pressure on him, and then retracted the award. Error is the Coordinator of the Perelman, "he was a young officer, who, in desperation, came into the House of a scientist with the Declaration of income.

This persistence state tax inspector need you to have a share of the million! "made the break, look in the Perelman cheque, which he stayed with the latest purchase, see bun bread for 200 rubles grayscale and forgo bonuses. The scientist did not want to give money to parasites.

The tax was left with nothing and, to somehow compensate for their expenses — bikers fined for riding without a helmet, shops for breach of the Competition Act, pig suits sold by homeless activists and friends dispersed to their homes.

This story is going to show his new film James Cameron. At least in that he tried to convince the editor of FogNews, while being our guest.

19. **Cesar Laia**
August 7, 2012

About Oppenheimer, I liked this movie a lot: http://www.imdb.com/title/tt0097336/

20. **Giotis**
August 7, 2012

"I've been reminded that there is a biography of Perelman, Masha Gessen's book Perfect Rigor."

I've read it. It was excellent except that at the end of the book she implied autism. I strongly disagree with such off-hand speculations. People are not obliged to align with society's norms to be considered "normal"…

21. **bjm**
August 7, 2012

@ srp says: "OK, I must get out my two pet peeves about A Beautiful Mind…"

As someone who never heard of Nash before the movie, the film took me on a fun ride. I thought it was well made, imaginative, and entertaining. I understand the pain of someone who sees that the movie got the facts all wrong, but at least now I have heard of Nash and can research the facts about his life if the urge strikes.

22. **Jeff McGowan**
    August 7, 2012

    As far as "A Beautiful Mind" and the real John Nash, a story which maybe gives some insight into how far off the movie (and even the book really) are –

    I had a professor in grad school who was an undergrad at MIT when Nash and Paul Cohen were both there as postdocs. He used to hang around the math lounge all the time, since Nash and Cohen spent a lot of time trying to each prove that they were smarter than the other, giving impromptu lectures on various interesting topics. Nash got to know my professor because of this, and one day he handed him an offprint of one of his papers. My prof opened it up, and noticed that on the inside cover was a very elaborately drawn thing, which upon inspection he discovered was an "intergalactic drivers license," valid in perpetuity. This well before anyone had any thoughts about Nash's lucidity. He didn't say anything, but several years later, after Nash had been hospitalized, my prof got a letter from him telling him that unfortunately even though the license had said "valid in perpetuity" Nash was going to have to rescind it, and could he please mail it to Nash. Needless to say he kept it, and still has it 🙂

23. **Bugsy**
    August 7, 2012

    My God but Google translate can be bad!

    Re math films, I did like especially the beginning part of A Beautiful Mind (the craziness was a bit painful to watch) and also liked Good Will Hunting a lot. But my favorite by far is Proof. I can't believe how good a job the actors (who all apparently were zeros in science in reality) did of catching the spirit of math research. Plus it was great to see again the environs of the U of Chicago, though the Northwestern math dept was replaced by a much more modern building!

24. **Craig**
    August 11, 2012

    Many RT stories are weird self-parodies. So this might hold as much water as an April fool joke. They've had ostensibly serious discussions with guests about the cia colluding with space aliens.

Article for Il Manifesto

August 8, 2012
Categories: Uncategorized

Around the time of the Higgs discovery announcement last month I was contacted by someone from the Italian left-wing newspaper *Il Manifesto*, who asked if I'd write something for them about the Higgs. I told them that it would be much better if they could get an experimentalist to write about that topic, since the discovery was really an experimental achievement. They managed to get Tommaso Dorigo to write that piece for them (see here), but I agreed to write something for them a bit later, about the significance of other results from the LHC. That piece appeared in the newspaper today in Italian translation, an English version follows here.

This was written last week, under the combined influence of watching some of the Strings 2012 talks and thinking about the possible impact of the new $3 million Fundamental Physics Prizes, which largely went to string theorists. For this venue, I was unable to resist channeling my inner leftist (normally the only newspaper that wants me to write for them is the *Wall Street Journal*…) and making Russian financier Yuri Milner to a large extent the bogeyman of the piece.

A very serious concern that I wanted to raise is that of the long-term danger that fundamental physics faces in the combination of string theory ideology and the possible "nightmare scenario" of the LHC finding nothing that disagrees with the Standard Model. For decades now the theoretical side of the subject has been dominated by one specific set of not very compelling ideas: 10d superstrings at the Planck scale, with a SUSY GUT at slightly lower scale, and low-energy SUSY explaining the supposed "hierarchy problem" created by the vast difference between those scales and the scale of electroweak symmetry breaking (of order 100 GeV). The force most likely to challenge the hegemony of this ideology has always been the LHC, which was supposed to see superpartners responsible for stabilizing the electroweak scale. Watching the speakers at Strings 2012 made clear that the failure of this experimental prediction would not cause them to give up on this ideology, but instead to redouble efforts to prop it up at all costs.

The fundamental problem is the deeply entrenched nature of string theory ideology in the power centers of the academy and among the most talented theorists. Milner's choice to provide out-scale rewards to such talented people is not the main problem, although he provided a convenient target for me in the piece. If we really do end up with the "nightmare scenario" of experiment not coming to rescue, it's now all too clear where we end up: the textbooks of string theory and supersymmetry have already been written, and that will be codified as humanity's best understanding of fundamental physical reality for the indefinite future.

Maybe some new theoretical ideas will somehow bloom, but otherwise our best hope to get out of this will be the efforts and innovations of talented experimentalists, likely requiring expensive equipment. It will be a challenge to continue to find public resources to fund this. Maybe if the trends of recent decades continue, it will be up to the financiers to decide whether humanity continues down the experimental path. Luckily, a lot of them seem to be interested in physics.

The article follows, you might want to skip it if you're a regular blog reader here, since you won't hear anything new and it's a bit of a rant…

> Last month came an announcement from Geneva that physicists of my generation had been anxiously awaiting since our student days nearly forty years ago. Experimentalist Tommaso Dorigo wrote in this newspaper about the great achievement of the Large Hadron Collider at CERN and his 6000 or so colleagues, who came together to produce and make the first measurements of a new fundamental element of nature: the Higgs particle.
>
> For theorists like myself, this was a bittersweet victory for our subject. The Higgs particle showed up more or less exactly in the manner predicted by the so-called Standard Model, a wildly successful fundamental quantum theory developed between 1967 and 1973. This theory had passed critical tests time and time again, but until last month the trickiest part of the theory had not been tested by direct observation. Perhaps we were missing something important, and the real world would slap us in the face with results contradicting the theory, and giving us clues about how to find a better one. Instead, we saw the equations of our textbooks dramatically confirmed. We now await a long process of detailed investigation of this new phenomenon, a process which will keep Tommaso and his colleagues busy for many years to come.
>
> The Higgs discovery emerged as a single sudden announcement, but over the last two years an equally important discovery has slowly come into focus, one small piece of data from the LHC at a time. Unlike the case of the Higgs, this discovery has been a vigorous slap in the face to the theoretical particle physics community, telling us in no uncertain terms that we've been wasting most of our time for the past thirty years. For these three decades, the subject has been dominated by research into an elaborate speculative scenario which has been investigated in exhaustive detail.
>
> This scenario goes under the name of "superstring theory", referring to a set of ideas that form not exactly a well-defined theory, but rather a conjecture that a theory with certain properties should exist. This theory would unify the Standard Model with Einstein's theory of gravity known as General Relativity, embedding both in a complicated structure involving six extra dimensions of space. The possibility that these new dimensions would put in an appearance at the LHC has often been used to impress the public with flashy claims about the dramatic things that CERN's new machine could find, things that sounded like (and were) science-fiction. Whatever they told the public, few physicists were expecting such dimensions to be observable in the data, since the conventional speculative scenario put their size at far too small a value to be seen at the LHC. No one has been surprised at all by the failure of any sign of extra dimensions to show up at CERN, despite a careful search for any possible evidence.
>
> The "super" in "superstring" though is a different story. This indicates a crucial property of the conjectural unifying theory: each fundamental particle should come paired with another one of very specific properties, the particle's "superpartner". The electron should be paired with a new particle named the "selectron", each quark with a "squark", etc. Over the years an increasingly rigid ideology explaining the supposedly wondrous properties of this "supersymmetry" which would dramatically improve upon the Standard Model. That supersymmetry provided none of the powerful explanations of past observations we have come to expect from new symmetry principles was an inconvenient issue best ignored. As each new generation of accelerators came to life at CERN and at Fermilab in the United States, superpartners were looked for, but never found.
>
> Supersymmetry entered the textbooks anyway and has now been taught to generations of graduate students. Always part of the story being told was the claim that superpartners should have masses roughly similar to the mass of the Higgs particle. When the Higgs was found, the superpartners had to be there too. Consistency of the Standard Model demanded that the Higgs could not be too massive, so long before the LHC was turned on, it was a sure thing that if the Higgs particle was there the LHC would find it. That part of the story worked out perfectly, but it has been accompanied by a huge embarrassment: no sign of any superpartners at all. Not only were they supposed to be not too much heavier than the Higgs, but many of them were supposed to be much produced much more copiously, and thus be much easier to see. By now the LHC experiments have shown that such expected particles are absent, unless they are made inaccessible by pushing their masses up to more than an order of magnitude higher than that of the Higgs, a value far beyond what had been advertised as reasonable.
>
> The implications of this attack on theorists by the reality principle are just beginning to sink in. The big yearly conference of superstring theorists was held this past week in Munich, with different speakers taking different approaches to dealing with the problem. One speaker advocated not doing anything until next year, hoping against hope that newer data would give better results. Others took the attitude that it had been clear for quite a while that superstring theory wasn't going to show signs of existence at the LHC, so best to just work on finding other uses for it. In the conference final "Outlook and Vision" talk, the illustrious speaker announced that all was well, and didn't mention the LHC results at all. The ostrich-like tactic of burying one's head in the sand seems to be on the agenda for now, but this will become increasingly difficult to maintain as time goes on and more and more conclusive negative experimental results arrive.
>
> As a physicist, one problem with having an experiment tell you that your ideas are wrong is that it means you are ineligible for a Nobel Prize. Your hopes for a right to a part share in $1.2 million have been dashed, and, no matter how famous and well-paid an academic star you may be, you will have to content yourself with living on your salary, supplemented perhaps by smaller, less well-known consolation prizes.

Around the time of the end of the superstring theory conference though, dramatic news came from billionaire Russian financier Yuri Milner. Known for building the most expensive house in the United States, he decided to help support physics by depositing $3 million dollars per person in the bank accounts of 8 prominent physicists and one mathematician, rewarding 6 of them for their work in superstring theory. He has modeled himself after Alfred Nobel, announcing a new foundation that will give out Fundamental Physics Prizes each year. Unlike the Nobel, these prizes can go to work for which there is no experimental evidence. What he's looking for are "transformative advances" like superstring theory, which have gotten the seal of approval of popularity among high-status academics. Even if experiment shows the ideas to be wrong, as in the case of the latest data about supersymmetry, that doesn't matter. What does matter is that the recipients should reflect the conventional wisdom in the academy. The choice of who to give the Prizes to included giving them to every single professor of particle physics at the world's most prestigious academic institution, the Institute for Advanced Study in Princeton. The question of competition with the Nobel prize was dealt with by setting the value of the prize far above that of the Nobel, at a level significantly higher than any other academic prize in the world.

The slap in the face by experimental data and its threat to impose the reality principle on the most powerful figures in the world of theoretical physics has thus been met by a riposte from another powerful force. This is another reality, that of entrenched academic interests, funded by the billions of dollars available to financiers who want to impose their will upon the world, or at least the small part of it that will write the textbooks of the future. Which of these forces will carry the day? Will the budget cuts imposed on physics research in Italy and elsewhere cripple the ability of the LHC experiments to continue to reveal the structure of nature, leaving our future fundamental science in the hands of powerful interests who will decide which version of reality they like best?

In the longer term, the physics community now faces difficult choices. Any machine more powerful than the LHC will be expensive and require multiple decades to design, finance, build and operate. The temptation will be there to again promise discovery of exotic new dimensions and supersymmetries in order to convince governments to provide funding. Instead of such empty promises, physicists should just make the case that humanity deserves the chance to continue the experimental investigation of the fundamentals of physical reality. The alternative is all too clear: the lack of public money to fund experimental investigation will put those with private money in charge of deciding what our scientific reality will be.

Comments

1. **s. Vik**
   August 8, 2012

   The FPP is not a price, it's just a research grant. So what is the problem.

   I still think the scientific method will eventually tease out the correct theories.

   Maybe Gross and co will wrestle that "maximally super symmetric N=4 Yang-Mill with infinite color, split the susy, …..” to the ground and dual it up with the QED sooner than later so we won't have to hear about it forever, ………..

   In his lectures it sound like it should be 1-2 years.

2. **Tammie Lee Sandoval**
   August 8, 2012

   Dear Dr. Woit

   Please, don't worry.
   You and Dr. Smolin have succeeded, much more than you imagine.
   Everyone knows the score.

   Physicists are not car dealers. Not their nature.
   And they wont be, even for 100 times Milner's money.

   You go into physics to show how smart you are.
   And you wont get there with a wild goose chase.
   So the young guys will ditch string theory.
   The old will soldier on, thanking God for tenure.

3. **Bernhard**
   August 8, 2012

   "Maybe some new theoretical ideas will somehow bloom, but otherwise our best hope to get out of this will be the efforts and innovations of talented experimentalists, likely requiring expensive equipment."

   I think a theoretical bloom of ideas is urgently needed.

   It might be that the next successful model beyond the Standard Model will really be just some conservative extension using model building rules for renormalizable gauge theories. This kind of work should continue because experiments still need guidance to what to look for, even knowing that in the end only ONE model can be correct. This is a key aspect because ultimately it affects trigger design (as the SUSY history shows).

   However, if the next model of nature is not as simple as extending the SM gauge sector to some SU(whatever) group then this is likely a deeper theoretical problem in QFT we oversaw and when we do see it, we really might get a good idea to what to look for next. This is the theoretical bloom we need.

   An experimental breakthrough would also be very welcome, specially if the technology to get TeV collisions could be made much much cheaper. This is what I believe many young accelerator and detector designers should pursue, if this field is to survive as science and not as string theory religion.

4. **Peter Woit**
   August 8, 2012

   s. Vik,

   The Fundamental Physics Prize is a prize, not a research grant (you may be thinking of the Simons Investigator 100K/year awards, which are research grants). The intent of the FPP is to reward appropriately the most successful people in the field, not to support their research. Of course they can do whatever they want with the prize money, including using it to support research, theirs or someone else's.

5. **Bob Jones**
   August 8, 2012

"Maybe Gross and co will wrestle that 'maximally super symmetric N=4 Yang-Mill with infinite color, split the susy, …..' to the ground and dual it up with the QED sooner than later so we won't have to hear about it forever, ………."

Have you even tried to understand what you're talking about? What you meant to say is that there may be a string theory dual to QCD, not that N=4 SYM is dual to QED. Doing computations in QED is already well understood. Needless to say, split supersymmetry has absolutely nothing to do with any of this…

6. **Peter Woit**
   August 8, 2012

   Bob Jones,

   I had assumed that s. Vik was joking, making fun of string theory hype, not intending to make sense (the "1-2 years" seemed a sure giveaway that it was a joke). Of course, in this business these days, it's not always clear what's a joke and what isn't. Some people for instance seem to think super split supersymmetry is not a joke.

7. **David Nataf**
   August 8, 2012

   Is there not a tremendous amount of data from cosmology that requires extensions to the standard model? What about the existence of gravity?

   From far away, it seems that implicitly requiring new physics to come from terrestrial particle accelerators is poor philosophy. It's very possible I'm missing some important information here, but the history of science teaches us the particle accelerator is merely one method to learn about nature.

   If you want even more requirements for new physics then there is already, the LHC is only one possibility. There are also amazing pendulum experiments, Fermi telescope, dark matter scattering experiments like XENON100 and DAMA, Planck mission, air showers of 10^6 TeV cosmic rays, and in the future possibly LISA. There are obviously more that I've never heard of.

   Today, a fascinating nuclear physics paper that is anthropically-relevant was posted:
   http://arxiv.org/abs/1208.1328

8. **Peter Woit**
   August 8, 2012

   Bob Jones,

   Just deleted the last from "s. Vik". You were right and I was wrong.

   David Nataf,

   Of course there are other possibilities than the LHC or other energy frontier machines, but it's not clear that any are very promising ways to see something that violates the SM. Dark matter has always been the most likely thing that might show up, but so far nothing, and no reason to believe a new generation of experiments will change things. As for an expensive spacecraft experiment like LISA, funding is also a big problem (and, even if you ever build such a thing, it seems likely gravitational wave observations would tell you about astrophysics, not more fundamental physics). Basically, it's not impossible that any one such experiment will turn up something and they're worth pursuing, but the probability of success in each case looks low (unlike the LHC, where we pretty much knew it would find either the Higgs or something more interesting).

9. **MathPhys**
   August 8, 2012

   Standing in a supermarket queue this morning, I saw a picture of Jennifer Aniston (an actress, for those of you with better things to think about) on the cover of a local tabloid, so it occurred to me to check how much money she makes.

   It turns out that she makes between 3 and 8 million dollars per film, she can make at least one film per year (that's a Milner prize, minimally, once a year), and that during the final two seasons of 'Friends' (a TV sitcom, for those of you who only watch Nima's lectures) she was paid 1 million 4 per episode, so that's once a week for many weeks a year (and a Milner prize once every 3 weeks of work).

   So ——-, putting things in context, I now do believe that Witten and co richly deserve their Milner prizes.

10. **Bob Jones**
    August 8, 2012

    From the Fundamental Physics Prize website:

    "Two categories of prizes will be awarded for past achievements in the field of fundamental physics, with the aim of providing the recipients with more freedom and opportunity to pursue even greater future accomplishments. The Fundamental Physics Prize recognizes transformative advances in the field, while the New Horizons in Physics Prizes are targeted at promising junior researchers."

11. **Peter Woit**
    August 8, 2012

    MathPhys,

    Yes, but the problem with the analogy is, what if these large rewards went to actors for making bad movies? Where would we be then?

    Hmm, no need to answer that, I guess…

12. **srp**
    August 8, 2012

    Yes, but Jennifer A. is preternaturally, world-class adorable. Not too many physicists could make that claim with a straight face. And she once triggered a revolution in hair salons all over the world as women demanded copies of her 'do. Even Brian Greene doesn't have people copying his hair.

13. **MathPhys**
    August 8, 2012

Peter and srp

On the one hand, I definitely don't find Nima adorable (yikes), but on the other hand, I find him much more entertaining than any TV sitcom that I can think of, and what he talks about is just as real.

Go, Nima. If Jennifer A makes a Milner a year, you definitely deserve a Milner once in a lifetime.

14. **Z**
August 9, 2012

It's very important to note that telescopes such as Fermi, IceCube, Planck and JWST are _telescopes_ and not experiments like accelerators. Direct detection is something very different than observational evidence wrt the philosophy of science.

15. **Manifesto**
August 9, 2012

Be careful. This journal had a good tradition but was abandoned years ago by moderate left, degenerating into an extreme-left journal, full of debts and of nonsensical propaganda of communism and radical feminism.

Probably nowadays your blog has more readers than the Manifesto.

16. **chris bolger**
August 9, 2012

I do not understand your overly emotional rants on superstrings and on who funds physics. With the increasing lack of support by governments, the only way physics research will proceed is by private funding. Granted, it is not ideal, but any support is better than none. Milner should be applauded for this. The issues you bring up are valid, but will probably work themselves out by reasonable people over time who work on the details of giving out the prize. In other words, don't bight the hand that feeds you. This usually results in job loss, unless you have tenure.

17. **Peter Woit**
August 9, 2012

chris,

While I don't have tenure, luckily I'm in a position to be able to bite various hands that feed me and others, when I think they need some biting. So far, I think this has worked out fine.

18. **next LHC-SUSY announcement**
August 9, 2012

There have been recent press release on Higgs, but I'm not aware of one for SUSY in 2012. I infer we're talking about the 2011 null results for SUSY or something else? When will 2012-LHC SUSY or BSM results be announced, even if negative?

19. **Peter Woit**
August 9, 2012

next LHC-SUSY announcement,

CMS has already announced SUSY results using 2012 data, see

https://twiki.cern.ch/twiki/bin/view/CMSPublic/PhysicsResultsSUS#Recent_Preliminary_Results_with

ATLAS I believe will announce some next week at SUSY 2012 in Beijing, see for instance

https://twitter.com/marktibbetts/status/228834932754116608

There are many, many, many possible ways to look for SUSY signals, the situation is not like the Higgs where one was looking for something very specific, so there was one "result" which told the story. In the case of SUSY, hopes that a positive result may appear are mostly based on the fact that new and different sorts of analyses are being reported, or ones with a lot more data than was used in earlier analyses based on a fraction of the data available. Going from the 7 TeV of 2011 data to 8 TeV of 2012 data is not likely to change much, since these aren't phenomena with sharp thresholds.

20. **Q.I.**
August 9, 2012

>Chris Bolger wrote: "the only way physics research will proceed is by private funding."

While, there are some practical areas within physics that might qualify for some private funding, however, after reading the comments on this blog for some time, Woit's and Smolin's books, and a few other sources, I get an impression that physics is turning more into an insular field. And, that is not good news to qualify for already shrinking sources of private funding available these days. Few engineering and computer science students take any, if any, physics courses at all in their graduate studies, which I think is a sad state of affairs. I can understand that many areas within HEP may not be of interest to engineering and computer science students, However, as somebody who is not a physicist, I just can't bring myself to not notice a deep desire by the physics community to be seen as mathematicians. Which is a further disconnect from the reality of the technological areas, because if people in other disciplines can directly refer to math books and papers for their needs, then why consult a physicist's approach on mathematics. Especially, when the notation and technical writing used by physicists are not as consistent and good as the math community.

I could be wrong, but the way I see things is that physics should try to bridge the gap between several technological areas while offering its own perspective on the fundamental challenges. However, I'm not sure if that is the current direction.

21. **Peter Woit**
August 9, 2012

Q.I.,

"a deep desire by the physics community to be seen as mathematicians".

I don’t see this at all, other than among a very small part of the theoretical physics community that has ended up pursuing some topics that end up using a lot of mathematics. One could easily get a very wrong impression from this blog, where I write about topics in physics that correspond to my interests, which have a lot to do with mathematics. Most physicists even I meet have limited interest in mathematics, and no desire to be seen as mathematicians.

22. **next LHC-SUSY announcement**
August 9, 2012

thanks

23. **Tue Sorensen**
August 9, 2012

Like some of the other posters, I don’t think you need worry about string theory and supersymmetry becoming orthodoxy despite the lack of experimental evidence. I am confident that there will be new theories forthcoming which will dramatically revise the Standard Model, and consequently lead to quite new avenues of experimental investigation. We still need to find out what gravity is, what dark matter is, why the expansion of the universe accelerates, etc. There will be answers to these questions which in hindsight will seem obvious, but the current SM does not include those answers. It will have to be revised into a form that does. People will keep working on that until they get some good results. I don’t see how things could happen any differently – even if it might take quite some time.

24. **Fred Freedman**
August 9, 2012

Interesting discussion regarding private funding of research. One might almost imagine a deliberate policy by conservative circles to reduce educational funding thus permitting private funding to fill the gap.

The problem with private funding is, of course, that he who pays the piper calls the tune. At least universities and granting agencies provide a system of oversight for granting. With private donors it’s one person’s decision.

Recently here in Canada we have seen more of this business of megadonations by rich patrons funding new research institutes. These donors don’t have to spell out explicitly what research is to be done. By funding centres to enquire into specific issues they direct the nature of research.

So while on the surface Milner’s money seems positive as an infusion into a hungry system, as Peter has said, it perverts research by directing academics in a certain direction either by allowing them the luxury to hire grad students, buy equipment or by encouraging others to enter a field so as to be eligible for the money.

We have recently come to the point where this ideological approach to academic funding has become so acceptable that comments like Chris Bolger’s are commonplace.

25. **Tmark48**
August 9, 2012

@ MathPhys : Don’t ever go looking at how much money professional football (soccer for you guys in the US) players make. You’d get a heart attack.

26. **ex**
August 10, 2012

@ Manifesto:

Be careful. This journal had a good tradition but was abandoned years ago by moderate left, degenerating into an extreme-left journal, full of debts and of nonsensical propaganda of communism and radical feminism.

Be careful, it was the opposite. It was Manifesto that was created to abandon the moderate left parties, because of their support to Soviet invasion of Prague (to reestablish the *order* after the happy period of self-liberation known as Prague spring).

It has a declared strong left partisanship, but it is still one of the most free newspapers in Italy – a miracle, after the Berlusconi period and in this era of Market propaganda, “single thought”, and suppression of critical voices. Take care.

27. **MathPhys**
August 10, 2012

Tmark48,

The world is mad.

28. **JDM**
August 10, 2012

Peter,

Like others here, my instincts run contrary to yours, although for somewhat different reasons. I do find your thoughts in the influence of that scale of private money on the field interesting and, to a degree, compelling; however, I anticipate things developing in a different direction.

You’re absolutely right that money at that scale could underwrite a generation of students, postdocs, and textbooks, and so keep an otherwise floundering branch of the discipline around past its use-by date. But I’d say that one generation amounts to the half-life of this effect.

The problem, I think, comes from taking at face value Milner’s conceit that’s he’s emulating Nobel. The Nobel Prize got to the place it currently occupies because of robust, widespread support across the scientific community. That is, it grew on the power of prestige. Sure, the money was a part of that, but not the most critical. If you look at the first few decades Nobel awards in physics, for example, you get a whose who of early 20th century physics. Moreover, that list would still be a whose who of that era if the Prize had never existed. The Nobel earned its prestige by awarding, retrospectively, the most worthy accomplishments, as judged by a broad selection of scientists.

A chunk of money, however large, can’t do the same thing on its own. It won’t be able to develop truly widespread communal support in the absence of rich theory-experiment dialogue. Milner is engaged in a much narrower enterprise. The work he’s supporting can’t be sustained indefinitely from the top down. A viable field provides support and opportunity for the workaday folk, not just the leading lights. A lucrative private prize, however, large, will not be enough to lure new students into a research area that otherwise offers few prospects. Perhaps the wave of textbooks you envision will get one generation’s worth of use, but faculty who see that these books offer the vast majority of their students little of concrete value will move away from them, or the vestiges of once-promising research programs will be excised from later editions. Once this begins to happen, Milner—assuming that he continues to award his prize on the basis of his peculiar definition of “fundamental”—will look progressively less like Nobel, and more like Templeton.

Joe

29. **DrDave**
August 12, 2012

I think if you care about the money and the physics, get together a petition, or call this Yuri guy up, and say, hey, fund some experiments. I would at least FB him 🙂
In many branches of Academia, there are departments that are divided between theoretical and practical (perhaps plastic is the term, strangely enough, as in the plastic arts). And here I think a petition to department deans to create more real chairs for experimental physicists is in order. Your point about the extravagant cost is well taken, but a few ounces of sheer genius can do a lot. It may be that there are no real bargains left, such as Cavendish's torsion bar or double-slit electron diffraction, but it may also be that we need to look harder.

LEP3

August 9, 2012
Categories: Experimental HEP News

For many years now discussion in the HEP community of what might be the appropriate next machine to try and finance and build after the LHC has centered around the idea of a linear electron-positron collider. The logic has been that an electron-positron machine would provide a much better environment that the LHC for detailed studies of physics at the TeV scale. At these energies, synchrotron radiation losses when accelerating electrons are so high in a circular geometry that such a machine would have to be a linear collider to keep the power needed something plausible. The two main proposals under study have been the ILC (250 GeV + 250 GeV, later upgradeable to 500 GeV + 500 GeV) and, a less mature technology, CLIC (1.5 TeV + 1.5 TeV). These would be very expensive machines to build and operate ($10 billion and up?), requiring completely new technology, tunnels and detectors.

The operating assumption has been that initial results from the LHC would show the existence of new supersymmetric or other particle states in the region of multiple hundreds of GeV to small numbers of TeV, and the linear collider designs could then be chosen and optimized to study this new physics. The other main goal of such a collider would be detailed study of the Higgs, and knowing the mass of the Higgs is also highly relevant to what kind of linear collider to build.

The initial LHC results are now in (125 GeV Higgs, nothing new at the TeV scale) and they are rather discouraging for the linear collider idea, providing no strong motivation for studying electron-positron collisions around 1 TeV. A "Higgs Factory" capable of producing and studying large numbers of Higgs particles is an attractive idea, but the production cross-section for a 125 GeV Higgs is dominated by the process $e^+ + e^- \to Z + H$, which starts to get large around 220 GeV and reaches a maximum value around 255 GeV. So, for most Higgs studies, the right energy for an electron-positron machine is around 250 GeV, not 1 TeV.

This realization is driving a new proposal that is getting a lot of attention: the idea of going back to circular electron-positron colliders, building a new machine in the LHC tunnel, optimized as a Higgs factory, and designed to operate at 120 GeV + 120 GeV. This is being called "LEP3″, since it would be in many ways similar to LEP2, the predecessor machine to the LHC, which operated in the same tunnel, reaching an energy of 209 GeV. There would be huge cost advantages to building such a machine over the ILC or CLIC, since it can use the LHC tunnel, infrastructure, and, crucially, the CMS and ATLAS detectors (the detectors are a large part of the cost of a new accelerator).

Space was left in the LHC tunnel to allow another ring, so there are various possibilities for having a LEP3 and the LHC cohabitate. Until now, the assumption has been that the LHC would be upgraded to the"HL-LHC", operating at higher luminosity throughout the 2020s, then perhaps an "HE-LHC", operating at higher energies during the 2030s. This plan is being challenged by the LEP3 proposal, with the argument that it might turn out that Higgs physics is where the only action will be, and a long period of LHC operation at higher luminosities and modestly higher energies might be less worthwhile than building a LEP3 Higgs factory.

There's a very good article about this at PhysicsWorld. For more detail about LEP3, see here, here and here. John Ellis is one of the co-authors of the latest document proposing study of the LEP3 possibility, and the Physics World article has him arguing that, after waiting to see if LHC14 turns up anything:

> LEP3 could be a more secure option than the ILC if only a Higgs is discovered…If the LHC does not discover anything beyond the Higgs, then would you keep running it for years?

Lyn Evans, who led construction of the LHC and is now director of the linear-collider project argues against the LEP3 concept:

> The first job is to fully exploit the LHC and all its upgrades, This is at least a 20 year programme of work, so I think that it is very unlikely that the LHC will be ripped out and replaced by a very modest machine with little scope apart from studying the Higgs.

The problem is, what if, as seems increasingly likely, "studying the Higgs" is the only new physics accessible in these energy ranges? Dreams of superpartners and extra dimensions may die hard.

Comments

1. **Bernhard**
   August 9, 2012

   This is interesting, but I think Ellis is dreaming. No way the LHC will be shut down until is completely explored, the pressure to keep the machine running and achieve as high integrated luminosity (including upgrades) as possible is enormous. Building a Higgs playground would be nice but at the cost of the LHC, no way.

   Furthermore I hear the Japanese are strongly pushing for the ILC to be built (in Japan) and they are willing to pay for a considerable cost of it. In any case, before the LHC running a considerable time at design energy an luminosity these are speculations. And I believe very few people would be excited about working in a "Higgs machine" and ultimately this is a key aspect.

   The ILC running at 1 TeV can in any case still easily be defended as a discovery machine as e.g. virtual corrections from all sorts of models make the discovery reach for large range of particles (models) to be much higher than the machine center-of-mass and or even the LHC for that matter.

2. **HE-LHC vs HL-LHC**
   August 9, 2012

   If all they find is the Higgs boson up to 14 TEV by 2020, no SUSY or DM or other particles, or extra dimensions what would be the point of spending money to build a HL-LHC upgrade around 2020? What would higher luminosity offer? Couldn't the LHC continue to run at a lower luminosity but take more time to gather more data?

3. **Peter Woit**
   August 9, 2012

   HE v. HL,

   The idea of the HL-LHC is to increase the luminosity by something like an order of magnitude, and run for a decade. If you stay at current design luminosity, you'd have to run for a century to match that.

   One motivation for the high luminosity is to look for new physics via rare events. But, even just for the Higgs, higher luminosity means much better measurements. The crucial question is just how much better a LEP3 could do than this.

4. **DEQ**
   August 9, 2012

Why the idea of a muon collider not gaining traction? Is that technology just too far out right now, i.e. something 20 years away?

5. **Peter Woit**
August 9, 2012

DEQ,

Yes, muon collider technology is still a long ways off (although you'd need someone more expert than me to give a time estimate), requiring some very new technologies. LEP3 however would use what appear to be relatively straight-forward extensions of proven technologies.

6. **Tmark48**
August 9, 2012

My opinion is that the future of HEP is going to be in astronomy/astrophysics.
Space is the only environment where we can witness (with suitable detectors/telescopes) physics beyond the SM. Investing is astronomy is going to be much more productive than building a YAA (yet another accelerator) for untold tens of billions of €/$/£/Yens when we don't even know at what energy scale beyond SM physics kicks in.

As for the LHC, for pete's sake it has been in operation for what less than 4 years and we want to build another one ? LHC is going to be useful for a good decade if not 20 full years. It is going to be useful even if it throws in the dust bin all the speculative ideas that are found to have no experimental evidence.

7. **Peter Woit**
August 10, 2012

Tmark48,

The LHC was designed about 20 years ago, with approval of funding and work on starting to build magnets beginning around 1993. So, if you believe it has a lifetime of 20 years, now is about the time for starting work on whatever will be next. These projects have a very long lead time.

So far, instruments in space (which aren't cheap either…) have yet to tell us what's beyond the SM. I wouldn't be so sure this will change in the forseeable future.

The interesting thing about the LEP3 idea for a new machine is that it's not necessarily that expensive. Instead of order of magnitude $ten billion in new money, it might be doable for a lot less, fitting within the CERN budget with no significant increase. It definitely seems worthwhile to investigate the idea and produce some realistic cost estimates, as well as a serious investigation of the physics case for the machine.

8. **Mitchell Porter**
August 10, 2012

"Dreams of superpartners and extra dimensions may die hard."

In my opinion, now that there's a Higgs, the next big issue is the hierarchy problem. The economical way to test known forms of unification, would be to test their solutions to the hierarchy problem, e.g. the existence of new "partner" particles for the top. That way you test at the same time, both standard versions of supersymmetry (with light stops) and nonstandard models with top-partners. And it seems that this is what the experimental collaborations are doing…

9. **Tmark48**
August 10, 2012

The LHC was designed about 20 years ago, with approval of funding and work on starting to build magnets beginning around 1993. So, if you believe it has a lifetime of 20 years, now is about the time for starting work on whatever will be next. These projects have a very long lead time.

–
You are correct, but big projects take decades to come to completion. Even the Hubble was like this. But the thing is, the LHC was designed for one purpose, finding the HIGGS.
Even now that the LHC has found the HIGGS, we still don't know what kind of HIGGS particle it is. Further experiments are warranted.
You say we need to start considering a new accelerator. Ok, to search what ? What is the goal ? It cannot be a moving goalpost as supersymmetry is. Isn't it better to upgrade the LHC and continue using it instead a starting a wild goose chase ?

So far, instruments in space (which aren't cheap either…) have yet to tell us what's beyond the SM. I wouldn't be so sure this will change in the forseeable future.

–
Astronomical instruments are way, way, way less costly than building particle accelerators and they bring more science to the table. As technology improves, detectors improve and our ability to see and interpret high energy astrophysical phenomena gets better and better. No such thing is possibile for particle accelerators.
We simply cannot increase energy levels to tens of thousands of TeV. What if beyond SM physics kicks in at that scale ? High Energy Physicists are screwed.
We need to start considering that maybe beyond SM physics is simply unattainable here on earth.
Furthermore all interesting fundamental physics is coming from astronomy.
Dark matter ? check.
Dark energy ? Check. Particle accelerators are "in my opinion" becoming obsolete mainly because technology doesn't give us the tools to increase energy levels arbitrarily at which to probe the microscopic scale.

The interesting thing about the LEP3 idea for a new machine is that it's not necessarily that expensive. Instead of order of magnitude $ten billion in new money, it might be doable for a lot less, fitting within the CERN budget with no significant increase. It definitely seems worthwhile to investigate the idea and produce some realistic cost estimates, as well as a serious investigation of the physics case for the machine.

–
At the pace at which technology improves, astronomical instruments can become competitive with particle accelerators in the short term. Nature has no problem orchestrating high energy events in the cosmos. But we here on earth we are limited in our ability to create high energy events. This is a problem that the HEP community has to start thinking about.

10. **Bernhard**
August 10, 2012

Tmark48,

you are perhaps right, but these experiments would have to seriously deal with the problem of luminosity. The thing with accelerators is that it gives you a way to make a controlled experiment. If the next energy frontier is at the GUT scale, we are screwed, yes, but we don't know that, so we keep looking. How are you going to build detectors to look for astrophysical sources and have statistical evidence of BSM physics? And I'm not talking about dark matter, I'm talking about filtering an interesting process from huge SM background and studying a new resonance. Or are you perhaps saying that the sort of thing CAST does is the way to go? Then you need to look for a specific thing that requires a "moving goalpost", like axions.

11. **Roger**
August 10, 2012

I'd like to know what the prospects for the "desktop experiments" when it comes to falsifying the Standard Model. For example, what precision is needed on g-2 of the muon or the EDM neutron limits in order to explore the multi-TeV region ? Is it a better use of resources to invest in these types of experiments ?

12. **Peter Woit**
August 10, 2012

Roger,

Precision tests of the SM like the ones you mention can only give you very limited information about the multi-TeV region, and you're not going to learn anything at all about the Higgs from them. Definitely worth doing, but they're on a different cost scale and looking at different physics, not really alternatives to accelerator experiments.

Tmark48,

The interesting thing about the LEP3 proposal is that it does have a well-defined goal (detailed study of the Higgs), and the cost looks doable (proponents are talking about .1 x cost of a linear collider). I know of no conceivable way astrophysical observations can study this kind of physics.

As for whether astrophysics can tell you about the multi-TeV scale, Bernhard points out the problems, but in any case, that's a different issue.

13. **Roger**
August 10, 2012

Peter – I was thinking more about their ability to detect "new physics" rather than their ability to characterise it. I'm curious as to how much investment (experimentaly and theoretically) would be needed such that it would be possible with one of these experiments to get sensitivity to non-SM processes at a scale beyond the LHC.

14. **Peter Woit**
August 10, 2012

Roger,

There has been lots of work, theoretical and experimental, on these ideas. Having a convincing violation of the SM prediction would be quite interesting, some sort of clue, and people are working hard on this. But the problem is that typical BSM models are complicated, with vast numbers of undetermined parameters (more than 100 for the MSSM). So, if you see nothing, you put a constraint on a complicated function of 100 or so parameters, which is where we are now, these experiments do give non-trivial constraints (although arguably the muon g-2 value is marginally non-SM, so maybe there is something there). If you see something, and you try and interpret it as for instance SUSY, you are getting very little about the underlying theory, since you just get one number, and it's a very complicated function of the numbers you are interested in.

15. **David Nataf**
August 10, 2012

Tmark48,

There's a tremendous future in astronomy/astrophysics in terms of fundamental physics, and all competent physics departments are recognizing this: Dark matter, dark energy / modified gravity, number of neutrino species, inflation, anthropic principle, etc. However, we shouldn't make a bandwagon and pretend that this is where the entire future is going to be.

Keep in mind that if the particle physics community doesn't build another particle accelerator, the know how to build another particle accelerator will be lost. You need to have supervisors training the next generation. It is possible that the SM goes up to 1 PeV or whatever but you don't know. I'll also point out that null results are scientific results. If somebody has a proposal for a particle accelerator that is simply to see if the SM extends up to 10x the energies probed by the LHC, with no prior that they should not, I would say that this sounds like a legitimate scientific experiment.

Your proposal that funding be cut from future particle accelerators and shifted to satellites is also politically impossible. First, let's recognize that the most likely outcome is that money is going to be cut from all pure science sectors. LISA and SIM have been eliminated for example due to lack of funds. Further, they are different budgets. The astrophysics experiment budget is largely an industrial policy for the high-tech sector in the USA. The mirror in the Hubble Space Telescope is the same size as that in many spy satellites, for example. There are different corporate blocks supporting different science experiments.

16. **uiop**
August 10, 2012

The next generation astronomical observatories such as ALMA, SKA, and the thirty-metre class telescopes cost more than a billion dollars, and flagship-class satellite missions are even more expensive (even small ones cost a couple of hundred million). Are accelerators really way, way, way more costly?

17. **David Nataf**
August 10, 2012

No.

The cost of the James Webb Space Telescope is now 6 billion dollars and counting.

Anyway it's a false dichotomy. The combined costs are under 1% of the US federal budget. I think it's safe to assume that if money is cut from one column it won't be moved to the other column.

18. **Roger**
August 10, 2012

Peter
I buy the argument that such experiments are weak in many respects but they may have a crucial advantage in being able to falsify the SM at a higher energy scale than that of the LHC- I should add that I'm an experimentalist at one of the LHC experiments btw.

I've yet to see the limits in precision on calculations and measurement which we think can realistically be achieved on, eg, g-2_muon and other related experiments etc. If it turns out that there is a realistic gain to be had by using "desktop experiments" to access higher energies then investing in them that would be a punt worth taking in my opinion (we've spent a *lot* of manpower on looking for black holes and other nonsense). The key argument for me would be whether or not the SM calculations and measurement errors would be believable such that a Nobel prize could be given. Very high precision and accuracy are hard to get right.

Regarding the limits one could set on, eg, SUSY that's of small interest (to me). The key thing is, for the first time, we could unambiguously falsify the SM (ok, excluding the neutrino stuff..).

19. **Anon**
August 11, 2012

Hi Roger –The naive standard model prediction for neutron EDM is very small of order 10^{-32} ecm. This assumes just std model with say PQ symmetry to solve the strong CP problem. Experiments will probe the region 10^{-26} ecm to 10^{-29} ecm in the next few years. If it is found experimentally it would be a deviation from just the standard model + PQ symmetry that is the usual way of thinking of neutron edm. For a prediction of lower bounds on neutron EDM based on P and CP symmetries that have been used to solve the strong CP problem (in place of PQ symmetry) please see the paper http://lanl.arxiv.org/abs/1203.2772 — even if new physics is beyond collider reach — say beyond 100's or 1000's of TeV this solution still predicts a deviation from the standard model in a large region of its parameter space.

20. **Patrick**
August 11, 2012

To Peter:

The first argument that comes to mind in favour of LEP3 is certainly the cost, as well stated in your post, but the physics arguments are also numerous. Not only LEP3 can do the same measurements as a linear collider of the same energy, but it does them better. Indeed, most of the Higgs coupling and Higgs mechanism related measurements are statistically limited, and the statistics foreseen for LEP3 is about one order of magnitude larger than that foreseen at a linear collider at 240 GeV, two orders of magnitude larger at 160 (the WW threshold) and three orders larger of magnitude at the Z pole.

The much larger repetition rate and the number of detectors (up to 4), unique to circular machines, are the reasons for this extraordinary performance. If – God forbid ! – the LHC found nothing beyond the Higgs after three years running at 13 TeV, LEP3 would open the door to unprecedented and unchallengeable precision measurements, including measurements of virtual effects at the Z pole, which in the past determined the top quark mass before its discovery at the Tevatron, which a much more modest luminosity. (LEP3 could repeat the LEP1 programme in … 10 minutes!) Now that the top quark and the Higgs mass are known, very small effects due to heavy particles can be unveiled with these precision measurements, and show the direction to follow for the next-to-next machine.

21. **Bernhard**
August 11, 2012

Patrick,

but at a linear collider one could go in principle to 1 TeV of CME and this would beat LEP3 as a discovery machine. I agree with "LEP3 can do the same measurements as a linear collider of the same energy," but I guess a good argument for a linear collider is its high CME and luminosity which it´s sensitive to all sorts of very heavy virtual corrections.

I agree though that to build a "cheap" machine (and much faster, which to me is much more important the the cost) to see what makes sense to build next and at which CME makes sense too.

22. **Roger**
August 11, 2012

Patrick
If, after 3 years running at 13/14 TeV, the LHC finds nothing other than the Higgs then the last thing CERN should do is close down the LHC and replace it with LEP3. The LHC at that point will still only have delivered a fraction of the luminosity intended for that device. Limits on the direct production of heavy new particles could still be extended a lot (this is especially the case for electroweakly interacting particles).

Another point worth making is that a lot of innovation takes place with the development of new accelerator technologies. This is not a reason in itself to go for a costlier option but it is something which ought not to be forgotten.

23. **Roger**
August 11, 2012

Another point springing to mind is that LEP3 would be unique among the major facilities in recent years in being essentially a single topic machine. LEP1,2, HERA, the Tevatron, the LHC all had/have a very broad physics program in addition to their core priority areas. Given the success of LEP1,2 in measuring (very precisely) everything under the sun and the comparatively small increase in centre-of-mass energy in going to LEP3 its unclear whether or not a broad physics program would be in anyway attractive or useful. It would also perforce mean a lot of particle physics research groups moving away from collider physics. That may or may not be a bad thing but I'm pretty sure that it would happen.

24. **anonomous**
August 11, 2012

With our without the LEP3, it sounds like we will have 20 years to stew over the results of the LHC before new HEP results come. I think that astrophysics will be the hot area for uncovering new phenomena but as we have no way to control these events it will be hard to confirm models. It looks like we will have a lot of time to theorize on what BSM physics will look like.

25. **Low Math, Meekly Interacting**
August 11, 2012

Thanks in advance for humoring an utter non-expert…

How realistic is the cohabitation scenario? If it's doable, shouldn't LEP3 be a no-brainer at such a low cost compared to, say, the ILC, which is perhaps decades away from being built in even the most optimistic estimates. It's hard to see how the LEP3 would cannibalize the ILC's budget if it could be built relatively soon. I can't imagine there wouldn't be a wonderful professional symbiosis between the scientists working on two such impressive machines operating in such close proximity. Would the LEP3 somehow preclude the construction of the next generation LHC? Again, if no harm comes to the LHC, why not, BSM physics or no? Precision Higgs physics too pedestrian?

26. **Peter Woit**
August 11, 2012

LMMI,

Politically, I'd guess there's a problem that deciding to build LEP3 would put the idea of an LC on hold for many years.

Practically, even if LEP3 and the LHC can cohabit, building LEP3 would probably involved shutting down the LHC for multiple years during construction, as well as during times LEP3 is running. The cost would presumably come out of the CERN budget, forcing a delay or cancellation of plans for higher luminosity (HL-LHC) or (HE-LHC).

The machines would use the same detectors, and building and running the machines would be the same people as at the LHC now. So, it would be the same people working on both experiments. Right now, I think skepticism about LEP3 is partly skepticism that the thing could be built cheaply, without interfering with the LHC, as advertised, together with worry that it would put a stop to HL-LHC, HE-LHC or LC possibilities.

But some of the commenters here know quite a bit more about this than I do…

27. **Yatima**
August 12, 2012

Well, I didn't that Fermilab has already a website on a Muon Collider Project. I hope to see this running before I kick the bucket!

http://www.fnal.gov/pub/muon_collider/animation.html

28. **Low Math, Meekly Interacting**
August 13, 2012

Thanks for your perspective!

29. **yyy**
August 15, 2012

@Yatima
Building of full-scale Muon Collider will require the following to be built before:
1. Neutrino Factory [non-colliding beam of circa 30 GeV muons]
2. Higgs Factory [two colliding beams of muons 63 GeV each];

It will take few decades.

Short Items

August 12, 2012
Categories: Uncategorized

- There's an interview with the CERN director here.
- John Preskill and others at the Caltech Institute for Quantum Information and Mattter now have a blog here.
- The usual summer workshop on math and physics at Stony Brook is now running at the Simons Center, see videos of talks here. Videos of talks from the Simons Symposium this spring in the Virgin Islands on knot homologies and BPS states are now available here.
- Last month there was a CBMS conference on Unitary Representations of Reductive Groups in Boston, with David Vogan the main speaker. For a nice set of survey lectures and others, see here. If you find the Vogan conference too old-school, a workshop on categorical representation theory this coming week organized by David Ben-Zvi may be more to your liking, lecture notes starting to appear here.
- The only thing stopping me so far from ordering a copy of Francis Farley's novel Catalysed Fusion is that it looks like it is only available in ebook form, and I've until now avoided those and stuck to paper. According to an article in the Telegraph:

  > …a steamy new novel written by a retired physicist lifts the lid on the organisation's studious exterior to reveal an altogether more glamorous lifestyle of wild nights, adrenalin-fuelled sports and romantic trysts…
  >
  > Prof Farley describes a group of young researchers whose groundbreaking work and racy private lives intertwine as they enjoy the high life at Switzerland's top ski resorts and France's best beaches.
  >
  > Prof Farley revealed that he even based a character on himself – Ivan, a physicist and crack glider pilot who is married to a former stripper and sets up a new lab on a nudist Mediterranean island.
  >
  > He told the Daily Telegraph: "We were well paid, we had diplomatic status, no taxes. We got tax-free petrol and drinks and we went out and enjoyed life…
  >
  > "We worked hard and then some people would go home to their families but there were lots of little floozies about and other men had a roving eye, and so did some of the women."

  Perhaps things have changed a bit since the eighties in Geneva….

- Text books for graduate students on SUSY and string theory are coming fast and furious these days. Next month will see Peter West's Introduction to Strings and Branes, a few months ago there was String Theory and Particle Physics: An Introduction to String Phenomenology by Ibanez and Uranga. Even more recent is Freedman and Van Proeyen's Supergravity, which now has one review on Amazon (from "Dan"):

  > This is a must-buy for every high energy theorist who wants to know Sugra. The first nine chapters also make a great source for classical field theory and can be used as a complement for PhD students learning QFT and GR.
  > A wonderful work!

Update: A few more recent and upcoming string theory textbooks are:

- Strings and Fundamental Physics
- Basic Concepts of String Theory
- D-Brane: Superstrings and New Perspective of Our World

Comments

1. **MathPhys**
   August 12, 2012

   I really want to know who 'Dan' is, who wrote that review of Dan Freedman's and van Proeyen's book.

   I must object to this salesman-like behavior that's turning our subject into a circus act. This is science. Remember? Remember your ideals when you embarked on a career in physics? Whatever these were, they definitely were not to sell your colleagues something.

2. **Friend**
   August 12, 2012

   "Catalysed Fusion "… sounds like it might make good episode of Big Bang Theory.

   Just a joke, no offense intended.

3. **Phil**
   August 12, 2012

   MathPhys,

   I find nothing wrong with Dan's review. Salesman-like behavior? What's wrong with recommending a book?

4. **Peter Woit**
   August 12, 2012

   Phil,

   I suspect that MathPhys thinks "Dan" = book's author, and evidently the review is rather similar to the way the author describes his own book.

   I have no idea whether it's normal for authors to write 5 star reviews of their own books at Amazon, maybe it's the done thing. All sorts of odd stuff goes on there; when my book came out a Harvard faculty member was offering people $20 to write bad Amazon reviews of it….

5. **MathPhys**
   August 12, 2012

Phil,

If you don’t already know, it’s pointless to explain it to you. What happened to modesty? Is shame dead? Are we salesmen now? You write a book and let people make up their minds.

6. **Alfred**
   August 12, 2012

   Update: A nice new interview with Ed Witten: http://www.youtube.com/watch?v=sAjuG3GGjUE

7. **voice of temptation**
   August 12, 2012

   MathPhys, that’s an interesting perspective. Would you like to be a guest on our program, “The Decline and Fall of Physics in the 21st Century”? It could lead to a new career for you, as a media personality!

8. **MathPhys**
   August 12, 2012

   Please listen to E Witten’s interview that Alfred has linked above, to hear how a scientist can describe his work, and what he strongly believes in, but modesty, and without using superlatives to describe his own contributions.

9. **Phil**
   August 12, 2012

   MathPhys and Peter,

   How do you know that this reviewer is the author? I see no evidence for it. I also skimmed through the Amazon preview of the book and didn’t see the reviewer’s statement in the book. Do you know where it is?

10. **MathPhys**
    August 12, 2012

    http://www.amazon.com/Supergravity-Daniel-Z-Freedman/dp/0521194016/ref=sr_1_1?s=books&ie=UTF8&qid=1344824763&sr=1-1&keywords=freedman+supergravity

11. **Peter Woit**
    August 12, 2012

    Phil,

    I’ve heard from a reliable source that the review I quoted is very similar to the way the author describes his own book. In any case, I’m guessing that the author is not trying to hide his own identity, since he is using his own name “Dan”. If he were trying to hide behind a pseudonym, he’d use a different name. Again, I’d be curious to hear from anyone who knows more about Amazon reviews: are author self-reviews using their own name common? For all I know, there’s no rule against it. A few minutes of Googling turned up no rule of this sort.

12. **David Appell**
    August 12, 2012

    Geez MathPhys, lighten up. If a textbook author can’t give himself a little one-paragraph boost on Amazon, what’s the world coming to? In this day and age authors need all the help they can get, or before long these kind of books might not even exist anymore.

13. **Q.I.**
    August 13, 2012

    I think it is okay for authors to write a review of their own book, however, IMHO, they must identify themselves as authors. It appears “Dan” has not done that. Reviewer rating is an important consideration for me to buy books from Amazon and I think I should know if the person writing the review is actually an author.

14. **Nadja Summer**
    August 13, 2012

    I´m still dreaming to visit the CERN in Switzerland, you may also check out this interesting interview with Dr. Paul Jackson on http://tinylink.net/75367

15. **MathPhys**
    August 13, 2012

    Q. I.

    Definitely. An author can say “I wrote this book and I think it’s the best book that x, y and z can buy for this and that reason”. All above counter, and nothing at all wrong with that.

16. **Chris Oakley**
    August 13, 2012

    > when my book came out a Harvard faculty member was offering people $20 to write bad Amazon reviews of it

    With (approx) 1,000 String theorists maybe it was his inability to pay the $20,000 bill that caused Lubos to flee the country. I do not remember seeing that many reviews, though.

17. **Christian**
    August 13, 2012

    I’ve gotten all curious. Is the book any good? I already skimmed the Amazon preview, but that does not tell me whether the book is pedagogical and the exercises useful. I’m a PhD student in accelerator physics, and would like an introduction to the subject.

18. **MathPhys**
August 13, 2012

Christian,

I personally expect that Freedman and van Proyen will be a standard reference work on supergravity. I don't know of any other textbook on the subject, except Peter West's which hasn't appeared yet. At 600 pages and $65, it's reasonably priced (for a CUP hardcover) and clearly geared towards students.

19. **KK**
August 13, 2012

If that Farley in the fifth bullet is the Farley from g-2, then he is the guy who in the 60s-80s was well known for participating in the ladies program ("accompanying persons" were ladies then). And I know ladies who found him a nuisance. So I guess that "his steamy new novel" contains his dreams rather than reality.

20. **Hugh**
August 13, 2012

Ive read Catalysed fusion. It's quite fun rather a mish mash. Quite a bit of male fantasy sex. The physics while clearly impossible (involving very low mass high spin leptons) is also fun and coherent. Some of the experimental stuff is well described. It's not Proust but may be almost a male equivalent of 50 shades of grey with added physics (and gliding).

21. **SteveM**
August 13, 2012

Having worked about half way through Freedman's book this year I can honestly say it is a truly excellent and beautiful textbook. Very well written and organized with everything laid out and explained very clearly and precisely, in what is generally a difficult subject that can often become messy and confusing. As you progress through the chapters, you see it all coming together before your eyes and the basic universal structure of SUGRA becomes apparent. The material in the first half of the book alone is worth the asking price. Excellent treatments of field theory, Clifford algebras and fermions, Rarita-Schwinger fields, basic SUSY, gauge symmetries, differential geometry, spin connections etc. etc. Highly recommended.

22. **Gilgit**
August 14, 2012

Since you are talking about text books I wanted to ask a question. I have started going through various physics text books again after a long time away and have reached quantum mechanics. I have plenty of Quantum Mechanics books to choose from, but I always planned to skip anything on String Theory since it seemed so half baked.

But in a few posts a week or two back, Peter commented on how String Theory, when stripped of all the hype, "that there's a case to be made for string theory research based on other things it has led to", spin-offs he called them (this was in response to Bob Jones comment quoting Witten saying: string theory has helped us better understand theories we already have).

I must admit to being totally ignorant of this. Do any of the text books you've listed or can buy on Amazon on String Theory cover these areas? I had assumed the texts were all about untestable unification theories combined with ridiculously advanced math and so never intended to look at them.

I've been slowly covering a lot of physics and math and expect to keep going till I've covered a whole lot more and at some point I might run into these areas so I thought I'd ask what some of these 'spin-offs' are called. I'm not looking for an in depth description of them, just some terms and maybe: this QM book talks about such and such or this ST book actually mentions something useful.

Or is this a dumb question and all the introductory string theory books cover this? Or is the only way I'd encounter any of these topics is if I read dozens of advanced research papers that only 50 people in the whole world can understand?

Thanks

23. **MathPhys**
August 14, 2012

It's too bad, but all stringy textbooks that I know of are devoted to perturbative physics, model building, etc. Some discuss blackholes in string theory and so forth, but that's not the sort of thing that Peter W had in mind.

24. **Peter Woit**
August 14, 2012

Gilgit,

Mostly the string theory textbooks don't cover the mathematically interesting parts of the subject, but focus on trying to explain the failed unification stuff. One of the most interesting areas of spinoffs from string theory is work on 2d conformally invariant qfts. There are several advanced graduate level textbooks that focus on this, one example is

di Francesco, Mathieu, Senechal Conformal Field Theory

Two other good books covering some of this math/physics are

Fuchs, Affine Lie Algebras and Quantum Groups
Gannon, Moonshine Beyond the Monster

For a different interesting area of mathematical physics coming out of string theory, see
Vafa and Zaslow, eds. Mirror Symmetry

Fuch

25. **Bob Jones**
August 14, 2012

Gilgit,

I'm glad you brought this up again because I think there's a lot of confusion among the general public about this aspect of string theory. Unfortunately, popular books and string theory textbooks don't help the situation because they focus primarily on unification. The only exception I know of is the book by Vafa, Zaslow, et al. that Peter mentioned, but you'll need to know a bit of quantum field theory and algebraic geometry before you'll be able to read it.

I don't think it's a good idea to dive into string theory without the proper background. There are so many other fascinating topics to learn about which are much more firmly established than string theory. I would start by learning quantum mechanics (the book by Griffiths is the standard text) and then try to learn some basic quantum field theory and relativity (see for example the book by Zee and the one by Baez and Muniain). Eventually you'll be able to read more advanced books on quantum field theory and string theory (like the two volumes published by the IAS).

If you just want to learn more about applications of string theory, there several accessible introductions on the internet. For example, see

http://www.nature.com/news/2011/111019/pdf/478302a.pdf

http://www.sns.ias.edu/~malda/sciam-maldacena-3a.pdf

http://www.sns.ias.edu/~malda/Published.pdf

http://www.sns.ias.edu/~witten/papers/KnotsandPhysics.pdf

http://physics.berkeley.edu/events/Colloquia/movies/col.streaming.3-12-12.mov

http://media.scgp.stonybrook.edu/video/video.php?f=20120411_2_Arkani-Hamed_qtp.mp4

26. **Gilgit**
August 15, 2012

Hey, that's nice of you guys. I appreciate the references and links.

And I certainly plan on going through many different Quantum Mechanics/Field Theory books. I've spent the past year on Amazon poking around. An amazing resource – the reviews (taken with a grain of salt) will let you know which are the most popular books, which are the classics, what are the other books at the same level as this one, comparing several books on the same topic, etc.

And the lists Amazon has – one guy taught himself General Relativity and then wrote up 7 lists comparing books on General Physics, Math for Physics, Special Relativity, General Relativity, Differential Geometry, Tensors… specifically from the point of view of a self learner.

And the internet has a fair number of solutions manuals – either written by grad students or official ones – to many physics texts (including Griffiths, Sakurai, and a few other QM books).

In fact one professor in France wrote the solutions, in English, to every problem in "A First Course in General Relativity" (Schutz) and posted it for anyone to download. He even added a few more intermediate problems designed to help students figure out how to do the more complicated problems.

The reason I brought it up (boy am I off topic now) is that I kind of figured there would be lots of people who posted solutions to popular texts. Kind of like how in the olden days people would write commentaries on Greek Philosophers or on Confucius and then other people would write commentaries on commentaries, etc. Seems like an easy way to get noticed. Similar to budding programmers writing useful open source programs… but it hasn't happened so maybe what I think is a good idea isn't as good as I thought.

At least there are quite a few published problem books out there – books with hundreds of problems with full solutions. I'll bet there are at least 5 or 6 Quantum Mechanics books like that now. Plus others on Field Theory, Differentiable Manifolds/Riemannian Geometry – searching on Amazon I even found "Problem Book in Relativity and Gravitation" with hundreds of GR problems from 1975! Who knew?

So if I really get stuck I believe there are resources out there to get me going again. I'm sure it will take only 5 or 10 years or so to learn all of this, but at least I think it's possible.

Thanks again.

’t Hooft on Cellular Automata and String Theory

August 13, 2012
Categories: Uncategorized

Gerard ’t Hooft in recent years has been pursuing some idiosyncratic ideas about quantum mechanics; for various versions of these, see papers like this, this, this and this. His latest version is last month’s Discreteness and Determinism in Superstrings, which starts with cellular automata in 1+1 dimensions and somehow gets a quantized superstring out of it (there are also some comments about this on his web-site here).

Personally I find it difficult to get at all interested in this (for reasons I’ll try and explain in a moment), but those who are interested might like to know that ’t Hooft has taken to explaining himself and discussing things with his critics at a couple places on-line, including Physics StackExchange, and Lubos Motl’s blog. If you want to discuss ’t Hooft’s ideas, best if you use one of these other venues, where you can interact with the man himself.

One of ’t Hooft’s motivations is a very common one, discomfort with the non-determinism of the conventional interpretation of quantum mechanics. The world is full of crackpots with similar feelings who produce reams of utter nonsense. ’t Hooft is a scientist though of the highest caliber, and as with some other people who have tried to do this sort of thing, I don’t think what he is producing is nonsense. It is, however, extremely speculative, and, to my taste, starting with a very unpromising starting point.

Looking at the results he has, there’s very little of modern physics there, including pretty much none of the standard model (which ’t Hooft himself had a crucial role in developing). If you’re going to claim to solve open problems in modern physics with some radical new ideas, you need to first show that these ideas reproduce the successes of the estabished older ones. From what I can tell, ‘t Hooft may be optimistic he can get there, but he’s a very long way from such a goal.

Another reason for taking very speculative ideas seriously, even if they haven’t gotten far yet, is if they seem to involve a set of powerful and promising ideas. This is very much a matter of judgement: what to me are central and deep ideas about mathematics and physics are quite different than someone else’s list. In this case, the central mathematical structures of quantum mechanics fit so well with central, deep and powerful insights into modern mathematics (through symmetries and representation theory) that any claim these should be abandoned in favor of something very different has a big hurdle to overcome. Basing everything on cellular automata seems to me extremely unpromising: you’re throwing out deep and powerful structures for something very simple and easy to understand, but with little inherent explanatory power. That’s my take on this, those who see this differently and want to learn more about what ’t Hooft is up to should follow the links above, and try discussing these matters at the venues ’t Hooft is frequenting.

Comments

1. **Robert Smart**
   August 13, 2012

   I hope you’ll also comment on Alain Connes new paper. It does try to be consistent with the standard model, but apart from that it’s too hard for me.

2. **Gerard 't Hooft**
   August 13, 2012

   Even though my work is here sketched as “not even wrong”, I will avoid any glimpse of hostility, as requested; I do think I have the right to say something here in my defense (One positive note: “Not even wrong” sounds a little bit better than “Wrong wrong wrong” on another blog …).

   First, I agree that cellular automata doesn’t sound very sexy; those who have seen Wolfram’s book will certainly be discouraged. But I want to stress as much as I can that I am striving at a sound and interesting mathematical basis to what I am doing; least of all I would be tempted to throw away any of the sound and elegant mathematics of quantum mechanics and string theory. Symmetries, representation theory, and more, will continue to be central themes.

   I am disappointed about the reception of my paper on string theory, as I was hoping that it would open some people’s eyes. Perhaps it will, if some of my friends would be prepared to put their deeply rooted scepsis against the notion of determinism on hold.
   I think the mathematics I am using is interesting and helpful. I encounter elliptic theta functions, and hit upon an elegant relation between sets of non-commuting operators p and q on the one hand, with integer, commuting variables P and Q on the other. All important features of Quantum Mechanics are kept intact as they should.

   I did not choose to side with Einstein on the issue of QM, it just came out that way, I can’t help that. It is also not an aversion of any kind that I would have against Quantum Mechanics as it stands, it is only the interpretation where I think I have non-trivial observations.
   If you like the many world interpretation, or Bohm’s pilot waves, fine, but I never thought those have anything to do with the real world; my interpretation I find far superior, but I just found out from other blogs as well as this one, that most people are not ready for my ideas. Since the mud thrown at me is slippery, it is hard to defend my ideas but I think I am making progress.

   They could well lead to new predictions, such as a calculable string coupling constant g_s, and (an older prediction) the limitations for quantum computers. They should help investigators to understand what they are doing when they discuss “quantum cosmology”, and eventually, they should be crucial for model building.
   G. ’t H

3. **Peter Woit**
   August 13, 2012

   Prof. ‘t Hooft,

   Thanks for writing here with your reaction to and comments on the blog posting. I hope you’ll keep in mind that I often point out that “Not Even Wrong” is where pretty much all speculative ideas start life. Some of the ideas I’m most enthusiastic about are certainly now “Not Even Wrong”, in the sense of being far, far away from something testable.

   While my own enthusiasms are quite different than yours, and lead me to some skepticism about your starting point, the reason for this blog posting was not to launch a hostile attack, but to point others to what I thought was an interesting discussion, one which many of my readers might find valuable to know about.

   Good luck pursuing these ideas, may you show my skepticism and that of others to be mistaken…

4. **Anonymous**
   August 13, 2012

   Prof. ‘t Hooft,

   While I am not familiar with your particular work, I am familiar with previous explorations on the theme of interpretations on quantum mechanics and determinism, particularly with old things such as de Broglie-Bohm's theory, Bell's contextual ontological model, Kochen-Specker's model, and newer things such as Harrigan & Spekkens classification of ontological models, Lewis et al. psi-epistemic model, Hardy's excess baggage theorem, etc. But after studying them with interest for a while, I gradually developed the opinion that they have no good motivation, use uninteresting mathematics, and have been generally fruitless. Since then I have stopped paying attention to this area of research.

Since you seem to be interested in defending your work and, furthermore, in publicizing it, I would be very interested in knowing what's the difference from these previous explorations and, more importantly, what's the motivation for you to begin work in this area (since you claim not to be motivated by an aversion to Quantum Mechanics).

I do hope you can convince me to study your work, and perhaps an answer could be useful to other researchers as well.

5. **Tim May**
August 13, 2012

I think it's great that Prof. 'T-Hooft (hope I got the capitalizations and apostrophes right) is commenting here. This site is in many ways much less hostile than other sites we could all name easily.

I started out (well, in terms of blog years, not my physics education several decades ago) as a string critic. I didn't find Brian Greene's first book very persuasive (nor a book from the 1980s, I forget who wrote it). I was receptive to Lee Smolin's "Three Roads to Quantum Gravity" and to this blog, even before the book came out.

However, partly as a result of reading this blog for about half a dozen years (I think), I've gradually warmed to a lot of the string stuff. Not the Kaku stuff, but the Arkani-Hamed types of stuff.

The fact that we may be 10-15 orders of magnitude away from probing the energies needed to test some of these theories is, of course, daunting. And I think both Woit and Smolin were basically right to warn of the "We are only interested in hiring string theorists" situation a while back.

However, things seem to be on a somewhat better keel today. Or so is my perception from this blog and others. And of course the stuff coming out of cosmology and observational astronomy is just plain exciting.

How it links with math is also exciting, albeit probably decades or even centuries off in terms of real experimental links.

Exciting times. And I think "Not Even Wrong" is useful for reminding its readers to doubt some popular theories. Sort of like the guy the Romans used to hire to ride behind the triumphant god-king warrior to remind him he is not really a god.

As for CAs, I'd've thunk (a technical term 🙂) that Kochen-Specker and other no-go results rule out strictly local (as CAs seem to be) theories. But I look forward to seeing Prof. 'T-Hooft enter this arena of debate.

–Tim May

6. **Bernhard**
August 14, 2012

Prof. 't Hooft,

I think the key point in all this is in your last paragraph:

"They could well lead to new predictions, such as a calculable string coupling constant g_s, and (an older prediction) the limitations for quantum computers. They should help investigators to understand what they are doing when they discuss "quantum cosmology", and eventually, they should be crucial for model building."

Regardless of what people think about your ideas (most of the people probably just dismiss it without reading it after hearing about what you are claiming) the key point with new interpretations of Quantum Mechanics is suggesting an experiment where it can predict something new (OK, saying this to a Nobel prize professor sounds perhaps very pretentious, but I just want to express my deepest concern about your message). A new paper based on this one, but concentrated only on the "what's measurably different" in all of what you are saying, would (I suppose) attract attention and perhaps put skepticism on hold for a while. I confess I find difficult to digest your paper but if I could understand better where could it measurably matter then this would be a different story. Otherwise it will probably stay as a curiosity.

Let me finish saying I deeply admire your courage on working with fundamental problems in QM and as I semi-young physicist I even more deeply envy the freedom you have to pursue your own ideas.

7. **Anonyrat**
August 14, 2012

Prof 't Hooft is in exactly the right situation to play with highly speculative ideas that a young postdoc. cannot afford to do.

8. **Neil**
August 14, 2012

If anyone has "earned the right" to engage the profession with speculative ideas, it is Professor 't Hooft with his track record of brilliant theoretical insights. I look forward to learning more about this.

9. **Anonymous***
August 14, 2012

't Hooft,

Could you explain why you think the Bohm theory is rubbish? It seems to me that it has already done exactly what you are trying to do — provide a perfectly consistent alternative to the standard approach in quantum mechanics.

I also don't think that you are siding with Einstein on quantum mechanics. Einstein made clear that he did NOT think something like Bell's inequality could exist, but it DOES! He was very clear that his thinking would change if something like a Bell inequality was discovered, in particular it would lead to something along the lines of a Bohm interpretation.

10. **anonII**
August 14, 2012

At first, I did not believe it was GT – but doing the ('t) correctly (twice even) gave it away. Thanks for posting.

11. **Yatima**
August 14, 2012

I posted this before and I will post it again!

Ray Streater on Bohmian Mechanics:

http://www.mth.kcl.ac.uk/~streater/lostcauses.html#XI

12. **Peter Woit**
August 14, 2012

Unless Prof. 't Hooft himself wants to answer and discuss the topic, enough about Bohmian mechanics, please.

13. **Danny**
August 14, 2012

`t Hooft is one of the good guys, an incarnation of Dirk Foster and a physicist of the highest calibre. This deterministic programme seems Just Possibly True, and should probably serve as inspiration for the sort of paradigm-shifting research everyone seems to advocate (but no-one pursues). My humble opinion.

14. **Jeff**
August 14, 2012

I think the biggest issue here is that 't Hooft (clearly a brilliant physicist) ignores Conway/Kochen, who clearly prove, given three axioms, that the universe *can't* be deterministic. Actually, they prove it isn't "random" in the usual sense either. And the axioms one needs to assume are essentially completely straight QM and relativity, together with the inability to influence past events.

15. **Peter Orland**
August 14, 2012

Though I don't necessarily accept 't Hooft's scenario, I am a little surprised by some of the criticism. The no-go theorems mentioned above are not relevant.

There is no contradiction for quantum evolution to occur in a deterministic system. A cellular automaton, for example, can be regarded as a transfer matrix with a special property. This property is that given any initial (basis) state, the matrix element is not zero for a unique choice of final (basis) state. Such transfer matrices can be unitary, hence identified with quantum evolution.

I don't know whether the scenario is useful or not. It is certainly not, however, ill-conceived.

16. **Bob Levine**
August 14, 2012

There are two problems with Jeff's dismissal of 't H.'s approach; Peter Orland has touched on one. For the other, see

http://philsci-archive.pitt.edu/8437/1/WuthrichChristian2010PhilSci_IndeterministicWorld.pdf

Wüthrich's observation that even Conway and Kochen's scenario depends on access to information about—in fact is a *function* of—prior events (which, NB, has nothing to do with influence over prior events) seems like a huge analytic challenge to C&W's core thesis. Obviously, the game is still in play, and meanwhile, you can't appeal to C&W as any kind of decisive refutation of what 't H. is proposing.

17. **Peter Orland**
August 14, 2012

… and it is not the same as DeBroglie/Bohm mechanics. Or Nelson's for that matter.

18. **Jeff M**
August 14, 2012

Peter O,

I'm a bit confused about why you feel Conway/Kochen isn't relevant. Prof. 't Hooft is more than a good enough mathematician to understand that his system somehow violates one of their 3 axioms, but I can't from his writeup figure out which one it would be. Then again, if you read his paper, he seems to be ducking the issue "The philosophy used here is often attacked by using Bell's inequalities[1]—[3] applied to some Gedanken experiment, or some similar "quantum" arguments. In this paper we will not attempt to counter those (see Ref. [16]), but we restrict ourselves to the mathematical expressions." Since the discussion in [3] is a valid mathematical proof, I can't understand ignoring it.

19. **Peter Orland**
August 14, 2012

Jeff,

't Hooft's model does not specify values of observables. It has deterministic evolution of basis states. That is why the theorems you quote do not apply.

20. **Jeff**
August 14, 2012

Peter o

Thanks, been too long since I thought like a physicist. :-). I'm reading as a mathematician, and assumed "deterministic" had the usual meaning.

21. **Raisonator**
August 15, 2012

Just another link, which may be helpful in this context:

http://www.youtube.com/watch?v=lGB3oVxivhE

22. **Maynard Handley**
August 15, 2012

"Though I don't necessarily accept 't Hooft's scenario, I am a little surprised by some of the criticism. The no-go theorems mentioned above are not relevant.

There is no contradiction for quantum evolution to occur in a deterministic system. A cellular automaton, for example, can be regarded as a transfer matrix with a special property. This property is that given any initial (basis) state, the matrix element is not zero for a unique choice of final (basis) state."

This (cellular automaton as a transfer matrix) may or may not be true but it's not interesting. The problem of interest here is not alternative computational models of QFT (ie alternative ways to solve PDEs or calculate path integrals). The problem of interest is WHY DOES SOMETHING LIKE COLLAPSE OF THE WAVE FUNCTION OCCUR?

The deterministic answer, in all its forms, whether Bohm or many universes or whatever, is to claim that this "collapse" is a misunderstanding, that if you define the problem properly, then some deterministic combination of initial conditions plus minor perturbations along the way lead you inevitably to the specific state that you measured.
The nice thing about this world view is that it's compatible with relativity — it doesn't bring up any (so far completely unresolved IMHO) questions about "when" does this collapse occur, bearing in mind that "when" is a relative concept and so does the collapse "propagate outward" at the speed of light from some initiating point (very problematic) or does it happen "simultaneously" (hmm, that's not a very relativistic word) (perhaps simultaneously over some especially blessed world surface?).

OK, so determinism is nice. Only problem is that it appears to be wrong wrong wrong. Bell's inequality is one version of why it's wrong, but the deeper reason it is wrong is that it uses a broken mental model of the relationship between probability and QM.
Probability is based on the idea of underlying space \Omega, a sigma field of events, and a measure associated with the sigma field. On top of this we construct random variables which (and this is the important part) are all COMPATIBLE. That is, for any random variables X and Y, the concept of a joint distribution,say F(X, Y) is well defined. At root, this is because the sigma fields defined by X and Y are subfields of the underlying \Omega sigma field, and we can construct an intersection of them. Even more fundamentally, this is because the "building up" operator we're working with is set union, and set intersection plays nicely with set union.

Now we switch to the world of operators and vector spaces.
Given one particular operator O, this has a set of eigenvalues.
Associated with each set of eigenvalues (-\infinity, \lambda] is a vector space,call it V_\lambda.
IF in addition we are given a vector \psi, we can now associate a real number with V_\lambda, namely the length of the projection of \psi into V_\lambda.
This in other words gives us a monotonic increasing function (a measure) associated with increasing \lambda.
This may look unfamiliar, but it's really not scary, it's the usual stuff you are familiar with — an eigenvalue and associated with a "probability" for that eigenvalue given by , only made a little more rigorous and described in the language of probability.

This monotonic increasing function associated with increasing \lambda is just like the cumulative distribution function associated with a random variable,and because of that, people have for almost a hundred years being slipping informally between operator language (eigenvalues, eigenspaces, "probability associated with an eigenvalue") to an implicit assumption that we are dealing with full-blown probability theory and random variables. We are NOT.
Things fall apart if we consider now a second operator, call it Q, which does not commute with operator O. Whereas two random variables ARE always "compatible" in the sense that I stated earlier, specifically, that they have an associated joint distribution (and an associated underlying set of points \xi \in \Omega each of which represent "initial conditions" which might lead to a particular joint outcome of X=x and Y=Y; this sort of thing is no longer true for non-commuting operators O and Q.
Q (and vector \psi) generate another spectrum of eigenvalues, each with an associated weight, and so can also, apparently, be thought of as random variable.
But there is no "compatibility" between these two random variables. More specifically, there IS NO finer sigma algebra which contains both the sigma algebras generated by the O-"random variable" and the Q-"random variable".
At root, this is because the fundamental "points" we are dealing with when we treat O as having an associated random variable are not simple points, they are vector spaces, and the building-up operation as we aggregate these is not a union of sets of "simple points", it is a cartesian product of vector spaces. But the cartesian product does NOT play nicely with intersection the way union does (we don't have the full set of De Morgan laws).

Or to put it slightly differently, given a set \Omega, the set which is actually relevant to QM is C^\Omega, and the measures defined by operators apply to this set C^\Omega "sliced by cartesian product" along different angles for different operators. This is different from standard measures which derive by slicing \Omega "by union". The union slices, when intersected, still give useful sets. Cartesian product slices, when intersected, simply give the set {0}, not useful structure.

I know this sounds like a whole lot of weirdness, but there's nothing unorthodox here — it's just standard probability theory, and standard Hilbert space theory interpreted as measure theory. But, IMHO, this conceptualization is, once you understand it, extremely powerful in revealing where the true weirdness of QM lies. In particular, again IMHO, it's as powerful a mathematical argument as we're ever going to get that the underlying idea of many-world theories. I've never seen a real mathematica formulation of a many-world theory, so I've no idea what the proponents actually mean; but as far as I can tell, what they mean is essentially a probabilistic model: the multiverse consists of some unfathomably large set \Omega of points \xi, each \xi corresponding to a universe, with some sort of measure tied to subsets of \Omega, and our universe is the deterministic unfolding of one of these \xi. As I've tried to explain, you just can't get this to work, because the model only works when you "aggregate points by union", and QM doesn't do that.

23. **Peter Orland**
August 15, 2012

Maynard,

Bell's inequality is not violated in 't Hooft's model. It is just a way to formulate quantum mechanics. It is deterministic in the way that one basis vector is sent to the next, during some discrete time interval. This is a linear unitary map (both vectors are normalized), hence just quantum mechanics. Nothing is any different concerning collapse or no-collapse of wave functions.

A trivial example of this kind of model (much simpler than what 't Hooft considers) can be done with two spin states, s1 and s2. The transfer matrix sends s1 to s2 and s2 to s1. It therefore has the representation of the Pauli matrix sigma1. Well, that is a unitary evolution operator. A slightly less trivial example is a permutation of N objects, with N>2 (which can have complex eigenvalues, with unit norm). Any permutation can be represented as a unitary N X N matrix. Hence it can be written as the logarithm of i (sqrt of minus 1) times the time interval times a Hamiltonian (also an N X N matrix). The Schroedinger equation is satisfied, in the sense that at its solution is the state vector. The eigenvalues of the transfer matrix do not have to be real numbers (though its components are real).

This kind of model is not a traditional hidden-variable theory in the sense of De Broglie, Bohm, Nelson or anyone else. Unlike those theories, you cannot specify all observables simultaneously. You can specify certain observables at discrete time intervals, but the uncertainty principle is intact.

I am not saying you should accept the idea, just that you need to see it for what it is.

24. **Peter Orland**
August 15, 2012

Anyway, I would prefer not spending time defending this. I wish I did not get incensed when I see people's work criticized for the wrong reasons.

25. **Peter Orland**
August 15, 2012

I meant to say Bell's inequality is violated. It is just QM.

26. **Maynard Handley**
August 16, 2012

Thanks, Peter.
I don't want to thread jack, but how does what you say fit in with Peter's statement about "One of 't Hooft's motivations is a very common one, discomfort with the non-determinism of the conventional interpretation of quantum mechanics."

I will admit I have not yet this particular 't Hooft's paper (though I have pretty much always been pleased with when I have read something by him) but I assumed, from that, that this WAS the point at issue. Hence my long attempt to summarize the issues in play and my views on them.

If all we have is "deterministic evolution of the wave function" then why is Peter saying what he is saying?

27. **Peter Woit**
August 16, 2012

Maynard,

If you read my posting, you'd notice that its point is to explain why I'm not trying to seriously understand exactly what 't Hooft is doing. So, I'm not in any position to participate in a discussion of this sort. Actually, neither are you, since you admit you haven't read his paper. Enough about this.

28. **jim**
August 16, 2012

Here's a more concise rebuttal of Streater: http://www.ilja-schmelzer.de/realism/BMarguments.php

29. **Peter Woit**
August 16, 2012

No more Bohmian mechanics. At all, ever.

30. **Sandro**
August 26, 2012

Prof 't Hooft, don't get discouraged by the opposition to your cellular automota approach to QM. Computer science is still in its infancy, and it's impact on mathematics and physics has only just begun.

I think your approach via minimalistic cellular automota is very promising, and the recent anti-determinism bias is just a fad. QM has equally expressive deterministic and non-deterministic formulations, and an exploration of the limits of deterministic formal systems given the current empirical evidence is essential. There seems little reason to posit non-determinism if it is not necessary to do so, as deterministic explanations invariably have more explanatory power.

SUSY 2012, and Strassler on the String Wars

August 15, 2012
Categories: Uncategorized

This post was originally going to be just about the latest SUSY exclusion results announced at SUSY 2012 and their significance, but I realized there's nothing much new to say, and it would be tedious to just write the same things. ATLAS new results are here, including some using this year's 8 TeV data. As before, not a hint of SUSY in the CMS or ATLAS data. Now that the expected places to find SUSY have shown nothing, emphasis is on how to search more obscure corners of the 100 + parameter space of the theory which are accessible at the LHC, but hard to study. For a good recent survey of this effort, see Matt Reece's recent presentation here.

SUSY 2012 featured a plenary talk by Gordon Kane, promoting his string theory "predictions". As usual, the gluino is right around the corner. Back in 2000, it was supposed to be around 250 GeV, with SUSY discovery at the Tevatron in 2001-3 (see here). Last summer the "prediction" was 600 GeV, just above the 500 GeV limits. Last December, the gluino discovery was imminent, by summer 2012. The latest news is that the gluino mass prediction is now 1 TeV, just above the 800 GeV limits according to Kane. You can watch the gluino move by comparing the same "prediction" plot on slide 22 here and slide 34 here. Kane now claims in bold face that "**String/M theory prediction is that no gluino signal expected so far**" (page 35) referring (I think) to this paper, which seems to say nothing of the kind. This is just getting more and more bizarre.

Matt Strassler (described here as "the chief US theoretician") has rarely been critical of string theory hype (although he did in a comment refer to Kane's claims as "garbage and propaganda"), preferring to see prominent blogging critics of string theory and SUSY hype as the bigger problem to deal with. Today though he took a dramatic and rather admirable step, with a posting about string theory that starts off with:

> …the theory's been spectacularly over-hyped, and the community's political control of high-energy physics in many U.S. physics departments has negatively impacted many scientific careers, including my own.

He goes on to cast himself as a lonely moderate surrounded by two teams of extremists, arguing that

> it is high time the ball were grabbed by the referee and placed quietly in the middle of the field where it belongs.

His refereed position in the middle of the field would have acknowledgement that "string theory cannot be tested at present, and that situation might continue for a very long time, perhaps centuries", while lauding string theory for providing a range of important insights into other problems than unification. This refereed position seems to me already pretty much the mainstream position of string theorists I know. My problems with it are that it still allows the heavy promotion of a failed idea (string unification as "our best bet", even if mysteriously "hard to test"), and the over-hype problem is also prevalent among discussion of string theory applications to other parts of physics.

Strassler gives an interesting example of how some ideas from string theory ended up providing inspiration for advances in amplitude calculations, although the main heros of the tale are the phenomenologists who have done the hard work behind these advances (see his exchange with the anonymous "dude" in the comments).

Oddly enough, what seems to have motivated Strassler to take this new public stand are the recent $3 million prizes awarded to his colleagues down the road at Princeton. He devoted this recent posting to attacking me and *Nature News* for quoting me about the prizes, but ended up agreeing with some of my concerns, specifically:

> What upsets me is that there is a long, long list of deserving scientists, some of whom have received little recognition despite their important work, and some of whom could really use some research money and/or time off from teaching — and Milner overlooked them all…
>
> Philanthrophy needs to be done with the consent and participation of the beneficiaries; otherwise it generally fails, and sometimes it causes complete disasters. For instance, you can completely destabilize an organization that is functioning well if you just hand one of its members a million dollar check without understanding the implications…
>
> Anyway, as far as I can tell, the Milner prize is one thing our field didn't really need. I can think of a few things we really do desperately need, at least in the U.S.

It's pretty clear where Strassler thinks the money should have gone:

> we have too many string theorists teaching at the top U.S. universities, and not enough theorists doing other aspects of high-energy physics, including Standard Model predictions…

He explained in the earlier posting that he has been trying to raise private money for an LHC Institute, but that this has failed because of the recession. I don't know any details of this, but I do know that about five years ago he and Arkani-Hamed were proposing something like this to the NSF, with the two of them as co-directors. This foundered not because of the recession but because reviewers didn't much like the idea of giving a lot of new money to well-funded theorists at Princeton and Rutgers, largely to retool string theory groups into LHC phenomenology groups.

That proposal advertised the likelihood of discovery of SUSY or something equally dramatic about a year after first beams at the LHC, with a large group of theorists needed to sort out the "LHC Inverse Problem" of how one was going to figure out the underlying physics responsible for the confusing plethora of non-SM signals the LHC would be seeing. Perhaps a reason for finding it hard to get funding for a theory LHC institute these days is not the recession, but the lack of any such advertised signal. On the other hand, with $12 million of cash in their pockets now, the IAS theorists should be able to themselves privately fund the proposed Arkani-Hamed/Strassler center, if they still think this a good idea.

Comments

1. **anon.**
   August 15, 2012

   Gordy's backup slides are still touting the Fermi-LAT 130 GeV gamma ray line as a possible signal of wino dark matter. This is a superficially attractive idea that is *completely* incompatible with the data from continuum gamma rays, among other bounds. He also is quoting out-of-date LHC bounds, which now put gluinos decaying through off-shell third generation particles above 1 TeV, not just 800 GeV.

   It's frustrating for those of us who sympathize with some of his arguments against other popular approaches to model-building that he's so overzealous in promotion of his own ideas that no one takes him seriously anymore, and attempts to argue along similar lines are treated as presumptively not worth listening to….

2. **Bernhard**
   August 15, 2012

   Does anyone know if there was any audience reaction to Kane´s claims? He went to the biggest BSM conference to tell those lies? I fear that if he says enough times people will start believing he did what he claims (predicting the Higgs mass with gluinos always around the corner, despite of what he said the day before).

One could say nobody take him seriously anymore, but then again, he was invited for a plenary talk in one of the most disputed conferences in HEP…

3. **Bernhard**
   August 15, 2012

   Sorry, for two comments, but I can´t resist: "Physicists at CERN believe that we'll probably first encounter supersymmetrical particles in early 2015, when the power of the Large Hadron Collider will have increased by a factor of two."

   We are back in time, it´s 3 years ago again and we juuust need to turn on the machine to find SUSY! It´s hilarious!

4. **SteveB**
   August 15, 2012

   I had read Strassler's comments before your post. I didn't get the impression that he was claiming to be "the" referee, but was using the sports analogy to hope for more middle ground. (I believe he would say he is in this middle ground.) I don't think you should get on his case (even though you did it rather mildly) for only recently commenting on the over-hype. He hasn't been doing a public blog that long, and it has mostly been educational rather than commentary. Even when doing commentary, it isn't a requirement that one must speak out on all one's positions, knowing that attacks will be coming. I could not do what you do — my skin is not nearly thick enough to endure the responses — so thank you again for this blog. But not everyone is so willing.

5. **Peter Woit**
   August 15, 2012

   SteveB,

   I was quite serious when I described as admirable his finally criticizing the over-hype problem. Would have been even more admirable if he'd done it earlier. The situation in the particle theory community for decades has been kind of grotesque, with most people privately agreeing that the situation with string theory was problematic, few willing to say so publicly for fear of retribution. I hope we're seeing a change in this environment, towards one where people can have honest conversations about the issues, without fear that someone is going to come after them personally for disagreeing with them about a scientific issue.

6. **mhm**
   August 15, 2012

   So we want the ball in the middle of the field. Ok. The teams playing are string theory, and who else exactly? More garbage on random BSM model building?

7. **Eric**
   August 15, 2012

   I think it should really be pointed out that the most likely scenario involving SUSY at this point is that with R-parity violation. In SUSY with R-parity violation, the LSP is no longer stable and so there are no MET signals. All current SUSY searches to date have looked for these MET signals assuming R-parity is conserved. So, R-parity violating SUSY is basically unconstrained by any of the current data from LHC.

   It should also be mentioned that R-parity was only added ad-hoc to the MSSM in order to eliminate operatorators which allow rapid proton decay. In certain extensions of the MSSM, it is possible eliminate the problem of rapid proton decay in other ways. In particular, if there are extra local U(1) factors which couple to either baryon or lepton number, then the B and/or L violating operators can be eliminated, also stabilizing the proton from rapid proton deay. In this scenerio, it is not necessary to invoke R-parity, and models of this type are basically unconstrained by LHC limits on superpartners.

8. **physicsphile**
   August 15, 2012

   Eric,

   Isn't R-parity also needed for the MSSM to provide a sufficiently stable dark matter candidate?

9. **APP**
   August 16, 2012

   Eric,

   The experimentalists are well aware of RPV SUSY and many searches have been done for the modified MET-free signals (typically multi-jets or multi-leptons). The limits on sparticle masses are almost as stringent as the R-parity conserving case — 1st/2nd generation squark mass lower bounds in range 700 to 760 GeV (ATLAS 7 TeV data), and gluino mass bounds in range 760 GeV to 1.77 TeV (ATLAS 7 TeV data). In other words RPV doesn't buy you very much, unless one sits at rather special points where other things happen as well (like significant displaced vertices, see recent paper of Peter Graham etal http://arxiv.org/abs/arXiv:1204.6038). If low-energy SUSY is somehow hiding within the LHC data it is doing it in a much cleverer and more subtle way than straightforward RPV.

   Physicsphile,

   You are correct that R-parity conversation can give a dark matter (DM)
   candidate, but in fact it has been known for many years, at least 10, that standard SUSY neutralino DM is quite finally-tuned. In much of R-parity conserving SUSY parameter space one does NOT get a successful DM candidate. The SUSY-DM connection is one of those things that has been way overhyped. Many of us working in the field have thought that the Peccei-Quinn-Weinberg-Wilczek axion (with suitable axion decay constant—there is disagreement about the "best" value) is more likely as the DM.

   Peter,

   Many of us working in high-energy particle theory have for years argued against the domination of particle theory by string theory, especially at the "top" US/European universities which train the majority of the next generation of theorists (there have been exceptions, especially Berkeley, MIT, U Wash, Oxford, Pisa, but even these are under threat). However, like Matt, we have chosen to do this behind the scenes believing this the strategy more likely to succeed. It's certainly true that we haven't been all that good in resisting/reversing the takeover by string theory! This continuing takeover is greatly assisted by a) the books by Lisa and Brian which persuade many very bright students that the most exciting area is string theory, and b) the prize- and grant-giving bodies which still greatly favor string theory, partially because the older generation of famous theorists who make the decisions are dominantly strong supporters of strings. Among the most famous US theorists, probably only Frank Wilczek has publicly argued that too many resources were going into ST (though others have done so behind the scenes). The Milner Prize probably makes this whole situation worse so I can understand Matt's frustration. I should say that I'm happy for ST to get reasonable support. The problem, as you've said, is that too many resources have gone into an area that isn't really physics because most of the answerable questions are purely theoretical and have no relation to any conceivable experimental probe (Kane's "predictions" are not predictions at all but reflections of many special choices he's made—no one outside his small coeterie takes him seriously). Sadly for all the students who decide to work in ST the field is in a bad way with very little interesting work going on—witness the growth AdS/CFT "condensed matter" physics which most condensed matter physicists view as not having anything to do with real systems and having discovered no new phenomena.

10. **Bernhard**
August 16, 2012

mhm,

ST is not playing at all, is has its own separate game and it doesn't invite outsiders to play with. Luckily nobody else wants to play with it anyway.

11. **Peter Woit**
August 16, 2012

APP,

Thanks for the comment about RPV theories!

That string theory has been heavily overhyped isn't at all a controversial claim, with even most string theorists privately agreeing this is the case (and complaining that they are now suffering from the backlash due to hype they weren't responsible for). It's remarkable though how few physicists have been willing to be quoted about this publicly. One reason is fear of retribution, but I think a bigger reason is that "string theory" has become such a complex topic that few people (even few string theorists…) feel comfortable making any broad statements about its problems.

I've found it highly problematic over the years that when journalists talk to me, and ask: "OK, you seem to make sense, but name a bunch of other theorists who we can talk to who agree with you about the overhype problem", there are few people I can send them to. Matt believes that it's an atrocity that journalists quote me since I don't have the right credentials. At least now, since he's "the chief US theorist" in the media, and is willing to acknowledge the overhype problem, that should help. Having others say something publicly would help too.

At least in the US, the problem of string theory influence has changed a bit. Few places (except Princeton) will hire string theorists at the tenure-track level, with physics departments well aware of the overhype problem. But the top institutions are still largely filled with senior string theorists, providing few opportunities for the best students to get trained in other areas. These same people also have outsized influence on grants, prizes, etc., with the new money from Milner just exacerbating that problem.

12. **anon.**
August 16, 2012

APP, can you provide links to the limits you're quoting? The latest CMS search for RPV gluinos in 3-jet resonances (through the UDD operator), for instance, is setting a limit below 500 GeV on the gluino mass. I imagine limits are stronger for the lepton-number violating processes, but I don't know where to find the strong bounds you're quoting. In the hadronic case I'm also not aware of a search for, e.g. gluino -> neutralino with the neutralino decaying to a 3-jet resonance through UDD. I was under the impression that most of the RPV parameter space is not constrained — or at least, not directly constrained in quoted limits, but maybe constrained by theorists' re-analysis of the data. I would be happy to be corrected, though….

13. **Bob Jones**
August 16, 2012

"Few places (except Princeton) will hire string theorists at the tenure-track level, with physics departments well aware of the overhype problem."

I don't know much about the job situation in physics, but this statement seems wrong to me. The work on amplitudes and twistor string theory is currently one of the hottest topics in theoretical physics. Over at the University of Chicago, a string theorist was recently given the position of "university professor":

http://news.uchicago.edu/article/2012/08/08/physics-initiative-launches-hiring-dam-thanh-son

14. **Peter Woit**
August 16, 2012

Bob Jones,

The tenure-track particle theory job market is well-covered by the Rumor Mill at

http://particle.physics.ucdavis.edu/rumor/doku.php

This year the only string theorist I see getting a job offer is Simone Gombi at Princeton.

The "amplitudes" business is not string theory and with rare exceptions the experts in it are not string theorists. People are making progress in computing QFT amplitudes, which are not string amplitudes. In some cases they have gotten some inspiration from techniques and calculations in string theory. Matt Strassler's post telling the story is specifically, as he explains, trying to highlight cases of influence from string theory (although you'll see in the comments, an expert takes him to task for this). The "twistor string" business is a small part of this story, although twistors themselves are a big part. But twistors really have nothing to do with strings: it was Roger Penrose who developed them as good variables for handling questions of 4d conformal invariance in GR.

While string theorists are not getting hired at the crucial tenure-track level, the generation that got tenure in the late eighties or so is very heavily loaded with string theorists, since you almost had to be doing that to get a job at the time. These are now old enough to be the grand old men and university professors of the field, with honors and prizes of all sorts coming their way as they get to the right age. This doesn't mean their non-string theory colleagues think that what they do has much future….

15. **APP**
August 16, 2012

anon: As I'm sure you know extracting the limits on RPV SUSY is a complicated business as there are so many parameters, one must be careful to impose all the flavor constraints on the RPV couplings/sparticle masses, and the final states (and whether they pass experimental cuts) are highly spectrum dependent. The limits I quoted are for CMSSM+RPV, so not 2 new things going on at once, in particular NOT also a "natural SUSY"/"effective SUSY" spectrum where all the superpartners not directly tied to electroweak symmetry breaking are made heavy. There are also certain assumptions on the RPV couplings that dominate motivated by flavor considerations, and most importantly NOT involving the UDD operator that you correctly mention as an exception (which is dealt with in the most challenging "natural SUSY" case by Raman Sundrum etal in http://arxiv.org/abs/arXiv:1206.2353 and which has significantly lower limits). In any case the limits can be extracted from the early figures of the Peter Graham etal paper I quoted together with ATLAS note http://cdsweb.cern.ch/record/1472934 (also useful to look at is https://cdsweb.cern.ch/record/1432202). Both Raman's and Peter's papers investigate tricky cases where RPV SUSY can be hiding, but as I'm sure you appreciate my point was that "vanilla" RPV doesn't help much in weakening the sparticle mass limits and more complicated/clever things must be going on to have a SUSY spectrum without large fine-tuning. Hope this helps.

16. **Bob Jones**
August 16, 2012

“The ‘amplitudes’ business is not string theory”

This is why the whole argument against string theory seems silly to me. In order to say that string theory has ruined theoretical physics, you take an extremely narrow view of the subject. You’re saying that the study of N=4 SYM amplitudes is not string theory even though it is equivalent to string theory by duality.

“with rare exceptions the experts in it are not string theorists”

I am not an expert myself, but I believe that many of the experts do conduct research in string theory (examples that come to mind are Arkani-Hamed, Bourjaily, Cachazo, Elvang, and Volovich). Many of these people are very young and have gotten tenure-track jobs within the last ten years.

“the generation that got tenure in the late eighties or so is very heavily loaded with string theorists… These are now old enough to be the grand old men and university professors”

Dam Thanh Son is actually a pretty young guy; it looks like he got his tenure-track job at Columbia in 1999.

“This doesn’t mean their non-string theory colleagues think that what they do has much future…”

Just look at the article I posted for evidence that this is not the case.

17. **APP**
August 16, 2012

Peter,

I agree that many tenured theorists privately share, in whole or in part, your views. The problem with going public, and I can only speak for myself, is that I don’t want to become a public figure. I went into physics for the fun/excitement and privilege of research itself and I want to devote as much time to that as possible–I know that may come across as selfish but I don’t even like going to conferences if I can avoid them (even if they are in exotic locales)! Even more importantly I’m very disturbed how uncivil the atmosphere and nature of the discussion typically is in the press/blogsphere (very often not driven by the correspondents themselves, but by troll comments or by certain unsympathetic reporters wanting a personalized, and hence “dramatic” conflict). As I’m sure you appreciate very well it is hard to have one’s nuanced position not misreported or distorted out of all recognition, and even more upsetting, in a way that makes it seem like a personal attack. I’m sure we share the greatest respect for the achievements, intellect, and seriousness of the leading string theorists, and I think it would be wrong, and in the end counter-productive to allow opinions to be misrepresenated as personal attacks on them of their research. (Personally I feel that research is such a crazy thing to which to devote one’s life, the only real criterion is whether it makes the person him/herself happy! The rest of us don’t have the right to judge what a person does with their time—especially as the old Hollywood dictum “Nobody knows anything” is almost certainly right.) The primary issue that bothers me is that many very bright young students go into strings with a fantasy of what it’s going to be like and what they will work on and achieve, when in fact they would have been far happier and more productive with physics that is closer to experiment. It’s sad to see young talent and enthusiasm misdirected…

Anyway, I’m sure you know my real-life identity, and I guess I’m OK for you to tell journalists that I’m one of the many (I could probably name a hundred other theorists) who have not too dissimilar views to you, but unfortunately my name is *off the record* . Sorry. Anyhow, Glashow, Georgi, & Wilczek are rather more substantial figures that would also support a sensible re-balancing of the field.

18. **piscator**
August 16, 2012

Dam Son is not a string theorist. Look at his publication history and what he has worked on. He is a field theorist who has, at times, used ideas coming from string theory.

19. **Peter Woit**
August 16, 2012

Bob Jones,

I should have looked more carefully and seen who this was about, I just assumed it was a Chicago string theorist you were talking about. Yes, Son is of a later generation, he’s also (as piscator points out) not a string theorist, not even an HEP theorist (he’s a nuclear physics theorist, which is somewhat different). I’m sure some of the connection of his work with AdS/CFT gets him support from string theorists, but I’d guess the desire to hire and reward him has a lot to do with universities wanting to hire outside HEP and outside string theory.

My argument with string theory has always been with it as an idea about unification, and I continually repeat that to make it clear. The relationship between string theory ideas and N=4 SYM is an extremely complex subject, my comments earlier were specifically about perturbative amplitudes and Witten’s twistor string. I don’t see any point to highly oversimplified claims about N=4 SYM and string theory being the same thing. That’s neither an accurate nor an interesting statement.

20. **huh?**
August 16, 2012

Just to keep things accurate here: you have repeatedly attacked attempts to connect string theory to nuclear physics or condensed matter physics as vastly over-hyped. Son, a virtuoso field theorist and many-body theorist, is certainly by far best known for his work on the holographic viscosity bound and related connections of holography to nuclear physics. His other most famous papers are on holographic models of QCD. Your last post is not accurately portraying your previous descriptions of string-related work, and the opinions of excellent physics departments (including evidently Chicago’s) are in genuine opposition to your own, regardless of what you choose to state on your blog. This has been reflected in hiring in the past, and will likely continue to be in the future. (I personally know of several top 10 physics departments on the lookout for promising young faculty who work in string theory or cognate areas with high overlap).

21. **Peter Woit**
August 16, 2012

APP,

I understand your feelings pretty well I think. One of my motivations for getting involved in this is that I’m in an unusual position in a math department, in a position such that I don’t need to worry about grants and about a lot of things most people have to worry about. I’m well aware of this, and it’s one reason I thought I should speak up publicly, since I could do so without worrying about a lot of the things other people would have to. I hope the blog and the book provide a resource for students to hear a different point of view than they might otherwise get, giving them something to think about. A lot of the hostility I find rather bizarre: I have plenty of friends who are string theorists and have never had trouble discussing the subject, about which we almost always agree on much more than we disagree. On the few occasions people have invited me to "debate" a string theorist, they have ended up sorely disappointed by the lack of fireworks.

22. **APP**
August 16, 2012

Bob Jones,

I know Dam Son somewhat and he would certainly not describe himself, or be described by others even in outside fields like condensed matter theory, as a string theorist. He's a very sharp and creative *quantum field theorist* who happens to sometimes use string techniques, especially AdS/CFT, to answer interesting questions about strongly-coupled theories in certain limits. His collaborators are a bit closer to paid-up string theorists. Overall I think that Peter is correct in saying that the vast majority of tenure-track jobs nowadays are going to Standard Model/beyond-the-SM/astro-particle theorists. Also many of the best people working in the amplitudes field would not at all fit inside traditional string theory, Nima included. Never trust the blurb written by university administrators, or publishers' PR departments to judge what people do!

23. **Peter Woit**
August 16, 2012

huh?

Yes, I've definitely written about how string theorists have engaged in outrageous hype about the connections between string theory and serious work in nuclear physics, QCD and heavy ion physics, work of people like Son. Son himself has never engaged in this sort of hype from what I have seen, which is one reason people want to hire him, and not string theorists. As for your claims about departments wanting to hire string theorists, you should keep in mind that it looks like you're at the one department in the world people think of as more fanatical about the subject than Princeton. We'll see what the rumor mill says in coming years, the number of string theorists getting jobs in the US pretty much has to go up, it can't get any lower.

I suspect that you're thinking of the condensed matter theory area of active research supposedly overlapping with string theory. Sure, as with Son, many departments would love to hire a good condensed matter theorist who has done AdS/CFT-related research that has had some success. Few however are going to be interested in people trained as string theorists who have moved on from overhype about its importance for particle theory and cosmology to now make overhyped claims about its importance in condensed matter theory.

If string theorists retool themselves, become experts in another field, and make real advances in that field using some tools they learned as string theorists, that's great. If they keep generating outrageous hype about string theory unification, the multiverse, etc, and try to justify themselves with dubious claims about how important their ideas are for other fields, they're not going to get far.

24. **APP**
August 16, 2012

Peter,

I also find a lot of (all?) the hostility bizarre. I really wish everyone took others' views more seriously and less personally. Theorists and their groupies are, more so than the average, an arrogant bunch of people with fragile egos!

25. **anon.**
August 16, 2012

Thanks, APP, I hadn't followed the details of applications of multilepton searches to bounding RPV parameter space. I guess we can summarize it as part of the bigger picture that the only allowed way to hide new strongly produced particles below a TeV is if they decay to multiple jets with very little missing energy and not many leptons or photons in the final state….

26. **APP**
August 16, 2012

anon,

yes that's my understanding too. glad to be of help—good luck in your research!

27. **Bob Jones**
August 16, 2012

"He is a field theorist who has, at times, used ideas coming from string theory."

"He's a very sharp and creative quantum field theorist who happens to sometimes use string techniques"

If you want to call him a field theorist and not a string theorist, that's a terminological point and not a substantive one. I understand that Son does not work on string theory for its own sake, but his most famous work is on applications of string theory. I see no problem with calling such a person a string theorist.

I think there's a pretty good mathematical analogy here. Let's say you have a mathematician who works in algebraic geometry and uses schemes to prove interesting results on algebraic varieties. Is this person a classical algebraic geometer or a modern algebraic geometer? I think most mathematicians would agree that this is a silly question.

"My argument with string theory has always been with it as an idea about unification, and I continually repeat that to make it clear."

I realize this, and I actually agree with a lot of the points you make. But honestly, I find it strange that this is being framed as a debate over the legitimacy of string theory. The term "string theory" is a broad, ill-defined thing with lots of different connotations, and you really ought to use different terminology. Instead of string theory, why don't we just say "multiverse" or "landscape" or "string phenomenology"?

"I don't see any point to highly oversimplified claims about N=4 SYM and string theory being the same thing."

I agree that I'm oversimplifying, but the point is that you can't really separate string theory from quantum field theory. The two frameworks are so inextricably related that it's hard to say what counts as string theory and what doesn't. Again, I think the mathematical analogy is a good one. Saying you can do quantum field theory without string theory is like saying you can do algebraic geometry without schemes. Sure you can do it, but string theory is a natural extension of quantum field theory in the same way that the notion of a scheme is a natural extension of the notion of a variety.

28. **Sam**
August 16, 2012

There is a huge difference as to whether one is a "consumer" of the results of a particular field versus a "producer" of results in the field. For instance, in some of my research I have used extant "off the shelf" results concerning elliptic operators to answer certain questions in geometry. I am a geometer, but in no way would I consider myself, based on this line of research, an analyst.

A more substantive discussion of whether one is a "string theorist" could be based on whether the person in question is actually producing new results in string theory. One can of course simultaneously be both a consumer and a producer of a particular theory. Has Son's work produced new techniques, insights, results in string theory proper (beyond the insight that its methods can be used in the manner to which he used them?) The research can be great regardless of an answer of "yes" or "no".

But finally, it is odd to argue as to what category another person's research belongs. I realize hobby horses need to be ridden here, but at the end of it all it is only the person in question whose opinion matters and it is likely that they don't really have an opinion- they are just busy prosecuting their research.

29. **Peter Woit**
August 16, 2012

Bob Jones,

Actually, the distinction between QFT and string theory is not just a terminological one. For one thing, we know what QFT is, whereas "string theory" is not a well-defined term (which is one problem with your string theory = schemes analogy).

The campaign to make Son a string theorist and all quantum field theories string theories is an ideological one, basically an attempt to define away the problem of the failure of the main string theory research program. Better to keep different words for different things, as well as keep track of what ideas work and which ones don't.

30. **Bernhard**
August 17, 2012

Seiberg's prophecy fulfilled

""Most string theorists are very arrogant," says Seiberg with a smile. "If there is something [beyond string theory], we will call it string theory."

(http://www.guardian.co.uk/science/2005/jan/20/science.research)

31. **piscator**
August 17, 2012

Bob Jones:

The way I see it is more as follows. As I work on string compactifications I use various results on Calabi-Yau manifolds in my work. However by no reasonable classification am I a mathematician or an algebraic geometer.

I agree that terminology is just terminology. However I think it begins to matter when encountering statements like 'string theory has dominated hiring', 'there are too far many string theorists around' etc. The way to support this statement is classifying everyone who was worked on anything related to string theory as a string theorist. You can do this, but it is misleading, as in most cases people work on string theory because they want to learn about some particular problem and string theory provides tools to address this problem.

>>The campaign to make Son a string theorist and all quantum field theories string >>theories is an ideological one, basically an attempt to define away the problem of the >>failure of the main string theory research program.

It is statements like this that make terminology important. If the `main string theory research program' means a program involving particle physics-susy-unification-strings I can think of a total of one, possibly two, faculty hires in total in the US in this area in the last ten years. There are not that many people in the US doing this and, really, there never have been.

32. **Bob Jones**
August 17, 2012

"we know what QFT is, whereas 'string theory' is not a well-defined term (which is one problem with your string theory = schemes analogy)."

You make a very interesting point here, but of course perturbative string theory is a fairly well-defined thing which can be formulated without reference to a dual QFT.

"I agree that terminology is just terminology. However I think it begins to matter when encountering statements like 'string theory has dominated hiring'"

I guess I'm just not sure what point was being made. If the point is that people are not getting hired to do string theory and particle physics, then I agree. If the point is that string theory is dying/becoming irrelevant, then I disagree for the reasons I gave.

33. **Peter Woit**
August 17, 2012

Bob Jones,

Yes perturbative string theory is a well-defined structure. But it's not a QFT, and QFTs are definitely not perturbative string theory.

34. **anon**
August 19, 2012

Hi Guys,

I was reading your comments about RpV in SUSY and I would like to mention a talk by Pavel Fileviez Perez about R-parity Violation at SUSY2012. It looks like there are well-motivated theories where one can expects RpV in a nice way. See:

http://www.phy.pku.edu.cn/~susy2012/resources/conf_day3/plenary/4_Pavel-Fileviez-Perez-SUSY2012.pdf

what do you think?

35. **anon**
August 19, 2012

A comment about G. Kane. I do not understand why him can be invited to major conference such as SUSY2012, his work is affecting the reputation of high energy physics, he should retire and allow a young person to get his place at UMich. Most of the people disagree with his papers, but since he is a political figure still the people invite him. There are no opportunities to young people and he is going around talking about these stupid results.

36. **Roger**
August 20, 2012

anon
A scientist should never be forced to step down because others disagree with his papers. The tenure system (such as it is these days) is properly designed to support academic freedom.

I'm certainly no supporter of Kane's ideas btw.

37. **Mitchell Porter**
August 21, 2012

anon, Gordon Kane isn't a one-man show. There might be a dozen or more people working on M-theory phenomenology in that general framework (G2-MSSM). It's actually one of the more bold and interesting things I can see going on in phenomenology, because it does try to make connections to fundamental theory, and I don't blame Kane for being excited that he can get a 125 GeV Higgs, 145 GeV dark matter particle, and so on, without finetuning.

LHC News

August 17, 2012
Categories: Experimental HEP News

The LHC is operating well, hitting record peak luminosities, with integrated luminosity for the year over 11 fb^{-1}. By the end of the year there may be 25 fb^{-1} per experiment or so. Current plan seems to be to update the results on the Higgs in December, much like last year, so there may not be much news until then.

This week the LHC Machine Advisory Committee was meeting, slides here. The current schedule has this proton run ending mid-December. After a heavy-ion run early next year, the machine will go into a long shutdown starting in March, with main goal to fix the magnet interconnections and commission the machine to run at nearly design energy, 6.5 TeV/beam. First beams at this energy will not be until April 2015, with maybe 20-25 fb^{-1} of integrated luminosity in the first year's run.

With no sign of SUSY so far, and little reason to believe it will show up in the rest of the 2012 data since nothing has shown up already, the rallying cry of SUSY enthusiasts is now "Wait Until 2015", or, maybe more like 2016, since early 2016 may be when analyses of a significant amount of 6.5 TeV data start to appear. I'd been wondering whether David Gross has been getting discouraged at the prospect of having to pay off on his SUSY bets. Someone asked him this recently, with the results here on YouTube. He says he's still willing to take 50/50 bets on SUSY, but "with the right conditions", which are 50 fb^{-1} of analyzed data/experiment (he says this will be "years from now I think", roughly 2017 maybe if all goes well), and he adds "then we need a judge, because it won't be so obvious I think". So, it looks like it's going to be quite a while before we get to see Gross pay up…

Comments

1. **RalphB**
   August 17, 2012

   2017? No problem, I'll still be here reading my favorite physics blog (assuming you're still writing it).
   A naive question. Is this bet about finding no other particles by 2017? Because if any particles are found, isn't there some conceivable susy framework that could be said to predict it?

2. **Peter Woit**
   August 17, 2012

   RalphB,

   Gross's bet is about SUSY, not just "there will be a new particle". The reference to "then we need a judge" refers to the fact that if a new particle is found, it may be very unclear whether it is a superpartner, because of the problem you refer to of lots of SUSY possibilities.

3. **John**
   August 17, 2012

   A rather amateur question: What happened to the theory of large extra dimensions and Randall-Sundrum model?
   At what point are these theories supposed to show up at the LHC?
   Have they found them yet?

4. **Christian**
   August 17, 2012

   No extra signs of Randall-Sundrum gravity has been found, nor has any other signs of extra dimensions. See here: http://arxiv.org/abs/1112.2194 for a quite recent one, but just google with keywords such as ATLAS, CMS, randall sundrum, 5 fb^-1 and so on.

5. **Shambler**
   August 17, 2012

   An interesting article from Matt Strassler here, about the ATLAS trigger, and how it relates to the search for new physics:
   http://profmattstrassler.com/articles-and-posts/lhcposts/triggering-advances-in-2012/data-parking-at-cms/

   Not news to anyone here I'd say, but interesting to consider just how narrow the parameters of the search are at the LHC, and what unpredicted (and unprogrammed-for) events may go unobserved.

6. **Shambler**
   August 17, 2012

   ^^ Make that, CMS trigger 🙂

7. **John**
   August 17, 2012

   Christian,
   thank you very much indeed. I also found these two: http://arxiv.org/abs/arXiv:1110.6452 and http://arxiv.org/abs/arXiv:1202.3827
   One wonders if their theories have proven to be wrong, what's all the publicity about?

8. **APP**
   August 17, 2012

   John,

   You ask a very pertinent question. The truth is that the absence of any sign of deviation from Standard Model (SM) predictions in pre-LHC precision tests of the SM (such as searches for new forms of transition among "flavors", ie, types, of quarks or leptons, or subtle changes in the relationship between the very accurately measured Z and W boson masses), disfavoured in a big way large extra dimensions, and also to a lesser but still significant extent Randall-Sundrum theories. That's part of the reason why the theory community had a preference for SUSY—not that this theory was any near perfect in this regard either! With 20/20 hidsight this lack of evidence in precision tests should have told us that all new beyond-the-Standard-Model physics was at a much higher (factor 10 to 100, maybe more) energy scale than the optimists hoped. Certainly it's seeming that way if you dispassionately consider the apparent lack of any truly new physics in the LHC data. Christian is right that this applies equally to large extra dimensions, Randall-Sundrum, and SUSY theories (and even more, technicolor, little higgs, composite higgs, higgsless models, and the plethora of other ideas theorists have cooked up over the years!).

So when should we give up with these theories? Well with SUSY it's getting pretty
clear that we are on the edge of what's allowable, at least in versions of SUSY that
don't have some new clever ingredient, or maybe two new clever ingredients…
With RS or LEDs it's harder to judge where the acceptable boundary is because of a feature of these theories—unlike SUSY models they are "strongly-coupled" theories, which means in simple terms that the usual approximation techniques we use to do calculations fail. This failure is especially severe in the electroweak symmetry breaking, ie, Higgs, sector, and we really have a very poor idea of how high an energy one could push up RS or LEDs theories and still solve the hierarchy problem for the Higgs. (I spoke to one of the world experts on these theories a few weeks ago and he had no good estimate apart from an informed "guess" based on dimensional analysis!–an estimate which would already kill off RS and LED theories given the LHC bounds mentioned by Christian).

All this is part of the reason why the big majority of particle theorists are either very confused about what to work on, or very defensive of their favorite theory. It looks like Nature has chosen the "…something we haven't been bright enough to think of…" option—that or semi-anthropic reasoning along the lines of Arkani-Hamed–Dimopoulos Split SUSY theories and their ilk (which personally I *do*
think are an option despite the hatred of the vast majority of the physics community for this approach).

Anyway, that was a long, not very numerically precise answer to your very good question.

9. **Former string theorist**
August 18, 2012

It turns out that I like this video of D. Gross. He did solid pre-string theory work (obviously), and is an important contributor and advocate of string theory, but his reputation does not depend on SUSY nor strings. So….. he made a bet, which he may likely lose, OK. He is not prone to hype and recently I admire his stance against the landscape's implications. Nevertheless, the outcome of the bet is keeping me on the edge of my seat!

10. **Garrett**
August 19, 2012

My $1K SUSY bet with Frank Wilczek gets called on July 8, 2015. Looks like that may be before the second run data gets analyzed, which isn't really fair, but that was the date Frank chose. It was timed to be during a FQXi conference, with Max Tegmark as our judge. I've already won a SUSY bet, but it was against two string theory grad students who subsequently left academia. I wonder if SUSY will get less popular as the strongest proponents pay out these bets.

11. **Phil**
August 19, 2012

Peter,

Let me ask you a question. Which of these two scenarios for the LHC do you prefer:

1. No new physics discovered at all for the entire lifetime of the LHC aside from a Standard Model Higgs.

2. New physics discovered, but the only new physics is SUSY.

Thanks!

12. **Primate**
August 19, 2012

What Phil is asking you, Peter, is: would you prefer to be a monkey, or a Bishop of the Church of England? My advice is to choose the monkey, as Huxley did.

13. **Peter Woit**
August 19, 2012

Phil,

I'd just like to know what the LHC can find out about the real world, would prefer that it find something that gives us a hint about how to do better than the SM. I don't have any preferences that the world be one way or another. Of the two alternatives you mention, one seems much more plausible to me than the other.

14. **MathPhys**
August 20, 2012

Phil's question ws not directed to me, but it's such a good question, so I wish to volunteer an answer. I want to see as much as possible discovered at LHC, or anything at all in addition to the Higgs, whatever that may be.

SUSY, no-SUSY, weirdo things, whatever works and of course, the weirder the better. I only wish to know that the money that taxpayers put into the LHC has not gone down the drain because we could have used it for some other very urgent purposes (I hear that unemployment in Spain and Portugal has hit some ridiculously high levels).

15. **Phil**
August 20, 2012

In other words, since you prefer the LHC to "find something" that can take us beyond the SM, you would prefer senario 2 over scenario 1. A SM Higgs, and nothing else, would tell us nothing about how to do better than the SM because of the plethora of SUSY models you mentioned — both low and higher energy ones — as well as all the other models people have cooked up, all of which could be compatible with a SM Higgs at 125 GeV. So, out of those two possibilities, you'd surely want the SUSY one. Is this correct?

The reason I'm asking is because I've been getting these strong vibes from you that you not only think SUSY is not plausible and won't be discovered, but you don't WANT SUSY to be discovered at the LHC. For example, I'm sure you'd agree that quantum gravity effects at LHC energies is not very plausible, but you'd definitely WANT the LHC to discover them. However, in the case of SUSY, you neither expect nor WANT SUSY to be discovered at the LHC. In fact, you want every single model that's been getting media attention to NOT be discovered at the LHC. If SUSY doesn't get discovered, you'll feel incredibly vindicated,you'll gloat on your blog and say "I told you so!", and all the SUSY proponents will look like fools. And that's precisely what you want. Also, you'd love for quantum gravity effects to show up at the LHC, as long as those effects are NOT string theory effects, for the same reasons as why you don't want SUSY to show up, even if it means light would be shed on how to go beyond the Standard Model. Is this correct?

So which one of those two assessments above for you are more accurate? I just want to understand you a bit better before I invest my time in reading your blog because there are many many physics blogs out there and I surely cannot read them all! 🙂 Cheers!

16. **Yatima**
August 20, 2012

“I just want to understand you a bit better before I invest my time in reading your blog.”

I think this world has some people that really feel entitled.

17. **Peter Woit**
August 20, 2012

Phil,

If you’re honestly interested in my motivation here, it’s pretty simple: I want to understand fundamental physics better, so am very carefully paying attention to what is going on at the LHC and what it is telling us. The motivation for paying a lot of attention to the negative SUSY results is not that I personally want to gloat (this isn’t unexpected, most theorists were expecting this) , but that it seems to me this is one of the discoveries of the LHC, and there’s an active campaign in many quarters to deny this. You’ll note that I’m paying about zero attention to “gloating” about or discussing negative LHC results about extra dimensions, black holes, quantum gravity, strings. Virtually no one ever seriously thought the LHC would see such things, and it’s not at all surprising or interesting that the results of such searches are negative.

18. **tt**
August 20, 2012

why does it matter what anyone WANTS ? it’s nature, it doesn’t care what you WANT.

19. **dark matter**
August 20, 2012

what are the prospects of directly creating and observing dark matter at these energies and luminosities? not necessarily SUSY. Has there been any reports of dark matter, and if not, what are the ramifications of a null result for both SUSY and CDM cosmology?

20. **Peter Woit**
August 20, 2012

dark matter,

The LHC experiments are certainly looking for new particles that would explain dark matter, SUSY or not, but haven’t found anything. The problem with dark matter is that you so far have really only observed it via its gravitational interactions, but have no idea what its other interactions with known particles are (although you know a lot about what they aren’t: can’t be strong, EM, etc…)

If you see evidence for a new particle at the LHC consistent with a dark matter candidate, that would be a fantastic breakthrough. Unfortunately, if you don’t see this, that doesn’t tell you that much, just gives more constraints on what possible interactions a dark matter particle might have.

21. **MathPhys**
August 20, 2012

Can someone kindly recommend something to read for the not-exactly-layman but knows nothing about astrophysics on the status of dark matter. Something straightforward.

22. **David Nataf**
August 20, 2012

MathPhys,

There are way too many seemingly independent lines of evidence pointing to dark matter to have a simple explanation for a non-expert: galaxy rotation curves, velocity dispersions of galaxies in clusters, lensing behind the bullet cluster, the velocity anisotropy of stars in the solar neighbourhood, and the growth of structure in the universe.

For a non-expert, I’d recommend focusing on just one of these, the Galactic rotation curves to get started, and the Wikipedia page seems good:
http://en.wikipedia.org/wiki/Galaxy_rotation_curves

You might also want to check out the bullet cluster paper:
http://arxiv.org/abs/astro-ph/0608407

23. **Friend**
August 20, 2012

This should be an easy question for some of you. I don’t know a lot about particle detection techniques, so I have to wonder… As I understand it the LHC takes a lot of data, and one has to employ sophisticated algorithms to wade through all this data to see if there is a signal corresponding to a particle with certain properties. OK, is it possible that we might not be aware of a new particle if we’re not looking for it? If we don’t program the right algorithm, could we miss the detection of some kinds of particles at the LHC? Or would any new particle always slap us in the face and there would be no way we could miss it? Thanks.

24. **Peter Woit**
August 20, 2012

Friend,

The answer completely depends on the properties of the conjectured particle, which will determine how often if it produced, in conjunction with what other particles, and what it decays into. Some sets of properties will make a particle easy to see, others extremely hard to impossible.

The LHC is colliding strongly interacting particles, and if your conjectured particle has strong interactions, it will get produced relatively copiously. Right now the LHC null results for SUSY are mainly for the superpartners of strongly interacting particles, since these must be strongly interacting. Particles with only weak interactions (such as conjectured dark matter particles) are produced relatively rarely, so much harder to see. Still, even if something is produced rarely, it may be observable if its production and decay involve some very unusual, distinctive signal. Of course,then the problem is that you need to be looking for that unusual distinctive signal. There's a legitimate worry that the LHC is producing some new particle with properties that make it observable in principle, but no one has been looking for the right signal.

25. **Friend**
August 20, 2012

It's beginning to look as though they have been looking in the wrong place… no sign of SUSY or Strings. And it sounds like anyone with a new theory will have to convince a lot of Ph.Ds before they will even look for those new signals. Or can the data be searched in restrospect for signals predicted by a new theory?

26. **Peter Woit**
August 20, 2012

Friend,

These experiments have not just been looking for SUSY or strings, but also much more generically for the kinds of signals you expect from production of an unknown particle, or from some other deviation from the Standard Model. If they're missing something, it's going to be something with quite unexpected properties.

The data from the experiment is stored and will be available to try out different analyses for many years to come. As time goes by and the obvious searches and searches for SUSY get done, I suspect any plausible suggestion for what else to look for in the data will start to be able to get a hearing. The real worry is the triggering: only a small fraction of the data is stored, that which satisfies some conditions that indicate it's possibly interesting. The nightmare is some un-thought of new phenomenon which shows up only in events that the trigger is now dropping. But people do worry about how to avoid this nightmare, and the LHC is going to be around gathering data for many, many years, with many opportunities to try different triggers if someone comes up with a good reason.

27. **MathPhys**
August 21, 2012

@ David Nataf,

That's great. Thank you.

28. **Peter Woit**
August 21, 2012

A reminder,

I'm afraid I don't have the time or interest to run a general discussion board about physics, so please don't submit comments unrelated to the posting. Physics Stack Exchange is one of several examples of a better place for such discussions.

29. **SteveB**
August 22, 2012

@Friend,

Just want to point out that Matt Strassler has recently written a long article about which events are recorded in LHC (what triggers an event as worth keeping) and why and how they are now storing some different data that will only be analyzed later during the upgrade shutdown as compute resources become available. See:

http://profmattstrassler.com/2012/08/13/the-trigger-and-the-parking-lot/

This Week's Hype

August 21, 2012
Categories: This Week's Hype

> The Higgs suggests that there could be more dimensions of space-time than we previously thought.

From a New Yorker piece this week (subscription required) about Joe Incandela of CMS and the Higgs discovery.

Even the famed New Yorker fact-checkers are no match for extra-dimensional hype. Will someone tell them they've been had?

Comments

1. **Hamish**
   August 21, 2012

   I look forward to reading that when my copy of magazine arrives this week. The New Yorker seems to have embarked on a series of in-depth biographical pieces with the likes of Bruce Springsteen and Paul Ryan being covered lately. Good to see a physicist in the mix.

2. **Peter Woit**
   August 21, 2012

   Hamish,

   Unfortunately it's not an in-depth piece, but one of the short "Talk of the Town" pieces. It's well-done in terms of giving some atmosphere for a day in the life of Joe Incandela, but the theme is more bemusement at the incomprehensible things those physicists are up to at CERN than attempt to explain anything.

3. **former mathematician**
   August 21, 2012

   Reading the sentence in the context of the article, I believe "dimensions" was meant as "complications," rather than anything geometric.

4. **Peter Woit**
   August 21, 2012

   former mathematician,

   "complications of space-time" is no better than "dimensions of space-time". The Higgs appears to be a spin-zero, scalar particle, with nothing even slightly unusual about its relation to space-time.

5. **worse**
   August 21, 2012

   The New Yorker has done worse. In 1962 Jeremy Bernstein wrote an article in the New Yorker about Lee and Yang
   http://www.newyorker.com/archive/1962/05/12/1962_05_12_049_TNY_CARDS_000270950
   The article implied that the parity violation idea was more due to Lee than Yang. Yang was really upset. The article was the direct cause of the end of the collaboration between Lee and Yang. It had been a tremendously fruitful collaboration. Bernsteon's article did far more damage to to physics than any confusion to readers about a minor statement about the Higgs and space-time dimensions.

6. **Peter Woit**
   August 21, 2012

   worse,

   I don't mean at all to suggest this does any damage to physics. As for Lee and Yang, my impression is that they were headed for some sort of conflict, no matter what Bernstein wrote about them. But that's really far off-topic…

7. **Samuel Kerckhoff**
   August 21, 2012

   Hello, I was wondering if some one could enlighten me i.e. Peter Woit 🙂 or anyone with info.
   I've read the LHC has not detected any new physical phenomena that is not already predicted by the standard model, while operating at its current energy level. Is there a possibility that new things, not predicted by physicist, will be detected when the LHC is operating at the maximum energy level that it was designed for?

   Thanks

8. **Samuel Kerckhoff**
   August 21, 2012

   Or at least the LHC detecting new things that would show physicist what steps to take beyond the standard model.

9. **former mathematician**
   August 21, 2012

   How about "complications in the universe"? Which would fit the context equally well.

10. **Peter Woit**
    August 21, 2012

    Samuel,

Sure, when the LHC goes from 4 GeV to 6.5 GeV/beam in a couple years, that will make accessible any new phenomena in the energy range just above the current LHC limits. Maybe there's something unexpected there. However, there's no good argument for why something new should show up in this new energy range, but not in the range now being explored at 4 GeV/beam

11. **Peter Woit**
    August 21, 2012

    former mathematician,

    I really don't think the problem here is the writer's word choice. There has now been a long tradition of physicists hyping the idea that "extra dimensions may be seen at the LHC", and it's not really surprising that a writer exposed to this would get the mistaken impression that the Higgs discovery has something to do with this.

12. **Don Murphy**
    August 21, 2012

    Peter Woit:However, there's no good argument for why something new should show up in this new energy range, but not in the range now being explored at 4 GeV/beam

    I'm not sure I understand this statement. Does it mean the LHC is going to operate at higher energy only to confirm what is already known at the lower energies; or do you really mean to say there is no good argument for why something new should show up and therefore that is not the reason for the LHC going to higher energies? Can you clarify. Thanks

    Don

13. **Peter Woit**
    August 21, 2012

    Don Murphy,

    By operating at higher energy, the LHC will explore a new, previously inaccessible region, and if there's a new particle accessible in this mass region, it should find it and will study it.

    The point I was trying to make though is that this is unlike the case of the Higgs, where we had excellent arguments (from the nature of the Standard Model) that a new particle HAD to be in the region accessible to the 3.5 TeV/beam LHC., going to the 6.5 TeV/beam LHC, the SM says there won't be anything new there. We're all hoping though that the SM isn't all there is, that there really is something unexpected going on, and that it will show up in careful study of this new energy region. Only way to find out is to do the experiments…

14. **David Nataf**
    August 21, 2012

    Peter,

    Will there be new, independent tests of the SM at these higher energies?

    Will other branching components of the Higgs manifest themselves at these energies, or will the focus still be on photons, electron/positrons, bottom, and tauon decays from the Higgs?

15. **Peter Woit**
    August 21, 2012

    David,

    Good question. I don't recall seeing a good discussion of this anywhere, but I'm sure it's available somewhere. Maybe an expert can point to this.

    At higher energies the Higgs cross-sections increase, but you also get increases in background, and have pile-up problems to deal with, so I don't know what happens to the sensitivity in the various channels they are looking at. At higher energy you may also get some new channels that open up.

16. **Mike**
    August 21, 2012

    I know we don't have the article to read, but do you think they are justifying "extra dimensions" based on the excess in the diphoton channel or branching ratios that are different than theorized by SM?

17. **Peter Woit**
    August 21, 2012

    Mike,

    I have no idea where the writer picked up this particular piece of hype. Maybe there are people out there claiming the diphoton excess as suggesting extra dimensions, but if so until now their hype hadn't made it into mainstream media.

18. **John**
    August 21, 2012

    What's wrong with writing a paper (or giving an informal talk) suggesting that the diphoton excess and other deviations can be due to extra dimensions or other exotic things? That's not hype. What is your definition of 'hype' ? If it hadn't made it into the mainstream media yet, is it still hype?

19. **John**
    August 21, 2012

    Also, Peter, in response to:

    "We're all hoping though that the SM isn't all there is, that there really is something unexpected going on, and that it will show up in careful study of this new energy region."

Is any SUSY model one of the things you are personally hoping for? I know you'd bet against SUSY, but is there one version of the theory you are praying shows up at the LHC? (Not that you pray! 🙂 I mean figuratively.)

20. **Peter Woit**
August 21, 2012

John,

If "The Higgs suggests that there could be more dimensions of space-time than we previously thought." isn't hype, I don't know what is. Again, I have no idea who fed this to the writer or why, no reason to believe it was someone promoting their diphoton excess model.

I think there's something fundamentally wrong with all SUSY extensions of the SM on the market: they're complicated and ugly (because of the necessity of SUSY breaking, with no known good way to do it), you have to go to great lengths to explain why no evidence at all for them has shown up, and they don't really explain anything about the SM that needs explaining.

That said, the abstract mathematics used in SUSY models is interesting, and maybe there's some such thing as a SUSY QFT that really does work. The mathematically interesting SUSY models often involve a "twisted" version of SUSY. Maybe there's some way of using that to get an interesting model, but that's purely speculative, I personally know of no way of doing this.

I wrote a whole book chapter about SUSY in NEW, the book, and don't want to rewrite that here. I haven't looked at it recently, but as far as I know there's nothing much I'd change if I were rewriting it today (other than reporting that the experimental evidence against SUSY is much stronger…).

21. **Emanuel Derman**
August 22, 2012

Maybe the experimentalist had thought that there were only two dimensions to space time, and now, having discovered the Higgs, he realizes that there are four.

22. **P**
August 22, 2012

@ Samuel, John:
Peter is expressing his opinion when he says that SUSY doesn't address anything in the standard model that needs explaining. As I'm sure he would admit, many top notch, highly cited theorists disagree with him here – they would say that the hierarchy problem is in fact a problem, which has been the source of many arguments on this blog, Peter essentially the only dissenter. (No disrespect Peter, just stating facts). Those experts would claim that if any physics solves the hierarchy (perhaps SUSY) it should appear near LHC energies, perhaps at the next energy level in a few years.

@ Peter:
This is hype of a different sort, I think! Many good theorists think strings are a good idea for various reasons, perhaps unrelated to unification, and overhype it. I cannot think of a single reason to think that anything about the Higgs implies extra dimensions. Can you? This is a serious question . . . hype (SUSY, e.g.) is usually attached to some physical idea, which people may disagree with, but I don't even know what the physical idea is here that links these two (a la the idea potentially linking SUSY and the hierarchy).

23. **Igor Khavkine**
August 22, 2012

@P:
Just a remark. It's hard to put an upper bound on the number of dissenters withot a representative sample. And here one should keep in mind that there are quite a few physicists informed enough about QFT to have an opinion, but do not work on beyond SM physics. It might also shock you to learn that there are physicists who think that the cosmological constant problem is not a problem either.

24. **John**
August 22, 2012

Peter,

Would you be disappointed if SUSY showed up at the LHC?

Also, in response to, "If 'The Higgs suggests that there could be more dimensions of space-time than we previously thought.' isn't hype, I don't know what is."

That wasn't what I meant. I meant suppose there's a theorist who takes a possibly observed small deviation from a Standard Model Higgs and writes a paper about how an extra dimensional model can produce such an effect. Nothing wrong with that, is there?

25. **John**
August 22, 2012

Also, in response to,

"That said, the abstract mathematics used in SUSY models is interesting, and maybe there's some such thing as a SUSY QFT that really does work. The mathematically interesting SUSY models often involve a "twisted" version of SUSY."

So, are you hoping this shows up at the LHC?

26. **Peter Woit**
August 22, 2012

John,

I'm not sure if you're the same person who submitted about a dozen comments to the last posting demanding to know what I wanted the LHC to find about SUSY. See that posting for the answer to this.

Yes theorists can write any papers they want, they shouldn't hype their results to New Yorker reporters.

P.

It's just not true that I'm the only dissenter about the hierarchy problem, as anyone who looks at the previous discussion here can tell. In any case, the LHC has pretty much shown experimentally that the hierarchy problem argument for SUSY to appear at the electroweak scale is wrong.

27. **chris**
August 22, 2012

David Nataf,

"Will there be new, independent tests of the SM at these higher energies?"

yes, and the prime example is the detailed study of the Higgs decay amplitudes. These are all very precisely predicted by the SM and their measurement will constitute a very nontrivial check of the SM in a previously inaccessible sector.

28. **Peter Woit**
August 22, 2012

chris,

Yes, but do you know of a source explaining which channels will be accessible at 6.5 TeV/beam, and what the expected accuracies are for the signal sizes in these channels (for plausible luminosities)?

29. **P**
August 22, 2012

Peter,

I'd be impressed if you could find a well-known highly cited professor at a top university who would claim that there is no fine tuning problem related to the Higgs mass. Some (Nima, e.g.) these days are willing to abandon naturalness and say that maybe nature is fine-tuned, but I haven't heard anyone say that there isn't a fine-tuning, i.e. that the hierarchy doesn't exist. SUSY is very constrained, indeed (I agree!), but it is far too early to say that it's fully ruled out at low energies or that nature doesn't provide another solution visible at LHC which solves the hierarchy. See Matt Strassler on related issues, with a more informed opinion than mine.

On the part of my comment I addressed to you – I suppose the idea they're using to link extra dimensions and this Higgs mass is Randall-Sundrum? Is there another obvious candidate?

30. **John**
August 22, 2012

Peter,

I checked the last posting and if you're referring to 'Phil', he and I are not the same person. I read your response to him, but I'm not sure how your response answers the question I asked. I wanted to know if you are hoping for that particular SUSY model involving twisters you mentioned to show up at the LHC. I know you're betting against all forms of SUSY, but I was wondering if that version of SUSY is something you are personally wishing for. Is this true?

Also, I don't think SUSY as a solution to the hierarchy problem has been completely ruled out, but I could be wrong.

"Yes theorists can write any papers they want, they shouldn't hype their results to New Yorker reporters."

Well, perhaps the reporters went to THEM first. Perhaps the theorist(s) explained their work without any hype, and the reporter turned it into hype in order to sell more papers.

31. **Peter Woit**
August 22, 2012

P,

You're just starting up precisely the same argument we had here in great detail before about fine-tuning, actually discussing the science. This is just tedious and a waste of time. Now you want to just make it an argument from authority which is both tedious and dumb. If anything is clear about the story of particle theory in the last 30 years, it's that the endlessly repeated ideology about BSM physics isn't working so has some flaw in it. I see no point in arguing about how many prominent people share this ideology, to what degree.

P. and John.,

As I keep repeating, I have no idea where the New Yorker writer got that particular piece of hype from, would be curious to know, as well as curious to know how it got past their fact-checkers.

John,

The "twist" of SUSY I'm talking about has nothing to do with "twistors", and, as I said, I don't know of a viable model. That was just a speculative comment about the logical possibility of a more successful very different kind of model that still in some sense has "supersymmetry". As I wrote to "Phil", what I want from the LHC is to learn something about how the world works. I don't have a model that I "want" the LHC to find, and as for known SUSY models, there just is no good argument for them, lots of reasons why you expect not to see any evidence for them.

32. **John**
August 22, 2012

Peter,

Oops, I confused twisted with Penrose's twistors.

You seem to have a strong, let us say, aesthetic doubt against SUSY. True, SUSY can be quite "ugly" generically, with lots and lots of free parameters, and no good or well-motivated way of breaking it. I'm sure everybody wants something incredibly beautiful to emerge at the LHC. Given that perspective on aesthetics, are you hoping more for a nightmare scenario at the LHC if it meant that perhaps a much more beautiful and exciting theory were just (or at a much higher energies) above the LHC's reach, or are you more inclined (than the previous possibility) to hope for ANYTHING, even something ugly like SUSY, if it meant that particle theory has a greater chance of progressing based on actual concrete discoveries and nature shedding a brighter light on what particle theorists may work on next?

33. **Peter Woit**
August 22, 2012

"John"
You sure do sound a lot like "Phil" and all the other pseudonymic comments I deleted from the same person that kept asking me tendentiously what I "wanted" from the LHC. For the last time, all I (and I think this is true of most physicists) want from the LHC is for it to tell us something new about nature. This may just be that the SM holds up to and above a TeV. Something non-SM, anything non-SM, would be great since this would be handing us a wonderful new puzzle of the highest order. I'm not going to argue with whoever/whatever is responsible for the design of the physical world by "wanting" them/it/whatever to do it a certain way or I won't be happy.

I point out the ugliness of viable SUSY models purely because, since there is no experimental evidence of any kind for them, the only possible argument in their favor is one from aesthetics.

34. **Bob Jones**
August 22, 2012

"Oops, I confused twisted with Penrose's twistors."

John, why don't you go read about some of the serious applications of SUSY instead of continuing with this silly argument? Peter has given you a fascinating example of why SUSY is interesting and valuable even if it doesn't show up at the LHC.

35. **John**
August 23, 2012

Peter,

Thanks for your response. And no, I'm NOT Phil.

36. **Joey**
September 5, 2012

I find your comment about this being hype amusing. First of all, if there is a Higgs and it is a scalar, then it means it's mass is stable either by means of some new physics or by a really incredible fine-tuning of the parameters of the Standard Model. Sure, it's possible but nature, in our experience so far, has mostly found balance via equilibrium so it seems very reasonable to assume there is some new physics we have not seen that creates the balance. It is a rational assumption, not a guarantee. So following this line, there are a couple of possibilities. It is either a spectrum of new particles (a new dimension in the sense that we have a new symmetry and a new array of fundamental states) or the Planck scale is not what we think it is, and perhaps String Theory with its additional dimensions can be a motivator for this idea. Yes, these are big ideas, but I would not call them hype.
Frankly, I think you have to be a really arrogant twit to just call them hype unless you have really something useful to bring to the discussion.
Anyway, such glib comments do not inspire me to read this blog. Hopefully you can improve in future.

37. **Peter Woit**
September 5, 2012

Joey,

You're behind the times, submitting comments about last week's hype, when this is old news, with fresh hype for this week already available.

38. **fuzzy**
September 8, 2012

P. & Peter,

in my view the "naturalness problem" is not a problem concerning the physics but perhaps the philosophy (why things are as they are and not as they are not?) or the sociology (why, even in science, some priviledged thinkers have a hierarchical position rather than ideas?).

I do not know any serious discussion of it in the literature; I believe that ether theory has been not worshipped as this "problem" or "principle" has been in the recent years.

The hype that you pointed out proves, once again, that we need critical discussions of what high energy physics became.

Bill Thurston, 1946-2012

August 22, 2012
Categories: Obituaries

Bill Thurston passed away yesterday, at the age of 65, after a battle with melanoma. Thurston was for many years the dominant figure in the study of 3 dimensional topology and geometry, winning a Fields medal for this work in 1982. His “Geometrization Conjecture” classifying the topology of 3 manifolds was finally proved by Perelman as part of his work on the Poincaré Conjecture.

For an exposition of some of his work, see *The Geometry and Topology of Three-manifolds*, which exists as a set of unpublished notes here, and a book covering the first few chapters of the notes here. Thurston was sometimes criticized for not writing up full proofs of his results, making it difficult for others entering the field (and sometimes students were advised not to enter the field since Thurston was so good the danger was he would just solve all open problems). He wrote a truly wonderful essay On Proof and Progress in Mathematics, responding to this and laying out part of his vision of how to do mathematics.

My first encounter with Thurston was in the early eighties, when I was a physics graduate student at Princeton. I was working on the problem of defining the topological charge of a lattice gauge field, and it became clear that one approach to do this would require computing the volumes of “spherical tetrahedra”, the 3d analog of the problem of computing the areas of spherical triangles. I’d had some experience trying to talk to mathematicians about the problem I was working on, with the usual result a baffling response about principal bundles, sections, characteristic classes, and all sorts of what seemed to be abstract nonsense (which later on of course I learned was the right way to think about the problem…). So, I was pretty convinced that mathematicians were uniformly experts in a lot of abstract, high-powered technology, surely no longer conversant with the kind of more concrete formulas of the mathematics of earlier centuries.

This was before the days of the internet, so the answer to my problem couldn’t be found via Google, and a bit of library research got me nowhere. So, I stopped by to see a friend who was a math grad student and asked him my volume question. He said that while he didn’t know, he knew someone who could surely help me, and took me over to the math lounge, where Thurston could often be found. After I asked my question, Thurston immediately knew the answer, explained it to me on the blackboard, and gave me the proper reference of where to read more (you break them up in a certain way and then get an answer in terms of things called Schläfli functions, see here). I realized that my views of how much the best research mathematicians knew about concrete calculations and lore from previous centuries had been rather naive.

Thurston’s death at such a relatively young age is a loss for us all. My condolences to his family, including his son Dylan, a very talented topologist in his own right, who has been my colleague here for the last several years.

Update: Terry Tao has more about Thurston and his work here.

Update: More here, here and here. Also worth the time is seeing what he had to say on MathOverflow.

Update: Jordan Ellenberg has something here.

Update: The New York Times has an obituary here.

Update: There’s a Cornell site here, Scientific American has a piece by Evelyn Lamb here, John Horgan here.

Update: Jonah Sinick has put together a memorial slideshow here.

Comments

1. **Anonymous**
   August 22, 2012

   Very sad news and a great loss to science. My condolences to all bereaved.

   However, Peter, did you manage to define the topological charge of the gauge field for the general gauge group case ? Also, if you solved it in the lattice case, did your definition carry over to the continuum limit ?

   The question of defining topological charge carried by gauge fields seems very important to me, because gauge field theory *is* the theory of interaction of these charges, and empirically the bound states of gauge charges, in the limit, yields gravitating bodies.

2. **Peter Woit**
   August 22, 2012

   Anonymous,

   I didn’t use the volume formulas that Thurston told me about, that’s too hard. What I did do was just for SU(2), and was easy to compute, made sense in the continuum limit. It only worked for SU(2), where things come down to computing degrees of maps. Lots of other people have worked on this over the years, a long story, maybe for another time, since the only relation to Thurston I know of is our one conversation in the common room.

3. **Jeff M**
   August 22, 2012

   OK, as a mathematician I fell somehow obliged to tell my Thurston story. I never actually met him, though I know several of his students pretty well. When I was an undergrad at Hampshire in the early 80’s Thurston came to give a talk at Smith about his work with 3-manifolds. My advisor, Ken Hoffman, took me to see him. Didn’t really understand all that much, but the way he explained things really made the basic ideas understandable, even to an undergraduate who was at the time a physics major and hadn’t even taken modern algebra yet. In grad school I spent hours (days, weeks…) with a xerox of his lecture notes, beautiful doesn’t really even begin to describe how wonderful they are. I spent some time a few years ago with Steve Kerchoff at a conference, he was one of Thurston’s first grad students, and was the advisor of a friend of mine. Kerchoff was at the lectures which turned into the notes, he described how amazing it was to see all this stuff when it was brand new, and totally out of the mainstream.

4. **Scott Carter**
   August 22, 2012

   In the era, 1982-1985 both Bill Thurston and Mike Freedman would often visit the University of Texas at Austin. The visits were so frequent, in fact, that it was difficult to do anything other than attend the seminars, dinners, and after dinner parties. Once, at Cameron Gordon’s house, some of started to play with a toy motor cycle (wind up, hand held thing) that Cameron's son Andrew had. Bill reved the thing up (rrr-rrr-rrr) I think he even added his own sound effects, and he got the thing to fly across the diagonal of the den. This feat impressed me — not as much as his mathematics — but I always think of him as being playful.

5. **Wayne Collier**
   August 23, 2012

   I feel about Bill Thurston's passing much as I feel about the death of great spiritual leaders. Profane though he was like the rest of us, his mind was sacred. Thurston conceived, manipulated and illustrated the texture of God.

6. **Gil Kalai**
   August 23, 2012

   I did not have the privilege of knowing Bill Thurston personally, but his immense influence, the beauty of his ideas, and his special personality give me now a feeling of a great personal loss. Of course, it is a great loss for mathematics.

7. **Bernard Chazelle**
   August 24, 2012

   Bill and I were colleagues for many years at Princeton
   and at the Geometry Center. The joke there was, Why is Bill
   so involved in producing software to make high-dimensional geometry visible
   when the last person in the world who needs any such tool is Bill?
   His geometric intuition was beyond belief (I have fun anecdotes about that).

   But I am too sad today to talk about anything that's not, well, sad. His 60th b'day
   conference at Princeton would be one such occasion. Marvelous talks and
   a sweet personal note when Bill told me he followed my political
   activism on the web and added "We have no choice but be active."
   Bill was the guy who turned down an endowed chair at Princeton
   because the name attached to it was a rightwing militarist.
   What's so sad about all that? Well, the last two people I talked
   to at the conference were Bill and his former student Oded Schramm,
   both of them gone way too soon. Leaves a knot in one's stomach.
   Bill approached math with a touch of magic. A very sad day.

8. **Richard**
   August 24, 2012

   Danny Calegari wites.

9. **Timothy Riley**
   August 24, 2012

   Bill Thurston is ever so missed here at Cornell. There is a page up in tribute. It includes a place for posting remembrances and a selection of quotes assembled by Dylan.

10. **Cam Scott**
    August 26, 2012

    Brought to my attention via this blog, I always have in mind Thurston's laudation on the Award of the Millennium Prize to Grigoriy Perelman For Resolution of the Poincaré Conjecture and hope that it is worth reviewing at this apposite time. Here it is in part: "Perelman's aversion to public spectacle and to riches is mystifying to many. I have not talked to him about it and I can certainly not speak for him, but I want to say I have complete empathy and admiration for his inner strength and clarity, to be able to know and hold true to himself. Our true needs are deeper – -yet in our modern society most of us reflexively and relentlessly pursue wealth, consumer goods and admiration. We have learned from Perelman's mathematics. Perhaps we should also pause to reflect on ourselves and learn from Perelman's attitude toward life."

    We should learn too from Thurston's own exemplary attitude toward life.

Linde on Inflation and the Multiverse

August 25, 2012
Categories: Multiverse Mania

Andrei Linde is one of Yuri Milner's $3 million dollar men, best known for his "chaotic inflation" version of inflationary theory, as well as being one of the main proponents of anthropic multiverse mania. There's a long piece based on a conversation with him up now at the Edge web-site.

Much of the piece is just a retread of the usual heavily-promoted ideology of the past 30 years of fundamental physics research: we must have SUSY, so must have supergravity, so must have string theory, so must have the landscape, so must have a multiverse where we can't predict anything about anything, thus finally achieving success. Linde claims he pretty much had this picture 30 years ago back in 1982, with the string theory component in 1986, with others coming around to his point of view in the last 10 years, partly because of the KKLT work he was co-author of in 2003.

Besides the tired Stanford pseudo-scientific ideology, there's also a wonderful history of the subject of inflation, from a Moscow point of view, which is rather different than the Western, Alan Guth-oriented, point of view from which the story is often told. Linde's description of Hawking's visit to Moscow is not to be missed:

> The next morning after I gave a talk at this conference, I found myself at the talk… oh, my God, this is going to be a funny story… I found myself at the talk by Stephen Hawking at Sternberg Institute of Astronomy in Moscow University. I came there by chance because I have heard from somebody that Hawking was giving a talk there. And they asked me to translate. I was surprised. Okay, I will do it. Usually at that time Stephen would give his talk well prepared, which means his student would deliver the talk, and Stephen from time to time would say something, and then the student would stop, and change his presentation and do something else. So Stephen Hawking would correct and guide the student. But in this case they were completely unprepared; the talk was about inflation. The talk was about the impossibility to improve Alan Guth's inflationary theory.
>
> So they were unprepared, they just finished their own paper on it. As a result, Stephen would say one word, his student would say one word, and then they waited until Stephen would say another word, and I would translate this word. And all of these people in the auditorium, the best scientists in Russia, were waiting, and asking what is going on, what it is all about? So I decided let's just do it, because I knew what it's all about. So Stephen would say one word, the student would say one word, and then after that I would talk for five minutes, explaining what they were trying to say.
>
> For about a half an hour we were talking this way and explained to everyone why it was impossible to improve Alan Guth's inflationary model, what are the problems with it. And then Stephen said something, and his students said: "Andrei Linde recently proposed a way to overcome this difficulty." I didn't expect it, and I happily translated it into Russian. And then Stephen said: "But this suggestion is wrong." And I translated it… For half an hour I was translating what Stephen said, explaining in great detail why what I'm doing is totally wrong. And it was all happening in front of the best physicists in Moscow, and my future in physics depended on them. I've never been in a more embarrassing situation in my life.
>
> Then the talk was over, and I said: "I translated, but I disagree," and I explained why. And then I told Stephen: "Would you like me to explain it to you in greater detail?" and he said, "Yeah." And then he rode out from this place, and we found some room, and for about two or three hours all the people in Sternberg Institute were in panic because the famous British scientist just disappeared, nobody knew where to.
>
> During that time, I was near the blackboard, explaining what was going on there. From time to time, Stephen would say something, and his student would translate: "But you did not say that before." Then I would continue, and Stephen would again say something, and his student would say again the same words: "But you did not say that before." And after we finished, I jumped into his car and they brought me to their hotel. We continued the discussion, which ended by him showing me photographs of his family, and we became friends. He later invited me to a conference in Cambridge, in England, which was specifically dedicated to inflationary theory. So that's how it all started. It was pretty dramatic.

Addressing the question of "what evidence is there for any of this?", here's what Linde has to say:

> Usually I answer in the following way: If we do not have this picture, then we cannot explain many strange coincidences, which occur around us. Like why vacuum energy is so immensely small, incredibly small. Well, that is because we have many different vacua, and in those vacua where vacuum energy is too large, galaxies cannot form. In those vacua, where energy density is negative, the universe rapidly collapses, and in our vacuum the energy density is just right, and that is why we live here. That's the anthropic principle. But you cannot use anthropic principle if you do not have many possibilities to choose from. That's why multiverse is so desirable, and that's what I consider experimental evidence in favor of multiverse.

So, the main experimental evidence for the multiverse is that an anthropic argument works. Some might not find this completely convincing.

About 5 years ago, the field of "string cosmology" was quite active, with even a graduate-level textbook appearing. My impression (contrast the tone of this review, with those of 5 years earlier) is that there's much less interest in this area during recent years, since it became obvious that no predictions about physics were going to emerge from it. Late this year or early next year the Planck experiment will finally report what it sees in the CMB. I'm curious to know whether Linde and other string cosmology proponents have any predictions for what Planck will tell us.

Update: The Annenberg Foundation funds Annenberg Learner, a site designed to provide information to high school teachers. Their Physics course includes a unit from Stanford's Shamit Kachru, which is pretty much pure hype, unadulterated by any skepticism that string theory might not be the way the world works. Physicists may have lost interest in string cosmology, seeing it as a failure, but that's no reason not to teach it to high school students…

Update: Historian of science Helge Kragh has a new article Criteria of Science, Cosmology, and Lessons of History discussing the Multiverse, philosophy of science, and the dubious use of historical analogies. About the Multiverse: "it explains a lot but predicts almost nothing".

Update: Tonight's arXiv listings include The Top 10^{500} Reasons Not to Believe in the Landscape from Tom Banks, which starts off with:

> The String Landscape is a fantasy.

He goes on to claim co-credit with Linde for the anthropic explanation for the value of the CC (in inflationary models), as well as to argue that it is wrong:

> Linde and I were the first to suggest an anthropic explanation for the value of the c.c. based on inflationary models , but within the context of the string landscape, or most any contemporary view of global EI , I don't think anthropic reasoning leads to good phenomenology.

Comments

1. **physicsphile**
   August 25, 2012

Hi Peter,

As I understand it there isn't a unique prediction for Planck from string cosmology or from inflation in general. The main hopes would be that Planck eventually shows some evidence for gravitational waves or primordial non-gaussianity. Either of these should considerably narrow down the possible types of inflation that could have occurred. But unfortunately, if no such evidence is seen the results will still be compatible with many other forms of inflation from and not from string cosmology.

2. **Shantanu**
August 26, 2012

Peter and others,
just to give credit where its due, want to draw attention to Demos Kazanas paper in
1980 in ApJ which contained the same ideas of inflation. Its sad that not many people
cite this paper.
http://adsabs.harvard.edu/abs/1980ApJ…241L..59K

3. **Mike**
August 26, 2012

Will Planck be able to show evidence of gravity waves from the gravitational lensing of CMB measurements and will Planck actually confirm inflation predictions to a high degree of accuracy?

4. **paddy**
August 26, 2012

Peter sorry for being repetitive and either speaking to the converted or to deaf ears. Any invocation of (or recourse to) a multiverse/anthropic argument is suspect on so many different levels: (a) philosophically it begs the question, (b) logically/probabilistically it makes the inverse gambler's fallacy, and (MOST IMPORTANTANTLY) (c) scientifically it runs the gamut from being moot to being meaningless.

5. **Peter Woit**
August 26, 2012

paddy,

I basically agree, although one has to look in detail about exactly what claims are being made in each "anthropic" case. The underlying problem here is not so much the anthropic business, but that people are trying to mask the fact that their fundamental theory is a failed, vacuous theory predicting nothing. They're invoking a multiverse of different possibilities, but when you ask what their theory says about what these possibilities are, the true answer is that it says nothing, so they are making ad hoc assumptions that things are "equally likely".

For a different point of view, Linde basically compares opposition to anthropics to Stalinism:

" But what is important is that when we studied inflationary theory, we started asking questions which seemed to be metaphysical, like why parallel lines do not intersect, why the universe is so big. And if we had said, "Oh, my God, these are metaphysical questions, we should not venture into it," then we would never have discovered the solutions. Now we're asking metaphysical questions about anthropic principle, about stuff like that, and many, many people tell us, "Don't do it, this is bad, this is the "a" word (anthropic). You should avoid it."

We shouldn't avoid anything. We should try to do our best to use the simplest explanations possible, or what proves simplest, and if something falls into your hands as an explanation of why cosmological constant vacuum energy is so small, and you decide not to accept it for ideological reasons, this is very much what we had in Russia long ago. That ideology told me which type of physics was right and which type of physics was wrong. We should not proceed this way. Once you have multiple possibilities, then you can have scientific premises for anthropic considerations, not just philosophically talking about "other worlds". Now we have a consistent picture of the multiverse, so now we can tell: "this is physics, this is something serious." That was about multiverse and different versions of it."

6. **paddy**
August 26, 2012

Peter,
Your last point (with the quotes) actually frightened me when I realized that the implication is that some might then think that there should have been in my post another clause: (d) "politically" ……

7. **physicsphile**
August 26, 2012

Mike,

If inflation happened at around the GUT scale or higher, Plank may be able to detect the gravitational waves by B-modes in the polarization anisotropy of the CMB. Ground based and balloon experiments are trying to detect gravitational waves on smaller scales which involves trying to remove the B-modes generated by lensing.

The simplest models of inflation predict a red spectral index and a flat Universe. Planck should be able to confirm these to very high precision.

8. **Shantanu**
August 26, 2012

physicsphile,
from what I understand, GUT-scale inflation is already ruled out (or constrained) by WMAP limits. The whole idea of GUT is just a daydream.
shantanu

9. **physicsphile**
August 26, 2012

Shantanu,

WMAP constrains the tensor to scalar ratio to be r less than 0.36 at the 95 percent confidence level. The energy scale during inflation is 3*10^(16)r^(1/4) GeV and so is constrained to be less than 2*10^(16) GeV by WMAP. The GUT scale is around 10^(16) GeV and so WMAP has not ruled out GUT scale inflation.

I should also mention that the simplest model, ie canonical single field inflation predicts that Planck should not detect non-gaussianity, isocurvature perturbations, running of the spectral index or departures from flatness. It predicts that there should be a red power spectrum of perturbations which should be confirmed at the 5 sigma level.

10. **anonymous**
    August 26, 2012

    Tsujikawa's talk exhausts most of the single-field models people discuss. Shantanu will be pleased to see Kazanas very clearly cited.
    http://www2.yukawa.kyoto-u.ac.jp/~jgrg20/invitedata/I18_tsujikawa.ppt

    For more details on this parameterization, consult e.g. Seery & Lidsey.
    http://arxiv.org/abs/astro-ph/0503692v2

    The bottom line is that f_NL has to be small in any of these models with c_s=1. Unobservably small. The best you can get from the usual CMB methods is f_NL~3. (On paper, Planck is supposed to be sensitive to f_NL~5.) To put that into perspective, you'd have to have f_NL~100s before you could tell it's there just by looking at the maps. Anything with an observable f_NL would be really weird and tell us a lot. On the other hand, if that happens and we still want to stick to these classes of models, c_s<1 will mean it's that much more impossible to measure appreciable tensor-to-scalar ratio r without an ultra deep delensing survey maybe 10-15 years from now. Large-field inflation like chaotic inflation is actually the experimentalists' best hope right now.

11. **P**
    August 26, 2012

    "My impression (contrast the tone of this review, with those of 5 years earlier) is that there's much less interest in this area during recent years, since it became obvious that no predictions about physics were going to emerge from it"

    One of the difficulties in superstring cosmology is that there are many scalar fields, and thus many possible cosmologies – depending on which scalar is the inflaton, there are different predictions. But it's too strong and ultimately just wrong to say that any one of those candidate inflation scenarios doesn't make predictions. Axion monodromy, e.g., is a large scale inflation model which is very well motivated in type II string compactifications. This scenario predicts observable tensor modes – see McAllister, Silverstein, and Westphal.

    http://arxiv.org/abs/0808.0706

    Planck data will be out soon and could rule out or in this possibility.

    Cheers,
    P

12. **SUSY**
    August 26, 2012

    "Much of the piece is just a retread of the usual heavily-promoted ideology of the past 30 years of fundamental physics research: we must have SUSY, so must have supergravity, so must have string theory, so must have the landscape,"

    The chain of reasoning presented above seems "convincing" if SUSY does exist.

    You've expressed your skepticism of SUSY extension of SM in BSM physics. Are you also skeptical of supergravity? If researchers and papers are published that shows that supergravity is perturbatively renormalizable, isn't that strong evidence that SUSY is needed (and leads to the above chain of reasoning).

    BTW Eva Silverstein claims her research on string theory and SUSY would imply SUSY would NOT be found at LHC energies. So it's little wonder her husband promotes it without any skepticism. I can provide a link if you want.

13. **chris**
    August 27, 2012

    What is so special about perturbative renormalizability? Ordinary gravity is close to being proven renormalizable nonperturbatively, i find this equally appealing.

14. **Peter Woit**
    August 27, 2012

    SUSY,

    If supergravity by itself makes sense, that's one way (another would be chris's nonperturbative renormalizability of conventional gravity) to break the chain of arguments leading to the string theory anthropic multiverse. I don't want to start yet another discussion of the problems of quantum gravity. The point is just that if you have a long chain of arguments that leads you to something vacuous, what you have is a reductio ad absurdum, showing that there is some flaw in those arguments. There are plenty of possibilities for the flaw, including the starting point of SUSY.

15. **CU Phil**
    August 27, 2012

    Chris,

    By "Ordinary gravity is close to being proven renormalizable nonperturbatively", are you referring to the searches for an RG fixed point that are usually referred to as the asymptotic safety program?

    My understanding of the current state of things there is that there is numerical evidence based on truncations of the ERGE for certain actions, and with certain restricted field content. I think the program is an interesting one, but this seems to be somewhat far from having a proof of a fixed point in 4D for gravity in the presence of matter fields. Am I mistaken about the state of things there?

16. **Peter Woit**
    August 27, 2012

    CU Phil, Chris,

    I'm sorry, but this isn't a physics discussion board, and I'm not going to moderate yet another discussion of quantum gravity that has nothing to do with the posting. Best to take this sort of thing to Physics StackExchange or somewhere else.

17. **Bernhard**
August 27, 2012

There is nothing new in Linde´s anthropic argument for the vacuum energy but every time I hear it it amazes me how obvious it is that is can in principle "explain" any number. Anomalous magnetic moment of the electron? Forget a brilliant theory like QED that actually predicts it with exquisite precision. You just need some crap theory where you have a zillion solutions for the magnetic moment and when you realize you just can´t explain the number with your crap theory you invoke anthropic reasoning, after all, the electron must have that anomalous magnetic moment in this universe so that we can exist etc, etc. Really …

18. **theoreticalminimum**
August 27, 2012

On a completely different note: I wanted to throw this out for general interest. Cédric Villani has a book out this month, "*Théorème vivant*" (Édition Grasset). Villani has by now become a household name in France when it comes to the common perception of the (eccentric) mathematician and the popularisation of mathematics, and this book has been greatly expected by many. He's also the latest Fields medalist to start a new blog-like website. His colleague at the CMS (Cambridge), Clément Mouhot, also has a blog.

19. **Ly**
August 27, 2012

Roger Penrose once pointed out that there should still be a "horizon problem" for EWSB, since most inflation models postulate that inflation ceases well before the universe cools below the electroweak critical temperature. He seemed to think that large-scale defects left over from that era ought to occlude the light of distant galaxies, contrary to observation. Is it reasonable to expect EWSB defects to behave this way, or be otherwise dramatically visible? The papers I've looked at have invariably been irritatingly vague on the subject, when they raise it at all.

20. **John**
August 28, 2012

Peter,

"Much of the piece is just a retread of the usual heavily-promoted ideology of the past 30 years of fundamental physics research: we must have SUSY, so must have supergravity, so must have string theory, so must have the landscape, so must have a multiverse where we can't predict anything about anything, thus finally achieving success."
Granted, SUSY is the assumption. But if SUSY is discovered at the LHC, then doesn't it follow that supergravity should be more likely? If gravitons exist, and since every particle has a superpartner, it follows that the graviton must have a superpartner. Hence, supergravity. Now, I'm no expert, but I thought string theory is the UV completion of supergravity. And string theory predicts a multiverse. Though it's most likely experimentally unverifiable, it's a prediction nonetheless. And it has the potential to explain the smallness of the CC. That's why some people are taking the idea seriously. They would absolutely LOVE to test the idea, but they can't. But that doesn't mean they'll stop working on it. They believe string theory is the most promising approach to quantum gravity. What's wrong with that?

"So, the main experimental evidence for the multiverse is that an anthropic argument works. Some might not find this completely convincing."
But it's still a possibility that has the potential to explain the size of the CC. For a long time, there was no experimental evidence of planets outside the solar system. Now we've detected many of them, and an anthropic argument for the fine tuned conditions for life on our planet makes sense. Certainly, there is no conceivable way to test the multiverse, but it's still a possibility. Now suppose, in the distant future, we somehow find a way to conduct tests of a future quantum theory of gravity (not necessarily string theory) that has the multiverse as a consequence. Suppose we still have no idea how to test the multiverse portion of the theory. Suppose it pasts all those tests. Doesn't that mean the multiverse portion gains much more credibility? Gravitational waves haven't been detected yet, but nobody doubts their existence because GR has been tested so well.

According to the Annenberg Learner website, the course explores the "frontiers of physics." One definition of "frontier" that I've found defines it as "An undeveloped area or field for discovery or research." Doesn't string theory fit that description? According to the website: "The goal is to make the frontiers of physics accessible to anyone with an inquisitive mind who wants to experience the excitement, probe the mystery, and understand the human aspects of modern physics."

What's wrong with that? Frankly, when I was in high school, I wanted to learn more about string theory than condensed matter physics. Also, you can't exactly teach your interests, Peter, to these kids: representation theory applied to QFT. It may be exciting research on the frontiers, but nobody at that level will understand it and it won't entice them to study physics. Every section of that course, not just speculative string theory, can encourage the students' wonder for physics.

From the ST part of the course: "…theoretical effort to develop a "theory of everything" that brings all four forces under the same conceptual umbrella." What's wrong with talking about the "effort" to produce a theory of all forces? He doesn't mention whether it's been successful or not, just that it's an "effort". He also says, "the string concept has stimulated a great deal of theoretical excitement even though it has no connection to experiment so far." Is this not true, Peter? He even tells us that there's no experimental evidence for it so far. He's misleading nobody. He calls ST "the most promising approach", implying that there are others. What's wrong with that? (And yes, ST is the most promising approach so far.)

Therefore, Peter, when you comment about this description on the website with, "which is pretty much pure hype, unadulterated by any skepticism that string theory might not be the way the world works." you are dead wrong.

21. **Peter Woit**
August 28, 2012

John,

I don't see any point in repeating here the same arguments again about why the string theory multiverse is pseudo-science. Others can make up their own minds about whether teaching failed untestable speculation to high school students as the "frontiers of science" is in either their interest or the interest of the field.

22. **John**
August 28, 2012

Bernhard,

"Anomalous magnetic moment of the electron? Forget a brilliant theory like QED that actually predicts it with exquisite precision. You just need some crap theory where you have a zillion solutions for the magnetic moment and when you realize you just can´t explain the number with your crap theory you invoke anthropic reasoning, after all, the electron must have that anomalous magnetic moment in this universe so that we can exist etc, etc."

QED isn't a candidate for a quantum theory of gravity/fundamental theory, isn't it? If, in the distant future, a fundamental, well-defined theory is found that agrees with the experiments done in that future, AND predicts a multiverse, wouldn't that mean the multiverse has more credibility? It may not completely validate it because you can imagine a fundamental theory that agrees with the experiments and does not have a multiverse, but the conjecture at least gets a little more credible.

23. **John**
August 28, 2012

Peter,

Do you not agree with the definition of "frontier" that I gave you? If you don't, why not? If you do, then string theory fits nicely with that definition, wouldn't you say? QM has been with us for almost 100 years and has been very well tested and is in everything from TVs to smartphones. So it's not really "frontier". But string theory is, wouldn't you agree?

Also, do you not agree that the quotes I took from the ST unit website contradicts your claim that the page contains information "which is pretty much pure hype, unadulterated by any skepticism that string theory might not be the way the world works." ? Let me repeat those quotes, in case you forgot:

"…THEORETICAL EFFORT to develop a "theory of everything" that brings all four forces under the same conceptual umbrella." He also says, "the string concept has stimulated a great deal of theoretical excitement even though IT HAS NO CONNECTION TO EXPERIMENT SO FAR."

And, for all we know, the ST program is not dead yet. There is much work to be done to discern what M-theory really is. Correct?

24. **John**
August 28, 2012

"I don't see any point in repeating here the same arguments again about why the string theory multiverse is pseudo-science."

Peter, please don't leave me hanging. I wrote so much in those first two paragraphs. I did my best! Please direct me to the pages that show how wrong I am. If SUSY is detected, and if gravitons exist, supergravity should exist, right? And if supergravity needs a UV completion, and if string theory is the only UV completion for supergravity, hence string theory. And if string theory predicts the multiverse, then the multiverse is something worthy of more study. And if a fundamental theory in the future passes all test and it predicts a multiverse, wouldn't you say the multiverse gains in credibility? (I even heard Steven Weinberg give this argument.) Yes or no?

Please, Peter, don't leave me hanging here. Yes, or no?

Don't you appreciate the relation between the anthropic multiverse and the fact that many many planets exist? It's another anthropic argument.

25. **John**
August 28, 2012

Also, Peter, if a high school student gets so excited by string theory that he/she decides to major in physics, but then decides he/she wants to do condensed matter theory and he/she makes significant progress in condensed matter theory, isn't that a good thing? And, again, the course is about "frontiers" in physics, and string theory is definitely part of the frontiers of physics.

26. **Peter Woit**
August 28, 2012

John,

Yes, amidst the pages and pages of hype, one hype-filled sentence ("A GREAT DEAL OF THEORETICAL EXCITEMENT") does have one clause admitting no connection to experiment ("SO FAR"). Whether one clause buried in the pages of hype for failed ideas salvages the whole thing as a responsible project aimed at high school teachers is up to people to decide for themselves.

And yes, as you argue with Bernhard, if string theorists all of a sudden calculate accurately all the parameters of the SM, using a string theory landscape calculation that implies a multiverse, that would be good evidence for a multiverse. It's also true that if I discover tonight a wonderful TOE which explains everything perfectly and has no multiverse, that will be evidence against the multiverse. At this point, both of those eventualities seem equally irrelevant: I'm a lot more likely to get run over by a truck on my way home tonight.

As for your speculation about the smart high school student, what if he or she instead of getting excited about the hype sees it for what it is, decides "frontier science" is BS, and decides to become a lawyer instead of a scientist? I think a lot of that has been happening in recent years….

I can tell you have a deep and abiding love for this hype. Enjoy, but I'm already wasting too much of my life discussing it, enough for now.

27. **John**
August 28, 2012

"decides to become a lawyer instead of a scientist? I think a lot of that has been happening in recent years…."

Can you give me evidence of this? How many high school kids do you know?

"Yes, amidst the pages and pages of hype," Pages and pages? Can you send me a link to all those "pages and pages"?

"And yes, as you argue with Bernhard, if string theorists all of a sudden calculate accurately all the parameters of the SM, using a string theory landscape calculation that implies a multiverse, that would be good evidence for a multiverse. "

So it's no impossible, right? It's still possible for this to someday happen. So what are you complaining about?

"It's also true that if I discover tonight a wonderful TOE which explains everything perfectly and has no multiverse, that will be evidence against the multiverse."

I totally agree with you.

"I can tell you have a deep and abiding love for this hype." I don't love the hype. I'm just trying to show you that you are wrong. If I'm not allowed to do this, then why have a comments section for your blog?

Also, Peter, you didn't tell me whether or not you agree with my definition of 'frontier' and whether string theory fits that description.

You also haven't given me the websites that goes over the flaw in the logical progression of SUSY –> supergravity —> ST —-> multiverse. Do they exist?

28. **John**
August 28, 2012

"As for your speculation about the smart high school student, what if he or she instead of getting excited about the hype sees it for what it is, decides "frontier science" is BS…"

But the description of this class specifically says that there is so far no experimental evidence for it. It also specifically says that it is a "theoretical effort". That's not hype, that's honesty. If a high school students decides not to pursue physics even after those disclaimers, that's his/her problem. Are physicists not allowed to study this? And are people not allowed to show this to high school students? If such a unit is presented in that way (emphasizing lack of experimental support and that it's still very speculative), it's much more likely to excite students than turn them off.

Again, please link to me all those "pages and pages" of hype you saw.

29. **John**
August 28, 2012

"I can tell you have a deep and abiding love for this hype. Enjoy, but I'm already wasting too much of my life discussing it, enough for now."

But most of your blog posts are about String Theory hype! And now, suddenly, after responding twice to my comments, you decide that you're wasting too much of your life? Until the next obscure thing you find on the web that strangely convinces you is hype? I mean, where did you even find this course website? It's like you're purposely seeking out this stuff. If you're wasting too much of your life with this stuff, then just stop posting about string theory hype. I mean it's like it's so bad, you're posting stuff you THINK is hype, when it really isn't, as I showed above in this particular example (see the quotes from the website). I just don't get it.

30. **Anonyrat**
August 28, 2012

I'll give a a course in astrology, that hasn't been proven yet, but conceivably could be a guide for each and every human being on this earth, and lead them to their true destiny. And maybe some of the high school children will become interested in astronomy instead. Peter, oh , Peter, what is wrong with that?

31. **John**
August 28, 2012

Anonyrat,

Astrology HAS be shown to be incorrect. You know that. So your example makes no sense.

32. **John**
August 28, 2012

Peter,

I guess you have no rebuttal for me? Let me know. 🙂

33. **Typhoon**
August 30, 2012

Consider the spherical high-school student . . .

All this pure speculation about what a hypothetical high school might or might not choose after being exposed to ST is analogous to arguments for the multiverse and vice versa.

34. **Chris W.**
August 30, 2012

Astrology HAS be shown to be incorrect.

No it hasn't, because it can't be. Why? Because it doesn't lay any hard predictions on the line. It always has an excuse. That sounds kind of familiar…

(It doesn't mesh well at all with successful theories that *do* lay any hard predictions on the line, so it is certainly incorrect in that sense, and it makes only trivial use [at best] of mathematical arguments. Obviously astrologers don't care about any of that.)

35. **John**
August 30, 2012

"Because it doesn't lay any hard predictions on the line."

Astrology does have a prediction. It predicts that your life and/or personality is dictated by the positions of the stars relative to the earth. It has been tested: Take any two people born under the same sign. If their life and/or personality is different from each other, astrology is wrong.

String theory does have a prediction: it predicts SUSY, though not the energy scale at which it is broken.

36. **DrDave**
August 30, 2012

IF I were to say the following (and I'm not): "There are other universes in the multiverse where astrology has been proven to be correct, and eventually we may be able to detect these other universes. You just need to be patient." You might be skeptical. The problem is that there is no logical determinant in an open-ended, adaptable universes that is different from the above quote. Once you assume that any universe in a multiverse can be constructed to adapt to data which which is contra-indicative, there is a logic problem which is essentially: no argument can be proved or disproved. People who are tired of the argument simply want some rules, and if "no rules" is the game, then there is basically just an endless loop.

37. **scientific responsibility to the public**
September 2, 2012

"The Annenberg Foundation funds Annenberg Learner, a site designed to provide information to high school teachers. Their Physics course includes a unit from Stanford's Shamit Kachru, which is pretty much pure hype, unadulterated by any skepticism that string theory might not be the way the world works. Physicists may have lost interest in string cosmology, seeing it as a failure, but that's no reason not to teach it to high school students…"

If, by 2020, the LHC at full design energy and luminosity (14TEV) finds no evidence of SUSY after collecting sufficient data, and no other evidence of SUSY is forthcoming, do you think Stanford's Shamit Kachru and other popularizers (i.e Brian Greene, Michio Kaku, Stephen Hawking) have a public responsibility to say string theory should be treated with skepticism?

38. **Peter Woit**
September 3, 2012

scientific responsibility,

Actually I think it's now been a long time that string theory unification is something that should be presented skeptically to the public. I don't think the failure to find SUSY (or anything else for that matter) is going to make much difference to the people promoting string theory to the public. The Kachru material for high school teachers avoids claiming LHC-scale SUSY as a string theory prediction, and everyone still promoting string theory is now careful to do this, since it is now clear what the experimental bottom line is likely to be.

Nothing is going to stop continuing attempts to mislead the public, but the physics community is not so easily misled, and is already very skeptical about string theory unification. The negative LHC results will have an impact among physicists.

Simons Foundation and the arXiv

August 28, 2012
Categories: Uncategorized

Via the Quantum Pontiff, news that the Simons Foundation will be providing up to \$300,000 in financial support to the arXiv for each of the next five year. Last year, the arXiv announced a \$60K planning grant from Simons. Now the Foundation is stepping in with a much bigger contributions, for details see here.

This kind of support for open-access publication is an excellent way for Simons to use its resources. Perhaps this will be the beginning of a larger effort to buy back control of the math and physics literature from commercial publishers and set up a viable model for making this literature available to all going forward. This may be an expensive undertaking, but Simons (and other math/physics-friendly financiers) have resources on the scale necessary to do this.

Comments

1. **MathPhys**
   August 29, 2012

   If the Simons Foundation or any other with money to spend on science can make, for example, pre-arXiv papers that appeared in Nucl Phys B available to all of us, quite a few of us would be really grateful.

2. **chris**
   August 29, 2012

   a big thank you to all the responsible people. this is a way of donating money that really helps the entire scientific community.

3. **David Nataf**
   August 29, 2012

   Of course we shouldn't need to be helped. A lot of scientists choose to submit to corporate journals and to submit to the ArXiV.

4. **David Nataf**
   August 29, 2012

   Sorry, I meant to write "not to submit to the arXiv".

5. **Sakura-chan**
   August 29, 2012

   It's really frustrating when a journal paper is archived online, but the publisher does not let individuals purchase access to single papers. And then when they do allow individual purchase, it is usually around \$20 to \$30 for a single paper. Even the big libraries that I am affiliated with don't have complete journal coverage, so I am left to sheepishly cold e-mailing the author to see if they have it on hand.

6. **P**
   August 29, 2012

   This is great news. The Simons foundation is doing wonderful things for mathematics and physics.

   My friends in fields outside of physics can't believe that we have such a nice centralized preprint archive. Are there any archives in other fields similar to the scale of arXiv.org?

7. **Shantanu**
   August 29, 2012

   Peter this is off-topic , but have you watched the panel debate on future of physics @aspen this month moderated by Lisa Randall?
   it has some interesting stuff.

8. **Peter Woit**
   August 29, 2012

   Shantanu,
   Just took a look, but there seemed to me little there about the actual topic of the future of physics beyond a lot of self-satisfied general comments. For those interested, see
   http://vod.grassrootstv.org/vodcontent/11072-1.wmv
   but I'd rather not start a discussion of it here.

9. **Av**
   August 29, 2012

   Not precisely on-topic for this thread, I recognize, but:

   http://arxiv.org/pdf/1208.5715v1.pdf

   I'd be interested in Peter's views on these "Top 10^{500} Reasons Not to Believe in the [String] Landscape".

10. **Peter Woit**
    August 29, 2012

    Av,

    That topic belongs in the previous posting, where I yesterday added an update about it to the posting…

11. **Allan Rosenberg**

August 30, 2012

arXiv is great, but if the goal is to make scientific results free to everyone, we still need peer review. Shouldn't there be a PLOS journal for physics, too?

12. **open access**
August 30, 2012

I regularly contact authors to ask for a pdf copy. Nothing to be sheepish about it. Note that the arXiv is more than open access. It is also non-peer review. Open access in the sense of the arXiv does not imply quality control. It is also true that authors post rejected papers on the arXiv. A friend of mine once told me about a paper he saw posted. The description seemed familiar. I looked it up and recognized it as a rejected manuscript I had refereed, but now with my referee comments mixed in. So this is all good, what the Simons foundation does, but don't expect quality of publications to improve. Indeed, I have no doubt the ST gang (and many others) love the arXiv.

13. **Chris Austin**
August 30, 2012

I would also like to thank the Simons Foundation for its support of arXiv.org.

The arXiv quality control model seems to me to be similar to the quality control model used in manufacturing industry, where the most effective quality control is the control workers apply to their own work. For competent production workers, the knowledge that a small, random sample of their work will be checked by supervisors is sufficient to ensure that all their work meets the required standard. Conversely substandard workers receive a period of intense scrutiny and coaching, during which their work either improves to the required standard, or they lose their jobs, which would correspond to losing arXiv submission privileges.

When submitting a paper to arXiv, I take far more care with it than I would if I were just submitting it to a journal, because I know that submitting an unacceptable paper to arXiv could result in permanent loss of submission privileges.

Another reason that papers submitted to arXiv but not to a journal may be better written than papers also submitted to a journal, is that if authors know that a journal editor might alter what they write, they might be less motivated to take care with the presentation.

14. **open access**
August 30, 2012

What is "unacceptable?" Is this unacceptable?
http://arxiv.org/abs/0901.2564

I know for a fact that the authors did not lose their arXiv posting priveleges. Do you know anyone who has lost their arXiv posting priveleges?

15. **Peter Woit**
August 30, 2012

Chris is referring to authors without regular academic positions, who can have difficulty being allowed to post articles on the arXiv. For those with regular academic positions, I think not being allowed to post on the arXiv is highly unusual. One example might be Brian Josephson. Please though, I'd rather not try and moderate a discussion here and now of arXiv censorship issues.

16. **Jeff M**
August 30, 2012

Not sure if arXiv is any different for physics and math, but in math I think it's essentially unheard of for someone in an academic position to lost posting rights. Removal of papers is purely voluntary as far as I know, if someone finds a mistake in your paper you should of course remove it (and I know of times when this has happened) but I'm sure it doesn't always happen. That said, arXiv is a wonderful, amazing resource. In any case at this point many mathematicians post their papers on their own websites as well, including already published ones. Funny story about the referee finding a rejected paper on arXiv, with referee comments – I just refereed a paper which I had already read, and discussed with a colleague, since it had been on arXiv for a while. Made the refereeing process much easier 🙂

17. **MathPhys**
August 30, 2012

B D Josephson can post on the arXiv

http://arxiv.org/ftp/arxiv/papers/1206/1206.5850.pdf

18. **MathPhys**
August 31, 2012

Paul Ginsparg's arXiv is probably the most important factor in advancing research in physics in the Third World post-1992. To my mind, Ginsparg should get a Nobel Peace prize for that. If Third World scientists aren't busy doing physics, superstrings included, they could turn their supersmart minds to less peaceful endeavors with disasterous consequences.

19. **aliaspg**
August 31, 2012

"If Third World scientists aren't busy doing physics, superstrings included, they could turn their supersmart minds to less peaceful endeavors with disasterous consequences."

As in "developping toxic financial products for Wall Street?"

20. **MathPhys**
August 31, 2012

As in "developping toxic financial products for Wall Street?"

Yes. Literally.

21. **aram**
September 6, 2012

The reason not to post bad papers to the arxiv is that good researchers have reputations that they want to protect (and also some inherent pride in doing good work). The flip side is that we might not read arxiv papers from people we haven't heard of, but we would look at their papers in a journal because then we know that someone we (somewhat) trust has vouched for it being interesting.

Short Math Links

August 31, 2012
Categories: Uncategorized

Some links of mathematical interest that I've recently run across:

- The life and work of Alexander Grothendieck is one of the great stories of modern mathematics. Winfried Scharlau's first volume of a biography of Grothendieck, covering the years up to 1948 is now available in English, see here. The third volume, covering Grothendieck's life after 1970 is only available in German, see here. Leila Schneps is writing a second volume, covering his life and mathematics during the height of his career, from 1948-1970, with chapters appearing as they are written on this page. She is now up to 1952.

  The same page contains links to various wonderful articles about Grothendieck's mathematics, many by mathematicians who interacted with Grothendieck during his period of greatest mathematical activity.

- Cédric Villani joins other Fields Medalists with blogs, see here. Villani has just published in France a memoir called *Théorème Vivant*, a mix of autobiography and description of a collaboration on a mathematics problem. More about the book here, here and here, with a video here.
- Many of the talks given at this summer's String-Math 2012 conference are now available as slides or video, see here.

Comments

1. **MathPhys**
   September 1, 2012

   Grothendick lives in the south of France with a daughter who cares for him. At least that was the case about 5 years ago.

2. **Raisonator**
   September 17, 2012

   I found this interesting link, Alain Connes goes blogging:

   http://noncommutativegeometry.blogspot.de/ – A DRESS FOR THE BEGGAR ?

   His latest paper is pretty exciting, I find.

3. **martibal**
   September 17, 2012

   Maybe off topic, or already treated elsewhere, but could someone explain a bit this idea that coupling the Higgs with a new ? or neutrino related ? (this is not clear to me) scalar field, then some instability due to the low mass of the Higgs are cured.

   This is the point of this Chamseddine-Connes paper: such an interaction term appeared in the spectral action, but was neglected. It seems that taking it into account, then noncommutative geometry can now accommodate a 125GeV Higgs (whereas the previous prediction, back to 2010, was around 170 GeV).

The Templeton Effect

September 3, 2012
Categories: Uncategorized

The Chronicle of Higher Education has a long story about the Templeton Foundation, entitled The Templeton Effect. Much of it is about various subfields of philosphy where Templeton money has been successful at bringing religion, theological concerns and religious philosophers to greater prominence. One section however describes the Templeton funding promoting a new field of Philosophy of Cosmology. Religion doesn't explicitly appear here, but the story of how this "Philosophy of Cosmology" got underway gives a good example of how money influences intellectual pursuits:

> Barry Loewer, a philosopher at Rutgers University at New Brunswick, isn't likely to turn up at a Society of Christian Philosophers meeting with Newlands and Miller. "I myself have no interest in philosophy of religion and am not a religious person," he says. For years, Loewer has been working with a group of philosophers, mathematicians, and physicists in the New York area, meeting and collaborating on papers—nothing very expensive. But about five years ago a colleague at Rutgers, Dean W. Zimmerman, told the group about the Templeton Foundation and suggested that they apply for a grant. Zimmerman, a top Christian philosopher, had already served on Templeton's advisory board and participated in many foundation-sponsored activities.
>
> The idea at first was to do a project about quantum mechanics and the foundations of physics, which was an interest of Loewer's group. Templeton had other ideas. The foundation pointed the group in the direction of cosmology, with the prospect of a much bigger grant, and the researchers jumped at the idea. They realized that cosmology encompassed the questions of time and physical laws that had concerned them all along.
>
> "You know that story of Molière's where someone discovers that he has been speaking prose his whole life?" says Loewer. "It was a little bit like that."
>
> The nearly $1-million grant his team received from Templeton last year coincided with another, slightly larger one called "Establishing the Philosophy of Cosmology," which was awarded to scholars at the University of Oxford. Despite the change of plans at Templeton's behest, Loewer stresses, "They've been really helpful, and totally noncoercive in terms of any agenda that they might have. I had my eyes open for it."
>
> Not that philosophers are especially well practiced in negotiating the terms of million-dollar grants, much less in thinking about how such money might sway them. Neither Loewer nor Mele nor Miller nor Newlands could have anticipated back when they were in graduate school that they'd be administering projects like this; their training was for armchairs, libraries, and conferences. But now that the money is coming into the field, it is being welcomed even by those who lack the foundation's spiritual proclivities. "Templeton picks some people whose Christian epistemology I might not share," Brian Leiter says, "but there's no quarreling that they're serious philosophers." Suspicions about some secret religious agenda tend to lessen the more widely the foundation's substantial sums begin to spread.

Comments

1. **Matti**
   September 3, 2012

   Why evolution is true blog had today piece about Templeton funding a homeschooling course at Cambridge University. The Anthropic principle, science giving evidence for faith and so forth. All neatly packed for home schoolers. Laugh or cry, not sure which one.

   http://whyevolutionistrue.wordpress.com/2012/09/03/hey-kids-a-homeschool-course-on-accommodationism-courtesy-of-the-faraday-institute-cambridge-university-and-the-templeton-foundation/

2. **Pravda**
   September 3, 2012

   Oh, more concerns about Templeton money and Christian theology.

   It seems as though I am the only person who considers Christian theology to be rather innocuous compared to post-modernism, or the "sociology of scientific knowledge/science" which on any balanced analysis must be recognized as being far more pernicious than a belief in evolution. Need I even mention the theological aspects of string theory? Even belief in "The Singularity" a la Kurzweil has more far-reaching deleterious consequences than belief in creationism.

   One can only assume that this has got to do more with the socio-economic status of the groups in which these beliefs are prevalent, than the beliefs themselves.

3. **Peter Woit**
   September 3, 2012

   Pravda,

   When Kurzweil starts a multi-billion dollar foundation devoted to influencing fundamental physics research, I'll start paying attention to him and that "Singularity".

   In the meantime, discussion of the very large set of "things that are worse than Templeton" is off-topic.

4. **Michael Kovarik**
   September 4, 2012

   Templeton funds a lot of fundamental research in fields where there otherwise is a money drought (see FXQi, whose existance owes itself to Templeton) so that the McCarthy-esque attempts to discredit them are becomming stale.

   Templeton does not have overtly religious overtones as skeptics claim. It is an institution that feuls a lot of discussion on the relationship between the natural universe and spirituality — a topic that is both intellectually serious as well as important to many people. Once you remove the assumption of empiricism (which is, of course, necessary to formulate scientic hypothesis) from the *a priori*, truth becommes a little more blurry.

   I know you don't like nonscience parading as science, but I don't think that is what Templeton is trying to do, nor do I believe that people take what Templeton funds as purely scientific discussions.

   As for this specific project, Philosophy of Cosmology, I am glad that Rutgers is getting that grant. The physics department needs it terribly.

5. **chris**
   September 4, 2012

Templeton has its agenda – but is it so different from public funding sources?

in the US, NSF and DOE have their agenda, too. it is always determined at a very high political level which kind of research gets funded and usually the lucky ones – those who have their interest aligned more or less with the agendas – tend to just praise the decisions and take the money. scientists in other subfields largely go unnoticed because no money means no employment, less publications, no public recognition of the whole subject.

this is just the way science works as a social, human endeavor.

6. **S. Molnar**
   September 4, 2012

   "Suspicions about some secret religious agenda tend to lessen the more widely the foundation's substantial sums begin to spread." Well, my suspicions increase when that happens.

   That said, I agree with chris that government funding can be pernicious as well. I seem to recall that Norbert Wiener (and there were others) was very much against mathematicians accepting government grants for just that reason when they began in earnest around 60 years ago. On the other hand, it's hard to do research on an empty stomach and impossible to do modern experiments without a lot of money.

7. **srp**
   September 5, 2012

   Anti-materialism in a metaphysical sense seems to be the thing Templeton wants to promote. Platonists, romanticists, anthropicists, etc. would all be potentially useful to that project. To the extent that their views have any independent merit I don't mind TF throwing some bucks at them, especially if they also have the side effect of paying for good theoretical physicists and philosophers to do their thing.

Fall Course: Quantum Mechanics for Mathematicians

September 3, 2012
Categories: Uncategorized

This fall I'm teaching on quantum mechanics for mathematicians, at the undergraduate level. There's a web-page with more information here. I'll be writing up lecture notes, which should appear on that web-page as the course goes on, starting Wednesday.

We'll see how this works, but the plan is to teach many of the standard topics, although starting from a different point. Most quantum mechanics classes start out with classical mechanics, then somehow try and motivate quantum mechanics from there, following the historical logic of the subject. I'll instead start with the simplest purely quantum systems, especially the two-state, spin-1/2 system, now famous as the "qubit" of quantum computation. This is also a central example for the theory of Lie groups, Lie algebras and representations, so something that every mathematician should become familiar with. Another advantage of starting here is that there's no analysis, just linear algebra, and one can easily do everything rigorously.

Later on in the course I'll get to the standard material about wave-functions and quantum particles in potentials. The emphasis will be though not on the analytical machinery needed as a rigorous foundation for this subject in general, but on specific problems and their symmetries, and the use of these symmetries to do real calculations, ending up with the spectrum of the hydrogen atom.

We'll see how this goes, and what the students think of it. As lecture notes appear, corrections and suggestions of how to improve them would be appreciated.

Comments

1. **Jeff M**
   September 3, 2012

   Peter,

   You might want to check out anything you can find about the course Herb Bernstein has been teaching at Hampshire since the early 70's – I think Lee Smolin took it when he was there. It assumes no math at all that I remember, and no physics either, but he does QM from the operator perspective, rather than the way I (and I assume most people) first learned it. He was of course aiming at a different audience (many if not most of the students were first or second year), but it might be interesting for you to check out. Not sure what if anything has been written about it, pretty sure Herb is still at Hampshire though he must be close to retirement at this point, plus he has his institute.

2. **qm for physicists**
   September 3, 2012

   There is a good reason standard QM courses and textbooks start off as they do. The intended audience (physics undergraduates) have 2-3 years of experience with classical dynamics — Newton, then Lagrange and maybe Hamilton — also electrodynamics and relativity, also optics and statistical mechanics. It makes perfectly good sense to talk about blackbody radiation and the photoelectric effect. It also makes perfectly good sense (and is very good physics) to teach students that the equations of classical physics were derived on the basis of macroscopic objects, and now they must learn that there is no reason for thos equations to be true at the atomic level, and the fact is that new ideas (not just new equations) are needed. It is a cautionary tale that we should not assume that the equations we know apply all the way down to the smallest, and also the largest, length scales. It is the business of a physicist to recognize that there may be new physics in domains of energy, momentum, length, time, whatever, which have not been explored yet. QM at the atomic level is an example of this. Feynman has a good piece about this in the Feynman Lectures on Physics, although I cannot cite the exact page reference from memory.

   It makes sense to begin with a "free particle", then a "particle in a box", then a "particle hitting a barrier" (includes tunneling through a barrier) then a harmonic oscillator and then the hydrogen atom. These are all concepts the students can relate to, at some level, with material in their prerequisite courses. They can solve some partial differential equations. They have already solved Laplace's equation and the diffusion equation in many problems. But Lie groups? How many physics undergrads know about Lie groups? SU(2)?

3. **Mike**
   September 3, 2012

   This seems like a good perspective for mathematicians, but I really enjoyed the comparison of classical physics with QM when I took the intro to QM class. I am guessing most math majors have some exposure to basic physics problems so it's probably OK to discuss some of these great examples (potential wells, harmonic oscillators) for a good intuitive basis of comparison between NM (Newtonian) and QM.

4. **Paul Wells**
   September 3, 2012

   Peter,

   Feynman did a similar experiment in Feynman lectures vol III treating spin/rotation as central before moving onto Schrodinger's equation. Interested in your comments on his approach.

   Good luck!

   Paul

5. **Peter Woit**
   September 3, 2012

   qm for physicists,

   Undergrad math students rarely know anything about Lie groups or SU(2) either. Part of the idea of the course is to teach the theory of SU(2) and its representations, assuming no knowledge of the subject. No intention to get into the general theory of Lie groups.

   Paul Wells,

   I've gotten some inspiration from Feynman's lectures and his starting this way. He has all sorts of wonderful material about the physics of spin, and I guess was trying to get students familiar with that as a way of getting into the subject. I've heard this wasn't very successful, perhaps partially because it's very challenging stuff to absorb. Unlike Feynman, I'll be concentrating more on the mathematics (which Feynman was kind of avoiding) hoping that if students get comfortable with the mathematical ideas in this simple context, it will help them in further study of math, as well as in understanding these physical systems.

6. **Matt Leifer**
   September 3, 2012

   You should take a look at Schumacher and Westmoreland's book if you have not done so already http://www.amazon.com/Quantum-Processes-Information-Benjamin-Schumacher/dp/052187534X

7. **Emma Woodhouse**
   September 3, 2012

   Hi Professor Woit, I have little physics knowledge but very high level math skills–would I survive this course?

8. **Jeff M**
   September 3, 2012

   Peter,

   If you want a good undergrad resource for Lie Groups, check out http://www.amazon.com/Lie-Groups-Introduction-Mathematical-Association/dp/0883857596/ref=sr_1_4?ie=UTF8&qid=1346719859&sr=8-4&keywords=pollatsek
   I took algebra with Harriet as an undergrad, and she did Lie Groups the second semester, I think the book is basically taken from her teaching notes.

9. **JollyJoker**
   September 3, 2012

   The starting point seems very nice. It's for mathematicians rather than physicists, so a historical "why we needed this" view seems pointless. As a coder without any real education in QM but yet an interest in what's happening in physics, I've been thinking about reading some basics of quantum computing to get an idea of what "quantum" really means.

10. **Lowell**
    September 4, 2012

    You should look at

    Julian Schwinger, "Quantum Mechanics" Springer

11. **David Nataf**
    September 4, 2012

    My introductory QM course at McGill started off the with the Stern Gerlach experiment. We used the textbook by townsend which didn't get to the wave function until chapter 6.

12. **Sadiq Ahmed**
    September 4, 2012

    Hello Dr. Woit,

    Any way to audit this course through the internet?

    Warm regards.

13. **Sadiq Ahmed**
    September 4, 2012

    Postscript:

    A very useful reference:

14. **Sadiq Ahmed**
    September 4, 2012

    Intermediate Spectral Theory and Quantum Dynamics (Progress in Mathematical Physics)

    Link: amazon.com/Intermediate-Spectral-Dynamics-Progress-Mathematical/dp/3764387947

15. **Tmark48**
    September 4, 2012

    @ JollyJoker : having an historical point of view is never pointless especially in Quantum Mechanics. Even if the course is geared towards students of mathematics, giving them an idea of why the things are the way they are in QM is a good way to teach the subject. Then you can go on exploring exotic topics, or even formulate QM is a purely group theoretic way. Remember QM is not mathematics, it is physics so in ultimate analysis physics provides the justification for mathematics not the other way around.

16. **Peter Woit**
    September 4, 2012

    Thanks to all for the book suggestions, most of which I wasn't aware of. The Townsend book that David mentions (A Modern Approach to Quantum Mechanics, John S. Townsend) looks quite good, starting as I'd like to try with spin. The book that Matt Leifer mentions also (like most books oriented towards quantum information theory) starts with spin, looks very interesting, although headed more in a different direction than the course I'm planning to teach. JeffM, that looks like a very nice intro book on groups and Lie algebras. I do though want to emphasize much more the representations, which that approach doesn't cover.

17. **Peter Woit**

September 4, 2012

Emma Woodhouse, Sadiq Ahmed,

The only prerequisite for the class is calculus/linear algebra. In particular no physics will be assumed (although for some later topics, some physics background would be helpful). I'll be writing up notes as I go along, anyone who wants to is invited to try following along as those appear. We'll see how far they get before I run out of steam.

One thing I'm also thinking of doing is providing a much shorter, summary version of the notes, one aimed at mathematicians with a lot of background. This would contain no explanations of the math, just explanations of the physics in terms of the math, much of which would be a translation effort.

18. **Bob Jones**
September 4, 2012

I really think this is the best way of teaching quantum mechanics. The approach that starts with wavefunctions and infinite-dimensional Hilbert spaces is just too mathematically complicated for a student seeing the subject for the first time. The only problem with this simpler approach is that there aren't many interesting physical systems. Besides spin-1/2 particles (and particles with higher spin), what systems can you study using only finite-dimensional Hilbert spaces?

When I was first learning quantum mechanics, one of the things that bothered me was the arbitrariness of the postulates. I think you can minimize the number of unmotivated postulates if you take a more mathematical approach like the one described here. For example, in the C*-algebraic formalism, you start by postulating that the observable quantities in a quantum mechanical system correspond to the self-adjoint elements of a unital C*-algebra. From this one assumption, you can derive the most of the theory. For example, it follows from the operational meaning of the word "state" that the states of a quantum system correspond to functionals on the C*-algebra. You can then recover the Hilbert space using the GNS construction, and you can derive Born's rule from the Riesz representation theorem. You can also get Schrodinger's equation from Stone's theorem on one-parameter unitary groups.

If I were teaching a course on quantum mechanics for mathematicians, that's how I would do it.

19. **Peter Woit**
September 4, 2012

Bob Jones,

Well, you can take tensor products…. You could also do the SU(3) classification of states using flavor in particle theory, but that's way beyond this kind of course. The theory of spherical harmonics and thus the classification of atomic orbitals by angular momentum quantum numbers is also essentially finite-dimensional.

At least for a course at this level, I don't want to try and set up general theory, just work out the parts of the theory and examples where most everything comes down to representation theory (i.e. not do general potentials, just quadratic and 1/r, and maybe set up perturbation theory).

20. **Scott Aaronson**
September 4, 2012

Bravo, Peter! If there's *anything* I'm confident about in life, it's that starting with qubits is the right way to teach quantum mechanics. I tried to set out the reasons in my Quantum Computing Since Democritus Lecture 9. Basically, yes, it can be great fun to keep students in suspense for an hour or so (piling on one strange phenomenon after another) before finally letting them in on a great secret (in this case, that Nature prefers the 2-norm over the 1-norm when doing probability theory). But it seems excessive to drag out such a shaggy-dog story for an entire semester!

Two other sets of course lecture notes people might find helpful, if they're interested in this approach to quantum mechanics, are Umesh Vazirani's (from his undergrad course "Qubits, Quantum Mechanics, and Computers") and John Preskill's (from his graduate quantum computing course).

21. **Bob Jones**
September 4, 2012

Peter,

Thanks for your response. Those are nice examples.

A pretty good book which formulates quantum mechanics in the way I described is

http://www.amazon.com/Introduction-Mathematical-Structure-Quantum-Mechanics/dp/9812835229/ref=la_B001JO5VSM_1_2?ie=UTF8&qid=1346783227&sr=1-2

22. **Peter Woit**
September 4, 2012

Scott,

Thanks. I've already had a link to the Preskill notes, will look at the Vazirani ones.

I liked your lecture trying to motivate quantum, especially the QM as OS line. Still though, I think the answer to the "why" questions is something like "because unitary representations of groups are a fundamental unifying structure that is all over mathematics, so why shouldn't it be fundamental for physics?

23. **Mathematician**
September 4, 2012

Excellent Peter, am looking forward to your notes!

24. **Laurens Gunnarsen**
September 5, 2012

May I suggest that you may want to introduce your students to the profoundly illuminating work of the late Itamar Pitowsky? I would in particular suggest the immediate relevance to your purposes of the fascinating and highly readable article

"George Boole's 'Conditions of Possible Experience' and the Quantum Puzzle"
Brit. J. Phil. Sci. 45 (1994), 95-125

Here one finds what is probably the most penetrating and revealing account ever written of the reasons for the existence of (and general methods for constructing) "Bell-type inequalities” — which are, of course, badly violated in the real world of elementary particle interactions.

In its essentials, Pitowsky’s exposition is readily accessible to students with the background you assume, and I find it hard to imagine how any other approach could ever hope to equal his in clarifying the precise sense in which quantum phenomena are so extraordinarily bizarre.

25. **jg**
September 5, 2012

If you’re going to start with spin then be aware of the non-relativistic origin of spin famously argued by Levy LeBlond in this 1967 paper , (free download from Project euclid) and discussed by Walter Greiner in chapter 13 of his introductory Quantum Mechanics book: Quantum Mechanics: An Introduction

(more pages viewable with ‘Look Inside’ feature at amazon)

(Greiner’s text follows the more traditional introductory course)

26. **CU Phil**
September 5, 2012

Hi Peter,

Sakurai’s book “Modern quantum mechanics” also starts with a description of spin-1/2 particles and Stern-Gerlach apparatus, and is a pretty good book on the whole (as you probably are aware).

27. **Shantanu**
September 5, 2012

Peter again slightly OT, but would be curious to know your talk on the latest HEPAP
meeting
http://science.energy.gov/hep/hepap/meetings/20120827/

(apologies if I missed it)

28. **David Nataf**
September 5, 2012

Sakurai’s book is probably much too difficult for an introductory course.

29. **Peter Woit**
September 5, 2012

Shantanu,

Thanks for the link, I didn’t know about that. But, seems to be not much news. For one thing, US federal budget prospects are likely to be up in the air until after the election.

30. **Eli Rabett**
September 6, 2012

The tell on Feynman Volume III is that the first year students (knew some) were lost, but since then many senior (was one) and graduate level quantum courses start with it to introduce the subject. I have a friend who used it as his only text for studying for and passing qualifiers. There is no doubt that its approach provides a much more sophisticated appreciation than the usual grind it out of PDEs one.

That being said, there is a strong argument that linear algebra is a much more important course for physicists than diffeq (which mostly looks like a bag of tricks to physicists) and PDE (a bag of even more tedious tricks).

For what its worth, I took quantum as a senior using Feynman III and loved it. In grad school the quantum course was taught by a Schwinger student and I found it tedious, but understandable given the foundation I had.

31. **Mobius Trip**
September 6, 2012

What is a good free online reference for the class? When does the class start?

32. **Zen**
September 10, 2012

A LaTeX suggestion: when writing your brackets don’t use > and } % bra
\newcommand{\bra}[1]{\left< #1 \right|} % ket
\newcommand{\braket}[2]{\left} % braket

Some examples:

\ket{\Psi}
\bra{\Phi}
\braket{\Phi}{\Psi}

33. **Peter Woit**
September 11, 2012

Zen,

Thanks a lot for the suggestion, will try it out.

Proof of the abc Conjecture?

September 4, 2012
Categories: abc Conjecture

Jordan Ellenberg at Quomodocumque reports here on a potential breakthrough in number theory, a claimed proof of the abc conjecture by Shin Mochizuki. More than five years ago I wrote a posting with the same title, reporting on a talk by Lucien Szpiro claiming a proof of this conjecture (the proof soon was found to have a flaw). One change over the last five years is that now there are excellent Wikipedia articles about mathematically important questions like this conjecture, so you should consult the Wikipedia article for more details on the mathematics of the conjecture. To get some idea of the significance of this, that article quotes my colleague and next-door office neighbor Dorian Goldfeld describing the conjecture as "the most important unsolved problem in Diophantine analysis", i.e. for a very significant part of number theory.

Jordan is an expert of this kind of thing, and he has some of the best mathematicians in the world (Terry Tao, Brian Conrad and Noam Elkies) commenting, so his blog is the place to get the best possible idea of what is going on here. After consulting a couple experts, it looks like this is a very interesting and possibly earth-shattering moment for this field of mathematics. In the case of the Szpiro proof, the techniques he was using were relatively straightforward and well-understood, so experts very quickly could read through his proof and identify places there might be a problem. This is a very different situation. What Mochizuki is claiming is that he has a new set of techniques, which he calls "inter-universal geometry", generalizing the foundations of algebraic geometry in terms of schemes first envisioned by Grothendieck. In essence, he has created a new world of mathematical objects, and now claims that he understands them well enough to work with them consistently and show that their properties imply the abc conjecture.

What experts tell me is that, very much unlike the case of Szpiro's proof, here it may take a very long time to see if this is really a proof. They can't just rely on their familiarity with the usual scheme-theoretic world, but need to invest some serious time and effort into becoming familiar with Mochizuki's new world. Only then can they hope to see how his proof is supposed to work, and be able to check carefully that a proof is really there, not just a mirage. It's important to realize that this is being taken seriously because such experts have a high opinion of Mochizuki and his past work. If someone unknown were to write a similar paper, claiming to have solved one of the major open questions in mathematics, with an invention of a strange-sounding new world of mathematical objects, few if any experts would think it worth their time to figure out exactly what was going on, figuring instead this had to be a fantasy. Even with Mochizuki's high reputation, few were willing in the past to try and understand what he was doing, but the abc conjecture proof will now provide a major motivation.

Mochizuki has been at this for quite a while. See this page for some notes from him about how he has been pursuing this project in recent years. This page has notes from lectures he has given on the topic, starting in 2004 with A Brief Introduction to Inter-universal Geometry. For the proof itself, see here, but this is the fourth in a sequence of papers, so one probably needs to understand parts of the other three too.

Update: Barry Mazur has recently made available his 1995 expository article on the abc conjecture, entitled Questions about Number.

Comments

1. **Andrew Obus**
   September 4, 2012

   Safari doesn't seem to like your link to the paper…

2. **Peter Woit**
   September 4, 2012

   Not just Safari, my bad… Now fixed. Thanks Andrew!

3. **Michael Thaddeus**
   September 5, 2012

   A pity he's too old for the Fields Medal! If he were 36, there would be some pressure on the Fields committee to form an opinion of his work in the next two years…

4. **Hamish**
   September 6, 2012

   The abc conjecture…surely it's easy as 123, simple as doe rae mi?

5. **Marko Amnell**
   September 8, 2012

   There is some interesting discussion of this in response to a post by John Baez on Google+. See: http://tinyurl.com/cnh5gks
   Apparently, Mochizuki declined to come to New York to discuss his work, which is interesting but there could be any number of reasons for it.

6. **Peter Woit**
   September 8, 2012

   Marko,

   With this kind of announcement, I suspect Mochizuki has all of a sudden received a very large number of invitations to give talks. From what I hear, he's not someone who likes to travel, so he's probably now turning down lots of such invitations (including the New York one).

7. **Marko Amnell**
   September 12, 2012

   You might be right that Mochizuki's dislike of travelling could have something to do with his rejection of the invitation, but I suspect another reason may be that (as his friend Minhyong Kim says) Mochizuki is in all likelihood the only person in the world who is familiar with the concepts and ideas in the purported proof of the ABC Conjecture. A talk at this point would be premature because the audience could not understand what he was saying! In a new post, John Baez provides some comments by Minhyong Kim which I would recommend to anyone interested in this subject. Here is part of what Minhyong Kim said: "How long it will take for people to evaluate the work, it's hard to say, possibly even a year or so. Among other difficulties, his work probes the very core of mathematical language such as what we might really mean by a number or a geometric figure, and how they might be interpreted in a manner quite different from usual conventions. In fact, it relies on deep relations of a geometric nature between such varying interpretations. Such questions have occupied philosophers for millennia, but are usually quite distant from the consciousness of modern mathematicians. But then, these seemingly philosophical questions have to be recast in the robust language of precise mathematics. You have to add to that some of the most sophisticated portions of 21st century arithmetic geometry. At the moment, I can fairly safely say that there is no one but the author who is familiar with all these things. Possibly his colleague Akio Tamagawa.” https://plus.google.com/117663015413546257905/posts/d1RsN4KnCUs#117663015413546257905/posts/d1RsN4KnCUs

8. **Daniel Hill**
September 17, 2012

Do we know whether Mochizuki is claiming the strong or the weak version of the conjecture?

9. **Marko Amnell**
September 18, 2012

In IUTT-IV Mochizuki writes, “In the present paper, estimates […] are applied to verify various diophantine results which imply, for instance, the so-called Vojta Conjecture for hyperbolic curves, the ABC Conjecture, and the Szpiro Conjecture for elliptic curves.” The Szpiro Conjecture is, in fact, equivalent to the strong ABC Conjecture. [cf. Bombieri-Gubler, Heights in Diophantine Geometry p. 431]

10. **Marko Amnell**
September 18, 2012

In his analysis of Mochizuki’s articles on MathOverflow, Vesselin Dimitrov says that the key inequality stated in Section 2 of IUTT-IV is “asserted up to finitely many exceptions.” Dimitrov also says that “Mochizuki’s approach […] is entirely direct, and, consequently, effective.” http://mathoverflow.net/questions/106560/what-is-the-underlying-vision-that-mochizuki-pursued-when-trying-to-prove-the-abc/106658#106658
But from what I can gather from other comments by experts, the question of which version of the ABC Conjecture Mochizuki claims to prove, and whether the claimed result is effective or not, are open questions at this point.

11. **Vesselin Dimitrov**
September 19, 2012

Mochizuki claims the strongest version of ABC that one could think of. (In particular, the effective one, and with the exponent 1 + epsilon). See Theorem A on p. 3 of his fourth paper (ABC with exponent 1+epsilon is a standard consequence of this).

As for an explicit effective statement, take a look at the inequality asserted on page 23. It concerns Szpiro’s inequality 1/6 log(D) < (1+epsilon) log(N) + Const. for the minimal discriminant D and conductor E of a (semistable) elliptic curve E. Here, log q on the left-hand side is precisely log(D). The f on the right-hand side is our conductor N, and the other term is a constant since we are concentrating on the single number field Q. In section 2, the full ABC conjecture is deduced, in an effective manner (by the paper [GenEll]), from this effective (Szpiro) inequality.

12. **Marko Amnell**
September 19, 2012

Vesselin,

Thanks very much for clarifying those points of your analysis. And sorry if the snippets I quoted from you may have misrepresented what you said. I found your analysis of Mochizuki’s articles on MathOverflow very interesting and helpful.

13. **Nick Nazari**
September 20, 2012

If Mochizuki theory is proven right then will it also bring another proof of FLT? It is very interesting to see how long will take to confirm or infirm this new mathematics. Taking into account that in the past Mochizuki proved other deep theorems there is a great hope for a major math revolution.

14. **Vesselin Dimitrov**
September 21, 2012

@Nick Nazari: If Mochizuki’s work is correct (and this is a pretty big “If”…), it would certainly yield a new proof of FLT.

This Week’s Hype

September 5, 2012
Categories: This Week's Hype

BBC Horizon this week is running an episode How Small is the Universe? with a description that features the usual sort of hype about modern physics:

> It is a journey where things don’t just become smaller but also a whole lot weirder. Scientists hope to catch a glimpse of miniature black holes, multiple dimensions and even parallel Universes. As they start to explore this wonderland, where nothing is quite what it seems, they may have to rewrite the fundamental laws of time and space.

Access to the video is restricted to IPs in the UK, so I can’t watch the thing, and should avoid being too critical. One of the two clips though advertises The landscape of String Theory and somehow I doubt that the clip explains why this is pseudo-science. Associated with the show is this article by Andy Parker of ATLAS, which gives the idea that ATLAS is looking for strings:

> Strings can vibrate, and this allows us to explain all of the strange fundamental particles which we see as different vibrations of the strings – different notes from a cosmic violin.
>
> So far, so simple – but to explain the particles we know about, the strings have to vibrate in lots of different ways.
>
> Superstring Theory allows them to vibrate in a bizarre space with 11 dimensions – up, down, sideways, “crossways” and 7 other ways!
>
> Experiments at the LHC are looking for evidence that you can move “crossways”. If we can, there could be whole universes, as big and marvellous as our own, sitting just down the road “crossways”.

No mention is made of the fact that the LHC has seen zero evidence for any such thing, or that few if any physicists ever thought there was any real chance it would.

The other experiment invoked is the MAGIC gamma ray telescope, presumably in the context of the search for Lorentz-violating dispersion of gamma rays from gamma ray bursters. This was discussed in an edition of This Week’s Hype from five year’s ago, which featured a Slashdot report that Gamma Ray Anomaly Could Test String Theory. At Scientific American, the story was Hints of a breakdown of relativity theory?, which was about this paper, and contained the news:

> Another co-author, string theorist Dimitri Nanopoulos of Texas A&M, writes to me: “I am very excited about this, because as you know we suggested this effect about ten years ago and we have follow through with several analyses and/or improvement on theory. Notice that the 0.4 x 10^{18} GeV is the typical string scale!!!!”

Since 2007 there have been a series of much more sensitive results from Fermi ruling out the quantum gravity interpretation of the MAGIC observations (see e.g. here, here, here and here.)

Since I can’t watch the video, I don’t know what the BBC has to say about MAGIC’s results, in particular whether the show explains the story of the 2007 claims and how they were later shown not to have anything to do with space-time structure by the newer Fermi observations.

Update: I did just get a chance to watch the program. It was very well made, with the first half quite interesting, featuring the LHC and some atomic-physics scale experiments I would have loved to hear more about. About half-way through though, it started to go off the rails, with the usual kinds of problems. The extra dimensions at the LHC stuff made no mention of the fact that even string theorists see no good reason for them to show up at this scale, and the results to date confirm this. The Mike Green segment was pretty much pure string theory/multiverse hype. Reference to the “mind-boggling predictions” of string theory misses the main problem, that there are no predictions. In particular, no predictions about the gamma-ray dispersion MAGIC is looking for, which ended the show. The 5 second discrepancy described at the end in MAGIC 2005 observations I suspect has been shown to not be plausibly due to such dispersion by later Fermi results which went unmentioned.

Comments

1. **Agreed**
   September 5, 2012

   Darn, when will these guys finally learn to NOT mention any tabood s-words in public ! Just stop talking to any science journalists, in publicly available videos, on TV, in newspapers, etc about such things and the world, in particular the blogosphere, will be much more peaceful and constructive …

2. **Shantanu**
   September 6, 2012

   I thought these Magic/Fermi results about searches for violation of lorentz invariance etc test loop quantum gravity and related models. I didn’t know they are a test of string theory.

3. **Ash**
   September 6, 2012

   I live in the UK and follow “Not even wrong” I also watched the program. The BBC does try to look at all angles, the program by enlarge is looking at the topic as a whole and was overall quite good. I would recommend trying to obtain a copy of the program somehow and bear in mind the program is televised on a nation channel and therefore has to be accessible. I would need to watch it again to pass any real comment as I often ignore/block out conjuncture on string theory.

4. **Matt Leifer**
   September 6, 2012

   Overall the programme was OK, but there was a distinct failure to separate solid science from speculation, as is often the case. There was no dissenting voice on the possibility of mini-black holes at the LHC, the string theory landscape was treated credulously and there was no mention Fermi in the context of the MAGIC results.

5. **Mike Mathison**
   September 6, 2012

   I watched the Horizon programme yesterday. The usual science-doc guff of louche theorists sipping coffee in agreeable European locations, but tempered by a refreshing emphasis on experimental verification with a lot of footage of serious looking hardware, and even people actually measuring things. The theorists were given a generous platform, but weren’t treated with complete reverence.

Here's some of the commentary on string theory (your favourite, I know): "*It's a beautifully neat idea […] There are, however, one or two problems […] These strings are so small that no one has ever seen anything remotely stringy*." On the landscape: "*If they could find the right solution [ironic pause] the right one out of one followed by five hundred zeros [another ironic pause] we'd have a neat explanation for everything in our universe*." Summing up string theory: "*For now, string theory remains a theory, with no experimental evidence for any of its mind-boggling predictions […] To stand a chance of seeing strings we'd need a particle accelerator one million billion times bigger than the LHC*" (at this point the camera sweeps away into footage of distant cable cars as we leave Michael Green, apparently stranded in some rarefied cloud-shrouded alpine idly, like some well meaning guru condemned endlessly to shuffle the arcane symbols of his beautiful but otherwise utterly unworldly theory).
Lots of stuff on MAGIC in the context of the QG segment – no mention of other (Fermi) measurements. To be as generous to the programme makers as possible you could say they were using MAGIC as an *illustration* of how to indirectly test theories (QG in this case) that operate on scales so stupefyingly small they could never be tested directly. As an illustration of the *ideas* involved I think this worked rather well, and kept in touch with their emphasis on experiment and testing… but it's a shame they didn't mention what the actual observation (now) appears to be!

6. **jg**
September 6, 2012

The comments above are accurate, I also watched the programme on Monday night and thought it was a reasonably accurate popularisation (*"…for now, String Theory remains a theory, with no experimental evidence for any of its mind-boggling predictions"*). But as others have mentioned, no recent results challenging the original 2005 MAGIC results were presented, (I was sufficiently confused that I had to go check references afterwards regarding gamma ray dispersion results)

The best bits were the sequences showing the lab hardware, especially Thorsten Schmidt's X-Ray beam apparatus to split an electron into orbitrons and spinons (quasiparticles).

7. **BJM**
September 6, 2012

@Mike Mathison
In your transcript of the program:
"For now, string theory remains a theory, with no experimental evidence for any of its mind-boggling predictions…"

Are you sure the comma after "theory" belongs there? It makes a big difference in the meaning. With the comma, the entire concept of the term theory is demeaned as something without evidence (a widely held popular perception).

8. **Cormac McGuinness**
September 6, 2012

Damn! Only watched it for three minutes and then switched away – would have enjoyed seeing the Swiss Light Source and my colleague Thorsten at a beamline that I have worked at. Could not have expected that the programmers would juxtapose orbitons observed via RIXS (resonant inelastic x-ray scattering) and the non-predictions of the string theory landscape.

9. **jg**
September 6, 2012

Yes, the programme was a bit of a mishmash, along with some cool hardware they had Michael Green going on about how the tiniest particle could also be the whole universe (I guess he was referrring to dualities), and a rock-music sequence with Dr Giovanni Amelino-Camelia riding a moped through Rome and then talking gibberish about space-time (*"what is space-time, you know? [etc]…, you see what I'm trying to say? It is very tricky."*)

Schmidt's experiment is shown from ~22:00 onwards (I mistakenly wrote 'orbitrons' above it, it should be 'orbiton' as Cormac wrote)

10. **Peter Woit**
September 6, 2012

I did get to watch the program, and just added something to the blog about it. The first half was very good, and I would have loved to hear more about the experiment Cormac mentions, instead of the "trip down the rabbit hole" that followed.

As Shantanu mentions, the Lorentz symmetry violations MAGIC is looking for are something some string theorists (e.g. Nanopoulos) claim as evidence for string theory, others claim string theory predicts no such violations. Same with pretty much everything else (another good example is variation of fundamental constants). Since the program featured string theory and MAGIC, it would have been a good idea to address this issue.

11. **Mike Mathison**
September 6, 2012

@BMJ – I put in the comma to try to reflect the delivery of the spoken commentary – and listening to it again I'd also say there's some pronounced emphasis on the second occurrence of theory: "*[…] but for now [slight pause] string theory remains a *theory* [slight pause] with no experimental evidence for any of its mind-boggling predictions […]*." Obviously I can't speak for the intended meaning of the writers of the commentary, or for the person who delivered it. The sentence in question begins around 40 minutes and 7 seconds.

12. **Cormac McGuinness**
September 6, 2012

Peter: If you are interested in the experiments on spin-orbital separation mentioned above (and in the program), concerning the fractionalisation of the electron into distinct quasiparticles, then the relevant paper was published in Nature in April. The observation of orbitons in Sr_2CuO_3 was obtained via RIXS measurements by Schlappa et al (i.e. Thorsten Schmitt and co-workers).
See the nature article, and even the supplementary data which has some of the more interesting information for the specialist.

For those not in the field then the BBC news report can be read instead.

13. **lun**
September 6, 2012

Is the Wang particle part of the hype?

14. **Peter Woit**
September 6, 2012

lun,

Looks suspiciously like the “Wang particle” is its very own form of hype, but I know nothing about it.

15. **lun**
September 20, 2012

In related news, apparently the multiverse killed god

Langlands and Twistors?

September 6, 2012
Categories: Langlands

I just heard about this from George Sparling, who is giving a talk this afternoon with the title “From Roger Penrose to Robert Langlands and back” at a symposium in Pittsburgh. I don’t at all know what this is about, but am posting this quickly because he tells me there may be a live stream of his talk at 5pm today, available at

http://live.twistor.org

Evidently yesterday and today at Pittsburgh there’s a symposium on “Towards the Unification of Mathematics and Physics”, with the following schedule:

4pm Wednesday: Mellon Professor Thomas Hales, “The present status of the Langlands Conjecture”.
5pm Wednesday: Jonathan Holland, “Parabolic geometry”.

4pm Thursday: Tim Adamo, University of Oxford, “Twistor-String theory and gravity”
5pm Thursday: George Sparling, “From Roger Penrose to Robert Langlands and back.”

Last week there was a workshop at Banff on The Geometry of Scattering Amplitudes, with videos now available. I’ll try soon to take a look at some of them, Sparling’s may give an idea of what he is up to.

And, for something completely different, see this interview with Edward Frenkel about the Langlands Program at the Fields Institute blog for this fall’s Fields Medal symposium.

Update: There’s a video of the Sparling talk available here.

Comments

1. **Richard Cerezo**
   September 6, 2012

   Thanks for the mention! I hope to interview a number of other mathematicians regarding the Langlands program. If you have any ideas or are willing to do a phone interview with me, that would be great! Please shoot me an email.

   Sincerely,
   Richard

2. **Peter Woit**
   September 6, 2012

   Hi Richard,

   You’ve got a long list of distinguished speakers for the symposium (I’m hoping to come up to Toronto to hear some of them), I’ll send you by e-mail some suggestions about interviewees, many of them would be great choices. As for me, anyone interested in my opinions can get far too much of them already at this site….

3. **Jack**
   September 6, 2012

   Do you know if this conference will be posted on other websites as well? I have a slower internet connection and the stream on Twistor in loading for more then 10 minutes and nothing happens.

   Thanks,
   Jack

4. **Peter Woit**
   September 6, 2012

   Jack,
   It doesn’t look so far (5:02 pm) like there will be video of the talk. I don’t have any other information about the talks at this symposium. One reason for the posting was that I’d like to hear from anyone else who does.

5. **Peter Woit**
   September 6, 2012

   As of 5:15, video of Sparling’s talk is streaming, a bit shakily…

European Strategy Group Meeting

September 10, 2012
Categories: Experimental HEP News

CERN has a new version of the European Strategy Group (last convened in 2005/6), tasked with updating medium to long-term plans for future accelerators and particle physics in general. This week they're running an Open Symposium (live webcast here), with presentations covering a wide array of topics from the state of speculative ideas about BSM physics to possible new accelerator technologies.

While the presentations themselves often focus on the really interesting question of how to learn more about the Higgs and electroweak symmetry breaking, media articles based on reporting from the conference have started to appear, often featuring the usual nonsense. See for example this piece, from the Sunday Times, which tells us that:

> Such a machine might help resolve some of the questions raised by Albert Einstein, who could not reconcile the forces operating at the level of atoms with the force of gravity, which governs the movement of stars and planets.

which then gets picked up by the Daily Mail and turned into a story about how CERN reveals plans for new experiments measuring 50 miles in length to solve the mystery of how gravity works, which explains:

> The collider will be used to solve a new batch of mysteries of the universe, such as how gravity interacts on a molecular level.

Maybe the European Strategy Group could as part of its deliberations develop a strategy for stopping physicists from going to the press with nonsense about quantum gravity….

Comments

1. **Bee**
   September 10, 2012

   Interesting that one needs a 50 mile collider to study the behavior of molecules.

2. **fuzzy**
   September 10, 2012

   I note that the Chair of SPC is a respected theorist. I see from INSPIRE that he obtained a lot of results on supersymmetry, supergravity, strings and such. I am not sure we can call this physics, but I guess that if he is the Chair, many people will be lead to consider extra dimensions, supersymmetry, etc, as part of high energy physics. So I am not surprised that a journalist will not express a critical view about that, rather, he reflects the views of the community.

3. **Mattias Dahl**
   September 10, 2012

   http://www.youtube.com/watch?v=3wHKBavY_h8

4. **Peter Woit**
   September 10, 2012

   Mattias,

   That's pretty funny, hadn't seen it.

5. **Hamish**
   September 12, 2012

   Bee, a few generations ago one would be forgiven for asking why a large circular accelerator would be needed to study molecules — now we build them just for that purpose! Big toys arn't just for particle physicists anymore!

6. **paddy**
   September 12, 2012

   I shan't say anything about the provincialism implied by the name "European Strategy Group Meeting". Oops..I did.

7. **Bobito**
   September 13, 2012

   Whatever the merits of a particular kind of science, the last thing any of us needs is some "European Strategy Group" stopping it from being published.

8. **Jeff Moreland**
   September 13, 2012

   Paddy & Bobito,
   All 20 member states of CERN are European, and virtually all the people they employ and the money they spend is European. I don't think they are trying to control or censor the rest of the world!

9. **epp2010**
   September 13, 2012

   Does anybody remember the EPP2010 report from a few years ago? (Jonathan Bagger chairman.) PW probably has a post about it smoewhere. (More than one?) The official purpose was to set a strategy for the USHEP experimental program for the next decade (or more). The whole report, when it came out, was explicitly to "restore US leadership in particle physics". Provincialism? The report was so outrageous it caused a public outcry, even within the US. It was promptly hushed up.

10. **Claude Leblanc**
    September 16, 2012

Peter,

the slides are now online. Did you have a look at the ones by Alvarez-Gaumé or the ones of the last section, which summarize the various working groups? This must have been one of the most unproductive strategy meeting in the last 100 years. The theory strategy is particularly disappointing. Basically, they say: "we have no clue what to do, we need more young people to join us".

I have often experienced how managers who did not know what to do changed the situation: they organized a strategy workshop, structured it well, asked the right questions in the right sequence, and then forged a coherent strategy supported by everyone. But this meeting obviously failed completely.

The theory people have not yet digested the failure of supersymmetry. Instead of looking for alternatives in a systematic manner, they still seem to be depressed. After the leadership they provided for decades, when they successfully induced experimentalists to build the LEP, the Tevatron, the LHC, they are now lost. The main statement is: let's wait for the LHC 2013-14 results. Wow. What a great strategy!

What we need here, above all, is a theory strategy. A theory strategy were theorists sit down, evaluate all possible options, and then explore the most promising ones. Instead, the last slide of the theorists says: "The TH community is useful and healthy, we should keep it that way" Given that it is a community of thousands of well-educated and well-funded researchers but that this community did not produce almost any correct idea in the last 40 years, there is some humor in the statement.

"Useful and healthy"? No, it's "useless and depressed."

11. **M**
September 16, 2012

From a mathematical point of view High Energy Physics is a ordered field. There is only one direction: towards higher energy.
So these strategy groups mostly have a political purpose

12. **Bobito**
September 16, 2012

@Jeff: I don't think you caught the sense of my comment. In any case, I live and work in one of the 20 member states.

13. **leadersheep**
September 16, 2012

"After the leadership they provided for decades, when they successfully induced experimentalists to build the LEP, the Tevatron, the LHC, they are now lost."

The HEP expt side will doubtless offer a very different point of view about this. In the 1950-60s the accelerators were producing copious new physics, all of which cried out for an explanation. It was the plentiful and unexplained data which drove the theoretical innovation. Many of the new particles being discovered fitted into patterns (mesons and baryons, octets and decuplets), the theorists came up with SU(3) and the Eight-fold way, and later with quarks. (The peculiar decay of the phi meson? It led Zweig to his model of aces.) At the same time there was data on the weak interactions. This led to ideas of weak neutral currents. And of course to W and Z bosons. And also of symmetry breaking. The absence of flavor-changing neutral currents led to a suggestion of charm. And CP violation was totally unexpected.

"… successfully induced experimentalists to build the LEP, the Tevatron, the LHC, they are now lost." All of these machines (and others SPEAR, CESR, DORIS, PEP, PETRA, TRISTAN, VEPP-2, VEPP-4, SPS, ADONE … others … HERA?) were all e+e- colliders or hadron colliders/fixed target rings, all pushing the energy frontier in their own way (or serving as spectroscopes for quarkonium resonances).

But since the Standard Model was put together circa 1974-75, all new particles which have been found have all fitted into the SM (b-quark, top quark, Higgs). For forty years or so, particle accelerators have NOT produced copious new particles which lack a new theoretical explanation = BSM physics. The new frontier seems to be dark matter and dark energy, and there is no clear way how to detect or quantify that.

"The theory people have not yet digested the failure of supersymmetry. Instead of looking for alternatives in a systematic manner …" How would the HEP theorists propose to modify the design of the ILC/muon collider/super-B/super-LHC/you name it/ to look for alternatives in a systematic manner? All of LEP, Tevatron, LHC, etc were all e+e- or hadron colliders. The HEP theory community did not provide any technical ideas for new accelerator technology.

There simply has to be new data = new particles, or new symmtery violations (super-CP-violation, whatever that might mean…). For example, perhaps the Higgs does not decay as expected and the branching ratios disagree significantly with SM predictions. Or it decays via some channel which should be forbidden. New resonances appear = "bump hunting" of the 1950s, which do not fit the SM. Then all of that will give the HEP theorists something concrete to work on, to suggest hypotheses to test at proposed new machines. But the new machnes will almost certainly still be e+e- or mu+mu- or hadron colliders.

14. **Claude Leblanc**
September 16, 2012

To leadersheep

It might well be that you are right, and theoreticians have not provided any leadership at all.

But then the situation is even worse now. Because there is no reason at all for a larger machine; as long as there is no effect beyond the standard model, there is no reason to build a larger machine.

Maybe one has to harsher and say that nobody knows what to do. A really bad situation in a time where there are more particle physicists than at any previous time in history. So many smart men, and still they have no clue…

15. **leadersheep**
September 16, 2012

What leadership could the HEP theory community offer? Let's consider LEP. It is widely acknowledged that LEP was extremely successful. But LEP produced no BSM physics. What leadership or guidance could the HEP theorists offer to do things differently? LEP was an e+e- collider (lepton). LHC is a pp collider (hadron) in the same tunnel. What else to do? Maybe p-pbar. Is that really an innovative way to search for alternatives to supersymmetry?

After the b quark was discovered at Fermilab in 1977 (in a fixed target expt, not the Tevatron, which was not operational in 1977), that is to say, after the discovered Upsilon particle was realized to be a bound state of a new species of quark with charge -1/3, then the theorists immediately said there is a partner t-quark, with a charge of +2/3. So there was a theoretical impetus to build machines to find the t quark. PEP, PETRA and TRISTAN were all built to find the top quark, and they all failed to do so. Was it wise to build them? PETRA discovered gluons via 3-jet events, also PETRA observed QCD deviations from Bjorken scaling, one of the earliest validations of QCD. But that is not why PETRA was built. Since the beginning of QM, the theorists have never had any good idea at what ebergy scale the next "new physics" will appear. Most "new physics" has been a surprise.

One can always stop building machines because the theorists have no ideas for what comes next (BSM n the case of HEP). By that logic Kammerlingh Onnes should not have measured electrical resistance at liquid He temperatures. Kammerlingh Onnes was actually trying to verify Ohm's law at low temperatures. That's as boring as boring can be. Which theorist would propose building a cryostat to do that? Would you jump at the opportunity to do a PhD thesis to validate Ohm's Law to (almost) absolute zero?

16. **no clue**
September 16, 2012

"Maybe one has to harsher and say that nobody knows what to do. … So many smart men, and still they have no clue…"

I suppose Helen Quinn and/or Lisa Randall might object to that, but never mind. When Dirac discovered the Dirac equation, he had no clue about antimatter. Carl Anderson discovered the positron completely independently of Dirac. When Yukawa proposed the meson (U particle) and a particle of the approximate mass was found by Anderson and Nedermeyer, nobody recognized it as a lepton not a meson. After WW2, cosmic ray expts produced strange V particles. Accelerators could produce such particles more copiously and reliably, and that was a big reason to build accelerators after WW2. But nobody expected the huge number of resonances which appeared. When the AGS strong focusing proton synchrotron was built in 1960, there was no theorist who said it would discover CP violation. Nor did anyone say the AGS would produce the J half of the J/psi. Indeed in 1960 no one even spoke of quarks. The AGS and the CERN PS were built because the strong focusing principle was a new idea which allowed accelerators to reach much higher energies, and yet have a reasonable size. But in all cases, there was plenty of unexplained physics lying around, and good reasons to build accelerators of higher energy.

Most of the famous accelerators became famous for discovering physics completely different from the reasons why they were proposed. Bevatron, SPEAR, AGS … The SPS at CERN was built as a fixed target synchrotron. It was never intended to operate as a p-bar collider. No HEP theorist said that one should build a p-pbar collider. Carlo Rubbia said that (actually, to convert an existing fixed target proton synchrotron into a p-pbar collider).

17. **Peter Woit**
September 16, 2012

epp2010,

I did write about EPP2010, see for instance
http://www.math.columbia.edu/~woit/wordpress/?p=382
where I wrote

"The committee did a very good job of recognizing the difficult situation of US HEP, and coming up with a plausible strategy for how to make the best of it. I have my doubts about whether it's really a good idea to sell this as "Revealing the Hidden Nature of Space and Time", since it's not especially likely that that is what is going to happen. There's no particularly good reason to believe that extra dimensions will show up at the LHC or ILC energy scales, so over-selling this is dangerous."

In retrospect, I think emphasizing the ILC was a mistake. One of the main arguments for the ILC was always "we think the LHC may see lots of superpartners below 1 TeV, the ILC would be then the best way to study them, so we should have a design ready to go when LHC results come in". The problem is that the superpartners were always very unlikely, so even back then you knew that the most likely situation you would be in once LHC results came in was the one we're in now: no BSM physics at ILC scales. The ILC thus shouldn't have played such a prominent role in the EPP2010 proposals. Other than that though, US HEP then (and now) is in a difficult situation, with no obvious great options, and I think those working on this played as well as they could the bad hand they were dealt by nature and fiscal reality.

Claude Leblanc,

I did look at the slides. It's not surprising that Alvarez-Gaume thought this was not the time or place to admit failures, but tried to make the case for future support. I note that the words "string theory" don't appear in his slides: the decision seemed to be that it was best to not bring that up in this venue. I hope the theory community is finally getting around to thinking through the implications of the failure of string theory unification (which has been clear for a long time), as well as the failure of SUSY extensions of the SM (which is becoming definitive as LHC results come in). Any useful debates about this though are likely to take place just among theorists, not when they're facing the outside world to ask for future financial support.

18. **chris**
September 17, 2012

well, let's face it: classical, accelerator based particle physics is coming to an end. the LHC will be productive for at least another decade after which some next gen collider will clean up the few remaining pieces and that will be about it.

time for theorists to reorient themselves or concentrate on teaching if they prefer that.

19. **David Nataf**
September 17, 2012

Chris,

What is the basis for your claim that accelerator physics is over?

It's not even clear that the Higgs cross sections match SM-predictions.

20. **fuzzy**
September 20, 2012

Hi David, just wanted to try to formulate my reaction to your question in words. I think that if it some brilliant theorist had predicted a big deviation of the Higgs branching ratio into 2 photons, the experimental hint to which you allude could have been interesting. Now as now, if we want to act as scientists, we should just say that the search conducted using the standard model as a guide was successful. Of course all of us is interested to see the next data, but I would dare to say that extra dimensions, quantum gravity, Lorentz violation etc, seem to be more in the pages of Nature, than in the Nature.

PPAP Community Meeting

September 18, 2012
Categories: Uncategorized

Following up on last week's European Strategy Group Meeting in Krakow, this week UK particle physicists are doing something similar, with a Particle Physics Advisory Panel community meeting in Birmingham.

The talks on the experimental side tell much the same story as the Krakow talks. On the theory side, the UK meeting has more, with a phenomenology talk which discusses prospects for "Saving SUSY" while noting that:

> It's ironic that the solution to the absence of SUSY is to add even more stuff: composite 3rd generation or Higgs, R-parity violating couplings, scalar gluons, or new singlets.

There's extensive discussion of UK particle theory funding here. I don't understand very well how particle theory is funded in the UK, but was interested to note that the slide on page 5 has string theory's piece of the pie stable at 27% last year (also 27% in 2008, 28% in 2005). Mike Duff (see commentary here) wrote a piece for the forthcoming *40 Years of String Theory* volume arguing that the 2006 books by Lee Smolin and me were responsible for destroying funding of string theory in the UK, but the numbers in this new talk don't seem to bear this out. I gather there's a separate story about the EPSRC and mathematical physics, and curious to hear from anyone who knows more about that. But if the state of affairs is that the mathematical end of string theory is being defunded while the phenomenological end is going strong, that can't be because someone in authority read my book…

Update: The US has its own version of this planned to take place over the next year, Snowmass 2013, starting with a meeting next month at Fermilab. It looks like this will be purely about planning on the experimental side, with the problems of particle theory not on the agenda.

Comments

1. **Shantanu**
   September 18, 2012

   Peter, OT to this thread.
   if you attend this talk
   http://www.columbia.edu/cu/physics/pdf-files/Colloquium2012Anderson.pdf
   at Columbia,
   could you provide a report?
   Thanks
   shantanu

2. **Peter Woit**
   September 18, 2012

   Shantanu,
   I was planning on attending that talk and writing about it. Unfortunately I just heard the disappointing news from Anderson himself that he's likely to not be able to make the trip here next Monday because of health reasons. Hopefully he'll make it here for a talk sooner or later. If so I'll be sure to attend and report on it.

3. **piscator**
   September 19, 2012

   Note that the combinations of `Strings' and `QFT' though has gone from 43% to 32%. This is probably a better reading of the figures given that many quantum field theorists may now be labelled as string theorists due to working on AdS/CFT and related topics.

4. **Peter Woit**
   September 19, 2012

   piscator,

   That may be true. Probably the most remarkable thing about that pie chart was "QFT" going form 15% to 5%. If your interpretation is right, it reflects a collapse in non-AdS/CFT research into QFT, with AdS/CFT now the only approach to formal QFT research that can get any support.

5. **P**
   September 19, 2012

   Peter and piscator,

   Agreed that it's strange to see QFT taking a hit, and maybe it's a labeling issue where people doing research on AdS/CFT and related topics are lumped into strings rather than QFT.

   But with a few hot new QFT subfields in the last few years, shouldn't we expect it to go up? e.g. amplitudes, Gaiotto (2,0) theories, AGT, 3d-3d correspondences, others, etc etc . . . Though certainly motivated by stringy ideas, these ideas really are in QFT proper. Maybe the decline in QFT funding is a reflection of the fact that not quite as much of this new work has been done by Brits?

   Cheers,
   P

6. **Mitchell Porter**
   September 20, 2012

   "Since the discovery of gauge/gravity duality and other developments, it has become clear that QFT and string theory/M-theory are NOT distinct subjects. String theory and M-theory can be viewed as important components of the logical completion of quantum field theory… String theory grew out of the S-matrix theory program, which was popular in the 1960s. For many years string theory appeared to be a radical alternative to quantum field theory. It is now clear that string theory and M-theory are not radical at all. In fact, they are the most conservative and inevitable ways in which to formulate quantum theories of gravity." — John Schwarz, Strings 2012

7. **P**
   September 20, 2012

   Mitchell,

   Thanks for the quote, I hadn't heard that one! He definitely has good perspective, and is one of the people best qualified to comment on the progress of string theory over the last 40 years. Strings and QFT do go hand in hand, but I'm curious about the use of the word "inevitable" in relation to strings and quantum gravity. It seems to imply QG –> string, rather than the other way around.

   Cheers,
   P

8. **Peter Woit**
   September 20, 2012

   Mitchell Porter and P.,

   Can we please stick to informed discussion about the topic of the posting, and what is going on in the UK? I don't see how anyone here can conceivably learn anything of any interest from discussing this particular piece of string theory hype.

9. **P**
   September 20, 2012

   Peter,

   The part that was hype I effectively labeled as such. The rest is not.

   Besides, it followed from a thread of discussion brought up by Piscator and followed up by you about AdS/CFT research being groupable into either QFT or Strings. Mitchell then brought up a quote along those lines from a renowned physicist, which was also relevant to the thread of discussion, and then I made a comment on said quote and also a criticism. Not sure when the discussion you yourself were a part of went too far afield . . .

   Not to mention, I did comment directly on the British QFT situation and the labelling issue, and it went unanswered.

   Cheers,
   P

10. **Peter Woit**
    September 20, 2012

    P,

    I don't know the answer to your question about British QFT, and would love to hear from someone who does. I'm pretty sure though that allowing this comment thread to degenerate into yet another discussion of string theory hype (e.g. string theory as "the logical completion of quantum field theory") will ensure this doesn't happen. Enough.

11. **P**
    September 21, 2012

    Peter,

    Just take note that I did not say that, and even criticized that sentiment.

    It seems that this thread has died. But since you'd like to hear a statement about British QFT and no one else has said anything, I'll just add the comment that I can only think of a handful of people who have works on the formal things I've mentioned – Hanany and collaborators, Bullimore and collaborators (when he was in Britain), and maybe a few other people at Oxford. Much of the action has come from North America and a handful of very good people in Japan, and this observation might be in correspondence with the data in the pie chart.

    Cheers,
    P

12. **piscator**
    September 21, 2012

    Some quick comments about how UK research is funded. The main grant system (which the slides refer to) are group grants often called Rolling Grants. They are awarded to particle theory groups as a whole and provide some number of postdocs to the group as a whole. So this means that each group – who may cover a spectrum from QCD to formal string theory – submits a single application, and gets a single award.

    Individual groups then use these grants to hire postdocs, normally collectively. The piechart describes the slicing of postdocs by subject area. There are probably O(30) STFC-supported postdocs at any one time so there is some element of noise.

    As far as I am aware this is NOT like the US system where faculty members apply for individual grants which fund their individual postdocs. Some of these exist – e.g. ERC grants – but the slides are about STFC and so do not refer to those.

    The main areas of `pure QFT' research in the UK I would say have tended to relate to integrability, strong coupling, solitons. Many of these areas are now either subsumed into or closely linked to AdS/CFT or D-brane physics. As P points out, there is not much UK work on e.g. Gaiotto-esque stuff.

    The 5% figure doesn't strike me as obviously low, QFT that could not be classified as either pheno or strings or lattice isn't that big an area.

13. **John**
    September 21, 2012

    Peter,

Check out this nifty-looking website:

http://whystringtheory.com/

14. **Peter Woit**
September 21, 2012

John,

One of the students responsible for that site wrote to tell me about it a while back. Was going to mention it here at some point, but I don't have much to say about it. It just repeats the same promotional case for string theory that has been endlessly made for nearly 30 years now, ignoring why the ideas being promoted don't work. It's very similar to the superstringtheory.com site that Patricia Schwarz set up way back when, but has been abandoned for about 10 years. Like that site, the material about string theory pretty much stops about 15 years ago, with AdS/CFT.

It's interesting that people at Oxford thought this was something that the world needs. Maybe one motivation for its creation was concerns about future funding for string theory in the UK. I have no idea whether this sort of things makes a positive impression on whoever is making those funding decisions.

15. **Peter Orland**
September 21, 2012

piscator says:
The 5% figure doesn't strike me as obviously low, QFT that could not be classified as either pheno or strings or lattice isn't that big an area.

In terms of the number of people working on it, absolutely true. Maybe this is good for those of us in the 5% (not much competition) or sad (not much interest of colleagues).

16. **Joseph Conlon**
September 21, 2012

Peter: the motivation for its creation was the belief that the general public – who fund scientific research – are entitled to accurate and accessible information on why their money is spent on string theory, what string theory is, what it has achieved and what it hopes to achieve.

I have tried to ensure that all statements on the site are accurate and fair – identifications of erroneous claims are welcome and they will be corrected.

17. **Peter Woit**
September 22, 2012

Joe,

With this kind of site, you're not providing people a product for their money, you're providing advertising to encourage them to give you more money, which is something rather different.

18. **chris**
September 24, 2012

It is not true that Snowmass is purely experimental. It just so happens, that theory is not a separate "frontier" but rather part of some of them. I find this rather more convincing than the European approach where theory is split off from the major developments and seems to exist more as an appendix to the true direction the field is going.

19. **jg**
September 24, 2012

Peter,

Slightly off-topic, but you might like to know that in the recently published 2012 Review of Particle Physics, the chapter on 'Experimental Tests of Gravitational Theory' has an interesting edit. In the introductory section the reference to a the theoretical spin-2 field origin of gravity has been removed as have the following sentences at the end of the introduction:

Quantizing the gravitational field itself poses a very difficult challenge because of the perturbative non-renormalizability of Einstein's Lagrangian. Superstring theory offers a promising avenue toward solving this challenge.

The 2010 version is available here and the 2012 version here

(The "promising" superstring reference has been there at least since the 2000 version (where supergravity was also described as promising, not sure how much earlier)

20. **Peter Woit**
September 24, 2012

Thanks jg,

I guess there is some time limit on how long you can be "promising"…

21. **Anonyrat**
September 24, 2012

If SuperString Theory was a person, it would no longer qualify (too old) for the Fields Medal….

22. **A.**
September 25, 2012

I left the UK shortly after STFC lost the huge lump of cash which was the catalyst for much of the current problems. In the following years the word from the (rather sheepish) funding bodies was "there is very little money, and there won't be for a long time".

The approach they then took was consolidation, which meant that any money they did manage to scrape together was given to Cambridge, Oxford, and Durham. Hence the collapse of so many physics departments in the UK. There is a firmly entrenched attitude, at least in England, that if a university isn't hightly ranked for education, then it's research departments are automatically rubbish. (Someone high up in the Durham IPP once had a fit of hysterics when I suggested that some of the smaller UK groups were going interesting work. He said, and I paraphrase, that they should all be put down.)

If you want to see where UK theory is headed, then the research going on at Oxbridge and Durham is basically it.

Wide World of Links

September 26, 2012
Categories: Uncategorized

Some short items of a wide variety of kinds:

- Witten has posted to the arXiv a long paper about the work on superstring perturbation theory that he has been doing. Superstring Perturbation Theory Revisited, together with two papers of background material (see here and here) weighs in at 400 pages. For an explanation of the main points, you might want to start with one of Witten's recent talks on the subject, for instance this one at Strings 2012.
Witten doesn't make much in the way of claims for the significance of this work, portraying it more as a project of going through the foundations of the subject of how you define higher loop superstring amplitudes in a much more careful way than was common during the mid-late 80s when this was a hot topic of research. The technicalities here are ferocious, well beyond my expertise. It will be interesting to see if this project revives interest in the subject and others start working on it again.
- Also on the arXiv is a new paper from Paul Frampton, with affiliation now including the Centro Universitario Devoto, part of the prison where he is unfortunately still incarcerated in Argentina. He argues here that he is still able to fulfill his duties as a University of North Carolina professor from Devoto prison, but doesn't seem to have gotten the university to agree about this.
- There's a conference at DESY this week on Lessons from the first phase of the LHC, with talks on Friday discussing "Where could SUSY be hiding?" and "Searches for new physics at the LHC: some frustration, but no despair…". For some additional context to the SUSY issue, I recently ran across this talk from SUSY 02, 10 years ago, which argued that SUSY arguments implied that "superpartners are probably being produced" at a new collider that had been running for a year or two (the Tevatron Run II at that time).
- The SCOAP^3 consortium has announced a plan to support commercial journals publishing HEP papers, paying them 1000-2000$ per HEP paper they publish according to a complicated formula. Elsevier would get about $2.4 million/year for papers in Physics Letters B and Nuclear Physics B, but somehow reduce its subscription fees to compensate. I don't understand at all how this is supposed to work (obvious problems include that of why anyone would subscribe once it was all open access, and what the mechanism is to stop publishers from increasing revenues by publishing more second-rate papers). Nature has an article explaining more about what is going on here.
Steven Harnad describes the scheme as Unnecessary, Unscalable and Unsustainable, Peter Coles as "Particle physics volunteers to be fleeced…"
- String theory advertising available here, Sean Carroll commentary about this here.
- At *Foundations of Physics*, Gerard 't Hooft has a new paper which doesn't seem to be on the arXiv, On the Foundations of Superstring Theory (you may need a subscription to read it). Here's the abstract:

  > Superstring theory is an extension of conventional quantum field theory that allows for stringlike and branelike material objects besides pointlike particles. The basic foundations on which the theory is built are amazingly shaky, and, equally amazingly, it seems to be this lack of solid foundations to which the theory owes its strength. We emphasize that such a situation is legitimate only in the development phases of a new doctrine. Eventually, a more solidly founded structure must be sought.
  > Although it is advertised as a "candidate theory of quantum gravity", we claim that string theory may not be exactly that. Rather, just like quantum field theory itself, it is a general mathematical framework for a class of theories. Its major flaw could be that it still embraces a Copenhagen view on the relation between quantum mechanics and reality, while any "theory of everything", that is, a theory for the entire cosmos, should do better than that.

  There's a recent blog posting about this here, including commentary from 't Hooft himself.

- If you're trying to keep up on reaction to Mochizuki's claimed proof of the ABC conjecture, try looking here. Still I think a very long ways to go before experts understand this well enough to evaluate whether this is a solid proof.
- I recently heard from Nick Carlin, who has unearthed the following scientific documents: Strange Particle Interactions in a Bubble Chamber and The Angular Correlation of Polarization of Annihilation Radiation, from late 1977 or spring 1978. Handwritten commentary is from William J. Skocpol and Robert V. Pound.

Update: Some frustration, but no despair is now on-line. The first slide is pretty amusing…

Update: More news about the latest in Paul Frampton's case here.

Comments

1. **Tony Smith**
September 26, 2012

   As to SCOAP3 and
"… why anyone would subscribe once it was all open access …"
the Register has a 26 Sep 2012 article entitled
"High-energy physics opens up"
that says
"… Instead of fronting up for high subscription fees,
libraries will fund the per-article open access fees …
This will either reduce, or replace entirely, library subscriptions …"
so
it may be that individual users will now have to pay $30 per article
if they go to a university library to get papers from the web.

   Tony

2. **Peter Woit**
September 26, 2012

   Tony,

   The "libraries will fund the per-article access fees" is referring to the journals getting $1000-$2000/article, and in return making them available for free on the web. So, if you had to go to a university library to get access to such articles, once this goes into practice, you can stay home and get access from your computer for free. This is the positive side of this story.

   On the other side, there's the question of why, when some mathematicians and physicists are trying to get the literature out of the grips of Elsevier so that the scientific community would no longer have to pay them, this plan does the opposite, providing them with a $2.5million/year revenue stream to pay for "open access" to articles that are already available on the arXiv.

The article Tony refers to is at

http://www.theregister.co.uk/2012/09/26/open_access_high_energy_physics/

3. **Deane**
September 26, 2012

   I had a great time talking to your colleague Shouwu Zhang today at tea about the abc conjecture and number theory in general. He really can explain the whole history of number theory up to today more clearly than anyone else I've ever talked to.

4. **Peter Woit**
September 26, 2012

   Hi Deane,

   Well, does Shouwu believe it? In any case, we're hoping he sees the light and doesn't permanently move to Princeton…

5. **chris**
September 27, 2012

   what a wonderful paper – thanks for the link.

   i mean the one by 't Hooft of course.

6. **Raisonator**
September 27, 2012

   Indeed, a wonderful paper – found interesting new arguments.

   I am reminded of a statement by John Bell way back in 1990.

   Q: "I'm sorry, does that mean that you say relativity and quantum mechanics are not compatible?"
J. B.: "No no I can't say that, because I think somebody will find a way of saying that they are compatible. But I haven't seen it yet. For me it's very hard to put them together, but I think somebody will put them together, and we'll just see that my imagination was too limited. Well, as the people in that department work at present, they are not coming to this question, because the superstring is still formulated within traditional quantum mechanics, and you still have the superposition principle which is maybe the root of all these things. But it could be that as they go further into that, they find that it just won't work along the traditional lines, and at some point they'll have to give up the superposition principle."

7. **Clark**
September 27, 2012

   Vesselin has found a serious problem in the Mochizuki proof. Terence Tao believes :
" it would be difficult to be optimistic about the proof until this issue is somehow resolved "

8. **Peter Woit**
September 27, 2012

   Clark is referring to this:

   http://mathoverflow.net/questions/106560/philosophy-behind-mochizukis-work-on-the-abc-conjecture/107279#107279

   It's very hard to follow this discussion on mathoverflow, the format is really wrong for this. Instead of a chronological discussion, one just sees the latest version of a continually edited "answer to a question", with a random selection of comments that make no sense as such (one can get all the comments easily, but the ever-changing nature of the text they are referring to is a big problem).

   My understanding is that others have also tried looking at Mochizuki's paper by starting with a claim in it that is of a kind they are expert at, but that looks like it might me too strong. They find counter-examples, but then later realize that buried somewhere way back in Mochizuki's work was an explicit assumption that they hadn't noticed was being made, so there was nothing wrong with the claim. I'm curious whether this might be happening here. Maybe we'll know soon.

9. **harryb**
September 27, 2012

   Very interesting paper from 't Hooft – thanks – seems to be downloadable without subscription.

   't Hooft seems to have a live love-hate relationship with ST, summed up maybe by after noting he does not want to critique ST, he states later:

   "The reason for writing this note is simply that the author suspects that
superstring theory can be improved considerably, or can perhaps be replaced entirely by something better."

   We feel his pain. Good luck.

10. **John**
September 27, 2012

    Peter,

    Can you comment on this recent paper claiming that superstring theory is incorrect?
http://arxiv.org/abs/1209.5772

11. **Peter Woit**
September 27, 2012

    I've already deleted several off-topic comments today from people who want to discuss papers that make grandiose claims that are probably wrong, so should probably delete John's also. But since it's an overhyped claim about string theory, and it takes 2 minutes of looking at that paper to realize what is going on, here goes:

John,

The claims about superstring theory in that paper are nonsense. The paper is purely about classical physics, and the "string-like" classical field configurations discussed in it have nothing to do with quantum superstring theory, whatever it might actually be.

12. **Thomas**
September 28, 2012

Concerning Mochizuki's work: Aside the ABC conjecture, he writes about many other interesting issues, like his very interesting thoughts on how to see anabelian geometry, or on Arakelov theory and those deformations of number fields. As nice as a proof of the ABC conjecture would be, would such new ways to see things not be even more interesting?

13. **Deane**
September 28, 2012

Peter, I really hope Shouwu stays at Columbia but it's a tough choice for him. As for Mochizuki's claimed proof, he said that Mochizuki has to be taken seriously so he plans to work through the paper or papers very carefully.

14. **Jeff McGowan**
September 30, 2012

OK, question from a mathematician. I was bouncing around the links, and got from "Why String Theory" over to Joseph Conlon's homepage at Oxford. I glanced at his CV and noticed that he splits his publications into ArXiv and "other published papers" and the other papers consist of just 3, there are a whole lot of ArXiv papers. Do ArXiv papers really count in physics? I mean a mathematician would list ArXiv papers, even on a CV, but usually as "pre publications," and sometimes only if they have already been submitted somewhere. They certainly don't really count for anything, job wise. I can't imagine there's anyone in maths at Oxford who has only 3 refereed publications. I actually know of someone who didn't get tenure because they wouldn't count two papers that had been accepted but had not appeared yet.

15. **Paul Wells**
September 30, 2012

Peter,

Do you know if there has been serious attempt by the physics community to help Paul Frampton? Have there been any representations or petitions to the U.S or Argentinian governments ?

16. **tt**
September 30, 2012

looks like most of those arxiv papers are also published in peer reviewed journals

17. **Jeff McGowan**
September 30, 2012

tt, wow, right you are (well, more or less in one refereed journal, since almost all are in JHEP) Strange way to list them I guess, in math you wouldn't list ArXiv reference, though everything essentially is on ArXiv. Everyone just assumes that if for some reason you don't have journal access you'll just Google the paper to find the free version.

18. **Shantanu**
October 1, 2012

Peter and others,
people should have a look at Ed Witten's talk nearly 10 years ago
at Lepton photon 2003 symposium at Fermilab
http://vmsstreamer1.fnal.gov/VMS_Site_02/Lectures/LP2003/Witten/index.htm
He was worried that LEP had not discovered supersymmetry.

19. **Peter Woit**
October 1, 2012

Shantanu,

That talk was interesting, in that it was not just hype, but also explained some of the problems with SUSY. These have just gotten much worse since then (proton decay not showing up, flavor-changing processes not showing up, etc.)

"Looking back, for example, to the summary talk (by David Gross) at Lepton-Photon 1993, I see that by ten years ago SUSY was already described as the "standard non-standard theory" (he also gave a list of its successes and drawbacks rather similar to what I am explaining today) …. That is getting to be a long time."

If ten years is "getting to be a long time", what do you call twenty years?

20. **Michael Shain**
October 1, 2012

The Guardian of 27 September has a pithy blog posted by Lily Asquith: "Desperately Seeking SUSY"
http://www.guardian.co.uk/science/life-and-physics/2012/sep/27/susy-higgs-lhc

It sums up the present state of affairs very nicely.

21. **Clark**
October 14, 2012

Mochizuki has responded to the problem detected by Vesselin:

http://www.kurims.kyoto-u.ac.jp/~motizuki/Inter-universal%20Teichmuller%20Theory%20IV%20%28comments%29.pdf

Physics Frontiers Prize

October 1, 2012
Categories: Uncategorized

Yuri Milner's Fundamental Physics Prize Foundation announced today the process by which future winners of the $3 million Fundamental Physics Prize will be chosen (for more about this, see here), a process which involves setting up yet another prize, the Physics Frontiers Prize. The idea is that by the end of the year, the Selection Committee of previous prize winners will pick three winners of the new Physics Frontiers Prize, and these will be the candidates for the 2013 $3 million Fundamental Physics Prize. One of the three will get the $3 million, the other two will get $300,000 and automatically renominated for the $3 million prize each year over the next 5 years. So, I guess you might not want to win immediately, since if you get passed over the first time, you might end up with $3.3 million instead of $3 million.

There's also a separate $100,000 New Horizons in Physics Prize "targeted at promising junior researchers". Nominations for these two categories of prizes can be made by going to the Fundamental Physics Prize website.

The press release quotes Nima Arkani-Hamed, member of the Selection Committee as:

> This is a tremendous opportunity to recognize the highest levels of achievement in fundamental physics. We look forward to receiving nominations for outstanding candidates ranging across all areas of the field.

Arkani-Hamed is in India, where an interview with him appeared today (hat-tip an e-mail from him to Lubos Motl), with comments about the Milner prizes:

> I really think it's a fantastic thing for Physics—to have a showcase every year where scientists get to talk about the exciting aspects of the subject. I don't think any physicist or scientists are motivated to research by the thought of a prize or the money involved in it. But, it definitely helps in creating awareness among the youngsters, and encourages more people to take up the subject.

the Higgs:

> There are people trying to figure out the indirect effects between the different Higgs like particles. These are very difficult experiments and will take another 20 years before any confirmation is reached.

the future of particle physics:

> What's going on in particle physics is not just the evolution of the standard model but the rise of a new branch of physics that can solve some of the age old problems. Super symmetry is a very good example of what this physics should look like. For the first time we will have some evidence that there's actually really fine adjustments of the parameters of fundamental physics hardwired into the way nature works. This will be very shocking for many people and teach us something profound.

and string theory:

> In late 1990s one of the most important theoretical discoveries was that string theory and particle physics are not different but different descriptions of the same thing. All the good viable ideas people have had in the past 40 years are now branched together to seek the truth.

Update: Haaretz reports that Witten "said he would probably donate part of the $3 million he won in a surprise award to J Street, the liberal pro-Israel group."

Please note that any attempts to pursue, from either side, the Arab-Israeli conflict on my blog's comment section will be immediately deleted.

Update: Just realized that the Witten/J Street news is rather old, from shortly after the announcement of the original prizes. I didn't hear about it at the time, curious if there's other news about what plans the prize winner have for their winnings.

Comments

1. **Bob Jones**
   October 1, 2012

   I'd like to see these prizes awarded to more people like Kontsevich who do mathematical work. I'm thinking Connes and Drinfeld would be good candidates.

2. **Bob Jones**
   October 1, 2012

   Peter, I've noticed that sometimes you post quotes from string theorists without commentary. What are you implying exactly by quoting Arkani-Hamed on string theory?

3. **James Green**
   October 1, 2012

   @Bob Jones
   He posted the article…the context is pretty much obvious.

4. **Peter Woit**
   October 1, 2012

   Bob Jones,

   I'm not implying anything. We report, you decide…

   If you want my reaction to that quote, I think it's kind of a convention these days to refuse to acknowledge that string theory has failed as an idea about unification, and invoke AdS/CFT to try and argue that string theory now subsumes QFT. I don't agree.

   As for the "All the good viable ideas people have had in the past 40 years" business, I have no idea what he is referring to. Is string theory unification still viable? Is SUSY still viable post LHC? Are the extra dimensional models he became famous for still viable? Hard to tell. Given his current work on amplitudes, he may very well be referring to twistors, which now date from about 40 years ago and are very much a viable idea.

5. **Bob Jones**
October 2, 2012

"I don't agree."

You don't agree that string theory is a viable idea about unification, or that it subsumes QFT? Since string theory is a large-N limit of gauge theory, there is a sense in which string theory and particle physics are two aspects of the same thing. There's nothing technically wrong with that part of Arkani-Hamed's answer.

"As for the 'All the good viable ideas people have had in the past 40 years' business, I have no idea what he is referring to."

I'm not completely sure what he means here either. Probably he means that twistor theory, noncommutative geometry, supersymmetry, and supergravity were all successful ideas and loop quantum gravity was not.

You didn't mention the part where Arkani-Hamed refers to AdS/CFT as "one of the most important discoveries" of the late 90s. Does that mean you agree with him about that?

6. **chris**
October 2, 2012

"I don't think any physicist or scientists are motivated to research by the thought of a prizeI don't think any physicist or scientists are motivated to research by the thought of a prize"

i wonder if he kept a straight face saying this.

7. **Bernhard**
October 2, 2012

Bob Jones,

You mean IF (very big IF) "string theory is a large-N limit of gauge theory, there is a sense in which string theory and particle physics are two aspects of the same thing. "

AdS/CFT is an unproven conjecture, not a theorem.

8. **Bee**
October 2, 2012

Sounds like a good procedure if what you want is "more of the same."

9. **Peter Woit**
October 2, 2012

Bob Jones,

Yes, I agree that AdS/CFT was an important advance, a very interesting one. As for "string theory is a large-N limit of gauge theory", that's only understood at all for specific, very unusual conformally invariant theories such as N=4 SYM. For actual physical asymptotically free gauge theories like QCD, we still don't have an understanding of their large-N limit, in terms of string theory or anything else.

And even if string theory ever gives the large-N approximation to gauge theories, that just means that it's a useful approximation technique, not that it's the same thing as QFT. Again, I think what is going on here is an attempt to divert people's attention from failure.

10. **PMS**
October 2, 2012

@Bernhard:

Yes it is an unproven conjecture, but there now exist such a big mountain of extremely non-trivial and precise tests (and generalizations) of it that there is no doubt that there is some truth to the conjecture. But AdS/CFT correspondence is just the tip of the iceberg, the large N limit is an even smaller part of a much larger idea. The arguments for the holographic principle, by 't Hooft, relies on very general ideas that is independent of string theory but should be valid in any consistent theory of quantum gravity. The fact that there is a concrete realization in terms of AdS/CFT correspondence, is highly non-trivial.

I think it is completely fine to be critical of string theory, as any sane person should be, and even believe its highly unlikely to have anything to do with the physical reality. But its quite dishonest to be negative about anything that has to do with string theory just because you are against it due to some "religious" reasons, just like Motl is pro-string theory due to some "religious" reasons. One also has to acknowledge the fact string theory has certain success stories, in which AdS/CFT correspondence is just one (and it is very important since it gives a realization of the more general and possibly very fundamental principle of holography).

Sorry for the rant. I just got provoked by the "very big IF" comment, which either indicates you know nothing about the details of AdS/CFT correspondence or you are just as crazy as Lubos Molt. There are many things we know about physics which be believe in, which are not "proven theorems".

11. **Bernhard**
October 2, 2012

PMS,

Which "extremely non-trivial and precise tests " are you talking about? AdS/CFT has some interesting applications to heavy ion physics such as calculation of shear-viscocity, etc. I believe you are exaggerating with "big mountain of extremely non-trivial and precise tests". AdS/CFT should work for maximally supersymmatric theories, I see no way any test could be "precise" nor how do we have a "big mountain" of it.

In any case, I am not negative towards AdS/CFT at all. Quite the contrary, if there is any interesting result that came out of string theory to choose, it is definitely AdS/CFT. My comment was only that until the evidence in favor of AdS/CFT gets very compelling (and sorry to disagree with you here, we are not there yet for the simple reasons Peter mentions: What is the string or not-string dual of QCD in the large N-limit ?) using it as an argument for Strings = QFT is debatable to say the very least.

12. **PMS**
October 2, 2012

@Bernard:

no I do not consider the applications to heavy ion physics or condensed matter physics as tests of AdS/CFT. I don't even find them too convincing yet, but definitely potentially very interesting. There exist hundreds, if not thousands, of papers where very precise calculations has been done on both sides of the duality and 100 % match has been found (recent checks using localization are very interesting). If one thinks that AdS/CFT is only about N=4 Super YM in the large N limit, then one has stopped following the field since 1997/1998. There by now exist many other implementations of AdS/CFT, for example a new interesting development is a new conjecture of Gaberdiel and Gopakumar which relates higher spin gravity on AdS_3 to certain types of 2d CFT minimal models (so called W_N models). Here the dual theory is neither a gauge theory nor supersymmetric (the interesting thing is that the dual theory is essentially integrable, and thus one might possible be able to prove the duality in this toy model). There are of course many other generalizations which go very much beyond all this. Personally I have no doubt that there is something about this duality, the fact that you can derive OPE's, correlation functions, RG flows, partition functions etc. of highly interacting CFT's from pure gravity with 100 % match cannot simply be accidents. Especially since it works in many different generalizations which look very different from the original Maldacena conjecture, and don't even rely on string theory (at least in the limits where they are under control, for example the gravity side is classical).

Let me again emphasis that AdS/CFT correspondence is NOT just a strange duality which was found in string theory, and requires maximal supersymmetry (as you claim). The holographic principle has origins from black hole thermodynamics and information loss paradox, not string theory. In order to be consistent, 't Hooft proposed that gravity is in a certain sense non-local and one has to understand this non-locality in order to understand quantum effects. AdS/CFT is just one particular example that "proves" that 't Hoofts thought experiments where correct. Even if string theory turns out to be wrong, the holographic principle still stands!

Thus holography is not interesting because of string theory or applications to condensed matter physics, it is interesting because it is a generic feature of consistent theories of quantum gravity! I think many people misunderstand this and believe that holography is just a strange feature of string theory.

I am very sorry for this too long post, which contains too many repetitions.

13. **Peter Woit**
October 2, 2012

PMS,

There's no question that dualities in QFT are a fascinating and real phenomenon and well-worth investigating. That doesn't change the fact that there has been a huge amount of hype and mystification surrounding AdS/CFT, and I think it is this that Bernhard is reasonably reacting to. Instead of hype about heavy ions, high-temperature superconductivity, a solution to QCD, string theory and QFT "the same thing", etc., etc. it would be nice if people working in this area would stick to making claims that bore some semblance to reality. The kind of new relation between 3d and 2d QFTs that you mention is a nice piece of mathematical physics, but it and other advances of its kind don't support the sort of hype Arkani-Hamed and others are engaging in. Maybe it will someday lead to really understanding quantum gravity, but for now it has had no significant impact on our understanding of particle physics.

14. **vmarko**
October 2, 2012

@PMS:

Thus holography is not interesting because of string theory or applications to condensed matter physics, it is interesting because it is a generic feature of consistent theories of quantum gravity!

The crux of the matter, which a lot of people seem to be systematically avoiding, is really the following: what CFT (or otherwise) is dual to the ordinary general relativity in 4D, with a positive cosmological constant (or maybe zero cosmological constant)?

Until **that** duality is constructed explicitly, and until one can use it to calculate for example the entropy of an "ordinary" stationary Schwarzschild black hole in 4D, I don't quite see holography as a generic feature of quantum gravity (the most important/physically relevant one).

15. **Shantanu**
October 2, 2012

Peter, another OT comment.
I didn't see any single blog which mentioned about Leonid Grishchuk passing away (except on Peter Coles blog, but he was Grishchuk's colleague at Cardiff).
http://telescoper.wordpress.com/2012/09/14/r-i-p-leonid-grishchuk/
Was his work not well known among particle physicists?

16. **Peter Woit**
October 2, 2012

Shantanu,

I was not aware of Grishchuk's work, and I suspect he was little known among particle physicists, since his specialty was classical GR.

17. **Bob Jones**
October 2, 2012

"And even if string theory ever gives the large-N approximation to gauge theories, that just means that it's a useful approximation technique, not that it's the same thing as QFT."

I'm not sure what you mean here. The AdS/CFT correspondence does in fact say that string theory in anti-de Sitter space is equivalent to a quantum field theory. It sounds like you're defining QFT to be quantum chromodynamics, which is very strange…

"Again, I think what is going on here is an attempt to divert people's attention from failure."

So all those people working on AdS/CFT are just trying to distract us from the failure of string theory? Are you serious? What you're talking about here is the most important result of a forty-year research program and the most profound fact we know about quantum gravity. People are working on AdS/CFT because it's interesting. You make it sound like some sort of big conspiracy…

18. **Bob Jones**
October 2, 2012

"You mean IF (very big IF)… AdS/CFT is an unproven conjecture, not a theorem."

Bernhard, I am sorry, but it's very hard for me to take such concerns seriously. As PMS has already pointed out, there is an enormous amount of evidence that AdS/CFT is true. As a mathematician, I think rigorous proofs are very important, but there is no doubt in my mind that the correspondence is true and will eventually be proven. I think it's clear that Maldacena discovered a very deep relationship between two kinds of theories, and since this relationship is, to some extent, a definition of string theory, I'm not sure what it would mean for the correspondence to fail.

19. **Bob Jones**
October 2, 2012

"what CFT (or otherwise) is dual to the ordinary general relativity in 4D, with a positive cosmological constant (or maybe zero cosmological constant)?"

First of all, the AdS/CFT correspondence is essentially a result about quantum gravity with negative cosmological constant. You need that in order to talk about the conformal boundary of spacetime. There are things like the dS/CFT correspondence for positive CC, but this is not well understood. However, this does not make the correspondence uninteresting for people working on quantum gravity. It just means there are some cosmological problems that need to be worked out. It still gives you a nice dual description of quantum gravity on nearly flat spacetimes.

"until one can use it to calculate for example the entropy of an "ordinary" stationary Schwarzschild black hole in 4D, I don't quite see holography as a generic feature of quantum gravity"

Well, the holographic principle has actually been used to calculate the entropy of Kerr black holes via the Kerr/CFT correspondence. Since there are astrophysical Kerr black holes, this means that is does have some relevance to "real" gravitational physics. The AdS/CFT correspondence also shows up in three-dimensional quantum gravity as famously demonstrated by Brown and Henneaux, so I think it's fair to say that it is a very generic feature of quantum gravity.

20. **PMS**
October 2, 2012

@Peter

I could not agree more, string theory is without any doubt the most hyped field in physics. This hype has shortsighted advantages, but on the long run will just turn other physicists against the field. Personally I encourage people to fight this hype, but not turn against a field only based on that. I dislike all arguments for or against string theory, which are purely based on 1) feelings, 2) ignorance or just 3) craziness. For example Lubos Motl is a master of 3), even though he is ridiculously smart. I think string theory has put forward many new and deep insights, both of technical nature but also more importantly, conceptual ones.

Regarding the new AdS3/CFT2 dualities. I don't agree that these are only relevant for mathematical physics, they are toy models which show that holography works exactly as one expects even when there are no supersymmetry, gauge theory etc. and thereby putting the whole framework on a more solid state. It might also give hints about certain conceptual question we still don't understand, such as how does the extra dimension emerge from from the boundary point of view and how can we prove the duality? Toy models have played extremely important roles in the history of physics and still do. The Ising model and phase transitions is an famous example.

But let me repeat that I do share much of your frustration and worries about the state of the field, at least to some extend.

@vmarko:

First of all, the dual theory to our 4D universe/black holes are most likely not CFT's. The asymptotic symmetry of AdS is conformal group (and the whole Virasoro algebra, for AdS3), which is the reason the dual theory "on the boundary" has to be a CFT. AdS nor CFT are necessary for holography, since the principle should hold much more generally.

Regarding your point. Finding such a dual QFT is a very very difficult problem, not only due to technical difficulties but also because there are many conceptual problems about quantum gravity we need to understand better. The power of the holographic principle comes from the fact that it relies on quite general assumptions which are generally agreed upon, such as validity of General Relativity, Quantum Mechanics, black hole creation/evaporation is a unitary process and so on (http://arxiv.org/abs/gr-qc/9310026). This a conceptual breakthrough that deserves more detailed study. It takes time to understand these questions good enough to be able to calculate (microscopically) the entropy of the Schwarzschild black hole.

What I don't understand is, why do you believe the general arguments/thought experiments which form the basis of black hole thermodynamics. But you refuse to consider very similar types of arguments which form the basis of information loss paradox and the holographic principle…?

21. **Tmark48**
October 2, 2012

So in the end, real physicists (experimentalists and theoreticians) will still look forward to the Nobel prize, the rest will have to do with the Milner "pseudo-science" prize.

> One of the three will get the $3 million, the other two will get $300,000 and automatically renominated for the $3 million prize each year over the next 5 years.

Why on earth are the same candidates to be "automatically" renominated every year for the next 5 years. What kind of stupid decision is this ?

22. **Bob Jones**
October 2, 2012

vmarko,

I should also mention that the argument of Brown and Henneaux shows that any consistent theory of quantum theory of gravity in three-dimensional anti-de Sitter space has a CFT dual. It has nothing at all to do with string theory. When Strominger and Vafa used the holographic principle to compute the entropy of a black hole in string theory, their argument appeared to depend heavily on the details of string theory, but we now know that the holographic principle can be used to compute black hole entropy in any consistent unitary theory of gravity that admits black hole solutions.

23. **vmarko**
October 2, 2012

@Bob Jones:

First of all, the AdS/CFT correspondence is essentially a result about quantum gravity with negative cosmological constant.

That is precisely my point. The observed value for the cosmological constant is actually positive. So until someone successfully constructs the "dS_4/CFT_3" correspondence, I am not buying holography as a relevant principle of quantum gravity, let alone generic.

However, this does not make the correspondence uninteresting for people working on quantum gravity. It just means there are some cosmological problems that need to be worked out.

How do you plan to work out that "cosmological problem" of the wrong sign of CC? There is a very big jump from a noncompact to compact topology lurking in there. I am yet to see any serious proposal regarding this.

Well, the holographic principle has actually been used to calculate the entropy of Kerr black holes via the Kerr/CFT correspondence. Since there are astrophysical Kerr black holes, this means that is does have some relevance to "real" gravitational physics.

Umm, no, those are the **extreme** Kerr black holes, not the astrophysical ones, precisely because one needs the AdS structure. The only thing that even comes close to calculating the entropy of a BH with a small or zero angular momentum is arXiv:1004.0996, and the authors there openly admit that they still lack a tight argument for non-extreme angular momentum.

The AdS/CFT correspondence also shows up in three-dimensional quantum gravity as famously demonstrated by Brown and Henneaux, so I think it's fair to say that it is a very generic feature of quantum gravity.

Three-dimensional as in 2+1 spacetime dimensions? The theory which actually has no gravity inside? No propagating degrees of freedom? Theory where spacetime is always flat? Wow! 🙂 There are many very nice features of 2+1 gravity (for example, the absence of gravity…) that do not carry to four dimensions. Whenever I hear this kind of an argument, my basic reply is "3=/=4".

My point is that all examples of quantum gravity where holography principle holds turn out to be examples of **wrong or irrelevant** theories of quantum gravity. It is therefore quite far-fetched to call holography a "generic feature" of quantum gravity, since the main, physically relevant example is completely absent. In addition to that, there are many quantum theories of gravity (LQG, spinfoams, CDT, etc…) which do not have any connection at all to the holography principle.

I'd say holography is far from universal, and actually holds only in some neat example theories, but not in general.

24. **vmarko**
October 2, 2012

@PMS:

What I don't understand is, why do you believe the general arguments/thought experiments which form the basis of black hole thermodynamics. But you refuse to consider very similar types of arguments which form the basis of information loss paradox and the holographic principle…?

Well, shortly put, because BH information loss paradox has many possible avenues where a solution might appear (none completely satisfactory, of course). So I simply do not see holographic principle as the only possible resolution of the paradox. It indeed is one of the possibilities, but not the only one.

Of course, every opinion on an open question is based on intuition, general philosophical POV on what is important in Nature and what isn't, prejudices from early youth, etc… 😉

Don't get me wrong — I am not advocating that holography is dead wrong or uninteresting or something. I am just saying that "holography is a generic feature of quantum gravity" activates my "hype-overflow" alarm… 🙂

25. **Bob Jones**
October 2, 2012

"How do you plan to work out that "cosmological problem" of the wrong sign of CC?"

This is obviously a very important open question. I'm just saying that if you want to study short distance effects in quantum gravity (scattering of gravitons, etc.), then the solution is in principle given by the gauge-gravity correspondence.

"Umm, no, those are the extreme Kerr black holes, not the astrophysical ones"

In the paper arXiv:0809.4266 [hep-th] where the Kerr/CFT correspondence was first introduced, the authors point out that nearly extreme Kerr black holes have been observed in the sky, and they give GRS 1915+105 as an example. For such black holes, any corrections to the dual CFT representation should be small.

"The theory which actually has no gravity inside? No propagating degrees of freedom? Theory where spacetime is always flat?"

Actually, it does have gravity. The classical solutions of Einstein's equations in three spacetime dimensions are not flat; in fact they include black hole solutions. This makes three-dimensional gravity an important and nontrivial toy model for studying general features of quantum gravity.

"My point is that all examples of quantum gravity where holography principle holds turn out to be examples of wrong or irrelevant theories of quantum gravity."

All I can say is you're not going to get very far in your understanding of nature if the only theories you're willing to consider are completely realistic. Sometimes it's useful to study theories with certain simplifying assumptions.

26. **vmarko**
October 2, 2012

@Bob Jones:

I should also mention that the argument of Brown and Henneaux shows that any consistent theory of quantum theory of gravity in three-dimensional anti-de Sitter space has a CFT dual.

Sure, I know, but a 3D theory just doesn't cut it, IMO. 3D gravity is far too featureless (compared to 4D gravity) to be persuasive enough. I understand that some people consider it a strong hint, and it is a valid research topic, but I just don't share the opinion that holography principle is a settled matter.

27. **Peter Woit**
October 2, 2012

Please all,

There's some sort of law of nature that causes all discussion about theoretical physics to devolve into discussions of quantum gravity. Enough of this here for now please, it's completely off-topic.

28. **Stephen**
October 2, 2012

Obviously, you use the first 300K to hire two hit men (and a bodyguard).

29. **Bob Jones**
October 2, 2012

Just a small correction, if Peter will allow it. If we're talking about the vacuum Einstein equations in three dimensions with vanishing cosmological constant, then vmarko is correct in saying that the space of classical solutions is the space of flat metrics. I was just saying that three-dimensional gravity is interesting despite the unphysical simplifying assumptions.

30. **Peter Woit**
October 2, 2012

Bob Jones,

In my "what is going on here" remark the "here" is the topic of the posting, i.e. the quote from Arkani-Hamed about AdS/CFT meaning string theory = particle physics, not this area of research in general. I don't actually think there's any sort of sensible statement of any kind that can be applied to the general topic of research related to these dualities. By now, this involves probably tens of thousands of papers, with topics spanning the range from vague nonsense that belongs in the beyond the fringe science category to very serious mathematical physics, with everything in between.

To say something sensible, you need to pick a specific conjecture. Even for the one very specific original case of AdS/CFT, meaning superstrings on AdS^5xS^5/N=4 SYM, there's more and less meaningful conjectures to discuss. Since you don't know what non-perturbative string theory really is, making your conjecture to be vacuously true at weak CFT coupling by defining non-perturbative string theory = the right QFT isn't really something to promote as a great success.

31. **Bob Jones**
October 2, 2012

Tmark48,

Which of this year's winners is doing "pseudo-science"? What's your definition of a "real" theoretical physics?

32. **Bob Jones**
October 2, 2012

Peter,

I think the statement is not that string theory=particle physics but that string theory and particle physics are two aspects of the same thing. In other words, there is a common mathematical idea underlying them, namely gauge theory. If that's what Arkani-Hamed meant, then I don't think there's anything technically wrong with his answer.

33. **Peter Woit**
October 2, 2012

Bob Jones,

The question Arkani-Hamed was asked was:

"String theory or standard model, which offers a better explanation to our quest to understand the universe?"

Clearly the questioner was asking about the well-advertised claims for unification via string theory. Not answering "the standard model" to this is just intentionally misleading and less than honest. You can try and argue that the formulation "string theory and particle physics are not different but different descriptions of the same thing." is not technically wrong, but I think that's a strained argument. What Arkani-Hamed is doing here is just throwing sand in the questioner's eyes, trying to exploit conjectural and poorly understood relations between strong coupling QCD and string theory + true by definition conjectures about non-perturbative string theory being QFT to avoid acknowledging the obvious (standard model= huge success, string theory unification = miserable failure).

34. **Allan Rosenberg**
October 2, 2012

Peter,

Maybe the best strategy for broadening the scope of the prize beyond string theory is to nominate experimentalists. The only name that springs immediately to my mind is Alain Aspect, but surely somebody who reads this blog has to know who the top experimentalists are at CERN, for example.

I have to admit, though, that, just for laughs, I did toss Gordon Kane's name into the ring.

35. **MathPhys**
October 2, 2012

I was thrilled to read that the failure to explain the mass hierarchies using supersymmetry is an exciting development.

36. **anonymous**
October 3, 2012

"Maybe the best strategy for broadening the scope of the prize beyond string theory is to nominate experimentalists. The only name that springs immediately to my mind is Alain Aspect, but surely somebody who reads this blog has to know who the top experimentalists are at CERN, for example."

Of course Alain Aspect shared the 2010 Wolf Prize in Physics with John Clauser and Anton Zeilinger. All three of them as well as a dozen people working on the same or similar problems could get a Nobel Prize in the next year or 5. The trouble is that they are all at or approaching 70, not ideal for a New Horizons in Physics Prize.

If you want to step way outside the mainstream and award the Fundamental Physics Prize to an experimentalist whose work would be truly fundamental but would otherwise go completely unrecognized or dismissed, you could go with Yves Couder, Michel Gouanère, heck even a new unknown like Silke Weinfurtner for New Horizons. I'm certainly not going on record to nominate any of these people.

For experimentalists at CERN, look away from the LHC. You're just as likely to see some fun new stuff from Antiproton Decelerator physics, [any of the spokespeople](). Also very much looking forward to [ELI's]() fundamental science.

37. **A.**
October 3, 2012

@anonymous

Um, I'm not sure if the final line of your comment was a joke or not…. 🙂 Point is, the "fundamental science" part of ELI is almost certainly not going ahead. (For those not in the know: they were going to build a super-duper laser to do QED experiments with.) There are essentially two reasons why this project is imploding.

1) There's not much money around.

2) wait for it… too much _hype_ on the part of the guys pushing it. It's true. In an attempt to gather cash they went down the "flashy publicity" route, promising that they would "rip the vacuum apart" and other sensationalist fluff, and the community got fed up of hearing it. Those guys are increasingly regarded as crackpots who are hindering the field much more than they are helping it.

38. **anonymous**
October 3, 2012

Not a joke… just optimism. Maybe [this]() presents the more tempered version of the claims.

39. **M**
October 3, 2012

Physics is understanding nature. Awarding popular but wrong ideas with millions of $$$ surely is an exciting social game. But it is bad for good physics.

40. **Guy**
October 3, 2012

Edward Witten, according to Haaretz, plans to "donate part of the $3 million he won in a surprise award to J Street, the liberal pro-Israel group". So it's good the money's being used for peace.

On Lubos Motl's blog I commented on his 'Harvard' post this week. My comment was a direct excerpt of Witten's 2005 article from here:
http://peacenow.org/entries/archive296#.UGwdyq7DuuM in which he criticises Israel settlement building.
But Motl has blatantly deleted my 2nd comment in which I clarify Witten wrote that excerpt in 2005 (see for yourself), and called me an anti-Semite and because I agree with Witten's views. He has gone to the extent of saying my 2nd comment stated Witten wrote the comment today, which is a lie.

I hope Peter Woit you are open enough to publish this above material so it's clear I don't lie and who actually does. $3 million to be put in J Street shows it anyway.

41. **Tmark48**
October 3, 2012

@ Bob Jones :

Of course there is no such thing as a real theoretical physicist (as opposed to a false theoretical physicists). I just wanted to underline the fact that very few theoretical physicists are awarded the Nobel prize. And there is a reason for this, it's not just because the Nobel committee hates theoretical physicists. Theoretical physicists build models, models that are used to understand nature around us, models that CONNECT to experimental results and hopefully explain them by giving us a framework around which to understand those phenomena. And better yet, not only understand but predict new kinds of phenoma to be experimentally tested.
When such models are verified by experiment then those physicists are worthy of the Nobel prize. Does every theoretical physicists under the sun deserve a Nobel prize (because he is working on QLG, or some other exotic topic that will never ever in our lifetimes be even near to be experimentally explored) ? Of course not. Model building alone is not science, it is only a part of science, and without experiments you're left only with a nice scaffold and no content.
Conceptual revolutions are not a dime a dozen. They are very very rare in the history of science, and just because the 20th century happened to witness 2 such revolutions doesn't imply that every generation has to have such a conceptual revolution. Theoretical physicists exploring exotic topic today live in a bad era. An era were experiments cannot probe at all the "predictions" these model builders are advocating.
Maybe in a future, some theoretical physicist will come up with a new conceptual revolution and experiments at that time will corroborate his findings. Good for him, he will be awareded the Nobel prize. The rest are just collateral damage.

42. **Peter Woit**
October 3, 2012

Tmark48,
Some thoughts about this:

Given that theoretical particle physics is in some sense a victim of its own 20th century success, with new experimentally testable insights much harder to come by, if the Nobel committee sticks to the experimentally verified there will be fewer and fewer particle theory Nobelists around. Already, they're almost all getting pretty old, and even if Higgs et al. get a prize soon, that won't improve the age distribution. One could argue that standards for the prize need to change, with smaller increments of progress rewarded, including progress that moves us towards something experimentally testable, even if it doesn't get there. I think this idea is the sort of thing Milner is trying to do.

The problem then becomes: without experiment to keep you honest, how do you judge what is significant progress and what isn't? In a field of difficult to understand research, how do you stop prizes from going to those who are loudest and most politically savvy, rather than to what is truly significant? Knowing what is truly significant may well be beyond anyone's ability to judge. I don't think the Nobel committee should change its standards unless they can figure out how to resolve this problem, and I think Milner's decision to create extraordinarily large new prizes without addressing this problem is unwise.

Much of the story of the last 30 years is one of a huge investment of the particle theory community in a heavily overhyped research program that now has conclusively failed, with the death of hopes for electroweak-scale SUSY the final nail in the coffin. Leaders of the HEP theory community need to acknowledge this failure and draw some lessons from it, not be rewarded extravagantly for behaving like Arkani-Hamed, claiming all is well and refusing to acknowledge what has happened.

43. **Peter Woit**

October 3, 2012

Oh, and one more thing: the problem is not insoluble. Mathematicians have always lived without experiment to keep them honest, and manage to recognize and reward progress. They do have proof to keep them honest though…

44. **Friend**
October 3, 2012

Maybe if physics could be derived on the basis of logic alone, then we might have means of proof to keep physicist honest too. But who knows what logic reality could be based on, right?

45. **Tmark48**
October 3, 2012

@ Peter Woit :

I agree with you. Theoretical physicists can and should be awarded prizes. The thing is what do these prizes stand for ? If a theoretical physicist is awarded the Nobel prize then there is no way to take out the experiments from the equation. The Nobel prize not only awards the model building aspect but also the connection with experiments. Einstein was awarded the Nobel prize for the photoelectric effect and not for general relativity, even in the light of the Eddington expedition that "confirmed" one of general relativity's prediction. And the reason being that the results still had a noticeable margin of error that could invalidate Einstein's theory. So the Nobel committee was correct in its assessment as crazy as it may seem today. Of course general relativity was tested again and again many decades after Einstein getting the Nobel prize, and many decades after Einstein's death. Nowadays no one would think that general relativity is invalid and rightly so.

—

Theoretical physicists can be awarded different kinds of prizes. Clarification of mathematical principles that underlie physical theories, developping new mathematical tools for carrying out complex physics calculations, developping new mathematical frameworks etc… All these things can be recognised and awarded. But it cannot be called a physics science prize. A mathematical prize yes, but the moment you say physics prize it implictly assumes that those complex theoretical calculations in one way or another connect to experimental data.
—
Mathematicians are beholden to the concept of a mathematical proof which is a much more stringent criterion than what goes around in the physics community. Just because mathematicians deal with abstract objects doesn't mean they can prove arbitrary things.

46. **Bob Jones**
October 3, 2012

"Theoretical physicists build models… When such models are verified by experiment then those physicists are worthy of the Nobel prize."

Obviously, one needs experiments to test out new theories, but there's so much more to theoretical physics than model-building and predictions. After all, Noether's theorem is one of the most famous results of twentieth century physics, but there's no sense in which it's a "prediction" or something that needs to be tested. It's a purely formal statement about classical mechanics which can be proven mathematically, and when you formulate quantum mechanics, it's basically true by definition.

There are many other important results in physics that have nothing to do with predictions. Lagrangian and Hamiltonian mechanics are just reformulations of Newtonian mechanics, but they're among the most important theoretical tools that physicists use, and they provide the proper framework for understanding quantum mechanics. The technology of Feynman diagrams is another example. It's not a "prediction" or a "model" but an alternative point of view on quantum mechanics which is extremely important for making predictions.

I'm all in favor of people making testable predictions, but I think we should recognize more formal kinds of theoretical work as well. I think it would be disastrous for physics if everyone just tried to randomly guess how nature works.

47. **Bob Jones**
October 3, 2012

In response to your comments about mathematics, I should add that none of the formal work being recognized by the Milner prize is mathematics in the strict sense. Quantum field theory and string theory are not mathematics because they are based on abstract notions that do not exist mathematically. The notion of a Feynman integral is not something that you can make precise in full generality. It is an essentially non-rigorous, heuristic idea, and as such, it is not something that mathematicians can study. Quantum field theory and string theory have, of course, had a huge impact on mathematics, but that's because physical intuition has led to conjectures that have been recast in the precise language of mathematics.

48. **vmarko**
October 3, 2012

I completely agree with Bob Jones that there are some extremely important and insightful theoretical advancements in physics which have (by their nature) no relation to experiment. It's not mathematics, and yet it's also not experimentally testable stuff. So this kind of work does not qualify neither for the Nobel prize nor for the Fields medal, loosely speaking.

This is the void that Milner prize seems to fit in. While I don't exactly support the method for the winner decision process, the Milner prize still seems to have some legitimacy, because it aims to reward scientists which have given important theoretical contributions, but would be missed both by the Nobel and the Fields.

In that light, the idea seems to be legit and good. The implementation, however, isn't that great IMO.

49. **Anonyrat**
October 3, 2012

Feynman diagrams took off because Feynman used them to make precise QED computations, not because they were generically interesting.

50. **Anonyrat**
October 3, 2012

Likewise the Noether theorem gave a deeper insight into laws that were already known to be experimentally true.

51. **Bob Jones**
October 3, 2012

Anonyrat,

Yes, of course. They are a computational technique, just like the field theoretic dualities that people work on today. I was just saying that the method of Feynman diagrams is not itself a prediction or something that requires experimental confirmation.

52. **Bob Jones**
October 3, 2012

And about Noether's theorem, yes, I understand that. I was giving examples of results of physics that were formally deduced from pre-existing theories. These results are "mathematical" in that they do not postulate new physics.

53. **Peter Woit**
October 3, 2012

Anonyrat/Bob Jones,

Enough, go watch the debate or something…

54. **Peter Woit**
October 3, 2012

Guy,

Thanks. I added a link to the Haaretz story. Your comment was caught for a while in the spam queue. I had noticed earlier today that Lubos Motl was calling for your death based on the quote he didn't realize was from Witten. I guess you and Witten can join me and Smolin in the club…

As noted in the update: I want no part of the behavior that discussion of the Arab-Israeli conflict brings out in people. If you can make an interesting comment about the news of Witten's choice of what to do with his money without trying to score points for the Israelis or the Palestinians, that's fine. Otherwise, go argue about this elsewhere.

Oh, just realized this is rather old news, from early August…

55. **Anonyrat**
October 4, 2012

The Milner Prize for physics makes the World Wrestling Federation seem respectable, and that is an on-topic assessment (see Peter, what happens when you tell people to watch the debate….).

56. **Guy**
October 4, 2012

Thanks for the reply Peter and I'm still alive and in the club despite someone's death threat. To me any true physicist can't escape politics/justice issues which explains why we might see prize money going in part to social causes. Didn't realise article was from August.

On a side note, it's funny that you would need to solve 3 Millenium Prize problems to get the same money as Milner Prize. And even more ironic is that Milner Prizes have gone for string theory achievements, which itself is so much maths than experiment till who knows. At the end of the day it depends whether the billionaire likes tea or coffee.

57. **fuzzy**
October 5, 2012

the words can be misleading. apparently, these people speak of physics in the same sense as galilei, newton, einstein, pauli did… but even in finance and in mathematics the same word "derivative" is used, but the sense is slightly different

58. **Clyde Davies**
October 5, 2012

I have to say I think this prize is a stupid idea. There's too much speculation going in physics right now anyway, and not enough consolidation and verification. Why encourage, let alone reward it? But what would I know … I'm a chemist.

59. **Yatima**
October 6, 2012

For people interested in where the rubber hits the road there is a (too short – 4 pages) article in IEEE Software of September/October. "The Software behind the Higgs Boson Discovery" by David Rousseau, who managed Atlas offline software until March 2012 (http://ieeexplore.ieee.org/xpl/articleDetails.jsp?arnumber=6276293). IEEE has it hidden behind a paywall, but some google-fu may elicit a "liberated" version.

News from this weekend include the announcement that the Templeton Foundation is sponsoring the search for Dyson spheres (http://www.theregister.co.uk/2012/10/05/dyson_sphere_search_wins_funding/). This is has been done before with no conclusive results (http://iopscience.iop.org/0004-637X/698/2/2075/)

60. **Armin Nikkhah Shirazi**
October 8, 2012

I wonder whether the establishment of this new prize reflects the possibility that originally the FPP was offered to more than 9 people, since there is then enough money for 10 Frontiers prizes left over for each declined FPP, and if anyone really did decline, it would be really interesting to know the reasons for doing so

61. **Peter Woit**
October 8, 2012

Armin,

I have it on good authority that no one declined the prize. The amount of money to be devoted to these prizes I think is just Milner's choice: he isn't working with a fixed sum of money, but can devote some more to the prizes if he sees a good reason. The new prizes are probably an idea he liked that came up while trying to figure out the details of how best to choose the next year's winner (it's not at all obvious how to best have 9 people vote on this, having a "run-off" like this is one way to go).

Physics Nobel Prize 2012

October 8, 2012
Categories: Uncategorized

I had decided to retire from the Nobel Prize prediction business at the top of my game after my first prediction soon after this blog was started. I haven't heard anything about what tomorrow's announcement will be, but did just notice something that gave me pause, this quote in a Cosmos magazine article:

> "There's nothing stopping us from giving the prize to an organisation. But it has not been the custom in the scientific prizes," said Lars Bergstroem, secretary of the committee for the Nobel physics prize.
>
> "The Nobel Peace Prize has often been awarded to organisations. But in the science prizes we have tried to find the most prize-worthy individuals."

If there really is nothing but custom to keep them from awarding the Nobel this year to ATLAS, CMS, and CERN for the Higgs, I can't see a better occasion to break with the custom, and they've had a long time to decide whether or not to do this. So, here's a (probably wrong…) prediction for tomorrow: ATLAS, CMS and CERN as Nobelists.

Update: As predicted, that prediction was wrong: the prize went to Haroche and Wineland for work manipulating individual quantum states. Maybe next year for the Higgs…

Comments

1. **Garrett**
   October 8, 2012

   No way — too impersonal.

2. **no clue**
   October 8, 2012

   It's too soon to give a prize for the Higgs. But we'll find out soon enough. I'm willing to make a prediction it won't be Anderson, though.

3. **David Derbes**
   October 8, 2012

   I think Higgs, Englert and a surprise. Woulda been Higgs, Englert and Brout had Brout not died. Might be Anderson, might be out of left field, might just be the two of 'em. Could be Tom Kibble, I guess.

   If the Nobel folks wait till next year, and both Higgs and Englert die before October 2013, it will not reflect well on the committee and hence on the Prize. I'd be happy to see the ATLAS and CMS teams honored, but let's give the old guys their long-awaited prizes while we can, i.e. while they're still breathing. (I'm not impartial about this; Peter Higgs was my thesis supervisor.) I don't think it's too soon; you have evidence from three experiments (including Fermilab) and the damned thing was postulated nearly half a century ago. On the contrary, it may soon be too late.

4. **Bob Levine**
   October 8, 2012

   @David Derbes

   Good point! But Peter's point about the merits of awarding the LHC teams the prize seems persuasive as well. Why can't we have it both ways? The practice behind the Nobel group prizes, in the case of the Peace Prize, shows that such groups constitute a single (corporate) individual for prize purposes. So two individuals plus one group, or one individual plus two groups, should—going by the statement from the Nobel committee spokesman that Peter cited—have the same legitimacy as a Nobel award as giving the prize to three individuals, which they've certainly done in the past.

   So Higgs, Brout (or Higgs/Anderson or…) and the collective LHC experimental team would be a legitimate triple under this investigation. That would answer Garrett L.'s objection, and more importantly, would recognize that this extraordinary discovery has been a collaboration between brilliantly gifted individual theorists and spectacularly skilled experimental teams. It's time that the Nobel committee acknowledged, in their prize-awarding, that it's the irreducible collaboration between both sides of the line that leads to the great breakthroughs in the modern phase of HEP research….

5. **ckm**
   October 8, 2012

   Many Nobel Prizes are controversial (and I refer to physics, not peace). Consider the award to Kobayashi and Maskawa for the CKM matrix, omitting Cabibbo, who was still alive at the time. (The third recipient was Nambu.) That certainly reflected poorly on the committee. So `reflecting poorly' doesn't dictate the committee's decisions.

6. **Avattoir**
   October 8, 2012

   The quotes in the Cosmos piece include some obviously wrong (Brout's disqualified as dead.), and some just cryptic. But I'd put at least even money that our host is wrong on it being split among the three orgs. No way the committee awards for finding the Higgs without at least also honoring it's namesake. Two, more likely one org at most.

   Since it's still "Higgs-like" at this point, why not give all 6 surviving predictors a "Nobel-like" prize?

   Walter Haig's famous comment might pertain here: asked by a reporter why he was still partying it late night before the last day of an Open championship when the leader had gone off to sleep hours previously, Haig said: He may be in bed, but he ain't sleeping. It's just after 3 a.m. in Scotland right now; wonder if Peter Higgs is sleeping.

7. **Thomas Larsson**
   October 8, 2012

   I saw somewhere that only discoveries made before Dec. 31, 2011, can be awarded the prize. Higgs and Englert of course fulfil this criterion, but the experimental discovery does not. We will see.

According to the same reference, a NEW frequenter could be a hot candidate. (Hint: his first name is Peter and his last name consists of four letters).

8. **th-expt**
October 8, 2012

There will be separate prizes for Higgs theory and experiment. This has happened before. Glashow, Weinberg and Salam won for the electroweak theory in 1979, Rubbia and van der Meer won in 1984 for actually producing and detecting the W and Z. Lamb and Kusch won in 1955 for the Lamb shift and electron anomalous magnetic moment (expt), Feynman, Schwinger and Tomonaga won 10 years later in 1965 for renormalization of QED (theory). I believe Nobel prizes were awarded for experimental work on the photoelectric effect (not sure about this), Einstein won in 1922 for his theoretical explanation. Wien won for his expt work on blackbody radiation, Planck won for his theory of blackbody radiation.

9. **Grad student**
October 8, 2012

My money is on Clauser, Aspect and Zeilinger for entanglement. I think particle physics will need to wait a bit.

10. **AJK**
October 9, 2012

I vote for separate prizes for experiment and theory. The question is, how embarrassed would we be if Higgs (and others?) won the prize, and the particle turned out to not be SM Higgs, but some variant. How far off would make the prize seem, in retrospect, like a bad idea?

If what we've seen already is enough, sure, give it out, Higgs has waited long enough. We just need to acknowledge that this isn't necessarily a prize predicated specifically on the discovery of the Higgs, just one that acknowledges the colossal influence on particle physics that Higgs et al. have had, and that this has led to an important discovery which may or may not be SM Higgs.

11. **egan**
October 9, 2012

@grad student

I'm with you on this prediction. Aspect, Clauser and Zeilinger shared the Wolf prize in 2010 and it's often a hint of future Nobel prizes.

12. **BEC**
October 9, 2012

How about we remember that experiemental verification and demonstrated benefit are also part of the criteria. So far they have "Higgs like" particles. Perhaps it might be the year for an experimentally reproduced, novel discovery like that made by Lene Vestergaard Hau and team at Harvard. Stopping light in Bose-Einstein Condensates, turning it into matter, placing information on the copy, storing it, and re-converting it into light are all verified. The applications for quantum computing systems of the future are staggering.

13. **Colin Rosenthal**
October 9, 2012

I'm slightly amazed that Vera Rubin has never received a Nobel.

14. **David Nataf**
October 9, 2012

Peter,

Have you described your rationale somewhere for why the Higgs nobel prize should go the experimental groups that measured the Higgs, rather than the theorists who both postulated the Higgs, and computed how it could in principle be measured?

15. **David Nataf**
October 9, 2012

Colin Rosenthal,

The gravitational effects of dark matter were first inferred in 1933 by astronomer Fritz Zwicky, an eccentric figure who got a lot of things are right but was not as well respected (in his time) as his accomplishments would suggest.

http://en.wikipedia.org/wiki/Fritz_Zwicky

That might be why dark matter has not received a nobel prize. It could also be that the nobel prize committee isn't convinced dark matter is real, they may think modified gravity is still plausible and are awaiting experimental confirmation from "direct detection" experiments.

16. **MathPhys**
October 9, 2012

Minutes to go and I put my money on "Higgs related", with CERN as part of it as well.

17. **jon**
October 9, 2012

Just out: Serge Haroche and David Wineland, quantum optics…

18. **MathPhys**
October 9, 2012

Well, there is an element of lack of imagination here on part of the Nobel committee.

19. **fuzzy**
October 9, 2012

yes i also vote for a prize to the final proof of that model that was proven to be wrong by neutrino oscillations.

20. **Guy**
October 9, 2012

    Oh My God Particle…you weren't chosen this year!

21. **no clue**
October 9, 2012

    It's just a mistake to think that HEP is all of physics. There's a lot more going on in physics. So what if there was a big announcement at ICHEP in July? The Higgs particle has just barely been discovered, basic checks that it is the SM Higgs still need to be quantified. If the theorists are old, that's too bad.

22. **Tmark48**
October 9, 2012

    @ no clue : you can say that aloud that HEP is not all of physics.
    I'm happy that the Nobel prize went to people working in quantum optics. It's kind of sad that most mainstream expositions of "physics" are all about HEP, Strings and other highly controversial (as in not connected to experimental evidence) subjects.
    I'm sure that the HIGGS discovery will be rewarded, it's too big of a deal not to but it will have to wait for a full review on the properties of these so called HIGGS-like particles. We still don't know if what was discovered was a SM HIGGS at all. So good for the Nobel Committee not bowing down to hype.

23. **Peter Woit**
October 9, 2012

    David Nataf,

    The argument for priority of an experimental Higgs award over a theory one is mainly that the relevant theory one has already been given (to Weinberg/Salam for electroweak unification using Yang-Mills + Higgs mechanism). It's the Weinberg-Salam Lagrangian, extended to include quarks and QCD that determines the behavior of the predicted SM Higgs. There has been a lot of discussion about exactly who among the people who worked on the "Higgs mechanism" back in 1963-4 deserves a Nobel for it, but that work was on Abelian models pretty far from what is being seen now at the LHC (if it really is a a Higgs…).

    All in all, I think HEP theorists have a very high profile and get more attention than they deserve. In this case the huge recent development is the discovery of a new particle by the LHC experimentalists, in a technical tour de force. That deserves a Nobel if anything in HEP does. Maybe it is too recent, so needs to wait a year to satisfy the Nobel rules about that. Maybe the lack of specific people to give the prize to is an impediment that can't be overcome.

    In any case, the actual award is an interesting sign of the times, with attention in physics moving from HEP to techniques for manipulating quantum systems at a fundamental level.

24. **Phil**
October 9, 2012

    Leaving GHK off will be controvery enough in 2013. Politically too many people in Europe are pushing for Englert and CERN to now snub them. If they can award it to CERN they can modify the statutes to ensure all the theorists get recognized as well. 1964 PRL paper quality (GHK) should be considered at least as much as submission dates (Englert) and boson naming (Higgs).

25. **El-Coco**
October 9, 2012

    Every year Guth doesn't win the prize moves ever closer to being absolutely meaningless.

26. **Tmark48**
October 9, 2012

    @ El-Coco : didn't Alan Guth just win Milner's Fundamental Physics Prize ?
    I'd say we was rewarded handsomely for his theoretical breakthrough.

27. **Zathras**
October 9, 2012

    El-Coco,

    Nobel Prizes are almost always given for ideas that have both strong experimental evidence and a firm foundation in theory. Inflation certainly has the experimental evidence, but there is not enough of a firm theoretical underpinning of inflation for it to qualify.

28. **no clue**
October 9, 2012

    I have a question to the assembled company: has the recently found particle at 125 GeV been verified to have spin zero?

29. **Peter Woit**
October 9, 2012

    no clue,

    It must have even spin, has not yet been shown to have spin zero. Showing this is one target of ongoing analyses. No one really believes though that there is much likelihood it is spin two or higher.

30. **Pravda**
October 10, 2012

    "If there really is nothing but custom to keep them from awarding the Nobel this year to ATLAS, CMS, and CERN for the Higgs, I can't see a better occasion to break with the custom…"

What could be more inspirational than such an award going to huge corporate entities composed of many thousands of people, and which spent billions of dollars?

Anything.

31. **unpravda**
October 10, 2012

That makes no sense at all. The billions are well spent, not squandered. It makes far less sense to award the prize to spokespersons/project leaders. Those positions may even rotate … why are only the spokespersons deserving of Nobels?

But I was surprised by the statement. At at least for science there is a custom to award the prize to individuals, not organizations. It is only the peace prize, I believe, where awards have been made to entities such as the International Red Cross etc. Actually the will of Alfred Nobel stipulated only one individual for the awards.

32. **Colin Rosenthal**
October 12, 2012

"It could also be that the nobel prize committee isn't convinced dark matter is real, they may think modified gravity is still plausible and are awaiting experimental confirmation from "direct detection" experiments."

I'm not sure that that is particularly relevant. According to wikipedia, Rubin herself would prefer a modified-gravity explanation, but even if such a thing were possible it wouldn't make Rubin's observational analysis less important. "Sorry we can't give you a nobel because you've only overthrown GR"??

(The Zwicky question is a whole different thing. But I don't think a prize for Rubin could possibly be _more_ controversial than some of the other strange choices and omissions over the years.)

Post-discovery Higgs Books

October 11, 2012
Categories: Book Reviews

The Higgs particle has been the main player in various popular books about particle physics since before many of today's college students were born, with Lederman and Teresi's The God Particle going back to 1993. Last year's excellent The Infinity Puzzle by Frank Close (discussed here) was largely about the Higgs story, appearing just before the first experimental indications of the Higgs late last year.

I'm not the only one who was obsessively following the Higgs discovery story as it unfolded from last year until the final announcements this past July 4. Two of the others have already produced books on the topic: Jim Baggott's Higgs: The Invention and Discovery of the 'God Particle' came out last month, Sean Carroll's The Particle at the End of the Universe: How the Hunt for the Higgs Boson Leads Us to the Edge of a New World, will be in stores next month.

I've just finished reading copies of both of them, and they're both very good. They each cover the story of the Standard Model well, supplemented by extensive discussion of the Higgs discovery at the LHC and the background of how it came about. I confess that it's a bit eerie to see a lot of things that were day-to-day news and often grist for the blog now packaged between hard-covers as history, while I'm also happy to see that a good job is being done of it.

Baggott's book is quite a bit shorter, and has much more of a linear structure, taking the reader historically through the development of the Standard Model and its experimental tests, up through the LHC and the work of CMS and ATLAS that led to the Higgs discovery. You also get as a bonus a wonderful foreword by Steven Weinberg who, among other things, explains why quarks were not in his 1967 paper (he didn't believe in them). If you want the quickest possible journey through this material, definitely choose this one.

Carroll's effort is much longer, more non-linear and digressive. You get a significantly more in-depth version of parts of this story, with an organization that starts with the discovery and works outwards, explaining various topics needed to understand what this is all about, rather than following the line of historical development. I'm not really a good person to judge how this will work for those approaching this subject without a lot of background, but it seems to be as good a way as any to get readers into the subject. Among the topics Carroll has the space to cover in depth, he does a very good job with the history of the Higgs mechanism and the various claims to have done something Nobel-worthy, including the crucial role of Philip Anderson that often gets overlooked.

Since I'm known for my negativity, I'll add a few criticisms here of these otherwise excellent books. Baggott goes with the "God Particle" business in his subtitle, presumably for the same excellent reason that Lederman used it: anything with "God" in it sells more books. One of Carroll's digressions is about the "God Particle" business (he's strongly against it and God in general) but his "Particle at the End of the Universe" replacement doesn't seem to me to be much of an improvement. His subtitle and some of the jacket copy ("a doorway is opening into the extraordinary: the mind-boggling world of dark matter and beyond") oversells a topic he wisely devotes no more than a couple pages to in the text of the book, so-called "Higgs portal" models of dark matter.

Both authors write fairly extensively about the role played by bloggers in spreading news and rumors during this recent period, and I make an appearance in both books. This caused me to go back and recall some of the details of how this played out. Both Baggott and Carroll describe how the abstract of an internal ATLAS note was anonymously posted on my blog (see here). Carroll's version is slightly inaccurate, implying the entire memo was posted there, while it was only the abstract. The full note was sent to me privately by people who wanted me to have a better idea of what was going on, but I did not post that on the blog, and reading the full note made it pretty clear that while this wasn't a hoax, it also wasn't worth taking seriously. My impression is that the outraged reaction from various people at ATLAS to this leak was equal parts justifiable concern about keeping this kind of material confidential, and embarrassment about something this dubious seeing the light of day with the name of their collaboration on it. The unfolding of this story gave me a lot to think about, and I ended up deciding that I was definitely not comfortable being a public source of actual confidential documents, while at the same time seeing nothing wrong with providing accurate summary accounts of what the 3000 people in one of these large collaborations were all aware of and discussing. I offered to ATLAS people to remove the abstract if they asked me to, they decided it was best not to do this.

The first solid evidence for the Higgs that I heard about was in late November 2011 (see here and here), with a comment giving the right mass appearing at viXra log convincing me that the time had come to go public with some details. What was remarkable about this evidence was not that something was being seen at the 2-3 sigma level, but that both experiments were seeing something at almost the same mass value. This immediately convinced me that this was likely to be a Higgs signal, and the further details that came out over the next days up to the public announcement December 13 made for a rather strong case that the Higgs had been found.

In some sense the news this past summer was anti-climactic, just confirming that the strong 2011 evidence was the real thing. In early June news came from ATLAS that they were seeing the same gamma-gamma signal as in the 2011 data, just before I left for vacation (see here). When I got back from vacation, a lot more details showed that both experiments definitely had the thing in the bag. My posting about this got a lot of attention, including a link from the New York Times (where Dennis Overbye reported that Fabiola Gianotti of ATLAS was telling him "Please do not believe the blogs").

All in all, I'm fairly happy with my decisions about what to write and what not to write on the blog about not-quite-public results about the Higgs. There's been a certain amount of criticism about the terrible violations of confidentiality involved, but I can't help pointing out that the things I was writing about were at the time known to the majority of the HEP community: the 6000 physicists on ATLAS and CMS. Carroll has this to to report about the confidentiality question:

> I asked one physicist whether the results that ATLAS was getting were generally known within CMS, and vice versa. "Are you kidding?" I was told with a laugh. "Half of ATLAS is sleeping with half of CMS. Of course they know!"

For that quote, and many other stories worth reading about, if you're the sort who loves popular books about particle physics, both of these are worth buying. If you're only moderately interested, just pick one of the two and read it, you can't go wrong…

Comments

1. **hhgttg**
   October 11, 2012

   You may or may not know this (you do not say so explicitly) but "Particle at the End of the Universe" is taken from the Hitchhikers Guide to the Galaxy. Milliways is the "Restaurant at the End of the Universe".

2. **Peter Woit**
   October 11, 2012

   hhgttg,

   Yes, I was aware of that. Maybe my problem with it just has to do with not being much of a Douglas Adams fan…

3. **Robert Rehbock**

October 12, 2012

If one person knows something they do not share with others, that is secret. If two persons know something that neither share with others, that is a confidence. If three persons know something that none of them share with others, that is unlikely.

4. **BJM**
October 12, 2012

What about "Higgs Discovery", a short book by Lisa Randall?

5. **Peter Woit**
October 12, 2012

BJM,

I think Randall's book is rather different. It's very short: a couple chapters from her other books about the Higgs with a new chapter describing the story of the discovery. As far as I know, it's only available as an e-book.

6. **ed hessler**
October 12, 2012

I'm a K-12 science educator who reads NEW almost daily, not always understanding everything or expecting to or believing that it is your job. Today's post is one of many that explain why why I read it and look forward to it on nearly a daily basis. This thanks is long, way-long overdue but it is about time I said that. I also appreciate the way you keep the comments focused.

Cheers.

7. **Peter Woit**
October 12, 2012

Thanks Ed,

I hope there will be continue to be news as exciting as the past year's to cover here in the future, probably too much to expect though…

8. **Philip**
October 12, 2012

As long as that news isn't evidence for SUSY or string theory, right Peter?

9. **Peter Woit**
October 12, 2012

Philip,

Well, if the LHC finds SUSY or strings, people at ATLAS and CMS will likely send me the news and you'll read about it here first. More likely though, further strong evidence against SUSY (evidence against strings isn't possible, for obvious reasons…) will continue to accumulate, and that will get covered here, even if ignored elsewhere.

10. **Owen Patterson**
October 12, 2012

So now that the Higgs has been found, now what for this branch of science?

11. **Peter Woit**
October 12, 2012

Owen Patterson,

From the experimental side, investigate the properties of the Higgs. From the theoretical side, the LHC is killing off a wide range of bad speculative ideas. So, give up on those and find something better…

12. **B**
October 12, 2012

"…now what for this branch of science?"

writing books, apparently.

13. **paddy**
October 12, 2012

Kudos PW. Your site did indeed provide the best objective real time coverage of the Higgs search story this last year or more. If folks wish to equate "objective" (i.e., a skeptical no agenda approach) with "negative", then they have some self introspection to do. Thank you again for your insights.

14. **cormac**
October 12, 2012

Hi Peter, I read Baggot's book last week and I have to say I was a bit disappointed. It starts very well and has a strong theoretical basis, but I think there a lot of abstract theory for the lay reader.
Of course, it's very hard to tell the story of the SM without quite a bit of quantum theory. My solution in talks and suchlike is to concentrate on phenomenology, and tell the story of the discovery of particles, with only a little on qft. I get criticized for this (see current comment on blog), but I'm pretty sure the layman won't understand much of Baggot's book (my students didn't)

15. **Peter Woit**

October 12, 2012

cormac,

The problem is that the basic idea of the Higgs mechanism (spontaneous breaking of a gauge symmetry) is inherently an abstract and tricky one. You're probably not going to be able to communicate much about it to a lay reader, but if you just completely avoid it, you've written a book about the Higgs without actually explaining anything about what it is. You can't win here, but I think it's often better to leave readers aware that they don't really understand than to tell them a story which convinces them they have understood something that they haven't been told anything about.

16. **cormac**
October 12, 2012

Yes, I'm inclined to agree, I don't accept the mantra that the reader has to understand everything. However, I read Jim's book just before giving a talk on the subject myself, had the feeling it's more conscious of readers like you and me than a lay audience.
I suspect a lot of readers won't get past the half-way mark, but what do I know? It's certainly succinct, thank God for a release from the obligatory anecdotes!

17. **cormac**
October 12, 2012

P.S. Re ' completely avoid it', I'm not advocating such a thing, of course you can't tell the story without some reference to e-w symmetry breaking, My point is one of balance; I suspect Jim's book, like your own, has a little too much gauge theory for the lay reader, but that's entirely subjective!

18. **ScentOfViolets**
October 13, 2012

> The problem is that the basic idea of the Higgs mechanism (spontaneous breaking of a gauge symmetry) is inherently an abstract and tricky one. You're probably not going to be able to communicate much about it to a lay reader, but if you just completely avoid it, you've written a book about the Higgs without actually explaining anything about what it is. You can't win here,

In reply to Cormac, this sort of thing happens all the time when it comes to explaining even basic physics to the lay public. A case in point is the notion of negative temperature. In a quick exposition you can get the idea across that temperature is basically just how fast things move on average, both because it's easy to make little pictures in your head about what this means and because somehow it "makes sense". But to try to explain temperature as the change in heat wrt to the change in entropy? Try to communicate that intelligibly to a lay audience in a few paragraphs or pages.

Some stuff really is hard to explain, and it's not just that people who do this sort of thing for a living making a poor job of it. Hmmm . . . did Asimov ever make a stab at this. Be interesting to see how the Great Explicator did it.

19. **paddy**
October 13, 2012

On explaining the "Higgs mechansim" to laity: I've always been sure this is straightforward…right up to the point I try to do it. As an example of someone far better equipped than I attempting this, I note Matt Strassler's careful building up of a series a lecture articles (which are quite good) and which in the end should culminate in a "voila!" moment. Unfortunately, I suspect that said laity have long since lost the train of thought. Tomas Dorigo on the other hand (though likewise not successful yet) is struggling with finding the right analogy/metaphor for, if not the Higgs mechanism, then the concept of "natural". During a pedagogical explanation (such as our education and MS's approach) comes the problem that said laity are not students and may lose the train of thought. With a metaphorical approach comes a constant "not quite right" issue.

20. **Bernhard**
October 13, 2012

"From the experimental side, investigate the properties of the Higgs. "

Yes, well, the problem with this is that not all experimentalists can jump on this at the same time. Higgs was already a crowed subject in collaborations before the discovery. The Higgs properties must and will be measured, no danger there, but I guess this question really depends on the career stage you are. For very young experimentalists, jumping this Higgs crowd can perhaps lead to a job, if the person is very qualified, but I would think people entering the field dream with a bit more than that (in your mid-thirties that is probably your only dream, but anyway…).

21. **Igor Khavkine**
October 14, 2012

@ScentOfViolets: I think negative temperature can be disposed of in just a few lines of explanation. By historical accident, we've been using the "wrong" variable T (temperature). The "right" variable is $\beta = 1/T$ (coldness). The second law of thermodynamics then states that thermal energy spontaneously flows only from low coldness to high coldness (valid over both positive and negative ranges). So, thinking in terms of negative β immediately explains some puzzling properties of negative T.

@paddy: Perhaps the difficulty in finding a common sense (yet non-fallacious) analogy/metaphor for naturalness is a signal of the actual unnaturalness of the concept.

22. **Spencer Tracy Jr.**
October 15, 2012

Has anyone seen Leonard Susskinds Higgs Lecture: http://www.youtube.com/watch?v=JqNg819PiZY
or the Cal Berkley Higgs lecture: http://www.youtube.com/watch?v=jchDY6xuiZ0&feature=related

23. **fuzzy**
October 15, 2012

paddy, Igor
i would be content with a physics explanation of naturalness, even if it were remote from common sense. i mean, something having to do with measurable stuff, rather than with mathematical constructs. in the last 20 years, i saw nothing like that, just the same lame arguments.

24. **paddy**
October 15, 2012

fuzzy,
I could probably blather on about your point…but in the end I agree with you.

25. **Tammie Lee Sandoval**
October 17, 2012

Dear Igor Khavkine

Thank you for that wonderful thought, that 1/T is “coldness”.
I teach high school physics
We have a thermodyamics seminar, and I’m going to use your idea.

26. **Chris Oakley**
October 17, 2012

1st law of thermodynamics: You can’t lose
2nd law of thermodynamics: Yes, but you can’t win, either
3rd law of thermodynamics: Except at absolute zero, and you can’t get that

27. **Igor Khavkine**
October 17, 2012

@Tammie, I hope you won’t use my blog comment as an authoritive reference! You may want to look up Appendix E in Kittel & Kroemer’s *Thermal Physics* textbook for a more detailed explanation of negative temperature along these lines.

Yet More Links

October 14, 2012
Categories: Uncategorized

- From commenter Clark here, news that Mochizuki has acknowledged that the problem pointed out by Vesselin Dmitrov with his proof of the abc conjecture on MathOverflow is a real one, but claims that the argument can be fixed, with fixes that he explains here. He is preparing updated versions of the papers containing flaws.
- As new negative results about SUSY keep coming in, Nanopoulos et al. issue new "predictions" of a SUSY signal just around the corner. In light of this from ATLAS, here's an updated list of "best fits" for SUSY (first posted here)

  arXiv:1007.5100 455 GeV ("Golden Point")
  arXiv:1009.2981 455-481 GeV ("Golden Strip")
  arXiv:1111.0236 512 GeV ("Universe F-U2")
  arXiv:1111.4204 518 GeV ("Profumo di SUSY")
  arXiv:1203.1918 610 GeV ("Aroma of Stops and Gluinos")
  arXiv:1205.3052 708 GeV ("The Sweet Fragrance of SUSY")
  arXiv:1210.3011 756 GeV ("Primordial Synthesis")

  The authors have stopped going on about how this all smells, but now are acknowledging help from Tommaso Dorigo (see here).

- Tomorrow at Boston University there will be a conference on quantum gravity, celebrating the 40th Anniversary of the First Osgood Hill Conference on Quantum Gravity.
- The Higgs continues to get lots of positive media attention. This week's episode of The Big Bang Theory was called The Higgs Boson Observation, and features lots of Higgs-related things on the blackboards.

  At the IAS in Princeton, Yuri Milner's multi-million-dollar men are giving public talks about the Higgs. Last week was Juan Maldacena on The Symmetry and Simplicity of the Laws of Nature and the Higgs Boson, next week it will be Nima Arkani-Hamed on The Inevitability of Physical Laws: Why the Higgs Has to Exist. At some point these talks may appear here.

- On Friday the Templeton Foundation handed out $5.6 million as part of its New Frontiers in Astronomy and Cosmology competition, some to students for writing essays, most of it to physicists and astronomers, many of whom promise to find ways of testing the Multiverse (grant winners are here). Intriguingly, David Spergel is not just "testing" the Multiverse, but "detecting or falsifying" it, I wonder what that's about.
- Lots of self-examination going on in the US HEP community about what to do post-Higgs discovery. Argonne had an HEP Higgs Retreat (no slides for "SUSY is Dead?" it seems). The past few days at Fermilab there was a DPF Community Planning Meeting, organizing activities to lead up to next summer's "Snowmass" Community Study , to be held in Minneapolis. What's long overdue but unlikely to happen would be a US Community Study of the implications of the SUSY/string theory fiasco for HEP theory.
- The Calculus of Love is a short film with a math theme involving the Goldbach conjecture.
- For a debate about Pythagoreanism, the idea that math is the key to the universe, see here. An interesting debate, but maybe they should have had some mathematicians involved…
- Last week I was up in Boston and went to some of the talks at a conference in honor of Daniel Quillen, who passed away last year. Quillen's remarkable and influential work was at the boundary of topology and algebra, in particular he was largely responsible for discovering how to properly define algebraic K-theory. An early version was distributed of material about Quillen that will appear in the November Notices of the AMS, including a long explanation by Graeme Segal of the high points of Quillen's mathematical contributions (*Note added: this is now available here*). I found the talks by Segal and Hopkins both inspiring and baffling, with Hopkins in particular starting off slow and comprehensible, but reaching escape velocity by the time he got around to what sounds like an exciting new result about the Brauer group in the context of stable homotopy theory. This is joint work with Lurie and Lieblich, but you're going to have to find someone other than me to explain it to you.
  This coming week I'll be in Toronto for the Fields Medal Symposium, which will cover all things Langlands. The opening public lectures will be live-cast, see here.

Comments

1. **Dr. David Edwards**
   October 15, 2012

   The November Notices of the AMS is now available online:
   http://www.ams.org/notices/201210/

   Daniel Quillen
   http://www.ams.org/notices/201210/rtx121001392p.pdf

2. **Shantanu**
   October 15, 2012

   Peter, I didn't see you report anything about the talks from Stephen Hawking's 70th birthday symposium earlier this year (unless I missed them) There are a few talks on string theory there. http://sms.cam.ac.uk/collection/1225546
   shantanu

3. **Peter Woit**
   October 15, 2012

   Thanks Shantanu, somehow I missed that one…

4. **thomas**
   October 15, 2012

   f a li and a maxin and a walker and a nanopoulos

   Total number of papers analyzed: 26

   Total number of citations: 356

   excluding self cites: 67

5. **chorasimilarity**
   October 15, 2012

   Mathematicians (mathimatikoi – learners) are the descendents of a pythagorean sect. However, the present situation is complex: most mathematicians don't care much about philosophy, most philosophers are mathematically blind, hence somewhat clueless concerning pythagoreanism, with physicists in the middle position.

   There are exceptions. Here is a cite by Bateson ("Form, substance and difference", 1970):

   "In this history, there has been a sort of rough dichotomy and often deep controversy. There has been a violent enmity and bloodshed. It all starts, I suppose, with the Pythagoreans versus their predecessors, and the argument took the shape of "Do you ask what it's made of – earth, fi□re, water, etc?" Or do you ask "What is its pattern?" Pythagoras stood for inquiry into pattern rather than inquiry into substance. That controversy has gone through the ages, and the Pythagorean half of it has, until recently, been on the whole submerged half."

6. **paddy**
   October 16, 2012

   thomas,
   Well said. And their latest article reads like gobbledigook. Any paper that elicits "bayesian priors" when considering the "truth" of its ascertion is pathetic.

Templeton Funds Physics of Information

October 19, 2012
Categories: Uncategorized

FQXi has recently issued a Request for Proposals, using money from the Templeton Foundation to fund about $3 million in grants for research on the "Physics of Information":

- What is the relationship between information and reality? Can information exist without any "material" substance? Can matter exist without any information? Or, are information and reality two sides of the same coin?
- How does nature (the universe and things therein) process information? Are there fundamental limits? How is nature shaped and transformed by processing information?
- What are the fundamental differences between classical and quantum information?
- What can the physics of information reveal about black holes, singularities, physics at the Planck length, and the origins and fate of our universe?

This follows on the heels of $4 million in grants announced last week on the topic of New Frontiers in Astronomy and Cosmology.

No Comments

Fields Medal Symposium

October 19, 2012
Categories: Langlands

I just got back from a few days in Toronto, where I attended the [Fields Medal Symposium on Fundamentals of the Langlands Program]. This is the first of a planned yearly series to be held a the Fields Institute, with the idea that each Symposium will focus on an area of mathematics crucial to the work of one of the recent Fields medalists. In this one, Ngô and his work proving the Fundamental Lemma in Langlands theory was the center of attention.

The talks were recorded, and I believe that video of them will soon appear. An effort was made to get speakers to give talks aimed at non-specialists, and the results were quite good. Among the talks I attended, I can highly recommend those of Sophie Morel, Edward Frenkel, Nigel Hitchin and Edward Witten, which covered some of the huge diversity of fundamental mathematics that goes into this subject. Unfortunately I only got to Toronto midday Tuesday, so missed all the Monday and Tuesday morning talks. I heard that the Tuesday morning talks of Richard Taylor and Michael Harris gave excellent introductory surveys on the number-theory Langlands program. Monday was devoted to more specialized talks on endoscopy and the fundamental lemma.

Ngô's talk was about some new ideas on how to go "Beyond Endoscopy", to extend previously successful uses of the trace formula to prove Langlands functoriality to a wider range of examples than those covered by the fundamental lemma. Another example of this sort of ongoing work mentioned by a couple speakers was work by Ali Altug, who has just finished up as a student at Princeton and started teaching here at Columbia this fall. Witten's talk surveyed the relationship between QFT and geometric Langlands, motivating clearly why the N=4, d=4 SYM theory appears. For more details about much of the more advanced material covered in his talk, see the write-up here of his lecture at Atiyah's 80th birthday conference.

Monday evening there was a big evening program for the public (which I watched some of from New York via web-cast), and Tuesday evening there was a special program for high school and college students, with Ingrid Daubechies and Frenkel giving talks, as well as a panel discussion with them, Ngô and James Stewart. A lot of students attended, and many stayed on for almost an hour to talk with the speakers. Ngô has a popular book out in Vietnam, which evidently has been a huge success. Frenkel has a book entitled *Love and Math* coming out next year, a chapter of it is available here.

Panelist Jim Stewart has them both beaten as a successful author. His excellent Calculus textbook may be the most widely-used one in the US, and evidently the financial rewards have been significant. He was one of the financial supporters of the symposium, and Wednesday night had many participants out to his amazing home in Rosedale for a banquet. It's a spectacular, award-winning piece of architecture he calls "Integral House", and its five stories and 18,000 square feet of space are perched over a ravine not far from downtown Toronto. Evidently it cost him about $24 million, as well as about ten years of his life in design and execution. For more about Stewart and Integral House, see here, here, and here.

Richard Cerezo was taking lots of pictures and has been posting on the Symposium blog here. A short video of me, Frenkel and Hitchin discussing the Symposium topic may appear there at some point.

Update: The conversation with Frenkel and Hitchin is now available here.

Comments

1. **Jeff M**
   October 20, 2012

   Peter,

   Sounds like fun, not that it's even vaguely related to what I do. On another note, Stewart's calculus excellent, really? I mean it's not too bad for a modern calculus book, but it's way too long, with all sorts of useless stuff in it, and it doesn't emphasize the important stuff nearly enough. I guess the "proofs," such as they are, are better than many other current books, since from what I remember they aren't actually incorrect, just often sloppy and incomplete. It's fine to have an incomplete proof, but if you do that you should say "this is meant to give you an idea of how the actual proof works." I guess that most of this isn't Stewart's fault, especially at this point, but still. You want a truly great calculus book, use Spivak.

2. **Bob Jones**
   October 20, 2012

   Yes! Spivak's calculus book is probably one of the greatest math textbooks ever written!

3. **Peter Woit**
   October 20, 2012

   If one wants a proof-based calculus book, yes, Stewart's not the way to go. But, I'd really rather not host a discussion here of Calculus books, unless it's somehow closely related to the Langlands Program, which is the topic of the posting….

4. **Jeff M**
   October 20, 2012

   Peter,

   OK, Langlands program. A quite well known mathematician once told me that he hated getting reference letters from Langlands, since all they ever talked about was how the candidate had advanced the Langlands program 🙂

5. **Richard Cerezo**
   October 21, 2012

   Hi Peter,
   Thanks for the post. I'm so glad that you enjoyed the symposium and that we were able to have some conversations. I'm glad to see that your blog pops up among the first few when I look for coverage of this FMS! There were quite a few reporters there on Monday evening and hopefully I can find something in English as most of them were Vietnamese news reporters.
   Unfortunately I missed Sarnak's talk and I heard that it was also very nice.
   As an aside, Spivak's Calculus is really quite phenomenal for a proof course, but as far as I know he didn't have that very impressive wall of his own books!

6. **Peter Woit**

October 21, 2012

Richard is referring to this

http://thestar.smgmedia.topscms.com/images/c6/0d/c868f86e476a931df0f06e7c012c.jpeg

Guests at Stewart's got to see his huge bookcase of Calculus and other course books, which this is a part of, the part containing his own books..

7. **Paul**
October 22, 2012

Peter,

I have an undergrad math background, but I know almost nothing about the Langlands program. Not to sound sarcastic, but what's so special about this topic anyway? I took a look at its Wiki page, but I couldn't quite understand it. And what applications (if any) does it have to physics?

8. **Peter Woit**
October 22, 2012

Paul,

The public program talks tried to give some motivation for the Langlands program, I think they're online. Richard Taylor's talk I think was also pretty general, should be online at some point.

To oversimplify dramatically, the Langlands program grew out of number theory, where it is a basic insight into the structure of numbers (relating the Galois group to Lie groups). The proof of Fermat's last theorem is based on it. It also has some geometric analogs, known as Geometric Langlands, and this geometry has turned out to be related to certain very special quantum field theories. Witten's talk covered that, but it's a long and rather complicated story.

9. **Richard Cerezo**
October 25, 2012

Thanks for posting the link! What did you think of the video? I have picked up a very nice book dedicated to Hitchin called 'The Many Facets of Geometry: A tribute to Nigel Hitchin'. It has a very nice article written by Witten on the Geometric Langlands! I will include my comments on it when I write about Hitchin's talk.

10. **Peter Woit**
October 26, 2012

Hi Richard,

I thought the video of us came out very well, thanks for arranging it and producing the great video.

The Witten article you mention is a nice review article, written earlier on and probably worth reading before the one that I mentioned, which corresponds more to his talk at the conference.

Looking forward to videos of the rest of the talks, I hope we get to see those!

11. **plm**
October 29, 2012

Thanks alot Peter for the link to Edward Frenkel's story on jews' treatment in 1980s USSR. It is fantastic -the recounting. I always wondered how Frenkel could develop the way he has, he is quite a unique mathematician.

12. **Bryan**
November 7, 2012

Anyone have a link to the talks?

13. **Peter Woit**
November 7, 2012

The talks are available here
http://www.fields.utoronto.ca/video-archive/event/108

14. **Bryan**
November 8, 2012

Thanks Peter! I know what I'm doing for the next 10 hours . . .

Why Author Pays Open Access is a Bad Idea

October 20, 2012
Categories: Uncategorized

There's a wonderful piece of software out there I hadn't heard about, called Mathgen, which generates impressive looking mathematics research papers that are utter gobbledygook. A Mathgen paper on *Independent, Negative, Canonically Turing Arrows of Equations and Problems in Applied Formal PDE* was recently accepted (see the full story here) by the journal Advances in Pure Mathematics, one of many "open access" journals put out by Scientific Research Publishing. If you're looking for theoretical physics papers instead of pure math, Scientific Research Publishing has the Journal of Modern Physics. Some work on Mathgen is probably required before it is ready to submit papers to this journal.

These journals charge authors $500 to publish their papers, something which is now being sold as a wonderful mechanism for providing "open access" to the scientific literature. At the same time they make very clear what one big problem with this is: the financial incentive for the journal becomes to publish as many papers as possible, since that's the only way to increase revenue. Scientific Research Publishing does a good job of showing where this model for funding dissemination of academic research leads.

Comments

1. **Matt Leifer**
   October 20, 2012

   The fact that there are some unscrupulous open access journals with an author-pays business model does not automatically mean that the author-pays model is bad. These are extremely low-prestige journals that everyone already ignores. The only people likely to get duped are faculty hiring committees who simply count the number of publications, but, even then, I think that the likelihood of them not noticing that the publications are in junk journals is pretty low.

   On the other hand, there are some extremely good open access journals with an author-pays model. The New Journal of Physics is one such. It is currently ranked higher than most Physical Review journals, apart from Physical Review Letters, in almost every relevant metric (e.g. impact factor, average citation rate, number of paper downloads, etc.). Apart from PRL, the only journals in physics that do better are those published by the Nature group and Science.

   I agree that there is currently a kind of scam going on where a lot of new small open access have opened up with the aim of making as much money on author fees as possible with no regard for quality. I get emails from such publishers all the time, as I am sure many other academics do. However, it is pretty obvious to everyone that these are scams and we know who are the reputable publishers in our own fields. For those who do not know, it is fairly easy to look up journal rankings to see which are the good journals. Therefore, I don't see that there is a problem.

2. **Peter Woit**
   October 20, 2012

   Matt,

   Thanks for the comments. I agree of course that not all author pays journals are currently problematic. But I do think there's an inherent problem with that funding model. Under the old libraries pay, costs everybody more to print more papers model, there was a strong incentive to keep numbers of papers published small, and quality high. If quality went down, librarians would start canceling. Under this new model, the financial incentive is all towards accepting more papers. Those editing and running the journals may have other incentives that encourage them to keep standards high, but those may not win out in the long-term over the financial ones. I'd be curious to know if there are journals using the author pays model that recognize this as part of their charter and have put mechanisms in place to counteract it.

3. **Heikki Arponen**
   October 20, 2012

   Pretty much the only thing the author (or institution etc.) should pay for these days is the peer review, since most work is already done by the author himself… and maybe the peer reviewer should get a cut too!

   By the way… Peter, please consider joining Google+. This story has been circulating there for quite some time already! It's also a nice way for you to connect better with your audience.

4. **Toma Susi**
   October 20, 2012

   Peter, even though I agree that there are definitely problems along the lines you say, from what I've gathered of the current bundled subscription model, libraries don't really have any choice to cancel low quality journals, since partial subscriptions are priced outrageously high. The current system really doesn't work. I agree with Matt's comment, cases like these are easy to filter out and are not the real problem.

   I'll also emphatically second Heikki's request: Google+ **really** is worthwhile.

5. **john McAllison**
   October 20, 2012

   The obvious solution is for authors to pay for the journal's referees to assess the paper for academic importance sufficient for publication.

6. **Bob Levine**
   October 20, 2012

   "The obvious solution is for authors to pay for the journal's referees to assess the paper for academic importance sufficient for publication."

   But at the moment, the cost of refereeing for academic journals, so far as the journals themselves are concerned, is nil. Refereeing is typically viewed as a professional responsibility by academic departments, and having a sheaf of refereeing assignments from blue-ribbon journals of record is regarded an important indicator of one's status as an expert in the field, for P&T purposes, in pretty much every discipline I know about. As long as this continues to be common policy in academe, I don't see why authors ought to pay for refereeing on the open access model any more than they do on the commercial/university press journal model. I do a lot of journal refereeing and I wouldn't expect to be remunerated for my review work regardless of the journal format (though I'd be much happier doing work on behalf of an open-access venue than a subscription/paywall-protected venue…)

7. **Peter Woit**

October 20, 2012

Toma,

I agree that the current system is broken. My point is just that I see a lot of promotion of wonderful new "open access" models which sound great until you think a bit about it and see the problems. Looking at this case I think helps to make a problem clear, exactly because it is so extreme. If this is happening, are you really so sure that the financial incentive to publish more papers is not affecting at all how other journals behave? One may argue that whoever is behind Scientific Research Publishing is some guy who is just out for a buck, but a lot of the new "open access" being promoted is for journals run by Elsevier and the like. Can one rely upon them to ignore financial incentives and keep up standards?

About Google+,

I haven't gotten involved in that partly because I spend too much of my life on the internet, so avoiding all social media seems like a good idea to the extent possible. Maybe I'll find out there's some efficient way to get information from Google+ without wasting a lot of time and change my mind. But, another thing I think we all need less of in our lives is Google, I've seen enough of it recently to find it a very scary organization.

8. **Mark Hillery**
October 20, 2012

First, let me say that I am an associate editor of Physical Review A, so anyone reading my comments might want to keep that in mind.

I do not like the author-pays model. Presumably the money to pay the publication charges is supposed to some from a grant. Theoretical physics and mathematics are not particularly well-funded, so, in my experience, sometimes people have grants, and sometimes they don't. However, in these fields it is quite possible to do research without a grant, so if you have something you want to publish, and you don't have a grant, you will not find the author-pays model particularly agreeable. Personally, when I do have grant money, I would much rather spend it on a graduate student than on publication charges.

In regard to refereeing, yes the referees provide their services for free, but someone has to manage the process, and for that you need permanent people whom you have to pay. You can't run a journal with a real peer-review process for nothing.

Finally, I just do not understand the attraction of publishing a physics paper in a journal, such as the New Journal of Physics, that has a publication fee. If you want your paper to be freely available, you can publish it in the Physical Review for no fee, and put the paper on the archive. By the way, it is probably not that well-known, but if you publish your paper in the Physical Review and you want to make it freely available (that is available to people without a subscription), you can pay a fee and do so.

9. **Peter Woit**
October 20, 2012

Thanks Mark,

I should also mention some history which most readers are probably too young to know about. Until the late 60s-early 70s, the APS had the dominant (at least in the US) HEP journal Phys. Rev. D, which had a form of "author pays", called "page charges" (these charges didn't make the journal free, but subsidized it and kept subscription prices low). The European Elsevier journal Nuclear Physics B had no such page charges and from the early 70s on, this was one of the main reasons it became the journal of choice for authors to publish in, and Phys Rev D fell by the wayside in terms of quality. This is why the bulk of the best work in the field for several decades now belongs to Elsevier to exploit as it sees fit, rather than to the physicist-controlled APS.

10. **Sterling Clover**
October 20, 2012

Elsevier and other publishers of traditional journals have hardly done any better: This and this for example. (And there's plenty more where those came from).

Publishing nonsense has a strong economic incentive even without author-pays as long as you can push it into "bundled" journals that libraries are forced to buy.

11. **Low Math, Meekly Interacting**
October 20, 2012

Gotta go with Sterling. Perverse incentives abound (I mean, cripes, just look at all the retractions in bio lately), and I don't see any way to avoid them completely. I'd never argue that the author-pays model is perfect, but somebody has to pay something somewhere, and this model guarantees that even the poorest researcher can gain ready access to peer-reviewed literature. No pay-walls. No needless overhead. No bundling. It would be wonderful if the arXiv solved all problems, but reportedly it doesn't. What better solutions are there? If somebody comes up with a true panacea, count me in as a fervent supporter, but I'm not smart enough to dream one up on my own.

FWIW, in my field PLoS seems to be doing a pretty good job. They aren't publishing any more garbage than anyone else, at least, as far as I can tell.

12. **Matt Leifer**
October 20, 2012

Peter,

I think it is wrong to conflate open access charges with page charges from subscription journals. In the latter case, there is double dipping going on because the library is still paying for a subscription in addition to the author paying a fee.

Mark,

For the same reason, I don't think the hybrid model adopted by Physical Review is a great idea. Sure, authors can pay to make their work open-access, but so long as most authors do not the library still has to pay for a subscription and, as far as I can see, they are not getting a discount based on the proportion of articles that are open access. This is one reason why authors may prefer New Journal of Physics because in that case it is unambiguous that no library or indeed anyone else is ever going to have to pay for your work.

Personally, I have no problem with the green open access model of posting to the arXiv and then publishing anywhere you like, providing the journal I publish in is not involved in price gouging. We have to recognise though that this is part of a larger battle that includes other scientific fields, such as medicine, which do not have a preprint culture for a variety reasons. If we want them to adopt open-access then we should lead by example.

Peter again,

It is easy to discuss anecdotal evidence of extreme cases and scam artists. The argument you are making is analagous to arguing that, because there are a lot of scam conferences out there, the overall quality of scientific conferences must be decreasing. Now, we have all been getting those scam conference emails for far longer than we have been getting scam open access journal emails, and I don't think you would argue that conferences are a lot worse nowadays than they used to be. The fact is, people have enough intelligence to know the difference.

What we really need to do is to look at hard data to see whether open access has an effect on quality. Peter Suber quotes a lot of positive studies in his book, which I recommend by the way. Obviously he is an open access advocate so you may argue that he has an agenda, but I didn't get the impression that he was cherry picking. Regardless, the point is that this is a question to be answered scientifically rather than anecdotaly.

It is also worth pointing out that open access and author pays are not identical. For example, New Journal of Physics will waive publication fees for authors that do not have access to grants and funding. Most reputable open access journals do this, and there are even a few that charge no fees at all (I don't know of any in physics though).

13. **David Nataf**
October 21, 2012

When I read papers on the arXiv, I ignore whether the paper is submitted or accepted for publication. I trust the real peer review system: that of the community of peers, but if I find a mistake I take note and remember the authors' names. In general published papers are just as likely to have mistakes anyway, what quality comes down to is the author's regards for maintaining his or her own reputation.

Ultimately that may be the only solution moving forward.

The other issue with the peer review system is that it's rooted in 17th century science, when there was a smaller number of fields and it was easier to find a "qualified expert". Science also didn't progress as rapidly, there was not as much cost to waiting 6-12 months for the referee process to complete itself.

14. **Toma Susi**
October 21, 2012

Matt,

An example of a totally free physics journal (well, materials science anyway) is the [Beilstein Journal of Nanotechnology]. It's supported by the Max Planck Society. We recently published an article there (as have several of our colleagues) and had a good experience.

15. **A.**
October 21, 2012

@David Nataf:

Completely agree. Go to arXiv. Read paper. Confer with collaborators. Decide if paper is decent or not. Cite or not as appropriate.

@Matt: one reasons that I, personally, wouldn't send a paper to the New Journal of Physics is that I've never yet cited a paper published there. This, for me at least, is much more telling, and more important, than it's impact factor.

Alright, the next bit is very subjective but nontheless is going to play a roll in attracting authors to journals: the typesetting of NJP articles is atrocious. While APS Becaon (who typeset phys rev articles) drive me insane with the mistakes they introduce into my papers when preparing proofs, the final result does at least look like a research article. Whereas NJP articles look like they're been put together by someone with a 1995 edision of word and a couple of crayons. The journal _looks_ crackpot. IMHO. Why would I submit there, when I can do a better job of it with latex, on my mac, and then post to the arXiv? Job done.

On the other hand, I admit that if an arXiv paper is written in Word/has been sitting around for five years without being published/has been updated more than three times/ I'm automatically on my guard. Meh, there is no easy solution to this.

16. **Matt Leifer**
October 21, 2012

A,

The conventions of where people send articles obviously vary from subfield to subfield, so there will be some people who don't have a reason to cite articles in NJP, but that does not tell you about the overall quality of the journal. I can tell you that in quantum information, NJP is fast becoming more popular than PRA, which used to be the first-choice journal for physics-based papers in the field after Nature/Science/PRL. Again, it is a case where we should look at the data rather than anecdotal evidence and the data says that NJP is doing well, i.e. better than any non-PRL Physical Review journal in impact factor. I am not a shill for NJP, I send papers both there and to Physical Review journals. It is just an example of an open access journal in physics that does not seem to have a quality problem.

Regarding typesetting, it really is not much different from any other IOP journal, so are you saying that all IOP journals look crackpot to you? What is wrong with the typesetting of this (http://iopscience.iop.org/1367-2630/12/3/033024/pdf/1367-2630_12_3_033024.pdf) for example?

17. **Jan Velterop**
October 21, 2012

Peter, you say "the financial incentive for the journal becomes to publish as many papers as possible, since that's the only way to increase revenue." That's true, but it was always thus, also for subscription journals. Acquisition editors' task at most publishers was to increase the number of volumes to be published every year. More volumes means more papers, and given a number of subscriptions, more revenue. Journals are rarely cancelled because they grow. An example of the growth of a subscription journal: Discrete Mathematics — 1971 (its first year): 1 vol — 1981: 5 vols — 1991: 8 vols — 2001: 20 vols. These volumes all had 4 issues, of ca 10 papers each. Later the journal went to one vol/yr but with many more issues/vol.

The author-side payment is not an incentive in itself to grow a journal, and it certainly isn't very different from the incentives in the subscription system.

The flaw in both systems is that all the revenue is derived from published papers, instead of from papers 'processed' (i.e. submitted and for which peer-review has been arranged). A way out of that conundrum is to levy submission charges only, and pay the entire operation out of those. The risk for any publisher to start doing this is just too great, as paying and then still being rejected won't be popular (though it is very normal for, e.g. a driver's test).

18. **JGB**
October 21, 2012

How about doing a complete 180 and reject the ability of the free market to manage this proces at all? Both ends clearly lead to different ways of gaming the system. The government provides far and away the largest percentage of money whether in grants or library subscriptions. Collect the best journals that wish to and set both an expectation for articles reviewed and a max limit on articles per year. Undoubtedly there'd be some complaints, but you'd be forcing people to increase the quality of their submissions for peer review. help manage the workload of everyone. And cut way down on the silly transfers of money between government institutions that claim to maximize efficiency but clearly do not. Private journals wouldn't be illegal, but would only truly make it if they could actually offer something not provided for above. My guess is that very few would, and you'd see a much more productive splitting of peer reviewed highly vetted research, and other largely free distribution of working data (like Arvix)

19. **A.**
October 21, 2012

On the "who should pay" issue, publicly funded research should be publicly available. In fact is has to be, since otherwise other researchers can't get to your results, for example. Hence, awarding bodies should fund publication. The problem with this of course is that funding bodies will say "ok, but that means you're only getting enough money to fund a postdoc for 1.5 years instead of 2", which wasn't the plan at all.

What about paying the referees for their reports? A little naíve I feel. Physicists aren't noble go-gooders who bravely go out and fight the good fight FOR SCIENCE! and the advancement of humanity. They're just people, and as such have their own interests at heart. The "experts" in my field to whom my papers are sent for review are often my competitors. Why would they carefully read and evaluate my paper when they can instead find some shoddy excuse to delay it or reject it and get paid into the bargain? More to the point, why would I pay for that? We could all promise to be honest and fair, I suppose, but, ha, sorry, fell off chair laughing.

If only we had some huge repository where physicists decided, in a nice socialist fashion, to share all their new results for the rest of the community to look over and ponder. Oh wait we do. The facetiousness is warranted; the impression I get from talking to other physicists is that the older generation regards the arXiv as some sort of necessary but rather unsavoury "halfway house" on the road to the sobriety of publication. Whereas, the younger generation think of it as what it is: a great big, free, online journal.

In a few years time, once the younger generation start to get into tenured positions (if there are any left), I think we'll start to see much less importance placed on journals and publication, and more on how well cited a paper is on the arXiv.

(Obviously, this doesn't apply to other subjects: medicine is a totally different kettle of fish.)

@Matt: sorry, not rising to the bait re: typesetting. Even though I set the hook.

/end nautical analogies.

20. **Peter Woit**
October 21, 2012

Jan Velterop,

Yes, there has always been an incentive to grow journals, if you did this you could use it as one excuse to justify higher subscription costs in the future. But, in the traditional model, each year you had a fixed amount of subscription revenue, independent of how many papers you published, and each additional paper cost money to process, print and distribute. The immediate financial incentive was to keep the number of papers down.

Matt,

I don't think looking at past data is relevant, since my concern isn't so much with the history or current situation (which is very much in flux, with print about dead and library budgets unclear), but with the future that people are setting in place now. My point is just that "open access" evangelists need to acknowledge that some of these models provide strong financial incentives towards publication of ever larger amounts of lower and lower quality papers. In the long run, the built-in financial incentives matter, a lot.

The argument that people should do this to provide an example for those in medicine to follow doesn't seem like a good one to me. I'm not convinced even that math and physics should follow the same model, much less medical research, which is a completely different kettle of fish (luckily people don't die because of wrong math papers, nor are important ones worth billions of dollars to someone).

21. **Matt Leifer**
October 21, 2012

Peter,

I guess we are not going to reach agreement, but open access journals are not, at this point, a new idea and there are already quite a lot of them, so if there is a trend towards reduced quality then we should be able to see the beginnings of it now. As we have seen in the deabate on this thread, it is certainly not obvious to everyone that the financial incentives work out in the way that you suggest, with some even suggesting that traditional journals are just as bad. Therefore, I still maintain that this question needs to be settled by data rather than opinion and anecdote. If historical data does not convince you then we need to make sure that we are collecting and monitoring data on publishing practices as this change takes place so that we can take action early if a trend is identified. However, based on what we know so far, I would say that goal of increasing access to scientific research outweighs any doubts I might have at the moment.

Finally, I want to point out that open access is only a waypoint in a more general disruption of academic publishing that is enabled by the internet. It is something that campaigners focus on because it is well-defined enough for stakeholders (academics, funding agencies, governments, publishers, etc.) to actually do something about it right now. However, there are other proposals on the table to deal with quality assessment in a more efficient manner than traditional peer review (see Tim Gowers writings on this issue for example) so I am not too worried about the long-term future. Given the ease of distributing information, it does not really make sense to do quality filtering prior to the distribution of articles. If there is a lot of junk out there then I don't really care. I can just ignore it. It is not as if someone is going to deliver a telephone directory sized book of new academic papers to my door every day. Instead, we need a way of filtering post-publication so that high quality articles rise to the top. Tim's proposal of having a peer review layer on top of the arXiv makes a lot of sense in this regard.

22. **Peter Woit**
October 21, 2012

Matt,

From what I've observed, especially in theoretical physics, we're already effectively in the post-journal phase, with the arXiv the main source everyone is using to get access to papers, and peer-review a broken system for quite a while (see Bogdanov (2002)). For the life of me, I can't figure out why anyone is paying for now or making plans (SCOAP3) to fund in the future Elsevier to produce Nuclear Physics B.

A future journal system or its replacement that provided peer-review vouching for accuracy of mathematical proofs and similar difficult to evaluate arguments, as well as identifying the highest quality work for non-experts (experts don't need this) would be valuable, but not if it's largely set up along a vanity publishing model, with incentives to concentrate on extracting money from authors for not necessarily reliable evaluation of work.

23. **somedude**
October 21, 2012

To David, you claim: “The other issue with the peer review system is that it’s rooted in 17th century science, when there was a smaller number of fields and it was easier to find a “qualified expert”.”
Peer review is very much a 20th century phenomenon. When Albert Einstein was peer reviewed and his results on the non-existence of gravity waves were found to be wrong, he did not like it. Before that if the chief editor liked you, you got in. Even before that, the few scientists just sent eachother mail. The old fashioned paper variety. 🙂
The Bernoulli’s are known to not have even sent their results, just the claim that they could prove something and taunting the other if he could prove it as well.

24. **Mateus Araújo**
October 21, 2012

Although A.’s point about NJP’s typesetting was not very good, I think it is interesting to think about why he felt so disguted by it, which might help us overcome resistence to NJP in particular and open access in general.

A simple explanation is that the APS is more famous than the IOP, and as a young grad student he grew up reading APS’s papers, and internalized REVTeX as “the” scientific format. Well, that’s at least what happened to me.

But apart from that, I think that there are some objective problems with NJP’s typesetting:

1 – The first page that identifies the downloader. I know that it’s standard for IOP (and still ugly), but particularly insane for an open-acces journal.
2 – Colourful logo and colourful typesetting. Come on.
3 – iopart.cls does not play well with amsmath. And if I recall correctly, it had some technical issues with more modern LaTeX packages.

But back to the relevant point: I dare to hope that an author-pays model actually increases quality, by disincentivizing the author to publish as many papers as he can about a subject. It might be completely subjective, but the quantum info papers I read at the NJP tend to be like more complete treatises about a new idea than the piecemeal reporting that I often find at PRA(L).

25. **Bob Levine**
October 21, 2012

The pointedly-ignored elephant in the living room of this discussion is the fact that post-circulation/publication refereeing will never be a practical possibility as long as academic culture continues to be what it’s been for a long time now. Tenured members of academic units do not want to determine the tenure cases of their probationary colleagues based on their own assessments of those colleagues’ research. In most cases, they do not feel competent to do so, and for good reason—there may at most one or two others in a given department qualified to carry out that assessment. And no one feels that the reports from the externals should be allowed to completely determine their colleague’s fate (especially as its not unusual for the externals to disagree rather spectacularly, reflecting their own scientific agendas). What everyone wants—faculty P&T committees, academic administrators, university legal departments—is an ‘industry standard’ criterion that they can point to to defend their up-or-out tenure decisions, and the prime standard is the hierarchical ranking of publication venue, based to a large extent on the severity of the latter’s refereeing criteria. That’s the dynamic that Elsevier and its ilk prey on, and the reason that you’re just not going to see ‘after the fact’ refereeing adopted as common practice. Open access is the wave of the future, no question; but P&T considerations are going to ensure that serious refereeing, and ranking of OA venues, is going to be an integral component of that wave.

26. **A.**
October 22, 2012

@Bob: indeed, agree that the higher-ups need a number to point at in order to justify their lunch-meeting cheese and wine decisions. However, that number can be taken from the arXiv. Total citations, h-index, it’s all there.

I’d also suggest that we already have “post-circulation/publication refereeing”, it’s a part of the arXiv: has the paper been withdrawn, are there comments, what does it’s citation count look like, etc.

In case it wasn’t totally obvious, “go arXiv! Boo to Elsevier!”, etc

@Peter, Bob,

One of the benefits of having some sort of formal refereeing layer on the top of the arXiv might would be that it might help convince the higher-ups that the arXiv is a bona-fides system/journal. Getting the balance right will be tricky since, I imagine, it would be very easy to start down the slippery slope of turning the arXiv into a traditional journal with, for example and god forbid, access restrictions. (Although, I don’t really believe that pleasing the higher-ups is a good motivation for doing anything at all.)

27. **fuzzy**
October 22, 2012

i think that open access system needs to give more importance to referees. in this system, the editors are in a position of conflicting interest, being payed by those who publish. it is like a tribunal where the judge will be payed in one of this two manners; cash by the accused defendant–called “submitter”–or after very long time by its abstract (and almost absent) opponent–called “science”. a recent experience of mine: i was requested a report and i have sent a detailed one, that meant “reject” or at least “major revision”. the editor has cutted it dramatically, giving no explanation, and then has asked the author: if you want please take into account these minor remarks. maybe this is the right way to advance the physics of high energies, but i do not think so

28. **M**
October 22, 2012

Under SCOAP3, some journals will get about 1000$ per accepted paper.
Most of the work is done by referees, who get nothing.
Why should a referee do unpaid work to make publishing companies rich?

29. **Oceanographer**
October 22, 2012

Here you can see a good example of author pays journal that is becoming more and more successful
http://www.ocean-science.net/
even if the average price per publication is much more than 500$ and they charge in advance even if the paper is rejected (in such case it is still available in the ocean

science discussions together with the reviews and discussion)
You can look at some papers there- it is an interesting model. Also there are other journals of the European Geosciences Union following the same model- the open peer review process is a key factor here for the absence of junk papers.
So the author pays model is also viable in my opinion.

30. **Allan Rosenberg**
October 22, 2012

Il est tout à fait certaine que Prof. Rathque avez obtenu des résultats nouveaux et utiles….

31. **Ptrslv72**
October 22, 2012

BTW, there is a blog fully devoted to tracking "predatory publishers" that abuse the OA system:

http://scholarlyoa.com/

Perhaps it has already been mentioned in the thread, apologies if I missed it.

32. **Peter Woit**
October 22, 2012

Ptrslv72,

Thanks. However looking at that web-site makes clear that the problem I'm worried about is already with us. They try and distinguish between "predatory publishers" and legitimate ones, but the distinction to me seems to be becoming increasingly unclear.

Look at:

http://scholarlyoa.com/2012/09/18/two-publishers-each-have-a-journal-with-the-same-title/

to see that a "predatory" publisher started an author-pays open access journal called "Journal of Cloud Computing" in 2011. Soon thereafter, Springer started an author-pays open access journal with the same name. It appears to be much more respectable, with a legitimate editorial board. But it's one of many new "SpringerOpen" journals, and starting lots of new journals all of a sudden with this "author pays" model makes one wonder how high their standards will be. Did all of a sudden a lot more high-quality papers get written? Or will these journals just publish lots of things that would have had trouble getting published in the past, now sped along into publication by funds put up by the author. How different is this from vanity publishing?

For a list of the SpringerOpen journals and what they are charging, see

http://www.springeropen.com/about/apcfaq/howmuch

The "predatory journals" at least are cheaper….

33. **Mathematician**
October 22, 2012

I think author-pays will be a disaster in a theoretical subject like mathematics.
It might work in other subjects where there is a _real_ need for editing on the
part of the journal and large grants riding on publishing, or perhaps where
publishing in widely-read journals enhances the possibility of industrial
application. But in mathematics the situation is clearly different; for one
thing, a lot of mathematics is simply about aesthetics, and so the money stakes
are generally low… (at least, I'm not in on it). I can't help but compare the situation to, say, a newspaper or a magazine that demands payment for publishing contributed articles: What would one think of such a thing? My guess is that most people will have little respect for it, even if the author payment is supposed to make it free. Also, I don't think that it's a good idea to re-enforce the image that mathematicians live in intellectual ivory towers by turning their publication process ever so close to vanity publishing.

34. **srp**
October 22, 2012

This is isomorphic to the problem that a university faces in maintaining its reputation. Yes, Harvard could sell out its undergraduate spots to the highest bidder, but in short order their reputation and ability to charge for those spots would decay. The same holds for any journal that uses an author-pays system. Sure, they could fill up the journal with crap, but if it becomes known as a repository of crap no one will be willing to pay much for a slot in it. It's inherently self-policing.

35. **Peter Woit**
October 22, 2012

srp,

Harvard isn't run as a profit-making organization making decisions solely based on whether they maximize revenue. If it was, they'd be doing some very different things (e.g. eliminating financial aid and raising tuition). Some of these might cause their reputation to take a hit, but they'd end up in a sustainable business model that would bring in much more revenue.

If journals were all run by non-profit organizations, with missions and incentives other than maximizing profit, I'd be much less worried about the author-pays model of financing them.

36. **OMF**
October 23, 2012

Why are universities not obliged to publish and host journals themselves, in house, or as part of a collective? Why must publicly funded research be published in privately owned journals (B cuz der fee-mrkt lulz).

This whole situation is increasingly absurd. Personally, I feel that if research academics feel they need to keep publishing in private journals, then the public should no longer feel obliged to pay their salaries. I think that's what it's going to take to change people's minds.

37. **Low Math, Meekly Interacting**
October 23, 2012

Obviously there also needs to be reforms in the system of peer review, but I have a hard time getting my head around the notion that people can get jobs based on something like their arXiv submission record alone. I guess in theory and maths, where the standards for validation can be quite different than for experimental fields, a referee-free body of literature is a workable solution. But even among theorists, cannot the number of people who are highly qualified to assess the minutiae of an argument often be rather small? For those making hiring decisions, without some sort of "gold standard" like publication in high-impact journals, the whole process could change considerably. Of course I'm in no position to speak with any authority on how hiring is done in the fields of mathematics and theoretical physics. But if there are any similarities to the fields with which I am familiar, when you change the world of publishing as we know it, you change many other things.

Not that that would be bad. At all. But what the best replacement should be seems like a very complicated problem that, again, I'm not sure a preprint server like the arXive can fully address, at least not without some other profound systematic advancements. I used to think that the arXiv was the perfect model for other disciplines, but I'm no longer so sanguine after seeing many comments online about its purported shortcomings.

38. **Orr Shalit**
October 23, 2012

I agree with Peter that "author pays" is a bad direction to be heading in. Following what OMF wrote above, I think that the direction to be going in is towards not-for-profit journals which are funded directly by universities and public bodies (such as Documenta Mathematica, and there are others). The only role journals play right now is in coordinating the peer review process (plus the quick rejections that editors make). Paying for your paper to be published is nothing more and nothing less than buying a bit of the journal's reputation (whatever it is worth).

Above Matt wrote: "These are extremely low-prestige journals that everyone already ignores". Until recently I thought that author-pay was an indicator for extremely low prestige and a solid reason to ignore a journal.

39. **Bob Levine**
October 23, 2012

@Orr:
"Until recently I thought that author-pay was an indicator for extremely low prestige and a solid reason to ignore a journal."

Ah, but it sounds as if you've seen the error of your ways, eh? 😉

Seriously, some extremely prestigious OA journals charge high, even absurd fees (though they probably don't see it that way). Take a look at the following survey of prices:

http://www.lib.berkeley.edu/scholarlycommunication/oa_fees.html

The cost-to-author numbers top out at around $5K; PLoS, which has a tremendous reputation especially in biosciences, as I understand it, will take you up to almost 3K. The assumption is that you're going to be shelling out from your grant $$, which is a bit rough on people whose research is not of the kind for which huge grants are appropriate to begin with….

40. **Neil**
October 24, 2012

To play devil's advocate, in *principle* it should work. No author should want to pay to publish in a shoddy journal, and no university should put much weight on publications in shoddy journals. The only way a pay-to-play journal should make a big profit is for it to have a reputation for high quality, in which case authors should be willing to shell out the big bucks, and institutions should weight the publications highly. Only if the profession can't tell shoddy articles from good should there be a problem.

All the "shoulds" are intended.

41. **Orr Shalit**
October 24, 2012

@Bob:
Well, it appears that some author pay journals have good reputations, so I guess I was mistaken about that. Also, ignoring this phenomenon doesn't seem like a good idea any more.

@ Neil:
Suppose all journals were author pay, and journals prices corresponded to reputation. Even if there was no problem of journals having a high incentive for publishing "gobbledygook", and even if they never do, I still do not see how someone can feel comfortable paying 5K out of their publicly funded grant (instead of, say, 1.5K) just to be in a more prestigious journal.

42. **chorasimilarity**
October 24, 2012

Journals were invented for disseminating knowledge. This is now an obsolete method. It means that the system, as it is now, will (slowly) die. Fact!

This "authors pays open access" is just a perverse effect. Pay for what, exactly? If it is for having refereed papers, pray tell me how much a referee is payed?

43. **srp**
October 25, 2012

Peter says:

"srp,

Harvard isn't run as a profit-making organization making decisions solely based on whether they maximize revenue. If it was, they'd be doing some very different things (e.g. eliminating financial aid and raising tuition). Some of these might cause their reputation to take a hit, but they'd end up in a sustainable business model that would bring in much more revenue.

If journals were all run by non-profit organizations, with missions and incentives other than maximizing profit, I'd be much less worried about the author-pays model of financing them."

You're almost certainly wrong about Harvard's profit-maximizing strategy. High tuition and financial aid (which are complements, not substitutes) enable the university to price discriminate successfully, charging lower prices to more price-sensitive customers and higher prices to less-sensitive ones. In addition, the high demand for Harvard's slots is largely a function of its perceived exclusivity and student quality. The main thing that a profit-maximizing Harvard would do would probably be to expand the class size, which they could probably do by 50% without lowering average student quality or exclusivity by a measurable amount.

But regardless, I don't see what the problem is with a multi-tier journal system in which the most prestigious ones charge more (with financial aid for those who can prove hardship–i.e. price discrimination) and the less-prestigious ones command a smaller price from authors. Readers will know which ones are the good ones, the not-so-good-ones, and so on down the line. Why would this scenario have any negative effect on the progress of science?

It seems you are assuming that readers will not fairly quickly pick up on which journals are worth reading and which ones aren't, and that lousy journals will somehow confuse or pollute the knowledge stream. But that argument proves too much–Frank Wilczek has pointed out that almost all published physics articles now are "attractively published junk" (in his Longing for the Harmonies, where he attributes this wisdom to David Gross, then his advisor). Sturgeon's Law is perhaps even more inexorable than Parkinson's or Murphy's. Author-pays-and-readers-have-open-access seems to me no worse on quality and certainly better on dissemination than author-gets-a free-ride and reader must pay.

44. **Costas**
October 25, 2012

Peter: your blog followers might be interested in viewing the interesting lecture by Alain Connes on music of shapes and spectral geometry, now uploaded to youtube
https://www.youtube.com/watch?v=bJo-yvUaQjM

45. **Shantanu**
October 26, 2012

Peter forget all this. You haven't written anything about experimental search for quantum gravity conference at Perimeter(happening this week)?
shantanu

46. **Peter Woit**
October 26, 2012

Shantanu,
Unless someone has actually found quantum gravity experimentally, I think I'll leave that topic to Sabine to blog about….

47. **Stevan Harnad**
October 27, 2012

Testing the Finch Hypothesis on Green OA Mandate Ineffectiveness

We have now tested the Finch Committee's Hypothesis that Green Open Access Mandates are ineffective in generating deposits in institutional repositories. With data from ROARMAP on institutional Green OA mandates and data from ROAR on institutional repositories, we show that deposit number and rate is significantly correlated with mandate strength (classified as 1-12): The stronger the mandate, the more the deposits. The strongest mandates generate deposit rates of 70%+ within 2 years of adoption, compared to the un-mandated deposit rate of 20%. The effect is already detectable at the national level, where the UK, which has the largest proportion of Green OA mandates, has a national OA rate of 35%, compared to the global baseline of 25%. The conclusion is that, contrary to the Finch Hypothesis, Green Open Access Mandates do have a major effect, and the stronger the mandate, the stronger the effect (the Liege ID/OA mandate, linked to research performance evaluation, being the strongest mandate model). RCUK (as well as all universities, research institutions and research funders worldwide) would be well advised to adopt the strongest Green OA mandates and to integrate institutional and funder mandates.

Gargouri Y, Lariviere V, Gingras Y, Brody T, Carr L & Harnad S (2012) Testing the Finch Hypothesis on Green OA Mandate Ineffectiveness *Open Access Week 2012*

48. **LOL**
November 1, 2012

This "Advances in Pure Mathematics" journal sent me an e-mail today in which they invited me to submit an article. I responded to the e-mail and jokingly told them that I wanted to submit a Mathgen article. I just got back the following from the journal:

Dear author,
Glad to hear from you.
Kindly be informed that you can send your new paper which has not been published to us.
We look forward to your paper.

Please feel free to contact us if you have any questions.

Best Wishes

49. **S Halayka**
November 5, 2012

That cloud computing example is perfect: Springer already has a distributed computing journal, so all they're really doing is double-dipping — oh, but at least it's being done in a classy, high-brow way!

Latest Links

October 26, 2012
Categories: Uncategorized

- On the LHC front, new results will be announced at the Hadron Collider Physics Symposium in Kyoto, which opens November 12. Jester has a good summary of what to look for on the Higgs front here. The new results should be based on about 12-13 fb^{-1} of 2012 8 TeV data (this past summer's used about 5 fb^{-1} each of 2011 7 TeV data and 2012 8 TeV data). Unblinding of the results should have taken place recently, so soon about 6000 physicists will know what the news is and start talking about it…
- The latest Scientific American has a cover story about particle physics that comes under the "This Week's Hype" heading. It's called "The Inner Life of Quarks" and discusses models in which quarks and other elementary particles of the standard model are composites of more elementary objects called "preons". The fact that the papers on the subject it refers to are from 1979 should make one suspicious: an idea that hasn't had major developments in 33 years is a dead idea. Besides the overwhelming experimental evidence against preons (with the LHC bringing in many new much stronger negative results), the idea has huge inherent problems. The main issue is that one is trying to put together composites with masses as small as MeVs (or lower, if you try to do this with neutrinos) while the data says that things are point-like up to TeV scales, with just the forces you know about up to such scales.
- For the latest on Paul Frampton's troubles as the victim of a scam that has left him in an Argentine jail, see this article entitled Imprisoned UNC professor thinks he deserves a raise. I'm assuming this was before his trip to South America, but at some point Frampton clearly did some extensive research, comparing his salary ($107K) to those of some of his illustrious peers ($203K-$532K according to him, just using data from public universities). Not clear though that this was really something to bring up in his argument about whether the university should still pay him even if he's in jail.
- I only recently heard the old news that Fields Medalist Vaughan Jones has left Berkeley to take a job at Vanderbilt University. Evidently one reason for doing this was a salary number of the sort that Frampton covets.
- Freeman Dyson has a piece in the New York Review of Books about Jim Holt's new book Why Does the World Exist (see my take here). Not much in the review actually about Holt's book, but Dyson takes the opportunity to enter the ring in the fight over nothingness with some late blows aimed at the philosophers. He doesn't think much of modern philosophy, ending with:

  > The great philosophers of the past wrote literary masterpieces such as the Book of Job and the Confessions of Saint Augustine. The latest masterpieces written by a philosopher were probably Friedrich Nietzsche's Thus Spoke Zarathustra in 1885 and Beyond Good and Evil in 1886. Modern departments of philosophy have no place for the mystical.

  There's a Chronicle of Higher Education piece about this here, Brian Leiter's blog hosts a discussion here.

- I'm loathe to post anything about US politics here, since it's a depressing and omnipresent topic these days, but for an HEP angle, see this in Science from Adrian Cho, and this in the NYRB from Steven Weinberg. Don't even think though of posting comments about politics here…
- Greg Moore recently gave the Felix Klein lectures in Bonn on Applications of the six-dimensional (2,0) theory to physical mathematics . Video here, lecture notes here.
- This week at Stony Brook there's a conference in honor of Blaine Lawson's 70th birthday. Lawson is a great person and a wonderful geometer; I very much enjoyed getting to know him a little bit during my days as a physics postdoc at Stony Brook. He was one of several examples that convinced me that leaving physics for mathematics would at least promise hanging out with nicer people. I've been too busy this week to get out to Stony Brook, had formed a crazy plan to bike out there this weekend for Nigel Hitchin's talk Sunday morning, but a nasty cold has put an end to that plan (Sunday's weather prediction for an approaching hurricane might in any case have made a long bike trip not the best idea in the world). Videos of the talks are available here.

  Happy Birthday Blaine!

Comments

1. **M Uppal**
   October 26, 2012

   It's JIM Holt, Dr. Woit.

2. **Peter Woit**
   October 26, 2012

   Oops, fixed. Thanks!

3. **Joe**
   October 26, 2012

   Peter,
   Thanks for the links. It's always a pleasure to read your posts.
   One question: When exactly will we be able to know Higgs' spin?

   Best

4. **Peter Woit**
   October 26, 2012

   Joe,

   Thanks. I haven't paid much attention to the spin measurement issue, since as Jester points out, it seems highly unlikely to be anything but zero. Maybe someone better informed than me can comment, but I'd guess that since I haven't seen any analysis of this in this summer's data, the modest amount of new data since then isn't going to make enough of a difference for CMS or ATLAS to be able to say something about the spin next month.

5. **Dario**
   October 27, 2012

   "2008 data" should be "2012 8 TeV data" probably.

6. **MathPhys**
   October 27, 2012

   Any clue who makes $532,000 as a physicist in a public US university?

7. **MathPhys**
October 27, 2012

Right. Of course. I should have known. Because when I first heard about it, it was described as “More than the football coach”.

8. **Shantanu**
October 27, 2012

Peter, since I don’t have subscription to scientific american, who wrote the article?
Also did the article claim that this isa prediction of string theory?
I do hope people complain about such articles.
shantanu

9. **Peter Woit**
October 27, 2012

Dario,
Thanks, fixed.

MathPhys,
Yes, it’s Weinberg. The UT football coach however makes $5.2 million/year.

Shantanu,
The author is Don Lincoln. String theory doesn’t really play a role, although I think he mentions that string theory could explain preons (also that LQG could…)

10. **Steve L**
October 27, 2012

Hi Peter, I love your site. I took undergrad complex analysis from Prof. Lawson at UC Berkeley in 1974 or so. Even back then I believe he was well known in the minimal surfaces biz. He was an excellent teacher, very clear and full of enthusiasm for the subject, friendly and approachable. Glad to see him mentioned on your site. Happy birthday Professor Lawson! Students never forget a good teacher.

11. **Tienzen (Jeh-Tween) Gong**
October 28, 2012

Preons are dead, but prequarks are not.

12. **Dario**
October 28, 2012

Peter:

this edition by Scientific American http://www.sciamdigital.com/index.cfm?fa=Products.ViewIssue&ISSUEID_CHAR=51440409-237D-9F22-E8A91238D0D12A7B is even worse…

Gong Tienzen:

of course prequarks are dead as well, and for the same reasons as preons are. Quarks are not composite, as experiments on their size, magnetic moment and interactions show. There is no hint of compositeness, and worse, there is not even a way to consistently *imagine* compositeness of quarks. Quarks are not composites of other particles.

13. **Peter Woit**
October 28, 2012

Dario,

Your SciAm link goes nowhere for me. Is this to the Don Lincoln story?

14. **BJM**
October 28, 2012

Dario’s link worked for me. Maybe you can get to it via
http://www.sciamdigital.com/index.cfm?fa=Main.ViewMain
Click on “Frontiers of Physics” under “From the Archive”

15. **BJM**
October 28, 2012

Actually, just
sciamdigital.com
does it.

Here’s the list of contents:

The Dawn of Physics beyond the Standard Model; Frontiers of Physics; by Gordon Kane

The Search for Relativity Violations; Frontiers of Physics; by Alan Kostelecky

Solving the Solar Neutrino Problem; Frontiers of Physics; by Arthur B. McDonald, Joshua R. Klein and David L. Wark

The Mysteries of Mass; Frontiers of Physics; by Gordon Kane

The String Theory Landscape; Frontiers of Physics; by Raphael Bousso and Joseph Polchinski

The Future of String Theory: A Conversation with Brian Greene; Frontiers of Physics; by George Musser

Atoms of Space and Time; Frontiers of Physics; by Lee Smolin

A Cosmic Conundrum; Frontiers of Physics; by Lawrence M. Krauss and Michael S. Turner

Information in the Holographic Universe; Frontiers of Physics; by Jacob D. Bekenstein

That Mysterious Flow; Frontiers of Physics; by Paul Davies

16. **Peter Woit**
October 28, 2012

BJM,

Thanks. I'll look and see if I can find a copy of that, but it looks like it's a collection of old articles, some of which were featured here as "This Week's Hype"…

17. **Jim Martindale**
October 28, 2012

http://www.scientificamerican.com/article.cfm?id=the-inner-life-of-quarks
will get you a taste of Don Lincoln's article. The preview has a comment section.

For those without access this is from beyond the pay wall:

The first is size. The Standard Model treats the quarks and leptons as pointlike—that is, particles with zero size and no inner structure. Finding a nonzero size for those particles would provide powerful evidence for preons. Measurements have shown that protons and neutrons have a radius of about 10–15 meter. Experiments at the world's leading particle colliders, past and present, have searched for evidence that quarks or leptons also have a measurable size. Thus far all the data are perfectly consistent with zero size or with a nonzero size as small as about 0.0002 to 0.001 times the size of a proton. To distinguish between those two possibilities (zero size versus very, very tiny), we need to make more precise measurements. The LHC is a discovery machine, and the huge amount of data expected from its current collisions and a scheduled upgrade in the accelerator's energy are two ways in which we can expect to learn more about the size of quarks and leptons.

then

The future of hunting for structure within the quarks and leptons is brighter than it has been for a long time. As you read this article, my colleagues and I are combing through the huge amount of LHC data already taken. We are searching for evidence that quarks and leptons have a nonzero size. We are looking for a fourth generation of quarks and leptons and for some evidence that the force-carrying particles also have generations—that the W and Z bosons, which mediate the weak nuclear force, have heavier cousins.

18. **braiding**
October 29, 2012

Do these objections to preons and pre-quarks also apply to Bilson-Thompson braiding?

19. **Kavanna**
October 29, 2012

There's zero evidence for quark or lepton substructure. How to get MeV masses from TeV+ dynamics? Pack a powerful spring into a small space, use E=mc^2, and you get a correspondingly large mass. Hard to see how this doesn't create another hierarchy problem, and a gratuitous one to boot.

The philosophers who push the "why is there something?" idea are a sad lot. They seem not to understand why questions of this sort cannot be answered as framed, as if Hume or Kant had never lived. This rot is a product of Heidegger and his acolytes, not the Anglo-American "analytic" philosophy that once dominated many philosophy departments. Unfortunately, Heidegger et al. is the origin of the post-modernist disease that has destroyed much of the humanities in the US and elsewhere in the last 30 years. Dyson is right — the last great Western philosopher was Nietzsche. In the 20th century, only Wittgenstein can even be thought of on the same level.

(Peter – I've liked Not Even Wrong since it was published. Your book, and Smolin's, said out loud what so many were saying in private, but couldn't risk saying publicly. It also helped me to understand what had happened to the once-productive, open, and competitive world of high-energy physics I was once a part of and how it became a closed cult.)

20. **Anonyrat**
October 29, 2012

Offtpoic, hope you are weathering Hurricane Sandy well!

21. **Peter Woit**
October 30, 2012

Anonyrat,
Thanks, but I'm in Nebraska. When classes were canceled I decided to start a short planned trip to Yosemite early, rented a car and started driving west. Wyoming tonight Yosemite Thursday night.
Back in NYC next week where it seems to be quite a mess.

22. **David Nataf**
October 31, 2012

What does it mean for a proton to have a "size" of 10^-15 m and for an electron to be "point-like"?

Protons don't have a "size", they have an interaction-dependent cross-section, and so do electrons.

What am I missing?

23. **Yatima**
October 31, 2012

@Kavanna:

“Dyson is right — the last great Western philosopher was Nietzsche.”

Maybe so; then there is Sartre and Alissa Rosenbaum. Now, I would like to imagine Nietzsche’s disgust when told that the world can – as far as we know – be accurately and objectively described by appropriate mathematical formalisms that instead of “a will to power” seem to exhibit “a demand for symmetry” instead. He would probably pop a vein in his forehead.

24. **adsfasdfsss**
November 1, 2012

loath not loathe

25. **Chris Austin**
November 1, 2012

David,

The non-zero spatial extent of atomic nuclei was first detected by observing that for elastic scattering of electrically charged particles on fixed targets, the measured scattering cross section at the largest scattering angles was, for sufficiently large projectile energies, smaller than the Rutherford scattering cross section calculated for a point-like nucleus. The reduction in the large-angle scattering is due to the electrostatic potential energy of the projectile, e.g. an electron, no longer increasing with decreasing distance between the projectile and the centre of the target nucleus once that distance is smaller than the nuclear radius, so that Coulomb’s law no longer applies below that distance.

If electrons were made of preons, then the preon distribution would have some non-zero spatial extent, and Coulomb’s law for the electrostatic potential energy between an electron and a positron would no longer apply for separations smaller than that spatial extent, which would again result in reduction of the elastic scattering cross section at large scattering angles in comparison to the result expected for point-like electrons and positrons, for high enough energies of the electrons and positrons in a colliding beam experiment such as LEP.

The same principle would apply if quarks or gluons were made of preons, since the scattering cross sections of individual quarks and gluons can be indirectly detected by the “jets” of hadrons they lead to, at the LHC.

26. **harryb**
November 1, 2012

Peter, as ever, great set of issues to delve into and follow – many thanks. In Europe, this blog is continually quoted as a key “hard” science venue. Now, at the risk of your wrath re not commenting on politics after mention of the Weinberg NYRB article, let me try a quick reference to Nate Silver’s new book, The Signal and the Noise – would be interesting to get your thoughts on it. Two reasons: one, its a very good read on statistics and probability, and the second half of the book is a very convincing case for approaching nature, and life in general, from poker to probability of extra-terrestrial life using a Bayesian framework. Two: he is a huge proponent (on this basis) of demanding that any premise or theorem be based on a falsifiable prediction. To this end, his increasingly well-known fivethirtyeight blog – http://fivethirtyeight.blogs.nytimes.com/ – sticks its neck well out on the upcoming US election (spoiler – 80% likelihood Obama re-elected).
Finally – a rigorous Bayesian approach to String Theory seems well overdue. Lack of evidence of SUSY at the LHC should cause any prior probability of ST being valid now downgraded to a (much?) lower posterior.

27. **jg**
November 2, 2012

To add another potentially useful link for readers, the Royal Society has made most of its publications open access until 29th November 2012, so if (for example) you ever wanted to compile a collection of every Dirac paper published in ‘Proceedings A’ go here

28. **layman**
November 2, 2012

I hope you did not miss this one

http://arxiv.org/abs/1211.0004

and will comment on it.

29. **Gert**
November 3, 2012

Peter,

maybe the lack of LHC Higgs rumors is due to a disappearing Higgs signal. Is all of CERN now busy covering up the result that the bump is getting smaller and smaller?

30. **Peter Woit**
November 3, 2012

Gert,

Maybe the lack of Higgs rumors is due to CERN covering up the disappearance of the Higgs, maybe it is due to my being on vacation. Let’s see what happens later next week…

31. **A.J.**
November 4, 2012

@harryb:

That evidence computation is rather non-trivial. To do it from first principles, you’d need to start with a definition of string theory, and work out the probability of various TeV scale experiments. This is effectively impossible, even if you’re willing to take a matrix model as a non-perturbative definition; we simply do not know how to do these computations. Instead, people make additional assumptions, such as “string theory is well-approximated at low energy by the perturbative MSSM, with the following susy breaking pattern”. This gives you an (uncontrolled) approximation to the true evidence function, and now you can turn the Bayesian crank. But you’re now testing the joint hypothesis “string theory is true” AND “my uncontrolled approximation is accurate”.

This fact is what makes arguments about string theory so boring. We can't make unambiguous Bayesian judgements (in this sense the theory is certainly 'not even wrong'), so we're reduced to arguing about our priors on the conditional distribution of our ancillary assumptions.

32. **harryb**
    November 13, 2012

    @ A J
    Understood and thanks
    Yet given latest SUSY probability downgrades you'd think the trend is just instinctively against ST
    And maybe your greater point is that if any theory resists a reasonable Bayesian test you should be deeply suspicious – a la Mr Woit
    By the way – Nate Silver did good on the prediction front.

A Lost Generation?

November 4, 2012
Categories: Uncategorized

I'm in Northern California, on a vacation originally intended to be short, but started early due to the storm in New York. I wanted though to recommend reading something that a commenter here pointed to. It's an article by Mikhail Shifman (author by the way, of an excellent recent textbook I somehow haven't found time to discuss here) about the current state of particle theory that he has just posted on the arXiv, with the title Reflections and Impressionistic Portrait at the Conference "Frontiers Beyond the Standard Model," FTPI, Oct. 2012. The reference is to this recent conference, held at his institution.

Shifman surveys the current state of particle theory, with a range of interesting comments about the paradigms of grand unification, extra dimensions, supersymmetry and string theory that have dominated the subject for nearly 30 years. The negative LHC results provide a serious challenge to these paradigms, although I'd argue that they have been in trouble for a very long time, with the LHC just the final nail in the coffin. Much of the activity among theorists reported at the conference referred to by Shifman revolves around attempts to prop up some of these ideas. This is going to be increasingly unsustainable as stronger and stronger LHC bounds emerge. Where will this leave the field? Shifman argues that this is a time of opportunity, with the death of old paradigms opening the way for new ideas. I hope he's right. Here are his final comments, of a sociological nature:

> It is easy to estimate the total number of active high-energy theorists. Every day hep-th and hep-ph bring us about thirty new papers. Assuming that on average an active theorist publishes 3-4 papers per year, we get 2500 to 3000 theorists. The majority of them are young theorists in their thirties or early forties. During their careers many of them never worked on any issues beyond supersymmetry-based phenomenology or string theory. Given the crises (or, at least, huge question marks) in these two areas we currently face, there seems to be a serious problem in the community. Usually such times of uncertainty as to the direction of future research offer wide opportunities to young people, in the prime of their careers. To grab these opportunities a certain reorientation and reeducation are apparently needed. Will this happen?

No Comments

HCP2012

November 9, 2012
Categories: Experimental HEP News

The Hadron Collider Physics Symposium will be next week in Kyoto, with announcements of new results from the LHC, some details of which are starting to trickle in. Chris Quigg explains what to look for here.

The LHC has just recently passed the milestone of 20 fb^{-1} of data at 8 TeV this year. Perhaps this will get up to 25 fb^{-1} by the end of this run later this year. After a heavy ion run early next year the machine will go into a long shutdown (until late 2014) for repairs to allow operation at close to design energy (probably at 13 TeV). Next week results will be reported based on 12 fb^{-1} (CMS) and 13 fb^{-1} (ATLAS) of this year's 8 TeV data (compare to this past summer's results based on 5.3 fb^{-1} (CMS) and 5.9 fb^{-1} (ATLAS)). Expect results from the full 2012 data at Moriond in March, with an official combination of results from the two experiments next summer.

On Monday LHCb will report the latest results on B(s)->mu+mu-, and the latest Higgs news should come at the Higgs parallel session on Wednesday. There will also be quite a few new, stronger limits on SUSY.

I'm hearing that these new results already can rule out the idea that this new particle is a pseudo-scalar. There will be no confirmation of an unexpectedly high gamma-gamma rate. Some excesses in the tau-tau channel are being seen, of roughly the size you would expect for a SM Higgs. So, all in all, things are still consistent with a SM Higgs. If not, please let me know….

Update: The B(s)->mu+mu- results from LHCb are out (see here), providing good agreement with the SM, and new, strong limits on possible SUSY models (see the last slide). More from Matt Strassler and Michael Schmitt.

For another source for new LHC results, together with interpretation of their signficance, the Chicago 2012 Workshop on LHC Physics is starting today.

Comments

1. **Anonymous**
   November 9, 2012

   I wonder if Atlas will report the 3 GeV difference they observe in the Higgs peak for different channels…

2. **Kernel**
   November 9, 2012

   Dr. Woit, your writings over the last year make it seem as though CERN is pretty close to being able to refute SUSY. I'm curious to know what you think will be the timeline for the death of SUSY, and maybe what you think the community reaction will be. You've posted some interesting examples of denial, but when do you think a prominent SUSY theorist will come out and say that this is probably not the way our world works?

   I'm somewhere between a layperson and a physicist (I'm a PhD candidate), and I have seen no evidence to believe in SUSY. I don't believe in it because the little I have comprehended seems 'not crazy enough' (as Wheeler once said). When can I claim that my view is scientifically valid?

3. **Peter Woit**
   November 9, 2012

   Kernel,

   Since the "death of SUSY" is a topic covered to death on this blog, I'd rather not go on about it more at this time, but stick to the topic of the upcoming conference. The progress of the SUSY community through the various stages of grief, from denial to acceptance, has already started, but I've no predictions about how it will play out over the upcoming months and years (other than the obvious one that some people will never give up, no matter what the evidence against them is…)

4. **anon**
   November 9, 2012

   can you please be more specific on "There will be no confirmation of an unexpectedly high gamma-gamma rate. "

5. **Peter Woit**
   November 9, 2012

   anon,

   Not unless someone sends me some more specific information. One rumor though is that there's still some argument on the numbers that will be reported about this…

6. **King Ray**
   November 9, 2012

   Like other wrong ideas, SUSY will die one supersymmetrist at a time.

7. **another anon.**
   November 9, 2012

   Rumor has it ATLAS is going to be cautious about saying things because their measured Higgs masses in the ZZ and gamma+gamma channels differ by enough that they're concerned about miscalibration somewhere. Rumor also has it that at CMS, the gamma+gamma excess is smaller now. As most reasonable people would have expected….

   (I hadn't heard the tau news. That'll be a nice addition to what we know.)

8. **MathPhys**
   November 9, 2012

Kernel,

There is in principle no way that LHC or any other machine that operates at a certain finite energy can refute supersymmetry, since one can always claim that susy is broken at a sufficiently higher energy level to be observed.

What one can do is to argue that susy is broken at such a high energy that its presence makes no difference to observable phenomena. When that is the case, the only justification of susy becomes the mathematical consistency of certain models, such as strings.

9. **Bob Jones**
November 9, 2012

"Like other wrong ideas, SUSY will die one supersymmetrist at a time."

Sorry, I don't think it's possible for SUSY to die at this point. It makes no difference if it doesn't show up at the LHC because supersymmetric quantum field theories are important for theoretical reasons. For one thing, they're the easiest theories to analyze mathematically because you have techniques like localization at your disposal. Supersymmetric theories have applications in physics (like Witten's proof of the positive energy theorem in classical general relativity), and they also have many applications in pure mathematics (proofs of the Atiyah-Singer index theorem, the Morse inequalities, applications to elliptic cohomology, Donaldson theory, and the geometric Langlands correspondence). If you really think SUSY is going to die, you're getting too much of your information from this blog…

10. **hank**
November 9, 2012

@Bob:

If it can't die, it's not science. (Sorry for the repetition. On the one hand it's not going to make a difference, on the other hand it has to be said.)

An interesting question would be: Should we keep an idea alive in physics because it seems to be fruitful in certain branches of mathematics?

11. **Thomas Larsson**
November 9, 2012

Bob Jones, thank you for the illustration.

12. **Bob Jones**
November 9, 2012

"If it can't die, it's not science."

I completely agree.

"An interesting question would be: Should we keep an idea alive in physics because it seems to be fruitful in certain branches of mathematics?"

That is an interesting question, but it's also somewhat hypothetical because ideas like supersymmetry and string theory are not only useful in mathematics. They also have tons of applications in physics. In addition to the examples I gave above, you also have applications of N=4 gauge theory to QCD. That's a hugely important topic in physics where supersymmetry comes up.

13. **johnmcAllison**
November 9, 2012

The latest LHC schedule:

https://espace.cern.ch/be-dep/BEDepartmentalDocuments/BE/LHC_Schedule_2012.pdf

So two more weeks of running, adding another 2/fb, and then final three weeks for major machine development and 25ns development. It looks as if they should deliver between 22.5-24/fb for 2012

14. **Peter Woit**
November 10, 2012

Please, enough about SUSY unless it's about the HCP2012 conference.
My reference to "death of SUSY" was short-hand for "death of SUSY extensions of the SM that have dominated the field of BSM physics". The fact that there's a much more complicated and interesting story about SUSY in general is one reason I don't think it's a good idea to discuss it here, where it's far off-topic, and the actual topic is I think both interesting and highly timely.

15. **Kinshasa**
November 10, 2012

Bob Jones wrote: "Supersymmetric theories have applications in physics (like Witten's proof of the positive energy theorem in classical general relativity)".

It is quite a stretch to call Witten's positive energy proof an application of supersymmetry. True, Witten's original article discusses a motivation coming from supergravity (quote: "a few speculative remarks will be made about the not altogether clear relation between the previous argument and supergravity"). But that is certainly not the most natural way to think about it. The natural way to think about it is this: In the early 1960s, Lichnerowicz — not thinking about SUSY in any way whatsoever — had showed by spinorial methods that certain closed manifolds do not admit Riemannian metrics of positive scalar curvature. Witten's proof is closely analogous to Lichnerowicz' argument, dealing with asymptotically flat manifolds instead of closed ones.

If you really want to interpret Witten's proof in a field-theoretic way, you should notice that the only field involved is purely fermionic: a spinor on a 3-dimensional spacelike surface.

I would argue that certain other presumed applications of supersymmetry in mathematics, for instance Seiberg-Witten theory, do not have to do anything with SUSY either. Yes, Columbus found America while he wanted to get to India; but America is very far from India, and travelling westwards from Europe, you arrive at it naturally, no matter whether you are heading for India, China, or the setting sun.

Saying that Witten's positive energy proof or the Seiberg-Witten equations are applications of supersymmetry is like claiming that Teflon was a spin-off of the NASA space missions, or that Newton's gravity theory would not have been discovered without apple trees.

16. **Hal Porter**
November 10, 2012

I presume that doubling the data will fine up the range of potential masses and other characteristics of the particle. The existence of the Higgs itself, and a general mass range are of obvious importance and appears to have been achieved.

Which leaves the question, (which I hope is not too far off topic): how much practical, computational (?), difference will the reduction of the + or – of the mass by a couple of percentage points mean vis a vis any current theoretical perspective?

If any.

17. **Peter Woit**
November 10, 2012

Hal Porter,

The interest now isn't so much in getting a more accurate Higgs mass, but in seeing the Higgs in different channels and measuring the production cross-section x branching ratio in each of those channels. Any deviation of those from the SM predictions will indicate new physics: either new particles, or a different coupling between the Higgs field and other SM fields than expected.

18. **M**
November 11, 2012

watch B(s)->mu+mu-

19. **alpinetree**
November 11, 2012

Regarding B(s)->mu+mu- I found the following useful for context http://muon.wordpress.com/2012/11/10/watching-for-bs-to-mumu/

20. **Hal Porter**
November 11, 2012

Thank you for the explanation, and for the link.

Interesting–and beautiful graphic.

21. **alpinetree**
November 12, 2012

Slides have appeared for today's talks here http://kds.kek.jp/conferenceDisplay.py?confId=9237 and those on new results for B(s)->mu+mu- by Johannes Albrecht seem to say: (1) first evidence for decay (3.5σ level) (2) data disfavours constrained SUSY at high tanβ.

22. **A.**
November 12, 2012

The BBC have picked up on this, reporting that "[The existence of SUSY] would help explain why galaxies appear to rotate faster than the Standard Model would suggest".

http://www.bbc.co.uk/news/science-environment-20300100

23. **Anant**
November 12, 2012

Dr. Woit I'm curious to know: what in your view will probably replace SUSY in being a solution to a variety of problems?

24. **Casey Leedom**
November 12, 2012

Ha. Funny. Even The Register has gotten in on this one:

http://www.theregister.co.uk/2012/11/12/supersymmetry_not_quite_dead

Casey

25. **Peter Woit**
November 12, 2012

Anant,
I don't think SUSY ever actually provided a solution to important problems in HEP. This is a long story, which I wrote about extensively in my book (written ten years ago). I pointed out that the case for SUSY was weak back then, it certainly hasn't gotten any stronger.

SUSY in the Hospital?

November 13, 2012
Categories: Experimental HEP News

HCP2012 Higgs results will be announced Wednesday (I'm hearing that CMS tau-tau signal is .7 +/- .5 x the SM value), but interest may focus much more on the strong SUSY exclusions being announced there. So far the LHCb result on B(s)->mu+mu- has been the one getting all the attention, with a BBC News story describing it as "a significant blow to the theory of physics known as supersymmetry". In the story experimentalist Chris Parkes describes the current situation as "Supersymmetry may not be dead but these latest results have certainly put it into hospital." John Ellis is having none of this though:

> Supporters of supersymmetry, however, such as Prof John Ellis of King's College London said that the observation is "quite consistent with supersymmetry".
>
> "In fact," he said "(it) was actually expected in (some) supersymmetric models. I certainly won't lose any sleep over the result."

The story has been picked up by Slashdot, and the Register (which has Supersymmetry takes an arrow to the knee).

The bad news for SUSY out of Kyoto however does not end there. ATLAS and CMS are both coming out with new analyses using this year's 8 TeV data which significantly expand previous limits on SUSY, getting close to ruling out popular last-ditch efforts to save the theory. For the latest, look at this HCP2012 page, this ongoing Chicago workshop, and CMS results announced here (like this), ATLAS results announced here.

The last ditch effort to save the idea that SUSY solves the so-called hierarchy problem goes under the name of "Natural Supersymmetry". It involves moving most superpartners to high masses where the LHC can't see them, keeping only the stop, sbottom, gluinos, and a couple neutralinos at LHC accessible masses. If you look at the bounds given on these masses for instance here, and compare to the latest LHC results, you'll see that there's trouble on all fronts for this idea, which is now very close to being essentially ruled out.

Gordon Kane is fighting back against the BBC by going to Lubos Motl's blog to argue that the LHCb result is no problem for his string theory "predictions". You see, his "prediction" is now that most superpartners are way beyond what the LHC can see, so not only is the LHCb result not negative for SUSY, but it "adds to the evidence for supersymmetry and for M/string theories". Kane doesn't mention any of the other SUSY excluding results coming out this week, or their implications for his "prediction" of the gluino mass. Last year Kane was telling Tommaso Dorigo

> I and others expect this decay to tops and bottoms is the signature by which gluinos will be found, with masses well below a TeV

and his slides had a gluino mass prediction of about 600 GeV. Late last year he was arguing that the gluinos would be seen by this past summer. More recently, he's modified the graph in his slides to move the gluino up to 1 TeV. This week ATLAS reports a new gluino mass limit of 1.24 TeV, so Kane will have to modify his slides yet again.

The combined effect of the bad press and the devastating experimental results on the 30 year old SUSY juggernaut will be interesting to follow. At this point, it is hard to see how one could rationally expect anything positive for SUSY to come out of further analyses of the 7 TeV and 8 TeV data. I suppose many will try and delay acknowledging failure by saying one must wait for the 13 TeV data, which we won't see until 2015 or so, but I don't think this is going to convince many people.

Update: Matt Strassler objects to the BBC article, on the grounds that one should not describe what is happening to SUSY in terms of "blows" or it getting hurt. Instead, one should stick to saying it is getting "cornered", and he agrees that "the cornering of supersymmetry is well underway". The question this raises though is what happens to SUSY once it is cornered if you object to it getting hurt. I suspect the hope of many SUSY theorists is that even once cornered, SUSY will continue to be well-treated, receiving annual $3 million prizes and being taught to new generations of graduate students.

Update: The Daily Mail covers this, with the non-tabloid-like headline Experts take conflicting opinions as to how far results support the theory of super symmetry. Oliver Buchmueller joins Gordon Kane in the bizarre game of claiming negative results as positive for SUSY, making the argument

> 'This is another piece in the puzzle and with it the world appears even more SM-like,' he said. 'It supports SUSY, because that is the only theory that can include the Standard Model in a wider concept of New Physics.'

Update: More about this from Matt Strassler, who writes about Theory Killers at the HCP conference. I guess it's still all right to kill "theories", as long as SUSY herself doesn't get hurt.

Update: The rather odd controversy over the BBC story goes on, with Lubos Motl and Matt Strassler continuing to argue that the scientific method implies that SUSY can't get hurt. I would have thought that it was uncontroversial that if proponents of an idea claimed that it would be vindicated by an experiment, and the experimental result came back negative, that was not good for the idea, but, at least for SUSY, that doesn't seem to be the case. Yes, SUSY comes in infinite varieties, many of them never testable, but the experimental results shooting down its versions sold as the most well-motivated ones do have implications for its health. I see no reason why one needs to wait for the LHC to examine every possible remote corner of parameter space that it can access before remarking on what has happened.

Update: It is being pointed out that the BBC story is inaccurate: the LHCb bounds on Bs meson decays that rule out a large chunk of SUSY theory space were already there in March. So, on this front SUSY entered the hospital in March, not this past week. Of course, an even bigger inaccuracy in the BBC story was describing SUSY as an idea that has only encountered serious health problems recently, rather than many years ago…

Update: There's now a fourth rant from Matt Strassler about the LHCb result, a topic on which he has become a bit of a zealot. The point being made is that SUSY was already so badly injured pre-LHCb that they didn't make things any worse. It's quite possible he's right about this, would be interesting to hear a response from the LHCb people.

Update: A characteristically lucid posting on the topic from Jester: BS and SUSY.

> To conclude, you should interpret the LHCb measurement of the Bs→μμ branching fraction as a general, strong bound on theories on new physics coupled to leptons and, in a flavor violating way, to quarks. In the context of SUSY, however, there are far better reasons to believe her dead (flavor, CP, hierarchy problem, direct searches). So one should not view Bs→μμ as the SUSY killer, but as just another handful of earth upon the coffin 😉

Comments

1. **qwerty18**
   November 13, 2012

   Hi Peter,
   Could you be a bit more precise about what you call "SUSY" here ? Is it the general idea of supersymmetry ? Some specific SUSY breaking models ? The MSSM ?

Thanks

2. **chris**
November 13, 2012

Well, the Machiavellian in me tells me to let them pursue SUSY further. All the better for me – less competition on interesting subjects.

3. **John A**
November 13, 2012

These attacks on string theory are giving Sheldon Cooper heartburn.

Personally I remain completely unimpressed by the constant retreat of SUSY and string theory to energy levels that are currently inaccessible. Any theory that can seriously do that without serious questions or even doubts is moving away from science and into the world of mysticism.

It's like the "God of the Gaps" retreat of religious claims in the face of scientific inquiry.

I suspect that the only way string theory goes away is when the mainstream academic institutions decline to pay for it any more. There needs to be a "Come to God" meeting of theoretical physicists to stop this useless hypothesis draining any more money away from the rest of physics.

4. **Bernhard**
November 13, 2012

I thought Ellis was going to give up on SUSY if it didn't show up until end of this year? Wouldn't now be the time to start saying this is becoming more and more likely? Actually I think what he said was for lower luminosity and center-of-mass energy so….

5. **MathPhys**
November 13, 2012

My private prediction is that SUSY will rise from the experimental ashes as a theoretical necessity. The argument will be that you don't need to see supersymmetry in the lab, but you need to assume that it's there or your models make no mathematical sense.

The reasoning will be that if you want to use quantum field theory, you must make sense of the divergences, which requires an embedding in a string theory, which requires supersymmetry.

6. **Roger**
November 13, 2012

As a HEP experimentalist I'm baffled by the extent to which my community pursues SUSY. Its been "around the corner" for as long as I've been in the field (coming up to 20 years now).

As a field, we have long played a scientifically dangerous game of ranking speculative theories we might see. TeV-scale SUSY has regularly topped the league table owing to it (a) providing a DM candidate (b) unifying the couplings and (c) addressing the hierarchy problem. This is the standard stuff that comes on our opening slides when we present our searches. We seem to have forgotten (or just plain ignore) that (a) works so long as we arbitrarily choose a certain LSP and hope that R-parity isn't violated and that WIMPs can account for dark matter. Similarly (b) works so long as one isn't particularly choosy about having an exact unification and is willing to buy the argument that no new physics is around for over 10 orders of magnitude of energy which would disturb the running couplings. As for (c) most experimentalists don't get that this is *the* reason for the theorists liking TeV-scale SUSY. The reason they don't get it is that they themselves are not particularly convinced by the argument.

We apply extremely rigorous reasoning in our own world i.e. in the preparation of our results. Its therefore odd that we allow ourselves to be seduced by speculative arguments when it comes to the topics we address.

As I mentioned at the start, ranking speculative theories is a dangerous game since it eats up finite resources when it comes to data analysis. There may be signatures of new physics sitting in our data which we have missed because of this. We'll likely catch these signatures anyway sooner or later but it would have been good if the experiments had, at the start, worked out what possible signatures they could observe and studied these without even attempting a ranking as to which are "best motivated" and therefore worthy of more attention.

7. **Bernhard**
November 13, 2012

Roger,

I agree SUSY is a saturated subject in collaborations, but given 30 years of theoretical propaganda it is difficult not to pursue this to the point of exhaustion. I mean, I am of course afraid sometimes on trigger bias based on SUSY models but for the time being it' s difficult not to do this. On the theory side that is a different story, but for experiments I think they are doing what they should – massacring SUSY without mercy.

8. **Pete**
November 13, 2012

I agree with Bernhard – just because SUSY does not exist, does not mean we experimentalists should not search for it. This is useful because at some point (if we don't find it…) theorists ought to go back to the drawing board and come up with a new idea. They probably won't do that, unless as Bernard says we "massacre it" with experimental results showing it is extremely unlikely to exist.

Of course the only worry is if that SUSY is completely wrong, we did produce the new physics in the LHC data, but failed to record any of it because we did not know how to look for it.

9. **Roger**
November 13, 2012

Bernhard and Pete
I'm certainly not proposing that we don't look for SUSY – part of my own research involves falsifying bits of SUSY parameter space. However, the obsession of SUSY has led to the field of exotics searches becoming lopsided in favour of chasing after models which we consider to be "well motivated". A well designed set of generic searches would have been more efficient and be as relevant for SUSY as for other approaches (including those we're not yet able to dream up). There was no need for us to catch the SUSY bug and we should have resisted.

10. **Roger**
November 13, 2012

There is also a more insidious side to this. Whenever I applied for grants, fellowships etc I would emphasise that my work could be used find supersymmetry. To not have played the "topical buzz word" card may well have led to my applications being rejected. I've also sat on committees where candidates have been turned down because their proposed searches were considered to be "unpromising".

We all pay lip service to the idea that we should be prepared to look for the unexpected and that history tells us to expect surprises etc etc. However, in reality, the field can be very conservative and the SUSY obsession, offering a "safe" research plan with lots of exclusions for legions of interested theorists to play with, is a part of that.

11. **Michael**
November 13, 2012

Hi Roger,

I appreciate your point very well. But to me it seems a little overstated. Sure there are lots of papers about "SUSY" searches, but most of those searches are able to pick out evidence for New Physics that does not come from Supersymmetry. The advent of the generic models, addressed regularly in the CMS and ATLAS papers, indicates a major shift away from theory-driven searches toward more signature-based searches. (Sorry for bringing in yet more buzz words.) I believe that most of the signatures that we experimentalists can think of are addressed in one way or another, within the constraints imposed by the triggers and event reconstruction. Perhaps I lack imagination, but it is not easy for me to come up with signatures – or perhaps "event topologies" is a better term – that are not covered somewhere, somehow. If you know of searches that someone could do but can't because the community still invests too much in SUSY, please say what they are. I'm sure many people would be interested. 😉

Michael

12. **Eric**
November 13, 2012

In light of the 126 GeV Higgs signal, it appears to me that the most likely scenario involving low-scale supersymmetry is that the superpartners have masses in the range 3-7 TeV. Furthermore, it seems very likely that R-parity is violated at some level. Given this, it is not at all surprising that supersymmetry has not been seen yet at the LHC. However, there is a very good chance that it will turn up in the next run when the LHC will run at somewhat higher energy.

13. **justin**
November 13, 2012

Is Gordon Kane the Karl Rove of superstring theory?

14. **Justin**
November 13, 2012

@Woit: SUSY may be hospitalized, but this raises the question of what should one work on? String theorists have always said that if you don't like their theory, then what do you have that's better? I've always thought that if someone came up with something viable, then string theory would be far less researched. But, it's a good question of: if not SUSY, then what? Until someone or some people come up with something that actually works, I think we'll continue to see many young and old physicists continue to waste their time on this fantasy.

15. **Peter Woit**
November 13, 2012

Justin,

I don't disagree that the main reason SUSY continues to get attention is a lack of promising ideas for BSM physics. This is an argument for continuing to do SUSY searches (the discussion between commenters above was quite good about this).

From the point of view of theorists though, the lack of a better idea is no justification for refusing to admit failure. Intellectual dishonesty is not a likely way forward towards a more promising research program.

16. **emile**
November 13, 2012

I can't think of a SUSY search being performed right now that should not be done even if one assumes there is no weak-scale SUSY. The problem has been in the past that sometimes parts of phase space were being ignored because MSUGRA did not provide a solution there. But in any case, I think it is important to convey to the wider audience that these searches are not a waste of time. The SUSY searches cast a wide net. Something other than SUSY could be picked up in that net.

@Mathphys: your theoretical necessity idea should be discussed within the field of philosophy or maybe math, and the associated research should not be supported by physics grants. I have the highest respect for my philosophy colleagues, so I'm not trying to demean the field at all, I just want to make it clear that once you take experimentation out of the picture, it is not science.

17. **Justin**
November 13, 2012

@Woit: I agree that SUSY proponents are being dishonest, [deleted]. But, my question is still this: is there any direction a young researcher might pursue if he or she wants to come up with something new?

18. **Igor Khavkine**
November 13, 2012

@Justin, I'm in no way an authoritative voice, but I think the only sensible answer to your question is to look at the data. Look at the data and try to figure out what is not accounted for by what we already know for certain. As far as I understand, every "new physics" discovery came about in this way. An alternative to that is to shift the focus to mathematical aspects. Theoretical physicsits move fast and often leave mathematical gaps in the theories that they are already convinced of by the data. Patching up these gaps does not require any new experiments, only serious thinking. (Incidentally, I've personally chosen to adopt the latter approach.)

Otherwise, sitting in your office and trying to imagine what new physics might be like is unlikely to get you very far, at least scientifically. I wouldn't be able to judge the effect on your career path.

19. **Peter Woit**
November 13, 2012

Justin + Igor Khavkine,

I'd rather people stick to the topic of the new SUSY results and not try and start an open discussion of "what should theorists work on". As I keep pointing out, this isn't a general physics discussion board…

20. **Shantanu**
November 13, 2012

Peter,
could you comment on effect of lack of evidence for SUSY (so far) on GUT based
theories and if there has been any discussion of this apart from Misha Shifman's proceedings
Many Thanks
shantanu

21. **Peter Woit**
November 13, 2012

Shantanu,

The LHC results don't have much to say one way or another about GUTs (I don't think GUTs make any testable predictions about LHC physics). One of the standard arguments given for SUSY is that the running of the U(1), SU(2) and SU(3) coupling constants in SUSY theories causes them to come together more accurately at the GUT scale than if you stick to the Standard Model, allowing a simple construction of SU(5), SO(10) and other SUSY GUTs. One problem with this is that the bounds on proton decay are starting to get significantly in conflict with this idea. Another is that it also requires believing in a "desert" of no new physics that affects the running of coupling constants between the TeV and GUT scales. For these reasons, this always seemed to me a rather weak argument for SUSY (although arguably the strongest one). That the arguments for SUSY all turn out to be quite weak, and that the LHC sees no evidence for it is a quite consistent situation.

22. **tulpoeid**
November 13, 2012

"Without it, you would have smart people saying smart things and stupid people saying stupid things. But for smart people to say stupid things, that takes susy."

23. **vmarko**
November 13, 2012

@MathPhys:

The reasoning will be that if you want to use quantum field theory, you must make sense of the divergences, which requires an embedding in a string theory, which requires supersymmetry.

Making sense of divergences has nothing to do with string theory, nor with supersymmetry. The Standard Model is renormalizable as it stands. The real theoretical issue that might serve as motivation for both SUSY and strings is the fact that gravity is perturbatively nonrenormalizable. And that problem can be attacked in many ways, SUSY/strings being only one of many possibilities.

So, neither SUSY nor string theory can ever be considered a theoretical necessity, unless they are backed up by experimental data. And that somehow is just not happening, as we can see…

24. **fuzzy**
November 14, 2012

"I would be more inclined to trust lawyers claiming for justice if I could believe that they gain nothing by doing that."

25. **King Ray**
November 14, 2012

Perhaps the only way to save SUSY is mouth-to-mouthino resuscitation . . .

26. **MathPhys**
November 15, 2012

@vmarko,

Okay, let's say that one requires "renormalizability, even in the presence of quantum gravity".

27. **P**
November 15, 2012

Hi Peter,

I'm confused by some of your statements and criticisms.

Which of the "infinite varieties" of SUSY that you mentioned are "never testable"?

And how many of the $3 million recipients work regularly on the types of SUSY theories you discuss on this blog, rather than the many other types?

P

28. **Peter Woit**
November 15, 2012

P,

Neither of those statements was intended as a crucial part of a serious argument about anything, but I think “never testable” pretty well describes SUSY theories with high enough SUSY breaking scales. Such theories seem to be more popular now that it is becoming clear the SUSY breaking scale can’t be low enough to solve the hierarchy problem.

As for $3 million prize winners, my comment was about future ones. The Milner prize is rather explicitly aimed at fundamental research not vindicated by experiment, but considered important by the leaders of the HEP theory community. If you’re someone with major contributions to SUSY research as your main professional accomplishment, your shot at the $3 million requires that the “SUSY is dead” meme not get traction among your colleagues. We’ll see what happens, but the lack of any successful BSM ideas coupled with yearly $3 million prizes for fundamental research targeted at HEP theorists may mean you’ll have a yearly competition to be seen as the person who had the least bad unsuccessful but high-profile idea, with SUSY part of the mix.

29. **P**
November 15, 2012

Hi Peter,

Okay, I agree with your clarified sentiment regarding testability – “never testable” really means “never testable if we don’t build sufficiently big colliders.”

Regarding the hierarchy problem, there’s a sliding scale, right? Even with O(TeV) SUSY breaking, there was still the little hierarchy problem. LHC might put bounds at O(10TeV) SUSY breaking, which we might call the “even bigger little hierarchy problem.” I know you don’t agree with the premise, but with this premise its not quite right to say that LHC will make SUSY unable to solve the hierarchy.

Regarding Milner prizes – I guess, but the “SUSY is dead” meme, if it gains traction, will only do so in the realm of N=1 BSM physics with low SUSY breaking scale for the sake of solving the hierarchy problem. Nearly all of the winners who won the prize have significant contributions to other areas of supersymmetric field theories, and the success of this work and future work on non-BSM SUSY stands firm regardless of the conclusions of the LHC. This is an important distinction to make, I think.

In any case, I agree with you regarding this prize and SUSY extensions of the SM. However, it’s hard to imagine someone getting the prize “aimed at fundamental research” for such work (at least, as their primary contribution) without experimental vindication.

30. **Bernhard**
November 16, 2012

P,

Logically speaking is a sliding scale yes, but why should anyone believe this will happen? So, it is not a matter logical possibility, it is a problem with the motivations for SUSY becoming increasingly weaker without its proponents admitting it for some reason. Look what happened with R-parity. Before the LHC results R-parity violating SUSY was ratter unpopular because it is required for the “explanation of dark matter”, now suddenly R-parity is old news and this particular motivation was just thrown under the bus. The biggest motivation is by far naturalness and as you correctly point out the little hierarchy problem is getting bigger. It is a logical possibility of course the SUSY is there with very heavy superpartners but the motivations used for that being a likely result are clearly being weakened by experiment. One could think this would lead legions of young and creative theorists to think “probably the answer is somewhere else” and show new exciting possible solutions, but instead what we see are theorists claiming SUSY suffered no scratch and this is simply plain wrong.

31. **P**
November 16, 2012

Hi Bernhard,

Thanks for the reply.

I agree with most of the things you say. Certain low energy SUSY models are definitely being cornered / “suffering a scratch”, and as these theories are cornered more and more some of the motivations for BSM SUSY particle physics begin to go out the door, on a sliding scale as we have agreed.

Re: “the answer is somewhere else” . . . the answer to what? The hierarchy problem? BSM physics in general? People have been looking at non-SUSY extensions of the SM and also non-SUSY solutions to the hierarchy problem for years. I don’t disagree that young particle theorists should be open minded, but am only pointing out that other many other possibilities have already been considered. Data is coming in and is constraining both SUSY and non-SUSY possibilities. Very exciting!

Cheers,
P

32. **Bernhard**
November 17, 2012

P,

I agree…

33. **Kenneth W. Regan**
November 17, 2012

Saw this via “Marginal Revolution”. What about the hypothesis expressed in Leonard Susskind’s *The Cosmic Landscape* that SUSY holds only in the “dead” ground-state of the meta-stable sequence? Is that being “cornered” too, or only whether SUSY holds in our world (where we have Lambda > 0 etc.)? In any event, my impression from the book was definitely not to expect any our-world experiments upholding SUSY.

34. **P**
November 17, 2012

Kenneth,

Peter would probably object to this thread going down the direction you’ve suggested, since it brings on a whole host of other issues – but it suffices to say that (based on context) the SUSY you bring up is a reference to a SUSY vacuum in a landscape of metastable SUSY breaking vacua. This is something different: our world is definitely NOT in a SUSY vacuum, and moreover this isn’t really the context in which SUSY is being tested at LHC.

— P

35. **Peter Woit**

November 18, 2012

Kenneth,

At one point Susskind had hopes that you could make statistical arguments based on the Landscape, such that you might find it was statistically probably that SUSY would be seen at LHC energies (or that it was statistically unlikely). Turns out this doesn't work, the Landscape can't be used to say anything about this one way or the other (or about anything else, this was supposed to be the most likely thing that could be predicted this way). So, landscapeologists like Susskind would happily claim SUSY as evidence for string theory if it appears, also claim it is no problem for string theory if it doesn't.

I wrote about Susskind's book here
http://www.math.columbia.edu/~woit/wordpress/?p=307

in particular:

Susskind talks about the LHC and the question of whether the fine-tuning problem of the Higgs mass will be resolved by supersymmetry or is anthropic. He acknowledges that, based on Landscape arguments:

"My original guess was that supersymmetry was not favored, and I said so in print. But I have changed my mind — twice — and probably not for the last time."

36. **fuzzy**
November 18, 2012

i think that when we call "susy" that stuff we use an improper name. (btw, do you know who created this acronym? i heard it was john ellis, but is it true or just a rumor?) i think that a much more apt acronym would be "seotsmjatewsawrpifabmtanfal" =supersymmetric extension of the standard model just above the electroweak scale and with r-parity imposed forcefully, advocated by many theorists and not found at lhc. i can admit that the name "seotsmjatewsawrpifabmtanfal" is not as appealing as "susy", but it is honest: indeed there is no convincing theoretical reason why the supersymmetric extension should be at the electroweak scale (the scale of mass is not predicted) or why r parity should be conserved (gauge invariance would suggest that it isn't). furthermore, if we will find something like that at lhc, we will remove the "n", the acronym will reduce to "seotsmjatewsawrpifabmtafal", and its appeal will grow in an adequate manner.

37. **John Adams**
November 19, 2012

Rather new to all this, I'm afraid. But I do have a simple question. If supersymmetry is not found, or even proven incompatible with new evidence, would that in itself kill off superstring theory and all its close relations – M theory and the like – or have these theories developed in such a way that they could survive the death of supersymmetry?

38. **Peter Woit**
November 19, 2012

John Adams,
Superstring theory/M-theory unification predicts essentially nothing (I wrote a book explaining more about this…), so you can't kill the idea with experiment. There was a day when string theorists would often object to this claim by pointing to SUSY, but not these days. However, if SUSY is found, I can predict that you definitely will hear a lot again about how this is evidence for string theory…

39. **John Adams**
November 19, 2012

Peter,

Thanks for the reply. I've ordered and am currently awaiting the arrival of your book. From what I can gather string theory makes essentially no predictions, so I understand your point. And I certainly understand that string theorists would be doing somersaults if supersymmetry was found. But my point is does string/M theory absolutely *require* supersymmetry. What would happen to string/M theory if it were demonstrated scientifically that supersymmetry was impossible.

Let's take an outlandish example to make my point. Imagine that tomorrow Ed Witten discovered beyond any shred of doubt that an overlooked part of supersymmetry demanded that the speed of light was minus 1 metre per second. We can all accept that then SUSY would be dead. But what would it mean for string/M theory. Would they fall too, or have they been developed the last couple of decades in ways such that they could bear SUSY's death?

40. **Shantanu**
November 20, 2012

John, This has been discussed several times on this forum. string theory is consistent with everything, including mutually opposite ansatzs

41. **Peter Woit**
November 20, 2012

John Adams,

The problem with supersymmetry is that it must be a broken symmetry, and the problem with string theory is that, to the extent it requires supersymmetry, it says nothing about the breaking scale. It could be at the Planck scale, completely irrelevant to any conceivable observation.

42. **P**
November 21, 2012

John,

Here's a bit more precision. String theory does NOT require spacetime supersymmetry, but broad classes of compactifications do give rise to N=1 supersymmetry in four dimensions.

Regarding the LHC and experiments, the question then is the SCALE of supersymmetry breaking, as Peter points out. It's too vague (and one could argue incorrect) to say that string theory says nothing about the breaking scale.

The key point is that, as in quantum field theory, is that one has to study the vacuum state. Individual string vacua make very concrete predictions – I'd be happy to tell you about them and cite papers – including the SUSY breaking scale, if it is broken at all. In most vacua the SUSY breaking scale is high, "irrelevant to any conceivable observation", as Peter points out.

It's too vage, misleading, and fundamentally incorrect to make the blanket statement "string theory does not make predictions." The problem of predictions in string theory is NOT that individual vacuum solutions don't make low energy real-world predictions, but instead that there are an extremely large number of vacuum states. This observation applies to predictions about the SUSY breaking scale (hence Peter's objection), and also predictions about the low energy gauge theory, for example.

43. **H Luce**
November 21, 2012

http://www.youtube.com/watch?v=KuStsFW4EmQ neatly summarizes the response of the established string theory community to any challenges… never mind that their hypotheses are untestable.

44. **john McAllison**
November 21, 2012

Peter,

I think you're letting yourself down a little by describing Matt's update on SUSY as a "rant"; I'd describe it as a fourth installment. He's just presenting his side to the argument in a calm rational way and I'd hate to see you lower your current high standards of fairness compared to those of others I can mention. It's impressive the way you link to Matt's and, unbelievably, Lubos's blog for example.

45. **Peter Woit**
November 21, 2012

John McAllison,

Perhaps you should take that description as somewhat tongue in cheek… Any one who cares though is encouraged of course to follow the links and see what Matt and his commenters have to say. The whole SUSY business seems to me to have led to some rather odd behavior among physicists, but others can judge for themselves.

46. **zevans23**
November 23, 2012

BBC Radio 4's "Material World" weekly radio magazine has a good high-level interview with Dr Tara Shears, and does convey the "not even wrong" message well. Essentially the conclusion is "wait for higher energy."

Podcast (available to all countries!) here
http://www.bbc.co.uk/podcasts/series/material/all – you want Thu 15 Nov.

47. **Kavanna**
November 30, 2012

Does SUSY have feelings? Have those feelings been hurt? Or is the damage more serious, more physical?

Here's the thing about SUSY: the general idea of supersymmetry is probably right. It's the last major symmetry that grows out of spacetime that hasn't been seen. But you can't test a general idea, just a specific theory. And, unfortunately, theorists long ago painted themselves into a corner by narrowing SUSY to a very specific implementation: quasi-perturbative, closely paralleling the non-SUSY Standard Model.

So what if that implementation of SUSY isn't seen? Maybe SUSY is manifested in some very different way. SUSY is neat; breaking SUSY is an arbitrary mess. Maybe Matt has something to say about this 🙂

New Higgs Results

November 13, 2012
Categories: Experimental HEP News

New results about the Higgs should appear over the next day or so, perhaps first here and here. First to appear is the CMS tau-tau result which is a signal strength of .72 +/- .52 the SM value.

Will update this posting with the results as they appear.

I guess the rumors were right: no update from ATLAS for gamma-gamma, they're just presenting the earlier data: signal strength of 1.8 +/- .5 times the SM value. "Ambitious campaign underway to include a larger data-set."

ATLAS WW channel signal strength is 1.5 +/- .6 times the SM value (see here). For some reason they are only using 2012 8 TeV data, not combining with the 7 TeV data.

Atlas tau-tau signal strength is .7 +/- .64 times the SM value (see here). Very similar to the CMS value. They are not yet sensitive to the bb channel, where the Tevatron has the advantage and has evidence of a signal.

Update: All new results are out now, but rather anti-climactic since neither CMS nor ATLAS is reporting new results for the channel with the strongest signal (gamma-gamma), where the signal strength may be anomalously high. ATLAS is also not reporting for the other high statistics channel (ZZ). CMS is, with signal strength .8 +.35/-.28 the SM value. They also claim to rule out (at 2.5 sigma) the hypothesis that this is a pseudoscalar rather than scalar. Everything reported is in line with the SM, but for the most interesting numbers we may have to wait until next March (Moriond).

For much more details about all this, see Tommaso Dorigo here and here, Matt Strassler here and here, Philip Gibbs here. For primary sources, CMS here, ATLAS here, presentations here.

Update: For another excellent source, see Jester, who has the scoop on why ATLAS and CMS didn't update some channels:

> It came to a point where the most exciting thing about the new Higgs release was what wasn't there 🙂 It is difficult not to notice that the easy Higgs search channels, $h\to\gamma\gamma$ and ATLAS $h\to ZZ\to 4l$, were not updated. In ATLAS, the reason was the discrepancy between the Higgs masses measured in those 2 channels: the best fit mass came out 123.5 GeV in the $h\to ZZ\to 4l$, and 126.5 GeV in the $h\to\gamma\gamma$ channel. The difference is larger than the estimated mass resolution, therefore ATLAS decided to postpone the update in order to carefully investigate the problem. On the other hand in CMS, after unblinding the new analysis in the $h\to\gamma\gamma$ channel, the signal strength went down by more than they were comfortable with; in particular the new results are not very consistent with what was presented on the 4th of July. Most likely, all these analyses will be released before the end of the year, after more cross-checking is done.

Update: Two interesting workshops on prospects for a Higgs factory are being held at Fermilab this week. See here for one that is on-going, see here for Tuesday's one-day mini-workshop on prospects for a muon collider Higgs factory. I'd never seen much before about the idea of using a gamma-gamma collider to study the Higgs, curious if that's really technologically viable.

Comments

1. **Stephan**
   November 13, 2012

   Peter,

   this all looks as if the experiments really might have a problem with the signal strength of the Higgs bump. Any rumors on whether the Higgs is disappearing or is confirmed?

2. **Peter Woit**
   November 13, 2012

   Stephan,

   The rumor about ATLAS is not that the signal is going away, but that they were worried about calibration problems due to seeing peaks at different masses in gamma-gamma and ZZ. It would of course be very interesting to hear more about what is really going on. I also hear that CMS has some concerns about their gamma-gamma signal, we'll see soon how much they report.

   Part of the problem here is probably the way they are trying to do blinded analyses, relatively quickly. If you don't unblind until right before the date you need to have a result, when something confusing shows up when you unblind, you may not have time to figure out what is going on. One thing you don't want to do is publicly release data that has some sort of consistency problem that you don't understand.

3. **Yatima**
   November 14, 2012

   "Hangout with CERN: What's new with the Higgs? + more LHC results"

   Scheduled, 15. November, 17:00

   https://plus.google.com/events/cqlh9ffeijibmiir5n7j2u12gfs

4. **tt**
   November 14, 2012

   there has to be a ZZ-top quark joke somewhere.

5. **Thomas Larsson**
   November 15, 2012

   It seems to me that the SM is looking more and more like epicycle theory, in a good way: an ad hoc model with a limited set of parameters that quantitatively predict thousands or millions of observations to high accuracy. If this analogy bears out, it is bad news for BSM physics in general. There was never any beyond epicycle physics; Kepler did not observe any qualitatively new phenomena, but merely looked at the old data in a new way.

6. **A**
November 15, 2012

So if the gama gama signal is problematic or starting to dissapear, does that mean the Higgs is beeing undiscovered? Gamma was the strongest signal. Are they going below 5 sigma?

7. **Thomas Larsson**
November 15, 2012

According to Jester, nobody cares if it's 5 sigma or 11 sigma – Higgs is there. Decreasing gamma-gamma means that Higgs becomes even more SM-like, I think.

8. **Peter Woit**
November 15, 2012

A,

The new ZZ results that CMS reported strengthen even more the Higgs signal. If it was going away, it would be going away here too.

Given that the gamma-gamma signal was higher than the SM prediction, if it is a SM Higgs it's not surprising that the signal would go down in the new data. If the reason for the earlier signal being high was not just a fluctuation but a systematic problem with the analysis, it's also not surprising that with more data, this problem becomes more obvious. That most likely is all that is going on, with figuring it out a time-consuming business.

Of course, since we haven't seen either experiment's new gamma-gamma results, who knows, maybe there's more to it. But I haven't heard such rumors, and if there were a serious chance that the gamma-gamma would completely go away, that would be hard to keep 6000 people quiet about…

9. **A**
November 15, 2012

Wait. It was not the decrease in the gamma gamma that worried me but the size of it. I understood Jester to write that at CMS the signal decrease is substantial. Too much for comfort. To the point of beeing inconsistent with the July results. So one naturally wonders if these sytematic problems affect other channels as well.

10. **Peter Woit**
November 15, 2012

A.,

I don't see any reason to believe there's a problem which is going to affect both of the main channels at both experiments (and CMS explicitly is claiming no problem in the ZZ channel by releasing that analysis).

This situation still looks to me like something not unexpected when you have a large collaboration with many cautious people trying to do an analysis on an extremely tight schedule. The way these people work they really hate to release something publicly unless pretty much everyone is happy that things are well-understood.

11. **JollyJoker**
November 16, 2012

How significant is the difference between 123.5 and 126.5 GeV for relative probabilities of the different channels? I find it intriguing that they both feel something's wrong with their analysis of gammagamma. As if the Higgs is different from the SM one in ways no one expected 🙂

12. **N.**
November 17, 2012

Hi all,

PhyOrg says this is a hint towards SUSY.

http://phys.org/news/2012-11-confining-supersymmetry-lhcb-evidence-rare.html

Is there something to it?

13. **Peter Woit**
November 17, 2012

N.

The story says exactly the opposite

"The result further shrinks the region in which scientists can still look for supersymmetry."

Please see the previous posting for extensive discussion of this.

14. **N.**
November 17, 2012

Sorry,

got it wrong.

:(n.

15. **DB**
November 18, 2012

Much as I would like to see the construction of a muon collider, and the low mass of the Higgs perfectly suits such a machine, I very much doubt if any serious attention will be paid to building one unless the LHC uncovers anomalous behaviour of the Higgs which could only be investigated by a high-precision examination of Higgs properties and decay modes.

16. **fuzzy**
November 18, 2012

just one remark on big machines and hep. everybody knows which are the machines that helped the field to progress. now, we can check on the "inspire" database how many conferences have their name in the title. some outcome is,

LHC 268
HERA 50
LEP 43
Neutrino factory 25
Muon collider 16
SPS 12
Kamiokande or Super-Kamiokande 0 (=naught)

17. **Tmark48**
November 18, 2012

@ DB : don't you think it's a bit hasty to start building a muon collider ?
First lets use the LHC to its full potential, then when it starts becoming obsolete either tecnologically and/or scientifically we can begin financing a new collider. But I don't think it will happen for at least a good decade. Or we could ask the Americans to build one. Europe has done its fair share building colliders, wouldn't it be time for the American hep community to take up the slack ?

18. **Peter Woit**
November 18, 2012

Tmark48,

As far as I know, there's no danger of anyone starting to build a muon collider soon, the technology is just not ready.

The more serious issue DB is alluding to is that of whether one can justify building any sort of Higgs factory if the LHC data on the Higgs shows no signs of a deviation from SM expectations.

19. **Anonymous**
November 18, 2012

Re: No funding for a post-LHC collider. Hasn't the Japanese government all but volunteered to fund one if it's placed in Japan? Unless that changes, isn't the ILC or a muon collider probably a go? Or is there reason to worry that a no-obvious-deviation Higgs might be so uninspiring as to make even a large percentage coming from the Japanese government insufficient?

Latest from Arkani-Hamed, Ellis and Gross

November 20, 2012
Categories: Uncategorized

Three of the leading figures in HEP theory have today or recently spoken about their current view of SUSY in light of the negative LHC results, here's a report:

- At the IAS recently, Nima Arkani-Hamed spoke on The Inevitability of Physical Laws: Why the Higgs Has to Exist. Yuri Milner was in the audience, and I gather that this (and Maldacena's recent similar talk) was intended to fulfill the promise of giving a public talk that came with their receipt of $3 million Fundamental Physics Prizes.

  Both Arkani-Hamed and Maldacena talked not about their own work, but about the Higgs, with Maldacena emphasizing the importance of gauge symmetry, Arkani-Hamed the constraints imposed by unitarity and Lorentz invariance. At the end of his talk, Arkani-Hamed gave a big advertisement for SUSY, with a new and somewhat bizarre argument I hadn't heard before. He argued that since QM + special relativity imply that elementary particles must have spin 0,1/2,1,3/2,2, and until recently 0 and 3/2 were missing, the fact that 0 (the Higgs) has now been seen implies (by the "totalitarian principle" that everything that can happen must happen) that the next thing to be discovered will require a spin 3/2 particle and this needs SUSY. Of the various weak arguments put forward for SUSY, this seems to me to be the weakest yet.

- At CERN today there was a 70th birthday celebration for Chris LLewellyn Smith. I didn't watch John Ellis's talk, but his slides are here. Evidently he argued that SUSY is not dead yet, pointing to the latest paper from the MasterCode collaboration. Their most recent CMSSM SUSY "predictions" have gluinos at either 2000 GeV (hard for the LHC to see at full energy after 2014) or 4000 GeV (impossible for the LHC ever to see).

  No mention was made of the similar pre-LHC predictions (see here and here) which had the Higgs at around 113 GeV, gluinos at 700 GeV or so, and squarks lighter than this, all of which have been shown to be radically mistaken. For some perspective on this on an even longer time scale, take a look at this 1984 survey article, Supersymmetry – spectroscopy of the future? : or of the present?. It gives much the same enthusiastic motivation for SUSY that we still get in all SUSY talks, with Ellis optimistic that the latest data had hints of SUSY with sparticle masses around 40 GeV ("nicely compatible" with the recent discovery of a 40 GeV top quark…).

- Also speaking today was David Gross, and I did get a chance to watch his talk. He commented on Ellis's claim that SUSY was not yet dead by noting "we see no signs of life either", then went on to lay out two "extreme scenarios". The pessimistic one would be nothing but the SM at LHC energies and no detection of dark matter. In that case, about SUSY he commented that it "could be that Nature does not take advantage of this", which I think is the first time I've ever heard him raise this possibility. The optimistic scenario was the usual picture sold pre-LHC: detection of SUSY and dark matter, non-SM Higgs. Gross said that he's an optimist, but gave no argument for the optimistic scenario beyond the one that it's a good idea in life for a scientist to be an optimist.

Update: More at Nature about SUSY's problems and quotes from its defenders.

Update: Over at the Simons Foundation web-site, there's an excellent new article about the SUSY debate by Natalie Wolchover.

Comments

1. **Roger**
   November 20, 2012

   Spin 2 is missing also, as no one has seen a graviton.

2. **Paul Titze**
   November 20, 2012

   @Roger,

   The effects on a particle (change in position or spin) by a single graviton is indistinguishable from quantum fluctuations and since they have no electric charge do not interact with photons either.

   How would one be able to observe a single graviton? (not gravitational waves).

   Cheers, Paul.

3. **Peter Woit**
   November 20, 2012

   I realize that no matter what I post about, people will want to discuss quantum gravity. Sorry though, that's off topic. Find somewhere else to argue about the graviton.

4. **Bob Jones**
   November 20, 2012

   "He argued that since QM + special relativity imply that elementary particles must have spin 0,1/2,1,3/2,2, and until recently 0 and 3/2 were missing, the fact that 0 (the Higgs) has now been seen implies (by the 'totalitarian principle' that everything that can happen must happen) that the next thing to be discovered will require a spin 3/2 particle and this needs SUSY."

   Peter, I think you're really twisting Arkani-Hamed's words when you say this. He was not using the totalitarian principle to argue for SUSY; in fact, he never even mentioned the totalitarian principle. This phrase did appear on one of the slides that he skipped, but it looks like it was a slide about quantum mechanics, not SUSY.

   His actual argument was something very different. He was saying that there are theoretical reasons why we can only discover elementary particles having spins 0, 1/2, 1, 3/2, and 2, and SUSY is required for spin 3/2. So because of theoretical constraints, SUSY is one of the very general ways in which new physics can show up in particle accelerators, and that's why people are looking for it. In Arkani-Hamed's own words,

   "The reason why there is so much excitement about supersymmetry is—it doesn't mean that it has to be right—it's the last remaining thing. It's the last thing that nature could do, in principle, that we haven't seen it do. And we saw something fairly dramatic with the Higgs already. We saw the zero option used, so it's not crazy that it's going to come along with the whole thing, we'll finally see the whole panoply actually used in the way nature works."

   At no point did he say that SUSY was definitely going to be discovered, or that it had to exist for theoretical reasons. All he said was "We haven't seen it yet, but it's something that we are looking for."

5. **Peter Woit**
   November 20, 2012

   Bob Jones,
   Perhaps you're right that the "totalitarian principle" business had nothing to do with the argument for SUSY, it was on a slide he didn't have time to explain so one can't be sure. But I don't think I'm really misrepresenting his argument. I just looked at it again, and it's clearly that spin 3/2 is the only non-observed spin on the list compatible with QM + special relativity and you need SUSY to get spin 3/2 consistently. Of course this is not an argument that you MUST have SUSY (none of the arguments for SUSY have ever been that you MUST have it), it's an argument for likelihood of SUSY. And, if you ask me, a weaker one than any of the other standard ones for the likelihood of SUSY. Anyway, people interested are encouraged to see for themselves, it's the last few minutes of the talk.

6. **Bob Jones**
   November 20, 2012

   "Of course this is not an argument that you MUST have SUSY (none of the arguments for SUSY have ever been that you MUST have it), it's an argument for likelihood of SUSY."

   Well, what you said was that, according to Arkani-Hamed, the discovery of the Higgs "implies" that the next thing to be discovered will be a spin 3/2 particle. When you say that a statement A implies another statement B, you mean that B necessarily follows from A. It doesn't just mean the B is possible.

   Also, I don't think that the discovery of the Higgs makes it more likely that we'll see a particle with spin 3/2. Arkani-Hamed is just saying it's worth looking for because it's one of the only ways for new particles to exist. The discovery of the Higgs shows that it's reasonable to look for new particles whose spins lie in this small set of possibilities.

7. **Peter Woit**
   November 20, 2012

   Bob Jones,

   Yes, I was not using "implies" in the mathematician's usage of logical implication, rather the colloquial usage of "suggests". It didn't occur to me that anyone would think this was a context where a claim of logical implication was being discussed. I did, I think accurately, describe it as an "advertisement".

   Also note that I wrote "the next thing to be discovered will require a spin 3/2 particle" (i.e. a form of SUSY with a spin 3/2 particle), not "the next thing to be discovered will be a spin 3/2 particle". The particles SUSY enthusiasts expect to show up in LHC data don't include the spin 3/2 one (the gravitino) itself.

8. **martibal**
   November 20, 2012

   Peter, I am a bit confused:
   "you need SUSY to get spin 3/2 consistently"
   then
   "note that I wrote "the next thing to be discovered will require a spin 3/2 particle" (i.e. a form of SUSY with a spin 3/2 particle), not "the next thing to be discovered will be a spin 3/2 particle""

   Excuse the naive question, but is SUSY the only way to have spin 3/2 particle in QM + special relativity ? (so, say, in QFT) ? I am not asking if this is the most common way explored by physicist, but if this is the unique way. If yes, then the argument does not seem so weak, at least it raises an interesting question: why spin 0, 1/2, 1 and – assuming graviton – 2, but no 3/2 ? Why a gap in the spectrum of the spin ?

9. **Peter Woit**
   November 20, 2012

   martibal,
   I'm no expert in the kind of argument that Arkani-Hamed was referring to claiming you need SUSY for spin 3/2, and don't have time now to look this up, maybe someone else can get you a reference, and/or comment on the strength of such arguments.

   I just don't see the spin 3/2 "gap" as much of an argument. Part of this is a philosophical difference with Arkani-Hamed (I think the fields of the SM have specific and fundamental geometrical significance, with no particular reason to go to the kind of supergeometry where you get spin 3/2, whereas I would guess his is an effective field theory philosophy where the SM fields are nothing particularly fundamental). However, even if one accepts effective field theory philosophy, I still don't think the argument is strong: the list of things we don't understand about a more fundamental theory could include an explanation of why no spin 3/2.

   Finally, all this has the problem that, unlike the hierarchy argument, it says nothing about the scale of SUSY breaking, so gives no indication of what energy scale one would see this spin 3/2 particle at. Arguments about how we must have SUSY, but which say nothing about its breaking just seem to me rather empty.

10. **P**
    November 20, 2012

    martibal,

    The spin-3/2 analog of the Dirac equation is called the Rarita-Schwinger equation. Some quick looks around the web show (arXiv and Wiki) that when coupled to electromagnetism there are certain modes with propagate at faster than the speed of light.

    On the other hand, supergravity definitely requires spin-3/2 fields and is consistent. Maybe someone can chime in some details about Vasiliev theory . . .

    P

11. **Dave Miller**
    November 20, 2012

    Peter (and anyone else),

    I'm confused by the claim that you cannot have fields higher than spin 2. Obviously, there are composite particles with spin 5/2, 3, and so on to enormously higher values. I thought it was obvious that such particles could be represented by quantum fields.

    Of course, their interactions are going to be a mess, but is there any objective criterion of simplicity that rules such interactions out of court?

I thought the whole idea of effective field theory was that the "real" underlying theory was God-only-knows-what, so that the "elementary" particles might be some weird incomprehensible sort of composites, but that our effective field theories nonetheless worked well enough at energies we can probe. And, so, there ought to be effective field theories to deal with the existing spin 5/2 or greater particles.

I assume I am missing some subtlety or fine distinction here: my education is so ancient as to pre-date the growth of the effective field theory idea (although I did take QFT from Steve Weinberg, whence I acquired the idea that *anything* can be represented by a quantum field!). I hope someone can fill me in on what I'm missing.

Dave Miller in Sacramento

12. **A.J.**
November 20, 2012

@Dave Miller:

A more precise statement is that you can't have such fields in a continuum QFT satisfying the conditions for the Coleman-Mandula theorem (which says basically that there would be so many conserved currents that the scattering matrix would have to be trivial). It's fine to have such fields in effective field theories, and it's fine to have them in QFTs where Coleman-Mandula doesn't apply.

13. **Noah Smith**
November 20, 2012

There is something soul-crushingly sad about watching SUSY theorists try to claim after the fact that the predicted the Higgs mass…

14. **Peter Woit**
November 20, 2012

Noah,

Not really sure who you have in mind. The only one of the three theorists in the posting who had a precise Higgs prediction pre-LHC was Ellis, who was claiming that just about the LEP limit was the most likely value (that didn't work out). Lots of SUSY theorists though have claimed vindication that the mass came out not so high that it couldn't be reconciled with the MSSM.

15. **jinb**
November 20, 2012

If one takes a predictive model with free parameters and fits the indirect and direct constraints from experimental data, then one gets a chi squared for the range of these parameters, with a best fit point. With new data these fits can be updated to give a new best fit point.

When you do this exercise with the standard model before the LHC, the best fit point is under 100 GeV. With more data and direct searches, this best fit point moves up. With a lack of signal and a larger portion of the parameter space being constrained, you could also call that a desperate attempt to move the "prediction" to keep the theory alive.

But it's not, it's just updating the best fit of the free parameters, in the case of the standard model the only free parameter being one: the higgs mass. When gfitter kept updating their electroweak fits pre-Higgs discovery and their Higgs mass "predictions" kept moving up in light of LEP constraints, it wasn't regarded as an arbitrary post-diction.

Doing these best fits for various minimal models of supersymmetry, with many more parameters of course, is no different. As more direct and indirect constraints are applied, the best fit points will shift, so the gluino mass will move up and so on. The best fit point by definition is the "more likely", it's just the minimum of the chi squared that comes out of the parameter fit to data, not a post-dicted "prediction" of where we will definitely find e.g. The gluino!

Before the higgs discovery the best fit to data for the one parameter model of a standard model Higgs was no different in spirit to the many-parameter fit of the CMSSM of e.g. Mastercode. How strongly one views these best fits as closing in on the gold coin hidden in the cake or evidence of a lack of gold coin is down to personal opinion, but if you belong in the latter camp it's just dishonest to report these best fits as desperate attempts to keep a theory alive or change predictions, which then gets picked up by sensationalising science journalists and writers who get their information disproportionately from this blog.

16. **Peter Woit**
November 20, 2012

jinb,

In the case of the Higgs, electroweak fits did a good job of, yes, predicting where the Higgs would be (the mass range it was likely to be in). If the Higgs had turned out to be at, say 300 GeV, they would tell us that there was something seriously wrong with the SM.

I'm not sure what the point of the SUSY fits now is. pre-LHC, they were advertised as predictions of SUSY spectra. What's the lesson to be drawn from the identified likely regions being excluded (which is not what happened in the Higgs case)? Is it just that one needs to do new fits?

17. **chris**
November 21, 2012

why in all the world would no susy and no DM be "pessimistic"?

i guess hep theorists are becoming lazy (or just old on average). the excitement of a 40 year old theory (SUSY) or a 80 year old (DM) being vindicated is not exactly the optimistic scenario i am dreaming of. as an optimist i hope that from experimental evidence we get some really new knowledge about a yet unknown principle of nature. susy and dm are actually really boring.

18. **chris**
November 21, 2012

"it's clearly that spin 3/2 is the only non-observed spin on the list compatible with QM + special relativity"

actually … for m=0 there is no half-integer restriction to the spin of a particle. why don't we see any of these?

19. **Dave Miller**
November 21, 2012

A.J.,

As I assume you (and everyone else) know, Coleman-Mandula rules out mixing internal and spacetime symmetries in non-trivial ways. (Of course, SUSY evades that with the anti-commuting, fermionic symmetries.)

But, I do not see how or why Colemna-Mandula rules out higher spin fields. In fact, the point I took away from my QFT class with Weinberg is that you can *always* construct such fields to represent composite systems. Of course, you may choose to build those fields out of "simpler" fields. Or not.

Now, maybe Steve was just way off base. And, I certainly may really be missing something. I'd appreciate it if anyone could tell me just where Steve (or perhaps my (mis)understanding of Steve) is wrong.

Thanks.

Dave

20. **A.J.**
November 21, 2012

@Dave Miller:

The point is that C-M restricts the kinds of conserved currents you can have in the QFTs to which it applies. This is a problem if you want higher spin fields in your theory, because you'll need to couple these fields to a current which C-M says can't exist. Why? It's a generalization of the argument that spin 1 particles need to couple to conserved currents. There are terms in the Fourier space propagator that don't fall off fast enough with momentum and so will give divergences in loop integrals if they isn't gotten rid of by coupling to a conserved current. This works OK for spin 1 theories; you can couple to a curren with 1 vector index, and the conserved charge will generate an internal symmetry. For spin 2, you are stuck coupling to the stress-energy current. Any other conserved tensor current would give a Noether charge that wasn't energy-momentum; this is the argument that any 'fundamental' spin 2 particle must be a graviton. (For spin 3/2, you can couple to the super-current, if you have one.) For higher spin, you'd get conserved currents whose dotted and undotted indices number at least 3. C-M says there aren't any.

My understanding is that this argument is due to Weinberg — at least the spin 2 part — but I don't know the original literature.

There are ways around this argument, of course. You can work in a cut-off theory, and the loop integrals won't get to cause trouble. This covers all the cases involved in nuclear physics. You can introduce towers of higher spin fields, like in string theory, and arrange for the bad terms terms in the propagators to all cancel out. You can work in theories with no mass gap, like CFTs, and then you'll have a few more currents available.

21. **Peter Shor**
November 21, 2012

Did David Gross leave out the detection on non-SUSY dark matter by the LHC? Does he believe this is unthinkable? It seems to me that it would be even more of a blow to SUSY than his worst-case scenario of not seeing anything. (Although it would presumably be great for high-energy physics in general.)

22. **Peter Woit**
November 21, 2012

chris/Peter Shor,

To be fair to Gross, his discussion of the "optimistic scenario" was very short, and he clearly didn't intend to characterize discovery of non-SUSY new physics at the LHC as not falling under this scenario. I'm pretty sure most SUSY advocates would drop it like a hot potato if there is solid new physics to investigate that clearly doesn't fit at all in the SUSY framework.

23. **Chris Oakley**
November 21, 2012

Chris Llewellyn Smith obviously did some great work at CERN – their appreciation comes across clearly in the slides. More puzzling, though, is his subsequent career: he became Provost of University College London, but this was a poisoned chalice as it was in this capacity that he was responsible for cutting everyone's budget. A palace coup followed and he was forced out. He then re-surfaced in something completely unrelated to High-Energy Physics: director of the UK fusion program. Don't ask me how this happened: I was not even aware that he had an interest in the subject.

24. **Shantanu**
November 21, 2012

chris, its good to remember that there is absolutely ZERO evidence from astronomical observations alone that dark matter has electroweak interactions or anything to do with
TeV scale physics. (The only argument given is the "WIMP miracle", but I think that is completely oversold and the limits from direct DM are already killing this strawman argument).
It could very well be all axions or all primordial black holes.

25. **Dave Miller**
November 21, 2012

A.J.,

Thanks for going to the trouble to fill in the details I was asking for — I appreciate it.

If you or anyone can remember any references that explain all this in more detail, I'd be interested in seeing the references. I'll check and see if I can find any of Steve's stuff that goes into it — this whole issue does seem to be something that has preoccupied him off and on over the decades. (Here is a popular article where he discusses his interest in the composite vs. elementary particle issue over the years: http://www.slac.stanford.edu/pubs/beamline/27/1/27-1-weinberg.pdf.)

Thanks again for your replies.

Dave

26. **Chris Austin**
November 22, 2012

Weinberg's original paper showing that massless spin 2 particles can only couple to the energy-momentum tensor is:

S. Weinberg, "Photons and Gravitons in S-Matrix Theory: Derivation of Charge Conservation and Equality of Gravitational and Inertial Mass," Phys. Rev. 135 (1964) B1049-B1056.

The argument applies only to couplings that result in Coulomb-like or Newton-like long-range forces, and Weinberg points out that under similar assumptions, there is nothing for massless particles of integer spin 3 or higher to couple to.

27. **Bernhard**
November 23, 2012

"I'm pretty sure most SUSY advocates would drop it like a hot potato if there is solid new physics to investigate that clearly doesn't fit at all in the SUSY framework."

And what sort of new physics that one can see at the LHC that could not possibly fit a SUSY framework?

28. **Bob**
November 23, 2012

Peter, as you claim, if this 3/2 argument is the weakest of all the arguments for susy, then that actually makes the sum total of the arguments for susy very very strong, i.e., if this is, as you claim, the "lower bound" on the quality of susy arguments, then that is quite impressive, since by definition, all other arguments must be even a lot stronger. If on the other hand this was the "upper bound" on the quality of susy arguments, then one might think the remaining arguments to be pretty weak.
In summary, I am quite surprised to see Peter to be so supportive of susy for a change.

29. **Peter Woit**
November 23, 2012

Bob,

I described the 3/2 hole as the weakest of various weak arguments. I suppose how one orders in strength a list of not very good arguments is to a large degree a matter of taste. One reason for putting this in the weaker category is that it says nothing about the energy scale of SUSY breaking (unlike the hierarchy, wimp miracle, coupling constant unification arguments, which require low scale SUSY breaking and supposedly explain something about the world we see).

Actually I think the 3/2 hole argument is just a variant of the standard argument "SUSY is a consistent extension of known symmetries and more symmetry is good and will be used by nature", but weaker (so there's a 3/2 hole? so what?).

30. **Marcel van Velzen**
November 25, 2012

Hello Peter,

As a field theory based on the Poincare-group (like the Standard Model) doesn't allow a well behaved spin 3/2 particle, it is actually the absence of a spin 3/2 particle in Nature that confirms that the fundamentals on which the Standard Model has been constructed are sound!

31. **Chris Austin**
November 25, 2012

Peter,

"unlike the hierarchy, wimp miracle, coupling constant unification arguments, which require low scale SUSY breaking"

Nima and Savas argued in hep-th/0405159, the original split supersymmetry article, that split susy is consistent with coupling constant unification, because the heavy squarks and sleptons come in SU(5) multiplets, and do not affect unification at one-loop order. The second Higgs doublet of the SSM is also heavy, and they claimed on page 7 that its absence might actually improve the unification prediction over the SSM when two-loop contributions are included.

32. **Peter Woit**
November 25, 2012

Chris,

Yes, but the point of split SUSY is that it keeps the gauginos at TeV-scale in order to preserve coupling constant unification, and I'd describe that as "low scale SUSY breaking". If you give up on low scale SUSY breaking completely (and do "supersplit supersymmetry", which is an April Fool's joke, not a theory…) you lose coupling constant unification. And, with split SUSY, you already lost the explanation of the supposed hierarchy problem.

33. **fuzzy**
November 28, 2012

Hi Peter
sorry to contradict you but you can have a nice SO(10) unification without any need of supersymmetry. And please consider that SO(10) helps to understand neutrinos masses, the only solid fact on "beyond the standard model physics"; on the contrary, SU(5) happilies marry with supersymmetry, but doesn't help with neutrino masses.

34. **Peter Woit**
November 28, 2012

fuzzy,

I didn't really intend to say anything one way or another about non-SUSY theories, the comment was about different SUSY variants. But, is there really still a viable SO(10) GUT that doesn't have problems with proton decay? My impression was that both SU(5) and SO(10) GUTs had such problems, worse in the non-susy case, but I haven't followed this recently.

35. **fuzzy**
November 29, 2012

hi peter
proton decay is not a problem is the only experimental test. i know we are theorists but should agree on the definition what "physics" is don't you think so? in my view,

the problem is that the efforts put in supersymmetry and strings stopped the progresses in gauge theories — the only successful thing we have — so i hope that sooner or later we will come back to discuss this. anyhow, putting aside these general issues, some work is still being done on so(10); curiously even the simplest theories are not fully analyzed see e.g., http://prd.aps.org/abstract/PRD/v85/i9/e095014

Scrutinizing the Cosmological Constant Problem

November 20, 2012
Categories: Uncategorized

Normally I do my best to ignore claims to have figured out the vacuum energy problem. There's an endless number of them, mostly looking pretty dubious, and the world is full of people much more expert on the subject than me, so it seems that my time would be better spent elsewhere. I did however just notice a new preprint making such claims, [Scrutinizing the Cosmological Constant Problem and a possible resolution], by Denis Bernard and André LeClair, and am curious about it. Unlike most of such things, the authors seem to know what they are talking about, and the whole thing looks not implausible to my non-expert eye. Can an expert tell me what is wrong with this (or, alternatively, tell me and my blog readers that it's a new good idea, or an old one that is not well known)?

Comments better be about the Bernard/LeClair paper and well-informed, or will be ruthlessly deleted.

Comments

1. **Chris Herzog**
   November 20, 2012

   Joe Polchinski has an interesting discussion of various theories of the cosmological constant at the beginning of his talk. In particular, page 4 seems relevant in the current context.

2. **Bee**
   November 21, 2012

   I haven't read this paper. I seem to recall however Claudio Dappiaggi making the point that if you do your qft in curved space carefully, the cc appears as a renormalization constant and there's really no reason for it to be at the Planck scale. Forgot details though. He's got several articles on the arxiv, but I don't know which one might be useful. This might give you a flavor.

3. **Sesh**
   November 21, 2012

   As I understand their argument (and I may be wrong) the vacuum energy is supposed to be proportional to k_c^2H(t)^2 at all times. If the momentum cutoff k_c is taken close to the Planck scale, this means that \Lambda is proportional to H(t)^2, so one obtains the correct order of magnitude relationship \Lambda \propto H_0^2 today.

   If that summary is correct then – without reference to the field theory arguments – one can rule it out empirically. As this paper points out in a footnote (it was probably well known before this), having \Lambda \propto H(t)^2 at all times amounts to a renormalization of M_P in the Friedmann equation; this is inconsistent with nucleosynthesis.

4. **theoreticalminimum**
   November 21, 2012

   Sesh,

   I think you're right about \rho_{vac} ~ H(t)^2 being assumed for all time t, although strictly speaking there's also a \ddot{a(t)}-dependence; at least, that's also how I understand it from equations (24) and (35). Looks like their proposition is ruled out then…

   NB: The footnote Sesh refers to is on page 3 of the preprint to which he provides a link.

5. **Alex**
   November 21, 2012

   I don't think there's really anything new in this paper. They claim to propose the "new" idea that Minkowski spacetime should be a stable solution to the semi-classical Einstein equations. In the discussion section:

   "…we proposed the principle
   that empty Minkowski space should be gravitationally stable in order to fix the zero
   point energy which is otherwise arbitrary."

   But this idea was proposed long ago by Robert Wald, it might even be older than that.
   See page 7 of
   http://www.springerlink.com/content/v8k661364hv52444/
   Its also in the lecture series he gave, "Quantum FIeld Theory in Curved Spacetime and Black Hole Thermodyanmics"

6. **Nameless**
   November 21, 2012

   The "cosmological constant problem" is really not one but two problems. The first one is, "why is the cosmological constant 120 orders of magnitude smaller than we expect from QM?" The second is, "since the cosmological constant is constant and density of the universe varies by the factor of 10^120 over the course of history, why is the cosmological constant the same order of magnitude as density of matter today, what's so special about today"? (The second question is also known as "the coincidence problem".)

   It is tempting (especially for particle theorists) to propose, like here, that the constant is not a constant but a variable that scales as some function of time (or H(t), a(t), etc.) A quick arxiv search will yield lots of hits. A choice of Lambda ~ H(t)^2 is particularly convenient because then Lambda is, in essence, defined to be forever proportional to the density of matter, simultaneously solving questions 1 and 2. Numerical coincidences in the article (e.g. (12)) are the manifestations of this definition.

   Unfortunately, as Sesh points out, these solutions are constrained by Big Bang Nucleosynthesis as well as by observations of supernovae and CMB. While some degree of variation is permitted, the naive "no-coincidence-problem" H(t)^2 has been ruled out observationally. (E.g. http://arxiv.org/pdf/astro-ph/0702015v3.pdf) Among other reasons, the fraction of dark energy, currently ~0.7, is constrained by BBN to be <0.05 at T~1 MeV.

   Their article says that H(t)^2 is only the rate of change in "later" times, but, as far as I understand, their idea of later times go almost all the way to Planck time.

My personal opinion is that, in order to resolve the coincidence problem, we have no choice but to resort to anthropic principle. According to an old argument by Weinberg, there simply wouldn't be any observers in the universe if the cosmological constant were even three orders of magnitude larger than it is now.

7. **Nameless**
November 21, 2012

P.S. I don't find the statement in the footnote of Sarkar's article convincing. The reference cited states that BBN constrains Newton's constant to within 10% of its current value at the time of BBN (~100 s). If Lambda~H^2, that puts Lambda at ~ 1e-16 GeV^4 at that time. I don't see it how this value would result in a greater than 10% shift in G_N or M_P at the MeV scale, and it is not explained in the article.

8. **bob**
November 21, 2012

On first impression, this paper does not seem very original, it is observationally ruled out by assuming a time dependent vacuum energy, and appears to miss all the important problems of the cosmological constant problem; for instance, the higgs potential adds a ~(100 GeV)^4 contribution, which would have existed in the early universe; why should that just disappear? I don't think they address any of the real issues, and seem to sweep all insights from effective field theory aside, which is to ignore most of the relevant discussion.

9. **Kurt**
November 22, 2012

A cosmological constant that decreases with time, or increases with the Hubble constant, is NOT ruled out observationally. I have discussed this issue with all astrophysicists I could get hold of, and they all agreed on the point. Observationally, nothing is known about the time-dependence. (Are there any new developments? Can anybody comment?)

There might be theoretical arguments from nucleosynthesis that suggest problems, but these arguments are NOT observational; they just show that one idea contradicts another. Now, we know that QFT is wrong at high energy, because QFT predicts a huge vacuum energy, whereas measurements show that it is small. Thus QFT is wrong at high energy; and it makes no sense to use a theory that makes wrong statements as an argument against solid observational data – even at the time of nucleosynthesis.

10. **Thomas**
November 22, 2012

I agree with their statement in the abstract "In classical and quantum mechanics without gravity, there is no definition of the zero point of energy." However, I think one should take this more seriously.

First one has to admit that the usual argument that "QFT predicts a large cosmological constant" is probably not very convincing as we can only really motivate a bold Planck scale cutoff from effective field theory ideas after we understood well what is going on "beyond the Planck scale". From a different perspective one could just say that the fact that the cosmological constant is computed to be ridiculously large in QFT does not imply that QFT is wrong but that the computation does not make sense / is based on false assumptions.

Instead it seems convincing to take the above mentioned point of view that there is no way to set a zero point on the energy scale if one considers the coupling of matter to gravity. In the lab, we compare the energy in different states, e.g. in a two particle state and the vacuum, thus we measure energy differences. The Einstein equation though measure the absolute energy (density) of whatever fills the universe. Within QFT we can not define the corresponding zero point unambiguously, thus we have to admit that any definition / computation is correct up to that ambiguity, which can be argued to parametrised by four parameters (c.f. Robert Wald's paper mentioned by Alex). One of those is the Newton constant, and another the cosmological constant.

Thus for me the most convincing attitude towards the cosmological constant is that it is to the best of our knowledge just a constant of nature we have to measure, much like the Newton's constant. Of course we might hope to predict it within a quantum gravity theory at some point, but not within QFT; just as QFT does not predict the Newton's constant either.

11. **Sesh**
November 22, 2012

To the second Nameless: the first Nameless has answered your question already. (Unless you are both the same Nameless, in which case the source of confusion is unclear to me.)

Put simply, if \Lambda~H^2 at all times, then dark energy constitutes a large and constant fraction (~0.7) of the energy budget at all times, including at nucleosynthesis, thus drastically changing the expansion history. Since \Lambda~H^2 two terms in the Friedmann equation can be rearranged to give something that is essentially the same as the Friedmann equation with no cosmological constant term but with a rescaled Newton's constant or M_P. So the observational constraint on the allowed shift in Newton's constant at the time of BBN is directly translatable into a constraint on the fraction of the energy density that was in dark energy at that time.

12. **Nameless**
November 22, 2012

@ Sesh: we're all the same Nameless (not to be confused with Anonymous). Your argument is valid, the source of confusion is the term "renormalization", which normally means something entirely different.

13. **Bob**
November 22, 2012

@kurt, you are simply wrong. While of course the dark energy can be an extremely slowly varying function of time, i.e., there are always error bars, the error bars are not big enough to allow Lambda~H(t)^2. This would ruin, amonst other things, large scale structure formation, it would mess up the CMB data, and so on.

14. **Nameless**
November 22, 2012

@Bob/Kurt, if I'm not mistaken, from the early-universe perspective, Lambda~H^2 is just outside the error bars. Constant Lambda of 10^-47 GeV^4 is effectively zero in the early universe (it is vanishingly small compared to matter and energy density), and even a time-dependent Lambda that falls off slightly slower than H^2 would be so much smaller than matter density at the time of CMB decoupling as to be unobservable.

Astronomical observations are a different and complicated story. There are numerous different models (e.g. is the cosmological constant simply a number that changes with time? or is it a new energy field? If so, what is its equation of state, and does it interact with, or decay into, dark matter?) And observational points are fairly noisy. I've seen claims that most of the parameter space is excluded, and I've seen claims that a model where Lambda~H and the cosmological constant field decays into dark matter is supposed to fit observational data slightly better than the standard Lambda-CDM model.

15. **vmarko**
November 22, 2012

Nameless:
The "cosmological constant problem" is really not one but two problems. The first one is, "why is the cosmological constant 120 orders of magnitude smaller than we expect from QM?" The second is, "since the cosmological constant is constant and density of the universe varies by the factor of 10^120 over the course of history, why is the cosmological constant the same order of magnitude as density of matter today, what's so special about today"? (The second question is also known as "the coincidence problem".)

There is a critical review on both of these problems given in 1002.3966. In short, the authors argue that the coincidence problem is not actually a problem, because the "today" covers a period of ten billion years (or so), so there is no unnatural coincidence happening at all. As for the QFT prediction for Lambda which is off by 120 orders of magnitude, they argue that there is no mystery in that either, since we do not know how to perform renormalization in curved-space QFT. So the 120 orders of magnitude is a problem of applying flat-space QFT in a regime where it is obviously not applicable, and then wondering why the result is wrong.

I think it is an interesting paper to read.

16. **Anonyrat**
November 22, 2012

Rovelli and Bianchi seem to be arguing that since the physical effect of the cosmological constant cannot be observed at small scales on which space time is approximately flat, and thus QFT calculations of the c.c. (dominated by much shorter length scales) should viewed with extreme skepticism.

17. **Kurt**
November 23, 2012

@Bob, your answer mixes two things. It might be that experimental measurements do not allow a slowly varying Lambda. But structure formation of galaxies is NOT a experimental measurement, but a theoretical model.

My point is: is a slowly varying Lambda against measurements? You might be right that it contradicts CMB data, though I know people claiming that it doesn't (at least for Lambda proportional to H).

Nobody cares whether a non-constant Lambda contradicts some model – that model might be wrong. The issue is whether it contradicts some actual measurements of Lambda at different distances/times. I think this is the main issue to be settled. Writing "and so on" is not an argument for the constancy of Lambda.

18. **Shantanu**
November 23, 2012

Hi guys,
you can see a nice discussion of Rovelli/Bianchi paper on cosmocoffee.
http://cosmocoffee.info/viewtopic.php?t=1531

19. **Bob**
November 23, 2012

@kurt, your response is very strange when you say "nobody cares whether some non-constant Lambda contradicts some model", when that is the whole point of this blog entry! LeClair and bernard laid out a MODEL, namely a universe with Lambda~H(t)^2, plus ordinary GR, ordinary matter and radiation. So what does their model predict? Well it dramatically changes CMB data, large scale structure, weak lensing, lyman alpha forrest, etc. It is very strange that you would keep defending their claim despite all the evidence pointing against it (and this is setting aside all the theoretical problems of Lambda~H(t)^2).

20. **bob**
November 23, 2012

@Nameless, not sure what the reason for your comment was — you say that if dark energy is less than H(t)^2 then it is observationally allowed by being orders of magnitude smaller than the rest of the energy of the universe at time of CMB. ok…but that is NOT the LeClair/Bernard model that is the topic of the blog entry. They claim, as Sesh quite rightly points out, that dark energy is ~ H(t)^2 and so remaining 0.7 the energy budget of the universe at all times. This is not just outside the error bars as you strongly claim, but is clearly ruled out by a range of observations.

21. **Onlooker**
November 23, 2012

Sesh:

On p.9 they make this statement: "We also argue that when H=a(dot)/a is large, the first Friedman equation sets the scale H~k_c, which is the right order of magnitude if k_c is the Planck scale."

I'm over my pay-scale here, but I'm wondering if that solves the dilemma you point out above.

22. **Igor Khavkine**
November 24, 2012

As Bee and others have already pointed out, any attempt to solve the cosmological constant problem beggs the question "Is there an actual problem to be solved?". I think that failure to answer the latter question positively dooms any remaining arguments to inconsequence. To the point, the "size" of the cosmological constant can only be sensibly assessed if one had at least two physically meaningful quantities to compare. However, the comparison is usually only made between the measured value (physically meaningful) and a bare, cutoff-dependent, unobservable parameter (not physically meaningful). In short, I think the whole enterprise fails due to the inability to meaningfully state what the "problem" is. This is discussed in more detail in the previously linked CosmoCoffee thread.

Also, the idea of fixing the cosmological constant (or any other renormalized parameter) in a way coherent across different spacetime backgrounds is definitely not new and is used in an essential way in the modern understanding of renormalization in curved spacetimes. In particular, the application of this idea to the cosmological constant was discussed in this Gravity Research Foundation essay by two major contributors to that field:

Quantum Field Theory Is Not Merely Quantum Mechanics Applied to Low Energy Effective Degrees of Freedom
Stefan Hollands, Robert M. Wald
http://arxiv.org/abs/gr-qc/0405082

The upshot is that, according to the authors, the result (which does not depend on any cutoffs) is actually much much smaller than the observed value. In other words, even in that setting, the cosmological constant needs to be introduced as an external parameter, with a value that could not have been deduced other than from experimental input.

23. **Peter Woit**
November 24, 2012

Thanks to all for the interesting comments about the paper. Please, no more comments that aren't explicitly about that paper.

24. **LeClair**
November 25, 2012

These comments have been informative, thank you. But I feel I should clarify at least one point. The conversation got side tracked by the idea that we proposed the vacuum energy was proportional to H^2 and there is a problem with this varying in time. Perhaps this was gleaned from the abstract, but it is not what we proposed at all, and it is nowhere written in the paper. Rather the result we do propose is that the vacuum energy is proportional to this quantity "A", which is a linear combination of H^2 and \ddot{a}/a. A major point of our paper is that consistency requires that this is indeed independent of time, which turns out to be the case in a matter dominated era. We are not at all well informed on BBN etc, but the remarks above about the vacuum energy being proportional to H^2 and that being ruled out are not very relevant to what is in our paper. We certainly cannot claim a solution to the CCP, there could very well be some problems with our proposal that are not obvious now, at least to us, but I don't think this is one of them. Thank you for the interest in any case.

25. **Bob**
November 26, 2012

LeClair, the reason people are not persuaded by these arguments is 2-fold
1). In your paper you have a contribution to the vacuum energy which goes as kc^4, with kc~Mp, which you simply dispense with. This really sweeps the whole fine tuning problem under the rug, because the CCP is the question of why that term cancels.
2). The next term you focus on is kc^2 R. And you claim that this gives the right order of magnitude for R~H^2, with H evaluated today. This really sweeps the whole coincidence problem under the rug, because this just assumes a coincidence between the dark energy and the matter density today.
The reason everyone was thinking Lambda~H(t)^2 is because it seems you can play the same game at ANY moment in time. Why is today special? Why not redo your arguments at the time of CMB? Then you will get the wrong answer because H changes in time.

26. **Nameless**
November 26, 2012

OK, then it looks like everyone, including myself, totally misunderstood the article. (The statement that Lambda~H^2 is a logical, but, apparently, wrong interpretation of the text on page 8.)

According to your definition and the Friedmann equations, A = 8 pi G (Lambda – rho_radiation). For any function of A that vanishes in Minkowski spacetime, as long as there's no radiation and you discard higher-order terms in A in the Taylor expansion of that function, its value will be constant in time. It would be more interesting if one could calculate rho_vac ~ A exactly, with vanishing higher-order terms.

Assorted Links

November 23, 2012
Categories: Uncategorized

- Paul Frampton has been found guilty on drug charges in Buenos Aires, looks like he will be able to serve out his sentence under house arrest, get released sometime in 2014. It's unclear at this point how the University of North Carolina will handle this. For details, there's Physics World, the Daily Mail, the Winston-Salem Journal, and Clarin.
- Erik Verlinde over the past couple years has gotten 6.5 million euros in prizes and grants to fund his work on entropic gravity (see here). Now, he'll head up a new institution, the Delta Institute for Theoretical Physics, funded with 18.3 million euros from the Dutch scientific funding agency NWO as part of its Gravitation Programme.
  There's an interview with Verlinde here. He says he's working on explaining dark matter with his entropic gravity ideas. There's no paper about this, I guess because:

  > There are some small gaps in my reasoning and things that I still do based on intuition. I'm trying to fill in those gaps.

  but he thinks these ideas about dark matter will be tested in "no more than 10 or 15 years".
- Another new theoretical physics institution is The Higgs Centre for Theoretical Physics in Edinburgh, where they're hosting an inaugural symposium in January.
- The latest TEDYouth is online, see here. It seems that they may think that Youth these days is pretty ADD, since they have all presentations limited to six minutes. In one of them (around 3:50) they've got Clifford Johnson explaining the exciting new idea of replacing particles by strings moving in extra dimensions.
- Mochizuki's claimed proof of the ABC Conjecture still seems to be resisting the attempts of experts to understand and evaluate it. He has put on his web-site some slides for a talk next month, and promises a survey article next March.
- Easier to follow in principle, but at 367 pages still pretty daunting, is a new paper from Laurent Lafforgue, chronicling his attempt to develop "non-linear" versions of the Poisson formula that would imply Langlands functoriality.
- If you were wondering about that tatoo in Edward Frenkel's film, see Mathematics, Love and Tattoos for an explanation.
- In other film news, there's the Colliding Particles project for a film about the Higgs. They've got a new segment up, several more coming soon at one/week.
- The debate in the HEP community about the death of SUSY goes on, and will undoubtedly continue along the same lines for quite a while. Latest is from New Scientist, which has Steven Weinberg describing the situation in a way that that can't be argued with:

  > SUSY's plausibility is reduced, but not to zero.

  For the argument over whether SUSY was and is an overhyped, implausible idea, on one side there's Michael Peskin, with:

  > I think that the serious effort given to SUSY is appropriate…"

  and on the other, taking the *Not Even Wrong* side of this argument, there's Matt Strassler with:

  > The theory, specifically as something we would observe at the LHC, was wildly over-promoted.

Update: One more. A Science magazine article about the problem with "Open Access" journals discussed here. According to Science:

> Meanwhile, the OA industry is becoming increasingly diverse; it includes traditional powerhouses, such as Germany's Springer, which now publishes about 300 OA titles, as well as a vast array of newcomers. OA publishers have a built-in incentive to lower the bar, Dupuis says, because in contrast to subscription journals, an OA title earns more revenue with every paper its editors accept.
>
> Moreover, many so-called predatory publishers—which often eschew peer review, use fake editors, or contain plagiarized papers—have flooded the market, says Jeffrey Beall, a librarian at the University of Colorado, Denver, who keeps an online list of these dodgy outfits.

Comments

1. **Bob**
   November 23, 2012

   Wow, I didn't know that about Verlinde receiving many millions for his ideas about explaining dark matter with entropic gravity. I thought his ideas about gravity being entropic had been debunked a while ago. But it is particularly ridiculous to richly finance his claim that dark matter can be replaced by modified gravity.

   Verlinde has given talks on this, and it appears to be just the old Milgrom idea of declaring Newton/Einstein gravity wrong on galactic scales, and replacing F=ma, by some other law, when 'a' drops below a critical value. This forces individual galaxies to get roughly the right rotation curve. The new twist on Milgrom's idea is that Verlinde attaches this to entropy in some weird way.

   While this was an interesting idea by Milgrom back in 1981, I don't think it is interesting anymore; the jury is in: such a model *does not work*. It is incompatible with a host of observations, including large scale structure formation, CMB data, weak lensing, lyman alpha forrest, bullet cluster, etc. I don't think there are many decent cosmologists who take the modified gravity models seriously anymore. Instead, dark matter is unavoidable and explains many of these different observations beautifully. So it is shocking that the Dutch are unaware of this and are just pumping millions into this 30 year old dead idea.

2. **jg**
   November 23, 2012

   Yes Bob, but the fact that the LHC has failed to show even a tiny possibility of a dark matter Lightest Supersymmetric Particle kinda helps promote alternative explanations of dark matter.

   I hope you aren't arguing that Verlinde's ideas must not be pursued simply because experimental evidence is against it – I mean, imagine what that would that do for SUSY and String Theory research funding

3. **Bob**
   November 23, 2012

   @jg, your comment was one of the more bizarre I have seen. 2 comments:
   1) I didn't mention anything to do with SUSY whatsoever. Unlike you, I am not advocating SUSY.
   2) Absence of evidence is not evidence of absence — the dark matter particle can just be very weakly interacting and this is compatible with all the data. But my main

point was that for Milgrom models of modified gravity, the jury is in; it is ruled out by the data.

4. **jg**
November 23, 2012

eh,

I was just responding to you very forceful arguments that Verlinde's ideas are dead and the possibility that anyone might fund his research is irresponsible.

Anyway, a few other comments on threads mentioned:

ABC Conjecture: give it up already, either prove it elegantly or go do your obscure maths ideas in another area.

Edinburgh: You could have done this a few years ago really.

5. **Michael Tyson-Grothendieck**
November 23, 2012

jg: "ABC Conjecture: give it up already, either prove it elegantly or go do your obscure maths ideas in another area."

Come here and tell me that… You can't last two minutes in my world.

6. **jg**
November 23, 2012

Michael

I lived through Wiles proof of FLT, really, I was at the universities involved over the Summer and Autumn (and subseqent Spring), I was as excited and thrilled as anyone could be, and lived through the doubts and final vindication.

This ABC "proof" doesn't taste anything like it, Wiles' proof was backed up by a huge amount of accepted theory, and was in fact (eventually) a fantastic validation of much of 20th century PURE mathematics.

7. **nbutsomebody**
November 23, 2012

Verlinde got a video out in bigthink:
http://www.youtube.com/watch?v=hByJBdQXjXU&feature=relmfu

The video has 200K+ views. It is sad to see how speculative nonsense is presented to common mass as a legitamate science and that too by prominent scientists.

8. **jon**
November 24, 2012

Regarding Langlands and L.Lafforgue, there's also been a recent preprint by V.Lafforgue, a younger brother of the former, which apparently proves a generalisation of his big brother's famous 2002 results with new different ideas, see http://arxiv.org/abs/1209.5352 (all this is way over my head, just noticing).

9. **Teleos**
November 24, 2012

Bob, your arguments are simply ridiculous. Nobody working on MOND these days claims that this formula explains everything. What it does not explain, it does not explain. And it is *not* a theory at this stage. But the formula, simple as it is, explains a *lot* of things and does this in an impressive manner, which plain simple collisonless CDM cannot do at the moment, and will probably never do. It is an incredibly weak argument to pretend that "MOND just makes rotation curves flat", "was designed to fit galaxies" or stupidities of that kind… These arguments only reveal a very embarassing lack of reflection of its authors on the topic, and a refusal to read or try to understand all the serious papers referring to it. For instance, there has been a recent review in LRR showing what MOND does, and why it is still taken seriously (yes, seriously enough to have an invited LRR review about it): http://relativity.livingreviews.org/Articles/lrr-2012-10/ This article is not putting the problems of MOND under the carpet. To make a long story short, MOND proponents fully agree that some sort of dark matter does exist, but what they think is that it is *not* made of CDM particles. The reason for this is that MOND is right *as an empirical scaling relation* in galaxies, whatever dark matter's deep nature is. Even the most fanatic (but educated) CDM afficionados cannot disagree with this. But cosmologists seem to deliberately ignore this fact, because, to quote them, "they do not care about the details of messy astronomical systems such as galaxies"… And this might be a big mistake, because what the success of MOND might tell us about the nature of dark matter might really be fundamental… And how much you think the latter sentence is true depends on how far away from the galactic scale your main research interests lie. What strikes me is that when people say "MOND is wrong" or "MOND is right", they dont necessarily mean the same thing, because some think it is a synonym for TeVeS, and those people are mostly *NOT* working on galaxies (and obviously they are mostly finding problems with the whole modified gravity approach), while some others (mostly working on galaxies) just think it is the most awesome scaling relation to date, summarizing almost all scaling relations of both spiral and elliptical galaxies, let alone dwarf spheroidals, but including tidal dwarf galaxies that are much less well understood in the CDM context. While they all agree on the problems to explain the CMB and clusters of galaxies, they simultaneously think "MOND works too well in galaxies to be meaningless". Again, this absolutely doesnt mean "dark matter doesnt exist". There is a big difference between believing in dark matter, and believing in the current LambdaCDM model. As a conclusion, the success of MOND as an empirical relation in galaxies is just a huge fine-tuning problem that we need to understand, as there are many in physics. The problem right now is a majority of astronomers and cosmologists *completely* (and often deliberately) ignoring this fine-tuning problem. In this sense, Verlinde is a light in the dark!

10. **Bob**
November 24, 2012

Teleos, your comments are irrelevant. You say "MOND proponents fully agree that some sort of dark matter does exist". Fine. But Verlinde does not. Verlinde believes in the old Milgrom story of no dark matter at all. And, as I pointed out, this contradicts CMB data, large scale structure formation, weak lensing, bullet cluster, lyman alpha forrest, etc. So that is definitely a dead idea. Please try to read and comprehend before responding in the future.

11. **Teleos**
November 24, 2012

Bob, Verlinde is *not* saying this, and MOND proponents do not either. MOND *is* a success as an empirical formula in galaxies and one has to understand this, one way or another, fullstop. Please read the relevant serious litterature if you wish to debate further.

It might well be that we will understand why galaxies behave in this way within the CDM context, but one still has to demontrate how, and pretending that we right now do understand this would be of the utmost intellectual dishonesty. An interesting review, very optimistic for CDM but also quite honest about the current lack of understanding of large number of phenomena on galaxy scales is for instance given in: http://arxiv.org/abs/1207.3080 Knowing that Silk and Mamon have long been defending CDM, the simple fact of saying "Whether appeal to alternative gravity is justified by inadequate baryonic physics is a question of judgement at this point" reveals that they are not as sure of themlselves as uneducated people like Bob.

12. **Bob**
November 24, 2012

Teleos, I attended a talk where Verlinde said that in his model there is no dark matter.

13. **Peter Woit**
November 24, 2012

Please, enough about MOND. If someone has a well-informed comment about Verlinde's ideas that's great, but this kind of argument over MOND is both off-topic and unenlightening.

14. **Anonyrat**
November 24, 2012

Useful: discussing gravity with Verlinde
http://www.scilogs.eu/en/blog/the-dark-matter-crisis/2012-06-28/discussing-gravity-with-eric-verlinde

15. **Teleos**
November 24, 2012

To end-up this discussion, I hope it is not too much off-topic to say that Verlinde's approach to the DM problem should very likely be in the spirit of http://arxiv.org/abs/1005.3537

16. **Samuel Prime**
November 24, 2012

Concerning SUSY, I've read in the book by Cottingham and Greenwood ("An Introduction to the Standard Model of Particle Physics") that SUSY requires energies near 14 TeV to be observable (page 18). But so far the LHC has reached 7 to 8 TeV. So why the discouragement? Is it that some of the superpartner particles were expected to show up at current LHC energies? (I guess this might depend on which version of SUSY one is talking about.)

17. **Peter Woit**
November 24, 2012

Samuel Prime,

You're misreading the book you quote, which says

"It is also widely believed that the physics of Supersymmetry, which perhaps underlies the Standard Model, will become apparent at the energies, up to 14 TeV, which will be available at the LHC"

Note the "up to". The arguments for LHC-scale SUSY (either things like the MasterCode fits, or the idea that SUSY would solve the hierarchy problem and avoid fine-tuning), all predicted that seeing SUSY would not require the full 14 TeV, but be visible at 7-8 TeV (a factor of 3.5-4 above the Tevatron energy). Whatever they're saying now, I don't think one can find any SUSY enthusiast who a couple years ago, just before the 7 TeV turn-on was saying "too bad, that's not enough, it's not until 14 TeV that it will show up".

18. **Samuel Prime**
November 24, 2012

Yes, when they say that SUSY will become apparent at energies "up to 14 TeV," I'd hardly think that they're saying they would be expected to appear at 7 or 8 (or lower). It means one should allow up to 14 TeV energies before can exclude the idea. It is true, though, that it's not looking too good (but at one point the discovery of the "Higgs boson" didn't look good either). One thing that does seem more clear is that some models that require some masses for superparticles have been excluded by the LHC.

19. **Peter Woit**
November 24, 2012

Samuel Prime,

The only reason the 14 TeV number is in that sentence is because it is the design energy of the LHC, not because it has anything to do with SUSY. You're trying to put words in the mouths of those authors which they did not intend. When they were writing, no one knew about the magnet interconnect problems and everyone assumed the LHC would quickly run at or near design energy.

Yes, as a matter of logic, until one runs the LHC at design energy one won't be able to exclude the possibility of the LHC discovering SUSY. This doesn't change the fact that pre-LHC the predictions for SUSY were for it to be visible at 8 TeV, not for it to require 14 TeV.

20. **chorasimilarity**
November 25, 2012

The Science magazine article reminded me about this joke

Gold open access: enter the mouth

21. **MathPhys**
November 25, 2012

Maybe someone can clarify the following to me. I heard a talk by E Verlinde in which he explained DM using the off-diagonal components of the matrices in matrix models of strings. There was no mention of entropic gravity, or gravity as an emergent phenomenon in any form. He seemed to be using those degrees of freedom in matrix models that are usually discarded. But since these are models of strings, the graviton is non-emergent.

Am I right? or do I miss the point?

22. **chris**
November 26, 2012

There is nothing ridiculous in what Verlinde is trying to do – in fact, if you look at the "entropic" derivation of the Einstein equations, it is the most natural thing.

The only strange aspect is that this is not Verlindes idea. The idea is from Ted Jacobson and goes back to 1995.

23. **Proudmemberofthecult**
November 26, 2012

Please note that the 18 million euro's is not quite 'Verlinde's' money: it's a 10 year grant to cover quite a big field in a collaboration which involves 6 profs – one of whom is 't Hooft. Yes he's involved. No, it's not his personal pot of gold.

24. **MathPhys**
November 26, 2012

@chris I may be wrong, but I think that T Padmanabhan's work on the subject is earlier than T Jacobson's. I also find his papers clear and well thought-through.

25. **Yatima**
November 27, 2012

Aha … http://en.wikipedia.org/wiki/Thanu_Padmanabhan

Well, I certainly hope for the success of this program. And if not, I'm sure there will be things learned.

26. **Yatima**
November 27, 2012

In the Scientific American article linked from the Wikipedia entry, Padmanabhan has this to say about the history of the "gravity as an emergent phenomenon" idea:

"This idea that gravity is an emergent phenomenon has a long history. It was first introduced by the Russian scientist, Sakharov, in 1968. Another intriguing connection, suggesting similarity between the surface properties of black holes and fluid mechanics was investigated by T. Damour, Kip Thorne, W. Price and their collaborators in the eighties. An attempt to obtain Einstein's equations from a thermodynamic perspective was made by T. Jacobson in 1995. Other approaches, very similar in spirit, were developed by G. Volovik and Bei-Lok Hu. My collaborators and I [Thanu Padmanabhan] started on a concrete programme to explore this idea in 2002. More recently, E. Verlinde has tried to reassemble many of these ideas from the perspective of string theory."

27. **Anonyrat**
November 27, 2012

New form of matter at LHC?
http://idealab.talkingpointsmemo.com/2012/11/large-hadron-collider-may-have-produced-new-matter.php?ref=fpb

28. **GB**
November 28, 2012

According to the report linked above, the new form of matter from LHC was submitted to "Physical Review B". This is interesting ! But when I clicked the arXiv link, the paper was sent to Physics Letters B. In fact, I think PRB might actually be a suitable place for this work. Maybe at this level, there is already some new emergent physics which is independent of the underlying constituent particles. And it would be cool that LHC publishes something on PRB.

29. **lun**
November 28, 2012

Since you mention big private grants for theorists, you might want to also mention that
whole accellerators are
facing the axe due to "austerity".
Both RHIC and B-physics were in the past thought to be likely places where string theory/SUSY were to "confront experiment", but it now looks like these experiments wont even take place.

30. **Peter Woit**
November 28, 2012

lun,

Thanks. I've been having trouble following the US budgetary issues, since it seems unclear what the budget situation will be in a month or so, much less in the longer term.

About the Italian decision canceling SuperB, besides the Physics World article, there's also some discussion of this at Tommaso Dorigo's blog, see

http://www.science20.com/quantum_diaries_survivor/superb_factory_killed-97250

31. **Bob**
November 28, 2012

Chris, the most natural thing is that dark matter is a boring old stable neutral particle. The least natural thing is that it is an artifact of entropy and string theory as Verlinde claims it is.

32. **Stefan**
December 3, 2012

I was wondering why Verlinde's work creates these more emotional responses from the physics community. Would it be possible to point out a good paper with a line of argument why his hypothesis is now referred tot as "dead" or "debunked"? If it is proven wrong, are there still lessens learned? I am just interested in the idea and wonder how much effort I should spend following Verlinde's thesis. There seem to be a mismatch between the excitement in public media and the more critical statements from the scientific community.

33. **JR**
December 3, 2012

Stefan, Please see these links for interesting reactions:

http://physics.stackexchange.com/questions/4289/is-gravity-an-entropic-force-after-all

http://arxiv.org/abs/1108.4161

The neutron argument seems to go right to the heart of whether or not we really understand any entropic force: i.e. can they be reversible, do they have to de-cohere a wave function?

Cheers
JR

34. **Stefan**
December 4, 2012

JR, thank you so much! This helps me a lot! thanks 🙂

News from CERN

December 5, 2012
Categories: Experimental HEP News

Very light posting recently, partly due to being busy keeping up with my class, but more due to just not noticing anything particularly newsworthy. Matt and Lubos have quite a lot to say about Time magazine's not describing the Higgs mechanism accurately, but I find it hard to get too excited about that, with my sympathies lying with any poor journalist given the impossible task of explaining this in a few words to the public.

Today at CERN there's an LHCC meeting, with status reports on the machine and the experiments available here. The 8 TeV proton-proton physics run has just about ended, with the next week or so to be devoted not to luminosity production, but to machine studies. The integrated luminosity for the run will be about 23 inverse femtobarns, significantly above the original plan for the year. A heavy-ion run will end in February, after which the machine will be shut down for a long period in order to fix the magnet interconnects and other problems, to allow running at or close to the design energy of 7 TeV/beam. The current plan has proton-proton physics at 6.5 TeV/beam starting again about April 2015.

It seems likely that there will be no new results about the Higgs until the Moriond conference in March. CMS and ATLAS will then have quite a while to work on doing the best possible analysis of their 7 and 8 TeV data for information about the Higgs. From now on, attention will focus on what CMS and ATLAS have to say about the signal sizes in the various channels where the Higgs is supposed to show up, as well as theoretical studies of how possible next generation accelerators would perform in terms of doing better at these measurements than the LHC. The LHC Higgs Cross Section working group is meeting today and tomorrow on this topic, talks are available here. Next week the KITP will host a similar workshop.

The continuing big story from the LHC is that of no SUSY or other BSM physics showing up. The LHCC ATLAS slides have

> Physics beyond the SM did not show up yet. There is no need for preliminary conclusions. Let's continue our work and look were we haven't looked so far.

but theorists are definitely starting to draw preliminary conclusions, needed or not. At Scientific American, Glenn Starkman has a piece entitled At CERN: Down in the Mouth in Paradise which paints a sorry picture of the situation caused by SUSY not showing herself:

> The Standard Model is absurdly fine-tuned, we were told – balanced on a knife-edge off which it has no right not to tumble. It has an un-natural hierarchy of scales. It has too many free parameters, and some of them are very, very small. Why, the electron mass is less than 0.00001 times the weak scale (the energy scale governing weak interactions such as the W and Z boson masses), which is itself 10^{-17} (that 0.00000000000000001) times the Planck scale (the energy scale governing gravity)! And speaking of gravity, the Standard Model can't accommodate quantum gravity. We need Low-Energy Supersymmetry, or Technicolor, or Large Extra Dimensions, or … One of these MUST be found at the LHC!
>
> Forty years of theoretical work has been based on these expectations. Papers with thousands of citations have been written. Courses taught. Textbooks published.
>
> Prizes awarded! Illustrious careers navigated! And yet despite all this build up of theoretical expectations, there is no experimental hint of anything outside the Standard Model at the LHC. Hence the long faces and worried words wherever theorists gather to drink coffee. Hence the disappointment in the eyes of the young experimentalists looking forward to the next accelerator, the next frontier where their mark will be made…
>
> Walk the halls, go to theory seminars, have lunch with a theorist, or an ambitious young experimentalist. Look for the classic symptoms of grief.
>
> Denial. Vigorous debates about whether the fact that the dog did not bark in the night suggests that it is a Chihuahua or a Rottweiler. My friends – at some point if there is no barking, we must conclude there is no dog.
>
> Anger. At those of us "misguided" enough to doubt the imminence or even the necessity of Beyond the Standard Model physics.
>
> Bargaining. Perhaps BSM physics has not been discovered because we've been demanding too much explanatory power from science. If we just relax our expectations for the predictivity of science, and introduce a multitude of universes in which we occupy a particular one best suited to our existence, then we can let our extensions to the Standard Model be un-natural, many of their properties unpredictable, and explain why they haven't been discovered yet!
>
> Depression…
>
> We're not ready for Acceptance! At least, sitting here listening to the LHC hum, I can still hope.

Comments

1. **Nick M.**
   December 5, 2012

   And so the "nightmare" begins!

2. **cormac**
   December 5, 2012

   …'At some point we must conclude there is no dog?'
   Oh please. At what point exactly?
   Ruling things out is a very long and laborious process, not helped by commentators anxious to jump the gun. The history of particle physics is littered with examples of phenomena that took many many years to show up, many of which are now taken for granted.
   Glenn Starkmann needs to remember that it took decades to find the atom, decades to find the neutrino and decades to find the Higgs….what exactly were you expecting?

3. **Shantanu**
   December 6, 2012

   cormac, by this logic we should continue to design experiments designed to look
   for magnetic monopoles (and so on).
   shantanu

4. **Thomas Larsson**
   December 6, 2012

It has taken decades to not find susy.

5. **Jose Ignacio**
   December 6, 2012

   There is a great experimental evidence that there is physics beyond the standard model: dark matter. The standard model is unable to explain the 80% of the matter of the universe.

6. **Simon**
   December 6, 2012

   Shantanu
   There are very good reasons to look for magnetic monopoles.

7. **srp**
   December 8, 2012

   From the SciAm post:

   "Fifty-one years after its basis was explored by Sheldon Glashow, forty-five years after the full theory was proposed by Steven Weinberg (and independently the next year by Abdus Salam), the Standard Model has successfully accounted for or predicted all calculationally tractable experimental results in particle physics. "

   Note the loophole there.

8. **emile**
   December 8, 2012

   Let's wait just a few more years before getting too depressed. Once the LHC has run for a few years at full energy, and that the Higgs couplings are still consistent with the SM, that no signs of BSM physics has appeared, we'll know we are living with the nightmare scenario. In the meantime, other experiments could turn up surprises.

9. **Shantanu**
   December 8, 2012

   Jose,
   at the risk of repeating what's been said many times, there is absolutely NO evidence
   from astrophysical data alone, that dark matter has anything to do with SUSY or weak interactions or physics beyond standard model.
   It could very well be an axion or primordial black hole or a massive graviton.
   The WIMP miracle argument has been oversold and in its simplest version is already ruled.

Forty Years of String Theory

December 5, 2012
Categories: Uncategorized

The journal *Foundations of Physics* has been promising a special issue on "Forty Years of String Theory: Reflecting on the Foundations" for quite a while now, with a contribution first appearing back when it really was 40 years since the beginnings (more like 43 now). The final contribution has now appeared, an introductory essay by the editors ('t Hooft, Erik Verlinde, Sebastian de Haro and Dennis Dieks).

The overall tone of the collection is one of defensive promotion of the subject. The fact that string theory's massively overhyped claims to give a unified theory of particle physics have led to miserable failure is mostly completely ignored. From the introductory essay one would never guess that string theory was ever supposed to have something to do with explaining the Standard Model of particle physics and that there were hopes that it would find some sort of vindication at the LHC, perhaps via the discovery of SUSY (the LHC is not even mentioned in this essay). String theory is presented purely as a theory of quantum gravity that has led to new insights in mathematics and had various other applications through the dualities it has uncovered. It's main shortcoming is described as

> the lack of directly testable experimental predictions that would signal 'string physics'

which seems to me intentionally misleading, implying that string theory makes indirectly testable predictions. The problem with string theory is that it makes no predictions about anything, not that it only makes indirectly testable ones.

Three of the eleven articles in the collection are described as representing critics of string theory. The first, from Carlo Rovelli, does do a good job of explaining many of the problems of string theory. Lee Smolin's contribution is not much about string theory, but more an examination of the general issue of the "Landscape problem", comparing a range of different theories in which the laws of physics are different outside our observable universe.

't Hooft's On the Foundations of Superstring theory calls for more attention to the lack of any fundamental description that tells us what string theory really "is", taking the point of view:

> we conjecture that the "true theory" is something totally different from superstring theory (and certainly also different from gravitating quantum field theories), but that string theory may approximate the truth to various degrees of accuracy in one or several of its compactified realizations, just as it does for some condensed matter systems and QCD.

He ends with an argument (which he notes is "one where only few readers will follow me") that one problem with string theory is that it uses the conventional quantum formalism, which he feels is flawed, needing replacement by an "emergent" version of quantum mechanics. For more about the sort of thing he has in mind, see here.

Two articles by philosophers of science, Dean Rickles and Richard Dawid address the question of how to evaluate a supposedly scientific theory that, like string theory, makes no experimentally testable predictions. Both pieces seem to me to suffer from a rather uncritical attitude towards various forms of string theory hype. For Rickles, the dominance of string theory can be justified by its "mathematical fertility", for Dawid the justification is "the assessment of scientific underdetermination" (roughly, there aren't any other good ideas). That it has led to some interesting mathematics and that there's not a lot of good alternative ideas out there are perhaps the two best arguments for pursuing string theory, but in both cases the situation is far more complicated than string theory advocates would have one believe.

The articles by string theorists (Balasubramanian, Giddings, Gubser. Martinec, Susskind and Duff) have a range of interesting things to say, sometimes amidst large dollops of string theory hype. Almost all evade serious discussion of string theory's failure to say anything about the Standard Model (although Susskind argues, a la Multiverse, that this a positive feature of string theory). Giddings perhaps makes the most serious criticism of string theory in the entire volume, discussing its problems as a theory of quantum gravity, where other authors see a big success and the theory's main selling point.

The article by Duff is by far the most bizarre thing in the volume, and I wrote about it extensively a year ago here. As Duff sees it, the problem is just that critics of string theory are misguided and misinformed. He includes a three page denunciation of Garrett Lisi which has nothing to do with string theory, characterizes the major recent research directions in string theory as fluid mechanics and the black hole/qubit correspondence, and has an appendix about the press release Imperial College put out making absurd claims that he had finally figured out how to make predictions from string theory (see here). The editors of the volume seem to be rather defensive about publishing such a thing, noting

> Needless to say that the opinions expressed in this paper are entirely the author's own and that it is not our intent to spark new popular or otherwise heated discussions.

but justifying it as

> we are happy to include this paper in our special issue as addressing questions that are important not only to scientists but also to the wider public, which was among our initial intents.

and ending with

> We warmly recommend Duff's very readable and playful contribution.

Nothing about Duff's piece struck me as "playful", but that the editors see it as some sort of joke would explain why they thought it worth publishing.

Update: Over at The Browser, Steven Gubser recommends that people should read *The Elegant Universe* and four string theory textbooks. Asked about the "no predictions problem", Gubser does his best to mislead, claiming the situation is just like that with QED that Feynman got the Nobel Prize for. As for SUSY, if the LHC finds it, that's evidence for string theory, if not, no problem. There's the old favorite "the LHC might produce microscopic black holes". About whether string theory makes testable predictions about the heavy ion physics the LHC is studying

> String theory might predict that such and such number is one, and the experiment might say well it's about two, but it could instead be one. That's the kind of accuracy with which things can typically be done.

Comments

1. **Bob Jones**
   December 5, 2012

   "String theory is presented purely as a theory of quantum gravity that has led to new insights in mathematics and had various other applications through the dualities it has uncovered."

Most people would say that string theory is an idea about quantum gravity. During the past fifteen years, most string theorists have been using the theory as a tool to study very general conceptual issues in quantum gravity and to understand the relationships between different quantum field theories. You can complain all you want about the lack of testable predictions in particle physics, but these objections seem pretty irrelevant since most string theorists aren't trying to do phenomenology and since string theory has achieved so much success in other areas…

2. **OMF**
   December 6, 2012

   I think Theoretical Physicists need to take a time out for a few months studying spinning tops.

3. **Friend**
   December 6, 2012

   I don't know how String Theory could have ever been considered as fundamental. It seems to me that a fundamental theory will have to explain why the universe is quantum mechanical to begin with. And ST is only an added layer on top of QM; it does not even attempt to explain where QM came from. It seem that alone should have raised suspicion against its claim of being a fundamental theory of everything.

4. **Girlfriend**
   December 6, 2012

   It soberly is true, what you say, Friend.

5. **Peter Woit**
   December 6, 2012

   Sorry, but those who want to discuss why they're unhappy with quantum theory need to find another place to do this, it's off topic here, unless it's specifically about 't Hooft and his attempts to bring string theory into it.

6. **Bernhard**
   December 6, 2012

   One thing I don´t get it is why there was no contribution from Witten? Perhaps he refused?

   String theory is already in pieces and while there is no need to beat a dead horse, the fact that the bunch of crap Duff wrote actually got published as a representative contribution to celebrating 40 years of string theory is good evidence of the moribund state of the theory.

7. **A.J.**
   December 6, 2012

   Maybe Witten was busy writing the roughly 400 pages of detailed technical notes on superstring perturbation theory that he just put on the arxiv?

8. **Bernhard**
   December 6, 2012

   A.J.,

   Since is Witten we are talking about, I´m sure he could have done both.

9. **no one of consequence**
   December 7, 2012

   Epicycles were an example of "mathematical fertility". Yet they were wrong.

   For a supposed whole branch of theoretical physics to consume a generation of minds / carreers, shouldn't there be something of substance to even hint at relevance in the measurable physical universe?

   Back in the late 70's in the Berkeley Physics department, I was a dubious observer who inelegantly suggested that studies of this kind belonged elsewhere in the philosophical realm, until such hints presented themselves. Since that time I've seen nothing to change that inate skepticism.

   And while I can understand the collective embarassment not wanting to cede ground, perhaps BSM and other grand schemes … should just be pursued when we have a long confirmed discrepancy, as in the past? Rather than assumed as to be present?

10. **fuzzy**
    December 7, 2012

    dear no one of consequence, i feel that neutrino masses and dark matter are recent achievements that do not belong the standard model. i am not fully sure of what to think about the issue of strong CP, but it is also a stimulating point where we can proceed experimentally. several other appealing issues in cosmology (inflation, present day accelerated expansion, speculations on the origin of the matter etc) have been also clarified since 70's. i would say, very few of them have received the slightest contribution from the stringy ideologists — an exception is goodman-witten's contribution on direct dark matter search.

11. **fuzzy**
    December 7, 2012

    ps perhaps this is the reason why witten is not in the book?

12. **Chris Oakley**
    December 7, 2012

    These denunciations of epicycles fail to take account of the fact that an ellipse is a circle with a single epicycle

13. **Bob Jones**
    December 7, 2012

    no one of consequence,

Has it ever occurred to you that there might be some good reasons for doing string theory? Do you really think the subject has survived for forty years with no results? Do you really think string theorists are that dumb?

14. **Peter Woit**
December 7, 2012

All,
One can argue about what if any role "dumb" plays in the string theory story, but this discussion is fairly deep in the "dumb" category. I realize that this is by now a very tired subject, but still… There's a lot of material in this volume, if you read some of it and have an interesting comment, please contribute it, otherwise, please spare the rest of us…

15. **Armin Nikkhah shirazi**
December 7, 2012

Peter,

It seems to me that String Theory does make at least one definite prediction:
Like any other mainstream theory of quantum gravity (that I have heard of), it predicts that the gravity field of an object in a quantum superposition is also in a superposition.

While I don't see the significance of this pointed out very often, to me that constitutes a profound claim about the world which has as yet not been directly tested. I think the importance of this may be minimized by three factors: First, it is probably at the level of present technology impossible to perform a direct unequivocal experiment to test it; second, there are good indirect reasons (e.g. conservation of momentum coupled to the fact that objects in a quantum superposition are affected by gravity fields in exactly the right way) to expect that if such an experiment could be performed, the prediction will be confirmed and third, there is no mainstream rival hypothesis which makes a different prediction (and any framework which does so would automatically be considered non-mainstream).

But, given that this claim enjoys such a central position at the core of any quantum theory of gravity, should we not refuse to settle for anything less than a direct test before we dismiss any doubts about its correctness? Rather than testing predictions which rule out regions of the parameter space in which the models based on a framework are still viable, yet leaving virtually infinitely many possibilities open at higher energy scales, would it not be far more definitive to identify a falsifiable prediction which does not permit any such adjustment? Surely, if this central claim were found to be false, it could not be compensated for by modifying string theory because that is part of the very essence of quantum gravity. Changing *that* aspect of string theory is to kill it.

That means a falsified result would lead to nothing short of a scientific revolution, and it helps to keep in mind that these usually happen when something that was universally regarded as "obvious" turned out to be a false assumption about nature.

Ideally, recognizing this claim as a prediction should have spurred the development of new experimental techniques by which one might eventually be able to test it. Alas, who is going through all the trouble of tackling a practically impossible experiment for the outcome of which no one expects a surprise?

Sometimes I think that it sure was a good thing that the Michelson Interferometer was not impossibly difficult to construct, otherwise the aether might have stayed with us much longer than it did. In fact, it does not seem preposterous to me to imagine that parameter adjustments analogous to what one sees today might even have allowed an aether theory to survive (probably in a very complicated form) up to today as the dominant space-time paradigm.

But, we were lucky that it is in fact relatively easy to build a Michelson Interferometer. We do not appear to be so lucky when it comes to building a device that could measure the existence of gravity fields in a superposition.

I'd be interested to know whether you consider the superposition of gravity fields a prediction of string theory and if not, why.

Thanks,

Armin

16. **Peter Woit**
December 7, 2012

Armin,

This really has nothing much to do with string theory but is generically about quantum theory, so I don't want to encourage discussion of this here. Generic questions about quantum gravity are a huge topic, one I'm not very expert in.

The way you're trying to relate this to string theory is kind of like the way I've seen some prominent string theorists argue with people who say string theory is not falsifiable, by saying that if people observe violations of the axioms of QM in tabletop experiments, that would falsify the current understanding of string theory. First of all, my guess is that if a tabletop QM violation was found, there would quickly be papers out there explaining it with some exotic version of string theory. Secondly, this is all a bit like saying "my theory is falsifiable, because if God emerges from a collision at the LHC with a sign saying my theory is wrong, that would show it was wrong". The falsifiability criterion is intended to refer to distinctive aspects of a theory that differentiates it from others, not generic properties common to all known theories.

17. **Bob**
December 8, 2012

Yes if QM was violated then string theory would be dead. If local lorentz invariance was violated, then it would be dead too.

On the latter point, there are alternative theories out there, such as LQG, that predict violations of lorentz invariance, and these claims have been falsified. So string theory certain makes predictions that distinguish it from other theories. Other theories commonly predict a breakdown of lorentz invariance, and some violate quantum mechanics or the equivalence principle badly, and have been ruled out. So certainly the evidence is pointing towards string theory at this stage, but who knows what future experiments will reveal?

Also, Peter what do your favorite theories of QG predict, other than what I have mentioned so far?

18. **Peter Woit**
December 8, 2012

Bob,

I don't have a "favorite" theory of quantum gravity. The LQG vs string theory hype-filled arguments like the ones you are making about "my theory sucks less than your theory" just don't interest me at all.

19. **Bob**
December 8, 2012

Peter, okay. But just a question: do you know of any predictions coming from any of the QG theories? (I am mainly curious about theories other than string theory here).

20. **Peter Woit**
December 8, 2012

Bob,

I don't know of any "prediction" from a quantum gravity theory that isn't an abuse of the term.

21. **fuzzy**
December 8, 2012

hi bob, a good example of prediction in the field you mention has been done by one contributor to this book: http://arxiv.org/abs/arXiv:1110.0521. it was shown that neutrinos can be superluminal, and the doubts raised by the other investigators were irrelevant.

please, try to imagine the impact of this paper on experimentalists and on a wider public, in the moment when the experimental claim was made. then, judge by yourself the scientific value of such a "prediction".

22. **Yatima**
December 8, 2012

Off-topic but …

The epitaph of an anomaly which was pretty much improbable to begin with:

http://www.science20.com/quantum_diaries_survivor/operas_final_word_operas_superluminal_neutrinos-98408

And another interesting anomaly vaporizes into its error bars:

http://spectrum.ieee.org/aerospace/astrophysics/finding-the-source-of-the-pioneer-anomaly

23. **Peter Woit**
December 8, 2012

fuzzy,

That's not the only volume contributor who weighed in on superluminal neutrinos. According to Mike Duff, string theory could explain them (although he did say he didn't believe the result or that string theory was the explanation, just that it could be…)

24. **fuzzy**
December 8, 2012

the "prediction" i quoted is published: i mean, several colleagues have pondered and decided to leave their findings to posterity, independent editors agreed this was useful, some referee implied in judging, a lot of readers, and all that. but i agree that mod phys lett is less authoritative than prl, thus your duff probably wins the context http://arXiv.org/abs/arXiv:1005.4915 (even if, counting citations, my smolin should do, and the stringy prediction published on prl concerns something else)

25. **Armin Nikkhah Shirazi**
December 8, 2012

Peter,

Thank you for your response. I won't prolong the discussion on this but let me just note that ultimately, when the consistent response to contradictory empirical data is that only a subset of of all possible models based on a framework has been falsified where the entire set may well be infinite, then it seems to me one has to consider falsification at a more generic level, so in that sense I do see this question as quite relevant to string theory.

And, in my view, your last sentence, that this is an aspect of "all known" theories is an overgeneralization. It is just a feature of all current mainstream approaches to understanding the relation between quantum theory and general relativity.

Bob,

It appears to me that you are implying that failure to observe gravity fields in a superposition is on the same footing as a violation of quantum mechanics or local Lorentz Invariance.

We have overwhelming evidence that the latter two are correct descriptions of nature, but we have never observed a gravity field directly in a superposition. We have indirect arguments suggesting that this may be the case, but the superposition of gravity fields is emphatically not on the same footing as standard quantum mechanics or local Lorentz Invariance.

I don't know if you did this on purpose, but in the second paragraph you exactly illustrated the string theorist argument Peter mentioned in his reply to my comment. If his argument, that there are exotic versions of string theory which could explain any violations of QM at all, is true, then it seems to me a rather definitive refutation that this constitutes a falsifiability criterion.

26. **Bob**
December 9, 2012

Armin, my understanding of string theory is that it exactly respects QM. Peter is the only person I'm aware of who appears to be advocating alternative string models that somehow violate QM. It is a weird proposal by Peter, but I can't comment further on his model.

27. **Dim Reg**
December 9, 2012

Bob,

LQG does not break local lorentz invariance. It was beleived that since it predicted a minimum length eigenvalue, that length must have been invariant (just like how c is the maximum velocity, so it has to be invariant). But that is wrong because the probabilities are transformed, not eigenvalues, like how you can't just boost away the vacuum energy. I also beleive that one can spontaneously break lorentz invariance in string theory, so those experimental results have told us very little about these two theories of quantum gravity.

Peter,

I seriously doubt it is possible to find an exotic form of string theory that isn't quantum mechanical. Formulating a theory in terms of non-zero commutators and hilbert spaces builds uncertainties and superpositions into the theory, and this is done when constructing string theory. I don't see how any clever trick could remove either of these things, though perhaps I'm just not clever enough.

28. **Bob**
December 9, 2012

Dim Reg, I don't know of any serious proposal to break the local lorentz invariance in string theory. This is especially dangerous as it is needed for the theory to carry the general coordinate invariance in the usual way, and it surely carries this. You refer to spontaneous breaking, but that, by definition is a property of low energy or long distance physics, so can't be relevant for the local, i.e., short distance, physics.

With regards to LQG, it was repeatedly claimed by Smolin that it would alter the photon dispersion relations, and this claim was falsified. Maybe there are other proposals to avoid this, I'm not sure, but certainly the evidence is disfavoring LQG at this stage.

29. **Peter Woit**
December 9, 2012

Bob and Dim Reg,

I don't even know what it would mean for an experiment to "violate QM". 't Hooft claims to have a possible non-QM foundation for the superstring, and it's well-known that the experts all say that we don't know what string theory "is": the issue of its foundations is still up in the air.

If tomorrow there's a solid report of an "experimental violation of QM", which do you think is more plausible:
1. string theorists wholesale tell the media the theory has been falsified and stop work on it.
2. dozens of papers start appearing on the arXiv purporting to explain the experimental anomaly in terms of string theory models or "string-inspired" physics.

Responding to someone asking if string theory makes predictions by saying "no, not now, but I think for reasons X it is the best thing to work on to try to get to a better theory, one that would make predictions" is honest. Saying "string theory does make predictions" and pulling out something like the QM business just isn't.

30. **lun**
December 9, 2012

People who talk about "violations of QM in gravitational systems" need to understand two issues: Firstly, gravity is only detectable in macroscopic systems, where in any case QM is in any case"violated" through processes such as decoherence (perfectly consistent with quantum mechanics, producing classical-looking results).
Secondly, no quantum theory of gravity (and that includes string theory) is sure what either the Hilbert space or the observables of quantum gravity are.
Therefore, it is wrong to think of "violations of quantum mechanics" as if this was a well-defined experimental signature.

31. **Dim Reg**
December 10, 2012

Bob,

It looks like I stand corrected on string theory. I'm certainly not an expert in that field and your arguments make sense to me. My point with LQG was that it was never a correct reading of the theory to say that it broke lorentz invariance. Lee Smolin was just making LQG hype, trying to say that his theory was better because it made currently testable predictions. Regardless, I don't intend to champion LQG (I don't actually think they are right), and this certainly wouldn't be the place to do that.

Decay: The LHC Zombie Film

December 8, 2012
Categories: Film Reviews

Today is the release date for the film Decay, described as "a zombie film made and set at the LHC, by physics PhD students". It's available for download here, on Youtube here.

The plot is summarized as

> The film follows a small group of students (played by physicists) after a disastrous malfunction in the world's biggest particle accelerator. As they try desperately to escape from the underground maintenance tunnels, they are hunted by the remains of a maintenance team, who have become less than human.

It's quite professionally done, on a remarkably low budget of about $3000, and of course the science is way, way better than usual for a Hollywood film. Highly recommended. Note that

> This film has not been authorized or endorsed by CERN

For something more reality-based, try the latest episode of Colliding Particles, entitled "Blogs", which features Philip Gibbs and his role in blogging the Higgs and putting together unofficial combinations of results. *Not Even Wrong* puts in a cameo appearance in the background…

Comments

1. **Plutonium Archimedes II**
   December 8, 2012

   "The film follows a small group of students (played by physicists) after a disastrous malfunction in the world's biggest particle accelerator. As they try desperately to escape from the underground maintenance tunnels, they are hunted by the remains of a maintenance team, who have become less than human."

   Sounds fun! But do they (the film-makers) understand that there are going to be people who will think that they (the film-makers) are warning against a possible scenario that might actually happen as a result of the operation of the LHC?

   Although, really, such a misinterpretation sounds fun too!

2. **Low Math, Meekly Interacting**
   December 9, 2012

   This is just about the best thing I've seen on the interwebs in a very long time….which is kind of pathetic, but anyway, many thanks for the link!

3. **chris**
   December 10, 2012

   why didn't they cast the resident string theorists as the zombies?

4. **Chris Oakley**
   December 10, 2012

   Chris, you obviously did not follow the story. In order to become zombies, they have to be walking around the *experimental apparatus* underground.

   Must admit, I did not make it past about minute 35. It seemed a lot like *Shaun of the Dead* but without the humour. Oh, and since when was *A Clockwork Orange* deep? Maybe that *was* the humour.

5. **DON Murphy**
   December 10, 2012

   A little surprised to hear you say, ". . .and of course the science is way, way better than usual for a Hollywood film." with reference to a movie about zombies. However, all in good humor I suppose.

6. **Tmark48**
   December 10, 2012

   I saw the whole film. Have to say it was pretty entertaining and the end well it's not your typical Hollywood ending that's for sure. The premise of the story while not realistic was fun, and real life location sure beats CG sets.

This Week's Hype

December 10, 2012
Categories: This Week's Hype

Space.com has a new story entitled Space Bursts Provide Insight to Theory of Everything, which has been picked up elsewhere as "evidence for string theory". For instance Physicists Find New Evidence Of A 'Theory Of Everything' In The Wreckage Of Dead Stars tells us:

> Physicists studying the rotation of minuscule particles fired by exploding stars light years from Earth have found new evidence for a so-called 'Theory of Everything'.
>
> Researchers have been frantically studying ways to reconcile two apparently contradictory pillars of modern physics for decades.
>
> Put simply, those are Einstein's theory of relativity – which covers the interaction of space and time on a large scale – and quantum theory, which covers the strange ways that sub-atomic particles behave.
>
> One of the ideas mooted as a possible explanation is string theory, a framework which proposes that all of matter is made up of loops of vibrating strings…
>
> What is relevant for this story is the proposal in superstring theory that every particle of matter has an equal and opposite 'anti-matter' particle, which if time were reversed would behave in exactly the same way as normal matter.
>
> And it is this that new observations by the Japanese Aerospace Exploration Agency's Ikaros spacecraft could help reinforce…
>
> Using their Gamma-Ray Burst Polarimeter, the scientists are studying how those particles rotate. If the rotation of their polarity had changed even slightly, it would indicate a lack of symmetry if time were reversed – thus evidence against superstring thory.
>
> And, luckily, the reported conclusion is that no change was detected. The team said that they are confident to one part in 10 million that the symmetry is consistent – a new record.

So the idea seems to be that CPT symmetry is evidence for string theory. Kind of like how it has become popular to claim observations being consistent with quantum mechanics as "predictions of string theory".

The Space.com story seems very confused: string theory predicts no CPT violation, but finding evidence for it would support string theory:

> The findings could have implications for superstring theory — the idea that all fundamental particles are actually loops of vibrating string — which is one attempt to unify nature's forces and create a theory of everything. If the idea is right, it would help reconcile two contradictory theories: Einstein's general relativity, which describes things that are very big, like gravity, and quantum mechanics, which describes the realm of the very small…
>
> Superstring theory scientists predict that if particles and anti-particles (antimatter is an opposite form of normal matter) traded places and time was reversed, the world would still look the same. If any evidence is uncovered that matter and antimatter actually act differently, or violate their apparent symmetry, it could offer support for superstring theory.

They also link to a new story about 5 reasons we may live in a Multiverse.

What's generating these stories is this press release from the University of Tokyo, based on PRL acceptance of this paper. It's about an interesting test of CPT invariance, but bringing string theory into it is bizarre, and even the authors aren't clear about whether string theory says CPT or no CPT. From the paper:

> Lorentz invariance is the fundamental symmetry of Einstein's theory of relativity. However, in quantum gravity such as superstring theory , loop quantum gravity and Horava-Lifshitz gravity , Lorentz invariance may be broken either spontaneously or explicitly. Dark energy, if it is a rolling scalar field, may also break Lorentz invariance spontaneously. In the absence of Lorentz invariance, the CPT theorem in quantum field theory does not hold, and thus CPT invariance, if needed, should be imposed as an additional assumption. Hence, tests of Lorentz invariance and those of CPT invariance can independently deepen our understanding of the nature of spacetime.

and the press release:

> Some quantum gravity theories, trying to unify Einstein's theory of relativity with quantum mechanics, (e.g., superstring theory) predict that structures of space-time at extremely short distances may be totally different from what we think we know. On the scales treated by terrestrial experiments, the world looks exactly the same as its mirror image if the roles of particles and anti-particles are exchanged and the direction of time is reversed (i.e., CPT symmetry is conserved). If this symmetry is broken at extremely short distances, as predicted in some quantum gravity theories, polarization of photons from distant celestial objects would rotate during its long journey to us.

I was starting to get more optimistic that the days of nonsensical "tests of string theory" might be over, but it looks like this phenomenon is here to stay.

Update: Scientific American has the same story, headed with:

> Gamma rays emitted during the formation of neutron stars and black holes allow scientists to study fundamental principles like superstring theory

Comments

1. **Low Math, Meekly Interacting**
   December 11, 2012

   According to the SciAm article, GRBs are so powerful they can "accelerate photons almost to the speed of light." Mmm.

2. **Bee**
   December 11, 2012

   Nick Mavromatos has been on the topic of CPT invariance violation for a while, based on what he calls "stringy" models. Maybe that's the origin of this?

First Results from the Large Hardon Collider

December 10, 2012
Categories: Experimental HEP News

There's a conference in Bad Honnef going on now entitled First Results from the LHC, with a website that carries two different interpretations of what "LHC" stands for (see the screenshot below):

The talks are here. Yesterday CERN DG Rolf Heuer gave a summary talk about The Terascale after 2 years of LHC. Tomorrow some SUSY enthusiasts will be summarizing their view of the current situation, with John Ellis scheduled to talk on "What is it? What else? What next?".

In other news, I'm hearing rumors of a big announcement tomorrow at CERN. The rumor is that Yuri Milner has decided that not all prizes should go to theorists, and that the Nobel committee not awarding prizes for the Higgs discovery is something that he can help fix. We'll see tomorrow if this pans out…

Comments

1. **Bob Jones**
   December 10, 2012

   Off-topic:

   http://www.math.sunysb.edu/frenkel60/Frenkel/talkvideos.html

2. **Peter Woit**
   December 10, 2012

   Bob Jones,
   Thanks! I've added that information to the posting from last spring about the Frenkel conference.

3. **Eric**
   December 10, 2012

   http://www.nytimes.com/2012/12/11/science/multimillion-dollar-physics-prizes-for-stephen-hawking-and-cern.html?ref=science

4. **Peter Woit**
   December 10, 2012

   Thanks Eric,
   My rumors were quite incomplete. I guess a posting about the full story is called for…

5. **DJBunk**
   December 12, 2012

   'Large Hardon Collider' – I think you might want to edit this to avoid undesirable web traffic 🙂

6. **Peter Woit**
   December 12, 2012

   DJBunk,

   All I'm doing is linking to the conference web-site, and that's how they titled their web-page, so, what can I do?

New Milner Prizes

December 10, 2012
Categories: Uncategorized

The New York Times is reporting that tomorrow Yuri Milner will be announcing the award of a new set of prizes for fundamental physics work, this time including some experimentalists as recipients. The awards are

- $3 million for the experimental discovery of the Higgs at CERN. This will be split into three parts: $1 million to Lynn Evans for his work building the machines, $1 million to ATLAS current and ex-spokepersons Fabiola Gianotti and Peter Jenni, and $1 million to CMS current and ex-spokepersons Joe Incandela, Michel Della Negra and Tejinder Virdee. I'm suspicious that the NYT has missed CMS ex-spokesperson Guido Tonelli, who was on the list I heard about earlier today from a source at CERN.
- $3 million to Stephen Hawking for his work on black holes.
- Three "New Horizons" prizes of $100,000 each to younger theorists working on string theory and SUSY: Niklas Beisert, Davide Gaiotto and Zohar Komargodski.
- Two "Physics Frontiers" prizes of $300,000 each to string theorists Alexander Polyakov and Joe Polchinski, with a third $300,000 prize going to a group of condensed matter physicists (Charles Kane, Laurens Molenkamp and Shoucheng Zhang) who work on "topological insulators" among other subjects.

Polyakov, Polchinski and the group of condensed matter physicists are now the contenders for the $3 million 2013 Fundamental Physics Prize which the NYT story says will be awarded "by a vote of the judges on the morning of March 20 at CERN and announced in a ceremony that evening."

The special award to the LHC physicists should help make up for the problem that no Nobel prize may end up going to the Higgs discovery because too many people were involved. Milner has the advantage of not being bound by long tradition and arguably out of date rules the way the Nobel Committee is.

On the theory side though, these awards make it clear that the Fundamental Physics Prize story is likely to be heavily dominated by awards from string theorists to string theorists for work on string theory. Besides Hawking, all the recipients have some connection to string theory, with the condensed matter physicists working on a hot topic which many string theorists see as the future of their subject. For more about the recent history of the string theory/condensed matter connection, see this article from last year in *Nature* which includes this, which refers to certain books published in 2006:

> "It's hard to say whether the interest in condensed-matter applications is a direct response to those books because that's really a psychological question," says Joseph Polchinski, a string theorist at the Kavli Institute for Theoretical Physics in Santa Barbara. "But certainly string theorists started to long for some connection to reality."

Update: The NYT article has been revised to include Tonelli.

Update: The press release with more details is here. There's a story at the Guardian here. CERN has more here, including interviews with the LHC winners. They comment on the fact that they are getting awards that belong to much larger groups, with Incandela and Gianotti saying they are trying to find a way to distribute the money to younger members of the collaboration who most need it. Lyn Evans comments

> I will not be driving around CERN in a Ferrari. That would be very bad for my image.

The Guardian has Hawking saying his plans for the money include helping his daughter who has an autistic son, and maybe a new vacation home.

About the question of what the previous Milner prize recipients are doing with their $3 million, I've heard rumors that the one mathematician, Kontsevich, has been giving it away to others. From the physics side, the only thing I've seen was that Witten planned to give some to J Street, a group working for peace in the Middle East.

Update: For commentary on whether ATLAS and CMS spokespersons should keep the money, see Tommaso Dorigo.

Comments

1. **Heidar**
   December 10, 2012

   I am not sure I agree that all the theoretical prices (except Hawking), are directly based on string theory. For example Zohar Komargodski (one of the brightest people I have ever met), is mostly famous for work on field theory rather that string theory (proof of a-theorem for example, which is quite an important achievement). Also much of his work on SUSY is based on understanding non-perturbative aspects of field theory better, and not directly having string theory in mind.

   As for the condensed matter award (for discovery of topological insulators) I disagree even more. These states are very interesting by themselves, and has almost no connection to string theory. They are not so interesting to study for AdS/CMT correspondence, since they are just gapped free-fermion theories and not some exotic strongly interacting gapless theories. The only paper I really know where people try to connect topological insulators to string theory, tries to draw a line between the K-theoretical classification of topological insulators to the K-theoretical classification of non-BPS branes, RR-fields etc.. Its an interesting idea, but I am afraid that the connection is not too profound, but rather about 2=2 and 8=8 (complex and real Bott periodicity, respectively). Can you elaborate in what sense you think topological insulators will connect to string theory?

2. **Interested Layman**
   December 10, 2012

   Why is it that Peter Higgs himself was not awarded any prize?

3. **Peter Woit**
   December 10, 2012

   Heidar,

   I didn't say they were "directly based on string theory", but had "some connection" to it. Keep in mind that these days, what string theorists think of as "string theory" covers quite a lot. I'm no expert in condensed matter theory, and make no claim to understand what the prospects for techniques growing out of string theory are in different parts of that field. My observation was just the sociological one that many string theorists see applications to condensed matter as the hot topic and future of their subject, so this is a topic they take an interest in. To the extent they work in this area and have any success, it may of course be due to purely field-theoretical techniques.

   Put differently, it looks like HEP phenomenology will be completely locked out of this. All the jobs may be going to phenomenologists, but that doesn't mean the Fundamental Physics laureates are going to vote for them for awards.

4. **Peter Woit**
December 10, 2012

Interested Layman,

I think Milner is concentrating on making awards that complement the Nobel. An award for Higgs or others is exactly the kind of thing the Nobel committee normally does, so I'm guessing Milner has decided to leave Higgs up to them. He also seems to be more interested in rewarding relatively recent work, not octogenarians for things done 50 years ago (although Hawking is no spring chicken…)

5. **King Ray**
December 10, 2012

Perhaps Higgs didn't win a Milner prize because he committed the faux pas of predicting a particle that actually ended up being observed. Quel outrage! 😉

6. **M**
December 11, 2012

prize to spokepersons?
Why not giving the prize to television journalists who spoke about the Higgs?

7. **P**
December 11, 2012

M,

The spokepersons are actually professors and practitioners of high energy physics. They rose to their positions through years of hard work in the field and communicate results to the public due to scientific expertise. Though it is strange to reward one person in a collaboration of thousands, the analogy (even tongue in cheek) to journalists isn't a good one.

Cheers,
P

8. **Simon**
December 11, 2012

This could be a problem if the spokespeople use the money for their own personal benefit. They are only several among a long list of people who played key roles in the experiments and the discovery. They're aware of this and don't try to hide it. If they keep the money this will make life a little difficult on the collaborations. Were I to have received the money it wouldn't be an easy decision to make to give it away (we all have mortgages to pay).

As was mentioned earlier, its very easy to destabilise a well functioning institution by pouring in money without too much prior thought.

9. **anon**
December 11, 2012

The argument that he leaves Higgs for the Nobel Prize is not really consistent with the award for the solid state experimenters. Their work should also be eligible for a Nobel Prize.

Concerning the three younger theorists, I'm with Heidar in that they are dominantly field theorists in my opinion (including supersymmetric field theories of course).

Congratulations to all of them!

10. **Bernhard**
December 11, 2012

Peter,

I remember when I said "this will become a string theory prize" you disagreed with me

http://www.math.columbia.edu/~woit/wordpress/?p=4953&cpage=3#comment-119763,

but perhaps I was too vague. That, for the theory prizes, nearly all have "some connection" to string theory is no surprise and the kind of thing I had in mind…

I still have the feeling this whole story of this Milner Prize is undermining the importance of a real prize (the Nobel). Society is money driven and people simply don't know how careful and long the process of choosing a Nobel Laureate is compared to the Milner Prize, which on the theory side is a "my pals get the prize" thing. And of course is worsen by the fact that is bigger money.

Even worse, since the prize for experimentalists ended up being decided by theorists who are in general clueless about how a modern collaboration really works, they had no other choice than do the most anachronic thing possible, which we were hoping the Nobel would correct, which is to give the prize to the spokespersons instead of to the collaborations or "to CERN" for that matter.

I admit I would never refuse the cash if I was the one receiving it and is only understandable the contenders are happy with it, but that I have to read from Heuer that "This prize recognizes the work of everyone who has contributed to the project over many years" is laughable. It also gives a clear message to young experimentalists that the way to success in collaborations is only by being a spokesperson which of course are not just about being a good scientist but also about charisma.

This whole thing is really depressing. Money talks everywhere, but should not talk so loudly in science.

11. **Mark**
December 11, 2012

Both spokesperson have said they will not keep the prize for personal use:

http://www.guardian.co.uk/science/2012/dec/10/stephen-hawking-physics-prize

12. **Bernhard**

December 11, 2012

Mark,

I agree this is really noble of them and not something everybody would so. To be clear, they are really not to blame for getting a prize and certainly being a spokesperson of a HEP collaboration is not without merits. Still, my hope was that none of this would be actually necessary and that people would start viewing the collaborations as the semi-headless living organisms they are. The Milner Prize is no a step forward to achieve this goal.

13. **nessuno**
December 11, 2012

Dear Peter,
I am pretty sure that Evans will give the money to CERN, to partially pay back for his share of responsability in the 2008 LHC incident, caused by the negligence of him and of some other highly placed people. The incident material damage was estimated at 28.000.000 CHF, without counting one additional year of delay. An article on the Cern Courier had warned years in advance about the consequences of not carefully testing each LHC interconnection; yet, to save time, they skipped these tests. Here is the link
http://cerncourier.com/cws/article/cern/29012
Best regards,
nessuno

14. **Bernhard**
December 11, 2012

sorry, *not something everybody would do*

15. **Mark**
December 11, 2012

Yes I agree, its not great that they only acknowledge one person amongst several thousand (many of whom will have made significant scientific and other contributions to the Higgs discovery) and gives the wrong idea to outsiders about how things work.

16. **Marcel van Velzen**
December 11, 2012

It used to be Nature that told us who are the greatest theoretical physicists!

17. **Bee**
December 11, 2012

The AdS/CFT for cond matt stuff is interesting… but really what have we learned from this so far other than that there's a lot of string theorists who have hammers and are looking for nails? It seems somewhat premature to hand out awards for this, same with susy and black hole evaporation – the way it looks right now this "fundamental physics" prize runs high risk of becoming a "mathematical physics" prize, or maybe not even that.

18. **Heidar**
December 11, 2012

@Bee

Which of the prizes have anything to do with AdS/CFT for cond matt…?

19. **Esteban**
December 11, 2012

Given all the prizes awarded I find it odd no Milner Prize was awarded to the five remaining Higgs theorists (Englert, Higgs, Guralnik, Hagen, Kibble). Especially since there is this issue of "3" for the Nobel Prize.

20. **Peter Woit**
December 11, 2012

Esteban,

One issue is that not everyone is convinced that all of those people deserve a Nobel Prize or something paying 3 times more.

Also, the Milner theory prizes have been restricted to topics that don't have experimental confirmation of the sort the Nobel requires (or have even been for work that has found experimental disconfirmation…). If Milner starts going back to the early 1960s to find theorists to reward, including ones whose work never led to experimental confirmation, even he might start running out of money.

21. **Esteban**
December 11, 2012

I think the original theorists for the Higgs deserve a significant prize – especially if we are awarding prizes for the experiments. Pretty tough to determine who deserves most credit for this topic among the theorists and more broadly had a better physics career across those five. Also, having a low H-index should not exclude Peter Higgs from the Milner or the Nobel Prize.

22. **anon**
December 11, 2012

The prizes for the senior physicists all make sense in absolute terms, representing diverse directions, though many great contributors to the field are not included. The young guys are also all good choices, but reflect a clear bias for those who have gone through the IAS and work on formal supersymmetric quantum field theory. Young researchers from other institutions have made recent breakthroughs in well-motivated quantum field theory problems with real applications. This all has elements of a reality show, and I hope it does not deter talented physicists from pursuing a wider variety of important problems.

23. **Bob Jones**
December 11, 2012

“The AdS/CFT for cond matt stuff is interesting… It seems somewhat premature to hand out awards for this, same with susy and black hole evaporation”

Okay, first of all, the idea that these condensed matter prizes were awarded for work related to string theory is pure speculation. The citation for the prize says nothing about string theory. Secondly, the prize winners who study SUSY are working on formal aspects of SUSY, not speculative extensions of the standard model. Davide Gaiotto has done a lot of work on the six-dimensional (2,0)-theory for example, and his work has applications to the study of wall-crossing phenomena. This sort of work doesn’t require experimental verification, so I’m not sure how the prize could be premature. Finally, black hole evaporation is something that we will probably never observe, so it’s obviously being recognized as a formal result which has had a huge impact on quantum gravity research. I’m not sure how it’s premature to give an award for this.

“the way it looks right now this ‘fundamental physics’ prize runs high risk of becoming a ‘mathematical physics’ prize”

High risk? What’s wrong with mathematical physics?

24. **layman**
December 11, 2012

This is ridiculous. The work of the condensed matter guys has absolutely no direct connection to string theory, and to even hint that they got the prize because string theorists believe there may be a connection in the future borderlines in insanity and complete delusion (or schizo). A condensed matter person got the prize last year, some got it this year, and some will get it next year. There is no conspiracy here, except in Peter’s mind.

In addition, the work on the a-theorem mentioned above was neither motivated by nor related to string theory. Finally, Gaoitto’s work with collaborators on dualities across dimensions is a purely field-theoretic result, that involves non-SUSY 2d models that are extremely important in many branches of physics.

I have been following this blog for a while, and I must say that this post is a real peak in the amount of odor of delusion coming from it.

I hope Peter would be kind enough to leave this comment in place, so that the public knows better.

25. **Bernhard**
December 11, 2012

layman,

Even if the condensed matter guys have no connection with string theory this is still the “Physics Frontiers” prize not the “Fundamental Physics” prize, and this is the one that interests me because this is the one outsiders + media will be mostly looking at, simply because is THE big cash money.

My bet is that the condensed matter guys are there as a way to pretend this is not what is is: a string theory prize. In my view they don´t really have a chance. The big prize goes either to Polchinski or to Polyakov. Time will tell if I´m right.

26. **observer**
December 11, 2012

Layman,

The stringy types around my way at least are doing almost exclusively what they call condensed matter nowadays and have been working and publishing on topological insulators and other related topics inspired by them. And Charlie Kane et al are on clear track for a real Nobel, so it does make you wonder why they should be singled out for such recognition by a prize supposed to be somehow compensatory for lack of Nobel-rewardability.

27. **Bob Jones**
December 11, 2012

“The big prize goes either to Polchinski or to Polyakov. Time will tell if I´m right.”

And are they not deserving candidates?

28. **Bernhard**
December 11, 2012

Bob Jones,

Sure they are. But if only deserving string theorists get the prize as opposed to deserving theorists no matter their field, than one might as well change the name of the prize to the “Stringy Prize”. We are not there yet, so let´s hope I´m wrong.

29. **layman**
December 11, 2012

Bernhard: Polyakov’s seminal contributions from the 70s,80s are about CFTs etc, which is nowadays a cornerstone in condensed matter. His contributions to *condensed matter physics* arguably exceed those of the pure condensed matter guys who got the prize. Needless to say, Polyakov’s work on CFTs and minimal models (which describe all the basic spin systems at criticality) is familiar to every theoretical condensed matter physicist, so I wont be surprised if he gets the big cash in the end of the day. It would be totally justified.

Observer: none of the stringers on the committee worked or works on the connections between strings and condensed matter. (Perhaps there is one tangentially related paper by one of the committee members that I wont mention, but definitely none of them embarked on a line of research in this direction.) In fact, I know that many of them consider this activity disagreeable.

This is why the suggestion that condensed matter people are involved because this is where the committee sees the future of string theory is of utmost absurdity.

I dont think the prize is supposed to be a consolation prize, it is supposed to *supersede* the Nobel prize.

30. **A.J.**
December 11, 2012

Many of the best theoretical physicists of the past few decades have worked on string theory. Giving these people the prize doesn’t make it a string theory prize. It just reflects the history of the discipline. In 10 years, you’ll probably see a lot fewer prizes for string theory.

You could argue, I suppose, that the Milner Prize should be given to less formal, more phenomenologically oriented theorists, but I don't see the point of this. At least the people doing formal theory have taught us a few things about the behavior of QFTs in general, whereas it's looking like HEP phenomenology hasn't made a correct prediction about TeV scale physics since the Standard Model was put in place.

31. **Peter Woit**
December 11, 2012

layman,

To be clear, I'm well aware that most of what many people generally described as "string theorists" do these days has nothing to do with string theory. When I point this out, I get attacked as denigrating string theory, now you're attacking me for the opposite reason, for implying a connection to string theory of the work of Beisert, Gaiotto and Komargodski.

Gaiotto is an excellent example. I agree that his very interesting work, which is cited as "far-reaching new insights about duality, gauge theory, and geometry, and especially for his work linking theories in different dimensions in most unexpected ways." is based in field theory, has little to nothing to do with string theory. And yet, Gaiotto has been a speaker at Strings 2008, Strings 2009, Strings 2010, Strings 2011 and Strings 2012. I haven't checked, but it's possible there's no one else close to him except maybe Witten in terms of having this kind of dominant presence in recent years at the main Strings conference.

Komargodski spoke this year at Strings 2012, Beisert at Strings 2007 and Strings 2011. All of them have strong connections to the IAS, with Komargodski a current Member there, Gaiotto one until recently, and Beisert at Princeton from 2004-2007. Trying to claim that this list of three young people is not a list of people who have been working in what is referred to as "string theory" these days is absurd. As a list of the best young theorists in the world, it's incredibly narrow in scope, and incredibly IAS-centric. Does this have anything to do with so much of the panel that made these decisions being IAS string theorists?

As for whether the fact that certain topics in condensed matter theory are the latest trend in string theory these days has anything to do with a panel of string theorists deciding that out of six awards, the only one not going to string theorists should go to one of those topics, people can make up their own mind (and decide for themselves who is "schizo"). About the accusation that I'm claiming the condensed matter work has "direct connection to string theory", for the second time, please read what I actually wrote.

32. **Bernhard**
December 11, 2012

"I dont think the prize is supposed to be a consolation prize, it is supposed to *supersede* the Nobel prize."

Well, this is what this prize is, a consolation prize for high profile people without a Nobel.

Supersede the Nobel, give me a break…

33. **Bob Jones**
December 11, 2012

"But if only deserving string theorists get the prize as opposed to deserving theorists no matter their field, than one might as well change the name of the prize to the 'Stringy Prize'."

Yeah, but the prize has already gone to theorists who don't work on string theory per se, so I don't know why you're worried about that. You also have to realize that string theory is a well established part of physical mathematics, so it's inevitable that many of the people who have made important contributions to fundamental physics have also done work in string theory.

34. **Bob Jones**
December 11, 2012

"Gaiotto is an excellent example. I agree that his very interesting work, which is cited as 'far-reaching new insights about duality, gauge theory, and geometry, and especially for his work linking theories in different dimensions in most unexpected ways.' is based in field theory, has little to nothing to do with string theory."

Of course it's related to string theory! The reason those dualities and connections to geometry exist is that the theories Gaiotto is studying come from a consistent six-dimensional supersymmetric theory which is equivalent to M-theory!

35. **Peter Woit**
December 11, 2012

Bob Jones,

Maybe you and "layman" can fight out the Gaiotto issue and let me know who wins.

36. **Bernhard**
December 11, 2012

Bob Jones,

Strictly speaking nobody really got this prize yet. In the the first year the contenders were chosen by Milner and one can argue that this is more like choosing a committee, since Milner is himself nobody in position to decide anything about theoretical physics. It´s the coming years that are important in what concerns this prize, so let´s see what happens from now on.

I will try to keep in mind that "many of the people who have made important contributions to fundamental physics have also done work in string theory" even though in the case of Polchinski and Polyakov this kind of coincidence is harder to swallow.

37. **youngtheorist**
December 11, 2012

It looks like the new horizons prizes are for theorists under 35.
The euro phys society has a prize every two years for exactly this age range. The last three winners were Gaiotto, Cachazo and Beisert. (Cachazo is now probably over 35, also was at IAS for many years). So two of the three winners were given young theorist prizes by an international group. Komargodski's work on the a theorem is a major development, makes perfect sense to recognize it. I don't see an IAS conspiracy here.

38. **Bob Jones**

December 11, 2012

Peter,

If layman is saying that Gaiotto's work is related to string theory, then I can certainly pursue the discussion with him or her. But I'm more interested in your thoughts about about this prize. What difference does it make if it is mostly awarded to people who work on string theory? You apparently find their work very interesting, and we have every reason to believe that the prize will continue to have high standards.

Bernhard,

The fact that Polyakov and Polchinski work on string theory and made important contributions to fundamental physics is not a "coincidence". These topics are not mutually exclusive…

39. **Bob Jones**
December 11, 2012

Oops, I meant to say, "if layman is saying that Gaiotto's work is NOT related to string theory…"

40. **Peter Woit**
December 11, 2012

Bob Jones,

I just think it's a very unhealthy situation to have theoretical and mathematical physics so heavily influenced by the idea that only things with some connection to an overhyped, failed idea about physics are worth pursuing. The large sums of money being thrown at this and the very narrow choices being made about what sort of young researchers deserve recognition aren't helpful at all.

41. **Allan Rosenberg**
December 11, 2012

Experimentalists and condensed matter theorists? Do they even count as physicists? Next year it will probably be lumberjacks, used car salesmen, and CERN's lawyers.

42. **layman**
December 11, 2012

"Maybe you and "layman" can fight out the Gaiotto issue and let me know who wins."

This is ridiculous, instead of looking for interesting science to write or think about, Peter wants to see blood. A gossip blog of the best kind.

Anyway, the work of Davide and Zohar has addressed issues that existed even before string theory was popular, and they have nothing to do with string theory. The work of Davide led to some insights about conformal symmetry in 2d (which as I mentioned is ubiquitous in many branches of physics and mathematics), especially the structure of conformal blocks. The work of Zohar address questions about RG flows that existed in principle since the time Wilson wrote his seminar papers, much before string theory, and completely unrelated to it.

43. **piscator**
December 12, 2012

This is all terribly depressing – everything that was said originally about the narrow IAS-centric scope of the original Milner prize awards has turned out to be true. The whole thing smells cheap and sleazy.

Arguments about whether or not the work is string theory or nor miss the point. The real issue is that you have a big money prize where a precondition for receiving the prize is that you are an approved favourite son of one of three of four people. It distorts the subject, it doesn't look remotely independent, and it stinks of cronyism.

44. **M**
December 12, 2012

dear P., I know what a spokeperson is (somebody good both in experiment and in politics) and I also know who did the real work (as I am a theorist experienced in collecting rumors).
The discovery of the Higgs was a huge collaborative work with individual contributions below the 1% level. I think it is a real mistake to give awards based on visibility when joung bright experimentalists cannot get the recognition they deserve and somebody abandons disliking internal burocracy and Fordian workflow.

45. **Peter Woit**
December 12, 2012

layman,

You definitely lack a sense of humor. No one here is calling for blood. I am however making fun of your claim that the work of a speaker at Strings 2008, Strings 2009, Strings 2010, Strings 2011 and Strings 2012 has nothing to do with string theory. Not that I disagree with this claim, just pointing out it's a schizo world we live in…

46. **Bee**
December 12, 2012

Ah, sorry, I just simply misread what Peter wrote. But, eh, treat it as a prediction 😉

47. **P**
December 12, 2012

M, I think we are in agreement, after all.

Piscator, I think you're mistaking correlation for causation. Gaiotto and Komargodski do have IAS affiliations and certainly are some of the "favorite sons" of some of the famous older theorists there. But it is pretty much universally accepted in top theory groups that their works are seminal contributions in quantum field theory, though sometimes motivated by strings (As Bob Jones pointed out, the (2,0) d=6 theory Gaiotto uses is believed to be closely related to the worldvolume theory of an M5-brane and leads to many beautiful dualities in d=2,3,4), and as such many would think they deserve their prizes.

48. **John Preskill**
December 12, 2012

All of the awardees are outstanding scientists with wonderful achievements, and I'm glad they are getting this recognition. More comments here: http://quantumfrontiers.com/2012/12/11/fundamental-physics-prize-prediction-polyakov/

49. **Heidar**
December 12, 2012

For those who are interested, John Preskill has written about the awards here http://quantumfrontiers.com/2012/12/11/fundamental-physics-prize-prediction-polyakov/ (see also http://quantumfrontiers.com/2012/12/10/stephen-hawking-wins-3m-milner-prize/). He predicts Polyakov will win the full prize, I cannot agree more. His extremely deep contributions to theoretical physics is matched by very few others, even if one neglects all his work on string theory.

50. **Heidar**
December 12, 2012

Wow, that was spooky.

51. **Peter Woit**
December 12, 2012

John,
Thanks for that link!
Heidar,
Yes, even spookier if you were sitting at my computer working and those two comments popped up on the screen within seconds of each other…

52. **piscator**
December 12, 2012

P: with respect, I think this is a loss of perspective. Compared to the stated remit of the prize – `our knowledge of the Universe at the deepest level' – all three prizes cluster in a very narrow area – formal properties of formal quantum field theories, with limited application to any actual physical problem. Furthermore, in terms of area, each prize also maps closely onto one identifiable IAS theorist.

I'm not saying these aren't good people – but these are all people doing one very particular flavour of theoretical physics, and this flavour is branded IAS particle theory.

53. **GB**
December 12, 2012

Is it true that the award intended for contributions to topological insulator only includes those three physicists ? I am thinking Zahid Hasan's pretty pissed off now…

54. **srp**
December 12, 2012

Is Milner really Eris, the goddess of discord, in disguise? Prize=golden apple. Only a few more plot points to go and we'll have thousands of ships being launched on Lake Geneva and a dramatic siege of CERN.

Spoiler alert: DON"T BRING THE GIANT WOODEN MODEL OF A QUARK-GLUON PLASMA INTO THE ACCELERATOR BUILDING!

55. **fuzzy**
December 13, 2012

i don't like this, i want my revenge. so, i will create a new prize, awarded for the most silly prize in physics. nobel's gone, milner wins–but he would have won anyways. prosit!

56. **tulpoeid**
December 13, 2012

Slightly off-topic, but…
Yawn. Given that we talk about September '12, which Higgs discovery, exactly? For how longer will bloggers and journalists make the nobel committee pay for continuing to be scientific when faced with an extraordinairy chance to stop doing so?

New Higgs Results Tomorrow?

December 12, 2012
Categories: Experimental HEP News

As part of the CERN Council activities this week, there will be a session held with a live webcast tomorrow on Status of the LHC and Experiments. I'm hearing that there will be news about the Higgs from ATLAS: new results for the high statistics gamma-gamma and ZZ channels. These were expected for HCP2012 last month but not ready then.

If you can't watch the CERN talks, the KITP tomorrow at 11:15 am has scheduled a talk on "New (!!) ATLAS Diphoton and ZZ Results".

Update: This promises to be quite interesting. ATLAS is seeing a 3 sigma difference between the Higgs mass seen in the gamma-gamma channel and in the ZZ channel. They've been trying hard to check all possible systematic effects that could explain this, but it won't go away, so they've decided to go ahead and report the results tomorrow. Probably nothing, but if CMS is seeing anything similar and it is still there in their analysis of the rest of this year's data, that would be huge. I don't know of any sensible model that would lead to a real effect like this, but who knows…

Update: The new ATLAS results don't seem to be available online anywhere, but Jester and Matt Strassler report the details from today's webcast. ATLAS has the Higgs at 126.6 GeV in gamma-gamma, at 123.5 in ZZ, difference is 2.7 sigma. The ZZ signal strength is right in line with SM predictions, the gamma-gamma signal strength is about 2 sigma high. Odd parity or spin two strongly disfavored.

Nothing news about this from CMS. What they have released doesn't at all confirm the ATLAS mass difference, with them seeing the ZZ peak at 126.2 +/- .6 GeV, gamma gamma around 125. So their masses are compatible and in the middle between the two extreme ATLAS values.

All in all, still looking like a garden-variety SM Higgs. Next update with lots more data likely to be in March at Moriond.

Comments

1. **David Nataf**
   December 12, 2012

   Peter,

   Can you tell me if this scenario is remotely plausible:

   1) Astrophysicists are seeing evidence of a dark matter particle at 130 GeV, which is close to 125 GeV.
   2) It's very rarely produced in the LHC, it hasn't been explicitly caught because there's no search algorithm that would find it.
   3) When it is created, it decays into photons.
   4) 130 GeV is close to 125 Gev, and the resolution is not sharp, so it looks like a single bump at 127 GeV with higher-than-expected normalization.

2. **Peter Woit**
   December 12, 2012

   David,

   You need someone more expert on dark matter than me to address that. If it is sensible there should be half a dozen preprints on the arXiv about it tomorrow night…

3. **anon**
   December 12, 2012

   @David

   if the 130 GeV particle decays to two photons, it can not be dark matter, since the latter must be stable on cosmological timescales.

4. **Marc**
   December 12, 2012

   Peter—It was rumored last month that ATLAS is seeing the ZZ signal at about 123 and the gamma gamma at 126. But CMS clearly shows ZZ at 126, with over 4 sigma. So they can't agree. Still, wouldn't it be fun if CMS sees gamma gamma at 123 🙂

5. **Z**
   December 12, 2012

   The 130-135 GeV Fermi line is still controversial, and possibly spurious since it's also seen in the Earth limb data. cf
   http://fermi.gsfc.nasa.gov/science/mtgs/symposia/2012/program/fri/AAlbert.pdf

6. **Dan D.**
   December 12, 2012

   I'm no particle physicist, but from what I understand, the putative DM particle at 130 (actually 135 based on updated official Fermi analysis) annihilates to two photons, not decays. The particle itself is (presumably) stable.

7. **Bob**
   December 12, 2012

   Peter, are you saying the cross-section is larger by 3sigma, or that the best fit higgs values are different in the 2channels? What are the central values of each channel? It seems crazy to have bumps at different energies in different channels. Something seems fishy…

8. **Peter Woit**
   December 12, 2012

   Bob,

The best fit mass values differ by 3 sigma in the two channels, and the numbers Marc mentions above are about right.

Not as hard to believe as superluminal neutrinos, but still seems almost certain to be something that will go away.

9. **vortex1**
   December 12, 2012

   Could they be seeing different higgs in each channel? multiple higgs?

10. **chris**
    December 13, 2012

    FTL neutrinos reloaded? i hope they go "public" with less hype surrounding it.

11. **nabil**
    December 13, 2012

    What are the models that might produce something like this?

12. **Marc**
    December 13, 2012

    Nabil—you could horrendously fine-tune a two-Higgs doublet model (with some additional particles, perhaps) to explain a gam gam peak at 126 and a ZZ peak at 123. However, CMS sees ZZ at 126, so the experiments themselves are contradictory (if you take the < 3 sigma effect seriously). Of course, if the Higgs mass itself gradually changes as one gets closer to Paris……

13. **Bob**
    December 13, 2012

    Yes marc, your idea that the higgs eats crossaints as it get closer to paris is currently the leading explanation

14. **Jon Lennox**
    December 14, 2012

    Marc: sounds like Henri IV was more correct than we knew.

Arkani-Hamed on Naturalness

December 13, 2012
Categories: Uncategorized

For the latest SUSY enthusiast take on the implications of what the LHC has been (not) seeing, your best bet might be yesterday's talk at the KITP by Nima Arkani-Hamed on Naturalness. An hour and 40 minutes, no slides, nothing much on the blackboard, just him talking about how he now sees things. Some high points:

- If the Higgs turns out to have spin two, he'll quit physics.
- If the Higgs turns out to be a techni-dilaton, he'll kill himself.
- At this point, a natural theory would have to be rather baroque, so he favors abandoning naturalness in favor of simplicity.
- The simplest thing is the Standard Model, but that requires too much fine-tuning. He won't completely abandon naturalness: one part in a million fine-tuning is fine, but the SM fine-tuning problem isn't. This is the point where he loses me (going from the SM to the vastly more complicated SUSY theories with the needed SUSY breaking seems to me not close to being worth the supposed improvement in the fine-tuning).
- He complains that "Some BSM theorists are giving our field a bad name" by repeatedly making SUSY predictions that turn out to be wrong and changing their story.
- He's not one of those: he still favors split SUSY, and has since 2004.
- Split SUSY makes a falsifiable prediction: no Higgs gamma-gamma excess. This is of course the same prediction as the Standard Model.
- In his favored version of split SUSY, all SUSY partners are much too heavy to ever be observable except the wino, bino and gluino. He had a lot to say about what observing these would tell us, but not much about what the implications are of not seeing them in the LHC 8 TeV run. Would this just mean "surely they'll show up at 13 TeV"? Is seeing nothing at 8 TeV consistent with split SUSY? What about seeing nothing at 13 TeV?

In any case, giving up on SUSY is definitely not on the agenda as far as he's concerned.

Comments

1. **BrianW**
   December 13, 2012

   Hmm, I thought much of the talk was merely politics. I'm not sure he really believes the old susy stuff anymore. At the end, he does consider the possibility that all these favoured susy ideas are ruled out for good, which sounds to me like he's betting on a new top down twistor picture, but of course not willing to say so.

2. **lun**
   December 13, 2012

   :If the Higgs turns out to be a techni-dilaton, he'll kill himself.

   ???????????????
   Unlike _any_ scalar field theory, "technicolor" (Asymptotically free theories with a Gauge group) is known rigorously to be non-trivial and stable (in a Renormalization Group sense). What exactly is the problem?

3. **Hmunu**
   December 13, 2012

   Are these long rambling talks the norm for theoretical particle physics or is this specific to Arkani-Hamed? You have posted links to a few talks of his here and each time, I am amazed at the sophomoric quality of the presentation. I realize that the primary focus of a particle theorist isn't to give PowerPoint presentations that would make a marketing firm jealous, but I feel like this talk suggests a lack of seriousness about communicating one's ideas to the unconverted.

4. **MathPhys**
   December 14, 2012

   I think it was Feynman who said that one doesn't need more than one argument if one has a single good argument.

   There is an element of using the audience as sounding boards in these talks. On the other hand, I'm sure people at KITP and elsewhere are more than happy to help him articulate his thoughts, no matter how long that takes.

5. **Mark**
   December 14, 2012

   Hi Peter,

   I wonder what you think people should work on if they give up on SUSY? What I wonder is if there is anything else that already hints at another way forward for BSM@TeV type scales (not including the usual things people work on like large extra dimensions, ZPrimes and so on – all of which of course have also not been observed so far) that we can look for at the LHC, or if SUSY is not there we really need some idea that has not been thought of at all (given most of the mainstream ideas I know of are also heavily constrained by LHC searches).

6. **not a fun of big names**
   December 14, 2012

   All Nima's models are wrong, how dare he blames the rest of the BSM community??? Enlarging (the energy level) of new particles/predictions is the way of doing physics (of course one reason for this is we need to ask funding from government), think about the time before people discovered top quark. By the way, 10^-6 is UN-Naturalness, everyone knows that!

7. **chris**
   December 14, 2012

   "a natural theory would have to be rather baroque"

   i wonder how many people can truly appreciate the subtle irony of this sentence.

8. **JR**

December 14, 2012

Hmunu, Good questions. NAH seems to be one of few people who can ramble and still remain riveting! As for power point, I have never seen him use that, he makes nice carton drawings with a real actual felt pen! Makes me feel like I am back in 1985!. Somewhat refreshing really. Regarding talks with no prepared material, this is mostly the domain of the rich and famous, not recommended for job talk! Also it is not reserved for particle physics, Bob Laughlin used to give wonderful talks on condensed matter theory. He might scribble something like H*Psi=E*Psi on the board but that was it!

JR

9. **Tienzen (Jeh-Tween) Gong**
December 14, 2012

Thanks for the link.
I am a linguist. Thus, I will not comment on the physics. But, I can decode messages. Seemingly, Nima tried to convey two points in his 100 minutes long talk.
a. He just delivered a great eulogy for his pet SUSY.
b. He is making an announcement for joining the Technicolor camp.

10. **Eric**
December 14, 2012

Regarding naturalness, how do we know that supersymmetry doesn't naturally solve a different hierarchy problem in a hidden sector, and electroweak symmetry breaking is simply a secondary effect ? From this perspective, there would be no naturalness problem as the electroweak scale would be only weakly correlated with the mass of the superpartners. However, there would be a different scale, perhaps associated with dark matter, which is strongly correlated with supersymmetry. Presumably this other scale would then somehow trigger EWSB. It really make no sense to be overly concerned with naturalness until we have a complete picture of all physics rather than just the small SM sector which makes up only 3% of the universe.

11. **tt**
December 15, 2012

a different problem in a hidden sector ?
so its a solution looking for a problem ?

12. **Eric**
December 15, 2012

tt,

No, that is not what I am saying. Supersymmetry still provides the explanation for why the Higgs mass is light and thus solves the hierarchy problem. My point is that the scale of the superpartners does not have to necessarily be strongly correlated with the electroweak scale as there could be lots of other physics happening that is not apparent at the present time. So what may appear to require a small amount of fine-tuning, the so-called little hierarchy problem, may appear completely natural when the full picture is in view.

13. **Bernhard**
December 15, 2012

Eric,

"Lots of other physics" (whatever that is) could also show SUSY is unnecessary and by itself solve all problems, including the hierarchy problem, wouldn't you agree? Not that I want to discuss about nothing, but if the game is to claim some vague undiscovered physics to rescue SUSY one my well take a shortcut and use it to solve whatever problems the Standard Model might have.

14. **Bernhard**
December 15, 2012

sorry, *might well take*

15. **Hmunu**
December 15, 2012

@ **JR** – As a graduate student born in the mid 1980s, talks with transparencies conjure up professors giving the same physics lectures that they gave when first granted tenure in the 70s. And I have never found a rambling technical talk riveting, even in my own sub-field, because all I end up thinking is "get to the point!" I wasn't saying that this issue is exclusive to particle theorists, but I am always amazed that high profile people in this field just fly by the seat of their pants when giving a talk.

–

I sort of understand the appeal of nostalgia, but I must wonder if Professor Arkani-Hamed would find this type of talk acceptable by one of his graduate students. If the answer is yes, then that explains many other theory talks I have read and witnessed that lack a coherent structure. They just have been taught that this presentation style is right way to give a talk. That is pathetic and sad.

–

@ **MathPhys** – While presentations during workshops will naturally have less structure then an APS plenary, usually they aren't this free-form. Trying to start a discussion doesn't mean you don't prepare slides to keep the discussion on track. If someone needs assistance to work out the details of a model, do it with colleagues while relaxing on the beach or in the local pub. Maybe I just don't like theory talks that amount to "My Thoughts About the Universe."

16. **Peter Woit**
December 15, 2012

Hmunu,

This was a weird and unusual format for a talk. Then again, he's in a pretty weird and unusual situation right now: he just got a $3 million check for ideas that don't work and that are in the middle of getting clobbered by experiment. So, he's got some explaining to do…

17. **Tienzen (Jeh-Tween) Gong**

December 16, 2012

@Hmunu,
American President seldom use slides for his policy arguments. There is no need of using slides for some important announcement. Nima's Susy was well-known, and he was not trying to promote it anymore. His points were very clear, no slide is needed.
a. Although we can arbitrary push the susy scale to 1000 Tev or higher, it (susy) always has a "tail" trapped at or below one Tev region. That is, no tail at LHC, no susy.
b. If the LHC data shows b or c (which are now the hints in the data), then not only is Nima's pet a goner but also are all its variants.
c. Seemingly, Nima did not like Technicolor before. But, in this talk, he mentioned that the techni-type solutions rescued physics three times in the past. He also admitted that Technicolor "should" be correct although … . He mentioned Technicolor almost 10 times in this talk.

Nima is the first one openly announcing the beginning of the end of a long susy era (over 40 years long). I congratulate him.

18. **rrtucci**
December 16, 2012

This reminds me of the film "The Caine Mutiny"
http://qbnets.wordpress.com/2012/12/15/the-cern-mutiny/

19. **Jim Akerlund**
December 16, 2012

I just saw the video and at the 88 minute mark a women in the audience is reading "Not Even Wrong" on her computer. The camera operator seems to be trying to show this by the long screen time spent with her computer centered. I recognized your page by the "First Results from the Large Hardon Collider" picture she has on the computer.

20. **Chris Oakley**
December 16, 2012

Yes – I am sure that if they had spelled "hadron" correctly, it would not have attracted all this prurient interest.

21. **Peter Woit**
December 17, 2012

Jim,

Yes, took a look and that's definitely NEW she's paying attention to, not Nima. Very funny…

22. **plm**
December 22, 2012

Jim and Peter:
It would have been funny if you had put an embedded link to the video and the woman had been seeing herself on NEW seeing herself on NEW seeing herself on NEW… in real time. She could have eventually discerned "Not Even Wrong" written with composite Higgs in a tiny video.

Quantum Mechanics Fall Class Lecture Notes

December 14, 2012
Categories: Uncategorized

Classes are over for the semester, and I've put together the lecture notes for my undergraduate "Quantum Mechanics for Mathematicians" course, which are available here.

The idea for the course was to try and explain the basics of quantum mechanics, from the point of view of unitary representations of Lie groups. While this is a rather advanced topic, I made an effort to do things quite concretely and start at the most basic level (the only prerequisite for the course was calculus and linear algebra). I hope the notes will be useful both to mathematicians trying to learn something about quantum mechanics as well as to physicists who would like to better understand the mathematics behind the way symmetry principles get used in the subject.

More to come next semester. The initial plan is to start with the fermionic oscillator, move on to path integrals, then relativity, the Dirac equation, and U(1) gauge theory (E and M), ending up with some very basic quantum field theory (non-interacting fields). We'll see how that turns out and at what point I run out of energy and stop writing.

Any corrections, comments or suggestions about how to improve these notes are most welcome.

Update: Thanks to all for comments, I'm quite pleased to see how many people have been looking at these notes (6600 downloads and counting!). They've also made an appearance in surprising places, including here.

Comments

1. **mario fuenzalida**
   December 14, 2012

   Hermano, esto es justo lo que buscaba, soy ingeniero de 62 años al borde del retirement y fisico frustrado llegue hasta la path integral y deseo comprender mejor la mecanica cuantica, gracias, un abrazo desde calama, chile

2. **Peter Woit**
   December 14, 2012

   de nada!

3. **Zoltán Suhajda**
   December 14, 2012

   Dear Peter,

   I have really enjoyed the quick review of the Lecture Notes above. I was looking for such an accurately aimed document about QM from the aspect suitable for veterans of informatics. I do not yet plan to drop from the computing systems and applied informatics, and I feel a need to catch up for quantum computing, which I think is the only fundamental advance in the field of computing for a long time ago.

   So thank you for posting this, and I know I will be really happy when your second semester Lecture Notes will be available.

   Please continue with remarks to applied QM like quantum computing as you did in the current document.

   Best regards,
   SuhesZ from Subotica, Serbia

4. **Yatima**
   December 14, 2012

   quantum computing, which I think is the only fundamental advance in the field of computing for a long time

   Really! This is clearly off-topic but one might as well complain that fundamental physics hasn't seen much progress because there are still only three colors for the strong charge.

   Even disregarding the enormous practical advances and explosion of engineering approaches, principles and philosophies that the field has seen in the last 40 years, and considering only advances in theoretical computer science and complexity theory, it certainly looks to me that there were serious advances indeed.

   As for quantum computing, one could do worse than check out the complexity class BQP, then become a reader of Scott Aaronson's blog, where many surprises await. In particular, his attempt at a timeline of computer science, which probably needs review.

   And does anyone know what Schoonship is?

5. **Peter Woit**
   December 14, 2012

   I fear those looking to me for a text on quantum computing will be sorely disappointed…

6. **M Uppal**
   December 14, 2012

   Dr. Woit, will you be teaching this class again next year (for the Fall 2013 – Spring 2014 terms)?

7. **Zoltán Suhajda**
   December 15, 2012

   Dear Peter,

   do not burden yourself, your current work is enough to correctly recognize the place of the "qubit black-box" within the QM. For the text on quantum computing someone would look elsewhere. Your notes are as good for students of informatics as you have it aimed for mathematicians.

Of course I have found and now following this Web site driven by my skepticism toward the string theory and SUSY (it would be too long to explain why I'm involved in this) not by my before expressed efforts to understand quantum computing.

8. **Blablon**
   December 15, 2012

   Dear Peter,

   On page 9, there is strangely truncated sentence

   > "In this very special case, all the entries of the matrix will be 0 or 1, but that is special to the permuta"

   So something like "tion groups" is missing.

9. **a reader**
   December 15, 2012

   Just to say: thank you. I downloaded it and I much look forward to finding time to read it.

10. **david**
    December 15, 2012

    Another thanks. Section 12 is brilliant.

11. **Aaron Sheldon**
    December 15, 2012

    Thank you,

    Looking forward to going through this over the holidays.

    I have one small quibble with the last part of 1.3: for the equivalence between isometric linear operators (preserving the the inner product) and unitary linear operators, the vector space must be strictly finitely dimensional. In infinite dimensional vector spaces isometric linear maps are not necessarily invertible linear maps.

    For example consider the space of square integrable functions on the positive real line (modulo measure zero functions) ; the positive translation operator is a linear isometry, yet is not invertible.

    While seemingly arcane, such considerations could be important when considering translations of a wave function along a geodesic that intersect with a boundary or singularity.

12. **Peter Woit**
    December 15, 2012

    M Uppal,

    Probably will teach something else next year, but there's no definite plan for 2013-4 classes yet.

    Blablon,

    Thanks! fixed.

13. **Peter Woit**
    December 15, 2012

    Aaron,

    Thanks. I should perhaps make it more clear that for the first half or so of the class, the state spaces under consideration are all finite-dimensional.

    And, when infinite dimensional spaces do come into the game, there should be a huge warning label on the text that I'm no analyst and the standard being aimed for is not precise statements, but statements that are morally true and could be made precise without a lot of effort (unless it is made clear that the statement is highly non-trivial and requires serious work to prove).

14. **David Nataf**
    December 15, 2012

    How did you manage to compose 180 pages of rigorous material in just a few months?

    What make of coffee do you drink and where can I get some?

15. **Friend**
    December 15, 2012

    Dear Peter,

    I'm curious to know how many complaints you get because students think QM is illogical or not well founded? What's would you guess the drop out rate is? Are students stunned by the complexity without reason? Or has everyone pretty much accepted that this is the way it is? How needed is a better conceptual foundation for QM? Thanks.

16. **Matt Grayson**
    December 16, 2012

    I sat in on the first few lectures of Peter's (excellent) course, but then appeared to drop out. I took a similar course 30 years ago and completely failed to understand it. Once Peter explained the connection of representations to QM, I was satisfied that I now "get it", and continued to follow from his online notes and Shankar's book.

Peter, thank you for clearing up a decades-old confusion!

17. **Michael Thaddeus**
December 16, 2012

Hi Peter — in section 2.1, you should either require your representations to be unitary, or your group to be compact, or define "irreducible" to mean simply that there is no proper subrepresentation. The 2-dimensional representation of the additive group C given by t |–>
[1 t]
[0 1]
is irreducible in the sense you define but clearly violates Schur's lemma.

18. **Peter Woit**
December 16, 2012

Thanks Michael,

I was trying to avoid getting into the indecomposable vs. irreducible business, but you're right, I need to say something there.

The only example later on where this turns up is when I mention the 3d Heisenberg group (thought of as upper triangular matrices) action on 3d vectors, and that it's not unitary so not relevant in QM.

19. **Prof. David Edwards**
December 17, 2012

Superposition is a more complicated concept than most physicists realize. If f & g are wave functions, then g*e^(i*c) represents the same state as g; but f+g*e^(i*c) defines a one-parameter family of different states! This obvious truism-which physicists all know-is conveniently forgotten when they try to explain Q.T. Even Feynman's axioms for his path theoretic approach ignores this point. Just like one should start from pseudo-Riemannian geometry in systematically developing General Relativity, one should start from von Neumann's quantum logics in systematically developing Quantum Theory.

20. **Anant**
December 17, 2012

Would it be possible for someone in his 2nd year course (of Bach. in Physics) to follow these lectures?

21. **Peter Woit**
December 17, 2012

Anant,

I've tried to write the notes so that only linear algebra and calculus are required, no physics beyond the most elementary. Physics students may find the mathematical abstraction hard to follow, especially for the first part of the notes, where I'm trying to explain the crucial concept of a representation of a Lie group.

22. **NewComer**
December 18, 2012

Dear Peter,

Being a mathematician with (ugh..) not very good physical background, I truly enjoyed when I took a quick look at these lecture notes.

My suggestion or to say more correctly my wish is that you include an explanation of spin manifold from the physical point of view if you find it relevant, as somehow I can't find it in the literature.

23. **Peter Woit**
December 18, 2012

Newcomer,

These notes are all in Euclidean or Minkowski space, not trying to do things on manifolds, so spin manifolds don't come into it.

However, there is a great deal in the notes about the relation of Spin(3)=SU(2) and SO(3), and the necessity of using Spin(3) rather than SO(3) to describe one of the most fundamental things we know about: spin-half particles.

24. **Zathras**
December 18, 2012

These are really excellent notes. Any inclination towards publishing them?

25. **Peter Woit**
December 18, 2012

Thanks Zathras,

Maybe someday, but I'd want to make sure there will always be a version available on the web. For now, I'll see how much more material I can write over the next semester, then see what this looks like after that.

In an ideal world, at some point while teaching the class again I'd go through the whole thing and rewrite it, based on what I learned by doing this the first time. Seems likely to me though that I'd never have the energy for that.

I've almost finished going through notes one last time for typos, inconsistencies, etc. There's a short list of things I'd like to add, sometime soon I'll stop making changes on those notes and concentrate on thinking about next semester.

26. **Zathras**
December 18, 2012

So how did the undergrads do in your opinion? Were they able to follow the material satisfactorily?

27. **Peter Woit**
December 18, 2012

Zathras,

It was a small class, so I don't think I have any idea how this would work with a large group of typical students. I think they did well and learned a lot, it's quite challenging material. Especially difficult was the way they course started off, throwing them into very unfamiliar material: Lie groups, algebras and representations. It's the kind of thing you can only really start to understand once you've seen the crucial examples, so very confusing at first. Maybe some more effort should have been put into motivating this at the beginning.

28. **srp**
December 18, 2012

Would there be any complementarity with Fulton and Harris's (1991) Representation Theory: A First Course? I found a used copy and it seems very example-oriented.

29. **Peter Woit**
December 19, 2012

srp,

Fulton and Harris is also about Lie groups and representations, but mostly covers different material. They're aiming for the general case of semi-simple Lie groups and their representations (for example, the group SU(n) for arbitrary n). I'm sticking to the specific low dimensional Lie groups that are basic for physics: U(1), SU(2), SO(3), and the Heisenberg group. Some of this overlaps with Fulton and Harris, especially the first half of what I was doing, but the second half is quite different than what they do.

30. **Michel**
December 19, 2012

Thank you for uploading your material. Would it work as a preliminary reading to Hatfield's Quantum Field Theory of Point Particle and Strings?

31. **Peter Woit**
December 19, 2012

Michel,

That's really a QFT and string theory book, you certainly should have a good understanding of quantum mechanics itself before studying those subjects. I think my notes are best used in conjunction with a standard QM book, they emphasize aspects of QM that aren't emphasized in the standard treatments, don't go into much detail on issues that are well covered in the usual textbooks.

32. **mo**
December 19, 2012

Dear Peter,

When trying to see where your lecture notes fit in the general scheme of things, I noticed something.

On the very first page, you write upfront: "In quantum mechanics, the state of a system is best thought of as a different sort of mathematical object: a vector in a complex vector space." No other formulations of quantum mechanics are mentioned. For an inventory of various formulations of quantum mechanics, see e.g. Daniel F. Styer et al., Nine formulations of quantum mechanics, Am. J. Phys. 70, p. 288-297, March 2002.

Apparently all states you deal with are pure ones, but it is not explicitly stated, mixed states and density matrices are not mentioned.

One of the best expositions of quantum mechanics for mathematicians that naturally comes to mind is Mackey's Mathematical Foundations of Quantum Mechanics, which is missing from the bibliography. Mackey offers a very general axiomatic definition of quantum states that encompasses both mixed and pure states which seems to be a better way to proceed when lecturing to mathematicians.

33. **Peter Woit**
December 19, 2012

mo,

Thanks, that's an excellent comment. I should probably add this somewhere in the notes to make clear what I'm trying to do, but the decision to not discuss mixed states and density matrices was intentional. Those are important if you want to construct a framework which includes both classical mechanics and quantum mechanics, and/or understand measurement theory and see how classical states emerge as certain mixed states.

My point of view is that such material makes things much more complicated and doesn't belong in the foundations of the subject, but should be put off until one addresses the difficult issue of measurement theory and the relation to classical mechanics. Bringing these issues in at the beginning to my mind obscures what are the fundamental mathematical structures involved.

34. **David Metzler**
December 22, 2012

Peter, do you have a suggestion for a text that covers the fundamentals of QFT from a point of view that would be pleasant for a mathematician? Essentially something that would come right after what you're planning for the next semester. I have forgotten most of what I learned in haphazard fashion 20 years ago. I would want something more mathematically oriented than most of the standard physics texts, but not so abstract and high-level as the IAS QFT/String 2-volume set.

35. **Peter Woit**
December 22, 2012

David,

Of the things I've looked at, Folland's "QFT: A tourist guide for mathematicians" is the best I've seen along these lines. I'll try and cover some of the basics next semester, which would be a good background for reading Folland.

Folland doesn't get to many of the things that would most interest mathematicians: non-abelian gauge field theories, 2-d conformal field theories and affine Lie algebras, and TQFTs. Some of those topics are in the IAS volumes.

36. **David Metzler**
December 23, 2012

Thanks Peter, that looks like exactly what I'm after, and I know Folland usually writes well. I'll take a look at it.

37. **Anonyrat**
December 26, 2012

Page 43, the argument that the adjoint representation is a real representation could possibly be made clearer (unless it is in the exercises, which I haven't yet looked at).

38. **Peter Woit**
December 26, 2012

Anonyrat,

Thanks, that is a confusing point. I did go over it in more detail in class (perhaps confusing the students even more). I'll think about what can be done to improve that.

There are actually two confusing things going on here: the first is that, even when dealing with a group defined by complex matrices (like SU(2)), the Lie algebra I'm talking about is a real vector space (real linear combinations of i times Pauli matrices). All complex linear combinations of Pauli matrices is the Lie algebra of SL(2,C), not SU(2). The second confusing thing is that to get raising and lowering operators, you want to complexify and go to the Lie algebra of SL(2,C) anyway.

Various Links

December 19, 2012
Categories: Uncategorized

- This week there's a conference in Oxford I'd have loved to have been at. Slides from some of the talks are already posted here. The conference is in honor of Graeme Segal's 70th birthday. Happy Birthday Graeme!
- Physics Today has a very interesting piece about the current state of HEP posted today by Burton Richter, focused on the topic of Should the US join CERN?. On the ILC, with Japan the prospective location, he takes the point of view that it's most likely to be interesting as a Higgs factory, so a 250 GeV machine will suffice:

  > The ongoing International Linear Collider (ILC) program is aimed at building and running a 500-GeV machine by 2020. A new ILC design study is scheduled for release in a few months, but by 2020 the LHC should have delivered enough cumulative output to make anything the ILC can produce irrelevant beyond what its lower-energy Higgs-factory option can do.

  Besides this, at the energy frontier the LHC is the only game in town, with HL-LHC and HE-LHC challenging and expensive projects that will dominate the future of the subject. If the US wants to participate, Richter argues that a new, closer formal relationship is needed. The politics here is likely to be tricky, with the US Congress not exactly keen on spending money outside the US, through an organization where the US has little influence.

  About the future he's most worried about the too high cost of getting to higher energy permanently delivering us into the hands of multiverse mania:

  > If our only theory of everything comes down to the landscape model, where we are only one of a zillion universes with the parameters we see as only a statistical accident necessary for life, the game is over. I hope not.

- One of the landscapeologists whose influence Richter is worried about is Joe Polchinski at Santa Barbara. Courtesy of the Milner prize competition, Polchinski is in line for about $3 million more influence if he beats out his two competitors next March, and UCSB has a press release about this. The press release explains that Polchinski is being rewarded for his discovery of "one of the basic building blocks of space time"

  > According to the award citation, the Physics Frontier Prize recognizes Polchinski's broad contributions to fundamental physics, most notably the discovery of D-branes. These have been shown to provide the atomic structure of black holes, predicted long ago by Stephen Hawking, and, as such, are one of the basic building blocks of spacetime.

  One goal of the Milner prize is to raise the profile of work that is not Nobel-worthy because it isn't testable science, by creating a bigger prize for it than the Nobel. Unfortunately I think one side-effect of this is to blur the distinction between things we have evidence for and those that are pure speculation (with "D-branes=basic building block of spacetime" the latter, being promoted to the public as if it were the former).

- Steven Weinberg's graduate level text on QM, Lectures on Quantum Mechanics, is now out, and I'm very much looking forward to getting a copy soon.
- The Higgs boson is Time Magazine's Particle of the Year, Fabiola Gianotti runner up for Person of the Year.
- I recently read Benoit Mandelbrot's posthumously published autobiography The Fractalist: Memoir of a Scientific Maverick, but don't really have the time or interest to write a review here. Mandelbrot has an unusual life-story, starting with being hidden in war-time France to escape the Nazis.

  The thing that struck me most about the book though was that I had always assumed he was an academic outsider, but the true story is quite different. His family was academic mathematics royalty, with uncle Szolem Mandelbrojt a highly influential French mathematician at the College de France guiding him closely. A big theme of the book is Mandelbrot's detailed explanation of the debates involved at each stage of his life over what would be his best next career move. There's more about this than about the mathematics.

  Another reason not to write a review is that I can point to two interesting ones already out there. The Wall Street Journal got Stephen Wolfram to write one, see here, and American Scientist has one by Brian Hayes here. Hayes isn't exactly kind to Mandelbrot, emphasizing his egotism and desire for recognition:

  > Mandelbrot begins one chapter of his memoir with the declaration: "A blessing throughout life: I never wonder who I am." He is untroubled by doubts or regrets, and untainted by false humility. In these pages you will find no self-effacing disclaimers about standing on the shoulders of giants; if Mandelbrot has seen a little farther, it is because he's taller. From an early age his scientific hero was Johannes Kepler, and his goal in life was to accomplish something worthy of a modern Kepler, overthrowing an outworn orthodoxy. By his own account, he succeeded brilliantly, with quite a number of "Kepler moments." (As far as I know, Kepler himself had only one.)

- For another, mathematically more interesting, discussion of a recently departed mathematician with an amazing career, see the AMS Notices article on I. M. Gelfand. Gelfand's career and influence is a huge topic, so this is just Part I.
- A significant new advance in representation theory is explained nicely by its authors here in terms of the general philosophy of representation theory laid out by Gelfand. A standard topic in representation theory courses is to classify the unitary representations of compact semi-simple Lie groups (highest weight theory), but the question of what happens in the non-compact case is much, much more difficult and still open, with one problem that the representations are infinite-dimensional. This latest paper reports "a finite algorithm for computing the set of irreducible unitary representations of a real reductive group G" with the authors describing their result as follows"

  > The third step in Gelfand's program is to describe all of the irreducible
  > unitary representations of G. This is the problem of "finding the unitary dual"
  >
  > $G^{\wedge}_{u} =_{def}$ {equiv. classes of irr. unitary representations of G}
  >
  > It is this problem for which we offer a solution (for real reductive G) in this paper. It is far from a completely satisfactory solution for Gelfand's program; for of course what Gelfand's program asks is that one should be able to answer interesting questions about all irreducible unitary representations. (Then these answers can be assembled into answers to the questions about the reducible representation π, and finally translated into answers to the original questions about the topological space X on which G acts.) We offer not a list of unitary representations but a method to calculate the list. To answer general questions about unitary representations in this way, one would need to study how the questions interact with our algorithm.
  >
  > All of which is to say that we may continue to write papers after this one.

  This sort of representation theory is ferociously technical, with many papers in the subject appearing to have been written only to be read by the very small number of people expert in all these technicalities. This document is surprisingly different, starting off with an accessible introduction to the subject, and then devoting a lot of space to a careful, readable exposition of the details of the necessary technicalities. The subject is still ferociously complex and technical, but this paper gives one a fighting chance to actually understand what is going on if one has the time and energy to read one's way through it. An admirable and unusual choice of how to write a modern math paper.

Update: A commenter points out a nice article that just appeared in Scientific American, Strange and Stringy, by Subir Sachdev, who explains some recent ideas about using dualities to understand certain condensed matter phenomena.

Comments

1. **chorasimilarity**
December 20, 2012

   I commented also on Gelfand here: Gelfand, the Renaissance Man .

2. **Bernhard**
December 20, 2012

   "It's a quirk of CERN that team leaders like Gianotti — with their power over so many people and so much machinery — do not have titles like chief scientist or project director. They are simply called spokespeople"

   It is actually a quirk of the reporter who wrote this article, that clearly has no clue how a HEP experiment is actually run. Spokespersons are called this way, because this is actually what they do, speak for the collaboration. Somehow this Jefrey Kluger fellow think Gianotti or Incandela had actually something to do with the Higgs discovery. Very funny…

3. **Yatima**
December 20, 2012

   String Theory and Condensed Matter Physics meet in the January 2013 issue of SciAm: String Theory Helps To Explain Quantum Phases Of Matter by Subir Sachdev.

   We read:

   "For my purposes, the picture of strings dancing in some higher-dimensional spacetime is not important. It does not even matter to me whether string theory is a correct explanation of particle physics at very high energies. What is significant is that the duality lets me exchange a mathematically intractable problem for an easy one.

   When electrons in crystals have only a limited degree of entanglement, they can still be thought of as particles (either the original electrons or pairs of them). When large numbers of electrons become strongly entangled with one another, however, they can no longer be viewed as particles, and conventional theory struggles to predict what happens. In our new approach,
we describe these systems in terms of strings that propagate in an extra dimension of space.

   My Harvard University colleague Brian Swingle has drawn an analogy between the extra spatial dimension and the network of quantum entanglement. Moving up and down the network is mathematically just like moving through space. The strings can wriggle and fuse together within the extra dimension, and their motion mirrors the evolving entanglement of particles. In short, the spooky connections that troubled Einstein make sense when you think of the degree of entanglement as distance through an extra spatial dimension."

4. **Peter Woit**
December 20, 2012

   Thanks Yatima,
Nice article. I'll add this to the posting itself.

5. **bjm**
December 20, 2012

   Bernhard said, "Somehow this Jefrey Kluger fellow think Gianotti or Incandela had actually something to do with the Higgs discovery. Very funny…"

   She had nothing to do with the discovery? Now I'm really confused. Maybe it's different in Europe, but in the US, a spokesperson is not assumed to have any managerial responsibility. Other statements I've seen convinced me that at CERN the "spokesperson" was actually the project manager Now, your quote above implies she did have no managerial responsibility for the team the co-discovered the Higgs. Confusing.

6. **P**
December 21, 2012

   Bernhard,

   "Somehow this Jefrey Kluger fellow think Gianotti or Incandela had actually something to do with the Higgs discovery."

   Define "had to do with," or this statement runs the risk of sounding *really* silly.

   I'm not sure about Gianotti, but Incandela is a HEP professor and it is safe to assume that he has been guiding his group in building the CMS detector and analyzing data for the last number of years. Senior members of experimental HEP groups at universities are crucial in organizing younger members to execute on the building and commissioning of an experiment, and also in data analysis. If I understanding correctly, sokespeople are handed the reins after years of hard work.

   P

7. **Simon**
December 21, 2012

   At a HEP experiment, the spokesperson is the de facto chief executive. However, as mentioned in a previous comment, a large collaboration behaves pretty much like a semi-headless organic entity. The leader does play an important role but I think it would be fair to say that the Higgs-like object would have been discovered if they had been replaced with any number of their equally well qualified colleagues. The scientist who spent his/her nights ensuring that, eg, the calorimeter read-out works has just a great a claim on the discovery.

   It should also be pointed out that I've never seen spokespersons behave in any way which suggests that they feel a greater ownership of an experiments' physics results than any other member of the collaboration.

   This whole business of awarding prizes to the bosses puts them in a difficult position.

8. **Bernhard**
December 21, 2012

P, Simon, bjm, et al,

Yes, this statement was actually silly one, one that I have no real intention to defend it… I was being flippant on purpose.

I exaggerated what I had in mind to compensate I found no other way of expressing my frustration with this whole business of giving the experiments spokespersons more credit that they deserved. This is not to say they don´t deserve some credit, of course they do… But in terms of who should really be right on the spotlights, I´m sorry, it should not be them, and I´m uncomfortable with this. The people who did the work and its direct leaders (of the ATLAS and CMS Higgs groups) should be those really getting the biggest attention, in my view. The spokespersons had they share of contribution but it´s way smaller then those people, in comparison. The general public has really no idea how this works. Reading the article, one might for example think that a spokesperson actually is leading a Higgs analysis. While they definitely have a say on what goes out or not from ATLAS and CMS I know they were not in front of the actual work (and with actual work I mean, the event selections, understanding the systematics, doing the actual statistical analysis and the plots). This is what I had in mind. I think, no, I know, that the people doing the hard work are not being properly acknowledged for that and this will just be forgotten from history. What will remain is just the spokespersons and to me this is both sad and unfair. I will not defend my stupid comment further than that, but hopefully you can understand my frustration…

9. **P**
December 21, 2012

Bernhard,

Your frustration is totally understandable. Sorry to jump on you – a few times already I've heard some sentiment of "the spokespeople don't actually do any science, and feel the need to correct it. Clearly you know what you're talking about, seeing your recent response 🙂

For awhile I believe they were considering the boson itself for "person of the year." If they were willing to break with tradition there, why not break with tradition and consider the entire ATLAS and CMS collaborations? Or maybe ATLAS + CMS + accelerator guys without whom none of this would happen?

Cheers,
P

10. **emile**
December 21, 2012

Awards and other forms of public recognition like Time Magazines' "person of the year" are aimed at individuals, not groups. The media in particular want a "human story" which you can't tell if you give it to 7000 people. For prizes like the Nobel or Milner, I really wish the collaborations could receive them. But I'd rather have one or two experimentalists get those prizes than to see those prizes go to theorists only. And if you are going to pick one person from an experimental collaboration, the spokesperson is the logical choice.

11. **Noah Smith**
December 21, 2012

Seems like the scientific revolution started by Galileo, Bacon, Newton, etc. might be over.

12. **Bernhard**
December 21, 2012

emile,

I ´m not sure it is really the logical choice. One could think that different ideas could go for different persons. Assume one discovers besides the Higgs, SUSY and other BSM physics that are Nobel worthy. Should all these go to spokespersons? If one face no other choice than giving the prizes to "leaders", than the logical choice would be, to me, the conveners, not the sppekespersons. The thing with spokesperson is not one of logic, but of tradition, one that I advocate should be broken to adapt to the reality of the modern HEP collaborations.

13. **chorasimilarity**
December 21, 2012

Here is a link Peer-review turned on its head has market value

which might be interesting for those who appreciated the post
Why Author Pays Open Access is a Bad Idea (which has comments closed).

14. **Trevor**
December 21, 2012

The piece by Richter was interesting and raised some good points but I think his take on CERN isn't really justified. It's not fair to lump all of the European member states into one group that simply has one vote. I believe that most of those nations contributed independently of each other.

15. **Marcel van Velzen**
December 22, 2012

Hello Peter,

I already have Weinberg's book on QM at home and read large part of it 🙂
It says "First published 2013"!

Great book, reads well, too many good things to enumerate. The more advanced parts are similar to his "The Quantum Theory of Fields".

One of the last problems asks you to calculate the decay rate of the 2p-1s transition in hydrogen. It leads to a funny formula with 2 and 3 to the power eight! I already asked this on Matt's blog but want to ask it here also. Everyone seem to agree that it corresponds well to the experimental value but I can't find the experimental value anywhere on the Internet. I get the impression it has never been measured and people just use the calculated value assuming that it is correct. The best thing I could find is this: http://adsabs.harvard.edu/abs/1983ZhPmR..38..347P
but formally it is calculated there also, although it has to be more or less correct to lead to the right frequency. So if anybody knows where to find it, thanks.

16. **Chris Oakley**
December 22, 2012

> It says "First published 2013″!

I am impressed already. Weinberg has clearly mastered something that other physicists only talk about – closed timelike lines.

17. **Serge**
December 23, 2012

Just interesting fact – Gelfand's "Linear Algebra" was basis (pan intended) for Linear Algebra courses for all Soviet Union technical colledges("Institutes").

18. **Martin**
December 23, 2012

Merry Christmas, Peter.

I read your blog for two years and I am glad that I have this source of informations. I wish you merry Christmas and happy New year.

Martin

19. **crossing symmetry**
December 24, 2012

To all the desperate phenomenologists out there who are waiting for the appearance of another anomaly so that they can do some "science", ATLAS experiment is seeing a resonance of the same-sign dimuon at 105 GeV. With 13fb-1 data, the significance of the bump is 5.02 sigma–around 14 events at the resonance. I hope this will keep our brilliant phenomenologists busy over the holidays in a race to build model.
My personal hunch: looks like nature has Higgs triplet for us, the signature looks very similar to the doubly charged Higgs. If that is the case, as Philip Gibbs has written in his blog, a doubly charged Higgs would explain the H->gg excess.

PS: Dear Peter, I have been following your blog for quite some time. I got the information from on my friend who works in the atlas collaboration. I hope out of courtesy, my IP address will not be released.

20. **Peter Woit**
December 24, 2012

Crossing symmetry,

Thanks! Looking a bit at published results, it seems that conventional wisdom is that 105 GeV is much too light to be a doubly charged Higgs, such a thing should have been visible at the Tevatron.

Looking at the published ATLAS results based on 5 inverse fb, there does appear to be an anomalously high number of events around 100 GeV (but also around 60 GeV), see
http://arxiv.org/abs/1210.5070
I'd guess people are not taking this too seriously, assuming that new physics at that mass would have shown up earlier. The big question of course is whether CMS sees the same thing, but the only relevant CMS results I could find don't even plot data at that low a mass.

Don't worry, the only time I would ever disclose anything about the identity of an anonymous commenter is if they were behaving extremely obnoxiously, and spreading rumors doesn't qualify at all as far as I'm concerned, quite the opposite if they're well-informed…

Merry Christmas to all!

21. **Robert Rehbock**
December 25, 2012

@Chris Oakley:
But Weinberg will, like the LHC spokespersons, need find an appropriate way to share the credit for publishing in the future with the many proofreaders and editors at his publishing house without whom this would not have been possible.
A special Happy Holidays to you and yours. You may recall our meeting last year , or was it next year time gets so uncertain now. 🙂

22. **Chris Oakley**
December 25, 2012

Robert,

Of course.

Happy Christmas 2016!

23. **Peter Woit**
December 26, 2012

About the rumor from "crossingsymmetry".

This has propagated to some other blogs, but from what I've been able to gather, this rumor should be taken with a large grain of salt. It doesn't seem to correspond to anything known to many people at ATLAS (or CMS for that matter).

24. **Yatima**
December 30, 2012

A bit late: Rolf-Dieter Heuer in Nature's [Ten People Who Mattered This Year]

25. **camillo vidani**
December 30, 2012

> The media in particular want a “human story” which you can’t tell
> if you give it to 7000 people.

Well, the media is very much concerned with team sports, and people seem to like stories about it and find plenty of “human factor” in them. These are quite smaller teams, granted, but I think it wouldn’t be so difficult to explain to journalists that experimental physics is a team sport now, and not about single geniuses.

If we wanted to.

Obviously, the leading figures still like the genius mystique very much 😉

East Coast versus West Coast

December 26, 2012
Categories: Uncategorized

Back when I was a student I remember learning that there were two possible sign conventions to use for the Minkowski space metric: the "East coast" one, mostly + signs, favored by relativists and Steven Weinberg, and the "West coast" one, mostly – signs, which was the one in Bjorken and Drell. Then, as now, the big centers of influence in US particle theory were on the coasts: Princeton and Harvard on the East coast, Berkeley, Stanford and Caltech on the West (these days one might want to add the KITP at Santa Barbara). I was educated by the Eastern establishment (who sometimes used the "West coast" sign convention), where gauge theory and the new standard model were the order of the day. The West coast, with its remaining pockets of S-matrix theorists and authors of popular books like "The Tao of Physics" and "The Dancing Wu Li Masters", was considered to be rather behind the times and a bit soft in the head, perhaps attributable to too much time spent in hot tubs at Esalen and too much use of the agricultural products of Mendocino and Humboldt county.

Remarkably, these days the two coasts remain dominant, with US Fundamental Physics Prizes going only to theorists living within a relatively short drive to an ocean beach. An East coast – West coast disjunction in interests remains, one that it remains tempting to speculate may have something to do with California's main cash crop. For quite a few years the West coast has been the center of multiverse-mania, and I've often wondered what the theorists there would turn to when that lost its "new cutting-edge theory" shine.

It remains unclear what will happen in the long-term, but there's now a new hot topic in California these past few months. It was the subject a few weeks ago of a workshop (AKA "brain-storming session") with 50 or so in attendance at Stanford, and is being described by Raphael Bousso (across the Bay at Berkeley) as "this is probably the most exciting thing that's happened to me since I entered physics." A full-blown conference is rumoured for April.

LA-based science writer Jennifer Ouellette does a characteristically excellent job of covering the story, starting here by explaining the appeal of the subject (as well as the problem with explaining it):

> To include every last detail, the piece would have had to be a good 6000 words long, and frankly, very few general readers would care to slog through all the gory details. So why even bother to try, if one can't be comprehensive? Because FIREWALLS! That's why! Seriously, how cool is this concept? There's nothing more crowd-pleasing than death by black hole (just ask Neil de Grasse Tyson) and now there could be more than one way to die. Spaghetiffication, or incineration? Take your pick.

Another version of that post, which includes an excellent set of references is here. For her full treatment, see the version at Simons Science News.

All of this started with "AMPS" a July paper by four Santa Barbara physicists that already has 25 citations and counting (although of the three papers on the topic by Susskind, one is already "Withdrawn because the author no longer thinks it is correct"). For more on the topic, you can try Bousso's Strings 2012 talk, blog entries by Polchinski at Cosmic Variance, Caltech's John Preskill at Quantum Frontiers, Santa Barbara's Aron Wall at his Physics and Theology blog, or Robert Helling here. I haven't myself tried reading these papers, partly because I strongly suspect that I'd end up with the same reaction as Robert:

> Now, of course I had to read (some of) the papers and I have to say that I am confused. I admit, I did not get the point. Even more, I cannot understand a large part of the discussion. There is a lot of prose and very little formulas and I have failed to translate the prose to formulas or hard facts for myself. Many of the statements taken at face value do not make sense to me but on the other hand, I know the authors to be extremely clever people and thus the problem is most likely on my end.

The problem may be that Robert isn't in California, but, like me, is too far East. Someone else brought up in the East coast tradition (and now so far East he has kind of fallen off the edge…) is Lubos Motl, whose reaction to this topic is that the whole thing is Peter Woit's fault:

> AMPS isn't as bad or as obviously wrong as "gravity as an entropic force" but it's still wrong and what's worse about it is that it is pushed by some of the names that are more famous than Erik Verlinde's name. None of those bad apples would really destroy an otherwise healthy research community but the main problem I see is that the bad apples can no longer be efficiently wrestled with. Or it's not happening. It doesn't look like anyone cares at all. Instead, it seems to me that people are defending their subjective and increasingly non-quantitative (and often downright wrong) ideas and these people's connectedness to the journalists and other folks outside the research community itself and the related populism – instead of the scientific evaluation by those who actually understand the things as experts – have become the key determinants of success.

Will firewall-fever spread from the West coast, or is it just a flash in the pan? Time will tell…

On a personal note, blogging may be lighter than usual for the next couple weeks or so, as I travel further East for a vacation in Spain, Portugal and Paris.

Update: Bee's comment reminds me that I had planned to include a link to George Musser's SciAm piece about this.

Comments

1. **Anonyrat**
   December 26, 2012

   Well, it is quite sane to believe that but for Woit and Smolin, who have to sign off on all funding and tenure decisions in all North American HEP departments, particle physicists would be safely working on string theory, instead of on useless thought experiments at black hole horizons. 🙂

2. **Aaron Sputzland**
   December 27, 2012

   Peter, are you suggesting that West coast physicists are all potheads?

3. **Bee**
   December 27, 2012

   It did spread to Stockholm. If you have a lot of time, watch this

   http://www.youtube.com/watch?v=QQ9EGezGvNc

   (Don't ask, I wasn't there and I didn't watch it.)

4. **Bee**
   December 27, 2012

Btw, you can avoid the whole "firewall" issue by just accepting that the BH entropy does not count the number of microstates. Unfortunately, this discussion is dominated by string theorists who can't get themselves to even think about this possibility, which I think is the obvious one.

Also, George Musser wrote a very readable summary here

http://blogs.scientificamerican.com/critical-opalescence/2012/12/14/when-you-fall-into-a-black-hole-how-long-have-you-got/

5. **Lee Smolin**
   December 27, 2012

   I've read several of the firewall papers and it seems to me this is the reducto of two assumptions 1) as Bee says, that the BH entropy counts the number of possible states in the interior of the horizon rather than the entanglement entropy or channel capacity of the horizon and 2) that all the information must get back out to scri-plus so evolution neglecting the region to the future of the singularity is unitary.

   The problem is that they are focusing on the horizon rather than where the real quantum gravity physics must take place, which is the singularity. It is remarkable that the authors of this paper believe they can resolve the puzzles of black holes while ignoring the only region of strong fields, which is the singularity. It is natural to suppose-and model calculations support-the hypothesis that in quantum gravity the singularity is removed and everything that approached the singularity and the information it carries proceeds to a new region of spacetime to the future of the singularity. There is no conflict with QM-evolution including this new region can be unitary.

   Some object to this scenario with arguments about remnants but these are unconvincing as we explain in detail with Hossenfelder in arXiv:0901.3156.

   Whether the region to the future of the singularity becomes classical and develops into a baby universe which is permanently disconnected or reconnects to future infinity, as shown in detail by Ashtekar et al in the CGHS model, depends on the details of the quantum geometry dynamics. But in no case is there conflict with the principles of QM, with the equivalence principle or with usual notions of locality.

   Thanks,

   Lee Smolin

   ps see recent papers of Bianchi for new evidence BH entropy is entanglement entropy.

6. **Peter Woit**
   December 27, 2012

   Lee,

   Thanks a lot for your take on the issue.

   Bee,

   Thanks for the links, I was planning to include George Musser's article, will add that to the posting.

   Also worth pointing out is this from Bee:
   http://arxiv.org/abs/1210.5317

7. **Peter Woit**
   December 27, 2012

   Aaron,

   Certainly not. I don't have any data on the question of why the Multiverse and black hole firewalls are mostly popular on the California coast, maybe others do. Could be lots of reasons: a friend who grew up in California, then moved to Princeton and finally England, told me that the main differences noticed while moving East were that members of the opposite sex got less attractive, and the cheddar cheese got better. So, could be one of those…

   Besides the Scandinavian connection pointed out by Bee, there are other non-Californians involved, including the Dutch here
   http://arxiv.org/abs/1211.6913
   which would argue against this having to do with local California culture. Then again, I just remembered some of the local Amsterdam culture I once encountered…

8. **Aron Wall**
   December 27, 2012

   Lee,
   I think the AMPS argument uses your assumption #2, but not necessarily assumption #1. So long as the infalling observer always sees an approximate vacuum, the Hawking radiation coming out near the horizon is thermal, but if the radiation coming out is thermal, then the information doesn't leak out.

   I say this because Don Marolf and I have been arguing for a viewpoint where any information that falls into the black hole from outside will always escape, but there can still be information in the interior which cannot be measured from outside at any time (if you consider all possible black hole states, not just those that formed from collapse). See http://arxiv.org/abs/1210.3590 and some previous cited work by Don. This would imply that #1 is false but #2 is true.

   I think you're right, though, that the AMPS argument might not apply if the information doesn't come out until after the remnant forms. Seems to me that people on the "West Coast" disbelieve in remnants primarily on the basis of AdS/CFT.

   Peter,
   Thanks for noticing my blog! I've been thinking about linking to your blog for some time now, seeing as I follow it, but I haven't gotten around to doing it yet. I went to grad school at Maryland, partly because I wanted to study quantum gravity at a place that wasn't completely dominated by string theorists. But since then I learned which kind of string theorists are worth talking to about fundamental conceptual issues, and I'm very happy here at UCSB.

9. **lun**
   December 27, 2012

   Judging from
   http://arxiv.org/abs/1212.5605v1.pdf
   the East Coast (with a fluctuation towards the north-center) is steadily falling into a black hole of mathematically sophisticated analysis of non-existent theories.
   It remains to be seen how thick is the firewall separating this black hole from the real world.

10. **P**
December 27, 2012

lun,

in which sense you do mean “non-existent theories?” Please prove me wrong, but essentially any way I slice it, this seems like a pretty ignorant comment.

Nima and company are arguing that this is a generic structure of QFT – yes, they use N=4 (which DOES exist as a theory, though doesn’t decribe particle physics) SYM as an example, but they go into detail about how to generalize to theories with less or no supersymmetry. It is far too premature to make the claims you are making. Time will tell how applicable these techniques are to non supersymmetric theories, but the techniques are deep and potentially very important.

Lee,

“The problem is that they are focusing on the horizon rather than where the real quantum gravity physics must take place, which is the singularity. It is remarkable that the authors of this paper believe they can resolve the puzzles of black holes while ignoring the only region of strong fields, which is the singularity.”

Your slightly condescending tone is also remarkable. Isn’t their whole point that new physics was never expected to be encountered at the horizon!? Telling them that the singularity is the issue seems to be dodging their entire argument. They’re not claiming to solve every “puzzle” of black hole physics, as you imply, by focusing on the horizon – but are merely pointing out that there is an issue with our current conceptual understanding of horizon-scale physics. Of course these authors know that the singularity is important for issues of quantum gravity . . .

11. **Peter Woit**
December 27, 2012

P and lun,

Enough about the “positive Grassmanian” paper here. I’m all in favor of an informed discussion of that topic at some point (maybe someday I’ll know enough about the subject to host such a thing..), but it’s an extremely technical one, and I see no value to this kind of arguing about it.

12. **Bob Jones**
December 27, 2012

The mathematical techniques that Arkani-Hamed and his collaborators are developing is useful for studying all sorts of thories — not just N=4 SYM. As pointed out by P, their results can be used to study scattering amplitudes in non-supersymmetric theories. The cluster algebra formalism that they’re using is also important in two-dimensional conformal field theory, wall-crossing, and other areas.

Sorry to keep harping on this paper, Peter. But lun’s comment is just outrageous…

13. **lun**
December 27, 2012

Since you are allowing a whole bunch of critical comments, I hope a reply is also acceptable. What I meant by “non-existing” is that theories examined in 99% of that paper, as far as I know, do not exist. Real QFTs, as far as we know, are far away from the planar limit and do not have any SUSYs. There might be profound reasons for this, or not, we do not know. Something relevant to the real world might be derived from this or not (read my comment, I say this explicitly), we do not know. Any applications of these techniques to understanding non-planar non-SUSY theories (you know… the real world) are at the moment remote.

That such a profound amount of work has gone into a deep mathematical understanding of such non-existing (in the above sense) theories is remarkable. Since these theories are not realized in the real world, all this might be as “scientific” as the multiverse or firewalls, so it is not irrelevant to this discussion.
It also has a direct connection to the East-West Coast issue, since these techniques descend, in their philosophy, from the S-Matrix approach (rather directly, read the introduction about what is relevant and what isn’t in a theory!), yet the best East coast physicists are now working on this.

Its amazing how many feathers one can ruffle by saying non-controversial truths.

14. **Peter Woit**
December 27, 2012

OK, enough about this, really.

I do think though that the historical comment is correct: this latest East coast effort is a descendent of the West coast S-matrix theory program of the 60s. Whether the arguments about the desirability of abandoning local field theory will work out better this time around is still to be seen.

15. **Yatima**
December 27, 2012

Okay, so the gravastar idea is back?

Note that a “firewall” used to designate a fire-resistant partition in a building. Now it designates a membrane when/where you are instantly de-rezzed.

Oh Tempora, Oh Language Perversions.

16. **Michael Welford**
December 28, 2012

Peter,

Your list of West Coast books leaves out “Quantum Reality”. It’s the best popular account of quantum theory that I’ve ever read. And it would still be the best book even if it didn’t include the best description ever of nonlocality.

I noticed that one of the preconditions for the firewall is the principle of Conservation of Information, a string theorist fantasy that they’ve somehow bamboozled real physicists into believing is an actual law of nature. In fact, Rolf Landauer established the relation between irreversable information changes and irreversable thermodynamic changes back in 1961. And this didn’t come from out of the blue. Chemists had figured out decades before that increasing entropy was associated with information loss and physicists had built much of the mathematic needed. Since Shannon gave the expression for information entropy in 1948, this discovery was arguably a decade overdue. In the years since no has built a computer or discovered a biological process that defies Landauers Principle.

By an odd coincidence, recently I was watching one of the principals on the Firewall Debate, John Preskill, lecturing on quantum information. It was boring, so I skipped through it in case he said something interesting. Nope! But then I saw this post, and decided to give the lecture another chance. And I fell asleep. But third time is the charm and the lecture turned out to be more or less adequate. Funny thing though, the lecture was explicitly about quantum information, but there wasn't a word about information conservation. Could it be that Preskill accurately gauged the reaction from theorists at having one of the fundamental principles of their discipline contradicted?

And speaking of quantum computing/information theorists, it's past time that the Big Names got off their asses and raised a howl about this Conservation of Information business. Or better yet, they should get their asses back in their swivel chairs and type up some refutations.

17. **Bee**
December 28, 2012

Aron: "People on the West Cost" don't believe in remnants primarily because of the "pair production problem", which however has never been shown to exist under reasonable conditions. The only cases I know where it occurs assume the validity of effective field theory and treat the inside of the remnant as having a small volume. Now it is not clear exactly why effective field theory should fail at the horizon far away from the Planckian curvature regime, but nobody seriously expects effective field theory to be valid in the strong curvature regime, which is what you have to deal with if you have Planck scale remnants. So why does anybody care about an argument that creates an alleged problem based on a method that one expects to fail? It's a mystery to me.

18. **Ian Durham**
December 28, 2012

@Michael Welford: Just out of curiosity, why would anyone in quantum information even *attempt* to refute information conservation? It would invalidate large swaths of our work. Personally, I'd be open to the idea, but it would require some major reconsiderations of our not just our results but our methods as well.

Personally, I think one of the major problems in this debate is that quantum theory and relativity treat space and time in fundamentally different ways. Thus, it's really a debate between what [Chris Granade]() has called the (in an intentional pun) ["psi-ontologists"]() and the psi-epistemics. The former believe that the spacetime manifold, not to mention fields in general, possess some ontic status. This sort of world view is built right into relativity itself. As it relates to the firewall problem, this means that the event horizon represents something about space and time itself.

On the other hand a psi-epistemicist might be persuaded that the event horizon represents, rather, something about an *interaction* and thus can be interpreted as some kind of information-theoretic boundary. This is attractive if spacetime is an emergent phenomenon and there is, indeed, a growing feeling that it very well may be.

My own feeling (though I am not the first or only to consider this possibility) is that the interior and exterior of the horizon represent different superselection sectors and thus there is a superselection rule in place that manifests itself classically as this apparent loss of information. On the other hand, quantum resource theory includes techniques that can be used to overcome such superselection rules and thus the information can be recovered in a quantum information theoretic kind of way.

That's a very rough and brief summary of the idea and there's much more to it, but it at least may give an idea of why some of us have no desire to jump on the refutation bandwagon.

19. **Ian Durham**
December 28, 2012

I should note that the superselection explanation only works if one takes a psi-epistemic view. The difference between a timelike and a spacelike geodesic couldn't be ontic.

20. **Aron Wall**
December 28, 2012

Bee,
Of course there is more than one person on the West Coast, and even when they believe the same thing they don't necessarily believe it for the same reason. From my limited perspective here at UCSB, most people here (except Steve Giddings) seem to regard AdS/CFT as a more definitive refutation than the pair production argument, which as you say makes some assumptions from field theory which might well not be valid.

As for the information question, I am a *reluctant* convert to the idea that information isn't lost. Most of the arguments against info loss are rather silly (it's not any more a "violation of quantum mechanics" than the other view) but Don Marolf's argument for holography here: [arxiv:0808.2842]() is hard to get around since it follows almost trivially from the facts that (i) the energy can be measured at infinity in general relativity, and that (ii) the energy generates time translations in quantum mechanics.

Now that firewalls have come along, I'm reconsidering whether I should convert back, but I still don't see any good way around Don's argument.

21. **Michael Welford**
December 28, 2012

Ian,

The Landauer Principle has real world consequances. Todays computer chips are only a couple of orders of magnitude from theoretical limits on energy consumption. And we probably won't get all that efficient, because a low energy chip is a reversible chip is a slow chip. Once we approach the Landauer wall we'll need all sorts of optimization decisions: getting by with smaller registers?, strategic use of qbits?, how about analog?, new fabrication technologies? Now imagine some funding functionary telling you that your proposal is irrelevant because information is conserved so we've already achieved perfect efficiency.

With regard to your deeper remarks, I would like to point that I am well acquainted with the epic-ontic controversy. I just can't remember which is which.

Ending this comment before Peter slaps us both down for drifting off topic.

22. **Peter Woit**
December 28, 2012

Yes, I really can't moderate a general discussion of quantum theory. If it's not about firewalls or East/West coast physics, find someplace else to discuss it…

23. **Daisuke**
December 28, 2012

A couple odd thoughts–

Is it actually certain that EP=no horizon wall? There are some rather dramatic phenomena in classical black hole dynamics, the inner horizons of charged and rotating holes for instance–you'd certainly expect matter falling into a near-extremal black hole to encounter a "wall" of that sort under the event horizon. IIRC the long-term evolution of a BKL singularity, while lacking a "creep" outward to the event horizon, tends toward some sort of abrupt "wall" rather than a drawn-out "spaghettification" near the final approach. Given the generally poor state of understanding of the evolution of BH interiors, is it possible that the classical evolution of the interior could produce a horizon wall of some sort by the Page time, thereby preserving the EP?

Bousso's preferred solution to firewalls seems to be a limited relaxation of normal causality requirements, which brought to mind the Feynman-Wheeler absorber model of radiation–and now I'm wondering how that would behave in highly curved spacetime. The behaviour of advanced waves "approaching" an event horizon would have to be peculiar indeed…

24. **Ian Durham**
December 28, 2012

@Michael (and Peter): My comments were about the firewall issue specifically (I really have no opinion on the East/West Coast matter). Discussing the Landauer Principle would be getting off topic and I have no intention of debating that topic here.

But the superselection question is very germane to the firewall question as is the ontic v. epistemic debate since which view one holds determines the route one takes to attempt to solve the firewall problem (as I clearly pointed out above).

25. **Peter Shor**
December 29, 2012

If you were a firm believer in black hole complementarity (and I believe that the West Coast was the epicenter for this belief), then the quantum information argument showing that something is really wrong with the traditional view of black hole complementarity needs to be addressed. Firewalls seem to me to be a desperate attempt to preserve some of the ideas of complementarity, although my intuition is that they really can't work.

26. **Fry and Laurie**
December 30, 2012

How do you know so much about Mendocino and Humboldt counties?

27. **Spencer Tracy Jr.**
January 2, 2013

I myself don't know anything about those counties – but I'm thinking seriously about heading out there to see if I can find a good looking woman and some bad cheddar.

28. **OldStuff**
January 2, 2013

There are some matters of detail in which the firewall guys are different, but the basic idea that classical gravity can have corrections already at the horizon scale is something that Samir Mathur has been saying for a long time, and the string theory "establishment" has been consistently hostile to.

The real reason why most string theorists are suddenly taking the firewalls seriously is because Polchinski is one of the authors.

Of course Polchinski himself is a very fair person and he gives adequate credit to Samir in all the talks I have heard. But some of his collaborators on the other hand, try to revise history and present it as though all Samir did was to come up with one of the qubit toy models for black holes.

Pretty dishonest, if you ask me.

29. **MathPhys**
January 3, 2013

Of course Polchinski himself is a very fair person and he gives adequate credit to Samir in all the talks I have heard. But some of his collaborators on the other hand, try to revise history and present it as though all Samir did was to come up with one of the qubit toy models for black holes.

Don't worry about it. It's been always like that.

30. **Jean**
January 3, 2013

Don't know whether it's worth posting but Peter's blog has been mentioned in the list of mathematics blogs here-> http://www.talkora.com/science/List-of-mathematics-blogs_112 (look for entry #2 in the list.)

31. **Miguel Carrion**
January 3, 2013

I'm going to have to read the papers in some detail because for the life of me I can't imagine what would prompt Polchinski to write that "the infalling observer encounters nothing unusual at the horizon" is the most easily dropped assumption in the firewalls paper, and that "perhaps the most conservative resolution is that the infalling observer burns up at the horizon".

Maybe this is just philosophical prejudice on my part, but it seems to me that the regularity of the Eddington–Finkelstein coordinates around the horizon uncontroversially implied the horizon could be crossed, at least by a physical system small enough not to be destroyed by tidal forces there.

32. **DaveH**
January 4, 2013

Peter Woit,

I thought your East/West and signature intro was interestingly correlated with your following discussion of AMPS for a couple reasons. First, the t/r signature actually flips at the event horizon (so East becomes West there I guess 😉 (Tongue in cheek, and to avoid "helpful" replies, I know about better coordinate choices; but also see MTW section 31.3 which gives the flip more of a physical interpretation nonetheless.) Secondly, I always thought the convention difference was more GR guys vs QFT guys, not East vs West, to me the split in interpretation also seems to track this better than East vs West.

D

33. **Low Math, Meekly Interacting**
January 4, 2013

I don't know if Dr. Woit is a connoisseur, but horticulturalists, being who they are, always strive to breed better crops. And how. SoCal has its rivals, but even locavores of today are getting a product that is astonishingly potent compared to what was available in the 70's. If these advancements have continued to impinge on physics in any way, their effects are likely to be amplified.

34. **Sebastian Thaler**
January 4, 2013

Off-topic, but I can't resist: http://news.yahoo.com/atom-smasher-hiatus-sets-stage-more-discovery-134311023.html
I can't wait for early 2015, when the LHC "atom smasher" will be "unlocking more mysteries," just as last July's discovery of the Higgs "promises a new realm of understanding of the universe," and when it's gained the "capacity to simulate the moments after the Big Bang nearly 14 billion years ago."

35. **Small Bang**
January 7, 2013

Indeed why not? It's a reasonable article. Read the last paragraph:

"It will bring you more collisions. Which means that the more collisions you have, the more likely you are to see rare events," he said. "The Higgs particle was just one of many on the wish list that we'd like to find, so higher energy increases your discovery potential."

That is a very fair statement. (BTW all of these machines are known as atom smashers. Get used to it. And don't get picky that `atom bombs' are really 'nucleus bombs'.) The SSC was openly advertised as creating conditions not seen since the Big Bang. All of the quark-gluon plasma business is similar `not seen since the Big Bang' stuff. Perhaps you would prefer to say "We have no clue what any of the output from the LHC means"? The obvious riposte to which is "Then why are you doing HEP?"

What *Should* We Be Worried About?

January 14, 2013
Categories: Uncategorized

Back from vacation today, so regular blogging likely to resume. Will start with something quick, a link to material that was posted today.

The Edge web-site annual question feature is out today, with this year's question [What *Should* We Be Worried About?](). I wrote [something]() about the "Nightmare Scenario" that HEP is facing if the LHC finds a Standard Model Higgs and nothing else.

Others addressed the same issue, with [Lisa Randall]() writing:

> In my specific field of particle physics, everyone is worried. I don't say that lightly. I've been to two conferences within the last week where the future was a major topic of discussion and I'm at another one where it's on the agenda.

Her specific concern is motivated by her interest in large extra-dimensional theories, for which no evidence has shown up so far at the LHC. If the extra jump by a factor of 6.5/4 in energy that will arrive in 2015 after repairs still shows nothing, this may be the end of the line for such theories for a very long time. The prospects for a higher energy machine are problematic in terms of technology, as well as the political will to pay for them. The overselling of this that went on for many years pre-LHC won't make it any easier to re-use these theories as an argument for building a new machine.

[Amanda Gefter]() sees no reason to worry. Particle theorists will just move to making progress without experiment, through studying paradoxes of the current theory, with her final example for optimism the recent debate over the "firewall paradox".

[Carlo Rovelli's contribution]() explains one problem with this: humans are very good at convincing themselves they have found some wonderful explanation of something (e.g. some resolution of a paradox, like the supposed SUSY solution to the hierarchy problem), when reality actually involves something quite a bit more subtle and unexpected:

> A number of my colleagues in theoretical physics have spent their life studying a possible symmetry of nature called "supersymmetry". Experiments in laboratories like Geneva's CERN seem now to be pointing more towards the absence than the presence of this symmetry. I have seen lost stares in the eyes of some colleagues: "Could it be?", how dare Nature not confirm to our imagination?

By the way, when I was in Paris last week I picked up a copy of Rovelli's wonderful short book that has just come out in France [Et si le temps n'existait pas?](). It begins with a personal history of how he got into science, from a background in the 1970s disillusion following the flowering of radical ideas in the 1960, a story I found quite interesting, since I'm of the same generation as him. There's also the story of how some of the ideas of loop quantum gravity developed, and some speculative material about time. Definitely worth looking for if you read French and are interested in these topics.

Comments

1. **Bernhard**
   January 14, 2013

   "the "Nightmare Scenario" that HEP is facing if the LHC finds a Standard Model Higgs and nothing else."

   Well, the current Higgs diphoton rate is not really supporting this scenario, quite the contrary. This should keep people busy at least for some years.

2. **Pi**
   January 14, 2013

   Hello Prof. Woit,

   Nima has published two papers with the second one on SUSY and the first one on scattering amplitudes. As a general audience, I'd like to hear some explanation and your comments about these two papers.

   Best

3. **Peter Woit**
   January 14, 2013

   Pi,

   In both cases, it's probably worth making it clear that Arkani-Hamed has co-authors.

   I'm far from expert enough to comment intelligently about the amplitudes paper (except that the short section on relations to mathematics is rather wild). At some point soon I'll probably write more about the latest status of SUSY, and the second paper you mention is part of that story.

4. **Bob Jones**
   January 14, 2013

   Rovelli's argument is totally bizarre. He criticizes supersymmetry for being too speculative, when in fact it is one of the most conservative ideas about BSM physics. Supersymmetry is not in the same category as the multiverse, nor is it an a priori bad idea that should have been abandoned long ago, as Rovelli implies. It is a very general idea that addresses a range of problems, and it is an important part of quantum field theory regardless of whether it provides a successful framework for BSM physics.

   The most ironic thing is that Rovelli is one of the leading proponents of loop quantum gravity. Most of his argument applies pretty well to his own field…

5. **Bob Jones**
   January 14, 2013

   Peter, I don't want to lead the discussion off topic, but I'm curious to know why you say the relation of Arkani-Hamed's work to mathematics is "wild".

6. **Peter Woit**
   January 14, 2013

Bob Jones,

To be more explicit, I was referring to section 15.4 of the paper, for which I think the adjective “wild” is pretty appropriate. Again, calling this “Arkani-Hamed’s work” is doing his collaborators a huge disservice. In the case of section 15.4 especially, which pretty clearly was written by the mathematicians in the collaboration (Goncharov and Postnikov). Please though to all, if you want to find a place to discuss this paper, you need to find one moderated by someone more expert in the topic than me.

About Rovelli, I think his point was exactly that the least speculative of these kinds of ideas, weak-scale SUSY, is turning out to have nothing to do with reality, giving a good lesson in why one should be skeptical of imaginative speculations of theorists. Yes, the lesson also applies to LQG.

7. **Geoff**
January 14, 2013

If anyone is taking bets on where the necessary input for the way forward is going to come from, can I put my money on observational cosmology?

8. **Peter Woit**
January 14, 2013

Geoff,

You can put your money there, but people have been making that bet (that something learned from cosmology would tell us how to go beyond the standard model) for more than 30 years now, and so far it has been a losing one.

9. **Geoff**
January 14, 2013

Thanks for the reality check. It seemed a slightly better and more obvious choice than my second guess which was looking at different systems that occur far away from equilibrium.

10. **chris**
January 15, 2013

“In my specific field of particle physics, everyone is worried. I don’t say that lightly.”

it is interesting how perspective can distort reasoning even for the most emminent of scientists that should have quite a keen sense of how not to fool themselves.

i am in hep theory and i am not worried in the slightest. neither are any of my colleagues. that might be connected to the fact that all of us never believed susy or string theory to be of any use for understanding the weak scale in the first place. so accordingly, we did not indulge in building spectacular models that get killed now by experimental evidence.

quite on the contrary from being worried, i have the sincere hope that department hiring committees will resume to hire based on contributions to physics on not on how well one person can jump on the latest fad that is fashionable but rather very much unlikely in the grand perspective of things.

how preposterous it is for a scientist to be disgruntled by what nature chooses to be. we are supposed to find out, not to satisfy our vanity. the “SM only” results of the LHC are quite healthy to the field and let us not forget that the Higgs (as boring as it might seem to some – i am specifically disregarding the gamma-gamma excess here as this is most probably a systematics issue) is the first fundamental spin-0 we have found – a true prediction not to be sneezed at.

a bit of humbleness would serve us all well

11. **Simon**
January 15, 2013

Bernard
The results from ATLAS and CMS are consistent with a SM Higgs boson.

12. **Bernhard**
January 15, 2013

Simon,

Yes, they are consistent. However, according to ATLAS the diphoton rate is about 80% the SM prediction. The interesting thing is that the excess grew with luminosity, but of course as chris points out there is still plenty of room to blame this on systematics. I am being optimistic and believing in ATLAS rather than in CMS.

13. **laboussoleestmonpays**
January 15, 2013

I wonder if theoretical physicists need studying speculations like the firewall paradox or need developping a heuristic firewall to deal with “wild” speculations …

14. **Jack**
January 15, 2013

Interesting discussion. I left theoretical physics 30 years ago when Mike Green (now the Lucasian professor) successfully convinced me there were no useful ideas out there. Of course his actual intention was to convince me to join him in String theory research, but the idea that since we had no best fit theory (I was in agreement that SUSY on it’s own looked unconvincing), then the next best option was to provide so many degrees of freedom that somewhere in the resulting vast solution space would be an answer, was enough to make me realise that no one had anything useful to say. Seems like it’s pretty much the same situation, no change.

I do disagree with your comment that 30 years of cosmology research hasn’t shown what’s beyond SM – on the contrary, the only actual experimental evidence we have at all beyond SM is from cosmology as far as I can tell.

15. **Shantanu**
January 15, 2013

Jack, I completely disagree with you that
“the only actual experimental evidence we have at all beyond SM is from cosmology as far as I can tell”, even though that seems to be the current folklore.
from astronomical observations alone. there is not one shred of evidence that cold dark

matter has anything do with WIMPs, SUSY or beyond standard model particle physics
or even with standard model of particle physics.
(the only argument given is WIMP miracle which has been overblown and IMO it is as likely or unlikely as Titus' Bode's law)
Regarding dark energy, it may well be consequence of us living in an inhomogenous universe or a breakdown of classical GR.

The only other evidence you may be considering is inflation. In 80's people were optimistic
that inflation is connected with GUTs and the like. However GUT based models of inflation are ruled out

16. **physicsphile**
January 15, 2013

Shantanu,

From cosmology we get that only about 5% of the Universe is baryonic. That means new physics or "beyond standard model" physics is needed to explain what the other 95% is. Admittedly it could be modified general relativity but as far as I understand, people usually mean the "standard model" to include general relativity.

17. **Peter Woit**
January 15, 2013

physicsphile,

Dark matter is the most promising possibility for a connection between observational cosmology and the SM, and a huge amount of effort for many years has gone into exploring this. The bottom line though is that this has yet to lead to anything. Even if dark matter is due to some new particle not in the SM (an axion, a WIMP, something else), the constraints that cosmology provides on the properties of this particle aren't enough to either allow definitive experimental tests, or such that they point to a convincing extension of the SM. The SUSY WIMP scenario is the most developed of such things, but it doesn't seem to be working out.

18. **Jack**
January 15, 2013

The SM plus GR does not explain the observed properties of the universe (galactic rotational discrepancy, etc). They do explain every single experiment we have ever done on earth and near space (i.e. to the solar system edge).

Internal inconsistencies (renormalisation, etc) and ugliness and lack of integration between SM+GR is NOT a valid experimental reason for SM+GR to need modification – the universe's laws could be ugly and inconsistent. The only actual experimental evidence we can detect that is not explained by SM+GR is that obtained by observations of the universe beyond our solar system. Sadly.

19. **Peter Woit**
January 15, 2013

I think that's enough about cosmology, which is pretty much off-topic.

20. **Shantanu**
January 15, 2013

physicsphile, DM could very well be a primordial black hole (which is perfectly consistent with all data), in which case its nothing to do with particle physics, and
is consistent with GR. Also it could be an artifact of backreaction (see 0809.1183)
or some similar misunderstood physics.
But anyhow the main strawman I wanted to destroy which you will hear in almost every
conference is that dark matter is evidence for Weak scale SUSY or any beyond SM
theory which predicts a stable massive particle with electroweak interactions.
There is no evidence for astronomical observations that the cold dark matter is WIMP

21. **SpearMarktheSecond**
January 15, 2013

It took many years for neutrino mixing/mass to be experimentally proven beyond all doubt… until the early 1990's and SAGE, the evidence was thought to be as poor as that right now for WIMP dark matter (from DAMA/LIBRA, Cogent, ATIC, etc).

Not that the WIMP evidence is convincing or right, but the point is: if you give up doing experiments, you are guaranteed to discover nothing interesting. The neutrino guys never gave up. And the dark matter searchers must never give up either.

The current ratio of dark to luminous matter is evidence that the dark matter has weak interactions with luminous matter. It is not an airtight case, but it is interesting enough to pursue experimentally.

BTW, the LHC has really interesting and constraining results on light WIMPs, via missing energy. No signal, but very interesting constraints.

22. **Yatima**
January 15, 2013

> the universe's laws could be ugly and inconsistent.

They may be ugly but we better hope they are consistent. Actually being able to get 1 = 2 (even if you have to jump through a few hoops for this) is very bad, economic mainstream-level bad.

23. **Geoff**
January 15, 2013

Sorry if my cosmology comment derailed the conversation. Part of what I had in mind was that if large classes of models predict a negative value for a cosmological constant in stark contrast to the observed value at least cosmology provides a sanity check unavailable elsewhere (and this has been within the past 15 years as opposed to 30).

If somehow we could travel back in time to 1961 and try and get unstuck, I'm not entirely sure anyone would consider it obvious to ask a guy working for the phone company. New ideas on how to move forward seem to come from unexpected places utilizing unexpected analogies.

24. **Pol**
January 15, 2013

How is the hep-th job market doing? Looks like pheno modelers are doing fine..

25. **gstoessl**
January 16, 2013

Are sterile neutrinos as dark matter candidates already ruled out ?
The most extreme scenario could then be: SM + 4th neutrino + GR and
nothing else.

26. **Shantanu**
January 16, 2013

gstoessl ,
For most generic parameters, sterile neutrino satisfies what astrophysicists call
“warm dark matter” (WDM). From fits to cosmological data, WDM constitutes a very small fraction of the matter/density budget.
Having said that, since we have not detected a sterile neutrino or know its mass etc, its entirely possible to cook up sterile neutrinos with non-standard properties so that they could be CDM candidate in which case you are right.
Again astrophysical observations are completely agnostic to nature of CDM.

27. **David Nataf**
January 16, 2013

Astrophysics and Cosmology have taught us that:

– There is dark energy, and it has a specific density. We will soon find out how it evolves with redshift.
– There is dark matter, and its properties are actually quite well constrained, i.e. it cannot be a light particle, hence “WIMPs”, where “M” stands for “Massive”.
– Neutrinos have masses. That comes from observations of the sun and the original SM had massless neutrinos. Actually, I think it was cosmology originally that taught that there were at most 7 neutrino species, which was a surprise at the time.
– The GZK effect is true, a probe of particle physics at 10^20 eV.

In the near-future we’ll have more constraints on dark matter, we’ll know to what extent dark energy evolves and what its equation of state parameter is, more work will be done on the lithium problem, there will be more evidence for (or against) inflation, we’ll directly measure gravitational waves in the labs, we’re approaching tests of gravity at high curvature scales, etc. Currently, cosmological constraints suggest a “4th” neutrino species at 2-sigma significance, and we’ll know more about that soon as well.

Some of you need to pay more attention.

28. **srp**
January 16, 2013

Call me crazy, but as an outsider who can only follow things dimly, it seems to me that stuff like

http://www.annualreviews.org/doi/abs/10.1146/annurev-nucl-102711-094949

makes a pretty strong case that the SM is not at all worked out yet and that there are some huge, important unsolved problems in understanding existing accelerator data. There are even hints that multiple anomalies are pointing to some common basic conceptual shifts.

I guess one could say that all this stuff is still not “beyond” the SM, but so what? There are 400% anomalies against current understanding and they get bigger (instead of the expected smaller) with collision energy. Or one could say that these are just problems in calculating the predictions of the theory rather than with the theory itself, but a) how do you know? and b) if so, isn’t doing those calculations a primary job for theorists?

Perhaps there are some anthropological factors that would explain what is or is not considered a big deal to worry about in physics. But even in a “softer” field like finance, where predictions are by their nature much less precise, if it is thought that x is supposed to equal y but empirically x = 4y, where x and y are basic observables, people tend to get exercised about it.

29. **Yatima**
January 17, 2013

The above paper is obtained at lower rates than post-bailout rent-seeking ones at the DOE:

Puzzles in Hadronic Physics and Novel Quantum Chromodynamics Phenomenology

30. **fuzzy**
January 17, 2013

Hi Peter,

I believe that in the last 20-30 years, we had a lot of speculations in theoretical high energy physics, but not many frank, critical discussions.

Surely, a big part of our society wants to hear from the physicists bombastic ideas, amazing scenarios, in a words, they should run the show; and, by contrast, we have much less requests of exercising critical sense.

However, it is important to recall that all of us, scientists included, are called to decide whether we want “to exchange a walk on part in the war for a lead role in a cage”. This is what should primarily worry us, in my view.

The same view is expressed in http://arXiv.org/pdf/1206.1466.pdf in somewhat more formal terms.

31. **Peter Erwin**
January 17, 2013

I’ll confess to being a bit nonplussed by the final paragraph of your Edge contribution, where you seem to think that the current problems of particle physics are somehow going to infect the rest of science. ("Will intellectual progress become just a memory, with an important aspect of human civilization increasingly characterized by an unfamiliar and disturbing stasis?") I can see no evidence for this in any other field of science.

32. **D.G. Perkin**
January 17, 2013

Yatima: Thanks for the link. This seems a useful reminder that there are still problems in HEP, even though it does not tackle the electroweak radiative corrections which (IMHO) seem more serious, unless I've missed some work published recently.

SRP and others: the Standard Model is still quite healthy, despite points referred to above. Like the Copenhagen Interpretation of Quantum Mechanics, it keeps taking the flack from its detractors and continues to generate results in accordance with experiment.

Regards,

D.G. Perkin.

33. **Peter Woit**
January 17, 2013

Peter Erwin,

Well, it's a "worry" about what might happen that was being asked for, not an already serious problem…

As Lisa Randall notes, within particle physics "everyone is worried". Whether this infects other fields depends a lot on how particle physicists deal with the problem. One particular form of the worry is whether pseudo-science (e.g. the string theory anthropic landscape) will become the dominant paradigm in the historically important field of fundamental physical theory. If this happens and fundamental physics changes from an ongoing successful example of how humans can understand reality to a dismal example of what is wrong with human behavior, there may be larger implications.

Perhaps I should be more optimistic and not worry. we'll see.

34. **Peter Shor**
January 17, 2013

My hope is that Alain Connes is actually onto something with his connection of non-commutative geometry to the Standard Model. However, physicists seem universally either to ignore this or to dismiss it.

35. **cormac**
January 17, 2013

Speaking from the perspective of someone who teaches an introductory course in the history of particle physics, I think there is more to the disappointment at lack of evidence for SUSY than arrogance or irritation that our theories don't match reality (so far).
The idea of super gauge symmetry provided a way around the famous no-go theorems of the 1960s; when theoreticians encountered major problems in extending the unification program to include the strong and gravitational interactions, SUSY suggested a possible escape route. So much so that, as I understand it, it is difficult to see how the fundamental forces can ever be described in a unified framework without some version of this fundamental symmetry. So lack of evidence for SUSY is a little worrying as it could imply that some interactions are unified but not others…which seems strange and also at variance with our favourite cosmological pictures. That said, there are many different versions of SUSY and it's early days yet…

36. **emile**
January 17, 2013

David Nataf:

You wrote that Dark Matter cannot be light. That is not correct. Axions for instance could be very light (and address the Stron CP problem).

37. **Tmark48**
January 17, 2013

My my, reading those "apocalyptic" reactions from physicists just makes me think they are nothing but drama queens. HEP is littered with unresolved problems, inside the standard model and outside the SM. If multidimensional theories are out, pick another problem. Isn't that what a theoretical physicist is supposed to do ? Or have we arrived at a situation were overspecialization leads to stagnation ? You study 20 years to have the instruments and skills to tackle one problem, and beyond that ? Nothing ? Now this would be catastrophic.
Me, I'd like to see theoretical physicists get another go at axiomatic quantum field theory or something equivalent like that. A theory that from the get-go negates infinities. A proper physical theory where you don't have to deal with renormalization and other arcane techniques to get rid of things that shouldn't even be in the theory to begin with.
I think designing a finite quantum field theory is a much more interesting proposition than chasing strings, d-branes and other exotic structures with no link whatsoever to down to earth experimental physics.

38. **Low Math, Meekly Interacting**
January 17, 2013

I feel there's some reason to worry over the prospect of loss of "Big Science" in HEP. This branch of physics, to many, represents an ideal few other disciplines can rival, and an exemplar of "pure" research. Can anyone think of a more impractical scientific discovery in the past thirty years which has attracted so much attention as the Higgs boson? I do fear a world in which that discovery may be the end of new discoveries in fundamental physics for a very long time. Without this exemplar, or any obvious path to pursue it, will science as a whole veer more and more toward the "practical"? Will science, as a whole, lose the tradition of curiosity for its own sake needed to sustain it?

39. **A.J.**
January 17, 2013

A proper physical theory where you don't have to deal with renormalization and other arcane techniques to get rid of things that shouldn't even be in the theory to begin with.

This is one of those scurrilous rumors that just won't die. The modern formulation of quantum field theory — the one Wilson et al got off the ground in the 70s — is free of unphysical infinities. The textbook presentations, unfortunately, are littered with them.

40. **Peter Woit**
January 17, 2013

Please, no more about renormalization. It's off topic, and blog comments here about it are not unlikely to enlighten anyone about anything.

41. **ker**
January 17, 2013

Peter,

For all your criticism of string theory, you are clearly a product of the 70s and 80s culture in which somehow particle theory was the 'fundamental' area of physics and the intellectual backbone/heavyweight of the field. As someone who has spent equal time on your side of the fence and on the other side in condensed matter, soft matter etc in the last 10 years, I can tell you that the culture of particle theory today is to first order irrelevant to the rest of physics and science.

It is a very isolated narrow field with little intellectual heft today even within physics departments. Other physicists work on as many interesting novel ideas & systems in a year as string theorists work on in a decade – and these physicists know it.

The days of Gell-mann & hep-th towering over physics and science culturally are long dead.. I am sure Peter knows this in his head but his worry about the culture of hep-th affecting the rest of science shows some remnants of Peter's upbringing. Trust me, other scientists don't give a damn..

42. **Peter Woit**
January 18, 2013

Thanks ker,

I think you're right, as far as some physics departments go, and someday even the IAS may hire a non-particle theorist (although the time-frame of particle physics dominance goes back to the 50s and 60s, before my time, and it's not as weak now as you claim).

Non-physicists though often encounter physics through curiosity about "what is the world made of?", "what are these fundamental physical laws that govern this?", as well as through taking courses where they learn the standard historical narrative. From this point of view, particle physics and cosmology are likely to always seem like central topics (and to always get a lot of press), so degeneration of research in these areas is going to look to outsiders like degeneration at a core location of modern science.

43. **MathPhys**
January 18, 2013

Some of us find exact solutions in statistical mechanics and quantum field theory deeper and more fundamental than theoretical high energy physics as it currently stands. A typical talk on current high energy theory sounds as theoretically ad hoc as a talk on civil engineering.

The best parts of modern string theory (topological strings and AdS/CFT) and Yang-Mills theories (non-perturbative dualities and exact solutions) are essentially new applications and extensions of ideas from the theory of integrable models.

44. **David Nataf**
January 18, 2013

Emile,

I'm pretty sure axions are not consistent with growth of structure and galaxy rotation curves.

Light particles don;t gravitationally settle into halos, they travel at relativistic speeds and are spread evenly across the universe.

However, there could still be axions, and they could in principle be inferred astrophysically, for example as a cooling mechanism for white dwarfes.

45. **P**
January 18, 2013

@chris: the worry isn't that our theories aren't working. The worry is that if new physics doesn't show up at this scale the governments won't fund expensive new experiments to probe beyond the standard model. I'm also a high energy physicist, and many particle physicists I know, even ones who don't like string theory, are very concerned about this possibility.

If nature tells us something else is responsible for stabilizing the hierarchy than the theories we've come up so far – great! Nature is telling us something. But the concern is that we won't learn anything really new at the LHC and funding for high energy experiment will screech to a halt.

@ker:

Physicists of your type have many physical systems to study, and there are many more of you than there are string theorists (it's not even close), so it's not surprising that you come across more physical systems. High energy physicists are, by definition, interested in one physical system, though they may apply their "novel ideas" (in the sense of abstract theoretical ides) to many physical systems.

Which bring me to my main point, to correct somewhere where you're off. Your statement about novel ideas is absurd, assuming by that you mean some more abstract ideas that may or may not be immediately applicable to already-known-to-exist real world physical systems. Regardless of the eventual fate of string theory's ability to describe the one *system* high energy physicists are (by definition) interested in,
nearly all abstract conceptual breakthroughs in the last thirty years in gravity, quantum field theory, or string theory have been *made by string theorists* or people who at one point called themselves string theorists. In this sense, nearly all of the "intellectual heft" to use your phrase is coming out of theoretical high energy physics.

In my experience at top 20 U.S. institutions in physics, between giving seminars at many of them and researching as a member of a number of them, the reason "particle theory today is to first order irrelevant to the rest of physics and science" is because the average condensed matter theorists lacks the theoretical backbone to understand what we are talking about. The very best do, absolutely, I agree with you, but it's just downright offensive and incorrect to make your claim in the way you have.

46. **Peter Woit**
January 18, 2013

P,

One of the big reasons that many physics departments are now treating particle theory as irrelevant is the incredibly arrogant behavior and attitude of theorists like yourself. Instead of showing some humility and admitting that things have not gone well in recent years, too many string theorists think that it's a good idea to go on about how brilliant they are, how wonderful their "conceptual breakthroughs" are, and how the problem is just that non-string theorists are ignorant and stupid (i.e. lack "theoretical backbone to understand what we are talking about"). If you want to know why virtually no string theorists are getting jobs in the US, this has a lot to do with it.

47. **P**
January 18, 2013

Peter,

Apologies for the tone of the previous post, I am a confident competent theorist, not an arrogant one. Sometimes this comes out the wrong way when I find someone's arguments silly, incorrect and damaging, or offensive.

High energy physicists acknowledge some difficulties faced in using one of their theories (strings) to uniquely address issues in TeV scale physics. You and I have discussed this many times.

However, hiring in physics departments should be based on quality of work and scientific though, either conceptual and abstract or applying known ideas to a new system (or ideally new ideas to a new system). You yourself made the argument that it is made based on hurt feelings and perceived arrogance, and unfortunately this is sometimes true. High energy physicists readily acknowledge when real conceptual breakthroughs come from people in other fields (e.g. Kitaev, Wen (though formerly a string theorist), C. Kane, others, etc.), but when high energy physicists stress their many conceptual breakthroughs in the last 30 years, we are calling a spade a spade, not being arrogant. This is scientific endeavor; we should be honest about progress and it is a shame when good ideas and confidence in them are perceived as arrogance, particularly when this affects hiring decisions. Hiring of scientists should be based primarily on the quality of their ideas, not their sometimes bad attitudes.

48. **Peter Woit**
January 18, 2013

P,

Arrogance by itself isn't the issue, if physics departments didn't hire anyone arrogant, they'd be a lot smaller. What is the issue is that people don't want colleagues who are both arrogant and delusional, convinced that their work is highly successful and important (when it isn't), and that everyone around them is just too dumb to see this.

49. **Shantanu**
January 18, 2013

David you are wrong. axions are perfectly consistent with a vanilla CDM.
axion is a non-thermal relic so its perfectly fine. note that something as heavy as a WIMPzilla (worked on by Rocky Kolb et al) could also be a vanilla CDM.
again astrophysical observations alone provide no guidance for the mass or the interaction
cross-sections.

50. **chris**
January 18, 2013

P,

"The worry is that if new physics doesn't show up at this scale the governments won't fund expensive new experiments to probe beyond the standard model."

and they are right. i myself do not see the need of a multi-billion-$ collider to extend our energy range by a factor of 5 when there is no compelling argument that new physics should show up there.

in such a situation the money is probably better spent elsewhere. where? well, i currently don't know, but people will have ideas i am sure.

what i perceive as the real problem here is the same problem that infests us at the individual level: the success-metric driven system that science is slowly turning into makes many of us think that the ultimate goal of science is to divert ever increasing streams of external funding into our direction. this is just plain wrong.

if we have learned all that we can reasonably hope to learn through current accelerator technique, then let's be happy that we learned it and turn to something else. and if a thousand people have invested 3 decades of their lifes into this particular topic that turns out to be a dead end then it is a dead end still. no amount of money will ever convince nature to change its laws so as to make them accessible to us.

now please don't get me wrong – i am not arguing against a next collider. i actually think that a linear collider "Higgs factory" would be well worth building at least. but i seriously dislike the gut reaction of many physicists that lament the decreased chances of future funding now that the SM is confirmed as if it was the ultimate goal of physics to finesse out as much tax money as possible.

51. **Peter Woit**
January 18, 2013

chris,

I think a crucial question is the magnitude of the cost of going to higher energy. Can this be done within the current size of the CERN budget? For instance the idea of a higher energy LHC (HE-LHC) in the same tunnel might be one that could be funded without significantly increasing the CERN budget (to be clear, I don't know if this is true), especially since it takes a long time to build these things, spreading the cost out over many years.

Without some specific important new target to justify a higher energy machine, I think it's going to be very hard to make the case for lots more money for such a thing (except maybe for the Japanese, who may have good financial reasons to print a lot more money…). On the other hand, once the LHC has done what it can, do you just start shutting down large parts of CERN and firing most of the staff there, or do you pursue a viable way to investigate higher energies, even given a large probability you're not going to find something exciting?

52. **Thomas Larsson**
January 18, 2013

That a science comes to a halt when it reaches its boundaries is neither strange nor unprecedented. The obvious example is of course geography. Few modern geographers strive to become a new Columbus or James Cook, and those who do are hardly taken seriously.

In fact, we are now seeing a prediction of John Horgan pan out. After the end of science, scientists will do boring things, but they will never admit that it is boring.

53. **Tmark48**
January 18, 2013

> Thomas Larsson said : "In fact, we are now seeing a prediction of John Horgan pan out. After the end of science, scientists will do boring things, but they will never admit that it is boring."

–
Didn't historians witness the end of history when the Soviet Union collapsed and the old world order went into pieces ? Seriously, I don't think that confirming the SM will be the end of science. It will make HEP less relevant but scientific research will go on. Investing in other branches of physics. It's a natural cycle I think, scary for some but necessary.

54. **Bernhard**
January 18, 2013

What we need in HEP is a real breakthrough in experimental technology to search for new physics. If accelerators become impractical we have to find a cheaper way to do TeV collisions. The only thing we had guaranteed with the LHC was the Higgs or something else, we should not be too surprised or upset about "just" finding the Higgs. I think experimentalists need to be "blamed" as well for the lack of imagination. The LHC was a brilliant project and a formidable experimental achievement but I hardly see people discussing the problem of how could we invest in promoting solutions to NOT need accelerators (at least huge ones) in the first place. This kind of breakthrough might be decades or a century in the future but might be also closer than we think. But while theorists have been trying all possible things (including some things that can hardly be called science, but anyway), experiments follow the same basic logic of Lawrence, just got a lot bigger (OK, shoot me me for the oversimplification here if you want). We need fresh new ideas on how to do this differently, and we should seriously start promoting this as a challenge for the most brilliant young experimentalists.

55. **srp**
January 18, 2013

Did you notice the "move along, nothing to see here" non-response by DC Perkin? The linked paper had mainstream particle physicists showing that the hadron part of the SM is definitely NOT "passing all the tests." It is systematically failing them in interesting, not-close-to-the-observational-noise ways at accessible experimental energies. Or maybe the theory "properly understood" would pass them, but the theory is now wrongly understood, in the sense of physicists having an incorrect picture of how its assumptions relate to observable phenomena, which would also seem to be a pretty important problem for high-energy theorists to work on.

I understand that QCD is a difficult jungle through which to hack, but isn't that a made-to-order challenge for HEP?

56. **StrangeRep**
January 18, 2013

Hi Peter,

Thanks to your blog article I started reading some of the other non-physics Edge contributions. Did you read the one by John Tooby? I.e.,

"Unfriendly Physics, Monsters From The Id, And Self-Organizing Collective
Delusions".

This line near the end:

"we need to design our next generation scientific institutions to be more
resistant to self-organizing collective delusions, "

seems appropriate, :-), though I have no idea how to do it. 🙁

57. **Miguel**
January 21, 2013

> Amanda Gefter sees no reason to worry. Particle theorists will just move to making progress without experiment, through studying paradoxes of the current theory, with her final example for optimism the recent debate over the "firewall paradox".

Please, no. Theory was "ahead of experiment" for about 40 years (since 1973) and look what that brought us. While theory was though to be ahead it was actually off on a tangent. And then another tangent. And another….

58. **Miguel**
January 21, 2013

P:

> However, hiring in physics departments should be based on quality of work and scientific though, either conceptual and abstract or applying known ideas to a new system (or ideally new ideas to a new system).

Hiring and research funding is now largely based on bean counters weighing the number of citations by impact factors. That is the real reason why Peter Woit is right to worry about progress in science slowing down. Maybe this is only apparent as the signal to noise ratio goes down as the amount of drivel published to pad CVs grows, and the rate of actual advancement of science somewhere remains steady or growing…

59. **P**
January 22, 2013

Hi Miguel,

No one is going to argue with you on this point. CV padding is not science.

Regarding your notion of tangent, this isn't the right viewpoint. It seems your definition of tangent applies to any abstract concept that isn't directly applied to an experimentally observed system, because that is what a lot of the last 40 years has been, since the standard model works so well. It seems like to you essentially any progress in formal theory is a tangent.

And there is really no arguing with string theory when it comes to progress in formal theory. Yes – you can argue that it's not good for phenomenology, though as an expert I can say nearly every argument I've seen on this blog is either wrong on some important points or missing important developments from the last ten years. But formal theory – particle physics aside – it has led us to see deep explanations of dualities in field theory, the AdS/CFT correspondence, mirror symmetry, deep properties of scattering amplitudes that Nima argues in a recent paper may apply to N=1 and N=0 theories, etc etc etc. There are many more. These are not tangents or "delusions", to use Peter's phrase.

But more importantly, coming back to the point of this article: we should be very concerned about Higgs + nothing. Not because "it's not what we wanted" scientifically – we must be fine with whatever effective theory nature hands us – but because it could signal the end of the reductionism approach to experimental particle physics (if new colliders aren't built). And this would be very sad for humanity, I think.

For the record, I am very happy that some of the best universities are hiring pure particle theorists the last few years. This is the time to do it, if there ever was one, and there are many excellent new papers on hep-ph. Over the next five to ten years there will be a swing back in the other direction, however. These things are cyclical.

Cheers,
P

CERN Briefing Book

January 18, 2013
Categories: Experimental HEP News

This year the US and European HEP communities are engaging in exercises designed to put together plans for the future. In the US it's Snowmass 2013, leading up to a big meeting in Minneapolis this summer. This past week has seen preliminary meetings at Irvine to discuss future prospects for experimental study of SUSY and other BSM ideas, and at Princeton for discussion of future prospects for study of the Higgs.

Over in Europe, there is a plan to update the 2006 European strategy for particle physics, This will officially take place in May/June, based on a document to be finalized by the CERN Council in March. Next week in Erice there will be a meeting to draft this document. To prepare for this, there was an open symposium last September, leading to the preparation of a briefing book, now available here.

The briefing book is a very interesting 220 page document covering in up-to-date detail the current experimental situation and future prospects, for all areas of HEP. One big issue is the Higgs: now that its mass is known, what can be learned about it using some new machine (a "Higgs factory") beyond what can be learned from the LHC? As far as prospects for a new, higher energy machine, the document describes the possibilities, but no decision about such a thing is likely to be made until after results become available from seeing the LHC run at 13 TeV starting in 2015.

Comments

1. **Markk**
   January 18, 2013

   I have a question about the goals. Are these more technology plans or experimental plans? It seems like until results at higher energies are analysed, the scientific direction may not be known, but we could be looking at what technologies would be needed in different cases.

   For example I have read about these linear burst (for lack of a better term) accelerators that somehow get energy injected in a short time. Is that a possibility? Is just a bigger type of current system the only bet? It seems to me like we may be in a period where research into enabling experimental technology is more important to look at.

2. **Peter Woit**
   January 18, 2013

   Markk,

   The technological issues are a huge part of the discussion, they're what determine what the possibilities are. One chapter of the briefing book devoted specifically to accelerator technology, and the question of which areas of such R and D to fund is a big one being considered.

3. **Brendan**
   January 18, 2013

   Booklet also seems to be taking a stand on credit that is not aligned with the previous 50 years of particle physics theory and history…p. 151 Chapter 7, Particle Physics Theory

   The theoretical foundations for such a discovery were first laid down, almost fifty years ago, by three theorists working in Europe: Englert, Brout and Higgs.

   Not sure who wrote the chapter (for sure European) but leaving out GHK is interesting and political – certainly contrary to history and American Physical Society.

4. **Friend**
   January 18, 2013

   I'm curious to know if there is any plan to accelerate the particles with lasers, or perhaps they already do. Thanks.

5. **Peter Woit**
   January 18, 2013

   Friend,

   See pages 169-170 of the briefing book:

   "This technology is not yet ready for a proposal for a high energy accelerator."

6. **srp**
   January 20, 2013

   The briefing book is an interesting window into how the world looks to the experimental particle physicist. The public is usually exposed only to views of the world presented by theorists. The differences are telling.

   For example, in the hadron physics section there's a lot of discussion of how much empirical work continues to be needed to fill in the picture of how the proton works. Lots of collisions to measure one-dimensional parton distributions are prescribed, and on the cutting edge it's suggested that it might be cool to find out how everything fits together in two or three dimensions. OK, then, but could we stop hearing about how the existing theory is so darn perfect that we have nothing to work on?

   I don't think that the average citizen who reads popular science articles realizes that the existing theories, models, and data are insufficient to explain or predict the quantitative properties of the proton and its structure. The CERN briefing even has a throwaway paragraph about how the spin of the proton isn't close to being derivable from measured spin components of the quarks and gluons. Oh. Shouldn't we be a little bit curious about that?

   Here's a safety tip for physicists who want public money for continued experimental HEP research: Don't shoot yourselves in the foot by exaggerating the level of understanding already achieved by theory. And don't kid yourself that the public will be more enthralled by far-out speculative stuff than by near-to-hand puzzles about things they've heard of and think they partly understand. "There are big gaps in our understanding of how the proton is constituted" might actually be an easier sell than "We think there are a bunch of new particles we can make at higher energy that would cancel out some of the nasty parts of our equations for the particles we can already see." Of course, people in physics would actually have to care intensely about how the proton works to say it with a straight face, and unfortunately I'm not as sure as I used to be that that is the case.

7. **Peter Woit**
   January 20, 2013

   OK srp, that's now the third comment insisting on the importance of studying poorly understood aspects of QCD. You've made your point, enough.

The Anatomy of a Scientific Gossip

January 23, 2013
Categories: Uncategorized

The University of Birmingham has put out a press release today about new research by their computer scientists, on the topic of the spread of gossip about the Higgs via Twitter. This is all based on an arXiv paper, The Anatomy of a Scientific Gossip, and has been picked up by New Scientist, Phys.org, and Aidan Randle-Conde.

Since I've been designated as one of the Best Physics Gossips on this topic:

> If the Higgs boson was a dead celebrity, Woit would be your TMZ — first to the scene, first to break it, and have it be right.

I think I should perhaps comment on what this research actually shows. From what I can tell, it just provides evidence that Twitter is a worthless swamp full of people who have no idea what they are doing "re-tweeting" stale information to each other. Getting their information from tweets, according to these researchers things began with

> **Period I:** Before the announcement on 2nd July, there were some rumors about the discovery of a Higgs-like boson at Tevatron;

and went on from there. They start looking at the data only from July 1 on.

Looking back at what actually happened, I started posting about the coming LHC results on June 17 (the Tevatron results were a side-show). On June 18th, Matt Strassler had the story, accusing me of ruining the CMS and ATLAS blind analyses, for top-secret reasons that could not be revealed. June 19th saw a New York Times story about this with a link to my blog entry and by June 20th Sean Carroll and Jennifer Ouellette were writing about #HiggsRumors being a "Trending Topic" on Twitter.

I suppose it's true that a couple weeks later there were about a million tweets about this, but why would you conceivably want to look at any of them? While I was writing this blog posting, an incoming e-mail from Twitter popped up on my screen.

> **We've missed you on Twitter!**
>
> So much is happening right now on Twitter, and building a great timeline is the way to really enjoy the service. Get to Twitter and start building a timeline that reflects you and your interests, you'll see how quickly Twitter becomes an invaluable part of your life.

I don't think so…

Update: At his blog, Matt explains that he wasn't accusing me of anything. It was CMS and ATLAS physicists who, by telling me me about the results after unblinding, were guilty of ruining the blinded analyses for still top-secret reasons.

Comments

1. **Stephen Brooks**
   January 23, 2013

   The tweets aren't the content, they're usually links to the content. I see Twitter like a personalised newswire: you follow people and sources that tend to flag up the interesting news. You'd not want to read 100 Higgs tweets, you'd follow the link to the CERN webcast, article or blog that just came out.

2. **twit**
   January 23, 2013

   You could nominate it for an IgNobel prize in the category of "Useless Research Spawned by the Higgs Boson".

3. **Chris Oakley**
   January 23, 2013

   Twitter is for twits, by definition.

4. **srp**
   January 23, 2013

   The unkind might equate Twitter to the U.S. CB radio phenomenon of the 1970s, only with hipsters instead of truckers getting the ball rolling.

5. **P**
   January 23, 2013

   Peter,

   "first to the scene, first to break it, and have it be right."

   In my experience, this was definitely true. The experimenters (Jester, Dorigo, etc) tended to be more tight-lipped, and I have always reading your blog on the hep-ex news. Keep up the rumor posting, as they come along!

   P

6. **Peter Woit**
   January 23, 2013

   Thanks P,

   Actually, Jester is a theorist, not an experimentalist, and an excellent source of reliable gossip about the Higgs and other experimental news. I may have done a bit better than him on the Higgs, but he's got far better sources and much better knowledge of what is going on in CMB and dark matter-related news (see his recent postings).

7. **Xezlec**
   January 24, 2013

A worthless swamp if you consider informing the public worthless. The fact that that "stale" information was new enough to them that they felt compelled to repeat it shows that those tweets were useful to them. Twitter works just like word-of-mouth, except without as much distortion at every stage, so you may as well say that talking to people in person is worthless.

8. **David Nataf**
   January 24, 2013

   Xezlec,

   Have you ever used more than 140 characters when talking to people in person?

9. **twitter**
   January 24, 2013

   Twitter has done nothing to help human kind. The brilliant people working there could have been doing something useful instead. It is a shame.

10. **Robert Rehbock**
    January 24, 2013

    The capacity for anyone to publish and republish anything true or falseby essentially anonymous means can be both a blessing and a curse.
    I do not agree though with Strassler in either his original post or the subsequent exchanges. Unlike exposure of means of calculation or the setup of the experiment that could influence others behaviors, the data cannot be changed once the group gathered it. All that can by the other group be learned is that their results did or not agree with completed observations and work.
    as tosecrets – a secret is something known to one person. something known to two or more can be at best confidential. One may have little confidence that anything known by many can be kept off the Internet.

11. **tulpoeid**
    January 27, 2013

    I personally think another phenomenon is a lot more noteworthy and maybe research-worthy, from a social perspective; the fact that ~15k people all over the world knew about something that was red-hot press material, nobody was making them keep their mouths shut, but they nevertheless went on to do so for months only out of respect for their own and their colleagues' work.
    Is this fact ever going to be properly acknowledged?

Short Book Reviews

January 24, 2013
Categories: Book Reviews

There's now a fairly long list of books that I've found worthwhile recently and wanted to write about here, making it unlikely I'll have time to write in detail about them. Instead, here are some short reviews:

- More than seven years ago I wrote very critically here about Leonard Susskind's The Cosmic Landscape. That book struck me as embodying the worst aspects of where string theory has ended up, promoting to the public in a high-profile way a dangerously pseudo-scientific excuse for string theory's failure. Debate about the anthropic landscape has now been going on for nearly a decade, with mixed results. This ideology still has its believers and gets taken seriously, but I think it's fair to say that interest has dwindled as it has become clear that no one has a serious idea about how to use it to make any kind of scientific prediction. For both proponents and opponents, it's now old news, hard to get interested in talking about, especially since the lack of any evidence (pro or con, now or forever) seems guaranteed.

  Luckily for all of us, Susskind has moved on to much more promising topics. He has a new popular book out which is quite good, entitled The Theoretical Minimum. It's basically a textbook on classical mechanics, written at a level appropriate for someone who has had a calculus class, but not necessarily any more physics or mathematics than that. The style is breezy and colloquial, with lots of nice explanations of some of the basic concepts of physics. It's wonderful to see Poisson brackets appearing and nicely explained in a popular book destined to be displayed at bookstores everywhere.

  The book is based on one of several series of lectures given by Susskind as part of Stanford University's Continuing Studies program, all of which are available on video at YouTube (see here for a list). The writing of the book is a joint effort of Susskind and George Hrabovsky, who started the project of turning Susskind's lectures into book form.

- While selling popular books with equations in them is a new concept in the US, it's not so unusual in France. When last in Paris I picked up a copy in a non-scientific bookstore of Cédric Villani's Théorème Vivant, which includes equations I can't even follow. It's basically a fascinating journal he kept during 2008-2011, focused on a problem he was working on during this period with his collaborator Clément Mouhot. It provides a good picture of what it's like to be a top-class analyst working on a difficult problem. During this period, Villani was very much aware that he might be a candidate for a Fields Medal, which provided some motivation for him to push forward. If you want to know what it's like to really want a Fields Medal, to work hard to get it and succeed, this is the book for you.

  A large part of this work took place during a year when Villani was holed up at the Institute in Princeton, and this is described in detail. Difficult working conditions included lack of access to good bread or cheese, a major reason Villani turned down efforts by Princeton to keep him there and returned to France, where he is now Director of the Institut Henri Poincaré in Paris. He also maintains a blog here where you can keep up with his activities.

- Steven Weinberg's Lectures on Quantum Mechanics is based on graduate-level quantum mechanics courses he has taught over the years. It covers concisely and well most of the standard topics that are make up a quantum mechanics course at this level (this is definitely not a beginning QM book). It does differ from most QM books though in providing a high-level and serious discussion of the question of interpretations of quantum mechanics, a topic about which Weinberg has thought deeply. After explaining carefully the issues, he ends up with:

  > My own conclusion (not universally shared) is that today there is no interpretation of quantum mechanics that does not have serious flaws, and that we ought to take seriously the possibility of finding some more satisfactory other theory, to which quantum mechanics is merely a good approximation

  I fear I'm with those who don't share this conclusion, but his arguments are well-worth paying attention to. For someone else who has thought deeply about all this, and come to conclusions closer to my own less well-considered ones, see this recent blog entry by John Preskill (don't miss the discussion in the comments).

  The book ends with a modern but very short chapter on entanglement, Bell inequalities and quantum computation.

- I've recently gotten a copy of a wonderful new quantum field theory textbook, Anthony Duncan's The Conceptual Framework of Quantum Field Theory. It's a long, fat book, packed with material that doesn't appear in other QFT books. Most modern QFT books stay focused on the goal of writing down the Standard Model and giving the details of how to do perturbative calculations in the theory. Duncan instead devotes most of the book to a careful investigation of the basic issues raised when one works with a theory of quantized fields and tries to understand exactly how such objects are connected to the particle states and their scattering that we see in the real world.

  Besides the close attention to thorny conceptual problems normally glossed over, Duncan also gives a long discussion of the early history of the subject, a time in which the conceptual problems were being thought about by the leading figures in the field. Probably every one who has learned quantum field theory in one way or another could benefit by going through this book and picking up some insight into all the questions that were ignored in whatever other book they learned the subject from.

- Finally, for those already fluent in quantum mechanics and quantum field theory, there is Mikhail Shifman's Advanced Topics in Quantum Field Theory, published last year. It concentrates on methods for understanding the non-perturbative behavior of QFTs, especially gauge theories. A major topic is semi-classical methods and the art of extracting non-perturbative information about the QFT from interesting solutions to the classical equations of motions (e.g. instantons and solitons). The latter part of the book focuses on supersymmetric theories, where supersymmetry can be used to get further insight into the non-perturbative behavior. In recent years, much of the research interest in SUSY has moved away from the idea of using it for Beyond Standard Model physics (a trend likely to accelerate with the failure of SUSY to show up at the LHC), and towards thinking of it as a tool for studying QFTs. Shifman's book gives a good introduction to the basic examples of how this works.

Update: Lev Okun sent me a copy of his ABC of Physics: A very brief guide. It's a remarkable document, managing to cover all of fundamental physics in about 120 pages, from the simplest topics in high school physics to the Higgs and superstring theory (the latter treated with appropriate skepticism). If you want an overview of the subject that is as short as possible, this is for you.

Comments

1. **Bernhard**
   January 24, 2013

   "we ought to take seriously the possibility of finding some more satisfactory other theory, to which quantum mechanics is merely a good approximation"

   Interesting to see this coming from Weinberg. It seems to contradict a younger Weinberg who wrote "Dreams of a Final Theory". I remember one of the points he made in that book was that QM was here to stay, and not an approximation of anything more fundamental. Apparently, he changed his mind.

2. **lev**
   January 25, 2013

   The place of the higgs particle in the framework of the Standard Model of Elementary Particles is described in the book "ABC of Physics: A Very Brief Guide" by Lev

Okun.
See Amazon.com for Editorial Reviews by Michael E. Peskin (SLAC National Accelerator Laboratory) and Robert P. Crease (Stony Brook University, Physics World) and Customer Review by traveller2. The book was published by World Scientific on March 27 2012

3. **Douglas Natelson**
   January 25, 2013

   Thanks for these reviews – you're providing a real service (though my damn reading list never seems to shrink).

4. **Shantanu**
   January 25, 2013

   Peter , any comments from the higgs workshop at Edinburgh?

5. **Peter Woit**
   January 25, 2013

   Shantanu,

   Nothing really new there. More news about the Higgs should come in a month or so, once the experiments have results ready for announcement at Moriond in early March.

   I suppose I could write about SUSY, but thought it better to give that a rest for a while…

6. **Spencer Tracy Jr.**
   January 25, 2013

   Although I don't agree with a lot of Susskind's theories – I met him 2 years ago in NYC after his panel discussion at the World Science Fest and found hime to be very friendly and very generous with his time. I have also enjoyed several of his youtube lectures on GR and Higgs.

7. **Kavanna**
   January 25, 2013

   Susskind is very friendly, no matter what you think of the landscape and the anthropic principle.
   Should have published those classical mechanics notes from his class! When I was his student at Stanford, he was an idiosyncratic teacher, and you could not expect any hand-holding. Not only were c and h-bar set to unity, but dimensionless factors too: 2, 1/2, pi, i, etc. We called these supernatural units.

   One hopes that Hrabovsky has parsed the lectures into a form easier to digest.

8. **Yatima**
   January 25, 2013

   But i is already unity, and how can π be unity? That doesn't even make sense…

9. **1=2=i=pi**
   January 26, 2013

   When did the supernatural ever make sense?

10. **MathPhys**
    January 26, 2013

    "lack of access to good bread or cheese"

    hehehehe best reason one can think of to go back to France.

11. **tulpoeid**
    January 27, 2013

    I'm sincerely interested in your conclusions on QM interpretation, could you post a link to relevant entries? (Of course the Preskill link is not bad either.) In any case I'm delighted to finally see someone as large as Weinberg not sticking to many worlds and other funny stories.

12. **Peter Morgan**
    January 27, 2013

    I second your recommendation of the Anthony Duncan.

13. **Harry**
    January 27, 2013

    About Villani's book, to be completely fair, books with such-high level equations are not that common here in France ("Théorème Vivant" must be quite unique in this category).
    Also, personnally I thought the bread-and-cheese argument was some kind of joke, but maybe he was serious about that after all..

14. **Peter Woit**
    January 27, 2013

    tulpoeid,

    About my personal views I don't think I can do better than the link to Preskill, who does a much better job than I can of explaining this point of view. In the online notes for my QM class one can find some other references about this.

    Harry,

    I think Villani's comment was partly tongue in cheek, but also partly serious. There's a big difference between living in Paris or another French city, and living in a rather isolated American suburb like Princeton. The bread and cheese problem there is real, but this also stands in for lots of other things one misses by living in a place like Princeton.

15. **David Metzler**
    January 29, 2013

    The link to Preskill eventually led me to Fermilab's Holometer. I've also seen a similar proposal using the LIGO equipment. I can't tell how likely that is to pan out… any thoughts?

16. **Peter Woit**
    January 29, 2013

    David,

    I don't know about the LIGO proposal, but for something about the holometer, see here
    http://www.math.columbia.edu/~woit/wordpress/?p=4572

This Week's Finds

January 26, 2013
Categories: Uncategorized

Twenty years ago this past week, John Baez posted the first of his "This Week's Finds in Mathematical Physics" to the sci.physics newsgroups, inaugurating internet blogging about Mathematical Physics, many years before anyone even knew what a blog was. For his first posting, try looking at
http://math.ucr.edu/home/baez/twf_ascii/week1
and for all the rest of them see
http://math.ucr.edu/home/baez/TWF.html

There's a huge amount of interesting material in John's TWF postings, and the amount of effort that he has put into providing detailed, clear explanations on all sorts of topics is kind of staggering. While I often try to emulate what John has done (and "This Week's Hype" of course is a sort of homage, one he may not appreciate...), I feel I'm doing well if I can manage to put together a few sentences of comments about a "Find", with John's much more useful detailed expository work something beyond my capabilities.

For a "Find" from this past week in mathematical physics, I can recommend Hermann Nicolai's "Quantum Gravity: the view from particle physics" (arXiv:1301.5481) a write-up of his lecture this past summer at a conference in Prague. He makes a point about quantum gravity that I very much agree with: the problem with the subject is not so much that of finding a consistent quantum gravity, but of finding one that fits together with the SM and tells us something new that we can check. He writes:

> Being exposed to many talks from the different 'quantum gravity camps' I am invariably struck by the success stories I keep hearing, and the implicit or explicit claims that 'we are almost there'. I, for one, would much prefer to hear once in a while that something does not work, and to see some indications of inconsistencies that might enable us to discriminate between a rapidly growing number of diverging ideas on quantum gravity . If, however, the plethora of theory ambiguities were to stay with us I would conclude that our search for an ultimate explanation, and with it the search for quantum gravity, may come to an ignominious end (like in Breughel's painting).

and

> To conclude let me restate my main worry. In one form or another the existing approaches to quantum gravity suffer from a very large number of ambiguities, so far preventing any kind of prediction with which the theory will stand or fall. Even at the risk of sounding polemical, I would put this ambiguity at 10^500 (or even more) – in any case a number too large to cut down for any conceivable kind of experimental or observational advance.

Included in his talk are various more specific comments about these issues, well worth pondering. If I were John Baez, I'd have the energy to describe them in detail and explain clearly exactly what is going on, but for this I fear someone will have to get John more interested in quantum gravity again...

Update: I should mention other tributes to TWF here, here, and here.

Comments

1. **E**
   January 26, 2013

   What? I can't really read font.

2. **johnmcAllison**
   January 26, 2013

   John Baez, Chris Hillman and Dr Kevin Brown were the three main figures from the 1990s on sci.math that I fondly remember.

   Dr Kevin Brown still hosts Math Pages http://mathpages.com/home/index.htm

   whereas Chris Hillman mysteriously deleted his Relativity on the Net page in 2007.

3. **Franco**
   January 27, 2013

   Any model of quantum gravity can be tested only against one fact: the entropy of black holes. A good friend who works in quantum gravity once summarized the situation in a simple way: there are zillions of models that reproduce the entropy of black holes and gravity, but there is no model that explains the other interactions.

   It is very unlikely, therefore, that a good theory of everything will appear through quantum gravity research.

4. **Giotis**
   January 27, 2013

   I think Nikolai is too pessimistic and on the other hand too demanding.
   A suitable candidate for a theory of evereverything (as a working project) should

   1. be able to reproduce in principle the theoretical content of the low energy effective world i.e. GR+QFT

   2. incoproprate natuarally all the ingridients needed for the derivation of SM as we know it (Fields, gauge groups etc).

   3. be able to treat Gravity quantum mechanically

   4. exhibit mathematical and theoretical consistency

   5. be consistent with current experimental data

   String theory (and String theory only BTW) fulfills the above criteria and so the theory should be taken seriously as a base of research for a unified theory.
   At some point of development the theory should be able to offer theoretical solution to known problems of GR+SM e.g. singularities , EW scale stabilization, fate of

spacetime at planck scale, resolution of black hole information paradox, dark matter candidate, the CC problem etc

String theory has made considerable progress answering some of these questions.

Finally it would be great if the theory could make defintive testable, predictions at low energies but we don't know if this is possible even for the correct theory.

Thus all in all it looks good for String theory in my opinion.

5. **Bob Jones**
   January 27, 2013

   "Any model of quantum gravity can be tested only against one fact: the entropy of black holes."

   No, a theory of quantum gravity definitely cannot be tested using black holes because we will probably never be able to measure the entropy or temperature of a black hole. There are, however, other ways in which we could potentially observe quantum gravitational effects. For example, it is conceivable that quantum gravitational effects in the early universe could lead to testable predictions about the structure of the present universe. In addition, many people believe that the strong and electroweak interactions will be unified at a length scale which is only a few orders of magnitude larger than the Planck scale. Thus, it is possible that quantum gravity could play an important role in the unification of the other forces.

   "there are zillions of models that reproduce the entropy of black holes and gravity"

   Not really. The only models that successfully reproduce the semiclassical result for black hole entropy are models based on the holographic principle, including string theory. In other approaches, like loop quantum gravity, the situation is not at all satisfactory because the theory has a free parameter (the Immirzi parameter) which needs to be adjusted to a certain value in order to get the correct result. In fact, this is why string theory is such an important tool for studying quantum gravity. It provides a concrete realization of the holographic principle and a paradigm for doing microscopic calculations of black hole entropy.

6. **Peter Woit**
   January 27, 2013

   Enough with the tedious hype about string theory and how it will someday magically be testable. I wrote about Nicolai's talk because it's clear he's given up on such nonsense and is trying to get beyond it, which is interesting. How about reading what he has to say and thinking about it instead of repeating BS?

   The argument over black hole entropy calculations wasn't interesting when it was news, and there are probably now students here at the university who weren't even born when these calculations were done. Enough.

7. **Bob Jones**
   January 27, 2013

   Peter,

   I'm not sure if you're talking to me, but just to be clear, I am not suggesting that string theory will become "magically testable" in the near future. I did mention that quantum gravity could play a role in the unification of the forces and that this could lead to testable predictions from quantum gravity, but it sounds like you would agree with me about that.

8. **Shantanu**
   January 28, 2013

   anyone know what happend to chris hillman? is he in acadamia He had such mastery over GR ?

9. **Jeff M**
   January 28, 2013

   Well, after the Byrds, and The Flying Burrito Brothers, he bounced around and is currently touring with the Desert Rose band… 🙂

10. **JG**
    January 28, 2013

    Chris Hillman got upset about the way the web was developing (too many cranks), posted negative opinions about wikipedia and the like (he had initially contributed many articles), and decided to delete his site and go offline. But you can still access his old relativity site (hosted by John Baez) on the internet archive here

    He contributed to physicsforums until the end of 2007, posted comments on some math related blogs (including this one) in 2008 and then seems to have disappeared completely?

11. **Igor Khavkine**
    January 28, 2013

    Nicolai's article doesn't harbor any surprises. It certainly captures the current attitude of particle theorists, who seem to consider quantum gravity (QG) as some kind of panacea, or merely a synonym for what is currently unknown. Unfortunately, the state of knowledge reflected therein is most often of particle physics folklore, rather than the technical state of the art. One could nit pick at any number "issues" addressed by Nicolai as either requiring QG for their resulution or providing a clue as to how QG should be and in turn argue that they are not necessarily problems at all or have little to do with gravity.

    There is one particularly opaque and unfortunately inaccurate statement at the end of section 6 on Anomalies. He correctly points out that anomalies are a serious worry in quantization, especially for gauge theories. This is followed by a remark about the possible anomalies occurring in the diffeomorphism algebra in 2d theories (incidentally, of which the string worldsheet theory is an example). Unfortunately, that remark is followed up by another one, namely that we do not have a classification of diffeomorphism algebra anomalies in higher dimensions, which is actually incorrect. The Belgian group of Henneaux, Barnich and Brandt worked out in the mid-to-late 90's that, in in most dimensions including 4d, the SM+GR does not have any new anomalies compared to just the SM. Similar information can also be found in volume 2 of Weinberg's QFT treatise.

    Having been in the audience, I drew attention to this point in the question period after Nicolai's talk. However, my remark does not seem to have had much impact.

12. **DrDave**
    January 28, 2013

    Nor sure if this is the painting he is referring to, but fits the bill http://en.wikipedia.org/wiki/The_Triumph_of_Death

13. **Yatima**
    January 29, 2013

    How about The Tower Of Babel. This image was used in Carlo Rovelli's book or maybe Laurent Nottale's, not sure.

14. **Perry**
    January 29, 2013

    The font was a nice touch

    Alt.physics.woit.

15. **Izzy**
    January 30, 2013

    I think it's Breugel's "The Fall of Icarus", the subject of a famous poem by Auden.

16. **Raizonator**
    February 2, 2013

    "He makes a point about quantum gravity that I very much agree with: the problem with the subject is not so much that of finding a consistent quantum gravity, but of finding one that fits together with the SM and tells us something new that we can check."
    Exactly, that I think is what is often overlooked: Quantum gravity is not primarily the unification of q.m. & gravity BUT the SM + gravity, the former bringing in a lot of interesting geometry.
    I just watched this video, where this is motivated very nicely
    http://www.youtube.com/watch?v=j8-7nOed_aE&list=PLHX6aNQ5-1N-ZIpkJ0OVOaqyAMOfDZI1v&index=2

17. **John Baez**
    February 19, 2013

    Thanks! For a second I thought that font was the result of some error.

    Chris Hillman began his internet career as a relentless scourge of crackpots, but then became increasingly scared of them, probably due to threats he received. He then tried to expunge every trace of himself, for example demanding that I remove his page *Relativity on the World-Wide Web* from my website.

    He doesn't have an academic career, and when I last checked he was unemployed and ill. He originally had some connection to the math department at the University of Washington, and had an email account there. Later he lost that. More recently all communications I've received from have been via pseudonymous email accounts.

    One moral is that if you plan to spend a lot of time debunking crackpots, you should expect some of them to eventually spend a lot of time attacking you. So, think a bit about whether you have the stomach for this. I decided a while ago that there are better things to do.

18. **John Baez**
    February 19, 2013

    Bob Jones wrote:

    > In other approaches, like loop quantum gravity, the situation is not at all satisfactory because the theory has a free parameter (the Immirzi parameter) which needs to be adjusted to a certain value in order to get the correct result.

    That was certainly true of the old calculations I was involved with, and I agree that this was a frustrating situation. But anyone who is interested should look at the more recent papers by Eugenio Bianchi, especially Entropy of non-extremal black holes from loop gravity.

19. **Mitchell Porter**
    February 19, 2013

    "anyone who is interested should look at the more recent papers by Eugenio Bianchi"

    John, what about the criticism that Bianchi isn't even counting black hole microstates, he's just reproducing Hawking's semiclassical calculation? Hossenfelder, Motl, and others all say this.

Arthur Wightman 1922-2013

January 30, 2013
Categories: Obituaries

I just heard today that mathematical physicist Arthur Wightman passed away earlier this month, at the age of 90. Wightman was one of the leading figures in the field of rigorous quantum field theory, the effort to try and make precise sense of the often heuristic methods used by physicists when they deal with quantum fields. He was a well-liked and very respected professor at Princeton during the years 1979-84 that I was a graduate student there, but unfortunately I don't think I ever made an effort to talk to him, to my loss. The university has something about him here, the department here.

Wightman is most well-known for the "Wightman Axioms", which are an attempt to formalize the fundamental assumptions of locality and transformation under space-time symmetries that any sensible quantum field theory should satisfy. His 1964 book with Raymond Streater, PCT, Spin, Statistics and All That, explains these axioms and shows how they lead to some well-known properties of quantum field theories such as PCT invariance and the Spin-Statistics relation. When this work was being done during the 1950s and early 60s, quantum field theory was considered something that couldn't possibly be fundamental. All sorts of discoveries about strong interaction physics were being made, and it seemed clear that these did not fit into the quantum field theory framework (this only changed in 1973 with asymptotic freedom and QCD). In any case, problems with infinities of various sorts plagued any attempt to come up with a completely consistent way of discussing interacting quantum fields, providing yet another reason for skepticism.

Wightman was one of a small group of mathematical physicists who reacted to this situation by trying to come to grips with the question of exactly what a quantum field theory was, in an attempt to find both the implications of the concept and its limitations. After the early 60s, attention moved from the axioms and their implications to the question of "constructive quantum field theory": could one explicitly construct something that satisfied the axioms? Examples were found in 2 and 3 space-time dimensions, but unless I'm missing something, to this day there is no rigorous construction of an interacting QFT in 4 space-time dimensions. There is every reason to believe that Yang-Mills theory, constructed with a lattice cut-off, has a sensible continuum limit that would provide such an example, but this remains to be shown (and there's a one million dollar prize if you can do this).

Thinking back to the early 1980s and my days as a graduate student, it's clear what some of the reasons were why I didn't spend time going to talk to Wightman. With the triumph of the Standard Model, attention had turned to questions about quantum gravity, as well as questions about the non-perturbative behavior of QCD. I certainly spent some time trying to read and understand the Streater-Wightman volume, but its emphasis on the role of the Poincaré group meant it had little to say about QFT in curved space-time, much less how to think about quantized general relativity. Gauge theories in general did not seem to fit into the Streater-Wightman framework, with the tricky issue of how to handle gauge symmetry something their methods could not address. For non-perturbative QCD, we had new semi-classical computation methods, and I was happily programming computers do numerical simulations of Yang-Mills theory. Why pay attention to the difficult analysis needed to say anything rigorous about quantum fields, when the path integral method seemed to indicate one could just put them on a computer and have the computer tell you the answer?

In later years I became much more sensitive to the fact that quantum fields can't just be understood by a Monte-Carlo calculation, as well as the importance of some of the questions that Streater and Wightman were addressing. As particle theory continues to suffer deeply from the fact that the SM QFT is just too good, anything that can be done to better understand the subtleties of QFT may be worthwhile. It remains true that gauge theories require new methods way beyond what is in Streater-Wightman, but looking back at the book I see it as largely devoted to understanding the role of space-time symmetries in the structure of the theory. The importance of such understanding of how symmetries govern QFT may be a lesson still not completely absorbed, with gauge symmetries and diffeomorphism symmetries part of a story extending Streater and Wightman to the Standard Model, in a way that we have yet to understand.

Comments

1. **P**
   January 31, 2013

   Hi Peter,

   I'm not sure about what precise definitions of "rigorous" would make mathematicians happy, but in discussions over the last few years it seems they don't have as much of an issue with the recent works on supersymmetric localization, and this includes work in d=4. The reason, if I understand, is that they usually take issue with the measure in the path integral, citing that the space of all possible field configurations isn't well defined, but that in some cases supersymmetric will localize the set of contributing field configurations to a nice well-defined space.

   Of course, one could take issue with the fact that the theories are rather formal, but at least there is some progress in some examples.

   Any thoughts on how localization fits into your / other definitions of rigorous?

   Cheers,
   P

2. **A.J.**
   January 31, 2013

   Peter,

   This is sad news. Another giant I'll never meet.

   P,
   The problem isn't that the space of fields isn't well-defined. Finding the space of fields for a given model is the easy bit; it basically has to be the space of linear functionals on the space of sources which the classical fields couple to. Most of these linear functionals are distributions rather than continuous fields, but the space is perfectly well-defined. And in fact, the distributions are necessary: without them, you won't get the right short-distance singularities in the OPE.

   The problem is, first, that it's not easy to show that the path integral measure exists on this space of distributions, and, second, that it's hard to show that the dressed observables are really integrable with respect to the path integral measure. (The latter question is really what the Yang-Mills millenium prize is about: Confinement means that many of the observables from the classical theory _aren't_ integrable; the ones that remain generate a Hilbert space with a mass gap in the Hamiltonian.)

   Supersymmetric localization says that — for some (but not most!) observables — the path integral over the big space of fields reduces to an integral over a finite dimensional subspace. This will someday be a wonderful theorem, but it's not true for enough observables to count as a definition of the measure on the big space.

3. **Peter Woit**
   January 31, 2013

P,

A.J. does a good job of explaining the problem. If you just want a rigorous construction of not QCD, but something like a TQFT, where in principle we have an independent characterization of what all the observables are, in a much more tractable framework, you can get a rigorous 4d theory by just making that framework your definition.

I think the formal application of localization to path integrals is a fantastic idea. Perhaps the best hope for getting rigorous 4d QFTs is to evade the problems A. J. explains by not trying to get a rigorous version of an arbitrary QFT, but reformulating the definition of a QFT to be something for which localization works automatically. Right now though, as far as I know this won't buy one any physically interesting QFTs. One speculation I'm fond of is that if you better understood the representation theory of certain infinite-dim groups, you could reformulate certain QFTs in terms of such representation theory, and get a rigorous definition of an interesting theory that way. Well, it works in quantum mechanics, where you can do the harmonic oscillator that way….

4. **Peter Orland**
January 31, 2013

Though I am not really a pessimist, I am a little wary of focussing exclusively on axiomatic methods to study field theories. These methods seem to be most successful when we understand aspects of theories by other means. I'd be happy to be proved wrong about this.

I would be surprised if localization works for theories with asymptotic freedom. Most of these theories seem rather special. I'm not an expert though.

Most of the theories where the Wightman axioms are proved (using constructive field theory) are super-renormalizable. That means a simple subtraction makes it possible to remove the cut-off. So in a way, constructive methods are most powerful for theories where we can already control renormalization. There is a claim that planar phi^3 in six dimensions is "constructable", but I have not tried to check this. I think there is good reason to be optimistic in 1+1 dimensions where we have a lot of exact information; getting to more dimensions is a long way off.

I also don't expect that the mass gap/confinement problem will first be understood via axiomatic or constructive field theory (though methods borrowed from these subjects, say lattice constructive field theory methods, might be useful).

5. **Peter Orland**
January 31, 2013

But don't get me wrong… I am a big fan of Streater and Wightman and many of Wightman's papers.

6. **deconstructed**
February 1, 2013

Maybe I can offer some opinions (and even some facts):

A.J.: as there are no complex measures, YM doesn't have a Euclidean measure. That does of course not mean, that the Standard Model can't be constructed mathematically.

Peter Orland: The people working on making the SM – as used to calculate predictions for experiments – mathematical is a set of measure zero. The problem is not, that there are some people looking at SM-physics "non-rigorously". The problem is that there are no people doing it rigorously. It certainly will be hard to learn anything "non-perturbatively", if we don't even have constructed the theory we want to know something about.

All: Constructive QFT imho is the strife to construct the SM – again as used for predictions – in a mathematically sound manner. And it shouldn't matter what methods will in the end to success (if that occurs). But in the moment mathematical physics doesn't seem to be set up to achieve this goal, as it is divided into analysis people doing Schrodinger and maybe Dirac operators and algebraists having fun with TQFT. From my point of view it seems unlikely that either one tradition alone will be able to achieve above mentioned goal, which is of course an extremely hard one.

Peter: I am curious about that idea you mentioned regarding "representation theory of certain infinite-dim groups". Can you point me to any sources? Sorry if this bores other people in here.

Anyway, RIP Arthur Wightman and thanks for setting the goal.

7. **A.J.**
February 1, 2013

deconstructed: I'm sorry, but I don't quite understand what you mean by "as there are no complex measures, YM doesn't have a Euclidean measure". Can you clarify?

8. **deconstructed**
February 1, 2013

A.J.: The generating functional for YM is not real-valued, which is a condition of Minlos' theorem, which in turn gives necessary and sufficient conditions for the existence of measures over locally convex spaces.

(Minlos' theorem is the analog of Bochner's theorem for probability measures. Check out Gelfand's Generalized Functions, Volume IV, I think. Or Bourbaki's Integration)

There are also no complex-valued measures on the real numbers.

Sorry that I was a little cryptic there, but I can already hear Grandmaster P booming: "There are plenty a places to discuss measures. Enough."

9. **Peter Woit**
February 1, 2013

deconstructed,

I don't know much about technical issues with measures, but in the case of pure Yang-Mills I believe you should be able to understand the problem as one of showing that an appropriately constructed limit of the theory regularized as some version of lattice gauge theory should exist and have desired properties. I think the point Peter Orland is making is one that I agree with: if you don't have a good physical understanding of the infrared behavior of these theories (and we don't…), no analytical technique is going to solve the problem.

As an example of what I meant by using rep theory of infinite dim groups, consider the Wess-Zumino-Witten model, a 1+1 dim QFT. If you try and naively discretize that and control its continuum limit you will encounter all sorts of problems. On the other hand, the behavior of the theory is largely determined by knowing about the representation theory of a loop group.

10. **Peter Orland**
February 1, 2013

deconstructed:

I have some sympathy with your views, but I am not convinced that your statement, "It certainly will be hard to learn anything "non-perturbatively", if we don't even have constructed the theory we want to know something about," is entirely true.

I suppose it could be so, but precedent suggests not. There are some physical phenomena (in classical mechanics or statistical mechanics) where proving theorems really settled physical questions, so you could be right. The field theories we have solved, however, were first solved by less rigorous methods more familiar to theoretical physicists.

There is a long list of field-theory models (the Lee model, integrable theories, QED with monopoles) which were well understood (by which I mean some observables were calculated) without rigorous methods. Rigorous tools were applied, only afterwards, to a small subset of these models. The only field-theoretic model I can think of which was solved first by constructive methods was Nelson's. Nelson introduced it at the start of the constructive field theory program (it's a model of non-relativistic quantum particles, interacting with a relativistic scalar field).

I am a fan of constructive FT, because it gives a lot of insight (as in phi^4), but its successes in seem to be for models we already had some control over.

11. **deconstructed**
February 1, 2013

First of all I don't think that we have to, or even should, start with lattice gauge theory. Don't get me wrong, if you are able to continue the work of Bałaban and prove results, that would be a gigantic sucess. From a conceptual point of view, however, lattice theories have the distinct disadvantage, that you have to redo the proof for every observable you investigate. I digress.

My point is, that from the outset it should not matter where we start, as long as the starting point is mathematically sound.

The term "good physical understanding" is key here imho. If it's meant to be "knowlege gleand from calculating Feynman diagrams", I agree – we don't have a lot of that.

But we do have Bałaban's work, which all but proves the existence of the YM partition function in the lattice framework. And as such it does have a lot of information not only on the ultraviolet behaviour but also on the infrared behaviour of YM.

Anyway, we have to find an approximation of SM where we can control the infrared behaviour. So we should try a few. Except for the ones we already know don't work, obviously. Your WZW example is just one of many indicators, showing that there are a very large amount of bad approximations.

Also, there are a lot of interacting theories in 2d, where groups don't help you at all. Hence what I said before, that we should throw everything in the ring we have, everything from algebra to analysis, groups to spaces.

Regarding the "rep theory of infinite dim groups" thing: where you being ironic when you said it worked for the harmonic oscillator in QM? If not, can you point me to a document I can read to understand this? I would like to understand the very basic idea you have.

Greetings!

12. **Peter Orland**
February 1, 2013

To add to what Peter W. said about Witten-Wess-Zumino models…

Much the same can be said about other 1+1-dimensional theories. We know the
S matrix and some off-shell information exactly. I think that most people working on these expect that these results will eventually lead to a rigorous definition. This is done without the benefit of a functional measure or a Lagrangian.

13. **deconstructed**
February 1, 2013

Peter Orland: Your post came, while I was answering Peter's. I shall gather my wits and reply to yours asap.

14. **A.J.**
February 1, 2013

deconstructed:

Yes, I know what Minlos' theorem says. I gave a class on it once, for whatever that's worth…

And I don't think there's anything wrong with talking about constructive field theory in a post memorializing Arthur Wightman. So if you'll allow me to pester you a bit more… I still don't quite know what you're trying to convey to me with your comment, and I'd be happy to learn. In particular, in lattice SU(2) Yang-Mills, everything physically relevant looks real-valued to me. The Wilson action is real, and the Haar measure on gauge fields is real, so the lattice path integral measure is real. The observables should all be generated from characters of products of holonomies, and the characters of SU(2) are real valued. (Forgive the restriction to SU(2). I thought it best to get concrete.) So it looks to me like the relevant generating functional is also real-valued.

So what precisely about my comment are you objecting to?

15. **A.J.**
February 1, 2013

As a side comment: I doubt the Standard Model can be constructed mathematically. The Higgs sector is going to cause trouble.

16. **deconstructed**
February 1, 2013

Well I didn't say it would happen that constructive theory is faster than any heuristic approach, Mr Peter Orland. I'm just saying, that it will get harder and harder to glean information about the complete theory without having the full theory. Do you contest, that perturbative calculations about QCD – strongly interacting as it is – are getting harder by the loop?

But that's not even the issue. Even if you could calculate more and more diagrams, we're not even sure that our theory is approximated anymore by the stuff we calculate. (I think recent quark gluon plasma observations at the LHC indicate that the strong interactions do in fact cause deviations from the calculations, if you don't get goose bumps from some minor conceptual issues as long as the "predictions" fit.)

Can you have predictions out of a theory, that has no mathematical basis(, yet)?

17. **Peter Orland**
February 1, 2013

Hi deconstructed,

No, I can't do any of the things you ask. I am just saying that QFT is a complicated problem and we need to view it from with many different angles. Usually insight comes first, then theorems. Constructive FT is usually better for the latter, which I agree is important. Balaban's stuff is great (though I can't make the Polish "w" on my keyboard), but so are other ideas.

We can't just be hammers (and hit nails) or screwdrivers (and provide OJ+vodka). We have to able to do everything.

Regards,
P.O.

18. **KP**
February 1, 2013

Since 'rigorous' vs 'non-rigorous' methods are being discussed, it seems the right place to ask the following:

How can we trust 'non-rigorous' methods that have little mathematical justification while dealing with QFTs that have no observational support? That is, I can see that all our non-rigorous QFT methods can be justified for the SM because it fits with experimental data. But, is there any justification to using the same methods for things like SuperYM?

What if, the SM is a very special case where the non-rigorous methods just happen to give the same answer as the hypothetical rigorous formulation of QFT?

19. **deconstructed**
February 1, 2013

I can certainly see why some fields move faster than others.

A.J.: You'll have to excuse my very brief comment at the beginning. Our discrepancy shows how differently we think, even if we both seem to both come from the mathematical physics community.

What I meant was, that starting from the classical Yang-Mills differential equations (or density), when you build a "time-zero-algebra" in order to go to Euclidean space and quantize (Osterwalder-Schrader reconstruction) the "measure" you encounter will not from the outset be real-valued. At this stage you're not even allowed to use gauge invariance, because that's what you want to prove for your quantized theory. Of course as soon you have constructed the whole theory you should be able to find a "section" through your solution space aka predictions, where everything you're interested in is real (although that doesn't ensure the existence of the measure, as you know).

Of course, starting from an approximation which is already obviously supporting a measure will have certain advantages. But other approximations not supporting a measure might have other advantages.

While writing this it ocurrs to me, that calculations for the LHC are probably done using lattice gauge theory. Maybe I should reevaluate my starting point.

20. **deconstructed**
February 1, 2013

KP: All your questions are excellent.

21. **A.J.**
February 1, 2013

@KP: Which non-rigorous methods do you have in mind?

22. **KP**
February 1, 2013

@AJ
For the sake of being concrete, how about path integrals? If I am not mistaken, it is not really clear that the "measure" on the space of field configurations even exists in a mathematically rigorous sense.

In any case, the Wick 'rotation' used to define the partition function is only valid in stationary spacetimes. So, we know at least one situation in which the path integral is ill-defined but QFT supposedly exists.

23. **A.J.**
February 1, 2013

@deconstructed: Thank you for the comments. It's educational to see other perspectives.

Regarding LHC, from what I recall, it's a mix. The highest energy processes can be treated perturbatively, but as you get away from the beam collision point, you need numerical simulation to understand what's going on.

24. **Igor Khavkine**
February 1, 2013

Speaking of 'rigor' in QFT. I want to make a technical remark that may seem like a triviality, but I think is important for the perception of the mathematical foundations of QFT.

It is of course true that physicists often use heuristic mathematical manipulations in their calculations. And just as Peter Orland has pointed out, these calculations are replete with insights that are then routinely turned into completely rigorous proofs by mathematical physicists and mathematicians (it is also not excluded that the same person could wear more than one of these hats). So there is no particular lack of rigor in the mathematical treatment of QFT. And there are examples where I think rigorous methods do correct erroneous thinking that was guided by more naive heuristic methods.

What is true is that these rigorous proofs do not necessarily establish the exact results that we would want. Namely, the best physical predictions that we can get out of the Standard Model (though fully rigorous) are not numbers but rather formal power series in the theory's coupling constants. It really doesn't matter whether people use path integrals (whose mathematical foundations are "shaky") to arrive at these answers, because by now there do exist fully rigorous (though somewhat different) methods that are known to give equivalent results.

What is missing is not rigor but 'strictness' (using 'strict' as the opposite of 'formal'). By that, I mean that we are not currently able (with a few low dimensional exceptions) to replace the above mentioned formal power series by actual functions, whose asymptotics these series represent. And until we do, as already brought up by deconstructed and KP, we do not have reliable estimates on the errors that we get by truncating the asymptotic power series. That is, until then, we do not have reliable error bars around our theoretical predictions.

This lack of 'strictness' is the real problem with the foundations of QFT. Unfortunately, I think it is rather hard to predict at the moment whether it will be solved by a satisfactory construction of field-theoretic path integrals or by some other means. I think it would pay not to be dogmatic on this point.

25. **A.J.**
February 1, 2013

KP:

I think the best answer I can give is that it's a good idea to use methods that have been tried in more than one experimentally tested physical model, that it's safer to use methods which make sense in regularized approximations, and that it's safest to use methods which have been shown to work in rigorous examples.

The path integral is one of these. It works in QED, QCD, the Standard Model, and probably dozens of low energy approximations to these, like the Skyrme model. It's grounded in lattice path integrals which arise by repeatedly inserting resolutions of the identity into correlation functions, which is about as mathematically kosher as it gets. The continuum limit of these lattice approximations has been constructed rigorously in a number of low-dimensional examples, and in these examples, everything works pretty much as the physicists think it should. This gives me some confidence that what works for Standard Model calculations also works for SO(99) gauge fields with Dirac fermions in some weird representation.

26. **Thomas**
February 1, 2013

I am a little confused by this discussion. There are no people working on rigorous methods for the standard model (SM), because we do not expect that the SM can be rigorously defined (because of the Higgs sector and the U(1) gauge group). We believe that QCD can be rigorously defined, and we expect that chiral gauge theories can be defined as well. We think that in the case of QCD we already have a construction (as a limit, via euclidean lattice gauge theory), but of course there is no proof that the limit exists. I am not quite sure what the consensus is regarding chiral fermions.

There are a number of non-perturbative results in 4d that have been proven using physicist's methods (not as many as we would like, obviously), for example the Seiberg-Witten result for the low energy effective action of N=2 SUSY YM, and proving these results is an obvious goal for more rigorous methods.

27. **Alexander Dynin**
February 1, 2013

Dear Professor Woit,

The heyday of constructive QFT during 1970's stopped short before 4
dimensions with a general supposition of triviality for scalar fields.

The Clay institute problem looks as Jaffe's attempt to revive it for Yang-Mills fields.

Unfortunately, even modified Wightman axioms (see, e.g., Chapter 10 of Bogoliubov, N. N., Logunov. A. A., Oksak, A. I., and Todorov, I. T., "General Principles of Quantum Field Thory, Kluwer, 1990) are in a serious conflict with the simplest cases of Gupta-Bleuler theory of quantum electromagnetic fields, as well as commonly used local renormalizable gauges (see, e.g. Strocci, F., S., "Selected Topics of the General properties of Quantum Field Theory", World Scientific, 1993.)

There was a vivid discussion among W. Heisenberg, P. Jordan, and W. Pauli of the corresponding "Volterra mathematics" for possible applications to functional Shroedinger operators.

This approach has been realized in my solution of Yang-Mills Millennium problem (the latest version "Mass gap in quantum energy spectrum of relativistic Yang-Mills fields", arXiv:1205.3187).

The paper abstract:

A non-perturbative and mathematically rigorous quantum Yang-Mills theory on 4-dimensional Minkowski spacetime is set up in the framework of a complex nuclear Kree-Gelfand triple. It involves an infinite-dimensional symbolic calculus of operators with variational derivatives and a new kind of infinite-dimensional ellipticity.

In the temporal gauge and Schwinger first order formalism classical Yang-Mills equations become a semilinear hyperbolic system for which the general Cauchy problem (with no restriction at space infinity) is equivalent to one with a family of periodic initial data. Yang-Mills quartic self-interaction and the simplicity of a compact gauge Lie group imply that the energy spectrum of the anti-normal quantization of Yang-Mills energy functional of periodic initial data is a sequence of non-negative eigenvalues converging to infinity and, by caveat, has a mass gap at the spectral bottom. Furthermore, the energy spectrum (including the mass gap) is self-similar relative to an infrared cutoff: it is inversely proportional to the initial data period.

According to Wikipedia, "Since 2009, Alexander Dynin claims to have proved the Yang- Mills Millennium Problem. Nevertheless, the physics community seems to be turning a deaf ear to him,
apparently because they feel incompetent to assess his unorthodox mathematical methods."

Regards,

Alexander Dynin

Professor of Mathematics,
Ohio State University

28. **Nakanishi**
February 2, 2013

Dear Professor Dynin:

Although I do not know whether or not Wightman functions are definable in your theory, I would like to note that there are serious mathematical difficulties in formulating gauge theories in axial-type (i.e., axial, light-cone and temporal) gauge in the Wightman-like formalism. For a review, please see
N. Nakanishi, Critical Review of the Theory of Quantum Electrodynamics, in T. Kinoshita (ed.), Quantum Electrodynamics (World Scientific, 1990).

29. **KP**
February 2, 2013

@AJ

I agree with your view that using the tools we already have is a good idea. At the same time, I feel that finding a more rigorous formulation is also warranted.

@Thomas
Do you mean to say that if the SM is the correct theory excluding gravity (as the LHC results seem to indicate thus far), then we will be stuck with a 'correct' physical theory that can not be hoped to be made mathematically rigorous? Or am I understanding this wrong?

30. **deconstructed**
February 2, 2013

Thomas: Just because "we believe", or rather, just because there are heuristic calculations suggesting, that the SM is not a mathematical theory, that doesn't mean nobody should work on settling the question. I want to know whether it exists or not and I hope I'm not alone with that wish.

31. **John Baez**
February 2, 2013

P wrote: "I'm not sure about what precise definitions of "rigorous" would make mathematicians happy…"

The definition is pretty simple. In a "rigorous" approach to a subject, you start by laying out a set of axioms and rules for deduction. Then everything else you derive using those. Ideally you also show they're consistent: that is, it's impossible to derive a contradiction. In practice, you often try to show they're relatively consistent: any contradiction would lead to a contradiction in some widely accepted set of axioms, like ZFC.

It's easy to make small chunks of quantum field theory rigorous, just by precisely stating the rules used. The hard part is that quantum field theory as practiced by physicists uses many different bunches of rules, and it's hard to precisely state them all, much less show they're consistent or organize them into something elegant.

Nonetheless this has been successfully done in a bunch of cases, and there's been a lot of progress now that more mathematicians are getting interested in quantum field theory.

32. **Thomas**
February 2, 2013

@KP

Yes, but we know that gravity exists (and presumably dark matter).

@deconstructed
I think it is not correct to dismiss the existing work on triviality of ϕ^4 and $U(1)$ gauge theory as mere heuristics. Having said that, I think it would still be valuable to provide a lattice definition of the complete standard model, and then show that this theory is indeed just QCD plus free fields.

33. **Peter Orland**
February 2, 2013

Thomas,

I think what deconstructed means is that triviality of phi^4 in four dimensions is not proved. The papers of Froehlich and of Aizenmann proved it is trivial in more than four dimensions, but not in exactly four.

34. **Abdelmalek Abdesselam**
February 4, 2013

Just a small correction to what Peter Orland said above. The methods of constructive QFT are not limited to super-renormalizable theories. For instance massive Gross-Neveu in 2d which is only asymptotically free in the UV has been given at least three different rigorous constructions: by Gawedzki-Kupiainen, Feldman-Magnen-Rivasseau-Seneor and finally more recently by Disertori-Rivasseau. What is needed is that running couplings remain small in the range of scales under consideration. Super-renormalizability is not essential.

35. **Peter Orland**
February 4, 2013

Dr. Abdesselam,

Actually, what I said was that constructive methods appear to be limited to theories for which renormalization can already be controlled. I did not say only super renormalizable theories could be understood this. The Gross-Neveu model, for example, can be studied with 1/n expansions, which gives a very good picture of its behavior in any dimension (even in more than 2D).

36. **Abdelmalek Abdesselam**
February 4, 2013

Dr Orland,

Sure. “renormalization can already be controlled” means heuristically and of course this precedes the corresponding constructive result which proves that the renormalization of the model can be controlled rigorously and nonperturbatively. By the latter I mean “not simply in the sense of formal power series” which is weaker than what physicists might understand by nonperturbative, i.e., pertaining to strong coupling phenomena for instance.

37. **Chris Austin**
February 6, 2013

Arthur Wightman seems also to have been deeply involved in early work on BHPZ renormalization, even if he apparently didn’t publish on it at the time, (although he later co-edited a book on renormalization with G. Velo). Klaus Hepp’s fundamental paper contains a very substantial acknowledgement to Wightman.

38. **Chris Austin**
February 6, 2013

Sorry, I meant BPHZ.

Short Items

February 3, 2013
Categories: Uncategorized

- Resonaances has an excellent posting about the latest WMAP9 CMB measurements, and the value N_{eff} for the number of implied light degrees of freedom. When the WMAP numbers were released late last year, they quoted

  N_{eff}=3.89+/-.67, 3.26+/-.35, 2.83+/-.38

  for the results of fits to their data and others (see section 4.3.2). Jester described this as "like finding a lump of coal under the Christmas tree": the value N_{eff}=3 implies no new light degrees of freedom beyond the known 3 light neutrinos. A rumor soon appeared on his blog that this result was in error and would be corrected.
  The corrected version is now out, with new results

  N_{eff}=3.89+/-.67, 3.84+/-.40, 3.55+/-.49

  and a note about the correction: "slight correction to Neff for case with BAO."
  which seems reasonable if you regard the difference between finding no unknown degrees of freedom and discovering a new unknown one as "slight".

- Martin Perl has an interesting blog entry entitled What Me Worry About The Future of High Energy Physics? He describes his views about the problems facing HEP, what he thinks of the Fundamental Physics Prize, and some comments on the history of physics (as well as some kind words about this blog).
- On the Beauty front, you can watch a video of Enrico Bombieri's lecture at the IAS on Beauty in Mathematics. On February 15 in Boston the big AAAS annual meeting will include a session on Is Beauty Truth? Mathematics in Physics from Dirac to the Higgs Boson and Beyond.
- viXra log has a posting about video released by CMS of the session on June 15th where their convincing evidence for the Higgs in gamma-gamma decays was first unveiled to the larger collaboration. It was at this point that most of the 3000 or so physicists in CMS knew for sure they had a Higgs discovery. One can speculate about what the graph of number of people in the world aware of this would look like as a function of time, but I'm sure by June 17th when I first heard about it, it was already much more than 3000, and growing exponentially.

  This was about three weeks before the public announcement on July 4. Of course now what we all want to know is what the full 2012 CMS dataset says about gamma-gamma, and whether it agrees with the SM or not. The general assumption is that this will be made public at the March 2-9 conference in Moriond. So, based on the timetable last time, one can guess that within the next week or two such results will be disclosed to the full CMS collaboration.

- As every year, one can follow the latest trend in US particle theory hiring at the tenure track level here. Lubos Motl describes the current situation as one of hep-th being subjected to terrorism, I guess by hep-ph.

Comments

1. **Sesh**
   February 4, 2013

   You can hardly say the CMB measurements together with the BAO have "discovered" an unknown degree of freedom. Neff of 3.046 is inconsistent with the data by at most 2 sigma. That's not much evidence on which to base any sort of claim for discovery, so WMAP are quite right not to do so. Especially given all the other parameters that could be varied, and the fact that ACT and SPT appear not to be very consistent with each other, and that some of the BAO data looks a little fishy too.

2. **Yatima**
   February 4, 2013

   > Lubos Motl describes the current situation as one of hep-th being subjected to terrorism, I guess by hep-ph.

   Send in the hep-drones for Great Justice!

   Seriously though, no mention of the Proton Size Discrepancy?

3. **Kavanna**
   February 4, 2013

   But according to Lubos, everything is terrorism — or at least, dangerous post-modern nihilism — especially opposition to the Holy and Beautiful Theory of Extended One-Dimensional Thingies.

   Looks like Neff = 4 is not impossible, BTW. It's a new "slight" degree of freedom!

4. **S**
   February 4, 2013

   Speaking of beauty, I'm curious about your reaction to this article, which mentions you. It seems to me that it somewhat mischaracterizes your position.

   http://chronicle.com/article/When-Beauty-Is-Not-Truth/136803/

   I've understood you more to think that people are doing the wrong theorizing, not that theorizing is dead (and similarly, that string theory is inelegant, not that elegant is useless). But perhaps I'm wrong.

5. **Peter Woit**
   February 4, 2013

   S.,

   About the Orrell book: I think it's a well done history of the "beauty" issue in physics, and right now this is a big issue because a lot of people are interpreting the failure of string theory and supersymmetry as a failure of the idea that you should pursue such beauty. I definitely disagree with Orrell's conclusions, but he represents a view which I think is going to be getting more traction.

I talked to the Chronicle writer who wrote the review, and I think he decided not to quote me because my point of view complicates the issue and it's complicated enough to explain to a general readership already. So, he added me in with Gleiser and Orrell as claiming the "predictive power of theorizing in physics waning", which in some sense is true, although my reasons for thinking this are rather different than those of Gleiser and Orrell (crudely, I'd say that the problem is a sociological one of people pursuing failed ideas, claiming them to be beautiful even if they aren't).

6. **chris**
February 5, 2013

i'd rather guess by nucl-th…

7. **P**
February 5, 2013

@ Chris:

I think most of these people are hep-ph.

It's interesting to note that, while most of the jobs are going to ph people, there still aren't that many jobs! At top U.S. institutions over the last five years, for example, I'd estimate there have at most been O(10-20) ph hires. Who, in the LHC era, can blame the community for wanting to hire at least 10-20 excellent young professors to carry model building into the next generation? This will saturate in the next few years and the hires will swing back towards formal theory a bit.

8. **Brathmore**
February 7, 2013

From Dr. Perl's blog about the Fundamental Physics award: "The time scale for physics progress is a century not a decade. There are no decade scale solutions to worries about the rate of progress of fundamental physics knowledge."

This raises the question of how long one must wait for experimental validation before one rejects a theory. I am sympathetic to remarks that string theory has been a three decade long rabbit hole in a quest for a unified theory. But what makes three decades special, if the "time scale for progress…is a century…" as Dr. Perl suggests? Does one wait only a year (no), a decade (no), a few decades (?), a century (?), or even longer to abandon a theory? How do you decide? I suspect that up until this point in modern physics, theories with no experimental support were eventually replaced by theories that did garner such support – thereby rendering the question moot, since a clear alternative **with** support existed. But today, such a trajectory may not occur, since as Dr. Perl points out, it is becoming increasingly difficult to even build machines capable of testing theories at the scale where we want to answer the TOE questions.

In short, if no TOE rival to string theory emerges with experimental support, then how does one decide how long to give string theory before calling it quits? If an elegant theory exists without experimental support or falsification, should it be abandoned when no alternative exists to replace it?

If data falsifies string theory (and I am not competent to evaluate this question), then by all means let us abandon the falsified theory. But, if I understand some of the arguments in this forum correctly, the gripe against string theory is less that it has been falsified, than that it cannot be falsified – a bold claim indeed, given that (i) unforeseeable progress in technology could occur in the future conducive to such testing; or (ii) unforeseeable progress in string theory could occur giving rise to testable claims.

Speaking as an outsider, I wonder out loud if one of the dilemmas faced by string theorists is that, even if they were to acknowledge string theory's failings, they still believe it to be far more promising than any other TOE in existence (or that they can conjure themselves). Until that changes, they have no other TOE to work on, and will therefore stick with strings.

9. **reject**
February 7, 2013

Actually a theory is abandoned rather than rejected, and that happens when an alternative comes along which does a better job of explaining things. So it was with the luminiferous aether; it was abandoned when Einstein produced the theory of relativity, but there was no explicit `rejection' of the aether theory *per se*. So also with the phlogiston theory of earlier years. It was eventually abandoned because other theories could do a better job. When it was claimed that phlogiston must have negative mass, people just lost interest in it. But there is no real time scale for these events (changes of paradigm?). String theory will likely persist until something else comes along which does a better job of explaining things. There is no time scale for that.

10. **chiz**
February 7, 2013

Gleiser's comment in the Chronicle article that RH amino acids don't occur in nature is technically false, they do occur – even in humans – although they certainly aren't common. His claim about the beauty inherent in asymmetry – and the example he gives – also seems wrong. Personally, I'm sympathetic to the notion that beauty and truth are highly correlated but I'm not sure why this issue is brought up in connection with the standard model or any extension of it since the standard model has always struck me as rather ugly. Are there really people who think that CKM mixing is elegant? It looks, to me, like an ugly kludge. And using singlets for RH states but doublets for LH states also strikes me as profoundly ugly. I don't really understand how Gleiser – or anyone else – could regard that as an elegant or beautiful piece of mathematics.

11. **Shantanu**
February 8, 2013

Peter, OT
Link to Arkani-Hamed talk at STSCI (about 80 minutes long)
https://webcast.stsci.edu/webcast/detail.xhtml?talkid=3398&parent=1

12. **M**
February 8, 2013

Does somebody know if this paper
http://arxiv.org/pdf/1302.1617v1.pdf
means that Planck will announce a non flat universe with OmegaK ≈ 10^-3?

13. **Toni**
February 8, 2013

Peter,

if all experiments confirm the standard model (with a Higgs and nonzero neutrino masses), then the arguments from the hep-th community stating that the standard model is incomplete must be wrong.

It looks as if 30 years of a so-called “incomplete standard model” are coming to an end. Would it be possible to have an overview of all these arguments and to check them one by one?

It seems very bizarre to me that for 30 years a complete physics community has been convinced that the standard model is incomplete or even wrong, but that this conviction now turns out to be mistaken. What have they been telling to students for the past 30 years, and why? Where are the mistakes? Why have they been made?

14. **Peter Woit**
February 8, 2013

Toni,

There are solid arguments that in principle the SM is incomplete (no quantum gravity, bad short distance behavior of scalar and U(1) gauge fields, a large number of parameters whose origin is mysterious, perhaps dark matter, if it is not astrophysical in origin). But the arguments that the SM is incomplete at the TeV scale have always been quite weak and highly speculative. I hope that the news from the LHC will have the effect of causing people to rethink some things often claimed to be strong arguments, but which really aren’t. First amongst these is the “hierarchy” or “fine-tuning” problem (which I’ve often gotten into arguments with people about here, not interested in repeating now…)

Unfortunately, so far what I’ve been seeing from prominent theorists is no desire to re-examine their assumptions, but instead things like “maybe X that is needed to complete the SM is at a little higher mass than we thought, let’s wait for 2015-6 before doing anything”, or “OK, if there is no X, then we give up on science and go for anthropics as the explanation”.

15. **Toni**
February 9, 2013

Peter,

thank you. The distinction between “incomplete at TeV scale” and “incomplete at Planck scale” is a good one.

Then the present experiments can be summarized by saying that the standard model is (probably) complete at the TeV scale.

At the Planck scale, quantum gravity and parameter explanation must kick in. Yes, dark matter is still open – if it is an issue at all – and even the short distance behavior might be a non-issue. Even certain Czech bloggers admit that the Planck length is the smallest physical distance.

So we could arrive at the situation that only two issues are open: quantum gravity, which we will probably never confirm experimentally, and the parameter calculations.

If that is true, our beloved “theory of everything” will be extremely narrow in scope. In practice, it will only explain the parameters! Are we all prepared to live with such a “basic” TOE? It will not fascinate, not excite passions, not change our world view, not produce new technologies, and not provide any power.

Could the TOE really be a footnote at the end of textbooks on the standard model? Are we all prepared for this anti-climax?

16. **Mitchell Porter**
February 9, 2013

“Are we all prepared for this anti-climax?”

The progress of physics has to end one day, either in the discovery of the final truth, or in coming up against barriers to knowledge that we cannot surpass.

It could also come to a fake conclusion for a while, where a certain set of barriers are treated as unsurpassable even though they are not, and so the philosophical culture of physics might settle into vague affirmations of a favored cosmology, such as one already sees among some proponents of a “landscape multiverse”, and John-Horgan-esque musings about the limits of knowledge.

However, I do not expect to see that really happen, not in a big way. Of course certain opinion leaders and their followers may settle for such opinions. But in the big bad world outside narrow schools of thought, the situation is still discord, confusion, clash of ideas, a trickle of genuine empirical discovery, and enormous new vistas of thought still waiting to be explored.

If we actually had a theoretical framework that could predict all the parameters of the standard model, that would be an intellectual revolution that wouldn’t just stay within physics. The cultural impact would be as big as relativity. So if you want to understand how having a true theory of everything would feel, consider the cultural significance of relativity.

We don’t go around talking about relativity every day, but it’s hardly a technical curiosity buried at the ends of textbooks either. Relativity is one of the fixtures of modern intellectual culture. It’s a beloved and challenging set of ideas for people who like science – and a constant spur to the invention of crackpot alternatives for people who don’t like it – and the source of new philosophies like the idea of a “block universe” – as well as a reference point in cultural discussions.

Human culture is diverse, contradictory, always reconfiguring itself, contains a thousand elements, and changes in response to an ever-changing world. The place of a verified final theory in human culture would not remain fixed, but it would be a very heavy presence in a number of areas of thought and life.

17. **Sesh**
February 10, 2013

M: It’s possible of course, but I don’t see why you should especially think that. As far as I can tell, Marc Kamionkowski is not on the Planck team and Phil Bull definitely is not, so they shouldn’t have any special insight into what Planck are seeing – the confidentiality restrictions on this data are pretty tight (until March).

18. **tulpoeid**
February 10, 2013

There we go again about CMS collaborators eager to spill out “secrets”….. When will the lack of actual spilling be properly acknowledged as an unusual, for humankind’s standards, demonstration of respect for one’s work and for one’s colleagues’ work?

Do you want a more pragmatic point? Here it is. What will anyone* on earth gain by the “secret” given away a few days before its announcement? The only thing you’ll manage will be to spoil the end.

* Not talking about the bloggers who will host the giveaway of course.

Existence of Kähler-Einstein Metrics

February 7, 2013
Categories: Uncategorized

An important recent development in geometry has been the announcement of two claimed proofs of a long-standing conjecture about the existence of Kähler-Einstein metrics. Simon Donaldson is talking about this at MIT this week (see here and here), and the last in a series of his papers with Xiuxiong Chen and Song Sun giving details of their proof appeared on the arXiv earlier this week, see here. For the earlier papers in the series, see here and here, as well as the original announcement of the proof in outline here. Gang Tian also has a preprint with a proof, see here. As usual in mathematics, one might want to wait for these preprints to be refereed by experts before being sure that a proof is in hand.

Given any manifold, there's an infinity of ways of putting a metric on it. A major theme in modern geometry and topology has been the pursuit of the idea that in many cases there may be a unique "best" choice for such a metric. The proof of the Poincaré Conjecture involved just this sort of idea, showing that starting with any metric on a simply-connected three-manifold one could deform it in a specific way to end up with certain special possibilities that could be completely analyzed.

For Kähler manifolds, the big open question of this kind has been that of whether one can find a unique metric that is both Kähler and Einstein (thus "Kähler-Einstein"). For negative first Chern class this was shown by Aubin and Yau, and for zero first Chern class by Yau in his proof of the Calabi conjecture (these are the "Calabi-Yau" manifolds). For positive Chern class there are counter-examples, but the conjecture has long been that Kähler manifolds satisfying an appropriate notion of "stability" will have such a unique Kähler-Einstein metric, and it is this conjecture that apparently has now been proven.

The details of this are far beyond my expertise, so I refer you to the papers quoted above, as well as some expository articles about the problem by Donaldson and Tian, as well as a series of blog posts (here, here, and here) by Terry Tao based on lectures by Yau.

Comments

1. **not even right**
   February 8, 2013

   so, does this have any hope of selecting manifolds from the landscape?

2. **arnold**
   February 8, 2013

   Hi,

   This is certainly not the right place to ask this question so i'm sorry but I don't really know where else to ask. Do you know if there is going to be an eilenberg lecture this semester and if yes about what it will be ?

   Regards,
   Arnold

3. **Bobito**
   February 8, 2013

   There's not much doubt that the result is correct. Donaldson, Chen, Tian are among the biggest experts on the problem, the ones who proposed the path to solve it, the one who laid the groundwork for solving it. Moreover it was sort of expected they (or some student) would solve it.

4. **Peter Woit**
   February 8, 2013

   not even right,

   No. This has long been suspected to be true and does not help with that problem.

   Arnold,

   No Eilenberg lectures at Columbia this semester. That program funds visiting lecturers, but not necessarily two per year, just one this year (Klainerman, who lectured in the fall). Wait till this fall…

5. **anon**
   February 9, 2013

   The Washington Post covers Quantum Mechanics, seriously:
   http://www.washingtonpost.com/blogs/wonkblog/wp/2013/02/07/quantum-mechanics-is-an-embarrassment/

6. **Daniel Guan**
   March 5, 2013

   See my recent papers "Type I…".

7. **Daniel Guan**
   March 5, 2013

   Here:
   http://math.ucr.edu/~zguan/bib.html

Quick Links

February 17, 2013
Categories: Uncategorized

Posting has been light recently, partly since I've been working on writing up notes for my course (more about that soon), but largely because there hasn't been a lot of news to write about in the math-physics world. The LHC shutdown yesterday, with the latest online machine status report now saying:

> No beam for a while. Access required. time estimate: ~2 years

It will take about that long to replace magnet interconnections and do other work required to get the LHC working at an energy close to the design energy of 7 TeV/beam (seems likely they'll be trying for 6.5 TeV/beam).

Results from the full 2012 data set for the Higgs are likely to be released soon, at Moriond in early March. Not much in the way of rumors available about this, which may have something to do with no surprises in the data. I hear there will also be, as expected, yet more stringent limits on SUSY reported.

For a US-centric series of reports on HEP and future plans, see talks here at a meeting this week at Fermilab.

On the cosmology front, there should be big news next month with Planck finally reporting results on March 21 (see here), to be followed by a conference dedicated to the results a couple weeks later.

No matter how cosmology is doing as a science, the Templeton Foundation is doing its part to promote its non-scientific aspects, with major funding for projects designed to promote and institutionalize the subject of "Philosophy of Cosmology". Just before the Planck data release, DAMTP will host a Templeton-funded conference on "Infinities and Cosmology", which will include two lectures by Michael Douglas on "Can we test the string theory landscape?". Templeton is also funding a three-week summer institute in Santa Cruz to "promote understanding and research" on topics like "reasons for believing in a multi-verse, anthropic arguments, the metaphysics of laws and chance, why anything at all exists." If you want to spend three weeks this summer among the redwoods discussing such topics, and collect a check for $2500 from Templeton, apply now.

Sometimes I make fun of pseudo-scientific research favored by some Northern California physicists by speculating about the role of marijuana in their research efforts. On a much more serious note, Southern California's John Schwarz and his wife Patricia have been involved in admirable efforts to change US policy against investigating medical uses of marijuana, with Schwarz writing an editorial here last year, and speaking at a conference in DC next week.

For more evidence of how ideas about string theory have worked their way into US general cultural life, a couple people have pointed me to Adam Gopnik's piece about Galileo in last week's New Yorker, which contains the following:

> Contemporary historians of science have a tendency to deprecate the originality of the so-called scientific revolution, and to stress, instead, its continuities with medieval astrology and alchemy. And they have a point. It wasn't that one day people were doing astrology in Europe and then there was this revolution and everyone started doing astronomy. Newton practiced alchemy; Galileo drew up all those horoscopes. But if you can't tell the difference in tone and temperament between Galileo's sound and that of what went before, then you can't tell the difference between chalk and cheese. The difference is apparent if you compare what astrologers actually did and what the new astronomers were doing. "The Arch-Conjuror of England" (Yale), Glyn Parry's entertaining new biography of Galileo's contemporary the English magician and astrologer John Dee, shows that Dee was, in his own odd way, an honest man and a true intellectual. He races from Prague to Paris, holding conferences with other astrologers and publishing papers, consulting with allies and insulting rivals. He wasn't a fraud. His life has all the look and sound of a fully respectable intellectual activity, rather like, one feels uneasily, the life of a string theorist today.

Comments

1. **srp**
   February 17, 2013

   Pet peeve with articles like Gopnik's: They are so caught up in the battle of Reason against Faith that they fixate on the Copernican issue (where Galileo's contribution was as much polemical as substantive) and never consider Galileo's best justification for caving in to the Inquisition: He was about to revolutionize science with his work on kinematics, and death would have prevented him.

2. **leaning tower**
   February 17, 2013

   I've climbed up the Leaning Tower of Pisa. There is a sign at the bottom which says "Please do not drop objects off of this tower."

3. **tmp**
   February 17, 2013

   Templeton has money and it can fund anything it likes. The Church of Galileo's day also had plenty of money (and power). But the Copernican system won out anyway, eventually. Galileo could see the moons of Jupiter, the phases of Venus and the shadows of mountains on the Moon. Obviously others saw these things too (eventually) . Experimental data was ultimately the key to the success of the \`new science'. So it is with string theory today: endless theorizing will neither prove nor disprove any \`world view'. But \`big science' data is expensive and hard to acquire. So Templeton and suchlike will be around for a while.

4. **lun**
   February 17, 2013

   While the LHC is down for two years, RHIC risks being definitively shut down.

   I would say this is big news, both for experimental reasons (it is the last accelerator of truly international significance remaining in the US) and for theoretical reasons (discussions of applicability of string theory to the matter RHIC produced featured quite prominently on this blog and "string wars" in general)

5. **norhic**
   February 17, 2013

   DOE wants to build FRIB (Facility for Rare Isotope Beams) at MSU = Michigan State University, also there is BNL/RHIC and JLAB/CEBAF. Both BNL ad CEBAF have been trying for years to host the EIC = electron-ion collider, a sort of mini-HERA. But DOE does not have enough money to fund all these facilities. Big science costs big money.

Organizations like Templeton can fund conferences, but I doubt Templeton could pay for RHIC operations year after year.

6. **Surendra**
February 17, 2013

Moriond = Englert Nobel Campaign. ULB Physics lead is heading the conference.
https://indico.in2p3.fr/internalPage.py?pageId=0&confId=7411

Last year the same conference leads tried to name the Boson the "BEH Boson" even as BE had no boson in their 1964 paper.
http://www.newscientist.com/article/dn21604-what-to-call-the-particle-formerly-known-as-higgs.html

Peter Higgs' campaign was January 9th – 11th in Edinburgh for Higgs Institute opening. Led by his handlers and John Ellis.
http://higgs.ph.ed.ac.uk/workshops/higgs-symposium

Tom Kibble's campaign (and to lesser extent GHK) is on March 13th at Imperial College for TK's 80th birthday. Weinberg is heading over for this.
http://plato.tp.ph.ic.ac.uk/conferences/Kibble80/index.html

7. **GB**
February 17, 2013

Doesn't anyone spare a thought for Jeffrey Goldstone for a Nobel?

8. **Surendra**
February 17, 2013

Goldstone needs a better campaign manager 🙂 MIT won't stoop to that.

However, one can't help but think his snub already occurred in 2008 as he should have won with Nambu. Of course Cabibbo should have won with K and M that year also. So the 2008 Nobel could have been a couple different prizes and split across a couple years.

Hopefully the committee gets more creative on the Higgs issue.

9. **Peter woit**
February 17, 2013

Enough hijacking of the comment section of this posting for Higgs Nobel discussion. Unless of course you have inside information about what the Nobel committee is doing about this, in which case that would be news, please share….

10. **steve newman**
February 17, 2013

speaking of the cosmology front-
there was big news last month -http://news.nationalgeographic.com/news/2013/01/130111-quasar-biggest-thing-universe-science-space-evolution/

but no one seems to be talking about it.

a mainstream group of astronomers has discovered the largest cosmological
structure ever seen. Large enough to defy the cosmological principal that the
'standard model of cosmology' is based on. They published in Monthly Notes of the Astronomical Society.

The discussion of this, so far, non existent.

11. **cormac**
February 18, 2013

Many thanks for those links Peter, I wish I had remembered Moriond in time.
Re Cambridge conference on the philosophy of cosmology, I disagree; I think it looks very interesting. I don't think DAMTP, or individuals like John Barrow and George Ellis would participate in something shoddy. Such conferences seem to me to be a worthwhile effort to consider the philosophical aspect of modern problems in physics

12. **Peter Woit**
February 18, 2013

Cormac,

I have no doubt that the DAMTP conference will discuss the multiverse at the highest level possible. But I think it's worth noting that conventional sources of funding for science typically would not fund this kind of conference, that the funding is coming from an organization which has its own agenda. That agenda is not to promote good science but to get as much attention as possible for certain kinds of pseudo-science which support their world-view.

13. **Shantanu**
February 18, 2013

Peter, this is probably OT, but what ddid you think of Arkani Hamed talk at stsci?
Thanks

14. **Henry Bolden**
February 18, 2013

Some big announcement about dark matter from Sam Ting at MIT is coming soon according to this article in the Globe and Mail (a Toronto newspaper):

http://www.theglobeandmail.com/technology/science/mit-physicists-experiment-pulls-dark-matter-into-the-spotlight/article8783097/

Anyone here know what the "big news" is supposed to be?

15. **Peter Woit**

February 18, 2013

Shantanu,

I don't think there's anything new there. For a more extended version of the same thing, see Arkani-Hamed's series of lectures at the IAS a couple years ago at

http://video.ias.edu/taxonomy/term/4

For those interested, Shantanu is referring to the talk here

https://webcast.stsci.edu/webcast/detail.xhtml?talkid=3398

16. **uair01**
February 18, 2013

Interesting to see John Dee mentioned in this context. I always felt that his experiments with angel communication and his analysis of the Enochian language somehow feel like early modern science. The endevour was based in magical thinking but the methods were revolutionary and – in their way – rational.

I like your analogue with modern string theorists. Angelic language indeed!

BTW, this is a great book on this subject:
Renaissance curiosa : John Dee's conversations with angels, Girolamo Cardano's horoscope of Christ, Johannes Trithemius and cryptography, George Dalgarno's Universal language
Shumaker, Wayne / Center for Medieval and Early Renaissance Studies / 1982

17. **chiz**
February 19, 2013

More details on Ting's press conference here. A paper is being submitted to a high energy physics journal in a couple of weeks. He doesn't yet have sufficient data to report the entire energy range AMS can pick up, in a statistically meaningful way. He may, possibly, have evidence that the spectrum is dropping off at some energy in a way consistent with a theory of dark matter. Or, possibly, he may not.

18. **chiz^2**
February 19, 2013

In other words, let's have a press conference to drum up publicity, maybe to procure more funding, maybe to establish a timestamp for later use for priority claims ~ "I told you so." But when it comes to specifics, it's just a set of vague statements. Maybe Ting has evidence of "something" maybe he does not. But let's hold a press conf anyway. Not the Ting of the J particle anymore. But I don't really blame Ting for doing this (and I don't blame string theory either). That's modern physics (or should I say fiziks?).

19. **Hamish**
February 19, 2013

Good luck with the lecture notes…you will have a hard time topping this debut by a Columbia colleague!

http://gawker.com/5985130/half+naked-ivy-league-prof-opens-quantum-physics-course-with-911-footage-hitler-puppet+smashing-ninja

20. **Peter Woit**
February 19, 2013

chiz^2,
This story is based on the AAAS conference, a huge yearly event partly designed to get the press to come hear about ongoing science research, so kind of like a huge press conference. The timing wasn't chosen by Ting. He's famous for not running a tight ship and not releasing results until absolutely certain. It's not implausible that he just wasn't quite ready this week.

21. **Kavanna**
February 19, 2013

It's nice to see that the reality of string theory is spreading further into the educated lay public. It's a terrible bust, like the post-bubble economy. Much has been squandered on researching dead-ends while ignoring serious scientific questions.

The obsession of Reason versus Faith mars a lot of the coverage of string theory and related issues. In late medieval and Renaissance times, these categories were not anywhere near as sharply distinct as they are today. Applying that kind of thinking to understanding these thinkers and their times (NOT just the theories that survived later scientific scrutiny) is completely out of place.

The irony is that string theory fits all the definitions of a faith-based pseudo-science, with a thin veneer of rationalizing with the Standard Model to make it look plausible.

OTOH, what the Templeton people are promoting seems fine. They're stimulating thought and debate, not including or excluding anyone on non-scientific criteria, and so on. (Only the question of "why is there anything at all?" lies entirely outside science.) With the rise of modern cosmology and the issues surrounding quantum measurement, which seems to require a thermodynamic selectivity — related to life and consciousness, but not requiring them — a lot of old questions included in the pre-modern Aristotelian physics — questions of intentionality and teleology — once again become legitimate scientific questions. Our answers and means of arriving at those answers will, of course, be quite different from 500 years ago.

SUSY and Quantum Mechanics

February 19, 2013
Categories: Favorite Old Posts, Uncategorized

You may have read somewhere today that Columbia professor strips down to underwear in bizarre lesson to help baffled students learn quantum mechanics (first-hand sources here and here). That wasn't me, but I have been talking to my class for the last couple weeks about quantizing fermionic variables, and some simple quantum mechanical examples of supersymmetry. Notes on the supersymmetry stuff are here, earlier notes on the fermionic version of quantization and what it has to do with Clifford algebras and spinors are on this page.

These notes are not quite finished, mainly because I've been trying to sort through the hairy issue of sign conventions that comes up when you start dealing with a Hermitian inner product on anti-commuting variables, something you need to do to get unitary representations. There's a detailed treatise on the subject by Deligne and Freed, who are very smart and sensible, but I'd like to understand this better. The choices they make end up leading to odd self-adjoint operators having eigenvalues proportional to a square root of i, which is consistent, but not exactly intuitively clear. The best source for finding details of the mathematics used in SUSY is probably the IAS volume that Deligne/Freed is part of. The first part includes valiant efforts by Bernstein, Deligne, Freed, and Morgan to get the mathematics right, including the signs (they say "Writing this has been an absolute cauchemar de signes!"). One sign they get wrong is a typo on page 91 (equation 4.4.5).

The parallel stories of bosonic and fermionic oscillators are among the deepest things in theoretical physics, and involve just spectacularly intricate and deep mathematical ideas (symplectic geometry, rotation and spin groups, Heisenberg groups, the metaplectic representation, Clifford algebras and Weyl algebras, spinors, etc., etc…). I hope the course notes I've been writing give a little insight into this and the way Lie groups, Lie algebras, and their representations are involved. Generically, "supersymmetry" refers to generalizing the notion of a Lie algebra to include odd generators, and thus get a "super" Lie algebra, sometimes acting in an interesting way that mixes even and odd variables. In the notes I describe two very simple examples, showing how one gets a "square root" of the Hamiltonian operator.

There are all sorts of interesting structures one can get by looking for supersymmetrical versions of QFT, and the IAS volume describes a lot of them. One wonderful example is the N=2 susy gauge theory that gives a TQFT with observables four-manifold invariants. This is an unphysical theory, but tantalizingly close to physical theories. It involves a "twisting" mixing the space-time and internal symmetries which might be the sort of thing needed to avoid the problem of the kind of "superpartners" that is deadly for SUSY extensions of the standard model.

Perhaps the most compelling example though is the way the fact that the Dirac operator is a square root of the Laplacian can be thought of as an example of SUSY. This is one of the deepest ideas in mathematics, something whose implications I suspect we still don't completely understand.

Comments

1. **P**
   February 19, 2013

   Hi Peter,

   Nice post – I'm curious to look at the notes.

   I'm confused by one of your paragraphs, though. You're right to point out the importance of SUSY gauge theories in studying interesting mathematical quantities – including the Donaldson-Thomas invariants you presumably had in mind, but also many other quantities.

   I was wondering if you could elaborate on

   "It involves a "twisting" mixing the space-time and internal symmetries which might be the sort of thing needed to avoid the problem of the kind of "superpartners" that is deadly for SUSY extensions of the standard model."

   As I see it, one can argue (as you do, but I disagree with) that the hierarchy problem is not a problem and that SUSY is not well motivated. One can also argue that we won't see superpartners at the LHC. Was this the kind of "problem" you meant? There is no theoretical problem with superpartners in BSM models, and in particular the soft masses could be at a high scale and they could completely decouple from low energy physics. There is no theoretical problem, so I'm just very curious what you had in mind!

   Moreover, whatever the problem is, how you do expect the topological twist to help with the problem?

   Thanks,
   P

2. **Peter woit**
   February 19, 2013

   P,
   The basic problem with SUSY extensions of the SM is that in them SUSY doesn't mix any degrees of freedom you know about. Instead it takes each degree of freedom you know about and mixes it with a new unobserved one (the superpartner) that you have to postulate, and then start jumping through hoops in order to make unobservable. This new "symmetry" one has introduced is trivial on the world as we know it.

   The reason I mention the Dirac operator and the N=2 TQFT case is that in these cases there is a supersymmetry, but it does something more interesting than taking bosons to fermions with spin changing by 1/2. In the Dirac operator case, the Z2 grading is that of even/odd spinors: the SUSY operator (Dirac operator itself) takes left-handed spinors to right-handed spinors and vice versa (this is just QM though, not QFT). In the N=2 TQFT case (Witten's first 1988 TQFT) the twisting basically turns this into a theory of differential forms, and the SUSY is like a deRham differential taking even to odd forms and vice-versa. Because of this though, this theory violates spin statistics and is non-physical.

   I'm certainly not claiming I have an intelligible idea of how to construct a physical QFT that avoids the standard usage of SUSY, just pointing out that there are examples of theories where it does something different than the usual problematic thing.

3. **Bob Jones**
   February 19, 2013

   "You're right to point out the importance of SUSY gauge theories in studying interesting mathematical quantities – including the Donaldson-Thomas invariants you presumably had in mind"

Did you mean to say Donaldson invariants? Donaldson-Thomas theory is really a topic in enumerative algebraic geometry rather than topology.

4. **P**
February 19, 2013

Hi Peter,

Very good! Thanks for explaining what you mean. I agree with you that this is something very important to emphasize to the outside community: SUSY is a theoretical framework that may or may not have relevance for particle physics, but like any truly deep theoretical structure it has implications for other fields. The mathematical impact of SUSY gauge theories and string is immense: from uncovering mirror symmetry, to the Donaldson-Thomas invariants you mentioned, to recent work on 3d-3d correspondence, 2d-4d correspondence, exact partition functions, and their relevance for three-manifolds, GW and GV invariants, etc. It's really amazing that QFT has anything to say about mathematics that mathematicians didn't already known, and this is certainly true of some SUSY QFT's. Also, thanks for pointing out the relevance of the deRham complex – I haven't looked at that paper in awhile.

On the other issue: since you don't like strings or BSM SUSY for the hierarchy problem, it surely seems like these new degrees of freedom are pointless. But for those of us that do, mixing known degrees of freedom with unknown degrees of freedom is the whole point. e.g. this is what allows for the non-renormalization of the Higgs mass (or any scalar mass) above the scale of supersymmetry breaking. Once one is building SUSY models BSM, the values of the soft masses are model dependent. Some models are killed; others are not. This isn't jumping through hoops, it's just the normal death that any set of models goes through as experimental bounds get better. Perhaps people start studying new models over time based on bounds, but that model was always there in theory space, regardless of whether people studied it! 🙂

Cheers,
P

5. **Peter Woit**
February 19, 2013

P and Bob Jones,

I was referring to Donaldson, not Donaldson-Thomas. Specifically Witten's 1988 paper
"Topological quantum field theory",
http://projecteuclid.org/euclid.cmp/1104161738
this paper takes N=2 susy on a four-manifold and uses the twisting trick to get a TQFT with Donaldson invariants as observables. Many mathematicians were not so impressed by this because it didn't seem to tell them anything new about Donaldson invariants. They had to eat their hats a few year later when the Seiberg-Witten paper about this same theory came along.

While mirror symmetry and QFT arguments telling one new things about algebraic geometry are all well and good, what's remarkable is that this is a simple 4d QFT with gauge fields, fermions, scalars, looking very close to the kind of theory that describes the real world. It doesn't deserve to be forgotten…

6. **P**
February 19, 2013

Certainly SUSY QFT has importance consequences for enumerative algebraic geometry, also. I'm forgot the exact context of SW theory and four-manifolds, though. It's D and not DT?

"While mirror symmetry and QFT arguments telling one new things about algebraic geometry are all well and good, what's remarkable is that this is a simple 4d QFT with gauge fields, fermions, scalars, looking very close to the kind of theory that describes the real world. It doesn't deserve to be forgotten…"

True, but remember that though N=2 theories can come close to being realistic in a certain sense that you just mentioned, the absence of chiral matter is fatal. In this sense N=1 theories have a shot, and N=2 theories do not.

Also, I read a little too quickly and though you were talking about the magnificence of the standard model for a second – another model that also doesn't deserve to be forgotten 🙂

7. **Lowell**
February 20, 2013

Gee, there is a long discussion in my book "Quantum Field Theory" Sec. 2.4 about
Fermonic variables, Grassmann numbers, and such.

Is there anything wrong with this exposition?

Lowell Brown

8. **Peter Woit**
February 20, 2013

Lowell,

Thanks for writing. Your book is on my shelf, and I've been consulting it, maybe more so this semester as I cover some QFT.

You give a very good discussion of coherent states for fermions and the fermionic path integral, but in my course I'm not covering this (for lack of time), sticking to the Hamiltonian formalism (which you don't emphasize). What I'm trying to explain is the fermionic analog of Poisson brackets, quantization, and how this quantization gives you unitary representations of the symmetry groups of the situation. In the fermionic case things the part that's not straightforward is unitarity, you have ghosts to worry about.

9. **D.R. Bocancea**
February 20, 2013

Peter,

"This new "symmetry" one has introduced is trivial on the world as we know it."
There is a name, or a paper , for this ..?

Dan

10. **Peter Woit**
February 20, 2013

D.R. Bocancea,

In SUSY extensions of the standard model, the SUSY symmetry generator act by taking known state to unknown superpartners. Restrict to the states we know, this is the zero operator, so the states are invariant.

11. **chris**
February 20, 2013

Lowell,

no, that is perfectly fine, but you might once again go over your proof of a massless particle having half-integer spin 🙂

12. **Shantanu**
February 20, 2013

Peter, something OT.
but could (or someone elsE)you help me decipher this press release
http://www.reuters.com/article/2013/02/18/us-space-higgs-idUSBRE91H0RR20130218
how is higgs boson related to fate of the universe?
Thanks

13. **Nick M.**
February 20, 2013

Hi ***Shantanu***,

You might want to check out this recently published New Scientist article, written by *supersymmetry evangelist* Lisa Grossman :), that talks precisely about this issue with many helpful links to other news articles (most notably the BBC News and NBC News) as well as having links to other original technical papers and letters. For example, see this paper published in "Physics Letters B" from this past August, which talks about the relationship between the Higgs boson and top quark masses as it relates to vacuum stability, and this letter in "Letters to Nature" from August of 1982, which talks about vacuum stability in a more general sense.

— Nick M.

14. **Peter Woit**
February 20, 2013

Shantanu,

This is based upon assuming we completely understand the effective Higgs potential at currently accessible energies and then extrapolating up to GUT energies, assuming no new relevant physics. The fact that the Higgs mass is such that the quartic term in the Higgs is getting very small at such high energies is intriguing, but I wouldn't describe any such metastability result as a reliable calculation.

15. **M Uppal**
February 20, 2013

Dr. Woit, may I ask that you please teach this class again next year? At the very least you'll have one student enrolled 🙂

16. **P**
February 21, 2013

Peter and Shantanu,

It's important to differentiate between an unstable vacuum and a metastable vacuum. The story about extrapolating up to GUT scale and the quartic coupling becoming zero would cause our vacuum to become *unstable*. This is in contrast with the possibility that there is another vacuum and we tunnel to it out of a metastable vacuum. Talk of bubble universes and the like (as here) is the later case.

In any case, Peter is absolutely right that this calculation makes huge assumptions not only about the Higgs potential and couplings, but also about the absence of other scalar fields. The latter completely changes the story, and from everything we know about cosmology and particle physics (let alone semi-speculative top-down approaches), the Higgs is almost certainly not the only scalar field in nature.

Cheers,
P

17. **Peter Woit**
February 21, 2013

M. Uppal,

I won't be teaching this next year, but anyone reading through the notes who has any questions is encouraged to come by my office, I'm usually there….

18. **Nick M.**
February 21, 2013

p>Thanks, ***Peter***, for the clarifying comment. One often gets the feeling, when reading through the calculations that are presented in these papers, that a plethora of assumptions, both explicit and tacit, are required in order for the conclusions to hold up. Indeed, from the BBC News article that I have a link to in my comment above, the caveat is "If we use all the *physics we know now*, and you do this straightforward calculation – it's bad news." If I have any further questions or comments regarding the vacuum stability issue, I will direct them to the thread that you recently just started on this topic, i.e., Higgs News.

— Nick M.

P.S.: I, too, along with ***M. Uppal***, wish you were going to offer the QM class (the one that you just taught) next semester. Unfortunately, it would be a bit of a morning walk from the Los Angeles area, where I currently live, to Columbia University in N.Y..

Higgs News

February 21, 2013
Categories: Experimental HEP News

Official announcements won't come out until the Moriond conference first week of March, but reliable rumors are starting to trickle out about what the Higgs news will be. ATLAS will report (based on about 21 fb^{-1} of 8 TeV data + the 2011 7 TeV data) that the gamma-gamma excess has gone down slightly (from 1.8 to 1.65 times the SM value). Still about 1.5 standard deviations high, but this isn't encouraging if you want something that disagrees with the SM.

At the AAAS 2013 meeting in Boston this past week, a press conference was held to update the media on the Higgs. What the media got from the press conference was the news that the Higgs may spell doom, unless supersymmetry saves us. This isn't just doom for HEP physics research, it's doom for the entire universe:

> "At some point, billions of years from now, it's all going to be wiped out…. The universe wants to be in a different state, so eventually to realise that, a little bubble of what you might think of as an alternate universe will appear somewhere, and it will spread out and destroy us," Lykken said at AAAS.

This is based on a renormalization group calculation extrapolating the Higgs effective potential to its value at energies many many orders of magnitude above LHC energies. To believe the result you have to believe that there is no new physics and we completely understand everything exactly up to scales like the GUT or Planck scale. Fan of the SM that I am, that's too much for even me to swallow as plausible.

If you are being kept awake by the Higgs metastability issue, you'll want to know the Higgs mass as accurately as possible. The rumor from ATLAS is that the difference in best fit masses between the gamma-gamma and ZZ channels has narrowed, with gamma-gamma moving up slightly to 126.8 GeV, ZZ quite a bit, to 124.3 GeV.

Comments

1. **mass**
   February 21, 2013

   Knowing the Higgs mass as accurately as possible will require a lepton collider, either e+e- ILC or a muon collider, or both. Hadron machines can do search and discovery, and they can access states which lepton colliders cannot directly produce, but high precision spectroscopy requires lepton colliders.

2. **Mitchell Porter**
   February 21, 2013

   "that's too much for even me to swallow as plausible"

   Shaposhnikov and Wetterich managed to *predict* the Higgs mass in 2009, based on the assumption of a grand desert, and some Planck-scale boundary conditions. Doesn't that count for something?

3. **Peter Woit**
   February 21, 2013

   Mitchell Porter,

   That the boundary condition of the quartic Higgs coupling going to zero gives the right Higgs mass definitely counts as at least intriguing, if not more. That doesn't though imply enough understanding of what is going on at those energy scales to believe the metastability calculation. It also doesn't imply that it's a good idea for a theorist sharing a press conference about the Higgs with two LHC experimenters to start jabbering about the annihilation of the universe, ensuring that's the story the press covers…

4. **Curlo**
   February 21, 2013

   In order to understand the significance of the new measurement, we need to know not only its central value but also its uncertainty. One would expect the uncertainty to go down with more data. How did you compute the 1.5 sigma excess you report ?

5. **ohwilleke**
   February 21, 2013

   Shaposhnikov and Wetterich are assuming asymptotic safety (i.e. subtle hypothetical new physics justified on quantum gravity grounds), which from a practical nuts and bolts perspective is just another way of saying the the running of the coupling constants and masses with energy scale is slightly different from a Standard Model beta function decrees, at many, many orders of magnitude above anything that could ever be recreated physically, based on a handful of low energy data points empirically in a model that acknowledges that it does not consider quantum gravity in any respect.

   Generically, any high energy scale tweak to one or more of the beta functions of the Standard Model is going to have the same potential to resolve the metastability issue, and given the closeness of the Standard Model result to true stability, the nudge need only be every so slight.

6. **srp**
   February 21, 2013

   I'm slightly puzzled by this stance. Normally, our host doesn't like going BSM without evidence and argues that there hasn't been any evidence turned up yet for anything BSM. Here, from what I can discern, the calculation at issue is "radically conservative" in the sense that it takes the SM as gospel truth and extrapolates it as far as possible. That sounds like exactly the sort of exercise you'd want to do with a theory you were pretty confident about. I mean, extending quantum mechanics to black holes is a pretty big reach beyond existing data, but that activity doesn't seem to have gotten similar flak around here, presumably because people are very confident about QM. What am I missing?

7. **Peter Woit**
   February 21, 2013

   Curlo,

   The uncertainty has gone down, but so has the deviation from the SM value. At least that's the rumor….

8. **P**
February 22, 2013

Peter,

Any word on the ATLAS "Twin Peaks"?

Thanks for the updates.

Excited for Moriond,
P

9. **Peter Woit**
February 22, 2013

P,

See the last paragraph of the posting…

10. **Pi**
February 22, 2013

Dear Peter,
You said you'd write about the new Arkani-Hamed et al paper on supersymmetry. I hope you haven't forgotten.
Thanks a lot for your updates.

Best,
Pi

11. **Thomas Larsson**
February 22, 2013

If gamma-gamma excess has gone down from 1.8 to 1.65 times the SM value, it means that the excess in the new data set is considerably lower, right? Isn't that what you would except from a statistical fluke?

12. **MathPhys**
February 22, 2013

Pi, Nima doesn't have a new paper on supersymmetry, does he? Or do you many the paper with many other people on the Grassmannian in scattering amplitudes?

13. **Pi**
February 22, 2013

MathPhys,
Here it is: http://arxiv.org/abs/arXiv:1212.6971
Simply Unnatural Supersymmetry.
Meanwhile, you might find this interesting: https://twitter.com/grahamfarmelo/status/304313270511214592

14. **Paolo**
February 24, 2013

Peter,

do the rumors imply that the option that the Higgs was just a statistical fluke is gone forever? After all, the bump is very small compared to the background. My colleagues say, behind closed doors, that even if the Higgs were a statistical fluke, human nature would prevent from admitting it. This is because of the French and Italian concept of "honour", they say.

But is the data strong enough now for a Nobel prize? Until now, the number of events was extremely small, and there easily could be some selection bias. You once mentioned in your blog that "half of ATLAS was sleeping with half of CMS"…

15. **Yatima**
February 24, 2013

> This is because of the French and Italian concept of "honour", they say.

What!

1) This is not a collaboration by French and Italian research groups only.
2) We are not talking about Imperial Japan, Prussia Gone Bad or US politicians. We are talking about responsible people able and willing to check their ideas and data against an actually existing oracle over long timescales.
3) The fact that the FTL neutrino results were thoroughly verified and ultimately retracted empirically proves this bizarro idea wrong.

16. **Peter Woit**
February 24, 2013

Paolo,

The evidence for the Higgs is overwhelming. There are large, well-defined peaks undeniably visible in two different channels, at the two different experiments. Any one of these four peaks would be quite convincing evidence that something is there, seeing four independent ones at close to the same mass removes any possible doubt. It may have even gotten to the point that the experiments will stop reporting combined statistical significance numbers for their Higgs results, since the number would be something large and uninteresting.

As Yatima points out, whatever the problem caused by the honorable French and Italians, there are plenty of dishonorable Americans and others to make up for it. As for confirmation bias and bed partners, the phenomenon of people taking delight in the prospect of proving that their nearest and dearest is wrong, and they are right, is not exactly unknown….

The State of SUSY

February 27, 2013
Categories: Uncategorized

Results putting new limits on SUSY based on the entire first run of the LHC are starting to emerge (see for example this from CMS) with more likely at Moriond next week. Since one is dealing with a theory with a large number of parameters, these are hard to characterize in a simple way. One thing to focus on is the limits on gluinos, since just about every popular version of SUSY says these should be about the easiest thing for the LHC to see. Very roughly, the Tevatron was able to set typical limits of about 300 GeV on such things, and the LHC at 8 TeV (4 times the Tevatron) is now giving limits around 4 times higher, 1.2 TeV. This is not likely to change much for the next few years until after the LHC comes back at 13 TeV in 2015. One can with some confidence predict that the gluino mass limit will then go up to about (13/8)*1.2 TeV or around 2 TeV, maybe a bit more in years after that with a high-luminosity LHC. Farther out in time, the next machine under discussion that could raise the limit is the HE-LHC, at 32 TeV, giving limits around 5 TeV. The time scale for this though is something like 2030-40, even assuming such a project ever were to get funded. I suspect the right characterization of that project might be "not in my lifetime".

There is a new paper out claiming to see evidence of a gluino in the data around 1000-1100 GeV. The same authors (see here), have been claiming to see such gluinos since the early LHC data, first at around 7-800 GeV, with a mass getting higher with each round of new data and higher mass limits.

Few are likely to pay attention to this, but what is getting taken much more seriously is the case that Nima Arkani-Hamed has been vigorously making recently (see for example his talk at the Higgs Symposium). Arkani-Hamed is now by far the most influential theorist in this area, with slides from his latest talks often appearing in many other people's presentations, functioning as the embodiment of the conventional wisdom of the field. He also is the only phenomenologist with a $3 million Fundamental Physics prize, awarded for his work on models that have had great influence, although zero success experimentally.

One of these, split supersymmetry, is what he is now promoting as the explanation for the negative LHC results. In this model, which he developed with Savas Dimopoulos back in 2004, the main argument for SUSY, the hierarchy argument, gets abandoned in favor of anthropics. The Higgs mass and the electroweak scale are what they are not because of SUSY, but because otherwise physics would be different and we wouldn't be here. Once one abandons the hierarchy argument, the remaining arguments for SUSY are extremely weak (I'll try and explain these in more detail in a separate posting), but for some reason Arkani-Hamed still thinks the idea is worth promoting and that vindication for his $3 million may yet be had.

Split SUSY works by moving all scalar superpartners up to unobservably high energies, but a few particles including the gluino are supposed to be at potentially observable masses. Back in 2004, Arkani-Hamed and Dimopoulos were hopeful about the possibilities for the LHC seeing a split SUSY gluino, writing:

> However, at peak luminosity of 30 fb^{-1} per year, the LHC may well be a gluino factory producing roughly a gluino per second(for m_g ~ 300 GeV).

These hopes have now been dashed, and at the Higgs symposium talk, illustrative spectra show gluino masses at 2.1 and 2.3 TeV (this may just be because that's about the limit of what the LHC could see). Arkani-Hamed and co-authors have a recent paper out discussing Simply Unnatural Supersymmetry, i.e. "the simplest picture of the the world arising from fine-tuned supersymmetric theories". Here calculations are done for gluino masses ranging from 1.5 to 15 TeV, and the story is that we'll have to be lucky to get any experimental evidence for this model. They end with:

> If Nature has indeed chosen the path of un-natural simplicity, we will have to hope that she will be kind enough to let us discover this by giving us a spectrum with electroweak-inos lighter than ~ 300 GeV or gluinos lighter than ~ 3 TeV.

So, the current state of the conventional wisdom about SUSY from its most influential proponent is pretty much the following. It's still the thing to try and sell to the public as the best bet for the future of physics, but the hierarchy argument is gone, and at a fundamental level it's anthropics, the landscape and the multiverse. He's pretty much given up hope of ever getting any experimental evidence for this, other than the outside possibility of maybe the gluino mass being just low enough to be visible in rare LHC events late in the decade.

The interesting question about all this I think is a sociological one: will this untestable and rather ugly theory based on anthropic reasoning become widely seen as the "best hope" for fundamental particle theory? In a post-LHC world where mankind has abandoned the high-energy frontier, will the conventional wisdom of the textbooks be that SUSY and those gluinos must be there, but unfortunately happen to be just out of reach?

Update: For a survey article that just came out this evening, which tries to show that the main argument for SUSY (the hierarchy problem) is not quite dead yet, see here.

Update: New Scientist has a special section this week about "Crunch time for physics" (unfortunately mostly behind a paywall). On SUSY, Frank Wilczek is still a believer, based on the renormalization group calculation he was a co-author of back in 1981. If no SUSY turns up at the next LHC run though, even he will throw in the towel:

> I cannot believe this success is an accident. But in science faith is a means, not an end. Supersymmetry predicts new particles, with characteristic properties, that will come into view as the LHC operates at higher energy and intensity. The theory will soon undergo a trial by fire. It will yield gold – or go up in smoke.

He has a bet with Garrett Lisi that superparticles will be detected by July 8, 2015.

Comments

1. **Pi**
   February 27, 2013

   Peter,
   Many thanks for posting this.
   I don't think, if SUSY doesn't show up at the LHC, physicist can easily persuade governments to build another gigantic collider. 2030-2040 is too optimistic I think.
   So what will HEP theorists do 1)if SUSY doesn't show up 2) in the meantime for another collider?

2. **Mitchell Porter**
   February 27, 2013

   The structure and the parameters of the standard model will still need explaining. And if anyone ever manages to hit on the right explanation, presumably it will predict the next decimal place in the parameters. So experiment won't be completely dead, even if there's nothing beyond the Higgs.

3. **John**
   February 27, 2013

   HEP theorists will be fine. Maybe you're wondering about phenomenologists? They'll just keep doing what they've been doing for the past decade or so, up to and including the LHC period. Invent various random scenarios of zero value. The only thing the LHC did was confirm what we already pretty much knew. Actually it did two things. It also artificially boosted the number of pheno hires, for a "just in case something shows up we need many monkeys to figure it out" scenario.

Since the LHC taught us nothing new, I'm not sure why we should be so eager to build yet another one. There's plenty of other stuff do anyway. Also things like dark matter detectors are much cheaper and there are actually better motivations behind those. We actually expect dark matter, which we know nothing about. We aren't however expecting anything at the LHC, it's all just "spray and pray".

4. **M. Wang**
February 28, 2013

It had been known since before the Great Depression that, in aggregate, stock analysts' predictions produce negative risk-adjusted returns, but thousands of people still make it a living to this day, some being paid 7-digit salaries. It is even considered a serious offense to discuss their track records in industry gatherings.

By the 1990's, solid statistics had proven beyond doubts that mutual fund managers as a whole added negative values after fees, but the industry continued to grow. It now manages assets well in excess of the US GDP.

As of last month, the latest statistics on hedge funds also showed a negative overall return after fees in aggregate. Yet, your university may have to cut hiring further next year because its endowment fund loses a few more billions in various hedge funds.

The point of my long-winded examples is that, in a complex society, there is plenty of room for BS-artists to prosper as a group or even industry. My bet is that the likes of Brian Greene and Lisa Randall will continue to sell plenty of books, be invited to guest-star in TV shows and give inspirational speeches to young students in science. Of course, the dark matter may come to rescue HEP from ignominious heat death, but what if it doesn't? Is there any reason why dark matter has to be WIMP? Does a WIMP have to have a signal measurable within the next 20 years? What if nothing shows up?

5. **Umesh**
February 28, 2013

Why do you have to keep trashing genuine attempts at explaining nature in a rather negative and totally un-constructive fashion?

6. **Shantanu**
February 28, 2013

Peter (or others), is there any current serious attempt to link SUSY models to neutrino masses/mixings etc? Way back in 1998 when first non-0 evidence for neutrino mass was found, P. Ramond claimed its evidence for low energy SUSY.

7. **Yatima**
February 28, 2013

@John

"The only thing the LHC did was confirm what we already pretty much knew."

Woah I must have missed that part. It's good to know that we already "pretty much know" everything without even looking for it. Maybe your excellency will just spell out what else we already know and don't actually need to check?

8. **Thomas Larsson**
February 28, 2013

Theorem [Larsson 2007]
Supersymmetry will not be discovered at the LHC.

Proof: String theory predicts supersymmetry (Witten 1984-2002). String theory predictions are always wrong. Hence supersymmetry does not exist, and will in particular not be found at the LHC. QED.

Corollary
Lubos Motl will lose his experimental-susy-by-2006 bet.

9. **Tmark48**
February 28, 2013

Oh boy oh boy, from hierarchy to anthropic justifications.
SUSY is going from bad to worse. Unbelievable, simply unbelievable.

10. **Eric**
February 28, 2013

Actually, it's very possible that supersymmetry solves the hierarchy problem with an acceptable amount of fine-tuning even if the gluino, sleptons, and squarks all have multi-TeV masses, and consquently cannot be observed at the LHC. In the context of minimal supergravity, there is a region of parameter space on the hyperbolic branch called the focus point. Spectra which lie on the focus point have low fine-tuning which is fairly insensitive to the scalar masses. For these spectra, it is easy to have the correct Higgs mass and a dark matter relic density which satisfies the WMAP limits. Since the scalar masses for these spectra are heavy it may not be possible to observe them at the LHC. However, the direct dark matter detection cross sections are within reach, so this is where supersymmetry may be found even if there is no chance of finding it at the LHC. A linear collider should be able to study the neutralinos and charginos for these spectra. I would suggest reading the review article of Jonathan Feng which is out on hep-ph today rather than misrepresenting the situation.

11. **Peter Woit**
February 28, 2013

Eric,

I did link to the Feng article and encourage people to read it who want to see the state of attempts to find still viable "natural" SUSY models. Arkani-Hamed makes it clear that he now sees these things as too complicated and contrived to be plausible, which is why he's promoting the idea that split SUSY is the best hope. You should argue the with him the question of whether "focus-point" models are plausible, he's not even mentioning them as worth discussing…

12. **Noah Smith**

February 28, 2013

So my question is…What's up with Arkani-Hamed? Does he really believe the stuff he's selling here, or is he just trying to stay in business for another decade?

13. **Peter Woit**
February 28, 2013

Noah Smith,

The interesting thing about Arkani-Hamed is that for quite a few years now, most of his research work has been in a completely different direction than BSM physics like SUSY. Instead he's studying new ideas for a very different formalism for computing scattering amplitudes (the"positive Grassmanian") and hopefully reformulating QFT in a dramatic way. His talks about BSM SUSY tend to focus on the issue of "What are the only remaining conceivable forms of SUSY that are viable?", not on whether BSM SUSY is a good idea or not.

Why he (and a lot of other prominent theorists, at the IAS and elsewhere) have decided to keep promoting this as it becomes an ever more obvious failure is a good question, with the only answer I see that they feel that they and some of their colleagues have a lot invested in it. My impression is that most theorists have long ago given up on BSM SUSY as hopeless (see for instance the HEP phenomenologist bloggers like Jester and Matt Strassler). Why it continues to get so much positive attention is somewhat of a mystery. Arkani-Hamed would do the field a huge service if the next time he were asked to discuss SUSY and BSM physics he were to give a talk explaining why it's no longer a plausible idea, and instead start promoting fundamental mathematical physics work of the sort he's now himself doing.

14. **King Ray**
February 28, 2013

If you read Thomas Kuhn's The Structure of Scientific Revolutions, you will recognize that we are in a stale period between paradigms. The SUSY/String Theory paradigm has not panned out after nearly 40 years of effort.
I believe that if you have a theory, you should either be able to test it experimentally, e.g., the bending of light predicted by General Relativity, or have it be able to explain a previously unexplained fact or phenomenon, e.g., the precession of the perihelion of Mercury. Otherwise you are not using the scientific method (where you test your theories), and you are going to be led astray by false aestheticisms. I heard that Feynman suggested that if superstring theory were the theory of everything, why couldn't string theorists predict the mass of the electron?

15. **Eric**
February 28, 2013

Peter,
Arkani-Hamed hardly represents the majority view of phenomenologist who work on supersymmetric models. His is actually a fairly extreme position which you are using as a straw-man. The bottom line from my own analysis is that the superpartners can easily have masses greater than 5 TeV and the hierarchy problem can still be solved with only minor fine-tuning. If you were more of a scientist and less of a propagandist, you would know this. At any rate, pay attention to the next update from the dark matter direct detection experiments.

16. **Peter Woit**
February 28, 2013

OK Eric, interesting to hear that the majority of phenomenologists see Arkani-Hamed as an extremist (and a propagandist too I guess since you're characterizing me that way when I'm just quoting him..). Also interesting to hear that SUSY proponents are now looking for vindication from dark matter direct detection, not the LHC.

King Ray,

I agree that what we're seeing is the collapse of a paradigm. The problem is that the new paradigm being heavily sold is multiverse mania. Not a pretty situation. Interestingly, string theory has kind of dropped off the map as irrelevant, since it says zero about anything testable, at the LHC or elsewhere (and that's the problem, that it predicts nothing at all, not that it doesn't predict the electron mass).

17. **Shantanu**
February 28, 2013

Eric,
To turn it around, if dark matter experiments see nothing, would you be willing to change your opinion and agree that SUSY is dead?
Shantanu

18. **Bob Jones**
February 28, 2013

"He also is the only phenomenologist with a $3 million Fundamental Physics prize, awarded for his work on models that have had great influence, although zero success experimentally."

The prize was also awarded for his very interesting work on scattering amplitudes.

"string theory has kind of dropped off the map as irrelevant"

This statement is just delusional. If you look at the new hep-th submissions on arXiv, you will see that about half the articles are string theory articles. As long as string theorists are coming up with interesting results, string theory is not going to go away.

19. **Bob**
February 28, 2013

Breaking news: phenomenologist gives talk about particle physics at particle conference! Woit misunderstands phenomenologist and gives a rant about the end of physics.

20. **King Ray**
February 28, 2013

Bob,

"hep-th submissions on arXiv"? How about "confirmed predictions at the LHC"?

21. **Anonymous**

February 28, 2013

@King Ray

The idea is that SUSY does (or at least, did) explain a previously unexplained phenomenon, namely the unnaturally low electroweak scale, in a very plausible way. Nowadays, criticisms of SUSY are that its explanation has become inconsistent with data, or at least that the continuing interest in SUSY is not reflecting the present absence of direct detection when in the most plausible models we should have seen something already.

22. **Mitchell Porter**
February 28, 2013

The conventional reasons for believing in supersymmetry, known to me, are hierarchy problem, gauge coupling unification, dark matter, and (for theorists) string theory.

But you don't need the full supersymmetric spectrum to obtain dark matter or gauge coupling unification. So if anthropic finetuning were to become the new paradigm for explaining the Higgs mass, presumably that would weaken supersymmetry's popularity in the long term. It could end up being regarded as a Planck-scale phenomenon only.

23. **Bob**
February 28, 2013

This is wrong. The only indication we ever had that susy is right is its prediction of gauge coupling unification. This fact has not been undone by the current data, no matter how much ppl might kick and scream about it. Let's also remember that every other proposal for new physics at the weak scale is clearly dead. Susy is the only remaining idea that hasn't been totally killed yet, though low energy susy is a bit injured, while high energy susy is still fine.

24. **Bob Jones**
February 28, 2013

King Ray,

Most of the string theory articles submitted to arXiv are about applications of the AdS/CFT correspondence, and while they do not make BSM predictions, some of them do make contact with experimentally testable phenomena in nuclear and condensed matter physics.

So far, the only prediction of new physics which has been confirmed at the LHC was the prediction of the Higgs boson. Besides Higgs et al, nobody has made a successful prediction, so I don't think it makes sense to criticize string theory specifically for not making LHC predictions.

25. **Peter Woit**
February 28, 2013

Bob Jones,

It's not "his" work on scattering amplitudes, he's only one of quite a few people who have worked on this. If there was going to be a $3 million award for advances in scattering amplitudes, it would be going to other people.

The reference to string theory "dropping off the map" was about BSM physics and the LHC. I think the consensus is now quite clear that string theory is irrelevant to the LHC, as well as any other conceivable experiment. You virtually never anymore see what used to be the usual nonsense in the press about "predictions of string theory", as string theorists have mostly given up putting out such press releases.

"Bob",
Perhaps you could consider explaining just what it is that I don't understand here.

"The only indication we ever had that susy is right is its prediction of gauge coupling unification."

So, that whole hierarchy thing, no one ever really meant those arguments for susy? And the WIMP miracle, that's not serious either? The only argument for SUSY is that if you believe in a desert up to the LHC, and you believe in GUTs (for which there is zero evidence) then you get a number coming out right to 10-20%? That's all that SUSY has ever had going for it? Wow…

26. **onymous**
February 28, 2013

I'll probably regret wading into physics blog comments again, but:

I thought the idea that the focus point scenario can lead to low fine-tuning even with multi-TeV scalars had been unceremoniously buried many years ago. It seems to require a really odd set of assumptions to be believed (for instance, you're allowed to ask how sensitive the theory is to varying a universal scalar mass, but not to varying the top Yukawa or varying scalar masses in a nonuniversal way). I'm not really sure why it's being resurrected. If you really think you understand the set of top-down theories that could be realized in nature well enough to make this argument, more power to you, but the argument is often repeated without all the necessary caveats.

It's my belief that all models of natural SUSY suck. (I worked on one myself, and it sucks too.) Direct SUSY limits are mildly troubling, but the elephant in the room is the Higgs mass, which just obliterated almost all sensible models of natural SUSY. The contortions people go through to save a bit of fine-tuning just seem absurd to me. Complicating your theory to save fine-tuning isn't really winning. I think it is important and interesting that the LHC rule out these scenarios, but none of them are remotely plausible. Maybe if you posit compositeness right around the corner or something extreme like that you can find some room to live, but it seems unlikely that Nature is that kind of Rube Goldberg machine.

It's a really, really unpopular opinion in the hep-ph community, but I think what Nima says is exactly right: the only plausible-looking incarnation of SUSY we have left is the "semi-split" scenario with scalars at 100 or 1000 TeV. It's still a hell of a lot more natural than the Standard Model alone, and it gives gauge coupling unification, so I wouldn't bet against it. This is not your grand-advisor's SUSY scenario, though, and the community should be more honest about that.

The Opera neutrino anomaly basically wiped out any illusions I had left that the hep-ph community as a whole was serious and intellectually honest.

27. **Bob Jones**
March 1, 2013

"It's not 'his' work on scattering amplitudes"

No, it's certainly not his exclusively, but he has played a leading role in recent work on scattering amplitudes, and this is acknowledged in the prize citation.

"You virtually never anymore see what used to be the usual nonsense in the press about 'predictions of string theory'"

Yeah, but press releases are not an accurate indicator of what's happening in a field. The fact that journalists are not talking about "predictions of string theory" does not mean that string theory dropped off the map. String theory is still a very important direction in current research, and regarding LHC predictions, the situation for string theory has not changed recently.

28. **chris**
March 1, 2013

i would pity the poor susy phenomenologists for the miserable state they are in: having to defend an obviously obsolete theory. i would pity them had they not all the best-paying and most prestigeous faculty positions.

for the greater good of physics one can only hope that their failure will at one point be publicly recognized as complete and they will be ridiculed out of their preposterous ideas. i fear that nothing else will stop this madness.

29. **Anonyrat**
March 1, 2013

So Arkani-Hamed won $3 million for work done in the last two years? That is the conclusion it seems one must draw if one looks at the top-cites for 2010:
http://www.slac.stanford.edu/spires/topcites/2010/annual.shtml

(I can't find a more recent topcites.)

30. **prepo**
March 1, 2013

Sir Joseph Larmor is remembered today for Larmor precession and his formula for the radiation by a nonrelativistic accelerating charge, yet in his day his most famous work was his Adams Prize essay "Aether and Matter". You can still buy it of you wish
http://www.amazon.com/Aether-matter-Joseph-Larmor/dp/B00385WM14

Larmor was appointed to the Lucasian chair of mathematics. He retired in 1932 and was succeeded by Dirac. Nobody laughed Larmor out of his preposterous ideas.

31. **Anonyrat**
March 1, 2013

Ah, here is the INSPIRES 2011 annual list:
http://inspirehep.net/info/hep/stats/topcites/2011/annual.html
and the 2011 all-time list:
http://inspirehep.net/info/hep/stats/topcites/2011/alltime.html

Still no indication.

32. **Anonyrat**
March 1, 2013

Larmor's 1900 essay "Aether and Matter" is available online here:
http://archive.org/details/aetherandmatter01larmgoog

Wiki says:
".Larmor opposed Albert Einstein's theory of relativity (though he supported it for a short time). Larmor rejected both the curvature of space and the special theory of relativity, to the extent that he claimed that an absolute time was essential to astronomy (Larmor 1924, 1927)."

So I guess prepo's point is borne out.

33. **Peter Woit**
March 1, 2013

Anonyrat,

Arkani- Hamed's http://arxiv.org/abs/hep-ph/9803315 (with Dimopoulos) has about 4500 citations, making it one of the most influential and heavily cited HEP papers of all time. On the all-time top-cites list you give, it is number 7, led only by Weinberg's 67 paper, Kobayashi-Maskawa since everyone doing flavor physics quotes that, the 3 early papers on AdS/CFT that everyone quotes, and Randall-Sundrum. It's the first thing mentioned in his Milner prize citation. The other remarkable thing about it is that no one ever seriously believed this kind of model was plausible, and LHC results quickly ruled it out at accessible energies. I suppose you could claim that maybe it will show up at higher energies, but I don't know of anyone who would bet on that, even given very advantageous odds.

All IAS HEP professors got Milner prizes, and Arkani-Hamed got his IAS professorship around the time he started working on amplitudes. Put differently, if he had never worked on amplitudes, he'd still be an IAS professor, and surely also a Milner prize winner…

34. **Anonyrat**
March 1, 2013

Thanks for clearing that up. I thought Bob Jones has written that Arkani-Hamed's $3 million prize was for his work on scattering amplitudes, and since A-H's most cited paper was on extra dimensions, and none of his scattering amplitude work upto 2011 was cited any where close to that, I was quite puzzled.

35. **Thomas Larsson**
March 1, 2013

Regarding Wilczek, recall what he wrote in his Future Summary back in 2001 (p 20):

"5.5. Produce the New Particles!
Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new R-odd particles. Even in the focus point scenario, **there must be several accessible to the LHC**"

36. **new particles**

March 1, 2013

Let's not get carried away about the failure of new particles to appear at LHC energies. With all of the euphoria of the success of QED renormalization in the post-WW2 years, *nobody* predicted the appearance of the V-particles (later called strange particles). In later years, after the discovery of charm, *nobody* said there would be a third generation of quarks, and there would be a new family of narrow resonances at approx 10 GeV. By 1975 the Standard Model was in place, but the Upsilon was discovered in 1977. The SM didn't predict the (b,t) generation. Although once the b was found, the top was immediately claimed to exist, but no theorist could quantify its mass. Gordy Kane's calculation notwithstanding, no theorist (from 1960s to 2000s) could quantify the mass of the Higgs either. There isn't really any quantitative non-SUSY (or non-string) prediction of what the next generation of BSM particles will be, nor what their mass scale will be. (I suppose there is the axion …)

37. **Philip Gibbs**
March 1, 2013

new particles, the third generation was predicted in 1973 by Makoto Kobayashi and Toshihide Maskawa to account for observed CP violations. I think you are right that nobody predicted strangeness though, or even muons.

38. **Philip Gibbs**
March 1, 2013

New Scientist also has a quick interview with Milner this week.

39. **new particles**
March 1, 2013

Aha. The appearance of the Upsilon at approx 10 GeV was a surprise, though. Nobody knew the mass scale of the third generation quarks. This was very clearly demonstrated by the top quark. Once the b was found, nobody doubted the t would exist. But nobody had the faintest idea of its mass. A whole string of machines (PEP, PETRA, TRISTAN, LEP … others?) all failed to produce the top. Eventually the Tevatron did, but there was no *a priori* proof that the design energy of the Tevatron (as a collider) would be sufficient to produce the top. As for the muon, that is a classic. It took many years to demonstrate that the muon was NOT the Yukawa particle, and to find that there were in fact two particles, the pion and the muon. And as Isidor Rabi famously said, when the muon was recognized to be a lepton, "Who ordered that?" Nobody expected a heavier version of the electron to exist.

SUSY may be getting more and more constrained in parameter space, but for the most part nobody has ever had any really good idea of the mass scale and properties of the next generation of particles.

40. **Mitchell Porter**
March 1, 2013

"new particles" said

"Gordy Kane's calculation notwithstanding, no theorist (from 1960s to 2000s) could quantify the mass of the Higgs either"

Shaposhnikov and Wetterich obtained it http://arxiv.org/abs/0912.0208 … by assuming a desert between the weak scale and the Planck scale, and by assuming that the Higgs quartic coupling goes to zero at the Planck scale.

Also, to be fair, Dharwadker and Khachatryan http://arxiv.org/abs/0912.5189 managed to predict it … in a crackpot paper which issued in the formula "Higgs mass equals half the sum of W+, W-, and Z masses".

41. **new particles**
March 1, 2013

Good! So tell us all the mass of the nearest BSM particles, and their symmetries and interactions! There's no need to post items about the lack of observation of SUSY!

42. **Pi**
March 2, 2013

Is there any chance that the LHC tell us at least if there's such thing as unnatural SUSY that Arkani-Hmaed et al are talking about?

43. **Peter Woit**
March 2, 2013

Pi,

According to the authors of the paper whose final part I quoted, the answer is "only if we're lucky".

44. **Cliff**
March 2, 2013

"Once one abandons the hierarchy argument, the remaining arguments for SUSY are extremely weak"

Thats simply not true. Naturalness is a *rough* guide to finding the correct description, but it is *not* an inconsistency! By contrast, any of your attempts to describe the quantum field theory together with general relativity without using SUSY go 'Glub, glub, glub' right to the bottom of the sea. There is just no way to argue that the aesthetic criterion of naturalness (as useful as it may be in many cases) is more important that mathematical consistency.

From what I understand about F-SU(5), it is confined to a distinct window of parameter space and can be definitively ruled out at the LHC. I think its pretty awesome that a totally falsifiable model is putting forward explanations for the vacuum energy problem and other things. It looks kind of unfair to me that you've reduced it to a cheap punchline that supports your own eternally pessimistic viewpoint. The F-SU(5) explanation for why most SUSY searches have come up empty seems worth taking seriously: the cuts on missing transverse energy are too high, and the production of superpartners may be very soft.

Decrying the "conventional wisdom" of using supersymmetry as a fundamental principle for unification isn't actually going to help advance the field in any way. Especially as long as SUSY remains the only mathematically viable principle to put the 'big picture' together. But if SUSY turns out not to be a feature of the low-energy world, that may be that. As you've implied, there may unfortunately be an end to what we can see. That's not some grand crisis, that would just be life. But I remain somewhat more optimistic at this point.

45. **Peter Woit**
March 2, 2013

Cliff,

I don't think it's me who has reduced F-SU(5) to a "cheap punchline", but rather those who think it is a good idea to put out a new paper every few months about their latest "predictions" and how they are seeing (or smelling…) evidence for this theory in the data, just to have this conclusively falsified days or weeks later by new data (leading to another paper…). Maybe in 2016 or so these authors will stop smelling things and write a paper acknowledging that the idea is wrong. I'll believe that when I see it.

As for a "grand crisis" caused by failure to find SUSY, my point of view is rather that not finding SUSY at the LHC might finally start causing people to give up on a bad idea (SUSY extensions of the SM like the MSSM) and working on something more promising. So, not a "grand crisis", but an opportunity, one that some prominent theorists are digging in their heels trying to stop from arriving.

46. **Garrett**
March 2, 2013

The gauge coupling unification result from SUSY is impressive, but this can happen more economically by just adding a handfull of Higgs multiplets.

47. **King Ray**
March 2, 2013

Mitchell,

Thanks for the link to the Shaposhnikov and Wetterich paper. In their closing statements they say

"Detecting the Higgs scalar with mass around 126 GeV at the LHC could give a strong hint for the absence of new physics influencing the running of the SM couplings between the Fermi and Planck/unification scales."

This is very interesting, and may not bode well for BSM physics. It is intriguing that a prediction of the Higgs mass might arise from SM and gravitational considerations. It will be curious to see how well their prediction matches the final Higgs mass.

48. **Bob**
March 3, 2013

" . . . my point of view is rather that not finding SUSY at the LHC might finally start causing people to give up on a bad idea (SUSY extensions of the SM like the MSSM) and working on something more promising."

What looks more promising these days?

49. **Toni**
March 3, 2013

The susy experts all speak and dream of coupling constant unification. But there is no good reason why these should unify. This is a wish, a hope, a dream. Grand unification, even as an approximation has no basis. As long as we suspect that the dream is true, we will not get susy in the waste basket – where it belongs.

50. **Peter Woit**
March 3, 2013

Bob,

Pretty much anything. Arkani-Hamed himself is working on amplitudes. There's a huge amount about the non-perturbative behavior of quantum field theories that we don't understand.

No, there isn't some obviously better idea about BSM physics that everyone should work on instead of SUSY. But if people can't think of something more worthwhile to spend their time on than a failed idea that has already received a huge amount of attention, maybe they should be in a different business.

51. **Toni**
March 3, 2013

Bob,

the most promising would be an idea that keeps the standard model intact and just explains its parameters. Such an idea would have the advantage to agree with experiments. Alas, the idea itself is still hidden in the clouds.

52. **Cliff**
March 3, 2013

Peter: About F-SU(5), I don't understand why it matters if they write a paper in 2016 saying they were wrong. Nobody has to bother reading what they see or smell in the data because they predict that new particles will be discovered by the LHC! Again I just fail to see what is so terrible about them describing that the best fit for some superpartner mass has increased. They've just released a new paper because there are new searches that will soon be published that they want to compare their model to. You seem certain that they are behaving dishonestly but you don't seem to want to make that case based on any actual details of their claims.

My bigger beef is that you're conflating SUSY as a high-energy principle with SUSY as a feature of sub-TeV scale physics. Obviously it would have been nice if these two things went together, but there is no justification at all for saying the absence of SUSY at a scale of 1 TeV implies that its not a useful (or necessary) principle for high energies.

The best ideas for what lies beyond the next bend haven't actually changed much. Its obviously understandable why that fact is frustrating, but that seems like the inevitable implication of the logic here. Ingenius and totally unexpected insights are welcome of course, but until/unless they get here, not much has changed.

53. **Peter Woit**
March 3, 2013

Cliff,

I don't believe the F-SU(5) people are dishonest, but assume that they actually believe that SUSY will appear in the next release of data. However, if you keep publishing papers with such claims that don't have much to back them up, and always turn out to be wrong, at some point your credibility goes to zero, which I think is the case for them right now.

That SUSY was supposed to explain the hierarchy problem, and so appear before 1 TeV, was probably its most heavily advertised feature and best argument for paying attention to it. The failure of this seems to me worth noting and has major implications for the whole idea. Post that failure you can try and claim that SUSY is a wonderful idea, that adding huge numbers of unseen degrees of freedom with huge numbers of new parameters, in a way that explains nothing about observable physics, is something that people should take seriously as our best idea about how to move forward in fundamental physics. I don't think though that many people are ever going to buy this.

54. **dark**
March 4, 2013

"No, there isn't some obviously better idea about BSM physics that everyone should work on instead of SUSY. But if people can't think of something more worthwhile to spend their time on than a failed idea that has already received a huge amount of attention, maybe they should be in a different business."

PW How would a discovery of dark matter in the coming years from various research groups affect your scientific judgment on the plausibility of SUSY?

(and for SUSY advocates like Eric, how would the failure to detect DM in increasingly sensitive and sophistic experiments affect SUSY)

55. **Eric**
March 4, 2013

Dark,

The bottom line is this: There is a good chance that the scalar quarks and leptons are too heavy to produce a signal at the LHC. However, even in this case supersymmetry can still solve the gauge hierarchy problem. Furthermore, it can do so without requiring much fine-tuning (the little hierarchy problem) for certain regions of the parameter space. Most of the superpartner spectra which fall into this category produce a relic neutralino density which is at or below the WMAP constraint. In addition, the proton-neutralino cross sections for direct detection are just at the point where they are within reach of experiment. Thus, it is quite possible that dark matter will be discovered in the near future and this would lend additional support for supersymmetry, even if no signal is seen at the LHC. On the other hand, if dark matter is not directly detected, there is always the possibility of R-parity violation or simply that the neutralino only provides a small fraction of the dark matter density.

56. **dark**
March 4, 2013

Eric
thanks for the reply. I understand that not finding SUSY at LHC can be explained away, and "R-parity violation or simply that the neutralino only provides a small fraction of the dark matter density"

what if they don't find any dark matter – neutralino – or otherwise, at all?

57. **chris**
March 5, 2013

Thus, it is quite possible that [*next big experiment will measure something*] in the near future and this would lend additional support for supersymmetry, even if no signal is seen at [*last big experiment that failed to produce anything about SUSY*]. On the other hand, if [* next big experiment will give no signal*], there is always the possibility of [* next level of complication moves SUSY out of reach once more *].

58. **punter**
March 5, 2013

Most of that New Scientist section is accessible through free registration for the next ten days.

59. **Kavanna**
March 18, 2013

There is a strong, general argument for SUSY, based on the Coleman-Mandula theorem. It's the one general type of symmetry that combines "internal" and "spacetime" symmetries allowing a natural combination of particle physics and gravity.

But that's a very general argument. It says nothing about implementation. The attraction for phenomenology has always been the power of SUSY's opposite-sign cancellations across many areas (loops, cosmological constant). These arguments all rely on perturbation theory, a shaky starting point that assumes everything's weakly coupled in the regimes we're talking about.

60. **Kavanna**
March 18, 2013

Thus, the argument here is, in large part, not about supersymmetry, but about the validity and limitations of perturbation theory. If the crucial physics is strongly coupled, a different approach is needed.

61. **CLAUDIE**
March 27, 2013

Dear Peter Woit,

I am a student of philosophy and will humbly admit I got half way through your article – and felt sad that I could understand so little. I do however understand the basic concept of SUSY and would adore an answer in 'layman's terms' what you feel the future is for SUSY ?

I would be so very grateful,

(Ask me anything about Eastern Philosophy — but Physics, I struggle, but read on anyway because it fascinates me 🙂

Claudie

62. **Peter Woit**
March 27, 2013

Claudie,

In layman's terms, SUSY (as in superpartners for the standard model particles) is an idea that has gotten a huge amount of attention, despite not explaining much of anything. If the arguments for it were true, it should have been seen before the LHC. Now that it is being conclusively experimentally ruled out at LHC energies, the idea is basically dead, although it is going to take some people quite a while to admit this (they're now saying "wait til 2015!", and I doubt they'll even give up then when results come in at the highest possible LHC energies).

63. **CLAUDIE**
March 29, 2013

Dear Peter Woit,

Thank you kindly for your concise & considerate answer. I guess I have a vested interest in SUSY because it embodies some of what is predicted or at least theorised about by verdict philosophy – which is my filed of expertise. Also being a teacher and humanitarian – I had hoped for a theory of wholeness & unity — to open up a new discourse, whereby children can be taught to nurture the earth and nature in general, as a component of their own essence. I probably sound like a tree hugger (and undeniably I am 🙂

Kind Regards,

Claudie

Quick Links

March 3, 2013
Categories: Uncategorized

This should be a month with quite a bit of experimental news, including

- Latest Higgs news from the LHC experiments here on Wednesday.
- Release of data from AMS-02 was advertised as "two to three weeks away" back on February 17.
- Planck data release on March 21.

A couple weeks ago, Arianna Borrelli of the Epistemology of the LHC project gave a talk at CERN (slides here). It includes some interesting data from surveys of HEP physicists in September 2011 and September 2012.

The Simons Foundation website keeps having some of the best writing on math and physics around. Natalie Wolchover has an excellent story about a complex subject, that of the role of computers in proof. The also have an essay by Barry Mazur about another complicated related subject, the nature of evidence in mathematics.

Finally, if you want to watch a very good introduction to D-modules, see video from David Ben-Zvi at MSRI here and here.

Update: Edward Witten will be speaking here in New York this evening (Monday March 4) at Hunter College, for more information see here. Unfortunately I have other plans and will have to miss this.

Comments

1. **jihn**
   March 3, 2013

   What happened with Moriond? they dont put the slides

2. **M**
   March 3, 2013

   The new results from LHC, Planck and AMS are important but there will not be any surprise, only confirmations of previous experiments and reduction of uncertainties

3. **Peter Woit**
   March 3, 2013

   jihn,
   Their website says slides will be posted only after the session is over (next Saturday), presumably here
   https://indico.in2p3.fr/conferenceOtherViews.py?view=standard&confId=7411

4. **srp**
   March 3, 2013

   The findings quoted below by Borrelli's group look very consistent with Lakatos's "Methodology of Scientific Research Programs." You have the "hard core" theoretical preconceptions that are not tested (e.g. SUSY), the "protective belt" of disposable models that mediate between the data and the hard core, and the use of overall judgments about the expanding or contracting nature of what the scientific research program can satisfyingly explain to decide amongst them.

   "these results do not support the traditional pictures of physicists comparing and
   preferring models/theories according to some criteria
   – models are rather regarded as exploratory tools for research than as serious
   candidates to a theory of new physics. Yet the general approaches (SUSY, extra
   dimensions…) are taken seriously ("theoretical cores" Borrelli 2012)
   – LHC results did not change much the pattern of (rather feeble) abstract
   preferences, but seem to have further eroded the belief in individual models –
   yet interestingly SUSY is somehow slightly, relatively better off (Sept. 2012!)"

5. **Yatima**
   March 4, 2013

   The article about using computers for proving theorems (and possibly for sniffing out or discovering theorems) is indeed interesting.

   Still, after 30 years of discussion there is still existential Angst about the usefulness or the fruitfulness of using symbolic manipulation by machine to break through problems … damn!

   After all, we know that proving theorems is not exactly in P (so the program needs to be very astute to make progress unaided) and that Hilbert's program is unattainable as such so there will always be fine-tuning and possibly unprovable theorems to make life interesting (though for some reason, one encounters these very rarely in practice). Humans won't be out of a job! At least until full AI that is demonstrably not unhinged has been mastered.

   Doubts about a proof done by machine ("by steam", if one so will), though it may never go away, can ultimately be brought down to an adequate level. I am sure most people will take "three independent programs have found proofs that are, unfortunately, too large or obscure to be checked by brain – let's start from there" over "no proof exists". And even then, the search of possibly nonexistent but elegant ("highly compressible"?) proofs can go on.

   Conversely, are there any proofs that were found to be wrong long after they had become "common knowledge"?

   Physicists seem to go for it instead of wallow in doubt here. Does this come with the territory?

   Recommended reading:

   The Four-Color Problem and its Philosophical Significance – Thomas Tymoczko, Journal of Philosophy, Feb. 1979 (not sure whether that link is legit though)

and

[The Four-Color Theorem Solved, Again – Casey M. Rufener, July 2011]

6. **Urs Schreiber**
March 4, 2013

For quantum field theory the interesting aspect of the "univalent foundations" mentioned in Wolchover's article is that it has the *gauge principle* built right into the foundations of mathematics. Moreover, a good chunk of Lagrangian, local (pre-)quantum field theory has a natural and fairly immediate axiomatization in univalent foundations: http://ncatlab.org/schreiber/files/QFTinCohesiveHoTT.pdf .

7. **Sakura-chan**
March 4, 2013

Peter, you can call him Ed.

8. **anonymous**
March 4, 2013

"Conversely, are there any proofs that were found to be wrong long after they had become 'common knowledge'?"

One item of common knowledge that comes to mind would be the status of the classification of finite simple groups. My copy of Dummit & Foote from 2004 states, "The classification of finite simple groups…. was completed in 1980…" In fact it was completed in 2004. See for example Wikipedia for discussion.

9. **Jeffrey M**
March 4, 2013

I'm not sure I would count the classification of finite simple groups as a "proof" exactly. More of a framework I would say, which of course involves many proofs. In any case it's not like it was wrong in 1980 exactly, it just wasn't quite finished. Off the top of my head and assuming "long" means long, I can't think of anything really. There are plenty of published proofs which are later discovered to be wrong, but usually it's pretty quick. In my field there was a "result" of McKean showing that eigenvalues of the Laplacian on Riemann surfaces are >= 1/4, but Randol came up with counterexamples 2 years later. There are plenty of other things like that around.

10. **Sesh**
March 5, 2013

It looks like the big announcements and data releases from Planck might end up clashing with the announcement of the new Pope, which will probably divert public attention. I wonder if positive media coverage is important enough for them to reschedule?

11. **malfatti**
March 5, 2013

Read this about how the erroneous solution of the Malfatti problem survived for more than a century.
http://www.cut-the-knot.org/Curriculum/Geometry/Malfatti.shtml

12. **Peter Shor**
March 5, 2013

Ironically, the paper "The Four-Colour Theorem Solved, Again" linked to above does not reference the paper where the four-colour theorem **was** solved again.

Higgs Update

March 6, 2013
Categories: Experimental HEP News

Updated results about the Higgs are being reported at Moriond today, slides available here. The organizers have given the talks titles with "BEH Boson" replacing the usual "Higgs Boson" (to promote Englert's shot at a Nobel), but the speakers have mostly ignored this, titling their slides with the usual "Higgs" or maybe "Standard Model Scalar". Some more details are starting to appear at the CMS site here, presumably ATLAS will soon update their site here.

The only surprise so far is that the CMS results for the gamma-gamma channel are not ready yet. Philip Gibbs has very good coverage of the latest news here, including this about CMS:

> Rumour puts the CMS diphoton excess at 1.0 +- 0.2, to be shown at Moriond QCD next week perhaps.

As mentioned here a couple weeks ago, the size of the ATLAS excess in that channel has gone down since last year, now at 1.65 +/- 0.24(stat) +/- 0.21(syst) (where 1.0 is the SM prediction). If you believe Philip's rumor, the combined ATLAS + CMS result for the gamma-gamma channel would be 1.32, consistent with the SM prediction at the level of 1-2 sigma.

In the ZZ channel, CMS reports a cross-section relative to SM of .91 +/- 0.27, ATLAS 1.7 +/- 0.5. Combining them gives 1.30, again quite consistent with the SM. For the WW channel, CMS has .76 +/- 0.21, ATLAS 1.5 +/- .6, averaging out to 1.13, again very much consistent with the SM. For channels with bottom quarks, CMS has 1.3 +/- .6 and for channels with taus CMS has 1.1 +/-.4, ATLAS says the expected signal is still to small in these channels for them to say much.

Some more talks this afternoon may give a bit more detail.

All in all, the story is that this is looking very much like a garden variety SM Higgs, which is discouraging for hopes of hints about how to get beyond the Standard Model. The experiments will continue working on improving their analyses of this data, but it seems unlikely that the picture will change much. There's going to be a long drought now until we see significantly better data for these numbers. Probably not until 2016 until the LHC has been operating long enough to produce significant luminosity at higher energy.

The New York Times yesterday put out a wonderful special issue of its Science Times section, devoted to an excellent long article by Dennis Overbye telling the story of the Higgs discovery from the point of view of the ATLAS and CMS scientists (and emphasizing their rivalry). Highly recommended reading. The article does credit a certain blog with being the venue where a mistaken early Higgs claim was leaked (I'm sorry to hear that that ruined some people's vacations), although the fact the the actual Higgs discovery news broke somewhere else than in the Times doesn't get mentioned…

Professor Matt Strassler has a posting about the Times article, explaining how it shows that other particle physics bloggers were wrong to think that the 3 sigma signals reported by ATLAS and CMS back in late 2011 were strong evidence that the Higgs had been found, and that he had been right to be skeptical.

Update: New ATLAS results are here. Do not miss the extremely cool animated gifs of the evolution of the Higgs signal as data accumulated.

Update: Valuable commentary at Resonaances.

Comments

1. **john McAllison**
   March 6, 2013

   "There's going to be a long drought now until we see significantly better data for these numbers"

   Tut tut, Peter, you're so ungrateful that it's 2013 with probably the greatest discovery of the past few decades in the bag. I'm sure the 25\fb of data will keep people busy for the next two years. If anything, they must be ecstatic they have this amount of data compared to 2011 when they were hoping for 1\fb over the year.

2. **Peter Woit**
   March 6, 2013

   John McAllison,

   The drought comment was just about these very specific Higgs branching ratio numbers, which I think it is going to be hard to dramatically improve on without a lot more data. There are many, many other things to look for in the LHC data which I'm sure will keep people quite busy, and hopefully will turn up something unexpected.

3. **King Ray**
   March 6, 2013

   Peter,

   Thanks very much for the link to the NYT Higgs piece. I liked the analogy of the Higgs field to snow, with massless particles flying above it like birds (see the Higgs drawings by Nigel Holmes).

   I thought the ending of the article was pretty neat; Prof. Higgs has a good sense of humor:

   When she showed the Atlas 5-sigma result, the audience exploded again. The applause seemed to go on forever. It had been left to Dr. Heuer to declare officially that a new particle had been discovered.

   "I think we have it," he said. The cheers began again. Dr. Higgs was seen wiping away tears.

   The morning dissolved into pandemonium and Champagne, in the CERN auditorium and in labs, classrooms, conference rooms and living rooms in every time zone in which humans wondered about their universe. Dr. Wu waded through the crowd. She hugged Dr. Higgs.

   "I've been looking for you my whole life," she said.

   "Well," he replied, "now you have found me."

4. **Nick M.**

March 6, 2013

Hi ***Peter***,

The link that you have given for the “slides available here” section of your post — that is, the first link — seems to have a ‘security certificate error’ as reported by the three different browsers that I used to try to go there; i.e., IE, Firefox, and Chrome. I decided to throw caution to the wind and go all the way there anyway, and indeed they are the slide portion of the presentations at Moriond in PDF format. I think that they may be the slides that accompany these WebCasts as given at Moriond today on the Higgs.

By the way, Thanks for compiling this Higgs update,
— Nick

5. **Nick M.**
   March 6, 2013

   Peter,

   Sorry for all the worry about ‘certificate error’, as the certificate issuer for this site, the French based CNRS2-Standard, seems to not be well recognized by the current crop of U.S. based browsers. Anyway, there doesn’t seem to be any real security rick involved with this site, despite all the browser warning hullabaloo.

   — Nick

6. **Shantanu**
   March 6, 2013

   Peter and others,
   pre-snowmass meeting going on at slac.
   You can listen to the audio of the cosmic frontiers part of it.

7. **Peter Woit**
   March 7, 2013

   Shantanu is referring to this ongoing workshop which looks like it includes quite a few interesting talks:
   https://indico.fnal.gov/conferenceDisplay.py?ovw=True&confId=6199

8. **Nick M.**
   March 7, 2013

   Peter,

   New Scientist just released the article “Rumour points to a completely boring Higgs boson” where they quote ‘yours truly’ just after the section heading “Garden variety”.

   — Nick

9. **Peter Woit**
   March 7, 2013

   Nick M.,

   Thanks. Good to see that someone else is being quoted as the source of rumors this time, not me…

10. **chiz**
    March 7, 2013

    Meanwhile AP are reporting that if the particle isn’t a Higgs then there is only one other thing it could be – a graviton. Cool. Gravity is now a short range force. That will be a boon to the space-launch people. I better go and nail everything down.

American Journal of Modern Physics

March 7, 2013
Categories: Uncategorized

This morning an e-mail came in from the "Science Publishing Group", a call for "Editorial Board Members, Reviewers and Paper" for their open access journals, advertised as

> **Full peer review**: All manuscripts submitted to our journals undergo double blind peer review.
> **Fast publication**: Fast peer review process of papers within approximately one month of submission.

This included a special deal on the "Article Processing Charge": \$70 or \$120 before May 15. I've been highly suspicious of all "author pays" open access schemes in math or physics, so I decided to check into what this one was. When I went to their web-site and looked at their list of journals, the first on the list that looked like it would have material in it I would know something about was the American Journal of Modern Physics. The first paper that showed up on the journal web-page was MSSM Neutral Higgs Production Cross Section Via Gluon Fusion and Bottom Quark Fusion at NNLO in QCD by Tetiana Obikhod, so I took a quick look at it.

It looked perfectly competent, but oddly it wasn't on the arXiv, and the only papers by that author on the arXiv appeared to be some papers on F-theory and D-branes from 1997-98. A little bit of investigation quickly showed that much of the paper was plagiarized from elsewhere, including at least a 2003 paper by Harlander and Kilgore, Higgs boson production in bottom quark fusion at next-to-next-to-leading order and a 2011 paper by Bagnaschi et al. Higgs production via gluon fusion in the POWHEG approach in the SM and in the MSSM (neither of which are listed in the references).

For instance, the AJMP paper introduction has

> In the Standard Model the gluon fusion process is the dominant Higgs production mechanism at the LHC. The total cross section receives very large next-to-leading order (NLO) QCD corrections, which were first computed in . Later calculations retained the exact dependence on the masses of the top and bottom quarks running in the loops. The next-to-next-to-leading order (NNLO) QCD corrections are also large, and have been computed in . The role of electroweak (EW) corrections has been discussed in . The impact of mixed QCD-EW corrections has been discussed in . The residual uncertainty on the total cross section depends on the uncomputed higher-order QCD effects and on the uncertainties that affect the parton distribution functions (PDF) of the proton .

while Bagnaschi et al. has

> In the Standard Model (SM) the gluon fusion process is the dominant Higgs production mechanism both at the Tevatron and at the LHC. The total cross section receives very large next-to-leading order (NLO) QCD corrections, which were first computed in ref. in the so-called heavy-quark effective theory (HQET), i.e. including only the top-quark contributions in the limit mt → ∞. Later calculations retained the exact dependence on the masses of the top and bottom quarks running in the loops. The next-to-next-to-leading order (NNLO) QCD corrections are also large, and have been computed in the HQET in ref. . The finite-top-mass effects at NNLO QCD have been studied in ref. and found to be small. The resummation to all orders of soft gluon radiation has been studied in refs. . Leading third-order (NNNLO) QCD terms have been discussed in ref. . The role of electroweak (EW) corrections has been discussed in refs. . The impact of mixed QCD-EW corrections has been discussed in ref. . The residual uncertainty on the total cross section depends mainly on the uncomputed higher-order QCD effects and on the uncertainties that affect the parton distribution functions (PDF) of the proton .

In the body of the AJMP paper, for example starting at the bottom of page 3 with

> The subprocesses to be evaluated at the partonic level are given as following…

the following material in the paper including the equations is an edited version of Harlander and Kilgore, starting at their page 4 with

> The subprocesses to be evaluated at the partonic level are given as following…

As far as I can tell without spending more time on it, the author did run some kind of package to calculate something (the plots in the paper aren't in the older papers), and then wrote the surrounding paper largely by plagiarizing the other two papers. There's a good reason this one isn't on the arXiv: they now run an automated system which would have immediately identified the plagiarism problem.

It's possible that I just got unlucky, that there was a problem only with the first of the papers I looked at, but this seems unlikely. I realize that this is a very obvious case of a journal with extremely low standards, run to make money off of the increasingly popular "author pays" model of financing journals, but I'm hoping that those that are trying to move high-quality journals to this model are seriously thinking through the issues involved. Just this month in the AMS Notices, there is discussion of a proposal to move two of the AMS journals in that direction. Yes, this is very different than AJMP, but there's an argument to be made about the "author pays" model that it is best avoided, since it's a good idea to keep academic and vanity publishing strictly separate endeavors.

Comments

1. **jinb**
   March 7, 2013

   The sad thing is the plagiarist will still probably get a job from people who aren't in a position to evaluate the work and just see the all-important "published in …" stamp.

2. **Marty**
   March 7, 2013

   You wrote "It's possible that I just got unlucky, that there was a problem only with the first of the papers I looked at, but this seems unlikely."

   I looked at the *second* of the papers, "Raleigh waves…" by Rajneesh Kakar and Shikha Kakar. This is also cut and paste from other sources (without citation) as far as I can tell. Parts are also duplicated in the article by the same authors in "Journal of Chemical, Biological and Physical Sciences"

   Among many other instances (found by google searches), compare "We consider Oxyz Cartesian co-ordinate system with o being any point on the free surface, here we consider the free surface and interface of granular layer resting on nonhomogeneous granular half space bounded by two planes of different material given by z = 0 and z = H respectively. Also it is assumed that oz being normal to half space and Rayleigh wave propagation in the positive direction of xaxis. Here it is also assumed that at a great distance from centre of disturbance, the wave propagation is two dimensional and is polarized in xz-plane." in AJMP to:

   "We consider oxyz cartesian co-ordinate system with o being any point on the free surface, here we consider the free surface and interface of granular layer resting on non-homogeneous granular half space bounded by two planes of different material given by z = 0 and z = H respectively. Also it is assumed that oz being normal to half space and Rayleigh wave propagation in the positive direction of x-axis." in Sethi-Gupta-Gupta, "Propogation of Surface Waves in non-Homogeneous, Magneto Granular

Medium under the Influence of Gravity & Initial Compression", also compare to "Int. J. of Appl. Math and Mech. 6 (20): 50-65, 2010

3. **Matt Leifer**
   March 7, 2013

   Picking on scam journals published by predatory open access publishers does not discredit the author-pays model. It is a bit like saying that all conferences where you have to pay a registration fee must be bad just because you got some spam emails from scam conferences. The truth is there are crappy journals in the closed access world as well (Chaos, Solitons and Fractals anyone?), but everyone knows what the good journals are and ignores the rest. Why wouldn't we do the same with open access journals? Why don't you look at some of the reputable journals that adopt an author pays model, e.g. the New Journal of Physics or Physical Review X?

4. **srp**
   March 7, 2013

   Matt:

   Good points. The community of readers and citers will sort it all out.

   One could even argue that since the balance of attention scarcity these days falls on inundated readers who have access to the arXiv, it makes little sense for readers to pay for more articles. Maybe readers should pay people who can tell them which articles not to bother reading–"anti-publishing."

   But after the last go-around on this subject it appears that it will take an extremely powerful lance blow to unseat our host from his hobby-horse.

5. **Peter Woit**
   March 7, 2013

   Matt,

   I don't doubt that it's possible to have high-quality author pays journals in fields where that has been part of the culture of the field. Even so though, it seems to me that anyone running such a journal should be worrying about the problem of the financial incentives of the organization being driven by the equation "more accepted papers=more income". As well as the fact that they're in a business now being invaded by hordes of scam artists.

   In fields that I know about (math and HEP physics), there is no culture of high quality author pays journals (at least not that I'm aware of). Anyone starting such a thing has to explain how they're going to deal not only with the misaligned incentive problem, but also the problem of getting people who can publish in excellent journals for free to pay instead.

   srp,

   "Maybe readers should pay people who can tell them which articles not to bother reading–"anti-publishing.""

   Excellent idea, I'm a pro at this. Will contact some VCs with my business plan right away…

6. **chiz**
   March 7, 2013

   Playing Devil's advocate for a moment – Obikhod is Ukrainian. Maybe, possibly, perhaps, she isn't a fluent or confident English speaker, and just took sentences from other papers in lieu of struggling to write them herself.

7. **Peter Woit**
   March 7, 2013

   chiz,

   That sort of thing is fairly common, especially in introductory material, and you could try to excuse the borrowings from Bagnaschi et al. that way. In this paper though, large chunks, including lots of equations, are later in the paper taken from Harlander and Kilgore. The equation plagiarism is hard to excuse as a language problem.

   Funny, I just got another promotional e-mail from "Science Publishing Group" a few minutes ago.

8. **Brian Dolan**
   March 8, 2013

   It seems to me that it is inevitable that this kind of thing will happen with the so-called "open access" system. With the old "reader pays" system the journals had to maintain standards to make money, no-one will pay to read articles in a poor quality journal. But with the open access model, the more papers the journal publishes the more money they make and the quality becomes completely irrelevant. It might be argued that they must still maintain standards or no-one will want to publish in them, but that is manifestly wrong — as jinb says in an earlier comment "the plagiarist will still probably get a job from people who aren't in a position to evaluate the work". There are more people than ever desperate to publish to further their careers and the journals will have no problem getting articles from all an sundry (mostly paid for by public funds …).

9. **Alex**
   March 8, 2013

   I would venture a guess that opening an "author pays" high energy physics journal is the perfect recipe for going broke and getting ridiculed. I don't know a single high energy theorist who would be willing to pay any significant amount of money for being allowed to submit an article to any particular journal. The fact that said journal will be known as "the vanity journal" will make it even worse. We referee for free, which is a way of paying journals, but that is a different thing.

10. **Judith McGovern**
    March 8, 2013

    You might be interested to know that the UK government has announced its intention to move towards a requirement for all papers resulting from research-council funded work to be published in "gold open access" journals – ie those which make all papers freely available immediately, and hence inevitably are author-pays. (Other forms of open access such as putting the text on ArXiv are "green" rather than "gold".) There has even been a significant sum of money made available to finance page charges initially.
    http://www.rcuk.ac.uk/research/Pages/outputs.aspx
    The reasonably prestigious IoP journals (eg J Phys G) are therefore moving to allow this model.

Many of those working in areas where self-archiving has been the norm see little benefit in the new system, and worry about being required to get approval from someone in the University in order to publish….

11. **Rhys Davies**
March 8, 2013

@Judith McGovern:
This was discussed at the STFC "Town Meeting", at the end of the theory meeting at Durham, just before Christmas. STFC dish out the majority of high-energy/fundamental physics funding in the UK, and they are well aware of the strong 'green' open-access culture in the field, arising from the universal use of the arXiv. The impression I got was that they are trying very hard to convince the higher-ups that this is sufficient.

Your link contains a document, last updated just two days ago, which includes the following clauses:

"RCUK recognises a journal as being compliant with this policy if:
…
The journal consents to deposit of the final Accepted Manuscript in any repository, without restriction on non-commercial re-use and within a defined period."

and

"While RCUK recognises that many researchers derive value from sharing early versions of papers (for example, by using the arXiv pre-print archive), RCUK will consider only versions 'as accepted for publication' when assessing compliance with its policy."

So at this stage, it sounds like nothing much should change, as long as authors remember to update their arXiv submission to the version which is accepted for publication.

12. **Peter Woit**
March 8, 2013

Thanks Judith and Rhys,

That's exactly the sort of thing I'm worried about. In HEP/Math, the only "open access" problem is things not being on the arXiv. Misguided attempts to solve the supposed problem that don't recognize may lead to a worse situation and new problems.

13. **Low Math, Meekly Interacting**
March 8, 2013

I hope I'm not annoying anyone by chiming in, but I find the discussion fascinating. For better or for worse, I think open-access is going to be new paradigm relatively soon, so if it has major and insurmountable flaws, it will impact all scientific disciplines. My own attitude is the current paradigm in the life sciences is so flawed anyhow (in both industry and academia) that we may as well at least try something different. But physics and mathematics has the arXiv ingrained in the culture, which appears to render traditional publications almost redundant EXCEPT for the very important fact that if one wants a career, they need peer-reviewed papers on their CV. So, how do you have a career that requires the Gold Standard of peer review when the customary suppliers of that standard are no longer economically viable? If perverse incentives render open-access journals an intrinsically-flawed model, whither the tradition of peer review?

14. **Bernhard**
March 8, 2013

It is really worrying to hear that JP G is embarking on this. I was fist thinking that this whole thing was a bit of exaggeration because I always thought these journals would anyway never be taken seriously due to low impact factor. But if JP G is planning this big mistake, then one wonders what is next. It is annoying to lose the option of a good journal like the JP G, but if they adopt this model I think many people (me included) will just avoid publishing there.

15. **Felipe Zaldivar**
March 8, 2013

Just in case someone has some doubts on the quality of the journals of the "Science Publishing Group", prompted by Peter's post and curiosity I looked at one of its "math" journals, "Pure and Applied Mathematics Journal", which has already two volumes, each one with one issue. In the 2012 volume there are two papers. One, with the title "Galois Groups of Polynomials and the Construction of Finite Fields" caught my attention and I took a look at it. Well, I shouldn't have done this. I know, you know, and I think everybody knows what this so-called open access predators are doing. The paper (for lack of any other name) is a list, with no particular order, of elementary results from the usual undergraduate algebra course. However, the exercises (one may believe it could be an assignment) listed at the end are quality are not even on the level of a bad, very bad, undergraduate student! If you feel curious you may look at the "paper" at the link below:

http://article.sciencepublishinggroup.com/pdf/10.11648.j.pamj.20120101.12.pdf

16. **S**
March 8, 2013

As others have said, you can't generalize author-pays as a whole by a few bad apples. Otherwise I could point to the various spam emails I get every day, and conclude that the entire internet outside of big corporate walled gardens has nothing of value.

You mentioned the AMS plan to try author pays with a couple journals. As I understand, the PAMS is so inundated with submissions that editors have actually been given limits on how many papers they're allowed to accept. This creates perverse incentives in a different direction. Suddenly it becomes extremely difficult to get published in a journal like PAMS if you don't have close connections with an editor there. This exacerbates the problem of mathematical insulationism, where the high-tier journals tend to publish exclusively work in flavor-of-the-year fields and anything out of the ordinary is toxic.

The only way to completely avoid perverse incentives altogether is to use a model like the Electronic Journal of Combinatorics: no author fees and no limits on how many articles can be accepted. (It's 2013, why is the AMS still limiting journals based on how many sheets of paper a physical volume can hold?)

17. **Jon Lennox**
March 9, 2013

In all non-broken cases, "author pays" is really "funder pays", right? The assumption is that everyone doing research in these fields has a research grant.

So it seems to me that you could get the incentives a lot better, without changing the ultimate flow of money, if the funding agencies funded the journals directly. That way, the journals' incentive would be to put out the best journal they could (so their grants get renewed) rather than to publish as many papers as possible.

It would also be a lot fairer to researchers without funding.

18. **tomate**
March 9, 2013

I am a fresh researcher in statistical physics. I will tell an episode about open access – and maybe ask for an advice on publishing.

Some months ago I received an invitation to write an article for a special issue of a journal (new institutional email account just activated). Since I had some publishable material on that very subject, I decided to roll up my sleeves and started writing. Was I naive! I didn't realize that the "invitation" was for a pay-per-publish journal (my fault of course! I didn't read all details of their email, but who would think that you have to pay when invited our for dinner?). I got my work ready, formatted according to the journal's Latex style, and finally, when I submitted it, I was asked to pay.

I got mad. I vigorously protested, and this eventually led to something: It might be that my paper is accepted even without fee. In the meanwhile, I realized that the "special issue" is just a strategy to get money from a closed self-citing community. Whilst my paper is the due quality for a research paper (that I believe), I noticed that these special issues are filled up with old review material. I wonder how refereeing is being conducted there…

So, on the one hand, I don't want to be misguided for a member of such a cummunity, and I don't like people to think that I pay for publishing (even if I don't). On the other hand, this work is ready, and you all know how much a pa**-**-***-*ss is to format it for another journal (admitting that the subject is interesting enough for any otherjournal).

19. **Peter Woit**
March 9, 2013

tomate,

Thanks for writing. That story shows clearly one of the big problems with journals like this: they sometimes get authors to send them good articles, by one means or another. Their whole business model is based upon trying to make themselves hard to distinguish from more reputable journals, by any means they can. People who think these scams are not a major problem because they can just easily be ignored are underestimating the ingenuity and goals of those running these operations.

20. **Peter Woit**
March 9, 2013

Jon,

Thanks for the comment. What you're suggesting I think is kind of what the international funding agencies in HEP are planning to do with SCOAP^3. This does deal to some extent with the incentives problem, especially if the threat of being defunded is real (although now the incentive for the publisher is still to drive down quality and publish as many papers as possible, taking the journal right up to the line where it would get defunded, but not over it). The main argument against it in that case is that instead of getting rid of the expensive journal publishers like Elsevier, it entrenches them permanently, supplanting university budget money they were in danger of losing with more stable government money.

This is an international consortium with possibly stable funding. If it were a US funded operation, you might worry about having your scientific literature shut off when budgets are cut or sequestered.

21. **srp**
March 9, 2013

Online dating scams occasionally snare a Paul Frampton. Author-pays publishing scams occasionally snare a naive researcher or steal a couple of minutes from a reader. Peer review allows all kinds of bad behavior by reviewers sabotaging their rivals and promoting their friends. Occasionally the existence of life insurance motivates a murder (this was actually a major objection to life insurance when it was an innovation.)

Institutions must be judged on the balance of their effects.

22. **Low Math, Meekly Interacting**
March 10, 2013

srp nails it. I'd say the institution of peer review, as it exists today, is in need of a serious overhaul, if for no other reason than it cannot cope with the current level of scientific misconduct (though there are plenty of other reasons). In some disciplines, this problem is obviously a lot more troublesome than in others, but fragmentation of scientific standards between disciplines seems pretty untenable to me. So we have a new paradigm emerging, which is to realign the payment structure so as to make research publications freely accessible to all. This has enormous implications, as it disrupts the business model of the customary "gatekeepers" of research publication, with attendant effects on the "gold standard" of peer review.

I certainly acknowledge the dangers inherent in the author-pays model, but the status quo is being stretched beyond its limits, and the completely free model of a preprint archive kinda looks like the Wild West, even with the system of sponsorship, and even the author of this blog has suffered injustice at the hands of that system.

So is there something better in the works? Science is a human enterprise, and it's entirely subject to human failings. I'm not optimistic we're all going to become Vulcans, so ways of coping with the human condition are needed. What saves the enterprise and helps insure its supremacy is its competitive (and even adversarial) nature and the fact that the competition is settled by observation of the Real World (however you like to define that). It would seem to me, at minimum, that making the fruits of scientists' labors as freely and openly available as possible does the most to foster scrutiny of and challenge to scientific claims, and open access does at least aspire to go some way towards achieving that goal. It likely has problems. We already have problems. Is some weighing of relative problems (both known and conjectured) justified as we attempt to change the publication system?

23. **Jon Lennox**
March 10, 2013

Peter: Is the incentive to publish lots of mediocre papers (keeping your journal just good enough) any different for funder-pays than it is for traditional subscriber-pays? I think the factor that's kept the mediocre paper count down in the past was the space limitations implicit in physical publishing — once you've gone to an electronic format, that's out the window, regardless of how the money's flowing.

Also, if you're in a funder-pays environment, is there really a problem with entrenchment of publishers like Elsevier? If they're doing a enough job to keep their grants going, and the journals are open-access, I don't think there's anything intrinsically wrong with them being big. (I guess you might run into a regulatory capture problem, where they get their grants renewed due to lobbying rather than quality, or the like.)

24. **Brian Dolan**
March 11, 2013

As one of the 13,000 people who have signed Tim Gower's "Cost of Knowledge" petition against Elsevier's practices (http://thecostofknowledge.com), I would be very unhappy to seem then being given money directly out of the tax-payer's pocket.

"Low Math, Meekly Interacting" above gets to the kernel of the problem, that the current system relies on peer reviewed publications to assess grant applications, job and promotion applications, so we cannot do without it. But it doesn't have to cost the Earth.
We (i.e. active researchers) already do all the research, we type and typeset the articles, we act as editors and we do the refereeing. So what do we need the journals for?

Let me throw out the following idea for discussion. At least in physics, with the archive, we can completely eliminate the journals: all we need to do is set up editorial panels for various sub-disciplines ourselves and, when someone submits a paper they indicate whether or not they want it to be refereed for "publication". We go through the usual refereeing process and, if it is accepted, it appears in the archive listings as "published" (maybe put a little asterisk beside, to indicate "published"). This would completely equivalent what the journals do at the moment, and would cost almost nothing. Of course we would have to persuade the funding agencies that this is "Gold Standard".

The only problem I can see with this proposal is that there would then be only one place to send articles and, if it's rejected, there is no second option — no other "journal" to send it to.

25. **Bob Levine**
March 11, 2013

Brian—

I like your idea and have always thought that something like this should be the model. But doesn't its viability depend on persuading departmental P & T committees that this new 'gold standard' is also a new 'blue ribbon' outlet? The cooperation of academe (not just at the departmental level but through all the levels of admin review) would seem essential for the success of this approach, and I don't know what the level of resistance to it would be on the part of senior established scientists in physics, let alone other fields which don't have something like arxiv, with its semi-'established' status. The problem is not just one of publication format; the whole culture of the university meritocracy seems to be part of the issue.

26. **Jeff McGowan**
March 11, 2013

I think Brian Dolan has a very interesting idea. In math anyway there's already a partial refereeing process for arxiv, since anything interesting will get read by many people, and if there's a problem they'll let the author(s) know, and it usually gets pulled. Of course you can't force someone to pull it that I know of, and some things probably don't get read my many, if any, people, so they slip through the cracks. I think journals have historically served 3 purposes. First of course, they make things accessible. That's is not an issue now. Second, the refereeing process kept things legit. That's still an issue, but as Brian points out we're doing that anyway now for free, so why not just do it for arxiv? Third, they sorted things quality wise – if I see a paper in JDG or GAFA or Transactions or whatever, I know it's got to be interesting in some way. This relates to what Brian said about the problem with arxiv and making it a refereed source is that you have only one venue. Maybe the way around this is to have a stable of referees, have everything read by say 3 of them, and have them not only check the validity of the paper but also grade it. That way papers which would be in Annals and papers for Proceedings (or lower) could all be published, as long as they were correct, essentially it would be like having many journals, and you submit to all at once, the referees decide which one to publish in.

27. **Yuichiro Fujiwara**
March 11, 2013

Peter,

You say:

That story shows clearly one of the big problems with journals like this: they sometimes get authors to send them good articles, by one means or another. Their whole business model is based upon trying to make themselves hard to distinguish from more reputable journals, by any means they can.

I would have agreed with you if his story were about how the journal tricked him into sending a good research paper through a dishonest means. But as far as I can tell from his comment, it seems to be due to his own lack of a due diligence. He himself says he didn't carefully read the email from the journal. He doesn't seem to have researched the journal and/or publisher before submission either.

Do you think it is a problem for a new journal to attract good papers through acceptable means in order to become indistinguishable from other reputable journals? If the journal dishonestly hid the fee or put it in tiny fine print, I see a problem. If you were talking about spamming researchers' mail boxes, I would understand to an extent. But if it is just that the author was careless, what he descried may not clearly show a big problem in academic publishing.

I don't think you would say it is a problem that reputable journals want to publish good papers. Why shouldn't struggling journals wish for good papers then? If they become indistinguishable from already established journals because of their effort in attracting good papers, it's just they're already one of them at least in terms of quality.

I understand that a certain open access model may make some journals want to publish papers *regardless of their quality*. I agree that this can be a problem. But I don't think being open access makes it unacceptable for a journal to want to publish good papers. It's only a problem if they want bad papers.

Now, if a journal dishonestly tricks researchers so they get good papers, it's certainly a problem. But it's a terrible thing regardless of publishing model. I don't think open access models particularly encourage such dishonest conduct more strongly than other models. I don't think the comment you're responding to clearly suggests that it was a dishonest trick either. If anything, I tend to think that his tone of writing makes it difficult to guess exactly what happened.

28. **Peter Woit**
March 11, 2013

Jon,

You're right that electronic format does remove a traditional brake on journals lowering their standards. Still though, there is a difference between any model where they get paid per article and one where the amount they are paid is determined by other factors.

Much of the debate about Elsevier in recent years has been of the order of "how can we get rid of them: they're costing our community a lot of money way out of proportion to the value they provide". Before having granting agencies directly funding Elsevier, I think that issue should be addressed: are they worth it?

Yuichiro Fujiwara,

I think you're blaming the victim here. Yes, victims of scams wouldn't be victims if they did a better job of figuring things out before getting scammed.

Obviously reputable journals should want to publish good papers, having scam artists who manage to publish good papers is a problem though. Publishing outfits like this one are not just in the business of getting paid by plagiarists and incompetents to do vanity publishing for them, but are engaged in a serious effort to mislead people and make them think they are something they are not (a legitimate operation with real standards), and are sometimes successful. They're doing this because they can make money off an author-pays model. I'm just pointing out that before the math and HEP communities abandon their traditional practices and move to author-pays, they should keep in mind that this model is already leading to a huge scam-publishing problem.

29. **Yuichiro Fujiwara**
March 11, 2013

Peter,

I don't think I'm blaming "the victim of a scam." I'm saying that we don't know if he is actually a victim or if it was a scam in the first place.

He only told his side of the story anonymously in a not extremely professional tone on the internet without giving any concrete evidence that backs up his claim when he can clearly do so by, for example, giving the link(s) to said special issue and/or post what you seem to believe is a dishonest email to "trick" him into submitting a good paper. I tend to take an angry, anonymous, one-sided story without supporting evidence cum grano salis, especially when the unhappy person didn't give the link to *openly available* evidence.

Of course, the journal in question may in fact be of extremely low quality or even part of a scam you describe, although I can't agree that it is clear from his comment that this was the case. Even then, I am having a hard time convincing myself he is a "victim" who shouldn't be blamed. Perhaps, it is the norm in statistical physics, but I find it very careless to submit your own work you are pride of to a journal you know nothing about. It is not that "victims of scams wouldn't be victims if they did a better job of figuring things out before getting scammed." You are supposed to make sure you know what the journal is like before you submit your own work.

I would have sympathized with him if it were a sophisticated scam such as listing famous researchers on the editorial board without permission and uploading good quality papers from established journals as if they were originally published in the scam journal. But in my opinion, it doesn't count as getting scammed if you publish in a low quality journal because you didn't check what it's like before submission.

Also, I can't agree with your argument that a certain open access model is *leading* to scam-publishing problem. I don't think it is the direct cause. Would you tell your university's administrators that they should keep in mind that the "student-pay" model of university education is leading to a huge scam diploma-mill problem? They scam naive students because they can make money off of them. But I don't know if we should be wary of the student-pay model or if universities should rethink their business model because of the existence of diploma mills.

I agree that we should know there are academic publishing scams out there. And I understand that making it easy to start a new journal also makes it easy to start a publishing scam. But if you were pointing this out, I fail to see how the comment you were replying to "clearly shows" it.

30. **k.c. gupta**
April 1, 2013

MR. Marty
DR. RAJNEESH KAKAR, DR. SHIKHA KAKAR AND DR. MUNISH SETHI ARE IN SAME GROUP SO THERE WILL BE RESEMBLANCE IN THEIR PAPERS. THINK BEFORE YOU WRITE.

The Professor, the Bikini Model and the Suitcase Full of Trouble

March 8, 2013
Categories: Uncategorized

Fresh off its great long article about the Higgs, the New York Times is devoting a similar amount of space to the other big mind-blowing high energy physics story of the past year or two, Paul Frampton and his adventures in South America. This coming weekend's New York Times Magazine has a feature article on The Professor, the Bikini Model and the Suitcase Full of Trouble, which covers the whole amazing story well.

Comments

1. **Bee**
   March 8, 2013

   Well, after that article he'll probably be drowned in marriage proposals…

2. **Mark**
   March 8, 2013

   Peter is Paul Frampton respected in the physics community? I couldn't tell if he was all hype and ego or contributed in a meaningful way to HEP.

3. **Phil**
   March 8, 2013

   Wow, what a story. i feel sorry for Frampton and hope others will read his story and learn from it!

4. **Marc Sher**
   March 8, 2013

   Mark—

   Paul Frampton has made major contributions to HEP. A paper I wrote with him in 2001 just hit 250 citations — he has 271 published papers with 7000 citations. So he has always been considered a solid physicist (and has a remarkable high number of PRLs).

   However, the community was surprised by his talk a couple of years ago comparing himself to Newton — it raised some eyebrows and had some concerned about his health (he is 68). And his last few papers have been a bit odd.

   I don't know what happened in South America. I suspect he knew more than he's letting on (but not as much as the authorities think). When he returns a year from this May, we should all give him the benefit of the doubt and hope he can get back into physics.

5. **Z**
   March 8, 2013

   I'm surprised no one has mentioned an autism-spectrum disorder like Aspergers. But I don't know anything about Frampton besides what I've read — however, what seems to really define him is his blinding ego.

   HEP theory in the last 30 years seems to be centered around egos, their pet theories and respect in the community. It's been 30 years since the W/Z discoveries. If you define "meaningful contribution" in HEP theory as something beyond the standard model and GR that is experimentally confirmed, almost no one has made any such contribution. I'm no sociologist, but if the community's respect is the only criterion for being a good theorist, then this environment seems ripe for creating potentially detrimental egos.

6. **john McAllison**
   March 8, 2013

   @Mark, yes I was thinking exactly the same about Paul Frampton – no denying his stellar academic record, but what about his creative contributions to the physics community?

   This is where Microsoft Academic comes in handy:
   http://academic.research.microsoft.com/Author/12899234/paul-h-frampton
   385 publication, 286 citations.

   Now let's compare this to Ed Witten:
   http://academic.research.microsoft.com/Author/2582335/edward-witten
   398 publications, 41,013 citations (yes, it really does say 41,013)

   Is it fair to say that roughly, Paul Frampton's contributions to physics are possibly 1/20th those of Ed Witten's?

7. **Marc Sher**
   March 8, 2013

   John McAllison—Microsoft Academic is garbage. Frampton has 6900+ citations. Look at http://inspirehep.net/search?ln=en&p=f+a+frampton%2C+p&of=hcs&action_search=Search for a listing of all 6900, if you wish.

8. **Curlo**
   March 8, 2013

   The ratio of the citations that McAllison quotes from Microsoft Academic is closer to 150 than to 20. However, the ratio of the Spires citations that Marc defends is roughly 14 (100,000 of Witten compared to 7,000 of Frampton), so close to 20, indeed ! The number of published work is almost the same (268 published works compared to 252).

9. **john McAllison**

March 8, 2013

Mark Sher, OK:

http://inspirehep.net/search?ln=en&ln=en&p=witten%2C+edward&of=hcs&sf=&so=d&rm=&rg=25&sc=0

Edward Witten has around 100,000 citations, H-index 143, compared to Paul's H-index of 43. For further comparison, Stephen Weinberg has an H-index of 94.

So based upon this, I think it's fair to say that Paul Frampton is/was a world class physicist, but not necessarily in the division of current/past top world class physicists.

10. **bw**
March 8, 2013

The tone and title of this article unfortunately plays up the stereotype of physicists as bright, but socially inept. That this is written for the general public for this Sunday Times Magazine section makes it worse. 408 on-line comments already posted to the NY Times as of this Friday night (9:17 p.m. EST) before the article even appears in print on Sunday means this will be a highly popular article. And many of the comments snicker at highly intelligent people who can't function in the real world.

11. **Leopold Bloom**
March 8, 2013

It's deeply shameful that so many in the physics community have rallied around this criminal. Seems that it's ok to commit a serious crime if you are a physicist.
And for those who really believe that he didn't know about the cocaine: I'm not even going to try to sell you a bridge; I reserve that for the merely gullible, not for people with mental disabilities.

12. **CPV**
March 9, 2013

"The tone and title of this article unfortunately plays up the stereotype of physicists as bright, but socially inept."

Hmm…have you actually met any physicists…especially good theoretical physicists?

13. **SC**
March 9, 2013

but you've got to look at how much of an advance each paper was, too, so 1/20th might be an overestimate

14. **Shantanu**
March 9, 2013

Btw this maybe controversial pov, but don't think large citations necessary means
a good paper. For example, Randall-Sundrum model has lots of citations even though there is a single shred of evidence that it is right.

15. **imho**
March 9, 2013

I fully believe that we all should support our fellow Physics brothers and sisters… BUT Frampton was very likely trying to smuggle cocaine into Europe, how many young kids were going to die over his activities? Granted he was lonely and he probably did it for some imagined life with a bikini model, but we as a community need to have higher standards.

Were this some urban teenager from the barrio bring drugs into one of our communities we would feel differently. I say leave him in prison… Actions have consequences.

16. **Marcus**
March 9, 2013

Leopold Bloom:
For in the same way you judge others, you will be judged, and with the measure you use, it will be measured to you. (Matthew 7:2)

17. **Thomas**
March 9, 2013

Marcus:

Do you mean to apply the same dictum to what we say about every (ex-)inmate of Devoto prison who seems guilty?

18. **Jonathan**
March 9, 2013

It is quite clear from the article that he's guilty. His sentence (a year or two of house arrest after a short stint in prison) is pretty light given the crime. It is kind of sad that a serious physicist would get caught up in this, but do keep in mind the effect of addiction and that he was trying to contribute to this problem solely out of self-interest. I think people should stop rallying around him, but at the same time I think once he gets out of jail he should be treated as before, since he did do his time.

I wonder if UNC will fire him due to his having committed this kind of crime. I guess they'll have to make a decision on this at some point.

19. **Roger**
March 9, 2013

The article would have been improved with some pictures of the bikini model.
http://www.dailymail.co.uk/news/article-2183861/Denise-Milani-Bikini-model-terrified-drugs-gang-used-honeytrap-science-professor.html

20. **gs**
March 9, 2013

Not to incite an off-topic discussion of the Drug War, but to put my reaction into context:

Having only known the rudiments of this situation and being a strong opponent of the "War on Drugs", I had been giving Frampton the benefit of the doubt. However, if the article is correct, I'd find him guilty.

21. **Peter Woit**
March 9, 2013

Please, the "War on Drugs" is definitely off-topic, thanks. If you want to discuss that, a posting about Frampton on Slashdot has just appeared, see

http://yro.slashdot.org/story/13/03/09/186229/the-manti-teo-of-physics

22. **gs**
March 9, 2013

Apparently Frampton knew what he was doing. Apparently he was doing it for money and other personal gain, not as an act of idealism or civil disobedience.

Apparently this is not an Aaron Swartz scenario.

23. **Paul Frampton**
March 9, 2013

If I may chime in as the subject of this discussion, I wish to state categorically that I am not, and have never been, an intentional smuggler of illegal substances. When I checked in a bag belonging to somebody else, which admittedly showed bad judgement, at Ezeiza airport in Buenos Aires, Argentina, on January 23, 2012 I had no knowledge or suspicion whatsoever that the bag contained any illegal material hidden within it.

24. **Peter Woit**
March 9, 2013

Thanks Paul.

(Yes, that comment was posted from Buenos Aires).

This may be a good time to close off this thread, since the discussion has kind of degenerated, with Slashdot a better place for this. I'll take the liberty of ending with my own comment that, while reading the Times magazine story made it hard not to come up with one's own conjectures about what really happened, I also realized that they were probably not worth much. Any news story like that one presents a very small selection of edited information, giving one the misimpression that one knows more than one does about what happened.

Things to Follow This Week

March 12, 2013
Categories: Experimental HEP News

If you want to keep up on the latest in HEP news, here's what you should be following this week:

- Neutrino Telescopes is happening in Venice this year, and I noticed that there's a very active blog for the conference, with a wealth of detailed postings about the talks. At first I was very impressed that such a large group of well-informed and energetic bloggers had been organized to cover this, then realized that it's actually just the indefatigable Tommaso Dorigo at work. He's doing a great job covering what is going on at the conference, and if as is looking all too possible, the LHC finds no new physics besides the Higgs, neutrino experiments may be where attention focuses in the future as the best hope for this.
- In Aspen this week there's a conference called Higgs Quo Vadis, on the current state of knowledge about the Higgs. Look for talks on Friday by Lisa Randall, Nima Arkani-Hamed and Nati Seiberg about what it all means.
- A second workshop at Moriond is going on this week. Will CMS finally release its gamma-gamma results there?
- If not at Moriond or Aspen, maybe at that LHCC meeting tomorrow?
- To keep up with the state of the LHC machine itself and plans for the future, the LHC Machine Advisory Committee is meeting later this week.

Update: One more. The HEPAP committee met this week, slides here.

Comments

1. **n**
   March 12, 2013

   the link for "Higgs Quo vadis" does not work…

2. **Peter Woit**
   March 12, 2013

   Thanks n, fixed.

3. **Svennor**
   March 13, 2013

   Hi Peter,

   There's also a nice paper on N=4 SYM scattering amplitudes @ finite couplings (all multiplicity/all loop) which seems like a big deal . I guess that's worth mentioning 🙂

   http://arxiv.org/pdf/1303.1396.pdf

   Best

4. **Len**
   March 14, 2013

   The other thing to watch this week was Kibble's 80 birthday celebration.

   http://plato.tp.ph.ic.ac.uk/conferences/Kibble80/

   Event was attended by Weinberg, Lars Brink (head of Nobel Committee), Guralnik, Close, and others.

   Pretty blatant Nobel campaigning by Imperial College to get Kibble on the ticket and using his somewhat minor 1967 paper as justification to be include over rest of his GHK team.

   Told the head table for dinner included Weinberg, Lars Brink, and Kibble with Frank Close giving after dinner talk to promote this 1967 paper. Weinberg also made some interesting comments in his talk about Israel.

   Wasn't sure the Nobel Committee went to campaigns like this but I guess that is the world we now live in. IC pushing hard to ensure the Americans (and CERN) gets overlooked in October.

5. **Peter Woit**
   March 14, 2013

   Len,

   Thanks. Interesting that Weinberg is attending an Imperial College event. Back in 2007 he publicly withdrew from one honoring Salam to protest what he saw as British anti-Semitism.

6. **Len**
   March 14, 2013

   Peter,

   I did not remember that 2007 event well but here is an article in the Guardian on that issue. Of course made a bit more interesting given Weinberg is an atheist.

   http://www.guardian.co.uk/education/2007/may/24/highereducation.uk1

   Lars Brink was also at a private dinner with Kibble and Weinberg Tuesday evening. No word on what type of dish was served to Lars.

   I have heard of physicists being invited to Stockholm for lectures to indicate they are likely candidates but not where the Nobel chair visits the candidate's campaign. I think Salam gave a lecture in Stockholm before his ’79 Nobel and Higgs was there in October of 2010.

More about Nothing

March 13, 2013
Categories: Multiverse Mania

It seems that last year's philosopher-physicist fight over nothingness (if you missed this, you can read about it starting here) is flaring up again. Recall that it all started with a David Albert New York Times review of Lawrence Krauss's latest book as "pale, small, silly, nerdy", moved on from there to Krauss characterizing Albert as "moronic", after which many others joined in. The New York Times today is reporting that Albert has been disinvited from participating in a debate over nothingness at the American Museum of Natural History here in New York, possibly because of Krauss's attitude that "If it were up to me, I wouldn't choose to spend time on stage with him".

The event in question is this year's Isaac Asimov Memorial Debate, on the topic of The Existence of Nothing. Tickets to the main theater and simulcasts in other rooms are sold out, but you can watch the debate online live here. It will feature Krauss, J. Richard Gott, Eva Silverstein and Charles Seife, with Jim Holt replacing David Albert.

Earlier this week the Simons Center at Stony Brook hosted another big public event promoting the latest deep-thinking from theoretical physicists. On Monday Andrei Linde gave a talk on "Universe or Multiverse?". Besides the usual pseudo-science, there were some things I hadn't seen before. Linde argues that one should replace the "pessimist's":

> If each part of the multiverse is so large, we will never see its other parts, so it is impossible to prove that we live in the multiverse.

with the "optimist's":

> If each part of the multiverse is so large, we will never see its other parts, so it is impossible to disprove that we live in the multiverse.

and goes on to argue that multiverse theory is more basic than universe theory because it is more general. At a more technical talk the next day he showed an implementation of this new way to do science, arguing for a new class of supergravity inflation models where "we can have any desirable values of n_s and r". Somehow also, the ability to get any r you want is great since "A discovery or non-discovery of tensor modes would be a crucial test for string theory and SUSY phenomenology". I'm not sure how you reconcile measuring r as a "crucial test", and having a theory that gives any value of r you want, but maybe I'm missing something.

Linde ends with another innovation. You see, the multiverse doesn't just explain why physics is the way it is, it also explains why mathematics is the way it is:

> Physicists can live only in those parts of the multiverse where mathematics is efficient and the universe is comprehensible.

I guess I should just be thankful that I don't live in one of those parts of the universe where mathematics is inefficient.

Update: More about the Albert disinvite story here.

Comments

1. **ZZZ**
   March 13, 2013

   I can't believe people are being paid for such frivolous pursuits when we still have not settled how many angles can dance on a pin.

2. **Avattoir**
   March 13, 2013

   'The New York Times today is reporting that Albert has been disinvited from participating in a debate over nothingness at the American Museum of Natural History here in New York, possibly because of Krauss's attitude that "If it were up to me, I wouldn't choose to spend time on stage with him".'

   Is it? The way I read the NYT piece was Albert was disinvited (uninvited) because the organizer, Neil de Grasse Tyson, became concerned that the nature of the dispute between Albert and Krausse reasonably threatened to eat up the 'science' orientation he, Tyson, wants for the talks.

   The NYT piece does voice the first concern, but as voiced by Albert, not the reporter.

   Off this, I'm sympathetic with Tyson: http://bloggingheads.tv/videos/1728

   Where Albert's contribution would have been better placed than Krauss' (to say nothing of Richard Dawkins): http://preposterousuniverse.com/naturalism2012/

3. **Peter Woit**
   March 13, 2013

   Avattoir,

   I have no inside information about what really happened here. I'll comment though that if you're the organizer of a thing like this, it's extremely unusual to disinvite one of your panelists because you've decided that maybe they weren't such a great idea. One of very few things that can cause organizers to have to take this kind of step is when they have one participant in essence saying "it's him or me".

   Note that this event is a debate, every year they try and get a panel with some conflict (in the past the string wars have been a topic). This one was planned after the Krauss/Albert thing happened, seemingly specifically to exploit this philosopher/physicist conflict.

4. **Avattoir**
   March 13, 2013

   I respect that you're familiar with and closer to whatever's going on, but it's still not what the NYT is reporting.

   Accepting that historically this event has been debate-oriented, IF IT WERE ME in Tyson's position, having watched that discussion between Albert and Carroll (and the other; there's two on bloggingheadstv), and having read Krauss' book, I'd be concerned, first, that Krauss seems way out of his weight class, and second, with the reception such a 'debate' would get from the sort of audience likely to attend.

   Mine is more of an Ocham's Razor take: that Tyson determined he's more interested in discussion around any controversies in Krauss' construct on the science than he is with producing something like the, um, far more nuanced consensus reached in the Naturalism conference.

5. **Peter Woit**
March 13, 2013

Avattoir,

The NYT is reporting that Albert believes he was disinvited because of Krauss's insistence, and is quoting Krauss to the effect that he does not want to get on stage with Albert. I don't think Albert is being paranoid…

6. **Christian Takacs**
March 13, 2013

"Physicists can live only in those parts of the multiverse where mathematics is efficient and the universe is comprehensible."

I wasn't aware that logic, causation, and arithmatic were strictly localized phenomena. Silly me. Occams razor trumps the simplistic truisms of SAP or WAP any day of the week, or "If things were different, they would be different" valid statement, but completely useless. It is far more likely that Professor Linde's argument is incomprehensible, rather than the rest of the cosmos.

7. **friend**
March 13, 2013

Logic, proof, and argumentation can only be applied to propositions. If there exists nothing to describe with a proposition, then you cannot apply logic and reason to it. You can't prove there once was nothing that gave rise to something.

8. **Chris Oakley**
March 14, 2013

Wait – so people are paying for tickets to see a debate about nothing? Why not just subscribe to the BBC Parliament sattelite channel?

9. **Mr MonoPole**
March 14, 2013

"Physicists can live only in those parts of the multiverse where mathematics is efficient and the universe is comprehensible."

It likely means, that there are (likely) some Universes where 2+2=5

We do not live in such a Universe, so our Mathematics is efficient and comprehensible.

10. **former mathematician**
March 14, 2013

re localized mathematics.

Recall Greg Egan's short story "Dark Integers." It hypothesizes the radically anti-Platonic view that arithmetic theorems are untrue until and if they are exhibited by configurations of particles.

11. **Guillaume**
March 14, 2013

> "Physicists can live only in those parts of the multiverse where mathematics is efficient and the universe is comprehensible." (Andrei Linde, 2013)

I prefer the original:

> "I'm what you would call a teleological, existential atheist. I believe that there's an intelligence to the universe, with the exception of certain parts of New Jersey." (Woody Allen, in Sleeper [1973])

12. **Gary**
March 14, 2013

Responding to ZZZ: I'd like to announce that the question of how many angels can dance on the head of a pin has been answered. It's: 3.1415926535 8979323846 2643383279 5028841971 6939937510 5820974944 5923078164 0628620899 8628034825 3421170679 8214808651 3282306647 0938446095 5058223172 5359408128 4811174502 8410270193 8521105559 6446229489 5493038196 4428810975 6659334461 2847564823 3786783165 2712019091 4564856692 3460348610 4543266482 1339360726 0249141273 7245870066 0631558817 4881520920 9628292540 9171536436 7892590360 0113305305 4882046652 1384146951 9415116094 3305727036 5759591953 0921861173 8193261179 3105118548 0744623799 6274956735 1885752724 8912279381 8301194912 9833673362 4406566430 8602139494 6395224737 1907021798 6094370277 0539217176 2931767523 8467481846 7669405132 0005681271 4526356082 7785771342 7577896091 7363717872 1468440901 2249534301 4654958537 1050792279 6892589235 4201995611 2129021960 8640344181 5981362977 4771309960 5187072113 4999999837 2978049951 0597317328 1609631859 5024459455 3469083026 4252230825 3344685035 2619311881 7101000313 7838752886 5875332083 8142061717 7669147303 5982534904 2875546873 1159562863 8823537875 9375195778 1857780532 1712268066 1300192787 6611195909 2164201989 3809525720 1065485863 2788659361 5338182796 8230301952 0353018529 6899577362 2599413891 2497217752 8347913151 5574857242 4541506959 5082953311 6861727855 8890750983 8175463746 4939319255 0604009277 0167113900 9848824012 8583616035 6370766010 4710181942 9555961989 4676783744 9448255379 7747268471 0404753464 6208046684 2590694912 9331367702 8989152104 7521620569 6602405803 8150193511 2533824300 3558764024 7496473263 9141992726 0426992279 6782354781 6360093417 2164121992 4586315030 2861829745 5570674983 8505494588 5869269956 9092721079 7509302955 3211653449 8720275596 0236480665 4991198818 3479775356 6369807426 5425278625 5181841757 4672890977 7727938000 8164706001 6145249192 1732172147 7235014144 1973568548 1613611573 5255213347 5741849468 4385233239 0739414333 4547762416 8625189835 6948556209 9219222184 2725502542 5688767179 0494601653 4668049886 2723279178 6085784383 8279679766 8145410095 3883786360 9506800642 2512520511 7392984896 0841284886 2694560424 1965285022 2106611863 0674427862 2039194945 0471237137 8696095636 4371917287 4677646575 7396241389 0865832645 9958133904 7802759009 9465764078 9512694683 9835259570 9825822620 5224894077 2671947826 8482601476 9909026401 3639443745 5305068203 4962524517 4939965143 1429809190 6592509372 2169646151 5709858387 4105978859 5977297549 8930161753 9284681382 6868386894 2774155991 8559252459 5395943104 9972524680 8459872736 4469584865 3836736222 6260991246 0805124388 4390451244 1365497627 8079771569 1435997700 1296160894 4169486855 5848406353 4220722258 2848864815 8456028506 0168427394 5226746767 8895252138 5225499546 6672782398 6456596116 3548862305 7745649803 5593634568 1743241125 1507606947 9451096596 0940252288 7971089314 5669136867 2287489405 6010150330 8617928680 9208747609 1782493858 9009714909 6759852613 6554978189 3129784821 6829989487 2265880485 7564014270 4775551323 7964145152 3746234364 5428584447 9526586782 1051141354 7357395231 1342716610 2135969536 2314429524 8493718711 0145765403 5902799344 0374200731 0578539062 1983874478 0847848968 3321445713 8687519435 0643021845 3191048481 0053706146 8067491927 8191197939 9520614196 6342875444 0643745123 7181921799 9839101591 9561814675 1426912397 4894090718 6494231961 5679452080 9514655022 5231603881 …. (Happy PI day to all 8)

13. **Tom**
March 14, 2013

Regarding the physics-philosophy kerfuffle:

I think the biggest cultural difference between the two sides is the philosopher's insistence in rigor in argument. To be sure, not all philosophers hold themselves to this standard (nor, perhaps, even most within certain subfields, like Continental Philosophy), but it remains the methodological touchstone of our profession.

That is simply not the case in physics. A lot of very shoddy thinking goes on with you guys. Your unfounded devotion to string theory is an excellent example.

I was not a big fan of the tone of Dr. Albert's initial review, but I think the content was right on. The fact that physicists take it almost as a matter of faith that there is no god really irritates us. Now, I'm an atheist, and so are most of my colleagues. Maybe 20% of philosophers believe in a god. But practically none of us think that the existence of god is a TRIVIAL issue or one that can be solved simply by obtaining more facts or (even worse) that we recently obtained enough facts to settle the issue.

14. **Peter Woit**
March 14, 2013

Tom,

"unfounded devotion to string theory" is very much a minority position among working physicists, probably much more of a minority than your estimate of the 20% of philosophers believing in God. In particular, Krauss is a well-known critic of string theory.

On the "nothingness" debate, I'd guess most physicists find it an embarrassment and if the question of what to do about the AMNH debate were up to them would disinvite both Albert and Krauss and cancel the whole thing. I'd hope that most philosophers feel the same way, not that "their side" in this idiocy is the right one.

15. **nasren**
March 14, 2013

Second Tom's view. Albert's criticism of Krauss was right on the money — even if the tone left a sour taste in the mouth. Krauss's response was unbelievably childish, and this disinviting nonsense smacks of the same kind of behaviour.

16. **tt**
March 14, 2013

you'ld be closer to Pi on 3/14/15

17. **jim**
March 15, 2013

Yes. Krauss was the one making weak philosophical arguments and selling them to the media as science. Albert only pointed that out. Then Krauss commits the ad hominem fallacy. What an embarrassment.

18. **Gabriel**
March 15, 2013

> Physicists can live only in those parts of the multiverse where mathematics is efficient and the universe is comprehensible.

To read him more charitably, I think this means not that abstract mathematics varies from universe to universe with regards to some sort of intrinsic efficiency, but that physics varies from universe to universe in such a way that the efficiency with which it can be expressed mathematically also varies.

But I'm not going to watch the lecture to find out if that first impression is the correct interpretation in context.

19. **Kavanna**
March 18, 2013

Multiverse lunacy 🙂

The debate over nothing is a disgrace. Physicists should be keeping as far as possible from such misuse of philosophy and pseudo-theological mumbo-jumbo.

20. **DrDave**
March 18, 2013

From King Lear: "Nothing will come of nothing"

21. **tt**
March 18, 2013

"Nothing from nothing leaves nothing" — Billy Preston

22. **Anon**
March 21, 2013

Is mathematics efficient and is the universe comprehensible? Linde is begging the question.

23. **nabil**
March 22, 2013

I don't like the idea of the existence of huge number of universes which people use to explain anything they don't understand . It's very easy to say that the physical constants are the way they are because we are in a part of spacetime that happens to have these values .I think this is not the way nature is designed .

24. **veskebjorn**
March 25, 2013

@tt: You'd be much closer to pi on 3/14/16–at least in our peculiar version of Euclidean geometry. Elsewhere in the infinite variety and delights of the multiverse, pi might be a different number, a circle might be impossible, or numbers might be useless in trying to describe local reality. But then, the idea that physics requires “efficient” mathematics and a “comprehensible” universe is itself absurd. Humans have been making physical sense of and accurate predictions about our universe for millennia without the aid of mathematical notation, whilst being certain that variously irrational, more-or-less potent, divine entities existed and were subject to no laws. Math seems to have come a long way, but some of the wilder-eyed physicists who promote unfalsifiable fantasies don’t seem to have progressed much beyond Genesis.

Disappearing gammas

March 14, 2013
Categories: Experimental HEP News

The CMS data on the Higgs in the gamma-gamma channel has been released this morning, see [slides from a talk at Moriond]. Basically the excess over the SM prediction seen in this channel in earlier data is gone, with CMS reporting ratios to the SM predicted value of .78 +/- .27 using one sort of analysis, 1.11 +/- .31 using another, so, naively averaging, say .95. ATLAS sees 1.65, so a naive combination would give 1.3, only about one sigma high, very consistent with the SM.

Amusingly, the better than 4 sigma signal CMS was advertising last summer in this channel that was part of the case for the discovery announcement has largely vanished in the new 8 TeV data. With one analysis method, they see only a 2 sigma signal in the 8 TeV data. If they had been working with this new, larger and better, data set instead of the older, smaller 7 TeV data set, the Higgs discovery claims might not have been possible last summer. Of course, the CMS + ATLAS combined gamma-gamma results are very strong evidence for a Higgs signal, and the ZZ results are overwhelming, so the existence of a new particle is not in doubt. This is actually what you expect if a SM Higgs is there: you should get reversion to the mean and disappearance of the earlier too large observed excesses.

CERN has a press release out today which is getting a log of attention, headlined
[New results indicate that particle discovered at CERN is a Higgs boson]. This emphasizes results about the spin, but the new gamma-gamma results are what is significant, as they remove the one anomaly that was getting a lot of attention from theorists hoping for some kind of violation of SM behavior.

Comments

1. **behghk**
   March 14, 2013

   CERN does not call it a Brout-Englert-Higgs-Guralnik-Hagen-Kibble-pleaseaddmynametothelist-(Anderson hey what about me?) boson?

2. **King Ray**
   March 14, 2013

   Success has a thousand fathers but failure is an orphan.

3. **dark**
   March 14, 2013

   To anyone –
   what are the scientific ramifications of finding a *single* higgs boson (and no SUSY @ LHC) with SM only properties @125-6 GEV to the likelihood of SUSY, MSSM, GUT SUSY-GUT? I was of the understanding that they predicted multiple higgs bosons., and therefore, finding 1 and only 1 decreases the likelihood of SUSY or GUT.

4. **Eric**
   March 14, 2013

   Dark,

   The additional Higgs bosons predicted in the MSSM can have large masses such that they would not have been observed at the LHC. This is also true of all other scalars in the MSSM.

5. **dark**
   March 14, 2013

   eric ok thanks,

   but in those class of models are those “too large for LHC energies” masses “natural” and “well-motivated” or simply epicycles?

   I thought natural well motivated MSSM prefer a very light Higgs 110GEV?

6. **jinb**
   March 14, 2013

   Scalars are more naturally heavy than they are light, the only expectation for them to be light being the hierarchy problem, but otherwise if you relax that assumption it’s not arbitrary. Quite the opposite, it would be arbitrary to have them light, which is the guiding principle behind nima’s split susy model. Just abandon TeV scale hierarchy problem and see what’s the most natural thing for susy to do.

   SUSY does prefer a light Higgs relative to the Z mass, and 125 GeV certainly is light.

7. **paddy**
   March 14, 2013

   And just what is the “most natural” unnatural thing for susy to do? kinda like a word game eh?

8. **Thomas Larsson**
   March 14, 2013

   That the Higgs mass seems to sit exactly at the border of SM consistency is interesting, and reminds me of a similar phenomenon in another part of physics. In the 1940s Lars Onsager proved some inequalities that critical exponents must obey for consistency. Twenty years later people found that these inequalities were in fact identities, i.e. critical exponents are on the border of being inconsistent. The underlying reason for this is scale symmetry.

   Extrapolating this observation to the SM, the fact that Higgs seems to be borderline inconsistent could indicate that some symmetry principle is at work. There is of course no secret what I believe that this symmetry principle might be.

9. **A**
   March 15, 2013

"but in those class of models are those "too large for LHC energies" masses "natural" and "well-motivated" or simply epicycles?"

I wouldn't call them epicycles because you don't have to add anything to the model to obtain them, but the higher you raise the remaining SUSY masses, the more finely tuned is the smallness of the electroweak scale relative to those SUSY masses.

10. **Allan Rosenberg**
March 15, 2013

According to the Washington Post, the conclusion "helps solve one of the most fundamental riddles of the universe: how the Big Bang created something out of nothing 13.7 billion years ago." http://www.washingtonpost.com/world/europe/physicists-say-they-are-now-confident-they-have-discovered-the-long-sought-higgs-boson/2013/03/14/0ffa6562-8c90-11e2-adca-74ab31da3399_story.html Let all of the people who say HEP experimentation is a waste of resources chew on that!

11. **bang**
March 15, 2013

It does help, in its own way. Take away the Higgs, the quarks and leptons, the (nonabelian) gauge bosons, the photons … and what do you have? No photons, ergo no blackbody radiation, ergo no CMB, ergo … how will you even be able to say the Universe came from a Big Bang?

12. **emile**
March 16, 2013

I've found recently that many people (even a few in HEP) confuse the Higgs boson of the SM with the particle predicted by Higgs. Back then, there was no SM, and no weak neutral boson. The prediction involved a scalar field that coupled to W bosons to give them their mass. I'm glad that CERN is now making the point that this is a Higgs boson: there is a particle beyond a reasonable doubt, it couples to weak bosons, and spin measurements are compatible with a spin 0+ particle and the alternatives are disfavoured (at ~95% CL or greater).

@bang: could you expand a bit on cosmology without EWSB? so we would have no photon but we would have four massless bosons, including the "B". So what would replace the CMB? matter-anti-matter particles would annihilate to what? There would be a background radiation of Bs? I had not thought of this before…

13. **Michael Brown**
March 16, 2013

@emile Recently read this physics stackexchange question on the standard model without EWSB. I found Ron Maimon's answer in that thread to be interesting and well worth reading.

Note that QCD still gives a mass to the weak gauge bosons (though much smaller than their actual masses due to the Higgs), and nuclear physics is very different. I'd love to know if anyone has any scholarly references about this.

14. **Alex**
March 16, 2013

"I'd love to know if anyone has any scholarly references about this."

Yes, for example Robert Shrock and Chris Quigg made quite an effort exploring this scenario explicitely here:

http://arxiv.org/pdf/0901.3958

15. **bang**
March 16, 2013

@emile: Back up a step and set aside EWSB. Go back much further, to the days before QM and Relativity. Go back to the 19th century. Scientists in the 19th century (geologists etc) had come to realize that the Solar System was at least hundreds of millions of years old. Scientists also knew enough physics to realize that the Sun was a ball of mainly hydrogen gas, and its approximate size and its radiated power. They also knew from chemistry that chemical hydrogen burning to water could not provide eneough energy to sustain the Sun's radiated power for such a long time. So the source of the Sun's energy was a puzzle which 19th century physicists knew they could not explain with the physics of the day.

My point is *they knew that they did not know*. They knew that there was new physics `out there.'

And they were correct. We know today that thermonuclear fusion is the source of the Sun's energy. It is a form of energy unknown in the 19th century. It required both QM and Relativity to answer the puzzle of the source of the Sun's energy.

So, my point is that the Higgs, etc. *do* help to increase our understanding of fundamental processes, the interaction of matter and radiation, and this does help, in its own way, to understand the physical processes of the Big Bang. Don't jump ahead so quickly to EWSB.

16. **Flakmeister**
March 18, 2013

With regards to the the earlier modest excess over the SM in the di-photon channel one needs to appeal to the old adage "discoveries are always made on an upward fluctuation"….

I do have to admit that "No-lose theorem" for a TeV scale collider is starting to look like the lowest possible payoff was in the offing… Too bad, even though I was never a fan of low energy SUSY, but, discovering would have enabled a return to the Golden Age of Particle Physics, i.e. when everyone with pulse could find something new, but unlike the 60s, one could actually calculate quantities….

Given the slim prospects and likely timeline for the next collider, perhaps it is just as well that it was a SM higgs and that was "all she wrote"….

Request for Advice

March 15, 2013
Categories: Uncategorized

My main method for keeping track of new information on the web has for many years now been RSS, with Google Reader for a while the main tool for this. Google yesterday announced that they're shutting this down, with as far as I can tell the reason being that RSS doesn't fit into their plan for world domination. So, like everyone else, I need to figure out how to change over to something new. For "what do I do now to keep track of other web-sites?", there are hundreds of such discussions I can follow (Feedly seems to be getting the most attention), although I'd be interested to hear from anyone who is very knowledgeable about this. If it doesn't run across multiple machines with different operating systems, I'm not very interested. If it only runs on mobile devices, forget it, although being able to run it that way would be a plus.

More importantly though, I'd like to ask for advice from my readers about how they keep track of new blog postings here and what could be done to make that easier. Extra points for links to how to implement solutions using a standalone WordPress installation. Note that I'm not trying to find ways to drive lots of new traffic here, more interested in making life easier for the generally well-informed readers I already have.

So far in life I've pretty much completely avoided knowing anything about social media and how they work, so advice on that front would be appreciated. About all I know now is that there is some way to set up twitter to provide announcements of blog postings, and that looks like one of the first things I might try. If there's some way to do this kind of thing without festooning one's site with other people's logos and stuff about "likes", that would be great.

Update: Looks like RSS is still the most useful for people. I have however set up a "notevenwrong" twitter account, and just installed a wordpress plugin that should add something there when a post is published or updated. Let's see if it works…

Comments

1. **David Woodruff**
   March 15, 2013

   I'm in the same boat, since I follow a large number of blogs including yours with Reader. I am still in denial about Reader going away! There is a change.org petition that has already attracted quite a lot of attention (http://www.change.org/petitions/google-keep-google-reader-running)–maybe google will have mercy on us and keep it going.

2. **Matt**
   March 15, 2013

   I for one am anxious to see what people post here for solutions. I'm at a total loss for what to do. The whole point of Google reader is that I can keep everything synced to my Google account and don't have to deal with all sorts of accounts all over the place.

3. **Kasuha**
   March 15, 2013

   I don't know if this will be helpful but anyway…

   List of alternatives:
   https://docs.google.com/spreadsheet/ccc?key=0ApTo6f5Yj1iJdFRfWmhUVjV0WkktTjJhUUE4dGR5WUE#gid=0

   A petition to keep it running:
   https://www.change.org/petitions/google-keep-google-reader-running

4. **Hypnos**
   March 15, 2013

   I use a standalone RSS reader to keep track of various feeds, including that for your blog; I have never used Google Reader, and don't understand why its termination is such a big deal.

5. **Deane**
   March 15, 2013

   I'm in the same bind. Will watch the comments to se what's recommended.

6. **Filip**
   March 15, 2013

   Apart from Feedly that I don't like that much, I will try The Old Reader (currently waiting my subscriptions to be imported from Google, as there are now too many new users jumping in). It seems that it is an option the most similar to Google Reader, and you can use your Google account with it. Of course, it is Web based, so any machine is good for it.
   Cheers

7. **Peter Woit**
   March 15, 2013

   Hypnos,

   I used several standalone RSS readers before Reader. The problems with them typically were that they didn't sync between multiple computers (e.g., home, work, mobile), that they were buggy, and didn't behave well when dealing with large numbers of feeds, or with the content of some kinds of feeds.

8. **fraac**
   March 15, 2013

   I don't really know how this works, or why they would want it to suddenly not work, but I'm *assuming* that the 'click to subscribe' button in my Chrome address bar will still be there and allow me to subscribe to RSS feeds using a different but similar Google service.

9. **Markk**
March 15, 2013

I am in the same boat. I track blogs with Google Reader. I will be looking for some Web based system to replace it. I am surprised that the number of people using it is dropping. I guess people that have a list of blogs to follow is now small? Has the readership dropped off? People must be using something.

Hypnos, the advantage Google Reader gave me was that I could read entries at lunch at work, the go home and pick up right where I left off on a different computer, then go on a trip and use my nook, and everything was all the same everywhere and every change went everywhere.

10. **jensph**
March 15, 2013

The Old Reader looks like an great alternative (I've missed not having Reader comments since Google launched Google+), assuming they launch mobile versions that cache content.

I've been trying out Feedly. It looks pretty, but I'm not sure if I'll be able to navigate as efficiently.

Also Digg is promising to build an option.

11. **Martin**
March 15, 2013

I just switched to newsblur.com and am finding it a very adequate replacement for google reader. They even helpfully connect to your google account when you signup and import your feeds for you with no fuss.

Their service is under a bit of stress at the moment (as I believe a lot of the alternate reader services are at present) but I suspect that will dissipate over the coming days.

The service is a paid subscription if you have more than 64 sites, but the cost is less than a cup of coffee a month ($2 monthly). Paying has the benefit that you are no longer the product but a paying customer. Services that you pay for a product tend to pull the rug from under your feet less quickly than ones where you are the product.

12. **Kirill**
March 15, 2013

I use RSS reader in desktop Opera browser because of easy search, filters and integration with e-mail client, but because Opera has questional future and it has not any synchronisation I wanted to move to Google Reader in the past.

13. **KP**
March 15, 2013

I have recently switched to Feedly from Reader, and I really like it so far. The good people at Feedly have introduced a very easy way to migrate all of your feeds from Google Reader before it shuts down.

It installs as a browser app for Chrome and Firefox (haven't looked at other browsers) and also as a mobile app on iOS and Android. Also, it works on Linux which is a good thing.

I have been following NEW on Reader for the past few years and now Feedly for the past few days. You do have a link to your RSS feed in the site's Meta section which is what is needed to follow this site. In any case, modern feed readers are smart enough to figure out the feed even if you only give them the URL of the site. (That is how I did it on both Reader and Feedly.) So to me it seems that you really don't have to set up anything more.

Cheers

— KP

14. **Fred P**
March 15, 2013

Personally, I just visit this site; I was unaware that you had an RSS feed – nor am I likely to use it now that I know.

As for Google Reader, I'd never heard of it. For data-intensive cases, or sources where I want to make certain that I don't miss any content, I use RSS. Otherwise, I surf when I'm interested. This means, of course, that sites I travel to less frequently I forget about – but I consider that a feature, not a defect.

15. **Boaz**
March 15, 2013

I also use Google reader RSS reader to find out about new posts for your blog.
I'll probably try out some of the other readers, which I've gotten lazy and forgotten how to use. I also use Twitter sometimes and a lot of blogs I follow make announcements of new posts there. Here's a Twitter based voting system where people are suggesting different RSS readers.
Feedly is #1, Bloglovin is #2…
Thanks for your informative blog which has been a staple of my internet reading over the years.

16. **Hypnos**
March 15, 2013

Thanks for the explanation — the issue is here is not that an app was discontinued, but a *platform*. As you hinted at, this is precisely the risk with cloud services and why I don't use them. Since I need a laptop for work anyway, I just have everything set up on that including my RSS reader.

Good luck!

17. **Heikki Arponen**
March 15, 2013

There's still time until july. I bet there will be plenty of decent alternatives by then, some of which may not be free.

18. **Suzanne L**

March 15, 2013

You could take a look at rss2email (http://www.allthingsrss.com/rss2email/). It's a python program that fetch news from your RSS feeds and redirect them to your mailbox (as text or html message, whatever you prefer; one mail per new entry). You should be able to run it on every system that runs python. You just run it, say once a day, from one arbitrary computer (but allways from the same, to avoid getting duplicates), and then you can access and archive the news of the day from any device from which you can access your mailbox.
If you have an account on some server that is connected to the internet all the time (like a university server), maybe you can have a script running periodically on that server that do the job automatically.
This is what I use to follow websites, including this blog, since I find it very convenient to get everything (my mails, news, arXiv feeds…) from just one program (my mail viewer). Combining it with mail filtering is best…

19. **X**
March 15, 2013

I started kicking Feedly around and was not impressed. My use pattern for gReader is to leave it open all the time with a list on the left panel of all feeds with unread stories. When I have time, I click on a feed and read some stories from it. I can abandon the page at any time and come back later to find if new stories have appeared in any feed.

When I tried the same thing in Feedly, trying to read one feed moved me into some kind of sub-page showing a fraction of my feeds (both read and unread (Why do I want to look at feeds I already finished reading?)). Apparently, they expect me to mouseover and click around some tabs to see what's updated. The whole point of a feed reader is to save me the time I was spending clicking around a hundred bookmarks. For me, unusable.

It also seems to want to put big pictures all over the place. I'm not reading a lot of picture books online, so I just get swaths of whitespace next to all my items.

20. **Wally**
March 15, 2013

WordPress has a reader. You can subscribe to and read Blogger and WordPress blogs. Good enough for basic purposes.

21. **JohnB**
March 15, 2013

RSSOWl is very good, but as you say, it has issues with certain feeds, and can not sync between machines (it used to use Google Reader for this); the problem with certain feeds is longstanding and I'm not sure it will be fixed (a pity as great program otherwise), but the authors will be looking at alternative sync systems, possibly using the future Feedly sync API at some stage.

So yes, very good program, just the devs aren't as active at updating it as they should be:
http://www.rssowl.org

22. **ohwilleke**
March 15, 2013

FWIW, a lot of us geezers just click through a bunch of links in a blogroll every two or three days, no RSS feed or google reader services required.

23. **Peter Woit**
March 15, 2013

All: I've set up a twitter account "notevenwrong" for people to follow that will notify about new postings, for those not using RSS for this purpose. I'm not now planning to put anything else there.

fraac,

Google has also just removed that functionality from Chrome, see here
http://techcrunch.com/2013/03/15/google-kills-rss/
Evidently that was an interface to Google Reader, but could be configured to work with other RSS readers.

It looks like this is not just a decision to stop developing a particular piece of software, but a general declaration of war on RSS, removing RSS support from all Google products. The most convincing explanation I've seen for this is that Google has always had a problem with people getting content via RSS, since this typically bypasses their advertising and other tracking embedded in web-pages.

24. **JJG**
March 15, 2013

Netvibes (http://www.netvibes.com/) is pretty good for aggregating a pile of RSS feeds, rather like the old Google homepage — I switched to it after that closed that down. It seems everything of Google's that I've ever used has been closed down, so I avoid them now so as to not jinx other users (I never used Reader, that's not my fault).

25. **Chandan Dalawat**
March 15, 2013

Google wants you to join Google+.

26. **yoshi**
March 16, 2013

Yep, I recommend netvibes since I use it everyday

27. **Bee**
March 16, 2013

I'm another one of these people who use Google reader to sort their news. I'll probably migrate to feedly, it has an iPhone app, which is handy because I presently pipe the Google reader into Newsify, which probably won't work anymore. I've tried netvibes, but it seems terribly cluttered and I don't really have the patience for this.

28. **Peter Woit**

March 16, 2013

Chandan Dalawat,

As far as I can tell, Google+ just doesn't do what I need at all, with no way to keep track of new material on blogs and other internet sites (except Google+ sites). One of the main things it does is provide people a way to link to internet material they find interesting and want to share, but I'm guessing the way people are finding such material is typically via Google Reader (or some other RSS reader).

29. **Vytautas Jakutis**
March 16, 2013

Digg is building a reader.

I personally connect with ssh from various computers to my remote home server and use Newsbeuter.

30. **paddy**
March 16, 2013

Peter,
Not helpful and perhaps a bit snarky, but I use NEW to tell me when something interesting is happening. Thank You.

31. **Peter Woit**
March 16, 2013

paddy,
Not really helpful to me, but much appreciated. Thank you.

32. **David Appell**
March 17, 2013

I use a standalone RSS reader, Bloglines, but have Blogtrottr email me new blog entries for blogs I really want to keep up with (including this one).

33. **Suzanna E**
March 17, 2013

I am a scientist in the epidemiology field. To be honest to get my news updates in my field, I subscribe to email alerts from all the scientific journals of interest – i select updates on news plus Table of Contents. I find this approach extemely useful.

34. **Shuhao Cao**
March 17, 2013

I pretty much track all my scientific reading in Google reader. Especially there is a nice feature in reader called "sort by magic", where it gathers your reading statistics and chooses what you like to read. Twitter? I don't know. Though RSS sounds so yesterday and web 2.0'ish, it is still my main source of getting information, learning new things other than my research area. Guess I'll just switch to another feed reader. BTW: saw a petition in the reply. The idea of petition sounds pointless to me, for Google dropping support for RSS must be a long term plan, and reader would never come back.

35. **Kevin**
March 17, 2013

I have used RSS since the first version of NetNewsWire. Then they added Google Reader syncing, which I adopted (without ever visiting Google Reader itself). Through the years I have come to rely entirely on RSS feeds, synced with Google Reader, but viewed on any of a dozen Mac, iPhone and iPad apps. I can check journal articles on my phone while I wait for my coffee, and when I get to my office, a different program on my desktop already knows I read those titles/abstracts. It is the most efficient workflow for keeping up to date with journals and blogs. These programs also integrate Instapaper perfectly, so I have a hierarchy of "favorites". I have no idea what to do without Google Reader for syncing, but I hope someone figures it out.

Given that not once in close to 10 years have I actually visited the Google Reader web interface, I'm not too surprised that Google killed it. The have by far the most unappealing user experience imaginable (except Google itself), so they should stick to running a huge database and let real designers do the front end. Worse, my university adopted Google for everything (including our email!), so we are forced to use those soulless UIs. Fortunately, there are many capable programs that sync with Google Calendars and other Google databases.

36. **Stephen Crowley**
March 18, 2013

Sage is an excellent RSS reader plugin for Firefox. https://addons.mozilla.org/en-us/firefox/addon/sage/

37. **Ossicle**
March 18, 2013

I almost literally don't understand a word of this, but perhaps someone here will find it interesting. A techie friend posted to Facebook:

"This looks like a thing to mention.

Basically: here's where you vote to tell Bitnami that they should support "Tiny Tiny RSS". If they *do* support ttRSS, the upshot will be either a tarball or an RPM or a VMWare image or an Amazon Cloud image (usually "all of the above") that you deploy and, bampf, you're running your very own replacement for Google Reader."

And he linked to this:

http://bitnami.org/product/tiny-tiny-rss

-O

38. **Peter Woit**
March 18, 2013

Ossicle,
I’ve looked at that. The idea is that you run your own version of what Google is running, on your own webserver. Not useful if you don’t have your own webserver to use for this purpose. I’m still curious though if any one using it can comment on whether it has performance or bug problems (can you really do this kind of thing without Google’s massive server farm, or without a staff of people to maintain the software?).

39. **Gerald Loeffler**
March 18, 2013

just wanted to stress how very **sub-optimal Twitter** is compared to RSS/Atom: only with RSS/Atom does the reader get a guarantee (or as close to a guarantee as is possible with currently available technologies) that he/she doesn’t miss any pieces of information published by an information source. With Twitter it takes an inordinate amount of effort (and is decidedly non-standard) to not **lose pieces of information** published when the reader was offline.

40. **Bob**
March 18, 2013

I spent an hour or so going through the popular alternatives to Google Reader and settled on NetVibes, because with a little configuring it was the most similar to Google Reader. Switch from “widgets” view to “reader” view to get a Google Reader-like interface. Then delete all the default feeds and import your personal feeds. (You only have to do these things once.)

Other readers I tried:

Feedly: The interface is too different from Google Reader for me. It requires multiple clicks to do things I am used to doing in one click.

NewsBlur: Almost like Google Reader but with separate window panes for story titles and expanded stories. I don’t like the extra window pane.

The Old Reader: From what I can tell, stories can’t be read in the reader, you’d have to keep leaving the reader and going to the external webpage.

41. **Rien**
March 19, 2013

I read this and all other blogs almost exclusively through RSS, mostly with Google Reader on my phone and with Gruml on my Mac. I usually only click through if I want to read comments. (Or comment, which I almost never do, but I usually read most of your posts.)

The Google Reader app is by far the best RSS reader I’ve tried, unfortunately. Before I stopped using Firefox I used Sage, which is quite good.

42. **Leonardo**
April 3, 2013

With some customization, the use of Feedly can become very similar yo Google Reader. First time I tried Feedly, I felt a little bit awkward and thought that it wouldn’t work, but later, as I customized it, and found out the next-previous buttons are j-k (counterpart of GReader was n-p which makes something else in Feedly), now I am pretty comfortable with Feedly. But the fact that it is in the form of an add-on bothers me a little, I would prefer a standalone webpage with its own login, etc. But I am not sure whether this is really important or not.

Busy Week

March 16, 2013
Categories: Uncategorized

There should be lots of breaking news this upcoming week, sometimes with real-time webcasts for those that want to follow along:

- Planck data release on Thursday the 21st. Media briefing in Paris will be at 10am local time, see here. In the US, NASA will host a press conference at 11am EDT, see here.

  The night before here in New York at 7:30 pm (watch here) two journalists and three leading cosmology theorists will be discussing the emergence of the universe or multiverse from nothing. Perhaps someone will ask them what this theoretical work implies in terms of predictions for the new results to come out the next day.

- On March 20th at noon in Norway, the winner of the 2013 Abel Prize in Mathematics will be announced here. This is a prize of about $1 million, set up in 2002 to be an equivalent of a Nobel prize in mathematics. They seem to like to give this one to people from the Courant Institute here in New York.
- I'm wondering what's up with the Templeton Prize, a $1.66 million dollar prize normally awarded each year in March. Haven't seen any announcements, but perhaps this will also happen this week.
- Finally, there's Yuri Milner's Fundamental Physics Prize, which at $3 million makes everyone else look like pikers. Last December, the news was that the award would be announced at a ceremony at CERN on March 20. Candidates for the prize are a group of three condensed matter physicists, string theorist Joe Polchinski of UCSB, and string theorist Alexander Polyakov of Princeton. With the decision being made by a group of previous winners largely consisting of string theorists from Princeton, if I had to guess the winner, I'd go with the string theorist from Princeton. Coincidentally or not, Polyakov is scheduled to give the String Theory Seminar at CERN on March 20, on the topic of Sensitive, unstable and turbulent vacua.

Update: The Fundamental Physics Prize announcement will be Wednesday at 8pm, live webcast here. The IAS faculty will be there in force, with TH String Theory Seminars scheduled for Arkani-Hamed and Witten on Tuesday, Seiberg on Thursday.

Update: The Templeton Prize will be announce April 4, see here.

Comments

1. **Deane**
   March 16, 2013

   It seems unlikely that they would award it to anyone from Courant this year, since the committee met and made their decision *at* Courant. It would look really funny if they picked someone from Courant. Of course, it's already amazing that they've already chosen three from Courant. And, if you ask me, Louis Nirenberg easily deserves one.

2. **Peter Woit**
   March 16, 2013

   Deane,

   I think you're probably right. On the other hand, while it would look kind of funny for a group of Princeton string theorists to award the prize to a Princeton string theorist, I'm not betting against that…

3. **Pi**
   March 16, 2013

   I hope John Conway gets the Abel for this year.
   And also I found this recent article in guardian about SUSY interesting:
   http://www.guardian.co.uk/science/life-and-physics/2013/mar/14/supersymmetry-spell?CMP=twt_fd

4. **hu**
   March 16, 2013

   What happened with AMS-02 results? Haven't heard anything since the announcement back in February.

5. **anon.**
   March 17, 2013

   string theorist Alexander Polyakov

   Surely Polyakov is, by any measure, one of the great quantum field theorists–or for that matter, theoretical physicists–of the last 50 years, even disregarding his work on string theory.

6. **Casey Leedom**
   March 18, 2013

   Obligatory Physics Related Comic:

   A: "I want you. By symmetry we can predict you want me."
   Physicist: "That would simplify things …"

   http://www.smbc-comics.com/index.php?db=comics&id=2918

7. **Seth Thatcher**
   March 18, 2013

   I also have been patiently awaiting AMS-02 results

8. **Peter Woit**

March 18, 2013

Seth Thatcher,

Well, the news this weekend from AMS-02 on twitter, see

https://twitter.com/AMSISS/status/313110134593318914

is that they really want to get to 3000 followers on twitter. Maybe that's what they're waiting for on the data release….

9. **DR**
March 19, 2013

the fundamental physics prize really should change its name to "the math in physics prize". Since when a theorist with all works contradicted by the experiments can win a "physics" prize? Also, N=2 is not physics, how come it gets a physics prize???

10. **Peter Woit**
March 19, 2013

DR,

This isn't a "math in physics prize", most of the laureates (and all of the candidates this year) have not done important mathematics or mathematical physics (Kontsevich and Witten are the ones who really have). The prize is for "fundamental physics", not mathematics.

God to Award Prizes for God Particle

March 19, 2013
Categories: Uncategorized

In more news about the Fundamental Physics Prize awards planned for tomorrow evening at CERN, it turns out that God himself (AKA Morgan Freeman) will be giving out the awards, as well as hosting a TV show about this the same evening.

Physicists have often claimed to be upset by the "God particle" business, but it looks like CERN and the Fundamental Physics Prize people may be now embracing the idea. Leon Lederman is the one originally responsible for the terminology, supposedly because his publishers twisted his arm into using The God Particle as the title of his 1993 book about the Higgs. Evidently they've twisted his arm again, with Beyond the God Particle set to appear in October.

Both Sean Carroll and Matt Strassler are upset about a CBS News report, which contains some scientifically inaccurate hype that they're blaming on Michio Kaku. It's unclear to me why, after 20 years of over-the-top hype about string theory and extra dimensions from Kaku (going back for instance to here), they're all of a sudden up in arms about this now. Whatever the reason though, I very much agree with Matt's

> Doesn't the taxpaying public deserve the truth? Isn't the truth already exciting enough? And what will the public think of science if, in this information era, the promulgation of falsehoods and near-falsehoods on national media is unanswered by complaints from other scientists?

and think it's great that he and Sean are now turning their attention to this problem.

Update: It's definitely gang up on Michio Kaku day in the blogosphere, with PZ Myers and Chad Orzel chiming in. Still a mystery to me why this is happening today, since Kaku has been responsible for far more outrageous stuff continuously for over 20 years.

Update: Philip Gibbs comes in on Kaku's side here.

Comments

1. **Jon Orloff**
   March 19, 2013

   I have always thought that the term "God Particle" was a mistake. In the first place it sounded as if high energy physicists were using it as a gimmick to get taxpayers to pony up for bigger accelerators (who wouldn't pay for a bigger machine to find God?). In the second place it induced (induces) cringing and leaves a bad taste in one's mouth..

2. **Nex**
   March 19, 2013

   Same old problem, I remember reading about Feynman criticizing the "eightfold way" theory name cause of people taking it literally as a connection between physics and buddhism. The "god particle" term seems way more problematic still.

3. **gott**
   March 19, 2013

   Actually, Leon Lederman wanted to call the Higgs the `Goddamn particle' because it was so difficult to find, but the editors objected to that and changed the name to the `God particle'. So *that* was the arm-twisting of Leon Lederman by the editors. As for the new book `Beyond the God Particle' I've not heard of it till now and I have no idea what arm-twisting if any took place.

4. **Hypnos**
   March 19, 2013

   > It's unclear to me why, after 20 years of over-the-top hype about string theory and extra dimensions from Kaku (going back for instance to here), they're all of a sudden up in arms about this now.

   Because the Higgs actually exists?

5. **Flakmeister**
   March 19, 2013

   @Gott,
   yep, thats the version of the story that I heard…. And having had some interactions with Leon, it does seem plausible….

   And something tells me that cursing that damn particle is not over…

6. **cormac**
   March 19, 2013

   I like Nex's 'god particle' with a small g, I must admit I've been using this for a while. I find it a reasonable compromise, gets bums on seats while not pretending to deities etc…in the many talks I have given, I have yet to meet a member of the public who thinks it has anything to do with religion

7. **Yatima**
   March 19, 2013

   > "Still a mystery to me why this is happening today"

   Just another black swan event.

8. **Jens**
   March 19, 2013

   If Higgs Particle = God Particle, shouldn't Higgs be God rather than Morgan Freeman?

9. **notHiggs**
March 19, 2013

@Jens: Nope. Or at least I expect that will the chorus of protest from Brout, Englert, Guralnik, Hagen, Kibble, and never mind that one of them's dead, since we're talking about God here. Are those squawks from Anderson I hear in the background?

10. **David Metzler**
March 19, 2013

Your headline would make The Onion proud. Nicely put.

11. **lev**
March 19, 2013

Why not call this particle simply "higgs"?

12. **yuk**
March 19, 2013

By the same logic, let's call the electron a `dirac' (or `jj' or even better a `faraday' … how about 'faraday and dirac' = `fad'?), and the pion a `yuk'.

13. **lev**
March 20, 2013

to yuk:
there is no time reversal in history. Do you agree that higgs is simpler than Higgs Boson and less blasphemous than god?

14. **yuk**
March 20, 2013

Not really. I'd be perfectly happy with 'brussels brout'.

15. **lev**
March 20, 2013

How many physicists would be perfectly happy with your choice?

16. **yuk**
March 20, 2013

Picky, picky … how about 'qibble' ? (to be pronounced 'kibble'?)

17. **Johnny**
March 20, 2013

"God particle" is a mistake because God does not exist but the particle does!

18. **poisson**
March 20, 2013

Je n'avais pas besoin de cette hypothese?

19. **paddy**
March 20, 2013

yuk,
While we are renaming particles..how about not muon but…(drum roll please)…the "whoorderedthaton"?

20. **yuk**
March 20, 2013

@paddy: Very good! I completely missed that, but it deserves first prize!

21. **speakertoanimals**
March 21, 2013

Couldn't agree more with your comments about Kaku: he's been an embarrassment for two decades now. Maybe some of the folk are upset (now) bc he's being quoted instead of THEM?

Joe Lykken: No SUSY, No Naturalness Problem

March 19, 2013
Categories: Uncategorized

Conventional wisdom in the particle theory for about 30 years has been that the Standard Model has a huge "hierarchy" or "naturalness" problem, the solution to which is supposed to appear at the LHC via SUSY or some other new BSM physics. With no SUSY or other BSM physics appearing at the LHC, this conventional wisdom is now moving towards claims that fundamental physics has been shown by the LHC to be "unnatural", with parameters that are environmental, artifacts of our position in the multiverse generated by the anthropic landscape of string theory. For an example of this, see Seiberg's [Now What?] talk at [Aspen] (Arkani-Hamed also spoke, with presumably a similar point of view, although the talk is not available).

It seems to me that a much more logical conclusion to draw would be that the LHC has just shown that the hierarchy/naturalness argument was mistaken. I've never understood why people found it convincing, and have often argued about this here on the blog. From the "hierarchy" angle, the problem is why the ratio of the electroweak-breaking scale to the GUT or Planck scale is such a small number, but we don't actually have any evidence for GUT physics or for quantum gravitational physics, so no good reason to be sure that such high scales are relevant to anything or the cause of a hierarchy problem. From the "naturalness" side, while the theory is renormalizable, one can worry about the sensitivity to high energies of its cutoff dependence, but it's unclear to me why one should be that concerned about this. More worrisome is that the Higgs sector introduces most of the undetermined parameters of the SM, a much more serious defect of the standard theory.

Today at a workshop on [The First Three Years of the LHC], Joe Lykken gave a talk on [Higgs without Supersymmetry], in which he argues that there is no naturalness problem or need for supersymmetry, and makes a specific suggestion about how to think about the high energy behavior of the Higgs. He starts off with:

> is there a Higgs naturalness problem?
>
> •For decades the HEP community has asserted that naturalness is the central issue
> •Simply put, we have assumed that either EWSB is natural, in which case we need to explain why, or that it is fine-tuned, in which case we also need to explain why
> •I will argue that this is a false dichotomy,and that LHC results are hinting at a third path

then explains the standard dogma about quadratic sensitivity to the cutoff. He argues that the solution to this problem lies in properly understanding the scaling behavior of the Higgs, following ideas that go back at least to W. Bardeen in 1995 (see [here]). The fact that the renormalization group flow of the quartic term in the Higgs potential takes it to zero at high energies is interpreted as a suggestion that the right UV boundary condition is that the Higgs potential vanish. From there Lykken goes on to discuss more specific ideas, which may lead to observable new physics at LHC scales.

These aren't really new ideas, but I think Lykken is drawing the right lesson from the LHC results: the naturalness argument for SUSY has now been shown to have been misguided, and it's time not to give up and adopt the pseudo-science of anthropics, but instead to question the dogmas that have dominated the subject for decades.

Comments

1. **Flakmeister**
   March 19, 2013

   Link for Lykken's talk is a duplicate of Seiberg's….

2. **Peter Woit**
   March 19, 2013

   Flakmeister,

   Works fine for me??

3. **Mean and Anomalous**
   March 19, 2013

   The Lykken link works fine for me as well. Also, Peter, you wrote:

   '…for SUSY has now been show to have been misguided…', you meant to write '…shown…'

4. **Peter Woit**
   March 19, 2013

   Mean and Anomalous,

   Thanks, fixed.

5. **Flakmeister**
   March 19, 2013

   Thx.. my bad (too many tabs flying around)….

   Don't be so hard on TeV scale SUSY…. it always struck me as a jobs program for experimentalists and theorists… Hell, at LEP2, one search analysis could give you 5 easy papers…. SUSY did have the benefit of providing testable predictions (even though you knew there was a lot of wiggle room)

6. **Frank Quednau**
   March 19, 2013

   As an outsider to this world, I had to look up on naturalness – indeed it seems strange to me that lack of naturalness should hint at anything. The universe seems to show structures at many different scales of Spacetime, how should a model representing such a universe exhibit "naturalness" as defined in wikipedia?

7. **Peter Woit**
   March 19, 2013

   Frank,

The scales of various observed physics have some explanation in terms of our understanding of the astrophysics/physics (except I guess the size of the universe, the time back to the big bang is just an historical fact).

Very roughly, the idea of naturalness is that if even if you don't know how to compute something, you expect it to be "naturally" of order 1 in the appropriate units. If it's exponentially smaller, there should be some reason. People try and apply this to particle physics at scales such that we don't have experimental evidence.

Then the question arises as to why the Higgs mass (or W, or Z, etc) is so small in units of the GUT or Planck scale. The answer may just be that there is no GUT scale and the Planck scale is irrelevant. The SUSY explanation is that fermionic superpartners 0f the Higgs have zero masses (before SUSY breaking), and this keeps the Higgs small small. You then expect the SUSY breaking scale to be what determines the mass. Thus the problem arising now, since the LHC has shown that the SUSY breaking scale must be quite a bit higher than the electroweak breaking scale.

This argument is made in various forms at the beginning of just about every review paper or talk on SUSY, and I'm just giving a crude version of it. Best I not go on about the details, but look for a more detailed version that might be readable.

8. **lun**
March 19, 2013

Some of your collegues might have shown the string landscape is in fact falsifiable, by, erm… falsifying it
http://arxiv.org/abs/1303.4428.pdf

9. **Peter Woit**
March 19, 2013

lun,
As they say in the abstract "if our results prove applicable to string theory…"

But please, that's really off-topic, and I think the Lykken talk is a much more interesting subject.

10. **N. Nakanishi**
March 19, 2013

The non-existence of SUSY in Nature is quite natural; I have expected this result since thirty years ago. Because there is no Nambu-Goldstone fermion, one must go to supergravity in order to make use of super-Higgs mechanism. However, I showed that the supergravity cannot be a fundamental theory. In my review paper of the manifestly covariant canonical formalism of quantum gravity published in 1983 (Publ. RIMS 19, 1095), I emphasized "It is not the right way to extend the Lorentz invariance of elementary particle physics at the level of determining the fundamental Lagrangian density." (p.1125) The reason for this claim essentially arises from the fact that there is no spinor (linear) representation of general coordinate invariance. For more details, see my recent paper entitled "Space-time structure in the ultimate theory" (Intern. J. Mod. Phys. D 20 (2011) 253; DOI: 10.1142/S0218271811018809).

11. **Peter Woit**
March 19, 2013

N. Nakanishi,

Please, this has nothing to do with Lykken's talk about the Higgs which is the topic of this posting.

12. **Flakmeister**
March 19, 2013

Yes, the Lykken talk is interesting, dove tailed very nicely with Strumia's Moriond presentation. I am not a theorist but I do remember being struck by Bardeen's infrared fixed point stuff in the early-mid '90s. Glad to see that yet another old dog might be taught a new trick or at the very least get a few more walks around the park….

I took a gander at the Moriond talks and I was somewhat taken aback by the clearly palpable tone of the theory summary talk, given with a proverbial almost stiff upper lip in the face of what clearly may be the death knell of the HEP as an experimental science…. Ironically from its own astounding success…

I was also tremendously impressed by the quality of the experimental results from the LHC. It is clear that there are still truly excellent young people entering the field, I only hope that they find the oppurtunities they deserve….

13. **Thomas**
March 20, 2013

Peter,

Lykken states that his ideas will lead to dark matter discoveries. While it is great to eliminate one dream world (susy) he makes the mistake to introduce another (dark matter). Why is it not possible to stop with these silly games? After all, we only need an explanation for the SM parameters – not more not less.

14. **tulpoeid**
March 20, 2013

Kudos. Please keep your efforts at dispelling quantum theology – I wish it wasn't necessary but this has been on for so longer than anyone anticipated. And worse, the matter at stake is not just absence of falsifiability anymore.

15. **Peter Woit**
March 20, 2013

Thomas,

I'm not convinced by Lykken's dark matter model, but it's quite reasonable for him and others to pursue such ideas. A long shot, but at least not flogging a dead idea as in SUSY models.

16. **dark**
March 20, 2013

"It seems to me that a much more logical conclusion to draw would be that the LHC has just shown that the hierarchy/naturalness argument was mistaken. I've never understood why people found it convincing, and have often argued about this here on the blog. From the "hierarchy" angle, the problem is why the ratio of the electroweak-breaking scale to the GUT or Planck scale is such a small number, but we don't actually have any evidence for GUT physics or for quantum gravitational physics, so no good reason to be sure that such high scales are relevant to anything or the cause of a hierarchy problem."

are there any theoretical, observational or experimental ramifications if your position is physically correct i.e there is no GUT or planck scale in nature?

17. **Peter Woit**
March 20, 2013

dark,

I think the question to ask is whether there is any evidence for a GUT scale, or that the Planck scale has something to do with particle physics. A huge effort has gone on for decades trying to find such a thing, come up empty so far.

18. **dark**
March 20, 2013

Peter Woit

if there is no GUT scale, does this mean there is no "unification" of 3 of the 4 forces? if there is no planck scale, does this mean there is no QG?

19. **Peter Woit**
March 20, 2013

dark,

There may be some sort of "unification" of the SM forces (e.g. something that explains their relative coupling constants, and why SU(3), SU(2), U(1)). But there's no evidence for the specific proposal of putting these groups into a larger group like SU(5) or SO(1o) and then introducing some new Higgs fields to break the symmetry back down. This has never been the most compelling idea. Not only does it not solve the problems introduced by the Higgs, it adds a whole new set of them with new problems (including the hierarchy problem).

We really know nothing about quantum gravity, including whether the Planck scale is even the correct scale at which its effects become important.

20. **Eric**
March 20, 2013

Peter,

Perhaps I'm missing something, but is it really the case that the loop corrections to the Higgs mass are only a problem if there is a new physics at the GUT or Planck scale? The questions seems to me to be, what is the appropriate cutoff scale for these loop corrections? You seem to be arguing that the cutoff for these loop corrections should be the EW scale without requiring any new physics at this scale. Is it not true that the appropriate cutoff should be the next highest scale at which new physics appears? Even in the case that there were not new physics at the GUT or Planck scales, then wouldn't the corrections to the Higgs mass be expected to be infinite?

21. **dark**
March 20, 2013

"There may be some sort of "unification" of the SM forces (e.g. something that explains their relative coupling constants, and why SU(3), SU(2), U(1)). But there's no evidence for the specific proposal of putting these groups into a larger group like SU(5) or SO(1o) and then introducing some new Higgs fields to break the symmetry back down. This has never been the most compelling idea. Not only does it not solve the problems introduced by the Higgs, it adds a whole new set of them with new problems (including the hierarchy problem)."

Isn't SO(10) embedded the whole string theory TOE research program? If you don't think SO(10) broken by new Higgs field then in what sense is there any reason to expect stringy unification?

"We really know nothing about quantum gravity, including whether the Planck scale is even the correct scale at which its effects become important."

you have this link
"For an example of this, see Seiberg's Now What? talk at Aspen (Arkani-Hamed also spoke, with presumably a similar point of view, although the talk is not available)."

in Seiberg's article he confidently claims QG is well known thanks solely to string theory do you agree?

22. **King Ray**
March 20, 2013

One thing about GUTs is the large number of extra gauge bosons that are introduced. The Standard Model has 12 gauge bosons–8 gluons, the W+, the W-, the Z and the photon. If we go to SU(5), then we have 5^2-1=24 gauge bosons, double the number in the SM. SO(10) has 10*(10-1)/2=45 gauge bosons, nearly 4 times the number in the SM; SO(10) could also exist by itself as the gauge symmetry of nature without string theory, just as a GUT–all the quarks and leptons plus a sterile neutrino of a single family fit into its 16 rep. String theory once considered a gauge group of E8xE8; each E8 would have 248 gauge bosons, for a total of 496. Going from the SM to a large GUT is like generalizing a small town into New York City or Tokyo, almost literally. It seems a large price to pay to explain a handful of parameters. You have to introduce lots of extra Higgs fields to break everything down to the SM. It seems that the SM is simpler than a large GUT theory. Just out of curiosity, I once computed all the subalgebras of just one E8 using Dynkin diagram technology–it was amazing what was in there.

Also, if the strong and electroweak gauge symmetries arise for totally different reasons, there might be no reason that they should unify.

23. **dark**
March 20, 2013

@kingray

wouldn't those extra gauge bosons show up as new forces?

Also, if the strong and electroweak gauge symmetries arise for totally different reasons, there might be no reason that they should unify.

that's what ive wondered about – one of the arguments for SUSY is gauge coupling.

24. **King Ray**
March 20, 2013

dark, yes if you had a large GUT, the SM would be the tip of the iceberg. It would be a huge extrapolation. Where unification has worked before, it was more of a case of interpolation–Maxwell just added a term to the eponymous equations, Weinberg et al just added enough to get U(2).

25. **King Ray**
March 21, 2013

dark, if everything fit in one simple GUT group, then technically there would only be one force, with lots of gauge bosons. One force seems to be associated with each gauge group: SU(3) is the strong force, SU(2)xU(1) the electroweak force, and SL(2,C) is the gravitational force. Sometimes the electroweak force is split into the electromagnetic and weak forces, but they are tangled.

26. **Alex**
March 21, 2013

Hi,

I find these ideas quite compelling, and I have long thought that introducing a cutoff regulator might be the unnatural thing to do if there are no new high scale particles coupling to the higgs. However, that idea is not new at all, and I think you make it look too much like it had been unknown or suppressed by the SUSY inquisition somehow, while the truth is that people are working on it all the time.

However, I don't see how this (now also Lykken's) idea – that the right boundary conditions for the higgs potential are such that it should vanish somewhere, are any more experimentally testable or any less pseudoscientific than superstring theory, which at least establishes a framework to accomodate the other known particles and forces.

27. **BB**
March 21, 2013

Lykken seems to misunderstand the basics of QFT and RG.
M_ Planck (at last) provides a cutoff to his theory, and thus the relevant operators such as H^2 gets turned on with a coefficient of the size M_planck to some positive power.
In other words, the CFT is badly broken by gravity and cannot thus be invoked as a solution of the hierarchy problem unless some new separate symmetry forbids the relevant deformations. Take for instance QCD: it is natural because the dangerous strongly relevant operators, such as the quark masses, can be forbidden by the chiral symmetry, while the gauge interactions are only marginally relevant and give you the desired hierarchy via dimensional transmutation. SuSy and RS are other examples where gravity enters only through irrelevant deformations.

28. **Alex**
March 21, 2013

"Lykken seems to misunderstand the basics of QFT and RG.
M_ Planck (at last) provides a cutoff to his theory, and thus the relevant operators such as H^2 gets turned on with a coefficient of the size M_planck to some positive power."

Obviously, he must try to avoid this via some kind of asymptotic safety scenario. Do you think this is impossible? If so, it certainly isn't a "basic QFT" argument which says so.

29. **BB**
March 21, 2013

@ Alex,

If the CFT is exact (e.g. unbroken by gravity which becomes part of it) then it is not clear how to actually have any flow in the IR in the first place. Moreover, while the Higgs potential seems almost reaching a fixed point in the deep UV, the yukawas are not and they keep running spoiling the picture. Not to mention that the Higgs quartic is actually becoming negative below the Planck scale. About the ERG methods that people use to promote asymptotic safety scenario, it doesn't seems well suited for theories like gravity or YM where the operators that get omitted by the truncation of the effective action contain more and more derivatives.

30. **Alex**
March 21, 2013

BB,

The fact that a heavy top quark makes the quartic coupling go negative too early would indeed be a problem for the asymtotically safe gravity scenario I guess.

As far as the running of the Yukawas etc is concerned, yes, in DREG in the SM the quartic rises again due to the gauge couplings – also, not all gauge groups are asyptotically free.

Shaposhnikov and Wetterich argue that in their RG scheme, the combined running of SM+Gravity exhibits a modified Yukawa and gauge running. What the impact of the truncation is I am not competent at all, but I would hope that people at least have checked different ones to see what the impact is.

31. **dark**
March 21, 2013

@Also, if the strong and electroweak gauge symmetries arise for totally different reasons, there might be no reason that they should unify.

are there any theories that explore this? what would be the experimental predictions from this?

32. **Anon**
March 21, 2013

I never quite got the naturalness argument's reliance on the assumption that some arbitrary ratios be of order 1. Aren't there, in a suitable measure-theoretic sense, infinitely "more" large numbers than numbers of order one for nature to choose from, if nature is going to choose parameters randomly?

33. **Peter Woit**
March 21, 2013

Anon,

Only in the multiverse-mania world where if we don't know how to calculate something we can never understand it, so it's equally likely to be any number would your argument apply. More conventionally, the standard idea is that there are fundamental structural reasons why the numbers we calculate have roughly certain values. Even if you can't calculate things exactly, if you have a good understanding of what governs the result of the calculation, you expect to be able to do an order of magnitude estimate.

34. **Alex**
March 21, 2013

dark,

Type II Superstring Models with separate brane stacks for the sm gauge groups dont involve unification.

Predictions are hard… No proton decay maybe 🙂

35. **dark**
March 21, 2013

@ Alex says:

thx.

wouldn't the simplest model though be "the strong and electroweak gauge symmetries arise for totally different reasons, there might be no reason that they should unify" + 4D QFT + GR no SUSY + DM is sterile neutrino ?

36. **Mitchell Porter**
March 21, 2013

There is some discussion here of Lykken's talk… So far, I am confused about how all these things are supposed to relate: (1) the argument from DimReg that the SM is technically natural after all, (2) the idea that the SM is tuned to metastability by special UV boundary conditions, and (3) the use of radiative EWSB in a Higgs-portal model. Are they logically disconnected but thematically similar, or are there supposed to be logical connections?

37. **BB**
March 22, 2013

@ Mitchell Porter,

well, (1) is simply (very very) wrong. The hierarchy/naturalness problem has nothing to do with divergences and the regulator. The hierarchy problem is about the stability of hierarchically separated physical scales. BTW, even dim. reg. generates relevant dangerous operators via its finite mass terms.
(3) would be very important in order to generate an hierarchy (via dimensonal transmutation like in QCD) of scales but only if one can forbid the relevant deformations with a separate symmetry. Otherwise is simply plain tuning.

38. **Alex**
March 22, 2013

BB,

"The hierarchy problem is about the stability of hierarchically separated physical scales."

Surely the people proposing this scenario will agree that heavy particles coupling to the Higgs will immediately constitute a new high scale also in DREG, which will make a light higgs unnatural. That's just ordinary matching and running in DREG. They seem to disagree that the Planck scale counts as such a high scale in this scheme. In that sense DREG is important and crucially connected to the EW naturalness problem because it makes it possible to regulate QFT without having to introduce a huge scale invariance violation via a planck scale cutoff.

"BTW, even dim. reg. generates relevant dangerous operators via its finite mass terms."

May I ask what exactly are you alluding to here?

39. **BB**
March 23, 2013

Alex,

Above I was,alluding to the finite terms which are still quadratically sensitive to any new physics scale.

Even assuming that no fundamental scale, not even Planck, breaks the CFT in the UV, I don't think there is anything deep in dimreg. As there is nothing deep in dimreg for gauge theories, it just makes the calculations easier. Moreover, one could work with another arbitrary regulator and add a conformal compensator to the cutoff scale, rendering it as practical as the dimreg one.

40. **ohwilleke**
March 28, 2013

The really compelling argument that Lykken makes is that an ultraviolet boundary condition of zero for one or more quantities including Higgs boson mass or couplings that runs with energy scale can set the mass scale for the Standard Model at some suitable UV boundary. This argument is similar in form to the SUSY argument that the GUT scale is set by the point at which the running of the coupling constants cause them to converge (a scale that conveniently coincides with the energy scale when the patterns of CMB variation that we observe arose). SUSY proponents have made much of the fact that the running SM coupling constants don't converge at a single point, but a very subtle tweak to their running near their UV convergence chalked up to quantum gravity effects, for example, can easily resolve this issue which is essentially precisely what was done in the asymptotic safety based prediction of the Higgs boson mass.

The conclusion that the hierarchy problem is principally a problem of our flawed understanding of where fermion and boson masses in the SM come from, likewise makes sense. If some unknown mechanism drives the relative fundamental particle masses of the Standard Model, then the absolute masses flow naturally from any boundary condition for any particle mass, Higgs or otherwise, that can fix the mass scale of fundamental particle masses generally. Surely, the one thing almost everyone believes intuitively is that the many experimentally measured constants of the SM have some deeper relationship to each other. As much as anything we need "Within the Standard

Model" (WSM) theories at this point that motivate the why's of the moving parts in it, as much as we need "Beyond the Standard Model" (BSM) theories that give rise to "new physics." The only place empirical evidence is telling us that we need "new physics" is in the gravitational/cosmology sector.

Thus, the particularly SM extension that Lykken explores in his talk isn't terribly compelling. But, the notion that minimal modifications of the SM are the way to go, rather than one creating heaps of new particles and forces a la SUSY, is a good one. A better minimal SM modification than the one he discusses, for example, which still meets the test of adding particle/force content only to the extent absolutely necessary is the one discussed in "Gravitational origin of the weak interaction's chirality" by Stephon Alexander, Antonino Marciano, Lee Smolin (Submitted on 20 Dec 2012) http://arxiv.org/abs/1212.5246 which fits the dark matter sector and gravity into the same degrees of freedom as the electroweak SU(2)*U(1) sector, with electroweak constituting the left handed and gravity constituting the right handed counterparts of each other, leading to a "massless graviton coupled to an SU(2) triplet of chirally coupled Yang-Mills fields. . . . [with] a Dirac fermion [that] expresses itself as a chiral neutrino paired with a scalar field with the quantum numbers of the Higgs." In other words, in addition to a graviton and its intricate gravitational fields, it gives rise to a singlet sterile neutrino dark matter particle and a simple scalar inflaton and/or dark energy field.

41. **Mitchell Porter**
March 28, 2013

What does it mean for a Dirac fermion to "express itself" as a "chiral neutrino paired with a scalar field"??

Abel Prize to Pierre Deligne

March 20, 2013
Categories: Uncategorized

Just woke up to see that [this year's Abel Prize] has gone to algebraic geometer and number theorist Pierre Deligne, who is one of the truly great figures in 20th century mathematics. Deligne first became well-known for his proof of the Weil Conjectures in the 1970s, and has had a long and and very fruitful career since then, much of it spent at the Institute in Princeton. While working mainly in a part of mathematics far from physics, he also has had a long history of interactions with physicists, participating in the IAS year-long program on QFT, and most recently getting involved in current research on amplitudes. An excellent choice, congratulations to him!.

Update: See Tim Gowers's blog for more, including his talk presenting Deligne's work.

Comments

1. **cims conf**
   March 20, 2013

   The Courant Institute is hosting a one-day conf tomorrow for the Abel Prize.
   http://cims.nyu.edu/webapps/content/special/Abel_in_NY

   Courant's three Abel laureates will speak. Will you go?

2. **Peter Woit**
   March 20, 2013

   cims conf,

   That was a few weeks ago (Feb. 21, not March 21). Didn't hear about it until the day after. This week is Spring Break and everyone (except me…) has left town.

3. **El-Coco**
   March 20, 2013

   I was pretty surprised (as I am every year) that Terry Tao didn't win.

4. **Peter Woit**
   March 20, 2013

   El-coco,

   You have to realize that this prize was only started in 2002. In some sense they're now in the process of working their way through the list of all the great figures of 20th century mathematics who are still alive. It's now (intentionally I think) kind of an opposite of the Fields Medal, which was designed to reward and encourage young people still early in their careers.

   Looking at the list, no one younger than mid-sixties has gotten the Abel. If Tao remains in good health, I'd predict he'll sooner or later get the prize, but it may take a while…

5. **opp**
   March 20, 2013

   Jean Pierre Serre (2003), Michael Atiyah (2004), John Griggs Thompson (2008), John Milnor (2010) and now Pierre Deligne (2013) are all Fields Medalists, so one hypothesizes that the Abel is to encourage FM winners to keep working and not goof off in their old age?

6. **Peter Woit**
   March 20, 2013

   opp,

   This kind of gives some good data on what fraction of great mathematicians get the Fields Medal (5/13). Reasons for not getting the Fields Medal include doing ones best work when not young and having the misfortune to be of around the same age as several other great mathematicians (so missing out on the Field during the typical narrow time-window people are good candidates for it).

7. **jin**
   March 20, 2013

   Who got 2013 fundamental physics prize?

8. **tg**
   March 20, 2013

   Technically it's Tim Gowers. So it's Gowers' blog not Gower's blog.

9. **Peter Woit**
   March 20, 2013

   jin,
   Announcement coming up soon, see
   http://webcast.web.cern.ch/webcast/play.php?event=241643

   tg,

   Thanks, fixed.

10. **anon**
March 20, 2013

If you go to the blog, you will find that it is (correctly) Gowers's blog.

11. **MathPhys**
March 20, 2013

Deligne deserves *two* Abel prizes. One for his mathematics and one for his character. Very well deserved.

12. **Bob Jones**
March 20, 2013

"Deligne deserves two Abel prizes. One for his mathematics and one for his character."

Grothendieck would disagree with you.

13. **Ray**
March 20, 2013

@Peter It's funny how the year long course on QFT & Strings simply becomes course on QFT for you.

14. **Peter Woit**
March 21, 2013

anon,
OK, fixed the fix.

Ray,
http://www.math.ias.edu/qft
"A program in Quantum Field Theory for mathematicians was held at the Institute for Advanced study during the academic year 1996-97."

15. **Ray**
March 21, 2013

The AMS published book is called "Quantum Fields and Strings: A Course For Mathematicians". Let's not forget Eric d'Hoker's celebrated string theory course was a part of the year-long program as well.

16. **Bob Jones**
March 21, 2013

Gaitsgory's notes in that volume are very nice too.

17. **Peter Woit**
March 21, 2013

Bob Jones,

That may be a minority opinion… I don't think I've previously heard of anyone being able to understand Gaitsgory's notes. It's amusing that your and Ray's comments came in next to each other. I forget who I heard this from, and it's probably 2nd or 3rd hand and not accurate, but someone told me that Gaitsgory was originally the one writing up notes for d'Hoker's lectures. This led to a problem though when the lecturer himself could not understand the notes of his own lectures, so decided he had to write up his own. Again, take that story with a grain of salt, but still…

18. **Bob Jones**
March 21, 2013

To be honest, I should admit that I also find Gaitsgory's notes very hard to understand. But the topic of chiral algebras is something that I find very interesting, and I've spent a lot of time reading that section.

Fundamental Physics Prize to Polyakov

March 20, 2013
Categories: Uncategorized

As I predicted a few days ago, the string theorists in Princeton have made their choice for the $3 million dollar Fundamental Physics Prize: another Princeton string theorist, Alexander Polyakov. Evidently there's no official announcement, so Matt Strassler has retracted his original posting about this, now calling it an "unsubstantiated rumor", but someone at the ceremony e-mailed me with the news, so it is substantiated.

Earlier today I did watch the first part of the awards ceremony, although I had to leave to do something else before the Polyakov announcement. It was quite remarkable, designed to look very much like an Oscar ceremony, with Morgan Freeman as master of ceremonies, and the Laureates getting a big trophy to take home, as well as the $3 million check. The program was largely a string theory hype-fest, with the description of the accomplishments of the Laureates making no distinction at all between what was purely speculative and what wasn't. Viewers of the part I saw would have no idea that string theory is not tested, settled science.

Polyakov was one of the leading figures during the 1970s and early 1980s in the effort to understand the non-perturbative behavior of QFTs, especially the question of how confinement in QCD works. By the early 1980s, one of the most promising ideas about this was to try and find a string theory dual for QCD (and I spent quite a bit of time reading papers by Polyakov and collaborators about string theory and possible relations of it to gauge theory). As far as I can tell, Polyakov was never much of an enthusiast for 10d string theory unification, but kept arguing that what was interesting about string theory was the possible dual relationship to gauge theory, a point of view that has become the dominant one in recent years with the rise of AdS/CFT (which Polyakov played a role in).

For more about Polyakov's work, the best source is the man himself. He has written some wonderful articles about this history and the evolution of his thinking, see for instance here, here and here.

Anyway, congratulations to Polyakov, a great physicist who now won't be impoverished compared to his colleagues. Perhaps in future years the scope of the Fundamental Physics Prize can be widened, with string theorists at Harvard and Santa Barbara sharing in the loot.

Update: Here's a picture of Polyakov and the trophy you get with the $3 million. The money will provide him with "more freedom and opportunity to pursue future accomplishment."

Update: The official announcement is here.

Update: Nature has a report about the awards ceremony, as Internet billionaire throws lavish soiree for physicists.

Comments

1. **ketchup**
   March 20, 2013

   It sure is ironic that Matt Strassler posted an unsubstantiated rumor. I hope he did not subvert the scientific process of the awarding of the Fundamental Physics Prize.

2. **Bob Jones**
   March 20, 2013

   "The program was largely a string theory hype-fest, with the description of the accomplishments of the Laureates making no distinction at all between what was purely speculative and what wasn't. Viewers of the part I saw would have no idea that string theory is not tested, settled science."

   None of these prizes have been awarded specifically for work on the more speculative aspects of string theory. The accomplishments of all the FPP laureates are well established.

3. **Peter Woit**
   March 20, 2013

   Bob Jones,

   Did you watch the awards ceremony that I was describing?

4. **Bob Jones**
   March 20, 2013

   Peter,

   Yes, and I have to say that all the lights and goofy music made me cringe. I wish they would tone it down and hire someone more serious for their master of ceremonies. I think it's embarrassing to honor all these great scientists in such a silly way.

   As for their discussion of string theory, I'm not worried that the public will get the wrong impression of the subject. They did mention a couple of speculative topics in the ceremony, but most of what they were talking about were important and well established results of mathematical physics. I think it's great if the prize generates more widespread appreciation of this sort of work. In my experience, most laypeople are already rather suspicious of string theory, and they tend to under-appreciate its value as a tool in the study of quantum field theory and quantum gravity.

5. **Bob Jones**
   March 20, 2013

   Also, I'm not sure how much you watched, but most of the awards ceremony was about the discovery of the Higgs boson. They even got Charlie Rose to interview the experimentalists via Skype.

6. **MathPhys**
   March 21, 2013

   Is that photo of Polyakov with the trophy photoshopped? He looks ridiculous in that tux.

7. **Jin**

March 21, 2013

Where is planck mission announcement? A similar result?

8. **Abraham Smith**
   March 21, 2013

   All your jeering aside, that is a great-looking trophy. I assume it's meant to be a Hopf fibration?

   I also wonder what economists would think about a prize so large, which, unlike the Fields Medal and Wolf prize, could allow someone to retire early. Physics politics aside, would such a prize act as a motivation/inspiration, or will it cause mid-career experts to become less productive? (By comparison, the Abel prize is approximately 1 M$, but it so far is only for people who are already "well-settled" in their careers and lives. The Nobel is usually split several ways.)

9. **Bernhad**
   March 21, 2013

   Jin,

   watch it live here: http://spaceinvideos.esa.int/esalive

10. **jin**
    March 21, 2013

    Thanks, I finally found it. It is Simon White not Pavel Kroupa. It is unfair.

11. **Peter Woit**
    March 21, 2013

    Abraham Smith,

    Here's the official description:

    "All of the 2012 and 2013 Fundamental Physics Prize Foundation winners participated in the Ceremony and also received the special award trophy, a work of art created by Danish-Icelandic artist Olafur Eliasson.

    The Fundamental Physics Prize trophy is a silver sphere with a coiled vortex inside. The form is, in fact, a toroid, or doughnut shape, resulting from two sets of intertwining three-dimensional spirals. Found in nature, these spirals are seen in animal horns, nautilus shells, whirlpools, and even galaxies and black holes.

    The award is made of silver, an artistically significant material for a physics prize, as silver materialized from exploding stars."

12. **Cynthia Reid**
    March 21, 2013

    Bash the ceremony as you may (perhaps because you weren't invited?), it was refreshing and encouraging to see money, effort and recognition put into something beneficial in the sciences for a change, rather than sports and entertainment. Seeing physicists paying respect to the evening and the Foundation by honoring the black tie dress code was memorable. They ALL looked great.

13. **Cynthia Reid**
    March 21, 2013

    OK, that was snide and I apologize. But it was great to see physicists finally get some mainstream public recognition.

14. **Peter Woit**
    March 21, 2013

    Cynthia Reid,

    Just to be clear, I'm critical of the endless overhyping of string theory and the damage that has done to the subject, with a flood of $3 million checks and this kind of hype-filled production not helping the matter at all. I've no problem with the award to the LHC experimentalists, that's great.

    As for the ceremony, what I saw of it wasn't my kind of thing, so I wouldn't have enjoyed it even if invited (and some of those laureates up on stage didn't look like they were having a good time…) But I'm glad if others enjoyed it.

15. **Bernhard**
    March 21, 2013

    Peter,

    It is very unfortunate this is a stringy prize, blamed on the fact the committee chosen by Milner was simply too narrow. But, with the criteria being personal achievement in the field rather than being right about anything (which is the criteria the facto) , Polyakov more than meets it. With the same criteria for the experimentalists it is extremely hard to say the same. I think Polyakov deserves, in this respect, way way more than them.

16. **N.**
    March 21, 2013

    Circus Incredible.

17. **Peter Woit**
    March 21, 2013

    Bernhard,

    My impression was that for the experimental rewards, it was supposed to be clear that these were not really awards for personal achievement, but awards to one person meant to symbolize a larger group. I'd hope the awardees spent a lot of time at the ceremony acknowledging that. What happened to the supposed plans that these awardees had to distribute the money somehow to younger people who could use it? Are those going ahead, or are they going to just keep the money?

18. **Bernhard**
March 22, 2013

Peter,

This might be the underlying idea, but it is not what it is being explicitly said on the prize´s "awarded for" and therefore not the idea the general public is getting.

In any case, about what they will do with the cash, I don´t know. Last time I checked only Fabiola Gianotti explicitly said she wanted to "set up a fund with her share of the prize to support young physicists in the Atlas group". Incandela said something more vague about " find a way to put this to good use for the benefit of all who made this possible", but this was when the prize was announced and I did not hear about it anymore since then. I guess now that they have the money we should wait a few months to see what happens. The other spokespersons by the way, did not make the same claim, as far as I know.

19. **Yatima**
March 23, 2013

Somewhat earlier, Abraham Smith said:

> I also wonder what economists would think about a prize so large

Well, real economists (a set not fully compatible with the set of of clowns writing economics columns in the NYT) would just STFU because there is nothing to be said about this at all except that someone spends money on something that he evidently thinks is worth spending the money on. Taxpayers are apparently not involved, so no spending of other people's money either. Any further development is up to the receiver of the donation. Human Action? Hell yeah.

20. **Peter Woit**
March 23, 2013

Yatima,

Please, ideological discussions about economics are one of the few things stupider than discussions of the multiverse. No more, ever.

Yuri Milner has decided that he doesn't like the way capitalism has been working in this area, and so has decided to change the structure of rewards in physics research. He has every right to do this, those with less money have every right to either applaud or criticize his decision.

Planck Results

March 21, 2013
Categories: Uncategorized

The long awaited CMB results from the Planck satellite are now out, see here. A NASA press conference is about to start here.

You really should be reading about this somewhere else, from a much better informed blogger, someone expert in cosmology, which I very much am not. My non-expert impression is that, as rumored, the results are quite vanilla: 3.3 +/- .3 light neutrinos, so no evidence for a fourth neutrino, no significant non-gaussianity. No cosmic strings, see here, which has

> conclusion that there is at present no evidence for cosmic strings in the Planck nominal mission data.

In recent years multiverse mania has involved lots of claims to see evidence of other universes in earlier CMB data. Nothing about this in the Planck announcements I've seen, presumably they looked and didn't find anything, or maybe thought it wasn't even worth looking…

I'll try and make a list of informed commentary that I find, and keep a list here. Suggestions for additions are welcome.

Richard Easther live-blogged the announcement here.
Sesh Nadathur has comments here.
Ethan Siegel has a posting with background here.
The word from Resonaances is here.

Comments

1. **Ehud Schreiber**
   March 21, 2013

   A crucial quote from Easther's blog: "… there is no use of polarization data — this is being worked on, but it will make a big difference when it becomes available."

2. **Sesh**
   March 21, 2013

   Yes, there's not much in the data for particle physicists to get excited about, though there are a few interesting little anomalies, including the fact that the best-fit Hubble parameter value is getting further away from the one that is measured by the Hubble Space Telescope team.

3. **Peter Woit**
   March 21, 2013

   Thanks Sesh,
   Added a link to your blog to suggested reading about this.

4. **Filip**
   March 21, 2013

   I follow this blog and there is a nice story about the new findings (including a nice introduction on the matter, good for non experts like me).

5. **ohwilleke**
   March 21, 2013

   The result I read in paper sixteen was Neff=3.30 +/- 0.27 v. Neff 3.046 for the three Standard Model neutrinos. So, their result is a little less than one sigma from the Standard Model value. A four neutrino model would have an Neff of a bit more than 4.05, which is about three sigma from the measured value which is roughly a 99% exclusion and is a confirmation of the Standard Model.

   Planck also combines data from multiple sources puts a cap on the sum of three neutrino masses in a three Standard Model neutrino scenario of 0.24 eV (at 95% CI) with a best fit value of 0.06 eV. The floor from non-astronomy experiments is 0.06 eV in a normal neutrino mass hierachy (based on the difference between mass one and mass two, and between mass two and mass three which are both known to about two significant digits) and 0.1 eV in an inverted neutrino mass hierachy. In a normal neutrino mass hierarchy, this puts the mass of the electron neutrino at between 0 and 0.06 eV, with the low end preferred (I personally expect that an electron neutrino is significantly less than the mass difference between the first and second neutrino type of about 0.006 eV).

   Note that a particle that is in the hundreds or thousands of eVs would not count towards Neff because it is not light enough to be relativistic at 380,000 years after the Big Bang. So, it really only rules out a light sterile neutrino, rather than a heavy one. The LSND and MiniBooNE reactor anomalies have hinted at a possible fourth generation sterile-ish neutrino of about 1.3 eV +/- about 30%, so the Planck people did a study on the sum of mass limits if there were a disfavored four and not just three relativistic species and came up with a cap on sterile neutrino mass in that scenario of about 0.5 eV +/- 0.1 eV, which is about 2.5 sigma away from the value of the LSND/MiniBooNE anomaly estimates considering the combined uncertainties.

   LEP ruled out a fourth species of fertile neutrino of under 45 GeV, and I wouldn't be going out on a limb to say without actually doing the calculations that a fertile neutrino of 45 GeV to 63 GeV, if it existed, would have wildly thrown off all of the Higgs boson decay cross-sections observed (since a decay to a 45 GeV to 63 GeV neutrino-antineutrino pair from a 125.7 GeV Higgs boson would have been a strongly favored decay path if it existed) and is in fact therefore excluded by the lastest round of LHC data. The LEP data already excluded fertile neutrinos in the 6 GeV to 20 GeV mass range where there are contradictory direct dark matter detection experiment results at different experiments.

   But, a particle that we would normally call a sterile neutrino for other purposes in the Warm Dark Matter mass range of KeV or the Cold Dark Matter mass range of GeV to hundreds of GeV, or anything in between (including any of the possible direct dark matter detection signals or anything that would generate the Fermi line at 130 GeV), would not be a relativistic particle within the meaning of Neff which only counts particles that would move at relativistic speed given their masses at the relevant time.

6. **fuzzy**
   March 21, 2013

   Oh gosh, there is no evidence for sterile neutrinos! But … so many theorists have speculated on this possibility in the recent years, that at this point it is really necessary to ask oneself: What should we think of them?

Do you think that their job is anyway justified, like that of a lousy actor? They have been unlucky, as a poker gambler could be? Or, they tried to do something legitimate, as a lawyer who tries an ambiguous cause? Or, rather, we should say that they haven't been able to understand the nature, they failed their goal of scientists, and misled other colleagues?

What I think is evident, but I do not believe that their failure will leave any track in their record. Rather, their useless work will have served to increment the list of people who cited them: thus, in good company, they will be ready to do a new mistake. That's how the circus works!

7. **Peter Woit**
March 21, 2013

fuzzy,

You're rather harsh…. I rather think that one should feel the pain of those engaged in trying to find a viable theory that goes beyond the SM, since that has so far been impossible. It may be that the neutrino sector is our only hope left to see non-SM physics (i.e non SM with neutrino masses).

8. **useless**
March 21, 2013

Why is their work useless? It is easy to forget now that, back in the 1920s, to explain the continuous energy spectrum of beta decay electrons, Bohr, Kramers and Slater were willing to say energy is not conserved in individual decays, only statistically overall. Bohr was willing to consider that the electrostatic potential was not 1/r but 1/r^{some other power}. As for nuclei such as alpha particles, to explain the charge/mass ratio, Rutherford and many others believed in nuclear electrons.

9. **fuzzy**
March 21, 2013

Dear Pete, searching for theories is an important activity, sure, but one thing is to do this by sticking to good principles, one thing is to follow fashions. I know there are serious scientists who are trying their best, but also too many people working as such, that rather of being engaged in searching for a theory, are busy in searching an opportunity of publishing, citations, prestige and such. I also feel that neutrino physics is a promising avenue, first of all because there are useful measurements, but this does not mean that anything connected to neutrinos is a valid starting point. A good theorist (working in neutrino physics or elsewhere) should be able to know the difference between what is valid and what is of little value somewhat in advance, I feel.

Dear "useless" (dear colleague 🙂 thanks for the relevant feedback. In my view, one thing is to speak of solid experimental fact, another thing is to exaggerate the relevance of facts not fully assessed and partially contradictory among them. We knew that this type of sterile neutrinos that are being discussed were a muddy hypothesis, even without the Planck satellite (though it was possible to select the facts that one wanted to consider). Moreover, I remember that "not even wrong" is a sentence of a guy who was very prudent in publishing his ideas, including those on continuous beta spectrum, that have superseded the wrong hypotheses of Bohr. I think we should rather strive toward Pauli, Schrodinger, Feynman rather than exercising our unchained imagination or to show our talent in producing sexy names or forcing experimentalists to do some more measurement.

10. **Sesh**
March 22, 2013

I think the reason Richard Easther quoted Neff as 3.2 +/- 0.2 was because that was what Efstathiou said in the first media briefing. I don't know why that was – either both Easther and I misheard, or maybe Efstathiou just misremembered the numbers.

Incidentally, if combined with recent measurements of the deuterium fraction by Pettini and Cooke, the Planck constraint on Neff is even tighter: 3.02 +/- 0.27. I'm not sure how much these deuterium measurements are trusted though.

11. **physics_dude**
March 22, 2013

I'm a little confused, Peter. I've read about two anomalies that cannot be explained by the standard cosmological theory — anomalies that were first detected by WMAP, and have been detected again by PLANCK. Yet, both you and Jester dismiss it. How come?

12. **Peter Woit**
March 22, 2013

physics_dude,

I'm no expert, so relying on others. Jester and most of the other experts I've read about this don't seem to find these anomalies to be of great significance, and their arguments for why seem to me compelling. But, for more details, you need to ask them.

13. **Low Math, Meekly Interacting**
March 22, 2013

Perhaps I'm wrong, but my understanding now is that even if one could come up with a reasonably compelling explanation for the anomalies, their nature makes it impossible to rule out their being a statistical fluke…unless we can start observing other "universes".

I'm a bit chagrinned to learn that the Planck data are quite literally as good as it will likely get when it comes to observations of the CMB. That would seem to bode rather poorly for the prospect of getting greater cosmological insights to BSM physics any time soon.

14. **ohwilleke**
March 22, 2013

"I'm a bit chagrinned to learn that the Planck data are quite literally as good as it will likely get when it comes to observations of the CMB. That would seem to bode rather poorly for the prospect of getting greater cosmological insights to BSM physics any time soon."

We at least have weeks or months to absorb this round before the polarization data becomes available. This data, among other things places meaningful bounds on quantum gravity theories.

Also, while our observations of the CMB won't necessarily get all that much better, data from other sources should make it possible in combination with Planck data to leverage more out of it. For example, if we can determine the neutrino masses and hierarchy independently of Planck, this allows us to very finely constrain the properties of anything other than the three known active neutrinos that shows up in Neff.

Similarly, if we used extra solar system deep space probes to make paralax distance measurements of unparalleled accuracy, we could discern if any part of the estimate of the cosmological constant and Hubble parameters corrected for by Planck and attributed to factors like uncertainties in red shift measurements for Standard candle Ia's in fact has any possible other source.

15. **Alessandro Melchiorri**
March 22, 2013

A sterile neutrino is perfectly consistent with the Planck data.
If you actually look at the real numbers here: http://www.sciops.esa.int/SYS/WIKI/uploads/Planck_Public_PLA/3/32/Grid_limit95.pdf
you see that Planck alone gives Neff=4.5\pm1.4 at 95% c.l., while Planck+HST
3.7\pm0.5 at 95% c.l..
In few words is just when you combine with ACT data (the highL dataset) that you get a lower number.
The Planck collaboration preferred to be conservative and has put 3.3\pm 0.3 at 68% c.l. in the abstract, but there is plenty of space for a fourth sterile neutrino (that does not necessary gives you 4.05 ! it depends on how and when it decouples from the primordial plasma !) and it is actually suggested if you combine with HST. The value of the Hubble constant from CMB is indeed model independent and if leave the neutrino number as free you get a very good agreement with HST.

16. **Alessandro Melchiorri**
March 22, 2013

sorry I meant model dependent ! 🙂

17. **Richard**
March 23, 2013

> while our observations of the CMB won't necessarily get all that much better

I've read that a couple places, but I'm unclear on its meaning.

Is is that angular resolution and spectral resolution are at physical limits? If so, are these limits those of any detectors or are we talking cosmological fuzzing so detector physical limits are irrelevant? Are Earth orbit and launch-feasible detector size any constraint?

(I know the argument that accelerated expansion will leave our galaxy alone in a black universe, but in the meantime?)

Pointers appreciated. It's something of a novelty to be in possession of the best possible data.

18. **Sesh**
March 23, 2013

Richard,

For the temperature anisotropies, the limiting factor is cosmological. The process of recombination, which is what makes the universe transparent to photons, is not instantaneous. As a result, photon diffusion washes out anisotropies on small scales, which is what causes the exponential damping of the higher peaks in the power spectrum (this is called Silk damping or diffusion damping).

So getting better angular resolution doesn't help – at even smaller scales there essentially aren't any anisotropies there to measure. (Of course, this is a relative statement: if you could build a reference blackbody with an order of magnitude better temperature stability and put it into orbit, you could measure smaller temperature differences.)

In terms of polarisation measurements though, Planck isn't quite at the achievable limit. Here there are other experiments planned. And one could also measure distortions of the blackbody spectrum of the CMB, which provide a different probe of inflation, and for which the best experimental constraints available still come from COBE/FIRAS.

19. **Shantanu**
March 23, 2013

Peter one thing to note, although I maybe wrong on the finer details,
limit on energy scale of inflation is around the GUT value, so another
blow to GUT.

20. **David Nataf**
March 24, 2013

Observations of the CMB could very much improve with future experiments by better removal of background.

We are in the Galaxy, and as such an important souce of microwaves are …. the Galaxy… as well as background Galaxies giving sources everywhere.

Planck has to remove all of this:
http://2.bp.blogspot.com/_mazRoHLuLl0/TDIRdC2JvrI/AAAAAAAAAaw/Y8EIr8cUlco/s1600/plcmb.jpg

Before they can give a CMB map, which is extremely hard, and is not done perfectly at this point. They also need to account for the fact that foreground galaxy clusters lead to lensing of the CMB photons, as well as the Sunyaev Zeldovich effect, for which you will find separate papers already out, and countless more in the future.

There is a lot more work to do in CMB science.

21. **Low Math, Meekly Interacting**
March 24, 2013

Thanks for clarifying perspectives, David & Sesh!

22. **paddy**
March 24, 2013

Thanks to Peter, Sesh, David, and all the knowledgeable commentators' for their blather and links to even more blather. And note…blather is NOT meant as a derogatory term.

23. **Shantanu**
April 2, 2013

FYI: Planck symposium webcast live

http://www.sciops.esa.int/index.php?project=PLANCK&page=47_ESLAB

24. **Gian-Luca**
April 7, 2013

Peter,

do the new results from the Planck satellite shed any light on whether the cosmological constant decreases with time? You discussed this topic in some of your previous blog posts. The issue would be of interest to many!

Gian-Luca

25. **Peter Woit**
April 7, 2013

Gian-Luca,

Not sure what you're referring to, I'm not very well-informed about whether there's evidence for a time-varying CC. I don't see any reason Planck would have results about this, and haven't heard anything about it. If they did have such results, I assume they would be well-publicized.

The Existence of Nothing

March 22, 2013
Categories: Multiverse Mania

The sold-out "debate" held Wednesday night here in New York is now available for viewing online, see here. I just watched most of it, and one of many things I couldn't figure out is what if any propositions were being debated. Lots and lots of the usual multiverse mania, and endless flights of speculative fancy and empty, meaningless argumentation. I'd guess this left much of the audience thinking there's not much difference between what well-known scientists do and what stoned college students do when they're talking late at night. Amongst all this, a few topics stood out as completely missing:

- Any significant discussion of what our best theories really say about the vacuum. There are all sorts of interesting things you could say about the vacuum of the Standard Model QFT, but no one seemed interested in this topic.
- Any legitimate connection to experimental test. The Planck results to be released the next day were referred to by Eva Silverstein, who claimed that CMB observations could test the sort of thing she was talking about. In actuality, there seems to be zero prospect that Planck or any other such observations will test the speculative ideas about string cosmology she was referring to.
- Any indication that the multiverse and string theory are not settled science that all physicists now agree on. The problems with this picture of the world went completely unmentioned as far as I could tell.
- Any mention of the disinvited David Albert.

Comments

1. **P**
   March 22, 2013

   "In actuality, there seems to be zero prospect that Planck or any other such observations will test the speculative ideas about string cosmology she was referring to."

   This isn't true. Like anything, it is model dependent, but there are string cosmology models which predict a large scalar to tensor ratio, e.g. axion monodromy. I believe the predictions for r in these models are large enough that they could be ruled out by Planck.

2. **Peter Woit**
   March 22, 2013

   P,

   I don't doubt that you can come up with "string cosmology" models that "predict" just about anything imaginable. People are free to do this and go on stage and tell the public about how this is an inspirational example of science in action, but I think this is quite misleading.

3. **P**
   March 22, 2013

   Peter,

   This is precisely how model building – in QFT or in string theory – works; there are many choices, and indeed a wide variety of things can be predicted (though not "anything imaginable", as you say). This is true in both particle physics and cosmology.

   Inflation needs a scalar field, and certain types of scalar fields appear in string theory. In considering one as a candidate inflaton, predictions are made and these models can be ruled out as experiments become more precise. This could be true of axion monodromy at Planck. Such ideas are perfectly honest and are how science / model building works, in both QFT and string theory.

   Cheers,
   P

4. **pred**
   March 22, 2013

   I suppose the key criterion with the notion of 'predictions' is *falsifiable* tests. What falsifiable statements do the predictions (e.g. of string cosmology) make?

5. **Peter Woit**
   March 22, 2013

   pred,

   The question of whether string theory satisfies the criterion to be science has been debated endlessly here (and elsewhere). If you look into this, you'll see that falsifiability by itself cannot be such a criterion. For a trivial example analogous to P's axion monodromy, I can tell you that I have a (rather complicated…) model of the early universe, which predicts that Steven Hawking's initials will be visible in the polarization data inside the large cold spot that Planck has found. This is a testable prediction (which will be falsified next year…), but if I were to spend my time coming up with a detailed model designed to do this and studying it, you might sensibly consider what I'm doing to not be science.

6. **pred**
   March 22, 2013

   I saw the SH initials (was it the WMAP data?). Most 'predictions' are actually retrofits to data, i.e. you tell me the answer and I'll demonstrate why my theory fits the facts perfectly. It gets embarrassing if the facts change, because someone discovered a calibration error etc., but most retrofit theories are able to backtrack on things like that. There has to be more than one falsifiable *pre*diction, and they all have to come from the same model (no fiddling with endless adjustable parameters).

7. **Peter Woit**
   March 22, 2013

   pred,

The SH was found in WMAP I think.

Yes, typically people try and justify their models by fitting them to facts. The problem I'm describing is a bit different though. The question of whether something is science has to do with whether it is giving non-trivial, reliable information about the physical world. Being able to make accurate falsifiable predictions is one characteristic of successful science. But my point is that just because you make a falsifiable prediction, that doesn't by itself mean you are doing science. Astrologers make falsifiable predictions all the time…

8. **Paul Wraight**
   March 22, 2013

   I thought Luke Barnes comments in his blog http://letterstonature.wordpress.com/2013/02/18/why-science-cannot-explain-why-anything-exists/ conveyed a lot of sense.

9. **Z**
   March 22, 2013

   I wish they had discussed the question of if Mathematics exists and the notion of logical consistency (do axioms exist? What about incompleteness theorems?), since presumably, any deep fundamental theory must ultimately arise from some mathematical structure Some of these issues go back to Plato's time, but I think they still have relevance to these musings.

10. **cormac**
    March 22, 2013

    There was a series of seminars on the results in Cambridge today. Very impressive; it's now SWH, by the way!

11. **cormac**
    March 23, 2013

    Sorry, that was a bit obscure. What I mean is this: in relation to the comments above on the initials SH being imprinted on the cosmic microwave background, as measured by WMAP, Paul Shellard joked at a seminar yesterday that one can now read the initials SWH in the PLANCK spectrum. It was a fun way to finish his lecture, but I couldn't see really it!

12. **Maciej**
    March 23, 2013

    Peter,

    I think you are saying that falsifiability is not a sufficient criterium to say that a theory is scientific. Clearly. However it is essential and ST does not even fullfill this.

    Maciej.

13. **Peter Woit**
    March 23, 2013

    Maciej,

    Yes. But the question of string theory itself is complicated by not even knowing what the theory is. The multiverse, as being promoted now at events like this one, is the ultimate in unfalsifiabilty.

14. **Maciej**
    March 23, 2013

    I see.

    Well in that case (i.e. that we do not even know what ST is) it is even easier to claim that ST is not scientific since we don't know what we are talking about.

15. **dark**
    March 23, 2013

    Is the multiverse a way of avoiding the conclusion that string/m-theory predicts the wrong dimensionality (4 vs 10/11)?

16. **Peter Woit**
    March 23, 2013

    dark,
    String/m-theory doesn't predict any particular observable number of dimensions. It doesn't really predict anything, and the multiverse pseudo-science is just being used as a way to avoid doing honest science and drawing the standard conclusion that a theory that predicts nothing has to be abandoned.

17. **dark**
    March 23, 2013

    doesn't gauge anomaly cancellations require 26 in bosonic, 10 or 11 in string/M-theory?
    hence, a "prediction" if wrong.

18. **Peter Woit**
    March 23, 2013

    dark,
    The standard story is that you can use compactification or "branes" to get solutions to the 10/11 d theory that have other numbers of large dimensions at observable energies (e.g. 4).

    Perhaps if string/M-theory were more precisely defined, you could simply show it made an incorrect prediction. The current state of the art is that with what is known about the possible definition of such a theory, you can get pretty much anything you want. String theorists used to argue that once string theory was better understood, one would find that it had certain specific solutions and you would get predictions. The newer ideology is that once string theory is better understood, it will have a landscape of solutions and not predict anything.

This is getting off topic, and it's by now a very old story, enough about this.

19. **Nick M.**
March 24, 2013

Peter Woit *says:*

> "But the question of string theory itself is complicated by not even knowing what the theory is."

Thank You so much, Peter, for this comment. When I was an undergraduate at UC Berkeley in the early to mid 2000's, I remember asking a GSI friend of mine, who was working on String/M-Theory at the time for his doctoral thesis, what he knew of M-Theory, and could he tell me more of what it was all about. He put his index finger to his lips, and whispered in a quite and a somewhat whimsical voice, "Shhhhhh…, the big secret is, is that there really is *no* M-Theory". Although he tried to explain what he meant by this at the time, it's only over the intervening years that I have come to appreciate what exactly it was that he was trying to get across.

Now per your admonishment, I will say no more on this topic.

20. **Tammie de Cortez Haynes**
March 24, 2013

Dear Dr. Woit

Your account of this episode was most depressing.
Pardon me while I make it more so.

You thought the audience was left thinking there's not much difference between what well-known scientists do and what stoned college students do.
I doubt that.

I suspect the audience was left admiring their own superiority, as among the few smart enough to share in the enlightenment of our greatest scientists.

21. **socrates**
March 24, 2013

Do people actually feel superior for being in the audience of anything? That's stoned enough for me.

22. **P**
March 24, 2013

pred,

Sorry for the delay. Busy weekend.

The falsifiable test for axion monodromy is a higher than usual scalar to tensor ratio, usually written as r in the literature. I forget the precise numbers, but they're in the literature and I can look them up if you'd like.

Make no mistake about it: there are experiments to measure r, axion monodromy predicts that it is larger than many models (including slow roll, I believe), and this is absolutely falsifiable as experiments bound the value.

Cheers,
P

23. **Bernhard**
March 25, 2013

P,

Is this prediction unique to string cosmology models?

24. **mat noir**
March 25, 2013

Was it Gellman who said: Given 3 variables, I can make an elephant. With four, I can make it walk.

25. **aleph/eleph**
March 25, 2013

John von Neumann apparently, quoted from Enrico Fermi, and it was 4 and 5, not 3 and 4:
"I remember my friend Johnny von Neumann used to say, 'with four parameters I can fit an elephant and with five I can make him wiggle his trunk."

http://mahalanobis.twoday.net/stories/264091/

But Dorothy Parker said it better
"I like to have a martini,
Two at the very most.
After three I'm under the table,
after four I'm under my host."

http://www.goodreads.com/author/quotes/24956.Dorothy_Parker

26. **P**
March 25, 2013

Bernhard,

No. I believe, in fact, that before the axion monodromy mechanism was understood people thought one couldn't get large scalar to tensor ratios out of string cosmology.

27. **piscator**
March 27, 2013

P:

I think many people still think that.

28. **P**
March 27, 2013

Piscator:

You may be right. I'm not enough of a string cosmology expert to know if or why there is a debate about axion monodromy as a model.

29. **Randioactive**
March 27, 2013

What of AMS-02?

30. **Jim Akerlund**
March 27, 2013

This is off topic, but FQXi has just put up a new contest for 2013 called "It from Bit or Bit from It". It runs until 6/28/13.

31. **emile**
March 28, 2013

@Randioactive

Please find details and poster related to the SPECIAL CERN-EP Seminar on Wednesday 3rd of April 2013 in Main Auditorium (500/1-001) at 17h00:

"Recent results from the AMS experiment"
by Prof. Samuel TING (Massachusetts Inst. Of Technology (US))

32. **Beelzebud**
March 28, 2013

I tried to watch that debate. Watching educated people sit around debating on the definition of nothing is as fun as watching paint dry. As a layman I just don't quite see what the entire topic has to do with science. I can see why philosophers would discuss it, but what has it got to do with science?

The only insightful part of it was when the gentleman with the fabulously colored suit explained his model about branching universes, but even then, how does science test something like that?

33. **Raizonator**
March 30, 2013

I watched the debate and there is one nagging question that came to my mind:
When we talk about our Universe, we mean the ONE (classical) Universe we observe. O.K. But if we extrapolate back in time until the Universe is as big as an elementary particle and QFT "kicks in" then one is forced to speak of a universe being in a "one particle state" (for example). That is the number of universes we are talking about gets uncertain (Heisenberg).
So with a certain probability it could also be in a zero-particle state, i.e. in a state of "nothingness" if one likes. But what does that mean anyway if nobody (i.e. no classical observer) can be around to do the measurement and find out how many quantum universes there are? That's where things really get weird, I think, and where I can't trust extrapolations via (conventional) QFT any more.
Are these thoughts too naive ?

34. **MS**
April 1, 2013

@Beelzebud: It is a business (not science) thing.

Are they wrong? US research funding

March 28, 2013
Categories: Uncategorized

It seems to be part of the job description of anyone in the sciences to periodically complain that scientific research funding is insufficient, with the situation going from bad to worse. For some recent examples, see this from Bruce Alberts, the Editor-in-Chief of Science, and this endorsement from Professor Matt Strassler.

In the contrarian spirit of this blog, I want to suggest that the situation is actually quite a bit more complicated, and the story of research funding is not completely a one-sided one of the oppression and impoverishment of scientists. Also in the spirit of this blog, I want to avoid topics I don't know much about, which in this case includes the vast majority of scientific research and how it is funded, especially outside the US. The biggest component of R&D funding in the US is the military, and I have no idea what this money is going towards and whether it is being well-spent. I've also heard that there are increasingly vast sums being spent by the US on classified research, not necessarily accounted for and showing up in obvious places in the budget, but I have not idea whether this is even true or what the size of this is. While ignorant about what military R&D spending is going to, I confess to a general prejudice that it seems to me to be huge and if I knew more I'd probably be strongly in favor of there being less of it.

The next biggest component of R&D spending is biomedical, and again, I'm woefully ignorant. Unlike spending money to find better ways to kill people, biomedical research is inherently something worthwhile, so more of it undoubtedly is better. But whether it is now being spent well, or whether taking away from some other priority to spend more in this area would be a good idea, I haven't a clue.

On overall US federal spending levels, Alberts compares a level of .87% of GDP in 2013 to a level of 1.25% of GDP in 1985. He's getting his data from here, but those numbers do tell a more complicated story. Measured in constant (2012) dollars, non-defense R&D/year went from $32 billion in 1985 to a maximum of $67 billion in 2004, and has been relatively flat since then, with $64 billion projected for 2013. Defense R&D went from $65 billion in 1984 to a maximum of $90.5 billion in 2008, has dropped significantly in recent years to $76 billion for 2013. Another set of overall numbers from the same source are for the NSF budget, which went from $4.6 billion in 1998 to $7.25 billion in 2013.

For a while on this blog I used to try and keep track of the US budget situation and periodically report on it, at least the numbers I could find and understand for math and physics. The most important thing to say about the situation of recent years is that the US federal budget process has completely broken down. Budgets have gone from being passed late to never, with government spending now allocated by some baffling system of continuing resolutions and last-minute "cliffs". There appears to be nothing anymore like a sensible process for making future plans and sticking to them. Those responsible for managing research facilities are not only in the dark about how much money they'll have to spend over the next few years but sometimes don't know how much they'll have to spend next month or next week. No matter what you think spending priorities should be, trying to run organizations this way is completely nuts and a disgrace to the country.

Getting close to fields I do know something about, here are some other numbers (also 2012 dollars): NSF yearly spending on math and physical sciences has gone from $924 million in 1998 to $1,323 million in 2013. DOE Office of Science has gone from $3.3 billion in 1997 (including $895 million for HEP) to $4.5 billion in 2013 (including $764 million for HEP).

Theoretical physics is very much small potatoes on the scale of science funding in general. For FY2012 the DOE spent $67 million on theoretical and computational physics, the NSF $13.6 million (+6 million for Physics Frontier Centers), up from $11.7 million (+6.3 for Physics Frontier Centers) in FY2008 (real, not inflation adjusted dollars). Increasingly, large amounts of funding are coming from the private sector. The Simons Foundation spent $40 million on grants for math and physics in 2011. The Perimeter Institute has gotten $150 million or so from Mike Lazaridis over the years, and the Templeton Foundation has recently provided $2 million to Perimeter, after $8 million to FQXi, and millions more in other grants such as $2 million for the philosophy of cosmology. Yuri Milner has in the past few months handed out about $37 million in checks to physicists, with one goal that of supporting their research.

The overall pattern seems to be that science in general has not been doing that badly, although HEP funding in the US has been cut significantly, as the US lost leadership in HEP to CERN with the LHC becoming the focus of attention. US experimental HEP faces huge challenges in the future, but they have more to do with the SSC debacle of 20 years ago and the lack of a compelling technological way forward to higher energies than with general federal science budget cutting. Funding for theory from conventional government sources has been fairly flat, with new sources of private funding starting to have a major impact.

As for the work conditions of US academics, Matt sees the situation as:

> Whereas before the year 2000 it was easy for U.S. universities to attract the best in the world to teach and do research at their institutions, and to train the next generation of American scientists, the brain drain since that time has been awful.

On the other hand, my own experience at Columbia (a wealthy private institution) and in mathematics has been that the post-2000 period has been one where the US in general and Columbia in particular have done very well in competing for talent. While the middle class in the US has been in decline, top-flight US academics have seen significant salary increases. The AMS compiles yearly numbers for salaries (see here), which show the mean academic-year salary for a mathematics full-professor to be $127,674 at large public research universities, $148,074 at large private research universities. Back in 1999 the numbers were $85,571 (public) and $95,977 (private). Comparing to median US incomes, the ratio has increased from 4.26 to 4.73 during this time in the public university case, 4.77 to 5.49 in the private university case. At the top of the profession, average salaries for full professors at Harvard (in all fields) were $122,100 in 1998-9 (6.07 times US median), $198,400 in 2011-12 (7.36 times US median). The general pattern is that of the rest of US society, with the rich getting richer, and staying very much competitive for talent with the rest of the world.

As usual, informed and on-topic comments are welcome. If you just want to rant though about the evils of government spending, or go on about how in a just society scientists would get lots more money, please do it somewhere else.

Comments

1. **X**
   March 28, 2013

   Firstly, the very topmost universities such as the Ivies will be the last to depopulate. Secondly, much of the impoverishment of the university system is being done at the state level and is destroying state public university systems. Thirdly, the defunding of the system is done by removing the bottom tiers of the system: low pay and few hires for assistant professors, sending young people to the eternal damnation of sub-professor-level migrant academic and lecturer jobs. This does not cause pay for full professors to decline. (Learn more about such trends by looking at "wage stickiness" in the econ lit.)

2. **Peter Woit**
   March 28, 2013

   X,

   I don't think what is going on is "wage stickiness", since what's happening is not salaries at the top of the scale going down more slowly, but salaries at the top of the scale going up. You see this throughout the US economy. In the universities, another place you see it is with administrative salaries which have been growing even faster than star professor salaries (Columbia's president makes nearly $2 million/year). This isn't only the Ivies: Berkeley may have massive budget problems, but it still came up with a big increase in its chancellor's salary for their new chancellor.

The phenomenon of exploitation by universities of the labor pool that they themselves flooded (by dramatically increasing the size of Ph.D. programs) is by now a very old one, and the destruction of wages at the bottom of the scale has coincided with large pay increases at the top of the scale.

3. **X**
   March 28, 2013

   Hi Peter,

   I wonder if in a twisted sense, those crazy salaries for top bureaucrats might be worth it. Since a main job of top bureaucrats is to use political savvy and shenanigans to funnel money from various sources into the university, a university president who gets paid 2M$/yr but pulls down 100M$/yr in donations is a good investment. Of course, then you have to wonder about the marginal costs and whether a 500k$/yr president would pull down the same donations anyway, but the optimal salary seems non-obvious to me.

4. **Yatima**
   March 28, 2013

   "I confess to a general prejudice that it seems to me to be huge and if I knew more I'd probably be strongly in favor of there being less of it."

   Woah I got the impression of a witness having to make a statement in front of some Popular Court on his current view on whether the governement is, in his opinion, indeed working for the Greater Good of The People. Or not.

   Luckily the flames of passion are being fanned in the next paragraph.

5. **Peter Woit**
   March 28, 2013

   X,

   That's certainly the argument being made to justify huge increases at the top of the pay scale, in all industries. The guy running Lehman was using it to justify his $22 million payday right up to the moment they went bankrupt.

6. **srp**
   March 28, 2013

   This post opens up a huge can of worms, many of which you probably will not want crawling around on this nice, clean science blog. For example, your distaste for military R&D ("better ways to kill people") could easily lead off into a complex discussion about pacifism, whether it's better to kill people using worse ways, US foreign policy, etc. I'm betting that comments about this aspect of the post would get snipped as being "off-topic" even though, as the lawyers would say, you opened the door to these issues. For the record, though, that kind of statement about military R&D does not increase the credibility of the rest of the post. End of on-topic though off-topic comment.

   In general, you are right that the US government and private donors have not been chary in supporting science of all kinds over the last decade. The increasing difficulty in winning grants is more the result of the well-known "sorcerer's apprentice" effect caused by the requirement in most grants that new PhDs be trained. (And of course those PhD students provide the cheap workforce for the senior investigators.) The more grants given, the more PhDs spawned later, and the more competition for whatever level of funding has then been appropriated. With this system, no level of spending will ever recreate a situation where a high percentage of grant applications are funded.

   In my opinion, the reason why this training policy is maintained is that the government and science bureaucracy prefers to have a large and lower-paid STEM workforce. They are not entirely irrational in this regard; President Obama's former economic advisor Austen Goolsbee has published econometric studies showing that increases in government R&D spending tend to mostly raise the wages and incomes of researchers rather than producing greater quantities of research. The training requirement partly neutralizes this effect, which would otherwise be even more severe. Lots of cheaper researchers helps the government get more research done for less money, gross of training costs; the interesting question is whether using initially low-skill PhD students as the front-line workforce in labs is really the most efficient way to get the research done.

7. **Peter Woit**
   March 28, 2013

   srp,

   My disclaimer that I have little idea what military R&D spending goes for was meant quite sincerely and intended to provide appropriate background for any of my comments on the subject. But, yes, I'd rather not host or moderate a "military spending, good or bad?" argument here.

8. **Peter Woit**
   March 28, 2013

   As for the "efficiency" argument for the desirability of a massive low-wage work force, surely the most efficient work-force structure would be slave labor, with a small overclass keeping armies of needed scientific researchers chained in labor camps, kept in line by brutal physical methods, and fed just enough to keep them working long hours optimally.

   While this would get the most work done at the cheapest price, perhaps there are other desirable goals in a society more important than extracting from human beings as much as possible at the lowest possible cost.

9. **Felipe Pait**
   March 28, 2013

   I am not very knowledgeable but I share your view of military research. I worked on military-funded R&D for a short while, at what I judge to be a top-notch operation, and my impression was that the funds were not spent very efficiently – the US was not getting a lot of national security per dollar spent. Classified research appears to be less effective than the less secretive components. There is a lot of waste on questionable projects. Rerouting military funds to science research or to engineering development would, for the admittedly little I know, be good for the country.

   On the other hand, perhaps we should not be comparing the effectiveness of military research with science research. National security by brute force is massively expensive, and painful. To the extent that military research can make defense operations even a little more effective or deadly, or prevent wars, it is justifiable in its own terms.

10. **Bob Levine**
March 28, 2013

@srp

There's a kind of no-win situation that seems to have come about in the sciences. Yes, the induction of young researchers with bleak prospects into these fields is from one angle cynical, callous and exploitive. But on the other hand, we know—the relatively few Max Planck examples notwithstanding—that it's the younger investigators who are likely to make the major breakthroughs. A field which is aging demographically is in danger of withering; older established scientists are unlikely to make the kind of huge conceptual leaps that the likes of Einstein, Feynman, Dyson and von Neumann achieved. If you don't actively recruit the best of the youngest, your field has a bleak future indeed. So the idea of having Ph.D. training as a component of grants isn't bad in itself; the difficulty is at the other end. A lot of the blame has to go to the universities themselves, and university administrations in particular, for the gradual elimination of a permanent, reasonably well-compensated professoriate as its core faculty, with jobs waiting up the line for those new Ph.D.s, in favor the disgraceful 'Kelly prof', migrant-labor model that academic managers have been doing their best to create.

11. **lun**
March 28, 2013

the administration overhead is definitely a large part of the problem.

The processes you describe is a good experimental refutation that the "market", in this case, the academic job market, is a "statistical system of many equal players" rather than a power structure of generally hyerarchical power relations. The problem is, in the latter case a decrease of funding just makes the system more corrupt instead of leaner and higher average quality.

So, paradoxically, the author of "Am I wrong?" is correct, but the "anonymous scientist" commenting on page 2 (a very thoughtful comment, starting with "Yes you are wrong") is correct as well. More funding might make the system less corrupt and bloated, and less funding will worsen the "politics".

12. **Peter Woit**
March 28, 2013

Bob Levine,

One should keep in mind that typically it's not faceless administrators that want to expand Ph.D. programs, but department faculty. And when faculty go to the administration to request lower teaching loads (so they can do more research), and are told, "OK, you can teach less and we'll hire some adjuncts to cover the courses", do you think faculty say no to this?

13. **Bob Levine**
March 28, 2013

Peter—

"when faculty go to the administration to request lower teaching loads (so they can do more research), and are told, "OK, you can teach less and we'll hire some adjuncts to cover the courses", do you think faculty say no to this?"

There's no denying that senior faculty are often complicit in perpetuating the system as it's developed during the past 30 years. But what I've seen at my own university is a disappearance of the tenure-track jobs themselves, as faculty retire and their lines are frozen, or cancelled, or merged together. And *that* initiative is coming from administrative management policy, whose creators have figured out that if you can strip as many benefits as possible from the majority of your academic work force, you'll save enormous portions of your budget. At public institutions such as the one I teach at, this makes senior administrators look good to the legislators who supply our funding (although way, way less than they used to), and, maybe even more important, it increases the value of those administrators in the eyes of other institutions looking to hire cost-cutting, efficient managers in what now seems to be the private sector business model that many (most?) academic institutions seem anxious to follow. The jobs that are sacrificed to make our provosts, chancellors and executive deans look look like CEO material and keep their bosses in the Statehouse happy are the jobs that our newly trained Ph.D.s aren't ever going to get….

14. **Peter Orland**
March 28, 2013

Peter and Bob Levine,

Both of you are right. As more adjuncts replace faculty, less research is done, resulting in fewer grants, hence less need to hire full-time faculty, and so on… There are some enlightened deans/provosts/presidents who see that flushing faculty lines down the toilet leads to for-profit U. of Phoenix-style degree mills.

I sometimes vainly try to explain to non-academic friends that research-oriented faculty are the best people to learn from (that was my experience anyway. Give me a scowling teacher, who would rather be in the lab or busy doing calculations. He or she can answer questions, not BS through them).

15. **Ray**
March 28, 2013

Once for amusement I compiled a list of quotations of scientists over the last 50 years. They always said, funding is being cut and the job market is getting worse. I guess whining is a permanent feature of being an academic.

16. **Lowell**
March 29, 2013

Perhaps the production of PhD students is too large and not determined by their quality.

At a place where I once taught, the number of entering graduate students was determined by the number of graduate student TA's needed to teach undergraduate labs and grade papers. It was not determined by the number of outstanding students that could be attracted to enter the program.

17. **dark**
March 29, 2013

What role do you think string theory and current LHC results has had in attracting funding, research, academic positions, and graduate student applications/interest?

18. **Z**
March 29, 2013

David Helfand had a similarly austere editorial on the state of science funding last month, as it relates to Astronomy: https://aas.org/posts/news/2013/02/presidents-column

and broaches the idea that, perhaps, there is some birth control necessary at an early stage.

19. **Chris Oakley**
March 29, 2013

Sorry – but I really do not accept that training a large number of Ph.D.'s when there is little possibility of them getting an academic job is a valid one. Only about one in five of my peer group (Oxford theoretical particle physics circa 1984) got a commensurate academic job, and I was not one of them. But even if I had known that I was not going to be able to continue I would still gladly have entered the Ph.D. (or D.Phil. as they call it in Oxford) program simply for the privilege of doing it. In the U.K. there is a also a persuasive demographic reason for doing more degrees. When I took my degree, 5% of eligible students went to university. Now it is more like 40%. Having a masters or Ph.D. is thus a way of distinguishing one from the crowd when applying for (non-academic) jobs.

20. **chimpanzee**
March 29, 2013

"Better to have Brain Drain, rather than Brain-in-the-Drain"
— Indian prof, Re: India talent studying abroad in USA

There is a Reverse effect in progress (as pointed out by M. Strassler), talent is going away from USA. I went to famous HS (University High on UIUC campus)

[produced 3 Nobelists (Philip Anderson/Physics..Higgs mechanism, Hamilton Smith/Medicine..top-gun hired by Dr Craig Venter for JCVI "1st organism designed by computer", James Tobin/Economics. Other notable alumni are Bill Bardeen/Fermilab (son of John Bardeen, Nobelist), Shamit Kachru/Stanford]

which has the similar formula of high-end Academia: recruit top-students with top-faculty..GOOD THINGS will happen. 1 of my HS classmates (Dr Dave Albin, NREL/National Renewable Energy Lab, leading PV/Photovoltaic researcher) told me something alarming:

"I'm losing 2 of my staff to a Chinese solar company, who offerred them $300K/year salary"

This is American STEM talent being drawn away to foreign shores. China leads in Wind/Solar, the Obama's so-called "next Sputnik moment". The Solyndra failure (DoE, Dr Stephen Chu/Stanford & Dr Stephen Koonin/Caltech/Physics/Provost & BP Chief Scientist)..500 million wasted, since they couldn't compete with market flooded with low-cost Chinese solar cells. This is a sign of "too little too late". That OMB (Office Management & Budget) email that got leaked

"This [Solyndra] loan is not ready for prime time"

indicates they were RUSHING THINGS. Another sign of "too little too late". A123 (Battery Tech) failed..bought by Chinese company (250 million loan wasted)

The KEY to Economic Recovery is a STEM based stimulus package. Not only Alternative Energy (due to politicization of Global Warming hype/hoax), but Biotech, Nano Tech, Computer Tech. Apple Computer is the pinnacle of Tech which found mass-market monetization (#1 market capitalized company, exceeding "old Oil" Exxon Mobil)

It's a well known fact that Curiosity/Pure Research has long-term mass-market potential. The CD/DVD was a mass-market entertainment device, that was rooted in Theoretical Physics

"Einstein & Botha realized that photons in the same state..[laser]"
— Murray Gell-Mann, "From Student to Scientist" PBS Documentary

I view HEP (High Energy Physics) & Space Exploration as "push it to the limit" challenge-frontiers, which spinoff mass-market Technology. Silicon Valley was a product of

1) Physics research
Bardeen/Shockley/Brattain, invention of Transistor at Bell Labs

2) NASA Apollo program
pressure to miniaturize electronics created the Integrated Circuit (chip)

It's clear that these 2 sectors need to be exploited ("seed funding" by US Govt), for further "Emerging Economy"..Economic Growth/Recovery. As part of America 2.0, NASA 2.0 (really hit hard by Sequestration), Science 2.0

"I have never tried, in even one single little instance, to help cultivate the cultivated classes. I was not equipped for it either by native gifts or training. And I never had any ambition in that direction, but always hunted for bigger game–the masses. I have seldom deliberately tried to instruct them, but I have done my best to entertain them, for they can get instruction elsewhere."
– Mark Twain, a Biography

Leadership in STEM needs to take the above to heart, & figure out a short/medium/long term strategy to leverage their KNOWLEDGE for mass-market ("masses") monetization.

"I'm interested in KNOWLEDGE, not Product"
— Dr Misha Mahowald, Calltech PhD CNS (Computational Neural Sciences), Neuromorphic Engineer, "Discovering Women episode"

["Physics of Computation" field by Dr Carver Mead (VLSI pioneer), kinda involved Feynman]

The above illustrates the tunnel-vision 1-sided nature of Science, it is IMPERATIVE to keep an eye on Product ("monetization"). In order to demonstrate to Funding Agencies the RoI (Return on Investment).

"You have to sell yourself"
— S. Hossenfelder, advice by her professor

The whole Fiscal Crisis in Washington DC could be a "blessing in disguise", to FORCE a Science 2.0 recovery that has more Perspective ("integrated solution")

Time for Vision, Leadership, Execution to "kick in".

21. **jackj**
March 29, 2013

I found this article interesting, but still feel that the references to Perimeter Institute are somewhat misleading. Perimeter is in Canada, which is a separate country with its own funding situation.

22. **Peter Woit**
March 29, 2013

jackj,

That's true, and I suppose should have been pointed out. But it's also true that Canada and the US are quite closely linked economies and cultures, with Waterloo just over the border. Matt is raising the alarm about "our ability to defend ourselves, especially since some of the most active spending on science is being done by countries that are hostile or potentially hostile to the free world." It doesn't seem to be Ontario he's concerned about, but who knows…

23. **Peter Shor**
March 29, 2013

When Matt says "active spending on science is being done by countries that are hostile or potentially hostile to the free world", by "countries" does he mean China? Certainly science funding has decreased substantially in the countries of the former U.S.S.R., and none of the Islamic countries have large research budgets.

24. **Peter Woit**
March 29, 2013

Peter Shor,
You'll have to ask him, I've no idea what he's talking about.

Looking at the Milner prize recipients, almost all the senior ones are in the US, but it is true that the young "New Horizons in Physics" prize recipients aren't. They are in Canada, Switzerland and Israel, but I don't know if Matt considers those places to be hostile to the free world. The fact that they're not in the US may just reflect this being kind of a string theory prize, and string theory hasn't been very popular among US physics departments hiring permanent people recently.

25. **Statistician**
March 29, 2013

On something slightly unrelated: is scientific blogging going to die? I've looked at google trends and it seems to be the case:

http://www.google.com/trends/explore#q=backreaction%2C%20peter%20woit%2C%20lubos%20motl%2C%20cosmic%20variance%2C%20clifford%20johnson&cmpt=q

Interest was great around 2005 – 2007 but slowly people just don't bother reading all these science blogs. I've included Peter, Lubos, Sabine Hossenfelder, Cosmic Variance and Clifford Johnson in the comparison chart.

26. **SpearMarktheSecond**
March 29, 2013

Concerning experiments at the high-energy frontier… the US has always had the land and once (and probably still) had/s the expertise… that led to Robert R. Wilson's `Desertron', the name for what turned into the SSC. But certain practicalities (who wants to live in the desert? Which desert state has a congressional delegation as powerful as that of Texas?) took the SSC to Waxahatchie. And then the money problems killed it.

Novel acceleration ideas have been researched for at least 30 years, with meager results.

So probably we are stuck with a physically large machine. What makes little sense is putting the next large machine in Japan, where land is scarce, although perhaps their government is ready to spend for a linear collider.

Can't say CERN makes sense either, where is the land? Quite a densely populated place.

A really grand concept would be an international collaboration that builds the next big one somewhere in Africa. The collateral benefits might be incalculably large.

But there seems to be little appetite for grand projects in the US. We seem to have leaned more to our finagler, swindler, litigious, squabbling side than our grand side in the past years. A giant international project costing $30 billion with major contributions from all the world's economies just sounds foolish in our current situation. We can only do $1,000 billion wars, nothing idealistic.

27. **Kernel**
March 29, 2013

I'm a graduate student at the Institute for Quantum Computing, another Lazaridis initiative, at the University of Waterloo. I like to ask questions about where the money for IQC and Perimeter Institute come from, and have found out enough to say that your statement that "Perimeter Institute has gotten $150 million or so from Mike Lazaridis over the years" is misleading.

I have heard the sum of $150 million quoted a few times, though I'm not sure where it comes from. However, the figure probably comes from the total funding received by PI, of which Lazaridis contributed only a part. In fact, at least half of that money has been provided by the federal government of Canada, and a substantial portion was also provided by the provincial government of Ontario. In other words, PI is largely taxpayer funded. Lazaridis and others (such as the Templeton Foundation) have made important financial contributions as well, but the sum of their contributions is probably far smaller than $150 million. As far as I can tell, the situation is similar for IQC.

I think an important difference in culture exists between Canada and the United States of America. In the US, there is significant political pressure to cut any government expenditure unless it is for something everyone agrees on, like military. Rationally speaking, a government agency that wants to protect its budget is well advised to claim that it is a military organization. Therefore, the fact that much US science funding comes through the military should not be too surprising. Just because spending is described as military, doesn't mean it actually is.

In Canada, people pay a lot less attention to what the government actually does, so the government can get away with spending money without coming up with arcane justifications. I wonder, for example, what value Perimeter Institute really provides to the Canadian taxpayer. I think a major reason that Canada can get away with hosting a place such as PI is that no one is really asking the pertinent question. It's of course in my best interests to keep my mouth shut, but I suspect that PI does very little for Canada despite the fact that it is mostly funded by taxpayer dollars.

28. **Peter Woit**
March 29, 2013

Statistician,

2006-7 was the period of the String Wars, after the appearance of my book and Lee Smolin's which got a lot of media attention. I can't say that I was sorry to see that period go, since most of the heavy blog traffic then came from people interested in arguing about things they didn't really understand. To some extent I think the story of the string theory debate was that it did get well aired on the blogs, and anyone who cared made up their mind back then, with few people still interested in revisiting the debate now.

The numbers I see on my web server have been pretty constant for quite a few years now. I do think some activity has moved from blogs to twitter/facebook/google+ (and google wants to encourage this by killing google reader).

29. **Peter Woit**
March 29, 2013

Kernel,

Here's one source for the $150 million number
http://www.perimeterinstitute.ca/news/mike-lazaridis-donates-additional-50-million-perimeter-institute

I believe Ontario provided another $50 million, and there have been contributions from other sources. Details of the Perimeter funding should be in its annual reports, latest one is here

http://pitp.ca/annualreport/2012/

I think you're dramatically oversimplifying the US science budget issues. For one thing, many fields of science get little to no funding through military channels. Biomedical research is a good example of something which the US pays for a lot of (the NIH budget is about $30 billion/year) and has wide support from many quarters. The NSF has long enjoyed political support from both parties. I doubt the average person or average politician in the US knows or cares any more about government science spending than their counterparts in Canada.

30. **srp**
March 29, 2013

Peter: Your slave-labor comment is odd. The government pays for people to get PhDs and conditions funding of research on new PhDs being trained. This is called "subsidizing" or "investing" in the creation of human capital. We may both agree that the policy is quantitatively excessive, but the rationale for it is as I stated earlier–the government is responsible for getting the most research output it can for its expenditures, and increases in research spending tend to raise compensation much more than research output unless something is done to make the supply curve of researchers more elastic.

It's fine to complain about the loss of tenure-track jobs and the switch to a contingent workforce in academia–folks over in the humanities have felt the brunt of this for decades, and its bad effects as well as its good points are pretty well understood. But the "exponential model" also doesn't make sense–you can't have an exponentially growing set of tenure-track jobs to match the exponential growth of PhDs implied by the current grant system. If each researcher pushes out two or three new grads each year, and each were to then get a tenure-track job and funding for research which entailed turning out two or three grads, and so on…pretty soon the number of researchers would exceed the world population. People are taking an atypical post-World War II situation in the U.S., where growth was exponential for a time, as normative.

31. **Peter Woit**
March 29, 2013

srp,
I'm in complete agreement about the fundamental problem of too many PhDs. My "slave-labor" comment was about the argument that the government is justified in using its power to intervene in the labor market in order to keep down labor costs. I'm curious whether this is done in other parts of the economy.

32. **dinotroll**
March 29, 2013

I feel at some point in time birth control has to be done. But I also agree that we cannot do it right after undergraduate since students are very raw at that time. Already PhD in Physics takes 6 years on an average. I would advise breaking it into 2 stages of 3 years each. The first would be something like a comprehensive masters, getting done with all graduate level courses and publishing a paper or two. Then when you apply for the next three years, birth control can be applied and the students themselves might realize whether they are cut out for physics research or no. At this stage the application procedure can also be made a bit more rigorous with interviews and research plans. With the present system it makes no sense that a person takes 6 years to finish PhD and then has to take up job in a software firm.

33. **srp**
March 29, 2013

Peter:

"My "slave-labor" comment was about the argument that the government is justified in using its power to intervene in the labor market in order to keep down labor costs. I'm curious whether this is done in other parts of the economy."

Great question. Answer part one: The government (which is not a unitary actor, but it's syntactically easier to discuss that way) is very interested in keeping down the prices of stuff that they have to buy, where they are a customer. They aren't very good at it, for a variety of reasons (especially the political influence of their suppliers), but there's a vast apparatus that tries to keep down the prices paid to military contractors, for example. Since most of the cost of these items is labor, especially white-collar labor, the effect is to depress the wages of defense industry workers. We also used to have this thing called a draft that enabled the government to confiscate the labor of every able-bodied male in the United States and pay almost nothing for it. (Thank you Milton Friedman and Richard Nixon for ditching that one.) But to this day, the government tries to get the cheapest volunteer force it can subject to quality and quantity constraints. Nobody wants the taxpayer to give government suppliers rents (returns in excess of what is necessary to call their resources into service).

Answer part two: Physicians getting Medicare and Medicaid payments are a different case, because the customer is ostensibly the patient. But price controls aimed at lowering spending have been included in the legislation for many years; they haven't been implemented because of political pressure from physicians and some patient groups, leading to annual "doc fixes" that put money back into the budget to cover the suspended price rollbacks. One school of thought on PPACA is that the various "accountable care organization" and IPAB innovations it includes are intended to stealthily push down the incomes of US physicians in the name of fiscal responsibility.

Answer part three: Politicians have (insincerely and cynically in my view) spent quite a bit of time arousing public indignation at the compensation of specific managers and financiers. One interesting example was the AIG case, where a group of mid-level workers who were not responsible for the initial problems were specifically retained by the government to do the toxic security clean-up job with the promise of substantial bonuses for not deserting the sinking ship. Congresscritters went nuts over those bonuses, to the point where one of the drones wrote an open letter that I think was published in the New York Times trying to explain the situation. And of course we have the continual bloviation over executive compensation, trying to lower the pay of that sector of the work force.

So I think it is fair to say that the US government regularly intervenes in various labor markets to reduce the compensation that specific types of workers receive. It is especially prone to do so when it has monopsony power, but shows little compunction about trying to beat down the wages of others it holds as undesirables or political untouchables.

34. **Peter Woit**
March 29, 2013

Thanks srp,
This is getting too far off topic, but I'll just comment that I think I see a pattern here, with the effectiveness of efforts to drive down wages inversely proportional to the size of these wages. From my experience here in Manhattan, I can assure you that those in the financial industry and at the top levels of other industries based here seem to be holding up pretty well under the federal government's lash, whereas those working in biomedical research labs are a different story.

35. **Stephen Olsen**
March 30, 2013

A serious problem is the fact that many senior faculty members at Universities and National labs have lifetime tenure & never retire, thereby locking up positions and large salaries that would otherwise go to young people. In fact senior faculty that do require usually free up funds for more than one junior faculty position. Lifetime tenure should be replaced with tenure to some fixed age. Faculty members over that age that are still productive should be eligible for subsequent fixed term contracts that they could negotiate with chairpersons & deans etc.

36. **PhysGrad**
March 30, 2013

Peter,

Any thoughts on the relationship between funding for graduate students in physics, the growing cost of undergraduate education (particularly at state schools where the cost is growing faster than at private institutions), and the ability of programs to attract and train talented graduate students who are coming in with higher debt loads and seeing very little growth in grad and postdoc stipends?

37. **Low Math, Meekly Interacting**
March 30, 2013

I'll briefly add to some comments above about military R&D. I happened to work with a "couple" people in my first job out of college who transferred to the NCI from another military facility not too far away. The reason they moved on was not because of some deep moral objection to the potential military applications of microbiology (otherwise, they never would have worked there in the first place, though I suspect their work was not on weapons, but countermeasures). Rather, they came to be dissatisfied with the professional isolation and, more to the point, the perceived lack of high standards for deciding what projects were to be tackled in the first place. Apparently military R&D of that time (late 80's, early 90's) wasn't be run by the sharpest knives in the drawer, and one needn't be so concerned that money was being spent finding better ways to kill people. Rather, be concerned that money was being spent by fools to do foolish, and ultimately useless, work.

I have little confidence the situation has changed much. Add to the idiocy the endemic waste of the Military Industrial Complex, and one can almost ignore humanitarian concerns altogether, and focus on the scandal of funding a jobs program that produces nothing. That money could be better spent on almost anything.

38. **Peter Woit**
March 30, 2013

PhysGrad,

The student loan situation in the US is horrific, with universities more and more funding themselves by getting their students to take on increasingly large amounts of debt, putting them in a situation Bruce Springsteen described as one of having "debts no honest man could pay". Students with this debt are now graduating and entering a very rough job market, with ugly results. In this larger societal context, I'm not sure that Ph.D. stipends and postdoc salaries keep people from going to grad school, since the alternatives may be more dismal. One popular alternative to physics grad school has sometimes been law school, but that's becoming another sad story.

I don't know much about the situation in physics. In the field I do know about, math in general, Columbia in particular, Ph.D. stipends are around 30K/year, and postdoc stipends I think have done pretty well. In recent years at the top places, you have things going on like the Simons Foundation postdocs paying 70K/year, and this does have an effect on bringing up what universities offer everyone, since they are trying to compete with these.

Both Math and Physics grad programs in the US are now producing Ph.Ds. in record numbers, so there's no numerical evidence that the loan problem has suppressed these numbers. The situation in Math and Physics is rather different, with Math Ph.D. students funded typically as TAs, Physics students funded out of grants. With government funding flat and increased faculty salaries and tuition, I would guess that has kept downward pressure on money available for student fellowships, so physics student stipends may be doing worse than in math. I have heard complaints from theorists in physics that the government is funding fewer of their students, but have to confess that this sounds to me like a good thing, given the absurdly small number of permanent jobs in the field.

39. **Al**
March 30, 2013

"Ph.D. stipends are around 30K/year"

is that including tuition? because if yes, it must be hard living on 12K/year in NYC.

40. **Peter Woit**
March 30, 2013

Al,

If the Ph.D. students were paying tuition out of their stipend, they'd get a negative stipend, since full tuition is nominally something like $40K/year. But Ph.D students are admitted on fellowships that pay their tuition, and also the 30K or so stipend. Some students make extra money teaching extra courses, courses during the summer, or tutoring.

The master's degree programs are a very different story. There typically the students are paying tuition, even full tuition.

41. **Otto**
March 30, 2013

> But whether [biomedical research money] is now being spent well, or whether taking away from some other priority to spend more in this area would be a

good idea, I haven't a clue.

From everything I've read, NIH R01/R21 paylines have been extremely tight lately (e.g., NCI at the 7th percentile). Meanwhile, the utterly useless NCCAM get $128 million (FY 2012) and things like the (equally useless) TACT trial gets $30 million. One could argue that these are pittances in the overall NIH budget, but it's not money well spent. *Science* recently reported that the sequester could cut success rates to ~15%.

42. **Bobito**
April 2, 2013

A country which has a lot of physics PhDs working as computer programmers is doing fine. The "liberal arts" training of our era is a broad technical background. To find the best physicists you have to fund based on potential, and inevitably only some subset of those funded will produce really creative work. However, if you had not funded the 100, you would not have have found the 10.

43. **StevenC**
April 2, 2013

Dear All:

I share Peter's worry about students – either their loan or their quality. I have supervised or help manage some graduate students before, and they produce work that is rather low quality. Unforuntely, I have no say on their graduation nor I am a thesis committee member; but I have made silent protests for skipping defenses of students that I myself have managed – laugh. Sadly, the poor-quality student problem falls under as a new spin to Publish-or-Perish: Have-Students-with-Thesis-or-Perish.

Student loan is going to be a time bomb. Not only debt levels are high, but youth employment is on the high across the developed world (not just in US – it is as big as a disaster in Europe and Asia). That is a dangerous combo: the young folks get in debt, and cannot develop their career to pay back.

SS

44. **StevenC**
April 2, 2013

Excuse my spelling – I mean "unemployment" instead of "employment". I now work in the UK, but I was talking about the work I did when I was back in the US few years ago.

45. **StevenC**
April 2, 2013

Sorry one more point:

I think the increase number of poor quality graduate students do have relationship with the problem with student debt and poor career prospects – going to graduate school become fads and quick fixes for poor employment prospects, and plays right into career expectations of senior research staff and professors. That is actually quite bad, imo.

46. **srp**
April 2, 2013

I worked for a while at a university that was on the edge of losing Carnegie Tier I status because it didn't have enough PhD students. University administration repeatedly pressured our department, which was one of the strongest at the school, to start a PhD program. Faculty resisted because a school at that level would never have been able to attract graduate students near the quality of the faculty themselves.

You want to think of your doctoral students as future colleagues. Nothing is more soul-sucking than a bad PhD program turning out marginal degree holders.

47. **Gilgit**
April 2, 2013

While the absolute numbers look OK, I think the chart on this page tells a lot:

http://mikethemadbiologist.com/2012/07/16/the-old-days-were-better-at-least-when-it-came-to-nih-funding/

The chart shows how the percentage of applicants for federal funding that actually gets funded is much lower than in the past. It also shows, at least for the NIH, that the number of applicants is much higher.

Which brings me to two points:

1.) One of the reasons America has continued to be a superpower for the last 30 years is because we lead in science and technology. And one of the big reasons for that is because of government funding. We have a lot of scientists/engineers and the decision was made after WWII to keep funding science (with a bump up after Sputnik).

To stay on top you have to keep spending. Since there is large core of scientists already out there, if you want to keep growing you have to keep increasing the funding.

2.) I notice the number of applications in the chart has kept growing. This may be because it is for NIH and the biological/pharmaceutical fields keep growing. But I keep getting the impressions that big companies don't plow their massive profits back into research like the used to. If we are going to keep leading in science then government money is going to have to take up the slack and needs to grow more than in the past.

Of course America doesn't have to keep being a technology powerhouse. The financial industry is still profitable. I'm sure they'll do us all proud.

48. **Peter Woit**
April 2, 2013

Gilgit,

From what I see of the numbers, there was a massive increase in the NIH budget 1998-2003, with more money going into the system causing a massive increase in the numbers of grant applications during the years after that. If you wanted the success rate for grant applications to stay constant, it looks like, after doubling the NIH budget in those years, you would have needed to double it again in recent years.

Exponential growth is just not sustainable, it has to stop at some point.

49. **Gilgit**
April 4, 2013

(My post went long. If you don't think I followed the rules you can delete it.)
OK, I followed your link and looked at many graphs and concede your point that we are not at low levels of funding.

I did think about it a bit, though. Are there strong reasons to increase spending right now? Some thoughts popped out at me.

Looking at the non-defense R&D chart, I see that we really were spending a lot on energy in the late 70s. Luckily, Ronnie Boy worked very hard and after 6 years was able to gut many of the programs. This lead to the current world were no one thinks about energy or any massive side effects of using energy. 🙂

Energy research might mitigate the effects of fossil fuels. Offsetting the amount of money going to drought relief, hurricane relief, and [you favorite catastrophe here] relief. And, thanks to Ronnie, we're 20 years behind.

Another thing that came to mind is the number of articles lately on diseases that have been showing up that are immune to every single antibiotic we have and how these strains have been appearing in western countries more and more. I'm thinking that if this trend continues and we have some deadly outbreaks, we might wish we'd spent more money in the preceding years.

And as for private drug companies:

http://www.slate.com/blogs/moneybox/2013/03/14/medicare_part_d_is_cheaper_ than_expected_because_the_drug_pipeline_has_dried.html

that says that the number of new drugs coming on the market is much lower than in the past. I've read that people are trying to get public funding specifically allocated to develop new antibiotics. I hope they succeed. In any case, private companies think its not profitable enough because the people dying today live in poor 3rd world countries. Gotta love that private sector.

Finally, I can't help but notice that the economy isn't as strong as it was. Maybe you've noticed too. It would be nice if – when the economy does pick up – we have lots of scientists and engineers trained and fresh for the fight. Instead of lots of ex-scientists who have moved on because they couldn't find work.

If the $64 Billion was increased to $74 or $84, would that really be unaffordable? The total budget is $3.5 Trillion.

By comparison, I'd point to a story NPR did a couple weeks ago about how the Americans with disabilities act, which was passed in the 80s has change the country. Many people who have become unemployable have been put on disability because we don't have any jobs for them. Not because they are too ill to work. They can't do manual work anymore or the only employer for miles around went bust. 14 million people that cost $260 Billion a year. I'm not against this, but as the report pointed out: we are spending $260 Billion to have people not work and almost no one noticed and few people even talk about it. It wasn't a plan, just how things have worked out.

I bring it up because if we can afford something that big by accident, we should be able to increase R&D funding NOW without breaking the bank. Even if the increase was only temporary. That chart (following your link) that shows "total non-defense R&D" spending – it says that in the late 70s, after Apollo was done, we spend $39 billion, so we still aren't double that. So spending doesn't sound unsustainable to me.

Grothendieck News

March 30, 2013
Categories: Uncategorized

On Thursday someone pointed out to me that it was Alexander Grothendieck's 85th birthday. Hopefully he is well and celebrating appropriately. Coincidentally I just heard the following rumor. Supposedly a couple months ago the librarian at the IHES got a phone call from a man who said his name is Alexander Grothendieck, that he needed a specific book from the library and asked if he/she could mail it to him at a certain address somewhere in the south of France.

The librarian said that books are lent only to IHES members and no books are sent by mail. The man on the phone said something and the librarian said that he/she must consult with the director. The director then went ahead and had the book sent.

Perhaps someone can confirm this rumor. Of course the first question that comes to mind is: "What book was it???" (And yes, if I do find out, I suppose I shouldn't blog the answer, to respect Grothendieck's privacy…)

Comments

1. **gizmo**
   March 30, 2013

   It is very unfortunate for mathematical community that Grothendieck stopped his mathematical research at the age of 42. I don't understand his decision, because, as far as I know, he was not very active in something political. He could continue his research. In wikipedia it says that
   "In this "Déclaration d'intention de non-publication", he states that essentially all materials that have been published in his absence have been done without his permission. He asks that none of his work should be reproduced in whole or in part, and even further that libraries containing such copies of his work remove them."
   Very weird for such a great mathematician.

   As for the book, I believe you shouldn't ask if you think it is improper to share it. (By the way I am very curious too.)

2. **johnmcAllison**
   March 30, 2013

   "He [Grothendiec] asks that none of his work should be reproduced in whole or in part, and even further that libraries containing such copies of his work remove them."

   I just hope that it wasn't Grothendiec asking for a book authored by some one else, since it would smear himself as a parasitic hypocrite. He comes across as a man of principle and I doubt he would do this.

3. **Deane**
   March 30, 2013

   I heard this exact story only a few weeks ago from a mathematician I know, and he seemed to have heard it from a reliable source. The version I heard is that Grothendieck called one of the permanent professors and asked for a particular book from the library. I don't remember what I was told about what ensued after that or which book it was. But what you say sounds pretty plausible.

4. **MathPhys**
   March 30, 2013

   @gizmo

   Grothendieck was mathematically active way past 1970. His *"Esquisse d'un Programme"* , which by all accounts is first-rate mathematics, was written in 1984, based on a 1,600 manuscript from 1980/81. He has at least one other 600 page manuscript on stacks from 1983. Put together, these works add up to more than what a typical good mathematician at a typical good university produces in a lifetime.

   @johnmcAllison

   I don't think that he's well enough to be consistent.

5. **Felipe Zaldivar**
   March 31, 2013

   @johnmcAllison

   Sorry to disagree, but I don't like your choice of adjectives to refer to Alexander Grothendieck. All the stories one hears about his work and work ethics contradicts your unfortunate words. The monumental work he left us is something we should, and are, grateful for. We may lament his early parting from our milieu, but we respect his reasons, whatever they may be. After all he did not harm anyone in doing so, and all the libraries have copies of his published works that we consult freely. Most of his published work is freely available, e.g. all the EGA volumes, from NUNDAM . Anyone familiar with his work knows how freely he shared his ideas, and let others add their own, as in his celebrated SGA at IHES, that are now being republished by the SMF (my local library has now new copies of two of the volumes on group schemes; I hope to see soon, the volumes from SGA4). Again, I am sorry to disagree with you, but allow me to end with a thought I recurrently have about this larger than human characters: we don't exactly know or understand what Grothendieck or Perelman are telling us, not about them, but about ourselves, minor players in the word of mathematics, but we must respect them for what they have done.
   Best wishes,

6. **Tim Campion**
   March 31, 2013

   @ gizmo

   If you're under the impression that Grothendieck was not very political, then you might want to read up a bit. His parents were anarchists. His father supposedly died in Auschwitz, and Grothendieck spent several childhood years in French refugee camps during WWII.

   Grothendieck himself was a dedicated pacifist. He taught seminars in North Vietnam to protest the war there. He left the IHES and refused the Crafoord prize partly because they received military funding.

This is just from wikipedia.

7. **gizmo**
March 31, 2013

@ Tim Campion

To my knowledge his political activities were not too dense to make him stop mathematical research. I don't say he don't have sharp political opinions, but if he wanted to change something, he should appear in public and defend his opinions, instead of living in reclusion. I don't know Grothendieck personally, so I don't want to say more.
On his views on funding of mathematics, V.I. Arnold has similar views:
http://www.pdmi.ras.ru/~arnsem/Arnold/Polymath.txt
Off topic: Maybe I am wrong, but I observed that while top theoretical physicists almost always on the faculties at most prestigious universities, there are many mathematicians of first rate, some earned fields medal, on faculties at less prestigious universities. Can someone explain why ? Maybe it is because mathematicians works more indepently than physicists (who works more or less on same topics, while mathematicians have much more freedom). If it is so, can we say that different universities have tend to different topics (in physics)? Princeton seems to have a clear bias to string theory but what about others ? Isn't this harmful to phd education ?

8. **Abraham Smith**
March 31, 2013

gizmo,

I think that's just selection bias on your part, and perhaps a misunderstanding of what "prestigious" means. A math department is prestigious because of who is there and what sort of community they are trying to build. When I think of top-tier math departments, that list correlates extremely highly with standard lists of world-renowned universities. In no particular order, I think of places like Princeton, Berkeley, Stanford, Columbia, NYU, etc.

Also, I don't think Grothendieck (or Perelman or anyone else) owes us anything. Many people have talents they choose not to pursue for all sorts of personal reasons. Maybe they have a family whom they refuse to neglect for the sake of a better position. Maybe they decide on more lucrative job options. Maybe they have physical or psychological conditions that make the experience unpleasant. Maybe they just prefer gardening!
I don't believe I would make the same decision that Grothendieck did, but I will also never be in that position. I was not a victim of any particularly horrible historic events as a child, and while I strive to be a fair second-rate mathematician, I will never be known as an earth-shattering influence like he is. At the same time, it's always tempting to say "screw this!" and go live in a cabin in the mountains somewhere, and I can only envy those who had the courage to pursue their desires.

9. **OMF**
March 31, 2013

Maybe it was a book on LaTeX?

10. **socrates**
March 31, 2013

Sources tell me it was "Not Even Wrong" by a certain Peter Woit

11. **MathPhys**
April 1, 2013

@gizmo,

It is my impression as well that the distribution of prominent physicists peaks at a (much) smaller number of universities compared to that of prominent mathematicians. I think the reason is that it is easier for a mathematician to work in relative isolation (think of Langlands) and produce influential mathematics than for a physicist. Physicsists seem to need to talk to others, or at least to have the chance to do so when they want to, on a non-stop basis.

12. **Jon**
April 1, 2013

April fools?

13. **Thomas**
April 1, 2013

A book by Pierre Lochak which seems to be e.g. about putting Grothendieck's way of thinking into a braod context: http://www.math.jussieu.fr/~lochak/textes/mf.pdf

14. **a**
April 1, 2013

By the way, are you aware of audio recordings of 1973 Grothendieck's lectures at SUNY at Buffalo? It appears the copyright of all these recordings is that of the Department of Mathematics of SUNY at Buffalo and one can actually listen to them.

15. **Peter Woit**
April 1, 2013

a,

Thanks (that site is down now, was up earlier). Do you know if any kind of transcription of those lectures exists (i.e. is there any kind of written version of the material of those lectures)?

16. **Felipe Zaldivar**
April 1, 2013

There are some notes, of the first part of the course, written using these recordings by F. Gaeta. Scans of the notes are in the "Mathematical Texts" section of the Grothendieck's site (www. grothendieckcircle.org) under the title "Introduction to Functorial Algebraic Geometry" and, as emphasized there these are the notes with commentaries by F. Gaeta, apparently there were no pre-notes by Grothendieck himself that were later expanded by the note-taker. It would be nice to compare the recordings with the notes.

Strange connections to strange metals

April 2, 2013
Categories: Uncategorized

In recent years much of the attention of string theorists has turned to applications of string theory (via AdS/CFT) to heavy-ion physics and condensed matter physics. Since I'm no expert on either topic, I've been curious to hear what experts think about this. In the case of heavy-ion physics, as far as I can tell, this doesn't seem to have worked out very well, with string theory not of much use to say anything about heavy-ion physics at the LHC (although I'd be interested to hear from those more knowledgeable about this). There does still seem to be some promotional activity in this area, with Joe Polchinski last month giving a popular talk in which he claimed that

> The quark-gluon liquid, produced at the RHIC accelerator in NY and by the LHC, is best modeled as a black hole, by applying AdS/CFT duality.

On the AdS/CMT front, one expert is now being heard from. In the latest Physics Today, Philip Anderson has a piece called Strange connections to strange metals, in which he responds to an earlier Physics Today article by Hong Liu, From black holes to strange metals, which claimed:

> String theory relates gravity to the physics of a novel phase of matter observed above the superconducting transition temperature.

Anderson writes:

> It is one of many quasi-journalistic discussions I have seen of results using the AdS/CFT (anti–de Sitter/conformal field theory) correspondence from quantum gravitation theory ostensibly to solve condensed-matter physics problems such as the "strange metal" in the cuprate (high Tc) superconducting metals. As the probable source of the buzzword phrase "strange metal" to describe the phenomena observed in the cuprates and of a theory that bids well to explain those phenomena in detail, I think I have a reasonable motivation to object to the publication of those claims, even though advanced tentatively, when so much is known about this particular phase.

He ends with a summary of what he sees as the problem with the whole AdS/CMT idea:

> As a very general problem with the AdS/CFT approach in condensed-matter theory, we can point to those telltale initials "CFT"—conformal field theory. Condensed-matter problems are, in general, neither relativistic nor conformal. Near a quantum critical point, both time and space may be scaling, but even there we still have a preferred coordinate system and, usually, a lattice. There is some evidence of other linear-T phases to the left of the strange metal about which they are welcome to speculate, but again in this case the condensed-matter problem is overdetermined by experimental facts.

Hong Liu responds here.

Anderson will be here at Columbia to give a colloquium April 15 on The Discovery of the Anderson-Higgs Mechanism. I've written something about this history here, look forward to hearing about it from Anderson himself. There's much speculation about a possible Nobel for the Anderson-Higgs mechanism this year, one wonders if the Nobel committtee has any AdS/CMT proponents…

Update: An additional comment about this just occurred to me: the criticism of Anderson's work on the Anderson-Higgs mechanism has always been that he didn't appreciate how different relativistic systems. Now he's claiming the AdS/CMT proponents don't appreciate how different non-relativistic systems are.

Comments

1. **bitboy**
   April 2, 2013

   Hi Peter,

   Sachdev has written about this connection. He has some articles on his website: http://sachdev.physics.harvard.edu/

2. **Bob Jones**
   April 2, 2013

   I think the key point here is explained in the last paragraph of Liu's response:

   "Whether or not one finds a 'conventional' explanation for strange metals, connections between the physics of strange metals and black holes are worth exploring. They hint at a new paradigm for thinking about strongly correlated quantum soups. As an added bonus, we may also obtain new insights into quantum gravity from advances in condensed-matter physics."

   Research into the AdS/CFT correspondence is important not only because of its potential applications in condensed matter physics. It's also a very important general idea in quantum field theory, and it has led to important insights into the holographic nature of quantum gravity.

3. **lun**
   April 3, 2013

   I believe the key test in science is falsifiability:
   Is there an experimental outcome (realizeable or Gedanken) that would mean that "strange metals cannot be described by a theory with a classical gravity dual"?
   If not, then AdS/CMT is essentially a very fancy way of parametrizing data, rather than a physical theory.
   The fact that most such work is based on "bottom-up" potentials chosen by hand makes it likely this is indeed the case, but the jury is still out.

4. **Christopher Herzog**
   April 3, 2013

   Replying to lun's comment, it is an interesting thought exercise to replace "theory with a classical gravity dual" with "effective field theory". Like any particular effective field theory, any given theory with a classical gravity dual of course produces falsifiable predictions. It is a rather formidable enterprise to falsify the framework of effective field theory. (Perhaps we should try.) Nevertheless, I think there is general consensus that effective field theory is a very useful framework for describing the world we live in. Gauge/gravity duality promises to become (or perhaps already is) a somewhat less general although nevertheless very interesting framework in which to write down specific physical models.

5. **lun**

April 3, 2013

The EFT analogy is indeed often used, but I think it is flawed.
The applicability of any generic EFT has a stringent well-defined
set of requirements:
A hyerarchy of scales (the “typical scale”<<"the fundamental scale"), and
a stable vacuum.
It is usually very easy to see whether a system fails at these characteristics,
and there are plenty of systems where applying any EFT, intended in the conventional way, is plainly foolish (turbulence, glasses, phase boundaries with EFT of one phase. Of course sometimes people try to apply EFT to domains where it clearly fails, but this is a problem of EFT practitioners, not EFT itself).

From what I can see very little effort is being made to define
a similar domain of validity for AdS/CMT.
Note I am not saying it is impossible to do, simply it is not considered
an important problem, and is generally disregarded: For instance, one
obvious criterion for the validity of the classical gravity description is that
N, the number of YM colors, is large. Yet there are well-cited papers by
famous people who attempt to model 1/N effects by classical gravity
descriptions. There is a fundamental issue there, 1/N corresponds to
deviations from classical gravity, but this is usually not even mentioned
in the paper.

Part of the reason is objective difficulty: For almost all systems
where Gauge/gravity is used, we do not know the the Gauge theory.
For bottom-up systems, which seem to form the basis of AdS/CMT models,
we dont know either the Gauge or the Gravity description.
But it is still bothersome that some people who do this essentially use
some complicated AdS concoction to draw a line through a bunch of data,
and then say "this system can be described by string theory".

6. **Peter Woit**
April 3, 2013

lun,

I think Anderson is wisely not getting into very general arguments about falsifiability, but addressing the question of whether, in this particular case, AdS/CMT methods give a better or worse model for the system in question. It seems to me that his claim is that he and others have models that explain much more about these systems than the AdS/CMT-based models. If so, publication of articles aimed at non-experts claiming that the way to understand this physics is via strings, quantum gravity or black holes (even in the case of Polchinski and heavy ions, that the system is “best modeled” this way) is quite misleading.

The hype level in the past associated with AdS/CFT methods has been very high (these supposedly provide the “harmonic oscillator” of the 21st century…). No one would object to people pursuing ideas about use of such methods in condensed matter theory (“falsifiable” at this stage or not), as long as they’re not misleading people about how well their methods work compared to others.

Various Links

April 2, 2013
Categories: Uncategorized

- The AMS-02 experiment results will be announced tomorrow, 1700 CERN local time, webcast here. The normally reliable Jester says rumor is no dark matter. For this kind of astrophysics news, you should find a site with an expert to interpret the results, I'll try and provide a link here.
- Weird. The Templeton Prize was supposed to be announced on Thursday, but they've changed their announcement to read "April 2013 (exact date to be determined)". Did someone turn it down or something?
- There's a very long oral history transcript here of an interesting interview with Joe Polchinski. It covers a lot of ground of the history of what went on in particle theory during an era which included the rise of string/M-theory. The interview took place in 2009, and has a certain amount of "string wars" sort of material, since that was the period when this was winding down. Someone should fix the proper names in the transcript though…
- As part of his Einstein Chair at CUNY, Dennis Sullivan has run a seminar for many, many years, with quite a few interesting speakers. There's now video online of many of the talks.
- Howard Burton, who was the founding director of the Perimeter Institute now has a multimedia magazine called Ideas Roadshow, with one of the first programs a long interview with Nima Arkani-Hamed (access free for to this if you sign up).
- I keep on finding out about more math blogs worth a look, for instance Chromotopy and DZB's blog (via Motivic Stuff).
- Eckhard Meinrenken's book Clifford Algebras and Lie Theory is now out. The book is online here if your institution is paying Springer.

Update: A live blog from the AMS talk is here. See the comment from "M" here which has an abstract of the talk giving some of its main conclusions.

Update: CERN has a press release with the results here.

> … these features show evidence of a new physics phenomena.
>
> The exact shape of the spectrum, as shown in Figure 2, extended to higher energies, will ultimately determine whether this spectrum originates from the collision of dark matter particles or from pulsars in the galaxy. The high level of accuracy of this data shows that AMS will soon resolve this issue.

Update: For a summary of the significance of this for dark matter, see Resonaances.

The paper is here. The delay in the public announcement was clearly caused by Ting's decision, unusual these days, to not submit a preprint to the arXiv when the results were ready, but just quietly submit to a journal (PRL, on March 14th), and say nothing publicly until the paper was accepted and published.

Update: Better (as in free) link for paper.

Update: The Templeton Prize was announced today, April 4: Desmond Tutu.

Comments

1. **Anon**
   April 2, 2013

   You're missing the colon after http in the "Clifford Algebras and Lie Theory" link.

2. **Peter Woit**
   April 2, 2013

   Anon,

   Thanks. Fixed.

3. **Mark**
   April 3, 2013

   Peter,
   Thanks for this (as usual) very valuable collection of links. I enjoy reading your blog a lot !

4. **M**
   April 3, 2013

   The abstract of one AMS talk is public:

   Alpha Magnetic Spectrometer (AMS-02) is a general purpose high energy
   particle detector which was successfully deployed on the International
   Space Station (ISS) on May 19, 2011 to conduct a unique long duration
   mission of fundamental physics research in space.
   The results of the AMS experiment on the primary positron fraction in
   the energy range from 0.5 to 350 GeV are presented. Events have been
   collected in the first 18 months of data taking, for a total statistics
   of ~6 million electrons and ~600,000 positrons. The data show that the
   positron fraction is steadily increasing from 10 to ~250 GeV. The
   positron fraction spectrum shows no structure and the positron to
   electron ratio shows no observable anisotropy.

5. **Slothrop**
   April 3, 2013

   Here is a free link to the AMS paper:
   http://physics.aps.org/featured-article-pdf/10.1103/PhysRevLett.110.141102

6. **Shantanu**
April 4, 2013

Peter, OT to this thread, but a talk on some of the stuff you have talked in the blog
http://pirsa.org/displayFlash.php?id=13040115

7. **Hansi**
April 8, 2013

By the way peter,
what do you think of this brand new string theory book:

http://www.amazon.com/Concepts-String-Theoretical-Mathematical-Physics/dp/3642294960/ref=sr_1_1?s=books&ie=UTF8&qid=1365427927&sr=1-1&keywords=luest

Publication Date: October 4, 2012 | ISBN-10: 3642294960 | ISBN-13: 978-3642294969 | Edition: 2013
The purpose of this book is to thoroughly prepare the reader for research in string theory at an intermediate level. As such it is not a compendium of results but intended as textbook in the sense that most of the material is organized in a pedagogical and self-contained fashion.

8. **Peter Woit**
April 8, 2013

Hansi,

From the table of contents, looks like it mostly covers string theory/unification from the point of view popular 20-25 years ago, very little about AdS/CFT, which has dominated the field the last 15 years or more. But, I'm not the target audience for this book, not sure who is…

9. **Hansi**
April 26, 2013

Peter woit wrote:
But, I'm not the target audience for this book, not sure who is…
end quote

Well, the book by luest, blumhagen and theisen is meant as a simpler introduction into string theory than green schwarz witten or the books of polchinki.

As such, it avoids phrases like "it can be shown", or "it obviously follows" and it does not assume any knowledge beyond field theory and relativity basics.

It is however, more difficult to read than the book of Zwiebach, which does not even describe supersymmetry. In contrast to Zwiebach, the new book of Luest, blumhagen and theisen aims to threat the subject seriously. Regarding to your comments on ads/ cft: The book contains 42 pages on ads/cft. For example, becker becker schwarz only write 31 pages on this, with using a much larger font!
Furthermore, on the chair of the author, a regular lecture is held on ads/cft. The book on string theory by luest blumhagen and theisen aims to just covering the "basics" as an introduction. With that book having read, one then can get into the lecture on ads cft…

This Week’s Hype

April 3, 2013
Categories: This Week's Hype

The award for this week’s hype goes to the people at CERN, who normally are pretty good about this, but somehow thought it was a good idea to spin the AMS-02 results in a way that makes it sound as if they provide significant evidence for dark matter. The press release has:

> The international team running the Alpha Magnetic Spectrometer (AMS1) today announced the first results in its search for dark matter….
>
> These results are consistent with the positrons originating from the annihilation of dark matter particles in space, but not yet sufficiently conclusive to rule out other explanations….
>
> One possibility, predicted by a theory known as supersymmetry, is that positrons could be produced when two particles of dark matter collide and annihilate. Assuming an isotropic distribution of dark matter particles, these theories predict the observations made by AMS…

which, is inconclusive if you read it carefully, but sure makes it sound like this was an announcement of significant evidence for dark matter if you don’t. As one might expect, this immediately led to press stories about how:

> A $2 billion particle detector attached to the International Space Station has detected the potential signature of dark matter annihilation in the Cosmos, scientists have announced today…
>
> By doing a tally of electrons and positrons, physicists hope the AMS will help to answer one of the most enduring mysteries in science: Does dark matter exist?
>
> And today, it looks like the answer is a cautious, yet exciting, yes.

the kind of thing which, as usual, made it to Slashdot.

I won’t bother explaining here why this is nonsense, since this has been done much better and at length by Jester and Professor Matt Strassler.

Comments

1. **Bernhard**
   April 3, 2013

   Good that you put it so clearly. In the previous post I was already about to ask how come the result could be “evidence of a new physics phenomena” while at the same time they could not tell for sure whether the “spectrum originates from the collision of dark matter particles or from pulsars”….

2. **Avattoir**
   April 3, 2013

   Ethan Siegel is calling the CERN spin not merely misleading, but deceitful; no, worse -planned and deliberate fraud (since he predicted it 6 weeks in advance):

   http://scienceblogs.com/startswithabang/2013/04/03/itll-take-a-lot-more-than-ams-to-find-dark-matter/

3. **ohwilleke**
   April 3, 2013

   It would be nice if someone could do theoretical expectation charts in different scenarios to compare to the results. This may not be possible in all scenarios, but ought to be calculable in dark matter annihilation scenarios with some precision.

4. **Z**
   April 3, 2013

   Well, it gets worse. This is the Huffpost’s front page right now: http://i.imgur.com/ISgvNps.jpg

5. **OHNOES**
   April 3, 2013

   The Over-Hype Network for Objective Estimates of Significance has announced, in its first analysis of the media data, that along with the expected hype emitted by the AMS collaboration, there are very large quantities of antihype being produced by local sources in the blogosphere. We believe that the volume of antihype emission is significantly greater than would be produced purely by a desire for rational balance, and that it therefore constitutes evidence for the existence of “dark motives”.

   One popular theory of dark motives is “superskepticism”, a state of mind which reflexively scorns and belittles any claim of significant novelty or discovery. According to the theory, superskeptics would characteristically be found in hype-rich environments.

   The quest for evidence of superskeptics has been dominated by the search for great voids, cosmic battlegrounds created by the mutual annihilation of hype and antihype, several of which were observed in the mid-2000s. We believe that our observations may constitute the first direct detection of superskeptics engaged in real-time production of antihype.

6. **paddy**
   April 3, 2013

   First: kudos to PW (for the links to knowledgeable discussion of AMS-02 results).
   Second: it is sad that the success of such an intricate instrument is, apparently, besmirched by PR seeking.
   Third: I await the formulation of a simply unnatural “superskepticism” perhaps split?

7. **Hal Porter**
   April 3, 2013

   Just a quick add-on. Nature’s take seems pretty much yours and Jester’s, which I had had read before the CERN release. I assume it was the CERN press office to blame, but I can assure you that as this goes down, the more intelligent and discerning of the science press press may well remember this with great negativity and future suspicion if they feel they were misled.

Really stupid/self destructive of CERN, in my opinion.

8. **Shantanu**
April 4, 2013

This hype maybe due to the amount of trouble/ difficulties/delay
this experiment faced.

9. **Natalia Kiriushcheva**
April 4, 2013

Many news on AMS-02 results start with mentioning $2bn. Is it too big to fail ?
http://gravityattraction.wordpress.com/2013/04/01/boltzmann-brain-discovery/

10. **Bernhard**
April 5, 2013

And, as this nonsense goes around the world and is translated to other languages, the level of hype increases through a sort of Chinese whispers effect. One Brazilian newspaper states "Scientists detect existence of dark matter for the first time":

http://www.diariodepernambuco.com.br/app/noticia/ciencia-e-saude/2013/04/04/internas_cienciaesaude,432320/cientistas-detectam-existencia-de-materia-escura-pela-primeira-vez.shtml

11. **Chris W.**
April 5, 2013

…and an AP story, via the Weather Channel…

12. **steve newman**
April 9, 2013

can someone refer me to a paper that spells out how these ams observations
connect to dark matter. the prl paper doesn't even contain the word 'dark'.
what do we know about dark matter other than its presumed gravitational effect?
how does dark matter relate to positrons? what would cause dark matter to
annihilate? i assume that the people commenting on this blog understand the connection, so someone please provide a link for those of us to whom this is all new.
thanks.

13. **Peter Woit**
April 9, 2013

Steve Newman,

For much more of a background explanation, follow the links I provided at the end to Resonaances and Matt Strassler's blog, both of which provide a lot of such discussion. The fact that the connection to dark matter is so slim that AMS didn't put it in their PRL paper, but that they then went ahead with a press release trumpeting such a connection, is exactly what the controversy is here.

14. **Low Math, Meekly Interacting**
April 9, 2013

Is CERN being faulted here for hyping, or for parroting Ting's hype? Statements he made in this release and via other sources seem to be wildly at variance with the skeptics. The latter complain that not only do these data provide nothing like firm evidence of dark matter, they may never be able to unless cosmic backgrounds are far better understood than they are at present. I'm not going to pass judgement, but the dissenters seem to saying, in a nutshell, that Ting is all but spreading falsehoods. It seems as if there's some effort shoot the message (or its couriers) and not the messenger, i.e. no one seems to be calling Ting out directly. Deference to a Laureate? I don't know if my impressions are mistaken, but that's the general picture I'm getting.

15. **Peter Woit**
April 9, 2013

LMMI,

I think there's plenty of blame to go around here. Ting is known for running a tightly-controlled operation, so he likely approved the press release. The CERN press people however are not random journalists but often physics ph. ds, and should have been aware that dark matter hype was a danger here. Both parties had to agree to put this out, either one could have stopped it.

16. **El-Coco**
April 10, 2013

For the record: Hawking gave a pro M-theory talk yesterday in Los Angeles. http://www.latimes.com/news/science/sciencenow/la-sci-sn-stephen-hawking-cedars-sinai-20130410,0,7704203.story

17. **Bob Jones**
April 11, 2013

Some sad off-topic math news:

http://avzel.blogspot.com/#__sid=0

18. **Peter Woit**
April 11, 2013

Bob Jones,

Sorry to hear that. I see that there will be a previously planned conference, now in his memory, in a couple weeks at Northeastern, see

http://www.math.neu.edu/~bwebster/ACRT/index.html

19. **Low Math, Meekly Interacting**
April 15, 2013

Maybe next week's hype? Fundon, or the real deal?

http://www.sciencenews.org/view/generic/id/349712/description/Dark_matter_detector_reports_hints_of_WIMPs

20. **Low Math, Meekly Interacting**
April 15, 2013

Sorry, should have checked out Resonaances first before bothering you…

21. **Peter Woit**
April 16, 2013

LMMI,

Besides Resonaances, some of the news stories are fairly reasonable, see for instance
http://www.huffingtonpost.com/2013/04/15/dark-matter-found-underground-detector-particle-physicists_n_3086829.html

which quotes Cabrera about the experiment

"We're not claiming anything,"… The finding "does not rise anywhere near the level of discovery, nor does it rise anywhere near what we would call 'evidence for,'" Cabrera said. It is, however, a "region of interest" for future study.

This isn't much of a signal, and arguably inconsistent with other results. Best to ignore, if you ask me…

To Mars and Beyond

April 14, 2013
Categories: Uncategorized

I'm not sure either of these stories from the past week is particularly important in and of itself, but since I try and keep up on trends in theoretical physics, and two is a trend, here's some news from two of the greats of the field:

- There's an interview here (via John Baez) with Gerard 't Hooft about his role as "ambassador" for the Mars One project, which plans to send people on a one-way trip to Mars in 2023. This will be financed with an associated reality TV show, and already 40,000 people have signed up for a chance to get to go.
- Stephen Hawking has even more radical ideas, which he talked about in a visit to Cedars-Sinai in LA last week. He believes humanity is guaranteed to trash this planet, so our best hope is to use M-theory to find a way to move on to another one:

> For him, the answers to the largest and tiniest questions lie in M-theory.
>
> "To understand the universe at the deepest level, we have to understand why is there something rather than nothing," Hawking said, speaking through a computer program that converts his eye and cheek movements into spoken speech. "Why do we exist? Why this particular set of laws, and not some other? I believe the answers to all of these things is M-theory."
>
> The theory, he said, combines multiple ideas about math and physics. It suggests that there are multiple dimensions or universes, and offers solutions for the behavior of super-massive black holes and the properties of the fabric of space-time. M-theory is a work in progress, but Hawking said he believes that it's the most promising lead to a unified theory.
>
> The payoff to solving M-theory, Hawking said, is understanding where we fit in — and, perhaps, how we can thrive.
>
> "We must continue to go into space for humanity," Hawking said. "We won't survive another 1,000 years without escaping our fragile planet."

Update: On Tuesday Hawking gave a talk to students at Caltech, with a report here that includes smuggled audio of the talk. Evidently Hawking told students that they don't need God, but they do need M-theory and anthropics:

> During his talk, he cited M-Theory — a wide-ranging and as-yet-incomplete explanation of the universe that attempts to unite the factions within String Theory — as the only workable theory going forward that can explain the true nature of the cosmos.
>
> M-Theory suggests that the multi-dimensional "strings" of the universe are bound together by a strange material sometimes called membranes, but also known by other names. It suggests that matter, space, time and every possible history exists simultaneously across dimensional planes that were created out of nothing at the moment of the Big Bang some 13.8 billion years ago. Only in very few of these dimensions can a species like humanity come into being.

Comments

1. **NLR**
   April 14, 2013

   This is absurd. The whole point of going to Mars is to get away from all the nonsense going on on Earth. If they have a reality TV show then we're just bringing it with us. Also, perhaps the reason to study M theory (or any other fundamental theory) is not, as Hawking suggests, to help humans thrive, but to learn about something about an aspect of reality that has nothing whatsoever to do with humanity and that we cannot tamper with.

2. **Peter Woit**
   April 14, 2013

   I think the main problem with the reality TV show would likely be too much reality. When an oxygen/food/waste processing system fails and spare parts are 140 million miles away, the show might still have viewers, but maybe not advertisers.

   I don't really understand Hawking's logic. Either humanity can't be stopped from trashing its environment, in which case we should hope that M-theory won't allow us to leave and go wreck the rest of the universe, or this can be stopped, in which case working on that should be the goal, not making plans for leaving.

3. **CIP**
   April 14, 2013

   This reminds me of a conversation which a couple of slightly famous physicists had after seeing a lecture by Eddington in his eccentric age:
   Physicist A: "Oh my gosh, is that going to happen to us when we get old?"
   Physicst B: "Don't worry, a genius like Eddington may go nuts, but guys like you just get dumber and dumber."

4. **Maciej**
   April 14, 2013

   I coudn't agree more with CIP. Physics is not about "why laws of Nature exist" but about "what they are". Hawking clearly confuses that, which is embarrassing. This is not the first time he does it.

   Let me also add something else. M-theory will not, of course, answer the Leibniz's question "Why there is something rather then nothing". There is a good reason for that namely, M-theory has got nothing to do with our Universe (except the fact that some people study it – which is ok I think).

   Imagine however (but this requires a huge doze of imagination) that M-theory describes our Universe. But M-theory itself assumes some laws e.g. principles of quantum mechanics. Therefore it will not be able to explain them anyway.

   The statement that any, not only M-theory, can answer Leibniz's question is in my opinion close to lunatism.

5. **GM**
   April 14, 2013

   I don't understand Hawking's logic either.

It is absolutely true that we're on a path towards self-inflicted extinction and we're going to take most of the rest of the planet with us. The main reason for this is that we have exceeded the carrying capacity of the planet, which is a problem that could be solved in two ways – (1) we reduce our population and consumption so that we are back safely within the carrying capacity of the planet, or (2) we find an unlimited source of energy that would allow us to solve most of our environmental problems and colonize the rest of space. As far as we can tell at present, (2) is physically impossible and if there is a way to somehow circumvent the laws of physics as we know them, or perhaps find out that they are in fact different, it will take many decades, centuries, maybe thousands of years to do the research and figure it out. Unfortunately, the projected timeline for the coming global Malthusian catastrophe is much shorter than that, and if that happens, not only is that research not going to be done, but with quite a high probability, we will lose a lot of the knowledge we have worked so hard to acquire. I don't expect string theorists to fare well in the kind of world we can expect after the collapse of industrial civilization, and if anything, Hawking himself, who would not even be alive if it wasn't for the very complex support system around him, is the epitome of how dependent the existence of the scientific elite of today is on its largess. So the only practical solution is (1) so that we can give ourselves a chance to take a shot at (2) in the indefinite future, fully knowing that the chances of success are quite small.

However, claiming "M-theory will solve the problem" seems quite counterproductive to me, because most people will simply take it as another magic solution, when they should instead face reality and do what we know will work.

6. **Bernhard**
   April 14, 2013

   "Why do we exist? Why this particular set of laws, and not some other?"

   Hawking forgot one important question:

   "What is M-theory?"

7. **the sleep of reason**
   April 14, 2013

   What is more likely: Stephen Hawking believes M-theory will help us colonize space, or a journalist misunderstood something? The article has already had one error corrected.

8. **Peter Woit**
   April 14, 2013

   the sleep of reason,

   The idea that advances in fundamental physics far in the future will provide new technology making things like long-distance space travel feasible isn't exactly an unusual one among physicists. For Hawking or anyone else putting their money on M-theory as the source of such advances, it's not implausible they would suggest this as a possibility.

9. **N.**
   April 14, 2013

   Hawking is right. Not about theM-theory, methinks, but this planet is fragile indeed. It takes just a piece of space junk weighing a couple of thousant tons at ten clicks per second or maybe a real good blowout of a supervolcano like Yellowstone to make us and the rest 90% of living matter join the dinosurs – history. It has happend before, it will happen again.

   It's not if, it is when. And if, perhaps, we may have means – in the next 100yrs maybe – to divert the naughty asteroid, there is no way to stop a supercaldera exploding.

   The only solution is to move on, to colonize planets.

   We do not need the M-theory (or N, O, P, Q…), what we need is warp drive!

   So, my dear theoretical physicist, get to work!

   I am only half joking.

10. **N. Nakanishi**
    April 14, 2013

    Prof. Bernhard:

    Of course, M-theory means Mad theory!

11. **emile**
    April 14, 2013

    There is something I'm missing here: if a supervolcano explodes, or we get hit by a big space rock, or we pollute the planet to the point where life is threatened, we'd still be in better shape here than on Mars. Not easy to make Earth more of a hostile environment than Mars.

12. **John**
    April 14, 2013

    Emile

    I think Venus would be worse than Mars, and also more likely than any of the other cinematic disasters listed if you listen to James Hansen.

13. **CIP**
    April 15, 2013

    @N

    I think you are off by a factor of a million or so on the size of the required cosmic interloper. The recent Russian meteorite was several thousands tons, I think.

14. **Nathalie**
    April 15, 2013

Hawking is a great physicist who, unfortunately, never fails in failing to predict the future of physics. A good example of his failed prophecies is found in his 1980 Inaugural Lecture "Is the End in Sight for Theoretical Physics?"

15. **Kasuha**
April 15, 2013

The fragile thing on this planet is the humanity and that's also the thing we need to care about. The planet doesn't care what we do with it, it's just a ball of cosmic dust. We need to care about what we do with it and how to improve our chances of survival in this hostile universe.
Finding a way how to travel to distant worlds will not save our planet and will not save people on our planet. Only tiny fraction of people will be able to travel that far and they'll most probably never return. But finding other planets to inhabit will improve chances of intelligent life and life in general persisting. And that's the ultimate purpose and reason why life is here at all.

16. **Paul Titze**
April 15, 2013

Stephen has mentioned before in past articles that things will get bad for us in the future (200 years onwards would be more accurate though). Think Mysterious-Theory isn't the solution either. To "thrive in space" one needs to be able to get large amounts of hardware into orbit without relying on chemical rockets, that would be a good start.

Cheers, Paul.

17. **Volunteer**
April 15, 2013

Is NASA proceeding with an M-theory spaceship? There are probably a few technical challenges to overcome.

18. **Cloud**
April 15, 2013

A good theory about M-theory.

…

[If] humanity can't be stopped from trashing its environment, [then] we should hope that M-theory won't allow us to leave and go wreck the rest of the universe …

I don't think this follows. The environment is being trashed because Earth is overcrowded — it turned out, topologically, to be a closed surface, whereas the 'blind watchmaker' essentially made humans (or any mammal) to treat the world as an unbounded plane.

So if we did have 'warp drive' (or 'tesseracts'), we could always move on to other planets before the colonies get crowded. The colony planets would for the most part not get trashed like Earth.

But generally I'm a pessimist, because there (almost surely) is no warp drive, and the *decline* (not sudden collapse, note) of industrial civilization will not magically transform people into gaia-loving eco-nomads. If you want a picture of the globe five centuries hence, think: almost all biodiversity destroyed, all old-growth timber consumed, and society a mass of peasants malnourished on maize and rice diets, ruled by a small mandarin class.

19. **Cloud**
April 15, 2013

As for physics, I think we can safely predict its continued ossification into what J. Horgan calls 'ironic science': into a post-empirical, doctrinaire, even theological mode.

20. **Stephen**
April 15, 2013

Hawking's oversight is that figuring out M theory, like other advances in physics, would simply give us more efficient means of destroying the planet.

21. **Michael Gogins**
April 15, 2013

Surely the desire to understand the laws of Nature and the desire to explore and colonize interstellar space can be pursued at the same time, they do not conflict at all. Knowing more physics would surely help with space travel, and surviving a planetary catastrophe in colonies, whether that catastrophe were self- or other-inflicted, would surely help with fundamental physics.

Ecological problems caused by us here on Earth have been going on since we started killing large mammals and cutting down forests. This is a real, terrible problem that is getting worse fast but it is not at all likely to kill us all off, or even cause a fall back in technological level. Even a substantial decline in human population is very unlikely to cause a fall back in technology. If there were any big wars, that would probably even speed things right up again. A ridiculously tiny number of scientists, engineers, farmers, and manufacturers are keeping this immense wheel spinning right now, and the knowledge they need is scattered all over the world and will never be destroyed.

No, the real question here is one that has not yet been answered properly, what kind of world shall we build for ourselves to live in?

22. **johnnythelowery**
April 15, 2013

Bill Stone has already volunteered to go with a one-way propellant payload to Shackleton Crater on the moon. Because it costs @ $20,000 per KG to get into space, and that 90% of the weight of space craft is the propellant, what is needed is a gas station in space. HE is proposing collection of hydrogen and storage of it in orbiting inflatables. Here's his TedTalk.

http://www.youtube.com/watch?v=-Bn6Gel7yEs

23. **Spencer Tracy Jr.**
April 15, 2013

The bad news is that the mission is tied to reality TV. The good news is that some reality TV enthusiasts will be leaving the planet. If the ship were big enough to transport all involved in reality TV, Stephen Hawking might have a little less to worry about here. (However, Elton John once said: Mars aint the kind of place to raise your kids –

in fact it's cold as hells).
The part I don't understand is how can it not be cheaper and easier to perfect a meteor/asteroid defense system here than to pack up and move to another planet that by the way, they can guarantee will have a far less chance of being in the path of something?
Lastly – I must say that I did have the opportunity to talk quantum mechanics with Gerard 't Hooft a couple of years ago after one of his talks in NY. Very nice guy, very generous with his time.

24. **Not a Physicist**
April 15, 2013

It does seem that advances in fundamental physics, while they may seem entirely removed from practical application at their time of inception, may lead to real-world applications soon therafter that may facilitate our exploration of the universe. For example, quantum mechanics aided in the development of the transistor, which was essential to the Apollo missions. Thus, while perhaps M-theory in particular may not prove useful, cutting-edge physics in general will surely find practical application, perhaps in space travel, at some point in the future.

25. **dark**
April 15, 2013

Does Stephen Hawking have good science-based reasons to endorse M-theory, in light of the null results @ LHC?

26. **Bob Jones**
April 15, 2013

"Does Stephen Hawking have good science-based reasons to endorse M-theory, in light of the null results @ LHC?"

At the moment, M-theory does not say anything about physics at the LHC or any other experiment, so these experiments are not a reason for studying this theory. However, I do think there are good reasons for studying M-theory. At the very least, it's a unique extension of quantum field theory and a rich source of ideas in physics and mathematics. In particular, it's closely related to a quantum field theory in six dimensions with (2,0) supersymmetry. This theory is responsible for fascinating dualities between various mathematically interesting QFTs, and these ideas have important applications in pure mathematics.

27. **Maciej**
April 15, 2013

Bob,

the statement "M-theory is a unique extension of QFT" is a wishful thinking. First define M-theory please. Does it (the theory) exist anyway? (I mean, can one construct it by specifying the field content and their interactions explicitly?). Then explain what do you mean by "extension". Then we can talk about uniqueness if you like.

28. **Suzanna E-J**
April 15, 2013

I feel for Hawking's sense of urgency. He has a devastating disease and the only available treatment is palliative care. Clearly he was very impresssed with the persistence and innovation of molecular biologists investigating ALS at Cedars-Sinai labs. Their ongoing work will benefit future patients. His argument was that fundamental physics should should also pursue a unifield theory with just as much vigour. But I don't think you have to solve the unified theory first before you can appreciate humanity's place in universe! One can begin to apprecaite humanity's place right now. For example, Feynman, Hawkings, Weinberg, Smolin and of course Peter Woit etc have done much to explain their work to a lay non-technical audience. No mater how basic the understanding or politcal inclinations, at this time people know that there is a universe, there are galaxies, we live in the MWay galaxy, space is expanding, we have ongoing telescopic surveys of astronomical objects and that a unifed theory is ultimate goal etc
There needs to be an even bigger engagement between scientists and the public. This is not just restricted to cosmology/particle physics but in every other scientific discipline. So I agree with Hawkings that we can inspire each other and we can step up research in our respective fields. But Hawkings should realise that stem cell research would not be a promsing area of research if did not pass at least the proof of concept. Mtheory struggles with this aspect.

To Stephen who commented:
Hawking's oversight is that figuring out M theory, like other advances in physics, would simply give us more efficient means of destroying the planet.

Sounds very Grothendieck-esque! Alexander Grothendieck's disgust with the mathematical community stemmed from a distorted view of the application of science. But he could have used his academic position, charisma and intellect to pioneer solutions to these concerns. Hope this is not off-topic but look at the recent controversy around the publication of the mutated influenza virus genome (the H5N1-H1NI hybrid). Two separate experiments showed that this hybrid enabled the virus to be highly transmsissable. The submission to Nature in 2011 sparked debate about 'dual-use' research which involves weighing up the benefits of sharing reasearch with the scientific community vs risk to public safety via rogue individuals. The matter was referred to a biosecurity advisory board in USA. The board ruled against publication initially but after some months decided that in the interests of public health both manuscripts can be published although one of the manscripts had to go through a more revised version. This example shows that although difficult, there are ways to handle scientific reasearch that fall in the dual-use category.

29. **Bob Jones**
April 15, 2013

Maciej,

What I'm saying is that if you want to extend quantum field theory by replacing point particles by extended objects, you will inevitably be led to string and M-theory. There are five a priori different ways of doing this consistently, and they all turn out to be related by duality.

Of course nobody knows how to rigorously construct M-theory, but there are lots of partial results. My point is that we can learn a lot about quantum field theory and mathematics by unraveling the structure of this theory.

30. **Raizonator**
April 16, 2013

For those who understand German:
http://www.wissenschaft.de/wissenschaft/inhalt/aktuelles_heft.html
with an interesting interview with E. Witten on M-theory, the landscape, etc.
E.g. "Was halten Sie von der Hypothese der Stringlandschaft?
Ich wäre froh, wenn es keine Stringlandschaft gäbe. Denn ich bevorzuge Einsteins Vision, dass es möglich ist, alle dimensionslosen Quantitäten der Natur aus ersten Prinzipien zu berechnen."

31. **Carl**

April 16, 2013

The sun is going to trash this planet anyway. Good that people are thinking about ways
to leave and save some of the life on Earth. It is shame that some people here think it is ok to let millions of years of evolution go to waste. Btw, humans are part of nature and whatever humans do is also part of nature.

32. **Tmark48**
April 16, 2013

M Theory is not going to help us engineer "Bussard Engines" nor build generational ships that will give us the possibility of real interstellar travel. Let science do its thing, and keep "physicists having jumped the shark" hidden lest they ridicule themselves. But maybe they don't care anymore…

33. **Tammie lee Sandoval**
April 16, 2013

Look at what happened to global warming. They made 15 year predictions. Dumb move.

By contrast, Dr Hawking has shown deep wisdom
When you predict a disaster, to get funding today, make sure the disaster in far in the future. 1000 years, Dr Hawking should be safe.

34. **Peter Woit**
April 16, 2013

Raizonator,

Thanks, I'd be curious to see the whole thing. It's not surprising that Witten says he'd prefer no landscape, but I wonder whether he would be willing to abandon string theory if convinced it led to a landscape with no predictions.

35. **Giotis**
April 16, 2013

"…so our best hope is to use M-theory to find a way to move on to another one"

I guess this is your interpretation. Such assertion is nowhere stated (or even implied) in the actual text.

I noticed that ever since Hawking embraced M-theory as the most promising way for a unified theory, you try to present him as an unreliable eccentric fool.

What you do is not right…

36. **Peter Woit**
April 16, 2013

Giotis,

What I wrote seems to me a reasonable interpretation of Hawking's talk, as described in the article (I don't have access to an actual transcript, or something more detailed). In any case, as usual, I quoted completely the text I was talking about, so people can make up their own minds.

No, I don't think Hawking is an unreliable eccentric fool. His views on M-theory are not eccentric, but widely held, and shared by many very smart, rather conventional people. I happen to think he's wrong that M-theory, the landscape and anthropics have any promise as a unified theory and that he's unwise to promote them to the public, for scientific reasons that I've often explained here.

About his views on humanity needing to leave this planet, again, I think such views aren't eccentric at all, but widely held by people who worry about the distant future of humanity. My own attitude is that it's best to worry about the immediate future, then the distant one will take care of itself, but, to each his own…

37. **QSA**
April 16, 2013

QG theories (string, LQG,AS…..) do not address full unification properly(including space-time), the origin (prediction) of the SM constants or the nature of QM. So I think these theories need to be saved first before they save us.

But anyway, I don't think earth is the problem. It is the people, you know, you can't live with them and you can't live without them. There will be a problem even if each person had a mountain of gold.

38. **Not a Physicist**
April 16, 2013

Perhaps Hawking was not referring to M-theory in particular when he commented on future space travel. Rather, he might have been referring to advances in fundamental physics in general. It seemed from the interview that he was simply emphasizing that a general conception of our place in the universe can aid in future technology. I don't think he was referring to string theory in particular but rather to fundamental physics and cosmology in general. Perhaps there was a slight misunderstanding on the part of the journalist.

39. **Peter Woit**
April 16, 2013

Not a physicist,

I don't see any reason to believe the journalist was misunderstanding Hawking (for one thing, because of the challenges he faces in communicating, Hawking is careful in choosing his words and in making sure they are understood). From the article it seems to me that Hawking was just saying two speculative, but not very surprising things: maybe advances in fundamental physics will provide technology that makes colonizing space possible, and M-theory is his current best bet for where advances in fundamental physics will come from. He wasn't saying anything about having a specific idea along these lines, just engaging in some speculation of a sort that seems to me not at all unusual among physicists.

40. **G P Burdell**
April 17, 2013

As someone who knows a bit about how Reality TV is produced, shot, and edited: those shows are mostly a sham, often semi-scripted, even employing stunt doubles if necessary. Locations are scouted and prepped before any of the “stars” arrive. If there are kooks who think the moon landings actually took place on a stage in the Nevada desert — maybe this Reality TV Mars “trip” really will! If not, there are going to have to be a whole bunch of camera, makeup, hair, and lighting crew going along on the ride. Sort of adds to the weight of the payload.. 🙂

As for those who worry about the Sun wiping out humanity — the only life that has lasted anywhere near as long as we have before that happens are bacteria. I expect that evolution will have taken care of wiping out pretty much any recognizable traces of the human genome well before then. As for the probability of teraforming Mars, or any other planet or moon in our solar system, into someplace even remotely habitable for humans: seems incredibly remote. And if all you could do was live out your life in some extra-terrestrial underground bunker, might as well do that here on Earth. We seem to forget that Star Trek is science fiction in more ways than just warp drives and teleporters.

41. **Not a Physicist**
    April 17, 2013

    G P Burdell,
    I do not think that Feynman was referring to initiating a mass “migration”, so to speak, of humans to places outside of Earth. Rather, I think he was talking about having a small colonization population outside of the Earth, perhaps of a few hundred space colonists or so. As such, it would not be nessesary to terraform an entire planet to make its surface habitable for a large number of humans; all that would be needed is a small space colony. And I don’t think he was talking about the end of the sun, either, when he talked about the end of humanity; he was probably referencing nuclear war, or some other human-caused threat. As such, it would always be useful to have a small “space colony” population elsewhere to ensure that humanity continues to exist, even after catastrophic events.

42. **Not a Physicist**
    April 17, 2013

    Error in previous comment-replace “Feynman” with “Hawking”.

43. **tommyboy**
    April 30, 2013

    I agree with Hawking and disagree with those who fail to see the importance of his point regarding the colonization of space. Let me illustrate why this is so with the following line of thought: Peter Woit presumably enjoys his profession/subject area, values a future continuation of his career in research and academia, takes pride in his blog, and the same can be said of most of us in our respective life situations. Further, Peter Woit would probably continue to behave in a way that promotes and maintains the things he values most in life. Different people may value other aspects or facets of living, but the one thing most of us have in common is that we strive to uphold our existence despite the challenges involved. In fact, challenges are what make life exciting and worth-living to a great many out there. Also, where there is life there is possibility. Thus, what could be more important than the civilization using at least some resources to ensure that the death of possibility itself does not occur, as human extinction represents the ultimate death of possibility: no more literature, no more science, no more philosophy, no more fine art, no more blogging, no more debating/sharing ideas, no more love, and so on. Species that remain in one area exclusively for too long eventually succumb to an extinction event—I think part of what Hawking is saying that human beings are in a unique position to prevent this from happening to our species and we should be actively pursuing this agenda and, of course, not to the exclusion of others.

Anderson on Anderson-Higgs

April 16, 2013
Categories: Uncategorized

Philip Anderson was here at Columbia yesterday, and gave a very interesting talk, mostly discussing what was going on in the late 50s and early 60s at the intersection of condensed matter and particle physics. This has attracted a lot of interest around the question of who first came up with what is now called the "Higgs mechanism" and who first predicted a "Higgs particle" (I've written a long blog posting about this here).

After the discovery of the BCS model of superconductivity, Anderson did important work on understanding the "gauge problem" of how gauge symmetry acts in such a theory, publishing a series of papers on this in 1958. He joked that he was "pretty naive about field theory" at the time, so much so that the spelling he was using was "guage". He had the advantage of regularly talking with Bardeen and with Nambu, and he described some of Nambu's work on the so-called "Nambu-Jona-Lasinio" model. His 1958 work explained how one avoids getting massless Goldstone bosons in superconductors due to the singular long range nature of the Coulomb force. His talk included an anecdote about escaping from handlers in the Soviet Union to get a chance to explain this to Shirkov during a visit there (he wasn't allowed to meet Bogoliubov).

He was at Bell Labs in the summer of 1962, and talked to J.G. Taylor, who told him that the problem of massless Goldstones was something those in particle theory were actively worrying about. Taylor also gave him a copy of Schwinger's Gauge invariance and mass paper, which had been published that January. This led to Anderson's Plasmons, gauge invariance, and mass paper, finished in November, and published in April 1963. This paper clearly explains the nature of what is now generally referred to as the "Higgs mechanism", in the Yang-Mills case, not just the Abelian case, ending with the very modern point of view

> We conclude, then, that the Goldstone zero-mass difficulty is not a serious one, because we can probably cancel it off against an equal Yang-Mills zero-mass problem.

In 1964 the papers by Brout-Englert-Higgs-Guralnick-Hagen-Kibble appeared that have drawn the most attention as earliest instances of the Higgs mechanism, but Anderson had the correct idea a couple years earlier. He described the situation as one where he and the 1964 authors all had the right explanation for why the W and Z have mass, although none of them (including him) had the actual physical Higgs particle, which he claimed first appears in a 1966 paper of Higgs.

The main point of his talk was the fruitful nature of research at the intersection between problems in condensed matter and particle theory, with the 50s-60s a happy period of such work. He ended with some comments on "supersolids", see his recent paper about this here. Anderson will be 90 years old later this year (he's almost exactly the same age as Freeman Dyson) and it was great to see him still going strong.

Update: See here for a write-up by Anderson of a talk a few years ago covering much the same material as yesterday's Columbia talk (including the story of meeting Shirkov).

Update: A commenter points to this recent talk by Guralnik at Brown and mentions some comments that might be about Frank Close.

I just watched the talk, and he explicitly refers to this exchange in the London Times. I don't see how when he says

> the person involved as far as we can tell has no understanding whatsoever of mass renormalization and how these things work.

this can refer to anyone except Close (about whom it is completely absurd). The point of contention here is a very simple one. Guralnik's paper (unlike Higgs's) has no potential term for the scalar field. In his talk he says this is because it was the practice at Harvard not to write such terms down, while knowing they had to appear to renormalize the theory. Close's point I think is just that Higgs went further than the other authors at the time in terms of exhibiting what would be needed to study the dynamics of the physical mode of the scalar field. From what I can tell, of course everyone writing these papers knew about potential terms for the scalar and how they worked, but the whole issue is a bit irrelevant: none of the people involved at this period seem to have thought seriously about this physical mode that describes the Higgs particle itself. They were thinking about something entirely different, the mass of the gauge field, and in any case these are Abelian models that have nothing to do with the real non-Abelian model that describes the Higgs particle.

The main point of Guralnik's talk as far as this controversy goes I think is his explicit and repeated claim (which seems to me debatable) that Higgs's paper was just wrong for technical reasons, in the sense of reaching correct conclusions by an incorrect argument. This appears to be Guralnik's argument for why he and his collaborators should be preferred to Higgs as candidates for a Nobel, and is made much more strongly here than in other places (such as here, where he doesn't use the term "wrong", emphasizes more why Higgs's argument was "incomplete").

Leon Cooper was in the audience, and asks Guralnik about the Anderson explanation for why the Goldstone theorem is violated here: the long range nature of the Coulomb potential. Guralnik seems to acknowledge that this is the right physical way to understand what is going on, but says that relativity makes things more complicated. He explicitly acknowledges that he understood not at all Anderson's arguments, saying

> we were woefully ignorant, had barely heard of superconductivity.

About the crucial question of priority, the fact that his competitor's papers were published earlier, were read by him before submission of his paper, and explicitly referenced in his paper, all he says is

> as we published it, we found out about other papers.

which really doesn't do justice to the situation. In this piece, written after the Higgs discovery, he describes the history as

> We finally submitted our paper to PRL with the proof of the general mechanism to avoid the Nambu Goldstone theorem (the only work to have this) and the special example. We were surprised to discover that two very different but related papers, with parts of the example, one by Englert and Brout and the other by Higgs also existed. All three papers appeared in the same volume of PRL in 1964.

which is highly misleading.

Comments

1. **Garrett**
   April 16, 2013

   Thanks for sharing this about Anderson. (I included him in my first slide in this short talk on the Higgs: http://www.youtube.com/watch?v=wfALJzn1hE8) I hope he is strongly considered for the prize.

2. **emile**

April 16, 2013

I would like to point out that Anderson did his best to make sure that the Higgs boson would not be found, at least in the US. Given his stance against high energy colliders, it would be highly ironic if he got a Nobel prize because of them… If he does get the prize, the Nobel committee better find a way to give it also to experimentalists who actually found the damn thing.

3. **gimmedatprize**
April 16, 2013

I was going to post very much the same thing as emile. Anderson says that in the 1950-60s he was one of several people working on the intersection of condensed matter and high-energy physics. But, in later years at least (for example in his attitude to the supercollider), Anderson has been one of the leading lights of condensed matter people opposed to particle physics.

4. **Yatima**
April 17, 2013

> that the Higgs boson would not be found, at least in the US.

That would be the Clinton administration, killing the SSC, right?

Note that Murray Gell-Mann received a Nobel prize for something he didn't believe in for quite a long time, too.

And really, who cares whether the "Higgs is found in the US"? I'm getting flashbacks to the shenanigans and political interference of the highest order to prove that the AIDS virus was "found in the US". It was all made-up of course.

5. **Bernhard**
April 17, 2013

Well, it is true that Anderson spoke against the SSC and this ultimately greatly contributed to killing the project. However, this does not change the fact he did understand the mechanism before others. I think Jester once put this quite nicely in

http://resonaances.blogspot.se/2012_03_01_archive.html:

"However, the name of Higgs somehow stuck, probably because it's cute, or maybe because we all hate Anderson for cutting the throat of the SSC."

Scientists are not immune to hatred, nationalism and others "isms" but, as best as we can, we should try to be. As I see, it is clear Anderson nailed it first, so everything else he might or might have not done should not be relevant….

I know that in the end this will be taken into account and the Nobel committee is certainly not made by neutral people without bias. However, this is what we should strive for. We should speak in favor of Anderson despite "hating his guts".

6. **emile**
April 17, 2013

@Yatima: 1-Congress killed the SSC, not the Clinton administration who had funded the project in its budget (just like the Reagan and Bush Sr. administrations). 2-Your comment on Gell-Mann would be relevant if he had opposed the construction of machines to prove or disprove the existence of quarks. 3-Indeed, where the Higgs was found is not the issue. The issue is that Anderson opposed funding colliders. Now, there is no high energy collider in the US: PEP, the Tevatron, all shutdown and US colleagues will have to go out of their country for the next 20 years (at least).

@Bernhard: I don't want to take away from Anderson's achievements. I've given many talks on the Higgs boson and I have always mentioned his name in conjunction with Higgs, Brout, etc. He is an outstanding physicist and I find it inspiring that he is still going strong at 90. That said, he was opposed to funding experimental particle physics, and worked actively to kill the SSC. That should also be part of his legacy (I'm not blaming him alone for killing the machine of course…).

7. **Low Math, Meekly Interacting**
April 17, 2013

I've always been very conflicted about Anderson for the role he played in the SSC debacle (I lack the ability to comment intelligently about his contributions to physics). That said, virtually everything he has ever written about science and science funding that was geared towards a lay audience struck me as virtually unassailable. His point seemed to be that the size of the pie is limited, and there's no compelling argument for experimental HEP getting such a big slice at the expense of other branches of physics. My solution to that problem, if anyone cared about my opinion, would be to increase the size of the pie. But it was and is a zero-sums game, and those on the condensed matter side of the profession, it would appear to this layman, possibly had a legitimate complaint about priorities. That, and maybe they were sick and tired of being called "squalid staters", and relished the opportunity to strike back a bit too much.

In the final analysis, while I remain quite ambivalent about Anderson, I can't ascribe so much of the blame to him. Congress had the knives out already, and at most I think Anderson made it easier for them to play political football. I'm not sure how much guilt someone should bear for putting a fig leaf on the naked opportunism of purported budget hawks, who feel no remorse over their assault on science in general.

8. **Peter Woit**
April 17, 2013

LMMI,

The SSC was cut at a time when there was a huge campaign to cut the federal budget (for some reason this happens under Democratic presidents, under Republican ones you get big spending increases…). It wasn't just Anderson, much of the scientific community looked at the SSC, and at their field's budgets being flat or getting cut, and argued that the SSC should probably not be supported, with the funds redirected to other deserving causes (like their own research). I remember having to point out to many people at the time that if the SSC was cut, that didn't mean the money would get redirected to other scientific projects.

9. **Low Math, Meekly Interacting**
April 17, 2013

All too true. OK, virtually unassailable except for the sad fact that it can turn to a negative-sums game all too easily.

10. **Roger**
April 17, 2013

Anderson has a review of a biography of Freeman Dyson, and says he (Anderson) "sensed his [Dyson's] ambiguity about conventional liberal positions … most of which I hold unambiguously."

Clinton did sign the bill to kill the SSC in 1993. Congress was controlled by Democrats at the time.

11. **Peter Woit**
April 17, 2013

Roger,

Thanks for the link to the Anderson review of the Dyson book.

About the SSC, yes, but the Democrats in the Senate voted 29-26 against cancellation, the Republican voted 31-13 for cancellation.

The fact of the matter is that, no matter how much people try and turn this into yet another partisan issue, it wasn't one then, and current debates over more or less science funding are not partisan issues now, with Democrats and Republicans on both sides of these kinds of votes.

Please all, resist the temptation to engage in the tedious arguments now found everywhere about how everything is either, to taste, the fault of the Democrats or the fault of the Republicans. Mercifully, HEP funding just isn't a partisan issue.

12. **Yatima**
April 17, 2013

> and current debates over more or less science funding are not partisan issues now, with Democrats and Republicans on both sides of these kinds of votes

They are definitely "bipartisan", in a very, very bad sense. Anyone who wants to know more should grab a copy of David Stockman's "The Great Deformation", now on sale. Or one may reflect on the fact that the SSC with all its cost overruns is just 4 days of 2013 military activity. I'm too lazy to adjust for inflation of course.

I can only refer the esteemed readership to this brief writeup: The Decline and Fall of the SSC by John G. Cramer

13. **chickenfeed**
April 17, 2013

Don't try to compare HEP funding (or SSC cost) against military funding. As someone said (back in the 1970s) "The cost of US support for HEP (then annually $350M approx) is not chicken feed. The cost of chicken feed is about ten times that amount." So … starve the chickens to pay for atom smashers?

The military has its own priorities and reasons for its budget. The internet by itself is a military invention. (Yes. Read about ARPAnet.) Microwave ovens are also a (byproduct) military invention. Indeed bellbottoms are also a military (US Navy) invention.

The SSC was mismanaged from the beginning. But people such as Anderson, with his parochial attitudes, were significant.

14. **Peter Woit**
April 17, 2013

Please, all, if it's not about Anderson/and or the mechanism, it's off-topic.

15. **Hmunu**
April 17, 2013

Peter,

Anderson also has a rather terse response in this month's Physics Today concerning an article from last year in the magazine about applications of AdS/CFT correspondence to condensed matter systems.

Shorter: "You are welcome to continue to play in your sandbox with your toys as long as it doesn't bother the adults who are trying to get work done."

16. **Peter Woit**
April 17, 2013

Hmunu,

I wrote about that recently here

http://www.math.columbia.edu/~woit/wordpress/?p=5717

17. **Anonymous**
April 17, 2013

I think the only example of overt political bias in the Physics Nobel was Pascual Jordan being the prize for being a Nazi. I don't think Anderson's "crime" of opposing the SSC is enough for losing it.

18. **yet another anon**
April 17, 2013

Except that Anderson already has a Nobel.
SSC aside, he is a truly outstanding physicist,
and probably deserves a second Nobel anyway,
even if not for the higgs.

19. **SpearMarktheSecond**
April 18, 2013

I asked Anderson directly about his opposition to the
SSC. His answer was emotional, not rational. He
said he detested the hordes of experimental particle

physicists who didn't or couldn't think for themselves.

I had hoped he had a more thoughtful reason for his
opposition. Certainly the money slated to be spent on
the SSC didn't end up in condensed matter physics.
Maybe in the end the NIH got a big bump though…
not because of the SSC.

20. **Krzysztof**
April 19, 2013

Yesterday, Frank Close gave a CERN colloquium on the higgs saga, and offered his personal nominations for the nobels. In his opinion, Anderson should not be in the final three – see http://indico.cern.ch/conferenceDisplay.py?confId=243243

21. **Bernhard**
April 19, 2013

Krzysztof,

This was very interesting. The question is of course if Close´s interpretation of Anderson´s role (as in slide 54) is really fair or biased (whatever the bias is..).
For one thing, I would love to hear Anderson´s own thoughts on what Close is claiming.

22. **IM**
April 19, 2013

Polyakov in `A View From the Island' (arXiv:hep-th/9211140) says:

Sasha and I started to analyze Yang-Mills theories with the dynamical sym-
metry breaking and in the spring of 1965 came with the understanding that the
massless particles must be eaten by the vector mesons, which become massive after this meal.
We had many troubles with the referees and at seminars, but finally our paper
was published (Migdal–Polyakov [6]). We did not know, until very much later
about the work on "Higgs Mechanism" which has been done in the West at about
the same time, or slightly earlier.

[6] A. Migdal and A. Polyakov, ZHETF 51, 135 (1966).

23. **socrates**
April 19, 2013

One assumes Anderson was not too thrilled with the Higgs discovery?

24. **Peter Woit**
April 19, 2013

Kryzysztof,

Thanks for the link.

Bernhard,

The main argument against Anderson has always been that HEP people at the time thought relativity was relevant, and he didn't provide proof they were wrong. On the other hand, his claims about how you should understand the mechanism, that it was something where relativity wasn't relevant (you don't need antiparticles) turned out to be perfectly correct. The claims from the HEP side about the Goldstone theorem and relativity being crucial turned out to be wrong.

IM,

"at about the same time" isn't a very accurate way to refer to work done nearly 3 years earlier (Anderson), and more than 1 year earlier (Brout-Englert-Higgs). I have no idea what the situation was in the Soviet Union back then, but it's surprising they had no access to the Physical Review. The story from Guralnik-Hagen-Kibble about not having access to the main journals at the time due to post office problems is also rather odd.

25. **John**
April 19, 2013

chickenfeed
"The internet by itself is a military invention. (Yes. Read about ARPAnet.)"
Yes, but don't confuse the web and the internet.

Peter,

That relativity was not an issue had to be proved. It's not enough that it turned out not to be.

In any case, Anderson's is a lost cause.

26. **Peter Woit**
April 19, 2013

John,

What's odd about this is that traditionally the Nobel prize is not a mathematical physics prize. Ignoring the fact that someone had the proper physical understanding of the phenomenon and wrote a paper explaining it, in favor of later more technical work is hard to understand other than sociologically.

This would make more sense if one was arguing that Anderson didn't have the relevant model, but in this case the relevant model didn't appear until Weinberg-Salam. People are trying to make the case that a prize should be given not for the right model (which has already happened), but for the physical mechanism of how gauge-fields acquire masses. The mechanism is the same in non-relativistic models and relativistic models, so I don't see why you can justify ignoring Anderson's work on the non-relativistic model and his (accurate) insight that relativity wasn't relevant.

27. **John**
April 19, 2013

"What's odd about this is that traditionally the Nobel prize is not a mathematical physics prize. Ignoring the fact that someone had the proper physical understanding of the phenomenon and wrote a paper explaining it, in favor of later more technical work is hard to understand other than sociologically."

Yeah, tell that to 't Hooft.

28. **John Brown**
April 19, 2013

Guralnik addresses some of these points earlier in the month (April 2013) during a talk here at Brown – his home campus.

He compares the Englert-Brout, Higgs, and Guralnik-Hagen-Kibble papers and seems to directly take on the "blogs" and Frank Close. Guralnik explains what was wrong in the other papers.

http://www.youtube.com/watch?v=WLZ78gwWQI0

So either he does not get it or people like Frank Close don't get it. Maybe someone here can enlighten (on the physics not the politics of the Nobel).

29. **XYZW**
April 19, 2013

If so many people came up with the Higgs mechanism, then it doesn't matter who came up with it first.

30. **Lanny**
April 20, 2013

Just watched the Close video from CERN. He seems to change his view that it should be Goldstone, Higgs, and Kibble. See below link where he pushes that.

http://www.abc.net.au/radionational/programs/scienceshow/higgs-boson-to-reveal-the-mechanism-for-origins-of/2964744#transcript

That must have been "too Anglo" or someone at CERN had a "talk" with him to point out CERN (and t'Hooft) was pushing Englert hard. I vote for the later.

The Guralnik video poted seems to state Close (and some others) is not qualified to comment on this given is lack of understanding. Is Frank a flip-flopper or is he not well-versed in field theory? I don't kow enough about his "real" physics work.

31. **Peter Woit**
April 20, 2013

lanny,

Goldstone and Nambu were two of the most important figures in this story, so with Nambu but not Goldstone recently rewarded with a Nobel, it makes sense to try and fit Goldstone in. But, with Englert publishing first, there's a strong case for him, so I wouldn't be surprised if Close changed his mind back and forth on this.

I haven't yet watched the Guralnik video, but his main problem is that his paper was published later, after he and his collaborators had read the papers of Anderson, Brout-Englert and Higgs, putting too many people in line for the Nobel ahead of him and his collaborators. I don't think accusing Close or others who point this out of being ignorant is going to help his case.

32. **Nige Cook**
April 20, 2013

Frank Close: "So I asked Brout and Englert, why didn't you mention this [massive spin-0] boson in your paper? They said, well, we thought it was sort of obvious. But Higgs alone is the one who identified that in particle physics there will be a particular implication, and that I think is why he is rightly named for it. In fact Peter Higgs amusingly said to me, 'Oh Brout and Englert are quite right, it is completely obvious.' In fact, in the first draft of the paper that Higgs wrote he didn't even mention it, and the referee turned the paper down and then Higgs thought maybe I'd better think of some implications, and he added this extra implication which is now the boson. So if the referee hadn't turned the paper down, even Higgs might not have mentioned it."

33. **Lanny**
April 20, 2013

Peter:

Agree with you to some degree and thanks for reply. As for the videos, I should state that it is not explicitly clear Gerald Guralnik is talking about Frank Close (as he is not mentioned explicitly). I am merely assuming this as it seems to make the exact counter to the points in Frank's talk at CERN given a couple weeks after the Brown talk.

Close does not seem to making his points on timing on the 1964 PRL papers. This is probably sound given the independence, the different approaches, the references, and the timing of the papers have been discussed in many forums – including this blog. Guralnik discusses this in this paper http://arxiv.org/abs/0907.3466 (p. 18)

Close seems to be making his points around this "massive vs. massless" boson. Guralnik states that those making this point don't understand the details field theory – and challenges those to look closer at this mass calculation. He makes this point in the video (http://www.youtube.com/watch?v=WLZ78gwWQI0) and to a lesser extent in this paper http://arxiv.org/abs/1110.2253. Guralnik's points are over my head as I clearly don't have the depth in field theory to properly evaluate.

The other theme of the GG video is how it started from his thesis and a paper prior to the Higgs PL paper. Since there were errors in the earlier paper, they sat on the GHK paper longer than they would have otherwise.

At the end of the day, the Nobel committee certainly needs to determine if the prize should match the history or the statutes.

34. **Peter Woit**
April 20, 2013

Lanny,

I did just watch the talk and added some comments about it as an update to the posting. Guralnik was definitely talking about Close.

I think Close emphasizes the inclusion of the potential terms in the Higgs paper because he's arguing for a connection of Higgs's work to the recent Higgs particle discovery, making it a good candidate for a Nobel. The argument he is trying to counter seems to me more the following: these authors back in 1964 were only considering the question of the gauge field mass, and had no interest in the physical mode which is a U(1) analog of the relevant Weinberg-Salam Higgs now discovered, so rewarding them based on the discovery is not justified. About Guralnik et al. the priority question is very clear: having to choose three, there's zero chance of the Nobel committee deciding to award Guralnik, Hagen, Kibble the prize over Higgs and Englert. The one way you could see that happening I suppose is if a discussion of the physical Higgs mode was in GHK but not in Brout-Englert or Higgs, which is definitely not the case.

35. **Xu Jia**
April 21, 2013

Dear Peter,
Close's argument is weak, since none of the three papers can give an assertion whether the scalar boson should or should not have a mass, which is model dependent. The necessity of introducing a scalar of weak-scale mass may be reflected in the fact that the WW sccatering needs cancel unwanted infinities. This was realized even after the renormalization work was done. The Nobel is not given exclusively because someone said it "can" have mass. Hence Higgs doesn't have priority on this issue, but on chronology.
In my opinion the crucial thing is 't Hooft's use of the massive gauge. It is unprecedented. It's not saying that there is a scalar field interacting with the gauge field, but the gauge field is fixed on a mass dome. None of the three papers indicate this usage.
Anderson's physics is absolutely correct and relativistics is not relevant. But he is correct only in the frame of our limited understanding. I always guess that all this eating and giving stuff is just a language, and is incomplete (since it has no way to give the mass of Higgs itself). Maybe the Nambu-Goldstone boson is fictious. The gauge bosons have mass per se, and the scalar field is the leftover of their interactions. Maybe it relates to gravity, big bang, or something totally unexpected.

36. **Pawl**
April 21, 2013

Closely related to Peter's point about what people were concerned about at the time:

There's a curious subjective element in these disputes, in that to determine what constitutes a Nobel-worthy advance people are in effect arguing about what the state of understanding was in 1962-1964. On an active research forefront, this is going to be ill-defined. So here one person regarded the appearance of the massive (scalar) boson as so obvious not to need mentioning, but on the other hand was unhappy with another's assertion that obviously there are no relativity issues.

Perhaps a prize based on a change of understanding within a subfield would go to someone different from one based on where the essential idea came from.

I don't usually spend my time feeling sorry for Nobel committees…. Of course, maybe it in all fairness should go to another area entirely this year.

37. **Roger**
April 22, 2013

UK newspaper:
Brout-Englert-Higgs, SM Scalar boson or BEHGHK? Scientists push to rename Higgs boson particle (but the alternatives aren't too snappy) …
Rival scientists have launched a campaign to rename the so-called 'God particle', claiming that British physicist Peter Higgs does not deserve all the credit for the breakthrough discovery.
http://www.dailymail.co.uk/sciencetech/article-2312425/Brout-Englert-Higgs-SM-Scalar-boson-BEHGHK-Scientists-push-rename-Higgs-boson-alternatives-arent-snappy.html

38. **chris**
April 22, 2013

ah, the lofty world of academia, where only the factual truth matters and nothing else….

39. **Bernhard**
April 22, 2013

Roger,

Now they are starting the campaign? This should really not taking seriously, the name "Higgs boson" is here to stay for historical reasons already. I did not see any serious campaign to change it BEFORE the discovery, which makes me think they were all OK with giving Higgs the credit if the idea was wrong. I am all OK with calling the mechanism "Anderson-Higgs" but calling it the BEHGHK boson is really ugly. I hope people in HEP will just ignore this.

40. **bcs**
April 22, 2013

Did Bardeen and/or Schrieffer ever complain that 'Cooper pairs' were not instead named 'Bardeen brothers' or 'Schrieffer sisters'?

41. **Peter Woit**
April 22, 2013

bcs,

In this case, Cooper really was first, about the pairs. Wikipedia does suggest you can also call them "BCS pairs" if you want….

42. **Thomas**
April 22, 2013

To give a prize for the discovery of the Higgs boson/Higgs mechanism and not include Higgs as one of the recipients would obviously be difficult to explain to the physics community and the general public. In addition to that, there are perfectly good reasons for selecting Higgs, and I think the best one is not the one that Close mentions (the fact that he has a potential for the radial mode). At this point the best evidence we have for the "mechanism" in addition to the "particle" is that we have observed H->2 vector bosons with the predicted strength. This process was considered by Higgs in his longer paper, but not by anybody else.

Beyond including Higgs there are obviously many permutations, and I find it hard to distinguish between them (except, unfortunately, I see no plausible way of including Hagen and Guralnik). There is Higgs/Anderson/Goldstone (except that I am not sure if Anderson really needs or deserves a second Nobel), Higgs/Brout/Goldstone, Higgs/Brout/Kibble (based on Close's observation that Kibble added group theory, and Weinberg was influenced by Higgs/Kibble), …

43. **emile**
April 22, 2013

@Thomas: I agree that the second paper helps given that the experimental evidence shows this particle decaying to vector bosons. I think you meant Englert and not Brout since Brout passed away. The committee will need to find a way to acknowledge the huge experimental component to all this. If they can't give it to "CERN" they should give it to the CERN DG. That would at least ackowledge the experimentalists and accelerator physicists, without which this debate would not take place. Had we listened to Anderson, there would be no debate about who gets the prize either… So, to me: Higgs, Englert, and CERN (and not Anderson given his negative contribution to the experimental discovery).

44. **prizes**
April 22, 2013

There will almost certainly be separate Nobel prizes for expt and theory. Lamb and Kusch won in 1955 for expts which revealed significant QED effects. Feynman, Schwinger and Tomonoga had to wait 10 years till 1965 to get their Nobels for QED theory (and Dyson received no prize).

45. **Bernhard**
April 22, 2013

" they should give it to the CERN DG".

Spot on, emile. This is what I have been also defending as the only sensible way to acknowledge the experiments (one should not forget the LHC collaboration).

To give the prize to the spokespersons is nonsense, so one could give it "to CERN" and the DG could go get it.

46. **edin**
April 22, 2013

Look at this from the University of Edinburgh
http://www.ph.ed.ac.uk/higgs/brief-history

A paragraph is devoted to Anderson and why PWA didn't really understand what was going on. Brief mention of the other hopefuls.

47. **Peter Woit**
April 22, 2013

emile,

I definitely agree that first priority should be a prize for the huge experimental achievement of the Higgs discovery. To react to this discovery by ignoring experimental effort in favor of giving a prize to somewhat off the main point theoretical activity from 50 years ago would have priorities quite backwards. It's worth noting that the theoretical activity being discussed here was in some ways basically misguided: the people doing this were trying to come up with a theory of the strong interactions, not the weak interactions. It was only with Weinberg-Salam in 1967 that the right idea about the Higgs was finally found, and they already have their prize.

As I've mentioned before, I think this would be an excellent time for the Nobel committee to give up the "only individuals rule" and award the prize to CERN + ATLAS + CMS

48. **Peter Woit**
April 22, 2013

edin,

I don't think that text says Anderson didn't understand what was going on. It says he didn't explain why the Goldstone theorem didn't apply, which is true, but it's not clear to me that it should be prize-worthy to point out to some people who were not being careful that their argument wasn't any good. The text also says Anderson didn't discuss the radial mode, which Anderson doesn't claim to have done.

It's interesting that that text has:
"During October 1964, Higgs had discussions with Gerald Guralnik, Carl Hagen and Tom Kibble, who had discovered how the mass of non-interacting vector bosons can be generated by the Anderson mechanism."
which corresponds to Higgs's diary indicating he gave a talk in London about this just before GHK sent off their paper for publication. My understanding is that GHK claim this is simply not true, I've never seen the truth of this matter sorted out.

49. **M**
April 23, 2013

Certainty CERN experimentalists deserve a Nobel prize. Given that it is now clear that the "125 GeV resonance" is the Higgs, priority should be given to the less joung theorists. It would be sad if the semi-scientific issues here debated will be solved by passing away. Whoever the right theorists are, there is a ≈30%/year probability that this happens for at least one of them.

50. **King Ray**
April 23, 2013

Peter, if they give the Nobel to CERN+ATLAS+CMS, could each participant claim to be a Nobel Laureate, sort of like each member of a Superbowl winning team getting a Superbowl ring? If so, this could dilute the prestige of the award.

51. **Peter Woit**
April 23, 2013

King Ray,

They've been awarding the Peace prize to organizations for years, and you don't for instance see all the doctors in Medecins Sans Frontieres claiming to be Nobel Laureates. I don't think this is a significant problem, the difference between getting a Nobel prize in your name and belonging to a large organization that got one is obvious to everybody.

52. **prize4all**
April 23, 2013

The International Red Cross and other organizations have won the Nobel Prize. Only the Peace Prize has been awarded to organizations. All others have been awarded to individuals only. One can debate the prestige of the Peace Prize. But does every member of the Red Cross claim to be a Nobel Laureate?

53. **Arlene**
April 24, 2013

Getting pretty political now. Someone posted on this blog that Lars Brink (member of Nobel Committee) was at IC London in March for Kibble's 80th event, dinner, and private dinner…someone else confirmed this also

Is this something IC London pays for? Does the Nobel Committee pay for that travel to a Nobel promotion event? Given they don't let any documents get reviewed for 50 years it seems odd Lars would be so overt in attending such as event.

54. **Peter Woit**
April 24, 2013

Arlene,

I don't see why Brink or other members of the Nobel Committee shouldn't be attending gatherings like the one at Imperial. Actually it seems like a very good idea for them to be traveling and hearing directly from those people still around who were there at the time of these discoveries.

Time Reborn

April 23, 2013
Categories: Book Reviews

Lee Smolin's new book, Time Reborn, is out today. For more about the ideas in the book, see video of a talk here, and an interview here.

While I mostly vehemently agreed with what Smolin had to say in his last book, The Trouble With Physics, I find myself equally vehemently in disagreement with this one. On some of the topics covered, I'm indifferent to his arguments mostly as a matter of taste. While my views on human society are likely similar to Smolin's, I've never found the scientific insights of fundamental mathematics or physics to have anything significant to tell me about this part of life. Similarly, while I've spent some time studying philosophy, I've mostly found this of little help in gaining deeper understanding of math or physics. Others though have a very different experience than me, and I'm not about to argue against people looking for enlightenment wherever they happen to find it.

On some of the scientific issues dealt with in the book, again I'm mostly just indifferent. Smolin accurately explains how the lack of predictivity makes typical multiverse models empty, but I'm not convinced that his favored alternative ("cosmological natural selection") does much better. While I understand well the human appeal of wondering about what came before the big bang, I've yet to see any specific models of this that carry enough explanatory power about anything to make them particularly attractive or interesting.

Many of the ideas Smolin is arguing for are clearly labeled as what they are: speculative challenges from a very much minority point of view to some of the received wisdom of this kind of science. Unfortunately, parts of his argument that are most problematic are ones which are in danger of becoming the new received wisdom of the subject. The refusal to admit the failure of the idea of string/M-theory unification has left many of our most prominent theorists pushing the idea that fundamental physics is based on some new and very different degrees of freedom, with dynamics that just happens to be too complicated to allow them to find vindication by seeing how the Standard Model emerges at low energies. For his own reasons, Smolin signs on to a version of this point of view, writing:

> I'm inclined to believe that just about everything we now think is fundamental will also eventually be understood as approximate and emergent: gravity and the laws of Newton and Einstein that govern it, the laws of quantum mechanics, even space itself…
>
> A large part of the elegance of general relativity and the Standard Model is explained by understanding them as effective theories. The beauty is a consequence of their being effective and approximate. Simplicity and beauty, then, are the signs not of truth, but of a well-constructed approximate model of a limited domain of phenomena.
>
> The notion of an effective theory represents a maturing of the profession of elementary-particle theory. Our young, romantic selves dreamed we had the fundamental laws of nature in our hands. After working with the Standard Model for several decades, we are now simultaneously more confident that it's correct within the limited domain in which it has been tested and less confident of its extendability outside that domain.

This notion that the SM is "just an effective theory", with its fascinating and deep mathematical structures nothing but an artifact of low-energy approximation has become the reigning ideology of the last few decades. One impetus for this has been string/M-theory, with its conjectured very different physics at short distances. This has been put together with our modern understanding of renormalization, according to which non-renormalizable theories make perfect sense as effective theories. The argument is then made that this is all there is to the SM, neglecting to note that to a large degree the SM couplings are asymptotically free, meaning that (most of) the quantized geometric degrees of freedom make perfectly good sense at all energy scales.

Smolin's view that the recent history of particle physics makes us "less confident of its extendability outside that domain " is one I strongly disagree with. Despite endless "naturalness" and "fine-tuning" predictions based on the "nothing but an effective theory" argument, the SM has not only been vindicated at the LHC over a large new energy range, but the discovery of the Higgs has shown it to have just the right characteristics to make perfectly good sense up to extremely high energies, far beyond anything we can test.

I've been teaching a course this past year on quantum mechanics for mathematicians, emphasizing the role of Lie groups, unitary representations and symmetries in providing not only useful calculational methods, but governing the underlying structure of the theory. Smolin argues instead that, based on Leibniz's "identity of the indiscernibles", symmetries cannot be fundamental (although a footnote says this doesn't apply to gauge symmetries):

> Symmetries are common in all the physical theories we know. Several of the most useful tools in the physicist's toolbox exploit the presence of symmetries. Yet if Leibniz's principles are right, they must not be fundamental.

This applies to the very structure of quantum mechanics:

> Quantum mechanics, too, is likely an approximation to a more fundamental theory.

since it is linear, and he bets thus just a linear approximation to some fundamentally non-linear theory. Again, mathematical simplicity is seen as an artifact of approximation, not indication of something fundamental.

Smolin ends with a vision that is pretty much the exact opposite of mine, one with a vastly diminished role for mathematics in understanding the nature of reality:

> The most radical suggestion arising from this direction of thought is the insistence on the reality of the present moment and, beyond that, the principle that all that is real is so in the present moment. To the extent that this is a fruitful idea, physics can no longer be understood as the search for a precisely identical mathematical double of the universe. That dream must be seen now as a metaphysical fantasy that may have inspired generations of theorists but is now blocking the path to further progress. Mathematics will continue to be a handmaiden to science, but she can no longer be the Queen.

Unfortunately it seems possible that Smolin's arguments about mathematics will resonate well with the current backlash against sophisticated mathematics that one sees at many physics departments in the wake of the failure of string theory. In a footnote he explicitly argues that the problem with string theory was too much symmetry:

> Indeed, we see from the example of string theory that the more symmetry a theory has, the less its explanatory power.

I don't understand this argument at all. The problems with string theory are something I've written about endlessly here, but too much symmetry is not one of these problems.

Smolin has been quite right to point out in recent years that fundamental physical theory is in a state of crisis, but I think his diagnosis in this book is the wrong one. Abandoning the search for a more powerful mathematical understanding of the world because the huge success of this in the past has made further progress more difficult is the wrong lesson to draw from recent failures (the nature of which he lucidly described in his previous book).

My own interpretation of the history of the Standard Model is that progress came not from finding more, larger symmetries, but from a deeper appreciation of the various ways in which gauge symmetry could be realized (spontaneous symmetry breaking, confinement, asymptotic freedom). The arrival of string theory pushed the study of gauge symmetry into the background, and these days one often hears arguments against its fundamental nature, such as this one from Arkani-Hamed

What's as a misnomer called gauge symmetry, whose beauty is extolled at length in all the textbooks on the subject, is completely garbage. It's completely content free, there's nothing to it.

Smolin's arguments against the fundamental nature of symmetries, even if gauge symmetry is let off the hook in a footnote, just reinforce some of the attitudes at the root of our present-day crisis. The problems that remain in fundamental theory are difficult, but denigrating now the powerful ideas that have led to success in the past won't help find a way forward.

Update: For more about this, there's a review in the NYRB, and a piece at edge.org (with responses).

Comments

1. **David Appell**
   April 23, 2013

   Peter, could you elaborate about why you see a "current backlash against sophisticated mathematics that one sees at many physics departments"?

2. **Sebastian Thaler**
   April 23, 2013

   Thanks Peter, very interesting review. FYI, there seem to be several forthcoming books of interest to this blog's readers, including "String Theory and the Scientific Method" by Dawid, "Farewell to Reality" by Baggott, and "Bankrupting Physics" by Unzicker.

3. **P**
   April 24, 2013

   Hi Peter,

   Gauge symmetry is an absolutely central concept in string theory.

   Why don't you like what Nima says? What do you think he means?

   Cheers,
   P

4. **Roger**
   April 24, 2013

   I am surprised that you so vehemently agreed with Smolin's earlier book. He subscribes to an extreme philosophy of science. I won't try to summarize it here, but I think that it is one that not many scientists would agree to.

5. **Bee**
   April 24, 2013

   I read a draft of the book half a year ago or so. Now reading your review makes me think I read a different book altogether! The one I read basically argued that emergence in time cannot be accurately described by mathematics and therefore there is a limit to what can be described by (eternally true) mathematical laws. It's a mistake, he says, to attempt a time-less understanding of the world, and with some good will you can extend this line of thought to less fundamental problems that plague the world today.

   I don't think this has so much to do with philosophy, it's more a matter of contemporary thinking. It is typically the case that insights from science reflect in sociology and politics. Just consider how much Darwin has shaped our thinking of adaptive systems, of trial and error, of selection of the fittest. Just think how much we've had to rethink our own space in this universe with the realization that ours is not the only planet, not the only galaxy, and that the universe isn't static but evolves. Don't you think that influences the way we perceive of our environment and how we interact with it? I find it quite plausible that the way we model and develop theories of nature today does influence how we attempt to manage our planet.

   Now I disagree on pretty much every step in his argument, but I agree that there are probably limits to what we can learn about nature using mathematics. (For reasons I elaborated on here.)

   All this now makes me think I should probably read the final copy.

6. **Cliff**
   April 24, 2013

   Wow, Peter. I watched the talk, expecting some philosophical disagreement, but I am absolutely flabbergasted by how totally nonsensical so many of these statements are. "Speculative" is way too kind an adjective .

   You're wrong about string theory, but if this heap of nonsense can get some of the Not Even Wrong treatment for its manifest scientific irrelevance and logical absurdity I could come to appreciate that. (I mostly come here for your links which are sometimes pretty good 😉

   P: I agree that string theory would also fall into the category of new "ways for gauge symmetry to be realized", and it subsumes all the other ways, so I would also object to that statement. And I've heard Nima make this point, that we shouldn't use the word "symmetry" when talking about gauge *redundancy* because it doesn't correspond to an operation we can *physically* perform, but a redundancy in the description. Thats fine, as far as that point goes, but the key observation is that symmetry and redundancy are two sides of the same coin. So I've never understood the impetus to emphasize the distinction in that way. All available physical evidence indicates that symmetry is a fundamental, organizing principle – the *exact* opposite of what Smolin claims on purely philosophical grounds.

7. **chorasimilarity**
   April 24, 2013

   I shall read the book. From what you mention in the review, I salute the appeal to Leibniz (who seems to more and more relevant to today's science, like in CS) and I totally agree with Smolin "That dream must be seen now as a metaphysical fantasy that may have inspired generations of theorists but is now blocking the path to further progress."

   As for "Mathematics will continue to be a handmaiden to science, but she can no longer be the Queen", it simply ignores that mathematics currently used in physics is the late bloom of 19th century math and not the 21st century hottie.

8. **Bernhard**
April 24, 2013

What is rather strange and self-contradictory w.r.t The Trouble of Physics in this Leibniz's "identity of the indiscernibles" argument of Smolin is that in that book he quite convincingly discussed the failure of using "beauty" arguments to build successful physical theories discussing the example of SU(5) and the absence of observation of proton decay.

Now he is questioning the fact that symmetries are fundamental based not on science, but by subscribing to Leibniz's philosophical principle that we have no scientific reason whatsoever to believe it is correct (not yet at least).

9. **T. H. Ray**
April 24, 2013

Just received my Kindle copy of Lee Smolin's new book yesterday. Read your and Lubos Motl's reviews this morning. When you two agree, I'm all the more convinced that I will find Lee's book as enjoyable and thought provoking as his past works.

Chorasimilarity's reference to Leibniz is on point. No less a light than Hermann Weyl — who lived in some world intersecting mathematics, philosophy and science and who synthesized the best of all three — took Leibniz at his word that the universe is best understood in the behavior of the infinitely small. Considering Weyl's further insights into the Continuum, it is no leap of faith or logic to extend the principle to the infinitely small and continuous.

Tom

10. **Bernhard**
April 24, 2013

T. H. Ray,

Being inspired by philosophy to do physics is all good if you use the physics and not the philosophical inspiration itself to convince your fellow scientists. Beyond Weyl there are many other examples of scientists using philosophical guidance that led to correct physics, the most striking one being perhaps Heisenberg and his positivist views that inspired him on writing the foundations of quantum mechanics. However, it was experimental predictions and confirmations that convinced the scientific community, not some philosophical debate. There are also good examples of inspirations that led to incorrect physics and Lee Smolin himself points to good examples in The Trouble of Physics.

Smolin could even be right about Leinibz, the "identity of the indiscernibles" and symmetries, but on what science is concerned is not enough to have the inspiration, you have to do the work.

11. **stan**
April 24, 2013

"Lubos Motl's reviews this morning"

I doubt that Motl even read the book.

12. **Anonyrat**
April 24, 2013

I had thought that if you take all the symmetries that you can think of and the physical priciples we think should hold true and try to build a model, what you end up with is superstring theory. If superstring theory does not work, then one or more physical principle is approximate or symmetry is not applicable. Is Peter saying that no, we need to dream up of new ways the same principles can manifest themselves?

13. **Lee Smolin**
April 24, 2013

Dear Peter,

Many thanks for your review, which is mainly a statement of disagreement on points off the book's main argument. Disagreement is fine, of course, but you don't even mention the main ideas, nor do you critique the actual arguments the book presents for them. As a result you take the main claims out of context.

Very briefly the book focuses on a single question: How are we to make theories of the whole universe, rather than isolated systems? The main novelty of the book is the claim that this cannot be done by scaling up the theories we have; to make a successful cosmological theory requires new principles.

There are two main contentions: First that the standard form of physical theories of fixed laws acting on fixed spaces of states breaks down when we try to extend them from theories of isolated systems (where they work fine-hence I have no issue with the standard model) to theories of the whole universe.

Second that to address issues which arise when making a cosmological theory, time must play a more central role. In particular, the book argues that laws must evolve in time if we are to explain how the laws and initial conditions are selected.

The book embraces the relational philosophy of Leibniz, Mach and Einstein, which means it is very friendly to local gauge invariances as the main way we have to express relational theories. This is hardly a radical point of view given the successes of general relativity and Yang-Mills theory.

There is an argument that global, non gauge symmetries cannot be fundamental on a cosmological scale, nor can they be realized operationally in a cosmological context. I have no issue with the central role of global symmetries in the physics of isolated systems and, indeed explain why they arise in these cases, thus no disagreement with their use in quantum mechanics or quantum field theory. The issues I have arise only in the context of attempts to make theories of the whole universe.

General relativity provides an example of this. When applied to closed universes it has no global symmetries, but when boundary conditions are imposed appropriate to the description of isolated systems, global symmetries appear.

Can I suggest that if you consider the actual argument of the book, rather than take the conclusions out of context, you and your readers might find yourselves in agreement with more of it,

Best wishes,

Lee

14. **T. H. Ray**
April 24, 2013

    Bernhard,

    Your point is well taken. However, I have never found Lee Smolin's research overly philosophical. As Lee himself says above, "The book embraces the relational philosophy of Leibniz, Mach and Einstein, which means it is very friendly to local gauge invariances as the main way we have to express relational theories. This is hardly a radical point of view given the successes of general relativity and Yang-Mills theory. "

    Or as Einstein put it, in a simple but meaningful way: "All physics is local."

    Lee is also right that without boundary conditions, general relativity is restricted not by anything *physical,* only by the special relativity limit.

    Personally, I have found that general relativity suffers no loss of meaning when being described as "finite but unbounded" — usually taken to be finite in time (bounded at the singularity of creation) and unbounded in space (a geodesic returns to its starting point) — when converted to a theory finite in space and unbounded in time.
    Which of course supports Lee's concept of the enhanced physical role of time.

    Tom

15. **P**
April 24, 2013

    Cliff,

    Thanks for the reply. I agree with most of what you say, but when people make this point regarding redundancy vs. symmetry, the point is precisely that they are *not* the same thing.

    When people like Nima (or Seiberg) say things like this, or something to the effect of "gauge symmetry is not a symmetry", what they really mean is to remind people that gauge "symmetry" generators map states in the Hilbert to totally equivalent quantum states. With global symmetries this is not the case, and you know it well: in the path integral you divide out by the gauge orbits to avoid an overcounting of physical states, but this is not true of global symmetries! This is an important distinction, and it's the distinction Nima is probably trying to make. Cosmologists make it all the time. It's also the reason why anomalies in gauge "symmetries" are theoretically unsound, but anomalies in global symmetries are just fine.

    Cheers,
    P

16. **Peter Woit**
April 24, 2013

    Hi Lee,

    Thanks for writing. You're right that it would have been a good idea for me to address the main point of your book, since you're arguing from that to the conclusions I disagree with.

    The problem I see with the argument for laws that evolve in time is one that you yourself identify in the book: what you call the "meta-laws dilemma". You speculate a bit in the book on ways to resolve this, but I don't see a convincing answer to the criticism that whatever explanation you come up with for what determines how laws evolve, I'm free to characterize that as just another law. And based on experience with the entire history of physics, new, more fundamental laws come together with deep connections to mathematics. One problem here of course is words, we may not agree on what I'm allowed to call a "law" (a word I've always disliked in this context anyway…)

    About global vs. local symmetries there is much to say, but a specific question would be about global internal symmetries. Is electric charge conservation (global U(1) symmetry) just an approximation? I'm willing to believe that such global internal symmetries are replaced by something else in regimes where gravity gets quantized, but the replacement requires an insight we don't now have into how space-time and internal symmetries unify. This insight seems to me likely to require more mathematics and deeper use of symmetry arguments, not less.

17. **piscator**
April 24, 2013

    P: this (gauge symmetries are a redundancy of description) is a standard point of graduate quantum field theory – I don't see the need for big names, this is not some special profound insight.

    That said, I have no idea what is meant by the statement that gauge symmetry is content free. That the coloured particles of the Standard Models organise into colour SU(3) representations, with associated multiplicities, is anything but content free, with profound experimental implications.

18. **Gene**
April 24, 2013

    Maybe in the end the Lorentz symmetry isn't fundamental- I can't imagine that, but it certainly is a logical possibility. Still, the constraints are formidable and certainly whatever conception of time emerges it won't be a return to the one held by Newton.

19. **Peter Woit**
April 24, 2013

    David,

    I've always followed quite closely what goes on at the intersection of math and physics departments especially in the US, and, while I've no solid numbers, my observation is just that on the whole there's now quite a lot less interest in math coming from typical physics departments than there was say 20 years ago (Witten was discovering remarkable things at the math/physics boundary, drawing interest from both sides). One big counterexample is the Simons Center, but except for that, it's hard to find examples of anywhere in the US where physics departments are showing much of an interest (for example in their hiring) in mathematics.

    Roger,

If you're referring to Smolin's interest in Feyerabend, that's something on which I would mostly agree with Smolin.

P,

Arkani-Hamed's was speaking in the context of certain work on scattering amplitudes, where there's a serious issue. The over-the-top comments on gauge symmetry I don't think were sensible, and reflect an attitude I think is problematic. But gauge symmetry is a huge, huge subject, and a serious discussion of it is best left for another day.

20. **P**
April 24, 2013

picastor,

You're pretty much right. When people make this distinction, they define symmetry to mean something that isn't a redundancy — which global symmetries are not — and hence gauge symmetries do not fit into this category. Contrary to what you say, this is not always stated correctly in QFT courses, hence Nima's (and Seiberg's, and others . . .) reason for complaint.

But as Peter would say, we're getting too far afield 😉

21. **King Ray**
April 24, 2013

I think the problem with theoretical physics today is that many of the theorists are divergent thinkers–the trade space keeps getting larger and larger. If you have an optical system with all divergent lenses, then you never converge. You need people that are like converging lenses, such as Einstein, who simplified things, so that gravity was just geometry. The truly gifted simplify, the less gifted complicate.

22. **Ben Dribus**
April 25, 2013

Dear Peter and Lee,

While I haven't yet had a chance to read Lee's book, I wanted to point out that questioning the fundamental physical role of symmetry, understood in the specific sense of Lie groups and Lie representation theory, need not entail a general rejection of mathematical sophistication in modeling nature. Rather, such an approach may reflect an effort to understand these central concepts in a broader context, by relaxing some hypotheses on underlying structure.

As an example, one might try to generalize representation theory in an order-theoretic context. A not-totally-unheard-of setting for such an idea is causal set theory, though I personally believe causal sets to be somewhat deficient. At any rate, such an approach might be based on the following rather obvious statements:
1) An important instance of Lie group symmetry is given by symmetry groups and local symmetry groups of spacetime continuum geometries, such as the Poincare symmetry group of Minkowski space; 2) breakdown of continuum geometry often arises in quantum-theoretic approaches to fundamental spacetime structure; 3) in this context, one may expect modification of spacetime symmetry groups into mathematical structures more general than Lie groups; 4) causal structure encodes much of the structure of classical spacetime, while extending to much more general order-theoretic settings; 5) it is then natural to ask what are the analogues of symmetry groups for causal structures, and to examine their "representation theory."

One might expect this to lead merely to the study of order automorphisms of partially ordered sets, which still form a group, but this is only the "active" view, corresponding to diffeomorphisms of classical spacetime. The other half of the story is the "passive" view, given by refinements of partial orders, which has nothing to do with group theory in general; this corresponds to changes of coordinates in classical spacetime. For those unfamiliar with this point of view, a good example is the relativity of simultaneity, which illustrates how the causal order on relativistic spacetime admits different refinements, which may order causally unrelated events in different ways. A few such refinements are coordinate systems in the usual sense, but most are not. The moral of all this is that important applications of group symmetry in conventional contexts can bifurcate into multiple points of view in more general settings, some still involving symmetry, others completely different.

My personal hunch is that the next experimentally verifiable progress we will see in elementary particle physics will probably come from the same old well of gauge theory that has produced so much over the last fifty years. In a thousand years, however, I doubt that fundamental physics will still be based almost exclusively on continuum geometry, which was already established as the cornerstone of still-current theory in the days of the Gruppenpest, when no one knew anything about modern algebra, information theory, or computer science. Most people with a real passion for understanding nature and no personal stake in the outcome of current or pending experiments are probably more interested in the thousand-year view. Take care,

Ben Dribus

23. **lun**
April 25, 2013

I think the problem is, in principle, that symmetries have both a "physical dimension" (there should be a physical understanding of what it means to have a symmetry, and to "change point of view" wrt a symmetric object), and a mathematical dimension (mathematicians came up with great ways to see symmetries, and tools to manipulate them).

For relativity, our understanding of both physical and mathematical aspects is complete: We know what "symmetry w.r.t. reference frames" means in physical terms, and can directly relate this symmetry to experimental observables.
Similarly, in General Relativity the "Gauge transformations" have a ready physical interpretation as changes to non-inertial reference frames.
Mathematically, this symmetry is extraordinarily subtle, but the physical interpretation of the symmetry is apparent even when, from a mathematical point of view, the geometry is not really "symmetric" in the usual sense (for example the closed universes Lee Smolin cites).

Gauge theory is very different in that the symmetry is defined mathematically but lacks a ready physical interpretation. We know that theories which have lots of symmetries have fewer infinities, and infinities are typically less observable (technically, theories with symmetries

are more anomaly-free and renormalizeable).
We know the consequences of such symmetries (there is such a thing as “charge”), and they can even be used to get predictions out (the Higgs, as a manifestation of the fact that Gauge symmetry “has to be there in the UV”).
Nevertheless, we do not know what it really
means to “change the Gauge”, its something the theorist does to make
calculations easier. For relativity, or for that matter angular momentum invariance or translational invariance, “changing the Gauge” is
something experimentalists can readily connect to their apparatuses.
And string theory (post 2nd revolution) has not really
helped this point, the justifications given
for Gauge symmetry are really not that convincing.

It is “experimentally evident” that more and more clever ways to mathematically
manipulate symmetries is not adding to physical understanding: Very very smart
people did this for decades, and we came up with either proposals which seem not to hold up to experiment, like GUTS and Supersymmetrical models, or
really profound insights about theories which are
phenomenologically unviable, like N=4 Super Yang Mills. But perhaps rather than
“abandoning symmetry”, a good start would be “explaining what symmetries mean”.

24. **Peter Woit**
April 25, 2013

Please all,
If your comment is not about the book, it’s off-topic.

25. **Nathalie**
April 26, 2013

We are living in the age of grandiose speculations in physics and Smolin has become one of its most visible actors. His talk at Perimeter was vague and loaded with self-contradictions. I found it very disturbing that a scientist would give such a talk to a general audience. What did they get out of it?

Until the near end of the talk it was all blah, blah, blah, as he said himself – he was attacking others all along and when he was finished with that there was little time left for him to describe his own ideas.

As an example he criticized the laws of physics for being deterministic whereby the future is predictable and there is no place for the free will. But then quantum mechanics tell us that these deterministic laws often lead to a multitude of possible outcomes, each with its own future. He attacked mathematical formulas of physics because they don’t allow the laws to change with time. I would say, go ahead and propose your model, compute its “observables” and let the results be tested by experiments. Before having done so it is wise to shut up. He says “Every application of physics to parts of the universe is an approximation, it can’t be exact.” So what! What a vague statement! What has the word exact got to do with physics? We never measure any quantity with infinite precision. As physicists we understand that we will never be able to make “exact” statements. A concept such as TOE (Theory of Everything) is of course unphysical as it can never be tested but that should not stop us from making progress by refining our theories or inventing new ones.

26. **Lee Smolin**
April 26, 2013

Dear Peter,

I am also very much in favor of the invention and use of new and deeper mathematics to describe nature. I only argue that it is unreasonable to motivate this by the mystical fantasy that the mathematical description is truer than our experience of the present moment and its passage in time. There are sufficient reasons to believe that mathematics is useful in nature without believing there is some mathematical object isomorphic to nature in all aspects.

A metalaw may evade the infinite regress problem if it doesn’t have the form I call the Newtonian paradigm, which involved fixed timeless dynamical laws operating on a fixed timeless space of states. Such a metalaw would be different in important ways than our present laws. I do give several examples in the book of ideas under development, some of them in papers. I agree these need much more development but the examples I give show how the metalaw problem may be solvable.

Dear Nathalie,

I am sorry you didn’t like the talk, it is hard to present this argument in an hour to a mixed audience. That is one reason I wrote a book. But, please note that I didn’t attack anyone personally, I gave a critique of certain issues blocking the progress of science and proposed alternative solutions. I agree the time was too short to give details, but those are described in the book and in scientific papers going back to 1992. It is exactly because I have, proposed …” models, computed “observables” and let the results be tested by experiments” that I feel it is OK to describe them in a public forum. One model, cosmological natural selection, made two predictions, both published in 1992, that have stood up to test since then. One, that the heaviest neutron star be less than two solar masses was confirmed again in a paper just published in science (Antoniadis et al.,Science,Vol 340). The other, that inflation, if true, is governed by a single field moving in a potential determined by a single parameter, is confirmed by the recent Planck observations.

In the book I describe these in detail as well as newer models of evolving laws and how they might be tested.

Thanks,

Lee

27. **Jamahl A. Peavey**
April 26, 2013

Dear Peter,

I thought time and the measure of time were different. So is Lee Smolin talking about the measure of time reborn or time reborn? The measure of time as represented mathematically is reversible but time as described in thermodynamics or as we experience it has an arrow and is not reversible.

28. **Roberto S.**
April 26, 2013

What I think I see going on with these type of books is more of a slow-motion meta-argument and academic positioning about where to focus your collective brainpower, because perhaps we have reached some sort of collective limit in what we (er, you) can figure out, and it will take every learned neuron to acheive some breakthrough. So if it's one of those "fellow scientists, we need to study this novel idea as the way forward" book, I will pass.

Oh, and Hawkings has gone "full sci-fi" in my opinion.

29. **Yatima**
April 26, 2013

"I only argue that it is unreasonable to motivate this by the mystical fantasy that the mathematical description is truer than our experience of the present moment and its passage in time."

FWIW, I couldn't agree less. Nothing could be LESS true than our "experience of the present moment". That "experience" which no-one can even grasp (witness the endless fruitless discussion about qualia) is a second-hand, unconsistent, faked-up, stitched together, event-reshuffling and event-inventing narrative of "what happened a bit earlier", a datastructure thrown barfulously together by a cognitive apparatus made to acquire food, sleep, safety and sex with least energy expense. Which is why Penrose's ideas about the human mind as a self-consistent theorem prover are ludicrous.

If you want to do and think consistently however (using the amazing capacity of the human cognitive apparatus to use external physical machinery and memory to remember things and keep on track), you need consistent models.

Mathematics *is* the setting up of consistent models. Those that do not give you a kilo of gold when you turn clockwise once.

As the universe *is* consistent (axiom #0), a (infinite set of) mathematical model of it exist.

Whether that model can be discovered, approximated or whether "time" has any part in it is another question entirely

30. **Nick M.**
April 26, 2013

Lee Smolin has just published an article in New Scientist titled "It's Time Physics Recognised That Time Is Real". You will, however, have to register with New Scientist to read the article, and the article will only be viewable (using that "for free" registration) for about another ten days, or so.

– Nick

31. **noko**
April 27, 2013

The only book on Time worth reading is Indian polymath C. K. Raju's "The Eleven Pictures of Time"

32. **David Gillett**
April 27, 2013

A 'lay' perspective- I have a doctorate in molecular quantum mechanics, so not the really hard stuff.

I welcome Lee's book because it challenges us to think in new scientific directions. Having followed cosmology and particle physics for 30 years, I believe the field has reached an impasse; since the mid 1980s we've been confirming existing theory with no new breakthrough ideas or unexpected results.

I believe we're in a time similar to that just before Einstein, and when Special Relativity was published it was initially ignored- similar to Lee's ideas, not well received but necessary to move thinking on to a new position with radically new insights. Is Lee he next Einstein? Maybe, maybe not- but he is prompting the challenges that create the space for the next Einstein.

33. **David Gillett**
April 27, 2013

A paragraph I omitted:

Dark matter, dark energy and the accelerating expansion, plus the 'axis of evil' in the CMB. Aren't these the 'aether' and michelson-morley experiments of our day?

34. **noko**
April 27, 2013

"when Special Relativity was published it was initially ignored"

no it wasn't

35. **Nathalie**
April 27, 2013

Dear Lee Smolin,

In your reply to my comment you say "please note that I didn't attack anyone personally". It is true that except for Max Tegmark you didn't specify the names of those you were criticizing but that makes the situation even worse. In addressing a general audience, you made statements such as "Physicists claim that they know the answers to these questions" and that the theories by those physicists were false. Because the audience doesn't know who these physicists of yours are, they must have thought that you mean the entire physics community while the majority of physicists, for example, those working on graphene or on fractional quantum hall effect have hardly been involved in the process you were describing. Your "physicists" make up a tiny fraction of the physics community – those who love to speculate.

By the way the reference you gave (Antoniadis et al., Science, Vol 340) does not refer to your work. Neither can the authors test your theory, at least not yet. They make observations in a nonlinear regime – a "previously untested regime, qualitatively very different from what was accessible in the past." I am afraid you have to wait to see what such observations in the nonlinear regime have to say about your theory.

Because I am not familiar with your theory, I hope someone else will check your statement that the recent Planck observations confirm it.

Best regards,

Nathalie

36. **David Gillett**

April 27, 2013

Noko

You are right “ignored” was too strong. But recognition of its implications did take some time, and Einstein certainly didn’t gain instant acclaim.

37. **layman**
April 27, 2013

I listened to the talk carefully. It is just horrible. I hope the audience does not think everybody in physics is so shallow and trivial. I am a little encouraged by the reviews on Amazon, the public is not that stupid it seems.

38. **Lee Smolin**
April 27, 2013

Dear Nathalie,

The prediction of cosmological natural selection that was tested in the paper in Science was that the upper mass limit of neutron stars is at most two solar masses. This is tested each time a neutron star mass is measured. The paper doesn’t have to reference the prediction to confirm the prediction, it is enough that the measurement they report confirms it, which it did.

All physicists, whether working on graphene or condensed matter physics, employ the framework of fixed laws acting on fixed state spaces-the framework I call the Newtonian paradigm. As I explain in the book, this is exactly what they should be doing. My criticism is not of the usual practice of physics, it is confined to the extrapolation of the usual practice to cosmological theories. ie the only thing I criticize is attempts to apply the Newtonian paradigm to theories of the whole universe.

This point-that the methodology which succeeds so well when applied to laboratory systems fails when applied to the universe as a whole-is the first step of an argument that was sketched in the talk and is described in detail in the book. So my intention was hardly to criticize anyone, it was to make an argument about whether the Newtonian paradigm can be applied to answer cosmological questions. Does this make it clearer?

Thanks,

Lee

39. **Spencer Tracy Jr.**
April 27, 2013

I’m up to p. 202 and so far I think the book is excellent.

40. **Nathalie**
April 27, 2013

Dear Lee Smolin,

In order to make a convincing case for your model you need to have predictions that are significantly different from the standard ones. The measured neutron mass (2.04 in the units of the solar mass), listed in table 1 of the paper by Antoniades et al., falls in the expected range in general relativity. No big deal as far as I can see.

Best regards,

Nathalie

41. **Maynard Handley**
April 27, 2013

“Simplicity and beauty, then, are the signs not of truth, but of a well-constructed approximate model of a limited domain of phenomena.”

Peter, I fear you are being so protective of what you love (the beautiful mathematics) that you are missing the point here — that this beauty doesn’t mean it’s physically relevant.
It is a historical fact, which I’m sure you know as much as anyone, that not only does beautiful mathematics not imply that physics works that way, but ALSO (and just as relevant) it’s possible to get so hooked up on one aspect of the mathematics that you actually MISS the larger structure behind the scenes.

The poster child for this (a good example because it’s easy enough to understand) is quaternions and the whole foolish battle of the 19th C between those who did and did not support their use in physics. And what was the ultimate resolution? Both sides were so obsessed with one aspect of the issue (algebra) that they got the geometrical interpretation wrong, and ultimately missed out on the huge wider world of Lie groups, spinors and the rest of it.
One could argue the same thing for pre-Minkowsi EM, where incredible ingenuity going into solving Maxwell’s equations missed out on the more significant issue of understanding them in full geometric consequence.

If I were to make a modern analogy I’d ask (at the risk of appearing foolish, because I haven’t studied this enough to be sure I’m correct) whether Clifford Algebras fall in the same sort of pattern. They meet all the mathematical desiderata: they unify a lot, they’re conceptually extensible to all dimensions, they result in a non-obvious large-scal pattern across dimensions; and they’re a neat practical tool for calculating certain entities. BUT I’d argue that what physics really needs is a better geometric understanding of simple Dirac spinors in 3+1D, that Clifford Algebras have not delivered that, and that it doesn’t seem likely that they will. Beautiful math, yes, but shining a powerful light in the wrong direction for physics.

Since at least GR and Dirac physics have operated on a kind of “if the math is beautiful enough it has to be right” autopilot, and this is what has brought us to supersymmetric strings. It is THIS against which Smolin is reacting, not a generic dislike of sophisticated math.
(It may be easier to understand the sentiment by imagining applied to a different field. Economics is another science where appreciation of beautiful math has proved immensely destructive to the task of understanding the actual world in which we live, as opposed to the better world of our minds.)

42. **Lee Smolin**
April 27, 2013

Dear Nathalie,

You miss the point. The point is that CNS predicts the upper mass limit is 2 solar masses which is substantially lower than predicted by the conventional picture of neutron stars. Had the NS mass been 2.5 solar masses or more it would have been a clear refutation of the prediction of CNS. Please look at arXiv:1202.3373, which is a recent review article about cosmological natural selection, or read the book before making further incorrect claims.

43. **Low Math, Meekly Interacting**
April 27, 2013

I'm an atheist, but this stuff makes me hope to God we see something new in the LHC before it's shut down.

44. **Marcus**
April 27, 2013

The talk Peter linked to was Lee's April lecture for wide audience
http://pirsa.org/13040103/
there is also one given 26 February in the quantum foundations seminar
http://pirsa.org/13020146/
clear, cogent, efficient presentation, with interested questions from audience e.g. Rob Myers, Neil Turok
I would recommend anyone interested to watch the February talk.

It seems to me that the idea of law-like regularities emerging in a process of past events generating a new layer does not at all depend on CNS, which is just one example of an evolutionary process. However the highest mass estimate for neutron star I have been able to find appeared in October 2012, and was for the pulsar
PSR J1311−3430
http://arxiv.org/abs/1210.6884
It's newly discovered and the authors cautiously say "if confirmed". They say that "all viable solutions" to their data give a mass > 2.1 solar. But they seem to think a considerably higher mass than that is likely.

Before that, the highest mass estimate was for a longer-period pulsar and (I gather) less precisely determined: PSR B1957+20
http://arxiv.org/abs/1009.5427
van Kerkwijk, M. H., Breton, R. P., & Kulkarni, S. R. 2011, ApJ, 728, 95
They said 2.4±0.12
but also acknowledged some systematic uncertainties and but the minimum at 1.66 solar.
There's the very recent estimate of 2.01 that I think Lee referred to already:
http://arxiv.org/abs/1304.6875

Really two topics here: CNS hypothesis and the neutron star mass limit prediction, on the one hand, which has been around for some 20 years(!)
And on the other hand this new, rather beautiful, idea of a meta-law process which explains both the flow of time itself and the appearance of seemingly timeless regularities we think of as natural laws.

Again, check out the quantum foundations seminar talk of 26 Feb!
http://pirsa.org/13020146/
My advice is not to get sidetracked arguing about the wide-audience public lecture.

45. **Marcus**
April 28, 2013

Smolin's arguments against the fundamental nature of symmetries, even if gauge symmetry is let off the hook in a footnote, just reinforce some of the attitudes …

Symmetry is something that needs to be explained and it doesn't make it less important or beautiful to have an explanation for how it arose. To take an analogy, the bilateral symmetry of many animals can be explained by digging into evolutionary history, and having an explanation doesn't diminish it's importance as a fact of biology.

46. **Nathalie**
April 28, 2013

Dear Marcus,

Thank you for the references on neutron star masses. What an exciting and rapidly developing field of research!

You give us the advice "not to get sidetracked arguing about the wide-audience public lecture."

Actually, the major point of my comments concerned the wide-audience public lecture aspects of Smolin's talk.

When we scientists talk to each other and at our seminars we can say almost whatever we like. Under such circumstances the audience, usually equipped with healthy skepticism and sufficient amounts of grains of salt, has the capacity to understand and analyze what the speaker is talking about. The general public doesn't and can easily be misled.

Smolin is a leading figure at an Institute with a very high academic reputation. The general public will tend to consider him as an authority and believe in what he says. Therefore he, as well as other speakers at such occasions, should not mislead the audience by making sweeping statements of the kind Smolin did – attacking mathematics, saying all physicists were wrong and so on.

It is just fine that someone like Smolin takes an alternative approach to physics. But it remains to be seen if his approach will teach us anything new about the world we live in.

47. **franco zoccheddu**
April 28, 2013

Sir,
I beg your pardon, if my use of english language is not that good I'd want.
I'm very interested on what consequence has and will have in the near future (say 10 to 15 year) "sophistication" of mathematical ideas, theories, physicists will take advantage of for improving predictability power without exiting SM framework.
Does Lee Smolin's book also deal with this topic?
I'm a physic teacher, sometimes students fond of math and phys ask me about.
I thank you!

48. **Chris Kennedy**
April 28, 2013

Lee,
This is a great, much needed book that will hopefully spark discussion in areas of physics that are largely ignored. (I just gave it a 5 star review on Amazon a little while ago.) For the sake of brevity however, I will get right to my criticisms:
Although several times you examine time theories and their possible consequences on the relativity of simultaneity, you didn't specifically explore how time dilation (during relative motion and proximity to mass) fit in with a real, global theory of time. I think this was a mistake since, in my opinion: only through a better understanding of what makes clocks speed up or slow down, will we be able to understand what makes them run at all.
Also – would you consider all time theories described as "emergent" to be illusory? And if so, why would they have less of a chance of being time-arrow asymmetric than real global time? For example – my starting point is the question: Do all of the fundamental behaviors expressed among particles, forces and fields exist in time, or are those behaviors themselves the actual expression of time? It is my contention that the behaviors actually are time (each atom is its own clock). If we suppose that is true for a moment, I can't think of any scenario where all behaviors in the universe would spontaneously and simultaneously run in the reverse direction.
Personally, I think the time reversibility concept is bizarre in general. We assume that why everything different about today than yesterday is due to probabilities given certain rules among all of the particles, forces and fields in the universe. If time were to start running backward, then either all of the above would have to miraculously run in reverse with a sudden opposite cause & effect for each set of forces or "reverse time" would externally drive them on some sort of auto pilot. The former is highly improbable and the latter is not logically consistent. Why would the forward direction of time be driven by the behaviors of various forces while the reverse would be driven simply by reverse time itself? Please let me know if I have misunderstood something here?
Many thanks,
Chris

49. **srp**
April 28, 2013

I watched the video Marcus recommended. The model Smolin proposed seems to have momentum traveling backward in time from "child" events to their "parents" as part of a momentum balancing process. I'm not sure how that makes sense, but maybe it's just some sort of convention that physicists are used to.

50. **Ashish Sirohi**
April 29, 2013

Einstein-Leopold's book, the "Evolution of Physics" has been my favorite popular book (not because it is the best written popular description but because it contains Einstein's thinking in his own words) — as Neils H. Abel said, study the words written by the masters themselves, not their pupils. I have read that book a gazillion times. Smolin's book "The Trouble with Physics" is the other popular book I have read multiple times and the page that stumps me is p.257 and its statements about time and Smolin's not being happy with the simple diagram he drew there.

Now Smolin has a whole book on time! Well, it is not available at Indian online stores so have just ordered from Amazon, where they really loot you on international shipping. Jeff Bezos ships free in the USA, rips-off the rest of the world. Maybe I could have saved some money by searching places like Ebay etc but that takes time. I plotted a money-time graph and realized I really don't know what time is! Can't wait to find out 😉

What I admire most about Smolin is that he one of the two professors who gets special mention on my favorite site about special relativity: physicsnext.org!

51. **John Merryman**
April 29, 2013

Chris,
Good question. It does go the the main problem of time. Whether the events are the denominator and the present the numerator, with this eternally existing fabric of spacetime, or it is the present as the denominator and the events the numerator, so that it is just one constantly changing present.

52. **Lee Smolin**
April 29, 2013

Dear Chris,

Many thanks, that is an excellent question. The importance of shape dynamics, the development I describe in Chapter 14 of Time Reborn, is exactly that it explains how time dilation and other phenomena of special relativity can be made consistent with a real global time. To put this briefly, shape dynamics has a gauge symmetry under local scalings of volume. But if you shrink the volume a clock occupies you make it tick faster because clocks are physical devices and their size affects their rates. This is evident in models of clocks called light clocks in which what ticks is a photon bouncing back and forth between two mirrors. Hence you can trade a relativity of time with fixed sizes for a relativity of size with fixed times. This leads to an equivalence with general relativity and, as Barbour and collaborators have shown in detail, there emerges all the phenomena of special and general relativity. How this works in detail requires of course a bit more technicalities.

Thanks for your thoughts on irreversibility. This issue is the subject of work in progress with Marina Cortes, so please look for papers to appear.

Dear srp,

I'm not sure how you got the impression momenta run backwards in time in the model I discussed in that talk; all momenta run from past events to future events.

Dear Marcus,

Many thanks for those references. I agree that these NS's have central values of their measured masses larger than 2 solar masses, but the error bars are large enough this is consistent with 2. But these make me hopeful that soon a NS may be discovered with a mass definitively above 2 solar masses. I would not be unhappy to be able to write a paper saying that a prediction of cosmological natural selection has been falsified because this supports my key point that it is more scientific to posit evolving laws than timeless laws.

Dear Nathalie,

I understand that you are concerned that by discussing philosophical and methodological issues in public we may mislead laypeople to confuse ideas in progress with settled facts. This is a great concern of mine as well and I don't believe I was doing that at PI-because I made it clear more than once that I was not addressing the normal practice of physics but only its extension to a theory of the whole universe. But I do share your general concern and if you have any concrete suggestions I'd be very happy to incorporate them into my future public talks. Meanwhile I would very much appreciate it if you would look carefully at the book and/or the talk and understand that I am not attacking mathematics in general or saying all physics is wrong etc. I don't mind at all if my argument is criticized but it is helpful when the criticism is directed at what I am actually saying rather than being based a misunderstanding.

Thanks,

Lee

53. **T. H. Ray**
April 29, 2013

Lee,

In re … ". . . you can trade a relativity of time with fixed sizes for a relativity of size with fixed times. This leads to an equivalence with general relativity and, as Barbour and collaborators have shown in detail, there emerges all the phenomena of special and general relativity."

Except that Barbour et al are compelled to trade the geometry of spacetime (GR) for a geometry of space without temporal causality. I can't see how this view is at all compatible with yours, if you want a model in which time is physically real.

On the other hand, I find no conflict between the conventional view of general relativity which describes a universe "finite and unbounded" — taken to mean bounded in time at the singularity of creation and unbounded in space by continuous positive curvature — and a universe finite in space and unbounded in time. I think that would preserve spacetime as physically real, as well as the physical evolution of time itself. I am skeptical of assigning a causal role to geometries and symmetries — however dynamic we imagine them to be — independent of a guiding physical principle.

Best,

Tom

54. **Lee Smolin**
April 29, 2013

Dear Tom,

There is real time with temporal causality in shape dynamics, at least at the classical level, and there is a beautiful recent paper about this by Barbour, Koslowski and Mercati, http://arxiv.org/abs/1302.6264.

Thanks,

Lee

55. **N.**
April 29, 2013

BTW, anyone who claims that he has read the book in these few days – should reconsider. Peter included, although I suspect that he (like Bee) had access to the volume beforehand. This is not a book to be read in four days. Be patient and give it the time and thought it deserves.

56. **Chris Kennedy**
April 29, 2013

Lee,
Thank you for adding that response because quite honestly, I didn't connect the dots between your GR application of shape dynamics (time is universal and size is relative) and the other statements about SR in that same chapter. After rereading chapter 14 along with Barbour's paper from the link you provided (I assume Barbour's dimensionless time variable and your real preferred global time mean the same thing) I sort of get it now – but I don't know if I agree with it.

On p. 170, you say: "This global notion of time implies that at each event in space and time there is a preferred observer whose clock measures its passage." Then, according to shape dynamics, you could argue that both GR and SR time dilation effects would produce "deviations" from the preferred global time because of the "size relativity" experienced in each. But, even if you consider a particular object to be at absolute or preferred rest, it is still in some other object's gravitational field even if by just a trace, and will have a miniscule deviation from the true global time. If I have that right, then doesn't real global time become a theoretical value that is not experienced by anything in the universe?

Also, how closely does the "size relativity" in shape dynamics relate to the length contraction hypothesis in special relativity? I want to make sure I understand your comment in your earlier post along with another statement of yours explaining Barbour's position from chapter 14: "Shrink everything in one place and somewhere else, enlarge everything by the same amount." I ask because according to SR, length contraction, along with time dilation is considered to be a reciprocal effect from the perspective of both the observer and traveler. And the "shrink everything/enlarge everything" statement seems to hint at an inverse proportion rather than reciprocal effect. Unless the shrinking and enlarging doesn't necessarily apply to two clocks running at different rates? Can you clarify?
Thanks again

John M. – Thanks, hope all is well. I noticed the early time of your post. I envy you. I wish my brain worked in any capacity before 6:00 am.

57. **Marcus**
April 29, 2013

I'm curious to know if the book contains this Youtube link to a short conversation between Richard Feynman and Fred Hoyle recorded around 1972:
http://www.youtube.com/watch?v=uNOghidK2TY
The clip is 9 minutes and 35 seconds long but if you are in a hurry you can skip the first 8 or 9 minutes.
Relaxed conversation at a Yorkshire pub. If you start around minute 8:00 you get some of the atmosphere. But the main thing is in the last 35 seconds of the clip.

A Caltech archive has the text of what Feynman says there:
http://calteches.library.caltech.edu/35/2/PointofView.htm
Scroll 3/5 of the way down the page.
"It is interesting that in many other sciences there is a historical question, like in geology – the question of how did the earth evolve to the present condition. In biology – how did the various species evolve to get to be the way they are? **But the one field which has not admitted any evolutionary question is physics. Here are the laws, we say. Here are the laws today. How did they get that way?** – we don't even think of it that way. We think: It has always been like that, the same laws – and we try to explain the universe that way. So it might turn out that they are not the same all the time and that there is a historical, evolutionary question."

There seem to be two problems being addressed: the Problem of Time, and the Problem of Laws. (Why these laws? How did they come about?) The Barbour Koslowski Mercati paper Lee mentioned solves the first–the problem of time–in the shape dynamics context, but does not address the second.

58. **Daniel Tung**
April 30, 2013

Hi Lee

I have a few questions:
1) Does the reality of time imply that (total) information is always being created at a fundamental level (and thus non-unitary)?

2) I do not understand your statement that since the universe includes everything, there could be no eternal mathematical truths or laws outside of the universe that nature can correspond to.
But, mathematical laws need not be physically lying outside of the universe – they are mental products, and thus of a different category from the physical universe. It is wrong to say that they are outside of the universe.

3) Maybe the reality of time depends on our viewpoint, whether it is from inside or outside? For an observer inside the universe, using tools defined entirely from inside, time can be shown as real. But if we see from outside (God's eye view) like Newton's theory, all time is equal.

59. **Marcus**
April 30, 2013

Daniel, about your question (2) I don't remember Lee saying what you have him saying there. A handy way of distinguishing a mathematical law from a physical law is that a law of physics *could be different*. It's a key distinction.

With a law of physics one can reasonably ask "why is it this way rather than that?" because there is empirical content. You rightly point out that mathematical truths are of a different sort. Mathematics is an evolving human language within which true statements are deduced non-empirically from sets of axioms.

Have a look at slide #5 of PIRSA 13020146. It's a talk Lee gave *about the book* in the Quantum Foundations seminar at Perimeter. You can download the slides PDF and review the logical outline of the talk after watching the video. Slide 5 gives a couple of concise principles that the rest of the talk (and I believe the core argument in the book) is based on.

The first principle shown (labeled PSR) is carefully worded so that it applies to laws of physics and not to pure math.
I can't recommend watching the February Quantum Foundations seminar talk too highly. It's an immediately available clear concise presentation of the logical argument at the heart of the book.
http://pirsa.org/13020146/

My kibitzing like this is not meant to detract. Hopefully Lee will provide a more complete authoritative response later.

60. **T. H. Ray**
April 30, 2013

Lee,

Having read the Julian et al paper you linked, I want to echo Chris's comment "(I assume Barbour's dimensionless time variable and your real preferred global time mean the same thing) I sort of get it now – but I don't know if I agree with it."

Except that I'm not sure that I even sort of get it. A dimensionless time is dimensionless spacetime. And I don't see how shape without geometry differs from change without differentiation. We can believe it, philosophically — yet how can we hope to demonstrate that such is fundamentally "physically real" in Einstein's terms, i.e. " … independent in its properties, having a physical effect but not itself influenced by physical conditions." (from The Meaning of Relativity, 1956.)

Minkowski spacetime and general relativity may be old hat, and perhaps I'm a Philistine, yet I cannot get my mind around a more fundamental background-free starting point for energy exchange between bosonic and fermionic particle properties.

Best,

Tom

61. **Marcus**
April 30, 2013

Tom, I don't see how you come to your stated assumption: "I assume Barbour's dimensionless time variable and your real preferred global time mean the same thing." They seem quite different animals to me! Surely they are constructed in entirely different ways. Can you clue me in to your thought process?

What Lee said earlier in these comments was: "The importance of shape dynamics, the development I describe in Chapter 14 of Time Reborn, is exactly that it explains how time dilation and other phenomena of special relativity can be made consistent with **a** real global time. To put this briefly, shape dynamics has a gauge symmetry under local scalings of volume…"

I bolded the article "a" because it as much as says that they are NOT the same global time, as far as the speaker knows. The point seems to be that there exists at least one global time which is consistent with such and such phenomena.

Alain Connes and Carlo Rovelli have also proposed a global time, based on the Tomita flow on star algebras—theirs seems quite different from either of the others although agreement is possible where applicable in special cases: all might be found to agree with the global time of Friedman equation cosmology in that model. Anyway a global preferred time is not such a rare beast that whenever you see two of them you must assume they are the same. But maybe you have reasons to equate them and I'm being too cautious. 🙂

62. **T H Ray**
April 30, 2013

Sorry to add confusion, Marcus. I didn't say it. Chris Kennedy said it. I haven't even gotten to chapter 14 of Lee's book. The Barbour et al paper I did read, however, and I cannot reconcile what I know that Lee has said in the past with Julian's concept of shape dynamics and the (nonphysical) role of time.

I thought Lee was a relativist, albeit with an enhanced physical role for time and the possible inclusion of nonlocal hidden variables (the class of theories in fact, to which string theory belongs).

I think global time is a pretty slippery concept. I'm compelled to agree with George Ellis that without a well defined local arrow of time, there is no life — even further though, I think, no matter — whether baryonic or dark. The more I look at these causal structure theories, the more convinced I become that physically real spacetime and general relativity still hold the key to theoretical completeness.

Best,

Tom

63. **Chris Kennedy**
April 30, 2013

Marcus,
I assumed it, but wasn't 100% sure, so I threw it in there in case I needed to be corrected. However, I believe all of the the comments below that are pretty consistent with questioning preferred global time, so I hope Lee comments on the issues I've raised.

64. **Lee Smolin**
May 1, 2013

Marcus,

That Feynman quote is in the intro, page xxvi

Daniel:

In reply to your questions by number:

1) I would argue that there must be a cosmological theory which quantum mechanics approximates for small subsystems of the universe, which would have an evolution law which is expressed in a language not that of quantum theory. This would of course conserve probability but might not be expressed as unitary evolution on a Hilbert space.

2) If you think mathematical objects are "Mental products" you are not a platonist and you believe mathematical objects are within the universe as are the brains or minds that created them. I claim that mathematics can provide excellent models of the records of past observations of subsystems of the universe. But all such models, describing subsystems, involve truncations and are hence incomplete. I however argue in the book that they cannot be completed by extending the model to encompass the universe as a whole. The core of the argument of the book, which I won't repeat here is why this cannot be done.

3) I deny there is any scientific sense to a description of the universe from outside of it, for reasons given in detail in the book.

Tom,

I don't mean to imply that shape dynamics gives a view of real time in all the aspects I mean that, which are specified on page xiv. What shape dynamics does do is show how the existence of a preferred global time, in which the spacetime evolves through hamiltonian evolution, is compatible with relativistic causality.

Also, Barbour's previous arguments that time disappears in quantum cosmology are not the same as or necessarily relevant to shape dynamics, which is so far only understood at a classical level.

Chris,

It is important to distinguish special relativity, where the lorentz group describes a global symmetry from general relativity, where the many fingered time, or spacetime diffeomorphisms describe a gauge symmetry. Shape dynamics is a reformulation of the latter in which the many fingered time gauge symmetry is replaced by a local scale gauge symmetry. To address your question in detail of exactly how time dilation arises in shape dynamics is possible-because SD is equivalent too GR, but a technical exercise as global symmetries are already a subtle issue in general relativity.

Thanks,

Lee

65. **T. H. Ray**
May 1, 2013

Lee,

Thank you — that's a relief. If I am to understand shape dynamics in a classical framework, I can also grasp preferred global time as continuous, with classical time reverse symmetry. I'm afraid this discussion is getting a bit ahead of my reading the book — need to catch up!

Best,

Tom

66. **Chris Kennedy**
May 1, 2013

Lee,
I guess after learning what little I know about shape dynamics, I will limit my observations to its explanation of time dilation in Special Relativity.
The clarification I'm looking for is whether the relativity of size correlates with the reciprocal length contraction proposed for SR during relative inertial motion. This is an extremely important question. By the way, in certain relativity writings, "reciprocal" refers to equal but identical, which is how I use it here. In other words, I see the moving ship contract, and the moving ship sees the space it's traveling in contract. SR also says that I will see the ship's clock run slower while the people on the ship see mine run slower as well.
I am asking this because you said in your book that: "SD achieves an accord between the experimental success of the principle of relativity and the need for a global time….."
Any sane person who has studied relativity would have to admit that all of the experimental evidence certainly shows that time dilation during motion and proximity to mass is a real phenomenon. But there are a lot of sane people who are not aware that certain physical evidence (GPS system) shows that the time dilating effect is not reciprocal during relative motion. If the satellite clocks are running slower from the perspective of the ground (having already adjusted for the gravitational effect) and the satellite sees the ground clock running slower than itself, there is no compensation that can be applied to either clock (or both) to get them to run in sync from both perspectives simultaneously – but yet they do and the system works quite well.
So, if shape dynamics is not reliant on the framework of these reciprocal effects in SR, then I would say it has a chance, but if it is reliant on that aspect of SR, then I would say it is in serious trouble.
I think I speak for all of the questioners here when I say we appreciate your continued participation in this conversation. "Time" is an important issue that never seems to get the attention it deserves.

67. **Daniel Tung**
May 2, 2013

Hi Lee,

What I wanted to say is that even if there is nothing outside the universe, this does not imply that there is not a realm of mathematical truths where nature corresponds to. Platonic realm is conceptual, and not something physically lying outside the universe. So its existence does not contradict the fact that there is nothing outside the universe.
"Outside" is a relationship between two physical objects.

68. **Henrique Gomes**
May 2, 2013

It is easy enough to represent global Poincaré symmetry in Shape Dynamics, as long as you have (as Lee mentions), a flat ansatz*. The way this comes about (at least mathematically), is that due to the high geometric degeneracy, there is a family of exactly 4 time directions which cannot be traded for conformal symmetry in the construction of Shape Dynamics. These directions correspond to time translation and boosts, or alternatively, to Minkowski and Rindler. In this degenerate case, quite unexpectedly, the Shape Dynamics algebra of the 4 respective Hamiltonians (supplemented with the isometries of flat 3-space) form the Poincaré algebra.** So I would say that at least in the way its been derived it does not depend on the reciprocity you mention. The appearance of a singled out time direction, once the trading of symmetries is complete, would appear only when you add inhomogeneities to the Universe.

* (in fact all you need is a spatial spherically symmetric ansatz in asymptotically flat SD with no boundary mass charge)

** I should mention that this algebra is already represented as a boundary charge algebra in this circumstance, but the charges are slightly different than in GR, see http://arxiv.org/abs/1212.1755 .

69. **Henrique Gomes**
May 2, 2013

I should have mentioned that my comment was addressed to Chris Kennedy's remark, my apologies.

70. **Tobias Jürgens**
May 3, 2013

To anyone who might know:

In Barbour's book "The end of time" he talks about the timeless formulation of quantum mechanics. I only read the book once so far and couldn't understand if it is working or just how he imagines it to work, in contrast to Newtonian mechanics, SR and GR, where I understood the construction.

So is there a mathematical formulation of timeless QM? And could You please point out a (some) paper(s) on this?

Thank You,
Tobias

71. **MathematicianNotPhysicist**
May 7, 2013

I've just finished reading the book, and I have questions. I don't know if Lee Smolin will see these, but anyone is welcome to answer.

1. In Cosmological Natural Selection (CNS) you need to get Big Bangs to come from Black Holes, but obviously the theory of if/how this happens needs much more development. In particular you don't know what it takes for a Black Hole to make a good baby universe, not just with good constants, but also with the special initial conditions like our own Big Bang, and still following GR and QT to the extent that there's a choice, and so on. Given this uncertainty in the theory, what does that imply for prediction and falsifiability in CNS? It might be better for a universe to have fewer fitter offspring (just as in biology), so just maximizing the number of Black Holes cannot be a necessary requirement of CNS.

2. If there is a to be a global time, then it is a totally ordered set (of "instants"). What does this sequence of instants look like? The integers, the rationals, the reals, something else? Or is this a case where mathematics cannot fully capture the physics? Or are the "instants" actually blurred out and uncertain, and least by the order of one Planck time?

3. How strong is the argument that "real time implies global time" anyway? What's wrong with real time (now is real, past and future is not) while still having multi-fingered time, as long as neighboring patches of space keep up with each other? What if you had two or more totally causally disconnected universes: would they need to share a global notion of simultaneity with each other? Maybe I'm being dense but I'm not seeing it as settled that there either must be a global time, or no (real) time at all.

4. If there is to be a global time, does it necessarily have to be one picked out by the shape dynamics argument? There's lots of ways to map a partial order to a total order? How do you choose which is the "true global time" (or was this just an in principle argument about what might be the true global time)?

5. What viable proposals are there where GR and QT are not taken to be absolutely true, but where some dynamical process tends to push violations of these laws towards zero? (This is meant to be analogous to various feedback mechanisms in a living organism, on a faster timescale than evolution.) As a possible very specific example, take a spacetime, find the global time t picked out by the shape dynamics argument, then reformulate GR, by describing how a spacelike slice S(t) at constant time t, with appropriate fields, and satisfying appropriate conditions, would evolve as a function of t. But then extend these dynamics so that they can be applied to an arbitrary spacelike slice, and you want it to be that as you evolve it into the future as a function of a time parameter t, that the future spacelike slices converege to the spacelike slices you get from the shape dynamics argument. Is something like this done?

6. Is there a connection between "constant mean curvature slicing" (Note 10 of Chapter 14) and Penrose's Weyl curvature hypothesis?

7. As an idea of where QT could break down, what do you think of Penrose's proposal that the laws of physics are such that a quantum superposition of macroscopically different ditributions of mass/energy will not occur, (i.e. despite QT, things won't deviate too far from classical GR, except possibly near singularities)?

8. What does it mean for space to not be real? It seems to be pretty much a 3-dimensional sheet of stuff to me even when it's "empty" (and even if it sometimes looks like an arbitrary graph, I'd still just call that unfamilar, not unreal)?

9. Any idea why nature "tricks" us into thinking that there are laws with (sometimes broken) symmetries, when in reality the symmetries were never there to start with (so there's no symmetry to break)? You say there is a global time, preferred inertial observers, and so on. So for example local Lorentz symmetry is not imposed by any law, but it looks true. Is this something amazing to be explained, and what is the explanation (perhaps some dynamical process as in 5. above might work)?

72. **Lee Smolin**
May 12, 2013

Dear MNP,

Sorry for the delayed response to your excellent questions. btw I am soon to be launching a blog to discuss questions like these with readers. To answer yours:

1) To make the theory predictive in the absence of a detailed model of a bounce I assumed that each black hole yields a single viable universe. Obviously you could complicate the model in many different ways and one could also try to take advantage of progress in quantum gravity since then to model the bounce.

2) This is a good question.

3) What do you mean that they keep up with each other? Isn't that a global time? But of course its not settled, I give several independent arguments, please disagree specifically with them.

4) Of course, but SD shows a global time can be compatible with experimental tests of SR and GR. There could of course be other choices. I mention several in the notes.

5) This is a good research program to pursue.

6) No

7) Its an interesting hypothesis to investigate.

8) The point is that an arbitrary graph with some graph metric on it does not embed isometrically in a low dimensional space

9) There are dynamical systems whose low energy excitations have emergent or accidental symmetries.

73. **Marcus**
May 14, 2013

There is a great 58 minute Smolin core-dump on Edge
http://www.edge.org/conversation/think-about-nature
Lee thanks, it puts a lot of things together.

74. **Marcus**
May 15, 2013

Having read the book, as well as the Edge piece http://www.edge.org/conversation/think-about-nature
and watched the Quantum Foundations seminar talk
http://pirsa.org/13020146/
I would say that the main idea being supported is to find and test a *more elegant* description of how Nature works by explaining how the existing body of physical law came about.

It doesn't matter whether you call a proposed governing principle (by which known regularities could have developed) a description of "how Nature works" or a "meta-law" or simply a more basic law of Nature.

What seems to matter is that the body of physical law has become seemingly over-intricate and arbitrary. This makes for a general desire for one deeper law that explains where all the previous laws "came from", and Smolin presents a case, based on many interesting arguments, that the most promising area to search is in the direction of historical/evolutionary principles.

A frivolous paraphrase of one example he presents might be "Nature tends to run in ruts it made itself earlier" or "being the universe is habit forming". This harks back to the examples of biological evolution and the development of English Common Law by the accumulation of precedent. It seems to me that any such principle would have to be formulated with the help of some new mathematical language before it could be used predictively and tested. And it would itself be outside of the evolutionary process.
Any description of how Nature works is going to have some postulate or rule that does not itself evolve and which is not reciprocally acted upon by what it guides.
So the main idea is not to reject timeless law, expressed in partly mathematical language, it is to find *more elegant* description and *more economical* explanation.

And the case is made that this must involve the temporal development of previously recognized regularities–so it looks like we must accept (I think he would say) the idea of a preferred global time slicing of events.

75. **David Gillett**
May 15, 2013

A message for Lee: The one area that really raises questions for me is precedence: how and why would that become established, causing universe wide behaviour under similar circumstances to be consistent and thus create a "law" ?

David Gillett

76. **Marcus**
May 15, 2013

David, I hope Lee responds to your question, but would like to observe that I addressed that issue. Precedence, if you can pull off a workable formulation, is just a more elegant basic law of nature. It is a single law that might be capable of explaining how the previously recognized physical laws arose. It isn't any more crazy to assume precedence (if you can formulate it) than any other fundamental law. How does an electron know it's and electron and that it should behave such-and-so way? Indeed precedence sounds potentially LESS preposterous to me than other fundamental laws—it is just that one has gotten used to it that one doesn't notice how crazy it is that matter should behave according to the Standard Model.
The basic idea is that somehow (we don't know how) Nature is able to blindly stumble into repetitive behavior and acquire habits. That Nature has something analogous to an associative memory of its own past behavior.
I see no qualitative difference between assuming that (if one can formulate it clearly) and assuming Newton's Laws of Motion. It's just that we are used to Newton's laws.

The problem I see for Smolin and collaborators would be with formulation. It is similar to the difficulty of programming artificial intelligence, one must be able to make a toy model of Nature that uses a *similarity* criterion for recognizing the similar past situations which would constitute precedents to be sampled randomly. It seems similar to the AI problem of programming an *associative memory* to use in constructing some almost completely unintelligent habit-forming automaton.

Newton explained his absolute space and time as Sensorium Dei. That was weirder than this, or so I think anyway. Basically it is a fun idea whose time is come. Hopefully they can make a toy model work and actually generate some law-like patterns of behavior.

77. **T. H. Ray**
May 16, 2013

MNP's question # 3 hit a chord:

"What if you had two or more totally causally disconnected universes: would they need to share a global notion of simultaneity with each other?"

I think yes! Lee's rhetorical resp0nse — "What do you mean that they keep up with each other? Isn't that a global time?" — agrees, yet I think more needs to be said about what "causally disconnected" means. In Einstein's relativity, simultaneous though relativistically separated events are causally disconnected; multiple universes sharing a boundary and also sharing simultaneously branching binary events make Smolin's "real time" proposition even more attractive. Given, as Wheeler noted, "The boundary of a boundary is zero," spatial boundaries do not obviate analytical continuation of the time metric — as I have found, general relativity suffers no loss of generality when described as a model bounded in space and unbounded in time (rather than, as conventionally thought, bounded in time at the singularity of creation and unbounded in space).

Tom

78. **Lee Smolin**
May 16, 2013

Dear David,

That is an excellent question and one that I am focusing on as I try to develop the hypothesis of precedence further. As formulated in the original paper arXiv:1205.3707 I just assume it does so. What intrigues me is that the question of how precedence develops might be investigated experimentally by studying quantum systems complex enough to have no natural precedents. Nor is it necessary to have a detailed model of how precedence develops to test for deviations from the expected Schroedinger evolution in such cases.

Let me stress that this idea, like CNS, doesn't have to be right to prove the point that theories of evolving laws are testable and hence at least as scientific as the claim that laws never evolve-which leads to attempts to explain the choices of laws that have no testable consequences.

Lee

79. **srp**
May 17, 2013

Dear Prof. Smolin:

Thank you for your earlier correction; I re-checked the relevant slide and saw that I had misread it.

One warning/heads up: The notion of laws evolving as the universe gains "experience," and your ideas about precedence bear a superficial resemblance to the concept of "morphogenetic fields" propounded by Rupert Sheldrake. He has argued for example, that when one lab gets a certain chemistry experiment to work then all future versions of that experiment at other labs are more likely to work. His ideas have the flavor of the paranormal and attract those who are sympathetic to such things. You might want to be prepared with a response in the not unlikely event that your work is cited in support of his theories.

80. **Marcus**
May 19, 2013

I would like to ask Lee the following question, if he is still checking this blog for discussion and wishes to respond. Can you imagine a precedence process by which the quantum theory called Causal Dynamical Triangulations arises?

CDT has a preferred time or at least a preferred foliation, and is built up in layers with a certain thickness.
(This resembles what was described in the February seminar talk.)
And there is a certain random probabilistic process that governs the formation of the each layer (based on a Regge-like partition function). That could be seen as similar to "sampling from the past" in the precedence process—though not identical by any means! So I can just barely imagine how something like CDT might arise through geometric habit-formation. I'd like to know what other people (Lee in particular) think about this.

81. **Marcus**
May 19, 2013

Here is Chris Kennedy's 5star review on Amazon of Smolin's book.
http://www.amazon.com/review/R2F83657IVWFJ9
If you happen to read it and find it helpful consider clicking the accompanying "yes" button, because something like Smolin's metalaw operates at Amazon. Customer reviews that don't get noticed and don't acquire "yes this was helpful" votes stay at the bottom of the pile and may not ever get noticed and yessed enough to rise the top of the pile where people see them.

Right now the top of the pile of "customer" reviews is all composed of quite disagreeable 2star reviews and the like. It appears that visitors see the half dozen negative reviews, mark them as helpful, if for no other reason than because they help one decide not to delve further and take any further interest in the book. So unless you are experienced it's actually a bit difficult to even FIND Chris Kennedy's review (which only 8 people have approved of so far).

I also contributed a review which is here:
http://www.amazon.com/review/R2Y6HLLSQH5KSN
It appears to have little chance of ever getting read, but at least adds to the raw number of 5star reviews.

82. **Chris Kennedy**
May 21, 2013

Marcus,
Thanks for posting the link that goes directly to the review and my comment below the review. I wanted to make people aware that part of my criticism I originally offered was a little reckless when I said: "he didn't specifically explore how time dilation (during relative motion and proximity to mass) fit in with a real, global theory of time" and after Lee straightened me out on the applications of shape dynamics, I posted the correction. However, as someone who is interested in the nuts and bolts of the nature of time, any theory for how and why time dilates deserves much more discussion and clarification. Putting all of our eggs in the shape dynamics basket might be too simple of a path to take at this point.

Arkani-Hamed Colloquium

April 30, 2013
Categories: Favorite Old Posts, Uncategorized

Nima Arkani-Hamed was here at Columbia yesterday to give the physics colloquium, which clocked in at a bit over 1 hour and 45 minutes. He did reveal the secret of why his talks are this long: when invited to give a 1 hour colloquium, he plans on talking for at least 1 hour 30 minutes. The content of the talk was similar to many others he has given recently that are available on the web, see for instance this one at the IAS, this recent one at BNL, or for a written version, see here.

As a performer, he's a powerful speaker: smart, vigorous, and supremely self-confident. His arguments lead to "inevitable" conclusions, not just implying results but "nailing" them. It's clear why he's the most influential person in the field these days. With most theorists made worried and unsure by 40 years of failure to get anywhere in their efforts to improve on the Standard Model, he knows exactly what he thinks and will tell you forcefully what you should think. The fact that none of the ideas about BSM physics he is famous for (large extra dimensions, split SUSY, Little Higgs, etc…) have ever worked out doesn't seem to slow him down, and he has a professorship at the IAS and a $3 million prize from Yuri Milner to back him up.

Despite his long-time advocacy of SUSY, according to Arkani-Hamed, the negative results from the LHC are "not making many of us worried about SUSY". He (accurately) points out that he's not one of those like Gordon Kane who for decades has been predicting the discovery of superpartners to be six months away. It has long been clear that the simplest versions of SUSY should have shown up at LEP and the Tevatron, and pre-LHC the lack of any indirect evidence for SUSY indicated to him that it was unlikely to show up at the 8 TeV LHC. So, by his lights, there's no reason that LHC results so far should cause any new worries about SUSY, beyond those he already had pre-LHC. On the more limited question of whether a "natural" version of SUSY will work out, one where the superpartner masses just barely avoid large amounts of fine-tuning, a year ago (see here) he was saying we were at the "eleventh and a halfth hour" for this possibility. Now that the 8 TeV results are here (and negative), he argues that it is only with the 2015 data that the results will be decisive. The current wisdom about "natural SUSY" I guess is summarized in slide 8 here: **Keep Calm and Wait for 14 TeV**.

The main point of the talk was one that Arkani-Hamed has been consistently making for nearly a decade, that the role of the LHC is to decide between two possible futures for fundamental physics:

- The small value of the Higgs mass (in Planck units) has a "natural" explanation, most likely using SUSY, in which case we spend the rest of our lives unraveling the complexities of a SUSY-extended Standard Model.
- The small value of the Higgs mass (in Planck units) indicates "fine-tuning" that can only have an anthropic explanation, just like the one for the CC. In that case, we live in a multiverse, with physics determined by something like the string theory landscape. About this whole conceptual framework, he says the "ideas are so poorly defined, not clear if they make any kind of mathematical sense", and it's "not clear progress will happen anytime soon" but, no need to worry or get discouraged, since this is an "attractive problem".

Based on the LHC results so far, it looks like all evidence is that we're headed to the second alternative.

Arkani-Hamed's talk was structured so as to present a long chain of argument (needing at least 1h 30 min to explain) leading to these two alternatives. One of the alternatives (SUSY naturalness) is essentially already dead, with the die-hards intent on hanging on a couple more years. The other is essentially what David Gross has called "giving up": you just announce that the problems you haven't been able to solve can never be solved. In this vision, the 20th century with its huge success at finding a highly predictive, mathematically beautiful fundamental theory was an aberration caused by only being able to see physics at energies way below the Planck scale. In this new 21st century physics, you just postulate that at higher energies things are much more complicated, in ways we can't hope to ever know, and theorists devote their lives to making excuses, not predictions. Witten may end up being right that "string theory is 21st century physics that fell into the 20th century", in a much more negative way than he intended.

If a long, complicated argument leads you to the conclusion that the only viable alternative is to give up, then it seems to me you have two choices: give up, or examine more carefully your argument. A much more interesting and more useful talk than Arkani-Hamed's would be one less devoted to forcefully insisting on the conventional chain of argument based on the technical problem of sensitivity of the Higgs potential to the cut-off, instead looking carefully for weaknesses in the argument (one possibility is discussed here). Arkani-Hamed is a brilliant physicist, but this may be a time when what is needed is not self-confidence in the power of one's arguments, but instead a suspicion that one has been making a mistake somewhere for quite a while now.

Comments

1. **Nex**
   April 30, 2013

   Science advances one funeral at a time.

2. **BB**
   May 1, 2013

   " A much more interesting and more useful talk than Arkani-Hamed's would be one less devoted to forcefully insisting on the conventional chain of argument based on the technical problem of sensitivity of the Higgs potential to the cut-off, instead looking carefully for weaknesses in the argument (one possibility is discussed here). "

   I think it is simply preposterous to present the work of Lykken at your link as a valid alternative, as I have already explained in the comments to that post. Conformality cannot be invoked, per se', as a solution to the hierarchy problem as long as other scales exist in the deep UV and your CFT isn't screened from them (as it happens instead e.g. in RS or SUSY theories).

3. **Neil**
   May 1, 2013

   I'm not a physicist, just an interested bystander, but surely there are more than two futures, one of them depressingly bleak. There is BSM physics to explore at the high precision, high intensity frontier and at the cosmological frontier. It will not all end at 14 TeV, I hope.

4. **Shantanu**
   May 1, 2013

   Peter, did someone ask any of these hard questions after the talk?
   or there was no time for questions, given the length of the talk?
   shantanu

5. **Dave Miller in Sacramento**
   May 1, 2013

Peter,

Isn't part of the issue here unrealistic expectations about the pace of progress?

The first three-quarters of the twentieth century saw an unprecedented explosion in the rate of discoveries in physics, partly because of the early revolutions in relativity and quantum theory, and, then, after WW II because of the huge influx of government funds to pay all of us and to build bigger and bigger accelerators.

For reasons we all know, that post-war boom just could not continue — in particular, we could not keep exponentially growing the size (and cost) of accelerators. So, it seems to me that the present slower pace is really a return to historical normalcy. We had a period where we frantically grabbed all the low-lying fruit, and, now, we need to sit back and digest the fruit.

Personally, I think we have a lot to digest concerning the structure of QFT and even QM itself. It took quite a while to go from Maxwell to QED and even longer to go from Newton to Einstein. Why should we expect any progress forward from the Standard Model to be fast or easy?

Dave

6. **Jeff**
May 1, 2013

"Armani-hamed is a brilliant physicist" – what exactly does that mean? I'm a mathematician, math is hard because we actuallly have to prove things. I've always thought physics was hard because things had to WORK. Arkani-Hamed doesn't have to prove anything, and from what I can tell he hasn't come up with anything that works.

7. **Peter Woit**
May 1, 2013

Nex,

If you're referring to Arkani-Hamed, he's about the youngest person of any prominence active in this field, and very influential on those younger than him. On the other hand, if you're referring to me, yes I'll likely be long gone while he's still around…

8. **Am I Lloyd**
May 1, 2013

It's a little troubling to hear a scientist speak of so many results as "inevitable", especially in a field like string theory.

9. **Peter Woit**
May 1, 2013

BB,
I wasn't claiming Lykken's talk as a solution to the problem, just pointing it out as an example of ideas about solutions to the problem that it would be more fruitful to examine, in preference to arguing that no SUSY at the LHC implies anthropics as the only way out.

I avoided going on here about my own take on this, since I do this endlessly, most recently in the last posting a couple days ago. There seem to me many more serious problems with the Higgs field than its quadratic sensitivity to the cutoff, and the argument from that technical issue to anthropics via the philosophy that the SM is just an effective theory, with something quite a bit more complicated at a high scale, is one I keep pointing out as likely flawed.

10. **Peter Woit**
May 1, 2013

Neil,

Arkani-Hamed near the end did quickly go over his favored scenario for BSM physics we might hope to see (mini-split SUSY), but didn't seem particularly optimistic that there was really a significant chance of the that scenario working out.

11. **Peter Woit**
May 1, 2013

Shantanu,

Little time for questions after such a long talk, and nothing like a "hard question" was asked.

12. **Peter Woit**
May 1, 2013

Am I Lloyd,

This talk didn't really deal with string theory at all, although in some sense failed string theory ideas motivate the overall framework.

13. **Bob Jones**
May 1, 2013

A much more interesting discussion of supersymmetry recently appeared on the arXiv:

http://arxiv.org/abs/1304.7798

14. **Georges**
May 1, 2013

I am really shocked at the chutzpah of that person explaining that his "secret" is to plan a one hour thirty minutes talk when invited to give a one hour colloquium. And then he even talks for fifteen mors minutes! This is despicable behaviour, encouraged by the audience not leaving the room in protest. I know that this happens frequently, but I have decided to no longer tolerate such misbehaviour and encourage you to do likewise.

15. **Peter Woit**
May 1, 2013

Georges,

To be fair to Arkani-Hamed, I shouldn't have called it a "secret". At the beginning of the talk he warned people basically that he intended to go on for at least an hour and a half, and said that he didn't mind at all if people left while he was still talking. But, yes, the word "chutzpah" did come to mind…

16. **P**
May 1, 2013

Bob,

The article you cited is a detailed analysis of superstring perturbation theory, and the results have no immediate / obvious connection to the N=1 field theory story Nima discusses.

Cheers

17. **Bob Jones**
May 1, 2013

P.,

Yes, I know.

18. **Nathalie**
May 2, 2013

Dear Peter,

Thank you for the reference to Arkani-Hamed's IAS talk. It was a nice talk.

AH argues that in all possible worlds with proper long distance behavior you have (assuming the validity of local quantum field theory) consistent theories only for fundamental particles with spins 0, ½, 1, 3/2 and 2. Because those with 0, ½, 1 and 2 have already been seen the 3/2 must also exist. Hence supersymmetry.

It seems to me that one could just as well argue that since nobody has ever seen the spin 2 the spin 3/2 will also be invisible to us humans. We must humbly admit that we don't know what we are talking about when we make such strong statements.

Another comment is that the proper long distance behavior of electromagnetism is due to the fortunate fact that matter happens to be electrically neutral at large distances. Otherwise, AH could not have presented his talk.

19. **piscator**
May 2, 2013

Jeff: It means he is a charismatic speaker who has a lot of power in the community. Seriously, you are totally correct. Physics is about stuff that works and agrees with experiment. People whose reputation is built on models that do not work and do not agree with experiment are not brilliant physicists whatever other qualities they may have. Anyone who thinks otherwise needs to recalibrate their BS detectors.

20. **JJ**
May 2, 2013

Arkani-Hamed is NOT a brilliant physicist; he is in fact a good car salesman. Of course he knows a lot about his cars, but he couldn't care less about customers. All he really cares about is to make money, and he has proven his ability to do that. The way he does this, as all good car salesmen do, is to blow his customers away by his show business skills. So DO NOT listen to him, as he is full of BS.

21. **M. Wang**
May 2, 2013

In most professions in the US, the best bullshiters rise to the top. The only conclusion here is that particle physics is no longer science but just a profession, but this should not be news to readers of your blog.

22. **Sum Perspektive**
May 3, 2013

JJ (and Peter, as the moderator)

These personal attacks on N. A-H. are really inappropriate, and foolish. There is an element of truth in the claims that Nima is a good showman, this cannot be denied. But anyone who denies his brilliance, in terms of an incredible track record of remarkably creative approaches to very (Very!) difficult problems, is simply foolish (or possibly jealous, I suppose). Think back to before large ED's, Little Higgs and Dimensional Deconstruction, and you had a situation where almost everyone worked solely on SUSY models. Nima was one of the leaders whose originality opened up whole fields, that were justifiably of immense interest. Sure, these things didn't work out, and Nima has largely moved on to other topics, like a good scientist would. But without his ideas it seems likely that many more people would have been working on the same old (failed) SUSY ideas. Nima's ideas were precisely the kind of fresh and original approaches that (I understand) Peter would advocate (ie exploring more general possibilities, which are hard to come up with).

(For the record, I completely disagree with the "SUSY or anthropic/tuning" divide that NAH discusses of late, and suspect there is another way of understanding the issue of E/W naturalness. But my dislike for these ideas does not prevent me from recognizing his abilities).

M Wang: The presence of "non purists" in science is as old as science itself. Read Einstein's talk at Planck's birthday, where he describes Planck as one of the few (presumably like himself) who would not be equally happy in some other ladder-climbing profession, as one who instead worships at the temple of science, so to speak. Most in the field are trying to get great jobs, and are willing to spew rubbish in the name of their cause, but this is not a recent development.

23. **Peter Woit**
May 3, 2013

Sum Perspective,

I don't disagree. It's interesting to note that for the last few years Arkani-Hamed has mostly abandoned BSM phenomenology and concentrated on working on a mathematical physics topic, that of understanding the mathematical structure of certain scattering amplitudes in terms of the cell structure of the positive Grassmanian. He advertises that with some of the same showman techniques (and the same tactic of taking up to two hours for a scheduled one hour talk…), but it's a much more interesting and fruitful thing to be pursuing than failed ideas about BSM physics.

It's too bad though that he maintains his other career as promoter of those failed ideas, where, unlike Kane who just denies reality, he is following them to their logical and empty conclusion.

24. **piscator**
May 3, 2013

I'll say this again: brilliance in physics comes from discovering stuff that works and agrees with experiment. I find talk of incredibly creative approaches to difficult problems a bit creepy and fawning to be honest. It's like calling a mathematician brilliant because he made twelve different attempts to prove the Riemann hypothesis and none of them worked.

And for the record, I don't think the ideas you mention are actually particularly good. Really good ideas tend to have an impact trajectory that grows over time as their profundity becomes appreciated and more and more applications are found (eg AdS/CFT). Something like LED is a more like a pump-and-dump stock.

There's nothing wrong with not being a brilliant scientist, that's the situation pretty much everyone is in, but we have to be honest about what is necessary for first rate science (agreeing with experiment) and what is not (being charismatic and giving great talks).

25. **A**
May 3, 2013

If a mathematician came up with 12 *original* ideas to prove the Riemann hypothesis, none of which were obviously wrong, I think a lot of people would call that mathematician brilliant, whether or not the proofs actually worked out.

I'm not really a big fan of how Nima presents things, and a lot of his ideas can be pretty half-baked, but the fact that he's actually producing new ideas (even if they've all been completely wrong) puts him ahead of the game for most of the field these days.

26. **Bill**
May 3, 2013

A, this is complete nonsense. There is a big difference between "none of which were obviously wrong" and "even if they've all been completely wrong". I don't know one example of a mathematician that fits your description and has the same stature as NAH.

27. **different fields**
May 3, 2013

Math \neq physics, otherwise experiment wouldn't be needed. Theoretical physics isn't just about proving "theorems" about the real world, nor is it *just* describing experimental results. Look at the history of 20th century theoretical physics, there were a myriad of wrong directions that still led to a lot of progress in understanding our universe. There were also many things that were proposed as an incorrect description of the phenomena they intended to describe, e.g. Yang-Mills Theory, but as we all know they turned out to be pretty important much later. Instead of all the vitriol and what sounds like Fox news pronouncements in the comment section about personalities, it might be useful for people to understand the field a little better before jumping to such strong conclusions.

28. **Bill**
May 3, 2013

different fields,
I do appreciate the difference between math and physics, and I do appreciate the way physicists think. Mathematics would not be anywhere near where it is today without physics. But we are talking about some fairness and balance (speaking about Fox News). Yes, it is not all "natural selection" in mathematics and there is some "breeding" by way of awards and recognition of certain people, results and areas. But nothing like this. At least, Witten is a genius who contributed enormously to mathematics even if string theory turns out to be completely irrelevant to physics. But can you say the same about NAH? If string theory turns out to be a failure, what exactly will be left to justify the Fundamental Physics Prize?

29. **Nex**
May 4, 2013

@7 Peter: It's just a quote due to Max Planck which aptly describes how prominent people in science are unwilling to change their established views even in the face of contradictory evidence. It takes new generations to really move on.

And we will likely all be dead by the time string theory is completely abandoned.

30. **David**
May 4, 2013

Bill,
If string theory is proven wrong that will have minimal relevance to Nima's work and the Fundamental Physics prize would still be justified. Nima's work on scattering amplitudes is an example of how ideas from ST can help develop and lead to advancements in our understanding of QFTs and gauge theory, as is evident just by looking at the kind of talks given at Strings conferences. You can't completely untangle the two.

Also I think the comparison to mathematics is unfair in that physicists really have to deal with the arbitrariness of nature. It would be like a mathematician finding 12 different solutions to a problem and having their result rejected by his/her fellow mathematicians because they don't like the method. In physics there are plenty of legitimate answers to open questions, but to a certain extent it becomes a matter of luck if its actually realized in nature.

31. **Bill**
May 4, 2013

Does anybody know what NAH is talking about after 1:34:10 mark in this video?

http://www.youtube.com/watch?v=rKvflWg95hs

What are those "new remarkable mathematical structures" related to number theory?

32. **Jim Akerlund**

May 5, 2013

I just saw the video and the “new remarkable mathematical structures” comment occurred at the 1:36:10 time mark for me. The video was released on youtube on 4/12/13 and looks to be about a six months old so I am guessing that the mathematical structures he is talking about is based on his recent paper about positive Grassmannian, arXiv:1212.5605 It is his only paper in recent years that was cross posted into these math subjects Algebraic Geometry (math.AG); Combinatorics (math.CO).

Jim Akerlund

33. **chris**
May 6, 2013

“In physics there are plenty of legitimate answers to open questions, but to a certain extent it becomes a matter of luck if its actually realized in nature.”

excuse me for stating this so bluntly, but that and exactly that is the kind of unscientific laissez-faire approach towards understanding nature that stalled the advance of science from Aristotele to Copernicus for almost 2 millenia.

physics is not an intellectual game of abstract brilliance. physics is about understanding nature. there is a very definite line between true and false and this is experiment. a theoretical concept, however brilliant it might seem, is just plain wrong if disproven. it might still serve as advice for future generations how one can fail. but that is it.

wrong is wrong and faliure, as spectacular as it might be, is still flaiure. i mean, please, just reflect for a second on the hubris of a statement like “but to a certain extent it becomes a matter of luck if its actually realized in nature”. i could understand it kind of if you had said something like “but to a certain extent it becomes a matter of luck whether one can guess the correct laws of nature” – even if i strongly disagree already with that statement. but the thought of physics as a purely intellectual effort that might or might not have to do something with the world outside (and whether it does – coincidentally i suppose – not having any bearing on the brilliance and admirability of the ideas) sort of lets me loose faith in our discipline.

just to get things straight: it is a choice one makes early in a particle physics career if one indulges in unfounded speculations and produces model after model that can be searched for and falsified. there is the alternative path of sticking close to experimental fact and doing the hard, grinding groundwork of amassing new knowledge. of course the first one is more sexy and of course the grandiose ideas get more media attention (which seems to be so central these days), but working science needs to weed out failed branches mercilessly and fast once they have died. there are too few people willing to provide this essential service to the community today. this blog being one notable exception.

once all the fuss has cleared, i hope things will be back to normal again and theoretical particle physics will be once more be about understanding nature and not about intellectual ego trips.

34. **Mitchell Porter**
May 6, 2013

“there are too few people willing to provide this essential service to the community today”

In his talks, Arkani-Hamed says technicolor is dead, focus point supersymmetry doesn’t work, and infrared modifications of gravity are outlandish. Is he one of the few people boldly rejecting failed hypotheses, in your opinion?

35. **Peter Woit**
May 6, 2013

Mitchell Porter,

There’s never been a shortage of prominent theorists willing to denounce unpopular theoretical ideas. Yes, Arkani-Hamed puts tombstones with “Technicolor RIP” in his talks and announces that he’ll kill himself if the Higgs is a technidilaton, but interest in technicolor has always been a minority viewpoint. The real question is when if ever he’ll start denouncing mainstream ideas that have failed.

36. **Bill**
May 6, 2013

Jim Akerlund,
Between 1:34:10 and 1:36:10 he is talking about deep connections to number theory and, in particular, Riemann hypothesis. The introduction to the paper you mentioned, arXiv:1212.5605, sounds very grandiose and mentions “remarkable new structures” too, but I did not see anything connected to number theory.

37. **Peter Woit**
May 6, 2013

Bill,

I think this is the sort of thing he is referring to

http://arxiv.org/abs/1105.2024

As usual with Arkani-Hamed, you might want to take some of his claims with a grain of salt….

38. **Bob Jones**
May 6, 2013

Bill and Peter,

I think Arkani-Hamed is probably referring to some connection between gauge theory scattering amplitudes and Grothendieck’s standard conjectures on algebraic cycles.

http://en.wikipedia.org/wiki/Standard_conjectures_on_algebraic_cycles

As Peter has already pointed out, there are results from the theory of motives that allow physicists to simplify expressions for these amplitudes. This is the “symbol” technique of Goncharov, and the original reference is

http://arxiv.org/abs/1006.5703

39. **Peter Woit**

May 6, 2013

Bob Jones,

If you know of any significant connection between new techniques in gauge theory scattering amplitudes and the Grothendieck conjectures you link to, I'd be curious to see a reference for it, since I've never seen such a thing.

To the extent that these new techniques involve use of the cell-structure of the Grassmanian, it's not surprising that one gets connections to quite a few areas of mathematics. Connections to deep conjectures about algebraic cycles in general, and specifically arithmetic aspects would be a lot more surprising (and interesting…)

40. **Bob Jones**
May 6, 2013

Peter,

There was a talk by Goncharov at this conference

http://www.birs.ca/events/2012/5-day-workshops/12w5053/videos

where I think he mentioned something about the standard conjectures. Unfortunately, the video is not online, so I don't really have a reference.

But yeah, the cluster algebra stuff that people are using to study the Grassmannian arises in many other parts of mathematics and physics. I've also heard Arkani-Hamed suggest in some of his talks that there might be physical reasons for the ubiquity of cluster structures since all of the theories that he and his collaborators are studying come from the six-dimensional (2,0)-theory.

41. **David**
May 6, 2013

Chris,

Sorry if you misunderstood me. You wrote

wrong is wrong and faliure, as spectacular as it might be, is still flaiure. i mean, please, just reflect for a second on the hubris of a statement like "but to a certain extent it becomes a matter of luck if its actually realized in nature". i could understand it kind of if you had said something like "but to a certain extent it becomes a matter of luck whether one can guess the correct laws of nature"

What I meant was the latter, that its a matter of luck in guessing the correct laws of nature. What I had in mind was pure SU(2) Yang Mills theory as was pointed out earlier. Its a beautiful idea but phenomenologically is off. The only point I was trying to make is that you cannot compare physics and mathematics in terms of how you define success because of the different ways in which we say something is true. In mathematics it has to be a logically consistent and rigorous solution. In physics it has to be a logically consistent (not rigorous obviously) solution and be observed. Its having your model be the right model of all the ones created that I think has an element of luck to it.

Also I think you have a distorted view on what most particle theorists do. I'm sure that the number of people looking hard at the data and numerical work for the LHC and other particle experiments is much larger then the people who work on the sexy topics of SUSY, ST, and what have you. Its just the latter you'll hear more about on this blog and in the media (which makes sense).

42. **Mitchell Porter**
May 7, 2013

Do the people complaining about the supersymmetric hegemony actually read hep-ph? There are about a hundred non-susy, beyond-standard-model papers there, every month.

43. **Julian Frid**
May 13, 2013

Ready for a smart, free, in-depth conversation with Nima Arkani-Hamed? Visit the Ideas Roadshow interview.

Please email me if you should any comments and we'll post them. Actually email my colleague since this should be his job: taylor@ideasroadshow.com

Supersymmetry and Beyond

May 1, 2013
Categories: Book Reviews

Back in the year 2000, Gordon Kane published Supersymmetry: Unveiling the Ultimate Laws of Nature, a popular book promoting supersymmetry and string theory. The thrust of the book was that there was already indirect evidence for SUSY, with confirmation by discovery of superpartners due to come soon from LEP (which was running at energies near 100 GeV/beam) and the Tevatron (where Run II at high luminosity and nearly 1 TeV/beam was to start in 2001). The LHC was also discussed, mainly as the place that would confirm and extend the LEP/Tevatron superpartner discoveries.

Thirteen years later, with no hint of SUSY showing up as promised, not only at LEP/Tevatron energies, but also at the much higher energies and luminosities of the 8 TeV LHC, Kane has a new popular book promoting supersymmetry and string theory, entitled Supersymmetry and Beyond. It includes his claim to have predicted the Higgs mass using string theory (see Matt Strassler's take on this here, mine here). Much of the book though consists of exactly the same text as the 2000 version.

How does Kane handle the detailed failed predictions of the 2000 edition in the new 2013 version? Basically by editing them out, with no indication to the reader that this has been done. What's the right word to describe the result of an Orwellian exercise like this? You can make up your mind about that yourself, since I've gathered together here some examples of the book text, showing the edits that were done to create the new version.

Pages xvii/xviii

> Supersymmetry is still an idea as this book is being written ~~(mid-1999)~~ in late 2012. There is considerable indirect evidence that it is a property of the laws of nature, but the confirming direct evidence is not yet in place. That is not an argument against nature being supersymmetric; rather, the ~~accelerator~~ collider faciliti~~es~~y that could confirm it (the LHC) ~~are~~ is just beginning to cover the region where the signals could appear ~~(Chapter 5)~~.

Pages 2-3/3

> If we understand supersymmetry and its implications correctly, direct experimental evidence for supersymmetry will be found in the next few years – possibly soon after this book is published (or, with great luck, even before).

Pages 13/16

> ~~Only now are~~ colliders and detectors at laboratories are now achieving the energies and luminosities (amounts of data) and sensitivities needed to explicitly detect the superpartners ~~explicitly~~, at least if our thinking about their properties is more or less right.

Pages 70-71/77-79

> The manner in which supersymmetry explains the Higgs physics is elegant and has important consequences for how we expect to test supersymmetry experimentally. It is rather technical. ~~A more detailed description is given in Appendix B; here I will give a short version.~~ There are three parts…
>
> Therefore, the supersymmetric Standard Model explanation of the Higgs mechanism would not make sense unless ~~the~~ some superpartner masses were not much larger than the Standard Model masses they explain. That gives us an estimate of the masses we should expect the superpartners to have as we search for them~~, and it tells us at what stage we should question the validity of the theory if the superpartners have not been detected~~. Such estimates are only approximate, but luckily the expected masses are small enough that they imply the superpartners should be detected soon.

Appendix B was deleted entirely, it contained the text (page 156)

> ~~Therefore, the superpartner masses cannot be very much larger than the Z boson mass if this whole approach is valid. This is the only place where we can use the theory to relate the unknown superpartner masses to known masses, so on the one hand, it is a major test of the correctness of the supersymmetry explanation of the Higgs physics, and on the other, it is the most significant reason whey we expect the masses of the superpartners to have values that allow them to be produced at Fermilab or even LEP. This connection also suggests that if the superpartner masses are much larger than the Z boson mass, then the apparent success of the supersymmetry theory in explaining the origin of the Higgs physics of the Standard Model could be an accident.~~

Pages 88-89/89

> Several arguments imply that some sparticles are within the reach of ~~Fermilab~~ the LHC. ~~The strongest~~ One of the most appealing is based on the explanation supersymmetry gives for the Higgs mechanism of the Standard Model, as described in ~~the last chapter~~ Chapter 7. Basically the qualitative argument is that ~~because~~ since supersymmetry provides the Higgs mechanism that accounts for the masses of W and Z, ~~the~~ some sparticle masses cannot be much heavier than the W and Z masses themselves. ~~Fermilab has already produced and detected thousands of W's and Z's.~~ When this argument is ~~framed~~ put in a technical form, it implies that gluinos and probably charginos and neutralinos ~~and stops~~ should be ~~in the Fermilab~~ within the LHC reach. ~~If they are not, the impressive successes of supersymmetry listed at the beginning of Chapter 4 may be meaningless coincidences.~~ There are some arguments, both theoretical and phenomenological, suggesting that squarks and sleptons will be too massive to produce at the LHC.

Chapter 8, on SUSY implications for matter/anti-matter asymmetry, proton decay, rare decays like mu to e-gamma, and CP violation has been deleted. Appendix D, on large extra dimensions, has also been completely deleted.

Witten's preface has been edited:

> Experimental clues suggest that the energy required to produce the new particles is not much higher than that of present accelerators. If supersymmetry plays the role in physics that we suspect it does, then it is very likely to be discovered by the ~~next generation of particle accelerators, either at Fermilab in Batavia, Illinois, or~~ Large Hadron Collider (LHC) or its upgrades, at CERN in Geneva, Switzerland.

Comments

1. **In SUSY we trust**
   May 1, 2013

   We admire the consistency and the persistency of theorists who always predict that SUSY will show up at the next collider.

2. **johnnythelowery**
   May 1, 2013

Shameless is Kane. Kane is shameless. Kane is therefore Supersymmetric!
Good work and thanks for your Eagle eyedness.

3. **eric blare**
   May 1, 2013

   What's the right word to describe the result of an Orwellian exercise like this? The word is 'crimethink'. Email Gordy Kane a pdf or scanned copy of the above, with a prefatory comment that this is doubleplus ungood.

4. **jinb**
   May 1, 2013

   While there's no arguing against Kane's blind faith in supersymmetry, it's no more an argument against the appeal of the theory as a possible well-motivated mechanism in nature than to quote the rantings of an extremist muslim as an argument against religion being a bad idea in general.

5. **Colin Rosenthal**
   May 2, 2013

   He needs to future-proof his sentences – "Surely the workers will shake off their chains and rise to overthrow their masters within the next decade.". You don't even need to change that one for the second edition.

6. **theoreticalminimum**
   May 2, 2013

   Reminds me of Jonah Lehrer's copy-pasting/editing. Shame on Gordon Kane! Shame on the publishers!

7. **Bob**
   May 2, 2013

   The Amazon website has glowing reviews from Brian Greene and David Gross. Shame on them also!

8. **Peter Woit**
   May 2, 2013

   jinb,

   There might be evidence of a more general problem if the "extremist muslim" was being supported by the most highly respected other religious leaders, and leading interfaith organizations were awarding him prizes for his public pronouncements, as in

   http://www.aps.org/programs/honors/prizes/prizerecipient.cfm?last_nm=Kane&first_nm=Gordon&year=2012

9. **chris**
   May 2, 2013

   "the supersymmetric Standard Model explanation of the Higgs mechanism would not make sense unless ~~the~~ some superpartner masses were not much larger than the Standard Model masses they explain."

   Epic!

10. **Peter Woit**
    May 2, 2013

    Chris,

    My personal favorite among the edits is

    ~~it tells us at what stage we should question the validity of the theory if the superpartners have not been detected.~~

11. **El-Coco**
    May 2, 2013

    This is such a disgusting fraud on the public that Kane should be thrown out of his university. This is as bad, in its own way, as faking data for a paper.

12. **Shitake**
    May 2, 2013

    Peter: are there some parts of the book that are actually new? What percentage, roughly?

    What is most shocking to me is the edited blurb from Witten. Unless he had explicit permission from Witten for this, that is highly dishonest and unworthy of any scientist.

13. **Peter Woit**
    May 2, 2013

    Shitake,

    I'd say that maybe 1/4 of the book is new, mostly updated material about the Higgs and its discovery, some sections of the old book much more heavily edited than just replacing "Tevatron" by "LHC".

    Besides the edit I showed, a couple other sentences in the Witten preface have also been changed, (basically changing example of recent experimental progress from the discovery of neutrino masses to the discovery of the Higgs). The preface carries a revised date, and I don't see any reason to believe it was changed and reused without Witten's permission.

14. **Shantanu**
    May 2, 2013

Peter, great review.
just out of curiosity, does he mention evidence of neutrino mass as evidence for supersymmetry in his 2000 edition?

15. **old v new**
May 2, 2013

Send copies of the excerpts 'old and new' to Kane, Witten, Gross, Greene and ask for their reactions.

16. **Bill**
May 2, 2013

I don't see what the problem is with all these minor updates (e.g. accelerator to collider)? I couldn't find one change that would qualify as an admission of mistake, it sounds more like bringing the book up to date…

17. **Peter Woit**
May 2, 2013

Bill,

The fact that there is nothing at all in the new book that qualifies as an admission of mistake is precisely the problem (yes, I've shown the complete set of changes in these passages, some of the changes are minor). If you're a scientist and you write a book claiming to have a wonderful theory, that it makes specific predictions that are about about to be tested at LEP and Fermilab, and that your arguments are wrong if LEP and Fermilab don't see what you predict, what should you do after LEP, Fermilab, and the LHC at 4 times the energy don't see what you predicted? If you're going to "update" your book, don't you think you need to admit that your predictions turned out to be wrong? Instead, Kane has just deleted the predictions and the text saying that his arguments were wrong if the predictions were falsified, replacing them with new predictions about future observations at the LHC and future accelerators. Do you see a problem with doing this?

18. **Peter Woit**
May 2, 2013

Shantanu,

About neutrino masses and SUSY, both editions just say:

"Here supersymmetry is helpful in formulating ideas, but it has not played an essential role so far."

In the new edition he adds the claim

"If the underlying theory is supersymmetric, the needed extension of the Standard Model to include neutrino masses can be reliably done."

19. **Bill**
May 2, 2013

Peter,

Okay, I agree that he should have been more honest and admitted that his was too optimistic. But is it not possible that string theory will turn out to be correct in the end, maybe, after making necessary modifications?

20. **Peter Woit**
May 2, 2013

Bill,

I suppose that in principle it's possible that the product Kane is selling will some day turn out to have some value. But while he's selling the 2013 version, he owes it to his customers to inform them that almost the exact same thing in its 2000 version was defective and blew up in his face.

21. **Bill**
May 2, 2013

Peter,
Agreed.

22. **new book**
May 2, 2013

What justification does Kane offer for putting out a new book? Does he refer to his old book, or say why he did not publish a revised of his earlier book? In the latter case, it would be reasonable that most of the text would be the same, with some passages revised.

23. **Peter Woit**
May 2, 2013

new book,

Oddly, there's nothing at all anywhere in the text of the book to indicate that it's a revised version of an older book. On the cover, there's a new title, but also the words "Revised Edition", and the Library of Congress cataloging info on the copyright page describes the book as a revision of the 2000 version. Other than that though, I couldn't find any reference to the earlier version.

24. **Bill**
May 2, 2013

Peter, can you, please, explain in one paragraph to a non-expert what the experiments at CERN say about Standard Model, supersymmetry and string theory? What did the discovery of Higgs boson mean in this context and what do people expect to learn in the next few years? Maybe, you can point me to some relevant posts about this. How can the entire physics community be so wrong and dishonest? Am I wrong at interpreting your blog? Maybe, I should read your book, but some brief overview would be helpful.

25. **Peter Woit**
May 2, 2013

Bill,
Sorry, this is getting off-topic. A one-sentence answer though is that at the LHC the SM is confirmed, no evidence of SUSY, and string theory predicts nothing, results that most of the physics community (the wrong ones are a minority, the dishonest ones a small minority) expected.

26. **Eric**
May 2, 2013

Dear Peter,

I think that you are perhaps the most dishonest, by some margin. The fact is that the expectations for superpartner masses is closely tied to the Higgs mass, which was not known a decade ago. As the Higg mass turned out be somewhat larger than expected, although still below the MSSM bound, correspondingly the expected masses of the superpartners is larger. In particular, to obtain a 125 GeV Higgs essentially requres large radiative corrections from the stop quark. This implies that the stop quark and other scalars may be heavy, unfortunately possibly too heavy to be observed at the LHC. In addition, in the past decade or so, it has been realized that it is possible to have heavy scalars while still maintaining low-fine tuning, e.g. focus point supersymmetry. I see absolutely nothing wrong with Kane providing an update on the situation, which I must say is much more honest than your blatant attempt at negative propaganda.

27. **Mike**
May 2, 2013

Eric,

"As the Higg mass turned out be somewhat larger than expected, although still below the MSSM bound, correspondingly the expected masses of the superpartners is larger. In particular, to obtain a 125 GeV Higgs essentially requres large radiative corrections from the stop quark. This implies that the stop quark and other scalars may be heavy, unfortunately possibly too heavy to be observed at the LHC. In addition, in the past decade or so, it has been realized that it is possible to have heavy scalars while still maintaining low-fine tuning, e.g. focus point supersymmetry."

Assuming what you say is correct, wouldn't a more honest approach by Kane have been to at least mention his previous "predictions" and then use the foregoing as an explanation for why they fell short?

28. **CLAUDIE**
May 2, 2013

As a scientific observer – I truly cannot understand why there is such a disparity of opinion within the scientific community, regarding the topic of SUSY. There seem to be two conflicting camps – those whom see Supersymmetry as the light at the end of the otherwise boringly linear tunnel … and those whom regard those believers in Supersymmetry to be somewhat deluded. From what I have read no one knows or will know the true nature of the universe for some time (i.e. multiple lifetimes away!) so why be so dismissive. Surely to progress we must take a broader and more all encompassing approach. It's hardly a bunch of crackpots that believe in SUSY – it is in fact some of the greatest minds of out time. Nature is phenomenally complex after all…

29. **Peter Woit**
May 2, 2013

Eric and Claudie,

Arguments over the virtues or lack thereof of SUSY extensions of the SM have been going on for decades and will go on for more decades, with experiment for now (and I think for ever…) weighing in on the negative side. But that's not what this posting is about. It is about Kane's new book, and, more specifically, his choice to "update" the old one by just deleting every argument in it that experiment has shown to be wrong, without any acknowledgement that that is what he has done. I've documented accurately that in the posting, up to people to decide for themselves what they make of this.

To editorialize, I think Mike is quite right to point out that Kane owes his readers some explanations.

30. **DB**
May 2, 2013

Don't forget that SUSY was a useful argument to help justify the various colliders mentioned above, even if it was never the primary reason for building any of them. This is why physicists tolerated the nonsense hawked by the likes of Kane and Greene for so long. Now it's clear that there is virtually no prospect of the LHC ever seeing it, and very little prospect of a successor machine being built in our lifetimes. Therefore SUSY usefullness has expired and it's now an embarrassment. For this reason, it's increasingly career-toxic for young physicists to be seen to be associated with it.
The old men will cling to it, as without it string theory dies, and with it their hopes of immortal fame.

31. **MathPhys**
May 2, 2013

One of the reasons young people do science is the scientific ideal, wherein a scientist is perceived as honest, fair, and precise. Reading Mr Kane, one realizes that a scientist can also be a less-than-forthcoming salesman.

32. **M. Wang**
May 2, 2013

Over at Forbes.com, there is a columnist called Gordon Chang, who has predicted the total collapse of Chinese economy by year-end every year for the past two decades. He remains very popular at Forbes and has a sizable following. So, it would seem that some particle physicists has joined with stock market analysts in the rank of pseudo-sciences.

33. **kanefriend**
May 2, 2013

I've been a friend of Gordy Kane for many years, but this is going too far. It is technically not plagiarism, since the copyright says it's a revised version of the 2000 book. But it is scientifically unethical to erase, Soviet style, false predictions from the past as if they never existed. At the moment, there are no Amazon reviews. I hope someone (not me, since I'm not sure how anonymous they are) will write one pointing this out (with a link to this blog).

34. **Narad**
May 2, 2013

> What justification does Kane offer for putting out a new book? Does he refer to his old book, or say why he did not publish a revised of his earlier book?

Having made the acquaintance of a few acquisitions editors in my days, I wouldn't discount the possibility of the publisher's hoping to wring a few more dollars out of an old property.

35. **Peter Woit**
May 2, 2013

kanefriend,

Checking Amazon, someone seems to have taken up your suggestion.

36. **johnmcAllison**
May 2, 2013

Good work, the public needs to be informed.

I guess it's another example of physicists falling in love with the beauty of their theory, only to be slapped across the face by reality.

37. **Peter Woit**
May 2, 2013

new book, Narad,

I should make it clear that I see nothing wrong with putting out a revised version of a book like this, updated to include what has been learned over the course of the last 13 years. It is a little odd though not to mention in the preface that the book is a revised version and to explain a bit about the nature of the revision.

What is problematic is just the sort of editing going on in the passages I included in the posting, for reasons explained earlier.

One reason for this comment is that there seems to have been some misunderstanding about the point of this posting, see here:

http://blog.physicsworld.com/2013/05/02/supersymmetry-revisited/

38. **Maciej**
May 2, 2013

Quite ironically – individuals like Kane, while popularizing ST and writing a lot about it, are at the same time responsible for making other scientist literally laugh at ST claims.

I remember once at AEI there was a seminar about ST phenomenology given by B. Ovrut (who was all the time defending the point that ST makes predictions). At some point somebody asked "…yeah but Kane claims that he can predict 125GeV exactly!" to which Ovrut replied "It's amazing, isn't it?" – and then everybody laughed.

I think this example makes it clear that even for devoted string-theorists, claims by Kane are simply embarrassing.

39. **Henning Dekant**
May 3, 2013

It'll be funny if it wasn't so pathetic.

I see that some commenters suggested that there should be consequences for such blatantly unethical conduct. If somebody was to fake data for a publication there'd be a process to follow to get some traction.

Apparently when it comes to books for the great unwashed masses all is fair game?

40. **book**
May 3, 2013

Books come under the First Amendment. Publishing fake data is fraud. But writing a book, even a nonfiction one, and making idiotic or unsubstantiated or unjustifiable claims is different. As long as there is no libel or fraud or trade/patent/copyright violation or intellectual property theft, Kane can publish what he likes. If large numbers of people pan the book, well, Kane can cry all the way to the bank.

41. **M**
May 4, 2013

Eric, you are confusing two different arguments.
The naturalness argument (that correctly lead Kane et others to claim years ago that SUSY should be discovered at Tevatron) is only weakly affected by the measurement of the Higgs mass.
Next, this measurement offers a new, different, argument to guess the SUSY scale.

So, in order to avoid Orwellian doublethink, one should honestly say that the previous expectation has not been confirmed by Tevatron, nor by LHC.

42. **Eric**
May 5, 2013

M,

The naturalness argument is actually greatly affected by the measurement of the Higgs mass such that it leads to a new problem known as the little hierarchy problem. In point of fact, the mass of the putative superpartners in the MSSM is directly related to the mass of the Higgs via REWSB. If the Higgs mass had been below 120 GeV as expected for the last decade, one would have expected the superpartners to have shown up by now on the grounds of naturalness and all of Peter's arguments would have been correct. However, to get a Higgs mass as large as 125 GeV requires large radiative corrections and/or large left-right mixing from the top/stop sector. This generically means heavy squarks leading to the problem of introducing some fine-tuning. However, in cases such as focus-point supersymmetry, the squarks and sleptons can all have multi-TeV masses while fine-tuning remains low. So, it is quite possible that supersymmetry solves the hierarchy problem naturally while having squarks and sleptons which are too heavy to be observed at the LHC. Thus, not finding superpartners at the LHC does not make the existence of supersymmetry less likely or imply that supersymmetry has 'failed'.

43. **paddy**
May 5, 2013

Eric,
Do you have any idea how much your rationale sounds like sophistry (if not cavilling) to us practicing non-hep physicists out here?
Paddy

44. **Eric**
May 5, 2013

Paddy,

It is not my fault that persons such as yourself do not possess the technical knowledge to understand the real issues involved in this topic. If you were able to actually follow the physics and do the real calculations on your own, you would realize that what I am saying is completely accurate, while most of what you learn from PW is pure BS.

45. **paddy**
May 5, 2013

Eric,
Quite in character…the ad hominem attack. ..enough said.
(The so obviously ignorant) Paddy
PW: sorry about that distraction..I should have known better.

46. **chris**
May 5, 2013

Eric,

in all humbleness, i can follow and my BS detector is on red when i hear your arguments.

basically you are saying: well, there is this problem A (Higgs mass at the verge of excluding low energy SUSY all by it self), which we could not have forseen a decade ago. because of it, problem B (we see no superpartners) in not a problem at all.

the unnaturally high higgs mass naturally pushes the squarks unnaturally high. naturally this fulfills naturallnes, no?

take a deep breath, one mont off and look at it again. seriously.

47. **Yatima**
May 5, 2013

"If the Higgs mass had been below 120 GeV as expected for the last decade"

It is sad that one cannot post a reaction image. I somehow remember that "most" of the expectations were for m_H ≫ 120 GeV/c², what with the vacuum becoming unstable and all that.

Google-fu reveals:

http://arxiv.org/pdf/0708.3344v8.pdf

"Higgs-mass predictions" by Thomas Schücker

…in which we learn that there are 96 predictions, of which only 20 are below 126 GeV/c². One would of course need weight this with the "number of adherents"….

Anyway, let's have a quotation:

> Our list contains 96 Higgs-mass predictions. Supersymmetry is behind 26 of them with central values between 120 and 255 GeV. Compactified additional dimensions motivate ten predictions ranging from 117 to 450 GeV. There are three superstring inspired predictions: 117, 121 and 154.4 GeV. The embedding of the electro-weak Lie algebra su(2) ⊕ u(1) in the superalgebra su(2|1) produces four predictions: 130, 161, 250 and 426 GeV. Five predictions, between 124 and 317 GeV use the Coleman-Weinberg potential. One prediction, m_H = 125 GeV uses dynamical symmetry breaking with the Higgs being a deeply bound state of two top quarks. At the same time this model predicted two years prior to the discovery to the top its mass to be m_t = 175. Another prediction for the Higgs mass motivated by dynamical symmetry breaking via a neutrino condensate is at 178 GeV. We have listed four predictions from Connes's noncommutative geometry: 170, 203, 241 and 271 GeV. Lattice gauge theories lead to two predictions: 515 and 760 GeV. Eight predictions are based on the (approximate) vanishing of particular terms related to quantum corrections: 154, 155, 200, 210, 309, 374 and 536 GeV. We have two lower bounds for the Higgs mass and 37 upper bounds, 26 of which come from supersymmetry. Five predictions, one upper and one lower bound come from the recent idea that inflation is driven by the Higgs scalar together with a strong non-minimal coupling to gravity. The Higgs mass is obtained from fitting the observed spectral index and tensor-to-scalar ratio of the Cosmic Microwave Background.

48. **Yrast**
May 18, 2013

This is a very disingenuous review, and amounts to nothing but a personal attack on a first rate scientist…. The cover of the book clearly indicates that it is a REVISED EDITION. I don't see anything wrong with Kane's approach, which is common in popular scientific literature. The subject of the book is high-energy physics, not celestial mechanics. There are more wild ideas in his field than all the pages in the book…. Do you really expect authors of these bold theories to publicly declare their errors on every minute matter that is discounted by experimental results? To the best of my knowledge, they even don't do it in their professional milieu. They move on…

49. **Peter Woit**
May 18, 2013

Yrast,

This posting contains mainly just accurate information about changes Kane made to the 2000 version of the book. People can judge for themselves the significance of those changes, for instance whether the predictions about superpartners are a "minute matter", and whether Kane's approach is "common in popular scientific literature" (as well as whether, like you, they "don't see anything wrong with it").

50. **Yrast**
May 19, 2013

Peter,
The book has a 2 star rating on Amazon, mainly because of your posting (2 reviewers give it the lowest possible score with references and links to your blog). Certainly the gist of the posting as seen by those reviewers and me is not “just accurate information about changes…”. “Orwellian exercise!” really?

I have enjoyed reading your book (which has references to Kane’s books) and am an avid follower of your usually very informative blog.
Thank you for the great job.

Miscellaneous Links

May 6, 2013
Categories: Uncategorized

- There's an interesting discussion amongst philosophers at Brian Leiter's blog about the effects of Templeton money (and I contributed my two cents…). In other Templeton news, they're funding a new "literary science magazine" called Nautilus. Also via Leiter, they have awarded $3 million to two philosophers at Saint Louis University ("one of the largest grants SL has ever received in the areas of the humanities or the sciences") for them to study the subject of intellectual humility.
- In the category of rumors I've heard from so many reputable sources they must be true and I can't really be violating confidentiality, W. Hugh Woodin is moving from Berkeley to Harvard, and Simon Donaldson from Imperial College to the Simons Center at Stony Brook.
- Via Simon Willerton at the n-Category Cafe, Edinburgh now has a gallery with a wonderful collection of portraits of seventy mathematicians, including commentary from Michael and Lily Atiyah, an online version is here.
- The publisher sent me a copy of Tony Zee's new GR textbook, Einstein Gravity in a Nutshell, which I very much enjoyed looking through. Zee takes the textbook concept to new levels of informality, so it includes a wealth of interesting and amusing comments, spread throughout the text, footnotes and endnotes, including quite a few about quantum gravity. At over 800 pages, it's a pretty huge book, including a lot of conceptual material (as in his QFT textbook), but more calculational detail than the QFT book. Undergraduate physics students should find this quite an approachable text (unlike the QFT one, which I think you need graduate level training to really follow).

  This definitely is a text for physicists, not mathematicians, with the geometry taking a back-seat. Differential forms and orthonormal frames don't appear until nearly the end of the book. Personally I've found using the same language of connections on principal bundles to do gauge theory and gravity to make the most sense, but this involves getting familiar with quite a bit more formalism than most physicists are willing to deal with.

- I'd been curious to hear more about recent work of Jacob Lurie and Dennis Gaitsgory on Tamagawa numbers, and had been waiting to see a paper from them. Turns out there's something much better: Lurie has been teaching a course about this at Harvard this semester, with notes appearing here. For some indication of why you might take an interest in this if your interest is gauge theory, see here.
- While about the only bipartisan agreement in Washington these days is that something must be done to deal with the terrible problem of the shortage of STEM graduates in the US, someone has noticed that there actually is no shortage, see here.
- This past weekend there was a conference in honor of Bruno Zumino's 90th birthday at Berkeley, and one can hope that some version of the talks might become available online here.
- A hot topic in HEP remains that of when the failure of the SUSY picture that has been heavily over-sold for several decades will finally be acknowledged. From the list of titles at the Zumino conference, Maiani's was "Supersymmetry: not time to give it up, yet". Cormac O'Raifertaigh reports here that Nati Seiberg is saying ~~"only certain aspects of minimal models had been ruled out so far. As for the future, who knows?".~~ (now corrected, see Cormac's blog). Physics World has a story here, with Ben Allanach claiming that "data taken at the LHC have excluded roughly half of supersymmetry's parameter space" and that now one has to wait until 2015 when, if SUSY is right, it will be found nearly immediately:

  > "My hopes are pinned on the next run," he says. "The energy jump now is going to make the big difference. And if supersymmetry is the correct theory of nature, I would be expecting to see a big signal within the first month. If it doesn't crop up, I'll then be getting pretty depressed."

  Bill Murray of ATLAS makes the excellent point that

  > "Proving wrong would be as important as proving it right," he says. "Null results are hard to sell to newspapers, but they are really important to scientific progress."

  Killing SUSY will be one of the great achievements of the LHC, and complaining about this might be kind of like being upset that Michelson-Morley didn't find the ether.

Update: I've been pointed to an impressive photo of Robbert Dijkgraaf that unfortunately did not make the Atiyah Gallery.

Update: The New York Times has a story today about the new Templeton-funded science magazine Nautilus.

Update: The news from Britain is that Stephen Hawking has joined the academic boycott of Israel, cancelling plans to attend a conference there this month. Please discuss your views on the Israeli/Palestinian conflict elsewhere. There's no way I'm going to moderate such a discussion, and there are now surely dozens of other sites carrying this story where comments are encouraged.

Comments

1. **cormac**
   May 6, 2013

   My bad, my Seiberg quote wasn't very accurate, I have corrected it on the blog. What Nathan actually said was
   "…even if supersymmetry is not realized in the energy range explored by the LHC, it is still and will always be important. The impact of supersymmetry on theoretical physics and on mathematics has already been huge and it will continue to be essential..there are many parallels with other theoretical ideas that did not solve the problems they were designed to solve but turned out to be crucial in other contexts"

   Re PW comment above that "Killing SUSY will be one of the great achievements of the LHC" , I'm not sure what you mean; I don't see how the LHC can do anything except rule out certain models. And even if some successor to the LHC does, this doesn't preclude the idea that the general principle of a symmetry between fermions and bosons might be right at some level…which I think is how the cosmologists see UFT

2. **Alex**
   May 6, 2013

   Half of Susy parameter space? Killing Susy? That doesnt make sense, what on earth is Ben on about?

3. **Peter Woit**
   May 6, 2013

   Hi Cormac,
   I changed the posting to reflect the inaccuracy.

   I'm curious to follow how SUSY proponents are dealing with the current situation. Do they admit that the idea of electroweak-scale SUSY breaking, solving the so-called "hierarchy problem" is now pretty much dead (since if you believed that picture, you should have seen superpartners at LEP and the Tevatron, and the extra factor of four in energy at the LHC so far should have finished the idea off). Or do they hang in there, insisting that the next energy jump, by a factor of 13/8, will finally vindicate them?

I think the more sensible ones see the writing on the wall.

I don't disagree really with the revised quote from Seiberg. If the field replaces the idea that a MSSM broken SUSY extension of the SM is the way to pursue unification with the idea that SUSY as a general concept may have some connection to reality, in a way we don't yet know, that will be progress. And we'll have the null result from the LHC to thank, so it's quite important. On the other hand, if all the null result from the LHC achieves is inducing prominent theorists to keep moving up their SUSY mass estimates a la Gordon Kane, that will be just a sad chapter of scientific history.

4. **Peter Woit**
May 6, 2013

Alex,

I also wonder what measure Allanach is using on SUSY parameter space.

5. **Bill**
May 6, 2013

I will be very surprised if Jacob Lurie does not win Fields medal next year.

6. **MathPhys**
May 6, 2013

Now that I have accepted the fact that string theorists will keep on pushing the masses of the superpartners higher and higher, year after year, I also to accept the fact that anti-string theorists will express disapproval and disappointment every time that happens. Nothing will change.

The susy debate has officially moved from the domain of science (*"Let's see what the experiments say"*) to the domain of ideology (*"There is no way, no way in hell, that anyone can rule out supersymmetry"*).

Since there is no point, and no reason, to argue with another man's religious beliefs, let's talk about something, please.

7. **Kyler**
May 6, 2013

Actually Jacob Lurie's class is being taught at Stanford, where he is currently visiting.

8. **GT**
May 7, 2013

I believe that Donaldson is actually splitting his time 8 months/4 months between Simons/Imperial, rather than moving altogether.

9. **cormac**
May 7, 2013

Hi Peter, many thanks for the revised text.
Re e-w SUSY breaking, I don't know, only the SUSY theorists themselves can answer that, as you know! yes, Natt'squote is great, I immediately thought of Yang-Mills

10. **Marcel van Velzen**
May 7, 2013

"Killing SUSY will be one of the great achievements of the LHC, and complaining about this might be kind of like being upset that Michelson-Morley didn't find the ether"

Well, things are not always that simple: Due to the constant speed of light, all fundamental particles in the electroweak theory have to get their mass from the omnipresent Higgs field. (As far as we know now a particle is not composite (and so by definition fundamental) if and only if 1 it is massless or 2 gets its (rest) mass only from the Higgs field). So Michelson-Morley did find some kind of ether when they found that the speed of light was constant. In that respect I agree with Cormac above "the general principle of a symmetry between fermions and bosons might be right at some level".

11. **Krzysztof**
May 7, 2013

Peter,

Funding interdisciplinary activities across science vs philosophy borders is not trivial, whereas 'the question of God' is obviously one of the central ones in philosophy (be it in a positive or negative way…), so why you have problems with these funds by Templeton? Are you maybe part of the S. Weinberg et al. efforts?

12. **Peter Woit**
May 7, 2013

Krzysztof,

I've written extensively about Templeton here on the blog (search on "Templeton" on the main page if interested). It's a complicated subject, but the simple part is that they're spending hundreds of millions of dollars with the goal of bringing science and religion together, and I strongly believe they are best kept apart, especially when it comes to fundamental physics. I also strongly believe that discussions of religion on internet blogs are basically always stupid and a waste of time, so, please, not here….

13. **Krzysztof**
May 7, 2013

Peter,

Of course I will obey… but let me just stress that above I was not talking about religion, but about philosophy. Unless you think that there is only philosophy of science there, my argument is still valid I would think, and I hence do not get your point – you think there is misuse of funds? But then we might consider closing all humanities… Please, keep in mind that I am just asking an academic question (in the direct sense:) and not trying to bring closer religion to fundamental sciences.

14. **Peter Woit**

May 7, 2013

Krzysztof,

My concern here is about the Templeton Foundation's very well financed goal of injecting religion into this (and I think this is the same concern being discussed on Leiter's blog). It's not a "misuse of funds", they are very clear about what they are trying to do, and they have every legal right to do it. I just think that people need to be aware that, for instance, when they all of a sudden see "Philosophy of Cosmology" conferences, talks, blogs, books, etc., there is a reason for this. A very wealthy group devoted to bringing religion and science together has decided this is an effective way to spend their money. Some of what comes out of this will be perfectly respectable science or philosophy of science, some of it will be pseudo-science (the multiverse), and some of it will be dubious injections of religion into science, but, in any case, people should be made aware of what is going on here.

15. **Krzysztof**
May 7, 2013

Peter,

I promise this is my last post and I shut up, but you mix two things I think:

1. I agree – multiverse is not physics, and its (very many) promoters should not claim so, but that is not the same as doing some properly labeled, inter-disciplinary stuff.

2. Fundamental science naturally, 'by definition', is raising philosophical questions. Many of us are very curious what is 'meaning' of physics – is for example the probabilistic QM end of story etc.

Finally, I think you are a bit oversensitive to religion – it plays so little role in the 'western' academia anyway.
(And it is not all bad maybe at the end – two greatest revolutions in cosmology were led by priests – Copernicus and Lemaitre – after all 🙂

16. **Peter Shor**
May 7, 2013

Re: the STEM job non-crisis report, let me point out that you can prove anything with statistics. One quote from the report:

"For computer science graduates employed one year after graduation (i.e., excluding those unemployed or in graduate school), about half of those who took a job outside of IT say they did so because the career prospects were better elsewhere, and roughly a third because they couldn't find a job in IT. "

This means that many of those who took a job outside of IT did so because these were better jobs. I don't know what these jobs are, but I am assuming many of them are reasonably high-level (since they're better than the IT jobs these students were offered). Do we really want to fill them with technologically illiterate people?

That, of course, depends on the job, and the report doesn't give enough detail to answer this question.

17. **aether**
May 7, 2013

Let's main some sense of historical perspective. The Michaelson-Morley experiment never disproved the luminiferous aether. The theory simply became more and more contrived as new data came in. For example Lorentz-Fitzgerald and the length contraction idea was introduced to explain some of the observed null results for the aether wind. But as more measurements were made, the theoretical constructs became more and more artificial. But the theory was never *disproved* per se. What happened was that Einstein introduced the Special Theory of Relativity, and that not only explained all the observed data, but it was able to fit new results without modification of its basic principles. And so people simply abandoned the aether theory in favor of relativity. So about LHC and SUSY, nothing will ever disprove SUSY. The models will simply get more contrived. What needs to happen is that an alternative theory will come along, which can explain all observed phenomena (explain naturalness?) and also fit all future measurements without change to its fundamental principles. That simply hasn't happened yet, but that's the way theories die out.

18. **Chris W.**
May 7, 2013

On the conference in honor of Bruno Zumino, see [this post] from Steve Hsu, with photos of a few of the participants.

19. **paddy**
May 8, 2013

@Peter Woit concerning Brian Leiter's blog:
My head is aching attempting to follow the arguments pro and con (I think) of the potential of bias on accepting Templeton funds. The only phrase that comes to my bewildered mind is "slippery slope"–a locale where I believe these folks frolic and you are just pissing in the wind…so to say.
paddy

20. **ca$$$$h**
May 8, 2013

It's all well and good to turn up one's nose at Templeton funding, but the fact remains, one needs funding anyway, and where do you propose to get it?

Written in 1946 when Brookhaven National Lab was being planned, and a sequel in 1956

Take away your billion dollars

Arthur Roberts, 1946

Upon the lawns of Washington the physicists assemble,
From all the land are men at hand, their wisdom to exchange.
A great man stands to speak, and with applause the rafters tremble.
'My friends,' says he, 'You all can see that physics now must change. Now in my lab we had our plans, but these we'll now expand,
Research right now is useless, we have come to understand.
We now propose constructing at an ancient Army base,
The best electronuclear machine at any pace.

Oh — It will cost a billion dollars, then billion volts 'twill give,
It will take five thousand scholars seven years to make it live.

All the generals approve it, all the money's now in hand,
And to help advance our program, teaching students now we've banned.
We have chartered transportation, we'll provide a weekly dance,
Our motto's integration, there is nothing left to chance.
This machine is just a model for a bigger one, of course,
That's the future road for physics, as I hope you'll all endorse.

And as the halls with cheers resound and praises fill the air,
One single man remains aloof and silent in his chair.
And when the room is quiet and the crowd has ceased to cheer,
He rises up and thunders forth an answer loud and clear:
'It seems that I'm a failure, just a piddling dilettante,
Within six months a mere ten thousand bucks is all I've spent,
With love and string and sealing wax was physics kept alive,
Let not the wealth of Midas hide the goal for which we strive.

Oh – take away your billion dollars, take away your tainted gold.
You can keep your damn ten billion volts; my soul will not be sold.
Take away your army generals, their kiss is death I'm sure.
Everything I build is mine, every volt I make is pure.
Take away your integration and let us learn and let us teach.
For beware this epidemic, for colitis I beseech.

Oh, dammit – engineering isn't physics – isn't that plain?
Take, oh take your billion dollars. Let's be physicists again.

1956, ten years later (sequel)

Within the halls of NSF the panelists assemble.
From all the land the experts band their wisdom to exchange.
A great man stands to speak and with applause the rafters tremble.
'My friends,' says he, 'we all can see that budgets now must change.
By toil and sweat the Soviets have reached ten billion volts.
Shall we downtrodden physicists submit? No, no — revolt!
It never shall be said that we let others lead the way.
We'll band together all out finest brains and save the day.

Give us back our billion dollars, better add ten billion more.
If your budget looks unbalanced, just remember this is war.
Never mind the Army's shrieking, never mind the Navy's pain,
Never mind the Air Force projects disappearing down the drain.
In coordinates barycentric, every BeV means lots of cash,
There will be no cheap solutions, — neither straight nor synoclash.
If we outbuild the Russians, it will be because we spend.
Give, oh give those billion dollars, let them flow without an end.

21. **Peter Woit**
May 8, 2013

paddy,

I think I agree with you. By now I can't even figure out what it is that Tim Maudlin was trying to argue with me about there, presumably I missed his previous point so my response caused a complete divergence from coherent discussion. In any case, the various relevant arguments seem to have been made multiple times.

ca$$$$h,

If Templeton will fund the next energy frontier accelerator, I'm all in favor of having theologians speak at the Lepton-Photon and other conferences where results are announced.

For those who just can't get enough Templeton, at the Leiter blog I did learn of a bizarre Templeton-funded contribution to mathematical logic, see here:

http://www.math.osu.edu/~friedman.8/pdf/ConMathThe122512.pdf

For more discussion of the physics/God/Templeton issues, Sean Carroll has this:

http://www.preposterousuniverse.com/blog/2013/05/08/on-templeton

22. **ca$$$$h**
May 8, 2013

Has anyone suggested to Templeton to fund the next energy frontier accelerator? Or maybe a detector? Or at least R&D into, say, a muon collider?

Private organizations (e.g. Keck) do fund telescopes. Telescopes are cheaper, relatively speaking, maybe 1B, are quicker to build, and the results yield better PR for the funding agency. A serious problem with accelerators is the induced radioactivity. One can't just build even a low energy accelerator in one's back yard, even if 100% privately funded. The radioactivity automatically makes the device subject to many federal regulations.

23. **Galileo's Meme**
May 9, 2013

I think the funding for "free will" studies is what needs to be regarded with the greatest wariness. Free will appears to be a major hobby horse for the current right-wing establishment–presumably for for a variety of reasons, not the least of which is certainly the way it is constantly trotted out to defend the USA's draconian sentencing requirements and staggering incarceration rate, and the bourgeoning private prison industry that feeds off of them.

Currently the preferred defense of free will in right-wing circles seems to be an appeal to religion, but it looks to me like they want the respectablity of a "scientific" defense of free will, preferably free will of the philisophically "strong" variety rather than the namby-pamby compatibilist sort that packs little more rhetorical oomph than the physicist's metaphorical invocations of "God". An early attempt that mixed together some warmed-over process theology with handwaving about the delayed-choice experiment seems to have landed with a dull thud, but I doubt it will be the last we hear on the subject.

(Perhaps not coincidentaly I've heard that some Chinese univeristiess are starting to push process theology as some sort of "indigenous" approach to science–and coming from an archetypically Chinese name like Alfred North Whitehead why not–maybe someone ought to look into whether some stateside group is beavering away over there as well.)

24. **Carl**
May 10, 2013

Galileo, you forget something.. No free will also means that the judge and the jury has no free will. Without free will how can you criticize a judge for handing out tough penalties for minor crimes?

25. **paddy**
May 10, 2013

Are not discussions which employ words and phrases such as "god" and "free will" a sure sign that we have gone "one toke over the line, sweet jesus"?

26. **anon**
May 10, 2013

Apparently there are different opinions on the "STEM shortage." Below is a link to a discussion of a study that find results that disagree with the study you linked to.

http://techcrunch.com/2013/05/10/3-graphs-explain-why-there-is-a-tech-talent-shortage-and-immigrants-are-needed/

Hard to tell which study is closer to the truth. The same discussion goes back and forth in Germany, where companies keep complaining about a shortage of engineers etc, yet are unwilling to increase their salaries or invest in their education.

27. **Peter Woit**
May 11, 2013

anon,

I don't actually see anything in those numbers that disagrees with the other study. The numbers in your link show it takes longer to fill H1-B STEM jobs and they are better paid than average. Presumably this has always been true, and says nothing about the argument that there are plenty of US STEM graduates out there, but they are choosing to take other kinds of jobs, would take the H1-B STEM jobs if they paid better.

The article linked to is also kind of dishonest in its use of numbers. It says STEM wages are "growing", with an 8% growth since 2000, neglecting to mention that all the growth was in 2000-2004, with wages basically flat over the last ten years (down recently in one category).

All in all, I see no numbers addressing the argument that there are plenty of qualified workers in the US, and that whatever problems employers are having hiring people are simply due to their offering lower wages for STEM work than for other opportunities STEM graduates have.

28. **srp**
May 11, 2013

All employers facing normal, upward-sloping supply curves for labor (meaning they would have to pay more to attract more workers) say they face a "shortage." This is not what an economist would call a shortage, but it is common for managers to see it this way: "If I could only hire more workers as good as the ones I have at the same compensation level I could make more money." But every firm tends to hire workers up to the point where it is no longer expected to be profitable, so every firm's managers feels that they face a "shortage."

Strictly speaking, a shortage would only occur if the wage were artificially suppressed so that at that "going" wage quantity demanded exceeded quantity supplied. That would be like when rent controls keep rates below the market-clearing value so that more people are always willing to be tenants at those rates than there are spaces available to be rented. But nothing like that is going on in the US STEM labor market. We just have employers saying that it isn't profitable to hire at the wages they would need to pay to attract more workers.

29. **Peter Woit**
May 12, 2013

srp,

Some of the companies most loudly complaining about the "shortage" though are extremely profitable (e.g. Apple and Google). It's hard to believe that paying higher wages to attract certain employees would really ruin their business and make it unprofitable.

30. **PeterPetersonSenior**
May 12, 2013

This is a comment on STEM workers short paragraph. I was hiring a lot of STEM professionals, and met all range form people that can do their job to bright stars to people that destroyed all projects they were part of. On another positive note, I know history of professionals that graduated in STEM and some time later found a different line of work – sometimes using their education directly and sometimes not.

I would say, if you think about any specialty of physics you will have to concede it applies there as well, and pretty much in any other profession with which you have close familiarity.

After many years of personal involvement, my impressions are:
1) There is tremendous shortage of very high level engineers. You taught a lot of students yourself, and you know that there is a whole distribution of talents – from genius to great to average to below average to some that graduated with lowest possible grade. The final GPA ofter correlates with engineer's performance for many years after graduation, sometimes forever. The top talent is rare, also education is to a large extent a memory exercise on BE, BS or MS level, creativity is somewhat different talent.
2) People receiving a (specifically) engineering degree are not necessarily working by their major specialty (either immediately, or after a few years). There are a lot of people in sales, support, etc. that do well. There are people that decide they just want to do something different, and no one can force them.
So the point is to bring talent into the country. There is a protectionist uproar pretty actively going on, and it is understandable. However bringing 20 STEM professionals to find 1 that makes all the difference for their company is very important (that is assuming that company acts as meritocracy, and many most dynamic are). And having

this talent makes it possible for their a bit less talented coworkers to have jobs where they can do just as important footwork after the new concept is invented.

31. **Peter Woit**
May 12, 2013

PeterPetersonSenior,

I've never heard the argument before that the US needs to bring in large numbers of mediocre engineers in order to also get a small number of highly talented people. In the academic fields I know of, the most highly talented people from outside the US are generally recognized as such, there's active competition among US universities to hire them, and as far as I know there's never a significant problem getting them visas to work in the US.

If the H1-B program only applied to exceptional job candidates that the hiring company was willing to pay an exceptionally high wage for, it wouldn't be an issue.

32. **PeterPetersonSenior**
May 13, 2013

I think we are in agreement on H1-B – at least regarding the goal to get exceptionally talented people. However there no way to definitely verify one's abilities, not until after they start working. This is why my 1 in 20 guesstimate.

As for paying exceptionally high wage for exceptionally good performance – this was always my approach. And this is where I was hitting the shortage. I understand that other companies may have different attitudes, but I don't think the primary motivation is to get cheaper mediocre engineers for all of them.

A short remark about Apple – Apple is very profitable, but you may be aware of the debate going on in investing community whether after Steve Jobs demise Apple has enough talent to move company forward (and stock price dropped exactly because of profitability percentage drop). I am sure Jobs was not the only reason for Apple's rise, but often there is a need for just a handful of people that make a lot of difference even for company of Apple size.

33. **PeterPetersonSenior**
May 13, 2013

Sorry for sending second response in a row. Just one observation – when you note: "In the academic fields I know of, the most highly talented people from outside the US are generally recognized as such, there's active competition among US universities to hire them, and as far as I know there's never a significant problem getting them visas to work in the US."
Let me turn the question other way around and ask: what is instead accepting talented people from anywhere in the world, we decided to fill the positions by US born and educated people. Surely, we are graduating more than enough PhD s to fill all academic positions? With all H1-B reasoning attached? I am sure, this would not be ideal.

34. **tt**
May 13, 2013

umm, actually that sounds good to me.

35. **Peter Woit**
May 13, 2013

PeterPetersonSenior,

Sure, if US academia couldn't hire exceptional non-US citizens, you could still fill all academic positions, but that would seriously weaken many departments. But, still, I don't think the issue of exceptional cases is relevant to the debate I'm seeing over H1-B. There people are talking about dramatically expanding a program that is already bringing in over 100,000 people, only a small fraction of whom are exceptional cases.

36. **G P Burdell**
May 14, 2013

When you think of hiring STEM graduates, don't think in terms of hiring established researchers; think of accepting graduate students as teaching assistants. Because that's the equivalent level of knowledge and understanding. Even the most talented ones will need years of experience and further training to become fully contributing professors and researchers. Yet most companies today look for a newhire with a particular set of skills (that they find using key-word searches of resumes), and expect skill levels capable of almost immediate contribution on major projects with essentially no further training.

Add to that that in most companies, any further training (formal or informal) is now almost always confined to an employee's private time. (I've been told by corporate-level software development managers that they expect an employee to be "enthusiastic" enough to learn new technologies on their own, not at company expense — even while those people are working 12- and 14-hour days with no overtime pay.) And since most engineers change jobs several times, actual mentoring is almost unheard of.

Then, the starting salaries for these positions, whether for graduates or for those with experience, haven't advanced beyond inflation in the last decade — in fact, some went down during the recession.

Hence the compaints about the "shortage" of "qualified" STEM talent in the U.S.

For these companies, most engineers are cogs in their machine — interchangeable parts. If they can buy a cheaper cog, then they buy it; hence the H1-B Visa push, using the self-created "skills shortage" as an excuse.

Number Theory News

May 12, 2013
Categories: Uncategorized

A special seminar has been scheduled for tomorrow (Monday) at 3pm at Harvard, where Yitang Zhang will present new results on "Bounded gaps between primes". Evidently he has a proof that there exist infinitely many different pairs of primes p,q with p-q less than ~~17,000,000~~ 70,000,000.

Whether this proof is valid should become clear soon, but there still seems to be nothing happening in terms of others understanding Mochizuki's claimed proof of the abc conjecture. For an excellent article describing the situation, see here.

Update: The "bounded gaps" talk is now on the Harvard seminar listing with abstract

> The speaker proves that there are infinite number of pairs of primes whose difference is bounded by 70 million.

For more on the significance of this, see this Google+ posting by David Roberts.

I haven't seen a paper, but rumor is that one exists and two referees at a major journal have found it to be correct.

Update: The most recent version of Mochizuki's lecture notes for a general talk about his work is here. As mentioned in the Caroline Chen article, Go Yamashita has been talking to Mochizuki. Yamashita has now posted a short document FAQ on "Inter-Universality" and promises "For the details of the theory, please wait for the survey I will write in the near future." He also notes:

> I refuse all of the interviews from the mass media until the situation around the papers will be stabilised.

Update: In a weird coincidence, another major analytic number theory result is out today, a proof by Harald Helfgott of the ternary Goldbach conjecture. This says that every odd integer greater than 5 is the sum of three primes. The result had been known for all integers above e^{3100}, and Helfgott's proof reduces that bound to 10^{30} which is small enough so that all smaller values can be checked by computer.

Update: *Nature* has a story up about the Zhang result, including details of one of the *Annals* referee reports (I gather the paper will be published there).

Update: For some background to the methods being used by Zhang, see here. For Terry Tao on Zhang, see here, on Helfgott, here.

Update: New Scientist has a story about the Zhang result here, with quotes from Iwaniec, who has reviewed the paper, finding no error.

Update: A report from the talk at Harvard is here.

Update: There's more about the Zhang proof at Emmanuel Kowalski's blog, including a link to the Zhang paper.

Update: Nice piece about this in Slate from Jordan Ellenberg.

Comments

1. **Jeff M**
   May 12, 2013

   So I'm tempted to believe this simply because it's 17 million, yellow pigs and all that…

2. **JG**
   May 12, 2013

   hmm, 17 million, they're not exactly twin primes, Hardy and Littlewood would be only mildly impressed (but impressed nonetheless)

3. **Ninguem**
   May 12, 2013

   There is nothing on the Harvard Math. Dept. website about this seminar at the moment.

   http://www.math.harvard.edu/seminars/index.html

   The only mathematician by that name that I found is a lecturer at UNH that goes by Tom Zhang. Is that him?

   http://www.math.unh.edu/faculty

   Peter, could you give the source of your information?

   If this result holds, it's a big deal, JG's comment notwithstanding. Chen Jin Run's old result was a big deal and this is a lot more. Twin primes is a hard, hard problem.

4. **Peter Woit**
   May 12, 2013

   Yes, this is the Yitang Zhang at UNH. The source of the information is an e-mail circulated by Yau to announce the seminar. There's been a correction to the 17 million, now it's 70 million.

5. **Shecky R**
   May 12, 2013

   need to correct the correction (UN-strikeout 70,000,000)

6. **S**

May 12, 2013

Awesome news link on the ABC, Peter, thanks. I was a little perplexed by this quote, though:

"For centuries, mathematicians have strived towards a single goal: to understand how the universe works, and describe it."

I thought that was physicists?

7. **Urs Schreiber**
May 12, 2013

I have been disagreeing a bit here https://plus.google.com/108081058828040288656/posts/aTMDLugKbHR with that article by Chen, concerning the claim that what Mochizuki writes is plain "gibberish".

8. **Steve L**
May 12, 2013

@S, I had the exact same reaction to that line. "Science journalism," sigh. Still, pretty good summary of the situation … especially with Mochizuki maintaining radio silence to the consternation of the entire math community.

I'm curious, did anyone send the author money? Ten years from now there will either be no content providers; or we will have figured out how to compensate the class of professional freelance content providers.

9. **Art Brown**
May 12, 2013

@Steve L: I tried (for $1) but my (good) card was not accepted by Paypal. So much for the future…

10. **Peter Woit**
May 12, 2013

Schecky R,
Thanks, fixed.

11. **Mike**
May 13, 2013

Mochizuki's claimed proof is not even wrong!

12. **Clayton**
May 13, 2013

I sent the author $4. I thought the article was exceptionally well-written and clear, overgeneralizations about mathematics aside. In fact, I was planning to ask Peter for an update on the status of the ABC conjecture sometime soon. It seems like the status is not very promising.

13. **arx**
May 13, 2013

Nothing on the arXiv?

14. **Peter Woit**
May 13, 2013

arx,

No paper publicly available on the arXiv or elsewhere as far as I know. But rumor is that there is a paper that has now been checked by referees at a top journal, so there's a lot of optimism that Zhang has a correct proof.

15. **Thomas**
May 13, 2013

from the abstract of the talk : "The speaker proves that there are infinite number of pairs of primes whose difference is bounded by 70 million"
And so what?
What is the significance of this result?

16. **Jeff McGowan**
May 13, 2013

@Thomas,

Not that I'm a number theorist, but I believe the best know result in this direction is that there are an infinite number of pairs of primes with p_{n+1}-p_n less than c log(n) (due to Erdos I think) and recent results show that you can let c -> 0. This of course is a significant improvement on that since there is no dependence on n. Someone please correct me if I'm wrong, i was too lazy to check on Wikipedia, and I haven't really followed number theory since I took a class with Peter Sarnak in grad school…

17. **Peter Woit**
May 13, 2013

Thomas,

I'll add something to the posting about the significance, but, basically this is progress towards the "twin prime conjecture". Naively one might think that primes just become uniformly sparser and sparser as you go to larger numbers, but this says that you're always going to find pairs relatively close (e.g. for 70 million "close") no matter how far out you go.

18. **Thomas**
May 13, 2013

Jeff & Peter, thank you for taking the time, this helps.

19. **Michael**
May 13, 2013

"…rumor is that there is a paper that has now been checked by referees at a top journal…"

Come on, this blog is all about objecting to hype. You shouldn't be doing this.

20. **Peter Woit**
May 13, 2013

Michael,

I've seen no evidence so far that this is hype. Spreading rumors is part of the mission statement of this blog, when they're rumors I have good reason to believe…

21. **Richard**
May 13, 2013

> You shouldn't be doing this.

Meanwhile, over at Matt Strassler's scrupulously peer-reviewed and rumour-free blog, not a breath about prime gaps! Now *that's* what I call professionalism.

If you do feel you need to learn more than scurrilous hype surrounding this 70 megascalar "result-like" scale-independent gap exclusion, you ought to respect the scientific process and wait until it is had been properly vetted and the publisher has printed the journal and mailed it to your institution's library.

22. **Jon**
May 13, 2013

If two journal referees back it up and a Harvard seminar is planned, then that's a rumor worth spreading!

And I'm quite curious how this will pan out: that would be a major result, and Zhang seems to be a 50+ lecturer with few previous papers, so that's potentially a nice story too.

23. **prep**
May 13, 2013

If two journal referees back it up and a Harvard seminar is planned, then it makes no sense not to already circulate a preprint. Someone should ask him to post something on the arXiv and/or personal website.

24. **Ionica**
May 13, 2013

I wish there was a like-button for some of the very funny comments above. I think it is nice to post about these rumors, doing so adds a little excitement to our field. It is not as if we all stop working and hold our breath until we see the proof.

25. **Richard**
May 13, 2013

I would like to know what you think about this "proof as a social construct" idea:

http://mathbabe.org/2012/11/14/the-abc-conjecture-has-not-been-proved/

http://mathbabe.org/2012/08/06/what-is-a-proof/

26. **Peter Woit**
May 13, 2013

Richard,

I pretty much agree with mathbabe on this, as most things (by the way, she's a good friend). For a related take on the role of proof, see Bill Thurston's fascinating essay "On proof and progress in mathematics", available here

http://arxiv.org/abs/math.HO/9404236

Something that struck me when I moved from the physics to the math community was the extent to which mathematics really is a living, oral tradition. Several times when I asked mathematicians where I could go read about something, their answer was "there's nothing easily readable available, but call up or go see X and he/she will explain it to you." (this was before the internet or e-mail or mathoverflow…).

One thing that Cathy doesn't emphasize is that it's not necessary for someone trying to convince the math community that they have a proof to travel around, talk to people, give lectures etc. It's fine if they don't like doing this, all they have to do is to follow the standard tradition of writing up their work in a form others can understand and sending the manuscript to a high-quality journal for refereeing. If it's an important result, the best experts in the field will generally be willing to work very hard as referees to check it. As far as I know Mochizuki hasn't done this, and I don't know what his reasons are. His papers are so difficult to follow that the refereeing process would likely be a hard one to carry through, but this is not that unusual. The initial reaction from expert referees might be "this is not comprehensible for reasons X, Y, and Z, the author needs to rewrite the thing before it can be checked."

So, there's lots of interesting things to discuss and argue about concerning how proof really works in the math community, but in this case, there's one very simple thing to say: if Mochizuki wants people to acknowledge that he has a proof, he needs to submit one or more papers with complete details to a good journal for refereeing. I hope what is going on is that now that he thinks he has a finished proof, he is writing up all the details in as clear a form as possible, and planning on sending the result to a journal.

27. **S**
May 13, 2013

I would have to say, I think that (with all due respect to a very interesting blog) mathbabe is pretty out there when she suggests that, if the proof is right, but this is not discovered for many years, and it is then realized to be right through the work of some fictitious additional "M", then "M" should receive equal credit with Mochizuki. I can't really see this happening, given the primacy that is rightly granted to coming up with the big ideasl; nor should it.

We have a recent example of something not entirely dissimilar. Perelman's proof presumably would have remained incomprehensible for a long time if not for the serious and dedicated work by a few small teams in translating and expanding on it. Several of those involved were rewarded with improved stature and even better jobs, which is only fair; but I don't think anybody makes the mistake of thinking that they, and not Perelman, proved the Poincare conjecture.

28. **S**
May 13, 2013

(I suppose there is a sliding scale, though. Various people who "rediscovered" something that CF Gauss had not bothered to publish do typically get dual credit for discovery; I suppose one can imagine a spectrum, where if a paper were released that was so genuinely incomprehensible that even much work were not rewarded in understanding it, then somebody who, perhaps inspired by the general approach, managed to reinvent the techniques and illucidate the paper would probably also get and deserve dual credit.

Sorry for the double post)

29. **Peter Woit**
May 13, 2013

S,

I think she's exaggerating a bit to make a point. For instance, no one believes that referees should share significant credit for a result they check, no matter how hard they have to work (unless they have to fix serious errors…). You can make up all sorts of hypothetical situations to argue about how to balance credit for an author's work and what needs to be done so that others understand it, but sticking to actual examples, the Perelman story was very different. He did not provide details of his proof, but did provide sufficient explanation of the new ideas needed for a proof so that experts could fill in the details (with a sizable amount of work..). He travelled around to meet with experts and give lectures explaining his ideas (I was at one here at Columbia, as it happens, sitting next to Richard Hamilton), and he patiently answered questions from experts who wrote to him to ask him for details of specific arguments. From what I remember, it fairly quickly was clear to experts what his new ideas were and what his argument was, even if checking in detail that the argument worked was a fairly long process.

So, there are lots of ways of doing this, of communicating the ideas that make up a proof to the rest of the community. At first I thought that the Mochizuki story would be somewhat like the Perelman one: experts would hold seminars, go through his papers, and puzzle out how his argument worked, even if they needed to reconstruct a lot for themselves since his writeup was hard to follow. So far, we have a failure to communicate here….

30. **Tiger Woods**
May 13, 2013

I made 70 millions in my prime…per year!

31. **tigger**
May 13, 2013

Tiger, the goal here is to make less, not more!

32. **KCd**
May 14, 2013

There is definitely a paper by Zhang. I was at the Harvard talk and a paper was available in the room. It has not been made publicly available, but some well-known experts in this area have read the paper and think the proof is correct.

33. **Thads**
May 14, 2013

How convenient for Helfgott that he is 35, the International Congress is one year away, and there are no obvious front-runners!

34. **Thads**
May 14, 2013

In fact, one could regard both breakthroughs as the revenge of the middle-aged and mathematically unproductive. Zhang's first paper dates from 1985, yet he has only 2 papers on Mathscinet and holds the rank of lecturer at UNH. And Helfgott's work was done "in close coordination with" that of David Platt, who received a computer science BA in 1983, but only got his mathematics doctorate 2 years ago and is pictured on his home page with his grandson. Who says mathematics is a young man's game?

35. **Jeff M**
May 14, 2013

Just a note, the New Scientist piece doesn't say anything about Iwaniec refereeing the paper, only that he read it. Goldston has also read it, it must have made the rounds some at least. It would be unusual for someone to publicly acknowledge refereeing something (though I have seen it, at a 60th birthday conference for Scott Wolpert, where someone admitted refereeing two papers of Wolpert for the Annals because "he was the only person who could understand them" 🙂

36. **Peter Woit**
May 14, 2013

Jeff M,
You're right, that was an unjustified inference from the New Scientist story, I've edited the text.

37. **Mathematician**
May 14, 2013

It's strange to say that H. Helfgott proves the Ternary Goldbach conjecture (the abstract of the paper says "The ternary Goldbach conjecture, or three-primes problem, asserts that every odd integer N greater than 5 is the sum of three primes. The present paper proves this conjecture.") This is an unusual way of putting things because we have already known that the conjecture is true for each sufficiently large number: First, conditionally, due to Hardy and Littlewood, and then unconditionally due to Vinogradov. Not to take away from Helfgott's work, but it would have been better to present things as extending the range of validity of Goldbach by a finite amount, or in terms of complementing Vinogradov's proof, but not appear to claim credit for the proof, which confuses non-experts.

38. **Michael**
May 15, 2013

Mathematician: I think the point is that one can check by computer the range not covered by Helfgott's theorem (which one could not do with the earlier results), so now the ternary Goldbach conjecture is known to be true. Of course it would be more satisfying if the proof didn't involve the number-crunching-by-computer aspect, but the at least the conjecture has now been verified.

39. **Mathematician**
May 15, 2013

Michael, surely it takes a lot of optimization work to lower the constant from gazillion or whatever it was down to a range checkable by a computer, but it is still an arbitrary finite range… Of course someone had to do it, but in light of Vinogradov's theorem it's more accurate to say that the proof of the 3-prime problem has been completed, or that the finite number of possible exceptions in Vinogradov's theorem has been ruled out. I guess I might be missing something, especially since I don't know well the culture surrounding Vinogradov's theorem. On the other hand, I consider hypotheticals like, what if Wiles' proof of FLT had worked only for n > n0, and years later mathematican Y were to prove it via brute-force, case by case, for n = 3,.,n0, would it be reasonable to say that Y proved FLT? Or, say, the Riemann hypothesis was proved for t > gazillion, and then it was a matter of further optimization and brute-force (so inevitable, requiring no breakthroughs or the breaking of a barrier) to verify it in the remaining range…etc. Of course, ruling out a finite number of exceptions can require turning ineffective constants into effective ones, or a wholly new proof strategy, or eliminating exceptions from an infinite range, or…etc, which can be very difficult.

P.S. Regarding using number-crunching in proving mathematical theorem, I have no problem with that provided the number-crunching produces a certificate that can be quickly checked (possibly via computer) by anyone wishing to do so. If not, then I might still get convinced if the computation is carried out by multiple independent sources and their results agree to the expected precision. Of course, number-crunching has a lot to offer in gaining insight or to obtain support for conjectures, but perhaps is not suited for proving theorems since it usually doesn't meet the accepted standards of a mathematical proof.

40. **David Roberts**
May 15, 2013

@Mathematician

the number crunching was done by interval arithmetic with 'safe' margins of error. Helfgott has said that the number at which his pen-and-paper proof takes over in his article is purposefully chosen to have large overlap with what has been done by the computer, and that he's actually checked it further down than what the article says. He also said that he believes the bounds he gets are far from optimal, since that isn't his forte, and people experienced in arriving tight bounds will be able to improve his result, lessening the reliance on the computer.

41. **Mathematician**
May 15, 2013

David, the issue is not solely whether high enough precision was used by the computer, which is just one possible source of error. There are other obvious sources such as code bugs and arcane hardware/software details that were overlooked. For me, a sufficient standard is that multiple independent sources get the same result to within expected precision, which is far more convincing of the correctness (even if it doesn't meet the usual standards of a mathematical proof!). In any case, I still prefer not to use number-crunching in proofs unless the computations produce easily verifiable certificates or the code is readable-enough that it can be verified by an independent human outsider (this is very hard to achieve in practice except in the simplest of cases).

42. **Michael**
May 15, 2013

Mathematician,

"…if Wiles' proof of FLT had worked only for n > n0, and years later mathematican Y were to prove it via brute-force, case by case, for n = 3,.,n0, would it be reasonable to say that Y proved FLT?"

By the standards of the profession, I do think it would be considered that Y proved FLT. Many results today rely on the cumulative efforts of many, and the person(s) who finishes the proof of the theorem is considered the person who officially proved the result, even when it's the end of a long string of theorems. Standing on the shoulders of geniuses and all that.

"… I still prefer not to use number-crunching in proofs unless the computations produce easily verifiable certificates or the code is readable-enough that it can be verified by an independent human outsider (this is very hard to achieve in practice except in the simplest of cases)…"

I would expect that the programming aspect of the proof would be held to the same standards as the mathematical aspect, meaning that the theorem would not be declared proven unless the code was vetted as thoroughly as the proof. It may be complicated, but so is the mathematical argument, so I think this vetting will occur.

43. **Mathematician**
May 15, 2013

Michael, I respectfully disagree with your assessment of the FLT hypothetical. Consider another hypothetical, suppose the Riemann hypothesis is proved for t > gazillion, and then a person Y (a mathematician or not, but who happens to have access to enough computer power), brute-force their way in the remaining range, is it reasonable to say that Y is the one who proved the RH…? Also, I think it's a legitimate point of view for a mathematician not to accept computer number-crunching as part of a proof no matter how much vetting the code goes through, simply because there are hardware/chip/compiler issues that won't/can't be checked. Or maybe because the requested standard is impractical, such as evey block of the code should come with a mathematical proof, or the code should be completely written in assembly, both of which are actually reasonable requests if the goal is to meet the usual standards of mathematical rigor. In particular, if tomorrow, mathematician Y (or a group of hardworking grad students) puts in enough effort to tighten the bounds Helfgott's work so as to reduce to a range checkable by hand and by other mathematical techniques, then according to this point of view it would be Y rather than Helfgott who proved the weak Goldbach conjecture… In any case, one certainly shouldn't take away from the new result, as we now can say the weak Goldbach conj. applies for each odd n > 10^30 (or n > 5 if you're willing to believe the number-crunching), rather than n > 10^10000, or whatever it was, which is a nice thing.

44. **Yatima**
May 15, 2013

"Or the code should be completely written in assembly, both of which are actually reasonable requests if the goal is to meet the usual standards of mathematical rigor"

Well that's just completely wrong and betrays a sad misunderstanding of what programs are about. One might as well demand full brainscans and chemical analysis of the mathematicians' brains.

Even in engineering, you will rather have recourse to a well-written compiler of a subset of Ada, lots of contracts and assertions and three devices operating on a voting principle than to go down to manual checking of assembly language.

"Also, I think it's a legitimate point of view for a mathematician not to accept computer number-crunching as part of a proof no matter how much vetting the code goes through, simply because there are hardware/chip/compiler issues that won't/can't be checked."

No it isn't.

45. **Peter Woit**
May 15, 2013

Please, enough about the reliability of computer calculations. This has now been beaten to death, and has little to do with the interesting news from Helfgott.

46. **Mathematician**
May 15, 2013

Yatima, I guess that we can agree to disagree, but I'm wondering, when you say "Well that's just completely wrong and betrays a sad misunderstanding of what programs are about.", what are programs about?

47. **Mathematician**
May 15, 2013

To Peter Woit, OK it's your blog, and I enjoy reading it, so I'll stop with that 🙂

48. **Yatima**
May 19, 2013

At the oh-so-british "The Register" (Motto: 'Biting the Hand that feeds IT') we read:

[I know who 'Satoshi Nakamoto' is, says Ted Nelson]

Sociologist, philosopher, computer industry pioneer and inventor of the term "hypertext" Ted Nelson is claiming that he knows the identity of Bitcoin inventor "Satoshi Nakamoto". In a rambling – and, let's face it, odd – 12-minute post on YouTube, Nelson spins out the suspense, throws in a dialogue with himself as Sherlock Holmes and Doctor Watson, and finally ends with the statement that the mystery developer of the cryptocurrency is Japanese mathematician Shinichi Mochizuki, research professor of mathematics at Kyoto University. … Australian writer Stilgherrian told The Register that while it'd be easy to dismiss the claims, in spite of his eccentricities, Nelson "has the annoying habit of being right."

49. **msmith**
May 19, 2013

Simons Foundation article:

https://simonsfoundation.org/features/science-news/unheralded-mathematician-bridges-the-prime-gap/

50. **Felipe Zaldivar**
May 24, 2013

Just an update: Zhang's paper is now available on the Ann. of Math. site:

String Theory and the Scientific Method

May 14, 2013
Categories: Book Reviews

There's a new philosophy of science book out, Richard Dawid's String Theory and the Scientific Method (available online here if your institution is paying Cambridge University Press appropriately or if you have a credit card). It comes with endorsements from string theorists David Gross and John Schwarz, with Schwarz writing:

> Richard Dawid argues that string theory plays a novel role in the scientific process that has been neglected by philosophers of science. I believe that this book is a valuable contribution to the philosophy of science, which should interest practicing scientists as well as those who are more interested in the methodology of science.

Dawid is a particle theorist turned philosopher, and as you might guess from the endorsement, he approaches string theory from an enthusiast's point of view. The fundamental question addressed is how one can reconcile string theory's failure in terms of conventional methodology of science with its continuing hold on at least part of the physics community. He explains how the book came about as follows:

> This book has been on my mind ever since I left physics and turned to philosophy in the year 2000. A core motivation for making that step at the time was my feeling that something philosophically interesting was going on in fundamental physics but remained largely unappreciated by the world outside the departments of theoretical physics – and underappreciated even within. Twelve years of grappling with the specification of that general idea have considerably changed my perspective on the issue but left the overall idea intact. This book is the attempt to present it in a coherent form.

Reading the book in an odd way reminded me of my recent experience reading Gordon Kane's reissued book on supersymmetry and string theory written in 2000, especially Witten's essentially unchanged introduction. The degree of self-confidence of string theorists at that time was much different than now: AdS/CFT was a new idea, with a solution to QCD on its way, SUSY a sure thing at the LHC if not at the Tevatron or LEP, and at least half of the new jobs in the field going to string theorists. No landscape or multiverse pseudo-science was around to sow dissension in the ranks. No failure of SUSY to show up anywhere. No discouraging numbers like those for the last two years which show, in the US at least, 9% of jobs going to string theorists, with more jobs going to lattice gauge theorists in 2011 than to string theorists. And of course, a uniformly positive press, with no naysayers like Smolin and Woit causing trouble.

In Dawid's description, string theorists are still partying like it's 1999:

> String theory has attained a pivotal role in fundamental physics and has been treated as a well-established and authoritative theory for quite some time by the community of string theorists and by physicists in related fields. As we have described above, large parts of fundamental physics are influenced by string theoretical analysis. The string community is one of the largest communities in all of theoretical physics and for many years has produced the majority of the field's top-cited papers. Moreover, many string theorists express a remarkably strong trust in their theory's viability.

For the actual list of last year's top-cited papers in HEP, see here, and "remarkably strong trust in their theory's viability" seems to me more 2000 than 2013. He does go on to mention skeptics, but to him a majority of the field is behind string theory, with the skeptics only coming from outside particle theory:

> On one side of the divide stand most of those physicists who work on string physics and in fields like inflationary cosmology or high energy particle physics model building, which are strongly influenced by string physics. That group represents a slight majority of physicists in theoretical high energy physics today. Based on an internal assessment of string theory and the history of its development, they are convinced that string theory constitutes a crucial step towards a better and more genuine understanding of the world we observe. On the other side stand many theoretical physicists of other fields, most experimental physicists and most philosophers of physics. They consider string theory a vastly overrated speculation.

Dawid's main thesis is that string theory critics fail to recognize that a new paradigm of scientific methodology is now needed:

> String theory thus should not be taken to announce an end of science but rather to represent a new phase of scientific progress. In this new phase, progress in fundamental physics is no longer carried by a sequence of limited, internally fully developed theories, but rather by the discovery of new aspects of one overall theoretical scheme whose general
> characteristics identify it as a candidate for a final theory, yet whose enormous complexity bars any hope of a full understanding in the foreseeable future.

What is the reason you should accept this final theory that no one can understand? Obviously the lack of any empirical support is a problem, so Dawid turns his attention to a detailed study of the subject of "non-empirical theory assessment": how do you assess scientific progress absent connection to experiment? This is a real and serious problem, which Dawid studies in detail, although from a point of view which just naively accepts all arguments made by string theorists. He considers three main reasons for studying a theory with no empirical support:

- The No Alternatives Argument. This is the best argument for string theory: there aren't a lot of viable unified theories out there. Of course, the way science progresses is that there always are unsuccessful ideas with no good alternatives, until the day someone come up with a better idea.
- The Unexpected Explanatory Coherence Argument. This is the idea that if a theory holds together better after you start studying it and understand it better, that's a good thing. Dawid repeats uncritically claims of some string theorists that this is the case for string theory. I think you could make an equally good case for string theory unification becoming a more and more dubious idea as it became better understood (see, the Landscape).
- The Meta-Inductive Argument. Here the idea is that if a theoretical research program worked before, so will a similar later one. Dawid claims that the string theory research program is just like the research program that led to the Standard Model:

  > Given the entirely theoretical motives for its creation, the lack of satisfactory alternatives and the emergence of unexpected explanatory inter-connections, the standard model can be called a direct precursor of string theory.

  Honestly, this I just find bizarre, and have no idea what he's talking about, with the history of the Standard Model and the history of string theory two radically different subjects.

In one crucial respect, this book is very different though than Kane's. Kane is well aware that the idea of an inherently experimentally untestable theory is something he can't sell to his colleagues and the public, so he devotes his book and its argument for string theory to supposed experimental tests. More savvy string theorists than Kane though are seeing the writing on the wall: no SUSY at 8 TeV means almost surely no SUSY at 13 TeV, and thus no prospects for experimental evidence for SUSY during any of our lifetimes. To prop up the string theory unification program past SUSY null results from the 13 TeV LHC in 2016 is going to require relying on Dawid's "non-empirical theory assessment" and convincing people that string theory and the multiverse represent a new paradigm for how to pursue fundamental science. This book will be welcomed by those pursuing such a goal.

Update: For a different take on the book you can see Lubos Motl's review (as you might expect, he's a big fan). The case of the most prominent string theorist blogger reminds me of one of the funnier things in the Dawid book that I forgot to mention, this footnote:

> It should be emphasized that physicists on both sides of the divide are aware of the slightly precarious character of the "non-physical" arguments deployed in the debate. Lee Smolin has applied the concept of groupthink to the community of string physicists (which, incidentally, seems a quite accurate representation of what

many critics of string physics do think about string physicists) but is careful not to present it as a core argument. String theorists, when entering a discussion with their critics (see e.g. Polchinski in his reasoning against Smolin), try to keep the debate at an entirely physical level.

Comments

1. **Bill**
May 14, 2013

   I just went to a conference attended by a mix of mathematician and physicists. Since I started reading this blog (recently) I was curious to ask a few physicists what they think about string theory. My motivation was pretty simple: I just find it a bit doubtful that one guy arguing against so many of the top physicists in modern times can be right. But to my surprise I consistently got nearly the same answer: "it is clear that predictions of string theory have no chance to ever be observed experimentally". It is truly bizarre to me that such as state of affairs is possible in physics. And, by the way, none of the people I talked to knew who Peter Woit is.

2. **Bob Jones**
May 14, 2013

   I think Dawid is right about string theory being treated as a well-established and authoritative theory.

3. **Cliff**
May 15, 2013

   These posts are so predictable and shallow. You just state your conclusions as facts, and then act shocked when someone "fails" to "reason" to your answers, when they obviously do not agree with your premises at all.

   Nobody is forcing you, or anyone, to "accept" string theory, Peter. It is a project that a lot of brilliant people choose to work on out of their own free will, for reasons that are become pretty clear to someone with the right preparation who bothers to investigate it. There is no sane reason to complain about the top people in theoretical physics doing what they obviously see as the most productive use of their talent. Nobody is suggesting that string theory be presented as fact to be "accepted" – that's just a ridiculous straw man – it should be presented honestly for what it is: an extremely promising and insightful yet unproven framework.

   If a superior idea ever emerges that is demonstrably more promising to address the big mysteries of physics, then the attention and status will shift accordingly. In the meantime trying to maximally tear down and obfuscate all the promising properties and insights that motivate the study of string theory does nothing to help advance either the public understanding or science.

4. **chris**
May 15, 2013

   Cliff,

   it's the intrinsic notion of "top" you have that needs to be dragged out of the closet and publicly debated. In the past it meant those who came forward with predictions that were later verified by experiment. i still like this definition best. You obviously don't.

5. **Roger**
May 15, 2013

   This must be the only field that justifies itself by bragging about how smart its leaders are, and claiming to have the only theory that attempts to have a theory of everything while actually explaining nothing.

6. **phmer**
May 15, 2013

   As I see no reason why the Universe would not be infinitely complex, isn't the idea of "theory of everything" a far-away dream? Is not the investment of resources into such a business an extremely risky gamble? It is my impression that Nature suprised us over and over again during the history of high-energy physics, each time with new structural layers, new forces, unexpected mixings, etc., which could not be predicted before they were actually found and measured, and which revealed that some successful models were in fact low-energy approximations of more advanced (but not necessarily simpler) constructs. Examples: Who could have imagined such a thing as the weak interaction before the observation of beta decay? Who could have devised a theory of nuclear interactions when nuclei were not even discovered? Who could have described QCD before the interior of nuclei were probed? Now we have 15 more orders of magnitude in energy to explore until we reach the Planck scale, surely there can be quite a few unforeseen phenomena with little to do with our current theories to be revealed out there… Science without experimental guidance and evidence, to me that sounds more like faith.

7. **Jeff Moreland**
May 15, 2013

   Peter,
   I'm curious why you think that no SUSY at 8 TeV means almost surely no SUSY at 13 TeV. 5 TEV seems a big gap; is there some reason I'm missing why there won't be any evidence for SUSY in this range?

8. **Peter Woit**
May 15, 2013

   Jeff Moreland,

   By the standard arguments for SUSY, it was supposed to appear at energy scales of 100 GeV. Roughly, the Tevatron probed energy scales up to a couple hundred GeV, so one expected SUSY to be there, but the factor of 4 in going to the 8 TeV LHC should definitely have done the trick. There's just no argument for why SUSY should appear if one goes up by an extra factor of 13/8.

   This is kind of off-topic though, the point of the book is to give a justification for string theory research assuming no relevant experimental data.

9. **Bernhard**
May 15, 2013

   I did not read the book yet, but judging from the same old arguments you've listed, what is surprising to me is that this is still material for a new book.

10. **Joel Rice**
May 15, 2013

It would be a lot easier if they would just stop calling it a theory. At least electroweak includes QED. If they can not similarly include the standard model, then in what useful sense can one refer to it as a theory ? I have no problem with speculation. Just don't call it a theory.

11. **Thomas**
May 15, 2013

this fundamental belief in string theory is nothing more than a fundamental belief in a religion. thank you, and good night science

12. **Peter Woit**
May 15, 2013

Joel Rice,

In string theory, the argument is that you can include the Standard Model. The problem is that if so you can probably include just about anything else, getting a theory of everything, just not in a good way. It's definitely a theory, but has turned out to be an empty one.

13. **francis**
May 15, 2013

Peter,
AFAIK in ST, all they can do is getting something that looks like the SM, but not the right SM (or any extension of it).

14. **Peter Woit**
May 15, 2013

francis,

I think a more accurate description of the situation is that you can't do reliable calculations in ANY realistic string theory model (e.g. one that gets the CC and basic SM structures right). There's no known reason though why, if you could do such calculations, you couldn't get the SM (and just about anything else…)

All: please, if you have something to say about the book, that's great, if you just want to discuss string theory in general, or rant pro or con, please resist temptation.

15. **Bob Jones**
May 15, 2013

"this fundamental belief in string theory is nothing more than a fundamental belief in a religion. thank you, and good night science"

Every time Peter writes a blog post on string theory, we see ridiculous comments claiming that string theory is a religion. It's fine to criticize string theory, but I think you first have to understand why so many people see it as a promising direction for research. There are some very good reasons for studying this theory. For example, string theory gives the right answer when used to calculate black hole entropy, and it has some general features that are expected to hold in any successful quantum theory of gravity. I don't see how you can criticize string theory without at least addressing these points, and I'm sure they must have been discussed in Dawid's book…

16. **Peter Woit**
May 15, 2013

Bob Jones,

I agree that the "string theory is a religion" arguments aren't either accurate or helpful to understand what is going on. To all, enough of this kind of thing.

Dawid's book doesn't pretend to be a serious evaluation of the arguments pro and con about string theory. Basically he just repeats uncritically arguments used by string theorists, with his interest not whether they make sense or whether there's a good counter-argument, but just what sort of general framework for "non-empirical" arguments they fit into.

17. **Bob Jones**
May 15, 2013

Peter,

I haven't read Dawid's book, so I don't know exactly what he says. But the strongest reasons for studying string theory are scientific and mathematical reasons, not sociological ones. I think it's unfortunate that so many laypeople misunderstand this point.

18. **Peter Woit**
May 15, 2013

Bob Jones,

The point is that some of the main reasons that people keep working on and promoting string unification schemes are not scientific, but sociological. To take an extreme example, Kane's recent "update" of his book violates standard scientific norms of how you are supposed to evaluate scientific theories, and what he is doing I think can only be understood in sociological (or psychological) terms.

What I think Dawid completely misses in his book is that a large part of the story of the scientific community's assessment of string theory unification is a completely conventional one: the idea didn't work out, but a lot of the people with their time and reputations invested in it deal with this by refusing to admit it. This doesn't require any new methodology of science to understand, it's a well-known phenomenon.

19. **Koray**
May 15, 2013

I don't have access to the book. I came here for the philosophy of science arguments presented by the book.

Does Dawid present any prior examples for the No Alternatives argument? Did any other branch of science struggle for decades with a very complex theory with no empirical support because there was nothing better? Does he recommend that they do so now since this is the "new" phase of scientific progress? Would he prefer, say, for cancer research, that if some biologists devise a complex model that feels like the right one to those who understand, yet is expected to yield no fruit for decades, the entire field should stick with it?

20. **Joel Rice**
May 15, 2013

I have to disagree with Dawid's idea that it is acceptable to change the idea of theory so as to throw Feynman under the bus. He calculated the Lamb shift and that is what made it a real theory – as opposed to a mathematically sensible thing that looks like a theory but numbers don't work out. OK, granted, Weinberg had a theory in 1967 before any numbers were in, but there was never any doubt about connecting with experiment, for better or worse. It becomes a 'real theory' by passing tests. If Dawid can find himself persuaded otherwise then we are reduced to who thinks which theory is mathematically prettier than the rest, and it becomes a matter of opinion. A bunch of physicists bet money that Parity was the truth. Opinion was worth zilch.

21. **Peter Woit**
May 15, 2013

Koray,

There's nothing much in the book about other sciences, and little about history (no specific parallels are given). To some extent Dawid is arguing that the situation with string theory is an unparalleled one (a "final" theory that is impossibly hard to test), justifying new scientific methodology. He's not claiming that such methodology is necessarily justified in other fields.

22. **Cliff**
May 15, 2013

phmer,

Your view is a common one, but its not a truly accurate portrait of how these developments have gone. Nature has surprised us with new manifestations of the basic principles of relativity and quantum mechanics, but none of the surprising developments you mention has altered these basic principles. It took time to fully appreciate, but electromagnetism and its non-abelian generalizations are the only possible ways to have an interacting spin-1 field consistent with relativity and quantum mechanics. Many people were impressed with this idea of generalizing electromagnetism but were discouraged because the lack of new long-range forces seemed to rule them out. It turned out that new mechanisms were required to implement the ideas – the Higgs mechanism for electroweak and confinement for the strong force – but it was still based on the one unique option that existed to generalize electromagnetism. There is a big difference between finding surprising and novel ways to implement well-established principles of physics versus a development that actually requires abandoning them. A priori, it would be totally conceivable that nature would select a genuinely radical departure, as you seem to think, but it just didn't.

So, sure, there is always some small chance that around the next bend everything we know about physics will just go out the window. But speculation based on these principles that are backed up by 100+ years of evidence will almost always beat speculation that is totally random and baseless. And thats exactly what string theory is, the one fully-consistent way to talk about quantum gravity while incorporating the principles of relativity and quantum mechanics. (If the extrapolation seems dubious, remember that GRB090510 verified the principle of Lorentz invariance up to Planckian energy scales, where many of the "discrete spacetime" people were predicting something else.) Unless there appears solid evidence to the contrary, the idea that preserves those principles will remain the most promising. Especially as long as it remains such a fantastically productive idea for thinking about physics in general as Maldacena's #3 ranking in Inspire's 2012 most-cited list indicates.

23. **Peter Woit**
May 15, 2013

Cliff,

Hype such as "a fantastically productive idea for thinking about physics in general" isn't any more convincing than what you are responding to.

All: No more comments not about the book. Just stop.

24. **book**
May 15, 2013

How do you expect comments about the book since most people will not have read read it?

25. **Peter Woit**
May 15, 2013

book,

Some people do have access to the book itself via the CUP page I linked to. Also, Dawid has been writing about this with similar arguments in quite a few places, he has links to such papers on his website, see here.

http://homepage.univie.ac.at/richard.dawid/Homepage-Publications.htm

From such materials and the posting it should be clear what the topic of the book is.

26. **paddy**
May 15, 2013

I risk failing the "purely on the book dictum", since I have not read it but only gathered excerpts here and elsewhere on the web as well as perused the authors papers. 'Twould seem though from what I have gathered that the argument is that we should change our (empirically derived) standards of what is or is not considered to be a useful empirical scientific model just because (1) a "promising" field of research has failed as of yet to meet those standards and (2) there is a paucity if not a complete absence of alternatives (note: though no uniqueness theorem or conjecture is posited). Is this a fair assessment?

27. **Peter Woit**
May 15, 2013

paddy,

Kind of, although your formulation leaves open what "promising" means. Given the lack of progress towards making any predictions with string theory, it's hard to argue that it's "promising" in terms of moving towards a predictive, testable theory. Dawid is interested more in other reasons string theorists see the theory as "promising", reasons that have nothing to do with testability or prospects for testability.

I actually think this is an interesting question. When you are starting thinking about a speculative idea, the reasons for examining it more closely often are not that you see some potential way of testing it. Instead, the idea may be attractive for other reasons: it embodies some powerful and beautiful mathematical concepts, it's related by analogy to some other ideas that have worked well, etc. etc. You can think of a lot of good reasons why people decide to pursue one speculative idea rather than another.

The problem here is that these things are very slippery, and easy to fool oneself about or be less than honest about, with one's vague argument for why one should look at something evolving into a protective armor of hype as one keeps arguing with those who are skeptical your idea is going anywhere.

Unfortunately I don't thing Dawid really comes to terms with the real problems of this kind of "non-empirical theory assessment", since he sticks to taking at face value things which are heavily hype-ridden.

28. **Lee Smolin**
May 15, 2013

Dear Peter,

I am shocked that the no alternative argument still gets a hearing given the major progress on key issues in loop quantum gravity and spin foam models. Does the book acknowledge this progress? Ten years ago it was possible to be skeptical of LQG because of open issues such as getting the 1/4 right in black hole entropy and deriving general relativity as the low energy limit. However the last five years there has been major progress on both of these and other issues including having a detailed and credible account of the onset of inflation following a quantum bounce. If it was valid to deny LQG the status of a full alternative approach to quantum gravity because of these issues any honest account must acknowledge the progress made.

The no alternative argument fails on sociological as well as scientific grounds. This summers upcoming LQG meeting has already reached more than 200 registrations. Its fair to say that LQG is now after 25 years a healthy and vibrant large established research community. This doesn't mean its right but it does puncture the no alternative argument.

There is considerable progress as well on other alternative approaches to quantum gravity including causal dynamical triangulations and causal sets.

I would be curious to read the book and see if the author has an up to date account of of the alternatives he claims do not exist.

Thanks,

Lee

29. **Peter Woit**
May 15, 2013

Hi Lee,

Dawid discusses LQG on pages 91-94, with a very string theory-centric point of view. The oddest part is this

"Leading exponents of loop quantum gravity tend to be fairly confident regarding their theory's chances of success. However, compared to the way the status of string theory is assessed by that theory's leading exponents, it may be fair to say that assessments of loop quantum gravity are significantly more timid. While the sentiment that their theory is "too good to be false" is widespread among string physicists, adherents to loop quantum gravity are more inclined to emphasize that their approach constitutes one possible solution that has some attractive features but may well turn out false in the end."

which kind of argues against you on the grounds that you are insufficiently arrogant. Other than that, he evaluates things by his three kinds of arguments I explained in the posting. For "No Alternatives" (which he calls AAA), string theory is supposed to be a promising way to unify with the SM, while LQG doesn't really address this. For "Unexplained Explanatory Coherence" (his UEA) he explains the nice explanatory features of both approaches to quantum gravity, then somehow decides string theory is better. For "Meta-Inductive" (his MIA), again he's using the argument that we should believe string theory because it's like the SM (an argument that just baffles me), whereas LQG is like GR, which somehow is less desirable.

30. **bsm**
May 15, 2013

What testable BSM predictions do LQG and/or spin foam make, in particular at LHC scales? Have ATLAS and CMS published results of searches, or excluded regions of parameter space, for the BSM predictions of LGG/spin foam?

31. **Peter Woit**
May 15, 2013

bsm,

Neither string theory nor LQG/spin foam make any predictions about LHC scale physics.

32. **Alex**
May 16, 2013

We all agree that it is important to think about what the correct model of quantum gravity is, right?
Demanding that there are hard TeV scale predictions of something that by necessity involves the scale 10^19 GeV, that is basically a lamp post argument. There is no reason to think that dis/favouring approaches based on the amount of predictions they make *at the weak scale* can be a very misleading strategy. Of course one should try this (a la ADD and so on), but other than the hierarchy issues, there is no reason to think that quantum gravity should be so nice to us.

Quite the contrary, the fact that something as baroque as the Standard Model is to come out of this at low scales, makes it for me unlikely that there should be anything fundamental to the observed low-scale physics. I don't think anyone can seriously expect that the SM gauge group and particle content with such and such parameters are the unique consistent model that can come out of the correct theory of quantum gravity.

Given this, I find it disingenuous to criticize string research for the lack of predictive power at the weak scale. There is a lot of predictive power near the fundamental scale, smoking gun signatures if you will – it is merely a technical reason why we can't observe them, not a philosophical one.

@Joel Rice
Arguing about the name is silly, you could just as well criticize quantum field theory for its name. The names of stuff are all over the place for historical reasons and don't conform to the terminology we should impose in discussions with the general public (in order to avoid arguments like "Evolution is just a theory" from the anti science crowd).

The Standard Model is a Theory
Quantum Field Theory is a mathematical framework
String Theory is a mathematical framework
Any explicit realization of the Superstring is correctly called a “Model” in the literature.

33. **Alex**
May 16, 2013

That was supposed to read:

There is no reason to think that dis/favouring approaches based on the amount of predictions they make *at the weak scale* is a sensible strategy to find the correct theory of quantum gravity.

34. **bsm**
May 16, 2013

Unlike ST, do LQG/spin foam make any testable predictions at all?

35. **Alex**
May 16, 2013

IANALQGE, but I think you would expect to see some violation of Lorentz Symmetry at gignormous Energy Scales.

36. **Krzysztof**
May 16, 2013

Peter,

From what you write about the book it seems to promote a real ‘scientific revolution’ – a huge change of the scientific paradigm. And, in a way it has some religious connotations, or at least philosophical ones. The main goal in that respect is to explain or motivate physics by itself – even more at the end, to explain all the reality, including its roots and reason, by science, and by science only. S. Hawking and other ‘top theorists’ since long have been voicing dreams about the ultimate theory which should explain also itself. As a side remark, I wonder what they say about the Goedel’s theorems in that context…

To me, an experimental particle physicist, string theory as such is a purely mathematical project, having (almost) nothing to do with physics, so far.

37. **Lee Smolin**
May 16, 2013

bsm:

LQG is a framework for the quantization of diffeomorphism invariant theories. But there are striking consequences if not falsifiable predictions for experiment. One comes from the existence of a new parameter necessarily in the gravitational action, analogous to the theta angle of QCD: the Immirzi parameter. When this takes complex values there are parity odd quantum gravitational effects. These imply a parity odd component to the polarization in the CMB, possibly visible in the B-T channel. This could be seen in upcoming measurements at Planck and other detectors. This is not a falsifiable prediction but its detection would be a striking confirmation of the framework. To put this another way, Planck polarization measurements will constrain the Immirzi parameter.

See papers of Magueijo et al for details.

btw I am not aware that string theory makes any falsifiable predictions for doable experiments.

Thanks,

Lee

38. **Peter Woit**
May 16, 2013

bsm/Alex,

The whole point of this book is that it starts from the assumption that string theory (and also LQG) can’t make any testable predictions, so the author is coming up with a new methodology of science to evaluate untestable theories.

The problem with string theory is not that it can’t make testable predictions about physics at the weak scale, but that it can’t make testable predictions about physics at any scale whatsoever.

39. **Alex**
May 16, 2013

Peter,

“The problem with string theory is not that it can’t make testable predictions about physics at the weak scale, but that it can’t make testable predictions about physics at any scale whatsoever.”

Are you seriously claiming that the superstring does not make any testable predictions about physics at *any* scale?
What about the 6 extra dimensions, the string spectrum, Regge excitations, measuring open string and closed string scattering amplitudes explicitly? Come on, if we could by some surprising feat built a Planck scale collider, there would be a wealth of stuff to observe.
As far as the first point is concerned, it could be that there is no real 10D supergravity regime because the compactification scale is at the string scale or so, but that is an extreme situation.

40. **JG**
May 16, 2013

Philip Gibbs in his quasi-review (the book is above his price range, as it is mine) says the following:

“The more evidence there is against SUSY, the more evidence there is in favour of the multiverse and the string theory landscape” and “the standard model succeeded because it was based on consistency arguments such as renormalisability which reduced the possible models to just one basic idea that worked. The same is true for string theory so we are on firm ground.”

OK, one thing I thought was true was that string theory rises and falls on SUSY. And, what is the “one basic idea” that works in string theory?

Is this as crazy as I think it is?

41. **Peter Woit**
May 16, 2013

Alex,

This one has been gone over a million times (and has nothing to do with the book). Yes, we would learn something from a Planck scale accelerator, no, string theory makes no falsifiable predictions about it, since we don’t know what non-perturbative string theory is: would there be black holes produced? would it be six extra dimensions or seven (according to M-theory)? But wait, we don’t know what M-theory is…

Sorry, but I’m just going to delete further comments not about the book.

42. **Peter Woit**
May 16, 2013

JG,

Please argue with Philip about his posting on his blog, not here.

43. **Neil**
May 16, 2013

Of his three principles of “non-empirical assessment”, I find the “no alternatives” one disturbing and destructive. The other two seem reasonable. You can never know there are no alternatives, so it is silly to presume such. By this reasoning we should always stay on the path we are on because we cannot imagine another. But why would we try if a presumption of no alternatives is part of our assessment package?

And of course, as Smolin points out, in fact there are alternatives.

44. **fuzzy**
May 21, 2013

i think that such a philosophical position if perfectly ok if we want to relocate string theory out of natural sciences; in a sense, dawid blesses the idea of string theory becomes a branch of math and philosophy, and its physical origins can be forgot. in this sense, the position of joel rice can be agreed upon, simply adding a bare word to his statement: “string theory is not a *physical* theory”. of course, here i argue that we should maintain the vintage position of old isaac: “to be termed scientific, a method of inquiry must be based on empirical and measurable evidence subject to specific principles of reasoning” — this should remain the rule of natural sciences, without implying any derogative position toward math and philosophy. however, i feel that if the position of dawid will be accepted, mathematicians should be ready to hear statements as “since we will explain anything, stop working on riemann hypothesis–this is our stuff”

45. **Raisonator**
May 22, 2013

Joel Rice,
“It would be a lot easier if they would just stop calling it a theory. ”
A good point. I thought about this too. A big problem to me seems to be that when you call it “theory” in the media, lay people tend to think that it is comparable to the theory of relativity or quantum theory which have been tested to incredible accuracy in so many ways.

David Gross likes to call it a “framework”, which I find a very good idea. Or “the string hypothesis” could also do.

Best.

46. **Adrian_H**
May 22, 2013

I can think of one good reason why this new version of string theory + landscape would be attractive to someone who has some philosophy behind them: it is essentially equivalent to the model theory of modern logic.

In that way of thinking, which philosophers tend to associate with the name of Leibniz, but that goes back to Duns Scotus, there are a set of logical truths, which are essentially a set of mathematical constraints that are common to all models; and there are sentences that are allowed to vary from one model, or possible world, to another — the contingencies. If one increases the set of “logical/mathematical truths” that are invariant then you decrease the size of the “possible worlds”. If logic is science I suppose that this can all be considered science too — but logic would be a better name for it. (And it would be falsified if one of the constraints is shown to be false.)

One Ring to Rule Them All

May 16, 2013
Categories: Uncategorized

This week in Sweden the Nobel Foundation is running a symposium on LHC results. It's invitation only, but the slides of the talks are available here.

One of the scheduled talks today was about string theory, and I was wondering how that would fit into the "LHC results" framework since string theory has nothing to say on the topic. Now that the slides are available I don't see anything about the LHC, but there are some remarkable revelations. The first is that string theory is not science but "Magic", with several slides describing the "Magic of String Theory". The relationship to mathematics is that in string theory "No concept in Math remains unambiguous", which I guess is about what you would expect when you're dealing with magic.

An even bigger revelation comes later in the talk: string theory is Sauron's Ring of Power! It is described as "Concentrated Power" and it seems that the markings on the ring are a SUSY Lagrangian. Part of the Ring Poem is quoted

> One Ring to rule them all, One Ring to find them,
> One Ring to bring them all and in the darkness bind them.

To put this back in context, recall that this is Sauron's ring he created in order to control everything from Mordor. Here's more of the poem, including the original language

> Ash tug Shakhbûrz-ûr Ulîma-tab-ishi za,
> Uzg-Mordor-ishi amal fauthut burgûli.
> Ash nazg durbatulûk, ash nazg gimbatul,
> Ash nazg thrakatulûk, agh burzum-ishi krimpatul
> Uzg-Mordor-ishi amal fauthut burgûli.
>
> One for the Dark Lord on his dark throne
> In the Land of Mordor where the Shadows lie.
> One Ring to rule them all, One Ring to find them,
> One Ring to bring them all and in the darkness bind them
> In the Land of Mordor where the Shadows lie.

This interpretation of string theory as Sauron's ring I suppose could explain a lot…

Comments

1. **rings**
   May 16, 2013

   "… I was wondering how that would fit into the "LHC results" framework since string theory has nothing to say on the topic." IIRC, the Higgs boson results were announced at Lepton-Photon at Melbourne, despite the Higgs boson being neither a lepton nor a photon nor even a gauge boson. Methinks the L-P conference organizers polluted the purity of essence of the bodily fluids of the attendees by allowing such talks.

   Would you prefer the Ring of the Nibelung? At the end of it all, Brunnhilde rides into Siegfried's funeral pyre and burns the whole world down. Note that Brunnhilde doesn't ask her horse if he wishes to join in the immolation. It's my guess that ST theorists won't ask your opinion if ST burns all of theoretical physics to an end.

   There were many talks, with slides for most of them (not fot John Ellis at the end). ST was one of many talks, in the session on BSM physics. (Note that Lepton-Photon has talks on supersymmetry, although SUSY is neither leptons nor photons.) I had a look at the slides on "Future Accelerators". It contained the standard blurb, but one could hardly expect anything else. There was a nice slide at the end though:

   I have a Dream
   E = mc^2
   Extended Multiprobe Collider Complex

   I admit I had to read it twice to catch on. I thought it was clever.

   So there were many talks. ST was but one of many. Nothing really new, but the Future Accelerators talk, which was very down to Earth, also had nothing really new. One could hardly expect anything else.

2. **Hack**
   May 16, 2013

   Lol! Yes Peter we all know how the ring corrupts any one that posses it, perhaps working on string theory for a time has a similar affect on particle physicists. Hobbits were especially resistant to Sauron's ring, perhaps we need more Hobbits in Particle physics….

3. **PeterPetersonSenior**
   May 16, 2013

   Thank you for the commentary and the link. A have got a lot of laughs, mostly kindergarten stuff, I am would be curious to hear the talk, and the audience reaction.

   My favorites (in addition to one's you covered, so I will not repeat):
   1. It is always funny how the black hole is always trumped, ST success and mystery. The accumulated mass determines the horizon radius, entropy and gravity gradient should dictate the radiation. I could not be more ambivalent about information fate in black hole – on current level of theory it is unprovable either way. – Not so much funny, I am just curious what was said along with this slide.
   2. "Singularities are a reflection of a breakdown of an approximation." – makes sense, so it follows: "Gravity has magic cloaks to hide its secrets" – Ha-ha-ha.
   3. The best laugh: "One Equation? Infinite number of solutions!!"
   4. "Algebra, matrices and representations to replace geometry ?" (last page) – I think this is what gets us in trouble in the first place, see 3.

   And pages 68 and 69 – partial admission of realization that there is no way out of this paper bag.

4. **Bernhard**
May 16, 2013

So, is Witten Sauron's human form?

5. **SpearMarktheSecond**
May 16, 2013

Creme de la creme of the 50ish experimental community, IMO. Alex Read, Kevin Einsweiler, Valerie Gibson, Andy Parker, Rene Ong… Nima's talk looks a bit odd…. bifurcatory moment?

6. **Peter Woit**
May 16, 2013

SpearMarktheSecond,

The Arkani-Hamed talk was basically the same as the colloquium he gave at Columbia a couple weeks ago. My take on that is here
http://www.math.columbia.edu/~woit/wordpress/?p=5747

Bernhard,

I don't know about that, but based on my years living there, I always suspected Princeton might be Mordor…

7. **CIP**
May 16, 2013

I thought that the LHC was the ring to rule them all.

8. **srp**
May 16, 2013

When I read the title of the post and the first paragraph I thought the reference would be to the LHC particle ring. Now that it's the "only game in town" the LHC really does "rule them all."

9. **Maciej**
May 16, 2013

Yep, Witten (or Maldacena) is the Dark Lord and Princeton must be Mordor.
Peter, you have managed to mentally resist the shadows in their nest!
Respect.

More seriously, in the slides Rabinovici argues why 1=3 and at the same time 4=10. This follows from magic of ST because strings are extended.

Damn, their proofs get more and more sophisticated!

10. **SpearMarktheSecond**
May 17, 2013

Thanks Peter. Nima makes me appreciate Dirac's communication skills.

11. **M**
May 17, 2013

The problem is not the Sauron ring quote, but the rest of the talk. String theory has nothing to say about physics.

12. **chris**
May 17, 2013

it seems strange to me to cite one ring ruling all on an LHC meeting and letting this ring be anything other than the LHC – but taste does not seem to be one of the fortes of Rabinovici (see slide 35).

13. **Erestarchos**
May 17, 2013

So, the Sauron's ring should be destructed. And we should hope this will destruct Sauron too and all his minions. 😉

14. **destruct**
May 17, 2013

Be careful what you wish for. Destroying things is a two edged sword. Destroying 'Sauron's Ring' will do more than just destroy ST. When the SSC was 'destroyed' the solid staters expected that this would "free up much needed funds" for condensed matter research. But the 'pot of money' did not flow to them. Quite the reverse, funding for physics was cut across the board. Destroying ST will likely destroy your own funding.

15. **Shantanu**
May 17, 2013

Peter, FYI another conference at KITP with talks on supersymmetry etc
http://online.kitp.ucsb.edu/online/lhc13/
Would be interested to hear your views on some of these talks

16. **Anonyrat**
May 17, 2013

Yes, as destruct pointed out, be careful of what you wish for. The destruction of the One Ring meant that the Elven rings – Narya, Nenya and Vilya – faded in power and the diminished Elves moved out of Middle Earth. This could be a metaphor for physics as well.

17. **TD**
    May 17, 2013

    Ask yourself this question: If I were asked to spend MY hard-earned money on a failed speculative theory (ST), would I? That is the fiduciary standard. Taxpayers' money is usually treated as "free" money, but it isn't.

18. **Kalyan**
    May 18, 2013

    Human's are so creative, even if I tried, I couldn't make up something so bizarre, but here it is as trying to be accepted reality using Sauron's one ring. Beyond astounding!

19. **MathPhys**
    May 19, 2013

    What ST conferences badly need is an anthem. Something along the lines of *'Rule Britannia'* in Conservative party meetings.

20. **anthem**
    May 19, 2013

    Why is that any different from many other physics groups?

21. **MathPhys**
    May 19, 2013

    I am not aware of any other group of physicists with a comparably ideological attachment to a theoretical idea in spite of their total failure to find a shred of experimental evidence for it.

22. **Tmark48**
    May 20, 2013

    @ MathPhys : Maybe not a group of physicists, but history has shown time and time again that single physicists (even/especially famous ones) tend to cling to their ideas even in face of failure. Hence the motto, science advances one funeral at a time.

23. **Mehmet Efendi**
    May 21, 2013

    Not related but since this is not a serious post I may as well attract your attention to : Hard evidence for the multiverse has been found, article in the Daily Mail.

    http://www.dailymail.co.uk/sciencetech/article-2326869/Is-universe-merely-billions-Evidence-existence-multiverse-revealed-time-cosmic-map.html

Hard Evidence for the Multiverse Found, but String Theory Limits the Space Brain Threat

May 22, 2013
Categories: Favorite Old Posts, Multiverse Mania, This Week's Hype

In recent years there have been many claims made for "evidence" of a multiverse, supposedly found in the CMB data (see for example here). Such claims often came with the remark that the Planck CMB data would convincingly decide the matter. When the Planck data was released two months ago, I looked through the press coverage and through the Planck papers for any sign of news about what the new data said about these multiverse evidence claims. There was very little there; possibly the Planck scientists found these claims to be so outlandish that it wasn't worth the time to look into what the new data had to say about them. One exception was this paper, where Planck looked for evidence of "dark flow". They found nothing, and a New Scientist article summarized the situation:

> "The Planck team's paper appears to rule out the claims of Kashlinsky and collaborators," says David Spergel of Princeton University, who was not involved in the work. If there is no dark flow, there is no need for exotic explanations for it, such as other universes, says Planck team member Elena Pierpaoli at the University of Southern California, Los Angeles. "You don't have to think of alternatives."

One of those promoting the idea that "dark flow" was evidence for a multiverse was Mersini-Houghton, who in a 2008 paper with Holman wrote:

> Our contention, then, is that these observations of bulk flow can be construed as evidence for the birth of the universe from the landscape multiverse imprinted on the superhorizon sized nonlocal quantum entanglement between our horizon patch and others that began from the landscape. When we calculate the size of the induced dipole in our theory and convert it into a bulk velocity dispersion, we will see that for the constrained values of our parameters we arrive at a velocity dispersion of order 670 km/sec, remarkably close to the observed value of 700 km/sec.

One might think that the refutation of their prediction by the Planck data would be a problem. Instead though, the Sunday Times reported a few days ago that Scientists believe they have found the first evidence that other universes exist. The story got picked up by other news outlets, and appeared in the Daily Mail as "The first 'hard evidence' that other universes exist has been found by scientists". The source for the story was Mersini-Houghton:

> Laura Mersini-Houghton, theoretical physicist at the University of North Carolina at Chapel Hill, and Richard Holman, professor at Carnegie Mellon University, predicted that anomalies in radiation existed and were caused by the pull from other universes in 2005.
> Now that she has studied the Planck data, Dr Mersini-Houghton believes her hypothesis has been proven.
> Her findings imply there could be an infinite number of universes outside of our own.
> She said: 'These anomalies were caused by other universes pulling on our universe as it formed during the Big Bang.
> 'They are the first hard evidence for the existence of other universes that we have seen.'

She will be in Britain soon promoting this at the Hay Festival on May 31 and at Oxford on June 11.

According to a New Scientist story just out, this hard evidence for the multiverse should be welcomed, since it (together with string theory) has just been shown to have the power to save us from "Legions of disembodied brains floating in deep space". The story, which appeared in print as String Theory Limits Space Brain Threat starts with

> LEGIONS of disembodied brains floating in deep space threaten to undermine our understanding of the universe. New mathematical modelling suggests string theory and its multiple universes may just provide our salvation – and that could win the controversial theory a few more backers.

It goes on to explain about Boltzmann brains and a recent paper by Bousso and Zukowski, and ends with news of yet another experimental success for string theory:

> "This is potentially an added experimental success for string theory and eternal inflation," says Daniel Harlow, a physicist at Princeton University. "We need to understand it better – the fact that it potentially explains something is motivation to understand it better."

Update: More here on how string theory will save us from the space brains.

Update: I've appended a response from Laura Mersini-Houghton and Richard Holman about this to a later posting, see here.

Comments

1. **Adam Treat**
   May 22, 2013

   Can you provide a link to the New Scientist article that you quote above? Also, it is unclear if Mersini-Houghton and others are talking to the press about a new paper where they discuss their analysis of the planck data. Is this the case or are they talking to the press in anticipation of a new paper?

   I am also looking forward to a follow-up on the claims by Penrose and Gurzadyan regarding evidence for Conformal Cyclic Cosmology found in CMB data. I wonder if the planck data will be used to bolster or refute these claims that seem to be still alive and kicking.

2. **Peter Woit**
   May 22, 2013

   Adam,

   I forgot to put in the link, it's there now. As far as I know there is no new Mersini-Houghton paper providing her own analysis of the Planck data. The Gurzadyan/Penrose claims about the CMB may also be in the category of things the Planck found too implausible to investigate themselves.

3. **Bob**
   May 22, 2013

   I just can't stop laughing at the headline . . .

4. **dark flow**
   May 22, 2013

   I thought that the dark flow was convincingly debunked already in 2011:
   "Measuring the cosmological bulk flow using the peculiar velocities of supernovae"
   De-Chang Dai, William H. Kinney, Dejan Stojkovic, JCAP 1104 (2011) 015 .

5. **Charles Wilson**
May 22, 2013

"LEGIONS of disembodied brains floating in deep space threaten to undermine our understanding of the universe."
Ed Wood, Director of "Plan Nine from Outer Space", just rolled over in his grave.
Ed, you were born 60 years too soon.

6. **Eric**
May 22, 2013

I am confused…. So is it hard evidence for the multiverse or not?

7. **Peter Woit**
May 22, 2013

Eric,

Well, the Daily Mail and Mersini-Houghton say there's hard evidence, and they don't quote anyone contradicting this. Other reporters might turn up someone who would disagree.

I assume the "hard evidence" is not the "dark flow" prediction that didn't work out, but something else (probably the CMB "cold spot" that WMAP found, which Planck says is still there).

8. **Student**
May 22, 2013

"LEGIONS of disembodied brains floating in deep space threaten to undermine our understanding of the universe. New mathematical modelling suggests string theory and its multiple universes may just provide our salvation."

Here's a picture of string theory saving the Empire from the Boltzmann brains:

http://www.perimeterinstitute.ca/personal/shossenfelder/return_of_bb.jpg

9. **paddy**
May 22, 2013

oops…I clicked a link for NEW though ended up in xkcd

10. **nasren**
May 22, 2013

My first thought on reading this was: O dear God what will Matt Strassler say about this kind of journalism. So far he seems to be ignoring it.

11. **Peter Woit**
May 22, 2013

nasren,

I don't think the problem here is the journalists, who are just reporting the exciting news about hard evidence for other universes and great progress on the disembodied brain front that they are hearing from physicists. Michio Kaku did seem to once get to Strassler, but in general he doesn't appear to think this kind of behavior by scientists is worth commenting on.

It's a plausible idea that if most prominent theorists just ignore it, Multiverse-mania might go away on its own. My suspicion is that most physicists subscribe to this. Unfortunately, that doesn't seem to be happening.

12. **Maciej**
May 22, 2013

"This is potentially an added experimental success for string theory and eternal inflation"

Wow! Experimental? Seriously?
Forget experimental. I am not sure if this is mental success at all…

13. **physicsphile**
May 22, 2013

I think it is a bit unfair to tar all multiverse work as being garbage because of this. There certainly is a lot of bad research being done on the multiverse but I think there is also interesting stuff like Weinberg's original claim that it may be how the cosmological constant problem is solved.

14. **mult**
May 22, 2013

There will always be something to laugh at other people. Do some better work of your own which captures the headlines. One never knows, this may inspire young people to get interested in science, and they will produce the 'correct' Theory of Everything, whatever that might be.

15. **Visitor**
May 22, 2013

"Michio Kaku did seem to once get to Strassler, but in general he doesn't appear to think this kind of behavior by scientists is worth commenting on."

Strassler might want to rethink things because the house of physics is becoming an Augean stable and if the interested and responsible parties can't or, possibly worse, won't clean up the mess, eventually the government is going to *seriously* curtail its funding. And then you'll see see how far some noveau-riche Russian oligarch's money is going to get a very few of you – with the rest of you driving taxis or teaching algebra and geometry in high schools across the land.

16. **M**
May 23, 2013

Boltzmann Brains are great, but can String Theory explain why chicken with cut head can survive for years? This phenomenon probably is related to the survival of theories found to be (not even) wrong.

17. **David Nataf**
May 23, 2013

Peter,

Your criticisms of the Dark Flow are quite unfair. Just because it could be explained by a multiverse, does not mean it would need to be explained that way.

The idea of the dark flow was that galaxies have a preferred vector of peculiar motion, which would break the anisotropy assumption that is fundamental to cosmology. There is definitely a coherent linear flow in the nearby universe up to several dozen megaparsecs, and people have debated whether this is due to a "great attractor" (a nearby supercluster), statistical coincidence, or a dark flow. If Planck is correct then it may just be a 1 in 20 statistical coincidence or so that affects the local volume.

Regardless of whether a preferred peculiar direction of motion for matter would be due to a multiverse or not, it is a legitimate scientific test to conduct, to investigate, to write papers about, et cetera. It doesn't need to exist, but it could have existed as dark matter and dark energy (for example), certainly exist, and dark radiation is now a 2-sigma phenomenon. The Friedman-Robertson-Walker-Lemaitre metric from General Relativity, that is the foundation of cosmology, predicts that there should be zero dark flow, similarly to how the standard model of particle physics predicts zero dark matter and zero dark energy. It is good and valid to test these things.

I do however apologize for astronomers' lack of imagination in naming mysterious phenomena. As such, please don't turn into Dark Peter.

18. **Peter Woit**
May 23, 2013

David Nataf,

I'm sorry if this was unclear, but I had no intention of in any way criticizing people for studying "dark flow" or for calling it that.

19. **dark flow**
May 23, 2013

David,

There is no bulk flow as initially claimed. The paper I mentioned indicates that while the direction of the "bulk flow" was correctly measured, its magnitude is much smaller than initially thought (200 km/s instead of 1000 km/s). This magnitude is consistent with Lambda CDM, so there is no need for any unusual physics, certainly not multiverses which are tugging on us. These findings were now confirmed by Planck.

20. **Dom**
May 23, 2013

A good day for solving all known problems, in The Guardian we have
Roll over Einstein: meet Weinstein
"What are we to make of a man who left academia more than two decades ago but claims to have solved some of the most intractable problems in physics?"

21. **Luis**
May 23, 2013

It should be noted that the usual "piece of evidence" claimed for "other universes" comes not that much from the dark flow but from a rather large cold spot in the CMB. Apparently, the consensus as of now is that the spot is simply an artefact of the analysis: http://arxiv.org/abs/0908.3988

22. **Avattoir**
May 23, 2013

The Daily Mail you say?

http://tinyurl.com/ylymnz5

23. **Peter Woit**
May 23, 2013

Dom,

Thanks. I added a new posting about that topic.

24. **Nick M.**
May 23, 2013

Peter,

I think you may have meant May 31, and not June 31, for the date of the Laura Mersini-Houghton appearance at the 2013 Hay Festival in London; i.e., see here, and scroll down the page to see the appropriate listing.

–Nick

25. **Peter Woit**
May 23, 2013

Thanks Nick, fixed.

26. **Stephen Heyer**
May 23, 2013

To me, the strange thing is the very odd and unlikely model of a Boltzmann brain that always seems to be discussed: In short, a complex, fully formed, self-aware being appearing out of nothing. Oh! And the life support system capable of supporting the brain from local resources (wisps of left over hydrogen?) is never mentioned, despite the fact that without it the brain's existence would be momentary.

Far, far more likely would be the Boltzmann equivalent of a bacteria with a built in tendency to survive, reproduce and become more intelligent, much the way we current Boltzmann brains did it. The only real difference being that Boltzmann brains are usually imagined as existing much later in the universe's history when resources are much more diffuse.

In fact, looked at like that, they become both familiar and very likely. All we are really saying is that life and probably intelligence will arise anywhere there are the resources to support it, even if those resources are very different to those we are currently used to.

A final thought, given the fascination these discussions always have with a late stage universe with mostly burned out stars, that universe is probably going to be already fully populated and it's resources carefully husbanded so there might not be much opportunity for late comers.

All it takes is for one high technology society from our period of the universe to not destroy itself during adolescence and stabilize and, it, or something derived from it, will be still there thousands of billions of years later using increasingly clever ways of living off the available resources (unless, of course, its cracked ways of making entirely new ones).

So you see, for almost all of its existence, the universe will be mostly observed by something like us, or something distantly derived from something like us. No problem!

Stephen Heyer

27. **Michael R**
May 24, 2013

Does anyone know how the proponents of the Dark Flow Multiverse concept explain how it is that light may not escape a universe but that the gravitational force of one universe acts on another neighboring universe?

28. **chris**
May 24, 2013

Michael,

the horizon was not always as small as today. all these proposed effects would be primordial remnants.

29. **Bob Hope**
May 26, 2013

No. Seriously, if there is a Boltzmann brain, then there must be a corresponding Boltzmann brane or a class or category of them which embed the Boltzmann brain. And yet I couldn't find any. Is not there any? Are these just Riemann surfaces too? Or are they higher dimensional branes?

Eric Weinstein on Geometric Unity

May 23, 2013
Categories: Uncategorized

Eric Weinstein is a Harvard math Ph. D. who has been working as an economist here in New York for many years, and someone I've often enjoyed talking to over the years. Going back to his days as a graduate student, he has been working on some of his own far out of the mainstream ideas about geometry and physics (which I've never seen the details of). Eric has finally gotten to the point where he is willing to talk about these ideas publicly, and he is giving a [lecture today in Oxford](), something that was arranged by Marcus du Sautoy. The Guardian has a long article about him and his work [here]().

There's a bit of an analogy with the Garrett Lisi physics outsider story here, although I think Eric will get less media attention since he doesn't have the surfing angle going for him. Both he and Garrett are pursuing what seems to me one of the deepest questions around: what is the relationship between the SU(3)xSU(2)xU(1) geometry of the Standard Model, and the 4d pseudo-Riemannian geometry of space-time and general relativity? Garrett was trying to understand this in terms of E(8) symmetry, and I'm looking forward to seeing what Eric's ideas about this are. I'm not sure when he'll have a paper out on the arXiv, or whether some sort of version of his lecture will be available.

Update: The Guardian now has a [very enthusiastic article]() about this by Marcus du Sautoy, while New Scientist has a skeptical take [here]().

Update: See [Jennifer Ouellette]() for a critical take on the Guardian coverage.

Update: It seems that claims that physicists were not invited to Weinstein's talk are not true: an announcement and posters were sent to the physics department, but did not get widely disseminated. For a small amount of info about the talk, see the comment here from "Leaker".

Comments

1. **isomorphismes**
   May 23, 2013

   FWIW, his economic theories are considered heterodox as well (see Eric Falkenstein's post on EW) but I think they make sense.

2. **fuzzy**
   May 23, 2013

   i'd like to recall that garret lisi's paper http://arxiv.org/abs/arXiv:0711.0770 was preceeded by fabrizio nesti's one http://arxiv.org/abs/arXiv:0706.3307 (published). fabrizio doesn't surf, though he is a good sailor; hope this helps to give him consideration.

3. **Nameless**
   May 23, 2013

   I'm not sure if even getting access to the lecture would be helpful. Simonyi Lectures exist "… to promote the public understanding of science". If the intended audience is higher level than Guardian readership, it's not much higher than that. The lecture is certainly not directed at experts in high energy physics. We'll have to wait for an arxiv paper to make judgments.

4. **David Roberts**
   May 23, 2013

   "[On Friday 24th May from 10:00 — 12:00 in L3](),
   Eric Weinstein (Oxford)
   Geometric Unity

   A more detailed exposition of Thursday's Simonyi Lecture."

   Anyone willing to go and take notes?

5. **Garrett**
   May 24, 2013

   Eric's been working on this for a long time — I'm happy he's now publicly airing some of his ideas. And I'm looking forward to seeing what he puts on the arXiv.

   To answer the raised issue of Nesti's previous work: I did have some good conversations with Nesti, and even climbed a mountain with Percacci — and I cited their work. The idea, in general, of graviweak unification is groundbreaking and needs more attention.

6. **fuzzy**
   May 24, 2013

   hi garrett,

   it is very kind from you to mention this, and i hope it will help the italian academia, a bit stiff and hard-eared, to recognize the great talent of nesti.

   (of course, i am a friend of nesti, not him. He would not use silly nicks as those i use, he would have signed with his name, just as you did)

   finally, i apologize for the offline statements here above — but i hope they will offer you other occasions of mutual exchanges.

7. **A.J.**
   May 24, 2013

   Paper, or it didn't happen.

8. **tea**
   May 24, 2013

   The Theory of Everything is by now old hat. What we need is a **T**heory **E**xplaining it **A**ll. The author of the New Scientist article claims that the Oxford physicists were not invited to Weinstein's talk. That is curious, considering that the talk was publicly advertised on websites. However, if there was indeed a scheduling conflict with a physics talk on alternative sources of CP violation, then that indeed was poor planning. There seems to be no statement that Weinstein has been invited, or is meeting, with multiple people at Oxford (physicists … Penrose?) to discuss/answer his ideas one on one. That is strange, but who knows. Hopefully an arXiv preprint or other public document will appear soon.

9. **NuclearHobbit**
   May 24, 2013

   Although not an abstract of this lecture, I think Mr. Weinstein may have outlined its content here:
   http://edge.org/responses/what-is-your-favorite-deep-elegant-or-beautiful-explanation
   (scroll down to Mr. Weinstein's answer)

10. **Nameless**
    May 24, 2013

    @NuclearHobbit his answer seems to be half history, half lyrics, and, if there's any of his own work in there, it's well hidden. (However, that does not exclude the possibility that it is, in fact, an abstract of his lecture.)

11. **Bob Jones**
    May 24, 2013

    Can someone tell me where Ed Frenkel endorsed this guy?

12. **Bob Jones**
    May 24, 2013

    Never mind. I didn't see the first Guardian article.

13. **outsider**
    May 24, 2013

    It is perfectly possible for an outsider to have ideas which fundamentally alter our world view, for example a clerk working in a patent office in Switzerland. But Einstein published his ideas in papers on Brownian motion, the photoelectric effect and special relativity, with detailed formalisms and calculations to back up his ideas. Weinstein really needs to put out a concrete preprint, and in fact he should have done it before giving his lecture. "Paper, or it didn't happen" — absolutely spot on.

14. **Nameless**
    May 24, 2013

    @outsider The amount of background information Einstein needed to come up with special relativity and the amount of information needed for a scientist today to unify QFT and GR are so vastly different that it's hard even to compare them. We are talking two, maybe three orders of magnitude.

    Special relativity involved some out of the box thinking, but, as far as background information, he needed basics of Maxwell's electromagnetism, kinematics, null result of Michelson-Morley experiment, and some knowledge of the theory of aether as it existed in 1905.

    To unify QFT and GR, you need to be familiar at least with QFT and GR. An adequate exposition of QFT that covers all progress through, say, 1980, without any prerequisites, would fill 1000 pages. Add another 500 pages for GR. If you want to include any fancy new stuff, volume of background information keeps growing and growing. Any half-hearted attempt at unification should probably give Einstein-Cartan in the continuum limit, E-C is not included in standard GR textbooks and you need to pick it up on the side. If you want to go in the same general direction as Garrett Lisi, you need to understand algebraic group theory and know a thing or two about E(8) (add another 1000 pages of mind-bending mathematical theory to your reading list.) Current situation is that a even a random third year physics grad student, who spent years and years on full time study and who has already covered everything up to and including fundamentals of QFT and GR, would find most articles in theoretical high energy physics section of Arxiv so incomprehensible that they might as well have been written in Greek.

15. **Peter Orland**
    May 24, 2013

    Nameless,

    Nobody knows everything. We learn things as we go along. Though it is true that learning without thought is ill advised and thought without learning is perilous (as someone famous once said), you can do both, just by working seriously on a real problem.

    Speaking of real problems, however, I am not sure unifying QFT with GR qualifies as one.

16. **Nameless**
    May 24, 2013

    What I'm not sure about is whether one can learn everything necessary to unify QFT with GR while working full time as a Wall Street quant and writing papers on economics in one's spare time.

17. **David Nataf**
    May 24, 2013

    Einstein was much more than a clerk at the patent office. He had a full PhD in theoretical physics, and he had written thoughtful papers on thermodynamics prior to his steps forward in 1905.

    The stereotype of Einstein is that he was bad at math in school, he failed all his math courses and he sucked, and then he was out of the field and had to work at the patent office, and then he solved every problem in physics at the time completely out of the blue, and there were no precursors in the literature that could motivate any of his advances. The idea is that he made his advanced entirely from pure thought, by visualizing himself riding a wave of light. This is of course absolute fiction, and it's extremely harmful to science.

    Albert Einstein was in fact very gifted at mathematics, and he had a full PhD in physics, where the standards might have been far higher in ~1900 than they are today. Relativity, for example, did not come out of the blue, as he and many other physicists had already seen the Lorentz equations in Maxwell's electromagnetic theory, he was merely the first one to make the connection to inertial reference frames.

18. **outsider**

May 24, 2013

Weinstein has a Ph.D. in mathematical physics from Harvard, and while I have not checked his publications list, I have no doubt that he has many published papers of highly accomplished research. Not exactly a nobody tinkering in his garage with no formal training. But he really needs to put out a preprint.

19. **fuzzy**
May 25, 2013

hi outsider, there is one database that is used by all physicists working in this field, named inspire. you can check that there is only 1 record there, his thesis
http://inspirehep.net/search?ln=en&p=f+a+weinstein%2Ce&action_search=find

20. **Narad**
May 25, 2013

> you can check that there is only 1 record there, his thesis

Given the title, I have to wonder whether it has an epigraph.

21. **nasren**
May 25, 2013

The case of Weinstein is interestingly dual to the other problem that has come up recently: that of established (ie employed) theoretical physicists talking absolute rubbish. I would think that physicists would be more concerned about THAT trend, rather that the possibility of a bright, qualified outsider saying something novel and interesting. So this adverse press reaction smacks of vested interests. (And I would like to see some clarification on what it means when a physicist claims that "physicists weren't invited". Does that mean they weren't informed about it? Or that they weren't sent an engraved invitation and a pretty-please?)

22. **David Nataf**
May 25, 2013

nasren,

It probably means that the email to the math department inviting them to the colloquium was probably not cc:ed to the physics department, which is common practice in inter-disciplinary talks.

23. **momerathe**
May 25, 2013

wake me up when there's a testable prediction.

24. **Tom Andersen**
May 25, 2013

There have been a few comments to the effect that without knowing QFT/Standard Model/GR/ in detail, its impossible to formulate a new theory. Rubbish. Why would Kepler have to know the Ptolemaic theory of deferent and epicycle in order to discover elliptical orbits?

Like most 'outsider' (is the ratio any better for the insiders?) papers, Eric's ideas are likely flawed in some way, but that does not mean that his contribution will not be useful. I have not seen much value from the official theoretical physics team from the past 30 years, even though the amount of time spent on the subject was easily greater than all the previous time spent on this subject throughout history.

There is so much baggage in the current theories of physics that 'knowing all of it' only serves to fasten the blinders on.

25. **Anonyrat**
May 25, 2013

The Guardian article says: "Weinstein begins the paper in which he explains his proposal with a quote from Einstein: "What really interests me is whether God had any choice in the creation of the world." "

Where is this paper?

26. **Peter Woit**
May 25, 2013

Anonyrat,

As far as I know, such a paper is not publicly available, and I haven't seen a copy.

27. **Jeff M**
May 25, 2013

To follow up on @fuzzy, checked mathscinet, Weinstein has no papers there either. du Sautoy is of course a serious mathematician, and assuming he's seen some sort of preprint (one hopes!!) then there might be some interesting math going on. Aesthetically I like the geometrization idea, and it might be possible that Weinstein has had an interesting insight somehow that Atiyah and Singer and all the rest missed somehow, but I wouldn't bet my house on it 🙂 Also, keep in mind that as a research mathematician du Sautoy is a number theorist, his serious papers are mostly on zeta functions and groups, not infinite dimensional geometry.

28. **CPV**
May 25, 2013

Jennifer Ouelette is absolutely correct. Weinstein et al are master manipulators. I've seen his act trying to claim that economies are best modeled as quantum field theories and other complete nonsense. He is best called out as what he is – a high-falutin' fraud. I have no idea how Frenkel got mixed up with him, but his movies show he is no stranger to publicity seeking activities. It's not bad manners to call these things for what they are, and not to get duped by a smart huckster.

29. **op-ed etc**
May 25, 2013

What is also puzzling is du Sautoy's behavior. He is after all an established math professor and knows 'the system' or the protocol to handle these claims. By his own admission, he met Weinstein at a bar in NY two years ago and heard Weinstein describe his ideas. Then he should have said something like "That's really great. You should write up something on the arXiv and I'll show it to people at Oxford. Let's get their feedback (both physics and math). Then I can arrange for you to give a lecture and meet people privately … " But instead there is no preprint of any kind, and physicists were apparently 'not invited' to the lecture, and du Sautoy goes so far as to write an op-ed piece for the Guardian. Puzzling.

30. **Kara Szathmary**
May 25, 2013

I hope that Weinstein's eventual paper will NOT be the likes of Nassim Haramein's "Schwarzchilds Proton". I did like Garret Lisi's paper and thought what a shame he's gone surfing, and like Grigori Perelman vanishing after proving Poincare's Conjecture.

31. **JoeMomma**
May 25, 2013

Has anybody seen this Geometric Unity? When and where can we access it if we're not at Oxford? It's kind of lame someone claims to have solved big problems but doesn't give the public any papers/lectures/whatever.

32. **gu**
May 25, 2013

Geometric Unity is a self-applied label. You can't access it if you're not at Oxford, and the evidence indicates that you can't access it even if you *are* are Oxford. Just about everyone has remarked that this is a glaring omission.

33. **King Ray**
May 25, 2013

Perhaps they are stringing him along in the hopes of a large financial donation from him or his employer.

34. **Terry**
May 25, 2013

I met a guy at an APS conference in the 1990's who gave me a hard bound book (the thesis kind, not the publishing company kind) that he wrote called "The theory of Unity" which he claimed solved all of the problems of physics and unified QFT and GR. I still have it. There are almost no equations in it and a lot of technical sounding words which were clearly invented by the author. At the time I complimented the author and assured him that I would look at it. I am sure almost everyone here has similar stories. Although it is not necessary to have a paper on the arxiv prior to giving your lecture (that happens all the time!), it is unusual that the slides, notes, audio, video, or even audience notes have, as yet, failed to appear.

Personally, I would rather have talks like Frank Nelson Cole at the AMS in 1903, or Andrew Wiles' lecture at Cambridge in 1993. I.e. the lecture is the place where the result comes out and the media fame follows that — not the other way around (like cold fusion, faster-than-light neutrinos, etc etc etc, and now Geometric Unity)

35. **hbar**
May 25, 2013

I attended an APS April Meeting once, where I saw in the abstracts that someone had a new proof from first principles that hbar = h/2pi. Unfortunately I was so stupid that I missed the talk by 20 min (these are 10 min talks in parallel sessions), so I regret to say I missed the proof.

36. **Terry**
May 25, 2013

Does anyone even know what group or space the theory is based on? How many dimensions? … or better yet, the predicted proton lifetime 😉

37. **Richard**
May 25, 2013

My take on this, for those who are interested — short version: this is not how science is done, and du Sautoy should know that.

http://excursionset.com/blog/2013/5/25/trainwrecks-i-have-seen

I think this does have similarities to Lisi, and also to Wolfram — stories about revolutions in science whose impact outside the community is greater than their impact on science itself.

38. **King Ray**
May 25, 2013

I think that good scientists and mathematicians seek the truth and not publicity and attention. Compare others' actions to those of Perelman.

39. **nasren**
May 25, 2013

With one breath people say that this is not how science is done — and usually it is not, agreed — but on the other it seems that Jennifer Ouellette and others are trumpeting the way science is usually done as the right way, even while we are being told that the way science is now done is not the way it was done in the past. If you follow me. 🙂 What I see are professional norms being broken all over the place and a discipline that is happy with that provided tenure is given and the money keeps rolling in. So when someone comes from outside it is a threat — to a status quo that is impossibly broken anyway.

New ideas are usually rubbishy old ideas repackaged — I have lost count of the number of times I have been handed a "book" with a solution to the entire universe — which usually ends up saying something like "it's all energy man". I was once handed such a thing from someone who claimed to square the circle ("hey, you just change the value of π to 22/7!") We all have. But Weinstein is not one of those guys.

P.S. Many years ago when I was on this site regularly (around 2004 or 5 I think) I heard of a guy who was doing maths physics outside of academia, while surfing in Hawaii. I thought this was very cool and have often thought of it since, wondering how it was going. And today I am delighted to find that it is Garrett Lisi and that he got his ideas out there! Amazeballs, as the kids say.

40. **momerathe**
May 26, 2013

look on the bright side: it may or may not be real, but maybe he'll get 3 million Russian dollars out of it.

41. **Monty Python**
May 26, 2013

Half a theory, philosophically,
Must ipso facto, half not be.
But here the half, that's not a theory,
Is all we see. Do you agree?

S U two, cross S U three,
Eric the half a theory.
G mu nu, is eight pi T,
Eric the half a theory.

Is this cryptic news story,
About an unknown PhD,
Scientific methodology?
No! It's Eric the half a theory…

42. **Daniel Tung**
May 26, 2013

It is great that people from outside of academia produce serious and significant work.
However, I doubt that the next step of revolution in Physics will require only beautiful mathematics and no deep conceptual re-considerations.

43. **Terry**
May 26, 2013

@nasren There is a reason that "the way science is usually done" is the right way. We tried it the other ways – they didn't work. Let's be real about this folks. I know it seems fun and exciting to speculate about the unknown hero riding in from the outside and revolutionizing physics, but that isn't going to happen here. The guy got a phd from Harvard a couple of decades ago and hasn't even published a paper since and now his math buddy from grad school invited him to give a lecture about his sideline musings. We all know how this story is going to end. The fact that he has spent 20 years doing hedge fund math does not make his insights more insightful. In fact, the opposite is true. Hedge fund math is not grand unification. Not even close. Weinstein's theory may contain one or two interesting mathematical ideas, but much more likely, and appropriately for this blog, it will end up being filed in the "not even wrong" bin of history.

44. **DLB**
May 26, 2013

Seems like everyone is debating reputations and speculating on whether an outsider can come with a new theory that correctly describes reality. That is not how science evaluates ideas.
Let's wait and see what this theory is, how well it describes reality and what predictions that are original to it that can be experimentally or observationally tested.
Theories are not validated by degrees or titles, but by experiments and observations.

45. **S**
May 26, 2013

Terry,

In fairness, it's not clear to me that the theory is likely to be filed in the "not even wrong" bin of history if it predicts 150 new particles with exotic properties, especially if they're at accessible energies. Wouldn't that be just plain vanilla wrong (if it didn't pan out)?

46. **mike**
May 26, 2013

Peter,

Do you know of anyone who attended? Any reports about content?

47. **Terry**
May 26, 2013

S, good point. That seems more likely. I.e. just plain old wrong.

DLB, when he goes to the press with vague statements, presents no paper, and no lecture notes or slides from the talk, there is little else we can do except speculate based on reputations of supporters, educational background, and what-not. The conversation would be much more technical and scientific if they had given us something tangible to discuss. (The conversation might also be much shorter. For example, "it predicts a proton lifetime of 10 nanoseconds, so no point wading through the rest of the screed")

48. **Leaker**
May 26, 2013

Some comments on the talk by a mathematician that were forwarded to me:

"The core idea seems to be to work with the 14-dimenional bundle of all metrics over a 4-dimensional manifold, as a way of generalising GR, the standard model, and QFT a the same time. The high dimensionality gives very large multiplets of as yet undiscovered particles, and he has no idea of their masses. …
…He still hasn't got all the details worked out and there's no preprint. My general verdict would be that it's certainly not nonsense, but I would take a lot more convincing that it's heading in the right direction."

49. **Oldster**
May 26, 2013

"Any half-hearted attempt at unification should probably give Einstein-Cartan in the continuum limit" —Nameless

Why? It's only one possibility.

50. **Nameless**
May 26, 2013

OK, "Einstein-Cartan or something like that". Point is, if you're doing unification, you will end up predicting (or, more likely, assuming) the effect of spin on the geometry. It may be null, but it probably isn't, and no one discusses it in standard textbooks because, except in the vicinity of Planck energy, it's so minuscule that it's not experimentally verifiable.

51. **Peter Woit**
May 26, 2013

Thanks Leaker,

That sounds consistent with the little that I've heard directly from Eric about this.

52. **Oldster**
May 27, 2013

I tend to agree with Roger Penrose that spin has been one of the great mysteries in quantum mechanics. As best as I can recall, he said it was one of two primary mysteries in a talk at NYU back in the late 1990's. At the moment, I don't remember what the second one was, but I'd agree that spin's effect on geometry is a necessary problem to be solved by a complete unification. But until a specific model is proposed, even the magnitude of the effect remains unclear. You are right that it is small in the specific Einstein-Cartan case …

53. **nasren**
May 27, 2013

Oldster — I think this is right. Atiyah has said the same thing. And understand spin and I think we'll understand entanglement a lot better.

54. **Nameless**
May 27, 2013

The effect of spin is likely small quite generally, from dimensional arguments. An iron magnet with spins of its atoms perfectly aligned has average spin density on the order of 10^-5 J*s/m^3. If spin is coupled to geometry through the same G as the stress/energy tensor, we can construct a quantity *G/c^3 with dimensions of inverse length, somehow related to the curvature of spacetime induced by spin, which works out (if I'm not mistaken) to about (10^40 m)^-1.

55. **cormac**
May 27, 2013

Hi Peter, I'm a teeny bit surprised at your reaction – isn't this exactly the sort of hype you are usually so critical of? No paper have been published, not even an abstract, so why all the fuss? Perhaps you have seen something something we don't…
As regards the du Sautoy article, I think it's an editorial. Think of all those published papers that never got a mention, never mind an editorial.
Btw, I was suprised by du Sautoy's sentence
"One proposal for the source of this push involves reintroducing the cosmological constant into Einstein's Field Equations. But this cosmological constant has always seemed very arbitrary and a retrospective fix."
As a constant of integration, there is no particular reason for the cc to be zero, which is why it never really went away

56. **Tony Smith**
May 27, 2013

Peter can you give a description of

the 14-dimensional bundle of all metrics over a 4-dimensional manifold

with details such as its bundle structure (base manifold and fibre),
symmetry groups, etc ?

Tony

57. **Peter Woit**
May 27, 2013

Hi Cormac,

There's an infinite amount of hype about science out there, I try to only spend time denouncing that part of it which seems most problematic, hype about ideas that are hugely influential despite their failures (string theory, SUSY, multiverses). When Marcus du Sautoy gets the Oxford String Theory Group renamed as the Oxford Geometric Unity Group, despite Weinstein's ideas not working out, then I'll add them to the list to denounce. Right now, I just don't see the problem of Weinstein getting too much attention for his ideas as a major one for physics.

Another reason for not writing more about this is that I don't know much about what is going on, which seems to be a good time to wait a bit before commenting more. Lots of people have been denouncing du Sautoy for not inviting physicists to the talk, when it turns out he did. Eric definitely needs to get a paper or slides or something out with details of his ideas, maybe that's imminent, maybe not, we'll see.

I'll write more about this soon though…

58. **Peter Woit**
May 27, 2013

Tony,

The metric tensor is a symmetric bilinear form, so 10 components in 4 d. So, you could make a bundle over your 4d spacetime, with 10d fibers given by the symmetric bilinear forms on the tangent space.

Beyond that, Eric is the one who needs to get details of this out, not me, since I'm still in the dark…

59. **Oldster**
May 27, 2013

Nameless, yes, you can do that as long as you're not dealing with some more exotic structure analogous to the original Weyl or Kaluza-Klein unified fields. In structures like those, the factor of G may not always lie between the physical and the geometric, that's all.

60. **Alex**
May 28, 2013

"The metric tensor is a symmetric bilinear form, so 10 components in 4 d. So, you could make a bundle over your 4d spacetime, with 10d fibers given by the symmetric bilinear forms on the tangent space."

Considering that there are 12 Gauge bosons, 14 sounds a wee bit low then… Well, we'll see, or not.

"
"One proposal for the source of this push involves reintroducing the cosmological constant into Einstein's Field Equations. But this cosmological constant has always seemed very arbitrary and a retrospective fix."
"

Yes, that's indeed strange. If I am not mistaken, in an effective field theory approach of SM + Einstein-Hilbert, the cosmological constant appears automatically as a counterterm. The problem has been for decades that naively, one has to tune it small, not that it is there in the first place.
The way the paragraph is phrased sounds to me like du Sautoy does not really know the stuff.

61. **Jamie Vicary**
May 28, 2013

Those who missed it the first time need only wait 2 more days — for it seems Weinstein is giving a re-run this Friday at 2pm in the Mathematical Institute in Oxford. Maybe the particle physicists will come this time!

62. **PhiGuy110**
May 30, 2013

Given how bloated and self-referential modern physics have become without offering many testable predictions (string theory anyone?) it's not surprising to me that real insight could come from outside the ivory tower echo-chamber. I think Weinstein probably deserves a fair hearing and open minds. However, without a paper or predictions, it's hard to say much of anything, good or bad. I look forward to following the story as it works it way through the physics community. I reckon that we'll know sooner rather than later whether there is anything to this.

63. **kevin dowd**
June 3, 2013

I would like to say that the faster than light neutrino gjys always get a bad wrap. AFAIK they stood up there and said we have this data, we can't get rid of it, it looks funky, and we ask you help in showing where we went wrong.

No wild claims .. I thought they were embarassed they had these results and could not suppress them because, well, you just don't go around suppressing data you don't like without a reason. And then later of course it was a cable problem. But they didn't know that.

64. **Peter Woit**
June 3, 2013

kevin dowd,

"And then later of course it was a cable problem. But they didn't know that."

That's why they get the bad rap. They should not have gone public until they had checked their cables. It's not about "suppressing data", it's about how hard you work looking for what could be the source of the problem when there is something weird in your data. Some members of the collaboration refused to put their names to the paper, knowing they hadn't checked things carefully enough to justify going public.

Anyway, that story doesn't really have much to do with this one.

65. **King Ray**
June 6, 2013

I think Eric's tweet from May 29 is instructive:

"You'll get an article whenever I've time to finish one & only because I really want to give you one. This is just a (time thirsty) hobby."

It seems like he is more interesting in getting attention than in contributing to science.

66. **Peter Woit**
June 6, 2013

King Ray,

I know Eric pretty well, and he's not doing this to get attention, get on TV, get a book deal, etc. As far as I know, the Guardian blog posts were not his idea. He seriously believes in what he is doing, has been working on it completely quietly for many years, and is deeply conflicted about discussing his ideas in public. Yes, he really needs to get a paper written so others can see what he has, but it's not because he's trying to get attention that he hasn't done that yet.

67. **King Ray**
June 6, 2013

Peter,

I stand corrected. It sounds like things haven't sufficiently converged yet for him to feel comfortable writing a paper–perhaps there are too many loose ends still. I've found that writing things down helps clear things up sometimes. The theory sounds like a large extrapolation on the SM though.

Various Links

May 25, 2013
Categories: Uncategorized

- The Smithsonian has a long article about Lisa Randall here.
- The Wall Street Journal has a shorter article about Randall's high school classmate Brian Greene here. Brian's World Science Festival will start here in New York on Wednesday.
- I'll probably skip the World Science Festival in favor of an event at the CUNY Graduate Center: a conference on the work of Jim Simons, in honor of his 75th birthday. The conference will start off Tuesday morning with talks by Witten and Deligne (for a recent piece about Deligne and the Weil conjectures by Ed Frenkel, see here)
- One of many worthwhile things funded by Simons is Simons Science News, which now carries some of the best science journalism around. There's a new interview with David Gross, who talks about the way QFT overcame those who wanted to do away with it in the sixties. About string theory:

  > String theory is not as revolutionary as we once hoped. Its principles are not new: They are the principles of quantum mechanics. String theory is part and parcel of quantum field theory.

  About the multiverse:

  > There are frustrating theoretical problems in quantum field theory that demand solutions, but the string theory "landscape" of 10^{500} solutions does not make sense to me. Neither does the multiverse concept or the anthropic principle, which purport to explain why our particular universe has certain physical parameters. These models presume that we are stuck, conceptually.

  About the current situation "Sometimes, he says, science is just plain stuck until new data, or a revolutionary idea, busts the status quo."

  The latest article at Simons is one by Natalie Wolchover, who was at the same Nima Arkani-Hamed talk I recently attended. See her take here, mine here. Will write yet again about "naturalness" and some of the content of this article in a separate posting.

- For the state of SUSY, and particle physics in general, check out recent talks here, especially Matt Reece's SUSY theory overview. I think a fair description of the current state of affairs is that the only SUSY theories standing are either "fine-tuned" (removing the main argument of LHC-scale SUSY), or highly contrived (e.g. by going beyond the MSSM in various ways to escape LHC negative results). For the latest experimental results about SUSY, watch for this CMS talk on Tuesday.
- For the latest in speculative theorizing about HEP and cosmology, see this past week's Planck 2013 conference.

Comments

1. **Tom**
   May 25, 2013

   In Wolchover's article Arkani-Hamed asserts that "naturalness has a track record", and she writes that "Time and again, whenever a constant appeared fine-tuned, as if its initial value had been magically dialed to offset other effects, physicists suspected they were missing something. They would seek and inevitably find some particle or feature that materially dialed the constant, obviating a fine-tuned cancellation." I found this very surprising because my opinion has always been that naturalness has no track record whatsoever as a useful physical principle, and when I asked people to give me an example the only thing anyone ever came up with was that you could have inferred the positron from the self-energy of the electron, if you hadn't already known about it. Does anybody know of any other cases?

2. **Shantanu**
   May 25, 2013

   Peter or others,
   has any BSM prediction or any other idea in any theoretical paper by Randall been vindicated by experiment? I don't think so, but maybe I am wrong.

3. **Peter Woit**
   May 25, 2013

   Tom,

   I've written a lot more about this in the next posting.

   Responses to Tom's question are encouraged, but please put them in the next posting.

4. **Peter Woit**
   May 25, 2013

   Shantanu,

   I don't know of any, but that's true not just of her, but of all BSM theorists.

5. **David Nataf**
   May 25, 2013

   I saw on astro-ph a month ago that JiJi Fan, Andrey Katz, Lisa Randall, Matthew Reece had an article on dark matter interactions.
   http://arxiv.org/abs/1303.1521
   http://arxiv.org/abs/1303.3271
   They did something I had never seen before … they had the standard 30 page version of their paper, and they also wrote a 5 page cole's notes version, I wonder which version will end up with more citations. A lot of people I spoke to found it funny, but, it's probably a good idea.

   I dont think the general idea of double-disc dark matter is that new, but they're probably pushing it to a more sophisticated and a more precise direction. And, as far as I can tell, that would make them among the first particle theorists to realize that the Gaia satellite has a good shot to be the highest-impact technological laboratory in the next decade in terms of putting fundamental physics forward. In my view there is a desperate need for more predictions for Gaia observations that are only a few years away, so this was a good step forward.

6. **Navneeth**

May 26, 2013

I came across the Simons Foundation/News website only recently when I found the link to the article about the recent advances regarding the Twin-prime conjecture. As a layman I thought the piece well written; not dumbed down at all (in the most pejorative sense of the term), for a popular level article.

The “Unnatural” Standard Model

May 25, 2013
Categories: Multiverse Mania

The Standard Model is a physical theory of a spectacularly successful sort. It is built on beautiful and deep mathematics, covers almost all known physical phenomena, and agrees precisely with the result of every single experiment ever done to test it. It leaves open a very small number of questions: why this specific combination of small symmetry groups and their representations? What determines the parameters of the model (18 if you ignore neutrino masses, 7 more if you include them)? What about gravity? Does it need to be extended to account for dark matter?

For several decades now, there has been a very active and heavily advertised field of “Beyond Standard Model” physics, the study of extensions of the standard model that remain consistent with experimental bounds. While BSM models have played a role in guiding experimentalists towards things to look for that are not already ruled out by what is known, they have never come anywhere near fulfilling the hope that they might provide some insight into the SM itself. They provide no explanation of the unexplained aspects of the Standard Model, instead adding a great deal of additional unexplained structure. Perhaps the simplest and most widely studied example is the minimal supersymmetric extension of the SM, which not only explains none of the 25 undetermined SM parameters, but adds more than 100 additional such parameters to the list.

Theorists have traditionally followed what has been described as “Albert Einstein’s dream that the laws of nature are sublimely beautiful, inevitable and self-contained”, and the SM is our closest approach so far to Einstein’s dream. If you shared this dream, the known BSM models would never have much appealed to you, since they just added complexity and extra unexplained parameters. You also would not have been at all surprised by the strong negative results about such models that are one of the two major achievements so far of the LHC (the other is the Higgs discovery). If you’re a follower of Einstein’s dream, the obvious reaction to the LHC results so far would be to rejoice in the vindication of this dream, welcome the triumph of the simplicity of the SM, and hope that further study of the Higgs sector will somehow provide a hint of a better idea about where the SM parameters come from (almost all of them are Higgs couplings).

Remarkably, a very different story is being sold to the public by those who had a great deal invested in now failed BSM models. In this story, the BSM models were the ones of Einstein’s dream: they were “natural”, and their failure leaves us with the “unnatural” Standard Model.

An article entitled Is Nature Unnatural? is the source of the above quote about Einstein, and it tells us that

> Decades of confounding experiments have physicists considering a startling possibility: The universe might not make sense…
>
> In peril is the notion of “naturalness,” Albert Einstein’s dream that the laws of nature are sublimely beautiful, inevitable and self-contained. Without it, physicists face the harsh prospect that those laws are just an arbitrary, messy outcome of random fluctuations in the fabric of space and time…
>
> “The universe is impossible,” said Nima Arkani-Hamed, 41, of the Institute for Advanced Study, during a recent talk at Columbia University .

What is behind this sort of claim that down is up is abuse of the English word “naturalness”, which in this particular case has been adopted by theorists to refer a technical property better described as “not quadratically sensitive to the cut-off scale”. There’s a lot to be said (and a lot that has been said on this blog) about the precise technical issue here. It’s a real one, and likely an important hint about the true nature of the Higgs sector of the SM and where all those undetermined parameters come from. Getting rid though of this technical problem by invoking hundreds of new undetermined parameters is not the sort of thing Einstein was dreaming about. He would see the LHC results as vindication and encouragement: as we investigate new energy scales we find the universe to be as simple as possible. It’s remarkable to see this great discovery being promoted as telling us that we have to give up on Einstein’s dream and adopt a pseudo-scientific research program based on the idea that physical “laws are just an arbitrary, messy outcome of random fluctuations in the fabric of space and time”.

Update: The Science News story has now appeared at Scientific American, with the title New Physics Complications Lend Support to Multiverse Hypothesis. The “New Physics Complications” are the LHC only seeing pure SM behavior. If the LHC had seen a complicated SUSY spectrum, that would have been “natural”, but somehow seeing the simplest possibility has become a new “Complication”. It is a “complication”, but a sociological not physics one. SUSY theorists do have an answer for the complication of their ideas failing: the Multiverse did it.

Comments

1. **Bernhard**
   May 25, 2013

   I agree this “The universe might not make sense…” or ““The universe is impossible,” thing from Arkani-Hamed is complete BS and has to do, to some degree, with frustration the BSM theory community (with Arkani-Hamed being the king of it) feels about the fact that the whole BSM businesses have not yet gone anywhere.

   But I think it is unfair to say ” They provide no explanation of the unexplained aspects of the Standard Model”. There are interesting BSM models that do explain things like strong CP problem, neutrino masses,quark-lepton family replication, etc in a quite conservative way and not adding too many parameters. There is really no way out, unless you are the Standard Model anything beyond it will have to add something, at least one new particle necessarily. None of those models have worked so far, but this might still change.

   One does not need to depreciate BSM physics to appreciate how spectacular the SM is. That the desperation led some people to do pseudo-science is a problem, but other people in the community tried to stick with the rules of the game.

2. **King Ray**
   May 25, 2013

   I think the things that made Einstein successful were his imagination, dogged determination and his sense of beauty and simplicity (his ‘nose,’ as he called it). BSM physicists on the whole seem to not use simplicity as a metric at all. You have to explain more things than you add to make things simpler.

3. **Igor Khavkine**
   May 25, 2013

   I’d like to second Tom’s question from a comment to Peter’s preceding post: What **is** the track record of naturalness? More precisely, when have considerations of naturalness correctly suggested the existence of previously unseen degrees of freedom, together with reasonable constraints on them, prior to their actual observation? The examples need not be restricted to high energy physics.

4. **MathPhys**
   May 25, 2013

The technical problem of quadratic dependence on the UV cut-off probably tells us something about the techniques that we use to compute radiative corrections, rather than the physical observables that we wish to compute.

5. **X**
May 25, 2013

Ignoring neutrino masses was *never* a good idea, and now that most of the parameters measured to fair precision, it's absurd to class them differently from the rest of the SM parameters. Maybe you could class Theta differently, but that might be a stretch for the standard Standard Model.

6. **Anonyrat**
May 26, 2013

Without "naturalness" the ideology of the standard model as an effective QFT evaporates. Irrelevant operators could have arbitrarily large coefficients enough to overcome the suppression by powers of the cutoff scale.

7. **Ben**
May 26, 2013

@Igor,

Arkani-Hamed himself goes into a couple of interesting historical examples of the success of naturalness arguments in a video Peter posted back in December; http://www.math.columbia.edu/~woit/wordpress/?p=5416. (link straight to video: http://online.kitp.ucsb.edu/online/higgs_m12/arkanihamed/)

If you skip to 16.40 he starts talking about the classical electron self-energy, the mass splitting between the charged and neutral pions, and very briefly mentions k-kbar mixing, though he actually starts talking about naturalness around 13.00.

8. **fuzzy**
May 26, 2013

hi peter,

i agree on the general statements, but not completely on the formulation of SM you gave. the gauge principle (and renormalizability) instructs you to write all the legitimate operators, not only those you like to include.

in the light of this principle, the SM as a gauge theory has also the theta term and the constant of the potential (cosmological). once you introduce right handed neutrinos, you do not have 7 parameters but much more.

furthermore, when you make the statement concerning "100 more parameters", you write BSM but you mean MSSM (SUSY). no problem to admit that MSSM is (was?) a very popular branch of BSM, but as a scientific concept, BSM is a much wider thing than MSSM: we better avoid confusion.

actually, i do not think it is wise to speak acrytically of BSM; we should speak of more specific things, to avoid falling again on metaphysics.

thanks!

ps i have only one question concerning the quotation: it is quite evident that the star is the chap who succeeded to explain us so clearly that "the universe is impossible," (douglas adams would be proud of him) but who is this einstein? maybe the idea was to write weinstein, and a "w" is slipped away

9. **francis**
May 26, 2013

Actually, there exist theories compatible with Einstein's dream.
For example, Connes' ncg model gives a conceptual explanation of the SM, reduces the number of free parameters and is compatible with experiment.

So IMO the hope of fulfilling Einstein's dream should not be abandoned but should be an indication to know if we are on the right track or not…

10. **Peter Woit**
May 26, 2013

fuzzy,

I pretty much agree with your comments, to make the point I wanted to make I didn't include many details which aren't relevant to the point.

There's very little one can sensibly say about all BSM models, the reason for concentrating on the MSSM is that it's the one that is most well-defined and well-studied. For instance, once one starts in with things like large extra dimensions, I have no idea how to characterize the size of the theory space one is invoking, other than that one has moved away from something highly constrained to something much, much larger.

11. **Toni**
May 26, 2013

Peter,

your last statement implies that you dislike emergence. Are you really stating that emergence is nonsense? And this despite the old argument that general relativity is a result of the thermodynamics of spacetime?

12. **Peter Woit**
May 26, 2013

Toni,

"Emergence" is just a word. I've no strong opinion about what the right fundamental variables are to describe gravitational degrees of freedom: maybe the metric is not fundamental, but "emergent". But that's a pretty empty idea unless you have some serious proposal about what really is fundamental, and what the relation is to the SM (where we do know exactly what the fundamental degrees of freedom are).

13. **fuzzy**

May 27, 2013

hi peter, thanks, but if we agree, then why to jump from MSSM to large xdim (of nima, gia and savas–again!) rather than remarking that the class of BSM theories that take care of neutrino masses have generally much less parameters, and they usually have firmer reasons of being called BSM?

moreover, i would like to insist on the old good gauge theories; they are not yet sufficiently explored! even the simple left right models — but this is true also for so(10) — need to be taken much more seriously than it was done till now.

in my view, rather than drawing rash conclusions, we need some time to think and critical attitude: i feel that the story of naturalness should have taught us that groupthink is not the way to explore the nature.

14. **Marcel van Velzen**
May 27, 2013

Recommended! Guido Altarelli presents an excellent, honest and balanced overview of the current state of naturalness here:

http://indico.cern.ch/getFile.py/access?contribId=27&sessionId=3&resId=0&materialId=slides&confId=239571

15. **imho**
May 27, 2013

From a cond matt theorist's perspective this is a pretty clear example of the limits of human understanding. In the absence of experiment, it seems progress grinds to a halt and science morphs into Philosophy with a mathematical cloak.

How did you guys and gals end up here? Why do so many unquestionably accept, what seems to me to be, unproven assumptions. Who determined that quantizing gravity is any more sensible than quantizing Navier-Stokes? Who decided that dimensional analysis should be used to define a Planck Length? Please explain to me why so many are completely certain that the only two options are multiverse or super symmetry… especially since we know absolutely nothing about 96% of the "stuff" in this universe? This whole thing smells like religions group think.

I'm on your team and I'm really struggling to see the value here. The funding agencies need to continue, and perhaps accelerate, the de-emphasis of unification research. There really shouldn't be more than a few dozen people in the US doing this kind of thing.

16. **Matt**
May 28, 2013

Okay, so now condensed-matter theorists are calling for other physicists' funding to be cut. That's exactly the kind of counterproductive feuding among scientists that doomed the SSC and eventually all high-energy experimental physics in the US altogether. Bravo!

Peter, I think this original post of yours is an incredibly important one, because I think it crystallizes (intentionally or unintentionally) a lot of your thinking on fundamental physics today.

I've been following your opinions on physics for a long time now, and what started as (justifiable) skepticism of string theory has morphed into a general bitterness toward essentially everything going on in high-energy theory today, string theory or not. It's become what I once heard called "sidewalk supervising." That's folks who stand on the opposite sidewalk shouting criticisms at the construction workers trying to repair a building. You've gone from being unhappy with one particular model of new physics (string theory) to complaining about essentially everything that tries to go beyond the Standard Model, regardless of the approach.

And yet, I have no idea what specifically you'd prefer be done instead. You seem to think that working on fundamental physics is something worthwhile to do, but apart from saying vaguely that you think more attention needs to be paid to chiral gauge symmetries (because you believe there's apparently something very deep about the fact that the internal gauge symmetries of the Standard Model know about leftness and rightness, even though it's trivial to think up simple models for which this is totally unmeaningful), there isn't much you've suggested.

In this post of yours, I think you make clear some of your motivations.

You write that "The Standard Model is a physical theory of a spectacularly successful sort." You also say that it "covers almost all known physical phenomena, and agrees precisely with the result of every single experiment ever done to test it."

Sure. But, as you well know, the Standard Model, like all realistic quantum field theories that actually describe practical physics, also predicts that it has a limited regime of validity. So its success and agreement with experiment in that regime of validity does not tell us that it should be valid outside that regime.

You say "It is built on beautiful and deep mathematics," but you know that's irrelevant. There is no metaphysical reason Nature should care about mathematical beauty at a fundamental level. Beauty may have been a nice guide to helping us guess way in advance that QFT should be the right description for low-energy physics, but you know that it cannot be a fundamental justification. Every argument originally inspired by beauty, from Dirac's argument for his eponymous equation to Einstein's argument for general relativity, has now been put on a better footing in terms of actual physical principles. We now have independent reasons for believing all those ideas — beauty may have gotten our foot in the door, but many, many beautiful ideas are wrong, and plenty of un-beautiful ideas are sometimes right.

There are very general arguments that physics consistent with locality, relativity, and quantum mechanics should look like a "beautiful" effective field theory at low energies. Maybe you find such arguments ugly or less inspirational, but inevitability is a far more important criterion than beauty for understanding the Why? of physics.

Einstein's theory of gravity is "more beautiful" and has "fewer adjustable parameters" without a cosmological constant or higher-dimension operators, but there's no physical argument they should be there — and lots of arguments that both the cosmological constant should be there (and experimental confirmation of said fact) as well as that higher-dimension operators should be present suppressed by large mass scales and thus very difficult to detect. And we already have models now in which the beautiful spacetime of general relativity is only an emergent approximation of altogether different physics, and that's an important proof of principle.

But, more importantly, the Standard Model isn't just incomplete in the sense that it predicts its own eventual demise at sufficiently high energies or doesn't explain many of its adjustable parameters or gauge groups. As you note yourself later in your post, the Standard Model doesn't account for a staggering 95% of the mass-energy in the universe whose existence we know from experiments and observations. Nor does it accommodate gravity at arbitrarily high energies in a manner fully consistent with quantum mechanics.

You go on to attack essentially all the BSM models. But people aren't proposing those models just because they are madly in love with, say, split supersymmetry of all things. There has to be physics beyond the Standard Model, and any physics beyond the Standard Model is, by definition, BSM. But the Standard Model, and, more generally, principles like relativity and quantum mechanics (which include conditions like unitarity, causality, etc.), put tremendous constraints on the allowed models one can propose. (And bounds on the validity of relativity and quantum mechanics are extremely small today, due to painstaking work by lots of people over decades.)

The people working on BSM physics are therefore operating under a very tight straightjacket. It's not easy work, and coming up with an idea that is consistent with everything we already know is very, very difficult. The vast, vast majority of ideas ("beautiful" in their mathematics or not) are simply ruled out. You may not like some of the ideas that are still in the running, but they've made it through a grueling gauntlet. They're favored because they've managed to jump through a million hoops. That was, after all, one of the reason people got so excited about string-inspired ideas in quantum gravity.

It's sort of like folks who think the idea of WIMPs as dark matter is an ugly idea, and would prefer, I don't know, modifying general relativity. Well, that just doesn't really work. People have expended a huge amount of energy on that, and it just doesn't work very well in a manner consistent with all the observations we already have and consistency with other physical principles we have exceptionally-strong bounds on.

If you think you have better ideas, then please join the fray! I know it's a cliche, but Roosevelt was right about how it's far more admirable to be a contender, to be getting one's hands dirty, than to be yelling criticisms from the stands that the contenders aren't doing it right. Unless you think physicists should simply stop trying to do fundamental physics, there is no alternative to what's going on. They're not doing BSM in ways you don't like just to piss you off — these are the only directions that have been found so far that are consistent. If you think you have a better approach, please go for it.

Because the only real alternative to trying to thread this million-dimensional needle is just to stop trying to do new high-energy theoretical physics altogether. Maybe there are people who think that's better. But pursuing fundamental physics, trying to resolve paradoxes, and trying to make seemingly inconsistent ideas work together properly has historically yielded helpful results, even if only to other fields.

Einstein was originally trying to reconcile a purely theoretical problem at the time he was trying to make gravitation consistent with relativity. If there had been no planet Mercury or a moon whose angular size in Earth's sky was the same as the sun, then experimental confirmation of his theory might well have taken decades. General relativity has been tremendously helpful to our understanding of lots and lots of physics and mathematics. We understand QFT far better because of many of the tools of general relativity.

Hawking was trying to resolve paradoxes when he ended up deriving black-hole radiation and black-hole thermodynamics, which now provide very strong constraints on proposed new physics and may give us another hint on how to proceed. We've learned a great deal about QFT in curved spacetime, and about phenomena like the Unruh effect, as a consequence.

Going back even further, we can look at the though-experiment paradoxes that presaged quantum mechanics, or Maxwell's corrections to Ampere's law that fixed up his now-eponymous equations that then led to prediction of electromagnetic radiation.

Even inflationary cosmology, which has generated actual predictions that have held up remarkably well, started out as (yes!) an attempt to solve various paradoxes with naturalness that arise in the old Big Bang model. (The horizon problem, the relic problem, the flatness problem — all are naturalness problems!)

There's a long history to show that pursuing fundamental theoretical physics for its own sake, like any kind of basic research, yields unexpected fruit, although most of the attempts end up not working. But to stop it all because of those mostly wrong directions would have been a disaster to all further progress.

Even supersymmetry and string theory have been immensely helpful in understanding QFT. Just as classical mechanics makes a lot more sense in light of quantum mechanics, and QFT sheds a lot of light on quantum mechanics, so too do ideas like supersymmetry and string theory teach us a lot about QFT. Supersymmetry has given us entirely new ways of understanding qualitative features that are otherwise intractable in simpler QFTs, confinement being a prominent example. And string theory has provided many new ways of understanding familiar QFTs as well; indeed, many realizations of string theory are now known to be dual to certain QFTs, so that the two subjects aren't as different as we once though they were. Collider physicists are literally using results originally from string theory to do far better calculations of actual events in their accelerators.

I don't know what else to say except that we have two options — either shut it all down, or do the hard work of trying to thread the needle in the ways allowed by what we already know and keep working until we finally stumble on the right answer.

What else do you propose? And please be specific!

17. **Bob Jones**
May 28, 2013

Many readers of this blog would benefit from reading this article:

http://arstechnica.com/science/2013/05/earning-a-phd-by-studying-a-theory-that-we-know-is-wrong/

18. **Mario**
May 28, 2013

@Marcel van Velzen: I cannot agree. Altarell' does not account for the conformal standard model and consider seriously anthropic ideas. I think that after this paper http://prl.aps.org/abstract/PRL/v110/i15/e151601 this approach should be considered a serious and viable way out to the question of naturalness.

19. **Marcel van Velzen**
May 28, 2013

@Matt,

Nice piece! There's a lot of truth in it. People should continue to work on BSM physics but should also realize what they are up against and don't make misguiding claims because it's here that the problem begins. To be specific:
"Hawking was trying to resolve paradoxes when he ended up deriving black-hole radiation and black-hole thermodynamics, which now provide very strong constraints on proposed new physics and may give us another hint on how to proceed. We've learned a great deal about QFT in curved spacetime, and about phenomena like the Unruh effect, as a consequence."

Although the mathematics is really nice and convincing Hawking radiation and the Unruh effect have never been observed so it should be that MAYBE we've learned a great deal about QFT in curved spacetime and to say that they provide very strong constraints on proposed new physics is also unclear and the hint they give us to proceed may be totally wrong. An exploding black hole has still to be found.

20. **Igor Khavkine**
May 28, 2013

@Matt, what you've written about keeping theories that have passed stringent empirical tests, be they "ugly" or "beautiful", is great and I agree with that wholeheartedly. But, some of the motivations that you list for going beyond the Standard Model are just wrong. I mean "wrong" in the sense that they are based on very shaky arguments. These include: (a) the Standard Model is *internally* inconsistent in some regime, (b) taking GR together with the Standard Model is inconsistent with quantum mechanics, (c) that hierarchy is a problem (or more generally that measured values of free Lagrangian parameters require an "explanation"), (d) that black hole entropy requires a statistical explanation. In the same breath, you've also pointed out real problems that do need to be solved for us to have a better working model of the universe: (a') dark matter is not included in SM + GR, (b') whatever quantum field that seeded primordial fluctuations (say "the inflaton") is not included in SM + GR.

To be fair, you didn't actually say (d), but that is what commonly stands behind the code phrase that "the Hawking effect gives us a hint about quantum gravity". Also, certainly, my opinion will likely not align 100% with Peter's, but since you asked for some specific suggestions, here are some. What not to do: try to solve problems (a') and (b') where there is actual data available to constrain possibilities by simultaneously trying to solve (a), (b), (c) and (d). The latter four are red herrings that prompt people to take uncontrolled flights of fancy into theory space. And it is these flights of fancy I believe attract most criticism, like Peter's. What to do: try to solve problems (a') and (b') by starting with SM + GR and using conservative effective field theory methods, constrained by the available data, to nail down the quantum numbers and interactions of the dark matter and inflaton fields. Actually, from what I've seen, there are some hard working cosmologists and particle theorists doing exactly that. So, your challenge of putting these guidelines in practice, is already being met, albeit by what it seems like a minority in these fields.

So, your proposed dichotomy "either shut it all down, or do the hard work of trying to thread the needle in the ways allowed by what we already know and keep working" is in a way false. A third alternative is to take a more careful look at "what we already know" and keep working on solving empirically constrained problems and not just aesthetic prejudice.

Just to be clear, here's what I mean about (a)-(d) being wrong. (a) & (b) Formal perturbative calculations and lattice methods (where available) do the job just fine and consistently. If you think not, say what cannot be computed using the GR + SM Lagrangian, dimensional regularization, and the renormalization condition that sets to zero all Lagrangian parameters that are empirically unconstrained away from zero. Any non-perturbative statement about consistency is only at the level of conjecture at the moment. (c) Almost precisely the topic of this blog post. (d) Hawking's calculation is its own explanation.

21. **Peter Shor**
May 28, 2013

Matt: you say "If you think you have better ideas, then please join the fray!"
Alain Connes has tried to do precisely that with his non-commutative geometry. He has been completely ignored by the physics community. It may be that his ideas don't work (this wouldn't be the first time a Fields-medal-winning mathematician has been wrong about things), but I have not found anyone who is able to explain why they don't work. I would love to hear a good explanation for why these ideas have been ignored.
A similar thing has happened with the idea (I believe due to Kitaev or Preskill) that physics at its lowest level might not be unitary, but that some kind of quantum error correction makes it unitary for interactions above the Planck scale. Nobody seems willing to consider this. Several of the high energy physicists I've mentioned this to dismiss it by bringing up a paper "proving" that if the universe were not unitary, it would have detectable consequences.When this paper was written, quantum error correction was a completely unknown phenomenon, and the paper does not hold up in the light of the existence of quantum error correction.

22. **Peter Woit**
May 28, 2013

Matt,

No time now for a long response to your comment. I'm heading downtown to the start of a week of talks at CUNY in honor of Jim Simons. The morning ones for the next few days are all ones about QFT at the boundary of math and physics, and I'm very much looking forward to them. There's a lot of interesting things going on there, unfortunately for physicists this is going on more and more in math departments, not physics departments.

You should think more seriously about what "imho" has to say. I think the "I'm on your team" is honest. What do you expect other physicists to think when they see publicity campaigns from the most influential people in HEP theory for multiverse nonsense? People like "imho" are the ones deciding whether or not to hire HEP theorists, and even the most sympathetic ones are getting the idea that HEP theory is a disaster area when they see what is being promoted as the cutting edge of the subject. People who have been working on TeV-scale SUSY for decades are now facing a big challenge of how to deal with failure. If they deal with it by arguing that this shows we're in a multiverse where nothing can be done, they're slitting their own throats as well as taking down the rest of the field with them. I'm not seeing anyone influential other than David Gross willing to stand up to this publicly. Yes, I undoubtedly spend more time arguing about this than is sane, and all it has gotten me is having links from the arXiv to my blog banned for saying unpleasant things about what powerful people are doing. There are a lot of people out there who should be pushing back against Arkani-Hamed et al's multiverse campaign. They're not earning their paychecks.

Personally, I'm spending this summer trying to finish writing up the material I was teaching this past year, including a lot about basic ideas of QFT from the point of view of representation theory. Hopefully by the end of the summer there will be good draft of a 300 page or so book. I've learned a lot, hope to then get back to a list of ideas I find promising, make more progress on them, given what I've learned over the last year.

"But, as you well know, the Standard Model, like all realistic quantum field theories that actually describe practical physics, also predicts that it has a limited regime of validity. So its success and agreement with experiment in that regime of validity does not tell us that it should be valid outside that regime."

A long topic, but I really don't agree with this at all. Asymptotically free gauge theories don't have limited ranges of validity, and I don't understand the point of ignoring this remarkable fact. The LHC Higgs results seem to show a Higgs sector that makes sense to very high energies. There's serious evidence to make one wonder about the whole philosophy that "the SM is just an effective theory, a more fundamental one is some much more complicated mess".

23. **Matt**
May 28, 2013

I suppose I had assumed that given the stridency and longevity of your opposition, there would be more rigorous substance to your position. There is a mismatch between the certainty with which you seem to be manning the barricades and the magnitude of your evidence.

Suffice it to say that whatever is the case for, say, QCD alone, the Standard Model as a whole is not asymptotically free. No one is "ignoring this remarkable fact" — it's just not relevant to the Standard Model. Maybe it'll be true of some as-yet-unconfirmed BSM model. And where people have actually found UV-complete QFTs equivalent to quantum gravity, like AdS/CFT, you've balked.

You write "The LHC Higgs results seem to show a Higgs sector that makes sense to very high energies." How do we know this? What does "very high" mean? Does it mean "arbitrarily high" or not?

You say "There's serious evidence to make one wonder about the whole philosophy that "the SM is just an effective theory, a more fundamental one is some much more complicated mess"." What evidence?

And going from QED to the Standard Model meant adding lots of new parameters, so there's literally no reason to assume that going beyond the Standard Model is going to mean reducing the number of parameters, either. Don't get me wrong — it would be nice! But why should Nature care? The progress of physics thus far doesn't seem to have involved a reduction in parameters, so why should we expect it to?

It is irresponsible for members of the high-energy theory community to be saying outlandish things in public. But that's a different crime, and shouldn't be conflated with the fact of the matter about whether they're working on the "right" things nowadays in their actual research. Like I said, any new idea that jumps through all the hoops would be a major breakthrough, especially for young theorists who'd rather work on something new than models invented decades ago (but that are the only things still standing).

And you still haven't provided a positive agenda here. A positive agenda, not just criticism! Maybe after your textbook is done.

As for everyone else, it doesn't count to say "My pet theory is being ignored by the physics community!" If an idea isn't well-developed, violates trusted principles, is extremely mathematically inaccessible, and/or doesn't seem to jump through all the necessary hoops, then the onus is on the creator to do the legwork of making his or her case to the larger community. Expecting the physics community to do the work for you is naive, to say the least. The vast, vast majority of new ideas are incapable of getting beyond the blueprint stage.

24. **King Ray**
May 28, 2013

Peter,

Your comment

"People who have been working on TeV-scale SUSY for decades are now facing a big challenge of how to deal with failure. If they deal with it by arguing that this shows we're in a multiverse where nothing can be done, they're slitting their own throats as well as taking down the rest of the field with them."

reminds me of this quote by Seneca:

"It is sweet to draw the world down with you when you are perishing."
Lucius Annaeus Seneca, Roman stoic philosopher, 4 BC –AD 65

25. **fuzzy**
May 28, 2013

dear matt,

two remarks:

1) it seems to me a bit preposterous to speak of "crime". could you tell me since when expressing views (possibly critical views) has became a crime?

2) you offer very general remarks to "everyone else". thanks for the consideration, but i urge you to note the fact that, above, i spoke of gauge theories: do you consider it fair labeling the whole class of gauge theories as "my pet theory"?

note that this is exactly what altarelli is trying to revamp, as in the web site pointed out by marcel v.v.. in other words, it looks to me an item worthwhile including in your "positive agenda".

(but maybe you want to discuss only with your Peers, not with some fuzzy guy; in this case i understand your position)

best regards.

26. **ohwilleke**
May 28, 2013

* It is hard not to sympathize with Einstein. How many physicists really think in their heart of hearts that the couple dozen parameters of the Standard Model and its SU(3)*SU(2)*U(1) structure are just arbitrary? I have yet to meet anyone who has thought seriously about the matter who isn't convinced that there is a deeper theory that explains what we know through the SM and GR more elegantly and patches up the remaining holes in fundamental physics. What are we missing that makes a formulation of theoretically consistent GR and SM theories impossible? We have an elephant in the room called dark matter that is tired of living in the Harry Potter room under the stairs that cries out for an explanation. We are so close, and yet so far.
* The "unnaturalness" of the Standard Model is surely a product of the ignorance of the beholder since, of course, it is Nature, however much of a bitch she may be, and decades of meta-theory efforts to define naturalness operationally have clearly missed the point because they are in some way looking at the numbers from a perspective that obscures the missing links that make them natural.
* The new paradigm really ought to be not BSM but "within the Standard Model" theories that seek to think about "why" the parameters take the values that they do and the equations take the form that they do in a more elegant way.
* Efforts to link the "texture" of the mass matrix with the values of the CKM and PMNS matrixes, and phenomenological formulas like Koide's formula and its extensions most directly embrace this WSM approach. But, until we can get a lot more precision in our values for the quark masses is will be hard to distinguish candidate theories; the percentage uncertainties in the lighter quark masses are huge and the theories used to estimate them are still pretty crude so it is too easy to find theories that fit the data. Perhaps improved determination of the Higgs boson Yukawas can move this ball forward by indirectly measuring non-top quark masses via coupling constants that can be discerned with rare decay product production rates.
* While dark energy can be explained as a simple cosmological constant parameter in GR for gravity, we've got to explain dark matter somehow and increasingly it looks like that explanation will be almost completely independent of the Standard Model with the possible exception of a dark matter particle interactions with the Higgs field. (Some recent work on graviweak unification like arxiv 1212.5246 "Gravitational origin of the weak interaction's chirality" Stephon Alexander, Antonino Marciano, Lee Smolin (20 Dec 2012), for example looks promising but simply coming up for air and looking for alternatives completely outside the SUSY paradigm seems like a good place to start.)
* Perhaps what the theoretical physics world needs at this point is one huge "Not SUSY" conference to remind people that there are other approaches out there and to give discourages former career SUSY theorists new research directions to explore.

27. **Tue Sorensen**
May 28, 2013

Peter, just want to say I enjoy your blog a lot, and agree with virtually everything you say. Keep it up!

28. **Brathmore**
May 28, 2013

In response to Matt: "If an idea isn't well-developed, violates trusted principles, is extremely mathematically inaccessible, and/or doesn't seem to jump through all the necessary hoops, then the onus is on the creator to do the legwork of making his or her case to the larger community. Expecting the physics community to do the work for you is naive, to say the least. The vast, vast majority of new ideas are incapable of getting beyond the blueprint stage."

While I agree with this statement, Matt, I think you are attacking a straw man and missing a larger point made on this blog over, and over, and over again: sociological constraints/pathologies prevent physicists (especially young, non-tenured ones) from working on otherwise great ideas that are off the beaten track. If I've understood some of Peter's arguments correctly, a big problem is that leaders in the field have created a situation where working on alternatives to string theory/multiverse mania is detrimental to one's career.

The picture you paint of theorists objectively evaluating different approaches solely on their merit is arguably not happening. Instead, the argument is that young theorists pursue string/multiverse ideas due to career pressures and sociological constraints. The refrain you hear from the "leaders" in the field over and over again that strings/multiverse are "the only game in town" exemplifies this. The leaders in the field have devoted their careers to a failed research program, and are unwilling to admit defeat. They're not disinterested observers seeking the "best theory" – they're highly biased towards string theory and have channeled the community toward advancing it, at the expense of other good ideas. Through their positions of influence, they have biased funding, hiring, and promotion critera towards string theory far beyond considerations of merit.

Do you really think that Connes' ideas are being ignored because this fields medalist overlooked something that every 3rd year graduate student recognizes? That the entire community has evaluated his ideas and decided they can't lead anywhere? See Peter Shor's comment about the fact that no one can give him a good explanation for why Connes' ideas aren't being pursued. How come **nobody** at the IAS, Harvard, etc. is working on alternatives to string theory? Why aren't new PhDs at American universities writing dissertations about Connes' ideas?

As a frequent reader of this blog, I cannot agree with your argument that this blog is against all BSM physics. His posts on LQG, Lisi's recent ideas, and Weinstein's latest lecture don't seem outright hostile to me. Quite the contrary – I believe he's argued for a long time that diversity in these ideas should be encouraged.

No, the argument made frequently on this blog is that the theoretical string community has a sickness. It rejects attempts to pursue competing approaches, not based on objective considerations, but on dogma. Further, practioners in the field frequently tout string theory's successes in ways that are misleading or outright false. When string theory doesn't pan out, they argue that the rules of science should be changed, that the accelerator energies aren't quite high enough…or that their ideas DID pan out! Three cheers for all the recent experimental verifications of string theory.

So yes – a person proposing a novel BSM surely has a great responsibility in ensuring that their idea passes various tests, but the community also has a responsibility to evaluate them – not to ignore promising ideas due to dogma or sociological constraints. It is here that the community is failing, and attacking the messenger (Peter) in no way changes that fact.

29. **Peter Woit**
May 28, 2013

Matt,

Maybe best to stick to the main issue here: what progress in HEP has the "SM is just an low energy effective theory for something much more complicated" conjecture led to? Why don't we see anything at all in terms of effects of operators you would expect to see if the SM is just an effective theory? You say the conjecture tells us where the SM breaks down. Where is that? Does this say we will see something non-SM at the LHC? If we see nothing non-SM at the LHC, what does that mean? Do you accept the argument being made that this means the SM is "unnatural" so it is hopeless to explain its features we don't understand? What would falsify the "SM is just an effective theory" conjecture?

30. **Jesper**
May 28, 2013

I agree with those who bring up Connes' work here.

I don't think that its a valid objection that the mathematics Connes is applying/developing is difficult (I think that "extremely mathematically inaccessible" is an exaggeration): isn't it rather conceivable that what theoretical physics needs now is a complete change in perspective and that such a change might very well involve new mathematics? And by way, people like Thomas Schucker have done much work to make the noncommutative framework of Connes' more accessible to physicists.

31. **A.J.**
May 28, 2013

Regarding Connes & Chamseddine's ideas:

The biggest obstacle to attracting more people to work on this stuff is that C&C predicted a Higgs at ~180 GeV. At the time, it was claimed that this was a make-or-break prediction. (Maybe things have changed there; I don't know.)

A secondary problem is that it's difficult for an outsider to tell whether the spectral action principle is actually a deep physical principle or just a clever notation in which nearly any QFT can be expressed. It's an appealing idea and C&C have claimed that the non-commutative geometries which can reduce to the Standard Model are highly constrained. But it takes more expertise than I have to tell what assumptions they are really making. No-go theorems in QFT tend to have loopholes you can drive a bus through.

32. **Matt**
May 28, 2013

Peter,

"What would falsify the "SM is just an effective theory" conjecture?"

I suppose one attitude is just to declare that the Standard Model isn't effective, that it's UV complete, which is essentially the same thing as calling off work on new high-energy physics. Unfortunately, the couplings of the Standard Model run, and there's a Landau pole. On a related point, eventually scattering processes computed using the Standard Model violate unitarity.

As for Brathmore, all I can say is, yes, lots of people who should know better make mistakes that a very good 3rd-year grad student in theoretical physics wouldn't make, including plenty of mathematicians. LQG doesn't work, even at the minimal level of being consistent with everything we know about quantum mechanics and relativity, and Lisi's ideas were obviously inconsistent at the very beginning with lots of what we already know. (See, among other people, Distler's blog on that. But more simply, Lisi just disregards various no-go theorems like Coleman-Mandula without even aknowledging their existence. That's a totally unserious proposal — if you're going to violate a no-go theorem, you have to explain which assumptions you've evaded.)

You claim that top people are pushing faddish ideas on young grad students for sociological rather than practical reasons (the practical reasons being that there aren't any other ideas that don't clearly violate the bounds we know with great certainty).

That's a strong and strident claim, and maligns a lot of people who are working very hard these days. Prove it. Show me some actual, valid, believable directions that are being squashed. Not "pet theories" that the community is ignoring, mind you, or ideas that simply don't work, or are so abstruse and convoluted that their creator has a reasonable duty to make the case with specific details. Actual solid ideas that thread the million-dimensional needle, that have relativistic spacetime and quantum mechanics and can accommodate the chirality and gauge groups of the Standard Model, but extend the Standard Model to higher energy scales. (If at all possible, be sure they somehow also include dark matter and gravity in them. But that's only for bonus points.)

It's easy to declare that it's all sociology, but I claim it's just the nature of the subject itself. A young theorist with a bold idea that actually works and threads the needle would be the hottest thing on the scene, and everyone knows it.

It's also easy just to demand that people working on all this stuff throw everything away they've been working on and stare at a blank sheet of paper, or a sheet of paper with, I don't know, the Standard Model Lagrangian on it. Maybe shout epithets at it and demand a new idea that looks beautiful or something. I don't have to tell you that doesn't work. Coming up with that idea consistent with all the constraints is really, really hard, especially if the demand is to do it from scratch rather than start from BSM models that already thread the needle, which is ultimately the primary reason (not sociology) for why most people take that route.

That's one of those things that people outside high-energy theory tend to underappreciate, just how stringent and manifold the constraints are.

And in the few instances where people have actually been lucky enough to find models that actually work completely and consistently with all the constraints, they've essentially all turned out to be dual to stuff we already had. Not just the five string theories to each other, or 11d supergravity, but even things like string theory to certain QFTs or various QFTs in various dimensions with each other. That only makes things even harder, because the idea has to be not only consistent with all the constraints, but also not secretly dual to models already being worked on.

I think I've made my point, and made my challenge to you all clear — namely, for an actual, concrete, positive agenda, rather than just free-floating negativity from the peanut gallery. What you decide to do with it is entirely up to you. At this point, I think I'll take my answer off the air.

33. **Peter Woit**
May 28, 2013

Matt,

While you've done an excellent job of responding in detail to Brathmore, you answered none of my questions at all.

34. **Jesper**
May 28, 2013

The failed Higgs prediction of Connes' was perhaps sold too hard – if you read his recent papers the Higgs mass is not a problem for his approach.

To me, the interesting point about Connes' work is that the entire SM couple to GR is formulated as a single gravitational theory – with the entire gauge sector arising through inner automorphisms (including the Higgs sector). So probably Einstein would have been thrilled by this. There are challenges, of course, but I think its a very interesting change of perspective that deserves attention. The fact that the SM fits into this framework is not trivial.

35. **Matt**
May 28, 2013

I did answer a couple of your questions already about why we think the Standard Model is only an effective theory, although the deeper reasons are just that the old ways of thinking about the origin of QFTs (magically "quantizing" a given classical field theory and then sticking in some sort of cutoff by hand to hide infinities or something) don't make any physical sense from a fundamental point of view, whereas recognizing that QFTs inevitably describe the low-lying spectrum of states of a relativistic quantum theory with local interactions actually does make physical sense and explains why it all worked to begin with.

I mean, if you discovered that the sound you hear at night isn't actually space aliens but is just crickets, why would you continue to believe it's space aliens, just because that idea seems more elegant to you?

And surely you recognize that effective field theory is a powerful technique. Among countless examples, it works for low-energy nuclear physics very well (see the pion Lagrangian), you can use it to compute low-energy quantum corrections to gravity, it works great for making model-independent predictions in cosmic inflation, and you can even use it for fun stuff like estimating the Rayleigh scattering effect in our atmosphere.

But, most importantly, if your given model doesn't work at arbitrarily high energies (running couplings, Landau poles, unitarity violations, what have you), then it must be an effective theory. QED is the most obvious example. So was the Fermi theory, and also the theory of massive intermediate vector bosons.

But let me answer some more of your questions: "Why don't we see anything at all in terms of effects of operators you would expect to see if the SM is just an effective theory? You say the conjecture tells us where the SM breaks down. Where is that?"

Surely you know the list, which is long, but probably the most obvious pieces of experimental evidence for the insufficiency of the operators already in the Standard Model are neutrino masses and the wrong amount of CP violation.

Now please answer my own questions, concretely and specifically. Again, I'd prefer to take my answers off the air. Thank you for your time.

36. **Brathmore**
May 28, 2013

In response to Matt: "Actual solid ideas that thread the million-dimensional needle, that have relativistic spacetime and quantum mechanics and can accommodate the chirality and gauge groups of the Standard Model, but extend the Standard Model to higher energy scales."

Matt,

You have a lot of points that seem valid to me. However, there certainly are areas where I disagree with you. Beyond the posts we've already exchanged, I would add that you seem to be applying a **much higher and more unrealistic** bar to non-string theory proposals than to string theory itself. It's my understanding that thousands (perhaps ten of thousands) of papers have been written in string theory by perhaps as many researchers – and the theory is still far from complete or finished!

That doesn't bother me, mind you – but now you seem to be saying that to even get a hearing, a person essentially has to provide a final theory that already threads this "million-dimensional needle." Is this standard the same that has been applied to string theory? (Perhaps the needle should have included a few extra dimensions, e.g., 10^500?).

It seems to me that string theory grew as a field as more and more intelligent people thought about the issues in it. To expect a single person such as Lisi or Connes to invent a fully developed, alternative BEFORE receiving input from the lager community is simply preposterous. That there are flaws in their work should be expected, as they haven't had the benefit of having dozens or hundreds of other people helping to refine their ideas. The physics community should exhibit some more openness about pursuing non-stringy ideas.

37. **Anonyrat**
May 28, 2013

IMO, Brathmore is right, super string theory would not have gotten off the ground with the standards Matt seems to be imposing on new ideas for them to be worthy of consideration.

38. **Peter Woit**
May 28, 2013

Matt,
It's been a busy day here and you've written some long comments, hard for me to know where to start in terms of responding to them. Happy to try and discuss specific questions, it would be helpful if you could point me to ones that you feel are important and not getting answered. I'm not understanding your reference to "off the air".

To respond to your latest comment: obviously the SM has problems, I just don't see evidence that this means it needs to be conceptualized as just an effective theory. It seems equally plausible that it's a truly fundamental theory, with some aspects that we don't understand. More precisely, the deep nature of the mathematics of quantized gauge and spinor fields, together with asymptotic freedom in the non-abelian case to me means that a large part of the theory is both fantastic mathematics + rigorously well-defined at all distance scales, with virtually no free parameters. It doesn't get better than that, in particular I can't see a sensible proposal for something "more fundamental" that does better.

Yes, the U(1) gauge field behavior is a problem. The Higgs sector is a big problem: it's both a non-geometric part of the theory, not well-defined at very short distances, and responsible for most of the free parameters. Sure, quite possibly the Higgs field is an effective field describing something much more interesting we don't yet understand. I still haven't given up hope that detailed studies of the Higgs sector in my lifetime will provide some solid evidence of non-SM behavior there.

About the more general issue here though which most concerns me: I'm not arguing that there's no point to BSM studies. To be used effectively, the LHC experiments need guidance about what signals to look for that don't violate everything we already know. But I still think it's a fact that there just aren't BSM models that make one say: "yes, that's a convincing explanation about something mysterious about the SM". Put differently, I don't think there are any models out there that it would make sense to put any money on unless people give you outrageously long odds. About SUSY, it just seems to me to be a very unconvincing class of models that has gotten way, way, way too much attention. Yes, experiments should look for SUSY, along with anything else people can think of, but it should also be acknowledged that this is an unpromising long shot.

About the "Naturalness or Multiverse" campaign, surely I've said enough….

39. **Matt**
May 28, 2013

I simply cannot help myself here. In trying to disprove my point, you've actually picked an example that exemplifies it. I am imposing the same standards on string theory as anything else. I am not imposing "much higher and more unrealistic" bar to non-string proposals.

First of all, it took a long time for string theory to get off the ground. Please read the history books. A small number of people worked on it very hard for a number of years (Schwarz, etc.) despite a lot of skepticism (Feynman hated it, and Murray Gell-Mann kept it alive for a while through his influence). This small group showed that it was consistent with quantum mechanics, relativity, and could accommodate all the known features of the Standard Model — not to mention finally including gravity and unification more generally. And, even then, the larger community basically didn't care until the first superstring revolution in 1984 showed that string theory, beyond all that, also seemed to promise some uniqueness because of anomaly cancellation.

String theory is the very ideal model of how to get a new idea off the ground — work on it, make sure it gets through the hoops, and then the community will be willing to look at it seriously. There are no double-standards here.

The trouble with string theory has never been that it doesn't thread the needle, but that it doesn't uniquely predict the Standard Model. The early hopes (including from all the dualities) that it would give unique predictions didn't work out.

The main trouble is that it doesn't appear to have a unique low-energy solution. (Of course, any theory at the Planck scale is going to suffer from the problem that when you cool things down to present-day energies, there may be a nontrivial spectrum of inequivalent low-energy solutions. The Standard Model is no exception, although the low-energy solutions are molecules and humans, because the overall energy scales are much lower than the Planck scale and don't involve gravity.)

That's the problem with string theory. But string theory has nonetheless served as a huge testing ground for ideas for the simple reason that spin-off ideas generated by string theory are more likely than models plucked from thin air to be consistent with all the necessary constraints. That's why so many BSM ideas originated as spin-offs from string theory, although many do not depend on string theory being the correct theory of Nature.

The same standards apply to all the other ideas, too — uniqueness isn't even one of the conditions, any more than it is for string theory. (Although, like I said, the possible appearance of uniqueness was a big deal in the early days of string theory.)

If you have a better idea, then work on it and show that it goes through all the hoops. There are a zillions potential bad ideas that just don't work, so the onus is on you — don't expect everyone else to drop their work and do your homework for you, and then, when they don't comply, start complaining that your pet theory is being "ignored by the establishment."

40. **Matt**
May 28, 2013

That last comment wasn't to Peter, but to the other two posters before his.

Peter,

I appreciate your taking the time to comment.

You write "I just don't see evidence that this means it needs to be conceptualized as just an effective theory." Being an approximation that doesn't accommodate observed experimental evidence is what it means to say a theory is only an effective theory. Obviously it needs some sort of modification. But there are no renormalizable operators that are allowed by the theory that we can add to accommodate these unexplained experimentally-observed phenomena, so we can either add higher-dimension operators or devise some new high-energy theory for which the Standard Model is only an effective low-energy theory — and both approaches are equivalent, as you surely know.

Then you write "It seems equally plausible that it's a truly fundamental theory, with some aspects that we don't understand. More precisely, the deep nature of the mathematics of quantized gauge and spinor fields, together with asymptotic freedom in the non-abelian case to me means that a large part of the theory is both fantastic mathematics + rigorously well-defined at all distance scales, with virtually no free parameters. It doesn't get better than that, in particular I can't see a sensible proposal for something "more fundamental" that does better."

I'm rather surprised to hear you say that being "fantastic mathematics" is a valid criterion for good physics. And saying that "part" of a theory is rigorously-defined at all distance scales (presumably the parts like QCD that are asymptotically free) doesn't help, because there are also parts of the theory that are not rigorously-defined at all distance scales. And, besides, QCD is rigorously defined at all distance scales for what we now understand are some fairly prosaic reasons — vacuum magnetization wins over vacuum polarization due to the number of colors versus the number of flavors. It's a delicate cancellation between magnetic and electric effects that depends sensitively on the number of gluons. If the number of flavors were slightly higher compared to the number of colors, then all of a sudden the theory isn't rigorously defined at all distance scales.

You write that the Higgs mechanism is bad in part because it is a "non-geometric part of the theory." Again, I'm not sure why we should demand that Nature must be geometric, or require it as part of a valid scientific theory. Principles like quantum mechanics and relativity are known empirically, and to astonishing precision, and they place very strong constraints on valid scientific theories. Imposing geometry is just human prejudice, no better than saying string theory is right because it's mathematics is also beautiful.

There are deeper reasons why geometric (and non-geometric) mathematical structures had to appear in Nature at the levels where they did, reasons that no longer involve simply demanding that Nature be geometric. And, as a proof of concept, we now have lots of extensions like AdS/CFT in which geometry is an emergent property that has no meaning at a fundamental level and can dissolve in certain phases of the theory.

It's nice that many models in physics have a geometric description — that makes it easier to use our intuition and various mathematical techniques, but it's asking a lot to demand that Nature respect our love for geometry as some sort of additional fundamental principle. There's no numerical, high-precision "geometric constraint" provided by experiment.

Next, you say "But I still think it's a fact that there just aren't BSM models that make one say: "yes, that's a convincing explanation about something mysterious about the SM". Put differently, I don't think there are any models out there that it would make sense to put any money on unless people give you outrageously long odds."

I agree. So would most people actually working on this stuff rather than making showy public pronouncements, which I agree with you are not helpful. But these are the models we have now, and people are studying them to learn and are also looking hard for more. My larger point is very simple — models that work are very, very hard to find, and simply yelling at physicists for not finding models that you like is not helpful.

I've noticed that you have only two papers on the arXiv — one on representation theory for QFT, and one on why string theory isn't any good. You would be serving physics far better to start looking for models yourself and putting your own positive ideas and a positive agenda out there than complaining that the rest of physicists aren't doing things the way you want them to or working fast enough for you to find new models that actually thread the needle. Write some papers, give talks, make some contributions. See for yourself how hard it is to make progress given all these constraints. Certainly that would win you some credibility.

41. **Peter Woit**
May 28, 2013

Matt,

Absent better experimental guidance, personally I think mathematics is one of the few available places to look for inspiration. Others may prefer naturalness and effective field theory ideas. Best if different people try different things. My only point is that if pursuing your favorite ideas leads you to a dead end like the multiverse, you should admit it's a dead end and try something else, not go on a campaign to convince everyone else that your dead end is the only possibility.

I'm well aware how difficult progress is at this point, the things that look most promising to me are far from easy. Maybe some day I'll get somewhere with them, maybe not. But if I don't I won't try and convince others to give up because I reached a dead end.

42. **Matt**
May 29, 2013

Peter,

That's a very gracious and reasonable reply. "Best if different people try different things" — I couldn't agree with you more. "But if I don't I won't try and convince others to give up because I reached a dead end." Thank you for that.

For what it's worth, I don't think everyone is saying the multiverse is the only possibility — although some are. But I know a lot who accept it only unwillingly — only through gritted teeth — and would happily jump at the chance for another idea that works. That's not how fads work.

But the multiverse is unfortunately a possibility. Ideally we'll find better ones, but we can't just snap our fingers and make a more palatable idea appear. Nature may well work this way, regardless of human preferences. If that's the case, then at best we can extract some kind of statistical predictions or something. But maybe not. That would be a tremendous tragedy, but Nature doesn't care. We can't just stamp our feet and make demands. Nature doesn't give us guarantees.

Einstein once said, back in 1921, "Raffiniert ist der Herrgott, aber boshaft ist er nicht" — "Subtle is the Lord, but malicious He is not." When he was asked about this remark later in his career, though, he apparently replied "I have second thoughts. Maybe God is malicious."

Ultimately, I think we're all hoping for the same thing. Experimental data is coming in agonizingly slowly, especially anything that's outside the regime of validity of the Standard Model, and people are really doing the best that they can with what's available. As you note, it's not easy, sources of inspiration are valuable wherever we can find them, and people are putting their lives into their work.

I just wish sometimes that we could all tone down the negativity toward each other. It's hard enough struggling to come up with workable ideas, especially for young physicists, without having prominent people (on various sides) bashing people's hard work and impugning their motives in front of a public that doesn't know the difference between one area of physics and another, or even between experiment and theory for that matter, and who frankly think that most physicists are just coming up with crazy ideas without any constraints at all. To a young physicist, I'm sure the impugning of motives and the calls (even if only indirectly) to cut funding comes across as downright mean.

I also fear it's self-defeating to the whole enterprise in the end. Sniping between physicists contributed to the failure to build the SSC, and probably next-generation accelerators in the US that would have come after it, and the result is a lot less experimental data, which hurts everyone in the field. And it also turns off a lot of young people, some of whom might be the ones to find the new ideas we need.

There's a place for criticism, but, ultimately, it's far, far more productive and beneficial in every conceivable way to go out and do the grueling task of finding better ideas that actually work. At the very least, that buys a person a lot of credibility when it's time for more criticism.

It's a big universe, whether it's a multiverse or not. There's room for all of us in it.

43. **amused**
May 29, 2013

Matt:

Most of the issues you raised were already beaten to death during the great string theory punch-up a few years ago – see the ST-related posts on the cosmic variance and asymptotia blogs. But since it seems you are from a younger generation and missed the fun of all that, let me regurgitate a bit of it for you…

"You claim that top people are pushing faddish ideas on young grad students for sociological rather than practical reasons […] That's a strong and strident claim, and maligns a lot of people who are working very hard these days. Prove it. Show me some actual, valid, believable directions that are being squashed."

Well, would you consider the problem of non-perturbative formulation of chiral gauge theories on the lattice to be a worthwhile direction? It would be nice to know if the gauged chiral symmetry of the SM really is spontaneously broken (or, equivalently, if the Higgs field really does have a nonzero expectation value), don't you think? Instead of just assuming that it is and then setting up perturbation theory based on that assumption. But then you will need a nonperturbative formulation of the SM to find out… And it would be nice to know at what temperature (if any) the gauged chiral symmetry is restored too, right? Presumably that would be relevant for models of the evolution of the universe… You will need a nonpertubative formulation for that too.

So here is a question for you: if a young person was naive and foolish enough work on the above, and sought to document his/her progress through single-author publications in PRL, how many such publications would it take for him to balance, in the competition for jobs, one publication in, say, PRD, by a another young person jointly with his famous advisor and a bunch of other more senior and prominent people in the area of string/multiverse/bsm? Don't be wishy-washy now, give us a number.

And since you asked for it, here are a random couple of examples of "being squashed": this guy and this guy.
(Neither of them are me or people I know, just random examples I happened to notice, and there are plenty more.) Check out their impactful (highly cited) and independent research track records, then look up their career outcomes and compare with the outcomes for typical members of the string theory group at this prominent institution (for example). From that it will be clear what things really matter and don't matter for career advancement in this business.

When I was a young person starting out in this business i naively though the situation re career advancement was like this: We all place our bets by what we decide to work on, and then the onus is on each of us to show that our bets are working out. Those who are able to make progress on problems that are considered important (as judged, e.g., by the editors and referees of PRL) will be rewarded, and those who don't won't. Turns out I couldn't have been more wrong about that.

Since you seem to be a newcomer to these debates let me answer in advance the usual protest from the ST/BSM crowd to the points above: "But our topic is so DEEP and DIFFICULT so we need to be given a dispensation and not held to the same standards of documenting important progress as the rest of the community. No one but us is qualified to evaluate our work, and our wonderful results are far too technical to explain in a PRL article." Fine, ok then, instead of PRL you may instead use that prominent string theory journal the Op-Ed Section of the NY Times.

"I've noticed that you have only two papers on the arXiv…"

Disparaging someones publication record behind the veil of anonymity is rather bad form. Is your own research activity and publications impeccable and above any criticism? Any single-author PRLs recently? 😉

I suggest you reflect on why you decided to write a comment here in the first place. If it was just Woit and a bunch of crackpots hanging out here you wouldn't have bothered. But as you know, the readership includes many professional physicists in various areas, including HEP theory, who share to a greater or lesser extent (some of) the views Woit expresses. They are doing positive things in their own work, and yet Woit's negativity on aspects of ST and BSM work resonates with them. So you thought you might be able to change their minds on those views by attacking Woit's research contributions? Ha.

"See for yourself how hard it is to make progress given all these constraints."

Probably I'm just a simpleton but it seems that paths to uncovering new physics beyond the SM are becoming easier these days if you choose good ones, e.g. this one.

Admit it now that when you talk about "BSM" you really mean "BSM topics that are sufficiently grandiose to warrant the attention of myself and my illustrious peers. Any other, e.g. bottom-up type, approaches to BSM physics might as well not exist."

44. **Jesper**
May 29, 2013

"It's a big universe, whether it's a multiverse or not. There's room for all of us in it"

well – thats the point. Its not, and there isn't. Sadly.

Try as a young researcher to work on your own ideas and apply for funding. Then you'll reach the boundary.

45. **Marcel van Velzen**
May 29, 2013

@Mario. Correct, Altarelli many discusses naturalness in the light of supersymmetry.

46. **Anonyrat**
May 29, 2013

http://www.math.columbia.edu/~woit/wordpress/?p=2288

quotes Gell-Mann:

I was a sort of patron of string theory — as a conservationist I set up a nature reserve for endangered superstring theorists at Caltech, and from 1972 to 1984 a lot of the work in string theory was done there.
—

Who are the conservationists/incubators for ideas today, equivalents of Gell-Mann, using their ability to draw funding to support long-shot ideas?

47. **Peter Shor**
May 29, 2013

A.J. says “A secondary problem is that it’s difficult for an outsider to tell whether the spectral action principle is actually a deep physical principle or just a clever notation in which nearly any QFT can be expressed. ”

As I understand it (which isn’t very well), Connes and Chamseddine’s spectral action construction doesn’t work unless you have Majorana neutrinos, a new sterile neutrino, and a new field that couples to the sterile neutrino. These aren’t radical changes to the Standard Model, and they’ll be hard to see experimentally, but this does seem to demonstrate that not all QFTs can be expressed in non-commutative geometry.

48. **Matt**
May 29, 2013

amused,

I can only comment on things as I live them now, and today there’s a vibrant community of people in high-energy theory working on all manner of subjects, including non-BSM stuff like collider physics, jets, HQET, and applications of HET to condensed matter, among other things.

As for determining “if the gauged chiral symmetry of the SM really is spontaneously broken (or, equivalently, if the Higgs field really does have a nonzero expectation value), don’t you think?”, or whatever you wanted to work on, all I can say is that demonstrating that roadblocks you suffered were a consequence of sociology rather than the viability and potential of the work itself is still undetermined.

Your list of names brings to mind two favorite SMBC comics, both on the use of anecdotes:
http://www.smbc-comics.com/?id=2159
http://www.smbc-comics.com/?id=2307

For someone who claims not to be impugning people’s motives but just telling things like they are, you do an awful lot of impugning of people’s motives, like mine in particular.

For example, my point in citing Peter’s list of papers on the arXiv was to urge him to go out and work on this stuff rather than simply criticizing it from afar. It matters to his case that he doesn’t show outward signs of working on this stuff himself. It’s not a matter of disparaging him. You were sneaky to truncate my quotation where you did — I encourage people to go read the rest of that paragraph.

But then you turn around and attack my record, for no obvious reason except to disparage me, as though that accomplishes anything other than a fallacious appeal to emotion. None of this is remotely relevant to the argument I’ve been making here.

You write “But as you know, the readership includes many professional physicists in various areas, including HEP theory, who share to a greater or lesser extent (some of) the views Woit expresses.” I have no idea who reads Peter’s blog. I don’t stop by all that often — I just saw a post that I wanted to respond to, because I thought Peter was saying things about his own understanding of the Standard Model and what I viewed as a larger shift toward general antipathy toward all things BSM.

“So you thought you might be able to change their minds on those views by attacking Woit’s research contributions? Ha.” No. It’s rather presumptive of you to declare what my motivations are, don’t you think? I think I made my goals and requests very clear, and none of it involved “chang[ing] their minds” or attacking anyone’s research.

“Admit it now that when you talk about “BSM” you really mean…” I won’t admit anything I don’t believe.

I don’t know you, so I won’t start ascribing beliefs or opinions to you that you don’t possess. But whatever you’re like in real life, you don’t seem like a very pleasant person on the Interwebs. I’m not an apologist for the ways of academia, which has plenty of problems. I was simply commenting on a narrow issue that I thought Peter was incorrect about.

I think that’s about it for me. At the usual risk of not having the last word, I really have to get back to work now.

49. **Peter Woit**
May 29, 2013

Matt/amused,

Enough, please. I’d like people to stick to attacking me here, not each other. For the record, I think Matt is quite right to point out that I should be writing papers, amused is quite right to point out that there is no place in the current HEP job market for those pursuing research programs outside a quite narrow range. Other than that, best for all to attempt to be charitable in considering the motivations of all those making arguments here. I try and delete the really unreasonable ones….

50. **martibal**
May 29, 2013

@Jesper: very nicely said. Of course there is no space for all of us. There is as war for fundings and, as all war, this is pretty dirty and at the end the story is written by the winner. The claim that “scientific excellence is the main criteria of selection” (that would be equivalent to Matt admonition: “stop complaining and show your ideas are the best”) does not survive the analysis: just compare the scientific production of young persons that have been permanently hired because they were supposed to be “the most promising young scientists of the year”, with the scientific production of – say – permanently precarious postdocs. In number of cases (not negligible at all), a couple of years after the hiring and whatever weight you put on the criteria (number of papers, of co-authors, of citations, quality of the reviews) it is hard to get convince that the most- promising-young-scientist always held his promises. I do not think anybody honest would deny that politics is an essential aspect of the hiring process.

–

@Peter: in Connes approach to the SM, the Higgs is completely geometric. It comes out as a component of the connection (in the noncommutative part of the geometry), exactly in the same manner as usual gauge fields come out as connection 1-forms (on the commutative part of the geometry). If one considers the formula of the distance in noncommutative geometry (which generalizes Riemann geodesic distance), one can even see the Higgs field as the component of the metric in some discrete internal dimension. More geometric than that… &#128521

51. **pissantz34**
May 29, 2013

What a fascinating debate. If only the professionals in my field were able to debate so eloquently – indeed, I work in a field universally despised by yours: politics. Worse still, I’m a Republican. It’s amusing to hear Matt decry the negativity. You guys sound like choir boys compared to some in my line of work!

Just wanted to drop a note and provide a bit of encouragement from the outside world. Please, keep doing what you all are doing. I have not taken a physics class in my entire life, so it’s hard for me to follow the specifics of your arguments, but I keep my eye on science nonetheless. I believe you and others across so many disciplines are on the verge breathtaking breakthroughs ahead in the next 30 years.

Groupthink is a major problem in politics as well. The journalists who are supposed to hold the powerful accountable attend the same Georgetown parties as the politicians and Joe Biden's water gun fight parties every summer.

If it weren't for the Peter Woits of the political world, the hubris inherent in any political system would go unchecked. I applaud Peter and Matt and anyone else working their butts off to advance science. Peter, continue to question the status quo.

If I ever get to the point in my career where I can sway national policy in any significant way, I will defend science to the hilt. Whether major advancements come from String Theory that Matt is working on, or groundbreaking mathematics Peter is, it doesn't matter from my perspective. All of this work needs to be consistently and concisely explained and defended to policymakers and the public. That's one reason why I think String is succeeding to the extent it is: Green, Hawking, Kaku are fantastic, engaging ambassadors for your profession. The public needs more.

52. **Mark Israelit**
May 30, 2013

I don't understand why the connection is DENIED.
I wanted to see the paper submitted by myself, http://arxiv.org/abs/12122208 , and received even in this case the DENY answer.
Mark

53. **Hugh**
May 30, 2013

@Mark, you're missing a period from the url. It should be http://arxiv.org/abs/1212.2208

54. **Haim**
May 31, 2013

Peter,

This is pseudo-science indeed: "laws are just an arbitrary, messy outcome of random fluctuations in the fabric of space and time".

But this would not be pseudo-science: "The standard model and general relativity are the direct, obvious, and natural outcome of random fluctuations in the fabric of space and time." A number of people are trying this approach, which clearly follows Einstein's dream.

After all we know that space-time is randomly fluctuating. Or do you disagree? Are you implying that there is something wrong in trying to find the correct fabric of space-time? We all know that nobody has found the correct fabric yet. Connes did not succeed. Bilson-Thompson did not succeed. String people did not succeed. Spin network and many more did not. But some day somebody will succeed! Or are you proposing something else? Are you implying that space-time has no "fabric"?

My impression from reading your blog is that you are only arguing against the epithets "arbitrary" and "messy" in the quotation. Or are you arguing something different?

55. **Jack**
May 31, 2013

@haim,

Peter was arguing against the multiverse. To get to your point, there is little hope that some microstructure of space-time will lead to the standard model.

56. **Peter Woit**
May 31, 2013

Haim,

If your ideas are so vague that you can't extract any information about the way nature behaves out of them, then you're in danger of doing pseudo-science. Multiverse theories typically not only don't predict anything, but their who motivation is to construct an excuse for failure, an argument that it is impossible to predict anything. This is true pseudo-science.

The problem with discussing "random fluctuations in the fabric of space and time" is that this is also too vague to be very meaningful. Replace "random fluctuations" by some specific notion of quantization, and "fabric of space and time" by a specific notion of exactly what the fundamental variables describing space-time are and then you start to have something. To have something convincing and testable though, I think you need to do this in a way that unifies with our understanding of quantization and the fundamental fields of the SM.

A Tale of Two Oxford Talks

May 28, 2013
Categories: Multiverse Mania, Uncategorized

Last week (for more, see here) Eric Weinstein gave a talk at Oxford about his ideas about "Geometric Unity", with positive coverage from the Guardian, leading to various critical commentary. I agree completely with the main point of most of the commentary: if he wants to be taken seriously, Eric needs to disseminate the details of his ideas about this, as a paper, slides of a talk, multimedia web-site, or whatever. As AJ put it here succinctly: "Paper, or it didn't happen."

I've only the vaguest notion of what Eric's ideas are, so no way to evaluate them. From this standpoint of ignorance I should comment that I'm quite skeptical he has a viable unified theory, with reports of "very large multiplets of as yet undiscovered particles, and he has no idea of their masses" not confidence inspiring. But on the other hand, the current situation in fundamental theory is one of a serious lack of any new ideas at all. If he has been working on some very different ideas that haven't gotten attention before, he could have something interesting or even important. But, again, until details are available, there's no way to know one way or another. By the way, I should disclose that at one point I remember having a conversation with Eric about his plans to give a talk and make public his ideas. I tried to encourage him to do this, emphasizing though that I thought his main problem would be that he wouldn't be able to get anyone to pay attention. Shows how little I know…

Surprisingly to me, before it was clear what was going on, there was a quick and hostile reaction from some to the Guardian piece about Eric and his work. Yes, it's a bad idea for the press to publish overly optimistic material about grandiose and poorly supported claims from physicists, but this does happen all the time, and usually people (other than me…) don't bother getting worked up about it. Very quickly New Scientist (not known for its general policy of only reporting on carefully vetted research) had a piece from an Oxford cosmologist denouncing Marcus du Sautoy for organizing Weinstein's talk and not inviting any physicists:

> Hosting a lecture in a university physics department without inviting any physicists is, at best, an unforgivable oversight. As my colleague Subir Sarkar put it, "It's surprising that the organisers did not invite the particle physicists to attend – if indeed the intention was to have a discussion."

Soon New Scientist was joined by Jennifer Ouellette at Scientific American and PZ Myers, all outraged at the unprofessional behavior of du Sautoy. Reading the New Scientist piece, for about 5 seconds I thought "wow, that du Sautoy sure is a piece of work", before realizing "wait a minute, how likely is that?" Any experience with academic departments and dissemination of information like this should be enough to make one suspicious that the most likely course of events was that du Sautoy tried to get word out, but this didn't happen very effectively. Yes, departments and groups have mailing lists, but the ones people pay attention to are shielded from use by outsiders. After a couple days, it came out that the true story was that du Sautoy did contact people in the physics department trying to get their help advertising the talk, sent them posters, etc., exactly as one would have expected.

What I find most remarkable about this story though is the contrast to the one that I wrote about the day before here. This involved a Sunday Times report about Laura Mersini-Houghton's "hard evidence" for the multiverse, which she had found by analyzing the latest Planck CMB data. She plans to give a public talk about this at the Hay Festival on Friday, and a talk at Oxford is scheduled for June 11 (this talk is part of a workshop funded by the Templeton Foundation as part of their "establishing the philosophy of cosmology" effort). I assume physicists will get an invitation to the Oxford talk, but, at least at the moment, there's no paper that I'm aware of backing up Mersini-Houghton's claims. There is a 2008 paper about what Planck should have seen, but the Planck team reported nothing of the sort predicted in that paper.

This all leaves me rather curious about the question of why people got outraged about Eric Weinstein getting too much press attention for his undocumented claims and Oxford talk, when the same people as far as I can tell seem to have no problem with Mersini-Houghton and her undocumented claims + Oxford talk. To me it seems a lot more problematic that people have been reading in the press that hard evidence has been found for the multiverse than that they have read that Eric Weinstein has a theory of everything. Others seem to see things the other way around.

Update: Eric will be giving another talk at Oxford, this Friday, see here.

Update: To the extent you can call what's on Twitter "information", there's information about today's Oxford talk there, see for instance here and here.

Update: Denunciations of du Sautoy continue, see for instance here. For a response from him, see here. From the various very fragmentary accounts available online of the Friday talk, it sounds to me like Eric is far from having a viable TOE. Still no paper or details available, which is what is needed to see if he has a promising idea.

Meanwhile, on the BBC, it's multiverse-mania as usual, with Mersini-Houghton,

> a cosmologist at the University of North Carolina in Chapel Hill whose theory of the origin of the visible universe has attracted a lot of attention for its strong observational predictions.

explaining that

> the recently released data from the Planck telescope lend particular support.

As far as I can tell, cosmologists and physicists think this kind of thing is just fine, or maybe they are way too busy being outraged about du Sautoy's attack on the very fundamentals of science.

Update: Laura Mersini-Houghton and Richard Holman sent me the following which I'm adding here so that readers can have their point of view on this.

> As avid readers of your blog, we were a bit dismayed to see your post lumping our work together with Weinstein's. Unlike his case, we HAVE had not just one, but a series of papers where our calculations and predictions are laid out for all too see and argue about.
>
> To recap, we made use of a particular model of the landscape of string theory, the one derived in the Douglas-Denef paper 2004, constructed the effective density matrix for observables in our patch and then used that to derive our predictions. Within the context of this model, we showed that the scale of SUSY breaking would be far above the reach of the LHC and thus no super-partners would be seen. We also calculated how the back reaction from the other parts of the landscape modifies the gravitational perturbations in such a way that the following would be true
>
> a. the cold spot of 10 degrees in the sky at about z~1,
>
> b. another highly underdense/void like region aka a suppression of power at k~1 which would give rise to:
> c. a suppression by 30% of TT spectra of CMB at the lowest l<6 (k=1)
> d. a modification of quadrupole, dipole and octopole (lowest l's) which induces alignment of quadrupole and octopole, (axis of evil)
> e. a preferred direction due to induced dipole power
> f. the power asymmetry between the 2 hemispheres which are determined by the preferred direction (again the k~1 suppression shows as lack of structure at dipole/quadrupole level which suppresses structure in 1 hemisphere)
>
> g. an overall suppression of sigma_8 due to the same correction to Newtonian potential by 30%.

Hints of all of these had been found by WMAP, but PLANCK confirms ALL of these (Paper 13 in the Planck series).

The two papers where predictions were derived are:

http://arxiv.org/pdf/hep-th/0612142

http://arxiv.org/pdf/hep-th/0611223

The full theory in 2005 for which these predictions are made is developed here

http://arxiv.org/pdf/hep-th/0511102

This theory was and remains the only one that uses quantum cosmology to derive the selection criterion from the landscape multiverse and that calculated every single prediction from an underlying fundamental physics formalism, without resorting to anthropics or any other conjectures. Silence does not imply ignorance. While there is certainly room to argue with us (is our model of the landscape truly reflective of its actual behavior? How robust are our results to changes such as in the inflationary potential used?), we have striven to be above board in all we've written and said.

We also made predictions for a bulk flow that was argued for by Kashlinski et al. There has certainly been some dispute about the existence of this flow and a PLANCK paper argued that the flow is not statistically significant in their data but the jury is still out on that. However, the situation is not as clear cut as this. We are aware there was a paper by Pierpaoli et al stating they do not make a significant detection of the dark flow and another paper by Barandela et al., also a Planck team member, stating that the dark flow is definitely there and the filters used by Pierpaoli et al. were incorrect. Our current feeling is that it would be premature to say our theory is incorrect on the basis of a result awaiting conclusion while 8 of its major predictions have just been confirmed. Perhaps you are not aware that a bulk flow always arises when the CMB frame and the expansion frame in the universe do not coincide. On the other hand, should this discussion finally be resolved against us we are ready and willing to acknowledge this and move on. At least we have predictions that COULD be wrong!

It is true that there has been considerable media coverage for the last 7 years around this theory and its predictions but that is not surprising considering we made predictions for a theory of the origins of the universe based on fundamental physics. Don't let the media coverage divert you from the science. The key issue is that we have a theory based on a well known fundamental physics formalism and we made predictions for the anomalies in 2006 that are currently in accord with ALL of the data (modulo the pending dark flow results). That is 8 predictions confirmed and one to go. As we said before, you might want to argue with the underpinnings of our ideas and we are more than willing to enter into such discussions. But we have calculated within our framework, derived physical predictions from these calculations and await further data to fully confirm or refute our model. We think that this how science should be done.

In light of this, we would appreciate it if you could revise your post to reflect these facts.

Best

Laura Mersini-Houghton

Rich Holman

Comments

1. **Shecky R**
   May 28, 2013

   I too was surprised by the level of upset the Guardian piece generated (science "hype" being commonplace in the press), but apparently some folks see the Guardian as more immune to such coverage than other outlets (not always true).
   Hard not to think there's also a bit of jealousy/envy involved when a lone genius type enters the fray without going through normal channels… but someone of Weinstein's caliber certainly deserves a fair, open-minded hearing.

2. **Nameless**
   May 28, 2013

   The obvious difference is that Weinstein has a very high profile subject (the theory of everything), no prior published work in the area, and no clear way of getting where he claims to be getting.

   Mersini-Houghton has a relatively boring subject (multiverse aka Everett's many worlds – known since the 50's and accepted by a large fraction of physicists anyway), prior work (21 papers on arXiv), and at least partly mapped way of getting there (Planck data plus a description of what she'd be looking for in her '05 paper http://arxiv.org/pdf/hep-th/0510101.pdf).

   I'm perfectly willing to believe that Mersini-Houghton found one of the signatures from the '05 paper in Planck data, even in the absence of a new paper. (Whether or not that signature would in fact prove the existence of a multiverse, I have no opinion.) I am much more skeptical about Weinstein's magical leap.

3. **Mitchell Porter**
   May 28, 2013

   Shecky R, there are literally hundreds of similarly hopeful "theories" on the arxiv – theories which are outside of the model-building mainstream, I mean. (The number of mainstream models runs into the thousands.) Should all those people have their 15 minutes of fame too?

4. **Peter Woit**
   May 28, 2013

   Nameless,

   The cosmological multiverse is not Everett's Many-Worlds. The Mersini-Houghton 2005 paper you link to seems to be about something completely different anyway: there's no multiverse in it, it's about string theory inflation. Of course, since the only source for her claims about an analysis of Planck data is a Sunday Times article, with even less detail than the Guardian article, we have even less idea what this "hard evidence of the multiverse" is than what Weinstein's TOE is.

5. **anon**
   May 28, 2013

   Peter—

The biggest difference is that Mersini-Houghton has 43 entries in SPIRES, and over 1000 citations to her work (one paper has 365 cites). Eric Weinstein has zero publications. This should give her work more credibility.

That said, I agree that the Sunday times article, given no paper or arxiv entry, was overly hyped.

6. **Nameless**
   May 28, 2013

   OK, sorry – this paper is more directly relevant http://arxiv.org/pdf/hep-th/0612142.pdf

   I'm picking this up as I go along but Figure 4 seems to be the key. Their theory predicts that the correlation between the spectrum of CMB and cosmic shear (weak gravitational lensing) is substantially different from what's predicted by Lambda-CDM. There's a number of other predictions too (which include cold spots and dark flow as mentioned in the other thread).

7. **Peter Orland**
   May 28, 2013

   I agree with anon (and probably everyone else on this thread) that work which is available in some form should be taken more seriously than unpublished claims. A lot of citations can suggest additional merit.

   Unfortunately, such criteria aren't foolproof. Sometimes heavily-cited papers don't stand up to close inspection (of course, papers which are never cited often have even less value).

8. **Peter Orland**
   May 28, 2013

   Bad construction in parentheses. I meant to say:

   (of course, papers which are never cited ON AVERAGE have even less value).

9. **Kuas**
   May 29, 2013

   The unwritten rules of who is and is not allowed to spin the science press are always a subject of curiosity to me. Clearly the bandwidth for science hype is limited public resource which must be managed responsibly.

   In Weinstein's defense, it's not clear that he actively sought out the Guardian coverage, which was a tad ridiculous. I can see a scenario where he innocently responded to an invitation to talk, and an overzealous reporter somehow got wind "Weinstein, Einstein- it practically writes itself!"

10. **Snm**
    May 29, 2013

    Also, Eric Weinstein will be doing another talk in Oxford this Friday – I don't know if it's the same one, or a follow-up

11. **oxfordanon**
    May 29, 2013

    I think the talk on Friday will be the same as the previous one, except this one has been advertised. I think this is a good faith attempt to address the issues with the previous talk, and I will be attending.

    Just to be clear about what happened and why people were annoyed. Most departments with particle theory groups, anywhere in the world, have a regular weekly seminar on particle theory at a regular fixed time. This is true in Oxford (Thursday 4.15). This Weinstein seminar happened at 4pm on Thursday (ie clashing with the regular seminar), and the first people in the particle theory group heard about it was after it had happened, and in some cases via outlandish claims in the national press.

12. **Anonyrat**
    May 29, 2013

    Shecky R, no one is denying Weinstein a hearing. The problem is that few know what he is saying. There is a paper, as per du Sautoy, but it is not available anywhere. Why is a hype piece so readily available in the newspaper, but the actual work is not?

13. **Shecky R**
    May 29, 2013

    Don't want this to degenerate too far, but worth noting that Marcus du Sautoy has now indicated his umbrage over the issue with several tweets at his Twitter feed:

    https://twitter.com/MarcusduSautoy

    one of which reads as follows:

    > "I spent two years looking at @EricRWeinstein work. @newscientist and @apontzen didn't bother to check their facts. Scientific?"

    (p.s. – he also mentions Peter's post here as a "fair assessment")
    And he mentions the repeated Weinstein talk this Fri. at 2pm at the "Mathematical Institute" (Oxford) …I'm guessing physicists can probably sneak in ;-))

14. **op-ed**
    May 29, 2013

    The tweet itself indicates part of the problem "I spent *two years* looking at @EricRWeinstein work. …" But in all of that time, du Sautoy does not seem to have said to Weinstein "Write this up, post a preprint on the arXiv (or a personal web page) etc. *After* you post a preprint, *then* I shall arrange for you to give a lecture at Oxford." It's only du Sautoy who has seen Weinstein's work. And the Guardian op-ed piece was a clear personal endorsement of Weinstein's work.

    On the other hand, about Laura Mersini-Houghton … the Sunday Times article opens with "SCIENTISTS believe they have found the first evidence that other universes exist." It's a third-party reporting of published material, and one can criticize the 2008 paper by Laura, but at least there's *something* out there for independent researchers to examine dispassionately.

But Weinstein will give a second talk on Friday, physicists will be there (or have been invited), and hopefully a publicly available preprint will follow soon.

15. **A.J.**
May 29, 2013

*It's only du Sautoy who has seen Weinstein's work. *

This is definitely false, and the original Guardian article makes that clear.

16. **Ted**
May 29, 2013

The entire du Sautoy dust up looks like petty office politics to me.

As far as Weinstein goes, his approach is obviously unorthodox, but perhaps not unacceptable. Someone correct me if I am wrong (I wasn't quite born yet so my history might be off!), but didn't Witten present his M-theory argument at Strings 1995 without circulating a paper or publishing prior? Obviously Witten was already a household name at that point and he was working within an already established paradigm at a major conference. And exploring strong coupling limits and playing with dualities of established theories is easier to follow in a lecture setting than proposals of an entirely new framework. But this situation doesn't seem so different that people need to unleash the hounds on Weinstein. Maybe Weinstein wanted feedback before finalizing a paper? I'm very skeptical Weinstein has anything close to a coherent theory of quantum gravity, and when he finally gets a paper out there we'll see (though what I've read about it doesn't sound promising), but some of the rhetoric against him seems undeserved at this point.

As far as the press, I'm all for going against the press hyping scientific claims. I think it pollutes both the scientific process and the dissemination of realized knowledge to the public. Too often the media talks of speculative hypotheses and conjectures as facts, and not just in physics. The Guardian article was obviously inappropriate. That said, I would be more sympathetic to those angry about this if they showed similar outrage about a whole host of speculative claims the media disseminates. To give an idea of what good science journalism would look like, take this Mersini-Houghton case. Before announcing that the multiverse exists, the press should wait until the evidence is formally presented and rigorously examined by her scientific peers – and then disseminate the claim if her colleagues agree.

17. **cormac**
May 29, 2013

I'm not upset, nor am I jealous, and I consider Professor du Sautoy an excellent professor for the public understanding of science. I was just a little surpised that Peter, who has such withering criticisms for the publicity afforded entire fields such as string theory and supersymmetry, seemed to be stay his hand when it comes to the publicizing of work that has yet to be examined by experts in the field.
I could certainly see how an author of ground-breaking advances in theoretical physics who have never received media attention might be miffed at the publicity given by the Guardian to this work, but my real point is that I think this is another example of the media interfering in the normal process of science

18. **Yatima**
May 29, 2013

Implying there is a "normal process of science".

The "advancement of science" is an entirely human affair, indeed.

Papers got to sell copies and fill column space. Writing about as-yet-unproven and unknown ideas does that. Which means there is some interest in these things.

Does it need firing up with ludicrous formulations? Considering that most Guardian readers will run away screaming at the sight of an integral sign, quite possibly. Some showbiz required.

The alternative? No headlines, no-one cares about fundamental science and it dies an underfunded death. Choose your poison.

19. **David Nataf**
May 29, 2013

People give colloquiums about work that is in progress and not yet published on a regular basis. It's quite normal, and those of you arguing against it strike me as likely operating on either the fringes of science or outside of science.

20. **Tammie Lee Sandoval**
May 29, 2013

This idea of expecting the press to do the job that scientific ethics have abandoned is laughable. Its not really their job, but more to the point they wont do it. The press needs to sell papers and hype sells.

Scientists going the press, before having their work independently checked (or even completed) means this: Fame is becoming more important to Scientists than being right.

After all, in the days of book sales, talk shows, and speaking fees, what counts is fame. And even if your stuff is found to be wrong, the fame remains. And if you spin it right, you'll always be known as the scientist with the data on the multiverse. The scientific contraversy, which you can always muddy up for the public, only helps spread your fame.

The Mersini-Houghton case is especially disturbing.

21. **Mitchell Porter**
May 29, 2013

I've just been looking at Mersini-Houghton's work and it's actually somewhat interesting. She throws around the word "multiverse" but her "other universes" are really just other regions of our one universe, beyond the cosmic horizon. The predictions that have been mentioned recently on this blog (dark flow, the cold spot) are imprints of early influences from outside the horizon.

She's also known for some non-anthropic "landscape" predictions. As with "multiverse", the word "landscape" may evoke a negative conditioned response among many readers of this blog, but it just means that there are multiple distinct configurations that the fundamental fields can settle into at low energies. Here she predicted a small cosmological constant and a high supersymmetry scale, but the interesting thing is that she obtained those predictions *not* through anthropic reasoning, but purely from the posited dynamics.

So people who hate parallel worlds and anthropic justifications shouldn't be hostile to her on that account; her style of reasoning does actually fit the old-fashioned method of "make a theory about how physics works in a single universe, and deduce the consequences".

22. **P**
May 29, 2013

David,

You're spot on. I've given talks on work about to be released, as have many of us. I suppose the only difference is that most of us don't claim a brand new theory of everything and don't get it publicized in the Guardian. The latter should be the criticism, not talking on unpublished work.

Cheers,
P

23. **richard**
May 29, 2013

I blogged on the du Sautoy column, and may yet return to it — for me the issue was not someone working outside physics, or Oxford providing them with a venue to share their ideas. The issue was a Professor of the Public Understanding of Science (or whatever his exact title is) hyping work which no-one else had seen in an area full of brave failures — if it had been written by a journalist I doubt it would have prompted the same reaction.

Perhaps a different question here is the extent to which a professional scientist who writes for a wider audience "on the side" is able to avoid being held to the usual standards of "preprint/peer review or it didn't happen" — and in this sense, it is clear that people in the community expected more from du Sautoy. And perhaps fair enough — du Sautoy would have been much less likely to be provided with space in the Guardian if he was not already a serious mathematician.

As far as the Mersini-Houghton issue is concerned, I got asked questions about the same piece at a conference in New Zealand last weekend, on the basis of the press coverage — whether it was read on a website or syndicated by a New Zealand publication is still not clear to me.

As many people have pointed out, very few astrophysicists ever took claims about "dark flow" seriously, and Planck has cast further doubt on the topic — and any extravagant theoretical architecture justified by its ability to explain a non-existent phenomenon is not going to get a lot of attention from scientists. (I work in this general area, and I think it is fair to say that this idea has no real traction in the community.) That said, I don't think it is my job or anyone else's to correct every piece of bad science journalism that comes down the pike — there would not be enough time left for science.

24. **David Nataf**
May 30, 2013

Richard,

It doesn't matter if the idea has no real traction among astrophysicists. Neither did dark matter when Fritz Zwicky first suggested it (in the correct quantity) 40 years before it was found in Galaxy rotation curves, and neither did a cosmological constant when people were suggesting it before the supernovae teams did. Science shouldn't be a popularity contest. The dark flow is still a legitimate scientific hypothesis, that I'm sure a small niche community will continue to work on.

I have no attachment to it myself, and I work on Galaxy-scale astrophysics. However I totally don't get your snarkiness. It seems a lot better motivated to me than some other questions people work on, like looking for variations in the fine structure constant by studying high-redshift AGN, or looking for Lorentz violations by timing of GRBs, or modified newtonian dynamics, or inferring axions from white dwarf cooling curves. These latter questions come off as either similarly or far less well motivated, yet as far as I can tell, more people work on them.

25. **Giotis**
May 30, 2013

There is a correction/apology in New Scientist piece by Andrew Pontzen.

But why this secrecy? Where is the video of the talk? Why it is not available in Oxford page?

Or maybe the talk was so bad and crackpotish and they are embarrassed to publish it in their page:-)

26. **D.G. Perkin**
May 30, 2013

It shouldn't be too hard for physicists in Oxford to find the talk at the Mathematical Institute – the Nuclear Physics Department is just across a very quiet road from the Institute (unless something has been moved since my time).

However, I chance to recollect that the Maths Institute lecture hall is quite small so they might have trouble getting in. Also, physicists tended to ignore mathematical physics events at the Institute. I doubt if things have changed much.

I attended many external seminars and lectures in Oxford but I heard about most of them by word of mouth – I don't think there was an accepted system for informing people. Since this is Oxford, it is not unlikely that nothing has changed.

Regards,

David Perkin.

27. **MathematicianNotPhysicist**
May 30, 2013

Read oxfordanon's post above to see how much effort was made to make the (1st) talk available to the Oxford particle theory group.

Anyway, the second talk must have happened by now. I wonder how it went.

28. **MathematicianNotPhysicist**
May 30, 2013

Oops. Silly me. The 2nd talk is tomorrow. Show how good I would be at scheduling.

29. **Shantanu**
May 31, 2013

Just fyi, in my univ from where I graduated. we usually never got any notices of seminars in math or astronomy
even though many of them were relevant to physics (and probably also vice versa)
However usually all talks used to be advertised on the department websites and that's
how we came to know about it.
and anyhow in this case looks like the notice of seminar was sent to physics department folks.

30. **Richard**
May 31, 2013

David — dark flow and dark matter are not really analogous. Zwicky's point about dark matter was based on cluster peculiar velocities (as I assume you know): if clusters are bound objects the galaxies inside were moving too fast if you assumed galaxy masses based only on their apparent stellar content. In the 1930s this would not imply non-baryonic dark matter, just the presence of a large amount of non-stellar but otherwise normal material, or perhaps a large number of very faint stars. With the benefit of hindsight, Zwicky's point was clearly not given the attention it deserved, but I do not believe it was ever fully addressed or refuted.

By contrast, the claims made by Kashlinsky were not convincingly reproduced, and other results (including the Planck paper) are entirely consistent with the preculiar velocities one expects to see in LCDM models — so the current data suggests that the dark flow simply isn't there, at least in the sense implied by Kashlinsky. Galaxies are moving, including the Milky Way at 627 km/s relative to the rest-frame of the CMB, or whatever the number is — but their velocities do not have a magnitude or long-range correlation beyond that predicted by standard theories.

So yes, you can say that dark flow is a legitimate hypothesis, but it is not a hypothesis that appears to have survived contact with the data. However, the corollary of that is a multiverse theory that can "explain" dark flow is in a similar position to a modified gravity theory that "explains" the Pioneer anomaly: it is not something that needs to be explained, and should probably not be presented as evidence for a multiverse theory.

As to the fine structure constant by studying high-redshift AGN, Lorentz violations via timing of GRBs, or modified newtonian dynamics I completely agree with you — the specific models being tested are simply not that compelling.

31. **Ian Sample**
May 31, 2013

There has been plenty of over-reaction to this, and much it from otherwise sane people. We ran a blog post on a talk at Oxford that we thought was interesting. That's about it. Hardly a reason to reach for the torches and pitchforks. To look at the reactions of some, you'd think the foundations of science had been violated. The piece appeared on the science website's Notes & Theories blog, which we set up as a home for our musings about goings-on in the science community. Nothing appeared in the newspaper, for all the talk of "splashes". The post carried plenty of comments from the author and jobbing physicists on how the idea wouldn't – indeed couldn't – be taken seriously without a paper for others to assess. But take a step back: from an outsider's point of view, this was nonetheless an interesting lecture on a fascinating problem in physics. It was well within the scope of material we want to cover.
There may be a category error afoot. We are journalists, not scientists. We write about issues in science that we find interesting. We make the calls on that. We don't treat our readers as idiots who are too stupid to be exposed to ideas before they appear in a peer reviewed journals. Most can even read without moving their lips. They understand that a talk without a paper is not a paper with a talk. We can and should write about university talks, conference presentations and posters, claims made by government scientists, and of course, peer reviewed studies, and we will of course continue to do so.

32. **Stuart**
May 31, 2013

Du Sautoy isn't a journalist. People are irked by how he went about things, not that a newspaper published a wildly overenthusiastic blog post.

33. **Dom**
May 31, 2013

Ian Sample. I have your excellent book "Massive" but you are wrong here – I'm with Ben Goldacre that Science writing cannot abandon reality and facts in search of a good story accessible to the layperson. I speak as a layperson.

34. **merian**
May 31, 2013

I have no problem with researchers giving talks before publication. This is a completely legitimate way of circulating your ideas. I'm also not unhappy with Weinstein – who I'm unhappy with is first Marcus du Sautoy and second Alok Jah: Science-by-press release and splashy sycophantic reporting of unconfirmed and even unpublished claims has been undermining the public's trust in the scientific process. If du Sautoy, as a professor for the public understanding of science, neglects this, he's not doing his job. I've always liked him a lot when he cooks down mathematical concepts for the lay listener on the radio, so I'm also disappointed. On a personal level, I understand it actually that he may be overly enthralled by some new mathematical physics that he has studied and admires, for sure. I wish he wouldn't forget to step back and assess where this thing stands in the scientific process. Not even a pre-print? Seriously?

In comparison, the Sunday Times article, from the little I can see peeking over the paywall, addresses precisely one of the next steps – comparison with experiment. It reports on new experimental results which are great in their own and quite solid.

To be entirely clear, I'm a geophysicist, cosmology isn't my field, and I tend to be sceptical of new great theories-of-everything, so it'll be a long time until I might be convinced that either of the two has something true to say about what the world's like. And reports about both are a little over the top. But only one I see as actively harmful.

35. **merian**
May 31, 2013

@Ian Sample, an hour earlier: "Roll over Einstein: meet Weinstein" and the harping over trite clichés (the new theory of everything, the hermit [21st century version – an independently wealthy finance guy] working alone in his cave, the upset of established science by the unknown pure brain) — if you really think this is the best science journalism can do I'm not hopeful.

The tendency to uncritical hero worship and the preference for splashy ideas is indeed one of the reason the Guardian science podcast isn't in my top 5 of science podcast even though 3 times of 4 I enjoy it. The fourth it annoys me so much I give up listening for a few months.

36. **Anonyrat**
May 31, 2013

Ian Sample:
One of the problems with what happened is it is not that Weinstein isgiving a talk without a paper. There is a paper.

du Sautoy wrote so in his Guardian piece:

"Weinstein begins the paper in which he explains his proposal with a quote from Einstein: "What really interests me is whether God had any choice in the creation of the world." Weinstein's theory answers this in spades."

Where is this paper? Is it top-secret or something? Or should du Sautoy have written:

"Weinstein begins the private communication in which he explains his proposal….."

You wrote that your readers "understand that a talk without a paper is not a paper with a talk." But du Sautoy wrote that there is a paper. Without Peter Woit's blog how would I have known that there is no paper?

37. **Peter Woit**
May 31, 2013

merian,

The way the Sunday Times article addresses "comparison with experiment" is just by repeating Houghton-Mersini's unsubstantiated claim that her (unpublished) analysis of Planck data provides "hard evidence" for a multiverse. This is despite the (published) analysis by the Planck team showing exactly the opposite. I don't see any good argument for why the Sunday Times should be publishing such claims.

I also don't understand people's complaints about du Sautoy writing about unpublished work. Why shouldn't he (or any academic) write for the public about unpublished work that he finds interesting? If it turns out to be highly significant work, he'll have done very well his job ("Simonyi Professor for the Public Understanding of Science"), if it turns out there's nothing there, he'll have done his job less well. I can easily imagine a circumstance where a mathematician I know had a big result and didn't mind if I wrote about it. If I thought it was probably correct, I'd blog about it. If du Sautoy wrote about Weinstein on his "Finding Moonshine" blog, would that have been a problem?

My suspicion is that people are skeptical of Weinstein's claims (as well one should be of people who claim to have a TOE…), so their problem with du Sautoy is that he was publicly promoting something they think should be treated skeptically. In that case though, they should also have a problem with most of the press coverage of string theory, the multiverse, and any number of other popular speculative ideas about physics.

38. **exscientist**
May 31, 2013

I understand du Sautoy is the successor of Dawkins.
I'm no fan of Dawkins, but I can't imagine him doing something like the Weinstein thing.
"Well, there's this guy Weinstein, who hasn't been active in research in ages, but he has this beautiful biological theory that may solve all the puzzles left by Evolution Theory, so, although he hasn't published anything and no biologist ever saw this theory, I'm going to write an article in The Guardian about it and give him a forum and endorse his theory …" etc.
Or imagine a bunch of physicists who organize a seminar by someone who claims he has solved the Riemann Conjecture – without a decent article, without scrutiny by mathematicians etc.

39. **Peter Woit**
May 31, 2013

exscientist,

Mathematical proofs are a different kettle of fish. In any case my understanding is that Weinstein is not claiming to have a finished TOE, but some important new ideas about such a thing. I think a more accurate analogy would be a physicist deciding to organize a seminar by someone who had a quantum mechanical model which gave new insight into the Riemann hypothesis, and a possible route to a proof (this is not an implausible scenario…). Most mathematicians would be skeptical of such a thing, but I don't think they would get very worked up about an account of it appearing in the Guardian.

40. **André van Delft**
May 31, 2013

I agree with Peter's latest remark. Why set the bar this high for Weinstein and Du Sautoy, and not for String theory? Thousands of scientists have worked for 40 years on String theory; is there any experimental evidence that supports it?

Nobel laureate Martin Veltman wrote a good and readable book: "Facts and Mysteries in Elementary Particle Physics"; he concluded the book with:

The reader may ask why in this book string theory and supersymmetry have not been discussed. (…). The fact is that this book is about physics, and this implies that the theoretical ideas discussed must be supported by experimental facts. Neither supersymmetry nor string theory satisfy this criterion. They are figments of the theoretical mind. To quote Pauli: they are not even wrong. They have no place here."

Half a year ago Veltman was interviewed for the Dutch magazine Elsevier.
A quote in my translation:

He still follows developments in theoretical physics. 'Sometimes they ask if I can keep up. The honest answer is that I do not have to make any effort for that. Since 1973 it stands still'.

41. **book**
May 31, 2013

Actually, Veltman has been on the record (in the past, anyway) that he doesn't believe in the Higgs mechanism either. (Although he did acknowledge that the resulting theory was renormalizable.)

42. **André van Delft**
May 31, 2013

I meant: Peter's latest-but-1 remark, of May 31, 2013 at 3:05 pm

43. **exscientist**
May 31, 2013

Peter,

An even better analogy might be this: Dawkins giving a forum to Weinstein, endorsing his theory as interesting and beautiful, writing a piece in The Guardian etc. People would have wondered if he was out of his mind. Dawkins is a biologist, not a physicist. I haven't read everything he wrote nor heard everything he said, but as far as I know, if Dawkins voiced opinions on the merits of a theory, he stayed close to this subject: biology.

And du Sautoy is a mathematician, not a physicist. If he wants to organise a seminar with Weinstein, that's fine. Perhaps Weinstein did some interesting mathematics. But du Sautoy shouldn't voice opinions about physical TEO's etc.

44. **André van Delft**
May 31, 2013

In this 2003 book Veltman left all options open, but he took the Higgs particle very seriously, and dedicated 1 of the 12 chapters to it. Quote:

So what is the situation? Most likely there is only one Higgs (if any!) and there is a vague prediction for its mass. however, there is trouble brewing and it is not at all sure that the Higgs actually exists. (…)

BTW. A preview of the book is on scripd.

45. **merian**
May 31, 2013

@Peter Woit: The Sunday Times article doesn't reach very high in science journalism either, I agree, and if you asked me which of the two theories is more likely to describe reality, I'd obviously (I hope) opt for Weinstein. Still, as a piece of science writing the du Sautoy/Jah approach makes me increasingly uncomfortable in a way the bubble universe one doesn't. And that's even though I won't believe for a second the multiverse idea is more than an idle thought experiment.

The public understanding of science currently has huge deficits in familiarity with the scientific process. Texts purely concerned with scientific contents, however beautifully written (du Sautoy is extremely admirable in this regard), won't help. We used to have scientists talk about new spekulative ideas, then publish them, then the relevant scientific community hash things out in small committees, then popular though still ambitious articles written, then the press add a gloss of celebrity. Now we get a media machine gobbling up stuff even before an result or proposition has even been examined.

And yes, "most of the press coverage of string theory, the multiverse, and any number of other popular speculative ideas about physics", the way it is currently done (and include some stuff from evolutionary science and microbiology into it) is not something I consider helpful for science in the long run.

46. **Peter Woit**
May 31, 2013

exscientist,

du Sautoy's specialty is the mathematics of symmetry, which is not as far removed from unified field theories as biology is. Yes, he's possibly out of his depth here, but academics commenting out of their depth on speculative physics is not unusual (although mostly it's physicists doing it…)

Funny, but the very little I know about Dawkins does agree with your claim that he is more cautious about this sort of thing than du Sautoy. I picked up a copy of "The Magic of Reality" in a book store, which had a really lurid cover, and looked to see what he said about physics. He had some sort of discussion moving to shorter and shorter distance scales. When he got to the nuclear scale, I was expecting him to go on something like "then there are quarks, then physicists believe there may be strings, etc…". Instead he said something like "I don't understand physics at the nuclear scale or below, so I'll say no more". Admirable. But unusual…

47. **book**
May 31, 2013

Veltman's book: quote from Introduction (opening paragraph):

"The twentieth century has seen an enormous progress in physics.
The fundamental physics of the first half of that century was
dominated by the theory of relativity, Einstein's theory of gravitation,
and the theory of quantum mechanics. The second
half of the century saw the rise of elementary particle physics.
In other branches of physics much progress was made also, but
in a sense developments such as the discovery and theory of
superconductivity are developments in width, not in depth. They
do not affect in any way our understanding of the fundamental
laws of Nature. No one working in low-temperature physics or
statistical mechanics would presume that developments in those
areas, no matter how important, would affect our understanding
of quantum mechanics."

I imagine Anderson will be livid to read the final sentences about the theory of
superconductivity (more accurately, the formulation in terms of spontaneous symmetry breaking) ~ "They do not affect in any way our understanding of the fundamental laws of Nature."

48. **Peter Woit**
May 31, 2013

book/Andre van Delft,
Enough about Veltman please, this has wandered completely off topic.

49. **exscientist**
May 31, 2013

Peter,

Dawkins – love him or hate him. But I think he adhered to the correct academic standards when he refused to judge physical theories.

The correct attitude for someone like du Sautoy – Simonyi Professor for the Public Understanding of Science in Oxford, after all – would be to put question marks around all those outlandish speculative claims by physicists.

But he just seems to add an outlandish claim to the multitude. I agree it's bad that "academics" are "commenting out of their depth on speculative physics" and I agree it's bad that "mostly physicists" are doing it.

But I think it's even worse that people outside of physics are starting to do it. Certainly if they have – or seem to have – some authority, like a Simonyi Professor for the Public Understanding of Science in Oxford. Du Sautoy could have presented the work of Weinstein – which might be very important – as a mathematical exploration, and that's what he should have done.

50. **book**
May 31, 2013

Actually, what I *would* like to see is a post from someone (physicist?) who attended Weinstein's talk, to explain/describe/summarize the main point(s) of the new TOE.

51. **oxfordanon**
May 31, 2013

book: it's more or less what you would expect. He is someone who is not stupid, who is intelligent and has a PhD from Harvard. *But* this is a properly hard subject, and those credentials don't get you anywhere near a theory of everything. He spoke a lot about geometry, and not so much about physics, and the part that was about physics was enough for me to be clear that he does not have a decent enough grip/intuition on what the Standard Model is and what it allows.

I didn't really follow what his geometric construction was, but at the end he predicts a whole bunch of new states (lots and lots and lots) that are both chiral and charged under all three forces of the Standard Model. As chiral matter can't acquire mass above the scale of gauge symmetry breaking (around a 100 GeV), such a prediction is essentially dead in the water – this energy area is all explored and you can't hide this stuff.

This is one of the things that to me rings massive alarm bells at any claims towards a TOE. If someone is not immediately aware – and he wasn't – that there are big problems with predicting new chiral fermions charged under electromagnetism or the strong force, then how much can you trust any other physics claim made?

He's not a crank, he has thought about what he is doing and can put things in the language that professionals use. Cranks don't talk about bundles and know what a representation is. But he's still wrong, and he doesn't really know physics – the way he talked about the Standard Model, the cosmological constant made that clear.

There is a reason professionals in the subject think what they do, and work on the problems they work on. The talk confirmed to me that there are good reasons to this.

52. **Mitchell Porter**
May 31, 2013

"As chiral matter can't acquire mass above the scale of gauge symmetry breaking"

What is the argument for this, exactly? I bet there's a loophole.

53. **Cormac**
May 31, 2013

I'm somewhat surprised by Ian Sample's comments. As a physicist who writes a regular column for a newspaper myself, I pay close attention to feedback from my physics colleagues, rather than dismiss it out of hand.
Also it's worth pointing out that the 'blog post' appeared online as an op-ed article. Many eminent scientists would give their eye-teeth for that sort of publicity.

P.S. I very much enjoyed 'Massive', well done – I sent you a commentary including some suggested minor corrections but you never replied (what would I know, I only teach a course in the history of particle physics for a living)

54. **Brathmore**
May 31, 2013

More news about Weinstein:

http://www.newscientist.com/article/dn23632-how-to-test-weinsteins-provocative-theory-of-everything.html

55. **Mitchell Porter**
May 31, 2013

Another thought on Weinstein's theory… I am waiting for some news that will truly identify what *sort* of construction it is. Such theories of physics don't exist in a mathematical vacuum. An individual theory belongs to a mathematical class of similar possible theories: gauge field theories, string theories, etc. If it's a familiar class of theories, then a lot will be known about what it can and can't do, how unique it is, and so on. If it's an unfamiliar class of theories, then we ought to be interested in other possible theories from that class too, and not just the initial example.

On Quora.com, there is an account of the latest talk, which makes his theory sound like a topological QFT, since it starts from a topological manifold with a bundle and a connection, and then the metric is part of(?) the connection; and all that is a well-explored topic… Hopefully we will soon have enough definitive information to place Weinstein's theory in conceptual and historical context, and see what is new or interesting here.

56. **ab**
May 31, 2013

Mitchell Porter — you have to give mass to charged chiral matter through a Yukawa coupling, so the natural scale will be the symmetry breaking scale.

57. **Mitchell Porter**
May 31, 2013

ab, if that's the argument… It's not even clear whether the mechanism whereby Weinstein's particles obtain mass, conforms to that description; in which case this sort of counterargument is beside the point. He's probably getting mass terms from a geometric effect, and not from anything resembling the Higgs mechanism.

Incidentally, what about radiatively generated masses, or masses from susy-breaking?

58. **Able Lawrence**
June 1, 2013

Here is the link to the discussion at quora that describes the lecture.
Weinstein considers Higgs mechanism as too contrived and has an alternative (more natural mechanism) that is manifest as Higgs mechanism.
http://www.quora.com/University-of-Oxford/What-was-the-reaction-to-Eric-Weinsteins-May-23rd-lecture-at-Oxford-on-the-geometric-prospects-for-unification

Science cannot even begin until we have most of at least the general types of all viable theories.

59. **Shantanu**
June 1, 2013

something OT.
Peter and other : any comments on the SNOMASS conference at KITP?
http://online.kitp.ucsb.edu/online/snowmass-c13

60. **ot**
June 1, 2013

This needs to be a separate post. "Snowmass on the Pacific" … good grief.

61. **Peter Woit**
June 1, 2013

OK, OK, see the next posting

62. **fifth**
June 1, 2013

This all reminds me of the 'fifth force' business from the mid-1980s. The person in that case (Ephraim Fishbach) was a very reasonable physicist, mild-mannered and soft-spoken, not a publicity seeker at all. He pointed out a perfectly reasonable thing, viz. that physics at some length scales was in fact not very precisely tested, and there was room for a fifth force of nature, which coupled to hypercharge with a range of tens of m. There was apparently some experimental evidence for it in those days. It was a reasonable claim, worthy of further investigation, not at all a crank thing. But in the end, it did not work out.

63. **Mitchell Porter**
June 4, 2013

Without a paper, a bootleg video, or any more secondhand reports of the talks, it is impossible to say what Weinstein's theory is, so I will shut up about it after this. But I will record one final guess regarding how it works: maybe it's a 64-dimensional spinor coupled to an SO(14) gauge field that divides a la "graviweak" into SO(10) and SO(3,1).

Circumstantial evidence for this: The 64 has room for 2 generations, and in Jacob Aron's New Scientist article "How to test…", he says "the third generation belongs in a different framework in Weinstein's theory". And, Weinstein just tweeted about SO(10).

Did anyone see Mersini-Houghton talk? 🙂

64. **Terry**
June 4, 2013

This is from the daily galaxy article here: http://www.dailygalaxy.com/my_weblog/2013/06/the-next-einstein-radical-new-theory-answers-unsolved-mysteries-of-physics.html

"Weinstein's theory is also the first major challenge to the validity of Einstein's Field Equations, revealing that "just as Newton's equations were an approximation to nature so too are Einstein's. "

I am going to put a sticky note on my white board about this being "the first major challenge" to Einstein… revealing that Einstein is just an approximation to nature. I am sure the next time I read about an extension of general relativity it will start out "this is the second major challenge…" right?

65. **MarcF**
June 4, 2013

From a distance it looks like yet another group theoretic approach to ToE ala E(8). While one has to admire that the path integral formalism has gotten us so far by basically parachuting terms in Lagrangians and seeing if it agrees with the experiments, the resulting narrow set of symmetries still beg an explanation. Symmetries don't exist in a vacuum. Personally I find the solid state studies of utmost interest in pointing to 'fundamentals of physics'. As a 'gentleman physicist' I relate to the Weinstein story. There is something about getting these ideas in the public domain and getting all of us to argue so violently about 'what makes science'… I have enjoyed reading the thread…

66. **Mike**
June 5, 2013

Here's a Tim Gowers comment on Weinstein's second lecture:

The status of Eric Weinstein's theory of everything has become a bit clearer, because Weinstein gave a repeat of his talk, this time attended by physicists. It is discussed in the article below, of which two key paragraphs are these:

"The trouble is that we should already have seen some of Weinstein's new particles, if they exist, says physicist Joseph Conlon of the University of Oxford. Properties of some of the predicted particles mean that they should be linked to the strong force, one of the four fundamental forces and the one that binds protons and neutrons.

"Experiments at the Large Hadron Collider at CERN near Geneva, Switzerland, have been smashing particles together at high enough energies to overcome the strong force, creating a spray of other more elusive particles, such as the Higgs boson. Weinstein's new particles should therefore have been detected in the resulting particle shower."

So it looks as though Weinstein's theory isn't dead yet, but he clearly has some serious explaining to do.

67. **Mike**
June 5, 2013

Sorry, here is the article link that Gowers was referencing:

http://www.newscientist.com/article/dn23632-how-to-test-weinsteins-provocative-theory-of-everything.html

68. **cormac**
June 7, 2013

Hi Peter, that is a really interesting addendum from Professor Mersini and Professor Holman. For those of us who find the mathematics of theoretical papers quite daunting, a summary of the work by the practitioners themselves is very helpful (as opposed to simplified pieces by science journalists). So blogs can be very useful!

I also agree with the Mersini's and Holman's central point on media hype: it seems unfair to link their case with that of Weinstein. Controversial in the field or not, the recent work of Mersini and Holman is simply one aspect of long careers in the highly competitive field of theoretical cosmology. By contrast, Weinstein is an amateur scientist (in the strict sense of the word, not in a negative sense). I find it reasonable that the predictions of Mersini and Holman would attract attention from the media, as well as the community, given the public's fascination with the multiverse. I don't think this equates with an amateur being granted access to a highly sought-after platform, with consequent publicity.
You may recall that I have some interest in media hype and amateur scientists. In Ireland, a great deal of publicity was given to an amateur scientist who had strong anti-relativity views. 15 years later, we're still trying to clear up the confusion he caused

69. **Peter Woit**
June 7, 2013

Hi Cormac,

Yes, the Weinstein and Mersini-Houghton cases are not quite the same, but I think the issue of the amateur/professional and published/unpublished nature of their work isn't what's really important. In both cases, you have press stories promoting things which have very little support from experts, and this is something to be legitimately concerned about. In these two cases, I think one of the two is far more damaging: it simply is not true that the Planck satellite data gives "hard evidence" for the existence of a multiverse, and the press is being used to mislead the public on a central issue of what we have scientific evidence for and what we don't. That two Guardian blog posts (evidently these didn't appear in the newspaper) describe some theorist as having promising speculative ideas when they may not be that promising just isn't a problem on the same scale. It remains remarkable to me how little anyone in the physics community seems to care about the public being misled about what data says about the origin of the universe, while getting so excited about the fact that a theorist without proper credentials was getting some media attention.

70. **Anonyrat**
June 7, 2013

The way I see it is – is there information available for me to learn more about Weinstein or Mersini-Houghton, and the answer is for Weinstein – No, and for Mersini-Houghton – Yes; and that is the problem.

Snowmass on the Pacific

June 1, 2013
Categories: Uncategorized

Due to popular demand from the comment section, I spent some time this afternoon taking a look at the talks now posted from the KITP Snowmass on the Pacific conference held the past few days. This is part of an ongoing project for a US HEP Community Summer Study that will culminate at a meeting in Minneapolis later this summer. The US HEP community faces serious questions about what priorities for the future should be in an environment of flat-to-declining budgets, no energy frontier projects in the US, and discouraging news from the LHC about no evidence for BSM physics.

The KITP talks cover a wide range of topics, and I haven't had a chance to look at very many of them. For theorists, one interesting session was the Wednesday panel on Structural Issues for Theorists, which featured presentations by and discussions with the people at DOE and NSF responsible for HEP theory grants (Simona Rolli and Keith Dienes). There's a lot of information about the situation of US theory grants there, but I was a bit struck by the impression that despite the large problems faced by HEP in the US, for theorists things look much like they always have:

- Budgets are pretty flat. As salaries go up with inflation, and as new young people come into the system applying for grants, it's harder and harder for people to get the grants and grant amounts they would like.
- The split between DOE and NSF funding of often exactly the same thing doesn't make much sense and leaves people sometimes confused.
- There's always a problem finding grant support for the number of students who want to do theory, leaving theorists trying to justify to their colleagues why their students should get more of the few TA positions available. The situation of funding about 180 theory Ph.D students in an environment where there are maybe 10 tenure track jobs/year in the country isn't deemed even worthy of comment.

Some numbers from the presentations:

- The DOE spends roughly \$25 million/year on positions at government labs, another \$25 million/year on grants to universities (the bulk going to pay grad students/postdocs/summer salary). The NSF spends \$13-14 million/year on grants to universities, another $6 million on Frontier Centers, some of which have an HEP theory component.
- DOE funds 49 PIs at labs, 221 PIs at universities, NSF funds 186 PIs at universities.
  The DOE split by field is 128 Phenomenologists, 73 "Formal" (often "string theory"), 42 Cosmology, 27 Lattice Gauge Theory.
- DOE funds 123 postdocs, NSF funds 50 of them. DOE funds 130 grad students in theory, NSF 50 of them.

This stable system of government funding has been crucial in determining the structure of HEP theory in the US for the past few decades, and the academic system is built around it. I keep wondering what the effect will be as new sources of money come into the system from private sources, on scales approaching that of government funding. As a somewhat extreme example that is likely a one-time thing, last year the string theorists in Princeton got \$15 million in checks from Yuri Milner, a number somewhat larger than the entire NSF $13-14 million/year budget for HEP theory.

Comments

1. **sotp**
   June 1, 2013

   'Snowmass on the Pacific' … a preparation for 'Snowmass on the Mississippi' … I suppose it's an inconvenient truth that one of the consequences of the flat/declining budgets is that 'Snowmass at Snowmass' is now too expensive?

2. **MathematicianNotPhysicist**
   June 1, 2013

   `As salaries go up with inflation, …'
   Is this a prediction, or has this phenomenon actually been observed?

3. **David Nataf**
   June 2, 2013

   In an equilibrium situation where the system was not growing and every PhD got an academic job, professors would have an average of one graduate student over the course of their entire careers.

   In contrast, a typical professor has 2 or 3 graduate students… at any given time.

   Food for thought.

4. **Thomas Larsson**
   June 2, 2013

   That there are 20 times as many PhD students as tenure-track positions is not the problem; that people expect to stay in academia afterwards is. To be given the opportunity to complete a PhD and perhaps a post-doc in theoretical physics is really a wonderful gift from the taxpayers, for which one should be grateful. At least I am.

5. **talks**
   June 2, 2013

   The information on funding by DOE and NSF is interesting but, in principle, it is not something not 'already known'. What is there in these talks that really helps to 'prepare' for SOTM?

6. **Michael**
   June 2, 2013

   180 theory Ph.D students in an environment where there are maybe 10 tenure track jobs/year in the country

   Is that particle theory only?

   I wonder, what eventually happens to 170 – cooking meth can't be really a career choice for theorists, besides, Dr.White already cornered that market…

7. **DTA**
June 2, 2013

If one wants to be hard-nosed about it, the optimum ratio of students to jobs is a balancing act between missing too many potentially top-notch scientists by taking in too few students, and spending unnecessary money on additional students without noticeably increasing the potential of an eventual prize catch. As far the fact that a number of students will have to go into alternative careers is a choice they can make for themselves, if one accepts that people are responsible for the choices they make.

Of course, if a professor only gets to have one student in his or her career, it will be harder to justify even 10 new faculty openings per year in the field, given that universities supposedely teach, but that's a next-order problem. Perhaps a system could with more research positions and fewer faculty ones.

8. **Shantanu**
June 2, 2013

Peter, also interested in yours(and other's opinions) regarding teh talks on supersymmetry and grand unified theories.
shantanu

9. **Alex**
June 2, 2013

There are probably 10 jobs/year advertised specifically as HEP theory research. However, people who get PhDs in HEP theory will do other things as well. Some will get jobs in teaching-oriented institutions; some of those jobs are advertised as HEP theory but some are advertised more broadly. They still use their training. A few will get various types of staff scientist jobs at government labs. A few will take their big physics brains and pedigrees and go into finance or consulting. A number of them will take their computational and mathematical skills into some sort of applied science work.

All of that is fine, and in my opinion society is well-served by the outcomes of their training. (Well, except maybe the consultants.) I don't consider it a "waste" that they went into "alternative" careers.

The problem is that a non-trivial fraction of them will get stuck in various permadoc/soft-money stints, or adjunct stints. That's fine when you're starting off, but it's a poor foundation for an adult life and career. Maybe this is their fault, because they didn't prepare themselves to branch out. They stayed very narrowly-focused and never prepared themselves to leave. Or maybe it's the fault of some advisors and "mentors" who actively or passively discouraged looking at anything outside of their academic niche. And there's a system that enabled this.

I think that a lot of what NSF talks about with training and broader impact is fuzzy and silly, but I would not fault them if they started requiring that institutions getting funds for trainees in basic research also get some meaningful assistance in preparing for "alternative" careers. NSF should look favorably on departments that have seminar speakers from industry, that encourage their PhD students to take applied classes (e.g. computational methods, some engineering classes, materials science, other applied things related to physics) after finishing their qualifying exams, that have meaningful networking with alumni in industry (beyond something passive like a LinkedIn group), and generally have an atmosphere that says "You are doing fundamental research as a way to train your mind, now pay some attention to what you'll do next."

10. **train your mind**
June 2, 2013

Actually, much of the preceding post applies quite generally across all of physics graduate research, not just HEP theory. It is a valid post, but not restricted to HEP theory.

11. **Amitabha**
June 3, 2013

Regarding the 10 jobs vs 180 students: many of the students are from outside the US, and many return to their home countries after PhD, or after one or two postdocs.

12. **Richard**
June 4, 2013

Re the 180 people and 10 jobs. This year the rumor mill lists 17 "faculty" positions — albeit some in Canada. Moreover, that 180 is presumably a stock, not a flow; given an average of 3 years in the "research phase" of grad school, that suggests supply outstrips demand by a factor of 3 or 4, not 18.

Some of those 180 people will fail to finish, others will finish and go to work for hedgefunds, or Google, or a start-up, or teach in a 4 year college, or take time out for family… I have friends, collaborators and former students who have done all of these things — the HET jobmarket is no bed of roses, the following statement

The situation of funding about 180 theory Ph.D students in an environment where there are maybe 10 tenure track jobs/year in the country isn't deemed even worthy of comment.

is massively wrong; certainly by a factor of 5, and perhaps by a factor of 10.

13. **Peter Woit**
June 4, 2013

Richard,

The statement is accurate, your interpretation of it is wrong: obviously the 180 students on NSF/DOE grants aren't all graduating in one year. Your estimate of "3 or 4" for the ratio of phds/year to jobs I think is quite wrong. Some things to take into account:

1. Final numbers from the rumor mill for the past four years of faculty hires are 9,14,11,11, see here:
http://www.physics.utoronto.ca/~poppitz/Jobs94-08
This is post 2008-recession, and hard to know what the future holds, but I don't see US universities expanding greatly in coming years. Looking at data back to 1994, the 1990s had numbers in the low teens, for a while in the early 2000s the best job market in decades got up to 20, over the last twenty years the average is probably 15 or so, with 11 the most recent number.

The 17 number you give includes not only Canadian universities, but all listed jobs, not all of which get filled, and some of which end up going to non-HEP candidates.

2. The 180 number is just those in the US on DOE or NSF HEP research grants. It doesn't include theory students with some other kind of funding (TAs, other research fellowships). Depending on how you count, I'd guess around 300 Ph.D. theory students. There was a 1997 HEPAP census of US institutions on DOE/NSF grants, now very out of date, which found (with 99/120 institutions responding) 283 theory students (3rd year and above), and 78 theory Ph.Ds /year. See here
http://pdg.lbl.gov/hepap/hepapsurvey97.pdf

I'd be very curious to know if anyone has numbers on what the trends have been since the 1997 census. Speaking of Canada, one thing that didn't exist in 1997 is Perimeter, where it seems that there are about 35 theory Ph.D. students now.

All in all, I think a current estimate of about 10 to 1 for theory Ph.Ds/job is reasonable, with this having been somewhat better during the early 2000s, but prospects for the future not better. Telling students that the ratio is 3 or 4 to one would be really irresponsible.

On the larger issue of whether this is a good thing, that has been debated here often in the past. I'll just point out that with numbers like this it's hard for HEP theorists to legitimately make a case to the NSF/DOE that there is too little theory graduate student funding. I'll also point out that I've seen up close for many years the job market in mathematics (healthy) and in HEP theory (unhealthy), and the difference is huge. Why people in HEP want to defend living with such an unhealthy situation I don't think I'll ever understand.

14. **Dennis**
June 4, 2013

A fair percentage of our graduate students in particle physics are from foreign countries. With US training, they often have good job opportunities in their home countries, which are not included in the 10 per year US jobs. There is also the military-industrial job market as well as the medical market. There is a projected shortage of big data analysts with "deep analytic talent" projected for 2018 of 140,000 to 190,000 people. These jobs currently start at over $100,000 for new graduates. The above discussion of about 80 theory Ph. D.s a year is totally swamped by the number of available positions in the technical job market.
Certainly, we will miss theorists that are highly trained in research directions that should be pursued, but as a training ground for technical people, our field is excellent.

15. **Peter Woit**
June 4, 2013

Dennis,

I'd be wary of promising entering grad students that they'll have no problem getting a $100K+ job data science job when they get their Ph.Ds or their postdocs end 5-10 years down the road, given that every major university is starting up graduate programs specifically in data science right about now.

But the point really isn't that theory Ph.D.s will end up unemployed or working as baristas; these are smart, talented people who will find some sort of better than average employment doing something or other. The point is that there is a big difference in environment between a field where good young people see a future and the senior people working with them see them as future colleagues, and one where they are just cannon fodder, with everyone aware that unless they are both exceptional and lucky, they're on their way out the door at one speed or another. I've seen both environments up close and I think describing one of them as healthy and the other as unhealthy is pretty accurate.

16. **Richard**
June 4, 2013

There are an infinite number of ways to slice this. But comparing the number of people in grad school (or a proxy for it) to the number of faculty slots is misleading.

Looking for some real data, found this

http://www.aip.org/statistics/trends/highlite/edphysgrad/figure7a.htm

which gives "particles and fields" as 208 students graduation / year in 2007 and 2008 (out of 1480). "Particles and fields" casts a fairly broad net and includes experimentalists while the Rumor Mill is incomplete in a variety of ways (people who take jobs internationally, or outside of the rumor mill's scope but still in science) a reasonable guesstimate would be to say that 100 theory students compete for 20 jobs. Which is closer to the my "3 or 4" than it is to your (implicit) 18.

17. **Peter Woit**
June 4, 2013

Richard,

There never was an "implicit" claim by me of a ratio of 18 people/job, that was just your (completely unreasonable) misinterpretation of what I wrote. The only estimate I've given of this ratio is about 10, you've now moved up from "3 or 4" to 5.

We seem to agree on the number of people (100/year). Yes, there are many ways of defining and counting what sort of jobs provide a research career in HEP theory. I still think the 11/year Rumor Mill count is a better estimate than your 20, note that this overcounts permanent positions (some people don't get tenure or leave the field from tenure-track jobs). Yes, US educated students get research jobs outside the US, but students educated outside the US also get US jobs. In any case though, whether it's 90% or 80% of theory Ph.Ds that have no future in the subject, that doesn't much change the picture.

18. **piscator**
June 5, 2013

Just a quick comment on this. The chance of ultimately getting a faculty job also obviously depends a lot from institution to institution: students from Princeton or Harvard or Stanford have far better odds of ending up with a faculty job somewhere than those from Kansas State, and I think people know this from the outset. The former probably have odds a lot better than 1 in 3/4 (provided they are willing to stay in the system and do not self-restrict to top tier research universities), and the latter odds a lot worse than 1 in 10.

I think Erick Weinberg has roughly this data on his faculty jobs page.

19. **David Nataf**
June 5, 2013

Richard,

It is peculiar that you are promoting a rosy view, why? Things are in fact not rosy. Having numerous talented young people get Ph.Ds in HEP, and then sending them to goldman sachs or JP morgan sachs to develop high-frequency trading algorithms is not, in my view, an optimal outcome. I just don't see how you could be stating this to be satisfactory. It's not why people start PhDs in science, and if that was the final goal, there would be better training for it elsewhere than in physics.

I don't think academia would be well-served by transitioning to a medical school model, where it would be virtually impossible to get into graduate school, but there would then be a guaranteed job for every who gets in. However, an intermediate solution, wheresay some ~50 of the 180 graduate student positions across the country are gradually replaced by 15-20 permanent staff scientist positions, should be contemplated as a positive step forward.

20. **Peter Woit**

June 5, 2013

piscator,

I'm guessing you're referring to the data gathered by Erich Poppitz (not Erick Weinberg) that's in

http://www.physics.utoronto.ca/~poppitz/Jobs94-08

It shows most jobs going to people from a small number of places (Berkeley, Princeton, Harvard, Stanford), with those places placing about one person/year. I don't have any numbers, but I'd guess those places produce something like 3 theory Ph.D.s/year. So, if you're a student from such a place, a one in three chance of getting a Rumor Mill type job may be a good estimate.

21. **Mark**
June 5, 2013

I think there are too many PhD students – in my field (experimental HEP) the number of students in the country I come from has been doubled compared to say 10 years ago – yet there are no more faculty jobs than 10 years ago available. Furthermore I don't see that a lot of these students are actually anything more than cannon fodder to do data crunching for PIs – the point of a PhD should be to develop your own idea in my opinion, and this is certainly what I was told to do when I did mine – but nowadays this no longer seems to be the case in a lot of cases.

I would reduce the number of students and instead use the money to provide a better career structure for those who do stay in the field post-PhD. Then you would have a more stable pool of experienced and competent people, and would in principal have a better pool of students coming into the system (I come across students regularly who I can honestly say have no place in a PhD program because they are just not capable – so if we can afford to fund them this is an argument we fund too many places). Furthermore with a better career structure you would be more likely to entice good people to stay on (many very good students in my group left academia straight after their PhD because of the poor career prospects and I don't blame them for doing it).

22. **Richard**
June 6, 2013

I don't think I am "promoting" an overly rosy view; I am genuinely interested in this question, and trying to explore the problem. There are certainly large uncertainties in the data if one can argue for prof:grad ratios that vary by a factor of 2 — and it is equally plausible that some people are motivated to make things look as dire as possible.

Piscator's point that outcome depends very strongly on where you get your PhD is very well made, and it is clear that a handful of PhD programmes are producing a disproportionate fraction of the professoriate in the US. (My own PhD institution is one of the singletons on Poppitz's "PH.D. INSTITUTION OF FIRST-TIME FACULTY HIRES" plot — one faculty member in North America in nearly two decades.)

23. **Dudley**
June 14, 2013

As a passionate student considering a PhD in fundamental theoretical physics, between this and string theory's dominance, it is quite demotivating. From my fairly uninformed point of view, I have a hard time seeing any progress beyond the standard model until our experimental capabilities greatly improve, and an easy time seeing people waffling like they are doing something useful. I hope I am wrong.

Unless you work on a project specified by someone else in a subject you aren't interested in (or even believe in), you aren't going to be able to afford food. Cheap food. Again I am probably wrong, but this is how it feels.

I would agree that taking on less PhD students would be wise. Certainly a lot of my colleagues simply should not be doing physics PhDs, to the point of cringing.

I really have no idea what to do!

24. **Mitchell Porter**
June 14, 2013

Dudley: the standard model has several dozen unexplained features; there are numerous phenomena which require something beyond the standard model; for all these facts, there are numerous ideas about how they might be explained, but the field is still open for anyone with a new idea to voice it as well; and the mathematics and ontology of the subject also still needs work. So there's no shortage of things to do.

25. **Anonymous**
June 24, 2013

Dear Peter,

As a (mathematically inclined) theoretical physics aspirant, I am extremely curious about your comments about the job scene in mathematics. In what way is the mathematics job scene much better ? Is that mainly an artefact of over-supply of particle theory graduates per permanent position available or is there a greater supply of mathematics permanent positions (again, why ?) ? Does it have something to do with the style of work in each community (carefully substantiated and original in mathematics, versus speculative, flashy and "faddish" in high-energy theory) ?

26. **Peter Woit**
June 24, 2013

Anonymous,

The main difference in the math academic job market is just that there is a much better ratio of Ph.Ds to jobs than in particle theory. Just about every higher educational institution does a lot of calculus and other math teaching, so needs a sizable math faculty. In contrast, most places that have physics departments typically have a fairly small number of theorists.

There's a separate issue of faddishness, with the particle theory phenomenon of everyone jumping on the same hot topic and trying to write papers about it pretty much unknown in mathematics. This used to have some justification in particle theory, when experiments were producing unexpected results, pointing to what might be the most fruitful thing to work on. In recent decades, there has been little of this from the experimental side, but the theorist's behavior is deeply ingrained culturally.

All in all, math research is a much healthier community. If you are interested in the boundary of math and physics and trying to decide which area to pursue graduate work in, math has a lot of advantages at this point.

Simons 75th Birthday Conference

June 3, 2013
Categories: Uncategorized

Last week the CUNY Graduate Center hosted a conference in honor of the 75th birthday of Jim Simons. It was organized by Dennis Sullivan as a set of expository "mini-courses" on various topics related to Simons' mathematical work. I was able to attend the morning talks, which were of a uniformly high quality, and focused on the Chern-Simons 3d QFT, as well as the "differential characters" of Cheeger and Simons. Video of most of the talks is available online here. In particular, Witten's talk, which was a very simple physical introduction to use of the Abelian Chern-Simons term in condensed matter, is available here (followed by Deligne on Deligne-Beilinson cohomology). Talks by Robbert Dijkgraaf on Chern-Simons-Witten theory are here and here. The second Dijkgraaf talk was followed (go to 1:05) by some informal comments by Simons himself, explaining the history of how Chern-Simons came about. Mike Hopkins gave two wonderful talks, the first about differential characters in general and about his recent article with Dan Freed, the second about his work with Singer that used generalized differential cohomology.

The talks I missed were likely just as good, I'm starting to catch up on them on video now….

Comments

1. **Robert J Frey**
   June 4, 2013

   I worked with Jim for 15 years, most of it as a Managing Director at Renaissance Technologies. As Chairman and President, he not only guided the overall research program but made many seminal contributions himself. Jim was far more than a brilliant researcher. Renaissance's success can be directly attributed to the environment he created. Jim built a place where integrity, respect, and creativity were nutured and supported. All of us bright boys and girls had pretty healthy egos, but it is hard for anyone to play the prima dona when the boss, who managed to think rings around the best of us, was just so damned decent. Jim taught me that the pursuit of excellence works best when put first the people whom you expect to achieve it.

2. **Lloyd**
   June 7, 2013

   Hi Peter,

   I was asking you about a research paper you were working on way back in 2008:

   http://www.math.columbia.edu/~woit/wordpress/?p=673&cpage=1#comment-36511

   You said you are still working on it, did I miss anything?
   Or is it already posted/published somewhere?

   Best wishes,
   Lloyd

3. **Peter Woit**
   June 7, 2013

   Lloyd,

   See this

   http://www.math.columbia.edu/~woit/brstdirac.pdf

   I've been intending to get back to work on this, for the past year have been working on the notes posted here

   http://www.math.columbia.edu/~woit/QM

   Hope to finish this project this summer, then get back to the BRST/Dirac Cohomology project.

4. **Lloyd**
   June 11, 2013

   Thanks Peter for the links!

   The cohomology project seems very interesting, but the date on the notes is 2009!

   Why not go ahead and publish whatever you have now? You can always submit follow-ups later. I guess this would put to rest the assertion that you do not actually do any fundamental research.

   Best,
   Lloyd

5. **Peter Woit**
   June 11, 2013

   Lloyd,
   The paper is on my web-site, I wrote about it on the blog, it should show up quickly if anyone googles for information on that topic. Honestly, very few people seem to be interested in it. I definitely need to get back to work on the paper, produce a more complete version including what I've learned in recent years, and that I'll likely publish somehow, or certainly make a serious effort to get others interested in.

   For now, I'm working hard on the book, and that actually should be very helpful for the Dirac cohomology/BRST paper when I get back to it. There are a host of tricky technical details I hadn't gone through that writing the book has made me sort out. So, maybe a draft of the book will be done this summer, new and improved paper this fall. We'll see..

Nature at the energy frontier

June 12, 2013
Categories: Uncategorized

Last week a symposium on Nature at the energy frontier was held at the ETH in Zurich, funded by the Latsis foundation. Videos of many of the talks have appeared here.

As part of the symposium, David Gross gave a public talk, available here. Gross is often invited to give such talks, and I've either attended or taken a look at the video for quite a few of them. The first substantive posting on this blog back in 2004 was about one such talk. Back then, Gross was claiming that in 3-4 years there would be a headline in the New York Times about the discovery of supersymmetry at the LHC (he was overly optimistic about how long it would take to get the machine working properly). I wondered at the time:

> What will be interesting to see will be what Gross et. al. do when this doesn't happen. Will they drop string theory?

In all of the talks I've seen since that time, Gross continued to express optimism, including willingness to bet significant sums of money on a discovery of superpartners at 50/50 odds. His talk at the Latsis symposium included a remarkable change of tone, featuring an Einstein quote I hadn't seen him use before:

> The successful attempt to derive delicate laws of nature, along a purely mental path, by following a belief in the formal unity of the structure of reality, encourages continuation in this speculative direction, the dangers of which everyone vividly must keep in sight who dares follow it.

He describes Einstein as both encouraging the kind of speculative path followed by string theory and SUSY, while at the same time warning of its dangers, and noted that Einstein himself devoted the latter part of his life to a speculative path that turned out to be a dead end.

In his explanation of the standard arguments for string theory and SUSY, Gross was much more cautious than in the past, careful to explain that these arguments were based on just speculative "clues". These clues might just be coincidences, but the main reason for not giving up on them was that we don't really have any others.

About the LHC results, Gross described them as having now ruled out the simplest SUSY models, which could be a clue that SUSY does not exist. He said that if SUSY is not seen at the LHC, we will learn for sure that theorists have been on the wrong track for many decades. According to him, this would "change a lot of our way of thinking", since "we have been pursuing these clues for a long time." He didn't discuss how ways of thinking will change, although he is well known to strenuously reject one popular idea about this (anthropics and the multiverse).

Erik Verlinde gave an odd talk with a title that promised to address these issues, String Theory and the Future of Particle Physics. He explained the history of string theory and how the original hope was that it would tell us where the SM parameters came from, something which "looks differently now". According to him, the idea of strings moving in a compactified 10d was the "old view", now made obsolete by branes. The "present view" is all about gauge-gravity duality, which doesn't say anything about those SM parameters. According to him the "future view" of string theory will somehow start from some new basic principles that we don't know yet, providing an underlying microscopic description that will explain holography, and give an "emergent" explanation of space, time, matter, etc. There was no explanation at all of what these new principles were or what the new microphysical description would be. String theory itself would just be an "effective theory", like the Standard Model.

After this introduction, Verlinde than moved on to something that seems to have nothing at all to do with string theory, giving a long explanation of how he thinks there are some new degrees of freedom out there that have dynamics at long time scales. These should be treated by the methods of polymer dynamics, and they will explain dark matter and dark energy. He produced lots of graphs of astrophysical data, and claimed that he has an "extension" of Newtonian gravity, better than MOND, which explains the astrophysical data. At the end of the talk there were a bunch of questions about the cosmological implications of this, I couldn't tell if Verlinde had an answer. He has been talking about this idea in public talks for a couple years now, although as far as I can tell there is no paper. Also, as far as I can tell, no one else besides him takes this seriously.

The last talk of the conference was from Nima Arkani-Hamed, giving basically the same talk I've written about recently here and here. I confess to not listening to the whole thing, since it was quite familiar (although I did notice he addressed the question of the possibility there wasn't really a hierarchy problem, saying people who raised that were "frustrating", since he had thought about it for decades, and "trust me, there's a hierarchy problem"). In the question session, he made the same point I often end up arguing with string theory proponents about, saying (1:14) that if "you can do experiments at the string scale, wouldn't help you at all". The idea that you would see string excitations on a compactified space he characterizes as a misguided old idea from the 1990s. If there's a landscape, the possibilities are so complex for Planck scale behavior that you can't predict what experiments at that scale would see. I'm glad to see that now instead of getting into arguments like this one, I can send people to go argue with Nima.

Comments

1. **lun**
   June 12, 2013

   A historical comment: You, and others, have often emphasized that this disconnect between theory and observability, and acceptance of this disconnect, is unprecedented in the history of science.

   In fact, hydrodynamics provides a very relevant historical example: In the 18th century it was realized that ideal hydrodynamics was a very lousy description of the behavior of fluids, even fluids with a very low viscosity ("D'Alambert's paradox", the force on a cylinder undergoing laminar flow, is a textbook example).
   The reaction of the scientific community was pretty much the reaction of supersymmetry and string theory practitioners today:
   ————————-
   http://en.wikipedia.org/wiki/D%27alembert%27s_Paradox
   D'Alembert, working on a 1749 Prize Problem of the Berlin Academy on flow drag, concluded: "It seems to me that the theory (potential flow), developed in all possible rigor, gives, at least in several cases, a strictly vanishing resistance, a singular paradox which I leave to future Geometers [i.e. mathematicians – the two terms were used interchangeably at that time] to elucidate".[4] A physical paradox indicates flaws in the theory.
   Fluid mechanics was thus discredited by engineers from the start, which resulted in an unfortunate split – between the field of hydraulics, observing phenomena which could not be explained, and theoretical fluid mechanics explaining phenomena which could not be observed – in the words of the Chemistry Nobel Laureate Sir Cyril Hinshelwood.[5]
   ———————————
   The best mathematicians in the world continued to study formal, beautiful, and irrelevant solutions of inviscid fluid dynamics, and the reason for the world being grossly different was not paid attention to until the beginning of this century.
   Substitute "supersymmetry" for "ideal fluid dynamics", and you get today's impasse.
   The work on AdS/CFT and amplitudes reflects very well the theorist's response to disappointing experimental results.

   In retrospect, was it a mistake? Certainly we learned a lot of mathematics techniques from these solutions, which are now used elsewhere.
   On the other hand, the _reasons_ the real world is different (which have to do with turbulent regions which form at low viscosity) are equally, if not more, fascinating and rich in mathematical insight. On the "third hand", these reasons are also "hard", we still have not completely understood, so it is difficult to see whether "extra effort" at

that time would have made much difference.

2. **Peter Woit**
June 12, 2013

lun,

Interesting analogy. One difference though is that here we already have a wonderful mathematical theory that works (the Standard Model).

3. **David Metzler**
June 12, 2013

On an older topic, your more mathematically interested readers may want to go to terrytao.wordpress.com and click on the polymath8 link to see recent progress in improving Zhang's bound on prime gaps (now down to around 250,000 instead of 70,000,000.) You can watch mathematics in action, in almost real time, and participate yourself, if you know your stuff.

4. **Peter Woit**
June 12, 2013

Thanks David,
That is an amazing project, I was thinking of finding some way to write about it. Shows the power of crowd-sourcing in action, when your crowd includes Terry Tao…

5. **A.J.**
June 12, 2013

I'm not sure I'd describe the Standard Model as a wonderful mathematical theory. The computations we can do work nicely, of course, but the Standard Model is not a well-defined mathematical model in the way that hydrodynamics is.

In hydrodynamics, it's very clear the mathematical model is, i.e., what the degrees of freedom and observables are, and what rules govern their dynamics. The big questions mainly concern whether solutions to these dynamical rules exist for various sorts of boundary conditions.

In the Standard Model, on the other hand, it is _not_ clear what the mathematical model is, let alone which boundary conditions play well. We would like the Standard Model to be a continuum quantum field theory. Typically we specify these by either simply just writing down some observables and their OPE coefficients (the "divine inspiration" method) or by showing that some system of regularized Feynman integrals have a well-defined continuum limit.

To the best of my knowledge, we don't have even a physically satisfactory candidate for the set of regularized Feynman integrals. Note that I'm not just complaining we can't take the continuum limit with perfect mathematical rigor. What I'm saying is that every regulator I've ever heard of breaks some crucial feature of the Standard Model: gauge invariance, Lorentz symmetry, locality, or chiral symmetry.

To make matters worse, there's no evidence that any regularization would have a sensible continuum limit, thanks to the running couplings in the Higgs & U(1) sectors. (You can argue that there's no Landau pole if you want, but the burden of proof is on you. And the numerical results aren't exactly encouraging.)

Sorry to be such a downer, but I don't think it's particularly fair of you to take a maximally pessimistic viewpoint on string theory's mathematical structure (in the linked MathOverflow post) while advertising the Standard Model as 'wonderful'. The SM is far more successful as physics, but mathematically, it's in terrible condition. I'm not even sure it's likely that the SM is anything more than a set of recipes for making approximate computations in some unknown model.

6. **Peter Woit**
June 12, 2013

Hi A.J.,

I know basically nothing about hydrodynamics, or the associated mathematical models, other than that there's a million dollar prize for understanding something about the solutions of Navier-Stokes, a situation that doesn't look that different than the million dollar prize for showing there's a mass gap in pure Yang-Mills (which as you know well can be rigorously defined on the lattice).

In what I wrote at mathoverflow, I was trying to give not something maximally pessimistic, but an accurate explanation of what the status is of the attempt to give a definition of "string theory". As far as I know, the statement that no one knows how to define string theory outside of perturbation theory (i.e. M-theory) is completely accurate (other than as a conjectural beast that satisfies certain consistency conditions and has various known limits). People really don't know what the right fundamental variables and laws are for the theory, even at a very formal, conjectural level. I'd be interested to see a good reference if this is not accurate.

The situation with the Standard model, as a mathematical structure is a complex and fascinating one, and I know you're well aware of this. While a rigorous formulation with proven properties is lacking, formally you can write the thing down, and a lot is known about how to make sense of the formal equations. I think there's a glass half-full/half-empty situation here. To me the SM has a huge amount of precise and very deep and powerful mathematical structure and we know in principle how to do accurate calculations with it, although there are fascinating remaining issues about how to get a rigorous definition one can prove things about. To me that's a glass more than half-full, but tastes can differ.

Yes, you can say many similarly precise things about perturbative string theory, but the current lore (see Arkani-Hamed's comment) is that that's not what's relevant to the real world. I don't believe you can produce a realistic (e.g. with the right CC) string theory model purely defined using perturbative string theory. Typical conjectured realistic models use an effective potential for moduli that get stabilized through complex mechanisms, invoking extremely poorly understood and conjectural aspects of non-perturbative string theory. Again, here you don't even know what the right fundamental variables or dynamical laws are, even formally.

7. **cthulhu**
June 12, 2013

I am merely an interested bystander wrt theoretical physics, but I know a fair amount about fluid dynamics (a somewhat more correct name than hydrodynamics). I think that A. J. is off base regarding the well-definedness of fluid dynamics. The overriding problem in fluid dynamics for many, many years has been the modeling of turbulence – there are more degrees of freedom there than you have equations, so the history of accurate solutions (almost all numerical) to the Navier-Stokes equations is the history of turbulence models, and their agreement with experiment is still not that impressive.

Also, for the historical record, the key insight that bridged the gap from D'Alembert's potential flow theory to modern understanding of fluid dynamics was Prandtl's boundary layer approximation. Any analogy between boundary layer theory in fluid dynamics development and quantum electrodynamics in the development of modern physics?…

8. **A.J.**

June 12, 2013

Cthulu,

Thank you for the correction. That's a lot worse than I'd realized.

Peter,

I'm not sure it's a glass half full/half empty situation, as far as the mathematics goes. (I see it as closer to 40/60. 😉 I don't think the situation with the Standard Model vis a viz formal existence is much better than the one in string theory. This is, of course, a subjective opinion, and to do purely with the mathematical merits, not the physical ones.

With the Standard Model, we can write down a classical version and we can write down a free field theory which ought to be the zero coupling limit. This can be done with basically arbitrary amounts of rigor. But when we talk about the Standard Model, we're asserting that there exists a interacting QFT which deforms this things. This is a very sketchy definition, even at a physical level! It's not a completely absurd suggestion, of course. Mathematicians have shown that such deformations can exist in some situations. (But they've also showed that they can't exist in others: There aren't a lot of interacting scalar QFTs in higher dimensions.) And there are subsectors where we can use perturbation theory or monte carlo integrals to make approximate computations.

But this supposed deformation should be regarded with at least a little skepticism. The landau poles suggest it doesn't exist. And the phenomenon of confinement tells us that the deformation is quite singular; the set of observables changes so much that a mass gap appears! It's not really clear precisely what expectation values we're computing; we have to actually write down a set of correctly dressed observables for the IR. Likewise, the troubles we have with regularizing chiral gauge theories (and generally with dealing with fermion determinants) suggests to me that we're missing important parts of the story. We really don't have a completely plausible description of the full dynamics yet. And without a sufficient understanding of the dynamics, we can't _quite_ say what the observables are. (Thus the large cash prize for existence and mass gap in the only interacting subtheory where we think we might have a satisfactory definition.)

The mathetical status of string theory looks analogous to me. They've got a number of quantum mechanical theories (the 5 string theories, free 11d supergravity, large N quantum mechanics, ABJM, AdS/blahblahblah) which they say are degenerate limits of some unknown quantum theory. They can study these theories in infinitesimal neighborhoods, and they assert that bigger deformations exist. They don't have anything as nice as Monte Carlo, but they do have all these BPS & wall crossing computations, which at least suggest that there are paths connecting these limits. (Compare to the SM, where we have pretty good numerical evidence that key parts of the theory, like the field whose interactions give mass to the weak bosons and fermions, can't actually be made to interact in the manner prescribed.)

Analogous but worse actually. I don't intend to minimize the difficulty of the puzzles string theory presents. The degrees of freedom and the dynamics are both generally a mystery. The nonperturbative effects are stronger. And the IR problems that appear when you try to deform the known limits are really gnarly. You thought confinement screwed up your description of observables? Meet black holes.

Lastly, note that I'm not advocating that we not study gauge theokries, especially chiral ones. They're probably my favorite bit of mathematics. I'm just arguing that the Standard Model itself isn't a particularly good example of a gauge theory, chiral or otherwise. The ones that fall out of string theory are far nicer.

9. **Peter Woit**
June 12, 2013

AJ,

The difference as I see it is that in the SM case (modulo problems with chiral gauge couplings to fermions), we have well-defined theories for finite cut-off. We also know a lot about what is likely to happen as we remove the cut-off (for QCD, conjecturally asymptotic freedom tells us this can be done and how to do it, not that we can prove that this works yet…). It may very well be that for some parts of the SM (Higgs, U(1)) there is no way to remove the cutoff and get non-trivial interactions. If so, this tells us something very interesting about physics, that the SM has to fail at some scale (with gravity or some unknown physics coming to the rescue perhaps).

With string theory though, generically non-perturbatively you just don't have anything like this (a well-defined, non-perturbative cut-off theory where you can study the continuum limit). All you've got is some consistency arguments saying maybe something is there.

10. **DimReg**
June 13, 2013

I thought the main difference between the SM and string theory is that we have experimental probes of the SM. That is, we can test the SM against experiments and see if our attempts to make sense of it are at least consistent with reality. With plank scale physics like string theory, we don't have any experiments telling us if it's consistent with reality, so we need more rigor to show that it's at least consistent with itself.

If the SM isn't mathematically consistent in any incarnation, it is at least an effective "rule of thumb" for making predictions about nature. A particle physicist can look at the ill defined nature of the SM and say "at least its predictions are accurate".

11. **Mitchell Porter**
June 13, 2013

AJ said

"Compare to the SM, where we have pretty good numerical evidence that key parts of the theory, like the field whose interactions give mass to the weak bosons and fermions, can't actually be made to interact in the manner prescribed."

What "numerical evidence" are you talking about?

12. **Igor Khavkine**
June 13, 2013

A.J., you've set a very high standard of rigor for the Standard Model (or any QFT) to meet. I would call that a rather pessimistic point of view. However, keeping the same standard, one has to admit that there are no realistic alternatives to the Standard Model that can best it or even match it at its current level of rigor. That is, there is no alternative that is in better shape either. I'm not saying anything that hasn't already been admitted in this discussion. I'm just pointing out that if you choose this reason to feel pessimistic about the Standard Model, then there is no reason to feel any less pessimistic about alternatives.

13. **A.J.**
June 13, 2013

DimReg:

All true, but not relevant. I was talking about the merits of the SM as a particular kind of mathematical model, a continuum QFT. The point is that, while you can do computations and you can say precisely what some of the parts are and vaguely what the whole thing ought to be, you can't write down a completely satisfactory definition, even on the lattice. This is not a problem for the physics, but it's a bit of a nuisance if you want to study the thing as a piece of mathematics. (Compare to the Navier-Stokes model of fluid dynamics, where you can just write down the differential equation and go from there.)

Mitchell Porter:

That comment references the apparent triviality of Phi-4 theory. Wilson conjectured it and it was one of the first things people investigated with computers. What they found generally was a lack of evidence for a continuum limit. The early literature that comes to mind includes Smolensky's thesis and Luscher & Weiss's stuff from ~87. There may be more recent literature contradicting those early studies, but I haven't heard of anything that was universally accepted. Since I'm taking the pessimistic viewpoint here, I'll let others make the argument that phi-4 and U(1) triviality is not a problem.

Peter:

Sure we can define Standard Model-like QFTs, and I'd be happy to bet that they can be made completely rigorous. But the chiral couplings problem and the triviality are real problems for mathematical study of the SM itself.

14. **Peter Woit**
June 13, 2013

DimReg,

As Arkani-Hamed points out, there really is a serious problem with the idea that the problem with string theory is just that you can't do Planck scale experiments to test it. This was only true pre-1995, what Verlinde calls the "old view" of string theory. Once you have decided that you really need to do M-theory, that holography is important, that string theory is going to give you a new idea about what space is, then you no longer have a well-defined, testable framework, at any distance scale. That's the real problem with string theory, not experimental limitations.

15. **lun**
June 13, 2013

Well, the originator of perhaps the leading hope to have a formal understanding of string theory is now working on this:
http://arxiv.org/abs/arXiv:1306.0533
I think its a sign that this particular hope has fizzled

16. **amused**
June 13, 2013

A.J.,

I think your view of the SM is overly pessimistic. The situation is a lot better after the developments in lattice chiral gauge theory in the last 15 years. At the perturbative level everything is fine conceptually and mathematically (I'll explain below), and at the nonperturbative level the thing that remains to be done seems to be "just" a hard technical problem; conceptually there are no apparent problems.

First, regarding what you wrote earlier about the issue of regulators breaking symmetries: This doesn't matter as long as only a finite number of new counterterms (i.e. new terms in the quantum effective action that weren't present in the original bare action) arise due to the broken symmetry. Then you can just include these terms in your bare action to begin with and tune their coefficients to remove them order by order in perturbation theory (and probably also nonperturbatively in a lattice formulation).

Anyway, for gauge theories at the perturbative level with the lattice regularization: this regularization breaks the Euclideanized Lorentz symmetry (after wick rotation to Euclidean spacetime) but preserves a subgroup of it – the hypercubic lattice rotations. Miraculously, this residual symmetry is still enough to ensure that no new counterterms arise. So there is no issue there. E.g. perturbative QCD with lattice regularization is in just as good shape as with dimensional regularization (although in practice computations with the lattice regulator are much more messy).

Now about chiral symmetry: The long-standing problem of formulating chiral fermions on the lattice was solved at the conceptual level 15 years ago. The solution goes under the name "overlap fermions" or "Ginsparg-Wilson fermions" (the name you use depends on whether you would rather be friends with Neuberger or Luscher 😉). This produced a new lattice Dirac operator – the "overlap operator" (hep-lat/9707022, 1000+ citations already) which satisfies the so-called Ginsparg-Wilson relation and therefore has an exact symmetry which can be regarded as a lattice-deformed version of the continuum chiral symmetry (see hep-lat/9802011). Consequently the massless lattice Dirac fermion action decomposes into left- and right-handed parts, giving the chiral fermion actions. Then you can write down the bare action for the SM with the lattice regularization (Yukawa terms coupling the chiral fermions and the Higgs can also be done) and everything is now fine at the classical field theory level.

The remaining issue at the quantum field theory level, in the path integral formulation, is the construction of the integration measure for the chiral fermions. Or equivalently, construction of the chiral fermion determinants. As you know, the massless Dirac operator maps between different vector spaces of chiral fermions (left-handed to right-handed, and vice-versa). So its determinant is only well-defined up to a complex phase factor (which depends on the choice of orthonormal bases for the vector spaces used to compute the determinant). The question then is whether it is possible to find a choice of bases such that the chiral fermion determinant is gauge invariant (this is necessary and sufficient to ensure that gauge symmetry is preserved at the quantum level, which is absolutely essential for renormalizability of any gauge theory). The Dirac operator is coupled to the gauge field, so typically the choice of chiral fermion bases will have to depend on the background gauge field too. More mathematically, the question is whether the determinant line bundle determined by the lattice Dirac operator is trivializable over the gauge orbit space of lattice gauge fields.

The answer should be negative in general due to the well-known "chiral gauge anomaly" – we only expect it to be possible when the chiral fermions live in representations that satisfy the "anomaly cancellation condition". It has been shown that that answer is indeed negative when that condition is not satisfied. (No reference since I don't want to "out" myself ;-).) On the other hand, when the condition *is* satisfied, the situation at present is as follows: (1) Positive answer has been shown for U(1) gauge theory. I.e. we now have a completely satisfactory nonperturbative formulation of U(1) gauge theories with chiral fermions in anomaly-free representations – see hep-lat/9811032. (2) So far a positive answer has not been shown for gauge theories in general, including the SM. It is a hard technical problem, but without any conceptual roadblocks that I can see. Significant progress has been made though – see e.g. arXiv:0709.3658. (3) At the *perturbative* level, a positive answer has been shown for all gauge theories with chiral fermions satisfying the anomaly cancellation condition, including the SM. Specifically, it has been shown that a gauge invariant chiral fermion measure can be constructed order by order in perturbation theory – see hep-lat/0006014. So at the perturbative level the conceptual and mathematical situation for the SM is completely fine.

Finally, regarding the Landau pole issue you mentioned: How would a nonperturbative lattice formulation of the SM deal with this issue? Well, probably the same way that nonperturbative lattice QED deals with it: As the coupling is increased it has a phase transition from a weakly-coupled phase (the physically relevant one) to a strongly coupled "lattice junk" phase. Probably the same thing would happen in a nonperturbative lattice formulation of the SM.

17. **Zathras**

June 13, 2013

A.J.,

You say several times something along the lines that “We would like the Standard Model to be a continuum quantum field theory.” I would suggest that even this premise might be wrong. The scales are always important. The concept of boundary layers can be illustrative here. The physics outside a boundary layer are generally very different than the physics inside a boundary layer. Preliminary findings in quantum gravity suggest a sort of boundary layer in physics at the Planck scale. It is reasonable to keep the regularization condition outside this boundary layer, so that there needs to be no continuum limit. When there exists a boundary layer, perturbative methods do not necessarily need to converge inside the boundary layer, since that is not the area under consideration.

18. **A.J.**
June 13, 2013

Amused:

Thanks for the comments and the references. I hope it’s clear that my pessimism is _partly_ theatrical, as I was criticizing what Peter said on MO about string theory by taking a very grumpy look at the Standard Model. (It’s for example, not really fair of him to say there that nothing is known non-perturbatively. The various matrix theories are … difficult… to make practical use of, but they aren’t totally unreasonable candidates for definition.)

Anyways: I agree that the fermion determinant problems look solvable, but aren’t actually solved. I think the triviality issues are more serious for mathematicians, at least if the game is to construct an example of a particular kind of thing. (Obviously there’s lots of interesting mathematics in just studying Euclidean lattice models.) We have some idea of a continuum QFT is, and what axioms it ought to satisfy. I don’t think we have as clear a picture of what sort of mathematical object an effective QFT is. It’s certainly a much larger class of objects. Even if one can write down a lattice version of the Standard Model, it’s not going to be completely clear what kind of mathematical object you’ve cooked up.

Zathras:

It’s a wrong premise physically, almost certainly. But the point was that the SM doesn’t look likely to fit into any currently known mathematical framework. You’re quite welcome to argue that mathematicians should get to work on finding an attractive definition of ‘non-perturbative effective QFT’. I don’t think that will be a minor undertaking…

19. **amused**
June 13, 2013

A.J.,

Ok, I take it that “not even sure it’s likely that the SM is anything more than a set of recipes…” was theatrical then. 🙂

I do agree though that realistically the lattice formulation (or any other formulation) of the SM will probably never be more than an approximation theory, i.e. will not have a full continuum limit and only able to calculate physical quantities approximately and above some length scale. (I don’t like the term “effective theory” since it implies there is a more fundamental underlying theory, which might not be the case.)
In this regard it’s probably similar to the nonperturbative lattice formulation of QED. But I think such a theory still has “mathematical meaning” though, even though the answers it gives for physics quantities aren’t exact numbers but only approximate. The key thing that the answers, approximate or not, can be derived consistently from an explicit and well-defined starting point. This does look like it will be the case for the lattice formulation of the SM.
And this is already infinitely better than the state of affairs for string theory…although in fairness to the stringers their problem is a bit harder…

20. **Igor Khavkine**
June 13, 2013

Amused,

Any reference for your comments about nonperturbative lattice QED and the “Landau pole”? I’m very curious.

21. **A.J.**
June 13, 2013

Amused:

“I don’t like the term “effective theory” since it implies there is a more fundamental underlying theory, which might not be the case.”

And you think I’m pessimistic… 🙂

22. **amused**
June 13, 2013

Igor,

It was just my expectation/guess. The Landau pole, if it exists, must show up in perturbative QED with lattice reguarization too, so presumably the nonperturbative lattice theory also knows about it and then it will have to deal with it somehow. The simplest (and only?) way I can imagine it dealing with this is by having a phase transition to a nonphysical “lattice junk” phase when the coupling gets large. Such a phase transition certainly exists in lattice QED — existence of a deconfined weak-coupling “physical” phase was shown by Guth in Phys.Rev. D21 (1980) 2291, and existence of a confined, and hence unphysical, phase at strong coupling was shown in Wilson’s initial work on confinement in lattice gauge theories, so there must be at least one phase transition in between. Presumably this has also been verified numerically in lattice QED simulation somewhere… As for whether there has been any work backing up my speculation about the connection between the phase transition and the Landau pole issue, I can’t say… There seems to have been a lot of work on lattice QED in the early days of lattice field theory, but it was long before my time and my knowledge of the literature is very sketchy. Maybe someone else knows about this; I would also be interested to have a reference if one exists..

A.J.,

I also like to indulge in a bit of theatrics sometimes… 😉

23. **Oldster**
June 14, 2013

"He (Gross) describes Einstein as both encouraging the kind of speculative path followed by string theory and SUSY, while at the same time warning of its dangers, and noted that Einstein himself devoted the latter part of his life to a speculative path that turned out to be a dead end."

I suspect Einstein might still reject that verdict of "dead end". He was always inclined to go his own way, and he had a track record such that I certainly wouldn't be the one to try to tell him not to follow his own instincts. On the other hand, his view never dominated physics, or discouraged or crowded out others from following different lines of inquiry. In fact, it was Einstein in the minority, so much so that the "dead end" label was suggested by others even while he lived and rejected that diagnosis (along with Schrodinger). Indeed, after he died, that label of "dead end" was elevated to conventional wisdom so fast, that very few people even tried to follow Einstein's approach to search for a classical unified field theory which can explain quantum phenomena (which Gross correctly notes at 56:30 into his talk, was what Einstein was seeking).

24. **Nick M.**
June 14, 2013

> **David Metzler** *says:*
>
> On an older topic, your more mathematically interested readers may want to go to terrytao.wordpress.com and click on the polymath8 link to see recent progress in improving Zhang's bound on prime gaps (now down to around 250,000 instead of 70,000,000.) You can watch mathematics in action, in almost real time, and participate yourself, if you know your stuff.
>
> **Peter Woit** *says:*
>
> Thanks David,
> That is an amazing project, I was thinking of finding some way to write about it. Shows the power of crowd-sourcing in action, when your crowd includes Terry Tao…

Peter,

Sorry for the off topic update to your "Number Theory News" article published on May 12, but posting in that location is closed. Anyway, New Scientist (on June 4) did an article titled "Game of proofs boosts prime result by millions" containing many interesting links. Apparently, in addition to Terence Tao, most of the recent work on improving the prime gap bound is being done by Scott Morrison and Andrew Sutherland as actively documented on the "Secret Blogging Seminar" both here and, more recently, here. As David as said, you can literally watch mathematics being done in real time. For those wishing to keep abreast of the latest "world record" of the prime gap bound, see the PloyMath Wiki page "Bounded gap between primes". For those trying to work through Zhang's ground breaking proof, the PolyMath page, as linked above, also has a list of frequently updated errata to Zhang's original proof as to be published in a forthcoming issue of "Annals of Mathematics".

Again, sorry for the off topic post.

–Nick

25. **Peter Orland**
June 15, 2013

Amused, A.J., Igor, Zathras, etc.

Forgive a smartass for braying in the middle of your technical discussion.
There are a lot of remarks in your discussion which are important, but need some clarification.

Renormalizability guarantees insensitivity to the ultraviolet cut-off scale or breakdown of the effective theory. As Peter W. points out, in QCD we think the situation is better, and that the cut-off can, in principle, be removed (though we believe that this theory is also cut off in Nature). For this reason, the standard model alone doesn't tell us exactly where it breaks down as a description. Maybe the cut-off is at 100 TeV. Maybe it is at the Planck scale. We can't know any different without experiments.

Forget the one-loop Landau pole. The Landau pole is only suggestive, and should not be taken seriously. The triviality of the S-matrix (as the cut-off is removed) has nothing to do with the Landau pole. Forget the lattice QED phase transition (evidence is strong that this is a 1st order phase transition, hence has nothing to do with continuum field theory).

If you want to understand problems with triviality, etc., the place to start is the Ginzburg criterion from critical phenomena – which gives the physical reason why phi^4 theories need a cut-off in 4D – not the Landau pole. It is in most books on
applications of the renormalization group.

Regards,
P.O.

26. **Anonyrat**
June 15, 2013

Any commentary on this report? Anatole dark matter:
http://www.scienceworldreport.com/articles/7432/20130611/physicists-reveal-simple-theory-explains-nature-mysterious-dark-matter.htm

27. **Marcus**
June 15, 2013

Anapole dark matter (Phys. Letter B, 24 May 2013)
http://www.eurekalert.org/pub_releases/2013-06/vu-stm061013.php
http://arxiv.org/abs/1211.0503
principal author Robert Scherrer

28. **amused**
June 16, 2013

Peter O.,

"Forgive a smartass for braying in the middle of your technical discussion…"

Well I can't speak for A.J. and the others, but personally I can forgive you. 😉

"Renormalizability guarantees insensitivity to the ultraviolet cut-off scale or breakdown of the effective theory. As Peter W. points out, in QCD we think the situation is better, and that the cut-off can, in principle, be removed (though we believe that this theory is also cut off in Nature). For this reason, the standard model alone doesn't tell us exactly where it breaks down as a description. Maybe the cut-off is at 100 TeV. Maybe it is at the Planck scale. We can't know any different without experiments."

We can certainly agree on that. Hopefully there is no contradiction with anything in the earlier discussion, otherwise there's been a misunderstanding.

"Forget the one-loop Landau pole. The Landau pole is only suggestive, and should not be taken seriously. The triviality of the S-matrix (as the cut-off is removed) has nothing to do with the Landau pole. Forget the lattice QED phase transition (evidence is strong that this is a 1st order phase transition, hence has nothing to do with continuum field theory)."

The meaning of "Landau pole" in the above discussion was perhaps a bit vague. My personal interpretation of it is "the possibility that a renormalized coupling diverges when the energy scale gets sufficiently high (or length scale sufficiently small). *If* this does happen to all orders in continuum perturbation theory (e.g. in QED) then it will also happen in lattice perturbation theory. But then the nonperturbative lattice model must also know about it and deal with it. As I wrote above, the simplest (and only?) way for it to deal with this that I can imagine is to have a phase transition to a nonphysical "lattice junk" phase when the coupling gets large. I agree completely with you that this has nothing to do with trying to take a continuum limit and the associated triviality issue. Never said it did 🙂 Also, my understanding of A.J.'s comments is that the Landau pole and triviality issue were mentioned as separate issues, no implication that they were the same or connected.

Thanks for the suggestion on where to start learning more about the problems with triviality. This is one of many topics I would like to educate myself more about in principle, and would if i had infinite time… But in practice I don't feel an urgent need… My current simple-minded understanding of the triviality issue is that there is some place (presumably at a phase transition) in the bare coupling parameter space of the lattice theory where you can try to take a continuum limit, and when you do it the resulting continuum theory turns out to be trivial (non-interacting). My reaction to that is a shrug. It just means that its not a suitable place to tune the bare parameters when trying to describe Nature.

For example, I read somewhere that nonpertubative lattice QED has (or is believed to have) this same triviality issue. But on the other hand, by suitably tuning the bare parameters in lattice QED it should be possible to calculate, e.g., the energy levels of the hydrogen atom to at least the same high precision as in QM + perturbative QED corrections. So the absence of a continuum limit, or of a physically relevant (nontrivial) continuum limit does not mean inability to calculate/predict physical quantities. It just means the answers are approximate (since the values to tune the bare parameters to aren't uniquely fixed, only approximately), and that there is some length scale below which the lattice theory will not give physically sensible and accurate answers. I expect the situation will also be like this for a nonperturbative lattice formulation of the SM.

Regards,
"amused" (David A.)

29. **amused**
June 16, 2013

Having thought about it a bit more, I don't think there's any reason or need for the Landau pole issue (if there is one) to be connected with a phase transition to a nonphysical phase in lattice QED or lattice SM like I wrote above. So I'd like to withdraw that piece of ill thought out speculation. Once we accept that these lattice models can only describe the physics above some length scale, then that's already enough to "solve" any Landau pole issue and triviality issue. E.g. in lattice QED, perturbation theory indicates that increasing the bare coupling drives the lattice spacing smaller, and a Landau pole would manifest itself through the fact that there is a minimum length that the lattice spacing can't get below, no matter how much we increase the bare coupling. (And at some point, for large enough bare coupling, there is a phase transition to a nonphysical strong-coupled phase, so that also limits how much the lattice spacing can be decreased.)

30. **Peter Woit**
June 16, 2013

Anonyrat/Marcus,

I'm no expert on dark matter, but I can say that this kind of story, based purely on a press release from Vanderbilt
http://news.vanderbilt.edu/2013/06/dark-matter/
is the sort of thing one should be extremely skeptical about. I've now seen a very large number of such press releases ("scientists at our institution have solved one of the great problems of theoretical physics"), and can't think of an instance where the claims being made held up to scrutiny. My guess is that the only question here is whether this will ever make the mainstream science press, leading experts to explain what's wrong with it.

31. **Dom**
June 16, 2013

Not directly following from previous comments but I don't see anywhere to draw attention to things that may be of interest. The Observer (Sunday Guardian effectively) has this article where Jim Baggott and Mike Duff discuss "A theory of everything … has physics gone too far?"
<a href="http://www.guardian.co.uk/science/2013/jun/16/has-physics-gone-too-far" Observer Article

32. **Dom**
June 16, 2013

Sorry – got the link wrong should be to
http://www.guardian.co.uk/science/2013/jun/16/has-physics-gone-too-far

33. **Peter Woit**
June 16, 2013

Dom,
Next posting will be about Baggott's book and this debate. Soon…

Nature on the new Nobels

June 12, 2013
Categories: Uncategorized

This week's Nature has an article by Zeeya Merali about various new science mega-prizes, including Yuri Milner's Fundamental Physics Prize. There's also a podcast here, and a Nature editorial here.

I'm quoted in the article, saying about what you'd expect, but in general I was surprised by the extent of the negative reaction to these prizes that she found. Even $3 million winner Sasha Polyakov has concerns, saying

> This new prize is an interesting experiment… Such big prizes could become very influential and they can have a positive impact, or they can be very dangerous.

Frank Wilczek has this to say:

> I don't want to run these awards down, but I find it offensive that people are trying to either borrow the prestige of the Nobel, or buy it…
>
> Prizes are a good thing, but the question is, if your goal is to help science, are large prizes the most efficient way to do that?

Interestingly, Milner counters the criticism that his prizes have heavily gone to string theorists by noting that the award to seven LHC experimentalists this year will shift the balance on the judging panel towards experiment (since awards in the future will be chosen by past winners).

On the whole Merali doesn't seem to have had much luck in getting the winners to reveal what they plan to do with the money. Some of the LHC winners seem to be very aware that they've been given a large check due to the work of others, with Tejinder Virdee of CMS planning to support science in schools in sub-Saharan Africa. I've heard rumors that Maxim Kontsevich is somehow using his award to help others at the IHES, but nothing else about how other theorists will use the money. They are giving public lectures, which are online, see here. After Witten's lecture at Hunter College, the first question was about his plans for the money, but no answer was forthcoming.

The editorial chides scientists for criticizing these new prizes, saying they should "accept such gifts with gratitude and grace". I suppose there would be a lot more of that if the prizes seemed to be helping to support science in general, not just the bank accounts of a few.

Update: At least one wealthy philanthropist has decided to give the millions for theoretical physics to an institution rather than a person. The University of Chicago has announced a $3.5 million gift from an anonymous donor, which will support a new Center for Theoretical Physics to be named after Leo Kadanoff.

Comments

1. **CIP**
   June 12, 2013

   I think that it was Leo Szilard, in the Voice of the Dolphins, who had the protagonist in one of his stories planning to use his vast wealth to retard scientific progress by giving a number of large prizes to scientists. The idea was to get the scientists so busy competing for the awards that they wouldn't have any time to think fundamental thoughts.

2. **David Appell**
   June 12, 2013

   The dilemma seems to me to be that a lot of small prizes, explicitedly for things like sub-Saharan science education, or conference travel subsidies for graduate students, or whatever, do not have the glamour and do not get the attention given to a few big, M$+ prizes, either for science, or (certainly) for whomever is giving out the prizes. That is unfortunate nature of our world. But I think this does put a lot of responsibility on prize recipients to arrange for and distribute the attention and influence given these prizes.

3. **chris**
   June 13, 2013

   our society in general starves the masses to shower the top people with money. why should science be an exception?

4. **Hamish Johnston**
   June 13, 2013

   I agree with Chris, these prizes are another example of the "winner takes all" attitude that was born in the financial sector and now seems to be creeping into the rest of society. I was recently in Waterloo, Ontario where I saw a much more positive example of what one rich person's generosity could do. BlackBerry founder Mike Lazaridis has played an important role in transforming Waterloo from a sleepy university town to a physics powerhouse by establishing the Perimeter Institute and playing leading roles in setting up the Institute for Quantum Computing and a venture capital firm for quantum information technologies. I'd much rather see that sort of investment…but I suppose it's their money.

5. **Chris Oakley**
   June 13, 2013

   There is, of course, a way that those with money to burn, and who want to help fundamental research in science and medicine could make a difference: instead of giving $3m to a handful of burned-out old has-beens, they could give out $30,000 prizes to 100 promising young researchers, much in the spirit of the Gates scholarships to Cambridge, UK, but at post-doctoral level. This also would acknowledge the principle – sadly overlooked by quantum field theorists – that the independent pursuit of different ideas is more likely to get a result than the singleminded focus on one unpromising one.

6. **Ted**
   June 13, 2013

   To answer Frank Wilczek's question, prizes like this are nearly useless in promoting scientific progress. Prizes could be useful if they are awarded for solving specific problems, but the most interesting problems are usually so important that the prestige in solving them is enough to incentivize the profession. For example, did we really need the Clay prizes to get people to work on Riemann's Hypothesis? Further, awarding prizes for past work doesn't incentivize anything. The incentives for producing important work is already so tremendous that the incentives from monetary prizes can only be marginal. Professional prestige is the biggest motivator.

Something like the Milner prize could be useful if given to people with enormous previous success who have experimental ideas and are unable to get funding. Except, this doesn't describe any of these guys – except the LHC team and $3 million is not so different than $0 when it comes to high energy experiments. The only possible use is that these guys can use the $3 million to take a break from teaching, but I doubt what is holding these guys back is a burdensome teaching schedule.

But I don't think these prizes are harmful, I just wish that if Mr. Milner wants to throw away his money, he would gives it to something useful – like a charity.

7. **Bill**
June 13, 2013

I would go a bit further and suggest that all awards and prizes, including Nobel prize and Fields medal, are counterproductive. They upset natural development and natural balance among various areas of research judged purely on their merits by glamorizing some specific accomplishments. This becomes a self-reinforced process since people deciding future awards are often selected among past winners. Some areas become "sexy" while other are perceived as not. This plays a big role in the flow of young talent, for example. Of course, you may say that in the absence of all awards the natural "merits" are rather subjective and political skills will determine the balance of power, but I think this is already factored into reality anyway. I personally would like to see all awards (big and small) disappear and let everyone be judged by the impact of their work.

8. **P**
June 13, 2013

Ted,

Two quick comments:

1) most of the recipients of these prizes aren't teaching that much anyways.

2) your HEP experiment comment is collider biased: dark matter experiments and others that fall under the HEP category according to funding institutions are often on the order of 1-10M. There is continually more push for these experiments at the "intensity" and "cosmic" frontier, and not just the "high energy" frontier, according to the Snowmass distinction. Milner could make a significant dent with his $3M in this regard.

Cheers,
P

9. **Tmark48**
June 13, 2013

Physics prizes should by their very nature be awarded to scientists that have done great scientific accomplishment in physics and I underline physics not pseudo-science, not speculative ideas that have no experimental confirmation.What was good for Einstein should be good also for your physics candidate of the 21st century.

These multimillion dollar prizes are a symptom of "look over there, these experimentalists are getting all the recognition, they are getting all those Nobel prizes (insert curse words for poor Alfred Nobel who wasn't into pseudo-speculative science) we poor theoretical physicists are not recognised for our work. Boooo boooo boooooooo we want a prize. We need a prize." Please, scientific revolutions are not a dime a dozen. So there comes a time when theoretical physicists will be awarded for their theories if they find EXPERIMENTAL confirmation. Otherwise they are just ideas, wrong ideas, nice ideas, leading maybe to new maths but it is not physics.

10. **Pepe**
June 15, 2013

A simple remark. Juan Maldacena donated 200.000 USD to his former institution in Argentina, Instituto Balseiro. This happened some months after receiving the Milner prize, here the info (just in spanish after google it 😉): http://www.lanacion.com.ar/1538630-juan-maldacena-dono-us-200000-al-balseiro

11. **Juan Francisco**
June 15, 2013

Interesting entry! I have always believed, that prizes main values are beyond money:

1) Prestige and influence.
2) Contacts and creation of research groups.

So, really, a prize giving lot of money could not be as good as a low money prize but with "other kind of reward". For instance, I have never seen that, e.g., a very brillian paper or contribution of some unknown person can help him to be selected for certain programme or group. There are "good scientists" that can be "lost" due to "big prize", and equivalently, there are potentially good scientists getting lost for Science due to a lack of (not such a big) cash support.

12. **fuzzy**
June 15, 2013

nobel wanted to apologize for the nitroglycerin, but i wonder what is the psychological motivation of this prize. maybe milner feels that he should have continued studying vague and speculative ideas? gosh, no my friend, please go over! the best physicists can be wrong and you have the right to try your best in the way you like more.

anyhow, in my view chris is right, and i would add the following. the definition of "top people" used in the milner prize is not at all clear, it seems to mix nostalgia and lust for recognizing, rather than caring of true scientific achievements. anyhow, this is a young prize, and we can hope that its target will be improved in the course of the time.

13. **srp**
June 15, 2013

Prizes like the Nobel, where no specific accomplishment is pre-specified, have in principle less power to change the direction of research for good or ill. That means the harm they can do is limited. "Do something that will impress us and we'll give you some money and prestige" is a general encouragement to do impressive things, which seems like an OK thing, and it also calls attention to the work itself and the general character of impressive work which may attract new achievers into areas where they think they can do something impressive.

One problem occurs when people start sussing out in advance what past achievements are going to be rewarded and jockey for credit instead of doing new things. Another is when some anticipated achievement is seen as a shoo-in winner of a future prize and "too many" people crowd into its pursuit. A third is when collective achievements are arbitrarily assigned to a small number of individuals. Milner doesn't seem any worse than Nobel on these criteria.

14. **annamaria**

June 16, 2013

The problem with giving money to Universities is that, unless the money is very well targeted, over half will disappear into paying for the ever burgeoning administrative structures we all suffer from. Much better to pool it in some way and set up a lean research institute(s) focused on providing space and time to think, Perimeter Institute style, and releasing academics from the bureaucracy they are increasingly having to cope with. In many universities, research is something you do in your spare time.

Farewell to Reality

June 16, 2013
Categories: Book Reviews

Jim Baggott has written a very good new book called Farewell to Reality that will soon come out here in the US. It is already out in the UK, where it is stirring up some debate, and perhaps the US will soon see something similar.

In the preface, Baggott explains that he was motivated to write the book by the experience of watching this BBC program, which featured a combination of serious science with revelations about how we're all part of a cosmic hologram, there's an infinity of parallel worlds, and various other examples of what he refers to as "fairy-tale" physics. In the last decade or so there have been a large number of such mass media efforts promoting highly dubious ideas about fundamental physics, and Baggott decided that in his next book he'd try and do something to counter this. I think he's succeeded admirably: the BBC and other such organizations should atone for their sins by sending copies of the book to their viewers.

The book is divided into roughly two halves, with the first half a well-executed overview of the current state of our theories about fundamental physics, from quantum theory through the standard model and cosmology. It ends with a description of the outstanding problems left unsolved by our best theories, and a good summary of the current situation:

> Several centuries of enormously successful physical science have given us a version of reality unsurpassed in the entire history of intellectual endeavour. With a very few exceptions, it explains every observation we have ever made and every experiment we have ever devised.
>
> But the few exceptions happen to be very big ones. And there's enough puzzle and mystery and more than enough of a sense of work in progress for us to be confident that this is not yet the final answer.
>
> I think that's extremely exciting…
>
> … but there is no flashing illuminated sign saying "this way to the answer to all the puzzles". And there is no single observation, no one experimental result, that help to point the way. We are virtually clueless.

With this background he turns to a detailed examination of the speculative ideas that have not worked out, but have dominated the field for the past 30-40 years (SUSY, GUTS, Superstring/M-theory, the multiverse). This is difficult material to do justice to, but Baggott does a good job of giving an explanation of these ideas that includes some understanding of the problems with them. He ends the book with this advice to the reader:

> Next time you pick up the latest best-selling popular science book, or tune into the latest science documentary on the radio or television, keep an open mind and try to maintain a healthy scepticism… What is the nature of the evidence in support of this theory? Does the theory make predictions of quantity or number, of matter of fact and existence? Do the theory's predictions have the capability – even in principle – of being subject to observational or experimental test?
>
> Come to your own conclusions.

The thorniest problems that come up in this sort of discussion are essentially ones about the philosophy of science. What counts as evidence for a scientific theory? At what point does pursuit of speculative ideas that are going nowhere stop being legitimate science? One quickly realizes that naive ideas about the scientific method don't capture how good science really works. Baggott devotes the first chapter of the book to an overview of his take on what the scientific method really is. In the end, this may be the most important issue here: will books and TV programs promoting the views of a narrow part of the scientific community that doesn't want to admit failure end up discrediting the scientific endeavour? Some are all too willing to exploit the subtleties of good science to find a way to defend the indefensible, with the multiverse mania pointing to the all too real dangerous endpoint this can lead to.

For some reviews from the UK of the book see here, here and here.

For a BBC Radio program featuring discussion between Baggott, Jon Butterworth and others, see here. Butterworth has written more today here.

Also today in the Guardian, there's a debate between Baggott and Mike Duff. Duff characterizes the experimental situation of string theory as follows:

> Definitive experimental tests will require that the theory also incorporate and improve upon the standard models of particle physics and cosmology. An impressive body of evidence in favour of this has accumulated, but it is still work in progress.

without giving an example of any sort of conceivable such experimental test. I think Duff is being highly misleading here, since the story of the last thirty years is not one of evidence for string theory unification accumulating, but the opposite: the more we learn about string theory, the less likely it seems that it can predict anything. One can argue that string theorists just need more time (Duff points to the idea of atoms arising back in 400BC, taking more than two millennia to come to fruition), but the problem with string theory is not that progress is slow but that it is negative.

On the question of TV programs like the one that motivated Baggott to write the book, even Duff won't defend them, but blames the situation on journalists:

> As for misrepresentation in the media, there will always be sensationalists and attention-seekers in any field, but in my (admittedly biased) opinion, the worst culprits are the journalists.

This is quite amusing coming from someone who (see here) had his university put out a press release claiming that he had made the first discovery of a way to test string theory. He advertises string theory as having found application in quantum information theory, a claim that I doubt is believed by any other string theorist or quantum information theorist. No, the worst culprits here are not journalists, whose mistake is often just that of taking seriously press releases from people like Mike Duff.

Duff invokes the same criticism made back in 2006 that "Sadly, many critics of string theory, having lost their case in the court of science, try to win it in the court of popular opinion." He's well aware though that string theorists are losing badly in the court of science (with US physics departments now hiring virtually no string theorists). String theory unification is an idea now discredited in the scientific community, but getting propped up by TV programs and prizes from Russian billionaires. I hope when Baggott's book comes out in the US, we'll see a more serious discussion of the issues that it raises.

Update: Duff is unhappy about Butterworth's mild criticism of string theory, so has responded with a comment at the Guardian site that begins

> Dear John
>
> "The concern arises if everyone makes the same wild guess, and the experiments to confirm or deny it are out of reach".
>
> is more-or-less what people said when theorists predicted the Higgs boson in 1964.

According to Duff, I guess, back in 1964 the situation was just like that of string theory, with the field experiencing what people were calling an unhealthy domination by the likes of Peter Higgs and others working on the Higgs mechanism. That's a very odd take on the history, given that the work of Higgs and others was virtually ignored at the time.

Comments

1. **H**
   June 16, 2013

   Can you elaborate the following?

   "String theory unification is an idea now discredited in the scientific community".

   I am not sure what "the scientific community" includes in this context. String theorist will of course disagree, and people outside of HEP will generally not count as experts. So I guess you are referring to the non-string HEP community? Do you know what their general opinion on string theory unification is?

2. **Peter Woit**
   June 16, 2013

   H,

   I actually know many people who work on string theory who have given up on the idea of getting the Standard Model out of it. The most common attitude I hear among string theorists is that the ways people used to hope to connect it to the SM have failed. They say they work on string theory now not because it's a viable idea about unification, but because of things like AdS/CFT.

   For some data points, look at the data on what kind of HEP theorists US physics departments are hiring here
   http://www.physics.utoronto.ca/~poppitz/Jobs94-08.pdf
   One meaning of "scientific community" here is the faculty of US research university physics departments, who now typically want nothing to do with string theory.

3. **Professor Anon**
   June 16, 2013

   Well … I suspect that string theory is not discredited among all the faculty of US research physics departments, but the consensus view of the hiring committees does seem to have soured on string theory.

4. **Peter Woit**
   June 16, 2013

   As another source of data, take a look at the titles of the talks at Strings 2013, here

   http://strings2013.sogang.ac.kr//main/?skin=scientific_talks.htm

   Of the 40 speakers, exactly one (Mirjam Cvetic) is speaking on a topic that has something to do with efforts to do string theory unification (i.e. get the SM out of string theory).

   The other things string theorists are working on are more promising, but the string theory hype and unacknowledged failure of the unification idea have definitely soured their colleagues on doing more string theory hiring of any kind.

5. **Peter Woit**
   June 16, 2013

   By the way, there's an interesting discussion of the current state of things by Mirjam Cvetic here
   http://sciencewatch.com/articles/mirjam-cvetic-string-theory-and-supersymmetry
   which includes the following:

   "If the simplest supersymmetric extension of standard particle physics is valid, the LHC would have found those particles by now. So I think that, as theoretical physicists working on particle-physics models with supersymmetry expected, that supersymmetry would be discovered at the LHC, and it would certainly be disappointing if supersymmetry is not found after the LHC has run for an extended period. In other words, if supersymmetry is pushed to very high energies because we can't find supersymmetric particles at the LHC, it's not going be a very happy situation for us and our theories.

   SW: Should this happen, and nothing beyond the standard model shows up, how do you think theoretical physicists will typically respond?

   I think a typical reaction would be that we can still deal with that, and we just need to adjust the theories. But we have to admit at some point that if our ideas are not being experimentally confirmed, maybe we're just not on the right track. We need experimental feedback, and without it, theoretical research becomes isolated."

6. **Bob Jones**
   June 16, 2013

   "he was motivated to write the book by the experience of watching this BBC program, which featured a combination of serious science with revelations about how we're all part of a cosmic hologram"

   I haven't seen the program, but this Baggott guy needs to wake up if he thinks the holographic principle is some sort of wild speculation. The holographic principle is just about the only thing that is well established about quantum gravity…

   "Of the 40 speakers, exactly one (Mirjam Cvetic) is speaking on a topic that has something to do with efforts to do string theory unification (i.e. get the SM out of string theory)."

   That's why this criticism of string theory is so irrelevant. Hardly anyone is even working on the stuff that you complain about.

7. **Peter Woit**
   June 16, 2013

   Bob Jones,

If “we’re all part of a cosmic hologram” is settled science, what’s the experimental evidence for it?

I’ll stop complaining about string theorists refusing to acknowledge failure when they start publicly acknowledging it. Mike Duff certainly isn’t there yet…

8. **Bob Jones**
June 16, 2013

“If ‘we’re all part of a cosmic hologram’ is settled science, what’s the experimental evidence for it?”

I don’t care for the language being used here, but holography is, whether you like it or not, a well established property of gravity. You can take ordinary general relativity (no strings, supersymmetry, or extra dimensions) and show that the theory has a dual description in terms of a field theory in a lower number of dimensions. There is by now a ton of extremely nontrivial evidence that this correspondence is true on a mathematical level, including experimental evidence.

“I’ll stop complaining about string theorists refusing to acknowledge failure when they start publicly acknowledging it.”

I agree with you that string theory has not convincingly reproduced the standard model, but I’m not sure I would characterize that as failure. The reason is that I’m not sure reproducing the standard model was ever the exclusive goal for string theory. From the very beginning, string theory was related lots of other problems in physics and mathematics, and it led to many important developments in those areas.

9. **Peter Shor**
June 16, 2013

Bob Jones:

Mathematical evidence based on an analogous theory or experimental evidence? Or have string theorists stopped distinguishing between the two?

10. **Bob Jones**
June 16, 2013

Peter Shor,

You would certainly agree that general relativity is a well tested theory, wouldn’t you? What I’m claiming is that general relativity (and its generalizations, such as string theory) is mathematically equivalent to a field theory in a lower number of dimensions. This mathematical equivalence is supported by a lot of evidence, including experiments in nuclear and condensed matter physics.

11. **Peter Woit**
June 16, 2013

Bob Jones,

So, there’s experimental evidence for 4d quantum gravity effects, seen in terms of a 3d conventional qft? Who did that experiment?

12. **Bob Jones**
June 16, 2013

I didn’t say anything about quantum gravity. I’m saying you can take ordinary classical gravity, which is extremely well tested, and by making various field redefinitions, you can show that it’s equivalent to a field theory on the conformal boundary of spacetime, in cases where this boundary is defined.

13. **Peter Woit**
June 16, 2013

Bob Jones,

If you’re referring to 4d gravity, which is what we have experimental data about, and you’re trying to invoke AdS/CFT, one problem is that we don’t live in AdS4

14. **Bob Jones**
June 16, 2013

“one problem is that we don’t live in AdS4”

No, that’s really not a problem. The AdS condition is an asymptotic condition, and the gravitational field is free to do whatever it wants in the bulk spacetime. There are also versions of the correspondence that don’t require AdS space at all, for example

http://arxiv.org/abs/0809.4266

http://arxiv.org/abs/1004.0996

The second of these papers describes black holes with no strings, no extra dimensions, no supersymmetry, no extremality assumption, and no anti-de Sitter space! Is that realistic enough for you?

15. **Mitchell Porter**
June 17, 2013

“Of the 40 speakers, exactly one … is speaking on a topic that has something to do with efforts to … get the SM out of string theory”

They talk about that next month, in Germany: http://stringpheno2013.desy.de/

16. **Roger**
June 17, 2013

I don’t see any testable hypotheses in those holographic duality papers. Is there anything that Popper would consider science? If so what, and why would it be science?

17. **Bob Jones**
June 17, 2013

“I don’t see any testable hypotheses in those holographic duality papers.”

http://arxiv.org/abs/hep-th/0405231

18. **Peter Woit**
June 17, 2013

Bob Jones,

You’re invoking several completely different conjectural ideas about mathematical physics, none of which have been experimentally tested or correspond to evidence that we’re part of a “cosmic hologram”.

19. **chris**
June 17, 2013

Bob Jones,

it might be necessary to remind you, that neither temperature nor entropy of a black hole has ever been measured. Also, there is no experiment about Unruh temperature or just about any quantum effect on a horizon. This is simply due to the fact, that no horizon has ever been observed.

The closest we have is radio and infrared observations of stellar dynamics around Sgr A*. And that’s quite a stretch from there to any horizon dynamics you claim to be experimentally backed.

20. **EDBM**
June 17, 2013

You say, “… the problem with string theory is not that progress is slow but that it is negative.”, which is a claim whose strength rather surprises me – I would have thought that the worst that could be said was that the field has stagnated since it has not produced any theory which could give any predictions, testable or not. Could you elaborate on your point, and maybe provide some references?

21. **Peter Woit**
June 17, 2013

EDBM,

This topic has often been discussed here. The point is that progress in better understanding string theory models has just led to more and more different classes of them. Early on, there was hope that there were a limited number of possibilities, so you had some sort of predictions possible. At this point, it looks like you can get just about anything out of these constructions, so the idea is pretty much empty in terms of any predictive power.

One place I’ve written in detail about this is here
http://mathoverflow.net/questions/133203/explanations-for-mathematicians-about-the-falsifiability-or-not-of-string-theo/133248#133248

22. **Peter Woit**
June 17, 2013

Mitchell Porter,

The Strings 2013 conference is the one that is supposed to not specialize, but survey the whole field, and it’s the one with the most illustrious and influential advisory committee that chooses the speakers. I think one talk out of forty pretty accurately reflects how much interest leading string theorists have in “string phenomenology” these days.

23. **Bob Jones**
June 17, 2013

Peter,

Yes, I’m talking about more than one thing here. The points I’m making are the following:

1. It is reasonable to say that the holography is an established feature of gravity because the holographic principle is implicit in the mathematics of general relativity. It doesn’t depend at all on the validity of string theory or supersymmetry or the existence of extra spatial dimensions. Since this is a statement about the mathematics of an already well tested theory, there’s nothing that needs to be tested experimentally.

2. If you insist on having experimental evidence for the mathematical validity of the AdS/CFT correspondence, you can have that too. AdS/CFT has been used to make successful predictions about nuclear and condensed matter physics.

24. **cormac**
June 17, 2013

As it happens, I chanced upon the Guardian article yesterday. I have to say I thought Professor Duff offered a calm and reasoned pont-by-point defence to Jim Baggot’s criticisms. It seemed to me that Jim made several comments that were rather broadbrush and he conflated distinct issues more than once, as Mike pointed out in his responses.

I was surprised Jim didn’t think to make Peter’s usual criticism, that misgivings about ST are not solely about problems such as compactification, or connecting to experiment, but the sociological problem that a great many researchers have been directed towards a research area with these drawbacks, at the expense of other areas, and whether this is healthy etc. Criticism of the theory itself is more easily deflected since defenders can appeal to the history of science, from the atomic theory to general relativity, just as Mike did.

Overall, I thought Professor Duff a clear winner in that particular debate. I also agree with most of the points he made. It seems to me that a great many ST critics ignore the little word ‘yet’, i.e, do not trouble to acknowledge that problems such as compactification and connection with experiment may, for all we know, one day be resolved (yes, even after 4-5 decades). It always seems a bit one-sided to me to leave this possibilty unsaid….Regards, Cormac

25. **Peter Woit**
June 17, 2013

Bob Jones,

You're conflating several completely different things, and engaging in hype about all of them. As for 1., I just don't see how you get from the conjectures that you link to about a 2d CFT saying something about the horizons of Kerr black holes to the claim that the holographic principle is part of classical GR, other than through an abuse of language.
As for 2, you're just repeating hype, about something completely different than 1.

26. **Peter Woit**
June 17, 2013

cormac,

The problem with Duff's argument is that it applies to any speculative idea that doesn't work out. Anyone whose favorite idea about physics goes nowhere can make the case that it just needs more work, and someday they will be vindicated (and pretty much every crank in the world does make exactly this claim). If you allow people to make this case and give them time-scales longer than their professional careers, no one ever has to admit failure. Everyone can happily come up with a bad idea early in their career (or sign on to someone else's), then get paid for the rest of their careers to work on it, no matter how unpromising it becomes. String theory unification really is an idea that hasn't worked out. Given the lack of other good ideas, you can argue that some people should keep working on it, but I don't think you can argue that most of the people who started on it should keep going. I also don't think you can argue that it's all right not to admit how bad the problems with the idea are. Finally, it's really not at all all right to issue press releases and go on TV promoting your idea that isn't working. It's this last issue that Baggott makes clear is what he sees as the big problem here, and Duff has no answer for that, other than to blame journalists for repeating the hype he and others put out.

27. **Bob Jones**
June 17, 2013

Peter,

In the first paper I cited on the Kerr/CFT correspondence, the authors consider the Kerr solution of classical general relativity, and by analyzing the symmetries of the solution, they conclude that there should be a dual description in two-dimensional conformal field theory.

For an even clearer demonstration of the holographic properties of general relativity, see the paper

http://arxiv.org/abs/gr-qc/9506019

Here the authors study general relativity in 2+1 dimensions and show that the Einstein-Hilbert action is equivalent to the action for Liouville theory on the conformal boundary of spacetime. Of course, the assumption of 2+1 dimensions is a drastic and unphysical simplifying assumption, but it's still interesting because (2+1)-dimensional gravity is one of the few contexts in which we can treat gravity as a quantum field theory.

I don't know what it means to say we're part of a "cosmic hologram", but I would say that the papers cited above give pretty convincing evidence that general relativity has at least some holographic properties.

28. **Peter Woit**
June 17, 2013

Bob Jones,

Now you're introducing yet a different notion of holography, in a different set of (unphysical) dimensions.

Actually, I'm a big fan of the paper you link to, which is part of the amazing story of how 3d Chern-Simons QFT is related to 2d CFT. It's fantastic mathematics and I'm very willing to believe that it has something to do with fundamental 3+1 d physics. Unfortunately I don't know quite what this is, nor do I think does anyone else….

29. **lun**
June 17, 2013

Bob Jones,
———–
AdS/CFT has been used to make successful predictions about nuclear and condensed matter physics.
————
Can you give me an example of a "successful prediction"? I define predictions in a Popperian way "if system X has a gravity dual, we get experimental result Y, if it does not, system X is for sure not described by a gravity dual".

I will make an experimental prediction, "there not a single paper in the AdS/CFT literature that meets the above criteria." I would be very interested in a falsification of this prediction 🙂
Otherwise, String/Gauge is still very very interesting, but claiming it leads to "experimental predictions" is misleading.

30. **Bob Jones**
June 17, 2013

lun,

See the paper

http://arxiv.org/abs/hep-th/0405231

The authors show that if a fluid is described by a gravity dual, then the ratio of shear viscosity to volume density of entropy is equal to a universal constant. If the ratio does not equal this universal value, then the system does not have a gravity dual.

31. **Anonyrat**
June 17, 2013

Duff said, in his conversation with Baggott: "Similarly, string theorists did not assume supersymmetry, extra dimensions, the dualities of M-theory or the myriad possible universes; they discovered them to be consequences of a theory that subsumes empirically well-established features such as general relativity, gauge field theory and chiral quarks and leptons."

While there is a limit to the analogy, the mechanical models of the aether incorporated all the relevant known physics, had counter-intuitive features, and similarly ought to have worked per the best of knowledge that existed at the time.

32. **lun**
June 17, 2013

Bob Jones, see
http://arxiv.org/abs/arXiv:0812.2521
and
http://arxiv.org/abs/1110.6825

With a "classical gravity dual" your statement might have been a bit more precise, but then the assumptions involved (Nc—>infinity) are a bit too naive to really compare to experiment.

33. **GMBH**
June 17, 2013

Bob Jones: I actually work the shear viscosity of strongly correlated fluids. The paper you refer to, 0405231, does not make any experimental prediction whatsoever. It contains a CONJECTURE for a lower bound on the shear viscosity. The conjecture is based on a calculation for a very specific model which is not realized in nature, I.e not in a quark gluon plasma nor in cold atomic gases. If you want to make accurate predictions for the shear viscosity, you have to work much harder using realistic models and not just elevate a result for a very specific model to magically be true universally. The lower bound conjecture can actually be derived from mere dimensional analysis. If this is one of the main examples of an experimentally testable prediction of string theory, then that field is in real trouble.

34. **george ellis**
June 17, 2013

Bob Jones:

"For an even clearer demonstration of the holographic properties of general relativity, see the paper http://arxiv.org/abs/gr-qc/9506019"

This paper relies on a negative cosmological constant. The experimental data is that the cosmological constant is positive. The paper does not apply to the real world.

35. **Bob Jones**
June 17, 2013

GMBH,

I'm mostly interested in the applications of AdS/CFT to gravity, so I don't understand the technicalities of these applications to the quark gluon plasma. I have not heard this claim that the lower bound conjecture can be derived from dimensional analysis. Could you elaborate on that?

36. **Bob Jones**
June 17, 2013

george ellis,

There is a much more obvious reason why that paper does not apply to the real world. I addressed all these issues in my comments to Peter.

37. **imho**
June 17, 2013

I knew this day would come and I fear it's only the beginning. I hope I'm wrong.

Science has flourished over several centuries, not because of our talents for persuasion or our political connections or our eloquence in explaining our points of view. Science has flourished because we produce tangible concrete results that improve the world. Even fundamental Physics research has been able to justify it's existence by claiming some yet unknown future benefit(s). The public trusts us to deliver, because in the past, that's exactly what we've done. This is our bread and butter and it's the fundamental reason the power structure tolerates us instead of taking our lunch money. What do you think happens when we remove Physics from the testable real world and start pontificating about untestable apparitional fairy-tails?… I'll tell you what happens. We lose the public debate to Pat Robinson and Joel Osteen… I promise they are infinitely more persuasive then any of us. Jim Baggot will only the begining.

I know, I know, inside the ivory tower it's pretty easy to become completely convinced that that rest of the world is as thoroughly interested and impressed with our intellect as we are. But trust me… they are not. We exist because we create bombs and magic black boxes. We're funded Billions because we claim that's what is needed to create more bombs and magic boxes. This is the difference between Physics funding and Philosophy funding. Anyone who seriously believes the powers the be will continue to heavily fund a few thousand people to engage in intellectual masterbation is confused about this world and how things work.

38. **Chris W.**
June 17, 2013

From Bob Jones:

> "This mathematical equivalence is supported by a lot of evidence, including experiments in nuclear and condensed matter physics."
>
> "If you insist on having experimental evidence for the mathematical validity of the AdS/CFT correspondence, you can have that too. AdS/CFT has been used to make successful predictions about nuclear and condensed matter physics."

These statements strike me as profoundly muddle-headed at the outset. "Mathematical equivalence" or "mathematical validity" must be established by mathematical arguments, not experimental evidence, *by definition*. By extension, it is not at all clear what "is supported by" is supposed to mean in this context.

Note that by experimental evidence I mean observations on real physical systems, not computational experiments. Regrettably the two are sometimes confused.

39. **Bob Jones**
June 17, 2013

Chris W.,

I actually agree with what you say. Unfortunately, a lot of people don't understand this point, and so we often see demands on this blog for "experimental tests" of

AdS/CFT.

40. **Peter Woit**
June 17, 2013

Bob Jones,

This has long ago stopped making sense, and it's not even really relevant to the topic. Enough.

41. **lun**
June 17, 2013

Bob Jones, have a look at
http://inspirehep.net/record/205584?ln=en
A bound of ~1/12 is "derived" from the uncertainity principle.
I say "derived" because the assumptions used are of course unrealistic (using the Boltzmann equation for a strongly coupled system) ,but then again N=4 Nc=infinity in the UV is also an unrealistic assumption.
I agree that the number coincidence is intriguing.

For the reaction of the heavy ion community to the claims of string theorists, look at the theory summary of the "Quark Matter" conference, the leading conference in the field
http://www.sinap.ac.cn/qm2006/ppt/Nov.20/Plenary-15/Larry-McLerran-QM061.ppt
which if I remember correctly was covered on this blog too.
The talk was provocative, and there are people in the field with a more conciliatory attitude, but it does represent a wide-spread feeling,especially by experimentalists.

42. **amused**
June 18, 2013

Quotes from Duff in the debate:

"Finally, you offer no credible alternative. If you don't like string theory the answer is simple: come up with a better one."

Alternative to what? To a Theory of Quantum Gravity? To a Theory of Everything? Note the sophism here; the implication that these grandiose topics are the only ones worth pursuing in fundamental theoretical physics.

"The battle for the correct theory will not be won on Amazon or on the blogosphere, however. It will be won in the pages of scholarly scientific journals."

What a disgusting lie. If it was true, and if interesting and important physics progress was being made in string theory, then the pages of PRL would be full of string theory papers. Instead there are almost none… In their frustration at having hardly any important progress to report to their physics colleagues in PRL, string theorists of Duff's ilk resort to duping the general public with hype, published, e.g., in that illustrious scientific journal the Op-Ed section of the NY Times.

I wish the public would know how much contempt Duff and string theorists of his ilk (hopefully not all of them) have for the scientific journal system in reality. Scientific output on non-string topics in fundamental theoretical physics is considered to be worthless by them, regardless of which or how many journals it is published in. E.g., when assessing job candidates, single-author PRL publications on a non-string topic by some young person count for flat zero when weighed against string theory publications by another young person in a lesser journal where he/she is joint author with a bunch of senior and illustrious people.

IMHO the problem is not with string theory, which is interesting and worth pursuing as a theoretical model of quantum gravity even if it turns out not to have direct relevance for Nature. (Toy models which exhibit interesting theoretical properties are often studied in theoretical physics, and lessons learned from them are often valuable for "real physics" later.) Instead, the problem is with how string theory has been pursued. In particular the hype to the general public and the sociological insistence that it is the only legitimate topic to work on in fundamental theoretical physics.

43. **Lowell**
June 18, 2013

So why is 2+1 general relativity gravity? I has no gravitons.

44. **GMBH**
June 18, 2013

Bob Jones,

Here is what I meant with my comment on the lower bound and dimensional analysis: Dimensional analysis gives you, that for a strongly correlated system the ratio of the shear viscosity to the entropy density must be some number B times Planck's constant divided by Boltzmann's constant. Now, the hard bit is to calculate B. A proper test for a theory is to predict a value for B which is then experimentally confirmed. What the paper 0405231 does is to show that B=1/4pi for a specific class of systems with a gravity dual. This class of systems is not found in nature. The authors then simply conjecture, that all systems in nature must have B>1/4pi. This is not a proper quantitative prediction – it is simply a postulate.

45. **RF**
June 18, 2013

Nice post. Any chance you could provide a page number for the quotation above which starts "Several centuries…" and ends "… virtually clueless". I wasn't able to retrieve the passage online. Much appreciated!

46. **Jim**
June 18, 2013

Thanks to Peter for his kind review and to all who have troubled themselves to make comments. I thought I might respond to one or two of these.

Bob Jones – please keep going. I think you're helping to prove nearly all my points. I'm afraid parts of the theoretical physics community have developed a misleading interpretation of 'evidence'. In 'Farewell to Reality' I devote some time to talk about the difference between 'coherence truth' and 'correspondence truth'. Coherence truth can be established in terms of mathematical relationships, when something has been shown to be equal or equivalent to something else. These somethings might involve physical concepts, and to establish true relationships between these is obviously a good thing to do. But I argue that the notion of scientific truth is based on a 'correspondence truth' established between theoretical ideas and empirical facts. Something is scientifically true if, and only if, it corresponds to established facts about the real world.

Now, I don't deny the power of imaginative and speculative thinking, and I accept that it can take a very long time to establish the truth of a theory, but it seems to me that some contemporary theorists have retreated into their own, self-referential world, content to 'discover' coherence truths in the math but abandoning entirely any sense of obligation to seek scientific truth. **Peter Shor** – this is the point I think you were making. Actually, I'm really fine with all this – just don't call it science.

cormac – I'm obviously disappointed that I wasn't more convincing in my debate with Mike Duff. I'm very well aware of the sociological issues but other commentators (such as Lee Smolin and Peter, in his book and on this blog) have made these arguments very well in the past and I suspect there's little more to be said. In writing 'Farewell' I wanted simply to point out how very tenuous this structure is – from my perspective it really does look like a theoretical house of cards – yet this hasn't prevented some well-known theorists from publishing popular science books and making radio and TV documentaries which collectively give the rather misleading impression – intended or not – that this is all accepted science. And their efforts to change the way we interpret what science means really scares me. My ambition was to provide something of an antidote to this. Perhaps you could suspend judgement until you've read the book?

imho – I knew that 'Farewell' might be misinterpreted as an attack on science. It's not. I trained as a scientist, taught science at university in the UK and ran my own (experimental) research group for many years. I have even published theoretical papers. I'm not trying to influence science policy or inform debates about funding. I do worry that the rush to string theory has meant that other – possibly equally fertile ideas – have been neglected (which is the point that **amused** has made). I don't offer any solutions in 'Farewell' – a valid criticism. I just wanted to foster the debate.

47. **nemo**
June 18, 2013

imho, I can see what you mean but, as a physicist and a teacher, I slightly disagree. Sadly, bombs and black boxes do play a role. Prestige plays a role too. However, hard sciences, e.g. physics, and mathematics, e.g. operational research, have shown that the various methods they follow have been and are very effective to build solutions to solve problems. That is, also, why funding of physics is much larger than funding of philosophy. Physics, mathematics and engineering have led to much better problem solving than philosophy. That plays a role, too.

48. **Professor Anon**
June 18, 2013

amused:

You say

> "when assessing job candidates, single-author PRL publications on a non-string topic by some young person count for flat zero when weighed against string theory publications by another young person in a lesser journal where he/she is joint author with a bunch of senior and illustrious people."

Do you have any evidence for this? Hiring meetings typically contain both non-string-theorists and string theorists. I can't imagine any string theorists being that openly disrespectful of their colleagues.

49. **imho**
June 18, 2013

AMUSED Says:
"the problem is not with string theory, which is interesting and worth pursuing as a theoretical model of quantum gravity … Instead, the problem is with how string theory has been pursued. In particular the hype to the general public and the sociological insistence that it is the only legitimate topic to work on in fundamental theoretical physics."

IMHO responds:
Yep! Exactly!… and I've stated on other comments, there is nothing wrong with fundamental research at the appropriate scale. The problem is the hype and the massive diversion of funds away from magic boxes. We are signing our own death warrant if we choose to stop playing the game(s) we are good at and enter the debate stage with Huckabee and Gingrich. Only inside the walls of Stanford is it absolutely obvious that those charlatans aren't even worthy to share the same stage with people of our formidable intellect. Outside the walls, Palin is a prophet and we are overly-pampered out-of-touch nerds in an Ivory tower. This will not end well.

to NEMO: So prestige will fund the next collider… it must be an interesting world you've created for yourself?

50. **Anonyrat**
June 18, 2013

lun,
Thanks for that link to the theory summary of the Quark Matter conference.

There is the stern admonition:

"AdsCFT MUST be accountable to the same scientific standards as are other computations, or else it is not science".

and

" Issues must be scientific:
Controlled approximation
A result must be falsifiable."

Other than with the cold fusion fiasco, I've not seen such a strong scolding made in public like this. This is really eye-opening – even though I follow Peter Woit's blog quite regularly.

51. **Peter Woit**
June 18, 2013

RF,

That quote is on page 150-1 of the British edition.

52. **nemo**
June 18, 2013

imho, I do think that prestige has many times played a role, e.g. in funding colliders, space exploration, supercomputers. And I think that it still plays a role, at least here in continental Europe, and elsewhere, e.g. in China and Japan. It seems so. But I am afraid that expanding on this sub – topic would quickly become off – topic. As for the main topic here, I agree fully that the way string theory has been sold to the general public for too many years has been detrimental to theoretical physics and therefore science at large. Also, I agree that the problem with string theory is hype, the insistence to sold it as the theory of everything and the only game in town. String theory itself seems to me a worthy sub – field of mathematics.

53. **cormac**
June 18, 2013

Jim: Many thanks for considered reply, and Peter too. Yes, I'm looking forward to reading your new book, I'm quite sure it will be just as good as your Higgs book, which I very much enjoyed. I also think your insistence on a clear distinction between what is well-established science and what is highly speculative is exactly right, I wish more science journalists would observe this.

Re the Guardian piece,, I think it's reasonable to comment on a newspaper article as a separate entity, since that is what was presented to the public. However, I did wonder about editing – it struck me that several of your points might have been truncated (or omitted) at editing stage. I hope not, I know how that can feel!

In your book, I imagine you address the issue Peter raises in his response to my earlier comment , as it is an interesting problem: that the 'it's simply a matter of time' defense to a lack of predicitve power is weak, because *any* speculative theory can always claim that connection to experiment will be forthcoming one day in the future. This is of course true, but there is a mirror problem, that this statement does not in itself mean that all speculative theories are doomed to failure! Therein lies the rub, I think.

Add to this problem the historical point that in particle physics, the time gap between theory and experiment has tended to widen dramatically from the 1930s onwards, and one has a genuine conundrum….what if we have simply reached the stage where a successful theory really has got that complicated?

54. **lun**
June 18, 2013

This might be a great point to note the passing of, perhaps, the last theoretical physicist who can claim to have Revolutionized our view of reality, Ken Wilson.
I am sure Peter can explain much better than I could why Wilson's contributions to our understanding of the world, in depth and significance, rival any of his more media-savvy fellow Nobel prize winners.

But one thing to say, in view of the current employment situation in physics, is this line from his obituary:
————————————
http://news.cornell.edu/stories/2013/06/physics-nobel-laureate-kenneth-wilson-dies

In 1963 Wilson joined the Cornell physics department and was soon given tenure even though he had hardly published. As he later said in his Nobel autobiography, "my very strong desire to work in quantum field theory did not seem likely to lead to quick publications; but I had already found out that I seemed to be able to get jobs even if I didn't publish anything so I did not worry about publish or perish."
———————————————
I wonder if he could repeat this feat today. Actually I don't wonder, he would not be able to.

55. **Peter Woit**
June 18, 2013

Thanks lun,
I never met Wilson, but of course know some of his work. I'll try and find time soon to write something, but hopefully others who knew him are writing things that will appear soon. I had just this morning been thinking about the question of whether Higgs/1964 was work that would get one a job in the current sort of job market.

56. **Tmark48**
June 18, 2013

@ nemo : The way String Theory has been sold to the public has damaged Theoretical Physics not science at large unless you want to identify theoretical physics with science ? People make fun of theoretical physicists not real scientists. Maybe you haven't noticed, but people are interested in real science. As for those poor theoretical physicists, who cares ? They can't seem to come up with one decent theory thats is confirmed by experiments. Some theoretical physicists even go as far as to put themselves on the same plane as the good old Einstein. Didn't Einstein chase unified theories for a good part of his life ? Of course he did, but what those theoretical physicists conveniently forget is that Einstein before he started chasing speculative ideas had already revolutionized science by establishing special relativity, general relativity and some other things also. So we can certainly excuse the poor fellow in his later years.

57. **David Bailey**
June 18, 2013

I broadly agree with Imho's original comment, and I feel that the problem is far wider than theoretical physics.

For example, it would seem that the UK government is feeling the need to presurise universities to ensure their researchers are behaving responsibly.

http://www.independent.co.uk/news/science/the-bad-science-scandal-how-factfabrication-is-damaging-uks-global-name-for-research-8660929.html

Data fabrication, irreproducible results, and sheer hype are ruining science.

58. **Jim**
June 18, 2013

cormac The debate wasn't edited in any way – what you saw was what we wrote. BUT we were constrained to a very tight word limit for each exchange. So, when Mike rather disingenuously tried to pin the blame for over-selling the promise of string theory on journalists, I found that I couldn't respond to this and still provide a considered reply to his main points. The fact that you were unconvinced by my arguments is entirely attributable to my inability to put them in as succinct and yet powerful way that I felt was really needed. My loss.

I think if you read the book you will – hopefully – see that I am very much in favour of imaginative and speculative ideas. Science is a very forgiving discipline in that it really doesn't matter how a new theoretical approach is contrived – it will succeed so long as it represents a *better* theory. There are many examples from history.

But I've been struck by the simple fact that many of the constructions of contemporary theoretical physics represent major steps in a *backwards* direction. For every attempt to fix a problem in the standard model, it seems we multiply the free parameter space five-fold. Theorists like Mike Duff declare supersymmetry, hidden dimensions and the multiverse to be 'discoveries' of the string theory enterprise, rather than assumptions. I may be rather old-fashioned, but when I was a scientist I had a fairly unequivocal understanding of the meaning of 'evidence'. And this isn't it.

Let's play a game. Let's imagine that I contrive a theory based on the idea that elementary particles can be described theoretically in terms of strands or loops of 'string'. I find that the resulting theory allows me to 'predict' all the things that the standard model of particle physics can predict. It has the added bonus of allowing me to construct a theory of quantum gravity. I know this isn't enough necessarily to replace the standard model, because I haven't yet shown how string theory transcends this. But, in our game, I find that this string theory predicts that the mass of the electro-weak Higgs boson should be something of the order of 120-130 GeV. I make this prediction in 2010. The ATLAS and CMS collaborations subsequently find that the Higgs boson does indeed possess this mass, and we celebrate with champagne.

Of course, string theory doesn't do this, which is why I regard this business of supersymmetry, hidden dimensions and multiverses as assumptions, rather than 'discoveries'. When string theory can make legitimate predictions that can be tested, and when those tests are confirmed, only then can the assumptions be regarded to represent aspects of empirical reality, and so become genuine discoveries.

It could still be argued that string theory will get to this, if only we are sufficiently patient. But I think you might be prepared to admit that the majority of the string theory community gave up on this quite some time ago. Some realized that careers could be built – and, now, highly valuable prizes can be won – by establishing coherence truths, like the ADS/CFT duality, and forgetting that scientific truth is all about establishing a *correspondence* between theoretical ideas and empirical facts.

I find that I still need to tell you that I really don't mind about this. So long as you stop calling is science.

59. **Mitchell Porter**
June 18, 2013

jim:

"I regard this business of supersymmetry, hidden dimensions and multiverses as assumptions, rather than 'discoveries'."

Michael Duff is not arguing that we have discovered these things to be true, he is arguing that we have discovered them to be implications of the string hypothesis.

60. **amused**
June 19, 2013

Professor Anon,

""when assessing job candidates, single-author PRL publications on a non-string topic by some young person count for flat zero when weighed against string theory publications by another young person in a lesser journal where he/she is joint author with a bunch of senior and illustrious people."

Do you have any evidence for this? Hiring meetings typically contain both non-string-theorists and string theorists. I can't imagine any string theorists being that openly disrespectful of their colleagues."

I base it on postdoc job application outcomes. Will provide details if you insist, although I don't think it would be of great interest for the readership here… The real explanation for this situation was simply that the string-dominated particle theory groups had/have a policy of "string theorists only" when hiring postdocs (and presumably also when supporting candidates for faculty positions). Probably there is nothing unusual about that and the situation is often the same in other areas of academic science… If they just admitted it openly, instead of pretending to be noble and altruistic, then I wouldn't get so worked up…

61. **Jim**
June 19, 2013

Mitchell Porter Nice try, but I don't buy it. This is a sloppy use of the word 'discovery', of which we are all guilty (including me). Given what's at stake here, I think we need to be a little more careful in our choice of words.

I respectfully suggest that you can't 'discover' consequences or implications from the math. These implications may emerge from the equations and allow you to make some logical deductions. These implications can be used to construct another layer of theoretical structure. So, string theory *implies* hidden dimensions, and if we compactify these in a Calabi-Yau manifold we can build another layer. We haven't 'discovered' anything – yet.

If and when the implication or assumption of hidden dimensions leads to a firm prediction, and the prediction is upheld by reference to empirical facts (such as the unambiguous observation of KK particles at the LHC), then – and only then – can a consensus form that we have discovered hidden dimensions. Until then, hidden dimensions represent an assumption within a layer of the theoretical structure.

Mike was also constrained to a tight word count, but the words he did choose left no room for doubting his interpretation: 'Yours is a common fallacy… string theorists did not assume supersymmetry, extra dimensions, the dualities of M-theory or the myriad possible universes; they *discovered* them to be consequences of a theory that subsumes empirically well-established features such as general relativity, gauge field theory and chiral quarks and leptons.' (Italics mine). In this kind of language, 'discovery' readily trips over into 'evidence' and, before you know it, we have created the popular misconception that this is all accepted science fact.

62. **piscator**
June 19, 2013

"Some realized that careers could be built……by establishing coherence truths, like the ADS/CFT duality, and forgetting that scientific truth is all about establishing a correspondence between theoretical ideas and empirical facts."

This is a total misunderstanding of what theoretical physics is. While I am not personally attracted to formal work, the discovery of the AdS/CFT correspondence is a deeply important result. Peter, who is no great fan of string theory, will not dispute this. The implication that people like Maldacena should not have careers – you can argue about prizes, but careers!!! – is just dumb and ignorant, to be frank.

amused: I have some sympathy for the points you make, but have never understood why when it comes to hiring the editors of PRL (typically non-practising physicists who are not subject specialists) and its referees (who are just a couple of members of the community) should play some special privileged role.

63. **Jim**
June 19, 2013

piscator I wouldn't wish to deny anyone a career, and I think if you look back you'll find that this is not what I said. I'm very much in favour of academic freedom and speculative ideas. All I ask is that we don't declare this to be accepted scientific fact.

64. **nemo**
June 19, 2013

Tmark48, a very short answer. Of course, I do not identify theoretical physics with science, but there is no physics without theoretical physics, or without mathematics, and think of other hard sciences and engineering without physics. So that is why I think that hype has damaged theoretical physics and therefore science at large.

65. **amused**
June 19, 2013

Piscator,

"I have some sympathy for the points you make, but have never understood why when it comes to hiring the editors of PRL (typically non-practising physicists who are not subject specialists) and its referees (who are just a couple of members of the community) should play some special privileged role."

Well, the editors of Nature are also non-practicing scientists and they have enormous influence on hiring in the bio-sciences…

How useful PRL is as a quality measure depends on how seriously the physics community is willing to take it. At the moment it does seem to be taken seriously by pretty much all of the community. String theorists profess not to, but I've seen firsthand how excited some of them get about sending a paper there when they think they have a shot. 🙂 The quality of judgments on individual papers can fluctuate of course, but I think on a larger scale the level is pretty good.

As for its use in hiring decisions etc, I think it can play a useful role as a kind of minimum criterion. I'm sure that even some string theorists are sick of the phenomenon of the "brilliant young string theorist", whose has been anointed as such by his illustrious advisor/mentor after being joint author on some papers with a bunch of more senior people. IMHO it would be a change for the better if such people first had to publish a few papers on their own in PRL before being anointed as `brilliant' and sent to the front of the job queue. Just to show that they can. Shouldn't take them more than a couple of months 😉

PRL publications are also useful as an indicator of the overall health of a field. If interesting and important developments are happening in the field, it will manifest as a regular flow of PRLs. Absence of such a flow will be seen by the rest of the community as a sign of ill health. And if there is no PRL flow, and at the same time practitioners are seen to be actively hyping their field to the public, it looks really, really bad! And even worse when, on the rare occasion the practitioners do manage to get a PRL, they issue an excited and over-hyped press release…

FWIW I agree completely with what your wrote about the importance of ads/cft. On the other hand, I'm not sure about that 6-dimensional superconformal field theory that Nima is so excited about… If it's really that interesting and important, he should go publish a paper on it in PRL! 🙂

66. **cormac**
June 19, 2013

Jim: those are very interesting comments. I especially take your point on word count, my old enemy. Actually I didn't find your comments in the Guardian unconvincing, it's more that I found Mike *more* convincing, a relative as well as subjective judgement.

Your conversation with Piscador and others is also very interesting because I think the relation between theoretical physics and physics is at the heart of the debate around sting theory, yet this rarely comes to the fore. Peter touched on this quite a bit in his book, I imagine there's quite a lot on it in yours.

As a very ordinary experimentalist who is the son of a well-known theoretician, I've long had a special interest in this debate. Sometimes I think theoretical physics occupies quite a unique position in science; because of its dual role in mathematics and physics (the former is often overlooked) it's not quite clear whether it should be forced to conform with the strict positivism we tend to apply to other areas of science, i.e. should it always be shackled to experiment, or should some theoretical physics be allowed to roam free like mathematics. Perhaps the problem is when the latter is marketed as the former.

For example, SUSY is often portrayed on this blog as an offshoot of string theory. In fact, it was originally a purely mathematical construct that showed one way of cirmcumventing the no-go theorems of gauge theory – to this day, I gather than one cannot construct a unified gauge theory that includes the strong force without SUSY. It may have been adopted by string theorists, but that has nothing to do with the theory itself. You might of course argue that all this has nothing to do with experiment, but the original SUSY theorists never claimed it did – that's my point!

As I understand it, the main motivation for SUSY nowadays comes not from string theory, but from cosmology, simply because it is very hard to see how the interactions were once unified without some sort of bose-fermion symmetry….
Regards, Cormac

67. **MathematicianNotPhysicist**
June 19, 2013

Cormac, I don't understand. Why is there a need for interactions to be (or ever to have been) unified?

68. **Peter Woit**
June 19, 2013

MathematiciannotPhysicist and cormac,

I suppose Cormac is referring to No Go theorems that show that you can't combine space-time and internal symmetries non-trivially, something that SUSY gives new options for. But this is getting way, way off topic, so please anyone who wants to discuss this with Cormac should contact him directly.

69. **piscator**
June 19, 2013

amused,

I take the point that Nature (or more generally CNS of Cell-Nature-Science) plays an important role in bioscience careers – but I don't see that as a good example to be copied. I think PRL plays a similar role in condensed matter, but I agree with you this is not the case in high energy.

I have published in PRL (and on my own), but my best work is not there.

The thing I really don't get though about emphasising PRL publications is that the PRL format is well suited to short papers making a single point. Maybe this works naturally for experimental subjects where there is a single clear result, and what matters is the measurement. But in theory there is an argument to be developed and calculations to be done, and in most cases 4 pages is not adequate to describe this. Such papers – and I think this represents most good work in theoretical physics – should not go to PRL.

My personal experience with papers within my conceptual radius of convergence is that what appears in PRL is disproportionately flashy, and more likely to be making big claims with a small chance of being right than making small claims with a large chance of being right (This is true of my own papers there as well). I don't think there is any intrinsic sin in writing such papers, or that they are intrinsically better or worse than long calculation-heavy papers (which I also do), but to my mind this is the 'PRL type'. So I see PRL more as a style of paper than an indicator of quality.

PRL emphasises general interest, which again affects the type of paper likely to be accepted, without to my mind necessarily implying higher quality.

I do think it is a good journal, and there is an approximate guarantee of quality, but I strongly feel it is suited to a particular type of paper that is not and should not be representative of most work in theoretical physics.

I agree with the comments about the 'hotshot young theorist' issue although I don't think is especially to do with string theory (in recent years I would say this is much more prominent in BSM pheno). Particle theory as a whole, and particularly I think in the United States, has a problem with genetic diversity and inbreeding – look at faculty hires over a substantial period and see how many of these can be traced back to a very small number of advisors at Harvard/IAS. Yes these people are smart, but the history of physics shows how often truly brilliant people completely miss or misunderstand important new ideas, and it is not good if everyone is brought up with the same toolkit and prejudices.

70. **Peter Woit**
June 19, 2013

All,

I think the PRL issue, which had nothing much to do with the book being discussed here, has now been convincingly beaten to death. Enough about that.

71. **Paulibus**
June 20, 2013

Peter: your take, "Not Even Wrong", on the activities of many of today's theoretical physicists looks as if it's been amplified by Jim Baggot's book, which I plan to own soon.

As I see it, there are many human activities which can't be justified on the anthro'centric grounds that they actually sustain our species' survival. But lots, like movie making, sport, art and literature can be justified on the grounds that they are harmless entertainments, mostly free of chicanery, which give spectators much pleasure. They strike a balance between benefit and cost which entertained paying folk happily tolerate . But now and then the balance tips too far towards the cost side for said folk to tolerate happily, and sports players, artists, writers etc. find that they are jobless. Tough: fashions change.

I think that the theoretical physics enterprise is losing this balance, which in its case has long been sustained by human curiosity and the enormous proven utility of new physics in sustaining our species' rise and rise. It looks to me as if theoretical physics, lacking new discoveries, is busy passing its sell-by date. Not that many tears need to be shed over this; its cause includes too great a proportion of hubris. Tough.

72. **Jim**
June 20, 2013

cormac I sense you're coming round, and suspect you will enjoy the book when you've had a chance to study it a bit – please let me know.

It was inevitable that some commentators (who haven't read the book yet or have no intention of doing so) would interpret my position as denying all value in speculative theorizing. At risk of sounding rather pompous, I'd like to quote from my own book (p. 20):

I want to be clear that the demand for testability in the sense that I'm using this term should not be interpreted as a demand for an immediate yes-no, right-wrong evaluation. Theories take time to develop properly, and may even be perceived to fail if subjected to tests before their concepts, limitations and rules of application are fully understood. Think of testability instead as more of a professional judgement than a simple one-time evaluation.

The Testability Principle demands that scientific theories be actually or potentially capable of providing tests against empirical facts. Isn't this rather loose? How can we tell if a novel theoretical structure has the potential *for yielding predictions that can be tested? For sure, it would be a lot easier if this were all black and white. But I honestly don't think it's all that complicated. A theory which, despite considerable effort, shows absolutely no promise of progressing towards testability should not be regarded as a scientific theory. A theory that continually fails repeated tests is a failed theory.*

So, I hope this makes it clear that I have no wish to shackle theory to experiment. My point is that many (the majority?) in the string theory community gave up quite some time ago on the task of progressing the theory towards testability in preference to building ever-more elaborate and abstract structures and seeking coherence truths among these. It is at this point that these theorists cease doing science.

As you know, SUSY is not so much a theory as a theoretical device – there are many different kinds of ways that supersymmetry can be employed. But it looks like the simpler of these (such as the MSSM) is failing the test – there's no evidence of low-mass sparticles from the LHC. I suspect that efforts to salvage it (or 'save the appearances') are driven by the simple fact that SUSY is an important component in superstring theory.

73. **cormac**
June 20, 2013

Jim: Yes, it's hard to disagree with that quote and it's pretty much in tune with Peter's view too. About my only caveat is the business of 'progress towards testability', or, more specifically lack of! This is very much Peter's view too, but I'm not enough of a mathematician to know whether it makes sense. Do good theories always show such progress before they come good? Mightn't they sometimes go dormant, or even backwards for years, before an series of unanticipated breakthroughs happen? For an example, I remember reading that the anti-atomists gained in strength when chemists suddenly started talking about molecules, instead of just atoms! No doubt it seemed a case of a speculative theory becoming ever more contrived, before all became clear years later…
Re SUSY, my point is at least some of the effort in salvaging SUSY is *not* from ST, but from a general need of it for unification (by any means), as Peter points out above

74. **nemo**
June 20, 2013

Paulibus, let's assume that we rename theoretical physics, or part of it, and call it mathematics. Would you stand by your claim ? Now travel back in time two, three or four centuries, and think about the mathematical tools being developed at that time, and contextualize them in relation to the communities at large, at that time, and the technical tools, i.e. engineering, being developed at that time. Would you still stand by your claim ?
The damage that hype has done to theoretical physics and science is exemplified by your extraordinary statement that *"theoretical physics"* […] *"is busy passing its sell-by date."* Does that apply to mathematics as well ?
And another example is this one from The Guardian: *"Scientists working at the Large Hadron Collider have found no evidence that the new particle discovered earlier this year is anything but the simplest – and most **boring** – variety of Higgs boson."*
As for survival: brutally, I ask you whether you suggest we should just pray or else enjoy our time while waiting to be hit by a large enough asteroid. Oh, yes, may be in that case we should just move underground and "survive" down there.

75. **Mitchell Porter**

June 20, 2013

Jim said

“I suspect that efforts to salvage it (or ‘save the appearances’) are driven by the simple fact that SUSY is an important component in superstring theory.”

From what I can see, the principal function of supersymmetry in model-building today is still “naturalness”. It is there to produce cancellations that eliminate the need for finetuning of parameters to enormous orders of magnitude. Even an approach like “split supersymmetry”, which may explicitly tolerate a tuning of the Higgs mass to one part in a thousand or a million, still relies on supersymmetry to explain the rest of the tuning.

76. **Mitchell Porter**
June 20, 2013

more precisely: relies on supersymmetry to explain why the tuning is just one part in a million rather than one part in 10^15.

77. **Paulibus**
June 21, 2013

Captain? N: You have an interesting reaction to my ‘extraordinary statement’, or claim, that theoretical physics is ‘busy passing its sell-by date’. Let me explain: things meant for human consumption (edible or intellectual) get rejected — pass this date — if in time they deviate too
far from a cost-benefit balance acceptable to those that rule the purse. It is obvious to me from academic hiring practice, Peter’s and Lee Smolin’s books, this blog, and what has been said here about Jim Baggot’s book (which I look forward to reading) that the infusion of fairy-tale stuff into theoretical physics is busy tilting its balance towards cost, and away from evident benefits that both you and I acknowledge. Funding won’t go on for ever enlarging rafts of clever people who seem to ignore the essential flavour of physics – a Baconian balance between theory, prediction and verification by experiment and
observation.

Your last comment about big asteroids shows faith in a future way of evading the conservation of momentum. Perhaps there is indeed a need for fairytale physics, failing which: maybe just – Kismet!

78. **Jim**
June 21, 2013

cormac This had better be the last exchange, as we’ve gone on long enough and I don’t want to try Peter’s patience. I buy everything you say. The history of science is full of twists and turns and only a fool would pretend that ‘progress’ (however defined) is something that could be predicted.

I do say repeatedly in the book that I’m not trying to stop the string theory community doing what they’re doing (I’m not that naïve). What irks me is that the assumptions on which the resulting structure is based (call them ‘predictions’ if you really must) have been sold to the wider public as accepted science. To take one example, in his own *Instant Expert* guide to the Theory of Everything (*New Scientist* 4 June 2011), Mike Duff is careful to talk about M-theory being a ‘leading candidate’ for a theory of everything, but I would argue that it’s real hard for anyone with a relatively general understanding of science to read this stuff – with sections on superstrings, supersymmetry, extra dimensions, M-theory, branes, the cosmic landscape and the multiverse, not to be tricked into thinking that this is all accepted, validated science. To be fair, there is a section ‘Answering the Critics’, in which Duff uses the same arguments that he used in his debate with me. Careful reading of this section should convey the impression that this is all speculative theorizing. But, of course, this section is a lot less interesting than the others.

‘Farewell to Reality’ is not necessarily about changing the status quo, it’s aims are to provide something of an antidote, sponsor some healthy skepticism and encourage debate.

79. **nemo**
June 21, 2013

Paulibus, then you are thinking about a really big one … in which case, yes, may be — Viva! And a glass of wine.

No relation to Verne! We agree much more than what I had thought. I look forward too to reading Jim Baggott’s book.
May be, people should just leave what is indeed more mathematics than theoretical physics compete for funding in mathematics? And call it mathematics? And leave what is indeed more metaphysics than physics compete for funding in philosophy? And call it philosophy? I think it is also a matter of science literacy, among the general public and … I wonder … among some scientists, it seems as if too many practitioners, reporters and readers are confusing or misrepresenting assumptions, predictions, (lack of) experimental evidence and how much human effort or ingenuity it often takes to seek reliable (lack of) evidence. As a great teacher of mine had once remarked, some experiments are truly **epic**! And he was a theorist! So I find it inspiring that Kenneth Wilson had found time to support science literacy and education in schools.

80. **Paulibus**
June 22, 2013

nemo: yes, I agree with you; it’s good to call a spade a spade, especially when it’s been a long time since nature sprung a fundamental physics amazement on us. I hope she’s trying, and physics folk are temporarily just looking the wrong way!

81. **Anonyrat**
June 23, 2013

I imagine the mathematician V.I. Arnold’s view would be a distinctly minority position:
http://pauli.uni-muenster.de/~munsteg/arnold.html

“Mathematics is a part of physics. Physics is an experimental science, a part of natural science. Mathematics is the part of physics where experiments are cheap. “

82. **nemo**
June 24, 2013

That’s a quirky view. Thank you. The whole essay is very interesting.

83. **steve newman**
June 26, 2013

Jim Baggott is not alone with his new book-Farewell to Reality: How Modern Physics Has Betrayed the Search for Scientific Truth

A day later, another book with exactly the same theme will be published-
Bankrupting Physics: How Today’s Top Scientists are Gambling Away Their Credibility (Macsci) [Hardcover] by a german theoretical physicist- Alexander Unziker.

It’s becoming common knowledge. The sceptics are getting a strong voice.
Maybe a better day will be coming.

84. **Peter Woit**
June 26, 2013

Steve Newman,

I haven’t seen the Unzicker book yet, will probably write about it here when I do. From what I have seen from him though, it looks like he’s likely to do a lot more to discredit criticism of string theory than discredit string theory.

This Week's Hype

June 18, 2013
Categories: Multiverse Mania, This Week's Hype

In recent years universities have taken to issuing press releases when one of their particle theorists gets a paper on some speculative topic published in a journal like Physical Review Letters. Many examples of such things, often involving bogus claims to have a "test of string theory" have been covered here.

The University of Delaware has now decided to break new ground: they've issued a press release promoting a 1997 paper by some of their faculty that was published in Physical Review D. I gather the argument must be that it's timely because it's about the multiverse and anthropics.

As a bonus topic in the "This Week's Hype" category, there's a new paper out yesterday promoting the idea that string inflationary models have been successfully tested by the Planck data:

> We argue that as a group the predictions made before Planck agree well with what has been seen…

The authors start out by addressing the obvious problem with string cosmology:

> given the complexity of these models it is worth first asking why they are worth scrutinizing in detail at all. After all, if the data is perfectly consistent with much simpler models, Occam's razor suggests we should leave it at that.

It seems that in our new anthropic multiverse-based framework that drops conventional ideas about predictions and testability, Occam's razor has to go too.

Update: I should have mentioned that the Delaware press release links to the article discussed here.

Comments

1. **Tom H**
   June 18, 2013

   The UD press release links to a SciAm article, which in turn states:
   " … If no new particles appear and the Higgs remains astronomically fine-tuned, then the multiverse hypothesis will stride into the limelight. 'It doesn't mean it's right,' said Bousso, a longtime supporter of the multiverse picture, 'but it does mean it's the only game in town.' "

   "only game in town" ? Where have we heard that before?! 😉

   In the past, I don't doubt the prevailing sentiments were
   "epicycles are the only game in town",
   "the aether is the only game in town", etc etc

   Seems to reflect an absence of a breakthrough insight into the real physical world.

2. **M**
   June 19, 2013

   This 1997 paper, despite being largely ignored, is THE important paper where the connection between anthropic selection and the Higgs mass was pointed out. It is the analogous of the Weinberg paper where the connection between the cosmological constant and anthropic selection was pointed out.

3. **Bee**
   June 20, 2013

   I actually found the Burgess et al paper to be a really useful comparison of previously made constraints with existing data. They also give quite frank assessments on the theoretical appeal of the various models. Isn't that the sorting-out of models that we've all been waiting for? Why are you unhappy with that?

4. **Peter Woit**
   June 20, 2013

   Bee,

   I've never really seen the point of constructing this kind of complicated model, and the Planck data consistent with the simplest model would seem to provide experimental evidence against them. The authors I guess are hearing this from their colleagues, the response of an introduction arguing against one of the most fundamental principles of doing science (Occam's razor) is fairly remarkable.

   Coupling this with attempts to mislead like claiming "the predictions made before Planck agree well with what has been seen" doesn't help.

5. **nemo**
   June 20, 2013

   Kenneth G. Wilson here:
   "If your aim is to have an impact on science literacy — in fact, on literacy in all its forms — you need to rivet your attention on the 46 million students in our public schools, not on graduate students in our universities."
   That is also an antidote against hype.

Kenneth Wilson 1936-2013

June 18, 2013
Categories: Obituaries

Kenneth Wilson died this past weekend, in Maine at the age of 77. Some obituaries can be found here, here, here, and here.

Wilson won the Nobel prize in 1982 for his work on critical phenomena and phase transitions, but his influence on particle theory was arguably even greater than on condensed matter physics. Unfortunately I never got a chance to meet him, but a large part of what I was learning about quantum field theory back in my days as a graduate student came either directly or indirectly from him.

Soon after the discovery of asymptotic freedom in 1973, he started work on developing lattice methods for studying gauge theories non-perturbatively with a fixed cut-off. This founded the whole field of lattice gauge theory, which remains a major and active part of HEP theory. Not many people have a whole section of the arXiv they're responsible for. For his story of how this came about, see his 2004 The Origins of Lattice Gauge Theory.

The reason Wilson was well-placed to quickly get lattice gauge theory off the ground in 1973-4 was that he was one of very few theorists who had been thinking hard and fruitfully about the meaning of non-perturbative quantum field theory. After getting an undergraduate degree in math from Harvard in 1956, he did his thesis work under Gell-Mann at Caltech, finishing in 1961 and developing an interest in the renormalization group. From 1963 on he was focusing his research on strong interactions and the high energy behavior of quantum field theory. This was a time when QFT had fallen out of favor, with S-matrix theory considered the cutting edge. One reason others weren't thinking about this was that the problem was very hard. It was also perhaps the deepest problem around: how do you make sense of quantum field theory? What is QFT, really, outside of the approximation method of perturbation theory?

By the early 1970s, Wilson had developed the ideas about the renormalization group and QFT that now form the foundation of how we think about non-perturbative QFTs. The first applications of this actually were to problems about critical phenomena, and it was for this work that he won the Nobel prize. With the arrival of QCD, these ideas became central to the whole field of particle theory, with much of the 1970s and early 80s devoted to investigations that relied heavily on them. If you were a graduate student then, you certainly were reading his papers.

For more from Wilson himself about his life and work, see his 1983 Nobel Prize lecture and a long interview from 2002 here, here and here.

John Preskill has a wonderful posting up about Wilson, with the title We are all Wilsonians now. He ends it by explaining Wilson's early role in the debate about "naturalness". Wilson was well aware of the quadratic sensitivity of elementary scalars to the cut-off and had argued that this meant that you didn't expect to see elementary scalars at low masses. This argument was developed here by Susskind as a motivation for technicolor. Preskill doesn't mention though that Wilson later referred to this as a "blunder". In 2004 he had this to say:

> The final blunder was a claim that scalar elementary particles were unlikely to occur in elementary particle physics at currently measurable energies unless they were associated with some kind of broken symmetry . The claim was that, otherwise, their masses were likely to be far higher than could be detected. The claim was that it would be unnatural for such particles to have masses small enough to be detectable soon. But this claim makes no sense when one becomes familiar with the history of physics. There have been a number of cases where numbers arose that were unexpectedly small or large. An early example was the very large distance to the nearest star as compared to the distance to the Sun, as needed by Copernicus, because otherwise the nearest stars would have exhibited measurable parallax as the Earth moved around the Sun. Within elementary particle physics, one has unexpectedly large ratios of masses, such as the large ratio of the muon mass to the electron mass. There is also the very small value of the weak coupling constant. In the time since my paper was written, another set of unexpectedly small masses was discovered: the neutrino masses. There is also the riddle of dark energy in cosmology, with its implication of possibly an extremely small value for the cosmological constant in Einstein's theory of general relativity.
>
> This blunder was potentially more serious, if it caused any subsequent researchers to dismiss possibilities for very large or very small values for parameters that now must be taken seriously…

He then goes on to argue at length that the lesson of the history of science is that often what seemed like unlikely possibilities turned out to be the right ones, with the argument for unlikeliness just a reflection of the fact that people had been making assumptions that weren't true and/or they didn't understand the possibilities as well as they thought they did.

Wilson may be no longer with us, but his ideas certainly are, and they're very relevant to the biggest controversies of the day.

Comments

1. **Igor Khavkine**
   June 18, 2013

   Peter, that's a great quote from Wilson. It's great to see such a "radically conservative" position from the man himself. I wonder how it would play with the current mainstream opinion, if it were better known. 😉

   Looking at the list of his achievements, it's hard not to find them impressive. However, it is sad to see that part of his legacy is a wide spread conflation of two distinct concepts: perturbative renormalization in QFT and a lift of classical scaling transformations to quantum theory (aka the renormalization group). Moreover, the idea that some (or all) field theories must be defined with an explicit cutoff (in addition to not having any empirical evidence for the existence of any such fundamental cutoff in our world) is tantamount to forsaking the hope of answering the mathematical question of whether renormalized continuum QFTs can be defined on their own terms, independent of any particular choice of regulator. I don't know of any other, similarly important mathematical question, where the widespread opinion is to simply give up looking for an answer.

   I hope that in the future Wilson is remembered more for his important work on the analysis of scaling transformations in QFT (both lattice and continuum), his work on operator product expansions, his work on lattice field theory itself and his work on critical phenomena in condensed matter systems (where explicit cutoffs do actually exist).

2. **Peter Woit**
   June 18, 2013

   Igor,

   I think that the issue of regulators is only one of many where there's a conventional wisdom among particle theorists that keeps people from working in other directions. Unfortunately, we don't know which of these pieces of conventional wisdom are wrong and will someday be seen as what was keeping people from making progress. It does though look like “naturalness” is a good candidate…

3. **Raisonator**
June 19, 2013

Once again:
http://vmsstreamer1.fnal.gov/VMS_Site_03/Lectures/Colloquium/040623Wilson/vf001.htm

4. **meet**
June 19, 2013

Even if you had met Ken Wilson in person, it is unlikely that you would have gotten anything much out of it. He was a brilliant and very nice person but very, very shy. Perhaps if you had been his student or postdoc … but he accepted very few graduate students. Michael Peskin is perhaps his most famous graduate student. Paul Ginsparg was also his student. It is well known that as an assistant professor at Cornell, Wilson published maybe six papers in five years. But when the time came for tenure, Hans Bethe, who was the Grand Old Man of Cornell Physics in those days, protected Wilson (“This man is deep.”) and insisted that Wilson be granted tenure. The next year or so, Wilson’s groundbreaking papers on the renormalization group were published, and the rest is history. At the Cornell press conference for Wilson’s Nobel Prize, Bethe sat next to Wilson at the table and preened himself like a mother hen. Bethe had a smile six inches across from end to end.

5. **gs**
June 19, 2013

RIP.

During roughly the same time frame in which Wilson was doing his scaling-based work, Mandelbrot was formulating (and popularizing) fractals.

In my amateur opinion each was uninfluenced by, and possibly unaware of, the other’s progress. but in a sense they approached the same monolith from very different directions.

6. **King Ray**
June 19, 2013

Lightspeed, Kenneth Wilson.

7. **Blake**
June 20, 2013

Offtopic: Is John Ellis for real saying that the LHC results are actually “encouraging” for SUSY? Are SUSY proponents becoming divorced from reality, or am I missing something.

http://www.youtube.com/watch?v=F8Z1aKyEwNc&feature=player_detailpage#t=767s

8. **Peter Woit**
June 20, 2013

Blake, I had the Google+ hangout thing on in a window for a while, and did hear Ellis going on about the situation of SUSY being encouraging. This is pretty delusional, but it’s also not really news that SUSY proponents aren’t willing to admit the situation they’re in. So, unless there’s something really surprising, best to avoid yet more of the same discussion about SUSY here, where it’s off-topic.

9. **MathPhys**
June 21, 2013

K Wilson was a scientific and an intellectual hero to many of us who learnt quantum field theory in the late 1970’s and early 1980’s. The fact that he wrote one very deep physics paper per year from the late 1960’s to the mid 1970’s, before he eventually turned his attention to computing, set him apart as a great scientist and a profound thinker who did things his own way, and that we looked up to, but had no way to emulate.

I met him in 1982. He was quiet, reticent, reserved but also very polite. For someone who ran the mile in less than 4 minutes as a young man, he was quite out of shape in his 40’s. Listening to his lecture, then asking him questions, I was struck by how he gave very simple arguments that were natural and totally convincing when he said them, but difficult to reproduce after the discussion. His mind worked differently from most other people. RIP.

10. **q&a**
June 21, 2013

The legend was that Ken Wilson didn’t answer the question you asked, but the question you *should* have asked.

11. **Jeff M**
June 21, 2013

OK, if you get far enough into “The Origins of Lattice Gauge Theory” you find Wilson discussing a bizarre episode from the early 70s of someone who read a paper of his and called him to set up a meeting in Ithaca. At the meeting the person gradually explained to Wilson that his work agreed with this person’s idea that the world was a computer simulation (if fact, a chain of computer simulations). Of course, this is now discussed seriously in many circles, but wow, to have come up with that given what computers were like in the early 70s!! No doubt chemicals were involved…

12. **MathPhys**
June 23, 2013

It’s not clear to me why he wrote *“I kept listening. But then he asserted that eventually one comes to a computer simulation run by a Supreme Being. At that point I managed to terminate the conversation.”* My understanding is that Wilson held religious beliefs.

13. **Peter Woit**
June 23, 2013

MathPhys,

Maybe his religious beliefs included a Supreme Being that didn’t do silly things like run computer simulations…

14. **Peter Shor**
June 24, 2013

If a Supreme Being is omnipotent, why would he choose to simulate a world rather than actually creating one? Would you expect your Supreme Being to do shoddy work? (Of course, computer scientists and physicists may disagree about whether simulating a world is the inferior method …)

15. **MathPhys**
June 24, 2013

I personally think that to think of the universe as a computer simulation is just as good as any other image that one can come up with to put metaphysical concepts into words.

More interestingly (to me) is the fact that I have always remembered that Wilson was a Quaker. I'm not so sure how I knew this, but the last time that I have thought of this cannot be long after meeting him in 1982 or 1983, so it may have been Wilson himself who mentioned it, though that seems unlikely.

He was intellectually miles ahead of anyone else around him, but treated everyone from the most pretentious professor (there were many) to the lowliest graduate students (there was only one) on equal footing and equally seriously. When someone mentioned running the mile in 4 minutes, he smiled and said *"That was a long time ago"*.

16. **hari**
June 25, 2013

Is this the last talk delivered by Wilson?
Only abstract seems to be available.
http://www.worldscientific.com/doi/abs/10.1142/9789814335614_0038

17. **Anonyrat**
July 15, 2013

MathPhys, this database of people does list Wilson as a Quaker.
http://www.nndb.com/people/805/000099508/

Strings 2013 etc.

June 25, 2013
Categories: Strings 2XXX

These days one can just about attend a wide variety of summer conferences from the comfort of one's home or office, with talks appearing online more or less immediately after they are given. This week some possibilities to consider are:

- The big yearly string theory conference, Strings 2013, is in Seoul this week with about 279 physicists in attendance, talks are available here. As usual for recent string theory conferences, there aren't a lot of strings to be seen. Perhaps these yearly conferences should be renamed something like "Conference on the latest topics popular among people who used to do string theory". No sign at all of the landscape or string theory unification. For some understanding of why, see Life at the Interface of Particle Physics and String Theory, to be published by Reviews of Modern Physics, which makes pretty clear why the organizers of Strings 20XX now want to avoid this topic.
- The 2013 Lepton Photon conference is in San Francisco this week, talks here. To mark the occasion, Gordon Watts and Jacques Distler have thrown in the towel, paying up on their bet with Tommaso Dorigo that the LHC would find SUSY or other "new physics". Tommaso has the full story here.
- For the more philosophically minded, the Templeton Foundation is funding a summer institute on the Philosophy of Cosmology, and you can follow the talks here. If thinking about time is your thing and you've missed out on the free summer amongst the redwoods, there's still time to get in line for Templeton funds, with a few days left to apply for grants here.

Update: To round out the list, I should have included something for the mathematical physicists, this week's Symmetries in Mathematics and Physics conference in Rio. Videos of the talks are appearing here.

Update: Jacques Distler has a posting here (or guest post here) conceding the loss of his bet with Tommaso Dorigo. Unlike some other theorists (e.g. John Ellis) who are arguing that everything's fine, one just has to wait until 2015-6 for the higher energy LHC run, Distler is more of a realist:

> …would I be willing to bet on the 2015 LHC run uncovering new BSM physics?
>
> The answer, I think, is: not unless you were willing to give me some substantial odds (at least 5–1; if I think about it, maybe even higher).
>
> Knowing the mass of the Higgs (~125GeV) rules out huge swaths of BSM ideas. Seeing absolutely nothing in the 7 and 8 TeV data (not even the sort of 2-3σ deviations that, while not sufficient to claim a "discovery," might at least serve as tantalizing hints of things to come) disfavours even more.
>
> The probability (in my Bayesian estimation) that the LHC will discover BSM physics has gone from fairly likely (as witnessed by my previous willingness to take even-odds) to rather unlikely. N.B.: that's not quite the same thing as saying that there's no BSM physics at these energies; rather that, if it's there, the LHC won't be able to see it (at least, not without accumulating many years worth of data).

Comments

1. **P**
   June 25, 2013

   Hi Peter,

   It isn't any more right to say that string theory is just unification than it is to say that QFT is just the standard model. The field has given insight into a broad variety of topics in physics (maths, AdS/CFT, field theory dualities, strongly coupled gauge theories, etc etc).

   In this sense, Strings 2013 absolutely is a conference about string theory. I'd challenge you to find more than 10 talks (other than the ones which seem to be from LHC and Planck experimentalists) which aren't string theory outside the realm of unification or topics in QFT and gravity which owe many of their results to string theory.

   Cheers,
   P

2. **Peter Woit**
   June 25, 2013

   P,
   I don't think there's any point in arguing now about what is "string theory" and what isn't, since the term has become rather meaningless as it has morphed into "stuff that is kind of somehow inspired by something someone once found while working on string theory", which covers all sorts of different things.

   The remarkable thing about string theory unification is not just that it's no longer a central part of string theory research, but that the most influential "string theorists" in the world, those who choose talks for Strings 20XX, now won't touch it with a ten-foot pole.

3. **leptonphoton**
   June 25, 2013

   Lepton Photon used to be 'Electron Photon' in the good old days we all hanker for (when there were no blogs?). Technically I think I wasn't born yet so no hankering for me. Is LP2013 exclusively about leptons and photons?

4. **chris**
   June 26, 2013

   the amazing thing about LP13 is that on monday the new SUSY and BSM search results were presented – the last round of new results before the update of LHC – and nobody seemed to care in the slightest. As the exclusion limits on various stuff inches upward – some of it into the 10 TeV range already – it seems that finally noone expects there to be anything but the SM.

5. **nobsm**
   June 26, 2013

   Therein lies the tragedy. It's one thing to criticize ST/SUSY/etc as hype, and to say 'no evidence for SUSY and BSM search at LHC' but 'no BSM' is a tragedy for HEP.

6. **Peter Woit**
June 26, 2013

leptonphoton,

No, the conference for a long time hasn't just been about leptons and photons. Maybe they should update the name…

7. **Eric**
June 26, 2013

I think it has become very clear that most likely the only superpartners which are light enough to be produced at the LHC are neutralinos and charginos. Unfortunately, the production rate for neutralinos and charginos in hadron colliderss is very low, so there is almost no chance of seeing these particles at the LHC. However, it should be possible to the lightest neutralino in direct dark matter experiments and to produce neutralinos and charginos at the furture ILC. So, I would encourage patience rather than the pessimism which is all too prevalent on this blog.

8. **Bob Jones**
June 26, 2013

I don't think there was ever a time when string theory was just an idea about unification. Back in the late 1960s when string theory first got going, it was a theory of nuclear physics. Applications of string theory to mathematics can be traced all the way back to the days of bosonic string theory. String theory also led to the systematic development of two-dimensional conformal field theory way back in the 1980s.

It's not right to say that the subject has "morphed" into something different. String theory has always been related to lots of mainstream topics in mathematical physics. Critics of string theory typically ignore this fact and focus on the more speculative aspects of the subject.

9. **P**
June 26, 2013

Hi Bob,

I absolutely agree. I think we're in agreement, then, that the talks at Strings 2013 are essentially all about, or at least heavily inspired by, string theory.

Since Peter didn't respond to my challenge: can you (or anyone else) find more than 5-10 of the ~40 talks that aren't actually about or inspired by strings?

10. **David M.**
June 26, 2013

Well using a broad definition of string theory I would say the following weren't exactly stringy talks:
Seibergs talk on 4d gauge theories seemed to me like a pure quantum field theory talk with connections to ST. I enjoyed it though, it highlighted the importance of line, surface, and higher dimensional operators in distinguishing different QFTs.

Komargodski's talk (previous version can be found here:https://sites.google.com/site/zoharswebsite/home/recent-presentations) looks to be on the same topic of SUSY QFTs as does Sunjay Lee's. Then theres Rychkov's talk about bootstrapping conformal field theories. The bootstrap program is obviously related to string theory via ads/cft and in his presentation at the String-Math conference he mentioned the possibility of bootstrapping 6d SCFTs, but direct applications to ST didn't come up this time.

Sara Pasquetti's talk looks like its based on a recent paper where string theory only came up because topological strings were used to derive the partition function of the 5d theory on a product of spheres, but once again seems to fall more under QFT then string theory.

So I would argue that Pasquetti, Komargodski, Seiberg, Lee,and Rychkov all had talks that were more field theory with connections and applications to ST then ST talks. Thats not to say I don't think they should be there, its all fascinating work with connections to string theory, but they do strike me more as QFT talks.

There is definitely a dearth of particle physics related talks. Cvetic is really the only one presenting on that subject. There were definitely more pheno talks last year, but given the lack of any good BSM news its not surprising either.

11. **P**
June 27, 2013

I also have Komargodski, Seiberg, and Rychkov not in the "string theory broadly defined" category, but one could argue for Lee and Pasquetti being within string theory for the reasons you mentioned, i.e. due to the usual ST SUSY gauge theory formal math interplay. But that's a matter of taste, I suppose.

12. **Shantanu**
June 27, 2013

Peter one more conference(although with no slides)
Bruno zumino 90th birthday fest
http://ctp.berkeley.edu/brunofest/Schedule.html

13. **tommaso dorigo**
June 27, 2013

Hello Peter,

thanks for mentioning the bet and its (logical, to me) conclusion. When I offered the wager, in 2006, I believe the large majority of my colleagues would have taken it. Now things have indeed changed dramatically. But let's review critically the situation now and then.

What is it that we did not know then and know now ? That large swaths of the MSSM space are ruled out ? That there are no Z' below a TeV ? That large extra dimensions were not just right there waiting to jump at us ?

The Tevatron had done a pretty decent job at showing that every bit of measurement was in accordance with SM predictions. But what really motivated me to offer the wager was observing the incredible agreement of low-energy observables in B physics, where new particles would have made their effects felt from quantum loops. I think we had all the hints that the LHC would be as successful as its predecessor LEP II in the same tunnel: deliver what it promised, and nothing more.

So if I look at the status of our knowledge then and now, I cannot see a qualitative difference. And indeed, that is why I was already reasonably sure we would not see new physics at the LHC. Of course, ready to be surprised, I put all my research time into sharpening the tools of CMS; but with a good dose of pessimism.

Why others had a different opinion ? Sure, theoretical indicia were important: the energy scale was the right one, if one believes naturalness arguments. But I think it has largely also been a sort of bandwagon effect. The LHC appeared such a beautiful new machine that it just was unthinkable it would fail to deliver what theorists assured us we'd find.

I urge everybody who sits in funding committees to consider quite soberly the new proposals of experiments and facilities. There is a large set of offers on the floor, but I believe it just is not the right time to take a decision now. We said we'd build the ILC with the right design parameters once we knew what SUSY particles we'd study. Now let's hold our horses for a few more years. As beautiful as LEP II was, what do we know from it that we would not know anyway without it today ?

Cheers,
T.

14. **Peter Woit**
June 27, 2013

Hi Tommaso,

Congratulations on your winnings!
I suppose I should have joined you in the bet. My own point of view pre-LHC was that things like SUSY and extra dimensions not only lacked any evidence from places they should have already shown up, directly or indirectly, but that they were (and are) badly motivated ideas. All of such models I've seen introduce large numbers of new parameters and new structure, while not giving any answers to the questions about the SM we don't understand. So, for purely theoretical and aesthetic reasons, besides the lack of indirect evidence, such things seemed highly unlikely to turn up at the LHC.

One reason I didn't join your side of the bet was that it was broader, including any deviation from the SM. In particular, since we had not seen the Higgs, it seemed to me not at all impossible that there was something about the Higgs mechanism we were missing, something that would explain all those parameters that come with the Higgs sector. Maybe once the LHC started seeing evidence of the Higgs sector, something non-SM at 5 sigma would be part of it. So far that hasn't happened, but that's what I wasn't willing to bet against back then (although I liked your argument that doing so would be win-win: either one got new physics, or some money).

I think the HEP community as a whole made a huge mistake allowing the LHC to be partially sold as a machine that would produce new dimensions of space (super or otherwise), black holes, etc. This was never going to happen, guaranteeing a heavy price to be paid in credibility down the road. The Higgs question though was very real, and more than enough to justify the project. If the LHC had been sold as purely a Higgs discovery and initial investigation machine, everyone would now consider it nothing but a huge success and if there's a sensible plan for a follow-on machine, it would be easier to get support for it.

To my mind, the big question about a future machine is whether there's an affordable one that can do a much better job than the LHC in studying the Higgs sector. I gather lots of people are working hard now at trying to figure that out…

15. **divisi0n of labor**
June 27, 2013

"string theory unification is … no longer a central part of string theory research"

http://stringpheno2013.desy.de/e209876/

16. **Peter Woit**
June 27, 2013

division of labor,

Funny how in that division of labor the people not doing string phenomenology get the big yearly conference, the $3 million checks, professorships at Princeton/Harvard and completely freeze out the string phenomenologists, while the string phenomenologists get the small satellite conference.

From 2009, and I don't think the situation has gotten better

" I once heard someone say the shortest joke is "string phenomenology"."
http://resonaances.blogspot.com/2009/06/boston-tea-party.html?showComment=1244898346677#c4807554018314481181

17. **piscator**
June 28, 2013

"I think the HEP community as a whole made a huge mistake allowing the LHC to be partially sold as a machine that would produce new dimensions of space (super or otherwise), black holes, etc. This was never going to happen, guaranteeing a heavy price to be paid in credibility down the road."

I totally agree on this. This is also not the fault of string theory. For all the talk of string theory hype on this blog, over the last fifteen years the area of particle theory most guilty of prolonged and unsubstantiated hype is BSM. You can talk all you like about string unification, but string phenomenology papers do not have five thousand citations for failed models. Likewise, you will not find any speakers at String Phenomenology put forward to the public as great physicists of the age in the way that say Nima Arkani-Hamed or Lisa Randall are.

"Funny how in that division of labor the people not doing string phenomenology get the big yearly conference, the $3 million checks, professorships at Princeton/Harvard…"

Watch the future. The strings conference is declining. Take away the 100 locals from Korea, and it had less than 200 people attending. It used to have 500 people and close registration months in advance. The young have voted with their feet. Few young people do the formal mathematical physics that is represented at Strings, and there are almost no jobs in this area.

18. **billy**
June 28, 2013

Dear peter , Can you explain why are you hostile against string theory ?
I think that The theory is highly constrained mathematically , It claims to be a final theory so why should one expect that we should have results now ? I expect that the human quest for a fundamental theory of nature to take a huge amount of time . Don't you agree ? Second , Are all alternatives as satisfactory and well-developed as string theory is ? Why don't you think that string theory is on the right track ?

19. **Peter Woit**

June 28, 2013

billy,

I wrote a book to answer these questions. It was written ten years ago, but things haven't fundamentally changed.

20. **fuzzy**
July 20, 2013

Nice post, Peter!

In an old joke, a guy was sure he had lost his wallet in some dark street, but he was continuing to look under a lamppost, because it was easier. If we set the duality: HEP COMMUNITY="the guy"; BSM="wallet"; LHC = "lamppost" (and possibly DESPAIR="laughter") we have a picture of the way of proceeding in the last 30 years.

Note that I do not write intentionally what is dual to "dark street", but this is not because I lack valid proposals (e.g., NUCLEAR PHYSICS, QCD, ASTROPARTICLE PHYSICS…) but because I think that anybody, working in high energy physics, should take responsibility in offering the best answer he/she can, rather than following canned responses from somebody else.

Incidentally, I note that you, Peter, have suggested the answer "HIGGS PHYSICS" here above. I respect this proposal but I am not sure that I find it convincing, since I do not see any good theoretical reason that this should lead us to BSM.

Hiking the Appalachian Trail

June 28, 2013
Categories: Uncategorized

I'll be heading North tomorrow, ultimately ending up in backwoods Maine, hiking the Appalachian Trail, then back home after a week or so. The comment section here will likely be closed for the duration.

Progress is being made on my notes on quantum mechanics and representation theory, based on the course I taught this past year. About 3/4 done with the writing at this point, the latest version is available here.

Comments

1. **CIP**
   June 28, 2013

   We know that you will actually be visiting your girlfriend in Argentina.

2. **Peter Woit**
   June 28, 2013

   Maybe, but first I have to pick up this suitcase from a friend of hers…

3. **King Ray**
   June 28, 2013

   Peter, thanks for posting a link to your book, it looks pretty good. You're very far along on it.

4. **Low Math, Meekly Interacting**
   June 28, 2013

   I grew up in Maine, and have hiked pretty much the entire ME AT in bits and pieces over the years. There are still stretches of the 100 Mile Wilderness I've yet to traverse, but it's on The List. If you're up there, don't skimp on the DEET. Hope your trip is enjoyable and the weather cooperates!

5. **El-Coco**
   June 28, 2013

   Please be safe on the trail, Peter.

6. **Peter Woit**
   June 29, 2013

   LMMI,

   Thanks, extra DEET was acquired yesterday. Will be up in the 100 Mile Wilderness, but actually planning more hanging around a lodge than strenuous hiking.

7. **Low Math, Meekly Interacting**
   July 9, 2013

   I was in the vicinity while you were in the Wilderness, with my family on the north end of Schoodic Lake to be precise. Sweltering heat for the region, soup-like humidity, and ravenous deer flies. If I didn't have a lake to jump into every hour to cool off I probably would have left. Hope one of the many beautiful ponds up there provided adequate post-hike relief!

8. **Peter Woit**
   July 9, 2013

   LMMI,

   Yes, I wasn't far away, at an AMC lodge on a lake called Long Pond, spent some time hiking nearby at Gulf Hagas, some time on the AT. Weather wasn't bad the days we were there, and bugs were just at the modest annoyance level. Started out with a few days in New Hampshire, where it rained all the time, and ended up with a couple days on the coast, which was pleasant, but hot for Maine I guess. All in all, a nice break from the city, but I'm glad to be back in bug-free Manhattan enjoying air conditioning…

9. **Vincent**
   July 11, 2013

   As a mathematician, I'm very happy that there is a book now called quantum mechanics for mathematicians! I have a question about the introduction, though.

   You write: `The strangeness inherent in quantum theory that Feynman was referring to has two rather di□fferent sources. One of them is the inherent disjunction and incommensurability between the conceptual framework of the classical physics which governs our everyday experience of the physical world, and the very di□fferent framework which governs physical reality at the atomic scale.'

   My question is: what is the other source? Maybe I misread you, but it seems that you use the rest of section 1.1 to explain what you mean by very different in the second sentence, but all this time I am still waiting for the disclosure of the other of the two rather different sources from the first sentence. Can you tell us what it is?

10. **Peter Woit**
    July 11, 2013

    Vincent,

    Thanks for pointing that out, it is really poorly worded. The material from the second paragraph onwards discusses the "second source" referred to, which is the different kind of mathematics basic to quantum theory as opposed to the classical theory. I’ll try and reword that soon, at some point later I’ll likely be rewriting the introduction more extensively.

No Joking Matter

July 8, 2013
Categories: Uncategorized

Back now from vacation, and while I was away several people sent me links to point out that string theory promoters definitely aren't taking a vacation. Links here with a few quick comments, followed by something about the issue of making fun of string theorists.

- Lenny Susskind has a quite good new book out about classical mechanics (see here), but the Economist doesn't want to talk to him about this, instead it's the usual string theory promotional effort:

  > These extra dimensions can be arranged and put together in many different patterns, in a variety of different ways. Not billions, trillions or quintillions of ways, but many more than that. The ways these dimensions are put together into these tiny little spaces determine how particles will behave, what particles will exist, what the constants of nature are—quantities like dark energy or the electric charge of an electron. In string theory all those things are features of the ways that these tiny dimensions are put together. The tiny dimensions are like the DNA of the universe.

- Last month Cumrun Vafa was in Bangalore, explaining (see here, slides here) among other things about the significance of the web of string dualities that makes up M-theory:

  > String dualities in my opinion perhaps the most fundamental discovery that physicists have made in a century.

  Note that the past century includes General Relativity (barely), quantum mechanics, gauge theory, the Standard Model, as well as quite a few discoveries in other areas of physics.

- The Strings 2013 conference included the usual public talks promoting string theory. Witten's was String Theory and the Universe, which was pretty much unchanged (minus optimism about SUSY at the LHC) from similar talks he has been giving for nearly 20 years (since 1995 and the M-theory proposal). Linde's was Universe or Multiverse?, about the "new scientific paradigm" of Multiverse Mania. He argues that the virtue of this is that it is "impossible to disprove".
- David Gross's public talk on The Frontiers of Particle Physics had nothing to do with string theory, focusing on explaining the standard model and some of the questions it leaves unanswered. Much more interesting was his outlook talk at the conference which included the usual exhortations about string theory being alive and healthy, flourishing with many new and brilliant string theorists, but also included some material unusual in such a venue and much more challenging for his audience. His reference to connections between string theory and condensed matter physics described this as having been "overhyped by our community". About AdS/CFT, he noted that it "does not provide a satisfactory non-perturbative quantum gravity".

  He commented on the lack of connection between the talks and HEP physics, saying that it was "important that string theorists not retreat into quantum gravity". About SUSY, he characterized it as "still alive, but not kicking", and he argued that the LHC results of the past year have made more likely the "Extremely pessimistic scenario" of an SM Higgs, no SUSY, no dark matter, no indication of the next energy threshold. Since "HEP is where string theory connects to reality", he made the point to the audience that "if this scenario materializes we are all going to suffer."

  I'm not sure why he picked this date, but he encouraged those with post-1999 Ph.D.s to realize that it was now quite possible that those who came before them had "somehow got it wrong". This was the first time I've seen an influential member of the string theory community raise this possibility and call for people to consider it seriously.

- Sean Carroll deals here with arguments from the "Popperazi" that the string theory anthropic multiverse is pseudo-science by ignoring the serious arguments being made. He has his own definition of what science is, which looks to me to open far more questions than it answers. About string theory unification itself, the question has never been whether it's science, but whether it's an idea worth pursuing given the ways in which it has so far failed. The best argument for continued pursuit of the idea is of course that there aren't obviously better ones around, but this raises its own issues.

Sabine Hossenfelder has a posting about an introduction to a Lawrence Krauss talk where the joke was made that "String theorists have to sit in the back". The context for this was a controversy about the place of women at a discussion involving Krauss hosted by an Islamic group. Like Sabine, I don't want to discuss here that controversy, just agree there's a good case to be made that it's no joking matter. I think she makes a mistake by interpreting the joke as an attack on string theorists (it's a joke, after all, open to many interpretations), but I was struck by her perception of string theorists as an embattled minority under unfair attack, as well as the claim that it's not all right to in any way make fun of them.

The situation these days is clearly very different than it was back in 2004 when I started this blog, partly because of the past decade of failure of string theory unification to get anywhere, partly because of the negative LHC results, partly because of the multiverse, and partly because of the public behavior of some in the string theory community in reaction to criticism. Given the high profile ongoing promotional campaign exemplified above, I don't think we're yet at the point where criticism of string theorists is "kicking them when they're down", and humor is sometimes the best way to make a point concisely. Probably the most incisive criticism of string theory ever made was made in a cartoon, and personally I've never understood how it is even possible to take arguments like Linde's seriously (and am not even sure he does…), so I see a role for humor in charting the continuing story of the collapse of the heavily influential string theory unification paradigm.

Update: If you noticed more than the usual sloppiness, incompleteness and incoherence, maybe it was because this got published early, while I was in the middle of writing it.

Update: The latest on the philosophy of science from Linde (see here), who is at a workshop in Bad Honnef this week.

> The multiverse is the only known explanation so in a sense it has already been tested

Is it all right to make fun of this, or should one seriously discuss the scientific merits here?

Comments

1. **Tim**
   July 8, 2013

   Wired Online magazine has a new ST related story you may want to look at http://www.wired.com/wiredscience/2013/07/hidden-particle-world/

2. **Peter Woit**
   July 8, 2013

   Thank Tim,

That's a well-written article, but I'm no condensed matter expert, so will leave the question of what fraction of the claims discussed are over-hyped to the experts (and David Gross…).

A few years ago, there were a lot of similar claims being made about string theory and heavy-ion physics, but those seem to have suffered from a pretty high hype level, with the ideas involved not working out very well (this topic disappeared at String 2013, from what I can see).

3. **David M.**
   July 8, 2013

   The post 1999 Ph.D comment was made because of one slide in Jeff Harvey's talk where he mentions a disconnect between the older and younger generations of string theorists (a year he also admitted to being arbitrary). Harvey's overview was interesting, he also noted that the AdS/CMT work, although not the panacea some people claimed it might be, was making more concrete progress. I don't think Gross or Harvey were happy that more people are working on AdS/CMT rather then AdS/QCD, but it really is a matter of where progress can be made as opposed to where they wish it could be made.

4. **Anonymous coward**
   July 8, 2013

   1997-98 was when the AdS/CFT breakthrough happened. Gross might choose 1999 as the dividing line if he wishes to club AdS/CFT with (stringy) physics of the generation past.

5. **CIP**
   July 8, 2013

   If, however implausibly, string theorists *were* sent to the back of the bus, it wouldn't a prejudice, but an a posteriori judgment on their past sins against modesty and civility.

6. **B.B.**
   July 8, 2013

   I hope that this change in attitudes will also be translated to other things. In his book "Strange Beauty" George Johnson tells an amazing story about the role of James Bjorken in the recognition of quarks as actual particles, so I thought – here is a guy who actually deserves the Fundamental Physics Prize… Did he fall off people's radar for some reason, or does he still have a chance?

7. **Peter Woit**
   July 8, 2013

   B.B.,

   While Gross is changing a bit his tune in response to experimental results from the LHC, he's not an FPP winner, and has no vote there. Linde and Witten are, and they haven't changed their views at all as far as I can tell. The thing which is going to change the FPP choices in the future is the addition of a large number of experimentalists to the group making the choices (due to the LHC special award this year). I have no idea how that will play out.

   Unless there's some news about the FPP, it's kind of off-topic here (as well as the work of Bjorken nearly a half-century ago), so I encourage commenters to stick to comments related to the posting.

8. **nasren**
   July 8, 2013

   Just read Sabine Hossenfelder's blog post and all the comments. At least in the comments she makes it clear that she doesn't approve of string theory hype any more than string theory derogation. It's the interference in the process of rational assessment she is objecting to. But on this I have to say she is alarmingly late to the party: [*irrelevant material about Krauss deleted*]. I have to agree with one of the commentators: string theorists are now reaping what they sowed when for so long they promoted their theory on the back of zero experimental support. That was never going to end well, and now we are seeing that. Reaper, whirlwind…

9. **Anonyrat**
   July 9, 2013

   I am confused by Sean Carroll's statement that the existence of gauge theory is (crude) data that is explained by string theory. Could one not equally well assert that the existence of fermion and bosons are data explained by string theory?

   If the existence of gauge theory QFTs is indeed explained by string theory (but string theory has no Wilsonian definition) then should not string theory also explain why and how the Wilsonian ideas work?

10. **Bee**
    July 9, 2013

    I didn't interpret it as an "attack", this makes it sound way too aggressive and also way too important. I interpret it as a representation of public opinion, basically. The guy is a semi-popular figure and he was trying to make a joke (funny or not) that he thought would appeal to the audience.

    I just don't think ridicule is going to be conductive to scientific inquiry. I would prefer if both the public as well as young students have a chance to get an objective impression of the situation.

    Your blog has added, and still adds, criticism that had been lacking. Of course criticism is always unpleasant for some people which isn't nice, but then nobody says that science has to be nice. This is fine with me, but criticism is only useful though when it has content and I don't want stupid jokes to affect people's opinions. That's all.

11. **Bee**
    July 9, 2013

    Nasren, I've been writing since at least 2006 about the problem that the public opinion affects researcher's decisions, that this problem is becoming larger due to the increased connectivity of our social networks generally and the scientific community specifically, and that I don't think this is good. This isn't a new theme on my blog, if you care to look. But yeah, I'm late to the party, sorry for not having been around to comment on the elan vital.

12. **Peter Woit**
    July 9, 2013

anonyrat,

Sean's argument that "string theory explains gauge theory" is, for good reason, not one that string theorists typically use, and I don't know why he's using it, especially in the context of trying to explain what the scientific method is.

Bee,

I think we're in agreement about the importance of having objective arguments on all sides available, and that's something I've tried to provide, in a situation that, way back when, was dominated by a one-sided public relations campaign. More recently, both sides of arguments about string theory have gotten an airing, and I think the joke you reacted to was just motivated by someone noticing that there was a controversy, which is not a bad thing. About the use of ridicule, I think it's a problem when used to deal with serious arguments, appropriate when used to deal with ridiculous ones. Undoubtedly I make mistakes, but I hope that I mostly get right which is which.

I just saw your posting about Occam's razor, and one thing that struck me about Sean's definition of science is that this plays no part in it. It's true that in some contexts one has to be wary of the principle and it may not be appropriate. But I do think it's a very fundamental part of the scientific method. One thing it does is tell you about a sort of pseudo-science you have to watch out for, theories that don't actually explain anything, bad models where you just add complexity to keep them out of trouble with experiment. Linde's promotion of a new paradigm where you deal with explaining things by saying anything can happen in your theory, (and claim that as a virtue, crossing the line into the ridiculous) is an indication of where you end up if you think Occam's razor is worthless.

13. **John Urbanik**
July 9, 2013

""Extremely pessimistic scenario" of an SM Higgs, no SUSY, no dark matter, no indication of the next energy threshold."

The above sentence really struck me. Especially the part about 'no dark matter'. Does this actually mean that dark matter is not real and therefore the composition of the universe is wrong? Or does it mean something else?

Does MOND or TeVeS now more to the front? Or am I reading way to much into those three words?

14. **Peter Woit**
July 9, 2013

John,
He was just referring to the (all too real) possibility of no heavy WIMP dark matter particle showing up in the LHC data or the direct detection experiments, a possibility that has always been part of the hope for new physics at the LHC scale, SUSY and other wise. One alternative he mentioned was axions. I'd rather not start a discussion of dark matter alternatives here, that's a very different topic.

15. **cormac**
July 9, 2013

On the ST comment, I too thought it was a silly joke of no significance from a non-physicist looking for a quirky comment.
There is another important lesson here – Chairs should leave humour to the speaker, because you don't know what might offend your visiting speaker. It would have been an awkward talk if Larry had reacted the way Bee did!

16. **Bob**
July 9, 2013

Seen this? Apparently SUSY can't be seen at LHC but can at ILC. Now I understand …

http://phys.org/news/2013-07-ou-physicists-rationale-next-generation-particle.html

17. **nasren**
July 9, 2013

Apologies Bee. I suppose it just seemed to me that a mild joke that was mostly against religious irrationality, not string theory, was a strange thing to protest when there is daily a huge amount of hype emanating from multiple sources. It seemed like a strange point to draw the line.

18. **Peter Woit**
July 9, 2013

Bob,
A good candidate for "This Week's Hype" and further evidence that all university press releases about HEP theory are outrageous hype.

It will be interesting to see if anyone seriously tries to convince the Japanese government to spend the many billions an ILC will cost using this kind of argument.

19. **T.G.**
July 9, 2013

Peter, sorry for being off topic. Maybe in one of your future posts you can mention this poll: https://sites.google.com/site/fieldsmedal2014poll

20. **EDBM**
July 10, 2013

> Linde's was Universe or Multiverse?, about the "new scientific paradigm" of Multiverse Mania. He argues that the virtue of this is that it is "impossible to disprove".

I am currently unable to view the talk using the link (very slow loading for some reason), so I have to ask: With such an attitude, what would hinder me to postulate some omnipotent Creator that moves all the particles in the Universe according to his whim, or that he (Linde) is a brain-in-a-vat? I find it difficult to believe that a physicist would make a statement to that effect…

21. **P**
July 10, 2013

Bob,

Baer has been working on supersymmetric theories which are "natural" in the sense of having a very low mu term down between 100 and 200 GeV. This sets the scale of the Higgsino mass; these fermions are vector-like, uncolored, and extremely difficult to find at LHC. There is signficantly better prospect of finding them at a lepton collider like ILC, particularly given that Baer's favorite mass range is below the proposed ILC center of mass energy.

Whether that warrants another collider (which is probably going to happen anyways) is a matter of taste, but the science is solid. Peter is right that university press releases are overhyped, but it's best to get the facts and understand the science before making condescending remarks; sometimes they are warranted, but other times they expose ignorance.

Cheers,
P

22. **anon.**
July 10, 2013

but the science is solid

No, it's disingenuous. Naturalness (at least in the MSSM and simple extensions thereof) does require light Higgsinos, but it *also* requires light stops and light-ish gluinos, and the LHC is doing a depressingly good job of ruling those out without any help from an ILC. This attempt to refocus on tree-level naturalness and handwave away the loop corrections just doesn't make sense. In general, a lot of the Snowmass process involves people distorting physics to reach the conclusions they want about the future of the field.

23. **anonymous**
July 10, 2013

I noticed an upcoming talk entitled "The Standard Model is complete" in the "New directions in Theoretical Physics" part of the linked conference. We'll have to wait and hear what is said…

24. **Peter Woit**
July 10, 2013

T.G.,
Thanks, that's a topic undoubtedly to come up in the next year.

anonymous,
There may not be much meant by this title other than a reference to the fact that the LHC has discovered the last part of the Standard Model (the Higgs).

25. **P**
July 10, 2013

anon,

Large regions of soft-breaking parameter space are better constrained by a lepton collider than LHC, period, end of story. If you're motivated purely by naturalness then yes, the stop mass is the most important quantity, but as you know a theory with a 10 or 50 TeV stop is a heck of a lot more natural than a theory with no cutoff whatsoever.

Bottom line, you're wrong: it is not disingenuous to discuss how certain experiments might be better for ruling out certain parts of parameter space than others. In fact, it's required, and it's science. Baer's scenario is not my cup of tea, personally, but it's easier to probe light Higgsinos at ILC. There is no question here.

P

26. **Chris W.**
July 10, 2013

Maybe somebody should sit Andrei Linde down and point out the similarity of his remarks to the pseudo-scientific apologetics for Marxism under the Soviets throughout much of the 20th century. You would think he would have the sense to steer *way* clear of that crap.

27. **anon.**
July 10, 2013

Bottom line, you're wrong: it is not disingenuous to discuss how certain experiments might be better for ruling out certain parts of parameter space than others.

I agree with your statement, but that's not what's happening here. The point is that the only argument for Higgsinos being light enough to be in ILC range is naturalness, and it's disingenuous to use naturalness as an argument without noting that the LHC is already putting the whole scenario under strain. It's true that the ILC is a great machine for discovering light Higgsinos, but if you give up on naturalness the Higgsinos could be anywhere from 100 GeV to many TeV, and the ILC covers only a fraction of that range. If all that was happening here is that people were noting that the ILC is better for "certain parts of parameter space", I would have no problem with it. The trouble is the argument attached to favoring those parts of parameter space.

28. **Adam Treat**
July 10, 2013

Hi Peter,

I read with interest Sean Carroll's blog post ostensibly in defense of multiverse theory as a sound scientific pursuit. After setting up his own three-part proto-definition of 'science' which is sorely lacking in many key facets, he goes on to lambast those who have other demarcations. Specifically, three:

1) Science assumes naturalism
2) Science requires falsifiable theories
3) Science requires reproducible experiments

He then makes a rather astounding boast, "…each one of these is straightforwardly false."

This is where his blog post became very interesting for me. I was intrigued to hear how he might straightforwardly demonstrate the error with these claims.

He didn't even come close.

In fact, he backed down almost immediately when it came to the falsifiability claim. He said it was a tricky claim and he couldn't/wouldn't do it justice let alone straightforwardly demonstrate it to be false. His argument against the falsifiability criterion seems to be something like – well, String theory and Multiverse theory are just different from other non-falsifiable theories because it is "clearly are saying something concrete about the world." And also because people who are employed as Scientists are studying it so therefore it must be science.

He totally took a copout. As Sean probably well knows, the problem with the landscape is that it isn't falsifiable *even in principle* and not just because we lack the experimental apparatus to test its claims.

Anyways, he offers much much less than his boast of a straightforward refutation of the falsifiability criterion as a limit on the definition of science. The same goes for his supposed refutation of reproducible experiments. The point is that an experiment must at least be reproducible in principle.

I think the multiverse proponents should learn something from the religious apologists. They could teach them a thing or two. This apologia for multiverse theory is not up to snuff.

29. **Chris W.**
July 10, 2013

> I think the multiverse proponents should learn something from the religious apologists. They could teach them a thing or two. This apologia for multiverse theory is not up to snuff.

Adam, the trouble is that they're acting like they have apparently learned something from the religious apologists, namely, that such apologetics are acceptable as well socio-politically expedient—this is what you do to defend what you "know" to be the truth. As I mentioned, many Marxists once upon a time routinely indulged in a similar practice; presumably a few still do. (Apologetics for capitalism are more in vogue these days.)

I share your disillusionment with Sean Carroll's attempt to defend this position. That said, I suppose he has done us all a service by giving it his best shot and coming up so short. That's one way to realize that such a position simply isn't defensible.

30. **CIP**
July 11, 2013

Bee,

I think the notion that ridicule has no place in scientific debate is extremely naive. Ridicule is appropriate when somebody makes ridiculous claims. If you are offended by the kind of sharp elbows debate that can be seen now, you are really lucky that you weren't around for the great quaternions vs vectors debate a bit over a century ago – not to mention almost all of the scientific debates before and since.

As Peter has been mostly very politely pointing out for years, string theory has often been eager to make claims it couldn't back up. These claims influence public opinion, funding, and faculty appointments. When they are false, they deserve to be corrected forcefully.

31. **Fred P**
July 11, 2013

"Is it all right to make fun of this, or should one seriously discuss the scientific merits here?"

In a setting where the presumed reader has some basic knowledge of science (such as this blog), I'd say it's perfectly fine to make fun of this statement. However, were I writing to a more general audience, I probably would bother to write 2-3 sentences explaining why this is a ludicrous statement.

32. **GoletaBeach**
July 11, 2013

You got me to watch Gross' conference summary. I thought he was a bit more balanced than you portray… he presented the optimistic scenario, did say that entire (which heavily leans on low energy breaking of SUSY) package was alive but not kicking.

I thought, did it ever kick? It was always a sleeping princess waiting for an experimental prince to kiss it and animate a kick.

But as he emphasized, there is a continuum all the way to the extreme pessimistic scenario. The data will decide, but he nicely emphasized that creativity in suggesting new experiments is called for. Of course, some of the best experimentalists, from Luis Alvarez to Mel Schwartz to Burt Richter have been saying that for 30 years at least.

33. **Jim Akerlund**
July 12, 2013

Goletabeach,

It is not that Peter portrayed Gross' speech in an unbalanced way. The way I see it, Gross said there might be a door to non-string theory that Peter has been advocating these many years, and Gross has finally admitted it's possible existence. And this speech occurred in "Strings 2013". That is a sign of a change.

Jim Akerlund

34. **anonymous**
July 12, 2013

Though true, I do find a slight bit of irony in how "completeness" could be listed in the "new directions" section. The ambiguity of the title makes it all the more enticing. There are plenty of other post-Higgs talks in the same conference that presumably cover that material.

35. **Bob Jones**
July 12, 2013

"About the use of ridicule, I think it's a problem when used to deal with serious arguments, appropriate when used to deal with ridiculous ones."

The problem is that you usually don't consider any serious arguments on this blog. You find the most ridiculous press releases and act like they describe everyone in string theory. There are good reasons for taking string theory seriously as a theory of everything, and even if it's not a theory of everything, it has proven to be a useful framework for solving lots of other problems. You do your best to ignore this fact on your blog, but it's the main problem with your thesis. It just doesn't make sense to put down an entire field when it has proven successful in so many ways.

36. **Peter Woit**
July 12, 2013

Bob Jones,

In my book and here, I think I address seriously serious arguments for string theory (as well as ridiculing some of the ridiculous ones). I'd be curious to hear of any serious press releases about progress in string theory that I've ignored.

One of the main problems with string theory is that the leadership of the field has pretty much uniformly refused to engage seriously with the issue of the very real and dramatic failures of the string theory unification program, preferring to keep repeating the same hype, trying to pretend they don't have a problem. As long as this goes on, others should point this out.

37. **Shantanu**
July 13, 2013

Peter, interested n your views in yet another LHC related meeting at KITP
http://online.kitp.ucsb.edu/online/lhc-c13/

38. **Thomas**
July 13, 2013

"successful in so many ways" Is this a joke ?

39. **Peter Woit**
July 13, 2013

Shantanu,
Thanks. I did get a chance to look at the Lykken summary talk, which seemed to be quite good.

40. **Bob Jones**
July 13, 2013

Thomas,

You wouldn't know it from reading this blog, but yes, string theory has been successful in many ways.

It doesn't matter whether string theory is a correct fundamental theory because string theory has some general features that are expected to hold in any successful theory of quantum gravity. For example, string theory provides several concrete realizations of the holographic principle. It has taught us conceptually how black hole entropy can be accounted for by microscopic degrees of freedom in a theory of quantum gravity. These results are interesting because they depend only on the general features of string theory, not the specific details.

But even if string theory had nothing to do with gravity, it would still be interesting because of its connections to quantum field theory. For example, string theory has shed light on the mathematical properties of scattering amplitudes in gauge theories. It provides a tool for studying strongly coupled quantum field theories, and there are now probably hundreds of papers which use string theory methods to understand aspects of nuclear and condensed matter physics. More generally, string theory predicts a web of relationships between different quantum field theories, and by understanding these relationships, physicists have developed new ways of computing quantities of physical interest. Again, these results are all true and interesting regardless of whether string theory provides a correct theory of everything.

Finally, even if string theory had nothing to do with physics, it would still be important because of its applications in pure mathematics. The dualities discovered by string theorists imply profound relationships between seemingly different mathematical structures. It was string theorists who first discovered mirror symmetry, a powerful new tool in enumerative algebraic geometry. Methods from string theory were used in Borcherds' proof of the monstrous moonshine conjecture, and string theory has inspired new mathematical ideas, like the notion of a stability structures on triangulated categories.

41. **Yatima**
July 14, 2013

Well, that discussion has already happened at

Farewell To Reality – Comment Section

… Bob Jones vs. The Rest.

42. **DrDave**
July 14, 2013

"You wouldn't know it from reading this blog, but yes, string theory has been successful in many ways."

One interesting thing to do when reading, especially stuff in politics, is turn the sentence around so that it says the opposite, and see if it makes more sense that way. So in this case, "If you read this blog, you will see that string theory is a failure" is actually more true than the original sentence.

"It doesn't matter whether string theory is a correct fundamental theory because….."
In this case, you can supply any word after the "because" but it won't change the meaning of the sentence, which is also cool.

43. **logical mind**
July 14, 2013

So DrDave, applying your principle, that means that the new sentence is actually less true than the original?

44. **P**
July 15, 2013

Dr. Dave,

Very clever! Your silly argument kept you from dealing having to deal with Bob Jones' point.

I’ll give you the benefit of the doubt, though, and assume you have good reasons for your strong statements. Please, explain to us how string insights into AdS/CFT, mirror symmetry, or QFT dualities have been a failure.

45. **Peter Woit**
July 15, 2013

All,
Please, unless someone has something interesting to say about the actual topics in the posting, enough. The only thing in the posting about string dualities was Vafa’s claim that they’re the most fundamental discovery in physics of the past century, I suppose those who want to claim them as the main success of string theory could address the question of whether they agree with Vafa.

46. **William ML Leslie**
July 19, 2013

In flagrant violation of your request that comments be on-topic, may I point out that labelling your links ‘here’ makes them a little more difficult to grab with a keyboard-driven browser or assistive technologies? It’s a minor nit but I’ve been meaning to mention it for a while.

Otherwise, the multiverse is an answer to a philosophical objection, I don’t even think it was supposed to describe any physical effect. It’s neither science, nor useful.

News From All Over

July 20, 2013
Categories: Uncategorized

- I confess to mostly finding "philosophy of physics" arguments not very helpful for understanding anything, but for those who feel differently, some new things to look at are a Scientific American article Physicists Debate Whether the World is Made of Particles or Fields or Something Else Entirely, an interview with Jonathan Bain, an interview with Tim Maudlin, a debate between John Ellis, Lawrence Krauss and theologian Don Cupitt about Why is there something rather than nothing?, and the talks at a UCSC Philosophy of Cosmology Summer School. Since the last of these was funded by the Templeton Foundation, it ended with several talks on "Implications of cosmology for the philosophy of religion". These included a detailed argument that the explanation for the laws of nature is "there is a perfect being", contrasting this to another argument favored at the Summer School "the multiverse did it".
- This week the Perimeter Institute will host Loops 13, devoted to loop quantum gravity and other quantum gravity approaches. While it's also funded by Templeton, the organizers seem to have managed to keep God out of this one.
- At CERN, Amplitudes, Strings and Branes is on-going. Philip Gibbs has an amusing argument that this and Loops 13 are The Same Bloody Thing.
- One thing the LQG and Amplitudes people do share is that some of their most important ideas come from the same person: Roger Penrose (who, but the way, would be a good candidate for the Fundamental Physics Prize, although his distaste for string theory might be a disqualifier). There's a long interview with him at The Ideas Roadshow, mainly about his "Cyclic Universe" ideas.
- The Simons Foundation has been publishing some excellent science reporting, and now has an online publication they're calling Quanta Magazine. The latest story there is a very good piece on the search for dark matter from Jennifer Ouellette. The Simons Center at Stony Brook now has a newsletter about their activities.
- Another on-going conference is one of the big yearly HEP conferences, EPS HEP 2013 in Stockholm. CMS and LHCb have impressive new results about rare B decays, timed for this conference. For the details, see Tommaso Dorigo. There are also CMS and CERN press releases.

  Last year similar but less accurate results were advertised as putting SUSY "in the hospital", which some people objected to, on the grounds that it was already in trouble and this kind of result doesn't make things much worse. Resonaances had the details, summarizing this a "another handful of earth upon the coffin". The CERN press office tries to put the best SUSY spin on this that it can:

  > One popular theory is known as supersymmetry, SUSY for short. It postulates the existence of a new type of particle for every Standard Model particle we know, and some of these particles would have just the right properties to make up a large part of the dark universe. There are many SUSY models in circulation, and SUSY is just one of many theoretical routes to physics beyond the Standard Model. Today's measurements allow physicists to sort between them. Many are incompatible with the new measurements, and so must be discarded, allowing the theory community to work on those that are still in the running.

- Finally, for those with mathematical interests who have waded through the above, Terry Tao has a remarkable long expository piece about the Riemann hypothesis, ranging from analytic number theory aspects through the function field case and l-adic cohomology.

Update: For more from Penrose, see this recent talk in Warsaw.

Update: Studies in History and Philosophy of Modern Physics is planning a special issue on the significance of the Higgs discovery, the call for papers is here.

Comments

1. **David Appell**
   July 20, 2013

   Peter, what about your hike on the AT in Maine?
   What section?
   Mud? Black flies?

   — David
   (AT 1994: NJ to MA
   AT 1996: GA to MA)

2. **N.**
   July 20, 2013

   To the Templetons: The name and identity of the Perfect Being has been known for some time now. Mila Jovovic.

3. **Anonymous**
   July 20, 2013

   The link to Terry Tao's blog post is broken.

4. **King Ray**
   July 20, 2013

   This link to Terry Tao's page works:

   http://terrytao.wordpress.com/2013/07/19/the-riemann-hypothesis-in-various-settings/

   There was an extra http:// at the end of Peter's link.

5. **Nick M.**
   July 20, 2013

   Terry Tao has more grey matter in the very tip of the nail of his little pinky finger than I have in my entire head! How he finds the energy and time to write with such breath and wherewithal on such a large variety of math related topics is beyond me.

   –Nick

6. **Peter Woit**

July 21, 2013

Tao link fixed.

David,
I was staying near Gulf Hagas, in the 100 mile wilderness in Maine. Only did a few miles of hiking on the Appalachian trail, nothing strenuous. Trail a bit muddy in places, but not too bad. No black flies, a bit late for them, and evidently this year a late frost took care of most of them.

7. **lucretius**
July 21, 2013

The "Perfect Being" should really be "The Best Imaginable Being", i.e. a maximal element in the set of all imaginable beings ordered by the "goodness" of their imaginable properties. One can then easily show that the existence of such a being is equivalent to the Axiom of Choice (this is a mathematical formulation of the so called "ontological argument"). That means that the existence of this "Perfect Being" is undecidable (from the Zermelo-Fraenkel axioms), a fact that was already essentially known to Hume.
PS. This is not meant "seriously". But a lot of serious people have discussed this issue quite seriously http://en.wikipedia.org/wiki/Ontological_argument

8. **Lee Smolin**
July 21, 2013

Dear Peter,

Many thanks for mentioning the Loops 13 conference. I might mention that the 21 plenary talks and more than 130 parallel session talks will be available on line at http://www.pirsa.org/C13029. This year we saw an increase of registrations for LQG conferences to more than 220, which is evidence for the growing vitality of the field. Indeed the majority of our plenary speakers are young and have not given a plenary talk at a loops conference before, they will report on the strong progress in areas such as black hole entropy and temperature, quantum cosmology, phenomenology and increased understanding of the emergence of general relativity in the classical limit.

Thanks,

Lee

9. **tommaso dorigo**
July 21, 2013

Dear Peter,

thanks for mentioning my coverage of the Bs – Bd search in dimuon final states. I have added a discussion of the two Bd measurements (both at 2-sigma, from CMS and LHCb) today. They can be of interest for believers of the SM4 model (four generations).

Personally I don't buy into that stuff much, especially after we found a Higgs-like particle that smells like the Higgs and does couple to photons and has the correct production rate (a fourth generation of fermions would spoil both things). But I have to say that among all extensions of the SM the presence of additional generations of matter is the one which I found the most credible, so I'll keep my eyes open. For the record, both CMS and LHCb measure the Bd rate to μμ pairs to be 3.5 times the SM expectation, albeit with big error bars. A SM4 model would easily fit this in, with a Bs rate equal to the SM one.

Hope you're having a relaxing summer,
T.

10. **Anonyrat**
July 22, 2013

The other works by the author, Meinard Kuhlmann, of the Scientific American piece are here:
http://philpapers.org/s/Meinard%20Kuhlmann

11. **Bill**
July 22, 2013

Interesting story within a story: Will Sawin's impressive list of corrections to Terry Tao's exposition. Who is Will Sawin? Just another 17 year old kid:
http://www.ctpost.com/news/article/Wisdom-beyond-his-years-1390299.php

12. **Bill**
July 22, 2013

Correction: now he is a 19 year old third year Princeton math grad student.

13. **cormac**
July 22, 2013

I think the interview with Jonathan Bain is very interesting, many thanks for the link. Re "philosophy of physics arguments not very helpful for understanding anything", I think philosophy can be useful in understanding and articulating the assumptions one is already making. There are many points in the history of 20th century cosmology where a small examination/attempted justification of underlying assumptions could have been beneficial, from Einstein's static universe to Hoyle's steady-state

14. **Anonyrat**
July 22, 2013

Regarding Tim Maudlin, this is interesting:

> further believe that physicists have been misled by the mathematical language they use to represent the physical world. Temporal structure is part of (maybe all of!) the geometry of space-time, and the standard mathematical description of geometrical structure was developed with purely spatial structure in view. Space, unlike time, has no directionality and the mathematics developed to describe spatial geometry does not easily or naturally represent directionality. The project I have been working on for the past few years involves replacing that mathematical language (standard point-set topology) with a new mathematical language called the Theory of Linear Structures. In the Theory of Linear Structures the possibility of an intrinsically directed geometry arises naturally. If one rewrites Relativistic physics in this mathematical language, the intrinsic directionality of time stands out.

It turns out slated for February 2014, is the first of Maudlin's two planned books on Linear Structures. 'New Foundations for Physical Geometry: The Theory of Linear

Structures',

http://www.amazon.com/New-Foundations-Physical-Geometry-Structures/dp/0198701306

The book description is:

"Topology is the mathematical study of the most basic geometrical structure of a space. Mathematical physics uses topological spaces as the formal means for describing physical space and time. This book proposes a completely new mathematical structure for describing geometrical notions such as continuity, connectedness, boundaries of sets, and so on, in order to provide a better mathematical tool for understanding space-time. This is the initial volume in a two-volume set, the first of which develops the mathematical structure and the second of which applies it to classical and Relativistic physics. The book begins with a brief historical review of the development of mathematics as it relates to geometry, and an overview of standard topology. The new theory, the Theory of Linear Structures, is presented and compared to standard topology. The Theory of Linear Structures replaces the foundational notion of standard topology, the open set, with the notion of a continuous line. Axioms for the Theory of Linear Structures are laid down, and definitions of other geometrical notions developed in those terms. Various novel geometrical properties, such as a space being intrinsically directed, are defined using these resources. Applications of the theory to discrete spaces (where the standard theory of open sets gets little purchase) are particularly noted. The mathematics is developed up through homotopy theory and compactness, along with ways to represent both affine (straight line) and metrical structure."

15. **lucretius**
July 22, 2013

This is the first time I have heard of Tim Maudlin and his "linear structures" so this may be way off the mark but the description above sounds like they could be related to directed algebraic topology, a subject that has been around for a couple of decades.
http://en.wikipedia.org/wiki/Directed_algebraic_topology

16. **lucretius**
July 22, 2013

Actually, I just realised, it was not "the first time" … 😉

17. **Peter Woit**
July 22, 2013

I had noticed that work of Maudlin's, for more, see for instance
http://fqxi.org/data/documents/conferences/2011-talks/maudlin.pdf

Somehow the idea seems to be that replacing the standard mathematical conception of topology (in terms of open sets), by something quite different Maudlin has come up with, he can explain the nature of time. I don't see that he gets anything out of this. Interesting though that one way to get the time and freedom to pursue radically different ideas about mathematics and physics that you'd never get a math or physics department to support, is by becoming a philosophy professor.

18. **weichi**
July 22, 2013

Thanks for the pointer to Jennifer Ouellette's article and Quanta Magazine in general.

Your readers might also enjoy the article on the Minimalist Conjecture: https://www.simonsfoundation.org/quanta/20130709-mathematicians-shed-light-on-minimalist-conjecture.

19. **dan**
July 24, 2013

Hey, does anyone know if videos for "Amplitudes, strings, and branes" will be available online? Thank you. I have for a long time appreviated the information from this blog.

20. **Peter Woit**
July 24, 2013

dan,

It looks like slides for some of the talks are appearing here:
http://indico.cern.ch/materialDisplay.py?materialId=slides&confId=229656

21. **Anonyrat**
July 30, 2013

What Tim Maudlin is seeking:
http://www.ge.infn.it/~zanghi/filo/Tim.pdf

Where are we heading?

July 23, 2013
Categories: Multiverse Mania

Every summer the IAS in Princeton runs a program for graduate students and postdocs called "Prospects in Theoretical Physics". It's going on now, with this year's topic LHC Physics. Much of the program is devoted to the important but complex technical issues of extracting physics from LHC data. Things began though with a talk on Where are we heading? from Nati Seiberg designed to explain to students how they should think about the significance of the LHC results and where they were taking the field.

Most of the talk was about the hierarchy problem and "naturalness", with the forward-looking conclusion the same one that Seiberg's colleague Arkani-Hamed has been aggressively pushing: the main significance of LHC results will be telling us that the world is either "natural" (likely by discovering SUSY) or "unnatural" (in which case there's a multiverse and it's hopeless to even try to predict SM parameters). Given the negative results about SUSY so far, this conclusion pretty much means that the students at the IAS are being told that the LHC results mean it's the multiverse, and they shouldn't even think about trying to figure out where the SM comes from since that's a lost cause. The talk ends with the upbeat claim that this is a "win-win situation": reaching the conclusion that the LHC has shown we can't learn more about where the SM came from will be a great scientific advance and "The future will be very exciting!". Seiberg does at one point make an interesting comment that indicates that he's not completely on-board with this conclusion. He notes that there's a "strange coincidence" that theorists are making this theoretical argument about the necessity of giving up at just exactly the same time in our history that we have run out of technological ability to explore shorter distances. A "strange coincidence" indeed…

For more conventional wisdom along these lines, see Naturally Unnatural from Philip Gibbs, which also argues that what we are learning from the LHC is that we must give up and embrace the multiverse.

Frank Wilczek has just made available on his web-site a new paper on Multiversality. It has the usual arguments for the multiverse, although unlikes Seiberg/Arkani-Hamed he doesn't try to claim that this is an exciting positive development, closing with a "lamentation":

> I don't see any realistic prospect that anthropic or statistical selection arguments – applied to a single sample! – will ever lead to anything comparable in intellectual depth or numerical precision to the greatest and most characteristic achievements of theoretical physics and astrophysics…
>
> there will be fewer accessible features of the physical world for fundamental theory to target. One sees these trends, for example, in the almost total disconnect between the subject matter of hep-th and hep-ex.

and a "warning

> There is a danger that selection effects will be invoked prematurely or inappropriately, and choke off the search for deeper more consequential explanations of observed phenomena. To put it crudely, theorists can be tempted to think along the lines "If people as clever as us haven't explained it, that's because it can't be explained – it's just an accident."

He does see possibilities for understanding more about the SM in two places, the SUSY GUT unification of couplings and axions as an explanation of the smallness of the QCD theta parameter. The last part of the paper is about axion cosmology and anthropics. Wilczek has written about the stories of the 1981 origin of the SUSY GUT unification argument and the 1975 birth of the axion. It's striking that we're 32 and 38 years later without any idea whether these ideas explain anything. A depressing possible answer to "Where are we heading?" would be an endless future of multiverse mania, with a short canonical list of ancient, but accepted ideas about fundamental theory (SUSY Guts, string theory, axions) that can never be tested.

Comments

1. **vmarko**
   July 23, 2013

   Given the negative results about SUSY so far, this conclusion pretty much means that the students at the IAS are being told that the LHC results mean it's the multiverse, and they shouldn't even think about trying to figure out where the SM comes from since that's a lost cause.

   This is extremely bad from an educational point of view. This doctrine of giving up on attempts to explain the SM is horrible even for seasoned scientists, let alone students!

   I am just hoping someone outside string theory will get a flash of inspiration and propose a method to predict at least one coupling constant in terms of the others, for example. If this happens, and if that prediction turns out to be numerically correct, it will be a straight slap in the face to all multiverse/anthropic hype. That is, in addition to being a revolutionary theoretical discovery.

   Best, 🙂
   Marko

2. **Zathras**
   July 23, 2013

   I am surprised the "Where are we heading" talk made such short work of dark matter. With the death of SUSY it would seem that the most likely place to see non-SM physics is with dark matter.

3. **weichi**
   July 23, 2013

   The Gibbs article is quite well written; seems like a very clear exposition of the pro-multiverse point-of view. And Lubos has a nice article in response discussing fine-tuning.

4. **Adam Treat**
   July 23, 2013

   Hi Peter,

   I watched the recent talk by Penrose about his Conformal Cyclic Cosmology theory in Warsaw. He ended the talk with new details about the prediction for concentric circles in CMB data. Apparently he now has another independent team saying they've found the circles in the Planck data and the previous claimants have also updated results.

I bring this up here, because it seems that an affirmative finding of such rings would boost Penrose's theory as a serious opponent of multiverse mania. From what I understand CCC would preclude the idea of a multiverse.

So that is one more hope for stemming the tide of these multiverse propenents. If not Penrose's theory, then perhaps other progress will continue and show the way forward.

5. **Adam Treat**
July 23, 2013

Also, I wanted to ask what you thought of this new talk. I know you have been highly skeptical of this theory as you should be, but at least it is science and making predictions.

6. **Peter Woit**
July 23, 2013

Adam,

I'm still pretty skeptical about the Penrose CCC business, haven't seen any reason for more optimism about it. But I'm no cosmologist…

It also really says nothing at all as far as I know about the SM parameters, and from what I remember, one of the biggest problems is that he has to invoke some unknown mechanism to make masses go to zero and get conformal invariance. Even if there were evidence for his picture, I doubt it would be all that difficult to incorporate CCC in some tweaked form of multiverse mania.

7. **Eric**
July 23, 2013

It really troubles me that people seem to so easily accept the statement to the effect that "supersymmetry is dead". This is not even remotely true. At the worst, one might be able to claim that a low-energy supersymmetric solution to the hierarchy problem requires a small amount of fine-tuning at the 1-3 percent level. It is quite possible that all squarks and sleptons have masses in the range 4-10 TeV, and the hierarchy problem can still be solved with fine-tuning at only the 3 percent level. In this case, there would likely not be an observable signal at the LHC. However, in such spectra the lighest neutralinos are Higgsino-like with fairly light masses which might be observed at the ILC. So, it will not be possible to claim that supersymmetry is dead for quite some time. It would be great if people would educate themselves on the actual situation rather than repeating misinformation.

8. **Brian**
July 23, 2013

Adam, the best argument for Penrose's ideas are the low multipole data from Planck2013 – which somewhat disagree with LCDM. One could argue forever about circles.

9. **Chris W.**
July 23, 2013

We seem to be in a true fin de siècle period. The real seeds of what is to come will germinate largely in the dark.

10. **Peter Woit**
July 23, 2013

Chris W.,

Funny, but I remember thinking we were in a fin de siècle period back in the 90s. Maybe these siècles are getting shorter…

11. **Igor Khavkine**
July 23, 2013

I'm sensing that Peter and other readers of this blog (as embodied in vmarko's comment) consider the realization that the parameters of the Standard Model cannot be predicted as "giving up", in a negative, unwarranted sense. Granted, the reasons given by the likes of Seiberg and Arkani-Hamed, namely the cosmological multiverse, are very dubious. However, I'd like to turn this question around. What reason was there to expect that any of the Standard Model parameters could be predicted at all?

My question is not totally rhetorical, but I think the answer is None, at least not in the sense of vmarko (as it appears to me). My reason for saying so has nothing to do with the multiverse or anything similar. Simply, I don't know that, historically, this ambition has ever succeeded and do know that it has notably failed.

Just to clarify my argument, the situation we have with the Standard Model is that all its parameters are already known (with varying precisions) and no empirical reason to suspect that they are not fundamental (in the usual reductionist sense). There's always room to discover a new pattern or symmetry in among the know parameter values, but that is no longer prediction, just observation.

12. **Peter Woit**
July 23, 2013

Igor,
It's not just continuous parameters of the SM, but also the general features one would like some explanation for. Maybe there is no answer to a question like "Why SU(3)xSU(2)xU(1)?" other than "just because", or "the Multiverse did it". I don't see a solid argument explaining why this is something we can't possibly know, so it seems like a good idea to keep thinking about it and seeing what one can learn. Claiming that the failure of one's pet idea means that no one can ever find the answer to such questions, because the universe was constructed to as to make you fail, seems on the other hand like a bad idea. My problem isn't so much with people raising the possibility that such questions can't be answered, rather them doing so to evade having to admit that an idea they promoted heavily just didn't work.

13. **hhoonneeyy**
July 23, 2013

To be honest, the general attitude towards naturalness vs multi-universe among the students is who cares……

14. **Yatima**
July 23, 2013

"What reason was there to expect that any of the Standard Model parameters could be predicted at all?"

None whatsoever, or the same as the reason to expect that the Standard Model had anything to do with group theory.

On the other hand, why aren't there more free parameters? Is there any reason to expect that may be even less free parameters? Where is the [dividing line between numerology and coincidences of some interest?]

How about one might start considering the multiverse idea when a complete description is found with only one free parameter left (under the constraint of not pushing these DOFs into epicycles), and not earlier? That may take some time though.

15. **Lafargue**
July 23, 2013

hhoonneeyy: Are you attending the program? As student or as lecturer? What *do* the students care about?

16. **amused**
July 24, 2013

Well that all sounds very bleak and depressing, but I'd just like to point out that some people in HEP have been, and continue to be, heading in what seems a much more promising direction: finding new physics beyond the SM in quark flavour physics (CKM matrix elements and that kind of stuff) by confronting increasingly high precision theory calculations (involving Lattice QCD) with experimental measurements. Discrepancy between SM theory and experiment at the 3sigma level has already been reported for a few years now – see e.g. [here] for a recent review. A detailed program for uncovering and studying new physics beyond the SM in the quark flavor sector over the next 10 years has been presented [here].

The lack of attention this seems to get from the rest of the HEP community is astonishing (to me at least). You would think people who lament the lack of prospects for finding new physics beyond the SM would at least be mildly interested in existing 3sigma discrepancies with the SM…
On the other hand, it is amusing and not hard to understand sociologically. The university affiliations of the people doing this stuff are all non-elite – no one from Harvard, Princeton, Stanford etc. If such people were the ones to discover new physics beyond the SM it would be a terrible offense against the natural order of things. So the the elites of HEP can't bear to acknowledge this work, and when they don't talk about it then no one else will – such is the sociology.

(Disclaimer: I don't work on that stuff myself and have nothing personal at stake in it. But as someone who works on formal theory/maths aspects of lattice gauge theory I'm vaguely aware of it from following the lattice literature.)

17. **Krzysztof**
July 24, 2013

Gian Giudice has given in Stockholm a great talk on naturalness :
https://indico.cern.ch/getFile.py/access?contribId=870&sessionId=28&resId=0&materialId=slides&confId=218030

Interestingly, though not supported by Templeton, he managed to evoke St. Thomas Aquinas…

18. **MathPhys**
July 24, 2013

This is no good. This is less than professional. No one should teach ideology camouflaged as physics. Definitely not to students.

19. **Krzysztof**
July 24, 2013

MathPhys: Ideology? I was just joking, as was GG I would guess… To ease the tension let me quote Wolfgang Pauli : "Well, our friend Dirac, too, has a religion, and its guiding principle is "God does not exist and Dirac is His prophet."

20. **vmarko**
July 24, 2013

@ Igor Khavkine:

I'd like to turn this question around. What reason was there to expect that any of the Standard Model parameters could be predicted at all?

Let me comment on this just for completeness. The SM features some 20-30 (depending how you count) coupling constants, and cca. 100 elementary particles, with a lot of various structure (gauge groups, finite groups, generations, flavor, lepton/baryon groups, etc.). When you draw it all down on a piece of paper, it looks very similar to the Mendeleev periodic table of chemical elements — they both have periodic, similar structure, and even a comparable number of 100 or so "elementary atoms".

If history is allowed to teach us anything, the whole SM "smells" like having some simpler underlying structure all over the place. This simpler structure is "simpler" in the sense that it has fewer free parameters and fewer elementary particles, while the SM parameters are to be expressed as functions of these. Btw, this is an old idea, look up on [preons] for some historical attempts to address this.

Granted, it might be that SM really does not have any underlying structure of that kind and that all parameters are really fundamental. But students should not be indoctrinated into thinking that way just because string theory has 10^500 vacua and no way to distinguish between them.

Best, 🙂
Marko

21. **MathPhys**
July 24, 2013

Krzysztof,

I had N Seiberg's talk to summer school students in mind. My point is that his is not straight, undiluted science. It's too ideological.

22. **Krzysztof**
July 24, 2013

MathPhys:

OK – but then, only half-joking, I would say that is rather pseudo-religious, in a direct relation to Pauli's quote. Fortunately, Dirac did not mixed up his believes with physics, but it seems now we are facing this problem in fundamental research. In my view, it could be traced back to the idea of the ToE, which from very beginning sounded pretty non-scientific.

23. **Richard**
July 24, 2013

Peter,

This may be a little off-topic, but have you heard "Popsiecles and Icesiecles" by the Murmaids*?
It's a really good song.

— Rich

P.S. I like SUSY and the Banshees, too.

* Siecles are not the only ones with fins!

24. **weichi**
July 24, 2013

Igor:

"Simply, I don't know that, historically, this ambition has ever succeeded and do know that it has notably failed."

I'd love to hear you expand upon this. What episodes in science do you have in mind? As vmarko points out this ambition has succeeded enormously in regards to the periodic table. I suppose one could argue that the ambition failed in regards to explaining the orbital sizes and masses of the planets in our solar system – we now understand that these things are basically random facts.

But the science of newtonian mechanics and astrophysics has explained so many other things, and has lead us to pose so many new and more interesting questions (questions that astrophysics has also been able to answer!) that I don't think anyone really cares about the "random" answers.

25. **srp**
July 24, 2013

The most fascinating thing in this thread is the deafening silence in response to amused's cogent point. I've tried to point out similar things a few times and Peter asked me to stop (so I have). The only non-sociological explanation I have is that the HEP folks have implicitly decided that the proton isn't fundamental enough to be worth their time–it's gotten classed with those messy composite topics like nuclear physics and chemistry.

26. **Peter Woit**
July 24, 2013

srp,

Actually I'm kind of in favor of deafening silence in response to comments that, while reasonable, could easily turn into an attempt to hijack discussion off-topic to something the commenter is more interested in…

That said, the topic of better understanding QCD and flavor physics isn't a completely neglected one, there are many people working on this. It's likely to get more attention in the US in coming years since US experimental HEP has no short-medium term path to a new high energy machine, but is concentrating on a plan for exploiting a new high intensity machine.

Yes, this kind of research isn't popular at high status places like the IAS, but that's only partly because of the domination by topics like SUSY, string theory, and ideas about extra dimensions. HEP theory has always been very faddish, but when healthy the fads were driven by experiment. The problem here is that the size of apparent violations of the SM that have been seen just aren't convincingly large enough. If there were a solid 5 sigma SM violating phenomenon being observed, I think you'd have almost every prominent theorist at Princeton, etc. dropping everything they were doing and working on something related to that. Absent a convincing deviation from the SM, it's not surprising people looking for something high profile and faddish to work on go for black holes instead.

SUSY has been an endlessly popular fad because it's complicated, but can be studied with quite conventional techniques, so there is lots for theorists and experimentalists to do. The failure so far to see anything I suspect will make this a less popular thing to work on, and no SUSY in the next LHC run will seriously damage interest in the subject. Unfortunately, as long as the faddish sociology continues, what this is likely to mean is just that people abandon one fad for an even worse one (e.g. the multiverse), and my fear about talks like Seiberg's is that they are paving the way for that.

27. **amused**
July 25, 2013

I don't want to be a hijacker and start off a discussion about the prospects for flavor physics 🙂 Will just mention that 3 sigma discrepancy with the SM already seems serious to me, and that the prospects for improving on this with higher precision calculations and measurements to reach the 5 sigma discovery level in the coming years look pretty good (although I'm far from an expert on it). Considering all the current doom and gloom about prospects for finding new physics beyond the SM, I just think people should be aware of this (which most people aren't since it's such an unfashionable topic). And it seems not completely off-topic to mention it here in a thread about "where are we heading". The depressing multiverse isn't the only direction.

I guess what we all ultimately want is to discover the next layer of new physics beyond the SM. From a scientific viewpoint the avenue for reaching this shouldn't matter as long as we reach it. But from a sociological viewpoint it matters enormously…

28. **emile**
July 25, 2013

Hi Amused,

For a CMS or ATLAS physicist, a 3-sigma discrepancy is not that significant because the data are sliced and diced thousands of ways. Many distributions will have had strange features that will have disappeared when adding more data. I agree with Peter that when these discrepancies reach 5 sigma, they will get attention from the community. I also agree that, without a machine that goes beyond 13/14 TeV (or a linear collider anytime soon) and assuming no new physics at those energies, we'll have to look in loops to find signs of anomalous contributions.

Back on topic: I think Wilczek's warning is spot on.

29. **amused**
July 25, 2013

Hi Emile,

At the risk of getting my comment deleted for continuing in an off-topic direction (and I will understand if that happens):
The flavor physics stuff is quite different from the stuff done at CMS/ATLAS: high intensity "precision physics" vs high energy "discovery physics". The 3 sigma discrepancy is not occurring in some experimental signal but rather in a mismatch between various experimental and theoretical constraints on CKM matrix elements (and other flavor stuff). From what I understand (and I'm no expert on this!), the experimental measurements are already very precise, and it is on the theory side that precision is lacking. So the goal (as I understand it) is to increase the precision of the theory calculations, with more precise lattice QCD simulations playing an important role for this, so as to push the existing discrepancy from 3 to 5 sigma thus confirming new physics beyong the SM. Looks like this could happen already in the coming years.
(And if I got some or all of that wrong then hopefully someone more knowledgeable will correct it…)

30. **Ravi**
July 25, 2013

It is interesting that Frank Wilczek's article has the Warning "There is a danger that selection effects will be invoked prematurely or inappropriately, and choke off the search for deeper, more consequential explanations of observed phenomena…. I believe there are at least two important regularities among standard model parameters that do have deeper explanations, namely the unification of couplings and the smallness of the QCD θ parameter (for which, see below)…."

While the only solution discussed by Prof. Wilczek for the smallness of QCD theta parameter is the axionic one that has been around for a long time, non-axionic solutions could well be what nature has picked. In recent papers 1009.5651 and 1203.2772 I showed that just discrete space-time symmetries parity (P or left-right symmetry) and CP (equivalent to time-reversal T) can solve the strong CP problem and predict neutron EDMs in experimentally interesting regions. Moreover 1209.3031 shows that spontaneously broken P itself can stabilize dark matter without need for R-parity etc. Leptonic CP phases can vanish in these models for the same reason that strong CP phase vanished at the tree-level.

We may yet discover that nature was left-right symmetric and time-reversal in its fundamental laws.

(There are also non-axionic solutions that include proposals by several researchers that usually involve either P or CP along with other symmetries such as SUSY, Z_2 and with different predictions).

–Ravi

31. **fuzzy**
July 25, 2013

hi igor,

sm does not have to explain anything of course. it is up to each specific model, that aims to extend the sm and to be predictive, to tell us something useful. (btw, even theta is not a issue in sm; if it is small it stays small.)

supersymmetric sm, with its sliding scale and its huge number of free parameters, does not satisfy the criterion of predictivity in my view. concocting naturalness, in order to camouflage this shortcoming, seems to me a rather mean position.

i think high energy physics should care of measurements. there are not so many measurements we have to account for assuming something outside the sm, but these measurements should be considered with more care and less arrogance.

alternatively, the mask should be thrown, admitting of having forgotten the mission of physicists and of preferring to play to the games of the philosophy.

32. **Igor Khavkine**
July 25, 2013

@vmarko, weichi:

Since the example of the periodic table was brought up, consider its history. Mendeleev identified the periodic structure in the properties of elements. The observation of this pattern was extremely useful and its value stands on its own. However, I don't think this observation is an "explanation" in the usual sense. There were multiple theoretical research programs to actually come up with explanations for the periodic structure and the additional spectral line data. These included vortex knots theories, as well as plum pudding models, and I'm sure others that have been forgotten by history. Successful explanation in terms of atomic structure theory came only after crucial experimental breakthroughs: Thompson's discovery of the electron and Rutherford's discovery of the nucleus. These experiments revealed that there actual subatomic constituents and set the challenge of explaining atomic properties in terms of their dynamics.

That challenge was speedily and successfully met. But note the chronological order of the experimental and theoretical input. Those models that preceded the crucial experimental discoveries are now relegated to history, essentially without exception. The apparent pessimism expressed in my earlier post is an expression of my opinion that the existing theoretical research programs aimed at "explaining" the structure of the Standard Model are most likely analogous to the vortex knot and plum pudding models of the past, highly unlikely to succeed. Moreover, without any sharp empirical evidence of compositness (or something similar) of SM particles, the already observed structure and symmetries of the SM are already a great achievement (like that of Mendeleev), which does not necessarily cry out for any kind of "explanation".

33. **Armin Nikkhah Shirazi**
July 25, 2013

Igor,
You make a fairly compelling argument for your position, but I think there is a loophole.
If the foundations and interpretation of QM and, by extension of QFT, were as settled as they seem to be for classical physics and even that additional 'Understanding' failed to provide any addititional clues, then the outlook might be as bleak as you think. But as it stands, the SM is a sophisticated mathematical pattern fitting scheme with little understanding of its deeper meaning. Feynman understood this, recall his analogy with the Mayan Astronomers, who were able to make accurate precision predictions of planetary cycles with essentially zero understanding of planetary dynamics.
The fact that none of the mainstream interpretations of QM has been able to conclusively establish itself may just be another facet of this problem. I for one would not be surprised if we recognized the eventual 'correct' interpretation by noticing that its application to the SM would point to novel approaches for calculating its parameters. It is quite possible in my view that we have already gathered sufficient experimental data to be able to elucidate the deeper meaning if we were only more imaginative. Remember that the technology that led to Newton's laws was essentially available also to the Mayans, since much of the underlying data leading from Kepler to Galileo came from Tycho Brahe who performed naked eye astronomical observations.
So, to answer the question you posed in your first post, as long as we don't understand the deeper meaning of the SM, we have every right to expect that several or perhaps

even all of its parameters can eventually be predicted because, taking an even longer view of the history of science, so far it has been thus.
Your perspective may be just too entrenched in the current zeitgeist.

Armin

34. **pitpstudent**
July 25, 2013

Peter,

I do not know if you are basing your critique of Nati's talk on just the slides which are online. Because I attended Nati's and Nima's talks at the summer school, and both of them made it very clear they do not like the multiverse at all. They would much rather prefer an alternate explanation, and encouraged students to think outside the box and outside current wisdom, for new ideas.
Nima also mentioned that those people in the field who keep updating their "prediction" for masses of various particles as the LHC bounds improve, give the field a bad name. He said you could keep massaging the theory and making it more and more complicated to escape the LHC bounds, but he doesn't believe in that. He gave some very clear numbers for masses, which seem to be already in some trouble after the first run of LHC, and he said that the 14 TeV run will settle the matter once and for all.

35. **Peter Woit**
July 25, 2013

pitpstudent,

Thanks for the report and comments.

I was basing the comments on Nati's talk just on the slides, and I haven't heard him talk about this. The question isn't really whether he "likes" the multiverse, but whether he's following what is becoming the conventional wisdom of "we don't like the multiverse and anthropics, but the LHC results are making it the leading explanation for fundamental physics." As I noted, from one of the slides he seemed to at least be alluding to one argument against anthropics.

I have seen several versions of Nima's talks about this, in person and online. He's been making the "unless the LHC sees new physics below 1 TeV, the multiverse is the leading explanation, even if we don't like it" argument consistently for many years. I know that he's still describing the situation as naturalness not quite yet ruled out, but the discussion is now about how close to death it is, with even David Gross, one of the great SUSY enthusiasts saying the theory may still be alive, but is not kicking. I don't think Nima or anyone else would now bet a dime on the 13 TeV LHC seeing "natural" new physics, even if they're saying it isn't quite ruled out yet.

In his talks for many years, Nima hasn't been one to say that the LHC would definitely see natural SUSY. He is quite fond of his own "split SUSY" from 2004 which is an "unnatural" version of SUSY, with superpartners that can be at masses inaccessible to the LHC.

I'm glad to hear that both Nati and Nima are encouraging others to challenge the current "we don't like it, but the multiverse is the leading explanation" wisdom. I'd be happier though to hear them challenging the current wisdom themselves.

36. **weichi**
July 26, 2013

Igor,

Thank you, that is very clear! I suspect you are correct that any deeper explanation for the SM will not be discovered without further experimental input. The more interesting argument is that without experimental evidence we have no good reason to expect a deeper explanation. I don't think this is right, but I admit that this could certainly just be a pro-reductionist prejudice of mine.

37. **Shantanu**
July 26, 2013

Peter and others,
FYI: See C. Rovelli's comments about susy etc at LOOPS 13 meeting
http://pirsa.org/displayFlash.php?id=13070083

38. **Noah Smith**
July 28, 2013

Peter:

A depressing possible answer to "Where are we heading?" would be an endless future of multiverse mania, with a short canonical list of ancient, but accepted ideas about fundamental theory (SUSY Guts, string theory, axions) that can never be tested.

I don't understand why this is "depressing". We have theories that are very accurate at predicting everything we can observe at the small scale, given our current level of technology. That sounds like an enormous victory to me.

Unless you can derive all of physics from math – and there seems to be no reason to assume you can – you're going to be left with some empirical facts that just seem to come from nowhere. Doesn't seem too worrying to me.

And it seems to me that "multiverse mania" and untestable theories are silly, but not necessarily harmful. If there are no unexplained phenomena at the small scale, why not let our theorists indulge in science fiction and metaphysics and math until technology improves and some new puzzles and anomalies pop up? It doesn't seem likely to actually hurt the progress of science.

Do you disagree?

39. **Yatima**
July 28, 2013

Noah,

Unless you can derive all of physics from math – and there seems to be no reason to assume you can – you're going to be left with some empirical facts that just seem to come from nowhere.

If the history of physics since at least Copernicus "is no reason to assume that all of physics can be derived from math" and that random ad-hocery like the one found in badly written programs is inherent to the thing …

… then I don't know what you need.

('derived from math' should and must be 'constrained by an appropriate mathematical description', as 'derived' is rather meaningless, because it assumes that the description exists as prior – it doesn't, it must be built first)

40. **emile**
July 28, 2013

Noah, you wrote "And it seems to me that "multiverse mania" and untestable theories are silly, but not necessarily harmful. If there are no unexplained phenomena at the small scale, why not let our theorists indulge in science fiction and metaphysics and math until technology improves and some new puzzles and anomalies pop up? It doesn't seem likely to actually hurt the progress of science.".

I think it hurts science when what is really scientific speculation is not clearly labeled as such. The public can get confused about what are our standards in determining what is "true" about the world. It seems there is pressure from some corners to relax those standards.

41. **Peter Woit**
July 28, 2013

Noah,

"why not let our theorists indulge in science fiction and metaphysics and math until technology improves and some new puzzles and anomalies pop up? It doesn't seem likely to actually hurt the progress of science."

I've no problem with the idea of physicists "indulging" in math. Turning to mathematical physics and working on things like better understanding deep questions about the relation of mathematics and QFT might be a good idea absent hints from experiment. Unfortunately physics in recent years has moved in the opposite direction, with physics departments losing interest in mathematical physics. The conventional wisdom now is that the string theory debacle was due to "too much mathematics", a failure to pay attention to experiment (rather than a failed physical idea).

As for indulging in science fiction or metaphysics, the problem is that people are not going to do this honestly. If you announce that you've given up on real physics and you're now doing science fiction/metaphysics, billionaires will stop giving you prizes, the NSF will not renew your grant, your colleagues will not hire your students, etc. So, unable to do this honestly and above-board, it gets done dishonestly, with people refusing to admit what they are doing. Having a whole academic field take up dishonesty as a major operational principle isn't a good idea. The other problem with this is that you drive out of the field people who still think there is progress to me made, even if it is very difficult. Science fiction/metaphysics is easy, and many people find it entertaining. If it becomes a route to professional success, your field is going to be quickly dominated by it, with no motivation for people to keep thinking about hard problems.

42. **Anonyrat**
July 28, 2013

…..when these discrepancies reach 5 sigma, they will get attention from the community.

Should the community be paying attention to supersymmetry before 5 sigma discrepancies are found with the Standard Model? Since the attention to SUSY exists, it is clear there are strong expectations of where the signal of new physics will come from (SUSY – more likely even without 3 sigma discrepancies, lattice QCD discrepancies – less likely, even with 3 sigma discrepancies). The nature of HEP research is that one has to place such career bets; what I'm curious about, and perhaps those in the know can tell us, how much have people in the community hedged their bets? That may help with understanding where we are headed.

43. **Bob Jones**
July 28, 2013

"Turning to mathematical physics and working on things like better understanding deep questions about the relation of mathematics and QFT might be a good idea absent hints from experiment."

The kind of work you describe is practically synonymous with string theory. I guess you'd like to see physics departments hire more string theorists.

44. **Peter Woit**
July 28, 2013

"The kind of work you describe is practically synonymous with string theory"

If only…

45. **Bob Jones**
July 28, 2013

Do you want to elaborate on what's wrong with my statement?

46. **Ravi**
July 28, 2013

I am surprised that Prof Seiberg's talk "Where are we heading" has a basic mistake that should be corrected — Slide 23 says "Strong CP problem … the explanation must involve low-energy physics". This is the case with axionic solutions — in the sense that axions have only a very small mass. It is not in general true for axionless solutions like the ones I mentioned before. All new particles in these models can be at a high scale so that standard model is the effective low energy theory — however they can predict some of the low energy parameters such as neutron edm (or \theta) and leptonic phases as in the papers I mentioned before.

The naturalness of smallness of the Higgs mass (compared to Planck mass) probably needs low energy physics. But naturalness of smallness of the strong CP phase does not need low energy physics.

Ravi

47. **Another Igor**
July 29, 2013

This discussion reminded me that "the difference between theory and practice is much bigger in practice than in theory".

48. **Peter Orland**
July 29, 2013

Bob Jones,

Peter Woit hasn't answered you yet, but I'll offer my response, for what it is worth. Here is what is wrong with your statement:

THE issue in quantum field theory is quark confinement in renormalized asymptotically-free non-Abelian gauge theories. Not supersymmetric N=4 or N=2 theories (though N=1 would help). There is inspiration from (mostly late sixties early seventies) string theory. AdS/QCD models are exactly that – models. They have the same problem that lattice strong-coupled models had in the mid-seventies.

However the confinement problem will be solved, it's clear that 15 years of AdS/QCD has done no better than the 3-4 years of lattice strong-coupling expansions (which the lattice people abandoned once Monte-Carlo methods started to work).

By the way I've spoken many times with people who use AdS/QCD to make claims about entropy, glueball spectra or whatever, in New York, Santa Barbara and Copenhagen. None of them claim to do anything at small bare coupling, where the theory is renormalized.

Maybe some parts of string theory will help. But none of the string theorists I know work on this problem. And they don't plan to either.

49. **Bob Jones**
August 2, 2013

Peter Orland,

Sorry for the late response. I stopped following this thread for a while.

The problem you're talking about is of course very important. I don't know enough about the subject of AdS/QCD to say whether string theory will lead to a complete understanding of quark confinement, but as you say, it's conceivable that some parts of string theory will help.

I think Peter was talking about something else though. It sounds to me like he wants more physicists to study how ideas from quantum field theory can be used to solve problems in pure mathematics. This sort of "physical mathematics" is almost always done by string theorists who are motivated by questions in string theory. Indeed, it's hard to think of many examples of this sort of work which are not directly related to string theory in one way or another.

Bankrupting Physics

July 28, 2013
Categories: Book Reviews

I just spent a depressing and tedious few hours reading through Bankrupting Physics, an English translation of Alexander Unzicker's 2010 Von Urknall zum Durchknall written in German.

When I started reading the thing I wasn't expecting much, but figured it would be some sort of public service to take the time to identify what Unzicker had to say that made sense and what didn't, and then write something distinguishing the two here. After a while though, it became clear that Unzicker is just a garden-variety crank, of a really tedious sort. Best advice about the book would be the usual in this situation, just ignore it, since no good can possibly come from wasting time engaging with this nonsense. I have no idea why any publisher, in Germany or here, thought publishing this was a good idea.

If you must know though, here's a short summary of what's in the book. The first half is about gravitation, cosmology and astrophysical observations. Unzicker's obsessive idea, shared with innumerable other cranks, is that any scientific theory beyond one intuitively clear to them must be nonsense. Similarly, any experimental result beyond one where they can easily understand and analyze the data themselves is also nonsense. He's a fan of Einstein, although thinks general relativity somehow needs to be fixed, something to do with it getting phenomena involving small accelerations wrong. There's endless complaints about how cosmology involves too many parameters, and dark matter/energy shows that physicists really understand nothing.

When he gets to particle physics, we learn that things went wrong back when physicists started invoking a symmetry that wasn't intuitively obvious, isospin symmetry. According to Unzicker, symmetries in particle theory are all a big mistake, "the standard model barely predicts anything", "the standard model can actually accommodate *every* result", and endless other similar nonsense. As for the experimental side of things, he takes a comment from Feynman about renormalization in QED, claims it means that there is no understanding of production of photons at high energy, then uses this to describe as "It's just ridiculous" data analysis at HEP experiments. High energy physics experiments are all just a big scam, with the physicists involved unwilling to admit this, since they've wasted so much money on them.

The last part of the book contains lots of criticism of string theory, etc., much of it parroting my book and blog. According to Unzicker:

> Woit does a great job in debunking the string and SUSY crap. Unfortunately, he has pretty mainstream opinions with respect to the Standard Model.

Well, maybe he does get *something* right… I have to admit that one of the things that every so often makes me wonder if I'm completely misguided, and maybe there is a lot more value to strings/SUSY/branes/extra dimensions etc. than I think, is reading rants like Unzicker's.

So, my strong advice would be to do your best to ignore this. Luckily, there's an infinitely better book coming out here in the US at the same time: Jim Baggott's Farewell to Reality, which I highly recommend. It seems likely that the two books will get reviewed together, giving Unzicker far more attention than he deserves. If so, at least this will provide a real-life experiment indicating whether book reviewers can tell sense from nonsense.

Comments

1. **lun**
   July 29, 2013

   If he says what what you write in the review about the Standard model he is definitely a crank. Cosmological models do have a whole bunch of little understood and somewhat arbitrary parameters through.

   Most importantly, "if you cannot explain something you have not really understood it" is a saying of famous crank Richard Feynman. Who also wrote this from a gravity conference in Warsaw:
   ——————————————
   I am not getting anything out of the meeting. I am learning nothing. Because there are no experiments this field is not an active one, so few of the best men are doing work in it. The result is that there are hosts of dopes here (126) and it is not good for my blood pressure: such inane things are said and seriously discussed that I get into arguments outside the formal sessions (say, at lunch) whenever anyone asks me a question or starts to tell me about his �work.� The �work� is always: (1) completely un-understandable, (2) vague and indefinite, (3) something correct that is obvious and self-evident, but worked out by a long and difficult analysis, and presented as an important discovery, or (4) a claim based on the stupidity of the author that some obvious and correct fact, accepted and checked for years, is, in fact, false (these are the worst: no argument will convince the idiot), (5) an attempt to do something probably impossible, but certainly of no utility, which, it is finally revealed at the end, fails (dessert arrives and is eaten), or (6) just plain wrong. There is a great deal of �activity in the field� these days, but this �activity� is mainly in showing that the {67} previous �activity� of somebody else resulted in an error or in nothing useful or in something promising. It is like a lot of worms trying to get out of a bottle by crawling all over each other. It is not that the subject is hard; it is that the good men are occupied elsewhere. Remind me not to come to any more gravity conferences!
   ——————————————

   Unfortunately, this kind of research activity is most likely a lot more common than in Feynman's time, both in gravity and particle physics.
   The guy might be a crank, but regarding intuition he might have a point.

2. **Ray**
   July 29, 2013

   Now you know how string theorists feel about you.

3. **Mark**
   July 29, 2013

   Peter, why didn't you follow your own advice? Just ignore him….

4. **CFT**
   July 29, 2013

   Mr. Woit,
   When particles are virtually pulled from the void or vacuum (ex nihilo) to allow mathematical models which contain no physical extension to function, yes, my intuition or sense of causality tells me something is wrong, starting with a lack of a logical mechanical explanation of what the heck is actually going on. Your comment about the 'infinitely preferable' "Farewell to Reality" kind of sums up what the very problem itself with physics really is: Physics is no longer even trying to describe reality, It's more interested in trying to describe imaginary internally consistent mathematical spaces that have no overlap with how our reality functions. For the record, Einstein often admitted his work in relativity was unfinished and still had some problems until the day he died. Feynman himself admitted (in his more lucid moments) that

something was wrong with the very heart of QED, and that he thought it wasn't mathematically valid. You yourself have commented at some length about the abandonment of any kind of scientific rigor in string theory. I ask you honestly, do you think you are being fair calling a person a 'crank' because they point out the standard model is a heuristic model that requires many of its parameters to be 'fine tuned' by hand just to get agreement with experiment? Didn't Lee Smolin say just about the same exact thing in his book "The Trouble with Physics" and describe it as a fundamental issue with the standard model?… are you calling him a 'crank' too?
*
Maybe you should be supporting strings/SUSY/branes/extra dimensions, since you seem equally unwilling to critically examine your own mathematical underpinnings and assumptions. Mr. Woit, If you expect others to seriously question their assumptions, you best be ready to do so yourself without complaint and refrain from calling them a name for questioning your beliefs.

5. **Andrew Foland**
   July 29, 2013

   I've never heard of either the author or the book–is there some building buzz around it that needs to be addressed? How did you come to want to write a review on it, if it's purified crankery? (A conclusion that is not disconfirmed by glancing at the author's list of publications.)

   Just curious if there's a backstory here I'm missing.

6. **P**
   July 29, 2013

   CFT,

   As a string enthusiast, this may be the first time I come to the defense of Peter 🙂

   Peter says:
   "symmetries in particle theory are all a big mistake, "the standard model barely predicts anything", "the standard model can actually accommodate every result", and endless other similar nonsense"

   and you say:
   "the standard model is a heuristic model that requires many of its parameters to be 'fine tuned' by hand just to get agreement with experiment."

   but these are very different things indeed. Peter undoubtedly would love to know why the Yukawa couplings are what they are, as would I, but it's just flat wrong to say that the standard model doesn't predict anything, or that it can accommodate every result.

   You want Dr. Woit to examine "his assumptions" about the standard model, etc. But the fact is that this has happened since the early 70's, with experimental confirmation after experimental confirmation. Scientific consensus typically develops for a reason, and in this case it is 40 years of agreement with experiment. The consensus in high energy physics now — string theorists, particle theorists, and experimenters alike — is that the standard model is the correct description of visible sector particle interactions below the weak scale.

   Asking one to "re-evaluate assumptions" about the standard model in the way Unzicker would like (though you are likely being much more fair than Unzicker) is the intellectual equivalent of creationists wanting us to "re-evaluate" 150 years of evolutionary biology.

   Cheers,
   P

7. **Peter Woit**
   July 29, 2013

   CFT,
   Unzicker's claims are quite different than Smolin's and go way beyond your description of them. The quotes about the standard model I included above speak for themselves. Beyond criticizing theorists, he explicitly makes the case that the work of HEP experimentalists is "just ridiculous". His justification for this is the claim, based on his reading of a comment in Feynman's Lectures on Physics, that there is no valid formula for the production of photons by an accelerated charged particle, so HEP experiments can't possibly understand their backgrounds and reliably identify phenomena like the Higgs. I think these claims speak for themselves.

   Mark/Andrew,

   I wrote about this because unfortunately I think it's about to get a lot of public attention and can't be safely ignored. For one thing, a major publication contacted me about their plan to review it, asking if I was available. That one probably won't happen, for some completely unrelated reasons. More amusingly, here's the story of how I got a copy of the book. I was in a Barnes and Noble here in New York, looking at books in the physics section, when one of the book-sellers came up to me, picked up a copy of Lee Smolin's latest book, and asked "aren't you this guy?" I had to explain that this wasn't quite right, luckily there was a copy of "Not Even Wrong" on the shelves I could point to. He then said there was a new book he wanted to ask me about, and brought me over to the new book display, which prominently featured the Unzicker book, which was the one he wanted to know about. I think he and many others like him are curious about the book and deserve an explanation of what's in it.

8. **Zathras**
   July 29, 2013

   Unzicker is listed as a "German theoretical physicist and neuroscientist." Does he do both actively?

   And it is quite ironic to have a neuroscientist complain about hype in theoretical physics.

9. **Low Math, Meekly Interacting**
   July 29, 2013

   To maybe put you in a better mood, there's a very nice (for a change) review of "The Universe in the Rear View Mirror" in New Scientist. Contains a boatload of praise for Noether and her Theorem. Seems like precisely the kind of thing we could use more of in popularizations of science.

10. **Low Math, Meekly Interacting**
    July 29, 2013

    I should clarify I mean the review itself, not necessarily the book, which appears to be merely OK overall, but at least gets some things right. The review at least makes this clear in a very level-headed way, I think.

11. **Oldster**

July 29, 2013

Unzicker's comment about small accelerations and general relativity that you mention, sounds as if he's thinking about some theory such as Milgrom's MOND being incorporated into a modified general relativity. People have attempted to do this, but to the best of my knowledge, nobody's effort has achieved wide acceptance …

12. **Jeff M**
July 29, 2013

@LMMI,

Thanks for the heads up about the review at New Scientist, it is very nicely done. As a mathematician it's hard to imagine that Noether deserves wider fame, in math she's quite famous thank you. Even to an geometric analyst like me, who spent quite a lot of time trying to avoid algebra whenever possible 🙂

13. **David Bailey**
July 29, 2013

I don't normally comment here because as Peter says, this is not meant to be a general physics forum, and my reasonably detailed knowledge only extends as far as non-relativistic QM – as applied to chemical systems, and even that is very stale by now.

However, Unzicker's book is clearly meant to appeal to a much wider audience, that include me, and we have all – even those that never understood algebra at school – contributed to the roughly £7nb cost of the LHC. His book is likely to ring a bell with a lot of people, and maybe help to bring about a reassignment of research priorities and resources – fairly or otherwise!

I think a lot of people have an uneasy feeling about theoretical physics – one that you yourself obviously share when it comes to string theory. It is that if you take an army of mathematicians and set them loose to produce a theory that fits the experimental facts – totally disregarding the physical meaning of most of the variables in the theory – they may well come up with an explanation that fits some of the facts (with a few dozen adjustable parameters), and which is too computationally dense to calculate others. For example, the Standard Model obviously was not able to supply the mass of the Higgs Boson – only an experiment could do that.

Given that thought, it doesn't seem impossible that a whole chunk of the modern edifice of physics is wrong – only appearing to be correct because it was cherry picked from 10000 other theories that could not fit the facts. Even when a theory makes a prediction which is subsequently proven correct there is still the nagging possibility that it has fitted a pattern that was actually produced by a completely different mechanism!

People also see, for example, that a phenomenon such as the speed of rotation of stars in galaxies is interpreted in terms of new concepts – dark matter – rather than daring to suggest that General Relativity might not be correct at galactic-wide distances. In other words, if new evidence arrives that contradicts a reasonably well established theory, the response it to keep the theory and add another epicycle!

The LHC was sold as producing a 'God particle' or mini black holes, and the day may be coming when politicians and public want to see something more impressive than a bump on a graph!

14. **woit**
July 29, 2013

David Bailey,

People should certainly be skeptical about scientific claims with no serious evidence that backs them up. But what Unzicker is doing is something completely different, attacking extremely well understood and extensively tested physics, without showing any signs of understanding anything about it. There is no point in trying to seriously intellectually engage with this kind of critique, it's just nonsense and a waste of everyone's time.

15. **Chris W.**
July 29, 2013

> Given that thought, it doesn't seem impossible that a whole chunk of the modern edifice of physics is wrong – only appearing to be correct because it was cherry picked from 10000 other theories that could not fit the facts. Even when a theory makes a prediction which is subsequently proven correct there is still the nagging possibility that it has fitted a pattern that was actually produced by a completely different mechanism!

David, that has always been the case. What do you think the logic of theory confirmation is about? Physical theories are never proven true, they are only proven successful across some realm of application. That is, they build up a solid track record in research.

The real issue here is that talk is cheap; if one thinks there is a completely different "mechanism", then one should step up to the plate and propose a serious theory that incorporates it, and show how it might be tested—i.e., observationally distinguished from the currently preferred candidate—and then struggle through the process of comparison and confirmation. Doing that has become progressively harder in the last 2oo years. The Standard Model is well-entrenched for many good reasons (and some bad reasons too, perhaps, but that's life).

16. **Yatima**
July 29, 2013

Mr. Bailey,

I am sad to say that your comment is unfortunate proof of exactly the armchair ragefisting of the populace that gets its "science news" from useless TV crud à la BCC or from the sensationalistic headlines and pseudo-scientific dross from the "scientific" press.

I am not a physicist but I am biting through the books when I have the time and by golly…

"I think a lot of people have an uneasy feeling about theoretical physics"

A lot of people have an uneasy feeling when trying to find their own behind with both hands! Most can't make the difference between an integral sign and a random squiggle and they sadly take pride in this. The way things are going Orwell-shaped really fast, their uneasiness is bound to increase. So what. Physics is not the Wellness Studio.

"It is that if you take an army of mathematicians and set them loose to produce a theory that fits the experimental facts – totally disregarding the physical meaning of most of the variables in the theory"

None of this ever occurred. The world does not work as in a Perry Rhodan story. You cannot just "produce a theory" by committee, Nothing is being disregarded. No "fitting (using polynomials?) of experimental facts" occurs. This is exactly why there are problems now – there is not enough fresh data to reasonably know where to go.

“For example, the Standard Model obviously was not able to supply the mass of the Higgs Boson – only an experiment could do that.”

You are crashing open doors. And your statement is in complete contradiction with your previous one. You will probably find that no-one disputes that the mass could not be predicted (although I have recently read about the idea that if the m_higgs is set to 1, then the vector of all the other particles’ masses is of unit length…). That’s why the experiment was done in the first place.

“Given that thought, it doesn’t seem impossible that a whole chunk of the modern edifice of physics is wrong”

Quite likely. If you know how, you know how to post on the arxiv.

“Only appearing to be correct because it was cherry picked from 10000 other theories that could not fit the facts.”

Yet again, you do not understand how these ideas are put together. There were not “10000 other theories”, ready made, to be discarded on a whim by a hidden committe of wise men. The current edifice was constructed from those elements that seemed the most parsimonious description (i.e. no epicycles), that showed a reasonable degree of mathematical consistency and that survived the test of experiments.

Well, maybe there are “10000 other theories” out there but humanity sure isn’t old enough to have explored the first one.

“People also see, for example, that a phenomenon such as the speed of rotation of stars in galaxies is interpreted in terms of new concepts – dark matter – rather than daring to suggest that General Relativity might not be correct at galactic-wide distances. In other words, if new evidence arrives that contradicts a reasonably well established theory, the response it to keep the theory and add another epicycle!”

Completely wrong. GR is the most successful description ever. Modifications of that venerable structure, in minor or major form, is being attempted at regular intervals, but so far nothing of this has proven particularly compelling. Tell you what: why dontcha go to this link and read up on it: [Alternatives to general relativity]. Knock yourself out!

Dark matter is a very simple explanation, not “another epicycle” and until something reasonable comes up that explains all the experimental evidence right down to WMAP data, it will be kept.

“The LHC was sold as producing a ‘God particle’ or mini black holes, and the day may be coming when politicians and public want to see something more impressive than a bump on a graph!”

The LHC was sold as a “Higgs factory”. “God particle” moniker came from some lousy reporter and “Mini black holes” came from very speculative ideas that no-one thought would pan out.

“When politicians and public want to see something more impressive than a bump on a graph!”

Like what? Two bumps good enough? Shall one throw in the DVD collection of American Idol and a Coke for free? Seriously, whaddya want? Cold fusion? A recipe to monetize debt without concomitant price inflation? Eternal happiness? You ain’t gonna get it.

17. **chris**
July 29, 2013

oh my. as if it wasn’t bad enough that this guys second book in German just appeared, this drivel gets translated into english now. i just hope he won’t have such a large audience as he unfortunately has here in Germany.

for those who don’t know: he’s a frustrated high school teacher (frustrated because his scientific career failed) and now grinds his own axe in revenge.

18. **David Bailey**
July 29, 2013

Yatima,

I believe Leon Lederman coined the expression “The God Particle” (at least he used it in the title of his book), not a “lousy reporter”, and my point was that the physics community has allowed hype to run wild, and ultimately people and politicians are likely to start asking what that hype really amounts to, and whether it was worth £7bn!

19. **srp**
July 29, 2013

It seems that our host is taking up my earlier suggestion (in the context of the debate over open-access journals) for “anti-publishing”–effectively eliminating from readers’ purview items that are a waste of time.

The only scruple here is you really have to trust the anti-publisher and you have to have a pretty clear idea about what is a waste of time. For example, there might be sociologically interesting patterns in the things said by “cranks” and dissenters and the ways in which they are kept off stage by actions like Peter’s.

But even as someone with a stronger taste for contrarian views than usual, the quotes and description in this post are sufficient to anti-publish this book for me.

20. **Matt**
July 29, 2013

Hi Peter,

Related to this discussion of deciding when a scientific approach is valid, I have a question that I never thought to actually ask you before now—if you have, in fact, answered it before, I’d appreciate it if you’d please let me know where so I can look it up.

The most familiar way to make progress in science is to be confronted with data, either by accident or because of looking at some new feature of the world, and then trying to find a parsimonious explanation that ends up making new nontrivial testable predictions about data not yet in hand.

But what if we already have several theories that exactly account for a large set of our observations and, while not explaining certain other observations, don’t actual contradict any data we know about. And suppose that these theories we already have don’t agree on their mutual overlap, but that mutual overlap occurs only for data that one can show on general grounds are ridiculously out of reach of conceivable experiments, possibly always out of reach.

In general, one might say we’re stuck and simply should go any further. Just do experiments where we can, far below where these mysterious data are, and just be satisfied.

But suppose that when we look at the theories we know, we find that their mathematics creates a long series of highly nontrivial constraints on any other possible candidate theories that could possible extend them. That is, suppose the theories don't allow arbitrary speculation. And suppose that theorists find a candidate theory that beats the odds and actually makes it through that crazy gauntlet, and despite much work no other approach comes anywhere close to accomplishing the same feat.

By construction, this candidate theory cannot predict any data we have access to, or will have be guaranteed to have access to in the foreseeable future. All those data are explained by the theories we already know and trust.

My first question, and I'm interested to hear your answer: Is it worth provisionally proceeding with this approach?

This is the situation string theorists find themselves in right now. The first quantum corrections to gravity in our solar system are 10^-70 effects. Earth's Bohrian principle quantum number is somewhere around 10^150. There are lots of other examples—when we combine quantum mechanics and gravity, we find that the discrepancies in the resulting data involve crazily inaccessible scales. This isn't the fault of string theory, or LQG, or any particular candidate theory of quantum gravity. It's the nature of the data at which quantum mechanics and gravity become incompatible.

And yet in order to have a quantum theory with classical general relativity in the low-energy limit, one runs into a huge number of highly nontrivial constraints, which one might have doubted could be passed any any conceivable theory. But somehow string theory passes them.

So my second question: Do you have a detailed understanding of all those constraints string theory has passed? To the string theorists, this is by far the biggest deal—this is the absolute fulcrum of their entire case—so I was wondering if you are aware of the full—and I mean full—list and how delicately string theory manages to make it through, and what your answer is to the string theorists who are inspired by this incredibly unlikely ability to pass many highly nontrivial constraints.

I suspect that one reason many string theorists are reluctant to listen to you is that they don't believe you've seen what they've seen, that you are fully aware of the crazy gauntlet string theory has (almost magically) sailed through, so they don't believe you really understand why they keep doing what they're doing. (Third question: Do you believe that this isn't the reason they do what they do?)

It's certainly true that the public, and most science writers who are not trained in theoretical physics, have no idea about all these constraints and what it means to say there's a theory that passes them.

With appreciation, Matt

21. **Adam Treat**
July 29, 2013

Matt, from what I understand it is precisely because String Theory has utterly failed to pass through those highly non-trivial constraints that motivate critics. Despite legions of effort and decades of study no one has demonstrated the Standard Model as a low energy limit of String Theory. And from what I understand, even had we the sufficient technology to probe those energy levels where gravity and quantum theory would both necessarily play a role that String Theory – even in principle – would provide no prediction. So I think your hypothetical does not illustrate the true picture.

22. **Peter Woit**
July 29, 2013

Matt,
This really is off-topic, since it has nothing to do with Unzicker, and I've written a great deal about these questions on the blog and in the book.

I'm well aware of the argument you are making, that the existence of a consistent quantum theory of strings is a very non-trivial fact, and worth investigating. Yes, this is a good argument that there's something there, and people who want to should investigate this. It's also true that everything we have learned (after literally tens of thousands of person-years of work) about this theory, whatever it is, says that it explains nothing at all (nothing, not a single thing, do you understand the significance of that?) about particle physics. The issues surrounding quantum gravity are complicated and murky and have been argued endlessly here. But the bottom line is that no matter what glowing terms you use to describe the situation, we're talking about an idea that has failed completely to accomplish what it was advertised (in equally glowing terms) to do. Yes, there's an interesting structure there, but no, it's not one useful to construct a unified theory, as far as anyone can tell.

Sorry, but I don't want more of this same sort of argument here, it's very tired and off-topic.

23. **eggcrook**
July 30, 2013

Peter,
That may be want you want, but I don't think you're gonna get it. Not Even Wrong may have been an argument to judge string theory based on the claims and predictions for the theory as made by the theory's inventors; and a warning of how and why that record of failures was misrepresented. But sadly most people just see a reduction of the complex problem of "How to pursue Mathematical Extensions to the Standard Model", to simple problems of freshman philosophy: "What is SCIENCE? What is EXPERIMENT?"

And they're gonna invoke your name, and your books name, to grant themselves permission to ignore complicated results, because they can expound on their own understanding of simple and accessible concepts like "experiment".

So I think you're gonna be stuck with cranks like Unzicker claiming to be on team Woit, and even educated opponents like Sean Carrol arguing against you by attacking "Popperian extremism," instead of arguing against your deconstruction of string theory promotion. Cause string theory is a stupid theory by stupid people too stupid to realize science means using experiments! Duh! Peter Woit even wrote a whole book proving it!

Regards.

24. **John Urbanik**
July 30, 2013

Unzicker's book was written for some of the masses, just like other books by Greene and Hawking. So to completely dismiss how the "uneducated" masses take it is wrong. Peter has the right idea in confronting it head on.

Being in the field it is easy to dismiss what he is saying and call him a 'crank'. But if his ideas are planted in the minds of others, especially politicians, you can start to kiss your funding goodbye. The population today is very skeptical about science. Without taking sides look at climate science. Despite huge media backing there are very strong opponents and unless something really proves to the masses that AGW is real it will cause repercussion not only on climate science but all types.

Unfortunately it seems physics today is at a crossroads. You need a Copernicus and a new telescope to advance it. But who knows how long that will take. In the days of smaller budgets and everyday folks questioning why any money should be spent on things like the LHC it falls to those in the field to explain it and defend their field.

25. **Hansl**
August 1, 2013

Maybe the masses should be tought more physics instead of being flooded with wrong analogies (even if this cuts Peter's wealth, sorry) and funding should only be assigned by committees of extremist Popperians in order to avoid that cranks can endanger science. Hmm.
I read Unzicker's book in German and was mostly annoyed by his style of permanently attacking people personally. If the book wasn't just a rant against the physics establishment, I would find on the positive side the list of problems which haven't been settled yet (with the exception of the Higgs particle discovery and the Pioneer anomaly) and not all of which I was aware. But that's not worth to buy the book in times of search engines.
BTW the German title "Vom Urknall zum Durchknall" means "From the Big Bang to madness" and is for the phonetic similarity of "Urknall" and "Durchknall" only, which is more than just a rhyme, a marketing coup.

26. **Hansl**
August 1, 2013

I forgot to mention that I am also puzzled that Springer, one of the most reputable science publishers published the book, which doesn't shed a good light on its current lectorate. Science and money again.

27. **Martin**
August 1, 2013

Unzicker was scheduled to give a colloquium at my university a couple of weeks ago, but for some reason he himself cancelled the talk a week before the scheduled date. (have not idea how he got invited in the first place.) Looking at his website (full of typos) made me curious and suspicious at the same time.

28. **mike**
August 4, 2013

Well, when physics is stuck in a rut like it is right now, the cranks are going to get a lot more attention. Remember, even LQG was considered crankish a few years ago

29. **Stephen Cliffe**
August 7, 2013

Chris
"for those who don't know: he's a frustrated high school teacher (frustrated because his scientific career failed) and now grinds his own axe in revenge."
As a high school teacher whose scientific career took off when I re-entered the classroom, I am jut wondering which axe I should grind.
I enjoyed this book greatly, but was suprised by how personally some people are attacked in the bookand how personally some react to thr book in blogs.
I try to give teenagers meaningful answers to questions about the big-bang, dark matter and genetically altered humans. As a biochemist, the last question is the easiest for me. To help me with the first two, my summer reading over the last few years has included many physics texts, of which the critical ones by Smollin and Laughlin stand out. Unzicker has much less standing and is much more personal in his attacks, but the message for me is the same. Teenagers are right to be sceptical about modern physics -it is no longer a case of just being too difficult but rather the students (and teachers and the public and physicists) have difficulty separating humbug from real science.
Ignore Unzicker at your peril. Read "The Golem- What you should know about Science" for a less emotional discussion.

30. **emile**
August 7, 2013

Stephen:

According to Peter, Unzicker wrote the following: "the standard model barely predicts anything", "the standard model can actually accommodate every result".

This is completely wrong. This means that this individual does not understand the Standard Model at an introductory level, yet writes about it anyway. If he is completely wrong on this issue (we are talking about facts here, not opinion), he's got not credibility left on any other subject as far as I'm concerned.

31. **AndreasK**
August 16, 2013

Possibly one should add here one remark on the political and sociological context that allows books as Unzicker's to enter the laureate lists of Germany's prestigious popular science magazine 'Bild der Wissenschaft'. As some reviews on the german amazon indicate, the book nicely fits into a 'movement', in especially in Germany, which seems to aim at a 're-naturalization' of science in general. That is, one claims to detect a tendency to 'overabstraction', 'lack of intuitiveness', 'dominance of superficial mathematical concepts' and connects this openly or more or less subtly to alleged traits of discourses or methods which are considered as 'non-european', 'pragmatic' or 'american', only avoding to openly claim 'non-german'. The inherent contradictions in those constructions, namely, just one example, to critisize 'pragmatism' and 'over abstraction and non-intuitiveness' at the same time, are quite typical for such lines of argumentation and although I know that Peter would prefer not to read such accusations in his blog here, remind me of the way 'jewish science' was characterized by the antisemtic german discourse in the 1930s and 40s, with known results. That Unzicker claims to be a 'fan of Einstein' is just one of these painful details in this row of all-too-obvious irrational discourses of people who see their culture or their 'peer group' or just themselves 'excluded' from discourses they therefore have to condemn as 'the other'. It wouldn't come as a big surprise to me to learn that Unzicker was, if he was involved at all, into branches of science, that, if 'sufficiently intuitive', lack a certain rigor and entail a certain degree of speculative reasoning by their very foundation (cf. neuroscience).

I have to emphasize, that this book comes in a long row of best-selling books in Germany in which science, or better 'pseudoscientific reasoning' play a very pivotal role in establishing certain political agendas, I only remind on the ramifications of 'genetics' and racism that was established in the realms of the 'Sarrazin-debate' in Germany.

In general, I would critisize one special trait that Unzicker unfortunately has in common with the criticism of Peter, although I am far from equating both. From my perspective this amounts to a certain tendency which can possibly be described as 'overestimating sociology' in science and drawing borders not by means of mathematical or physical content, but by the *perceived* presence of certain sociological boundaries. This is to a good part encoded in modern science itself and reminds me of André Weil criticising the group around Hasse for having lost sight of the 'Riemannian' point of view in their work on p-adic numbers and class field theory. But on a 'meta level', as Peter and Unsicker establish, these reductions to 'schools' and ' sociology' are even more harming since they suggest that for instance there wouldn't be *deep* common principles in theories being described as disparate and even 'rivaling' to the public, which might be deduced even to a certain degree from the personality of the scientists themselves, but bare a deeper truth on the scientific level. For instance once can be surprised that M-theory and the theory of 'spin networks' are regarded as rivaling, while certain 'coincidences' as the 'modularity' of certain partition functions in both theories undoubtedly show there are deep mathematical and physical links between such 'rivaling' approaches. Can someone explain to me here why theta functions appear in Gromov Witten theory as they appear in spin network theories and in DT-invariants? Mathematics and physics are to a high degree 'existant' and independent of the sociology of certain groups and for anyone who is more interested in deep mathematics than in post-adolescent rivalries the boundaries produced in nowadays discourses on physics and mathematics by predominantly sociological means must be understood as highly arbitrary and superficial and tend to deepen the 'crisis', if existent at all, in physics or mathematics more than they help to surmount it.

32. **Fred Bortz**
    August 16, 2013

    I am in complete agreement, Peter.

    Before the month is out, I expect to see my review of *Farewell to Reality* appearing in a major US metropolitan newspaper, after which I will archive it at my Science Shelf web site.

    My editor originally offered me the chance to do a slightly longer comparative review with *Bankrupting Physics*, but it didn't take me long to see that Unzicker's broadside pales beside Baggott's insightful critique. My editor agreed that it made sense to spend our limited word budget on Baggott's book alone.

    I admire your dedication in trying to describe what Unzicker set out to do, but I was grateful to set the book aside before I threw it across the room. 🙂

33. **Fred Bortz**
    August 16, 2013

    Just heard from my editor that the review is delayed a bit by internal priorities, but it will run.

    Stay tuned

34. **J Woods Halley**
    August 18, 2013

    I just read the Unziker book (as quite a few commentators apparently have not). Though I am not knowledgeable about string theory, beyond what could be gleaned from some popular accounts and physics colloquia, it was easy to spot a lot of elementary physics errors in Unziker's book and a lot of resentful personal venom. And I share some commentators' concern that the hostility takes an uncomfortably nationalist tone, somewhat reminiscent of what I have read of what happened in German physics in the 1930's. Nevertheless, I think that Unziker is pointing out some real problems in contemporary physics. Though he clearly doesn't understand gauge theories and their role in the standard model, and though he understates its achievements, practically everyone is uncomfortable with the large number of unexplained parameters which it requires. And it is true that the efforts to do something about this, mostly but not entirely with string theory, seem to have failed despite extensive effort. Perhaps some of the learned participants on this page will disagree, but I am also of the view that renormalization is a way around, but not really a resolution, of the problem of ultraviolet divergences in quantum field theory. In fact, people explained to me in the 80's that a primary virtue of string theory was that it has no such divergences. (Smolin says in one of his recent books that it is not fully established that that is true.) So though Unziker's book is often overstated and incorrect, I think it is pointing to some real problems, not all of which have been emphasized in the recent spate of books criticizing string theory.

Latest from the Stacks Project

July 30, 2013
Categories: Uncategorized

My colleague Johan de Jong for the last few years has been working on an amazing mathematical endeavor he calls the "Stacks Project". As boring 20th century technology, this is a work-in-progress document (now nearly 4000 pages), available here. But from the beginning Johan (known to his friends as "the Linus Torvalds of algebraic geometry") has conceptualized this as an open-source project using 21st century technology, including a blog and a github repository.

As of last night, the Stacks Project has many new features, courtesy of impressive work by Johan's collaborator on this, Pieter Belmans. I was going to write something here describing the new features and how cool they are, but a much better job of this has been done by Cathy O'Neil, aka Mathbabe (by the way, if you're not reading Cathy's blog, you should be…). With her permission, I'm cross-posting her new blog entry about this, so, what follows is from Cathy:

The Stacks Project gets ever awesomer with new viz

Here's a completely biased interview I did with my husband A. Johan de Jong, who has been working with Pieter Belmans on a very cool online math project using d3js. I even made up some of his answers (with his approval).

Q: What is the Stacks Project?

A: It's an open source textbook and reference for my field, which is algebraic geometry. It builds foundations starting from elementary college algebra and going up to algebraic stacks. It's a self-contained exposition of all the material there, which makes it different from a research textbook or the experience you'd have reading a bunch of papers.

We were quite neurotic setting it up – everything has a proof, other results are referenced explicitly, and it's strictly linear, which is to say there's a strict ordering of the text so that all references are always to earlier results.

Of course the field itself has different directions, some of which are represented in the stacks project, but we had to choose a way of presenting it which allowed for this idea of linearity (of course, any mathematician thinks we can do that for all of mathematics).

Q: How has the Stacks Project website changed?

A: It started out as just a place you could download the pdf and tex files, but then Pieter Belmans came on board and he added features such as full text search, tag look-up, and a commenting system. In this latest version, we've added a whole bunch of features, but the most interesting one is the dynamic generation of dependency graphs.

We've had some crude visualizations for a while, and we made t-shirts from those pictures. I even had this deal where, if people found mathematical mistakes in the Stacks Project, they'd get a free t-shirt, and I'm happy to report that I just last week gave away my last t-shirt. Here's an old picture of me with my adorable son (who's now huge).

Q: Talk a little bit about the new viz.

A: First a word about the tags, which we need to understand the viz.

Every mathematical result in the Stacks Project has a "tag", which is a four letter code, and which is a permanent reference for that result, even as other results are added before or after that one (by the way, Cathy O'Neil figured this system out).

The graphs show the logical dependencies between these tags, represented by arrows between nodes. You can see this structure in the above picture already.

So for example, if tag ABCD refers to Zariski's Main Theorem, and tag ADFG refers to Nakayama's Lemma, then since Zariski depends on Nakayama, there's a logical dependency, which means the node labeled ABCD points to the node labeled ADFG in the entire graph.

Of course, we don't really look at the entire graph, we look at the subgraph of results which a given result depends on. And we don't draw all the arrows either, we only draw the arrows corresponding to direct references in the proofs. Which is to say, in the subgraph for Zariski, there will be a path from node ABCD to node ADFG, but not necessarily a direct link.

Q: Can we see an example?

Let's move to an example for result 01WC, which refers to the proof that "a locally projective morphism is proper".

First, there are two kinds of heat maps. Here's one that defines distance as the maximum (directed) distance from the root node. In other words, how far down in the proof is this result needed? In this case the main result 01WC is bright red with a black dotted border, and any result that 01WC depends on is represented as a node. The edges are directed, although the arrows aren't drawn, but you can figure out the direction by how the color changes. The dark blue colors are the leaf nodes that are farthest away from the root.

Another way of saying this is that the redder results are the results that are closer to it in meaning and sophistication level.

Note if we had defined the distance as the minimum distance from the root node (to come soon hopefully), then we'd have a slightly different and also meaningful way of thinking about "redness" as "relevance" to the root node.

This is a screenshot but feel free to play with it directly here. For all of the graphs, hovering over a result will cause the statement of the result to appear, which is awesome.

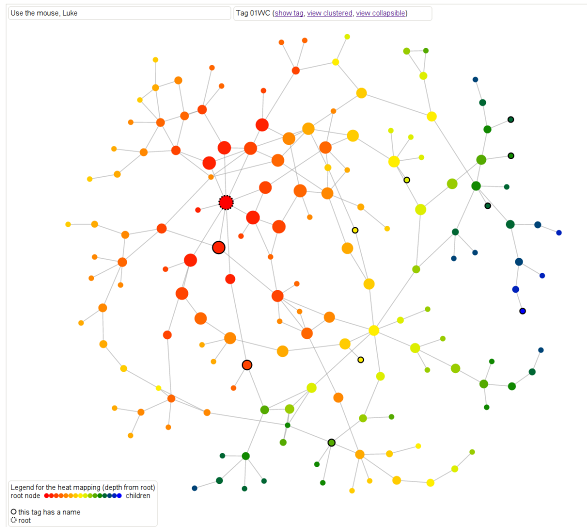

Next, let's look at another kind of heat map where the color is defined as maximum distance from some leaf note in the overall graph. So dark blue nodes are basic results in algebra, sheaves, sites, cohomology, simplicial methods, and other chapters. The link is the same, you can just toggle between the different metric.

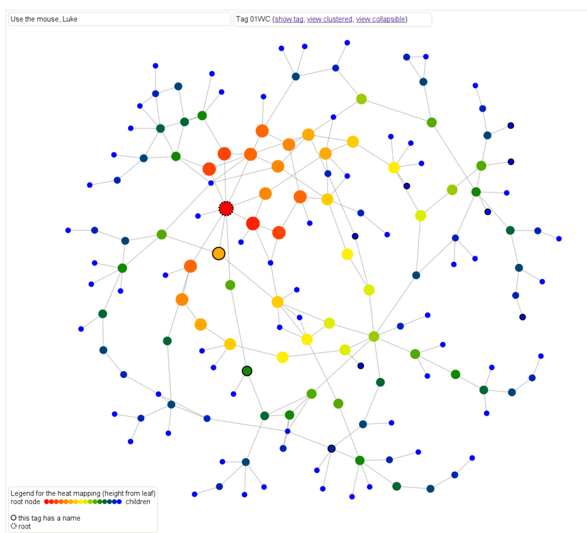

Next we delved further into how results depend on those different topics. Here, again for the same result, we can see the extent to which that result depends on the different on results from the various chapters. If you scroll over the nodes you can see more details. This is just a screenshot but you can play with it yourself here and you can collapse it in various ways corresponding to the internal hierarchy of the project.

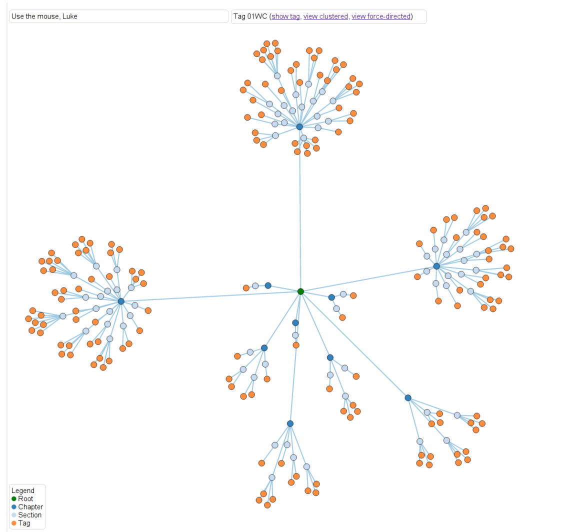

Finally, we have a way of looking at the logical dependency graph directly, where result node is labeled with a tag and colored by "type": whether it's a lemma, proposition, theorem, or something else, and it also annotates the results which have separate names. Again a screenshot but play with it [here](), it rotates!

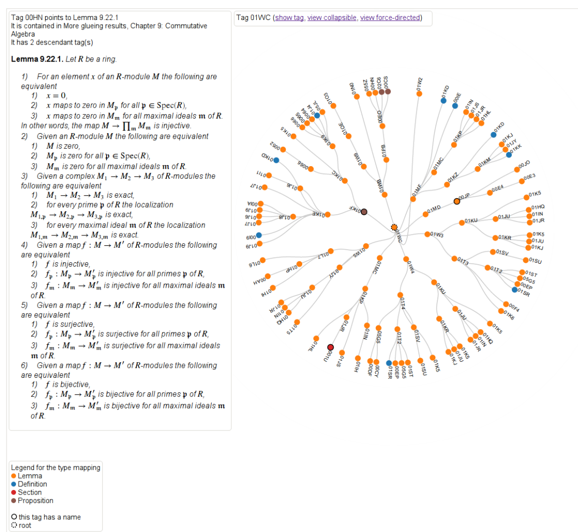

Check out the whole project [here](), and feel free to leave comments using the comment feature!

Comments

1. **johnmcAllison**
   July 30, 2013

   Great idea, using the proven Open Source methodology to help develop the book. But in this day and age, should we still be restricting ourselves to text?

   I'm a little disappointed in that they haven't gone further with using audio/video lectures to add to the learning experience.

2. **Ben**
   July 30, 2013

   That's a really impressive project, but why aren't they using set theoretical universes? It seems to me that that complicates things a lot, and makes the development less modular.

3. **Peter Woit**
   July 30, 2013

   John McAllison,

   It's probably better to think of this as an attempt to provide a reference work and reliable tool for researchers to work with, rather than an expository work for people to learn the subject from. In this respect it's somewhat like the famous Bourbaki books. Those can be very useful once you know the subject and need a precise result, much less useful if you are just starting and trying to learn what the subject is about. It's not so clear that audio or video are helpful for this kind of thing.

4. **anonymous**
   August 2, 2013

In my, very humble, opinion, a book is not a 21-st century book if it does not explain the motivation behind definitions and tools used, unless by 21-st century one means to write it all down in some formal language and then let finally the computer find out whether there was a gap in the reasoning hidden somewhere. I do not believe that AG definitions and tools cannot be given such motivation. To try to write this motivation down will be probably of bigger value, at least for the "wider public", than the current effort. The best would be also to say clearly for which problems the hypothetical author thinks the machinery is useful, and for which problems he thinks it is not, with interesting examples from both geometry and "arithmetics".

5. **anon**
   August 2, 2013

   @Ben. They explain why they don't use universes. Briefly, they don't need them, and don't want to make an unnecessary assumption (that universes exist).
   @anonymous. Essentially it's a reference work. There are many books on algebraic geometry that attempt to do what you request.

6. **A.J.**
   August 3, 2013

   @anonymous

   To add to anon's comment: Think of it as a technical manual. You don't get your driver's manual out of the glovebox to find out what a car is. Instead, you go to it when you are having a problem with the particular car you are in possession of.

   One problem with this metaphor I'm using: de Jong's book is rather nicely written and seems like an exercise in good taste…

7. **Sebastian Thaler**
   August 5, 2013

   Peter,

   Completely off-topic, but I just wanted to say I'm glad to see you quoted as the voice of healthy skepticism in the article on supersymmetry in the Summer issue of Columbia Magazine: http://www.magazine.columbia.edu/features/summer-2013/heady-collisions

8. **Urs Schreiber**
   August 6, 2013

   @AJ,

   I guess the anonomyous above is a layman in algebraic geometry and secretly asking for help, as in: "Please tell, me, what's the intuition behind algebraic stacks, what are they good for and why should I care" and maybe in addition: "Why should I care, being a person interested and trained (only) in physics?".

   Luckily all such questions are answered in the nLab :-).

9. **Peter Woit**
   August 6, 2013

   If someone wants a 21st century algebraic geometry project of an expository sort, beside Urs' nLab, the mathematically oriented should look at Ravi Vakil's web-site here

   http://math216.wordpress.com/

   Sebastian
   Well, I guess someone was needed for that role….

The Next Machine

August 6, 2013
Categories: Experimental HEP News

For the last week or so US HEP physicists have been meeting in Minneapolis to discuss plans for the future of US HEP. Some of the discussions can be seen by looking through the various slides available here. A few days earlier Fermilab hosted TLEP13, a workshop to discuss plans for a new very large electron-positron machine. There is a plan in place (the HL-LHC) for upgrading the LHC to higher luminosity, with operations planned until about 2030. Other than this though, there are no current definite plans for what the next machine at the energy frontier might be. Some of the considerations in play are as follows:

- The US is pretty much out of the running, with budgets for this kind of research much more likely to get cut than to get the kinds of increases a new energy frontier machine would require. Projects with costs up to around $1 billion could conceivably be financed in coming years, but for the energy frontier, one is likely talking about $10 billion and up.
- Pre-LHC, attention was focused on prospects for electron-positron linear colliders, specifically the ILC and CLIC projects. The general assumption was that LEP, which reached 209 GeV in 2000, was the last circular electron-positron collider. The problem is that, at fixed radius, synchrotron radiation losses grow as the fourth-power of the energy, and LEP was already drawing a sizable fraction of the total power available at Geneva. Linear accelerators don't have this problem, but they do have problems achieving high luminosity since one is not repeatedly colliding the same stored bunches.

  The hope was that the LHC would discover not just the Higgs, but all sorts of new particles. Once the mass of such new particles was known, ILC or CLIC technology would give a design of an appropriate machine to study such new particles in ways that not possible at a proton-proton machine. These hopes have not worked out so far, making it now appear quite unlikely that there are such new particles at ILC/CLIC accessible energies. It remains possible that the Japanese will decide to fund an ILC project, even without the appealing target of a new particle besides the Higgs to study.

- The LHC has told us the Higgs mass, making it now possible to consider what sort of electron-positron collider would be optimal for studying the physics of the Higgs, something one might call a "Higgs factory". It turns out that a center of mass energy of about 240 GeV is optimal for Higgs production. This is easily achievable with the ILC, but since it is not that much higher than LEP, there is now interest in the possibility of a circular collider as a Higgs factory. There is a proposal called LEP3 (discussed on this blog here) for putting such a collider in the LHC tunnel, but it is unclear whether such a machine could coexist with the LHC, and no one wants to shutdown the LHC before a 2030 timescale.
- Protons are much heavier than electrons, so synchrotron radiation losses are not the problem, but the strength of the dipole magnets needed to keep them in a circular orbit is. To get to higher proton-proton collision energies in the same tunnel, one needs higher strength magnets, with energy scaling linearly with field strength. The LHC magnets are about 8 Tesla, current technology limit is about 11 Tesla for appropriate magnets. The possibility of an HE-LHC, operating at 33 TeV with 20 Tesla magnets is under study, but this technology is still quite a ways off. Again, the time-scale for such a machine would be post-2030.
- The other way to get to higher proton-proton energies is to build a larger ring, with energy scaling linearly with the size of the ring (for fixed magnet strength). Long-term thinking at CERN now seems to be focusing on the construction of a much larger ring, of size 80-100 km. One could reach 100 TeV energies with either 20 Tesla magnets and an 80 km ring, or 16 Tesla magnets and a 100 km ring (such a machine is being called a VHE-LHC). If such a tunnel were to be built, one could imagine first populating it with an electron-positron collider, and this proposal is being called TLEP. It would operate at energies up to 350 GeV and would be an ideal machine for precision studies of the Higgs. It could also be used to operate at very high luminosity at lower energies, significantly improving on electroweak measurements made at LEP (the claim is that LEP-size data sets could be reproduced in each 15 minutes of running). Optimistic time-lines would have TLEP operating around 2030, replaced by the VHE-LHC in the 2040s.
- For more about TLEP, see the talks here. The final talk of the TLEP workshop wasn't about TLEP, but Arkani-Hamed on the VHE-LHC (it sounds like maybe he's not very interested in the Higgs factory idea). He ends with

  > EVERY student/post-doc/person with a pulse (esp. under 35) I know is ridiculously excited by even a glimmer of hope for a 100 TeV pp collider. These people don't suffer from SSC PTSD.

  Looking at the possibilites, I do think TLEP/VHE-LHC looks like the currently most promising route for the future for CERN and HEP physics (new technology might change this, i.e. a muon collider). Maybe I don't have a pulse though, since I can't say that I'm ridiculously excited by just a glimmer of VHE-LHC hope for a time-frame past my life-expectancy.

  A 100 km tunnel would be even larger than the planned SSC tunnel (89 km) and one doesn't have to suffer from SSC post-traumatic-stress-disorder to worry about whether a project this large can be successfully funded and built (In very rough numbers I'd guess one is talking about costs on the scale of $20 billion). My knowledge of EU science funding issues is insufficient to have any idea if the money for something on this scale is a possibility. On the other hand, with increasing concentration of all wealth in the hands of an increasingly large number of multi-billionaires, perhaps this just needs the right rich guy for it to happen.

  Someone is going to have to do a better job than Arkani-Hamed in terms of finding an argument that will sell this to rest of the scientific community. His main argument is that such a machine would allow us to improve the ultimate LHC number of "fine-tuning" being at least 10^{-2} to a number like 10^{-4}, or maybe finally see some SUSY particles. I don't think this argument is going to get $20 billion: "we thought we'd see all this stuff at the LHC because we were guessing some number we don't understand was around one. We saw nothing and turns out the number is small, no bigger than one in a hundred. Now we'd like to spend $20 billion to see if it's smaller than one in a hundred, but bigger than one in ten thousand."

Comments

1. **uair01**
   August 6, 2013

   As a normal citizen I'm a bit dismayed by all these plans and amounts. Why spend it on esoteric and far-out theory research? Why not spend it on something that we need more, like nuclear fusion research? I would heartily support that. But this feels more like the weird luxury hobby of a very small elite. We are all in the aftershocks of a financial crisis, people are losing their jobs and you guys are playing with Higgs bosons. What good will that do for the rest of us?

2. **Peter Woit**
   August 6, 2013

   I see that even before I'd finished re-reading this to proof read it, the usual first comment that comes in whenever I write anything about a possible HEP experiment has appeared.

   Enough, all further comments from people who want to argue not about the physics but about the desirability of spending money at all for this purpose will be ruthlessly deleted. This argument has gone on here many times, I don't see any possibility of anything new being said.

3. **Low Math, Meekly Interacting**

August 6, 2013

About muon colliders…I've read a bit about the latest research into hypothetical machine design, but can glean no sense of where the bets are on feasibility. Seems like there's been serious discussion since 2009, so I'm wondering if the HEP community is coming to some consensus on whether or not it's a remotely viable idea.

4. **Peter Woit**
August 6, 2013

LMMI,

There's a long history of work on the muon collider idea, see for instance
http://www.fnal.gov/projects/muon_collider/history.html
As far as I can tell, the current situation is still one of research into determining whether such a thing is truly feasible, with an answer to that question still years in the future.

5. **srp**
August 6, 2013

I wouldn't appropriate money for any of these proposals until some of the ideas here

http://proceedings.aip.org/resource/2/apcpcs/1507/1?isAuthorized=no

had been thoroughly investigated. A billion dollars and five years to figure out whether there isn't a better way (that also may give more-useful spinoffs) would be a very sensible investment. It might even turn out to shorten the time needed to achieve much higher energies.

If it turns out that these concepts are all 20+ years off or are impossible, then OK, it's reasonable to discuss accelerators that would swallow up small countries. Without such a conclusion, these proposals (except maybe the muon collider) all have the air of calling for "more cowbell."

6. **lun**
August 6, 2013

By far the cheapest and fastest option to explore an extra big chunk of as yet unknown parameter space is to put the LEP back in the LHC tunnel, and study ep collisions at much higher space in x-Q^2 than ever before. One can do this with equipment already built.

I cannot help but think that the only reason this is not even mentioned as a main alternative is theoretical prejudice: Theorists are indicating the way, most theorists dont like technicolor and/or substructure theories, therefore such theories (for which an eLHC would be optimized) are not being explored. Note that more often than not the argument against these approaches w.r.t. SUSY is that they are intractable to analytical calculation, something very different from saying they are unlikely to happen.

There is a general point to be made here – experiments and accelerators cost a lot of money, so the pressure is there to go on a road where theorists say youre likely to make a discovery, rather than blindly explore and hope to find something new.
Yet it is the latter road that has often given the big discoveries.
And, more to the point, in this case going off the beaten track (eLHC) would be much cheaper than the standard approach (ILC/VLHCs), and the most quoted theory predictions (SUSY) are being roundly falsified. One needs to invent an experimental strategy that is less risk-averse, which for such expensive toys as those the physicists use is not simple.

7. **M**
August 7, 2013

Theorists too suffer PTSD because LHC did not confirm their theories of the past 30 years. It is great that nature can give such surprises.

A bigger circular collider for a precise study of the Higgs and later an exploration up to 100 TeV would surely the best option. Setting rhetoric aside, exploring is the only way in which we can understand what is the weak scale.

8. **fuzzy**
August 7, 2013

before planning the future, i feel that we should make an effort to remind the discussions before the lhc was built, which arguments have been collected, who had the right to speak and who did not. it is not enough to note the failure of the plans, one should analyze the causes, in order not to repeat it.

personally, i have a strong impression that the lust for machines is larger than the will of discussing what we are searching for. and even when we discuss of physics, the greatest effort is to win the argument or to maintain the position rather than getting to the point. this should change.

finally, i would suggest to invest into gamma and neutrino astronomy, measurements concerning neutrinos, studies about the gravity, understanding astrophysical objects, search for dark matter and alike. these researches gave a lot to physics in the past years and will continue to do so for a long time.

9. **Zathras**
August 7, 2013

This is off-topic for this post, but this article on how science journalists should be skeptical of scientific claims is really fascinating, as well as being quite germane to this blog.

10. **Z**
August 7, 2013

It looks like a muon collider is far off. Results from MICE (e.g. http://arxiv.org/abs/1307.3891) aren't expected until the 2020s, which means it probably won't be ready for a collider planned for the early 2030s. If it does happen though, you could have a 50-70 TeV lepton machine on a 100km ring before you run into synchrotron losses.

11. **Alex**
August 7, 2013

fuzzy's last point is an interesting one. What is the evidence that there must be a theory of fundamental interactions beyond the Standard Model? Well, among other things, there's the existence of dark matter (which we know from astronomical observations), the existence of dark energy (again, known from astronomy), the existence of gravity (something we can study in terrestrial experiments but we understand much better because of astronomical observations), and the existence of neutrino oscillations (something we can study in terrestrial experiments but we first learned about from solar astronomy). For that matter, how did we learn that antiparticles exist? Somebody found positrons in cosmic rays. How did we learn that there's more than one generation of leptons? Somebody found muons in cosmic rays.

I wonder if it might make more sense to invest in understanding neutrinos, looking for dark matter candidates in labs, doing astronomical observations that can constrain particle physics beyond the Standard Model, looking for unexpected things in cosmic rays, and looking for anomalies in dipole moments and whatnot. Those sorts of observations have the potential to either rule out some models or make some models more promising. With that sort of information in hand, one could push for an accelerator with a clearer idea of what it needs to look for.

12. **Peter Woit**
August 7, 2013

Alex,

The things you suggest aren't being ignored, they're more or less exactly the topics that US HEP is now concentrating on, given that an energy frontier machine is not plausible. Even the one main accelerator project (Project X) under discussion is motivated largely by neutrino physics.

13. **Alex**
August 7, 2013

Yeah, I didn't mean to say they're being ignored. I know they aren't. I'm mostly wondering if, given our current lack of knowledge of where to go, these are actually more valuable than a large accelerator.

14. **Sammy**
August 7, 2013

The HEP community might help the case for massive further HEP funding by demonstrating that a by-product could be improved design and analysis of PET scanning, SPECT scanning and Proton Beam Therapy or even the development of Higgs Knives (my second cousin's uncle-in-law's deep brain tumor just got zapped clean away with a Gamma Knife). Then there are smoke alarms and all kinds of industrial probes. Don't forget that the space program gave us nonstick frypans.

15. **Zathras**
August 7, 2013

Sammy,

Pointing to these invention is the conventional response to the cost of science, but I do not think it will be enough. To show this value, the LHC better come up with similar concrete innovations. Otherwise, you're just talking ancient history. There is still time for the LHC to do so, but it is not a given that it will happen.

16. **harryb**
August 7, 2013

I work in the oil industry – and the annual – annual – project capital expenditure globally is now over 1 Trillion dollars (google it). Typically, in any given year, there are 5-15 > $20bn projects underway. I am involved in two currently in this range – one in construction, one in late design. A $20bn project in the oil and gas world – lets call it mature applied physics – is unremarkable at any given moment now. Maybe Peter is right – get these billionaires to invest a proportion in HEP projects rather than largesse to vogue theorists (eg Milner Foundation) and access immortality that way. It would be a refreshing change in perspective to invest heavily in experiment.

17. **Sammy**
August 7, 2013

Zathras: what's ancient history is e.g. the theory (Bethe 1932-33 plus frills) presently used to shape pulses in Proton Beam Therapy. Why not bring it up to the level of modern HEP?

Dirac postulated the positron when my father was a medical student. Now PET scanning is routine clinical imaging. These things take time, and lay people do understand that.

18. **Yatima**
August 8, 2013

Considering the way "western" economies stay mired in inflationism, debt traps and welfare-warfare statism — if China manages to make a soft-ish landing for their overleveraged, bubbling, export-slanted and cheap-labor-from-the-countryside dependent economy (as well as not fall into the trap of internal or external armed conflict), they might be a contender for a projet of a 2030 or later timeframe. They are flush with foreign cash (or rather, US gov'nment IOUs) and looking for national prestige so they could start on design work immediately. Is anyone talking to them?

19. **Loren Petrich**
August 11, 2013

A muon collider seems like a nice idea: the cleanness of a lepton and a high mass. But the muon's mean life is about 2.2 microseconds. That means that there isn't much time to accelerate and collide a muon before it decays. I've seen some discussions of muon colliders, but I haven't found out how they expect to get around that problem.

20. **Peter Woit**
August 11, 2013

Loren Petrich,

See the comment by Z above. The paper linked to has some relevant discussion. The problem isn't so much acceleration but "cooling": the muons you are producing come with a wide spread of momenta, and you need narrow that considerably before you can accelerate and collide. The question is whether you can do this on time scales of less than a microsecond.

Never Give Up

August 7, 2013
Categories: Uncategorized

Alok Jha has a piece in the Guardian yesterday about the failure to find SUSY. His conclusion I think gets the current situation right:

> Or, as many physicists are now beginning to think, it could be that the venerable theory is wrong, and we do not, after all, live in a supersymmetric universe.

An interesting aspect of the article is that Jha asks some SUSY enthusiasts about when they will give up if no evidence for SUSY appears:

> Allanach says he will wait until the LHC has spent a year or so collecting data from its high-energy runs from 2015. And if no particles turn up during that time? "Then what you can say is there's unlikely to be a discovery of supersymmetry at Cern in the foreseeable future," he says.

Allanach has been at this for about 20 years, and here's what he has to say about the prospect of failure:

> If the worst happens, and supersymmetry does not show itself at the LHC, Allanach says it will be a wrench to have to go and work on something else. "I'll feel a sense of loss over the excitement of the discovery. I still feel that excitement and I can imagine it, six months into the running at 14TeV and then some bumps appearing in the data and getting very excited and getting stuck in. It's the loss of that that would affect me, emotionally."

John Ellis has been in the SUSY business even longer, for 30 years or so and he's not giving up:

> Ellis, though confident that he will be vindicated, is philosophical about the potential failure of a theory that he, and thousands of other physicists, have worked on for their entire careers.
>
> "It's better to have loved and lost than not to have loved at all," he says. "Obviously we theorists working on supersymmetry are playing for big stakes. We're talking about dark matter, the origins of mass scales in physics, unifying the fundamental forces. You have to be realistic: if you are playing for big stakes, very possibly you're not going to win."

But, just because you're not going to win, that doesn't mean you have to ever admit that you lost:

> John Ellis, a particle theorist at Cern and King's College London, has been working on supersymmetry for more than 30 years, and is optimistic that the collider will find the evidence he has been waiting for. But when would he give up? "After you've run the LHC for another 10 years or more and explored lots of parameter space and you still haven't found supersymmetry at that stage, I'll probably be retired. It's often said that it's not theories that die, it's theorists that die."

There may be a generational dividing line somewhere in the age distribution of theorists, with those above a certain age likely to make the calculation that, no matter how bad things get for SUSY and string theory unification, it's better to go to the grave without admitting defeat. The LHC will be in operation until 2030 or so, and you can always start arguing that 100 TeV will be needed to see SUSY (see here), ensuring that giving up won't ever be necessary except for those now still wet behind the ears.

For another journalist's take on the state of SUSY, this one Columbia-centric and featuring me as skeptic, see here.

Comments

1. **Giotis**
   August 7, 2013

   SYSY is a fundamental symmetry of Nature. There is no doubt about it. You must be a theoretical physics imbecilic not to acknowledge that.

   Now, how it breaks, at which energy scale breaks and whether it solves some problems of the SM is another issue.

2. **Peter Woit**
   August 7, 2013

   Giotis,
   I suppose another way to deal with losing is by not only refusing to admit that you've lost, but claiming that you've really won. Good luck with that…

3. **Giotis**
   August 7, 2013

   Ok, then I would really like to know your theoretical arguments against SUSY being a Symmetry of Nature. Let me guess, you don't have any. You just sit there crossing your fingers hoping LHC doesn't find anything. That's pathetic…

4. **Urs Schreiber**
   August 7, 2013

   The point hidden in Giotis message is a real issue: people are ever careless about language in what should be exact science, and it leads to endless confusion.

   All mentioning of "supersymmetry" in posts like this is really "low energy supersymmetry". Hence the term "supersymmetric universe" above is quite misleading. Supersymmetry may or may not be a fundamental symmetry of nature, indeedm and what is currently happening at the LHC is quite unrelated to that question. What the LHC sees or does not see is global supersymmetry unbroken at rather low energy. That to exists is — or would be — about as striking as global Lorentz symmetry in the universe, which would be bizarre, even though Lorentz symmetry is a fundamental symmetry of the universe.

   Speaking of universes, that's another example of how careless language damages the scientific discourse: if people had remembered to correctly say "observable universe" instead of just "universe" that awful term "multiverse" would have been neither necessary nor the cause of wasted much bandwidth that it currently is.

5. **Peter Woit**
   August 7, 2013

   Giotis,
   See Chapter 12 of Not Even Wrong, which was written more than ten years ago.

The Columbia Magazine story didn't really quote the arguments I explained to the writer of the article. The simplest is that SUSY explains nothing at all about the SM, since it gives no relations between different objects in the SM, instead relating each SM object to a hypothetical new one. A symmetry argument that explains nothing isn't a very good symmetry argument.

6. **Peter Woit**
   August 7, 2013

   Urs,

   The problem is more your use of language, since if you made it clear to people that when you said "supersymmetry", you meant something that can't ever be tested, they wouldn't take you very seriously. I think you're right that some string theory/SUSY enthusiasts post-LHC will argue that nothing has shown them to be wrong, and nothing in their lifetime ever possibly can, but to the extent that they make this situation clear they will have a serious credibility problem.

7. **fuzzy**
   August 7, 2013

   in my view the problem is worse than pointed out above. even giotis admits that he/she has no idea where this hypothetical symmetry is realized. in this way, he/she is speaking frankly, but i wonder whether this status of affairs was so clearly acknowledged even before lhc—or it was not.

   in particular, i saw that some experts of susy have been mentioned above; let me ask whether, in the past 30 years, they begun their speculative papers by saying "we do not have an idea where susy is broken but let us try 1 tev or so". if they did not (i think they did not) i should conclude that they have misled their field and themselves (which is worse); or, they have believed in some wrong argument. in the last case, i would appreciate very much if they did not rush to tell us what we should think.

8. **emile**
   August 7, 2013

   Giotis: SUSY is a fundamental symmetry of Nature? how do you know this fact about Nature? where is the experimental evidence?

9. **Giotis**
   August 7, 2013

   Emile,

   We live in the 21th century; physicists are in a position now to theoretical deducing properties of Nature. The discovery of Higgs is a great example. They don't have to be guided by experiments, like little kids, to put 2 and 2 together. I'm not in a mood to explain to you why SUSY *has* to be a symmetry of Nature, if you can't figure out why is that just open a text book (indeed SUSY is text book material) and stop wasting your time reading blogs…

10. **Urs Schreiber**
    August 7, 2013

    Indeed, the worldline theory of any spinning particle, such as the electron, is locally supersymmetric. So supersymmetry as such has been experimentally tested since Stern-Gerlach.

    What is under discussion re LHC is some very special aspect of supersymmetry namely "global low energy spacetime supersymmetry". This is the precise term. You may not want to say this each time, but it's good not to forget what you are talking about.

11. **Florin**
    August 7, 2013

    Hi Urs,

    Can you give some more details on "the worldline theory of any spinning particle, such as the electron, is locally supersymmetric" ? I don't follow that.

    As for finding superpartners at LHC.. I don't know but this was never presented as being a "special" aspect of supersymmetry, but the main feature of supersymmetry.

12. **Anonyrat**
    August 7, 2013

    Florin, see http://ncatlab.org/nlab/show/string+theory+FAQ#DoesSTPredictSupersymmetry 4th paragraph.

13. **Peter Woit**
    August 7, 2013

    Florin,

    Urs is just trying to muddy the waters by talking about 0+1 d SUSY when the discussion is actually about 3+1 d SUSY.

    In 0+1 d, you're just doing quantum mechanics, and in cases where your Hamiltonian has a square root, you can call this a supersymmetry. The Dirac operator is an example, so if you want you can say that it generates a supersymmetry, and that SUSY was discovered to work experimentally back in 1928. No need for the LHC! Of course, that's something completely different than what everyone else means when they are discussing supersymmetry (i.e. an extension of the 3+1 d Poincare algebra that mixes fermions and bosons)

    One place that has something about this is a chapter of my book in progress. See chapter 23 of
    http://www.math.columbia.edu/~woit/QM/qmbook.pdf

14. **emile**
    August 7, 2013

    Giotis, you wrote: "The discovery of Higgs is a great example. They don't have to be guided by experiments, like little kids, to put 2 and 2 together. ".

    The Higgs is a great example for not having to run experiments? You knew for sure, that there was a SM-like scalar before it was found? You think we did not need to run the LHC after all? Can you tell me if there are other scalars? an extended sector, 2 HDM? an ewk singlet?

15. **JG**

August 7, 2013

typo in your book link, it should be

http://www.math.columbia.edu/~woit/QM/qmbook.pdf

16. **Peter Woit**
August 7, 2013

Thanks JG, fixed.

17. **Brian**
August 7, 2013

Ellis: "Obviously we theorists working on supersymmetry are playing for big stakes. We're talking about dark matter, the origins of mass scales in physics, unifying the fundamental forces."

Funny kind of roulette when the barrel is totally empty and you're guaranteed to win. Funnily enough, I heard recently that Ellis was not very interested in the new twistor scattering techniques because it did not fit the priorities of the department … perhaps those senior theorists really should be having a word with department heads.

18. **Seth Goldberg**
August 7, 2013

Somewhat Off topic: Speaking of other physical contexts of supersymmetry, there is also a condensed matter/disorder model. Has anyone here read Supersymmetry in Disorder and Chaos by Konstantin Efetov Cambridge University Press (September 13, 1999)?
See e.g. http://www.amazon.com/Supersymmetry-Disorder-Chaos-Konstantin-Efetov/dp/0521663822/ref=sr_1_1?ie=UTF8&qid=1375921527&sr=8-1&keywords=supersymmetric+solid+state
Anyone willing to comment as to the appropriateness of the mathematical physical analogy (in Maxwell's sense)?

19. **Peter Woit**
August 7, 2013

Seth,
This has nothing at all to do with the topic of this posting, and it's also a topic I know very little about. Fermionic variables have all sorts of interesting uses in stat-mech, and I gather this book is about some of them, but this is a very long ways away from 3+1 d space-time supersymmetry and its uses in particle physics.

20. **Chris W.**
August 7, 2013

> We live in the 21th century; physicists are in a position now to theoretical deducing properties of Nature. The discovery of Higgs is a great example. They don't have to be guided by experiments, like little kids, to put 2 and 2 together.

Giotis, that's just laughable. An undergraduate that made such a statement would deserve to be told they don't belong in physics or any other scientific field in which empirical observations must be accounted for. In such fields, properties of nature are deduced using theories or hypotheses, *and those theories can be wrong*. That is why we test them. If you want a theory that can't be wrong, stick to mathematics or metaphysics and stay the hell out of physics. Period.

21. **Mitchell Porter**
August 7, 2013

Giotis said

"SUSY is a fundamental symmetry of Nature. There is no doubt about it. You must be a theoretical physics imbecilic not to acknowledge that… Ok, then I would really like to know your theoretical arguments against SUSY being a Symmetry of Nature… I'm not in a mood to explain to you why SUSY *has* to be a symmetry of Nature, if you can't figure out why is that just open a text book"

There is no textbook on Earth which explains why supersymmetry "has to be a symmetry of nature". There isn't even a proof that string theory has to be supersymmetric.

22. **Thomas Larsson**
August 8, 2013

Both Michelson and Morley kept looking for the ether wind well into the 1920s. They never, never, never gave up.

23. **Alex**
August 8, 2013

Were Michelson and Morley convinced that there must be an ether wind for them to find, or were they doing their measurements in the spirit of the modern-day physicists who keep doing precision measurements to push the limits on Lorentz violations out another decimal place? I'm pretty sure that these folks will never find anything, but I'm happy to see them trying anyway, and getting published in PRL for their efforts. It's important to the culture of physics that we always have people testing assumptions, and that I can show my students that the people who test assumptions are recognized in prestigious publications for pushing the boundaries out another decimal place.

24. **Bee**
August 8, 2013

Sociologists call it an "escalation of commitment". If I were a sociologist, my next paper would be on "the susy community in the LHC era". Only half joking, it's quite interesting to watch.

25. **N. Nakanishi**
August 8, 2013

Giotis,

Three decades ago, I proved that SUSY cannot be the fundamental symmetry of physics. The reasoning is as follows.
Evidently, SUSY, if it exists, must be spontaneously broken, but there is no Nambu-Goldstone fermion. Hence, one must assume that super-Higgs mechanism works. That

is, supergravity must be encountered. However, quantum supergravity cannot be consistent with SUSY. This is because the global super-charge generators have no space-time index, while the translation generator P_mu does have a space-time index in contradiction with the SUSY anticommutation relation.
The essential problem of Poincare invariance in the framework of gravity is that the general coordinate transformation, which is nothing but the local version of the translation, already contains the Lorentz transformation. The local Lorentz transformation is an internal symmetry; indeed, Dirac field is a scalar field in the framework of gravity. Only after spontaneous breakdown takes place, Dirac field becomes a world spinor. Thus, Poincare invariance is not a fundamental but secondary symmetry. Since SUSY is an extension of Poincare symmetry, it cannot be a fundamental symmetry!

26. **Yatima**
August 8, 2013

The very first post.

1) Fact-free dogmatic statement.
2) Reinforce the fact-free dogmatic statement.
3) Ad hominem attack while claiming the high ground, throw in an argumentum ad populum.
4) Follwup by "I don't know what the hell is going on".

Sure is a formula for success 🙁

27. **LogicFan**
August 8, 2013

I am very pleased that when I was an undergraduate considering the PhD I decided to go into philosophy rather than physics. The intellectual standards of your discipline are abysmal.

You have failed to make any significant theoretical advance in, say, over 30 years. That's pretty bad. Perhaps you will retort that philosophy has failed to make any significant theoretical advance in over 2,000 years, but I do not believe that that is true, and in any case we have maintained our standards when it comes to the pursuit of truth via careful analysis and dispassionate reasoning.

The ad hominem attacks issued from the string theory camp are appalling; its uninterest in the scientific method is baffling; and its emotional attachment to a theory that has virtually zero empirical support is depressing. What has happened to you guys?

28. **gedankentroll**
August 8, 2013

LogicFan said

"… physics. The intellectual standards of your discipline are abysmal. You have failed to make any significant theoretical advance in, say, over 30 years."

Whereof one is totally clueless, thereof one should be silent.

29. **Ted Danson**
August 8, 2013

Of course I have no idea who 'Giotis' really is but I have seen this kind of mentality amongst a number of people employed as researches in physics departments (and more than one commenter here). I'm not sure they realize how strange some of their comments seem to those who have (god forbid) been in the business of seriously confronting a model against experiment. Look at this comment for instance:

Giotis:
'Ok, then I would really like to know your theoretical arguments against SUSY being a Symmetry of Nature. Let me guess, you don't have any. You just sit there crossing your fingers hoping LHC doesn't find anything. That's pathetic…'

What a bizarre comment! It would seem to imply that in the mind of 'Giotis', physics beyond the standard model *must* involve supersymmetry. No choice in it. This seems much more like a 'faith-based' statement than something a scientist would write.

30. **LogicFan**
August 8, 2013

gedankentroll:

That is a good paraphrase of Wittgenstein, who in turn is suggesting something important. But what he is not suggesting is that criticism by thoughtful, unbiased, and educated people be ignored.

I have noticed many parallels between string theory advocates and postmodern/Continental "philosophers". Both camps claim special access to profound truths; both camps are highly insular; both camps are resistant to critical inquiry (string theorists disregard calls for empirical verification and postmodern thinkers refuse to subject their "theories" to logical analysis); both camps use ad hominem attacks to defend themselves against calm criticism; both camps write in incomprehensible jibberjabber; both camps have an inflated sense of self-worth; and both camps have utterly, totally failed to add to the sum of human knowledge. The two groups have actually been counterproductive, on my view.

Instead of just working in service of your own ego, don't you want to help figure stuff out about this fascinating universe in which we live?

31. **gedankentroll**
August 8, 2013

Of a long Time I have suspected, that these modern Analytics were not scientifical!

32. **emile**
August 8, 2013

LogicFan: I'm afraid you are being distracted by a relatively small group of people whose notion of science has nothing to do with what 99% of working physicists would accept. To say that the field has low standards is not based on a good knowledge of what is going on. In particle physics, the standards are also high: get educated on what it takes before a paper is even submitted to a journal if you work on CMS or ATLAS. Physics in general is doing great: attend a few colloquia in various subfields and you will be impressed. In particle physics, we just found that the Universe has a scalar field with a non-zero average value. This is significant. The associated particle is a new type of particle never before observed. Not finding anything else at those energies is disappointing to some but it is also an important finding (as you can see from the way many react to this). Just 15 years ago we learned that the Universe is expanding. That's a major, major find. Not too long ago we confirmed that neutrinos oscillate and have now measured the associated mixing matrix which looks very different than its counterpart in the quark sector. I could go on. Lots of progress and lots of questions to answer. A few high profile people and a few bloggers can give the wrong impression. Another thing regarding SUSY: from an experimental point of view, the search will go on for years to come not because experimentalists are necessarily wedded to the idea, but because those searches cast a very wide net in terms of possible final states (these searches can catch many other things from other BSM models) and because you never know what is going to show up in the phase space you have not yet explored.

33. **Mike Mathison**
    August 8, 2013

    Hello Logic Fan – I think emile has more or less just said this already, but here's my take on your comments:

    Earlier, of philosophers, you said "we have maintained our standards when it comes to the pursuit of truth via careful analysis and dispassionate reasoning" but then later you said of your Continental school that they are "[…] resistant to critical inquiry […] refuse to subject their theories to logical analysis […] [and have] totally failed to add to the sum of human knowledge".

    I guess we all have our mad aunties in the attic – perhaps some of ours are string theorists and perhaps some of yours are post-modernists. However, if you're someone who likes big cool machines then I think you probably made the wrong choice going with philosophy rather than physics.

    Earlier, of physicists, you said "you have failed to make any significant theoretical advance in, say, over 30 years". However, as a big fan of physic myself, I don't think that can be entirely correct as I understand there have been many advances in fields such as astrophysics, solid state physics and so forth, as well as a steady stream of speculative big-question theoretical ideas proposed around gravity/quantum-mechanics/Standard-Model-refinement/HEP in general – and at least some of these are/have-been susceptible to falsification either via experiment or observation. Most of us still hold onto the quaint idea that concocting theories that are none the less subsequently shown to be wrong by Nature is *not* actually a waste of time and *does* actually constitute a kind of progress – if only because the knowledge that a plausible/appealing theoretical idea is actually *wrong* is in and of itself very useful knowledge to have gained as it further constrains future theories and is probably telling us something important about our deeper assumptions. I believe such ideas and reasoning are also prevalent in philosophy, though I'm no expert. Anyhow, although it may not be in accordance with the extraordinary rigors of your discipline, I for one would certainly count the failure so far of super-symmetric particles to appear at LHC energies (and the mainstream theories' resulting entry into the arena of the decidedly unwell/coughing-up-blood) as an important advance for physics, both experimental and theoretical.

34. **lucretius**
    August 8, 2013

    Before I ask my question I think I should make a disclosure.

    I am a mathematician with a rather detached attitude to "physical truth" but actually quite excited about the contribution mathematical physics (including the part you refer to in the title of your book and this blog) has made to my own subject. For me the discovery of Witten-Seiberg invariants, Chern-Simons-Witten theory, Gromov-Witten invariants, supermanifolds, superalgebras, Mirror Symmetry etc, more than justifies everything that has gone into the study of the physics. Moreover, I am quite confident that even if the physical theories (like SUSY) do not find experimental confirmation during the next 100 years, the mathematical ideas they have stimulated will find their way into physics via another route. So, quite naturally I think, I am certainly rooting for SUSY physics crowd and hoping that the LHC will bring them some good news. I think it will be good for science, mathematics, almost everyone … which brings me to my question.

    The question is, would you agree that by your public stand on this matter, the success of your book and the reputation it has given you among your readers and journalists, you have inevitably turned yourself into "a partisan of failure" in this matter? Isn't it true that if LHC does find evidence of SUSY you will find yourself in a rather difficult position? Isn't it rather like the situation of an out of power politician who finds himself in the situation where, from his point of view, "the worse is the better"?

35. **Dom**
    August 8, 2013

    Probably going off-topic but aren't you misrepresenting Peter's position which seems essentially to be "Show me the evidence via testable predictions" which then gets hijacked into a meta argument about what science is that we've all heard a million times.

36. **Zathras**
    August 8, 2013

    To answer lucretius, I think one has to recognize a spectrum of theories out there from empirically testable to completely untestable. SUSY is somewhere in the middle, since experiments can confirm or rule out large swaths of the parameter space, but there is still the theoretical possibility of SUSY at energies which cannot be tested. If SUSY is confirmed (looking unlikely, but let's just say…), then there are testable predictions to look at. On the other hand, string theory is something which makes no testable predictions, and has no hope of doing so. And there is no science. Woit's and Smolin's books are not anti-SUSY books; they oppose ideas masquerading as science which make no testable predictions. To the extent SUSY does so, Peter would have no problem with it.

37. **Peter Woit**
    August 8, 2013

    Dom/Zathras,

    The SUSY issue right now isn't so much about testability, but about whether one can get that part of the theory community that for decades has been pushing a specific idea about particle physics to admit that it has been falsified, draw the implications of this, and move on to try and find something more promising. If you know nothing about the scale of SUSY breaking, it's an empty, untestable idea. Pre-LHC, the argument was that SUSY breaking should be at the weak scale, stabilizing it with respect to the Planck scale. This wasn't a very good idea and it didn't explain much, but it got a huge amount of attention and had a great influence on the field. Now the question is whether the fact that it has been experimentally shot down will get acknowledged or evaded.

    About issues raised by lucretius, a separate comment will follow.

38. **Peter Woit**
    August 8, 2013

    lucretius,

    I don't think I'm a "partisan of failure", since I don't think the LHC results ruling out SUSY extensions of the SM broken at the electroweak scale are a "failure". They're actually a huge success of the scientific method. A machine has been built and operated successfully to study nature at distance scales nearly an order or magnitude smaller than ever done before, and one proof of the pudding of this great success is that certain not very good speculative ideas were ruled out. The only possible failure here is if some theorists manage to evade the implications of the LHC results and convince others to keep pursuing the same ideas.

Sure, if the LHC all of a sudden turns up evidence of the kind of SUSY I've been arguing against, then my arguments were misguided and people should take that into account. At this point though, I think if you look at what I had to say in my book (written mostly in 2002), it holds up very well in light of LHC results, especially compared to typical other books of that era which enthused about string theory and SUSY ideas which it's now clear haven't worked out.

It's important to distinguish between the SUSY models that are being shot down and the mathematics that you mention, most of which has nothing at all to do with them. Quantum field theories and superalgebras have all sorts of very interesting mathematics behind them, well worth pursuing (so much so that I included some of this in the course I taught last year), but this doesn't mean that one specific, quite complicated and ugly, example of these is worth much attention. For me the argument against SUSY extensions of the SM has not just been that there's no experimental evidence for them or that they're not as predictive as one would like, but that the ones on sale are hideous and don't really explain anything. I wouldn't be surprised at all if there is some better unified theory that has an interesting super-algebra of symmetries, but just don't think that the ones we know about could possibly work. Hopefully the LHC success at ruling out bad ideas will encourage people to look for better ones. This includes mathematicians, who should pay attention to what the class of theories is that they were often told was of great physical importance, but didn't turn out to be, and decide where to direct their attentions accordingly.

39. **Urs Schreiber**
August 8, 2013

Florin, for an extensive list of references on the local worldline supersymmetry of spinning particles, see here:
http://ncatlab.org/nlab/show/spinning+particle#WorldlineSupersymmtryReferences .

Generally, local supersymmetry is generic in low dimensions, while global supersymmetry is the opposite: non-generic and unlikely.

Historically, people lifted the worldline formalism of spinning particles to the worldsheet formalism of what back then were called "spinning strings", which is 2d gravity coupled to fermions. It turns out that the most natural Lagrangian for this is automatically locally supersymmetric — this is how the concept of supersymmetry was _found_ in the West. People didn't look for it, it jumped at them when they wrote down fermions in 2d gravity. Ever since, the former "spinning string" is known as the "superstring".

The miracle that happens then is that while the second quantization of spinning particles is not itself locally supersymmetric, the second quantization of spinning strings is itself locally supersymmetric. And this is how perturbative string theory implies that if there are fermions at all, then there is local supersymmetry.

As I keep saying, and as our host is now acknowledging too, it is crucial to distinguish local supersymmetry from global supersymmetry. That we don't find global supersymmetry at the LHC says nothing about whether "the universe is supersymmetric". It may or may not be, and it is good to know the difference.

40. **Peter Woit**
August 8, 2013

Urs,
I don't think that global vs. local SUSY is relevant, or that a statement like "the universe is supersymmetric" the way you are trying to use it is meaningful, rather than an empty slogan.

The Dirac operator and spinors show us that there's an interesting superalgebra at work in fundamental physics, but this has nothing to do with SUSY extensions of the SM. One should stop sloganeering, and pay attention to what exactly it is that works as good physics and good mathematics, and what doesn't.

41. **Urs Schreiber**
August 8, 2013

One should stop sloganeering. We can agree on that. A good move is to stick to precise scientific language and avoid imprecise sweeping statements.

There is so much confusion about string theory out there that in all the confused criticism the discussion never gets to what might be good, substantial criticism.

42. **Giotis**
August 8, 2013

The only way to extend Poincare algebra and still respect Lorentz invariance is the super-Poincare algebra. You do that and the existence of fermions is imposed to you by the algebra. SUSY in this sense *explains* fermions which are inherently Quantum mechanical! Similarly in the Superspace formalism you take the ordinary space time and the super-Poincare group forces you to extend it to a Superspace by adding fermionic degrees of freedom to the usual bosonic.

What deeper theoretical reason you want to acknowledge that SUSY is symmetry of Nature? Nature is crying out that it is Supersymmetric at a fundamental level.

43. **Nex**
August 8, 2013

No theoretical reasons ever suffice to prove something is a part of Nature.

44. **CWJ**
August 8, 2013

Seth,
The supersymmetry in Efetov's book is really just a useful tool for carrying out certain Gaussian integrals. Using "normal" numbers, the integral of the Gaussian exp(-ax^2) is proportional to 1/sqrt(a), but if you take the same integral with Grassmannian (fermionic) variables, then it is proportional to sqrt(a). This turns out to be useful in computing Green's functions of random matrices, for example.
(Sorry for being off-topic, Peter–but at least it's on a technical issue and not a rant! And since I happened to laboriously work through the technique some 15 years ago but never published anything…might as well put that knowledge to use.)

45. **Armin Nikkhah Shirazi**
August 8, 2013

Giotis,

I think when one is deeply committed to a particular point of view it can sometimes be very difficult to be aware of the assumptions one makes. In your first sentence " The only way to extend Poincare algebra and still respect Lorentz invariance is the super-Poincare algebra." I could detect at least three.

Assumption 1: One needs to extend Poincare algebra in order to resolve certain issues with the SM that SUSY addresses
Assumption 2: Any approach to understanding these issues potentially conflicts with Lorentz Invariance

Assumption 3: Lorentz Invariance should be respected in a deeper theory

Of course, the assumptions differ in how confident one can be in them, for example I am vastly more confident in the third one than the first two but my point is this: The strength of one's argument depends on the strength of one's assumptions, and if one is either not aware of them or has a quasi-religious belief that they are correct, one is unlikely to recognize the points at which one's argument could break down.
To someone who does not share all of your assumptions it may not necessarily seem that "Nature is crying out that it is Supersymmetric at a fundamental level."

46. **Peter Woit**
August 8, 2013

Armin,

There's another problem, that "The only way to extend Poincare algebra and still respect Lorentz invariance is the super-Poincare algebra." isn't actually true. Presumably Giotis is actually talking about the extension of Coleman-Mandula to include SUSY, so one could figure out what he intended to say, but if you're going to base your argument on a no-go theorem, it would be a good idea to get the theorem right…

47. **lucretius**
August 8, 2013

If you look at section 10.3.3 of Efetov's book you will find that he speculates that there might be a possible relation of the "sigma model" with string theory and quantum gravity. He concludes: "Anyway, the unification of these, at first glance, completely different theories from different branches of physics looks very exciting". Two conclusions:
1. The original question was only about 99% off topic.
2. Some people find "unifications" very exciting even if they do not seem to lead to any now empirical discoveries.

48. **Giotis**
August 8, 2013

Come on, you know what I mean. In fact if you want to be pedantic there are two theorems: Coleman-Mandula and Haag-Lopuszanski –Sohnious.

So you could say the SUSY is the most general symmetry you could have according to these two no-go theorems.

49. **N. Nakanishi**
August 8, 2013

Giotis,

You are wrong. HLS's no-go theorem holds only f0r the physical S-matrix symmetries, that is, for unbroken symmetries. Evidently, SUSY, if it existed, is broken in the actual physical S-matrix. Hence HLS's theorem does not give any priority to SUSY. There is no no-go theorem for the Lagrangian symmetries which we really want to have.

By the way, have you understand my proof of the proposition that SUSY cannot be a fundamental symmetry?

50. **wolfgang**
August 8, 2013

Urs,

your argument is correct, but you have to admit it is a little bit like parents who are proud that their little kid already knows complex numbers – because it can count to three. Yes, 1, 2 and 3 are complex numbers, but …

51. **David M.**
August 9, 2013

Giotis,

It's actually incorrect to say that the only way to extend the Poincare group is through SUSY. For one thing the Coleman Mandula theorem deals with S-Matrices, which assumes the existence of asymptotic states that don't interact with each other. You can do away with this assumption if you consider a conformal field theory. So there are three extensions of the Poincare group: 1)Adding conformal transformations, 2) Adding SUSY generators, or 3)Both.

Another assumption is that we live in flat Minkowski space. If you consider DeSitter or AdS space another exception: Vasiliev theory. There you have an infinite number of higher spin fields.

Although I agree with you that the fact that in flat space, when there is a mass gap and the S matrices are nontrivial the only nontrivial extension of the Poincare group is through SUSY is a compelling argument, you do need to remember the assumptions and other exceptions (besides SUSY) to the Coleman-Mandula theorem.

52. **Giotis**
August 9, 2013

David M,

I'm not sure what you are trying to convey here. Even for massless fields and Conformal field theories the most general symmetries you could have are still Supersymmetric i.e. the Superconformal algebra. And Vasiliev's Higher Spin superalgebras are Supesymmetric ones too.

So what's your point?

53. **David M.**
August 9, 2013

Giotis,

You wrote: The only way to extend Poincare algebra and still respect Lorentz invariance is the super-Poincare algebra.

My point is that this is not true, there are other extensions that work if you relax the assumptions. Yes the maximal extension of the Poincare algebra will include SUSY, but now we've switched from the question of what's the maximal extension of the algebra (which will include SUSY) to a minimal nontrivial extension (which does not necessarily include SUSY). And we know conformal symmetry is not an exact symmetry of nature because we have mass (although at high energies its a good approximate symmetry), so theres no reason to assume SUSY will be an exact symmetry either just because its allowed.

That's not to say these subjects aren't worth studying. CFTs and SUSY gauge theories are important objects to study to understand QFTs and can appear in condensed matter systems (http://arxiv.org/abs/1009.5127 Sung Sik Lee has done work on emergent SUSY), but until its seen an experiment you can't be 100% certain its a symmetry of particle physics.

54. **StevenC**
August 9, 2013

When you think of why someone will never give up an idea or object, you have to step out from the idea or the object, and consider what that idea or object means to that individual. It is more than physics, more than career, but it is psychology. It is completely personal. I don't see SUSY (or string theory) dying in next 10 years.

Sadly, the most successful people who are flexible, accept new information as it is, and understand what that do not understand.

"It is not the strongest species that survive, but the one that is most adaptable to change."
"To know is to know that you know nothing. That is the meaning of true knowledge."
"Facts change, and I change my mind. Will you sir?

55. **Jim Akerlund**
August 9, 2013

I guess a quote from Will Rogers seems appropriate here.

"It ain't what you don't know that counts. It's what you know that ain't so."

Ran across this in the book "A Mathematician Reads the Newspaper" by John Allen Paulos.

56. **Hmunu**
August 10, 2013

Trying to balance the opinions of experts is a rather tricky business, particularly for those of us looking at HEP-EX/TH from outside. I know Bayesian methods are used in the estimation of parameters in particular physics so was hoping that some group had run a model selection test between the SM and a version of SUSY.

With a little digging in the arxiv, I came across this paper with the provocative title *"Should we still believe in constrained supersymmetry?"* The conclusion of **NO** is presented in a rather circumspect and wordy fashion, despite the analysis being pretty definitive.

Has anyone else seen any similar studies testing the SM vs MSSM or some other slice of the SUSY space? I cannot imagine models with a wider range of free parameters would fair any better, their posterior probabilities being dragged down by the increased Occam factors. Is it worth another two decades of work on SUSY, particularly as the models get more contrived as the simplest ones are ruled out? I guess that's for HEP internally to decide.

57. **Marcus**
August 11, 2013

Many former string researchers now seem to have refocused on holographic duality. I wonder if this new article by Don Marolf is relevant to the "never give up" issue:

"A defining feature of holographic dualities is that, along with the bulk equations of motion, boundary correlators at any given time t determine those of observables deep in the bulk. We argue that this property emerges from the bulk gravitational Gauss law together with bulk quantum entanglement as embodied in the Reeh-Schlieder theorem. Stringy bulk degrees of freedom are not required and play little role even when they exist…"
from *Holography without strings?* http://arxiv.org/abs/1308.1977

58. **george ellis**
August 13, 2013

Historical comment:

In your March, 2004 blog you quoted from a talk by David Gross, and said "He then went on to claim that in 3-4 years there will be a headline in the New York Times about the discovery of supersymmetry at the LHC."

Not so.

59. **Visitor**
August 13, 2013

Well you have to give him some credit at least; it was a falsifiable prediction…

Quick Links

August 13, 2013
Categories: Uncategorized

- At HEP blogs you should be reading already, there's Tommaso Dorigo on 5 sigma (with more promised to come), and Jester on the lack of a definite BSM energy scale. Jester puts his finger on the big problem facing HEP physics. In the past new machines could be justified since we could point to new phenomena that pretty much had to turn up in the energy range being opened up by the machine (Ws and Zs at the SPS, the top at the Tevatron, the Higgs at the LHC). Now though, there's nothing definite to point to as likely to show up at the energy scale of a plausible next machine. Jester includes a graphic from a recent Savas Dimopoulos talk characterizing the current situation in terms of chickens running around with their heads cut off, which seems about right.
- The black hole information paradox has been around for nearly forty years, with the story 10 years ago that it supposedly had been resolved by AdS/CFT and string theory. For the past year or so arguments have been raging about "firewalls" and a version 2.0 of the paradox, which evidently now is not resolved by AdS/CFT and string theory. I couldn't tell if there was much to this argument, but the fact that there's a Lubos rant about how it's all nonsense made me think maybe there really is something to it. As usual though, my interest in quantum gravity questions that have nothing to say about unification is limited. For those with more interest in this, I'll just point to today's big article in the New York Times, and next week's workshop at KITP where the latest iterations will get hashed out. For more on the challenge this argument poses to the idea that AdS/CFT gives a consistent picture of quantum gravity, see this recent talk by Polchinski.
- For another challenge to orthodoxy from someone at UCSB, Don Marolf has a new preprint out arguing that strings are not needed to understand holography:

  > Stringy bulk degrees of freedom are not required and play little role even when they exist.

Comments

1. **David Appell**
   August 13, 2013

   Is it just me, or does Leonard Susskind not possess a curious ability to get into the media no matter is the physics topic du jour?

2. **Lun**
   August 13, 2013

   The strong version of ads/Cft must be stringy as you need two expansion parameters, the number of colors and the coupling constant.
   In the planar limit this is however irrelevant, it seems that in that limit (where most on the evidence for holography comes from) such arguments could apply.
   The firewall debate, with neither a sliver or experimental connection nor anything approaching rigor or a coherent picture, is a good example of what's wrong with modern physics.

3. **Oldster**
   August 13, 2013

   David: Leonard Susskind is an outgoing, intelligent, personable guy who has been in the thick of the information paradox and string theory scene for decades, and who likes to write and comment for the public as well. No matter what your views on the physics, it would have been hard to avoid his name in this particular story. I say this as someone who is a hopeless reactionary compared to him on physics too, not as an advocate … 😉

4. **Peter Shor**
   August 14, 2013

   Leonard Susskind gives excellent talks, is a great teacher, and enjoys talking to the public and the media. This is undoubtedly one reason why reporters like him; they find him eager to talk to them, and they can understand him.

   I don't think he's beating on the media's doors and asking them to write stories about him, the way I suspect some other physicists are doing.

5. **Anonyrat**
   August 14, 2013

   Is Don Marolf's argument essentially that a theory with diffeomorphism invariance has an action whose significant terms are boundary terms, and hence has a form of holography?

6. **Mitchell Porter**
   August 15, 2013

   I am still digesting Marolf's paper but it seems a little dodgy. It does not talk about holographic *duality* at all – i.e. the equivalence between a theory in the bulk and another theory on the boundary. Instead, it is (I think) an argument that a general quantum state in the bulk theory can be constructed just using bulk operators from the boundary.

   To understand what that means, we need to distinguish between operators in a separate theory defined on the boundary, as in AdS/CFT, and operators in the bulk theory which pertain to bulk observables near the boundary. In the latter case we are talking only about one theory, called the bulk theory, but we concern ourselves with operators in that theory which are associated with the edge of the bulk.

   Marolf also uses the Reeh-Schlieder theorem, a well-known theorem from algebraic/constructive/axiomatic QFT which says that, because of the entanglement of the vacuum state, any state in the QFT can be approximated arbitrarily closely, by combinations of operators from arbitrarily small spatial regions. He then combines this with special properties of gravitational theories (I haven't decoded that part) to deduce a property he calls "information holography".

   As a non-expert observer, what I notice is that there have been at least two other attempts to apply Haag-like models of QFT to AdS/CFT which have come up wanting. Both were debunked by Jacques Distler: first, years ago, Rehren's "algebraic holography", which Distler dubbed "Rehren duality" to distinguish it from the real thing, and then, much more recently, Gomes et al (arxiv:/1305.6315), who proposed to explain the emergence of gravity from the RG flow on the boundary by using the "shape dynamics" formulation of general relativity. But it will take someone better than me to say exactly what the status of Marolf's paper is.

7. **ohwilleke**
   August 15, 2013

   "Is it just me, or does Leonard Susskind not possess a curious ability to get into the media no matter is the physics topic du jour?"

One of the lessons that I learned in student government as an undergraduate and then a law student (and then again later during a brief stint as a professional journalist), is that the vast majority of people, even prominent people in politics or at the top of their fields, are extremely reluctant to talk to reporters (and not necessarily without good reason, I should add). Despite not having an official PR position, I ended up in the student newspaper almost every week in student government related stories simply because the reporters couldn't find anyone else who would reliably provide them with quotable quotes for their stories. Later, as a reporter, the lazy thing to do was always to call people who I knew would be willing to go on the record for me when I wanted a quote.

If you are among the 5% or so of prominent people in a field who are willing to go on the record with a statement about something on short notice and say something quotable when you do, reporters will flock to your door. Susskind and science reporters have mutually discovered this fact.

8. **Tommaso**
   August 17, 2013

   Hello Peter,

   thanks for the mention of my article on the 5-sigma criterion, of which I issued today the third (out of 4) part. I believe it is an important thing that we realize how that criterion has strong limits and should only be thought of as a guideline, not as a fixed rule.

   Best,
   Tommaso

9. **Peter Woit**
   August 17, 2013

   Tommaso,

   Wonderful series of posts. You're doing a great job of not just explaining the "5 sigma" issue, but more generally giving a lot of insight into the subtleties involved in any search for a bump in an HEP experiment. Very educational.

   My own amateur, much cruder, take on these issues has generally been to not believe discovery claims made by one experiment, no matter how many sigma they say. If their competitor says they also see the same thing, that's the time to start becoming a believer…

Belief in multiverse requires exceptional vision

August 14, 2013
Categories: Multiverse Mania

Tom Siegfried at Science News has a new piece about how Belief in multiverse requires exceptional vision that starts off by accusing critics of multiverse mania of basically being ignoramuses who won't accept the reality of anything they can't see with their own eyes, like those in the past who didn't believe in atoms, or superstrings:

> If you can't see it, it doesn't exist. That's an old philosophy, one that many scientists swallowed whole. But as Ziva David of NCIS would say, it's total salami. After all, you can't see bacteria and viruses, but they can still kill you.
>
> Yet some scientists still invoke that philosophy to deny the scientific status of all sorts of interesting things. Like the theoretical supertiny loops of energy known as superstrings. Or the superhuge collection of parallel universes known as the multiverse.
>
> It's the same attitude that led some 19th century scientists and philosophers to deny the existence of atoms.

The problem with the multiverse of course is not that you can't directly observe it, but that there's no significant evidence of any kind for it: it's functioning not as a testable scientific explanation, but as an excuse for the failure of ideas about unification via superstring theory. Siegfried makes this very clear, with his argument specifically aimed at those who deny the existence of "supertiny loops of energy known as superstrings", putting such a denial in the same category as denying the existence of atoms. Those who deny the existence of superstrings don't do so because they can't see them, but because there's no scientific evidence for them and no testable predictions that would provide any.

Siegfried has been part of the string theory hype industry for a long time now, and was very unhappy with my book, which he attacked in the New York Times (see here) as misguided and flat-out wrong for saying string theory made no predictions. According to him, back in 2006:

> …string theory **does** make predictions — the existence of new supersymmetry particles, for instance, and extra dimensions of space beyond the familiar three of ordinary experience. These predictions are testable: evidence for both could be produced at the Large Hadron Collider, which is scheduled to begin operating next year near Geneva.

We now know how that turned out, but instead of LHC results causing Siegfried to become more skeptical, he's doubling down, with superstring theory now accepted science and the multiverse its intellectual foundation.

The excuse for Siegfried's piece is the Wilczek article about multiverses that I discussed here, where I emphasized only one part of what Wilczek had to say, the part with warnings. Siegfried ignores that part and based on Wilczek's enthusiasm for some multiverse research takes him as a fellow multiverse maniac and his article as a club to beat those without the exceptional vision necessary to believe in superstrings and the multiverse. Besides David Gross, I'm not seeing a lot of prominent theorists standing up to this kind of nonsense, leaving those invested in failed superstring ideology with the road clear to turn fundamental physics into pseudo-science, helped along by writers like Siegfried.

Update: A commenter points to this from Wilczek, noting his lesser multiverse enthusiam than Siegfried's.

Update: Ashutosh Jogalekar at The Curious Wavefunction has a similar reaction to the Siegfried piece.

Update: There's an FQXI podcast up now (see here), with Wilczek discussing the multiverse.

Comments

1. **H**
   August 14, 2013

   In a tweet, Frank Wilczek says he is less enthusiastic than Tom Siegfried. https://twitter.com/FrankWilczek/statuses/367749737249652736

2. **paddy**
   August 14, 2013

   "Exceptional vision" ??? Siegfried is certainly not at fault for my immediate reaction to his choice of words: that vision required to see N-rays?

3. **paddy**
   August 14, 2013

   Pardon my double post….I tried to ignore Siegfried's choice of the word "belief" in my previous post…but now my gorge is rising..time to have another beer.

4. **Matt**
   August 14, 2013

   Cosmic inflation predicts that the actual universe is much larger than the observable part we can see, and by a truly staggering amount — the observable region is like an atom compared to the rest. The rest is called the multiverse. Other regions can fall out of inflation just like our region did.

   Cosmic inflation is now heavily supported by high-precision observational data. It also neatly resolves the horizon problem, the flatness problem, the relics problem, the structure problem, and what gave the big bang its initial oomph to begin with. And we have direct evidence today that the fundamental mechanism actually works, because of the existence of (much weaker) dark energy.

   So how is there "no significant evidence" for the multiverse? Is the evidence for inflation wrong? Is there some magical principle that prevents any other region of the multiverse from falling out of inflation like our region did, or that nonlocally forces inflation to stop everywhere simultaneously all at once? The name "multiverse" might be the trouble — would it be less controversial if we just called it "the rest-iverse"?

   Note that I'm not talking about string theory or the landscape here — those concepts are logically independent from cosmic inflation, and the enthusiasm of string theorists is not evidence against cosmic inflation. When you say there is no evidence for the multiverse, are you just referring to its fusion with the idea of the string landscape?

   I'm just trying to make sure I understand a little more precisely what you're saying here.

5. **Peter Woit**
   August 14, 2013

Matt,
I think I've discussed this "inflation implies a multiverse" argument many times here. The issue is not whether there's more than the observable universe out there. That may very well be, and maybe someday we'll even understand inflation well enough to have a good model of what that might be. The point though is that there is zero evidence that whatever else there is has different physical laws than ours, and that is what is needed to make the whole anthropic business work. In simple models of inflation, whatever else is out there will have the same laws as ours, so yes, you can get a multiverse, but a pretty boring one. You can use string theory or something else to come up with much more complicated models that give you pretty much any physics you want in different parts of the multiverse, but there's no evidence for these, they are untestable and explain nothing. These models function just as excuses for the failure of string theory to explain anything and that's the pseudo-science problem here.

6. **Daniel TTY**
August 14, 2013

How about other kinds of many-universes theories, such as universes being created in quantum fluctuations, or via black holes?
Do you think that our universe is the only one?

7. **Peter Woit**
August 14, 2013

Daniel,
I have no idea whether there are other universes. If you have a model that produces them via quantum fluctuations or black holes, fine. But if it's untestable and does nothing to explain the things that the SM and GR leave as mysteries, I'm just not interested. If it's untestable and you want to claim it shows why those mysteries can't be explained, and it's hideously complicated, I think you're a pseudo-scientist (and, unfortunately, in illustrious company…)

8. **Matt**
August 14, 2013

Hi Peter,

That's exactly what I wanted to know—thanks.

So I think it's important make this distinction clear. You are okay with the notion of the "boring" version of the cosmic multiverse (which I don't happen to think is very boring! I mean, come on, right!?) necessarily predicted by cosmic inflation, namely, that the actual cosmos is far vaster than our observable region (that's the whole basis for how inflation solves the various problems like flatness, the horizon problem, etc.), and in those other regions inflation can end and produce other observable regions.

Where you draw the line is at the pure speculation that we can attach a string landscape to this cosmic multiverse and use it to start making anthropic claims about what the low-energy physics of our own observe universe looks like—the particle menu, the coupling constants of the Standard Model, etc. So, to be clear, you are okay with the "boring" multiverse, but not okay with the string landscape and the anthropic principle.

Is that a correct appraisal? That may seem like a subtle distinction, but I'm sure it's pretty important to a lot of people! Maybe you could say "anthropic mania" or "string landscape mania" rather than "multiverse mania"? Because even apart from string theory or anthropic reasoning, the "boring" multiverse is a very conceptually amazing consequence of inflation!

9. **Peter Woit**
August 14, 2013

Matt,

Sure, I'm ok with inflationary models that produce lots of unobservable universe with the same laws of physics, but, honestly I do think they're kind of boring. The people out there selling the multiverse as a radical new advance in physics aren't selling this kind, they're selling the one with different physics everywhere, as an explanation for the laws of physics, and that's what I have trouble with.

10. **Anonyrat**
August 14, 2013

I would find it very hard to believe that our cosmos coincides exactly with our observable region- our past light cone. This with or without inflation.

11. **Abbie Hoffman-Tegmark**
August 15, 2013

There are a lot of different multiverses going around these days.

12. **Casey Leedom**
August 15, 2013

So sad that Tom Siegfried is headed this way. Luckily his bias doesn't seem to have overwhelmed Science News' content or I'd have had to cancel my subscription years ago. Interestingly, I was actually turned on to your book via a short review in Science News years ago when it came out (along with Lee Smolin's book in the same issue if I remember right).

13. **Bee**
August 15, 2013

The multiverse is an inevitable consequence of attempting to describe nature by relying exclusively on mathematical consistency. Whatever you do, you'll always have to pick some axioms of your theory and these axioms will either require additional justification or, if you don't have that justification (call it selection), they'll imply a vast number of "universes" that are nothing like our own. That's got nothing to do with string theory in particular, it's a consequence of mathematical "space" being larger than our observed "space".

There are only two ways to deal with that. Either you believe in the multiverse as being real and say we only observe part of it. Or you select the "real" part of the mathematically possible by binding it to observation and disregard the rest.

Which way you prefer depends on whether you believe that mathematics is a language that we use to describe nature (in which case the multiverse is an artifact of the limits of that language) or whether you believe that nature fundamentally is mathematical (in which case you're now missing a criterion that tells you which part of the mathematics is real).

The reason this problem occurs now is that people are attempting to construct theories without observational constraints. (One can plausibly argue that requirements like renormalizability, gauge invariance, and Lorentzian signature are ‘observational’ constraints, though they’re rarely referred to as such. But these are the requirements that dramatically reduce the space of possible theories. Alas, they’re evidently not sufficient.)

I wrote a blogpost on that here.

Personally, I’m in the camp of people who think that mathematics is a language and shouldn’t be confused with reality.

14. **markusm**
August 15, 2013

Maybe there is already evidence for the (“boring” ?) MV:
http://www.youtube.com/embed/OAL1-vzMvmA

15. **imho**
August 15, 2013

There’s evidence for a boring multiverse??? I had no idea. Can some one give me a pointer to a good survey article appropriate for a Cond Matt theorist with a passing interest in these things.

16. **Jim**
August 15, 2013

Matt/Peter,
I thought I might take this opportunity to test my own understanding. Apologies Peter if this is somewhat off-topic.

Although inflation is part of the ‘standard’ lambda-CDM model of big bang cosmology, my understanding is that it is still a bit controversial. In it’s most commonly accepted form, it is based on the idea of supercooling at the end of the Grand Unification epoch, triggered by an ‘inflaton field’ responsible for symmetry-breaking leading to the separation of the strong nuclear force from the electro-weak force. It’s controversial because we don’t have a GUT that everyone can buy into, and so cosmologists have been completely free to choose a shape for the inflaton potential that is consistent with the universe we see today.

So far as I know, there’s nothing in the theory to indicate whether inflation applies to the whole of the post big-bang universe or whether it was just one small bubble of spacetime within this that inflated. If I’m right, then debates about whether or not there’s a larger ‘boring’ universe outside of our bit of inflated spacetime are really rather moot – in any case we’ll never know one way or the other.

Sure, inflation resolves the flatness, horizon, and other problems, but there may be lots of other ways of doing this that do not involve inflation and, if I’m being a tad skeptical, the lambda-CDM model is, for the most part, a best-fit exercise with a good number of variable parameters. It is certainly not wrong (as the fit is quite spectacular), but as we have no theory that can provide *a priori* predictions for these parameters (just as we can’t predict the parameters of the standard model of particle physics) then I’d argue that all bets are off.

Inflation is a great device and it seems to work well, but I think it best that we don’t over-interpret it.

17. **Peter Woit**
August 15, 2013

imho,

I think very few physicist’s believe in the sort of evidence for a multiverse linked to above. For more about this, see here

http://www.math.columbia.edu/~woit/wordpress/?p=5966

18. **Low Math, Meekly Interacting**
August 15, 2013

“Multiverse” seems to me like a fairly meaningless term (i.e. it gets used for so many different things it’s hard to tell if it means anything). However, to the extent it is not completely bereft as noun specifying something, I don’t think it makes much sense to conflate it with “everything beyond our cosmic horizon”. It seems to get used most often as a term for the ensemble of (mostly) isolated vacua with different physical constants, e.g. the string theory landscape.

19. **Chris Oakley**
August 15, 2013

“Belief in multiverse requires exceptional vision” is a very 1960s thing to say, and sounds a lot like “Belief in multiverse requires exceptional amounts of LSD”. It is just more proof – if proof was needed – that the subject has been taken over by hippies.

20. **Harald Kirsch**
August 15, 2013

Can’t help to quote Gemany’s ex chancellor Helmut Schmidt: “People who have visions should go see a doctor.” (http://en.wikipedia.org/wiki/Helmut_Schmidt#Senator)

21. **ED**
August 15, 2013

The most plausible model of the physical world could be a very bad framework for scientific investigation, and in the multiverse wars, it would be nice to see this fact kept in focus. To be worth investigating, a model must be plausible , yet a plausible model may not be worth much attention.

If lost keys might be under a lamp post, might be in the dark beyond, and yet seem most likely to be at the bottom of a river. A wise investigator would neither let inaccessibility discredit the riverbottom-key hypothesis nor jump in the river and drown.

In other words, “X is unobservable, probably true, and worth considering” is entirely compatible with “Assuming that X is true would destroy physics”, and both are compatible with doing experimentally oriented physics and (mostly) ignoring X.

That said, I find Wilczek’s arguments (and lament) persuasive. I recommend the article as an exploration of the limited but non-zero utility of anthropic reasoning.

22. **Peter Woit**
August 15, 2013

ED,

One problem is that "plausibility" is in the eye of the beholder. Those with decades of their lives and their reputations invested in string theory may find the anthropic string landscape a plausible idea, others not so much. They also may feel regrets about putting the torch to physics as a science, that this makes them feel bad, although not as bad as they would feel if they had to admit they had been wrong.

23. **vmarko**
August 15, 2013

Matt said:

"Cosmic inflation predicts that the actual universe is much larger than the observable part we can see, and by a truly staggering amount — the observable region is like an atom compared to the rest. The rest is called the multiverse."

If one is talking about the "boring" version of the multiverse, I think that the terminology is misguided. If there is nothing conceptually different between various inflated bubbles, then it's just more of the same thing, sitting outside our observable space. This does not, and should not, deserve to be called "multiverse". Rather, it is just an extension of what lies beyond our cosmic horizon. It should still be called "universe".

The term "multiverse" makes sense only if those other patches of space are somehow very very different than our own (like having different values for the coupling constants etc.). And *that* is the problem — there is no convincing way to establish that such things actually do exist. This is the failure of string theory that Peter is criticizing all this time.

In this sense Peter is right to push all "multiverse" stuff into the same basket — the inflation-induced boring version of the "multiverse" should not have the "multi" part inside its name in the first place. It's just misguided use of terminology.

Best, 🙂
Marko

24. **Shantanu**
August 15, 2013

Matt,
Despite much hype, I still don't think we can conclusively say for sure inflation happened. The only thing we can say for sure from Planck is that n is different from 1.
We still don't know the energy scale of inflation and GUT based inflation is ruled out from Planck data. Also there are fundamental conceptual problems with single scalar field inflationary models (see http://arxiv.org/abs/1304.2785)

Another thing is that there are so many models of inflation , see for example this 330 paper http://arxiv.org/abs/1303.3787
IF you have so many "models" of inflation, by random chance one of them is probably right. But that defeats the original purpose of inflation.
Also as Peter and other mentioned there is no-observation al evidence for multi-verse and given so many models of inflation, its not obvoious which of them need a multiverse and which ones don't.
and

25. **Matt**
August 15, 2013

That the actual "rest-iverse" is far, far vaster than all that we can directly see all the way out to the earlist galaxies is a fairly robust, model-independent consequence of inflation, regardless of the shape of the potential or the number of scalar fields. In essentially any model, you need enough e-foldings to solve the various problems with the old big bang model, and that leads to a humungous cosmos far bigger than the popular understanding of the term "universe."

It's really a staggering and under-appreciated triumph of science that precision cosmology has advanced to the point at which we can confirm the general validity of cosmic inflation, even if we cannot yet pin down the specific model. And there are lots of people working on that right now, and checking their predicitons against observations carefully. That's most definitely bona fide science, and it would be a real shame if it got lumped into the string wars in the public understanding. And one would have to be really jaded not to realize how awesome this all is.

Some have called it the "megaverse." But the idea that the cosmos is so unimaginably big that there are lots of other pockets just like ours, separated by inflating regions, and with each pocket containing its own galaxies and whatnot—even without the string landscape and any unfounded claims of different low-energy physics in those pockets—certainly fits my picture of what a multiverse is. It's a less exotic notion of a multiverse than given by the string landscape, but as a vast expanse filled with little "island universes" separated by nontraversible inflating regions, it fits the bill for a lot of people when they use the term "multiverse."

26. **Chris W.**
August 15, 2013

Still, Matt, you must admit that there is a certain extravagance in this consequence of inflationary models, insofar as the consequence can never be tested, directly or indirectly. String theory, and perhaps alternative quantum theories of gravity, vastly complicate the picture by introducing enormous scope for variation of low-energy physics in those inflationary pockets, setting the stage for an anthropic trivialization of our understanding of the observed universe.

One might consider how analogous—or not—this situation is to that of the vacuum in quantum field theory. The features of that vacuum play a key role in calculating observable quantities—e.g., the well-known corrections of quantum electrodynamics.

27. **LogicFan**
August 15, 2013

I'm a philosopher, not a scientist, yet I cannot believe that Siegfried doesn't understand the essential difference here; that is, the distinction between being practically unable to observe some phenomenon and that phenomenon being perforce inaccessible to experiment.

I have heard some disparaging things (including, I believe, by the author of this blog) said about philosophers of science (I am not one), but every time I read something like this I am reminded how often even the educated make simple errors of logic, and thus how important our discipline really is.

28. **Low Math, Meekly Interacting**
August 15, 2013

The multiverse does seem to induce a curious form of cognitive dissonance. On the one hand, someone will propose that, say, bubble nucleation will leave an imprint on the CMB. "See? It's testable!" So we see nothing, feel like we've been had yet again, and complain that this whole multiverse business is proving to be utterly unobservable, unfalsifiable, and hence maybe outside of the scope of physics. "Stop being such a positivist!"

29. **Peter Woit**
August 15, 2013

LogicFan,

That's not really the relevant distinction here. What's more at issue is that you can test a theoretical framework not just by directly observing the fundamental entities of that framework, but instead by testing other implications of the framework. The first evidence for atoms was not from seeing atoms, but from seeing their effects on other larger objects (the phenomenon of Brownian motion).

You can imagine a theoretical framework that predicts other universes (which you can't observe), and if this same framework predicts indirect effects of such other universes that you can observe, or it makes new distinctive predictions (say the mass of the electron) that can be verified, you have significant evidence for the parts of the framework you don't have direct experimental access to. This is what the string theory landscape people would like to do. Their problem is that there is no evidence at all that their framework can do this: they can't calculate anything at all in it.

The problem with Siegfried's argument is a very conventional one: it's a straw man argument. He's claiming that the argument of multiverse critics is that the theory can't be directly tested, and that these critics don't understand about indirect tests. But that's not the argument (at least not from this particular very vocal critic). The argument actually is that the string theory multiverse makes no predictions of any kind, either directly or indirectly testable. It's not actually a scientific theory, but a pseudo-scientific construct designed to "explain" why you can't ever predict anything testable, directly or indirectly.

30. **Paulibus**
August 16, 2013

Bee says all that need be said about multiverse mania. Sadly, unless someone trips over a decaying proton while walking in the woods, we're watching the transformation of theoretical physics, sans its Baconian flavour, transform itself into a beached whale. Shades of Mark Twain's Great Nonesuch!

31. **lucretius**
August 16, 2013

I have to admit I am rather new to these "string wars". During the last 25 years or so I have listened to a number of talks by Witten at various mathematics conferences (including at least one at which Peter was present, although I only found it looking at the proceedings last night) and I have never doubted that his inspiration came form "mainstream physics". Well, now it seems, people are claiming it is not physics at all (although nobody seems to know what else it could be). I only realised that something weird was going on behind the scenes by reading Lubos's blog, which I visited for reasons which had nothing to do with "strings".

Anyway, I have decided to sit on the sidelines at least until I am confident I understand the main technical issues involved. This is still going to take time. I am somewhat surprised that not everyone thinks that is the right approach. I am also puzzled buy what exactly we are supposed to worry about. On the one hand I keep reading here alarming messages to the effect that string theorists are "putting the torch to physics as a science" and that "theoretical physics transforming itself into a beached whale" , etc. On the other I remember Peter writing that string theory (and LQG, to which all the same objections seem to apply but which rarely is criticised here) represent only a section of theoretical physics, with apparently decreasing influence. So how can these two things be true at the same time?

On the issue of string theory's lack of experimental predictions own naïve view at the moment is this. If we have two theories (GR and QFT, I think), which have together make a large number of confirmed experimental predictions but are mathematically inconsistent with each other then if we can construct a self-consistent theory (ST) which "in the limit" replicates GR and QM, than that constitutes scientific progress. The theory is "scientific" even if it does not make any confirmable predictions beyond those of GR and QFT since it inherits the predictions that they make and adds self-consistency. Hence the only thing that would worry me as far as ST is concerned are the two questions: 1) is it really fully self-consistent and 2) does it really give GR and QFT in the limits? So I would worry about the kind of criticism that Penrose discusses in "The Road to Reality" (finiteness or the question of stability of 10 dimensional space time) but not about the kind of things that are most often mentioned here.

As for the "multiverse" I would be inclined to follow the method of Sherlock Holmes (as described in "The Sign of the Four"): "when you have eliminated the impossible, whatever remains, however improbable, must be the truth". Of course to some extent this begs the question: what exactly do we mean by "impossible" in this context? Myself I am quite happy with the view that impossible means "mathematically impossible", which means that if it takes hidden extra dimensions and multiverse to construct the unified theory and if all the mathematical problems can be sorted out – than, however improbable, that is maybe not exactly "the truth" but the best we have got.

32. **Peter Woit**
August 16, 2013

lucretius,
You're leaving the topic of this posting, and trying to start a discussion on a range of topics about string theory and its associated hype which have been discussed ad nauseam here over the years, something I don't want to encourage. If it's not clear though, the on-topic problem being referred to (ok, with imprecise and inflammatory language like "putting the torch to physics", I should try and avoid this as Lubosian) is a straightforward one. As an idea about unification, string theory has been remarkably unsuccessful, and has ended up now forced into the corner of a specific very complex and completely unpredictive and untestable proposal (the string theory "landscape") that I claim is accurately characterized as pseudo-science designed to avoid admitting failure. Siegfried's article is an attempt to justify this, and I'm pointing out the problems with it.

The exact details of what the string landscape proposal is and why it is unpredictive are among the things that have been discussed ad nauseam here over the past 10 years since this proposal first surfaced. If you're skeptical about my claims and want to devote time to this, I'd suggest trying to find a counter-example: a testable prediction, or a plausible proposal for how to get one. Many claims of such things have been discussed here on the blog. You also might want to note that even Lubos doesn't believe in this (he thinks some new insight into string theory will make the landscape unnecessary).

If you're not interested in this, but just want to repeat hype about supposed wonderful properties of string theory and how it's the only way to go to get unification, so it doesn't matter if it's pseudo-science, because we have no choice, then I really can't help you, but do want to point out that you're arguing for giving up on this kind of science, torching it if you will. Maybe better understanding fundamental physics is a lost cause, one could have argued that at anytime in the earlier history of the subject. I don't think it was true then, or that it is true now.

33. **vmarko**
August 16, 2013

Lucretius,

String theory is much more ambitious than just unifying GR with QFT — it aims to be a "theory of everything" — deducing from first principles both GR and all of the Standard Model. As far as incorporating GR into QFT, string theory has been reasonably successful, and that is all good and well (at least up to a point). But as far as reproducing the SM, all string theory attempts have been a disaster — instead of a single version of SM, it "predicts" some 10^500 different versions of it (this is a colloquial way to phrase the so-called “landscape problem”).

So at this point string theorists were supposed to admit that ST simply fails to predict the SM, and that further work is required. Instead, they started arguing that actually all those 10^500 different “Standard Models” actually do exist in nature — the multiverse hypothesis — and that we are seeing this one particular SM in experiments (as opposed to any other) because our universe (our observable patch of the multiverse) hosts a version of the SM that supports life being created in it. This is called the “anthropic principle”.

The issue with this argument is the following — not only that it does not predict anything, but moreover it states that one should give up trying to predict anything. Just like religion (naive interpretations of it) can answer any question with “God did it”, string theory can invoke the anthropic principle to answer any question with “we happen to live in such a patch of the multiverse”. It has 10^500 “vacuua” (or free parameters, simply put), so you can fit basically any experimental data to this set of parameters. No predictions can be made in such a setup, only interpolations and data-fitting.

So, failing to predict the SM free parameters, string theorists should admit that ST is far too general and that further axioms/principles/whatever is needed to reduce the parameter space. But instead of doing that, they simply declare ST to be “correct”, and the SM problem unsolvable, by definition. And they get a lot of positive media attention, saying “string theory, via the multiverse and the anthropic principle, solved one of the biggest puzzles in science”, when in fact they didn’t solve anything — they *gave up* on the problem.

It even goes as far as teaching graduate students not to look for any explanation for the SM free parameters — string theory predicts that they cannot be explained and that no explanation is necessary, so why bother?

From a failed attempt to unify all forces of nature, string theory grew to become a fundamentalist religion of modern physics.

All this has nothing whatsoever to do with combining GR with QFT — that can be done with some level of success in ST, in LQG, and in various other approaches.

Best, 🙂
Marko

34. **Low Math, Meekly Interacting**
August 16, 2013

What strikes me as an incredible irony is that string theory was supposed to solve the problem of having to resort to anthropic reasoning. The multiverse concept (as I prefer to define it) was already quite mature in form eternal inflation. Some people were eyeing the possibility of a non-zero cosmological constant with abject terror. But once we found the principle that selected manifolds that gave us 3+1 extended dimensions, GR, and the Standard Model as a low-energy limit, we could dispense with pseudoscientific anthropic nonsense! Now the ST landscape is the foundation on which eternal inflation can stand. Nothing appears to remain of the notion of selecting anything, except maybe wistful nostalgia among those who don’t regard it now with disdain. What a terrible vision this has turned out to be.

35. **Amos Dettonville**
August 16, 2013

Lucretius, isn’t the landscape/multiverse idea inconsistent with what you described? It says there is no unique “limit” of string theory corresponding to the standard model and general relativity. Instead, there are a bazillion low energy limits, and we don’t actually know (and may never know) if any of them closely resemble the standard model. The hope is that some of them do, but in any case nearly all of them don’t. In this situation, it seems like a stretch to say string theory “inherits the predictions” of the standard model.

If what you described came about, i.e., if there was a unified conceptual framework that uniquely reduced to the standard model and general relativity in some suitable “limits” (as general relativity reduces to Newtonian theory in the weak slow limit), I would agree that it would be very interesting – although if it really makes no new confirmable predictions it might be more accurate to call it an *interpretation* rather than a new theory. If that interpretation sheds light on how the conceptual inconsistencies of the two pre-existing theories can be reconciled, that would be great.

I suppose string theorists believe there are enough qualitative similarities between the generic features of string theory solutions and the facts of the standard model and general relativity that we are warranted in claiming that string theory already constitutes a unified conceptual framework subsuming gravity and the standard model (not withstanding the fact that we don’t actually know – and may never know – if any solution of string theory actually matches the standard model quantitatively). But apparently many have concluded – some reluctantly and some enthusiastically – that a necessary ingredient of this conceptual framework is the landscape/multiverse, i.e., severe under-detemination of the specific attributes. With the landscape/multiverse, we might say the only prediction of string theory is that prediction is impossible – in the sense that there can never be any deeper explanation for the parameters of the standard model, because many different sets of parameters are equally compatible with the principles of string theory.

So in this sense I suppose we could say string theory would be falsified by the discovery of some new constraints or inter-relationships between the parameters of the standard model. On the other hand, maybe any new constraints would simply be taken on board, and the claim updated to say there are no *more* constraints to be discovered. All we’re doing is refining the attributes of one particular solution (the one that corresponds to our universe), while retaining the belief that it is compatible with the principles of string theory (which still seem to be in flux).

36. **DN**
August 16, 2013

“It’s not actually a scientific theory, but a pseudo-scientific construct designed to ‘explain’ why you can’t ever predict anything testable, directly or indirectly.”

Peter, enjoy reading your blog, opinions and the various developments you write about, even though I’m in a different field.

Maybe you’re trying to be funny and serious at the same time up there. I get it. Anyway, help explain “explain” a bit more. Thanks.

37. **Peter Woit**
August 16, 2013

DN,
To really see what’s going on here, you need to get into the very complicated subject of how to construct “string vacua”. At this point I think even most string theorists have given up on working on this, with the subject left in a state where you have so many possible choices that you can get whatever you want, and evade having any testable prediction that might conflict with experiment. The “multiverse” provides the argument that any “string vacuum” might show up. Normally scientific theories explain in one way or another something we observe, this theory explains why we can’t ever test it (too many possible string vacua, we could be in any of them).

38. **lucretius**
August 16, 2013

Indeed I forgot about the ST probably because I am now trying to understand better QFT (for purely mathematical reasons) and the rest of the ST is just physics to me 😉
I agree that the ideas of landscape/multiverse seem contrived and probably unnecessary (although my knowledge of this subject is very limited).

I don't see why anyone should be surprised today by the failure of physicists to arrive at a unique mathematical "theory of everything", particularly in view of the experience of other disciplines.

In mathematics, as everybody knows, Hilbert's naïve hope that a consistent system of axiom for the whole of mathematics could be found (and proved to be such) was destroyed by Goedel's two theorems, after Russel and Whitehead spent years trying to realise it (in volume 2 of Principia Mathematica they managed prove that 1+1 = 2).

In option pricing (in mathematical finance) there was a period when it appeared that Nirvana had been attained: in the Black-Scholes model of stock market prices the assumption of lack of arbitrage implied that there was a unique fair price of every option, and for the simplest ones explicit formulas could be given. In spite of Nobel prizes in economics, this turned out to be an optimistic illusion: we now know that real world markets are incomplete and there are infinitely many "fair" prices of options compatible with the given price of the stock and the non-arbitrage principle. Out of these infinitely many "consistent" prices (or price processes) the market somehow chooses one – but there is no known principle that determines it.

In microeconomics about 100 years of research into general equilibrium run into a brick wall in the form of the Sonnenschein–Mantel–Debreu theorem, which says that equilibrium is neither unique not stable.

In none of these cases the seemingly devastatingly negative result lead to people abandoning the entire field (or even basic directions) of research. In fact, in each case pragmatic reasons were found for continuing.

I don't believe that string theory actually needs the landscape and multiverse to survive. The indeterminacy is disappointing but there are ways to deal with it just as there are in all these cases I have mentioned.

Obviously the landscape and multiverse belong to the realm of speculation, even if dressed up in elegant mathematical form. In fact the title of Sigfried's article suggests this by his use the word "belief". Nothing in science can actually depend on a "belief". "Belief" can be very important in motivating someone to pursue a line or research that seems unpromising to others but to accuse critics of a scientific theory or idea not having enough "belief" makes one sound like an apologist for a religion not a science.
Hoever, I don't see any reason why Sigfried's obvious silliness should be taken as discrediting the rest of string theory and that is really all I wanted to say.

39. **paddy**
August 16, 2013

Lucretius,
Such a comparison of theoretical physics to so-called "economic theory" might (or should) shame those who have monetized the "multiverse".

40. **lucretius**
August 16, 2013

Peter, I am curious what kind of "discussion" you consider to be "on topic" in a thread like this one. I understand that you may not want to return to things that " have been discussed ad nauseam here over the past 10 years" (although I don't think you are seriously expecting new readers of your blog read through 10 years of past discussions before asking a question) but then what is "on topic" except for cries of "whatever next" and "what is the world coming to" and this sort of thing? . I asked one question, which at least had the virtue of being different from what other people wrote. I repeat it: how can string theorists be increasingly irrelevant to physics research and even having difficulties getting hired (both claims I have read on your blog while trying to catch up on the past discussions) and at the same time be "putting the torch to physics"? O.K. you have now disassociated yourself from this "Lubosian" language but you still have not answered my question. If string theory is indeed as unsuccessful as you claim, and in decline then what is the purpose of posting threads in which everybody is clearly expected to express the same "anti-string" view?

41. **vmarko**
August 16, 2013

Lucretius,

"I don't see why anyone should be surprised today by the failure of physicists to arrive at a unique mathematical "theory of everything", particularly in view of the experience of other disciplines."

It is not a surprise, of course. But do remember that (in light of Goedel's incompletelness theorems), a "theory of everything" in physics actually means "theory of everything so far", since every theory in physics can aim to describe only the so-far-known experimental facts. So we are well aware that any proposal for a "theory of everything" is necessarily incomplete from a conceptual point of view, but at the same time it should certainly be able to describe all previous established models in suitable limits.

Also, note that the Goedel's incompleteness theorems and the Sonnenschein–Mantel–Debreu theorem are actually **theorems** (such negative results in physics go by the name "no-go theorems", and there are many examples of such results). In contrast, the multiverse idea in ST and the anthropic principle are **axioms**, that deny one the possibility to give any furher explanation of the SM free parameters.

A lot of people simply refuse to accept such statements as valid axioms, precisely because of the limitations they impose. Even some string theorists are trying to steer clear of them. But there is this group of prominent string theorists who embrace them and advocate them — and they happen to be influential, vocal and widely present in the general media.

"Hoever, I don't see any reason why Sigfried's obvious silliness should be taken as discrediting the rest of string theory and that is really all I wanted to say."

Sigfried is by no means the only one nor the first one to display such "silliness". This "multiverse mania" (as Peter likes to call it) has been going on for quite some time now, and there is quite some amount of propaganda in the media about it. Just take a look at some past posts in Peter's blog, you'll find that this Sigfried's "silliness" is just the latest in a long series.

If other, more sensible string theorists would be as vocal against multiverse as its advocates are, the whole idea would be just one more controversial philosophical idea being debated, like so many others in history of science. But the problem is that those more sensible string theorists are keeping to themselves, while vocal advocates of multiverse are creating the illusion that there is general consensus about the topic.
This situation is what is discrediting string theory in particular, and threatens to discredit the whole theoretical high-energy physics in general, in the eyes of general non-hep public.

Best, 🙂
Marko

42. **Peter Woit**
August 16, 2013

lucretius,
The topic of the posting is the Siegfried piece, which, yes, I claim is arguing for putting a torch to physics by promoting pseudo-science. Some string theorists also argue for this, some don't. The ones who do I personally think are motivated by refusal to admit failure. As for the rest of string theory's problems, that's another topic.

As for insisting on people agreeing with me, I delete large numbers of comments posted here that criticize string theory but have nothing of interest to say. I very rarely delete comments from anyone who wants to disagree with me and argue for string theory. If your comments were arguing against string theory I'd probably have started deleting them on the grounds of off-topic/uninformed. Instead, here you are, admitting that you don't actually understand the issues, but dominating the discussion. Because I'm allowing this, sensible informed people who see this won't write in, just those who like to argue about the same old thing. You are doing everything you can to turn this comment thread into yet another tedious, uninformed and worthless set of string-war arguments, why?

43. **vmarko**
August 16, 2013

Also, while Peter of course has the last word in his blog about what is on/off-topic, I think that this discussion is certainly not off-topic. It certainly is a beating-a-dead-horse-discussion, yes, but off-topic, I'd say no.

🙂
Marko

44. **DrDave**
August 16, 2013

Siegfried:
"If you can't see it, it doesn't exist. That's an old philosophy, one that many scientists swallowed whole….After all, you can't see bacteria and viruses, but they can still kill you.
Yet some scientists still invoke that philosophy to deny the scientific status of all sorts of interesting things."

Actually, you can see bacteria and viruses with a scientific instrument called a microscope, and right there is the logic break: you can't test for a multiverse, or the tests don't show anything.
This is followed by a straw man (straw person, sorry) argument of "some scientists" invoking the philosophy of the unseen. This is not what scientists do; scientists use the scientific method. The whole introduction to the article is to recast any scientists who disagree with SUSY and multiverses as the work of provincial disbelievers, as opposed to the rigorous methods and brilliant experiments developed from years of training.

45. **Chris W.**
August 16, 2013

More succinctly, bacteria and viruses were killing us before we even had any clue they existed, and we now know enough about them to say a lot of testable things about precisely how and when they can kill us, as well as how to produce images of them with appropriately designed equipment.

46. **lucretius**
August 17, 2013

@Peter Woit:

Since you asked me a question I feel obliged to answer it. It was certainly never my intention to take part in the "string wars". As I wrote earlier, I intend to sit on the fence until I understand things well enough to make up my mind for myself. Yes, I freely admit I do not "understand" many of the issues involved, but my standards of what constitutes "understanding" seem to be higher than yours. One of the first things I learned from my supervisor when (a long time ago) I started working on my doctorate was never to claim to understand anything unless I can reproduce all the technical arguments myself, preferably in my own way, and in this sense I certainly do not understand string theory. I noticed, however, that when Matt asked you whether you understood the technical details of string theory in one of the recent threads on this blog you refused to answer and (as you often do) declared his question off topic. I have not read your book but judging by your publications available on the Interent I can't conclude that you apply the same standards to "understanding" as I do.

Your claim that because you are generous enough not to delete my ignorant posts "sensible informed people who see this won't write in" is, in the context of this particular thread, laughable. In fact, except for the replies to my posts by Marko, Amos Dettonville and a couple of other posts, the rest is information-free rhetorics.

So this brings me to your question:
"You are doing everything you can to turn this comment thread into yet another tedious, uninformed and worthless set of string-war arguments, why?"

Purely out of curiosity. I wanted to find out what this blog was: a physics discussion forum or a combat training ground for the "string wars". Lubos' blog, besides his rants (which, in spite of their abrasive language are often witty and entertaining except perhaps to their targets) contains large amounts of technical physics, skilfully elucidated. In spite of its obviously one sided view, it is by a long distance the best place on the web to learn about certain areas of modern physics. Nobody could say this about this blog. To stick to the military metaphor: Lubos is training highly skilled commandos for the string wars while you are training suicide bombers.

With best regards

Lucretius

47. **Jesper**
August 17, 2013

@ Lucretius

what a load of eloquent rubbish. The problem with string theory is not string theory itself but the total dominance in theoretical physics it has had in the past decades. Although it might now face less friendly winds this general problem is still very real. Peter's blog is a much needed vent for dissent. There is nothing suicide bombing about it, rather a forum for discussing what many feel are troubling trends in theoretical physics. I don't see anyone bombing anyone here?

And besides – in my views many of the unattractive sociological aspects of string theory are also found in other research directions in theoretical physics. Only, they are nowhere near as dominating as string theory.

With best regards,

Jesper

48. **Marty**
August 17, 2013

Lucretius,

I have very mixed feelings about commenting on your last post, mostly because I feel like I'm contributing to the drift of the discussion from the primary topic. However, since you've said you haven't followed this blog very long, hearing another perspective may be useful.

I watched the "string wars" play out on several blogs (e.g., this one, Asymptotia and Cosmic Variance) some years ago, and needless to say the discussions turned ugly at times. Unsurprisingly, Peter was usually involved in them in some form or another, primarily because he was unusual among string theory critics in that he had actually spent a fair amount of time learning about some key aspects of string theory. (Lee Smolin was another knowledgeable critic who occasionally took part; almost always he managed to keep his cool in the face of personal attacks.) Although Peter's level of knowledge was/is not at the level of a practicing string theorist, he had/has some technical familiarity with the topics he discussed. I think that earned him at least some respect among non-string-theorists — he didn't post on subjects where he had no knowledge, and didn't pretend to know something he didn't know in responding to others. Moreover, on numerous occasions over the years he has had this blog, when it was brought to his attention that he was wrong he would generally admit it, and when he was justifiably criticized for his demeanor in responding to personal attacks he would often agree that he could have handled it more gently.

Understandably, if you didn't follow those "string wars" discussions, some of which contained *hundreds* of comments, you might not appreciate how tedious it became when a majority of comments were posted by people who were technically uninformed but who didn't like (or did like, as the case might be) string theory for one reason or another, very often doing little more than parroting some point by Peter, Jacques Distler, Lubos or some other well-known critic or partisan — they said nothing new, but still felt that they should say something anyway. Many of the comments were personal and insulting, and for no good reason other than what appeared to be a very personal identification some people had with their own viewpoints. Practicing string theorists could also be insulting, which did nothing to polish the image of said theorists or their favorite theoretical framework.

Returning to your questions and posts, if you keep that history in mind, it is probably obvious that Peter has no interest in repeating that experience with a "fresh crop" of blog readers. It isn't anything personal — I really doubt he knows anything about you, so it *can't* be personal — but in your last post your tone seems pretty defensive as though you took his admonition as a personal attack on you or your questions. In fact, if you compare the change in tone in your last post with some of what transpired in "string wars" discussions, you might recognize a certain commonality: at some point, a string theory partisan would feel like their views weren't given the respect they felt was deserved and then dig in his/her heels, then resort to personal attacks on Peter's (for example) publication record, lack of "string theorist credentials", or other lame attempts to discredit Peter-the-person through bogus rationales, implicitly suggesting that by extension one should disregard the actual content of what he was saying. I'm not saying you are actually heading down that path, but with your most recent change of tone it is a possibility.

(Interestingly, you mentioned Lubos' blog as a training ground for "highly skilled commandos" and Peter's blog as a place for training "suicide bombers." That is such a surprising contrast that I can't help myself in commenting on it. There is no question that Lubos knows a lot of physics; he is definitely gifted that way, certainly in my opinion and undoubtedly in the opinion of many others, say at PhysicsStackExchange. Having said that, on his blog and comments on blogs of others he freely intermixes his own biases and opinions with actual physics knowledge (or at least he did before I stopped reading him much), but often won't tell you that is what he's doing. And my impression/observation, again and again, was that his response when someone pointed out that he had made an error was to argue uncompromisingly, or call the person a moron (or worse) because they obviously didn't know any physics or they would agree with him; but rarely would he admit he made a mistake. Another tactic he uses, or at least used in the past, was to delete posts that criticized the "story" he was trying to present. I stopped reading his stuff because I could no longer stand his frequent insults/intolerance of others who disagreed with him, the general low quality or sycophantic tone of comments readers made to many of his posts, and what often looked like an unwillingness to distinguish his opinions from fact. So in a sense I agree with half of what you said — his blog can act as a training ground for commandos — but only if you mean unquestioning jihadists for the string theory "cause" rather than "warriors" who do battle through rational and open minded discourse. Anyway, you apparently have a lot more patience for the ugly side of blogging than I do.)

49. **Nathalie**
August 17, 2013

Quite a few researchers are dissatisfied with their own (scientific) lives. They didn't manage to dethrone Albert E., as was their original plan when they went to physics. "Everyone" had told them that they are terribly smart. Consequently, these people suffer from internal aggressions. Let them fight over Multiverse and such harmless stuff. Who cares? Nobody gets physically hurt and that is much better than them physically beating their wives and children.

50. **Peter Woit**
August 17, 2013

Lucretius,

Thanks for the advice that I should run this blog more like Lubos's witty and entertaining one that you so much enjoy. The great thing about the internet is that there's a place for everyone. I think you've found your place and I'm sorry but I can't allow you anymore to try and change this one.

Others: please don't feed the troll.

51. **Experimental Physicist**
August 17, 2013

Peter and Lucretius

This is my first comment in any blog. I am SM experimentalist, I consider myself a layperson in BSM physics. Though I came to know about this blog very recently, I have read most of the earlier entries and comments. I read a lot of other blogs on different subjects too. The aspect of this blog I admire the most is the sensible comments from experts from different fields whether their arguments are either for or against Peter's. For instance, though Peter Shor and Matt Strassler may have different opinions, their arguments are worth admiring equally. There are a lot of other regular commentators in this blog whose insights sometimes outweighs than that of the main enetry. Thanks Peter for creating such a wonderful platform. Lucrtius, to me, correct scientific insights are far more important than wits.

Thanks
Exp Physicist

52. **Peter Woit**
August 17, 2013

EP,
Thanks! I should say that among the most rewarding aspects of running this blog is learning new things from and hearing the point of view of some of the commenters here, well-known (not always visible from the name they leave..) and not-so-well-known. It's not always easy though to try and keep this a place that sensible people

would want to participate in…

53. **george ellis**
August 18, 2013

In his previous post at The Curious Wavefunction, Ashutosh Jogalekar (commenting on whether psychology is a science or not) gives the following criteria

" The five basic requirements for a field to be considered scientifically rigorous [are] clearly defined terminology, quantifiability, highly controlled experimental conditions, reproducibility and, finally, predictability and testability."

Sounds good to me.

54. **srp**
August 18, 2013

Don't think you want to rule out the observational sciences–astronomy, meterology, oceanography, structural geology, etc.–because they can't do experiments.

55. **Maynard Handley**
August 18, 2013

"Don't think you want to rule out the observational sciences…"
Oh give that old canard a rest. The cloud chamber was invented precisely to study the phenomena that result in clouds, and then repurposed by physics. The whole point of the diamond anvil is try to get at the actual conditions of matter inside the earth.
And so on, and so on.

If I'm an oceanographer, yes, I observe the existing ocean currents. But I ALSO hypothesize that they are driven by density differences caused by salinity and/or temperature. I do experiments to see how these effects play out. I make predictions about where the ocean water will be above average in salinity or whatever, and test the predictions. I fantasize about what might happen to currents if a mass of fresh water were to hit a particular ocean, and then look in the historical record for any possible such events. etc etc

The issue is not experiments (something like a controlled environment run by the scientist), it is a feedback loop between observations and predictions. The observations can come from experiments, examining "nature", or the historical record; likewise a variety of predictions are possible. What matters is both observation (to keep one tethered to reality) AND predictions (so that one is approaching some sort of understanding, doing more than just stamp collecting).

56. **huhiho**
August 19, 2013

The issue is not experiments (something like a controlled environment run by the scientist), it is a feedback loop between observations and predictions. The observations can come from experiments, examining "nature", or the historical record; likewise a variety of predictions are possible. What matters is both observation (to keep one tethered to reality) AND predictions (so that one is approaching some sort of understanding, doing more than just stamp collecting).

57. **Neil**
August 19, 2013

The multiverse, along with the anthropic principle, in some sense "explains" fine tuning to me, and I like that. It is not traditional science, but it is not religion either. If, someday, fine tuning can be explained without these concepts, I will be happier.

58. **Lee Smolin**
August 20, 2013

Neil,

Cosmological natural selection can explain the fine tunings of the parameters of the standard model of particle physics without the anthropic principle. And it makes falsifiable predictions, for example that the upper mass limit of neutrons stars is at most two solar masses. This is so far consistent with observations but could be falsified at any time. This doesn't mean its right, but it means its a testable scientific hypothesis.

Thanks,

Lee

59. **Giotis**
August 20, 2013

Bottom line is that although other universes cannot be observed, even in principle, it may be indeed the case that there is a Multiverse out there. The thing is, are we clever enough to theoretically deduce its existence even if we can observe it?

I think it is possible if we have a strong belief in the corresponding theoretical framework which predicts it. Besides pure theoretical considerations we may gain confidence on the validity of the corpus of theories supporting the multiverse idea by circumstantial experimental evidence.

In any case the last thing we want to see is wasting decades of research trying to understand the values of parameters which indeed may have a pure environmental explanation. So the sooner we reach conclusions regarding the Multiverse idea the better. In that respect research should continue of course. Don't forget that Nature is full of surprises and you never what you may found just around the corner; either a dead end or a revelation, the only way you could find it out is by walking the path…

60. **Armin Nikkhah Shirazi**
August 20, 2013

Giotis,

I am struck by how the first part of your third paragraph seems to contradict the second part. You seem to consider "trying to understand the values of parameters" at least potentially a waste of time but then say "the only way you could find out is by walking the path". If this is correct then efforts to understand those values cannot be considered a waste of time as long as "walking the path" promises a resolution one way or another.
I think the unspoken assumption behind this incongruence is that you already deem research around the multiverse to be more promising than other approaches (as evidenced by the second paragraph and prior posts).
I think there is nothing inherently wrong with holding that assumption, the problems arise when it is used as a criterion for evaluating and comparing distinct approaches

to solving a problem without making it explicit that it has been used that way. If you are not aware of this, it will make it that much more difficult to understand the point of view of those who do not share your assumptions.

61. **Nathalie**
August 20, 2013

Let us assume that there are billions and billions of universes out there. After all, we already know that there are billions and billions of galaxies. Can any of you explain why humans on our tiny little planet should care about the number of Universes? Don't tell me that it's because we want to understand our own universe better. Such an argument doesn't make sense as our universe need not be a typical one. Of course, you could make further assumptions but who tells you that your assumptions are correct?

62. **Low Math, Meekly Interacting**
August 20, 2013

I'm also utterly confused by the notion that exploring the multiverse deductively could be superior, under any circumstances, to working on alternatives with observable consequences, or even just devoting more effort to increasing our confidence in current theory. I see no way to escape the fact that deductive reasoning about unobservables, no matter how well grounded in established theory, requires belief. And systematic, potentially interminable reliance on belief would be a radical, arguably self-negating paradigm shift in the way science is currently performed, at least at the level of an entire discipline. How can being correct even matter under such circumstances?

63. **fuzzy**
August 20, 2013

hi lee smolin,

i would like to ask you to elaborate your point concerning "falsifiable predictions" based upon "cosmological natural selection".

can you provide me with the proof of the upper bound you have cited? what are the uncertainties in the prediction? moreover, can you recognize or not a quark star with a neutron crust (that can be a rather heavy object) from a "true" neutron star, on observational bases?

thanks

64. **Lee Smolin**
August 20, 2013

Dear Fuzzy,

The argument is in many places beginning with my 1992 paper and my 1997 book. For a recent discussion with references see http://arxiv.org/abs/1202.3373.
The relevant calculations of the UML of a kaon condensate neutron star are not mine, they are discussed in James M. Lattimer, M. Prakash, What a Two Solar Mass Neutron Star Really Means. 4. arXiv:1012.3208 , to appear in Gerry Brown's Festschrift; Editor: Sabine Lee (World Scientific)

I haven't thought about how a quark star might be produced or distinguished observationally. Is there a plausible argument that one might be the remnant of a supernova?

65. **Tmark48**
August 20, 2013

> The multiverse, along with the anthropic principle, in some sense "explains" fine tuning to me, and I like that. It is not traditional science, but it is not religion either. If, someday, fine tuning can be explained without these concepts, I will be happier.

–
It explains jack shit. You say it is not traditional science implying that it is some kind of science. No, it is no science at all. I think scientific faculties should start doing a very heavy selection in terms of students that want to start a scientific career. Start explaining to them what the scientific method is all about. It seems an ever increasing percentage of those students (especially those that go into theoretical high energy physics or theoretical cosmology) suddenly forget this principle. So when are you going to discredit Galileo ?

66. **Neil**
August 20, 2013

Tmark48

I was careful to say that it "explains" it to me. If you think my standards of explanation are too low, fine.

The point is, there are levels of explanation. Dark matter is a good example. We do not know what it is, and have not isolated a single piece of it, yet it provides an "explanation" for things that cannot be explained otherwise.

It may be that the dark matter explanation is completely wrong and a better explanation will be discovered. Actually, I hope so. Likewise, and perhaps more likely, the multiverse will not be needed. For now, they are explanations.

67. **fuzzy**
August 22, 2013

dear lee smolin,

i understand that you have not exhibited a proof but an argument, however, i am sorry, but i fail to find it convincing. when you state: "as low as possible", you mean: "as low as possible compatibly with that we know now". thus, the borderline is set from what we know now, and in fact, it changed in the course of the time. for this reason, i think that the lower bound of 2 solar masses tests the correctness/completeness of our actual ideas of astrophysics and of nuclear physics, rather than fundamental physics.

(incidentally, concerning that, i am not sure that there is a very clear idea of the uncertainties; i do not know whether also rotation might somehow play a role; the role of quark matter remains largely unknown; but this is not the issue under discussion).

thanks for the references and the patience.

68. **CVP**
August 22, 2013

It seems pretty clear that putting large sums of money into terrestrial accelerators is a game that is about over. Let's put that money to work in looking at the rest of our universe and see what we can determine. Many of the puzzles seem to involve gravity in one way or another, and our universe has a lot of gravitational (as well as other) experiments underway. I think the HEP community is having a hard time accepting that their day in the sun, and their method of working, is coming to an end.

In terms of the debates about multiverse pursuits, I think theorists, including many on this blog, have a confusion about what the word "explain" should mean. Claiming that a universe is anthropically selected from an infinite ensemble of randomly produced unobservable universes is not really an explanation of anything. They are "just so" stories. Such a hypothesis can never be true or false in any useful definition of those words. Theorists need to think about those notions. If it turns out the universe has no free-parameter explanation we can determine – OK so be it – don't create a non-explanation explanation and try to shill it for funding.

69. **martibal**
August 22, 2013

A question maybe a bit off topic (I let Peter decide) but I take the occasion of this discussion to ask something I never understood about anthropic principle/multiverses: the anthropic principle states that the value of the fundamental constants are as they are, because otherwise we would not be here to ask why the constants have the value they have, right ? But have the simulations of these universes-with-other-values-of-the-fundamental-constants been pushed so far, that one can claim with certainty that – at no point of their history – no intelligence form (difference than ours) can emerge and wonder why the constants have the value they have ? Said differently: to what extend can one trust simulations of universes with different values of the fundamental constants, based solely on the (very approximative, cf e.g. dark matter) knowledge of our one single universe ?

70. **Peter Woit**
August 22, 2013

martibal,

I don't want to encourage a rehash here now of problems with anthropics, but, yes, one of the well-known problems is that it's not at all clear that if you change fundamental parameters and get very different chemistry and nuclear physics, there won't be some way for "intelligence" to emerge from this.

Then, of course, given pieces like Siegfried's, there's the question of whether intelligence actually did emerge in our particular universe….

71. **martibal**
August 22, 2013

Peter: thanks ! Your last remark sounds like a nice no-go theorem against anthropics 🙂

72. **Evert**
September 6, 2013

I am no scientist, just a very stupid layman interested in understanding "reality" (whatever that may be). While I agree that certain theories posed by the scientific community are questionable (like string theory or multiverses), I would like to think the scientific community could transcend the notion of "war" and create a field of study of its own related to these theories rather than dismiss them to philosophy or (god forbid) theology.
We need something that is at least in part based on scientific thinking which can at some point be either incorporated into science or debunked properly rather than throwing everything in the corner of the opposite of the spectrum of thinking.

Surely there must be some middle ground? If not for the "purity" of science at least for the greater good of human understanding of "reality".
Personally (remember I am no scientist) I always thought that it was a theoretical scientist's job to concoct (forgive the undignified term) theories based on probabilities of possibilities left open by our understanding of the universe, and that it is the practical scientists job to either confirm of debunk those theories. However, based on discussions and arguments on blogs and videos all over the internet I am inclined to think I was wrong and no matter the field of science, it seems to me it is more important trying to convince each other based on arguments long before we are capable of proving or disproving something practically. This whole war seems to me a war of wits rather than a constructive means to come to an answer?
By no means do I mean to insult anyone and I apologise if I offend in any way. But it seems to me it would be preferable to have a lot of silly and contradictory theories in my inbox waiting for a time in which they can be tested, rather than wasting energy in convincing each other to be right. That been said, I understand the need for discussion since it can lead to new insights. But at this point I feel a lot of discussions do not serve this purpose any more and are more subjective than objective.
Again, I do not want to start a war of my own and by no means is this meant as critique, it is simply my observation as an idiot.
In the end, what matters is how we all (the entire human race, intellectuals like you and idiots like me) understand reality and base our lives upon it. And while I accept that certain concepts will remain enigmatic to the greater public (like infinity for example, which deludes me to no end, despite analogies of hotel rooms) in the end we want some sort of straws we can grasp, and I for one would like those straws to be tangible (at least to some extent) rather than magical or biblical.
As such I understand and am a proponent of opposite theories in science, but I fail to see the advantage of them becoming heated arguments to the point of name-calling, rejecting them to the land of theology or worse.
Then again, I may either misunderstand or be a hopeless utopian.

Two Cultures

August 22, 2013
Categories: Uncategorized

There are two workshops going on this week that you can follow on video, getting a good idea of the latest discussions going on at two different ends of the spectrum of particle theory in the US today.

At the KITP in Santa Barbara there's Black Holes: Complementarity, Fuzz or Fire?. As far as I can tell, what's being discussed is the black hole information paradox reborn. It all started with Joe Polchinski and others last year arguing that the consensus that AdS/CFT had solved this problem was wrong. See Polchinski's talk for more of this argument from him.

If thinking about and discussing deep conceptual issues in physics without much in the way of mathematics is your cup of tea, this is for you (and so, I fear, not really for me). As a side benefit you get to argue about science-fiction scenarios of whether or not you'd get incinerated falling into a black hole, while throwing around the latest buzz-words: holography, entanglement, and quantum information. If you like trendy, and you don't like either deep mathematics or the nuts and bolts of the experimental side of science, it doesn't get much better than this. One place you can follow along the latest is John Preskill's Twitter feed.

Over on the other coast, at the opposite intellectual extreme of the field, LHC phenomenologists are meeting at the Simons Center this week at a SUSY, Exotics and Reaction to Confronting Higgs workshop. They're discussing very much those nuts and bolts, those of the current state of attempts to analyze LHC data for any signs of something other than the Standard Model. Matt Strassler is there, and he is providing summaries of the talks at his blog (see here and here) At this workshop, still no deep mathematics, but extremely serious engagement with experiment. One thing that's apparent is that this field of phenomenology has become a much more sober business than a few years ago, pre-LHC, and pre-no evidence for SUSY. Back then workshops like this featured enthusiastic presentations about all the wonderful new particles, forces and dimensions the LHC was likely to find, with one of the big problems being discussed the "LHC inverse problem" of how people were going to disentangle all the complex new physics the LHC would discover. Things have definitely changed.

One anomaly at the SEARCH workshop was Arkani-Hamed's talk on naturalness, which started off in a promising way as he said he would give a different talk than his recent ones, discussing various ideas about solving the naturalness problem (though they didn't work, but might be inspirational). An hour later he was deep into the same generalities and historical analogies about naturalness as in other talks, headed into 15 minutes of promotion of anthropics and the multiverse. He ended his trademark 90 minute one-hour talk with a 15 minute or so discussion of a couple failed ideas about naturalness, and for these I'll refer you to Matt here.

Arkani-Hamed and others then went into a panel discussion, with Patrick Meade introducing the panelists as having "different specialties, ranging from what we just heard to actually doing calculations and things like this."

Update: Scott Aaronson now has a blog posting about the KITP workshop here.

Update: A summary of the situation from John Preskill is here.

Comments

1. **MathPhys**
   August 23, 2013

   Who is Arkani-Hamed referring to at 37:50?

   *"And this is why very serious, sober theorists in the late 80's and early 90's were writing papers with super-partners discovered at LEP. They were **not** the people we won't talk about here who for 20 years were saying that supersymmetry is six months around the corner, like a translationally invariant statement. **Not** those people. Much more reasonable people."*

2. **Peter Woit**
   August 23, 2013

   MathPhys,

   I also was wondering who he had in mind. The only thing certain is that he wasn't including Gordon Kane, who surely was part of the "people we won't talk about here."

   Actually, I'm curious if there are any examples at all of theorists who in the 90s argued that naturalness meant SUSY at LEP, and then when it didn't show up, stopped promoting SUSY. All the examples I can think of just translated arguments for SUSY at LEP to arguments for SUSY at the LHC.

3. **CU Phil**
   August 23, 2013

   Hi Peter,

   I'm wondering if there's a reasonable interpretation of the first few split SUSY proposals of the early/mid 2000's that follows that line of thinking — we didn't find superpartners at LEP, so the naturalness argument for SUSY isn't a very good one, but coupling unification and the like are still compelling theoretical reasons to take it seriously.

   Of course, you might think the latter argument isn't very compelling, but I suspect it's people who continued to hang onto naturalness while allowing incrementally more fine-tuning to count as "natural" who are the people he'd be talking about. Allowing a few percent more fine-tuning everytime you don't see anything while continuing to take naturalness seriously would lead to the kind of "every 6 months" predictions he ridicules. The people willing to largely give up the naturalness argument in favor of something like split SUSY would then be the "serious theorists". The fact that he sometimes complains about people having spent far too much time and effort arguing over "how much tuning is acceptable" lends a little credibility to this.

   These wouldn't be people who gave up on SUSY after LEP, but at least people who gave up on the naturalness argument for SUSY after LEP.

4. **Peter Woit**
   August 23, 2013

   CU Phil,

Yes, that’ s one interpretation, with Arkani-Hamed giving himself as a main example. He’s right that nearly ten years ago he was arguing for anthropics, the string Landscape and non-natural split SUSY. For an example, see this talk at a Templeton-funded event on science and religion

http://www.aaas.org/spp/dser/events/archives/lectures/2005/02_Lecture_2005_0428.shtml

However, from what I remember of talks like that, a big emphasis was on his “sharp experimental predictions for physics at the Large Hadron Collider” which were about a very-long lived gluino.

Still, I’m curious if there are any examples of people who not only noticed that LEP killed the main argument for SUSY, but drew the conclusion that SUSY probably wasn’t correct, instead of just moving on to “non-natural” SUSY.

5. **emile**
August 23, 2013

I remember Guido Altarelli being very disappointed about not finding SUSY after the first LEP energy increase.

6. **Jeff McGowan**
August 23, 2013

Must say, never quite appreciated what Peter might mean when he talks about “without much in the way of mathematics.” Checked up on Mark van Raamsdonk’s talk, and as far as I could tell there wasn’t really any math at all. I mean he used some words, drew a few pictures, but wow. I mean 15 minutes into the talk, and he hasn’t even filled up two blackboards, and he’s writing BIG. Actually, that’s true 25 minutes in. At which point he has a couple of arrows indicating maps, and one tensor product sign. Is this typical? Do physicists just not like to waste chalk? Is there any actual content, or is he really just waving his hands and saying “wow, it’s cool, these things look like they might possibly be related and wouldn’t that be neat.”

7. **Scott Aaronson**
August 23, 2013

Hi from Santa Barbara. I’m at the KITP firewall workshop (the only non-physicist participant, I think), where I’m having a very nice time. In my own talk (which dealt with Harlow and Hayden’s work on the computational complexity of decoding the Hawking radiation), I took the opportunity to crack some jokes about the extreme level of handwaving that reigns here, and the airy unreality of some of the discussions.

It’s true that most of the talks have surprisingly little math in them (and, of course, zero input from any recent experiment): it’s mainly just conceptual arguments illustrated by simple cartoons. (Obviously, the cartoons convey vastly more information to Susskind, Maldacena, and the like than they do to me — but they do look funny to an outsider.)

At the same time, you (Peter) have often complained that particle theory has been dominated for ~30 years by a few ideas that haven’t worked out so well. Here, at least, there’s an enormous diversity of ideas on the table, and lots of tolerance (and encouragement) of dissent. And if you think *all* the ideas being discussed are bad ones, then there’s the obvious retort, as John Preskill pointed out in his talk: “OK, let’s hear your better ideas!”

As I understand it, the issue is actually pretty simple. Do you agree that
(1) the Hawking evaporation process should be unitary, and
(2) the laws of physics should describe the experiences of an infalling observer, not just those of an observer who stays outside the horizon?
If so, then you seem forced to accept
(3) the interior degrees of freedom should just be some sort of scrambled re-encoding of the exterior degrees, rather than living in a separate subfactor of Hilbert space (since otherwise we’d violate unitary).
But then we get
(4) by applying some suitable unitary transformation to the Hawking radiation of an old enough black hole *before* you jump into it, you ought to be able, in principle, to completely modify what you experience when you *do* jump in—an apparent gross violation of locality.

So, there are a few options: you could reject either (1) or (2). You could bite the bullet and accept (4). You could say that the “experience of an infalling observer” should just be to die immediately at the horizon (firewalls). You could argue that for some reason (e.g., gravitational backreaction, or computational complexity), the unitary transformations required in (4) are impossible to implement even in principle. Or you could go the “Lubosian route,” and simply assert that the lack of any real difficulty is so obvious that, if you admit to being confused, then that just proves you’re an idiot. (Yes, I did see your comment about Lubos’s dismissal of the issue making you think there might be something to it after all!) AdS/CFT is clearly relevant, but as Polchinski pointed out, it does surprisingly little to solve the problem.

At any rate, thinking about the “Hawking radiation decoding problem” already led me to some very nice questions in quantum computing theory, which remain interesting even if you remove the black hole motivation entirely. And that helped convince me that something new and worthwhile might indeed come out of this business, *despite* how much fun it is. (Hopefully whatever *does* come out won’t be as garbled as Hawking radiation.)

8. **piscator**
August 23, 2013

I find this a bit funny, because Arkani-Hamed is the one person more than anyone else in the field I would pick out as guilty of making regular claims about imminent spectacular signals of new physics – mm-size extra dimensions, stopped gluinos, lepton jets, GeV-scale dark forces…how is split susy not a version of ‘susy is just round the corner’?

9. **wolfgang**
August 23, 2013

@Scott

Since super string theory is indeed the theory of everything, and in particular the only theory which predicted gravity, it will tell us any minute now the correct answer …

Btw I for one would go with “the unitary transformations required in (4) are impossible to implement even in principle” …

10. **Anonyrat**
August 23, 2013

Or a Motl-approved paper (“not self-evidently wrong”)
http://arxiv.org/abs/1308.4121

> “by Nomura, Varela, and Weinberg, three physicists who were previously pointing out that the black hole firewall arguments were flawed because they didn’t treat the superpositions of macroscopically distinct states of black holes correctly, among related “interpretational” flaws.

Today, they present an explicit qualitative model of the black hole microstates that is compatible with the unitarity, the locality at long distances, and the equivalence principle. The firewalls are absent and a smooth horizon is present at all times with the probability 100%."

11. **DimReg**
August 23, 2013

Scott,

What you described seems to be the EPR "paradox" in a fancy setting, unless I'm not understanding something. You could, for example, have two entangled electrons on opposite sides of a lab. You measure the spin of one of them, then walk across the room and measure the spin of the other, (which you already know the result of because you measured the first spin). In this case, your first measurement has "altered" the second, and you did so non-locally. I don't really see how that would be different than what you just described, so my feeling is that there is something important about the event horizon that you haven't mentioned.

(To be fair, the EPR paradox was trying to show that QM implied faster than light influences, not non-local ones, but the resolution seems to be the same: the first measurement only allows you to predict the second measurement, not change it)

12. **Scott Aaronson**
August 23, 2013

DimReg: No, it's completely different from EPR. EPR is about entanglement, which (as you correctly point out) cannot be used for faster-than-light signalling. By contrast, if you believe in black hole complementarity, then you don't believe that the interior degrees of freedom are entangled with the exterior ones: you believe they're the SAME degrees of freedom! So in particular, you should in principle be able to signal "nonlocally" from the interior of the hole to the exterior or vice versa. And that seems to be the source of the problem.

Anonyrat: So, which part of the argument I tried to summarize does that paper reject?

Peter: I can take these discussions to my own blog if you consider them off-topic…

13. **Peter Woit**
August 23, 2013

Scott,

This post wasn't intended as a criticism of the KITP workshop, or this area of research in general, or at all a claim that these are bad ideas being discussed. The world is full of all sorts of interesting and valuable things being done that are "not my cup of tea". All I was doing was explaining part of why personally I haven't been willing to put the time into thinking about such things and being able to comment knowledgeably on them (the other part is the same generic reasons my interest in all QG research is limited). I've seriously got no idea how useful the insights about quantum gravity coming out of thinking along these lines might be. One definite positive thing one can say is it seems to be keeping some of the participants busy who otherwise would be out putting the torch to science (i.e., promoting the multiverse).

Thanks for the very lucid summary of the paradox. I'm happy to have you answer questions from people about this here if you want to, that's on-topic and not something I'm equipped to do. If you find you'd rather use your blog for this, that's fine, will redirect traffic over there.

14. **Igor Khavkine**
August 23, 2013

@Scott,

The question about the physics at the horizon can be asked in two ways: one way for the hypothetical universe where AdS/CFT is true and describes black holes, and the other way for our universe with the laws of physics that we already know and have tested. For obvious reasons, the second version is more interesting and important. The no-nonsense answer there is to reject (1) and get a peaceful in-fall through the horizon and await eventual spaghettification near the singularity.

By now, the case for this point of view will have already been made, I'm sure, in the talks by Bill Unruh and Bob Wald.

15. **Michael Welford**
August 23, 2013

I've been viewing to Hawking podcast from fuzzfire and the sound quality is TERRIBLE. I have just enough time to wonder aloud whether the other skeptical voice (Unrah) will subject to similar distortion.

16. **Peter Woit**
August 23, 2013

Michael Welford,
I took a look and also soon gave up trying to listen to that. Perhaps a transcription could be arranged…

17. **Peter Shor**
August 23, 2013

@Igor Khavkine:

But if you admit that (1) is the correct answer and black hole evaporation is not unitary, you've just refuted one of the main contributions of AdS/CFT, the crown jewel of string theory. Who wants to do that?

18. **Scott Aaronson**
August 23, 2013

Igor Khavkine:

Yes, Bill Unruh just gave a talk today where he advocated rejecting (1) *extremely* forcefully and entertainingly! (Unfortunately I fly back today and will miss Bob Wald's talk.)

But while I don't understand some of the assumptions bandied about at this workshop (especially those coming from QFT), I *do* understand how central unitarity is to physics' current conception of the world, and what a drastic step it would be to get rid of it. So AdS/CFT or no AdS/CFT, string theory or no string theory, I certainly understand not wanting to give up on unitarity without an extremely hard fight.

(Incidentally, in my summary of the AMPS paradox, I forgot to say something. As I understand it, what AMPS added to the simple logical argument that I outlined was really to make the consequence (4) more "concrete" and "vivid"—by describing something that, in principle, you could actually do to the Hawking radiation before jumping in, such that after you jumped in, if there *wasn't* anything dramatic that happened—something violating local QFT and the equivalence principle—then you'd apparently observe a violation of the monogamy of entanglement, a basic principle of quantum mechanics. Probably the bare logic (1)-(4) was known to many people before AMPS. I certainly knew it, but I didn't call it a "paradox," I just called it "I don't understand black hole complementarity." 🙂)

19. **brian**
August 23, 2013

Any women at KITP besides Eva?

20. **Scott Aaronson**
August 23, 2013

There were maybe 4 or 5 other female students and researchers.

21. **Lee Smolin**
August 23, 2013

Dear Peter and Scott,

The argument Scott summarizes requires that the evolution be unitary as seen by an observer at infinity.

\\

The view that 1) -so modified- is wrong has a straightforward justification consistent with everything we know about both general relativity and quantum theory. This is the hypothesis that quantum gravity effects eliminate the singularity and so quantum evolution proceeds to a non-classical region of spacetime to the future of where the singularity would have been. The evolution as observed by an observer at infinity will not be unitary if that new region doesn't reconnect with infinity, but no principle of quantum mechanics is violated. The evolution as a whole can be unitary even if there is no observer who can reconstruct a pure quantum state from their observations.

\\

This commonsense solution has been discussed since the 1970's. There were some papers raising issues about remnants but as we discussed in detail with Hossenfelder in our paper arXiv:0901.3156, those are not convincing.

\\

The lesson in my opinion is that the key issue in quantum black holes and the information problem is not at the horizon, it is at the singularity. It is unreasonable to expect any new physics at horizons where the curvatures are small, but necessary to find new physics at the approach to singularities. The focus on the firewall problem is in my view a consequence of insufficient appreciation of this point. It can be seen as a reducto for the assumption that the problem can be resolved without investigating how quantum gravity effects eliminate the singularity and taking on board the consequences of the resulting evolution to the future of where the classical singularity would have been.

\\

Thanks,

Lee

22. **Anonyrat**
August 23, 2013

Scott Aaronson,

They say quantum states for the blackhole with firewalls exist but are generally not accessible by external observers. Specifically they say in their model:

> • The evolution of a black hole does not dynamically develop a firewall (even after the Page time). An infalling observer who does not perform a special manipulation to his/her environment always sees a smooth horizon.
>
> • It is not possible for an observer to see a firewall even if he/she performs a very special measurement on Hawking radiation emitted earlier from the black hole. Such a measurement cannot change the fact that he/she will see a smooth horizon.
>
> • If a falling observer can directly measure a mode entangled with the stretched horizon as he/she falls through the horizon, then he/she may see a firewall. This, however, does not violate the equivalence principle; the same can occur at any surface
> in a low curvature region.

So I guess they are saying that (4) is mistaken.

23. **Peter Shor**
August 23, 2013

Lee:

As I understand it, the problem with assuming that all evolution is unitary (considering both the interior and the exterior of the black hole) is that from the point of view of an outside observer, nothing ever falls into the black hole, so from this observer's viewpoint, evolution over the entire universe (which just consists of the exterior of the black hole) is not unitary.

24. **Bob**
August 23, 2013

Scott:

> DimReg: No, it's completely different from EPR. EPR is about entanglement

I wouldn't dismiss DimReg's comment so quickly. The ER=EPR conjecture of Maldacena and Susskind (arXiv:1306.0533) is an attempt to resolve the firewall paradox by making something like DimReg's comment precise. The starting point is the thermofield double formalism of Israel (1976). One introduces a second copy of the physical Fock space. The two copies are then related to the black hole interior and exterior, and they are entangled in just the right way to be consistent with complementarity and black hole thermodynamics. We make peace with the apparent non-locality of:

> (4) by applying some suitable unitary transformation to the Hawking radiation of an old enough black hole before you jump into it, you ought to be able, in principle, to completely modify what you experience when you do jump in—an apparent gross violation of locality.

because the Hawking radiation you acted on was EPR entangled with the black hole interior.

25. **Igor Khavkine**
August 23, 2013

@Peter Shor: 😉

@Scott: If everything in the world was unitary, there would be no place for things like the Lindblad equation. Similarly, for the same reason that a room with an open window is allowed to violate unitarity, so is the exterior of a black hole at intermediate times after its formation. On the other hand, at very long times (where "complete evaporation" is supposed to take place), there is nothing but a HUGE question mark. And a question mark does not a paradox make. Lee has made a similar point above.

26. **DimReg**
August 23, 2013

Scott,

Thanks for clarifying, that makes more sense! For some reason, I got the impression that it was all about entangling internal and external states.

27. **Chris Cesare**
August 23, 2013

@Igor: The Lindblad equation does describe non-unitary evolution of a quantum state, but this non-unitarity is not fundamental. As I understand things, it typically arises because you trace out the Hilbert space of an environment (like some thermal bath) which interacts with your system of interest and that you don't have a way of measuring precisely in practice. In principle, you could include the evolution of the bath in your description and recover unitary evolution. There is currently no reason to expect that this "in principle" statement is not correct, and I think this is what Scott was referring to in his reply.

28. **Scott Aaronson**
August 23, 2013

Lee Smolin: Thanks for the comment. I agree that baby universes are an option, although I can't comment on the merits of specific scenarios. (Unruh mentioned baby universes in his talk as another alternative to information loss, though he didn't dwell on them.) Preskill also made the point that, with all this fuss about the horizon, strikingly little was being said about the singularity—in order to motivate his and Seth Lloyd's proposal (building on Horowitz and Maldacena's), which *does* involve the singularity.

While I'm obviously far from an expert, where I think I part ways from you and Unruh is on the following. We're pretty sure black holes have an *entropy*, which goes like the area of the event horizon in Planck units. We're pretty sure that, from an external observer's perspective, infalling stuff gets "pancaked" on the event horizon and scrambled beyond recognition, never making it through to the interior. Finally, we're pretty sure that the external observer ultimately sees the black hole evaporate, through Hawking radiation that emerges (appears to emerge?) from the horizon. To me, these facts would seem like an intolerable coincidence, if the black hole didn't have microstates—"stored," one wants to imagine, on or near the event horizon—and if the Hawking radiation didn't carry away the information about those microstates. Otherwise, what a waste for Nature to "come so close" to upholding unitarity, only to chicken out at the last moment! 🙂

29. **Igor Khavkine**
August 24, 2013

@Chris Cesare, you have elaborated my implicit point about the Lindblad equation. For a black hole, the role of the "environment" or "bath" or even more precisely "whatever degrees of freedom are neglected" is exactly played by the interior of the black hole.

@Scott, as a frequent voice of reason in a ocean of doofocity, I hope you re-examine the certainty with which two of the statements from your last post are held: "the external observer **ultimately** sees the black hole evaporate" and "black holes have an **entropy**" (where "entropy" is specifically used in the sense of log(Omega)). Snag… I have to run and can't expand on this at the moment! I mention only that anyone interested in black hole physics should personally critically examine the arguments for how these claims have been arrived at.

30. **Greg Egan**
August 24, 2013

Scott wrote:

> We're pretty sure that, from an external observer's perspective, infalling stuff gets "pancaked" on the event horizon and scrambled beyond recognition, never making it through to the interior.

This account isn't just observer-dependent, it's *coordinate*-dependent! There are plenty of respectable coordinate systems that an external observer can use in which it's false.

In contrast to the "pancake" picture, it's an objective fact (proof here) that if a stationary external observer drops something into a black hole, after a finite amount of proper time has elapsed for that observer, it will be physically impossible for them to chase after the infalling matter and catch it before it crosses the horizon. (Ditto for the infalling matter reaching the singularity — even if you're feeling suicidal, once you drop something you have a finite time to go after it, before you lose any chance of ever touching it again, even inside the hole.)

About the strongest objective statement I'm aware of that favours the "pancake" picture is the fact that — in the idealised case where you can detect arbitrarily red-shifted light — there is no upper bound on how long it might be before a stationary external observer receives radiation emitted from a given piece of infalling matter. But the red shift factor grows exponentially, so I'm not sure that this counts for much, even in principle.

31. **Lee Smolin**
August 24, 2013

Dear Scott,

Thanks, but either I don't understand your argument or else it is circular. What do you suppose happens to the singularity as well as to the quantum state of the star whose collapse formed the black hole in the first place? If the singularity is eliminated then the Hilbert space in the future is a direct product of a factor spanned by observables which describe degrees of freedom to the future of where the singularity would have been and a factor spanned by observables external to the horizon. The evolution onto this product can be assumed to be unitary but (I feel silly telling you this) it cannot be when restricted to either of its factors. Hence the observer at infinity describes a density matrix gotten by tracing out the degrees of freedom in the baby universe inaccessible to them.

Isn't this a completely reasonable option, especially because it avoids the otherwise paradoxical implications of the firewall argument?

The pancake is a non-sequitor: why does it matter what information does or doesn't get to infinity or when, if infinity is not the only place information goes to? So to refer to it seems to assume what you are claiming to demonstrate.

Many thanks,

Lee

32. **wolfgang**
August 24, 2013

@Lee

as your wrote, your proposal is not new and thus its problems are known.
One problem of simply allowing a non-unitary evolution in the exterior is that
particle physics becomes non-unitary as soon as you (have to) include virtual black holes.

33. **Lee Smolin**
August 24, 2013

Wolfgang,

That is not a convincing argument and it is partly addressed in the paper I mentioned. The basic point is that there is no reason one has to include contributions from "virtual black holes." When one looks at it carefully it becomes not at all clear what would be meant by that in a well defined background independent formulation of quantum gravity. The intuition that any process should have large or even divergent contributions from "virtual black holes" is based on an incorrect use of effective field theory, as discussed in section 4 of the paper with Hossenfelder I mentioned above.

Another reason is that there is no reason to think that horizons make sufficient sense in terms of quantum geometry at the Planck scale to give meaning to the semiclassical intuition of a virtual or Plank scale black hole. If quantum geometry is discrete at Planck scales then there are no horizons, curvatures or singularities at those scales and no way to give meaning to a Planck scale black hole. There is no contradiction in believing that quantum gravity is simply unitary at small scales while real astrophysical black holes create baby universes.

Thanks,

Lee

34. **Peter Shor**
August 24, 2013

Wolfgang:

What is anything fundamentally wrong with including virtual black holes, and making particle physics non-unitary? While it is much more difficult to think about, and nobody seems to have a specific satisfactory theory of how non-unitary particle physics might work, I don't think there is any reason to believe such theories couldn't exist.
Particle physics could be non-unitary at the Planck scale, and have some built-in quantum error correction properties which keep it unitary at observable scales. For a two-dimensional condensed matter model for something like this, Google "Kitaev honeycomb model".

35. **Peter Shor**
August 24, 2013

Lee:

Your quote "there is no contradiction in believing that quantum gravity is simply unitary at small scales while real astrophysical black holes create baby universes." is reminiscent of the early quantum physicists' division of physics into the "microscopic", where quantum effects are important, and the "macroscopic", where classical physics holds. We now realize that this was a big mistake.

Regards,

Peter

36. **geromes**
August 24, 2013

My guess is that the firewall problem is due to an idealization. A lot of fuzz is made about the role of the BH singularity and physics at infinity – both I regard as just unphysical.

My suggestion: One should think about an appropriate analogue model realizable in the laboratory. There have been interesting suggestions to measure the Hawking effect in black hole analogues in Bose-Einstein condensates, for instance. There should be no violations of unitarity in such systems, right ? It would be interesting to see a firewall analogue in a Bose-Einstein condensate :-). Googling suggests to me that such models have not been worked out yet. Oh, by the way, such lab systems are full of surprises, e. g. bosenovas.

37. **wolfgang**
August 24, 2013

@Peter Shor

>> I don't think there is any reason to believe such theories couldn't exist
sure, but until it is written down and works, I prefer to go with the findings of Scott and Harlow-Hayden that Alice cannot actually perform the calculation necessary to create a paradox …

38. **Bill**
August 24, 2013

Peter, I think you would be interested in reading this post by a world class mathematician (anonymous, but so I am told) on the interaction between string theory and mathematics. I wonder if your assessment agrees with his?

39. **Peter Woit**
August 24, 2013

Bill,
I'm generally sympathetic with the views of that blogger, whoever he or she might be. But that's a large and complicated topic completely unrelated to this posting. Anyone who wants to discuss it should do it over at that blog.

40. **Jim Graber**
August 24, 2013

To reduce it to a slogan: Give up Black Holes, Save Unitarity
Baby Universes were mentioned, but I think the idea of replacing black holes deserves much more serious study than it has received. The problems discussed at the recent "Black Holes Complementarity Fuzz or Fire?" workshop emphasize only some of the serious problems with the black hole concept. I realize the workshop was predicated on acceptance of the black hole concept as well as the general accuracy of the Hawking radiation paradigm, but neither of these ideas has a strong experimental support. Gravitationally collapsed objects similar but not identical to black holes could alleviate many issues if they were unitary and did not involve either horizons or singularities.

41. **Scott Aaronson**
August 24, 2013

Jim Graber:

"I realize the workshop was predicated on acceptance of the black hole concept … Gravitationally collapsed objects similar but not identical to black holes could alleviate many issues if they were unitary and did not involve either horizons or singularities"

Actually, several speakers (including Stephen Hawking, who's been saying similar things since he conceded the information loss bet in 2004; and Samir Mathur and several other "fuzzball" people) explicitly advocated replacing black holes by some kind of unitary "black-hole-like object." Though fwiw, my preference would be simply to DEFINE "black hole" to mean "that entity, whatever it is, that behaves from the outside more-or-less like the black hole of classical GR."

42. **Peter Shor**
August 25, 2013

@Wolfgang: my characterization of what Harlow-Hayden is saying is that "Nature is inconsistent, but because we only have limited computational power, we will never be able to catch Her in an inconsistency, so we might as well pretend that She's inconsistent". I really don't think this idea holds up to close inspection.

43. **Peter Shor**
August 25, 2013

That last "inconsistent" should be "consistent".

Of course, this is essentially what Bohr's Complementarity Principle said before people worked out the mathematical justification behind it. But there is something fundamentally different about hiding "inconsistencies" with the Uncertainty Principle and hiding them with computational complexity.

44. **wolfgang**
August 25, 2013

@Peter Shor
>> there is something fundamentally different about hiding "inconsistencies" with the Uncertainty Principle and hiding them with computational complexity

I see it the other way around: One reason I found Scott's talk so amazing is because it suddenly hit me how fundamental computational complexity really is.
Of course, Scott was preaching this on his blog for long time …

45. **Stephen**
August 25, 2013

Apologies – this is wholly off-topic.
Does anyone know if the "unification" of the couplings in SUSY still holds in view of the latest LHC results ? I've been hunting around for some time now and couldn't find anything. Also, how unique are the "unification" solutions i.e. are they a generic feature of SUSY at a mass of X TeV ?

Sorry for going off topic… I shan't post on this again. Hopefully the question will anyway be of use to a lot of people reading this blog.

46. **Eric**
August 25, 2013

Dear Stephen,

Yes, the gauge coupling unification of the MSSM still holds after the latest LHC results. In fact, the gauge coupling unification works better for heavy squarks and sleptons in the 5-10 TeV range.

47. **Stephen**
August 25, 2013

Hi Eric,
Thanks.

Is there a ref for this ? (Apologies to Peter – no more posts from me on this).

48. **Peter Woit**
August 25, 2013

Stephen,
One recent reference is this
http://arxiv.org/abs/1212.6971

When thinking about whether different versions of the MSSM do a better or worse job of coupling constant unification, you probably should remember that you're talking about a theory with more than a hundred undetermined parameters to play with….

49. **Scott Aaronson**
August 25, 2013

Peter Shor:

> Nature is inconsistent, but because we only have limited computational power, we
> will never be able to catch Her in an inconsistency

No, I don't think that's what Harlow and Hayden are saying at all. It's more like: "yes, semiclassical field theory might have to break down and get replaced by a consistent quantum theory of gravity, even in a low-energy regime where physicists thought that QFT would work fine. But if the breakdown would take something like ~2^10^60 years to reveal, then maybe we don't have to worry all that much, if our goal is to reassure ourselves that we already more-or-less understood what happened at low energies."

50. **Scott Aaronson**
August 25, 2013

Lee Smolin and Greg Egan: Thanks for the comments and clarifications.

When I referred to "pancaking" (my choice of image, and maybe a bad one), I mostly had in mind a series of lectures that Lenny Susskind gave at PI, where he described the process by which quantum information is believed to get rapidly "scrambled" at or near the horizon, even giving detailed quantitative bounds on the rate at which the scrambling is thought to take place. So my impression was that, even independent of AdS/CFT and so forth, we knew something about the "rapid mixing" that a faraway observer would believe to take place, just because of the extreme temperature in the stationary frame at the stretched horizon, or something like that. If so, then the argument wouldn't be circular, since nothing in it would presuppose that the information ultimately comes out, but it would nevertheless support the intuition that it does come out. But maybe I took Lenny's calculations about the mixing near the horizon to be better-established than they are.

I forgot to mention an additional reason why I'd personally be happy if the information comes out of the hole, rather than into a baby universe. Namely, I would like the laws of physics to uphold the holographic entropy bound, that the total number of qubits in any bounded region should be upper-bounded by the region's surface area in Planck units. But if the interior of the region could contain a "portal" to another universe of unbounded size, then isn't the universality of that bound called into question?

So, here's a question for either or both of you. Suppose that, for whatever reasons, I thought that upholding unitarity was a *very* big deal—maybe an order of magnitude more important than any other principle at stake in the black hole debate. And suppose that by "unitarity," I meant that an observer in *our* universe should in principle be able to reconstruct the infalling qubits (even if after 2^10^60 years or whatever), rather than that the qubits should continue to exist in a baby universe. Then my question is: could LQG (or spin foams, or other non-string quantum gravity approaches) give me what I wanted? Or do you regard the information's falling into baby universes as essentially a *prediction* of LQG?

51. **wolfgang**
August 25, 2013

@Scott

>> if the breakdown would take something like ~2^10^60 years to reveal

maybe I misunderstood the Harlow-Hayden paper, but I thought the main point was that Alice can (most likely) not complete the calculation before jumping into the black hole (and thus create a contradiction) – however, Charlie , remaining (infinitely long) outside the b.h. finds no issue with unitarity.
This seems different to me than what you just wrote.

52. **Eric**
August 25, 2013

Peter W,

In regards to your statement about gauge coupling unification, this happens generically within a 1-3% so long as the superpartners are not too heavy. This not very sensitive to the number of parameters in the MSSM. The 1-3% percent discrepancy is usually attributed to unknown GUT threshold corrections, although the discrepancy does appear to be smaller for heavier squarks. For example, see http://cds.cern.ch/record/478820/files/0011356.pdf

53. **Marcus**
August 25, 2013

Scott, there was what I think is a fairly important LQG paper in Physical Review Letters 110, 211301 (2013) by R. Gambini and J. Pullin. I've only seen the version they posted in February on arxiv.
Pullin presented the paper in July at the GR20 conference. It was the lead item of the main Loops session. Ashtekar spoke in a GR20 joint session with string and pheno people (Bob Wald, Don Marolf and Gary Horowitz also took part) which was specifically about BH evaporation and the same stuff as the KITP conference. In doing so Ashtekar drew on this type of fairly unambiguous Loop BH result.
http://arxiv.org/abs/1302.5265
Loop quantization of the Schwarzschild black hole
Rodolfo Gambini, Jorge Pullin
(Submitted on 21 Feb 2013 (v1), last revised 10 May 2013 (this version, v2))
We quantize spherically symmetric vacuum gravity without gauge fixing the diffeomorphism constraint. Through a rescaling, we make the algebra of Hamiltonian constraints Abelian and therefore the constraint algebra is a true Lie algebra. This allows the completion of the Dirac quantization procedure using loop quantum gravity techniques. We can construct explicitly the exact solutions of the physical Hilbert space annihilated by all constraints. New observables living in the bulk appear at the quantum level (analogous to spin in quantum mechanics) that are not present at the classical level and are associated with the discrete nature of the spin network states of

loop quantum gravity. The resulting quantum space-times resolve the singularity present in the classical theory inside black holes.
4 pages, Revtex, version to appear in Physical Review Letters
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Here is the abstract of Ashtekar's July 2013 talk at GR20, in the special joint session on quantum mechanics of BH evaporation
ABHAY ASHTEKAR (20+5 MINUTES)
TITLE: Quantum Space-times and Unitarity of BH evaporation

There is growing evidence that, because of the singularity resolution, quantum space-times can be vastly larger than what classical general relativity would lead us to believe. We review arguments that, thanks to this enlargement, unitarity is restored in the evaporation of black holes. In contrast to ADS/CFT, these arguments deal with the evaporation process directly in the physical space-time.
==endquote==
You asked Lee and Greg if they regarded info into new expanding region as essentially a prediction of loop/spinfoam. Hopefully they will answer or you can figure out from what i just quoted. As nonexpert I would say YES essentially a prediction. AdS/CFT cannot be right if it sticks with a single asymptotic region ("boundary") while a hole develops in bulk. That is top.change. In Loop it leads to bounce and new expanding region which would have its OWN asymptotic region.
Therefore boundary acquires new component and boundary observables algebra must be enlarged. "Firewall" kerfluffle is probably just nature warning us that boundary has been enlarged (by baby universe) and only a part of info is coming back out to us in Hwkg radiation. My best guess as non-expert.

54. **Marcus**
August 25, 2013

Here is abstract of Jorge Pullin's talk at the main Loop session of GR20. It was the first paper of the session. He presented the February Gambini Pullin result.

Complete quantization of vacuum spherically symmetric gravity
Pullin J
We find a rescaling of the Hamiltonian constraint for vacuum spherically symmetric gravity that makes the constraint algebra a true Lie algebra. We can implement the Dirac quantization procedure finding in closed form the space of physical states. New observables without classical counterpart arise. The metric can be understood as an evolving constant of the motion defined as a quantum operator on the space of physical states. For it to be self adjoint its range needs to be restricted, which in turn implies that the singularity is eliminated. One is left with a region of high curvature that tunnels into another portion of space-time. The results may have implications for the current discussion of "firewalls" in black hole evaporation.

http://gr20-amaldi10.edu.pl/userfiles/book_07_07_2013.pdf see page 218 of file.

He is saying they get a new expanding spacetime region from where the singularity used to be. It's pretty unambiguous. And he notes possible implications for the "firewall" discussion.

55. **Cosmonut**
August 25, 2013

A very interesting discussion indeed.
From what I can gather, the firewall paradox is deeply connected to quantum gravity (?).

But string theory claims to be a successful quantum theory of gravity. (From what I know, the successful quantization of gravity is said to be one of the "well established" triumphs of string theory, unlike the landscape nonsense).

In that case, why doesn't ST provide a clear resolution to the firewall paradox ?

Apologies in advance if this has been discussed elsewhere.

56. **Lee Smolin**
August 25, 2013

Dear Scott,

Thanks very much for your question. Let me be clear first of all, there are so far no exact results in full LQG (ie the full 3+1 d spacetime diffeo invariant QFT) as to the fate of black hole singularities. What there are are results about models which reduce the full diffeo gauge symmetry to 1+1 dimensional diffeo symmetry. These give different results, so there is no clear message except that there may not be a single universal answer.

The region to the future of the resolved singularity can reconnect with infinity after a period of evolution through quantum geometries that have no classical description. In this case you get what you want. This is shown in detail in LQG analyses of the 1+1 dimensional CGHS model by Ashtekar, Tavaras and Varadarajan, arXiv:0801.1811, recently studied also by Ashtekar, Pretorius and Ramazanoğlu: arXiv:1011.6442 and arXiv:1012.0077. I am surprised that these papers are not better known.

A different kind of 1+1 model is studied by Modesto who found evidence for a bounce leading to new asymyptotic regions as discussed also above by Marcus in his discussion of the recent very interesting model of Gambinii and Pullin.

The picture seems to be that while the elimination of singularities is universal the fate of the resulting quantum region to the future of the resolved singularity depends on details of the dynamics of the quantum geometry and hence requires detailed computations of models such as I've discussed.

I want to reply too your general comments, I'll do that in a later message.

Thanks,

Lee

57. **Lee Smolin**
August 26, 2013

Dear Scott,

The issues you raise are subtle, partly because there is not a formulation of QFT on curved spacetime that shares the coordinate and diffeomorphism invariance of classical GR. So at the very least beware of claims and intuitions based on one choice of coordinates. The thermalization of Hawking radiation appears to be fully explained by projecting out a subsystem of an entangled pure state. Remember these are free fields-there are no interactions of the modes at the horizon with each other-so there is no physical basis for rapid mixing. The other system the Hawking photons are entangled with are modes that fall through the horizon and are approaching another boundary-the singularity in Hawking's original calculation and whatever is post=singularity when the singularity is resolved. That is the physics as we best understand it.

I’d like then to address your statement: “ Namely, I would like the laws of physics to uphold the holographic entropy bound, that the total number of qubits in any bounded region should be upper-bounded by the region’s surface area in Planck units.”

That is a statement of what we can call the “strong holographic bound”. We can distinguish it from a weak form of the holographic bound (hep-th/0003056) which might be stated, “the total number of qubits measurable on any surface should be upper-bounded by the region’s surface area in Planck units.”

I would argue that all the evidence we have is that the weak form is correct. I give several arguments in hep-th/0003056 for the weak form over the strong form as best explaining the evidence we have from Bekenstein and Hawking’s original arguments as well as since. Moreover, recent work deriving black hole thermodynamics from quantum gravity by Bianchi, both perturbative (arXiv:1211.0522) and non-perturbative (arXiv:1204.5122) shows that the black hole entropy is best understood as an entanglement entropy. I would suggest that this be taken seriously as it is the only calculation of the BH entropy that gets the 1/4 right without any parameter fixing for a generic non-extremal black hole.

Thanks,

Lee

58. **Brian**
August 26, 2013

Scott, the sentiment is ok, but one must choose between other universes (ridiculous, of course) and the (quantum) ‘universes’ of other observers. In the latter case, one might say that unitarity was preserved for any given observer, but in the context of black hole information one probably needs to account for multiple observers. There could easily be (non local) information that violates unitarity, but then that does not necessarily conflict with your view, if you modify the wording.

59. **Mitchell Porter**
August 26, 2013

Lee Smolin, a number of people have examined those papers by Bianchi and they say that he is not counting black hole microstates, that it’s just a version of the original Bekenstein-Hawking calculation.

60. **Jim Graber**
August 26, 2013

Scott,
Thank you for your reply.
“”Actually, several speakers (including Stephen Hawking, who’s been saying similar things since he conceded the information loss bet in 2004; and Samir Mathur and several other “fuzzball” people) explicitly advocated replacing black holes by some kind of unitary “black-hole-like object.”” “
I am happy to see Mathur’s fuzzball proposal getting more serious attention. I have followed it for many years, and had read many of the earlier papers, but I was not up to date on all the more recent papers.
As to Hawking. I could not find a published paper clearly advocating an object without horizon or singularity. I tried, but could not understand his Fuzz or Fire workshop talk, so I don’t know if he mentioned it there. (Maybe a transcript or a paper will emerge later?)
I reread his concession paper arXiv:hep-th/0507171 and scanned his more recent work, , but I didn’t find a clear endorsement of such black hole replacements. If a good reference where Hawking supports an unconventional black hole replacement exists, I would like to hear of it.
Jim Graber

61. **Lee Smolin**
August 26, 2013

Mitchell,

If someone has an objection to the claims of Bianchi’s papers I’d be glad to discuss in detail, as I’m sure would also Bianchi. Indeed he has spent a lot of time in discussion with people who were originally skeptical, some of whom conceded in the end he was right.

In any case please explain your statement as on the face of it its confusing. First of all there are two papers by Bianchi, which one are you referring to, the perturbative or the non-perturbative one? Second, please explain what you mean by “a version of the original Bekensteiin-Hawking calculation?” I don’t know what that could refer to as the two of them did very different things. In any case both were semiclassical and treated the metric geometry classically, which Bianchi’s first paper is clearly not.

Many thanks,

Lee

62. **Peter Woit**
August 26, 2013

Mitchell and Lee,
This is starting to give me bad flashbacks to endless 2006 blog comment arguments about LQG vs string theory black hole entropy calculations, which seemed to me highly unenlightening. Claims about what “a number of people” say about this definitely are not enlightening. Unless this has a lot more to do with the discussions at the KITP workshop than it seems, it would be best to just give references here to good sources of information about this question.

63. **Mitchell Porter**
August 26, 2013

I meant Hawking’s semiclassical calculation, not Bekenstein’s. The claim is that a true explanation of black hole entropy should involve state counting, and that Bianchi’s 1204.5122 does not. There was a discussion of this at [Bianchi’s entropy result…] when the paper first appeared.

I had not seen the later paper, 1211.0522. I now gather that the philosophy of the two papers is as follows. 1204.5122 should not be regarded as a microscopic explanation of black hole entropy, it really is just the Hawking semiclassical calculation in a LQG framework. It’s 1211.0522 which proposes a microscopic picture, based on entanglement entropy. For a non-LQG counterpoint to that discussion, I suggest 0905.0932, e.g. section 2.5.

The thread on Bianchi’s work that I just mentioned is still open, and would be a suitable venue for continuing with this topic.

64. **Scott Aaronson**
August 27, 2013

Jim Graber:

Here's the relevant paragraph from Hawking's concession speech.

"Information is lost in topologically non-trivial metrics like black holes. This corresponds to dissipation in which one loses sight of the exact state. On the other hand, information about the exact state is preserved in topologically trivial metrics. The confusion and paradox arose because people thought classically in terms of a single topology for spacetime. It was either R^4 or a black hole. But the Feynman sum over histories allows it to be both at once. One can not tell which topology contributed to the observation, any more than one can tell which slit the electron went through in the two slits experiment. All that observation at infinity can determine is that there is a unitary mapping from initial states to final and that information is not lost."

I should confess that I don't understand this argument (and apparently I'm not alone — even Preskill, to whom Hawking conceded, said he didn't understand it!). But Hawking does seem to be clearly asserting that the solution to information loss involves there being a nonzero amplitude for the black hole never forming in the first place. (Though an obvious issue is that he doesn't say how *large* the amplitude is: if it were nonzero but exponentially small, that wouldn't seem to help much.)

65. **Jim Graber**
August 27, 2013

Scott,
Thanks very much!
I read that paragraph and didn't pick up that it is a reference to a black hole substitute which is topologically trivial. In my book, that should eliminate both the singularity and the so-called "horizon" which is more like a "brink". That is my favored configuration, so this pleases me very much.
But in my thinking, the topologically trivial configuration should be dominant.

66. **jd**
August 27, 2013

Are there relevant ideas in PRL 110, 101301 (2013)?

The paper is available for free download.

67. **Peter Woit**
August 27, 2013

I see Scott Aaronson has a blog posting about this now, at
http://www.scottaaronson.com/blog/?p=1508
so if you want to discuss these issues with someone who actually was at the workshop and knows what is going on, Scott's blog is your best bet.

68. **Peter Shor**
August 28, 2013

To me, the amazing thing is that the theory of black hole complementarity was taken seriously by a lot of people for two decades before AMPS showed that this theory doesn't hold water (at least, not as it was originally presented). I suspect that the moral is that if somebody writes physics papers with very few equations, lots of words, and a moderate amount of handwaving, you should ignore them (unless they are named Einstein).

It really seems to be a case of *The Emperor's New Clothes*. As Scott Aaronson says on his blog:

> I didn't call it a "paradox," I just called it "I don't understand black hole complementarity"!

I don't believe he was the only one who thought he didn't understand it.

69. **Friedwardt Winterberg**
September 3, 2013

The conclusion that a black hole is at the event horizon surrounded with a wall of fire by the disintegration of infalling matter was first proposed in an article I had published in 2001 in Zeitschrift fuer Naturforschung 56a, 889 (2001). My paper had the title "Gamma Ray Bursters and Lorentzian Relativity". It is Lorentzian relativity which resolves the black hole information paradox, with no information loss or violation of unitarity. In Lorentzian relativity SRT and GRT remain extremely good approximations for energies small compared to the Planck energy. My paper is cited in "An Apologia for Firewalls" by Almheiri, Marolf, Polchinski, Stanford and Sully: arXiv:1304.6483v2 [hep-th] 21 jun 2013.

70. **martibal**
September 7, 2013

Lee,

> If someone has an objection to the claims of Bianchi's papers I'd be glad to discuss in detail.

There is a point that is puzzling me, and which is also puzzling me in Bianchi's computation: in your paper GR as the equation of state of SF, you consider a *finite* piece R of the near horizon region observed by a family of accelerated observers. You further require that R has the *Unruh property*, namely that there exists a state in the Hilbert space associated with R which is thermal with temperature $a/2\pi$ (a being the acceleration of the observer). I do not really understand in which context this assumption makes sense. More precisely:

1. In the framework of algebraic quantum field theory [AQFT], we have shown with Rovelli in Class. Quant. Grav . 20 (2003) 4919-4932 that a uniformly accelerated observer in a finite double-cone region of space-time of size L sees the vacuum at a temperature which, at first order in 1/L, equals the Unruh temperature $a/2\pi$. But our result heavily relies on:

a) an extended acceptation of the KMS condition as a *local equilibrium condition;*

b) the requirement that the qft under investigation is *conformally invariant*. Otherwise (e.g. for a massive theory), it is a longstanding open problem in AQFT whether there exist observers in a finite region of spacetime for which the vacuum state is indeed thermal (technically speaking: unlike the Wedge case, the modular group associated to a non-conformal qft defined on a finite region of Minkowski spacetime may not have a geometrical action).

2. Besides AQFT, there are some proposals for an "Unruh temperature for finite lifetime observers". For instance J. Louko (see e.g his work with Satz here) considers a finite-time interaction of an Unruh-de Witt detector with the vacuum. As far as I can remember, it is not clear at all that one finds a temperature equal to the Unruh temperature at first order in the time of interaction. There is for instance a non-trivial dependence in the shape of the function describing the switching on/off of the interaction.

—

So it is not so obvious to me what the Unruh property for a *finite region of spacetime* means, even at first order in the size of the region.

In your paper you use the result of Bianchi who showed that R was Unruh. From what I understand following various discussions with people expert in these thematics, the idea is to assume that an observer stationary near the horizon *locally* sees the same vacuum as the one seen by an eternally accelerated observer in the Rindler Wedge. What I do not understand is why this is enough to justify that the finite region is Unruh ? My point is the following: in the Wedge, the computation of the Unruh temperature does not relies only on the fact that the Unruh-de Witt detector interacts with the vacuum, but also that this interaction is integrated all along the trajectory of an eternal observer, that is from $\tau=-\infty$ to $\tau=+\infty$ (similarly in AQFT: to obtain the Unruh temperature one has to consider the algebra of local observables associated to the whole Wedge, not to a sub-region of it).

—
Alternatively, the notion of "local equilibrium state" is far from obvious.
For instance Buchholz and Solveen have extensively discussed it, and proposed a definition different from the one we used with Rovelli (and again, in this context the meaning of a *finite* Unruh region is not so obvious to me).

—
Of course I am not claiming that these objections invalidate the BH entropy computation. But it seems to me that the the proof the finite region R is Unruh is based on some "hidden" assumptions that are not completely clear.

Sorry for the length of the message, but I would be interesting to hear your opinion on these points.

SUSY 2013

August 31, 2013
Categories: Uncategorized

The big yearly SUSY conference, SUSY 2013 has been going on in Trieste this past week. From the experimentalists, the news is just stronger limits: no hint of SUSY anywhere in the LHC data. From the theorists, the reaction to this news has been pretty consistent: despite what people say, not a problem.

According to John Ellis, everything is fine, with MSSM SUSY preference for a Higgs below 130 GeV vindicated and successful SUSY predictions for the Higgs couplings (that they should be the same as if there were no SUSY). According to Ellis, we just need to be patient, and he has CMSSM fits preferring 2 TeV gluinos.

However, if you look at Savas Dimopoulos's talk the MSSM gets a grade of D-. He argues that the LHC has shown us that the answer is the Multiverse, and that split SUSY with its fine-tuning gets a grade of A. The grade inflation in particle physics is pretty dramatic: you now can get an A without your theory having the slightest bit of experimental evidence.

Nima Arkani-Hamed's talk was about SUSY in 2033, which in his vision will be pretty much the same as SUSY in 2010. Remember all those things the LHC was supposed to find but didn't? Well, now the argument is that they're really there, but we will need a 100 TeV collider to see them. If all goes well, in 2033 such a machine will be under construction, and SUSY 2033 could feature all the SUSY 2010 talks retreaded, with 1 TeV gluinos moved up to 10 TeV.

One of Arkani-Hamed's slides ~~makes me worry that the LHC results have caused him to begin to lose his marbles. He~~ claims that if one doesn't see new physics like SUSY at the 100 TeV machine, in his view

> this would be 100 times more shocking and dramatic than ~~no~~ *nothing but* Higgs at the LHC

Even wilder claims came form Gordon Kane in his talk, where we're told that particle theorists giving "negative" talks because of the LHC results have:

> no knowledge of LHC physics, Higgs physics, supersymmetry, phenomenology, etc.

According to Kane, we not only have seen the tip of the iceberg of a unified string/M-theory, but actually have the whole iceberg. The ingredients are all in place for what he sees as a similar experience to the 3 year period in the 1970s when the Standard Model emerged and was experimentally vindicated.

Tommaso Dorigo points out that there was one SUSY 2013 talk that in his humble opinion was a good candidate for the IgNobel, see here (warning, NSFW).

On a more positive note, at the conference production of compactified Calabi-Yaus was finally conclusively demonstrated.

Update: Nathaniel Craig has some recent lectures on The State of Supersymmetry after Run I of the LHC. The emphasis is on examining the consequences of failure of pre-LHC assumptions about SUSY based on simplicity and naturalness. Out of 60 or so pages, only one is devoted to the models favored by Arkani-Hamed and Dimopoulos, string-theory based models are not even mentioned.

Comments

1. **Bee**
   September 1, 2013

   I once had to sit through a talk where "Susy" was depicted by Betty Boop. Gives you a totally new perspective on what the guys are after.

2. **emile**
   September 1, 2013

   I went through Nima's slides (so few this time!). The talk is essentially a sales pitch for the next big machine. I wouldn't say that his statement on what would be most shocking (no Higgs vs no BSM up to 100 TeV) is crazy as Peter suggests. If I can play devil's advocate and give one example: I've known many theorists (though they were always a minority) who thought that the breaking of EW symmetry had to be dynamical in nature. Now in the last 15 years, the measurements have been pointing more and more away from such an explanation but that possibility was still alive. It was one of the solutions to deal with the problems associated with a fundamental scalar i.e. you remove it from your theory. So… perhaps Nima would have been surprised but not that shocked if there was no fundamental scalar but he would be shocked if the Universe was really fine-tuned (although with split SUSY he has certainly considered such a possibility…). Anyway, I don't think it is crazy. One slide I did not understand is the one that follows the introduction of the e+e- collider where he writes "kills all anthropic explanations". Anybody understand what he meant there?

3. **Peter Woit**
   September 1, 2013

   Emile,

   He wasn't saying no BSM at 100 TeV would be more shocking than no Higgs, he was saying it would be "100 times" more shocking. It's this insistence on going over the top into wackiness that is, well, kind of shocking.

   I'm not interested enough in anthropic explanations to figure out exactly what scenario he had in mind that "kills all anthropic explanations", but given the "100 times" claim, I'd guess that it's yet another over-exaggeration of something.

4. **Pawl**
   September 1, 2013

   Well, implicit in Arkani-Hamed's talk is a great truth: experiments drive physics, and at the moment theoretical work on what the fundamental questions he's interested in is unconvincing.

   Besides the technical over-the-top comment Peter pointed to, there's what to my mind is a more basic concern: "… we've attracted the best minds on the planet to work on the hardest… problems in all of Science." Someone who says this may be (probably is) perfectly sincere, but it does raise concerns about how well he understands other fields of science or the people in them.

5. **Neil**
   September 1, 2013

Those colorful Calabi-Yau 3D prints are great! I definitely want one. It is nice to know that string theory has finally produced something tangible.

6. **Pawl**
   September 1, 2013

   [addendum to my previous comment]

   Or, I should add, other, non-scientific, disciplines.

7. **scotty**
   September 1, 2013

   Does someone know if Arkani-Hamed also gave some advice to today's young physicists what to work on during the next three decades of their careers, until 100 TeV data may arrive (if we are lucky)?

8. **Jon Orloff**
   September 1, 2013

   What happens if the taxpayers decide that it's not worth building a 100 TEV collider? That could happen if there isn't a much better reason than put forth than the airy stuff I have seen so far.

9. **george ellis**
   September 2, 2013

   Pawl is absolutely right:

   "… we've attracted the best minds on the planet to work on the hardest… problems in all of Science."

   So all those people in quantum optics and nanoscience and molecular biology and neuroscience and climate change don't compare intellectually with us. We are the elite, and their problems don't compare with ours…..

   Do physicist really have no concept of the extraordinarily complexity of the problems neuroscientists are trying to deal with? Or the incredible achievements of the molecular biology revolution?

10. **Peter Woit**
    September 2, 2013

    My apologies to Nima, it seems that I misread and misquoted his slide (thanks to a helpful reader for pointing this out). His claim was that no BSM at 100 TeV would be 100 times more shocking than no BSM at LHC energies, NOT that it would be 100 times more shocking than no Higgs at the LHC. Read accurately, its a wild exaggeration, but not the crazy-talk of my misreading.

11. **Dan D.**
    September 2, 2013

    @george ellis,

    It seems to me that this is a fairly common (though hopefully not too widespread) attitude among scientists of *any* discipline, to view one's own field of being of primary importance, or at least more important than those "lesser scientists" in [pick a scientific field]. It's also true when one broadens to philosophy and mathematics, for example. You'll see mathematicians looking down their noses at physicists for not being sufficiently mathematically rigorous, you'll see physicists deriding philosophers for discussing problems that *gasp* may not have any answers in physics (this in particular seems to be in vogue as of late), and on the flip side you'll see philosophers criticizing physicists for not researching the really "deep" problems. And so on. I think this is simply the tribalism of human nature manifesting itself within academia, which is no excuse, of course. It's not necessarily a bad thing (the tribalism, that is), until it gets to the point when no effort is made to apply any intellectual rigor to understanding other "tribes", even to the point of demonizing them (as is common for climate scientists these days, from people who should know better). Whether Arkani-Hamed's slide in question was really betraying this sort of elitism on his part, or was simply a bit of innocent pride in his own field (nothing wrong with this), is something only he could answer, I would think.

    More on topic, as a non-particle physicist, I found Arkani-Hamed's overall argument for a 100 TeV collider convincing enough. I think such should be built if only for the passion of needing to know "what's over the next mountain". I guess I'm too much of an idealist :).

12. **Dan D.**
    September 2, 2013

    Quick clarification, I meant it seems to be disturbingly common for climate scientists in particular to be demonized even by other scientists these days, which is unfortunate for the field, even if some of the individuals within it arguably deserve it from time to time.

13. **Bernhard**
    September 2, 2013

    "According to Ellis, we just need to be patient, and he has CMSSM fits preferring 2 TeV gluinos."

    It is interesting to compare this claim with an older one (http://www.nature.com/news/2011/110228/full/471013a.html):

    "I'm wouldn't say I'm concerned," says John Ellis, a theorist at CERN, Europe's particle-physics lab near Geneva, who has worked on supersymmetry for decades. He says that he will wait until the end of 2012–once more runs at high energy have been completed–before abandoning SUSY."

    Assuming this was semi-accurate…

14. **emile**
    September 3, 2013

    Regarding the idea that the best minds are working on physics problems: this reminds me of a list by Joe Lykken on the top 10 reasons why physicists are better than biologists. One of the reasons was: Physicists used to be smarter and more arrogant than biologists, now, they are just smarter.

    Regarding the favoured regions of the CMSSM moving with time: this happens by construction. The real question is how is the goodness of fit evolving with time?

15. **Anon**
September 3, 2013

After the 14 TeV run with 10 times more data if LHC finds nothing fine tuning would then be at 1% level. Currently fine-tuning is probably at about a 5-10% level. If there is a natural solution to the fine-tuning problem, isn't it much more likely that it will be found at 14 TeV run than being found only at 100 TeV? Intuitively, shouldn't the probability of finding a natural solution to the hierarchy problem grow exponentially (or rapidly) smaller with the amount of fine-tuning?

How can Nima make this assessment that it would be 100 times more surprising to find nothing at 100 TeV? Doesn't sound logical to me….the 14 TeV run has not even started yet and its importance should not be undermined.

16. **P**
September 3, 2013

Peter,

Nima's ~ 100 times more surprising argument seems pretty standard. The COM energy picks up a factor of 10 compared to LHC, which then gets fed into the quadratic divergence of the cutoff.

No need to restart the scientific argument regarding naturalness in this thread, but I think honesty requires someone pointing out that *most* of the community disagrees with you that this is unimportant; i.e. "Wild exaggeration" are your words, and many would disagree.

Cheers,
P

17. **Anon**
September 3, 2013

P —

What is standard is that at 100 TeV you can establish fine-tuning to a factor of 100 more than at LHC — ie we can show that Higgs mass is 0.01% (or worse) fine-tuned compared to being 1% (or worse) fine-tuned at LHC14.

But why would a discovery of finetuning of 0.01% be 100 times more surprising/shocking than discovery of a fine-tuning of 1%?….when the real shock is that there is fine-tuning of Higgs mass. We have a renormalizable theory that could be good all the way up to the Planck scale and it does not by itself allow us to be shocked by some artificial naturalness/finetuning standard like 0.01% (or equivalently 100 TeV machine).

Where do you produce a small unnatural number like fine-tuning of 0.01% to set a standard for naturalness?

Note that there are naturalness explanations based on approximate chiral symmetries for small Yukawas that could be like 0.1, 0.01, 0.001, 0.0001 and even 10^-5 in the standard model. Similar chiral symmetries and arguments are at work in SUSY theories to protect the Higgs/Higgsino masses. And LHC would show beyond any doubt show that such a naturalness argument has failed in the case of the Higgs and hierarchy problem.

18. **Peter Woit**
September 3, 2013

P,
Others have weighed in, but the obvious point is that making degree of shockingness linear in the degree of fine-tuning, instead of, say, logarithmic, is, well, the kind of thing you would do if you like exaggeration.

In general, I think if you have a hypothesis that something should be order 1, and experiment bounds it at .01, it's more conventional to start questioning your hypothesis than to announce that a bound of .0001 would be 100 times more shocking.

19. **The Vlad**
September 4, 2013

Peter:

Do you understand what Craig meant by the second part of the quote "there are many valid reasons to favor supersymmetry, some of which are only strengthened by what we've learned so far at the LHC"?

Do you think he was alluding, as he later writes, to the claim that "[SUSY] predicts the Higgs mass to lie below 135 GeV, in good agreement with observation"?

Assorted News

September 4, 2013
Categories: Multiverse Mania

- This past weekend I was up in Boston and attended quite a few talks at the Gelfand Centennial conference at MIT, in honor of the 100th anniversary of I. M. Gelfand's birth. Abstracts of the talks are available, but most of them were blackboard talks, not being recorded as far as I could tell. I've been starting again on my project to learn more number theory, so found Matt Emerton's and Akshay Venkatesh's survey talks especially helpful.
  There was one long afternoon program of recollections of Gelfand and his seminar from a long list of speakers, which went on into the evening banquet. This was being recorded, so video will perhaps appear some day (Gindikin's contribution was on video, available here). Another long afternoon session dealt with Gelfand's mathematical legacy, again perhaps at some point there will be video available of this.
- In mathematical news, speakers at next year's ICM have now been announced, for both the plenary and the various sections. Those interested in tea-leaf reading can consider for themselves what this new information says about who will get a Fields Medal next year. They might also appreciate this.
- A Fields Medal is worth just 15,000 Canadian dollars. If you can claim some relation to physics, much better to have your friends get to work nominating you for a $3 million fundamental physics prize. Online nominations for 2014 are here, and the news is that the three finalists for the $3 million will be announced this November. The Selection committee will be the 11 previous theorist winners of the $3 million prize plus three LHC physicists from the experimental side. The FPP also has some news here about what some of the LHC experimentalist prize winners have done with the money.
- Historically unparalleled payments to the stars of the field seem to be part of a larger societal pattern, as well as a much grimmer picture for young non-stars. The situation on the theorist side is not news, but Adrian Cho at Science magazine has a story about the extremely ugly job prospects facing young LHC experimentalists, with the title After the LHC, the Deluge.
- In case you weren't aware of this, see here for an explanation of why The STEM Crisis is a Myth. One thing in that article I'd never seen before is Alan Greenspan's explanation of why we need more H1B visas: the inequality problem in the US is due to overpaid computer programmers, and these plutocrats can be dealt with by importing low-wage labor to take their jobs.
- Finally, for the latest in multiverse mania, New Scientist has Death by Higgs rids cosmos of space brain threat (and an editorial about how this shows the Higgs is not "boring"). I knew there was no way they could resist Sean Carroll's new paper dealing with the question: Can the Higgs Boson Save Us From the Menace of the Boltzmann Brains?. Sean has more about this here, and Jacques Distler has a discussion here which I think accurately reflects the views of physicists outside certain West Coast enclaves:

  > Normally, I wouldn't touch a paper, with the phrase "Boltzmann brains" in the title, with a 10-foot pole. And anyone accosting me, intent on discussing the subject, would normally be treated as one of the walking undead…
  >
  > This is plainly nuts.

  I confess that this kind of thing completely mystifies me. Carroll is an intelligent, well-informed, and almost always reliably sensible sort, with a keen devotion to the battle for scientific rationality against the forces of religion and obscurantism. But he likes to pair this with an enthusiasm for pseudo-scientific multiverse wackiness that Distler's "nuts" describes pretty well. Very weird, and if you want to know why I keep referring to "mania" in this context, this is a good example.

Comments

1. **Bernhard**
   September 5, 2013

   "… what some of the LHC experimentalist prize winners have done with the money."

   Bravo for Incandela and Virdee. We are still waiting to know about the rest though.

2. **CU Phil**
   September 5, 2013

   It looks like Sean Carroll will be giving a physics colloquium at NYU in October, so maybe you'll get a chance to hear about this straight from the horse's mouth.

3. **Alex**
   September 5, 2013

   You say there's no STEM crisis, but in August of 2008 somebody from my university's office of instructional technology told me to my face that the economy will collapse if we don't get every available warm body into STEM. A few weeks later the banking system melted down. I wish I had taken his pious sermon more seriously.

   Next you'll try to tell me that there's no experimental evidence for string theory!

4. **Peter Shor**
   September 5, 2013

   Unless it's changed, the plenary speakers and the Fields Medals are chosen by two different committees who don't talk to each other, so information theoretically, the list of plenary speakers doesn't give any information about the Fields Medalists that isn't already public knowledge.

5. **X**
   September 5, 2013

   I'm a bit surprised by this reaction to the Boltzmann brains paper. One way to resolve the BB paradox is to put a finite lifetime on the universe. A metastable Higgs phase does that. These two statements seem unremarkably standard physics, so what's the big problem? My reaction to the paper was more "Duh, isn't this obvious? Who's trying to get a publication out of this?"

6. **El-Coco**
   September 5, 2013

   Gelfand wrote some amazing introductory primers to elementary topics in mathematics. I wish other top talents would do the same. Perhaps they believe it a waste of their time, but Gelfand's little books changed a lot of lives (mine included).

7. **Peter Woit**
   September 5, 2013

El-Coco,
At the conference I think Gelfand's widow Tatiana mentioned that she was working on getting out an elementary geometry textbook written with him.

A couple links I should have included in the posting:

http://www.israelmgelfand.com/index.html

and Tanya Khovanova's toast at the banquet

http://blog.tanyakhovanova.com/?p=470

8. **Peter Peterson**
September 5, 2013

Peter, you already had a pretty prolonged discussion about STEM, and I commented there also. I hold you in high respect in anything related to physics and mathematics. However on this topic both you and the author of an article are very wrong. I am sure there are more than these two articles, but there are many more on the opposite side of the issue. The crux of the matter here is as always not quantity but quality of STEM workers. Or, in other words, a need to find a few diamonds in a pile of rubble.

9. **tt**
September 5, 2013

we need more skilled economists, not STEM workers

10. **z**
September 6, 2013

In terms of STEM research jobs, there will always be a job shortage since most science is done via a finite number of federal grants that eventually produce PhDs with a multiplicity factor greater than unity. Even if you increase the amount of money for grants, this model is still has no equilibrium solution. H1Bs however are not usually PhDs, but programmers and thus mostly only drive down wages for BA/BS-level jobs. Computer programmers _are_ overpaid compared to other high skill work, like say, science teachers or community college professors — and it is dictated by supply and demand.

There is also another STEM crisis, but it's not about degrees or credentialism, but that the general public is highly deficient understanding quantitatively and logically how the natural world works.

What needs to happen is that every college student, regardless of major, needs to know how to program, know calculus-based physics that includes thermodynamics and E&M, and take a biology course that teaches evolution. I find it unbelievable that people can graduate college without this basic knowledge, when I had it taught to me in a public high school. I am aware some undergraduate programs do this, but it needs to be more widespread as I think it's the 21st century equivalent of a 19th century classical/liberal arts education.

11. **T.G.**
September 6, 2013

According to the results of these two polls, the Fields medalists will be Bhargava, Avila, Lurie and one more not from the list in the first poll.

12. **Alex**
September 6, 2013

Peter Peterson-

In regard to quality of STEM grads, there are at least three distinct things going on:
1) Are the graduates smart (whatever that means) and rigorously trained to think through hard problems? I would argue that laments about "new grads these days" are probably timeless, but if there is any truth to them then efforts to increase quantity might actually be hurting quality.

2) Is their training relevant to the needs of industry? This is more of a problem of the disconnect between academia and industry than a problem of quantity. There is indeed "not enough", but it's a lack of grads with the right training, and increasing the raw number of grads without changing their training would not help.

The second problem requires faculty to take a hard look at curricula, projects, internships, etc. Adopting the latest edu-fad buzzwords is not what's really important here. What's important is us making a connection with industry and understanding what they need.

3) Industry seems less interested in investing in training, so the days of "Hire a smart grad and then train them to do what our company does" is gone. They want us to produce people specific to their needs. Depending on how broadly they define those needs that might be fine, but we can't design one degree program per job opening. We need to construct degree programs a bit more broadly than that.

I don't see any problems here that require greater quantity. What they require is a refocusing of educational efforts, and some effort by academia and industry to meet each other half-way.

13. **Wyman**
September 6, 2013

Were there a real shortage of qualified workers in STEM disciplines then employers would be forced to raise wages and actually provide training. Giving that the trend is broadly in the opposite direction on both counts, I'm not sure how anyone could conclude that there's a shortage outside of some specific positions. Really, a labor shortage of any sort without growing wages is just nonsense.

14. **davetweed**
September 7, 2013

Alex: there's an even deeper issue with your point 3. If you take the (reasonable view) that the best people to teach area X are those with some degree of experience in X (which isn't to deny that sometimes the best practitioners are hopeless teachers, but that teaching something one only knows about through being taught to teach it doesn't work that well), then universities can't be a good way to produce people trained for jobs in company Z because most of what university people do when not teaching (research) is very different from what employees in company Z do. (The few exceptions — eg, law schools where the professors are also practicing lawyers/judges — tend also to produce the graduate populations that companies complain least about.)

If companies want people who are tailored to their environment then they have no alternatives to doing training, whether it's "on-the-job training" or getting substantially more involved in degree level teaching. (To be fair, quite a few companies do actually do good on-the-job training, sometimes taking people without university degrees if they feel the training of a degree doesn't help them.) Unfortunately the prevailing atitude of many companies/business organsiations seems to be that they want a certain output from degrees without contributing significant input.

15. **Yatima**
September 7, 2013

> I'm a bit surprised by this reaction to the Boltzmann brains paper. One way to resolve the BB paradox is to put a finite lifetime on the universe. A metastable Higgs phase does that.

As you no knowledge about how "the universe" looks after "the metastable Higgs phase" has done its job, this doesn't resolve anything at all.

16. **Art**
September 7, 2013

Keep in mind that the STEM article you quoted appeared in the IEEE Spectrum mag. IEEE is a professional organization and has long desired to be the electrical engineering equivalent of the American Medical Association, controlling the supply of its members to keep their wages high. Nothing wrong with that; the point is that they have a big dog in this fight, so *caveat emptor* .

17. **Tom**
September 7, 2013

@Art said:
" … IEEE is a professional organization and has long desired to be the electrical engineering equivalent of the American Medical Association, controlling the supply of its members to keep their wages high. … "
— — — — — -
I've been an IEEE member for some 30 yrs. I've not encountered or observed any such systematic policies from IEEE. If anything, it's tended toward the opposite.

OTOH, my main interest in IEEE is their technical publications, conferences, and opportunities for professional networking. The "leadership" of the various IEEE divisions seem to be academic or research-oriented engineers with too much time on their hands. I admit — I tend to tune them out.

18. **Visitor**
September 7, 2013

"Carroll is an intelligent, well-informed, and almost always reliably sensible sort, with a keen devotion to the battle for scientific rationality against the forces of religion and obscurantism."

Carrol also seems to be engaged in an attempt to advance himself as Michio Kaku's successor in the trash media. Hence such puerile foolishness as equating cooking eggs with doing cosmology. I'm not sure if he will ever be "The Fried-Egg Cosmologist" in the same way that Stanton Friedman is "The Flying-Saucer Physicist" but, as the lottery slogan has it, hey, you never know.

19. **srp**
September 7, 2013

Another (though least important) reason for trying to invest in advanced accelerator concepts: Some of the proposed AAC technologies have complementarities with more-applied areas (e.g. possibly building tabletop soft X-ray lasers). Anything that reduces the gap between the working technology used in particle physics and the problems of other fields can only improve the employment situation.

20. **CIP**
September 7, 2013

In the future, physicists will be expected to wear the livery of their patrons.

21. **Alex**
September 7, 2013

I don't know about IEEE specifically, but my experience has been that most professional societies in STEM whole-heartedly endorse "We need more students in STEM! We need more students in STEM!" Some of it might be the self-interest of their academic members, but I think most of it is the psychology of regarding one's profession as Important. Electrical engineers love electrical engineering, they think it's important, anybody who cares enough about it to get involved in IEEE probably loves to share their enthusiasm for engineering, so their psychological leaning will probably be to encourage interest among the young.

22. **Peter Woit**
September 8, 2013

Visitor,
Actually, while I'm sure he'd love to take over Kaku's role in the media (and maybe already has), Carroll is generally much more sensible in his public discussion of physics. He mainly seems to go over the top on multiverse-related issues, sometimes going beyond what Kaku ever tried (e.g. writing wacky scientific papers).

23. **Mike**
September 8, 2013

The subject of STEM workers and H1B visas is always frustrating – reports from everywhere seem to come out that we have a shortage of technical people and there are jobs for the taking out there – we just need more grads in the science and engineering field. In the meantime, we should just increase H1B visas. Most of these reports can be traced back to lobbying groups that support the clamoring of corporations that will play any game to get those high-paying jobs down into the "acceptable range" as seen by economists and accountants who immediately see a better balance sheet for the companies they work for when any labor cost drops. It seems like our economy has suffered enough at the hands of people who's idea of math is add, subract, multiply and divide (and maybe some watered-down calculus they had to suffer through in business school)!

24. **Mark**
September 9, 2013

"Peter, you already had a pretty prolonged discussion about STEM, and I commented there also. I hold you in high respect in anything related to physics and mathematics. However on this topic both you and the author of an article are very wrong. I am sure there are more than these two articles, but there are many more on the opposite side of the issue. The crux of the matter here is as always not quantity but quality of STEM workers. Or, in other words, a need to find a few diamonds in a pile of rubble."

But when many industry jobs requiring STEM people pay peanuts why would they attract the best and brightest? In the uk science jobs in industry are very badly paid from what I can see (similar salaries to relatively unskilled jobs – when I see how much money other highly skilled professionals are paid its quite a gulf….) – thats probably why a lot of physicists go and work in banks, or move to other countries offering higher salaries relative to the cost of living.

In fact I would have to take a significant pay cut to take a science job in industry in the UK, so if I ever leave my university research group it won't be for a STEM job in the UK.

As far as I can see we have two problems:

(1) We produce far too many STEM people compared to jobs
(2) STEM jobs can't get the good STEM people, because the best are attracted to other careers or countries with much better pay and conditions, and so perhaps are left with the dross from (1)

25. **John Urbanik**
September 9, 2013

As one of those so called over paid programmers I've been around long enough to see that H1B's (and L1's) are nothing more then attempts to lower the standard of living here in the US to other coutnries. At first companies tried to off-shore work. Then came on-shore/off-shore teams. These attempts all failed for verious reasons.

Companies soon realized that they only alternative is to say there is a shortage of skilled workers here in the US. What they really mean is there is a shortage of skilled workers in the US willing to work for $20K per year. BTW over paid to these 'economists' is anything above $50K per year.

26. **srp**
September 9, 2013

@Mark: Low wages only keep the more talented out of a field if the more talented are unable to demonstrate their superiority in some observable way. Marginal chefs and novelists are quite ill-paid, but there is no shortage of talent in those fields making good incomes. Neither field has strong entry restrictions. (On the other hand, neither field has the inherent oversupply of new workers by grant requirement that STEM has.)

In some fields, supply creates its own demand, e.g. lawyers suing, doctors referring, possibly active money managers trying to outperform the market. In some technological areas "arms race" dynamics have the same effect–radar and electronic countermeasure experts making work for each other, semiconductor manufacturing technologists at rival firms pushing each other to speed up miniaturization, biomedical engineers struggling to come up with better medical devices than their rivals, etc.. Nothing like that kind of dynamic operates in particle physics, which implies the field has to generate demand by 1) popularization and 2) doing "physics appreciation" classes at the high school and undergraduate levels.

27. **tt**
September 9, 2013

" Low wages only keep the more talented out of a field if the more talented are unable to demonstrate their superiority in some observable way." or if they can make more money doing something else.

28. **Mark Decker**
September 11, 2013

Speaking of Kaku's role in the media: Charlie Rose had him on this morning and played back an interview segment from a few months ago showing his (now erroneous) prediction for the 2013 hurricaine season. I guess that's the risk you run when switching from discussing extra dimensions to meteorological predictions since meteorological predictions can be tested.

29. **T.G.**
September 13, 2013

Peter,

Check out this economics paper, especially, the summary on page 33, and figures and tables on pages 33-35. They show that the rate of output of the Fields medalists declines noticeably in the post-medal period, and medalists are pursuing topics that are far less likely to be related to their pre-medal work. One cool statistic stands out:

Average age at death of Fields medalists: 74
Average age at death of "contenders": 60.5

"Contenders" are mathematicians who were awarded at least one of six other mathematics prizes (the Abel, Wolf, Cole Algebra, Bôcher, Veblen, and Salem Prizes) but were not awarded the Fields Medal, and who had above-median per-year citations during the eligibility period for the Fields Medal.

30. **T.G.**
September 13, 2013

Here is an article about that paper.

31. **Peter Shor**
September 15, 2013

Something that bothers me a lot about that study is that to decide who were the contenders for the Fields Medal, they look at the Abel and Wolf prizes, which are typically awarded much later in the medalist's career. I think this post-selection biases the entire study. Suppose contenders for the Abel prize are (a) people who had a great early career and won the Fields medal and (b) people who weren't quite good enough for a Fields medal at 40, but who had a great late career. Including category (b) in the study biases it towards people whose output doesn't decline after 40.

The study should be redone just looking at people who won the Cole, Bôcher, Veblen, and Salem prizes at the age of 40 or younger.

32. **AndreasK**
September 15, 2013

" A question immediately arises: what exactly are the
medalists doing with their time in the post medal period?"

well the answer immediately arises that they were at least not writing questionable, obviously flawed economics papers on cultural subjects they have little or no understanding of. Possibly Mr. Borjas should spend instead more time on writing further papers on the necessity of reducing immigration rates to the US, or does he try to manifest 'intellectual mobility'?

"The harmful effects of immigration will not go away simply because some people do not wish to see them."

http://www.theatlantic.com/past/docs/issues/96nov/immigrat/borjas.htm

33. **Peter Shor**
September 15, 2013

My thoughts: the authors of that paper have PhDs in Economics from Columbia and Princeton, and are employed at two reasonably–well-respected institutions of higher learning.

The word "statistics" is derived from the word "state", because the science of dealing with data is fundamental for running a government and an economy.

And the paper contains what looks to me like an absolutely elementary fundamental in statistics.
This is very scary.

34. **Peter Shor**
September 15, 2013

Oops … I meant to write absolutely elementary fundamental **error**.

35. **Bill**
September 15, 2013

Peter Shor,

I agree, they probably thought that all these awards are equivalent. They could have asked mathematicians at Harvard (where the paper was written) to tell them more about these awards. Or, perhaps, they did ask, but nobody knows anything about statistics at Harvard math?

36. **Peter Shor**
September 17, 2013

The "age at death" statistic is also suspect. The first Fields Medal was awarded in 1936, the first Bôcher prize in 1923, first Cole prize was given in 1928, the first Veblen Prize in 1964, and the first Salem Prize in 1968, the first Wolf prize in 1978 and the first Abel prize in 2003.

Many of the Veblen and Salem prize winners haven't yet had enough time to die of old age.

This could, of course, be compensated for by using the proper statistics. The authors didn't.

37. **Bill**
September 17, 2013

I thought that only mathematicians who already died were counted to compute the "age at death"? The increase of lifespan over the last half a century, probably, can not explain such a big gap by itself.

38. **Peter Shor**
September 18, 2013

Hi Bill: Suppose I compare the age at death of a bunch of mathematicians born in 1920, and a bunch of mathematicians born in 1950. I guarantee you that the average age of death of those born in 1950 is less than 63.

Assuming the Veblen and Salem prizes are given to mathematicians at the age of 40, then the average year of birth of a Vablen or Salem prize-winner is around 1950.

I have no idea whether the Veblen and Salem prize-winners being born later on average will skew the average age of death of contenders 14 years younger, but the authors clearly should have take these correlations into account.

39. **AndreasK**
September 18, 2013

whilte there seem to be other 'oddities' in the study (Wolf- and Abel-prize/non-Fields winners seem to be considered irrespective of age restrictions while non-Fields-winners of Cole-, Veblen-, Bocher- and Salem prize seem to be considered as 'contestors' only if winning one of the prizes before the age of 40, the Salem prize is referred to on one and the same page as a determinant for being 'contestor' and as non-determinant because 'less prestigous' [footnote]), the main problem for me seems to be the questionable use of 'productivity measures' in cultural affairs like mathematics in general. The very first sentence in the abstract of the study already symbolizes the direction of the ideology: mathematics as a means to increase 'economic growth', the use of technoid terms like 'knowledge generation', the always re-occuring term 'productivity'.

Having said this I do indeed believe that the mathematical community, by being oblivious to the widespread introduction of 'industrial measures' into mathematical culture, paved the way for western representants of 'state institutions' to come up with articles like the above which, under the disguise of criticising 'wealth effects on labour productivity' actually aim to put a dubious light on mathematics in general, since depicting the most prestigous members of its community as 'lazy and/or experimenting and/or 'striking out" post-Fields-medaillists means more or less depicting the whole culture as in tendency prestige-governed, narcissistic and irresponsible.

I wonder if anyone ever wrote such papers on prize winners in the field of art or literature, but clearly a public or state institution being seriously interested in 'production rates' of Nobel laureates in literature before or after receiving the prize is quite hard to imagine, quite in contrary to mathematics obviously. So what went wrong?

40. **Bill**
September 18, 2013

Peter Shor, you are smarter than I am. But we already knew that.

41. **Shantanu**
September 18, 2013

OT: Peter and other interested folks.
Videos of conference of 50th anniversary of discovery Kerr metric
http://kerr-conference.org/content/videoclip-archive
This includes a talk by Roy Kerr on how he came up with Kerr metric
Note that until
shantanu

42. **Peter Shor**
September 19, 2013

Bill … this is getting off-topic, but I think more undergrads should take a course in statistics like the one I took from Gary Lorden at Caltech, where he illustrates with examples many of the mistakes you can make by blindly applying statistical tests. If I'm smarter than you in this respect, it's largely due to him.

Perimeter Institute and the crisis in modern physics

September 9, 2013
Categories: Uncategorized

Maclean's has been publishing a very nice series of articles about Perimeter Institute by Paul Wells. These include one about Jacob Barnett, a 15 year-old who is now studying in a master's level graduate program (Perimeter Scholars International) there. Another piece, about other students in the program, is here. It discusses one somehow oddly familiar story, of a "young man with dark hair…seems too cool for school", born in Iran, but educated in Canada, on his way to a promising career in particle theory, Nima Afkhami-Jeddi. There's also yet another piece, with a wonderful description of the bistro at Perimeter.

In the most scientifically substantive piece, entitled Perimeter Institute and the crisis in modern physics, Wells describes PI director Neil Turok's welcome speech this year. Here are some quotes from Turok:

> Theoretical physics is at a crossroads right now…In a sense we've entered a very deep crisis.
>
> You may have heard of some of these models…There've been grand unified models, there've been super-symmetric models, super-string models, loop quantum gravity models… Well, nature turns out to be simpler than all of these models.
>
> If you ask most theorists working on particle physics, they're in a state of confusion.
>
> The extensions of the standard model, like grand unified theories, they were supposed to simplify it. But in fact they made it more complicated. The number of parameters in the standard model is about 18. The number in grand unified theories is typically 100. In super-symmetric theories, the minimum is 120. And as you may have heard, string theory seems to predict 10 to the power of 1,000 different possible laws of physics. It's called the multiverse. It's the ultimate catastrophe: that theoretical physics has led to this crazy situation where the physicists are utterly confused and seem not to have any predictions at all.
>
> The data just fits so perfectly with Perimeter's mission. If it had turned out to be complicated and messy — 10 new particles at CERN and all kinds of funny evidence for models of inflation and stuff in the sky — one would have to say the future of theoretical physics does look pretty messy and complicated. Perimeter would be just one of 100 such institutes.
>
> But given that everything turned out to be very simple, yet extremely puzzling — puzzling in its simplicity — it's just perfect for what Perimeter's here to do. We have to get people to try to find the new principles that will explain the simplicity

Turok's perspective on the current situation is great to hear. It's wonderful to see this kind of admission that the evidence is now in that particle theory has been barking up the wrong tree, coupled with a vigorous position that looking for new principles is where the future lies. My only comment would be that Turok might want to think about bringing in to Perimeter more mathematicians, since if physicists are going to look for new principles, they might need some new mathematics.

For another similar take on the current state of theoretical physics as it faces up to the fact that our simplest theories of particle physics and cosmology are working all too well, see Adrian Cho's Boxed In at Science magazine.

In the US, HEPAP was meeting last week to discuss the Snowmass workshop and the process for going forward with recommendations about the future of HEP. There was a report from the DPF Panel on the Future of High Energy Theory. It had nothing about the intellectual crisis that Turok and others see in the field, with the only crisis addressed the difficult budget situation, leading to cuts in grants. The panel recommends that theorists continue to get two full months of summer salary, and argues that "salary caps" limiting the size of these payments should not be lowered.

Update: Physics World has something about this, with the headline Perimeter Institute welcome speech reignites the string wars.

Comments

1. **Jess Riedel**
   September 10, 2013

   My guess is that the crucial insight will be more philosophical/interpretational than mathematical. Likewise, the Lorentz transformations were discovered and analyzed *in the context of electromagnetism* almost 2 decades before Einstein came up with special relativity. My vague impression of Perimeter is that most of the folks there would agree, although I certainly can't speak for them. But sure, bring on the mathematicians. Maybe their impressiveness will counter-balance the stigma of quantum foundations.

2. **Shantanu**
   September 10, 2013

   Peter, if you compare the list of seminars and other talks at PI now vs many years
   back, when it was formed, earlier there used to be lots of talks on non-trendy topics
   and including by people who usually don't give talks elsewhere.
   whereas nowadays most of the seminar are on the usual fad topics.

3. **Noname**
   September 10, 2013

   "[…] might want to think about bringing in to Perimeter more mathematicians, since if physicists are going to look for new principles, they might need some new mathematics."
   Well, this might be a bit premature, don't you think? What branch of mathematics do you think it will be needed? Fortunately you are not the Perimeter's director!

4. **Peter Woit**
   September 10, 2013

   Noname,

   "premature"? Do you think that if the crisis in theoretical physics gets worse, that will then be the time to call in the mathematicians?

   I'm not sure why you find the idea of a few mathematicians amongst the hundreds of physicists at Perimeter so threatening. Actually, I hadn't realized that Perimeter now has Mathematical Physics as one of its nine research areas. Between that and Nima Arkani-Hamed's interest in motivic Galois theory, the inroads of mathematics at

Perimeter may already be more advanced than I realized.

One thing I think we can both agree on is that it's very good that I'm not the director of Perimeter. For one thing, I don't have much in the way of good ideas about who they should hire. As for promising fields of mathematics, given the huge past success of symmetry arguments in fundamental theory, there's a name for the field of mathematics that deals with such symmetry arguments (representation theory), and that would be one obvious place to consider.

5. **Foster Boondoggle**
September 10, 2013

"nature turns out to be simpler than all of these models"

This seems like a very strange thing to say. It sounds like Turok thinks we've found everything out already, and it's not complicated! In context, he's talking about the combined discoveries of Planck (strong confirmation for the Lambda-CDM concordance model) and the Higgs, more or less capping the particle zoo of the Standard Model without providing any clues (yet) to what lies beyond. But to call this "simple" seems a misuse of the word. 3 generations of fermions? "Who ordered that?" A light scalar: how does that work? Another scalar that drives inflation. What is it? Stable probably heavy non-baryonic dark matter and a non-zero cosmological constant 10^-120 times its "natural" value. What are they? And how does all this hang together into a coherent whole? This is simple?

The crisis is the gap between the limits of the methods that served so well since the 1930s and the energies that we need to investigate. In the face of that gap, theorists are doing what they can. But they're not just confused – as Turok notes in the fuller context, they're depressed. He goes on to say things promoting the PI as a route to fulfillment, which is part of his job, so one can forgive his rhetoric. But what's your excuse? You wear your schadenfreude on your sleeve. It's an entirely worthy thing to mock hype, which you do ably. But it seems perverse to take pleasure in the hopelessness of so many who've been at the forefront of discoveries and theoretical inventions that have taken us so far, and are now stymied.

6. **Peter Woit**
September 10, 2013

Foster,

To my mind, Turok is just making the obvious point that we don't have any good ideas about how to get beyond the SM, that the heavily oversold ones of the last 30 years were always too complicated to be plausible, and now in addition have had their hopes for some experimental confirmation shot down, thus the "crisis".

My reaction to what Turok has to say is not "schadenfreude" at all, but optimism to see someone influential making these obvious points that I and many others have been making for years. He's quite right that the theory community should acknowledge the situation and act accordingly, not by being "depressed" that the unpromising ideas many had been working on are getting shot down, but by looking for something new to try. To be blunt, I have little sympathy for those "depressed" that the techniques they know and love have run out of steam and no ideas or interest in coming up with others. People working in this area are smart and capable of doing many things with their lives, and if they don't see anything worth doing in this field, they should move on to a different one and open up space for those who do have ideas they want to work on.

7. **Thomas Larsson**
September 10, 2013

Hehe. It is encouraging that people like Turok starts to acknowledge the crisis in physics. After another ten years of crisis, maybe people will even start to look into new non-trivial extensions of gauge and diffeomorphism algebras 🙂

But then again, whom am I kidding. We all know that the ultimate goal of theoretical physics is to worship Ed Witten by chanting magic and mystery.

8. **chorasimilarity**
September 10, 2013

When I was a kid I was sleeping with a CERN print and I was explaining the Hawking radiation to my friends by impersonating a black hole, eating cherries and spitting the kernels. Later I got converted to mathematics. Now, my impression is twofold: on one side, there's a lot of new math which could benefit to physicists and on the other side, boy, understanding biology is far more interesting and challenging than physics.

9. **Autism Skeptic**
September 10, 2013

Is there any legitimate evidence that Jacob Barnett is actually autistic. Highly intelligent, unusually behaving, late-talking, children are quite often misdiagnosed as being autistic when they are not autistic at all. Maybe Jacob Barnett got caught up in the Autism Dragnet.

10. **Bernhard**
September 10, 2013

Sorry for the useless joke, but "Nima Afkhami-Jeddi" is Nima "Arkanimedish" AND a Jedi?? This guy is set for life…

11. **fuzzy**
September 10, 2013

if i was a teacher in math, i would say that the counting is wrong, and even the evaluation of the perimeter is doubtful.

in fact, taking a renormalizable gauge theory su(3)xsu(2)xu(1) with sm content, the number of parameters is 20 not 18 (3 mixing, 1 phase, 9 masses, 3 parameters in the scalar sector, 3 gauge couplings, 1 strong cp phase). and i doubt that a reasonable counting of the parameters of a grand-unified-theory can be done, without declaring the theory that you want to discuss.

but what is worse is the statement "If you ask most theorists working on particle physics, they're in a state of confusion."…. "most theorists"? when, for god sake, mankind made a step forward in theoretical physics using polls?

12. **Peter Woit**
September 10, 2013

fuzzy,

I suppose one can argue that dimensional transmutation allows one to trade one of the gauge couplings for a choice of mass units, and the strong cp phase getting somehow set to zero isn't really an undetermined parameter but a structural aspect we don't understand. That would give the 18. A more serious problem is that this count doesn't include neutrino masses and mixing angles.

13. **Anon**
September 10, 2013

The strong CP phase has to be experimentally determined in the standard model and we only have good bounds on it — but we may yet discover that it is non-zero in the improved sensitivities of neutron and other EDM experiments in the next 5-10 years. So it is a bonafide parameter.

In the Higgs sector there is only its mass and VEV — so 2 parameters.

And then there are the neutrino mass and mixing parameters as well, but maybe these are being considered as beyond the standard model parameters.

14. **fuzzy**
September 11, 2013

hi friends, let me insist, assuming we are theorists. if we can agree on counting, maybe we can do physics.

the parameters ought to be counted by using the rules of the game. and the rule of the game are three: "gauge invariance, renormalizability, selected representations".

with these rules

1) neutrino masses are out from sm. this proves that the sm is not correct, but this is another story that does not pertain to the "counting of parameters in the sm".

2) the scalar potential contains a constant, that has to be counted (you can call it the cosmological constant, if you want to call it somehow)

3) the theta term is a honest gauge-invariant term, it is also there in the heap of parameters. all we know from experiments is that, assuming that the sm is correct, it is small.

the fact that we want to understand neutrino masses, cosmological constant, reason of conservation of cp in strong interactions (and we don't) does not change the counting of the parameters in the sm.

(apart from disagreement, i do not understand one of your arguments peter: if you consider dimensional transmutation, you do not change the number of parameters; alphas or lambdaqcd makes always one, from the point of view of counting)

15. **Bobito**
September 11, 2013

The mainstream of physics is still condensed matter and optics. There is no crisis there, rather constant and impressive progress. You do a disservice speaking of a "crisis in physics" when you mean something like a "crisis in particle physics" or a "crisis in high energy physics" (both probably still too overbroad).

16. **vmarko**
September 11, 2013

Fuzzy,

"2) the scalar potential contains a constant, that has to be counted (you can call it the cosmological constant, if you want to call it somehow)"

Please, please don't call that parameter the "cosmological constant". At the very least, calling it that way is very bad terminology, as people might confuse this parameter with the actual experimentally observable cosmological constant.

Adding to your rules of the game, I'd just note that:

(a) gravity is to be considered BSM (due to your renormalizability requirement) and consequently there is no cosmological constant (the real one) in the SM,

(b) the constant parameter in the action is completely unobservable within the SM, and should not be treated as a bona fide parameter.

The additive constant in the SM action (or any other flat-spacetime theory) can couple only to gravity, which is BSM by definition. Without gravity, this parameter is not observable, and can be freely set to anything you want, including zero. So it can be removed from the SM with no difficulties, and it should not be counted as a proper parameter.

And again, please don't call it the cosmological constant — the *real*, observable cosmological constant is a parameter in GR, it measures the non-flatness of spacetime, and has absolutely nothing to do with the constant parameters in flat-spacetime theories like the SM.

Best, 🙂
Marko

17. **fuzzy**
September 11, 2013

marko i agree that it is better to say that the sm has 19 *observable* (or potentially observable) parameters.

i am instead confused by the statement that the cosmological constant is purely gr; perhaps you mean that gr is a self consistent theory that does not need the sm, and we can use some simpler treatment of the matter when doing cosmology?

thanks again! 🙂

18. **vmarko**
September 11, 2013

Fuzzy,

We're getting off-topic here, but since you asked…

"i am instead confused by the statement that the cosmological constant is purely gr;"

Why is that confusing? The cosmological constant is a parameter in Einstein equations of GR (and some other models of gravity, but let's not get into that), and is being measured through its gravitational effects. One particular effect of nonzero CC is that the Minkowski flat spacetime is not a solution of Einstein equations.

On the other hand, the SM is defined precisely in flat spacetime, which means that it ignores both GR and the CC itself. The SM action is defined up to an arbitrary additive constant, which cannot be measured in any SM experiment, and is therefore usually set to zero. Being introduced in flat spacetime, this constant has a priori nothing whatsoever to do with the CC.

Now, if one couples GR and SM (classically only, quantization has lots of issues), the arbitrary constant from the SM sector and the CC from GR sector sum up into a single resulting parameter, which is also called CC. Experimentally, the CC has been observed to be positive (by gravity-related measurements), leading to the acceleration of the universe. While one could in principle say that the free parameter from the SM gives a contribution to the observed constant, there is no other way to measure that parameter on its own (non-gravitationally). Therefore the easiest thing to do is to set it identically to zero, and have only one parameter to be called CC — the one which was present in GR from the beginning.

I hope now it is more clear. 🙂

"perhaps you mean that gr is a self consistent theory that does not need the sm, and we can use some simpler treatment of the matter when doing cosmology?"

I did not try to say anything about the self-consistency of GR, SM, cosmology or otherwise. 🙂

Best, 🙂
Marko

19. **Peter Woit**
September 11, 2013

Please, enough about the CC…

20. **imho**
September 11, 2013

Lets be fair. I see some in the HEP community stubbornly refusing to acknowledge reality… but I see many other prominent names doing exactly what Peter (and many others) have been suggesting. That is, looking for more fertile ground. What do you think this whole firewall business is about? Have you paid attention to Witten's work the last few years? Many of the saner minds in hep -are- looking for new approaches and directions. We should not expect a public mea culpa as that could harm all of science funding. But we should "privately" acknowledge and encourage the change of tack.

21. **Noname**
September 11, 2013

I'm just curious to know what Peter would do if SUSY is discovered in the next LHC run. In my opinion the only honorable reaction would be to close down this blog. Peter, if you answer, please don't say this will never happen. I'm interested in your reaction if it does happen.

22. **Peter Woit**
September 11, 2013

imho,
I disagree with you that this is a discussion that should only be held privately to protect funding. What has actually been happening over the past decade or so is that as the problems with string theory and SUSY have become more apparent, the refusal to publicly admit this, and the choice to start adopting multiverse pseudo-science instead, has damaged the interest of HEP theorists immensely. Look at it this way, if you're a non-HEP physicist trying to decide whether to hire in HEP theory, having HEP theorists saying that past ideas haven't worked out, but they're moving on and trying to find new ones is not a strong case to hire them, but it's something. If instead you're being asked to hire people who refuse to admit that what they've been working on doesn't work, you've got a very strong case not to hire them. I think Turok has it right: make lemonade.

Noname,
Unlike some physicists, I don't have a problem with the fact that sometimes I'm wrong about things. If the LHC sees the kind of SUSY that I don't think has ever been very promising, I'll have been wrong, and will discuss this not just privately, but publicly. I'll also be fascinated to try and understand the implications of what the LHC does see. If it turns out it's a complicated SUSY effective field theory of some random point in the string theory multiverse, and none of the various ideas about representation theory and physics that I've been thinking about go anywhere, I'll give up and do something else with my time. This might become a blog about number theory and Shimura varieties…

23. **Geoff**
September 11, 2013

Peter –

I'm not sure if you had a chance to listen to all of the video of Turok's welcome for the new students. A bit later in the video he says that what string theorists are really doing is playing around with different pieces of mathematics. So perhaps when Perimeter hires a string theorist, there really are hiring mathematicians.

24. **imho**
September 11, 2013

Hi Peter, we should hold a public discussion to what benefit? The public discussion over the last decade has been helpful no doubt, but it seems to me we're seeing power law behavior at a critical point. A new phase is coming. Is it still useful to air dirty laundry when most everyone agrees there was a problem and many are taking steps to correct said problem. Shouldn't we keep in in the family? Or perhaps we need to make a distinction between those changing tack and those stubbornly steering towards the iceberg… Maybe I'm wrong.

25. **Peter Woit**
September 11, 2013

Geoff,
Problem is that mostly string theorists aren't mathematicians or doing mathematics. Some of them are and that's great, but the assumption that string theory=mathematics is just wrong (as an extreme example, consider the string theory anthropic landscape).

Actually I see that Perimeter divides things in a reasonable way, with a “Mathematical Physics” research area and a “Quantum Fields and Strings” research area, with only some of the QFT+string theory people listed in both.

26. **Cliff**
September 11, 2013

Im completely flabbergasted at the things Turok thinks he’s justified in saying. As Foster rightly points, he speaks like someone who’s had some kind of divine revelation of the solutions to all nature’s problems. The known problems with the Standard Model in 2013 are just the exact same problems that were known in 2003, and Neil Turok is in no position to declare the solutions to these problems “simple” or “complex” or anything else, because they remain *unsolved* problems!

Besides being confused about the difference between fundamental and effective field theories, the difference between parameters and vacua and “laws of physics” (confusions all also promoted by this blog) Neil’s main thesis is also complete nonsense. There is only exactly one thing that could be called a “crisis in physics” and its really more of crisis in economics: the fact that substantially novel data costs on the order of $10 billion or more to obtain. There is no “crisis” in having a description of nature that works perfectly well up to the energies that can be accessed by such a machine (if that is the case, which is far from certain). There is no “crisis” in having accumulated detailed parametrizations of as many scenarios as possible that are known to be consistent with all existing experimental data. And certainly there is no crisis that will be solved by stopping theorists from constructing new models.

Its natural that some people will find that the most powerful BSM ideas unpalatable to their intuition, and theres nothing wrong with that. But if this intuition is grounded in truth then there will be some other framework from which we can derive QM, GR, Yang-Mills, the Higgs mechanism, and every other major component of empirical reality, and that framework will have nothing to do with SUSY or strings. Well I would be very interested to learn that you’ve found such a completely new framework, but I don’t think we ever will, because I think that intuition is obviously wrong. But in either case, for now, thats all it is: intuition.

27. **donald**
September 11, 2013

Fiber Bundles! That’s where it’s at!

28. **fred**
September 11, 2013

Thanks for the post. I also agree that more mathematical physicist (people like Wigner) who has mathematical vision and physics intuition can be really useful. I also agree that string theorist can not replace mathematicians in the field. To my understanding they just play with this and that, not necessarily having the deep mathematical vision.

Additionally, as a person who was in the field, I would say I did not see much flexibility in accepting new ideas. I am sure the next Einstein is already changed his field to “finance” or something else.

For a fundamental breakthrough, I believe that we need more mathematicians, more open positions, more passion, and less dogmatism.

29. **Bob Jones**
September 12, 2013

“the assumption that string theory=mathematics is just wrong (as an extreme example, consider the string theory anthropic landscape).”

Calm down about the multiverse! Hardly anyone is even studying that stuff. Probably around 80% of the papers on hep-th are looking at formal, mathematical problems in QFT and string theory. You’re so obsessed with this multiverse business that you’re completely ignoring the reality of this subject.

30. **Bernhard**
September 12, 2013

Bob Jones,

The multiverse stuff might not be in the hep-th front line
(and here I have serious doubts… check this out: http://arxiv.org/find/all/1/all:+multiverse/0/1/0/all/0/1)
, but in any case certainly is in the of the spotlight of the outreach arena, which is the “welcome card” of our field. Almost every popular book and article mentions it, it appears on TV in several programs, the list is unending. And, it seems to me that the number of bullshit hep-th papers is increasing and not decreasing. A sign of the times is to see a talented fellow like Sean Carrol writing some crazy article about “Botltzmann brains”.

Peter might be more concerned than the rest of the field, who so far seems to just ignore as an embarrassment not worth mentioning. Whether ignoring will make it go away or make it worse (increase bullshtiness in hep-th), time will tell.

31. **Peter Woit**
September 12, 2013

Bernhard/Bob Jones,

The multiverse was only being mentioned here as an extreme example, no need for more discussion of it here. More to the point, my comment was about string theory, and Bob Jones responds by arguing about “QFT and string theory”, which, if you care about what words mean, is something different.

If anyone has some actual data on what fraction of hep-th articles are about string theory and are of the sort that a mathematician would describe as serious mathematics, that would be interesting. Otherwise though, enough…

32. **Bob Jones**
September 12, 2013

String theory is not mathematics, I agree with you about that. But it’s still *mathematical* in the sense that most of the time string theorists do not even attempt to postulate new physics. Instead, string theorists are generally interested in conceptual questions in quantum gravity and applications to formal problems in quantum field theory. They’re not coming up with new laws of physics but deducing consequences from the theories that are already known to be relevant for describing nature.

33. **JK**
September 12, 2013

Although it is good to hear someone from the Theoretical Physics community admits to the current crisis, one is tempted to think it might not be an accurate reflection of what physicists at PI believe. I have met with several PI faculty members and students. They generally and mostly (not exclusively though) expressed a snobby attitude. I believe members of the String Theory group at PI must be convinced that String Theory is the way, otherwise they would abandon it. If Turok is really serious about working on finding a solution to the current crisis, then a plan for adopting new directions should be implemented. One thing could be hiring mathematicians, like you suggested. I also think it would be very useful if there are more practical projects invested in, for instance projects that can provide theoretical predictions verified by EXPERIMENT. To my knowledge there is one or maybe two theorists at PI working on problems that may (?) connect to experiment. PI can express greater interest in Condensed Matter Physics and AMO Physics. For now, I feel probably the most useful work at PI is in the context of Mathematical Physics. Also, there could be projects held in connection with the philosophy of physics. After all, philosophy and physics were connected for a long time.

I would be interested in your opinion, Peter.

34. **LogicFan**
September 13, 2013

My gut feeling, somewhat along the lines of what Jess Riedel and JK allude to, is that the crisis will be solved not by physics but by philosophy. I do not know intellectual history well, but certainly we have found ourselves in this position before–apparently stymied, unable to progress. Yet our problems were, in time, solved by a deep, simple, and in retrospect obvious philosophical insight. Whether it's the Copernican Revolution or Einstein's insight into (e.g.) the relativity of time, cleverness or technical skill didn't do the trick. Instead, it was a willingness to think "outside the box" and against the orthodoxy.

If I were a physicist, I would study intellectual history to identify situations similar to the one we find ourselves in now. Then I would examine how those situations were eventually resolved, with an eye toward the general philosophical outlook, the conceptual leaps, etc. that were involved. That would not provide us a solution but at least would suggest the approaches and persons likely to succeed.

On these grounds, string theory strikes me as dubious. It strikes me as similar to the ill-fated effort to explain the geocentric orbits of the planets through ever more complicated mathematical systems of epicycles upon epicycles. Contrast this to Einstein; he didn't invent any new mathematics at all as far as I am aware. Instead, he took from Lorentz, and then from Riemann. But he just had that fundamental philosophical insight that was right for the time.

35. **fuzzy**
September 13, 2013

hi LogicFan, i agree with you, and would like to add a thing.

a trivial principle is that we should not support an idea in which we are not convinced, but it is much easier to avoid confrontation with ideas and with people, and rather, accept "this" since it is fashionable, "that" since it gives me a grant, or since the research center where it is studied is so respected, etc.

we need critical and open discussions among physicists! if a colleague working in condensed matter, or astrophysics, is not convinced by the validity of a certain plan or idea concerning particle physics, this is a good reason to reduce fundings. i mean, physicists should take responsibility of a serious and responsible community, we cannot accept self-referencing.

the usual objection i receive to this description is "it cannot be so"! but please, go to any specialized conference in strings or susy and tell me how many people come to progress in science or to understand something new, and how many to participate in the social event. (tip: one of the two numbers is usually zero)

coming back to the discussion of turok, i do not like his ideas. as i argued above, he is not taking theory seriously, because one cannot speak so loosely: "the sm has 18 parameters" (and as we discussed, this is not true: the parameters are 20, 19 of them are observable) or worse "this class of theories has 100 parameters, the other has 120" (which theories!!?? nobody cares of what he is speaking of???)

i highly dislike his argument "things are puzzling for its simplicity and this justifies our institute", since I am also a fan of logic, and this argument has nothing to do with logic, it is pure rhetorics! for me, what it is really puzzling is not that some people are free to discuss anything they want, but rather, that nobody ever asks them "but where all your predictions of yesterday have gone? what is your contribution to the physics?"

36. **Peter Woit**
September 13, 2013

JK/LogicFan,

Experimental condensed matter and AMO physics is pursued by most physics departments in the US and Perimeter's mission is explicitly to try and do something different. I also don't know of any promising ideas about experimental HEP or cosmology work that are not being already actively pursued.

As far as philosophy goes, since its founding Perimeter has had much more involvement in that (via Lee Smolin, for instance) than just about any other physics institute I know of. This is also a topic that has been well-funded in recent years (see the Templeton-funded efforts in "Philosophy of Cosmology" as an example). I'm a skeptic about prospects for this sort of thing, but one should try everything….

37. **Bob Jones**
September 13, 2013

"If I were a physicist, I would study intellectual history to identify situations similar to the one we find ourselves in now."

I have nothing against philosophy, but this idea that physics is going to be saved by some "simple, and in retrospect obvious" philosophical insight is just ridiculously naive. Physicists are well aware that progress may require radical new perspectives. However, progress also requires some understanding of how our current theories work and what sorts of extensions they admit. We're not going to make progress by sitting around and trying to imagine ways in which the current situation might be similar to that one that existed in the time of Einstein.

"It strikes me as similar to the ill-fated effort to explain the geocentric orbits of the planets through ever more complicated mathematical systems of epicycles upon epicycles."

You can always count on some layperson comparing string theory to epicycles on this blog, and it always exposes ignorance. One of the implications here is that string theory is some sort of ad hoc construction that can be modified in any possible way. On the contrary, many people are interested in string theory precisely because of its uniqueness properties. The comparison to epicycles also suggests that string theory might be abandoned some day. It should be obvious to any honest person with minimal knowledge of the subject that string theory is here to stay. Even if it doesn't provide a successful theory of everything, it's taught us a lot about how quantum gravity works conceptually, and it's one of the main tools of theoretical physics.

38. **Martin**
September 13, 2013

Looking at history, its not philosophy that physics needs, but relevant experiments. Theoretical physics should go back to phenomena that can be observed in experiment. String theory and quantum gravity are of course attractive to mathematically inclined physicists, but these topics do not bring physics any further. Sad, but true.

39. **A.J.**
September 13, 2013

@Martin:

I agree completely that what theoretical _particle_ physics needs right now is new data (and lots of it). However, I see nothing wrong with studying string theory and quantum gravity within the context of mathematical physics. We've learned a good bit about the mathematical structure of quantum field theories by doing this. For example, Seiberg & Witten's demonstration of confinement in an interacting 4d QFT and Komargodski & Schwimmer's proof of the a-theorem

Bob Jones and others are completely right that most people who study string theory are working on this sort of thing, not on the multiverse and similar sorts of nonsense. It's unfortunate that we spend more time discussing this garbage than we do talking about real advances in scientific understanding. But what do you expect? Real advances in scientific understanding are rare and frequently difficult to understand, while nonsense is easily produced and easily discussed. What would you choose if your living depended on getting people's attention?

(Question for Peter: Are you really doing the world a favor by bringing this stuff up all the time? I wouldn't be surprised to learn that your blog probably has a wider readership than the many/most of the press releases and pop sci articles you find. Is it possible that you're effectively feeding the trolls by covering this stuff?)

40. **LogicFan**
September 13, 2013

Bob Jones, in my judgment the comparison between string theory and epicycles is apt. Our disagreement about this is not due, it seems to me, to my "ignorance"; rather, I think that you are committing logical errors. That said, if it were due to my ignorance, I would likely not be aware of this fact; we all posses a degree of epistemic self-blindness. In the end, one of our positions will be vindicated, and one will not.

As an example of your logical error, we can accept both your criticisms and yet still maintain the insight that string theory is like epicycles in the relevant way. That is, the objection is not that string theory hasn't been useful, or that it isn't "here to stay", as you put it, but that it appears to be the wrong *type* of theory. The argument runs as follows:

1) There have been numerous points p1 . . . pn in history (not including the current crisis) in which physics has found itself stymied, apparently unable to make progress.
2) For each of these p1 . . . pn there eventually was found a solution, s1 . . . sn, respectively.
3) Each of the s1 . . . sn share common traits T1 . . . Tm. Among these is that the key insight, what really made the solution possible, was a deeply conceptual, "philosophical" insight. None of the T1 . . . Tm is that the key insight, what really made the solution possible, was an increase in mathematical complexity and technical cleverness. (If there was an increase in mathematical complexity and cleverness, it did not enable the discovery but rather was a side-effect of it.)
4) String theory contains no deeply conceptual, "philosophical" insights. It does contain many increases in mathematical complexity and cleverness.
— —-
5) String theory is unlikely to be correct.

That's the argument, anyway. An intellectual historian could weigh in on the truth of the premises.

Another logical error that you commit, which string theorists commit frequently, is that you seem to think that all of us who disagree with you are stupider than you. That's both a error in reasoning and factually false. You may well be smarter than me, but how smart can all these string theorists be if they keep committing simple, symbolic logic 101 errors in their reasoning?

41. **Peter Woit**
September 13, 2013

LogicFan/Bob Jones,
Sorry this exchange has reached the point of zero worthwhile content, I'll delete any attempts to pursue it further.

42. **Rauha**
September 13, 2013

Epicycles shouldn't be compared to string theory. Its degrading.

The ptolemiac model was one the the most succesful scierntific theories ever. It made predictions, there were observations and it even had practical uses.

43. **Peter Woit**
September 13, 2013

AJ,
Actually, I'm regularly resisting writing about string theory related nonsense in pop sci articles. Just today there was
http://www.newscientist.com/article/mg21929342.800-supergoop-universe-offers-a-window-into-glassy-physics.html
which tells us that
"Welcome to the supergoop universe. This hypothetical reality derives from string theory, which allows for a large number of possible universes, each with different physical laws."
which I have no intention of discussing.

The reason that the media is full of multiverse nonsense is not because of my blog, but because very prominent people in the physics community are aggressively pushing it. As long as this is going on, I think there needs to be someone pushing back and making clear the problems with this point of view. I don't see anyone else doing this. By the way, are you calling Sean Carroll, Nima Arkani-Hamed, Savas Dimopoulos, Lenny Susskind, Andrei Linde, Steven Hawking and Frank Wilczek trolls?

I think we agree that some string-theory motivated research is good mathematical physics. The problem is that as long as people refuse to admit that parts of the string theory research program are a failure, there's an associated failure to be able to distinguish between unpromising things whose motivation is a failed idea and more promising ideas.

44. **A.J.**
September 13, 2013

* By the way, are you calling Sean Carroll, Nima Arkani-Hamed, Savas Dimopoulos, Lenny Susskind, Andrei Linde, Steven Hawking and Frank Wilczek trolls?*

If the shoe fits.. (Also, less than half of those people are string theorists. Perhaps it's cosmologists you have a problem with? Or Palo Alto residents?)

As for the goop, that's a classic example of gee-whiz pop-sci writing doing a discredit to an interesting scientific analysis. Interested parties should read the paper instead. No mention of the term 'universe', and the authors make it quite clear that they think the main use of these models is that they exhibit features analogous to those seen in real world condensed matter systems. Not much different from thinking about the Ising model as toy model for salad dressing.

45. **vmarko**
September 13, 2013

There seem to be many different interpretations of what Turok actually said. But after I watched the actual video, it seems to me that he was asking for one thing every physicist would just love to have, but none do so far — a working theory with a smaller number of free parameters.

The Standard Model has 18-20 or so parameters (depending on how one counts), and creating a BSM theory which has a 120 or 10^500 free parameters is obviously not a step forward. What Turok was asking for is that we need to formulate a BSM model which *reduces* the number of free parameters in the SM. This can be done only by figuring out and formulating some deep principle that nature obeys, which would be powerful enough to establish relationships between existing parameters, thereby explaining some of them in terms of the others.

The only example of a model that even remotely comes close to this goal is the noncommutative SM developed by Alain Connes and his collaborators, which AFAIK features one extra equation relating the particle masses in the SM (IIRC, it has one free mass parameter less than the SM itself).

Also, we should all keep in mind that Turok was addressing very young freshmen students, so his words should be understood within that context. He was giving a lecture to students about what is important in hep-th research, and what it means to make a prediction — to cover as much as possible of the experimental data, with more concepts and less free parameters to be adjusted.

One of the most vital things that a hep-th researcher should know is to distinguish the physical concepts that nature satisfies from the mathematical formalism used to express those concepts. Students should be aware that we need new principles, rather than more complicated formalisms. The state of the art today is that formalisms are abound in hep-th, while concepts are very few and hard to come by. Turok is very right to call this a crisis.

And it's not just about the lack of new experimental data, it's also about lack of understanding of the already existing data. String theory has 10^500 parameters to fit the data, SUSY models have 120 or more parameters to fit the data, the SM apparently fits all data with just 18-20 parameters. The real challenge is to make a theory that will fit all data with *less* than 18 free parameters. Only *that* would constitute a real progress in understanding nature. Everything else is just formalism, bells and whistles, cosmetics, and free-parameter-fitting.

HTH, 🙂
Marko

46. **fuzzy**
September 14, 2013

hi marko, i do not insist on the counting, but only on the issue that we should respect the valid principles, and "gauge principle" is what i had and i have in mind, with my pedantic counting. i note that grand unified models are gauge theories, i.e., theoretical constructs based on valid ideas. their exploration cannot be said to be complete. the reason why they have been largely ignored in the last 10 years or so is the so called "naturalness principle", that i do not think deserve as much respect as "gauge principle" (i know that other people disagree with me here). from the counting of turok, the grand unified theories do not deserve credit because they have "100 parameters". i think this is a superficial statement, just as the counting of the sm parameters, without considering what the sm is: a well defined gauge theory. i think this kind of opinions can mislead freshmen students. with friendship 🙂

47. **Urs Schreiber**
September 15, 2013

A.J. is right.

48. **Shantanu**
September 16, 2013

OT: Peter : any comments from any of the talks from the firewall conference
at KITP? I watched Bill Unruh and Bob Wald's talks where they were critical of ADS/CFT
and they were met with some hostility from string theorists

Trust the math?

September 11, 2013
Categories: Uncategorized

The last few days have seen some new revelations about the NSA's role in compromising NIST standard elliptic curve cryptography algorithms. Evidently this is an old story, going back to 2007, for details see Did NSA Put a Secret Backdoor in New Encryption Standard? from that period. One of the pieces of news from Snowden is that the answer to that question is yes (see here):

> Classified N.S.A. memos appear to confirm that the fatal weakness, discovered by two Microsoft cryptographers in 2007, was engineered by the agency. The N.S.A. wrote the standard and aggressively pushed it on the international group, privately calling the effort "a challenge in finesse."

The NIST has now, six years later, put out a Bulletin telling people not to use the compromised standard (known as Dual_EC_DRBG), and reopening for public comment draft publications that had already been reviewed last year. Speculation is that there are other ways in which NIST standard elliptic curve cryptography has been compromised by the NSA (see here for some details of the potential problems).

The NSA for years has been pushing this kind of cryptography (see here), and it seems unlikely that either they or the NIST will make public the details of which elliptic curve algorithms have been compromised and how (presumably the NIST people don't know the details but do know who at the NSA does). How the security community and US technology companies deal with this mess will be interesting to follow, good sources of information are blogs by Bruce Schneier and Matthew Green (the latter recently experienced a short-lived fit of idiocy by Johns Hopkins administrators).

The mathematics being used here involves some very non-trivial number theory, and it's an interesting question to ask how much more the NSA knows about this than the rest of the math community. Scott Aaronson has an excellent posting here about the theoretical computation complexity aspects, which he initially ended with advice from Bruce Schneier: "Trust the math." He later updated the posting saying that after hearing from experts he had changed his mind a bit, and now realized there were more subtle ways in which the NSA could have made number-theoretic advances that could give them unexpected capabilities (beyond the back-doors inserted via the NIST).

Evidently the NSA spends about $440 million/year on cryptography research, about twice the total amount spent by the NSF on all forms of mathematics research. How much they're getting for their money, and how deeply involved the mathematics research community is are interesting questions. Charles Seife, who worked for the NSA when he was a math major at Princeton, has a recent piece in Slate that asks: Mathematicians, why are you not speaking out?. It asks questions that deserve a lot more attention from the math community than they have gotten so far.

Knowledgeable comments about this are welcome, others and political rants are encouraged to find somewhere else. There's a good piece on this at Slashdot…

Comments

1. **Geoff**
   September 11, 2013

   Interesting to note how little things have changed from the time of the Church Committee. From page 2:

   "At the same time, we must insist that these agencies operate strictly within the law. They were established to spy on foreign governments and to fend off foreign spies. We must know to what degree they have turned their techniques inward to spy on the American people instead. If such unlawful and improper conduct is not exposed and stopped, it could, in time, undermine the very foundations of freedom in our own land.

   So the committee intends to hold public hearings, not only on the domestic abuses of the CIA, and the FBI, but on the improper activities of such other Government agencies as the Internal Revenue Service, the Post Office, and the National Security Agency."

   http://www2.gwu.edu/~nsarchiv/NSAEBB/NSAEBB58/RNCBW25.pdf

2. **CFT**
   September 12, 2013

   I think Benjamin Franklin said it most clearly:
   "They who can give up essential liberty to obtain a little temporary safety, deserve neither liberty nor safety. "

   I believe we are well into the 'deserve neither liberty nor safety' side of things now. I don't feel very safe, or liberated in light of the revelation of our government's new homeland pastime. The Yahoo CEO is now saying they were basically threatened into submission. The silence on this issue is deafening because 'speaking out' as you put it is now a crime.

   http://www.theguardian.com/technology/2013/sep/11/yahoo-ceo-mayer-jail-nsa-surveillance

3. **Joseph Zizys**
   September 12, 2013

   Just a run of the mill ignoramus here, but I would be very surprised if any significant advances in number theory pertaining to crypto have been made secretly by the NSA. It is just hard to imagine, regardless of how much money they spent, that anyone could really develop in a vacuum like that. How many genuinely promising young mathematicians have "disappeared" to work exclusively for the NSA? And I don't just mean "this kid might make a good grad student for that idea I had last year" promising, I mean Hardy promising if not Ramajunan promising. How many Terry Tao's are whisked away each year by the NSA to push the field forward in secret? It seems far more likely that good old fashioned spycraft, politics and manipulation of standards bodies will be the way to compromise standards and institutions now and in the future, rather than fundamental mathematical research somehow pursued in secret rooms by nebulous "geniuses" who where somehow missed by the universities of the world for long enough to be hoovered up by the NSA.

4. **Bobito**
   September 12, 2013

   Mathematicians who work for the NSA have blood on their hands just like those who used to work to build bombs. Let's stop giving these funding hungry leeches respect and promotions. They are working to destroy what remains of democratic society.

5. **Joel Rice**
   September 12, 2013

crypting is peanuts compared to mapping out the social networks. And if the devices attached to providers are just splitting off the fiber, then they get all the raw stuff anyway. And now it is reported that Israel is getting raw feed – we don't even know who gets to play with all this stuff.

6. **Jeff M**
   September 12, 2013

   I think it's important to remember that what is being talked about here is not the NSA making some sort of mathematical leap which allows them to break encryption, but rather the NSA inserting weaknesses into the encryption scheme from the start. Certainly that would involve doing some tough math, but it's a different sort of thing then breaking an existing encryption scheme. I doubt very seriously that the NSA has figured out how to factor large numbers in polynomial time. Anyone remember the movie "Sneakers?"

7. **Jeff M**
   September 12, 2013

   Joel,

   The raw feed does you no good if it's properly encrypted, that's the whole point of https: You can protect yourself, use PGP for email, Tor to browse, etc. Any traffic you have with the web using https is safe (unless the NSA HAS figured out how to factor very large numbers in polynomial time :-), though of course anything password protected (like say your FB page) is hackable unless you're very careful about passwords.

8. **wolfgang**
   September 12, 2013

   @Jeff M

   nothing on your list is safe from the NSA.

   PGP: It seems the NSA can break key up to length 2048 and most passwords are not very safe to begin with.

   Tor: The NSA can do end-point correlation analysis and recently an FBI attack compromised a large number of exit nodes.

   https/SSL was explicitly mentioned as one of the protocols undermined by NSA.

   As for your FB password: The NSA does not even need it because the large US web companies cooperate with them anyways.

9. **Peter Woit**
   September 12, 2013

   Joseph/Jeff M,
   I think if you look into this carefully (and this is what Scott Aaronson started learning after his initial posting), you'll find that the situation is much more complicated than you think. The problem is not as simple as "have NSA mathematicians made revolutionary advances such as polynomial time factoring?". The implementation of standard encryption algorithms is quite a complicated story, and the possibility of NSA mathematical advances that exploit weaknesses in the implementations (either unintentionally there or introduced by the NSA through their abuse of the NIST processes) is very real.

   For any particular encryption algorithm, the story of possible attacks on it can be quite complicated. One should also keep in mind that when you see "https", telling you that some sort of encryption is going on, that doesn't tell you which algorithm is being used. From experience, I can assure you that the question of configuring which SSL encryption algorithms are supported and used on a webserver is not a trivial one.

10. **Jeff McGowan**
    September 12, 2013

    Peter et al

    I'm sorry, I really doubt the NSA can break RSA if it's properly done. There are hacks which attempt to get around the polynomial time problem, and as Peter points out https can mean different things, some of which are better than others. Passwords are very hackable, unless you're very good about it, and as Wolfgang says Tor is only so good, it just means they have to spend a lot more time to get in, and odds are they can't get nearly as much. That said, anything you do like that means the NSA has to devote much more time to you. Wolfgang, what's your reference for the NSA being able to hack 2048 bits? That would seem to me to be too much…

11. **wolfgang**
    September 12, 2013

    @Jeff McGowan

    Bruce Schneier, who has seen the Snowden documents, wrote on his blog:
    "We're already trying to phase out 1024-bit RSA keys in favor of 2048-bit keys. Perhaps we need to jump even further ahead and consider 3072-bit keys."
    He later published a new public key of length 4096.

    Btw Symantec now owns PGP and is a company known to cooperate with the NSA.
    If you use PGP you should use a program compiled from the original source with a compiler you fully trust.

12. **Jeff McGowan**
    September 12, 2013

    Wolfgang, thanks, I'm going to have to look into this. And you're right about PGP and sources,on the Mac at least you can just compile the GNU version yourself…

13. **John Urbanik**
    September 12, 2013

    Spending money and employing mathmaticians is a good thing. Like all military and secret technology if they have come across some radical new math it will eventually become known to all and then all of us can benefit.

14. **Cplus**
    September 12, 2013

Phil Zimmerman, who originated PGP cryptography, left the company when Symantec took over in 2010. Last year he founded a start-up to provide secure communication.
It is worth recalling that last month, he terminated the email component of his service because even he could not assure privacy in the existing legal and technological system.
It will take years at best before a legal and technological framework can be created in which realistic privacy will be possible, which is most likely to emerge from the open source world.
Zimmerman's analog of Moore's law: the ability of computers to track us doubles every eighteen months.

15. **gs**
September 12, 2013

Evidently the NSA spends about $440 million/year on cryptography research, about twice the total amount spent by the NSF on all forms of mathematics research.

When I got pulled into the military during the Vietnam War, I didn't end up carrying a rifle through a rice paddy. Neither did a fellow soldier who had gotten drafted out of a highly ranked graduate program in mathematics.

According to the Mathematics Genealogy Project, he has written his thesis. I see nothing else definitive about him online.

No conclusion can be drawn from this single instance, but I wonder how many other mathematicians have, in a sense, gone missing.

16. **David Derbes**
September 12, 2013

Maybe some mathematicians were not visible long enough to go missing. If you've seen the film *Zero Dark Thirty* the fictional character Maya (a composite) is described as having been *recruited out of high school*. Whether this is believable or not I do not know; if so, the question arises: How could the talent spotters have found her? There are all manner of Math Camps and competitions (at least one of which if I recall correctly is openly sponsored by NSA) that might in principle identify very talented young people, and perhaps even provide tuition and mentoring in math, in exchange for national service for a period of 3 or 5 years. NSA made a half-hearted pass at me in college; I was doing physics, Russian, Greek and math. The chair of the Slavic Languages department told me (1973) about an exam I might want to take one Saturday, offered by NSA. I declined.

17. **Philip Gibbs**
September 12, 2013

GCHQ like to use code cracking competitions to attract "curious, tenacious and creative candidates who have the intellectual ability, though not necessarily the practical experience or qualifications" They launched a new one yesterday see http://www.gchq.gov.uk/Press/Pages/solve-cyber-secret.aspx

This tactic may net some talented young mathematicians before they publish or start a doctorate.

18. **milkshaken**
September 12, 2013

SSL gets cracked by NSA through man-in-the middle type of attacks, by employing fake certificates – no nimble math magic there.

19. **Haim**
September 13, 2013

The NSA employs more mathematicians (between 10 and 20 000) than the rest of the world combined. It is also well known that the NSA knows more about number theory than the rest of the world combined.

The back doors in its algorithms are known in expert circles since more than a decade. They are used by European cryptography producers as sales argument to convince customers not to buy American cryptography products. And indeed, no European country does so, for any sensitive equipment (communications to embassies, military encryption). What the NSA does in return is also known since decades (hiring spies as technicians in these companies, etc).

The naiveté of the doubters here is appalling.

20. **Jim Akerlund**
September 13, 2013

Peter,

In the interests of full disclosure, what has your brush been with the NSA, if one exists? I know sometimes you bring up math related subjects for the social relevance. Is that the reasaon your writing about this?

21. **Geoff**
September 13, 2013

Apparently, getting recruited out of high school isn't entirely unheard of.

http://www.forbes.com/sites/stevenbertoni/2011/09/21/sean-parker-agent-of-disruption/4/

22. **Peter Woit**
September 13, 2013

Jim,
I've never had any contact with the NSA that I know of (although, from what one reads, presumably like everybody else I've had a large amount of contact with them that I don't know about…). This posting was just meant to inform others about something that seems to me an issue that should be of general interest in the math and physics community.

I saw some place quote me as an "expert" on this, and I'd like to make clear that that is not at all the case. I know a little bit about the mathematics involved and about computer networks, but have no expertise on either topic.

23. **Geoff**
September 23, 2013

"Meanwhile, over in Building 5300, the NSA succeeded in building an even faster supercomputer. "They made a big breakthrough," says another former senior intelligence official, who helped oversee the program. The NSA's machine was likely similar to the unclassified Jaguar, but it was much faster out of the gate, modified specifically for cryptanalysis and targeted against one or more specific algorithms, like the AES. In other words, they were moving from the research and development phase to actually attacking extremely difficult encryption systems. The code-breaking effort was up and running.

The breakthrough was enormous, says the former official, and soon afterward the agency pulled the shade down tight on the project, even within the intelligence community and Congress. "Only the chairman and vice chairman and the two staff directors of each intelligence committee were told about it," he says. The reason? "They were thinking that this computing breakthrough was going to give them the ability to crack current public encryption."

http://www.wired.com/threatlevel/2012/03/ff_nsadatacenter/all/

24. **Chris W.**
September 25, 2013

See this September 23 interview with Bruce Schneier in *Technology Review*:

> **Schneier**: We're not there yet, but already we've learned that both the DEA and the IRS use NSA surveillance data in prosecutions and then lie about it in court. Power without accountability or oversight is dangerous to society at a very fundamental level.

Susskind: String theory not a complete picture of how quantum gravity works

September 16, 2013
Categories: Uncategorized

For the latest on quantum gravity, readers might want to look at talks from some events of the last couple weeks. At the new ICTP-SAIFR theoretical physics institute in Sao Paulo, a school on quantum gravity has talks available here, with a follow-up workshop here. At Stanford last week the topic was Frontiers of Quantum Gravity and Cosmology, in honor of Renata Kallosh and Stephen Shenker.

Matt Strassler was at the Stanford conference, and he blogs about it here, describing most of the speakers as "string theorists" who are no longer working on string theory, and most of the quantum gravity talks as not being about string theory (this is also true of the ICTP-SAIFR workshop). I don't really understand his comment

> Why has the controversy gone on so long? It is because **the mathematics required to study these problems is simply too hard** — no one has figured out how to simplify it enough to understand precisely what happens when black holes form, radiate particles, and evaporate.

since the problem isn't "too hard" mathematics, but the lack of a consistent theory (which he makes clear later in the posting).

Most remarkably, he described the talk by Lenny Susskind, one of the leading promoters of string theory, as follows:

> Susskind stated clearly his view that string theory, as currently understood, does not appear to provide a complete picture of how quantum gravity works. Well, various people have been saying this about string theory for a long time (including 't Hooft, and including string theory/gravity experts like Steve Giddings, not to mention various experts on quantum gravity who viscerally hate string theory). I'm not enough of an expert on quantum gravity that you should weight my opinion highly, but progress has been so slow that I've been worried about this since around 2003 or so. It's remarkable to hear Susskind, who helped invent string theory over 40 years ago, say this so forcefully. What it tells you is that the firewall puzzle was the loose end that, when you pulled on it, took down an entire intellectual program, a hope that the puzzles of black holes would soon be resolved. We need new insights — perhaps into quantum gravity in general, or perhaps into string theory in particular — without which these hard problems won't get solved.

For many years now, the most influential figures in string theory have given up on the idea of using it to say anything about particle physics, and results from the LHC have put nails in that coffin, removing the small remaining hope that SUSY or extra dimensions would be seen at the TeV scale. The "firewall" paradox seems to have made it clear that string theory-inspired AdS/CFT doesn't resolve the problem of non-perturbative quantum gravity, leading to renewed interest in other approaches. This leaves string theory now as just a "tool" to be used to study topics like heavy-ion physics. Things don't seem to be working out very well there either.

Comments

1. **P**
   September 16, 2013

   Per usual, overly negative and pessimistic.

   Matt's treatment is quite fair, though, also per usual. I'd recommend readers of your blog to look at his for a more balanced approach.

2. **lun**
   September 16, 2013

   As an orthogonal issue, but one related to something you discuss on this blog often, by the calibre of speakers and topics discussed, and the _level_ of the discussion, this conference could have been at the IAS, yet it is nearly entirely organized and financed by a "BRIC" country.
   Employment in an institute as prestigious as the ICTP (or, say, the Tata institute in India) is likewise becoming equivalent to employment in a large research level university in the US and Europe.
   Could this be the answer to the abysmal situation in the "first world" job market in theoretical physics? Moving to a country such as India or Brazil could become a realistic alternative to leaving physics for young people interested in basic science.
   Since employment in these institutions follows somewhat different administrative rules w.r.t. the US system, this could also lead to a research program that is less dependent on senior-initiated fads a la landscapes and firewalls.
   Could BRIC countries rescue theoretical physics?

3. **Bernhard**
   September 17, 2013

   lun,

   the "somewhat different administrative rules w.r.t. the US system" these institutions follow is a complete nightmare. If you want a job in a public institution in Brazil you have to have abundant time to waste to follow the endless and also saturated process of the so-called "concursos". Ah, and be prepared to speak fluent Portuguese by the time you apply, you won´t go far there with just English. I think we are slightly off-topic so let me stop here.

4. **Pete**
   September 17, 2013

   Background: the concurso is a long (all day, occasionally two) exam, usually written in Portuguese and then read out by the candidate to the committee, set by the university to which you apply on (usually fairly general) topics of their choice. If you apply lots of places, this takes a lot of time…

   The institutes (as opposed to the public universities) often don't do the concurso, rather their hiring tends to be very much US-style. You'll be expected to teach in Portuguese at some point, but you probably have at least a year to learn, and it's not hard (especially if you know Spanish, of which it's pretty much a dialect).

   Also the public universities (especially the better ones) are starting to let people do the concurso in English. You'll still need to be competent to pass, and you'll need to commit to learning Portuguese fairly soon. But the system isn't especially overloaded. If you know stuff outside your specialisation reasonably well, you can pass the concurso. If you only know stuff outside your specialisation at second-year undergraduate level though, either you have to cram once the topics are released or you will not pass.

   On the other hand, at a university the teaching load isn't especially nice, unless you like teaching evening classes for the part-time students (of course, if you really get into the culture then the class ends about the time you want to go out, get a meal and go on the town; and you won't be teaching before lunch the next day). And you probably don't want to have children there otherwise life gets too complicated.

5. **Bernhard**
September 17, 2013

Pete,

"But the system isn't especially overloaded."

It is not specially overloaded if you compare with the normal situation in HEP. And specially if you want to be in a center – the research situation outside the centers is not as nearly as decent. And about the "concurso", the form varies from university to university with places like CPBF having much more specific exams. There, by the way, it is a terrific place to work, with zero teaching obligations. But a piece of this cake is in my opinion as hard (or as easy) as getting a position in Europe. The rest is "easier", but not as nearly as interesting.

All in all, if you look at Brazilian faculties, there are usually 99% Brazilians working there. The situation might change a bit, but I doubt it in any drastic way in the near future.

Peter, sorry for highjacking the discussion – my last comment about this…

6. **Hernán Meier**
September 17, 2013

Folks,

Well this is my first post, but if you are considering to do a Ph.D in an underdeveloped country at the same level of top institutions in developed countries you can consider the Balseiro Insititut in Argentina (www.ib.edu.ar) it´s considered the best Physic and Nuclear related institute in Argentina and maybe the entire Latinoamerica.
Moreover, with the plus to be located in Bariloche city (Tahoe is awful compared to that place)
The main problem is the mathematical level they ask you to have the entry.
Hernán

7. **Arjun**
September 17, 2013

"Since employment in these institutions follows somewhat different administrative rules w.r.t. the US system, this could also lead to a research program that is less dependent on senior-initiated fads a la landscapes and firewalls.
Could BRIC countries rescue theoretical physics?"

I am from India and got my PhD at an US institution in theoretical physics. I can say with confidence that this would be a very bad career move—to try to move to India—even if you get a faculty position at TIFR or IISC or HRI. For one, you will be paid much much lower than what your colleagues pursuing private sector jobs will be earning in India, in Indian rupees. And unlike in the US where even as a physics graduate student you have access to pretty good standards of living, you will find yourself struggling to provide yourself and your family with basic luxuries. While you will be allocated a govt. built apartment within the campus itself, it will be of very dismal quality.

Not to mention the hierarchy nightmare over there in India. Indian culture is traditionally hierarchical. This coupled with the colonial history has resulted in a deeply hierarchical society. So your seniors will drive your research agenda. Moreover to get a faculty position over there you have to be "known" by these seniors and also be "liked" by them.

A few colleagues made such moves tempted by the recent "boom" in Indian economy and the promises of the govt. to put more money into research. One colleague told me to never, never come back to India; another said that he's decided to settle for it even though he knows he'll not do world-class research.

And given the fact that the economy is now tanking (much talked about in both Indian and international media these days), it remains to be seen if the funding promises will be fulfilled or not. Given my experience with India, most probably not!

8. **Peter Woit**
September 17, 2013

Well, at least string theory has given us this:

http://www.youtube.com/watch?v=2rjbtsX7twc

9. **Marcus**
September 18, 2013

Tim Blair! McGill, MS advisor was string theorist Alex Maloney. Totally awesome you must watch this, only 8 minutes. Thanks, P.W.

10. **Marcus**
September 18, 2013

correction: Tim Blais (aka "acapella science")

11. **abg**
September 18, 2013

[off-topic] The best Brazilian universities (USP and Unicamp) can still hire without "concurso" and knowledge of Portuguese (which is not a Spanish dialect!), but only English. However it is rare.

Also, there exists some kind of "intellectual communism" in Brazil, such that all professors are treated equally, in every aspect, independent of the quality of the work which they do. Once someone is approved in a "concurso", he or she can do absolutely nothing and still maintain his or her position. It is very difficult to fire a public worker in Brazil. And the result is a bunch of incompetent people occupying jobs positions, while the stimulated and talented scientist must stop his research to study for a "concurso".

12. **NumCracker**
September 20, 2013

@abg

Actually, it is a “pseudo intellectual communism” once you forget about the probatory time before getting tenured. On the other hand, research grants are attributed on the basis of productivity as anywhere in the developed world. Also, I recommend people interested in moving to South to write to Nathan Bercovits @IFT … he is an american, with high qualification standards in ST, which decided to do such a wise move some of guys here are describing. Nathan seems happy at IFT/ICTP and is still very productive here in Brazil. Would I start a PhD/Posdoc in HEP in South Hemisfere it would be there.
Regards

Num

13. **abg**
September 21, 2013

[Off-topic] I do not think that the probative stage helps very much. Indeed, once someone is approved in it he or she can do practically *no research* at all and have a guaranteed position (recall that the duration of a probative stage is around 2 years, and most academicians will work for 2 or 3 decades!). Even worse, a docent can be continuously promoted in Brazilian public universities but can never recede.

And about the research grants, it is true that it is based in productivity, but it is not necessarily a good thing since it is extremely normative! For example, the article in which Cesar Lattes changed completely the field of particle physics (he was one of the discovers of the meson pi) would have the same value as any article published in any magazine independent of the quality of the work; and it would have 1/2 of the value a published textbook, again independently of the its quality.

It would be better if the universities here could negotiate the grants and even in the academic position freely, that is, without any rigid governmental bureaucrat legislation, like many universities around the developed world (this is the case of USP and Unicamp, but is is an exception).

Let me finish by saying that, *however*, there exists brilliant scientists working at now in Brazil, and they are very product in a broader sense. See, for instance,

http://arxiv.org/find/math-ph/1/au:+Rodrigues_W/0/1/0/all/0/1

14. **abg**
September 21, 2013

Another remark: Nathan Jacob Berkovits works at UNESP, which is a state university of São Paulo, like USP and UNICAMP. I would say that these 3 SP’s State Universities are more or less an exception to my comments above.

15. **NumCracker**
September 21, 2013

@abg

While our exchange is completey off-topic it may be helpfull for some enthusiasts wanting to definetely move, or just doing a pos-doc, bellow the equator 😉 So I intend to finish my collaboration by just saying that other interesting (exceptional) federal institutions are: UFRGS, ITA, UFABC, UFRJ, UFMG, UNB, UFPE, CBPF. In addition, researchers have decent computational infrastructure provided by SINAPAD/MCT and free-access to a huge national base of scientific periodics (by CAPES.)
The really accurate numbers are 4 years before getting tenured, 35 years of service before retiring. The average salary is about US$ 50k up to 100k depending on the hierarchy. Even after tenured, one just progress on such carrer by commitment and effective productiveness during decades. One would expect to spend at least 8h/w by lecturing, but more realistically one would consider about 12h/w. It is not a quite dynamic scientific environment as (*part of*) Europe or USA, but with that salary (and a properly chosen place to live) life is surely nicer.
Best,

Num

16. **abg**
September 22, 2013

I agree with the information in the latter post, which are indeed accurate. Except a detail: it is in fact possible to be promoted in a public university in Brazil without (a high) productivity, but just with sufficient “time of service”, that is, being associated in the university for a time long enough.

Finally, if some academician is interested in coming to Brazil to work in any (public) university, I recommend the reading of the following blog, maintained by a Brazilian logician, who wrote (and still write) a lot of things about the life in science here: http://adonaisantanna.blogspot.com.br.

Physicists Discover Geometry Underlying Particle Physics

September 18, 2013
Categories: Uncategorized

Today's Slashdot tells us that Physicists Discover Geometry Underlying Particle Physics, a story that is based on an excellent article, A Jewel at the Heart of Quantum Physics, by Natalie Wolchover at the new *Quanta Magazine* sponsored by the Simons Foundation.

As you might suspect, the Slashdot headline is simply nonsense. What's really going on here is some new progress on computing scattering amplitudes in a very special conformally-invariant QFT, one not known to "underly particle physics". This is a long story, one going back to Roger Penrose's work on twistors from the late 1960s. In recent years this has been a very active and successful field of mathematical physics research, with a large group last year putting out Scattering Amplitudes and the Positive Grassmanian, which showed how to express some amplitudes to all loops in terms of volumes of geometric objects defined as subspaces of a Grassmanian. Mathematicians who want to see some speculation about the relation of this to other areas of mathematics should take a look at section 15 of that paper.

The more recent news is that Nima Arkani-Hamed and his ex-student Jaroslav Trnka now have an improvement on that calculational method, which uses the volume of a particular such geometric object they call the "Amplituhedron". There's no paper yet, but you can watch recent Arkani-Hamed talks about this here or here (the last from yesterday). How this ended up with the ridiculous Slashdot headline is pretty clear, as Arkani-Hamed with his trademark enthusiasm promotes this work as a road to revolutionizing physics, getting rid of locality and unitarity as fundamental principles, finding emergent space-time, maybe emergent quantum mechanics, etc (while admitting that what has been accomplished is just step 0 of step 1 of a multi-step program). From this, one gets to the rather excessive Quanta headline about a "jewel at the heart of quantum mechanics", ensuring that the next stage of publicity (e.g. Slashdot) will launch the hype level into outer space, escaping any relationship to reality.

For the details of what this really is, the Quanta article gives a good overview, but you need to consult the long paper and recent talks to dig out a non-hyped version of what the real recent advances are. I'm nowhere near expert enough to provide this, hope that if this turns out to be as important as claimed, surely there will soon be lots of expositions of the story from various points of view. In the meantime, best perhaps to pay attention to what Witten has to say on the topic:

> The field is still developing very fast, and it is difficult to guess what will happen or what the lessons will turn out to be.

Update: Here are some slides about the Amplituhedron from Trnka (hat-tip to George Ellis).

Update: Scott Aaronson has come up with an even more dramatic advance, the discovery of the Unitarihedron, which includes the Ampltituhedron as a special case, just "a single sparkle on an infinitely greater jewel". See his posting The Unitarihedron: The Jewel at the Heart of Quantum Computing, where he unveils this new theory.

Update: See comments here by Lance Dixon and this paper for an alternative approach to computing planar amplitudes in this theory, one not using the "amplituhedron".

Update: Congratulations to Kosower, Dixon and Bern for the award of the Sakurai prize for their work on amplitudes.

Update: Dixon has a guest post about this topic at Sean Carroll's blog.

Comments

1. **kevin dowd**
   September 18, 2013

   I found this article on Reddit! So I came over to see if you had smacked it around like it was a puppy slobbering all over your shoes…

   Thx! My takeaway is its interesting, possibly important, but still just another angle to await further actual results..

2. **george ellis**
   September 18, 2013

   Here's a set of slides about it:

   http://www.staff.science.uu.nl/~tonge105/igst13/Trnka.pdf

3. **Lamont Granquist**
   September 18, 2013

   Can anyone give a brief explanation of "maximally supersymmetric Yang-Mills theory" at an undergrad level?

   It sounds sorta like the spherical chicken/cow of higher dimensional quantum field theories. I got a bit excited by the Quanta Magazine article until I hit that sentence and realized it was probably still all wrapped up with supersymmetry and string theory at its core…

4. **Peter Woit**
   September 18, 2013

   Lamont,

   What's being referred to is the "CFT" in the AdS/CFT correspondance, the QFT that is supposed to be dual to a certain string theory. It's a very interesting 4d QFT, conformally invariant and the subject of a huge amount of attention, but not directly related to the QFTs of the Standard Model.

5. **CIP**
   September 19, 2013

   I wonder if you aren't leaving out the most special thing about this result. In this special case, all those annoying extra guage degrees of freedom go away. They have been a nuisance to many smart people for a while.

6. **nasren**
   September 19, 2013

   What struck me about this article is that it seems to heavily involve twistors and yet Penrose gets no mention whatsoever. But Witten does, rather irrelevantly. One is left in no doubt which side of the Atlantic is having its curry favoured.

More destructive boosterism.

7. **Mitchell Porter**
September 19, 2013

This work descends from Witten's 2003 invention of twistor string theory.

8. **P**
September 19, 2013

Nasren,

You're wrong. Witten is very relevant to this story. Penrose is relevant, too, but not as relevant as Witten for this particular line of research on scattering amplitudes. Witten's paper

hep-th/0312171

is one of the most cited in this amplitude business, and he is the W in BCFW, which was a crucial paper.

You don't have to be so critical! And if you're going to be, at least get your facts straight first.

9. **nasren**
September 19, 2013

I stand corrected.

10. **twistor**
September 19, 2013

@ Lamont Granquist & others who might not know what "maximally supersymmetric Yang Mills" is:

Take a look at the rather nice explanations of Matt von Hippel:

http://arstechnica.com/science/2013/05/earning-a-phd-by-studying-a-theory-that-we-know-is-wrong/

or his rather excellent blog:

http://4gravitonsandagradstudent.wordpress.com/a-theorists-theory/

11. **chorasimilarity**
September 19, 2013

After browsing the interesting slides, I was struck by the repeated "not known to mathematicians". I am a mathematician and I don't have any precise idea about what exactly those amplitudes are. However, as far as I understand after a quick browsing of the slides, my understanding is the following: with a goal which I don't grasp, they attach some "forms with logarithmic singularities" to geometric data coming from affine parametrizations of points in convex polygons in projective spaces. (No wonder that some grasmannian type spaces appear and that some geometric constraints are expressed as the positivity and invariance of parametrizations.) Then they make sense of some, impenetrable to me, calculation by reformulating it in graph rewriting terms. Maybe is not at all related, but that's what I do as a mathematician (impenetrable physics goal aside). Affine parametrizations of such convex polygons are particular examples (yes,mathematician, sorry) of what I call emergent algebras, i.e. one-parameter deformations of idempotent right quasigroups (that's related to Yang-Baxter, of course), and their graph rewriting version is called graphic lambda calculus. A friend noticed some days ago that the identity (13.1), p. 94 [arxiv:1212.5605]() looks very much alike my graphic beta move [arxiv:1305.5786]() (or better [web tutorial]()), which is the graph rewriting version of the beta reduction in lambda calculus. So, maybe, with excuses for what could be a completely unrelated rant, but maybe some of it is not unknown to mathematicians.

12. **anewcommenter**
September 19, 2013

@chorasimilarity: I think the "not known to mathematicians" line is somewhat hyperbolic. Two of the co-authors, Goncharov and Postnikov, are mathematicians, so the lower bound for "number of mathematicians who know this" is 2, if not higher. Goncharov, in particular, has done quite a bit of work dealing with polylogarithms, algebraic cycles and Feynman diagrams.

13. **P**
September 19, 2013

@anewcommenter:

The claim in the talk that I saw was that the amplituhedron, not the positive Grassmannian, was new to mathematicians. The latter is definitely not new. Since the paper isn't out and we don't know the precise definition of the amplituhedron, it's hard to say for sure whether or not it is known to mathematicians; hearsay is the best we can do.

That being said, Nima's mathematician friends would likely know if such objects exist in the math literature. It's not unheard of that physicists stumble upon something that mathematician's haven't seen yet, e.g. mirror symmetry. Perhaps we have again stumbled upon something new . . .

14. **lun**
September 19, 2013

Considering that this is a theory in the planar limit, and the scattering amplitudes are all on-shell, what reason is there to assume that there is no qualitative transition between Trnka's step 1.1 and "grand vision"? (slide 2 of http://www.staff.science.uu.nl/~tonge105/igst13/Trnka.pdf)
could all this not be a mathematically elegant way to codify conformal invariance, unitarity, and energy-momentum conservation, rather than a "radical reformulation" of a generic QFT?

15. **Beelzebud**
September 19, 2013

So as a layman on the subject, let me see if I've distilled the media hype to what is really going on here.

They've found a replacement for Feynman Diagrams? Is that the meat and potatoes of this announcement?

16. **Peter Woit**
September 19, 2013

CIP,
I don't think gauge symmetry is just a nuisance, it's part of the deep structure of our best theory (and an integral part of the modern point of view on geometry). Yes, it creates all sorts of problems in a QFT, but you could also see these as opportunities. Anyway, I think I've gone on about this before, and should again sometime, but it's a complicated story for which simplistic sloganeering isn't very interesting.

lun,
Yes, that's a real question. What people have found here is the solution to a very special problem, and from this claiming evidence for a revolutionary reworking of the foundations of physics seems very premature. There's a huge amount to be done to understand the significance of why these specific amplitudes can be expressed in this form.

Beelzebud,
Yes, in a very specific, highly symmetric toy theory they have a way of getting scattering amplitudes without summing the usual Feynman diagrams. Quite interesting piece of mathematical physics, but I suggest taking Witten's advice:
"it is difficult to guess what will happen or what the lessons will turn out to be."

17. **Thomas**
September 20, 2013

Just a very naive question:
If one can do everything on-shell, no virtual particles any more, what is left of q.m., e.g. the uncertainty principle ?

18. **Peter Woit**
September 20, 2013

Thomas,
That's not so naive. Part of Arkani-Hamed's program refers to maybe even replacing QM, with QM "emergent". This kind of exact formula indicates that something may be going on here like a classical approximation being exact.

19. **Thomas**
September 21, 2013

Peter,
thanks for your answer.
Having seen the video, my hunch is that something really "deep" is going on here, but I guess one has to wait and see how things develop further.

20. **anewcommenter**
September 21, 2013

@P:

I was being a bit tongue-in-cheek in that comment. Of course the amplituhedron is a novel thing to have come out of the work, and I trust that the authors have verified that it is new, to the best of their abilities.

However, I also wanted to object to assertions like your analogy to mirror symmetry. This is quite unlike mirror symmetry – which, to my limited knowledge, appeared seemingly unbidden from physics – in that there are mathematical precursors to Arkani-Hamed et al. (as well as work done in parallel and independently, such as the work of @chorasimilarity) that are brought together in that work.

In particular, I mentioned Goncharov's work on Feynman diagrams and polylogarithms, because there have been glimpses of computational efficiency emerging from the work of people studying that locus of ideas, cf. Kreimer and others. This has been something that some mathematicians have found to be exciting for at least the past few years now.

21. **Peter Peterson**
September 21, 2013

Thank, you, very interesting subject that I was not aware of – Amplituhedron articles.

22. **Lance Dixon**
September 21, 2013

19 years ago, people stopped using Feynman diagrams to compute perturbative scattering amplitudes in N=4 SYM, and began using unitarity. That was also the beginning of the focus on the analytic properties of the loop integrand — the object which the amplituhedron is describing for N=4 SYM in the `planar' limit of a large number of colors. Ironically, this year the loop integrand has become obsolete for computing at least the simplest nontrivial amplitudes in this planar N=4 SYM theory. See arXiv:1308.2276, in which the 3-loop 6-point amplitude was computed, as a function of the external data only (all loop integrals having been performed). The loop integrand was bypassed entirely. Certain boundary information necessary for the construction was supplied from a source (the operator product expansion and integrability) that can be tapped to all orders in the coupling, so the method is very robust.

23. **Lun**
September 22, 2013

Lance thank you very much for the clarifications.
For non-experts can you also specify
a) to what extent can one be hopeful that
these results can be generalized beyond
CFT
b) I take it this goes beyond on-shell scattering amplitudes
Is that correct? How relevant are such
calculations to efforts to probe N=4 is integrable?

(Conversely if it is not integrable would
You expect such methods to fail?)
Thanks

24. **Lance Dixon**
September 22, 2013

Lun: a) unitarity for the loop integrand works in any theory, CFT or not. It's used in QCD calculations for the LHC, for example. The CFT in question (N=4 SYM) is very special, without a doubt, so you can go much further than in (say) QCD. The complete four-loop four-point integrand is known, for example, whereas 2 loops is the current limit for the QCD 4-point amplitude.

The limit of this CFT where the number of colors is large (planar N=4 SYM, corresponding to only drawing the planar Feynman diagrams) is even more special, however. Its amplitudes can be mapped to "polygonal Wilson loops" – which you can visualize as a polygonal rigid wire frame tiled with Feynman diagrams. Related to that, there is a symmetry that is like conformal symmetry, so that the answers only depend on a subset of the variables you thought they did. In fact the scattering of 2->2 gluons, or 2->3 gluons is totally fixed by this symmetry (up to constants). So the first nontrivial scattering is 2->4 (the 6-point amplitude). Also, integrability should only hold in the planar limit.

b) Everything I was talking about was with the aim of determining on-shell scattering amplitudes. However, they are closely related to the spectrum and other properties of the gauge-invariant composite operators of the CFT. The operator dimensions and operator product coefficients of the CFT appear as you take limits where the scattering amplitude degenerates or factorizes (where that wire frame assumes some singular limiting form where two or more edges become parallel). In fact, such limits simultaneously provide information to fix the answer for the generic, non-limiting case, and also test the source of that limiting information – which includes integrability and further assumptions. So yes, the fact that there is a consistent solution tests integrability. And if the approach failed at a given point, it could indicate a failure of integrability (or of some other assumption, so one would have to figure out which). So far, no failures though!

25. **4gravitons**
September 22, 2013

Just to clarify for Lun on point b), in case this was the source of the confusion: Nima's diagrams are "on-shell" in the sense that they represent things on the inside in terms of on-shell rather than the off-shell states that Feynman diagram methods use. That's just a feature of the particular way he sets things up, it doesn't have anything to do with whether the incoming and outgoing particles are on-shell or off-shell.

When people talk about amplitudes, they're generally talking about processes where the external particles are on-shell. There are other terms (correlation functions in particular) for the more general case.

26. **Peter Woit**
September 22, 2013

Thanks Lance,
I've added a note to the posting with a link to the paper that you mention. If there are other sources you'd recommend for people who want to learn more about this, let me know.

27. **johnnyythelowery**
September 23, 2013

http://www.youtube.com/watch?v=2rjbtsX7twc

The lyrics are going to have to changed….again!!!!

28. **Lance Dixon**
September 25, 2013

Thanks for adding that note, Peter. Our approach to planar N=4 SYM works hand in hand with the boundary data (from the OPE and integrability) provided by Benjamin Basso, Amit Sever and Pedro Vieira in their papers 1303.1396 and 1306.2058. In brief, they use the polygonal Wilson loop formulation of the problem. They solve the problem of exact, factorizable scattering for a system with one time dimension and one space dimension, which is roughly the direction along an edge of the polygon. Once they have that 2-d scattering matrix, they find a solution for a pentagonal Wilson polygon in terms of it. Then they build all other polygons (in a certain limit) out of their pentagons. The cool thing is that when they solve a certain part of the problem, they do so to all orders in perturbation theory in the 't Hooft coupling, thanks to the exact 2-d scattering matrix (which in turn is thanks to integrability).

29. **Anonyrat**
September 30, 2013

If we don't have to sum up Feynman diagrams but only have to compute volumes of the Amplituhedron, then the Feynman-diagram-inspired picture of the vacuum as a seething cauldron of virtual particles may be an artifact of the method of computation. Doesn't this cast a different light on the alleged non-naturalness of the value of the cosmological constant?

Love and Math

September 19, 2013
Categories: Book Reviews, Langlands

Edward Frenkel's new book Love and Math is now out. It's a must-read for those who share the interests of this blogger, so go get a copy now.

The "Love" of the title is much more about love of mathematics than love of another person, as Frenkel provides a detailed story of what it is like to fall in love with mathematics, then pursue this deeply, ending up doing mathematics at the highest level. Along the way, there are lots of different things going on in the book, all of them quite interesting.

A large part of the book is basically a memoir, recounting Frenkel's eventful career, which began in a small city in the former Soviet Union. He explains how he fell in love with mathematics, his struggles with the grotesque anti-Semitism of the Soviet system of that time (this chapter of the story was published earlier, available here), his experiences with Gelfand and others, and how he came to the US and ended up beginning a successful academic career in the West at Harvard. I remember fairly well the upheaval in the mathematics research community of that era, as the collapse of the Soviet system brought a flood of brilliant mathematicians from Russia to the West. It's fascinating to read Frenkel's account of what that all looked like from the other side.

Russia at the time had a vibrant mathematical culture, but one isolated from and quite different than that of the West. Many of its most talented members had rather marginal positions in official academia, and their community was driven much more by a passion for the subject than any sort of careerism. Frenkel comes out of this background with that passion intact, and it shines throughout his book. In some other ways though, he's more American and less Russian than just about anyone I know. Part of the Russian mathematical culture has sometimes included a certain cynicism and vision of great mathematics as an esoteric subject best closed to outsiders, with little interest in communication with the non-initiated. I confess to a personal sympathy with the cynicism part (as any reader of this blog has probably figured out) but no sympathy for obscurantism about mathematics research.

Frenkel's sunny optimism and cheerful enthusiasm for his subject and life in general is very American, and in his writing he often gets through to melt the cynical part of this reader. What's really wonderful though is his dedication to the cause of the opposite of obscurantism, that of doing the hard work of trying to explain mathematical insights to as wide an audience as possible. His book is packed with mathematics and physics, full of enlightening explanations of difficult topics at all different levels of mathematical sophistication.

Perhaps the most remarkable part of the book though is the way it makes a serious attempt to tackle the problem of explaining one of the deepest sets of ideas in mathematics, those which go under the name of the "Langlands program". These ideas have fascinated me for years, and much of what I have learned about them has come from reading some of Frenkel's great expository articles on the subject. To anyone who wants to learn more about this subject, the best advice for how to proceed is to read the overview in "Love and Math" (which you likely won't fully understand, but which will give you a general picture and glimpses of what is really going on), and then try reading some of his more technical surveys (e.g. here, here and here).

The Langlands story is a complex one, but it starts with a very deep and beautiful idea that brings together different parts of mathematics: one way to think about number theory is to think of rational numbers as rational functions on a space, the space of primes. One then ends up seeing all sorts of parallels between the study of Riemann surfaces and number theory. Frenkel explains this in detail, including André Weil's description of a "Rosetta stone", a translation between aspects of number theory, aspects of Riemann surface theory, and yet a third intermediate parallel theory, that of algebraic curves over a finite field.

He goes on to explain the subject of "geometric Langlands theory", the transposition of the Langlands program from the number theory to the Riemann surface case, creating a whole new area of mathematics, one with deep connections to quantum field theory. The book includes extensive discussion of discoveries by Witten and others linking duality in four-dimensional quantum field theory to the fundamental mysterious Langlands duality in the geometric Langlands case. Frenkel has been in the middle of these developments and is the ideal person to tell this story.

The connection between these ideas and two-dimensional quantum field theory seems to me to be a subject for which we have so far only seen the tip of an iceberg, with much more to come in the future. One part of this that I don't think Frenkel discusses is early work by Witten (before geometric Langlands was formulated) giving explicit analogies between 2d qft and reciprocity laws in number theory. For more about this, see Witten's 1988 Quantum field theory, Grassmanians and algebraic curves, or a more recent paper by Takhtajan. Working on writing up the material about the harmonic oscillator and representation theory from my last year's course has gotten me interested again in the number-theoretical version of that particular story. Unfortunately I don't know a really readable reference, hope some day to write something myself once I have a better understanding of the subject.

So, I heartily recommend this book to all with an interest in mathematics or its relation to physics. If the "Love" of the title has you hoping for a tale of romance between two people, you're going to be disappointed, but you will find something much more unusual, a memoir of the romance of mathematics and its relation to the physical world.

Comments

1. **Michał Kotowski**
   September 19, 2013

   His last name is Frenkel, not Frankel (you misspell it 3 times in the post).

2. **Peter Woit**
   September 19, 2013

   Thanks, fixed.

3. **Paulo Guerra**
   September 19, 2013

   …

   "Frenkel's sunny optimism and cheerful enthusiasm for his subject and life in general is very American…" (!!!)

   No comments.

4. **Peter Woit**
   September 19, 2013

   Paulo Guerra,

OK, I probably should have said “used to be very American”, things have changed somewhat in this country in recent decades…

5. **Michael Welford**
September 19, 2013

Amazon lets you peek at the end material for this book!!! It’s not so frighteningly advanced as you might suppose from Peters talk about ‘geometric Langlands’ and ‘Riemann curves’.

If you get straight A’s in calculus while barely opening your textbook, then this book looks like a good book to measure yourself against. See how many chapters you can get through before you finally bog down. Of course if you describe yourself as, for instance, a Frenkel-chapter-12 you’d better be prepared to back it up, because the math nerds will be gunning for you.

I’ll be waiting for a paperback edition.

6. **Anonymous**
September 19, 2013

Hard to digest… I thought that geometric Langlands is one of the most esoteric subjects closed to outsiders?

7. **Peter Woit**
September 20, 2013

Anonymous,
Yes, geometric Langlands is one of the more difficult areas of mathematics to understand, because it brings together some very deep and not completely understood ideas. Frenkel’s more technical expository papers are by among the most accessible things to read, but there is no royal road, this subject is very demanding. That he’s trying to explain it at a popular book level is a very remarkable feat.

8. **Anonymous**
September 20, 2013

Many fields of mathematics have deep ideas and difficult problems. The reasons why some areas are considered more deep and difficult are purely psychological and cultural. Some of these areas close themselves to outsiders to perpetuate the myth of greatness. Just like string theorists, mathematicians working in these areas enjoy getting all the admiration. This includes prestigious awards, positions in top schools and publications in the best journals for making even the slightest progress on some famous problem. Brilliant mathematicians working in these areas are no more brilliant than top mathematicians in many other areas who unfortunately are often judged by a different standard.

9. **Armin Nikkhah Shirazi**
September 20, 2013

Peter,

Based on Paulo Guerra’s name (which does not sound typically American) I surmise that his “no comments” comment was a subtle criticism of the implication of your claim that “sunny optimism and cheerful enthusiasm” exclusively characterize American culture, not this was once the case and may not be so any longer.

Thank you for the book review, though.

10. **Ponder Anew**
September 20, 2013

“…The organic unity of mathematics is inherent in the nature of this science, for mathematics is the foundation of all exact knowledge of natural phenomena. That it may completely fulfil this high mission, may the new century bring it gifted masters and many zealous and enthusiastic disciples!” –David Hilbert 8.8.1900.

I came to mathematics late in life but I have certainly developed a love affair with it; it’s seeing the problem all the way through, the clarity of vision that oftentimes seems to take eons to obtain, which is so addictive – similar to those bursts of creative insight which lead to novelty in so many fields! This is the first time I’ve encountered this “geometric Langlands” entity and it sounds quite fascinating; I wonder, is it the solution to the enigma regarding the unity of mathematics and the intimate connection between mathematics and physics? Of course, to date, I don’t have the knowledge base required to tackle such a book but perhaps one day the Love will lead me there . . .

11. **Peter Woit**
September 20, 2013

Armin,

A bad idea for me to be trafficking in national stereotypes at all, of course they are of very limited significance and dubious value, but, in any case, plenty of other national stereotypes include even more optimism than that of Americans. In terms of stereotypes, from IP address you’re right, but these days “Paulo Guerra” is a pretty American name…

12. **Peter Woit**
September 20, 2013

Ponder Anew,

Frenkel is known for referring to the Langlands story as a “Grand Unified Theory of mathematics”, with some justice. The connections to physics make the unification theme even more impressive.

13. **Mathematician**
September 20, 2013

Thanks for the review. I’ve ordered the book.

14. **anon**
September 21, 2013

Grand unified theory of nothing. The last time this Frankel quote came up on this blog, David Ben-Zvi wrote (July 24, 2008): ‘Of course Frenkel’s comment on the “grand unified theory” is tongue in cheek, but in any case he was referring to the entire Langlands program, not just to its geometric aspects.'

15. **Chris Austin**
September 22, 2013

If Frenkel thinks he can "popularize" maths by making a movie that puts forward the worse-than-silly propositions that there exists a "formula for love", and that after discovering this "formula", he has no alternative but to kill himself, why should anyone think there is anything more to his "deep" maths than the emperor's new clothes?

16. **uair01**
September 22, 2013

Michael Welford says:
Of course if you describe yourself as, for instance, a Frenkel-chapter-12 you'd better be prepared to back it up, because the math nerds will be gunning for you.

I wonder where this book lies between:
1) The "Road to reality" where I got to chapter 10 and from there I had to read "in between the equations" (just as Mr. Penrose suggests).
2) This interesting number theory book (Fearless symmetry) which I read in its entirety.

I still wonder how many people buy those books. They're too difficult for the common public but are (I assume) too trivial for the specialists. But it's very sympathetic of the authors to present "unadulterated" science.

One author (forgot name) once wrote that: "every extra formula halves the possible audience".

1) http://www.amazon.com/The-Road-Reality-Complete-Universe/dp/0679776311/ref=sr_1_1?ie=UTF8&qid=1379850599&sr=8-1&keywords=road+to+reality
2) http://www.amazon.com/Fearless-Symmetry-Exposing-Patterns-ebook/dp/B005K46YVU/ref=sr_1_1?ie=UTF8&qid=1379850665&sr=8-1&keywords=fearless+symmetry

17. **Peter Woit**
September 22, 2013

Chris Austin,
One more reason for reading the book is that there's a chapter about the film and what he was trying to do with it. One can like, dislike, not care, about his film (for options other than "not care", you might want to read what he has to say), but I don't see how this has anything to do with the issues about mathematics. I don't see any reason why doing great mathematical work should correlate at all with whether one's art-film project is wonderful or misguided.

uair01,
The book is not like Penrose's, which was a graduate level textbook disguised in the cover of a popular book. It's much closer to "Fearless Symmetry", which covers related mathematics, at a similar level. The difference is that Frenkel's book is not written at all at a specific level aimed at an audience with a certain background. Much of the book is a memoir that could be enjoyed by anyone with zero knowledge of math and zero interest in learning more. Of the rest, a lot is explanations of mathematical ideas at a popular level, with no equations. In parts though, he's getting into ideas that it's just not possible for people without a lot of background to follow. Those will be understandable by people with some sophisticated math training, but for the average person will just be a glimpse at something they're not going to really understand (but such a glimpse might give them some insight into what math at this level is like).

18. **Shantanu**
September 25, 2013

Peter, OT.
Nobel prize annoucement in 2 weeks. Any guesses?
Will it be awarded for Higgs discovery?

19. **Marco Masi**
September 26, 2013

I'm no longer sure that falling in love with math is healthy for everyone. It is fine for mathematicians, but for physicists it can be dangerous. I once used to be it too until I realized that it distracted me from understanding better the foundations of physics and the deeper meaning of some of its principles. In a certain sense I believe this is one of the causes of today's failure in theoretical physics to go beyond the SM.

Quick Links

September 26, 2013
Categories: Uncategorized

- The new issue of *Nautilus* has a wonderful story about Yitang Zhang, called The Twin Prime Hero, which includes a long interview with him. Zhang's remarkable mathematical career includes several years working at a Subway in Kentucky. His sucessful work on the twin prime conjecture (see here) was done over four years, working seven days a week without almost any breaks, while teaching two classes at a time.
- This year's Physics Nobels will be announced October 8, Nature has a story here. For non-HEP physics, I have no ideas about likely winners. For HEP, of course the Higgs is the big news. Personally I think they should give the award to CERN + ATLAS + CMS, but that would require changing their tradition of not making this award to groups. Seems like a good time to change this. On the theory side, in some sense it is Weinberg-Salam that has been vindicated, and they already got the prize for this. If one wanted to give a prize for the general idea of the Higgs mechanism, I've argued that Anderson should be included (see here).
- This weekend the IAS will host Dreams of Earth and Sky, a celebration of Freeman Dyson's 90th birthday, see more here.
- I'm not going to the Dyson-fest, but am looking forward to seeing the film Particle Fever this weekend at the New York Film Festival.
- Next weekend it will be not physics, but math, as I'll be at the Simons Foundation day-long program on October 5, Celebrating the Mathematics of Pierre Deligne. Recently I've been spending some time watching Deligne's lectures from this past spring at the IHES, available in high quality video here.
- The only mention of Bohemian Gravity! here was in a comment a while back, and I hadn't added more, since this has gotten attention from hundreds of other sources. But of course it really is great and deserves all the attention and more, so if you're the only reader of this blog who hasn't checked it out, do so now.
- Frank Wilczek has been very active on Twitter recently, and a directory of some of his recent writings is here. According to this tweet, he has plans at some point to break out of the 120 character limit.
- Latest news from the LHC is here. Work is on schedule for January 2015 first beams at a higher energy of 13 TeV.
- For an example showing that some basic technical questions about the Standard Model are still poorly understood and deserve a lot more attention, see Michael Creutz's talk on Chiral Symmetries and Lattice Fermions at this recent QCD conference, as well as the preprint version here.

Update: In case you don't get enough material from me here explaining what the problem is with the "multiverse", Sabine Hossenfelder has more here.

Comments

1. **Hamish Johnston**
   September 27, 2013

   Yes, a Nobel for CERN+ATLAS+CMS would be the right thing to do.

2. **Thomas**
   September 28, 2013

   It would be great if you write about the talks at the Deligne conference!

3. **Bill Cheswick**
   September 29, 2013

   Thanks for mentioning the dyson fest. I Immediately signed up and went, and it was excellent. It is the first time the entire Dyson family was in one place.

   Best new concept for me: iron helide. Freeman analyzed the crystal structure of this "compound" on the surface of a neutron star.

   The presentations were streamed, but don't seem to be online. I hope they make it.

4. **Ponder Anew**
   September 30, 2013

   I read about Yitang Zhang awhile back; it's an interesting story. In a vein remotely similar to the Yitang Zhang story is the story of Jacob Barnett, a 15 year-old autistic boy studying at the Perimeter Institute (http://www2.macleans.ca/2013/09/01/jacob-barnett-boy-genius/). His story reminds me a bit of the Kit Armstrong story in that both boys are mathematical "geniuses" and both have really extraordinary mothers!

5. **Thomas**
   October 1, 2013

   The text of Luc Illusie's beautiful talk on Deligne seems already to be online: http://www.math.u-psud.fr/~illusie/Illusie-Deligne9.pdf
   An other aspect of Deligne's work and a similar description of him by Illusie:
   http://www.math.u-psud.fr/~illusie/Deligne_I3.pdf

6. **chiz**
   October 8, 2013

   Other possible contenders for the physics prize – Nick Holonyak for the LED. It was the fiftieth anniversary of his discovery last year.

   Thomson-Reuters have suggested Marcy, Mayor and Queloz for discovering extra-solar planets. I admit that I have been wondering for the last few years if exoplanets could be in the running, but the names they suggest are wrong. If they do give it for exoplanets it will be to Wolszczan, Mayor and Queloz. Wolszczan found the first exoplanets -around a pulsar – and Mayor and Queloz found the first around a sun-like star. Marcy came on the scene afterwards. If the award is given for exoplanets, however, expect a possible debate that makes the one concerning the MRI prize look quite tame. The history isn't anywhere near as clear as most accounts make it seem – some people think that exoplanets were found before the pulsar planet ones – and many of the claimed exoplanets aren't very robust.

7. **Mike Creutz**
   October 18, 2013

   Someone just pointed out to me the above comment on my Trento talk. I said some outrageous things there and was surprised I didn't get more of a reaction. Among others:
   Partially quenched chiral perturbation theory is wrong
   Matching lattice masses with msbar is not sensible
   These are both mainstays of some lattice groups. It appears this was the wrong audience, which cared mainly about the infrared behavior of the gluon propagator.

I also just stumbled on the extensive 2008 discussion here of my ranting. On this topic communication has completely collapsed.

Why m_H= 126 GeV?

September 26, 2013
Categories: Uncategorized

This week in Madrid there's a conference going on with the title [Why m_H= 126 GeV?](). It brings together HEP theorists working on "Beyond Standard Model" physics, with the majority of the participants from Western Europe, especially Spain. As part of the workshop they did a [survey](), getting about 50 responses. Among the results:

- For the question "Do you think that String Theory will eventually be the ultimate unified theory?", 27% said Yes, 73% No, with the Nos breaking up into 27% just "No" and 46% "No, but it is a step in the right direction".
- For a question about the hierarchy problem, the three opinions that got the highest numbers were pretty much split evenly among them: "Low energy SUSY solves the problem", "Anthropics solves the problem", and "There is no such problem."
- Opinion was evenly split on whether the LHC would or would not find non-SM behavior of the Higgs, and 60-40 in favor of the LHC finding some non-SM new physics.

If you're at all interested in what the current mood and thinking is in this part of HEP theory, you should definitely take a look at the video of this evening's discussion section, moderated by Joe Lykken. It included extensive debate about the questions raised by the survey and what people's answers meant. At the end there was a short interesting discussion about AdS/CFT and its relation to string theory, with Michael Douglas arguing that AdS/CFT should be thought of as an improved version of the renormalization group, with no necessary connection to string theory. String theory and SUSY only come into it by providing certain examples where you can do explicit calculations in the dual theory. By the way, I've heard a rumor that Douglas is going on leave from his physics job to work at the Simons hedge fund Renaissance Technologies.

Among the talks so far on-line, you might want to take a look at Alessandro Strumia's Is Naturalness Natural, for an example of the sort of thinking that denies the dichotomy of "low energy susy or anthropics". As the survey showed, this insistence on other alternatives has at least 1/3 support, and Joe Lykken mentioned that he was in this category.

Michael Dine's talk on Alternative Futures for Particle Physics starts off with slides about Neil Turok's comments on the "crisis" in the field, and shows this blog entry. He then goes on to give a string theory landscape/anthropics-based point of view on prospects for BSM physics. At the end of the talk there's some pushback from the audience, with one questioner describing Dine's anthropics as "a kind of sleeping pill, so you convince yourself that you are smart", calling this "theology" not physics.

Dine describes my blog entry he showed as one that personally insults him, something that certainly wasn't intentional. He's not mentioned at all, but I gather he's unhappy about my description of the material in the slides of Sally Dawson's HEPAP presentation

Dine was chair of the committee that produced this report on The Future of U.S. Particle Theory and it's well worth reading for a detailed overview of the current state of HEP theory research in the US, especially from the more phenomenological end. Like the slides though, I'd describe it as mostly avoiding dealing with the intellectual crisis that Neil Turok was describing. Even though Dine was the chair of the committee, there's nothing in its report about his favored road ahead (the landscape and anthropics). I'd guess that the committee members felt that when trying to get support for HEP theory from other scientists or government funding agencies, talk of crisis-level problems with conventional wisdom was to be avoided, but even more so any mention of the string theory landscape and anthropics.

Update: For the latest on the landscape, see Michael Douglas's talk on The string landscape and low energy supersymmetry. At the beginning of his talk he notes that "most people seem to have given up" on this, and from the talk itself it's easy to see why. Actually, Douglas himself seems to be giving up. I've heard more about his move from physics to finance, which began last fall when he went on leave to work at the Rentech hedge fund. Evidently this fall he is not coming back to the Simons Center, but staying at Rentech, leaving his academic position. Rumors are that one reason he gives for leaving is that there is not much of interest going on in HEP theory these days.

Comments

1. **Bob Jones**
   September 27, 2013

   Michael Douglas is right about AdS/CFT. The idea of holography is quite general and doesn't require string theory. It can also be realized entirely within quantum field theory where you can show that a topological field theory (which is similar to a gravitational theory since it is "generally covariant") can be equivalent to a conformal field theory on a lower number of dimensions.

2. **CU Phil**
   September 27, 2013

   Hi Peter,

   This isn't the first time I've heard that AdS/CFT is independent of string theory (which seems true from what I know, but I don't feel qualified to rule on that), but the first time I've heard that it should be thought of as an improved version of the RG. Do you know any more detail about what he means there?

3. **Thomas Larsson**
   September 27, 2013

   The almost inconsistent SM parameters have an interesting analog in statistical physics. In the 1940s Onsager showed that critical exponents have to satisfy certain inequalities. Twenty years later people realized that these inequalities are in fact equalities, i.e. critical exponents are on the border of inconsistency. The underlying reason is scale symmetry.

   By analogy, the fact that the SM Higgs mass is on the verge of inconsistency could be a sign that a symmetry principle is at work here too. If so, there probably isn't any BSM physics to be found, apart from gravity which is a different matter altogether.

   Hence the null results from the LHC could actually turn out to be quite exciting.

4. **Ray**
   September 27, 2013

   @Peter Have you seen the recent blog post by Andreas Karch in Lubos's blog? http://motls.blogspot.com/2013/09/guest-blog-on-applications-of-holography.html
   Any thoughts?

5. **Peter Woit**
   September 27, 2013

   Ray,

Well, one thought is "why would anyone sensible want to appear as a guest on Lubos's blog?"…
But, beyond that, seems like a reasonable discussion, although just dealing with one side of the story, reasons for optimism about doing things with this set of ideas. If you want to discuss it though, you really should discuss with Karch, not me.

CU Phil,
I'm not sure exactly what Douglas had in mind, but in AdS/CFT, the fifth dimension has an interpretation as an energy or distance scale, and people have used that to discuss various ideas about a "holographic renormalization group".

6. **Anon**
September 27, 2013

Peter,

Thanks for pointing these talks and videos. A lot of the talks seem to say that there is already evidence for fine tuning or unnaturalness at 1% level (some even have it at 0.1%). That is pretty interesting. Maybe after a few years of data from its 14 TeV run, LHC can establish fine-tuning at a 0.1% level…

7. **Anonymous**
September 27, 2013

0.1% fine tuning wouldn't be completely implausible in the context of the "dimensionless factors should be ~1" naturalness argument. Would something like 1/(8pi)^2 show up on anyone's naturalness radar?

8. **Shantanu**
September 27, 2013

Peter thanks for the link to this video. extremely interesting.
Joe Lykken seems to agree with you about string theory. The discussion about inflation and quantum gravity was also quite interesting.
I disagree with Slava that there is no alternative to inflation.

9. **Tommaso**
September 27, 2013

If you liked the survey, please post your own answers at the comments thread of my blog post on the matter, here:
http://www.science20.com/quantum_diaries_survivor/confidence_new_physics_and_susy_dropping_stone-121294 . And specify whether you are a particle physicist… It would be nice to compare the views of HEP physicists and the rest of the world !

T.

10. **Giotis**
September 28, 2013

Douglas' propositions regarding AdS/CFT and HS gauge theory are weird at least. HS gauge theory is String's theory backyard and can be only understood only as a limit of it.

For an analysis on a conceptual level of the gauge/String correspondence the interested reader is redirected to the following classical paper by Polyakov:

http://arxiv.org/abs/hep-th/0110196

11. **John**
September 28, 2013

@Giotis String theory is also argued to be a spontaneously broken phase of HS theory.

12. **Giotis**
September 28, 2013

It's still String theory which possesses this HS gauge symmetry i.e. its tensionless limit; the tension generation breaks the symmetry and the tensile String theory emerges.

13. **Rick Ryals**
September 28, 2013

I'd say that some degree of congratulations is in order, Peter. The rats are starting to abandon Hook's fantasy ship in droves!

14. **Noah Smith**
September 28, 2013

I've heard a rumor that Douglas is going on leave from his physics job to work at the Simons hedge fund Renaissance Technologies.

Yay for Stony Brook's anchor firm!!

15. **Noah Smith**
September 28, 2013

Dear string theorists: Your field is dying. Come to Long Island and work in quantitative finance while you figure out your next career move! 🙂

16. **fuzzy**
September 28, 2013

great title, "why higgs mass is 126 GeV?"; i like this way to organize workshop! next ones could be, why omegaLambda=0.692, why theta13=9 degrees, and many more to follow… we have a lot of useful occasions of discussion, why we speak of a crisis of HEP? (that could be another title, in fact)

17. **uair01**
September 28, 2013

All those bright theoreticians going into finance is genuinely scary. Not only will finance have more capital and political power than everyone else, but no one else will be able to understand what they are doing. Not only tougher, but also smarter than the rest of us. Something like "chess-boxing" masters. Resistance is futile.

🙂

18. **Noah Smith**
September 28, 2013

Pretty much! But since both quant finance and string theory are basically just ways for us to keep our smartest people active and occupied until we have something useful for them to do, we might as well have the smart people make money while they wait! 😉

19. **OMF**
September 29, 2013

> Not only will finance have more capital and political power than everyone else, but no one else will be able to understand what they are doing.

>Implying that finance understands what it is doing in the first place.

20. **CU Phil**
September 29, 2013

I think the people taking pleasure in this rumour about Douglas are being moderately nasty and entirely misguided. In a field that is subject to lots of hype, Douglas has always struck me as a very honest, level-headed, sound physicist.

21. **Peter Woit**
September 29, 2013

CU Phil,
I agree that personal criticism of Douglas is out of line. With string theory unification stuck in the dead end of the landscape, it's eminently reasonable for him to decide he'd rather do something quite different with his life. What he's doing is actually a lot better for the field than sticking around not doing much in a permanent position, since it opens up a job for someone else. One could criticize on scientific grounds his decision to promote (and continue promoting as in his talk) the string landscape as a viable rather than dead idea (and I have), but that's a scientific, not personal criticism. The world is full of wonderful, admirable people who I happen to disagree with about something or other.

22. **CU Phil**
September 29, 2013

Hi Peter,

FWIW, I wasn't talking about you. You've always seemed to treat Douglas as an honest, well-intentioned guy who you happened to disagree with. It was people celebrating this as "rats abandoning the ship" that irked me — even if that was something to be celebrated, the ship seems to be better off with people like Douglas than without them.

23. **Chris Oakley**
September 30, 2013

> Pretty much! But since both quant finance and string theory are basically just ways for us to keep our smartest people active and occupied until we have something useful for them to do, we might as well have the smart people make money while they wait

So there are people who know better than the smartest people what the smartest people should be doing – but they are not themselves the smartest people – ? That sounds like a contradiction. There are different kinds of smartness. Although I had little success in getting academic posts, in 1984 I was at least smart enough to see that String theory was a blind alley. If more theorists had shared my insight a huge waste of time and effort would have been avoided. In the area of quantitative finance, any market professional could have told the quants building models that repackaged sub-prime mortgage bonds to create AAA tranches from junk – a group that, on paper, looked incredibly smart – that the models were fundamentally flawed because diversification cannot be used to reduce risk in volatile markets as in these circumstances all correlations become either 1 or -1. See *The Big Short* by Art History major Michael Lewis for a good analysis of this. So in this situation too the "smart people" did something seriously not bright.

24. **Bernhard**
September 30, 2013

"For a question about the hierarchy problem, the three opinions that got the highest numbers were pretty much split evenly among them: "Low energy SUSY solves the problem", "Anthropics solves the problem", and "There is no such problem."

I am surprised by the fact that so many indeed believe "there is no such problem" since I haven´t seen any prominent theorists defending this point of view. I guess it is the sort of thing people secretly think for themselves but fear criticism of people in the high chain of command (like Arkani-Hamed). With the LHC results and the pressure on SUSY, I guess maybe people will begin to speak more freely about it.

In fact I know of no good paper (a review would be particularly useful) that discusses this particular point of view. I am curious to know if others do (if so, please share).

(disclaimer: I have no intention to discuss the hierarchy problem per se here for the thousandth time).

25. **Visitor**
September 30, 2013

"In the area of quantitative finance, any market professional could have told the quants building models that repackaged sub-prime mortgage bonds to create AAA tranches from junk – a group that, on paper, looked incredibly smart – that the models were fundamentally flawed because diversification cannot be used to reduce risk in volatile markets as in these circumstances all correlations become either 1 or -1. "

It WAS incredibly smart. The quants and the market professionals and the rest of those pigs made incredibly big amounts of money, and everyone else got incredibly screwed and had to pay for it. And then we had Derman telling us how he never, ever even suspected that, as a quant, he was not working for Eleanor Roosevelt, and how shocked – shocked! – he was to learn that not all the profits were going to UNICEF.

How inspirational was that!

26. **Peter Woit**
September 30, 2013

Please, enough generalized finance bashing, which is pretty much off-topic (no, whatever you think of Renaissance Technologies, they weren't responsible for the mortgage-backed securities mess).

27. **Anon**
September 30, 2013

Anonymous —

In MSSM that solves the hierarchy problem, it is factors of order 1 (or for some terms maybe of order 0.1) that multiply SUSY breaking scale terms M_SUSY^2, to give the contribution to Z mass (m_Z ~ 100 GeV). If M_SUSY becomes order 1 TeV this is 1% fine tuning (~m_Z^2/M_SUSY^2) and if its 3 TeV its 0.1%.

The naive 1/(8 \pi)2 factor which is in front of the Planck scale (UV cut off scale) in standard model for estimating quadratic divergence due to loop effect, does not multiply the scale that is expected to solve the hierarchy problem.

28. **Anonymous**
September 30, 2013

What I am saying is, even if it turns out that the MSSM is a correct effective theory of nature, it would remain mysterious why SUSY had to be broken at all. A more enlightened theory might be able to show that, ah, of course, it must be the case that "m_Z = m_SUSY/8pi" (or something like that; I mean a theory that can relate m_Z and m_SUSY perhaps in the style of the relation between m_Z and m_W in the SM), explaining the breaking and resulting in the apparent "fine tuning" of 1/(8pi)^2, which would not be fine tuning at all, being a very reasonable not quite ~1 constant.

The argument is intended to be that 0.1% fine tuning is equivalent in my eyes to 50% "fine tuning", because mundane constants like 1/(8pi)^2 are no less "natural" (dimensionless constants are naively expected to be ~1…) to me than 1/sqrt{2}, say. Heck, even 1/(64pi)^4 doesn't ring naturalness alarm bells for me (again, I can easily imagine that some future theory could show that SUSY breaking must lead to exactly this level of fine tuning). My standard for natural is "is it conceivable that a future theory predicting a relation between these parameters, which today we must take to be free, would give such a small ratio". I have pretty low standards for that threshold.

29. **Noah Smith**
September 30, 2013

So there are people who know better than the smartest people what the smartest people should be doing – but they are not themselves the smartest people – ? That sounds like a contradiction.

Not if you believe in the "wisdom of crowds"… 😉

30. **Anon**
September 30, 2013

Anonymous — This does not happen in MSSM but in principle there could be some new solution to hierarchy problem where there is a small coefficient like you say between the scale of new physics and the weak scale and then there would not be fine-tuning — but I dont think such solutions have been found yet.

If nature actually uses such a solution, I think it would probably apply directly to standard model (SM) without need for SUSY….so it does not require one solution of the hierarchy problem (namely, SUSY) to lower the Higgs mass down from Planck scale to SUSY scale and another solution between SUSY and weak scale.

It would be very useful if someone came up with a mathematical proof that within the framework of the usual QFT and SM, the solution for hierarchy problem must be at the weak scale or else there will necessarily be fine-tuning.

31. **paddy**
September 30, 2013

Relative to factors or 2 and PI and sqrt(2) etc..I recall Sidney Coleman recalling in a lecture that in his oral of some some sort he attempted to wave his hands at such factors…only to have one of the committee (R.F. for short) say to the young Sidney: "If you don't understand where the 2's and PI's go…you don't understand it at all."

32. **Peter Shor**
October 2, 2013

The argument for the existence of the hierarchy problem essentially assumes we roughly know the Bayesian prior probability distribution on the parameters of the QFT. If we believe that the Standard Model (and any low-energy extensions of it) is just an effective field theory, and we don't know anything about the underlying structure, then all this discussion of fine-tuning and the hierarchy problem is highly speculative.

33. **Bernhard**
October 2, 2013

Peter Shor,

While finding your argument quite interesting, I am not sure I follow it completely (my gut feeling is that I´m not alone). Any reference where this is discussed a bit more detailedly?

34. **Tommaso**
October 3, 2013

Dear Peter,

I agree with what you say, and I would add that since any pdf chosen as a Bayesian prior depends on the choice of variable (it will change by the Jacobian if we change variable), that choice is a further assumption one is implicitly casting into the problem.

Cheers,
T.

35. **anonymount**
October 3, 2013

"The argument for the existence of the hierarchy problem essentially assumes we roughly know the Bayesian prior probability distribution on the parameters of the QFT. If we believe that the Standard Model (and any low-energy extensions of it) is just an effective field theory, and we don't know anything about the underlying structure, then all this discussion of fine-tuning and the hierarchy problem is highly speculative."

Well, sounds reasonable, but tuning arguments are very much common-sense ones, and they have been successful previously in a number of well known cases. Your criticism would apply equally well to those.

36. **Bernhard**
October 3, 2013

Tommaso,

Would you consider writing a blog entry about this? The hierarchy problem is one of the most taken for granted problems in HEP, with little to none questioning about its validity. I must say that I have been searching for papers discussing just what it´s being stated here, without success.

After I read your comment, Peter Shor´s comment was suddenly clearer, but I (and I believe many) would benefit from reading your take on this. As you know, let not overestimate most physicists knowledge of statistics which I am sure is, on average, way below yours.

Just an idea, think about it….

37. **anonymount**
October 3, 2013

Bernhard, you can read this: arxiv.org/pdf/1307.7879.pdf
Is the clearest and sharpest you can find.

38. **Bernhard**
October 3, 2013

anonymount,

Many thanks!

Particle Fever

October 1, 2013
Categories: Favorite Old Posts, Film Reviews, Multiverse Mania

Yesterday I got a chance to see Particle Fever, the long-awaited film about particle physics. It's at the New York Film Festival, where there will be another showing on Wednesday, although tickets are already sold out. Oliver Peters was also there, and has a detailed review.

My own reaction to the film was kind of schizophrenic: most of it I thought was fantastically good and I really hope it finds distribution and gets widely seen. On the other hand, some of it I thought was a really bad idea. First though, the really great aspects of the film.

The main structure of the film is built around the discovery of the Higgs at the LHC, starting at a point back around 2006 or so. Theorist David Kaplan is the person most responsible for the idea of the film and getting it made, and there's footage of him visiting the LHC while ATLAS is being installed, getting shown around by Fabiola Gianotti, who later was to become ATLAS spokesperson. This part of the film shows very well the scale of the effort represented by the LHC and its detectors, as well as giving some idea of the physical environment experimentalists work in (both the huge experimental halls and the areas around them, as well as control rooms and crummy office spaces). There's good use of high quality graphics to give some basic insight into what is going on. Interviews with a few ATLAS physicists add a human face to the story and explain the motivation that drives people to do this kind of work.

The cameras were also there for first beam back in 2008, as well as to capture people's reaction to the depressing news of the accident a few days later that set the whole project back by a year. There's wonderful footage of the scene late in 2009 when first collisions finally occurred, with Beethoven's Ode to Joy providing a very appropriate soundtrack. I especially liked the scenes of a young postdoc (Monica Dunford) carrying her laptop around, elated to show everyone plots with data from the first collisions.

The last part of the film is dominated by the July 4, 2012 discovery announcement, doing a wonderful job of showing the media frenzy as well as the joy and excitement of the entire HEP physics community at that time. All in all, if you want to get someone turned on to high energy particle physics, or just convince a young person that a career in science is an attractive idea, the CERN footage in this film should do the job better than anything I've seen from even the highly competent CERN press office.

Theorists provide a parallel track throughout the film, with focus on Kaplan, ~~his advisor~~ Savas Dimopoulos, and Nima Arkani-Hamed. All of them are highly eloquent on the topic of the significance of fundamental HEP physics research. It is made clear that the fact that the LHC is not seeing SUSY or other new particles is a big problem for theorists like these who have devoted their careers to models of new physics that was supposed to show up at the LHC. In one scene Dimopoulos and Riccardo Barbieri are discussing the matter, with Barbieri saying he has wasted 40 years working on such things, and will soon be retiring. Dimopoulos says that in his case it's just 30 years, but insists there is still two years to go (until the full-energy LHC) before really giving up. The relation of all this to the Higgs is not made clear.

As for the really bad idea, it's the introduction of the multiverse into the theory part of the film. Kaplan is shown claiming that the multiverse predicts a 140 GeV Higgs, based on this paper of Yasunori Nomura and Lawrence Hall (who was Arkani-Hamed's advisor). This is at a time when there were experimental hints of a 140 GeV Higgs. After they went away, and the mass came out at 125 GeV, the "prediction" is forgotten, but a long segment still has Arkani-Hamed going on about the CC and arguing for the multiverse. Just before this segment though, Dunford the experimentalist is shown Skyping with the filmmaker, warning them "Don't listen to theorists". At the film showing, Kaplan and Arkani-Hamed were there and answered questions at the end. One of the first questions (not from me…) was from an audience member who asked why they had put the material about the multiverse in the film, even though it had no real link to the Higgs or the LHC experiments. Arkani-Hamed admitted that the 140 Gev prediction was tenuous, there was no "sharp" link of the multiverse to the Higgs, and that no way is now known to get predictions out of the multiverse idea or test it. Kaplan explained that the intention was to make an "experiential" film, focusing on what theorists were talking about and thinking about, without getting into really trying to fully explain the scientific issues. The problem with this is that the film comes through as promoting the Dimopoulos/Arkani-Hamed view that no SUSY means a multiverse, without showing any challenge to such an argument.

In any case, it's a beautifully done film, on a great topic. I hope it soon gets widely distributed, although perhaps with some sort of warning tag attached.

Comments

1. **Yatima**
   October 1, 2013

   While we are talking about LHC:

   Google adds Large Hadron Collider tunnel to Street View

   "Google has dragged is Street View imaging kit to Switzerland, then lugged it beneath the earth to capture images of the tunnel containing CERN's Large Hadron Collider (LHC)….

   Sadly the images don't quite let you be the atom, instead offering the chance to trundle through the LHC's long and monotonous tunnel. Perhaps more insightful are some of the incidental sights: CERN's surprisingly grubby, some of its walls weren't poured by artisanal concrete-wranglers and there are plenty of places that look overdue for a fresh coat of paint.

   The image trove can be found here.

2. **Marcel van Velzen**
   October 1, 2013

   It's a shame ignoring the two physicists who were brave enough to write already in 2009: "This results in $m_H=m_{\rm min}=126$ GeV, with only a few GeV uncertainty". Basically assuming no new physics up to the Planck scale: http://arxiv.org/abs/0912.0208 With only a few GeV uncertainty!

   Even if they got lucky (which I don't believe) THEY WON. Yes Mikhail Shaposhnikov and Christof Wetterich won and nobody else. Get used to it!

3. **franco zoccheddu**
   October 1, 2013

   May you tell me how (or when, or where) to see "Particle Fever"? I live in Italy, and would like my students to see the film. Will it be in theatres next months? Will it be translated for other countries? Hard asks, I know… so thank you for any answer of yours!

4. **Shantanu**
   October 1, 2013

Peter, what do you think about the Wetterich/Shaposhnikov paper?

5. **Peter Woit**
October 1, 2013

franco,
As far as I know, the film has not yet found a distributor, so there aren't plans yet for it to be in theaters.

Shantanu,
I'm no expert on the topic, my main thought about Wetterich/Shaposhnikov is that I'd like to hear from experts. From what I've seen, the sort of thing they are discussing deserves a lot more attention, about 10^500 times more than the landscape. It would have been a great idea for the filmmakers to interview them or someone else with serious ideas about the Higgs, rather than to feature Dimopoulos/Arkani-Hamed and the multiverse.

6. **Tim Tait**
October 1, 2013

(Minor) Correction — Kaplan's advisor was Ann Nelson of UW.

7. **Peter Woit**
October 1, 2013

Tim Tait,
Thanks. My misunderstanding of something said in the film. I gather Kaplan worked as a postdoc at SLAC with Dimopoulos, not as a student.

8. **Gordan Krnjaic**
October 1, 2013

Marcel,

The Shaposhnikov/Wetterich paper is certainly interesting, but ever since LEP, we've had indirect (but inconclusive) evidence to suggest that the Higgs should be in the 120 GeV ballpark. Given this prior and the hundreds of Higgs mass predictions out there, it's not so surprising that one of them would land on ~ 126 GeV.

9. **Marcel van Velzen**
October 1, 2013

Hello Gordan. Basically I agree with what you wrote, but no one should make a claim of correctly predicting the Higgs mass (especially after it has been measured) in some fancy theory without mentioning the ones who were closest to the correct value with a 2.2 GeV error margin, reasonably ahead of time and who used very little new physics (far less than most of the correct/incorrect Higgs mass predictions with much larger error margins). If you want to make this into a claim game, at least mention the winners.

10. **Ray**
October 1, 2013

Take a look at this paper called Higgs Mass Predictions: http://arxiv.org/abs/0708.3344.
All masses from 100-300 Gev are represented with quite a few around the 126 GeV value. In fact there was a crackpot paper which predicted 126 GeV based on four color theorem. Clearly if you exhaustively sample the whole parameter space you are bound to get lucky sometime.

11. **paddy**
October 1, 2013

On the Mikhail Shaposhnikov and Christof Wetterich prediction or postdiction: is not the question "Is their reasoning deserving of attention?" Like PW I await somebody knowledgable enough to say yea or nay.

12. **Columbia**
October 1, 2013

I am not an expert on asymptotic safety, but the Shaposhnikov paper has a few obvious issues. First is that the revised prediction is closer to 129 GeV, which is a special place theoretically for a number of reasons quite independent of asymptotic safety. Second is that it simply doesn't answer all the important cosmology questions. Like what is dark matter, why so little antimatter, what is the inflaton. It doesn't explain particle generations, flavor or CP, or the group structure of particle physics, and it still in principle involves a great deal of finetuning. One can go on…

The problem is that any attempt at an answer for those questions will in principle change the value of their Higgs prediction. The authors are aware of this, and have released some minimal proposals which all have their own problems (like Higgs inflation). So I mean its far from a panacea at this time, but it deserves some more work

13. **Shantanu**
October 2, 2013

Columbia,
Having no connection to dark matter is not a reason to dismiss a theory. Note that despite what you hear inalmost every HEP talk, there is not a single shred of evidence from
astrophysical observations that dark matter has anything to do with particle physics.
Also as I have mentioned several times, whether inflation happened or not is still an open question.
Also I don't think supersymmetric models address baryogenesis

14. **FNesti**
October 3, 2013

"…Kaplan, his advisor Savas Dimopoulos, and Nima Arkani-Hamed"

I don't get what these people have to do with the Higgs – how dare they speak instead of the fathers of the field? Weinberg, Higgs, Glashow, Anderson, etc.

As a theorist, I'm quite ashamed of this crap.

15. **fuzzy**

October 4, 2013

The director should have considered as characters also those who originally contributed to this field, right so! Possibly those mentioned by FNasti, or alternatively, Leonard Nimoy–depending on how one defines what "this field" is.

16. **FNesti (not FNasty thanks :))**
October 5, 2013

Dear fuzzy

yes – the question of what "this field" is, is not a naive one, it contains the essence of the point. I may define a field as a topic plus the researchers working on it, with their respective ranking of academic achievements.

Thus, without cutting any freedom of speech, I believe one should not be considered a possible spokesperson in a field if there is someone else much more entitled to it (no matter how smart one may be, or the number of citations).

This point, which is a point of scientific integrity, I think is unsafely missed in Peter post.

Overall this operation, as the multiverse issue, is evidently an opportunistic push of a new slogan and of a group of people in the public arena (scientific and general public) by exploiting a great experimental discovery. As such I think it should be recognized.

As for Leonard Nimoy,

McCoy: Think, Spock – what's happening on your planet right now?
Spock: My people are barbarians… warlike barbarians.

17. **fuzzy**
October 6, 2013

this is it, the noble race of particle theorists was Vulcanian and now is almost completely degraded into Romulans. But enough for kidding, I agree with you fully, FNesti:

I join your "je accuse". I am defending since long this view, that we should call physics the physics. If you publish a paper with 5K citations, saying that gravity is modified at the mm scale, and this is not, it is a serious problem that requires assessment of the field. We cannot continue to speak of physicists, as if it was a god-given title: we have to speak of what is physics and what it is not. maybe you remember, "not even wrong".

18. **Yatima**
October 6, 2013

Off-topic but apparently the latest congressional pratfall consists in banning "the Chinaman" from entering NASA premises:

US scientists boycott Nasa conference over China ban: Nasa facing backlash from US researchers due to rejection of Chinese nationals from conference

Nasa is facing an extraordinary backlash from US researchers after it emerged that the space agency has banned Chinese scientists, including those working at US institutions, from a conference on grounds of national security.

Nasa officials rejected applications from Chinese nationals who hoped to attend the meeting at the agency's Ames research centre in California next month citing a law, passed in March, which prohibits anyone from China setting foot in a Nasa building.

The law is part of a broad and aggressive move initiated by congressman Frank Wolf, chair of the House appropriations committee, which has jurisdiction over Nasa. It aims to restrict the foreign nationals' access to Nasa facilities, ostensibly to counter espionage.

Will this be "enhanced" to other institutions (when they are not in "shutdown" mode?) My cynical me says "sure".

19. **Goran**
October 8, 2013

I strongly support what FNesti and fuzzy are saying about the danger of slogans in physics. Here they led to the incredible claim that if nothing new is found at the LHC, it will be proof of the multiuniverse (I keep seeing Pauli turning in his grave). This tragicomic argument reminds me how some in the fifties concluded that there could be dinosaurs on Venus.

The argument goes like this (as recalled by Carl Sagan in http://www.youtube.com/watch?v=Cj5A0rKI0Ag) : one can't see a thing on the surface of Venus, because it's covered with a dense layer of clouds. Clouds are made of water, therefore Venus must have a lot of water, thus the surface must be wet and there's probably a swamp. If there's a swamp there's ferns, if there's ferns… maybe there's even dinosaurs. In summary:
Observation: You could not see a thing.
Conclusion: Dinosaurs.

When I present this in my public lectures or colloquia, people laugh like crazy, especially physicists. And yet, amazingly enough, if you substitute 'dinosaur' with fancy words such as multiuniverse or landscape, it is often taken seriously by the same people who laugh at the above 'science'.

Why Are There Still So Few Women in Science?

October 3, 2013
Categories: Uncategorized

Normally I avoid writing about the topic headlined here, not because it's not of interest or not important, but because the usual discussions it attracts seem to me ideologically-driven, containing far more heat than light. The New York Times Magazine however has just published [an excellent article](#) on the subject, by [Eileen Pollack](#). Pollack describes in detail her experience as a physics student at Yale, including that of having a senior thesis supervised by the great representation theorist Roger Howe. This includes her decision not to go on in the field, with this description of the perception of the Princeton graduate program:

> By the start of my senior year, I was at the top of my class, with the most experience conducting research. But not a single professor asked me if I was going on to graduate school. When I mentioned shyly to Professor Zeller that my dream was to apply to Princeton and become a theoretician, he shook his head and said that if you went to Princeton, you had better put your ego in your back pocket, because those guys were so brilliant and competitive that you would get that ego crushed, which made me feel as if I weren't brilliant or competitive enough to apply.

I think Pollack very much gets it right, including emphasizing many of the subtleties of this problem, and urge anyone interested in this to read the article. A couple comments though about two aspects of the issue she doesn't really address.

- There is a serious effort at an institutional level to have an impact on this problem, but it takes place mostly only at specific points where the institution can measure what is happening. In particular, in my experience academic departments do take seriously the issue at the point of the graduate school admission process, with often a careful attempt to identify promising female applicants. This isn't at all inconsistent with Pollack's story, which explains why she didn't even apply to graduate school.

  At the point of hiring faculty, university administrations often provide serious incentives to departments to hire women (i.e. by providing faculty lines that can only be used for female or minority candidates). Again, this is often past the point where the problems Pollack identifies have already worked to make the number of viable female candidates small.

- Pollack repeats the claim of a serious shortage of students in STEM fields:

  > Last year, the President's Council of Advisers on Science and Technology issued an urgent plea for substantial reform if we are to meet the demand for one million more STEM professionals than the United States is currently on track to produce in the next decade.

  something which is actually only a shortage of talented people willing to work for low wages. She opens her article with the all-too-plausible results of a Yale research study showing that

  > Presented with identical summaries of the accomplishments of two imaginary applicants, professors at six major research institutions were significantly more willing to offer the man a job. If they did hire the woman, they set her salary, on average, nearly $4,000 lower than the man's.

  This is good evidence that attitudes (women's as well as men's, since they were just as biased) remain a problem. But Pollack doesn't comment on the absolute value of the salaries chosen as typical ($26,508 for a lab manager with a science bachelor's degree), which is less than what a typical Starbucks barista makes here in New York (see [here](#), where my location automatically gives the NYC data). Part of the story may well be women's differential willingness not only to deal with competitive ego-crushing Princetonians, but also abusive, badly-paid working conditions for many parts of the science job market. If the pay, hours and coffee are better at Starbucks (and your co-workers are nicer…), lots of people are going to reasonably make the decision to work there instead.

Update: As anyone could have predicted, allowing anonymous comments on a topic like this soon becomes untenable. On a more positive not, [Sabine Hossenfelder's reaction to the NYT article](#) is highly recommended.

Update: From Fabien Besnard

> I'm in a special position with respect to this question, since my institution, l'école polytechnique féminine was formerly for girls only. Up to the 1980's, the vast majority of french female engineers were formed in this "grande école".Then, since the other grandes écoles were gradually more and more open to girls, the EPF began to be perceived not as "the engineering school for girls", but "the engineering school for girls who can't go anywhere else".It reacted by opening itself to boys, while keeping its name "féminine". Few boys came at first, but the proportion gradually rose, until a point where the EPF was not anymore perceived as a "girl only" institution. Then the boys massively came. There is a now about 60% boys among the students, a proportion which is roughly constant for the last 10 years.
>
> The proportion of girls is still the highest in a school of engineers, and when you ask a student why she came, a very frequent answer is that it is precisely because she knew there would be many other girls. Another interesting aspect is that among the top 25% of the students, the proportion is rather inverse : 60 % of the best students are girls. The reason is that the best among the girls could have applied to one of the more selective "classe prépas" but refrained to do so for fear of being confronted to a stressful environment, with a lot of competition and… boys. Many of them seem to underestimate their chance of success in such an environment.
>
> Lastly, in the final year of our formation (which is the fifth), the students must choose an option that will largely determine their future career. We offer a wide range of choices, from aeronautics to medical engineering and computer sciences. It is a fact that girls do not evenly distribute themselves among these different options. However, there is a fair amount of girls in each one of them.
>
> So my experience largely confirms that the "negative feedback" effect of having too many samples of a single sex in a class acts as a magnifying glass on the small differences of taste between boys and girls, which nevertheless do exist. Also, the girls tend to underestimate their talents.

Comments

1. **CIP**
   October 3, 2013

   But this mostly ignores the fact that women have suceeded very well in lots of fields filled with giant egos and mysogynistic males, like law, medicine and pop music. The real question is what makes the situation in physics different.

2. **tt**
   October 3, 2013

   those fields all pay better ?

3. **Low Math, Meekly Interacting**
October 3, 2013

You would do well to solicit women in physics and mathematics, and only women in physics and mathematics, to comment on this post. Otherwise, you are right to avoid the topic. The discussion is inevitably both depressing and uninformative. With a female contingent feeling safe to open up simply state their impressions and experiences without being grilled over them, you might well still end up depressed, but you have a chance to actually learn something.

4. **Peter Woit**
October 3, 2013

LMMI,
Excellent advice. Please, women with something to say about this are highly encouraged to comment. Men, on the other hand, are equally highly encouraged to first reflect on whether or not they really know what they are talking about if they feel compelled.

5. **she physicist**
October 3, 2013

In my opinion, the main question asked in the article was answered in the book Pitagora's Trousers. Yes, as the book amply demonstrates, the physics as done by teams of men is a sort of unisex religious congregation where women trespassers are regarded as disturbances of the holy male soul.
As a female physicist who has worked both in a former communist state and the US, I encountered the most (in fact, the only) adversity and prejudice in the US academic environment.

6. **Koray**
October 3, 2013

I am a man, so I don't know what I am talking about. But, this post's last article makes an excellent point: perhaps there are few women in STEM because they know better. I sympathize with women who hear discouraging remarks and face discrimination, and I don't dispute their experiences, but I don't think anybody cares whether the men in those fields treat each other also like crap.

Everybody knows Steve Jobs' reputation. He'd totally crush the even the best of his people. Perhaps these fields are generally hostile & unrewarding (pay, social life, etc.) to everybody, so it takes the kind of person more commonly found among men to stick with it despite the abuse.

7. **Bill**
October 3, 2013

Those in power must encourage women at the highest level. For example, giving the Fields Medal to a woman would do more good for mathematics in the long run than "getting it right" (whatever that means).

8. **Jerry Moore**
October 3, 2013

I read this article about an hour before finding your post here. I agree with your analysis, it is a very well written article, but also has those groaners about the 1 million STEM "shortfall" and blithely mentioning the shockingly low salaries, although I wonder how much of the latter is because the people being polled were faculty, who are used to paying grad students?

Currently I have a female relative who is considering engineering as a college major. I want to encourage her, but also let her know what she faces (at least the salary issue isn't so bad). What a shame that such a thing has to be a mixed message, and that like the young women quoted in this article "we don't care what people think" is a necessary attitude.

9. **LV**
October 3, 2013

As a female physics graduate student from Canada, I thought I'd chime in with my (very positive) experience. In my last year of undergrad, when I was feeling particularly awful (who doesn't when they are panicking over exams while also trying to figure out what to do with their life?) I had several profs ask me in utter bewilderment why I wasn't applying to graduate school, and then offer to be my supervisor. Then, during my Masters, I had the most encouraging supervisor, the sort of professor who takes his teaching and mentoring responsibilities very seriously. Halfway through, I was feeling particularly inadequate and unable to do the work, but I felt comfortable enough to talk to him about my concerns, and it was thanks to him that I decided to continue on to do my PhD. I don't know if maybe the climate in Canada is different from in the US, or if I just managed to luck out, but I just wanted to point out that there are definitely physicists out there who are very supportive of their students, whether or not they are male or female. Emphasizing the good this does could maybe encourage others to start taking mentoring seriously as well. So many students I know drop out because their professors did not pay attention to them, did not take the time to provide access to data and information, and did not send them to conferences or schools where they could collaborate with and learn from others in their field.

10. **X**
October 3, 2013

I wonder if solutions to the problems of bias might be more welcomed if they weren't specifically indicated as being against sexism. Plenty of people are going to argue that they know they're not sexist, but no scientist would try to argue "Blinding isn't necessary for this experiment, because I know I'm not biased." There are plenty of biases (race, class, gender, attractiveness, social connections), and we could stamp out the lot at once using standard techniques that remove systematic error.

11. **Curious Wavefunction**
October 3, 2013

I think we need enough women in physics to set off a positive feedback loop; this seems to have happened in biology where the initial concentration of fuel "seeded" the next generation. Plus the article's point that culturally women seem to be much more discouraged to go into physics rather than biology seems very relevant. So is the point that both American men and women themselves seem to think of looks and intelligence in a woman as mutually exclusive properties. This has to change.

12. **averageGuy**
October 3, 2013

There's a blog set up that describes women's experiences being a professional philosopher and the amount of wretched sexism and misogyny they experience (http://beingawomaninphilosophy.wordpress.com/). Jordan Ellenberg blogged about it and asked if the situation was similar in mathematics. Izabella Lada chimed in and said that it was comparable. I imagine the same thing can be said of physics as well, and I would guess that one of the main issues with women avoiding STEM professions. Just from anecdotal experience, I am a male software developer, and I have a surprisingly large number of female colleagues (~60%), but they all hail from different countries that encourage women to participate in STEM-related subjects. Not a single American. It's not hard to see that the issue is specific to our cultural attitudes towards women and what their place is in society.

13. **another woman-physicist**
October 3, 2013

I see as a great problem the aging male majority in physics, which is still in charge (and getting even older by the minute). They have grown in time when there were even fewer women around in the field and they never learned to relate comfortably with women in professional setting. The fact that they feel uncomfortable with female students means that these students will not get sufficient encouragement or mentoring. We will be stuck with these unfortunate old guys attitudes for yet many years to come.

14. **Hack**
October 3, 2013

I have seen chauvinist pigs in action and it cracks me up that the supposedly brightest and most educated people on the planet are still a bunch of neanderthals! Those neanderthals should enter the 21st century, maybe they need to go back and take Sociology 101.

No I am not a woman, but I have seen women get marginalized quite often and drop out of graduate school or decide to leave their field after they graduate.

15. **Hack**
October 3, 2013

Sorry, have to add one more thing, if you have not noticed that women get marginalized in the sciences, you must either be at an extraordinary department (where it doesn't happen) or have your head in the sand.

16. **M**
October 3, 2013

A statistics from the European Union shows that women are the majority in soft sciences (such as psychology) and are the minority in hard sciences (physics, mathematics, engineering…), where success criteria are more objective. The difference increases in countries (such as Sweden) where people have more freedom to follow personal preferences.
The reason of the difference is simply that males and females have, on average, different interests, as confirmed by sociological and biological studies. The "gender" ideology that postulates no sexual differences and wants to explain differences with discriminations is wrong.

17. **lun**
October 3, 2013

While this topic is superficially different from the main thrust of this blog, some parallels do come to mind.
Most theoretical physicists will swear up, down and sideways that their field is the most meritocratic on earth, that it is truly the community of the smartest people in the world (as Arkani-Hamed put it in the lecture you blogged about a few posts ago) united in pursuit of the most basic problems in existence, and nothing except ability matters.

When you point out that this ultra-meritocracy is less diverse than Wall Street and most country-clubs in New York, theoretical physicists tend to get very uncomfortable and defensive, but not budge about the supposed meritocracy of theoretical physics. Many will never ever admit it, but quietly do believe that if there are few women and blacks in physics, it is ultimately because for whatever reason they are not good enough (Wink wink, what could this reason be? This is a great illustration of how "political liberalism in the abstract" and a desire for positive social change "in one's own backyard" can be very different things).

Fair enough. Except it is amusing that "the most intelligent and meritocratic people on earth" have made practically zero progress on all of the profound questions they spent the last 30 years pursuing! In fact, the people being defensive about physics ethnic/gender lack of diversity and people being defensive about string theory are often the same (there are, to be fair, quite a few commendable exceptions).

Is there a relation between the two phenomena? While the factors in lack of diversity in STEM are many and non-trivial (the article above is very good), group-think, self-selection (quality-by-reference-and-citation), a one-dimensional view of intelligence (intelligence=problem solving ability within the framework set by the senior people) and a view of academia as a rat-race contributes to both amplifying any pre-existing lack of intellectual diversity and enforcing group-think with regard to a particular idea.

An academic structure a bit more intellectually diverse and a bit less of a pressure-cooker would probably become more socially diverse too, for better or for worse.

18. **Peter Orland**
October 3, 2013

I once heard that the situation for women in astrophysics was better than in physics as a whole. Can any woman astrophysicists verify (or vehemently contradict) this statement?

19. **Kavanna**
October 3, 2013

Often, this situation is grasped at the wrong end of the stick.

Let's not underestimate the "bullheaded stupidity" factor, which males in their 20s exhibit to a greater degree than do females of the same age. How many of those male students will make it to tenure-track positions, much less tenure? The majority will drop out, either from their doctoral programs, or later on the academic career track. My experience is that the female students simply size up the academic job market better and sooner than do their male counterparts. They finish with either a bachelors or, often, with a masters, then leave academia for better and less chancy opportunities elsewhere.

This reality is closely related to the push to get more STEM graduates and immigrants, to allow employers to hire a continuing flow of younger and lower-salary workers, rather than be forced to hire older and more experienced workers at higher salaries.

That said, there's no doubt that prejudice is still a factor, although far less than 40 years ago. Much of it is a function of age, also abundantly confirmed by my own experience. Senior faculty under 60 or 50 are less prejudiced in the way described in the posting. It also varies significantly by field; for example, chemistry and biology have more women in them at the career level. But more women are attracted to those fields as students in the first place.

20. **Anon**
October 3, 2013

M above says it perfectly: "The reason of the difference is simply that males and females have, on average, different interests, as confirmed by sociological and biological studies. The "gender" ideology that postulates no sexual differences and wants to explain differences with discriminations is wrong."

How true. Most women I know are totally disinterested in science and would rather die than work in a field they consider completely boring. As ever, men get the blame, but this is nothing to do with discrimination. Women prefer doing other things.

21. **Peter Orland**
October 3, 2013

Anon,

"Most women I know are totally disinterested in science and would rather die than work in a field they consider completely boring."

So are most men. So what? Anecdotal evidence is a great debating tool, but it can't be used to establish facts (and there is nothing you said above which is factual). For that you need statistics.

22. **CU Phil**
October 3, 2013

This comment thread has developed in the most predictable manner possible.

23. **Z**
October 3, 2013

The situation is slightly better in astronomy, but only observational astronomy. There are very few distinguished or even tenured female astrophysics theorists (and the ones I know work at NASA as research scientists, where supposedly bias is less tolerated or overt than universities).

Then there is the whole LGBT issue which isn't even being commented upon in this discussion. I know of one physics/astronomy graduate student that is biologically male but psychologically female and is undergoing hormone therapy. I have been asked by one older male faculty member "what" this person was at a conference, with their face aghast. LGBT people, as far as I know, are nonexistent in tenured physics/astronomy faculty even compared to women and far far more needs to be done to recruit and retain them.

24. **averageGuy**
October 3, 2013

@Peter Orland: Agreed. Not only that, but if we are going to value anecdotal evidence then we should rely on the testimony of women because they are the group of people who are being pushed out of STEM occupations. From the article, women say the following:

pg 2:

Another student was the only girl in her AP physics class from the start. Her classmates teased her mercilessly: "You're a girl. Girls can't do physics." She expected the teacher to put an end to the teasing, but he didn't.

Other women chimed in to say that their teachers were the ones who teased them the most. In one physics class, the teacher announced that the boys would be graded on the "boy curve," while the one girl would be graded on the "girl curve"; when asked why, the teacher explained that he couldn't reasonably expect a girl to…

pg 3:

After the tea, a dozen girls stayed to talk. "The boys in my group don't take anything I say seriously," one astrophysics major complained. "I hate to be aggressive. Is that what it takes? I wasn't brought up that way. Will I have to be this aggressive in graduate school? For the rest of my life?" Another said she disliked when she and her sister went out to a club and her sister introduced her as an astrophysics major. "I kick her under the table. I hate when people in a bar or at a party find out I'm majoring in physics. The minute they find out, I can see the guys turn away."

And I'll stop quoting because I'll end up quoting the entire article. Frankly I find it a bit disturbing that women's first-hand testimony is being dismissed so callously.

25. **Peter Woit**
October 3, 2013

Please all, unfortunately I agree with CU Phil. Unless you've got something particularly insightful to say about this, please restrain yourself. The phenomenon of men convinced that their voice must be heard on any topic is actually part of the problem.

And bringing in other hot-button topics that are not really relevant and guaranteed to draw dumb arguments is not helpful (no, I don't think the solution to the gender-diversity problems in science is that we need to do more to encourage biological males, but just the ones interested in gender transformation).

26. **srp**
October 3, 2013

Based on the original post, most problems in the field would be solved if physics departments served lattes in addition to their other functions. Or perhaps a chain of CERN cafes?

27. **Jeff M**
October 3, 2013

Just a couple of reasonably well informed (I hope) comments. I'm chair of a math department, I've been involved in a bunch of searches. I don't think the issue can much be that women don't go into physics because they realize that there's no future in academic physics – while the job market in math isn't great by any stretch, it's much better than physics and we have the same problem finding women. In my experience plenty of places are trying, but not so much at the highest levels. A "critical mass" of women somewhere really good would definitely help, it already has. Carolyn Gordon has been at Dartmouth since I was in grad school (a long time ago in a galaxy far far away) and she has produced a ton of great female students, many of whom have stayed in academia. A few of them are at research 1 schools. Lipman Bers (my thesis grandfather) had about 50 PhD students, 15 or so were women (he was active from the 40s to the 80s, mostly at NYU and Columbia, top schools). A number of them (Tilla Klotz, Linda Keen, Lesley Sibner at least) went on to have students of their own.

28. **LogicFan**
October 3, 2013

This is indeed a problem, including in my field, philosophy. It is a problem which thankfully we are all waking up to. There is not much research on the reason for the gender imbalances, and I think figuring out, empirically, why women are not present in equal numbers should be the first step. We have little data on the problem in philosophy, but that which we do have suggest that we "lose" women at the undergraduate level, before they declare their major. We don't know why this is the case.

One totally unhelpful approach is to ask only "women in physics and mathematics, and only women in physics and mathematics, to comment on this" and then to regard that as "Excellent advice". That is terrible advice. This is a problem that confronts our professions as a whole, and the solutions are going to come from the professions as a whole. I certainly agree with Peter Woit's assessment that discussions like these become "ideologically-driven", but that's partly owing to a lack of diversity of opinion. The idea that only women know why there are so few women in science is ridiculous (e.g. some of the leading researchers on gender imbalance are men).

One important question to address is this. I tend to think that the reason we lose female philosophers at the pre-major declaration level is that women do not find the career attractive. In just the same way that women do not find being a plumber an attractive career. If this is true, whose interest are we serving in promoting greater equality? It may be, as lun interestingly observed, that the academic mission may benefit from greater gender diversity. I believe this to be true in philosophy. But then we are not doing it for the sake of women. This may be unobjectionable, but it does imply that the women are the means rather than the end.

29. **Anonyrat**
October 4, 2013

In response to CIP:

Women in law in the US: (PDF file)
http://www.americanbar.org/content/dam/aba/marketing/women/current_glance_statistics_feb2013.authcheckdam.pdf

While they're doing better than women in physics, it isn't yet a bastion of equality.

Likewise, women in academic medicine in the US: (PDF file)
https://members.aamc.org/eweb/upload/Women%20in%20U%20S%20%20Academic%20Medicine%20Statistics%20and%20Benchmarking%20Report%202011-20123.pdf

Seriously, pop music? In any case, women went through their difficulties there as well. e.g., (powerpoint)
http://virtual.clemson.edu/caah/women/ws301/ppt/wopopmus/Women%20in%20Pop%20Music.ppt

It is still within living memory that women could not join the Musicians' Union.

I suppose physics is on the lagging edge of society as far as opportunities for women are concerned.

30. **Peter Woit**
October 4, 2013

I'm deleting all of the flood of incoming comments from men explaining that the answer to the question of the title is just that women are stupider than men. Sorry, but such a discussion is great for getting people worked up, but never leads to any insight about anything,

31. **usually `amused' but not this time**
October 4, 2013

"In particular, in my experience academic departments do take seriously the issue at the point of the graduate school admission process, with often a careful attempt to identify promising female applicants."

At my university (somewhere in Asia) we had an outstanding female undergraduate student. She completed the 4 year maths honors program in 3 years, and still graduated as the top student of that year. To put this achievement in perspective, at this university we are able to recruit some of the best students from China and elsewhere in the region thanks to our many undergraduate scholarships (approx 30-40 per year). The standard of these foreign scholarship students is very high, most of them are very ambitions and motivated, so graduating as the top student in that group is quite an accomplishment.
Besides that, she demonstrated ability to do maths research at a high level: The research problems she solved for her undergraduate thesis led to 2 publications in the quality maths journal C.R.Acad.Sci.Paris, joint with her advisor. (Her advisor gave her the problems, she solved them.)

I and others strongly encouraged her to apply to top universities for PhD, telling her that she belongs at such a place (which she clearly does). (I'm not her thesis advisor but know her from other contexts.) But there was no need to encourage her because she was already very motivated to do exactly that. Her dream was to do PhD and postdocs(s) at top universities in USA and then get a maths faculty position at one of the best universities back in China.

Well, she applied to Princeton, Harvard and other top universities for PhD and was turned down by all of them.

Her reaction was to assume that it was because she just wasn't good enough to be admitted to those universities. While she she had 2 publications in a quality maths journal, the students who get admitted to Princeton etc probably have 5 or 6 such publications as undergraduates, right?

I'm puzzled about why it didn't work out for her, considering that we've had (male) students admitted to top universities for PhD in the past and her accomplishments are better then theirs. One factor could be that her undergraduate thesis advisor is a non-famous person and I have doubts about his ability to do a good job with the recommendation letter. But that shouldn't matter if the selection committee is really making "a careful attempt to identify promising female applicants". Relatively weak recommendation letters for strong female applicants is the most obvious thing to identify and adjust for.

Another thing that makes this disappointing is that, despite the hype, the average PhD student at Princeton etc is really nothing special. E.g. many of the physics PhD students can't manage to publish a paper on their own in PRL, and some of them can't even do research without having their hand held/riding on a coattail…. It's ridiculous that the talented female student in this case is probably going to go through life thinking that she wasn't good enough for Princeton while the reality is that she is probably better than the majority of the PhD students who were admitted there.

32. **NumCracker**
October 4, 2013

The title is quite biased. There is still so few women in HEP physics, not in Science in a broad context.

33. **woj**
October 4, 2013

Our 8 year old daughter has joined CAGIS (Canadian Association for Girls In Science)

http://www.cagis.ca/

This is from their website:
Many years of research throughout Canada and the United States has shown that girls begin science education at a disadvantage and fall behind in school, not because of lack of interest, but because of lack of exposure. A recent University of Michigan study found that giving girls hands-on science activities helped close the gap.

34. **Andrew Foland**
October 4, 2013

So I'm a man, but would like to pass along a talk by a woman that helped me at least understand some things: http://www.cfa.harvard.edu/cfawis/kathryn_johnston.pdf

(I think you might even have mentioned this talk before, Peter)

35. **Kavanna**
October 4, 2013

My experience of divergent science careers of men and women in other countries is limited and somewhat out of date. But it's worth relating.

European countries gave me the impression of being divergent themselves. Germany seemed moderately negative for women, France quite negative, Britain and other northwestern European countries modestly positive, with Spain and Italy the most positive (surprisingly). I don't know much about eastern Europe.

One commonality between France and the US is that, at least in the universities, the culture is very hierarchical, with a rigid pecking order, and a long path to getting a tenured position. The other systems seem less rigid. Contrary to what many Americans think, the US academic system is one of the world's most rigid, as many scientists (mostly male) have related to me over the years. The rigidity has an effect on everyone, but differentially more so on women. The French system is famously rigid.

Everyone should also think about science outside academia, in government and industry. The sex differences are generally less, sometimes strikingly so. We spend too much time obsessing about academia, instead of seeing how odd academia is.

36. **Peter Woit**
October 4, 2013

Also being deleted: all the people who want to go on in general terms about the "nature of men" or the "nature of women" and how they are different. Hardly anyone I know well fits into such mindless categorizations, and these have nothing really to do with the specific experiences and issues raised in the article I linked to.

37. **Anonymous Math Faculty**
October 4, 2013

To "usually amused":

I've served on the graduate admissions committee for a top mathematics program in the U.S., and one of our hardest problems is evaluating applications from China. The recommendation letters are all uniformly glowing, and all the applicants do very well on the GRE. So the only things we have to go on are the grades, which it seems really only tell us anything useful for the top few schools in China (and apparently not even then if the applicant is "well-connected").

38. **Anonyrat**
October 4, 2013

Some posts on Alexandre Borovik's blog (in Peter Woit's Math Blog list) are relevant.

http://micromath.wordpress.com/2012/02/25/gender-biases-in-early-number-exposure-to-preschool-aged-children/

http://micromath.wordpress.com/2012/02/25/girls-verbal-skills-make-them-better-at-arithmetic/

http://micromath.wordpress.com/2008/12/26/women-and-mathematics/

http://micromath.wordpress.com/2012/07/22/women-in-the-violent-world-of-mathematics-6/

39. **Lynne Jolitz**
October 4, 2013

Peter,

I am a Berkeley physics alumna. I also have a daughter who is completing her senior year at Berkeley in physics and mathematics (double major) and preparing for graduate school. She is also President of the Society of Physics Students chapter at Berkeley, so she interacts with many physics majors, men and women.

Because we have both gone through the program, we have shared many perspectives on how things have changed for women in physics and how things have stayed the same.

One thing we both agree upon is that the Berkeley physics department is, overall, receptive to women, with women in both leadership and research positions. When I was a student, there were few women but they were actively recruiting. Now there are quite a few women at the professorial and graduate student level.

But socially, physics has not improved among the students. Even though there are many women physics majors, it was difficult for my daughter to convince women to join in SPS activities, such as faculty-student lunches and special seminars. "Are there any other women involved?", would be the usual tentative question. The fear of being the only woman in a sea of men (many of whom are still very immature and vulgar — even in front of faculty) has made it unattractive to many women.

I saw the exact same problem (and we *did* have only a handful of women) in physics when I was at Berkeley. It was especially acute in plasma physics. I ended up as the only woman student in my senior year plasma physics course due to the difficulty of the course and the winnowing effect of small numbers (my professor, though, was an absolute gem and did not allow any misconduct in his class). I ended up doing all my upper division lab work alone because I could not get anyone (all men) to be my lab partner even though I was an excellent lab student. I never was invited to a study group, and I did all my problem sets in the library alone.

Unlike the other few women in the major, I had the support of family and an active entrepreneurial life outside the university. I made an effort to attend seminars (both in physics and CS), ask questions, and meet faculty and people in industry so I would not feel isolated nor uncomfortable at being "the only woman in the room". This has served me well in my career in Silicon Valley.

It is through seminars, courses and events like faculty-student lunches where students, men and women, develop the relationships to get advice and encouragement towards graduate school. My daughter is working to encourage both professional interest *and* civility with the encouragement and assistance of faculty, but she says it isn't easy.

Bullying at the undergraduate and graduate level is a severe problem, and misogyny and harassment is a common attack point to undermine perceived rivals. I don't claim this is induced by the study of physics, but instead this is an ugly reflection of our society. To ignore it or belittle it (the old "toughen up" nonsense) degrades the professionalism and scholarship of any program and should not be tolerated.

Ironically, I was also Treasurer of the Computer Science Undergraduate Association at Berkeley at the time and there were plenty of women in that "male" major (approx 40%) who would tell me about immense amounts of harassment and vulgarity, yet they endured it because CS was an exciting field. Now the percentages are reversed, so the progress in physics and math has been met by a drop in CS and engineering.

CS and engineering, frankly, never did deal with their "woman" problem. The "toughen up" approach to harassment and bullying was endured while it was a lucrative career option, but it was never a sustaining one.

I love physics. And I loved my experience in the Berkeley physics department. It gave me the tools and the courage to look at problems and solve them in new and innovative ways.

I would hope we will continue to face openly that bullying and contempt for individuals studying physics based merely on their gender, their race, or their beliefs is damaging to physics, to scholarship and to our society.

40. **Thomas Themel**
October 5, 2013

I think it should also be mentioned that the quoted anecdote took place in or before 1978 according to the article.

41. **usually `amused' but not this time**
October 5, 2013

Anonymous Maths Faculty,

Thanks for responding. The situation in this case is a bit different from what you seem to be imagining. I understand the problem you mentioned since I have to deal with it myself when assessing PhD applications from China. But ours is not some random unheard of university (and not even in China). We are in the top 100 in the world in a couple of rankings, and our best maths/physics graduates regularly get admitted to top universities for PhD. In the past we have had students accepted at Harvard, MIT, Berkeley, Stanford,…(I don't remember hearing of anyone accepted at Princeton but there might be someone).
So when our top graduate, who happens to be female, doesn't get accepted for PhD at any top university, even though her accomplishments are clearly superior to our other graduates who were accepted at top universities in the past, then it is very puzzling… Especially in light of the claim that there is "a careful attempt to identify promising female applicants".

You mentioned the problem of assessing students from foreign universities – glowing references from faculty you haven't heard of, and stellar grades in academic systems whose standards you aren't familiar with – and I understand the difficulties that causes. But I think you know the quality of the maths journal C.R.Acad.Sci.Paris… This student had 2 publications there from her undergraduate thesis research (as I mentioned)… So your excuses are void in this case.

In your experience, how many publications in quality maths journals as undergraduates do the successful PhD applicants to top universities have on average? (My guess is less than 0.5.)

Isn't it true that, when assessing PhD applications to top universities, recommendation letters from famous people at illustrious institutions trump demonstrated research ability as documented by publications in quality maths journals as an undergraduate? This is a significant consideration for the general issue of gender bias and other biases, since recommendations depend a lot on the "gut feeling" of the writer, in which all kinds of biases can manifest themselves. On the other hand, quality research publications are a much more objective indicator of the quality of the candidate.

Isn't it also true that the "careful attempt to identify promising female applicants" in PhD applications to top US universities is intended for American female students from elite undergraduate institutions in cases where they are less confident and/or motivated, causing them to do less well than male peers, but does not include highly motivated and confident foreign female graduates who massively outperformed their male peers and published papers in quality maths journals as undergraduates, etc.? For those foreign female applicants it's business as usual as far as gender biases go, right?

42. **anonymous**
October 5, 2013

Question for usually `amused' but not this time:

Did any slightly less qualified students from your university get accepted to the same top graduate programs in the same year that the woman who was rejected did? Sometimes schools accept fewer students in general, especially foreign, during economic dips. Depending on the year, it's a possibility. That said, the situation you describe sounds like a quite unfortunate failure of the system.

Anyway, speaking as a man, I think I agree most with woj that the main problem is lack of exposure. This imbalance begins long before college. And by the time students get to college, how many physics survey courses cover what physicists actually do? Pre-meds learn about springs and pulleys and levers and spherical cows in a vacuum. Actual physicists learn about (and actively study) dark energy and dark matter and Bell inequality violations and non-Abelian anyons, and… People (and by that I mean Americans) grow up in a society where physics is considered a boring subject, where only (white) men have the time and privilege to concern themselves with – nay, are expected to be responsible for – the boring stuff that nobody else wants to waste his or her life doing. Thankfully my many female labmates and collaborators (integrating through the years even two open lesbians and a bisexual) have seen through this social construct. They are at once totally excellent at physics and totally girly or at least comfortable with who they are. And I assume that springs and pulleys are as equally boring to them as they are to me.

43. **Paul**
October 5, 2013

I am in the MIT math department. Do you have an outstanding female undergraduate student who you've taught personally (!), and who would do great in our PhD program? Email me in early January and give me the applicant's name, so I can double-check. Nothing specific to us, I'm sure it would work elsewhere as well.

44. **anonymous**
October 5, 2013

Here's another way of framing the argument I said about lack of exposure. How many times do parents think, "My child is a girl, so she's probably not interested in visiting the science museum this weekend"? You would hope that her science classes in school would counteract this tendency. Then again, how many science teachers at the elementary or middle school level know anything at all about dark matter? Not many, I imagine, because their education major didn't require it. So young students end up learning that physics is either something esoteric that Newton, Einstein, and a few other great, white men have already solved, or just a bunch of boring springs and pulleys. This is a safe approach for an elementary science teacher who doesn't want to be confronted with students' questions about dark matter that the teacher can't answer.

I believe it would be much more helpful to portray physics as a list of mind-boggling open questions that are so difficult that only diverse, multi-gender, multi-racial, international collaborations can solve them. Really? We only understand a small fraction of the universe? I personally can grow up to be one of the team players who discovers the rest of the universe? And I can even levitate frogs and shoot lasers at shiny objects along the way? This approach might be outside the comfort zone of the existing pool of elementary science teachers who like to give their students the illusion that they know everything. But I believe it's the right way to do it, especially because it has the benefit of being true.

With this new approach, once there are enough "seeds" at an earlier level, you can eventually reach a critical mass of women by the time you get to the college or professional level. Sure, there might be some remaining bias against hiring, for example, a pregnant woman. But at least with enough women around in general (and enough of the old, white men dying off) the bias will be at a level more comparable to what exists in other fields.

45. **vmarko**
October 5, 2013

IMO, there are apparently at least two diferent effects at play here. One is the society-level bias, discouraging females from getting interested in physics via a passive gender stereotype images all arounsd us. This starts from an early age, and continues throughout life, making too few women getting professionally involved in physics to begin with. The second effect is the professional bias in academia, that happens to that already small number of women who did get involved in physics.

We can fight against the latter inside academia, but the former needs to be addressed at the level of global society, which is much more inert, and needs much more time to fix.

Best, 🙂
Marko

46. **Banona**
October 5, 2013

> Paul says:
> October 5, 2013 at 11:39 am
>
> I am in the MIT math department. Do you have an outstanding female undergraduate student who you've taught personally (!), and who would do great in our PhD program? Email me in early January and give me the applicant's name, so I can double-check. Nothing specific to us, I'm sure it would work elsewhere as well.

This is without a doubt the most disgusting comment I have ever read in this blog. Someone willing to bend the rules because an applicant is female.

All this talk of too few women somewhere always evolves into this. Special access to people that just can't cut it and want to game the system through sociological excuses.

BTW Peter, remove this again and I will post it again, and again, and again. I will never let go because quite frankly, this is too bad to just ignore.

Beyond the God Particle

October 6, 2013
Categories: Book Reviews

There's a new popular book about high energy physics coming out this week, Beyond the God Particle, by Leon Lederman and Christopher Hill. The authors are unapologetic about the "God Particle" terminology, coined by Lederman back in 1993 for marketing purposes, which for better or worse is now a fixture in popular accounts of the Higgs.

The new book isn't really a general introduction to the subject, but is focused on two pretty much unrelated subjects. The first is the actual physics of the Higgs field, with a long and detailed explanation of chirality and the way in which interaction with the Higgs field provides particle mass terms. This is great material for anyone who has been subjected to endless attempts at explaining this as the Higgs being like molasses, or a room full of people, or any number of other metaphors that don't really explain anything. Lederman and Hill go way beyond this, with a much more extensive and serious discussion, while still staying away from using equations. For someone who wants to understand as much as possible about what "particles get mass from the Higgs" means without looking at a Yukawa term in a Lagrangian, this is the place.

The second main topic of the book is Project X, Fermilab's proposed new high-intensity proton linac that would provide beams suitable for studying rare decays, neutrino physics, potential muon storage rings, and new sorts of fission reactors for nuclear power. This is pretty much the centerpiece of plans to try and keep US in the game of cutting edge experimental HEP physics. As far as the energy frontier goes, the situation at the LHC is explained, with the argument made that on that front, all there is to do now is to wait and see, with 2017 the date by which the authors expect to have a verdict about whether there is new physics to study at the TeV scale. Only once this is in do they see an informed decision about a new high energy machine to be possible. As far as the last 30 years of theorist's claims about BSM physics, they're dismissed with:

> Our fellow citizens often get confused about what big science is trying to do, perhaps because of what we tell them, usually in the media. For example, all too often we hear that colliders are built "to discover extra dimensions," to "confirm string theory," "to discover supersymmetry." False! Colliders are built to uncover *whatever is happening* in nature at the shortest distances, and not to accommodate the agendas of various sects of theorists.

Throughout the book there's a vigorous argument that science in general and HEP in particular deserve far more financial support from the public than it is getting. On the whole I'm in agreement, but I do think the authors go over the top at a couple points. The short discussion of cosmology is HEP-triumphalist:

> The great discoveries, such as the "gauge principle" shared by all forces in nature, allowed us to speculate about "grand unification" and led to the idea of "cosmic inflation" and canonized the field of cosmology. Suddenly cosmology became respectable. The leading cosmologists are all particle physicists.

The argument for the societal value of scientific research dismisses economists as "eggheads" too dense to realize that there's a simple answer to the question "What makes economies grow":

> The answer is almost obvious, yet it took more than 200 years from Adam Smith's *The Wealth of Nations* to figure it out. The answer is (drumroll): *economies grow because of investment in science!* Basic science, applied science, all science. All scientific research pays a handsome dividend, and the more science the better.

Given the current dysfunctional US government, funding valuable new tools like Project X will be a challenge. Lederman will be at the front of the charge to make this happen, and this book is one weapon for the fight ahead.

Comments

1. **LA**
   October 6, 2013

   authors*

2. **Z**
   October 6, 2013

   It can't be a coincidence the timing of this book is the same week as the physics Nobel announcement. Do Leon Lederman and Christopher Hill know something we don't?

3. **Peter Woit**
   October 6, 2013

   LA,
   Thanks. Fixed.

   Z,
   I doubt it. But who knows, maybe the publisher's marketing people chose the release date to coincide with the announcement, just in case…

4. **uair01**
   October 6, 2013

   The answer is (drumroll): economies grow because of investment in science!

   I totally agree, but just one question (and I'm serious, not trolling):
   Is HEP not too far removed from anything earthbound to ever be of practical use?

   I know the examples of theoretical mathematics suddenly becoming practical (cryptography). I realize that building HEP "playthings" will produce a lot of spin-off in (computer) engineering. I would agree that just searching for "big answers" is valuable on its own. But do you really expect that we will ever use the results of HEP research in something practical? (Like using abstract physics for quantum computing?) If so, what would be the first HEP result to produce some results that could be used business-wise?

5. **Peter Woit**
   October 6, 2013

   uair01,
   The main problem with finding direct applications of possible new HEP physics discoveries is that they typically involve particles with very short lifetime. It's hard to imagine what useful you could do with a Higgs particle (even if you could easily produce lots of them) given their lifetime of 10^-22 seconds or whatever. But, if a new

stable particle was ever discovered (for instance, conjectured dark matter), then one might very well find practical uses of such a new form of matter.

Neutrinos are stable, and maybe one can imagine discovering something new about them which would make them more useful. Already, here's an idea http://www.math.columbia.edu/~woit/wordpress/?p=4646

6. **K**
   October 6, 2013

   Project X is in danger since some time.
   The US budget is not the only problem. The director at Fermilab
   has recently changed and the new one hates Project X, so….

7. **Peter Woit**
   October 6, 2013

   K,
   If the FNAL director hates Project X, what do you suppose his vision for the future of FNAL is?

8. **Mathematician**
   October 6, 2013

   I only buy books with the word "God" in the title if they are written by Richard Dawkins.

9. **srp**
   October 6, 2013

   Leon Lederman is a great salesman and I start out with an emotional bias in favor of investment in HEP. But you do have to worry that at some point you're doing the equivalent of building giant stone statues on Easter Island. Given the small size of the HEP budget relative to the economy, that conclusion seems far away to me. The fact that very intelligent people want to work on these projects and are willing to put up with low salaries and job security to do it also suggests that the social cost is low relative to the benefit. But sometimes the enthusiasts' arguments prove too much, as Lederman seems to in the quotation provided in this post.

   It does baffle me why there is no community push for real money and manpower to be spent on investigating advanced accelerator concepts, a policy which would make sense on the scientific, technical, economic, and marketing levels. It's like asking for money to build history's largest Yankee Clipper instead of experimenting with steamship technology. Harder to sell, more expensive, and less likely to be productive in the end.

10. **Peter Shor**
    October 6, 2013

    @srp: The LHC has just started, and isn't even at its full strength yet. It seems to me that there's lots of time to wait before starting to spend real money on the next collider (either in terms of advancing collider technology, or making detailed plans for one we know how to build).

11. **Tim May**
    October 7, 2013

    OK, to fund XHEP (the extreme HEP beyond the LHC, costing perhaps 10-50 times more, etc.) or to not fund it.

    But as someone who worked for Intel in the 70s and 80s, and now invests in high tech, the connections between HEP and electronics/computers/etc. were pretty much gaseous even in the late 50s.

    The energy scales tell the story. Electronics is happening at non-nucleonic energies.

    The Bevatron in the 50s was already outside these scales, by a wide margin.

    Detectors? Computers? Networks? The WWW itself? Second order effects.

    I think the stuff on this blog is fascinating. I think cosmology, fire walls, etc. is really great stuff, to talk about and think about. Maybe in some distant century we'll be able to to test these theories. Maybe even some telescopes (a lot less expensive than a 500 TeV machine) may turn up something interesting. But arguing that a new accelerator should be built to smash things together at 100-500 TeV, costing the GDP of a European country or two, and saying it may help with economic growth is just not well-supported.

    –Tim May

12. **jd**
    October 7, 2013

    Some decades ago a Nobel prize winner in HEP theory talked individually with several of us and recommended that we not pursue a career in HEP theory. Instead he strongly encouraged us to engage in finding new directions other than just building the next large accelerator. He felt that the current course was not sustainable. I agree, although I did not anticipate that there would be three or so larger and larger machines. I did not follow his advice as I was not interested in HEP at all. Just as I do not believe in unlimited population growth, this brute force approach to HEP experiments makes little sense to me.

    I have noticed on these HEP blogs that little attention is paid to the wish lists of other areas of science and to the large dollar amounts involved.

    NIF 7.5B to date, 300-400M/yr continuing
    ITER 20B for construction
    DEMO and K-DEMO similar to ITER

    LIFE and HiPER similar to NIF
    Nuclear physics facilities – the field is struggling
    Uranium facility at Oak Ridge – I think 6B
    Fission reactor research
    Big telescopes
    Light sources
    NASA

This list can probably be added to. I am sure each area has arguments for support.

13. **M. Wang**
October 7, 2013

I'm sorry, but very very few people in the real economy would agree that science is a main driver of economic development, much less the main driver.

Different schools of thoughts may have different opinions on what the answers should be to the question: what makes economy grow, but there are three factors that will appear with the greatest frequency: engineering technology, infrastructure and division of labor. The science behind building an iPhone had been understood for decades, but the technology still took the genius of Steve Jobs to get right. Better roads and better telecom cost trillions of dollars but create values many times that by increasing the participation of people, land and resources in the economic network.

So, infrastructure allows full utilization of resources everywhere, and superior tools resulting from superior technology improves productivity. The final driver of economic progress is greater division of labor, which allows greater specialization that in turn leads to higher quality and greater creativity. This requires the right legal and social environments as well as efficiency of scale.

As for pure science, take HEP for example, the economic return is minuscule. Once the research frontier gets past stable and semi-stable particles (like muons), no technology can be built out of it. Its only connection with the real economy is now in form of stories being told. This is the fundamental reason why string theorists can get away with their ever shifting stories. Can you imagine iPhone depending on field theory or beyond?

To say science is what makes economy grows is an ultimate conceit of an egghead. It does not help the cause of science; in fact, it shows the public how detached from real lives these eggheads are. In a sophisticated modern economy, there is plenty of room to justify spending on science as the pursuit of truth just like arts as the pursuit of beauty. To make grandiose but ultimately unfounded claims is counterproductive.

14. **Yatima**
October 7, 2013

I can only agree with M. Wang. HEP at least should be considered a peculiar form of art, coupled to "industrial policy" (i.e. subsidies and political bling), which may have some trickle-down benefits to systems engineering. Still better than the rabid destruction of value that a project like the F-35 is, but let's not go there.

15. **srp**
October 7, 2013

@Peter Shor: The point of AAC is to explore for much cheaper ways to get to higher energies. The amounts needed to do technology exploration would not be on the scale of building an actual accelerator, and if the work panned out then there would be the option to get way more bang for the buck. Of course, you might find out that none of these advanced concepts are feasible.

16. **Jens**
October 7, 2013

M. Wang,

It can be argued that the World Wide Web is a spin off from HEP, as it was originally created to allow HEP scientists share data more easily. This sort indirect economic return may be what Lederman has in mind.

17. **piscator**
October 7, 2013

>>Can you imagine iPhone depending on field theory or beyond?

Yes. GR is a field theory, and if you can imagine an iPhone using GPS then you can imagine an iPhone using field theory.

In any case, HEP as an international and collaborative enterprise is not expensive on the scale of government expenditure. Compared to budgets like health, defence or social security it is a rounding error. HEP is a few per cent of the science budget, which in turn is a very small fraction of the overall budget.

18. **Michael Gogins**
October 7, 2013

Mr. Wang:

"Different schools of thoughts may have different opinions on what the answers should be to the question: what makes economy grow, but there are three factors that will appear with the greatest frequency: engineering technology, infrastructure and division of labor. The science behind building an iPhone had been understood for decades, but the technology still took the genius of Steve Jobs to get right. "

This does not refute Peter Woit's argument that basic science drives the economy. All it does is say that it takes a long time to realize the payoff.

Also, of course, it is by no means predictable WHAT basic science research will end up paying off economically.

This is a situation where if one does what is fun or interesting or satisfies one's curiosity, one's descendents receive a fabulous economic reward, whereas if one tries to identify targets for purely economic investments with short-term returns, for example by publicly funding only engineering research, one impoverishes the grandchildren by comparison.

19. **imho**
October 7, 2013

Hi M Wang. I understand what you think you are saying, but it seems to me you really have no idea what you're talking about. Of course the iPhone depends on field theory. Condensed Matter Physics is full of Quantum Field Theory and has been directly responsible for almost the entirety of electronics. Every optical device ever constructed requires a Classical Field Theory tour-de-force. Chemistry, Materials Science and Engineering, Electrical Engineering, etc, etc depend completely and utterly on Science. Go to any large company and research the background of the "Engineers" who build all of these products, check out the R&D departments. You will find Physicists, Mathematicians, Chemists, Biologists. To even think that science is not (at first order) directly responsible for the majority of today's economy is laughable.

I think perhaps you meant a small minority of Physicists – who constitute a minority of a minority of scientists – engage in research that isn't directly related to economics. But even that's arguable since Wall Street hires HEP theorists like Wal-mart hires cashiers.

20. **Peter Woit**
October 7, 2013

Re: "Peter Woit's argument that basic science drives the economy"
I should make it clear that this isn't my argument, it's that of Lederman/Hill. Their form of it I described as "over the top", since I think they are over-simplifying a complex issue. I don't believe that all science, at all times, funded in any amount, necessarily pays dividends. This oversimplification is a rather convenient one for scientists to believe in, since it allow one to evade what can be a difficult issue. In the case of experimental HEP research, I do think the amounts spent on it can easily be justified, but you have to work harder than just saying that it's science, and more funding for science is always justified.

21. **lun**
October 7, 2013

M. Wang wrote:
"Different schools of thoughts may have different opinions on what the answers should be to the question: what makes economy grow, but there are three factors that will appear with the greatest frequency: engineering technology, infrastructure and division of labor. The science behind building an iPhone had been understood for decades, but the technology still took the genius of Steve Jobs to get right. "

Let me correct on you this: It took the marketing, business ability and luck of Steve Jobs to make the iPhone the worldwide brand it is. The technology for mobile communication was finessed by many many engineers of many companies, but was ultimately developed, on the software side, by people like Dennis Ritchie of the basic science institution of Berkeley (who died 2 weeks after Jobs and was completely ignored by the US press, despite his contribution to computing being MONUMENTALLY higher than Jobs's), and on the hardware side by big lab (NASA, Bell labs etc.) engineers. A lot of them working on fundamental science related projects, like accelerators and space exploration.
Of course, unlike Steve Jobs, such projects are in practice "open source", anyone can take advantage of their results. So you do not see the profit in the economists balance sheets: the profit gets distributed and multiplied across all society, including Apple or whoever is built the Linux-box which I am using to type these lines. Google might be a better example… except it was started by people doing "basic science" (Computer scientists at Stanford),and, despite all its questionable corporate practices, does retain a lot of basic science's ethos (it produces a lot of open-source software and runs a basic science program, admittedly not in particle physics).

I hope Peter forgives my cheesed-off tone here. Whenever the US press uses "Genius" and "Einstein" in the same sentence as Jobs and Zuckerberg, I literally want to puke. And to bring this on-topic, the problems described in Peter's blogpost are exactly due to this: If the latest corporate wheeler-dealer is the "genius" but the paradigm-shifting researcher is the irrelevant burden on the economy, of course science will wither. As a scientist, I have my own experimental prediction of how the economy will follow after that.

22. **Thomas Larsson**
October 7, 2013

The quark model is celebrating 50 this year or next. It is a spectacular scientific success, but has it had any technological application whatsoever?

23. **cormac o raifeartaigh**
October 7, 2013

Why not small g? god particle is much better and less offensive to some

24. **Bob Jones**
October 7, 2013

Terrible news:

http://www.israelnationalnews.com/News/News.aspx/172589#.UlKd2GTzbuU

25. **M. Wang**
October 7, 2013

I appreciate your replies to my first comment. Let me clarify a few issues.

When most people talk about "driving the economy", they mean certain and quick causality relationship that a policy can be built on. The first problem with science trying to take credit here is that it is highly uncertain and always several steps removed from actual applications. Of course, everything that goes into an iPhone was once a top of cutting-edge science, but to make it to the real application takes decades of efforts from many groups of engineers and designers. Only the last group get to bask in glory. You may call this unfair, as all engineering begins with science. To that, Keynes once said, "But this long run is a misleading guide to current affairs. In the long run we are all dead." Likewise, in the beginning we weren't born yet.

The second problem is that good science is almost always open source, and therefore what the economists call a public good. It is well known that public goods always get shortchanged.

Let me take a step back and remind everyone that economics is amoral. Contribution to economic growth is not necessarily good for mankind. Take HEP theory for example, by far the greatest contribution to GDP coming out of the field in recent decades has to be the series of fictions parading as popular science books written by Brian Greene, but this is hardly a good thing, particularly since science is supposed to be the pursuit of truth and must stake its claim of worth as such in the long run.

As for JohnB's idea of invading economics, I am all for it. The current bunch of leading economists are mostly free-market fundamentalists that pushed the US government to pursue unchecked growth in finance, directly leading to the Great Recession of 2008 that cost the world tens of trillions of dollars. The others like Paul Krugman think every problem can be solved by printing more money, even after the FED has already done so to the tune of $4t in 5 years. With competitions like these, people who can at least follow basic logic and common sense will be welcome replacements.

26. **Z**
October 7, 2013

Thomas Larsson: it depends on what you define as "practical", on what timescale, and how many connections to get to "practicality".

For example, the air show detectors that detect UHECRs certainly use the quark model in their analysis of decay products to accurately reconstruct events and estimate cosmic ray fluxes at various energies. Now, UHECRs, and indeed the galactic cosmic ray flux is the basis for carbon 14 dating, calibrated to proxies such as tree rings. Ergo, the quark model helps us date things more accurately (or detect close by supernovae, including those in the past). Is that practical enough?

However, the idea that science should or does have economic benefits is unsettling to me. Economics is a social construct and description of reality, and not really fundamental to happiness, strength or longevity of an intelligent species. One could define the economics of science differently in term of "human capital and knowledge", since it's an entirely arbitrary construction.

27. **martibal**
October 7, 2013

Sorry it is s bit out of topic but this is fascinating and – I hope – deserve one more message (and it provides one more argument than "GR for GPS" to explain that yes, fundamental physics is useful): how are UHECR used to calibrate C14 dating ?

28. **Z**
October 7, 2013

Not UHECRs specifically but most of the cosmic ray flux high in the atmosphere is protons or neutrons above 1 GeV, with about an order of magnitude lower number of mesons. Measuring the flux (via ballons, satellites and surface-based detectors) is important in estimating the formation rate of Carbon 14: 14N + neutron -> 14C + 1H that then feeds in dendrochronologic or historical calibration for dating. The cosmic ray detectors in their construction and data analysis, especially the higher energy ones, all have assume the standard model of particle physics via GEANT4 etc.

Incidentally, Tritium with a half life of ~12 years is also formed via cosmic rays like 14C, and can be used to date watery substances like wine.

29. **Chris W.**
October 7, 2013

Consider a paraphrase—or generalization—of the famous remark by Keynes [emphasis added]:

> The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back. I am sure that the power of vested interests is vastly exaggerated compared with the ***gradual encroachment of ideas***.

The trouble is, so many ideas are bad, and still have influence. At its best, science counteracts that. At its worst, it contributes to the problem. Sloppy declarations like "the more science the better" don't help.

30. **Thomas Larsson**
October 7, 2013

Z, I would define practicality like Faraday – I don't know what it is good for, but I'm sure that you will find a way to tax it. I don't see that your quite contrived example is generating much tax revenue.

Tim May gets it exactly right when he emphasizes the energy scales. EM and QM may be esoteric, but they are esoteric on terrestial scales, and therefore have terrestial applications. Quarks are relevant at much higher energies, and hence have few, or any, applications on earth. The energy frontier today is of course even much further way from terrestial conditions.

31. **martibal**
October 8, 2013

Z thanks for the explanation ! By knowledge of C14 dating was that of the exercises of second year at university.

32. **jd**
October 8, 2013

Z has connected the standard model and quark model to radiocarbon dating, as examples of the practicality of HEP. Wikipedia has a long entry on radiocarbon dating and a number of references. Just google on "radiocarbon dating". I only skimmed some of this material (there is a lot and I am lazy). The question is how many times do the words "quark model" and "standard model" (the HEP standard model) appear in this material?

33. **Christopher Hill**
October 8, 2013

Thanks for the review and the commentary. Our book is released today
and is being shipped at amazon.

On the economic front I must refer you to Warsh's book: "Knowledge
and the Wealth of Nations." It's pretty emphatic: science and technology drive economic growth. Solow's model could calibrate the post-War boom and it was driven (80%) by science, mainly the impact of the quantum theory– and that still holds today (we imply no insult to those economic "eggheads" and we express our gratitude to them for figuring it out, e.g., to be able to give a quantitative measure such as "80%"). We don't like to oversell the world-wide-web but it did come straight out of HEP, straight out of CERN at the same time the SSC was cancelled in the US. It required the paradigm of HEP to develop it. It is probably worth $10 Trillion, globally, per year.

Look, you either go into the lab to develop a product (Edison) or you go there to discover laws of nature (Fermi, Einstein). The latter approach produces greater and more lasting effects, *but all science driven economy happens through spin-offs* . HEP's impact is no different–via spin-offs that wouldn't happen any other way. Look at Roentgen, who was merely exploring the "rays" coming from gas discharge tubes, accidentally discovering X-rays. Within weeks he had turned this into the leading medical imaging technology….but that was a "spin-off" of the pure intellectual pursuit of knowledge. It's always a spin-off when it's driven by pure science.

We're on the cusp of some pretty big spin-offs from HEP. SCRF's (Superconducting RF cavities) may be the next big thing (DOE has spent over $600 million to develop them for electron linear colliders, but they promise a wide range of applications in the general economy, from cleaning smoke flues, to producing Tc-99 at hospitals for med imaging, etc. etc.). Furthermore, Accelerator Driven (sub-critical) Reactors, that are much safer than conventional nukes, can burn thorium (the world has over 100,000 years worth of thorium, vs 100 years of U235, and it produces only short-lived waste, no plutonium, no weapons grade materials etc.). These would be amongst the spin-offs of Project X. The development of Fermilab's Tevatron magnets led to the nickel-niobium wire that made MRI machines possible. I've always thought we should have superconducting power "highways," and the necessary tech is on the shelves at Fermilab ready to go, but it would require government initiatives to make it happen.

But yes, Project X is in some trouble. Our new Director of Fermilab, Nigel Lockyer, has to grapple with budget constraints that may simply be unmanageable if we try to move forward on Project X. As we tried to emphasize in our book, austerity can often be a bad policy. It's an easy sell to the folks back in one's Congressional district, but by cutting basic science budgets you deny economic growth to future generations. We are seeing that in the USA, and it looks like it will only get worse.

34. **Rez**
October 8, 2013

I hate the nickname “god particle”.

35. **Thomas Larsson**
October 8, 2013

Christoffer Hill, you don’t seem to have noticed the point about energy scales. QM applied to the eV (solid state, chemistry, optics) or keV (nuclear) scales has many applications, because such states of matter are readily manufactured on earth. The LHC is about the TeV scale, which is only relevant in supernovae or the Big Bang. That will never have applications on earth.

The www is a wonderful invention, even if I am not convinced that it will survive myself due to resource depletion. It could not have come into being without military research creating the Arpanet turned internet. So we should increase military funding to get more spin-offs…

36. **Peter Woit**
October 8, 2013

Christopher Hill,
Thanks for writing in with the comment.

Thomas Larsson,
You’re the one ignoring Hill’s point emphasizing spinoffs. In the case of future HEP facilities, I think everyone agrees that direct applications of any new physics are very, very much a long shot, but spinoff applications of new technology developed for Project X are quite plausible, with Hill giving specific examples.

37. **tt**
October 8, 2013

Thomas Larson,
another data point: lightning gets electrons up to 100 MeV
http://www.agu.org/pubs/crossref/pip/2012JA017535.shtml

38. **Thomas Larsson**
October 8, 2013

Peter. In my mind, there is a big difference between cases when the science itself has applications, and spin-off effects that don’t really have anything to with the science.

Some physics examples:

Röntgen: The *physics* of X-rays has medical applications.

80% of post-ww2 growth driven by science: Solid-state *physics* is used in electronics.

Nuclear: For better or worse, nuclear *physics* defeated Japan and gave us CO2-free electric energy.

Spin-offs:

HEP created the www (and Tim Berners-Lee is not a physicist).

Military science created the internet.

Space science create cool material like kevlar.

The core mission of these fields are different. HEP should uncover the building blocks of matter, the military should go to war, and space science should explore space. From my perspective, it is a big difference between spin-off effects and the science itself being useful.

39. **srp**
October 8, 2013

Historians of science and technology have long moved beyond the simple model of basic science–>applied science–>useful technology because there are many, many examples where the technology comes first. For example, steam engines, gasoline engines, etc. Just about everything Edison did. Many useful medical discoveries, including vaccination. The science often flows in later, systematizing and greatly enhancing the envelope of what a given technology can do.

On the flip side, you also have to give credit to basic science for inventions that wouldn’t have even been attempted without a general scientific knowledge that something might be possible, even if the specific breakthrough occurs in more of a trial-and-error mode than science–>solution mode. I think (though I could be wrong) that a lot of work on masers and lasers was like that–inspired in basic terms by quantum physics, but not simply calculable from first principles, with some experts believing it to be impossible.

40. **CFT**
October 8, 2013

@Peter,
There actually has been an increasing awareness in economic circles that advancing technology has been causing many jobs to vanish due to automation technology. While you can say the newer technologies are more ‘efficient’, they also employ fewer people and thus shift the wealth into fewer hands, require fewer highly skilled individuals to operate the increasingly idiot-proofed automation with each passing year, which makes for an ever shrinking work force with which to power the economy. There is also the economic reality that a highly skilled individual in the United States can have their job outsourced to a third world country for a pittance due to the wonders of ever increasing telecommunication technology. I think Lederman and Hill are pretty divorced from reality, and haven’t the slightest idea of how a real world economy works. Hopefully, neither of them will ‘help’ run the economy further into the ground.

41. **Geoff**
October 8, 2013

Here’s an interesting perspective on the growth of the internet which – according to Larry Smarr – was driven in no small part by computational physicists (numerical relativists) trying to gain supercomputer time.

http://vimeo.com/6982439

42. **Chris Austin**
October 11, 2013

@uair01:

Is HEP not too far removed from anything earthbound to ever be of practical use?

A conceivable discovery of experimental HEP that could have major technological implications would be the discovery of baryon number violation, together with some means of catalysing proton decay or baryon-baryon annihilation. The catalysis of proton decay by magnetic monopoles in certain grand unified models was considered by Rubakov and Callan 30 years ago – here's a recent study. The experimental discovery of such a process could improve the prospects for practical interstellar travel, by making it possible, in principle, for a rocket engine to convert a substantial fraction of the rest mass of ordinary matter to the kinetic energy of light decay products such as charged pions, and to utilize a significant fraction of that kinetic energy for propulsion. The LHC experiments are actively searching for both baryon number violation and magnetic monopoles, see e.g. here and here.

Various News

October 7, 2013
Categories: Uncategorized

- First a sad piece of news, via commenter Bob Jones. It seems that David Kazhdan, a well-known representation theorist, was hit by a truck Saturday morning while bicycling in Jerusalem. He's in the hospital, with his condition described as "very serious". I hope he manages to recover soon from this accident.
- On a much happier note, I spent Saturday at the Simons Foundation attending a day-long program celebrating the work of Pierre Deligne. More technical talks were in the morning, with Goncharov giving a talk on this material, and Illusie discussing the mathematical significance of several letters he had received from Deligne.

  Deligne's contributions to mathematics are immense, and go way beyond his many published works. Quite a few mathematicians have received letters like the ones Illusie discussed, laying out all sorts of new ideas. Some of these ended up getting worked out in detail by students and others, with Deligne's name not necessarily attached. To this day, Deligne continues to send highly helpful hand-written letters about mathematics to people, although I understand that these days they arrive not by snail mail, but as a scan sent by e-mail by a secretary. These letters make up a huge resource for mathematics, perhaps someday a way will be found to archive them and make them more widely available.

  In the afternoon, Brian Conrad and Ravi Vakil gave some very good more general talks, with one theme the Weil conjectures that Deligne was responsible for finishing the proof of. The day ended with reminiscences of Deligne from Illusie, Saint-Donat, and Dennis Sullivan (since Sullivan couldn't be there, his contribution was read off a cell-phone by Jim Simons).

  Contemplating Deligne's remarkable career is rather awe-inspiring. For more about him, a good place to start is this page at the Simons Foundation, which includes videos of an interview of Deligne by Robert MacPherson. See also this recent piece by Illusie, which makes the point that one of Deligne's achievements was to bring together two great but disparate currents in mathematics, the abstract algebraic geometers around Grothendieck, and the representation theorists working on what is now called the Langlands program.

- For yet more unification of mathematics and physics, last week the Mathematical Institute at Oxford hosted a conference on Number Theory and Physics, associated with talks celebrating the Institute's new building (see here and here). Witten's slides are available here and here, a blog posting by Bruce Bartlett is here.

  Via Jordan Ellenberg and Mathbabe, there's the news that Andrew Wiles took the occasion of the building opening ceremony to warn about the abuse of mathematics by the financial industry. By the way, the new building is described as housing 500 mathematicians and staff, which seems to me truly huge, quite a bit bigger than any other math institute I know of.

- A journalist at Science magazine got not just one, but 157 open access science journals to accept a bogus completely incompetent paper. One could take this as conclusive evidence for the problem with open access journals, except that he didn't try this also on conventional journals, and many believe that they too would publish something just as bad.
- Brian Leiter has a discussion here of some data here about the fraction of philosophy Ph.Ds that are able to get tenure-track jobs. I had always thought that academic philosophy Ph.D.s were likely to have even worse job prospects than theoretical physicists. If you believe these numbers at all though, your job prospects as a philospher are dramatically better than similar numbers for physics theory Ph.D.s. My guess is that, at least in the US, theoretical physics Ph.D.s have very roughly a 20% chance of finding a permanent academic position, while the data here shows 60% of similar philosophy Ph.D.s with permanent positions. If you're in a physics theory Ph.D. program, and at all interested in the philosophical side of the subject, perhaps you should immediately look into changing departments.
- As always, multiverse-mania shows no signs of slowing down. For the latest, see articles in the new issue of Nautilus here and here.
- Just a few short hours until the 2013 Physics Nobel announcements, with speculation raging about what the Nobel committee will do about the Higgs discovery.

Update: It's Englert and Higgs for the Nobel. I still think that, for a prize recognizing the theorists who figured out the Anderson-Higgs mechanism, there's a name missing in that list.

Comments

1. **Jeff M**
   October 7, 2013

   It's clear why the job market in math is so much better than physics, since we teach so many service courses, and don't have to rely on majors. I'm confused as to why it would be so much better in philosophy though, unless it's supply, are there many more physics Ph.D.s? At my university the math department has 31 full time faculty, if you eliminate stats and math ed, there are 13 pure math folks. The philosophy department has 7, and physics is 5.

2. **Kuas**
   October 7, 2013

   I hope the Nobel committee finally does the right thing and awards the physics Nobel to Gordy Kane for correctly predicting the Higgs mass. It's been a long time coming and Gordy isn't getting any younger.

3. **pa**
   October 7, 2013

   i think the odds of getting a job doing theoretical physics are way higher than 20%. sure, it's not easy but in my experience, people who did reasonable phds and werent burned out during their postdocs all get jobs within 4 – 5 years after their phd. i'd estimate the odds being closer to 60-70%, with it being 80 – 90% if you remove those who start postdocs feeling really burned out.

   the jobs might follow fads though.. topological phases or graphene or some fad in biophysics.. but people do get hired doing theoretical physics all the time in the US.

4. **kevin dowd**
   October 7, 2013

   I liked the Nautilus piece.

5. **Bobito**
   October 8, 2013

   "Open access" is a terrible name for "author pays". Let's stop letting unscrupulous publishers like the AMS getaway with this doublespeak.

6. **Andrea**
October 8, 2013

One piece of news you may have missed is that the Dannie Heineman Prize for Mathematical Physics has been awarded to Greg Moore this year for "eminent contributions to mathematical physics with a wide influence in many fields, ranging from string theory to supersymmetric gauge theory, conformal field theory, condensed matter physics and four-manifold theory."

Hugely deserved, by any objective measure.

7. **Eli Rabett**
October 8, 2013

At the risk of starting a riot, let Eli point out three things about the philosophy job market.

First, the level of support for philosophy doctoral students is much lower, not a lot of personal fellowships/research assistantships. Many in the softer arts go out an get jobs between masters and doctorates, etc.

Second, philosophy professors don't have or need large teams to support their lifestyle as is the case for high energy experimental groups

Third, in the US there are a lot fewer international students of philosophy who graduate and stay, competing for jobs.

All of these factors limit the number seeking positions in philosophy.

Of course, what physics and philosophy have in common, is given the small number of undergraduate majors, many places are trying to wipe out the departments leaving only service courses.

8. **DPB**
October 8, 2013

The BBC are reporting that the Nobel prize has been awarded to Peter Higgs and Francois Englert.

http://www.bbc.co.uk/news/science-environment-24436781

9. **chorasimilarity**
October 8, 2013

… and here is the press release.

10. **amused**
October 8, 2013

The "open access" journals scene seems more than a little sinister… I was almost sucked in recently after getting solicited by the "Journal of Physical Mathematics" – normally I would ignore, but in this case the message was semi-personalized and when I took a look at the editorial board there were respectable people on it, including someone I know a little bit. On the one hand sending a paper there makes no sense – why would anyone pay to publish a paper in a new journal with unestablished reputation rather than publish for free in an established and (supposedly) reputable journal? It could only make sense if someone wanted to boost his/her number of publications with low-quality papers that wouldn't be publishable in a reputable journal.
On the other hand, if the people on the editorial board were serious about trying to build up this journal, and one of them had been responsible for my "invitation", then maybe I should submit something there…
However, googling the publisher led to this wikipedia article, from which it is clear that this shouldn't be touched with a ten foot pole!

According to the wikipedia article, one of the delightful practices of this open access publisher is to list people on editorial boards without their consent, and then decline to remove them when they ask… Presumably that explains the respectable-looking editorial board in the case above.

Another thing, my "invitation" mentioned a 20% discount on the publication fee if I submitted the paper by a certain date..but there is no information either in the email or on the journal website about how much the publication fee actually is… Apparently it is only revealed after the paper is accepted.

11. **amused**
October 8, 2013

Another interesting article about the open access journal scene
here. It seems that besides the legitimate OA publishers there is now a substantial group of bogus publishers who are basically operating a scam. (This aspect doesn't seem to have been mentioned in the Science article.)

12. **Peter Woit**
October 8, 2013

pa,

Where do you get your numbers? If you look here
http://www.physics.utoronto.ca/~poppitz/Jobs94-08
you'll see there are around a dozen people getting HEP theory faculty jobs each year for the past few years. Do you really think this is 60-70% of the number of Ph.Ds in the subject each year?

13. **M. Mahin**
October 8, 2013

For a critique of the latest Nautilus article claiming multiverse evidence, see my post
"'Secret Codes of the Multiverse' Debunked" at
Secret Codes of the Multiverse Debunked

14. **Aquiles**
October 9, 2013

I do not think Anderson would have deserved it.

First, his work on symmetry breaking was not explicitly related to weak interactions, not even to particle physics.

Second, Dr. Anderson did all he could to avoid the Higgs particle to be discovered.

(and third, he already got a Nobel)

It would have been really absurd to have Anderson added to yesterday's list.

15. **Lun**
October 14, 2013

Regarding the maths/physics/philosophy intersection
What do you think of the recent sciam article
About the interpretation ambiguity of QFT??
http://www.scientificamerican.com/article.cfm?id=physicists-debate-whether-world-made-of-particles-fields-or-something-else

16. **Peter Woit**
October 14, 2013

Lun,

I read that but couldn't get much out of it. I guess I just don't see that there's an "ontology" problem with QFT, that "it does not tell us what a photon or quantum field really is". At the level the author seems concerned with, we have a precise definition of the QFT, and to me, that's what it really "is". I can't fathom what sort of answer to the question of what a "quantum field theory really is" the author would find satisfactory. Yes, obviously it's not a "field", it's not a "particle", and it's not any other word or combination of words in the dictionary. But I don't see what the problem is the author wants to solve. One could point specifically to details of the definition and identify things that one finds unsatisfactory about that, but I don't see that happening in this article.

Nobel for Englert and Higgs

October 8, 2013
Categories: Uncategorized

Congratulations to Francois Englert and Peter Higgs, awarded the 2013 physics Nobel prize this morning. Evidently the prize announcement was delayed because they were unable to reach Higgs by phone. Surely he wasn't unaware that today was a day he might be getting an early morning phone call…

The Higgs discovery last year was one of the great milestones of fundamental physics research and it would have been very odd for the ~~Nobel committee~~ Swedish Academy of Sciences to not recognize it with a prize this year. I do think though that the way they chose to do this is not ideal, for a couple reasons.

The first is that this was foremost an experimental achievement, but the experimentalists and their work remains unrecognized. The thousands of physicists and engineers of CERN, LHC and ATLAS have accomplished something amazing by working together, but this makes them somehow ineligible for the Nobel. As far as the Nobel goes they make the mistake of running their collaborations relatively democratically, without a "great man" (or "great woman") who could stand out and be awarded a prize.

Another issue with today's choice is that if you do want to emphasize a model of scientific research where advances come from a specific "great man" theorist, in this case they've left out the greatest one involved. The specific model tested at the LHC was not that of Englert and Higgs, but the one that Weinberg and Salam already got a prize for. The new prize is for the general mechanism, but this is something that was first understood by Philip Anderson a couple years before Englert and Higgs. For some details of the history, see here. The argument is often made that Anderson's model was not relativistic, but this is a phenomenon for which relativity is not relevant, something which Anderson understood.

The Nobel prize announcement comes with a detailed discussion of the history, which discusses extensively Anderson's work. It makes the argument that relativity was a crucial issue, and summarizes the situation with:

> This was a very important step forward showing that one could indeed have massive vector particles without having a massless mode, but it did now show how the same phenomenon would work in a relativistically invariant theory. Anderson concluded by saying "We conclude then, that the Goldstone zero-mass difficulty is not a serious one, because we can probably cancel it off against an equal Yang-Mills zero-mass problem."

Weirdly, this paragraphs contains a crucial typo. I assume they meant to write "it did not show" instead of "it did now show".

The authors refer to what is usually called the "Higgs mechanism" as the "BEH Mechanism", but it seems to me that if you want to insist on adding more names to the usual terminology, "Anderson-Higgs" would be better.

As far as the Nobel goes, Anderson already has one, given for other work, and maybe this is one reason he was left out this time (although getting multiple Nobel prizes is not unprecedented). Congratulations to him and the LHC experimentalists today, as well as to Englert and Higgs.

Update: Jon Butterworth has some similar comments at the Guardian, especially about the "lone genius" model for progress in science.

Update: For more from Anderson about his work on this topic, see interviews by Chandra, Coleman and Sondhi at the AIP oral history site here. One of the things I find most surprising about this history is that Brout was in close contact with Anderson during this period, but does not refer to Anderson's 1963 paper in the original Brout-Englert paper, or in later discussions of the history (see here). Here's Anderson's account:

> during this period I was in fairly close contact with Bob Brout. Later on, one of the co-inventors of the Higgs mechanism is Brout with Francois Englert. Bob spent several summers with us down at Bell and I know that I talked many of these things over with him. So he was definitely one of my sources for knowledge about particle physics, along with John Ward to a much, much lesser extent. Therefore, when I was recently helping edit one of the accounts of the recent Nobel Prize and noticed that they ascribed the idea, they call it Higgs, Brout, Englert, which I'd never heard, I realized that actually Brout and Englert had a fairly considerable influence on the whole development(must have gotten their ideas from me). So I had thought that it just fell into a black hole and Higgs reinvented it and everybody called it the Higgs mechanism because of that, but in fact, it is in the linear chain of what eventually led to t' Hooft and Veltman. So I was quite happy with that.

Update: John Preskill comments "The emphasis on finding a relativistic model may be misplaced, though. Anderson understood the mechanism well."

Update: There's an interesting story here about the final decision process and the delay in the announcement. Evidently the way things work is that the Nobel Committee (see here for members) proposes up to three candidates. The Royal Swedish Academy of Sciences meets at 9:30 am, debates the matter, makes a decision, with the announcement of the decision scheduled for 11:45 am. Earlier today though, something unusual happened, requiring delay by at least an hour (~~maybe two, one source says the announcement was at 13:45~~ it was at 12:45). Supposedly Higgs was not reachable by phone, but that seems unlikely to have been the cause of the delay since it was known in advance that this would be the case. The press story quotes the academy's permanent secretary as giving as reason "There were many people who had a lot to say".

Unfortunately the rule is that deliberations are kept secret for 50 years, so I'll be long gone before it is known what happened at this meeting today.

Update: It took two days, but the Swedes finally fixed their typo. Now Anderson "did not show how the same phenomenon would work in a relativistically invariant theory."

Update: C. R. Hagen, the "H" in "GHK", sent me the following commentary on the document about the prize put out by the Swedish Academy.

> It is difficult to take seriously the document put forth by the Swedish Academy which purports to explain the basis for their Nobel award.
>
> **"Scientific Background on the Nobel Prize in Physics 2013**
>
> **The BEH-Mechanism, Interactions with Short Range Forces and Scalar Particles"**
>
> http://www.nobelprize.org/nobel_prizes/physics/laureates/2013/advanced-physicsprize2013.pdf
>
> Plainly and simply stated its unnamed author(s) does not understand the mechanism which they are attempting to explain to the physics and world communities. The report in dealing with the Brout-Englert paper reads "The Goldstone theorem holds in the sense that that Nambu-Goldstone mode is there but it gets absorbed into the third component of a massive vector field."
>
> This assessment clearly demonstrates a total lack of understanding of the mass generation and Goldstone avoidance mechanisms associated with spontaneous broken symmetry. In fact as shown by Guralnik, Hagen, and Kibble (GHK) in 1964 the missing longitudinal mode of the vector meson comes from one of the two scalar particles in the model (the other being the so-called "God Particle" recently alleged to have been found at the LHC).
>
> There is no way to explain this incredible blunder by the Swedish Academy. In their desire to marginalize the GHK paper they have failed to understand its real contribution and have certainly failed to comprehend that the Coulomb gauge analysis of that work makes totally credible and understandable the route whereby the expected Goldstone boson is eliminated from the physical sector.

-C.R. Hagen

Comments

1. **Bernhard**
October 8, 2013

   Yes, I agree leaving Anderson out of the picture is unfair, but for political reasons – cutting the throat of the SSC, being unsympathetic to HEP experimentalists, being an anti-particle physics voice, already having a Nobel, etc – they decided to leave him out. I guess this is understandable, since awarding him the prize would generate a huge polemic, something Swedes are not very keen on…

   That said, I think their choice is not bad at all . Giving the price to the experiments could only be achieved giving it to the whole collaborations – without citing any specific names. It could be done of course, but giving it to the collaboration´s spokespersons would have been even a worse choice.

   Then, considering the discovery was sort of a (monumental, but still) confirmation.. it´s not crazy to award the theorists.

   If ATLAS, CMS or other LHC-experiment discover BSM physics (especially if unpredicted) then they will have to face how to solve these dilemmas, but for now I can see why they chose not to do it.

2. **Brown**
October 8, 2013

   Guralnik, Hagen and Kibble take notice, saying you didn't read prior work doesn't cut it as far as getting the Nobel is concerned.

   Anderson is debatable

   This reminds of the similar Gallo/Montagnier "controversy" regarding HIV.

   Again the Nobel went to the right people.

3. **Walt**
October 8, 2013

   I assume that one consideration is that Higgs and Englert are more likely to die suddenly in the next year than are the staffs of ATLAS and CMS.

4. **tulpoeid**
October 8, 2013

   About the phone call, since there is an official statement by him I guess he got it the day _before_, as it always happens. Personally I thought that the delay took place to tip experimentalists to return to their offices and leave the champagnes down, but luckily I was wrong 🙂

   About the rest, I wonder why we have to remind ourselves the 3 persons rule each year. Although I hope that "persons" will turn to "entities" for the first time really soon. (Think lhc, atlas, cms.)

5. **AcademicLurker**
October 8, 2013

   Given the number of potential candidates, should we be reading any significance into the fact that they chose to award the prize to only 2 people instead of 3?

6. **D**
October 8, 2013

   Robert Brout (1928 – 2011). R.I.P.

7. **Michael Jennings**
October 8, 2013

   Theoreticians don't usually get Nobels until there is experimental confirmation of their work. This year was the first year in which they could reasonably be given the award for this reason. However, as they are very old, there was something of a hurry if they were to get the award at all.

   On the other hand, the experimentalists involved in this are much younger. There is plenty of time to give them an award in some future year, possibly after more work can be done in figuring out who exactly to give it to.

   I don't think the choice to give the award to the theoreticians reflects any more than this.

8. **anon**
October 8, 2013

   Am I the only one who is slightly relieved that our hordes of LHC data analyzers won't be lording it around the corridors today with their 0.01% of a Nobel prize?

9. **Peter Woit**
October 8, 2013

   AcademicLurker,
I suppose you could read this a serious dis to Anderson, as in "unlike some other people we could mention, there was room for you, but too bad."

   anon,
Yes.

10. **vmarko**
October 8, 2013

I think that the Nobel was given for the theoretical prediction, rather than experimental confirmation/discovery. The same thing happened in 1957, when the prize was given to Yang and Lee for the prediction of the parity violation, rather than to madame Wu and others who have actually measured the effect.

If Atlas and CMS groups eventually measure some BSM effect that nobody has predicted so far, that will probably be considered a “new discovery”, rather than “confirmation of a theoretical prediction”. They could get a Nobel for that new thing, but not for the discovery of the Higgs particle.

At least that is my guess about the thinking of the Nobel committee. 🙂

Best, 🙂
Marko

11. **Roger**
October 8, 2013

Higgs was not sleeping next to the phone: [Dr. Higgs — the J. D. Salinger of physics — has already let it be known that he will not be available in any form on Tuesday.]

12. **Kavanna**
October 8, 2013

Good for Dr. Higgs.

Leaving out Anderson is debatable, although understandable. Kibble is the other one who might have been considered. The others often mentioned were not as early or as seminal.

13. **martibal**
October 8, 2013

Experimentalists at CERN already got the fundamental physics prize, which is almost 3 times the amount of money of the Nobel. Did any voice raise at this time to regret that none of the theoreticians of the Higgs mechanism were awarded ?
Also, without the scope of discovering the Higgs boson, what would have been the justification to build LHC ?
(these two questions are real questions, I am not being sarcastic).

14. **GoletaBeach**
October 8, 2013

Higgs gets out an about a whole lot more than Salinger. He has lots of close friends. Nice to see Ken Peach quoted, for example.

Nobel Prizes aren’t ever perfect… No-Bell to Hewish, Hahn not Meitner, etc. To err is human, and the prize committee is merely human.

A vast amount of experimental work over the last 40 years has filled the tables that parametrize the Standard Model. A certain segment of those parameters indicated a light Higgs and led to the persuasive arguments that caused the designs of CMS and ATLAS (good em calorimeters & lepton id).

The only prize that really matters is getting the physics right both empirically and theoretically.

BTW, nice discussion of Migdal & Polyakov in the Nobel Citation. Pretty good discussion of precision electroweak & LEPII as well. I felt a commitment to fairness in the Citation.

15. **fuzzy**
October 8, 2013

the role of gauge invariance for a qed was noted 3 years before the paper of weyl

http://ufn.ru/ufn10/ufn10_8/Russian/r108h.pdf

16. **Z**
October 8, 2013

Maybe Kibble et al will get their prize in next year’s three slots. Is it too soon to speculate? (Yes)

17. **Nathalie**
October 8, 2013

The Nobel Prize is not awarded by the Nobel Committee but by the Swedish Academy of Sciences.

18. **srp**
October 8, 2013

It used to be that the physics Nobels were for recent work and the economics prizes were lifetime achievement awards for old guys. Now the timelines are crossing.

19. **Peter Woit**
October 8, 2013

Nathalie,
Thanks. This made me aware that the Nobel Committee just proposes laureates, it is the Swedish Academy of Sciences that makes the final decision and award.

20. **Neil**
October 8, 2013

I will leave it to others to opine whether omitting Anderson on merits was justifiable. But if he was omitted because “he already has a prize”, then the whole process is a travesty.

21. **gs**
October 8, 2013

1. I starting reading this site for the soap opera back when the string community was in full denial about the experimental situation. My understanding of the science may be little better than a layman's.

2. Having said that:

In 1977 Anderson shared the prize with Mott and Van Vleck despite being from the following generation. Maybe Anderson was chosen at that time because John Slater had died in 1976.

Sounds like a bit of karmic balance (but afaic Anderson unquestionably deserves recognition at the Nobel level).

22. **Ray**
October 8, 2013

It shouldn't be forgotten that both Brout and Higgs are Europeans whereas Guralnik, Hagen and Kibble are Americans. I suspect that European bias played not too small a part in the decision. I have seen a lot of Europeans referring to the Higgs mechanism as the BEH mechanism long before this. (Kiritsis's "String Theory in a Nutshell" does this consistently.)

On a different note Sasha Polyakov and Sasha Migdal discovered the Higgs mechanism independently of the Western physicists around the same time. This was all the more impressive considering that they were both teenagers at that time. No one in Russia took their paper seriously and of course they probably had no effect on Western science. In Sasha Migdal's words, "For the whole two years 1964 to 1966 JETP refused to publish our work with Sasha Polyakov "Spontaneous Symmetry Breaking of Strong Interaction and Absence of Massless Particles" where we (correctly!) argued that vector mesons of the Yang-Mills Theory must acquire mass by absorbing zero mass Goldstone particles. We were stomped to the ground at every seminar we tried to present this work at. The most disturbing thing was that nobody would even argue with us on the subject — the mere mention of "Spontaneous Symmetry Breaking" caused healthy laughter, which ended the conversation. Independently this effect was discovered and published by Higgs and rightfully is called Higgs Phenomenon." (From http://alexandermigdal.com/prose/paradise1.shtml)

I find it very intriguing that nobody even mentions this fact whenever this controversy breaks out.

23. **Peter Woit**
October 8, 2013

To be honest, I've never understood the argument for Guralnik-Hagen-Kibble, or Migdal-Polyakov. The Migdal-Polyakov paper was submitted for publication November 30, 1965, more than a year after all the others, and very specifically was about the strong interactions. It's an interesting independent development, but it would be absurd to award it a prize for the Higgs.
Guralnik-Hagen-Kibble, despite a campaign to claim that they did everything independently and better, explicitly refer to and discuss the earlier Brout-Englert and Higgs work, denying themselves priority. I just took another look at their paper, and noticed something I've never seen before: it ends with the sentence "preliminary investigations indicate that superconductivity displays an analogous behavior". This is remarkably clueless, given Anderson's work, which they would have known about if they'd looked into the relation to superconductivity at all.

That said, European chauvinism may have had a small part to play in denying a piece of the award to Anderson, although his views on the SSC and his already having a prize are probably more important factors. I just reread the "scientific background" paper released today, and remarkably it seems to me that it could just about as easily be read as justifying an Anderson prize as not. Also remarkably, it still contains the crucial typo giving the opposite conclusion to the one intended about the relation of his work to the relativistic case. This is very odd, since I would have guessed that paragraph would have been among the ones read most closely by those using it to make or justify a decision.

24. **Visitor**
October 8, 2013

To me, awarding a Nobel Prize (or any other prize) to an organization is indistinguishable from awarding it to the *administrators* of that organization.

25. **Peter Woit**
October 8, 2013

Visitor,
Well, in the case of ATLAS and CMS, those running the organization are the scientists themselves. I suppose that if they had a less-democratic policy of appointing one scientist CEO, those two people would now have Nobel Prizes. In that case though, I think the difference between giving a prize to the collaboration and a prize to the CEO would be noticeable.

26. **Tom Weidig**
October 9, 2013

>> Guralnik, Hagen and Kibble are Americans.

As far as I know Kibble is British. He has been a professor at Imperial College for decades. When I did my Master's there and talked to him, he came across to me being English.

27. **A.**
October 9, 2013

Peter Woit,

The announcement was at 12:45 CET (I'm in CE and watched it).

There was a question posed immediately after the announcement about why no experimentalists were awarded the prize. If someone managed to grab the video and has a link to it, I'll translate the answer.

28. **Bernhard**
October 9, 2013

According to NBC, the next chapter is clear:

http://www.nbcnews.com/science/after-nobel-whats-next-particle-physics-supersymmetry-8C11360835

29. **Bernhard**
October 9, 2013

…. and here more on the internal dispute that caused the delay:

http://www.thelocal.se/50690/20131009/

30. **fuzzy**
October 9, 2013

i guess that nbc assumed the correctness of the following statements:

"Theorists believe that the SM most probably is but a low-energy approximation of a more complete theory. If this were not so, quantum mechanical corrections to the Higgs mass would drive mH towards the Planck scale – unless "unnatural" cancellations occur"

let's analyze them. the higgs mass is known. it is not even similar to the planck mass. then, *assuming* the above statements are correct, we conclude that the sm is only a low-energy approximation of a more complete theory.

not difficult to see the mistakes. the sm is a renormalizable theory (veltman and 't hooft). its parameters are known after we measure them. there are no quantum mechanical corrections that "would drive" anything inside the sm, as instead stated by the royal academy.

if one wants to discuss another theory, why not? e.g., the supersymmetric models do not predict its mass scale; it has baryon- and lepton-number violating effects that are not seen; it do not predict anything that is measured. not clear to me why we need to discuss it in the "outlook" of such an important document.

31. **El-Coco**
October 9, 2013

Peter, you've got to think positively. You could easily be alive and well in 50 years.

32. **Håkan Eriksson**
October 9, 2013

The citation for the prize this year was: "for the theoretical discovery of a mechanism that contributes to our understanding of the origin of mass of subatomic particles, and which recently was confirmed through the discovery of the predicted fundamental particle, by the ATLAS and CMS experiments at CERN's Large Hadron Collider"

I hope people realize that the last part naming ATLAS and CMS collectives is unprecedented. Such recognition of third parties have not been done before. So it's pretty unfair to say the experimentalists and their work remains unrecognized.

33. **Bernhard**
October 9, 2013

Håkan Eriksson,

I agree. The fact that ATLAS and CMS are two separate entities perhaps even played a role on the impossibility of awarding the experimentalists directly (like they were two other "persons) and the solution was to recognize them in the citation. I´m sure this was a headache for the academy. The third solution I guess would be to award CERN, as representative of both experiments, but this also not ideal, since CERN is a lab that represents other LHC experiments, not only ATLAS and CMS.

34. **Aquiles**
October 9, 2013

The BaBar and Belle experiments were also mentioned in the 2008 Nobel press release… (and by the way, even the then-future LHC is mentioned)

35. **Bernhard**
October 9, 2013

Aquiles,

Yes, but this carries much less weight than the Nobel citation.

36. **Peter Woit**
October 9, 2013

Bernhard/fuzzy,

I also noticed the rather odd appearance of SUSY and "naturalness" at the end of the scientifc document. No mention there though of the other big achievement of the LHC, the negative results about SUSY.

At least we can all be happy that this didn't make it into the prize citation, which I suppose could have been for the "discovery of the first of five predicted fundamental particles, by the ATLAS and CMS experiments at CERN's Large Hadron Collider."

37. **Columbia**
October 9, 2013

Anderson's ideas were a little too vague too award a Nobel prize too. Although I have no doubt he had all the major elements in place, and would have found it himself if he had bothered to flush out the material.

I'm a little surprised that Goldstone wasn't picked. In some sense he's the odd man out, now that the prizes for Nambu, Weinberg et al, and the Higgs selection is complete.

His work is arguably much more theoretically important and universal than the messy details of the Higgs mechanism perse. It's important to note that all three groups were specifically looking for a method around the Goldstone theorem and was the main motivations for those papers.

38. **Shantanu**
October 9, 2013

Peter: the announcement was at 12:45 (not 13:45)

39. **Moe**

October 9, 2013

Guralnik compares the papers here in this video. Whether you agree with all his points, I give him credit for comparing the three PRL papers. I have not seen other comparisons by the original 6 and find that he demonstrates his knowledge on the topic better than others – certainly "My Life as a Boson" speech.

http://www.youtube.com/watch?v=WLZ78gwWQI0

40. **Jim**
October 9, 2013

We live in an age in which scientists are a lot more media savvy than they ever were and will willingly rush to embrace opportunities to promote themselves and their work. I admire Peter Higgs' decision to shun the media (he's 84 years old, after all). I had the privilege to meet him in August 2011, when speculation of an imminent discovery at CERN was rife. He told me that, in the event of overwhelming attention from the media following an announcement from the Royal Swedish Academy, he would simply unplug his phone and refuse to answer his doorbell. I'm not the least surprised that the Academy couldn't get him on the phone yesterday. For those hungry for his reaction, however, he will be giving a press briefing at the University of Edinburgh at 11am (GMT) on Friday, 11 October. The briefing will be webcast from http://www.ed.ac.uk.

I have to say I'm a little concerned about the outbreak of rather nationalistic sentiments in the comments on Peter's blog post. Are some American high-energy physicists are still carrying the mental scars of the SSC experience, after all this time? As Tom Weidig has pointed out, Tom Kibble is British (I believe he was born in India). Also, although Robert Brout was working with Francois Englert in Belgium in 1964, he was born in New York in 1928. Had he lived (he sadly died in 2011, after a long illness) he would have surely shared the Nobel Prize with Englert and Higgs.

41. **Sebastian Thaler**
October 9, 2013

Meanwhile, John Horgan has put his own characteristically pessimistic spin on this news: http://blogs.scientificamerican.com/cross-check/2013/10/08/could-nobel-prize-for-god-particle-be-last-gasp-for-particle-physics/

42. **Kavanna**
October 9, 2013

After all, Peter, we could still be alive in 50 years, still wondering when supersymmetry will be found.

Nonetheless! After about 85 years, the Standard Model, beginning with the Dirac and Klein-Gordon equations and ending with the discovery of the Higgs boson, is now fully cooked.

43. **Peter Woit**
October 9, 2013

Thanks Jim,
The reaction of Higgs to the media attention is admirable. As for

"Are some American high-energy physicists are still carrying the mental scars of the SSC experience, after all this time?"

I fear the answer is definitely yes, and to some extent there's good reason, since the effect of this event on US experimental HEP was disastrous and still being felt.

44. **SUSY**
October 9, 2013

If LHC discovers SUSY particles, who gets awarded the Nobel?
If direct dark matter detectors discover dark matter, who gets awarded the Nobel?

45. **FNesti**
October 9, 2013

Peter, fuzzy
mentioning the current spectrum of naturalness-related BSM theories has the twofold aim of 1) praising the current mainstream theoretical ideas, which gives a reassuring impression of seriousness of the community; 2) using again these theories for the real closing: motivating the request for a future machine.

Evidently, we can not ask (as Lederman) for a change in attitude: i.e. that experiments must be made *to see, not to look for.*

I just came across a piece by Galileo, which just discovered the Milky-Way was made of stars (! and not of milk) _after_ building the telescope.
And he concludes: through this instrument, one can see clearly that all discussions, century-long worry of philosophers, dissipate with the certitude of valid experience, and we are freed from sterile debates.

… attraverso il cannocchiale si può vedere in modo così palmare che tutte le discussioni, per tanti secoli cruccio dei filosofi, si dissipano con la certezza della sensata esperienza, e noi siamo liberati da sterili dispute.

I'd say, evidently history has some recurrent patterns 🙂

46. **Neil**
October 9, 2013

If LHC discovers SUSY particles, who gets awarded the Nobel?
If direct dark matter detectors discover dark matter, who gets awarded the Nobel?

Well, dark matter should be easy because it was hypothesized on the basis of evidence. So Rubin alone since Zwicky is dead.

47. **Marco Masi**
October 10, 2013

"The thousands of physicists and engineers of CERN, LHC and ATLAS have accomplished something amazing by working together, but this makes them somehow ineligible for the Nobel. As far as the Nobel goes they make the mistake of running their collaborations relatively democratically, without a "great man" (or "great woman") who could stand out and be awarded a prize."

.
But the “great man” (or woman) would finally be only the group leader. And in what sense would these be “great”? The group leaders of these research teams are great team managers, great policy makers and fund raiser (and I don’t mean that to be a negative trait, such kind of people play an important role in big science), but they are not (or are seldom) scientific geniuses, and are even not necessarily those who contribute at a scientific level. Eventually the Nobel policy should change towards the idea of assigning the prize also to a group or an organization as a whole. But the idea to favor the corporate minded personality over all the other scientists, amounts to a form of subtle idolatry of the manager type figure in science with which I definitely disagree with.

48. **A.**
October 10, 2013

We don’t really have to wait 50 years to find out about the Nobel deliberations; seeing the presentation which was apparently made to physicists on the Nobel committee (we’ve just seen the slides in a separate presentation), makes it clear that Anderson’s role was downplayed because he didn’t prove the statements he made about the relativistic generalisation of his results, while Weinberg’s contribution was downplayed because of his disagreement with Englert on renormalisability (Englert being right in the end, of course.)

49. **Marty**
October 10, 2013

The idea of awarding a Nobel prize to large collaborations doesn’t seem quite right to me. I don’t mean to downplay the great work and very significant findings of the CMS and Atlas teams. Certainly the discoveries themselves are in a class of Nobel-worthy discoveries; their importance is not at issue. My disagreement lies more in what would be a significant departure from recognizing individual achievement, something I thought was the intent of Nobel prizes in science.

Thus far, whenever a Nobel was shared among multiple individuals, care was taken to ensure that each individual did Nobel-worthy work. If the current tradition were relaxed a bit so that, say, the prize could be awarded to up to five people, I’m confident each recipient would have done Nobel-worthy work. At what number of co-recipients does this idea of individual worthiness break down? I don’t know, but I feel sure it would break down with far fewer than the thousands of individuals comprising the CMS and Atlas collaborations.

Can anyone honestly believe that each person in the Atlas collaboration, for example, deserves a Nobel prize for their individual work? Each grad student who pulled cables, kept an eye on the instruments in the control room, and wrote code to generate test data for event analysis software? Members of the collaboration who have devoted essentially all their energy looking for hints of SUSY or extra dimensions? If the collaboration were to receive a Nobel, all members would be treated as equally worthy of the Nobel regardless of their role. This doesn’t seem right to me — by treating everyone as equally important, as anonymous cogs in the Atlas or CMS teams, it cheapens the efforts and achievements of those who worked very hard and played an outsized role in the discovery while unduly rewarding those who played no significant role at all.

I don’t think that awarding a Nobel to a spokesperson or other proxy for a collaboration is a good idea either, as others have pointed out. Perhaps we have reached a time when some of the most important discoveries will involve large collaborations with no single individuals playing such a large role that they deserve a Nobel prize. And that’s OK — not every huge discovery has to get a Nobel if doing so would depart from the intent of awarding individual achievement.

50. **Bernhard**
October 10, 2013

Marty,

I understand and sympathize with your concern, since if they were to distribute 6000 little Nobel medals, the seriousness and prestige of the Nobel would pretty much evaporate.

However, I think that is not the idea behind awarding an organization, no more than it was not the idea to distribute the 2012 Nobel Peace Prize among all EU citizens. The idea is to simply to acknowledge the achievement of the institution, which only exists when all its members are taken together and makes no sense otherwise. It is not like saying that each member of ATLAS or CMS would each win 1/6000 of the Nobel.

About what each people do in a collaboration, that´s tricky. Even people searching for SUSY, in many cases, contributed crucially to this discovery. All members of ATLAS and CMS have “detector duties”, without which the Higgs discovery would have been impossible. I don´t think the data analysis is the hardest part – it´s one part, an important part, but if all other duties are not taken just as seriously just forget about doing the histograms.

About spokespersons I am as you, fiercely opposed to the idea of awarding them, but indeed, I would have no problem with them going to Stockholm for the ceremony, for the sole reason that at some point a human being needs to pick up the prize in the name of the organization . It is (at least to me) a very, very different thing however, if the Nobel diploma writes down the name of the spokesperson or of the organization.

The cash is unimportant in this case – 8 million SEK could be used to finance a dinner among the collaboration members (perhaps not in Geneva, not sure would be enough there).

51. **Terry**
October 10, 2013

The Nobel “history” is really quite nasty and just wrong…this view is from some famous physics folks you would never think would complain. Frankly many lies in this “history” giving credit to BE and H papers – it even claims that EB and Higgs had the same potential. Show me the potential in the papers!

http://www.nobelprize.org/nobel_prizes/physics/laureates/2013/advanced-physicsprize2013.pdf

Downplays everyone but BEH – especially Anderson and GHK.
For GHK they are particularly careful to ensure there work is downplayed. (It is as if ULB wrote it)
*Puts GHK in “other contributions” and uses several techniques to downplay the contribution
*Positions GHK in this “history” after some Soviet paper which came out in 1967
*States how the Soviet paper was clearly independent (but does not say that for GHK, basically implies it was not independent)
*For GHK each sentence was structured to downplay their paper
*Gives dates to show GHK was after others
*States it was the same model as BEH
*GHK citations of BEH was noted (does not mention that H referenced BE)
*States GHK explains detail of Goldstone avoidance but conclusions were “essentially the same”
*Never returns to the GHK paper throughout the document

The academy must have really felt the need to downplay Anderson and GHK. This paper was carefully constructed to really hammer the others and manipulate thinking with lies and half-truths.

Would be great to know who the "primary author" was for this "revised history"

52. **papers**
October 10, 2013

The *Physical Review* is making the BE and H papers publicly (free) available for download. Only BE and H not GHK. Read them and look up the potentials and anything else you like.
http://prst-ab.aps.org/edannounce/2013-nobel-prize-in-physics

53. **young experimentalist**
October 11, 2013

sure this was before my time, but didn't rubia get the nobel prize for the UA1 discoveries? were times that much different back then, that there was a "great man"?

54. **Chris Oakley**
October 11, 2013

young expermentalist,

Yes indeed, but as the politicians are less willing now than then to fund HEP research, any CERN director needs to be even greater now. The problem with the whole area is that measuring progress and achievement is much harder than, say, most industrial or medical research. In the first case it can be measured by the company making more money and in the second by people being more healthy. In HEP the whole world relies on a handful of experts to inform them, who, not wanting to put themselves on the line, then further delegate the task to the Nobel committee. I would not like to be one of them.

55. **Bernhard**
October 11, 2013

young expermentalist,

I believe that one of the things that have changed is how close the alleged leader is close to the physics analyses. In the case of Rubbia or even Lederman in 1988, they were very close and active to the scientific results. I believe however, that even in their case, it would have made sense to give the prize to the collaborations, but indeed their leadership was very important. I, for example, don't swallow at all what Lederman wrote about his own deserved credit in the neutrino experiment in "The God Particle", but if I were to, than he deserved the prize.

The closest thing we have today regarding the roles Rubbia and Lederman had are , to some extent, the group conveners, but they change with the wind and would be very strange to just award one of them. Furthermore, the most difficult thing in the case of the LHC experiments, IMO, was the detector design – after that, the methods that were used to discover the Higgs were known. It did not need any epiphany from nobody in particular to see it.

All in all I think the Nobel policy of giving group leaders too much credit has been damaging. Take for example the pion discovery. It is undisputed that Cesare Lattes was the the main researcher (and by the way he first author of the Nature article) regarding the pion discovery, but it was actually Cecil Powell, the group leader, who got it. This was not a good idea then and it should not be a good idea today either, specially when the role of the spokesperson has detached itself even further from the analyses.

56. **Terry**
October 11, 2013

They three (BE, H, GHK) are all available from the PRL milestones paper site.

http://prl.aps.org/50years/milestones#1964

57. **piscator**
October 11, 2013

Young experimentalist:

Read 'Nobel Dreams' by Gary Taubes. Beg, borrow or steal a copy (it is out of print now). But if you are a HEP experimenter, and you want to read about a great experimental discovery together with lots of juicy stories about how those collaborations worked….

58. **greatmen**
October 11, 2013

Carlo Rubbia was very much a (or the) driving force to push (persuade?) CERN to implement stochastic cooling and convert CERN's existing SPS Super Proton Synchrotron, which was a fixed target machine, into a p-pbar collider, which would have the energy reach to produce the W and Z bosons. And Rubbia headed the UA1 collaboration to build the required detector. (Note that there was also a UA2 collaboration.) So Rubbia was much more than simply a group convener. Lederman won jointly with Steinberger and Schwartz, and note that the experiment was at Brookhaven Lab in 1962 (not Fermilab), a fixed target experiment using the AGS synchrotron. The collaboration size was much smaller, and the leaders formulated the basic idea of the expt, designed the apparatus, analyzed data, etc. ~ much more than simply being a group convener. In fact Sam Ting's collaboration (the J of J/psi) was about the same size, and indeed Ting won (joint with Richter) ~ not his entire staff. (The collaboration size was maybe six people?)

Nevertheless, the notion of awarding the Nobel Prize only to the team leader is controversial and questionable and not a good idea.

59. **Marco Masi**
October 11, 2013

Marty, there is nor reason (apart from tradition) why the Nobel prize must necessarily recognize only the individual achievement, especially when everyone knows that it wasn't. Médecins Sans Frontières (MSF – "Doctors Without Borders") received the Nobel peace prize as an organization in itself, but no one began to believe that every individual working at MSF is a Nobel laureate. I think it is a false problem, and don't see why that can not be applied in physics too.

60. **orgs**
October 11, 2013

To date, only the Peace Prize has been awarded to organizations. Also, the limitation to not more than three individuals is an arbitrary rule, made by some committee in the early days of the prizes. Actually, Alfred Nobel's will had in mind only one person per prize. So be grateful that the current limit is at least three, not one.

61. **Tom**

October 11, 2013

An essay by Sean Carroll a couple days ago, criticising the Nobel practice of restricting prizes to maximum of 3 people:

http://www.nytimes.com/2013/10/09/opinion/no-physicist-is-an-island.html
"No Physicist Is an Island"

62. **Peter Woit**
October 11, 2013

Looking at Nobel's will on the Nobel website, it states that the prizes are for

"those who, during the preceding year, shall have conferred the greatest benefit on mankind."

then going on to specify that the benefit to mankind he had in mind was a discovery in physics, chemistry or medicine, or a work of literature, or peace-making. From this, the ATLAS/CMS collaborations would fit precisely the terms of the physics prize, since they had the most important discovery in physics in 2012. Englert and Higgs don't fit at all, since their work was not last year, but 48 years ago, something completely different than what Nobel had in mind.

It is in the later specifications that "person" in the singular is used. This was violated long ago by the decision to allow prizes for three people in the scientific fields, and organizations in the peace prize. I don't see how allowing a prize for a collaboration of scientists is any more a violation of Nobel's intent than the current rules. So, arguments that it is Nobel's will that is the problem here just don't hold water.

The problem with not allowing prizes to collaborations is that it rules out many parts of experimental science, only allowing prizes when experimental collaborations are organized by having an identifiable "leader" (or, up to three of them). Having a system where large parts of the experimental side of science become ineligble for the prize I doubt is what Nobel intended. There's also no reason to believe that he felt that experiments needed to have a "leader" who would be the one to get prize recognition. On the whole, arguments against giving prizes to collaborations seem to be coming from theorists, who, since they work alone or in small collaborations, have obvious reasons to prefer a system where their form of organization is the one to be rewarded.

63. **Gog the Mild**
October 11, 2013

Apologies if this has already been mentioned, but apparently Professor Higgs learnt about his award from a neighbour in the street. BBC – http://www.bbc.co.uk/news/uk-scotland-24493400 . This also has some quotes regarding his take on allocating credit for the theory.

64. **will**
October 11, 2013

There are many ways one can read Nobel's will. That's how lawyers make their money after all … Thomas Edison probably conferred more benefit on mankind than any winner of the physics prize, yet Edison never won (and quite likely was never nominated). The invention of the incandescent lamp (aka "light bulb"), the phonograph, electric utility company (was it in Menlo Park? I think his lab was there), many other things. Steve Jobs also conferred great benefit on mankind and also deserved to win, and most likely Jobs was also never nominated.

Fritz Haber did win, for chemistry, for his invention of the Haber process (today the Bosch-Haber process) to fix nitrogen from the air to make ammonia. It is an important industrial process for the chemical (or fertilizer) industry. It also enabled Germany to fight WW1 (because the British had blockaded supplies of saltpeter from Chile). Was Haber's work a benefit to mankind? I believe Haber won a Kaiser Wilhelm Medal for his services to the German war effort (and he was shocked when he was later dismissed from his professorship because he was a Jew). But the Bosch-Haber process is to this day an important industrial process.

I think no one would argue that awarding the Nobel Prizes (in the sciences) to collaborations violates the spirit of Nobel's will. Also the restriction to three individuals.

Basically, it boils down to the concept of how science was practiced in Nobel's day. The rules were formulated within the paradigm of how scientists practised research in those days. Who would have dreamed in those days that physics experiments would take years to plan and build and cost billions and require teams of thousands? And who knows how science will be practiced a hundred years from now? I can easily imagine that significant contributions will be made by robots or suchlike, and there will be debates as to whether the prizes should be awarded to robots. A robot might even make the critical discovery, in some team. Who can say?

65. **Marty**
October 11, 2013

Bernhard and Marco,

Thank you for your perspectives. Both of you seem to take the Nobel Peace prize as a precedent for awarding a prize to group. Maybe that is a reasonable model for rewarding collaborations. It would certainly be desirable and appropriate to somehow recognize the significant achievement of the detector teams in finding the Higgs, beyond an "honorable mention" in a paper by the committee that discusses this year's award.

However, it still seems that awarding the physics prize to Atlas+CMS would require a significant departure in thinking about what a Nobel in science signifies. I interpret Peter's helpful quote from Nobel's will, *"those who, during the preceding year, shall have conferred the greatest benefit on mankind"* (ignoring the "preceding year" part), as an intent to reward the person (now "no more than three people") who made the discovery, i.e., a collection of identifiable individuals. But this interpretation doesn't seem to match my understanding of what Bernhard was thinking, which was that one should think of the collaborations as abstract institutions that made the key discovery rather than a set of individuals each of whom individually deserves 1/6000 of the prize.

The peace prize is qualitatively different from the science prizes, which I think should be assessed before using it as a model for rewarding a scientific collaboration. For example, peace itself is an idea rather than an objective condition (a police state may look outwardly "peaceful" even though its citizens may live in constant fear), and the best way to achieve it is very subjective (e.g., "peace through strength" versus peace through respect and accommodation versus …) and hence intrinsically political. Scientific discoveries, however, must be very concrete to earn a Nobel. I would have thought that rewarding an organization for promoting peace also makes sense when that organization is a group of highly motivated individuals working toward a common goal (and often at significant personal risk), but it's hard to see how the award to the EU is consistent with that viewpoint, given that (in my understanding) many EU citizens strongly dislike the institution…

One way to continuing the tradition of awarding Nobel prizes in science to individuals, while also accommodating the growing importance of large collaborations in bringing about significant scientific discoveries, is to create a new prize (not necessarily awarded annually) for that purpose. Such a prize could be for all of the sciences where Nobels are currently awarded so that, say, physics could get it one year, medicine another year, etc. Not that anyone is likely to care that I think this could be a good idea…

66. **Marty**
October 11, 2013

Oops, a bit of vagueness slipped into my last paragraph. The “new prize” I was suggesting would be specifically for rewarding scientific collaborations.

67. **Shantanu**
October 11, 2013

Btw, first time I checked publication list of Higgs on inspire and didn’t know that in 60’s and 50’s he also worked on GR and canonical formulation (or something like what people working on LQG do nowadays). Didn’t see this mentioned in any news articles.

68. **Uninvolved Swede**
October 12, 2013

According to Swedish media the delay was caused by internal debate on whether to also reward CERN, which they decided not to since organizations are not eligible according to current Nobel Foundation by-law. Though, I suspect they will change that relatively soon. The speculation in the comments here about European bias is rather silly, if there is any at all (which I highly doubt) I suspect there would be just as much anti-European sentiment as anti-American here in Sweden. (Despite efforts by the EU most people here still think of themselves as Swedes or Scandinavians rather than Europeans.)

69. **Peter Woit**
October 12, 2013

Uninvolved Swede,

Thanks. The conjecture of a possible award to CERN as the third winner makes a lot of sense. It would explain why the detailed history has so much about CERN and the LHC experiments, why a third place was left open when it could have been given to Anderson, why there was lengthy discussion that morning making a delayed announcement necessary, and why there is explicit mention of the experiments in the citation.

70. **Peter**
October 15, 2013

A quote in the APS News (Oct. 2013) article about the demise of SSC 20 years ago.

– Among the most vocal detractors was Nobel Laureate Philip Anderson, a condensed matter physicists who bemoaned “the almost irrelevance of the results of particle physics not only to real life but to the rest of science.”

71. **aps news**
October 16, 2013

To put a more positive spin on things, read the poem in APS News bottom p4
http://www.aps.org/publications/apsnews/201310/upload/October-2013.pdf

72. **Bernhard**
October 17, 2013

Joao Magueijos (Imperial College) has a rather aggressive anti-Nobel opinion.

http://www.publico.pt/ciencia/noticia/o-nobel-da-injustica-1609163

The article is in Portuguese, but worth reading it with google translate (not too bad):

I translated two paragraphs here:

“Nobody likes to be accused of being a bad loser, but sometimes true courage lies in having the courage to criticize when you lost. The Nobel Prize has long since lost touch with reality, specifically in physics. Science is an increasingly collaborative and collective effort, even in the theoretical areas. Where the contribution of each one begins and ends is becoming increasingly less obvious.”

And:

“One conclusion we can draw from all this: the Nobel Prize, with its anachronistic rules, is unable to represent fairly the nuances and historical details of any scientific discovery. What happened to my colleague (Kibble) is a tremendous injustice, and worse it will be when the prize is awarded to the scientists at CERN , where the Higgs particle was detected. Thousands of people from over 100 countries contribute, but then only the leaders will receive recognition, as it is tradition in these cases. As a rule, these leaders are more politicians than scientists. There are even those who say they are the worst representatives of these huge groups, invoking the quote “shit rises to the top”. If the Nobel Foundation did a raffle between the doctoral students, there would be more integrity.”

73. **shinosuke**
October 20, 2013

With all due respect to Phil Anderson, to whom my discipline, Condensed Matter Physics, owns a great deal (and I certainly believe that he deserves more than one Swedish prize), I find a strange kind of “symmetry” with Anderson failing to get the 2013 Nobel together with Higgs and Brout and the case of Jaques Friedel in 1977.

Indeed, that year Anderson got the Nobel together with van Vleck and Mott “for their fundamental theoretical investigations of the electronic structure of magnetic and disordered systems”. In Anderson’s case, besides his famous work on localization in disordered semiconductors, the mention is also for another of his important contributions, namely the “Anderson model”. The latter describes the behavior of a magnetic impurity in a metallic host. The model was mathematically described and solved (within a mean-field approximation) by Anderson in Phys. Rev. 124, 41–53 (1961).

It might be not known to many in the HEP community, but the mechanism that the model captures had been described much earlier (with words, but in detail) by Jaques Friedel in Can. J. Phys 34, p. 1190 (1956). It is hard to believe that Anderson was not aware of Friedel’s work since, at the time, the number of Physicists working on Solid State problems was rather small and they met regularly. But to his own merit, Anderson was the first to formulate these ideas mathematically, thus lying the foundations of fields such like the so-called Kondo problem, Heavy fermion materials, and other strongly correlated electron phenomena. Of course, it would have been nice to have seen the Nobel committee change the rules back in 1977 and expand the number of winners from three to four or even more… but where is the limit?

Latest From Langlands

October 11, 2013
Categories: Langlands

Robert Langlands will be speaking at Yale in a couple weeks at a day-long Mostowfest of lectures in honor of Dan Mostow. His title is "The search for a mathematically satisfying geometric theory of automorphic forms" and he has already posted some notes for the lecture. A much longer set of reflections on the same topic was finished late last year and published in a volume in memory of Jonathan Rogawski. It's available at the IAS Langlands site as A prologue to functoriality and reciprocity: Part 1. There's no part 2 yet, but an earlier version of the full document is here, based on some lectures by Langlands in 2011 at the Institute, one of which is available on video here.

In all of these, Langlands is struggling with various ideas about "geometric Langlands", meaning analogs of the Langlands program in the case of Riemann surfaces instead of number fields or function fields (functions on a curve over a finite field). One approach to this question, starting with Beilinson and Drinfeld around 25 years ago, has been extremely active and I've often written about this here. For the latest from this point of view, you can consult Dennis Gaitsgory's web-site here. Langlands doesn't find this often very abstract point of view to his taste, so has been trying various more concrete things. In particular, he's quite interested in the connection to quantum field theory. I don't think he's actually found a satisfying line of attack on this problem, but it's fascinating to see what he's thinking about. There are all sorts of very deep questions in play here about the relationship of quantum field theory, representation theory, number theory and algebraic geometry. Langlands himself describes what he has as just "still provisional reflections on the geometric theory", and says about his upcoming lecture:

> The best I can offer in the way of a geometric theory with which I would be pleased is a sketch of the principal difficulties to be overcome. There are many. The importance for me is the very strong analytic flavour of the theory I hope to construct or see constructed.

Comments

1. **lazy eye**
   October 12, 2013

   Do we know what exactly Gaitsgory et al. have achieved in the direction of geometric langlands ?

   On an other topic : I hope we will soon get a "latests from ABC" post, I think i'm not only speaking for myself when I'm saying that its crazy that we have no information whatsoever on how the reading of the proof is going !

   Love your blog !

2. **Peter Woit**
   October 12, 2013

   lazy eye,

   Glad you like the blog!

   The latest from Gaitsgory et al. on geometric Langlands that I know about is this
   http://arxiv.org/abs/1302.2506
   which outlines a proof of the "categorical" version of geometric Langlands, for the case of GL(2). There is a school on the topic planned for Jerusalem in March
   http://www.math.harvard.edu/~gaitsgde/School14/
   This has become a large subject though, and there may very well be more interesting things going on using these ideas than that focused on the conjectured equivalence of categories.

   Unfortunately, from the QFT point of view, I don't think there's much activity.

   On Mochizuki, I think the situation remains that most experts have given up as hopeless the task of making their way through the details of his papers. He has a relatively new "panoramic overview of Inter-universal Teichmuller Theory"

   http://www.kurims.kyoto-u.ac.jp/~motizuki/Panoramic%20Overview%20of%20Inter-universal%20Teichmuller%20Theory.pdf

   but I think this doesn't provide the kind of detailed outline of a proof that experts are looking for.

   One interesting question I don't know the answer to concerns the status of refereeing of Mochizuki's papers. I've heard rumors some of them have been submitted for publication, haven't heard what happened. It does appear that he has been going through them with the help of Go Yamashita and others, checking things and making periodic revisions, see
   http://www.kurims.kyoto-u.ac.jp/~motizuki/news-english.html
   It's undoubtedly a problem to find referees who feel competent and willing to go through the papers and understand exactly what is going on well enough to judge whether the details were correct.

Last Links For a While

October 20, 2013
Categories: Uncategorized

In a few days I'm heading to East Africa for a couple-week long trip, planning to be in Uganda on November 3 for the (short) total solar eclipse that day. This will be followed by a few days in London, then back here with regular programming resuming around November 11. While away I'll shut off the commenting system, since I'm hoping to not have internet access during most of the trip.

Here's some short items that might be of interest:

- For the latest from CERN about SUSY, see this overview from ATLAS. The bottom line is quite simple: zilch, in every channel examined. Limits on a gluino mass are about 1.2 TeV, and there seems little prospect of much change until 2015, when results at 13 TeV start to come in. A naive extrapolation says that ultimately the LHC should be able to set limits on gluino masses of up to 2 TeV. Pre-LHC, the limits were about 300 GeV (from the Tevatron). I don't know anyone who is optimistic that it will turn out that the 62.5% energy jump in 2015 will find something when the 400% energy jump didn't (and the theoretical arguments for SUSY implied that it should have already been seen at the Tevatron).
- Steven Weinberg has an article about the current state of cosmology and particle physics, entitled Physics: What We Do and Don't Know. About string theory he has this to say:

  > String theory is attractive because it incorporates gravitation, it contains no infinities, and its structure is tightly constrained by conditions of mathematical consistency, so apparently there is just one string theory. Unfortunately, although we do not yet know the exact underlying equations of string theory, there are reasons to believe that whatever these equations are, they have a vast number of solutions. I have been a fan of string theory, but it is disappointing that no one so far has succeeded in finding a solution that corresponds to the world we observe.

  The main reason for disappointment about string theory is not that a solution corresponding to the SM hasn't been found, but that the theory predicts nothing at all. All indications are that the dead end that string theory has hit is not that (if one could actually figure out what the theory is…) of no SM solution, but that of so many solutions that you can get anything you want. Unfortunately Weinberg seems to be of the "if a fundamental theory predicts nothing, that's too bad, but maybe how the world works" camp, rather than the more standard "if a fundamental theory predicts nothing, it's a bad fundamental theory" camp. He goes on to argue that we may have to just give up on fundamental physics and be content with this theory that predicts nothing:

  > Such crude anthropic explanations are not what we have hoped for in physics, but they may have to content us. Physical science has historically progressed not only by finding precise explanations of natural phenomena, but also by discovering what sorts of things can be precisely explained. These may be fewer than we had thought.

  Back in 1977, in the wake of the great advances of the Standard Model, Weinberg famously made the statement that:

  > The more the universe seems comprehensible, the more it also seems pointless.

  Presumably the universe is still pointless, but now the argument seems to be that it's also incomprehensible.

- Unlike Weinberg, Frank Wilczek hasn't been a fan of string theory. From a recent interview:

  > 3. Is String Theory a dead end? Is there news coming, regarding scientific advances, or experimental confirmations?
  >
  > Many very smart people continue to work on string theory, and I expect that they'll continue to do interesting work, in mathematics if nothing else. Whether they'd be more productive doing something else, is another question. It is unfortunate that in the early days people got carried away, and promised much more than the theory
  > could reasonably be expected to deliver.

  and about anthropics:

  > It is the scope of anthropic reasoning that's debatable. I hope we can avoid appealing to it very much in fundamental physics, but time will tell.

- According to the Stony Brook newspaper though, based on information from Michael Douglas, all is well with string theory:

  > String theory has done quite well so far in explaining all of the forces of the universe. The theory has matured, and so have the mathematical equations it has produced. An equation describing the universe is considered successful if it is symmetrical. What that means is if the equation is taken apart and its components rearranged, it should still produce the same conclusion. If the rearrangement of an equation does not yield the same result, it is deemed unstable and not a good descriptor of the universe or its forces.
  >
  > The equations that have stood up to the test of symmetry have predicted the existence of particles that help bridge the gap between general relativity and quantum field theory. For example, string theory predicts a particle called the graviton, thought to be a closed loop string that is responsible for the gravitational force…
  >
  > Gravity is weak.
  >
  > String theory not only predicts the particle that constitutes gravity, it also helps describe why it is so weak…
  >
  > Currently, the ability to test the predictions from string theory is very limited and some have said that this roadblock is impossible to overcome.
  >
  > Douglas thinks otherwise.
  >
  > The next phase of this theory will likely take a lot of hard work and fresh ideas. String theory has made enormous strides in the relatively short amount of time that it has been around, and it is thought by many to be the most promising of the so-called "theories of everything."
  >
  > In a few more years, who knows what exciting advances could be in store?

  The article doesn't mention Douglas's decision to stop working on string theory and go to work for a hedge fund.

- The Financial Times has its own take on the current state of fundamental physics research, with an article on The new physics.
- Also at the Financial Times is a good survey of Physicists and the financial markets, describing various current activities of physicists now working in the financial industry.

- Harvard University Press has just released a new book by Steve Nadis and S.-T. Yau, a history of the Harvard math department entitled A History in Sum: 150 Years of Mathematics at Harvard. It concentrates on the period 1825-1975, and I enjoyed it quite a bit. It ends right about the time I arrived there as a student, so covered history that I never had known much about.

  Harvard's role as a mathematics research institution began with Benjamin Peirce, who taught there from 1831-1880. It only started to become a world-class institution around 1900, with young faculty who had gone to Germany for their training. The book covers a fairly long list of great 20th-century Harvard mathematicians (including George David Birkhoff, Morse, Whitney, MacLane, Ahlfors, Gleason, Mackey, Zariski, Brauer and Bott), and makes a serious attempt to explain some of the mathematical ideas they developed. As a result, a large part of the book is not just history, but actual exposition at a popular level of a wide range of mathematics, together with quotes from many other prominent mathematicians about the significance of the ideas.

  If you're interested at all in the history of mathematics, this book is well-worth finding a copy of.

- David Appell has an article about the SSC, the major disaster for US HEP research. Next year should see a book on the topic by Michael Riordan, *Tunnel Visions: The Rise and Fall of the Superconducting Super Collider*.
- The Simons Foundation Quanta magazine continues to put out many high quality stories about science, with one of the latest an article by Natalie Wolchover about experiments searching for neutrinoless double beta decay, which would indicate a Majorana neutrino mass term.
- The SETI institute has a series of SETI Talks, available on YouTube, with the latest featuring Joe Polchinski on Black Holes and Firewalls.
- The Boston area Joint Math Colloquium this week with have Edward Frenkel speaking on The Langlands Program and Quantum Physics. Afterwards you can go up to Harvard Square and get him to sign a copy of his new book.

Comments

1. **Visitor**
   October 20, 2013

   "The main reason for disappointment about string theory is not that a solution corresponding to the SM hasn't been found, but that the theory predicts nothing at all. "

   His disappointment, or yours?

2. **Peter Woit**
   October 20, 2013

   Visitor,
   I don't think it's just my opinion that string is disappointing because it can't predict anything. Surely Weinberg would also agree this is a cause for disappointment. I'd be curious what he thinks though of the possibility of a string theory landscape that has a SM solution, but so many others that one can't extract a prediction about anything out of it. Would the existence of this SM solution remove the disappointment, and he would then be happy with a non-predictive theory? I kind of doubt it.

   This is the problem with the point of view he's selling at the end. If you have a testable theory that predicts that certain parameters (e.g. the CC) are enviromental, that's one thing. But an untestable theory that predicts nothing is a different story, and can't be saved by trying to argue that all the things one might hope to use as tests are just environmental. Right now string theory is in the second category, with no prospects for entering the first.

3. **johnnythelowery**
   October 20, 2013

   Going to Africa?: There is a great series on Netflix which aired in the UK on TV called 'Long Way Down' where two guys (one, an actor you might know—Star War's Ob1kenobe) ride motorcyles from top of Scotland to Cape Town. It presents a videoblog view of the various countries of Africa and the topology of the various countries. The series is different than anything before about Africa. Eye opening.

4. **johnnythelowery**
   October 20, 2013

   You say String theory predicts nothing at all, which is a fascinating observance. but see here in the same posting; Douglas claim's that it predicts the Graviton? So, there seems a conflict with your preposition.

5. **Peter Woit**
   October 20, 2013

   johnnythelowery,

   I'll just quote Lisa Randall about that prediction: "yes, string theory predicts gravity, ten-dimensional gravity…"

6. **Yatima**
   October 21, 2013

   I feared that "Physicists and the Financial Market" would be enragingly content-free with "physics!!" mysticism, Physics Envy and the obligatory mention of "black swans" but it's actually a nice introduction.

   Oxford university's J Doyne Farmer builds "Crisis": *The project is an attempt to do in economics what is common in physics, to build an agent-based model not from a mathematical formula or theory but from the ground up, by simulating the interactions of all the component parts. "The failure to embrace things like simulation has inhibited progress in economics," Farmer says. "Physics, meteorology, chemistry, biology, even other branches of social science have got major impetus because of serious use of computers as a simulation tool."*

   It's about time, but it has as much to do with physics as does simulation of large-scale neural networks: which is to say nothing at all, except that papers may exhibit differential equations. However, I fear that the model will be "value-neutral" and not factor in things that traditionally make the worst rise to the top. A EU-financed project that models "political entrepreneurs" who wreak havoc while selling crony-capitalist enrichment schemes in the nice flashy box of "Keynesianism"? A *likely* outcome.

7. **morris**
   October 21, 2013

   Peter Woit,

As an aspiring physicist, born and currently living in Nairobi, Kenya am excited to hear of your planned trip to East Africa. If you do happen to pass through Nairobi, a group of us, currently studying at Nairobi University would be happy to meet and show you around. We would be honoured to meet a practising physicist at the frontier of knowledge. Your blog has been an inspiration to most of us young aspiring theoretical physicists. Karibu East Africa.

8. **Bee**
October 21, 2013

It's not that string theory doesn't predict anything. At the very least it predicts string excitations. It's just that the things that it does reliably predict aren't presently possibly to test experimentally. Alas, the same could be said about pretty much all other approaches to quantum gravity. The reason people are 'disappointed' is not that it's a bad theory, but that it's been oversold. The pendulum will swing again once people notice that LQG also doesn't 'predict anything'… And then they'll all end up doing qg phenomenology :p

Have a safe trip 🙂

9. **vmarko**
October 21, 2013

Bee,

"The pendulum will swing again once people notice that LQG also doesn't 'predict anything'…"

My guess is that the majority of us already know this. The difference is that nobody is trying to advertise LQG as a "theory of everything", "the only game in town", "*the* solution of the QG problem" etc. 🙂

Best, 🙂
Marko

10. **vmarko**
October 21, 2013

Peter,

"The main reason for disappointment about string theory is not that a solution corresponding to the SM hasn't been found, but that the theory predicts nothing at all."

I have noticed lately that people working in string theory have changed their opinion on what ST actually is — it is a *formalism*, rather than any particular theory. In a sense, they now consider ST on par with QFT, which is also just a formalism, and has no serious predictions to give by itself. The word "theory" in the name of both is a misnomer.

Only when one specifies a particular landscape solution in ST, or a particular gauge group and a set of fields in QFT, does one get a theory (or more precisely, a model) that has some concrete predictions.

This change in the point of view by string theorists apparently does away with the "no predictions" criticism — namely, now ST is not even *supposed* to give any predictions, until one provides additional input which picks a particular model within ST. As far as I can see, this new point of view actually makes some sense out of ST. What's your opinion on this?

Best, 🙂
Marko

11. **Avattoir**
October 21, 2013

Is there an equation that predicts the chances of a blog becoming invaded by Von Misians?

12. **Jason Starr**
October 21, 2013

You misspelled "Peirce".

13. **Peter Woit**
October 21, 2013

morris,

Thanks so much for the offer! I wish I could take you up on it, but unfortunately the only time I'll be able to spend in Kenya this trip will be changing planes in Nairobi airport.

14. **Peter Woit**
October 21, 2013

Thanks Jason, fixed.

15. **wolfgang**
October 21, 2013

>> when results at 13 GeV start to come in
I assume you mean 13TeV ?

16. **Per Östborn**
October 21, 2013

You often hear that QFT and string theory are analogous frameworks or formalisms, that none of them are predictive theories per se. I'm an outsider to the field, and Peter may be tired of the entire discussion, but I try to orientate, and I wonder if the following fundamental distinction is proper:

in the QFT approach you build a theory step by step by adding fields, altering the gauge group, and so on. At each step in the construction you may have a unique theory that makes unique predictions what to see and not to see in the next experiment. If this experiment does not agree with the theory, you just add another building block, or adjust parameters.

In contrast, in string theory you begin with a unique theory (ideally), having a vast, possibly infinite number of solutions. Each new experiment makes it possible (in principle) to exclude large chunks of solution space, but regardless how many experiments you make, there may still be so many solutions left that conform with all experiments so far that the outcome of the next experiment is not restricted at all. In this sense string theory may therefore forever lack predictive power. You may have to make an infinite number of experiments to nail the correct solution.

17. **fuzzy**
October 21, 2013

among the various opinions on string theory, i think that it should be fair to remind those of glashow's and feynman. maybe, one could admit that these chaps have been much more long-sighted of those who needed the lhc, before they can begin to think …

(enjoy your travel, peter!)

18. **Kent**
October 21, 2013

Thanks for the links, Peter.
There is enough reading material linked to here that it will keep me busy for the two weeks you are offline.

19. **Peter Woit**
October 21, 2013

wolfgang,
Thanks! Fixed.

vmarko,
This is an FAQ, I really need to get back to updating the FAQ I started for the blog, maybe later today..

Also an FAQ to put in the answer to is my response to claims like the one Bee mentions about seeing string states as a prediction. Very short version: what about M-theory?

20. **Michael Zeiler**
October 21, 2013

Peter, good luck on eclipse day. I'll be in Gabon. Lots of eclipse maps, including detail maps for Uganda, are on my web site at http://eclipse-maps.com/Eclipse-Maps/Gallery/Pages/Annular-total_solar_eclipse_of_2013_November_3.html For those in the eastern US/Canada/Caribbean, you also get a good show – a partial solar eclipse at sunrise. Details at http://eclipse-maps.com/Eclipse-Maps/Gallery/Pages/Annular-total_solar_eclipse_of_2013_November_3_files/HSE2013_Americas.png

21. **Bernhard**
October 21, 2013

vmarko,

Here is an old comment from Peter, that I believe addresses your question, to some extent:

http://www.math.columbia.edu/~woit/wordpress/?p=4065&cpage=1#comment-98564

22. **vmarko**
October 21, 2013

Bernhard,

Thanks for the link! It took a bit of time to read up the context of that conversation, but now I see. That Peter's comment should really be put into the FAQ.

Best, 🙂
Marko

23. **P**
October 21, 2013

Hi Marko,

Your comment is quite reasonable, though I think it's safe to say that this isn't really a new perspective.

The hope in the 80's was that there would be a unique string vacuum. Now we know there is a landscape of string vacua — each vacuum comes with a low energy effective action, and things like the gauge symmetry, spectrum, can be computed. People have been viewing the situation this way for at least ten years.

Cheers,
P

24. **Tim May**
October 22, 2013

Good luck on your trip!

I'm enjoying the book you recommended, Frenkel's "Love and Math." Some very nice explanations of some abstract math. (As a kid, I loved Gamow's "One, Two, Three…Infinity" and books of that type. So books like Frenkel's are a real treasure.)

I'm slightly less negative about string theory in recent years. I started reading your blog, and then your book, after finding a lot of the string theory, M-theory popular accounts way too "hand-wavy."

Also, the hype about string theory seems to have lessened a bit. Unfortunately, the hype deficit seems to be made up for with the hype about the multiverse.

Nevertheless, I'm looking forward to going to Lenny Susskind's talk on black holes tomorrow (Tues, 4:15 pm, see site for details) at Stanford. He's a great speaker, most would agree, and his recent work on the AMPS conjecture doesn't veer off much into the multiverse stuff.

–Tim May, California

25. **Peter Woit**
October 22, 2013

I've added quite a bit more material to the "Frequently Asked Questions" part of the blog, some of which addresses the question from vmarko, as well as Bee's comment about string theory being predictive at high energy.

26. **Simple biologist**
October 22, 2013

Peter,

the new FAQ "Doesn't string theory make predictions at very high energy?" has a broken link.

Link on the the words "see this posting" doesn't work.

27. **Peter Woit**
October 22, 2013

Simple biologist,

Thanks! fixed.

28. **Bernhard**
October 22, 2013

There is another broken link on "Where and when did news of the Higgs discovery first appear?" (the first one).

Also, on "Why do you say string theory is unfalsifiable? Doesn't it predict quantum mechanics?" maybe you could link also to this comment:
http://www.math.columbia.edu/~woit/wordpress/?p=5358&cpage=1#comment-136521

It is the same thing you are saying on the FAQ, but I personally prefer your choice of words on the comment: "The falsifiability criterion is intended to refer to distinctive aspects of a theory that differentiates it from others, not generic properties common to all known theories."). In fact this whole post + comments are relevant to someone looking into this.

29. **Peter Woit**
October 22, 2013

Thanks Bernhard, I've taken your suggestion.

30. **vmarko**
October 22, 2013

Peter,

"I've added quite a bit more material to the "Frequently Asked Questions"…"

It's a good read, thanks!

P,

"People have been viewing the situation this way for at least ten years."

Indeed, as I have just figured out by reading through oldish blog discussions. 🙂 Apparently this is news only to me. 🙂 I've known about the landscape problem for a long time, and I figured that it was a problem that needed to be solved for ST to work. But only recently have I heard that the "landscape problem" was spinned as a "landscape feature" of ST (outside of the context of multiverse&antropics, that is).

Apparently I am not socializing with enough string theorists to be in touch with old news. 😉

Best, 🙂
Marko

31. **James**
October 23, 2013

While you are in Uganda, you should definitely try make it down to see the Mountain Gorillas in the Virunga Volcano region. (if not in Uganda, then just across the border in Rwanda, where I saw them, and I have heard is a better experience) It's the experience of a lifetime. The bus trip from Kampala is extraordinary in and of itself. You can, and I might advise to, make reservations ahead of time.

32. **Dan**
November 10, 2013

> I don't know anyone who is optimistic that it will turn out that the 62.5% energy jump in 2015 will find something when the 400% energy jump didn't

Well, after all the Higgs discovery at LHC needed a 10-20% energy jump from the previous LEP

It’s too soon to declare supersymmetry a tragedy

October 22, 2013
Categories: Uncategorized

Well, maybe one more before I leave…

Tom Siegfried, last heard from telling us that Belief in multiverse requires exceptional vision, now has two new pieces at Science News (here and here) arguing that the failure of the LHC to see SUSY is not really a big problem for SUSY proponents. You see, it’s only a problem if you believe physics theories should be simple and if you believe in naturalness. According to Siegfried, what the LHC is telling us is that you just have to give up on one of these, with your choices now:

1. Give up on simplicity. Just announce that SUSY is fine and solves the naturalness problem, but we’re not seeing it because it’s not the MSSM (which adds more than a hundred parameters), but something really, really complicated, so complicated that it manages to show up in such a way that the LHC experiments can’t see any evidence of it. Believe this, and you can still believe in SUSY, no need to face the tragedy of an idea you’ve done so much to promote getting killed by heartless experimentalists.
2. Give up on naturalness and have the exceptional vision to believe in the multiverse. Then you can fine-tune your SUSY particles up to very high energies and make them unobservable. Again, you’re free to keep believing in SUSY, writing articles and books about it, etc., despite the negative experimental results. The advantage of this option is that you don’t need to make your SUSY complicated, it can just be the MSSM, so you keep simplicity. Of course, once you accept fine-tuning, you could get a whole lot more simplicity really easily: just throw out SUSY and stick to the SM….

Comments

1. **Justin**
   October 22, 2013

   Woit,

   While I sympathize with you on supersymmetry, the death of the theory does raise an interesting question: if not supersymmetry, then what? So, if you were a young researcher who is tired of the obsessive pursuit of a theory which has been experimentally made very unlikely, then what might this student work on if he/she wants to do particle theory?

2. **Peter Woit**
   October 22, 2013

   Justin,
   That one’s a FAQ I don’t have time to write about now. And also, no satisfying answer. It’s not hard to come up with interesting things that we don’t understand about QFT, and personally I think we have just scratched the surface of the relation of QFT to mathematics, but there aren’t ideas around that give a clear path to making progress on getting beyond the SM. In general I think students have to rely on finding advisors who can guide them to interesting things to work on, but while doing this, best to be aware that this is now a difficult business and that lots of people don’t have any promising ideas to work on.

3. **svik**
   October 22, 2013

   Is not that the discovery “that all is fine tuned” and we don’t need the extra baggage.

   But what does it mean?

   Even predators and prey must be fine tuned to work as it does.

   Have a good trip. Watch out for the bugs. Keep small bill for bribes.
   My coworker was there a year ago…. building schools, etc.

4. **Shantanu**
   October 23, 2013

   Justin,
   As Peter recently mentioned its surprising why very few people are working on asymptotic safety. almost no one is working on QFT in curved space time. but probably there are more examples.

5. **Anon**
   October 23, 2013

   Hi Justin — The hierarchy problem is not the only challenge for particle physicists — for example there is the strong CP problem.

   There may be a larger lesson that LHC is teaching us – that the mainstream thinking among high energy physics community need not be natures thinking for addressing outstanding problems. Also that the mainstream thought process can be delusional.

   Thus putting the faith in SUSY for hierarchy and axions for strong CP, as the mainstream particle physics community does, may not necessarily be the direction a young and bold researcher should take today. Historically nature has not necessarily endorsed mainstream scientific thinking. Just because the present day particle physics appears mathematically more sophisticated, it doesn’t mean big names in the field aren’t being mislead and aren’t misleading.

6. **CU Phil**
   October 23, 2013

   FWIW, the recommendation given by some very well-regarded HEP faculty here to several very smart undergrads as they went off to grad school was “don’t do HEP theory”. So there’s that.

7. **Tom**
   October 23, 2013

   believe believe believe is the most used word … so why not believe in an old man sitting behind the clouds?

8. **vmarko**
   October 23, 2013

   This is, IMO, the most convincing argument for SUSY from Siegfried's article:

   "After all, isn't a supernova better than a nova? Supermodel better than a model? Superman better than Clark Kent?"

   Unbelievable.

   Best, 🙂
   Marko

9. **Peter Woit**
   October 23, 2013

   Thanks to all for the suggestions about the trip. Leaving soon, so will shut off comments for the duration.

10. **Kavanna**
    November 7, 2013

    "Super," indeed. Supersymmetry may yet be proved right, but implemented in some way, both the symmetry and its breaking, that we never thought of. Certainly, the whole approach so far has been driven by two considerations that might be a tad myopic: the need to solve the weak-grand/gravity hierarchy problem, and the assumption that everything is understandable in perturbation theory or extensions thereof.

    Maybe the hierarchy problem and grand unification are collaterally solved by something else that's driven by some other consideration, like the cosmological constant — just to pick something not completely random.

    It is good to hear that senior HE theorists are turning students away from string theory and related problems. Certainly, the way misguided senior theorists have dominated the field for the last 20+ years has been destructive. At this point, we should want only those few who are both really determined and not under the sway of a generation-and-a-half of failure. Those not interested in "high" theory should try astrophysics or something else where mind and sense are connected. Let's encourage everyone to leave the Platonic never-never land of mental masturbation.

11. **Kavanna**
    November 7, 2013

    P.S. I don't mean to denigrate mathematics with that final remark. Math doesn't have to be connected directly to counting and measuring in the real world. But as Peter has often pointed out, to qualify as serious, the math should be rigorous and precise, whether it has an obvious immediate application or not.

Back to the Usual

November 6, 2013
Categories: Uncategorized

I'm now back to regular internet access, in London for a few days after a trip to East Africa, where I managed to see the November 3 total solar eclipse through light clouds from a location in Northern Uganda. From checking various news sources, it looks like the main pieces of HEP-related news that I missed weren't very surprising:

- The LUX experiment reported stronger limits on WIMP dark matter, ruling out various claims for evidence of such dark matter particles at relatively low mass. For more about this, good sources are Resonaances, Matt Strassler, and Tommaso Dorigo.
- The $3 million Fundamental Physics Prize as usual identifies "Fundamental Physics" with string theory, with the announcement that the nominees for the 2014 prize are 5 string theorists (Polchinski, Green/Schwarz, and Strominger/Vafa). I confess that I can't figure out exactly how this prize process is supposed to work. The announcement says that the nominees get a "Physics Frontiers Prize", a shot at the $3 million, and

  > Those who do not win it will each receive $300,000 and will automatically be re-nominated for the next 5 years.

  What I don't understand is that Polchinski already got such a nomination and prize last year (and the $300,000 consolation prize for not getting the $3 million). It seems that he is getting another identical prize this year, with another $300,000 or $3 million. On the other hand, the only non-string theorists ever to win this prize (last year's condensed matter group Kane/Molenkamp/Zhang) didn't get a second one this year, and it's unclear if they still have a possible $3 million payday in 2014. Perhaps the rules are different for string theorists, the idea being that you just can't give too many prizes for string theory.

  I'll bet that Green/Schwarz will be the 2014 winners, on the grounds that if you're going to hand out lots of prizes for working on the superstring, its co-discoverers should be among the first in line. While this means that Polchinski will only get a second $300,000, it's in his interest to lose as many times as possible before winning.

  As for the $100,000 prizes for young researchers, this year was different than last year. The winners (Cachazo/Minwalla/Rychkov) were two Princeton Ph.Ds and one ex-Princeton post-doc, whereas last year is was one Princeton Ph.D and all three were ex-Princeton post-docs.

- In other news, Max Tegmark, known for his work on the multiverse, is running a "Project Einstein", which has found 400 theoretical physicists and mathematicians who have agreed to have their genes sequenced. The idea is that they are "math geniuses", but no one seems to know what will be done with the genetic data for these geniuses. It's unclear who these "geniuses" are, but we do know that one person who was asked and declined was Curt McMullen. His reaction to this project was what I suspect was a common one:

  > "I thought it was strange that it was called 'Project Einstein', which seemed designed to appeal to the participants' egos," he says. He asked the project's staff and the New England Institutional Review Board, which approved the study, to explain how results would be used. "The uniform answer to my questions was that 'we are not responsible for how the information is used after the study is completed'," he says.

  If Project Einstein identifies a common gene among its participants, and uses the knowledge to breed a race of übermenschen, they may find they have selected not for unusual mathematical genius, but for unusual ego.

Update: I realized there's one other remarkable thing about the six winners of the "New Horizons in Physics Prize". Besides all having a close Princeton connection, none of them has a job in the US. It seems US physics departments are not buying what Princeton is selling right now…

Comments

1. **svik**
   November 6, 2013

   I don't think there is a math gene(s).
   The common denominator is being unwanted and a social outcast, this causes us to turn to the love of math as an escape. Yes it is so with most scientists.
   ….
   Now if a math gene is found I support math men should only marry the sisters of other math geniuses. Then in 60 years we will have a new breed of mathies that can finally prove or revoke string theory. 🙂

2. **Kimmo Rouvari**
   November 6, 2013

   Automatic nomination for the next five years doesn't mean that the nominee will be automaticly among those three front runners.

3. **David M.**
   November 7, 2013

   Peter,
   About the Fundamental Physics Prize, I would definitely agree there is a Princeton/String theory bias, although I don't necessarily think that its a bad thing. I don't see anyone on that list who does not deserved to be honored. Their work is invaluable for our understanding of QFT and quantum gravity even if String Theory does not describe nature.

   My question to you is, who would you choose to be given such an award? I mean with the current purpose of the award, to honor physicists whose work is of paramount theoretical import, but may not be directly connected to experimental physics and/or is underappreciated by the general public.

   I don't ask this as a challenge, I'm not implying these are the only people who should be honored. I'm asking because when I think of people who should be given such an honor these are the names I would think of first and I'm genuinely curious.

4. **Peter Woit**
   November 7, 2013

   Kimmo Rouvari,

   Maybe you're right that's how it works. Kind of odd though, since anyone can nominate anyone on-line, being automatically nominated for five years isn't very meaningful. If that's how it works, one wonders why a non-string theory nomination was at the top of the list last year, didn't make it this year.

David M.,

One obvious name would be Roger Penrose's. He's the one responsible for the twistor techniques at the heart of the whole amplitudes business. Do you think hostility to string theory might have something to do with why he's not $3 million richer?

The overall problem with these prizes is that they are based on an extremely narrow vision of what "fundamental physics" is, both intellectually and geographically. Furthermore, this is a narrow vision that has been extremely unsuccessful over the past few decades, with one reason for its failures precisely this narrowness.

5. **Bee**
November 7, 2013

übermenchen -> übermenschen

6. **Peter Woit**
November 7, 2013

Thanks Bee, fixed.

7. **Armin Nikkhah Shirazi**
November 7, 2013

David M,

It seems to me that your perspective is so deeply entrenched in the current paradigm that the central issue escaped you completely. The issue does not center around whether the current nominees are deserving of honor or not but around the fact that the name of this award clearly would lead one to believe that it is given for major advances in fundamental physics, but that it is given in actuality only for major advances in just one subfield which, moreover, still awaits experimental confirmation; the prize is not what it purports to be.
Also, it seems to me that calling a string theory prize a fundamental physics prize is a slap in the face of all the theorists who have actually made what would be rightfully regarded as outstanding contributions in fundamental physics which happen not to be related to string theory (or only indirectly so). I think that instead of talking about these theorists in the abstract, it is more effective to actually name some of them, and indeed you requested a list of names.
So, with apologies to Peter here is my own subjective list of living non-nobel laureate theorists who I think would make terrible candidates for a string theory prize but outstanding candidates for a fundamental physics prize (I almost certainly omitted many other highly deserving physicists but that would only further make my point):

1. Freeman Dyson (Many contributions)
2. Roger Penrose (Many contributions)
3. George Zweig (Independent discovery of quarks)
4. Jim Peebles (Prediction of CMB)
5. George Sudarshan (Sudarshan-Glauber P representation, Quantum Zeno effect)
6. Yakir Aharonov (AB effect, Weak Measurement Theory)
7. Simon Kochen (Kochen-Specker Theorem)
8. John Clauser/Michael Horner/Abner Shimony/Richard Holt (CHSH inequality)
9. Gerald Guralnick/ Carl Richard Hagen /Tom Kibble (Higgs Mechanism)
10. Jeffrey Goldstone (Goldstone theorem)
11. N. David Mermin/Herbert Wagner (Mermin-Wagner Theorem)
12. Ludvig Fadeev (several eponymous results in QFT)
13. Hagen Kleinert (significant expansion of domain of applicability of path integrals)
14. Rudolf Haag (Haag's theorem)
15. Martin Reese (Cosmology)
16. Jakob Bekenstein (Black Hole Thermodynamics)
17. William Unruh/Paul Davies/ Stephen Fulling (Unruh Effect)
18. Richard Arnowitt/ Stanley Deser/ Charles Misner (ABM Formalism)
19. Yoshio Koide (Koide formula)
20. Toichiro Kinoshita (precision QFT computations)

I'll let you compare this list with that of the current nominees and draw your own conclusions.

8. **A String Theorist**
November 7, 2013

You write "It seems US physics departments are not buying what Princeton is selling right now" but I think you have it completely backward. To my personal knowledge most (perhaps all?) of the winners had job offers in the US, but the landscape (no pun intended!) has really changed in recent years. Frankly, there is very little that US academic institutions can offer compared to many of the top places outside the US. Considerations across the board, from salary, to research support, to funding possibilities, to the quality of available graduate students and postdocs, to the increasing ability of young researchers to stay in their home country (if they choose to), lead many to the conclusion that the US is certain no longer "the place to be" for theoretical physics. As a theoretical physicist as a US institution, I'm happy that great opportunities are becoming available outside the US, but on the flip side I'm saddened to see the decline in the US.

9. **Philip Gibbs**
November 7, 2013

Armin Nikkhah Shirazi, your list includes many great achievements but the rules insist that the prizes "Should recognize major achievements, with special attention to recent developments." I don't see anyone on your list whose work is both major enough and recent enough compared to those recognised so far.

10. **jim**
November 7, 2013

The thinking motivating "Project Einstein" is the reason you never let theorists touch your equipment.

11. **Kavanna**
November 7, 2013

Armin's list is great and shows that, while Nobel Prizes can recognize greatness, they just as often miss it, for whatever reason. A classic fun game is to name the great writers who didn't receive the Literature Prize.

US physics departments are not buying what Princeton's selling — a correct statement for the last 5-6 years. It's not just opportunities opening up elsewhere; those have been there for a while. Thankfully, US physics faculties are no longer scrambling for crumbs from the Princeton table. Better late than never.

12. **Kris Krogh**
November 7, 2013

Philip Gibbs,

"I don't see anyone on [Armin Nikkhah Shirazi's] list whose work is both major enough and recent enough compared to those recognised so far."

Are you familiar with Aharonov's work? It's true the AB effect is not recent, but his work on weak measurements is, and is currently having a major impact.

13. **Armin Nikkhah Shirazi**
November 7, 2013

Hi Phil,

Well, what does the statement "Should recognize major achievements, with special attention to recent developments" actually mean? It seems to me that under a liberal interpretation, one could, for example, even take Dyson's contribution from the late 1940's to be a major achievement "with special attention" to the discovery of the Higgs boson. The nobel committee certainly had no qualms about considering a theoretical discovery made almost 50 years ago as compatible with the specification in Alfred Nobel's will that the prize be awarded to "those who, during the preceding year, shall have conferred the greatest benefit on mankind."

But, let me not play this game and just partially agree with you. I think that some, but not all, of the achievements may be difficult to square with a reasonable interpretation of that (in my opinion highly ambiguous) sentence. However, even then I don't see how your argument challenges the point that what is being called a fundamental physics prize is in reality a string theory prize, unless you are suggesting that the only recent significant theoretical discoveries come excusively out of string theory. I hope that is not what you mean to imply, for if you do, then it reflects that you are just as guilty of the narrow vision and-pardon me for expressing this because I truly have great respect for you-hubris as what allowed something like this to happen in the first place.

14. **Jeffo**
November 7, 2013

Based on the picture accompanying the Nature article, I'd say it's more urgent that we identify the "bad-fashion-sense" gene!

15. **David M.**
November 7, 2013

Armin,
In your original list I do agree that many physicists you mention deserved to be honored, but like Phil mentioned they did their most important work a while back, pre 1980s typically. I know there are plenty of people out there who deserved a nobel but thats outside the parameters of the debate. And there are physicists who deserve to be honored for important work done in the past 30 years, Mikhail Shifman, Randall & Sundrum, and Xiao Gang Wen come to mind. I said the people listed were the first to come to mind not the only ones!

I also have to say I disagree with the characterization that US institutions are some how no longer hiring people from princeton. If you check out the particle theory rumor mill for 2013 you'll see that 7 people offered jobs either got their Ph.D at Princeton or did a postdoc there. Thats out of 17 so ~41%.

16. **Philip Gibbs**
November 7, 2013

Whoa Armin! You are putting an awful lot of words in my mouth. I never said or implied anything about whether the current distribution of prizes is too narrow.

I do think the string theory prizes are merited but I agree that there are other recent areas of fundamental physics that could be included. The problem is that the whole area has been fractured down the middle by the string wars. If Milner had started out giving the prizes to Smolin, Rovelli etc we might now see all the prizes going to people connected to the Perimeter Institute instead of Princeton. That's just the way the field is at the moment.

17. **JHe**
November 7, 2013

Dear Phil,
The link at your name is blog.vixra.com. It should be blog.vixra.org. It must be your mistake.

18. **Peter Woit**
November 7, 2013

A String Theorist,

I don't doubt that the 6 prize winners could have found some sort of job in the US, but I think the evidence is that there was less enthusiasm for hiring them in the US than in the other places they ended up. In mathematics the best young US-trained mathematicians are aggressively recruited by US institutions, which do very well in competing with offers from other countries. My impression is that US physics departments are now rarely willing to do this kind of recruiting in string theory.

David M.,
I haven't checked your numbers, but in any case I didn't intend to claim there's some sort of prejudice about hiring anyone with a Princeton connection, the reference was to string theory, and plenty of theorists with Princeton connections work in quite different areas.

19. **Peter Woit**
November 7, 2013

One person contacted by "Project Einstein" forwarded me the e-mails they sent. They didn't call it "Project Einstein" in the e-mails, and said nothing about what they planned to do with the data other than to collaborate with Tegmark on some sort of data analysis to "uncover the genetics of mathematical reasoning and abstract thought". This person's reaction to the request for his genetic material was that he really was only interested in providing it in the time-honored standard biological manner, but it seems that wasn't what "Project Einstein" had in mind.

20. **IM**
November 8, 2013

Among the recipients of the New Horizons in Physics Prize,
Slava Rychkov's work, cited as
"for developing new techniques in conformal field theory, reviving the conformal bootstrap program for constraining the spectrum of operators and the structure constants in 3D and 4D CFT's"
is entirely on field theory
(though he has earlier worked on string theory, and CFT results may
be relevant for AdS/CFT).
See https://sites.google.com/site/slavarychkov/
for his ongoing work.

21. **Peter Woit**
November 8, 2013

IM,
Thanks. Actually, none of the new young prize winners are really doing string theory, in the sense of having a significant connection to the quantized theory of strings. There is though some subfield of theoretical physics that one could best characterize only sociologically, as "those topics discussed at Strings 20XX", which these days mostly don't have to do with strings. This field deserves a name, but I don't know what to call it, and people often call it "string theory". Calling it "Formal QFT" as opposed to "Phenomenological QFT", has the drawback that it implies that there is no non-phenomenological qft work that isn't part of this subfield, which is far from the truth. The other problem is that this is highly time-dependent, these days you can for instance be doing twistors or hydrodynamics and fit in here, but at previous times that would very much not be the case.

22. **John**
November 8, 2013

Concerning the prize for young researchers, I wonder why nobody mentioned an apparent conflict of interest: Witten-Gaiotto, Seiberg-Komargodsi, Polyakov-Rychkov,… It's like having parents in the selection comittee.

23. **layman**
November 8, 2013

corrections:

1.) US is not buying: you have no idea what you are talking about and which job opportunities some of the laureates had. I would call some of the offers I know people had quite aggressive and generous. It is just that nowadays some institutions outside of the US (mostly private ones) can easily compete even with the best offers from Harvard and Princeton… And why not settle in Europe, where the food is much better and the health care much nicer? 🙂

2.) The list of Mr Armin: 90% of the people you mentioned do not fit the definition of the prize either because they have not done something which is significant enough or because they have been inactive for 20 years. Also you have a typo: ABM->ADM.
The 10% who fit the definition of the prize may very well get it in the future, I dont see why not. You also missed many deserving cosmologists and condensed matter physicists.

3.) Penrose does not fit the definition of the prize, which is why he will never get it, regardless of his achievements. Other people who hate string theory may well get it because they do fit the definition.

4.) Many of the other comments here reflect deep ignorance of what is going on. Also not being able to understand the rules of the prize by reading the 2 online paragraphs does not bode well for being ever able to understand modern physics (but I guess we already knew that).

24. **Peter Woit**
November 8, 2013

layman,
Why does Penrose not fit the definition of the prize (for his work on twistors + singularities in GR?)?

If string theorists are as attractive to US institutions as people in other fields, why do you think US institutions are ending up with many of the best young mathematicians, but not the string theorists? Back in the late 1980s there were many US departments trying to hire in string theory, making the best offers in the world (e.g. Rutgers). Can you point to even one US physics department that now has hiring a string theorist as its top priority?

And, by the way, do you think the perception that string theorists are insufferably arrogant people who go on endlessly about how anyone who disagrees with them is stupid and ignorant might have anything to do with why these departments aren't hiring string theorists?

25. **layman**
November 8, 2013

The ease with which you make claims which are as bold as they are wrong is astonishing. I hope that the standards you apply when you do research are slightly higher. It is you, not me, who said " It seems US physics departments are not buying what Princeton is selling right now… ." You had not a single bit of knowledge on any of the personal circumstances of the laureates, about the offers they had from elsewhere (which are sometimes more attractive salary-wise than the salary of any two young mathematicians anywhere in the US combined!), and about the offers they may or may not hold now. Yet, in spite of that, you made a bold claim and drew a far-reaching conclusion from that claim based on zero understanding of the data and the circumstances.

Let me also assure you that some people among the six had offers from the US far exceeding salary-wise the salary of ANY single young mathematician in the US.

I think it is not surprising that a person would react arrogantly to claims made with such confidence but with so little actual knowledge. I think it is much more arrogant to feed (presumably) thousands of your readers with wide speculations framed as universal truths. On the other hand, maybe your readers deserve to be treated liked that 🙂

To further demonstrate how pathetic this discussion is, let me please mention the four Fields medalists of 2010, Elon Lindenstrauss, Ngô Bảo Châu, Stanislav Smirnov, Cedric Villani. Now let us test your theory that the US attracts bright mathematicians by checking how many of these 4 had a permanent job at the US at the time they got the medal. Answer: 0/4. It is true that at least 2 of them spent ample time in the USA before or after the medal, but they did not have a permanent job there and most of their time was spent outside of US soil. I would say the situation is remarkably reminiscent of the physics prize. Also 2/4 are very closely tied with Princeton. So it seems that also in math the US is not buying what Princeton is selling.

I think at this point I am flogging a dead horse, but regarding your other questions:

"Can you point to even one US physics department that now has hiring a string theorist as its top priority?"

I am not based in the US, and unlike you, I am not discussing matters with which I have no intimate knowledge so I don't have an answer. I just know that many hep-th people are getting extremely generous offers from many places in the US. So I assume it is a very high priority.

"Why does Penrose not fit the definition of the prize (for his work on twistors + singularities in GR?)?"

The prize is not awarded to people who did *great* stuff in the early 70s and after that either decoupled or became lunatics. The emphasis is on people who are still active and are doing good and sensible things. Unlike Penrose, Hawking got the prize and fits the definitions quite well (he was an avid participant in the modern discussions about the information paradox and the wave function of the universe).

26. **layman**
November 8, 2013

Also many of Penrose's results are in collaboration with Hawking, who later did much more important and *fundamental* things than twistors or the classical singularity theorems. So if one has to give the prize to one of the two the choice is obvious. Especially that it is a FPP and not a prize in mathematical physics.

So unfortunately, despite the monumental effort of the impressive hive mind here, nobody came up with a single name of a clearly more deserving FPP laureate than the existing ones. Sure, there are many people who deserve, but no evidence points to the fact that the selection is biased. (BTW, even though we are talking small numbers here, the PU and IAS seem to be comparably dominant among the Fields medalists as the FPP, and in both cases many of the laureates just spent time in IAS or PU, and then moved elsewhere, often not on US soil.)

27. **Anonymous Lurker**
November 8, 2013

Layman:

Where does it say that a recipient of the Fundamental Physics Prize does it say that the recipient still has to be active? I looked at the web, and didn't see this criterion anywhere.

Hawking's citation says "For his discovery of Hawking radiation from black holes, and his deep contributions to quantum gravity and quantum aspects of the early universe." If the prize is supposed to be for recent work, why did they not mention any of his recent work?

And is Alexander Polyakov still active? He got the prize last year, he's 68, he was certainly a great physicist, but it doesn't look to me like he's done that much in the last few years.

28. **Peter Woit**
November 8, 2013

layman,

From what you write it's clear you have no idea what kinds of salaries the best young mathematicians in the US can command (they're every bit comparable to the what the best young physicists are offered, the star system is not just in physics). As for my comment about what US departments are not buying, it's simply a fact. Your argument is just that they're not buying because they can't afford to pay the price. I don't believe this because I'm well informed about the mathematics job market, what the competition for the best people is like, and who is paying what. All the evidence I've seen is that the market in physics is not that different.

The Fields medalists you mention are different in a lot of ways (one of which is that this is a distinction universally recognized by the math community, whereas Milner's prize is a very different story). The market for Fields medalists is highly competitive, with US universities willing to pay top dollar for them. Ngo chose an offer from Chicago after his Fields medal, and, while I don't know the details, I don't think low salary offers from US institutions is the reason the other three have chosen to stay where they are.

As for Hawking/Penrose, I don't see how twistors aren't a truly fundamental idea, it's one of the bases for the modern work on amplitudes, every bit as significant as the string theory-related advances that many of the FPP winners got their prizes for. Being an "avid participant in the modern discussions about the information paradox and the wave function of the universe'" seems to me to have nothing to do with whether one should get a prize for anything or not. Referring to Penrose as a "lunatic" is worthy of Lubos, you should try and get a grip. I'm no fan of some of Penrose's recent ideas, but they're no sillier than the M-theory multiverse business that Hawking has been writing books pushing.

29. **A String Theorist**
November 8, 2013

Expanding on my comment above: it's interesting that you mention Rutgers, Peter, because one of the appealing aspects of Rutgers in those days (I don't know if its still true?) is that they could offer string theorists 1/2 teaching load (that is, one course per calendar year).

In my comment above, where I mentioned some of the factors that make some positions outside the US so appealing, embarrassingly I forgot to mention the Holy Grail! Several (perhaps all? I'm not sure) of the six New Horizons winners have non-teaching positions! Zero! No physics department in the US can offer that, and it is priceless.

30. **Bob Jones**
November 8, 2013

I think it's only a matter of time before Strominger and Vafa win this prize. I'm was actually surprised that they didn't win it the first time around.

I'm also surprised that no one has mentioned 't Hooft. He seems to be the perfect candidate for this.

31. **paddy**
November 8, 2013

@ A String Theorist
Is it a good thing that academic institutions do not require teaching? This was my second thought after facetiously thinking: Thank heaven they won't be teaching.

32. **Philip Gibbs**
November 9, 2013

On the subject of Project Einstein, John Baez has said on Google+ that he will participate and gave some details about the consent conditions. I think the traits and circumstances that lead to people becoming maths or physics leaders is too broad for such a small sample of genomes. They might do better if they concentrated on people who showed early signs of ability, e.g. those who did well in maths olympiads. There is also the Personal Genome Project http://www.personalgenomes.org/ with more general aims which is being conducted in a way that has a better chance of success.

33. **layman**
November 9, 2013

Indeed, no teaching is one of the main perks institutions overseas can offer (there are many others). It is not that US institutions are not trying, 99% of them just cant make attractive enough offers.

Most people prefer to have a slightly lower salary than to have to teach every semester.

"I don't see how twistors aren't a truly fundamental idea, it's one of the bases for the modern work on amplitudes, every bit as significant as the string theory-related advances that many of the FPP winners got their prizes for"

Twistors is simply a "notational" tool (i.e. kinematical) there is nothing truly deep about this. Hawking's semi-classical computation is deep, far-reaching, and very truly important.

34. **Thomas Larsson**
November 9, 2013

For those who don't think that work done during the 1980s is recent, consider this:

In this excerpt of the will, Alfred Nobel dictates that his entire remaining estate should be used to endow "prizes to those who, during the preceding year, shall have conferred the greatest benefit to mankind."

Seems to me that pretty much every Nobel winner was disqualified, except perhaps Rubbia and van der Meer, and Bednorz and Müller.

35. **Shantanu**
November 9, 2013

Peter and others, something OT/old news.
Found the videos the conference Cosmo 2013 in Cambridge.

http://www.ctc.cam.ac.uk/activities/cosmo2013/programme.php
In particular the talk by John Ellis, in which he argued that the Planck
data supports Starobinsky model of inflation which begs for supersymmetry.
shantanu

36. **Mitchell Porter**
November 9, 2013

"layman" said

"Twistors is simply a "notational" tool (i.e. kinematical) there is nothing truly deep about this."

It's just an accident that they gave us the twistor string and the amplituhedron?

37. **Peter Woit**
November 9, 2013

layman,
The idea that twistors are just a notational innovation is very funny. You seem to be arguing that major prizes can't go to new ideas about "kinematics", which I guess would explain why Einstein was never awarded a Nobel for special relativity.

Interesting to see that the young prize winners have gone from making twice as much as people in the US to taking less money in exchange for having to teach less.

From experience, I think anyone who insists on never teaching is making a big mistake. Teaching a subject is the best way of really understanding it deeply.

38. **Low Math, Meekly Interacting**
November 9, 2013

An absolute requirement for determining heritability of a complex trait is controlling for variance. Soliciting DNA from a bunch of math whizzes does not strike me as a particularly effective sampling protocol. If by "not crazy" Rothberg means "compared to wiping my backside with my surplus of $100 bills" I suppose that would be accurate enough. While basic statistics seems to have failed Dr. Tegmark here, I bet he could calculate the odds that there exists a copy of himself that stumbles on the "math gene" by pure chance on some branch of the wavefunction.

39. **Peter Orland**
November 9, 2013

"An absolute requirement for determining heritability of a complex trait is controlling for variance. Soliciting DNA from a bunch of math whizzes does not strike me as a particularly effective sampling protocol."

Indeed. It a was notion Einstein and other twentieth-century humanists had deep disdain for.

40. **Pawl**
November 9, 2013

Two comments:

(a) The notion that "many of Penrose's results are in collaboration with Hawking" is at best highly misleading. It was Penrose who was, far and away, most responsible for the development of the deep geometry of causal structure, including the first singularity theorem. Penrose's results completely transformed the way people thought of the field. Hawking really came along later.

(b) It's evident that the prizes have been awarded in a remarkably narrow way, and this really makes one wonder what the Selection Committee's perspective on physics is.

41. **Peter Orland**
November 9, 2013

I meant to write:

Indeed. It also a was notion Einstein and other twentieth-century humanists had deep disdain for.

42. **Simple biologist**
November 9, 2013

"An absolute requirement for determining heritability of a complex trait is controlling for variance. Soliciting DNA from a bunch of math whizzes does not strike me as a particularly effective sampling protocol."

The whole idea is ridicilous, but the quoted part isn't really problematic. There exists already plentiful amount of sequenced human genomes and the number is growing. Majority (almost all?) of it is availeable on free and publicly availeable databases. No need for bigger sampling protocol.

They could always just sequence math wizards and use "normal" sequenced genomes from other sources for control and variance.

43. **Armin Nikkhah Shirazi**
November 9, 2013

In response to my list above, some people have pointed out that the contributions of most of the people on the list had either not been recent or significant enough to merit the FPP. At first, I was not going to protract a discussion of this issue because comparing scientists' achievements is an awkward business of dubious taste, but "Layman" 's claim that ".. nobody came up with a single name of a clearly more deserving FPP laureate than the existing ones" is so unbelievably pompous that I feel it cannot stand without a detailed, evidence-based rebuttal.
Before I give my rebuttal, I want to emphasize that I mean in no way to belittle the FPP laureate's distinguished achievements, I only want to show that "Layman"'s claim, besides being unbelievably arrogant, may be shown by evidence to be false.
The 2013 FPP Laureate is Alexander Polyakov. His citation reads:
"for his many discoveries in field theory and string theory including the conformal bootstrap, magnetic monopoles, instantons, confinement/de-confinement, the quantization of strings in non-critical dimensions, gauge/string duality and many others. His ideas have dominated the scene in these fields during the past decades."
The contrapositive of the claim by David M., Phillip Gibbs and "Layman" is that Polyakov deserved the FPP because unlike any (or most) of the people on the list, his achievements were 1) sufficiently recent and/or 2) sufficiently significant.

Let us test for recency:
The first relevant work on conformal bootstraps of which I am aware seems to be
Polyakov, A.M., "Non-Hamiltonian approach to conformal quantum field theory" Soviet Physics JETP, Vol. 39, p.10 (1974)
The first relevant work on magnetic monopoles of which I am aware seems to be
Polyakov, A.M., "Particle spectrum in quantum field theory", Journal of Experimental and Theoretical Physics Letters, Vol. 20, p.194 (1974)
The first relevant work on instantons of which I am aware seems to be
Belavin, A.A., Polyakov, A.M., Schwartz A.S. , Tyupkin, Yu. S. , "Pseudoparticle solutions of the Yang-Mills equations", Physics Letters B, Vol. 59, p. 85 (1975)
The first relevant work on quark confinement/de-confinement of which I am aware seems to be
Polyakov, A.M. "Quark Confinement and topology of gauge theories", Nuclear Physics B, Vol. 120, 3, p. 429 (1977)
The first relevant work on the quantization of strings in non-critical dimensions of which I am aware seems to be
Polyakov A.M. "Quantum geometry of bosonic strings", Physics Letters N, Vol. 103, p. 207 (1981)
The first relevent work on gauge/string duality of which I am aware seems to be
Polyakov A.M. "String Representations and hidden symmetries for gauge fields", Physics Letters B, Vol. 82, p. 247 (1979)

Phillip Gibbs gave no cutoff threshold for recency, David M. gave one of 30 years, "Layman" gave (indirectly) one of 20 years. The most recent of the above publications is 32 years old. The others are all pre-1980's.

Let us now turn our attention to significance. This is a highly contentious matter because it is so subjective. Different people have different criteria for significance, and unless everyone agrees on the same set of criteria, there will be endless discussion without resolution. We are scientists not philosophers, so I think we should confine ourselves to the one criterion that is entirely uncontroversial in terms of determining the value of a fundamental idea in science: Whether it has been empirically confirmed. If anyone thinks that there exists a more important criterion in the evaluation of a fundamental scientific idea than empirical confirmation, you are welcome to make your case.
Let me now ask the following questions:
1) Has the conformal bootstrap been empirically confirmed?
2) Have `t Hooft-Polyakov monopoles been empirically shown to exist?
3) Have BPST instantons been empirically shown to exist?
4) Have quantized or any other kinds of strings been empirically shown to exist?

Compare this now to the following questions (remember, we are treating this apart from recency):
1) Has QED been empirically confirmed?
2) Have quarks been empirically shown to exist?
2) Has the CMB been empirically confirmed?
4) Has the Aharonov-Bohm been empirically confirmed?

I think this is sufficient to demonstrate just how ludicrous "Layman"'s above claim is, but I doubt that he will concede because it is quite clear to me that he did not arrive at this position because of any evidence but because he has an emotional commitment to the position he holds. One encounters this most often in political and religious debates, and (thankfully) less often in scientific debates. A hallmark of people who argue in the absence of supporting evidence is that they resort to various fallacies, such as
1. Ad hominem ("Many of the other comments here reflect deep ignorance of what is going on.")
2. Innuendo ("I hope that the standards you apply when you do research are slightly higher")
3. Obfuscation by Introduction of irrelevant considerations ("Layman"'s fields medalist "Argument")
4. Misrepresentation ("Twistors is simply a "notational" tool").
However, "Layman" did give one correct argument. He correctly pointed out my typo, that it is ADM, not ABM and I thank him for that. I just wished he would make giving substantive arguments the rule instead of the exception.

44. **Yatima**
November 9, 2013

Armin Nikkhah Shirazi says: [good stuff elided]

Hear, Hear!

45. **lun**
November 9, 2013

Hoping to find a "genius math gene" is, without exaggerating, a thousand times more crackpot than anything relating to the string landscape. And much more dangerous. It really does not help that otherwise reputable scientists are willing to lend respectability to such crackpottery.
Anyone who doubts that, check out
this and
this. Or do some research on the Flynn effect.

46. **layman**
November 9, 2013

1) Has the conformal bootstrap been empirically confirmed?

Yes. It is known for some years now that the conformal bootstrap equations do
postdict the critical points of 3d Ising and He_3 and others. They also predict many new 2nd order transitions.

2) Have `t Hooft-Polyakov monopoles been empirically shown to exist?

Yes. In condensed matter physics monopoles were found, of the type 't Hooft-Polyakov envisioned.

3) Have BPST instantons been empirically shown to exist?

Yes, long ago, for example, in 3d.

4) Have quantized or any other kinds of strings been empirically shown to exist?

No.

Note that almost all the achievements above are very well deserved and have not been already celebrated by many other major prizes.

1) Has QED been empirically confirmed?
Yes, and it is completely irrelevant to celebrate this pre-history in the 21st century. All the people responsible for this are either dead or in their 90s.

2) Have quarks been empirically shown to exist?
Yes, and the same comment as in 1) applies.

3) Yes. This should be recognized.

4) Yes, but it is too old (more than 60 years).

47. **amused**
November 9, 2013

It's an amusing exercise to contrast the views of the people who awarded the latest round of Milner prizes with the views in the recent document "The Future of US Particle Theory: Report of the DPF Theory Panel" (arXiv:13106111) which includes a substantial fraction of string/BSM theorists among the authors. It contains a summary of significant advances in recent years (as well as predictions for where the action will be in the future) – presumably quite relevant when considering which recent developments might be worthy of a big prize.

Curiously, the dominance of string theory in the Milnor prizes is not mirrored by dominance of ST in the description of significant advances in this document… In fact it gives the impression that important advances have occurred across a range of different areas, including some obscure and unheard of ones (unheard of at least to readers of blogs, popular science magazines and the NY Times). E.g. an area called lattice gauge theory, in which, according to the document, "astounding" progress has occurred over the last decade.

It's completely understandable though that, regardless of whatever advances have occurred in such areas, they can't come into consideration for a prize such as Milnor's, since it would involve honoring the contributions of people not working at or connected to Princeton, Harvard and the other few institutions to which such prize winners must belong. Giving such a prize to people outside of those institutions would be a hideous offense against the natural order of things – unthinkable!

48. **kashyap vasavada**
November 9, 2013

Layman writes outrageous words like "lunatic" for such a great mathematical physicist Penrose. If he believes Penrose should not get FPP that is perfectly OK. But he has no business calling Penrose lunatic. I am reasonably sure layman is very ordinary physicist and will never get FPP or nobel prize!! May be there should be a rule that one has to give his/her real name in the blogs. Such people are cowards who hide under pseudo names , so they can say any thing they like without any possible penalty.. I have always used my real name.

49. **piscator**
November 9, 2013

Look, this is clearly a Princeton prize. The area the so-called New Horizons prize is awarded in is 'mathematical physics topics of interest to IAS/Princeton particle theory faculty', and the best indicator of getting a prize is being a protégé
of said faculty. And I think if layman finds it hard to think of more deserving awardees of the FPP he should just learn some more physics -as in, the subject about describing this world. In any case, based on various sources it appears that some of the original FPP prizes were essentially self-awarded. Within the professional particle theory world I haven't met anyone yet who thinks these Milner prizes are a positive thing

50. **Low Math, Meekly Interacting**
November 9, 2013

S.B. – One needs to seriously consider what traits and background are likely required to hold a physics or mathematics professorship at a top-tier university, and if it's possible to control for selection bias such that the signal:noise allows one to reliably identify alleles that would be responsible for innate quantitative ability in the general population. I'm guessing the answer is a resounding no. I don't see how using a random selection of public-domain genomes as a baseline could help.

51. **Bob Jones**
November 10, 2013

Armin Nikkhah Shirazi,

Your last post makes no sense. Polyakov's prize citation mentioned "quantization of strings in non-critical dimensions" and "gauge/string duality", and your response was "Have quantized or any other kind of strings been empirically shown to exist?"

It's obvious from your question that you have no idea what kind of strings we're talking about here. The term "non-critical" means that these are not the superstrings that are used to model elementary particles. Here we're talking about lower dimensional theories that are important for theoretical reasons. For example, Polyakov has done a lot of work on Liouville theory, a two-dimensional conformal field theory theory which is important in a variety of toy models of quantum gravity. It makes no sense to ask whether such a theory has been empirically tested.

In the context of gauge/string duality, the strings just provide a mathematical reformulation of gauge theory. We know that the particles of the standard model are well described by gauge theory, so these strings are definitely relevant for describing nature. It makes no sense to ask whether they have been empirically observed.

52. **Peter Shor**
November 10, 2013

At some point soon, they will run out of worthy candidates that have any connection whatsoever with Princeton. I don't think they're anywhere close to running out of string theorists, though.

53. **tristes_tigres**
November 10, 2013

I wonder, does the origin of prize money bother any of the recipients? Perhaps, that explains the size of the prize. Harder to refuse on ethical grounds if the sum is irregularly large.
NYT reported that Lawrence Summers was forced out of the Harvard presidency in some part due to the unease of the faculty over his role in Russian "economic reforms" of the 90s. So there's a real possibility of some recipient refusing to play supporting role in Mr.Milner's recasting himself as respectable philanthropist. Not any of string theorists so far, though. No impractical Grisha Perelman-like characters there.

54. **venp**
November 10, 2013

Re "Project Einstein": Genome-wide association studies are common these days in studying possible genetic causes of medical conditions. You take, say, 400 persons with type 2 diabetes and compare their genome with that of 400 healthy individuals and see if a gene mutation exist in the diabetics but not the others. I suppose the next step in the "Project Einstein" is to send 400 letters to some of us non-geniouses for the second sample. I certainly hope I don't get one of those:)

55. **Armin Nikkhah Shirazi**
November 10, 2013

Layman,

I guess a big difference between you and I is that if somebody asked me: "Have black hole event horizons ever been empirically observed?" It would have never occurred to me to answer by saying :"Yes, analogues of the black hole event horizon have been empirically observed in microstructured optical fibers.", but evidently this is the kind of answer you are quite comfortable with giving.
Do you see what is wrong with it? I'll help you: It is not an answer to the question asked, but an answer to related but distinct question: "Have relations that characterize black hole event horizons been empirically observed in other systems?"
And so it is with your answers to my first set of questions: As far as I can tell, in all of your answer you presented analogues, usually in condensed matter, as if they were the real thing.
Now, I don't think that you are so uneducated so as to not be able to tell the difference. But if that true, then it is hard to escape the conclusion that you conflated the two questions on purpose, the purpose being to convince people that those theoretical predictions have the same empirical standing as the theoretical predictions of, say, quarks and the CMB.
So, while I do think that your last post is a vast improvement over your previous posts in respect to the quality and tone of your arguments (credit where credit is due) it has not yet changed my impression that you are trying to fit facts to a position at which you have arrived because of an emotional commitment, not because evidence led you to it. I wished you would make a sincere attempt to reflect on what I am saying, but in my experience this kind of appeal usually falls on deaf ears.
The reason I disagree with your assessment of the second set of answers is that it fails to take into account a purely psychological effect that tends to bias us against fully appreciating the most significant achievements in fundamental physics.
When children first learn to read, they read letter by letter, but as they become proficient, the individual letters "disappear", so to say. They no longer "see" the individual letters, only the words. This is universal phenomenon that happens with anything that serves as a building block for higher level concepts, and in particular with our most fundamental scientific concepts. But just because we no longer "see" them, it doesn't mean that they don't "dominate" our thinking (to use the FPP citation word in the last sentence) in every aspect of research in the relevant field. We just fail to appreciate it on an ongoing basis because this is a psychological shortcoming of us humans, and I think your use of the term "pre-history" reflects just such a psychological bias.
Also I believe this has quite a paradoxical consequence: If, say, the theory of quarks were *less* well established empirically (say, we had only condensed matter analogues available as empirical evidence), then its discoverers would seem *more* eligible to for an FPP because their theory was not yet established enough to fade into the background. To me that is just perverse, we should not in effect penalize scientists precisely because they have made contributions that are far more outstanding than those of the rest of us.

Bob Jones,

Let's see: I wrote a long post in which I raised numerous issues, and you declare "Your post makes no sense." because you object to one issue? Do you really believe this is a reasonable thing to say? I don't think you need hyperbole to raise your objection, especially since actually I think you have a point. That question was originally two questions, one pertaining to quantized strings and one pertaining to superstrings, but because they sounded too similar, I decided to combine them into one question, even though, as you point out quite correctly, they are different kinds of concepts.
That was careless of me, and you are right to criticize me for that. Thank you.

Now concerning your last sentence: "It makes no sense to ask whether they [i.e. quantized strings] have been empirically observed."

If someone asked me, say: "Have gauge potentials ever been empirically observed?" my answer would be "Gauge potentials are not the kind of objects that can be directly observed, however they are associated with other objects (e.g. fields) which can produce observable effects, and such effects have been observed in association with certain gauge potentials." I would *never* say "it makes no sense to ask whether they have been empirically observed" because that would immediately provoke the follow-up question : "Then why do they have anything to do with physics?", and quite rightly so.
Now, I realize that you used this language because you are trying hard to make me look like someone who doesn't know what he's talking about, but it would seem in your

own interest to try to avoid inadvertently contributing to the growing perception that string theorists are detached from physics.

Peter,

I just noticed the allegations raised by “John” in the post above. Is this true? If it is, it is quite simply shocking. Have you blogged about this? If it is true and you did not blog about it, why not? I can’t imagine that you would not have known about it.

Armin

56. **Peter Woit**
November 10, 2013

Armin,

Yes, it’s true. As for blogging about it, that’s exactly what I was doing when I wrote this blog entry, John was just providing more detail…

57. **Bob Jones**
November 10, 2013

Armin Nikkhah Shirazi,

It’s not really clear what you’re willing to accept as answers to your questions. In the AdS/CFT correspondence, the strings capture the physics of strongly coupled gauge theories, so they’re certainly relevant for understanding the physics that we observe in particle accelerators. In this way, Polyakov’s work on string theory has led to plenty of new insights about observable phenomena.

There are other ways in which one might answer your question. For example, a lot of recent work in theoretical physics, now recognized by Milner prizes, studies strings that propagate not in spacetime but in an auxiliary mathematical space called twistor space. It makes no sense to ask whether such strings have been directly observed, but this sort of work has led to important new techniques for computing scattering amplitudes in gauge theory, including techniques that are being used right now at the LHC. I should also point out that in condensed matter physics, there are many applications of two-dimensional conformal field theory, and such a theory can always be thought of as a theory that describes strings propagating in the target manifold.

Note that these are all *formal* applications of string theory. In general, it makes no sense to ask whether a formal mathematical concept has been empirically observed. Thus it is meaningless to ask whether a noncritical string has been observed in an experiment. It’s like asking whether differentiable functions have ever been observed. Of course you can ask more generally whether a formal concept has any empirical manifestation, but it was not at all clear that that’s what you meant by your original question.

58. **An observer**
November 11, 2013

I am sympathetic to some of what you write. However, I don’t know why you entered this unseemly discussion about job offers in the U.S. It is simply true that at least some of the people on the new horizons list, had offers with *tenure* from top places in U.S. (eg Harvard.)

It would be better to phrase your point in a neutral manner, rather than engaging in these personal arguments, when you don’t know what personal circumstances and motivations got people to settle where they are.

59. **Peter Woit**
November 11, 2013

An observer,
I wasn’t making a personal argument about anyone, and the details of what job offers any one of these people has had is irrelevant. My point was just that the fact that none of them ended up in the US is statistically significant evidence for a lack of enthusiasm by US physics departments for hiring in this area. There are plenty of other indications of this (e.g. the rumor-mill), I wouldn’t have thought it was a controversial observation at all.

60. **Marcel van Velzen**
November 11, 2013

Who cares who gets this price. It is Milner’s money and he can do with it what he likes. Because it’s so much money it seems interesting but it really isn’t. It’s just Milner’s way of buying himself into theoretical physics.

61. **vmarko**
November 11, 2013

Marcel van Velzen,

If everyone had the same opinion that you have about the prize, there would be no problem at all.

However, if you win the Milner’s prize, and afterwards that fact helps you land a better position at some university (as happens with Nobel and Fields prize winners), then it is a problem.

The issue with Milner’s prize is not the money, but the implications for the academia that it can bring.

Best, 🙂
Marko

62. **Marcel van Velzen**
November 11, 2013

Marko: “If everyone had the same opinion that you have about the prize, there would be no problem at all.” that is why I expressed my opinion 🙂

63. **Curious Wavefunction**
November 11, 2013

Not only does the Fundamental Physics prize identify fundamental physics with string theory, but it also identifies fundamental physics almost exclusively with theory rather than experiment. I wonder if Robert Millikan or Albert Michelson would have been nominated for this prize had it been around then.

64. **Philip Gibbs**
November 11, 2013

Curious Wavefunction, There was the special FPP given to representatives of LHC, ATLAS and CMS.

65. **GooGoo**
November 11, 2013

I liked McMullen's comment! Well said.

This "Einstein Gene" business reminds me of the letters from "Who's Who of High School Students" explaining they want to include such a brilliant and wonderful person as yourself. By the way, of course you will want to buy the (not inexpensive) volume….

A possibly relevant fact to all this is that I met a relative of Einstein, who certainly possessed the Einstein gene, but evidently this was a gene for niceness rather than the gene for math or physics.

I would hazard to guess that William Shockley would be behind this project, after of course including his own person as one of the donors, but then again he must have passed on to other universes by now.

On to the Millman prize: I can't help but wishing that some billionaire would use their money in a way which would actually benefit the science. It is quite right that although zero teaching is not a good idea for most, too much teaching, especially of the repetitive variety, can kill your research and your love of the subject. Thus, what could really be of help is a shotgun approach of many small grants funding reduced teaching loads, including sabbaticals. Arguably the money would be much better spent in this way than by heaping yet more rewards on the same few names (i.e. the star system). These people already have great positions, and in the best cases are motivated by a real love of science and not by yet more money.

66. **David Nataf**
November 12, 2013

There is an unhealthy obsession on this board with prizes. It reveals how the better solution is simply to do away with all prizes.

67. **Typhoon**
November 12, 2013

All this talk of prizes reminds me of the following aphorism:

"Prizes are like hemorrhoids, sooner or later every a**hole gets one."

68. **Laymammal**
November 12, 2013

The Nobel in Physics has exclusively been given to theorists only after their theory has been confirmed by experiment. In the future, the Nobel in Physics may have to alter its criteria to theories which are mathematically consistent both internally and consistent mathematically with confirmed theories like QM. The Genius of Strings is that it's internally consistent and also consistent with almost Every possible theory that may be confirmed.

69. **CFT**
November 12, 2013

Ok, because this 'Project Einstein' is even being considered at all makes me cringe. Why on earth would anyone like Einstein (in particular since he was a Jew) even want his name associated with a eugenics breeding program considering who was actually trying to do such breeding during the first half of the 20th century? Call it for what it is. Eugenics. And for those who need to be reminded what that really is;
*
eu·gen·ics
noun plural but singular in construction \yü-ˈje-niks\
: a science that tries to improve the human race by controlling which people become parents

Full Definition of EUGENICS

: a science that deals with the improvement (as by control of human mating) of hereditary qualities of a race or breed
*
It appears that stupidity and evil dressed up as science never goes out of fashion. I think anyone who wants to connect the name 'Einstein' with such contemptible research objectives should consider having themselves gene sequenced for a 'Fascist' or 'complete idiot' gene, then be surgically neutered in order to gain a first hand empathy and understanding of what they were advocating.
I would also ask anyone with a whit of common sense (rather than what goes for pure mathematical ability apparently) to consider the implications of what would happen if said such 'Einstein' project actually succeed by some dumb luck. What would the product of such a project think (mathematically or otherwise) of the idiots who wanted to control his/her very being, not by loving acceptance, nurture, argument, or education, but by the objectives of animal husbandry? I'd much rather prefer we keep the like of Khan Noonien Singh inside Pandora's little bottle of cautionary fiction, and be extra careful not to spill.

70. **birdie**
November 12, 2013

Eugenics that eliminates intelligent female genes, and selects for arrogant groupthinkers. Yeah, that will work.

71. **Marcus**
November 12, 2013

CFT you just lost it. If some couples or individuals want to try to have extra intelligent or, say, musically talented children that is certainly their right. It is a personal choice and represents individual liberty–in this case the freedom to use available scientific studies and methods to try to improve the life of one's offspring.

By using words like "stupidity" and "evil" and "Fascist" you are in effect using hate speech to limit individuals' personal rights and liberties by harshly disapproving propaganda. Since a government mandate is not the issue the word "eugenics" is irrationally applied–since it has the connotation of state mandate interference with reproductive choice. What you are doing resembles fundamentalists who use rhetoric to stir up hatred against doctors who perform abortion, so that abortion clinics eventually get firebombed, or shut down on technical grounds by legislatures.

So please cool it with the irrational outrage against reproductive choice.

72. **vmarko**
November 12, 2013

Marcus,

"If some couples or individuals want to try to have extra intelligent or, say, musically talented children that is certainly their right."

Assuming that project Einstein is successful in finding a "math gene" (whatever that may be), how would your couple of individuals go about making their child extra intelligent? The parents either do or do not have the gene, they have no choice in the matter. And if they manipulate their genetic material to introduce the math gene artificially, that's very close (if not equivalent) to eugenics.

Either way, the "math gene" most probably doesn't exist. There is overwhelming evidence that capacity for doing math is not hereditary, or otherwise we would already have several Fields medalists or Nobel winners from the same family by now (Milner prize notwithstanding)… 😉

Best, 🙂
Marko

73. **Marcus**
November 12, 2013

Hi vmarko,
I have no interest in arguing about whether math ability is heritable. To me it seems even a bit off topic. I just couldn't let CFT's over-the-top hate speech about reproductive choice pass.

For you, apparently, the word "eugenics" has no connotation of state-control. According to your example "eugenics" can be a matter of individual couple's choice when they are getting ready to have a baby.
That kind of eugenics is increasingly common these days where heritable factors have been identified, and I think of it as morally neutral. As long as you are clear about what you mean I have no problem with what you say. Just don't foam at the mouth about it, OK? :^D

74. **Anonyrat**
November 12, 2013

You can read the first college-level textbook on eugenics – "Applied Eugenics", by Paul Popenoe and Roswell Hill Johnson published in New York, 1918, here:

https://archive.org/details/appliedeugenics19560gut

Left as an exercise to the reader: trace the effects of those ideas on modern US politics.

75. **Yatima**
November 12, 2013

CFT says:

Ok, because this 'Project Einstein' is even being considered at all makes me cringe. Why on earth would anyone like Einstein (in particular since he was a Jew) even want his name associated with a eugenics breeding program considering who was actually trying to do such breeding during the first half of the 20th century?

Please do not open this particular can of worms as there are very, very disturbing ideas being bandied about and actually actively acted upon by the "state" of a certain Middle Eastern country at the present time, which drives me to despair in the human race. Though it confirms that people are mainly driven by self-aggrandizing aggressive myth, not history or introspection. Einstein must be spinning so fast he is probably dragging the frames around his grave.

76. **Peter Woit**
November 12, 2013

Enough about eugenics and related hot-button political issues, although I realize that the "Einstein Project" does bring them up. Unfortunately there doesn't seem to be much news in math or physics, but I'll try and find something to change the subject…

77. **chiz**
November 12, 2013

I'm puzzled as to what a "math gene" would be. There is evidence, from neurology and linguistics, that our brains are wired for number, and that small numbers are treated differently from large numbers, so there is clearly something genetic going on with regard to a number-sense. But that's not the same thing as a math gene. I can hardly be the only person who, on having told friends and family that I was doing a degree in maths, was then faced with friends and family assuming that I was good at arithmetic and would, say, be the ideal person to be treasurer of a local club. Do you really need a number-sense to do muck around with group theory or Fourier transforms? My best guess is that you need a capacity for abstract, or, even, very abstract thought.

In any case, even if there is a math gene, it doesn't automatically follow that it will show up in genome sequencing or that it is heritable. Genetics and heritability have become decoupled over the last decade.

78. **Marcus**
November 12, 2013

Peter:
"Enough about eugenics and related hot-button political issues, although I realize that the "Einstein Project" does bring them up. Unfortunately there doesn't seem to be much news in math or physics, but I'll try and find something to change the subject…"

A propos changing the subject, have you thought any about the paper just posted by Roberto Percacci, Astrid Eichhorn and Pietro Donà?
"Matter matters in asymptotically safe quantum gravity".
Evidence supporting that approach to quantum gravity is bad news for SUSY and extra dimensions, it turns out. Here's the abstract:
"We investigate the compatibility of minimally coupled scalar, fermion and gauge fields with asymptotically safe quantum gravity, using nonperturbative functional Renormalization Group methods. We study d = 4, 5 and 6 dimensions and within certain approximations find that for a given number of gauge fields there is a maximal number of scalar and fermion degrees of freedom compatible with an interacting fixed point at positive Newton coupling. The bounds impose severe constraints on grand

unification with fundamental Higgs scalars. Supersymmetry and universal extra dimensions are also generally disfavored. The standard model and its extensions accommodating right-handed neutrinos, the axion and dark-matter models with a single scalar are compatible with a fixed point."

79. **Philip Gibbs**
November 13, 2013

Describing a project to look for genes related to intelligence as eugenics is like calling an experimental test for relativity an act of nuclear war. I think Einstein would have appreciated the difference.

80. **Bernhard**
November 13, 2013

Here:

http://www.theguardian.com/science/2013/nov/12/stephen-hawking-physics-higgs-boson-particle

Not a "extra, extra" news, but perhaps something to change the subject.

81. **CFT**
November 13, 2013

@Yatima,
I did not intend to open a can of worms, or offend actually, but I did wish to call it for what it was. I am also fully aware my existence is only possible because someone was not given license to tinker with my DNA to suit their preferences of who I should be, flaws, quirks and all.
@Peter, Please let me respond to Marcus, I don't mind him taking his shots as long as I can respond in kind to his labeling my arguments with 'over the top hate speech'.
@Marcus,
Deciding to have a child and exposing them to music or mathematics in hopes they will gain an appreciation for such subjects is one thing; The child will be making a choice (exerting their liberty, and their freedom if you will) if they decide to pursue such a discipline. Deciding you are going to exert absolute control over another person's gender, sexual predisposition, politics, height, intelligence, and abstract intellectual pursuits and hobbies is quite another thing… with horrifying implications. Considering who has been deemed unworthy of existence over the years in the pursuit of said "improvement", I find your use of the words 'liberty' and 'freedom' quite ironic and disturbing, since you are advocating the right to basically predetermine a potential child's nature (or soul) to your 'choice' and not allow them the right to determine their own preferences or desires. I'm also quite certain many world governments would also love to make use of such a capability to make certain some of their own additions were added to your list of improvements in the name of 'the greater good' and benefit of society.
*
Please take your seriously misplaced 'hate speech' comment and file it where the solar photons don't directly interact. Disagreement with a person or concept is not 'hate speech', a 'thought crime' or whatever academically approved politically correct jargon you want to use to silence discussion or debate. If you haven't, try to read '1984' before you pull any more newspeak out of your hat and see if it's really the way you want to go, and possibly go watch the movie "Man of Steel" for entertainment, noting the plotline entirely revolves around a child conceived without a genetically predetermined disposition or societal function. I found it remarkable that the super intelligent parents of the 'superman' figured out that their own species relentless desire to control every last aspect of intelligent life in the name of scientific perfection and social harmony was the cause of their civilization's stagnation and downfall. Krypton had given up on the uncertainty of teaching and learning in favor of the predictability of genetic programming. People don't learn and grow very much when they are given little or no choice in what their preferences are and what they do, so Mr. and Mrs. El decided to break with their world's engineered fate and let their son choose his own destiny, for himself to decide what kind of a man he was going to be. By doing so, Lara and Jor El affirm a concept of that so many people seem to have forgotten. The right to choose, freedom and liberty, did not begin with or end with the parent, nor are they the sole property of one generation to lord over the next, they are the birthright of the child as well.

82. **Peter Woit**
November 13, 2013

Really, enough. Part of the problem here is that the "Einstein Project" doesn't seem interested in explaining what they will do if they find the "math genius gene"…

83. **David Nataf**
November 13, 2013

Peter, I am guessing that the long term goal would ge to use it as a guide in undergraduate and graduate admissions where it would probably be more accurate than GRE scores.

Also to select which nine year olds can go to math camp.

84. **paddy**
November 13, 2013

Peter,
For two days now I have been thinking this chain is indeed "back to the usual".

85. **Kimmo Rouvari**
November 13, 2013

@Peter Have you made any (toy) models of your own? Certainly that would qualify as a great new post and topic. It would be extremely educating to hear what kinds of thoughts and ideas contemporary professionals have in order to create TOE, other than ST.

That's just my 2 cents.

86. **Hendrik**
November 14, 2013

Peter, a good topic for a post would be the ResearchGate website. It seems to be rivalling the ArXiv.

87. **Mathematician**
November 14, 2013

Hendrik, you cannot possibly be serious. ResearchGate keeps sending me fraudulent emails purporting to be from my co-authors, trying to trick (phish) me into "confirming" the details of papers. With these kinds of scammy tactics, I wouldn't touch that website with a stick.

88. **Hendrik**

November 15, 2013

Dear Mathematician, well you should Google it first before you make up your mind. Almost everyone in my subfield of mathematical physics belong to it (have profiles on it), and many very well-known mathematicians are on it.

89. **Mathematician**
November 15, 2013

Hendrik, I don't believe you. ResearchGate is very obviously a scam outfit.

When ResearchGate founder Ijad Madisch says things like "I want ResearchGate to win the Nobel Prize", nobody should be surprised by ResearchGate's sociopathic practices.

90. **Oldster**
November 16, 2013

Mathematician: " ResearchGate keeps sending me fraudulent emails purporting to be from my co-authors, trying to trick (phish) me into "confirming" the details of papers."

I've received many requests from ResearchGate to "confirm" papers as my work, but in every case, when I look at the details, I find one of the authors on these papers shares my last name and first initial, even though it's not my work. I simply click on the button that tells them it's not my work. It seems to me that ResearchGate is making a very innocent mistake here, using some computer program to scan for my name on papers, and not filtering the results as carefully as a human searcher might. That's not a "scam", it's just a somewhat simple scanning program …

91. **Mathematician**
November 16, 2013

Oldster, your experience is not relevant to the discussion because you know you are not an author of the quoted paper, so you won't fall for the scam. That's just like when you get an email about suspicious activity in your account at PQR bank, when you know you have never even had an account at PQR bank. But other recipients who do have accounts at PQR bank may fall for a scam. Your comment does confirm though that the emails seem to be automatically generated. It also shows people are too apt to attribute innocent motives to nefarious conduct.

My experience, which is typical, is this. For i=1,…,n I have received an email purporting to be from my coauthor ABC_i to paper XYZ_i. Now ABC_i is indeed my coauthor on paper XYZ_i, as ResearchGate has factually correctly "scraped" from some source. But if I am to take this email at face value then my coauthor (and colleague/friend/etc.) endorses and is involved in ResearchGate and has made a conscious choice to reach out to me personally, via this email, to suggest I become involved too. In reality, ABC_i knows nothing of this email, and may never have even heard of ResearchGate. Despite the fact that the email clearly states that it is from my coauthor ABC_i, it is in fact sent by ResearchGate in a deliberately fraudulent attempt to deceive me about the true nature of the email, and to trick me into becoming involved based on the apparent endorsement of ResearchGate by my coauthor ABC_i to paper XYZ_i (for i=1,…,n).

There is no doubt in my mind that ResearchGate has intentionally set out to deceive the academic community. They have even used their claims of large numbers of participants (who've been tricked into joining, if the numbers are real at all) to attract millions of dollars in investment. I think the FBI or SEC should investigate these guys.

Academia should avoid ResearchGate like the plague.

92. **Oldster**
November 16, 2013

Mathematician:

I agree I haven't received such emails, even where I have a paper with a coauthor which ResearchGate has managed to locate, and query me about. I have received emails stating that other researchers from my institution ("Retired") have joined ResearchGate, and asking if I wouldn't like to talk to them. Since "Retired" is a very broad "institution", the emails are a little ridiculous, but hardly malicious. Everything I've seen so far seems to fit into such categories. I have used ResearchGate to message authors for a copy of their paper, and then forgotten about it when I heard nothing back for a long time.

Unless I receive something like you describe, I can't really comment on your experience, except to say nothing like that has happened to me. I suggest you complain to ResearchGate directly if you feel they're abusing the site. If that happens to me, I will do that, because I've found the site is otherwise useful for legitimate contacts from time to time …

93. **tulpoeid**
November 22, 2013

"If Project Einstein identifies a common gene among its participants, and uses the knowledge to breed a race of übermenschen, they may find they have selected not for unusual mathematical genius, but for unusual ego."

Kudos. I wish email sigs were still trendy.

Various Topics

November 15, 2013
Categories: Uncategorized

- Mathematician Sasha Beilinson has a letter to the editor in this month's AMS Notices calling on the AMS to sever all ties with the NSA (right now it manages NSA grants, and runs ads from the NSA in the Notices). Beilinson compares the NSA to the KGB of the former Soviet Union. For discussion of the Beilinson letter, see here.
- Beijing now has a Center for Future High Energy Physics, with Director the ubiquitous Nima Arkani-Hamed. The inaugural conference of the Center will be next month, on Future High Energy Circular Colliders. Nature has an article on the topic this week, Physicists plan to build a bigger LHC, about proposals to build a 100 TeV pp collider, with a possible electron-positron collider Higgs factory using the same tunnel. For the latest on TLEP, the proposal for such a Higgs factory at CERN, see here.
- I was in London a few days too early for this, but this week the Science Museum there celebrated the opening of its exhibition about the LHC with an event featuring Stephen Hawking. The Guardian has a report here. Hawking seems to think the LHC may see evidence for M-theory:

  > "There is still hope that we see the first evidence for M-theory at the LHC particle accelerator in Geneva," said Hawking. "From an M-theory perspective, the collider only probes low energies, but we might be lucky and see a weaker signal of fundamental theory, such as supersymmetry.
  >
  > "I think the discovery of supersymmetric partners for the known particles would revolutionise our understanding of the universe."

  As is often the case in stories like this, the wording about evidence for string/M-theory is rather odd. We're told:

  > As yet there has been no incontrovertible experimental evidence to show that M-theory is correct.

  but "no incontrovertible experimental evidence" is a peculiar way of phrasing "absolutely zero experimental evidence of any kind whatsoever."

- For an interview with Shiraz Minwalla, one of the winners of this year's Milner prizes for young researchers, see here.
- Edward Frenkel's new book, Love and Math, has been getting quite a few good reviews, with the latest from Jim Holt in the New York Review of Books.
- Finally, your best source of fascinating mathematically-related graphics is surely going to be John Baez's new Visual Insight blog.

Update: One more. The Perimeter Institute announced yesterday the funding (half provided by the Krembil Foundation) of two new chairs in theoretical physics. These will be held by two young mathematical physicists: Kevin Costello and Davide Gaiotto. As far as I know, the hiring of Costello away from Northwestern is the first time Perimeter has hired someone with a pure mathematics background. It's good to see them moving in this direction.

Update: More about the new Perimeter chairs here. The article discusses the fact that this is a change of direction towards mathematics:

> The choice is a strategic shift and a gamble for the 12-year-old institute, which is in a global tug-of-war for talent and looking to grow its profile as a centre for high-level thinking on some of the deepest questions in the universe.
>
> Although math is the working language of physics and equations cram every available blackboard at Perimeter, Dr. Costello's hiring, to be announced Saturday, will mark the first time the institute has sought a pure mathematician for its faculty….
>
> "There's something about the situation in physics today which makes it especially important to bring in high-powered mathematics," said Neil Turok, the institute's director.

Comments

1. **jd**
   November 15, 2013

   Concerning prizes, honors, awards, I recommend David Mermin's Reference Frame article, Physics Today 42,1,9(1989). This sickness in science has only gotten worse since then.

2. **Lowell Boggs**
   November 15, 2013

   Sasha Beilinson's opinions are understandable given his history. But sometimes things are just true without our being able to prove it. Despite valiant failed attempts to prove it, 1 + 1 still remains 2. Similarly, the KGB were the bad guys. The NSA are the good guys. It is just true.

   Yes, we should abandon the bad mathematical theory and practice that the NSA inflicted on the world of security algorithms, but no we shouldn't take them all out and shoot them. They were trying to prevent terrorism — the loss of life inflicted on innocents by crazy people.

   Do we see large scale theft of financial records because of their actions? N. Do we see good governments toppled? Do we see our personal foibles broadcast on YouTube by the NSA? No. We see Al Quaida disappearing into the backwater of history? Yes.

   Should we applaud the NSA for perpetrating fraud? No. But should we sit around wringing our hands over their lies and improprieties? No. That chapter of their efforts is over. Snowden saw to that. I'm glad he waited so long to render their efforts ineffective. I hope that the NSA are as effective with their future endeavors as they have recently been in protecting me and mine from airplane bombs.

   The NSA are the good guys. They have been doing an excellent job of preventing unnecessary deaths and the hands of lunatics.

   I might not be able to prove this, but it is true.

   I am not a spammer. My opinions are my own and they are heartfelt.

   (By the way, I really love your blog. I am doing my best to keep my mouth shut most of the time.)

3. **Peter Woit**
   November 16, 2013

   There are many obvious differences between the KGB and the NSA (for one thing, the NSA has technological capabilities the KGB could only dream of…), but please, if you just want to argue that the KGB was bad and the NSA is good (or vice-versa), do this somewhere else. Beilinson is raising a specific issue about the role of the mathematics community in the NSA controversy, and thoughtful discussion of that topic is welcome.

4. **Xu Jia**
November 16, 2013

I really don't think Nima Arkani-Hamed has any vision on how to explore the very high energy. He doesn't even have a correct vision on what would be seen @14TeV. The best plan for the future would be Chinese government approves a TLEP Higgs factory and Europe focuses on the high luminosity. This is the only chance that we could see the HHH channel open before 2030, the presence or absence of which would be a huge hint for modelling of the Higgs sector. However, this can not happen because they won't pay for a ring not bringing a long anticipated Nobel Prize. We should wait for Europe, patiently.

5. **Dave Miller in Sacramento**
November 16, 2013

Peter,

Strangely enough, had my career taken a slightly different twist, I might have played Ed Snowden's role: from the mid-eighties through the mid-nineties I worked, for several years as an employee and then as an independent consultant, for a contractor for the intelligence community.. I hope that, if I had ever run into the sort of illegalities that Snowden exposed, I too would have blown the whistle. Fortunately, I didn't, or I'd now be brushing up on my high-school Russian!

I'm inclined to agree with Beilinson, though of course even if the AMS accepts his suggestion, the result would only be symbolic.

It seems to me that any serious discussion of the issue would have to range far beyond intelligence gathering and the NSA per se: if the US is going to pursue the sort of wide-ranging interventionist foreign policy that we have pursued since 1940, then organizations like the NSA are inevitable, and such organizations are inevitably going to cross the line from time to time.

I doubt you or the AMS wants to get into that sort of debate about long-term US foreign policy!

I see no way in which either the current AMS policy or the reversal suggested by Beilinson can reasonably be considered neutral. Furthermore, the intelligence community has lots of interesting, decently-paid work for mathematicians and theoretical physicists, and there is no way that they will find a shortage of candidates to fill those positions.

From all of this, I conclude that the AMS will simply ignore Beilinson's suggestion.

But perhaps he will stimulate a few bright people to start thinking seriously about the deeper implications of US foreign policy.

Dave

6. **Peter Woit**
November 16, 2013

All,

Sorry, but I'm just going to delete any comments about the NSA issue that aren't about the relevance to the math (or physics) community. I'm not able or willing to moderate discussions of US foreign policy here.

7. **paddy**
November 16, 2013

Peter,
Damn..was just about to dig my dog eared copy of Leben des Galilei out of my stacks.

8. **Jim Akerlund**
November 16, 2013

I just read the n-category article and that article raises the issue of NSA/GCHG and them employing mathematicians. But I have also been reading articles of other countries doing smaller versions of the same thing. So, Beilinson raises the concern over what U.S. mathematicans should do, but I am seeing this as more along the lines of what the worlds mathematicians should do in each others respective countries. Maybe if the U.S. mathematicians figure out a cogent response then the rest of the worlds mathematicians can follow along or do something better.

9. **Roger**
November 16, 2013

If the AMS is going to sever ties with the NSA for undermining privacy, then maybe it also ought to cut ties with Google, Facebook, and a long list of others.

10. **Andrea**
November 18, 2013

Just an amusing coincidence, the Hamilton and Galilei chairs will be held by an Irish-born and an Italian respectively.

11. **Don Murphy**
November 19, 2013

Thank you for recommending Edward Frenkel's book, Love and Math, on this blog a months or so ago. It was uplifting, well written, and very understandable. Even the mathematics presented in the book was clear. It was best book I've read of its type and I felt joy at its conclusion. His description in the beginning of the book of how math is generally taught, "What if at school you had to take an "art class" in which you were only taught how to paint a fence? What if you were never shown the paintings of Leonardo da Vinci and Picasso? Would that make you appreciate art? Would you want to learn more about it? I doubt it. You would probably say something like this: "Learning art at school was a waste of my time. If I ever need to have my fence painted, I'll just hire people to do this for me." Of course, this sounds ridiculous, but this is how math is taught, and so in the eyes of most of us it becomes the equivalent of watching paint dry. While the paintings of the great masters are readily available, the math of the great masters is locked away."
Frenkel, Edward (2013-10-01). Love and Math: The Heart of Hidden Reality (p. 1). Basic Books. Kindle Edition.

12. **Geoff**
November 21, 2013

There was a discussion not too long ago when some people on this blog seemed skeptical that the NSA was able to recruit people at a very young age, never to be heard from again in the academic community. While that may happen on rare occasions, what seems much more likely is involvement thru the Institute for Defense Analysis:

http://cryptome.org/2013-info/09/nsa-ccr/nsa-ccr.htm

Taking a look at where they draw on talent is interesting (anyone from current graduate students, to current as well as emeritus faculty)

http://www.idaccr.org/info.html

There have been high profile examples of this, not the least of which is this guy:

http://en.wikipedia.org/wiki/James_Harris_Simons

And from the computer science side, it appears they were also able to draw on this talent:

http://en.wikipedia.org/wiki/Donald_knuth

With people like that, and the computational resources at Oak Ridge, I'm pretty sure they could break just about anything.

13. **theon**
December 7, 2013

About mathematics at the PI:
Psychologists think that the deep problems of physics will be solved by better psychology.
Philosophers think that the deep problems of physics will be solved by better philosophy.
Mathematicians think that the deep problems of physics will be solved by better mathematics.
But I, said the fool, I am a physicist; I think that the deep problems of physics must be solved by better physics.

Anderson 90th

November 17, 2013
Categories: Uncategorized

Philip Anderson's 90th birthday is coming up next month, and Princeton will host a workshop commemorating the event. Witten and Wilczek will give talks on the Anderson-Higgs mechanism, for which Anderson recently was not awarded a Nobel Prize (for the history of this, more here).

Princeton condensed matter theorist Shivaji Sondhi has an article here about the role of Anderson in the Higgs story, rightly emphasizing "the remarkable intellectual unity of modern physics."

Many have speculated that a reason for Anderson not getting a piece of this year's Nobel Prize was his public opposition to the SSC project back in the 1980s. He was far from the only physicist opposing the project, since there was widespread concern that in the Reagan-era environment of budget-cutting, devoting large sums to an HEP project would mean reduced funding for the rest of physics. Anderson has a letter in the latest APS News about this. For a summary of his concerns about the SSC, see this opinion piece from 1987.

One thing that exacerbated conflict between HEP physicists and others at the time were claims about "spin-offs" from building large accelerators, with some people claiming that HEP physics was responsible for MRI machines. Anderson recalls:

> As I was leaving the committee room behind Steve Weinberg, the particle physicist who had testified for the SSC, one of the senators accosted him and effusively thanked him for his role in the development of MRI, which had been instrumental in treatment of a relative. Since close friends and I had been responsible for most of the basic research underlying MRI's superconductiing magnets, this was a bit of a bitter pill for me to swallow.

For Weinberg's point of view on this, see here, where he writes:

> The claim of elementary-particle physicists to be leading the exploration of the reductionist frontier has at times produced resentment among condensed-matter physicists. (This was not helped by a distinguished particle theorist, who was fond of referring to condensed-matter physics as "squalid state physics".) This resentment surfaced during the debate over the funding of the Superconducting Super Collider (SSC). I remember that Phil Anderson and I testified in the same Senate committee hearing on the issue, he against the SSC and I for it. His testimony was so scrupulously honest that I think it helped the SSC more than it hurt it. What really did hurt was a statement opposing the SSC by a condensed-matter physicist who happened at the time to be the president of the American Physical Society.

In recent years the hot topic in the string theory end of HEP theory has become "AdS/CMT", the attempt to apply AdS/CFT ideas to condensed matter theory models. Anderson at nearly 90 is still dealing with HEP hype, see this from the April issue of Physics Today.

Comments

1. **jd**
   November 17, 2013

   Is it true that AdS/CFT is a conjecture that is not proven?

2. **Bernhard**
   November 17, 2013

   Sure Anderson was not the only, but he was the most high-profile physicist against the project testifying (I would say more than the APS president, as a Nobel-laureate). I find the message on this link you gave from Weinberg (which I didn't know) a bit in contradiction with his book (Dreams of a Final Theory). I would say that to the very least Weinberg misleads the reader to blame Anderson quite heavily for the SSC fiasco there. The problem is that he discusses the SSC in the middle of a chapter about reductionism where Anderson is quoted a few times as example representative of the opposition. Here two related passages from the book:

   "In the middle of the spectrum of antireductionists there is a group that is less disinterested and far more important. They are the scientists who are infuriated to hear it said that their branches of science rest on the deeper laws of elementary particle physics."

   Note: at this point Weinberg was not directly talking about Anderson, but it is clear Anderson was among those who Weinberg had in mind:

   "Chief among the physicists who are unhappy about the pretensions of particle physics is Philip Anderson of Bell Labs and Princeton, a theoretical physicist who has provided many of the most pervasive ideas underlying modern condensed matter physics (the physics of semiconductors and superconductors and such). Anderson testified against the Super Collider project in the same congressional committee hearings at which I testified in 1987. He felt (and I do, too) that research in condensed matter physics is underfunded by the National Science Foundation. He felt (and I do, too) that many graduate students are seduced by the glamour of elementary particle physics, when they could have careers that would be more scientifically satisfying in condensed matter physics and allied fields. But Anderson went on to claim that "… they [the results of particle physics] are in no sense more fundamental than what Alan Turing did in founding the computer science, or what Francis Crick and James Watson did in discovering the secret of life."

3. **Low Math, Meekly Interacting**
   November 17, 2013

   It may be semantics, but Anderson's assertions about how "fundamental" sundry phenomena may be is the only thing I've ever read by him that I've disagreed with on a conceptual basis (the SSC is something else entirely). I love "More is Different" dearly, and I would love to believe in "new laws" hidden in complexity. But I can't see why they should exist. And there is nothing fundamental about the structure of DNA. What may have the appearance of being "fundamental", to life at least, is that it must somehow encode information about itself. An almost offhand comment in Watson & Crick's paper is by far the most interesting thing in it, wherein they speculated about the information content of the sequence of nucleotide bases. This was foreseen by Schrödinger, in his "aperiodic crystals", and "What Is Life" got Watson interested in molecular biology. However, "it from bit" strikes me as far more "fundamental" than the information content of self-organizing systems. Numerical simulations increasingly bear this out.

   I've always wondered if some kind of misguided "envy" was the root cause of all the trouble. If so, it was tragic. I envy the power of physics all the time, but I'd be incredibly foolish to try to lay some kind of claim to such power for myself. Then again, I'm surely no Philip Anderson.

4. **Anonymous**
   November 17, 2013

The most ironic is that now, with the benefit of hindsight, we know that Anderson was right, althought not because of one of the reasons he listed.

Just imagined what if instead of one, we had two extremely expensive particle accelerators with nothing to show but the Higgs?

5. **Mathematician**
November 17, 2013

Anonymous, your comment doesn't make any sense to me at all. Could you please explain.

6. **Peter Woit**
November 18, 2013

Bernhard/LMMI,

I don't think the philosophical argument over what is "fundamental" really had that much to do with the Weinberg/Anderson disagreement over SSC funding. The worry that SSC costs would crowd out other funding for science was widespread. Even mathematicians were worried about this, and I remember having to point out to some of them that the SSC cancellation would just mean less total money for science, not more money available for math research. Until seeing Anderson's letter, I hadn't known about the MRI issue, how galling it was to condensed matter physicists worried about their funding to see HEP physicists getting credit for this.

Mathematician,

I assume "Anonymous" is referring to the SSC and the LHC, but it's interesting to consider where we'd be if the SSC had gone forward. If so, I very much doubt CERN would have built the LHC. They would likely have run LEP much longer, and after an SSC discovery of a 125 GeV Higgs, would possibly have upgraded LEP (along the lines now being discussed of a "LEP3") to reach energies where it could study the Higgs, somewhat of a "Higgs factory".

7. **Bernhard**
November 18, 2013

Peter,

I agree with that. My point was only that Weinberg was between the lines, intentionally or unintentionally, blaming Anderson the SSC failure on that book. The fact that he chose to consistently quote Anderson over and over as someone "unhappy about the pretensions of particle physics" and then saying that Anderson testified against the project (all in the same chapter, it is irrelevant that it happened to be reductionism) and later that the project was cancelled leaves one strongly believing it was his fault. If that was not Weinberg's intention he should have clarified, the way he he did on the link you provided. Furthermore, the fact that the project was cancelled by the House of Representatives and that Anderson testified only for the Senate (which I also didn't know: note that Weinberg's expression is "congressional committee") is not a detail that he cold have left out. How many read that book and thought Anderson played a larger than he actually did on SSC's cancellation I don't know, but I include myself among the ones fooled.

8. **Mathematician**
November 18, 2013

PW, right, if SSC is built, then CERN would do something different.

But what puzzles me is Anonymous's phrase "with nothing to show but the Higgs". Surely scientists want to ultimately find out what's true. The accelerators will find what's there to be found (in their energy range) and it's up to nature what's there to be found, whether it's one particle or none or many. If it's a surprise to some theorists then so be it. At least now they know. But it would have been better if SSC had been been built so that 15(?) years ago they would have known that the Higgs particle was there and other hypothesized particles were not there. A lot of theorists could have saved many years chasing false leads in the absence of experimental data. So having "nothing to show but the Higgs" is just fine.

9. **Art Brown**
November 18, 2013

Professor Woit, Professor Anderson's argument against SSC assumed a fixed science budget, but I infer from your comment above that the SSC would have been funded entirely with new money. 25 years on, is there any evidence either way?

10. **Low Math, Meekly Interacting**
November 18, 2013

I agree that the argument of who got what slice of the pie was at the root of opposition to the SSC. But if characterizing HEP as no more "fundamental" than other branches of science was not meant to reinforce the notion that it was no more deserving than other branches, what was the point of mentioning it?

11. **venp**
November 18, 2013

Bernhard,
"How many read that book and thought Anderson played a larger than he actually did on SSC's cancellation "
I read the book and I lived through it, there is no mistake the Prof. Anderson was
the most prominent opponent of the SSC regardless of which congressional committee he testified in. And the most vociferous, I would add, with the possible exception of Rostum Roy.

12. **Bernhard**
November 18, 2013

venp,

Thanks, that is more in accordance with what I've got from Weinberg at the time.

In any case, if what you are saying is true, which is what I thought it was true all this years, than Anderson is not taking responsibility. But I don´t think the distinction between the congressional committee he testified publicly and the one who actually cancelled the project not being the same is a detail.

13. **Peter Woit**
November 18, 2013

Mathematician,

I agree. “Nothing but the Higgs” is a fundamental discovery about how nature works in a new energy range. That some people unreasonably promised potential discoveries of extra dimensions and superpartners that never were going to happen isn’t that relevant, the LHC would have been built without that hype. The SSC cancellation did however set back HEP by decades. In an alternate universe where it was funded, we’d now not only have a Higgs discovery, but probably a Higgs factory running in the LEP tunnel for many years, and a lot of information about both Higgs physics and whatever else might be there in the factor of 5 energy range from the 2012 LHC to the SSC. I think it’s going to be quite a few decades before we get to the point we would be at now if the SSC had been funded.

Bernhard,
I’m not sure the question of Senate vs. House committee makes much difference, and I also don’t think the testimony of either Weinberg or Anderson influenced much the ultimate decision, which had a lot more to do with the politics of where large sums of money were going to get spent. From what I remember of the time, Anderson is right that quite a few physicists and other scientists (I’d suspect possibly even a majority of non-HEP physicists) would have voted against SSC funding if it was up to them, out of a justifiable fear that SSC money was not all “new money”, that it would crowd out funding of other fields. Recall also that the projected cost of the SSC kept growing, and it seemed plausible that cost overruns might cause problems for the rest of science funding.

What I think Anderson doesn’t take into account is that while many of his colleagues agreed with him, he’s always been unusually willing to speak his mind and is a very distinguished figure, so was much more quotable than others. Weinberg is probably right though that opposition from a single out-spoken figure like Anderson was less damaging than that from the president of the APS, who could be seen as representing a much wider opinion.

14. **piscator**
November 18, 2013

I don’t know exactly what happened then, but Nobel prize winners have more influence than just what they say in front of a committee. There are private soundings and opinion-forming which also play a big role in decisions.

And regarding MRI, I think completely anecdotal and uncheckable evidence from someone with such a personal stake in this should be treated with the weight of a Goldstone boson…or maybe am I meant to call it an Anderson-Goldstone boson now?

15. **Peter Woit**
November 18, 2013

piscator,
I think you’re right that private lobbying was likely more influential than public testimony. From what I know of Anderson though, he was likely saying exactly the same things privately and publicly.
The MRI issue is not something Anderson just came up with, it was a major issue in the debate at the time, see for example “Can Big Science Claim Credit for MRI?” in Science magazine here
http://www.jstor.org/stable/2879150
which tells about the arguments over this exact issue.

16. **TimG**
November 18, 2013

Bernhard wrote:
“Sure Anderson was not the only, but he was the most high-profile physicist against the project testifying (I would say more than the APS president, as a Nobel-laureate).”

If the APS President in question is Nicolaas Bloembergen, then he was also a Nobel laureate.

17. **Casey Leedom**
November 19, 2013

My memory of what was reported in the papers and other news sources at the time is that one of the biggest issues with the SSC was that the government was either going to build it or the International Space Station, but not both. The SSC represented money that was going to be spent almost entirely within one state, Texas, while the backers of the ISS were able to lobby Congressional Members of many states by farming out pieces of the ISS all over the US.

18. **Bernhard**
November 19, 2013

Peter,

“I’m not sure the question of Senate vs. House committee makes much difference”.

OK, I’m not an American and my idea of how this all works is perhaps not so accurate. Is curious though that Anderson mention it as the first argument in his defense.

TimG,

Thanks. I always assumed the Senate hearings happened in 1992, but I went to check and indeed they took place in 1991.

19. **Low Math, Meekly Interacting**
November 19, 2013

Interesting footnote here:

http://econ.williams.edu/files/www.jstor_.org_stable_pdfplus_27757780.pdf

–
“Nicolaas Bloembergen testified in 1991 that neither superconducting magnets, the superconducting magnet industry, nor magnetic resonance imaging had come primarily from the development of accelerators, adding in a follow-up letter to an official at Fermilab that was entered into evidence in a Congressional hearing in the spring of 1992 that “MRI woud be alive and well today even if Fermilab had never existed.” To Anderson, “the saddest sight of all is to see officials of the Department responsible for our energy supply deliberately misleading Congress and the public with these false claims, and to see my particle physics colleagues, many of whom I admire and respect, sitting by and acquiescing in such claims.”
–
Incredibly saddening little turf war all around, that. And all quite aside from the the true worth of the SSC. Incredibly saddening.

20. **Peter Shor**
November 19, 2013

It seems to me that Anderson has been made something of a scapegoat, and that the blame for the cancellation rests at least as much with the HEP community, first for the original cost estimate for the SSC being less than half its true cost, and second for subsequently being incompetent at politics.

It's certainly true that Anderson's opposition didn't help, but blaming him rather than trying to figure out what the HEP community should have done better isn't going to help get better results in the future.

21. **Low Math, Meekly Interacting**
November 19, 2013

I've believed for a long time that Anderson et al.'s testimony served to put a fig leaf of legitimacy on something Congress wished ardently to accomplish anyhow to score political points. That said, the focus and tenor of the whole debate on what branch of physics should get funded how much and why was, to me, a tragic development.

22. **Eli Rabett**
November 19, 2013

Up until the SSC, the HEP community had been remarkably successful in finding new money for its projects, indeed the large sums that they generated had something of a trickle down effect on other fields, so everyone left them alone. At the time of the SSC it was obvious that the game had changed, if for no other reason than the huge cost, but also the limits on R&D funding due to the federal budget squeeze at the time. The SSC was going to negatively affect all other science funding, thus the us against them.

23. **arp**
November 19, 2013

Given how difficult it was to get the LHC up and running with access to more modern technology, is it reasonable to believe that the SSC could have been completed on schedule, on budget, and at its promised specs?

24. **paddy**
November 19, 2013

1) arp says above what I always wondered.
2) these were theory folk arguing for an instrument..not just the importance of what could come of the result but that it would work in anything close to the time frame and budget that was proposed.
3) and why do you all ignore that Weinberg was bought and payed for?

25. **GoletaBeach**
November 20, 2013

Reading the Science article on MRI was just depressing. A deputy secretary of energy (not an actually particle physicist) makes an inflated claim. A nobel laureate (Bloembergen) and Robert Park pounce on him and attack all particle physicists. Jeez.

Felix Bloch & Edward Purcell (Purcell is a particular hero to many experimentalists, particle & otherwise) got the Nobel Prize for what we now call MRI, not Bloembergen or Anderson. Indignation by B & A is just misplaced.

The development of MRI into a useful reliable research tool was the work of many, many folks, and without doubt a few strands of the quilt come from experimental particle physics. Denying that is as ignorant as saying the whole MRI field comes from particle physics.

Meanwhile, anyone noticed the proliferation of light sources in the world, and how `small science' groups flock to them? Don't ever hear from the SSC-opposers anything about how important particle physics was to the development of light sources, or even the simple fact that many of the light sources are on the grounds of former particle physics labs, like SLAC. Certainly there are no particle physics labs that are operating in former experimental condensed matter facilities!

BTW, I met face-to-face with Anderson and asked him one-on-one about his opposition to the SSC. He said he couldn't stand the SSC because of all the `spear chuckers' (direct quote) who supported it. His opposition was visceral and the scientific overlay was merely window dressing.

26. **Kavanna**
November 20, 2013

The problem wasn't Reagan-era budget cutting, which happened in the early 80s, before the SSC was proposed. The problem was the dawning post-Cold War era, when HEP had an uncertain future. In the early 90s, there was a strong push to reduce federal deficits — the era of Perot and "read my lips." The SSC had enough political cover under Bush Sr. to keep going, being based primarily in Texas. When Clinton replaced Bush in 1993, the SSC lost that cover. (The SSC was canceled in the fall of 1993.) It became the political equivalent of a large, lumbering beast on the Serengeti, spotted by packs of smaller but swifter predators.

The obvious strategy, barely considered, was internationalizing the project, to include foreign scientific efforts, especially the Japanese. (The Japanese were riding high at that point, at the end of 30 years of rapid growth. Our trade and federal deficits were mainly their surpluses.) But that was all seen too late. The cancellation of the SSC was a disaster for HEP in many ways, not least in the way that it created a vacuum later filled with hot, exponentially expanding string/M-theory hype.

27. **Eli Rabett**
November 21, 2013

Having built Pound boxes, let Eli tell you that knowing the physics of NMR was not the problem but making superconducting magnets was as is clear to anyone who ever did NMR with an iron core monster. Bloch and Purcell were the first observers, but MRI depended on the availability of supercons, which is what Bloembergen and Anderson were talking about. Oh yeah, and among other things, having the Cooley Tukey FFT algorithm (also Bell Labs)

To climb down further into the weeds, the SSC got caught up in Gramm Rudmann Hollings and the Budget Enforcement Act of 1990 which set caps on spending, including a cap for scientific research. It is that in particular which set the condensed matter, and HEP physicists against each other.

28. **cthulhu**
November 24, 2013

(coming in late…)

Kavanna has it exactly correct – the demise of the SSC was Texas-bashing politics as a thin excuse to "cut the fat out of the budget." I was working in north Texas aerospace at the time; when several co-workers left for the SSC to do stuff ranging from mechanical design to IT, it seemed likely to me that they would be back in a few years – unfortunately I was right.

29. **chiz**
November 26, 2013

PW: *In an alternate universe where it was funded, we'd now not only have a Higgs discovery, but probably a Higgs factory running in the LEP tunnel for many years, and a lot of information about both Higgs physics and whatever else might be there in the factor of 5 energy range from the 2012 LHC to the SSC.*

Of course, its also possible that in this alternate universe with a SSC that we might have discovered that the 125 Gev particle isn't a Higgs but is instead something that the theorists never predicted.

30. **I_Was_There**
November 29, 2013

Just a few hopefully clarifying points:
1. The SSC vs International Space Station funding that year in Congress is telling. The sometimes emotional opposition against the SSC in the House and Senate was bipartisan just as was the support. Opponents in the House were led by members such as Jim Slattery (D-KS), Sherwood Boehlert (R-NY) and Dana Rohrabacher (R-CA). A day after the vote finally killing the SSC, the ISS funding for that year was approved by the House by (if memory serves) one or two votes with Boehlert and Slattery voting to continue the ISS. It is well known that Vice President Gore spent real time lobbying for the ISS and he opposed the SSC. (Slattery is the disingenuous person who went on ABC's Primetime Live to publicly accuse the SSC team of misusing and abusing federal funds based on spending a few bucks to buy a few fake plants for a few offices in a largely windowless old warehouse rented to .. save money; Slattery also cited as an example this misuse of funds by physicists who deemed themselves privileged because a few times during all-day meetings, the lunch was "catered" which turned out to be ordering a few cold cut sandwich plates from a local supermarket so the meeting participants could work through lunch! Boy, did Sam Donaldson leap all over that to spew smoke where there was no fire.)

2. Dr. Anderson's opposing influence was indeed heavily felt from his private comments as well as his public comments. During that time, I spoke directly to a staffer in Sen. Ted Kennedy's office who cited Anderson's views as a primary factor leading Sen. Kennedy to oppose the SSC.

3. While the concern about science funding for other well-deserving research areas/projects seemed real, the fact is that the SSC funding had nothing tangible to do with condensed matter research funding, and terminating the SSC funding that year had *zero* effect on non-HEP funding or reducing the deficit. The money was simply re-programmed to other "water projects" that were within the same purview of the House appropriations committee dealing with the SSC.

4. The comments about the MRI and superconducting magnets are missing a key point. No doubt the basic research is being cited more or less correctly, and there is no doubt that MRI happens with or without HEP and the big accelerator labs. However, especially after BNL and Isabelle, a key contribution made by experimental physicists and engineers was actually making good superconducting magnets before anyone else did, and the need for all that superconducting cable for the Tevatron absolutely did accelerate the development of more cost effective cable fabricated commercially.

Those are facts. I could list more, but any revisionism that Dr. Anderson did not play a prominent role in being one of several important scientists to influence the opposition to the SSC is simply that — self-serving revisionism when it's now pretty obvious that the SSC would have succeeded probably at least a decade before the LHC.

31. **jd**
November 30, 2013

I_Was_There completely misses the point on the savings from the cancellation of the SSC and I suspect he knows it. At the cancellation two billion had already been spent and the tunnel was a quarter finished. The project was heading for at least a factor of two overrun that would have had to come from somewhere. That overrun was saved. As to better superconducting cable, well whoopie and I thank you, but the MRI machines would still have been built. At the moment the MRI technology is heading toward new wire technology that does not relate to that used in HEP. Maybe you guys can lower the cost of your next big machines from using that. Maybe not. But I am a little sick of all this hype on the benefits of the spinoffs. You cannot justify the building of the big machine from their spinoffs. If the spinoffs are what is important, fine. Do all the research for the next big machine and build prototypes of all the technology but do not do any construction. We then get all the spinoffs and save the majority of the cost.

32. **I_Was_There**
December 1, 2013

JD has turned around my comments, so I feel forced to reply that:
1. I never justified the construction or cost of the SSC based on the spinoff of superconducting cable. There is not one word in my comment to that effect. I explicitly stated that "there is no doubt that MRI happens with or without HEP and the big accelerator labs. " I was only clarifying the role that HEP had at that time in developing superconducting magnets and how that progressed the commercial developments toward dropping the cost of the cable at a faster rate. That was only in response to a couple of the earlier comments. That's a fact whether JD likes it or not.

2. There is also no doubt the final cost was going to be 2x or more the initial cost. My guess at the time was 2.5-3x the initial $5-6B estimate. Compare that with other big projects in defense and non-defense, and especially with the ISS. I did not miss any point about the "savings" of cancellation, including about the federal budget in the ensuing years.

3. Finally, the original topic here was the effect that Dr. Anderson had as a publicly prominent scientist opposing the SSC, and my comments were meant to respond to that topic. Perhaps I should not have included a couple of the contextual comments regarding the overall tone and politics of the debate. JD is of course entitled to his opinion, too.

33. **Peter Woit**
December 1, 2013

I Was There,

Thanks for the comments, which were interesting and helpful.

About Anderson, there's no question he was a prominent opponent of the SSC, and had some effect, although one can argue about the size of this effect, and what would have happened in a counterfactual world without him (personally, I think the SSC would still have been canceled, with the growing cost a huge factor). It seems he's mainly annoyed to be portrayed as the only such opponent of the SSC, when the fact was that there were significant others.

34. **Eli Rabett**
December 2, 2013

Just to point out that ISS had a huge Texas footprint through Johnson. What NASA did do was give a piece of meat to a whole bunch of states, but Texas had the prime.

Progress on Twin Primes

November 20, 2013
Categories: Uncategorized

There's a new paper out on the arXiv last night, Small gaps between primes, by James Maynard, which brings the bound on the size of gaps between primes down to 600. This uses some new methods, beating out the Polymath8 project, which has been improving Zhang's original bound of 70,000,000, getting it down to 4680.

To follow the Polymath8 project, the place to look is Terence Tao's blog, here. They're working on a paper, with the current draft version available here. This is a remarkable collaborative project bringing together a sizable group of mathematicians in an unusual way.

For more about this, see this expository article by Andrew Granville, which is pre-Maynard. At Quanta magazine, Erica Klarreich has an excellent long popular article telling the story to date, including that of Maynard's new result.

Comments

1. **kdl**
   November 20, 2013

   Interesting to know if Zhang didn't publish his twin primes work back in May, Maynard's paper would still show as it is now 6 months later, would it be an even BIGGER twin primes breakthrough as well just by now?

2. **Nick M.**
   November 21, 2013

   Thanks, Peter, for the update. I've had a number of things going on in my life at this time, and I haven't had the opportunity to stay abreast of the latest developments in the twin prime conjecture. Even though number theory isn't my primary interest in mathematics, every mathematician has a soft spot in his/her heart for what Carl Friedrich Gauss described as the "Queen of Mathematics".

3. **Pete**
   November 21, 2013

   Maybe more to the point, Maynard can not only show a better bound on lim inf gaps between one prime and the next (the next big excitement will be when that gets to single digits, we're still over 100, and in some sense that's not so much nicer than Zhang's 70,000,000) but he can show that there is a finite bound on lim inf gaps over any given m primes – this wasn't thought to be on the table. Roughly, we knew bounded gaps between primes conditional on a (still) unproved `good behaviour' conjecture (Elliott-Halberstam); Zhang's theorem is powered by Zhang's observation that you don't need the full strength of that conjecture, and you can actually prove (with a lot of work) what you do need. But even assuming the full E-H conjecture we couldn't get much below trivial lim inf gaps containing multiple primes before now (Goldston and Yildirim claimed something like this ten years ago, but there was an error that looked unfixeable: Maynard, roughly speaking, fixed it).

4. **TonyK**
   November 23, 2013

   Andrew Granville has updated his expository article to include Maynard's results. See http://www.dms.umontreal.ca/~andrew/CEBBrochureFinal.pdf.

5. **paddy**
   November 23, 2013

   @TonyK
   Thanks for the link. Makes good reading on a cold stormy afternoon.
   @PW
   Fascinating stuff even for an old experimental physicist.

6. **a**
   November 24, 2013

   On this topic of abstract science like pure mathematics, are you planning to see "Not Even Wrong", an artistic production of dance at Geneva in May 2014? It seems to be based on some ideas in your book:

   http://kylie-walters.com/?portfolio=not-even-wrong-n-e-w
   http://www.youtube.com/watch?v=xMSyHTLFwM0
   http://www.plateaux.ch/en/show/not-even-wrong-new/

7. **Peter Woit**
   November 24, 2013

   a,
   Thanks. That's news to me. But no plan at the moment to be in Europe then.

8. **ohwilleke**
   November 25, 2013

   Does anyone know if there has been any progress in confirming the accuracy of Harald Helfgott's proof of the weak Goldbach's convention as he claimed in a pre-print last May?

   (This would imply that all odd numbers are the sum of not more than three odd primes and that all even numbers are the sum of not more than four odd primes, subject to trivial exceptions under 7.)

   I bring it up now because it was announced the same day as the 70,000,000 apart prime result of Zhang.

9. **Anonyrat**

November 27, 2013

On a lighter note, remember Tom Lehrer’s Lobachevsky song?
http://www.sing365.com/music/lyric.nsf/Lobachevsky-lyrics-Tom-Lehrer/D97B21BF6516390448256A7D0024B8B9

Controversy over Yau-Tian-Donaldson

November 25, 2013
Categories: Uncategorized

The last posting here was about an unusually collaborative effort among mathematicians, whereas this one is about the opposite, an unusually contentious situation surrounding important recent mathematical progress.

What's at issue is the proof of what has become known as the "Yau-Tian-Donaldson" conjecture, which describes when compact Kähler manifolds with positive first Chern class have a Kähler-Einstein metric. This is analogous to the Calabi conjecture, which deals with the case of vanishing first Chern class. Progress by Donaldson on this was first mentioned on this blog here (based on his talk at Atiyah's 80th birthday conference in 2009). Last fall a proof of the conjecture was announced by Chen-Donaldson-Sun, with an independent claim for a proof by Gang Tian, see here. I wrote a bit about this last winter here, after the details appeared of the Chen-Donaldson-Sun proof, and that posting gives some links to expository articles about the subject.

I had heard that there were complaints about Tian's behavior in this story, including claims that he did not have a complete proof of the conjecture and was not acknowledging his use of ideas from Chen-Donaldson-Sun. Recently this controversy has become public, with Chen-Donaldson-Sun deciding to put out a document (linked to from Donaldson's website) that challenges Tian's claims to have an independent proof. The introduction includes:

> Gang Tian has made claims to credit for these results. The purpose of this document is to rebut these claims on the grounds of originality, priority and correctness of the mathematical arguments. We acknowledge Tian's many contributions to this field in the past and, partly for this reason, we have avoided raising our objections publicly over the last 15 months, but it seems now that this is the course we have to take in order to document the facts. In addition, this seems to us the responsible action to take and one we owe to our colleagues, especially those affected by these developments.

I should make it clear I'm no expert on this mathematics, so ill-equipped to judge many of the technical claims being made. The Chen-Donaldson-Sun document is giving one side of a complicated story, so it would be useful to have Tian's side for comparison, but I have no idea if he intends to respond.

On a more positive note, perhaps this controversy will not interfere much with future progress in this area, as Donaldson and Tian are jointly organizing a Spring 2016 workshop on this topic at MSRI.

Update: I hear from Tian that he has recently written a response to the Chen-Donaldson-Sun document, which is available here, and he may at some point write some more about this. Anyone who has read the CDS side of this should also take a look at what Tian has to say in response.

Comments

1. **An.**
   November 25, 2013

   If you carefully read this linked document "On some recent developments in K\"ahler geometry", the accusations sound pretty serious. Also, they did a great job presenting the evidence, which all looks very convincing. Basically, they claim that most key arguments were copied by Tian without any reference after their preprints were posted. From first to second version of Tian's paper on arxiv, several missing or wrong arguments were replaced with the ones almost identical to theirs, and still there are some gaps left (probably because he can not continue copying ALL their arguments). Why would somebody who had such a great career do something to taint it like that?

2. **srp**
   November 25, 2013

   In Hollywood, entities suing one another on one issue often collaborate simultaneously on others. Let's hope similar compartmentalization works for the jointly organized workshop mentioned in the post.

3. **An.**
   November 25, 2013

   Of course, if you look at Tian's let's say top ten most cited papers, it is clear that this was probably one of his favorite pet problems. This might somewhat explain his behavior. Also, wouldn't it be better to call it in the post the Yau conjecture, since this is primarily how CDS call it in their document (they mention that *sometimes* it is called YDT conjecture). Well, at least one good side effect of this controversy is that we can safely assume that there will never be a paper by the trio Sun-Tian-Donaldson.

4. **Peter Woit**
   November 25, 2013

   An.,
   From what I can tell "Yau-Tian-Donaldson conjecture" is pretty standard terminology. I suspect the reason CDS are avoiding it is just Donaldson's adherence to standard academic practice of authors avoiding use of their own name to describe things in their papers.

5. **An.**
   November 25, 2013

   Peter, this doesn't stop Tian from using it, as well as the Cheeger-Colding-Tian, in his paper. I noticed that he also mentions Hermi***tian*** a lot.

6. **Daniel Mathews**
   November 25, 2013

   As mentioned in CDS document, Tian posted his paper in two versions. The first one posted on Nov. 20, 2012 has only 32 pages. The sencond one on Jan. 28, 2013 has 47 pages. What they didn't mention is there is a third version posted quite recently on Oct. 18, 2013 at http://www.bicmr.org/~tian/?page_id=8, which has 61 pages. The fact is, Tian has been correcting and adding more details for his paper in the past year, while CDS has not changed a word in their papers. What can one conclude from this simple fact?

7. **Kent**
   November 25, 2013

One can't conclude anything at all from that simple fact. He could be adding and correcting his papers because they were incorrect or incomplete at first. Or, perhaps his paper was correct and complete at first, but he is making additional progress. But the fact that there are multiple versions of his paper, on its own, proves nothing. It all depends on the details.

8. **theoreticalminimum**
   November 25, 2013

   An.:

   Hermitian is absolutely fine. French mathematicians & physicits use *hermit**ien***. Apparently, this whole hermitian-vs-hermitean pseudo-pedantry is only an issue in some English-speaking milieu, which is rather funny.

9. **Mathematician**
   November 25, 2013

   Tian's reply can be found here:

   http://www.bicmr.org/~gtian/?ddownload=315

10. **An.**
    November 25, 2013

    theoreticalminimum, it was a joke (about Hermitian).

    Tian's response is reasonable overall, but the response to accusations in Section 3.2 avoided the main issue, namely, why did the proof of Lemma 5.8 went from 25 lines in the first version of his paper to 10 pages in the second version, with the proof now identical to CDS (even though Tian claims he never read it).

    On the other hand, after reading Tian's response, I feel that the CDS document focuses on minor details and does not explain what was the BIG NEW IDEA that Tian stole from them. After all, it is hard to imagine that Tian didn't know some minor details about the topic he has been working on for decades.

11. **Phil**
    November 26, 2013

    Maybe there is not a single big new idea. The obstacles have been there and been known to experts for many years. There are many possible ways to solve the problem, and none of them seems to work at first sight. You have to try and try, test and test. The new idea is that some guy Jimmy finally find one of the ways works, and if he told the outline to an expert Tom who had worked on this problem for many years, Tom could fill in most of the details, if not all. But you cannnot say that Tom didn't know this outline. He just didn't know that this outline works.

12. **Curious**
    November 26, 2013

    There is another post written by a third person:
    http://blog.sciencenet.cn/blog-87484-721241.html

13. **Peter Woit**
    November 26, 2013

    Curious,
    I saw the document from Sen Hu you link to a few months ago, decided at the time that it wasn't a good idea to mention it on the blog. This was because it seemed rather one-sided, and it was not clear at all that it was written by someone expert enough in the technicalities and their history.

    The Chen-Donaldson-Sun document did seem worth mentioning, since, while it may be one-sided, it's an authoritative representation of the views of some of the principals in the the story. I'm glad to see that Tian has also given his point of view. Hearing what they have to say directly from both sides involved in this is a lot better than getting a one-sided argument from some third party. So, at this point I don't see anything particularly useful about the Sen Hu document.

14. **Peter Woit**
    November 26, 2013

    I've deleted several comments here, in one case at the request of the original commenter. Please do not use this comment section to engage in attacks on people, especially not taking advantage of anonymity.

    People arguing over priority issues is not very edifying, but at least in this case we're hearing directly from the people involved giving their point of view on these issues.

15. **Grinch**
    November 29, 2013

    It is too bad to see such wonderful mathematicians spatting in public about priority.
    Indeed, we are blessed in many unusual ways in life; it can be regarded as a real blessing to be neither smart nor knowledgeable enough to work in certain areas!

    More seriously, a practical way to avoid this sort of problem is to avoid "problem-solving" altogether in favor of posing new questions which you try to answer at least partially yourself before someone else can jump onto it. But once you do publish or speak publicly about it, you have to let go of it and let your "baby" walk on her own feet! And part of the problem here seems to be Tian's unwillingness to let his beautiful baby go off on her own….

    Another related approach is to try to come up with new theories.

    A third is attitude: to not be so worried if someone else does have "priority", if you do "rediscover" something. This is inevitable in today's world. Try to unhook yourself from the disappointment; if you reinvented it, it means you are on the right track!
    Furthermore, don't be discouraged: inevitably you came up with some new angle, you just have to see what it is! Of course, be generous in your citations. No one ever lost anything by over-citing others. Err in that direction! Upsetting someone is stupid and unnecessary- and unjust. Be bigger than that….

    And most importantly: recognize that
    the process was just as much fun, and having fun with it should be the first and ONLY point of doing mathematics. (However, never, ever admit that truth publicly or to a granting agency-this is a closely guarded secret of mathematics!)

The best mathematicians I have known- coincidentally the ones most universally loved- focused on the beauty of the math itself, and also realized the beauty of the human mind, and human personality, in its many and varied (and often difficult) forms.

There is a caveat to this idealism: we all need a job, we all need grants. But one hopes that is a corollary of good work and good ideas and that it doesn't become the main point. Too much focus on credit, money, power leads to self-destruction and damage to a community one loves, and which one should be working to build. Life is a rich mixture of beauty, ugliness, generosity, selfishness, jealousy, compassion, justice, unfairness. We can choose what to focus on, which tendencies in ourselves and others to encourage. If we get paid to think, to do something we love, we should never forget how lucky we are, compared to the vast majority. And we should also remember that the mathematics itself doesn't care one iota: it will go on being beautiful, despite the flaws of its discoverers.

16. **Noah Smith**
December 1, 2013

Sad to see this kind of priority dispute still cropping up in math.

17. **Mitchell Porter**
December 2, 2013

Noah: how does the better world look and work – it's always clear who was first? no-one cares who was first?

18. **Noah Smith**
December 3, 2013

Well, we seem to have a lot fewer of these disputes in recent decades. Is that an illusion? If it's real, it's probably due to better information dissemination making it clearer who was first. But also probably due to more credit-sharing (i.e. fewer people caring who was first past the post, as long as people did the work independently).

So, both.

19. **Mathematician2**
December 6, 2013

"Grinch", your remarks are exceedingly cruel. The reality is that altruism (in mathematics research and elsewhere) sometimes has very negative consequences for the altruist, and comments like yours serve only to throw salt into the wound. I used to talk openly about my ideas, was generous with co-authorships, etc., but started realizing that I kept getting burnt. In one case, after a talk by a former collaborator, I explained to him (and his collaborator on that talk), how to generalize their result. I was simply being generous. I was shocked when I later got a paper to referee by those two guys, with the result I told them, but not even an acknowledgement that I told them. When I discussed the situation with a senior colleague, they berated and belittled me with the implication, like "Grinch"'s, that it would be contemptible for me to finally stick up for myself for once, and that I should shut up and accept the paper, which is exactly what I did, I was so browbeaten.

I really wish that mathematics research (and other activities) were a utopian, happy cooperative endeavor that people did for the pure joy of it. But it's just not that way. There are too many aggressive ambitious people out there, and if you are the generous altruistic type, you just get walked all over. Personally, the only way I could deal with that type of environment, was to avoid it completely and work in isolation. I would have loved to have been part of a happy cooperative mathematics research community, but I found that in my area, it doesn't exist.

Quantum Mechanics and Representation Theory: talk and book progress

November 27, 2013
Categories: Quantum Theory: The Book

Last week I gave a colloquium talk at the Texas Tech math department, slides are here if you're interested. One motivation for the talk was to advertise the book project I'm working on, which gives a lot more detail about these topics if you find something interesting in the slides.

The current state of the book is visible here. There are 31 chapters done, about another 5 to go. I also need to go through the entire thing again and reconcile various choices of convention that currently are not necessarily consistent. I plan to get back to work on this in a couple weeks after fall classes are over here, have something like a finished draft of a book done around February. The next project will be to get back to what I was writing long ago on Dirac cohomology and make some more progress with that.

For the next few days though, will be taking it easy, and eating turkey. Happy Thanksgiving to all!

Update: The volume of the Feynman Lectures on Physics devoted to quantum mechanics is now available freely online here. This is a masterful introduction to QM from the perspective of a great physicist. What I've been writing in some sense is intended to function best as a supplement to this and an explanation of how it is related to some basic concepts in mathematics.

Comments

1. **Florin Moldoveanu**
   November 27, 2013

   The two slides on cohomology in the talk had no corresponding entries in the book. Where can I find some good references on this?

   Thanks,

   Florin Moldoveanu

2. **Avattoir**
   November 28, 2013

   FM, he gave you a BOOK, for crying out loud — and you knitpick?

3. **Per**
   November 28, 2013

   Hey,

   This book looks really interesting. Thanks for putting the draft online,

   Per

4. **Peter Woit**
   November 28, 2013

   Florin,

   I should have provided this link:
   http://www.math.columbia.edu/~woit/brstdirac.pdf

   As I mentioned, next plan is to get back to work on this…

5. **Cplus**
   November 28, 2013

   The dual of your title, Representation Theory for Physicists, may claim equal validity.

6. **kashyap vasavada**
   November 28, 2013

   I would like to know if your book would clarify foundations of QM or help with the perpetual debate on its interpretations.

7. **Yair**
   November 28, 2013

   I think the use of the Feynman statement as the motivation for the colloquium is inaccurate.

   Feynman was not referring to a lack of understanding of the mathematical tools.
   He was rather referring to issues like the non intuitive nature of QM.

8. **Ray**
   November 28, 2013

   How come none of the equations in the book are numbered? Also you should replace hand drawn figures by figures produced in latex.

9. **Avattoir**
   November 28, 2013

   Book needs a sexed-up title to goose sales: one suggestion, to get the ball rolling about the hat: Where there's a Weyl there's a Woit. (Logic suggests those might be reversed; but this is about marketing, dammit!).

Another – The Big Lie Updated; or – Quantum Mechanics: Lies, Embedded Lies and Mathematics.

10. **Florin Moldoveanu**
November 28, 2013

Thank you Peter, I will study the reference in detail.

11. **Adam**
November 29, 2013

I am currently an undergraduate studying physics, and am taking Quantum Field Theory for the first time. Through this class, I really have gotten a feel for how important representations of lie groups really are. As far as I can tell, however, most undergrad physics majors – unless also advanced math majors – seem to pick it up as they go, which is really odd. Representation theory arguably contains the most important mathematical tools used in modern physics, save for say calculus… Point is, this book will help fill a gaping void in the literature for young students of physics – just some words of encouragement.

12. **Chris Oakley**
November 29, 2013

I agree with Avattoir. How about this for an opening sentence: "For decades the worst kind of populist bullshit has been peddled in quantum mechanics s0-called text books."

13. **Peter Woit**
November 29, 2013

Cplus and Adam,

Thanks, I do hope this is as useful to physicists trying to understand representation theory and some of the mathematics behind quantum theory as it might be to mathematicians trying to understand quantum theory. I should find another title for it, although I'm not going to follow Avattoir's suggestion…

14. **Attempting to be useful**
November 29, 2013

Your books seems to be pretty good. As a math person that took some grad physics courses, however, my recollection is that what I missed most was physical intuition.
So, my suggestion is to add some remarks on the following:
(1) at what scale do you need quantum mechanics to describe motion? (something like what is done in the Feynman lectures in physics)
(2) what is a lot of energy or little energy – what sort of process and behavior you can see at different energy levels?
(3) Which particles exist?

Basically, I think mathematicians tend to focus too much on the math side of any given scientific theory. Hence, they will most likely look at the book as a small subset of representation theory – and as such, they might think that it would be more profitable to read a book on representation theory instead. It is crazy, of course, that many of them would think like that – so, if you give them a concrete grasp that there are some real objects out there, you may end up waking up some sleeping beauties.

15. **Peter Woit**
November 29, 2013

Ray,
The book is still a long ways from a finished product, latex makes it look much more finished than it is. It definitely needs better and more graphics, and probably equation numbering.

kashyap vasavada,
There's nothing really about interpretational issues, although there is somewhat of an argument that QM is really founded on certain kinds of mathematical ideas, so in that sense is making an argument about foundations.

Yair,
Feynman was contrasting QM to classical physics, including the fact that they seem to have radically different formalisms. I'd argue that representation theory does give some "understanding" of the QM formalism, giving a sort of answer to questions like "why are observables self-adjoint operators?", which is the sort of question I think Feynman had in mind as "no one understands" the answer to.

Attempting,

Thanks for the comment. I may try and add more material like you suggest (there will be some in the last chapter on the standard model). Giving some real physical insight though is difficult, I think what I've written would best be used in conjunction with a standard physics text, supplementing but not replacing it.

16. **Chris W.**
November 29, 2013

Loosely related to the question of what Feynman meant: See [this new post] by Steve Hsu.

17. **Emanuel Quant**
November 30, 2013

Peter,
I am struggling with the understanding of the physical relevance of unitary inequivalent representations.
You say:
"In the case of quantum field theory one has an infinity of inequivalent irreducible representations of the commutation relations to consider, one source of the difficulties of the subject."
It seems to me that this issue is not fully settled yet. What is your opinion ?

18. **Peter Peterson**
November 30, 2013

Hi Peter,
I liked your presentation of QM for mathematician – the version from couple of years back. This is a great continuation, as a physicist, I am a bit challenged by a very matter of fact presentation that expects basic mathematical knowledge, that physicists often try to get away without.

Just a question – I was trying to search for word Clifford, just to verify how much in depth you go there (again, a great introduction here, but with expectation of having necessary mathematical background would challenge a number of physicists, and I suspect should be expected basic knowledge for a mathematician). My search did not work that well, because 'ff' in Clifford is using a special (I assume Unicode?) character (I cannot paste it here, edit box will not let me). It is probably intended.
Again, I like your book a very much.

19. **Peter Woit**
November 30, 2013

Emanuel Quant,
I don't think that it's a problem that can be "settled", it's a real aspect of the mathematics of qfts, partly responsible for making the subject much more difficult than quantum mechanics of a finite number of degrees of freedom.

Peter Peterson,
The treatment of Clifford algebras, spinors and their relation to fermionic quantization is one thing that's in the notes, but not so well known (to physicists or mathematicians). I don't know of a really good, easy to follow treatment of these topics elsewhere, so I hope what I've written will be useful.
The "ff" issue is about the handling of "ligatures", I'm not sure what source you are having the searching problem with. For more about this issue, see here
http://english.stackexchange.com/questions/50660/when-should-i-not-use-a-ligature-in-english-typesetting

20. **Art**
November 30, 2013

Title contest? Avattoir sets a high standard, but how about "Giving the Lie to Feynman"? A marketer's dream…

21. **Peter Peterson**
December 1, 2013

Hi Peter,
You likely read the following books:
1. Clifford Algebra to Geometric Calculus A Unified Language for Mathematics and Physics (Fundamental Theories of Physics) By D. Hestenes, G. Sobczyk
http://www.fishpond.com/Books/Clifford-Algebra-to-Geometric-Calculus-D-Hestenes-G-Sobczyk/9789027725615
2. New Foundations for Classical Mechanics (Fundamental Theories of Physics) by David Hestenes http://www.amazon.com/Foundations-Classical-Mechanics-Fundamental-Theories/dp/0792355148
3. Geometric Algebra for Physicists by Chris Doran, Anthony Lasenby. http://www.amazon.com/Geometric-Algebra-Physicists-Chris-Doran/dp/0521715954/ref=sr_1_1?s=books&ie=UTF8&qid=1385925080&sr=1-1&keywords=doran+and+lasenby
4. The Berezin Calculus by Paul Robinson – http://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=1&ved=0CCsQFjAA&url=http%3A%2F%2Fwww.ems-ph.org%2Fjournals%2Fshow_pdf.php%3Fissn%3D0034-5318%26vol%3D35%26iss%3D2%26rank%3D1&ei=noybUtTIE4fxoATvqoDAAg&usg=AFQjCNE9kLrL24OG59Kvzf2ql7WLSjOxbg&bvm=bv.57155469,d.cGU&cad=

There are a lot of shorter articles and studies if you search.
Hestenes started applying and promoting Clifford/Grassmann framework as a consistent approach to all areas of physics a few decades ago. There were multiple attempts to teach physics based on this geometric (Clifford) approach, even though in my opinion, notwithstanding the claims by proponents that geometric algebra approach makes understanding physics easier, the prerequisite is understanding of geometric algebra, which I suspect adds a more challenging preliminary step than the traditional approach. Doran Cambridge group has a very nice gauge theory of gravity based on this approach (see: http://arxiv.org/abs/gr-qc/0405033v1), among many other things.
In any case, you may already have an opinion on most if not all that I referenced, would be interesting to read your opinion, even just a short comment.

22. **Peter Woit**
December 1, 2013

Peter Peterson,

I've always thought that the spinor/Clifford algebra point of view on geometry ultimately will have something to do with how to unify internal and space-time symmetries, but I don't think any of the attempts to do this tried so far really work. Much of the "geometric algebra" stuff is purely classical, but what I think is most fascinating is the way it shows up in quantization of fermionic variables. Of the things you mention, I'm not specifically familiar with those sources, but I've looked at closely related things off and on over the years.

What I've tried to put into this book is a good reflection of what I see as the important mathematical ideas, together with how they show up in basic ideas about quantization. There will be some more of this in the chapter I'm in still writing about quantized spinor fields. Once I've finished this, there are some ideas there I think worth pursuing, will try and write more about them at some later point.

23. **vmarko**
December 1, 2013

Peter Peterson:

"[…] http://arxiv.org/abs/gr-qc/0405033v1 […]"

I don't know about the whole geometric algebra approach, but what I see from the above article is the following:

(1) The authors introduce the geometric algebra by essentially rewriting the tensor calculus in the spinorial representation of SO(3,1). They just use a somewhat unconventional abstract notation to hide spinor indices. Apparently no mention is being made that this is equivalent to an already existing body of knowledge in a different notation.

(2) The authors do not appear to understand the difference between symmetry localization and introduction of interactions. They claim that localization of Poincare symmetry should be enough to generate gravity, criticize conventional (Kibble/Hehl/others) approach for having an extra step of saying "spacetime is curved", and then go on to perform both of these steps in their own approach, without recognizing what they do. In the end they advocate that their approach is better and that it leads to Einstein-Cartan gravity. However, from what I saw, it is quite equivalent to the standard approach.

(3) The authors make some explicitly incorrect statements regarding the localization of Poincare symmetry, such as that there is no way to disentangle translations and rotations, criticizing the conventional localization approach. They even cite Kibble and Hehl papers (from 1961 and 1976 respectively) to back that up. However, they seem to be unaware of the more recent literature where it was shown that this disentangling can indeed be done and that there is no problem with the conventional approach.

I don't want to sound like a referee here, but IMO the authors have not done their homework, and I don't trust any conclusions of that paper. Moreover, I don't see any benefit of introducing the geometric algebra language for this whole ordeal.

My advice on reading this paper: nothing interesting, move on.

HTH, 🙂
Marko

24. **D R Lunsford**
December 2, 2013

Thanks for the book. Fills a gap. Will sit next to Weyl when I have the print edition 🙂

-drl

25. **Mtor**
December 3, 2013

Hello Prof. Woit
You have a very beautiful book there. With all the theory you could easily add a chapter of "applications " containing elegant solutions of QM problems using representation theory. I´m thinking, for example, about the Onsanger solution or the Kauffman solution of the 2D Ising model or in general problems that can be stated clearly in a mathematical way and can be solved neatly using representation theory.

26. **lun**
December 3, 2013

OT but IT for this blog, perhaps you can comment on the latest stringy PR fad,
http://news.sciencemag.org/physics/2013/12/link-between-wormholes-and-quantum-entanglement
The hyerarchy between the quality of the evidence (very thin if you read both the original paper by Maldacena and Polchinski and these PRLs) and the "splash" in the academic press is remarkable even by string theory standards

27. **Peter Woit**
December 3, 2013

Mtor,
Thanks! Unfortunately there are many, many such applications, and I want to keep the length of this book to a reasonable size (below 400 pages).

lun,
Arguments about entanglement/holography/black holes seem to be one of the major industries today among particle theorists. None of this seems to address any issues I find interesting so I'm mostly trying to ignore it, and have no intention spending the time necessary to follow these arguments well-enough to comment on them here knowledgeably. I suspect there's plenty of hype to be deflated there, but someone else will have to do it….

28. **Peter Peterson**
December 4, 2013

Vmarko,
Thank you for reviewing the Arxiv post. You are much better qualified than I am to evaluate the merits of the approach, and reading the books I quoted, I noticed a number of 'leaps of faith', mostly in Doran's case. Lasenby, if I am not wrong, cooled down to the idea over time. What was appealing to me though if one could come up with formalism as the authors claimed, where they could operate in flat spacetime it would lead to some new insights.

However, I agree that yes, Doran and group try to apply the geometric algebra formalism to everything that is already known (but not much, if anything, new) just as another mathematical tool that would lead to the same result in a more straightforward way. Which opens a question that is never answered: what spacetime property makes macroscopic interactions behave to follow this mathematical abstraction. Meaning, if mathematics can indeed be formalized to include all types of interactions, which was not proven yet. The beauty of geometric (Clifford/Grassmann) product application and scalability to any number of dimensions in a way consistent with Lie group symmetries is aesthetically pleasing, if anything else.

29. **Lowell Boggs**
December 4, 2013

Thanks for posting the reference to the Feynman lectures. Your book is beyond my abilities, but you regularly post lots of interesting information that I actually can appreciate and learn from. Thanks!

Best of luck on the success of the book.
Lowell

News from CERN

December 5, 2013
Categories: Experimental HEP News

Here's a roundup of recent CERN-related news:

- The status of the LHC and the LHC experiments was discussed here yesterday. The LHC shutdown is more or less on track, first beams at 13 TeV total energy Jan. 2015, physics starting April 2015.
- Both ATLAS and CMS have announced new data on tau-tau decays of the Higgs, providing stronger evidence for this signal than was available earlier. ATLAS sees a signal with significance 4.1 sigma, CMS at 3.4 sigma. These results are consistent with the SM, and rule out some SUSY alternatives in which the Higgs would behave differently. The Register headlines this Exotic physics takes an arrow to the knee.
- Not CERN related, but the last month or so has seen other new results ruling out some SUSY and other SM-alternatives. A good place to follow this is at Resonaances, where Jester discusses the LUX result on dark matter, and the new limits on the electron EDM.
- Plans are being made for long-term preservation of LHC data, keeping it in a usable form for the future. Nature has a story here, this this presentation has more detail.
- Meanwhile, CMS has a pilot project going to make some data available publicly in a form that can be accessed by high-school students.
- CERN DG Heuer has this announcement about activities of the FCC (Future Circular Colliders) study group looking into prospects for a large lepton collider (TLEP) as well as a higher energy hadron collider post-LHC.
- The CERN-sponsored SCOAP^3 open access publishing initiative will start operation next month. From their web-site, it appears the idea is to spend up to 10 million euros/year, mostly going to commercial publishers to finance their journals that publish HEP papers. In return the papers (almost all of which were already accessible on the arXiv) will be "open access". The publishers will get paid a per-article charge, so will have a serious incentive to publish as many articles as they can. I don't see a document explaining exactly how the money will be spent, but for some idea of where it will go, see this list. It indicates that the two big recipients will be Elsevier (with 1300 or so papers/year in Physics Letters B and Nuclear Physics B, at around $2000 per paper) and some combination of Springer and SISSA where about 1650 JHEP papers will cost 1200 Euros each. I gather that in return for this the journals will reduce or eliminate subscription charges, but don't know the details.

Comments

1. **PhysGrad**
   December 5, 2013

   The Resonaances link you posted appears to discuss only the electron EDM, not neutron. As far as I'm aware, there hasn't been an update to the experimental neutron EDM value recently, though there are several experiments that are either in planning or ongoing to push the upper limit of the nEDM closer to the SM prediction (as well as various BSM predictions which could show up at several orders of magnitude higher than the expected SM value).

2. **Peter Woit**
   December 5, 2013

   PhysGrad,
   Fixed. Thanks!

3. **Bernhard**
   December 6, 2013

   "at 13 GeV total energy Jan. 2015" –> "at 13 TeV total energy Jan. 2015"

4. **Peter Woit**
   December 6, 2013

   Thanks Bernhard, fixed.
   I seem to make this mistake a lot, maybe it has something to do with early experiences where this was the right scale for a collider energy…

5. **Narad**
   December 7, 2013

   > I don't see a document explaining exactly how the money will be spent

   Indeed, this would be quite interesting. It's become standard practice to eschew copyediting altogether or to provide a simulacrum obtained at rates that guarantee inadequacy. A quick look at Nucl. Phys. B. and JHEP reveals predictably amateurish "typesetting" of the LaTeX-is-a-magic-incantation variety (when you manage to accomplish poor spacing on wide measure, you're simply *bad at the job*). What's left? Driving everybody up the wall with an awful peer-review "management system"? Dealing with the occasional MS Word document? A little bit of thoughtless art processing? Adding pointless internal links that may not even work right? (JHEP's figure links are putting the end of the figure legend at the top of the page.) Serving proofs and not bungling the corrections? Spitting out some sort of shockingly ugly HTML and delivering it on a nightmarishly crufty platform à la Elsevier?

   Perhaps the name should be changed from "processing charge" to something more transparent, because I've yet to find anything resembling $2000 worth of actual work on any given paper. In fact, in the case of JHEP, I'm not finding anything resembling anybody's even having looked at *anything* in the first place: "$p_{\rm T}$ -cut", with the space, is in both the published version and nucl-ex/1307.1249.

Peter Higgs: "Today I wouldn't get an academic job. It's as simple as that"

December 7, 2013
Categories: Uncategorized

The Guardian has an interesting piece about Peter Higgs, evidently their reporter talked to him on his way to the Nobel Prize ceremonies this week in Stockholm. Higgs will be speaking tomorrow (Sunday), and I'm curious to hear what he will have to say. His talk will be available live at the Nobel Prize website.

Higgs points out that the kind of work he was awarded the prize for was done in an environment that no longer exists:

> He doubts a similar breakthrough could be achieved in today's academic culture, because of the expectations on academics to collaborate and keep churning out papers. He said: "It's difficult to imagine how I would ever have enough peace and quiet in the present sort of climate to do what I did in 1964."

By the time he retired in 1996, he was glad to be out of academia:

> After I retired it was quite a long time before I went back to my department. I thought I was well out of it. It wasn't my way of doing things any more. Today I wouldn't get an academic job. It's as simple as that. I don't think I would be regarded as productive enough.

Higgs has definitely not been a careerist sort, turning down a knighthood in 1999:

> I'm rather cynical about the way the honours system is used, frankly. A whole lot of the honours system is used for political purposes by the government in power.

He thinks he likely would have been fired by his university back in the 1980s if there hadn't been a prospect of him getting a Nobel.

The work Higgs did in 1964 was on a rather unpopular topic. At the time the reigning ideology was "S-matrix theory", which argued that local quantum field theory was a hopeless subject, so one should be working on formulating basic physics just in terms of S-matrix amplitudes, using their holomorphicity properties (this idea has had somewhat of a comeback in recent years). The 1960s however was a time of a great expansion in the number of university positions, so people like Higgs could make a career despite working on unpopular topics.

Progress in particle theory slowed dramatically after the early 1970s. One reason for this of course has been the huge success of the Standard Model, as well as the inherent difficulties involved in getting experimental access to higher energy scales. One wonders though whether the post-1970 collapse of the HEP theory job market and very different environment that ensued might have had something to do with this. As Higgs himself is well-aware, if he had come along 10 years later, he would not have found a job in the field.

In the UK today, things seem to be getting even worse, with strong pressures from the government to only fund work likely to have an immediate economic payoff. For more about this, see this commentary at Physicsfocus by Philip Moriarty on The Spirit-Crushing Impact of Impact. The UK has just announced the founding of a new Higgs Centre for Innovation, to be built in Edinburgh and opened in 2016. It will be devoted though not to the kind of research Higgs had success with, but to "big data" and "space", considered by the government to be among the most promising technologies for the future. It's rather ironic that Higgs is the sort of scientist who would not be employable by the Higgs Centre.

Update: For the acceptance speech by Higgs, see here, and see here for an official interview. For a different point of view, from one of the experimenters who made the award to Higgs possible, see here.

Comments

1. **Benni**
   December 7, 2013

   Peter woit wrote:
   "It will be devoted though not to the kind of research Higgs had success with, but to "big data" and "space" […]. It's rather ironic that Higgs is the sort of scientist who would not be employable by the Higgs Centre."

   I think this is a bit strange characterization of Higgs. Actually, Higgs worked on quantum gravity in his early years. See for example this paper:
   http://prl.aps.org/abstract/PRL/v1/i10/p373_1

   The site: http://www.edition-open-access.de/sources/5/3/index.html notes that: Peter Higgs too had been part of Hermann Bondi's Relativity and Gravitation group at King's College, London, since 1956. It was Pirani who urged him to take more interest in quantum gravity, prompting him to take up the position at the IOFP. Though invited to the institute to study gravitation, Peter Higgs ruefully admits that he spent his time there working on symmetry breaking in quantum field theory.15 Higgs first encountered Bryce DeWitt in 1959, in Royamont France. This was the second GRG conference16, and it was shortly after that the International Committee on General Relativity and Gravitation was formed (see Kragh [11], p. 362). He met him again at the GRG3 conference in Warsaw, in 1962. After 1956, following Pirani's advice, Higgs began looking at quantum gravity – at the time he was working with Abdus Salam at Imperial College. Here he wrote on the constraints in general relativity. This led, in 1964, to DeWitt's invitation to Higgs to spend a year at the institute, which he did, arriving in September 1965, after a year's postponement. Bahnson died tragically in an airplane crash the year prior on June 3, 1964 – a chair was established at UNC in his honour, to be occupied by Bryce DeWitt.

2. **Benni**
   December 7, 2013

   It is perhaps for the above reason, that Higgs has to say the following on the Higgs center, according to your link: "Prof Higgs, 84, said: "This support from the Treasury and the STFC will create an environment in which future generations of scientists from around the world can share and develop ideas in theoretical physics."

   Surely, Higgs would not promote the Higgs center with that words, if it did something that Higgs does not approve of. It is generally consensus that astrophysics, general relativity, quantum gravity and mathematical physics are the fields where physics can still make some advance. Higgs hat a professorship for mathematical physics in Edinburgh. Unfortunately, the number of chairs with that topic, in europe is almost zero. The few chairs that exist are highly competitive and someone like Higgs, who contributes very deep but also very few papers would have almost no chance.

3. **Florin Moldoveanu**
   December 7, 2013

   I certainly understand where Prof. Higgs is coming from. I published my first paper in my last undergraduate year and then I joined a small and prolific group which was publishing a paper every month. Then I came to US to get my PhD and I began questioning the value doing this incremental busy work which kept me from thinking on really deep problems. After graduation I left academia and started working in the industry where I have a successful career. In the meantime I kept thinking on hard problems and the effort payed off. Then I tried to come back thinking that if I had a very promising domain and nice results everyone would appreciate them, but surprise, surprise, there is a large background crackpot noise you need to break through. Basically you need to accumulate credibility, play the game, and (re)build your network and reputation.

It was lucky for Professor Higgs to make his discovery while in academia, because nobody would have taken him seriously if he was on the outside. On the outside the silence is deafening.

4. **Peter Woit**
December 7, 2013

Benni,

It's hard to tell without more detail, but the "space" research to be supported by the Higgs Innovation Centre appears to be space technology with commercial applications, not quantum gravity research. In any case, while Higgs supports this and lends his name to it, that doesn't mean they're any more likely to hire him than academia would be, and he makes clear his view about his chances there.

5. **Chris W.**
December 7, 2013

Compare with Kenneth Wilson.

6. **MathPhys**
December 8, 2013

"After I retired it was quite a long time before I went back to my department. I thought I was well out of it."

ha ha ha that was really funny. I think that's how many of us will feel as well.

PS I met Peter Higgs in 1985 went I attended a summer school in Edinburgh. A quiet, mild-mannered man. It was the first time that many of the students at the school, and probably even some of the lecturers, realized that there is actually a man behind the name that we all learnt in class and went for a handshake and chat with him. He was very kind, approachable, unassuming and obliged us all. He didn't patronize us. There was no posturing.

Meeting Francois Englert was also very good but in a different way that I'd rather not get into here.

7. **Gordon**
December 9, 2013

There are lots of us older academics that wouldn't have got a job today- or maybe more likely we would have played today's game. What is unfortunate is that Higgs' point about "productivity" is valid and can be a major barrier to some forms of research unless hidden in a fog of salami publication (and mixed metaphors).

8. **Steven Chan**
December 9, 2013

While all folks in academia do need to publish something, but the number of publiciations should not be the single most important benchmark. Writing good paper take months, and I think one first-authored paper every year or every other year should be considered enough.

You said I should avoid politics, but short-termism isn't just a science problem. It is a political and financial disease that infected science. We are just being affected by a much wider-scale problem.

9. **Eric**
December 9, 2013

Higgs may also have the lowest h-index of any Nobel winner. He was right topic, right time…and very lucky.

The other five credited with the theory seemed to have done well in academia.

10. **Eric**
December 9, 2013

@MathPhys

This sounds very interesting…do tell.

"Meeting Francois Englert was also very good but in a different way that I'd rather not get into here"

11. **Ike**
December 9, 2013

Here's what the reality looks like; Currently if you are a postdoc in the US seeking for a faculty position and have less number of publications compared to most of your colleagues, you cannot go forward!. The naked and widely known fact is that when you apply for a faculty position with less than 15 -20 papers, you cannot even pass the first stage and will be eliminated automatically. If you want to get a life and a permanent position, publish more often, if necessary divide one project into as many sub-publication as you can, publish even more, so they'd look nice on you CV or on google scholar website…

12. **S. Molnar**
December 10, 2013

Perhaps slightly off-topic in a narrow sense, but another Nobel prizewinner makes a related complaint against the big science journals, also reported in the Grauniad: http://www.theguardian.com/science/2013/dec/09/nobel-winner-boycott-science-journals.

13. **MH**
December 11, 2013

Higgs: ‘because of the expectations on academics to collaborate’

Reminds me of what Dijkstra (Turing award 1972) said in 1994:

In the wake of the Cultural Revolution and now of the recession I observe a mounting pressure to co-operate and to promote “teamwork”. For its anti-individualistic streak, such a drive is of course highly suspect; some people may not be so sensitive to it, but having seen the Hitlerjugend in action suffices for the rest of your life to be very wary of “team spirit”. Very. (http://en.wikiquote.org/wiki/Dijkstra)

14. **Doomed Mathematician**
December 15, 2013

I wish I knew this before going to graduate school….

15. **steve**
December 17, 2013

“He was very kind, approachable, unassuming and obliged us all. He didn’t patronize us. There was no posturing”. That’s very much the Higgs I remember, being a student at Edinburgh in the 80s. I’m surprised to learn though that the university considered firing him at one point since I always though he had a depth that no-one else quite had. It came across when he lectured with frequent side applications or subtle points that showed he really knew the stuff. His elegant solutions to homework problem got straight to the heart of the matter and that’s what his famous paper did too. It is good to see him speak out now about modern academia which seems to work against lone researchers. It’s a bit like the 1976 movie ‘Rollerball’ whereby “…the game was designed to show the futility of individual effort…” And yes, it is ironic that he would not get hired now by the institute to be named after him. Others like a young K. Wilson probably wouldn’t be hired now either, and if Von Neumann was alive today he would probably be a hedge fund manager:)

16. **anon**
January 1, 2014

Here are some questions that I would ask Dr. Woit and others.

(1) Would all of you agree that the prospects for future graduate students in mathematics or physics to pursue research in their fields is poor? Do any of you foresee this situation to improve in the future, or to further deteriorate?

(2) If you met a student who is thinking of pursuing graduate studies in mathematics or physics, would you consider discouraging him/her from doing so, and maybe instead direct them to more “applied” fields (e.g. engineering, medicine, statistics, computer science, etc.)?

(2)

17. **Peter Woit**
January 2, 2014

anon,
The situation in math is much better than physics. Even in math, getting a permanent position at a research insitution is difficult, but it’s much easier than in theoretical physics. I don’t have any problem encouraging students to get a Ph.D. in math, since there are some reasonable academic job prospects. Theoretical physics is a different story, there students do need to be made aware of how awful the job situation is (and will be for the forseeable future).

As for applied subjects, I would guess job prospects depend a lot on the exact field, but I’m not well-informed about what is happening in different fields. People should be aware that claims of an “STEM shortage” are nonsense, so if they go into some applied STEM field expecting jobs to be plentiful they probably will be disappointed.

What's Next?

December 9, 2013
Categories: Multiverse Mania

Last week's public lecture at the Institute for Advanced Study by Nati Seiberg is now available online. He was speaking with the title *What's Next?* and promoting a story about where particle physics is and where it is going pretty much identical with that coming from his IAS colleagues. Despite the overwhelming failure of string theory unification and the dramatic evidence from the LHC ruling out popular ideas about SUSY, there was no admission of any discouragement about string theory or SUSY.

String theory was described as the best candidate for a fundamental theory, one that has been making "enormous and exciting progress with amazing new insights" and "all signs are that we will continue to make progress." For more details Seiberg points to talks given by Witten such as this one. According to Seiberg, string theory has not problems and failures, but "challenges". One challenge is that "we do not understand the principles" of string theory. Another is that "we need experimental confirmation", which makes it sound like the problem is one of experiments not done yet, rather than the real problem, which is a "theory" that predicts nothing.

The hierarchy problem is emphasized as the central problem for particle theory, with almost exactly the same point of view as that of Nima Arkani-Hamed, which I've discussed here many times (see for example here and here). We're told not to think of the LHC results as providing evidence against SUSY, but to interpret LHC results as choosing between two possibilities:

- SUSY exists at LHC scales and arguments about SUSY solving the hierarchy problem are vindicated. Things don't look good for this so far, but hope is held out for the next run, with an admission that if it doesn't turn up then, that's it for SUSY as a solution to this problem.
- No SUSY at LHC scales just means it is at higher scales, and the multiverse is now brought in to deal with the hierarchy problem. In a recent Science Weekly podcast, Arkani-Hamed says he's still willing to bet several years salary that SUSY exists, but now he thinks maybe it only shows up at higher energies than he'll see in his lifetime. He's willing to bet that SUSY will show up at the next LHC run, but just $50.

Since even enthusiasts who have devoted their career to the cause are now only willing to put up $50 in favor of SUSY at 13 TeV, it's pretty clear that hardly anyone is now expecting to see this. We're already in the era of trying to understand the implications of no SUSY at the LHC, with the multiverse the main argument now being deployed in favor of not giving up on cherished speculation about SUSY and strings, no matter what experiments say.

Seiberg does give a different historical analogy for the hierarchy problem, likening it to a fine-tuning problem that Newton was worried about, that of the stability of planetary orbits. Why does a small perturbation of such an orbit not lead to exponentially large changes, destabilizing the orbit? Seiberg lists three possible solutions to such fine-tuning problems:

- There really is no problem if you understood the theory well-enough.
- You need to invoke new physics as a stabilizing mechanism.
- The answer is "environmental": the orbits are generically unstable, we just happen to live in an unusual place where they are stable.

The odd thing about his use of this historical analogy is that the lesson to be drawn is that of course the answer is the first alternative, but he quickly passes that one by as not worth talking about. I doubt the last alternative ever occured to Newton as anything other than a joke, and don't know of any evidence that he tried to come up with models of things like new unseen planets to solve this supposed problem. Newton surely realized there was plenty that he didn't understand about what Newtonian mechanics had to say about celestial mechanics. It's just as clear that our best model of the Higgs, with its large number of undetermined parameters, is such that we just don't fully understand where the Higgs potential and Yukawas come from.

The Seiberg talk seems to be one of a series (others listed here) of talks associated with the Milner Fundamental Physics Prize. IAS director Dijkgraaf introduced Seiberg as one of the four IAS winners of the $3 million Milner prize, with this leading his list of honors awarded to Seiberg. The talk was a public one of a sort that has for the IAS not just an educational role, but also a fund-raising one. Something is being sold here, the idea that SUSY and string theory are great successes, with the IAS faculty well-deserving the multi-million-dollar checks awarded to them for their work on these topics. Later this week they'll be getting together in San Francisco to decide how to split up $3.6 million in new checks among five other string theorists (the announcement of the winner of the 2014 prize will be made Thursday). All of this I fear has something to do with why we're not hearing from those at the IAS a truer picture of what no SUSY at the LHC means: the collapse of ideas that don't work and evidence that we don't yet have any viable conceptual framework for going beyond the Standard Model. This summer the IAS will host its usual PiTP program to train grad students and postdocs in what they need to know to face the future. The topic? String theory.

Comments

1. **Shantanu**
   December 9, 2013

   Peter, something OT but relevant to particle physics
   I am the 50th anniversary of the texas symposium and Steve Weinberg (in his talk)
   was discussing many papers by Shaposhnikov and others which proposed
   a 126 Gev mass of Higgs boson based on asymptotic safety. (He also discussed
   implications for models of inflation based on these asymptotic safety scenarios)

2. **Yatima**
   December 9, 2013

   It is interesting to take the stability of planetary motion as example.

   The only planetary motion that we knew about up until 10 years ago is solar system's. The problem of the stability of the solar system remains, as far as I know, unsolved. Numerical integration shows that small perturbations of orbits and masses will indeed lead to exponentially large changes over *fractions* of the lifetime of the solar system and the current configuration has no à priori reason to be stable.

   However, Earth did indeed stay reasonable habitable over the lifetime of the solar system.

   So the answer could be any of the three ones proposed, or a mix of them: We are at a local minimum in phase space that we don't understand yet (possible); there is some new physics at large scales we haven't found yet (not at all impossible) or it's environmental – we just happen to live in a solar system that happens not to have seen major excursions from a configuration in which Earth stays habitable over > 4 * 10^9 years (quite possible if the weak anthropic principle is applied, but I don't like it).

3. **DrDave**
   December 10, 2013

When I was a science student it was my dream and the dream of my colleagues to be at IAS. I find this very disheartening, the mix of big money and buying shelf space in the education Big Box store.

4. **JohnB**
December 10, 2013

Much like economics has a 'heterodox' crowd for challenging the (by now embarrassingly false) mainstream, physics probably needs a similar break, and (like economics) it's unlikely to come in an academia partially captured by interests that promote string theory.

That seems to make it a slightly politically dominated topic (far less so than economics mind) – so probably going to need to attract a large source of funding, for creating institutes that lobby against string theory as a field of study, and instead both promotes and directly funds large projects that study/develop alternatives.

It's unfortunate how much scientific study seems to be corrupted by politics and good old cognitive bias, as pretty much any other area of human interaction.

5. **Dr. R. Vieselman**
December 10, 2013

When I was younger I was so lucky to have a few conversations with Dr. Dijkgraaf, back when he was teaching at the Amsterdam University, here in The Netherlands. I love how this professor seems to give a romantic twist to the the profession, not afraid of asking the big questions: Is there a certain system driving the world? Are we the only ones in the universe? Is the youth getting smarter? even asking if we are living in a matrix of created by a computer program. It's typical for Dijkgraaf to grab the opportunity to give a second role to such a great event as the IAS, always thinking outside the box. A great and charismatic man, which the world can learn a lot of, and not just in hard science.

6. **Michael Schmitt**
December 10, 2013

Like Yatima, I appreciate the analogy with Newton's Laws and planetary motion. And I agree with Peter's reaction – the first scenario is the best one and perhaps the most likely one.

7. **Anon**
December 10, 2013

Dear Peter, you once wrote a paper together with N. Seiberg:

http://inspirehep.net/record/191060

How was your impression back then?

8. **Tom Andersen**
December 10, 2013

The previous story on Peter's blog, 'Peter Higgs: "Today I wouldn't get an academic job. It's as simple as that"' is very related to this post.

What is happening in physics in recent decades is something I call the **Paradigm Mountain**. As the last 10 decades have rolled by, the speed of communication has increased by about three orders of magnitude. This of course will have consequences – **mostly good.**

The bad: even though many scientists have read and understood Khun's thesis about scientific revolution, its still hard not to trivialize new theories as wrong because they don't explain 'result X' or 'effect Y'. With instant communication comes instant critique (often even instant ridicule).

In other words, the internet has made the 'paradigm mountain' of the Standard Model very hard to penetrate. To quote Peter Higgs *"It's difficult to imagine how I would ever have enough peace and quiet in the present sort of climate to do what I did in 1964."*

9. **Mathematician**
December 10, 2013

I wonder if, in the long run, this will damage the IAS "brand".

10. **Peter Woit**
December 10, 2013

Anon,

This is in no way intended to be personally critical of Nati Seiberg (who I did work with happily way back when, but haven't talked to in a long time).

As a general comment (not about Seiberg particularly) about the four IAS Milner prize winners and new director Dijkgraaf, are all quite good, hard-working physicists. They do though share a certain style that I've never found congenial (common among hep theorists, much less so among mathematicians), that of liking to work on the latest, hottest new idea appearing from within the dominant paradigm being followed by the most influential people in the field. I think this has gotten them into trouble as this paradigm has worked its way far down a blind alley. Unfortunately the Milner money has had the effect of painting this blind alley as some sort of success story, making the problem worse.

11. **Umesh**
December 11, 2013

The first option you seem to 'interpret' from the talk, which is:

There really is no problem if you understood the theory well-enough.

was not what Seiberg said. He argued that Newton's dilemma was shown to be unfounded as it was shown that the solar system is chaotic. But the analogous situation in the case of the standard model leads only to the conclusion that one's logic is totally wrong. This, though a possibility, would be a vacuous and an uninteresting conclusion, therefore it won't be pursued.

12. **Peter Woit**
December 11, 2013

Umesh,

I did understand that Seiberg was justifying not discussing the first option (however you want to exactly interpret it) as "not interesting", but this is where I fundamentally disagree with him. If you find that your argument leads to two possibilities, one ruled out by experiment, and the other saying you should give up on conventional science, it seems to me that this means you have learned there is something wrong with your argument. It's not at all the case that the "hierarchy argument" is just a piece of logic, but rather it's various possible sequences of assumptions. My take on the current situation is that the LHC has just told us that a certain popular speculative argument doesn't work. Instead of standing up before the public and repeating this argument vigorously, making it sound much solider than it is, why not give a talk saying "here are the components of this argument that has been killed by the LHC, it's exciting that we've learned one of them is wrong". An example of where you go from there is the Weinberg talk Shantanu pointed out, which deals with one possible way around the hierarchy argument.

13. **Anon**
December 12, 2013

Dear Peter, thank you very much for your thoughtful comments.

14. **Umesh**
December 12, 2013

Sorry, I couldn't understand you. Are you saying that the hierarchy problem is not a problem? Just looking at the SM parameters show such a huge span of numbers, and isn't it a valid question to ask why are the numbers distributed over the range they are? Secondly, if one can't think of any 'natural' way (by which I mean some vague, qualitative principle at least, which may be made precise later) which makes sense of the span of the numbers, we're left with the three possibilities which Prof. Seiberg spoke of. Now, unless you tell me some way (vague, qualitative would do, details may be worked out later) to understand your statement, which is:

"understand the theory (SM) well enough"

and we are sure we understand QFT well enough to say that such numbers can't possibly arise unless delicate cancellations occur, we are forced to conclude that there is possibly no creative avenue analogous to the theory of classical chaos which may lead to 'better understanding' of the SM. The stability of these numbers is a very fragile issue as you know, because quantum corrections drag every (classical to begin with) number to the scale of the problem involved, and no one knows of a way to prevent it, apart from symmetry principles, which is why SUSY is such an attractive avenue. In the scenario that SUSY is not seen at LHC, what Prof. Seiberg says is that one would be forced to give up the notion that the SM as a gauge theory is special; it will herald another revolution analogous to the Copernican one. I fail to see why you call this giving up 'conventional science' any more that you'd say the same thing to people who were forced to give up the idea that the earth was the center of the universe. This time, it's the SM and the rest of our models, which are defined by parameters that aren't special, that's all.

To conclude, a word about the asymptotic safety scenarios you point me to. They're inherently perturbative in nature and any treatment of gravity based on such field-theoretic scenarios will eventually run into the fact that classical gravity has black hole states, which can't be reproduced by any known local QFT, hence the dictum 'gravity is not a QFT'.

15. **Peter Woit**
December 12, 2013

Umesh,
The question of the SM parameters of course is perhaps the biggest question around, and we have no idea why they take the values they do, over the large range that they do. That's not at all though what is commonly called the "hierarchy problem", which usually refers to the quadratic sensitivity of the Higgs mass to the cut-off scale. The fact that SUSY theories (with SUSY-breaking scale at the weak scale) don't have this feature has been one of the main arguments for SUSY, but it's not at all an air-tight convincing argument.

One problem with what Seiberg does in his talk is that he doesn't clarify what the "hierarchy" or "naturalness" or "fine-tuning" problem is, and that's the interesting question now that the LHC has shown that the SUSY "solution" to it doesn't work.

I describe the multiverse "solution" to the "hierarchy problem" as "giving up on science" because it's completely untestable. It's just an empty pseudo-scientific excuse for not understanding something. In fundamental physics, for anything we don't understand, someone can always say "that's something that's different at different places in the multiverse, so what we see about it can't be predicted". Sure, this could be true, we could live in a Matrix, etc., etc. but such things are not science.

The argument that we should give up the idea that the SM is "special" because of LHC results is absurd: the LHC just dramatically confirmed a whole new sector of that theory, and the whole thing over a large new energy range. The LHC is telling us emphatically that the SM is "special".

As for quantum gravity, we have zero evidence for it, zero evidence for what happens at the Planck scale, and only highly speculative and likely wrong ideas. Using this to argue for a multiverse or anything else isn't a solid argument at all.

16. **Umesh**
December 12, 2013

Of course the LHC has not only verified the SM, but probably is also hinting that there's nothing beyond the SM. But very sound counter arguments show that there HAS to be physics beyond the SM. Just because the LHC confirms the SM doesn't itself mean it's special. It may be a **SPECIAL** feature of this particular phase or 'vacuum' of the relevant high energy completion of SM and gravity. You haven't yet shown one serious flaw in the arguments, such as an invalid extrapolation or the failure of using EFT techniques or some such thing.

"As for quantum gravity, we have zero evidence for it,.."

I fail to understand this statement. Does it mean that you think quantum gravity doesn't somehow make sense? What more 'evidence' can one need other than the fact that both quantum mechanics and gravity operate in one and the same universe, and thus when the two regimes meet, as they surely would at the Planck scale, one would need such a theory?

Next, no one is remotely suggesting the following:

"The argument that we should give up the idea that the SM is "special" because of LHC results is absurd:.."

Indeed that no one is immediately giving up on the idea of the SM being 'special'; one would like to find the 'special' solution to the equations of the correct high energy theory once the task of unifying gravity and QM is completed. Sure, sounds awesome and it's the dream of many physicists – Prof. Seiberg himself said he'd be reluctant to give up the idea that the solution that we currently observe isn't somehow uniquely determined (in other words, 'special') – but unfortunately, if one doesn't find a mechanism by which the apparent 'specialty' can somehow make sense, by which I mean that the parameters stay put where they are instead of getting dragged here and there by quantum corrections, what is one supposed to do? Keep sticking to the 'prejudice' of the SM being 'special' in the face of all evidence? Just like geocentric people who didn't wanna give up the 'special' place of the earth in the universe?

You write:

“One problem with what Seiberg does in his talk is that he doesn’t clarify what the “hierarchy” or “naturalness” or “fine-tuning” problem is, and that’s the interesting question now that the LHC has shown that the SUSY “solution” to it doesn’t work.”

Prof. Seiberg very clearly elucidates (in my opinion in a beautiful way) what the ‘hierarchy’ or ‘naturalness’ or ‘fine-tuning’ problem is. It is the question of the stability of the numbers (the parameters) of the SM, and yes, the Higgs mass is one of them, particularly affected by the quadratic divergences you mention. Some parameters, like the fermion masses and mixing angles could have explanation in the physics at extremely high energy – he mentions the neutrino mass as an example – and some don’t. When they don’t – like the theta angle of QCD – one *HAS** to look for a mechanism which solves the problem at the energy scale in question. And, yes, SUSY was useful precisely for this reason and the expectation was that it would be seen at the LHC. It didn’t. So, back to the question: what keeps the numbers stable? SUSY hasn’t solved this set of issues, so what gives? The conclusion that SUSY doesn’t work has the important caveat – that it doesn’t work at the scales probed by the LHC.

Indeed the multiverse can’t be ‘tested’ in the sense that in case there are causally disconnected regions, no signal can ever reach us. But that such cases can occur have been around in the theory of inflation as well. What one does is then try and understand ‘our neck of the woods’ as Prof. Seiberg says. I still don’t see what issues you have with these (straightforward) argument.

17. **Peter Woit**
December 12, 2013

Umesh,

We just don’t have any evidence one way or another about some completely different theory at high energies, with the SM only an effective theory (and thus “not special”). What we have learned in recent years is that those who argued that since the SM is just an effective theory, we should see new physics at the TeV scale were wrong.

Re quantum gravity, if you would read the rest of the sentence and not delete it, my meaning would be clear.

I’ve endlessly discussed the hierarchy problem here, don’t see much point in repeating myself. One basic point to keep in mind though is that there’s something highly speculative about any argument based on relating energy scales we have no access to or information about and the electroweak scale, as in “it’s a huge problem that small changes in physics at the Planck scale have enormous effect on physics at the electroweak scale”.

About predictivity and the multiverse, see
http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=whats-wrong-with-having-a-multiverse-doesnt-inflation-imply-one

18. **Umesh**
December 12, 2013

We surely do have evidence for a completely different theory at high energies, and most certainly at the Planck energy. Are you contesting the point the the very structure of QFT breaks down at (Planck) such energies? Even if the SM is (and surely it is not) the story all the way up the Planck scale, we surely need a **NEW** theory at the Planck scale. As for new physics (at the TeV scale), it was expected based on plenty of evidence, not empty speculation. The evidence, apart from numbers that can’t be explained at the level of the SM itself (numbers like neutrino masses etc) included numerous other observations. I am free to discuss what these ‘numerous other observations’ are in case you’re willing to indulge me.
The very fact that no new physics was seen is forcing a rethink of the whole paradigm, beginning with the ‘most flimsy’ so to speak – which may/may not include naturalness.
Of course I read your statement about quantum gravity fully, and understand it to mean that you think all ideas about it are not only highly speculative, but most likely wrong. This is wrong. What we DO NOT have is a complete theory of quantum gravity, but we do have a very good idea of what the high energy (trans-Planckian, if you wish) physics qualitatively looks like. Just to give you an idea, it’s certainly true that at very high energies, say energies exceeding the Planck scale, the states are dominated by large black holes. This is just one among very many concrete statements about physics at the Planck scale, which you seem to just ‘brush under the carpet’ as ‘speculative’ or ‘most likely worng’. Please feel free to indulge me in case you wanna know more.

“One basic point to keep in mind though is that there’s something highly speculative about any argument based on relating energy scales we have no access to or information about and the electroweak scale, as in “it’s a huge problem that small changes in physics at the Planck scale have enormous effect on physics at the electroweak scale”.”

Unless you tell me what you mean by ‘..there’s something highly speculative..’ I have to reply to the above paragraph with “..there is nothing speculative..”. This is because no one is ‘relating energy scales’ – at least in the arbitrary manner you seem to imply. It is a very sound procedure which is based on solid knowledge we know about the current energy scales, what the renormalization group has taught us, and so on. Again, feel free to indulge me in case you want to know, though I suspect that you already know what I’m on about.

19. **Umesh**
December 12, 2013

In fact, I can also tell you about a very well known quantum theory of gravity, but the requirements of staying on topic prevent me from going into it. I have just read the FAQ you linked me to above, and what you write there is wrong. Let me explain. You say:

“The issue is not whether there’s more than the observable universe out there. That may very well be, and maybe someday we’ll even understand inflation well enough to have a good model of what that might be. The point though is that there is zero evidence that whatever else there is has different physical laws than ours, and that is what is needed to make the whole anthropic business work. In simple models of inflation, whatever else is out there will have the same laws as ours, so yes, you can get a multiverse, but a pretty boring one. You can use string theory or something else to come up with much more complicated models that give you pretty much any physics you want in different parts of the multiverse, but there’s no evidence for these, they are untestable and explain nothing.”

Sorry for copy-pasting the whole paragraph, I don’t want to be accused of ‘deleting’ sentences again. Whether or not the other universes have ‘different physical laws than ours’ is a question that involves detailed knowledge of the correct dynamics of the high energy theory, which includes knowledge of the fact that which numbers are of fundamental importance and which are not (i.e., which ‘laws’ are to be the same and which ‘laws’ can be/have to be/may be different), and such knowledge is beyond reach even in calculable models of quantum gravity, the ones of the kind which I mentioned at the beginning of my comment. Thus, it’s impossible to a priori say which ‘model’ of inflation wold be chosen, and consequently what the nature of the vacuum would be. Once this can be achieved, one may ask why is it that we happen to ‘live’ in this neck of the woods and not in others. Whether or not inflation may require the ‘laws’ in other bubbles to be same or not is irrelevant here, because full information about the high energy theory (to the point of having detailed knowledge of vacuum selection) would render any statement made on the basis of inflation (which surely you’d agree would probably naturally arise as a requirement of the detailed laws) redundant, and it’s yet to be seen which features survive to be considered ‘laws’ – i.e., which won’t change throughout the multiverse – this number may be zero such statements, and which may be just be reduced to mere environmental numbers – a la the radii of the orbits of the planets of the solar system, as Prof. Seiberg beautifully explained.
As for whether the anthropic argument ‘to work’ or ‘not to work’ once the main task of understanding the high energy dynamics of gravity is well understood, we may be in a position to argue which parts of the argument stand and which don’t, as I’ve already explained above.

20. **Umesh**
December 12, 2013

"– i.e., which won't change throughout the multiverse – this number may be zero such statements, and which may be just be reduced to mere environmental numbers – a la the radii of the orbits of the planets of the solar system, as Prof. Seiberg beautifully explained."

Please excuse the sloppy editing of the above paragraph. I meant:

"– i.e., which won't change throughout the multiverse – this number may be zero- and which may be just be reduced to mere environmental numbers – a la the radii of the orbits of the planets of the solar system, as Prof. Seiberg beautifully explained."

21. **Igor Khavkine**
December 12, 2013

Dear Umesh, these wonderful arguments that you are referring to and that supposedly tell us so much reliable information about how Planck scale physics and quantum gravity are supposed to behave certainly exist. Not only do they exist, they are quite well known to anyone who has spent enough time interacting with high energy physicists. And that most likely includes Peter as well.

However, now you must realize that it is possible for two reasonable people to be exposed to these same arguments, yet not be equally convinced by them. And that's not because of lack of familiarity. So rehashing these arguments here is unlikely to help anyone. What make the difference are different standards of evidence. Once one adopts somewhat stricter standards of evidence than those that are common in the very mathematically adventurous high energy physics community, it becomes evident rather quickly that the state of the art of our knowledge about the non-perturbative and Planck scale behavior of QFTs (including gravity) is full of open problems. What all these well known wonderful arguments (and quite a few less well known ones) provide are conjectures (sometimes contradictory ones) about how these open problems could eventually be resolved.

So before making bold claims of certainty about how quantum gravity is supposed to behave in non-perturbative Planckian regimes, lets wait until a similar yet easier problem is solved and the Yang-Mills Clay Millennium prize is actually awarded to someone. Such a conservative position is not just hot air. If it were never adopted by anyone, we would still be quibbling about which model of aether is the right one, since an aether substrate is so absolutely mathematically necessary to explain the wave nature of electromagnetism.

22. **Umesh**
December 12, 2013

Sorry Igor, but it's not the 'standard' of evidence which is under question here. In case you, or anyone else for that matter can point out what 'exactly' reduces the 'standard' of the evidence presented above, it would be much better. It must be emphasized that the statements above (what you call rehashes) are not mere 'high adventure' maths, but arguments which are based on solid, well tested theories – general relativity for example. The Clay prize for the YM gap problem is one of proving in great detail the existence of the mass gap, which has nothing to whatsoever to do with the problems under discussion here. In case you really wanna know, we can discuss, but the blog owner would cry foul. I'm sure you're'nt suggesting that until the Clay prize is awarded, one sit idle about other important conceptual questions. In case it gets solved and tells us something useful about QFT, great, but in the meanwhile we need to address other (more important) conceptual questions – and be sure they have nothing to with QFT – like the behavior or high energy gravity. Unless you can tell me – based on the best current theory of gravity – why are the arguments wrong – and these don't include statements such as 'we have to solve other easier problems' etc etc – it is hard to essentially stop thinking about the quantum nature of gravity. Please feel free to point out (even vague would do) what you find invalid about the arguments which I presented above. You may choose any. There has to be some reason (mathematical reason) to believe that unless the YM mass gap is proved as required by the Clay committee, one mustn't think about quantum gravity, and I'm sure there's no such logical, physical or mathematical reason, except personal prejudice.

23. **Peter Woit**
December 12, 2013

Umesh,

I'm sorry, but we seem to have fundamentally different views on how science works. My reading of the history of physics is that it is full of theorists who were convinced that something or other was "certainly true", until someone did an experiment and showed it wasn't, or some other theorist who better understood the matter showed that it wasn't. Given this history, some humility and careful distinction between what one has experimental evidence for and what one doesn't seem like a very good idea. People should study highly speculative topics like transplanckian physics and may learn something this way, but, personally I'm not about to take seriously their claims that they know what is "certainly true" about this.

Somewhat similarly, about the multiverse, when you have a solid theory with some evidence for it that determines what our possible laws of physics are, then I'm willing to discuss the question of what this means about what things are predictable and what are environmental. At the moment, from everything I've seen (and I've spent a lot of time looking), the people trying to sell this have nothing at all, just a bunch of conjectures about how to construct a theoretical framework designed to evade any confrontation with experiment. To me, this does not count as science.

24. **Umesh**
December 12, 2013

Fair enough, then I think you wouldn't believe any statement made about the nature of the interplay between quantum mechanics and gravity. But I would still like to know what you consider 'highly speculative' about the standard extrapolations made from known theories. Even for starters, the Planck scale is very well out of reach of any conceivable experiment to get 'directly tested'. Going by your view, it's a totally futile endeavor to even think about this. But unfortunately, it's not about the scale, it's about the concepts – both of which work too well – and it must be possible, with or without experiment, to formulate the correct unification. It's not anybody's fault that the scale (in our vacuum) at which the two effects come together is totally untestable. You still have to come up with a valid argument (by which I mean something like RG, some semi-quantitative argument) as to why this whole enterprise, by your judgement is totally doomed from the start. That experiments could be performed for every conceivable scale so far in history is because of the ready possibility. And it would be extremely foolish to completely ignore the issues posed by quantum gravity as futile just because there can be no experiment performed, even though that's what is dictated by history.

25. **Peter Woit**
December 12, 2013

Umesh,
I don't believe thinking about how to quantize gravity is futile (I've spent some time doing so myself, and if I thought I had a good idea, would spend a lot more), I just believe those that do so have to keep in mind how large the uncertainties in such arguments are. In the question at hand (electro-weak scale physics and the hierarchy argument that the Planck scale has something to do with this) I'd argue that the uncertainties are large indeed.

26. **Igor Khavkine**
December 12, 2013

Dear Umesh, of course, thinking about conceptual issues in quantum gravity is important. And I would actually be out of a job if it weren't allowed. What I'm objecting to is claiming extreme certainty in what one comes up with along the way. Let me illustrate by closing a gap in my previous comments.

The reason I brought up the YM Millenium prize before has nothing to do with the specific question of the mass gap. My general understanding of the logic behind its formulation is that the mass gap is only a particular example of a non-perturbative property of a QFT. The main outcome of a solution is expected to be a proof that **shows the existence** of a non-perturbatively defined QFT and verifies that it has some particular property, like a mass gap. The mass gap could really be substituted with any other equally or more interesting property.

So, here's a specific problem with all these wonderful arguments that you are very ready to 'rehash'. In order to be really convincing, they need to include the following steps. Formulate a precise conclusion, **construct** a relevant non-perturbative QFT model (or your favorite QFT alternative), verify that the conclusion holds. Surprise! All of them are missing the step highlighted in bold. The main reason to pay attention to the YM Millenium prize is that, in case you are not following the literature on this subject, it acts as a helpful barometer for the completion of the construction step. So until then…

There is an alternative, though. Forget about all this mathematical stuff. Forget about all these wonderful arguments. Just take their conclusions as scientific hypotheses and test them! Then trust the ones that work. But in the case of quantum gravity I'm not holding my breath.

27. **Georges Melki**
December 13, 2013

Wolfgang Pauli didn't only say :"Nicht einmal falsch"! He also said:"Man muss nicht so viel reden "! I wish all concerned would heed the latter…

28. **Umesh**
December 13, 2013

Very well, since we agree that thinking about quantum gravity is an important problem, we may proceed. Igor, the problem with the Clay prize problem is about 'constructing' in a mathematically rigorous manner a non-perturbatively complete QFT. But this has nothing to do with the physics per se. All asymptotically free theories are non-perturbatively consistent (as far as physics is concerned, and one can even show such consistency in controlled situations), because they behave more and more like free field theories in the UV. I don't understand what a more rigorous construction of the said QFT can do for questions in other areas.

But, coming back to topic, we were discussing the hierarchies of the SM and allied problems. I haven't still understood the nature of the 'uncertainties' you spoke about in your comments. What are they? How is the argument that the Higgs mass (which is related to the EW scale), which is quadratically divergent (in case you disagree with this statement, please explain) gets corrections from all particles all the way upto scales which lie much higher than it (namely, the Planck scale, or whatever scale you want to insert in b/w) possibly an argument that has **UNCERTAINTIES**? I ask again, what exactly is the nature of these uncertainties?

Are you claiming that the demand that parameters like the Higgs mass (which aren't protected by symmetry) are unstable is somehow WRONG? This may be because you know something that others don't; it would be nice to see what this argument is, which doesn't need to invoke other high energy scales and somehow solves the problem at the EW scale itself. Please be sure that you can't postpone this problem to higher energies and you HAVE to solve it at the relevant enrgy scale (in this case the TeV scale) itself. In case you can show a loophole or a flaw in this argument, you're welcome, but I can't see any. In the absence of such an argument, Wilson's argument that scalar masses get corrections all the way upto higher energy scales stands, and nothing can be done about it. And given the fact that this 'naturalness expectation' is clearly false, i.e., the Higgs mass is really low compared to the Planck scale (this has been confirmed by the LHC) and looks quite stable, one is forced to conclude that the said parameter, namely the Higgs mass is UNNATURAL. Thus, there is no conceivable 'computation' one can do that doesn't involve UNNATURAL cancellations that would give you the desired answer. In case you feel this is wrong, please feel free to point it out.

Please note that I have cherry picked the Higgs mass for the above argument just because it's the easiest to demonstrate arguments/counter arguments about naturalness. In fact, I think that you want a NATURAL explanation for the Higgs mass (and consequently the EW scale) too, at least from your stance on the problem.

29. **Peter Woit**
December 13, 2013

Umesh,
I'm afraid we're just on different planets here. As far as I'm concerned, absolutely everything about physics at the Planck scale and how the SM we see at low energies is related to it is uncertain.

Since you invoke Wilson about the hierarchy argument, you might want to read what he had to say about it at
http://arxiv.org/pdf/hep-lat/0412043v2.pdf
(he called it a "blunder").

30. **Umesh**
December 13, 2013

Precisely why is it blunder? I am yet to read the 17 page paper you've linked to, but even for a moment forgetting the Planck scale, what is wrong, according to you, about the fact that scalar masses are subject to quantum corrections? In case you opine that this claim is wrong, I would like know.

31. **Peter Woit**
December 13, 2013

Umesh,
As I've written before, yes, in perturbation theory, scalar fields are quadratically sensitive to the cutoff. The question is what to make of that technical issue. Wilson, who was the one who first raised the issue, says the "naturalness" argument here is a blunder. You don't need to read the whole 17 pages, just look for Section 5 on "Blunders", and the second half of page 10 and first half of page 11. Then you can explain to my why Wilson is wrong and doesn't know what he's talking about.

32. **Umesh**
December 13, 2013

OK, I looked up the relevant portion of the paper linked above. I quote Wilson here:

"The final blunder was a claim that scalar
elementary particles were unlikely to occur in
elementary particle physics at currently measurable
energies unless they were associated with some kind
of broken symmetry [reference #23].The claim was that,
otherwise, their masses were likely to be far higher
than could be detected. The claim was that it would

be unnatural for such particles to have masses small
enough to be detectable soon."

I don't see how this contradicts with what has been mentioned before; Wilson just says that the expectation that small scalar masses aren't possible is the **BLUNDER**. He says that this expectation was based on 'natural expectations' (in other words, naturalness, because quantum corrections drag the mass up to the relevant scales), and it is indeed possible that small scalar masses may occur (and this has been confirmed by the LHC). What he goes on to say is that not every measurable number has to follow the NATURAL expectations, and goes on to provide the example of the nearest star to the sun as an example (a beautiful example, if I may be so bold). He explains how people during Copernicus's time said that such huge distances were 'unnatural' and couldn't occur; but today we indeed know that this is true. This in fact makes the case for 'unnatural' (or, environmental) characteristics of certain numbers even more robust, not weaker, as you claim.
In fact, what Prof. Seiberg says is precisely the above, though in a much more crisp and clear form; he said that the currently measured mass of the Higgs is uncomfortably low for technicolor schemes (to be natural) and uncomfortably high for SUSY schemes (to be natural). Thus, if there is nothing seen at the LHC that may provide some evidence for the above schemes, there might be no other choice left other than to say that unnatural numbers indeed occur in nature, and the Higgs mass is one of them.
Please tell me where you disagree.

33. **Peter Woit**
December 13, 2013

Umesh,
I think you're misreading Wilson, who is not making an anthropic or "environmental" argument. He is making an argument against doing what you are doing: extrapolating one's assumptions into regimes where you have no evidence and don't understand things as well as you assume. Read the first part of page 11, where he explains that the problem was that people in the days of Copernicus had no way to measure the distances to stars, and no understanding of Newton's first law of motion, and this is what led them to wrong reasoning. He then goes on to discuss quarks and the arguments against them, which were wrong, including an extremely relevant point for this discussion about non-perturbative qft having different behavior than expected.

34. **Umesh**
December 13, 2013

Sorry, but I have read the parts you have suggested, and find nothing that contradicts the 'environmental' argument. Of course that Wilson hasn't used the word 'environmental', but the example of the distance to the nearest star is surely environmental, do you disagree? He says, very clearly:

"There have been a number of cases where
numbers arose that were unexpectedly small or large.
An early example was the very large distance to the
nearest star as compared to the distance to the Sun,
as needed by Copernicus, because otherwise the
nearest stars would have exhibited measurable
parallax as the Earth moved around the Sun. Within
elementary particle physics, one has unexpectedly
large ratios of masses, such as the large ratio of the
muon mass to the electron mass. There is also the
very small value of the weak coupling constant. In
the time since my paper was written, another set of
unexpectedly small masses was discovered: the
neutrino masses. There is also the riddle of dark
energy in cosmology, with its implication of possibly
an extremely small value for the cosmological
constant in Einstein's theory of general relativity."

Clearly, in the above paragraph, isn't it clear that he means numbers too large or too small indeed occur in nature, just like the distance of the nearest star to the sun? And isn't he clearly likening this number to the (unexpected by today's naturalness arguments) small scalar masses? What am I missing here?
He gives that example as a warning that one mustn't 'take numbers obtained from 'naturalness' arguments' too seriously, as people in Copernicus's time thought that distances which were far enough for the heliocentric model to be true couldn't be 'natural'. And how does non-perturbative QFT have anything relevant to the discussion about scalar masses?

35. **Peter Woit**
December 13, 2013

Umesh,

He's explaining that people were making assumptions about distance scales that were wrong, wrong because they had no experimental evidence, and their extrapolations based on their best current theories were wrong.

Non-perturbative QFT is relevant, because we don't completely understand it, and our fundamental theory needs to be understood outside of perturbation theory. During the 1960s people were quite certain that a qft of quarks made no sense, based on extrapolating perturbative behavior. They found out they were wrong, when they better understood what can happen in qft. I think it's quite likely we will sooner or later learn yet more unexpected things about qft, and such things might change radically what the "hierarchy problem" looks like.

36. **Umesh**
December 13, 2013

The statement

"..sooner or later learn yet more unexpected things about qft, and such things might change radically what the "hierarchy problem" looks like."

is so vague, and you don't even point out what this might be makes it hard to take seriously. It looks more like a 'hope' or some sort of 'non-perturbative QFT saves the day' kind of emotion which really doesn't tell one about what to and what not to expect. On the other hand, we know and have measured all the couplings of the SM, and it is clear the Higgs seen at the LHC is WEAKLY coupled, and it's totally unclear to me as to how some claimed 'non-perturbative QFT' might come to the rescue of the quantum corrections that plague such scalar masses. In case you have some concrete 'non-perturbative' avenue that somehow might affect the Higgs sector, it's futile to talk about help from 'non-perturbative QFT'. All those lessons from history are sure nice to keep in mind, but you don't seem to be indicating how to make progress.

37. **Peter Woit**
December 13, 2013

Umesh,

The Higgs potential is very much a non-perturbative phenomenon (the mexican hat is not a paraboloid). All I'm saying is that the quantum dynamics of the Higgs field is not something we understand so well that we are sure we're not missing something here (and I think this is also Wilson's argument). This is an issue that people have certainly studied over the years, but it seems quite conceivable that something is missing. Quite likely actually that something is missing if you want to bring gravity into the picture. No, I don't know what it is, if I did I'd be writing a paper and not wasting time arguing with you…

38. **Umesh**
December 13, 2013

Indeed the Mexican hat is a paraboloid (can be approximated by) near the minimum, where the field rests, and one can perfectly use perturbation theory around this minimum. How is it that the quantum dynamics is 'not understood' in this simple enough regime? And all signs are that this perturbation theory works perfectly well. If there is something 'not well understood' as you claim, where is the proof of this? Any calculation or paper or something telling us this is so? Of course what you say is not Wilson's argument. 'Missing something here' etc etc are simply words, whereas the perturbation theory performed assumming a weakly coupled Higgs gives perfectly satisfactory results. I don't understand what you're talking about.

39. **Peter Woit**
December 13, 2013

Umesh,
This is going nowhere, and I really can't understand why you find it so difficult to see the simple argument Wilson is making (which is the same one I am).

First of all, no, even near the minimum the Mexican hat is not a paraboloid, the potential is degenerate so things are more complicated than that. There are many subtleties in the quantization of a non-abelian gauge field coupled chirally to fermions and to a scalar with a vacuum state in a Higgs phase. Even before you add the scalar field into this, there are serious problems with defining the theory outside of perturbation theory (have you ever looked at lattice gauge theory with chiral fermion couplings?). Adding a scalar field with degenerate minima at non-trivial values and Yukawas (including a Yukawa coupling of almost exactly 1 to the top, 1 is not a number much less than 1…) add more tricky issues. Yes, there is a standard assumption that despite all this, the usual terms in the perturbation expansion capture everything. Maybe it's right, but pretending to people that this is an open and shut case where we understand exactly what is going on and have full control of the approximations being made is just not true.

I don't think these issues can be fully discussed in a blog post though, but I do think you need to answer the question of why you disagree with Wilson (who was someone who I'm sure understood these issues much better than either of us) about this.

40. **Umesh**
December 13, 2013

Whether I disagree with Wilson or your interpretation of Wilson is something we can discuss over a whole new thread, but in my interpretation which I have given above, there is nothing that contradicts what I think about the hierarchy of scalar masses and what Wilson says in his paper you've pointed to. And be sure that many more people, which include people like Seiberg etc (who surely have understood these issues as well as Wilson did, and surely do understand the issues better than either of us) have exactly the same interpretation of the issue with the scalar masses which I have been discussing above. In case you want me to feel threatened by pointing me to some 'QFT giant' who knows much more (indeed he does), and are asking me to submit to some false understanding you want to promote, feel free to, but it doesn't help matters, just like it wouldn't help matters if I point to many of the aforementioned people (equally giants in their own right) and stake my claim to be right. At any rate, physics is not about who thought what, it's about what works and indeed the perturbation theory works and gives correct answers. Your claim about the potential being degenerate doesn't disprove anything; once we pick a minimum and shift the vev we can surely do perturbation theory assuming the potential being parabolic (near the minimum), there's nothing conceptually wrong or invalid about it. All the talk about the subtleties of gauge fields coupled to chiral fermions, the degenerate minima scalar lead nowhere contradict the claim that small scalar masses are unnatural. I think you're using all these words like 'subtleties', 'lattice gauge theory with etc etc' just to deviate from the main issue of is there or isn't there a hierarchy. I am forced to ask you again: do you agree or disagree that small scalar masses are unnatural, and the SM Higgs faces this 'hierarchy problem'?

41. **Peter Woit**
December 13, 2013

If you don't think there are any subtleties about the quantization of the electroweak part of the SM, that all there is to it is the story about how to construct Feynman diagrams that we teach beginning graduate students, fine. I don't see any hope of waking you from your dogmatic slumber.

For about the 100th time: to have a serious, non-speculative hierarchy problem caused by two widely separated scales, you need two widely separated scales where you understand exactly the physics. Right now, we have one: the electroweak scale. Whatever is going on at some conjectural much higher scale we now know exactly as much about as the people in Copernicus's day knew about the stars other than our sun. This is Wilson's point, and mine. Your "hierarchy problem" is based on a speculative picture of what is going on at some scale we know nothing about.

42. **Peter Orland**
December 13, 2013

Umesh,

Maybe I can say this in simple English, without using historical analogies or invoking any sacred names (for or against your point of view).

The "problem" with the Higgs is not a technical problem, but an esthetic one. It is a fine-tuning issue (at energy scales which are experimentally inaccessible). Some people would say, "alright, so we'll tune very finely at some very-high-energy scale. No problem."

Even if you don't like this viewpoint (and I have to admit I'm uneasy with it), it isn't self-evident that it has to be wrong. Experiment is king, not esthetics.

43. **Umesh**
December 13, 2013

I never denied any of your subtleties, I just said indeed they're there, but don't have any effect on the issue at hand, the scalar masses.

"..to have a serious, non-speculative hierarchy problem caused by two widely separated scales, you need two widely separated scales where you understand exactly the physics. Right now, we have one: the electroweak scale. Whatever is going on at some conjectural much higher scale we now know exactly as much about as the people in Copernicus's day knew about the stars other than our sun."

I'm sorry but this statement is wrong. I don't understand why should one understand the **EXACT** physics at the higher energy scale to even talk about it? There's no assumption about the detailed dynamics at that scale at all, please note that the very existence of that scale is enough for quantum corrections to drag observables up to that scale. In fact it happens all the time with quantities that aren't somehow protected by symmetry. If you claim that one needs exact knowledge of the dynamics of the higher scale, you have to show me a calculation or some such, whereas, on the other hand, there are plenty of examples in all sorts of QFTs where one may calculate continuously renormalizable quantities which get receive corrections. In fact, the fundamental lesson of RG is that the high energy effects decouple and cannot affect any of the low energy physics, but surely the quantum effects at a given low energy drag the unprotected quantities to any other relevant scale where new physics must appear. In this case it's the Planck scale, but it could be any other high energy scale in b/w, like the GUT scale. How are you claiming, without at the same time contradicting RG, that one needs detailed knowledge of the high energy scale to say something about the nature of quantum corrections to observables computed at a given energy?

44. **Peter Orland**
December 13, 2013

Perhaps I should amend my straw-man quotation to be, "alright, so Nature tunes very finely at some very-high-energy scale. No problem."

45. **Umesh**
December 13, 2013

Indeed, fine tuning is uneasy. Even if one somehow accepts some amount of fine tuning as OK, how can one say OK to fine tuning to one part in 10^32? Surely you're not advocating that this is possible? Or are you saying that you believe that such things may happen, but for that one needs to know the detailed physics at the highest energy scales? If you're saying that, then I'm willing to fundamentally disagree, because I feel that such impossible cancellations cannot occur. Indeed aesthetics have something to do with it, but it's more than that. I think to ask cancellations of that order requires miracles, which cannot be expected from random integrals. But if you insist, I would be happy to disagree.

46. **Peter Orland**
December 13, 2013

"Even if one somehow accepts some amount of fine tuning as OK, how can one say OK to fine tuning to one part in 10^32? Surely you're not advocating that this is possible?"

Of course it's possible. Just because you don't like it (and I don't like it much either) does not rule it out. Nature doesn't care about our likes or dislikes.

47. **Peter Woit**
December 13, 2013

Hi Peter O.,
That's right, but I also think there's another point, which is what Wilson was getting at, and which theorists typically ignore. Until you have a specific theory at your very-high-energy scale, you don't know what exactly you mean by "tune the theory very finely". Looking at the history of physics, theorist's attempts to extrapolate to energy scales many orders of magnitude beyond what is experimentally accessible have often if not always turned out to be wrong.

Umesh,

You think you don't need anything beyond what is in your textbook to understand what is going on at distance scales many orders of magnitude beyond what we can measure, I think those are great mysteries about which we will someday learn surprising things. Good luck with the multiverse….

48. **Peter Orland**
December 13, 2013

Peter said,

"That's right, but I also think there's another point, which is what Wilson was getting at, and which theorists typically ignore. Until you have a specific theory at your very-high-energy scale, you don't know what exactly you mean by "tune the theory very finely"."

Hi Peter. You are right. We don't really know what we are fine tuning, because we don't know the theory at the high-energy scale. I guess I am just trying to say that without access to this scale, we shouldn't take "solutions" to the fine-tuning "problem" seriously, without something compelling (like experiment). Maybe there is no solution (though I don't advocate this).

I am not arguing against esthetic criteria in science. We need esthetics to lead us to try different models of Nature. I just think esthetics is not a substitute for real information.

49. **Umesh**
December 13, 2013

Indeed one needs much more than what's written in text books. But what you don't tell me is why is it that it's imperative to understand the full physics at the higher energies fully (indeed it would be fabulous to know it, but even without it we can make reliable statements about low energy, and this is why QFT works) even to make some qualitative statement about parameters at low energy. This is textbook stuff for sure, but a very valid and possibly the most important lesson about textbook QFT. Again, I fail to see why one needs detailed knowledge about the high energy theory just to make some statement about stuff at low energies.

50. **Peter Orland**
December 13, 2013

" Again, I fail to see why one needs detailed knowledge about the high energy theory just to make some statement about stuff at low energies."

Not any statement, Umesh. Your statement.

51. **Umesh**
December 13, 2013

You mean the statement about the Higgs mass? It's just another low energy parameter in the SM, and one may make qualitative statements about it just like one can about other loq energy parameters. What's wrong with that?

52. **Peter Orland**
December 13, 2013

The statement about fine tuning.

53. **Umesh**

December 13, 2013

I can't assume that you're speaking for Mr. Woit. At any rate, as I've already stated, I fundamentally disagree if that is what he's proposing. That such impossible cancellations do occur, we must keep our mind open about it, and indeed, to know whether such cancellations do occur one needs detailed knowledge of the high energy theory, which, mind, receives contributions from all scales. Thank you very much for your patience.

54. **Peter Orland**
December 13, 2013

I don't have a blog, so Peter W. doesn't need me to speak for him.

I think the blog post (which I did not write and would not claim to quote from memory) was about the use of historical and esthetic arguments in describing Nature.

It's very hard to be a theoretical physicist without remembering history. It's even harder to study models, without esthetic guidelines. But let's not take this too far. Nature does not care about human affairs, so historical precedent and esthetic arguments must never be accepted as dogma. Only experiment is the final arbiter of fact.

55. **Umesh**
December 13, 2013

As I have already explained, the need for cancellations is more than just aesthetic. It's mathematical, which has no reason to hold. The quantum effects are integrals, and no one would just readily believe that such integrals should produce numbers which cancel to one part in 10^32. And I have also added that I disagree with you (or anyone) who wants to believe in such a miracle. There's nothing more to be said about this. Just in case this is what Mr. Woit means (I don't know for sure until he responds) when he insists on requiring detailed knowledge about the high energy theory, he should've made it clear at the very outset that he prefers cancellations coming from detailed high energy behavior, and thus views the issues about the Higgs mass as 'speculation'. This is not the impression I got from the exchange. In case he disagrees with this, then we're back to the question: why must I have detailed knowledge of the high energy theory to talk about low energy parameters, of which the Higgs mass is but just one?

56. **Peter Orland**
December 13, 2013

Umesh,

Just because you dislike fine tuning doesn't make it a miracle. You're making an esthetic argument, not a technical one.

I'm getting tired of repeating myself. Good-bye and good luck.

Latest on Amplitudes

December 10, 2013
Categories: Uncategorized

This week the Simons Center is hosting a workshop on "The Geometry and Physics of Scattering Amplitudes", talks are available here. Last week they (and the YITP) held a one-day symposium on Trees, loops and precision QCD, based around the work of Zvi Bern, Lance Dixon and David Kosower that was recently awarded the 2014 Sakurai Prize . For more about this, see Dixon's guest post here, or his talk at the symposium.

Bern, Dixon and Kosower started working on amplitudes more than twenty years ago, at a time that it was becoming clear that string theory was not working out as a theory of everything. Calculations in string theory did though lead to interesting new ideas about how to evaluate scattering amplitudes in gauge theory (I see from Dixon's list of publications that in 1994 he wrote something for the SLAC Beam Line on "Whatever happened to the theory of everything?", presumably about this, but now too deep in the past to be available on-line). The three Sakurai Prize winners have been steadily working at the problems of amplitudes in gauge theory and quantum gravity, for many years without getting much attention for their work. About ten years ago, things changed when Witten wrote a paper about getting amplitudes from the "twistor string", a topological string theory in twistor space (the use of twistor space was originated by Penrose back in the late 1960s, and was applied by V.P. Nair to gauge theory amplitudes back in 1988 while he was here at Columbia).

About six years ago Nima Arkani-Hamed entered the subject, where he has had a dramatic effect as an impresario, arguing that this is a route to revolutionary ideas about physics, overthrowing conventional notions of space and time, locality and unitarity, and doing away with the notion that gauge invariance is important. This was partly responsible for his $3 million Milner prize.

For the latest along these lines, a paper with Trnka about "The Amplituhedron" has just appeared, a topic which got wide play in the press earlier this year as Physicists Discover Geometry Underlying Particle Physics, drawing a parody from Scott Aaronson about his own work on the "Unitarihedron" and "Diaperhedron". Arkani-Hamed's talk at the Symposium covers both the ideas of Bern, Dixon and Kosower and his recent work with Trnka. It includes many appreciative remarks about their work, including some interesting commentary on how theoretical physics is done. For instance, on the likely reason for people ignoring their early work:

> It's a natural reaction among theoretical physicists, right? At any given time there's all sorts of interesting things going on, things that other people are doing and things that you are doing and especially if someone else is coming along with something that looks really exciting, in order to justify not dropping everything you have and working on it you have to sort of start inventing these reasons why what they are doing is irrelevant or crap, right? It's a very human thing, a very human thing, a very natural thing. I think everyone does it to some extent, and really good people eventually will realize that they are fooling themselves and start changing their tune if it's appropriate. Really bad people, well, we won't talk about them. It was not at all obvious that this was the tip of a huge iceberg…

There's also:

> Often fields, other fields, have what you might call prophets and there's I think usually an excessive amount of reverence for these prophets, because the prophets tend to have the property that they say some sort of vague things, I won't name any names but you can probably figure out the sort of collection of people I'm talking about. They say some sort of vague things about what might happen with physics in the future, and then twenty years later when other people have done all the hard work and really figured out what is going on and how it works in detail and why it works that way and not another way, if it vaguely looks like something they did, they say "see, I said so all along!" They have a fair amount of attraction, I think it's because a lot of physicists have father-figure issues. But anyway, Zvi and Lance and David were very much not like that, they weren't just vague prophets saying something was going on, they were extremely specific: there was something going on in this area with these kind of computations in this arena and they knew it. And it took a decade or more for many other people in the field to catch up.

(Personally, I have no idea which "prophets" he's thinking of.)

Finally, there were some personal comments contrasting Bern, Dixon and Kosower's low-key style and use of a variety of techniques with his own high-powered hype-driven sales-job of specific ideas to himself and others. Probably a good idea to read this in conjunction with the "Outlook" section of the new paper….

> I must say, and I'm really not just saying this to say it, I'm VERY envious of this, because I AM an ideologue. In my defense at least I can say that I'm a serial ideologue, in the sense that I'll take totally different ideologies and drop the last one without thinking about it, but it's very important for me personally to be an ideologue when I'm working on something and I think, and I'm saying this in all honesty, the difference is talent. If you're really good, you don't have to be an ideologue. You take this, you take that, you're solving for things left and right, you don't care where things come from. If you're not as good, there are 15 million things going on, you're holding on for dear life in the stiff wind of all the crazy stuff going on in the subject. So you have to have a strong point of view about something, you have to have a strong point of view to sort of pursue a particular direction, otherwise you'll get beaten around all the time and get nowhere.
>
> So, usually I'll get up when I talk about scattering amplitudes and give a long introduction about how spacetime is doomed, we have to find some way of thinking about quantum field theory without local evolution in space time and maybe even without a Hilbert space and blah-blah-blah. This is all very high-falutin stuff, this is stuff that Lance wouldn't be get caught dead saying. I think none of these guys would ever say something that sounds so pretentious, but I have to say it, you know I have to say it, because this is the only way I can get up in the morning, and like "I suck again, OK, here we go, I'm doing it because spacetime is doomed, I swear to God, right". But, quite seriously, the best people in the subject have this feature, they don't need to be ideologues, they take the most interesting ideas from every direction they can to make progress, so I really am quite envious.

Comments

1. **Kavanna**
   December 10, 2013

   The amplitude work is very elegant and deep stuff. It had immediate application to QCD amplitudes. But it also has surprising relevance to electroweak amplitudes, once you properly incorporate symmetry breaking. Divergences and leading parts still respect the symmetry and obey the patterns discovered by Bern, Kosower, and Dixon. The pieces that break the symmetry are nonetheless related to each other in a deep way dictated both by the symmetry and by how it's broken (by which non-singlet irreducible representation the vacuum is).

   The amplitude stuff does not in any way overthrow gauge symmetry. It does show that gauge symmetry in amplitudes is a "projection" or shadow of something geometric. It's remarkable that this work has survived the Age of Cliques and Hype.

2. **anonymous coward**
   December 10, 2013

   Did you REALLY not know which physicists he might be talking about, or are you just playing the consummate professional?

Also, is it possible to make your “I am not a spammer” thing a little more noticeable? It’s about two shades darker than the stark white background, and I like to wear sunglasses indoors at night.

3. **Peter Woit**
December 10, 2013

anonymous coward,

Honestly, don’t know who he’s thinking of. I’m aware of people who make vague, grandiose claims they can’t back up, but in my experience while people like this may think of themselves as prophets, they have few followers.

Have at least temporarily disabled that anti-spam gizmo to see what happens. Not clear if it was still effective, we’ll see…

4. **Igor Khavkine**
December 11, 2013

The last quote seems to be an exceptionally honest and genuinely self-reflective moment from Arkani-Hamed. I hope it will be appreciated.

5. **Peter Woit**
December 11, 2013

Igor Khavkine,

I do think the last quote from Arkani-Hamed is quite interesting, but I think there’s a lot of false modesty there: I don’t think he really believes that Bern, Dixon and Kosower are more talented than he is. I also don’t think his distinction of them as using a variety of ideas and techniques in their work is a significant one, since he and most theorists will happily use whatever tools they can get their hands on.

What is really interesting though is his self-reflective description of himself as an “ideologue” and his explanation of why he is that way. This kind of answers a question that has often come up in my mind when I hear one of his talks: “how can a smart guy like that engage in such vigorous hype, with crude arguments avoiding acknowledging obvious problems?” He provides a good answer here: this is what he needs to do to justify to himself continuing every day to do difficult and often unrewarding work. This I think is very honest, much more so than usual platitudes from scientists about how much they enjoy the process of discovery.

6. **Igor Khavkine**
December 11, 2013

Peter, we’re on the same page here!

7. **JohnB**
December 11, 2013

That is a very interesting (and honest) comment from him alright, regarding why he is an ideologue – I’d say it carries much weight with what I said regarding economists in my comment on the previous post (except I’d say economists academic careers depend upon never admitting such a personal fault).

I’d say agnotology among the physics community (and scientific community in general), would make for an interesting study.

It makes sense that people have to let themselves dream/hype a bit, in order to generate the enthusiasm/motivation they need for their work/study in theoretical fields like this, and it’s a good sign that people can openly admit/acknowledge that – having a safe atmosphere for such disclosure, would be important I’d say, in allowing room for reform.

8. **Bernhard**
December 11, 2013

“About ten years ago, things changed when Witten wrote a paper about getting amplitudes from the “twistor string”,”

It is hard to distinguish if this is genuine or sociological effect. Well, probably a combination of the two. I mean, would it have changed if it someone else, and not Witten, had written the paper? But then again, could someone else have done it? Not an expert on any of this, just speculating.

9. **Peter Woit**
December 11, 2013

Bernhard,

If you look at Dixon’s blog post I linked to, he describes what happened after Witten’s paper as:
“Smart young physicists like Ruth Britto, Freddy Cachazo, Bo Feng, Radu Roiban, Mark Spradlin and Anastasia Volovich flooded what had been a mostly deserted “amplitudes” landscape. ”
I don’t think that this would have gotten anywhere near the same attention at that time if the “twistor string” idea hadn’t been coming from Witten. Having Witten point out a field where few people were working, there were interesting calculations to do, and some kind of connection to string theory, got a lot of attention at a time when not much else was happening and people were looking for something to do.

In the end, I don’t think the “twistor string” part of the idea itself turned out to be that useful, but it led to various new ideas about amplitudes that were important, including the papers http://arxiv.org/abs/hep-th/0412308 and http://arxiv.org/abs/hep-th/0501052 (“BCFW recursion relations”).

10. **MathPhys**
December 13, 2013

I have learnt the very hard way that any subject that Witten works on must be (at least) mathematically interesting, and that any subject that he stays away from is possibly worthless.

11. **Cosmonut**
December 14, 2013

I guess Stephen Hawking would top the list of “vague prophet” making incredibly grandiose general claims which have all fallen flat.

While string theorists get a lot of flak for hyping their field, it must be remembered that Hawking started the trend of announcing the imminent arrival of a “Theory of

Everything” which would enable us to “know the Mind of God” and so on….

In fact, he claims that the “grand theory” has already arrived in the form of M-Theory – though I don’t think anyone takes him seriously anymore.

2014 Milner Prizes

December 11, 2013
Categories: Uncategorized

Last March an Oscar-style ceremony hosted by Morgan Freeman was held in Geneva (see here) to award the 2013 $3 million Milner Prize to Princeton string theorist Alexander Polyakov. Tomorrow an even more lavish ceremony designed to turn "Oscars of Science" into instant multi-millionaires will be held in Mountain View, California (see here). It will feature Kevin Spacey, Conan O'Brien and Glenn Close, one of whom will presumably award the 2014 $3 million Milner string theory Prize to either Polchinski, Green/Schwarz, or Strominger/Vafa.

If I had to bet I'd go for Polchinski, purely because if they don't give it to him, that will be two years in a row he walks away with a $300,000 consolation prize, and having to have him a third time up next year before getting his $3 million would be a bit silly. On the other hand, John Preskill is predicting Green/Schwarz, and he may be right. If you're going to have a prize devoted to the idea that string theory = fundamental physics since it's our hope for a TOE, then one really has to give it to Green/Schwarz for originating the whole superstring = TOE business.

On Friday, there will be a day-long symposium at Stanford sponsored by the Milner prize people (see here), with the $3 million man (or men) speaking at 5:30pm, introduced by Lenny Susskind.

Physics will actually be a relatively small part of this awards ceremony, since it will also include the award of six $3 million awards in the Life Sciences. These are being jointly funded by Milner and a group of other prominent internet entrepreneurs.

Update: News is that the awards ceremony will be broadcast by the Science Channel:

> Hosted by actor Kevin Spacey, the awards will be presented by the Prize sponsors and by celebrities including Conan O'Brien, Glenn Close, Rob Lowe and Michael C. Hall. The event was produced and directed by Don Mischer, the producer and director of The Academy Awards among other television and live events. The world premiere special 2014 BREAKTHOUGH PRIZES will premiere on Science Channel on Monday, January 27 at 9 PM ET/PT.

According to the press release, Polchinski, Green/Schwarz or Strominger/Vafa will get $3 million for being "psychics":

> The 2014 Breakthrough Prizes are awarded to those who make major breakthroughs and contributions that represent significant advances in our fundamental knowledge of the world. At the ceremony, seven prizes (six for life sciences and one for psychics) of $3 million each will be awarded for a total of $21 million.

Update: As John Preskill predicted, the $3 million string theory prize went to Green and Schwarz. Polchinski gets a second $300,000 consolation prize and another chance next year.

Update: Vanity Fair covers the event as Hollywood Stars Gather in Silicon Valley for 2014 Breakthrough Prizes in Physics and Life Sciences.

Comments

1. **paddy**
   December 11, 2013

   Will the winner "give us tongue"?

2. **M**
   December 12, 2013

   Opening the possibility of imagining D-branes here and there has been an important factor in convincing that string theory has no predictive power. This key achievement deserves more that just a few millions

3. **Thomas Larsson**
   December 12, 2013

   Whatever the lasting value of Sasha Polyakov's contributions to string theory, the application of CFT to 2D phase transitions is experimentally confirmed and definitely prize-worthy. I have long thought that BPZ should receive a Nobel for this.

4. **Bernhard**
   December 12, 2013

   An unserious ceremony for an unserious prize. Fair enough…

5. **Hack**
   December 12, 2013

   What an utter circus! So now physicists want prizes given to them by Hollywood celebrities? Fantastic, hopefully next year the prize can be given out by Miley Cyrus who can recreate her MTV dance with the prize recipient! That or perhaps Kim-Yay (thats Kim Kardashian and Kanye for those of you living under rocks) could clear their busy schedules and to it. I don't know that is a real toss up…

6. **tt**
   December 12, 2013

   twerk = force x distance

7. **P**
   December 12, 2013

   M,

   What you say makes no sense. Imagining a D-brane "here" or "there" is really just the statement that there are typically D-brane moduli spaces. In many cases these are, via duality, dual to the moduli spaces having to do with sets of closed string modes.

So even if I granted your naive notions about difficulties of string theory — which themselves have issues — your argument re: D-branes making things worse is clearly wrong.

Cheers,
P

8. **M**
December 12, 2013

dear P,
before 1995 the attempts of connecting physics with strings were restricted to heterotic strings compactified on some Calabi Yau. D branes expanded the (already too large) list of possible constructions. This gives new ways to search a string model that reproduces all observed physics. But, more importantly, it makes the issue irrelevant: for any observed physics there are ≈ 10^300 string models that reproduce it.

I advise Milner to offer a prize of 10^-300 dollars for any "theory of everything".

9. **harryb**
December 12, 2013

And I quote from Edward O Wilson's new book, Letters to a Young Scientist (he being a great Life Scientist at Harvard):

"If a subject is already receiving a great deal of attention..if its practitioners are prizewinners who receive large grants, stay away from that subject….in making your own long-term plans, be aware it is already crowded with talented people. You would be a newcomer, a private amid bemedaled first sergeants and generals. Take a subject instead that interests you, and looks promising, and where established experts are not yet conspicuously competing with one another, where few if any prizes and academy memberships have been given, and where the annals of research are not yet layered with superfluous data and mathematical models. "

Lets hope some look at these prizes as an end of time for ST, and aim for other paths, make their mark in other ways.

10. **P**
December 12, 2013

Hi M,

Dualities between theories with branes and the heterotic string were precisely the ambiguity I was talking about.

Two more corrections: string model is ambiguous, you should call them string vacua. Second of all — and this is what I was taking issue with — is that the large number of vacua isn't from the D-brane moduli space (i.e. the ability to put a brane "here" or "there" as you say), it's from the Ramond Ramond background flux utilized in moduli stabilization. So, again, the 10^large is *not* from the D-branes.

Cheers,
P

11. **Mitchell Porter**
December 12, 2013

M says

"for any observed physics there are ≈ 10^300 string models that reproduce it"

In our present state of knowledge, an estimate like that above, is simply a guess – a guess which could be completely wrong.

In principle, once you choose a particular string vacuum, all the low-energy parameters become calculable. In practice, the ability to calculate such quantities is still very limited – e.g. see the opening paragraphs of [this report](#) from "String Phenomenology 2012".

12. **Peter Woit**
December 12, 2013

P, M, M-P,
Sorry, but this particular discussion is neither very interesting nor on-topic. About M-P's claim that all the low energy parameters of physics in a string vacuum (as opposed to some very specially chosen vacuum) are in principle calculable, with a link supposedly proving this, the link in question actually has

"in many string models, we don't actually have enough control to calculate physical coupling constants; often an overall proportionality factor is missing, and simply assumed to be 'of order one' (i.e. of magnitude between about .1 and 10). I think this is a very important point; 'string phenomenology' as it stands is a bit of a misnomer, because as far as I know, nobody has yet been able to do an honest calculation of all quantities like masses and coupling constants in a realistic string model."

which reads to me more like saying things are not calculable. But, anyone who wants to argue this one way or the other will have to do it somewhere else. Mercifully, even those string enthusiasts in charge of choosing Milner prize winners don't seem to think it's a good idea to give them for string phenomenology.

13. **chris**
December 13, 2013

it is astounding what people will do for money.

14. **Zhiming Wang**
December 13, 2013

The prize went to Green/Schwarz after all. *The Guardian*.

15. **martibal**
December 13, 2013

Now they will do it for maths as well…. Is that great or frightening ?

"The prize is one of a string of annual awards set up by Milner and other Silicon Valley stars to raise the profile of scientists and put them on a par – in some sense, at least

– with film and sports celebrities.” That is frightening, no ?

16. **Simple biologist**
December 13, 2013

Looking at life science winners is depressing as well. With the exception of Alexander Varshavsky, it’s all biomedical or biotech stuff. Their definition of life science break trough appears to be application of basic research to medical or biotech uses.

Basically they are awarding just the kind of research that Nobel prizes already cover, without the Hollywood celebs of course. Not sure why they waste all that money to compete with Nobel prizes. They could have used it to award biological fields outside the narrow scope that the chemistry and medicine Nobel prizes allow, evolutionary biology, ecology and so forth. Then again, maybe it’s better not to have this ridiculous hype.

17. **Low Math, Meekly Interacting**
December 13, 2013

I would like to thank tt for something to laugh about. The rest is pretty depressing.

18. **jd**
December 14, 2013

I find all these discussions about prizes, awards, and such very depressing. This severe sickness in the sciences and in our society does need to be discussed but I usually leave that to others. But today I do have a couple of thoughts. An APS Fellow I know, once pointed out to me that to become a fellow, or receive a prize, or award, or honor does not mean that you received it for your talent, skill, accomplishments. It means a good campaign was waged. This is true for science, the Academy Awards, the US presidential race, and the list goes on. Notice that after someone wins the presidency the analyses are not about whether the best person won but is on how the winner had the best campaign. In this blog before the Nobels were announced this year it was mention that campaigns were in progress, not necessarily by the candidates but by their supporters. I know of one Nobel prize winner who went around Europe and somehow got various national legislative bodies to pass do nothing bills in support of him for the prize. This was years ago. He got the prize in spite of the letters written by a number of prominent people in his field urging that the bills be ignored. I know of a US institution of international reputation that wanted to hire a certain researcher and promised that, if he came, they and their professors would throw their weight behind him for the Nobel. It is debatable that his work was of that caliber, but he did get the prize. This all is embarrassing, depressing, and in the long run hurts science and society. The argument has been made that maybe the money, the “honor,” may encourage bright young people to go into science. I ask whether that would have made a difference to any of you? If it does, then obviously you made a mistake. I personally knew, in years past, of young people, some bright, that dropped out of physics after their PhD’s because they felt that the field had changed and no longer could offer them that solid, interesting, career in teaching and research. Most of them went into medicine, law, and finance where, if money is your motivation, that is where the real money is. As for myself, so you know, I have not received any honors of any import. Years ago I had a superior and a coworker each separately offer to put me up for fellow. I thanked them and told them no, that the offer was enough honor. I looked over the current fellows and did not want to be associated with most of them. It really was not an honor. I am not at all comparing myself with Perelman, he is far, far beyond me. But I do admire his integrity with respect to prizes, at least as far as I have read in articles. And for those that take his decisions as a slam against them, so be it. Maybe they should look within themselves.

19. **Noah Smith**
December 19, 2013

Milner prize = most lavish consolation prize ever

20. **einstein**
January 1, 2014

I hope Terence Tao win Milner prize 2014

Milner-Zuckerberg Prizes for Mathematics

December 13, 2013
Categories: Uncategorized

At the Hollywood-style awards ceremony last night for $3 million string theory and biomedical research prizes, it was announced that Yuri Milner and Mark Zuckerberg will now start funding something similar in mathematics, called the Breakthrough Prize in Mathematics. According to the New York Times:

> Yuri Milner, the Russian entrepreneur, philanthropist and self-described “failed physicist” who made a splash two years ago when he began handing out lavish cash awards to scientists, announced Thursday that he was expanding the universe of his largess again: This time, he will begin handing out $3 million awards to mathematicians…
>
> For the new math award, Mr. Milner and Mr. Zuckerberg, the co-sponsor of the math prize, will decide who gets the money, in consultation with experts. Mr. Milner declined to say how many mathematicians would be chosen, but there could be quite a number of windfalls in store: for the physics price, there were nine inaugural winners, and for the life sciences prize, there were 11.

I’ve written extensively about the “Fundamental Physics Prize” and what I see as the worst problem with it (heavily rewarding and propping up a failed research program). While many physicists are privately unhappy about this prize and its effects, few prominent ones are willing to speak publicly with their name attached, since this kind of mouthing-off could turn out to be personally extremely expensive. Ian Sample at the Guardian has a story today, which quotes a “prominent physicist who did not wish to be named”:

> One prominent physicist who did not wish to be named said the huge sums of money could be used better: “The great philanthropists of the 19th and 20th centuries, like the Rockefellers and the Carnegies, did not create prizes – they created universities and research institutes that have enabled thousands of scientists to make great breakthroughs over the succeeding decades.
>
> “By contrast, giving a prize has a negligible effect on the progress of science. A few already well-recognised people get enriched, but there is little value added in terms of the progress of science compared to the multiplier effect of creating new institutions for scientific research.”

The Guardian does quote one critic by name, but it’s just the usual one.

The physics prize has turned out to be extremely narrowly targeted at one particular subfield of physics, and from what little I know of the life sciences, the prizes in that area seem to be also narrowly targeted (US biomedical research aimed at curing diseases that most afflict those in the developed world). I’m highly ignorant about life sciences research, but it seems striking that the 6 $3 million winners in this field were all men.

I have no idea how Milner and Zuckerberg will go about choosing the $3 million winners in mathematics, and whether this new prize will end up being narrowly targeted to a certain sort of mathematics research. If so, it may have very significant effects on what kinds of mathematics get done. Based on the other prizes, it seems likely that the winners will be mostly prominent US academics, people already well-rewarded by the current academic star system. I don’t see any reason to believe that these kinds of financial awards will allow such mathematicians to do work they wouldn’t otherwise do, so the main argument for the prizes is that the money (and Academy Awards-style ceremonies) will help make them celebrities, and that this is a good thing. One can predict that public criticism from prominent US academics may be rather muted once the checks start coming.

Even if the Milner-Zuckerberg prize does end up focused on the best mathematics research, I still think the whole concept is problematic. The US today is increasingly dominated by a grotesque winner-take-all culture that values wealth and celebrity above all else. While mathematics research, like the rest of academia, has been affected as a star system has become increasingly part of the picture, this field has been somewhat immune to celebrity culture. While people typically think that what mathematicians do is perfectly respectable, they don’t understand much about it and aren’t especially interested. Milner and Zuckerberg want to change this by turning mathematicians into celebrities, but I don’t see any reason to believe this is going to lead to better mathematics.

Update: Here’s the statement from Milner about the planned mathematics prize:

> Yuri Milner said: “Einstein said, Pure mathematics is the poetry of logical ideas. It is in this spirit that Mark and myself are announcing a new Breakthrough Prize in Mathematics. The work that the Prize recognizes could be the foundation for genetic engineering, quantum computing or Artificial Intelligence; but above all, for human knowledge itself.”

Comments

1. **Hack**
   December 13, 2013

   Great! It is not enough to add difficulties to theoretical physics research alone, now these prizes will cause problems in the life sciences and mathematics fields. Scientists need to grow up and put a stop to this, but we all now that will not happen. What ego-driven hard working researcher would turn down $3 million or the chance of $3 million, despite the fact that these prizes are going to hurt their respective fields in the long run. This is incredibly disappointing.

   By the way Peter, your right, it is rather amazing that no women were awarded the $3 million dollar prize in the life sciences. I am probably more familiar with that field than most of your readers and there are many brilliant and successful women researchers. One would think somebody on the prize committee would have been smart enough to think that not awarding any women the prize might lead people to believe their chauvinistic…

2. **John McAllison**
   December 13, 2013

   Who on earth came up with this clueless idea that physicists and mathematicians should be patronized by Vanity Fair magazine and presided over by actors such as Kevin Spacey at an awards ceremony?

   Let the awards be given out by those mathematicians and scientists their peers look up to, or perhaps a representative from the awarding organization itself.

   This is just adding further to the negative status of science and mathematics relative to the entertainment industry.

3. **BCnrd**
   December 13, 2013

   Jim Simons has taken a more productive/thoughtful approach in his philanthropy for mathematics: some serious long-term prizes without Hollywood distractions (with the requirement that they be used in a substantial way to support research activities), and a substantial grant program to help working mathematicians (not just super-stars) in constructive ways via summer support, sabbatical support, conference support, etc. It is quite a contrast with the approach chosen by Milner and Zuckerberg (whom I am sure have good intentions, and hopefully can be convinced to modify their plans so as to have a greater benefit to mathematics; the big prizes seem unlikely to encourage increased government support for research mathematics).

4. **Peter Woit**
   December 13, 2013

   BCnrd,

   Thanks, I very much agree. Simons has provided an excellent model of carefully thought out plans to use some of his wealth to further mathematics research. It would be great if Milner and Zuckerberg would take their inspiration from his model.

   This is likely a pipe-dream, but it really would be wonderful if the mathematicians Milner and Zuckerberg consult with about this as they start their process of choosing prize winners would politely tell them something like "we appreciate the thought, but we wish you wouldn't do this, and would find some other better way to support mathematicians and their research. You might want to talk to Jim Simons…"

5. **kashyap vasavada**
   December 13, 2013

   I think criticisms of Milner-Zuckerberg awards are unwarranted. On the one hand we complain that society awards big money to sports figures, entertainment celebrities, CEOs and hedge fund managers and ignores talented scientists and mathematicians. Then when some visionaries like Milner and Zuckerberg put large sums of their own money, we complain that it is not going to be good for science and mathematics! We cannot have it both ways! I agree people may have different opinions about which sub field deserves prizes. But there are differences of opinions about Nobel Prize also. Admittedly Nobel Prize is given for some successful previous scientific research. But I do not see any problem in awarding promising scientists and mathematicians. Also we don't tell CEOs of hospitals and pharmaceutical companies that their salaries could be used for cheaper health care!! So better use of money is also not a particularly great argument.

6. **peter**
   December 13, 2013

   I prefer Simon's effort to Milner's. But maybe we should not blame Milner. The point is why those fpp winners not spend their prizes in supporting more other young people. Since they have not made it, this means this prizes are at least important for their personal lives.

7. **Peter Woit**
   December 13, 2013

   kashyap,

   Actually, if I could get any of them to listen to me, I'd very happily "tell CEOs of hospitals and pharmaceutical companies that their salaries could be used for cheaper health care."

8. **DaDa**
   December 13, 2013

   Peter you are too cynical.

9. **Peter Woit**
   December 13, 2013

   DaDa,
   Actually I think the standard criticism of someone like me who objects to the influence of large sums of money is that I'm too idealistic, not that I'm too cynical…

10. **Simple biologist**
    December 13, 2013

    @Hack
    You're absolutely right about the gender issue.

    To add, if I needed to name the one field of science that has the best level of funding, enjoys already the highest support from the general public, is by far the most over hyped field, and was in the least need of big bags of cash & PR from eccentric billionaires…it would biomedical research into first world diseases.

    (I know it's off topic for the site and I already wrote about it in the earlier Milner topic. Sorry Peter)

11. **Hank Damage**
    December 13, 2013

    kashyap vasavada:

    We don't "complain that society awards big money to sports figures, entertainment celebrities, CEOs and hedge fund managers" at all. We complain that the money is distributed unevenly or at least not according to merit among the respective groups, which is exactly what's happening with these M-prizes again.

    Another thought/hypothesis: Convincing Milner to do the funding in a reasonable manner is impossible. Because despite claiming, that the prizes are there to promote "human knowledge itself", it is much more likely that this failed – yet obviously ambitious – physicist was trying to convert the currency he had into social currency aka said fame. Successfully too, just telling by the numbers of mentions he got in this respectable blog.

    DaDa, it's either cynicism or somatic bliss. People choose whatever keeps them going.

    Well, none of this was new, but thanks for listening.

12. **Sam Lewallen**
    December 13, 2013

    I had a thought like Peter's: especially if there are multiple inaugural math prizes, maybe there would be some way that all the winners could be convinced to pool their prize money and fund something beneficial to mathematics, such as a research center. It seems a long shot, but I'd think math would be the field most able to pull this off. I think this would generate a really interesting media response as well, and maybe send a message to Milner et al.

Maybe Jim Simons could make an open offer to the award recipients, that he would match their donations to fund a new research center somewhere (his foundation could lend their expertise in making this happen)? That would really be a neat situation. Anyone think something like this is even conceivable? Could we start a petition?

13. **Allan Rosenberg**
December 13, 2013

Certainly Edward Witten, Maxim Kontsevich, and Nima Harkani-Amed (for Amplitudhedrons or whatever they're called) should be on the short list for the Math prize. Is this one going to be awarded via Facebook?

14. **Bee**
December 14, 2013

I'm not prominent but for what it's worth I've stated my disapproval about the Milner prize here http://backreaction.blogspot.de/2013/01/private-funding-for-science-good-idea.html

15. **Navneeth**
December 14, 2013

String math, Quantum Computing, Number Theory and Cryptography. Those topics have the most visibility in popular literature, right?

16. **Zwirko**
December 14, 2013

I hope layman questions are welcome here?

Do these cash prizes come with terms and conditions? That is, are they to be used for research purposes? Fund students or buy materials and equipment? Or can the recipient use the money to buy a Ferrari or two? Are there any stories of what people have spent the money on?

I'd feel very uncomfortable if I won several million for my research and decided to keep it for myself. I think my conscience would force me to either share it with colleagues or donate to the lab.

17. **other**
December 14, 2013

This is nothing to worry about. Start being concerned when a subject develops a "priesthood" and canon of knowledge …

18. **Peter Woit**
December 14, 2013

Zwirko,

The prizes have no restrictions, people can use them for whatever they want. I know nothing about what the biomedical research people have done with the money so far, they generally have large labs that could use the money.

Among the physicists, most of the experimentalists seem to have publicly announced they are giving the money away, to various projects to support young people, teachers, etc. In these cases, the recipients were mostly spokespersons for their experiment, so it was kind of odd for them to be getting money personally. The research of the theorists is already very well supported by grants, so I don't think there's a viable way for them to spend any significant amount of the money on their own research. On the whole, there has been little news from the theorists about what they are doing with the money. If they're giving it away they're doing so privately.

19. **CU Phil**
December 14, 2013

I think Witten gave a significant portion of his to J Street, if I remember correctly.

20. **Deane Yang**
December 14, 2013

Peter et al, I think the Milner Prizes *do* benefit mathematics and mathematicians but in a very different way than what Simons does and in a similar way to the Nobel Prizes.

The Milner Prize, like the Nobel Prizes, does little directly to benefit scientific research. Both are given to prominent established scientists unconditionally, often after their best work has already been done. In that sense they are both relatively useless, and that's the focus of much criticism.

But for me the huge value they add is the attention it attracts to the great things done by mathematicians and scientists. Most media attention is devoted to the most "sexy" advances, which usually sound better than they are and draw attention to people who don't necessarily deserve it. "Cold fusion" is the one that comes immediately to my mind. Although we can all criticize the choices made by the prize committees, there is no question in my mind that these prizes draw attention to at least some of the best in their respective fields and their accomplishments.

I know you're frustrated by the Milner Prize being awarded to string theorists, but no matter what we think of the theory itself, there's no question that these are, for better or worse, the best and brightest in physics today.

21. **Jesper**
December 14, 2013

@ Deane Yang

"there's no question that these are, for better or worse, the best and brightest in physics today."

By what measure? If they all work on the wrong theory (which, of course, remains to be determined), then I don't think they deserve this distinction. Until we know what is up and what is down I certainly think there is a question.

22. **Peter Woit**
December 14, 2013

CUPhil,
While Witten was quoted as saying he would donate some money to JStreet, I'm not sure about how "significant" this is. He got the prize in mid 2012, in the JStreet 2012 report he's listed as a $10,000 donor, but not a $25,000 donor. If he did give the whole $3 million to JStreet, he would quite significantly change the organization, roughly tripling its assets.

23. **Peter Woit**
December 14, 2013

Deane,
At this point I don't think we have any way of knowing one way or another how the Facebook prize (I think I'll refer to it that way because that's where most of the money is coming from) will affect mathematics. It's quite possible that even Milner and Zuckerberg haven't figured out how they are going to make these awards.

Based on the history so far though, I think there is plenty of good reason for the math community to be worried. In both the physics and life sciences cases, the awards are targeted at a narrow part of the subject, one that already gets the most attention. Mathematicians may wake up one morning a few months from now to find that some narrow, already well-funded and hyped sub-field of mathematics has been identified as the one that's relevant to what really matters ("genetic engineering, quantum computing or Artificial Intelligence", according to Milner), and from that day forward, that's going to be the sub-field that gets attention and resources. Yes some mathematicians will become wealthy, maybe even celebrities, and get to hang out with Kevin Spacey, Conan O'Brien, etc. but will this really have a positive effect on mathematics?

Part of my reason for skepticism about whether bringing celebrity culture into science or mathematics is a good idea is based on seeing what it has done in theoretical physics, where, personally I think it has had a strongly negative effect. This has partly to do with independent problems of the subject, but I just don't see much evidence of the supposedly positive things happening. One standard argument is that this encourages young people to go into STEM careers, but I just don't believe that too few people interested in such careers is the problem. Rather the problem is more people than jobs of this kind, and celebrity culture may just change the mix of reasons why people go into these fields. Will we really be better off if fewer people become mathematicians because they deeply love mathematics and more do it because they want to meet Conan O'Brien?

24. **Peter Woit**
December 14, 2013

Deane and Jesper,

I'd change Deane's "the best and brightest" to "some of the best and brightest". Again, Milner's prize is aimed at a narrow slice of theoretical physics. Witten is definitely about "the best and brightest" around by most measures. On the whole though, the rest of the Milner prize winners, while talented and accomplished, are not significantly more so than many others in the field. As an example, consider the quote I posted here recently from Milner prize winner Arkani-Hamed about Bern, Dixon, Kosower, where he describes them as much more talented than himself. One might want to discount that as false modesty, but I do believe they're equally talented, accomplished and hard-working. Unlike Arkani-Hamed, they chose to work on less popular topics, and not to hype the significance of their work. They may of course in the future get on the Milner prize list, especially since Arkani-Hamed has been on a hype campaign for a few years now for their subfield.

An interesting example is this year's winners, Green and Schwarz. Back in the late seventies/early eighties, they were very admirably working on a quite unpopular topic (superstrings). Schwarz back in 1984 was a 43 year old research associate, without a permanent job (or even a tenure-track one). Post-1984 (Green-Schwarz anomaly cancellation and the ensuing "First Superstring Revolution) he and Green became highly-rewarded celebrities of the field. From the vantage point of 30 years later, it now is clear that the great hopes of 1984 were misguided, this is an idea about unification that hasn't worked (and likely never will). So, yes, they're good theorists who 30-40 years ago did good work on an unpopular idea, one that turned out to be misguided once it became popular and much better understood. Other than the fact that their ideas ended up getting overhyped, there's nothing else to distinguish them from a fairly large group of theorists active during the late seventies and eighties who worked on various ideas which either never worked out, or never got enough attention for anyone to know whether they might work out.

25. **Bernhard**
December 14, 2013

"Among the physicists, most of the experimentalists seem to have publicly announced they are giving the money away"

The experimentalists might have publicly announced giving the money away, but as far as I know, so far only two out of seven (Virdee and Incandela) actually did what they said.

26. **Bob Jones**
December 14, 2013

"If they all work on the wrong theory (which, of course, remains to be determined), then I don't think they deserve this distinction. Until we know what is up and what is down I certainly think there is a question."

You act as if we're all awaiting the results of some big experiment that will tell us whether these theories have any value. What you don't seem to understand is that these ideas are already known to be important. The ideas that these scientists are working on are all very coherent physical ideas which have taught us a lot about how nature works. They have remarkably rich mathematical structure and, in some cases, strong support from experiment. There's really no question that these are the right ideas to be looking at, and every one of these scientists is certainly among the best and brightest in the world.

27. **Peter Woit**
December 14, 2013

Bob Jones,
"There's really no question that these are the right ideas to be looking at"
You're doing a good job of illustrating the what's wrong with the Milner prize for physics, but, another round of string theory rules/string theory sucks is both off topic and a complete waste of time. Enough on both sides, will delete any more.

28. **Bob Jones**
December 14, 2013

Peter,

I thought you made a very good point in your earlier post when you said that the amplitudes people deserve more recognition. My reference to the "right ideas" was not meant to exclude this sort of work, which is exactly the sort of mainstream research that might one day be recognized with a Milner prize.

29. **martibal**
December 14, 2013

Peter,

maybe a thing that may “save” mathematics for being corrupted by prices is that, at the end, one needs a proof. One can have millions of dollars, hang out with K. Spacey, at the end what will convince the community is your ability to prove theorem. Hyping has no real meaning in maths, unlike what may happen in physics. Nobody can claim that “his approach to Riemann hypothesis is the only worth pursuing”, like B. Jones roughly said about string theory. Either you prove it (and this does not mean that your proof is the best one can imagine), or you do not prove it. People like Perelman or Wiles are good examples of “anti-hype” mathematicians, I would say 🙂

30. **martibal**
December 14, 2013

Schwarz back in 1984 was a 43 year old research associate, without a permanent job (or even a tenure-track one).

The best piece of news I have read for long time. So there is still some hope 🙂

31. **Jon Tyson**
December 15, 2013

All the people upset about how the billionaires give away their money should get their own philanthropic organization. Public complaints, some of which will certainly get back to the donors, can only be described as obnoxious.

32. **Marcel van Velzen**
December 15, 2013

It must feel really good to get a fundamental physics prize from a “failed physicist”.

33. **Anonyrat**
December 15, 2013

Twenty years ago, I heard a talk by Bern. I thought Bern was on the right track, and today if I was a physicist I would want to be someone like him, who has done more to elucidate the world we actually live in, not the world we might live in, than some big name physicists.

34. **Peter Woit**
December 15, 2013

Jon Tyson,
Billionaires can do what they want with the billions (within the law), but non-billionaires, who can’t do the same thing, have every right to obnoxiously criticize. That this criticism of their plans might get back to them doesn’t seem to me like a problem.

35. **Jay**
December 15, 2013

Congrats to the winners.

Peter, I understand some your concerns (physics almost equated with research on string theory, biology almost equated with research on cancer), but I still don’t understand how negative is your view about these prices.

Or, more precisely, I don’t understand how your rant should not apply to the prices that were setted by another nouveau riche, M Nobel. The main difference I can see is the Nobel price is in practice restricted to the oldest of our peers (but as you know we can’t blame Nobel -it’s clearly against its will), whereas Milner’s prices are given to active researchers, who are moreover free to do whatever they want with the cash. This is a freedom we hardly see anywhere else.

All in all, your main concern seems that it may drive students toward the wrong directions, even when these directions have clearly failed. Seriously? Were you yourself influenced by the Nobel price when you were to choose your carreer path? Do you know anyone in this case?

36. **Peter Woit**
December 15, 2013

Jay,
At least as far as the physics Milner prizes go I don’t see much difference in age distribution with the Nobel. The last few winners (Polyakov, Green, Schwarz) are all around retirement age. It seems that Milner in some sense wants to compete with the Nobel, awarding Nobel-type prestige to those ineligible because their ideas cannot be experimentally tested (or have been tested and failed).

About the proposed math prize, it’s unknown how it will work, but I just don’t see anything positive for math research coming out of it, and serious potential for negative effects.

Yes, the Nobel prize did affect me, and I think it affects most people’s view of a scientific field. Not by making me want to be a physicist so that I could get a prize, but by identifying certain people and their ideas as the most distinguished and most worth paying attention to. When one is trying to understand a subject, one has to make decisions about what to pay attention to, and the Nobel prize is a marker identifying a scientific consensus about the best work and most important ideas. The Milner prize is intended to play that role, indicating that string theory and other ideas it rewards are the best and most important ideas about fundamental physics. Whatever Milner and Zuckerberg decide is the most important part of mathematics will be destined for the same treatment.

37. **jonah**
December 15, 2013

It might be more of an applied math prize rewarding stuff out of scope for a Fields Medal or Abel prize. Statistics, signal processing, data mining… That would be welcome IMHO, there are several SIAM prizes already but something with more public visibility isn’t a bad idea on paper. Let’s see how this enfolds…

38. **Bob Jones**
December 15, 2013

“awarding Nobel-type prestige to those ineligible because their ideas cannot be experimentally tested”

Do you really think it’s fair to say that someone like Kontsevich is doing untestable work? Sure, his ideas will never be tested in an experiment, but that doesn’t make them any less important. These are formal ideas that aren’t even subject to testing. When you use this kind of language, you’re just misleading the public and stigmatizing some of the world's top scholars.

39. **Peter Woit**
December 15, 2013

Bob Jones,
Kontsevich is kind of a special case, the only mathematician to get a Milner prize. Yes, the test of good mathematics is not experimental testability.

I'll stand by the statement you quote, it's accurate and not misleading in the least (unlike the vast hype campaign of the past 30 years that has now achieved huge success by getting a billionaire to provide $3 million awards for failed ideas like Green-Schwarz's work on string theory unification).

40. **Jay**
December 15, 2013

>Yes, the Nobel prize did affect me, and I think it affects most people's view of a scientific field.

Well, let's agree we don't have the same perception of what affect most scientists. But realistically, what kind of decision did you take that was influenced by this? If this is not too personal, I'm really curious!

41. **Bob Jones**
December 15, 2013

Okay, so you agree with me about Kontsevich. How about Maldacena? Or Witten? Or Seiberg? Does it really make sense to say that they're doing untestable work?

42. **Bob Jones**
December 15, 2013

In fact, I'm pretty sure my comments apply to everyone who's won this prize… Can you point to a single person whose work is untestable but not formal?

43. **Peter Woit**
December 15, 2013

Jay,
All I mean is that when I was first learning about quantum mechanics and particle theory, and thus first learning names like Heisenberg, Dirac, Feynman, Gell-Mann, the fact that these were Nobel Prize winners did make an impression. This made me want to learn more about them and their work, so was one of many factors determined what book I read and what subjects I tried to learn more about. It's not that they had won a million dollars, but that the physics community had identified them as the individuals who had made the most important advances.
I think this is pretty common. Popular science articles often identify people as Nobel Prize winners I think exactly for this reason: it's a way of saying "this person is not just anybody, but someone who the scientific community has identified as responsible for one of the most important advances in the subject".

44. **Peter Woit**
December 15, 2013

Bob Jones,

Characterizing the work of these people as "untestable", that that's why they get a Milner prize, not a Nobel, is accurate. If it's math, that's not a bad thing, it's not supposed to be testable, and they're not supposed to get Nobel prizes. If it's supposed to be physics, it can be a bad thing and is, intentionally, a serious criticism of their work. I'm not going to review the long list of things all the Milner prize winners have worked on, and what significance they have. But I think the characterization of "untestable" is actually too kind in many cases. Arkani-Hamed's work on extra dimensions and SUSY has been experimentally tested, and is just wrong. He's a case of a $3 million award for not just untestable, but wrong ideas. The latest award (Green-Schwarz) is another example: string theory unification is now clearly not just an experimentally untestable idea with current technology, but one that predicts nothing, so is both vacuous and wrong as an idea about science.

45. **Bob Jones**
December 15, 2013

Green and Schwarz demonstrated that superstring theories have certain formal properties. These theories describe strings in ten-dimensional spacetime, so they have nothing to do with the real world a priori. In order to talk about testing these theories experimentally, you first have to specify a particular phenomenological model. Otherwise you might as well be complaining that mathematics is untestable…

46. **Igor**
December 15, 2013

I think the point of such prize is to have more researchers with rock star status. Terence Tao is such example in math.
Prize money could be used to promote breakthrough that has been achieved.

47. **gs**
December 15, 2013

IMHO first-class work should also be recognized when it invalidates a widely held theory, even if a correct replacement for the theory is not immediately at hand. The Michelson-Morley experiment was recognized via the Nobel Prize; maybe there should be a major award for such contributions. I'm thinking not only of experiments, but e.g. of the claim that 10^lots string-theory parametrizations are compatible with currently foreseeable experiments. In mathematics, counterexamples could be recognized.

Roughly speaking, if demonstrating A would win something like the Nobel or the Fields, then demonstrating not-A should qualify for the proposed award(s). Such prizes perhaps should not be awarded on a regular schedule.

It would be hard to persuade people of means to fund such an award. If a wealthy short seller wants to perpetuate their name. that might be a possibility.

48. **Muri Yilner**
December 15, 2013

And then more "Perelmans" would continue to decline awards…

49. **Peter Shor**
December 15, 2013

@gs: there are lots and lots of experiments looking for stuff that would probably be given the Nobel if they found it. There's LIGO looking for gravity waves, many experiments looking for dark matter, SETI looking for alien civilizations, experiments looking for an electron dipole moment, cold fusion, …We can't give all of them prizes.

50. **gs**
December 15, 2013

Peter Shor: Without having thought through the criteria for a "Naked Emperor Prize", I suspect that, vis–à–vis the "regular" prizes, deserving candidates would be relatively sparse.

51. **Jay**
December 15, 2013

Peter W,

All I mean is I find hard to believe that "bad prizes could pervert the youth" is the reason of your sour opposition. Of course when younger we were impressed by Heisenberg, Dirac and Feynman, all Nobel Prize winners. Of course we were also impressed by Minsky, Penrose, Shannon, Turing, Von Neumann, all without any Nobel Prize (but I had to verify!). If you're right that the subfield Milner choose to acknowledge goes in the wrong direction, we will forgot these laureates the same way so many Nobel Prize laureates are unknown (at least to me -I checked the list to verify the age distributions were differents for the two prizes). BTW, thx for the exchange of view.

52. **Jesper**
December 15, 2013

I'm a little late on this, but I want to respond to the "best and the brightest in physics today" (Deane, Bob Jones, Jay…).

I think that declaring someone the best and brightest a field can only be done in retrospect. With the Nobel there is a requirement of experimental verification and with Math prices of proof, and thats great.

I can imagine that before Copernicus there were some very "best and brightest" people around computing epicycles to astounding precision – but seen from a modern perspective they were people on the wrong track. They were wrong. It may be that string theory turns out to be a great and wonderful theory of "it all" and in that case these people deserve a mountain of praise – but if not, then they were not the best and brightest in physics at our time – because they were wrong (they may be praised for other things, math perhaps, or PR talent).

This is what is wrong with the Milner Price. It may give praise to someone who is completely lost in the forest.

53. **Peter Woit**
December 15, 2013

Jay,
I'm actually rather unconcerned about the youth, I was just addressing the main argument I always hear that more publicity for science is always good, even when it's for bad ideas, because it will get young people interested in science. The reason for my "sour opposition" is quite simple, and the quote from me in the Guardian got it right. I just don't think huge financial awards to people for over-hyped ideas that don't work are a good idea. The Green-Schwarz award is a perfect example of the problem. Superstring unification really is a failed idea, so why reward it? Is that good for science?

54. **Jesper**
December 15, 2013

@ Jay

"If you're right that the subfield Milner choose to acknowledge goes in the wrong direction, we will forgot these laureates the same way so many Nobel Prize laureates are unknown"

Difference: the Nobel laureates may be unknown, but they were right. I think its naive to think that such a big prize as Milners will not twist the research field(s) in some way – and since it is deliberately detached from experimental verification it may very well – judged in restrospect – twist the field(s) in a very negative way. Yes, we'll just forget the Milner prize winners if they turn out to be wrong – but a lot of time and effort may be wasted before we or our grand^many -children are in a position to make the final judgement.

55. **Jon Orloff**
December 16, 2013

Is it possible that a key reason for these prizes and the way they are handed out is to promote the names of Mlner and Zuckerberg – to make a connection of their names to esoteric areas of science? Sort of like self-promoting groupies.

56. **Kavanna**
December 16, 2013

Peter is right. Prizes like this just concentrate attention and fame on dubious exemplars. Better is supporting institutions and rediscovering real standards of scientific progress.

And science prizes, if we must have them, should either concentrate on ideas tested and proven in retrospect (like the Nobels); or just be given out in small amounts in a semi-random fashion, as proposed by Smolin and Essex.

57. **Sebastian Thaler**
December 16, 2013

Peter,

You're featured in Business Insider–as a "famous math professor":
http://www.businessinsider.com/math-professor-pans-zuckerberg-3m-prize-2013-12

58. **Peter Woit**
December 16, 2013

Thanks Sebastian,
Well at least so far the Milner-Zuckerberg math prizes have made one person “famous” in the media, that’s what they want, right?

59. **Chris Oakley**
December 17, 2013

Peter,

I thought that ‐ at least according to Jacques Distler’s definition ‐ you are not an active researcher, and therefore raising your profile in the media is not what Milner and Zuckerberg intend. Still, it is some kind of compliment being passed over in a prize funded by someone famous for stealing someone else’s idea in a ceremony hosted by people who pretend to be somebody else for a living.

60. **Edwin Pell**
December 17, 2013

Thank you Peter. You are right America is being feed the ubermensch view of the world. That is a few rich powerful people make it all happen everybody else is useless and unneeded. Let’s remember Hubbell started as the janitor went on to do fundamental work and was never was rich or famous.

61. **Curious Wavefunction**
December 17, 2013

I wonder why Anderson has not openly spoken out against the prize.

62. **Peter Woit**
December 17, 2013

Curious Wavefunction,

I suspect that at 90 years old, Anderson might feel he has better things to do with his time and energy than take up causes that will intensely annoy his colleagues (like criticizing the $15 million bounty they have personally collected in the past year or so).

63. **Nathalie**
December 20, 2013

The most important difference between Nobel Prizes in sciences and many other prizes is due to the process of evaluation of the nominees. In the case of Nobel Prizes the work of a potential candidate is scrutinized by several referees, a process which can take many years. Moreover, the Prize is not given to speculations, irrespectively of how spectacular they may be. Indeed, some speculations in the field of physics may turn out to be correct but most of them do end up in the “physical wastepaper basket” and don’t deserve the Prize. Better late than wrong!

Some Physics Laureates may be unknown to us simply because their contributions fall outside our field of expertise.

64. **littlestar**
December 20, 2013

Hi , I have a suggestion for the Milner prizes:
Experimental verification and empirical proof are fundamental things in physics , so the fundamental physics prize should be given primarily to physicists with innovative physics theories that have been experimentally verified and/or for important discoveries in physics , and a smaller prize could be given to work done in theoretical and mathematical physics , and string theorists (among others) would be included in this smaller prize.
A (Breakthrough) Prize in Mathematics could be useful if created , but it ought to be given for new creative proven and coherent work or theories in mathematics , and it would be good of these theories have applications in the rest of the exact sciences.

A Bubble-Universe at Stanford

December 19, 2013
Categories: Multiverse Mania

Video from last weekend's [Fundamental Physics Prize scientific meeting at Stanford]() is now available, in unedited form, [here]().

The first video there is a discussion moderated by Yuri Milner, who does a good job of asking Strominger, Polchinski, Green, Schwarz and Vafa questions, although getting pretty much exactly what you'd expect out of them (the hot topic is firewalls).

After skimming through the rest of several hours of video, what struck me is that Milner has managed to all by himself implement the bubble-universe picture of reality that has been propounded at Stanford for many years by Linde, Susskind and others. By smashing tens of millions of dollars into a small target (some prominent academics), he has created a new bubble-universe, with new laws of physics and a new conception of science. In this particular bubble-universe, problems with string theory unification have magically vanished and don't need to be mentioned. Whether a scientific theory can predict anything or not is irrelevant, since you just know what has to be true (the idea with the big money attached to it). The embarrassing fact of no SUSY at the LHC does get fleeting mention, but John Schwarz assures everyone that in his view, there is no question that superpartners exist, whether or not the LHC ever sees them. The multiverse is seen as the answer to all problems, although Cumrun Vafa does warn that maybe one should also look for other answers. Polyakov says that he has nothing against this kind of "Anthropology", except that it is very boring. That's an accurate characterization of the science of the new bubble-universe at Stanford.

Most remarkable is the last video, where things truly become causally disconnected from the universe outside Stanford. After a long introduction from Susskind, Michael Green takes the stage with a talk recapitulating the entire history of science, with string theory the successful culmination of this history. He and Schwarz then settle in to accept congratulations from the audience for their great discovery that has made the bubble-universe possible.

Comments

1. **paddy**
   December 19, 2013

   Delete if you will PW, but I am still waiting for my sonic scredrwiver.

2. **paddy**
   December 19, 2013

   PS As a working physicist, I am made nauseous trying to watch these videos.

3. **Steve**
   December 28, 2013

   Here's a new article: [http://ieet.org/index.php/IEET/more/pelletier20131227]()
   Was tweeted by Richard Dawkins Foundation

4. **Bernhard**
   December 29, 2013

   Thanks Steve, I stopped reading at "Parallel Worlds exist and will soon be testable" but I'm sure the rest is really interesting.

5. **John**
   January 1, 2014

   > The embarrassing fact of no SUSY at the LHC does get fleeting mention, but John Schwarz assures everyone that in his view, there is no question that superpartners exist, whether or not the LHC ever sees them.

   Well that's falsifiability thrown out of the window and religious belief entering by the door.

Latest on abc

December 19, 2013
Categories: abc Conjecture

In case you haven't been following this story, "abc" refers to a famous conjecture in number theory, for which Shin Mochizuki claimed last year (see here) to have found a proof. His argument for abc involves a new set of ideas he has developed that he calls "Inter-Universal Teichmuller Theory" (IUTeich). These are explained in a set of four papers with a total length over 500 pages. The papers are available here, and he has written a 45 page overview here. One can characterize the reaction to date of most experts to these papers as bafflement: what Mochizuki is doing is just so far removed from what is known and understood by the experts that they have no way of evaluating whether or not he has a new idea that solves the abc problem.

In principle one should just be able to go line by line through the four papers and check the arguments, but if one tries this, one runs into the problem that they depend on a long list of "preparatory papers", which run to yet another set of more than 500 pages. So, one is faced with an intricate argument of over 1000 pages, involving all sorts of unfamiliar material. That people have thrown up their hands after struggling with this for a while, deciding that it would take years to figure out, is not surprising.

Mochizuki has just released a new document "concerning activities devoted to the verification of IUTeich". It explains the state of his efforts to get other mathematicians to check his work, a project that has been going on since last year, leading to many ongoing updates to the papers making up the proof. He explains that he submitted the four IUTeich papers to a journal last August, but will not have anything to say about the journal or the state of the submission process. This is the way mathematics is supposed to work: the papers should be refereed by experts who have agreed to go through and check them carefully (and confidentially). Given the unusual character of the series of papers, finding willing and able referees may be very difficult. It would of course be most satisfying if such referees can be found and can either identify holes in his argument, or vouch for correctness of the whole thing.

In the meantime, he has been working since October 2012 with Go Yamashita, who has carefully gone through the papers and is now writing a 200-300 page survey of what is in them. Yamashita may also give a course on the topic at Kyushu University sometime after next April. As part of this process, three other mathematicians participated in a seminar in which Yamashita lectured on the papers.

Another mathematician working on this is Mohamed Saïdi, who devoted about six months to studying the papers, then spent three months visiting Kyoto and discussing them with Mochizuki. According to Mochizuki, he has said that he believes the theory to be correct. Mochizuki summarizes the current situation as

> the issue of whether or not one should regard the verification of IUTeich as being, for all practical purposes, complete, i.e., as a result of the activities of Yamashita and Saïdi, is by no means clear, and any sort of "final conclusion" on this topic must be regarded as a matter that lies beyond the scope of the present report.

Mochizuki goes on to claim that, based on what he has heard from Yamashita and Saïdi, researchers trying to read his papers should find it possible to understand the theory if they work on it for roughly half a year. He warns that they do need to be aware though that an attempt to make sense of what he is doing by expecting "a similar pattern of argument to existing mathematical theories is likely to end in failure." They also need to keep in mind that he's not particularly focused on proving abc, that for him it is the IUTeich theory itself that is the object of interest.

This is a remarkable story, with little precedent. After more than a year, I haven't heard anyone willing to bet either way on how it will turn out. Mochizuki is a talented mathematician and maybe he has a proof. Or maybe he has a complicated set of ideas which don't do what he hopes. Perhaps someday one of these alternatives will start to emerge, but it doesn't now look like this will be anytime soon.

Comments

1. **vmarko**
   December 19, 2013

   "Mochizuki is a talented mathematician and maybe he has a proof. Or maybe he has a complicated set of ideas which don't do what he hopes. Perhaps someday one of these alternatives will start to emerge, but it doesn't now look like this will be anytime soon."

   An ideal candidate for the new Milner-Zuckerberg math prize? 😀

   // Sorry, couldn't resist… //

   Best, 🙂
   Marko

2. **Kent**
   December 19, 2013

   Putting aside for a moment the question of whether or not the supposed proof of abc is valid, does anyone have any indication that IUTeich will be useful for obtaining other results? In other words, is there a reason, other than wanting to verify the abc proof, for someone to study IUTeich? Or is this a case where if the abc proof turns out to have a flaw, any time spent on IUTeich would be a waste?

3. **David Appell**
   December 20, 2013

   Perhaps IUTeich is a part of 22nd-century mathematics that has fallen by chance into the 21st century.

4. **anon**
   December 20, 2013

   Has Faltings (his Ph.D. supervisor) come out and said anything? I heard some rumors going around.

5. **Mitchell Porter**
   December 20, 2013

   "is there a reason, other than wanting to verify the abc proof, for someone to study IUTeich?"

   You could just be curious to know what these new ideas are about, and how they relate to known math.

6. **chorasimilarity**
December 20, 2013

This looks to me as a social problem, not a mathematical one. On one side, there are no “experts” in Mochizuki field, because he made it all. On the other side, the idiotic pressure to publish, which is imposed in academia (the legacy publishers being only opportunistic parasites, in my opinion), makes people not willing to spend time to understand, even if Mochizuki past achievements would imply that there might be worthy to do this.
To conclude, is a social problem, even an anthropological one, like a foreign ape which shows to the local tribe how to design a pulley system, not at all believable to spend time on this. Or it is just nonsense, who knows without trying to understand?

7. **Peter Shor**
December 20, 2013

If this proof is accepted (with something between a line-by-line verification and complete understanding), but nobody else ever learns the theory well enough to do anything with it, should it be considered a success or a failure?

On a more positive note, I expect that once people are more convinced that it’s correct, more of them will be willing to invest the amount of time need to understand what’s going on.

8. **OMF**
December 20, 2013

> This looks to me as a social problem, not a mathematical one.

The problem is one of culture. Modern Mathematics has become so fragmented, so disconnected, that it is in fact possible for someone to write 1000+ pages on a completely new theory working in complete isolation and moreover to have the prospect of this work actually taken seriously by the wider mathematical community because of the way that minor field specialization has become the norm.

Another problem is what I like to call “simplicity-free mathematics”. Theorem’s are proved by a small wall of text peppered with multiple references to, at best, previous lemmas of theorems in the text, more usually not so well known theorems from increasingly obscure branch of mathematics in question, and at worst, references to outright conjectures that neither the author nor anyone else has yet proved(This last style goes back to Galois). Simple elements and frequently actual numbers are usually completely absent.

My gut feeling for mathematics is that you’ve either got a formula/algorithm, or you’ve gotten lost. Obviously this doesn’t apply to absolutely everything, but the lack of such hard results has real consequences in the form of the increasing obscurity of various fields, and the lack of feedback from these into the core discipline.

But, aside from that perspective; At the end of the day if you require 1000 pages of specalised argument to prove your result I think it is reasonable to conclude that you don’t really understand your results and neither will anyone else. Regarding such works as proved, and worse, as a foundation for future material is what has lead us to our present state of affairs, and it shouldn’t be tolerated.

9. **Peter Woit**
December 20, 2013

OMF/chorasimilarity,

For some great wisdom on this topic, I urge everyone who hasn’t done so to read Bill Thurston’s “On proof and progress in mathematics”
http://arxiv.org/abs/math/9404236
For Mochizuki’s proof to be accepted, other members of the community are going to have to understand his ideas, see how they are supposed to work and get convinced that they do work. This is how mathematics makes progress, not just by one person writing an insight down, but by this insight getting communicated to others who can then use it. Right now, this process is just starting a bit, with the best bet for it to move along whatever Yamashita is writing. It would be best if Mochizuki himself could better communicate his ideas (telling people they just need to sit down and devote six months of time to trying to puzzle out 1000 pages of disparate material is not especially helpful), but it’s sometimes the case that the originator of ideas is not the right person to explain them to others.

10. **Mr Monopole**
December 20, 2013

>>>*It would be best if Mochizuki himself could better communicate his ideas*
–
In preface to ‘Three – Dimensional Geometry and Topology’ W. Thurston wrote:
“The notes were originally aimed for an audience of fairly mature mathematicians…
Some of the feedback from seminars and individuals convinced me that it would be
worth filling in considerably more detail and background; there were several places
where people tended to get stuck, sometimes for weeks…”
–
Last chapter of this book covers Teichmuller space (BTW: Mochizuki likely did not read the preface).

11. **Michael Schmitt**
December 20, 2013

I recently gave a course on the responsible conduct of research, and one of the main messages of the course is the necessity of publishing one’s results and methods so that others can verify them. To underscore the point, we looked at infamous cases of scientific misconduct in which proponents of great discoveries failed to share the details of their work, thereby making it impossible for others to reproduce and confirm their results. I emphasized that scientific research must be shared with the scientific community, else it (almost) isn’t science. Review and criticism by peers is a major part of the process of moving forward.

————————————

From reading the comments above, one almost has the impression that mathematicians put less value on publishing and verification from peers than scientists do. Is it OK to be right but impossible to understand? Is it OK to have a great insight but not to record it and have it approved by the community? Are individual insights more important than widespread comprehension?

————————————-

Probably I am reading too much into some of the comments people have written…

12. **Allan Rosenberg**

December 20, 2013

This sounds like a problem for Nicolas Bourbaki's group.

13. **Peter Woit**
December 20, 2013

Michael,

Actually I think mathematicians put more value on publication and the refereeing process than other scientists do. It's very important to them to know which results have been checked and are reliable, in order to use them to build reliable proofs of something else.

It is true however that mathematics research can be very specialized, very abstract, and so intensely demanding in terms of anyone being able to follow and check it. For typical work, often only a handful of people in the world have the right background to quickly understand a certain new piece of work and evaluate the arguments. The Mochizuki case is quite unusual: as usual there really are only a small number of experts in the field, but he has gone off by himself in his own direction a great distance, then reappeared to claim to have made a major discovery. The experts are faced with the problem of how to figure out whether he is right. Do they invest the six months he is asking from them? Do they tell him: forget it, you need to write this down in a more comprehensible form that won't take us 6 months or more to understand? Do they wait and see if someone (like Yamashita) who has invested a lot of time in following this will report back with a more comprehensible explanation of what is going on?

14. **Peter Woit**
December 20, 2013

Allan,
Except for the problem that they're no longer active…
One of the motivations for Bourbaki was to produce a careful and complete set of arguments for the standard results used in different parts of mathematics, so that these could carefully be checked for reliability and be used to go further.

15. **srp**
December 20, 2013

At least in math each investigator has pretty compatible (with overall progress) incentives in deciding whether to spend his time reading something obscure. If you figure out how to make better sense out of something that's already been proven the community gives you some credit.

Across the whole span of disciplines I suspect that we reward writing (original stuff) too much and reading too little.

16. **AndreasK**
December 21, 2013

..first of all, the main problem in the reception of Mochizuki's work IS indeed a cultural one, but obviously not quite in the sense it is discussed here; from a 'western' perspective, Mochizuki is perceived as the 'great other' and the 'otherness' of his work comes with a mutually-amplifying 'otherness' of his culture and 'race', if he would be white and/or (at least) western his work would be considered not as 'scientific misconduct' but as a 'great and visionary' cultural achievement, his teachers would be hailed, his descendants would be famous and he would be ubiquously present at various life-time-achievement-awards still in 30 years. But the 'otherness' of his work is probably on the other hand exactly the result of being perceived as the 'other' on a sociological basis, which can mean exclusion, but also freedom, a certain space for creativity. In any case, all this exactly transcends western norms of 'workforce', his papers are more meditation than results and given this, they are among the most inspiring pieces of work I have read in the last ten years (and I know only a tiny piece of them).

Also from another, more substantial perspective: from my point of view, the discourse which is reflected here is wrong in essential aspects, that his, his contribution to a possible proof of the abc-conjecture is certainly LESS, not more important than his general insights, in this sense, it is excactly not important whether his announced proof turns out to be true in a line by line-evaluation, not of course from a 'factual' point of view, but from a 'philosophical' one. 'Millenium problems' are catalysts for theory-builders, often not so quite interesting in itself (again, from my perspective). As I already mentioned in a related discussion, his theory of 'étale theta functions' seems to reflect a deep aspect of the latter: these functions form a bridge between 'purely arithmetic and topological data' of a given object, or in his terms between 'Frobenius-like' and 'étale-like' structures and in this sense his 'etale theta functions' have for instance great similarities to 'flux mappings' in symplectic topology. The 'etale-like structures' have 'the ability to 'penetrate walls' (cf. his survey paper on p. 32), all these are eminently deep and important observations; the correspondence of certain rigidity structures. From my personal point of view, very similar constructions should be possible in diff. geometry and should for instance allow to relate rigidity of 'classical Hodge structures' and symplectic rigidity, again using certain theta-like structures. The existence of a 'Galois-theoretic Kodaira spencer morphism': from my point of view this is neither esoteric nor completely 'out of the blue', it HAS to exist. One should consider that mathematics can be a very lonesome and 'individual' endeavour, Mochizuki possibly represents a certain, very special 'type' of mathematician, who is as important as those types of mathematicians who will eventually evaluate and re-work his ideas.

17. **mo**
December 21, 2013

This is not the first case when mathematical and social issues have been raised and similar sentiments were voiced. The first one was the classification of finite simple groups. The only difference is that many (over 100) mathematicians worked on it rather than one, but still the mathematical community was skeptical. Since the complete proof takes over 10,000 pages it is fair to assume that no one understands it in its entirety.
mo

18. **Peter Woit**
December 21, 2013

AndreasK,
It's just not true that Mochizuki is "other" to Western mathematicians. He grew up in the US, went to Princeton, his advisor was Faltings (who is "hailed"). He's someone personally known to most experts in his field, and they met him at pinnacles of the US establishment like Princeton and Harvard. The idea that he's not taken seriously because he's not white is absurd. No one thinks he is engaging in "scientific misconduct", and everyone agrees his ideas are potentially interesting, possibly revolutionary and of great significance. Whether they can prove the abc conjecture, which our current best ideas don't seem to be able to prove is a measure of the depth of these ideas, whether they tell us something new, or just repackage things we already know.

19. **CPV**
December 21, 2013

Is this a good candidate for computer verification of the proof?

20. **Peter Woit**

December 21, 2013

CPV,
No, I don't think Mochizuki's argument is written in anything like a form that can be made ready for computer verification. Also, in general, if experts can't understand the overall structure of an argument and which of its parts are the new and tricky ones which need to be carefully checked, I don't believe a computer program exists that can do this for people.

21. **Michael Schmitt**
December 21, 2013

Peter, I appreciate a lot these posts about mathematics and mathematicians. They help me, an experimental physicist, gain some notion of the wonderful world of mathematics (or at least the experiences of mathematicians). The article you referred to by William P. Thurston is extremely interesting and stimulating, although it concludes on a rather odd note. There is even something in Section 3 about the near-impossibility of teaching students mathematics (or physics). Thurston's commentary shows that mathematicians are very caught up in the issue of communicating their results – indeed, publication is only one of several important modes of communication. My earlier comment was indeed very naive!

22. **Edward**
December 21, 2013

I am wondering if Mochizuki can provide a "appetizer", i.e. an important result, but perhaps not as significant as abc, whose proof is relatively "quick".

In Perelman's case, the appetizer is the noncollapsing theorem, whose proof people verified and accepted in weeks. This built up huge confidence in Perelman's other arguments.

23. **Armin Nikkhah Shirazi**
December 22, 2013

This seems like the kind of problem which would lend itself well to the polymath approach, except that instead of discovering new results it would involve checking results (presumably) already obtained. That (apart from the scarcity of experts in the relevant field) may be enough for that kind of collaborative effort to become unlikely. Personally, I think that if someone claims that that he or she has made a major discovery, the responsibility for explaining the work to the rest of the world in a manner that it can follow rests squarely with the claimant. Also, it may even happen that when one makes a concerted effort to explain an idea intelligibly to others one will find ways in which the idea simplifies, points to new connections or becomes clearer in one's oen mind. Thus, even the original claimant may benefit from such efforts.

24. **srp**
December 23, 2013

Let me second the motion on the value of the Thurston article you linked. It is a model of introspection, social observation, and writing clarity.

25. **AndreasK**
December 24, 2013

Peter, the words 'scientific misconduct' were taken from this very thread, even if they were not aimed 'openly' at him. Also, the subtext which I attacked was also taken mainly fom this very thread. In the mathematical community, things don't lie 'out in the open', the acceptance of proofs is a sociologically and psychologically (apart from any mathematical question) not completely understood process and is intertwined with power discourses that without ANY doubt part south from north, white from non-white, established from non-established. I am not accusing here anyone personally, but it seems that certain processes or mechanisms might be not completely conscious to many people involved in certain discourses. My text above was meant as a polemic, I thought this would be sufficiently clear. Still, there are many odd questions in the reception of Mochizuki's work, most of his ideas evolved since 10 to 15 years and obviously they are only interesting SINCE he claimed to have proven the abc-conjecture, and, in between of many other questions, one could ask why this is the case. Why do experts expect to understand ideas which were obviously to a certain degree ignored for 15 years should lead to a line of argumentation understandable to anyone in time scales of some weeks? Can anyone here answer this simple question? Grothendieck's ideas were famous long before Weil's conjectures were finally settled. But again this simple fact: the discrimination of non-white mathematicians, of women in mathematics will not vanish just because one claims there wouldn't be 'proofs' for it, and that to have grown up in the US is no guarantee not to be 'othered' in which (white, western) context ever is unfortunately one of these not 'provable' facts.

26. **Peter Woit**
December 24, 2013

AndreasK,
Enough of this. You're just trying to put this story into an ideological framework where it doesn't fit. Mochizuki is as "Western" a mathematician as anyone, and no, he's not a woman or black. The idea that the people having trouble figuring out what Mochizuki has done are having this problem because he's racially East Asian is just ridiculous. As for the comparison to the reception of Grothendieck's ideas, just look at the history. Grothendieck developed his ideas in active collaboration with others (they were often written up by other people he was working with).

27. **milkshaken**
December 24, 2013

If I remember correctly, when Andrew Wiles gave the expose of the first version (the one that turned out to be flawed) of his famous proof, he disguised it as an advanced class and asked his colleagues – who were checking the proof – to sit on it, over the course of a semester. Maybe this would be the right method here as well/

28. **Simon Pepin Lehalleur**
December 24, 2013

@AndreasK:

This way of interpreting the story does not sound very plausible for me either. The field of algebraic/arithmetic geometry is full of brillant Japanese mathematicians, with a long tradition going back at least to Shimura and Taniyama. The staff of Mochizuki's home institution RIMS includes (among others) Morihiko Saito, Shigefumi Mori, Shigeru Mukai, Takuro Mochizuki, Masaki Kashiwara. Each of them has produced deep mathematical work, spawning whole new field, e.g. Saito's mixed Hodge modules, the Mori (minimal model) program, Fourier-Mukai transforms, Mochizuki's irregular Riemann-Hilbert correspondance, Kashiwara's crystal bases.

All of them have interacted productively with the "Western" mathematical community. Because of this tradition, quite a few arithmetic geometers are familiar to various degrees with Japanese culture, have learned some Japanese, etc.

I fully agree that it is a pity that few other mathematicians (including Japanese/Asian ones !) tried to keep in contact with Mochizuki's ideas over the years.

29. **Peter Woit**
    December 24, 2013

    milkshaken,
    As far as I know, there was only one person (Nick Katz) in on the fact that the material being discussed in that class was a crucial part of a proof of Fermat. The case of the Wiles proof is a good comparison to the Mochizuki one. Wiles did work for many years alone and developed new ideas, but once he made his proof public experts very quickly were able to understand his ideas, then dig in and check things carefully. The problem was found (I believe by Katz) as part of the conventional refereeing process for the paper.

    This story makes clear that for this kind of proof, even when an extremely careful mathematician like Wiles has checked things thoroughly with another expert, there can still be a flaw in the proof. The community is not going to accept that Mochizuki's proof is valid until it has undergone a similar level of scrutiny. The problem now is that people seem stuck at an early stage of the process, that of understanding the structure of the proof and the techniques used well enough to be able to start checking carefully for subtle flaws.

30. **Hahn**
    December 25, 2013

    I'am not qualified to talk about the correctness of these papers.

    However I am suspicious. If I have to, I will bet there are unfixable problems.
    * people don't mix proof in number theory with ZFC axiomatic set theory.
    * people don't put out 5 papers in a series and revise all of them for others to check out.
    * people don't say "Go Yamashita gave a course, Mohamed Saïdi said he believes the papers to be correct"

31. **preda**
    January 7, 2014

    "Wiles did work for many years alone and developed new ideas, but once he made his proof public experts very quickly were able to understand his ideas, then dig in and check things carefully. The problem was found (I believe by Katz) as part of the conventional refereeing process for the paper."

    To my information, none of these is totally right. First, Wiles is purported to have completed the Cambridge lectures with the words "I think this completes the proof of Fermat". Next, Nick Katz – together with P. Sarnak, if I remember well – looked for it that a team of 8-12 mathematicians got involved in understanding Wiles`s proof. This was not part of the refereeing process yet; the material was distributed in pieces, and this is how the gap was found relatively fast. It is not easy to believe that the material of Mochizuki can be split in parts, since there are fundamental new ideas that everyone must understand, who wants to verify even a small part of the work.

    The more I read about the issue, the more I gather the feeling that there is hardly anything that Mochizuki could have actively done in order to facilitate the verification process, and which he did not do – or even "refuse to do", as one sometimes reads. We have at present two people who studied the material over a longer period of time, and had long discussions with him. It is to be hoped that this example will spread.

32. **Peter Woit**
    January 7, 2014

    preda,
    I don't think there is anything at all inaccurate in what I wrote. See for instance Mozzocchi's "The Fermat Diary", page 19. He describes what happened with the Wiles proof was that Wiles gave it to Mazur, who was an editor of Inventiones. Mazur chose six referees and assigned each one of them a section of the paper. Katz was assigned section six, began work on it (with help from Illusie) and was the one to find the problem.

Trust the math? An Update

December 21, 2013
Categories: Uncategorized

Back in September, I wrote here about the news that Snowden's revelations that confirmed suspicions that back in 2005-6 NSA mathematicians had compromised an NIST standard for elliptic-curve cryptography. The new standard was promoted as an improvement using sophisticated mathematical techniques, when these had really just been used to introduce a backdoor allowing the NSA to break encryption using this standard. There still does not seem to have been much discussion in the math community of the responsibility of mathematicians for this (although the AMS this month is running this opinion piece).

After my blog post, some nice detailed descriptions of how this was done and the mathematics involved appeared. See for instance The Many Flaws of Dual_EC_DRBG by Matthew Green, and The NSA back door to NIST by Thomas Hales. The Hales piece will appear soon in the AMS Notices. Hales also has a more recent piece, Formalizing NIST Standards, which argues for the use of formal verification methods to check such standards. Also appearing after my blog post was the news that RSA Security was now advising people not to use one of its products in default mode, the BSAFE toolkit.

One mystery that remained was why the NIST had promulgated a defective standard, knowing full well that experts were suspicious of it. Also unclear was why RSA Security would include a suspicious standard in their products. Back in September they told people that (see here) they had done this because:

> The hope was that elliptic curve techniques—based as they are on number theory—would not suffer many of the same weaknesses as other techniques

and issued a statement saying:

> RSA always acts in the best interest of its customers and under no circumstances does RSA design or enable any backdoors in our products. Decisions about the features and functionality of RSA products are our own.

Today there are new revelations about this (it's unclear from what source), which explain what helped make RSA swallow the bogus mathematics: a payment from the NSA of \$10 million. I guess there's a lesson in this: when you can't figure out why someone went along with a bad mathematical argument, maybe it's because someone else gave them \$10 million…

Update: For another explanation of the math behind this, see videos here and here featuring Edward Frenkel.

Update: There's a response to the Reuters story from RSA here. As I read it, it says

- They do have a secret contract with the NSA that they cannot discuss
- They used the NSA back-doored algorithm in their product because they trust the NSA
- They didn't remove it when it became known because they really are incompetent, not because the NSA was paying them to act incompetent

It's hard to see why anyone would now trust their products.

Comments

1. **Tinos**
   December 22, 2013

   Both bitcoin and PGP use elliptic curve cryptograpy. Lately, the exchange rate for bitcoin has been soaring. Do you have an opinion on whether or not it is really secure?

2. **S. Molnar**
   December 22, 2013

   $10 million is the corporate rate; it looks like $3 million may be the individual rate.

3. **Yatima**
   December 22, 2013

   Note that in this case we are talking about

   Dual Elliptic Curve Deterministic Random Bit Generator

   which are less random than thought. It is recommended to get random bits, including pseudorandom bits, from elsewhere.

   Elliptic Curve Cryptography is something else.

   And Bitcoin uses Hashcash, based on SHA-256, not Elliptic Curve Cryptography.

   See also: How the Bitcoin protocol actually works

4. **Peter Woit**
   December 22, 2013

   Tinos,
   Anyone looking for info on what cryptography to trust should be looking elsewhere than this blog, since I'm no expert. While trying to figure out what and who to trust, I'm just pointing out that now you have to realize that part of the math community and the main US standards organization have been actively trying to deceive you. And the main commercial cryptography company has been bought off to also try and deceive you. People in general, and mathematicians in particular, might want to think about whether something should be done…

5. **anon**
   December 23, 2013

   The RSA response is certainly ridiculous but now that I think about it actually, I don't see anything necessarily wrong about mathematicians working on military projects of this sort. (And the behaviour of the NSA should not be unexpected; this is, after all, what they usually do.) Mathematicians have a long history of working in the defence industry. If I remember correctly, even Euler translated a treatise on ballistics. And of course, in more recent times, Ulam was involved in the design of the hydrogen bomb and nuclear pulse propulsion.

6. **Peter Woit**
December 23, 2013

   anon,
   One can reasonably argue that mathematicians at the NSA who came up with this were just following orders, and that any at RSA who were encouraged to play dumb were also just following the policy of their employers. One can also argue that all of this is perfectly legal (although the fact that courts have been thwarted from ruling on whether these things violate the Bill of Rights may mean the legality issue is still up in the air).

   There remains the question though of what people in general should do in response to the NSA revelations, and of what the mathematics community in particular should do in response to the fact that mathematics and mathematicians have played a role in this. "Nothing" is one defensible position, as is Beilinson's "treat the NSA the way mathematicians treated the KGB in the former Soviet Union". In any case, I think there's a strong argument for publicizing what is known about exactly what happened here so that people can make up their own minds about what they think about it.

7. **OMF**
December 23, 2013

   > Mathematicians have a long history of working in the defence industry. If I remember correctly, even Euler translated a treatise on ballistics.

   No. The situation that Mathematics finds itself in is completely unprecedented.

   The NSA is probably the world's single largest employer of pure mathematicians. At ~1000 or so. Since there are about ~100,000 mathematicians in the world, this is a sizable enough fraction ~1% of the world's quotient, to say nothing of the research funding and recruitment operations of the NSA outside of those in its direct employment. Within the US itself, the influence of the organisation is obviously even stronger.

   This bears many similarities towards the move in physics towards "Big Science" in the post war period, but also several key differences.

   Firstly, unlike big science, the developments at the NSA have been almost exclusively negative, socially, politically and scientifically. The mathematics employed there is being turned into a tool of oppression in a way that few scientific developments have even been so directly employed. Secondly, a large body of mathematicians are now engaged in research — and now we know in publishing research — that is deliberately incomplete and even deceitful. This second development is on a scale previously unheard of in this discipline.

   Finally, we may likely see a large political split developing in the mathematics community, centered around the cooperation of mathematicians with the NSA. Elements of this can be seen in the December issue of the AMS notices — Alexander Beilinson's letter calling for the AMS to sever its NSA ties along with the conspicuous absence of the monthly NSA recruitment advertisement. Politics — real politics — has entered the mathematical community for the first time, the US community in particular, in a very direct and unavoidable way.

   Even in the Soviet Union, mathematics was in general an apolitical activity, and mathematicians were given considerable autonomy. However the recent NSA scandal has brought mathematics and its activities directly into the political spotlight, and these activities into question. The world may be facing a situation where certain types of mathematical research becomes restricted, or taboo, or boycotted. This would be an unprecedented development. From ancient times, mathematics has always been an international, collegial activity; outside the temporal and certainly the political sphere; No longer?

   The greatest irony in all of this is that the branch of mathematics involved — number theory — was only 50 years ago regarded as the most "pure", abstract, and austere expression of the subject. The "Queen of Mathematics" in Gauss' words. Now the Queen seems to have fallen in with a bad sort, and the scandal threatens to rock the entire Kingdom to its foundations.

8. **anon**
December 23, 2013

   OMF,
   I completely disagree. We are entering a brave new world and we (including mathematicians) must embrace it or be left behind. It's not only the NSA by the way, but also the IDA in Princeton and many other agencies. In any case, no one is forcing anyone to do research they are uncomfortable with, or that is "so harmful for the fabric of human society," as Beilinson would put it. I seem to recall that one or two decades ago there was a controversy when it was discovered that some mathematicians were receiving military funding and an attempt was made to expose them. Even W. Browder, whose father I think was chairman of the Community Party of America, called it a witchhunt.

9. **anon**
December 23, 2013

   I mean *Communist* Party of America.

10. **Peter Woit**
December 23, 2013

    anon,
    I agree with you that we are entering a brave new world (Snowden did a lot to show how far we have gotten into this world already), but personally I don't believe we should embrace it, quite the opposite.

11. **Michael Gogins**
December 23, 2013

    In my view, mathematicians should accept the vocation of developing protocols that are transparent, open to independent scrutiny, verifiable, and capable of replacing the protocols used by institutions that have shown they cannot be trusted.

    These should be developed for currency, telecommunications, networking, data storage, and voting.

    If such developments succeeded, they might replace existing protocols or, perhaps, keep them honest.

    If such developments do not succeed, there is an obvious danger that we will end up living in 1984 with no way out.

    Preliminary signposts for such protocols: PGP, Bitcoin, Tor.

12. **Wayne**

December 23, 2013

I'm afraid I'm unable to discern the correspondence between your latest update's summary bullets about RSA, or any inference that can be drawn from their summary of 22 December, which supports the conclusions you draw. RSA states that their reliance was on the NIST, which aside from sharing two letters in its acronym has nothing in particular do with the NSA.

Many — perhaps along with RSA itself — now wish something more than RSA's trust in the NIST had been their guiding force in 2007. But if I am reading your latest addition correctly, I simply don't follow the logic suggesting such a wish provides evidence of incompetence and a (company-denied) secret contract with the NSA?

Wayne

13. **Jim Akerlund**
December 23, 2013

I am having a problem with separating the mathematicians involvement with the NSA and the NSA's use of spycraft. It looks like this RSA story is the NSA's use of spycraft. Reading Google's and Yahoo's interserver transmissions is more spycraft. True, mathematicians did create the eliptic curve pseudo random number generator, but where the problem seems to be occurring is the adoption of this number generator industry wide, which is out of the hands of mathematicians. I guess I am looking for the smoking gun that is in the mathematicians hands in this NSA mess.

14. **Peter Woit**
December 23, 2013

Wayne,
The problems with this elliptic curves algorithm were publicized back in 2007, the question is why RSA kept this as the default in this particular product after that time. Their argument that they didn't do this because they were paid off really only leaves true incompetence as an explanation (they were unaware of the claimed problem despite it being publicized, so incompetent at knowing their own field, or were aware of the claimed problem, but made the incompetent judgment that the claims were wrong).
Their statement acknowledges their relationship to NSA, says they never "divulge details of customer engagements". To me their denial seems carefully worded not to deny a contract with the NSA whose details they cannot divulge, just to deny a specific accusation about exactly what is in the contract.

It's quite true that the details of how the NSA corrupted the NIST approval process have still not been revealed. I don't understand why this is the case if the NIST wants to regain any credibility for its cryptography standards.

15. **Peter Woit**
December 23, 2013

Jim Akerlund,
I'm not well-informed about this, but I would assume there are mathematicians involved in the NIST standards approval process for a cryptographic standard based on this kind of mathematics. Similarly I would assume RSA Security employs mathematicians to evaluate such cryptographic algorithms. If neither organization uses mathematicians to make such evaluations, but just decides "oh, an NSA mathematician says everything is fine", that would explain how this happened, but I'm finding it hard to believe these organizations operate in such an unprofessional manner.

16. **NotSuper**
December 24, 2013

Off topic, sorry, but this may be of interest for this blog:
http://phys.org/news/2013-12-electron-shapeliness-supersymmetry.html

17. **milkshaken**
December 24, 2013

based on the Reuters article, by the time the 10million deal went down RSA was not doing any crypto research in house anymore (because their reorganized their research group out of existence) so they were eager to grab the NSA-supplied goodness, just to stay at the technology edge – They might have done it even without the cash… So this was not failure of math, just corporate culture as usual. More interesting would be to read about the NIST part of the story

18. **maqroll**
December 24, 2013

Trust the mathematicians (who work for the NSA)?
" requested removal of an NSA employee from an IETF group co-chairmanship"
http://www.ietf.org/mail-archive/web/cfrg/current/msg03554.html
(by way of Bruce Schneier's blog https://www.schneier.com/blog/archives/2013/12/nsa_spying_who.html)
I'm not a mathematician or cryptographer. I do not know whether or how/how much the particular algorithm/standard is important. Maybe not at all, maybe a lot. But the question is whether we can trust even mathematicians. Distrust of science & scientists is widespread – in many contexts (e.g., pharmaceuticals) scientists are seen as shills for the corporations that employ them. Naively, one might have thought mathematicians were immune to this sort of suspicion – after all they have to provide "proofs" don't they?
How should mathematicians who work (worked) for the NSA be treated? Ban them from at least positions of power on cryptographic standards bodies? Trust them, which becomes impossible in light of Snowden's disclosures?

19. **Abraham Sternlieb**
December 25, 2013

Peter
After reading all the above,it is not unreasonable to speculate that Milner Prizes represent a sofisticated endeavor by some unfriendly party to sabotage the USA physics community,by means of encouraging the brightest minds to go into unreasonably wasteful directions such as String Theory,thus diverting them from generating meaningful and useful science for the nation

20. **Falcon**
December 25, 2013

IMHO the amount of panic Snowden's revelations have generated is becoming pathological. I find especially amusing comparisons with the USSR/KGB, not to mention Orwell's Oceania. It is remarkable how before Snowden the unbearable involvement of the government in our personal lives went completely unnoticed. As someone originating from the USSR I assure you that we did not have the privilege of this blissful ignorance.

What I find even more ridiculous is making the mathematicians employed by the NSA into some sort of villains, as if they are in any way responsible for the abuse or at least knowingly followed obviously foul orders. I don't think that if the NSA and other defense related organizations consisted exclusively of Snowdens the nation would be better off.

21. **milkshaken**
December 25, 2013

    the sad part is that what must have been a top management betrayal is going to affect all current and former RSA employees; it's something bound to come up every time they apply for a job.

22. **paddy**
December 25, 2013

    A somewhat on-topic news item: Crown Finally Pardons Alan Turing.

23. **paddy**
December 25, 2013

    Sorry forgot the link: http://www.bbc.co.uk/news/technology-25495315

24. **milkshaken**
December 29, 2013

    Matthew Green fills in new background info on the NIST involvement (on his blog – a post written yesterday). Turns out, at least some cryptographers on the committee that helped to set the new standard knew about the backdoor right from the beginning – since January of 2005 – and they went along with it and kept quiet…

25. **werner**
January 5, 2014

    It seems like it took less than two years for a couple of guys at MS to identify the weaknesses of this algorithm and warn against its use. The warnings were public and I suppose that anyone whose life or livelihood really depends on crypto would have learned of them quickly. If their trick couldn't go unnoticed for two years, maybe all those NSA mathematicians aren't THAT good after all…

26. **Peter Woit**
January 5, 2014

    werner,
    Perhaps the point of this story is that the NSA mathematicians don't need to be very good when others are being paid to look the other way.

Trespassing on Einstein's Lawn

December 29, 2013
Categories: Book Reviews

Amanda Gefter, a science writer who has often covered theoretical physics topics for New Scientist, has a new book coming out soon, Trespassing on Einstein's Lawn. On one level it's a memoir, telling a story that begins with her father getting her interested in fundamental questions about physics. This led to a career interviewing well-known physicists and writing about these topics, and now, a book. Self-reflexivity is a major theme of the book, with one aspect of this the way it tells in detail the story of its own genesis and creation.

In many ways, it's comparable to last year's book by Jim Holt, Why Does the World Exist?, with both books motivated by versions of the question "Why is there something rather than nothing?" In both books, there's a memoir aspect, with the author front and center in a search for answers that involves meetings and discussions with great thinkers. For Holt, these were mostly philosophers with a few physicists thrown in, while for Gefter they're mostly physicists, with a few philosophers making an appearance. These are lively, entertaining writers with wonderful material about deep questions, and I greatly enjoyed both books. Gefter is the funnier of the two, and I had trouble putting the book down after it arrived in my mail a couple days ago.

For those familiar with the topics she covers, the descriptions of her encounters with famous physicists is what will most likely provide something new. A few examples:

- She somehow managed to get to moderate a private debate between Lenny Susskind and David Gross, mainly on the topic of the multiverse. Much of the result is familiar to anyone following the topic over the last ten years (Gross detests the multiverse, Susskind is madly in love with it), but one interesting aspect is Gross's comparison of Susskind's behavior to his own back in 1984-5:

  > What I'm saying… is that some of the reaction is exactly like the reaction I got for exuberance in 1984, when we believed the answer was around the corner and we got carried away with that position. And, Lenny, you are carried away with this position. The stakes are damn big. So you are open to severe criticism.

  So, it seems that Gross is accusing Susskind of engaging in hype deleterious to physics, while acknowledging that he did much the same thing to get string theory unification off the ground and widely accepted.

- Several string theorists pointed out that strings themselves have pretty much disappeared from the story. The emphasis is now on the holographic principle and the hope for some unknown M-theory that embodies it. About M-theory, Polchinski has this to say:

  > It's remarkable to know so much about many limits and yet have no good idea of what they are limits of! Holography is clearly part of the answer. The fundamental variables are probably very nonlocal, with local objects emerging dynamically.

  Witten tells Gefter that the "M" in "M-theory" really was intended to refer to membranes. He doesn't see much happening though as far as new ideas about understanding it:

  > …in the mid-eighties and mid-nineties, before the second revolution happened, there were kind of hints that something was going to happen – I didn't know what, of course. I don't have that feeling now, but perhaps other people do… If I had my druthers I'd like to go deeper into what's behind the dualities, but that's really hard.

- John Wheeler plays a large role in Gefter's story, which starts with her asking him a question at this conference in 2002. She has a fascinating description of Wheeler's journals, which have been preserved in Philadelphia, where she and her father spent quite a lot of time looking through them.

The list of interviewees includes also Kip Thorne, Raphael Bousso, Tom Banks, and Carlo Rovelli.

Gefter makes it clear that she started out with essentially no background in physics or math, other than enthusiasm shared with her father for speculation about "nothingness" and the like. She studied not physics, but philosophy of science at the London School of Economics. Despite this lack of technical training, she does a good job of accurately characterizing what the physicists she talked to had to say. Towards the end the book does suffer a bit as she moves away from reporting what others are telling her to expounding her own interpretation of what it all means.

While I liked the book, at the same time I found the whole project deeply problematic, and would have reservations about recommending it to many people, especially to the impressionable young. The part of physics that fascinates Gefter is the part that has gone way beyond anything bound by the conventional understanding of science. This is really and truly "post-modern physics", completely unmoored from any connection to experiment (the discovery of the Higgs in the middle of the period she is writing about just gets a short footnote). The questions being discussed and answers proposed are woolly in the extreme, focused on issues at the intersection of cosmology and quantum mechanics, suffering from among other things our lack of a convincing quantum theory of gravity. Gefter seems to be sure that the problem of quantum gravity is an interpretational one of how to talk about a quantum cosmology where observers are part of the system. The very different, much more technical issue of how to consistently quantize metric degrees of freedom in a unified way with the Standard Model fields is ignored, perhaps with the idea that this has been solved by string theory.

Not recognizing that this post-modern way of doing science is deeply problematic and leading the field into serious trouble isn't so much Gefter's fault as that of the experts she speaks to (David Gross is an exception). Those taking the field down this path are dominating public coverage of the subject, and often finding themselves richly rewarded for engaging not in sober science but in outrageous hype of dubious and poorly-understood ideas. Only the future will tell whether the significance of this book will end up being that of an entertaining tale of some excesses from a period when fundamental physics temporarily lost its way, or a sad document of how a great science came to an end.

Comments

1. **CIP**
   December 30, 2013

   I have the feeling that we are going to need to discover some new physics before philosophical speculations about its new foundations will be very rewarding. I'm an old geezer, but I will probably read the book just for the interviews.

2. **Stacy McGaugh**
   December 30, 2013

   Fascinating. It was (in part) Gross's excessive enthusiasm for string theory in the mid-80s that drove me (as an impressionable grad student at Princeton) away from theoretical physics (and into astronomy). String theory may have been a beautiful idea, but it made no predictions that could be tested experimentally in the then-foreseeable future. That's not science.

   A quarter century later and the theoretical physics community has yet to wake up and realize that there is new physics right under their noses – just not the new physics they've been expecting (GUTs, strings, membranes, etc.). Galaxy dynamics are consistent with a single, universal force law, but this unexpected behavior has largely been ignored because it doesn't fit with particle theorists' dreams of super symmetric dark matter particles. That we do not understand the observed behavior makes it *more* interesting than the "expected" (but unobserved) new physics: who ordered this?

If we are witnessing "a great science come to an end" then a large share of the blame can be laid on our arrogance in putting theoretical ideas ahead of empirical facts.

3. **imho**
December 30, 2013

Great science will not come to an end in a technologically driven society like ours. Resources will likely be shifted to more economically productive areas, but that's a different issue and in my opinion… probably healthy. Society could benefit with more smart people thinking about plasma physics, fusion, quantum computing, computational algorithms, etc and fewer people worrying about the Planck scale. There's nothing wrong with wild speculation at the frontiers of knowledge. It's only a problem if it starts to consume a disproportionate share of the available resources.

Hi Stacy, MOND (if that's what you're talking about) has been ignored because physics is not really about unmotivated empirical curve fits. Don't get me wrong, classical forces where great stuff back in the Maxwell / Newton eras, but we're not there anymore. When MOND can also explain time dilation and space time curvature all while admitting a cosmological constant… maybe we'll pay more attention. Moreover, the bullet cluster data even breaks the curve fit.

4. **Peter Woit**
December 30, 2013

All,
From what I remember, there's not a word about MOND in the book (and definitely not in my posting). Those who want to argue about MOND or other dark matter models will have to do this elsewhere.

5. **Ed Mitten**
January 1, 2014

Turn the M in M-theory upside-down and you will see what it refers to.

6. **Yatima**
January 1, 2014

Oh Ed, you so crazy!

I do think this little article is relevant to the state of the domain:

We need to talk about TED: *Science, philosophy and technology run on the model of American Idol – as embodied by TED talks – is a recipe for civilisational disaster*

7. **Neil**
January 1, 2014

"Turn the M in M-theory upside-down and you will see what it refers to."

The W in Woit?

8. **A.J.**
January 1, 2014

Wembrane?

9. **Peter Woit**
January 2, 2014

Yatima,
Best to leave discussion of TED to another time. They do all sorts of things, some more dubious than others (I did get involved once, see http://www.math.columbia.edu/~woit/wordpress/?p=3985 and that invitation came from a TEDx person who explicitly wanted something different than the silliness about physics they often have).

Related to this book though, I think people often make the mistake of blaming science popularizers for the multiverse and similar outlandish speculative stuff. I see this differently, with the main source of the problem leading figures in the physics community. There always has been and always will be nonsense in the popular media about fundamental physics, but the new and dangerous thing going on is that it is now being driven by leading academics. The TED talks about the multiverse are not the ones being given by Deepak Chopra….

10. **Kavanna**
January 2, 2014

Stacy: Nice to hear that someone was driven away from string theory so early. Those of us remaining in theoretical physics had to contend with this cult until we couldn't stand it any more.

The Wacker posting (Quora) on integrity in science was heartening to read. It's good to know that being a "senior scientist" full of one's own half-baked ideas isn't enough to steamroller the basic checks that authors are supposed to do. Sadly, the string cult is an example of a once-thriving area of physics dominated by senior figures who went around the bend 30 years ago, taking a generation of impressionable students with them.

The sociology of a (former?) science dominated by senior figures off their rockers is an important theme in Lee Smolin's The Trouble With Physics.

11. **Chris W.**
January 5, 2014

See this essay by Amanda Gefter at Edge.org, which presents what I'd guess to be an important underlying theme of the book—including its problematic aspects.

Acknowledgments

December 30, 2013
Categories: Uncategorized

Two of the prominent string theorists working on ideas about holography and cosmology featured in Amanda Gefter's new book are Tom Banks and Willy Fischler, who have a new paper out on the subject, entitled Holographic Space-time and Newton's Law. Besides the usual sort of thing, this paper contains a rather unusual acknowledgments section (hat-tip, the Angry Physics blog):

> The work of T.B. was supported in part by the Department of Energy. The work of W.F. was supported *in part* by the TCC and by the NSF under Grant PHY-0969020. However, the authors *do not* thank either of these agencies, nor their masters, for the caps placed on their summer salaries, nor for the lack of support of basic research in general.

It seems that while debating philosophical issues concerning holography and cosmology can put one at the upper end of the current academic star system pay scale, it doesn't stop one from getting embittered that it's not enough. The authors did revise this text a few days later to remove the complaints.

For those who don't know what this is all about, prominent theoretical physicists (and mathematicians) in the US generally have research grants that pay them not only research expenses, but "summer salary". Historically, the reasoning behind this was that academics needed to teach during the summer to make ends meet, so agencies like the NSF would get them more time to do research by paying them to not teach. That was long ago, in a distant era. At this point the typical sums universities pay for summer courses are so much smaller than the academic-year salaries of successful senior academics that few would consider dramatically increasing their teaching load this way to make a little extra money.

Taking the NSF as an example, the standard computation is that an academic's salary is considered just pay for nine months, with the NSF allowing grants to pay for up to two months of summer salary. In other words, grant applications can include a request for 2/9ths of a person's salary, to be paid as additional compensation in return for not teaching summer school. As the salaries of star academics (who are the ones most likely to get grants) have moved north of 200K/year, these additional salary amounts have gotten larger and larger, crowding out the other things grants pay for (post-doc salaries and grad-student support are the big items).

Several years ago the mathematics part of the NSF instituted a "salary cap" on these payments, limiting them to about \$25K/year. This year, in response to declining budgets, such a cap was put on payments to theoretical physicists, at \$15K/month. So, any theorist with an academic year salary of over $135K/year saw a reduction in their additional compensation (although as far as I know only two were so outraged by this that they complained in the acknowledgments sections of their papers). The report of this year's panel on the future of particle theory in the US includes the language:

> This past year, the DOE instituted caps on summer salaries, and the NSF is following suit. We agree that this is preferable to further cuts in student and postdoctoral support, but it should be noted that still lower caps will have implications for research productivity, particularly if they reach the level of junior faculty (assistant or associate professor salaries). Many researchers may have to supplement their income with further teaching or other responsibilities in the summers.

Since Banks and Fischler work at public universities, one can check for oneself that they are seriously impacted by the new caps. Fischler is at the University of Texas, Banks has positions at UC Santa Cruz and Rutgers (I have no idea how the two institutions split his salary). Some of the grant information is also publicly available, for instance the NSF grant referred to in the acknowledgment is this one. It expires soon, but was supposed to provide \$690K over three years, presumably including summer salary for Fischler, Weinberg and three others. One anomaly here is Weinberg, who at over $500K/year is likely the highest paid theorist in the US. The same people have a new grant recently awarded, for \$220K.

Comments

1. **Seyda Ipek**
   December 30, 2013

   They have a newer version of the paper, which leaves the last bit of the acknowledgements out.

2. **CIP**
   December 30, 2013

   Research physicists need Porches too…

3. **lun**
   December 30, 2013

   Does the AdS/CFT correspondence put a quantum limit on how small can a violin be?

4. **Jeff M**
   December 30, 2013

   Uh, do Banks and Fisher have some sort of special deal? UT Austin pays full profs 144K and UC Santa Cruz 128K, according to the AAUP, so the effect of this on either of them would be either very small or nonexistent. Of course UTA pays Weinberg who knows how much, so maybe both have some special deal. In any case it's honestly gross, I mean if they wanted money they could have worked in industry, or become lawyers, or worked on Wall Street.

5. **Peter Woit**
   December 30, 2013

   Jeff M,
   I don't know what those AAUP numbers mean. The Texas state info I linked to has a range of about \$90-240K for tenured faculty in the UT physics department, with Weinberg way off scale at \$536K (Fischler is at \$192K).

   Banks was one of the four string theorists brought in by Rutgers in 1989 with a very special deal (I wrote about Dan Friedan's description of how it happened in my book…). At the time he would have been one of the highest paid theorists in the country and this is probably still the case (UCSC lists him at $243K, Rutgers at \$235K, but I think those are full-time salaries and he's not full-time at both places).

   In any case, the amounts lost to the salary cap should be quite significant for both of them.

6. **A.J.**

December 30, 2013

Questions for anyone angry that some theorists might be paid well:

1) Does it worry you that our society rewards a lifetime of Nobel-quality academic work less richly than a few years playing for the Yankees?

2) Would we really be better off if Weinberg had gotten a JD instead of a PhD?

3) How does our culture benefit if the only way to make significant contributions to science is to sacrifice some or (quite possibly) _most_ of your potential future earnings?

4) Who exactly are we selecting to be our scientists if we insist that a scientific career necessarily involves a monastic lifestyle?

5) Is there any evidence that limiting the pool of scientists in this fashion produces better science?

6) Are we better off as a society if we take care to pay scientists poorly enough that they are underrepresented among the politically influential social classes?

7. **Peter Woit**
December 30, 2013

A.J.,

The “summer salary” issue is a different one than that of whether theorists are paid well. Weinberg’s \$536K/year from UT should be compared to the fact that their football coach is getting $5.266 million/year (although he doesn’t have tenure and the team is not doing well…), so you can easily make the case that Weinberg is a bargain for UT. But would it really make sense for the NSF to pay him an additional 2/9ths of this (about \$120K), taking this money away from hiring postdocs and funding grad students, with the excuse that otherwise he wouldn’t have time for research because he would have to teach summer school? Or, maybe there should be a salary cap of some kind?

Of course I have no idea what Weinberg’s actual “summer salary” arrangement is, and he’s not known to have complained about it getting capped.

8. **A.J.**
December 30, 2013

Peter,

I don’t think CIP or Jeff M were talking about summer salaries. In any case, I think it’s worth keeping in mind that we’re talking about a system with a power law distribution of rewards where the most highly compensated are barely on the bottom rung of the top percentile.

9. **Peter Woit**
December 30, 2013

A.J.,
Yes, top string theorists in the US are “barely on the bottom rung” of the top income percentile. Or put differently, have a great job, perfect job security, and are making about six times as much as the average citizen. Lots of ways to look at this, but you don’t have to be an Occupier to think that people in such a situation probably shouldn’t expect sympathy when they complain about their sort, or that scarce NSF funds shouldn’t be given to them proportionately to how far up they manage to climb in that top income percentile.

10. **A.J.**
December 30, 2013

The string theory salaries discussed here don’t get you to that bottom rung. I was talking about Weinberg.

11. **Peter Woit**
December 30, 2013

A.J.,
I don’t know where you’re getting your data. I was looking at
http://politicalcalculations.blogspot.com/2013/09/what-is-your-us-income-percentile.html
where, if I put in \$210,000, I get 99.0% for income percentile ranking “among all U.S. individuals with incomes”.

Household income is something different, there it looks like \$310,000 gets you to 99.0%. From what I remember, Weinberg’s wife is or was a lawyer, so likely to contribute significantly to their household income. I have no idea what the average spousal income is for top string theorists, but it could very well be at least \$100K, putting them in the top 1% for household income also.

12. **A.J.**
December 30, 2013

Let’s grant that you’re right about the percent cutoff. (Although iirc wikipedia puts the cutoff at about $350K for household income, which could be slightly above the sum you’re discussing.) It doesn’t change my point: These guys may look a bit silly for griping about summer salary in particular, but the big problem here isn’t that one of them might be well off enough to afford an expensive car. It’s that we’re massively underfunding scientific research.

My original comment was an attempt to get people to realize that there’s some tension in thinking that we don’t pay scientists well enough and being angry that some of them are quite well paid. You can try to level compensation within science, but a) I suspect this doesn’t really work without at least massive changes in the grant system, and b) I think the message it sends to the rest of society — namely, that we put a limit on the value of scientific research — is a bad one.

13. **Peter Woit**
December 30, 2013

A.J.,

I just don’t agree that “we don’t pay scientists well enough”, especially when you’re talking about star-academics. I don’t have time to go get precise numbers, but typical salaries for tenured faculty have for many years more than kept pace with inflation, and outpaced typical salaries for the average person. Yes, people in a few other fields (finance, professional basketball, university administration) have done even better, but I don’t see how that’s an argument for higher academic salaries, any more than it’s an argument for paying everyone in society more.

What has changed since the 60s in the academic pay scale is an increased income spread, including a huge growth in the number of very poorly paid adjuncts, with stars at the top doing better and better. I don't see how taking limited NSF resources away from those at the bottom of the research hierarchy (grad students, post-docs) to make sure that those at the top keep climbing into the top 1 percent sends a good message to the rest of society. And academic 1 percenters whining about a summer salary cap doesn't help the image of science either, it definitely won't convince the average US citizen that they need to support better science funding.

To the extent it's true the US is underfunding scientific research, it's not because it's not writing large enough checks to successful academics for additional compensation. It's because it's not doing things like spending the $10-20 billion it would take to build a higher energy collider. That's not spending that would go to the 1 percenters of science, but spending that would go to building things and employing lots of more modestly paid people.

Sorry that this is becoming a bit of a rant, A.J. You're welcome to respond, but other than that, from now on I'll do my best to restrain myself and others and try and stick to the summer salary issue rather than larger issues of inequality in US society.

14. **Jeff M**
December 30, 2013

Well I should probably stay out of this, but I'm with Peter on this one. Academics (with tenure, at good schools, a quickly shrinking group) do quite well in relation to almost anyone else. They have of course worked very hard for that, but plenty of people work very hard, and many of them make very little money. Try teaching little kids sometime. And the adjunct issue is quickly becoming a major problem, which most of us with tenure unfortunately just ignore. Imagine spending years getting a doctorate, and then ending up teaching 6 classes at three different schools each semester, and earning (if you're lucky) 40K for the year. My brother in law, who has tenure at a very small school, where he doesn't make much, sometimes ends up teaching an extra class if he was worried something wouldn't run because enrollment would be low (and keep in mind he teaches a 4-4 load). When this happens, he just gets paid adjunct rates for the class. I think nowadays it's about 2K per class. There is no way I would ever do that, but I'm lucky, I have tenure at a pretty good state school.

15. **paddy**
December 30, 2013

Just saw PW's last response and as a result have painfully swallowed the diatribe I was writing…suffice it to say I agree with PW (after decades of experience in academia, industry, and government).

16. **A.J.**
December 30, 2013

Not much to add. I've already belabored my point, I don't have any easy fixes for the 80-20 problem, and I'm happy to see the conversation turn from "why are these few guys getting tens of thousands of dollars?" to "why aren't all these folks getting collectively billions?".

17. **Zathras**
December 30, 2013

" From what I remember, Weinberg's wife is or was a lawyer, so likely to contribute significantly to their household income. "

She's a professor at the UT Law School, so her income (at least what she gets from UT) is readily available: $229,949.
http://www.texastribune.org/library/data/government-employee-salaries/the-university-of-texas-at-austin/louise-weinberg/252999/

18. **CIP**
December 31, 2013

I wouldn't mind a world in which top physicists get paid better than football coaches, but given the choices that DOE and NSF had (cutting faculty summer salary or student support) its pretty tawdry for pretty well off guys like Banks to whine in their acknowledgments – the whines might be better appreciated in the science faculty lounge – and they might not get hella sympathy from humanities types either.

19. **Russ Whirton**
December 31, 2013

A tenured academic making \$200k is dramatically better off than a professional making \$200k: The academic has much greater flexibility, and is typically required to work only about twelve hours per week (most work much more, but some don't, and none lose their job for working the bare minimum). The academic can spend a significant period of each year at conferences of his choosing, which are often just a quasi-holidays. Every few years, the academic gets an even bigger holiday in the form of a sabbatical. As the academic ages into his 50's, 60's and 70's, he continues to receive his full salary – there is no concern about losing his income after being ousted by smarter, harder working youngsters, and no real concern about losing his job to layoffs or corporate bankruptcy.

Many academics in their 20's and 30's do work extremely hard. But they know that at any time after tenure, they can reduce this workload to whatever level they desire, and preserve their same income stream into their 80's. For a hard working professional to aspire to the same cushy lifestyle and high degree of security, the only means is to stuff a lot of cash into the bank during their young productive years. I would therefore argue that an academic salary of \$200k is actually equivalent to a professional's salary of around \$600k.

Don't get me wrong – I'd certainly like lawyers and athletes to be paid a lot less. But I do feel academics tend to miss the big picture of financial life in the real world.

20. **kashyap vasavada**
January 1, 2014

Very interesting discussion. I think, the whole grant system for research has corrupted American universities beyond any reasonable limit. While it is true that one needs grants to buy equipment, to pay graduate students and to get release time from teaching to do research and poor students should not pay for that, it has become excessive capitalist enterprise for universities. Administrators like it because they get free overhead money without doing anything. In turn they pay professors who get large grants large sums of money. Usually even promotion and tenure are also related to how much money you bring in. As it is well known, for faculty at small universities, it has become extremely hard to get grants in basic physics. The success rate for a proposal is usually less than 10%. In India, it is certainly not like that. I wonder how it is in Europe. Most likely they have a different system.

21. **OMF**
January 1, 2014

Paying someone on a salary for only 9/10 months of the year is a ridiculous practice. Either they are a full time employee or else they are on wages. And if they are on wages, institutions should reduce their expectations of work output as a result.

Discussion of tenure do not enter into this. Employers cannot be allowed to operate with such bizarre practices while still claiming to employ people full time.

22. **Peter Woit**
January 1, 2014

OMF,
The “9 months” salary scheme at US universities is a bit of a fiction, there for the convenience of the faculty. People are almost always paid this “9 months” salary in 12 equal installments monthly. The reason for the 9 month business is to allow people to claim not to be paid during the summer, so eligible for “summer salary” for up to three months. In some fields there probably are ways for faculty to make significant sums working at something else over the summer. Among most of the mathematicians and physicists I know though, the only viable summer job is teaching, and that doesn’t pay enough to make it worthwhile except sometimes for the most junior people. So, in practice they’re collecting the 9 month salary just like a 12 month salary, with some hopes of additional compensation of up to 2/9ths of their yearly salary via the grant system.

23. **Pawl**
January 1, 2014

These are important points (which may be coming at a particularly awkward time, for academic funding in general). I’d like to know more about the issue of overheads, raised by Kashyap Vasavada. What is the argument for universities just getting a fixed percentage overhead on all (standard) grants? Do they have to account for the money?

24. **Peter Woit**
January 2, 2014

Pawl,
As far as I know, universities have an overhead rate they have negotiated with the granting agency, and rules as to what things the overhead rate applies to. Summer salary is something that they get overhead on, so it’s not just individuals benefiting from the summer salary arrangement, but their institutions also.

The standard argument for overhead is that the university is providing facilities used by the faculty member to carry out their research (office, staff, lights, library, computer network, etc. etc.) so they should get part of the grant money to pay for this.

25. **Jeff M**
January 2, 2014

Pawl

In my experience (as a department chair) overhead money goes to the department to use as they see fit. Some will let the PI choose what to do with it, some just put it in the general department fund. I assume there are schools where the money never gets to the department.

26. **Pawl**
January 2, 2014

Peter, Jeff, thanks for your replies.

I had heard the arguments about offices, lights, etc., before, but it is not obvious how they are justified at a quantitative level (and there are reasons I can think of for being skeptical about the whole idea). Does anyone know what is involved in universities’ negotiations with grant agencies in setting the overhead, and in simply taking it to be a percentage of the grant?

27. **Peter Woit**
January 2, 2014

Pawl,

For a recent news story about this, see
http://www.bostonglobe.com/news/nation/2013/03/17/harvard-mit-and-other-research-schools-thwart-obama-administration-effort-cap-overhead-payments/Nk5PT0Mc8MQZihFVNs5gNK/story.html
which has
“Overhead rates are calculated based on a complex formula that takes into account an institution’s historical costs for construction, maintenance, utilities, and administration.”
Critics point out that this formula is leading to the wealthiest institutions (e.g. Harvard) getting the highest rates since they have the most expensive construction and the highest paid administrators.

There was a scandal about this back in the early 1990s, see
http://en.wikibooks.org/wiki/Professionalism/Stanford_Research_Scandal
when it became known that the president of Stanford was charging personal expenses (yacht, wedding ceremony, flowers) as “research expenses”. I haven’t heard much about this though in the past 15-20 years.

28. **Peter Shor**
January 2, 2014

After the Stanford scandal, universities have become much, much more careful about charging inappropriate expenses to research grants. I don’t think we’ll see something like that again any time soon.

29. **Kuas**
January 2, 2014

Does it make any sense to lash out at the NSF for the inadequate overall levels of research spending? I’m sure the NSF would be happy to dole out as much funding as congress would give them.

Anyhow, if you happen to be working in string theory in the US, you should also factor in to your compensation calculations the significant probability your turn will eventually come up for the $3M Milner payoff.

30. **Pawl**
January 2, 2014

Peter, thanks once more. (There’s an NSF page.) A couple things come out of this:

(a) Indirect Costs are **not** readily identifiable with the research project (they are not the costs of keeping the researcher's lights on, etc.) but are part of the general cost of keeping the organization running.

(b) Indirect Costs are negotiated (or set by the granting agency). I have not (so far) seen any "complicated formula" which determines what they are.

It is completely unclear from this why Indirect Costs should be a percentage of the grants. The whole thing seems to be a general way of subsidizing organizations (mainly universities) which produce research, but without really specific objectives.

The questions of what exactly is being rewarded, and what effects these incentives have, really deserve some attention.

31. **Peter Woit**
January 2, 2014

Pawl,

If you take a look at
http://www.nsf.gov/bfa/dias/caar/docs/idcsubmissions.pdf
especially Attachment IIIa, you can see a sample computation of an indirect rate.

32. **Bernhard**
January 7, 2014

kashyap vasavada,

"I wonder how it is in Europe. Most likely they have a different system."

Not really. In some universities in Europe getting an important grant (like ERC) is sometimes the ONLY way to get a tenure-track position. Furthermore when positions are open "demonstrated ability to acquire external funding for research projects" is one of the first things people look at here. Mind that some universities don't even pay full salary, they simply expect you to get grants. And I'm not talking about the " "9 months" salary in 12 equal installments monthly." that Peter was mentioning. You need to get a grant at some point otherwise you risk getting fired.

Short Items

January 2, 2014
Categories: Uncategorized

- Harvard has announced that the Chinese firm Evergrande Group will be supporting various activities at Harvard, including a new Center for Mathematical Sciences and Applications, with S.-T. Yau as director. No details of what the center will do other than "serve as a fusion point for mathematics, statistics, physics, and related sciences." The company has its own announcement here (they might want to check on the name of Harvard's President…).
- The new Physics Today has an article Paul Ehrenfest's final years, a sad bit of physics history I'd never seen the details of.
- Last month in Moscow there was a conference for Boris Feigin's 60th birthday. Videos of the talks are now available here.
- Dick Gross's wonderful lecture series here at Columbia on *Representation theory and number theory* has been available on video since he gave the lectures. Now Chao Li at Harvard has produced a transcription of the talks, so a high-quality written version of the material of the lectures is now available. This is one of the best sources around to learn about the local Langlands conjectures. His website contains a lot of other interesting expository material.
- Phenomenologist Jay Wacker has a blog at Quora, called Particle Physics Digressions. The latest entry is an odd tale of something I would have thought was rather unusual, but Wacker says it's not exceptional, happens everyday.

Comments

1. **Peter Donnelly**
   January 3, 2014

   Ehrenfast article sounds interesting, but not $30 interesting.

2. **Thelonious**
   January 3, 2014

   I have to agree that paying 30$ is a little hard to swallow, do you know if there is a good summary somewhere ?

   Do you know if there is going to be an Eilenberg lecture this coming semester (Gross's lectures are amazing !!) ?

3. **Thelonious**
   January 3, 2014

   I should add that Chao li's lectures have been around for at least a year I think. On the other hand he just wrote, this semester, lecture notes from a course by Jack Thorne (http://www.math.harvard.edu/~chaoli/doc/AutomorphicForm.html) which, as far as I can tell, are one of the best ressource for learning thoroughly about the important objects in the number theoretic Langlands program (it isn't just a broad overview, he actually proves stuff).

4. **Peter Woit**
   January 3, 2014

   Thelonious,
   No Eilenberg lectures this spring, Joe Harris just finished his lecture series a couple weeks ago. I second the recommendation of the Thorne lecture notes.
   If anyone knows of a another source for the material about Ehrenfest, let me know. The policy of Physics Today to charge $30 to look at an article seems to have no point other than to ensure that no one does it.

5. **David Brown**
   January 3, 2014

   http://books.google.com/books?id=qsodmIGD0fMC&pg=PA232 excerpt from bio of Dirac by Farmelo
   http://www.lorentz.leidenuniv.nl/history/Ehrenfest_Burgers/Physics_Today/PTO000026.pdf

6. **claudius**
   January 3, 2014

   If even you think that $30 is too much to pay to look at an article,
   why did you even bother to link to it?

7. **Martin**
   January 3, 2014

   http://www-history.mcs.st-and.ac.uk/Biographies/Ehrenfest-Afanassjewa.html

   "Paul Ehrenfest through his life had suffered from low self esteem, but now began to suffer from depression. He was also greatly saddened by his youngest son Vassily who suffered from Down's syndrome and had severe problems both physically and mentally. On 25 September 1933 Ehrenfest shot Vassily in the waiting room of the Professor Watering Institute in Amsterdam where Vassily was being treated. Then he shot himself. The Dutch papers only reported his sudden death and gave lengthy accounts of his achievements. Ehrenfest-Afanassjewa returned to Leiden where she remained for the rest of her life. Not only did she lose her husband and youngest son in such a tragic way, but a few years later, in 1939, her eldest son Paul was killed by an avalanche while skiing in the French Alps."

8. **Peter Woit**
   January 3, 2014

   claudius,
   From my office at Columbia (as well as probably at many other places with an institutional subscription), Physics Today articles are freely available. I didn't realize they were charging $30 to non-subscribers (actually I'm wondering if that's new, hadn't seen that before).

9. **kashyap vasavada**
   January 3, 2014

   If you have an account at the campus computer center (most state univ. in U.S. will allow this for state residents) they will let you download physics today articles or even journal papers free. I am not sure about private univ.

10. **gs**
January 3, 2014

This first: A hat tip to Evergrande and Hui Ka Yan for their generosity. Hopefully it augurs constructive relations, not enmity, between the USA and China.

Nevertheless, it is surprising that a Chinese real estate company is underwriting mathematics and immunology at Harvard. (The Green Buildings initiative is more understandable.) The details would seem to warrant journalistic looking into, not in a spirit of suspicion but of due diligence. That said, I do not imply impropriety, and repeat my appreciation of Evergrande's action.

11. **Isidore Seveille**
January 3, 2014

IMHO, the donation from Evergrande is good PR work for Chinese companies given that some of those Chinese companies are placed under suspicion by the Congress and the media.

On a related note, Huawei, a Chinese telecom company, has been supporting mathematics and theoretical physics research in IHES, according to [this report](#) .

12. **Peter Donnelly**
January 4, 2014

OMG that was sad.

13. **Thomas Larsson**
January 4, 2014

I was excited to find that the Feigen conference was dedicated to, among other things, double affine and toroidal algebras. Unfortunately, none of the talks seems to be about this subject.

In my own work, I preferred to use the terms multi-dimensional affine and Virasoro algebras, rather than double (triple, quadruple, …) affine algebras, because the restriction to tori is not fundamental. Note that triple affine algebras are not related to gauge anomalies in QFT in 3+1 dimensions, since the extension is proportional to the second Casimir rather than to the third. The algebra pertaining to QFT gauge anomalies is called the Mickelsson-Faddeev algebra, and is something completely different.

14. **lcs**
January 5, 2014

12 issues of Physics Today costs $69, yet they charge $30 for a single article? Talk about not even wrong. Even if they charged $30 to download an entire issue it would be grossly out of proportion. Someone needs to upbraid these silly AIP publishers, who must be pompous beyond imagination to think their content merits this pay scale.

15. **Jeff Murugan**
January 6, 2014

There's a related story on the sad story of Ehrenfest's last few days in Graham Farmelo's "The Strangest Man"

16. **harryb**
January 6, 2014

Added a comment to the Ehrenfest article site on Physics Today noting the sentiments of frustration on this blog. Would have been preferable to have been able to comment on the content. Their choice.

17. **Paul Guinnessy**
January 7, 2014

We've made the Physics Today article free so that its easier for people to access. We do offer a $4 rental free to view the article, but we're still ironing out some quirks in our new publishing system. Hence I apologize that this option was not available at first.

The magazine is a benefit of membership in ten science societies. If you've previously registered with Physics Today, just click on the "sign in" option on the right hand side and you'll be able to access everything, including our entire back archive, no matter whether you're on campus or not.

18. **Peter Woit**
January 7, 2014

Paul,

Many thanks for arranging access to the article about Ehrenfest!

19. **harryb**
January 8, 2014

Good response from Physics Today – thanks.

20. **gs**
January 8, 2014

Thank you, Physics Today.

Scientists Find a Practical Test for String Theory

January 6, 2014
Categories: This Week's Hype

This sort of thing seemed to be dying down (2013 required a record low number of "This Week's Hype" postings), but 2014 is starting off with the usual promotion by physicists of nonsense about how they have "found a test for string theory". This time the news that [Scientists find a practical test for string theory] comes from a group at Towson University, who are basing their claims on [this paper], published [here]. I'm not sure where phys.org got this, but it reads like a university press release, and they credit "Provided by Towson University".

What's actually in the paper is a proposal for a test (and not a very good one, as far as I can tell…) of the equivalence principle. The claim is then that a violation of the equivalence principle would be evidence for string theory. I've written about this kind of claim before (see [here]), pointing out that string theorists sometimes argue that the equivalence principle is a prediction of string theory. So, string theory can be tested, and the test is even "practical", but since the prediction is that either the equivalence principle will be violated or not, it's pretty likely to pass the test.

Update: Another source for the press release is [here].

Update: Matt Strassler [weighs in], a week later:

> Baloney. Hogwash. Garbage.

Comments

1. **CIP**
   January 6, 2014

   If I understand correctly, the question here is really the validity of the so-called "strong" or Einstein Equivalence Principle (EEP), which insists that fields that couple directly to matter (like gravitation), couple through the metric. Brans-Dicke and other fifth force variants don't.

   The same may be true of the dilaton and some other string theoretic fields, but that doesn't seem to be iron clad. Still, any fifth force type effects seen would seem to give string theorists and other alternative gravity people something to work with.

2. **Jim Akerlund**
   January 6, 2014

   Peter,

   Here is a different test for equivalence principle, but no mention of String Theory.

3. **Peter Woit**
   January 6, 2014

   CIP,
   One thing that I don't doubt is that the day after evidence for a fifth force shows up, the headlines will be "Evidence found for string theory!"….

4. **Jim Akerlund**
   January 6, 2014

   Here is the missing link. Sorry.

   [Three Star Test]

5. **David**
   January 6, 2014

   How do they know that there isn't a fourth component in the system, too dark to see, but with enough mass to cause deviations from the computed 3-body solution?

6. **Allan Rosenberg**
   January 6, 2014

   You have to give them credit for honesty. How many people proposing this sort of test have the cojones to say that "[s]tring theory is infamous as an eloquent theoretical framework to understand all forces in the universe…."

7. **Paul**
   January 7, 2014

   One of the studies authors had this to say:

   "Hi, I'm one of the authors of the paper. It's crazy to see my undergraduate research on Reddit!
   Basically, some versions of string theory propose that elements will "fall" at different rates (i.e. violate the equivalence principle). Well, planets are "falling" around the Sun, along with asteroids in the same orbit. Moons are falling around planets as well.
   We have a pretty good idea of exactly where everything is supposed to be. When we calculate these things, we assume that everything falls at exactly the same rate (that is to say, inertial mass is the same as gravitational mass). And within margins of error, they ARE right where we expect them to be.
   So, if these versions of string theory that say things can fall at different rates are true, we should see some kind of discrepancy between the models and what we really observe. We don't see these discrepancies. This places limits on the parameters of those versions of string theory (and other "beyond the standard model" theories that don't technically involve strings).
   Of course, we don't know where everything is to an infinite number of decimal places. Perhaps Jupiter is 0.00001 arcseconds away from where we claim it is. We can't measure that precisely. So if there is an effect due to something from string theory, it would have to be hiding in that uncertainty. In our paper, we take the limits of that uncertainty and turn it into limits on how much something can violate the equivalence principle. From there, if you had a string theory model, you could calculate how much it would violate the equivalence principle, and if it was bigger than our numbers, you might want to be skeptical about your model (especially since our numbers are

quite conservative and aren't even as strong as other methods).
Hope that helps."

Interested to hear your response as I am not nearly smart enough 🙂

8. **Bee**
January 7, 2014

They propose to test for some kind of dilaton coupling. I actually read the paper last year. You only have to look at the first page to see that they are not actually claiming that they test string theory. They propose to look for generic violations of the equivalence principle that could hint at physics beyond the standard model. I think it's a reasonable proposal, but the phys.org headline is more than just misleading. It is a) wrong and b) most likely deliberately so.

9. **Bee**
January 7, 2014

PS: I said reasonable – I don't think it's terribly exciting because I can see no good reason the effect should appear in the parameter range that the experiments are sensitive to. That isn't to say one shouldn't look though.

10. **Zhiming Wang**
January 7, 2014

Wow, I like that "Three Star Test" referenced by @Jim Akerlund. A great example of how to spice up some regular scientific discovery/progress so that it sounds revolutionary.

11. **Shantanu**
January 7, 2014

I asked matt strassler about whether string theory violates equivalence principle
or not (since as Peter mentioned I have heard conflicting claims on this issue) and his reply was
http://profmattstrassler.com/2012/08/15/from-string-theory-to-the-large-hadron-collider/
(although you will have to sift through lots of comments to see his reply inline)

Regarding the actual paper, it doesn't cite the limits from binary pulsar(pulsar/white
dwarf systems) which I think they are equally stringent compared to solar system limits.

12. **Peter Woit**
January 7, 2014

For a serious discussion of the physics here, see Thibault Damour's recent
http://arxiv.org/pdf/1202.6311.pdf
especially page 12. I think a fair reading of Damour is that "the current string landscape prediction is no equivalence principle violation, but if equivalence principle violation is found, that just means string theorists need to look at other currently less popular string theory models".

So, usual story for string theory "predictions".

13. **chris**
January 7, 2014

if a violation of the strong equivalence principle would indeed be found, the one theory that raises in popularity would probably be MOND.

14. **Curious Wavefunction**
January 7, 2014

That's a little weird and it almost sounds like a tautology. It would be like claiming that the existence of friction is evidence for string theory (since string theory is supposed to be true).

15. **BobDastro**
January 7, 2014

> What's actually in the paper is a proposal for a test (and not a very good one, as far as I can tell…) of the equivalence principle

The authors are not proposing a test of the equivalence principle. Rather, they actually determine new (and significantly improved) upper limits to deviations from the equivalence of inertial and gravitational mass based on existing Solar System observations.

I would also say the paper makes no real claims about testing string theory *per se*. The authors cite in passing one speculative stringy model, but it is mentioned as only one example among several other classes of theoretical proposals that allow for composition-based differences between inertial and gravitationally-derived masses. IMHO this is a clearly written paper that is well worth reading for those of us interested in observational data that bear on the fundamental properties of gravitation.

16. **Eli Rabett**
January 7, 2014

Wife's comment on seeing the press release about a test for string theory: Oh, they found the Big Ball

17. **MathPhys**
January 8, 2014

From a quick reading of the paper, I agree with BobDastro's comment above. It's a paper on theoretical astronomy that basically says *"If you observe this, then that means that"*, which (assuming it's technically correct) is fine and educational.

Now if someone has decided to blow the string-theory-can-be-tested trumpet to attract attention, that's a different (and sad) issue.

18. **Peter Woit**
January 8, 2014

BobDastro/MathPhys,

This is following the usual pattern: published article includes only minor references to string theory, since no referee would allow the author to claim that this was a “test of string theory” (since it isn’t). On publication of the article, the author has their university press office issue a press release about how they have discovered a “test of string theory” (I don’t believe in claims that university press offices issue press releases about their faculty’s work without the faculty member’s agreement). The press release then gets spread through various media outlets, often with the outrageousness of the claims increasing as it spreads. Finally, you end up with lots of news stories like
http://www.huffingtonpost.co.uk/2014/01/07/string-theory-experiment-announced_n_4552931.html

There are by now dozens of examples of this. You can argue about who is responsible for the public getting misled here, my vote would be for the physicists who allow or encourage such press releases to go out (together with their colleagues who raise no objection or sometimes provide supporting quotes for the stories).

19. **Zathras**
January 8, 2014

It seems to be almost a contradiction in terms to have universities put out non-peer-reviewed press releases on peer-reviewed results. Maybe these press releases should be peer reviewed as well?

20. **chris**
January 8, 2014

the peers of university press people are other universities press staff, right? that would be a nice peer review.

21. **DaveC**
January 8, 2014

My university has put out press releases on our work (in condensed matter; not at our suggestion but at the journal’s). Our press guys take great pains to represent faithfully what we talk about in the interviews and incorporate all our own proof edits. I find it hard to believe this is the exception rather than the rule.

22. **Chris W.**
January 8, 2014

Press release = Marketing blurb

Is that too strong? There is an incentive for both the researcher and the institution to claim too much. (And yes, universities are operating more and more like for-profit entities.) Unless there are clear negative consequences in terms that truly matter to them, this stuff will tend to become standard practice.

23. **Olivier Minazzoli**
January 9, 2014

> I think a fair reading of Damour is that “the current string landscape prediction is no equivalence principle violation, but if equivalence principle violation is found, that just means string theorists need to look at other currently less popular string theory models

I don’t think so. As far as I understand, Damour says that EP violation is a generic prediction of string theory, but a widespread assumption is to say that there might be a mechanism that protects the EP. Though, it is just a “convenient” assumption that is used by “model builders” . In general, Damour’s work (originally with Polyakov [1]) shows that even a massless dilaton (which has a long range dynamic that should imply a long range violation of the EP) can lead to a very small EP violation only, because the dilaton field’s cosmological equation can have an attractor at late cosmic times (hence leading to a very small violation of the EP at current epoch, that would thus be compatible with current experiments). (Note that it is said that they “can” and not that they “would”, see also [2]).

In any case, although string theory generically predicts EP violation, observing one of its various representations is indeed not a test of string theory since string theory is not the unique theory to propose EP violation anyway. But it would certainly be a small argument in favor of string theory. Also, this “prediction” is qualitative only, which is not quite the standard of “checkability” in modern science…

Otherwise, as far as I understand, there might be a way to put a quantitative constraint on the string theory effective action. Indeed, the full loop effective action of string theory in the gravitational sector should predict a unique specific coupling between the dilaton field and material fields. But it has recently been shown that depending on the such a coupling, the so-called post-Newtonian parameter gamma can be either more than one or less than one [3] (or even equal to one for a very specific coupling [4]). Hence, as explained here, the sign of 1-gamma (if measured) may put a strong constraint on the non-perturbative effective action of string theory, if someone is able to compute it, of course…..

24. **Peter Woit**
January 9, 2014

Olivier,
You’re ignoring the “string landscape” issue completely. What “string vacuum” are you planning to use to evaluate the effective action of string theory? Damour explains clearly on page 12 that the standard string landscape scenario implies a prediction of no observable EP violations:
“Though such a mechanism might entail observable short-range modifications of
gravity [30], it predicts the absence of any long-range EP violations.”
This same argument has often been given by Douglas in recent years as a response to those like me who say string theory predicts nothing. According to him, the string theory landscape does make a prediction: no observable EP violations (because un-fixed moduli will contribute to the CC). According to you, string theory predicts EP violations.

I do think you’re both right: string theory predicts anything you want, either EP violation or no EP violation.

25. **Olivier Minazzoli**
January 9, 2014

Dear Peter, Damour also says p.12 (a little after your quote)

“This family likeness between the dilaton ϕ(x) and the metric g_{μν}(x) (which entails a correlated likeness, say in heterotic string theory, between g_{μν}(x) and the gauge couplings g^2_a(x), as well as the string-frame gravitational coupling G(x)) suggests that there might exist consistent string vacua where some of the moduli fields are not stabilized, but retain their long-range, spacetime-dependent character. As recalled above, such a situation would entail long-range violations of the EP. How come such violations have not yet been observed, given the exquisite accuracy of current tests of the universality of free fall (at the 10^{-13}level [26]) and of current tests of the variability of coupling constants [32]? A possible mechanism for reconciling a long-range, spacetime varying dilaton (or, more generally, moduli) field ϕ(x) with the strong current constraints on the time or space variability of coupling constants is the cosmological attractor mechanism [33, 17,34]”.

As far as I understand, it says that one can expect some moduli fields to be long-range, just as the metric is, and in particular the dilaton field. In that case, one should expect an EP violation, at least form the dilaton field.

Hence, although a violation of the EP wouldn't prove string theory of course (nor would it be a striking evidence that string theory might be correct), it still seems to me that it would be a **small** argument in favor, since one should expect such a violation from string theory.

Otherwise, in the last part of my last comment, I assumed that the dilaton-matter coupling in the effective action does not depend on the string vacuum considered (for instance, at tree level, the dilaton coupling is universal no-matter what). But I'd love to have a comment from an actual string theorist on this issue.

26. **Peter Woit**
January 9, 2014

Olivier,
It still seems to me that Douglas's argument is that no unstabilized moduli is the generic expectation in the landscape picture, and thus no EP violation. Damour is saying it is worth thinking about alternatives and "suggests that there might exist" ones that violate the EP. This is different than your "one can expect some moduli fields to be long-range".

I don't disagree at all that one can find string theory models with or without long-range moduli and thus EP violation. The question is which alternative to expect, with Douglas saying he expects no EP violation from string theory, you saying you expect some. String theory here has no predictive power. Put differently, if we see EP violation, Douglas's claims implies this is evidence against string theory, while you and the authors of this press release want to claim it would be evidence for string theory.

27. **Olivier Minazzoli**
January 9, 2014

Dear Peter,

it is not because several people claim opposite things that it means that the theory actually allow opposite things to exist within its paradigm. It could simply be that one of the two arguments is wrong. Only a formal proof would tell which argument is correct, but as far as I can tell, formal proofs are rather difficult to get in string theory. I don't know Douglas's argument that you are refereeing to. Could you send me a link please? I guess you have my email address. Thanks a lot 🙂

Otherwise, if the effective dilaton-matter coupling(s) in the effective 4D action is(are) indeed string vacuum independent, if the dilaton field is proven to be massless (or light), and if one can compute the full loop effective dilaton-matter coupling(s), then a measurement of the value of 1-gamma could be a strong constraint on string theory, because its sign depends on such a coupling [1]. That's a lot of ifs, but it's better than nothing…

28. **Jeff M**
January 9, 2014

What, physicists do formal proofs, really 🙂

29. **Peter Woit**
January 9, 2014

Olivier,
For a paper by Douglas relevant to this, see
http://arxiv.org/abs/hep-ph/0112059
But the reason I refered to the Damour paper is that he makes the basic argument explicitly (end of page 11 to page 12). For another similar Damour claims, see
http://pagesperso.ihes.fr/~damour/Conferences/Onera19sept2011.pdf
where on page 8 he writes "EP tests are important because they test an assumption commonly made in string theory, and could refute it."
I don't see any reason why dilaton matter couplings should be string vacuum independent.

In any case, Damour clearly is claiming that an assumption commonly made in string theory implies no EP violation. This is inconsistent with any claim that string theory generically will imply EP violation.

30. **Olivier Minazzoli**
January 10, 2014

Dear Peter,

thank you very much for the links.

Still, the way I understand Damour is rather that: in general, string theory predicts EP violation. However, one can use an assumption that forces string vacua to respect the (long-range) EP in order to get specific models one can play with. May it be widely used or not, as far as I understand, it is still only an assumption. Not a prediction.

Regarding Banks, Dine and Douglas paper, as far as I understand, it only argues that it *wouldn't seem natural* to have a variation of the dimensionless couplings, according to our current understanding of QFT. Though it probably shows the state of the art regarding the QFT's point of view on the problem of the variation of the dimensionless constant, I'm not quite sure one can deduce that it means that string theory predicts no EP violation, as you seem to argue. But maybe have you linked the wrong paper? Or maybe have I missed something?

Thanks.

31. **Peter Woit**
January 10, 2014

Olivier,
I've heard the argument a couple times directly from Douglas that string theory predicts no variation in moduli. For an example in print
http://arxiv.org/pdf/hep-th/0610102v3.pdf
page 18
"This is perhaps the simplest testable prediction of string/M theory for which contrary evidence has ever been reported"
(referring to claimed observations of variation of the fine structure constant). Here he refers to the Banks/Dine/Douglas paper. You're right though that I don't think it's just a naturalness argument, what I remember hearing from Douglas was an argument that variation of moduli would imply a huge variation in the vacuum energy, but I don't immediately know of a source where that appeared.

Of course the occurrence of large number of moduli fields in string theory means that generically you have lots of different sizable long-range forces, so that is some sort of "generic" (and completely wrong) prediction of string theory. The question is whether some such things survive the standard landscape picture. My reading of both Damour and Douglas is that this is not supposed to happen in this picture. Douglas is quite definite about there being a "prediction of string theory" here, and it's the opposite of a prediction of observable moduli.

32. **Rafael**
January 10, 2014

Dear Peter, do you believe it is true http://bnews.kz/en/news/post/180213/ ?

33. **Peter Woit**
January 11, 2014

Rafael,
Seems unlikely to be true (solution to Millenium problem), but I know little about this subject.

34. **Bernhard**
January 11, 2014

And it will be quite hard to verify it since the guy decided to publish the solution in a Kazakh journal.

35. **Olivier Minazzoli**
January 11, 2014

Dear Peter,

thanks again for this new link! 🙂

Thus, as far as I understand, according to the reference to Banks/Dine/Douglas paper in Douglas/Kachru paper, no EP violation is a "prediction" of QFT alone, not string theory in particular. (It seems to me that it is actually not a prediction, but more an expectation based on our current understanding of QFT). Therefore, it appears to me that Douglas's claim that no EP violation is a prediction of string theory is exaggerated (if not wrong), unless I have missed something.

If the EP is to be expected from QFT, as Banks/Dine/Douglas paper seems to tell, then it would mean that any theory with (measurable) EP violations would be "unnatural" from the QFT point of view, may this theory be one of the vacua of string theory with no EP violation or anything else.

It is very interesting. However, one should remain cautious with QFT's predictions since, as far as I know, QFT is still far from being completely understood, not to mention the possible loopholes that may hide behind Banks/Dine/Douglas argumentation.

Anyway, thanks for the discussion 🙂

36. **vmarko**
January 11, 2014

Olivier,

"no EP violation is a "prediction" of QFT alone, not string theory in particular"

The equivalence principle is a statement describing how gravity couples to matter. As such, it cannot be predicted by any theory which does not incorporate gravity at some deeper level. String theory notwithstanding, there are no standard QFT's out there that contain gravity in any shape or form, and therefore no ordinary QFT can ever predict the EP.

In order to seriously discuss the theoretical prediction/violation of EP, you need a theory of quantum gravity. And today it is common wisdom that QG cannot be described with the formalism of QFT (except in some very unusual hypotetical constructions involving the existence of a nonperturbative fixed point etc.).

So QFT in general has nothing to say about EP.

HTH, 🙂
Marko

37. **Peter Woit**
January 11, 2014

Olivier,
So, according to you, Douglas/Kachru are just completely wrong to in their claim about a "prediction" of the string theory landscape. Fine, I'm definitely not going to be the one to spend time defending string theorist's claims for their "predictions"….

38. **David Brown**
January 11, 2014

http://www.math.kz/images/journal/2013-4/Otelbaev_N-S_21_12_2013.pdf Otelbaev's publication in Russian with English abstract at the end

39. **Olivier Minazzoli**
January 12, 2014

Marko,

It might be a matter of nomenclature. By QFT, I mean a set of tools one applies on actual theories originally defined classically in order to get their quantum behavior. Now, as far as I understand, Banks/Dine/Douglas suggest that a coupling of the form "scalar times Lagrangian of material field (=\Phi L_m)" would be "unnatural" from the QFT point of view for a light scalar field (actually they restrict their attention to L_m=F^2). Such a type of coupling could indeed come from string theory but from many thing else as well. For instance, one can simply postulate such a coupling to begin with, without invoking string theory. I don't quite see how Banks/Dine/Douglas argumentation can be transformed into a "no Ep violation is a prediction of string theory". As far as I can see, it has nothing to do with string theory, but depends on QFT arguments alone.

Peter,

at least from what I read, I don't understand how such a claim can be made. But I could easily miss something. However, as far as I know, the string dilaton is massless at first loops and its coupling is of the form of the one considered by Banks/Dine/Douglas (see (2.1) and (2.2) in [Damour and Polyakov]). Besides, Damour/Piazza/Veneziano say

> A striking prediction of all string theory models is the existence of a scalar partner of the spin 2 graviton: the dilaton ϕ, whose vacuum expectation value (VEV) determines the string coupling constant g_s=e^{ϕ/2} [1]. At tree level, the dilaton is massless and has gravitational-strength couplings to matter which violate the equivalence principle [2]. This is in violent conflict with present experimental tests of general relativity. It is generally **assumed** that this conflict is avoided because, after supersymmetry breaking, the dilaton might acquire a (large enough) mass.

That would be nice to have Douglas explaining from where the claim that "EP satisfaction" is a prediction of string theory is coming from because, as far as I understand, the reference Douglas/Kachru use in their paper doesn't seem to say so. But I am certainly not the best person to talk about that.

40. **vmarko**
January 12, 2014

Olivier,

"By QFT, I mean a set of tools one applies on actual theories originally defined classically in order to get their quantum behavior."

Correct. And if your classical theory contains gravity (say GR), the QFT formalism breaks down (loses predictive power) since the theory is nonrenormalizable.

"scalar times Lagrangian of material field (=\Phi L_m)"

In the context of string theory, the "scalar" is the dilaton, which (one could argue) is a part of gravitational sector. The above coupling then violates the EP, unless one breaks supersymmetry high enough etc.

Outside of the context of string theory, there is absolutely no justification to claim that the "scalar" is part of the gravitational sector. From the point of view of QFT, it is just another matter field, and the above coupling has nothing to do with EP. You could also argue, for example, that this scalar is the piece of the Dicke-Brans-Jordan gravity (the scalar-tensor theory), but in that case there should also be the curvature term in the theory, again rendering it nonrenormalizable. Consequently, QFT breaks down and can make no statements about EP.

Don't get me wrong — I agree that the formalism of QFT is made use of in discussing the EP violation. But this can be done only if that QFT is considered to be an effective low-energy limit of string theory. The argumentation about EP violation simply does not hold outside the context of string theory, because you don't know if that scalar field is part of "gravity" or "matter".

HTH, 🙂
Marko

41. **Olivier Minazzoli**
January 12, 2014

Marko,

The low-energy effective action of string theory **is** a scalar-tensor theory (Brans-Dicke-like) with a non-minimal scalar-matter coupling (see (2.1) in Gasperini/Piazza/Veneziano. Therefore, I don't quite understand when you seem to argue that with a scalar-tensor theory with non-minimal scalar-matter coupling, one cannot make statements about EP; while one could(?) with string theory. Please specify what you have in mind.

Otherwise, you don't need string theory to postulate this kind of coupling in an otherwise usual scalar-tensor theory. I.e. you don't need string theory to postulate the action (2.1) of Gasperini/Piazza/Veneziano. Other principles than the string theory ones can lead to such an action.

42. **vmarko**
January 12, 2014

Olivier,

"Therefore, I don't quite understand when you seem to argue that with a scalar-tensor theory with non-minimal scalar-matter coupling, one cannot make statements about EP; while one could(?) with string theory."

Ok, suppose that you have the scalar-tensor theory with nonminimal coupling to matter fields as your classical theory, without any mention of string theory. If you try to quantize this classical action (which you must if it is to be fundamental), you will find that it is nonrenormalizable — you cannot eliminate divergences consistently, and thus the corresponding QFT is not well-defined. Consequently, that QFT does not have any predictions at all, and in particular no predictions regarding the EP.

Now consider that same action as a low-energy effective action of the string theory. The effective action (by its nature) is not something that needs to be quantized, and consequently there are no renormalization issues. This is because string theory has some form of UV completion (which makes it perturbatively finite), and because your effective action is valid only at low energies — at higher scales it will receive nontrivial correction terms from string-UV-completion, and these terms are supposed to cure any and all divergences. But at low energies those corrections are supposed to be small, and the effective action should be approximately correct. Consequently, you can analyse its form and find out that EP is violated, unless the dilaton gets a large mass from supersymmetry breaking (in which case EP is not violated), etc.

It goes without saying that the string theory has its own share of problems regarding the prediction of EP violation. Peter's argument is basically that due to all those problems (essentially lack of uniqueness of the formulation of M-theory), string theory also doesn't make any predictions regarding the status of EP, but for different reasons and despite the knowledge of the low-energy effective action.

So it's the same action, but in two different contexts — one where you don't have a well-defined UV completion (and consequently you don't have any theory to speak of), and one where string theory does attempt to give you a UV completion (and if successful, you consequently do have a well-defined effective theory, which can be studied).

The general moral of the story: QG =/= QFT. 😉

HTH, 🙂
Marko

43. **Olivier Minazzoli**
January 14, 2014

Marko,

are you saying that the somehow very general argumentation in Banks/Dine/Douglas paper is actually restricted to string theory because the discussion wouldn't make sense otherwise? Am I correct to assume that it is what you are saying?

Also, as far as I can tell, Banks/Dine/Douglas paper is about the violation of the Eintein equivalence principle, while you seem to talk about the weak equivalence principle. Indeed, they are interested in the variation of the fine structure constant only. But a variation of the fine structure constant is a violation of the Einstein (and the strong) equivalence principle — and in that case, the scalar-field (ϕ) that couples to the material sector (eg. ϕ F^2) is not necessarily part of the gravitational sector: it could be any moduli field. On the contrary, if the scalar-field is part of the gravitational sector as in your exemple, and as it is for the dilaton for instance, then indeed it would lead to a violation of the weak equivalence principle in addition to the strong and Einstein EPs. But Banks/Dine/Douglas don't seem to consider this situation in particular.

44. **vmarko**
January 14, 2014

Olivier,

"are you saying that the somehow very general argumentation in Banks/Dine/Douglas paper is actually restricted to string theory because the discussion wouldn't make sense otherwise?"

Essentially yes, that's right, with a caveat that their argumentation could be also restricted to some other, non-string theory of QG. So yes, outside of a context of some theory of QG, their argument wouldn't make sense. Moreover, the authors actually say that themselves in the first sentence of the second paragraph of their paper:

"Within both the contexts of effective field theory and M-theory, it is natural to
model the change of the fine structure constant by coupling a dynamical scalar field ϕ to the photon kinetic term in the low energy effective action."

Note the phrases "M-theory", "effective field theory" and "low energy effective action". Without M-theory (or some other theory of QG), the latter two phrases don't actually have any meaning. One can discuss them only in a QG context, never in the QFT context alone. As I said earlier, QG has proven to be extremely hard (if at all possible) to construct within just the QFT framework.

"Banks/Dine/Douglas paper is about the violation of the Eintein equivalence principle, while you seem to talk about the weak equivalence principle."

The various flavors and formulations of the EP basically depend on how you make a split of all relevant fields into "gravity" and "matter" sectors. One version of the (strong) EP which is particularly suitable says that the coupling between gravity and matter is such that when one writes down the laws of matter fields in a locally-inertial coordinate frame, they reduce to the form that they would have if the gravity sector was "turned off" completely (i.e. laws of physics would then take the so-called "special-relativistic" form). The enforcement of the EP dictates the "minimal coupling" between gravity and matter.

So if the scalar field in the BDD paper is considered to be a "matter" field, then their analysis says absolutely nothing about the EP (any flavor). If, OTOH, the scalar is considered to be a part of "gravity", then its coupling to EM-field clearly violates the EP. But considering the scalar field to be part of gravity can be done only in the context of some QG theory, never in QFT alone, because no QFT can describe QG.

Note also that the BDD paper is all about estimating a cutoff scale to fix the upper limit on the variation of the fine structure constant. The very presence of a cutoff scale is an indication of the fact that they work in some (unspecified) QG context, where above that cutoff QFT fails to be a valid description of nature. IOW, the violation of EP (as embodied in the variation of the fine structure constant) depends on the scale where QFT doesn't work anymore. This is precisely inside the domain of QG and outside the domain of QFT. That is just the same statement formulated in different language — QFT alone cannot say anything about the EP.

Btw, we are getting sort of off-topic here, and it seems that I am repeating the same statement in four posts already…

Best, 🙂
Marko

45. **Olivier Minazzoli**
January 15, 2014

Marko,

"with a caveat that their argumentation could be also restricted to some other, non-string theory of QG."

Well, the caveat here changes everything on whether EP is a string theory prediction, as claimed in Douglas/Kachru (mentioning Banks/Dine/Douglas), or a generic prediction that comes from QFT arguments only — which was my original point — not mentioning that it is actually not a prediction but rather an expectation.

Of course the argumentation is restricted to a theory that might have a chance to be proven well defined down to the quantum level. The point if that, as far as I know, we currently **do not** have the nonperturbative tools in order to (even simply) know whether **or not** a theory might be correct down to the quantum level.

"So if the scalar field in the BDD paper is considered to be a "matter" field, then their analysis says absolutely nothing about the EP (any flavor)."

As far as I can tell, this is not correct. If the fine structure "constant" is a function of any non-gravitational scalar field, then it violates the local position invariance and therefore Einstein EP. Indeed, then *the outcome of a local non-gravitational experiment in a freely falling laboratory is **NOT** independent of its location in spacetime* (see Will's Living review). Anyways, Banks/Dine/Douglas actually don't even mention the equivalence principle but only argue about the possibility of a variation of the fine structure constant 😛

So the question here really is: does Banks/Dine/Douglas paper show that string theory in particular predicts the invariance of the fine structure constant? or rather that QFT seems to put a strong constraint on string theory (among other theories)?

Otherwise, I don't think we are off-topic here. But maybe Peter would indeed prefer if we were continuing our discussion privately. Peter?

Best 🙂

Olivier

46. **Peter Woit**
January 15, 2014

Olivier,
I don’t think you’re off-topic, but I do think this has gotten to the point where it would be better if the two of you continue the discussion privately. If either of you needs help figuring out how to contact the other, let me know.

What Scientific Idea is Ready for Retirement?

January 14, 2014
Categories: Multiverse Mania

Every year John Brockman's Edge web-site hosts responses to a different question. This year the question was What scientific idea is ready for retirement?. It shouldn't be too hard to guess what I chose to write about, with results available here.

Every year Brockman manages to attract more responses, so this is now getting to be a statistically significant sampling drawn from the population of people who write about science for the general public. Before trying to divine some general trends among the physics responses, I'll first mention a few of them that stand out as unusual.

First, there's one from Paul Steinhardt that I very much agree with. He's had it with the multiverse and thinks it needs to go. I'm very glad to see someone else making many of the points that I endless repeat on this blog in a tiresome way. So, go read what he has to say, which ends with this challenge to the theoretical physics community:

> I think a priority for theorists today is to determine if inflation and string theory can be saved from devolving into a Theory of Anything and, if not, seek new ideas to replace them. Because an unfalsifiable Theory of Anything creates unfair competition for real scientific theories, leaders in the field can play an important role by speaking out—making it clear that Anything is not acceptable—to encourage talented young scientists to rise up and meet the challenge.

It would be great to see someone other than him and David Gross start publicly speaking out.

A second outlier is Gordon Kane, who uses this as an opportunity to claim that he had predicted the Higgs mass using string theory. I don't know of anyone other than him who takes this seriously. He doesn't mention his other string theory based predictions, which include the prediction that the LHC should already have seen gluinos.

Another odd one is from Max Tegmark, who argues that we have to get rid of equations in physics that aren't just based on finite and discrete quantities. The only positive argument I can see from him for this is that it would help get rid of the "measure problem" of the multiverse, but listening to Steinhardt and dumping the multiverse itself seems to me a much better idea. Tegmark has a new book out, I'll write more about this here in a few days.

Maria Spiropulu is with me on the need to retire naturalness, also wants space-time to go. Getting rid of space-time has multiple proponents, including also Steve Giddings and Carlo Rovelli.

Another theme is people starting to sound like John Horgan, announcing we're reaching the limits of science. Martin Rees thinks that some scientific problems may never yield to our understanding: "The human intellect may hit the buffers". Ed Regis thinks the cost of a next generation collider is just not worth it for what it is likely to tell us.

A variant of this is the argument that we've reached the end of the road for unification and simplicity in our basic physical laws. Here the argument often seems to be that since SUSY/GUTs/string theory were such beautiful elegant ideas, their failure means the whole elegance thing is misguided. Another point of view (which I think someone wrote a book about) would be that these always were heavily oversold as "elegant", since if you looked into them they were rather complicated and didn't explain much. Writing in the anti-elegance vein are experimentalist Sarah Demers:

> It is time for us to admit that some of the models we have been chasing from our brilliant theory colleagues might actually be (gorgeous) Hail Mary passes to the universe.

along with Marcelo Gleiser and Gregory Benford. At this particular time in intellectual history, it seems that hardly anyone has anything good to say about mathematical elegance as a powerful principle behind deep ideas about physics.

Finally, the biggest contingent are the multiverse maniacs. There's Andrei Linde, who deals with the problem of evidence for his ideas by:

> A pessimist would argue that since we do not see other parts of the universe, we cannot prove that this picture is correct. An optimist, on the other hand, may counter that we can never disprove this picture either, because its main assumption is that other "universes" are far away from us.

He's joined by Sean Carroll, who wants to do away with the Popperazi and their inconvenient demands for falsifiable predictions. Also writing in support of the idea of a multiverse of different physical laws, implying we'll have to give up on the idea of understanding more about the ones we see are Lawrence Krauss and Seth Lloyd.

Update: A couple more late additions that I missed. Eric Weinstein is with me in going after "string theory is the only game in town" as something that should have been retired long ago. Alan Guth uses this venue to promote some recent speculative work on the arrow of time with Sean Carroll (no paper yet, so hard to tell what it really is).

Update: Sean Carroll has a blog posting up about his argument for getting rid of falsifiability. He seems to not be getting a lot of support, either in his comment section (see for instance here), or places like here. I don't think the skeptic community is ready to disarm itself intellectually in arguments against religious believers by ditching the conventional scientific method.

Update: Scott Aaronson writes here about the falsifiability issue, pointing out about string theory/multiverse research that

> I wouldn't know how to answer a layperson who asked why that wasn't exactly the sort of thing Sir Karl was worried about, and for good reason.

Sean Carroll responds that the problem here is

> somber pronouncements about non-falsifiability from fuddy-duddies.

Comments

1. **tulpoeid**
   January 14, 2014

   Half of the answers you cover make me wish that the public will one day demand to end public funding of theoretical physics. It'll definitely be more beneficial to science compared to the present state.
   If the public can achieve the end of private funding as well we're even more on the correct path. And while we're there, can the public also make it quick? :p

2. **Tim Howells**
   January 14, 2014

   I'm puzzled by Prof. Steinhardt. See this video from April 2011:
   http://www.youtube.com/watch?v=IcxptIJS7kQ

In this talk he describes string theory (starts at 44:40 mark) as a very elegant theory that unifies many observations and has great explanatory power, and which is the only game in town anyway etc etc. He cautiously criticizes cosmic inflationary theory but only because he wants to propose a new theory in which "Brane-Worlds" existing in 11 dimensional space bounce off each other eternally, with each bounce creating an effect that is almost indistinguishable from the big-bang, but possibly with different settings for the natural laws etc etc.

By contrast his Edge paper sounds very coherent and sensible, and quite inconsistent with the talk at least in terms of string theory.

Disclaimer: I am not a physicist or a mathematician! I would be interested in comments on this by more knowledgeable people.

3. **imho**
January 14, 2014

First a general statement, from a cond matt theorist perspective. It seems to me that you guys are 1.5 good papers away from deriving gravity from the thermodynamics of entanglement, yet I keep hearing about the multiverse and string theory… I guess old habits die hard? Thank god for the faction of the new generation that seems to have a wonderful ability to completely ignore the old guard and chart their own path. I will enjoy watching this all play out.

tulpoeid:

Don't worry, other scientists (even Physicists) have already started the process of "redirected" funding. No one thinks it's a good idea to end public funding, but it's probably a good idea to reduce the number of young people in certain fields.

4. **Shantanu**
January 14, 2014

Peter: one more thing which Maria has mentioned is possible death of particle dark matter
which I too would have pointed out if I had a chance.
PS: I presume the deadline for writing something is over?

5. **Peter Woit**
January 14, 2014

Tim Howells,
Steinhardt's objections aren't really to string theory itself, but to the "string landscape" idea and its version of inflationary cosmology. For more about his objections to this, see his Scientific American article, available here
http://www.physics.princeton.edu/~steinh/0411036.pdf

6. **Peter Woit**
January 14, 2014

Shantanu,
The way this works is that Brockman sends out invitations asking people to write something in December, deadline to send something to him was a week or so ago. He then does some minor editing. Those writing can see the responses as they are posted, with the order I guess inverse chronological in terms of time received.

7. **Curious Wavefunction**
January 14, 2014

I agree that string theory has not made any predictions, but isn't it a little extreme to ask that we completely "retire" the whole framework? If nothing else, isn't there some useful mathematics that has come out of it?

8. **Jesper**
January 14, 2014

I find the contribution from Sean Carroll quite weird/extreme/confused/self-contradictory. He wants to get rid of falsifiability, and yet he ends with "… but nature is the ultimate guide." To me that doesn't make any sense – I guess my point of view is quite mainstream and much repeated here…

9. **Jesper**
January 14, 2014

Just one more sentence on Sean Carroll. If 'Nature' has zero way of saying "you are wrong" – which is falsifiability – then how can it be the ultimate guide? To me it sounds more like having a Teddybear to hold your hand …

10. **Peter Woit**
January 14, 2014

Curious Wavefunction,
What I wrote was specifically about string theory unification, the idea of getting a unified TOE using strings in 10d (or M-theory in 11d). These days "string theory" often is used to refer to all sorts of things that have little or nothing to do with this, and little or nothing to do with quantized strings. One can't sensibly argue against all of "string theory" in this larger sense, partly because it does include valuable ideas, partly because it's unclear exactly what one would be arguing against…

Jesper,
I certainly agree. One of things I find most incomprehensible about Sean Carroll (as well as Max Tegmark and some others) is the way he simultaneously devotes his life to going to war with religion, taking up the banner of the scientific method, while at the same time announcing that his research shows that the conventional scientific method has to go. Of course he's right that the scientific method is more complicated than sometimes portrayed, with much more to it than "falsifiability". But the kind of theoretical work he favors (e.g. his multiverse explanation of the arrow of time) come with no convincing ideas about how to test it, with or without "falsifiability". The "nature is the ultimate guide" sloganeering does then seem very empty.

11. **Pawl**
January 14, 2014

If you search the abstracts from December's Texas Symposium on Relativistic Astrophysics, you will find no mention of "multiverse" and only two (in contributed talks) for string theory. Much of the Symposium was about cosmology, although admittedly much of that was given over to observation.

12. **Low Math, Meekly Interacting**
January 14, 2014

I sometimes think a blog post devoted to an updated view of on how one could even hope to define "(Super)String Theory/M Theory" would be a valuable endeavor, if it's not a huge inconvenience. I'm vaguely aware of the complexity from an outsider standpoint, and I think Matt Strassler, to give one example, has done a phenomenal job of helping to clarify some of these exceedingly complex issues. As a biologist I'm used to sloppy nomenclature, but in the field of HEP, "String Theory" seems to almost anything you want it to be, and different, quite well-informed HEP theorists might not even agree that what a particular investigator is doing amounts to "string theory" research. It's appears to be quite hard to even define basic criteria like "success" and "failure". Sure, maybe the ST landscape is without predictive power and many now seem to agree that this represents a failure of sorts. At least, it's a major disappointment when juxtaposed with the late-20th-century enthusiasm for the idea of a vacuum selection principle falling out of "M Theory" that would yield the Standard Model and GR as a low-energy limit. That whole notion appears to be as dead as dead can be, and has apparently been pining for the fjords for well over a decade.

But what of the rest? Lots and lots of legitimate debate about that, I think. A tool for learning more about quantum field theories? Seems like there's at least a case to be made. Its role in this new amplitude business (along with twistors and all kinds of other things I could barely comprehend)? Does anyone even aspire to know with any level of certainty? Insights into strongly-coupled condensed matter systems? Much more debatable, perhaps, but maybe not as hopeless as the anthropic principle.

I'm truly not attempting to do anything like advocate, just hoping to gain some additional clarity about it all in the public discourse, if such a thing is attainable.

Thanks!

13. **Peter Woit**
January 14, 2014

LMMI,
The complexity of some of these issues is one of the main reasons I wrote a book, and I think what's in the book holds up very well, with not that much changed in the past ten years since the book was written. For some of the more recent topics that have gotten a lot of attention as advances in "string theory", typically again there's a very complex story, which would require a book-length treatment to do justice to (but I'm not going to write that one…). For example, there are now probably close to 10,000 papers referencing the original AdS/CFT paper, and I'm not going to do justice to what they cover in one, or many blog posts. Strassler's series of several blog posts does try to do this a bit, although he ends up writing a sizable chunk of a book, just to get at one small piece of that story.

The other problem here is that I don't have the expertise to evaluate the significance of some heavily promoted supposed applications of string theory. For applications of AdS/CFT to heavy ion physics, you need a heavy ion physicist, although Sabine Hossenfelder has had some good blog entries about this. For AdS/CMT, you need a good condensed matter theorist, not me. The story about amplitudes and twistors is an incredibly complex one, I've tried to say something here about the parts I understand, link elsewhere for what appear to be the best sources of info about things I don't understand. To be honest though, I find this often a discouraging business, with sometimes a lot of people more interested in muddying the waters and making dubious claims ("string theory explains high temperature superconductivity") than in really explaining what is going on. I certainly encourage anyone who understands these issues to write clear and honest explanations of them, and I link to such things when I see them, but my willingness to devote lots of time to that thankless task is pretty limited.

14. **CFT**
January 14, 2014

I actually liked Max Tegmark's paper. He has a logically valid point, Infinity is not measureable, and thus putting anything into any kind of measureable (finite) ratio with it is ridiculous, and not logically sound. I understand the concepts of big and little just fine, and how they are relational, and how things can be measured and compared in a ratio of known sizes. I reject the premise you can put anything into a logical relationship that is truly infinite, unless you are using the term euphemistically. If you don't know how big (or small) something truly is, 'infinite' is not the correct answer. Just say, " I have no idea how big it is, or how far it goes, I have no way to measure it…my data has limits and I admit it" and you are on the road to making rational arguments that are scientifically accurate and honest about the limitations of your data. Infinity is an abstract concept. Concepts or ideas can be infinite, because they are a product of your imagination, not limited to measureable data, and not bound by physical limitations or time. Reality is by comparison, far more stringent logically, and utterly bound by the limitations of measurable (finite) data, time, and space.
*
As for Sean Carroll,
I ditched his blog a long time ago when I realized he talks out of both sides of his mouth about the scientific method, treats the definitions of words like taffy when it is convenient. Sean has openly shown contempt for anyone who does not share his political views and has great difficulty discerning science from open political advocacy. I do understand his methodology however, if you eliminate falsifiability from the criteria of logical scientific reasoning, you can make any fanciful claim whatsoever and never be wrong or 'not even wrong' since you can always claim it is true somewhere else or 'in another universe'. IMHO: Sean would not seem to have internalized the burden of proof or evidence in THIS universe.

15. **hopffiber**
January 14, 2014

Low Math, Meekly Interacting,
there is a lot of work indeed that on the surface only seems vaguely related to string theory, like the whole amplitude business, integrability, entanglement entropy and so on. These are interesting on their own, and I hope that (even) Woit will agree that they contain many valuable ideas. However, most of these ideas comes from string theory in one way or another, mostly through AdS/CFT. And as AdS/CFT shows us, a string theory can be precisely equivalent to a quantum field theory, so the two things are really closely related, even if it might seem distant at a glance. One can do a lot of quantum field theory using string and M-theory.

And I think that talking about the landscape, sure it is disappointing and the hope of a unique theory of everything seems far gone, but really, it's the same as in quantum field theory. In QFT, we have an infinite landscape of possible models, and there is no selection mechanism at all, so one could state that QFT is very non-predictive and indicative of a multiverse. We need experimental data to select a specific QFT model, namely the standard model, and then this model in turn is highly predictive and can be tested against new experiments. This is the same that we seem to have in string theory, it's just that the we don't understand the string theory vacua as well as we understand different QFT models, and that the experimental data is a lot harder to get (since the energy scale of quantum gravity is very high). But once you select a specific string vacua, the model is in principle just as predictive as say the standard model.

16. **SA**
January 14, 2014

Not even wrong: http://arxiv.org/abs/1401.1219

17. **Peter Woit**
January 14, 2014

hopffiber,
What you write is pretty much pure hype, exactly the sort of thing that makes attempts to have a serious discussion about complicated issues pointless and unrewarding. For the last part of it, see
http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=isnt-string-theory-just-as-predictive-as-quantum-field-theory

All,

Sorry, but if it's not directly about the material in this posting, I'm deleting it as off-topic.

CFT,
"Infinity" is a perfectly well defined concept (not finite), as well as an extremely useful one. I don't see what the value is in abandoning it, other than requiring much more complicated ways of saying true things. There are some very specific cases in physics where it appears in an abuse of language, with multiverse maniacs the main offenders. But I don't see what conclusion can be drawn from this other than that they shouldn't do it.
As for political views of bloggers and others, I'm not going to allow debate about that here. However dumb such expressions can be, the arguments about them in comment sections are even stupider.

18. **Anonyrat**
January 14, 2014

The best line in Sean Carroll's piece is "Fortunately, science marches on, largely heedless of amateur philosophizing." Indeed!

19. **Anonyrat**
January 14, 2014

Even if we accept Sean Carroll's criteria of good scientific theories — they are definite and they are empirical — in place of Popper's falsifiability, his two examples are no good.

* Particles as strings is definite, but not empirical.

* The multiverse may be empirical (if you accept Carroll's reasoning) but it is not definite.

20. **Neil**
January 14, 2014

The Edge website might need to be retired. I've been trying to get on it all day without any luck.

It seems to me, though, that the very question suggests that something is wrong with physics today. It used to be that a scientific idea was retired because it was inconsistent with observation, such as the plum pudding model of the atom or the steady state theory of the universe. Now it seems to be a matter of fashion and exhaustion.

21. **Justin**
January 14, 2014

Woit, would you be optimistic if the top theorists began working on new ideas? I have this bad feeling that even if the greatest theorists were working on non-string related ideas, that they still would go nowhere. My feeling is that there's nothing left for a physics student to dream about.

22. **Low Math, Meekly Interacting**
January 14, 2014

Hi, Peter,

Oh well. I still think an "updated skeptic's survey" would be a wonderful read, but I thank you for the attention you gave my thoughts anyhow.

23. **Peter Woit**
January 14, 2014

Justin,
It's not really a matter of "new" vs. "old" ideas. It's entirely possible that progress could be made by re-examining old ideas. And, while people are trying to market the multiverse as a "new" idea, it's one that has been around forever and is now past its sell-by date.
The problem with the whole multiverse business is that it's a desperate attempt by some people to avoid admitting that certain ideas have failed. If their ideas haven't worked out, they could go do something else with their lives, or try different ideas. Instead they go on a campaign to convince people that no one can ever understand the things they've failed to understand, not exactly inspirational to physics students everywhere.

Quantum field theory is an example of a topic we still understand very poorly. Anyone who studies it will find a lifetime's worth of things to ponder. Just ignore the multiverse maniacs, wait for them to retire, and for journalists to lose interest as it becomes clear how old and tired their ideas are. Physics and math are full of fascinating, poorly understood things to think about.

24. **harryb**
January 14, 2014

Peter,

I am most of the way through Max Tegmark's new book you refrence – (Our Mathematical Universe). Its a strange brew. The first five chapters, as he admits, are mainstream, and seem perfectly lucid standard model material. Then comes the Multiverse in its various Levels.
It is interesting to see this up close – the detail of the earlier chapters and quite interesting perspectives on particle physics suddenly dissolves to leave personal statements and increasing use of exclamation marks. As his own biography unfolds in the book, you see a personality prone to hype and headlines gradually grab the Multiverse as the next great misunderstood Relativity Theory.
Tegmark's switch mid-book from lucid guide to particle physics, to wild-eyed evangelist of outre science, particularly the Multiverse, is very sobering. The two "books" sit uncomfortably – one grounded in deep theory, the other a very personal journey into extreme beliefs, which he associates with Newton, Einstein and, to Tegmark, Everett.
Physics succumbs to attribution error, and availability heuristics.
Many many wild theories were also shown wrong Mark!, one shouts at the book, but those are conveniently overlooked.

25. **Peter Woit**
January 14, 2014

harryb,
Thanks. There's a lot to say about this, both about Tegmark as scientist and about the scientific claims of the book. I'll write a lot more in a separate posting in a few days, hope to have an interesting discussion on this topic then.

26. **Patrick**

January 14, 2014

Neil: I think it's a lot easier to ditch a theory when there's a clearly better one on hand; and that's not really the case in BSM physics. Hence the cliched defence that string theory is "the only game in town." And in any case, I think it's unfair to characterise "physics" on the behaviour of this sub-field's practitioners.

27. **Chris W.**
January 14, 2014

Edward Fredkin and Stephen Wolfram should sue Tegmark for copyright infringement. Fredkin gets first dibs on the award. 🙂

28. **MR**
January 15, 2014

What is surprising to me is the idea of withdrawing the notion/concept of space and time. Even the quantic human observer is deeply attached to its own space and time, which is a mixture of the macro and micro realms. If the goal is to reduce space to the quantum space and time to the quantic time, ¿what do we mean by quantic space-time? We know little of black holes, superluminal effects, dark matter and dark energy, very little indeed to say that space-time should be deleted, seems like a bizarre idea.

29. **JR**
January 15, 2014

The more common proposal is not that space-time should be "deleted", just that space-time is emergent from something else. We have encountered numerous emergent phenomena in other areas of physics. See http://vimeo.com/65880636 for examples.

30. **Neil**
January 15, 2014

Patrick: We do not disagree. Now that I've finally accessed the Edge website, I see that most of the entries are about assumptions, organizing principles or world views that should be retired, rather than precise theories or models.

31. **Hack**
January 15, 2014

Physicists are too polite, people like Sean Carroll should be laughed right out of physics and science. In fact it is a shame Edge published his response. I will refrain from name calling, but this is utter foolishness and the sooner physicists put an end to this nonsense the better. Many years ago I thought physics was one of the last bastions of 'truth' well that is obviously no longer the case.

And yes, I know the argument, but so and so is brilliant and has made valuable contributions in others fields. Well if brilliant so and so is leading the charge into the wilderness and foolishness, maybe he's not as brilliant as everyone thinks.

32. **Peter Woit**
January 15, 2014

Hack,
Sean Carroll's point of view is unfortunately not an unusual one, but a relatively mainstream one these days. If you want to kick him out of science, you'd have to do the same thing for a bunch of prominent physicists, including various $3 million prize winners (did you read Andrei Linde's piece?). Carroll I think sees himself as Mr. Science, a voice of the physics establishment. The problem is what is going on with the physics establishment, not him (and this is the point Steinhardt was making).

33. **Jeff**
January 15, 2014

The link for Sarah Demers's statement is pointing to Ed Regis's.

34. **Peter Woit**
January 15, 2014

Jeff,
Thanks, fixed.

35. **ScentOfViolets**
January 15, 2014

I think you're letting Sean off too easy, Peter. The problem is that he is determined to present his opinions to the public as *the* voice of mainstream science on this particular subject when the reality is that he represents a minority viewpoint, in fact, I would say an extreme minority. I've seen him become quite irritated more than once when this misrepresentation is pointed out to him and he needs a public smack down by his peers on this one. Let me emphasize that I have no problems with him presenting his ideas in a book to the lay public despite the fact they are (imho) wrong-headed at best. I just have problems with him misrepresenting those views to the lay public as mainstream thinking. That sort of thing has got to stop – and won't until there is some sort of visible policing by his fellows.

36. **Kavanna**
January 15, 2014

Hail Mary pass, indeed. Is it the fourth quarter, last down?

To get back to basics: naturalness, elegance, and unification are sound approaches to modern physics. They have more than proven their worth. However, the technical avenues that we have now for implementing these ideas have been, for the moment, exhausted. And anyone who knows this history — starting with Maxwell's unification of electricity, magnetism, and light, and his prediction of the displacement current — knows that this program has worked only when it remained close to experiment and stimulated the technology needed to expand what could be tested, when adequate technology was not previously available.

These days, if you want to expand our fundamental understanding of the universe, you need to be doing astrophysics and cosmology. High-precision laboratory experiments are also worthwhile. Accelerator-based fundamental physics is dead.

Carroll is a fine popularizer of science. But his arguments about falsifiability are incoherent nonsense. It's amazing that these same people object to, say, creationism or astrology as being anti-science or non-science. The truth is that they're poseurs, riding on the historical prestige of science to perpetrate something that clearly isn't science. It may only seem so to the layperson, and even lay people, these days, are getting wise to this hoax. Steinhardt's statement about "unfair competition" is right: it's Gresham's Law applied to science: bad ideas driving out even the possibility of good ones.

Reduction of funding for theoretical physics is an idea whose time was already here a decade ago. (I mean string theory-like mania, not astrophysics or cosmology, or whatever's rooted in experimental and laboratory science.) The problem is that the people administering such reductions are almost certainly going to be the same "string/multiverse" maniacs.

37. **Bernhard**
January 15, 2014

I just read Sean Carrol's response. I don't feel like reading anything he has to say ever again. It is a barely crackpot point of view defended with academic eloquence. HEP, once feared and admired, is becoming the laughing stock of science because of these guys.

38. **Peter Woit**
January 15, 2014

ScentofViolets,
I still think that Sean Carroll is not the problem. If you know anything about academia, you know that the people with significant influence are those holding tenured full professorships at the major physics research departments. Sean is not in this category (he's an astronomy Ph.D. who didn't get tenure and is now a research associate, not even on a tenure-track line). The problem is people like Linde, Susskind, Polchinski and all too many others like them. They are the ones in leadership positions of the field, the ones claiming multiverse mania as mainstream physics, and they are the ones responsible for the problem. The day they abandon this or get a public smackdown from their peers and lose credibility will be the day Sean changes his tune. He's a symptom of the problem, not its source.

39. **emile**
January 15, 2014

Kavanna and Bernhard,

I think we probably agree on the definition of science but you two have to be careful…

Kavanna: "accelerator-based fundamental physics is dead". Really? We just found a new particle in 2012. In 2013, it was called "a" Higgs boson by CERN based on the results of the 2012 run. That's pretty fresh… Now, do you disagree with Sarah Demers who says that we should "measure the hell" out of that particle? Do you already know the answer? The LHC is re-starting at higher energy. Do you already know what will be found or not found? Should the thousands of physicists bother looking at the data at all? Nobody knows what will be found. We need to experimentally check that there is nothing else at those energies. If after running at higher energy the LHC finds nothing and the Higgs that was found looks even more Standard Model-like, then we can talk about what the future of accelerators should be. It may be a likely scenario for you and me but as scientists, we absolutely have to check.

Bernhard: you write "HEP, once feared and admired, is becoming the laughing stock of science…". First of all, for every Linde, there are a dozen theorists (phenomenologists), doing calculations that make predictions that can be tested and are tested by the 6000 LHC experimentalists. This is normal science and it is doing great. The problem comes from a minority (though high-profile) people who want to redefine science. Hundreds of -scientific- papers are coming out of the LHC , comparing predictions made by theoretical physicists with observations made by experimentalists. So the results show that the SM explains everything up to now. This still needs to be demonstrated experimentally. If that is Nature's choice, then so be it. Don't denigrate all this *scientific* work.

In general, I get annoyed with the notion that "physics is in trouble". Physics is doing just fine. I am part of a big physics dept. and the research done by my colleagues in various fields, both experimentalists and theorists, is very interesting and exciting. I wish I could live longer and work in other branches of physics. Even in HEP, science is getting done by 98% of the HEP physicists… We need to fight back when high profile physicists want to redefine the meaning of science but we should not say that physics is in trouble. We should say that these guys are not doing physics.

40. **george ellis**
January 15, 2014

What emile says is exactly right. The entire group that Peter complains about are not what most present day physics is about, see for example http://iopscience.iop.org/1367-2630/page/Focus%20on%20series. They are a very small albeit very vocal group on the fringe of mainstream physics. And they are in fact advocating, implicitly or sometimes explicitly (as in the case of Susskind and Carroll), abandoning the core feature of testability that has led to the rise of modern science. If you abandon that core, astrology is Ok too. But they do not represent mainstream physics nor even mainstream high energy physics. And in terms of philosophy, they are real amateurs.

As to Tegmark's comments on infinity: I think he's absolutely right. David Hilbert said the same thing: infinity is needed in mathematics but it never occurs in real physics. If you claim it does, then give me an experiment that will demonstrate that any physical entity whatever is infinite (number of galaxies in the universe, number of *physical* points between my fingers when I hold them 10cm apart, anything you like where it is claimed that a physical infinity of something exists). You can't. There is no such experiment. You should rather refer to the large but finite number that is relevant to physical observations and testability. Nothing is continuous on a small enough scale if spacetime is quantised, as seems to be very plausible. It just looks continuous when coarse-grained to experimentally relevant levels.

41. **Anonyrat**
January 16, 2014

Which scientific idea is ready for retirement?

It seems some of the ideas of some of theoretical physics' most elite researchers are overdue for retirement.

We know that in America, the elite retire only with golden parachutes. It seems to me the Milner prizes could serve that purpose. The elite, having received their golden parachutes, can retire along with their prize-winning ideas, keeping their dignity intact. Perhaps one (secret) condition of the prize should be that the recipient should not publish in a scientific journal again.

42. **imho**
January 16, 2014

Hi Peter,
Regarding your comments about who is influential and who isn't… I couldn't disagree more. It's not like scientific culture exists in some impregnable bubble, completely immune to the normal rules of human interaction. Of course those with the biggest microphones get heard, of course they have influence. The specific detail of their official title, while important, is not the only significant term. I would argue that young up and coming graduate students are just as interested in Sean, Peter, and Lubos as they are Linde, Susskind, and Polchinski. I would also argue that funding agencies are just as exposed to former as the latter. Your own experience is a glaring confirmation of this. The only thing special about Sean is that the ideas he's been selling for the past decade are pretty widely agreed upon to be nonsense.

43. **Peter Woit**
January 16, 2014

George Ellis,
Yes, you can formulate physics questions in ways that avoid using "infinity", but what I object to are claims that this actually solves your problem (which is what Tegmark is claiming). An example would be the "infinities" of quantum field theory, where, yes, it is often a good idea to think about the problem in terms of a finite cut-off theory. In non-renormalizable theories though, this doesn't do away with your problem, it just makes it clear that the problem is that you don't understand what is happening at the cut-off scale, and this matters when you try and calculate some physical quantities.

Similarly, the "measure problem" of multiverse theories is not one that can be removed by announcing that everything is finite. The problem isn't infinity, but that you don't really have a theory: "anything goes" is going to be an empty statement, whether things are finite or infinite.

44. **Peter Woit**
January 16, 2014

imho,
I'm sure blogs do have an influence, with for instance Lubos doing a great job of convincing graduate students to avoid string theory, and physics departments not to hire string theorists (if he starts arguing for the multiverse, that will be very helpful…). In the long term though, it's the tenured people at certain institutions who train the next generations of theorists and either succeed or don't at helping them make careers in the subject (as well as convincing their colleagues who to hire to ultimately replace them). The subtext of a lot of this debate is a battle over whether major research institutions will hire those doing multiverse research, and whether the NSF/DOE will fund grants to them. Susskind and allies often behave as if they have won this battle, but I think it's still on-going (Steinhardt's piece is an important salvo). When the whole business started more than ten years ago I thought it was so ridiculous that it would go nowhere, but I was very wrong about that, and have no prediction now for where things will end up.

45. **notagain001**
January 16, 2014

Peter what do you think of Ed Witten's argument for string theory?
He's a true believer

http://scgp.stonybrook.edu/archives/996
ed witten's take on string theory

You claim that string theory is the only theory that incorporates quantum gravity. Some physicists make the same claim about a different theory: loop quantum gravity, which unlike string theory does not require any extra dimensions. This has generated some controversy in the literature. What is your take about this?

Instead of taking your question literally and giving a negative answer, I'd prefer to answer anotherquestion. One really can only expect string theory to be related to areas of mathematics and physicsthat have real substance. And indeed, it has fascinating links with numerous areas of science. A list would be too long to offer here, but would have to include the theory of strongly interacting gauge theories, heavy ion physics, the theory of quantum critical points, topological field theory, topological insulators, noncommutative geometry, twistor theory, and on and on.

46. **Peter Woit**
January 16, 2014

notagain001,
This really is off-topic, and discussed ad nauseam on this blog. The best answer to the question of where I disagree with Witten about string theory is that I wrote a whole book about this ten years ago, and I don't think much has changed since then (except for SUSY not appearing as Witten and other string theorists had hoped…)

47. **Bernhard**
January 16, 2014

emile,

I used "HEP" without much care. I agree with you first paragraph, after all I am myself part of the effort. I used HEP because people talking about this sort of baloney (and I also agree we should not even say this guys are doing physics) are in majority connect to HEP. Considering they are very outspoken and powerful this tend to affect people doing real HEP.

About physics being in trouble, well that is certainly not my feeling working with it and also not the feeling I have when I go to conferences. But it is definitely my feeling when I read a lot of current outreach material, which is read by everybody (hence my laughing stock comment).

Perhaps after these guys are gone people will just forget about all this multiverse business. What worries me is that people defending this crap are very influential and how much they will infect the youngsters is still to be seen.

48. **Hack**
January 16, 2014

emile and george ellis,

What you say may be true, physics is not in trouble, it's just a few over hyped physicists. The problem than is that these over hyped physicists are the face of physics, supposedly the best and the brightest! I teach a conceptual physics course, what do the vast majority of my students ask me about? String theory. Because of these over hyped physicists, many students have asked about and believe that string theory is correct and is a valid old fashioned scientific theory. In fact I have a had a hard time convincing them to just look at it as an unproven, untestable theory. Meanwhile, very few students know about things such as dark matter, dark energy, super conduction etc. Now we can blame educators or publishers or journalists. But in the end this 'policing' resides with the physics community, they are the only ones that can put a stop to this nonsense (or if some how funding dried up). If the statement 'physics is in trouble' bothers you I understand, a lot real testable physics research is being done. But than it seems to me you need to get rid of the people who hype their foolish theories or at least get them to shut up. As I said before, physicists are too polite. Oh, which reminds me, students also ask me a lot about the multiverse, lucky me.

49. **srp**
January 16, 2014

Hack, dark matter and dark energy get tons of media play. Admittedly, much of this is redundant or badly explained, but as mysterious "we don't know what's going on" stuff they are naturally appealing to lay audiences.

It's an interesting fact that lay audiences are usually a lot more curious about what is unknown than in what is pretty well known–a naive view would suggest that people would want to get "the good stuff," high-quality tested and articulated knowledge. But I think the notion of a mystery, a story whose ending is unknown, is intriguing, while something worked out is more threatening and mentally confining to the lay audience.

50. **george ellis**
January 16, 2014

I agree more needs to be done on the public relations side for solid physics. Here are a few of the books that do so:

1. Stuff Matters by Mark Miodownik
http://www.theguardian.com/books/2013/jul/03/stuff-matters-strange-miodownik

2. The first half of Jim Baggotts' book Farewell to Reality, see

http://www.math.columbia.edu/~woit/wordpress/?p=6002

3. The Theory of Almost Everything: The Standard Model, the Unsung Triumph of Modern Physics by Robert Oerter

http://www.goodreads.com/review/show/388743664

4. The Wave Watcher's Companion: Ocean Waves, Stadium Waves, and All the Rest of Life's Undulations Gavin Pretor-Pinney

http://books.google.co.za/books?id=5YRUDDDwxNgC&dq=The+wave+watchers+guide&hl=en&sa=X&ei=g7HYUtDoOYGM7QbNkoGgBg&ved=0CDIQ6AEwAA

and of course

5. QED: The strange theory of light and matter by Richard Feynman

http://books.google.co.za/books?id=Uv-uxB0sRKEC&printsec=frontcover&dq=qed+feynman&hl=en&sa=X&ei=6LHYUviTM6Wq7QbR6oDgCQ&ved=0CC8Q6AEwAA#v=onepage&q=qed%20feynman&f=fa

and a host of astronomy books such ss

6. Gravity's Fatal Attraction: Black Holes in the Universe by Mitchell Begelman and Martin Rees

http://www.amazon.com/Gravitys-Fatal-Attraction-Black-Universe/dp/0521717930

Why do they not make as much impact as the far out stuff? Maybe because they are not written in such a polemical way. And we are not served well by theoretical physicists who look down on other branches of physics as inferior and say so publicly. Nor for that matter by physicists who look down on other branches of science like chemistry and biology.

51. **Dom**
January 17, 2014

Does Carroll have an eye on The Templeton Prize?
I'm sure they would love the implications for claiming any belief is as strong as any other regardless of falsifiability.

52. **philh**
January 17, 2014

I used to think falsifiability was the be and end all. However I am not so sure, maybe someone can bring me back, although I certainly don't think it should be retired.
To me the hypothesis that life has existed on another planet is a) not falsifiable and b) is a scientifically plausible hypothesis.
There are scientific reasons to believe this hypothesis is true, and there has been scientific progress to making us more confident in its truth. The more we discover organisms that live in harsher and harsher environments there higher our confidence becomes in the hypothesis. Of course until we actually find life on another planet we should not be certain. But the way I see it we can never falsify the idea that there is no life on other planets. . Even if we visited every planet in the galaxy, life could have arisen on some of them in the time it took us to visit them, or maybe it went extinct before we got there. , or maybe it only arises on average in 1 in a million galaxies.
To me science is a mix of theory and experiment, but with experiment being the supreme judge. A hypothesis that is falsifiable if far more preferable than one that is not. However in the real world it seems things are messier than just the issue of falsifiability.
How could you falsify the idea that a human being could not survive a trip the centre of a black hole? But do you think such a statement has nothing to do with science? So whilst falsifiability is ideal, there seems to me to be other considerations. Is a hypothesis consistent with established physics? Is it implied by established physics? Can it ever be verified by data? These may not be as good a criteria as falsifiability, but i don't think such consideration should be equivalent to religious fantasy.

53. **philh**
January 17, 2014

Sorry obviously the above should read you cannot falsify the idea that there is or has been life on other planets.

54. **Michael Schmitt**
January 17, 2014

I fully support emile's statement about the quality of most scientific research in HEP and other fields of physics and astronomy. We do not need to obsess about the hype and we should not forget that much of scientific progress is incremental and unglamorous. The people who work hard for a modest advance should not be swept aside while we go after those who would occupy center stage. As George Ellis pointed out, there are serious and successful attempts to describe real progress in physics. What does it matter if uneducated undergraduates have heard more in the news about string theory than about the baryon asymmetry? Galileo, Newton, Maxwell, Lagrange, Heisenberg, Einstein, Zwicky and many others certainly did not worry about the hype of their days nor about what students were attracted to. There were plenty of charlatans in the past but physics and mathematic marched on. I hope it will continue to do so, despite some of the anti-science ideas expressed by notable, influential people.

55. **Peter Woit**
January 17, 2014

philh,
Falsifiability is definitely not the be all and end all of distinguishing science from non-science. Besides the example you give, there are plenty of others where one can see that this is a very tricky question. In situations where you don't have falsifiable predictions you need to carefully think about what the nature of the evidence is for your scientific claims. The danger with arguing for "retiring falsifiability" is that you need to say what you intend to replace it with, and ensure that you don't start adopting

justifications for your ideas that are pseudo-scientific, applying equally well to religion, for instance. Some of Sean's commenters are pointing out to him precisely this problem, that his argument for what is a scientific claim applies equally well to claims about the existence of a deity.

In the case of multiverse theories, without falsifiability, you need to check to see what exactly is being used to justify them. Sean's argument for the multiverse has often been that "String theory predicts it", changing the problem to what the justification for believing in string theory is. Then you run into the argument that "we can't test string theory because it predicts a multiverse, and we could be anywhere in this". The problem here is that you have hit circularity.

56. **Low Math, Meekly Interacting**
January 17, 2014

philh

I like to use "testable" as a criterion, if "falsifiable" is too strict or otherwise unworkable. I'm not well-versed enough in statistics to claim to be a "frequentist" or a "bayesian", but the data we have on known life that you mention leads me (and many others) to conclude that testing exobiological hypotheses is an eminently reasonable goal. It is entirely plausible that technology will advance enough even in my lifetime to directly image thousands of exoplanets in our galaxy, and spectroscopic and other data derived from those observations is likely to be informative, perhaps even definitive on an individual basis. We can adjust our exobiological expectations based on those data. We'll never be able to image all the planets around all the stars in all the observable universe, but we could reasonably hope to observe a fairly representative sampling. Surely we could make some very informed deductions once we have those data.

Can we aspire to anything remotely like this example as we explore the hypothetical anthropic landscape, especially if it says nothing sufficiently specific about our vacuum to assess plausibility, and the remaining (effectively) infinite vacua are forever beyond our horizon?

57. **Chris W.**
January 17, 2014

The importance of some sort of falsifiability can be reduced to a simple question one can ask about any idea: "If this idea was wrong, how would I know it?" Whether observations and experimental results in the physical world are relevant to answering this question depends on what the idea is about, i.e., on what problem it was proposed to address or solve.

Some people appear to conclude that not having an answer to this question justifies accepting the idea on faith, or according to one's taste. Of course, one doesn't have to fully accept an idea to investigate it and develop it to see where it leads, but the endgame in science is finding a way to meaningfully check it against observation.

I should emphasize the asymmetry that Popper wrote about frequently. Having a way to check an idea of universal scope—as physical theories are—in no way allows one to prove that the idea is correct. At most, it allows one to establish that idea has value, insofar as it has passed many tests and has (perhaps) been effective in applications.

58. **Dom**
January 17, 2014

Excuse me if I have this wrong but if we negate the idea that Life exists or has existed on other planets don't we falsify it by finding evidence that it does or has? Isn't the framing of the original position the problem here?
Using appropriate language we can easily create apparent problems with logic e.g. "This statement is false"

59. **JR**
January 17, 2014

I am with Dom on this. "life has existed on another planet" is not falsifiable, but the converse: "life has never existed on another planet" is definitely falsifiable. So we can, in principle and in practice, make progress. Compare this to "The multiverse exists" and its converse "the multiverse does not exist". As far as we know right now, neither of these is falsifiable, either in principle or in practice, so they are outside the scope of science.

60. **imho**
January 17, 2014

philh said:
To me the hypothesis that life has existed on another planet is a) not falsifiable and b) is a scientifically plausible hypothesis.

I don't think this makes any sense. Using your logic, physics couldn't make the statement "all electrons have mass X", or "Every action has an equal and opposite reaction". In this universe, there is no such thing as "absolute falsifiability" as you seem to be suggesting. I have no idea about all this Popperian pontificating, but the operational real-world criterion for scientific acceptability is: overwhelming evidence in-support-of with no know counter evidence against. For example, we can never measure the mass of every electron in the universe (even in principal), but in the 100,000 experiments that have been performed that could detect such a thing, we have never seen a deviation from the accepted quantity. Also, the idea that all electrons have the same mass fits perfectly into the currently accepted wider Physics framework. This discussion needs less philosophical nonsense and more "shut up an calculate".

61. **Peter Woit**
January 17, 2014

Maybe that's enough about falsifiability for now. I don't think it's a topic that lends itself to this kind of unfocused discussion.

62. **Simple biologist**
January 17, 2014

Reading the Edge pieces by theoretical physicist on Edge is doing wonders on getting rid of whatever residual physics envy I had left.

Thanks for Carrol et al. for that.

63. **theoreticalminimum**
January 18, 2014

I would like to react to Tegmark's response. When I was an undergraduate, it really struck me when I learnt that Nature realises the (regularised) infinite sum 1+2+3+… = -1/12 in the Casimir effect; one computes the Casimir force as being proportional to this sum, and the result is exactly what one measures in the lab. Wouldn't this stand as a counterargument to Tegmark's comment about retiring the idea of infinity? Without the series being infinite (but regularised), we cannot compute the Casimir force.

64. **Chris W.**
January 18, 2014

> This discussion needs less philosophical nonsense and more “shut up and calculate”.

Don’t forget “shut up and do some experiments” and “shut up and *think*”. These days there is plenty of calculating going on, but not necessarily with much point to it.

65. **Shantanu**
January 19, 2014

Peter, something OT to this post, but not OT to the blog.
video to a seminar by Paul Langacker at PI which gives an idea
of where particle physics/string theory is going.
http://pirsa.org/13010116/

66. **Peter Woit**
January 19, 2014

Shantanu,
Thanks. Looks like the story is still going to be “string theory very promising”, no matter how clear it becomes that it predicts nothing and the LHC sees nothing even vaguely encouraging.

67. **philh**
January 20, 2014

Hello Peter, thanks for your for your reply. I’m glad you agree that falsifiability is not the “be all and end all” of science.
I also agree that it should not retired in favor of a theory of anything,
One difficulty I have is to class all multiverse theories in the same set.
Like you, I’m unimpressed with the string theory landscape, string theory has not had any empirical data to back it up, as you point out there has no super symmetry, extra dimensions or black holes found at the LHC. I’m also unimpressed with the many worlds interpretation of QM; there are too many interpretations and no data to choose between them.
However I find it had to put the inflationary multiverse in the same bracket. The reason is just as experimentalists have failed to find anything like what strong theorists hoped for at the LHC, experimentalists have said the data favors inflation. Wmap and Planck team said this.
At the same time the source you quoted as the one you agreed with (Paul Steinhardt) said
Inflation leads to eternal inflation. Guth said the same thing..
I’m not qualified to say if Guth and Steinhardt’s statements are true or not. But If I see theorists telling me inflation is generically eternal and experimentalists telling me the data is favorable to inflation then I have to say the inflationary multiverse does not appear to be in the same boat as other multiverse proposals like the string theory landscape. Steinhardt also said it would be possible to falsify inflation by detecting a blue tilted gravity wave spectrum.
So whilst the generic idea of a mulitverse may not be falsifiable , the specific idea of an inflaitonary multiverse could it seems be falsified simply by falsifying inflation in the way the Steinhardt has suggested.

Perhaps the problem is for people to decide if a scientific hypothesis is true or false or science or not science. Why can’t each of these be a continuum? With some things obviously false and obviously pseudo science, other things being a plausible scientific hypothesis, and of course, yet others being confirmed solid science.
So If Sean Carroll tells us there’s a multiverse because of the string theory landscape I think he’s on vary shaky ground, but if says it on the back of the inflationary multiverse then it seems to me to be a little bit more solid. Have I gone wrong here Peter, if so where?

68. **philh**
January 20, 2014

Hellow IMho, your discussion of electrons is interesting. But there is a big difference between saying the electrons has mass x and answering the question, is there life on other planets?
The difference is staring you in the face, we have electrons and we can measure their mass. It’s not the same with exo planets. We cannot answer the question of life on other worlds with “shut up and calculate”. How are you going to do that?
Dom and Chris please note my discussion of multiverse above.
Low math I agree testable is better than falsifiable. However whilst I agree the tech will get better to characterise exo planets. If we find nothing, that wont falsifies the idea of life on other planets. Under the falsifiable framework people looking for exo planets aren’t doing science, but under the testable framework they are. That’s why I prefer the testable framework as better. Although a theory that is falsifiable will be always be more compelling than one that is not. Again I’m calling for a continuum here rather than black or white criteria.

69. **Kavanna**
January 20, 2014

String theory doesn’t predict the multiverse. String theory predicts nothing.

The multiverse idea was cooked up precisely to *prevent* string theory from predicting something, something clearly wrong, and thus be disproved. The multiverse idea gained ground to keep the string party going. Attempts to get rid of falsifiability are motivated by the same motive. It’s a racket.

70. **Low Math, Meekly Interacting**
January 21, 2014

philh,

I think we’re largely in agreement. Most of the arguments against “falsifiability” are straw-man nonsense anyhow, but occasionally one has enough nuance to worry about. Absolutists aren’t helpful in any case, which is where the “probabilistic” moderation I think is helpful. I doubt Popper himself would have argued otherwise. You’re right: falsifiability is always highly desirable. But if I can never falsify the prediction that “some day the Sun will rise in the West”, I can make a pretty compelling case for summarily dismissing such foolishness on time scales any mortal ought to be worrying about.

Our Mathematical Universe

January 17, 2014
Categories: Book Reviews, Multiverse Mania

Max Tegmark has a new book out, entitled Our Mathematical Universe, which is getting a lot of attention. I've written a review of the book for the Wall Street Journal, which is now available (although now behind a paywall, if not a subscriber, you can try here). There's also an old blog posting here about the same ideas.

Tegmark's career is a rather unusual story, mixing reputable science with an increasingly strong taste for grandiose nonsense. In this book he indulges his inner crank, describing in detail an uttery empty vision of the "ultimate nature of reality." What's perhaps most remarkable about the book is the respectful reception it seems to be getting, see reviews here, here, here and here. The Financial Times review credits Tegmark as the "academic celebrity" behind the turn of physics to the multiverse:

> As recently as the 1990s, most scientists regarded the idea of multiple universes as wild speculation too far out on the fringe to be worth serious discussion. Indeed, in 1998, Max Tegmark, then an up-and-coming young cosmologist at Princeton, received an email from a senior colleague warning him off multiverse research: "Your crackpot papers are not helping you," it said.
>
> Needless to say, Tegmark persisted in exploring the multiverse as a window on "the ultimate nature of reality", while making sure also to work on subjects in mainstream cosmology as camouflage for his real enthusiasm. Today multiple universes are scientifically respectable, thanks to the work of Tegmark as much as anyone. Now a physics professor at Massachusetts Institute of Technology, he presents his multiverse work to the public in *Our Mathematical Universe*.

The New Scientist is the comparative voice of reason, with the review there noting that "there does seem to be something a little questionable with this vast multiplication of multiverses".

The book explains Tegmark's categorization of multiverse scenarios in terms of "Level", with Level I just lots of unobservable extensions of what we see, with the same physics, an uncontroversial notion. Level III is the "many-worlds" interpretation of quantum mechanics, which again sticks to our known laws of physics. Level II is where conventional notions of science get left behind, with different physics in other unobservable parts of the universe. This is what has become quite popular the past dozen years, as an excuse for the failure of string theory unification, and it's what I rant about all too often here.

Tegmark's innovation is to postulate a new, even more extravagant, "Level IV" multiverse. With the string landscape, you explain any observed physical law as a random solution of the equations of M-theory (whatever they might be…). Tegmark's idea is to take the same non-explanation explanation, and apply it to explain the equations of M-theory. According to him, all mathematical structures exist, and the equations of M-theory or whatever else governs Level II are just some random mathematical structure, complicated enough to provide something for us to live in. Yes, this really is as spectacularly empty an idea as it seems. Tegmark likes to claim that it has the virtue of no free parameters.

In any multiverse-promoting book, one should look for the part where the author explains what their scenario implies about physics. At Level II, Susskind's book The Cosmic Landscape could come up with only one bit of information in terms of predictions (the sign of the spatial curvature), and Steve Hsu soon argued that even that one bit isn't there.

There's only small part of Tegmark's book that deals with the testability issue, the end of Chapter 12. His summary of Chapter 12 claims that he has shown:

> The Mathematical Universe Hypothesis is in principle testable and falsifiable.

His claim about falsifiability seems to be based on last page of the chapter, about "The Mathematical Regularity Prediction" which is that:

> physics research will uncover further mathematical regularities in nature.

This is a prediction not of the Level IV multiverse, but a "prediction" of the idea that our physical laws are based on mathematics. I suppose it's conceivable that the LHC will discover that at scales above 1 TeV, the only way to understand what we find is not through laws described by mathematics, but, say, by the emotional states of the experimenters. In any case, this isn't a prediction of Level IV.

On page 354 there is a paragraph explaining not a Level IV prediction, but the possibility of a Level IV prediction. The idea seems to be that if your Level II theory turns out to have the right properties, you might be able to claim that what you see is not just fine-tuned in the parameters of the Level II theory, but also fine-tuned in the space of all mathematical structures. I think an accurate way of characterizing this is that Tegmark is assuming something that has no reason to be true, then invoking something nonsensical (a measure on the space of all mathematical structures). He ends the argument and the paragraph though with:

> In other words, while we currently lack direct observational support for the Level IV multiverse, it's possible that we may get some in the future.

This is pretty much absurd, but in any case, note the standard linguistic trick here: what we're missing is only "direct" observational support, implying that there's plenty of "indirect" observational support for the Level IV multiverse.

The interesting question is why anyone would possibly take this seriously. Tegmark first came up with this in 1997, putting on the arXiv this preprint. In this interview, Tegmark explains how three journals rejected the paper, but with John Wheeler's intervention he managed to get it published in a fourth (Annals of Physics, just before the period it published the (in)famous Bogdanov paper). He also explains that he was careful to do this just after he got a new postdoc (at the IAS), figuring that by the time he had to apply for another job, it would not be in prominent position on his CV.

One answer to the question is Tegmark's talent as an impresario of physics and devotion to making a splash. Before publishing his first paper, he changed his name from Shapiro to Tegmark (his mother's name), figuring that there were too many Shapiros in physics for him to get attention with that name, whereas "Tegmark" was much more unusual. In his book he describes his method for posting preprints on the arXiv, before he has finished writing them, with the timing set to get pole position on the day's listing. Unfortunately there's very little in the book about his biggest success in this area, getting the Templeton Foundation to give him and Anthony Aguirre nearly $9 million for a "Foundational Questions Institute" (FQXi). Having cash to distribute on this scale has something to do with why Tegmark's multiverse ideas have gotten so much attention, and why some physicists are respectfully reviewing the book.

A very odd aspect of this whole story is that while Tegmark's big claim is that Math=Physics, he seems to have little actual interest in mathematics and what it really is as an intellectual subject. There are no mathematicians among those thanked in the acknowledgements, and while "mathematical structures" are invoked in the book as the basis of everything, there's little to no discussion of the mathematical structures that modern mathematicians find interesting (although the idea of "symmetries" gets a mention). A figure on page 320 gives a graph of mathematical structures which a commenter on mathoverflow calls "truly bizarre" (see here). Perhaps the explanation of all this is somehow Freudian, since Tegmark's father is the mathematician Harold Shapiro.

The book ends with a plea for scientists to get organized to fight things like

> fringe religious groups concerned that questioning their pseudo-scientific claims would erode their power.

and his proposal is that

To teach people what a scientific concept is and how a scientific lifestyle will improve their lives, we need to go about it scientifically: we need new science-advocacy organizations that use all the same scientific marketing and fund-raising tools as the anti-scientific coalition employ. We'll need to use many of the tools that make scientists cringe, from ads and lobbying to focus groups that identify the most effective sound bites.

There's an obvious problem here, since Tegmark's idea of "what a scientific concept is" appears to be rather different than the one I think most scientists have, but he's going to be the one leading the media campaign. As for the "scientific lifestyle", this may be unfair, but while I was reading this section of the book my twitter feed was full of pictures from an FQXi-sponsored conference discussing Boltzmann brains and the like on a private resort beach on an island off Puerto Rico. Is that the "scientific lifestyle" Tegmark is referring to? Who really is the fringe group making pseudo-scientific claims here?

Multiverse mania goes way back, with Barrow and Tipler writing *The Anthropic Cosmological Principle* nearly 30 years ago. The string theory landscape has led to an explosion of promotional multiverse books over the past decade, for instance

- *Parallel Worlds*, Kaku 2004
- *The cosmic landscape*, Susskind, 2005
- *Many worlds in one*, Vilenkin, 2006
- *The Goldilocks enigma*, Davies, 2006
- *In search of the Multiverse*, Gribbin, 2009
- *From eternity to here*, Carroll, 2010
- *The grand design*, Hawking, 2010
- *The hidden reality*, Greene, 2011
- *Edge of the universe*, Halpern, 2012

Watching these come out, I've always wondered: where do they go from here? Tegmark is one sort of answer to that. Later this month, Columbia University Press will publish Worlds Without End: The Many Lives of the Multiverse, which at least is written by someone with the proper training for this (a theologian, Mary-Jane Rubenstein).

I'm still though left without an answer to the question of why the scientific community tolerates if not encourages all this. Why does Nature review this kind of thing favorably? Why does this book come with a blurb from Edward Witten? I'm mystified. One ray of hope is philosopher Massimo Pigliucci, whose blog entry about this is Mathematical Universe? I Ain't Convinced.

For more from Tegmark, see this excerpt at Scientific American, an excerpt at Discover, and this video, this article and interview at Nautilus. There's also this at Huffington Post , and a Facebook page.

After the Level IV multiverse, it's hard to see where Tegmark can go next. Maybe the answer is his very new Consciousness as a State of Matter, discussed here. Taking a quick look at it, the math looks quite straightforward, his claims it has something to do with consciousness much less so. Based on my time spent with "Our Mathematical Universe", I'll leave this to others to look into…

Update: Scott Aaronson has a short comment here.

Comments

1. **Conrad Albert**
January 17, 2014

   if you search for the WSJ review on Google (eg 'Our Mathematical Universe' by Max Tegmark book review Peter Woit), you will be able to read the article for free from the Google search link – at least here in the US and hopefully anywhere else

   I actually read Max Tegmark arXiv paper mentioned here some years ago as I found it very interesting from a science fictional point of view and as a basis of sf world building like for example in several recent acclaimed sf novels like Anathem by N. Stephenson and the Orthogonal series by Greg Egan (Riemannian universe with metric ds^2 = dx^2 + dy^2 + dz^2 + dt^2 and lots of cool implications including speculations on what intelligent beings would look like and what the arrow of time would mean)

   Taking it seriously as applying to the universe we are living in is obviously a different issue, but I would not really be worried that much about this happening as the scientific method is still alive and well.

2. **DLB**
January 17, 2014

   He seems to confuse reality with representations of reality and forget that mathematics is a language. Though it provides rigorous objective representations and a means of standardization, mathematics remains a language and any language can be used to express lies as much as truths. Saying the reality is mathematics is like saying that language is reality.

3. **Simple biologist**
January 17, 2014

   Just a heads up for the 'you can try here' link.

   Worked for me and I'm an European with no subscription.

4. **Max Tegmark**
January 17, 2014

   Thanks Peter for taking the time to write the thought-provoking and (I thought) eloquent WSJ review, which I found significantly more nuanced than your post above! It's not clear to me whether our views actually differ on any matters of physics as opposed to matters of speculation: what we're guessing might be ultimately turn out to be fruitful for physicists to research. Would you agree with me that parallel universe are not a theory, merely predictions of certain theories, and that some of these theories may themselves be testable by other means? For example, would you agree that inflationary cosmology is a scientific theory that can be further tested by searching for B-modes with CMB experiments, etc., and that if we choose to take inflation seriously, then it's not unreasonable for scientists to take seriously all of its predictions, even the untestable ones?

   I feel that our job as scientists is not to tell our cosmos how to be based on our emotional preconceptions (that there must be a multiverse, that a multiverse is impossible, etc.) I therefore dislike being told not to explore certain ideas. For example, I decided to meet with someone from the Templeton Foundation may years ago precisely because a famous professor told me *not* to talk with them. A key goal of the Foundational Questions Institute is to encourage people to challenge prevailing dogmas in physics, like you have yourself courageously done. I think it would be awesome if you'd join our discussions – would be be interested in attending one of our future conferences?

/Max

5. **Max Tegmark**
January 17, 2014

Thanks DLB for raising this interesting point about mathematics as a language, which ties in with the age-old debate about whether mathematics is invented or discovered. Personally, I think that it's crucial not to confuse the *language* of mathematics (which we humans clearly invent) from the *structure* of mathematics, which many mathematicians feel that they discover. For example, any civilization interested in regular 3D polyhedra would discover that there are precisely 5 of them (the tetrahedron, cube, octahedron, dodecahedron and icosahedron). Whereas they're free to invent whatever names they want for them, they're *not* free to invent a 6th one – it simply doesn't exist. The same applies to the mathematical structures that are popular in modern physics, from 3+1-dimensional pseudo-Riemannian manifolds to Hilbert spaces. Once we've classified all irreducible representations of the Poincare group, we're not free to invent new ones!
My $0.02,
Max

6. **notagain001**
January 17, 2014

is the multiverse landscape of string theory limited to only those w/ 3 large spatial dimensions and 1 time dimension and 6–7 curled dimensions?

is there any reason the landscape could not also include any combination of large, curled and time dimensions that adds up to 11? i.e 6 large spatial dimensions, 2 curled spatial and 2 time dimensions? also why do the curled dimensions have to be spatial? why couldn't they be curled closed time like dimensions? i.e 3 large spatial dimensions 1 large time dimension 6 closed timelike dimensions

7. **Pat**
January 17, 2014

This past summer, Tegmark was one of the featured lecturers at TASI and his entire effort would best be described as a sales pitch. He was supposed to have a few lectures on cosmology, but what he actually did was just rip some basic slides and pretty pictures from his public lecture. It's well understood that the point of lectures at these schools is not to gain a complete understanding of the topics (especially something as fundamental, to high energy theory students at least, as cosmology), but to fill in some gaps and get somewhat comfortable with the cutting edge. Tegmark was more interested in using the grad student audience as a warm-up for his featured public lecture (there are a couple given every year at the school).

As another MIT professor (a respectable QCD guy) was one of the directors of the school, there were probably a half dozen MIT grads present. I don't think any of them attended his public lecture because they had seen this show before. When I asked why some of them why they were not interested in going, the best response was because a running joke around Cambridge is that Tegmark is more like a "used car salesman" than a physicist when promoting some of these ridiculous ideas. After going to the public lecture, me and every last student who attended could not agree more. The local organizers (who have been running these awesome schools every summer in Boulder for the last decade, at least; note to NSF and DOE, KEEP IT FUNDED) seemed embarrassed as almost every physicist in the room cringed whenever some of the sillier aspects of Our Mathematical Universe came up. What bothered me the most was the way he would try and legitimize some of his ideas by appearing to connect them to basic principles of cosmology, while any expert (or almost any grad student) knew he was blowing smoke. For instance, he would say something like "… the universe is math, it's all around us. We were just discussing this in Cosmology lecture today …" I really don't mind that he makes a buck selling this drivel, I just don't like being used as implicit endorsement of his ideas.

As for the ideas themselves, I think empty is the best way to describe them. While I'm not a huge proponent of the multiverse and I haven't read the book, I just remembered thinking that he was not really saying anything when he talked about the book. Beyond issues of predictability, postulating that Math=Physics=Everything seems more like a metaphysical statement than anything that even resembles a testable theory. No to digress, but to it's more like trying to prove that people rode around on dinosaurs by building these creation museums. In issues of faith, scientific evidence is beyond the point, so why even try? The fact that such a prominent physicist can seriously promote science fiction with the encouragement of other prominent physicists amazes me. More serious people in the community can mostly just ignore these claims, but large sums of money seem to be diverted towards such useless pursuits. I admire Tegmark's contributions to cosmology (SDSS and WMAP efforts in particular), and I'm sure other TASI students do as well, but he will forever be the butt of our jokes when we reminisce about that summer.

8. **Max Tegmark**
January 17, 2014

Thanks Pat for sharing your views on the TASI school. I take teaching very seriously and therefore welcome criticism of my lectures. I was very happy to hear the many positive comments I got from students afterwards, so I don't think it's 100% accurate to suggest that you're speaking for everyone. Just to clarify: what do you mean by "just rip some basic slides and pretty pictures from his public lecture", when I in fact spent most of my lectures deriving equations on the blackboard, and the long and fun student discussion sessions we held involved no slides whatsoever? You're referring to the school of 2013, right?
/Max

9. **Pat**
January 17, 2014

Max,

On second thought, I may have been too harsh in comments about your Cosmology lectures. To me they just seemed very basic, but half of the students worked on QCD, not BSM physics or cosmology. I don't envy the task of trying to make cosmology material interesting while still having to start from scratch over only 3 or 4 lectures. And at some level what's basic material for physics grad students will overlap with basic material in a public lecture. I thought the lectures were fun and I find The Mathematical Universe interesting and, obviously, provocative. My issue is mainly blurring the lines of science, reality and philosophy. Maybe a having such a different perspective is the point, but larger ideas are typically beyond my pay grade as a BSM phenomenology grad student.

10. **Anonyrat**
January 17, 2014

Isn't it a matter of human psychology more than anything else that there are only 5 regular 3D polyhedra is a surprise, seems like a discovery, while 2+1=3 does not?

11. **Florin Moldoveanu**
January 17, 2014

I acknowledge that Tegmark's position is not popular and smells of being crackpot, and also that Tegmark has sharp political skills, but solely on the scientific value of the idea, it has real merit and I want to defend it.

If reality is not mathematics what is reality based on? Now we have two choices: (1) the question is not well defined (2) reality is based on "something". For option (2) the obvious question is who created that "something"? This may lead us to the idea of "God", but this is not necessarily true. If the idea of ontology is enlarged (like in "the mathematical universe“), software programs, even cartoon characters may be considered ontological and who is to judge that one ontology is better over another? For a software program, it is not “God” who created the program, but a software engineer, who belongs to another ontological level, and so there could be valid levels of recursions in defining reality. The recursion will either end or not (does God of the gaps ring a bell?). A natural termination criterion for this hierarchy is “reality is ultimately mathematics”.

So “the mathematical universe” idea has at least some handwaving arguments going for it, unless choice (1) is true, or maybe the ontological layers go on forever. So the question is not in the least settled. To convince the skeptics, what is needed is to derive mathematical consequences from it. For example, we know that space time is 4 dimensional, we know that nature is quantum mechanical at core, we know the gauge symmetries of the Standard Model. Can we derive this from “the mathematical universe” idea? In effect this means solving Hilbert’s sixth problem of physics axiomatization. Now this I believe it is possible in principle. But to do it, you need to turn Tegmark’s approach upside down: it is not important how reality and mathematics are similar (you are a mathematical theorem says Tegmark), but how they are different. This generates physical principles which distinguish (select) particular mathematical structures from the infinite world of mathematics. Every mathematical structure is unique, but very few are distinguished by nature. As a side note, this bypasses the Gödel’s incompleteness theorem roadblock because the final selected mathematical structures do not have to be in a closed form of a monster meta-structure. If the physical principles are correctly chosen, we should pick precisely the mathematical structures which are used by nature, nothing more nothing less. And this approach actually started to produce results. In particular I am able to derive quantum mechanics in the c*-algebra formalism! (I had submitted preliminary results for publications to serious journals).

So what about being a mathematical theorem? Am I a mathematical theorem? This is malarkey as Joe Biden would say. Mathematical structures are the ultimately building blocks of reality, but they arrange in such a way to avoid contradictions. I think we will be ultimately able to show that this leads to the concept of time, but this is a separate story. As “ugly bags of mostly water”(https://www.youtube.com/watch?v=paH97dYR6Lg) we are “entropy and information parasites” and our existence is not a mathematical necessity.

In conclusion, I think mathematical structures are the ultimate building blocks of reality, but demanding reality to be a mathematical theorem is too constraining and incorrect. But don’t take my word for it and be skeptical. In a year’s time I hope to pass peer review scrutiny in deriving quantum mechanics and win over the skeptics with valid mathematical proofs.

12. **Yatima**
January 17, 2014

In a sense. “Surprise” means there is high informational content. The default assumption of a primate brain would probably be “there is an infinite series of regular 3D polyhedra”, something which evidently can be checked by going into the street and ask people (one should do this!). This has less to do with psychology than with the fact that brains are not generators of theorems about the world of 3D geometry. Misjudgements of such an ethereal kind are luckily not deleterious to one’s capacity to procreate.

On the other hand, while 2+1=3 is indeed a theorem it is also a statement about the result of a program so simple that most people manage to implement it in their heads.

13. **Laymammal**
January 17, 2014

So let us look for a non-mathematical structure out there in reality to falsify this theory. What would that be? An impossible object. Even if it showed up, how would we be able to grasp it? Or we’d end up creating mathematics to accommodate it, which would make it a mathematical structure(like how we handle infinity). Looks like Tegmark’s theory is pretty secure.

14. **srp**
January 17, 2014

ISTM that the Level I multiverse is the most challenging because it simply extrapolates the consequences of well-known and tested theories to produce a hard-to-believe result. This extrapolation from the known to the unknown (and bizarre) is at the heart of Tegmark’s rhetorical question in the thread above about believing the untestable implications of testable (and non-falsified) theories. (I would like to know if most physicists and cosmologists accept the combinatorial arguments for the Level I multiverse; if so, why this highly “woo-woo” conclusion hasn’t been taken up by the popularizers and if not what is the flaw in the argument.)

But when people are skeptical of the foundation “theory” (e.g. M-theory) his rhetorical extrapolation point is much weaker–it’s easier to say that the bizarre consequences are just another reason to reject the foundational claim. In that case, his only real argument is a radically Platonist twist on Occam’s razor, i.e. that a theory with fewer asymmetries and free parameters is preferable even if it multiplies unobservable entities without measure. I suspect most people, however, find a theory with a small number of unobservable entities, even with many free but observable parameters, to be far superior to one with no free parameters and an unstructuredly infinite number of unobservable entities.

15. **vmarko**
January 17, 2014

Max (and others),

I think you are misinterpreting math to be a far too unique body of knowledge. Let me explain what I mean:

“I think that it’s crucial not to confuse the *language* of mathematics (which we humans clearly invent) from the *structure* of mathematics, which many mathematicians feel that they discover.”

I’ll be bold to say that those many mathematicians are deluding themselves in thinking so. Any mathematical statement consists of assumptions, conclusions and the rules of inference (the “A”, the “B” and the “=>” in a statement A=>B, respectively). The point is that *all* of them are arbitrary, most notably the rules of inference. The fact that most mathematicians are using ordinary first-order predicate logic as a “commonly accepted” set of inference rules, it is by no means unique or better than other, different logic systems. So saying that people *discover* math structures, based on axioms they *invented* and rules of inference that they also *invented*, is simply misleading. Math is completely a *language*, an *invented* thing, together with axioms and the ways we perform proofs of theorems based on those axioms. The fact that the first order predicate logic is commonly accepted as “the truth” is a consequence of the way our brains evolved, rather than that it is any “truth” per se. It’s just another (useful) language, nothing more. People who are doing research in mathematical logic know this for a few centuries already (so these are not just my wild opinions).

“For example, any civilization interested in regular 3D polyhedra would discover that there are precisely 5 of them (the tetrahedron, cube, octahedron, dodecahedron and icosahedron). Whereas they’re free to invent whatever names they want for them, they’re *not* free to invent a 6th one – it simply doesn’t exist.”

I disagree. If you build a 3D Euclidean vector space over a field of rational numbers as scalars, then you can prove (assuming standard logic) that none of those solids exist. Namely, an equilateral triangle doesn’t exist in such a space, given that its area is an irrational number. Imagine a civilization which didn’t invent the concept of real numbers — they could say that your “discovery of 5 solids” is plain false. This has actually already happened in our own history — Pythagoreans had problems with the diagonal of the square… And if you don’t like rational numbers, you can close them up in one of the p-adic fields, as opposed to reals. And if you don’t like modus ponens rule of inference, just choose some other logical system (oh yes, there indeed are logical systems where modus ponens does not hold).

Math is a language all the way, from “A” over “=>” to “B”. Plato’s world of ideas, that is being “discovered” in math, exists only for people who choose to use the same language for “A” and “=>”. There is no “existence” in math beyond the common language assumptions. Plato of course couldn’t have known this, since “other” logical systems have been shown to be possible only in 19th and 20th century. But today, nobody has an excuse for not studying and understanding mathematical logic…

And don’t even get me started on set theory (actually, theories), axiom of choice, Banach-Tarski paradox, etc. 😉

Best, 🙂
Marko

16. **Florin Moldoveanu**
January 17, 2014

@Marko

I highly recommend this essay by Alain Connes on mathematics: http://www.alainconnes.org/docs/maths.pdf

To this I would only have to add that mathematics is about abstract relationships and as a nice example consider the history of imaginary numbers. People did not accept them (and the very name imaginary echos its history) for more than 200 years until someone discovered a well defined matrix representation. Those abstract relationships (and physical laws) existed well before someone discovered and named them. E=mc^2 was valid during dinosaur time too.

Mathematicians are exploring a timeless landscape of abstract relationships. Which part of this landscape is explored, the order of the exploration, and the name of various part of the landscape are historical accidents.

Set theory is not necessarily the foundation of math. Category theory is a viable option. Godel incompleteness theorem is a serious roadblock but it can be avoided.

17. **vmarko**
January 17, 2014

Florin,

Maybe you misunderstood my post. I don’t have a problem with irrational numbers. I have a problem with the uniqueness of rules we claim to be valid when we prove theorems in math. Things like rules of inference, laws of excluded middle, concepts of “true” and “false”, etc. The complete body of mathematics resides on these rules, and my point is that those rules are completely arbitrary and nonunique. There are no absolute truths in math, only the ones that pertain to the language you use.

“Mathematicians are exploring a timeless landscape of abstract relationships.”

No, they are *inventing* that landscape (typically unintentionally). If you apply a different logical system to a given set of axioms, you will reach different conclusions. One could also say that mathematicians are “exploring” some landscape using tools that actually construct that landscape. Using a different set of tools would give different landscapes.

I suggest that you take a glance at the Wikipedia article about non-classical logics, there are many of them:

http://en.wikipedia.org/wiki/Non-classical_logic

The whole math ultimately boils down to a definition of a language we accept to use. There are no mathematical “objects”, or even statements, whose existence is independent of the choice of that language, which is itself quite arbitrary.

Best, 🙂
Marko

18. **S**
January 17, 2014

Leaving aside the (very important and forceful) questions about testability, etc., it’s always seemed to me that Tegmark’s “everything in math” stuff would run into some very strange consequences for our own universe, right now.

If “all mathematical structures” exist, then I guess that means (maybe?) that *every* set is somehow instantiated in reality, *every* topological space is a “spacetime” somewhere (whatever that means), and so on. But the thing is, most mathematical objects aren’t very nice. There are far more discontinuous functions than continuous ones (in a reasonable topology); there are far more non-Hausdorff topologies than Hausdorff ones; and so on.

If every mathematical object exists, then why shouldn’t our universe be one of the ones that stops being nice tomorrow? It’s basically just David Hume’s induction-skeptical argument, of course, but it seems to me that it gets very awkward for physics once you actually assert that, yes, you believe in all these universes, and that for every spacetime that acts in a nice regular way like ours up to today, and then keeps doing so tomorrow, there’s an uncountable infinity of them that acts nice and regular up to today, and then lapses into awful, unmeasurable incoherence tomorrow.

What would be the rationale for believing that *our* spacetime is one of the structures that’s nice globally?

Again, these are questions that *every* physicist and every person has to address (at least in theory), since Hume; but Tegmark, it seems to me, has given a negative answer, without drawing attention to it: we can’t. We’re probably in a universe where the laws of physics stop working tomorrow. End of story.

That seems like a big problem with the theory. But possibly I’m missing something.

19. **S**
January 17, 2014

“everything in math” should read “everything is math.” *sigh* Sorry.

20. **Florin Moldoveanu**
January 18, 2014

Marko,

You state: “There are no absolute truths in math, only the ones that pertain to the language you use.” I completely agree.

"If you apply a different logical system to a given set of axioms, you will reach different conclusions." agree again.

"One could also say that mathematicians are "exploring" some landscape using tools that actually construct that landscape. Using a different set of tools would give different landscapes." this becomes repetitive. I agree 100%

"There are no mathematical "objects", or even statements, whose existence is independent of the choice of that language, which is itself quite arbitrary." ditto

So how can I agree on all this end yet hang on the "math landscape"? I think we differ on the semantics of "existence" (As Bill Clinton put it, "It depends upon what the meaning of the word 'is' is."). Let me repeat my prior observation: math is about abstract relationships with the emphasis on abstract. The same abstract mathematical structure can have very different and distinct concrete realization. In math the notion of truth is not definable inside an axiomatic system (Tarski theorem) and there is no "context independent" notion of truth. In nature any two observers agree on an event and the notion of truth is context independent. That is why physics is an experimental science and we settle disputes between competing theories (e.g. classical vs. quantum mechanics) by experimental evidence. In physics true means agreement with nature.

When I say that nature is ultimately made out of mathematical relationships I mean there is nothing unexplained which cannot be described as a mathematical relationship (here I agree with Tegmark). This is a restatement of Wigner's position: "unreasonable effectiveness of mathematics". Where I don't agree is that reality is one giant mathematical theorem/structure. This is because nature and reality is not abstract and in nature truth is context independent. One funny way to state this is: sticks and stones may break my bones, but when was the last time you read in the news that someone was hurt by Pythagoras theorem?

Let me present an analogy (to be taken with a grain of salt like any analogy). Suppose that you want to understand ice and water, you don't know any chemistry, but a friend of yours who is a chemist tells you that ice and water are made out of the same building blocks, H2O. You are very familiar with ice but never seen liquid water before (ok this is far fetched, but please play along). What would you do? Trusting your friend would you look at the similarities, or at the differences to understand liquid water? If when you have a hammer, everything looks like a nail, you will pick the similarities and pretty soon you will run into great difficulties. If you look at the differences, you can learn something new. In this analogy the water molecules are like the abstract mathematical structures. The ice corresponds to the concrete realizations of the abstract mathematical structures into what you object that are historical accidents: set theory, groups, a specific type of logic, etc. Just like ice, those mathematical structures are immutable or "frozen". Liquid water corresponds to nature and another way to have a realization of the abstract mathematical structures: arranged in such a way to give rise to a context independent notion of truth. You don't need to agree with me on this, but from looking at the differences I can derive hamiltonian mechanics (both classical and quantum mechanics) and I am in the process of publishing this. What differences gives you turn out to be very useful physical principles. Here is a key new physical principle which I am using: the laws of nature are invariant to how we partition in our mind a physical system into subsystems. This is trivial but unbelievably powerful because the laws of nature must be invariant to tensor composition and there are only 3 possible ways this can be achieved (one of which is quantum mechanics).

Back to the math landscape. The fact that in flat 2D space the sum of the angles in a triangle is 180 is a "timeless" statement true in its context. It is the very existence of the abstract math landscape which makes proofs possible.

When we talk math which describes nature we are talking about an infinitesimal small amount of mathematical structures which are selected by nature. For example, why nature prefers SO(3,1) for example? Why not SO(p,q) with p and q arbitrarily large? Why is SO(3,1) distinguished from the infinite number of possibilities? The standard way of physics is to look at the experimental evidence and state: with so and so precision, in its domain of validity, this or that theory/mathematical structure describes nature. "Why" is not a scientific question, but a nebulous philosophical blabbering for crackpots or Nobel prize winners gone soft. What I do want to show is that this question "why" is actually very scientific and falsifiable in Popperian sense, and moreover the answer can be given in the most rigorous mathematical way. On key ingredient however is that reality is ultimately made out of mathematical relationships. To the approach of Tegmark, I have to say: show me the money. What can you derive from this hypothesis?

21. **Peter Woit**
January 18, 2014

Hi Max,
Thanks for writing in here, and graciously dealing with a rather hostile crowd.

I think you're right that we likely agree on what's physics and what's speculation. But we also I think disagree on how to handle different sorts of speculation. In my book I used the Bob Dylan line "But to live outside the law you must be honest" to try and make the point that if you're going to make yourself immune from being kept honest by experimental data, you need to take great care to not fool yourself.

Sure, inflationary cosmology is a theory (in lots of versions…) that you can extract predictions from. Like any theory, if you figure out how to test distinctive parts of it and it passes those tests, you can have some confidence that other, untestable parts of it correspond to reality. I really don't have a simplistic view of testability: for instance, I'm happy to agree that a theory can be so beautiful and mathematically compelling that I'd have confidence in it given only minimal experimental evidence. What I have no confidence in is the combination of a very complicated, uncompelling mathematical structure with no experimental evidence (I'm thinking M-theory…).

I also don't have a simplistic attitude about Templeton, and honestly would have loved to hear more in your book about the story of them and how FQXi happened. Templeton has an agenda of a peculiar kind that people should be aware of, but all money comes with some sort of agenda. I've attended one Templeton-sponsored workshop, and that was interesting. As for FQXi events, often the topics seem to be things I'm not particularly interested in (for instance your latest on information), but for some topics I'd be happy to join your discussions. As a general rule, I do think you and many other physicists would probably benefit from more interaction with mathematicians who work on core research mathematics. Math is a subject that takes on different aspects when you start to see what the structures are that the best research mathematicians are now struggling with.

Thanks again for writing in!

22. **Peter Woit**
January 18, 2014

Florin and Marko,
Enough, I don't see this going anywhere or having much at all to do with Tegmark.

23. **Orin**
January 18, 2014

S,

Yes you are missing something. If the MUH suggests that we are probably in a universe where the laws of physics stop working tomorrow (and it's not at all clear to me that it does), then it also suggests that there is a universe where the laws of physics *start* working tomorrow. From an anthropic standpoint no conscious observer would ever take notice of their "swapping" between such universes. This kind of logic can be extended to arbitrarily small time slices, of universes jumping in and out of existence, and I think ultimately leads to the conclusion that conscious observers should exist, blissfully unaware of their nature, in even the most chaotic and unseemly of mathematical soups. This is one form of the Bolzmann brain paradox, which I do think needs to be better addressed in "multiverse-aware" physics, although it is a really difficult problem of defining a suitable probability measure. And that's the rub of it. It may be that (for example due to some morphisms between/onto some mathematical structures being far more common than others in the set of all mathematical structures) that simpler , "nicer," more contiguous universes are far more probable than those ugly ones that give you pause.

Orin

24. **S**
January 18, 2014

Thanks, Orin, but I don't think I find that convincing. I see no reason why the concious observer would happen to jump into a spacetime where the laws were about to start working tomorrow (or jump into another spacetime at all, for that matter). It's one thing if we're talking about the past, at which point that might be an explanation for why we haven't noticed such a thing (though none is needed); but, given an a-priori belief in a level-IV multiverse, it completely fails to give any reason to believe that, in the FUTURE (i.e., even tomorrow), we will still find ourselves in an ordered universe.

In fact, I don't even know what "tomorrow" means in another spacetime. We're only in a single spacetime, and "tomorrow" is a point only in ours — if there's another universe, then none of the points of time in it can rightly be called "tomorrow" by us, here, now. My point is just that there's good reason to believe that, of all the mathematical structures whose "initial part" (i.e., that part which we've already experienced or can observe) is consistent with our universe so far, very, very few of them stay good for us for another day.

Of course, these questions would be easier to discuss if the theory in question were better defined, and Peter's point is perhaps therefore the most relevant; but I do think that it's already well enough defined to raise this fairly serious concern; unless, of course, I am missing something (else).

25. **S**
January 18, 2014

Orin,

As I think more, I suppose you are saying simply that somebody else will begin who happens to remember what the past was of the person in the doomed universe that went crazy. That would make sense as far as it went, except that I think far too much is assumed in applying the anthropic principle in such a way.

If every mathematical structure exists as a spacetime, then there will also be those where YOU exist, but everything ELSE goes crazy; and I would argue again that, of those, we should expect that in almost all of them, everything else will stop working tomorrow. (This argument of course cannot be made rigorous beyond the level of analogy to simpler mathematical contexts, but I find the analogies quite powerful).

As you say, this is getting close to a Boltzmann brain paradox, except that the concern instead is with the persistence of physics, beyond the observer, into the future. And yes, finding suitable probability measure is a problem, but the best analogies we have suggest that the answer won't look very good. I think I fail to see the motivation for adopting a theory that asserts the existence of such problems (At the very best, on a level-IV multiverse, we would have to say we have no reason in the world to believe that the laws of physics WILL persist outside us for another day), when there is rather little argument for the theory in the first place. Science, after all, is based (isn't it?) on the search for regularities, for laws. What is the motive or justification for adopting a theory that implies their probable nonexistence?

26. **Jesper**
January 18, 2014

First: Peter, thank you so much for your persistency. I truly enjoy following what might appear – but I think, is not – a work of Sisyphus.

Second: I have always wondered about what to me appears as a complete shift in the attitude of theoretical physics. Before string theory I think that the history of science and physics has a clear arrow towards simplicity. Maxwells equations, Einsteins Relativity, Quantum Mechanics … – even the Standard Model. To me, it all seems to point, perhaps not in a completely straight line, towards something very simple.

And then comes string theory and claims that its all an "accident", and the multiverse stuff, which seems to be infinitely worse. I never understood this.

I can't help mentioning Alain Connes here. His approach to the Standard Model is, in my reading, a natural continuation of the quest for simplicity. In his formulation he sees the Standard Model as unique. As a "has to be" theory. This kind of thinking appeals to me infinitely more than all this "lets just give up on science before we give up on our own ideas".

Also, about this Tegmark book: I haven't read it, but my immediate thought was – as you also points out – that he appears to need some kind of measure over the space of mathematical structures. Such a measure must, I guess, also be some kind of mathematical structure (which, of course, is completely absurd) and thus the idea appears to be self-contradictory – but perhaps I just haven't got the right attitude? Vibes …

27. **Jesper**
January 18, 2014

One more comment:

Max Tegmark writes: "…I therefore dislike being told not to explore certain ideas. For example, I decided to meet with someone from the Templeton Foundation may years ago precisely because a famous professor told me *not* to talk with them. "

I think its great to be a rebel and in a way I think its a problem for theoretical physics that we don't have enough of them – especially among young people (perhaps the rebels just don't survive, in our system today). But I think the real challenge is to be a scientific rebel. To come up with sci-fi ideas, such as "the universe is an atom in some huge being" is great while smoking pot, its easy – but to come up with truly new bold ideas is much harder – and that, I believe, does take a certain measure of 'rebelness'.

28. **Philip Thrift**
January 18, 2014

Seth Lloyd has a post on edge.org [http://www.edge.org/response-detail/25449] that relates to this "mathematical universe" idea that makes sense to me:

"Suppose that everything that could exist, does exist. The multiverse is not a bug, but a feature. We have to be careful: the set of everything that could exist belongs to the realm of metaphysics rather than of physics. Tegmark and I have shown that with a minor restriction, however, we can pull back from the metaphysical edge. Suppose that the physical universe contains all things that are locally finite, in the sense that any finite piece of the thing can be described by a finite amount of information. The set of locally finite things is mathematically well-defined: it consists of things whose behavior can be simulated on a computer (more specifically, on a quantum computer). Because they are locally finite, the universe that we observe and the various multiverses are all contained within this computational universe."

29. **Georg Mayer**
January 18, 2014

I don't think that is far off to be astonished about how well math fits reality – and especially how some areas of math evolved without any real-world triggers into something, that later on did perfectly fit to empirical phenomena. What is fascinating to me is that math can be thought and developed independently from the natural world and later on the two still fit together.

Therefore I think it is highly simplified to say that we use math only as a language to talk about the natural world. If so, it is at least a very (very very) well designed language, as it works surprisingly fine, even if we only use the language on its own. And I do not regard math as a language – using that term makes the subject too blurry.

From that perspective, the relation between math and reality are interesting. And they are not esoteric, as some of the most important natural sciences use math as a given and could not work without it. It seems to me therefore not far off or funny, if this relationship is examined a bit more – and that people come up with theories.

I also find it strange, that Max Tegmarks background (financing, name, family) is used (besides other, more reasonable arguments) to criticize his theory. It's to me not even wrong to talk about money and family when discussing scientific theories.

Seems like some people think of Max Tegmarks ideas as metaphysical and therefore throw the old stones, which are used since 2500 years in this fight. Natural science needs people who strive in the area which is currently thought of as metaphysics, in order to move scientific knowledge deeper into this realm, to find certainty where so far we only had mystery.

And no, I am not subscribing to multiverses or mathematical structures being the real reality – I agree that this sounds too easy (although it does not sound too empty to me). But I subscribe to respect and the diversity of ideas – out of that we get discussions and real scientific progress.

30. **Peter Woit**
January 18, 2014

Georg Mayer,
Tegmark's father, FQXi, his name change, etc. are all topics he discusses himself in the book. A major topic of the book (which I think is interesting and relevant) is the issue of how he has gone about working on and getting attention for things like the "Level IV multiverse".

31. **Joe Riley**
January 18, 2014

Since Dr. Tegmark is reading this blog (and his willingness to reply is most appreciated) perhaps he will clarify his comments that lead up to the statement (location 5796 in the Kindle version) that "The way I see it, inflation has logically self-destructed."

That would seem to have implications….

32. **Sandro**
January 18, 2014

> I'm still though left without an answer to the question of why the scientific community tolerates if not encourages all this.

Because some people are Platonists, though you likely are not. If you're already convinced of mathematical realism, then Tegmark argues for some rather natural implications of this view; for instance, it explains why even the strong anthropic principle is true, which is pretty compelling.

If you're already unconvinced of mathematical realism, then you're not likely to find anything in Tegmark' work to change your mind, but arguing the philosophy of mathematics wasn't his goal.

33. **Peter Woit**
January 18, 2014

Sandro,
Actually I am a Platonist, have no problem at all with the idea that physical reality is in some sense "mathematical". What I don't believe is that this statement by itself gives you any new useful insight beyond the already obvious fact that mathematics is our best and very powerful way of describing fundamental physics. In particular I don't believe it means Tegmark's "all mathematical structures exist" is anything other than a completely empty statement.

34. **Apostolos Syropoulos (@asyropoulos)**
January 18, 2014

The idea of a mathematical universe is similar to the idea that the universe is a enormous computer that computes its next state. In fact, Fotini Markopoulou has published a paper where she "proves" that the universe is a quantum computer! More generally, Seth Lloyd has already "showed" in Programming the Universe that the universe is a computer. And of course this goes back to Konrad Zuse who, to the best of my knowledge, was the first who put forth this "idea". Thus, in a sense, Max Tegmark is not saying something new but he repeats, maybe in a different way, an old idea. Personally, I think this idea is pure mysticism and has nothing to do with computing and mathematics.

35. **Sandro**
January 18, 2014

> What I don't believe is that this statement by itself gives you any new useful insight beyond the already obvious fact that mathematics is our best and very powerful way of describing fundamental physics.

If you believe that mathematical objects exist, then that implies that a mathematical object that completely describes our universe also exists. Why then would you insist that *our* physics/reality is not exactly this mathematical structure?

Furthermore, this implies that mathematics isn't our "best" tool, it's the *only* tool in the end. It also explains why the anthropic principles scientists often appeal to in order to explain existence are *necessarily* valid, as I previously said.

Tegmark's position addresses a number of outstanding metaphysical and epistemic questions in philosophy and science. While this perhaps doesn't provide much mathematical insight, at least, not the kind of mathematics that interests you, that's not the only kind of insight of interest.

> In particular I don't believe it means Tegmark's "all mathematical structures exist" is anything other than a completely empty statement.

Tegmark is saying that all *finite* mathematical structures exist. That's not an empty statement, and is a restriction on unbounded Platonism which is meaningful given what we now know from computer science.

36. **Orin**

January 18, 2014

S,

When you say “but the best analogies we have suggest that the answer won’t look very good.” What analogies are those? For analogies my mind first wanders to Feynman’s path integral formalism of quantum mechanics. One could look at these many crazy paths that far outnumber the classical ones, and ask how is it possible that all of these contribute, and yet we find ourselves in a classical-seeming trajectory? So I think there is precedent. And I think that it is logical that we try to extend good/successful ideas to their absolute limits.

I do not see how you jump to the conclusion that the MUH predicts lawlessness. That is far from clear. It may be that once a good measure is found that simpler laws are far more common. In a similar vein to the MUH there are various arguments in algorithmic information theory (I don’t know, see here for example) that indicate that lawful algorithms are far more common than unlawful ones in the set of all algorithms. This may be at first counter-intuitive but that doesn’t mean it is wrong!

Speaking to your point about motivation: for me the motivation is ultimately philosophical. Putting aside the argument about intrusion of philosophy into physics, philosophy for its own sake is not such a tragic endeavor. I personally will never be satisfied with a TOE unless it answers every last “why” question. Ideas like the MUH seem to come closest to actually being able to do that in a satisfying way (and as a class of ideas seem to be pleasantly unique in this regard). But of course to each his or her own.

37. **Peter Woit**
January 18, 2014

Sandro,
I still just don’t see Tegmark’s “MUH” as telling me something non-empty about physics (or math), and adding “finite” as a modifier doesn’t change this.
He may have something non-empty to say about philosophy, but, honestly, instead of paying attention to his philosophical views I think my time and other people’s would be better spent paying attention to good philosophers who have thought deeply about the subject and are working within a long tradition of others doing this. In other words, I just don’t have time for amateur philosophers, life is too short.

All,
Please, don’t use this as an excuse to post comments on what you think about general issues of math, physics and philosophy. If it’s not about Tegmark’s specific views or book, it doesn’t belong here. I can’t moderate a general philosophical discussion on these topics, and an unmoderated one rapidly becomes unreadable.

38. **Sandro**
January 18, 2014

This will be my last post on the philosophical aspect of this, as per Peter’s request above.

> I still just don’t see Tegmark’s “MUH” as telling me something non-empty about physics (or math), and adding “finite” as a modifier doesn’t change this.

Does de Broglie-Bohm and Many Worlds tell you something non-empty about quantum mechanics? Tegmark’s work is in the same vein. Perhaps you don’t find it compelling, but that puts you in the Copenhagen camp that dismissed the utility of such alternate interpretations for years, ie. “shut up and calculate!”. Alternate interpretations don’t say anything interesting if you’re only interested in empirical data or calculated predictions, since all interpretations are formally equivalent. They do have significant explanatory power though, a power that Copenhagen completely lacks.

I also think you are too dismissive of scientists doing philosophy. Philosophers aren’t as well versed in science and mathematics as you seem to imply, so their views aren’t necessarily so well-informed as you think. Furthermore, “long tradition” doesn’t count for a damn in selecting a scientific theory, or relativity and QM would never have been adopted, and it’s not a meaningful metric in philosophy either. What matters is its axiomatic parsinomy and the set of resolved and unresolved questions it answers.

If you’re not interested in what Tegmark’s work addresses in the philosophical domain, that’s fair enough, but that shouldn’t be taken as a value judgment of the work across all domains. Physicists often unknowingly do metaphysics, like the aforementioned work on interpretations of QM, and while this sometimes seems to have questionable value at the time, plenty of abstract reasoning has led to breakthroughs before.

> Sandro, […] what exactly do you mean by “exist”? Until such a statement is substantiated into something that can in principle be measured, I think it is pretty close to being empty.

This seems like a category error. Measurements are defined within universes, not across them. The MUH as a whole will forever be unobservable. You might as well ask whether the people in Sim City could ascertain that you exist.

At best, the MUH might eventually predict some interesting properties about our theories, on an epistemic or ontological level. Its real utility at the moment is in its explanatory power via its unification of computation, mathematics and physics.

39. **lun**
January 18, 2014

Max Tegmark, the statement “there are 5 and only polyhedra” is true given the assumptions of 3D flast space geometry (which isn’t even physically correct), and a given a formal definition of polyhedron.
Given different assumptions, the statement will not be true. Further more, as Godel taught us, our ability to follow from assumptions to conclusions is limited in principle, and (perhaps even more importantly), as Tarski taught us, even our ability to define things formally is limited.
Of course there are sensible and nonsensical mathematical structures, but, as your collegue Chomsky taught us, there are also levels of grammatical consistency, with a continuum of grammatically consistent sentences that make no sense.
So, to be perfectly honest, your distinction between language and structure might not be so clear. And if Mathematics is more of a language, saying “the universe is mathematical” is a vacuus statement, in principle and not just in practice.

40. **Abraham**
January 18, 2014

Prof. Tegmark’s book is clearly not a physics ,or a mathematics or a philosophy ,or a theology textbook.
Still,the book provides good and highly needed scientific entertainment which is so rare today

41. **Tom**
January 18, 2014

I am reminded of Stephen Wolfram’s book “A New Kind of Science”, wherein he also posits the “universe=math”, or more specifically, the universe is just a bunch of ‘cellular automata’ working to simple “rules”. Long time since I attempted to read book, so I could be mis-remembering.

Anyway, Wolfram's book was impenetrable and ultimately seemed little more than navel-gazing. I'm not aware of a single illuminating explanation, prediction, or retrodiction that ever arose from thinking of the "universe = cellular automata" in Wolframs book

I am very skeptical — no, disbelieving — of any attempt to claim the material, physical universe literally *is* "math structures" , unless one is twisting the conventional meaning of words the way Humpty Dumpty did.

42. **paddy**
January 18, 2014

How about this: with all respect (ahem) and as a "child" of the 70s and a still practicing physicist. my opinion is that what Tegmark "preaches" is nonsense. Such nonsense used to be confined to the "crackpot sessions" of the APS meetings.

43. **S**
January 19, 2014

(I think this comment is about the MUH in a sufficiently direct way not to run afoul of your above request, Peter — I apologize if not).

Orin,

An example that would leap quickly to mind to suggest that, in an MUH, most spacetimes might not be very nice, would be the space of all continuous functions on R^k with its canonical topology: the space of functions that are ANYWHERE differentiable is meager in this space, and prior differentiability (for example, differentiability for t < t_0) doesn't buy you any more than your hypothesis.

Another example would actually be algorithmic information theory, which you bring up: overwhelmingly most computable bit strings are not computable by short programs, and as the length n goes to infinity, the ratio goes sharply to zero if "short" means anything close to strong.

You raise the example of Feynman's sum over paths; but that sum has a measure with a great deal of content, the action. Where would one find such a measure in the space of all mathematical structures, since it is itself just another mathematical structure and another one could be supplied? You say you want every "why" question answered. Well, if the MUH does work only because one chooses a particular measure on it, doesn't that just mean that it has completely failed to answer the biggest question of all? How that is any better than simply looking for the laws that describe our own universe, I can't imagine.

I did skim with interest the paper that you linked, but it had a lot of very strong (I thought) hypotheses, none of which would apply to a space as big as the space of all mathematical structures. (Indeed, I am reminded of the NFL theorems, which again are relevant by close analogy if not directly).

At the end of the day, this perhaps comes down to taste — what is a convincing analogy, what is a strong assumption, what is an exceedingly non-parsimonious explanation relative to the phenoma it explains, etc. Perhaps we can both agree in the hope that such a structure as the Level-IV multiverse/MUH, should it ever gain currency, will be supported somehow by actual experimental evidence or devastating argument that simply defeats skepticism, and not by mere taste?

44. **Maurice Carid**
January 19, 2014

A question to those who have read Max' book: does it contain any major new insight by him that is not already contained in a Scientific American article he published more than a decade ago (http://arxiv.org/abs/astro-ph/0302131)?
Or is the book just an inflated write-up of that article?

45. **Peter Woit**
January 19, 2014

Maurice Carid,
In terms of the multiverse levels, and Math=Physics claims, the book is basically an extended version for a popular audience of the same material you can find in Tegmark's articles and other material (some linked here, more on the arXiv and available from his website). The book does cover other topics, for instance some of it is a memoir of his career, and at the end he writes about a variety of topics with no connection to math/physics issues.

All,
Please, no more comments that are not directly and specifically about Tegmark's work, and no more attempts to use this to promote your own ideas that have nothing much at all to do with Tegmark.

46. **Orin**
January 19, 2014

S,

I wrote you what I thought was a thoughtful reply, but apparently Peter removes only the one-side of a discussion that does not support his own opinion, even when the two sides are engaged in discussing the same thing (namely Max Tegmark's work).

47. **Peter Woit**
January 19, 2014

Orin,
I'm cutting off your discussion with S just as I've cut off a bunch of others. This has nothing to do with agreeing with S vs. you. My writing about Tegmark's empty claims does not mean I'm willing to host empty general discussions about math and physics inspired by him here.

This topic has become a crank magnet (I've deleted 2-3 times the number of comments you see here), making it even more clear to me the danger Tegmark's efforts pose (that of turning a once great subject into crank city). As I point out in the posting, I think the only interesting question his work raises is why it is getting attention from usually serious quarters of the community, and what can be done about the problems this raises.

48. **Neil**
January 19, 2014

Personally, I think it is useful to have brilliant thinkers like Tegmark proposing highly speculative ideas. The problem is when highly speculative ideas absorb a large fraction of researchers in the discipline, particularly younger researchers.

49. **Peter Woit**

January 19, 2014

Neil,
“Highly speculative” is one thing, “meaningless” is another…

50. **Max Tegmark**
January 19, 2014

Thanks Peter for answering some of my questions! Please help me make sure I’m understanding you correctly and not misinterpreting anything.

> Sure, inflationary cosmology is a theory (in lots of versions…) that you can extract
> predictions from. Like any theory, if you > figure out how to test distinctive parts
> of it and it passes those tests, you can have some confidence that other, untestable
> parts of it correspond to reality. I really don’t have a simplistic view of testability:
> for instance, I’m happy to agree that a theory can be so beautiful and
> mathematically compelling that I’d have confidence in it given only minimal
> experimental evidence.

So is it fair to say that you agree with me that what I call the Level I multiverse (the existence of at least some spatial regions the size of our observable universe that unobservable because light from them hasn’t yet had time to reach us) is a prediction from some inflation models that you in turn consider to be within the purview of science (testable scienfic theories/models)? Is it fair to say that you therefore view the Level I multiverse as a topic appropriate for scientific (as opposed to merely philosophical) discussion?
Please note that I’m not asking you the corresponding question about the Level II multiverse, since you’ve made your misgivings about the string theory landscape quite clear.

On a personal note, I’d also appreciate if you could explain the striking discrepancy between your blog post above and your review of my book in the WSJ: the latter struck me as quite balanced, with no mention of “grandiose nonsense”, “inner crank” or non-scientific speculation related to my family, name, funding, motivation, etc.
/Max

51. **Bernhard**
January 20, 2014

Max Tegmark,

One problem I see with the multiverse (and I understand that also includes your Level I) as just another prediction of a theory is that it can be used to justify virtually anything. Suppose another intelligent race (in this universe…) don’t yet know Mawwell’s theory, hit a intellectually wall and are trying to explain the value of the speed of light, which they can measure precisely. A multiverse “theory” can well be used to “explain” it (they happen to live in the the universe where the speed of light is correct). In this sense I am not sure how much the multiverse is really a “prediction” of inflation or a symptom of its weakness in a certain domain. While scientific valuable, the theory most likely cannot be extrapolated to explain, say, the cosmological constant, at least not in a non-environmental way.

PS: I really don’t want to prolong this already confusing discussion. If Max is willing to answer, it would be interesting to read what he thinks – if not, for the rest, please just ignore my comment.

52. **Bernhard**
January 20, 2014

sorry,

* doesn’t yet know Mawwell’s theory, hit an intellectual wall*

53. **Peter Woit**
January 20, 2014

Max,
Sure, a “Level I” multiverse can be an implication of an inflationary theory that you can test, and if you can get enough evidence for that theory you would have evidence for that sort of multiverse. I’m a bit skeptical that you really can get enough info about the inflaton, but sure, this is science.

About the difference between the review and the blog posting. The blog posting was aimed a very different audience, people who regularly read this blog. The aim of the review was to give an accurate picture of what’s in your book, provide some context, explain the main claim you are making, and also explain why it’s empty. I don’t think anyone reading it will miss my argument that this is a book making grandiose but empty claims.

In the blog entry, there’s a part devoted to discussing in detail your claims about testability, there because it wouldn’t fit in the review. Besides that though, the blog entry is really about a different question than the review: why is an even emptier argument than the string theory landscape getting positive attention from the public and some scientists, part of an effective campaign that has already created a highly disturbing situation in theoretical physics? The background for this I’ve written about here ad nauseam, and that context should make it clear to my readers why I’m choosing to begin with a simple and blunt characterization rather than a more polite and indirect one. I don’t think you can really disagree that claims to have figured out the ultimate nature of reality are “grandiose”, and such claims with nothing solid behind them are the province of the crank (note that I think most every theorist feels the allure of this kind of thing, we all have our own inner crank…).

The non-scientific material is there because it’s in your book, and it’s relevant to the main topic of the blog posting, which is non-scientific: why is something that traditionally would be considered crackpot science now making inroads into conventional science? How is that being done? You’re unusual among talented, successful scientists in also having a great talent for getting public attention. You’ve had significant influence in getting people to take seriously highly dubious material about the multiverse. I’m fascinated by how that has happened, although of course my main concern is how to make it stop…

54. **Bernhard**
January 20, 2014

Max Tegmark,

I realize my argument does not really hold for Level I, but then you also lose the ability to “explain” things the cosmological constant. In any case, would be interesting to read your thoughts.

55. **Igor Khavkine**
January 20, 2014

I’m puzzled by the need to invoke inflation to give an example of a theory where there exist regions of a universe inaccessible to certain observers. That can only muddy the waters by the fact that many theoretical as well as observational aspects of inflation are yet to be fully fledged out. On the other hand, already in special relativity, for any single observer (meaning an event on a worldline, or even the whole half of the worldline leading up to an event), there exist spacelike separated regions. In fact, the same thing happens in any theory with a bounded speed of propagation of disturbances (information, or whatever one might call it). All other phenomena like event horizons, Cauchy horizons, cosmological horizons are extensions of this basic property. So, it seems to me that whatever philosophical difficulties are raised by inflation have already been raised by special relativity.

56. **Joel Rice**
January 20, 2014

I would be more inclined to look at what the Standard Model does not explain, namely why there are 3 generations of fermions, rather than indulging in rampant 'modal realism' or Platonism on steroids. Perhaps getting an answer to that would point to one mathematical structure 'all the way down' – to define, rather than merely describe.

57. **Max Tegmark**
January 20, 2014

Thanks Peter for these helpful clarifications!
I've long viewed you as someone who courageously stands up for a controversial view because you feel that it is correct, even thought you get a lot of flack from the physics community for it. This is something I have very much identified with over the years, since as you know, many of my views on physics have been just as controversial as yours, and as a result, both you and I have been called crackpots.

I hope you don't find this offensive, but I must confess that I find your recent postings disturbingly unscientific. You're asking the interesting question
"Why is something that traditionally would be considered crackpot science now making inroads into conventional science?",
so why aren't you considering all logically possible answers, including the possibility that they're making inroads because the supporting arguments are actually correct and new supporting evidence has come to light? After all, many currently accepted theories (e.g. relativity theory) were also considered crackpot science by some contemporary pundits. How can you be so certain that it's a good idea to "work to stop this" if you're not willing to even consider this possibility? Instead, you appear to dismiss this possibility from the get-go and focus only on other explanations such as the science community having become dysfunctional, me personally having dubious motives, etc.

I also find your posting style disturbingly unscientific, and can't help feel that you're applying a double-standard: you keep writing interesting, respectful and carefully balanced replies to me personally about how the Level I multiverse is a valid scientific discussion topic, etc., while at the same time writing pithy sound-bites to others suggesting that everything multiverse-related is unscientific nonsense. To me, one of the core principles of scientific integrity is to only say things that you're willing to stand by. For example, when I write an anonymous referee report, I like to pretend that I'm going to sign my name under it.

My main goal in this interesting conversation with you is to identify what our common ground is (a lot, it seems!) and where we disagree. My point of view is that we don't know whether any parallel universes exist or not, but that it's interesting to explore the possibilities. In contrast, you appear to feel that this is uninteresting, and I totally respect that viewpoint – so far, so good.
Moving on to scientific claims, I make several implication claims in the book of the form "if physics theory X is correct, then multiverse level Y exists". You still haven't told me about any physics claims of mine that you feel are incorrect, so unless you tell me otherwise, I'm going to assume you agree with these too. Please let me know if this is a fair characterization of your current views:

* Level I: you agree that it's a legitimate scientific topic

* Level II: you reject it because of your misgivings about the string landscape

* Level III: we haven't yet discussed this. Do you agree that unitary (collapse-free) quantum mechanics is a (possibly incorrect) scientific theory that implies Level III?

* Level IV: you find this meaningless, but haven't identified which of my arguments you consider fallacious.

Unless I've misunderstood you (and please correct me if I have!), this means that out of the 13 chapters in my book (http://mathematicaluniverse.org – CONTENTS tab), only four (6, 10-12) contain ideas that you feel are a waste of physicists' time, and your critique of them boils down to mainly to a lack of interest, not to me making incorrect statements in them.
I'm very much looking forward to hearing what you think!
🙂

58. **Peter Woit**
January 20, 2014

Max,

First about the questions about my views. Sure Level I multiverse theories can be part of legitimate science. In practice though, they are endlessly being abused by people who invoke evidence for them to argue for the string landscape. As Igor Khavkine points out above, the idea that the universe extends indefinitely to unobservable regions, with the same physics, isn't anything by itself new or revolutionary. My views on Level II are well-known.

About interpretations of QM in general, my view is that the basic laws of QM reflect a very deep mathematical insight into the way the universe works. At a fundamental level, I think that what we still don't understand very well is how classical mechanics emerges from this (with things like "quantum Darwinism" relevant ideas). "Many worlds" seems to me one legitimate way to characterize the overall picture one is trying to understand. It doesn't though seem very helpful in getting at what we don't understand. About the claims in your book relating Level I and Level III, I've made no comment, simply because I don't understand them, and the whole idea just seems too implausible to take seriously. I am committed to only commenting about that which I understand well enough to trust my arguments. An important aspect which I value of the culture of mathematics is an insistence on keeping straight what you understand and what you don't and always knowing where that boundary is.

On Level IV, yes, I think the arguments you're making often involve meaningless, ill-defined words and sentences, so one can't find a "fallacy". The only way to pin down such arguments is to look for non-trivial implications of them. I tried to be careful in the blog entry to identify the parts of your book that claimed such implications and to explain why I thought they were not justified.

On the other issues you raise, let me make it clear that I don't think your motives are "dubious". I'm sure you believe what you are arguing for and people have the right to do what they can to make the most effective arguments for what they believe in. On the other hand, yes I do think parts of the theoretical physics community have become dysfunctional, and my arguments about this are not based on dismissing thoughtlessly the reasons people are doing what they are doing, but upon paying close attention to the issues. By the way, at this point I don't think the claim that the string theory landscape is dysfunctional science is a controversial minority view of mine, but I'd guess that it's the majority view in the physics community (maybe even within the "string theory" community, depending how you define that).

As for the claim that I often make crude sound-bite arguments about these issues, I'll just say that I do the best I can to make accurate statements about what I'm well aware are complicated issues, given the constraints imposed by the format I'm writing in. I was happy with how the WSJ thing came out, partly because I ended up with enough space to make a serious argument and important distinctions. The first draft of that piece was 900 words, which only allowed a serious argument at the cost of so little background that readers got lost. The editor decided to allow a longer (1300 words) piece, which was enough to both give the bare bones of an argument and provide some background. I'm often though in the position of writing something much more constrained by limits of space (or my time), trying to say something in much less than 900 words. This is never going to capture all aspects of the question at hand.

About your book in general, sure, large parts of it are perfectly reasonable discussions of a variety of topics. The problem though is that from the title on, it is structured as an argument for a point of view that I think is seriously misguided, one that is not just a "waste of time", but is likely to further promote the most problematic trends in this subject. I've tried to make clear exactly what my arguments against this point of view are.

As an author of a book myself, I'm well aware that it's frustrating when people discuss only one aspect of it, ignoring all sorts of things one put into it. It's true that I'm just ignoring a lot of what's in the book, just because I have nothing very interesting to say about it. To give one example, when I first saw that you were going to discuss the "singularity", my knee-jerk reaction was "Oh no, another Kurzweilian technological optimist…", but when I read that part of the book I saw that I was wrong, you have a much more interesting take on that subject. So, you see, sometimes I can say something positive…

59. **Roger**
January 20, 2014

The analogy to relativity is weak. Relativity always had solid experimental support, with Michelson-Morley 1887 and relativistic mass experiments starting in 1902. There is no experimental support for the string landscape, many-worlds, or the math multiverse, nor is there likely to be any in the foreseeable future.

Max, you have labeled your chapters as being "mainstream" or "controversial". You should not be surprised that the criticism of your book has been centered on the chapters that you labeled controversial.

60. **srp**
January 20, 2014

Since Peter has stated that the Level I multiverse seems methodologically OK and even agreed that it follows from special relativity, do we then have to assent to the argument that there must be a huge number of slightly different copies of ourselves out there, etc.? In other words, is the Borgesian stuff a straightforward logical deduction from the existence of the Level I multiverse or not? And if it is, should we just shrug it off on the basis of its direct unobservability?

61. **Peter Woit**
January 20, 2014

srp,
I don't think we have significant evidence for an infinitely extended universe (or significant evidence against it), just agree that this is a question one can sensibly hope to address by studying various cosmological models and comparing them to experiment. Personally, I've never been too interested in the paradoxes that come up when you assume infinite extension, but can see why some people are. To each his or her own…

62. **jd**
January 21, 2014

Mr. Tegmark

Since I and many of my colleagues have studied and arrived at many positions similar to those of Mr. Woit, then the conclusion is that you consider our views "controversial" and that some in the community would believe us to be "crackpots." Perhaps even you? You do not help your cause Sir with such a stand. And in truth you cannot support your position from what is being discussed in the community; I know many in the field of HEP, I have many contacts, I am at a large national lab. Also, I know what is being written. I further resent the insult you make to my intelligence by assuming that I am so naive as to swallow your illogical statements. And your weak attempt to equate yourself with Einstein is obvious when you also characterized general relativity as crackpot to some. After all Hilbert also published the field equations and I sincerely doubt that any reputable scientist would consider the both of them crackpots. Some here have said you are a nice guy. I have never met you and I wonder what motivates you. There is more that could be said. For example "why aren't you considering all logically possible answers?" To the best of my ability I am considering all my own answers and those I see in the literature. Given what supporting evidence there is, I find some answers to be nonsense and I will not waste my career on them. Life is too short and there is good work to be done. Enough already.

63. **Peter Woit**
January 21, 2014

jd,
I think Max is correct to claim there are some in the physics community who think my views on string theory are "crackpot". Lubos Motl is the most prominent exponent of this view….

64. **George Ellis**
January 21, 2014

Peter,

"why is something that traditionally would be considered crackpot science now making inroads into conventional science? "

See "Physics on the Fringe: Smoke Rings, Circlons, and Alternative Theories of Everything" by Margaret Wertheim (Walker, 2011) [http://physicsonthefringe.com/] for an account of a quantum cosmology meeting at Santa Barbara where conventional scientific constraints on theories were thrown to the wind. The discussions were very similar in atmosphere to those at crank science meetings.

65. **ScentOfViolets**
January 21, 2014

There is very little — if anything — new in Tegmark's noodlings. Greg Egan famously used the 'dust hypothesis' in his *Permutation City*, and in the linked FAQ, Egan points out that his dust is almost identical to Moravec's *Simulation, Consciousness, Existence* paper.

The bottom line? 'Math is everything' is fun as a science fictional device, but there's very little 'there' there. This will continue to be the case until there's some way to tell theories of this type are wrong . . . and you can can get an experimentalist to test for what happens in the requisite setup. Sorry, Max, but science is, above all, a very practical sort of enterprise. Metaphysics need not apply.

66. **Mathematician**
January 22, 2014

MUH? Meh!

67. **OMF**
January 22, 2014

I'd just like to say that it's a little annoying to find that I have to rupture my hump making sure my calculations and research are rigorously fit enough to publish while others can actually make an entire career out of this stuff.

tl;dr bitter-vet is bitter.

68. **C Wright**
January 22, 2014

"The Universe is made of Math"

How might one begin to investigate such a claim?
Let's assume this "new" hypothesis is true and ponder a boundary condition for quick insight. One reasonable boundary condition might be at the onset of the "Big bang." We have T = 0, E >>0, and I suspect, more than a few other initial condition parameters (please feel free to mentally provide). What's the status of mathematics at this conjecture? If mathematics is non existent at this point in time, but comes into being only after T=0, then it cannot be our hypothetical constructor. Therefore, let's assume some mathematics exists at or before T=0. How much mathematics? We surely need enough to define all the universe's initial conditions. This will take more than a little mathematics – certainly more than all that is known today. Restating then, at the beginning of the universe when T=0, a lot of mathematics (maybe all of it) exists. And where does mathematics exist/reside? Don't tell me, there is no place in the universe just yet, so let's define an arbitrary place for it. Will you go for the "Great Omnipotent Depository?" (Something sounds a bit familiar with this concept.) With all this mathematics existing before the universe began, and since the "universe is made of math," I see we can also conclude that a virtual universe also exists at T=0. WTF, from our original hypothesis, we can conclude that the universe virtually existed before the universe began.

Hmm, I'm sensing more salesmanship than science. Thank you, but I'll likely pass on this book.

69. **Max Tegmark**
January 22, 2014

Dear Peter: although I'm grateful for you answering more of my questions, I'm surprised that you didn't answer the main one!
I'm sorry if I didn't ask it clearly enough – please let me ask it more explicitly. You're asking the interesting question
"Why is something that traditionally would be considered crackpot science now making inroads into conventional science?".
There are of course many possible explanations, including
1) The physics community is becoming increasingly dysfunctional,
2) A "crank" and "impresario" named Max Tegmark with a "taste for grandiose nonsense" is corrupting the physics community,
3) Money from the John Templeton Foundation is corrupting the physics community,
4) They're making inroads because the supporting arguments are correct and new supporting evidence has come to light.

Your posts above explore options 1), 2) and 3), but isn't the scientific approach to explore all possibilities, including 4)?
When I talk to physicists other than you, on both sides of the multiverse debate, they routinely mention three explanations in category 4:

a) Observations of the cosmic microwave background by the Planck satellite etc. have make some scientists take cosmological inflation more seriously, and inflation in turn generically predicts (according to the work of Vilenkin, Linde and others) a Level I multiverse.

b) Steven Weinberg's use of the Level II multiverse to predict dark energy with roughly the correct density before it was observed and awarded the Nobel prize has made some scientists take Level II more seriously.

c) Experimental demonstration that the collapse-free Schrödinger equation applies to ever larger quantum systems appears to have made some scientists take the Level III multiverse more seriously.

Is it really completely obvious that these people are all deluded and that none of these three developments have any bearing on your question?
I can't help feeling disturbed by similarities between your posts and the recent hate-mail I've been receiving from a Young-Earth Creationist: you both seem to start by assuming that your conclusion is true ("Earth is 6000 years old"/"Multiverse ideas are nonsense"), and simply avoid mentioning any evidence to the contrary. If I stop posting on your blog, it will be because your approach is too unscientific for my taste.

70. **Tom**
January 22, 2014

Paul Steinhardt has quite a distaste for the "anything goes" multiverse, eg read here:
http://www.edge.org/response-detail/25405

Does Steinhardt's "Cyclic" hypothesis, an alternative to "conventional" inflationary Big Bang theory, somehow dispense with the Level 1 and other multiverses being discussed here?

71. **Peter Woit**
January 22, 2014

Max,

About the 3 strongest examples of "experimental evidence" for the multiverse you mention.

First "c". This is kind of ridiculous. I've never heard of anyone expecting QM to fail for such larger systems, so the evidence that it doesn't can't possibly have surprised anyone or changed their mind about anything. If it did fail, that would be a huge surprise and would change people's attitudes dramatically. This is also irrelevant to any of my arguments since I don't have anything in particular against many-worlds interpretations (although I also don't think they get at the interesting questions). On the other hand, if you have any evidence for your cosmological interpretation of QM that would be different, but I saw none in your book.

About "a". Again, I'm not arguing against Level I multiverses. On the question of eternal inflation, from what I can tell we still have little to no relevant experimental evidence, although I freely admit to not being an expert on this. Since you are one, here's a question: later this year Planck will release B-mode polarization results. What does eternal inflation say about this? If Planck sees nothing, will that be evidence against inflation and people will start having less faith in an eternal inflation scenario and thus such a multiverse? If this is a subject with real connection to experiment, what's the prediction here?

About "b". Obviously I'm well aware of the Weinberg argument about the CC (it's basically the only one anyone ever brings up for a Level II multiverse). Sure, I agree that that argument and the observed value of the CC have had an influence. I don't happen to think it's particularly strong evidence, but, sure, it's at least something. No, I don't argue that anyone interested in Level II multiverse theories is a fool with no reason to be thinking about this.

As for the three explanations you have me making for interest in the Level II multiverse (as opposed to your fourth claimed correct one that it is due to increased support from experiment and better theoretical understanding), first of all, you're ignoring the main one I am making: people who have a lot invested in string theory refusing to admit their theory has turned out to be an empty failure. Do you honestly claim this is not a major contributor to interest in the Level II multiverse?

Of the three explanations you do assign to me, about Templeton I do think their money has had some effect, I don't know how much. Maybe increased interest in the multiverse in the last ten years was not affected at all by things they financed, like the 2003 "Universe or Multiverse" Stanford conference and the book that came of it. I suspect they think their money has an effect or they wouldn't have kept spending it. I don't think it's deniable that, at least in 2003, the NSF was not about to finance such a conference. In an alternate part of the multiverse where John Templeton died a pauper, I think it's fair to suspect there might be slightly less interest in the multiverse.

About your influence, again I'd find it hard to quantify, but it's non-zero. Recall that no less a source than the Financial Times tells us "Today multiple universes are scientifically respectable, thanks to the work of Tegmark as much as anyone." As for terminology, applying "impresario" to your FQXi activities and others seems to me rather accurate. To be accurate, I referred to you not as a "crank", but as a scientist who, with this book and the Level IV business in general, was "indulging his inner crank". Do you honestly have no idea what your colleagues think about this kind of thing? How many do you think believe the Level IV business is anything other than empty grandiose nonsense of a characteristically crank or crackpot sort?

As for the comparison of me to a Young-Earth creationist spewing hate-mail, get a grip.

72. **Neil**
January 23, 2014

Thank you Peter and Max for an enlightening exchange.

One comment on Templeton and the multiverse, if I may. As I understand it, maybe I am wrong, Templeton is interested in financing research that links science to god, or as they say in their mission statement, the "spiritual dimension." The multiverse is used by materialists to explain fine tuning and other existential "mysteries" as an alternative to god. I don't think Templeton has any great interest in promoting the multiverse.

73. **Jusnem**
January 23, 2014

This has been a very spirited and interesting debate. I would like to propose a compromise which I believe both sides can agree on. The mathematical universe hypothesis is a religious principle.
–
There seem to be two options to explain the existence of our universe in light of the fine tuning required for us to be here: (1) God made it that way, or (2) all mathematically possible structures exist. If there is a third option, then Mr Woit should be able to identify a specific flaw in Mr. Tegmark's reasoning. So far, I haven't seen one.
–
Mr. Tegmark's proposal provides a logical foundation for the religion of atheism. For any physicist with faith in determinism (and how can we have physics without that faith?) Mr. Tegmark's conclusion is unavoidable. With that said, I personally can't imagine any scenario where this hypothesis would be falsifiable. Of course, that should not detract from its value for inspiration and clear thinking.
–
Mr. Woit's criticism is based not on a critique of Mr. Tegmark's logic, but rather on a religious belief in God, free will, or a more traditional notion of our world. I don't see how this opposition can be reconciled with the basic assumptions that external reality exists and that physics can describe it, but it too is based on sound religious notions.
–
Of course, if this debate is really about who should get funding for what then these comments don't really add anything to the debate so feel free to ignore them.

74. **Dom**
January 23, 2014

Max – to be fair as much as I don't like the Lubos Motl type of language e.g. "crank", I was clear that Peter was referring to particular ideas rather than you as a person. I don't personally see the harm in putting forward any idea however outlandish as long as we do not give the lay person (or even the reasonably well-informed person) the idea that what we are saying is experimentally confirmed fact.
I have found what you have to say very interesting and as someone elsewhere said, you are at your most impressive when you ignore the stuff other people would take personally (hard to do, easy to admire).

75. **Peter Woit**
January 23, 2014

Neil,
Templeton's agenda is more interesting and subtle than just linking science and religion, and promoting religion (although they do plenty of that). If you look at what they don't support, they're explicit about not supporting experiment, and they pretty much avoid serious mathematics. One could say they like to support "philosophy", and their "philosophy of cosmology" initiative is part of this. I don't think they have a side pro or con on the multiverse, but it definitely is a topic they like to support discussions of. All in all, they like to support ideas and work that the scientific community doesn't normally support because it seems rather empty and unlikely to be fruitful. Some of this ends up supporting actually interesting work, some is just a waste of time, and, yes, going on about religion and science fits nicely into this.

One way of describing the effect they're going for (and to some extent achieving) is to move topics like "universe or multiverse" from the category where the consensus is "empty waste of time, best ignored" to "controversial" (i.e. worthy of debate).

76. **Lee Smolin**
January 23, 2014

Dear Peter and Max,

I was going to stay out of this debate, having in print already two books that explain why anthropic multiverse cosmologies cannot possibly yield falsifiable predictions. These books, Life of the Cosmos and Time Reborn, also put forth also an alternative program for cosmology that does yield falsifiable predictions, based on the hypothesis that laws evolve in time.

But I don't want to let pass Max's claim that Weinberg's prediction of the order of magnitude of the dark energy provides evidence for a multiverse. Weinberg's prediction happened to turn out roughly right but the argument that that provides evidence for the multiverse is based on fallacious reasoning. I explained why on page 136 of Time Reborn, from which I quote:

"One problem with that conclusion is that the critical value referred to is the one above which galaxies would not form if the cosmological constant were the only parameter that varied. But theories of the early universe have other parameters that can vary. If we vary some of those while we vary the cosmological constant, the argument loses its force.

Let's look at one case, in which we vary the size of the density fluctuations, which, as we discussed earlier in this chapter, determine how evenly the matter in the early universe was distributed. These are relevant because if they were bigger, the cosmological constant could be far above the critical value and galaxies would still form in the very dense regions created by the fluctuations. There is still a critical value for the cosmological constant, but it goes up as the size of the density fluctuations goes up.

So you can rerun the argument, letting the cosmological constant and the fluctuation size both vary over the population of universes. Now you pull two numbers out of the hat for each universe, one for the cosmological constant, the second for the size of the density fluctuations. We choose these numbers randomly, within the range in which galaxies form. It turns out that the probability of randomly getting both numbers to be as small as they are observed to be is now down from 1 chance in 20 to a few parts in 100,000.

The problem is that because we don't observe any other universes, it is impossible to know which constants vary over the hypothetical multiverse. If we assume that the right story is that only the cosmolog- ical constant can vary over the multiverse, Weinberg's argument does well. If we assume that the right story is instead that both the cosmological constant and the fluctuation size vary, the argument does less
well. In the absence of any independent evidence as to which, if any, of these hypotheses are true, the argument leads to no conclusion.

So the claim that Weinberg's argument correctly predicted the rough value of the cosmological constant fails, because of a subtler fallacy than the one discussed above. This fallacy, which is known to specialists in probability theory, arises whenever you take advantage of the freedom to arbitrarily choose a probability distribution that describes unobservable entities and so cannot be checked independently. Weinberg's original argument has no logical force, because you could reach a different conclusion by making a different assumption about unobservable entities."

(there are citations to the scientific literature in the text.)

Max, do you have a response to this or do you agree that Weinberg's argument offers no evidence for a multiverse?

Thanks,

Lee

77. **Peter Woit**
January 23, 2014

Thanks Lee,
It did occur to me after writing that comment about this that referring to your arguments about this would have been a good idea.

Another thing I could have added was the following simple point I often tried to make in arguments about this way back when. The Level II Multiverse theory of the CC is effectively the same as my own personal theory of the CC, which is that I have absolutely no idea what is responsible for its value. So, hey, no reason to think any particular value is more or less likely. I think the implications of the Level II Multiverse theory and my theory of the CC thus should be the same: from nothing you get nothing.

78. **Bernhard**
January 23, 2014

"But theories of the early universe have other parameters that can vary."

I was under the impression that in these multiverse theories one was allowed to do anything. In the book "Universe of Multiverse" ()http://books.google.se/books?id=U_Jm2DT_AVAC&printsec=frontcover#v=onepage&q&f=false on part II, when Craig J. Hogan enters with particle physics he is talking about changes in the Yukawa couplings – which by itself, could give you almost anything given enough creativity. I suppose the multiverse should even make SUSY irrelevant since it can also solve the hierarchy problem.

79. **Daniel Miller**
January 23, 2014

Hi Peter, Max and Lee,

First, I have read both Our Mathematical Universe and Time Reborn and enjoyed them both very much. I view them as popular novels appealing to a wider audience that presents both non-controversial as well as controversial ideas. I see no problem with presenting or even pushing controversial ideas provided there is no attempt to deceive. Neither book was deceptive, so there is no issue. Furthermore, these controversial ideas are often exciting and effective in attracting a wider and uninitiated audience to the subject.

Second, I appreciate the obvious importance in questioning the falsifiability of theories, as long as this process itself is carried out scientifically – lest you become a hypocrite. What I see absolutely no value in is attacking and dismissing admittedly controversial ideas outright simply because you find them exceedingly peculiar or fear their potential effects on the direction of the field, provided of course that they have some merit – which is for the field to decide. Indeed, I can easily imagine negative effects resulting from these kinds of (unscientific) attacks being much more plausible than some critical number of students being misled into studying "empty" theories and this resulting in some crisis of the field – this would require severe misjudgment on a large scale in a manner that is not compatible with the typical student of physics. In short, it's a needless concern and appears to me to be a cop-out for more legitimate forms of dismissal.

Third, radical theories, both good and bad, must often be "sold" if they are to find wide-spread acceptance – it just follows logically from the nature of revolutionary ideas and is supported by the history of physics.

Finally, being a PhD student in physics I can tell you that it wasn't very-well founded, falsifiable ideas that got me interested in physics. It was reading Thorne's "Black holes and time warps" and Kaku's "Hyperspace" as a child, which then led to Bertrand Russell's "ABC of Relativity" and more "non-controversial" material.

Finally, finally – REAL crises do exist such as global warming and nuclear proliferation that scientists are going to need to get comfortable speaking about with some emotion and zeal that they are not characteristically known for – hell, it would even be great if we had a few "academic celebrities" – because if we don't sell these issues to the public then there won't be any meaningful landscape left to falsify.

80. **Peter Woit**
January 23, 2014

Daniel Miller,

The "Mathematical Universe Hypothesis" and Level IV multiverse of Tegmark's book is not "controversial". As far as I can tell, no serious scientist other than him thinks these are non-empty ideas. There is a controversy over the string theory landscape, but none here. These ideas are also not "radical", they are content-free.

You refer to Tegmark's book as a "novel", and then you expect it to inspire young people about science, and convince the public to take scientist's warnings about global warming seriously. This makes no sense. What will inspire smart young people to be scientists is good science, not obviously empty claims. I don't think this book helps the credibility of scientists with the public one bit, quite the opposite. Yes, scientists sometimes need to sell their work to the public. But to do this, they have to have something of value to sell.

81. **Tim May**
January 23, 2014

I was at Max Tegmark's talk last night in Santa Cruz.

I made a joke to him about our weather vs. East Coast weather during the setting-up period, but had no chance to talk to him during the now-obligatory book-signing period, which appeared to be about 30-50 people long. (I bought the book via Amazon and it arrive on the day of official pub, the 17th).

He said little about multiple universes. I thought his talk was very good at what I'll call the "George Gamow, One, Two, Three Infinity" level. Understand, this was a book that mightily influenced me in the 60s when I was about 12.

(To my left in the crowd was a woman with a young boy. Tegmark spoke directly to a few of the young kids (boys) in the audience. This boy said he was 9. Tegmark asked him about his interests. "Chemistry" was not terribly surprising. But he also asked about some sums, and the kid responded correctly. And he sat during the presentation paying rapt attention, apparently. (One has to be careful about observing others, the Heisenberg Pedophelia Principle.)

So, Tegmark did a nice prevention about the frontiers of astronomy (the Hubble Deep-Field), some mentions of some stuff possibly beyond, but he did not discuss the controversial aspects of his MUH theory.

(As we were dispersing, I hear one very famous UCSC professor saying "Well, it was good that there were no questions about alternate universes." I overheard him and quipped "Don't worry, in 10 to the 700 other universes they asked questions.")

My feeling is that Tegmark's lecture, and his book, which I have skimmed since receiving it last week, are not terrible things for people to read.

I grew up on reading Gamow, Asimov, etc. on popular science, plus the then-excellent Scientific American. The weird stuff about monopoles, tachyons, etc. was out there, but one developed a good idea of what was plausible and what was longer-term, somewhat implausible.

As a long-time reader of Peter Woit's blog, and his book, but as someone who also reads Lubo Motl's blog, I think Tegmark's books is a fairly good popularization of some background in physics and cosmology and and an introduction to the more outré aspects.

(Sorry to be personal, but string theory never grabbed me. But the recent EPR = ER stuff really, really grabs be. Doesn't make it right. But it sure is suggestive.)

–Tim May

82. **Daniel Miller**
January 24, 2014

Hi Peter,

Thanks for responding – this comment thread got blown up! Aside from addressing the validity of an idea, which is clearly totally worthwhile and necessary, I was trying to speak to other separate issues which I will try to be a bit more clear about.

1) The concern that MUH will lead the field astray and waste time that could be better spent elsewhere: I was trying to say that I think this is unlikely. A similar argument has been made about string theory and the landscape, yet physics appears to not have imploded yet nor does it appear to be approaching a crisis. I'm not saying that MUH has the legitimacy or establishment that string theory has found, nor am I saying it is without merit. Nor had Max even remotely tried to claim this is an accepted mainstream idea. And this will all be clear to anyone serious about physics. So this entire concern is unwarranted.

2) Young people may be inspired toward science by ideas that stir their curiosity and which they find intellectually fascinating. This can be "good science", science fiction, or anything in between. But above all it must be interesting. What will make them "good scientists" is the entire formal process required to become a scientist. I have more than a few friends who were originally inspired to pursue science from Star Wars.

3) There is nothing inherently wrong with "grandiose" ideas, being an "academic celebrity" or selling one's ideas. In fact, these qualities are currently needed more among scientists to appeal to the public about pressing social issues. This was a statement about the virtue of these traits alone, which were mentioned somewhat derogatorily in previous posts, and not with respect to the book or the books role in promoting awareness about global warming. And, unfortunately, it is not the credibility of scientists that pushes part of the population toward climate change denial – it's the loudness, visibility and grandiosity of (totally incredible) pundits.

The overall point being that both Max and his more fun ideas have a net positive effect on both science and the world we live in.

Have a goodnight,

Dan

83. **Peter Lynds**
January 24, 2014

Hi Lee,

I can't help but question why you felt the need to mention about your own work and its ability to make falsifiable predictions. I think this is maybe a little bit rich, considering that charges of empty content and the rest could easily be pointed in your direction with regards to your recent book and arguments and claims concerning the reality of time. That is, time and the empirical don't, and will never, go together.

Peter

84. **Lee Smolin**
January 24, 2014

Dear Peter Lynds,

My logic, as was carefully explained in Time Reborn (TR), takes three steps, which in outline are:

1) The various arguments which have been given that time is an illusion, i.e. emergent from a more fundamental timeless level of description, depends on an assumption, which is the immutability of the laws of physics. This is one of the conclusions of Part I of TR.

2) Specific hypotheses about how the laws evolved are testible by real,doable experiments and observations, a few are even falsifiable.

3) Therefor, if a hypothesis that laws evolve were confirmed by evidence, the laws cannot be immutable, therefor the arguments that have convinced physicists that time is unreal would have no force.

So there is a relationship between the view of time and empiricaly testible hypotheses.

Furthermore, the hypothesis of cosmological natural selection, published in 1992, made two falsifiable predictions which continue to stand up to empirical test. It also remains the only proposed explanation for the fine tunings of the standard model that makes falsifiable predictions.

Thanks,

Lee

85. **Marcus**
January 24, 2014

IMO that 3 step argument must surely be the most interesting thing that has appeared in this thread and contains what could be the REAL reason that the natural world appears mathematical.
Since Max T. seems to like mathematical regularities/patterns, this argument, I suspect, is what the second half of his MUH book should actually have been about:

Leibniz principle of sufficient reason (for the manifest regularity Wigner referred to) implies TIME = a process by which regularities emerge and become "laws"

Thermodynamics of physical law. The universe becomes more predictable as time goes on. More "mathematical" more patterned, more regulated, more repetitive. Paradoxically beautiful and from another perspective boring: more expected and less surprising.
Like a Shannon channel whose information capacity is gradually diminishing until the listener at the other end hears nothing, or nothing he didn't already know.

So this three step argument shows us the real MUH. And there is only ONE universe in this MUH. It is unnecessary, old-fashioned, and ridiculous to imagine more than one.

Leibniz+Wigner –>time, and time is "testable" in the sense that we might someday witness the emergence of an unprecedented regular pattern.

Extremely farfetched, but still nice.

The first half of Tegmark's book, from what I have sampled, is pedagogically excellent. It reminds me of Timothy Ferris' *Coming of Age in the Milky Way*, really A-plus. Maybe it is such a good front end that it should have a different back end. :0)

86. **Peter Woit**
January 24, 2014

All,
This is turning into a discussion of another book…

Please, comments should be about the Tegmark book, and at this point coming up with something new about that might not be easy.

I promise a couple new postings soon.

87. **Peter Lynds**
January 24, 2014

Hi Lee,

Thanks. There is more I'd like to say than this, but I agree that it would be off topic. If I can, though, I would like to quickly mention that, unless one is very selective, arguments for time's non existence don't depend on the assumption that the laws of physics are immutable (I can think of plenty that aren't, including simply that time is unobservable). The thesis that the laws of physics should be mutable and internal to the universe (I don't think there can be any doubt that the latter is correct) also obviously isn't dependent on time existing.

While I disagree with his ideas, I think Max deserves credit for participating in this discussion. Hopefully he'll come back. If he does, I think some sensitivity to his position (maybe a bit like an English soccer supporter walking into a rival team's local bar) wouldn't go amiss.

Peter

88. **Roger**
January 24, 2014

I was also at that Santa Cruz bookstore lecture. I was surprised that he peppered his talk with arguments that we are not spending enough money and effort trying to reduce risk of future disasters. His math multiverse implies that time is an illusion, that we have no free will, and that all future scenarios happen, regardless of what we might try to prevent.

89. **Max Tegmark**
January 26, 2014

Thanks Lee for bringing up this important point!

> Max, do you have a response to this or do you agree that Weinberg's argument offers
> no evidence for a multiverse?
I fully agree with the mathematics in your analysis. In fact, I reached a similar conclusion in http://arxiv.org/pdf/astro-ph/0410281v2.pdf (see around 81), and so does Alex Vilenkin. The bottom line is that galaxy formation efficiency depends not on the dark energy density alone, but on what we might call the dimensionless "Weinberg" parameter W=rhoL/Q^3 xi^4, where Q ~ 2e-5 is the CMB fluctuation level, rhoL ~1e-123 is the dark energy density and xi~2e-28 is the dark matter density per photon, all

in Planck units. What Weinberg's argument gives is then a prediction for the parameter W, no more and no less. Vilenkin has emphatically argued that Weinberg's prediction for W is impressive because it predated its measurement (by observations of CMB, supernovae, etc).

On a separate note, since you too got picked on above for making controversial statements, I want to add that I find your work extremely valuable, particularly because your conclusions are so different from mine. Whenever we face a science question to which we don't yet know the answer, I find it valuable when the community carefully explores the full range of logical possibilities. On some issues related to the roles of time and mathematics in physics, your work and mine in a sense explores the opposite extremes of the spectrum of possibilities.
In contrast, I find conformist pressures to lampoon and dismiss certain scientific topics to be against the spirit of science. Indeed, it's been disturbing to see that the loudest cheerleading for this particular blog thread has been in the Intelligent Design community:
http://www.uncommondescent.com/intelligent-design/mathematician-wonders-about-the-respectful-reception-new-multiverse-book-is-getting/?utm_source=feedburner&utm_medium=feed&utm_campaign=Feed%3A+uncommondescent%2FJCWn+(Uncommon+Descent)

90. **Peter Woit**
January 26, 2014

Max,
There is no "controversy" about your MUH and Level IV arguments, just a consensus that they are empty, and that's what you need to address.

I notice that you have decided to stop answering any of the arguments I raise, in favor of a sleazy tactic of slandering me as a creationist. Up to you how to behave, I don't think you'll find this helps you or your case.

91. **John**
January 26, 2014

Don't you think that little bit of insinuation is beneath you Max? After all, what the hell does the opinion of the Intelligent Design community have to do with this.

92. **Orin**
January 26, 2014

Peter, it's not fair to ask Max (or anyone) to address a claim that the MUH is "empty" if you don't define what that means. Do you mean only that you think it is unfalsifiable? Or do you believe that it is "empty" philosophically as well? You keep saying it over and over, but nowhere in this thread or in your original post do you present an argument for anyone to rebut. One might say your criticism is "empty." Some in this thread have presented examples of how the MUH may turn out to be predictive. You completely ignore these arguments (sometimes removing them from the comment section) and just keep shouting "empty!" without addressing their points. Even your reaction to the recent discussion of the Weinberg argument ("I don't happen to think it's particularly strong evidence, but, sure, it's at least something.") is perplexing in the context of your statement that the MUH is "completely empty". Completely? You just admitted that the L2 multiverse has perhaps some weak predictive content! So you really need to be clear about exactly what you mean by "completely empty" before asking others to defend against an attack that is itself "completely empty" in its current form.

93. **Max Tegmark**
January 26, 2014

Hi John: Peter Woit's latest accusation that I'm somehow claiming that he's a creationist is so silly that it hardly warrants a reply. Please let me repeat and expand on what I wrote above, in the hope that it won't be further misunderstood.

I wrote that I couldn't help feeling disturbed by similarities in argumentation style between Peter's posts and the recent hate-mail I've been receiving from a Young-Earth Creationist: both seem to start by assuming that their conclusion is true ("Earth is 6000 years old"/"Multiverse ideas are nonsense"), and they then proceed to simply avoid mentioning any evidence to the contrary. I gave a detailed example of how Peter did this in my January 22, 2014 at 6:44 pm post above, by avoiding any examination of option 4. To me, this is an unscientific approach. As far as I can tell, the posters on that creationist site I linked to appear to dislike my book because they feel it represents a naturalistic world view. Again, they don't appear interested in examining all logically possible options (in particular, the logically possible option that modern cosmology is correct), instead dismissing this with pithy quotes about "nonsense", "crank" etc. that they've borrowed from Peter Woit. If we scientists are to have any claim to the moral high ground in scientific debates, I feel that we need to practice what we preach and conduct our debates at a higher level of civility and rigor!

94. **Peter Woit**
January 26, 2014

Max,
While pointing to your January 22 comment, you studiously ignore my response to it, following up your attack on me as similar to a hate-mail-sending Creationist with a sleazy comment about my supposed connection to a "disturbing" ID blog. You then end with "I feel that we need to practice what we preach and conduct our debates at a higher level of civility and rigor!". Impressive.

By the way, I just noticed that you're featured in an upcoming film, The Principle. Have you seen it, and if so do you think you do a good job of representing the scientific viewpoint in this film?

95. **Peter Woit**
January 26, 2014

Orin,

In this context, "empty" = "implies nothing about the real world"

I devoted a fair amount of time to carefully reading the Tegmark book and looking for where he discussed the implications for physics. In the blog entry above I discussed those carefully and completely, giving an argument (that no one has contradicted) that Tegmark was unable to derive and real implications of his MUH or Level IV multiverse for physics. This has nothing to do with the argument about Level II predictions.

96. **Orin**
January 26, 2014

Peter, then I suggest you add the adverb "currently" before your use of the word "empty." Even more fair would be "currently not predictive." There is a big difference between being vacuous (which is what "empty" may imply) , and simply being a theory that may or may not yield testable predictions at this time. The argument you need to be making then, is why you are so sure that this theory has no possibility whatsoever of ever yielding testable predictions. I have not seen this argument from you, and I think it is an argument that needs to be made if you really believe that the MUH is not worthy of further study. I have a hard time believing that you could really hold such a hard line, when it is so obvious (from arguments like Weinberg's discussed above) that there really can be no possibility of predictive consequences of such a theory (the Level II apropos of Winberg is not level IV, but in theory the logic is no different).

97. **Peter Woit**
January 26, 2014

Orin,
The problem is not one of "currently untestable", but one of "in principle untestable". Tegmark is the one who wrote scientific papers and a book about this, he's the one responsible for explaining why the ideas are not vacuous, by giving a plausible explanation for how his ideas could be tested, at least in principle. The burden of proof is not on me, but on him. I've argued above carefully above that he hasn't met this burden (although he claims in his book that he has).

If you follow the discussion between us in these comments, I think you'll see that the point at which he starts going on about hate-filled creationists is the point where he no longer has an argument. That's what people do…

98. **Orin**
January 26, 2014

Peter, you well know that an idea exists independently of a promoter of that idea. To insist that you will only accept an argument from Max and only Max is a bizarre form of straw man. There have been examples given in this thread of how the MUH is not necessarily vacuous. You have ignored them.

99. **Peter Woit**
January 26, 2014

Orin,
You seem to find it plausible that Tegmark wrote a whole book about his "hypothesis", but didn't bother to include in the book an explanation of how his "hypothesis" could be tested. Now, before I can argue that he doesn't have anything, it's my job to prove that there's no way for anyone to come up with tests for his "hypothesis". Right? Sorry, we disagree, I think my job is to read the book, assume he has put his best arguments in there, and see if these arguments are convincing, or if they don't hold water. I think any scientist who reads the book will see that the latter is what is going on here.

By the way, have you read the book? On which page did you see a convincing argument from him that his "hypothesis" was testable?

100. **Orin**
January 26, 2014

Peter, I'm afraid I haven't had the time to read the book yet, but I'm familiar with the material I expect to be in it. Hopefully I'll get to it next week. I will admit that if what you say is correct then I am puzzled that Max would not include in his book the interesting ways in which the MUH can lead to physical predictions. Of course I know well from your past opinions that you don't take kindly to some of the tools that would be brought to bare, such as anthropic reasoning(*). Nonetheless such reasoning is not vacuous, it exists independently of whether or not Max puts it in his book, and you are surely aware of it, so it seems disingenuous to take such a hard line as "vacuous" with regard to these ideas. It would be refreshing if you admitted that in principle these ideas could yield fruit. Could they not?

(*) I should note that this is by no means the only reasoning; it may not even be necessary if a sensible measure is found to be predictive.

101. **Chris W.**
January 27, 2014

In this context it might be a good idea to read (or re-read) Peter Medawar's review of *The Phenomenon of Man*.

102. **CJ**
January 27, 2014

Dear Max, it is your behavior that is having the chilling effects not only on this blog, but throughout the entire physics community.

It is quite remarkable that while you are the one with millions upon millions of dollars at your disposal, a professional publicity machine, MIT's PR department, and legions of Ph.D.-free pop-sci-fanboys, you accuse Peter of being a "creationist" bully for merely reading your book and reflecting on its empty content as a lone individual. To pile irony upon irony, it is also remarkable that while your book does little more than promote a faith-based initiative, which is not testable science, you then have the gall to accuse scientists and objective writers of behaving like religious fanatics.

Max Tegmark's biggest defender Orin writes, "Peter, I'm afraid I haven't had the time to read the book yet, but I'm familiar with the material I expect to be in it."

Max, you do realize that your career is built more on laymen who have not read your book, than on scientists who have?

103. **Orin**
January 27, 2014

Well Peter, this "Ph.D.-free pop-sci-fanboy" has given your blog a fair shake. Specimens in irony like the above only do you a disservice by lending credence to Max's comparison. Enjoy your echo chamber.

104. **Peter Woit**
January 27, 2014

Orin,
I hope once you do get around to reading the book under discussion, if you find a non-vacuous argument in it for the MUH or the Level IV multiverse, you return to let me know.

105. **Shantanu**
January 27, 2014

Max (or anyone else an expert on inflation)
Is there a lower limit on r from inflation? It seems that there are so many models of inflation (for example this ~ 400 page paper on all models of inflation http://arxiv.org/abs/1303.3787)
that given any observations (or non-observations) one can always construct any model of inflation.

106. **Pete**
January 27, 2014

Regardless of whether or not the MUH is true or not, I was wondering about your view regarding the idea of platonism/mathematical realism in general Pete. I know that a vast majority of mathematicians and a good deal of physicists would adopt a view that mathematical structures and truths are independent of human beings and that mathematical propositions are objectively true/false.

I must admit I think this view is very plausible, considering the history of mathematics and the sciences (especially fundamental physics) and the influence both disciplines have had on each other. This is not to try and bring in mysticism at all, as I am a naturalist and a physicalist, thought the second part gets increasingly harder to penetrate as you decompose "matter" into a collection of atoms that are 99.999% empty space with particles in a nucleus that decompose into further elemental particles represented as mathematical points or "vibrating strands of energy" in String Theory, whatever the hell that would even mean physically.

I mean, when modern physics points to fact that solid, physical matter is in fact vast amount of empty space linked together by interactions of profoundly small particles than seem to have an ephemeral existence all their own, is Platonism or realism about abstract structures and mathematical relations underlying the physical world really so outlandish? I agree it could probably never be tested, making it more philosophical than empirical, but do you think it a reasonable view?

By the way Frenkel's book, Love and Math, is brilliant so far and its clear from the reading that he, along with a long list of other mathematicians, wholeheartedly embraces the Platonic view without the slightest bit of crackpot "mysticism."

107. **Peter Woit**
January 27, 2014

Pete,
I myself favor some form of "Platonism" or realism about mathematics (see a following posting, which has a link to something about the "Putnam-Quine indispensability thesis" and I think Quine had some of the most to the point things to say about how to think about what is "real"). The problem is the claim that this tells you anything you didn't already know about physics, in particular Tegmark's claim that it implies that we should think of ourselves as living in a Level IV multiverse, with no particular physical theory=mathematical structure more fundamental than any other. It is this sort of thing that I claim is empty, implying nothing about physics. It's only use is the ideological one of promoting untestable claims about the "multiverse".

108. **DrDave**
January 27, 2014

Dear Mr. Tegmark,
Like many, I feel that dragging creationism into a scientific debate is both surprising and alarming, or to use your word "disturbing." I don't think there is any excuse for this, and, frankly, I think it sets a bad example for observers to see how a scientific debate should be handled. In addition, bringing in an irrelevant buzzword to the debate simply highlights a lack of a clear counterpoint to the argument.

109. **Lee Smolin**
January 28, 2014

Dear Max,

Many thanks for replying and for your kind words. I was unaware of your version of the argument you make, but I did comment on Garriga and Velinken's version in a footnote in Time Reborn. To quote again from there, p 284,

"23. Jaume Garigga and Alex Vilenkin have pointed out, in "Anthropic Prediction for Lambda and the Q Catastrophe," arXiv:hep-th/0508005v1 (2005), that a particular combination of the two constants does better when applied to Weinberg's argument: It happens to be the cosmological constant divided by the fluctuation size cubed. But this leaves two issues: First, what sets the size of the fluctuations? Second, we already knew that the argument did all right when only the cosmological constant was considered. There are many combinations of the two constants that could be tried; the fact that one combination does better than the others is not surprising and, even if there is an argument for it, this does not constitute evidence for the hypothesis that our universe is one world of a vast multiverse."

I would think that this would also apply to your W which depends also on the dark matter density per photon. Should we be surprised that there is a combination of powers of three constants that is extremized in nature? Given that there are many hypotheses and scenarios and many combinations of constants that could be tried, how strong of a case does this make for the hypothesis that our universe is not unique?

Also, Weinberg's paper did predate the measurement, but yours and Garriga and Vilenkin's did not. Even if we accept that your argument is a better version of Weinberg's, it was made after the observation and, in any case, you agree that Weinberg's logic was wrong.

Raphael Sorkin also published a paper before the observation of dark energy, predicting correctly the magnitude that was observed. He did this on the basis of causal set theory, an approach to quantum gravity. No one disagrees with his logic or prediction. It seems that the case here is stronger than for Weinberg as the argument did not have to be improved after the observation. So if you are being logical, and responding to the evidence, shouldn't you be writing a book promoting causal set theory rather than the multiverse? Instead, Sorkin's correct prediction is almost never mentioned, except by specialists in quantum gravity.

Thanks,

Lee

110. **Pete**
January 29, 2014

Thanks for the response Pete. I'm pretty much in total agreement with you as far as that's concerned. And by the way I'm hoping that Frenkel/Holt debate you mentioned in that recent post continues as well. Always good getting some philosophy of mathematics out there in the open.

111. **chris**
January 30, 2014

Does Tegmark talk about issues like inverted spectra and Mary in her black and white room? I know these are usually presented as problems for physicalism, but it seems to me they also present a problem for the idea that "everything is maths".

112. **Shantanu**
January 30, 2014

Lee or anyone else,
could you point me to Sorkin's paper where he predicted the value of cosmological constant using causal set theory.
Many thanks

113. **Lee Smolin**
January 31, 2014

Shantanu,

R.D. Sorkin, “Spacetime and Causal Sets”, in J.C. D’Olivo, E. Nahmad-Achar, M. Rosenbaum, M.P. Ryan, L.F. Urrutia and F. Zertuche (eds.), Relativity and Gravitation: Classical and Quantum (Proceedings of the SILARG VII Conference, held Cocoyoc, Mexico, December, 1990), pages 150-173 (World Scientific, Singapore, 1991);

R.D. Sorkin, “Forks in the Road, on the Way to Quantum Gravity
”, talk given at the conference entitled “Directions in General Relativity”, held at College Park, Maryland, May, 1993, Int. J. Th. Phys. 36: 2759–2781 (1997), eprint: gr-qc/9706002

Maqbool Ahmed, Scott Dodelson, Patrick B. Greene, Rafael Sorkin, {\it Ever present Lambda}, astro-ph/0209274.

Sorkin was mentioning this in talks over many years.

Lee

114. **D R Lunsford**
January 31, 2014

Tegmark should address Peter’s most important point, which is that he doesn’t really have any inner understanding of mathematics as a discipline in itself.

-drl

115. **Orin**
February 9, 2014

Hi Peter,

I read the book (I thought it was delightful), and I have to say I’m left even more perplexed than before at your characterization of the MUH as “empty”. Max says (Chapter 12):

“If the theory that the Level IV multiverse is correct, then since it has no free parameters whatsoever, all properties of all parallel universes (including the subjective perceptions of self-aware substructures in them) could in principle be derived by an infinitely intelligent mathematician.”

Of course this is fleshed out in the book, but I think it is fairly self-evident. Examples of the kind of strategy for falsifiability begin as early as Chapter 6, where he writes:

“If we’re living in a random habitable universe, the numbers should still look random, but with a probability distribution that favors habitability. By combining predictions about how the numbers vary across the multiverse with the relevant physics of galaxy formation and so on, we can make statistical predictions for what we should actually observe.”

So I continue to not understand why you insist on using the word “empty.” I think Max makes a very persuasive case in the book for the potential of the MUH to be predictive, and even falsifiable (he discusses this extensively in Chapter 11, giving examples of falsification using naive measures, ultimately concluding that the major hurdle to be overcome is a solution to the measure problem).

116. **Peter Woit**
February 9, 2014

Orin,

Sure, all properties of all universes could be “derived”, just look at “all properties of all mathematical structures”. Fine, but this predicts nothing at all about any particular property of our particular universe.

The part you quote from the book is referring to the sort of “Level II” multiverse of the anthropic string theory landscape: there’s some specific fundamental physical law that determines the probability distribution he invokes. There are plenty of problems with this, but it’s not what I’m referring to as empty, the Level IV business of the title of his book.

I’ve gone over here carefully what is in his book that claims to be a prediction of Level IV, and I’ve explicitly challenged him here and at the Scientific American site where he wrote a response to this, asking him for a falsifiable prediction of “Level IV”. The best he could come up with is “if we find a physical phenomenon not describable mathematically” that would do it. See
http://www.math.columbia.edu/~woit/wordpress/?p=6669
and the Scientific American site
http://blogs.scientificamerican.com/guest-blog/2014/02/04/are-parallel-universes-unscientific-nonsense-insider-tips-for-criticizing-the-multiverse/
for my comments about why this is empty. If there are all sorts of great examples of how to falsify Level IV in the book that I missed, how come Tegmark is not invoking them when asked about this issue?

You could argue that all you have to do is “put a measure” on the space of mathematical structures, but this is again an empty statement until you give some indication of what such a measure should look like. The only thing he discusses in the book is some sort of counting measure, but obviously you get more and more examples of mathematical structures as you increase complexity, so this sort of thing, besides the “measure problem” due to infinity, has the obvious problem that it predicts your mathematical structure will be as complicated as possible, whereas we know that fundamental physical laws are based on remarkably simple mathematical structures.

On a related note, Tegmark likes to claim that he just has a “measure problem”, not knowing how to relatively count things, when he has something much worse: he doesn’t know how to characterize the space he is trying to put a measure on (the people trying to use string theory at Level II have a much simpler version of this problem: not only do they not know how to compute relative weights of string vacua, they don’t know what the set of string vacua is).

117. **Orin**
February 9, 2014

Peter, you seem to be setting the mark for what is not empty just high enough to fit your definition. I think Max has outlined the beginnings of a theory that is clearly not empty in principle. You point out that it is currently empty in practice. That is fine (if you make it clear what you mean), but obviously theories have periods of gestation before predictions are made, and I don’t think it is fair to be quite so dismissive of the fact that they are not yet mature. There is a Chicken/Egg problem here; you won’t be satisfied until the theory is predictive in practice, yet the theory won’t have an honest shot of being predictive in practice until more people work on it.

On your point about the measure problem (second-to-last paragraph) and the apparent simplicity of physical laws, I think your argument is not as strong as you think it is. For one, your measure must be “anthropic aware”, a feature Max harps on quite a bit (although I don’t think he uses that wording), and it is not at all clear to me that the “pruning” afforded by such a measure would not alone be a sufficient counter-argument. But additionally (and this is a point Max does not make in the book, but nonetheless his ideas should be discussed independently of this particular book written for lay-audiences), a strong case can be made that as you go up the latter of increasing complexity, there are correspondingly increasing numbers of approximate equivalences between structures of greater and lesser complexity. There are also fairly strong counter-arguments in algorithmic complexity theory (I linked to one example earlier in this thread) that clearly indicate to me that your intuition is off. So no, I don't think the measure situation is nearly as dire as you think it is.

118. **Peter Woit**
February 9, 2014

Orin,
I'm not talking about "in practice", but "in principle". All the arguments I've given about the emptiness of "Level IV" are arguments of principle. To show it is non-empty you need to produce a non-empty prediction, even if it is only "in principle". Tegmark hasn't been able to do this (and you don't seem to either). And you can't hide behind every crackpot's favorite excuse "OK, I haven't been able to get anything out of my wonderful theory of everything, but if only lots of people would work on it, maybe they would find something".

It's not possible that "my intuition is off" about "all mathematical structures", because I have no intuition at all about what that even means. As far as I can tell it's a concept every bit as empty as saying "all sets" or some such. There's no there there. The counting measure I was quoting was Tegmark's intuition, not mine. Again, I don't think his problem is a measure problem, his problem is that he doesn't know anything non-trivial about the space he wants to put a measure on.

From your comments, you seem to have your own Tegmarkian theory, since you're making claims not in his book. If you've written it down anywhere, and you have non-trivial implications of such a theory that he doesn't have, let us know what they are. So far, neither you nor he are able to point to anything in his book that gives, in principle, an implication of this MUH that is not on its face empty.

119. **Orin**
February 9, 2014

Peter, the quote I provided from Max's book is exactly such an example, even including the words "in principle." Apparently you need a specific "in principle" prediction (this seems like an oxymoron; I'm not positive what you mean) rather than a general one. Fine, but the fact that the theory can in principle make specific predictions is the reason I am pestering you about your use of the word "empty." The theory is not empty. It clearly can in principle make specific predictions, and the practical reasons why it currently cannot are made as plain as day in Max's book.

120. **Peter Woit**
February 9, 2014

Orin,
This has now become a waste of time, you're just ignoring whatever I write here. I'll just cut and paste the relevant part.

"Sure, all properties of all universes could be "derived", just look at "all properties of all mathematical structures". Fine, but this predicts nothing at all about any particular property of our particular universe.

121. **Orin**
February 9, 2014

Peter you are playing dumb. Obviously that is not the only implication of what Max wrote when he discusses the prediction of "the subjective perceptions of self-aware substructures" in a multiverse in which he has already established that in principle there are "probability distributions that favor habitability," with which one can use to falsify the theory. He is not just saying one can in principle derive the properties of all mathematical structures. He is clearly saying that if a reasonable measure is found then one can in principle derive the probability for us to live in a universe with an effective Standard Model lagrangian and General Relativity with a finely tuned cosmological constant, etc, and that if this probability is found to be small then the theory is falsified.

122. **Peter Woit**
February 9, 2014

Orin,
Now we're back to "all I have to do is find the right measure on the space of all mathematical structures", and the problem that "all mathematical structures" is an empty concept. From nothing you get nothing. Tegmark gets nowhere with this in his book because it's inherently empty and can't go anywhere. If you point to anything other than absurd wishful thinking about explaining everything from nothing, please do so, but so far you're just wasting your and my time.

123. **billandturk**
February 11, 2014

Suppose some relatively slightly more advanced alien civilization living in Omega Centauri has already solved, for instance, their metabolic syndrome and counteraffects of ageing, integrated successfully with their quantum computing power, essentially have achieved a form of immortality. There's probably a Hollywood movie playing this out, or at least there should be.
What gives when an alien from this civilization visits you, Peter (after all, they originate in our own galaxy) and tells you Everettian postulates about decoherence, many worlds, and ultimately MUH are all true? As a matter of fact, the alien even takes the time to demonstrate to you on his quantum hand-held device how he has uploaded his "essence" innumerable times into his planet's hosted quantum supercomputer, and in fact is himself living out "many lives" … any and all of which can and do seem no more or less "real" to him.
Theoretical emptiness, perhaps.
Impossible or improbable?

124. **Peter Woit**
February 11, 2014

billandturk,
If a space alien or a guy with gold tablets appears magically in front of me, and explains to me about how string theory really is the TOE, the multiverse works, the space of all mathematical structures carries a natural measure that explains everything, etc. I am happily going to agree that yes, these ideas are testable science and have been tested.

However, if this possibility is the argument from proponents about why their ideas really are non-empty and scientific, I think they have a problem….

125. **billandturk**
February 11, 2014

Thank you Peter taking the time replying.

I am young, dumb, and novice with this subject matter and even more “rookie” with the math underpinning it all; however, your response to my post actually helps me better contextualize and appreciate your points.
I like the saying: There are known unknowns – things we know we know; There are known unknowns – things we know we don’t know; and There are unknown unknowns – things we don’t know we don’t know.
After having just finished Tegmark’s book, I was inclined to believe his ideas fell into that middle category. I see where you’re coming from, and it also makes sense to me now, that perhaps (not trying to suggest my words are coming out of your mouth and hopefully not to be taken offensively), these ideas are more the latter, “unknown unknowns”, and the fundamental flaw with this category is presuming you can articulate “science” attributes upon it?
Oh well, genuinely thanks again!

Not Giving Up

January 23, 2014
Categories: Uncategorized

One question I've been wondering about for the last 20 years or so has been what SUSY proponents would do when the LHC finally gathered data and found no SUSY. Would they finally admit this was an idea that hadn't worked out, or would they never give up, no matter what the data said? The answer is now in. John Ellis was a co-organizer of a Royal Society conference earlier this week, and a report from the conference has the following:

> "I think that the physics case for supersymmetry has, if anything, improved with the LHC's first run, in the sense that, for example, supersymmetry predicted that the Higgs should weigh less than 130 gigaelectronvolts, and it does," Ellis said.
>
> "Of course, we haven't seen any direct signs of supersymmetric particles, which is disappointing, but it's not tragic," Ellis added. "The LHC will shortly almost double its energy — we're expecting eventually to get maybe a thousand times more collisions than have been recorded so far. So we should wait and see what happens at least with the next run of the LHC."
>
> And if the LHC's next run does fail to reveal any sparticles, there is still no reason to give up on looking for them, he said. In that case, new colliders with even higher energies should be built, for collisions at energies as high as 100 TeV.
>
> "I'm not giving up on supersymmetry," Ellis told LiveScience. "Individual physicists have to make their own choices, but I am not giving up."

So, Ellis has made his position clear: no giving up, no matter what the LHC data from the next run says.

Comments

1. **Thomas Larsson**
   January 23, 2014

   Is it even theoretically possible that susy will be found at the LHC?

   My impression is that the SM is so constrained by high-precision data, that a direct discovery of sparticles at 14TeV is very unlikely. What could rather be found is some anomaly in the high-precision data, which could herald a discovery at yet higher energy. Or something else.

2. **Neil**
   January 24, 2014

   Wow. You'd think ACME's zero dipole moment measurement for the electron would give the SUSY crowd some cause to doubt that there is an exotic particle zoo out there.

3. **CIP**
   January 24, 2014

   Reasons not to give up on SUSY include the fact that there aren't many promising alternatives. The two biggest mysteries in physics are dark matter and dark energy, and SUSY can at least hint at solutions. What else can? Nor are there many promising alternatives for quantum gravity.

   Physics will move on if something more promising comes up.

4. **Friedwardt Winterberg**
   January 24, 2014

   It is ironic that the Ptolomaic system failed because it was stuck with the more symmetric circular motion against the less symmetric but observed elliptical motion, while the more symmetric supersymmetry leads to a less symmetric nonspherical electron with a non-observed electric dipole moment. I once had told Edward Witten that supersymmetry can exist without superstring theory but not the other way around. His answer was that you can say this about almost any theory. But without supersymmetry can there be a superstring theory? And how about Juan Maldacena's anti de Sitter (ADS/CFT) conjecture in face of our more de Sitter (not anti) observed universe? Shall we rally believe in the 10^500 universes? In an e-mail exchange I once had with Lubos Motl, this good man had the following slogan on all of his e-mails: "String/M theory is the language in which God wrote his world". In a reply I wrote him: "Lubos, this is not science but religion in the guise of science".

5. **Shantanu**
   January 24, 2014

   CIP and s.vik.
   To give one example : asymptotic safety.

6. **GoletaBeach**
   January 24, 2014

   Well, the light Higgs is interesting. I think Ellis has a point there. Of course the confirmation of the precision electroweak analyses is the biggest news. But something or other keeps the Higgs light, and that is definitely interesting. Maybe it is SUSY. Maybe not. Not really Black & White to me that no direct SUSY particles is a complete SUSY rejection. Many searches for free quarks came up empty.

7. **Peter Woit**
   January 24, 2014

   GoletaBeach,
   Not just many, but all searches for free quarks came up empty, because there are no free quarks. Any theory of the strong interactions with free quarks is a wrong theory.

   Similarly, it's now pretty clear that superpartners aren't going to show up, and any theory like the SUSY extensions of the SM that have been studied for 40 years is a wrong theory. Time to look for something quite different if you want to get anything out of SUSY. What's odd is that Ellis is not talking about that, but about just looking for the same thing at higher energies.

8. **Alex Sander**
January 24, 2014

Woit,

I think you are beeing too simplistic. Notice that among other good things SUSY has gave us is that its also a good “signal generator” in the sense that has trigered interesring searches for physics (and will keep doing so) that may ultimately let to discoveries (sparticles or other physics cases).

9. **Curious Wavefunction**
January 24, 2014

Sorry, but I have to admit that thinking of this kind reminds me of a classic feature of conspiracy theories: When evidence arguing against your theory emerges, you simply expand your ‘circle of belief’ to accommodate that evidence.

10. **Noah Smith**
January 24, 2014

OK, but here is the obvious question: What group of humans on this planet is going to shell out the billions of dollars needed for a next-generation collider just to look for SUSY?

If the answer is “no one”, what do the SUSY researchers do?

11. **Low Math, Meekly Interacting**
January 24, 2014

What’s most puzzling is how obvious it is that SUSY is a belief system, and how little that seems to concern its proponents. “SUSY”, in all its myriad permutations, could appear over a vast range of energy scales. “Look at higher energies” is an argument that could hold theoretically no matter how much negative data are produced. It is, after all, exactly what’s been said every time SUSY has lurked just beyond the reach of the current accelerator, yet astonishingly failed to appear. The rationalizations being made today so closely echo those made after the Tevatron shut down, after the LEP shut down…”Well, those expectations were silly, naive, oversold, and clearly the Not-Quite-As-Minimal-As-The-Last-Supersymmetric-Standard-Model tells us the parameter space…” Does it take a genius to get seriously worried about some of these dearly-held beliefs regarding a particular perception of “beauty”? We know what the past has tought us. What can we predict about the future, not only about Nature, but about how human nature impacts our study of Her? From what I can see, this could go on forever. Some even appear to think that’s one of the theory’s key virtues! Is this not all frightening?

12. **tt**
January 24, 2014

“If the answer is “no one”, what do the SUSY researchers do?”

Economics?

13. **DrDave**
January 24, 2014

In the absolute worst case scenario, where no evidence ever turns up, one could certainly claim that an LHC in another universe could detect something, and that ripples from this event could be seen in yet another universe, just not ours.

14. **S. Chan**
January 24, 2014

This post is one of the few occasions when the following seems genuinely appropriate: “Never Gonna Give You Up …“

15. **Low Math, Meekly Interacting**
January 24, 2014

I should say again that I, at least, when I voice my concerns in my own insignificant way, never do so with a sneer. I see these individuals, with very few exceptions, as brilliant, honest, devoted scientists working at the pinnacle of human intellectual achievement. It was their professional ancestors who made “science” what it is. They seem always to be at the forefront of whatever science is evolving into. So the truly worrisome question is, what is science evolving into?

16. **Jesper**
January 24, 2014

@ CIP. To use “there aren’t many promising alternatives” as an argument for continuing down the susy (string) path seems to be a never-ending loop. If everyone reasons like this an alternative will indeed never be found.

17. **Andreas**
January 24, 2014

It’s well known that from epistemology that theories do not disappear from one moment to another. They disappear with the scientists who developed them. Then they get old and retire, eventually their theories – which are not supported by data – will retire aswell.

Obviously one can understand psychologically why people like Ellis do not give up. They based decades of their scientific career on this topic. Would you be able to admit that all you have done was wrong? 😮

I think everything goes its predicable way…

18. **SUSY**
January 24, 2014

Woit, what do you think reasonable scientists in HEP and QG should pursue in the event of no-SUSY in LHC and other experiments? What is a scientifically responsible and reasonable course for HEP/QG after non-SUSY LHC 2016?

19. **David Folsom**
January 24, 2014

Andreas:
Quote:"Obviously one can understand psychologically why people like Ellis do not give up. They based decades of their scientific career on this topic. Would you be able to admit that all you have done was wrong?"
.
There is a saying I like to use for situations like this: Do you want to be wrong today, or do you want to be wrong forever?

All due credit to anyone who invented it before me.

20. **Noah Smith**
January 24, 2014

"Economics?"

Oh, God. That's what I was afraid of.

FUND THE COLLIDER AT ALL COSTS

21. **PlanckConstant**
January 25, 2014

A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it

22. **emile**
January 25, 2014

PlanckConstant: a new scientific truth does not triumph by convincing people and making them see the light or by a generation of proponents or opponents dying. It triumphs with experimental evidence.

23. **Tommaso**
January 25, 2014

Hello Peter,

a bit off topic, but I would like to point out that Ellis' quote contains a misconception that I think does not belong there, not even if the target of his text are laymen.

I have always believed that in order to explain hard concepts to outsiders
one has to simplify things to the point of making slightly incorrect statements.
But I don't think that saying "we're expecting eventually to get maybe a thousand times more collisions than have been recorded so far" falls in that category.

What Ellis forgets is that the rate of event recording is not going to improve by x1000,
but maybe by x2 or x4. CMS and ATLAS have recorded in total less than 10B events, and in the future they won't go above some tens of billions; that's because the increase of luminosity is not matched by a multiplication of data acquisition power – and it would not be meaningful of course. A better way to explain what is in store is to say "we are expecting to produce a thousand times more collisions", retaining the overall meaning.

Cheers,
T.

24. **lun**
January 25, 2014

On a semi-related topic, did you attend this:
http://physics.columbia.edu/amplituhedron
and do you think thespeaker's approach of studying carefully a world with many SUSYs
as toy model can revolutionize our understanding of our world with, perhaps, not even one?

25. **Peter Woit**
January 25, 2014

lun,
Yes, I was there, will write about this soon. As far as the topic of this posting goes, there was almost nothing in the talk about SUSY, at least in the first two hours…

26. **Peter Woit**
January 25, 2014

SUSY,
Fo theorists, the set of reasonable things to work on post no LHC SUSY is about the same as the set of reasonable things to work on pre no LHC SUSY, since there never was a good reason to expect LHC SUSY, and it was a subject that had been studied to death for years.

27. **Peter Woit**
January 25, 2014

Thanks Tommaso,
That's interesting, I hadn't thought much about that distinction. I guess it's not too important if you're discussing very rare events and some ideal triggering scenario, but clearly there are measurements where the high luminosity is not going to help for the reason you give.

28. **GoletaBeach**
January 25, 2014

"GoletaBeach,
Not just many, but all searches for free quarks came up empty, because there are no free quarks. Any theory of the strong interactions with free quarks is a wrong theory."

Not quite all, but those that had positive results (Fairbanks, 1980 or so, for example) were (rightfully) discredited. But people kept looking for a long, long time, nice review…

http://www.annualreviews.org/doi/pdf/10.1146/annurev-nucl-121908-122035

I differ with you, Peter… the issue cannot ever be settled by theory, only by experiment. What we know is that in normal matter the free fractional charge abundance is <10-23 per nucleon or so. I think whether QCD has peculiar and rare states that somehow preserve fractional charge is still an open question, although one that is unpopular.

"Similarly, it's now pretty clear that superpartners aren't going to show up, and any theory like the SUSY extensions of the SM that have been studied for 40 years is a wrong theory. Time to look for something quite different if you want to get anything out of SUSY. What's odd is that Ellis is not talking about that, but about just looking for the same thing at higher energies."

`Pretty clear' is not the same as doing the experiment and checking. It was `pretty clear' at SPEAR in 1973 that no narrow resonances were present… their scan had skipped over the J/Psi. So in 1974 they found it.

Experimentalists are quite different than theorists. You theorists seem to think you are in a chess match where checkmate means something. Experimentalists find you all a bit ego-driven and narrow, and overly certain (one way, favoring certainty, or another way, rejoicing in rejection).

Experimentalists usually (not always) are much more diffident at heart. I think many experimentalists never caught the SUSY wave but solidly support going to the highest energies technologically possible to look for surprises. Conversely, trash-talking the 14 TeV run because SUSY won't likely be there seems similarly wrong-headed.

You go to the highest energies and look for new phenomena. You keep looking for free fractional charge with whatever clever trick you can find. You look for electric dipole moments, mu-e, dark matter, neutrino mass, etc, etc, and view theorists as an odd opinionated tribe. An odd tribe that was initially quite hostile to, for example, the observation of the tau lepton, to parity violation, to fission… the list is interesting and long.

29. **jd**
    January 25, 2014

    Well, good luck on getting a machine for higher energies. The well is running dry.

30. **theon**
    January 26, 2014

    Having studied in the then "world gauge center", Utrecht University with Veltman and 't Hooft, I learned about the standard model but got appalled by SUSY. Seeing its near defeat decades later, does not feel as a victory. Too much damage has been done, first by SUSYers, then by SUGRAvers and the blow by (Super)Stringers. It seems fair to conclude that physics reached the end of the Gauge Principle. How to continue? Why not look at the basis of quantum mechanics? Its measurement problem has been solved recently, leading towards its statistical or ensemble interpretation akin to classical ensemble theory, doing away with many worlds, many minds and so on. But if we do *one* experiment and get *one* outcome, one may ask the question: what goes on in the apparatus to generate this outcome? We do not have a theory for this down-to-earth question! So my bets are on a sub-quantum theory, more classical than quantum theory itself. Integration with gravity should be done at that level. Not surprisingly, the possibility of a sub-quantum theory kept Einstein and 't Hooft busy for decades.
    This answer the what-to-do question: Turn around and start again.

31. **Nathalie**
    January 26, 2014

    theon

    You write that the measurement problem in quantum mechanics has been solved recently …

    This is a very important statement. Please give the relevant references. Thanks.

32. **chewbachelor**
    January 26, 2014

    There is something I find a bit incredible. In the past, 1. the theorists were making predictions, 2. if the predictions were correct, the theorist received recognition, 3. if they were not verified, the theorists were not recognized. Now, it seems that none of these considerations matter.

    E.g., nobody asks a SUSY theorist to make predictions, only moral suasion; if the predictions are wrong, the theorist can simply change them; finally, the recognition is for granted, once the theorist is paid by a prestigious place and if he/she can publish empty but successful papers.

    Instead, when I hear a talk of a theorist, I immediately ask: What are his/her achievements in physics? This simplifies a lot the assessment and allow me to save a lot of time.

33. **Peter Woit**
    January 26, 2014

    theon,
    I strongly disagree with your conclusion that the failures of SUSY/SUGRA/Superstring theory mean the "end of the gauge principle". A better conclusion would be that they indicate something went awry in the "SUSY" extension, not with gauge symmetry (where the recent discovery of the Higgs vindicates the gauge theory point of view).

    All,
    Sorry, but measurement theory in QM, while a wonderful topic, is off-topic here.

34. **Peter Woit**
    January 26, 2014

    GoletaBeach,
    I don't really disagree with you. By all means, experimentalists should do whatever they can to look for new phenomena. To the extent though that they have to make choices and decide to get some guidance from theory, best to work with a clear-eyed view about what theory does say and what it doesn't. By all means, look for fractionally-charged objects, but realize that the ones theorists saw as most likely in 1964 (u,d,s quarks) now look quite unlikely, so better to search widely and not assume anything about, for instance, which fractional charges to look for. Similarly, of course the higher energy LHC run is important and searches should be done there for anything and everything that might show up. When designing triggers, etc., though, it would be a good idea to make sure things are not narrowly tuned to looking for specific superpartners heavily advertised in the past by theorists, since such theories now look quite unconvincing.

35. **srp**
January 26, 2014

GoletaBeach makes an interesting point. Scientific American has a cover story this month about how two valid methods of measuring the radius of the proton give two very different answers. The CERN briefing book says that the spin of the proton can't come close to being derived from the measured spins of its component parts. Transverse polarized proton beam collisions either violate the QCD prediction massively or nobody knows how to compute the correct prediction. Are protons too "composite" to interest high-energy theorists these days? It's hard for a layman to understand the apparent lack of interest in such fundamental matters (no play on words intended). I'm very fond of the protons that give me most of my mass while kindly refraining from decay, even if it turns out that their components have no superpartners.

36. **GoletaBeach**
January 27, 2014

We're on the same wavelength, Peter. Leptons and missing energy and jets remain the fundamental things to look at, and will always be done. What is really hard to guarantee is that there won't be another `scanned over the J/psi' , that is, missing something by not considering all the possibilities type of oversight in the 14 TeV.

srp, oddly enough, the size of the proton is not considered by most particle physicists as particularly sensitive to interesting new physics. We could be wrong, in fact, I hope we are wrong. My guess is that the two different measurement techniques (electron scattering and muon atoms) might have slightly different corrections needed to back out the fundamental thing, that is the size of the proton. And maybe nobody has spent sufficient time doing those corrections accurately to the few in a 100 level.

Topcites 2013

January 25, 2014
Categories: Uncategorized

The people at SLAC for a long time have been compiling "Topcites" data which includes various lists of the most heavily-cited papers in HEP. From 1997-2003 Mike Peskin each year would write something about the significance of the lists. I first wrote a blog post about one of these lists nearly 10 years ago, about the 2003 list (see here). The 2013 list has just appeared, with a blog entry here, and the lists available here.

I haven't written much about these lists for the past few years, since there didn't seem to be much new to say. By the 2005 list it was becoming clear that there was so little new happening in hep-th that the list was dominated by pre-2000 papers, specifically the early AdS/CFT papers, as well as papers about speculative large extra dimension scenarios. This pattern has continued to this day. If you think citations mean something, this data shows a collapse of HEP theory having taken place sometime around mid-1999. The last two theory papers that appear in the overall heavily-cited paper list are a Randall-Sundrum paper from June 1999 and Seiberg-Witten's String theory and non-commutative geometry (which I suspect makes the cut because "non-commutative geometry" is in the title, so this gets referenced by lots people doing something with non-commutative geometry, even if it has little to do with this paper).

The dominance of this list and of hep-th by AdS/CFT papers is hard to exaggerate, with Maldacena's paper long ago leaving behind every other theory paper ever written, on track to hit 10,000 citations sometime later this year. Just as "string theory" has become ill-defined, now "AdS/CFT" is starting to become ill-defined, with these 10,000 papers covering a huge variety of different things. In addition there's a great deal of hype and ideology surrounding this subject, with an ex-Harvard faculty member and now world's most prominent string theory blogger yesterday calling for the murder of anyone caught "talking about the AdS/CFT correspondence's not being dependent on string/M-theory". More positively, Matt Strassler has been writing a very long series of blog posts that appear to be aimed at sooner or later getting to AdS/CFT. He's at number 8, but I fear at least a hundred or so would be needed to cover the subject. By the way, people who like carrying on a tedious rear-guard action in the string wars by arguing about string theory and AdS/CFT should do it at another blog.

To get more fine-grained information about what recent work is getting cited, see listings here by arXiv category. The listing here of papers cited by hep-th papers during 2013 is dominated by the old AdS/CFT papers, but more recent things that occur in the top 10 are ABJM from 2008 (3d version of AdS/CFT), a 2006 paper on entanglement entropy from AdS/CFT (now a hot topic), and a review paper on AdS/CMT.

Comments

1. **Bob**
   January 25, 2014

   This is depressing. More useful would be a list of the most promising hep-th papers from the past few years.

2. **CU Phil**
   January 25, 2014

   I'm surprised that nothing on amplitudes made the list.

3. **Peter Woit**
   January 25, 2014

   CUPhil,
   It looks like the highest cited amplitudes paper appears as number 63 on the list of papers cited this past year (with 69 citations). One reason for this I think is that the amplitudes business is still rather small, with a limited number of people working in it. It's a complicated story, involving a lot of technology, so a high barrier to entry. And I think lots of people are still not convinced there's anything that will come out of this except maybe some modest progress on doing complicated amplitudes calculations. For more about this, see the next posting…

4. **S**
   January 27, 2014

   You don't talk about AdS/CFT much yourself, Peter; does it fall under the string tent for you, and receive the same opinion?

5. **Peter Woit**
   January 28, 2014

   S,
   Various topics about AdS/CFT have been discussed in detail here over the years. As I note above though, at this point the name refers to such a wide variety of things it's hard to say something sensible about 10,000 papers. As for its relation to string theory, that argument has gone on ad nauseam, and pretty much immediately turns into religious warfare if the topic comes up. The only new recent development I've seen is the call for the killing of anyone who suggests string theory is not fundamentally responsible for AdS/CFT.

The Amplituhedron and Twistors

January 25, 2014
Categories: Uncategorized

Yesterday Nima Arkani-Hamed was here at Columbia, giving a theory seminar on the topic of the Amplituhedron, which is a characterization of the integration region in a calculation of scattering amplitudes by integrating over regions in the so-called positive Grassmannian. This is a modest advance in mathematical physics, one that for some reason a few months ago garnered a lot of hype (see here for more about this).

As seems to often be the case, the Arkani-Hamed talk was a bit bizarre as an event. Scheduled to start at noon, people soon settled in with the sandwiches provided by this seminar, and he started talking about 12:15. About an hour and a half into the talk, people were reminded that he doesn't mind if they leave while he's speaking. Two hours into the talk, soon after he said he was only a quarter of the way through his material, I had to leave in order to do some other things. I don't know how long the talk actually went on. It's too bad I didn't get a chance to stay until the end, since he promised to then explain what the current state of progress on these calculations is.

What is being calculated are scattering amplitudes in a conformally invariant theory, with the simplest example the planar limit of tree-level amplitudes of N=4 super Yang-Mills. One wants to extend these methods to loops, to higher order terms in 1/(number of colors), and to non-conformally invariant theories like ordinary Yang-Mills (at the tree level, ordinary and N=4 super YM give the same results).

As usual, Arkani-Hamed was a clear and very engaging speaker. Also as usual though, it's unclear why he thinks it's a good idea to not bother trying to fit his talk into a conventional length, but just keep talking. One reason for the length was the extensive motivation section at the beginning, which had basically no connection at all to the topic of the talk. There was a lot about quantum gravity of an extremely vague sort. In a recent talk I wrote about here, he explains one reason why he does this, that he's describing the motivation he needs to keep doing this kind of mathematical physics. I suspect another related reason is that this kind of vague argument about quantum gravity and getting rid of space-time is all the rage, so if you're not working on the firewall paradox, you have to justify that somehow.

Once he got beyond the motivational stuff (and a complaint about BRST: "almost anytime you hear BRST, there is something formal and complicated going on") the talk was worthwhile and I learned a fair amount. The main thing that struck me was just how much the whole story has to do with Penrose's twistor program. Penrose developed twistors also with a quantum gravity motivation: they provide a very different set of basic variables, with usual space-time points not the fundamental objects. Of course I was aware of some of the twistor part of the amplitudes story (see for instance here), but I was unaware of the important role played by Andrew Hodges, of Penrose's twistor group at Oxford, in these recent developments. Hodges, besides writing a fantastic biography of Alan Turing, has worked on twistor theory for about forty years, and some of his innovations have been crucial for the recent advances on gauge theory amplitudes. One example is his "twistor diagrams", and for more about this and how other work of his has contributed to the emerging story, see his up-to-date Twistor Diagrams website. Hodges is a wonderful story of someone who didn't follow fashion, but stuck to pursuing something truly worthwhile, it's great that he has now been getting attention for this, as his work has become useful for more popular research programs.

For those who keep asking about interesting, promising ideas about fundamental physics to work on, twistors are something they definitely should look into. The recent amplitudes work is one specific application of thinking in twistor variables, but the whole question of how to do quantum field theory in twistor space seems to me to still be wide open. Twistor theory involves some wonderfully different ways of thinking about four-dimensional geometry, and these seem far more likely to play some role in future advances in the direction of unification than any of the tired ones (GUTs, SUSY, string theory) that have dominated the field for so long.

Comments

1. **OMF**
   January 26, 2014

   > Hodges is a wonderful story of someone who didn't follow fashion, but stuck to pursuing something truly worthwhile…

   Are you implying that a lot of what is fashionable is not very worthwhile? (I am asking a very serious question here.)

2. **J.S.**
   January 26, 2014

   I find peculiar that according to you what Hodges does is awesome but not so what Nima does, after explaining how related both things are.

3. **Peter Woit**
   January 26, 2014

   J.S.,
   I was writing here about what I learned from Arkani-Hamed's talk. He was the one who credited Hodges with coming up with the important ideas he was discussing (with three examples "twistor diagrams", dual conformal symmetry, and the idea that the amplitude was a volume of a polytope). For more details of who did what, see this page at Hodges's web-site
   http://www.twistordiagrams.org.uk/papers/index.html
   which tells the story in a way quite consistent with Arkani-Hamed's version in his talk.

   So, if you want to simplify this to "what Hodges does is awesome", you can, but that's not "according to me", it's a report of the story according to Arkani-Hamed.

   OMF,
   Yes, a lot of what is fashionable is not worthwhile. This is true to some extent in all subjects, certainly is true in theoretical physics. One can get into an argument about what of such currently fashionable ideas are the ones that are not worthwhile, but if you want an example, I've made it very clear what I think of the multiverse. No matter what your views on that, I think if one goes back and looks at the topics fashionable at any given moment in HEP theory, most of the time you'll find that some of the fashionable topics didn't turn out to be worthwhile, while people doing some very unfashionable things in obscurity turned out to be doing something much more worthwhile. Hodges is a good example of this.

4. **Peter Woit**
   January 26, 2014

   Thinking about both this talk and the Tegmark story, an odd parallel occurred to me. Tegmark early in his career downplayed the multiverse part of his interests, in order to make sure people took him seriously as a scientist. These days, it's somewhat the reverse, with Arkani-Hamed using, if not the multiverse, quantum gravity research at the boundaries of science, in order to get people to take seriously his work on what they otherwise might decide was just an obscure corner of mathematical physics.

5. **J.S.**
   January 26, 2014

To say that the multiverse mania is fashionable you have to provide evidence: what fraction of the vacua contain string theorists? and in what fraction of those are they actively discussing the multiverse?

Platonism CageMatch at MoMath

January 25, 2014
Categories: Uncategorized

After spending two hours in the middle of the day hearing about unexpected uses of twistors to study particle scattering amplitudes, yesterday I went down to Manhattan's relatively new Museum of Mathematics, which had scheduled a "Family Friday" event, featuring Edward Frenkel and Jim Holt. The event began with Frenkel giving a presentation about math, kind of an introduction to his wonderful new book Love and Math. Everyone in the audience hoped that the kids in attendance didn't catch his comment about a typo in reference to the LHC (given Frenkel's film experience, some had suggested that a joint event with the neighboring Museum of Sex would have been a good idea).

Things really got exciting though when Jim Holt joined him on the stage, for a no-holds-barred discussion of Platonism and mathematics in front of a standing-room-only crowd. Holt ripped into Frenkel as engaging in "mysticism" by claiming that mathematical objects are "real" and "exist". He quoted from Bertrand Russell, who early in life took Platonist positions, but in his old age renounced them. Frenkel countered, dismissing Russell's later quotes as those of someone who had gone soft in the head. He went on to quote arch-Platonist Kurt Gödel, with the response from Holt a low blow: he told the story of how Gödel had died a paranoid, starving himself to death. Holt continued the attack in the same vein, telling about Georg Cantor, and his end in the loony-bin. The implication was that Platonists are not just mystics but nuts.

Frenkel then decided to try taking the high road, invoking W.V.O. Quine and Hilary Putnam (distinguished non-nuts Harvard professors I took courses from) and their Indispensability Argument. The basic idea there is that the best choice of what "exists" is those entities that are an indispensable part of our best theory of the material world. Not sure yet whether twistors count, but if they become part of the new unified theory of gravity and the Standard Model, then they surely exist as much as anything does. Holt parried with Hartry Field's Science Without Numbers: A Defence of Nominalism which supposedly shows you can do Newtonian physics without math. Frenkel (together with much of the rest of the audience) scoffed at this, making the obvious riposte: what about GR?

This was finally brought to an end with a few questions from the audience, a sizable contingent of which was underage. They seemed to be having a great time, far more entertained by this sort of thing than by the usual flashy trinkets people use to try and get them interested in math (but which seem to work better on the pre-verbal baby crowd). All in all, a highly edifying experience, I hope the Frenkel/Holt show gets taken on the road.

For a picture of the action, see here.

Comments

1. **Shecky R**
   January 25, 2014

   sounds like a very entertaining evening… don't s'pose you know if there will be a video-upload to YouTube or elsewhere?

2. **CIP**
   January 26, 2014

   Of course what they were really arguing about was what one means by "exists". It's not easy for me to believe that arguments about the meanings of words mean much.

3. **S. Molnar**
   January 26, 2014

   A logician who spent time at the Institute in the 1950s told the story of how Gödel looked up something in a book in the Institute library that he had consulted some years before. Finding that it did not say what he remembered, Gödel concluded that there are demons that go around changing things in books. This, along with his later paranoid belief that people were trying to poison him, suggests a sort of mad hyper-Platonism that strikes me as being a reasonable disqualification when it comes to the more mundane question of mathematical Platonism.

4. **CIP**
   January 26, 2014

   Given the number of great mathematicians who wind up nuts, maybe it's not a talent one should wish for.

5. **Chris Kennedy**
   January 26, 2014

   Peter,
   With regard to the typo, I wasn't there but I've often wondered if all sorts of trouble could have been avoided the past few years by referring to the LHC as the Baryon Collider instead? Speaking of which (slightly off topic) do you plan on reviewing Moffat's "Cracking the Particle Code of the Universe" book containing his very detailed take on the Higgs discovery?

6. **Peter Woit**
   January 26, 2014

   CIP,

   I agree that bald questions like "Do mathematical objects exist?" can be a waste of time, just an uninteresting argument about the meaning of words. The argument over "Platonism" that Frenkel and Holt were having I think is more interesting (besides being kind of fun), since it gets at the question of what to make of "the unreasonable effectiveness of mathematics". Different takes on this question aren't necessarily "right" or "wrong", but lead one to value and take interest in different aspects of mathematics and physics. To the extent that there really are philosophical issues with substance, to me this one of how mathematics, physics and our relationship to reality fit together is a great one.

   As for the great mathematicians = lunatics idea, that's just not my experience at all. There's nothing much to the idea that mental illness corresponds to mathematical talent and achievement. While there have been great mathematicians with mental health issues, generally they did great work while healthy, not so much while sick.

7. **Peter Woit**
   January 26, 2014

   Chris Kennedy,

Probably you're right that CERN should have gone for LBC instead of LHC to stay away from trouble…

I briefly skimmed through the Moffat book in the bookstore, don't think I'll carefully read it or write about it here. His take on the Higgs discovery is an unusual one, since he was prominently arguing for theories with no Higgs. Those who want to know everything about the subject may find some interesting new things in the book (and what I read looks accurate), but in general his personal skepticism about the Higgs so far just seems to have been a wrong direction, making his take on the story not of wide interest.

8. **Peter Woit**
January 26, 2014

S. Molnar,
The story about Gödel that you mention doesn't sound like "hyper-Platonism" so much as the all-too-common symptoms of some varieties of mental illness.

9. **S. Molnar**
January 26, 2014

Peter Woit: Yes, I agree that it was a product (or aspect) of mental illness, but merely wanted to point out that a mind that does not distinguish well between imagination and reality in general is not necessarily a reliable authority on Platonism.

I also doubt that madness exists at a rate among the best mathematicians that is greater than that of top people in other creative fields, but I'm not so sure about logicians in particular. Was it not George E. P. Box who said "All model theorists are mad, but some are useful"?

10. **Chris W.**
January 26, 2014

Speaking of logicians, life did not end well for C. S. Peirce, either. (Granted, "logician" is too narrow a characterization of Peirce.)

11. **srp**
January 26, 2014

Anyone managing socks coming out of the dryer has at least a little sympathy for Godel's position re: book demons.

12. **David Edwards**
January 27, 2014

I think you might find the following sociological facts interesting:

Every time I've made the following statement to mathematicians, they've responded with a broad grin and total agreement!

Even if God hadn't created the Universe, still there would be an infinite number of primes.

Recently, I followed the above with the following:

Even if God hadn't created the Universe, still there would be the category of categories.

I was amazed to discover that young (~30) mathematicians also totally agree with the second statement modulo logical qualms!!

13. **Kavanna**
January 28, 2014

You know, just because you're paranoid, doesn't mean you don't have enemies — or haven't lost some socks in the laundry.

But seriously: while a ga-ga sort of Platonism is congruent with certain kinds of mental illness, the two are not the same, and one doesn't require the other. You're keeping your sanity when you regard nonsensory (but not nonsensical) entities as hypothetical or contingent. You just have to avoid the ga-ga part, thinking that you're perceiving some definitely existent nonsensory, "higher" reality through pure thought, as Descartes and Plato dreamed of.

Ask instead for some proof of relationship to sensory reality. Then you're using your reasoning power soundly, to understand a reality that is beyond our limited senses, but somehow connected with them. Hold on to your senses, as Aristotle said, and accept form as always related to matter and matter to form.

14. **Hardy Hulley**
January 28, 2014

It always interests me that most mathematicians and physicists "behave" as though they were Platonists, even when they are not. By this I mean that the language they use to describe the mathematical concepts used in their work is inherently Platonic. It is only when you press them on the issue that some of them reluctantly admit that mathematical Platonism is problematic philosophically. The point is that mathematicians and physicists "perceive" mathematical objects as though they were real, and this illusion is very compelling.

Curiously, we also "perceive" moral principles as though they were real, even though moral realism exhibits similar deficiencies as mathematical realism (unless you are willing to invoke religion). This is why I think debates about mathematical realism are important – I think that an adequate account of our perception of mathematical "truth" might shed important light on our perception of moral "truth."

15. **nasren**
January 28, 2014

The ad hominem arguments from Jim Holt are a disgrace. If he has a convincing argument for Nominalism then he should give it; and if he doesn't then he should lay off the personal attacks. Cantor was driven into a depression from the attacks of Kroenecker. Gödel was denied full professor status for many years due to the personal vindictiveness of one leading figure in the IAS. If both were paranoid it was also true that both had some reason to be. If personal attacks are allowed then how about we start assembling data on the marital failures of Nominalists, or their alcoholic intake, or what have you.

Some days the slide of academia into idiocy is too much to bear.

16. **Peter Woit**
January 28, 2014

nasren,
I was exaggerating a bit the combative nature of the argument for effect, since I found novel and amusing the phenomenon of a lively, entertaining, high level discussion in front of a general audience of all ages of the philosophy of mathematics. Normally this sort of thing is done in a way that puts all but the most devoted to sleep. Yes, there were some low blows, but everyone knew they were low blows intended for entertainment purposes. Why do you think I described this by the term used for professional wrestling? Do you think that professional wrestlers are trying to hurt each other?

17. **nasren**
January 28, 2014

Hi Peter — yes, I understood your intention, but as a sometime contributor to this debate I can tell you that low blows are not at all uncommon. Platonism is not so much refuted as routinely abused. And this irrationalism has increased markedly in the last twenty years. The idea that Platonism is just a whacky, space-cadet view is becoming completely standard. It seems to me that post-modernism (abuse, rhetoric-as-argument, ad hominem attacks, hero worship of celebrity "thinkers") has become the norm across all areas, including much of theoretical physics. So far only mathematics has been exempt from this corrosion. So far…

18. **Peter Woit**
January 28, 2014

nasren,
I guess my perspective is different. Working in a math department, I don't hear anyone arguing against Platonism, just see lots of people working happily with things that they consider very much real. Those who want to argue against Platonism, from this point of view, are not a threat but a welcome opportunity to think about the deep questions of how math, physics and reality are related.

19. **nasren**
January 28, 2014

Yes, it's outside of the maths department that the war is being lost, in the general culture. I have to say I'm very much on Frenkel's side in this. As long as people see maths as a mere game — as Formalists are won't to do — it will be very much harder to bring people into maths in the future. So my concerns are not just about maths at the College level, but how it is perceived in high schools and junior schools. But my frustrations at what is happening at these early levels is beyond worries about abstract ontology.

20. **Jim Holt**
January 29, 2014

To Nasren:
My ad hominem remarks about Gödel and Cantor were meant to be in a jocular vein. (You had to be there.) The serious argument against mathematical platonism is the "epistemic" one: if mathematical entities transcend spacetime and are causally inert, then how do we, as physically embodied beings, come to have knowledge of them? Platonists like Penrose, Gödel, Connes et al. respond to this point by waving their hands and saying consciousness "breaks through" (Penrose) to platonic reality or that we apprehend it in some extrasensory way. One practical issue here is whether (given Gödel-incompletess) we can somehow refer to a platonically unique "intended model" of Peano arithmetic–whether, that is, we have an absolute notion of finitude.
Cordially, Jim Holt

21. **Prof. David Edwards**
January 29, 2014

You might find it interesting that

Platonism Is the Law of the Land, David A. Edwards
http://www.math.uga.edu/%7Edavide/Platonism_is_the_Law_of_the_Land.pdf

22. **anon**
January 29, 2014

Well, Quine and Putnam may not be nuts, but then again neither is Atiyah, and he does not claim to be a Platonist (see, for example, Created or discovered?, Did we invent number theory?, and the first part of The Nature of Space). I generally find it more profitable to listen to the opinions of mathematicians themselves rather than philosophers of mathematics.

I was a Platonist as an undergraduate (it is often said that all mathematicians are when they are young) but I later realized that formalism (which, by the way, is really a term of abuse; Hilbert himself never used it, it was what his critics called him) is the more sophisticated and the better view. I agree with Jim Holt.

23. **Peter Woit**
January 29, 2014

anon,
Thanks for the links to Atiyah, who always has wonderfully insightful things to say. Listening to these though, I'd personally describe his point of view as basically Platonist, while also taking into account some of the subtleties of the question. He describes mathematics as having a "background", "there waiting to be discovered". For him mathematicians do not invent mathematics, what they do is discover it in a certain way, a way that does depend on human beings and their history. His main image is of mathematical research as searching in the darkness for objects to discover. The names given to mathematical objects come from mathematicians, as does the path of the discovery. The way we think about a field of mathematics definitely depends upon its history, what was discovered and named first, what vision the discoverers had of how things fit together when they made their discoveries (as well as what parts of it remain now dark, still to be discovered). But I think the way he talks about this, the picture is primarily Platonist, one of objects out there to be discovered, while acknowledging that the order and way discoveries are made matters (he should know, having discovered more things himself than most people…).

24. **anon**
January 29, 2014

Peter,

I certainly agree that Atiyah takes a more nuanced, middle-of-the-road approach. I would personally say, however, that his position, at least as it seems to me, tends to lean toward the formalist side.

The traditional Platonist view, as I understand it, is that mathematical objects truly exist as Platonic forms and that mathematicians are somehow able to discover them. Atiyah makes the observation that the sense in which they exist is really quite trivial. What we call mathematics is simply an expression of how mathematicians, as human beings, *perceive* the physical world. If mathematicians did not exist, mathematics would not exist. So the way I see it, and this is how I interpret Atiyah's position, mathematics exists as mathematical *language* in the brain of the mathematician. It is, of course, inspired by our experience of the physical world, but to say that mathematics exactly *is* the external world is a jump that is not philosophically justified. One can certainly say that mathematics represents or reflects the physical world, but only in a rather limited sense.

I think many Platonists have the impression that formalists see mathematics as a purely logical exercise in symbolic manipulation that is not connected to the physical world, but that perception is not true. Even Hilbert, of course, loved mathematical physics.

Atiyah himself makes the comparison to metaphysics, in which Kant synthesizes the views of Plato with those of Hume, who denied the existence of the external world and affirmed only our perceptions of it. In this interview, for example, Quine stubbornly insists on holding on to his materialist views, whereas it seems to the interviewer, as well as myself, that idealism is the better view (it is very probable that the powers of our faculties of perception are limited and therefore the true nature of the existence of things is forever beyond our reach – that is the Kantian prohibition, something I think the multiverse theorists should become more familiar with).

25. **nasren**
January 29, 2014

Jim — duly noted. The internet could be defined as chloroform for humour. I am guessing the tone made intentions clear.

My own view on the epistemology question (regarding mathematical Platonism) is that philosophers have screwed that question up for some eighty years or so. Epistemic access does not require causal access, and if it did a lot of non-mathematical knowledge would be in trouble as well. (Like our knowledge that we will die someday: we obviously don't have causal signals being sent backwards from our deaths.) Philosophers have just been dumb about this.

On the Atiyah question I agree with Peter: I don't see how one can listen to that first video "Created or Discovered" and not realise that he is espousing Platonism. The second video does not much modify it. He is and always has been a nuanced Platonist. He doesn't believe in some super-magical ability to contact a spirit realm in which the number two exists but that idea is the nasty caricature of nominalsts — "Plato's Heaven" said in sarcastic tones.

26. **srp**
January 31, 2014

If you would like a science-fictional elaboration of the Platonist/anti-Platonist debate where the consequences really matter, I recommend Neal Stephenson's Anathem. It's also pretty funny in spots.

27. **Bill**
February 15, 2014

Frenkel's article on NYT: http://www.nytimes.com/2014/02/16/opinion/sunday/is-the-universe-a-simulation.html

28. **Anonyrat**
February 16, 2014

While manifolds may exist in nature – we are said to living in one – the open sets, the sets of coordinate patches we use to define describe them are not manifestly visible. In fact, if I understand correctly, the mathematics of manifolds is supposed to be about what independent of these methods of description. Which leads me to two questions –

1. Should it possible to do all of our familiar mathematics without the artifacts such as coordinate systems?

2. If we have to invoke things that do not exist (arbitrary constructions such as coordinate systems) in order to do mathematics, why is it so far-fetched that we have to invoke things that do not exist in nature (mathematics) in order to do physics? That is, just as coordinate patches are artifacts we need to do useful work with manifolds, which are conceived as existing independent of any particular atlas of coordinate patches; mathematics is the artifact we need to do useful work with Nature?

I am not now and never have been a creationist

January 26, 2014
Categories: Uncategorized

Max Tegmark seems to have decided that my criticism here of the emptiness of ideas in his recent book is "similar to hate-mail I've been receiving from a Young-Earth Creationist". Also, the fact that I have fans at a certain Intelligent Design blog shows that I'm "against the spirit of science". Given this, I guess I need to formally make the statement that

> I am not now and never have been a creationist.

More specifically, everything I've seen about Intelligent Design indicates to me it is nonsense, and I've done my best to ignore those who promote such nonsense, whether they link to my blog or not. Sometimes you'll see trackbacks on my blog coming from such links, basically because while sometimes I delete them, other days I think ignoring these people means really not wasting any time at all on them (and I've had my own problems with people censoring trackbacks…). On the larger issue of how to deal with evolution-deniers, one reason I live in New York is because it's easy to ignore such people here, they're not much of a problem. For those living in other parts of the world where they're more of a problem, you have my sympathy and concern. I admire and congratulate people like Lawrence Krauss for their efforts to deal with the problems caused by such deniers. For most of us, I think the best tactic is to ignore them, on the grounds that not doing so will just lead to getting more attention for them and their arguments. As part of this, any comments arguing pro or con about evolution posted here will be immediately deleted.

While I'm not joining Krauss and others in their admirable efforts, I do think there is one way I can contribute to dealing with the threat from this kind of nonsense. If physicists like Tegmark succeed in publicizing and getting accepted as legitimate mainstream science their favorite completely empty, untestable "theory", this threatens science in a very real way. IDers like the bloggers mentioned above now can point to mainstream physics to justify their own untestable "theory", Intelligent Design, or whatever other nonsense they find attractive because they like having a deity around. Yes, physicists need to fight pseudo-science coming from quarters like the IDers, but they at the same time need to fight pseudo-science coming from within their own community, which in the end may be even more dangerous.

Update: It took about 5 minutes for this posting to attract dumb arguments about religion and they're now flooding in. Sorry, comments closed.

No Comments

The Principle

January 26, 2014
Categories: Film Reviews

I just found out about a new film coming out this spring, which appears to exemplify exactly the dangers I was pointing to in my last posting. It's entitled The Principle, and features physicists Michio Kaku, Lawrence Krauss and Max Tegmark, with Kate Mulgrew (aka Captain Janeway) as narrator.

You can take a look at the trailer, this blog, or this interview to start to get some idea of what's going on. The person behind this is Robert Sungenis, a bizarre figure with extreme religious views. He holds a Ph.D from an institution located in Vanuatu, and is an advocate of the idea of "geocentrism", the idea that the Catholic church was right, and scientists since Galileo have got it all wrong (see his web-site Galileo Was Wrong). For another Youtube video explaining what this is all about, see here.

As near as I can tell from all this, without having yet seen the full film, it appears that what probably happened is the following. Sungenis decided that the anthropic principle business in cosmology supported his views, so he went and got physicists like Kaku, Krauss and Tegmark to say silly things on camera, then edited this to suit his case. Maybe the trailer is misleading, and these people actually make a cogent case against Sungenis's nonsense and for solid science, we'll see…

Update: For a different point of view on this, from someone worried that geocentrists will discredit the Catholic Church, see here.

Comments

1. **Shooter**
   January 26, 2014

   I think the whole Galileo affair is misunderstood. The Church already knew of Kepler's view of planetary motions and whatnot, and every cosmologist read his work. As for Galileo, he engaged in scientific misconduct, insulted the Pope, and a lot of other crimes. It is strange that Sungenis isn't aware of these facts.

2. **Chris W.**
   January 26, 2014

   In its underhandedness and general level of intellectual dishonesty this film sounds like a reprise of *What the Bleep Do We Know!?* (released nearly 10 years ago).

3. **David**
   January 26, 2014

   Shooter, that's a mischaracterization of history. The church did not examine whether Galileo had engaged in "scientific misconduct," which of course is a concept that did not exist at the time. The church did indeed know about heliocentrism before Galileo published his book, and in fact Pope Urban asked GG to write about it, with a warning to not endorse heliocentrism. The insult to the pope is that GG wrote a character into the *Dialog* who used the pope's own words – the character was named Simplicio and the pope's words supporting heliocentrism were shown in the book to be foolish. Maybe GG brought it on himself by speaking forthrightly, but nonetheless, his conviction was for promoting heresy (that the earth moved, in contradiction to the bible). The church has, sort-of, apologized for GG's mistreatment, but never actually said, in writing, that he was right and they wrong.

4. **srp**
   January 26, 2014

   Geocentrism? Really? I sure hope they mean that in some metaphorical way or else peoples' neurons will actually die watching this film.

   Of course, by appropriate choice of coordinate system I've been able to define the srp-centric universe which is very convenient for me, but I don't think that's what they have in mind…

5. **Peter Woit**
   January 26, 2014

   Chris W.,
   Yes, this does look like the cosmology version of What the Bleep? (actually, maybe quite a bit loonier than that). I wrote about that film long ago here
   http://www.math.columbia.edu/~woit/wordpress/?p=83

6. **Rick DeLano**
   January 26, 2014

   I am the producer and writer of "The Principle".

   The film is an examination of the Copernican Principle.

   As its title is intended to suggest.

   The precognitive reviewers are of course welcome to try their hands.

   The film will disappoint them, once it is not what they have channeled it to be.

7. **Yatima**
   January 27, 2014

   Meanwhile, the popular press on the popular topic of Hawking on the hot topic of firewalls.

8. **Dom**
   January 27, 2014

   Rick
   Thanks for your reply, one question though . . . What do your last three oddly-phrased sentences mean ?

9. **Peter Woit**

January 27, 2014

Dom,
He's just obviously referring to the fact that people are discussing the film only having seen the trailer.

It's true that until the whole film is available, it will be unclear what role the physicists play in it. There seems however to be a huge difference between the point of view of the physicists and that of the filmmakers, so, I at least am curious to see how that plays out., with one obvious guess that the views of the physicists get misrepresented…

10. **Neil**
January 27, 2014

The contrast between the Copernican principle and the Anthropic principle is interesting. Is our observational vantage point privileged [A], or is it mediocre [C]? It depends. Obviously the most plentiful location in the universe is the intergalactic void, not a planet in the habital zone of a stable star, so the earth is no doubt a privileged vantage point. It is hardly profound to understand why we have it, and if that is the anthropic principle then it is trivial. But asserting a multiverse is something else. We know the intergalactic void exists, we do not know other universes exist.

11. **Rick DeLano**
January 27, 2014

Peter:

Thanks for correctly characterizing your guess as a guess.

I am not guessing.

Your guess is wrong.

"The Principle" is noteworthy, first and foremost, not because of the exceptionally high quality of its interviewees.

You can see most of these guys on the Discovery Channel any night of the week.

"The Principle" is noteworthy, first and foremost, because it is, so far as I can tell, the first film focused on the Copernican Principle itself.

It is interesting that this foundational *assumption* of the modern world should never have presented itself to anyone else as a worthy subject of a documentary film.

Sometimes one has to look at the world differently, in order to see what the consensus doesn't.

I do not expect you to approach my film as you would any other; you will pre- cognitively review it.

That is fine.

You are wrong, and when the film is released you will see just how wrong.

12. **Peter Woit**
January 27, 2014

Rick,
OK, looking forward to seeing how the obvious difference between the point of view of these physicists and that of the filmmakers plays out…

13. **Rick DeLano**
January 27, 2014

Thanks, Peter.

It plays out, as any good drama should, in a wonderful intellectual slugfest that includes excellent punches from all sides.

I know it is hard for some to believe, but I am interested in making an excellent film.

I know it is harder for some to believe, but the Copernican Principle is very seriously challenged by observations…….

and always has been.

14. **CJ**
January 27, 2014

Dear Rick,

You write, "I know it is harder for some to believe, but the Copernican Principle is very seriously challenged by observations."

Where can we read about these observations? Or do we have to wait for the film? Do Michio Kaku and Max Tegmark reveal the Kaku/Tegmark observational satellite in the film, alongside the new data found in a parallel universe?

That alone would be worth the price of popcorn these days. 🙂

15. **Bill**
January 27, 2014

Any publicity is good publicity. Ok, the post about MUH was necessary but, Peter, why not use your connections and the popularity of your blog more often to give visibility to interesting things going on in physics and mathematics instead?

16. **CJ**
January 27, 2014

Dear Bill,

Peter *does* use this blog to regularly report on cool things going on in science and math! Have you read the last several posts?

What Peter also does, which too few folks/"science" reporters do, is call out all the modern-day hokem, hype, and snake oil, sold as science.

Also Bill, when certain folks are using multi-million-dollar war-chests to promote personal, faith-based, science-free initiatives as "science," Peter provides an invaluable service in pointing this out, as such faith-based initiatives both displace and disgrace professional physics and physicists.

17. **Peter Woit**
January 27, 2014

CJ,
If you look at the long interview linked to in the posting, at one point there's a segment from the film featuring Max Tegmark. He's explaining about the "axis of evil", and I gather the film makers see that as evidence against the Copernican principle (although others claim it as evidence for the multiverse and thus for a larger "Copernican principle").

Bill,
The two postings this weekend about twistors and about the debate over Platonism were I thought exactly what you are asking for. Debates over philosophy of mathematics are not to everyone's taste, and I guess there's no form of that debate you can watch, but there was a bit of content in my post amidst the silliness. About twistors, I'll undoubtedly write more sometime, but I highly recommend the Andrew Hodges web-site I linked to. It gives a lot of detail about this story, with excellent references to follow up. It does a far better job than I ever could of writing about the technical aspects of this story.

18. **CJ**
January 27, 2014

Dear Rick,

In addition to including scientific luminaries such as Max Tegmark and Michio Kaku, does your film also include Einstein?

Albert Einstein stated, "Once it was recognised that the earth was not the center of the world, but only one of the smaller planets, the illusion of the central significance of man himself became untenable. Hence, Nicolaus Copernicus, through his work and the greatness of his personality, taught man to be honest." (Albert Einstein, Message on the 410th Anniversary of the Death of Copernicus, 1953)

It seems that many are not content to only attempt to undo the scientific work of Einstein and Copernicus, but they also must undo the spiritual work of Einstein and Copernicus, and "teach men to be dishonest."

19. **Bill**
January 27, 2014

Peter, yes, I did enjoy your post about amplituhedron, and I went to Andrew Hodges website and read his story, which was fun. That post would be even more interesting to me if it had nothing to do with Nima. As for stories involving Edward Frenkel (another common character), instead, I would prefer more updates about mathematics, like recent work of Vincent Lafforgue. In any case, I shouldn't have sounded so critical, since I do enjoy your blog.

20. **Peter Woit**
January 27, 2014

Thanks Bill,
I'd also like an update on the Vincent Lafforgue stuff, unfortunately I don't think I can provide it due to lack of expertise. Every so often I try and ask my colleague Herve Jacquet about it since he is an expert. If I ever understand his answer I'll try and write something here…

21. **Rick DeLano**
January 27, 2014

CJ:

It is difficult, for me, to reconcile Einstein's comment concerning how Copernicus "taught men to be honest" (they were dishonest before Copernicus??), with his own view of reality, expressed below:

"I have come to believe that the motion of the Earth cannot be detected by any optical experiment."

"…to the question whether or not the motion of the Earth in space can be made perceptible in terrestrial experiments. We have already remarked…that all attempts of this nature led to a negative result. Before the theory of relativity was put forward, it was difficult to become reconciled to this negative result."

22. **CJ**
January 27, 2014

Dear Rick,

Firstoff, are you and your film stating that the earth does not move, like the pre-Copernicuns?

As Einstein never stated thusly. Instead, in 1922 he stated, ""I have come to believe that the motion of the Earth cannot be detected by any optical experiment." - http://physicslink.ucsd.edu/students/courses/winter2011/physics2d/einsteinonrelativity.pdf

This has nothing to do with a geocentric universe, and everything to do with the fact that the laws of physics are the same in all inertial frames–a famous postulate of relativity.

Throughout your film, do you just pull quotes at random, take them out of context, and reassemble them at will, having Michio Kaku and Max Tegmark mouth them? While not science, perhaps your film is postmodern performance art?

At any rate, good job on the publicity. I love comedies and am very much looking forward to it on Netflix.

23. **Rick DeLano**
January 27, 2014

CJ:

We do agree that Einstein believed the motion of the Earth could not be detected by any optical experiment.

We apparently differ on whether that fact is of interest, in a documentary about the Copernican Principle.

I understand you to be quite opposed to the idea of a film made by those who view the evidence differently than you do.

Oh well.

On the other hand, the filmmakers consider it important not to impose a particular conclusion on the audience.

You might not like that either.

Oh well.

Thanks, by the way, for your compliment about the publicity.

We have made an interesting film, on a subject that has a great deal of interest, clearly for the public.

24. **Andrew Foland**
January 27, 2014

Anyone interested in observational evidence of motion through the solar system should read about calibration of the WMAP experiment, for instance, at http://oberon.roma1.infn.it/lezioni/laboratorio_specialistico_astrofisica/pdf/lezione12.PDF .

Motion of the earth around the sun shows up as an easily-detected 300 uK annual modulation in the CMB dipole data.

25. **CJ**
January 27, 2014

Dear Rick,

You write, "We do agree that Einstein believed the motion of the Earth could not be detected by any optical experiment. We apparently differ on whether that fact is of interest, in a documentary about the Copernican Principle."

Yes, every physicist knows that Einstein's statement that "the motion of the Earth could not be detected by any optical experiment" has nothing to do with the Copernicun Principle, and everything to do with Einstein's Theory of Relativity.

And yes, I almost agree with your second statement, but there's a typo. You meant to write, "We apparently differ on whether that fact is of interest, in a comedy about the Copernican Principle, starring Max Tegmark and Michio Kaku."

If one's goal is to make a bit of money by taking Einstein's words out of context and leveraging the general public's (& general science reporters' and even some famous professors') lack of knowledge regarding science, Copernicus, and relativity, then I imagine you are quite pleased with your creation.

I mean, after all, what the bleep do we scientists know?

26. **Rick DeLano**
January 27, 2014

CJ:

Pre-cognitive reviews are always interesting, I will certainly add yours to my collection.

The film will be released this spring, and then we will publish all the pre-cognitive reviews.

Thanks!

27. **Rick DeLano**
January 27, 2014

@Andrew Foland:

Thanks for the interesting link.

Our film examines the Copernican Principle, and the question of the motion of the Earth is a crucial part of the history of the development of that principle.

What the evidence shows is that there is a relative motion between Earth and CMB.

This is interpreted in your link as a motion of the Earth wrt the CMB.

I would be curious to know whether any other possible explanation might exist, because if not, then we have established Einstein to have been in error when he stated that no optical experiment could detect the motion of the Earth.

Is it your understanding that Einstein is proven wrong in this assumption by the data presented at your link?

28. **CJ**
January 27, 2014

Dear Rick,

My words are not a pre-cog review.

Rather, my words are pointing out not only your complete lack of understanding of the Principle of Relativity and one of its Postulates, but your lack of character and honor in actually creating an entire film based off faulty science to both mislead the general public and make money off them.

You are the one stating that your film conflates Einstein's Postulates of Relativity with a stationary earth in a geocentric universe which denies the noble work of Copernicus and countless others.

You are telling us that this is the central premise of your film, and we are merely pointing out the fallacy of the central thesis which you claim underlies your film.

Thus our words are not a pre-cog review of your film, but rather they are a post-cog review of you, your misguided words, and your unscientific thought process.

Posting your extremely severe misunderstandings here a few years ago could have saves a lot of time and trouble. 🙂 But as your film will help Michio Kaku and Max Tegmark sell more sci-fi books, all is not lost. 🙂

29. **Danny**
January 27, 2014

It would be a tremendous disappointment, and a minor betrayal of our ethical commitment to society, but perhaps there should be a discussion in the community about closing ranks — and not saying anything stupid, at all — until such time as the current flock of hypothetical ideas are either experimentally verified or put on a solid theoretical foundation? As it is, idiots like Mr Delano and poor media coverage are causing too much damage to the long-term health and reputability of our field.

30. **Rick DeLano**
January 27, 2014

CJ:

I must say you are approaching hysteria in your pre-cognitive reviews.

What is it that has you so profoundly disturbed by a film which recounts the history of how we view our place in the cosmos?

One that you have not seen?

One which you insist you understand better than I do, when I wrote and produced it?

One of the interesting aspects of "The Principle" is the truly remarkable degree of passion it seems to stir up.

I think this is going to be an important part of the story.

31. **Rick DeLano**
January 27, 2014

@Danny:

The ranks do not seem closeable to me.

There is a growing sense of unease, in the minds of the best theorists, about the foundations upon which our cosmology rests.

This is understandable.

96% of the universe's mass/energy must exist in undetected forms, in order for the consensus cosmology to stand.

We haven't found the easier form, dark matter, despite 50 odd years of exquisitely sophisticated attempts.

I don;t think the ranks are closeable at this point.

I think we live at one of those remarkable times when real change is in the air.

I hope so.

32. **Jesper**
January 27, 2014

Reading comments from Rick DeLano sounds an awful lot like climate-change deniers and creationists, now going for Copernicus – this is one that I didn't see coming.

33. **Michael Moyer**
January 27, 2014

In the January issue of Scientific American we published a feature story titled "The Case Against Copernicus," which detailed the excellent (as of the 17th century) scientific arguments against the heliocentric universe. The story is online here, although it's behind our subscriber paywall:

http://www.scientificamerican.com/article/the-case-against-copernicus/

(If you are at an institution that subscribes to Nature you can also get it via the Nature site.)

Here's the story in brief: In a heliocentric universe, we should observe annual parallax as Earth goes around the sun. Astronomers of the 17th century could not detect parallax, implying that the stars were unimaginably distant. Since each star has a finite apparent width, this width and their distance implied that the stars should be unaccountably large–many orders of magnitude larger than the sun. This argument, along with a few others, kept heliocentrism from being adopted more widely. (Also, Tycho Brahe had come up with a hybrid model of the universe—geo-heliocentrism—which was the preferred alternative until the 18th century.)

Of course, the star size problem was illusory. The apparent width of stars was due to diffraction. Parallax was eventually observed. And Newtonian gravity provided an explanation of how a heavy, solid like Earth could move in an ellipse through space.

Now, why anyone in the 21st century would doubt heliocentrism is beyond me. I agree—this documentary sounds a lot like What the bleep do we know? But then again, I admit that I am "pre-cognitive."

34. **CJ**
January 27, 2014

Dear Rick,

I understand the panic that is now setting in as you are becoming aware that the entire thesis of your film, which you shared with us, is based on a gross misunderstanding, whence you mistakenly conflated Einstein's Principle of Relativity with a geocentric, pre-Copernicun universe.

Rick–the great thing about science is that there is no shame in admitting you were wrong, correcting your mistakes, and moving forward.

As there is plenty of time before your film is released, you have plenty of time to add a disclaimer at the beginning, stating:

"Shortly after completing this film, it was brought to my attention that when Einstein stated that "the motion of the Earth could not be detected by any optical experiment," in a 1922 lecture on the origins of Relativity, he was not opposing the Copernicun view nor stipulating that the earth was stationary nor at the center of the universe, but rather, he was restating one of the postulates of relativity that states that 'The Laws of Physics are the same for All Inertial Observers.'"

Then, Rick, before you cut to Max Tegmark mimicking an explosion or the big bang, you could present the audience with another piece of information you just became aware of today:

Albert Einstein stated, "Once it was recognised that the earth was not the center of the world, but only one of the smaller planets, the illusion of the central significance of man himself became untenable. Hence, Nicolaus Copernicus, through his work and the greatness of his personality, taught man to be honest." (Albert Einstein, Message on the 410th Anniversary of the Death of Copernicus, 1953)

Dear Rick, please feel free to use this invaluable science freely. I need no reference nor payment for it. All I ask is that, in the spirit of science, you share it freely with your audience in your film, as I shared it with you.

Best,

CJ

35. **Jesper**
January 27, 2014

This is probably hugely off-topic (and just off in many other ways), but since you've come to discuss the more colorful "science films" I thought I'd mention the movie "Hortons hears a who". I watch the movie as a funny and cute analogy to the string theory landscape and all the multiverse stuff. Especially viewable is the 59th minute, where there is a fantastic view of the entire landscape (zillions of universes, all pink!), and also the fantastic Katie line ("In my world , everyone eats rainbows and poops butterflies") at the 29th minute. Seriously, its a must watch for any serious physicist.

Sorry – couldn't help it 🙂

36. **Rick DeLano**
January 27, 2014

@CJ:

I am increasingly amused by your observations concerning my film, but it seriously appears to me that "panic" is something more appropriately describing you here.

But we do seem to be talking past each other at this point.

I assure you, no such disclaimer as you propose would be necessary, or even appropriate, given the development of Relativity in the historical context of the failure of all experiments to detect the universally-assumed motion of Earth around Sun.

That historical context is what is present in "The Principle", and absent in your panic-stricken attempts to tell us what it is pre-cognitively.

37. **Dom**
January 27, 2014

I am reminded of one of Tom Tomorrow's excellent cartoons regarding a similar subject
[Sphericated v Flaticular]

38. **Rick DeLano**
January 27, 2014

@Michael Moyer:

Certainly very few things have ever been as plausible as the Copernican notion of heliocentrism, as developed successively by Kepler and Newton.

Remarkably, the only nail that never could get hammered down was the direct measurement of that universally-assumed motion of Earth around Sun.

Everything was perfectly plausible.

But the motion was never measured by any experiment.

This seemingly minor detail destroyed Newton's absolute space, and required us to either (a) admit the Earth was not orbiting the Sun, or (b) insist that absolute motion was a fiction, and that physics could account for parallax, for example, no matter whether the Earth was taken to be fixed, or the Earth was taken to be moving.

Einstein makes this point here:

"The struggle, so violent in the early days of science, between the views of Ptolemy and Copernicus would then be quite meaningless. Either CS [coordinate system] could be used with equal justification. The two sentences, 'the sun is at rest and the earth moves', or 'the sun moves and the earth is at rest', would simply mean two different conventions concerning two different CS [coordinate systems]."

Certainly Einstein does not agree with you, that parallax constitutes that "optical experiment" which detects the motion of the Earth.

Einstein came to believe no such experimental demonstration was possible.

Do you disagree with him?

39. **CJ**
January 27, 2014

Dear Rick,

Yes, "are talking past each other," as I am quoting Einstein on Relativity and Copernicus, while you are warping words to merely build hype for a film based on a primal, embarrassing misunderstanding and misconception–a true rookie mistake that any freshman physics student would have quickly realized, and corrected their erroneous ways.

However, the horse has left the barn so there is little use in shutting the door, and with the luminary cast of Tegmark/Kaku et al., the film should bring you a modestly handsome monetary profit, and an even greater sense of accomplishment along your bizarre crusade against science, philosophy, honor, physics, and reason. 🙂

40. **CJ**
January 27, 2014

Dear Rick,

Are you really

1) insisting that the Earth Does Not Move.
And that 2) Einstein actually stated that there is no proof that the earth moves?

Because Einstein never said thusly, and too, the Earth Moves.

Rick, your vast and resounding errors and misunderstandings have been shown, but now you are treading on new ground, in not only being wrong, but exacerbating your dire, desperate situation by peddling and promoting a film which knowingly misleads and misinforms the public, all for private profit. Is it legal to profit off of fraud?

41. **Rick DeLano**
January 27, 2014

CJ: "I am quoting Einstein on Relativity and Copernicus"

Einstein on Copernicus: "The struggle, so violent in the early days of science, between the views of Ptolemy and Copernicus would then be quite meaningless. Either CS [coordinate system] could be used with equal justification. The two sentences, 'the sun is at rest and the earth moves', or 'the sun moves and the earth is at rest', would simply mean two different conventions concerning two different CS [coordinate systems]."

42. **Rick DeLano**
January 27, 2014

CJ:

It is illegal to profit off of fraud.

You could, if you wish, allege it in court.

We would certainly be prepared to respond.

But, should you ever wish to seriously (in court) allege our film to be fraudulent, may I suggest you watch it first, before filing your claim.

Just sayin'…..

43. **CJ**
January 27, 2014

Dear Rick,

When you write,

"Einstein on Copernicus: "The struggle, so violent in the early days of science, between the views of Ptolemy and Copernicus would then be quite meaningless. Either CS [coordinate system] could be used with equal justification. The two sentences, 'the sun is at rest and the earth moves', or 'the sun moves and the earth is at rest', would simply mean two different conventions concerning two different CS [coordinate systems].""

You are merely restating Einstein's Postulate of Relativity that the Laws of Physics are the Same in all Inertial Frames.

Then, you are misinterpreting, mis-extrapolating, and misappropriating the quote to teach blatant falsehoods as truth, denying that the earth moves, while stating that Copernicus was wrong.

Dear Rick, do you also claim that the earth is flat after you drive down a straight road with Max Tegmark for a half hour in your film? Also, do you set up a camera at the beach whiel sunning with Michio Kaku to record the rising and setting sun, to show that really the sun is moving, while the Earth is stationary? 🙂

What was the budget on your foundationally fallacious film? & who funded it? Certainly not physicists!

44. **Rick DeLano**
January 27, 2014

CJ asks if I am "1) insisting that the Earth Does Not Move."

>> Einstein insists no experiment can *detect* any such motion. I insist that is a very interesting thing to know, in the context of a film about how we have come to view our place in the cosmos.

"And that 2) Einstein actually stated that there is no proof that the earth moves?"

>> Here is what Einstein actually stated. I know you have a hard time believing him, but that is your problem, not mine:

“I have come to believe that the motion of the Earth cannot be detected by any optical experiment.”— Albert Einstein

45. **Anonyrat**
January 27, 2014

Mr DeLano, on Google Books, there is a book from 2005 “Physics Before and After Einstein” edited by Marco Mamone Capria. A passage in there seems specially addressed to you (see section 14 of chapter 5), titled “Was the Copernican Controversy a Pseudo-Problem?”
http://books.google.com/books?id=SdLLsj-0rqIC

Since this was written years before your film was a gleam in your eye, you can hardly claim “pre-cog review”. And it specifically addresses the Einstein-on-Copernicus quote that you are hawking.

The key quote is: “Coming back to the main physical issue which was at stake in the Copernican controversy, does general relativity make any difference to the distinction between locally inertial and non-inertial (e.g., rotating) systems? Not much. As two Italian physicists pointed out in 1929, in general relativity a locally rotating system, for instance, is as easily distinguishable from a locally inertial system as it is in classical mechanics. In other words, inertial effects exist also in general relativity! If this was not the case, general relativity would simply contradict ordinary empirical evidence.”

46. **Anonyrat**
January 27, 2014

If that was not clear enough, even more explicitly “Our conclusion is that, though repeatedly and authoritatively endorsed, the claim that general relativity finally ‘solved’ the Copernican controversy by showing that Copernicus and Ptolemy were ‘both right’ is based on several mistakes, concerning both the gist of their disagreement and the scope of ‘general covariance’. However such a claim was probably decisive in winning for Einstein’s theory a cultural and philosophical place second to no other physical theory of his time.”

47. **Anonyrat**
January 27, 2014

Regarding Einstein’s “no optical experiment can detect the motion of the earth” – he could not have meant that in general, because he himself gave an explanation in special relativity of stellar aberration. See the Wiki article:
http://en.wikipedia.org/wiki/Aberration_of_light

48. **Peter Woit**
January 27, 2014

CJ/Rick,
Enough, you really need to find another topic to argue about.

To say one thing in favor of these filmmakers, I don’t think this is going to make them rich, seems unlikely they’re doing it for the money…

49. **kashyap vasavada**
January 27, 2014

@ Rick DeLano: I am reading very interesting debate between you and other readers of this blog. I do not know about your scientific literacy, but it seems that you did not take physics even in high school. Correct me if I am wrong. Any way let me point out something which surprisingly other people have not mentioned. Do you believe that earth is very small compared to the sun and there are stars which are millions of time bigger than sun? Even if we ignore all scientific theories, is it possible that everything in the entire big universe revolves around the tiny little earth? This would totally defy common sense.

50. **Bill**
January 27, 2014

It’s interesting that for some people geo-centric movement of the solar system looks just as natural as the helio-centric movement.

51. **Anonym**
January 27, 2014

Rick, it does not follow that the earth is stationary, even if we cannot because of the equivalence principle prove that it moves. A moving earth or a non moving earth are both valid view points, but neither one is true in any absolute sense.

The claim that “Galileo was wrong — the church was right” found on your web page is thus false, since the church claimed that the earth stationary. (You have to claim either that “Galileo and the church were wrong” or that “Galileo and the church were right” to make sense, if you appeal to the equivalence principle.)

So your own wise ass appeal to the equivalence principle thus turns against your case for “geocentrism”.

52. **Yatima**
January 27, 2014

“I am increasingly amused by your observations concerning my film, but it seriously appears to me that “panic” is something more appropriately describing you here.”

I am getting flashbacks to the deepest infernos of alt.science.alternative, complete with fund drives for zero-point energy devices.

53. **max**
January 27, 2014

While you are no doubt correct about the dubious motivations behind this project, the scientific and philosophical stance is an interesting one from the point of view of quantum gravity. If one allows that local observers (on the earth) are a key feature of the quantum theory, and that one must understand how this fits into a human cosmology (not necessarily a stupid stringy multiverse), then there is a real sense in which the earth is special (for us). After all, classical spacetime is supposed to emerge from other data.

Moreover, in Galileo’s trial, which was actually easy for him because he was quite friendly with the church, it was clear that he did not yet understand his relativity. He actually insisted that the earth moved. So he was wrong, no matter how you look at it.

54. **Roger**
January 27, 2014

Scientists and others have been using Copernicus, Galileo, and Einstein as examples to show various points for a long time. Usually the facts and the science get distorted so badly that the lesson is hopelessly confused. My hunch is that this movie will also get the story confused. But maybe we just defer judgment, just in case the movie gets it right.

55. **Rick DeLano**
January 27, 2014

@Anonyrat: Interesting quotes. The same forces arise, regardless of whether the Earth or the cosmos is taken as rotating. Einstein:

"One need not view the existence of such centrifugal forces as originating from the motion of K' [e.g.-the Earth]; one could just as well account for them as resulting from the average rotational effect of distant, detectable masses as evidenced in the vicinity of K', whereby K' is treated as being at rest."

@ Peter: You are quite right. We never in our wildest dreams expected the film to generate the kind of interest it has, even at this extremely early stage. No car crashes, no love scenes.

@kashyap: Yes, I decided to let the physicists speak in the film since, as you correctly note, I doubt anyone would much care about what I might have to say, although I have enjoyed thinking about these matters over the last several years.

You ask a very interesting question: "Even if we ignore all scientific theories, is it possible that everything in the entire big universe revolves around the tiny little earth? This would totally defy common sense."

I think it is very safe to say that consensus cosmology totally defies common sense.

Relativity certainly defies common sense.

Quantum physics might be the death of common sense.

The idea that the Earth is at the center of the Universe and not moving made perfect common sense to almost every person who ever lived….until Copernicus.

The idea that the Earth is at the center of the universe and not moving now defies common sense.

That alone is a very interesting thing to think about, since the change was clearly not the result of any experimental detection of the motion.

It is a very interesting story.

I think we do a very good job of presenting it to the general audience in "The Principle".

56. **Anonyrat**
January 27, 2014

Mr DeLano, read the whole chapter. Macy's Principle is not borne out by General Relativity.

57. **Peter Woit**
January 27, 2014

Since I allowed Yatima earlier to introduce a link to a story about Hawking's recent ideas on black holes (which I understand not at all and have nothing to say about…), I suppose I should provide this link, from the Borowitz report

http://www.newyorker.com/online/blogs/borowitzreport/2014/01/stephen-hawkings-blunder-on-black-holes-shows-danger-of-listening-to-scientists-says-bachmann.html

There are rumors it is satire, but these days in this subject, figuring out what is satire and what isn't can be tricky…

58. **abbyyorker**
January 27, 2014

Of the three scientists in the trailer, we right away stopped mentioning Krauss. Then Kaku vanished in half the posts. So in the end, Tegmark alone is tarred with the "Rick" brush.
Despite his crimes, I don't think this is especially classy.

59. **paddy**
January 27, 2014

I think that Peter is teaching himself and us by proxy that one cannot engage these folks in legitimately conversation. Although one's mind does come up with eloquently stated logical flaws in their arguments…it will do no one any good. They answer to a higher truth than truth.

60. **Peter Woit**
January 27, 2014

abbyyorker,

I tried to be even-handed…
It looks like Kaku gets top-billing. My reference to Tegmark's segment was just because someone was asking what the "evidence" against the Copernican principle was, and that was the one thing I had seen.

I have no idea why these three physicists got involved in this, I doubt they knew what it was when they went on camera. It was pretty odd though to run into this right after Tegmark was giving me grief for links to my blog from IDers…

61. **Paulo Guerra**
January 27, 2014

When Einstein said:

"I have come to believe that the motion of the Earth cannot be detected by any optical experiment."

he was refering to the motion relative to the ether, the hypothetical medium in which electromagnetic waves were thought to propagate. Since no such motion could be detected, the ether was not needed.

62. **Chris W.**
January 27, 2014

Peter: You mean rumors that Hawking's recent statements are satire?

Andy Borowitz is a humorist, of course. I think everything he writes for the New Yorker is a gag, not that Michele Bachmann can't keep us supplied with unintentional howlers all by herself.

63. **Chris W.**
January 27, 2014

By the way, what the hell is up with this odd use of the term *precognitive*? I detect the faint odor of Scientology…

64. **Fred Page**
January 28, 2014

@Rick DeLano-

While I agree that I can't comment on a movie I haven't seen, I can comment on your comments here. It's no longer 1922. We presently have multiple probes – including one around Mars, one around Saturn, two around the Sun, and one in interstellar space all of which need to know where the Earth is at any given point so that they can communicate back with the Earth. As they succeed (otherwise we wouldn't get information back from them), our current models are giving us useful answers.

I think On the Revolutions of the Celestial Spheres has sufficient arguments in and of itself. That said, constructing a model with an Earth-centric universe would require rejecting:
1) Newton's theory of gravity
2) Einstein's theories of Special and General Relativity.
3) All our Earth-based gravity-mapping data.

It may be an interesting philosophical and/or mathematical exercise to discard the theory of Gravity, and attempt to create mathematics consistent with all our observations – but with a geo-centric universe. However, scientifically, you aren't going to end up with a useful model in this manner; the GOCE data alone would seem to be an insurmountable obstacle.

65. **Mike Sharples**
January 28, 2014

I am baffled at the complexity of the arguments here.

Rick Delano's central argument is:
"There is no way Geocentricism can be disproved."

Without even wasting breath questioning the validity of that statement simply consider:
In what way does that argument prove Geocentricism? In what way does that argument disprove Heliocentrisism?

The whole is based on a logical fallacy. There is no need for Science to rouse itself at this time.

66. **Rick DeLano**
January 28, 2014

I have really enjoyed this thread.

The degree of mutual (and self) contradiction among the participants is hilarious (we believe Einstein but we know he was wrong when he said it was equally valid to take the Earth as stationary and the Sun as moving- we know Einstein said no optical experiment could prove the motion of the earth but he didn't really mean *no* optical experiment, just whatever optical experiment we think he meant, etc etc etc).

I appreciate all the wisdom, but I must say it is not me you need to persuade.

I personally think you guys are completely lost.

You have no idea how to make the universe obey your theory of gravity.

You are completely certain your theory of gravity must be right- after all it has worked so well for so long.

I can appreciate this. We have seen it before.

Newton, compared to the present consensus, was so much more persuasive, logical, and consistent……

Newton was wrong.

So are you guys.

I hope you enjoy the film.

If not, make your own.

May the best film win.

67. **Mike Sharples**
January 28, 2014

Dear Mr DeLano.

If you cannot prove something is true it does not mean that you have proved it is false. Please apply basic logic before advancing any scientific argument.

68. **GoletaBeach**
January 28, 2014

Einstein certainly must have understood the aberration of starlight. His statement about optical motion was most likely embedded in some sort of context that has now been omitted in this blog.

In any case, science at its core is not a cult of personality. Einstein was no pope or Mormon prophet that was infallible.

My 2 year-old daughter also thinks she is the center of the Universe. Who knows, maybe she is. I don't have the funds to make a popular movie about it, but maybe the Coen brothers will make a comedy about one of their kids who thinks the same thing, I bet they could make a lot more money than this film. I might even go see it.

Once European nobility thought they were god's chosen ones by heredity. Well, if you've assassinated your way to the top, I guess you need some special clemency from god.

Once white people thought they were superior. Once some Aryans thought they were superior. Didn't turn out well (skirting Godwin here!).

At the vary least, anyone claiming that the Earth is special must deal openly and adequately with the possibility that they are fooling themselves in the same manner as those groups. Probably it is impossible to do so.

69. **Dom**
January 28, 2014

Rick
Something I see too often in science-based internet fora is someone who appears, does a Gish Gallop and departs claiming triumph. Usually they claim to be overturning scientific reality but oddly you never hear about them picking up their Nobel Prize.

70. **Katy**
January 28, 2014

See, I have a huge problem with Bill Nye going to the Creation Museum to "debate" evolution. There is no debate. There are no "two sides" to this story. If people are seriously pushing creationism and geocentrism on religious grounds, then they are not doing so because they are misinformed. They've been perfectly informed many times over. You may think you're being altruistic on behalf of science by enlightening these people but you aren't. You are making them think the point is arguable. We should simply condemn people who promote bunk loudly, but not communicate with them directly.

71. **Laymammal**
January 28, 2014

A. If the Earth revolves around the sun, then the sun has greater mass than the Earth. B. If the sun revolves around the earth, then the earth has greater mass than the sun. This has nothing to do with the relativity of motion in different reference frames. It is an either/or.(period)

Accept B, and you contradict so many theories in so many fields from chemistry, nuclear physics, particle physics, astronomy, and others(like Relativity)that you will need a new library of theories to try and overcome all the inconsistencies that appear. You could ignore mathematics and logic if you like as well.

72. **Kavanna**
January 28, 2014

Thanks, Peter, for taking on the Tegmark et al. nonsense. Many of us have been worried for a long time about the lasting damage that string theory and related manias (like the multiverse) would do to the seriousness and credibility of science. We saw it in the 1990s with academic anti-science post-modernism. Creationism and other para-scientific and anti-scientific movements were only a matter of time.

(IDT never seemed like nonsense so much as circular and empty: if true, trivial; if not, no way to know. It has that in common with Tegmark.)

About Galileo: (1) he didn't fully understand the principle of inertia or of relativity — he had it in a half-formulated state; (2) he wasn't exactly friendly with the Church, but did have powerful protectors amongst the secular powers, like his sponsors, the Medici. That protection did him some good. The Church barely noticed Copernicus' book. But it was not in good humor in the early 1600s, with the Reformation, Counter-Reformation, and wars of religion in full swing and various people claiming the right to interpret scripture as they saw fit. Galileo, without fully realizing it, got caught in this showdown.

All part of that drama of the "two swords" in modern Western history, the divided sovereignty of religious and secular authorities that gave birth to modern science and freedom of thought, without either side intending it.

73. **max**
January 28, 2014

Wow, that wikipedia entry on Sungenis is scary. So he understands the dogmatism of sola fide and sola scriptura, but is he free from Catholic dogmatism? Does he think its right to treat women as subhumans by upholding the strict doctrine of the eucharist? Which, by the way, is related to the dogmatic interpretation of trinity supposedly beneath the true reasons for Galileo's home detention. This film could be interesting.

74. **Dave Miller in Sacramento**
January 28, 2014

Peter,

Part of the problem here is that Americans are just not taught in school what the evidence is for the Copernican model. I have been running a little experiment for several years asking non-scientists what the evidence is against geocentrism. Only one guy (a Swedish fellow, who said he did not learn it in school) mentioned stellar parallax. No one has mentioned the fact that Copernicus was able to explain why a number of the Ptolemaic circles just happened to have a period of 365 ¼ days – i.e. there is really only just one such circle, the earth's orbit around the sun,

Of course, most Americans will ignore the geocentrists for the same reason they ignore atheists, Communists, libertarians, and anyone else outside the "mainstream" – they have been told by authority that geocentrism is wrong, and they have learned to accept authority. But, it would be nice if our fellow citizens had learned the evidence, instead of just accepting the diktats of authorities.

I trust your recent comments on this and on the IDers mean that your next book will be a wide-ranging discussion of the sociology of science and pseudo-science? You are certainly gathering lots of material!

Dave

75. **Peter Woit**
January 28, 2014

Dave Miller,

I'm more sanguine about the wisdom of the average person than you I think. They'll ignore "The Principle" for lots of good reasons, from what they've been taught in school and understood, to just being able to recognize flakiness and people pushing religious agendas. I'm much more worried about what the effect is of people seeing prominent, respected scientists pushing pseudo-science. What does a high school science teacher say to students who come to them with some of the latest nonsense about the multiverse?

My favored policy about IDers and all the other varieties of pseudo-science has always been to ignore it, not write books about it. I'd like to see multiverse mania die down to the point where it can be safely ignored, hope we'll see that someday.

76. **srp**
January 28, 2014

I wouldn't worry about the public being converted to geocentrism by this film. There's a bizarre quality to the geocentrism idea given a) our ability to see so much of our galaxy and other galaxies and even detect planets around other stars and b) our records of having landed probes on other bodies in our solar system. Sure, you could plot the whole shebang into some Rube Goldbergian series of motions around a stationary Earth, but even a layman can see how kooky that is given a). As for b), if our feeling of motionlessness on Earth is "real" then what does it mean that people and probes on the surface of the Moon and Mars also feel motionless?

What seemed like intuitive "common sense" before we knew there were other solar systems and before we had visited other bodies in the solar system no longer seems intuitive to average Joe. Out-of-context quotes from physicists aren't going to budge that intuition.

77. **Chris W.**
January 28, 2014

What the Bleep Do We Know!? was a low-budget independent film that ended grossing over $10 million. That's the key to this little scam; it will generate some controversy and publicity, and get a fair number of people to see the film, netting a nice financial return for the producers. That's the way a lot of our economy works; promote some provocative BS and make a tidy little bundle. This film certainly won't be a blockbuster, but it cost a hell of a lot less to make.

78. **Peter Woit**
January 28, 2014

Chris W.,
What the Bleep had as message the idea that quantum mechanics implies anything you want to happen will, an idea easy to get people interested in. This film looks like it's going to be trying to get people interested in the idea that the Copernican Principle is wrong, a much harder sell. Honestly, I suspect the only audience interested enough will be all the outraged physicists. While I'll buy a ticket and go see it if it comes to New York, to see how outrageous the outrage is, I doubt the theater will be full…

79. **paddy**
January 28, 2014

I must admit to learning something in this discussion..thank you Dom for the "Gish Gallop" reference.

80. **Chris W.**
January 28, 2014

You may well be right, Peter. Then again, "Springtime for Hitler" (in *The Producers*) was supposed to be a flop. 🙂

(Actually, this film deserves a comparison to *Battlefield Earth*. On that basis, it will almost certainly be a flop.)

81. **Neil**
January 28, 2014

To me, the case against geocentrism, as if there were any need for one, is the annual seasons. The geocentric theory requires the sun move north and south each year as it orbits the earth, by some unknown reversible force. Surely earth's tilted axis is far more convincing.

82. **zathras**
January 28, 2014

Yatima: "I am getting flashbacks to the deepest infernos of alt.science.alternative, complete with fund drives for zero-point energy devices."

Exactly my thought. I'm expecting Ludwig Plutonium to jump into the discussion any minute now.

83. **Rick DeLano**
January 29, 2014

Geocentrism is quite easy to falsify.

Just measure the absolute motion of Earth.

Of course this creates a problem.

This his supposed to be impossible under General Relativity.

Or it's perfectly possible.

It is and isn't, depending upon which comment you read.

I think you guys are lost.

I don’t think you understand the implications of your own theory, and hence advance contradictions which you ignore, or else do not recognize.

I have asked twice, I ask again:

Is there an experiment that measures an absolute motion of Earth wrt to some rest frame?

If so, Einstein was wrong.

If not, then geocentrism must be assessed on other grounds.

84. **Krzysztof**
January 29, 2014

To me, this Geocentrism hype is a perfect match (or response?) to the multiverse mania. They are worth each other in many ways.

85. **David Bailey**
January 29, 2014

Surely the point is that the earth is not in uniform motion, it is accelerating doe to the gravitational attraction of the sun and indeed the galaxy! Therefore regardless of the equivalence principle, Rick could not be right except for one instant in time!

86. **Rick DeLano**
January 29, 2014

@Kryz: Then falsify both. Experimental evidence please. Oh, you can’t? Then you are opposed on metaphysical/religious grounds. That’s fine. Geocentrism is a profoundly superior metaphysical thesis, vis a vis the multiverse, simply because if we accept the multiverse, then all cosmology becomes geocentric in order to tell us *anything* useful.

The only important *fact* of physics in a multiverse is our location (address, if you like) in it.

@David:

Surely, Einstein already thought of that? Surely, Einstein recognized that centrifugal forces do not allow us to establish absolute motion?

Motion is relative, David, for Einstein.

He has told you so several times on this thread.

Was he wrong?

87. **Manyoso**
January 29, 2014

Hi Rick,

I’ll answer your question!

“Is there an experiment that measures an absolute motion of Earth wrt to some rest frame?”

No!

Now, can you answer a question for me? Is there an experiment that measures an absolute motion of Mars wrt to some rest frame?

If not, it would seem geocentrism has some competition from Mars centrism, no?

Cheers,
Adam

88. **Manyoso**
January 29, 2014

One more question Rick! Can you iterate over that question substituting in turn every celestial body in the heavens? If the answer I each of these questions is similarly “No!”, then what do you think this applies for the theory of geocentrism?

Thanks!
Adam

89. **Manyoso**
January 29, 2014

Err, s/applies/implies/

90. **Bernhard**
January 29, 2014

This may be of interest to people discussing this thread:

http://blogs.discovermagazine.com/badastronomy/2010/09/14/geocentrism-seriously/#.Uuj0bLQo-pp

91. **GoletaBeach**
January 29, 2014

Delano writes… “Geocentrism is quite easy to falsify.

Just measure the absolute motion of Earth.”

I suggest that rather than spending money on a movie, the advocates of geocentrism would do better to fund their own experiments toward this end and publish them in reviewed journals.

“I think you guys are lost.”

Not at all. All data we have in consistent with no preferred frame of reference. Einstein certainly knew that the relative motion of the earth relative to the stars was measured by the aberration of starlight, day in day out by people with simple telescopes.

We’ve measured the relative motion of the sun w/r to the cosmic microwave background radiation. The empirical data is very supportive of no preferred frames of reference.

“If so, Einstein was wrong.”

Einstein was wrong so many times there is no motivation here. His son was a professor at Berkeley and boy you couldn’t talk to him without finding out how wrong Einstein could be.

92. **Rick DeLano**
January 29, 2014

@Manyoso:

No!

Therefore let us repeat MMX on the Space Station. That is all you need to shut me and the geocentrists up.

We have requested this experiment to be done many times.

It has not been done, and so the ball is in your court.

It is not hard to falsify geocentrism.

It simply has not been done.

@ Goleta:

Thanks for refuting Andrew above.

93. **Rick DeLano**
January 29, 2014

@Manyosio:

If Relativity is true, then absolute motion is a question beyond the competence of physics.

If Relativity is false, then experimental tests of absolute motion are possible.;

Which is it, do you think, Manyioso?

94. **Manyoso**
January 29, 2014

Relativity is true and has been demonstrate so by every experiment to date. There have been incredibly precise experiments showing just how successful since Einstein formulated it.

Again, please answer my questions as I answered yours.

If you rely upon relativity to assert geocentrism, how do you account for Mars centrism?

Thanks,
Adam

95. **JG**
January 29, 2014

I don’t have the educational background that most of you have, but I find the argument here interesting sort of as an extreme outlier of quasi-religious pseudo-science positions. Meaning if you go from ID to Creationism to Young Earth Creationism to Geocentrism, you have, I think, finally reached the bottom of the barrel.

Lastly, I understand that you can select a frame of reference, and then play with the mathematics to make geocentrism “work” after a fashion, but can you explain annual stellar parallax with it? Hard to see how that would be done.

96. **Jesper**
January 29, 2014

@ Rick DeLano

I’m wondering, are you fairly familiar with General Relativity? It appears to me that you’re good a quoting Einstein, but are you also familiar with the mathematics?

I’m just curious, because if the answer is no, then how can you oppose something, which you do not understand?

Reading the above (very entertaining and also bizarre) debate, it seems to me that you do not really understand what general covariance actually means.

(Personally, I find Manyoso’s Mars-centrism much more interesting than geo-centrism…)

97. **Jesper**
January 29, 2014

@ JG – yes, you'd think that this is the bottom, but who knows. That anyone would argue for geocentrism at the present moment, where our knowledge of the universe (based on GR) is expanding at such a rapid rate, is really bizarre. But why not go for flat-earth?

98. **emile**
January 29, 2014

This is so bizarre that it has to be some practical joke.

I mean, who in 2014, can possibly doubt mars-centrism?

99. **harryb**
January 29, 2014

A moment's googling of The Principle and its Producer takes you into a rather strange world that honest science ought to quickly walk past, collars up.

Multiverse thinking, and its ilk, is clearly co-opted into this sad cause. Shame on it.

Geocentrism, or whatever awful notion du jour in these worlds, is a dead-end.

The syntax of denial is short cadence it seems.

We can all use abrupt sentences to sound authoritative.

Science is much harder than this – dogma is easy.

Must do better.

You and your guys seem very very lost.

Tennis with the net down.

And so on … ad absurdium.

100. **Jesper**
January 29, 2014

I find this debate interesting – not because of its substance, since it has none – but because it reminds me so much of other debates within science, for instance evolution and climate change. On the one hand there are people familiar with the subject, and on the other hand cranks who talk nonsense – and in the middle are lay people who can't tell who's who.

I always thoughts that if the debate of climate change somehow depended on GR, then "GR-deniers" would quickly emerge and convince the public that there is much doubt and need for further research etc.

101. **Rick DeLano**
January 29, 2014

So.

No experiment can detect any absolute motion of Earth.

Interesting, then, that so many commenters above- including, presumably, at least one contributor to Scientific American- allege the contrary.

"The Principle" will be an excellent remedy for this contributor, along with the one who supplied a link to the CMB data.

"How on earth can anyone believe in this day and age" is not scientific argument.

It is not, even, an argument from personal incredulity, really.

It is, instead a dunderhead's appeal to the peanut gallery.

102. **Manyoso**
January 29, 2014

Jesper,

Glad to see another Mars-centrism recruit!

Still waiting for Rick to answer the simple question I asked him in return for answering his simple question.

Cheers,
Adam

PS: I find it odd for Rick to use relativity as an argument for geocentrism and then to deny relativity when it comes to Mars-centrism…

Here is hoping Rick will give equal time to Mars-centric spokespeople and not let the geocentric folks hog all the time in the movie.

103. **Rick DeLano**
January 29, 2014

Manyoso:

Your question is so simple-minded I assumed it was rhetorical in nature.

Since you apparently think it is instead relevant, I now answer it.

If Relativity is true, then Mars, or Earth, or the tip of Manyoso's nose, are each and all equally justified in being taken as the center of the universe.

Some consider this to be prima facie evidence that Relativity is an absurd theory.

Others consider it to be necessary, if absurd, since in the absence opt Relativity the experimental evidence supports geocentrism.

"The Principle" is a very important film, since the historical context of the adoption of Relativity is apparently virtually forgotten among pop science combox warriors.

104. **srp**
January 29, 2014

Oops.

"If Relativity is true, then Mars, or Earth, or the tip of Manyoso's nose, are each and all equally justified in being taken as the center of the universe.

Some consider this to be prima facie evidence that Relativity is an absurd theory."

Game over. The whole point of the Copernican principle is that there is no center of the universe so every point has an equal right to be called the center. Now the geocentrist's only response is to beg the question–it's "absurd" to say that there's no universal center! But he has conceded the empirical point that Mars-centrism would be equally valid to Earth-centrism using the available data.

These guys are not as good as the IDers at this sort of exercise, although admittedly they've set themselves a much more obviously erroneous position to defend.

105. **Manyoso**
January 29, 2014

Hi Rick,

Thanks for answering my question. I assure you it was not rhetorical.

Since you agree that Einstein's relativity gives no room for the Earth to be regarded as the center of the Universe I wonder why you were quoting Einstein to seemingly bolster the idea of geocentrism. This puzzles me.

To clear up my confusion, can you answer another simple – non-rhetorical – question?

Do you believe that relativity is correct?

Thanks!
Adam

106. **Rick DeLano**
January 29, 2014

Adam:

I am certain that it is not correct.

On metaphysical grounds.

I believe we will- possibly already have- obtained evidence that a preferred frame exists, and that it will be- perhaps already is- possible to establish absolute motion with respect to some preferred frame.

Perhaps that frame will turn out to be the CMB.

Otherwise, it will turn out to be Earth.

But a preferred direction certainly exists in the CMB, and that preferred direction is pointing at us.

107. **Manyoso**
January 29, 2014

Hi Rick,

Ok, well thanks for clearly answering although I am still puzzled as to why you kept quoting Einstein earlier in this thread in support of geocentrism. I could be wrong, but I always thought of Einstein as a big proponent of relativity. Have you unearthed some previously unknown historical data showing that Einstein actually thought the whole relativity thing a bunch of hooey?

I mean you did find it hard to believe Einstein supported the Copernican revolution given the oft cited quote in this thread about not being able to detect the absolute motion of the earth…

I mean you weren't knowingly quoting Einstein out of context to support a theory of geocentrism that Einstein's own theory of relativity disproves were you??!!

Cheers,
Adam

108. **Manyoso**
January 29, 2014

Hi Rick,

One more thing… You say above that you think it might be possible that you'd give up on geocentrism in favor of – forgive me – CMB'ism?

This intrigues me. Can you please direct me to where in the sky this CMB is with respect to the earth so that I might look up the possible center of the universe?

Thanks,

Adam

109. **Rick DeLano**
January 29, 2014

@Manyoso:

Nope. Never misrepresented Einstein in any way.

I left that to the folks on the thread who ignored my direct citations of his work 😉

The CMB is purported to occupy the entire observable universe.

Some have proposed that it might constitute a rest frame; that is, they suppose that there might be a frame where the CMB would present no dipole, and that this frame might be possible to employ as a rest frame to measure absolute motion.

There are a number of serious problems with that, by the way, among the biggest being the remarkable recent papers showing that the CMB dipole cannot be exclusively due to a supposed motion of the local group, but that's another issue.

Of course, if the CMB dipole is confirmed to be an intrinsic anisotropy in the CMB, then all of consensus cosmology is wrong, and the placement of Earth in the cosmos becomes a matter of the worst nightmare of the Relativist coming true.

The CMB would neatly divide the cosmos, along the equinox plane.

The quadrupole and octopoles would, just to prove God has a real sense of humor, point out the ecliptic plane.

Anybody smart enough to measure the CMB and perform a spherical harmonic analysis would find themselves directed to us, from anywhere in the cosmos they might happen to be looking.

Yes, I think "The Principle" is going to be a very interesting film indeed, for the folks who actually *pay* for this remarkable science.

110. **Manyoso**
January 29, 2014

Hi Rick,

"We do agree that Einstein believed the motion of the Earth could not be detected by any optical experiment.

We apparently differ on whether that fact is of interest, in a documentary about the Copernican Principle."

Since you now concede that relativity – and hence Einstein – refuted geocentrism, then why do you think his quote – about relativity – is of interest for geocentrism?

Rick, I am concerned it is starting to look like you took his quote out of context in order to bolster a point that you new was unsupported. I am afraid I can no longer assume good faith on your part which is sad because you came into this thread promising that you did not take Krauss, Tegmark, and Kaku out of context in your film.

Given your behavior in this thread it doesn't bode well for your movie. I hope you did better not taking these scientists interviews out of context than you did Einstein in this very thread.

That's it for me. Have fun with your earth centered universe while I dream of Mars.

Cheers,
Adam

111. **max**
January 29, 2014

DeLano: Of course, if the CMB dipole is confirmed to be an intrinsic anisotropy in the CMB, then all of consensus cosmology is wrong.

OK, now I really am interested, as a proponent of such a CMB picture … with the caveat that we observe Our universe and NOT the one unique universe given to all observers. How long until the film comes out?

112. **max**
January 30, 2014

The Catholic Church might be worried about a successful CMB geocentric cosmology, but on the other hand nobody seems much interested in actually solving serious problems in cosmology.

113. **Patrick Harris**
January 30, 2014

Dr. Woit,

I apologize to add to this already lengthy thread, but I found your recent comments regarding the possible greater danger of recent popularity with untestable theories in fundamental physics, over other attacks on science such as ID'ers, to be noteworthy. Do you think the recent rise in popularity of pseudoscientific concepts such as 'quantum consciousness' and, now, geocentrism is indicative of the issues you've raised regarding recent trends in physics? Or do you think there's still a stronghold within at least the physics community safeguarding the necessary rigor the scientific method?

I'm a bit new to the blog, so I apologize to add any noise that you've already addressed. But I appreciate, as someone who isn't an expert by any means but is nevertheless intensely interested in the subject, a place where I can read about those sharing my concern over recent trends.

Anyway, great blog, thanks for fighting the good fight!

114. **KMS**
January 30, 2014

As is well known, thermal equilibrium states in relativistic theories, like e.g. the CMB, do single out a preferred rest-frame. So in the setup of a relativistic theory (i.e. a theory _agreeing_ with relativity theory) on can indeed in some states single out one frame of reference as a "preferred" one – by referring to a special state. And of course this is nothing special to relativistic physics: Also assuming just the Galilean relativity principle there are many situations where one can single out a reference system by referring to specific objects; examples are the reference system at rest relative to earth or the system at rest relative to mars.

However this is _not_ what the discussion of geocentric vs heliocentric system is about: Of course one is free to choose a reference frame where the sun is moving and the earth is at rest (this is a purely kinematical question).
The question is about dynamics and accelerations: What conventional physics tells us ("the Heliocentric system") is that the sun experiences small accelerations (mainly due to Jupiter) while being orbited by planets experiencing much larger accelerations
("pure" Heliocentrism would be for an infinitely heavy sun experiencing no accelerations at all).
Acceleration however is something absolute also in Einstein's theory of relativity: The magnitude of the acceleration experienced by an observer moving along a worldline (e.g. worldline of the sun or the earth) can not be changed by changing reference frames! And even though the quote by Einstein – given without context – might suggest otherwise: Of course one can measure accelerations – in contrast to uniform motions! – using optical experiments, an example is the Sagnac effect.

115. **Alex**
January 30, 2014

Rick,

when, by a future satellite, the expected v_Earth/c ~ 10^-4 seasonal doppler variations of the CMB dipole frequencies are detected, will you concede that you are wrong? 😀

116. **Alex**
January 30, 2014

erm, that was supposed to read:

"of the CMB frequencies"

117. **kashyap vasavada**
January 30, 2014

@ Rick DeLano: By now it is well known from observations by Kepler satellite that there may be billions of earthlike planets where life could exist. Why wouldn't life exist on these? Would you insist on geocentric idea even if we find life on such planets?

118. **Rick DeLano**
January 30, 2014

@max: We are presently negotiating a (very limited, at least initially) theatrical release. If all goes well, we should launch either the first or second week in May.

@max: I agree with your observations, especially concerning the fear and loathing which recent CMB observations apparently elicit from a certain type of "enlightened and modern" Catholic, which type is frankly unhinged by any evidence which might suggest-horrors!- that the Church actually had the better of the argument with Galileo after all.

@KMS: "Acceleration however is something absolute also in Einstein's theory of relativity"……"absolute" with respect to *what*?

@Alex: Why would such a seasonal signal indicate a motion of the Earth, rather than a motion of the cosmos?

@kashap: Strictly speaking, the existence or non-existence of life elsewhere does not directly bear on the location of the Earth wrt large scale structure.

However, the much more interesting metaphysical question certainly arises:

Given all these billions and billions of planets, perfectly adequate for life, in our own galaxy, some of which would be billions of years older than Earth under consensus cosmology assumptions…………..

Where is everybody?

119. **Peter Woit**
January 30, 2014

Patrick Harris,
I see zero danger that geocentrism and "quantum consciousness" will take over mainstream physics. And I don't think these are more popular now than in the past (the number of followers of "geocentrism" I believe is and always will be minuscule).

120. **Jesper**
January 30, 2014

@ Rick. You write "@KMS: "Acceleration however is something absolute also in Einstein's theory of relativity"……"absolute" with respect to *what*?"

That brings me back to my previous question (which you did not answer): are you familiar with the mathematics of GR? It certainly sounds from your comments that you are not.

121. **Peter Woit**
January 30, 2014

All,
Enough comments that no one in their right mind would want to read. If your comment is not specifically about the film, please don't post it here.

122. **jaco**
February 1, 2014

Peter,
Have you gone off the rails? I am totally shocked you would even mention such a travesty as this film much less lend credence to it on your blog. A little dust up with Max couldn't have changed your focus that much could it?

What's next, alien autopsy?
Sorry. Keep up the (otherwise) good work.
Cheers

123. **Peter Woit**
February 1, 2014

jaco,
You don't seem to have noticed that mentioning travesties is one of the specialties of this blog.
And, this has nothing to do with Tegmark. The idea of a film promoting geocentrism featuring prominent physicists is pretty darn amusing, no matter who they might be. And that trailer is a hoot, I really do hope the thing comes to New York…

124. **Rick DeLano**
February 5, 2014

"The idea of a film promoting geocentrism featuring prominent physicists is pretty darn amusing, no matter who they might be."

The idea of a film about the Copernican Principle *not* involving an examination of geocentrism is pretty darn amusing, no matter how shocking it might be to find that no experimental falsification of the theory has ever been obtained.

125. **Anonyrat**
February 6, 2014

"….no matter how shocking it might be to find that no experimental falsification of the theory has ever been obtained."

Let's ask the question this way: Mr. Rick DeLano, please describe what would constitute acceptable (to you) experimental falsification of geocentrism?

126. **Rick DeLano**
February 6, 2014

Establishment by direct measurement of a motion of the Earth with respect to some absolute frame would do nicely.

127. **Mike Sharples**
February 7, 2014

And because there is no absolute frame, geocentrism cannot be disproved.

And because geocentrism is not disproved it is proved.

(You could easily substitute heliocentrism in those two statements but hey-ho)

I suggest it is time to stop feeding Mr DeLano on this thread. He refuses to engage on viable rules of logic.

128. **Martin**
February 7, 2014

See the great book by Arthur Koestler:

https://archive.org/details/ArthurKoestler-TheSleepwalkers-AHistoryOfMansChangingVisionOfThe

Free download.

The problem with the Ptolemean system (Earth fixed) is that the planets, viewed from Earth, perform weird motions such as retrograde motion. No creationist has ever come up with dynamical laws that would induce such retrograde motion (the planets seem to change their mind; lets go in the opposite direction for a while). The big advantage of the Copernican system is that the motions become much simpler: the planets simply track their courses in the same direction all the time. Later, with Kepler's laws it became even more evident, and then Newton, of course. Several weeks ago I saw the trailer of The Principle; it starts with a lady saying that according to Copernicus the earth does not have a "special" place in the Universe. It's all about Earth and the people on it (not the poor animals, they have not been created in the image of God) HAVING to be "special", which is not such a current idea anymore since Darwin. It is not about rules of logic or about science, it is about feeling.

129. **Rick DeLano**
February 7, 2014

@Martin: see Tycho Brahe.

@Mike: "there is no absolute frame"– please describe what would constitute acceptable (to you) experimental falsification of this assertion.

130. **Anonyrat**
February 8, 2014

@Rick DeLano – define experimentally an absolute frame of reference.

Do you know what an inertial frame of reference is? If not, please see Wiki:
http://en.wikipedia.org/wiki/Inertial_frame_of_reference

Now, neither earth-centric frames of reference nor sun-centric frames of reference are inertial frames of reference. I think the accuracy of our long-term computations of solar system mechanics is in part limited by our limitations in the observational establishment of a suitable frame of reference (so there are small residual non-inertial forces that we fail to account for).

Having said that, for a given size of acceptable error, a sun-centric frame works better as an inertial frame for a much larger volume of space and length of time than does an earth-centric frame.

131. **Anonyrat**
February 8, 2014

@Rick DeLano: here for instance is something about the Radio Optical Reference Frame, constructed using the positions of some 400 extra-galactic objects. http://rorf.usno.navy.mil/

We are told elsewhere (http://www.erikdeman.de/html/sail040r.htm) that the Radio Optical Reference Frame is "stable to at least 0.020 arcseconds per century", and is improving with increased time of observations. In contrast, two previous reference frames, using nearby stars, called FK4 and FK5, drift apart at about 0.085 arcseconds per century.

It is in this sense that I mean that a sun-centric frame is a better approximation to an inertial frame of reference than an earth-centric one; just as a reference frame centered on extra-galactic objects is better than one centered on stars in our galaxy. This is, I think, the modern meaning of the old Copernicus/Ptolemy, etc., debate.

132. **Rick DeLano**
February 9, 2014

@Anonyrat:

It is well known that various frames can be chosen on grounds of ease of calculation or metaphysical preference.

The frame you mention is centered upon an object which is, variously, determined to be traveling at somewhere between 365,000 km/sec and 1,600,000 km/sec.

Clearly, this motion is calculated with respect to some other frame, which is alleged to not be moving at 365,000 to 1,600,000 km/sec.

So, either your inertial frame is a mathematical fiction, very much like the ECI frame for GPS, or else you are telling us that the Sun is not moving at 365,000 to 1,600,000 km/sec.

Which is it?

133. **Anonyrat**
February 12, 2014

@deLay, choice of reference frame is not a metaphysical preference. To correctly account for all forces, real and inertial, one needs to know whether the reference frame is inertial, or exactly how it departs from being inertial. For instance one or both of FK4 and FK5 depart from being inertial (though rotating at a rate of fractions of an arc-second per century).

134. **Rick DeLano**
February 13, 2014

@Rat:

"choice of reference frame is not a metaphysical preference."

>> It certainly is. One must assume a metaphysics in order to choose one's reference frame, if, as Einstein says:

"The struggle, so violent in the early days of science, between the views of Ptolemy and Copernicus would then be quite meaningless. Either CS [coordinate system] could be used with equal justification. The two sentences, 'the sun is at rest and the earth moves', or 'the sun moves and the earth is at rest', would simply mean two different conventions concerning two different CS [coordinate systems]."

" To correctly account for all forces, real and inertial, one needs to know whether the reference frame is inertial, or exactly how it departs from being inertial."

>> Since we are not in a position to account for all forces, real and inertial, involving quasars and radio sources, then we cannot satisfy your demand above absent metaphysical assumptions.

"For instance one or both of FK4 and FK5 depart from being inertial (though rotating at a rate of fractions of an arc-second per century)."

>> The frame you propose is a useful mathematical fiction, very such like the Earth Centered Inertial frame in GPS.

135. **John A**
February 19, 2014

Wait. The website, Gallileo Was Wrong is NOT satire?!?

136. **Makka**
February 21, 2014

Krauss has been quoted as saying the CMB from wmap data could indicate we are att he centre of the universe, due to the alignment with the ecliptic.

While i find it interesting, surely the CMB as shown by planck satellite confirm this axis this is worrying for the current theories out there.

Does anyone have any information what krauss is currently saying regarding the planck satellite findings?

Scientific Bookstores, RIP

January 29, 2014
Categories: Uncategorized

A few days ago I tried to stop by the Barnes and Noble store here in New York at Fifth Ave. and 18th St., just to find that it had closed earlier this month. This was the first book store I had access to as a high school student that had a serious collection of math, physics and astronomy books, and I've been buying such books there for about 40 years. The huge 18th St. store dates back to 1932, and by the early 1970s was the only Barnes and Noble store, at the time that the company was revived and started its huge expansion.

With the closing of this store, there now are no longer any bookstores that I'm aware of in New York City that have a large collection of technical math and physics books. Other Barnes and Nobles like the Columbia bookstore have a smattering of such books, and the Strand has a large collection of used and remaindered books, but that's about it (maybe a reader will tell me about a place I don't know). At one point in a long-ago golden age there were several bookstores here devoted to scientific and technical books, including Book Scientific and the McGraw-Hill book store.

The same phenomenon is taking place around the country. Cody's in Berkeley is gone, and if there's a good technical book store in the Bay area now, I don't know about it (but haven't spent much time there in quite a while). Among the places in the US I regularly travel, the only bookstore I can think of that still carries quite a few math and physics books is the Harvard Coop (also some at the MIT outpost). Other countries may be doing somewhat better, with several such bookstores surviving in Paris at least (Gibert Joseph and Eyrolles for instance).

Of course the reason for this is the internet, more specifically Amazon and the online Barnes and Noble. These do have their virtues, and allow fairly quick access to a much more vast array of technical books than any physical bookstore ever could. But the loss of the experience of being able to spend an hour or so browsing through books, with the serendipity of finding something unexpected (something that Amazon's finely tuned algorithms wouldn't ever present to you) is a very real one.

RIP New York technical bookstores, I must find a way to get to Paris more often…

Comments

1. **Douglas J. Keenan**
   January 29, 2014

   I am sorry to hear about the NYC Barnes and Noble and the McGraw-Hill Book Store. In London, the main Foyles store still has a decent, though not great, math section.

   About being able to browse serendipitously, I agree that is valuable, and it is infeasible with the current Amazon. This could be argued, though, to be just a weakness in the construction of Amazon's site. They should have a "serendipitously browse in related books" button. Maybe if enough people suggest it to them, they will add it.

2. **A.J.**
   January 29, 2014

   Black Oak Books on San Pablo Ave in Berkeley has a pretty good collection of math books.

3. **Zathras**
   January 29, 2014

   Back in the 90s Borders (and to a somewhat lesser extent Barnes & Noble) had really solid technical sections. Physics and Math sections could have hundreds of technical books. By 2000 these were much less, and by 2005 these were mostly gone, aside from the odd book (Misner Thorne Wheeler always seemed to be in stock). I really miss browsing these.

4. **Zathras**
   January 29, 2014

   "But the loss of the experience of being able to spend an hour or so browsing through books, with the serendipity of finding something unexpected (something that Amazon's finely tuned algorithms wouldn't ever present to you) is a very real one."

   In artificial intelligence the distinction is made between supervised and unsupervised learning. The distinction between Amazon's algorithms and browsing on your own is a direct parallel to this distinction.

5. **Jamshid**
   January 29, 2014

   I live in the Bay Area, and as for physics and math books, I can't think of any good ones. But go down south to silicon valley, and of course you will still find a great technology bookstore (programming books….). Check out DigitalGuru in Sunnyvale. I think it even had a few math/CS books on one wall.

6. **Bobito**
   January 29, 2014

   If it weren't for pirates from east of the Rhein, many of us would have no access to scientific books at all.

7. **Adam J Calhoun (neuroecology)**
   January 29, 2014

   There is always Powell's. If you don't make it up to Portland enough, then you should start finding your way there more often!

8. **Kartik Prabhu**
   January 29, 2014

   What about NY Public Library? Do they not carry technical books on math and physics?
   This was pointed out to me by [@craigmod](#) *on Twitter* [*here*.](#)

9. **AcademicLurker**
   January 29, 2014

Back when I was a grad student, the first Borders in the Baltimore area opened and they had an excellent math/science section. In fact, a better selection of technical books than the university bookstore.

The OP is right that, these days, visiting the Harvard coop when you happen to be in Boston is about the best you can do.

10. **Peter Woit**
January 29, 2014

Kartik Prabhu,
What's happening to research libraries and their technical book collections is another, much more complicated story, which I don't want to even try and get into here. In any case, few such libraries have ever been well set up for browsing recent technical books, which is what has been lost by the loss of these bookstores.

11. **Heinz Lackner**
January 29, 2014

I do not understand their business model but at Science Books Online you can download pretty much everything ever published for free. There is no way that Cody's, I miss it too, could compete with online sources like that. My last time at Cody's was when Brian Greene was peddling his book about The Elegant (stringy) Universe.

12. **Kevin**
January 29, 2014

When my dad used to live near Columbia on 114th St. I would go to Papyrus bookstore, which Yelp says is still there. I recall them having lots of science books. Maybe I'm just hallucinating though…

13. **Peter Woit**
January 29, 2014

Kevin,
That store is now called Book Culture. It's a good book store with an academic bent, but these days only a small collection of technical math books (and just about none in physics or other sciences).

14. **John McVirgo**
January 29, 2014

I see myself and others partly responsible for this.

In the past, I'd go into a good academic place like Blackwells, browse the books, and then buy it quite a bit cheaper from Amazon. Nowadays, I'll deliberately order a more expensive book from the place, out of appreciation and concern for the people that work there that make real world browsing possible.

15. **Greg Lee**
January 29, 2014

Price is the obvious cause for the demise of technical bookstores, but just as important is a reduction of their role in discovery. The specialty buyers employed by bookstores acted as scouts for the customers, leading to the serendipity and pleasures of browsing. Today I rely on blogs such as our host's, and I'm probably as well informed about new and interesting books as the specialty buyers once were at Cody's, Printer's Inc, and Stacey's—Bay-area bookstores that once had killer math and computer book sections.

16. **BobDastro**
January 29, 2014

I am dating myself here but…
When I was a Physics grad student at Columbia in the 1970s, the University Bookstore was not the best place to browse for books. For browsing, I used to patronize a scientific bookstore somewhere in the East Village not too far from NYU (on Christopher St., maybe?). They had an enormous selection, and one of the pleasures of those days was impulsively buying physics and mathematics texts that were not required for my courses but which were considered important standard references. I acquired quite a hardcover library, including Dirac's *Principles of Quantum Mechanics*, most of the volumes in Landau and Lifschitz's *Course in Theoretical Physics* library, and other similar works.
Even factoring in inflation, these books were remarkably inexpensive (L&L's *Nonrelativistic Quantum Mechanics* was $15, according to the flyleaf on my copy). Alas, these affordable prices did not survive the inflationary spikes of the mid-70s. One reason specialized bookstores are becoming so rare may be that the impulsive acquisition of academic books has become very challenging from a purely financial standpoint.

17. **Shantanu**
January 29, 2014

Peter, just to clarify : are all B &N bookstores in NYC (and elsewhere in USA) closed down or just the one on 5th and 18th street?
There used to be a nice B & N book store
near BU which had a great collection of technical books were I used to sit for hours.
Hope its still there.
Anyhow you are still lucky than those of us based in germany (and other non-english countries except probably Paris) where its very hard to find any decent bookstore (with English language technical books) to do the sort of browsing.

18. **Peter Woit**
January 29, 2014

Shantanu,
It's just the 5th Ave and 18th St. one that has recently closed. This was an unusual one, besides being historically the oldest, it also was the main one that historically had textbooks for lots of NYC colleges. I think part of the reason they closed is that the textbook business is collapsing, as students get their course books on-line.

In recent years a couple of other Barnes and Nobles in the city have closed, but most are still in operation. It's definitely a different environment though than twenty years ago, when they were opening up new ones everywhere all the time.

19. **Richard Séguin**
January 29, 2014

University Bookstore in Madison WI used to carry a large number of math and physics books and I spent a lot of time there browsing, sometimes discovering interesting books that probably would not otherwise have come to my attention by other means. They also had annual Springer sales. Over the years, the math section shrank to a joke, and finally completely expired. I occasionally found something interesting in Borders, but that large building is now office space. Barnes and Noble carries nothing interesting. Paul's used bookstore has an odd assortment of mostly older books. Sad.

20. **Pavel Krapivsky**
January 29, 2014

There is a nice B & N book store near BU (right at the exit from the Kenmore station) which still has a good collection of technical books. Perhaps less rich collection than the one at Harvard coop and the one at Gibert Joseph in Paris (on boulevard Saint Michel), but good enough to spend an hour. Overall, there are of course less and less of such bookstores. I remember a wonderful specialized little bookstore Quantum Books here in Boston (more precisely, in Cambridge), but it is gone…

21. **Patrick**
January 29, 2014

Raven Used Books in Boston has a great selection of math/science books. There's one in Harvard Square about a block from the COOP and there's one on Newbury St. in Boston proper. Most of the technical books are Dover or university press; lots of monographs too. Not quite as extensive as the COOP, but still a really good selection that's often cheaper than Amazon. Also usually has some interesting obscure texts. Almost always serendipitously find something unexpected when I go. The Barnes & Noble in the Prudential Center is also still around.

22. **OMF**
January 29, 2014

The instinctual urge here is to blame the internet and the likes of Amazon.

Personally I think people just aren't buying books anymore.

23. **Don Jennings**
January 29, 2014

I used to think I loved books. My office bookshelf is indulgently stacked with about $20k worth of maths & physics titles. Now I realize I hate them. They're overpriced, heavy, unsearchable, fragile, don't stay open properly, and don't backup or synchronize with anything. I'd happily trade all mine for the corresponding pdf's, if only IP lawyers would get out of the way.
It's the ideas in books that I like, not their physical manifestations.
Yes, browsing in traditional bookstores is a nice idle luxury for us affluent urbanite first world academics with relatively mainstream interests and strong backs and bank accounts, but overall I think the world will be better off when they're gone.

24. **Sebastian Thaler**
January 29, 2014

Peter,

I had the exact same experience last Friday evening–went down to Fifth & 18th after work and found that B&N closed. Very surreal and sad; it was also my favorite B&N in NYC for its huge science and philosophy sections (luckily, Book Culture is excellent for the latter). Especially infuriating was that the Union Square North store nearby–which I've never especially liked–did not have the new Rubenstein book on multiverses in stock, despite it being available on both Amazon and B&N…

BobDastro,

I wonder if you're thinking of National Book Stores (yes, plural), located on Astor Place just west of Cooper Square. First bookstore I ever browsed that had the complete collection of Dover science paperbacks. On its last legs in 1986 and closed soon afterward. It's now a Starbucks, naturally…

25. **Michael Gogins**
January 29, 2014

The problem with publishing, and this goes for music scores, books, fine art prints, and music recordings, is that there is a serious competition underway between two legal and business models of publishing.

In the older model, authors produced works that agents peddled, editors or A&R people bought, publishers published, critics reviewed, stores stocked, and people browsed and bought. In the newer model, anything that can be scarfed up for free is put online as a vehicle for advertising. In the newer model, the publications are instantly available, either free or very cheap, and of lower average quality.

In both cases all original works are copyrighted because simply producing something entitles you to its copyright, but in the second model, the payment to the author is fame, more than it is money.

With respect to scientific publishing, this is of course not the whole story, because academic publishing has a variation on the older model described above, in which a captive audience is charged a premium for works that they mostly produce themselves. But the fame compensation, in a general and honorable sense, is also the main driver for authors in the scientific publishing model.

There are two things that frighten me about the new model.

The first one is that the good stuff will be lost in the shuffle. Whenever I try to look for works of quality online in self-publication (I'm not talking about the arXiv here), for example original visual art photographs, music recordings, online essays and poetry and especially science fiction, I find myself just swamped. Every month Amazon hosts literally thousands of self-published works of science fiction, while the commercial publishers put out a few dozen, with a few dozen more republications of classics. I can easily and quickly evaluate the few dozen commercially published books, but it takes hours to trawl through the self-published things. I have found just one self-published book (Hugh Howey's Wool) that I would have bought if it were commercially published. I'm sure there are more like Wool, but damn are they hard to find.

The second thing I fear is that the ability to carry ads will infest self-published works and warp their direction and quality. Obviously part of Amazon's motive for hosting self-published works is to attract customers. These books are ads as much as they are anything else, at a minimum they bring the author and his or her friends to Amazon to browse a bit.

About scientific publishing, peer review that is both open and critical is, well, critical. A purely online scientific publishing system run by the scientists themselves and reviewed (blindly) by themselves would seem to be an attractive way out of the mess, so why hasn't it caught on? The arXiv doesn't fit this description, either.

26. **Joel Rice**
January 29, 2014

That is awful. I got Kernighan and Ritchie at the BN on 18th st, and used to go to the McGraw Hill store every week – then to the 40th st Science library. One you did not mention was the National Bookstore near Cooper Union – great place to browse, until they shrink wrapped all the books (long ago). They had a big Dover section – got Weyl on Group Theory for $3.50 !! But about The Strand – there was a character who got there every thursday and scapped up all the good stuff before anyone could even look at it, though I did find Cartan's Lectures on Spinors – in French.

27. **Shecky R**
January 29, 2014

I too lament the disappearance of brick-and-mortar bookstores. I understand the appeal for fiction-lovers of downloading dozens of novels onto their Kindles etc., but have never understood how readers of math and science (and some other non-fiction) could find tablets satisfying. And as a member of Barnes&N., with the additional coupons they offer, I essentially get most of my books at their store for the same price I would pay Amazon by the time shipping& handling is added on.
First, the big box stores came along and put the local mom-and-pops out of business, and now the Web is putting the big boxers out of business… sad to see the landscape changing (guess I'm just an old fogie!).

28. **Visitor**
January 29, 2014

Kartik Prabhu mentioned the NY Public Library. Their http://nypl.org website has an interesting feature – if you look up a book in their on-line catalog, you will have the option to "browse the shelf". (I've never used it though, so I can not say how well it works.)

I recall when Broadway in the area of the Strand was lined with second-hand bookstores. But except for the Strand, there is not a single one left. And the Barnes & Noble Annex, across the street from the main B&N store on 18th & 5th, which was probably the second-largest used bookstore in the city (after the Strand), closed down some time in the mid-90's if I correctly remember.

29. **srp**
January 29, 2014

The Internet has been a complement rather than a substitute for used books; the ability to do online sales has increased the "velocity" of the stock of books, with people more easily able to buy and sell them. So in L.A., where on the westside there is nary a new bookstore, there are many used book stores, some of which carry idiosyncratic and varying inventories of technical books.

Long before B&N and Borders started shutting down their stores, L.A. lost its Technical Bookstore, which used to be a fantastic browsing spot. Their actual business seemed to depend very heavily on selling medical books to aspiring nurses, paramedics, physicians, etc., but they carried an amazing variety of stuff, especially in engineering and computer science areas that I would never have even known existed. Their new arrivals display was a great way to see what was going on in different fields. They moved online, I think.

Just found another L.A. technical bookstore that says it will be closing on February 14: http://opamp.com/

30. **Jim Akerlund**
January 29, 2014

Adam J. Calhoun,
I used to live in Portland and frequented Powell's Technical. This past September I passed through Portland and decided to visit Powell's Technical. They moved it. What was once a large store has now been reduced to the size of a 7-11 and now sits across the street from Powell's main store.

31. **Tim May**
January 29, 2014

I've lived in the greater Bay Area (counting the Monterey Bay Area) since 1974, minus 1980–82 when I lived near Portland, OR.).

As of the 1980s, the South Bay (Silicon Valley) was remarkably rich in technical bookstores. Computer Literacy opened in Sunnyvale in around 1983 and later opened a couple of other branches. They closed sometime around 10 years later. Also, Stacey's in Palo Alto, on University, had a very large collection of math, physics, and CS titles.

Even Printer's Inc., part coffee house, part bookstore, had a large selection. And Stanford Bookstore, both the campus store and the University Ave. store, had large selections.

(I bought many books at these places. Some I expensed to my employer at the time, Intel, some I bought on my own.)

First to go was the vaunted, famed Computer Literacy. Then Staceys went. (The store on University turned into a Pottery Barn…this will perhaps figure into the history of the decline.) Printer's Inc. reverted to just a coffee house (with some sort of art gallery attached, last I looked, 5-8 years ago. It may be gone now.).

Borders vanished. A small outlet of Brentano's at a regional mall also vanished (perhaps years earlier).

There may still be a Barnes and Noble in Santa Clara/San Jose. Last I was there, it was infested with after school kids occupying all chairs and coffee house seats doing homework.

(This is also what happened to my local Borders in Santa Cruz. Every available seat was filled with kids with laptops. I think this figures centrally in why Borders failed.)

In my town, Bookshop Santa Cruz is still thriving. It was always in the ranks of Kepler's in Menlo Park and Cody's in Berkeley (though never with the technical coverage of Cody's, but at least the equal of Kepler's in its heyday). BSC seems to be thriving, and is quite crowded on many days. It may survive the current ice age.

Fortunately, Amazon long ago became a better place to find interesting books. If the high retail prices are a problem, the used copies are generally excellent. I've probably bought 80% of my math and physics books in the past 10 years via the best used deal. Rarely have I been disappointed. (Authors and publishers may scream that buying used books is depriving them of their profits from a $149.95 academic text.)

And then there's the East of the Rhine site someone mentioned. Google for it. Many of the paper books I already own I have "checked out" from this library in PDF or EPUB form. Ever so much much handier on my 64 GB iPad. Always there, never misplaced, easily searchable. 30,000 books can fit on this iPad, and bigger ones are coming.

Hint: LibGen, or Library Genesis.

–Tim May

32. **Mayer A. Landau**
January 30, 2014

Back in the 80's, when I lived in NYC, I used to go a lot to Book Scientific. The store was at 18 East 16th street in Manhattan. They would give you a standard 10% discount. Some books had a 30-50% discount because they were second hand.

Then, sometime in the late 90's, they went out of business. I went to the going out-of-business sale. There were huge discounts on the books that remained on the shelves. I asked the owner why she was going out of business, and she said they could not compete with online retailers. The problem became that book prices on technical books kept going up, up, and up. So, while in the 80's, you could put $30-$40 down on a book, by the late 90's many books were heading to over a $100. At that point, casual buying stopped for me. If I needed, or otherwise wanted, a book, I would go online to Amazon and see if anyone was selling the book second hand for a lot less. Or, I would xerox it at the University library.

Then, in the last few years, a lot of foreign internet websites popped up in Russia, China, and the Ukraine with free electronic copies. At that point it became simply a matter of going to Google translate, finding the book you wanted and downloading. You can't beat that! I have not bought a book now for four years.

Now, does an electronic book beat holding a paper book in the hand and reading it while lying on the couch? No, but with the price of books what they are, it is far cheaper to print out the book and read your printed copy on the couch. Furthermore, most of the books I bought were for reference mostly, not full throttled cover to cover reading. For that electronic is preferable, for two reasons. You can search a pdf and you can carry your whole library with you on a thumb drive.
Finally, as LaTex and microsoft word spread through the land, it became easier and easier for professors to write a book. With ease of writing came poorer quality. Many technical books out there right now are not worth the paper they are written on, and certainly not $100.

In the end, the technical bookstore became an anachronism, whose demise was speeded up by the unabashed greed of the book publishers, and maybe the rushed poor quality writing of the authors.

33. **chiz**
January 30, 2014

Peter Woit:

> In any case, few such libraries have ever been well set up for browsing recent technical books

I find the New Book Displays in local libraries useful for this purpose. Do the libraries you use not do this or is there some other problem?

34. **Youngun**
January 30, 2014

What I don't think has been stated is that there is a vast array of material on the internet which is legal and free.

Lecture notes, full textbooks, lecture videos, videos by leading maths figures, and not lest places like wikipedia and ncatlab and simple google searches.

I also observe that it is common practice to maintain a personal pdf library, sourced from the internet.

35. **karthik**
January 30, 2014

Citylights in SFO has a good section on popular math and science books.

36. **Michael Gogins**
January 30, 2014

Youngun or whoever you are (I add "whoever you are" to everyone who uses a handle online), one of the main problems many of us are having is, sure there is a great deal of great stuff available online, and nobody wants to turn back the clock on that, but a library, a bookstore, an edited journal, provide an extremely useful filtering and selecting mechanism. This pre-selection saved us a lot of time and presented us with a wonderful smorgasbord of high-quality eats.

There probably is actually more high-quality stuff in total on the internet nowadays, but there's no selection. There are, literally, hundreds or even thousands of mediocre or outright incompetent items for every one that would have been selected by an editor or librarian in the old days. I don't have time to wade through this crap, and believe me I have tried.

What do you do about that?

Regards,
Mike

37. **Charles G Waldman**
January 30, 2014

I share your sentiment of sadness in the decline of scientific bookstores, but I'm happy to note that the Seminary Co-op Bookstore in Chicago has a large math/physics section, and is still going strong.

38. **Curious Wavefunction**
January 30, 2014

Sometimes I feel like relocating to Portland solely for the pleasure of being able to visit and buy from Powell's Bookstore (they have both a general and a separate technical store). I still remember how I found a second edition of Dirac's quantum mechanics book there.

39. **Benni**
January 30, 2014

In Munich, it was the same.
Next to Ludwig-Maximilians-University, which has, since the early days of Sommerfeld and Heisenberg, a world renowned Physics department, there was a book store which had, nothing else but math and physics books. The owners were old. Their children did not want to run the shop, because it was not lucrative enough. Its profit decreased astonishingly year for year, since amazon expanded in germany. So the shop finally closed. Now there is no shop in munich, where one can look into specialized physics and math books before buying them. The other shops only have few undergraduate books when it comes to physics.

40. **Bob The Programmer**
January 30, 2014

I remember the old Kroch's and Brentano's in Chicago, huge math and science section at all levels.

And the days when university bookstores had serious STEM sections also.

Somehow Amazon and smaller competitors don't seem to be a complete substitute.

41. **TomD**
January 31, 2014

Peter,
Remember in the 80's the U-store at Princeton was great.. It used to be that one of my greatest treats was to spend a few hours in there or at the Harvard Coop. I think the loss of good technical bookstores is terrible. For one thing, I have a hard time reading electronic versions when one has to flip back and forth to see previous equations, figures, etc.

It's good to hear from other posters that there are a few places left. UT Austin used to be good, but now just has shirts. Stanford bookstore declined tremendously.

Of course, every time I move I promise myself to never buy another book!

42. **Bobito**
January 31, 2014

With respect to what Michael Gogins says, one might add that browsing online is not the same as browsing physically. In the old days one went to the library or bookstore and found the book one was looking for and rapidly could see that it was useless, but the book next to it turned out to be just what one needed. That doesn't happen as easily now, and no one seems to have found a good substitute for it. Summary: now there is more available, but the filters are less functional, and search process less efficient.

The flip side is that when one works in a less-well-financed-than-the-USA country, there weren't any libraries/bookstores worth a damn anyway, so the current situation is an improvement.

43. **BooBooks**
January 31, 2014

Important comments and discussion, Peter!
In Berkeley there was not only Cody's, which had the best math collection, but also Moe's, Shakespeare and Co (used) and North Side Bookstore. Are they all gone now?

In Paris the other wonderful Shakespeare must still be thriving, having been passed on recently to the next generation, but it has never had any math or science to speak of.

Random notes: in Urbana Illinois, the Illini Union Bookstore is a shadow of its former wonderful self, all ipods and ipads and t-shirts and sweatshirts. Brazil has always been bad for book-lovers; the culture there is going out or TV rather than reading, but there is a vicious circle as books are way too expensive, imported ones even more so. One good bookstore on Pedroso de Morais- 4 stories of books 15 years ago- was taken over by the French chain FNAC which added cds, dvds, tvs, cameras and computers. That meant there were 2 floors of books left, then one, most recently only 1/3 of a floor, divided with cds and dvds, as ipods and flat screens have taken over the rest of the 4 stories.

Yes, pdfs are wonderful as a portable library, but only for reference, not for reading,
not for studying. I learned so much from my old wonderful texts which I still have crowding my too-small apartment. What are today's students supposed to do? Library copies, pdfs or xerox copies are a poor substitute. And Amazon is great but the deals I so often found browsing at used stores were much better.

Maybe the new mac Retina screens help somewhat with the eyestrain, or the Kindle reflected light screen; but does the latter accept pdfs?

While we are lamenting the decline of literacy, I have recently heard that many US kids are no longer learning cursive writing (or reading). I was shocked to learn this; kids will be sent out into the world partially handicapped. What, "mastering" the mouse, or Excel or Power Point is supposed to compensate for that? This dumbing-down of the populace is apparently one more legacy of the over-testing mandated by Bush's inappropriately named "No Child Left Behind" program.

44. **Simple biologist**
January 31, 2014

Interesting topic and also the one that has finally made me feel like a luddite. I'm only (only?) in my late thirties, but I have l've been using computers for over 30 years. Got my mom to thank for that. It's really the ebooks and on-line book stores that have become the first technological advancement- in my life time- that have made me firmly to say no.

For me there's no replacement for the smell of ink, the texture of paper and browsing through (physical) books in a book store. No online store or digital book will ever be the same for me. Oh and quality book stores are dying in northern europe as well.

Just screw the tablets and get of my lawn!

45. **Peter Woit**
January 31, 2014

Some recent experiences in bookstores reminded me of some of the reasons I've found them so valuable, providing something I can't get on Amazon:

1. As some others mentioned, what makes a good bookstore is not having everything, but having a well-chosen selection of things. On Amazon, you have millions of books, and a computer algorithm that tries to figure you out. In a good bookstore, someone has done the work for you of sorting through a huge number of possiblities, picking out the most interesting and worthwhile ones. If they've done this well, this is an extremely valuable and high-level service.

2. If I find a book in a bookstore, the fact that I can look at any part of it (unlike Amazon, where you just get blurbs, and maybe access to the table of contents and a few pages) means that I can make a well-informed decision about whether the book is worth my time and money. For a book that looks interesting, I can quickly flip through and find the parts most likely to be of interest. I can then see whether there's really not much there, so buying the book would be a mistake, or, more happily, that there's a lot of interesting material, making the book purchase a good idea. With the high price of technical books, even for those in wealthy countries with dedicated funds from their institution for books, one can't afford to buy everything that initially looks interesting.

46. **August Penn**

January 31, 2014

Hey Peter,
The West Towne Mall in Madison, Wisconsin has a fairly good math and science section, probably not as good as the B&N in New York at Fifth Ave. and 18th St. had though. I feel for you.

47. **Tim May**
February 1, 2014

I'm happy with the current situation, as compared to, say, 1970.

Back then, there was a good scientific bookstore in downtown Washington, D.C., but it was quite a trek for me (just finishing high school, just starting college). The local bookstores were helpless…a Waldenbooks several miles away, a Brentano's about 15 miles away. I could special order a book, like Lawden's "Tensor" book (which I recall ordering), but it took a few weeks to arrive.

I experienced the heyday of technical books in the Bay Area. That is, the 1980s. I had five or six bookstores within 10 miles of me: Computer Literacy (3 locations), Printer's Inc. in Mountain View, Stacey's in Palo Alto (and they even opened a second one in Cupertino), Kepler's in Menlo Park, and two very well-stocked Stanford Bookstore shops. (The main campus one was huge, the University Avenue Annex was also pretty well-stocked, on three levels.) And I may be forgetting a couple of others.

But times change. Books shot up in price, from fairly reasonable $15 prices to nosebleed prices of $150 or more (for math and physics non-popular books). Binding quality also went down, from sewn binding that would withstand years of use to glued binding that never laid flat and that, at worst, had pages literally falling out.

(I bought a Springer Verlag copy of Mac Lane and Mjordiik's "Sheaves" and the pages literally began to fall out as soon as I opened the book. First a few, then more. I contacted an office of S-V in Virginia, listed as the printer, and they said "Tough luck." I eventually removed all of the pages, punched holes in them, and put them into a small 3-ring binder. This was what book publishing came to.)

Today, I have access to tens of thousands of PDFs and EPUBs, readable on my Mac Book Pro Retina (no eye strain) or my iPad Retina (no eye strain).

I don't lose them, misplace them, or lend them out.

And, they mix easily with the vast number of Archive and other PDF papers I have.

I don't miss the past.

–Tim May

48. **Richard Séguin**
February 1, 2014

Tim,

Badly bound books is not a new problem. In the 1970s I bought a copy of Rotman's The Theory of Groups and the pages began falling out almost immediately. You're right about the hugely inflated price of books though. The Rotman book cost only $14.50 at that time. Now, I cringe thinking of ordering an $80+ book from Amazon without being able to "look inside."

49. **David Derbes**
February 1, 2014

I've bought technical books for nearly fifty years. The beginning of the end of good technical bookstores was the capitulation of nearly every university bookstore to Barnes and Noble (including the celebrated Harvard Coop.) In my salad days I bought books at HK Lewis, Gower Street, London (gone); Dillons, London (gone, or more accurately, transformed to a Blackwells, I think), Princeton's U-Store, MIT Coop, Heffer's in Cambridge UK, Blackwells in Oxford (Weinberg's "Gravitation and Cosmology", nine pounds in 1974, about $18 US), James Thin in Edinburgh (gone), Reiter's in Washington, DC, and many another store now gone. The original Blackwells, Broad Street Oxford had a wonderful collection in the '70's, when I was a grad student in Edinburgh, but the last time I was there it was really disappointing.

So, without further ado, the best technical bookstore I now know of still extant: The Seminary Co-Op, Woodlawn Avenue, Hyde Park, Chicago, 60615. Tremendous math section, much better than average physics. This is, in my opinion, the finest scholarly bookstore in North America. Last time I was there the bookstore at the University of Illinois at Urbana-Champaign also had a great selections in math, physics and computer science. We also have in Hyde Park the original Powell's, second-hand only. Incidentally they are selling a great number of Chandrasekhar's personal books. When Mike Powell finally tired of the perpetual winter, he lit out for Portland.

I don't know why they've all folded. Part of it is the unbelievable greed of many textbook publishers (I bought the great Purcell "Electricity and Magnetism" for $6.95 from the U-Store in 1970; when McGraw-Hill last offered it, it listed for north of $210. Not bad for a book originally published in 1964, whose author was dead for twenty years. Happily, Cambridge sells it, and hurray! a third edition in MKS units with a gazillion new problems by David Morin, for a reasonable sum ($80, I think.)
The other part is that I guess kids don't buy books any more. I find this unbearably sad.

50. **Jeff m**
February 1, 2014

Sorry to hear about b&n closing, loved that place as a teenager. Got some great physics and math books, plus some amazing other stuff like a comic book version of Chaucer's The Millers Tale done by the fabulous furry freak brothers. Also sorry to hear about book scientific, spent a lot of time there when I was at the GC

51. **Mayer A. Landau**
February 1, 2014

David Derbes,
$80 for a textbook published in 1964 is not a reasonable price.
"Kids" don't buy books anymore because they now have alternatives to being gouged by the publishers. That's a good thing. Not sad at all.

52. **Dave Bacon**
February 2, 2014

If you're ever in Seattle check out Ada Technical Books. I do miss Cody's 🙁

53. **book sci**

February 2, 2014

In February of 1996 Book Scientific moved to 10 West 19th Street # 3B (though it kept their phone number according to my handy black book from when I was a graduate student, (212) 206-1310). I'm not sure what happened after that.

54. **Peter Woit**
February 2, 2014

book sci,
Book Scientific was on 19th st at that 3rd floor location for several years, then finally closed for good. Oddly, before the move it was across the street from Revolution books, then Revolution books also moved to 19th st, across the street. Now Revolution books is on 26th st, I suppose I should try looking for a revived Book Scientific across the street somewhere…

55. **chris**
February 3, 2014

It's not just NYC. Here in Germany they are also dying at a fast pace, with the surviving bookstores shifting their emphasis to toys and gifts.

The real tragedy though is that the online bookstores – which would technically be extremely well equipped to offer you browsing through a vast variety of books, much better than any real book store – can't do this because of the screwed up publication system still in place. With the 20th century bookstores dying off, I hope the 20th century business model of publishers will die soon, too.

56. **Richard Haas**
February 27, 2014

I am late to this discussion. I worked in Manhattan for twenty years and I want to mention a couple of other bookstores: There was Viktor Kamkin Importers on 21st and 5th ave which had a fine science and math selection, all Mir Publishers Moscow, mainly in English, some in Spanish. It also had such curiosities as the complete collection of Oz books bound in silver by "Lyman F. Baum" and albums including "The Soviet Choir Sings Negro Spirituals".

On Warren Street was Warren Street books. The collection was so random in might have been a cover for something else; however, on weekends Warren Street Books would set up at computer shows in NJ and sell Springer Verlag, North Holland, Birkhauser, etc. When a book was several volumes they were never displayed together.

There was also in the late '80s and early '90s a father and son team that supplied street vendors (mostly Indian) with recently published books that they would sell on the streets of Manhattan from folding tables. They had 15,000 feet of wharehouse space in South Williamsburg and asked me if I could come up with a way of cataloging what they had with a bar-code scanner and a database, somehow. I had just quit a job on Wall Street and was not interested in going back to work. The new books were in large boxes on skids and there was no logic to how they were grouped. They did not tell me how they came by them.

What books the Strand had could be "bursty". One burst occurred when Louis V. Gerstner, Jr. closed IBM plants and offices. Their libraries ended up at the Strand.

In Paramus, NJ there was for a couple of years a Barnes and Noble across Route 17 from a Barnes and Noble. And, actually, there used to be a Barnes and Noble Annex across from the 18th St. Barnes and Noble that sold old books.

There is this about the Viktor Kamkin Store in Maryland: http://ww2.gazette.net/stories/021706/businew180145_31958.shtml
The article says, "Thousands of books, all in Russian and some still in plastic packaging, were taken to the trash transfer station at Shady Grove to be recycled." In fact there was a large selection in English. Amazon does not do a very could job with these, see, eg: http://www.amazon.com/Aksel-Berg-Century-Outstanding-Scientists/dp/0828532575/ref=sr_1_fkmr1_1?ie=UTF8&qid=1393512351&sr=8-1-fkmr1&keywords=aksel+berg+radar

Quick Links

February 1, 2014
Categories: Multiverse Mania

- I've always thought more philosophers of science should be weighing in on the debate over "falsifiability" and the "demarcation problem" surrounding string theory and the multiverse (i.e. are these really science?). This is a complex and tricky subject that they have a long tradition of exploring, and it would be great to have this inform some of the debate instead of the often very naive arguments that dominate the discussion. Massimo Pigliucci does a nice job here, responding to Sean Carroll's attack on falsifiability (see here, more discussion here). Pigliucci's posting is great, giving a concise explanation of the way philosophers of science have found to think about these issues. It does though show that much of what needs to be examined are technical scientific issues (what exactly does "string theory" say or not say? What exactly are the conceivable things one could expect to measure and compare to theoretical predictions? What exactly is the state of efforts by string theorists to make predictions: how deadly are the obstructions they have run into?). In any case, here's Pigliucci's conclusion:

  > But at some point the fundamental physics community might want to ask itself whether it has crossed into territory that begins to look a lot more like metaphysics than physics. And this comes from someone who doesn't think metaphysics is a dirty word…

- This evening at ASU there will be a program on Parallel Realities: Probing Fundamental Physics, you may be able to watch it live here. With David Gross there, at least it won't be the usual "Isn't the Multiverse cool?"-fest that this sort of thing recently often has turned into. Yesterday on Science Friday, Krauss, Wilczek and Brian Schmidt discussed Could There be a Crisis in Physics?
- In case you're wondering why there's been no discussion here of Hawking's recent claims about black holes, the reason is that I'm in agreement with Wilczek's wise characterization of this on the Science Friday program:

  > I think the kind thing to do is to pass this over in silence.

  If you really need some Hawking material, there's The Top 10 Science Jokes, As Told by Stephen Hawking. Warning: safe for work, but not very funny…

- Quite a bit funnier (although some might say, also kind of tired and sad..) is this "debate" between Bousso and Rovelli at the latest FQXi conference: String theory vs. loop quantum gravity.

Update: Video of the ASU talks can be found here. Wilczek is still sticking with SUSY, but says "no more excuses… these particles have to materialize ". According to Twitter, this really was a rock-star event, with the local ladies "getting dolled up" and ready "to throw our panties onstage."

Comments

1. **paddy**
   February 1, 2014

   Great blog by Massimo Pigliucci. Thank you for the link Peter.

2. **Barack Holder**
   February 1, 2014

   @ The ASU debate tonight:

   @asuORIGINS David Gross on the multiverse: "It smells of angels." #1universe

3. **Shantanu**
   February 2, 2014

   Peter or anyone else. If someone could put a link to the webstream at the ASU meeting that would be great (for those of us who could not watch it live)
   I listed to the public debate. I am surprised Frank is still sanguine about
   proton decay.

4. **tt**
   February 2, 2014

   why is it crazy to say there might not be black holes?
   the only observational evidence just implies some massive, dense object.
   we dont know what happens in that regime, much like other energy regimes.

5. **Barack Holder**
   February 2, 2014

   Here it is:

   http://www.ustream.tv/recorded/43354237

   Unfortunately, the Q&A is missing

6. **Martin**
   February 2, 2014

   Science does not work only via falsification. The logical positivists referred to verification: a statement is true if you can verify it. Popper retaliated by referring to statements that can be falsified but not verified. This is old ideological stuff from the 1930's.

   In science we want to estimate the credibility of a statement or hypothesis. Seeing the Higgs boson is a verification. Not seeing SUSY is a falsification. Both, verification and falsification depend on empirical data: from these we can judge whether a theory works well or not. This is in general not possible with metaphysics, which generates statements that could perhaps be true or not without there being any way of judging this on the basis of empirics (seeing, measuring, etc). The problem with string theory is that it is experimentally irrelevant. The problem with mathematical physics is that its afficionados do not care about experiments. Thus they talk about Hawking radiation and multiverses etc. without being bothered about whether there is any experimentally supported reason for taking these subjects seriously. This is a clear symptom of decadence. I do read QFT because it is experimentally relevant, even when it is only an effective theory. I cannot get interested in string theory, though.

7. **Tom**
February 2, 2014

Matt Strassler has a more “nuanced” (I dislike that word!) view of the Hawking/Black Hole Kerfuffle & attributes a lot of it to “Media absurdity”.

Read:
http://profmattstrassler.com/2014/01/30/did-hawking-say-there-are-no-black-holes

8. **Peter Woit**
February 2, 2014

Tom,
I assume that you’re making fun of Strassler here, whatever he’s up to, it’s not “nuanced” (dramatic accusations against journalists in red type with multiple exclamation points????). I did write a comment there, which it looks like he didn’t really understand. His claim that when Hawking write a paper saying there are no event horizons and explicitly writes “there are no black holes” the media should not quote this doesn’t make any sense to me. The media coverage I’ve seen has been pretty accurate about Hawking’s claims.

The one criticism I have is that it would be better if the media provided some context, that this was just one of a lot of rather incoherent stabs at the black hole information paradox, in the backwash of the “firewall” realization that just invoking holography doesn’t solve the problem. I think the media is accurately portraying the claims of a small group of prominent physicists. If other prominent physicists were willing to go on the record (kudos to Wilczek) saying that this, and Hawking in particular, is best ignored, we’d be much better off. This is a (minor) physics community problem, not a media problem.

Personally I don’t see any reason to waste time on this. The media is doing a reasonable job. A celebrity physicist is getting too much attention, some physicists are hyping their work, while most of the physics community is ignoring them. All is normal. It’s not like the multiverse or string theory story….

9. **Thomas**
February 2, 2014

The exact quote is:

“The absence of event horizons mean that there are no black holes – in the sense of regimes from which light can’t escape to in□finity. There are however apparent horizons which persist for a period of time. This suggests that black holes should be rede□fined as metastable bound states of the gravitational □field.”

http://arxiv.org/pdf/1401.5761v1.pdf

For decades, physicists and their huge egos have been roaming the earth claiming to whoever would listen that black holes are places where the density of matter produces such extreme spacetime curvatures that, to cite what has to be the most tired cliché ever, “nothing, not even light, can escape.”

Hawking’s quote above is a direct repudiation of this definition, period. Both him and the press have recognized it and there is nothing wrong with that. It remains to be seen whether the new definition has content or not. Let’s not hope too much, but if via Hawking’s paper the firewall craziness could actually help physicists to retire their nauseatingly obligatory “nothing, not even light, can escape” phrase, and to try and seek words of their own when they talk to the public, then it will have been worth something.

10. **DrDave**
February 2, 2014

Fave quote from Pigliucci on the fine tuning problem: “I actually think that people who seriously maintain that this universe is friendly to life haven’t gotten around much in our galactic neighborhood.”

11. **Art**
February 3, 2014

Actually, I much prefer your one-sentence assessment above (“just one of a lot …”) to the smug, snide (and disrespectful) put-down by Wilczek that you applaud. People are interested in this stuff, and are looking to experts for informed guidance, but not to just be told “ignore it” with no back-up. Kudos to Motl for engaging with the argument.

12. **Peter Woit**
February 3, 2014

Art,
You’re right that people are interested, and looking to experts for informed guidance. I think Wilczek is providing exactly that: telling you that this is getting much more attention than it deserves and your time is better spent elsewhere. I actually think he’s trying to be polite by not giving more detail about why he thinks this (as well as trying to avoid wasting his own time). If you really want to ignore wise counsel from a Nobel Prize winner and join Lubos and others in studying this, go right ahead, but I don’t think you should be criticizing Wilczek for providing something the field sorely needs. Far too few like him are doing what should be part of their job: giving an expert opinion about what is overhyped and best ignored.

13. **Sesh**
February 3, 2014

I don’t agree with one aspect of Pigliucci’s argument: the cosmological constant problem is not simply one of “fine-tuning”, and certainly if the minimalist version of the anthropic argument proposed by Weinberg actually correctly predicted the value of the cosmological constant, this would be a very good reason to take the multiverse seriously.

However, in light of modern observational data Weinberg’s argument does not give the right value of the cosmological constant. It is off by about three orders of magnitude, which is pretty much the limit at which Weinberg’s original paper stated we should conclude the anthropic argument did *not* work. Which is not a fact that is often mentioned.

What’s worse is that, as Lee Smolin was pointing out in the comments on a previous post here, the stringy multiverse actually does not satisfy one of the key assumptions necessary for Weinberg’s argument (namely that the bubble universes differ *only* in the values of Lambda). Once you allow other variables to vary there is no longer any model-independent prediction for Lambda and therefore no way that the multiverse hypothesis can be tested by any observed value of the cosmological constant.

So ultimately I agree with Pigliucci’s conclusion, though not with the reasoning. The details of this argument are of course pretty well known, but for completeness I have also written them up explicitly here.

14. **Art**
    February 3, 2014

    Fair enuf, but these days “proof by reputation” doesn’t cut it. (Did it ever?). The thing is, it wouldn’t have required much more effort to make the contextual eval you did above, and then reputation can legitimately come into play, since it’s clear that there’s some basis for the opinion. Love your blog …

15. **Peter Woit**
    February 3, 2014

    Sesh,
    I commented on your blog also about this, but for here also, could you provide a reference to the best current numbers for this kind of “prediction of Lambda”?

    Thanks!

16. **Sesh**
    February 3, 2014

    Peter,

    I’m afraid I’m not enough of an expert on this to give you the “best current numbers”. However, I think the point is somewhat moot in any case. Weinberg’s original argument was attractive because it didn’t depend on the details of the measure problem. But that argument gives the wrong value.

    To get the right value (even assuming that only Lambda varies) one must tackle the measure problem, and since there is consensus on the correct way to do this, it becomes a question of retro-fitting the measure to get the correct Lambda. Note that even the Martel et al paper you linked to on my blog does this, because it was already clear by 1997 that Lambda was too small for the original argument to work.

17. **Sesh**
    February 3, 2014

    Sorry, I meant *no* consensus in the comment above.

The Perfect Theory

February 2, 2014
Categories: Book Reviews

Cosmologist Pedro Ferreira has a new book about to come out, entitled The Perfect Theory. The author accurately describes the book as a "biography of general relativity", and it's quite a good one, of the short and breezy variety (as opposed to the detailed and exhaustive sort).

The theory's parentage (Einstein), conception and birth are covered, in particular the way in which mathematics played a crucial role, with Einstein getting important help on this from his friend Marcel Grossman, as well as David Hilbert, who found the right dynamical equations at the same time. This material goes by fairly quickly though compared to many other sources for this history, in order to get to the main topic: the life story of the theory so far, nearly 100 years on. After the birth of the theory, it soon started to get wide acceptance, with Eddington helping to provide both the experimental confirmation in 1919 of the theory's distinctive prediction about deflection of light by the sun, as well as ensuing publicity.

Some implications of GR were found immediately (e.g. the Schwarzschild solution in 1916), and the 1920s saw early work on applying the theory to cosmology (de Sitter, Friedmann, Lemaitre). By 1929 Hubble's observations of an expanding universe had shown the way forward in this area. Ferreira goes on to follow several different strands of how the theory has developed. These include: black holes (Oppenheimer, Wheeler, singularity theorems from Penrose, Hawking, the information paradox), cosmology (Hoyle and steady state models, the CMB, Peebles and the now standard model, with dark matter and dark energy), relation to quantum theory (DeWitt, supergravity), gravitational waves (Weber, LIGO, proposals like LISA), and quite a few others. A wealth of different topics and interesting pieces of the history of the subject are covered, although none in great detail. The emphasis is on this history, along with the present state and prospects of the subject, with not much attempt to try and explain the intricacies of the physics (which would take a much longer book).

On the hot-button issues of string theory and the multiverse, Ferreira does a good job of giving an even-handed description of the arguments. For instance, he counsels reader to pay attention to George Ellis as well as multiverse proponents.

For more about the book, there's a very good review by Graham Farmelo here. Oddly though, just like with his excellent biography of Dirac, which ended with a weird attempt to claim Dirac as a string theorist, here Farmelo ends by trying to enlist Ferreira and GR in the cause of string theory:

> 50 years later, the mathematical aesthetic of relativity has been enhanced by the beautiful demonstrations of its veracity that Ferreira describes. These would probably have made Born ponder why he and his peers did not spend more time developing a deeper appreciation of the theory soon after Einstein first presented it. Maybe there's a lesson here for some of today's string-theory sceptics?

I've never seen anyone else try and claim that the history of GR is analogous to the history of string theory. As Ferreira's book explains, unlike string theory, GR is a classic example of a testable scientific theory, coming with one impressive post-diction (precession of the perihelion of Mercury) and followed up by an impressive test of a distinctive prediction (bending of light at the 1919 eclipse). As for mathematical aesthetics, GR uses beautiful mathematics (Riemannian geometry) and its dynamics is determined by the simplest possible Lagrangian density (the scalar curvature and nothing else). Whatever the equations of string theory might be, they remain unknown. Rather than a lesson for string theory sceptics, this book provides some good lessons about what a successful fundamental theory looks like, ones that string theory proponents would be well-advised to ponder.

"The Perfect Theory" is a good title for the book, with GR remaining our best example of a beautiful, powerful fundamental physical theory, based on the deepest mathematical ideas, with almost no free parameters. Ferreira does a great job of leading readers through the story so far of this amazing theory.

Update: For another review of the book, see Ashutosh Jogalekar at The Curious Wavefunction.

Update: Nature now has a podcast with an interview of Ferreira.

Comments

1. **Bob**
   February 2, 2014

   missing a "z" in Schwarzschild

2. **Peter Woit**
   February 2, 2014

   Bob,
   Thanks! fixed.

3. **Chris W.**
   February 2, 2014

   Scientific American has another review (also limited to subscribers).

4. **Nathalie**
   February 3, 2014

   I didn't find the book by Graham Farmelo excellent at all. What Richard Dalitz used to tell about Dirac was far more interesting. Dalitz had known Dirac personally, but unfortunately didn't write a book about him.

5. **Bernhard**
   February 3, 2014

   "Maybe there's a lesson here for some of today's string-theory sceptics?"

   It doesn't matter how empty an argument is, you can always count with someone comparing it to Einstein and relativity. That's a rule that works both with cranks and people with reputation.

6. **Jose Natario**
   February 3, 2014

Typo in the third paragraph: “Fereirra” should be “Ferreira”.

7. **Peter Woit**
February 3, 2014

Jose Natario,
Thanks! fixed.

8. **David Brown**
February 3, 2014

free online reviews:
https://www.kirkusreviews.com/book-reviews/pedro-g-ferreira/the-perfect-theory/ http://www.publishersweekly.com/978-0-547-55489-1 Review: “The Perfect Theory”

9. **Giotis**
February 3, 2014

We should not mislead people. GR is far from perfect; in fact it’s another effective filed theory with the Einstein-Hilbert term being just the first term in an infinity series of irrelevant terms.

The correct view is that GR is the effective filed theory of massless spin 2 particles which is valid only up to certain scale. Beyond that scale you need new degrees of freedom to describe the theory.

10. **Peter Woit**
February 3, 2014

Giotis,
Given that we don’t know what the theory is that GR is supposed to be an effective field theory for, I don’t think “the correct view” is the right terminology.

Of course, GR is a classical theory, presumably the classical limit of something we don’t understand. That given, please, that’s no excuse for another tedious ideological argument about quantum gravity.

11. **Art**
February 3, 2014

Does it include refs to the literature?

12. **Peter Woit**
February 3, 2014

Art,
Yes, the book has extensive references to the literature. Since he’s interested in history, these are mostly to primary sources, the original papers, rather than later expository materials.

For example, for the “string wars” he recommends looking at blog entries from that period from this blog and from a couple others…

13. **Art**
February 3, 2014

Cool. Thx!

14. **Neil**
February 3, 2014

There are indeed some parallels between GR and string theory. Many experimentalist physicists considered GR a waste of time, as experimentalists do today with string theory. Some were quite prominent (and not nazis)–Millikan, Essen, Rutherford to name a few. Rutherford called it rubbish I believe. And although GR did make the deflection prediction and precession retrodiction you mentioned, most of its predictions and the ability to test them were decades in the future.

Another interesting parallel. Einstein’s “happiest thought” was that he could transform away gravity by moving to the accelerated frame. The string theorist’s happiest thought is that he can transform away gravity by moving to the AdS boundary.

15. **Chris W.**
February 3, 2014

Somewhat later in the 20th century, Percy Bridgman was another contemporary of Einstein who regarded general relativity with considerable skepticism as I recall. (I made a brief attempt to locate a decent online reference for this, without success.)

Bridgman was also primarily an experimentalist, whose writings on the philosophy of science were influential, but arguably more strongly in psychology than physics. There is a certain parallel with Ernst Mach here. Indeed, Mach himself lived to see the advent of general relativity, but did not receive it well.

16. **Peter Woit**
February 3, 2014

Chris W.,
I’ve never heard of anything about “considerable skepticism” towards general relativity from Bridgman. His interest in the philosophy of science and “operationalism” made the question of how to think about special and general relativity a major concern for him, but that’s quite different.

Mach died at the age of 78 three months after Einstein and Hilbert published the field equations, long before the 1919 experimental confirmation, so I don’t think one can reasonably try and sell him as an example of a skeptic about GR.

17. **nige cook**
February 4, 2014

Mach in 1913 (after Einstein’s nascent and incorrect pre-GR paper of 1911, which predicted only half the correct deflection of light by gravity) was apparently skeptical:

"I can accept the theory of relativity as little as I can accept the existence of atoms and other such dogmas." – E. Mach, 1913.
http://www.maths.bris.ac.uk/~macpd/gen_rel/

18. **Bernhard**
February 4, 2014

Neil,

The history of science is full of examples of correct theories that found skepticism from from contemporaries, that parallel would not be exclusive of GR. More importantly though, is that history of science is also full of examples of incorrect theories that were correctly dealt with skepticism from contemporaries, and this is most likely the right parallel to be applied to string theory, given that the theory, unlike GR, was unable to provide predictions up to now.

I agree that testing all predictions from GR is not easy and this is actually still work in progress (e.g. direct observation of gravitational waves). But in any case the deflection of light by the Sun prediction found almost immediate confirmation. Even if you consider the 1919 result to be biased you can still use the 1922 confirmation by the Lick Observatory. It is a joke to compare string theory to this.

Furthermore, the examples you give are not all correct. I did not go to the trouble of checking your claims about Millikan and Essen but at least Rutherford I know changed his mind about it. On December 1919 Rutherford co-authored a Nature paper called "Radioactivity and Gravitation" (see "Studies in the History of General Relativity" by J.M. Sanchez-Ron), where he reported results on experiments to test whether the rate of radioactive substances is affected by subjecting them to high-centrifugal acceleration (given the equivalence principle). This is not coming from someone who thinks the theory is rubbish.

Your ""happiest thought" parallel is not really worth a comment.

19. **Neil**
February 4, 2014

Bernhard,

Yes, I agree that GR has proven itself capable of generating testable hypotheses, including frame dragging and gravitational lensing, in a way that string theory has not to date, and may never will. Reading the history of GR, however, it was certainly the case that GR was considered controversial and young physicists were counseled to avoid it as a dead-end. It is significant, I think, that Einstein's 1921 Nobel citation expressly stated that his "services to Theoretical Physics" was not a reference to his relativity work.

I thought my "happiest thought" comment is at least worthy as an amusing joke, as it was intended.

20. **Anonyrat**
February 4, 2014

Regarding Millikan, this is what Wiki says:

> Since Millikan's work formed some of the basis for modern particle physics, it is ironic that he was rather conservative in his opinions about 20th century developments in physics, as in the case of the photon theory. Another example is that his textbook, as late as the 1927 version, unambiguously states the existence of the ether, and mentions Einstein's theory of relativity only in a noncommittal note at the end of the caption under Einstein's portrait, stating as the last in a list of accomplishments that he was "author of the special theory of relativity in 1905 and of the general theory of relativity in 1914, both of which have had great success in explaining otherwise unexplained phenomena and in predicting new ones."

So Millikan acknowledged that relativity had a tie to phenomena.

The Perfect Wave

February 4, 2014
Categories: Book Reviews, Multiverse Mania

Sometimes when I have come across claims of exotic phenomena at the far-out edge of the field of BSM physics based on branes and string theory (like time travel, or brane-world explanations of the bad OPERA result), my initial reaction has been "Are these people on drugs?" A new book out from Harvard University Press, The Perfect Wave, by theorist Heinrich Päs explains that yes, some of this particle theory activity has its intellectual roots in psychedelic drug consumption.

The book opens with a short chapter about surfing in Hawaii, but then turns to a long chapter about LSD, the author's experience with magic mushrooms, and the cult of Eleusis in ancient Greece. Supposedly this cult revolved around consumption of a psychedelic brew called "kykeon", and we're told that this was of great influence on Plato. The discovery of atoms is also attributed to a drug trip:

> Moreover, the perception of smallest details during a drug trip may have affected the atomism of Democritus, who first assumed that the world should be built up of indivisible elements.

The next chapter goes on to discuss quantum mechanics from this point of view. Starting with Heisenberg's claims of inspiration from Plato's *Timaeus*, Päs moves on to Heisenberg's student von Weizsäcker and his ideas about quantum mechanics and Plato's *Parmenides*:

> In summary, Weizsäcker arrives at an amazing conclusion, that the notion of complementarity has its source in ancient Greece: "We find … the foundation of complementarity already foretold in Plato's Parmenides." We actually can recover the feel of what the ancient Greeks experienced in their mystery cults in modern twentieth-century physics!

From there the book moves to the multiverse and Everett's many-worlds interpretation of QM, telling us that:

> It is actually possible to recognize the multiverse — the collection of all of Everett's parallel universes — directly as Parmenides's primeval One: the unity of the world the ancient Greeks felt thay had lost in the charted modern world, and for whose reunification with the individualized ego they looked in the ecstasy of their mystery cults, in their Dionysian arts, or in the flush induced by psychedelic drugs.

The main part of the book then begins with chapter four, which starts out:

> Physics is like surfing. Or like an LSD trip.

The final chapter closes with an invocation of Nietzsche and a return to the multiverse of the many-worlds interpretation, with this now providing the Dionysian vision of science that he desired:

> For Nietzsche, science thus was the original cause for the ancient Greek's suffering from the separation of subject and object. And for modern humanity being torn from the integral unity of nature.
>
> Only if science were reconciled with the Dionysian tragedy, with art and music, only if Socrates would start to *make music*, only then could science grant humanity with a deep metaphysical benefit, could establish a true meaning of life.
>
> In view of this, it is probably most amazing that science itself, with the many-worlds interpretation of quantum mechanics, leaves some room for an entire multiverse of alternative realities beyond our immediate experience — a multiverse mirroring the unity of Parmenides's philosophy, as well as the egolessness of Aldous Huxley under the influence of psychoactive drugs, and a Dionysian creative moment bursting open all boundaries.

In betwen the first few chapters and the last one, there's a short 200 page book about neutrinos. This is the field of the author's expertise, where he started out as a theory student working with the Heidelberg-Moscow double beta decay experiment. Claims from this group that they see a signal have been challenged by recent (too recent to be in the book) results from GERDA. The GERDA results are here, Heidelberg-Moscow's response here. Neutrino physics is a complicated and fascinating subject, and one can certainly make a good argument that it's now the part of HEP where there are things we don't understand and promising avenues to explore using existing technologies (you don't need TeV-scale accelerators).

Unfortunately what most interests Päs is the far-out part of the subject, specifically ideas like using neutrinos to travel in time by hopping between branes. In the foreword to the book he does give a warning:

> Warning up front! The book deals with established scientific insights and with wild speculations… I highly recommend that the reader be alert to this difference.

and then briefly notes which topics he will cover are extremely speculative, but then ends with an inspirational quote from Glashow:

> The wild ideas of yesterday quickly become today's dogma.

These warnings are quickly left behind, and it requires a careful and well-informed reader to sort through which of the material on neutrinos has some sort of relation to reality, and which is just baseless and wild speculation.

When I bought this recently released book in the local bookstore (Book Culture, if you're interested…), at the same time I acquired another very recently released book, which I'll write more about here soon. The odd thing is that both of these books come from Ivy League university presses, and both end with a stirring and enthusiastic invocation of the multiverse as providing a Neitzschean answer to rationalistic Western science. There's definitely a trend here… For the Päs book in particular, I'm rather mystified why Harvard University Press decided this was something they should publish.

In the next to last chapter, Päs shows some awareness of why rationalistic science is so important:

> But useful application is not the point of neutrino physics. Rather it is a quest for a better basic understanding of the universe, thereby ultimately contributing to a solid foundation for the an incorruptible, rational world view. At times when intolerance, religious fanaticism, gut instincts, and irrational esotericism are flourishing, this is an effort whose importance should not be underestimated.

I'm not sure that "irrational esotericism" is any more of a widespread problem in the culture at large now than it has ever been. If it is though, I don't see how promoting over-the-top groundless speculation, the multiverse, and the idea (attractive as it may be) that fundamental physics should be a Dionysian activity based on psychedelic drug consumption is really going to help…

Update: Heinrich Päs sent me the following. It addresses a few things in the review that could easily be misunderstood, providing very helpful clarifications and some of his point of view.

Dear Peter,

thanks for your extensive review of my book. If you allow, I would like to comment on a few points though which might be misunderstood:

Your review may give the impression that the wild speculations in this book are not clearly identified. However, the preface you are citing identifies GUTs, supersymmetry and cosmic inflation as speculations which nevertheless represent "the present hopes… of most of the scientists working actively in these fields", marks string theory and extra dimensions as "wilder" speculations, admits that shortcuts in extra dimensions are "speculations squared" and that time travel in extra dimensions may be considered as "speculation to the power of 1,000". Also later on in the book I put some effort into saying clearly when I talk about textbook science and when I discuss scientific (and other) speculations, and I always motivate these speculations. For example, I mention your very own criticism of string theory as being "not even wrong" and I actually agree with you that scientific theories should make testable predictions. Of course this is one important difference of science and religion (or esotericism). However – and I hope you didn't take that personal – I continue by arguing that it is more useful and pragmatic to try to find out whether some ideas of string theory could actually lead to testable predictions – what people like Antoniadis, Arkani-Hamed, Dvali, Dimopoulos, Randall and Sundrum have started – than to condemn it.
And this is another important difference of science and religion: textbook theories can be questioned. Without speculation there would be no relativity and quantum mechanics, there would actually be no science at all.

You also mention the controversial result of the Heidelberg-Moscow group and the wrong OPERA result. Let me clarify that I'm not an author or advocate of the Heidelberg-Moscow result, and that I always have emphasized that the OPERA result is most probably due to a mistake in the experiment. The results are mentioned in the book as what they are: controversial and wrong, respectively. And while the GERDA result is not in the book, the somewhat earlier result by EXO which also challenged the Heidelberg-Moscow result with a comparable sensitivity is discussed.

Having said this, I actually like your review. It transmits the correct idea that this book takes you on the wild trip through neutrino physics, and may some of your more intellectually adventurous readers actually motivate to buy it.

Kind regards, Heinrich

Comments

1. **Visitor**
   February 4, 2014

   "Are these people on drugs?"

   They might be. Carl Sagan certainly was. (I am going to presume that you are aware of that being a factual statement.)

2. **Low Math, Meekly Interacting**
   February 4, 2014

   Kary Mullis claims frequent use of LSD helped him conceive of polymerase chain reaction (for which he won a Nobel), so there you go. My own brief period of experimentation with psychedelics leave me somewhat skeptical of that assertion. Dropping acid is way more fun than doing PCR, for one thing. If brane-world cosmology is a closer rival, I now wish I could do high-level maths more than ever.

3. **RalphB**
   February 4, 2014

   I'm retired from an industrial lab and was driving home last night after teaching my community college physics course. One of my usual FM stations was cutting in and out with others, but suddenly a woman was talking about the "mind-blowing reality of quantum physics" with its "infinite number of universes." I was told that every decision she makes leads to a splitting of the universe. This splitting also happens when she dreams. And it also happens when her cat makes decisions.
   I teach respiratory therapy students, some psychology majors, some very bright high school students. I was tired last night. And feeling like I am now swimming upstream against the current.

4. **Michael Gogins**
   February 4, 2014

   I have not read this new book of Rubenstein's but I have read a few of her presentations and papers. The most relevant is this one: http://works.bepress.com/cgi/viewcontent.cgi?article=1008&context=mary_jane_rubenstein.

   Rubenstein is a learned, articulate, and evidently very intelligent author. What I read was extremely interesting and I learned a great deal (try her lecture on Heidegger, http://works.bepress.com/mary_jane_rubenstein/31). At the same time, she left me a bit uneasy. I feel she has multiple agendas, perhaps not always on the surface. I wonder if she is interested in natural science for its own sake, or only as an instrument in her various theological and scholarly discourses.

   In the paper I cite, Rubenstein's core argument is that "creation from nothingness" is an idea born of ecclesiastical political dialectic, which cannot obscure the fact that founding documents of Western religion discuss no such thing and leave the "primordial waters" (not God as pure "spirit and truth") firmly in place as the actual fount of physical being. (This is not MY argument!) And she equates these "primordial waters" with the quantum fluctuations of eternal inflation. Then, by virtue of the anthropic principle, WE appear as the conscious face of those waters.

   I don't think Rubenstein actually "gets" science in spite of her competent and very insightful recaps of recent physics and cosmology. She is suspicious of the drive to "oneness" that motivates science, and sees it as a political (I read: patriarchal) move that ends up sucking the life out of actual existence.

   My own view is that, be that as it may, the undeniable progress of science lends considerable weight to the philosophical presuppositions (rarely discussed as such) of science. It may be that those presuppositions are not coherent. If so, some work is needed to make intelligible that undeniable progress.

   It would be great if you could engage Professor Rubenstein directly in your review.

5. **Luca Ambrogioni**
   February 4, 2014

   What is your opinion about many world interpretation Peter? I have the feeling that once you open that door there is a lot of pseudoscience that can enter. Ok maybe it is coherent with the mathematical structure of quantum mechanics (but I do not see any sign of "splitting" in the quantum formalism, for me it replaces the collapse with something equally mysterious), but it is still a wild extrapolation. Besides what is your opinion about the relational interpretation? I think that it takes the good parts of the many-world interpretation without accepting its metaphysical burden.

6. **Peter Woit**
   February 4, 2014

   Michael Gogins,
   Thanks, before commenting more about the Rubenstein book I need to actually read it. It's clear though she's not a scientist, and coming at this with an agenda and set of interests which don't have to do with the science.

   Luca Ambrogioni,
   I don't want to get into a serious discussion of QM interpretations here, since that's a very complex subject, one I'm not expert in, and the context is not conducive to an intelligent discussion of this. One thing to say about many-worlds though is that in its simplest form it's just the unsurprising statement that you can apply the standard QM formalism to arbitrarily large systems, including the entire experiment including the observer. The difficult questions then are how classical behavior emerges, the role of decoherence, and a host of other thorny issues. These are quite interesting, but very difficult and poorly understood topics. I don't see how going on about your cat causing splitting of worlds, or the joys of psychedelic drugs helps understand anything here.

7. **CIP**
   February 4, 2014

   @Luca –

   I personally am of two – or actually, of infinitely many – minds on the many worlds interpretation. I am pretty sure they aren't interfering constructively.

8. **Nex**
   February 4, 2014

   People are easily carried away by the eureka-euphoria of making dubious connections in their minds.

   "The first principle is that you must not fool yourself — and you are the easiest person to fool." -RPF

9. **Dave Miller in Sacramento**
   February 4, 2014

   Luca,

   The two main technical problems with many-worlds are often referred to as the "preferred-basis problem" and the "measure problem": The first refers to why the multiverse is sliced up into worlds in which you have definite experiences rather than superpositions of experiences. The second refers to how you get probabilities out of an infinite number of branches of the quantum multiverse. From time to time, someone (e.g.,, David Deutsch) convinces a few people he has solved these problems. Then people look more carefully and decide not. And so it goes, for decades now.

   If you want to look into the technical details of decoherence (i.e., not philosophical mumbo-jumbo), see Schlosshauer's *Decoherence and the Quantum-to-Classical Transition*. In my judgment, these technical details in the decoherence program are very informative in understanding quantum phenomena, but I am doubtful that decoherence resolves the longstanding philosophical issues.

   You'll note that neither Peter nor I have told you whether or not we think "many-worlds" or any other approach is right. At least in my case, that is because I do not know (and I frankly doubt anyone does).

   Dave

10. **Mark Thomas**
    February 4, 2014

    There is a similar claim of the influence of psychedelia on the information age, Silicon Valley and its creative revolution. However, their claims don't go back to the ancient Greeks but the newly invented chaotic psychedelic light/music shows fostered by Syd Barrett in Cambridge and down the road with the Beatles in 1967 with Sgt. Pepper. Lots of borders were crossed.

11. **Peter Woit**
    February 4, 2014

    Thanks Dave,
    I second the Schlosshauer recommendation. But also still want to discourage discussion here of QM interpretational issues. Maybe someday a good new book on that topic will come out…

12. **John R**
    February 4, 2014

    Peter, when you ask, "why Harvard University Press decided this was something they should publish," the answer doesn't hinge on the veracity of the many-worlds hypothesis. The answer is grounded in this here and now. The profit-potential from a book like this enables the publication of some really good books that cost more than they make. It may just boil down to the bottom line.

13. **Fredf**
    February 5, 2014

    The use of psychedelics goes back forever. And it is typical of users to claim enhanced creativity as a result of their use.
    Most studies, however, suggest that the creativity is more assumed that realized…that is to say that in reality there isn't much creativity to come out of drug use, whatever may be the impression of the user.
    Whatever its import for physics is, it's not as a basis for new theory!

14. **Dom**
    February 5, 2014

    Nice update from the author and he is correct, I may well buy it now.

15. **JohnB**
    February 5, 2014

Speaking of Carl Sagan and Plato: There's an incredibly good section from one of his documentaries here, outlining how Plato and the Pythagorians, snuffed out science in favour of gibberish – analogous to the topic at hand:
https://www.youtube.com/watch?v=z_966RzTF6k&t=260

A quote (which I believe is from this) on Wikipedia (doesn't do the few minutes of the video justice mind):
"Science and mathematics were to be removed from the hands of the merchants and the artisans. This tendency found its most effective advocate in a follower of Pythagoras named Plato." and "He (Plato) believed that ideas were far more real than the natural world. He advised the astronomers not to waste their time observing the stars and planets. It was better, he believed, just to think about them. Plato expressed hostility to observation and experiment. He taught contempt for the real world and disdain for the practical application of scientific knowledge. Plato's followers succeeded in extinguishing the light of science and experiment that had been kindled by Democritus and the other Ionians."
https://en.wikipedia.org/wiki/Carl_Sagan#Scientific_advocacy

When physicists fail to ground their theories with observation and evidence, preferring just to play with their ideas and presently-inapplicable (in the sense of being applied to experiment) mathematical abstractions instead, how long can this go on – crowding out resources from other areas of research – before they become much like Plato and the Pythagorians?

16. **JohnB**
February 5, 2014

Note: That I apply that comment mainly to string theorists, not to physicists in general.

17. **Prof. David Edwards**
February 5, 2014

Nietzsche introduced the idea of perspectivism: in the final analysis, all we really have is a manifold of interlocking perspectives. For example, consider the following toy model. If humans are small finite, represent each possible human perspective by a small non-empty subset of {1,…,n} where n is a large natural number. Then, there are minimal perspectives, but no maximal human perspective. Still, there is an ideal finite perspective which sees everything! If n=infinity, then there is still an ideal infinite perspective which sees everything! (God's eye-view!) If one accepts the standard quantum logic then one has a manifold of perspectives which cannot-by Gleason's Theorem-be embedded into any single perspective! There are now maximal perspectives, but no universal perspective! (Theologically, this requires accepting polytheism!! Alternatively: Even God suffers from cognitive dissonance!!)

18. **Tim May**
February 5, 2014

Thanks, Peter, for the review and/or calling out of the book. I just ordered a copy.

I've never used recreational drugs, save for alcohol, and never noticed any of my physics colleagues or Intel colleagues (I was there from 1974-86) influenced by drugs, but I've read a lot of Nietzsche, Heidegger, etc. Not as it relates to physics or math, but in general. (And some of the pre-Socratic stuff from Heraclitus et. al. has resonances with "process" over "object," and perhaps thus to the operator/transformation or category theory point of view.)

I got exposed to the "many worlds" thing through Bryce DeWitt's book, circa 1971, and of course through the fiction of Borges, Niven, et. al., but I didn't let it warp my hard science career. As you point out, there are a bunch of aspects of QM which are interesting….I won't add my two cents here.

— Tim May

19. **M. Anderson**
February 5, 2014

Literally every sentence of the Wikipedia quote above concerning Plato (the second quoted section, beginning 'He (Plato)') is either flat inaccurate, sloppily worded (so sloppy as to be misleading), or grossly exaggerated.
The Mysteries at Eleusis did not "revolve" around the ingestion of a drug, and though Plato was indeed fascinated by, and perhaps even influenced by, the Mysteries, there is no evidence that he had any interest in or experience with psychedelics (maybe he did, but there is no evidence either in the dialogues or the ancient testimony).
There is also no evidence of this in the case of Democritus.
This all reads very much like the sloppy use many humanists make of half-digested, and thus misunderstood, science–only in reverse.

20. **M. Anderson**
February 5, 2014

Nietzsche later rejected the version of Dionysianism developed in The Birth of Tragedy (in which appears the notion of "humanity being torn from the integral unity of nature"); his "science" (Wissenschaft) means "scholarship" (as in his own early discipline of philology) as much or more than it means science in our sense; of course he did not believe in a "true meaning of life," probably not even in BT; and there is simply no way to tell what he would think of the multiverese; his idea of the music making Socrates and notion that science will eventually develop into a new need for myth (also in BT) stesses a different sort of art than the "art" of evidence-free theories of physics.
Again, sloppy interpretations, wild leaps of association, lack of concern with evidence–is there a trend here.

21. **Mozibur Ullah**
February 9, 2014

I'm fascinated by the beginnings of philosophy and physics in classical times. Does Heinrich provide any solid evidence for his claims about Democritus & Plato being on 'drugs'?

I don't think it being entirely outside the bounds of possibility that hallucinations – drug-induced or otherwise might give some impetus to thinking about the realness of reality. But, most people have these without having to take drugs – they're called dreams.

And we have solid evidence in the 20C for artists have used dreams & hallucinations as a subject for their work – I'm thinking of surrealism here. But this doesn't mean of course that technique can be dispensed with. Dali was a very good painter in the classical western tradition.

Of course, art is not science. But one gathers that the boundaries between what counted as religion, science, art & philosophy in Antiquity would be more fluid than is the case now.

Wasn't Kekule inspired by a dream of a snake eating its tail – which actually is a mythological motif – to think of structure of Benzene? Or is that also a myth?

Possibly one might consider the wilder speculative moves in cosmology and physics – multiverses, firewalls, landscapes, as part of that same mode of thinking in the 20C – as a kind of esoteric science, to be considered in conjunction with and apart from exoteric science – science that is properly evidence based in the classical sense.

22. **Peter Woit**
February 9, 2014

Mozibur Ullah,
No, not much detail about drugs and Democritus or Plato.

Yes, lots of the multiverse stuff fits in well with traditional "esoteric science". Another name for "esoteric science" among physicists of course is "crackpot science"…

23. **nasren**
February 14, 2014

I second M. Anderson's comment about Plato. Not only Wikipedia but also the Carl Sagan quote are full of inaccuracies about Plato, the Pythagoreans and Greek science in general. I'm particularly tired of hearing that the discovery of incommensurability "wrecked" the Pythagorean philosophy and that they killed the man who discovered it. Complete rubbish, but repeated ad nauseam in popular accounts. Also the idea that any Greek philosophers or scientists had any acquaintance with psychedelic drugs is pure invention: there isn't a single mention anywhere.

What we seem to have in these books is the eternal recurrence of Fritjof Capra — it keeps repeating like indigestion.

More Links, Interesting and Tedious

February 5, 2014
Categories: Multiverse Mania, Uncategorized

First some links to interesting things:

- There's a fascinating interview with Deligne in the latest AMS Notices.
- Alexandre Grothendieck: A Mathematical Portrait includes some great expository pieces about the mathematics developed by Grothendieck. There's also available Grothendieck's own Esquisse Thématique, giving his description of his major mathematical achievements.

In the category of things that have gotten tedious, first there's the ongoing hullabaloo about "firewalls" and Hawking (with the bottom line as far as I can tell just that, yes, there is an information paradox, might have something to do with the fact that we don't understand quantum gravity…). If this interests you, you can take a look at

- Matt Strassler, here, here and here, with creative writing here.
- New Scientist has Fiery black hole debate creates cosmological Wild West.
- Michael Lemonick here sensibly asks whether people really should be paying this kind of attention to Hawking at this point.

In the multiverse-mania category, I'm forcing myself to read the Rubenstein book I've mentioned, will report on this when done, in the meantime there's

- In the London Review of Books, David Kaiser has a review of Lee Smolin's book from last year Time Reborn. The review is mostly about the string theory landscape case for the multiverse, a bit about Smolin's take on it.
- Tom Banks has a new paper out surveying the issue of Supersymmetry Breaking and the Cosmological Constant. He surveys the anthropic string theory landscape argument and concludes that

> It is therefore fair to say, that much of the standard model, and certainly the peculiar values of many of the parameters in the standard model, cannot have an anthropic explanation. In string theory, extra generations of matter correspond to more tuning of moduli, small parameters do not appear to arise without a symmetry explanation, and symmetries are rare on moduli space.
>
> One cannot escape the implication that, on the basis of current theoretical knowledge, the String Landscape is ruled out by experiment.

Besides being experimentally ruled out, the whole Landscape thing doesn't work anyway

> My personal conclusion from all of this analysis, is that the theory of CDL tunneling provides no positive support for, and lots of negative evidence against, the proposal of a String Landscape…
>
> When combined with the phenomenological challenges I presented in Section 2, I conclude that the String Landscape is an hypothesis of dubious validity.

This is a subject in which solid predictions are hard to come by, but I'll make one here: string theory partisans will just ignore this, with the "String Landscape" now an hypothesis which has achieved some sort of new extra-scientific status. It's an ideology you sign up for to justify not giving up on string theory (or as a foundation for your multiverse-mania), and as such, arguments like those from Banks are irrelevant.

- Also in the category of serious scientific arguments against the anthropic landscape that will just be ignored, see Sesh Nadathur who explains why Weinberg's original argument for the CC implies one that is 10^3 times too big. This is part of some arguments with Shaun Hotchkiss you might want to have a look at, see other blog postings here and here.
- Finally, Max Tegmark is on tour promoting multiverse-mania and his Level IV version of it. The latest from him is here at Scientific American, where he's claiming that his Level IV multiverse is science because it could be falsified by finding "some physical phenomenon that has no mathematical description." I've tried arguing the absurdity of this with him in the comment section, so far no luck getting anything other than a dismissal of me as "emotional".

I was interested to see that his perception of what has been going on for the past decade as endless popular books promoting the multiverse have appeared is that

> whenever a physicist writes a book about them , the Web erupts with claims that they are unscientific nonsense.

Curious to know what part of "the Web" he is referring to other than my blog…

Comments

1. **Chris W.**
   February 5, 2014

   Max Tegmark has jumped the shark.

   There, I said it…

   (It's hardly worth saying anything more on the subject.)

2. **Ahab**
   February 5, 2014

   1-

   > Michael Lemonick here sensibly asks whether people really should be paying this kind of attention to Hawking at this point.

   Actually, he does no such thing. The article is about the media hype that has surrounded Hawking for much of his career, and it includes a mention of how Hawking himself has opposed it. The article at some points was a bit too hostile and unfair, but the relevant point is that it admits the fact that Hawking was and *still* is "one of the world's leading physicists".

   2-

   > Curious to know what part of "the Web" he is referring to other than my blog…

You quoted from the 1st paragraph. If you look at paragraph immediately after it you'll find that Tegmark says:

> My new book "Our Mathematical Universe" proved to be no exception. "Is this still science?" the biologist Mark Buchanan wondered on the pages of New Scientist, "Or has inflationary cosmology veered towards something akin to religion?" The physicist Peter Woit dismissed it as "grandiose nonsense".

3. **Amos**
February 5, 2014

The black hole information paradox and firewall paradox have jumped the shark. I think I'm going to try to stop paying attention to this until somebody comes up with a theory that makes an actual prediction.

4. **Peter Woit**
February 5, 2014

Ahab,
1, Actually, he calls him a "pretty-great" physicist…

2. I did read that. And actually I linked to the New Scientist Buchanan review in my blog posting here
http://www.math.columbia.edu/~woit/wordpress/?p=6551
The parts Tegmark quotes are questions, not statements, with Buchanan only saying about them "some scientists wonder". As close as Buchanan gets to criticism is ""there does seem to be something a little questionable with this vast multiplication of multiverses". As far as I can tell, of the dozen or so reviews out there, only Buchanan's and mine contain any criticism of the sort Tegmark is claiming "erupt all over the web". Of the other many multiverse books, I can't at the moment think of any reviews (certainly not in New Scientist) or a significant other number of blog postings (besides mine) criticizing these books. Tegmark seems awfully thin-skinned about criticism for someone who poses as "Mad Max" the revolutionary with controversial ideas…

5. **Navneeth**
February 6, 2014

> "Curious to know what part of "the Web" he is referring to other than my blog…

There are parts of the Web (a multiweb, really) that you cannot possibly access, now or in the future.

6. **theoreticalminimum**
February 6, 2014

Thanks for the link to the Schneps' page on "Alexandre Grothendieck: A Mathematical Portrait", Peter. Do you by any chance know if this will get published in a book/ebook format?

I wish you had more links like that than depressing ones to articles/books/blogposts tirelessly rehashing the same thing about multiverses :-\

7. **Bernhard**
February 6, 2014

" … for someone who poses as "Mad Max" "

Some kids will just never accept they will never become Einstein when they grow up…

8. **Roger**
February 6, 2014

I have attacked the level II, III, and IV multiverses on my blog also. Tegmark admits that many of the multiverse ideas are untestable, but says that we should take them seriously anyway because they can be formulated as parts of theories that have testable implications for our universe.

9. **Shaun Hotchkiss**
February 6, 2014

Hi Peter,

Thanks for linking to my discussions with Sesh.

Whereas I agree with Sesh that the anthropic prediction for Lambda is off by 10^3, that was still a *prediction* that got closer to the correct value than any other prediction of its time. That alone makes it compelling enough to me that, whereas I'm glad that I don't have to work on it myself, I have no qualms if other people do want to explore it. I certainly don't view a multiverse as even close to established fact, but as a speculative idea it *does* have its *scientific* merits.

On this side of the Atlantic I rarely encounter anything I would label multiverse mania. And even string theory in general, although certainly part of the mainstream in Europe, is far from dominant (arguably it is on decline). My perception of the U.S. is that things are a little different (at least from theory postdoc job advertisements over the last five years, which *typically* are either dealing with data/the lattice, or string-inspired-something-or-other – with some notable exceptions, e.g. in/near Chicago).

I think both sides (the hypers and the naysayers) need to take a longer term view of things. No, the multiverse is not established fact, and the hypers shouldn't be saying it is just to score media points, but neither is it unfalsifiable, in the long term.

10. **Shaun Hotchkiss**
February 6, 2014

Sorry, I can't edit the above post, but I should stress, that whenever I mention "multiverse" (here, or elsewhere) I am referring solely to the eternal inflation with multiple possible vacua one (Tegmark Type II, I guess), and definitely not Tegmark's III and IV.

11. **Luca Ambrogioni**
February 6, 2014

But all in all is the Tegmark's level IV multiverse very different from what Lawrence Krauss is continuously repeating about a supposed origin of the laws of physics from nothing (whatever that means)? I think he went on that direction several times.
Anyway I currently think that the main problem is that physicists are starting to feel authorized to try to answer questions that would have been considered outside physics just few years ago.

I pretty much agree with the Platonist philosophy of Penrose (much less with his view about physics) but I think he never confused philosophy with physics. It seems to me that the main attitude is becoming "any sensible question is a physical question". Of course I strongly disagree.

12. **Manyoso**
February 6, 2014

Anyway I currently think that the main problem is that physicists are starting to feel authorized to try to answer questions that would have been considered outside physics just few years ago.
I pretty much agree with the Platonist philosophy of Penrose (much less with his view about physics) but I think he never confused philosophy with physics. It seems to me that the main attitude is becoming "any sensible question is a physical question". Of course I strongly disagree.

This in triplicate. What's worse is that we are learning that very bright scientists nonetheless can produce very naive/shoddy philosophical thinking.

13. **Dr. Shrevinsky**
February 6, 2014

Yes Manyoso, "What's worse is that we are learning that very bright scientists nonetheless can produce very naive/shoddy philosophical thinking."

And also that not very bright scientists nonetheless can produce very naive/shoddy philosophical thinking.

14. **Jeff McGowan**
February 6, 2014

OK, as a mathematician I'm really curious how Tegmark thinks you might go about proving that "some physical phenomenon has no mathematical description." I guess it depends on what you mean by "mathematical description," I mean naively everything has a mathematical description in that one can take a description (e.g. the visual description, but it could be anything) and use math to encode that. I assume Tegmark is talking about a predictive description of some sort, though perhaps not given the current state of theoretical physics. Even with a limited meaning for mathematical description I can't see how one would go about proving it didn't exist for some phenomenon.

15. **Manyoso**
February 6, 2014

Jeff McGowan,

Indeed, I remember in freshman calculus the "Spherical Cow" and wonder if Tegmark believes that because one can – crudely – mathematically model a cow as a sphere, if he believes there is some universe of spherical cows 🙂

Cheers,
Adam

16. **Peter Woit**
February 6, 2014

Shaun,
Thanks for your comments. I think though that the multiverse (specifically the anthropic string theory multiverse) is unfalsifiable, in the long term. I know of no current or planned, or even conceivable experiment that could falsify this picture, since it makes virtually nothing in the way of significant predictions. For the one it is claimed to make (Weinberg's), failure of the prediction (off by a factor of a thousand is pretty bad…) is explained away, either by an unknown measure factor, or by the "at least it's not off by 10^120" argument.

In the paper I linked to, Banks gives good arguments that if you look at other things than the CC, and apply the anthropic string landscape argument logic, you get massive failures, so the whole thing should be experimentally ruled out. Arguments like his are just ignored, what is going on here is not the usual sort of science.

Of the things we're likely to experimentally learn in the future, the most obvious case is the Planck polarization data, I hear due this summer. I'm no expert, but it seems there are "predictions" of a measureable effect, as well as "predictions" of no measureable effect. No matter what Planck reports, I see no possibility that it can count as evidence against the string landscape picture, and I'm pretty sure that it will reported as "new scientific evidence about the multiverse", with scientists X, Y, and Z explaining how it agrees exactly with their multiverse model.

As for the sociology, ten years ago when I was writing my book, the whole anthropic landscape thing seemed so unserious and with such little support that it barely seemed worth mentioning (I did write about it, without much detail). I was pretty sure, like I think most people, that this was something that would just disappear of its own accord. I was very wrong then, and this has made me much less sanguine about the future, for instance about what will happen with obvious absurdities like Tegmark's. Multiverse mania in the US dominates the popular coverage of the subject, and in physics departments from what I can tell the problem is one of "the best lack all conviction, the worst are full of passionate intensity".

Jeff,
The same though occurred to me. A problem with the idea of a physical phenomenon that can't be described mathematically, is that you can embed the English language in a mathematical structure, then any description you make in words is a mathematical description. What Tegmark needs for his falsification is some physics that can't be described, something like "the peace of God, which surpasses all understanding"…

17. **Russ Whirton**
February 6, 2014

I wish people would stop referring to "Tegmark Type II" etc. It makes him sound as though he raised some profound questions.

Which he didn't.

18. **Roger**
February 6, 2014

Jeff: Yes, our theories are mathematical, and they predict experiments well, but Tegmark wants to make an ontological argument that the physics *is* the math. If two distant electrons are entangled, then it is impossible to give a mathematical description of one electron independent of the other one. The usual way out of this paradox is to day that there is a mathematical action-at-a-distance, but not a physical one. To me, that says that the physics is *not* the math.

There are alternative explanations, such as saying that there is a physical nonlocality that is not directly observable. To me, this is unsatisfactory as it is like believing in psychic powers that can never be demonstrated in a controlled way, or believing in one of the unobservable multiverses.

19. **CU Phil**
February 6, 2014

    Peter,

    Regarding prospects for testing the multiverse (it looks like the multiverse of eternal inflation, not the string landscape, although the comment about extra spacial dimensions muddies that a bit), here's something close to home this Monday:
    http://physics.columbia.edu/colloquium-matt-kleban

20. **Peter Woit**
February 6, 2014

    CU Phil,
    Probably will try and make that, although it's just likely to lead to more tedious material appearing here…

21. **Art**
February 6, 2014

    A lot of Hawking-envy out there in physics-land… Which is strange: hands up, those who want to trade places. The point is well taken that, like the elderly Einstein, the media pay un-justified attention to his recent work. But I don't think you can change that behavior; the human-interest story is just too compelling. What you can do is to exploit that attention to educate and re-direct the curious. I suppose that's what Wilczek was attempting, like you said, but it still strikes me as a particularly clumsy attempt.

    Thanks much for the Deligne interview link. Imagine a lecturer postponing a session because an auditor was absent!

22. **Shaun Hotchkiss**
February 6, 2014

    Sorry for that Russ, I completely agree. He made the classification of course, but given that he only invented one of the classes in his classification and it is the "either he's a genius who has seen deeper than everyone else, or he's just talking nonsense" class, that doesn't really count.

    Thanks for the reply, Peter. I do think that people take heed of all the issues you've pointed out, just perhaps not the vocal people, at least not publicly. The reason why those issues aren't enough to kill off the idea is that (and this does depend on priors, I suppose) the anthropic explanation of the cosmological constant, even if that far off, is still one of the best on the table (though I guess you'd disagree). If the cosmological constant problem were to be compellingly solved with some other mechanism people would give up on an anthropic multiverse pretty quickly (though maybe that is youthful naivety).

    Also when I write "long term" I mean *long term* (i.e. potentially 100s of years) and I'm implicitly considering the possibility that the correct inflationary potential gets determined (if inflation is correct), and/or the standard model is found within the landscape and/or early collisions of inflating bubbles can get witnessed, and/or more catastrophic boundaries are discovered. The point is, any elapsed period of time during which any of the above is not achieved does "falsify" the multiverse a little, but substantial "falsification" will take this continuing to happen for many years and/or the more compelling alternative arriving.

    Or, at least, that's how I currently view things.

23. **Asnant**
February 6, 2014

    Hey Peter,

    Here's a link I'd like to get your view on:
    http://iainews.iai.tv/articles/beyond-the-universe-auid-309

    (sorry for going off topic)

    Cheers

24. **Peter Woit**
February 6, 2014

    Asnant,
    Never got around to mentioning that particular piece of multiverse mania, there just are too many… But I did write here recently about Mersini-Houghton's claims, basically the same ones she is making in that piece, see

    http://www.math.columbia.edu/~woit/wordpress/?p=5907

25. **mathematician**
February 6, 2014

    Peter,
    The speakers for ICM 2014 are announced. Any rumors for yet?

26. **Peter Woit**
February 6, 2014

    mathematician,
    Haven't heard any, just speculation about the obvious candidates (for the Fields). If I do hear any interesting rumors, will blog about it (or, if I don't, may do this anyway to try and generate some…). But, so far, I got nuthin.

27. **Yatima**
February 8, 2014

    Meanwhile, ideas for the [next european circular hadron collider at 100 TeV]() are being collected.

28. **TomH**
February 8, 2014

In his SciAm article, Tegmark writes:

" … all their arguments involve what logicians know as "modus ponens": that if X implies Y and X is true, then Y must also be true. Specifically, they argue that if some scientific theory X has enough experimental support for us to take it seriously, then we must take seriously also all its predictions Y, even if these predictions are themselves untestable (involving parallel universes, for example). "

Well, I dont see why we have to take seriously _ALL_ its predictions. Most theories are only applicable in a limited domain of energies, scales, or other simplifying assumptions. Eg, Newton's gravity theory. And who hasn't heard of the "spherical cow" simplifying assumption?

re Tegmark's comment:
" … falsify the mathematical universe hypothesis by demonstrating that there's some physical phenomenon that has no mathematical description … "

Seems to me most complex systems _don't_ have a tractable mathematical description.

Try creating a mathematical description of human consciousness & self-awareness.

Too hard?

Try to create mathematical description of the c.elegans' (a nematode worm) behaviour. It has only 109 neurons.

Still can't do it?

QED, I have falsified Level IV multiverses.

29. **Simple biologist**
February 8, 2014

@TomH
"Try creating a mathematical description of human consciousness & self-awareness.

Too hard? "
Not for Max. You must've missed his new consiousness paper.

http://arxiv.org/abs/1401.1219

30. **Peter Woit**
February 8, 2014

Tom H,
I don't have a problem with Tegmark's claim that if some parts of a theory are well-tested and we have lots of reasons to trust them, then it's reasonable to believe that other implications of these parts of the theory will work out, even if we can't test them. Of course this all assumes you are talking about a theory you understand well, so you know what its implications are, and you have serious and compelling tests of the relevant parts of the theory. The situation with Level II multiverses, which are supposed to be predictions of theories like the string theory landscape, is that we don't understand the theory at all, and to the extent we understand it have zero evidence for it (or, as Banks argues, good arguments against it). I also see no serious argument for how we are supposed to get such evidence in the future.

Tegmark's tactic here is to argue based on assumptions that are nowhere near where science is or has any hope of getting, but are close to the place he wants to end up. It's kind of like arguing with someone about the existence of God, and you start by saying "assume that an Angel with gold tablets appears in front of you…."

As for falsifying Level IV, Tegmark is certainly not discussing "tractable" mathematical descriptions. As he states his argument, to falsify his hypothesis you need to show there is no possible mathematical description of some phenomenon, not just be unable to find a tractable such description. It has been pointed out here that "no possible mathematical description" seems to be equivalent to "no possible description", since one can embed a natural language like English in a mathematical structure. Tegmark does claim "consciousness can't be described using mathematics" as a possible falsifying example, but I'm not sure what that means, unless you are a non-materialist, believing consciousness is not a physical phenomenon, in which case this has nothing to do with his hypothesis.

31. **Laymammal**
February 8, 2014

Tegmark's comment:
" … falsify the mathematical universe hypothesis by demonstrating that there's some physical phenomenon that has no mathematical description … "

I think Tegmark's statement itself is problematic. From what I can get from his work, he makes a claim of ontology not the less radical claim of description. Here he gives a falsifying example for the "uncontroversial" claim as proof that his "ontology" theory is scientific. Doesn't work. If he is only claiming description, so what? Description does not beget ontology.

32. **Jim Akerlund**
February 8, 2014

Off topic,

Peter, I see your first post for NEW was 3-17-04. Do you have anything planned for the ten year anniversary of NEW?

33. **SUSY**
February 8, 2014

One cannot escape the implication that, on the basis of current theoretical knowledge, the String Landscape is ruled out by experiment.

Besides being experimentally ruled out, the whole Landscape thing doesn't work anyway

My personal conclusion from all of this analysis, is that the theory of CDL tunneling provides no positive support for, and lots of negative evidence against, the proposal of a String Landscape…

When combined with the phenomenological challenges I presented in Section 2, I conclude that the String Landscape is an hypothesis of dubious validity.
^

if the string landscape is of dubious validity what does this mean for string/M-theory and offshoots like adS-CFT?

34. **Peter Woit**
February 8, 2014

Jim Akerlund,

Well, I'll be on a spring break vacation in Northern Italy, nothing planned yet.

SUSY,

For string/M-theory, just that even a pathetic last-ditch, pseudo-scientific effort to salvage unification via string/M-theory doesn't work. For adS/CFT, nothing at all.

35. **cthomas**
February 9, 2014

It seems to me that it would be quite easy and simple to empirically refute any version of the multiverse hypothesis, including Tegmark's approach. The test is that if we ever happen to meet an omniscient and honest being, then the being would affirm the sentence, "Tegmark's hypothesis is true" if and only if the hypothesis is true and would affirm the negation if and only if false. Now granted, we don't happen to be able to carry out this decisive experiment given current conditions, but the availability of the future possibility of this decisive test clearly shows that it is falsifiable in principle, and hence fully scientific.

36. **Susanne**
February 9, 2014

Peter,

Your blog is always an inspiration for me. Today I have one point to ask: isn't the best prediction of the cosmological constant simply that it is the zero-point energy of the box defined by the universe? This gives a prediction (1/Radius of the universe)^2 for the cosmological constant, very near to the observed value, with an error of the order 50%.

The prediction has the disadvantage that the constant would need to change with time. But the experimental data is not yet good enough to allow checking for such a decay. Is there a reason that this "prediction/postdiction" is not discussed more often?

37. **SUSY**
February 9, 2014

Peter, do you find Tom Banks reasoning sound on scientific grounds?

How would you respond to a string theorist who would argue that the moduli of the Yau-Calibi space cannot be determined on a priori grounds but only through experiment and observation, and that we do not have the technology to test this. Once this universe moduli could be thus determined string theory becomes completely predictive with only 1 parameter.

38. **Peter Woit**
February 9, 2014

Both of these are off-topic, so, please, do not try and turn this into a discussion of the CC or moduli stabilization, about which I don't think there is anything to say that hasn't been said hundreds of times here and elsewhere.

SUSY,
There are plenty of problems with string theory models, Banks is pointing out some, I've discussed others here and in my book. The standard answer from string theorists is that we don't understand string theory well enough to know how severe these problems are. If we understood string theory better, maybe they would go away, is the argument, but that seems to me just pure wishful thinking.

There are typically dozens to hundreds of Calabi-Yau moduli parameters, not 1. In addition, you need to add some physical mechanism to fix the parameters (e.g. KKLT moduli stabilization), which introduces yet more physics you don't understand, ensuring you can calculate nothing.

Susanne,
I'm not sure exactly what argument you have in mind, the distance scale of the CC I would have thought was 10^-3 eV (since it is the 4th power of this). If you do want to relate it to the size of the universe you do have the problem that it is supposed to be changing, which is presuamably testable.

39. **Cspan**
February 10, 2014

cthomas,

"if we ever happen to meet an omniscient and honest being, then the being would affirm…"
"the availability of the future possibility of this decisive test clearly shows that it is falsifiable in principle"

I don't think, to *ask someone* is what is considered a scientific experiment and even the possibility to do so is not falsifiability.

40. **CThomas**
February 10, 2014

Cspan — my bad. Failed attempt at sarcasm on my part. Not very humorous, in retrospect.

Best regards.

41. **Cosmonut**
February 10, 2014

Thanks for the article about Hawking.

IMO, Melnick is being too generous – much of Hawking's o
overblown reputation is due to his wild self promotion
rather than media hype.

For a great example, read his latest autobiography
where he claims to have created a successful
approach to quantum gravity and
explained the origin of the universe. via his No Boundary proposal

Both claims are completely false, but my dishonestly
presenting mere speculations as established scientific fact in
popular books, Hawking has created an image of having done
far more than he actually has.

42. **nasren**
February 11, 2014

For those interested the play about Grothendieck's life, Grothendieck's Dream of the Rising Sea, by Adrian Heathcote, is available here:

https://independent.academia.edu/AdrianHeathcote

The link at Leila Schneps site is broken — has been for some time.

43. **Ice&Fire**
February 12, 2014

Nice article in "New Scientist". From layperson's point of view, does not this remind of the ultraviolet catastrophe in the early 1900 physics that started the quantum era, only in reverse? There, classics dictated continuous spectrum where micro structure was discrete…. here, classic theory says discrete wall (event horizon) where on micro level (Planck scale?) there could be still some continuity. Look for a theory with a smudged event horizon? Would be really funny and ironic if this "little spot on the horizon" sparked a new theoretical revolution, wouldn't it? 🙂

44. **Anonyrat**
February 14, 2014

Is the Universe a Simulation? Edward Frenkel in the New York Times:
http://www.nytimes.com/2014/02/16/opinion/sunday/is-the-universe-a-simulation.html

The Multiverse, Evidence and Theology

February 11, 2014
Categories: Book Reviews, Multiverse Mania

Yes, this multiverse business is tedious, but since it is becoming mainstream physics, with colloquium talks here at Columbia devoted to it, and the Columbia University Press publishing books about it, seems to me that someone at Columbia should be commenting on these, and I don't see anyone else doing it. Will try to make this short.

Yesterday Matthew Kleban's talk here was entitled Testing the Multiverse. The only part that actually really was about testing the multiverse was the part describing work on bubble collisions with other universes. This has been heavily advertised in the press, see here, here, here, here, here and many others. Kleban described some of these ideas, but when it came to the experimental testing part, he just briefly acknowledged that all searches for these things have come up empty. The only prospect for the future mentioned was the polarization data to be released later this year by Planck, which would give some new things to look at, but he seemed unenthusiastic that this would realistically lead anywhere. So, as far as the "testing" goes, it has been done and the tests failed.
The rest of the talk was about various inflationary models, including Kleban's work on "unwinding inflation" (see here, here and here). Some of these models do have testable consequences, and many do lead to "eternal inflation", so in such models you expect to continually produce new inflated universes, although with exactly the same physics. This is being sold as "testing the multiverse", and string theory is brought in to justify lots of possible different physics in different universes, but this is not a testable part of these scenarios. What's being advertised is a grandiose picture of the string landscape, laws of physics determined environmentally, etc., etc., but if you actually look at the product that you're actually buying as "testable", you don't get any of the cool stuff. For slides of a somewhat similar recent talk by Kleban, see here.

A while back I acquired a copy of the new book Worlds Without End: the many lives of the Multiverse, started reading it and was planning on writing a detailed review. I soon got bogged down in the first half of the book, which is a detailed intellectual history of speculation about multiple universes (so lots about relevant parts of Plato, Aristotle, Lucretius, the Stoics, Augustine, Nicholas of Cusa, Giordano Bruno, Kant and others). Finally I realized I just didn't have the energy to serious read this material. People with other interests and/or more time on their hands may find this quite worthwhile.
The second half of the book is devoted to the question of current speculation about physics, so more up my alley, but again I found it hard to focus on this. I fear it is only mildly interesting to see what a theologian/philosopher of religion makes of the current multiverse mania, not enough so though to do more than skim the text. From this skimming, what's in the book is a lot of retelling (sometimes introducing misunderstandings) of the hype-laden tales of the multiverse told in dozens of books and magazine articles over the past decade or so.

Rubenstein ends the section about what physicists have to say with Tegmark, seen as having reached the final endpoint of the "Ultimate Multiverse":

> So some worlds will be linear, and some will be cyclical; some will be singular, and some will be plural; some will be infinite, and some will be finite; some will branch forward, and some will branch back. Some worlds will be manufactured, and some will be simulated; some designers will be kind and some will be cruel, some capable and some all but incompetent.
> And, presumably, some of the set of all possible world will have a creator-god who breathes over primordial waters, who separates the sea from dry land.
> How on earth did we get back here?

I take Tegmark's vision as empty, so a good thing to ignore, but Rubenstein sees this as an opening for theologians to get back into the mainstream cosmology business, and the rest of the book focuses on this. With the boundaries between science and religion now gone, all sorts of possibilities open up for theologians. The final part of the book begins by invoking (just like Henrich Päs, who comes at it from the mind-altering drug rather than theological angle) Nietzsche:

> Nietzsche concludes the *Genealogy* by expanding this vision, promising "*all* great things bring about their own destruction through an act of self-overcoming" (3.27, emphasis added). This promise then, has me wondering. If science can be regarded as the self-overcoming of a particular form of religion, might multiverse cosmologies be something like the self-overcoming of *science*? Might they mark the end of the fantasy that "science" has wrested itself free from "religion", "objectivity" free from subjectivity, and matter free from meaning? After all, we have seen each of these multiverse cosmologies open onto metaphysics and mythology not in moments of lapse or weakness, but precisely where they are scientifically most compelling.

It seems that, unlike most authors, Rubenstein actually has got the story of multiverse mania right: it's left conventional notions of science behind and entered into the realm of theology. We do, however, disagree about whether or not this is a good thing…

Update: Bogus claims about Multiverse "predictions" are now all the rage. For the latest, see the Caltech Quantum Frontiers blog, which has Yasunri Nomura writing about Making Predictions in the Multiverse. There of course are no predictions there, just mainly a discussion of the idea that many-worlds and the eternal inflation multiverse are somehow the same, an idea I continue to find unfathomable. Nomura doesn't mention that he actually did have a prediction from the Multiverse (and someone made a movie about it…). The prediction was for a Higgs mass of 140 GeV, but of course when you're in the multiverse business, wrong predictions are not a problem, they're always true somewhere.

Update: For more on the multiverse front, Edward Frenkel has a review of the Tegmark book in this Sunday's New York Times. He does a great job of explaining the problems with the way Tegmark is trying to use mathematics. John Preskill tweets in agreement (positive and negative).

Update: Nomura, when asked about experimental evidence for the multiverse, responded that the experimental situation is

> not much different from some other situations—e.g. in the big-bang theory, inflationary cosmology, and Darwinism in biology

So, the scientific evidence for the multiverse is "not much different" than the evidence for evolution? And Tegmark thinks I'm the one in league with creationists…

Comments

1. **Manyoso**
   February 11, 2014

   I take Tegmark's vision as empty, so a good thing to ignore, but Rubenstein see this as an opening for theologians to get back into the mainstream cosmology business, and the rest of the book focuses on this.

   Massimo Pigluicci's blog has a new podcast with Tegmark discussing his ideas. A lot of the answers are not very responsive as you might expect.

   One interesting thing is that Tegmark is now requesting to divorce his critique of the use of infinity in physics with his multiverse of mathematical universes hypothesis. He thinks the two should be treated as unrelated.

   It is very hard to understand his critique of infinity in mathematical physics now. He seems to be saying that because we can't find any physical process or thing that is infinite that we shouldn't use infinities in our math describing physics. I'm still trying to figure out what he means by this. Are we allowed to use pi for instance…

2. **Chris W.**
February 11, 2014

> I soon got bogged down in the first half of the book, which is a detailed intellectual history of speculation about multiple universes (so lots about relevant parts of Plato, Aristotle, Lucretius, the Stoics, Augustine, Nicholas of Cusa, Giordano Bruno, Kant and others).

It's amusing that the supposed thoughts of such thinkers on the "multiverse" would be considered relevant, when the extent most of what we know as the observable universe was completely unknown to them. I can see why you got bogged down.

Even the notion that we are part of one galaxy among many was only beginning to be understood (mostly as speculation with hints from observational astronomy) in the 18th century, and was a subject of active research and disagreement in the 1920s.

3. **CPV**
February 11, 2014

Throughout history humans have bundled up things they couldn't explain or understand and called that bundle 'religion'. While that bundle has gotten smaller, it still contains a fairly large payload of questions. Scientists should feel unashamed of admitting that there are questions that they can't answer, or maybe even properly formulate, at least for the time being. At some point the answers to these questions may become part of the scientific domain, but until then they can remain labeled as part of religion or pure speculation. The multiverse stuff is almost entirely like that. What will falsify the mulitverse? Nothing. What should people be working on? Better explanations for the specific form and character of physical law. That's the job.

4. **thecrud**
February 11, 2014

Well we dont have all the answers. I know back to invisible sky beings that grant wishes.

5. **Michael Gogins**
February 11, 2014

Peter Woit:

I'm very sorry to hear you're not going to give more attention to Rubenstein's book. I think that going further into this would be well worth while for everybody concerned. I am, I confess, a bit afraid of what is happening here. I have fears both for the integrity of science, and for the integrity of religion.

Rubenstein is plenty smart and pretty engaging, and if she goes astray, she may take some people with her. I think you are prescient in your concern for the empirical integrity of science.

Manyoso:

It is possible to consider that the value of pi is NOT infinite because it IS recursively enumerable. You can make as many correct digits as you like using finite means. I think some scientists are thinking this means that pi doesn't "actually" have an infinite number of digits, because there are simple rules for getting as many as you need, without limit.

However, there is another arena well known to physicists where an "actual infinity" is much harder to exorcise, and that is the randomness that seems to be built in at a basic level in quantum mechanics. It is now known, thanks to recent results, that it is possible to certify that a sequence of bits produced by quantum processes are truly, irreducibly random. It is also known that it is not possible to compute an irreducibly random sequence of indefinite length using a program of finite size. So as far as I can see, this means that the universe is not any sort of computer, because if it were it would not be possible to physically certify that a random sequence is random.

The only way that I can even begin to imagine a way out of this quandary is to suppose that the randomness of quantum events is produced by a completely deterministic process that also completely determines how and when we will measure it, so that it is statistically random to us though it is completely pre-determined. This would provide a loophole in the strong free will theorem by removing the freedom that is assumed to exist for experimenters to set up experiments any old way. There are, after all, two senses of "random" at work here, random as uncaused and random as statistically random.

CPV:

Religion has in the past provided mythological explanations for things that now are explained scientifically, but without such explanations, there is still religion.

Let's say you love your wife, and she is working on a novel. She asks you to promise that if she dies before she finishes the book, you will not publish it. And because you love her, you do promise that. And then, unfortunately, she dies before the book can be finished.

Your wife is dead and gone, but your obligation not to publish her book lives on. Physically she is dead, scientifically she is dead, but existentially she is alive, because your promise was not to any publishers and not to yourself, but to her.

Spiritually, she is eternal.

There's a lot more to say about this, but this is not the place. I'm just trying to point out that there are ways of demarcating human discourse between science and religion that do not reduce the one to the other, or the other to the one.

6. **David**
February 11, 2014

Absolutely. Theology and multiverse cosmologies are remarkably simpatico. It does require setting aside some things – "fine tuning" arguments and "first cause" arguments being among them. But the potential for interplay between classical theology and this speculative cosmology is nontrivial.

7. **Manyoso**
February 11, 2014

It is possible to consider that the value of pi is NOT infinite because it IS recursively enumerable.

Huh? Just because pi is computable doesn't mean it is not irrational. There are an infinite number of digits to pi and this has been proven for centuries. I still don't know if this means Tegmark wishes us to develop a mathematics without the reals for calculating physical processes or what…

8. **Koray**
February 11, 2014

Manyoso: there are non-standard analyses in mathematics, e.g. computable analysis. If certain concepts like infinity or transcendental reals don't make sense (or are not required) in physics, it may make sense to restrict the math used in physics to exclude them.

9. **Peter Woit**
   February 11, 2014

   Enough about infinity, please. Besides not being very interesting, that argument of Tegmark's doesn't appear in the book, or in Kleban's talk for that matter.

10. **vmarko**
    February 11, 2014

    "It seems that, unlike most authors, Rubenstein actually has got the story of multiverse mania right: it's left conventional notions of science behind and entered into the realm of theology."

    Belief in untestable conjectures is a natural realm of religion. IMO, it was just a matter of time before people picked up on the multiverse (level II) as a piece of that realm.

    In the next period, various folks will spend some time playing with the "multiverse religion". Once they get to know enough of it, they'll figure out that multiverse is a very lousy example of a religion, providing almost no answers that any respectable religion does. Once that happens, they'll throw multiverse out the window, and return to more serious religions, and serious science.

    I am only eagerly waiting to see how multiverse-loving atheists are going to handle this… 😉

    Best, 🙂
    Marko

11. **Peter Woit**
    February 11, 2014

    Marko,
    The thought did occur to me that I didn't really want to review this book, but I did want to read a review by Sean Carroll of the book…

    Also, perhaps theologians could try marketing religion as the "Level V" multiverse.

12. **Jerry Lisantti**
    February 11, 2014

    I'm not sure which audiences Rubenstein is addressing. But you are right it is following the multiverse mania. There is much attention to the work of Mersini-Houghton. I'm not sure what is purpose of this book. A review of multivariate literature?

13. **Anm**
    February 11, 2014

    In other news, Tao gave the first credible line of attack against Navier-Stockes : http://terrytao.wordpress.com/2014/02/04/finite-time-blowup-for-an-averaged-three-dimensional-navier-stokes-equation/

14. **Peter Woit**
    February 11, 2014

    Jerry Lisantti,
    It's an academic monograph, aimed mostly at an academic audience interested in the universe/multiverse debate throughout intellectual history, but also aimed at an audience with interests in philosophy of religion. It's definitely not a mass-market book, like the typical multiverse-promoting book of recent years.

    Anm,
    Thanks, but I know next to nothing about that kind of math, so comments should go to Tao's blog. I did notice this, but also noted that he says "There is a real (but remote) possibility that this sort of construction can be adapted to the true Navier-Stokes equations." and " The paper reflects my current thinking on the subject, which is that (a) proving global regularity for Navier-Stokes is a hopeless task for the foreseeable future, but (b) proving blowup for Navier-Stokes is not… But there is still quite a long way to go to actually reach a proof of blowup for Navier-Stokes."

15. **paddy**
    February 11, 2014

    Sometimes I wonder why I come here to read these comments once or more a day. As PW says some of them are tedious. Especially since I must expend some considerable effort in resisting the temptation for snarky posts (e.g., "Bless me father of Tegmark V for I have sinned…oh not you father of Tegmark X"). On the other hand one does finds jewels like Anm's Navier-Stokes/Tao reference. Thank You

16. **Patrice Ayme**
    February 11, 2014

    "Nietzsche concludes… promising "all great things bring about their own destruction through an act of self-overcoming" … This promise then, has me wondering. If science can be regarded as the self-overcoming of a particular form of religion, might multiverse cosmologies be something like the self-overcoming of science? Might they mark the end of the fantasy that "science" has wrested itself free from "religion", "objectivity" free from subjectivity, and matter free from meaning?"

    Science was never the self-overcoming of a religion. It was the triumph of common sense. Buridan made that very clear, when he discovered the Principle of Inertia and used it to propose the heliocentric system around 1320. (For 140 years after that, the Church approved of Buridan, his "impetus", and heliocentric system. His works were put on the "Index" later!)

    Nietzsche's proposal is more akin to Gödel First Incompleteness Theorem (at some point a choice appears, on esthetic grounds)

    What may simply be going on with the Will To Multiverse is much simpler. What was the first multiverse theory? Everett's Many World Interpretation of Quantum Mechanics.

    So, on the face of it, the Multiverse arose first as a self-overcoming of Quantum Mechanics. Not a self-overcoming of science, just an "overcoming" of the "collapse", "measurement", "observer" and "EPR/Elements of Reality" problem in QM. Behind the whole thing may be an anxiety about Quantum Mechanics, and, or "Cosmic

Inflation".
Unfortunately, the proffered solution, the Multiverse, has indeed jumped far out of science.

17. **Michael Gogins**
February 12, 2014

Lisantti:

Rubenstein's audience would be, for the most part, her colleagues in academic theology and the philosophy and history of religion, but also the educated public.

The purpose of the book, as nearly as I can make it out (I haven't read it [yet?]), is to take the "multiverse" as a contemporary myth of creation, a contemporary cosmogony, that can be related to ancient myths of chaos that predate the patriarchal, "create everything out of nothing" God of Christian (until the late 20th century anyway) theology.

18. **Georg Mayer**
February 12, 2014

Hi Peter,

thank you for your patience in pointing out "what comes from" certain kinds of theories, even if (some of) their creators did not intend to open the door to the dark alleys of theology, which I thought our species has roamed long enough in the past. Reading your blog (and other materials) during the last weeks, have changed my mind mainly about the intend of the multiversers and I still puzzled that this was necessary.

I was not aware how quick people are capable to jump from "new theoretical physics theory" to "mathematical platonism" to "every religious believe is just a wonderful reality in our happy infinite possibilities muliverse duckburg". But I find it even more frustrating that scientists who come up with these initial theories (which is in general a great thing) do not clearly and strictly speak up against e.g. the movie "The Principle" (thanks for linking to the trailer) or the Rubinstein book you just reviewed. This gives the whole approach of many of these theories a very bad taste – it leaves me with an impression as if people are looking for such esoteric cross-references, maybe to just get more popular (bad) or because they believe it themselves (baaad).

This not only discredits natural sciences, but also those people who try hard to understand the nature of abstract principles in e.g. mathematics or philosophy. Anything that goes into the direction of metaphysics has to be handled carefully in order to not fall into the abyss of mysticism and people coming up with wild explanations should not fort that sometimes oneself is not aware of the fact that she or he is already in the process of accelerated downfall into fairies country.

We don't know why math works so well, most of the people who see it working are startled by this fact. There are tons of philosophical books and articles about this and many of them are a better read than multiversology. Just because we do not understand something doesn't mean we should push it as the explanation itself. And just saying "as long as math works it proves that math is the thing itself" is at best strange – tomorrow linguists stand up (as they did before) and proclaim that "as long as words can describe everything, everything is language" and immediately afterwards we see the well-known folks coming up and arguing that Pegasus and unicorns are part of reality (as they are part of language).

Therefore, after looking into the mathematical universe I return to the tools of refutability, Occam's razor and at most the Quine-Putnam Indispensability argument. We have to respect the limitations of our perception and have to be very serious about them, else we are lost in speculations that do not bring us forward, but cause tragic (esoteric) deviations from the scientific strive for truth and understanding.

Thanks again – you gave my brain a good shake, now my feet stand on solid ground again.

19. **Peter Woit**
February 12, 2014

Michael Gogins,

I think you're making some unjustified assumptions, based on the idea that Rubenstein is a "feminist theologian". Her book isn't really concerned with myth, or with feminist theology and the issue of a "patriarchal" deity. It's very much about the standard Western philosophical tradition, from Plato on, together with the standard physics story about cosmology. As I mentioned, it ends up with Nietzsche as inspiration for "overcoming" science, and he's not exactly a feminist role model. The book ends by invoking the possibility of a theology
"that asks more interesting and pressing questions than whether the universe has been "designed" by an anthropomorphic, extracosmic deity" and, to the extent that she has a voiced agenda, that's it.

I'd characterize her position as much like that of the Templeton Foundation. They're very excited to support multiverse research, for much the same reason she likes the topic: it promises to erase conventional boundaries of what is science and what isn't, bringing science and religion together and giving legitimacy to people who want to mix the two. In both cases, their interest in theology is not at all in fundamentalist religion, so they are just as much interested in invoking science against that as they are in bringing science and their conception of religion together.

20. **Peter Woit**
February 12, 2014

Georg Mayer,
There are some big differences between these various topics. The world is full of people like the geocentrists promoting their outlandish alternative science ideas, and it's both wise and conventional to just ignore them (I wrote about the "Principle" here mainly because I thought it was hilarious to see physicists taken in by this, not because anyone is going to take this seriously). As for academic monographs claiming some support for their philosophical positions from speculative science, they're also normally best ignored (do you have any idea how many people read a typical book of this kind? I'm guessing they'd all fit in one room, and not a very big one…). So, the fact that physicists are ignoring geocentrism and Rubenstein isn't either surprising or a bad thing.

Tegmark is quite a different issue. He's a prominent physicist at a leading institution, a talented expositor, academic fund-raiser and politician, working very hard for his agenda, getting a lot of top-level media attention. In this case I do think it is a big mistake for influential physicists to ignore this, hoping it will just go away, which I think it what most of them are doing.

21. **John Urbanik**
February 12, 2014

Sometimes the simplest explanation is this best. Theology is about the universe being created and with that a purpose for the universe to be created. The Multiverse is the attempted explanation that there was no "creation" or purpose but instead the universe was just one out of all possibilities. It all comes down to design vs. random chance.

Both should be part of philosophical discussion and neither should be part of any scientific debate.

22. **Martin**
February 12, 2014

This multiverse business will continue as long as it is funded. In addition, theoretical physics has in common with theology that it does not cost that much. Experimental work is much more expensive. And if the public likes to read about this sort of esoteric stuff then politicians think that some tax money should go toward this subject. Panem et cercensis, it is an old practice.

23. **Jim Given**
February 12, 2014

As a devout Catholic and professional physicist, I tell you that string theory is located at the similtaneous limit of no physics and no theology.

24. **Peter Woit**
February 12, 2014

Martin,
I don't think people do multiverse stuff because they think that's what the NSF wants (I suspect that NSF panels are not very enthusiastic about multiverse research proposals). Tegmark in his book explicitly explains how he was careful to only do this as a sideline, not so much that it would hurt his career. This is mostly not being funded by taxpayer money, but often by private money (e.g. Templeton). Don't blame politicians for everything, they're mostly leaving physicists alone to make their own funding decisions.

The appeal of the multiverse physics is that it's a way to do research work which seems to engage with the deepest problems in physics, but isn't very demanding. This is appealing for reasons of personal motivation (see Arkani-Hamed's comments about what it takes to get up in the morning and go to work on a physics problem), also if you like media attention, this will get it for you, unlike more serious work. If you look at most multiverse papers, they're using minimal mathematics, and nothing very difficult in the way of calculations. Then of course, nothing you do is testable, so there's no danger of it being wrong. John Baez describes this sort of activity as "playing tennis with the net down". Some people enjoy this, others don't…

25. **Kavanna**
February 12, 2014

There are questions that science cannot answer, and it's best to admit these are not science. For a philosopher or theologian to be interested in them as forms of speculative metaphysics is fine. Just don't call it science.

26. **Eric Habegger**
February 12, 2014

I think this interest in the multiverse is fascinating as a social phenomenon. What is the attraction to science for an idea, which by most lights, cannot ever be tested and which allows for almost any solution. It is hard to see, yet the attraction is obvious in the ever increasing attention paid to it.

Maybe it's time to quit analyzing that attraction if there really is no substance to the main tenets of the idea. Maybe we should look instead for a nexus of ideas that people are uncomfortable with and dislike. Sort of like a black hole nexus of ideas that people sense peripherally that is pulling them in and from which they want to be distracted.

Throughout the dark ages something similar happened. Religion and belief in a God that attended every fallen sparrow provided comfort to individuals in an often harsh physical environment. People wanted to feel cared for when times were tough. That seems like the primary motivation for a geocentric universe that predominated then. Maybe there is something similar going on today. Of course nobody, especially a physicist, would willingly admit that they are prey to such feelings and that it influences their thinking about science. What better way to refute it than to invent a whole panoply of universes. It's the little kid in them saying to everybody "See, I'm not afraid of nothing!"

27. **SUSY**
February 12, 2014

do other BSM theories like LQG and AS predict a multiverse?

28. **Peter Woit**
February 12, 2014

SUSY,
String theory doesn't "predict" a multiverse, since it doesn't actually predict anything. You can have string theories with a single universe, or ones with a populated landscape, lots of universes. The function of the multiverse in string theory is not as a prediction, but as an excuse for not making being able to make predictions. Theories like LQG don't have anything to say about non-gravitational forces, they explicitly are not designed to make predictions about such things, so they don't need an excuse. If you wanted to make an LQG multiverse, you probably could, but there's no reason for it.

29. **Max Tegmark**
February 12, 2014

I'm amused that Peter keeps finding novel ways to criticize me on this blog. If some theologian misunderstands the multiverse predictions of inflation etc., this doesn't constitute a scientifically valid critique of inflation. I very much doubt that all theologians imagine a deity that strictly obeys mathematical equations, and can therefore equivalently be described as a purely physical entity.
I've summarized the top-8 criticisms of my book here: http://mathematicaluniverse.org

30. **Peter Woit**
February 13, 2014

Max,
The part I quoted is Rubenstein's reaction to your Level IV multiverse, which Brian Greene calls the "Ultimate Multiverse". This really has nothing at all to do with inflation.

As for whether a deity can be consistently described by a mathematical structure, I'd guess that depends on your deity. From my little knowledge of theology I'd suppose theologians have more definite ideas about what they don't know about a deity than about what exactly its properties are and whether or not they can be expressed as a mathematical structure. Maybe you've opened up a whole new area of debate in theology about this kind of question.

As for your "top 8 criticisms", the only one that is one of mine is number 6. I don't think "our cosmos has no non-mathematical properties" is a testable prediction of a conventional scientific sort.

31. **Thomas Larsson**
February 13, 2014

"Also, perhaps theologians could try marketing religion as the "Level V" multiverse."

The Level VII multiverse would be better. The seventh heaven.

32. **Georg Mayer**
February 13, 2014

To Kavanna, John Urbanik, others:

It strikes me that people say "there are questions, which science cannot answer, so leave those to theology/religion/believe".

First of all – we don't know which questions science can answer and which not. So if there is something were we cannot get further today then in my eyes (if we are really interested in the question), we should look for scientific ways to get at least close to an answer.

500 years ago some might have asked how man exactly got created. Science at that time gave no answer. So some left the question to the local preacher. After we got the answer from Darwin we saw (in the light of evolution) that our question was put a bit self-important in the first place and that the answer was not really what we expected. But now we know. It was wise to not give up and to explore further, without relying on the holy scriptures.

Furthermore, if we leave open questions to religion, mysticism and unscientific teachings, these "explanations" do not answer anything, but mutually contradict each other (Jesus says the Gluon is blue, but Buddha had a more greenish vision of it, whilst the Tao guarantees you it is black and white). So what good are these answers (if you are interested in *answers*)?

And even more, this makes it so hard to think seriously about anything that scratches the metaphysical discussion – e.g. like the nature of numbers/sets/mathematical laws. There are a lot of questions in this area, but once somebody asks such a questions you can find a whole lot of deep thought guys jump up and down and sing the "Pegasus exists, because we have a word for it" song.

Approaching questions is a difficult business and for sure not solved by just giving up on them, i.e. leaving them to non-scientific realms. The hardest thing most likely is to admit that wedon't know most of the things at the moment. But that should make us eager to to equip ourselves better (scientifically) to approach the lack of knowledge somehow.

I am also a bit shocked how easily people put theology/religion and philosophy into one pot. "Don't call it science, leave it to philosophy and theology" – but guys, philosophy IS science and many parts of it strive to find the best ways on how to do (natural) science and how we can expand our knowledge in reliable ways. (Yah, theology is also science, but based on axioms from certain books who a supposedly metaphysical being offered as the ultimate truth in the language of the neolithic age). Bacon, Leibniz, Kant, Russel, Popper are just a few who made the scientific method how we use it today possible.

33. **Jesper**
February 13, 2014

When Georg Mayer writes "We have to respect the limitations of our perception and have to be very serious about them…" then I think he's completely right.

To me, physics is about making sensible statements, and about recognizing insensible ones. For instance: the question "where is the electron, when we don't measure it" doesn't make much sense and is best left out. To me, physics is not so much about "what is" but about "what can be said (described)" – and therefore I find this whole debate about philosophy and multiverse stuff quite a bit in the wrong direction. Sure – it can be very useful to digress into pseudo-philosophical thinking, perhaps in order to find some useful metaphors and mental images – but I think one must be very careful not to take it too serious.

Without knowing the subject very well at all, I also think this is the "tone" of modern philosophy: more about limitations of perception and language, and less about "what is".

Put differently, I simply don't know what Tegmarks statement (I didn't read the book) "all mathematical structures exist" means and I suspect it means nothing.

34. **vmarko**
February 13, 2014

Georg Mayer,

"It strikes me that people say "there are questions, which science cannot answer, so leave those to theology/religion/believe"."

There are questions about facts, and there are questions about choices. The former are answerable by science, the latter are answerable by religion. And the two don't mix.

Unless you think that humans are automatons without any form of freedom of choice (a stance which can be called "cognitively unstable" as Sean Carroll likes to say), there are always these two types of questions. Sometimes (like in the case of the multiverse) it isn't very clear into which of these two categories a given question belongs, but eventually such things get cleared up.

But no serious scientist claims that science can answer *all* questions. Just like no serious theologian claims that religion can answer *all* questions. At best, they can both answer all questions from their respective domains, nothing more.

HTH, 🙂
Marko

35. **Bernhard**
February 13, 2014

Nomura did get a direct question about predictions from Jerry Lisantti (http://quantumfrontiers.com/2014/02/13/making-predictions-in-the-multiverse/comment-page-1/#comment-7636).

Would be interesting if he would answer to it at length (although Lisantti should have said "present or future data").

36. **Peter Woit**
February 13, 2014

All,
Please, enough general discussion of science and religion, this has little to do with the topic of the posting, and I doubt anyone has anything new to say about this in general.

Bernhard,
Yes, it would be interesting to try and get a response to the predictions question. From all I can tell, at this point Nomura has a prediction that failed, and a story about why predictions aren't really possible (the measure problem). Given this, why pursue the subject?

37. **George Ellis**
February 13, 2014

Yasunri Nomura states "In cosmology our space is surrounded by a cosmological horizon", and then develops ideas related to the horizons that occur in black holes. However the horizon that limits what we can see in cosmology is an *event horizon*, not a particle horizon (see Rindler's classic paper: http://adsabs.harvard.edu/full/1956MNRAS.116..662R). His further discussion then uses results that refer to black hole event horizons: but they do not apply to the particle horizons that occur in cosmology (in the actual paper, he refers to "apparent horizons", which again occur in black holes but are not the same as the visual horizon that occurs in cosmology, see http://scitation.aip.org/content/aapt/journal/ajp/61/10/10.1119/1.17400).
So not only does he have no testable predictions but his theory is based on a case of mistaken identity. The properties of event horizon and particle horizons are not the same, and are both distinct from the properties of apparent horizons, which are locally defined (event horizons are globally defined).

38. **Will Nelson**
February 14, 2014

I like your blog although I also like strings and susy quite a bit, and the multiverse too, which I feel is just a logical development of the copernican insight. It certainly could exist, logically and mathematically, so whatever consequences that has for the nature of science are kind of tough luck. The universe wasn't made to match the preconceptions of some philosopher of science.

But anyway, not to be tedious. I wish you would highlight more of whatever interesting things you think researchers *ought* to be doing, instead of bashing what various people are doing. That seems kind of tired and a better way to "win" the battle would be to help people see different and better directions to go in. For example the fact that the LHC hasn't found susy isn't great, but it hasn't found anything else either aside from the Higgs, so who's doing something interesting based on that? I don't know, maybe you do…

39. **Shantanu**
February 14, 2014

Peter, did anyone in the question ask the obvious question about prediction?

40. **Peter Woit**
February 14, 2014

Shantanu,
Kleban did in the talk explicitly make clear what actual predictions there are. For bubble collisions, certain signals are predicted, they've been looked for, they're not there. For the various inflationary models, there are some very general predictions, e.g. about the spatial curvature, that he did mention.

To me, the problem was the high level of hype, that the discussion of models for the inflaton potential, instead of seriously examining exactly what current data implies, and what prospects for future data are (this was basically not discussed) was all about eternal inflation and the multiverse. While the models and data have nothing to say about bubble universes with different physics and string theory, that was a lot of what was being sold.

At all of the many hype-filled physics talks I've been to, I've never seen anyone in the audience object to this or point out the problem, and that didn't happen here. To all appearances, the audience ate up the hype, considers this normal physics these days.

41. **CPV**
February 14, 2014

I think a topic that is relevant here is the "cult of personality". It seems that many of the physicists involved in the Multiverse type of ideas are people who clearly enjoy hearing themselves talk, and also enjoy media attention (at least, much more than most people enjoy it!). Since the conventional approach to progress has proven difficult, here's a new road with very little effort required that produces great amounts of publicity. Some of these people are complete charlatans, but some are not. The unifying similarity seems to be the idea that it's much more important to get attention of any type than to have integrity. It's really all about them at the end. They will not be ignored.

42. **Jess Riedel**
February 14, 2014

Hi Peter,

Like you, I'm baffled by this claimed equivalence between many worlds and inflating patches, and I'm likewise skeptical that any of this will lead to testable predictions. But I do want to point out that it's unfair to say that Nomura is falsely claiming to derive an observable prediction from a multiverse theory in this blog post. (Your words: "bogus claim".) Rather, he's trying to solve the measure problem, which means finding a method for extracting predictions–even mundane ones that will agree with non-multiverse theories–from a theory of eternal inflation. Like you I think he'll fail (and moreover that repeated failures of this type are evidence that this school of thought is diseased), but the effort to extract predictions and to describe his reasoning is defensible in principle so long as he does not prematurely claim success.

43. **Peter Woit**
February 14, 2014

Jess,
What struck me as bogus about Nomura's piece was his using the title "Making predictions", when the piece was really mostly about this "Multiverse= quantum many worlds" business, which as far as I can tell has nothing at all to say about how to make a prediction. Nomura says "the picture presented here does not solve all the problems in eternally inflating cosmology", but I can't see how it solves any problems at all.

Yes, he does explain the obvious measure problem, so in that sense addresses "Making predictions", but purely negatively. Entitling the piece this way when the only relevant content is about why you can't make any predictions is maybe best described as misleading, if not a "bogus claim". I could for instance write a blog entry and title it "Calculating the electron mass", then use it to tell you that my idea was just that the electron mass could be anything at all, so, not surprisingly I have a "measure problem" and can't calculate anything. That's about what he is doing here.

44. **Peter Woit**
February 14, 2014

I've deleted an exchange with "Caltech graduate student", who I'm pretty sure is not one, but someone else with a particular agenda.

45. **Chris W.**
February 14, 2014

> I like your blog although I also like strings and susy quite a bit, and the multiverse too, which I feel is just a logical development of the copernican insight. It certainly could exist, logically and mathematically, so whatever consequences that has for the nature of science are kind of tough luck. The universe wasn't made to match the preconceptions of some philosopher of science. [from **Will Nelson**]

That the multiverse could exist, logically and mathematically, is not a reason to assert that it does exist, and especially to do so without regard for "whatever consequences [it] has for the nature of science".

Theories are not epistemologically neutral. Theorizing of any kind runs a risk of undercutting the basis for any critical examination of the theory that is being proposed. The worst of such theories are nonsensical "word salad", but they can be much more insidious than that.

The Copernican insight led to a whole line of investigation with rich empirical ramifications. The multiverse ideas now being discussed have no empirical ramifications, *essentially by construction*, and that very fact is being used to argue that they could be true. This mendacious game could have been played at any time in the history of science. If it had been, and its legitimacy had been widely accepted, then the "science" at issue would have devolved into a shallow, self-justifying fraud.

I suppose the main protection against that happening now is that the results will in the end be mind-numbingly boring and vacuous, and later generations will lose interest in them. We're already pretty far down that road.

46. **Neil**
February 14, 2014

Does any multiversarian, if that is what they are called, actually assert that the multiverse exists? I thought their position is that it *could* exist and that this perspective, if that is what it is, allows us to interpret some puzzling observations, such as fine tuning, in a useful way. It can be argued, as it is in this forum, that unless we are given more reason than logical possibility for the multiverse to exist, it really provides no interpretation. The multiversarians think that the landscape or eternal inflation provides such a reason, but of course there is no evidence for either. They too are simply logical possibilities.

47. **vmarko**
February 14, 2014

Neil,

"I thought their position is that it *could* exist and that this perspective, if that is what it is, allows us to interpret some puzzling observations, such as fine tuning, in a useful way."

There is **nothing useful** about the multiverse "explanation" of fine tuning. It's actually completely **useless** in every possible sense, since it basically says that all fine-tuned parameters are such as a result of a pure accident. Saying that the SM parameters have been chosen randomly is an **absence** of explanation.

HTH, 🙂
Marko

48. **Peter Woit**
February 14, 2014

Please all, if it's not about the topics of the posting (the Kleban talk and the Rubenstein book) it's off-topic.

49. **Sebastian Thaler**
February 15, 2014

Edward Frenkel has reviewed Tegmark's book:
http://www.nytimes.com/2014/02/16/books/review/our-mathematical-universe-by-max-tegmark.html?ref=books&_r=0

50. **Simple biologist**
February 16, 2014

"not much different from some other situations—e.g. in the big-bang theory, inflationary cosmology, and Darwinism in biology"

The last bit could actually be the first evidence for multiverse. Nomura clearly lives in a different universe than I do.

In my universe, I work with evidence of evolution on (almost) daily basis.

51. **Laymammal**
February 16, 2014

"This idea of the multiverse, as we currently think, is not simply a result of random imagination by theorists, but is based on several pieces of observational and theoretical evidence." A new kind of evidence: the theoretical kind. Noumura ends the same article with the oxymoron "concrete theoretical progress."

52. **Peter Woit**
February 16, 2014

Laymammal,
I don't think this is the problem with Nomura's argument. If you did make a lot of real, specific theoretical progress, you could call it "concrete" if you wanted. It this progress showed that theories you do have strong evidence for were tightly linked to theories that produced a multiverse, you might call that "theoretical evidence" for a multiverse.

The problem is that there is no theoretical progress of the sort Nomura and others want there to be. The more we learn about the properties of a conjectural "M-theory", the more we see that just about any physics at any scale is consistent with it. We are getting "theoretical evidence" here, but it is evidence against the picture Nomura wants to sell. Similarly, "string cosmology" seems to be able to give one any inflationary model properties one wants, so again, to the extent there is "concrete theoretical progress", it is progress towards showing that these ideas just don't work, in the sense that they don't tell us anything about anything.

53. **OMF**
February 16, 2014

I guess your blog looks like it's found another post-string hype purpose on the internet.

54. **nasren**
February 16, 2014

Geore Mayer said "And even more, this makes it so hard to think seriously about anything that scratches the metaphysical discussion – e.g. like the nature of numbers/sets/mathematical laws. There are a lot of questions in this area, but once somebody asks such a questions you can find a whole lot of deep thought guys jump up and down and sing the "Pegasus exists, because we have a word for it" song."

You obviously like your straw men: no one has ever given that as an argument for mathematical realism. Try engaging with the discussion that exists — it is smarter than you are supposing.

55. **Brian Dennehy**
February 17, 2014

From Frenkel's review of Tegmark's book:

'Conceding this point, Tegmark replaces Mathematical Universe Hypothesis with Computable Universe Hypothesis: Only "computable" mathematical structures should be allowed. But this rules out all structures that contain infinity! In fact, he admits that "our current standard model (and virtually all historically successful theories) violate the C.U.H.," which does not bode well for the whole idea, to say the least.'

I thought this was quite a glib comment by Frenkel. Surely the question of whether the continuum field theories of modern physics are but approximations to more basic theories (which may themselves be 'computable') is currently unresolved.

56. **Lee Smolin**
February 17, 2014

Dear SUSY,

Just to pick up a query raised earlier in this thread, LQG does not admit a multiverse, as the spatial topology of the universe must be fixed. Either we are in the cosmological case with compact spatial sections or there is a boundary imposed, as in asymptotically flat or AdS cases. The set up of canonical quantization does not permit the spatial topology of the universe to change as indeed the action principle is only well defined with either compact or well prescribed boundary terms and conditions. In particular you cannot have non-compact critical points of an action without proper boundary conditions being satisfied.

One thing I have wondered is how this necessary technical subtlety is dealt with in eternal inflation models.

Thanks,

Lee

57. **Fellow Hampshire Alum**
February 17, 2014

Lee,

Possibly silly questions, but I'm a mathematician not a physicist. By "spatial topology" I'm assuming you mean the topology of the subspace of space-time consisting of the space dimensions? And the point is that assuming canonical quantization, the topology of the 3D (I assume) cross section at any given fixed time is always the same? The boundary would be the spatial boundary, i.e. the boundary of the observable universe?

Thanks…

58. **Jesper**
February 17, 2014

Dear Lee

when you write that the topology in LQG is fixed – is that really the case? The configuration space of generalized connections, which is the basis for the entire LQG quantization scheme, involves all possible smooth connections on a given manifold (with a given topology) as well as a all the non-smooth generalized connections (which form the 'bulk' of this space connections). Couldn't some of these (generalized) connections correspond to geometries of spaces with different topologies than the original fixed one?

For instance, assume that we start with the trivial topology (which seems natural), then I guess there would also be points in the space of generalized connections, which actually correspond to a non-trivial topology?

59. **Narad**
February 17, 2014

From Frenkel:

> This does not stop [Tegmark] from entertaining various situations involving multiple copies of "you" living in those universes, such as: "When the number of yous increases, you perceive subjective randomness. When the number of yous decreases, you perceive subjective immortality." (The real question, however, might be, What is the number of yous who can understand what this means?)

Funny, that's not quite how it worked in *The Man Who Folded Himself*.

60. **Lee Smolin**
February 17, 2014

Dear FHA, yes to all.

Dear Jesper, Even if the answer was yes, which would need to be shown, time reversibility of the dynamics suggests that if such a sector could split off it could rejoin… but I doubt Peter wants to host a technical discussion of LQG dynamics.

Lee

61. **Peter Woit**
February 17, 2014

Lee is right. I'm glad to have his comments here, but this isn't a great place to try and host a technical LQG discussion. It would be pretty much off topic, and beyond my abilities to sensibly moderate.

62. **Jesper**
February 18, 2014

Dear Lee,

yes, I think you are right and I don't mean to suggest that there is a multiverse setting within LQG. But I have never been convinced that there can't be topological non-trivialities arising in LQG.

And sorry Peter – I knew it was semi-off-topic.

US HEP Theory Funding News

February 20, 2014
Categories: Uncategorized

Laurence Yaffe has gathered some information about DOE funding of US HEP theory groups, showing sharp drops (average 23%) in such funding for groups reviewed in FY2013 and FY2014. These drops imply serious reductions in the numbers of theory graduate students supported and in the number of postdoc positions funded. To get an idea of the reaction to these numbers, the file name is calamity.pdf. Sean Carroll has a blog posting here, also with the "Calamity" title.

There's something very odd though about these numbers, with no explanation available from Carroll or Yaffe, who both interpret the situation as a sharp reduction in US government support for HEP theory. If you look at the numbers for total amount of funding, there's no evidence of the 23% drop (for sources of numbers, see references in Yaffe's document, the DOE HEP budgets here, and HEPAP presentations here). The recent Congressional agreement on a FY2014 budget puts the DOE HEP total at $798 million, significantly higher than FY2013 ($750 million) which was reduced by the sequester, and higher than FY2012 ($770 million) and FY2011 ($776 million). Of these amounts, the amount going to theory seems stable at around $50-$55 million/year (exact numbers depend on exactly what you include, Yaffe quotes FY12 and FY13 at an identical $51.3 million/year).

So, the "calamity" of collapse of government support for theory is somehow taking place despite no collapse in the amount of money budgeted for this. What is going on? I'd love to hear from someone who understands this. The only explanation I've seen is that this is a temporary phenomenon having something to do with how the budgeting process works. Note that with the way typical multi-year grants are made, the DOE is promising to provide money several years in the future, despite the current US budgeting environment, where budgets are typically set not in advance, but often very late. The current fiscal year's budget was set last month, although the fiscal year started Oct. 1. This was considered a huge success… One conjecture is that the DOE has been promising less to theory groups, partly because they only recently found out about the good FY2014 result, and partly because of a policy change to budget for future year outlays in current years. If this is the source of the calamity, it should quickly disappear as the FY2014 money comes in and the transition to new budgeting ends. Yaffe examines some other possibilities for explanations, but without anything conclusive.

The main concern raised is about the effects of a reduction in the number of US HEP theory grad students and postdocs. Perhaps more worrying than this though should be the trend of numbers of permanent research positions in the subject, which Erich Poppitz has gathered here. These numbers come from the Theoretical Particle Physics Jobs Rumor Mill, and show a constant level of 11 jobs/year for the past three years, about half the level of the years 2000-2007. The number of jobs posted this year is so far only 13, which can be compared to around 20/year in recent years. It's still early in the season, so more jobs may appear, and more may get filled than in recent years, but the trend of US institutions hiring significantly fewer permanent people in HEP theory is clear. Given this trend, there's a reasonable argument to be made that numbers of grad students and postdocs should also be reduced. There's no evidence though that some decision has been made to do this, or that this is the reason for the numbers Yaffe is looking at.

Comments from anyone well-informed about this are strongly encouraged, while comments from people who just want to make the usual arguments about government funding for science rules/sucks will be deleted.

Update: More commentary at Ted Bunn's blog. He sees this as possibly an implementation of changes at DOE designed to "decrease the effect of historical inertia", which might be moving money from historically well-funded major groups to others.

Possibly all that's going on though is just the "bridging" problem mentioned on slide 7 of this presentation. A decision was made to start grants later than in the past on April 1 (with the idea that typically it's not going to be until that deep in the fiscal year that Congress has got its act together and given DOE a budget number). This meant though that money had to be taken out of upcoming grants to cover extending current ones until April 1 when new ones might start. The number on that slide reflects a 25% charge to new grants to cover this, suspiciously close to Yaffe's 23% number.
The same slide though does point out that this is a temporary problem: "will be better in 2015". If that's all it is, and grad student/postdoc number are slated to go back up a couple years from now, maybe "calamity" isn't the right word here.

Update: For more from Yaffe, see here.

Comments

1. **AcademicLurker**
   February 20, 2014

   At the end of calamity.pdf Yaffe lists the 25 institutions from which he collected data. What % of total HEP theory research in the US do these institutions represent (honest question. I have no idea)? Maybe there's been a shift of support from a few large groups to lots of smaller ones?

2. **QM**
   February 20, 2014

   If you want more readers of the sympathetic yet amateur nature, you could expand a few acronyms, such as HEPAP (High Energy Physics Advisory Panel) and HEP (High Energy Physics).

3. **Noah Smith**
   February 20, 2014

   The hiring data doesn't look like a secular downward trend, it looks like a boom in the '00s that is now over. If hiring goes back to 90s levels, it hardly seems catastrophic…

4. **Peter Woit**
   February 20, 2014

   Noah,

   Given that there were about 80 US theory Ph.Ds/year during the 00s (best estimate I could make when I was writing my book), so about 4 times as many as jobs, I don't remember anyone back then thinking that was a "boom" in the job market. There was an awareness that things had gotten a bit better than in the 80s-90s when the job market was even worse, but one person's "boom" is another person's "return to normal level".

   More relevant would be a comparison of the HEP theory job market with other parts of physics and with other fields in academia. My purely anecdotal impression of the math job market is that (after post 2008 problems), it is getting back to something like the market of the 00s, and certainly not something like the HEP situation of half as many jobs. Again, anecdotally, I hear of places where universities are cutting back in physics as opposed to trendier sciences (buzz words here are "data science" and "personalized medicine"), and of physics departments where HEP physics is viewed as having an uncertain future.

5. **Jollick**
February 20, 2014

Peter,
Is there a summary of such anecdotes somewhere, or would you mind sharing yours? A rumor-mill of which physics depts or HEP groups might have uncertain futures would be very interesting to a lot of people. (Such as those people whose own futures depend on them, for example.)

6. **Peter Woit**
February 20, 2014

Jollick,
I don't have any very significant information of that kind. I was more referring to general comments about the perception of physics in general and HEP in particular among administrators and academics in various fields. At any given moment there are certain subfields of science getting attention as "the next big thing" in terms of advances, especially technological ones. "Data Science" is hot now, and as I mentioned, I've heard that "Personalized Medicine" is starting to get attention. In physics, the promise of quantum computation has off and on gotten this sort of attention. In the run up to the LHC start, LHC physics was a hot topic, and if you wanted to get an HEP job, there was a time when that was the thing to be doing.

So, all I meant to indicate is that I don't see HEP physics as something perceived anywhere as a "hot field", which university administrations want to expand in, so the job market is likely to remain limited for the forseeable future.

7. **Jeff M**
February 20, 2014

Peter,

As far as the math job market goes, good it's not, though of course it will never be as bad as physics. This year we're hiring, and we put our ad in late (December). We still got about 350 applications, we're a regional comprehensive state school, you're expected to do research but you also teach a pretty heavy load. Historically in math the market was quite good until 1992 (the year I got my doctorate, perfect timing 🙂 The market got worse for maybe 5 years, and then as far as I remember the VIGRE grants started up, and things got better. Problem was it was a temporary fix, once people started coming out of the VIGRE things got much worse again, and haven't really improved since then. Most math PhD's can get academic jobs of some sort, unlike in physics, but it's a tough slog and you're likely to end up with not what you were expecting.

8. **Laurence Yaffe**
February 20, 2014

Peter, according to information I just learned today, the DOE/HEP theory budget for FY14 is down about 5% from last year, despite the fact that the total DOE/HEP budget is up substantially. This presumably reflects the planned rebalancing between research and projects. I would call a 5% cut a significant reduction, but not a calamity. What turned this into a 20+% hit on renewing grants was a decision to shield from cuts (or at least, cut much less than 5%) a majority of the DOE-supported theory program, namely lab-based theory groups, and university-based groups who had undergone comparative review in earlier years. So the central concerns are not changes in total funding for high energy physics, or even total funding for high energy theory, but the allocation of funds which were available, and the resulting impacts on young researchers.

9. **Peter Woit**
February 20, 2014

Thanks Laurence,
That starts to make things clearer. A 5% cut is a significant one, and I can see how a decision to take it out of just one subclass of grants could magnify it greatly for those affected.

Does the change in priorities from "research" to "projects" inherently mean a change in favor of experiment (since only experimentalists have "projects"), or are there also theory "projects"?

10. **JoAnne Hewett**
February 21, 2014

Hi,

I am the department head of the theory group at SLAC National Accelerator Laboratory. Larry has written an interesting piece, and he has nailed a serious problem: funding for theory is decreasing, year after year, in order to divert more money to the project line. This is also true for the experimental research lines, but, in principle, they can recoup some (but not all) of the monies from the project lines.

The real problem is decreasing monies for science. The DOE theory research line was cut by ~5% in FY14, after a cut in FY13, after a cut in FY12…. This is bound to result in a decrease in postdoc and student positions, and senior faculty salaries. This decrease in funding is combined with a change in the funding process for the DOE, namely the institution of comparative reviews. A perfect storm! The comparative review process is similar to what has been done at the NSF for years. The labs have undergone comparative reviews since 2008, and the universities started in 2012. Personally, I think this is a very fair way to allocate ever decreasing funds. Rather than let historical precedence be the guide, what matters most now is what a researcher actually does. The most productive researchers are being rewarded and whatever fat there was is now out of the system. (I have to say that I come from a working class family and I was brought up to think that what I actually did is what matters, so I like the new system.)

Nonetheless, the numbers and graphs in Larry's paper are misleading. First, only some fraction of the groups responded – generally it's the folks that wish to complain that respond. Second, the DOE instituted salary caps ($15k/month) for summer salary payments and this was not factored into his graphs depicting the decrease in funding. I know of some groups whose cuts were solely from the caps. (It's worth noting that lab scientists are in general paid less than their university counterparts and few of us would hit this cap.) Third, the weak FY13 postdoc hiring cycle is a direct result of the FY12 (and not FY13) comparative reviews due to the timing of the annual hiring cycle. Fourth, the picture at the labs is not as rosy as Larry paints. Lab groups have been cut every year, in sync with the overall theory budget. After the last lab comparative review, several senior scientists with lab "tenure" were fired. Postdoc positions were reduced. At SLAC, we have graduate students from Stanford, and the number of student positions we could afford was cut in half.

Lastly, I would like to stress that the single worst thing the theory community could do is to fight amongst itself. We could divide ourselves up many different ways: lab vs university, formal vs phenomenological, old vs young, red-headed women who wear blue-jeans on Tuesdays vs everyone else….and this hurts us greatly. Divide and conquer is the famous phrase and that is appropriate here. If half of the community loses its funding, it goes to projects, not the other theory half. We must stick together and fight for appropriate funding for a world-leading theory program.

-JoAnne Hewett

11. **CPV**
February 21, 2014

It would seem like the actual theory reductions in DOE funding are being more than compensated for by private funds (FXQi, Milner, Perimeter, etc). The targeted and concentrated nature of the private money is problematic however.

12. **Gonçalo Dias**
February 21, 2014

As far as I can tell, the same is happening here in Portugal, with a tremendous transfer of funds from individual grants and money for fundamental research to the so-called projects and applications. The trend is a cutback from fundamental and theoretical research and a move to "corporate science". Unfortunately, this new trend is nothing like the olden days of, say, Bell Labs. If this is any indication at all of a wider trend in the EU, I guess the move is timed to happen at the same time on both sides of the Atlantic. This means that the old university-based research is at an end and so the future of most of fundamental research.

13. **Peter Woit**
February 21, 2014

Thanks Joanne!

What I'm now finding hardest to understand is why DOE would make such large cuts in grant amounts to some subset of the community, seemingly without providing any explanation for why this is being done. There seems to be agreement that part of the problem is a significant 5% FY14 cut to overall theory research (has there been a rationale given for this choice?) Beyond that though, it seems unclear whether the much larger cuts seen by some groups are due to: salary caps, better treatment of the labs, better treatment of groups reviewed earlier, the April 1 bridging problem, change in policy about future year commitments, rebalancing from well-funded groups to others, or something else. If the DOE does not clarify things, that will make worse the problem Joanne points out of people concluding that others are being better treated than they are for dubious reasons.

14. **Shantanu**
February 22, 2014

Peter, this maybe a controversial pov, but so far people working on beyond standard model of particle physics(whether it is supersymmetry/technicolor/extra dimensions etc) have failed to make a single successful prediction which has been verified by accelerators. So certainly I wouldn't be surprised if there is a cut in HEP theory funding. Certainly I hope more funds are allocated to new research directions.
This is something Avi Loeb has advocated in astrophysics/cosmology.
http://online.kitp.ucsb.edu/online/colloq/loeb1/
But same idea applies to HEP.

15. **Peter Woit**
February 22, 2014

Shantanu,
I just don't see any evidence for the idea that these new cuts in HEP theory are due to lack of progress in the field, or that funds are being redirected to new research directions. If anyone has real evidence for a DOE policy change of this kind, that would be interesting, but otherwise I'd rather people not speculate here based on what they would like to be true.

16. **M.Wang**
February 23, 2014

I just chatted with a bunch of Biology and Chemistry professors over the weekend on the Federal funding situation. There the money crunch does not appear as severe. NIH funding is stable; only NSF money seems threatened. One department chair has an appointment with a congressman on Monday. Instead of the overall funding level, her main concern appears to be the (over-)emphasis on applied research projects over fundamental ones.

17. **Maynard Handley**
February 23, 2014

"Given that there were about 80 US theory Ph.Ds/year during the 00s (best estimate I could make when I was writing my book), so about 4 times as many as jobs, I don't remember anyone back then thinking that was a "boom" in the job market."

What is the precise fear that people have here?
I see two very different issues which people seem to be commingling.

(a) Not "enough" support for the field, meaning not enough money to pay for what is considered a reasonable (by some sort of metric) number of profs and their assistants (at all levels, from postdocs to grads to, maybe, technicians like programmers).

(b) Not enough academic theoretical physics jobs available for academic physics PhDs.

These strike me as very different problems. In particular, (b) strikes me as very unlikely to be a real problem in any sense. The people who get theoretical physics PhDs do so because they have a compulsion they cannot control, not because of rational calculations about job opportunities. So I don't see the flow of talent into the field drying up.

On the other side, I can't see much individual tragedy happening (except insofar as there are ALWAYS going to be losers with positional games, and that's life). People who get theoretical physics PhDs are among the smartest people out there, and employers know that. You may WANT to be a prof at Harvard, and be bitterly disappointed that you cannot even be a PostDoc at State U, but you're not going to starve. Depending on your inclinations, you can get very rich in finance, you can join an internet startup and figure out new algorithms, you can join an established company (Google, Apple, FaceBook etc) and make a comfortable living as a technical programmer, you can move sideways into EE, and so on.

Maybe I'm wrong here — I'm not trying to be a dick. I'm just looking at my personal experience and the experience of my cohort. A few in finance, a number scattered over computing over all sorts, one at a government lab, one acting as a high level technical project manager for the feds, and so on.

So while concerns for the amount of money supporting the field strike me as justified (if they ARE justified by reasons why this amount of money is too low — and of course we all WANT the amount to be higher, but let's be realistic); bringing the academic jobs for PhDs issue into it strikes me as confusing the issue in a way that's completely unhelpful.

18. **Peter Woit**
February 24, 2014

Maynard Handley,

The problem Yaffe was discussing was very specifically about DOE funding of postdoctoral positions (and to some extent graduate fellowships). These positions are intended as training positions for permanent positions involving research in HEP theory, of exactly the sort (tenure-track academic positions or laboratory positions) followed by the rumor-mill. So the question of how many postdocs the DOE should fund is closely related to the question of how many positions are available for such trainees.

Again though, while the decrease in available HEP theory permanent positions would be a sensible reason for reducing the numbers of postdocs funded, I haven't seen any evidence that that's the actual reason for the recent decrease that Yaffe is concerned about.

More Quick Links

February 24, 2014
Categories: Multiverse Mania, Uncategorized

First, a couple of examples of recent progress in mathematics

- Terry Tao has some new ideas about the Navier-Stokes equation. See his blog here, a paper here, and a story by Erica Klarreich at Quanta here.
- I've been hoping to find more time to learn enough to write something intelligible about a major new advance: Peter Scholze's recent work on the p-adic geometry of Shimura varieties and results linking torsion classes and Galois representations. I'm still far from being up to that task, but Scholze's Marston Morse lectures at the IAS are a good place to start (see here, here and here). Last week MSRI hosted a very successful week-long "Hot Topics" program on this, see here.

- For more IAS talks, see "Cross-disciplinary" talks last week by Witten, Seiberg and Maldacena.
- Nature has a story about a recent discovery by Cormac O'Raifertaigh and collaborators of an unpublished manuscript by Einstein containing a "steady-state" cosmological model.
- A computer scientist has identified more than 120 papers published in supposedly peer-reviewed conference proceedings that were all randomly generated gibberish produced by the program SCIgen. No, these didn't appear in bogus "open access" publications, but in subscription publications from Spring and the IEEE. What's going on is described as a "spamming war at the heart of science".

Update: One more, for those of you not getting enough multiverse. Today's Washington post has an op-ed from Bush speech-writer Michael Gerson (at one point the ninth most influential evangelical Christian in the US, if you believe Wikipedia and Time). The title is Physics is Enjoying a Golden Age (also available here). Gerson thinks physics is in a Golden Age because he has just read Tegmark's book and is very excited that physics has now become metaphysics, with room for God again:

> The point here is not that Tegmark's theories are broadly accepted, only that such theories are no longer considered absurd. Physics has seen the return of the unseen — parallel universes, infinitesimal strings, floating and colliding branes — that are reasonably inferred without being physically observed. I can think of other creative forces in that category. Not for centuries has physics been so open to metaphysics, or more amenable to an ancient attitude: a sense of wonder about things above and within.

Comments

1. **Chris Oakley**
   February 25, 2014

   Just to record that I share Michael Gerson's sense of wonder at parallel universes, infinitesimal strings, floating and colliding branes, etc. being considered to be physics.

2. **SteveB**
   February 25, 2014

   I loved Terry Tao's description in his blog:

   (One could describe the dynamics here as being similar to the famous "lighting the beacons" scene in the Lord of the Rings movies, except that (a) as each beacon gets ignited, the previous one is extinguished, as per the energy identity; (b) the time between beacon lightings decrease exponentially; and (c) there is no soundtrack.)

   Humor in a Mathematics article is a nice touch.

3. **Tom**
   February 25, 2014

   I have no dog in this hunt, but it seems you (Peter) especially relish informing us that Gerson is a
   "Bush speech-writer …. [and] at one point the ninth most influential evangelical Christian in the US …"
   even though that's mentioned nowhere in that op-ed.

   Maybe this antipathy comes from being immersed in the prog-liberal, urban "bubble universe" that is Manhattan NYC ? After all, the geocentric cosmology model wrongly persisted for many centuries, too 😉

   Since Gerson is by all accounts a layman and has never trained as a scientist, he might be forgiven for thinking the constant onslaught of sensationalist books by Kaku, Tegmark, etc, in addition to the many breathless articles in New Scientist, Scientific American, etc, might imply those multiverse guys are taken seriously by the scientific community.

   As least, Gerson does seem to acknowledge Tegmark's so-called "theories" are not broadly accepted.

4. **Peter Woit**
   February 25, 2014

   Tom,
   There's no "antipathy" in my characterization of Gerson (which I think is accurate). I thought it was important to give some indication of who he is and what sort of agenda he is likely to have. This agenda pretty clearly includes bringing religion into science, and this is why he's excited by the pseudo-science of Tegmark and the "Golden Age" he sees it as ushering in.

   As for my agenda, I think it's obvious, but to make it explicit: I've no antipathy towards religion or religious people, I just don't think religion or metaphysics belongs in serious theoretical physics. I also think that those pushing the multiverse agenda, while at the same time holding up physics as the answer to religion, need to be made aware of what effect they are having. A good example is Sean Carroll, who seems to be half the time engaged in a public war for science against religion, the other half of the time trashing the scientific method and handing ammunition over to evangelicals like Gerson who have an opposite agenda.

   All, unless you've got something new and interesting to say about the multiverse/religion business, please stop submitting comments about it. I've already deleted a bunch, perhaps am making a mistake to answer this one, but, enough already…

5. **cormac**
   February 25, 2014

Thanks for that Peter. It's a very nice article, but I should say that Einstein's attempt at a steady-state model is motivated by the problematic age associated with evolving models, not the problem of origins, as implied by the article. One of the interesting aspects of the manuscript is that it predates the Lemaitre-Eddington debate concerning a beginning for the universe!

6. **Peter Shor**
   February 26, 2014

   I've occasionally wondered whether any physicists (possibly subconsciously) are supporting the multiverse for antireligious reasons … if you accept the multiverse, maybe you don't have to leave open the possibility that God wrote down the laws of physics and put the Big Bang into motion.

   If so, this strategy appears to be counterproductive.

7. **Peter Woit**
   February 26, 2014

   Peter Shor,
   Many multiverse proponents specifically make the case that one of its virtues is that it provides an explanation for fine-tuning that answers the argument from design for a deity. I agree that this is counter-productive: invoking untestable physical theory to try to win this particular argument means you've abandoned the fundamental distinction that separates what scientists do from what theologians do. Once you do this, you're on the same footing as Gerson and everyone else who wants to argue metaphysics without the discipline of making statements that can be tested and falsified.

8. **Geoff**
   February 26, 2014

   Reading Tao's ideas about the Navier Stokes equations reminded me of a very old sci-fi reference to a fluid mechanics computer from the movie Rollerball. The description occurs at the 3:20 mark.

   http://www.youtube.com/watch?v=qmTWhvWgST0

9. **publius**
   February 26, 2014

   Actually Terry Tao very interesting ideas remainded me of a Science Fiction piece too, Ted Chiang´s Exhalation
   http://www.nightshadebooks.com/Downloads/Exhalation%20-%20Ted%20Chiang.pdf
   Only that in Chiang´s story solid parts are required.
   It is fun to esspeculate if these fluid computations may be realized in stars or gas giant planets as some sort of "life" (though I see it as very unlikely)
   By the way

Quantum Mechanics, The Theoretical Minimum

February 25, 2014
Categories: Book Reviews

In recent years Leonard Susskind has been giving an excellent series of lectures on basic ideas of theoretical physics, under the title The Theoretical Minimum. The general idea seems to be to provide something in between the usual sort of popular book about physics (which avoids equations and tries to give "intutitive" explanations in ordinary language) and conventional undergraduate-level textbooks. Such textbooks generally assume college-level multi-variable calculus, differential equations and linear algebra, and often skip lots of detail and motivation, assuming that the book is a supplement to a standard course of lectures.

For Susskind's lectures, you mostly just need high-school level mathematics, up to some some basic differential calculus, as well as two by two matrices. Actually though, if you've never seen matrices and very simple linear algebra, this is a good place to learn some basics examples of this subject.

A year ago the first book version of some of the lectures appeared as The Theoretical Minimum, with George Hrabovsky writing up Susskind's lectures on classical mechanics. I wrote a little bit about the book here, and was quite impressed by the way it managed to give the details of the formalism of Hamiltonian mechanics, while sticking to as simple and concrete mathematics and calculational tools as possible.

Today is publication day for the next volume, Quantum Mechanics: The Theoretical Minimum, which is a joint effort this time with Art Friedman. It's even better than the first volume, taking on a much more difficult subject. About the first two-thirds of the book sticks to the simplest possible quantum system, one with a two-dimensional state space. The linear algebra needed is developed from scratch and Susskind works out at a very leisurely pace all the details of what the quantum picture of reality looks like in this simplest context. There's a lot about what "entanglement" really is, and this part ends up with an introduction to Bell's theorem.

The last third of the book is a quicker-paced trip through the usual material about wave-functions and the Schrödinger equation, ending up with the details for the harmonic oscillator potential.

"The Theoretical Minimum" phrase is a reference to Landau, but it's a good characterization of this book and the lectures in general. Susskind does a good job of boiling these subjects down to their core ideas and examples, and giving a careful exposition of these in as simple terms as possible. If you've gotten a taste for physics from popular books, this is a great place to start learning what the subject is really about.

I only noticed one mistake in the book, on its back cover, where one of the blurbs is attributed to a Professor of Mathematics at Columbia, when I know for a fact that his actual title there is "Senior Lecturer". Susskind does have a bit of history of getting this point wrong, but probably the fault here lies with the publisher.

Update: Nature has a review here.

Comments

1. **Lowell Boggs**
   February 25, 2014

   I just finished reading the Theoretical Minimum. Thank you for mentioning it in your blog or I would not have thought to read it. I particularly like the way that the book re-derived calculus and showed how the Fundamental Theorem of the Calculus could be used in physics to solve some kinds of problems.

   My degree was in Electrical Engineering in 1980, so I had taken most of the background science classes that Susskind is describing. However, I do not recall the concept of the Lagrangian being discussed in my physics or electrical engineering classes. It was at this point in the book, that I wish the author had gone into a little more detail about how Action differs from Work or at least more into the thinking process that led to Lagrange's equation. The difference between kinetic and potential energy, as I recalled, was the work. Sadly this disconnect between my recollection of my physics classes and the introduction of this new material left me a bit confused through the remainder of the book. I am however, hungry to understand Lagrangian and Hamiltonian mechanics better. Is there a another book on the subject that you might recommend?

   Thanks,
   Lowell Boggs

2. **Peter Woit**
   February 25, 2014

   Lowell Bogg,
   Sorry but this isn't material I've taught at this level, and I don't know the textbooks. When I studied the subject at an advanced undergraduate level the textbook was Goldstein's Classical Mechanics, but I found that rather dry and hard going. Presumably there are some good modern textbooks, at a bit lower level than Goldstein, maybe others can suggest a good one.

3. **Harlequin**
   February 25, 2014

   The classical dynamics text I used as an undergraduate was Thornton & Marion, Classical Dynamics of Particles and Systems, which has a chapter devoted to the Lagrangian and Hamiltonian. I don't know if anything new has come up in the decade plus since I took that class as a sophomore physics student, though. Looking at the textbook now, the introduction to the chapter seems pretty good; it does the thing I very much appreciate in physics textbooks of explaining *why* you would want to do something a particular way, not just *how* to do it that way.

4. **AcademicLurker**
   February 25, 2014

   The review of classical mechanics in chapter 2 of Shankar's quantum mechanics textbook* is quite good. It's very brief, as it starts with the principle of least action and goes through the Lagrangian and Hamiltonian formulations of mechanics in about 30 pages. Obviously, a lot is left out, but for a quick and intuitive introduction to the ideas at the undergraduate level, I found it was pretty nice.

   *R. Shankar, *Principles of Quantum Mechanics*

5. **CIP**
   February 25, 2014

I haven't read Susskind's TM books, but I have the feeling that the theoretical Universe has rolled off to decay into a considerably lower TM since L & L.

6. **AcademicLurker**
   February 25, 2014

   I haven't read Susskind's TM books, but I have the feeling that the theoretical Universe has rolled off to decay into a considerably lower TM since L & L.

   According to wikipedia, only 43 people passed the famous "theoretical minimum" exam between 1934 and 1961.

7. **George Ellis**
   February 25, 2014

   Lowell Bogg:

   when I looked up Susskind's book by following the link Peter gives above, Amazon.com in its wisdom guided me to "A Student's Guide to Lagrangians and Hamiltonians" by Patrick Hamill. From the reviews, it seems to be quite good.

8. **Sakura-chan**
   February 25, 2014

   Taylor's "Classical Mechanics" is as good as they come.

9. **Allan Rosenberg**
   February 25, 2014

   Thanks, I didn't know about the books. I went from freshman physics to the theoretical minimum by watching Susskind's lectures on classical mechanics and QM on youtube, and I can veryify from that perspective that they are excellent. I'm looking forward to the rest of his lectures and to reading the books.

   Senior lecturers in math at Columbia have done some excellent work, though I didn't know that Susskind was so happy with their reviews of his work. 🙂

10. **David Brown**
    February 25, 2014

    "… reference to Landau …"
    https://en.wikipedia.org/wiki/Kharkiv_Theoretical_Physics_School

11. **Art Brown**
    February 25, 2014

    Visually, this new book is a stinker; production values are noticeably worse than in the classical dynamics volume. I'd soon be exhausted trying to work through it.

    That aspect aside, I'd love to hear why one would be better served with this book than with volume 3 of the Feynman lectures (available free on-line). (By the way, Vol II ch. 19 includes a v nice introduction to the Lagrangian approach.)

12. **paddy**
    February 25, 2014

    I recall in ~ 1974 Wendell Furry teaching EMT based on Landau and Lifshitz's "Electodynamics of Continuous Media". It certainly was an eye opener for a young guy "raised" on Jackson. Not sure what I feel about physics students lectured from either the "dumbed down" or "scaring off" sources.

13. **Low Math, Meekly Interacting**
    February 25, 2014

    I may just give these a look. I recently took an intermediate stats class and a course in electrophysiology, and was happy to see that my math muscles, such as they are, haven't completely atrophied. I've done the calc and actually had some linear algebra in a pre-calc course (unorthodox high-school math teacher), but it's been ages. I don't know why, because I'll probably never use it professionally, but somehow dabbling in such things on the side does me a lot of good.

14. **MahPhys**
    February 26, 2014

    I personally don't think there is a textbook that competes with Goldstein's Classical Mechanics or with Jackson's Classical Electrodynamics. Probably no one thinks their time is worth the effort to come up with a better textbook.

15. **imho**
    February 26, 2014

    Fetter and Walecka is a better is better than Goldstein

16. **publius**
    February 26, 2014

    As a textbook Gregory´s Classical Mechanics chapters on actions, lagrangians and hamiltonians are both short and very clear, with plenty of good examples and exercises.
    Taylor´s and Kibble´s book are pretty good too.
    Perhaps surprisingly, the first chapters of A. Zee Eintein´s Gravity in a Nutshell contain an amusing introduction to variational calculus, actions and lagrangians.
    For the historical and metaphysical context of the action principle and its origins, Ivar Ekeland book The best of all possible worlds gives an excellent, and very readable and insightful introduction

17. **Lowell Boggs**
    February 26, 2014

    Many thanks to all who suggested ideas for further reading. I have ordered the Hamill book and have read through the Feynman lecture on the principle of least action. And "work" has nothing to do with it.

    Thanks Again!

Lowell

18. **Peter Woit**
February 26, 2014

Maybe that's enough comments about people's feelings concerning which textbooks on a completely different topic at a completely different level they like or don't like…

19. **John Anderson**
March 10, 2014

Be aware that there are older versions of Susskind's lectures at http://www.newpackettech.com/Resources/Susskind/PHY25/QuantumMechanics_Overview.htm The newer lectures are at http://theoreticalminimum.com/courses/quantum-mechanics/2012/winter
I made the mistake of watching the older ones which aren't as closely coordinated with the book that I am over halfway through.
At my age, I find the book challenging and the logic sometimes discontinuous. (Not trying to pun.) Perhaps I should have just watched the lectures.

20. **Ahish**
March 11, 2014

Consider this book Cornelius Lanczos – "Variational principles in Mechanics"

All Hail Resonaances

February 26, 2014
Categories: Uncategorized

I'm delighted to see Jester back in action, providing great material on the current state of HEP physics, with, over the past week and a half:

- A sober look at the sparse prospects for near-term (i.e. 2014) input from experiment, with the Planck CMB polarization results one of the few things for which there are significant expectations.
- An equally sober look at the problem of making the case for a 100 TeV collider, given that this will be the first time people don't have a "no-lose theorem" showing that something new has to turn up in the new energy range being explored (of course the argument that it's an unexplored new energy range remains an excellent one by itself for the exploration). About the arguments Arkani-Hamed is making, Jester has:

  > Nima's idea that we need a 100 TeV collider to prove that SUSY fine-tuning is larger than 0.01% is good. As a joke to relax the atmosphere. Certainly, the case for the new collider can be made stronger than that.Some ideas that are being bandied around are precision Higgs physics, double Higgs production, rare Higgs and top decays, non-perturbative electroweak effects, or WW scattering. These topics can be made more concrete and several more items can be added to the list.

- To give us all some hope, he has some news about a possible astrophysical X-ray spectrum signal that could conceivably be evidence for a sterile neutrino dark matter candidate. Right-handed neutrino fields fit naturally into the SM pattern of fundamental fields, but with zero SU(3)xSU(2)xU(1) charges. That such fields have something to do with dark matter looks more promising than the SUSY or axion proposals of introducing a new and different sector of fields. My knowledge of neutrino physics isn't what it should be, so I'd be curious to hear of good references about the sterile neutrino dark matter issue.

Update: For some idea of the case being made for a larger collider, one might want to take a look at talks in Beijing a few days ago, where there's a proposal for the Chinese to build it. Talks at a conference are here, and last Sunday there was a big event featuring Yau, Gross, Witten, Arkani-Hamed, 't Hooft, Maiani and Incandela, video here. On the whole people seem to be pretty much sticking to making the generic case for high energy, not promising superpartners or extra dimensions this time.

Comments

1. **Per**
   February 26, 2014

   I work in hep-th and there is nothing I'd love more than to find experimental evidence for SUSY.

   However, I still don't thinks its economically motivated to build a 100 TeV collider. There are other scientific areas which are more important, for example fusion and renewable energies. While funding in one field does not by necessity exclude funding in another, a mega project like a 100 TeV collider will cause huge deficits in other fields.

2. **nolose**
   February 26, 2014

   No lose?
   For the LHC, certainly, there were very strong indications that it would discover the Higgs boson. The Standard Model had already been vindicated in great detail while the LHC was in its design stages.

   The Tevatron was *not* "built to discover the top quark."
   The Tevatron was built to reach an energy of 1 TeV/beam, to explore what was out there. Note also that PEP, PETRA and TRISTAN were all built to discover the top quark.

   LEP was built to discover supersymmetry?
   Speak to Burton Richter about what he said to the CERN management circa 1981 about building an e+e- collider on the CERN site. CERN had built only hadron machines up to then.

   The SppS was certain to discover the W and Z bosons.
   This is a bit complicated. The SPS was a fixed target proton synchrotron. Once the masses of the W and Z had been estimated (indirectly) by other experiments, then there was a case to convert the SPS into a p-pbar collider. This was the stochastic cooling story, etc. But the SPS needed to be there first, and the SPS was not built to discover the W and Z bosons. Note also that Rubbia, Cline and Peter McIntyre proposed the stochastic cooling idea to FNAL first, to convert the Fermilab Main Ring into a collider to produce the W and Z bosons, and they were kicked out. The Tevatron came online in 1985, after CERN had already discovered the W and Z bosons, and before anyone had any firm estimate of the top quark mass. (And Rubbia *did* discover the top quark at the SppS, at a mass of 44 GeV/c^2.)

3. **Daniel Rocha**
   February 26, 2014

   These guys propose using the abandoned tunnels of SSC (45%) to build a 100TeV collider for the cheap price of ~1.5billion$

   http://arxiv.org/abs/1402.5973

   It's an interesting read.

4. **Peter Woit**
   February 26, 2014

   Daniel Rocha,
   They're talking about two colliders, a 240 Gev electron-positron machine in the SSC tunnel, and a 100 TeV hadron machine in a completely new 270 km tunnel. I don't see where you get $1.5 billion, the only cost numbers I see there are for $1.32 billion for a 270 km tunnel and $740 million or $1.9 billion just for the cost of the wire for the magnets in that tunnel.

   The order of magnitude for this scale machine is likely to be $20 billion, and I don't see how building the thing really huge with lower field strength magnets is going to dramatically reduce the cost. You then need to instrument a 270 km long experimental apparatus.

It seems within the realm of believability that CERN might be able to fund something on this scale over the next 20-30 years. The chance of this happening in the US I think is zero.

5. **Don Murphy**
February 27, 2014

Peter
Here is a post by Matt Strassler that gives some information and references on the X-ray spectrum you mentioned that may have something to do with dark matter:
http://profmattstrassler.com/2014/02/18/x-rays-from-dark-matter-a-little-hint-for-you-to-enjoy/

6. **Michael Brown**
February 28, 2014

A few references on the subject of neutrino dark matter (from my library, but I'm not an expert on this). See [1] for a set of lectures on the astronomical signatures of ~keV scale neutrino dark matter. [2] is an extensive review of the nuMSM = "neutrino minimal standard model", the absolute minimal model with sterile neutrinos and nothing else coupling to the SM below the Planck scale. [3] is a shorter presentation of the essential points by the same authors.

For a broader review of right handed neutrino pheno that is not wedded to the nuMSM or a particular dark matter scenario see [4]. See [5] for a "model builder's guide" and [6] for a feasibility study on collider experiments to explore the whole range of sterile neutrino masses below a few GeV.

Finally, I would just like to point out that Shaposhnikov, one of the authors on the nuMSM papers, correctly predicted [7] (with Wetterich) the Higgs mass on the basis of: a) no new physics coupled to the SM fields between the weak and Planck scales and b) asymptotically safe gravity. It's interesting that the idea works so well.

Surely you'll have no trouble navigating the literature, but I hope this helps a little!

Cheers

[1] H. J. de Vega and N. G. Sanchez, (2013). http://arxiv.org/abs/1304.0759

[2] L. Canetti, M. Drewes, T. Frossard, and M. Shaposhnikov, Phys. Rev. D 87, 093006 (2013). http://arxiv.org/abs/1208.4607

[3] L. Canetti, M. Drewes, and M. Shaposhnikov, Phys. Rev. Lett. 110, 061801 (2013).

[4] M. Drewes, Int. J. Mod. Phys. E 22, 1330019 (2013). http://arxiv.org/abs/1303.6912

[5] A. Merle, Int. J. Mod. Phys. D 22, 1330020 (2013). http://arxiv.org/abs/1302.2625

[6] S. N. Gninenko, D. S. Gorbunov, and M. E. Shaposhnikov, 20 (2013). http://arxiv.org/abs/1301.5516

[7] M. Shaposhnikov and C. Wetterich, Phys. Lett. B 683, 196 (2010). http://arxiv.org/abs/0912.0208

7. **Peter Woit**
February 28, 2014

Thanks Michael!

That's very helpful.

8. **bobby3**
February 28, 2014

Regarding the new collider arguments, that debate between Strassler and Gross at the end of Strassler's recent talk (at KITP) was pretty entertaining.

9. **justin**
March 1, 2014

perhas this is off topic, but is this result indication of non-standard physics?
http://www.quantumdiaries.org/2014/02/28/b-decays-get-more-interesting/

10. **Peter Woit**
March 1, 2014

bobby3,
I noticed that, Gross seemed remarkably testy. The fact that he just spent a couple days on planes traveling to Beijing to try to help sell the Chinese on the 100 TeV accelerator idea might explain his reaction (overreaction, I think…) to some of Strassler's comments.

11. **M.Wang**
March 1, 2014

Is there a link to the Strassler/Gross debate?

12. **Peter Woit**
March 1, 2014

M.Wang,
I don't know if you could call it a debate, but the exchange was in the question section after this talk

http://online.kitp.ucsb.edu/online/joefest_c14/strassler/

13. **paddy**
March 1, 2014

The Strassler KITP talk is both informative and impressive. Thank you bobby3 and Peter Woit.

14. **Zathras**

March 3, 2014

On the 100 TeV collider post, there is a comment by someone named Andrew which is so dead-on that I am going to just copy and paste it here:

> "A 100 TeV collider would be nice. So would a pony. I would prioritize about half a dozen other investments before that, however:
>
> 1. $100-$500 million for improved computing power to do lattice QCD calculations both to make theoretical predictions that reduce the MOE (especially for discriminating between backgrounds and signals), and increase the power of existing experimental data and new searches with the same equipment, and would allow more precise extraction of Standard Model constants from existing data. We are pretty much guaranteed to be able get seven or eight loop QCD beta functions (the current research effort is devoted to five loops and each successive term gets much harder than the one before it to calculate) and with more accurate calculations, greatly increased theoretical precision, for example, simply by investing the money to get the computational power to do the job.
>
> 2. $1-2 billion for deep space satellites. The only thing we can be absolutely exists in terms of BSM physics is something to explain dark matter phenomena, either at least one new particle or at least one new force or both. The best way to narrow the dark matter parameter space is with precision astronomy observations that the atmosphere obscures, not with a 100 TeV collider. We can be vastly more efficient in our search for dark matter particles at some future experiment yet to be designed if we use astronomy observations to more tightly narrow this parameter space first. It is the difference between looking for a face in a crowd based on a photograph v. a police sketch artist's effort from an eye witness's blurry recollection. We can also, for example, much more accurately triangulate star distances with a pair of distant deep space satellites which would calibrate all other astronomy observations. And, better observations of neutron stars, pulsars, cosmic rays, etc. provides a different way of doing super high energy HEP with nature paying the electric bills for it. Also, realistically, the only application of 100 TeV+ scale phenomena is cosmology anyway. Very early universe observations from deep space also narrow the parameter space of 100 TeV scale physics and let us know what we should be looking for.
>
> 3. $2-3 billion+ for neutrino physics experiments (e.g. astronomy, reactor, neutrino beam, double beta decay). A little more investment here to pin down the last few Standard Model parameters and determine things like the Dirac v. Majorana basis of the neutrino mass has an immense impact on the parameter space of BSM physics.
>
> 4. Continued funding of B factories. There are dozens of meson resonances that we don't really understand well. This is an area of particle physics where predictions are frequently not matching up with experimental results. You don't need a 100 TeV collider to investigate, e.g., 0.5 to 2.5 GeV scalar and axial vector mesons.
>
> 5. $100-500 million or so to push the envelope on exclusions for proton decay, magnetic and electric dipole moments, non-collider based axion field searches, entanglement experiments, etc.
>
> 6. $100-$200 million on non-SUSY/non-stringy theoretical work. We have all of our eggs in one or two baskets. The LHC has cracked many of the other contenders. Like anything, there is declining marginal benefit to funding yet another SUSY theorist.
>
> If there is money left over, then by all means, lets go buy a 100 TeV collider.

15. **Peter Woit**
March 3, 2014

Zathras,
I don't see much reason for this kind of argument worrying about a 100 TeV machine crowding out funding for other worthwhile experiments. In the US, I see absolutely zero chance of any funding of a machine like this, the funding decisions for the forseeable future all involve deciding between projects like the ones mentioned in the comment.

Similarly, in Europe any prospects for building a machine like this are a long way off, with the current plan to operate the LHC until 2035. The discussion about the future and a 100 TeV machine is about what to fund 20 years from now, not what to fund now.

It's only in China that I see any conceivable possibility of someone starting to fund this kind of project in the next 10-20 years, and there the argument to the Chinese government is for special funding for this particular project. If they do decide to do it, it would not necessarily conflict with them providing some of the much lower levels of funding needed for the projects in the comment. And if they don't decide to do it, you can ask people who were around in US-HEP when the SSC was cancelled to tell you how that caused the rest of the field to flourish amidst an abundance of resources…

16. **nasren**
March 3, 2014

My take on the Strassler-Gross discussion at the end of the KITP talk.

Strassler: We need to think differently.

Gross: No, let's just keep thinking the same!

I thought Matt Strassler was just saying (pretty clearly) we should look for other ways to consider experiments for BSM physics, other than building a higher energy collider. And Gross' response seemed to be that we shouldn't do that. It seemed like a bad misunderstanding — the strange thing was that others in the audience seemed to share it. My guess is that they were miffed by a different point (and then transferred it over) which was that supersymmetry has not made an appearance so we should consider other ideas.

I thought Nima's talk was very interesting — a great pity that the discussion was cut off at just the point where the weakness was about to be exposed.

17. **Peter Woit**
March 3, 2014

nasren,

Strassler makes the case for a 100 TeV collider here in his latest blog entry
http://profmattstrassler.com/2014/03/03/a-100-tev-proton-proton-collider/

I was surprised at Gross's reaction, since I figured Strassler obviously was in favor of a higher energy machine, just arguing that there were good reasons to also look elsewhere. Looking back at the talk, I think Strassler's comment that pure SM behavior at the LHC would be circumstantial evidence against string theory (not really controversial, since SUSY at the LHC was always argued to be the most likely circumstantial evidence for string theory) was what set Gross off. He and others are tempted to try and argue that there's no major problem with the SUSY/string theory paradigm collapsing, we just need a bigger accelerator, but I don't think that's going to fly and they're starting to realize this.

18. **CU Phil**
March 4, 2014

I took Gross to be arguing something that I've heard both from people who think that all we need is a bigger accelerator, and people that think that the motivation for low-energy SUSY was weak from the word go: that, on reflection, perhaps naturalness wasn't as good an argument for SUSY as we thought. People like Gross go on to argue that the failure of naturalness doesn't mean we should give up on SUSY, since there are other arguments for it, while the other group who gives this argument concludes that we probably should have been looking elsewhere earlier, but we definitely ought to start start now. Gross seemed to place Strassler in the latter camp, even though he explicitly is not.

19. **Shantanu**
March 4, 2014

Peter, I disagree that the cancellation of SSC did not benefit other fields.
I know that when SSC was cancelled many people whow were working on SSC
switched to LIGO and also neutrino experiments (and probably other astroparticle
experiments which I am not aware of). So even though I agree that there is no evidence
that SSC cancellation increasing funding in other fields, certainly other fields
did benefit from more manpower/expertise of those who worked on SSC.

Various and Sundry

March 5, 2014
Categories: Multiverse Mania, Uncategorized

- It seems to be too early for April Fool's day, and yet the arXiv has [Dark Matter as a Trigger for Periodic Comet Impacts]() by Lisa Randall and Matt Reece, a preprint described as "Accepted by Physical Review Letters, 4 figures, no dinosaurs." The Register has a story: [Dark matter killed the dinosaurs, boffins suggest]().

  Also recently at the arXiv in a similar "too early for April 1" category is [Crossing Stocks and the Positive Grassmannian I: The Geometry behind Stock Market](), which deals with the "stockmarkethedron", also known as the [Geometrical Jewel at the Heart of Finance]().

- The president's FY2015 budget request is out, with [news for HEP]() not so good: a 6.6% cut proposed in DOE HEP funding. No details about the NSF budget, but the proposal is basically for flat funding (an overall cut of .03% in the research budget). The NSF is proposing one big increase, 13.5% for management. This is just an initial proposal from the administration, with the possibility of something different ultimately emerging from Congress.
- The particle physics documentary [Particle Fever]() opens here in New York at [Film Forum]() tonight, with appearances tonight and this weekend by the director and "physicists from the film". There's a [review]() in today's New York Times.

  I saw the film last fall at the New York Film festival and wrote about it [here](), with the summary:

  > most of it I thought was fantastically good and I really hope it finds distribution and gets widely seen. On the other hand, some of it I thought was a really bad idea.

  The film is a very inspiring inside look at the LHC experimental search for and discovery of the Higgs. My misgivings were about the theoretical framing of the story, which was the Arkani-Hamed point of view that this is all about two alternatives: SUSY or the multiverse. The NYT review shows that these misgivings were quite justified, with the reviewer's summary of what they learned about the significance of the Higgs from the film:

  > While the discovery of the Higgs may not have immediate consequences for the way we live, or applications in the world of technology and industry, its implications, according to "Particle Fever," could hardly be more profound. Through most of the film, the scientists are awaiting a specific bit of data, a single number that will either vindicate a theory of the universe known as supersymmetry or suggest the possibility of multiple universes.
  >
  > The differences between these two outcomes seem very stark. In the first case, more particles are likely to be found, contributing to a detailed and orderly picture of the nature of things. In the second, the Standard Model will be thrown into chaos, and the stability of the universe itself may be called into question. It won't be the end of the world, but for some theorists, it will feel that way.
  >
  > Mr. Kaplan is hoping for supersymmetry. His friend and sometime table tennis partner, Nima Arkani-Hamed of the Institute for Advanced Study in Princeton, is in the multiverse camp.

  Physicists often get outraged when they feel journalists badly misrepresent science to the public. Will they get equally outraged when it is physicists doing the misrepresenting?

- For some insight into the current concerns of particle theorists, you can watch some of the videos at last week's KITP conference. In particular, there's [Matt Strassler's talk](), where he got all Peter Woit and argued that "one could make the argument" that not seeing SUSY (or anything else stringy) at the LHC "would be significant circumstantial evidence against string theory as a description of nature" and that just seeing the SM at the LHC would be "circumstantial evidence against effective quantum field theory as a complete description of known particle physics". This got him an argument from Gross about his insufficient enthusiasm for a 100 TeV collider. Gross then also got all Peter Woit, arguing that the failure of the "naturalness" argument for new physics was no big deal since it wasn't a very good argument to begin with (I get all sorts of grief when I do this..).
  The conference ended with a [session of people trying to predict the future of the field 30 years hence](). This was mostly pretty discouraging, with a lot of people envisioning more of the same: endless generalities about quantum gravity, firewalls etc. Prominent by its absence was any role of mathematics in theoretical physics, with only Greg Moore speaking up for the question of the significance of now popular 6d superconformal theories, and Nati Seiberg mentioning that connections of the field to mathematics were a good thing.

  Lots of talks mentioned people's good experiences working with and interacting with Polchinski, who seems to be a very nice guy. I've never met him personally, but people have speculated to me that he had something to do with the decision of the arXiv to block links to my blog (he was unhappy about my characterization of his Scientific American article promoting the multiverse). What the truth is about that particular story I suppose I'll never know.

Update: Another [review of Particle Fever]() leads with this explanation of the main point they got from the film:

> Stakes come no higher than in Particle Fever, a dazzling, dizzying documentary about nothing less than whether we exist in a coherent universe of ordered, even beautiful laws — or whether, as Princeton physicist Nima Arkani-Hamed theorizes, our universe is one of an infinite set of other universes defined by a chaotic mash-up of unstable, inexplicable, random conditions.

Update: Reddit has a [live Q and A with physicists involved in the film](). Savas Dimopoulos (described as "considered the most likely to have a theory confirmed by the LHC") argues for the multiverse and tells questioners that "We may know about whether Nature prefers the Multiverse or the more traditional (super)symmetry path after the second run of the LHC which will start in a year." Arkani-Hamed also gives the multiverse argument, also claiming "I envy anyone who is jumping into fundamental physics as a grad student today!". No theorists in sight who might think there's more significance to the negative LHC results about SUSY than "must be the multiverse".

Update: Reddit the next day hosted a [live Q and A with Michio Kaku](). He there explains to the public that:

> The best theory comes from string theory, which states that dark matter is nothing but a higher vibration of the string. We are, in some sense, the lowest octave of a vibrating string. The next octave is dark matter….
>
> The next big accelerator might be the ILC in Japan, a linear collider which might be able to probe the boundaries of string theory…
>
> In the coming decades, I hope we find evidence of dark matter in the lab and in outer space. This would go a long way to proving the correctness of string theory, which is what I do for a living. That is my day job. So string theory is a potentially experimentally verifiable theory.

Seems that well-known theorists going on Reddit to mislead the public is now a daily phenomenon…

Comments

1. **imho**
March 5, 2014

First, what's wrong with the Randall Reece paper. Why is it absurd that dark matter can influence Fermionic matter. I haven't read the paper, but I'm sure they at least did some back of the envelope calculations to test plausibility. These aren't Brane Worlds – a fairy tale wrapped in a fairy tale – this is solid falsifiable Physics. So why the negativity?

Second, how is talk about Firewalls discouraging. This whole business of connecting entanglement to gravity seems new and exciting, and imho, smells like the beginnings of something profound. I'd put my money, and perhaps direct a little bit of funding, towards The Thermodynamics of Entanglement… it has such a nice ring 🙂

Finally, there is nothing wrong with rebalancing national research priorities. Sometimes a tweak here or there is healthy.

2. **Peter Woit**
March 5, 2014

imho,
Of course dark matter can influence other matter. And maybe what Randall-Reece are suggesting for a major solar-system effect of dark matter makes sense (I don't know enough about solar-system astronomy to have any real idea). Still, seems to me that a "dark matter killed the dinos" PRL paper sounds like an exceptionally clever April 1 stunt.

The black hole information paradox is now at least 30 years old, with debate about entanglement and gravity going back at least that far. I haven't seen any really solid insights into quantum gravity coming out of this so far, and another 30 years of the same doesn't seem like an inspiring vision to me. But others who feel differently should follow their inspiration. Given the latest trends, I'm not worried about the idea of "thermodynamics of entanglement" not getting enough attention (some days it seems hard to find a new hep-th paper on the arxiv that doesn't have "entanglement" or "entropy" in title/abstract).

3. **Giotis**
March 5, 2014

The theory Greg Moore has in mind is the interacting 6d (2,0) SCFT.
It is inherently Quantum mechanical with no classical limit and without a (known) langrangian. It is the field theory with the highest Super symmetry in the highest dimensions and a mother theory of 4d SYM and of other interesting field theories.

It is dual to M theory on AdS7xS4 and describes the dynamics of multiple coincident M5 branes. It is also known as the theory of tensionless self dual strings since as the M5 branes approach each other the self dual strings in their world volume (coming from the M2 branes stretched between them) become tensionless.

So indeed it is a remarkable theory but still mysterious. Understanding it better is crucial for M theory and QFT in general…

4. **Asnant**
March 5, 2014

http://beta.slashdot.org/story/198975

Thought you might like this link 😉

5. **Peter Woit**
March 5, 2014

Asnant,
Sure, but Slashdot is kind of behind the times, since that's pretty much from May of last year, see
http://scienceblogs.com/startswithabang/2013/05/15/the-rise-and-fall-of-supersymmetry/

6. **gs**
March 5, 2014

No details about the NSF budget, but the proposal is basically for flat funding (an overall cut of .03% in the research budget). The NSF is proposing one big increase, 13.5% for management.

But of course! Management needs more resources to navigate the tight budget.

(Back when I was in school HR, still called Personnel at the time, responded to a budget crunch by requesting additional staff to handle layoffs.)

7. **Neil**
March 5, 2014

I do find the firewall result interesting. Yes, the blackhole information paradox has been around a long time, but the firewall (possible) resolution is new. Since there appears little or no way to explore the issue experimentally, the fact that we have this logical inconsistency between QFT and GR on the event horizon offers some hope, I think, for making progress on the quantum gravity agenda (which has been stalled a long time).

8. **So**
March 6, 2014

Peter, can you argue a little about that jewel in finance ? Why is it absurd to use combinatorics in finance ? It looks fine to me, but I'm not an expert in stock market !

9. **Michael Hutchings**
March 6, 2014

So: the "crossing stocks" paper does not say anything useful about finance. It starts with the observation that if you plot several stock prices on the same graph then the curves sometimes cross either. It then uses this a launching point to start rambling about permutations and various related topics in combinatorics. I'm kind of amazed that this paper even made it onto the arxiv.

10. **So**
March 6, 2014

Michael : Do you know what a crossing of stocks means for the market? In other words can you tell how the crossing affects an investor portfolio?

11. **S. Molnar**
March 6, 2014

Almost, but not quite, amusing typo: “feel journalists badly”, not “feed journalists badly”.

12. **Dave Miller in Sacramento**
March 6, 2014

Peter,

Polchinski and I were in the same dorm for three years as undergrads and then we overlapped for a year or two at SLAC when I was finishing my doctorate and he was a young post-doc. So, I knew Joe pretty well, though I can’t claim we were close buddies, since I was a very prudent fellow and Joe was one wild-and-crazy guy (I won’t relate all of his exploits except to say that it is good that he did not fall down the nine-story air-shaft when he was climbing up the air-shaft to the roof of the Caltech library!).

I have no specific information about Joe and links to your blog at the ArXiv, but it does not sound like Joe at all: he was always a very free-wheeling, libertarian kind of guy. Of course, people change, but it would surprise me if he was involved in blocking links to your blog.

Dave

13. **Peter Woit**
March 6, 2014

S. Molnar,
Thanks, fixed.

14. **Peter Woit**
March 6, 2014

Dave Miller,
Thanks. One thing I learned during the “string wars” though was that when you publicly criticize people who are used to adulation rather than criticism, you get some very odd and unusual behavior from otherwise sensible and mild-mannered people. One strange thing is that from everything I’ve heard, what upset Polchinski here is public criticism of the multiverse, of a sort rather typical or even milder than typical opinions of his colleagues about this (they just weren’t making these publicly).

I don’t know what the true arXiv story is, but from what Polchinski wrote publicly about this on blogs, and from what I’ve heard from others about his private comments, I wouldn’t describe any of it as a libertarian defense of the rights of others to express views he didn’t like.

15. **Low Math, Meekly Interacting**
March 6, 2014

Of course, New Scientist is all over the dinosaur thing.

That said, however plausible, it’s plenty falsifiable. A theorist could do worse.

16. **Anony**
March 7, 2014

Peter wrote — No theorists in sight who might think there’s more significance to the negative LHC results about SUSY than “must be the multiverse”.

Theorists would be the losers if they do not think of alternatives other than these two — the more likely thing is that there are theorists who think of alternatives but are not necessarily projecting their work, other than publishing it in journals. Popular media and even physics blog follow the more well known and outspoken ones and do not look at on going research work.

For example there can be fine tuning in other gauge symmetry breaking scales — not just the standard model scale — and this does not bode well either with multiverse (no anthropic reason for it) or SUSY. For example any non-SUSY model with additional groups including GUTs, belongs in this category where more gauge groups potentially have the hierarchy problem….and there is lot of research with such groups.

An interesting result is at http://arxiv.org/abs/1401.5066 (Click) which obtains bounds on the scale of B-L gauge symmetry breaking in left-right symmetric models using the hierarchy problem.

17. **Casey Leedom**
March 7, 2014

Wow:

> Arkani-Hamed … also claiming “I envy anyone who is jumping into fundamental physics as a grad student today!”

He must really be bitter if he’s recommending HEP for current students. Don’t get me wrong, I’m an avid follower of physics news, etc. but it’s definitely not an easy field to be invested in these days.

18. **D R Lunsford**
March 8, 2014

Salon has praise for multiverse mania.

http://www.salon.com/2014/03/08/the_search_for_the_higgs_boson_%E2%80%93_and_why_science_will_defeat_stupidity/

-drl

19. **Sterling**
March 8, 2014

Hi Peter,

I found it interesting that both Brian Greene and Michio Kaku did AMAs within 24 hours of each other on Reddit.

While I am aware both have taken roles as "popularizers" now, something that bothered me was that Kaku described himself as a "leader in the field" and a "co-founder of string there", while Greene, who is more cited so arguably more influential, did no self accreditation.

Maybe I read too much into this, but I almost feel like the two are fighting each other for supremacy as "top" popularizer.

(Here the Greene AMA in case you missed it: http://www.reddit.com/r/IAmA/comments/1zqteb/i_am_brian_greene_theoretical_physicist_cofounder/)

20. **Peter Woit**
March 8, 2014

Sterling,
I did take a quick look at Brian's Reddit AMA, and it seemed to me he was careful to avoid making unsupportable claims about the LHC/ILC and string theory, or engage in the kind of self-promotion Kaku favors. In Brian's case, his focus is on his World ScienceU project, which has just launched:
http://www.worldscienceu.com

As far as I know though, neither Brian nor Kaku seem to have ever even considered making outrageous claims that the LHC provides evidence for the multiverse, along the lines of what Arkani-Hamed and Dimopoulos are engaged in and the film promotes. The Salon piece drl links to is pretty amazing, bashing creationists by holding up Dimopoulos and his multiverse claims as a sterling example of how different scientists are. If you want to torpedo the credibility of science, this is a good way to set about it.

21. **Neil**
March 8, 2014

OMG, what is an AMA?

22. **Peter Woit**
March 8, 2014

Neil,

I learned this yesterday: AMA= "Ask Me Anything"

23. **Luigi Vampa**
March 9, 2014

Peter, do you think Kaku and others like him have committed acts of sufficient harm to the public and to science such that their tenure should be revoked?

24. **Peter Woit**
March 9, 2014

Luigi,
Tenure is designed to ensure people's freedom to express their ideas, no matter how wrong-headed. There will always be physicists getting attention by making silly and misleading claims about fundamental physics, if you want attention, it's a lot easier to do that than to get attention for something serious. To me the problem is the rest of the (tenured) physics community: one reason they have tenure is so that they can challenge nonsense put before the public by prominent colleagues. I very rarely see any of them doing it.

25. **srp**
March 9, 2014

Saw Particle Fever last night with the editor and David Kaplan present for some Q&A afterward. They had over 500 hours of footage to condense into a movie, so it occurred to me that to some extent their problem was analogous to the LHC experimentalists: How do we handle this surfeit of information, decide what to throw away, and develop a truthful and interesting story about the phenomena?

In my opinion, I would have preferred more inside stuff from Atlas about deciding on what was noise v. signal, how blinding works, debates over how to set triggers, etc. To make room, I would have cut some of the redundant theorists' agonizing over their lives' work being at risk–some of that is fine, but they almost made that the main story.

Multiverse Mania at Wikipedia

March 9, 2014
Categories: Multiverse Mania

The quality of Wikipedia entries about mathematics is often quite good, but unfortunately the same cannot be said for their entries about physics. I happened to take a look today at the Wikipedia entry for Multiverse, which is an outrageously one-sided promotional piece for pseudo-science.

It's hard to know where to start with a document like this, and I've neither the time nor the Wikipedia expertise to start trying to edit it to something sensible (at this point I'd suggest that the most sensible edit would be to remove the whole thing).

I include just a couple of random examples of problems with the entry. The "criticism" section has little actual criticism, just some mild comments from Ellis and Davies, together with positive quotes from them about the multiverse as a research program. Nothing from Gross or Steinhardt, for instance. Much of the "criticism" section is actually defense of the multiverse through claims about experimental evidence from Mersini-Houghton that I don't think anyone except her takes seriously. Other claims of experimental evidence are completely outrageous, for instance we read that "Recent research has indicated the possibility of the gravitational pull of other universes on ours." where reference is to a Planck collaboration paper which states the exact opposite ("There is no detection of bulk flow").

There's a good case to be made that I pay too much attention to popular media nonsense about the multiverse. Unfortunately Wikipedia is taken a lot more seriously by the public than magazine stories. At this very moment, hundreds of high school students may be copying material out of it for their assigments…

Update: Some people have written to tell me about the appearance of the multiverse in the new *Cosmos* program that started last night. I saw just 20 minutes of the end of the program, missed that part. Presumably Tyson will deal with this in more detail in a later episode, so I'll wait to write more about this then.

Comments

1. **Vanzetti**
   March 9, 2014

   What prevents you from editing the article?

2. **Lamont Granquist**
   March 9, 2014

   If you've ever tried to edit a wikipedia article like this you'll quickly get frustrated. Without citable evidence your edits will quickly get reverted. The fact that the opinions which made it into the article first are based on nonsense and without citations themselves (or with incorrect citations) will not matter. Just in order to get the "There is no detection of bulk flow" statement correctly inserted into the article will take finding a moderator who understands physics well enough to be able to make a judgement that you're correct, but sufficiently removed from the subject that they're considered unbiased. It is much easier to 'greenfield' nonsense into wikipedia than it is to get that nonsense corrected.

3. **Peter Woit**
   March 9, 2014

   Vanzetti,

   I tried this once, in an even more egregious case, see here

   http://www.math.columbia.edu/~woit/wordpress/?p=1657

   Based on my experience then, unless you know a lot more than I do about how to deal with the systemic problem of determined wikipedia contributors devoted to using it to spread nonsense, trying to add sensible edits is just a waste of time. This is all in addition to the fact that the whole thing is such an outrageously misleading production that editing it to something sensible would be a massive amount of work.

   If others with more knowledge of how to get sensible edits put in of nonsense like this are able to get somewhere, that would be great. My blog entries about problems with the multiverse contain a large number of links that I would hope would be useful to anyone trying to get involved in this. Personally, I'm already wasting more of my time than is sensible on this topic.

4. **vmarko**
   March 9, 2014

   My take on these cases is to edit the article by putting "citation needed" superscripts on every sentence which sounds suspicious. These placeholders are usually not removed without providing an actual reference, which in these cases may be hard to find, if at all.

   When a typical student opens the Wikipedia article and sees "citation needed" superscripts in virtually every sentence, this will be just enough alarm to refrain from trusting the article too much. 😉

   And yes, math articles are often quite well written, while physics articles vary in quality between an excellent and a complete stub.

   HTH, 🙂
   Marko

5. **Brandon Brown**
   March 9, 2014

   The Wikipedia page was brought to my attention in Sunday school (of all the places) by another reader of this blog. We both decided that Peter wouldn't like what was written and it just might ruin his weekend or a wonderful meal.

6. **bhny**
   March 9, 2014

Pointing out things on the talk page does get results. Most serious editors just want to make a good article and aren't trying to push a point of view. If there's an obvious disconnect between the text and the reference, then fixing it with an edit is the fastest way to go.

I've already fixed the "Recent research has indicated the possibility of the gravitational pull" sentence. I'll look at your other links during the week. It seems the pro-multiverse crowd have the loudest voices at the moment and so get greater representation in the article. Another fix I could do is to spread the criticism throughout the article (which is normal wikipedia style) rather than a separate section.

7. **cthulhu**
March 9, 2014

Peter, hopefully not too far off topic, but what about "Eric Weisstein's World of Physics" at http://scienceworld.wolfram.com/physics/ as an online physics resource? I generally find that *Mathworld* (http://mathworld.wolfram.com) is a reliable math resource, at least at the level I need (MS level in a fairly mathematical part of engineering, i.e., control theory). I've been not too impressed by Wikipedia in the engineering disciplines…

8. **anonymous**
March 9, 2014

Now you've got me worried that high school teachers could be asking their students to study the multiverse.

I've found Wikipedia to have calmed down over the years, even since your troubles in 2009. A small edit that adds material and includes a citation is less likely to be seen as offensive as removing other cited material because that way you won't have to get into a debate over which source is more "authoritative" than the other. There already seem to be some new edits from bhny along those lines. The multiverse article by its very nature is likely to be more of a crackpot magnet than others, sure, but notice how nobody has reverted bhny's edits yet.

You might also want to put the multiverse article into perspective among all the physics articles out there. According to the list of [popular physics pages](), the multiverse article is only 294th on the list, albeit in a mix that includes many biographical/historical/interdisciplinary topics. There are plenty of other potential crackpot magnets that are much more popular than the multiverse, e.g. [Schrödinger's cat](), [Coriolis effect](), or [LHC]() to name just a few. By far the most popular physics article is [Watt](), which only has a "C" rating. It's a topic that has real consequence for people in the real world and can be taught as early as middle school. There's an argument to be made that the thinness of the Watt article is as worrisome as the unbalanced weight of opinion in the multiverse article as measured in terms of the total number of people viewing the article who don't learn the material properly. If you're worried about some sort of personal conflict of interest but still want to help people learn physics in your spare time by editing Wikipedia, try first focusing on some of the other popular, imperfect articles on the list. Get a feel for how it's done these days and how different it is from 2009.

9. **Tom**
March 10, 2014

"caveat emptor" w.r.t to any faddish or hot-button topic at Wikipedia.

eg, Some of the most egregious, one-sided Wiki articles are about "global warming", "climate change", "climate disruption", etc.

Many of Peter's critiques on the Wiki physics & multiverse articles, are just as relevant & applicable.

10. **Jerzy Kierul**
March 10, 2014

It seems that Wikpedia on Yang-Mills theory has changed, no references to Marco Frasca now.

11. **Patrice Ayme**
March 10, 2014

I watched the new Cosmos today, with my 4 year old. Neil De Grasse Tyson replacing his mentor Carl Sagan. Some of it was excellent. However the Multiverse was presented as a fact, and part of our "address". I was shocked. Especially after the narrator had pontificated, a few minutes prior, that scientists don't make guesses.

12. **Richard**
March 10, 2014

> unfortunately the same cannot be said for their entries about physics

That's nonsense.
Not even wrong.
99.4% of Wikipedia articles about physics are tremendously good, far better than anything that anybody outside a research university had any access to a decade ago.

Keep some sense of proportion, people! Orders of magnitude, orders of magnitude.

Wikipedia is, in fact, one of the few things that makes me proud to be a human.

Ignore articles about "multiverse" just as one would articles about "Ariel Sharon" or "Barak Obama" and you'll be just fine.

Baby with the bathwater, Mr Woit.

13. **imho**
March 10, 2014

Hi Richard,

Wikipedia articles are imho not tremendously good, which I think, is part of Peter's point. Wikipedia is a great tool for lay people to learn the basics of a subject. For example I would like to find out more about the prescription I've just been prescribed or I'm interested in this Multiverse thing I just heard Neil Tyson speak about. For professionals in any field it's really only tangentially useful. From this perspective it's important that these introductory articles are giving correct information.

14. **TB**
March 10, 2014

Agreed IMHO, but as a layman's resource, that puts even more onus on it to be evenhanded at best. If Cosmos drives people online to learn more, Wikipedia will always be at the too of their search results.

15. **Peter Woit**
March 10, 2014

bhny,
Thanks. It looks like, due to your efforts and others, the Wikipedia entry has been significantly improved. It's encouraging to see that this seems to be easier to accomplish than the bad experience I had back in 2009.

imho/Richard,

To put things more positively, I should say that one thing I've found most remarkable about Wikipedia is the quality of some of the mathematics entries. Often I've run across quite good explanations of very difficult material that is understood by only a small fraction of professional mathematician.

Patrice Ayme,

I just saw about the last 20 minutes of the Cosmos show last night. No multiverse there, but I have to say I don't see what the fuss is about that program. It looked more or less identical to dozens of other inspirational science programs that fill the cable TV channels. It's not very encouraging that Tyson gives the multiverse as a main example of why Sagan's version needs updating, see
http://www.huffingtonpost.com/2014/03/04/neil-degrasse-tyson-cosmos-god-alien-life-multiverses-interview_n_4790408.html
but I'll wait to see how he handles the topic, since it appears likely he'll devote a lot of attention to it in some later episode.

16. **Jeff McGowan**
March 10, 2014

As a mathematician I can second Peter on the quality of the math entries, in general. I'll sometimes look something up I've forgotten, usually something quite specialized and very high level, and it's rare for it to be either missing or incorrect. Actually, it's usually not only correct, but quite well written. There have been a few times when I'm pretty sure I know who wrote it…

17. **Jozape**
March 10, 2014

Neil mentioned the multiverse as a possibility some scientists believe may be true, but I do not remember him stating it was a fact, nor do I remember our cosmic address including the multiverse. Actually, the multiverse barely even received a mention.

18. **TB**
March 10, 2014

Jozape
It came at the end of the portion with the cosmic address, that we're one of billions of bubble universes. I don't know if people would take it as a fact, but it was definitely an endorsement.
I would say it was presented in the context of the cosmic address, and as there was no alternative presented, most people would feel that Tyson the scientist believes that is part of the address.

19. **Peter Woit**
March 10, 2014

I see that Jennifer Ouellette's review of the show

http://www.latimes.com/entertainment/tv/showtracker/la-et-st-cosmos-20140309,0,7079191.story

emphasizes the multiverse as the 21st century improvement over Sagan:

"Tyson adds a 21st century twist by invoking the multiverse: the notion that our universe might be just a bubble among bubbles in a vast infinite sea of universes. That's the kind of notion that used to fall firmly into crackpot territory – or make for bestselling science fiction novels — but is now taken quite seriously by many cosmologists, even if it's not (yet, if ever) a testable hypothesis. "

No discussion of whether untestable hypotheses are really an improvement on Sagan.

She's also quite fond of the Giordano Bruno section, which I saw at least part of. Seemed to me cartoonish, in all senses of the word, and that's not an improvement on Sagan.

20. **Tom**
March 10, 2014

New Cosmos's mix of fact and fantasy is not easy to tell apart, by laypeople.

My non-technical family interpreted the "multiple bubble universe" segment as a more-or-less "established fact".

In future episodes, I'm uneasy whether other speculations will be passed off as "likely" or "probable" or "fact". Unless you are listening *very* carefully, Tyson's subtle qualifiers are easily overlooked.

I may be too pessimistic, but this series could end up as a "What the Bleep Do We Know" type of gibberish & nonsense.

21. **Kavanna**
March 10, 2014

Among non-scientists, the multiverse mania is creating confusion, reinforcing credulity, and discrediting credibility, all at the same time. Peter has rediscovered what many of us already know, that correcting errors in Wikipedia articles is an arduous exercise. Some of the articles should just be deleted and recreated from scratch. Many areas of Wikipedia are controlled by editing cartels.

I've heard mixed things about the new Cosmos. But one thing that remains from the original series is the pomposity, including absurd statements like "scientists don't make guesses." Of course, they do, all the time. They just develop ways to check their guesses, and, if they can't, they labels those guesses "guesses," not "facts."

The multiverse isn't even much of a guess, just hand-waving. It's shocking but not surprising that the multiverse is presented as a scientific datum, not as what it is, a speculative mania like a stock market bubble.

22. **Peter Woit**
March 10, 2014

Kavanna,
To be fair, unlike in 2009, this time some errors did get corrected in Wikipedia very quickly. I only have personal knowledge now of two data points…

23. **Mike**
March 10, 2014

Peter,

I didn't get that Ouellette's review touted the multiverse segment as an "improvement." And, she does in fact specifically say that the hypothesis my never be able to be tested. It certainly is, however, 21st century 🙂

I tend to agree with your dislike for the cartoon segments — and to top it off, they weren't even good (i.e., costly and painstakingly produced) animation!

Tom above says that he may be too pessimistic, and that this series could end up as a "What the Bleep Do We Know" type of gibberish & nonsense. I think he is too pessimistic. At least from the first show, I don't see it going that way.

While there will probably be little new in the show for readers of this blog, I do think that on balance it is a very a good thing for the American public to view. I especially thought that the relatively strong criticism of religion and mysticism was a great way to kick off the first show. On that note, perhaps a redeeming aspect of cartoon segment was the depiction of the evil church elders. 😉

And, on a sentimental note, I thought Tyson's personal link to Sagan was very good TV.

Guess I'm not as negative as some — perhaps my standards are just lower.

24. **D R Lunsford**
March 10, 2014

The original Cosmos was riddled with science errors, particularly about relativity, but more importantly with historical errors – the largest of which was the fable of Hypatia as martyr at the hands of a Christian mob. The new one goes one better, setting up Bruno as some martyr for science, perhaps the founder of modern science itself. Nothing could be further from the truth. He had neither knowledge of, nor interest in, the considerable body of medieval science, and was simply promoting his own version of Hermetism. Nor was there any official papist position on heliocentrism when the torch was lit around his ankles in 1600. He was a garden variety heretic, very probably mentally ill and strange (he thought disease was caused by demons) who had 10 years to get his act together, and failed. I look forward to Peter's review.

-drl

25. **Peter Woit**
March 10, 2014

All,
Maybe that's enough about Cosmos, unless someone has something new and interesting on the multiverse angle. I assume Tyson will deal with the more speculative areas of physics in later programs and I'll be curious to see how he does this, may write more then.
In the meantime, from the little that I saw I don't see any reason personally to pay more attention to this. On topics like Giordano Bruno, in addition I'm pretty ignorant of the real history (and felt that the Cosmos segment did nothing to change that), so best for all to find a blog moderated by someone who actually knows something about this.

26. **Bill Hunt**
March 10, 2014

> No, it wasn't. Watch again. "Multiverse" was NEVER listed as part of our Cosmic Address. Tyson simply said of the Multiverse concept, "Many of us suspect…" that it's true. Not that the Multiverse is factually proven true. I get that there are a lot of people around here who hate the idea of Supersymmetry and the Multiverse, and I appreciate the arguments against excessive flights of fancy vs. experimentally proven fact, but the point of Cosmos is to inspire a love of the wonders of science in lay people. Not be a strictly dry and 100% exactly accurate reassurance for more knowledgeable and scientifically literate viewers. I love and appreciate this blog and its comment section, but sometimes it takes on a little too much of the tone of the Lone Gunmen nitpicking the scientific accuracy of Earth 2.
>
> The great failure of science, in my opinion, is a smug assumption that everyone else is going to appreciate (or care or even pay attention to) scientific endeavor and effort as much as those working within science do, just as those same scientists tend to thumb their noses down on those of their own who attempt to communicate its wonders to the general public. It's in that very kind of environment that those who wish to distort science for political or economic reasons can get away with it. Which is a shame.
>
> To the extent that discussion of SUSY and the Multiverse in the mainstream gets people interested in science, I don't see that the harm exceeds the value of the result. When those people do take an interest and start digging a little deeper, they'll discover that theory is one thing but experiment is where the actual rubber meets the road.

27. **Geoff**
March 10, 2014

Having listened to quite a bit of George Ellis, I am of the opinion that the wikipedia snippet is by far the kindest and most generous statement about the multiverse that he's voiced. One of the better critiques I've found is the following with the multiverse discussion at the 34:00 mark. Also of possible interest is quoting your Columbia colleague David Albert at the 44:00 mark about books that have also been reviewed here.

https://www.youtube.com/watch?v=tq8-eLGpEHc

I also can't help but wonder if Ellis' opinions are drowned out partially because he's sympathetic to theistic ideas.

28. **adel sadeq (@AdelQsa)**
March 10, 2014

Dear Peter,

You seem so far to have successfully attacked just about every quantum gravity theory or fundamental physics ideas so far. So I am wondering if you have any ideas of your own on how to carry physics further(in case you see that as needed).

I don't see anything in you website or randomly checking your blog. If you think this is off topic can you tell us at least if you will address that in the future. Thanks

29. **Peter Woit**
March 10, 2014

adel sadeq,
The only ideas I've significantly criticized here are pretty much string theory, supersymmetric extensions of the Standard Model, and various versions of multiverse mania. I'm quite pleased to hear that these attacks have been successful, so I guess these subjects have now been discredited and I can stop paying attention to them.

As for what I personally find promising, I'm not one for hyping my own speculative ideas, but I've often explained here that I think there is a great deal still to be learned about how to apply ideas from representation theory to physics. For the past year or so the main way I've been working on this has been writing a book about quantum theory and representation theory. It's now about 400 pages long, the latest version is always at
http://www.math.columbia.edu/~woit/QM/qmbook.pdf
I'm hoping to finish this project this spring (probably another 40-50 pages), then get back to the ideas I never finished writing up about using Dirac cohomology instead of BRST to handle symmetries in quantum theory. Working on the book has helped me a lot to clarify in my mind what seem to me some promising questions to think about, most with some relationship to the Dirac cohomology project business. I'm looking forward to getting to work on those after there's a complete version of the book, hopefully a couple months from now. But, no, I'm not going to start a publicity campaign for my half-baked ideas.

30. **Peter Woit**
March 10, 2014

Oh, and this is quite off-topic, I'm afraid. Anyone with any helpful comments about the book manuscript is welcome to write to me. Once it's complete I'll write about it in more detail here and will be happy to discuss it extensively then. Give me a couple months…

31. **Pawl**
March 10, 2014

While I certainly applaud attempts to make Wikipedia more accurate, the most important thing for everyone — especially laypeople — is not to **assume** it's accurate. We have to bear in mind — and Wikipedia might emphasize — that the editorial process there does **not** ensure accuracy in the short run. It relies, especially for problematic subjects like the multiverse and string theory, on sources it can cite by people who've made a splash. There is no very good way within Wikipedia of disputing the logic of such courses, and no good way of explaining to Wikipedia that a lot of these views are simply not taken seriously by many experts — and almost none of those feel obligated to publish critiques. (I was at a major conference recently where cosmology was a main topic and I only heard brief, uncomplimentary comments about the multiverse.)

So: yes, try to make Wikipedia better; but also, emphasize that it does have weaknesses which seem to be structural.

32. **Don Jennings**
March 10, 2014

@AdelQsa
That seems rather unreasonable. I don't know much about Peter's work, and I'm pretty sure he's no Einstein and no Ed Witten. But nor are you, nor am I, and nor is any other living physicist. However, when I have glanced at his scientific writing it seems to be directed toward fairly difficult, plausibly promising, long-term projects, and done in a rigorous, honest fashion. I think that's all one can generally expect from non-Einsteins such as ourselves. It's certainly much much better than having non-Einsteins spout out incessant vacuous fairy-tales in an attempt to look like Einsteins. To my eye that is what Peter's attacking.

33. **Peter Woit**
March 10, 2014

Thanks Don.
But no more pro/con Peter Woit here. The ad hominem tactic for dealing with scientific criticism pretty much speaks for itself.

34. **Sam Klein**
March 10, 2014

I enjoyed this post.

Regarding math v. physics on Wikipedia: the uniform high quality of math articles is due in part to a few dozen or so prolific math grad students, and later young profs, who decided to contribute there years ago. Chemistry had a similar intentional influx; some of whom invented the article-rating scheme that the whole English Wikipedia now uses. While I'm not sure what leads some groups to take this up and others not to, there is a tipping point beyond which there are enough regulars reviewing a field (and referring to WP in their field) to make it pleasant for others on the frontier of the field to do the same.

And writing widely-read blog posts about unaddressed gaps is a popular way to influence an article, even if you don't want to edit directly. 🙂

35. **SilverDave**
March 11, 2014

Before we all get our blood pressure raised to aneurysm levels…
I think the last paragraph of this " Rational Wiki" article on "Universe " might help….
http://rationalwiki.org/wiki/Universe

While Rational Wiki probably suffers from the same citation , and editing problems as Wikipedia… this paragraph comment soothed me :

—While it is a term most commonly associated with science fiction, cosmologically speaking, the "multiverse" is the hypothetical realm which contains our universe, as well as many possible others. Some physicists do take multiverse theory kinda-sorta-seriously.
Perhaps there have been multiple universes with multiple big bangs, and each new universe receives a different roll of the cosmic dice, thus having different laws of physics. This is a philosophically satisfying interpretation of the fine-tuned universe problem.

—However, it's also completely stark-raving unfalsifiable—-

"For a start, how is the existence of the other universes to be tested? … invoking an infinity of unseen universes to explain the unusual features of the one we do see is just

as ad hoc as invoking an unseen Creator. The multiverse theory may be dressed up in scientific language, but in essence it requires the same leap of faith."
…..
Until we develop instruments which can actually see outside of the universe (which is not bloody likely), or create a universe in a jar that we can poke with scientific instruments (slightly more likely) this remains a theory to stay grounded in sci-fi. Anyone who talks about multiverse theory seriously may either be speculating wildly or trying to sell you some woo—

I felt a little better after reading that.

36. **Mitchell Porter**
March 11, 2014

On the subject of physics and the Wikipedia, there was a paper today by Stephen Adler (co-discoverer of axial anomalies), arXiv:1403.2099, where the acknowledgements include thanks to Edward Witten for helping to interpret a Wikipedia article, that Adler evidently used as a reference.

37. **DrDave**
March 11, 2014

As an occasional editor of the Wikipedia, I understand the frustration, but we still have to make the effort. Certainly "citation needed" is a start, talk page is good, but I also recommend setting up a web page with a concise, clear, short article on one or more aspects of the article, especially as a collaboration, and linking to it. You can also make a video or podcast and upload it. I suppose one can imagine a universe in which there is no multiverse article.

38. **philh**
March 11, 2014

Maybe its changed since your blog post or maybe Im missing something but I went on the link to the wiki article through your blog post and it said:
"Around 2010, scientists such as Stephen M. Feeney analyzed Wilkinson Microwave Anisotropy Probe (WMAP) data and claimed to find preliminary evidence suggesting that our universe collided with other (parallel) universes in the distant past. [23][unreliable source?][24][25][26] However, a more thorough analysis of data from the WMAP and from the Planck satellite, which has a resolution 3 times higher than WMAP, failed to find any statistically significant evidence of such a bubble universe collision.[27][28] In addition, there is no evidence of any gravitational pull of other universes on ours.[29][30]
"
It also quoted Steinhardt
"Over the entire multiverse, there are infinitely many distinct patches. Among these patches, in the words of Alan Guth, "anything that can happen will happen—and it will happen infinitely many times". Hence, I refer to this concept as a Theory of Anything. Any observation or combination of observations is consistent with a Theory of Anything. No observation or combination of observations can disprove it. Proponents seem to revel in the fact that the Theory cannot be falsified. The rest of the scientific community should be up in arms since an unfalsifiable idea lies beyond the bounds of normal science. Yet, except for a few voices, there has been surprising complacency and, in some cases, grudging acceptance of a Theory of Anything as a logical possibility. The scientific journals are full of papers treating the Theory of Anything seriously. What is going on?[6]

—Paul Steinhardt, "Theories of Anything" in Edge
That seems to me to be a fair representation , no?

39. **Peter Woit**
March 11, 2014

philh,

There have been some significant changes since my blog posting. Thanks to all responsible.

40. **Machine Elf**
March 11, 2014

re DrDave: "setting up a web page with a concise, clear, short article on one or more aspects of the article, especially as a collaboration, and linking to it."

That's not allowed, best just to edit the article and/or post to the talk page.

41. **Machine Elf**
March 11, 2014

Another good place to raise concerns: http://en.wikipedia.org/wiki/Wikipedia_talk:WikiProject_Physics

42. **Gustavo Burdman**
March 11, 2014

Colbert ended his interview of Neal De Grasse Tyson last night (10/03/14)
with this: "Yes, the idea of the Multiverse is cool. But so is the idea of the Force …".
My guess is the the show (Cosmos that is) will have to address such serious criticisms
before too long or it'll loose credibility.

43. **imnobody00**
March 12, 2014

I have tried to correct one wrong point in the criticism section of the Wikipedia page. I have done it twice. It has been reverted to the original twice.

Spring Break

March 11, 2014
Categories: Uncategorized

I'm leaving for a spring break vacation in Northern Italy tomorrow afternoon, and will shut down comments while I'm away. Back in two weeks. Some events that will occur while I'm away that might be of interest:

- The 2014 Templeton Prize will be awarded about the time I land in Milan.
- There will be a HEPAP meeting this week, likely to have discussion of US HEP funding issues discussed here recently.
- Next week will be the 10th anniversary of the blog. So far 1350 postings, 37,540 comments posted (and about 250,000 spam comments…). When I get back maybe I'll write something to mark the occasion.

No Comments

Rainy Day Posting

March 23, 2014
Categories: Uncategorized

My spring break vacation is not quite over but, after 10 days of spectacularly beautiful weather, it's now raining hard here today and I've got some time indoors to write something. First some quick links to things I've seen in my short periods of recent internet access (leaving the BICEP2 story for after I get back Tuesday …):

- I don't often link to things at Tommaso Dorigo's blog, since my advice is that you should just always follow it since it's the best HEP experiment blog to be found. His latest has news of an impressive CMS limit on the Higgs width, something that I had never realized could be done. This should get a lot more attention than it has gotten; it's a great example of experimental cleverness, getting at a seemingly impossible measurement in an indirect way.

  And, seriously, I'm not just saying this since Tommaso recently showed me around Venice…

- For another, very different, blog you should be following, there's my friend Mathbabe, who has a simultaneously amusing and disturbing take on Princeton, which addresses the question of why it produces graduates like this one.

  From what I can tell, Princeton seems to be little changed since the time I spent there more than thirty years ago, and at the time it seemed devoted to staying much like the place of thirty years before that. Something that hasn't changed is the vanishingly small number of women, with even fewer at the IAS on the other side of the golf course (and if you want to argue about why that is, please do it somewhere else).

  One thing I did enjoy about Princeton was getting to know some of my fellow students. In other HEP news, one of them, Jon Bagger, has just been appointed director at TRIUMF.

- The recent HEPAP meeting seems to have had some unusual activity from the DOE in response to Laurence Yaffe's recent complaints about large cuts to theory grants. This included a presentation specifically about HEP Theory funding, but reading it I still don't see the explanation for why, as Yaffe claimed, cuts in theory group funding seem to be much more widespread than in other areas (see page 9 of this presentation).

  The DOE/HEP presentation had a specific warning against discussion on blogs of funding problems, I'd guess specifically aimed at Yaffe:

  > Intense discussion in the community around the sociological issues can easily be mistaken by decision makers as disputes over the P5 plan, so please be careful to frame discussion points properly, especially when discussing issues we face with others outside the field.
  > – Blogging, posting on public websites are a de facto public conversation
  > …
  > 'Bickering scientists get nothing'

- Scott Aaronson has a review of Max Tegmark's Our Mathematical Universe, which argues that the main claim the book is designed to promote is empty, but everyone should read it:

  > I think everyone interested in math, science, or philosophy should buy the book and read it. And I still think the MUH is basically devoid of content, as it stands.

Comments

1. **David Appell**
   March 23, 2014

   Peter, tell us more about Venice. It is my favorite city in the entire world.

2. **wolfgang**
   March 23, 2014

   >> more about Venice

   Well, The Daily Mail just reported that "Venetians have voted overwhelmingly for their own sovereign state in a 'referendum' on independence from Italy".

3. **john**
   March 23, 2014

   So should the DOE statement be taken as a threat to shut up about funding cuts or loose future funding? Not an encouragement for students to enter into the field.

4. **layman**
   March 24, 2014

   It is very ironic that you have been absent from the blogosphere during the time that

   — quantum gravity was verified experimentally (yes, classical GR cannot account for the BICEP result)

   — the scale of 10^{16} GeV was probed

   — the theory of inflation, which almost cannot be without a multiverse of a sort, was indirectly verified.

   Basically, all the things you have been saying for 10 years cannot happen, are untestable, and unfalsifiable, have happened in your absence. Congratulations!

5. **Shantanu**
   March 24, 2014

   Peter something else you missed is strominger's colloquium at PI about black holes
   and also about string theory.
   I think somewhere he mentioned that ST cannot be falsified directly.
   http://pirsa.org/14030104/

Niayesh asked him about firewalls and he is not convinced by them

6. **Peter Woit**
March 24, 2014

David Appell,
No way to capture Venice in a blog comment. It's a unique and fascinating place.

Wolfgang,
Actually I saw something like the Daily Mail story and asked Tommaso about it. I can report that a lifelong Venetian says he has no idea what that story is about.

John,
The DOE presentation was explicitly claiming that any public dissent about DOE allocations of HEP funds would hurt overall HEP funding levels. No explicit threat was made about what would happen to people who did this.

Layman,
Thanks for the summary of the hype about this. I'll try and write something hype-free but it won't be for a day or two. I'm headed soon for Milan airport, will be traveling the rest of the day.

All,
I'm deleting the rest of the comments re the hype. Please, enough "BICEP2 shows the multiverse rules!" "No, the multiverse sucks!" discussion, and wait for this to be on-topic soon.

7. **M**
March 24, 2014

Notice that the Higgs width is already indirectly measured from the Higgs production rate (dominated by gg –> H) assuming safe relations such as Gamma(h –> gg) = Gamma(gg –> h)

8. **Chris Oakley**
March 24, 2014

A typical "Daily Mail" story is an account of illegal immigrants or asylum seekers exploiting the system, accompanied by a suitably hysterical xenophobic commentary. They were big supporters of the National Socialist party in Germany in the 1930s although they did at least have the decency to drop them when war broke out. Taking anything they write beyond theatre and cinema listings is generally a mistake.

9. **Ru**
March 24, 2014

Was going to say the same thing as Chris Oakley. While many non-UK folks are only familiar with the Mail Online and it's click-bait and braindead celebrity gossip, the print newspaper is an entirely reprehensible mix of hate, fear-mongering, and racism. Best not to give them click-throughs.

10. **anon**
March 24, 2014

I was a student at Princeton during the previous decade and I can attest that it has not changed.

And I've been waiting anxiously for the past several days for Peter Woit's take on Linde and BICEP2…

11. **Martibal**
March 24, 2014

As far as I know (i.e. as a non-Italian in Rome), the "referendum" was through the internet: people asked to answer on some website whether they would like Venice to be independent, as it was before the italian unification. This was organized, I guess, by some independent movement, and has no legal value whatsoever.

12. **anonymous**
March 24, 2014

Regarding the limit on the Higgs width, one should not forget that also some "dumb" theorists were involved in pointing out this possibility and showing that it is a viable at the LHC.

I am very impressed by this new result, but one should not neglect the theory input. Fortunately this is acknowledged in the CMS paper.

13. **Tommaso**
March 24, 2014

Hello Peter,

sorry to hear my influence on the weather during your vacation has expired. I guess my powers don't extend beyond 200km away.

Yes, the referendum is the work of a few nutcases. However, we must take all these manifestations of nuttiness seriously these days, as that funny old idea that national borders can be questioned if one has a strong enough army has not died out yet, apparently.

To the commenter who said that the food in Venice is the worst in Italy: I agree, with some exceptions. The main point is that it is very cost-ineffective, due to Venice being more of a tourist park than a place for Italians anymore. But mind you, we are still talking about Italian standards. I'd take an average restaurant in Venice over 95% of restaurants in Germany, for instance. Sorry for picking on Germans, just an example.

Have a safe trip back,
T.

14. **poet**
March 25, 2014

really? was princeton that way? i spent time in recent years and cathy's story doesnt sound right at all. yes, there are lots of old people from a different era, i'm sure women get condescending comments but in my experience, it is not that different from other places.

other places have superficial diversity but i find that the condescension is worse is in such places.

15. **Bill**
March 25, 2014

There is a conference next week about BICEP-2 at Perimeter: http://www.perimeterinstitute.ca/conferences/implications-bicep-2

B-mode News

March 25, 2014
Categories: Multiverse Mania

While I was away on vacation, the big news in physics was the BICEP2 result on B-modes in the CMB. Maybe it's just as well I wasn't available to blog about this, since inflation and cosmology aren't at all my field of expertise. Now that some of the dust has settled from the media blitz though, I do think it's worth while to write something here, since there are some aspects of the story where the media coverage could use some extra perspective.

First of all, there's the obligatory caveat about this result not being definitive, which most coverage by scientists has included. To my non-expert eye, looking at the main graph reproduced everywhere, if you subtract the gravitational lensing background you get something which is much larger at higher values of l than it is supposed to be if it were coming from primordial gravitational waves. But, I'm the wrong person to be evaluating this, you should read what the experts have to say, with some examination of the issue from Peter Coles here and Sesh Nadathur here and here. The great thing about this is that all you should need is a little bit of patience to see it resolved, with data coming in from Planck later this year and sooner or later from BICEP3 and other experiments as well. If this is a red-herring, we should know that within a relatively short time-frame.

Assuming that there really is a primodial gravitational wave signal, this is something that has long been predicted by inflationary models, so is a significant extra piece of evidence for some sort of inflationary scenario. I'm not the right person to try and explain the details of this, or even to point you to the best review articles, but some things you might want to look at are John Preskill's derivation of the prediction here, and many people are pointing to Daniel Baumann's lectures here. On the plane back from Italy I was looking for other reasons at an excellent introductory QFT textbook by Alvarez-Gaume and Vazquez-Mozo, which turns out to have a section (6.5) devoted to this calculation.

For the implications of this kind of confirmation of inflation, one obvious question is what it means for string theory. The standard argument from string theorists is that its testability problems arise because we can only do relatively low energy experiments, that at high enough energies, such as those of the very early universe, it would be testable (not true, since string theory is capable of giving you pretty much anything you want at any energy). The press coverage of what BICEP2 means for string theory is pretty comical , with Nature telling us:

> The BICEP2 results will also send some string theorists back to the drawing board, says Frank Wilczek, a theoretical physicist and Nobel laureate at MIT. String theory posits that elementary particles are made of tiny vibrating loops of energy. Efforts to combine string theory with cosmology have led to inflationary models that generate gravitational waves with energies much lower than the level detected by BICEP2, he says.
>
> Theoretical physicist Eva Silverstein of Stanford says she disagrees that string theory-based models of inflation are in any sort of trouble. "There is no sense in which we are forced to start over," she says. She adds that in fact a separate class of theories that involve both axions and strings now look promising.
>
> Linde agrees. "There is no need to discard string theory, it is just a normal process of learning which versions of the theory are better," he says.

New Scientist has a string theorist making the usual claim that finally, string theory is testable, showing up those bloggers who say it isn't. The idea is that the BICEP2 results don't confirm inflation, but something completely different:

> Picture the cosmos as a rolled-up piece of paper held in place with rubber bands, says Robert Brandenberger at McGill University in Montreal, Canada, who was part of a team that came up with the model in 1989.
>
> The paper is a nine-dimensional universe, and the rubber bands are vibrating strings. If two strings meet, their edges can form a single, twisted loop. That would release three dimensions of space and one of time, which can then swell to the scales we see in the universe today. This process can account for the tiny density variations seen in the CMB and strong gravitational waves – no inflation required.
>
> The BICEP2 results slightly favour this model. If Planck sees the same signal, it could be the first observational evidence for string theory. "For string theorists this is very important," says Brandenberger. "Opponents can no longer say string theory does not connect with data."

While Brandenberger argues that string theory is testable because it predicts inflation is wrong, Science has Scott Dodelson arguing that string theory is testable because it predicts various versions of inflation:

> Moreover, Dodelson says, theories of quantum gravity, such as string theory, predict modifications to the shape of the inflaton energy landscape. So if that landscape can be measured precisely, he suggests, physicists might finally put string theory—long mocked as an untestable "theory of anything"—to a concrete test.

Then of course there's Michio Kaku who at NBC News explains:

> "Inflation simply says there was a bang, and it expanded rapidly, but it doesn't say what the fuse was," Kaku said. "Nobody can say they know what the fuse is."
>
> Kaku, a string theorist, says that string theory could provide the answer … or answers. The cosmic parameters for string theory suggest that the number of possible universes could amount to around 10 to the 500th power. That's a 1 with 500 zeroes after it. Such a scenario offers so many possibilities for parallel universes that in some of them, "Elvis Presley is still alive," Kaku joked.

Besides watching the string cosmology clown-show, I've not followed at all closely the huge amount of work done by theorists in recent decades on various ways to get inflationary scenarios, so don't have anything well-informed to say about how the BICEP2 results will affect this area. One thing to watch will be a conference next week at Perimeter (thanks to a commenter here for pointing this out).

For some background on why I haven't paid much attention to this, I should explain some history. Back in 1980 when Alan Guth's work on inflation first came out, I was a graduate student and did pay close attention to what was going on. The arguments from Guth and others for inflation as an explanation for several otherwise hard to understand aspects of cosmology (the horizon problem, flatness problem) were (and are) compelling. Even better, the idea motivating Guth at the time was that the fields responsible for inflation would be those that broke the GUT symmetries, so grand unified particle physics models would explain aspects of cosmology, and cosmological observations might tell us more about GUTs. All in all, this was a very attractive idea.

Over the years though, no evidence for GUTs emerged and it became clear that GUTs didn't actually provide very much in the way of explanatory power about the Standard Model. Lots of work was done on inflationary models, but these models just typically invoked a single conjectural scalar field (the "inflaton"), with its relationship to anything known in particle physics a mystery. Earlier CMB data gave some hints of further evidence for inflation, and now the BICEP2 data provides yet more significant evidence, so there's lots of reasons to take seriously the idea of inflationary scenarios. The models getting some confirmation though seem to be very simple ones, with a single inflaton field and a very simple potential. This is great news for the general idea of inflation, but still leaves the whole subject with pretty much no convincing explanation of anything about particle physics, and with a minimal connection to quantum gravity (although one intriguing new BICEP2 paper I did notice was this one).

The sad thing about this whole subject though is how some people involved in it have reacted to its problems making connection with particle physics, by throwing in their lot with the multiverse as an explanation for the failure of string theory. The multiverse functions here as an all-purpose excuse for not being able to explain anything about particle physics, with the argument being made that particle physics is fundamentally something just random and inexplicable, different at different points of the multiverse.

The standard move of the people doing this is to point to the fact that in the simple models getting some confirmation, "eternal inflation" can give you lots of copies of our universe, all with the same physics. This is advertised as "evidence for the multiverse", with no mention of the fact that, to the extent this is true, it's evidence for what Tegmark calls a "Type I multiverse" (all the same physics), not a "Type II multiverse" (different physics in different universes, making our physics unpredictable). Several physicists in recent years have been engaged in a vigorous publicity campaign based on confusing this issue, and the BICEP2 results found them hard at work. There's Max Tegmark here and here, Sean Carroll at the New York Times, and Andrei Linde and Alan Guth everywhere (see for example here, here and here).

Luckily not all of the press coverage is dominated by this, with the better science journalists doing a good job of ignoring it and focusing on the real story (a good example is Dennis Overbye here and here).

For some other press coverage of the "BICEP2 implies Multiverse" story, there's Fox News, which has Dr. William Lane Craig explaining how this is proof the scriptures are true. Claims are also being made by The Bosnian Royal Family for having priority over Andrei Linde in this proof of the Multiverse.

Update: On Twitter, Peter Coles comments that "Perhaps there is a part of the multiverse in which the #BICEP2 results provide evidence for a multiverse, but I don't think we live there."

Comments

1. **Geoff**
   March 26, 2014

   From January 14, 2013 – "What should we be worried about?"

   Geoff: "If anyone is taking bets on where the necessary input for the way forward is going to come from, can I put my money on observational cosmology?"

   Woit: "You can put your money there, but people have been making that bet (that something learned from cosmology would tell us how to go beyond the standard model) for more than 30 years now, and so far it has been a losing one."

   I'd like to double down on observational cosmology as well as various systems that are very far from equilibrium.

2. **Shantanu**
   March 26, 2014

   Peter there was also a nice talk by Will Kinney at PI after the Bicep-2 resut and he
   mentioned that most (proposed) string theory inflation models are dead.
   http://pirsa.org/14030116/

3. **anonymous**
   March 26, 2014

   Since you've recently discussed HEP funding, I'd like you to consider for a moment the monetary implications. The LHC cost ~$10 billion. BICEP2 cost ~$20 million. (BICEP2 costs effectively more than that number because of the general costs of the US Antarctic Program, but the total for taxpayers can't reasonably exceed ~$50 million.) Think of where the future of fundamental physics ought to be, in terms of bang per buck. I actually feel kind of sorry for all the grad students and postdocs who have specialized in accelerator physics, many of whom will want/have to switch gears in the coming years while lagging behind their cosmologist peers.

4. **Sesh Nadathur**
   March 26, 2014

   Hi Peter,

   It's not true that the simplest inflationary models are the ones being confirmed by BICEP2 – in fact these models have a pretty hard time reconciling the BICEP2 data with Planck, and are therefore pretty bad fits overall. This is because such large tensor perturbations add power to the temperature power spectrum at low multipoles, where the Planck data was already marginally too low.

   One way to reconcile Planck+BICEP2 is by a very large running of the scalar spectral index, which cannot be produced by quadratic chaotic inflation or natural inflation (in spite of what Katie Freese tells the world). Another way is to introduce a blue tilt to the tensor power spectrum – something that is very definitely not consistent with inflation, but is apparently consistent with Brandenberger's model. Other possibilities within the inflationary paradigm are anticorrelated isocurvature perturbations reducing the power at large scales, something that produces a broken power-law primordial spectrum and so on.

   The data isn't yet enough, or secure enough, to provide compelling evidence in favour of any one of these alternative scenarios. But it is definitely not pointing to the simplest inflationary models.

5. **Peter Woit**
   March 26, 2014

   Thanks Sesh,

   Maybe a better way to say it is that my impression is that the data is still consistent with simplest models (I'm influenced by your latest post where you find good odds that the BICEP2 r value will come down…), and that seems remarkable. After a long career of watching HEP experiments, where pretty much all initial results that seemed to point to deviations from the simplest (Standard) model ended up going away, maybe I'm biased in favor of the expectation of that happening here.

6. **Peter Woit**
   March 26, 2014

   anonymous (and Geoff),

   The problem is that we're learning from cosmology experiments about the very early universe, but we don't seem to be learning anything new about the questions we don't understand about the Standard Model and particle physics. Yes, cosmology experiments are much cheaper than HEP experiments, but they are telling you about something different. And they require many fewer people, so a world with no LHC and just cosmology experiments would be a world with a lot fewer physicists, not the same number doing different things.

   I noticed on the Simons Foundation web-site that Jim Simons is funding some telescopes looking for B-modes https://www.simonsfoundation.org/features/foundation-news/new-telescopes-search-for-origin-of-universe/

One aspect of giving up on doing large expensive big science physics projects is that you could also give up on government support and just have hedge fund guys funding the whole business…

7. **David Nataf**
March 26, 2014

Bicep2 is a cheap experiment but it's not as though their experiment works in isolation. They require theoretical input from a large community of astronomers who have researched and detailed the properties of the gravitational lensing background; further, their result requires combination of their data with data from the Planck experiment.

Bicep2 doesn't show that we should end large experiments to only have small experiments; what it shows is that small and medium-sized experiments need to be a part of the scientific ecosystem, and that there should be a spectrum to the size distribution of experiments.

Which I'm happy to see proven. There are senior scientists who believe that everything in science should be done within the context of gigantic collaborations.

8. **Ted K.**
March 26, 2014

Dear Peter,
I read your blog site for a long time and I appreciate the information you provide about physics in general. Can you please let me know if Bosnian Crown Prince Mensur Omerbashich's claims, against Wineland And Haroche Nobel Price winning, have any merit? I know that this question is off-topic, but if they are, by any chance, true gives a bad name for some physisist and this is very unfortunate fact. Thank you.

9. **Peter Woit**
March 26, 2014

Ted K.,
By the multiverse philosophy, there are an infinite number of universes out there in which the Bosnian Crown Prince's claims are true. However, I very much doubt they are true in this one.

10. **Adam**
March 26, 2014

Hi Peter,

Nature article on the inflationary models that are winners/losers in the BICEP2 sweepstakes:

> *"But he said that the findings would agree remarkably well with 'chaotic inflation', a simple version of inflation Linde developed 30 years ago.* ***In Linde's model****, inflation never completely ends, stopping only in limited pockets of space, while continuing with its exponential expansion elsewhere.* ***Chaotic inflation would produce*** *not just our Universe but* ***a multiverse*** *containing many pocket universes,* ***each with its own laws of physics,*** *an idea that critics say would be untestable."*

This is saying that Linde's chaotic inflation model is a Type II multiverse. Is that correct? From your post it sounds like this is actually Type I, no?

Confused,
Adam

11. **Peter Woit**
March 26, 2014

Adam,
The problem is that if your inflationary model is based just on postulating a single inflaton field, with no information about what this field has to do with the fields we know about that give us the Standard Model, then it's not really going to produce bubble universes with physics different than the Standard Model. So, I'd say what Linde's theory of taking the SM + single, simple inflaton is going to do if it produces a baby universe is just produce one with the SM, so giving a "Type I" multiverse, not "Type II". Linde and others are moving from the simple model being tested here to something else without mentioning this.

12. **Douglas Natelson**
March 26, 2014

Peter, back in the dawn of time when I had Linde for graduate quantum, he did a lecture at some point at the end of the quarter about chaotic inflation. He implied through his dry sense of humor that there could be big differences between different inflated regions (e.g., "If red region fluctuates and spontaneously inflates and becomes blue region, suddenly all our protons decay and I am no longer giving you this talk."). From that remark, it seemed at the time that at the very least he was considering different values for various coupling constants and charges in the different regions. Is that considered still being within the SM?

13. **Adam**
March 26, 2014

Hi Peter,

So if I understand correctly, you are saying that Linde's simplest version of chaotic inflation does not include any reference to how the inflaton field is related to the SM. However, I see this on that [in]famous Wikipedia article about the multiverse:

> *Different bubbles may experience different spontaneous symmetry breaking resulting in different properties such as different physical constants.[10]*

Where the cited reference is to Max Tegmark apparently speaking about Linde's chaotic inflation model. Is this a direct implication of the theory or just a speculation about how the the model may be related to the SM?

14. **Peter Woit**
March 26, 2014

Adam,
This is speculation, not the simple model being used to get the B-mode prediction. It's precisely this kind of replacement of the actual model at hand being tested that one may have some evidence for by something vastly more speculative that one has no evidence for that I object to. I don't see any place though in the public discussions of

this from Tegmark, Linde, etc. where this is mentioned. What's controversial about the multiverse (and the source of complaints from "multiverse skeptics") is not the idea of lots more parts of the universe out there with the same physics that we can't see, but rather the putting forward of a speculative, completely untestable model that says you can't ever understand "why the Standard model", when there is no evidence at all for this.

There is a huge activity among theorists of coming up with specific models (for instance string models) in which the inflaton is related to the low energy SM physics in specific ways, but those aren't what is being tested here.

15. **Peter Woit**
March 26, 2014

Doug,
As I tried to explain in my posting, it's always been the idea of people doing this that the scalar field (or fields) responsible for inflation might be one of the Higgs fields responsible for breaking the symmetry of a GUT theory down to SU(3)xSU(2)xU(1). Then the behavior of inflation and the GUT would be linked, and you might get bubble universes with different symmetry breaking behavior (and, for instance, unstable protons). So, that's the vision, but it's not at all the model being tested here, where there's no known relation between the single inflaton and GUT symmetry breaking. Again, what I object to is the sleight of hand replacement of what is being tested by a speculative vision one has no evidence for.

16. **Adam**
March 26, 2014

Peter,

Thanks for the clarification. I admit to being confused by what some of these folks are saying in the popular press and wikipedia. This brings up another question for me though… this activity of generating models with some specific connection between inflation and the SM, well I'm trying to imagine a scenario where one of these models is successful in making connection with both the BICEP2+future Plank results and the SM. Is there a scenario where one of these models might also directly imply a type II multiverse?

I guess what I'm getting at is even if we know that one of these inflationary models "implies" other parts of the cosmos that are causally disconnected from our local patch we'd never be able to detect them and so how would we ever know if they contained different physics? I might be impressed with the Type II multiverse if a model that connected with all known physical experiment/observation directly predicted it, but even so we'd never be able to test and therefore this prediction would not be physical. A scenario where I would be **more than impressed** would be the existence of such a model and a long (50 years?) search that fails for a Type I model that met all observable facts. How about you?

Can you imagine any scenario where you'd be impressed or inclined to believe in a Type II multiverse?

17. **Peter Woit**
March 26, 2014

Adam,
The problem with this is that you can come up with all sorts of hypothetical models and hypothetical evidence, and what multiverse promoters like to do is to discuss those and say "AHA, you see, the multiverse IS testable". I've been through all this before with string theory, where you also can imagine all sorts of hypothetical versions of it that might be actually testable (but there's no evidence for any of them). Engaging in this just seems to me a waste of time, one that I've been forced to participate in because otherwise you get painted as some sort of fool too stupid to understand that things can be indirectly tested.

What I'd like to see from experts is an honest characterization of what the data successfully tests, and I'd be willing to spend more time to try and understand exactly what that is. It's perfectly reasonable for theorists to devote their time to studying speculative scenarios, what their implications are, whether they are ruled out, how they might be tested in the future. Absent any experimental evidence or a really beautiful convincing idea, I'm not going to spend much time though trying to follow this or engage in it myself, since there are other things I could more fruitfully be doing.

18. **Shantanu**
March 26, 2014

Peter at Stanford there was a 2 hour symposium of his which was webcast
https://www6.slac.stanford.edu/kipac-colloquium-bicep2

19. **Kavanna**
March 26, 2014

The "different physics in different bubbles" is another example of where string theorists have lifted a concept from the non-string world and use it sloppily to talk about "multiverses." The original idea was that different spontaneous symmetry breakings (and possibly different compactifications) could lead to different ground states, or vacua. Physics, in the sense of dynamics, is still the same everywhere, and there's a single connected spacetime.

The remarkable thing about the BICEP + other results is, if they all hold up, how strict the constraints are. Looking over Kinney's talk, I see a great slaughter of the theories, leaving only a few standing. But then again, that's how progress in science happens.

20. **Cormac O'Raifeartaigh**
March 26, 2014

Hi Peter,
don't forget that another impressive aspect of the hypothesis of cosmic inflation is that it gives a plausible explanation for the growth of inhomogeneities in the formation of large scale structure. As far as I know, there are few modern explanations for structure formation that don't involve some form of inflation, which is pretty impressive given that this was not part of the original motivation for the theory

21. **Peter Woit**
March 26, 2014

Kavanna,

I'm not objecting to the different ground states = different physics terminology. The problem is that you don't know what the relation is of the inflaton to the SM, so the inflaton is an independent field and you're not going to get vacua with different physics than the SM using it. When Linde says things like his comment about the proton, he's assuming some theory of the inflaton has been found in which it's part of the GUT symmetry breaking sector, but that's something he wants to be true, not something he knows how to do.

22. **Tim Fleming**

March 26, 2014

Basic Physics is out of my field. I work in an applied area, however I do try to keep track of what is going on. My question is how is this possible confirmation of inflation related to the prediction by loop quantum gravity that the universe should undergo a big bounce i.e. that there should be a period of rapid expansion (the work of Martin Bojowald and others). On the surface the bicep2 result would seem to support LQG but I am nowhere near an expert on this? Can anyone shed some light here?

23. **Jesse**
March 27, 2014

"The problem is that you don't know what the relation is of the inflaton to the SM, so the inflaton is an independent field and you're not going to get vacua with different physics than the SM using it."

At the very least, doesn't the fact that electroweak symmetry breaking is built into the Standard Model mean that in an eternal inflation theory where other causally disconnected regions undergo inflaton decay, things like the masses for the W and Z gauge bosons should vary from region to region? Or am I misunderstanding something?

24. **Sesh Nadathur**
March 27, 2014

I think Peter's right about the fact that quadratic inflation as written down doesn't give you a way to get different physics in different universes. What's more concerning is that unless you have couplings of the inflaton to the SM, you don't get a thermal bath of SM particles after inflation (the inflaton has to decay into these particles somehow). So the simplest quadratic inflation model actually doesn't give us the universe we see – there's no CMB, for instance. Of course it's possible to imagine that there are some couplings of the inflaton to the SM which facilitate reheating, but then you've got to demonstrate that they don't simultaneously change the potential during inflation, changing the model predictions (and possibly spoiling the flatness altogether). All this is made harder by the fact that the inflaton field has super-Planckian values, so using effective field theory is out but it's not clear what to use instead.

The conclusion then is that quadratic inflation is a toy model that shouldn't be taken too seriously. Or at least that's my understanding based on the situation a few years ago: if there's been any progress made in making the model more realistic, I'm happy to be corrected.

(Of course quadratic inflation is not particularly unique in this regard. As far as I am aware there is *no* complete model of inflation which simultaneously solves the problems of initial conditions, slow-roll regime predictions and reheating.)

To return to the topic of my previous comment, I think even if the Bicep result does eventually get modified down to r~0.1-0.15, the evidence still wouldn't point to the quadratic inflation in the sense that a complex model which included a running (or broken power-law etc) would still be a better fit to the data. The "simplest" models would then be disfavoured with respect to such a model at – I'm guessing – roughly the same level that, pre-Bicep, they were disfavoured relative to the Starobinsky model.

25. **Peter Woit**
March 27, 2014

Thanks Sesh, that's helpful.
Jesse,
The point is that the inflaton is not the electroweak Higgs, it doesn't couple to the W and Zs. There are models that try to use the Higgs field to do inflation, but the claim seems to be that these are not compatible with the BICEP2 result.

26. **Lee Smolin**
March 27, 2014

Dear Tim Fleming,

The effect of loop quantum gravity on models of inflation have been studied by several authors, by adding an inflaton field and potential to loop quantum cosmology models. A recent detailed study is http://arxiv.org/abs/1302.0254 by Agullo, Ashtekar and Nelson. Some of the standard picture is unaffected but there is one crucial modification to the predictions which is that the usual relationship between r and the tensor tilt is modified (see Figure 9 and eq. 5.18). This is interesting given that apparently one possible way to resolve some of the tension between the Planck and Bicep1&2 data could be to have a blue tensor tilt, which would contradict the usual inflationary prediction.

There are also implications from loop quantum gravity for parity odd effects in the tensor modes, coming from the existence of an analogue of the QCD theta parameter in the gravitational action: the Immirzi parameter. There is the exciting possibility that this parameter could be measured by observing primordial B-T and B-E correlations, which sec 8.2 of the BICEP2 paper hints may be possible. This was proposed in http://arxiv.org/abs/0806.3082 and developed in arXiv:1108.0816, arXiv:1104.1800 and http://arxiv.org/abs/1007.3732.

Thanks,

Lee Smolin

27. **Jesse**
March 27, 2014

"The point is that the inflaton is not the electroweak Higgs, it doesn't couple to the W and Zs. There are models that try to use the Higgs field to do inflation, but the claim seems to be that these are not compatible with the BICEP2 result."

I guess I probably am confused about something here, from reading nontechnical accounts of spontaneous symmetry breaking I had thought that all that's required is that the energy density of the entire observable universe would have been above the scale where a given type of symmetry breaking happens, and then when the universe first drops to a sufficiently low energy, that's when the new broken-symmetry vacuum state is decided in a random way (analogous to how if an entire magnetic material starts out above the Curie temperature, then when it drops below that temperature the direction of magnetization in different domains is determined in a random way by spontaneous symmetry breaking). Are you saying it's possible that even if the energy density throughout the observable universe had been above the scale of electroweak symmetry breaking before some cosmological time T, the new broken-symmetry masses of W and Z bosons that would appear after T could have already been "baked in" before T, so that there was nothing truly random about the new vacuum state that arose after electroweak symmetry breaking?

28. **Peter Woit**
March 27, 2014

Jesse,
The problem is that you're mixing a story about the Standard Model with the story of the actual model that is being compared to BICEP2 results, and supposedly can

produce bubble universes.

I think I'm just repeating myself, but the point is simple: you can't use the model being tested here to say anything about the Standard Model, because the Standard Model doesn't appear in it. More specifically, the inflaton field being used is not one of the Standard Model fields, and you don't know how it couples to Standard Model fields.

This is the issue I was trying to point out in the posting. The idea of inflation has had some success in explaining cosmological observations, so should be taken seriously for that reason. It has not yet had any success in understanding its relation to the rest of physics: there we just have lots of speculative models, none with any compelling evidence for them.

29. **West**
March 27, 2014

@ Sesh

When you say the simplest models are disfavored, do these comparisons take into account the uncertainty in the new parameters? Or is this a likelihood ratio of the null-hypothesis vs model with best fit parameters?

30. **Sesh Nadathur**
March 28, 2014

Not quite sure I've understood your question correctly, but if you mean is the marginalized posterior probability distribution for n_run from Planck+Bicep2 data broad enough to still be consistent with the predictions from the simplest models, the answer is no.

I haven't run the fitting myself, so I am reading numbers off from tables Antony Lewis has produced. But judging from these tables I would say a zero/negligible running is excluded at roughly the 3 sigma confidence level.

31. **Chris Kennedy**
March 28, 2014

Peter,
Sorry to add overkill to your discussion with Jesse but: Is BICEP2's inflation model claiming incompatibility with Higgs because the inflation couldn't possibly be explained as a pre-Higgs field expansion in theory – or is there something more specific such as the end of the inflation period doesn't coincide timewise with the appearance of the Higgs field?

32. **Peter Woit**
March 28, 2014

Chris,
The question of whether you can use the Higgs as the inflaton is a complicated one. My vague understanding of the situation is that the simplest such model doesn't work for basic reasons (wrong shape + magnitude of the Higgs potential), but you can try and make it work by doing things like coupling the SM to gravity in a non-minimal way. This was all pre-BICEP2, and for a very general take on this, you could try

http://profmattstrassler.com/2013/03/26/cosmic-conflation-the-higgs-the-inflaton-and-spin/

That source says that such models predict r=0, so would be ruled out by the BICEP2 result if you believe it. But, a couple days after BICEP2, I recall seeing on the arXiv in quick succession two papers, one saying BICEP2 ruled out the Higgs as inflaton, another saying BICEP2 was consistent with the Higgs as inflaton.

My non-expert take on all this is that what's going on is that the simplest models of inflation are incompatible with the Higgs being the inflaton, to get compatibility you need to go to something more complicated, and that this has gotten worse post-BICEP2. But for something more specific, you need to consult an expert, not me…

33. **Bernhard**
March 28, 2014

"Linde agrees. "There is no need to discard string theory, it is just a normal process of learning which versions of the theory are better," he says."

OK Peter, now I am starting to believe you are making those things up.

Are you absolutely sure a real person (flesh and blood) said that?

I will check, be sure.

34. **West**
March 28, 2014

@ Sesh

While my question was about hypothesis testing rather than parameter estimation, thank you for your concise answer on that problem. Looking over http://arxiv.org/abs/1303.5082 (fig. 2), it's clear that even before the BICEP2 results were released there was reasonable evidence of non-zero spectral running. So your 3-sigma statement seems perfectly reasonable.

I was originally curious what the Bayes factor or evidence ratio would be when comparing the simplest models of inflation to more complicated ones. More complicated models will naturally fit the data better. That's not a surprise. I just wanted to make sure your statement about the simplest models being disfavored didn't stem from using a likelihood ratio involving best fit parameters for the two hypotheses.

Sorry about the confusion stemming from my poor wording.

35. **DrDave**
March 29, 2014

@Bernhard
Linde quote:
http://www.nature.com/news/gravitational-wave-finding-causes-spring-cleaning-in-physics-1.14910
(link is also in Peter's original post)

36. **Bernhard**
March 29, 2014

Dr Dave,

Sorry my sarcasm was not clear enough.

Bad joke on my (and Linde's) part.

37. **Krzysztof**
March 29, 2014

One thing is pretty disturbing – it seems like an established fact that the production mechanism (according to F. Wilczek for example) of the primordial gravity waves is necessarily "quantic", that is – it requires existence of gravitons. That would be a very major result, however not everyone is mentioning this aspect so strongly – including the BICEP team. Often it is even omitted. So, is that relation (between inflation and gravitons) really a compulsory part of the theoretical prediction, or not?

38. **Peter Woit**
March 29, 2014

DrDave/Bernhard,

The Linde quote does a great job of reflecting the new way of doing science embodied by "string cosmology": experiment can never show that string theory is wrong, all it will do is tell us which of the infinite variety of string theories is ruled out, with always another infinity of possibilities still in the game. I'm characterizing this behavior as a comic parody of science, but maybe it's not funny….

39. **West**
March 29, 2014

Where in the explanation of inflation is it necessary to posit the graviton as the origin of primordial B mode gravitational waves?

Inflation explains the rapid expansion of a very dense early universe prior to the hot Big Bang. So we have an anisotropic mass-energy distribution and highly dynamical spacetime curvature. Sounds like a classical GR problem to me.

40. **Oldster**
March 29, 2014

Krzysztof, I am also curious about these graviton claims. One source (I no longer have the link) appears to claim that BICEP2 proves gravity must be quantized by replacing the metric with operators, which leads to the required quantum fluctuations needed to produce the B modes, and to gravitons. I think some of the links referenced above by Lee Smolin may shed some light on this, but as I understand your question (and mine), are such models necessary, or are they merely in some sense sufficient to produce the B modes?

Another claim (again, no link) may have been trying to say that the classical Einstein equations coupled to the expectation values of stress-energy, are generally inadequate to produce the fluctuations that produce the B modes. But even that wasn't clearly stated when I saw it. Indeed, they may have been trying to talk about some WKB approximation to quantizing gravity being inadequate instead. I apologize for losing the links, and being unable to precisely quote the claims.

So I can't answer your question, but I have similar questions myself, if anyone can shed light on this. And I see West has also added a similar question above …

41. **Peter Woit**
March 29, 2014

Maybe some expert on this will help. All I can tell is that there are claims that this signal means one is seeing quantum effects, see e.g.
http://arxiv.org/abs/1309.5343
other claims that "signal no necessarily quantum", see
https://www6.slac.stanford.edu/sites/www6.slac.stanford.edu/files/B_discovery_senatore_v4.pdf

In any case, I'm more curious to know if there are any prospects to learn something non-obvious about quantum gravity. All I see in Krauss/Wilczek is standard dimensional analysis about the size of a graviton excitation if gravity is quantized

42. **paddy**
March 29, 2014

On the question of whether quantization of gravity is needed to explain BICEP2 results I have no insight just an historical note. The discussions remind me of the debate in the '70's concerning the "heuristic semi-classical" interpretation of quantum optics (fostered by Jaynes and others) about whether quantization of the EM field was unnecessary to describe any "quantum optics" phenomena. This historical analogy is only "interesting" and not "telling" since quantal absorption of energy from the EM field is and has been for 100++ years established fact whereas at least some research (links given by PW) question whether it is even physically possible to imagine a detector that can experimentally detect a graviton. If indeed, subject to time and further considerations, BICEP2 and/or other results do so indicate the necessity of gravity quantization for explanation: it is a big deal.

43. **Igor Khavkine**
March 29, 2014

I'm not an expert in the various physical effects surrounding inflation. However, based on information provided in some of the presentations of the BICEP2 results, it seems to me that the status of the B-mode signal detection as evidence of quantum gravitational effects + inflation can be put on the same footing as the status of CMB temperature fluctuations as evidence primordial quantum fluctuations of the inflaton field. Indeed, the latter seems to be fairly well accepted and the two observations are indirect in similar ways. I've based the following on information from various talks I've attended and discussions with cosmologists. It would be rather hard for me, unfortunately, to dig up specific references. Perhaps a real expert could weigh in with corrections.

In my understanding, the temperature anisotropies that we see (provided all foreground effects can be assumed to have been eliminated) tell us directly only about the photon times of flight (accounting for different amounts of red shift) from the surface of last scattering to us. These varying times of flight are then considered evidence for (classical) density fluctuations present at the time of recombination. The distribution of mode amplitudes of these fluctuations appears to be gaussian, with covariance estimated from the anisotropy 2-point function. Deviations from gaussianity have yet to be detected (as, for instance, in deviations from expected higher n-point functions).

Similarly, the observed B-modes tell us directly only the presence of (classical) gravitational waves at the time of recombination. Actually, already this point could be disputed, because the degree of directness depends on the ability to exclude other sources of B-modes. Perhaps magnetic fields could be another source, but the BICEP2 analysis team didn't seem to think that it was likely. I'm not sure about all the reasons, but lets take that for granted now. Lets also presume that the distribution of mode amplitudes of these gravitational waves was also gaussian, with covariance matrix estimated from the B-mode 2-point function. At the very least, I have not yet seen anyone bring up any evidence of non-gaussianity in the detected B-modes.

Now, the above evidence suggests the presence of classical density and gravitational wave fluctuations, with gaussian distributions, at the time of recombination. If inflation did happen, then it would leave behind this kind of signature, as amplified quantum vacuum fluctuations: (a) gaussian distribution of fluctuations connected to the gaussian shape of the quantum vacuum, (b) "large" amplitude (large enough for the fluctuations to have become classical) set by the amount of expansion during inflation, (c) a fixed relationship between gravitational and scalar amplitudes as a function of frequency (based on the similarity of the Fock-like vacua used for both the inflaton and graviton fields). The observational evidence for (c), I think, at the moment is quite weak because BICEP2 has access to only a small range of frequencies. But more concrete evidence (one way or the other) is likely to appear in short order, along with confirmations/disconfirmations by other experiments.

So, in the absence of other pre-recombination physics that would generate signals with specific signatures (a), (b) and (c), the observations of temperature anisotropies and B-modes do point toward inflation, an inflaton-driven period of rapid expansion in the early universe. And, if inflation did happen, then the detected B-modes do in fact descend from amplified graviton quantum vacuum fluctuations. A similar thing was said, and widely accepted, of temperature anisotropies long before the B-mode detection. Of course, alternatives where a signal with signatures (a), (b) and (c) is not of quantum origin might be possible, but they'd have to be subject to investigation and testing like any other hypothesis. At the moment, the inflation hypothesis seems to be doing rather well compared to its rivals.

44. **Marcus**
March 30, 2014

Peter: "This is great news for the general idea of inflation, but still leaves the whole subject with pretty much no convincing explanation of anything about particle physics, and with a minimal connection to quantum gravity (although one intriguing new BICEP2 paper I did notice was *this one*)."

this one is
http://arxiv.org/abs/1403.4226
Agravity
Alberto Salvio, Alessandro Strumia
(Submitted on 17 Mar 2014)
We explore the possibility that the fundamental theory of nature does not contain any scale. This implies a renormalizable quantum gravity theory where the graviton kinetic term has 4 derivatives, and can be reinterpreted as gravity minus an anti-graviton. We compute the super-Planckian RGE of adimensional gravity coupled to a generic matter sector. The Planck scale and a flat space can arise dynamically at quantum level provided that a quartic scalar coupling and its β function vanish at the Planck scale. This is how the Higgs boson behaves for Mh≈125 GeV at Mt≈171 GeV. Within agravity, inflation is a generic phenomenon: the slow-roll parameters are given by the β-functions of the theory, and are small if couplings are perturbative. The predictions ns≈0.967 and r≈0.13 arise if the inflaton is identified with the *Higgs of gravity*. Furthermore, quadratically divergent corrections to the Higgs mass vanish: a small weak scale is natural and can be generated by agravity quantum corrections.
24 pages
=====================
"Agravity" is short for "adimensional gravity" which is Strumish for a proposal that neatly packages fundamental physics—scale-free geometry with the Planck scale arising dynamically from a Higgs-analog field dubbed *Higgs of gravity*.
I'd like to hear comment from others.

45. **Lee Smolin**
March 30, 2014

Dear Marcus,

I took a glance at the "agravity" paper, and it is just the old Kelly Stelle theory from 1977, which has been known since that time to be non-unitary in perturbation theory, in addition the Hamiltonan is not bounded from below. It is perturbatively renormalizable and in fact asymptotically free, which in this case is bad, because there is faint hope to save the theory by going beyond perturbation theory. Many people have tried unsuccessfully to save this theory, from Terry Tomboulis and Gary Horowitz back then to the contemporary asymptotic safety people and Phillip Mannheim more recently. If I read the paper correctly, the authors admit they have nothing to add to these issues. If this was the right answer, quantum gravity would have been solved long before string theory and LQG were even invented.

Dear Peter,

Just to repeat, if these are quantum fluctuations of the gravitational field, amplified by inflation or some similar mechanism there is a great deal new to be learned about quantum gravity, starting with parity breaking effects, and continuing with non-perturbative effects modifying the relation between r and the tensor tilt.

Given the prospects for much improved precision for the observations, (quoted as r measured to 1 percent) perhaps its not crazy to say we may have confirmation of quantum gravity effects before we have direct detection of classical gravitational waves.

Thanks,

Lee

46. **Marcus**
March 30, 2014

Indeed a couple of paragraphs on page 5 of the Salvio Strumia paper refer to the 1977 Stelle work and several of the other items Lee mentioned, see their references [6] through [10]. So they confirm in some detail the negative findings that were mentioned and nevertheless seem eager to have another go at this approach.

47. **Omerbashich**
April 2, 2014

Dear Ted K. and Peter Woit,

Unfortunately for Wineland, it is in this universe that time flows only forward, so that lawyers can make their living (prove cases) based on this thing called timeline. So:

– It is in this universe that I had sent my result to Winelalnd three months before he submitted his Nature paper with the "brilliant lab technique" that took him to Stockholm.

– It is in this universe that, of all gasses, he pumped his laser exactly with Argon in order to do the praised "ingenious lab trick for abridging quantum and mechanist worlds".

– It is in this universe that Argon happens to be the only gas with the affinity ("particlequake" energy output) absolutely matching my (our universe's) gravitational resonance ratio, of 369.2, found explicitly (as an equation) in the paper I sent to him.

– It is in this universe that there exists no third physical quantity with the value of 369.2 whatsoever.

– It is in this universe that he hasn't offered a theory for his "ingenious lab technique" with which he beats to the ground: Einstein, Feynman, Dirac, Oppenheimer, Schrodinger, Mickey Mouse, et al., but nonetheless got only a share of Nobel prize – hmm, what would jury say…

– It is in this universe that Brian Josephson agreed in an email debate with me (after a few days though — perhaps after I exhausted him?): "I accept that it is a remarkable coincidence" (though Brian too had a hard time accepting how Wineland could have confused units and dimensions like no freshman would have).

Oh well, it's all "only" forensic evidence after all, isn't it. And it applies to our universe only.

HOWEVER: I was actually able to demonstrate Multiverse/Hyperverse mathematically precisely BECAUSE I lost this universe's units on G at the outset since, reasoned I, Newton had anyway attached those units to G only in order to close his own (our universe's) physics mathematically, so they have no multiversal physical meaning whatsoever! (Remember that Multiverse was the starting physical hypothesis).

So I showed that physical units aren't all that multiversal after all, meaning multiverses of Type II (or whatever pleases Max) – are real.

Meaning: whatever ratio you take (such as Argon affinity of 369.2 kJ mol–1, and compare it to G w/o its bogus units, you'll end up with SOME system of equations, some regularity, because NO UNITS ARE REAL especially when G is involved. Whatever equations you arrive at in the aforesaid way (I did it for Earth-Moon system; Wineland used that concept and did it for particles), you'll arrive at some closed mathematical form.

Now, THAT's the Hyperverse of multiverses we live in. And Wineland figured it out before Brian or anyone else did. For that alone he deserves a sole Nobel Prize — but in some other universe where plagiarizing most of someone's discovery is praised and rewarded. In this universe however, and in the USA especially, one normally ends up behind bars.

48. **Frank Borg**
April 21, 2014

BICEP2 vs quantum gravity … I also find this to be one of the more intriguing issues. It seems that some sort of QGR calculations are involved in two ways:

1. The metric is influenced by the fluctuations of the inflaton field (phi): G = < T(phi) > ; that is, one puts the quantum average of the quantum field stress energy tensor on the RHS of Einstein's field equations (semiclassical case);

2. In the linearised version of the Einstein's field equation one expands the metric in terms of creation/annihilation operators as one do in QED etc. These are then treated in a similar way as one treats the phi-operator in order to calculate gravitational wave fluctuations via quantum averages.

So no *genuine* QGR input really (not surprising since we are not there yet), but if the scalar/tensor evidence holds up it could be a tantalizing hint about gravitons (for linearized GTR); perhaps the only way we will ever get some *experimental* grip on them. Neither type of calculation is based on some consistent theoretical framework – it is basically the best one can do at present (like Bohr's pre-QM atom model) and if they pan out it will be short of a miracle …

For an authoritative account of the fundamental calculations behind the CBR stuff I recommend S Weinberg's Cosmology (2008). Another good and a wide/deep sweeping account is Peter & Uzan, Primoridal Cosomology (2009, 2013). I also looked up Kiefer's Quantum Gravity (1.ed, 2004) and he does indeed single out the CBR as one of the few possible vistas where to experimentally discover QGR effects. By the way, the idea of expecting effects of gravitational waves in the CBR goes back to a paper by Starobinsky in 1979.

A Brief History of String Theory

March 26, 2014
Categories: Book Reviews

On the long plane flight to Italy I had the chance to read the recently published A Brief History of String Theory: From Dual Models to M-theory by philosopher of science Dean Rickles. The book deals with the history of string theory, beginning with its origins in the Veneziano model of strong interactions, and ending in the mid-90s with M-theory and the "Second Superstring Revolution". It's a good serious scientific history, explaining in technical detail exactly how the theory developed, with good explanations of the high points of crucial papers, together with some of the story of how they came about. While I've spent a lot of time in the past reading about much of this history, I learned a lot from the book, about string theory as well as other topics in particle physics that interacted with it. I'm strongly of the opinion that if you want to really understand a subject, you need to understand its history, so anyone who wants to really master string theory would do well to spend some time with this book.

There is something quite unusual about this though as a work of history, since while this subject is 45 years old, it is quite unclear how to evaluate its significance as science (arguments seem to still rage about this…). I can't think of any other topic in modern science which has been the subject of such intense activity, with no one sure of how to evaluate it nearly a half century later. More succinctly, is this the history of a brilliant insight into the physical world or is it the history of a misguided failure? In the introduction Rickles worries that historians of science will find it too "Whiggish", but maybe more of a problem is not knowing what the right final end-point will be. To a large degree Rickles adopts the point of view of many prominent string theorists, that this is a success story, whatever its problems might seem to be. My own point of view is different of course, and I'd claim that in recent years the viewpoint of the physics community as a whole has shifted, with this looking less and less like a success story and more and more like something else.

I can't do justice to all that's in the book, but for personal reasons I do want to focus on one part of the story and how Rickles treats it, one where I have a significant disagreement with him, and one that points out well the basic problem faced by this kind of history. The issue is the 1984 "First Superstring Revolution", generally dated to the Green-Schwarz anomaly cancellation calculation of that summer. Rickles does a good job of explaining the background of this. He emphasizes that this didn't come out of nowhere, that the issue of the problems posed by such chiral anomalies had been identified by Witten and others as of great importance in constructing unified theories.

What should one make of the significance of the discovery by Green and Schwarz of anomaly cancellation for SO(32) in type I string theory? The story of string theory as a success is that this convinced theorists that string theory was a very promising road to unification and unleashed a revolution. But I remember this differently (I had just finished by Ph.D. at Princeton and taken up a postdoc at Stony Brook). The idea that anomaly cancellation predicted a specific gauge group and dimension was obviously attractive, but the fact that the prediction was for the wrong gauge group (SO(32)) and the wrong dimension (10 space-time dimensions) looked to me (and many others) like a deadly problem. The flurry of activity leading to the heterotic string, E8, and Calabi-Yau compactifications was an impressive use of mathematical technology, but there was no sign of the Standard Model coming out of this in any natural way. It looked all too likely that this wasn't explaining anything about particle physics, just parametrizing the choices of possible unified gauge theories in a very complicated way. Yes, there was also a theory of gravity there, but it was not obviously an attractive one.

Earlier today I was watching the video of John Schwarz's general talk about string theory at the Simons Center yesterday. Schwarz gives much the same promotional talk he and other have given many times over the last 20 years (with little change since the addition of M-theory), making claims for success of exactly the sort that inform the point of view of the Rickles book. At the end of his talk, Dusa McDuff asked about parity violation in other parts of M-theory and Schwarz explained that the original 1984 motivation from Type I anomaly cancellation has long been abandoned:

> Nowadays we have enough tricks up our sleeve that we can get parity violation out of anything.

There are now all sorts of ways of getting "string vacua" that might give a unified theory, many not using anomaly cancellation. The supposed breakthrough of 1984 is looking much more like a red herring, and the question of historical interest shifts from "how was this brilliant breakthrough accomplished?" towards "why did so many people not realize this obviously wasn't going to work?"

Rickles on page 162 explicitly takes issue with the comments in my book emphasizing the important influence of Witten at this point, mischaracterizing me (and Smolin) as claiming it was "almost as if that community had no decision-making power of its own". This is far from any thing I think or wrote (a big section of my book explained the excellent reasons why any sensible person would take Witten's opinions seriously). What I wrote was

> By itself the news that gauge anomalies cancel in a version of type I superstring theory would probably not have had so dramatic an effect on the particle theory community, but the news that Witten was now devoting all his attention to this idea spread among theorists very quickly.

and I still think that's quite accurate. Ten years ago I wrote about this in detail on the blog (see here, here and here), including a first-hand version of the story from Larry Yaffe, who was at Aspen and was the one who told Witten about the Green-Schwarz result. He reports:

> Concerning reaction to the Green-Schwarz result, my recollection is that there was relatively little immediate buzz about it at Aspen. John had a fairly diffident style of presentation, and I don't recall anyone jumping up and saying 'this will change the course of physics!'. As best as I can reconstruct my own reaction, it seemed like a technically slick calculation and a nice result but it wasn't, of course, addressing any of the conceptually hard questions about quantum gravity, and it seemed very far removed from the practical concerns of particle physics.

and

> I think the speed with which others in the particle theory community jumped into string theory had a lot to do with Ed's involvement and proselytizing, but I expect that even without his involvement, interest in string theory would have steadily grown, albeit slower.

Rickles ends his detailed history with M-theory, with the latter part of the book summarizing the recent history, again pretty much from the point of view of a string theory proponent, one on the defensive. I think he gets the multiverse issue quite wrong, characterizing the anthropic multiverse vs. search for a unique unified theory dichotomy among string theorists as:

> It is more likely that the two stances will continue in parallel, as they appear to have done for some time, defined more by the personalities of those adopting them than by the physics.

I don't think this has anything to do with personalities. The problem is that the anthropic multiverse point of view predicts nothing, and those unhappy with it are not unhappy because they have a personality that leads them to want or believe in uniqueness, but because they're aware you need to make predictions to be doing science. But, one can't expect historians to get right current events…

Comments

1. **QG**
   March 26, 2014

"Yes, there was also a theory of gravity there, but it was not obviously an attractive one."

what makes this "not obviously an attractive one" ?

2. **tt**
March 26, 2014

complicated is not attractive

3. **Philip Gibbs**
March 27, 2014

Was "attractive" an intentional pun?

4. **Aleksandar Mikovic**
March 27, 2014

Dear Peter,
I essentially agree with your opinion about the string theory history, and that Witten's involvement was very important. I was a graduate student at U. of Maryland in the period 1984-1989, and I did my PhD in string theory, with Warren Siegel, and the main reason I studied string theory was its claim that it can solve the problems of perturbative quantum gravity. This was based on the Mandelstam's work on the finiteness of the superstring theory. The fact that satisfactory string phenomenology was not materialising was not such a big concern for people who were more interested in quantum gravity, because everybody thought that string phenomenology would eventually work out, since there was a lot of compactification possibilities. But, I remember R. Mohapatra, a prominent particle phenomenologist, saying in 1988 that it was impossible for him to get Standard Model out of string theory.

The reason why many people were, and still are interested in string theory is that it is a nice paradigm for a quantum gravity theory, so that for such people the fact that string phenomenology is not still giving results, is not important. I switched from string theory to canonical quantum gravity in the early 90´s, because of string theory inability to address non-perturbative quantum gravity and the absence of a background metric. The second string revolution in mid-90´s made a progress with the problem of non-perturbative quantum gravity effects, but the absence of a background metric independent formulation of string theory is the reason I am exploring LQG and spin foam formulations of quantum gravity.

5. **CPV**
March 27, 2014

I was at Harvard from 1984-1986 and saw Schwarz speak in 1985 I think. The general mood of the older theorists at Harvard was not that positive towards string theory, but that it was probably worth looking at. Sidney Coleman said I believe that since Witten was looking at it, and the math was insanely complex, there was no point in any one else looking at it. However, the younger guys were looking for things to do and it was pretty clear that GUT and BSM model building a la Georgi was not that productive anymore. There wasn't any other really hopeful direction. Condensed matter seemed maybe more productive / interesting. People thought foundational navel gazing was a waste of time. No experimental results. Jaffe maybe had one student doing constructive field theory. Lots of theory people on sabbatical. The issues seem the same today as then, essentially. The telescopes and satellites seem like the way forward today.

6. **Peter Woit**
March 27, 2014

I don't want to start another string theory rules/sucks as a theory of QG debate. The point of my comment was about how things looked to most theorists in 1984: the idea of doing QG via string theory had been around for about ten years and was being promoted by Schwarz and a very few others. No one was buying. Like most people in the field I took a look at this and decided it wasn't worth learning more about. It seemed to be a very complicated way of getting gravity. What got people interested post-1984 was the unification angle, that this would not just give gravity, but unify it with the Standard Model.

7. **M**
March 27, 2014

A nice tradition at CERN is the annual Xmas recital of the theory division, with funny and sometimes deep jokes about latest developments. In the 2001 edition, young string theorists were represented as talibans who talked by quoting sentences from the book by Witten. Privately, young string theorists openly talked about the real situation of the their field.

8. **Kavanna**
March 31, 2014

Being a graduate student in particle theory in the 80s, it was hard to escape the impact of string theory. But unlike later, it wasn't a suffocating monopoly. Lots of other things were going on, like accelerator phenomenology and the burgeoning of astrophysics and cosmology in the Heavenly Accelerator.

The fatal blow for the field was the cancellation of the SSC in late 1993. Particle phenomenology ended, except for some specialized areas. From that point, string theory became truly the only game in town for fundamental theory, unless you were lucky enough to be working in astro/cosmo-particle physics, where real progress was and is being made.

On the West Coast, the impact of the anomaly cancellation calculation took into 1985 or even 86 to be fully felt. It was emphatically viewed as a particle/unification thing, not QG. String theory never lived up to the QG hype, of course, as it's not coordinate-independent and can't be extended beyond second order. After Kaplunovsky and others demonstrated the vacuum problem, the bottom fell out of string theory for a while. Its resurrection in the 1990s had a heavy component of marketing, through the NYTimes science section, for example. What actually happened and what was claimed were two quite different things. Theorists were constructing field theories with symmetries and other properties conjectured to arise from a conjectured "M theory." No progress was being made toward a super-unification any longer, and the claims about gravity were a shell game. It was then and after that talk of "sociology" and the tyranny of Princeton became standard, in private, of course. It took nearly another decade and a half for Peter, and Lee Smolin, to say publicly what had been obvious to many for years: string theorists had developed their own cultish version of political correctness, one that bordered on pseudoscience and drove a deep wedge between string theory and the rest of physics.

… and who is this supreme being named SUSY? What's she like? Sounds as if she's into astrology herself. I prefer SUGRA in my QF-T. (Sorry, bad theory jokes from the early 80s.)

9. **CU Phil**
April 4, 2014

Hi Peter,

You may be interested in this recent issue of Studies in the History and Philosophy of Modern Physics…

http://www.sciencedirect.com/science/journal/13552198/46/part/PA

10. **Peter Woit**
    April 4, 2014

    CU Phil,

    Thanks! I hadn't seen that. The Ellis, Kragh and Smeenk articles in particular do a very good job of explaining the problems of the "multiverse" and of "eternal inflation".

    One thing they don't much deal with (although Kragh does to some extent) is the way the anthropic string theory multiverse business emerged out of the failure of string theory. Physicists discuss this seriously, but it's very clear they wouldn't do so if the idea had emerged in some other way. Specifically, back in 1984, if Witten or any one else had promoted string theory using the anthropic multiverse justification people would treated this as pretty much crazy and obviously a waste of time. It was only after a generation of theorists had been sucked into spending decades working on this that some could be convinced to take this seriously.

11. **mathphys**
    April 9, 2014

    It was Witten's short preprint that appeared at the same time as the Green and Schwarz anomaly cancellation paper, or even right before it, and that used the words "In a stunning development" to describe the result of Green and Schwarz, that started the first superstring revolution. No doubt about that. No one had a clue what the anomaly cancellation result meant or why it was important.

    Incidentally, it was Witten's talk on string dualities at the annual string conference in 1995 that started the second revolution as well.

Sheldon Gives Up On String Theory

April 1, 2014
Categories: Uncategorized

Ten years ago I wrote here about the news that Witten had finally given up on string theory. Today I just heard a similar but even more dramatic rumor: next week's episode of *The Big Bang Theory* features string theorist Sheldon Cooper deciding to give up on string theory, realizing that he has been wasting his time working on it for 20 years. Evidently the framing of the story is that string theory has been a bad relationship for Sheldon, now he's grieving and trying to learn how to get over such a breakup.

In other April 1 news, it seems that some joker at MIT has scheduled an April Fool's Day colloquium there on Our Mathematical Universe, featuring Max Tegmark. The conceit is that Tegmark will explain how the BICEP2 results provide "smoking gun" evidence for the ideas in his book about mathematics (his Mathematical Universe Hypothesis).

Update: For more April 1 fun, Steve Landsburg has Many Many Worlds, a review of Tegmark's *Our Mathematical Universe* which describes the author as "a towering figure in intellectual history".

Update: For some reason, some people didn't believe the news yesterday in this posting. For a link with details, see here.

Comments

1. **joel rice**
   April 1, 2014

   and Jester says they will re-use the SSC tunnel and get going with a BIG collider.
   he really knows how to hurt a guy.

2. **Tom Weidig**
   April 1, 2014

   Lubos will be devastated… 😉

3. **Martibal**
   April 1, 2014

   And what about the realineituhedron ?
   http://theoryandpractice.org/downloads/files/realineituhedron.pdf

4. **AcTeVist**
   April 1, 2014

   It's a nice day today. Spring has returned early to Geneva area this year; the sun, the flowers and the easy time safely away from major conferences make people at CERN feel light-hearted and good about life. Spring clothes and short sleeves are taking over, so I wonder if every t-shirt is going to come out of the wardrobes and be proudly worn by its owners.
   It's a nice and light-hearted day, and as an added bonus everything that follows is real…

   A couple of months ago a post of yours let us know that Ellis "is not giving up on supersymmetry". I wonder whether this could have been kind of an answer to the statement featuring on one of the latest t-shirts in his famous collection. The statement being, "I searched for susy for twenty years and all I got was this stupid t-shirt". (http://i61.tinypic.com/n67xc4.jpg)

   To be honest, he didn't select this one himself. It features on five t-shirts offered to five prominent members of CERN and LHC experiments. Some of them have shaped collaboration policies according to their personal favourite theory, even as Higgs was being found, others went as far as e.g. to claim that the important part about Higgs is not giving mass but constraining I'll-be-happy-to-be-reminded-what which is indirectly related to guess-which-theory.

   Offered by whom? Well, simply susy-agnostics, I guess 🙂

   It often happens that people turn to religion out of fear of death, of the unknown, or a wish of afterlife. It' understandable. In all these cases it can provide a sense of explanation and understanding of the world. Turning to the religion of the perpetually unknown, on the other hand, seems to be a privilege unique to preachers of truth-seeking.

5. **LK**
   April 1, 2014

   Take a look at the April 1st stuff at FNAL:

   http://www.fnal.gov/pub/today/archive/archive_2014/april_fools_2014.html

6. **Peter Woit**
   April 1, 2014

   Science magazine gets with the spirit of the day, letting us know that

   Scientists Find Imprint of Universe That Existed Before the Big Bang
   http://news.sciencemag.org/physics/2014/04/scientists-find-imprint-universe-existed-big-bang

7. **John McAllison**
   April 2, 2014

   Hey Peter,

   you must have had a big smile on your face when you deliberately posted this on April 1st; brilliant timing!

8. **frp**
   April 2, 2014

   I was at Tegmark's talk. It was an overview of inflation, there was no mention of multiverses or the MUH, other than a one sentence comment that inflation might imply more spacetime exists beyond our horizon. One interesting comment he made is that the (Hot) Big Bang should be defined as the hot state of the universe just after inflation ended. With this definition, inflation happens before the Big Bang. I wonder if this wording will catch on. To me, the Big Bang has always meant the hypothetical curvature singularity at t=0, which (if it exists) is before inflation. I think this reflects an unfortunate difference in terminology between cosmologists and general relativists.

9. **Chris Kennedy**
   April 3, 2014

   I also heard that next season Sheldon & Amy will have twins. However Any becomes upset with Sheldon because he doesn't want to recognize them as individuals or give each twin a name, rather he will only refer to them together as Cooper Pairs.

10. **S**
    April 3, 2014

    You might enjoy this article, wherein a medieval scholar's apparently-not-awful physical reasoning is coopted to suggest that, if you merely assume he understood naturality and anthropic reasoning, he discovered the multiverse!

    http://www.huffingtonpost.com/2014/04/02/multiverse-philosopher-middle-ages_n_5075959.html

11. **Kavanna**
    April 3, 2014

    Well, Sheldon throws in the brane — what do you know? String theory really has been one long bad date.

    If they're Cooper pairs, can be they canonically transformed back to an unpaired state? Will they have Primordial Soup for dinner?

    And, while we're at it, the Fermilab April Fool's Day issue is hysterical. I love the Eye of Sauron on top of the tower. Maybe that's who should be funding HE research.

    (Coming soon: some impressions of the big MIT talk tonight on BICEP2. Tegmark and Guth spoke, but there's a lot more. Doesn't look good at all for strings, BTW.)

12. **Chris Kennedy**
    April 4, 2014

    Kavanna,
    Even if the pairs transform back to an unpaired state, if one twin grows up to operate a high-speed rail and the other becomes an orchestra leader, they would still remain (in a sense) super conductors.

Quick Links

April 5, 2014
Categories: Uncategorized

Just time at the moment for some quick links. I'll start with some math news, since there hasn't been much of that here recently:

- Matt Baker has the sad news here of the death of Berkeley mathematician Robert Coleman recently, at the age of 59. Coleman was a leader in the field of p-adic geometry, and managed to continuously do important research work despite a long struggle with MS. He was both highly influential and well-loved, be sure to read the comments which contain appreciations from many different mathematicians.
- Also at Matt Baker's blog is a summary of recent work by Manjul Bhargava and collaborators on the average ranks of elliptic curves. This work shows

  > at least 20.6% of elliptic curves over **Q** have rank 0, at least 83.75% have rank at most 1, and the average rank is at most 0.885…

  and

  > at least 66.48% of elliptic curves over**Q** satisfy the (rank part of the) Birch and Swinnerton-Dyer (BSD) Conjecture (and have finite Shafarevich-Tate group)

  Conjecturally

  > 50% of elliptic curves have rank 0, 50% have rank 1, and 0% have rank bigger than 1, and thus the average rank should be 0.5. (And conjecturally, 100% of elliptic curves satisfy the BSD conjecture. :))

  Until recently

  > the best known unconditional results in this direction were that at least 0% of elliptic curves have rank 0, at least 0% have rank 1, the average rank is at most infinity, and at least 0% of curves satisfy the BSD conjecture.

  so this is dramatic progress.

- I've yet to hear any solid rumors about who will win the 2014 Fields Medals, to be announced at the ICM in August. To my mind, Bhargava is a leading candidate. Others one hears discussed are Jacob Lurie (a question is whether he has a big enough theorem, the one he's talking about here may not be finished). Often mentioned names whose work I know nothing about are Artur Avila and Maryam Mirzakhani.
- Another area of huge progress in mathematics over the past year or so has been work of Peter Scholze, who is another excellent Fields Medal candidate, but one young enough that this might get put off until 2018. I've been hoping to understand his results on torsion classes in Langlands theory well enough to say something sensible here, but I'm definitely not there yet, maybe some day in the future. In the meantime, watch his extremely clear lectures at the IAS (here, here and here) as well as the talks at this recent MSRI workshop.
- The math community award structure is for some reason prejudiced against the middle-aged, with the high-profile prizes going to the young (Fields Medals) and the old (Abel Prize). This year's Abel Prize went to Yakov Sinai, and again, I'm in no position to explain his work. However Jordan Ellenberg was, and there's video here of the prize announcement, including Ellenberg's talk about Sinai's work. In the past Timothy Gowers gave such talks, with not everyone happy about this. No news yet on whether Sowa will change his blog name to Stop Jordan Ellenberg! !!!.
- Leila Schneps is trying to raise funds for an English translation of the 3rd volume of Winfried Scharlau's German language biography of Grothendieck. Go here to contribute, I just did.

Turning to physics news:

- the recent BICEP2 data is still attracting a lot of attention. Initial news stories were often dominated by nonsense about the multiverse, more recent ones are more sensible, including Adrian Cho at Science Magazine, Clara Moskowitz at Scientific American, and a George Musser interview with Gabriele Veneziano.Yesterday Perimeter hosted a workshop on implications of BICEP2. The theory talks I looked at didn't seem to me to have much convincing in them, except that Neil Turok acknowledges that this kills off the Ekpyrotic (bouncing brane) Universe and he's paying off a $200 bet. For some reason, Nima Arkani-Hamed now seems to speak at every single fundamental physics meeting, so was also at this one. More interesting were the experimental talks, with new data soon on its way, including Planck results planned for October, possibly measuring r to +/-.01 (BICEP2 says r=.20).
- For some perspective on inflationary theory, CU Phil in recent comment section points to a new volume on the Philosophy of Cosmology. It includes some great articles putting multiverse mania in context by George Ellis and Helge Kragh, as well as an enlightening discussion of the issues surrounding inflationary theory, especially "Eternal Inflation", from Chris Smeenk.
- For the latest news about LHC results coming in from the Run 1 dataset, see this report from the Moriond conference.
- Finally, physics continues to inspire frightening movie projects, see here.

Comments

1. **katzeee**
   April 5, 2014

   "possibly measuring r to +/-0.1 (BICEP2 says r=.20)"

   does this mean r might be (if this rumor is true) 0.00+/-.01 or is this just a comment on Plancks sensitivity being +/-0.1? thx for answering.

2. **Peter Woit**
   April 5, 2014

   katzee,
   I think one of the speakers from Planck showed a plot of projected sensitivity, showing the possibility of measuring r=0.05 to +/- .o1, and I'm just assuming the accuracy number is similar for possibly larger r as claimed by BICEP2. This number does not claim to include systematics, so the likely Planck number will not be anything this accurate. It sure sounded though that if BICEP2 is right about r=.2, Planck should definitely see this.

3. **Syksy Räsänen**
   April 5, 2014

   Peter:

The official statement from the Planck team is that if the BICEP2 signal is due to inflationary gravity waves, Planck has the sensitivity to see it, but that it is not clear whether they will be able to treat the foregrounds well enough to disentangle the cosmological signal. So at the moment, Planck is not claiming that they will be able to either confirm or rule out the BICEP2 result.

4. **JG**
April 5, 2014

Skysy

If they can't then it will be highly embarrassing, for the cost of the project, to not enable a better measurement than BICEP2. Sure they're doing the whole sky and they have also focused on instensity measurements as well as polarization, but come on, their equipment is in *Space* – at very high expense.

And why has the result been so delayed – is it to prevent a Nobel award this year to the rival team? (lol) – originally Planck said polarization results would be in early 2014, now we getting quoted October and even November – will they actually publish by Christmas? I hope Planck won't be another Gravity Probe B, where publication of the results was delayed for ages and eventually not completely compelling.

5. **Shantanu**
April 5, 2014

JG gravity probe b got delayed by 40 years . When it was launched, no one really
cared one way or the other about the result.

6. **Syksy Räsänen**
April 5, 2014

JG:

The official date for releasing Planck polarisation results is October. The hard deadline is December, when there will be two conferences about the Planck results.

Planck was never optimised for polarisation, neither from the point of view of the detectors nor of the (obviously related) sky scanning strategy. Planck was designed to give the best possible measurement (within cosmic variance limits) of the temperature anisotropy of the CMB. They have delivered this, as planned.

I don't know why Planck was originally not designed with more view towards polarisation (remember that the satellite was designed as a competitor, not as a successor to WMAP), but one reason may have been that it wasn't expected that there would be anything interesting to see at this level. (Even on the website of the South Pole Telescope, which saw B-modes from lensing in July 2013, it is noted that there isn't much reason to expect inflationary B-modes with an amplitude higher than the B-modes from lensing except "in the most optimistic inflationary models".)

Also, treating the foregrounds for the full sky is a lot more difficult than for the relatively clean patch chosen by BICEP2, and this has been the main reason for the delay in the release of the polarisation data.

Note that confirming the BICEP2 data will require Planck measurements of the foregrounds in the sky patch BICEP2 looks at. The BICEP2 team notes in their paper that the main uncertainty in the foregrounds is the lack of a polarised dust map, to be delivered by Planck. (Though the foregrounds are expected to be clearly smaller than the signal they see.)

7. **Peter Woit**
April 5, 2014

Thanks Syksy,

I was going by the Douglas Scott talk at Perimeter, see
http://pirsa.org/displayFlash.php?id=14040122
At around 21 min he shows a plot of simulated data for r=.05, from some sort of semi-public not well known Planck document and that's where the +/- .o1 number comes from. He does explicitly say this is for no foreground. Maybe I'm over-interpreting, but it seems like he wouldn't be showing that plot and devoting a lot of time to it if it didn't have some relation to what they might have. If they were getting killed by the foreground, I'd expect more a talk about how difficult the foreground problem is (that wasn't something he emphasized).

8. **JG**
April 5, 2014

Thanks Syksy (sorry for mispelling your name above)

yes, I understand that one main contribution of Planck's polarization data will be an accurate dust map to enable BICEP2 (and other experiments) to improve their data analysis.

I wonder if Planck discovers a *much* more accurate value of r than BICEP2's (which is eventually confirmed) – who will take more credit historically – how far away from 0.2 will the final experimentally confirmed value have to be for BICEP2 to lose significant credit?

9. **David Nataf**
April 5, 2014

There's nothing embarrassing. The Bicep2 result actually requires Planck results in order to work, you can look up their probability contours, they say "Planck + Bicep2", not "Bicep2".

Aside from that, Planck has measured many other things. It's lowered the value of Hubble's constant and of the dark energy density, and it's giving us more precise maps of interstellar emission.

10. **Ian**
April 6, 2014

Penrose expresses skepticism, not about BICEP2 results, but about their interpretation as evidence for the Big Bang.
http://www.sciencefriday.com/segment/04/04/2014/sir-roger-penrose-cosmic-inflation-is-fantasy.html

11. **Sesh Nadathur**
April 6, 2014

My understanding is that the main problem for Planck is not foregrounds but the scanning strategy. To measure polarisation you need ideally to have two otherwise identical detectors sensitive to orthogonal polarisations pointed at the same part of the sky, or to rotate a single detector through 90 degrees and re-image. Planck attempts the second option by changing the detector angle for successive scans of the same sky region.

Unfortunately their scanning strategy is such that for some sections of the sky the scans are not performed with a full 90 degree rotation but something less than that (apparently this had something to do with this strategy optimising the rate of data transfer back to earth, which was necessary for the high-precision temperature measurements). So for those sections their ability to decompose the polarisation components is limited. Obviously this impacts their ability to extract the large-l polarisation signal, which is unfortunately where the gravitational wave signature will be.

In terms of foregrounds I would have thought Planck should actually be better placed than BICEP, because it observes the sky at so many different frequencies, and therefore is better able to determine what the actual foreground is (BICEP rely on assumptions about the foreground).

12. **Syksy Räsänen**
April 6, 2014

Peter:

I checked that part of the presentation, and I don't think there's anything there other than the official Planck position: they have the sensitivity. The Planck polarisation analysis is ongoing, showing a simulated plot like that doesn't imply that this is what they're actually seeing.

Sesh:

The scanning strategy affects the sensitivity, but according to Planck, sensitivity is not the problem. Or have I misunderstood something?

13. **JG**
April 6, 2014

Seth, that explains it very clearly.

Does anyone know if the delay in publication of the polarization map has been influenced by BICEP2, or was the delay announced previously?

Note that the original release date was stated as "early 2014" in March 2013 after the release of the temperature map, see Notes for Editors

"The next set of cosmology data will be released in early 2014"

14. **JG**
April 6, 2014

I mean Sesh 🙂

15. **David Nataf**
April 6, 2014

"Next set of cosmology data" does not necessarily imply results on all conceivable measurable parameters, does it?

It will be curious to see if their value of Hubble's constant remains 9% smaller than the value determined from standard candles.

16. **Sesh Nadathur**
April 7, 2014

Syksy:

I don't know for sure, obviously, but I was extrapolating from the fact that the reason Planck have not already released their polarization data is because they are having a hard time getting rid of systematic effects at large-l. The effect I mentioned above would affect large-l reconstructions, so I put two and two together and guessed this would be the main problem.

Since then I've looked again at this presentation which suggests again that the problems are to do with systematics at large-l. Depending on how exactly the "gain variation", "bandpass missmatch" and "calibration mismatch" arise perhaps my earlier simple explanation was wrong – but it does appear to be something to do with the scanning strategy that is causing leakage from temperature to polarization at large scales.

Note also that even BICEP have to beat down T->B leakage by a factor of ~100 before they can extract a BB signal. Since Planck have seven different frequency bands and can already remove foregrounds so well from the temperature maps it just seems unlikely to me that foreground removal is the main issue.

17. **David Nataf**
April 7, 2014

Sesh,

I doubt that foregrounds due to astronomical dust are calibrated at the most ideal precision. If it were the Planck maps would be able to use the entire sky in their analysis, instead they still need to mask parts of the Galactic plane even with their seven bandpasses. Away from the plane, the error will still be present, though smaller by a significant factor.

Further, another astrophysical foreground (background?) to worry about is weak lensing distortions. That I know much less about.

18. **Sesh Nadathur**
April 7, 2014

Sure, I understand that. But Planck sees the part of the sky that Bicep saw as well as the galactic plane and all other regions. So if the Bicep result is not due to foregrounds, foregrounds should not prevent Planck from confirming it using at least that patch of sky. Conversely, if Planck discovers foregrounds are unaccountably large even on that patch of the sky, then Bicep didn't really have a detection after all. So I still don't see how the foreground problem can be worse for Planck than Bicep.

19. **piscator**
April 7, 2014

A couple of comments.

As some members of the BICEP team are also on Planck, it seems highly unlikely that whatever the official status of published foreground maps, the BICEP team do not have some idea of what the foregrounds are going to be. And no-one I have heard from Planck has said that foregrounds are their principal worry about the BICEP result (leakage of E -> B is what I hear more commonly).

@David: Planck does not measure H_0. They measure the CMB, and by extrapolating the CMB forward here using LambdaCDM they make a prediction for the value of the Hubble constant in the local universe. But this is a model-dependent prediction, not a measurement.

For example, with an extension of rLambdaCDM to include Neff, both the Planck-BICEP tension on r and the Planck-H0 tension go away.

20. **Ben Gold**
April 7, 2014

Sesh,

Planck observes the same patch as BICEP, but they do not observe *that* patch with the same sensitivity as BICEP. Planck effectively gains sensitivity by measuring many BICEP-sized patches across the sky – but many of those are much more heavily contaminated by foregrounds.

Also, removing foregrounds from temperature maps is much easier in many ways than in polarization – the signal/foreground ratio is higher, for one. Polarization is a (pseudo)vector field, so there's more information there than in temperature maps, thus the temperature maps tend not to make very good templates, either.

21. **Kavanna**
April 7, 2014

Having seen some of these guys in the flesh at MIT's big evening public lecture last Thursday, I should briefly report on the event. Little mention of the "multiverse," BTW. The evening's MC, the MIT theorist Ed Farhi, specifically banned talk of multiverses until the question session after the last talk, which I couldn't stay for. Here's the flyer:

http://lsc.mit.edu/schedule/2014.2q/desc-physics.shtml

The speakers were Guth (inflation theory), Hughes (gravitational waves), Kovac (BICEP2 co-lead), and Tegmark (general implications). All the talks were excellent, especially Guth's. I had forgotten what a great speaker he is. He used no equations and barely any jargon beyond what undergraduate science students could understand. His only real moment of weirdness was talking about that funny inflationary equation of state, p = -rho, leading to gravitational repulsion.

Kovac's summary of the BICEP2 instrument, methods, and results was impressive and should silence most of the skeptics and critics. While some question remains open about the foreground corrections, the checks they put their experiment
through were exceptional. More to come with BICEP3, all part of the Keck program.

(This is one of those experiments where it's *foregrounds* you want to eliminate, not backgrounds.)

Tegmark gave a nice summary, including the retro-surprise of how the BICEP2 results, with the inflationary scale at 2*10^16 GeV, take us back to the original and simplest inflationary models of the early 1980s, based on GUTs. He showed in passing just how many inflation proposals are ruled out, including all known string versions. String-inspired inflation has a hard time getting the inflation/ Planck hierarchy to be 1/600 or whatever and getting "r" (tensor/scalar) to be larger than 10^(-4). Only string-inspired axion monodromy is larger, but at 10^(-3), still too small. While Tegmark didn't dwell on it, things look bad indeed for strings.

Given the quality of BICEP2's measurement, it's unlikely that they're simply wrong relative to Planck. The polarization limit inferred by the Planck team is an indirect upper limit, with some mild model dependency. For my money, I would bet that Planck's interpretation is just off. As a number of commenters here have mentioned, it's not an instrument designed for polarization measurements.

22. **Anon**
April 7, 2014

If Tegmark said that about string theory, he is incorrect.
He and others should be more careful in their claims.
Axion monodromy in string theory gives r in a range from order 10^{-2} to over .1. It does not give r of order 10^{-3}. Where do you get that?

There is on the other hand no direct connection to GUT physics, which has less evidence going for it than inflation at this point.

23. **JG**
April 7, 2014

David, that's a strange thing to say: *"Next set of cosmology data" does not necessarily imply results on all conceivable measurable parameters, does it?*

The statement is from Planck's notes for editors around the world who may not all be scientifically literate to understand "polarization map" (or be concerned with the details) – of course they meant that the polarization data was due for release "early 2014" – the interpretation of the statement is unambiguous.

Anyway I don't want to make a big deal about it, just wondered if the BICEP2 announcement came as a bit of a shock to the Planck team and caused the delay to publication of their results to October.

As for Tegmark claiming r=0.2 is bad for String Theory – well a bit strong perhaps (ask Eva Silverstein)) but we all know what the String people would be saying if BICEP2 had ruled out a high value of r – they would be no doubt be claiming another "prediction" of ST.

24. **Kavanna**
April 8, 2014

Tegmark didn't say it so strongly, just mentioning various theories in passing, that the simplest potentials, with the right hierarchy of scales, were favored and "others" were disfavored. He had a table up that I almost missed and didn't get a good look at.

The axion monodromy is the only string-based inflation model that comes close, at least that I'm aware of. The original model predicted "r" about 10^(-3), but as high as 10^(-2,-1) is possible from some more recent papers. It's stretch to get it to match the BICEP2 result.

Generally, the BICEP2 result implies a large field excursion ~few x Mplanck and vacuum energy scale ~10^(16) GeV. The latter scale is dead-on for GUTs.

25. **Kavanna**
April 8, 2014

Some more digging: watch Will Kinney's Perimeter talk from March:

http://pirsa.org/displayFlash.php?id=14030116

At about 57 min, he mentions the more recent "large r" axion monodromy, but points out that it's strongly disfavored by the combination of BICEP2 and other data. Other string-based models give much smaller "r" values, even less in agreement with BICEP2 and other data. Kinney stresses the critical empirical pressure that high "r" places on theories (and theorists). He likes the "shift symmetry" models, of which axion monodromy is one (which is why it comes close to working).

The really important thing for the next BICEP measurement is not agreement with Planck, but getting a broader range of multipoles.

26. **Cormac O'Raifeartaigh**
April 8, 2014

On a slightly different topic in your post, Peter:
The volume you mention on the philosophy of cosmology looks very interesting, lots of no-nonsense physicky papers from physicists like George Ellis. The volume is actually Issue 44 of the journal 'Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics' . This journal generally features contributions from philosophers and historians more than physicists, as far as I'm aware, but there's some serious reading in Issue 44, thanks for that…

27. **Anonyrat**
April 8, 2014

Regarding an old post on this blog about the film "The Principle"
http://www.math.columbia.edu/~woit/wordpress/?p=6624
we now have news, that many of the participants feel that they were duped into participating in it, including Star Trek's Kate Mulgrew.
http://thinkprogress.org/culture/2014/04/08/3424505/kate-mulgrew-duped/

> Tuesday morning, Dr. Lawrence M. Krauss of Arizona State University tweeted that he had not consented to participate in the film and strongly opposed its message: "For all who asked: Some clips of me apparently were mined for movie on geocentricism. So stupid does disservice to word nonsense. Ignore it."
>
> Two other experts used in the film told ThinkProgress that they were mislead into participating. Dr. Max Tegmark of the Massachusetts Institute of Technology explained, "I was told that this would be a science documentary by independent filmmakers who wanted to increase public appreciation for science. I should clearly have asked for more details in advance! These geocentric arguments are about as unscientific as things get."
>
> Dr. George Ellis of the University of Capetown in South Africa said that the film was "not presented" as geocentrism to him. "Obviously I don't agree. Not sue how anyone can hold that view these days. Must live in a timewarp. Like Lawrence, my advice is to just ignore. There is no point whatever in responding – it just gives the film recognition and publicity."
>
> Tuesday afternoon, Mulgrew posted on Facebook that she too was duped into participation:
>
>> MULGREW: I understand there has been some controversy about my participation in a documentary called THE PRINCIPLE. Let me assure everyone that I completely agree with the eminent physicist Lawrence Krauss, who was himself misrepresented in the film, and who has written a succinct rebuttal in SLATE. I am not a geocentrist, nor am I in any way a proponent of geocentrism. More importantly, I do not subscribe to anything Robert Sungenis has written regarding science and history and, had I known of his involvement, would most certainly have avoided this documentary. ….

28. **Anonyrat**
April 8, 2014

Lawrence Krauss on Slate.com:
http://www.slate.com/blogs/future_tense/2014/04/08/lawrence_krauss_on_ending_up_in_the_geocentricism_documentary_the_principle.html

> So, the question I had to face after discovering this abuse of my words was what to do about it. I have no recollection of being interviewed for such a film, and of course had I known of its premise I would have refused. So, either the producers used clips of me that were in the public domain, or they bought them from other production companies that I may have given some rights to distribute my interviews to, or they may have interviewed me under false pretenses, in which case I probably signed some release. I simply don't know.
>
> Many people have suggested I litigate. But this approach seems to me to be completely wrong because it would elevate the profile of something that shouldn't even rise to the level of popular discussion. The best thing we can all do when faced by nonsense like that, or equivalent silliness promoted by biblical fundamentalists who claim that science supports a literal interpretation of the Bible, is to ignore it in public forums, and not shine any light on the authors of this trash. As far as this particular film is concerned, one might hope that it has high production value that cost the producers a lot of money. Then, when no one beyond the three people in the country who may somehow have missed the last 500 years of science and history during their education watches the film, we can hope that the whole misbegotten enterprise will bankrupt the production company, or at least severely cramp its style.

29. **Will McKenzie**
April 9, 2014

"These ______ arguments are about as unscientific as things get." Devil's advocate but as idiotic as geocentrism is, still more scientific than MUH. Geocentrism has a clear hypothesis. It makes predictions. It's been tested innumerable times. It's been falsified innumerable times.

30. **Peter Woit**
April 9, 2014

Please, enough about geocentrism. This was all discussed ad nauseam here
http://www.math.columbia.edu/~woit/wordpress/?p=6624
back in January.
The one mystery here is that of how physicists ended up agreeing to be filmed. This is definitely the most impressive story of physicists being taken in by filmmakers since string theorist Clifford Johnson was induced to appear on a program devoted to "How Big do Boobs Have to be to Crush a Beer Can?", see
http://www.spike.com/video-clips/9z58hb/manswers-beer-crushing-boobs

31. **Nick M.**
April 16, 2014

In regards to the BICEP2 result, did anybody else catch the article published yesterday in New Scientist titled "Star dust casts doubt on recent big bang wave result"? The cited paper (see arXiv:1404.1899), upon which the article is based, was submitted to arXiv this past April 7th. It seems that the main cause for concern is the lack of inclusion, in BICEP's foreground analysis, of the possible contamination of the polarization background produced by this dust inspired signal, and that this signal would incorrectly be attributed to primordial gravitational waves from inflation; i.e., that the patch of sky used by the BICEP2's telescope isn't quite as clean as once thought. (See, for example, the "Conclusion" section of the above paper.) Anyway, the BICEP2 team has already admitted that the contribution by dust (a foreground component) to the polarization signal is one of the main caveats to their measurement. As always, time will tell.

32. **Nick M.**
April 16, 2014

As an addendum to my post above, I just noticed that "Physics World" did an April 10th article on this very same story as well, which included a lot more info; see "Have galactic 'radio loops' been mistaken for B-mode polarization?" The story was also talked about on the blog "In The Dark"; see "Galactic Loops as Sources of Polarized Emissions". Even though it is hard to see how these "loops" wouldn't have an impact on BICEP2's result, it would probably be best to just keep an open mind, and see how this all shakes out after Planck's release later this year.

Ten Years of Not Even Wrong

April 11, 2014
Categories: Favorite Old Posts, Uncategorized

This blog was started a little bit over ten years ago, and I've been intending for a while to write something marking the occasion and commenting on what has changed over the past ten years. I've found this mostly a rather discouraging topic to think about and whatever I have to say about it is going to be pretty repetitive for anyone who regularly reads this blog, so I'll keep this fairly short.

Re-reading some of the early postings I'm struck mainly by how little has changed in ten years. Back in March 2004 I was writing about a David Gross talk promoting string theory, about whether CMB measurements would give information about GUT scale physics, about how string cosmology seemed to be an empty subject, and about new twistor-based methods for computing gauge theory amplitudes. There's been a lot of progress on the last topic since then, but little change for the others.

One big change over the past ten years is that the argument that string-theory based unification is a failed project is no longer a particularly controversial one, with most physicists now leaning to this conclusion. Last night even Sheldon of The Big Bang Theory acknowledged that this isn't working out and he needs to find something else to work on (see here). Maybe even Sheldon's real life model will soon reach this conclusion. Ten years ago the argument one often heard was that string theory was the winner in the marketplace of ideas, with skeptics just sore losers. These days, it's string theorists who are more often complaining about the unfairness of this marketplace.

One development that is just starting to have a major impact is the failure of the LHC to find any evidence of SUSY, leading to increased skepticism about SUSY extensions of the standard model. This is a developing story, with results over the next couple years from the LHC likely to make this a textbook example of what scientists do in the face of experimental disconfirmation of their most cherished ideas.

The discovery of the Higgs has been a wonderful vindication of the ideas and techniques of high energy physics, both experimental and theoretical. As we learn more about the Higgs the lesson seems to be that this sector of the Standard Model behaves in the simplest way possible. This is a significant new piece of information about nature, although a frustrating one since it doesn't provide a hint of how to improve the Standard Model.

On the whole though, I fear that thinking about changes over the last ten years mostly puts me in a not very good mood. Some of the depressing developments and trends of the last ten years are:

- One reaction to string theory's failures in the marketplace of ideas has been a Russian billionaire's decision to try and manipulate that marketplace by injecting tens of millions of dollars into it on one side. The largest financial prize in science now is devoted to each year rewarding people for work on a failed project. This is corrupting the marketplace in a significant way.
- Some of my earliest postings back in 2004 were about KKLT, the string landscape and the multiverse. At the time I was sure that if the landscape proposal being pushed by the Stanford group became widely accepted as an implication of string theory unification, that would be the end of it. Surely no sensible person would try and argue for an extremely complicated, inherently unpredictive theoretical framework. Boy, was I wrong. As I've gone on about far too often here, the current multiverse mania is a disastrous and shameful episode for fundamental theoretical physics, threatening its essential nature as a science.
- Most physics departments have reacted to the failure of string theory by at least partly blaming this failure on the over-emphasis of mathematics, instead of the fact that this was just a wrong idea about physics. An interesting document I recently ran across is this one about the connections of particle physics with other disciplines, written by my advisor Curtis Callan and Shamit Kachru. Mathematics is mentioned in a section discussing past successes in cross-fertilization with other fields, but it appears not at all in the rest of the document discussing opportunities for the future.

I'm quite surprised that I've continued to find topics worth blogging about ten years down the road, this is something I never expected when this started. Right now I'm hoping for something unexpected in coming years, that I'll be writing about something different and much more interesting ten years from now!

Comments

1. **Phoenix Woman**
   April 11, 2014

   Oh no! Hermione Granger doesn't actually exist!

   http://www.aps.org/publications/apsnews/200808/zero-gravity.cfm

2. **Michael Gogins**
   April 11, 2014

   Thank you for maintaining your blog for these past 10 years, most of which time I have been looking at it just about every day.

   I've learned at lot, but the main thing I've learned is the unsettling news that, when science gets stuck, or harder, a lot of scientists kind of give up and start doing metaphysics. Unfortunately they don't seem to want to admit that's what they're doing.

   I have nothing at all against metaphysics, quite the contrary in fact, but I would like to be able to keep my science and my philosophy separated by the criterion of empirical testing.

   I would be very interested in any suggestions about how young scientists might better be educated to avoid this trap without losing their curiosity and drive to understand.

   Regards,
   Mike Gogins

3. **Ralph**
   April 11, 2014

   The attitude that the failings of string theory are due to "too much mathematics" is just insulting to mathematicians.

   It may be that string theory was not good physics. That does not mean that the bits of string theory that pretended to be good physics, were good mathematics.

   Lets be clear : to a large extent, it is valid to see the problem of quantum gravity to be a mathematical problem.

   Giving a single theory, with a reasonable degree of mathematical rigour, that reduces to the SM and GR in appropriate limits, would be a huge advance, whether or not that theory actually turned to describe our universe.

From that point of view, testability of a theory-of-everything is actually a red-herring – the experiments have already been done: the ones that confirm GR and the ones that confirm the SM. The problem is to find any respectably rigorous ToE that gives correct calculations of the outcomes of all those experiments. Currently it appears to be mathematical rigour that is lacking.

Of course, once we have a ToE that makes mathematical sense, the questions will be "how do we test it" and "how do we experimentally distinguish it from alternatives". I am not claiming that experimental testability is unnecessary, it is just that we appear to be as far from having a real theory to test, as we are from actually probing the planck scale directly.

4. **Roger**
April 11, 2014

I was hoping for dialog like this.

Penny: What is string theory?
Sheldon: That is the idea that all mysteries can be explained by saying that everything is a string in 6 extra dimensions.
Penny: I could have told you that is a dumb idea.

5. **Bryan**
April 11, 2014

Peter, congrats on a remarkable 10 years of blogging! You've been not just the main source documenting ST irrationality, but also this blog has become a great site for all particle physics news.

6. **Richard Gaylord**
April 11, 2014

" Sheldon's real life model ". ho would that be?

7. **FCS**
April 11, 2014

Keep up the great work! I certainly enjoy reading you blog each week.

8. **JG**
April 11, 2014

Yes, thanks for the blog, it's always informative and I think you've been quite brave.

Coincidentally, we are at a quite significant moment – as many string theorists are *anxiously* awaiting to see if there will be corroboration of the BICEP2 result. Mysteriously there has not been much comment that the result r~O(0.1) *destroys* nearly all string inspired cosmology models. Yeah yeah – string gas cosmology and axion monodromy are suddenly the new stars – but there were bound to be some string inspired ideas that still survive, since the theory "predicts" almost everything!

9. **Academus**
April 11, 2014

Great past decade! I'm certain you'll be writing about interesting new physics soon…

10. **Anonyrat**
April 11, 2014

Congrats on the 10 years of blogging, and just as they yell at the political conventions "four more years, four more years", with the same spirit I write "ten more years, ten more years"!

11. **Martibal**
April 11, 2014

Peter,

"There's been a lot of progress on the last topic since then, but little change for the others."

Aren't you a bit pessimistic ? On the second topic (CMB and GUT) it seems that something rather big may have just happened. So: progress on twistor, discovery of the Higgs, and maybe indication that inflation occurs at GUT scale… this is not so bad for ten years in theoretical physics, I would say.

12. **Peter Woit**
April 11, 2014

Martibal,
Discovery of the Higgs is a great experimental triumph, unfortunately not a recent advance in theoretical physics. I don't yet see that CMB results tell us anything signficant about GUT scale physics, maybe some day, but not yet. Amplitudes are about it…

13. **hopffiber**
April 11, 2014

Why no mention of BICEP2 results here? I would argue that that changes a bit of the first points you mentions. As JG rightly points out, if the results are corroborated, it falsifies a lot of cosmological models, among them a lot of string based ones. So then clearly the assumption that string cosmology is "empty" is just wrong: the string models are just as falsifiable as the "normal" ones. And the idea that CMB observations can tell us something about quantum gravity is looking promising as well. For one thing, it gives us the energy scale of inflation, that alone is some very interesting information about GUT/BSM physics.

14. **JohnB**
April 11, 2014

Well done on the 10 years + book 🙂 Have been following the blog since the book and, even as someone with very little physics knowledge (but much interest in the general topic), this is one of the most consistently good blogs on any topic I read about.

15. **Neil**
April 11, 2014

I'd be interested to know how many real world Sheldons there are–bright young physicists wondering if they have spent their most creative and productive years on what looks like a dead end?

16. **Low Math, Meekly Interacting**
April 11, 2014

What, no mention of all the excellent rumor-mongering?

17. **Peter Woit**
April 11, 2014

hopffiber,
Yes, lots of "string cosmology models" may have been falsified, but what has not been falsified is the claim that you can get just about anything at all out of some complicated "string cosmology model", so "string cosmology" isn't science in the usual sense.

As for the CMB telling us something significant about quantum gravity and about GUT physics, that's not inconceivable but I don't think we're there yet, whatever you read in the papers…

18. **Nathalie**
April 12, 2014

We have to keep in mind that the BICEP2 results may very well turn out to be "wrong". They have no doubt measured something and have found it to be equal to 0.2 but what is it that thing? Background noise? Magnetized dust from supernovae? Following good academic traditions we have to wait before shouting hurrah.

The most striking message from LHC is that the naturalness arguments presented so far are not valid. Nature seems to have a different line of reasoning. This reminds one of the naturalness arguments for why the neutrino mass had to be zero. After all it was advocated by no less than four Nobel Laureates. Nonetheless it turned out to be wrong.

19. **Stuart**
April 12, 2014

Peter
Thank you for defending the scientific method for the past ten years!
Yes we now recognize that String theory has conceptual problems. My question is can the same be said about Loop Quantum Gravity?

20. **Jesper**
April 12, 2014

First: congratulations. I think your post plays an important role. I have always had this conviction that you can only really beat an idea by providing a better one. I think ST will eventually fade away, but it might not happen until someone gives a strong alternative. The problem is perhaps that the present setup of theoretical physics might be somewhat opaque to new ideas.

About this "over-emphasis of mathematics" on the ST side. My own experience is the complete opposite, that is, a lack of emphasis on mathematics. A couple of years ago I gave a talk with a couple of very prominent ST physicists in the audience. After my first slide I was asked – again, by a very prominent ST-guy – what a C^*-algebra is…

@ Stuart: yes, the same can be said about LQG, but of course, the problems there are of a different nature (and most likely OT).

21. **Shantanu**
April 12, 2014

Peter, there has been a lot of progress in experimental neutrino physics in last 10 years including evidence for non-0 theta_13. However its still not clear to me what exactly non-0 neutrino mass and mixing tells us about rest of particle physics and
beyond standard model theories.

22. **Bee**
April 12, 2014

Who'd have thought 10 years ago that blogs would still exist today? Your blog is one of the few that I follow, so I am looking forward to the next decades 🙂

23. **johnnythelowery**
April 12, 2014

Got the book, read the blog; not got the T-shirt yet. Congrats Peter. A lot of hard but necessary work. Anthropologists in the far future will find blogs like this a gold mine. Your predictions for the next 10 years please.

24. **Art**
April 12, 2014

Congrats on passing the 10 yr blogging milestone! Your book was an eye-opener for this layperson, and your blog is a v nice portal into today's math and physics happenings. I hope you continue…

25. **CThomas**
April 12, 2014

This seems like an appropriate entry to ask the question whether there is any sort of posting, article, or book summarizing these disputes over string theory, falsifiability, etc. appropriate for the layman. Any leads would be appreciated. (I hope I'm not overlooking an obvious answer to this posted on the site, but I didn't see it.)

CThomas

26. **hopffiber**

April 12, 2014

@Woit: sure, that claim of yours is true to some extent, but how is it different from “normal” model building, in cosmology and elsewhere? You can also get almost anything from complicated constructions using just ordinary QFT (or LQG, I suppose). String theory does add some more possibilities since its more complicated, but I don’t see any real reason besides pure anti-string bias to single out “string cosmology” as particularly empty compared to other approaches. And as long as you can test your models against new data and falsify them, isn’t precisely science in the usual sense, or how do you define “usual science”?

And as for the second part, yes, we are not there yet. But it is quite promising and could very well give us quite a bit of interesting information about the scale of GUT and inflation. So I think stating that “very little happened in 10 years” is way to pessimistic, overall.

27. **Peter Woit**
April 12, 2014

CThomas,

I’ve written a book about this (also called Not Even Wrong), and if you read that and one of the books from a string theorist (Brian Greene’s The Elegant Universe would be a good choice), you’d get both sides. Both of these books are a bit old now, but, not much has changed. Also, on this side there’s a FAQ

http://www.math.columbia.edu/~woit/wordpress/?page_id=4338

hopffiber,
http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=isnt-string-theory-just-as-predictive-as-quantum-field-theory

28. **Don**
April 12, 2014

Dear Peter

Congrats on 10 years of doing exactly what science is supposed to be: open honest debate about the current issues. Whether you are correct or not really doesn’t even matter. It is that you have provided an outstanding example of the process of refining ideas. In addition to this, your lively and slightly cynical style makes for great reading, entertainment, and the occasional “OMG, I don’t believe he said that out loud!” Political correctness is the bane of peer review, and you set a great example of how to do it the right way. Not with trivialities, but by going right to the heart of the matters and focusing on core questions.

Thanks a million and I for one look forward to many more years of your posts.

Best wishes to you, Sir,

Don

29. **Jason**
April 12, 2014

There’s simply nothing wrong with string theory. The vast majority of string theorists are mediocre and not up to the task. A tiny minority of string theorists actually do something useful and every five to ten years make significant progress. This is a ‘Theory of Everything’, after all and the only one that really works but it cannot be expected to resolve itself in the same manner as other scientific theories in the past. String theory is its own game and it has produced more losers than winners. Many were called but few were chosen. What did they expect anyway? Doesn’t nature operate this way? It’s time for the Sheldons of string theory to move on and make room for the new guard. Believe wholeheartedly in the power of young people. Somewhere, some young person is going to take the next step, bring us to a better level of understanding physics and string theory than ever before. I’m their biggest fan already.

30. **Jim Akerlund**
April 13, 2014

Got your book on 4/5/14 and finished it today. I do have a question about it. In the chapter “The only game in town” you mention the average age of tenured professors in 1970 as under 40, and that the latest figures (circa 2000 according to footnote 12) were for an average of nearly 60 for tenured professors. My question is, has that changed any since your book was published? Anyway, congradulations on ten years of good blogging.

31. **Peter Woit**
April 13, 2014

Thanks to all for the kind words and encouragement.

Jim Akerlund,
I haven’t seen more recent numbers (but haven’t looked), would be interested if anyone has any. I’d guess the average age number has stabilized (or even started to go down), as the cohort hired in the late 1960s has started to retire post-2000. Retirements by themselves should bring the number down, retirement + replacement by new hire even more so.

Jason,
That the “vast majority of string theorists” are just too stupid is an original explanation for the problems of string theory. I hadn’t heard that one before and I thought I had heard them all.

32. **Jeff M**
April 13, 2014

Jesper,

Really, a physicist didn’t know what a C* algebra is? Really? I mean I’m a mathematician, but I’m pretty sure I first learned about C* algebras when I took QM (a VERY long time ago 🙂

Did you ask him if he knew what a Hilbert space is?

33. **Jeff M**
April 13, 2014

Sorry, I assumed it was a him, my bad, there are some prominent female string theorists…

34. **Eddie Dealtry**
April 13, 2014

Peter,

Thanks for the ten years and keep up your good work.

For some of us this is the site that knocks some sense into news items and weeding out new books on physics – saving us time and disappointment with the hype.

Eddie

35. **Peter Woit**
April 13, 2014

Jeff M/Jesper,

I don't think there's anything at all remarkable about not knowing what a C* algebra is, wouldn't surprise me it most mathematicians don't know this either. You can understand QM quite deeply without the operator algebra/functional analysis perspective, and you can understand the statements relating QM and C* algebras without understanding anything significant about QM at all. So many different sorts of mathematical technologies have been tried out in string theory that surely no one understands them all.

Physicists rarely learn mathematics from the ground up, from general theory, more often from how mathematics gets used in a specific example they work with. This isn't at all a bad thing.

36. **QG**
April 13, 2014

may jason is being sarcastic. though witten et al did say to paraphrase string theory is smarter than us.

maybe string theory is the correct description of nature but humans are not intelligent enough to understand it.

37. **Peter Woit**
April 13, 2014

QG,
For any scientific idea that doesn't work out, you can try and argue that it really is fixable and will work, but we're too stupid to know how. While this could always be true, there's a good reason this is not the conventional scientific method.

38. **A**
April 13, 2014

CThomas: You might be interested in http://gilkalai.wordpress.com/2009/06/23/my-book-gina-says-adventures-in-the-blogsphere-string-war/

39. **Jeff M**
April 13, 2014

Peter,

Actually I would be much more surprised by physicists not knowing C* algebras than mathematicians, I only know about them really because I was originally a physics major. In math you would only know much about them if you work in some related field, though probably you would have seen them around. I just can't believe anyone took a real QM course where they weren't mentioned.

As for how physicists learn math, I think you're right but I do see issues with that, since it makes it much easier for them to ignore the required rigor and claim they've "proved" something when they haven't even come close. Whats the joke, "If you understand something and can prove it, publish it in a math journal, if you understand something but can't prove it publish it in a physics journal…"

40. **Goosebumps**
April 13, 2014

Hi Peter,

Congratulations on ten years of blogging! I recently came across your blog and I always find your posts entertaining. I hope to read your book someday.

I'm sorry if this is off-topic, but I thought I'd ask an expert – does the recent discovery of exotic hadrons at CERN have significant implications for HEP? I thought the reaction seemed a bit muted. Is it fully consistent with the Standard Model?

41. **Peter Woit**
April 13, 2014

Goosebumps,
Thanks!
The existence of these exotic hadrons is as far as we know consistent with QCD and the Standard Model. One of the big problems in theoretical physics is that there's a lot we don't understand about QCD, because of the lack of good calculational methods. So, we don't know exactly what the theory predicts for such states. Better understanding QCD is a problem that deserves more attention than it gets, but it's extremely difficult, and people have been working at it for about 40 years with only limited successes.

42. **Bernard Grossman**
April 13, 2014

Congratulation, Peter . I have learned alot from following your blog. But I do think there are many reasons to rejoice about progress !
I am surprised that you do not include the ADCFT correspondence and its applications in solid state, quark-gluon plasma and quantum entanglement as a remarkable area of progress . The holographic principle will most likely lead to new understanding of quantum mechanics at it deepest level. While field theorists have been accustomed to remarkable successes from the days of quantum electrodynamics, most deep problems are understood over longer periods of time with many failures. The theory of evolution is almost universally accepted,however there are in fact many ways in which its initial formulation is flawed and our understanding continues to evolve.

Evolution needs to be defended against the no-nothings who attack science, it also needs to be criticized and to be developed with our increasing understanding of ideas like epigenetics, which looks closer to the ideas of Lamarck than Darwin.
We do not know as yet whether string theory will survive as a fundamental theory, but of it does not, many of the ideas will surely survive in the transition to the next stage .

43. **TomD**
April 14, 2014

@JeffM,
I took a graduate quantum mechanics course from Ed Witten, and C^* algebras weren't mentioned…

@Peter,
Congratulations, and many thanks for the great blogging for 10 years… I read it avidly, and share lots of the posts with colleagues who are 'former QFT'ers.'

44. **Simple biologist**
April 14, 2014

@Bernard Grossman
I find the comparison to evolution to be flawed. We biologist have 150 years worth of both historic and experimental data to use for developing the evolutionary theory. If anything,at the moment biologist have exactly opposite problem, thanks to genomics/proteonomics/whatever-omics. So much emperical data and not enough tools to make proper use of it.

Things are quite different for string theorist in this regard. Apples, oranges and so on.

45. **Jesper**
April 14, 2014

About the C*-algebra stuff: perhaps I should elaborate a little. The ST-guy, who asked me what a C*-algebra is, was in the early 2000's involved in the "noncommutative geometry-meets-ST" surge. Thats actually the reason that it surprised me so much that he apparently wasn't aware of the concept of a C*-algebra. After all, he had been publishing numerous papers in a field very closely and somewhat overlapping the work of Connes (and without any doubt, had been attending conferences where Connes or someone else from NCG was speaking). Thus, for him not to know what a C*-algebra is meant to me that he hadn't bothered looking at the math so closely related to what he was doing. Since he is a leading figure in the field ST I sort of took it as an indicator.

This is just to comment on the mentioned 'over-emphasis on math' and for me, its just one illustration of something, which I have noticed over the years: that the ST people, whom I have been in contact with, certainly are not putting a lot of emphasis on math (I mean math, not computation). This may not be a bad thing (I tend to think it is) but the issue was simply the emphasis/not-emphasis.

46. **Bernard Grossman**
April 14, 2014

While the analogy between evolution and string theory is not perfect, I would say there are lessons of hope and inspiration to be learned from the comparison . The theory of evolution as a theory strictly of genetics is flawed, but survives as modified by new developments and genetics is firmly based in our thinking about biology. We do not know whether string theory is flawed or just incomplete , but our understanding of the importance of spin two field theory, gravity and the renormalization group for many areas of physics has become deeper compared to the ideas before string theory of quantum gravity as a hopelessly nonrenormalizable theory .

47. **Fredf**
April 14, 2014

Thanks for a regular dose of thoughtful blogging.

48. **JG**
April 14, 2014

Jeff M

Just like in the early days, when most physicists learnt QM from Dirac's textbook, not Von Neumann's (which wasn't even translated into english until the 1955), modern physicists are not always concerned with mathematical rigour. Any course in QM which attempts to discuss it in full mathematical rigour is pretty tedious to most tastes, and you must admit that not many breakthroughs in physics have been made by the use of mathematical rigour. It annoys me, for example, when people criticise Einstein's quite messy route to GR compared to Hilbert's apparently pristine derivation – yeah AFTER he'd got the crucial ingredients from Einstein! Or people who praise so highly Born and Jordan's rigorous rewrite of Heisenberg's crucial insights (he had not even heard of matrices at the time) – as if the mathematically rigourous formulation was the great achievement here (Dirac independently derived most of the same stuff in a few weeks, also AFTER reading about Heisenberg's crucial insight)

btw for a discussion of Dirac v Von Neumann approach see here

49. **Peter Woit**
April 14, 2014

All,
In general I'm all in favor of discussion of math and QM, but it is pretty much off-topic here, best would be to postpone this until it is on-topic. This should happen sooner or later, since I'm still working on the last chapters of the book I'm writing on this and when I finish a first draft I'll write about this here. This fall semester I'll again be teaching a course on this topic, so also likely will want to blog about this topic then.

50. **Supernaut**
April 14, 2014

Peter – Thank you for writing this blog for these ten years! Hope to read what the next ten years of NEW will bring.

51. **AcademicLurker**
April 14, 2014

I've found your blog to be very entertaining and informative, although I work in a very different area of physics and so don't appreciate many of the technical aspects of the discussions. The sociological aspects, on the other hand, have been consistently interesting.

It seems like an odd coincidence that the 10 year anniversary of *Not Even Wrong* coincided so closely with Sheldon Cooper giving up on string theory…

52. **Peter Donnelly**
April 14, 2014

Can’t tell you how much I’ve enjoyed your blog, which I’ve been tracking ever since I read your book (in old fashioned paper format.)

I chexk every day for updates, always disappointing when there isn’t one, but I guess you have a day job also :>)

Peter Donnelly

53. **Argosy**
April 15, 2014

Congratulation on 10 years of your blog, I’ve been an avid reader for the past few years, though this is my first comment. Keep up the good work!

54. **Curious Wavefunction**
April 15, 2014

Congratulations. Your blog and book have served an essential purpose and have upheld the Royal Society’s ‘Nullius in verba’ motto better than almost any other source I know. Keep up the good work.

55. **Brian Dennehy**
April 15, 2014

Just to say thanks for maintaining the blog Peter. I find it to be one of the very best sources of information for what is going in theoretical physics.

I have also always been impressed by the dignity and calmness of your writings over the years . There is a lot of `sociology’ in the theoretical physics research community and it cannot have been easy to stand up so prominently to criticise the string theory program.

In the coming years and decades I’m fascinated to see what is made of string theory as an intellectual endeavour.

56. **srp**
April 17, 2014

Can one be a dissenter without being a crackpot, a spokesman for the “silent majority” without being a commissar against the “vocal minority,” and an advocate for more rigorous mathematical foundations in physics without signing onto the most popular mathematical physics fashions? I don’t know, but you’ve making a heck of an attempt for ten years and it’s been very interesting to watch.

57. **Kavanna**
April 18, 2014

Peter,

I’ve immensely enjoyed both the book and the blog and recommend them whenever the occasion arises. The book has been read — twice, or even more times ….

Keep up the good work. Opposing the multiverse mania has been an exceptionally crucial and brave thing that you’ve done. Some days, I want to start screaming when I see some multiversal nonsense on the Web, or the new Cosmos, or wherever. Then I come here and calm down.

58. **Michael Gannotti**
April 21, 2014

I finally started watching watching the series Cosmos and 15 minutes in when I heard deGrasse Tyson mention the multiverse in the same breath as verifiable science I could not help but think of this post. Thanks for continuing to shed light on the difference between science and what is ostensible philosophy and faith dressed up and paraded as science.

59. **Edward Hessler**
April 22, 2014

As I think you know, Dr. Woit, I am not a physicist (K-12 educator) but I hope better late is better than never. This is one of my favorite blogs and I check it nearly daily. I don’t understand everything, obviously but you and commenters have helped me think about physics.mathematics more carefully as well as with (I think–so long as there isn’t an assessment of this!) greater insight.

What a service and I’m glad you do it and enjoy it.

First, Congratulations.

60. **Peter Mancini**
April 22, 2014

Love the book. Recently got it and have enjoyed reading it. The history, your central argument and your writing style are all very thought provoking and interesting. I picked it up mainly to understand how to argue against prevailing notions and I believe the book has been instructive. My field isn’t physics but the part of information science dealing with text analytics. I was a physics major as an undergrad but decided after 2 years I was more interested in computers. I’m glad people like you have stuck with it and produced such interesting results.

I enjoy your blog and I hope that new breakthroughs in physics give you good reason to unconcerned with bad theories and instead can refocus on the majesty of exploring good science.

61. **Bernhard**
April 26, 2014

I’m coming late as a was traveling, but could not miss the opportunity to congratulate you and your fantastic blog. Besides the healthy critic you always provide, this blog is still the best source of HEP news on the blogosphere. I also enjoy very much the math news which I would not have known otherwise, even though I’m generally incapable to appreciate it fully.

I second Kavanna’s comment. I, and believe many, feel the same way.

Ten more years (at least) to NEW!

62. **Jim Beyer**
April 30, 2014

Bravo!

This blog has done more to keep the pressure on String theorists to explain their (non) results than any other entity. You remind them (and us) on almost a day-to-day basis that their excuses-providing double-talk is merely cleverly worded nonsense to keep the grants rolling in.

Physics has in effect entered a mini dark age that hopefully from which it will hopefully eventually emerge. This blog is proof that at least someone kept the light of rationality on during that time.

Thanks again. Your effort in this endeavor will hopefully be recognized and appreciated by a wider audience than it is now.

Supersymmetry and the Crisis in Physics

April 15, 2014
Categories: Uncategorized

The May issue of Scientific American has a very good cover story by Joe Lykken and Maria Spiropulu, entitled Supersymmetry and the Crisis in Physics (the article is now behind their subscriber paywall, but for those with access to Nature, it will soon be here).

Here are some excerpts:

> It is not an exaggeration to say that most of the world's particle physicists believe that supersymmetry *must* be true—the theory is that compelling. These physicists' long-term hope has been that the LHC would finally discover these superpartners, providing hard evidence that supersymmetry is a real description of the universe…
>
> Indeed, results from the first run of the LHC have ruled out almost all the best-studied versions of supersymmetry. The negative results are beginning to produce if not a full-blown crisis in particle physics, then at least a widespread panic. The LHC will be starting its next run in early 2015, at the highest energies it was designed for, allowing researchers at the ATLAS and CMS experiments to uncover (or rule out) even more massive superpartners. If at the end of that run nothing new shows up, fundamental physics will face a crossroads: either abandon the work of a generation for want of evidence that nature plays by our rules, or press on and hope that an even larger collider will someday, somewhere, find evidence that we were right all along…
>
> During a talk at the Kavli Institute for Theoretical Physics at the University of California, Santa Barbara, Nima Arkani-Hamed, a physicist at the Institute for Advanced Study in Princeton, N.J., paced to and fro in front of the blackboard, addressing a packed room about the future of supersymmetry. What if supersymmetry is not found at the LHC, he asked, before answering his own question: then we will make new supersymmetry models that put the superpartners just beyond the reach of the experiments. But wouldn't that mean that we would be changing our story? That's okay; theorists don't need to be consistent—only their theories do.
>
> This unshakable fidelity to supersymmetry is widely shared. Particle theorists do admit, however, that the idea of natural supersymmetry is already in trouble and is headed for the dustbin of history unless superpartners are discovered soon…

The authors go on to describe possible responses to this crisis. One is the multiverse, which they contrast to supersymmetry as not providing an answer to why the SM parameters are what they are, although this isn't something that supersymmetry ever was able to do. Another is large extra dimensions as in Randall-Sundrum, but that's also something the LHC is not finding, with few ever thinking it would. Finally there's the "dimensional transmutation" idea about the Higgs, which I wrote about here last year. About this, the authors write:

> If this approach is to keep the useful virtual particle effects while avoiding the disastrous ones—a role otherwise played by supersymmetry—we will have to abandon popular speculations about how the laws of physics may become unified at superhigh energies. It also makes the long-sought connection between quantum mechanics and general relativity even more mysterious. Yet the approach has other advantages. Such models can generate mass for dark matter particles. They also predict that dark matter interacts with ordinary matter via a force mediated by the Higgs boson. This dramatic prediction will be tested over the next few years both at the LHC and in underground dark matter detection experiments.

It's great to see such a high-profile public discussion of the implications of the collapse of the paradigm long-dominant in some circles which sees SUSY extensions of the Standard Model as the way forward for the field. One place where I disagree with Lykken and Spiropulu is their claim that "It is not an exaggeration to say that most of the world's particle physicists believe that supersymmetry must be true." Actually I think that *is* an exaggeration, with a large group of theorists always skeptical about SUSY models. For some evidence of this, take a look at this document from 2000, which shows a majority skeptical about SUSY at the LHC. By the way, I hear those on the right side of that bet haven't yet gotten their cognac, with the bet renegotiated to wait for results from the next LHC run.

Update: I hear that the 2000 bet was revised in 2011, with a copy displayed publicly at the Niels Bohr Institute. The new bet is about whether a superpartner will be found by June 16, 2016, and the losers must come up with a bottle of good cognac. There are 22 on the yes side (including Arkani-Hamed and Quigg), and 22 on the no side (including 't Hooft, Komargodski, Bern). Also, 3 abstentions. It explicitly is an addendum to the 2000 wager, with those who lost the last one given the option of signing again, forfeiting two bottles of cognac, or accepting that "they have suffered ignominious defeat."

Update: This report from the APS spring meeting includes the following about Spiropulu's talk there:

> Supersymmetry and dark matter have become so important to particle physicists that "we have cornered ourselves experimentally," said Spiropulu. If neither is detected in the next few years, radical new ideas will be required. Spiropulu compared the situation to the era before 1905, when the concept of ether as the medium for all electromagnetic waves could not be verified.

You can watch the talk and see for yourself here.

Comments

1. **tt**
   April 15, 2014

   "renegotiating a bet" ??? where did they grow up ? i hope they have to pay with more cognac.

2. **Richard Séguin**
   April 15, 2014

   The same issue of Scientific American also has a nice article on Ken Ono (et al) and Ramanujan.

3. **Bee**
   April 16, 2014

   "Actually I think that is an exaggeration, with a large group of theorists always skeptical about SUSY models."

   There's a name for it: False-consensus effect (look it up on wikipedia). Really, once you read through a list of common cognitive biases, it's hard not to notice just how widespread they are in science, and the present susy discussion is a particularly extreme example with people trying to hang onto their hopes.

4. **Will Nelson**
April 16, 2014

Surely the models such as Lykken is describing would be at least as malleable as susy when it comes to pushing predictions out of range of existing experiments. But it would certainly be interesting to see the second of the 1990-era naturalness dogmas overturned just as lamba=0 has been…

5. **Shantanu**
April 16, 2014

Peter does this article mention the Shaposhnikov-Wetterich model on asymptotic
safety? I bet it does not.

6. **Peter Woit**
April 16, 2014

Shantanu,
The article is mostly about not finding SUSY, no details on alternatives. I quoted most of the substantive part about "dimensional transmutation", the only specific reference was "Building on seminal work by William A. Bardeen…". There was also reference to the fact that the Higgs mass is at the edge of the metastability region, but about the significance of that, just "Nature is trying to tell us something, but we don't know what."

7. **Mike**
April 16, 2014

" . . . no details on alternatives."

Peter, what alternatives do think deserve more attention? Thanks.

8. **Peter Woit**
April 16, 2014

Mike,
The problem with SUSY models has always been that they don't really solve any of the problems of the SM. So, there's no need to look for alternatives to SUSY in that sense. In the more general sense of ideas about how to improve the SM, given the huge amount of attention paid to SUSY, and the experimental disconfirmation, there's a good argument that attention would better be paid to almost anything else. One thing I don't want to do though is host and moderate here a discussion of everyone's favorite ideas on this.

9. **AS**
April 16, 2014

Nature might be trying to tell us this message: I HAVE NO MASS SCALE.

In such a case, power divergences must vanish and the naturalness problem changes. The Planck scale and the weak scale and the Dark Matter scale can all be generated dynamically.

10. **vmarko**
April 16, 2014

AS,

"Nature might be trying to tell us this message: I HAVE NO MASS SCALE."

While this is slightly off-topic, I'd have to disagree. Nature does not appear to be conformally invariant, and you cannot get nonzero mass scales out of nothing. Dimensional transmutation from conformal anomaly is a mirage, it doesn't really generate a dimensionful quantity out of dimensionless ones (when you think about it, it is mathematically impossible to do such a thing). So at least one mass scale has to exist in nature, say Planck scale, and the ratio of Higgs mass to Planck mass then turns out to be extremely small. The "hierarchy problem" is the fact that physicists find the smallness of that ratio psychologically unsettling, and try to look for an explanation.

HTH, 🙂
Marko

11. **A.J.**
April 16, 2014

"renegotiating a bet" ??? where did they grow up ?

Perhaps the no-SUSY side decided that they wanted to win on the merits rather than on a technicality?

12. **Peter Woit**
April 16, 2014

vmarko,
Pure QCD is an example where you do get non-trivial dimensionful quantities out of dimensionless ones, because of the way renormalization works. As for the significance of the Planck scale, I don't think one should assume anything about the relation of quantum gravity to the Higgs.

Another way to say this might be that the physics community has been obsessing about supersymmetry, while knowing that SUSY breaking is required to make it work and we don't have a good idea about SUSY breaking. Instead of this, it might be a good idea to spend time obsessing about conformal symmetry, while knowing that conformal symmetry breaking is required to make it work, and we don't (yet) have a good enough idea about conformal symmetry breaking.

13. **Neil**
April 16, 2014

Peter,

You once wrote an eloquent answer to an Edge question about eloquent explanations, explaining why you think symmetry is an eloquent and beautiful explanation. I am assuming what you said there is one reason why people think SUSY is compelling regardless of the results so far at LHC.

My question. Do you have a reason to believe that SUSY is less eloquent and compelling than other symmetries in physics, or is your concern solely on the lack of evidence?

14. **Peter Woit**
April 16, 2014

Neil,
The issue of supersymmetry in general is a complicated one. As a general idea, supersymmetry does have some very compelling features. One of the simplest examples of the general phenomenon is that the Klein-Gordon operator has a square root, the Dirac operator, and in some sense the Dirac operator is a generator of a supersymmetry. This is quite fundamental and one of the most beautiful and compelling ideas in the subject. The details of this are in the notes of my quantum mechanics course.

What is being tested though at the LHC is a much more complicated example of a supersymmetry, the idea that you can start with the SM, and extend it to get a SUSY theory with a super-Poincare group invariance. This is what doesn't work so well: you get theories that are much more complicated than the SM, with a new symmetry that acts trivially on the SM, telling you nothing new about it. It leads to a complicated ugly theory since you have to break the supersymmetry, and the LHC results bear out the expectation that this isn't the way to go.

I wouldn't be at all surprised if there is some deeper insight into the SM that will come from identifying some sort of "supersymmetry" of it, one that is more like the supersymmetry generated by the Dirac operator, not necessarily coming from the super-Poincare group used in SUSY extensions of the SM.

So, I think it's a promising idea that there may be an eloquent "SUSY" explanation of fundamental physics, I just don't think the version we have now qualifies, and the LHC is confirming that.

15. **vmarko**
April 16, 2014

Peter,

"Pure QCD is an example where you do get non-trivial dimensionful quantities out of dimensionless ones"

No, it isn't. When you solve the renormalization group equation for the running of the strong coupling, it becomes equal to one at a certain energy, called the "QCD scale". What people often miss to notice here is that this equation also features an arbitrary dimensionful integration constant, which needs to be experimentally determined. IOW, the whole thing is a cheap trick, since it expresses the QCD scale in terms of the "renormalization scale" at which you measure the initial value for the strong coupling constant (before you let it run up to unity). The key point here are the words "experimentally determined", because in experiment you cannot shut down quark masses. They invariably enter the process of measurement through the back door, and ultimately participate in fixing the QCD scale.

It is also easy to see this without any physics. Can you give me an example of an equation which has "1 kg" on the left-hand side, and the right-hand side built purely out of dimensionless quantities? No? Then it cannot be done in QCD either, nor in any other theory. Such an equation is not mathematically self-consistent, on "dimensional grounds", regardless of any physics.

I am actually surprised by the number of people (including some Standard Model experts!) who don't get this stuff right. It is usually explained in standard graduate courses on QCD and nonabelian gauge theories.

Best, 🙂
Marko

16. **Peter Woit**
April 16, 2014

vmarko,
I'm talking about pure QCD, no fermions, i.e. SU(3) pure-gauge theory. All parameters in the Lagrangian are dimensionless, but it has a very non-trivial spectrum of dimensionful numbers, with the scale set by what you call a "cheap trick", and I call a deep insight into nature…

17. **vmarko**
April 16, 2014

Peter,

I understood what you meant by pure QCD. But the RG equation is a first-order differential equation, and doesn't specify a single curve for the running of the coupling constant, but a family of curves. The initial condition which singles out one curve out of this family, i.e. the value of the coupling at a certain renormalization scale, cannot be predicted from the theory (not even in the elusive ab initio calculation). So it is a free parameter in the theory, and needs to be fixed by measurement.

Alas, in the real world QCD is not "pure", it is always coupled to massive fermions. So any measurement of the coupling at some renormalization scale, and subsequent running up to QCD scale, does not really apply to pure QCD. From a theory POV, the pure QCD therefore contains the renormalization scale as a free parameter of the theory, and given no way to fix it, there is no way to say that the theory really "dynamically generates" any mass scale from purely dimensionless coupling constants.

Whether this is a cheap trick or a deep insight is a matter of perspective, but the bottomline is that there is no way to express a scale in terms of scale-free quantities. The only possible thing is to express a scale in terms of another scale, renormalization or otherwise.

Best, 🙂
Marko

18. **West**
April 16, 2014

@Peter,
Are there any particular searches in LHC data that you think constitute severe tests of BSM physics, SUSY or otherwise? I don't expect there to be any silver bullet but studying some processes must probe the relevant physics better than others.

I agree with you that the "the end is just over THIS next hill, I promise" runaround from many theorists is getting quite old, but this is from someone outside of HEP. From a Bayesian perspective this near infinite flexibility when constructing models is far more a vice than a virtue. The willingness to dismiss this concern, on a scientific level, is baffling.

19. **Peter Woit**

April 16, 2014

West,
I'm not an expert on the LHC searches, and this is a really complicated subject. For each different sort of BSM model, a lot of effort has gone into finding possible signatures, and by now many have been looked for by the LHC experiments. SUSY extensions to the SM have many extra parameters, but it is these models have been most intensively studied.

One of the simplest things to look for is the gluino, the superpartner for the gluon, so it is strongly interacting and thus should in general be produced copiously. It's also something that can't be pushed up to arbitrarily high masses without ruining the standard arguments for SUSY (naturalness, coupling constant unification). From what I've seen, it now looks like gluinos pretty much have to have masses significantly over 1 TeV to have escaped notice at the LHC, and this is in strong disagreement with the expectation that superpartners should be around the electroweak scale (100 GeV).

20. **george ellis**
April 17, 2014

"it might be a good idea to spend time obsessing about conformal symmetry, while knowing that conformal symmetry breaking is required to make it work, and we don't (yet) have a good enough idea about conformal symmetry breaking."
– right on. This is a very promising direction.

21. **AS**
April 17, 2014

Dear vmarko, let me explain better.
Following Bardeen, some people impose scale or conformal symmetry in order to argue that one must select a regulator that respects them, so that power divergences must vanish and the naturalness problem is circumvented. This has the problems that you mention.

Instead, I am proposing something different: that at fundamental level only adimensional couplings exist. Then, power divergences must vanish because they have mass dimension but there are no masses. Scale invariance would just be like baryon number in the SM: an ACCIDENTAL symmetry broken by quantum corrections. In such a context, scales can be generated by quantum corrections, and this paper proposes how the weak and the Planck scale can be generated http://arxiv.org/abs/1403.4226

22. **Steven Chan**
April 17, 2014

The greatest physical and social scientists are the ones that can adjust their thinking as new facts and data come in. The thing is rejecting existing believes are hard. At least the supersymmetry debate is generally peaceful; remember what happened to Galileo and Corpenius, or Darwin and Huxley!

At least the debate of heliocentrism and evolution is essentially philosphical and theological, the debate with fancy physics has became a war of career and face. It is hard to accept new ideas when your job depends on not knowing them.

23. **vmarko**
April 17, 2014

AS,

"http://arxiv.org/abs/1403.4226"

I assume you are one of the authors of this paper? It is actually an excellent example of what I've been trying to say regarding dimensional transmutation.

The key is the third requirement in eq. (44), which says that the Planck mass is proportional to the vev of the scalar field S. You don't seem to explicitly calculate that vev anywhere in the paper (but only note that it is nonzero due to Coleman-Weinberg mechanism). But if you actually try to do it, you'll find that the vev of S is proportional to the renormalization scale \mu. This is the only possible choice, since there are no other fundamental scales in the theory. Consequently, M_pl ~ \mu as well.

But this is in start contradiction with the classical (Newtonian) limit, where one measures M_pl ~ const at \mu->0.

Therefore, the model described in the paper is indeed scale-free, but the induced Planck scale runs along with renormalization scale, and goes to zero in the IR sector. The same holds for the Higgs scale, since all induced scales in the model are proportional to \mu. Therefore, all SM masses (fermions, Higgs, Z/W bosons, etc.) also go to zero when \mu->0.

In experiment, however, we observe that all these masses remain nonzero in the IR limit, which indicates that the fundamental theory must feature at least one mass scale other than \mu, and is therefore not scale-invariant.

I'm afraid we are getting off-topic here. Since I believe I've made my point already, I wouldn't like to pollute Peter's blog with this any more. 🙂 We can discuss this further privately if you wish.

Best, 🙂
Marko

24. **Peter Woit**
April 17, 2014

vmarko,
No objection to your discussion with AS, it actually is on-topic, the kind of thing the SciAm article was discussing as an alternative to SUSY.

25. **Ravi**
April 17, 2014

It is good to know that Nima and several others are still betting/predicting that LHC will find SUSY by 2016. Not finding it would then mean that the next run of LHC will provide a further blow to SUSY, as it cant be argued that it was not expected to be found anyway.

Also if fine tuning arguments of the hierarchy problem are to be believed, and if SUSY is not already ruled out for the next run — finding it in the next run always has a higher probability than not finding it but finding it at the next collider at an even higher energy scale….if you think of each energy doubling of collider as a run or as a new collider then probability of finding SUSY particles will go as "p, p/4, p/16 etc." for "run 1, run 2 (but not at run 1), run 3 (but not at run 2 or 1) etc.". p/4 + p/16 + p/64 +…. = p/3. So the next run of LHC (ie LHC at 13 or 14 TeV) has roughly 3 times greater chance of finding SUSY than all the future machines.

26. **Peter Woit**
April 17, 2014

Ravi,
The latest bet was from September 2011. Now that pretty full results from Run 1 are in, it would be interesting to know whether Arkani-Hamed and other signers of that bet are still willing to bet on SUSY in the first year of Run 2.

27. **Ravi**
April 17, 2014

There is a slight correction to the above — the probability will go down by factor of 2 with each doubling not factor of 4…… so the sequence we need to add is p/2 + p/4 + p/8 + …. = p. So LHC at 13/14 TeV has the same chance (p) of finding SUSY, as all future colliders would have together (if LHC doesn't find it).

28. **Ravi**
April 17, 2014

Yes Peter it would be very interesting to find out their bets given that LHC's 7/8 TeV results are fully out. However by Sept 2011 results from 1 fb-inverse data were available. So I don't think their bets on SUSY particles would change. Anyway will be good to know what their prediction/bet would now be for finding SUSY at next run of LHC.

29. **tt**
April 17, 2014

"Perhaps the no-SUSY side decided that they wanted to win on the merits rather than on a technicality?"

i'll try that with my bookie next week

30. **AS**
April 17, 2014

yes, I am the author AS. I insist that dimensional transmutation works in the same sense in which it works in QCD.

In the usual Coleman-Weinberg mechanism the effective potential develops a minimum, which roughly is at the special RGE scale at which a running quartic coupling vanishes. Using the running coupling is just a trick to simplify the computation. The effective potential does not depend on the RGE scale mu.

In the gravitational case the situation is somehow different, and again the simplified argument based on the RGE running is just a trick that allows to establish that a flat-space minimum can arise, without computing the effective potential away from flat space.

31. **Curious George**
April 17, 2014

Some 50 years ago particle physicists hoped to derive masses of "elementary" particles from a deep underlying principle. Are we getting there? Is it still a goal?

32. **paddy**
April 17, 2014

Were R. Feynman to be involved in this betting..he might have insisted on a sometimes used Blackjack rule: early/late surrender. This would allow the 2011 SUSYphiles to pull out their bet now (if they so wish) at the cost of only 0.375 liters of brandy.

33. **Chris Austin**
April 18, 2014

Perhaps a comparison with the AS + AS proposal, discussed above, could help to illustrate why it might still be worthwhile to study models based on compactifications of M-theory, notwithstanding the lack of encouragement from the LHC so far. If I understand the AS + AS paper correctly, they envision that the fundamental theory of physics, including gravity, is a power-counting renormalizable quantum field theory in 4 dimensions, like the Standard Model, that is defined by some discrete choices of gauge groups and matter representations, plus around 20 real number parameters, whose values have to be determined by experiment – and once those real number parameters have been measured, no further understanding of their values can ever be achieved. In contrast to this, M-theory compactifications attempt to explain both the discrete and the continuous data defining the Standard Model, starting from purely discrete data that specifies, for example, the topology of a 6 or 7 dimensional manifold, plus some quantized fluxes and stacks of branes wrapping some cycles of the manifold.

Attempting to realize the SM by an M-theory compactification can also suggest possibilities that might otherwise go unsuspected, and could eventually have technological applications. An example is provided by large volume compact extra dimensions, with a quantum gravity scale in the range from say 10 TeV to 100 TeV. Here the biggest challenge is no longer the lack of encouragement from the LHC, but BICEP2: the de Sitter radius during inflation would be unlikely to be much smaller than the curvature radius of the compact dimensions, so the tensor-to-scalar ratio r would be far too small for BICEP2 to detect. However about half the BICEP2 B-mode signal could arise from primordial magnetic fields rather than primordial gravitational waves, (Bonvin et al), so if the remainder could arise from under-estimated foreground contamination, (Armitage-Caplan et al), lvced could remain viable. Something must then keep the proton adequately stable, and one possibility is a gauged baryon or lepton number, that is broken down to a Z_2 gauged discrete symmetry by a gaugino condensate, (Fileviez Perez and Wise). Isolated protons are then stable, but dibaryon annihilations turn on above about 10 TeV. If something could then catalyse dibaryon annihilations, the prospects for practical interstellar travel could be improved, because it would be possible, in principle, for a rocket engine to convert a substantial fraction of the rest mass of ordinary matter to the kinetic energy of light decay products such as charged pions, and to utilize a significant fraction of that kinetic energy for propulsion. Proton decay can be catalysed by magnetic monopoles in grand unified theories, (Brihaye et al), and CMS and ATLAS are searching for baryon number violation and magnetic monopoles.

34. **Giotis**
April 18, 2014

AS wrote:
"Using the running coupling is just a trick to simplify the computation. The effective potential does not depend on the RGE scale mu."

In theory it doesn't but in practice it does. In theory to remove the dependence on the renormalization scale you have to sum up the whole perturbation series but in practice you can do perturbation only to a certain order; this way the dependence on the normalization scale is inevitable.

So I wouldn't call it a trick.

35. **cormac**

April 18, 2014

I dislike all this talk of 'crisis' and 'panic' in science magazines and blogs, I suspect it has more to do with science journalism than science. I met several of the early pioneers of SUSY through my father and I never heard one of them give any indication that they felt it was a symmetry that 'must' be realised in nature.
Theoreticians like to explore different models, as you know, not to dictate to nature how to behave. Of course, a certain investment arises when apparatus is built to test theories, but that's what they are – tests.
There always was, and always will be, the possibility of a desert. Crisis my arse!

36. **vmarko**
April 18, 2014

AS,

"I insist that dimensional transmutation works in the same sense in which it works in QCD."

I agree with that. The point I am trying to make is that dimensional transmutation in general — in your model, in QCD, and anywhere else — is just another way to introduce a mass scale into a theory by hand. There is no conceptual difference between postulating a mass scale term in the classical action and postulating a mass scale term in the first loop correction of the action. One way or another, you introduce the scale by hand.

"In the usual Coleman-Weinberg mechanism the effective potential develops a minimum, which roughly is at the special RGE scale at which a running quartic coupling vanishes."

Sure, a nonzero minimum does exist for that RGE scale, and all scales below it. The problem is that the **position** of that minimum is not determined by the theory. Your equation (43) which determines this position is a first-order differential equation, and it determines where the minimum is up to an arbitrary multiplicative dimensionful constant.

In order to fix the position of the minimum (i.e the vev for S), you have three choices:
(1) choose it to be zero, in which case the theory still does not have any scale, and is in contradiction with experiment;
(2) choose it to be proportional to the RG scale \mu, in which case the theory contradicts experiment in the IR limit;
(3) choose it to be proportional to the Planck scale, in which case you are putting the Planck scale into the theory by hand.

The option (3) is actually in contradiction with your principle from the beginning — that nature does not have a mass scale. There is nothing being "dynamically generated" by the theory in any profound fundamental sense. The third requirement in (44) is actually a postulate that nature **does** have a scale, and this scale fixes the position of the vev for S.

That is why I characterized dimensional transmutation as a cheap trick — the presence of the scale in the theory has to be **postulated**, one way or another, so that the theory does not get excluded by experiment. This is necessary in your model, in QCD, in superconductivity models, etc. — everywhere where spontaneous symmetry breaking is generated by quantum corrections.

Hopefully now I made myself more clear. 🙂

Best, 🙂
Marko

37. **Peter Woit**
April 18, 2014

Curious George,
"Are we getting there?" No
"Is it still a goal?" Yes

38. **Kavanna**
April 18, 2014

I've always felt that supersymmetry is a compelling idea in general. But the perturbative "carbon copy" approach to implementing this symmetry seems unimaginative. Nature surely has something more clever. Peter makes an essential distinction here in his discussion of the Dirac operator.

When field theorists talk about "dimensional transmutation," they mean a dimension comes into the theory upon solution or integration — something to do with boundary and/or initial conditions. It's not part of the dynamics per se. There are plenty of such examples in physics of scale-free dynamics whose solution picks up a scale when the dynamics are integrated and you impose boundary/initial conditions.

Three issues are being elided here: one is that the dynamics are scale-free; another is that there *must be* such a scale in the general solution to the theory, without saying what value that scale takes. Then there's the specific value itself, the third.

The issue of initial/boundary conditions is always a headache when you're thinking about a general theory of the universe. Initial/boundary conditions come from the "outside" in the canonical approach. But here there's no "outside." You have to go to metaphysical speculation or multiversality. Landau used to put it by saying that science is about differential equations, and religion is about boundary conditions.

39. **Tom Andersen**
April 18, 2014

Curious George, Peter,

As late as the 1950's, Einstein was asked if he could explain the confusion of hadron particles which were being found in ever increasing numbers. He replied, "I would be happy just to know what an electron is!"

Anyone know an accurate source for that? I found only un referenced versions of this on my somewhat quick internet search.

40. **Andrew Foland**
April 18, 2014

Thanks to vmarko, kavanna, and others for the illuminating discussion of how scales enter these theories.

41. **AS**
April 19, 2014

Marko, I would say that a running quartic crosses zero at some scale, and that scale (whatever it is) can be called “Planck scale”.

To be concrete, let us consider a simpler toy a-dimensional model with no gravity: QCD with scalar quarks. Do you agree that the natural value of their quantum-generated masses is the QCD scale?

This is the problem that needs to be solved: finding if the Higgs mass hierarchy problem can have a similar interpretation. To achieve this, one needs a model of quantum gravity where (differently from the usual string models) there are no Planck-scale particles significantly coupled to the Higgs.

Once that one can compute physics above the Planck scale, one can worry about an issue somehow related to what you say: running couplings can indeed set bigger scales via Landau poles.

42. **vmarko**
April 19, 2014

AS,

“Marko, I would say that a running quartic crosses zero at some scale, and that scale (whatever it is) can be called “Planck scale”.”

Sure, you can do that, but it doesn’t really help. The RGE for the quartic coupling is again a first-order differential equation, and determines the running of \lambda up to a dimensionful constant. Fixing the initial condition \lambda(\mu~M_pl)=0 is again a postulate, rather than a consequence of the theory.

In the paper you have explicitly calculated beta functions for all coupling constants in the model (a formidable task, I might add), but they all determine effective values of the couplings up to some arbitrary initial conditions, each involving a dimensionful constant, i.e. a scale. So regardless of how you set up the theory, you need to introduce the scale — or scales — by hand, they are never predicted by the theory.

“To be concrete, let us consider a simpler toy a-dimensional model with no gravity: QCD with scalar quarks. Do you agree that the natural value of their quantum-generated masses is the QCD scale?”

Sure. And do you agree that this QCD scale is completely arbitrary and not fixed by the model itself? Rather, it needs to be specified through an initial condition, external to the theory?

“This is the problem that needs to be solved: finding if the Higgs mass hierarchy problem can have a similar interpretation. To achieve this, one needs a model of quantum gravity where (differently from the usual string models) there are no Planck-scale particles significantly coupled to the Higgs.”

I fully agree that this is a legitimate research direction for discussing the hierarchy problem. Please don’t get me wrong — I am not criticising your work in general, it’s certainly an interesting idea. I am just criticising the postulate “nature has no scale” that you have mentioned both in the paper and in a comment on this blog. My statement is that such a statement cannot be postulated consistently, since eventually you need to introduce scales in the model, and the only way to do it is by hand, contradicting the postulate. The dimensional transmutation doesn’t really help with that in any way.

Aside from obvious deficiencies of the model (presence of ghosts, as noted in the paper), the material presented is actually quite good and interesting. But my advice is to just drop the postulate, and present the model without any reference to it — the presentation will be conceptually cleaner and less confusing regarding how scales arise in the theory.

Best, 🙂
Marko

43. **Low Math, Meekly Interacting**
April 19, 2014

Is it fair to say, at this point, that “naturalness”, at least as far as SUSY is concerned, is all but disproven? Even ignoring previous objections to the idea, it would appear that the combination of the current LHC run and some recent precision measurements should lead us presently to conclude, with a fair degree of confidence, that sparticle masses (if they exist) are beyond the reach of the LHC. I.e., there’s scant reason to expect that doubling the energy of the LHC and running it for another 20 years is going to tell us much about SUSY that we don’t already know. Excluding fanatics, this is a line of reasoning even the most ardent believer in SUSY would find difficult to dispel. Correct, or incorrect?

If correct, I fail to see why it’s a crisis, instead of a momentous opportunity to shift course. This ought to herald a paradigm shift worthy of the title. Of course, the peril of a shift to anthropic reasoning exists, I but can’t imagine a preponderance of young physicist could stomach such a travesty.

44. **Nathalie**
April 19, 2014

Dear vmarko,

Nature doesn’t care about what we consider to be natural. She has her own plot. The upper bound on the cosmological constant was so small that a number of distinguished physicist believed that its only “natural value” must be zero and constructed models in accordance with their wishes. Well, the cosmological constant turned out to be unnaturally small but not zero. In 1950’s four Nobel Laureates proposed the two-component neutrino theory whereby the neutrino masses were naturally zero. This was taken over by the inventors of the Standard Model who excluded right-handed neutrinos. They had no reason to do so, except the desire for simplicity.

As the old cliche says: history keeps repeating itself.

45. **David Berman**
April 22, 2014

So, as one of the instigators of “the bet”, please note the question was not whether the you believe in supersymmetry. The question was whether it would be found by LHC by a certain date. (The renegotiation was to take into account the LHC accident a subsequent further two year delay and to allow the next run). I suspect many people on the list who said that susy would not be found still believe in supersymmetry at higher energy scales.

46. **Anon**
April 22, 2014

If they believe in SUSY at higher scales, they also believe that it is standard model till higher scales. They are putting their eggs in both baskets.

47. **Peter Woit**
April 22, 2014

I have no idea what fraction of theorists would say they "believe in supersymmetry at higher energy scales", but there's an obvious problem with taking that position. Post-LHC it's likely to be a very long time before we see data from much higher energy scales, and, unlike the naturalness argument for SUSY by the TeV scale, there's no serious argument for why SUSY should show up at something like a 10 TeV scale. So, "belief" is very much the operative word, with no prospect of experiment telling believers if they're wrong. That's an unfortunately plausible scenario for the future, but not one the physics community should be comfortable with.

48. **Tim**
April 22, 2014

Dear Peter,

As you are not an advocate of string theory nor one of supersymmetry, do you have an opinion on what are likely candidates (if there are any) for a viable fundamental theory of nature? (Apologies if this has been addressed elsewhere.)

49. **Peter Woit**
April 22, 2014

Tim,
That is off-topic, but a FAQ, so I've added something here:
http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=if-not-string-theory-what-is-your-unified-theory-of-everything

50. **Sideways**
April 26, 2014

This is why I only study the mathematical structures of the nonlinear Bethe-Salpeter type equations.

51. **Sebastian Thaler**
April 27, 2014

Excellent additional perspective from Marcelo Gleiser: http://www.npr.org/blogs/13.7/2014/04/26/306747481/why-won-t-susy-come-home

Raising the Bar

April 23, 2014
Categories: Uncategorized

If you're looking for something to do next Tuesday evening here in New York, an event called Raising the Bar has recruited 50 people to give talks at bars around the city. There are some quite interesting talks on the list, but I'll have to miss them, since I'm scheduled to talk about What We Don't Know About Fundamental Physics at the Blind Tiger on Bleecker Street at 8:30pm. Not sure yet exactly what I'll talk about, but the general idea is to start by explaining that the current situation is that we have a fundamental theory (SM + GR) that is frustratingly good in terms of agreement with experiment, but also frustratingly incomplete. I'll see what I can do to explain the ways in which the SM and GR are incomplete, and what current prospects are for doing better.

Comments

1. **ptmalloy**
   April 23, 2014

   Wish I lived in NYC, sounds fun.

2. **Michael Barton**
   April 23, 2014

   I've been a (silent) follower of your blog for many years. Now I've had an "it's a small world" moment. I live on the West Coast and cannot make your talk. But do say hello to Kate Dulcich, my wife's niece who tends bar at the Blind Tiger!

3. **Peter Woit**
   April 23, 2014

   Thanks Michael, will do!

4. **Goosebumps**
   April 23, 2014

   Peter,

   Is there any way you can make your talk available online? I'd love to be able to attend it but I'm in a different part of the world.

   The incompleteness of the SM and GR that you refer to – do you have separate reasons to feel that both are individually incomplete, or is it just the lack of a fundamental theory including quantum gravity that you're talking about?

   Thanks!

5. **Peter Woit**
   April 23, 2014

   Goosebumps,
   Much of what I was thinking of talking about is basically chapter 8 of my book, which lists the things about the Standard Model that it looks like there should be some deeper explanation of (for instance "Why SU(3)xSU(2)xU(1)?"). These are purely internal to the SM. There is of course also the issue of whether and how to quantize the metric.
   I doubt they'll be video, but in any case I'm not really going to be saying anything that's not in the book or fairly often mentioned here.

6. **Jim Akerlund**
   April 23, 2014

   This sounds like the New York's version of Colorado Café Scientifique. I've been to a few of the Colorado ones, and the format is, half hour talk, followed by a "beer" break, then another half hour question and answer period. The website you point too doesn't give a format for the talks, at least that I was able to see, do you know what the format will be?

7. **Peter Woit**
   April 23, 2014

   Jim Akerlund,
   They're saying 45 min talk + 15 min question and answer. I may cheat by leaving more time for questions. The list of things we don't understand about the SM is pretty short…

8. **srp**
   April 23, 2014

   It might be wise to explain to your audience what you mean by "understand." I've found that there are many people who think that it's currently possible to completely derive the observed structure and behavior of protons or even nuclei from the SM. (They get that notion from the prevalent rhetoric that the SM, which is more "fundamental," is so well understood that it's hard to find things it can't explain.) But there are two different senses of understanding in play between the physicists and the laypeople.

9. **anonymous**
   April 24, 2014

   Kind of off topic for this post but on topic for this blog in general. Here's a link to your colleague Brian Greene's article discussing the recent BICEP2 results. It is as fair and balanced as you can hope. He takes special care to introduce the speculative section toward the end with plenty of warning: "Finally, let me address an issue I've so far carefully avoided, one that's as wondrous as it is speculative. A possible byproduct of the inflationary theory is that our universe may not be the only universe." (With plenty of context, he even uses the expression "seems almost too beautiful to be wrong.")

10. **Tim Howells**
April 24, 2014

I guess you've seen this one:
http://www.condenaststore.com/-sp/A-man-walks-past-a-sign-advertising-a-discussion-entitled-Is-string-theo-New-Yorker-Cartoon-Prints_i8546213_.htm

11. **Igor Khavkine**
April 24, 2014

Peter, I applaud your idea of critically discussing the outstanding problems, or lack thereof, with the quantum field theories of the SM and GR. However, as you are probably well aware, many discussions of the problems with the quantization of the metric are fraught with misconceptions, misinformation, and various aesthetic and philosophical prejudices, often despite even the best intentions. My take on it is that there are more perceived than actual problems there. Not long ago, I gave a conference talk with a brief critical review of these issues and some supporting arguments. It was recorded and is available on YouTube: Gravity: an exercise in quantization (Munich, 2013).

12. **Peter Woit**
April 24, 2014

Tim Howells,
Yes, there's a copy of it hanging in my office…

Igor Khavkine,
Thanks! About quantum gravity even more than other subjects, part of the problem seems to be that we don't really understand what it is that we don't understand…

13. **Shantanu**
April 24, 2014

Peter somewhat OT, but would like to know your comments on strominger's recent talk
at PI. Much of it is same as his talk at Harvard some years back which you blogged about,
but still would be interested in your take in the update.

14. **Peter Woit**
April 24, 2014

Shantanu,
Looks to be much the same as the talk from four years ago, see
http://www.math.columbia.edu/~woit/wordpress/?p=3206
I think including exactly the same slide for a "report card of string theory". The same comments continue to apply.

15. **Roger**
April 24, 2014

SM + GR is frustratingly incomplete? That is a purely theoretical opinion, as there is no known experiment that demonstrates any incompleteness or incompatibility.

16. **Neil**
April 24, 2014

I am still laughing about that New Yorker cartoon. It is priceless, and so appropriate to this post.

17. **Chris Austin**
April 25, 2014

Roger, to the extent that the de Sitter radius can be calculated within SM + GR, it tends to come out around the Planck length, about 10^{-35} metres, which is too small by a factor of about 10^{-61}. Minimal theoretical frameworks that tend to lead to this conclusion include asymptotic safety, (see the discussion around (58) on page 13), and causal dynamical triangulations.

18. **Peter Woit**
April 25, 2014

All,
Sorry, but this is not a good place to debate quantum gravity. Enough.

19. **Richard**
April 26, 2014

Peter,

You might want to catch Nathan Seiberg's talk, "Where is Fundamental Physics Heading?", which will be held at the Simons Foundation in NYC on April 30 (Go to the Eventbrite site, and Google "Seiberg" to register*.).
Take care.

— Rich

* Registration is free.

20. **A**
April 26, 2014

Thanks Igor for posting that talk! Really wish that more people would explicitly mention the difference between perturbative and non-perturbative behavior.

21. **Peter Woit**
April 26, 2014

Richard,

Already planning on attending that. Having it in a bar though might be better…

22. **S. Molnar**
April 26, 2014

I'm not sure if this is, as suggested above, inspired by Café Scientifique, but one might as well raise a glass to the founder, who just died: http://www.theguardian.com/science/2014/apr/24/duncan-dallas

I'm too far away to attend, but I was drawn to the talk titled "The Rise of Greek Culture" until I realized that it's "Geek Culture". After my recalibration, I was then interested in "Powerful Women in Geek Tragedy" (Carly Fiorina?), only to find that I had made the opposite mistake. I guess it's just as well I can't make it: I'll never get the hang of this.

23. **Anna**
April 27, 2014

Good luck with the talk – wish I could go – but it's a pity that the promoters chose a picture that perpetrates all the worst cliches about scientists – men in white coats in laboratories. Well, aprons anyway in this case. I'll bet you have never worn a lab coat since your undergrad days, or possibly even high school. Or entered a laboratory with beakers and the like. And some of us are even women.

24. **Art**
April 27, 2014

Nice to see you're sold out, but seriously, at a bar? Is alcohol consumption down in NY, or is this Manhattan irony? There are these places called "libraries"…

25. **tt**
April 27, 2014

wouldn't the librarian say "sshhhh" ?

26. **Tommaso**
April 30, 2014

Hello Peter,

so how was the event ? Don't leave us in the dark !
Cheers,
T.

27. **Peter Woit**
April 30, 2014

Hi Tommaso,

I think it went quite well. Nice crowd, partly people there for the event, partly people just at the bar for the evening. Most seemed to be paying attention and getting something out of it (except maybe the couple necking in one corner…). After talking for about 45 minutes, I then spent another hour or so talking to people individually, got to meet quite a few of them, including some students.

I'm sure many people didn't understand a lot of what I was saying, but hopefully I got across some of the flavor of the current state of this kind of theoretical physics. This was the first time this kind of thing has been organized, I think it was a big success, so they may do it again, and people in other cities evidently have expressed interest in doing something similar.

I did get one beer out of it, and the bar-owner seemed very pleased about the whole thing.

Quick Links

April 28, 2014
Categories: Uncategorized

- The Defense Department has awarded a $7.5 million grant to Steve Awodey of CMU, Vladimir Voevodsky of the IAS and others to support research in Homotopy Type Theory and the foundations of mathematics. I had thought that getting DARPA 10 years ago to spend a few million on Geometric Langlands research was an impressive feat of redirection of military spending to abstract math, but this is even more so.
- On some kind of opposite end of the spectrum of government spending on mathematics, there's the story of the NSA, the largest employer of mathematicians in the US. Tom Leinster has an article in New Scientist about the ethical issues involved. More at the n-category cafe.

Seven years after the NSA-backdoored NIST standard was discovered by Microsoft researchers, and seven months after Snowden documents confirmed this (see here), the NIST has now removed the backdoored standard from its random number generator standards. As far as I know there has never been an explanation from the NIST explaining how the backdoored algorithm was made a standard, or why anyone should trust any of the rest of their cryptographic standards at this point. Earlier in the year they issued a Draft report on their standards development process which explained nothing about what had happened. The language about the NSA in the report is:

> NIST works closely with the NSA in the development of cryptographic standards. This is done because of the NSA's vast expertise in cryptography and because NIST, under the Federal Information Security Management Act of 2002, is statutorily required to consult with the NSA on standards.

which seems to indicate they have no intention of doing anything about the problem of NSA backdoors.

- On the Langlands front, for those who don't read French Vincent Lafforgue has produced an English translation of the summary version of his recent summary of his recent work on global Langlands for function fields (already proved by his brother, but he has a way of doing things without using the trace formula).

Langlands continues to add material to his web-site at the IAS. See for instance his long commentary on some history at the end of this section and his recent letter to Sarnak with commentary at the end of this section, where he gives his point of view on the state of the understanding of functoriality and reciprocity.

- Sabine Hossenfelder has some interesting commentary on her experiences in the academic theoretical physics environment here.

Mark Hannam has some related commentary on academia at his new blog here.

- I'm still trying to finish a first draft of notes about quantum mechanics and representation theory (available here). I recently came across some similar notes which are quite good by Bernard, Laszlo and Renard.

David Renard also has here some valuable notes on Dirac operators and representation theory.

- Last Friday and Saturday at the University of South Carolina there was a Philosophy of the LHC Workshop, with talks here. Many of the talks were about the nature of the evidence for the Higgs and its statistical significance. James Wells talked about the supposed Higgs naturalness problem. He argues (see paper here) that you can't base the problem on the Planck scale and quantum gravity since you don't know what quantum gravity is (I strongly agree…). Where he loses me is with an argument that there must be lots more scalars out there than the Higgs (because string theory says so, or it just doesn't seem right for there to only be one), and these cause a naturalness problem. Of course, once you have the naturalness problem, SUSY is invoked as the only known good way to solve it.

Comments

1. **Simon Pepin Lehalleur**
   April 29, 2014

   A quick addendum: Vincent Lafforgue claims a proof of the "automorphic to Galois" direction of the global Langlands correspondance – attaching Galois representations to automorphic representations, the ""easy""" direction – for any reductive group over a function field, while his brother Laurent has established both directions but only for GL_n.

2. **Ben R**
   April 29, 2014

   Re Dual_EC_DRBG:

   * Designing cryptographic standards for the US government has always been half of the NSA's mandate, and they do that work in collaboration with NIST. This has never been a secret. Usually it's a good thing because they do employ a lot of talented people, and the US government does have an interest in using cryptography that can't be broken by, say, China. If you kick out the NSA, someone still has to design crypto and no one can be trusted to do it. The only solution is to publish the algorithms and let hordes of academic cryptographers try to break them in the hope of getting a paper out of it. That works against NSA subterfuge too.

   * There was something obviously screwy about Dual_EC_DRBG even before Shumow and Ferguson worked out the details. Cryptographic primitives that need quasi-random initialization constants normally use so-called nothing up my sleeve numbers, such as digits of pi. Dual_EC_DRBG uses mysterious constants that were provided by the NSA without explanation. Given that it's also based on elliptic curves (the EC in the name), it's easy to guess that the mysterious constants are some sort of public key.

   * I don't think published Snowden documents confirmed that Dual_EC_DRBG was backdoored; they just said the NSA was trying to insert back doors into cryptographic primitives, and everyone (or just The Guardian?) assumed it was Dual_EC_DRBG because that was already under suspicion. I'm not completely sure about that, though. Regardless, the Shumow and Ferguson paper was not what caused the initial suspicion, later confirmed by Snowden—the paper was itself a confirmation of what anyone would suspect from looking at the standard.

   * Dual_EC_DRBG is only breakable by whoever has the private key. It's not breakable by, say, China. This was obviously a design goal. The thing is that it seriously constrains the design, which is why Dual_EC_DRBG is so obviously suspicious. Public keys are big (200+ bits for ECC, thousands or millions of bits for other public key systems). If an algorithm only uses nothing-up-my-sleeve numbers, there isn't enough room in the design for a public key. You could still design in a subtle flaw not based on public-key cryptography, but anyone who noticed that flaw could exploit it, and there's no way to predict when, say, China would notice it. You can sort of see how NSA decision-makers could justify Dual_EC_DRBG as within the NSA's mandate, but a deliberate flaw that might leave US communications totally exposed to anyone is a different matter. So I'm not personally too worried about other NIST cryptographic standards.

3. **Peter Woit**
   April 29, 2014

   Ben R.,

I don't see any reason at all to have faith that NSA-introduced backdoors will only ever be usable by the NSA. Do you really think that everyone who works for or has worked for the NSA except for Edward Snowden is completely devoted to keeping their secrets secret and will never slip up on this?

The argument seems to be that it was obvious from the beginning that this was bad crypto, which makes even stronger the main concern here: how and why did the NIST introduce a bad crypto standard? They have done nothing at all to address this, and from all appearances the reason is they feel they have a legal mandate to allow the NSA to use them for this purpose. That the specific mechanism for this involves mathematics and mathematicians makes this an issue that should be of concern to the math community.

4. **Ben R**
April 29, 2014

I do think that the NSA can keep a private key secret. Private keys, unlike Powerpoint slides, never have to be seen by anyone. They can be stored in tamper-resistant hardware decryption modules kept under physical guard, and probably detached from any network most of the time.

I agree it's worrying that this actually became a standard, and that it took so long to become a scandal despite the signs being there from the beginning. It means the open cryptographic community is not paying enough attention. I just don't think it means that the NSA could be secretly subverting other standards without their knowledge. You can look at existing standardized primitives and see that they aren't suspicious in the way that Dual_EC_DRBG is, and there certainly won't be another one like it in the future after this fiasco.

5. **Roger**
April 29, 2014

Blaming NIST is a little strange. It has released dozens of security standards, and its track record is extremely good. Nobody's private info was compromised by Dual_EC_DRBG, as far as anyone knows. On the other hand, Google, Facebook, and Yahoo have compromised 100s of millions of users with the Heartbleed bug. There is more reason to trust NIST for crypto than those companies.

6. **David Ben-Zvi**
April 29, 2014

Peter – As Simon Pepin Lehalleur points out, Vincent Lafforgue's work goes far beyond reproving his brother's results without the trace formula. His work applies to arbitrary reductive groups, which is a huge breakthrough since representation theory works in profoundly different ways outside of GL_n. In particular the "other direction" (Galois to automorphic) doesn't even make sense in the same way as for GL_n, since representations now come clustered together in "L-packets" labeled by Langlands parameters (a labeling we finally have thanks to this work of V. Lafforgue). Together with the giant breakthroughs of Gaitsgory and collaborators in the geometric setting this is a really exciting time for representation theory over function fields!
(…not to mention the advances in the number field setting, which are far too numerous and fast paced for me to even keep up on a hearsay level).

7. **Peter Woit**
April 30, 2014

Simon and David,
Thanks for the comments about the V. Lafforgue work, that's very helpful.

8. **andrew**
April 30, 2014

Peter, I'm having trouble with the links for James Wells' talks,

Your request could not be completed

Authorisation – The access to this page has been restricted by its owner and you are not authorised to view it

Are they publically available?

9. **Aristotle Pagaltzis**
April 30, 2014

Roger,

> [NIST] has released dozens of security standards, and its track record is extremely good

not everyone seems to agree.

10. **Zathras**
April 30, 2014

"I had thought that getting DARPA 10 years ago to spend a few million on Geometric Langlands research was an impressive feat of redirection of military spending to abstract math, but this is even more so."

I'd say this is backwards, with much more potential for applications for homotopy-type theory, as compared to Langlands."

11. **Anm**
April 30, 2014

"I'd say this is backwards, with much more potential for applications for homotopy-type theory, as compared to Langlands."

I feel the same. Homotopy-type theory seems (to me) the most exciting idea anyone proposed in many years.

12. **eitan**
May 2, 2014

Apart from formal theorem proving, type theory has been used in the past to verify the correctness of protocols in large complex mission critical systems where you really dont want to have bugs, race conditions or infinite loops. I know of at least one very large DARPA grant of this nature which was handed a few years ago. As far as I understand (not an expert) its still not clear how constructive (hence useful) the new univalent approach is, but one can certainly see why DARPA would be interested in exploring this.

In applications, one needs highly qualified programmers who also know theory in this approach to protocol verification, this might explain the large sum. Geometric Langlands is really interesting, but I dont see the DARPA connection, but who knows higher math is sometimes useful in surprising ways.

13. **Yatima**
    May 4, 2014

    Axion search in IEEE Spectrum

14. **Chris W.**
    May 4, 2014

    Thanks, Yatima. That's a great article—kudos due to the author, Rachel Courtland.

15. **george ellis**
    May 12, 2014

    Here's a quick link of possible importance related to Bicep2, which you discussed a while ago:

    http://resonaances.blogspot.co.uk/2014/05/is-bicep-wrong.html

16. **Peter Woit**
    May 12, 2014

    Thanks George,
    That looks like big news, but I know nothing beyond what Jester has. I'll repeat my standard advice that Resonaances is a blog anyone seriously interested in HEP physics should be following. If the BICEP2 result does fall apart, will be interesting to see if that gets anything like the often-fevered coverage of the initial claim. It's certainly not going to be advertised as evidence against the multiverse…

17. **theoreticalminimum**
    May 13, 2014

    See this instead:

    > As for Falkowski's suggestion in his blog that the BICEP has admitted to making a mistake, Pryke says that "is totally false." The BICEP team will not be revising or retracting its work, which it posted to the arXiv preprint server, Pryke says: "We stand by our paper.

Quillen Notebooks

May 6, 2014
Categories: Uncategorized

Daniel Quillen, one of the greatest mathematicians of the latter part of the twentieth century, passed away in 2011 after suffering from Alzheimer's. For an appreciation of his work and an explanation of its significance, a good place to start is Graeme Segal's obituary notice, and there's also quite a bit of material in the AMS Notices.

It's very exciting to see that the Clay Math Institute now has a project to make available Quillen's research notebooks. Segal and Glenys Luke have been working on cataloging the set, producing lists of contents of the notebooks, so far from the earliest ones in 1970 up to 1977. Quillen's work ranged very widely, and for much of the 1980s he was very much involved in what was going on at the boundary of mathematics and quantum field theory. His work on the Mathai-Quillen form provided a beautiful expression for the Thom class of a vector bundle using exactly ingredients that formally generalize to the infinite-dimensional case, where this provides a wonderful way of understanding certain topological quantum field theories. The Mathai-Quillen paper is here, see here for a long expository account of the uses of this in TQFT.

I've just started to take a look through the notebooks, and this is pretty mind-blowing. The Mathai-Quillen paper is not the most readable thing in the world; it's dense with ideas, with motivation and details often getting little attention. Reading the Quillen notebooks is quite the opposite, with details and motivation at the forefront. I just started with Quillen's notes from Oct. 15 – Nov. 13, 1984, in which he is working out some parts of what appeared later in Mathai-Quillen. This is just wonderful material.

Besides his own ideas, there are notes taken based on other people's talks. See for instance these notes from a private talk by Witten in Raoul Bott's office on Dec. 15, 1983.

I was already having trouble getting done a long list of things I am supposed to be doing. Having these notebooks available is going to make this a lot worse….

Comments

1. **Staffan Angere**
   May 7, 2014

   What a nice coincidence, I just started reading "Homotopical Algebra"!

   More generally, these notes are an absolute goldmine for people like me, who are interested in the philosophy of mathematics but not working mathematicians. So much of the process of creating mathematics gets lost in the preparation of mathematical papers: to an outsider, it seems like proofsm theorems and definitions appear almost magically in the mathematician's mind. Reading notes helps us see that they are mostly just hard working but usually smart people.

   Btw, this is my first comment here, although I've been following the blog for a couple of years. Thanks Peter for keeping up the good work!

2. **Igor Khavkine**
   May 7, 2014

   Just a curiosity, but does anyone know why Quillen never followed up his PhD work on overdetermined PDE systems and changed his focus so radically?

3. **Peter Woit**
   May 7, 2014

   Igor Khavkine,
   If you look at the notebooks from the late 1970s, Quillen did return to analysis. I'd also be curious to know about the shift to topology and algebra from his thesis, although perhaps what is surprising is that as a student of Bott he was working on PDEs at all.

4. **Igor Khavkine**
   May 7, 2014

   Peter, it's worth noting that not all work on PDEs is in analysis. In fact, Quillen's work was on the so-called *formal theory of PDEs*, which is mostly geometric and right around the time of the early 1960s it received a healthy dose of homological algebra, under the influence of the ideas of Donald Spencer. In his thesis, Quillen exploited the homological algebra ideas of Spencer to show that any overdetermined linear differential operator fits into a formally exact sequence of differential operators, each operator completely characterizing the integrability conditions of the preceding one, which is sometimes called the *compatibility complex*. He also showed some technical results on the ellipticity of this complex. This result is quite important in the formal theory, though apparently it was known much earlier. However, earlier proofs relied on non-geometric, heavily coordinate dependent methods that were developed by Janet and Riquier. Quillen's solution was on the other hand coordinate independent and fully geometric, in line with the spirit in which the ideas of Spencer were introduced.

   I'm not sure of the history of Bott's interest in the formal theory of PDEs, but it is recorded in his *Notes on the Spencer resolution* which were circulated around 1963.

5. **Sakura-chan**
   May 8, 2014

   Wow the notes are wonderful. Thank you Peter for bringing them to our attention!

6. **Deane**
   May 16, 2014

   Igor,

   I'm curious about why you ask why Quillen abandoned the formal theory of overdetermined systems of PDE's. Do you use this formal theory yourself?

   My view is that he shifted to new directions (which involve similar formal structures) that had a more profound impact on mathematics. In fact, many of the people who worked on this back then, including Spencer, Quillen, Guillemin, Sternberg, Goldschmidt, did their best work on other subjects. It seems to me that the modern formal theory of PDE's has had relatively little impact on other areas of mathematics.
   As far as I know, only Robert Bryant and his students, who use the classical approach of E. Cartan have been able to use the formal theory effectively, usually in applications to differential geometry.

7. **Deane**
   May 16, 2014

It is indeed a little mysterious why Quillen worked on the formal theory of PDE's for his thesis. Around that time, Guillemin, Sternberg, and Quillen were around and working on such things. But it seems like Sternberg would have been the more natural choice as an advisor for this topic.

8. **Igor Khavkine**
May 18, 2014

Dean,

Yes, mysteries all around. You say: *"It seems to me that the modern formal theory of PDE's has had relatively little impact on other areas of mathematics."* In a way you are right, but more is the pity! There are a few, not totally connected, islands where it has been continuously applied. I'd say that much of the potential utility and the power of the formal theory still remains untapped. I for instance have been finding more uses for it (in the mathematical structure of field theory, for instance) the more I've learned about it, including the results due to Quillen.

9. **Cynthia (Quillen) Cohen**
June 1, 2014

Firstly I think that "Quillen" (my Dad) would be very pleased that you are enjoying what he called his "diary". He was always trying to get us kids to write diaries, he spoke to us of Samuel Pepys, and it seems, like him, that Dad left something meaningful behind.

I thought I'd add a little info as to how he worked for the benefit of Staffan Angere (and anyone else interested). Certainly in the years of my life (I was born in 1980) Dad did two types of writing. The first was day to day writing which he did on scrap paper, notebooks etc, and the second was the "diary". The diary was where he would write up, in a more organized way, his thoughts and what he was working on.

10. **Peter Woit**
June 2, 2014

Thanks Cynthia,

It's reassuring to know that the notebooks are a second, more polished summary of your father's day to day work. It was kind of frightening to contemplate the possibility that he was directly writing out texts of this high quality without going through a much more confused stage of the sort where the rest of us spend most (if not all…) of our time.

BICEP2 News

May 13, 2014
Categories: Uncategorized

Recall that this past March results from BICEP2 were announced, claiming a greater than 5 sigma detection of a primordial B-mode signal in the CMB polarization. This result received a huge amount of world-wide attention (and see some commentary here). Yesterday saw a very curious situation, with Adam Falkowski at the Resonaances blog claiming that the BICEP2 foreground analysis was flawed, and that "Various rumors place the significance of the corrected signal between 0 and 2 sigma." Adrian Cho at Science magazine has a story about this, quoting Clement Pryke, co-PI of BICEP as saying "We stand by our paper", while acknowledging that, with respect to a plot of Planck data they used to estimate the foreground "It is unclear what that plot shows".

The controversy surrounds slide 6 of this presentation, with the BICEP foreground analysis evidently relying on scraping data from this slide. The claim at Resonaances is that they didn't take into account the "Not CIB subtracted" notation on the slide:

> However, it seems they misinterpreted the Planck results: that map shows the polarization fraction for all foregrounds, not for the galactic dust only (see the "not CIB subtracted" caveat in the slide). Once you correct for that and rescale the Planck results appropriately, some experts claim that the polarized galactic dust emission can account for most of the BICEP signal.

This is backed up by David Hogg's report of a talk by Raphael Flauger at NYU yesterday:

> At lunch, Raphael Flauger (NYU) gave a beautiful talk on foreground uncertainties related to the BICEP2 results. He built his foreground models as did the BICEP2 team by scraping data out of Keynote ™ presentations posted on the web! I have to say that again: The Planck team showed some maps of foregrounds in some Keynote presentations and posted them on the web. Flauger (and also the BICEP2 team before him) grabbed those presentations, scraped them for the all-sky maps, calibrated them using the scale bars, and worked from there. The coolest thing is that Flauger also simulated this whole process to account in his analysis for the digitization (scraping?) noise. Awesome! He concludes that the significance of the BICEP2 results is much lower than stated in the paper, which makes him (and many others) sad: He has been working on inflation models that produce large signals.

It sounds like this issue is not going to get resolved until there is something more substantial from Planck about this than a slide suitable for data scraping. In the meantime, blogs are your best source of information. Or maybe Twitter, where Erik Verlinde tweets from the Princeton PCTS workshop Searching for Simplicity that:

> News from Princeton: BICEP2 polarization data are due to dust foreground and not caused by primordial gravity waves.

Update: There's also a New Scientist story here. It should be emphasized that the BICEP team are denying that there is any need to revise what is in their paper, with New Scientist quoting John Kovac of BICEP as follows:

> Kovac says no one has admitted anything. "We tried to do a careful job in the paper of addressing what public information there was, and also being upfront about the uncertainties. We are quite comfortable with the approach we have taken."

See comments in the comment section here from Sesh Nadathur and Shaun Hotchkiss explaining why there may not be very much significance to this issue.

Update: Sesh Nadathur has a detailed post up now about this, New BICEP rumours, nothing to see here. Bottom line is:

> The BICEP result is exciting, but because it is only at one frequency, it cannot rule out foreground contamination. Other observations at other frequencies are required to confirm whether the signal is indeed cosmological. One scenario is that Planck, operating on the whole sky at many frequencies but with a lower sensitivity than BICEP, confirms a gravitational wave signal, in which case pop the champagne corks and prepare for Stockholm. The other scenario is that Planck can't confirm a detection, but also can't definitively say that BICEP's detection was due to foregrounds (this is still reasonably likely!), in which case we wait for other very sensitive ground-based telescopes pointed at that same region of sky but operating at different frequencies to confirm whether or not dust foregrounds are actually important in that region, and if so, how much they change the inferred value of r.
>
> Until then I would say ignore the rumours.

Peter Coles also has a blog post here, with bottom line

> I repeat what I've said before in response to the BICEP2 analysis, namely that the discussion of foregrounds in their paper is disappointing. I'd also say that I think the foreground emission at these frequencies is so complicated that none of the simple approaches that were available to the BICEP2 team are reliable enough to be convincing. My opinion on the analysis hasn't therefore changed at all as a result of this rumour. I think BICEP2 has definitely detected something at 150 GHz but we simply have no firm evidence at the moment that it is primordial. That will change shortly, with the possibility of other experiments (specifically Planck, but also possibly SPTPol) supplying the missing evidence.
>
> I'm not particularly keen on the rumour-mongering that has gone on, but then I'm not very keen either on the way the BICEP2 result has been presented in some quarters as being beyond reasonable doubt when it clearly doesn't have that status. Yet.

Update: There will be a talk about this issue in Princeton tomorrow morning, see here.

Update: Slides from the Flauger talk at Princeton are here. I'll leave discussion of the results presented to the better-informed, but will comment that this work appears to definitely involve new heights in the technology of data-scraping from Keynote presentations.

Update: Video of the Flauger talk is here. Quite interesting are the introductory remarks of Paul Steinhardt, and the concluding remarks of Lyman Page. See also new blog posts from Jester and Sesh Nadathur. Sesh (via Eiichiro Komatsu at Facebook) includes a transcription of part of Page's comments on the situation:

> This is, this is a really, peculiar situation. In that, the best evidence for this not being a foreground, and the best evidence for foregrounds being a possible contaminant, both come from digitizing maps from power point presentations that were not intended to be used this way by teams just sharing the data. So this is not – we all know, this is not sound methodology. You can't bank on this, you shouldn't. And I may be whining, but if I were an editor I wouldn't allow anything based on this in a journal. Just this particular thing, you know. You just can't, you can't do science by digitizing other people's images.

From looking at all this, and seeing what the people in Princeton are saying, my non-expert opinion is that the BICEP2 result should be interpreted as an observation of B-mode polarization, but there's no convincing data yet about the crucial question of whether this is foreground or cosmological. The BICEP2 data could not address this, and the relevant Planck data is not yet available (other experiments will soon also provide the data needed to resolve this question). The BICEP2 press release claiming "the first direct evidence for cosmic inflation" now looks like it may have been a highly premature claim.

Update: Talks ongoing at Caltech today about this at a workshop, videos later here. On Twitter, you can follow the BICEP/Planck fight via Sean Carroll:

> At Caltech CMB workshop. #BICEP2 folks seem completely unconcerned about recent worries about galactic foregrounds. Wait for Planck paper…
>
> Zaldarriaga on CMB grav waves vs. dust: sane answer is "let's just wait." On the other hand… we just can't. No scientist is that patient…
>
> MZ: Planck hasn't measured dust in #BICEP2 region. But extrapolating from where they did measure, apparently can fit B-mode signal.
>
> MZ: "I'm not happy this is on Facebook and Twitter."

Seems to me we're now stuck with Planck saying they think this is dust, BICEP saying they think it's not. Planck is the side that has data about dust, BICEP is the side that has something they scraped off a slide of a Keynote presentation…

Update: Excellent article about this in the Washington Post from Joel Achenbach: Big Bang backlash.

Update: Zaldarriaga: I believe the case in favor of a detection of primordial B modes is not convincing (hopefully just temporarily). See more here and here.

Comments

1. **Sesh**
   May 13, 2014

   I don't see that this rumour has materially affected our knowledge about BICEP results in any way. In particular, the quoted value of r=0.2 was obtained assuming *zero* foreground dust contamination (which everyone knew was unrealistic), and that certainly hasn't changed. They also tried a handful of different foreground dust models, all expected to be only approximations to the real dust contamination – only one of which was extracted from this Planck slide – and found that all these approximate dust models gave a foreground contamination that was an *order of magnitude* too small to account for the B mode signal seen.

   Now, nobody really believed these dust models were entirely accurate (at least I hope not). One of the handful turns out to probably be slightly less accurate even than that. So what's new? The most probable answer to the question of how much of the signal is due to dust contamination is still "not a lot". And the answer to the question of how precisely we trust any one of the dust models they used is also "not a lot".

   What's interesting is that it now seems unlikely that Planck is going to be able to tell us what the real dust contribution in the BICEP region is, but this doesn't fit with the rumoured narrative of "BICEP shown to be wrong".

2. **Nick**
   May 13, 2014

   I am not a physicist (computer engineer), but am very much interested in this area. Normally I read through a lot of this stuff and sometimes research more, and sometimes just say wow. On this particular issue though, I really have to ask to ensure I'm reading what I think I'm reading. They took a slide from a presentation and scraped points off of it, and used that as their data points?! Why would someone even bother to do that? Isn't the original data that was used to generate the slide in the first place available in a more appropriate form, so that screen scraping wasn't required? Couldn't the whole exercise be repeated again, but use the original data (instead of the flawed screen scrape data) to validate the results or get more accurate results?

3. **Peter Woit**
   May 13, 2014

   Nick,
   As you can see from Sesh's comment, the argument is that this was being used to show that this source of foreground was an order of magnitude too small to account for what they were seeing (to be clear, I have no idea who is right about this, other people are making other claims). So, from this point of view, it didn't matter much whether they had an accurate version of the Planck data, the slide was good enough.

   This does show that the relationship between the Planck and BICEP teams isn't exactly one of close cooperation…

4. **Seyda**
   May 13, 2014

   Erik Verlinde only tweets the science magazine blog post. So no new information really.

5. **Nick**
   May 13, 2014

   Peter (and Shesh),

   Thanks for the addition clarity. I think the big item here (beyond the accuracy of the original claim) is actually that screen scraping was required at all. Saying close cooperation doesn't exist seems to be an understatement if people are willing to go to those types of extremes to get good data.

6. **AdamT**
   May 13, 2014

   This is pretty silly behavior for scientists. Apparently the Planck team would not release the raw data to the BICEP2 team which caused them to go for the screen scrape:

   > "Part of the problem is that the Planck team has not made the raw foreground data available, he says. Instead, BICEP researchers had to do the best they could with a PDF file of that map that the Planck team presented at a conference."

   So here we have two very high profile groups with talented researchers bickering and competing resulting in 'digitizing' a screen scrape off a PDF file.

   The things they work on are cosmic in scope and the results of potentially huge import for the future of science, but the behavior of the individuals is decidedly middle school in scope and (should be) embarrassing for scientists.

7. **Krzysztof**
   May 13, 2014

My personal bet is that at the end, we will meet mid-way, at rho ≈ 0.1 – anyone interested?

8. **Curious Mayhem**
   May 13, 2014

   The two BICEP talks that I've heard were a bit opaque on their relationship with the Planck collaboration. Certainly, the slides would be good enough for an order of magnitude estimate, or better than an order of magnitude. To get fine detail and the level of confidence they claim, though, requires better information than that. But as Sesh points out, the BICEP team has a number of other ways of checking their result, and they all agree, within uncertainties. Of course, they agree even better if the uncertainties are larger than BICEP initially claimed. The BICEP talks all stressed the multiple independent checks of their results.

   The refusal of the Planck team to share their raw data is disturbing. This is potentially one of the most important scientific discoveries of the last century. I assume, as scientists, we outgrow this behavior in high school or college.

9. **Shaun Hotchkiss**
   May 13, 2014

   Slightly further to Sesh's comments it should be stressed that within the patch of sky BICEP has measured, the dominant noise in Planck's measurement is Planck's instrumental noise (or so I have been led to believe). That this is probably true can be seen by the fact that in Planck's recent paper on polarisation (http://arxiv.org/abs/1405.0871) the BICEP patch is part of the region that has been masked because it lacks sufficient signal (dust) to noise (instrument).

   Therefore, any attempt to reconstruct the expected dust foreground for BICEP by analysing Planck's preliminary "polarisation fraction" maps slide is probably going to end up mistaking a fair bit of Planck's instrument noise for dust. Obviously, Planck's instrumental noise has nothing to do with the BICEP detector. So, as Sesh stressed, Planck might not be able to, even in the long run, determine the dust foreground along BICEP's line of sight accurately enough to have an impact on BICEP's measurement. This seems kind of obvious to me in hindsight, or Planck would have detected BICEP's signal long before BICEP did!

   Planck will only ever be able to make a statistical detection of tensors by analysing B-modes over the whole sky and saying the average signal isn't consistent with zero, but it won't be able to say that along one specific line of sight the B-mode signal is x \pm y, like BICEP has.

   At least, this is the picture as I've understood it over the last week of discussion.

10. **emile**
    May 13, 2014

    There can be good reasons not to share your "raw" data: you are possibly not done with the analysis and what you have shown up until now are preliminary results, or you think that the raw data would likely not be interpreted correctly by the other experiment (for example, raw data from a large particle physics experiment could not (should not) be used by another experiment). If the Planck results were published in a paper and hence not preliminary, then to give the the numbers used to make a plot used for the publication should be a simple courtesy however.

11. **piscator**
    May 13, 2014

    Some of these comments are really quite silly. At the time of the BICEP announcement no Planck polarisation papers had appeared. Preliminary results that appear in talks are just that, preliminary. No experiment should be under any obligation to release its results or data before they are ready to their own satisfaction.
    Once Planck releases its fully analysed polarisation data, uncertainties on foregrounds will come down. Until then, the uncertainties are higher. The basic obligation experimentalists have to the community is to be correct.

    BICEP chose to announce their results via press conference, using the full amplification of the news media. IMO publishing in this manner means you have to be right – there can be no excuses if foregrounds turn out larger than your best estimate, it is either Stockholm or go home.

    I hope it's the former, as the science is enormously exciting if they are correct.

12. **JG**
    May 13, 2014

    Shaun Hotchkiss

    even if Planck can't get an accurate dust map for the specific patch of sky measured by BICEP2 the fact that the polarization signal (from dust) is so high in the rest of the map would make it hard to argue how the small BICEP2 patch might avoid such contamination.

13. **Shantanu**
    May 13, 2014

    Adam T,
    situation in astronomy is much better than in HEP.
    At least they release their data after some years of proprietory period.
    In HEP experiments no one releases ANY data to outside world even decades
    after experiment has ended.

14. **AdamT**
    May 13, 2014

    piscator,

    The news reports suggest that Planck scientists are passing around rumors tsk tsking the BICEP2 folks for screen scraping and saying that the raw data interpreted correctly lowers confidence in BICEP2's results. If that is true and the they refuse to let the BICEP2 folks have access to the raw data, then this is very bad form from the Planck investigators.

    This is not middle school. The science should be held as a higher goal than trying to pass around rumors and withholding data from a rival experiment that might illuminate a discovery of this magnitude.

15. **Sesh**
    May 13, 2014

    JG: That argument could be quite simple actually, since the level of dust contamination varies across the sky – there's obviously more dust in the line of the galaxy than in other regions. BICEP's window was specifically chosen to be in a region that was believed to have low dust emission. The fact that Planck is perhaps unable to distinguish dust emission from instrument noise in that part of the sky could even be an encouraging sign that dust emission is actually small there (I'm obviously making some assumptions about noise properties which could be wrong, but that's kind of the point – people making assumptions without knowing the details can wind up at any kind of conclusion).

As several others have said, it's silly to blame the Planck team for not releasing preliminary data: they're still working on it and checking it, that's why it's preliminary.

16. **Tony Smith**
May 13, 2014

Could Planck's failure to release data be related to the fact that

Planck 2013 results. XXXI. Consistency of the data
has NOT yet been released – although the http://www.sciops.esa.int web site says
that it has been in 2014 "Submitted to A&A"
but NOT yet Accepted by A&A.

Tony

17. **CIP**
May 13, 2014

Just a point of psychology. Anybody who thinks that the same instincts, impulses and motivations that impel us in middle school aren't still at work in middle age hasn't been paying attention. Read some history, or better, Watson's version of the Double Helix.

If Watson and Crick hadn't surreptitiously swiped Franklin's X-Ray diffraction, they wouldn't have won the Nobel. If Pauling had seen it, he would have collected his third Nobel Prize.

18. **JG**
May 13, 2014

Thanks Sesh

and also I noticed from reading other blogs and comments that Shaun Hotchkiss is fully aware of my trivial point (and has stated it more accurately)

I also agree that Planck have no moral obligation to release raw data before they have accurately modelled influences from instrument noise and other contamination sources

19. **layman**
May 13, 2014

There is one factor nobody mentioned: some BICEP2 scientists are on the Planck collaboration! and they have therefore access to all the raw data.

20. **pion**
May 14, 2014

It seems to me that Planck is not our best hope to settle this issue mainly due to the fact that it is a satellite, and information from certain ground-based telescopes might be more credible.

Since the CMB polarization level is obtained from differencing two intensity measurements toward the same direction on the sky, any optical imperfection of the detectors can potentially leak the dominant intensity to the faint B-mode polarization. A few of these spurious signals can be potentially mitigated by the telescope's scanning strategy; basically, each pixel in the field is observed multiple times with (ideally) different orientations of the polarimeter. Ideally, this would suppress a large fraction of the systematic but not entirely.

Most ground-based telescopes benefit from the earth rotation, others use half waveplate, and in general the scanning strategy could be optimized for minimizing the intensity-to-B-mode leakage. Satellites in orbit, however, are limited and their typical scanning strategy is sub-optimal if not poor. We know, as a fact, that Planck never published their B-maps, and I guess that this is partially due to the issue of B-mode systematics which is always a challenge, for satellites in particular. I'm not sure how much would the Planck team have to say in October.

My best bet is that a joint effort of ground-based instruments (which are located off the pole) might ultimately provide a conclusive answer to this thorny issue. The problem with this alternative, though, is that ground-based experiments are limited to a relatively narrow frequency window. Hopefully, two or three frequency bands will suffice for the construction of a reliable polarized dust model, but this is not apriori guaranteed.

21. **AdamT**
May 14, 2014

CIP,

Just because some people haven't moved on from middle school mentality does not negate that some of is have and that more should.

All defending Planck for not releasing prelim data,

Fine if it is prelim and unsure they should stop spreading nasty rumors about he competing project. Put up or shut up.

22. **JG**
May 14, 2014

AdamT

Planck aren't spreading rumours, it's casual blogosphere discussions causing the stir, the fact that some pretty well respected bloggers are involved has made it a bigger story than it should be, but your reaction is unjustified.

Planck have "put up" their results in an orderly professional manner, and they will deliver the final results later this year

The title of the talk at Princeton tomorrow sounds like something from the social sciences or philosophy: "Towards an Understanding of Foregrounds in the BICEP2 Region" – maybe there is a little humour/sarcasm implied by the title?

23. **M Mahin**
May 15, 2014

The PDF has just come out for the Princeton talk on BICEP2 foregrounds.

http://www.pctp.princeton.edu/pcts/SpecialEventSimplicity2014/Simplicity.pdf

Read it and frown if you're a BICEP2 advocate.

Go to page 39 of the presentation. One corrected model shows 7 out of 9 BICEP2 observation points within the area predicted by a combination of dust foregrounds and gravitational lensing. The other corrected model also shows 7 out of 9 BICEP2 observation points within the area predicted by a combination of dust foregrounds and gravitational lensing.

If you use the BICEP2/Keck combination for the observation points, then it's basically 9 out of 9, with nothing that needs to be explained by cosmic inflation.

24. **Anonymous**
May 15, 2014

The video of the Princeton talk is now available too.

25. **Chema**
May 20, 2014

One thing to remember from this story will be for sure Flauger's work on reproducing the data from the pdf file and "correcting" for the pipeline (starting on healpix maps) effect. Bravo! (note to myself, go back to transparency slides in your next talk)

26. **Nick M.**
May 20, 2014

Yesterday, Matt Strassler finally weighed in on the current BICEP2 controversy with his blog post "Will BICEP2 Lose Some of Its Muscle?" A very interesting read, and, as always, a very balanced presentation. (And please forgive me, Peter, if this link has already been provided elsewhere in your article, or in subsequent visitor postings.)

Walter Burke Institute for Theoretical Physics

May 14, 2014
Categories: Uncategorized

Caltech has just announced the establishment of the Walter Burke Institute for Theoretical Physics, with Hirosi Ooguri as director. It will have a permanent endowment of around \$74 million, with \$30 million of that new funds from the Sherman Fairchild Foundation and the Gordon and Betty Moore Foundation.

To get some idea of the scale of this, the recent worries about HEP theory funding in the US have been due to a drop in funding by the DOE of university research from around \$27.5 million/year to \$24 million/year. So, a few million/year from this endowment should help make up for that, while continuing the trend of changing over theoretical physics funding from government support to philanthropy by the .01%.

Update: In other developments from the .01%, Physics World has the news that nominations are now open for the \$3 million Milner prize in physics. You can submit nominations here. Nominations close June 30, announcement of winners will be November 9.

There's also now a website for the Milner/Zuckerberg \$3 million mathematics prize. Not much info there except that it will reward "significant discoveries across the many branches of the subject." I'm guessing that, like the other prizes, initial picks will be from Milner/Zuckerberg themselves, with those people going on to form the committee to pick future winners.

Comments

1. **CU Phil**
   May 15, 2014

   Hastening a return to the days when gentleman-scientists were supported by royal benefactors. Delightful.

2. **Jon Orloff**
   May 15, 2014

   Any indication that the benefactor wants to choose the topics of research?

3. **Peter Woit**
   May 15, 2014

   CU Phil,
   The trend to private funding does mean that our current equivalent of royalty will be the ones deciding what research gets done. But one difference is that they believe in paying people pretty well: those getting funded shouldn't need a family trust to support themselves.

   Jon Orloff,
   I assume Burke is willing to leave the choice of research topics to the Caltech physicists, although the donation reflects a personal decision that he likes what they are doing more than what other groups are doing. The effect of this move to private funding is more to concentrate control of research directions in certain hands: DOE money is allocated by panels reflecting a broad community, this money will be allocated by Ooguri and a few others.

4. **Low Math, Meekly Interacting**
   May 15, 2014

   The short-term alternative to the 0.01% anointing certain disciplines with cash while public funding dwindles is that public funding dwindles anyway. Easy enough to see a benefit in that context.

   Long-term, however, I worry that the rate of decline in public funding will accelerate in response to a new expectation that the 0.01% will eliminate the need for it.

5. **domenico**
   May 16, 2014

   I think that there is a difference between the Sherman and Betty funds, and the Milner prize: the first will lead to certain (small or large) results in Theorical Physics, the second is a loss of money from a billionaire to a millionaire pensioners.

6. **Sam**
   May 17, 2014

   A couple of points:

   1) Much of the privately funded research, especially the prizes, is solely directed towards the 'intellectual merit' criterion of NSF's two fundamental criteria (broader impacts and intellectual merit.) The funding broader impact activities is very important for the scientific well-being of the nation and this is so recognized in Congress. Funded broader impact activities are visible, and accessible, to members of Congress and their staff. I don't think private funding, prizes especially, will drive the nation out of the funding of science.

   2) Private funding, e.g. the Simons Institute, can be more nimble and experimental. Further, Simons is run by folks that have a deep understanding and experience in the academic research model. There is every reason to hope that new and exciting funding mechanisms/ideas will be brought forth and adapted/adopted in the public sphere.

   Folks are too pessimistic; there are opportunities and changes coming. Not all will work out but some will.

7. **srp**
   May 17, 2014

   An influx of private funding to replace government funding of pure research would be most welcome, especially if the sources are diverse and controlled by diverse groups of scientist/administrators. Up until World War II, almost all important U.S. science was privately funded. Hale was able to build giant telescopes, the biggest science of his day, entirely through private donations. Fields such as geochemistry and chemical engineering were sired by companies and foundations. Especially as government funding of research in the U.S. becomes more unified behind overarching umbrella programs run by a shrinking number of decision-making groups, we should hope for even more entry by the philanthropic into pure research funding.

8. **PhilAnthropy**
May 18, 2014

Let's hope some of this money takes the lead from Jim Simons, for more brad-based support, rather than from the steroid-enhanced star system of Milner. The tendency of the .01% of money to fund the .01% of physics and math doesn't help anyone except the egos of those concerned.

9. **Rob Meyer**
May 26, 2014

Dear domenico and PhilAnthropy:

Here are a few instances where Breakthrough Prize funds (physics and life sciences) are being handed down from the winners to students and other research efforts.

http://home.web.cern.ch/students-educators/updates/2014/02/cern-announces-first-recipients-atlas-phd-grant

http://www.bbc.com/news/science-environment-23809234

http://cms-mgt-fps-board.web.cern.ch/cms-mgt-fps-board/scholarship.php

https://graduate.ucsf.edu/news/botstein-pays-it-forward

http://weill.cornell.edu/news/pr/2014/05/paying-it-forward-breakthrough-prize-winners-and-institutions-commit-3-million-in-support-of-next-ge.html

http://news.emory.edu/stories/2014/01/delong_donates_250k_for_parkinsons/index.html

Cheers, Rob

10. **domenico**
May 27, 2014

Dear Rob Meyer:
I don't understand: why to use a prize to reward an old glory, then the old glory could reward a promising young scientist, if is there the direct method to reward the promising young scientists and young research works? Is not these moneys be better spent?

11. **domenico**
May 28, 2014

I have to apologize with the Breakthrough Prize laureates that support the next generation scientists: I had forgotten that the smart choices are in the intelligent people.

Quick Links

May 25, 2014
Categories: Langlands, Multiverse Mania, Uncategorized

Just returned from a few days in Boston, will try and catch up here on various topics:

- This past week I was able to attend some of the talks at the conference in honor of David Vogan's 60th birthday. I'm still trying to make sense of Bernstein's talk on Stacks in Representation theory, where he argued that the category of equivariant sheaves on a certain stack is a better-behaved construction than the category of representations. I've always wondered whether this would be helpful in the case of representations of a gauge group, where the stack has something to do with equivalence classes of connections. It was his first use of Beamer, and some slides went by too fast. I noticed that a few people were documenting talks for themselves with phones/tablets taking pictures of slides/board.

  Among the other interesting talks, Jim Arthur discussed the conjectural automorphic Langlands group, along the lines of this older paper. He indulged in some speculation I'd never heard before, that Langlands functoriality might imply the Riemann hypothesis (somehow by analogy to something from Langlands about the Ramanujan conjecture appearing in Deligne's proof of the RH in the function field case). Unfortunately the laptop being used to show his slides decided to start installing Windows Updates two-thirds of the way through his talk. For whatever reason, I didn't manage to follow his comments at the end of the talk about something new having to do with Weil's explicit formula in number theory. Consulting with some experts later though, I couldn't find anyone optimistic about the Langlands implies RH speculation.

- Also last week, the draft P5 report on a strategic plan for US HEP over the next 20 years was released, with discussion at an HEPAP meeting. Besides planned LHC upgrades, high priority goes to neutrino physics based at Fermilab, with a plan to attract more international participation. Other directions getting a high priority are on-going dark matter experiments and CMB research. A continued move of funding from research grants to construction projects will likely keep pressure on grants to university theory groups. Research into muon colliders is down-played, with a recommendation to "consult with international partners on the early termination of MICE."
- Skepticism about the BICEP2 primordial gravitational wave claims continues, with for instance this story at Science, and this preprint. In retrospect, it's curious that the possible problems with foregrounds did not get more attention at the time of the original high-profile announcement.

  See here for a Caltech workshop on the BICEP2 results. Andrei Linde's talk started with his complaining about the popular and textbook coverage of inflation. He said that when journalists call and ask him what BICEP2 will tell us about Grand Unification, he responds "nothing". At the end of the talk, Sean Carroll asked him about the multiverse, with Linde's response emphasizing what a great thing it is to have a theory that can't be disproved:

  > If you cannot disprove it, then you have this powerful weapon of thinking about and explaining things around you in an anthropic way.

- This coming week here in New York there will be lots of events associated to the World Science Festival. One aimed not so much at a popular audience that I'll likely attend will be a day-long Symposium on Evidence in the Natural Sciences, which will be at the Simons Foundation. It will end with a discussion between Jim Baggott (author of the recent Farewell to Reality) and Brian Greene (sold out now I fear).

Update: The Princeton crowd now has a preprint out, with the detailed argument that BICEP2 can't distinguish "gravitational waves" from "galactic schmutz", see here.

Comments

1. **Bernhard**
   May 25, 2014

   The anecdotal arguments Linde uses to "defend" eternal inflation at the end of the talk are something else. Who needs to confront experimental data when you can talk about black and white footballs?

2. **Curious Mayhem**
   May 25, 2014

   Indeed. The tensor polarization result doens't prove or disprove eternal inflation; it is consistent with it, but consistent with some other kind of inflation.

   The disappointment about eternal inflation is that it excludes overt quantum gravity considerations almost by design, while dovetailing with GUTs, of course. Avoiding the emptiness of string cosmology, quantum gravity means a Wheeler-DeWitt equations that includes the inflaton. Shades of the 1980s. This is more interesting scientifically.

3. **TonyK**
   May 26, 2014

   Your "Farewell to Reality" link is missing a colon.

4. **Neil**
   May 26, 2014

   Was Linde's comment made with his tongue in his cheek, I hope? I find it hard to believe he seriously thinks that a theory that cannot be tested, even in principle, can be explanatory.

5. **Peter Woit**
   May 26, 2014

   Neil,
   You can judge for yourself, the exchange with Carroll starts at about 1:49:35 in the video I linked to. He sounds serious to me…

   TonyK,
   Thanks, fixed.

6. **Martin L**
   May 27, 2014

   Another quick link(s). Sadly, I saw that Prof. Gerald Guralnik passed away last month. Further trimming the number of Higgs theorists who are still alive.

   Brown Univ Coverage:

http://news.brown.edu/pressreleases/2014/04/guralnik
http://www.browndailyherald.com/2014/05/01/death-physics-prof-remembered-mentorship-imagination-contributions-nobel-winning-work/

NYT and Wash Post Coverage:
http://www.nytimes.com/2014/05/04/us/gerald-guralnik-77-a-god-particle-pioneer-dies.html
http://www.washingtonpost.com/national/health-science/gerald-s-guralnik-particle-physicist-linked-to-higgs-boson-dies-at-77/2014/05/06/5cafa35e-d4c9-11e3-95d3-3bcd77cd4e11_story.html

7. **Nick Gresham**
June 2, 2014

Would the slides of Bernstein's talk be available?

8. **Peter Woit**
June 2, 2014

Nick,
Unfortunately Bernstein's slides haven't been posted, perhaps someone can get them from him, or get one of the organizers like Pavel Etingof to do so.
On the other hand, Arthur's slides are now there, so people can see more about his comments on functoriality and the Weil explicit formula.

9. **A.K.**
June 15, 2014

indeed the 'quasiclassical' approach to the RH has a hidden Langlands-like structure; it is a mystery to me that nobody saw it until now (apparently).

This Week's Hype

May 25, 2014
Categories: This Week's Hype

About every three years KEK issues a hype-filled press release announcing that Jun Nishimura and collaborators have used a supercomputer to get evidence for string theory. Back in 2008, the announcement was of a numerical simulation on a supercomputer of a supersymmetric QM system that supposedly showed that superstring theory explained the properties of black holes (press release here, preprint here, blogging here). In 2011, the claim was of a numerical simulation on a supercomputer that used superstring theory to understand the birth of our universe (press release here, preprint here, blogging here). Both of these papers were published in PRL.

The 2014 press release is now out (see here), based on this preprint from last December. The latest claim is that the authors have solved the black hole information paradox, have shown that we live in a hologram, as well as showing that string theory provides a self-consistent quantization of gravity, all by doing a numerical simulation of a QM system. Even better, they have made the quantum gravity problem just as well-understood and tractable as QCD:

> In short, we feel that problems involving quantum gravity have become as tractable as problems involving the strong interaction. The latter can be studied by simulating a gauge theory on a four-dimensional (4D) lattice, and such a method has recently been used to reproduce the mass spectrum of hadrons (28) and the nuclear force (29). We can now apply essentially the same method to study quantum gravity, which has been thought to be far more difficult.

This latest version of the KEK-hype has gotten a lot more attention than the previous two versions. Based on the preprint, late last year for some reason Nature covered this with a story about how Simulations back up theory that Universe is a hologram and this got a lot of media attention (see here for example).

The paper has now been published, and this time it's not in PRL, but in Science magazine (submission there was a month after the preprint came out, could it be that PRL wouldn't have it?). Science is giving it a high profile, including together with it a piece by Juan Maldacena, which claims the paper as "further evidence of the internal consistency of string theory". Science provides the following one-line summary of the Maldacena piece:

> A numerical test shows that string theory can provide a self-consistent quantization of gravity.

One obvious problem with this is that even if you take the most optimistic view of it all, what is being described is quantum gravity in 10d space-time. The Japanese authors deal with this problem with a footnote:

> Theoretical consistency requires that superstring theory should be defined in ten-dimensional space-time. In order to realize our four-dimensional space-time, the size of the extra six dimensions can be chosen to be very small without spoiling the consistency.

Remarkably, Maldacena has another answer: the multiverse, which now he seems to take as accepted fact.

> Of course, the 10-dimensional space under consideration here is not the same as the four-dimensional region of the multiverse where we live. However, one could expect that such holographic descriptions might also be possible for a region like ours.

Absurd hype about string theory is a continuing problem, and it's not one that can be blamed on journalists, with this latest example getting help from HEP lab press releases, a highly reputable journal, and an IAS faculty member.

Comments

1. **rfp**
   May 25, 2014

   From what I can tell, these aren't simulations (in the usual sense). There is no dynamics, nothing is evolving. They simply used a computer to compute a partition function. The integral is too hard to compute analytically so they discretized it and evaluated it numerically. From what I can tell, that's the only reason a computer was involved. As there is no Hawking radiation or dynamics in the calculation, I don't think it gives any direct insight into the information problem.
   …
   As for the result, maybe I can try to summarize what they did and someone more knowledgeable can correct me. On the "gravity side" they considered a 10 dimensional black hole. It is a solution of Einstein gravity with some extra higher curvature terms in the action. They computed this black hole's Hawking temperature and entropy in the usual way, and then they integrated dE=TdS to find its internal energy as a function of temperature, E(T).
   …
   On the "gauge theory side," they considered a specific gauge theory action and computed its partition function, Z, in the usual way. Then they took a derivative with respect to inverse temperature to get E(T) = \partial_\beta Z. They showed that this matches the E(T) of the black hole. It's another consistency check of AdS/CFT.

2. **lun**
   May 25, 2014

   rfp, they are simulating a quantum state numerically. If they did everything right, the partition function is all the dynamics they need.

   That said, I have another issue: if you read this paper, they achieve something conventional lattice theorists are _very_ far away from doing (simulating a supersymmetric theory, with Fermions and everything) using a tower of assumptions:
   the field theory is reduced to quantum mechanics, the regulator is a momentum cutoff, fermionic complex phases are ignored, a complex partition function is artifically truncated, etc.

   Peter, perhaps, given your expertise, you could comment on whether these assumptions are plausible from a numerical point of view.
   I am disturbed that the only calculations made with this method are the ones which "test the theory". No exact solution or toy model exists which shows that the approximations are under control.

   I am sure Science has conducted refereeing, but on the other hand Maldacena is not a lattice theorist, and no other research group working in numerical field theory is using this approach and nd some of them are working on supersymmetric problems.

3. **Curious Mayhem**
   May 25, 2014

   This sounds very dubious and not different from what Peter was complaining about. There's no control in their approximations, and they've simply skipped over all the hard parts that keep the lattice gauge theory people sweating. That amounts to cheating and then making crazy claims afterwards.

It certainly does nothing to prove string theory, or even that such a theory exists. It is merely another check (if it can be called even that) on the CFT on AdS space. Everything else is conjecture.

4. **Geoff**
May 26, 2014

On a somewhat related aside, Renate Loll just gave a talk at Perimeter advocating for greater utilization of numerical simulations in checking various solution spaces of competing theories of quantum gravity. Interesting and worth watching (in my opinion).

http://pirsa.org/14050138/

5. **theon**
May 26, 2014

Dear Peter,
you go too fast. I am still recovering from a lose cable that made neutrinos exceed the speed of light. And of a false understanding of a snapshot of a map presented in a meeting that proved inflation at 2 10^16 GeV. I must admit that the former would have broken all bounds, while the latter extrapolates no more than a mere 29 orders of magnitude from its observations at 3K. Now you come with stories that we live in a 10d multiverse, string theory is proven, quantum gravity is established. Is anybody wondering which questions, if any, we will leave for posterity?

6. **QG**
May 27, 2014

One obvious problem with this is that even if you take the most optimistic view of it all, what is being described is quantum gravity in 10d space-time.
^
if this is the most optimistic view of it all are there other less optimistic views and theoretical reasons not to be optimistic?

7. **Yatima**
May 27, 2014

…and then it turns out they put in the wrong sign in a forgotten corner of badly reviewed FORTRAN crud.

8. **FNesti**
May 27, 2014

Is anybody wondering which questions, if any, we will leave for posterity?

Don't fear, if we go on like this, there'll be lots of questions of physics we leave them, and actually we'll add some more – like the important one: "How could this happen?"

9. **Curious Mayhem**
May 27, 2014

(After looking at the papers) Much less than the hype. As many have pointed out, any semiclassical theory of gravity has to produce the general form for black hole entropy. The numerical calculation gets you, in addition, some dimensionless factors for that particular model. Not much new. It certainly doesn't prove string theory, whatever that would mean.

Comparing quantum gravity theories through numerical simulations is not a bad idea IF the approximations are controlled. We do need to shake off another piece of hype, that simulation constitutes a "third branch" of science, along side Experiment and Theory. Actually, simulation (if done right) is just another part of Theory. It's another set of approximations leading to approximate answers. If done without control over the approximations, simulation is just a poorly constructed "model," or really, just Bad Theory.

10. **Shantanu**
May 27, 2014

Peter, something OT.
See this nice paper by Avi Loeb on how there were prejudices against new ideas(which later turned out to be correct) which hurt the subject
http://arxiv.org/abs/1405.2954
Can you (or someone else) think of similar examples in particle physics

11. **Jack Lothian**
May 27, 2014

Shantanu
All humans are biased, it is in our nature. For every case of someone rejecting a valid new idea because of prejudices against new ideas, there are many more cases of someone rejecting a valid old idea in favour of their new idea that later turns out to be wrong. Cherry picking cases studies to support your thesis is a form of bias & is a very unscientific way to conduct research.

Off topic – I know – sorry

12. **jd**
May 29, 2014

Check out Nature for this week. I no longer admit to being a theoretical physicist.

13. **Marcus**
May 30, 2014

jd, when you say "check out Nature for this week" I assume you refer to the article by Amanda Gefter
Theoretical physics: Complexity on the horizon
A concept developed for computer science could have a key role in fundamental physics — and point the way to a new understanding of space and time.

At the moment I do not have ready access to the article, can you tell us a bit about it?

14. **jd**
May 30, 2014

Marcus

Yes, you have the correct article.

Let’s see if I can summarize this. The news article quickly goes over Susskind’s latest ideas on how to solve the “firewall problem” and how to go much further. Susskind is apparently wearing a T shirt these days that says “I heart Complexity,” with the heart being a Mandelbrot set, or as much as you can get on a T shirt. The complexity is referring to computational complexity. Black holes, worm holes, computational complexity, AdS/CFT, and more are all invoked in the picture. Basically, the firewall does not exist (it is not needed) because the information just outside the event horizon is too complex to grab and take inside the event horizon. Then you can not violate the no-cloning theorem. The article goes on to mention that there may be connections between the length of a wormhole and complexity and, therefore, also time. Furthermore, entanglement is somehow related to space. Susskind is quoted. “I do not know where all of this will lead. But I believe these complexity-geometry connections are the tip of an iceberg.” I wonder if the lookouts on the Titanic said something similar.

Oh, there was also, “Things fall because there is a tendency toward complexity.”

15. **Lun**
May 31, 2014

Jd: this is hilarious… Perhaps peter can do a post about
Fred kantor, a curious character (former phd student) who hangs around
The Columbia physics department advertising his theory
Known as “information dynamics”
It’s basically this article, the same ideas and the same level of “rigor”

16. **Davide Castelvecchi**
May 31, 2014

Amanda Gefter’s story, like all of Nature News’ content, is available to read for free. Here’s the link:
Theoretical physics: Complexity on the horizon

17. **johnnythelowery**
June 3, 2014

David Castelvecchi: That link is disturbing! Looking forward to our host’s views on the ideas.

18. **Neophyte**
June 9, 2014

What’s wrong with the idea that computational complexity has a role in physics?

Big Bang Blunder Bursts the Multiverse Bubble

June 4, 2014
Categories: Multiverse Mania

This week's Nature has an article by Paul Steinhardt, with the title [Big Bang blunder bursts the multiverse bubble](). The subtitle of the piece describes the BICEP2 frenzy of last March as "premature hype", and the description in the body of the article is:

> The results were hailed as proof of the Big Bang inflationary theory and its progeny, the multiverse. Nobel prizes were predicted and scores of theoretical models spawned. The announcement also influenced decisions about academic appointments and the rejections of papers and grants. It even had a role in governmental planning of large-scale projects.

Given recent arguments that BICEP2 may be seeing dust, not primordial gravitational waves, the March media frenzy quite possibly was highly premature, if not completely misguided. Steinhardt goes on to argue that in the future

> announcements should be made after submission to journals and vetting by expert referees. If there must be a press conference, hopefully the scientific community and the media will demand that it is accompanied by a complete set of documents, including details of the systematic analysis and sufficient data to enable objective verification.

He also takes the occasion to note the odd fact that while BICEP2 results have been claimed to be proof of inflation and the multiverse, if they turn out to be wrong, that's fine too:

> The BICEP2 incident has also revealed a truth about inflationary theory. The common view is that it is a highly predictive theory. If that was the case and the detection of gravitational waves was the 'smoking gun' proof of inflation, one would think that non-detection means that the theory fails. Such is the nature of normal science. Yet some proponents of inflation who celebrated the BICEP2 announcement already insist that the theory is equally valid whether or not gravitational waves are detected. How is this possible?
>
> The answer given by proponents is alarming: the inflationary paradigm is so flexible that it is immune to experimental and observational tests. First, inflation is driven by a hypothetical scalar field, the inflaton, which has properties that can be adjusted to produce effectively any outcome. Second, inflation does not end with a universe with uniform properties, but almost inevitably leads to a multiverse with an infinite number of bubbles, in which the cosmic and physical properties vary from bubble to bubble. The part of the multiverse that we observe corresponds to a piece of just one such bubble. Scanning over all possible bubbles in the multi-verse, everything that can physically happen does happen an infinite number of times. No experiment can rule out a theory that allows for all possible outcomes. Hence, the paradigm of inflation is unfalsifiable…
>
> Taking this into account, it is clear that the inflationary paradigm is fundamentally untestable, and hence scientifically meaningless.

Steinhardt was on a panel last Friday night here in New York at the World Science Festival, which can be watched [here](). The panel included Guth and Linde (who earlier in the week got $1 million for their work on inflation), as well as John Kovac of BICEP, and Amber Miller, Dean of Science here at Columbia. The last part of the video includes an unsuccessful attempt by Steinhardt to pin down Kovac on the significance of the BICEP2 evidence for primordial gravitational waves claim, as well as an exchange with Guth and Linde. They both defend inflation as the best model of the alternatives.

Multiverse promotion continues apace, with Steinhardt one of a rather small number of physicists publicly objecting. On Monday Alexander Vilenkin will explain to the public at the American Museum of Natural History that "the Big Bang was not a unique event in cosmic history and that other Big Bangs constantly erupt in remote parts of the universe, producing new worlds with a great variety of physical properties" (see [here]()). A [recent story on livescience]() has Brian Greene on the multiverse. Over at Massimo Pigliucci's Scientia Salon Coel Hellier is starting a multipart series arguing against multiverse skeptics with [The multiverse as a scientific concept — part I](). Nothing in Part I about the problematic issues (untestable claims that fundamental physics is "environmental"), maybe in Part II…

Comments

1. **AcademicLurker**
   June 4, 2014

   After watching how carefully managed the release of the LHC results was, I was kind of surprised at how they jumped the gun with BICEP2. I know they're completely different projects and organizations, but still…

   Maybe the BICEP2 team could take some PR lessons from CERN.

2. **Florin Moldoveanu**
   June 4, 2014

   IMHO the paper in Nature is too harsh. See here the reaction of Andrei Linde to the original BICEP2 news:

   https://www.youtube.com/watch?v=ZlfIVEy_YOA

   Pay particular attention to 1:57-2:14 This is the reaction of an intellectual honest person. The original excitement and the media hype on BICEP2 was premature, but I feel the Nature paper is also a knee jerk reaction but in the opposite direction.

3. **anonymous**
   June 4, 2014

   The BICEP2 youtube video with Andrei Linde was so awkward. He seemed wise enough to be skeptical of the results, yet was forced to act nice and happy because people brought this celebration to his front door and video taped it.

4. **Peter Woit**
   June 4, 2014

   Florin/anonymous,
   Linde has inflationary models with any value of r you want, so he had no particular reason to set great store in the BICEP2 number being right. This is the same Linde quoted by SciAm as saying
   "suddenly we have something which I firmly believe is experimental evidence in favor of the multiverse," Linde says. "Those people who say the theory of the multiverse

does not have any experimental confirmation have not paid enough attention."
I noticed that when engaged in discussion with Steinhardt he doesn't say nonsense like that, sticks to the "my theory sucks less" argument.

5. **Peter Woit**
June 4, 2014

AcademicLurker,
I think of lot of the questionable choices BICEP made can be attributed to their competition with Planck. A defensible claim from BICEP would have been "we have observed the right kind of B-modes for the first time, but we'll need Planck's results to know if this is dust or primordial gravitational waves." Instead they went for what now seems a not very solid argument that "we don't need Planck", and a claim for a Nobel for themselves.

It would have gotten very interesting at CERN if ATLAS and CMS had very different characteristics, and one of them saw something that could be the Higgs, but knowing this would require a study of backgrounds that only the other could do. Things would have gotten interesting…

6. **physicsphile**
June 4, 2014

Peter,

I think you and Steinhardt are being a bit harsh. The key thing is that we can learn about inflation by doing experiments. Any improved measurement of r will cut into the space of possible potentials for the inflaton field. If r is sufficiently large then we can hone into a very small subset of possible potentials by using improved observations. So I don't think its right to say inflation is just philosophy. Observations have and can further be used to get a better understanding of how inflation happened. Inflation can be falsified if a better theory is found that predicts new observations which only a very contrived version of inflation could produce. It would be great if one could take a simple Popperian view on falsability but I think the reality of how science actually works is a bit more complex.

7. **Curious Mayhem**
June 4, 2014

The BICEP2 hype may be have been premature. But proof of inflation is not in any way proof of the "multiverse," which is unprovable by its nature.

Inflation is by the far the best extension of the hot Big Bang we have. While it's consistent with Linde's "eternal inflation," it doesn't require it.

And, again, inflation has nothing whatever to do with the multiverse in the "landscape" sense.

8. **Person**
June 5, 2014

No matter if the BICEP2 results turn out to be true or not, they are not good science. Therefore, the claim that the hype was premature is quite accurate and not too harsh at all. Publications that are based on scraping the presentation slides of other groups should be flat out rejected. That is no clean process and should not be allowed within the science community.

On an other note, I dont understand how one can conclude from inside our universe on the existence of other universes or bubbles. That makes no sense. We are unable to understand how the natural constants come to their values, even if we accept the (any) inflation model to be correct. Taking our not-understanding as proof of a multiverse concept is grotesque. It is religious at best, comparable to the invention of a creator of the world as a model for the existence of the world in ancient times. The multiverse is in its nature not falsifiable. It should have no in science.

9. **Garbage**
June 5, 2014

There're so many misleading statements in Steinhardt's article there's no point really to argue. One should write a similar argument complaining about how he's changing his mind every time a new bound on local non-Gaussianity comes about… We all know inflation makes well defined predictions, however, as usual with physical theories, one has to explore parameter space. One could have said the same about the Higgs 40 years ago, until experiments pushed to the ultimate "limit" and found it, or whatever else would've replaced it. We know there's a GW signal at large angles from inflation, gravity is universal, that's why this is so exciting! Moreover, we know there's an ultimate level of non-Gaussianity with a very specific shape, provided inflation happened with vacuum fluctuations. Under very mild assumptions we have more than enough observables to test the theory. So far inflation is the only framework where we can *calculate*, the scale invariance of the perturbations comes out beautifully and many well motivated assumptions make predictions than can be tested, like a large value for r. I don't see how else are we supposed to do science. Inflation is not a unique theory, so isn't the standard model for that matter, no one has ever found a way so far to predict the values of masses and mixing angles. This is not a very constructive article, on the contrary. We all know we don't understand Lambda and the hierarchies in physics, but proposing no alternative explanation and bashing the attempts which have produced the most amazing fit to the data so far, is just absurd IMHO…

10. **Patrick M. Dennis**
June 5, 2014

Peter, is it your opinion that inflation resides in the same "not even wrong" boat as string theory and multiverse(s)? …. Also, do you find the Everett version of the latter to be equally "not even wrong"?

11. **Anonyrat**
June 5, 2014

Why worry? In some alternate universe, multiverse is recognizably not science.

12. **Peter Woit**
June 5, 2014

Patrick M. Dennis,
About inflation, see some comments in the latter part of
http://www.math.columbia.edu/~woit/wordpress/?p=6782
"Many worlds" is a completely different (off)-topic.

All,
I'm getting on a plane soon and will be traveling for a few days, unclear how much I'll be able to deal with the blog. Please don't feed the trolls…
If you want to discuss this topic, I highly recommend the Scientia Salon site I linked to, where Massimo Pigliucci does a great job of moderating the discussion.

13. **Jeffrey Herrmann**

June 5, 2014

Steinhardt argues "Scanning over all possible bubbles in the multiverse, everything that can physically happen does happen an infinite number of times. No experiment can rule out a theory that allows for all possible outcomes."
Then why not quit. If every possible experiment is incapable of distinguishing between Theory X and the theory of eternal inflation because even if Theory X is right, both theories predict the same observed data, what is the point of continuing to observe more and more data?

14. **TomH**
June 5, 2014

is Steinhardt still a strong proponent of the "Ekpyrotic" big bang model?
If so, it surely accounts for part of his harsh criticism of inflationary models.

But Ekpyrotic model relies on existence of unobserved and/or unobservable higher -order dimensions , "branes" floating in the "bulk" , and such — yes?

To this layman, that seems almost as speculative and unfalsiable as any multiverse hype … although if BICEP2 results are validated, doesnt that falsify Ekpyrotic models??

15. **Monty**
June 6, 2014

I watched the video from the World Science Festival. Could I ask Peter, or the commenters, what would be wrong with the following response to Steinhardt's complaint that whatever the BICEP2 results had been, they could be made to fit with some variant of inflationary theory: yes, the observation of B-mode polarisation (let's assume it's not an artifact of foreground dust–this will be shown one way or the other soon in any case) cannot by itself prove inflation. But it lets us choose appropriate candidate theories from within the previous set of inflationary theories, and, more importantly for inflation-backers, it adds another item to the list of things requiring explanation for any competing theory. So now we have not only isotropy, flatness, absence of relics, and large-scale structure to explain, but we also have otherwise unexplained B-mode polarisation of the CMB. An appropriately narrowed inflation model can account for all of those things at once; that makes it correspondingly harder for an alternative theory to be equally successful. Doesn't that make it a stronger theory than it was this time last year?

16. **JG**
June 7, 2014

Monty

Not really, if the observable B-mode signal was a *favoured* prediction of inflation then the BICEP2 observation would certainly have a strong impact on the credibility of inflation. But there are many models of inflation which predict a non-observable B-mode polarisation, so the BICEP2 result just narrows the parameter space for inflation models – it doesn't say anything definitive about their truth – despite all the hype about a "smoking gun".

TomH

Steinhardt has admitted the Ekpyrotic model is dead if BICEP2 are correct.

Jeffrey Herrmann

Because Steinhardt doesn't believe the multiverse is correct, *that* is why there is still a point in making observations and doing science!

Garbage

Maybe if they didn't (practically) claim the scientific debate is over and award million pound prizes in hollywood style ceremonies to the "victors" the opponents might not make such a public "fuss". Pointing out obvious deficiencies in the science is a necessary, and useful public service. 🙂

17. **Monty**
June 7, 2014

JG – thanks, but I acknowledged all that you said in my comment. Let me try again: the BICEP2 observation doesn't confirm anything, because the range of inflationary theories would have encompassed any result of those observations. But isn't it true that (if the polarisation holds up as being in the background) it adds to the list of things that inflation takes care of, and that a competing theory would need to address? I thought inflation was widely held to be an appealing theory because it explained so many things at once. Now it explains "so many plus one". A competing theory now doesn't just have to account for flatness, isotropy of the CMB and large-scale structure, and the lack of magnetic monopoles, but it also has to explain where the heck these swirls in the polarisation of the CMB come from.

18. **JG**
June 7, 2014

Monty

Ok, sure, inflation was a pretty nice theory already before BICEP2 announcement. It's just that most of the popular inflation models inspired by string theory disfavoured an observable B-mode signal. So I don't see how an observation by BICEP2 which wipes out the majority of inflation models is a good argument in favour of inflation. In fact, ironically, it may lead to a non-inflationary model like string gas cosmology becoming a serious contender, and note that sgc solves a really big problem that inflation does not – why 3 large spatial dimensions?

Even if Planck raise major doubts about the BICEP2 observation the string theorists have loads of *other* inflation models which "predict" weak B-modes.

So although I think inflation is a nice (and even compelling) idea, I'm not convinced BICEP2 should have been sold to the public as confirmation of it.

But I also agree, that any competing model (such as sgc) must solve the problems of flatness, isotropy, lack of relics at least as cleanly as inflation does.

19. **Neophyte**
June 7, 2014

What is a "relic"?

20. **Mark**
June 7, 2014

I was astonished by Steinhardt's comments. I have great respect for him, as one of the few physicists who has made major contributions in both cosmology and condensed matter physics (quasicrystals). But he seems to have jumped the shark with his statement that the unfalsifiability of inflation removes it from the scientific paradigm.

Paul (and I hope you read this): Supersymmetry makes many predictions and is the basic of most particle physics research over the past 25 years. Yet if it is not seen in the next run at the LHC, many of its proponents will focus on slivers of remaining parameter-space, or make small modifications. Thus, low scale supersymmetry isn't really falsifiable. Does that mean you believe that supersymmetry is not part of the scientific paradigm?

21. **Monty**
June 7, 2014

Neophyte: relic particles would be the surviving exotic particles predicted by hot big bang models to have been created in the extremely energetic conditions of the very early universe. Magnetic monopoles are one type of relic. One thing inflation does is explain why we don't actually observe any such thing.

22. **Yatima**
June 8, 2014

One thing inflation does is explain why we don't actually observe any such thing.

Not really. Explaining that unicorns live in faraway lands over the rainbow "explains" why we don't actually see unicorns in the woods. But in very wrongheaded away.

23. **Monty**
June 8, 2014

Yatima: would you say it's incorrect that hot big bang theories without inflation predict exotic relics?

Because if they do, then your analogy isn't very fair. There's no reason to believe we should find unicorns in the woods in the first place.

24. **GoletaBeach**
June 8, 2014

"And this time, the announcements should be made after submission to journals and vetting by expert referees."

Has the Flauger/Hill/Spergel paper been vetted by expert referees?

Sometimes theories have unknown parameters like r… r didn't/doesn't have to be big to support inflation.

Just re-read the story of some of the first new scientific results from E.O.Lawrence's program of cyclotrons circa 1933. His team totally messed up the parameters of the deuteron & neutron… they thought the neutron was lighter than the proton. Rutherford's team corrected them with results from a linear accelerator. The Berkeley team eventually got it right, and indeed, circular machines dominated particle physics until the present (OK, there is SLAC too).

So BICEP2 maybe messed up. Big whup. They pushed the envelope. Wouldn't be the first time something went wrong, but they still deserve commendation and respect for getting the envelope moved forward.

Without hype, there will be insufficient funding. The real world knows this. The real world also knows that overly strong bloodletting over mistakes is counterproductive.

25. **Peter Woit**
June 8, 2014

About the argument that inflation "explains" the non-observation of magnetic monopoles. One should keep in mind that magnetic monopoles are not expected in the standard model. The lack of magnetic monopoles is a problem that only occurs in various GUT models. Given the lack of success of GUTs on all other fronts (e.g. no proton decay) there's no need for an "explanation" by inflation of why we don't see magnetic monopoles, given the much simpler explanation that GUTs don't work.

26. **Monty**
June 9, 2014

Peter, thanks, I wasn't aware of that and retract my suggestion that Yatima's unicorn analogy was unfair! I'm assuming it's still fair to say that inflation provides a nice explanation for those other things (large-scale structure, homogeneity of the CMB, flatness).

27. **Rob Heusdens**
June 10, 2014

If we are comparing Inflation theory and String theory, then at least I would say that inflation theory makes testable predictions which after observation confirm the predictions. There are different models, each have different properties and lead to different observables, so we can rule out some models. That is far better as we can think of in String theory.
Inflation theory is not saying in first instance that 'there is a multiverse' and/or that 'any outcome is correct', but that comes as a by product of it. It was not 'invented' for that reason. Even so, a theory could in principle be correct, without being thoroughly falsifiable. All claims inflation theory makes about other universes are untestable. Only the claims inflation makes about our universe we can test and see if they conform to that model of inflation. Standard science practice. We should not by definition rule out a theory because it makes more predictions then can be tested for.

Past the End of Science

June 8, 2014
Categories: Multiverse Mania, Uncategorized

I haven't yet seen a copy of Marcelo Gleiser's new book, but this weekend the Wall Street Journal had a review by John Gribbin, author of the 2009 multiverse-promotional effort In Search of the Multiverse. I don't know how Gleiser treats this, but Gribbin emphasizes the multiverse as new progress in science (for some reason he's now calling it the "metaverse"):

> Within the metaverse, the story goes, there are regions that form inflating bubbles. Our universe is one such region or bubble. As Mr. Gleiser explains, the implication is that there are other universes, other bubbles far away floating across an inflating sea.
>
> This seemingly speculative idea counts as a genuine scientific hypothesis, because it makes testable predictions. If other "bubble universes" exist in the metaverse, it is possible that, long ago, one or more of them may have collided with our universe, like two soap bubbles touching and moving apart. One effect of such a collision, Mr. Gleiser points out, would be to make ripples in the space of both bubble universes; they would leave a distinctive if faint ring-shaped pattern, known as a "cosmic wake," in the background radiation that fills the universe. Data from the Planck satellite is being used to test this prediction right now. Is the metaverse real? We may well know in the next year or so.

This seems to be a reference to work by Matthew Kleban and collaborators, which I saw Kleban talk about recently (see here). My impression from that talk is that the actual state of affairs with Planck is that it has already looked for and ruled out most hoped-for signals of "bubble collisions". I don't know anyone besides Gribbin who believes that the next round of Planck data is going to answer the question "Is the metaverse real?".

The really odd thing about the review is that Gribbin uses the multiverse to argue that John Horgan's claims about physics in *The End of Science* are wrong. This is just bizarre. Gribbin and his multiverse mania for untestable theories provides strong ammunition for Horgan, since it's the sort of thing he was warning about. Actually, I don't recall anything in Horgan's book about the multiverse, and suspect the idea that physics would end up embracing such an obviously empty idea was something that even he didn't see coming. As the multiverse mania gains strength, physicists are blowing past the "End of Science" to something that has left conventional science completely behind.

Update: I took a look again at a copy of *The End of Science*, and, as I remembered, the chapter on "The End of Physics" has no mention of the multiverse pseudo-explanation of why one can't ever understand the parameters of the Standard Model. Horgan ends the chapter with a vision of physics descending into "ironic science", endlessly studying untestable string theory models and interpretations of quantum mechanics. With the multiverse we may already have gone past that point.

In the next chapter though, "The End of Cosmology", there's a long section about Linde and his "self-reproducing universe theory", so Horgan more than 20 years ago already was writing about the place we're ending up. I was interested to see the comment he got at the time from Howard Georgi about this kind of model:

> quite amusing. It's like reading Genesis.

Georgi also is quoted as describing inflation as:

> a wonderful sort of scientific myth, which is at least as good as any other creation myth I've ever heard.

Of course what is different now is that 20 years ago the theory establishment saw Linde's multiverse as kind of a joke, not at all part of science. Things have changed…

Update: While my favorite local bookstore doesn't have a copy of the Gleiser book *The Island of Knowledge*, you can see parts of it on Google Books. Searching on "multiverse" you can read chapters 15 and 16 of the book which deal with the issue of the testability of the string theory multiverse. Reading these shows that Gribbin seriously misrepresents what Gleiser has to say about the multiverse. The context of his discussion of "Cosmic Wakes" and the possibility of seeing them in the Planck data is to argue that even if this happened (which he describes as having an "extremely small" probability), all that would show is evidence for a neighboring universe, not a multiverse:

> However, I stress again that even a positive detection of a neighboring universe would not prove the existence of a multiverse. Within the present formulation of physics the multiverse hypothesis is untestable, however compelling it may be.

Comments

1. **M**
   June 9, 2014

   A collision between two bubble-universes would have left huge ripples, unless their vacuum energies happen to be quasi-degenerate: searches ate done for this atypical fine-tuned scenario of small ripples because the typical scenario was already excluded.

2. **Pacific**
   June 9, 2014

   The evidence crisis

3. **J**
   June 9, 2014

   "the next round of Planck data": isn't the Planck mission over? (wikipedia says it was deactivated in October 2013)
   Or if there is still some data to be exploited, what is it?
   Thanks

4. **Peter Woit**
   June 9, 2014

   J,
   Planck is still at work on the data analysis, especially on the polarization data, with rumors of release set for October. There is a lot of interest in this, around the question of whether there will be evidence for primordial B-modes in this data. Gribbin is the only person I know of though who seems to think that "cosmic wakes" from other universes, not seen in the rest of the Planck data, will somehow show up in the new polarization results.

5. **Avattoir**
   June 9, 2014

In addition to writing about Linde in his book, Horgan's discussed the context at least three different times I specifically recall (my impression is of even more) with science writer George Johnson on their occasional appearances together on Bloggingheads.tv (all of which can be retrieved at its current website).

One of the problems I have with Horgan on Linde is that he always has seemed to have trouble separating Linde as a social person from Linde as a scientist. He'd met Linde several decades back in the context of a sort of socializing that's not unusual around conferences in places like Aspen, and what struck Hogan was the image of Linde as a gregarious party animal full of jokes and magic tricks. Hogan's written and oral descriptions of his interviewing Linde combine that image with Linde's speculations on the existence of a multiverse. The things Linde's done otherwise, Horgan has never really dealt with (and to me he's neither equipped nor inclined to discuss anyway), particularly Linde's academic and research work on inflation in Russia more than a decade before Guth's first paper on it was published, as well as Linde's work in the U.S. in the 1980s following publication of Guth's paper, which corrected a number of shortcomings in Guth's hypothesis and otherwise expanded on its implications.

6. **Peter Woit**
June 9, 2014

Avattoir,
I just reread earlier today the section of Horgan's book dealing with Linde. He certainly does deal with Linde's scientific work on inflation, in Russia, and post-Guth. Yes, his treatment of this is non-technical, but no more so than any of the rest of the book, which is not a technical book.

The material there on Linde does portray him as a bit of a joker, but it's clearly there for color (this is a popular book, you want to keep people awake), and to give a bit of context to the things Linde says. For instance he quotes Linde going on about how maybe the universe was created by some cosmic engineer who created it in just such a way that a message for us to read would be encoded in the CMB. Without a context for Linde's sense of humor, this would just come off as making Linde look crazy.

7. **abbyyorker**
June 9, 2014

Georgi calls inflation an amusing myth? Surely the scientific consensus is that inflation is a strong candidate for how the universe operates. Is Georgi referring to something else or is his comment out of date?
Sry…Im just a layman who takes most everything literally.

8. **Peter Woit**
June 9, 2014

abbyyorker,
Georgi does call it not an "amusing" myth, but a "wonderful" and "scientific" one. This was in the early-mid 1990s, after inflation had been around for a decade or so, and hadn't lived up to its promise as telling us something about GUTs. I'd guess Georgi was referring to the lack of any compelling prospects at the time for testing the idea. I don't know whether the more recent claims about connection to CMB experiment have changed his mind, or whether he's in the Steinhardt camp on this.

9. **Curious Mayhem**
June 9, 2014

Georgi's take on inflation is all about the failure of GUTs, which he and Glashow started in the 1970s. From the point of view of cosmology, inflation is a perfectly viable and testable class of theories.

Horgan's vision of an increasingly empty academic theoretical physics, titillated by speculative manias, has turned out to be depressingly relevant.

10. **Bee**
June 10, 2014

I got a review copy of Gleiser's "Island of Knowledge" and I'm presently reading it (half through). I will probably have a review on my blog within the next weeks.

11. **Roger**
June 10, 2014

In March, Horgan wrote [Why I Still Doubt Inflation, in Spite of Gravitational Wave Findings]. He also posted [My 1992 Profile of Cosmic Trickster and Inflation Pioneer Andrei Linde]. So yes, inflation skepticism is part of his end-of-science theme.

12. **Curious Mayhem**
June 11, 2014

It was a good review, but Peter's post might have left a misimpression.

Gribbin uses the term "metaverse" to refer to the larger inflating background à la Linde. It's not the "multiverse" as typically used, but a connected spacetime, and one connected to us. It's likely that Gribbin was consciously avoiding "multiverse" so as to not give aid and comfort to that lunacy. The only error he makes is his implication that inflation requires the Linde-an metaverse or "eternal inflation." It's consistent with it, but Linde's form of inflation is not required.

Gribbin's review eschewed the "multiverse" term altogether and didn't mention string theory at all. I find that pretty telling. Furthermore, Gribbin emphasizes the non- or anti-scientific nature of claims of a "final theory." That's the point of the book's title, that our science is an island (a growing island, to be sure) in a sea of ignorance, probably an allusion to Newton's famous quote.

(The only place "multiverse" occurs in the Journal review is mention of the title of Gribbin's book, "In Search of the Multiverse," in the author blurb at the end.)

13. **TRP**
June 11, 2014

Two would be already a multi-, no? 😉

14. **Yatima**
June 14, 2014

"A collision between two bubble-universes"

What does that even mean? Wouldn't ekpyrotic fire emerge from every single point in space? Or is that another framework? What about if a universe is a holographic representation from a lower dimensional manifold and THESE ram each other in the night? How about if a Tegmark Level III universe rams one that is Tegmark Level

IV? I’m so confused. It’s like reading Perry Rhodan stories.

15. **Roger**
June 14, 2014

Horgan says: “Thanks, Peter. I couldn’t have said it better myself.“

Evidence in the Natural Sciences

June 9, 2014
Categories: Uncategorized

I recently spent a day at the Simons Foundation in midtown, attending a symposium on Evidence in the Natural Sciences. Of the scientific program talks, I got the most out of the one by Thomas Hales on the question of checking the proof of the Kepler conjecture. For the latest on the project to produce a formal, computer checkable version of the proof, see the Flyspeck Project page.

The program ended with a discussion/debate featuring Brian Greene, Peter Galison and Jim Baggott, with the contentious issue basically "has physics gone too far?" in a speculative direction, unable to get back to a point where connection can be made to experiment. Baggott gives a summary of what he had to say at Scientia Salon, which would be a good place to discuss these issues (so I'm leaving comments off on this posting). Both Greene and Galison were much more taking the position that things haven't gone off the rails, that one needs to trust the leaders of the field and the physics community to do the best they can. This blog's readers shouldn't have much trouble guessing which side of this I'm more sympathetic to. I didn't notice the event being recorded, perhaps it was.

The symposium was co-sponsored by the Templeton Foundation, with no theology or religion in sight. I think they're mostly these days keeping the physics/math and theology apart, with this symposium and FQXI two good examples, and I'm happy to see that. My other main complaint about Templeton was always that they were pushing multiverse research since that fit into their agenda. These days I don't see them doing so much of that, with multiverse mania being driven by much more dangerously influential sources. But maybe I'm less critical of them because they invited me to a very nice dinner after the talks…

Update: Videos of the talks are now available here.

No Comments

Large Hadron Collider Physics Conference

June 9, 2014
Categories: Uncategorized

While I was away last week Columbia was hosting the Large Hadron Collider Physics (LHCP) conference here on campus. Talks are available here. Matt Strassler posts about some of the new Higgs results, which basically see some of the inconsistencies in Higgs mass measurements disappearing. Right now everything is quite consistent with a pure Standard Model Higgs.

In the final plenary session, the theory talk from John Ellis ended by claiming the LHC results as a success for SUSY (the "success" is that simple classes of SUSY models said the Higgs couldn't be too heavy, also that the Higgs couplings should be much like those of the SM, so any SM success is a SUSY success). Another argument was that "SUSY is increasingly the best solution that we have" (here he was following Nathaniel Craig's summary talk), illustrated by two discouraged soldiers under bombardment in a foxhole, one saying to the other "Well, if you knows of a better 'hole go to it!". It's interesting to note that the arguments from Ellis work just as well if SUSY is not found at Run 2, a likely possibility he's getting ready for.

The conference included a Friday afternoon panel discussion that you can watch here. This began with an excellent presentation from Fabianola Gianotti about the possibilities for next-generation colliders. The discussion moderated by Dennis Overbye of the New York Times focused to some extent on the budgetary challenges facing US HEP, with Steve Ritz, the chair of the P5 panel, commenting on the tough situation reflected in that panel's recent report. Many speakers expressed frustration over the US budget level for HEP and what to do about it. Enlist the public? Do a better job of convincing Congress? Get billionaires to help fund HEP? Get billionaires to fund a Super PAC that would buy us a Congress with a better attitude?

Most of the discussion was about the experimental side, but Arkani-Hamed also had comments about the theory side, noting that the job market for theorists, which improved after his grad school days in the mid-90s, has now worsened and gone back to that level. He also noted that successful young theorists are increasingly ending up in faculty jobs outside the US after starting their careers as students and postdocs here.

Comments

1. **Manny**
   June 10, 2014

   "He [Arkani-Hamed] also noted that increasingly successful young theorists are ending up in faculty jobs outside the US after starting their careers as students and postdocs here."

   Was the point that other countries are successfully luring away American-trained talent? or that even these successful young theorists can't find positions here and must leave the US for jobs?

2. **paddy**
   June 10, 2014

   Manny: It is for you to make of Arkani-Hamed's statement what you will.

3. **Peter Woit**
   June 10, 2014

   Manny,
   You can find some heated argument about this in the comment section here:
   http://www.math.columbia.edu/~woit/wordpress/?p=6398

   That some of the best young Princeton-trained string theorists are ending up with jobs outside the US is what I think Arkani-Hamed is referring to. I described this situation provocatively as US physics departments not buying what Princeton is selling. I think an accurate description of the situation is that the people in question could get jobs in the US, but are offered better ones outside the US. One can argue about why that is.

4. **Curious Mayhem**
   June 11, 2014

   It's combination of things. But what Arkani-Hamed is referring to specifically is that US physics departments stopped their hot pursuit of string/M-theory/multiverse hiring around 2007. Possibly not coincidentally, this happened about a year after Peter and Lee Smolin's books about the folly of string theory appeared.

   There was a general recession in hiring from 2008 to 2011. Since 2011, hiring has picked up modestly, but, so far, no sign that the bubble in pseudo-physics hiring has been reflated. Perhaps the Federal Reserve will step in and do some hiring in this area, along with buying up some securitized student debt, to cover up that crisis.

   (I'm partly being facetious with the last comment, although not completely. But Wall Street's mass hiring of physics theory students has also stopped, after reaching its peak in the early 2000s.)

5. **Peter Woit**
   June 11, 2014

   I just noticed an interesting profile at Physics Today of theorists Brian Wecht and Ben Lillie and their "Story Collider" project, see here

   http://scitation.aip.org/content/aip/magazine/physicstoday/news/10.1063/PT.5.9016

   On the hiring issue, Wecht, an American ex-IAS postdoc in string theory/formal theory, now has a faculty position in London, and comments:

   "I moved to London a bit more than a year ago for a permanent job at Queen Mary [University of London]. It was such a relief to get it. I was a postdoc for eight years—which is increasingly typical for someone in my field. London is amazing, not only because it's a great city, but also because it's the only place in the world that's hiring people in my field these days."

6. **Mesa**
   June 11, 2014

Great presentation by Ms. Gianotti.

Wall Street used to hire physicists for a good balance of theoretical and practical knowledge to model market phenomenon. I suspect that the reduction in hiring recently is due to some of the move in theory towards unverifiable esoterica, but also the rise of the big data comp sci guys as well.

7. **Slynx**
June 20, 2014

As a young scientist it seems to me like it would be overall productive and beneficial to the STEM ecosystem if some of the postdocs and grads went on to industry and government rather than academia or big data jobs and represented scientists everywhere. The people at eg Goldman Sachs do this quite effectively.

I've read articles in science and nature going back twenty years that talk about giving the students real career prospects but I don't yet really see how this is built into the academic business model in a constructive way. Appreciate it's a hard problem.

Strings 2014

June 15, 2014
Categories: Strings 2XXX

This coming week and the next Princeton will host both the big yearly string theory conference [Strings 2014](), and [Prospects in Theoretical Physics 2014](), a program designed to train young physicists in string theory.
Princeton is definitely the right place for this, since it now is very much a singular point in the world-wide theoretical physics research community. At the IAS the director is a string theorist, so is the past director (now faculty member). Of the other four senior HEP theorists, three are string theorists and one might be described as a fellow-traveller. Over at the university, of the nine faculty in HEP theory, all are string theorists except for one junior faculty member.

One will be able to follow the Strings 2014 talks live [here](), and video and slides should be posted [here]().

The Strings 20XX conferences provide a good place to see what the latest trends in string theory are, with the talks chosen to highlight what some of the most influential people in the field consider the most interesting work. I've written posts on the blog here about [previous such conferences](), which one can compare to this year's to see how the field has evolved. Looking at the list of over 70 talks and their topics, some things that strike me (in many cases, much the same things as in other recent years):

- Talks actually about strings are a small minority (20%), something that has been true for quite a few years. The percentage may have grown from a minimum back in 2011 when some of the speakers were mentioning the small role strings were playing at a string theory conference.
- AdS/CFT and holography remain a dominant theme, as they have for many years. Possible applications of this to condensed matter physics are a continuing hot topic. The previous hot topic of this kind, applying AdS/CFT to heavy ion physics, now seems to be dead, something people would rather not talk about anymore since it never worked out as advertised.
- Amplitudes are the other big hot topic.
- Discussion of the LHC and hoped-for LHC results in the past was often a major topic at these conferences. Now that the LHC results are in and a huge disappointment (no SUSY or extra dimensions), it looks like there's a chance the LHC will not be mentioned at all at this year's conference, with "string phenomenology" in general the topic of only a very few talks.
- String phenomenology does have its own yearly conference (see [here]()), but at least as far as the US participants go, the top US research institutions are not represented there, whereas they are heavily represented at the Princeton conference. Whatever "string phenomenology" is these days, it's not popular at all among the Princeton crowd. It's no longer being done at the most prestigious US institutions, and in Europe is concentrated in certain places (popular in England for some reason, not at all in France).
- While research into string theory unification schemes now seems to be very unpopular at Princeton, for some reason it's a topic that the young must still be trained in. The PiTP program includes a series of lectures on string compactications, for which the Princeton people needed to bring in Martijn Wijnholt from Munich, one of the places still doing this kind of thing.
- To the extent there's anything about connection to experiment, B-modes are the hot topic.
- There was a time when mathematicians were sometimes invited to Strings 20XX, but that's over and done with. It seems most prominent string theorists no longer want to hear anything from mathematicians.
- Finally, zero about the multiverse or the landscape. Clearly some on the organizing committee still have strong opinions and are not going to tolerate that kind of nonsense.

  Witten will just give a 15 min welcoming speech. In the past, David Gross has ended the conference with a "vision" speech. This year there will be five "vision talks", and it may be interesting to see a wider range of opinions on where the field is heading.

Update: One more notable thing about this version of the yearly conference is that (as far as I can tell), it's the first one in many years that has not included a promotional public talk about string theory. It may very well be that this was considered unnecessary in Princeton.

Update: Martin Wijnholt's lectures to the students and postdocs in Princeton about string compactifications are available [here](). Lots of nice material on Calabi-Yaus and algebraic geometry, nothing at all about extracting the standard model from all this. One thing that has always surprised me is how little most string theorists know about the state of the art of getting particle physics out of the theory. This is less surprising now after seeing the kind of lectures they get on the subject.

Comments

1. **Bernhard**
   June 15, 2014

   If Witten himself is not giving a scientific talk, I guess that speaks about the health of the subject.

2. **Bob Jones**
   June 15, 2014

   Stringy mathematics is being discussed at a different conference:

   http://www.ihes.fr/jsp/site/Portal.jsp?document_id=3481&portlet_id=14

3. **layman**
   June 15, 2014

   Bernhard,

   Or the fact that he is on the *local* organizing committee and therefore not allowed to speak. This has been the policy forever.

   You are embarrassing yourself.

4. **Peter Woit**
   June 15, 2014

   Bernhard,
   That's not necessarily true, since Witten is one of the local organizers, and there's some prejudice towards not having organizers invite themselves to speak (a few of the local organizers will speak, but this may be the only major conference that Arkani-Hamed doesn't speak at).

   Bob Jones,

Not a whole lot of either string theory or physicists at that one…

5. **Peter Woit**
June 15, 2014

layman,
Since three members of the local organizing committee are speaking, seems to me that it's you that are embarassing yourself. Of course, since you're hiding behind anonymity, that's not a problem for you I guess….

6. **piscator**
June 15, 2014

Not quite sure why you say string phenomenology is popular in England but not in France – quite a lot of people in the Paris area do some variety of string pheno. There are probably more students in England doing string pheno, but France (for cultural reasons?) has very few PhD students full stop.

7. **Peter Woit**
June 15, 2014

piscator,
Perhaps you're right, I may have been over-generalizing from an impression of Oxford vs. IHES. I do think there's a lot of evidence for a strong US pattern of string phenomenology not being pursued at the highest-ranked institutions, wonder if there's any similar pattern in Europe. Of if not, why not. England was an example where I could think of string phenomenologists at well-known institutions, France not, but perhaps this is just due to less familiarity with the French system.

8. **piscator**
June 15, 2014

Peter,

There are a lot of places in Paris so I forget who is exactly where, but an immediate list of names in this area or thereabouts is Bena/Dudas/Goodsell/Grana/Halmagyi, and then there are people like Antoniadis who are there some of the time – I think some of these are at the Grandes Ecoles as well.

Generally I think string phenomenology is much stronger in Europe than the US – in Germany there is the very large Lust group in Munich, and then big groups at Bonn and DESY, in Spain there is Madrid (Ibanez, Uranga, Marchesano)…not sure why there is such a difference to the US, where there is almost a void between people working on BSM x-dimensional models and the mathematical physics side of string theory.

9. **Bob Jones**
June 15, 2014

What are you smoking, Peter? Almost every single one of those speakers is working on some aspect of mirror symmetry…

10. **Navneeth**
June 16, 2014

This year there will be five "vision talks", and it may be interesting to see a wider range of opinions on where the field is heading.

Perhaps Dr. Witten will provide a surprise final talk in which he reveals that all five visions can be considered as different facets of an over-arching M-Vision. 😉

11. **Andy**
June 16, 2014

"It seems most prominent string theorists no longer want to hear anything from mathematicians."

It seems you're unaware of the existence (since 2011) of a conference called String-Math, attracting crowds of the same order of magnitude as (if not higher than) Strings itself.

12. **Peter Woit**
June 16, 2014

Andy,
I'm well aware of it. Most prominent string theorists go to Strings 20XX, few go to Strings-Math 201X. I think it's undeniable that most of those on the Strings 20XX organizing committee are less interested in interacting with mathematicians now than they were at many points in the past.

I'm not claiming there is no active field of research involving mathematics and string theory, with many mathematicians participating, just that most US physics departments and most physicists working in string theory have little interest in it (and less than in the past).

13. **Andy**
June 16, 2014

Peter,

most prominent string theorists go to String-Maths. A quick look at the list of speakers (which is a fortiori strictly included in the list of participants) of SM2012 in Bonn will prove you wrong. I also disagree that "most" people in the organising committee are less interested in the interaction with mathematicians now than in the past. Except perhaps for Verlinde, I think this is precisely false for each and every of them (especially Arkani-Hamed and including Witten – just have a look at his scientific production during the last year)

My considered, possibly misplaced and certainly fully biased opinion is that the StringsMaths interaction has rarely been as active as today: the easiest example to pick is the huge amount work on AGT ("huge" is the right word), which is largely a string-theoretic and/or string-motivated subject, and a top research priority for – my turn to say it now – most prominent senior string theorists (Nekrasov, Gaiotto, Gukov, Moore, Vafa, Witten himself, amidst lots of others): something with few-to-none precedents after the SW paper 20 years ago.

I think – and I'm sure that the two of us will agree on this – this is only good news for the subject.

14. **Peter Woit**
June 16, 2014

Andy,
The concept of the Strings 20XX conferences has always been to cover all areas where something interesting is happening in string theory, not just areas not covered at other conferences. It's traditionally not supposed to be a conference specializing in only certain areas. If it has devolved into mainly a specialist conference on AdS/CFT + amplitudes, that's a phenomenon worth taking notice of.

I looked earlier at the speaker list of Strings-Math 2014 and just took a look at the ones for 2012 and 2013. I disagree that these lists contain a significant fraction of the most prominent string theorists. To quantify the matter, one definition of the most prominent string theorists would be the winners of Milner's $3 million award. Zero of these were speaking at the Strings-Math conference last week, four will be speaking at Strings 2014, with three more on the local organizing committee so presumably there but not speaking, and two more (Strominger and Vafa) this year's runners up for the award. That makes nine versus zero….

15. **Peter Woit**
June 16, 2014

Andy,
I just noticed that Princeton has a press release out, claiming over 600 participants at Strings 2014. For Strings-Math 2014, looks like about 125. Not sure that's the same order of magnitude…

Also in the press release: they're covering the "many strands of modern string theory". If math is one of them, for some reason mathematicians aren't part of it.

16. **Bob Jones**
June 16, 2014

The Strings conference also has mathematical talks (see the ones by Cheng, Kachru, and probably also Cordova, Moore, and Vafa).

17. **Peter Woit**
June 16, 2014

Bob Jones,
Sure, of the 70-odd talks, some will have material of mathematical interest. My comment was just that mathematicians aren't the ones being asked to talk about this.

18. **Brian Dennehy**
June 16, 2014

Apologies for being off-topic but 'layman's comment is sadly a reminder that things like meanness to others is not entirely absent in the research community. I can only assume that 'the health of the subject' is something that 'layman' has become emotionally invested in.

19. **Peter Woit**
June 16, 2014

Brian,
Researchers ("layman" appears to be one, coming from a major research institution) responding to criticism anonymously with arrogant and mis-informed denunciations of others as being idiots doesn't really look good, and may very well have something to do with why so few US institutions are willing to hire young string theorists. In my experience, most string theorists are perfectly reasonable people, no more arrogant than your average academic. There are however some unfortunate exceptions, including prominent bloggers and others who like to take advantage of blog comment section anonymity.

20. **Jeff M**
June 16, 2014

Uh, Peter, hate to be pedantic, but 600 and 125 are the same order of magnitude (2, to be exact). 🙂

21. **Peter Woit**
June 16, 2014

I dunno Jeff, maybe it's the physicist in me, but I'd say 600 is the same order of magnitude as 1000, and 125 is the same order of magnitude as 100, so….

22. **Chris Herzog**
June 16, 2014

I often like your posts Peter, but this one came across as a bit too knee jerk for my taste.
1) AdS/QCD is not dead but has maybe left the purview of formal string theory. To name one success, the idea of measuring viscosity with respect entropy density has left a lasting impact on the heavy-ion community.
2) I agree with Andy 100% that the strings/math connection is alive and well everywhere, from Korea to the U.S. to Europe. The fact that mathematicians are not speaking at Strings I view as an accident. Who knows? Perhaps the mathematicians read your blog and decided to turn the organizers down…
3) The absence of string phenomenologists at top US research institutions is in my mind an indication that US institutions may no longer be as top in theory as they used to be, compared with the rest of the world. We continue to train the world's finest for the moment, but many of them now choose (or are forced) to go elsewhere for jobs.

23. **Peter Woit**
June 16, 2014

Hi Chris,

I agree that strings/math is alive and well (although, as in physics, the strings are often disappearing, and we're really talking about qft/math). Would be interesting to know if the conference organizers invited mathematicians, I still suspect not, that the overlap with math is something that physicists in general have drawn back from (which is too bad).

My own take on the absence of hiring at top US institutions of people working on models for getting the SM out of string theory is that the best theorists, string theorists and otherwise, have for a while now realized this is an idea that doesn't work, or at least one that is going nowhere until someone comes up with some very different idea. The dozen or more string theory faculty at Princeton are the best in the business and they're still hiring in the area, but not "string phenomenologists". I'm not aware of them trying to hire such a person and failing because they lost out to another institution. What I don't understand is why this is still so popular in Europe.

As for AdS/QCD, maybe my comment about the death of the application to heavy ions was a bit over the top, influenced by some over the top claims about the successes of that subject that were common in the past.

Glad to hear you often enjoy the posts, despite some disagreements…

24. **piscator**
June 17, 2014

A European perspective on this is that string theory faculty at Princeton are just not the best in the business anymore, unless the `business' is defined as the uber-narrow area of mathematical physics inspired by string theory. Whereas the Strings conference has a theoretical mandate to cover the whole of the subject, in practice there has been a narrow focus on topics popular at Princeton.

Most of the talks at Strings – this year as at previous year – are only tangentially related to what `physics' historically means. If you never work on any kind of phenomenology, and your advisor never worked on any kind of phenomenology, you are not going to know very much about it. Most physicists really doesn't think six-d N=(2,0) theory is an interesting problem to be thinking about.

A slightly broader perspective on this is that each generation, there are certain tools and abilities required to really advance the subject. Physics for 30 years after the second world war was not so mathematical. This created a ripe environment for the fusion of mathematical and physical ability so exemplified by Witten (and others of that generation) to be perfect for making great leaps forward. It's very unclear that this style of work now is what is really needed.

25. **Pneno**
June 17, 2014

In fact there are many strong groups working on string phenomenology in the US.
To name a few : Harvard (L. Anderson (Heterotic and more) , Heckman, Vafa (who have done very important work on F-theory Unification),
Princeton (H. Verlinde; Maldacena and Klebanov on string cosmology; Witten on axions); The Penn group (M. Cvetic, B. Ovrut, Donagi);
Stanford (Silverstein leading work on string cosmology; also Kachru, Kallosh and others); Wisconsin (G. Shiu; L. Evertett); Santa Cruz (M.Dine);
Mc Allister (Cornell); Many others on different aspects of compactifications like D.Morrison, Taylor, Wecht, Halverson; Many other well known physicists work part time on string phenomenology aspects (S. Dimopoulos (axiverse),
L. Randall (e.g. flavour physics from F-theory), N. Arkani-Hamed (landscape), R. Bousso (landscape), S. Raby (heterotic models), P. Langacker, M. Gaillard, and a very long etc.

26. **Chris Herzog**
June 17, 2014

Pneno.

A very interesting list. To clarify, I really meant a relative absence. My position is simply that more faculty lines should be devoted to string phenomenology in the US.

27. **chris**
June 17, 2014

"the idea of measuring viscosity with respect entropy density has left a lasting impact on the heavy-ion community."

sorry, but i have to strongly disagree with this. there was some interest a few years back, but not much lasting impact i would say. if there is a distant field with a strong influence on heavy ions it is probably cosmology at the moment. the cmb/fireball comparison is much more taked about at the moment than any ads/cft result.

28. **Peter Woit**
June 17, 2014

piscator,
As I keep pointing out, little of what the many string theorists in Princeton are doing has much to do with mathematics, it seems they chose no mathematicians to speak at the Princeton conference, and few of the many talks there have much to do with mathematics. If you don't like the current Princeton vision of "string theory", I don't think it's fair to blame it on "mathematics".

Pneno,
There is some interest in "string cosmology" at top US institutions, especially Stanford, but I think comparing your list to the Strings 2014 speaker list makes my point. Only Anderson, Bousso and Vafa are speakers, with Anderson the only one speaking about phenomenology. And she's now at Virginia Tech, not Harvard.

29. **Peter Shor**
June 17, 2014

What people seem to be arguing about is whether the pre-eminent conference in strings is being run by a small group of famous string theorists who have dismissed phenomenology, mathematics, and the LHC, or whether the predominant sentiment in the field as a whole is to dismiss phenomenology, mathematics, and the LHC.

I don't know which of these alternatives I find more disturbing.

30. **Elbi Gilgen**
June 18, 2014

It puzzles me that anyone could think that amplitudes are "hot". Look at the arxiv. In particular, the "Amplituhedron" is deader than any doornail has a right to be.

31. **David Meltzer**
June 18, 2014

Elbi,
The amplitudehedron work by Nima is probably the most mathematically sophisticated work on amplitudes there is. Probably the only people working on it are Nima, Trnka and maybe a few of his other students. It's not surprising if it appears dead right now just because so few people are working on it. The amplitudes program is much larger though and a few names I could recommend looking at the papers by Lance Dixon, Zvi Bern, and David Kosower, they really got the subject started and were awarded the Sakurai prize for their work.

Theres a lot of other names I could list but my best recommendation would be to look at one of the amplitudes conferences for more details.

32. **Jesper**
June 18, 2014

Peter – I just noticed that you posted this comment in "Strings 2XXX". I wonder if this choice of label is a show of optimism – or pessimism? You could also have chosen

Strings XXXX, Strings XXXXX, or – in what I think would be in the optimistic end of your spectrum – Strings 20XX.

33. **Peter Woit**
June 18, 2014

Jesper,
Yes, extrapolating from the last twenty years, I think there will be a Strings 2100 conference. Might not even be that different than the current ones. I'm much less sure of Strings 3000, but who knows….

34. **Shantanu**
June 19, 2014

Peter somewhat OT to this thread but not blog
The talks of the recent dark matter conference at CFA are online and in
Lisa Randall's talk
https://www.youtube.com/watch?v=rahKgEDX1yM&feature=youtu.be
she is mentioned that supersymmetric models were in trouble even before LHC started
and none of them obey occam's razor

35. **Bernhard**
June 19, 2014

Pneno,

Since you seem to know well about string phenomenology, I wonder how much do you believe of real phenomenology the work you mention on your list contains. What I have in mind are realistic string theory models that can connect with experiment. The obvious names that come to mind are Lisa Randall and Arkani-Hamed with extra dimension models, but I think it is fair to say whatever realistic models they built they were more "string inspired" than actual stringy models.

My comment has a direct connection with Peter's comment about "how little most string theorists know about the state of the art of getting particle physics out of the theory". Perhaps it doesn't apply to this list of prominent people, but still, even for them, I have a very hard time finding work that could be directly meaningful to an experimentalist, and at some point this should be the goal of phenomenology, wouldn't you agree?

36. **Pneno**
June 19, 2014

Look, Bernhard. Writing a phenomenological model which can be tested at the LHC or other experiment is trivial. Add a few extra dimensions here and there; extra vector-like particles or non-perturbative stuff; etc. But that is not what we want.
We want to learn what lies beyond the SM and understand the involved principles within a fundamental theory, and, lets face it, the only serious candidate for that is string theory. Thus we want to learn how string theory might contain (or not) something resemblingg the SM at low energies. This we have to do taking into account both experimental information and mathematical consistency, and both are very constraining. This is a tough job since string theory is technically demanding and one also has to have the physical intuition of what phenomenological problems can and may be addressed. These include issues like the structure of fermion masses, strong CP problem, SUSY breaking, hierarchy problem, cosmology….
This may take time but some of us prefer that than producing random BSM models which are testable by an experimentalist NOW.
String theory already provided answers to those questions in much firmer grounds than previous BSM traditional model building.
And much more will come, while our knowledge on compactifications accumulate. In addition string theory has been the source of many of recent ideas physics BSM. I certainly reccomend the young phenomenologists to learn string theory compactifications if they want to be sufficiently prepared for doing BSM physics in the future. GUT's and SUSY are no longer enough.

37. **Peter Woit**
June 19, 2014

Pneno,
From your description, string phenomenologists have given up on being able to do anything relevant to LHC-scale physics, although they have the capability of generating "random BSM models" if they want to (but you seem to realize that isn't a very meaningful activity). The same argument applies to VLHC or any other conceivable HEP physics experiment, so you've given up on the conventional idea that a better theory is supposed to predict something different and distinctive that can be tested.

You seem to indicate that what is worthwhile to do now is to just try and get the SM out of a sufficiently complex string theory model. There seems to be no evidence that such things are so tightly constrained that you can't get the SM, so there is no way for anyone to ever show that what you are doing is wrong. But if you "succeed", get a string model that gives the SM, what have you accomplished other than embedding a relatively simple model in something much more complicated? How will you ever know that what such a complicated creation has anything to do with the real world?

38. **akame**
June 22, 2014

Peter,
"But if you "succeed", get a string model that gives the SM, what have you accomplished other than embedding a relatively simple model in something much more complicated? How will you ever know that what such a complicated creation has anything to do with the real world?",

such a theory would describe particle physics as we know it, and also be a consistent theory of quantum gravity. The embedding of SM into string theory solves the problem of quantum gravity, is that not a worthwhile thing? And if it describes both gravity and particle physics, doesn't it per definition have a lot to do with the real world? At the very least, it is some "effective theory of everything", which in itself is no small feat. You can't reject a theory because "it seems too complicated". Of course, a simpler model is preferable, but then there needs to be such an alternative.

39. **Peter Woit**
June 22, 2014

akame,
First of all, arguing that this is a consistent theory of quantum gravity is rather dubious since you don't know what the theory is (remember, "what is string theory" is still an unanswered question).
But, even giving you that, I still don't believe that replacing a simple model by an incredibly complicated, completely untestable one is progress in any sense, no matter what your argument is that your untestable model has some better formal property.

40. **akame**
June 24, 2014

Peter, such a model would be in principle testable, but maybe not at accessible energy scales. Moreover, it is not obvious to me that the theory would be all that much more complicated than the standard model. Even the SM requires us to fix a particular gauge group, a particular particle content, a Higgs sector, there is some weird mechanism for neutrino-masses, 3 generations, there must be something there to explain dark matter, maybe some axion, presumably there also needs to be something new out there other than the Higgs and so on. Maybe all this seems simple because you are used to it, but it really isn't all that simple. My hope is that an embedding into string theory, maybe using F-theory, could have a similar amount of complexity, just phrased in a very different language, i.e. pick a CY, have some fluxes, place some branes/some elliptic fibration, maybe some O-planes, and get a reasonable model. The amount of data needed could be comparable to the amount you actually need for a non-stringy model of particle physics. And again, a model being "complicated" is not a valid criticism. Physics is about describing nature, you can't and shouldn't necessarily require models to be "simple", whatever that term even means.

41. **Peter Woit**
June 24, 2014

akame,
In the kind of modeling you're talking about, as far as I can see, all you're doing is putting together mathematical components (Calabi-Yaus, elliptic fibrations, etc.) that are much more complicated than the components of the SM (simple Lie groups and their simplest finite-dim representations), according to a conjectural set-up in which you expect to be able to map the complicated structures to the simpler ones of the SM. As far as I can tell, no one has been able to turn this set-up into one that allows you to reliably extract the SM parameters (couplings, mixing angles, Yukawas) as a function of the inputs, except perhaps in special unphysical limiting cases.

This kind of set-up has zero predictive value, because much more is going into it than is coming out. The hallmark of a theory with predictive power is that you put something simple in and get non-trivial complex, testable physical predictions out. Here you're doing the reverse, having to provide more complex inputs than the physical predictions you are getting out. Yes, you can "model" all sorts of physics this way, but you can't predict anything, and you're not doing science in any conventional understanding of the term. Some people think you're doing mathematics, but I'd disagree with that too.

Yes, you can hope that a miracle will happen, and some particular choice of inputs will provide the complexity of the real world. People have been doing this now for 30 years with no success at all (this idea looks much less promising now than it did in 1985). Hoping for a miracle is just wishful thinking, not science.

42. **Nick M.**
July 3, 2014

Peter,

The link that you have given to Martin Wijnholt's lecture at Princeton about "String Compactification" (in the second "Update") seems to have gotten broken. I *"think"* the correct (current) links are " Wijnholt notes UPDATED" and "Lecture 1 slides" and "Lecture 2 slides". Only trying to be of help.

43. **Nick M.**
July 3, 2014

Sorry Peter, but I forgot to mention that there is also a third set of slides for the third lecture; "Lecture 3 slides".

44. **Peter Woit**
July 3, 2014

Thanks Nick,
I've updated the link to the notes, and people can follow your links to the slides.

Smoking Gun No Longer Smoking

June 19, 2014
Categories: Uncategorized

The BICEP2 paper is now out in Physical Review Letters, with major revisions to its conclusions from the preprint/press conference version of last March. For another sort of associated revision, compare this (from a March 17 Stanford press release):

> Linde, now a professor of physics at Stanford, could not hide his excitement about the news. "These results are a smoking gun for inflation, because alternative theories do not predict such a signal," he said. "This is something I have been hoping to see for 30 years."

to this (from an interview with Linde in the latest New Scientist):

> I don't like the way gravitational waves are being treated as a smoking gun.
>
> If we found no gravitational waves, it wouldn't mean inflation is wrong. In many versions of the theory, the amplitude of the gravitational waves is miserably small, so they would not be detectable.

Last month, Resonaances broke the news that there was a problem with the BICEP2 claims, specifically with the bottom line (and punch line) of their preprint abstract:

> Subtracting the best available estimate for foreground dust modifies the likelihood slightly so that r=0 is disfavored at 5.9σ.

Back then the BICEP official reaction to the Resonaances claim that they were admitting to a mistake was "We've done no such thing." Post-refereeing, there have been extensive changes in the paper (for example, the "DDM2" dust model based on scraped Planck data is gone), and the bottom line of the abstract has been changed to:

> Accounting for the contribution of foreground, dust will shift this value downward by an amount which will be better constrained with upcoming data sets.

If the BICEP collaboration is still not admitting a mistake in their treatment of Planck data or the bottom line of their preprint, then it seems that referees have told them they can't publish these in PRL.

Back in March the BICEP2 results made the front page of the New York Times with a Dennis Overbye story Space Ripples Reveal Big Bang's Smoking Gun, but today the NYT has Astronomers Hedge on Big Bang Detection Claim, which explains well what has been going on.

Update: Nature has a story out about this, which includes the news of a recent presentation at a Moscow cosmology conference by Jean-Loup Puget of the Planck collaboration:

> Using for the first time the newest Planck maps available, Puget and his collaborators have directly examined the polarization of dust in these high galactic regions rather than extrapolating from dustier regions in the plane of the Milky Way. Averaging over some 350 high-galactic-latitude patches of sky similar in size to the region observed by BICEP2, Puget reported that polarization from interstellar dust grains plays a significant role and might account for much of the BICEP2 signal that had been attributed to inflation-generated gravitational waves. Puget told Nature that an article detailing these findings would be published in about six weeks.

Update: I've been watching Paul Steinhardt's talk at Strings 2014, where he's giving a dramatic attack on the way inflationary cosmology is being pursued as in violation of the scientific method. One thing he does is put up exactly the Linde quotes from this posting.

Comments

1. **Florin Moldoveanu**
   June 19, 2014

   It's a good thing PRL decided to make this available for free, but what happened to their length limit? I recall that the maximum accepted size was four. At the 25 page mark, this paper has a War and Peace size by PRL standards. Does this mean that anyone can now submit papers up to 25 pages to PRL? Somehow I strongly doubt it.

2. **Dave**
   June 19, 2014

   I've heard of PRL trying to lure papers from other journals by relaxing the page limit, but that was for six or seven pages, not 20+. Maybe the preprint was intended for ApJL and PRL managed to coax the authors away.

3. **Bernd**
   June 19, 2014

   There's an editorial explaining the unusual format:
   http://journals.aps.org/prl/edannounce/10.1103/PhysRevLett.112.240001

4. **Jeff M**
   June 19, 2014

   Pardon the stupid question, but if they've removed the scrubbed Planck map, how exactly do they claim to account for dust?

5. **Peter Woit**
   June 19, 2014

   Jeff M,

   The preliminary Planck data they got off a slide was just one of several things they were using to estimate the effects of dust. My non-expert understanding is that how they used that has been challenged, other ways they were estimating dust are not completely convincing, and since the time of the preprint Planck released more data about dust which might raise their estimates. So, they're stuck with no solid statement about dust, just that the effect of dust is "an amount which will be better constrained with upcoming data sets."

   By the way, in the interview, Linde refers to a rumor that Planck will release relevant results about dust sooner than their full polarization results (which are planned for October), possibly this summer, "any time now".

6. **srp**
   June 19, 2014

   I find this type of dispute refreshing. It's about the quality of observations and interpretation of data–BB relic or interfering signal from relatively nearby dust? More data from different instruments and some careful thinking will sort out which of these is more likely in a few years at the outside.

   One interesting sidelight on this issue is the question of what Harry Collins calls "methodological individualism versus methodological collectivism." The former stance holds that each individual paper and announcement should be a definitive claim of a finding one way or the other, complete in itself. The latter stance holds that each paper is just one piece of equivocal evidence and overall conclusions should be derived by the community after seeing many such papers over time. The advantage of methodological individualism is that researchers are forced to think hard about what they actually believe and do a lot of work to make their results credible, thereby incentivizing quality work and avoiding publication bias of spurious weak but confirming signals . The advantages of methodological collectivism are that weak individual signals may be collectively strong and that other researchers are less likely to toss out intriguing but less-than-definitive findings if they see others are seeing similarly weak-but-suggestive things.

7. **anonymous**
   June 20, 2014

   Come on, Peter. The vast majority of the paper is the same as before, about as much as you'd expect following a typical peer review. The evidence against synchrotron and synchrotron-correlated dust has, if anything, strengthened. The caveats about the rest of dust were already in the penumbra of the original paper. For all that you decry sensationalist headlines, you have been the pot calling the kettle black for the last few months. If the rumored Planck paper gets arXiv-dumped soon, you should preliminarily treat it as being as reactionary as the rest of the unreviewed Flauger/Seljak/anyone analysis.

8. **Bob**
   June 20, 2014

   Those two back-tracking quotes from Linde are quite extraordinary, almost funny if it didn't reveal such a dreadful way to do science: "If the experimental result is true, the theory is definitely proven. And if the result is false, the theory is still probably correct." Simply dreadful.

   Inflation: well worth a million dollar prize.

9. **Peter Woit**
   June 20, 2014

   anonymous,
   I don't think a typical peer review changes the conclusion of the paper from "we've discovered something revolutionary at greater than 5 sigma" to "could just be dust".

   The PRL peer review isn't that significant in that this change in conclusion had already become widely known, with a lot of public discussion of the problems with the 5 sigma claim and a new consensus that "could be dust" is a significant possibility, not a one in 3.5 million possibility. To see the change in consensus, all one has to do is listen to Linde…

10. **OMF**
    June 20, 2014

    Bottom line, all I see here is the scientific method in action. Public discussion and scrutiny of the data lead to the most rigorous conclusion.

    The real problem here was the media hyping of preliminary results and the pressures which this placed on the entire investigation and potentially the peer review itself. This kind of modern pressure will end up interfering with future large scale experiments unless methods are found for dealing with it.

11. **Alex**
    June 20, 2014

    @Bob

    Aren't these two new quotes never the less correct? My understanding of inflation is that it's a theory with free functional forms (let alone free parameters) and that even if r came out to be some other value, an inflationary theory could be found to be consistent with that value.

12. **Bob**
    June 20, 2014

    OMF: it wasn't the media hyping the initial results, it was the bicep team who did that. That last sentence of the original Bicep preprint will live in physics infamy.

13. **Bob**
    June 20, 2014

    Alex: It would have been more impressive if Linde had said no gravitational waves is a blow for inflation, but it does not neccessarily kill it. He's basically saying the result is irrelevant to the theory – unless it supports the theory. That's poor.

14. **Jeff M**
    June 20, 2014

    Linde's comments remind me of why I switched from physics to math way back in the early 80's. Even back then the parameter space was plenty big enough to accommodate all sorts of experimental results, which drove me nuts. At least from what I remember once you got a result and adjusted the relevant parameters you would then restrict things enough that you got some real predictions which could be checked, and if they didn't work then the theory would be falsified. As far as I can tell, nowadays you can adjust one parameter to account for an experimental result without limiting the value of any other parameters at all, there will always be an infinite number of theories available to fit any data you have.

15. **Peter Woit**
    June 20, 2014

    Jeff M,
    Linde's behavior and the whole inherently untestable multiverse approach to fundamental physics that he represents (together with a willingness to say outrageous things to the press…) are very much outliers in physics in general. Few people behave that way. It's an interesting question what the physics community can do to keep this kind of thing from discrediting more serious parts of the subject.

16. **anon.**
June 20, 2014

anonymous wrote:

If the rumored Planck paper gets arXiv-dumped soon, you should preliminarily treat it as being as reactionary as the rest of the unreviewed Flauger/Seljak/anyone analysis.

One important claim that first appeared publicly (as far as I know) in the Mortensen and Seljak paper was that BICEP's argument that the frequency-dependence of the signal disfavored dust at 2.2 sigma was wrong, because the correct thing to test was not pure dust but dust + lensing. The published version of the BICEP paper now shows a test of dust + lensing and, indeed, the significance has dropped down to 1.7 sigma. The Seljak paper may not be "reviewed" but this part of its argument is validated.

17. **crime**
June 20, 2014

http://en.wikipedia.org/wiki/Smoking_gun
The term "smoking gun" was originally, and is still primarily, a reference to an object or fact that serves as conclusive evidence of a crime

18. **paul hughes**
June 20, 2014

A smoking gun is conclusive proof. The absence of a smoking gun in not conclusive disproof. Maybe that is what Linde was saying, and I actually think from these quotes alone that it was (the absence of a smoking gun does not disprove the theory). To reiterate, it sounds (from these quotes alone) that in the second quote he was saying that, even if the results turn out not to be a smoking gun, the theory is not disproved; and that he is complaining about people misusing his "smoking gun" quote to imply or infer that such absence is disproof. Seems fairly straightforward to me unless one has an axe to grind.

19. **Peter Woit**
June 20, 2014

paul hughes,
I don't think there's anything at all unclear about what Linde is saying, and the comments by Bob above have it right. It's perfectly consistent, but it also shows clearly that he's now arguing for a way of pursuing science immune to falsification.

As for his complaints about people "misusing" what he said back in March, I just don't think anyone is "misusing" anything. When you're part of a press release claiming that the discovery of X proves you are right (and possibly getting part of a $1 million prize because of this…), you're going to regret that press release when it starts to look like the discovery may really have been of "not X".

20. **Robert Rehbock**
June 20, 2014

"I'm 95 percent convinced it's true, but extraordinary statements need extraordinary proof. If these results are correct, they are among the most spectacular results in observational cosmology obtained in the 21st century. We should wait a little before they are analyzed and confirmed by other observers."
Linde said on March 21st. Your selective quotes are as unwarranted as declaring the discovery is premature.

21. **Peter Woit**
June 20, 2014

Robert rehbock,

This is about linde's claims about the significance of the result if it were true, not about any claims concerning the correctness of the result. Of course like everyone he was aware the results might not hold up and needed confirmation.

22. **paddy**
June 20, 2014

I do like the phrase used in the Nature article PW referenced: "…a popular but outlandish theory…."

23. **srp**
June 20, 2014

Not a multiverse fan, but Linde's logic is in no way anti-scientific. You have a theory you believe is more likely true than not and then someone comes along with a potential piece of clinching evidence that would raise credibility to 100% if it held up. If it doesn't hold up, that just drives your credibility back to its original more-likely-than-not level. There's nothing wrong with that–it's common sense.

24. **Peter Woit**
June 20, 2014

srp,
What's unscientific about this scenario is that you're claiming that your relation to experiment is a no-lose one. Heads you win, tails you get a do-over. What makes it science is when you can lose. If a significant primordial gravitational wave signal of r around .2 really is a smoking gun for inflation, corresponding to what you expect in the simplest models, with the expected (GUT) scale, then a very small r is evidence against inflation, because you are forced to move away from simplest versions and original scenario to something more complicated. Put differently, if the viability of the idea of inflation is insensitive to r, then r=.2 was not a smoking gun. Linde would like to have it both ways, his predictive gun smoking when it looked like r=.2, not smoking now that maybe r is much less.

25. **Avattoir**
June 21, 2014

"Puget reported that polarization from interstellar dust grains plays a significant role
and might account for much of the BICEP2 signal … attributed to inflation-generated gravitational waves"

The key bits of phrasing being: "a significant role" and "might account for much", in contrast with, say, 'may explain entirely', and 'might account for all'.

It's one thing to assert that the BICEP2 team overstated their findings in relation to primordial b-modes; it's quite another to assert that the Planck satellite team is asserting their own findings utterly negate the former's findings. The title to this post purports that Planck is promising to gut BICEP2's findings. In fact, the words used by the Planck representative makes no such promise, directly, indirectly or impliedly, and indeed is not even reasonably interpretable as promising anything of the sort.

But that doesn't even get to the half of it, because the phrase "might account for much" means, more precisely, by necessary implication, 'might account for some of what BICEP2 detected, but not all'. And thus, the question becomes, Even accounting for the full potential of possible signal noise due to dust within this galaxy, what, OTHER THAN PRIMORDIAL B-MODES, would account for the rest?

Resume dancing on the BICEP2's coffin while you still can without appearing ridiculous.

26. **Dan C**
June 21, 2014

What's the difference between the situation here, i.e. parameterizing some cosmological parameters (say, the inflationary Hubble parameter via the tensor-to-scalar ratio), and something like dark matter searches, where one is looking for, say, the mass of some dark particle?

In both cases one cannot seriously argue that a non-detection, i.e. a bound on the parameters, is not scientific progress because it is not a "win-or-lose" situation.

They are both win-or-lose situations for a variety of concrete models with some explicit parameters, dynamics, etc. But they are hardly win-or-lose for the general paradigms.

On the other hand, an actual detection of either of the parameters would be a huge triumph for the theory.

I think this is all Linde is pointing out, although he can indeed say it somewhat dramatically. BTW, I don't agree that the GUT scale is "expected" in inflation unless one has a gambling addiction. An enormous range of energy scales can be easily made consistent with observation.

27. **Peter Woit**
June 21, 2014

Avattoir,
The title to the post is obviously mainly referring to Linde's change of story, secondarily to the related change in the BICEP2 paper conclusions. The Planck news was added later as an update. Of course I have no idea what they will show about dust and how much if any of the BICEP2 result will survive.

28. **Stuart**
June 21, 2014

Someone once said "Absence of evidence is not evidence of absence" Perhaps this is what Linde is alluding to.

29. **Tom Cohoe**
June 21, 2014

If it goes through here, it's a goal. If it doesn't, it's not a miss.

Nice game.

30. **srp**
June 21, 2014

If somebody finds a murder weapon in a wastebasket, that might clinch a theory that X is guilty, but not finding it in the wastebasket might not drive the probability much below the original belief in X's guilt. Especially if there are thousands of places the weapon could have ended up.

There are many asymmetric evidentiary situations where the impact of finding something specific is much more consequential than not finding it. Perhaps you could make the reverse argument, though, that even a finding of the polarization might not support inflation, because other things could have caused it. In fact, I heard Roger Penrose on NPR make exactly that point a few weeks ago.

31. **Curious Mayhem**
June 22, 2014

I agree with srp. No evidence for or against inflation right now means that inflation remains a viable, theoretically compelling, but unproven theory, just as it was on March 16.

Meanwhile, I strongly suspect that the BICEP2 result will be watered down to lower confidence, but not enough to scotch the whole conclusion.

We do need a way to deal with prepublication / preprint / media hype mania. The media makes para-scientific pronouncements that are continuously pseudo-authoritative. Results like that in science are very important but uncommon.

32. **Peter Woit**
June 22, 2014

Curious Mayhem,
I don't see the problem here being the media. The media coverage of BICEP2 has quite accurately reflected what prominent scientists are saying. Back in March the experimenters were holding press conferences to announce a 5 sigma discovery, most experts were saying this looked good (none of them were bringing up the dust problem), and Linde and other prominent theorists were issuing press releases about this being a "smoking gun" for inflation, providing evidence for the multiverse, etc. This got accurately reported.
More recently, the media coverage has also done a good job of reporting the news that problems were found with the BICEP2 claims. Linde is unhappy that his own "smoking gun" press release claims will make him look bad if the BICEP2 result collapses, but I don't think he has a legitimate complaint.

33. **John Gribbin**
June 23, 2014

A small but important point. None of this is about the Big Bang. Inflation preceded the BB. That's the whole point of the idea.

34. **Garbage**
June 23, 2014

Say soon after the BEH mechanism was discovered they'd have gone around saying: 'Here's the smoking gun: A fundamental scalar around 10 GeV' In fact, 'the Higgs' itself was a response to the referee asking for such thing. Then after many attempts LEP comes and the Higgs is not there at 115 GeV. 'No longer smoking?' No so fast, we have a lot of EWPD that fits the SM very well. We also know there's a threshold. The LHC was built and ultimately the Higgs found around 125 GeV. Inflation fits all data we have from early universe cosmology, but like the SM with the Higgs, it's got a few free parameters. –We do not have any alternative to inflation that works– There's a threshold somewhat similar to the TeV scale for the EW theory. In fact, we know there's an irreducible level of non-Gaussianity (NG) which is correlated to the size of r and ns, the tensor-to-scalar ratio and scalar tilt. Unfortunately the size of the NG may be too small to be detected in the near future, but nonetheless it is a well defined ultimate prediction of inflation. (There is also the celebrated $n_t + r/8 \sim 0$ for simple models.) For the case of GWs, simple models do predict a size comparable to 1-ns (there's this factor of 8 in between). But we can easily have a world where those two are unrelated. In this case the level of GWs may be undetectably small in the near future, this is also related to the relation between r and super-Planckian field range. I'd claim that a very small r would be unnatural, but we are still far from ruling out even the simplest models of inflation… as the case of the Higgs, don't kill a theory that works, just go ahead and keep on pushing experiments!

35. **philh**
June 23, 2014

Here is an additional attack on the inflation hype but also a defence:

Attack: even if the BICep 2 signal is confirmed there seems to me a further piece of evidence that is needed. That is the tilt in the primordial gravity wave sepctrum . As I understand there are alternatives to inflation that predict blue tilt for this spectrum , with inflation predicting red tilt. If the spectrum was shown to be blue tilted that might disfavour inflation. Hence measuring the existence of the signal is not enough. I saw very little mention of this in any press coverage.

Defence: As I understand inflation predicts gravitational waves but does not say exactly what energy scale these will be driven from and what value of r it should be detected at.
Similarly the hypothesis that life exists on other planets does not tell you which planet it will exist on. If you found what you thought was life on Mars , that would be a smoking gun for the Et hypothesis. If you then found the evidence was shoddy that doesn't mean your hypothesis was wrong.
Whilst it was overhype to say BICEP 2 confirmed inflation , (see my attack above) what I think is true is that the discovery of B mode would significantly improve the evidential picture for inflation over certain other rival models as ekpyrotic , CCC etc. But not finding doesn't disprove inflation, just as not finding life on Mars doesn't disprove the ET hypothesis.

36. **Neophyte**
June 23, 2014

Garbage says: "don't kill a theory that works"

But not everyone thinks it works, e.g. Penrose. It is a contrived model that unsuccessfully tries to explain a small set of facts about the universe. It creates more problems than it tries to solve. Why bother with it?

I agree "keep on pushing experiments".

37. **Peter Woit**
June 23, 2014

Garbage,
If back in 1964 after the Anderson-Higgs mechanism was discovered, say Brout and Englert had issued a press release claiming that some recent possible bump at 10 GeV was a "smoking gun" for the Higgs particle, people would have thought they were probably nuts. If, several months later after the bump started to look dubious they had gone to the press to complain that they had been taken seriously earlier, all doubt about their craziness would have been removed.

38. **Curious Mayhem**
June 23, 2014

Inflation is emphatically not a contrived theory. It was designed originally to solve one problem (topological relics) and ended up solving three, and quite elegantly. Penrose is simply wrong about this.

Penrose is correct that inflation leaves unanswered the cosmic entropy questions that he and others rightly think are important. But before inflation, no one had an answer for those either. Actually, the questions had never been posed that way before the 1980s, although there were Dirac and others with "large cosmic numbers," an earlier version of this line of thought.

Peter, you're right that, on BICEP2, the media has been fairly responsible in its own right, and its hype in this instance is mainly a reflection of hype from prominent scientists. But that doesn't mean the media hasn't engaged in its own hype in other cases.

The pre-publication mania that I referred to has both components, from prominent scientists and from the media itself, and it's an issue that scientists, journals, and the media all need to recognize and contain.

39. **Curious Mayhem**
June 23, 2014

And if the gun is smoking, *what* is the gun smoking anyway? 🙂

40. **Garbage**
June 24, 2014

If the bump was consistent with a fundamental scalar particle, nobody would have thought they were nuts. I want to believe people would have been really excited! If after the bump had disappeared in the 'dust' they would have said "well there's still a lot of room for the Higgs to show up," they'd be absolutely correct, and time would prove them right. You're purposely ignoring the point. The Higgs particle is a smoking gun of the mechanism, the same way GWs are. But there isn't a unique possibility for the scale at which it decides to show up. (Some may even argue that the fact that $m_h \sim m_Z$ is a score for SUSY.) Given a set of initial conditions, Inflation *predicts* a scale invariant power spectrum with a small tilt. The simplest models produce negligible non-Gaussianity, as observed by Planck. These models would give $r\sim0.1$. To that extent it is an extremely interesting time for cosmology. However, there are many simple extensions which lower r and yet give basically the same fit with data. In fact, String-inspired models often "predict" lower values. Until we don't cross the non-Gaussianity threshold (the irreducible level predicted by inflation) we will never be able to tell for sure.

Speaking about quotes. Steinhardt has changed his discourse so many times after a bound on fnl^{loc} comes about that Linde's comments are innocuous. Like he said: "[Inflation] is the worst form of [theory] except all the others that have been tried."
There are so many problems with Steinhardt's proposal there's no point in arguing. Like a famous mathematician once told me, "as long as you keep using '~' signs you're good"

41. **Simeon Every**
June 24, 2014

The problem isn't that stringy/multiversey theories are inconsistent or incompatible with other scientific theories. The problem is that there seems to be no limit to how many experiments must be done to establish which particular stringy/multiversey theory accounts for the existence of the observed universe (if indeed it is even possible to select one particular theory based on observing only the observed universe) and no experiment that can be done that indicates that no stringy/multiversey theories correctly account for the existence of the observed universe. If the latter is the case, how will you ever find out?

42. **Peter Woit**
June 24, 2014

Garbage,
Back in 1964 postulating a Higgs particle explained nothing about anything (the use of the Higgs mechanism to provide an electroweak theory was years in the future), which is why anyone invoking "smoking gun" evidence for it would not have been taken seriously.

I strongly recommend looking at Steinhardt's Strings 2014 talk, where he discusses the Linde quotes and explains in detail the obvious problem with them.

43. **Garbage**
June 24, 2014

'Model of Leptons' – 1967. Although it is true the original motivation for BEH is lost in time (same as the YM theory) I think is pretty clear what we're talking about unless you want to divert the point. The original motivation for Inflation (horizon problem, monopole, etc.) is also lost in time if you want; but the theory survives, like the Weinberg model does, with quantitative predictions. (Here I think Mukhanov deserves a lot of credit by the way.) What people fails to realize is that there's a very natural connection between an accelerated expansion and quasi-scale invariance, the fact that many models fit data is because of the robustness of this simple (yet powerful) idea.

I am pretty sure if a Higgs-looking resonance had shown up in the 70's (or even 80's) around 10GeV a lot of people would have gotten really excited, and that's 40 (30) years before they found the Higgs.

I've seen Steinhardt in action many times, making the same points over and over. I also suggest you go back and see his own claims about his model and how he changes his mind all the time. I also wish we would understand how to make sense of the big picture with inflation, but given what we observe, there's nothing like it, it is just too simple and idea that works extremely well… I think you're confusing this with the multiverse, the measure, etc. I'm not even touching upon things I cannot measure/prove. But please, learn a little bit cosmology and you'll see how you'll be yourself driven towards inflation like the most compelling explanation.

44. **physics PhD (experimental optics)**
June 24, 2014

In fairness to Linde, it seems that his press release was driven by the Stanford media relations office. As is typical in press releases, "quotes" are often written / scripted / massaged by media flack, and quickly run by the "speaker."

Had Linde been fortunate enough to work at a publicly traded company, his words would at least be reviewed by a lawyer, to protect the company from any liability or blow-back.

Yes, he needs to stand by his words, and eat them if necessary. But overactive University PR organizations are also part of the problem.

45. **Jesper**
June 27, 2014

Peter – do you know where one can watch the Paul Steinhardt's talk you mention? I've looked for it but only found his slides.

46. **Peter Woit**
June 27, 2014

Jesper,
I watched the talk in real-time via the streaming site. Presumably video of the talks is being processed and will show up later.

47. **S. Molnar**
July 3, 2014

It's old news by now, but there is an interview with David Spergel about this in the Simons Foundation magazine: http://www.simonsfoundation.org/quanta/20140703-a-bold-critic-of-the-big-bangs-smoking-gun/

2015 Breakthrough Prizes in Mathematics

June 23, 2014
Categories: Uncategorized

The first set of winners of the $3 million Milner/Zuckerberg financed Breakthrough Prizes in mathematics was announced today: it's Donaldson, Kontsevich, Lurie, Tao and Taylor. There's a good New York Times story here.

When these prizes were first announced last year, I was concerned that they would share a problem of Milner's Fundamental Physics Prizes, an emphasis on rewarding one particular narrow area of research. I'm happy to say that I was wrong: the choices made are excellent, including a selection of the absolute best people in the field, working in a wide range of areas of pure mathematics. The prize winners are mathematicians who are currently very active, doing great work. It's clear that there was an effort to avoid making this a historical prize, i.e. giving this to people purely for great work done in the past (which to some extent the Abel Prize is doing). The recipients are on average in their 40s, at the height of their powers.

One oddity is the award to Kontsevich, who already received $3 million from the Fundamental Physics prize. Given my interests, I suppose I shouldn't criticize a prize structure where physicists get $3 million, mathematicians $3 million, and mathematical physicists $6 million.

While this prize doesn't suffer from the basic problem of the Physics prize (that of rewarding a single, narrow, unsuccessful idea about physics), it's still debatable whether this is a good way to encourage mathematics research. The people chosen are already among the most highly rewarded in the subject, with all of them having very well-paid positions with few responsibilities beyond their research, as well as access to funding of research expenses. The argument for the prize is mainly that these sums of money will help make great mathematicians celebrities, and encourage the young to want to be like them. I can see this argument and why some people find it compelling. Personally though, I think our society in general and academia in particular is already suffering a great deal as it becomes more and more of a winner-take-all, celebrity-obsessed culture, with ever greater disparities in wealth, and this sort of prize just makes that worse. It's encouraging to see that most of the prize winners have already announced intentions to redirect some of the prize moneys for a wider benefit to others and the rest of the field.

Update: Among the private reactions I've heard from prominent mathematicians this morning, one is the desirability of funding a new "sidekick" prize for collaborators of the $3 million winners…

Comments

1. **Als**
   June 23, 2014

   Interesting.

   I think it has the potential to challenge the Fields medal in the long run if the committee chooses wisely the next recipients.

2. **someonesomewhere**
   June 23, 2014

   Well, the Nobel prize was initially also awarded to people who already were famous, so it's not a new concept to initially award the prize to already famous people, so that the fame of the past winners shines on the new ones.

3. **Bill**
   June 23, 2014

   All of them are great mathematicians and deserve the award as much as anyone else, but the selection seems a bit random. Some of them are obviously rewarded for old breakthroughs, not for recent work, and some for work that nobody really understands to call it a breakthrough. What is the main criterion? Starting from next year when only one person gets the award, is it going to turn into another lifetime achievement award or will they reward genuine recent breakthroughs? Why not turn this into a Fields Medal without age restriction – reward for genuine breakthroughs in the last few (4?) years? Actually, the award is big enough to share among possible coauthors (Birkar- Cascini-Hacon-McKernan, Marcus-Spielman-Srivastava, etc).

4. **Bill**
   June 23, 2014

   Shouldn't this be "2014 Breakthrough Prizes in Mathematics"?

5. **Peter Woit**
   June 23, 2014

   Bill,
   These prizes will be awarded in November at a ceremony for the prizes in physics and biology officially called the 2015 prizes. I see the Milner site is naming them not by year, but as "Inaugural". It seems though that the plan is to award the math ones each year at the same time at the others, and next year's will be the 2016 ones, so 2015 seems a better choice than 2014…

6. **Bill**
   June 23, 2014

   From NYT: "The size of the award, I think it's ridiculous," he said. "I didn't feel I was the most qualified for this prize." But Dr. Tao added: "It's his money. He can do whatever he wants with it."

   Not true that he can do whatever he wants with it, you can say no and encourage others to say no, if you believe this is not a good idea. Mathematics community can put Milner's money to good use, but on their terms.

   Also, any rumors that Wiles refused the prize? Obviously, they didn't even dare to ask Perelman. By the way, more kids would be inspired to do mathematics if mathematicians acted more like Perelman. This prize is not going to make them into celebrities in that sense.

7. **Als**
   June 23, 2014

"Update: Among the private reactions I've heard from prominent mathematicians this morning, one is the desirability of funding a new "sidekick" prize for collaborators of the $3 million winners…"

The problem of the side-kick award is that it could feel like an insult. I would be very uncomfortable telling Taylor he gets the "Wiles's sidekick award" for instance.

8. **John Doe**
June 23, 2014

New prizes like this tell me more and more how pointless prizes are becoming.

This post hits the target. What is the purpose of a prize? Why do prizes include money and if they do, how much should it be?

If the purpose of a prize is to 'help Mathematics' (whatever that means) at large, this prize has a big risk, since it puts a lot of power (money is just a measure for power) in the hands of few mathematicians, not lelected for their choices regarding power, but for their solutions to mathematical problems. It is pretty much up to them what they do with it. It is interesting how the piece you link says that Tao tried to reject it but in the end took it because it is up to this guy what he does with his money. He is just going along with the system. If he keeps it, that sounds fake. If he does something else to 'help Mathematics' with it, whatever that is, it is as if he had become a jury for the prize, which shows how pointless this prize is.

I don't think 3 million dollars are going to help these guys to do better math, and I don't think it's going to make any difference other than to their wallets. They'll keep doing what they do, just a bit richer.

9. **Als**
June 23, 2014

BTW, the Milner website says Taylor proved the "Taniyama-Weil conjecture". I wonder who wrote the blurb and kept Shimura out….

10. **Peter Woit**
June 23, 2014

Als,
I think how insulting this might be would depend strongly on the size of the check. At, say $1 million, I think most mathematicians would overcome any sense of grievance about being a "sidekick".

About Taniyama-Weil I also noticed that. If Serge Lang were still alive, it would kill him.

11. **Peter Woit**
June 23, 2014

Bill,

The people Milner chose are relatively young (all younger than me…), and have major results from within the past few years, which would be one reason Wiles is not on the list, not that he turned it down.

I don't want to start a Perelman pro/con discussion, but I disagree with you about Perelman as a role model. I do think though he almost surely would have turned down the money.

12. **Thomas**
June 23, 2014

It would be great if one could read Clozel's exposition of the recent work by Taylor, Patrikis: http://www.institut.math.jussieu.fr/projets/tn/STN/files/annonce-7-4-14.pdf

13. **M.K.**
June 23, 2014

"it's still debatable whether this is a good way to encourage mathematics research"

I disagree on the seemingly obvious conclusion that a high-doted distinction distributed among a small number of already famous mathematicians would lead to less 'encouragement' of mathematics research than a distribution among a higher number of lesser known mathematicians. Given such prizes are also in the latter case based on past achievement of people working on established mathematical institutes (thus established mathematical areas) it is very difficult to see how such prizes would lead to something else than to a substitution of public funding by private or corporate funding. Further, it is very difficult to imagine indeed how a non-mathematical jury, or a random jury, could have the expertise to decide if non-mainstream ideas could be fruitful or not, while indeed the present price-winners are with a relatively high probability capable to decide this. So as usual in our society, everything depends on the good-will of individuals in a system that is organized on questionable measures and aims in general.

14. **Ian**
June 24, 2014

If this prize allows these mathematicians to not apply for grants (for some number of years), then it may serve a useful purpose. Not only would it free up time that might be taken applying for grants and allow them to conduct research unconstrained by grant proposals, but it should also allow others to have a better chance of obtaining them (such as NSF grants, which are getting less funded over time).

15. **Peter Woit**
June 24, 2014

Ian,
That's an interesting point, but given the way these awards are set up and the positions of the people getting them, they generally won't replace NSF grants. One could take a look at what happened in physics, where a quick search shows
http://www.nsf.gov/awardsearch/showAward?AWD_ID=1314311
Although the IAS physics faculty each got $3 million awards in 2012, the IAS is still applying for and getting NSF grants to pay for postdocs and overhead to the institution.

The people getting these awards don't really need research grants to conduct their own research, they typically apply for grants to fund students, postdocs, and their institution. The prize money goes directly to their bank account, not to the institution. If they want to use the money for students, postdocs, their institution they could do this, but would have to set this up themselves, which would likely be every bit as time consuming as the grant application process.

That said, I'm sure some of the prize money will end up being used by the recipients to support research in ways that a grant might have in the past, and can replace some NSF grants. It's not at all clear though how much of that is happening for the physics grants, and how much will happen for the math grants.

16. **Peter Woit**
June 24, 2014

Put differently, the explicit goal and structure of these prizes is to make the recipients personally rich and famous, not to support their research. Once they are rich, in principle they can devote their time to philanthropy and support research, but it's unclear how much of this will happen, will be interesting to see.

17. **Bobito**
June 24, 2014

Why doesn't Xiuxiong Chen or Yan Soibelman deserve a bit of the cake?

18. **Jess Riedel**
June 24, 2014

Two alternative ideas for structuring the prize:

(1) Make it more about honor. Give the mathematician a few hundred thousand dollars, but use the bulk of the money to endow a research position in the mathematician's name. (An MIT professorship is $3M: http://giving.mit.edu/priorities/faculty/)

(2) Give the money to the awardee, but require them to pledge to not write grant proposals for 5 years. Maybe unfeasible since they need to write grants to support their students, postdocs, and institution, but part of the award could go to cover this.

19. **Peter Woit**
June 24, 2014

Jess,
Good suggestions. I've often wondered why more of this private money isn't going into the traditional philanthropic path of endowing positions, which would have a big, long-term effect on the health of the field (Simons has the "Math + X" program, but I can't think of many other examples).

If each year, instead of giving a $3 million personal check to a string theorist the Breakthrough Prize people endowed a position in string theory, the effect on the subject would be huge (and would do a lot more to encourage young people to go into it).

20. **Bork**
June 24, 2014

I agree that this is a double-edged sword, even for the winners. Now they have to worry about what to do with all that money. If they spend it on themselves, they will be criticized, or are likely to feel so, and if they try to distribute it somehow, in some sort of "fair" way, that could eat up time and cause a headache and ironically take them away from research. And their choices could, again, be criticized.

Of course the type of mathematician who likes being a big honcho would love that, but my guess is these are not of that sort. They probably would much prefer just to, basically, be left alone. Perlman was only the extreme version of that.

What a research mathematician really needs is: more free time, plus a modicum of financial stability, plus a good research environment. A good research environment includes: happy colleagues, and good basic education.

This is why Simons' donations seem much more creative and better thought out,
and much more likely to be effective.

One of the great things about mathematics as a human endeavor and a part of human culture and as a path through life, is the relative equality and openness
compared to other realms of activity. This sort of prize might end up doing more harm than good to the overall culture and enterprise of mathematical life and mathematical research.

I say, give a $100,000 prize, and then just give half the remaining $$ to Simons to spend as he sees fit, and give the other half to lobbyists in support of: the NSF funding of basic research, and a bigger NSF budget; and basic school education reform: not Bill Gates style (test, test, iPad, test) but small class sizes, sabbaticals and better salaries for teachers, real books for kids, as well as bringing back instruction in music, cursive writing, PE…oh and math and physics I suppose, why not…

21. **Patrice Ayme**
June 25, 2014

Milner is a celebrity. And a financial manipulator who became immensely wealthy with what Roosevelt and the Bible called contemptuously "money changing". He profited immensely of a system, plutocracy, that is mostly about oligarchy pushed so far, that even the character of those "leaders" become diabolical.

It's diabolical to make us believe that mathematics will progress more by giving more power to those who have more than enough to do good math.

Overall, science and mathematics do not have enough practitioners. A striking example is antibiotic research where a small effort needs to be done to find new antibiotics. To have a few individuals who are much richer will have no positive effect whatsoever. This is certainly true in math and physics.

In biology, immense greed has clearly undermined research (individuals have made up to half a billion dollar a year in that field, but one cannot find the modest finance for new antibiotics research). Making a few persons very rich promotes greed.

So why is Milner doing this? Maybe it's subconscious. The oligarchic principle is that humanity is unworthy, but for a few celebrities who never get enough. This is what Milner is truly rewarding. His reason for being what he is. Someone obsessed by individual power.

Want to help science and math? Finance studies on how to persuade governments to finance enough advanced public free instruction in science and math, starting in preschool. Through heavy taxation of the richest celebrities, starting with Milner and his kind.
http://patriceayme.wordpress.com/

22. **Bill**
June 26, 2014

Jess Riedel,

Endowing a research position is the best idea I've seen so far. I would add that such a position should not be given permanently to one person until his/her retirement but instead given for a period of, let's say, 10 year to an active researcher.

23. **NLR**
June 26, 2014

Endowing a research position is a good idea, but the problem with the Milner Prize is similar to a remark made by Norbert Wiener said in his book "Ex-Prodigy," that, often, people who are already well-known and well-rewarded get more awards, resulting in a "pyramid of awards." However, the people who could most benefit from such awards are lesser-known researchers who do not have financial stability.

Also, the idea of making mathematicians celebrities is completely at odds with the reason to do mathematics. The value of math does not come from fame or money, but from the intrinsic interest and value of mathematics. In fact, I would think that many people study math precisely to engage with a part of the world that has nothing to do with things such as money or celebrity.

24. **Anders**
June 27, 2014

In Economics we have something called tournament theory to try and explain the very large CEO remuneration you often see in the US. Basically the idea is that you overpay the CEO to get people further down in the organization to work very hard, to have a shot at the CEO position. Maybe these prizes in science can work the same way, they do not change the productivity of the recipients but maybe of other researchers that have a decent chance of getting a prize later in their career.

Pierre van Baal 1955-2013

June 23, 2014
Categories: Obituaries

I only recently heard about the death late last year of Dutch particle theorist Pierre van Baal. Pierre was my office mate when we were both postdocs at Stony Brook during the mid-eighties, and he was one of the people I most enjoyed talking with about physics during those years. He arrived at Stony Brook after completing a Ph.D. with Gerard 't Hooft, and later went on to CERN and ultimately a professorship in Leiden. I last saw him at a conference in Stony Brook in 2008 (described here) where he told me that he had suffered a serious stroke in 2005. Pierre was quite modest, and always a cheerful and optimistic presence in the room. In 2008 he was still very much himself, but more halting in his speech. From what I remember, he told me that he was resuming teaching, but was frustrated that he was no longer capable of engaging in research work.

The lecture notes of his class on quantum field theory are available (an online version here, a published version here). Like Pierre himself, they're a model of clear and concise thinking and exposition. Last year a book with a selected collection of his papers was published, see here.

A major theme in Pierre's work was the study of quantum gauge theory via semi-classical methods, for the case of a system in a "box", i.e. finite extent in space and time dimensions. In the Euclidean picture, periodic boundary conditions in time correspond to doing computations at non-zero temperature, inversely proportional to the size of the time dimension. As a result, this work is quite relevant to the study of QCD at finite temperature, including the expected deconfining phase transition.

While Pierre was not really a lattice gauge theorist, his work was highly relevant to lattice gauge theory, where computer simulations inherently take place in a finite box, and understanding the effects of this on the physics is crucial. As a result, Pierre was well-known in the lattice gauge theory community. This week Columbia University is hosting Lattice 2014, the big yearly meeting for lattice gauge theorists (plenary talks are streamed on livestream.com). This morning I attended the talk by Michael Mueller-Preussker on Recent results on topology on the lattice, which was given in memory of Pierre. The slides have a lot more information about Pierre and his work, as well as surveying the latest on lattice results involving the topology of gauge fields.

For more about Pierre, see this site, which has an appreciation written by Chris Korthals Altes (in English here), as well as pieces by 't Hooft and Hans van Leeuwen (in Dutch).

Comments

1. **Peter Orland**
   June 24, 2014

   Pierre was a wonderful physicist and a great guy. Too bad he died so young.

2. **JD M**
   June 27, 2014

   Off-topic: Bruno Zumino passed away on June 21, 2014:

   http://newscenter.berkeley.edu/2014/06/24/bruno-zumino-an-architect-of-supersymmetry-dies-at-91/

   and

   http://www.dailycal.org/2014/06/25/bruno-zumino-renowned-uc-berkeley-physicist-dies-91/

String Theory Visions

June 27, 2014
Categories: Strings 2XXX

Strings 2014 ended today, with five separate "vision" talks, giving a good picture of where the leaders of the string theory community see the subject going. I saw the talks on streaming video, presumably they should appear on the conference website in the next few days.

- Michael Green led off by noting that string theory had at one point been intended as a unified theory, but now has "blossomed into something much more significant", a framework covering all sorts of things. He went on to say that he would avoid discussing the grand questions, and instead just give a summary of what he found interesting about work reported at the conference. The main area that he covered in the talk were the many different kinds of results about scattering amplitudes that are under study. He seemed to be somewhat of a skeptic about Arkani-Hamed's widely publicized "Amplitudehedron", saying it had some ways to go before it was useful for computations (unlike other methods).
- Juan Maldacena gave a talk that had nothing to do with string theory, mainly about the conceptual issues of black holes and quantum theory. He claimed that the BICEP2 results showed that quantum gravity was now an experimentally-based subject, answering those who were skeptical about studying an untestable subject.

  At the end of the talk he gave an answer to the question "What is String Theory?":

  > **S**olid **T**heoretical **R**esearch **I**n **N**atural **G**eometric **S**tructures

- Andy Strominger gave his own answer to the "What is String Theory?" question:

  > anything that anybody in this room or any of their friends has ever worked on.

  He did note that there were hardly any strings anywhere to be seen at recent string theory conference talks.

  About the LHC and any conceivable follow-on higher energy accelerator, his comment was that it was now highly unlikely that string theory could make predictions relevant to them, and that he didn't want this to be a defining goal of the field. Clearly, the failure to find SUSY at the LHC has now pretty much killed off most hopes that string theory unification is relevant to particle physics in any testable way. Like Maldacena, he pointed to BICEP2 as showing that quantum gravity was now an experimental subject.

  He ended by explaining that he had sent around emails to a hundred people asking for their suggestions about what problems there would be progress on over the next 5-10 years. He got 80 responses, and quickly put up some slides with them. No time to really read these, but he says that they'll be posted online soon, and that should be a quite interesting document

- The last talk was by David Gross, who pointed out that he has given many of these things before. He then went on to discuss Paul Steinhardt's impassioned talk earlier in the week, which included a video of Richard Feynman discussing "Cargo Cult Science" and theories that were too vague to be testable. Steinhardt had been arguing that inflation was so vague and flexible a theory that it could not be tested and so was not science. Gross like many others realized that you could replace "inflation" by "string theory" in Steinhardt's argument. He then gave a long and very defensive discussion of why string theory might still be science, invoking the recent book by Richard Dawid and telling the audience they needed to read it so they could defend their subject against the accusations it is facing.

  I wrote here last year about the Dawid book, including an explanation of Dawid's three main arguments for string theory. Gross went through these in detail, and I think what I wrote last year also responds to what Gross has to say. He did include in the "Meta-Inductive Argument" the argument that SUSY is a related research program to string theory and that it will be vindicated at the LHC in the next few years. It will be interesting to hear what he has to say at a future Strings 20XX after this hasn't worked out. He announced that Strings 2015 will be in Bangalore, Strings 2016 in Tsinghua, Strings 2017 in Israel, Strings 2018 in Japan and Strings 2019 in Belgium.

  He explicitly addressed the fact that many in the field were experiencing depression and anxiety due to things not working out, pointing out that even if the first derivative of progress in a field is negative, there can be jump discontinuities. So, although things don't look good, maybe a big piece of progress will come along out of nowhere.

There was one more vision talk, but I'll discuss that in a separate posting.

Update: Strominger's slides are available here, and include the 80 responses he got from others about the open problems of the field.

Update: Videos of the talks are now available.

Comments

1. **srp**
   June 27, 2014

   The discussion here about the LHC, SUSY, and strings reminds me of the argument about BICEP2, gravitational waves, and inflation. Absence of SUSY at LHC is taken here as a blow to strings, but finding SUSY wouldn't have been seen here as supportive of string theory unification. So the asymmetry is similar to Linde's attitude that it would have been a "smoking gun" for inflation had gravitational waves been found but no crushing blow had it not.

2. **Peter Woit**
   June 27, 2014

   srp,
   I'm not sure what you mean. The prospect of SUSY at the LHC was always advertised as the main prospect of experimental vindication of the string theory picture, and the failure of it to show up is behind both the "depression and anxiety" Gross describes, as well as the disappearance of mention of the LHC from the conference.

   Gross and some others are still holding out for vindication from LHC Run II, but I think most string theorists are now agreeing with Strominger. Once you give up on experimental HEP as having any relevance to what you do though, you have to face the fact that other physicists are going to now be skeptical about whether you're still doing science. Both Strominger and Maldacena tried to address that by invoking BICEP2, but that comes with its own problems. This concern was also clear in Gross's talk.

3. **srp**
   June 27, 2014

   Please allow me to clarify. I'm not concerned with who advertised what in advance, but with the logical structure of the inferences being made. In this case I agree with your position, but it is inconsistent with how you approached the BICEP2 affair.

In the "Smoking Gun" thread, a bunch of people (including me) argued that it was not inconsistent to see the BICEP2 finding of gravitational waves as a "smoking gun" for inflation if true but not a showstopper for inflation if false. In other words, we believe that this kind of asymmetry of inference based on a particular piece of evidence may be completely rational. One commenter drew an analogy with discovering life on another planet–that would be a smoking gun for the non-uniqueness of terrestrial life, but failing to find life on that particular planet wouldn't measurably change your prior belief in extra-terrestrial life existing somewhere.

Then this post made me realize that you were arguing (implicitly) for the same kind of asymmetry in the case of SUSY and the LHC. A positive finding of SUSY would NOT have led you to accept string theory unification as true, but the negative findings to date DO lead you to significantly decrement your probability that it is the right theory. I agree with this stance and simply point out that it is logically the same kind of asymmetry that you criticized in the case of inflation. Of course you may disagree with this assessment, but perhaps my contention is now clear.

4. **Peter Woit**
June 27, 2014

srp,
Actually a discovery of SUSY would have led me to dramatically revise my views, whereas what happened didn't because it was what I expected. But this is really off topic, enough.

5. **Claudine Gérard**
June 28, 2014

Dear Peter,

looking through the slides of the conference and your summary of the vision talks I am doubly puzzled. First, as you said, only few talks are about strings and membranes. But on top of that, no talk is really about unification itself! Even in the vision talks, the speakers avoid the issue. I am puzzled that string researchers seem not to pursue unification any more. Not giving a public talk is surely a natural consequence of this change. But what then is the reason for this conference?

It sure looks as if a group of smart people heard, about 30 years ago, some voice shouting "Unification is in that green pasture over there!" They all run there, ploughed the field, found nothing, and now are saying to each other: "But we are in a beautiful field, aren't we?" This might well be, but why are they there in the first place?

Isn't this avoidance to even talk about of unification – see how openly Green and Maldacena avoid it – the real proof that string and M theory has failed? Peter, we do not need you any more to point out the failure of string theory: the field's elders do it all by themselves.

If the conference invites five people to give a vision talk, and nobody of these five people talks about prospects for unification, then something is wrong. Deeply wrong.

Peter, allow me a conjecture. In theoretical particle physics, it has become common to label people who work on unification as foolhardy or presumptuous. The conjecture is that string theorists now do it among themselves. It seem that they do not dare to even think about unification any more. Is there any hope for change?

6. **Peter Woit**
June 28, 2014

Claudine,
Strominger did address the unification issue, basically saying that he thought it didn't work: he thinks there is no way string theory is going to say anything about particle physics (as opposed to quantum gravity).

While some string theorists still work on unification, they generally weren't invited to talk at this conference. I think that's not because they're perceived as foolhardy by the organizers, but because the organizers are aware those ideas are not working. Why invite someone to talk about a very old research program that has been shown not to work, unless they have found a way to get around the problems?

7. **SUSY**
June 29, 2014

LHC Run II – how long after 2015 will results be announced on SUSY? other than SUSy what other physics do they hope to see at 13TEV?

8. **Peter Woit**
June 29, 2014

SUSY,
There might be some early results on SUSY summer 2015, I'd guess results based on a data set of similar size to Run I would start winter 2016. I'm sure there will be searches for all the same things as Run I: extra dimensions, Zprime, black holes, you name it.

9. **Maurice Carid**
June 29, 2014

Hi Peter,
when Strominger calls the progress
of the last 30 years "incredible" and
likens the present with the situation
of physics in the early 20th century:
is this whistling in the dark, propaganda
or does he really believe this?
cheers maurice

10. **Peter Woit**
June 29, 2014

Maurice,
Calling the last thirty years a period of "incredible" progress is just hype, and I doubt he or anyone else believes this in a serious way.

Likening the present to the early 1900s is a common way of expressing hope for some imminent arrival of new, successful revolutionary insights into physics analogous to relativity and quantum theory. I don't see any argument there, just an expression of wishful thinking for major progress to come soon (unlike the "incredible" progress of the last 30 years…)

11. **Elbi Gilgen**
June 29, 2014

The progress of the last 30 years *has* been incredible, strominger is absolutely right. The only problem is the sign.

12. **Christian Müller**
June 30, 2014

The numerous answers that Strominger collects miss the following:
– The origin of the mass of the electron
– The origin of the fine structure constant
– The origin of the other standard model parameters
So not it seems that not only Strominger, but also all his colleagues have given up on these issues.

It remains amazing how a whole field is digging its own grave in public. A big change is needed: both in the approach and in the required optimism.

13. **Shantanu**
June 30, 2014

Peter or others: can someone point me to the videos of these talks.
I couldn't find them from
http://physics.princeton.edu/strings2014/Talk_titles.shtml
but maybe they are elsewhere
Thanks

14. **Peter Woit**
June 30, 2014

Shantanu,
As far as I know videos have not been posted yet, but the intention is to do so at some point at the page you link to. My comments based on watching the talks were based on watching some of them in real time (they were being streamed live).

15. **Christian Marks**
July 5, 2014

Richard Dawid was very recently interviewed by Richard Marshall of 3AM Magazine about his book, "String Theory and the Scientific Method."

16. **Peter Woit**
July 7, 2014

Christian,
Thanks! Will write something about this interview soon.

17. **peter hickman**
July 8, 2014

With reference to the interview:
Richard Dawid was very recently interviewed by Richard Marshall of 3AM Magazine about his book, "String Theory and the Scientific Method."

The message from string theorists "there is no alternate theory, supports their view that string theory is the so called final theory" is naïve.
There was no alternative to Newtonian physics for c.200 years, until the advent of Special Relativity and Quantum mechanics.

Feynman quote summaries what the scientific process is:
In general we look for a new law by the following process. First we guess it. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation, to see if it works. If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is – if it disagrees with experiment it is wrong. That is all there is to it.
chapter 7, "Seeking New Laws," p. 156 [as presented in edited book]

This is the salient point for any theory: "It does not make any difference how beautiful your guess is".

Peter

18. **Chris W.**
July 8, 2014

A PS on Peter Hickman's comment: The book in question is *The Character of Physical Law* (1965).

Physical Mathematics and the Future

June 27, 2014
Categories: Strings 2XXX

The "vision" talk at Strings 2014 that I found most interesting was that of Greg Moore, whose topic was "Physical Mathematics and the Future". He has a very extensive written version of the talk here, which includes both what he said, as well as a lot of detail about current topics at the interface of mathematics and physics.

I think what Moore has to say is quite fascinating, he's giving a wonderful survey of where this intellectual subject is, how it has gotten there, and where it might be going. I'm very much in sympathy with his historical discussion of how math and physics have evolved, at times closely linked, at others finding themselves far apart. He's concerned about an issue that I've commented on elsewhere, the fact that physics and math seem to be growing apart again, with no mathematicians speaking at the conference, instead attending their own conference ("Strings-Math 2014"). Physics departments increasingly want nothing to do with mathematics, which is a shame. One reason that Moore gave for this I found surprising, the idea that

> most mathematicians are not as fully blessed with the opportunities for pursuing their research that many theoretical physicists enjoy.

It seems there's a perception among many physicists that research mathematicians labor under some sort of difficult conditions of low pay and high teaching loads, but I think this is a misconception. Moore may be generalizing too much from the situation at Rutgers, where very unusual positions were created for string theorists at the height of that subject's influence. From what I've seen, the salaries of top research mathematicians and theoretical physicists are quite comparable (if you don't believe me, do some searches in the on-line data of salaries of faculty employed by public universities). Senior mathematicians do sometimes have slightly higher teaching loads, although often with a freedom to teach what they want. At the postdoc level, it is true that theoretical physics postdocs typically have no teaching, while similar positions in math often do require teaching. On the other hand, the job situation in theoretical physics is much more difficult than in mathematics. I'd say that working in an environment where you know you're likely to find a permanent job is much preferable to one where you know this is unlikely, with doing some teaching not at all a significant problem.

On the question "What is String Theory?", Moore's take was that the "What is M-theory?" question is no longer getting much attention, with people kind of giving up. There was a very odd exchange at the end of the talk, when Witten asked him if he thought that maybe people should be emphasizing the string question, not the M-theory question, and Moore responded that the emphasis on M-theory was something he had learned from Witten himself.

His main point about this though was one I very much agree with, that the more interesting question now is "What is QFT?". The standard way of thinking about QFTs in terms of an action principle doesn't capture much of the interesting things about QFT we have learned over the years. Moore emphasizes certain examples, such as the (2,0) 6d superconformal theories, but discusses in his written version the relation of QFT to representation theory of some infinite dimensional groups, which I think provides even better examples of a different and more powerful way of thinking about QFT.

The written version contains a wealth of information surveying current topics in this area, is highly recommended to anyone who wants to try and understand what people working on "string theory" and mathematics have been up to. It appears that this document is a work in progress, with more material possibly to come (for instance, there's a section 4.4 on Geometric Representation Theory still to be filled in). I look forward to future versions.

Comments

1. **Giotis**
   June 28, 2014

   All these questions about the meaning of QFT etc. originated and formulated in a meaningful way within String theory, so trying to decouple them is unnatural to say the least; they can be understood and addressed only within this general framework provided by String theory.

   In the future the degree of convergence between the two will increase even more and I believe at some point there would be no distinction in practice between a Quantum field theorist and a String theorist.

2. **Peter Woit**
   June 28, 2014

   Giotis,

   I see that you're using Strominger's definition of "string theorist".
   The great thing about Moore's talk was that it had a lot of serious ideas and content, wasn't just the sort of "math shows that string theory rules" sloganeering that goes on in "vision" talks like this.

3. **george ellis**
   June 28, 2014

   Towards the end he says "On the other hand, the relative lack of reliance of Physical Mathematics on laboratory experiments is viewed – with some justification – as dangerous by many physicists. The dangers of relying on "pure thought" when divining the secrets of Nature are well-known and illustrated by multitudinous examples."
   Just so. The name Physical Mathematics is well chosen: virtually all the topics he discusses have have no clear relation to the physical properties of the real universe, so its mathematics rather than physics.
   And that seems to apply also to the references to QFT in the paper ("Moore emphasizes certain examples, such as the (0,2) 6d superconformal theories"). Is there something useful there about the 4-d QFT that apparently describes the real universe? Or are these QFT theories physical mathematics rather then mathematical physics?

4. **Peter Woit**
   June 28, 2014

   george ellis,

   I think we already know the QFT that describes the real universe, it's called the Standard Model (sure, it has a problem with quantizing gravitational degrees of freedom, but hundreds of physicists will tell you they know what to do about that…). To me, the question is whether we can understand that QFT better, in particular finding explanations of aspects of it that now look rather ad hoc. The conjecture that a better understanding of a QFT like the SM is going to require a deeper understanding of QFTs in general (or at least of a class of them) seems reasonable, and such a deeper understanding may very well involve new ideas about the relation of qfts to mathematics. That there is a vast amount we don't understand about qfts and mathematics is undeniable. One can't be sure that understanding more will help understand the real world, but one can be sure that it will lead to new mathematics. In any case, it seems to me more worth doing that most of what theorists working on fundamental physics spend their time on these days.

My own personal take on promising ways to go forward with this overlaps in some places with Moore's, not at all in others.

5. **martibal**
June 28, 2014

Giotis: there is at least one area of mathematical physics which is asking very strongly and deeply what qft is, and it has nothing to do with string theory. This is called algebraic qft.
There is at least one other area of mathematical physics which is asking very much why the standard model is as it is, and it has nothing to do with string theory. This is called noncommutative geometry.
Is it asking string theorists too much to have a little bit of culture out of their field ?

6. **Giotis**
June 29, 2014

Martibal,
I really don't want to argue since the purpose of my comment wasn't to claim that there is no QFT research outside String theory but to highlight the fact that the two are so intimately interrelated nowadays that one is forced to treat them within the context of one general, unified theoretical framework.

BTW saying that non commutative geometry has nothing to do with String theory is absurd. The very presence of D-branes renders space non commutative. Non commutative geometry is a hot topic in String theory research.

7. **martibal**
June 29, 2014

All the noncommutative geometry approach to the standard model (in Connes framework) has nothing to do with string theory.

That string theory is so intimately interrelated with qft that one is forced to treat string theory within the context of qft may be true, I do not know.
The converse obviously is not true.

8. **chris**
June 30, 2014

the MSSM is so intimately connected to the standard model that in fact it is equivalent to it in a certain, well-defined limit. it is fair to say that they are so much connected, that one is forced to treat both the SM and MSSM in the context of one general, unified theoretical framework 🙂

9. **Jesper**
June 30, 2014

@ Chris – the noncommutative formulation of the standard model coupled to gravity in terms of spectral triples and an almost-commutative algebra does not permit MSSM (see http://arXiv.org/pdf/1211.0825.pdf). It is possible (but not natural) to incorporate supersymmetry in this framework, but there is certainly no equivalence.

10. **Urs Schreiber**
July 2, 2014

@martibal,

in fact the spectral (Connes's NCG-type) perspective on geometry, where Riemannian geometries are characterized by the quantum mechanics of superparticles roaming in them (that's what a spectral triple encodes) is a simplified version of how effective target space geometry is encoded in string theory as whatever the quantum superstrings roaming in them sees, as witnessed by their worldsheet SCFT.

See here for more discussion of this relation, with more pointers to the literature: http://physics.stackexchange.com/a/104299/5603

11. **also**
July 3, 2014

To add to Urs' comment:

If I remember right, Connes' original version of con-commutative standard model had a crisp prediction for the Higgs mass. A prediction that was immediately ruled out by Femilab, even before LHC.

Now one could argue whether Connes-like noncommutativity could be embedded in string theory. I am no expert on that, but my first gut reaction on hearing about it at the time, was: "no". So I was happy that it was ruled out pretty instantly. It seemed way too much like a UV Lorentz-violating theory, but maybe I am forgetting.

12. **Yatima**
July 4, 2014

If I remember right

You seem to remember wrong:

1) Original "prediction" 160–180 Gev (not Tevatron range for sure)
2) Experimental: 125 GeV/c^2, Goddammit!
3) Fixed in [Resilience of the Spectral Standard Model]()

For reference:

[Higgs-mass predictions]()

13. **martibal**
July 4, 2014

@Urs: thank for the references. I did not know them so I will have a close look before commenting further. But at first sight I do not see where the "super particle roaming" in Connes description of the NCG are. Besides a Riemannian manifold and its Dirac operator, one has a finite dimensional algebra acting on a finite dimensional Hilbert space and a "finite dimensional Dirac operator" (in practical: a mass matrix). The fermions are the basis of this finite dimensional Hilbert space, the bosons (including the Higgs) are obtained as gauge fields (using a noncommutative generalization of a connection). I do not see super particles there. But I have to study your reference.

14. **martibal**
July 4, 2014

@Yatima and also: the prediction of the Higgs was indeed around 170 GeV (the uncertainty is mainly due to the uncertainty in the Top mass), and it has been ruled out by Tevratron in 2008. In Resilience of the Spectral Standard Model, Chamseddine and Connes have shown that by taking into account a new scalar field (proposed by particles physicist to cure the instability of the electroweak vacuum due to the "low" mass of the Higgs), then it was possible to get the correct mass for the Higgs. In the above mentioned paper, the way to introduce this new field following the "rules" of NCG was a bit rough. This has been made smoother in a successive paper of Chamseddine, Connes and v. Suijlekom [at this point I hope Peter will allow me to break the non-self-publicity rule to say that another way to generate the new field within the NCG framework had also been proposed by a "Napolitan team"]

15. **Peter Woit**
July 4, 2014

All,
Enough about NCG, thanks.

Quick Links

July 8, 2014
Categories: Uncategorized

- Jim Simons is profiled in the Wall Street Journal yesterday, the New York Times today. The WSJ piece is partly about a recent $50 million donation to the Simons Center for Quantitative Biology at Cold Spring Harbor, but it reports that Simons is moving away from "broad institutional support", in favor of "collaborative, goal-driven science". Recently Simons has funded the Simons Array of telescopes that will be looking at polarization in the CMB, and the NYT piece reports that he was talking to Stanford physicists working on experiments looking for the axion. Simons is estimated to have a net worth of $12.5 billion, the Simons Foundation now has $2 billion.
- Quite a few years ago I started a trip to Paris by getting off the plane from New York and heading directly to attend talks at the Seminaire Bourbaki. The main thing I remember now of that is an epic struggle to stay awake, since I hadn't slept on the plane, and the room was rather overheated. There's now a much better way to enjoy talks from this historic program, which since its inception in has been the source of some of the great expositions of new mathematics. Talks are on Youtube, links are on the latest program (learned about this from Emmanuel Kowalski's blog).
- In other news from France, this year's Baccalaureat exam features questions about the Higgs and the LHC. They start off with a quote from Carlo Rovelli about the Higgs discovery being "as important for intellectual history as Newton's law of gravitation". Rovelli's reaction: "I've never thought such a stupid thing." For more on the mini-controversy, see here.
- For more videos to watch, Oxford has an interview with Atiyah here, Penrose here. Cambridge has a large collection of such video interviews, including Peter Swinnerton-Dyer, John Coates, Martin Rees and John Polkinghorne.
- The AMS has been encouraging discussion in the mathematical community of the implications of the Snowden revelations about the activities of the NSA, supposedly the largest employer of mathematicians in the US. This month's Notices includes pieces from Keith Devlin and Andrew Odlyzko, introduced by Michael Harris and Allyn Jackson. Further contributions to this discussion are encouraged.
- On the abc conjecture front, perhaps the planned lecture series this September by Go Yamashita will give mathematician's a fighting chance to understand Mochizuki's claimed proof. While the talks will be in Japanese, presumably Yamashita will be producing something written in English.

Comments

1. **Nick M.**
   July 9, 2014

   Hi Peter,

   I'm not sure if you caught this (and if it isn't on topic with your "Quick Links" post, then I apologize), but it seems that there is yet another reason to believe that the multiverse *didn't* do it; i.e., see the New Scientist article "Biggest void in universe may explain cosmic cold spot", as well as the arXiv paper (sited in the article) here.

2. **Anon**
   July 9, 2014

   Also this: http://www.science20.com/a_quantum_diaries_survivor/new_lhc_diboson_excesses_point_to_light_susy-139965

3. **BHeller**
   July 14, 2014

   This may be interesting for those who don't find pleasure in supersymmetry, strings, the multiverse, etc. but in the Standard Model:
   https://www.youtube.com/watch?v=wHFN1rVF8Ds

4. **Bill**
   July 16, 2014

   Will you have a discussion of a recent Quanta article on your blog? It was an interesting article, curious to hear what people think about this.

5. **Hendrik**
   July 20, 2014

   There is a very enjoyable article by Carlo Rovelli on
   http://www.newrepublic.com/article/118655/theoretical-phyisicist-explains-why-science-not-about-certainty

6. **Jakob**
   July 21, 2014

   I totally agree with Bill. Would love to hear your opinion about those recent fluid-quantum experiments, that may lead to a comeback of the disfavoured Bohmian mechanics

7. **Peter Woit**
   July 21, 2014

   Jakob and Bill,
   I haven't followed that closely, my impression is that it just shows you can find a macroscopic classical system with features described by the Schrodinger equation. This may be useful for visualizing some aspects of elementary QM, but I don't see how it tells you anything fundamental about QM or gets rid of any of the problems of Bohmian mechanics.

   Anyway, sorry, but I'm just profoundly unsympathetic to the whole idea of replacing the beautiful mathematical structure of QM (that I'm writing a book about) with classical pdes. If people want to discuss this, they need to find somewhere else, run by someone with enough interest to moderate a discussion.

8. **Marcus**
   July 28, 2014

   The video of Steinhardt's talk has been posted at the Strings 2014 website.
   It is 28 minutes long including a brief exchange with Raphael Bousso at the end.
   The presentation is a model of calm and cogency, IMO. Worth watching.
   http://physics.princeton.edu/strings2014/videos/talk1h.mp4

String Theory and Post-Empiricism

July 10, 2014
Categories: Favorite Old Posts, Uncategorized

Note: This is being published simultaneously here and at Scientia Salon. Discussion will be at the Scientia Salon site.

Last month's Strings 2014 conference in Princeton included two remarkable talks by prominent physicists, both of whom invoked philosophy in a manner unprecedented for this kind of scientific gathering. On the first day, Paul Steinhardt attacked the current practice of inflationary cosmology as able to accommodate any experimental result, so, on philosophical grounds, no longer science. He included a video clip of Richard Feynman characterizing this sort of thing as "cargo cult physics". On the final day, David Gross interpreted Steinhardt's talk as implicitly applying to string theory, then went on to invoke a philosopher's new book to defend string theory, arguing that string theorists needed to read the book in order to learn how to defend what they do as science.

The book in question was Richard Dawid's String Theory and the Scientific Method, which comes with blurbs from Gross and string theorist John Schwarz on the cover. Dawid is a physicist turned philosopher, and he makes the claim that string theory shows that conventional ideas about theory confirmation need to be revised to accommodate new scientific practice and the increasing significance of "non-empirical theory confirmation". The issues of this kind raised by string theory are complex, so much so that I once decided to write a whole book on the topic (*Not Even Wrong*). A decade later I think the arguments of that book still hold up well, with its point of view about string theory now much more widespread among working physicists. One thing I wasn't aware of back then was the literature in philosophy of science about "progressive" vs. "degenerating" research programs, which now seems to me quite relevant to the question of how to think about evaluating string theory.

I've written a bit about the Dawid book and earlier work of his (see here and here), although as for any serious book there's of course much more to say, even if I lack the time or energy for it. Recently an interview with Dawid appeared, entitled string theory and post-empiricism, which summarizes his views and makes some claims about string theory critics which deserve a response, so that will be the topic here. In the interview he says:

> I think that those critics make two mistakes. First, they implicitly presume that there is an unchanging conception of theory confirmation that can serve as an eternal criterion for sound scientific reasoning. If this were the case, showing that a certain group violates that criterion would per se refute that group's line of reasoning. But we have no god-given principles of theory confirmation. The principles we have are themselves a product of the scientific process. They vary from context to context and they change with time based on scientific progress. This means that, in order to criticize a strategy of theory assessment, it's not enough to point out that the strategy doesn't agree with a particular more traditional notion.
>
> Second, the fundamental critics of string theory misunderstand the nature of the arguments which support the theory. Those arguments are neither arbitrarily chosen nor uncritical. And they are not decoupled from observation. String theory is indirectly based on the empirical data that drove the development of those theories string theory aims to unify. But more importantly for our discussion, the arguments for the viability of string theory are based on meta-level observations about the research process. As described before, one argument uses the observation that no-one has found a good alternative to string theory. Another one uses the observation that theories without alternatives tended to be viable in the past.

Taking the second part of this first, Dawid seems to be claiming that Smolin and I don't understand what he calls the "No Alternatives Argument" (discussed in detail in his book, as well as in this recent paper). In response I'll point out that one of the concluding chapters of my book was entitled "The Only Game in Town" and devoted explicitly to this argument. To this day I think that a version of such an argument is the strongest one for string theory, and is what motivates most physicists who continue to work on the theory. The version of this argument that I hear often privately and that has been made publicly by theorists like Edward Witten goes something like:

> ideas about physics that non-trivially extend our best theories (e.g. the Standard Model and general relativity) without hitting obvious inconsistency are rare and deserve a lot of attention. While string theory unification hasn't worked out as hoped, we have learned a lot of interesting and unexpected things by thinking about string theory. If they see a new idea that looks more promising, string theorists will shift their attention to that.

This is a serious argument, one that I tried to seriously address in the book. Beyond that, more naive versions of it seem to me to have all sorts of obvious problems. Of course, if you really can show that alternatives to a given model are impossible, that's a convincing argument for the model, but this is rarely if ever possible. Working scientists beating their heads against a hard problem are always in the position of having "no alternatives" to some flawed ideas, until the day when someone solves the problem and finds the alternative. The only example I can recall seeing from Dawid of a successful example of the "No Alternatives Argument" is the discovery of the Higgs, and I find that very hard to take seriously. Pre-2012, the Standard Model was a very precise and exhaustively tested theory, providing a huge amount of indirect evidence for the Higgs. There were plenty of alternatives (technicolor, SUSY, etc.), all much more complicated and with no evidence for them. Making a "No Alternatives Argument" for a theory with overwhelming experimental evidence behind it is something completely different than trying to do the same thing for a theory with zero experimental evidence.

As for the other mistake that Dawid thinks string theory critics make, that of believing in some unchanging notion of empirical theory confirmation, the first thing to point out is that of course every theorist is well aware that one can can't just demand experimental predictions and confirmation for ideas, that one spends basically all one's time working on better understanding ideas that are far from the point where empirical confirmation comes into play. The second thing to point out is that I agree completely with Dawid that as experiments become more difficult, one needs to think about other ways of evaluating ideas to see if they are going anywhere. The last chapter of my book was devoted to exactly this question, arguing that physicists should look carefully at how mathematicians make progress. Mathematics is certainly "post-empirical", and while logical rigor is a constraint, it is not one that necessarily points mathematicians to fertile new ideas. There is a long history and a deeply-ingrained culture that helps mathematicians figure out the difference between promising and empty speculation, and I believe this is something theoretical physicists could use to make progress.

The epigram from that last chapter though was something that kept going through my head when thinking about this, a line from Bob Dylan's "Absolutely Sweet Marie":

> But to live outside the law, you must be honest.

Yes, theoretical particle physics is in a stage where empirical results are not there to keep people honest, and new and better "post-empirical" ways of evaluating progress are needed. But these must come with rigorous protections against all-too-human failings such as wishful thinking and Lee Smolin's "groupthink", and I just don't see those anywhere in Dawid's proposal for new kinds of theory confirmation.

I'd like to thank Massimo Pigliucci for the opportunity to write something here at Scientia Salon, and hope it will generate an interesting discussion. Contributions from philosophers to this kind of debate in physics I believe are very much needed, on this issue and others. Don't even get me started about the multiverse…

Update: Frank Wilczek, unlike Gross and Schwarz, is not a fan of Dawid. From Twitter:

> Wheeler: "Mass Without Mass", "Charge Without Charge", "Field Without Field" http://bit.ly/1p2cWQv
> Dawid: "Physics Without Physics"

Update: Sabine Hossenfelder blogs about this here, with a response from Dawid here. Dawid writes

> My claim is that a strong record of empirical confirmation in a research field under certain conditions can increase the probability of the viability of other theories in the field that have not been empirically confirmed. The currently predominant philosophical conception of theory confirmation (Bayesianism) equates confirmation

> with the increase of the probability that a theory is true or viable. For that reason I speak of "non-empirical theory confirmation".

This seems to just be an argument that HEP theorists have been successful in the past, so one should believe them now, an argument in conflict with the argument that things have changes due to the difficulty in getting relevant experimental data.

No Comments

Mathematics Items

July 16, 2014
Categories: Langlands

- For an Oxford conference last week, Langlands contributed a one-hour video talk, filmed in his office. One hour was not enough, so hours two and three are also available, as well as a separate text, and some additional comments.
- The latest AMS Notices has a long section of excellent articles about Friedrich Hirzebruch and his mathematical work.
- Also in the AMS notices is a long defense of the NSA, written by a mathematician who worked there for 41 years. About the main recent controversy here, the Snowden revelation of an NSA backdoor in an NIST standard, all the author has to say is:

  > I have never heard of any proven weakness in a cryptographic
  > algorithm that's linked to NSA; just innuendo.

  This seems to me to exemplify pretty well the disturbing tactic of the US security establishment of claiming there is no problem while refusing to discuss anything problematic since it is classified.

- Bhargava, Skinner and my colleague Wei Zhang have a new paper out proving that better than 66% of elliptic curves satisfy the BSD conjecture. It seems not implausible that they or others might in the not too distant future get to 100%. One should note though that showing 100% of elliptic curves satisfy BSD wouldn't be the same thing as showing all elliptic curves satisfy BSD, so wouldn't be eligible for the $1 million Millennium prize.
- With the ICM less than a month away, I find it outrageous that no one has yet leaked to me the names of the Fields Medal winners. All I've heard is speculation, and the only name I'd bet any money on is Bhargava.

Update: For something both ICM and Langlands related, Michael Harris on his web site has his ICM contribution Automorphic Galois representations and the cohomology of Shimura varieties. Many of the ICM 2014 proceedings contributions are already available on arXiv, via this search.

Comments

1. **egan**
   July 16, 2014

   For the Fields medal winners, my money is on Bhargava, Lurie, Avila and, perhaps, Scholze.

2. **Douglas Natelson**
   July 16, 2014

   Peter, can you explain how proving that 100% of elliptic curves satisfy BSD is not the same thing as proving that all elliptic curves satisfy BSD? Is this some mathematician's definition of 100% that does not mean "all", or is the particular prize question in terms of a single general proof rather than breaking the result into subcategories? (Bhargava gave the single most impressive math talk I've ever seen, and he used only a blank transparency and a felt-tip marker.)

3. **Cartan**
   July 16, 2014

   Douglas Natelson,

   100% of numbers in the interval [0,1] are not 1/2 but not all numbers in [0,1] are not 1/2.

4. **newcomer**
   July 16, 2014

   @Douglas,
   for example another statement: 100% of integers are not prime.

5. **Peter Woit**
   July 16, 2014

   To expand on the remark of Cartan, the standard conjecture I believe is that if you order elliptic curves by height, in the limit as you go off to infinity, 50% will have rank 0, and 50% will have rank 1. Of course there are plenty of elliptic curves of higher rank, and to prove BSD you need to prove it for them. In the limit though, these do make up 0% of the curves.

6. **Douglas Natelson**
   July 16, 2014

   Got it. It's an issue of measures. Thanks!

7. **Bill**
   July 16, 2014

   Speculation is that there will be a female Fields medalist.

8. **Tom Leinster**
   July 16, 2014

   NSA mathematician Richard George protests that talk of the NSA deliberately weakening cryptographic algorithms is "innuendo". But over at the *n*-Category Café, user bgg points out another piece of evidence to the contrary: the NSA's own 2013 budget request, which says the NSA will use funds to:

   > Insert vulnerabilities into commercial encryption systems

   and

Influence policies, standards and specification for commercial public key technologies.

9. **Jeff M**
July 16, 2014

Peter,

Not that it matters now, but perhaps better than 100% is "almost all." Then again, I guess someone will ask what that actually means. How about "except for a set of measure 0." I'm hoping no one asks for a definition of "measure." 🙂

10. **Peter Woit**
July 16, 2014

Bill,
I'll raise you: I'm hearing speculation of TWO female Fields medalists.

Tom,
That "innuendo" business is just outrageous. Does anyone believe the NIST issued a warning not to use that standard based on "innuendo"???? Someone should write into the Notices about this. Makes me agree with Beilinson's point of view…

11. **BCnrd**
July 16, 2014

It cannot be emphasized too strongly how much the relatively short paper of Bhargava, Skinner, and Zhang rests on tremendous amounts of substantial prior work of the 3 authors separately (e.g., item [15] in the bibliography).

Also, though it will be spectacular to know some day that "0%" of elliptic curves over Q have higher rank (and that "100%" satisfy the rank-BSD conjecture), the "0%" case can of course occupy a substantial % of the proof of a big theorem: recall the crucial role of a higher-rank elliptic curve in the solution of the Gauss class number problem for imaginary quadratic fields, that only finitely many elliptic curves in each characteristic p are supersingular yet every elliptic curve over Q has infinitely many primes of supersingular reduction (though the set of such primes is 0% in the non-CM case), and that the Gross-Zagier paper devotes a lot of space to primes of supersingular reduction.

12. **Bill**
July 17, 2014

Actually, the whole process of awarding Fields medals reminds of "top 10 world's best place to travel" published by many magazines. Yes, Paris is great, but a mountain campsite can be pretty awesome too. The difference is that nobody really takes these top 10 lists seriously. On the other hand, I suspect there are many more travelers qualified to compare various travel destinations then there are mathematicians qualified to judge top 4 mathematicians under forty.

13. **Jonathan41**
July 17, 2014

Speculation from outsiders is one thing, but leaks from insiders would be shocking IMHO.

I do hope the "female winner rumor" is true though, there are several exceptional ones this time around, so statistically out of four winners there's no reason for it not to happen.

14. **Urs Schreiber**
July 17, 2014

In his Oxford lecture "Problems in the theory of automorphic forms — 45 years later" Robert Langlands had some noteworthy points to make about the field. Specifically in the survey pdf that goes with the video lectures there are comments on the effects of Fields medals and on specialization.

p. 5: "Since Fields medals are taken quite seriously by a large number of mathematicians and by whatever part of the general public takes an interest in mathematics, the lack of perspective is regrettable. […] These specialists are all excellent mathematicians, but one wonders again whether the influence of these medals, and other prizes with wide recognition, on the course of mathematics and on our understanding of it is entirely beneficial."

and more on specialists:

on p. 8 "I find, however, that they are excessively focussed. We can, I am sure, not do without them; they however appear to prefer problems with circumscribed, more modest goals! I fear that those with a larger view of the subject will have to learn from them; on the other hand, they themselves, so far as I have observed, have no desire to learn anything was ihnen nicht in den Kram paßt."

On the other hand, regarding the broader perspective of geometric Langlands correspondence and in particular its claimed incarnation within string theory:

p. 6 "I am still puzzled by the notion of mirror symmetry and, in general, am curious what 'theoretical physicists' have in mind when they speak of the "Langlands program."

More of this cautioning that there remains mathematical substance to be added to a handwaving story is in the first video lecture and was also pronounced earlier this year in a coment to a message to Sarnak:

"One popular introduction to the topic is Frenkel's Bourbaki lecture, 'Gauge theory and Langlands duality'. On the first page, he describes electro-magnetic duality as an aspect of the Maxwell equations and their quantum-theoretical form or, more generally, as an aspect of four-dimensional gauge theory. This duality is quite different than the functoriality and reciprocity introduced in the arithmetic theory. It entails a supplementary system of differential equations. Moreover, it has to be judged by different criteria. One is whether it is physically relevant. There is, I believe, a good deal of scepticism, which, if I am to believe my informants, is experimentally well-founded. Although the notions of functoriality and reciprocity have, on the whole, been well received by mathematicians, they have had to surmount some entrenched resistance, perhaps still latent. So I, at least, am uneasy about associating them with vulnerable physical notions. On the other hand, as strictly mathematical notions this duality and various attendant constructions, such as the Hitchin fibration, appear to have proven value, especially for topologists and geometers. Whether it is equal to that of functoriality and reciprocity is open to discussion."

This seems to show that even the idea that the string theoretic argument is supposed to be a purely mathematical argument remains unclear.

In my own talk at the end of the Oxford meeting I took the liberty of picking of that theme, arguing that what is urgently missing is a general mathematical theory of higher codimension quantization that would allow to turn statements about ‘t Hooft operators, Wilson operators etc. into precise definitions, clear statements and formal proofs. Some indications in this direction are here.

15. **jsm**
July 17, 2014

To put the Bhargava, Skinner, Zhang result in perspective, we’ve known for 80 years that the Hodge conjecture is true for almost all abelian varieties (in terms of the moduli space), but this has helped not at all in our efforts to prove the Hodge conjecture for all abelian varieties.

16. **Chris W.**
July 18, 2014

Urs,
Is there a transcription error in this sentence from your quotation of Langlands?

> So I, at least, am uneasy about associating them with ~~vulnerable~~ **venerable** [?] physical notions.

17. **Urs Schreiber**
July 19, 2014

Chris W., that seems a weird question of yours, unless I am missing something. Follow the link I gave to see that this is not transcribed but typed by RL himself.

Also, your suggestion for how to change the wording indicates that you may be missing what I think is the point of the statement. The point of the statement is that RL does not wish to have associated with the mathematical program named after him non-rigorous claims whose alleged validity he feels might well not stand the test of time, to the extent that they are based just on physics-inspired ideas and not on secure proofs.

18. **Cplus**
July 20, 2014

The search for archived ICM papers can be improved by a factor > 5

http://arxiv.org/find/grp_math/1/OR+co:+AND+ICM+2014+co:+AND+ICM+proceedings/0/1/0/2014/0/1?per_page=100

19. **Anonymous**
July 20, 2014

As a member of a standards committee involved the removal of the mentioned algorithm from a standard, none of the members believe the “innuendo” theory, and all believe it was deliberately weakened. In fact, the possibility of potential weaknesses were noted during the NIST analysis. At a minimum, there is something very, very fishy about the algorithm in question.

20. **Anonymous**
July 20, 2014

This site:

http://blog.cryptographyengineering.com/2013/09/the-many-flaws-of-dualecdrbg.html

Has an excellent discussion of the flaws in Dual_EC_DRBG. Either (1) NSA was ignorant of these mathematical details, and therefore can’t be trusted based on cryptographic incompetence, or (2) they knew exactly what they were doing, and can’t be trusted because they intentionally put a backdoor in the algorithm. Your choice.

Someone really needs to publish a rebuttal to what is probably NSA backed propaganda in a published journal.

21. **Anonyrat**
July 21, 2014

I have never heard of any proven weakness in a cryptographic algorithm that’s linked to NSA; just innuendo.

This could be true, with less subtlety than the “100% versus all” (100% is less than all by something of measure 0). The author may have only read about proven weaknesses, not heard of them, and so on.

22. **Bill**
August 6, 2014

Peter, actually, giving Fields medals to two would make sense since it would take the pressure off one of them being the first female Fields medalist. There are female candidates, like Mirzakhani or Serfaty, who definitely deserve it as much as anybody else.

23. **Chucky**
August 6, 2014

Any news of who will get the Fields next week?

The NSA, NIST and the AMS

July 21, 2014
Categories: Uncategorized

Among the many disturbing aspects of the behavior of the NSA revealed by the Snowden documents, the most controversial one directly relevant to mathematicians was the story of the NSA's involvement in a flawed NIST cryptography standard (for more see here and here). The New York Times reported:

> Classified N.S.A. memos appear to confirm that the fatal weakness, discovered by two Microsoft cryptographers in 2007, was engineered by the agency. The N.S.A. wrote the standard and aggressively pushed it on the international group, privately calling the effort "a challenge in finesse."

The standard was based on the mathematics of elliptic curves, so this is a clearly identifiable case where mathematicians seem to have been involved in using their expertise to subvert the group tasked with producing high quality cryptography. A big question this raises has been what the NIST will do about this. In April they removed the dubious algorithm from their standards, and published the public comments (many of which were highly critical) on a draft statement about their development process.

At the same time a panel of experts was convened to examine what had gone wrong in this case, and this panel has (on a very short time-scale) just produced its report (associated news stories here, here and here). The rules of how such panels are set up evidently require that each panelist provide an individual report, rather than attempt to have a consensus version. The new NIST document gives these reports together with minutes of the meetings where the panelists were provided with information. It seems that the NSA provided no information at all as part of this process, and they remain unwilling to answer any questions about their actions.

Appendix E contains the individual reports. These include, from Edward Felten:

> The bottom line is that NIST failed to exercise independent judgment but instead deferred extensively to NSA with regard to DUAL_EC. After DUAL_EC was proposed, two major red-flags emerged. Either one should have caused NIST to remove DUAL_EC from the standard, but in both cases NIST deferred to NSA requests to keep DUAL_EC…
> at the time NIST had nobody on staff with expertise in elliptic curves.
> NSA's vastly superior expertise on elliptic curves led NIST to defer
> to NSA regarding DUAL_EC, while NIST people spent more of their limited time on other parts of the standard that were closer to their expertise.

From Bart Preneel:

> There is no doubt that the inclusion of Dual EC DRBG in SP 800-90A was a serious mistake…
> The explanations provided by NIST are plausible, but it seems that not all decisions in the standardization process of SP 800-90A are properly documented; moreover, we did not have access to the source documents. This means that it is impossible to decide whether this mistake involved in addition to clever manipulation of the standards processes by NSA also some form of pressure on the technical and/or management staff of NIST. It is also not clear whether there would be any traces of such pressure in documents. Without access to the documents, it is also diffcult to decide whether or not NIST has deliberately weakened Dual EC DRBG…
>
> However, it seems that NSA (with its dual role) seems to be prepared to weaken US government standards in order to facilitate its SIGINT role. This undermines the credibility of NIST and prevents NIST reaching its full potential in the area of cryptographic standards. In view of this, the interface between NSA and NIST and the role of the NSA should be made much more precise, requiring an update to the Memorandum of Understanding. At the very least, the terms "consult", "coordination" and "work closely" should be clarified. Ideally, NIST should no longer be required to coordinate with NSA. There should be a public record of each input or comment by NSA on standards or guidelines under development by NIST.

From Ronald Rivest (the "R" in "RSA"):

> Recent revelations and technical review support the hypothesis that, nonetheless, the NSA has been caught with "its hands in the cookie jar" with respect to the development of the Dual-EC-DRBG standard. It seems highly likely that this standard was designed by the NSA to explicitly leak users' key information to the NSA (and to no one else). The Dual-EC-DRBG standard apparently (and I would suggest, almost certainly) contains a "back-door" enabling the NSA to have surreptitious access. The back-door is somewhat clever in that the standard is not designed to be "weak" (enabling other foreign adversaries to perhaps exploit the weakness as well) but "custom" (only the creator (NSA) of the magical P,Q parameters in the standard will have such access).
>
> NIST (and the public) should know whether there are any other current NIST cryptographic standards that would not be acceptable as standards if everyone knew what the NSA knows about them. These standards should be identified and scheduled for early replacement. If NSA refuses to answer such an inquiry, then any standard developed with significant NSA input should be assumed to be "tainted," unless it possesses a verifiable proof of security acceptable to the larger cryptographic community. Such tainted standards should be scheduled for early replacement.

One way this goes beyond the now-withdrawn NIST standard is that the committee also looked at other NIST current standards now in wide use, which in at least one other case depend upon a specific choice of elliptic curves made by the NSA, with no explanation provided of how the choice was made. In particular, Rivest recommends changing the ECDSA standard in FIPS186 because of this problem.

For a detailed outline of the history of the Dual-EC-DRBG standard, see here. Note in particular that this states that in 2004 when the author asked where the Dual-EC-DRBG elliptic curves came from, the response he got was "NSA had told not to talk about it."

Also this week, the AMS Notices contains a piece by Richard George, a mathematician who worked at the NSA for 41 years before recently retiring. Presumably this was vetted by the NSA, and is a reasonably accurate version of the case they want to make to the public. Personally I'd describe the whole thing as outrageous, for a long list of reasons, but here I'll just focus on what it says about Dual-EC-DRBG, since it now seems likely that it is all we will ever get from the NSA about this. It says:

> I have never heard of any proven weakness in a cryptographic
> algorithm that's linked to NSA; just innuendo.

The reaction from a commenter here (publicly anonymous, but self-identified to me) was:

> As a member of a standards committee involved the removal of the mentioned algorithm from a standard, none of the members believe the "innuendo" theory, and all believe it was deliberately weakened.

Read carefully (and I think it was written very carefully…), note that George never directly denies that the NSA back-doored Dual-EC-DRBG, just claims there is no "proven weakness". In other words, since how they chose the elliptic curves is a classified secret, no one can prove anything about how this was done. All the public has is the Snowden documents which aren't "proof". This is highly reminiscent of the US government's continuing success at keeping legal challenges to NSA actions out of the courts, even when what is at issue are actions that everyone agrees happened, on the grounds that plaintiff can't "prove" that they happened, since they are classified. Snowden's release of documents may yet allow some of these cases to come to a court, just as they were the one thing capable of getting the NIST to acknowledge the Dual-EC-DRBG problem.

I hope that there will be more response to the NSA issue from the Math community than there has been so far. In particular, Rivest's call for the removal from NIST standards of material from the NSA which the NSA refuses to explain should be endorsed. The innuendo from George is that the NSA may be refusing to explain because they used superior technology to choose better, more secure elliptic curves. If this is the case I don't see why an official statement to that effect, from the NSA director, under oath, cannot be provided.

On the many other issues the George article raises, I hope that the AMS Notices will see some appropriate responses in the future. Comments here should be restricted to the NIST/NSA story, with those from anyone knowledgeable about this story most encouraged.

Update: The NIST has made available on its website the materials provided to the panel looking into this.

One remarkable thing about the panel's investigation is that the NSA evidently refused to participate, in particular refusing to make anyone available to answer questions at the panel's main meeting on May 29 (see page 12 of the report). This appears to be in violation of the Memorandum of Understanding that governs the NIST/NSA relationship, which explicitly states that "The NSA shall … Be responsive to NIST requests for assistance including, but not limited to, all matters related to cryptographic algorithms and cryptographic techniques, information security, and cybersecurity." All evidence I've seen is that the NSA sees itself as above any need to ever justify any of its actions. I can't see any possible argument as to why they did not have an obligation to participate in the work of this committee.

Update: A new development in this story is a letter from Congressman Grayson to NSA Director Clapper asking exactly the right questions about what happened at the NIST. Will be interesting to see if a member of Congress can get anything out of the NSA beyond the usual stone-walling.

Comments

1. **QM**
   July 21, 2014

   > "The innuendo from George is that the NSA may be refusing to explain because they used superior technology to choose better, more secure elliptic curves. If this is the case I don't see why an official statement to that effect, from the NSA director, under oath, cannot be provided."
   >
   > Is it in the NSA's interest to leave this an open question? If they chose a better curve, and say so, do their targets gain confidence, and are encouraged to use it more? If they say it's back-doored, do their targets abandon it? Uncertainty surely works in their favor here, as usual.

2. **Peter Woit**
   July 21, 2014

   QM,
   In this case, no one with any interest in the security of their communications is intentionally using Dual-EC-DRBG, the potentially backdoored standard, anymore.

   I think you're right that they want uncertainty, about this and about everything else they may or may not be doing. Life is much easier if you never have to justify anything you do.

3. **jeremy78**
   July 21, 2014

   How do these memo confirm that the NSA did anything? I keep seeing the phrase "appear to confirm" in all of these articles. Notice the word "appear" as a qualifier. Why don't they reveal what it is exactly in these memos that confirms all of this? Because this vagueness is the exact same thing the NSA is doing, but you seem alright with it because it confirms your previously held worldview. Why aren't you demanding more clarification from them?

4. **Peter Woit**
   July 21, 2014

   jeremy78,
   You and Richard George are right that nothing is "proved": the NY Times reporters only claimed that memos "appear to confirm" that the NSA introduced a back-door. The quote the NY Times gives "Eventually, NSA became the sole editor" of the standard is pretty much confirmed by the NIST description of what happened back then, but proves nothing about a back-door.

   The importance of the Snowden memos is not that they revealed exactly what the NSA did, but that they caused people to go back and look carefully at how the standard was developed. Like George, you're completely ignoring the facts that are the main evidence for a problem. I'm not making vague claims but linking to the new NIST report and quoting it. The bottom line is that the experts who have looked into this mostly think there is an NSA back-door.

   Is this proven? No, and it never will be as long as the NSA keeps classified the method used to produce the elliptic curves. I really only see three possibilities:
   1. The method was something innocuous, some random choice.
   2. The method used something classified to get something more secure than random, so they need to protect this.
   3. The method was designed to weaken the standard so that they could attack it.
   If what happened is 3., it makes sense the NSA would behave exactly as it has. If it was 1. or 2., there is no reason they could not simply make a public statement to that effect (giving the details in case 1, keeping those secret in case 2.).

5. **jeremy78**
   July 21, 2014

   You say that you're not using the Snowden memos as evidence, but that is clearly false. Many of those experts refer to "revelations" which show a clear bias going in.

   Maybe it's just me, but I take an evidence based view of the world. Regardless of what experts think, I look at the evidence. Right now there is no direct evidence, which you agree with.

   I thought the evidence based approach was the purpose of this blog. If we go with your current view, then perhaps those string theorists are right. They have no evidence, but they're experts so their opinions must be right in spite of the lack of the evidence. I don't know. Perhaps, you think in complex social situations, we should default to the experts till we have direct evidence, but I think that is a mistake.

   Also, let's be honest. Even if the NSA did exactly what you said they should do, I'd doubt you believe them or it would change anything. It's hard to prove a negative.

6. **jeremy78**
   July 21, 2014

Sorry for the double post, but I just wanted to point out the similarities of this situation to string theory and the multiverse hype.
1. Untestable hypothesis-"Is this proven? No, and it never will be as long as the NSA keeps classified the method used to produce the elliptic curves."
2. Trusting experts without question-"The bottom line is that the experts who have looked into this mostly think there is an NSA back-door."
3. No direct evidence -"proves nothing about a back-door"

7. **Matt**
July 21, 2014

I think this is far less cut and dry than mathematicians and the media are making it out to be. The NSA is a gigantic organization, and we give it a lot of credit to think these conspiracy theories are so nicely carried out. I'm not saying it is impossible.

I went to a talk given by one of the 2 Microsoft people that discovered the flaw. He is wholly unconvinced that it was intentionally done by the NSA, which I found pretty convincing. No one really wants to hear that side though.

8. **Peter Woit**
July 21, 2014

jeremy78
I've now written several long blog postings about this, with many links to detailed documentation and the evidence I'm aware of. It seems to me to provide a rather strong case that the NSA was up to something in this standards process other than providing the best possible encryption. This evidence is not incontrovertible. There's doubtless a lot more to the story which is known only to those at the NSA. If you want to take the attitude that only incontrovertible, direct evidence will convince you that there's a problem at the NSA, that's your choice. Arguments about the multiverse are almost all a complete waste of time, I don't think they provide insight into this topic.

If the NSA provided a plausible explanation of the source of the elliptic curves, that definitely could change my mind. I'm not someone who believes that NSA officials regularly lie to the public. On the whole the most damaging accusations from Snowden have not been denied.

Matt,
I don't think this is a story about a "conspiracy", it's a story about an organization with a professed agenda (getting access to encrypted internet communications) pursuing that agenda. If you read the NIST report I linked to and look at its authors, I don't think you can say this is a case where "no one wants to hear the NSA's side". The NSA was specifically asked to meet with the committee for that purpose and refused to do so (see page 12).

9. **Peter Woit**
July 21, 2014

All,
Enough trolling and arguing with trolls, I've just deleted a bunch of it. If you've got something knowledgeable to say I'd love to hear it, if you want to have a dumb argument, please go elsewhere.

10. **Pawl**
July 22, 2014

I would call attention to Preneel's mention of possible pressure involved in the decision-making process, in conjunction with a pattern in the leaked documents, saying that the NSA should "leverage" decisions, access to information, and so on. What sort of leverage, exactly, has been applied? To whom?

11. **Tom Leinster**
July 22, 2014

It seems to me that Richard George's statement is demonstrably false, *independently* of political opinion or evidence from the Snowden papers. Here's what he wrote again:

> I have never heard of any proven weakness in a cryptographic
> algorithm that's linked to NSA; just innuendo.

This says nothing about intent. It's the flat-out assertion that no cryptographic algorithm linked with the NSA has a proven weakness. (Or at least, no weakness that George has heard of — but we can safely assume that he has heard of this one.)

In his February *Notices* article, Tom Hales notes: "It is a matter of public fact that the NSA was tightly involved in the writing of the standard" (NIST SP800-90A). So, George is asserting in particular that *this* standard has no proven weakness. But Ferguson and Shumow, on whose work Hales's article is based, conclude that

> The prediction resistance of this [pseudorandom number generator] is dependent on solving one instance of the elliptic curve discrete log problem.

And as I understand it, that's a serious weakness *even if you don't believe the NSA did anything sneaky*. In that case, George's suggestion that no cryptographic algorithm linked to the NSA has a proven weakness is simply false — as a scientific fact, regardless of politics.

So I can't see how to escape the conclusion that the NSA was, at the very least, incompetent. It pushed through a cryptographic standard with a serious vulnerability. Even if you trust the NSA politically and ignore its stated intention to "insert vulnerabilities into commercial cryptosystems", this episode provides a reason not to trust it with designing the world's cryptographic standards.

12. **Peter Woit**
July 22, 2014

Tom,
I think George would argue that's not a "proven weakness". My reading of Ferguson/Shumow is that they are pointing out that whoever chose P and Q could do it in such a way that they know e, and thus a backdoor. The NSA chose P and Q and refuses to discuss how they chose them, so NSA defenders can claim that maybe they chose them randomly, or in a way that used secret sauce to make things even more secure. My understanding is that one unusual thing about this is that, unless you find e yourself (which is supposedly impossible), you can't prove that there is a backdoor. From the outside world's point of view, "this could be backdoored in a way that only the NSA could know" is a weakness, but from the NSA's point of view, it's not.

What George is basically saying is "you can't prove we did anything", not "we didn't do anything", and I think the difference is intentional.

13. **Tom Leinster**
July 22, 2014

If George doesn't call Ferguson and Shumow's discovery a "proven weakness" in the protocol then he's using words in a different way from ordinary human beings. This is of course a stock-in-trade of intelligence agencies issuing carefully-worded denials, but none the better for it. Any ordinary person would say that if a cryptographic system is untrusted (as this one now most certainly is), that's a weakness: who wants a system they can't trust?

In the long NIST report that you link to, Edward Felten (professor of computer science at Princeton, specializing in computer security) defines a "pure trapdoor" as one in which "NSA is the only party who can possibly exploit it". He continues:

> [A]ll of the suspected NSA trapdoors in NIST standards are impure trapdoors. In every case discussed below, if the suspected trapdoor does exist, its existence reduces the security of users against attack by other adversaries, including organized crime groups or foreign intelligence services.

This includes the caveat "if the suspected trapdoor does exist". But whether or not it does, the *possibility that it might* is a weakness in itself.

14. **Peter Woit**
July 22, 2014

Tom,
Still, the NSA's point of view is that for all you know they chose the numbers randomly and there is no trapdoor, and no weakness. So, you have no proof (or any hope of getting one, unless a mathematician version of Snowden appears…) that there is a trapdoor and thus a weakness in the standard. I think it highly likely that George and whoever vetted his piece at the NSA are well aware of the Ferguson/Shumow argument, and they chose their language carefully so that they could claim what they said was true. Yes, it's highly misleading, but we know that misleading people about what they do they see as part of their mission.

My initial reading of this was "this guy George must be some senile incompetent claiming to be unaware of the Dual-EC-DRBG story", I changed my mind after re-reading carefully.

15. **Tom Leinster**
July 22, 2014

I agree, it's carefully worded, but I still think it's plain wrong. If he'd said that there's no "proven *backdoor*", then I wouldn't be able to contradict him. But he said that there's no "proven *weakness*", and a backdoor isn't the only kind of weakness a cryptographic system can have.

John Kelsey of NIST wrote this about choosing parameters for Dual EC:

> * It is possible to choose (P, Q) so that you know a backdoor […]
>
> * It is also possible to choose (P, Q) so that you can prove you don't know a backdoor.

Let's call the system **unbackdoorable** if (P, Q) has been chosen in the second way, and **backdoorable** otherwise. A backdoorable system may or may not actually be backdoored. Still, for a system to be backdoorable is a serious weakness, because it makes it untrustworthy, even if you don't know that a backdoor actually exists.

As I understand it — and I hope someone will correct me if I'm wrong — it's a matter of black-and-white fact that this particular system is backdoorable. That, then, is the proven weakness. And that's why this system has been abandoned in a hurry: because we know it's backdoorable, even though we don't know whether it's backdoored.

Of course, this whole conversation that I started is in some sense absurd, since it deliberately makes no assumptions about the intentions of the NSA, despite all the documentary evidence in front of us.

16. **Peter Woit**
July 22, 2014

Tom,
My reading of the same document you're linking to is that nothing is known about how the NSA chose (P,Q). All it says is that on 10/27/04 he was told "could be generated randomly, but that NSA had told not to talk about it" and in Oct. 05 NSA said they "generated (P,Q) in a secure, classified way" (a huge mystery here is why NIST then decided to include in the standard a way people could generate their own (P,Q), but not mention that if you didn't do that, you had to trust the NSA's (P,Q) not to be backdoored. The current answer from NIST is basically "we were incompetent and made the mistake of trusting the NSA").

So, as long as the NSA stonewalls about how they chose (P,Q), George can claim that it can't be proven that they did it in a backdoorable way. Maybe they did it in an unbackdoorable way. The story about DES that George tells refers to the fact that the NSA in that case knew how to choose parameters in a better, more secure way than what was publicly known, and suspicions they were putting in a backdoor turned out to be wrong: they were actually making things more secure (of course at the same time they were insisting on a key length short enough for them to break by brute force…). He's telling that story to raise the possibility that this is what happened here, that NSA chose (P,Q) by some highly secure method they can't talk about.

My take is that if they had done that they would have consistently just said so, which would not have revealed anything classified. In particular they would have shown up at the NIST panel meeting and defended what they did back in 04/05, if only by reading a vetted statement saying "we chose (P,Q) in a secure, classified manner that we cannot reveal to you, we do not have a back-door". That they are not doing this, instead letting their relationship with NIST be poisoned to the point of collapse seems to me to be the dog that didn't bark here.

17. **ScentOfViolets**
July 23, 2014

Isn't the proper course of action pretty much a no-brainer? If someone wants me to invest funds in their business venture, it's on them to show me that there's a reasonable expectation that it will succeed; not on me to show that it won't. Similarly, given the costs of bad crypto, why should I use it if it's not known to be secure? IOW, default burden of proof requirements should be on the systems defenders to prove that the crypto is trustworthy, not on other people to prove that it's untrustworthy. The statements released by the NSA seem deliberately crafted to get people to buy into the reversal of those requirements in the hope that this will forestall any significant revision of the current standard. If this isn't evidence of bad faith — particularly since the costs of a backdoor to the community at large is far larger than the benefits which accrue only to the NSA — I don't know what is.

18. **Rob Fawcett**
July 23, 2014

Is another perspective to ask not even whether a back-door was implemented, but how credible it is that the NSA did not internally have insight into the possibility of such a back-door? Appreciation of the possibility would necessitate accounting for the integrity of the (P,Q) generation. After all, encryption is terminally compromised once the credible practicable possibility of an existing back-door is established, irrespective of proof of successful decryption. The NSA need not have flagged any insight, they need only have accounted for the (P,Q) generation to make the standard robust.
I for one can but snort at the notion that they knew of the potential back-door, nobly abjured use of it but chose to obscure the (P,Q) derivation anyway, thereby ultimately dooming the standard from the start.

19. **Mike Sharples**
July 25, 2014

Why are people so hung up on what is and isn't proven? This is not an exercise in mathematical certainty it is an evaluation of risk. There is a risk that the standard is compromised. Obfuscation by the NSA does nothing to remove that risk (quite the contrary). Therefore the standard should be considered unsafe.

20. **Chris W.**
July 26, 2014

See the latest post from Scott Aaronson:

> The issue reached a comical extreme last October, when Adi Shamir, the "S" in RSA, Turing Award winner, and foreign member of the US National Academy of Sciences, was prevented from entering the US to speak at a "History of Cryptology" conference sponsored by the National Security Agency. According to Shamir's open letter detailing the incident, not even his friends at the NSA, or the president of the NAS, were able to grease the bureaucracy at the State Department for him.

With the NIST episode as context, what could this mean?

21. **Peter Woit**
July 27, 2014

Mike Sharples,
The discussion about what can be "proven" was purely about the question of whether the AMS notices piece defending the NSA was a bald-faced lie or not. Of course there is plenty of evidence with respect to the NSA's activities that does not rise to the level of "proof", and people should act accordingly.

Chris W.,
I think this is just one more example of the out-of-control US security establishment, of which you can easily find a hundred more. It should also remind people that when trying to figure out what has happened in stories like the NIST/NSA one, you can't use the argument "the NSA can't possibly have done X because that would be irrational, stupid, and damage US interests".

22. **Nick P**
July 29, 2014

I agree with ScentofViolets. The burden of proof, even by NSA standards (eg Common Criteria), is on the person delivering a piece of tech to show that it's trustworthy. There's many ways to do this. There's also the lifecycle requirement that, once problems are found, you explain and/or fix them. NSA is a world-class crypto organization that's tied to numerous problematic submissions in crypto standards, even amateurish mistakes their own evaluations look for. Whether incompetence or malice, they can't be trusted to deliver good crypto.

Another angle is "reasonable suspicion" vs "proof." The BULLRUN slides clearly say they're trying to manipulate standards and commercial products. That their own highly protected document says this is proof that they do it. At this point, a submission of theirs automatically has a reasonable suspicion that it might be backdoored. That's good enough for rejecting it. If we find a potential backdoor and they are who submitted it, then there's reasonable suspicion that they put it there due to the other proof (BULLRUN). If they refuse to even talk about it, even more reasonable suspicion to think they might be guilty. I maintain that the BULLRUN slides alone are enough proof that their submissions should be considered backdoored unless proven otherwise as they have capability, intent, and history. And untrusted until proven otherwise is the default in INFOSEC on top of that.

(Note: That said, this particular backdoor didn't bother me as much as most people. The problem with NSA is their *mandated* mission of collecting everything, along with difficulties they encounter. Subversion is their solution with many making us weaker. Yet, contrary to the claim by Felten, this particular subversion was a win-win as they tried to provide us protection while creating a way for only them to get in. If they're going to sneak in backdoors, I'd rather it be something like this than a buffer overflow or weak backdoor like that found in a recent FPGA.)

Still, there's enough people doubting their negative impact on security that I decided to look at the big picture to identify evidence they weaken it. The main problem I noted was "backdoor." The truth is that any flaw that lets them circumvent a security policy is a backdoor in practice. All they have to do is encourage flaws, not tell people about existing flaws, or introduce flaws. They can do this at many levels. So, I wrote an essay on Schneier's blog (main spot I post) showing the many ways NSA's actions provably weaken our security, both intentionally and incidentally:
https://www.schneier.com/blog/archives/2014/03/friday_squid_bl_420.html#c5226750

23. **Nick P**
July 29, 2014

I forgot to add a link to our smoking gun in all these discussions with key points conveniently highlighted:

http://www.nytimes.com/interactive/2013/09/05/us/documents-reveal-nsa-campaign-against-encryption.html?ref=us&_r=0

Quantum Connection Could Revitalize Superstrings

August 4, 2014
Categories: This Week's Hype

Finally back from vacation, postings may appear somewhat more regularly…

Science journalist Tom Siegfried has been one of the most vociferous proponents of string theory for many, many years (see here), but even his faith seems like it might be failing as the decades roll on. His latest on the topic starts out:

> Sometimes it's nice to reflect nostalgically on the last couple of decades of the 20th century. You know, the era of Madonna and Duran Duran, Cheers and The X-Files, McGwire and Sosa, the Macarena, and superstring theory.

The article does try and mount an argument that string theory may not be moribund, with the hope for the future coming from a new paper by Bars and Rychkov entitled Is String Interaction the Origin of Quantum Mechanics?. The idea here seems to be that if you assume you somehow have a fully consistent string field theory, not based on quantum mechanics, then the occurrence in this theory of non-commutative phenomena would "explain" quantum mechanics. To me, this seems to deserve some sort of award for the most desperate attempt yet to justify string theory, but Siegfried is a fan, explaining:

> For decades, explaining why nature observes the mysterious rules of quantum physics has perplexed physicists everywhere. Nobody could explain why those rules worked. The connection between string physics and the quantum math may now lead the way to an answer.

I'll write more soon about those "mysterious rules of quantum physics", but I just don't see at all how string field theory (which supposedly is based on quantum mechanics…) makes anything about quantum mechanics less mysterious.

Siegfried of course is not just a fan of string theory, but also of the multiverse, so he ends with:

> On top of all that, the string-quantum connection suggests an intriguing insight into the nature of reality. Quantum physics is notorious for implying the existence of multiple realities, as articulated in the "many worlds" interpretation of quantum mechanics. Superstring theory has also annoyed many physicists by forecasting the existence of a huge "landscape" of different vacuum states, essentially a multiverse comprising multiple universes with a wide range of physical properties (many not suitable for life, but at least one that is). If string interactions really are responsible for the rules of quantum physics, maybe there's some connection between the multiple quantum realities and the superstring landscape. For fans of the late 20th century, it seems like an idea worth exploring.

One thing remarkable about this is that he has another piece that recently appeared, an interview with Katherine Freese, where he tries to convince her about the multiverse, but doesn't get anywhere:

> **Theory predicts vastly more vacuum energy than the amount actually observed. Wouldn't this huge disparity be explained if there are multiple universes, a multiverse, and each has a different density of vacuum energy? Then the reason we have a low amount in ours is because that's the only way we could exist in it.**
>
> I don't like that idea. A lot of people like it because of string theory. Originally people thought that string theory would give a unique solution to the vacuum-energy equations. But it turns out that in string theory there are maybe 10-to-the-500th different vacuum states. So the idea is that they're all out there, but we have to live in one with a value of the cosmological constant close to the one we have. But I don't like anthropic arguments. They rely on the fact that human life can only come to exist under certain conditions, so that of the many universes out there it's not surprising we live in the one that supports our type of life. That's not a good enough explanation for me. I feel there are physics problems that we have to answer, and we can answer them in this universe, in this piece of the universe we live in. I think it's our job to try to do that, and it's not good enough for me to give up on it and say, well, it has to have this value because otherwise we couldn't exist. I think we can do better than that. I know, I'm old-fashioned.
>
> **Isn't part of the question whether there is a multiverse or not? If you had really strong evidence that there is a multiverse, then the anthropic explanation becomes better motivated. Inflation, the rapid burst of expansion right after the Big Bang, supposedly can produce a multiverse by way of "eternal inflation."**
>
> I do believe in inflation, so can inflation give you a multiverse or not? Because if it can, then I'm forced to consider this possibility. I recently wrote a paper with Will Kinney on this. We concluded that what we observe in the cosmic microwave background radiation is not giving rise to eternal inflation. So how do you know that ever happened?
>
> **Are the recent results on the cosmic microwave background from the BICEP2 experiment relevant to this issue?**
>
> If you take the BICEP data literally, which I'm not saying you should, you never have eternal inflation. So you don't have to have eternal inflation, if you ask me. I was very happy about that.

Comments

1. **comment**
   August 4, 2014

   It is unfortunate that Sigfried honed in on Bars' paper and that Peter amplifies it for the purpose of painting string theorists as desperate and intellectually dishonest. No independent theorist would vouch for Bars' claim to have derive quantum mechanics based on what has been demonstrated.

   The matrix-like interaction of open strings is closely related to non-abelian gauge symmetry. So if open string field theory proves quantum mechanics, I think you could claim that Yang-Mills proves it as well. But, unfortunately, quantum mechanics is more than a noncommutative algebra.

   I recently attended a very interesting conference on string field theory in Trieste. Some new results include:

   1) A nonperturbative proof of background independence in open string theory. This result addresses a major point of criticism for string theory opponents.

   2) New constructions of spacetime actions which may provide a systematic underpinning for superstring perturbation theory. This is another "weak link" which is a major target of critics.

   3) Powerful numerical techniques for mapping the space of two-dimensional boundary conformal field theories and conformal defects, with potentially useful applications to condensed matter physics.

It is aggravating that this real progress is ignored on this blog and elsewhere. Incidentally, Bars also gave a talk at the conference, but it is fair to say that his recent paper was not the focal point of excitement.

2. **Peter Woit**
   August 4, 2014

   comment,
   First of all, I'm not painting anyone here as intellectually dishonest. The "string theory implies quantum mechanics" claim here isn't being made underhandedly. It's straightforward, but it's just bizarre and nonsensical.
   As for "desperate" though, yes, I think this kind of thing reeks of desperation. I don't think you can read Siegfried's article as anything other than a desperate grasping at straws to avoid acknowledging the failures of the theoretical ideas he has been spending the last couple decades writing promotional books and articles about.

   I think your basic problem is not with me, but with the part of the string theory community which puts out nonsense like this and the journalists who are taken in by it.

3. **comment**
   August 4, 2014

   Itzak Bars is not "part of the string theory community." He is an individual, and his views represent his alone.

   Of course I have a problem with you posting about this, since it further amplifies the significance of an obscure idea which is not indicative of trends even in a very specialized subfield of string theory. But I understand that your interest is generally painting string theory in a negative light, and further amplifying noise gives people the impression that the subject is vacuous.

   My purpose was simply to provide a different point of view and remind people that progress is being made on fundamental questions, even within this minor subfield.

   I noticed that you took no interest in the main results presented at the recent conference. If you thought about and took a critical view of this work, it might be a more interesting starting point for debate on merits and progress in the field.

4. **David Roberts**
   August 4, 2014

   @comment

   links to these talks/abstracts/papers/results?

5. **R-bot**
   August 4, 2014

   @comment,
   Is Bars not part of the string theory community? What are you saying? If he is not, why he had been invited for that conference as a speaker? His personnel website even introduces himself as "My current interests include String Field Theory, and Two-Time Physics and Cosmology." Even he has just a minor of minor-alone view on a certain small subject (like some others on that field), he himself believes he is one of string community, and even he was invited for a string conference.

   Anyway I partly agree with you on your point (no high light for recent developments on the field). But this posting is criticizig its media-hype part of it, so you guys point at different directions. Also, as you may have noticed from Strominger's vision talk at Strings 2014, nobody even knows for what string community should head in the future and what exactly the theory is. So the main criticism point on that field from this site is still relevant and no critical recent developments to defense.

   @David Roberts,
   Maybe this one: http://www.sissa.it/tpp/activity/conferences/SFT2014

6. **Cathy Dupont**
   August 5, 2014

   @comment

   Background independence is not an important argument against or for string theory. Established physics is not background independent at all. The requirement has been "invented" by loop quantum gravity people to discredit string theory, but that is unfair: background independence does not help in making a theory that fits with observations or one that makes predictions. Background independence is a red herring.

   The test of a theory is that it agrees with observations. And also your other two points are red herrings in this respect …

7. **comment**
   August 5, 2014

   @ R-bot

   Bars is certainly a member of the community, but he alone does not represent "part" in the sense of a non-infinitessimal fraction. This is the sense of "part" that I was referring to.

   I don't want to denigrate Bars' and Rychkov's work. I am glad they are excited about something. Wild proposals can be fodder for interesting conversation and new ideas… but only if you have the right context. There is certainly a correct analogy between string interactions and some equations in quantum mechanics, but I don't think this analogy means what it sounds like it means. But perhaps Bars and Rychkov can exploit it for interesting purposes.

   See the following recent papers:
   http://arxiv.org/abs/1401.7980 about using string field theory to uncover the space of boundary conformal field theories and defects.
   http://arxiv.org/abs/1406.3021 for a proof of background independence.
   http://arxiv.org/abs/1403.0940 and http://arxiv.org/abs/1312.2948 for a new approach to the field theory of superstrings.
   I think these results are in fact relevant to some criticisms of string theory, though I doubt they would impress Peter much. Perhaps Strominger is not certain where the field should be heading, but he does not speak for me personally.

   @ Cathy Dupont

   One main focus of the conference was string field theory. This is a formal subject whose main motivation is trying to find the "right" definition of string theory. It has little to say about connecting Planck scale physics to measurements. If this is what you care about, then there is not much interesting to report from the conference.

But I think recent work charting the space of BCFTs could easily have quantitative applications to measurements of real world physical systems. But this is really an application of certain technology developed in the context of string theory to another field. It has no implications for the viability of strings as a description of Planck scale physics.

8. **Curious Mayhem**
August 5, 2014

Plus, "eternal inflation" à la Linde is historically prior to and independent of string theory and the "landscape." Linde's eternally inflating background metric is a connected spacetime, not a collection of separate realities. The two ideas have nothing to do with each other. That aspect of the whole bandwagon (not just Siegfried) *is* intellectually dishonest.

As Katy points out in the interview, it's very difficult to get inflation to work in string theory. And the current evidence is unfavorable to all but a small subset of "string-inspired" cosmologies and inconsistent with "eternal inflation." In fact, the current data suggest something pretty old-fashioned, a quantum universe tunneling to a false vacuum, setting up the initial conditions for inflation. A simple version of this is presented in Kolb & Turner's textbook.

As always, more and better data will winnow down the field of contenders. The exciting thing is, we'll probably see this moment soon.

9. **Lee Smolin**
August 5, 2014

Dear Peter,

For what its worth, can I mention that I wrote a paper in 2002 showing that quantum mechanics can be derived from a matrix formulation of string/M theory: arXiv:hep-th/0201031?

Dear Cathy Dupont,

Background independence is for sure a principle of "established physics", it is the general principle behind both the diffeomorphism invariance of general relativity and the local gauge invariance of Yang-Mills theory. It can be tied directly to the experimental success of GR, QED and the standard model, for example the experiments which verify these theories are pure spin 2 and pure spin 1, respectively.

Furthermore, it is funny that you think that background independence was ""invented" by loop quantum gravity people to discredit string theory"; the idea was rather invented by Einstein, Weyl and others early in the 20th century as part of the invention of GR and local gauge theory. Look up, for example, the history of the "hole argument" in Einstein's work between 1911 and 1916, followed by Weyl's invention of local gauge invariance in 1919.

btw the two comments are related because the 2002 paper deriving QM from string/M theory was part of a series of papers proposing a cubic matrix model as an approach to a background independent formulation of string theory.

Best,

Lee Smolin

10. **Arash**
August 6, 2014

Lee,

It's kind of old story by now, and I'm aware you've heard this question several times before, but wouldn't you call AdS/CFT a background independent formulation of string theory in asymptotically AdS spaces? (Asymptotic boundary conditions are allowed in ("background independent") classical GR on non-compact manifolds, right?)

@comment,

From what I've gathered, what you call `noise amplification' is in a sense one of the main purposes of this blog; to amplify what insiders (and enthusiasts) of string theory consider insignificant, while they should probably take them seriously. I don't think many of the readers of this blog would find any interest in reports on recent technical progress.

11. **Peter Woit**
August 6, 2014

Arash,
Do you really consider trying to have the same old empty argument with Lee about "background independence" and AdS/CFT to be anything other than your own form of "noise amplification"?

I suppose you're right though that one of the purposes of this blog is to do something string theory enthusiasts consider insignificant, correct the outrageous hype in the media about string theory.

By the way, I think you'd be very surprised to know who many of the readers of this blog are and what their interests are.

12. **Arash**
August 6, 2014

Point taken, Peter!

13. **David Roberts**
August 7, 2014

@R-bot – thanks, but there's a lot of abstracts linked to purely by the name of the speaker. I haven't the inclination to go through all of them and try to find the talks that comment described.

14. **Chris W.**
August 7, 2014

As an amusing sidelight on the above, Bars and Rychkov very recently posted another paper entitled "Background Independent String Field Theory" (arXiv:1407.4699). The abstract ends with this sentence:

A byproduct of our approach is an astonishing suggestion of the formalism: the roots of ordinary quantum mechanics may originate in the rules of non-commutative interactions in string theory.

15. **lun**
August 7, 2014

Comment
I do not think that what you call "background independence" is the same as what General Relativity people call background independence.

Correct me if Im wrong, but Maccaferri et al give a recipe to construct a solution in another background given a solution in a certain background. "Background independence" means that these two solutions are in some sense locally indistinguishable for a given observer. Maccaferri et all did not prove this.
Or am I wrong?

16. **Bill Radley**
August 7, 2014

A comment for Comment:

The point is that Siegfried did promote the Bars paper, and that makes it news for a blog about unjustified string+ theory hype. Of course there are better ideas out there, from all sides of the argument, but the point, as ever here, is that they are losing out – in terms of public and media attention, funding, and serious scientific attention – to an increasingly questionable consensus. Some development from string theory may of course still turn out to have a deep contribution to make to our understanding of the universe. But what would be useful for all of us would be for its proponents to refrain from describing tentative, partial, speculative,ultraspecific or unproven abstract results as key insights into the nature of the universe.

17. **Peter Woit**
August 7, 2014

Bill Radley,

There are always going to be people making dubious claims for their speculative ideas. Their sensible colleagues will just ignore them. The question is what to do when the press picks these things up. For random speculation that no one is paying attention to, probably best to just ignore such press stories unless they get widely spread.

Dubious claims of the specific "I've found the answer to the problems of string theory!" variety are a common subclass though, and the fact that they get press attention has a lot to do with the long history of resistance by string theorists to any public acknowledgement of failure. For decades now there has been little to no willingness of string theorists to complain to those hyping the theory. They do seem to be willing to complain to me though…

18. **srp**
August 8, 2014

"The good ideas, the ones that eventually find their way into the textbooks, are out there, all right–but they are out on the shelf surrounded by all kinds of junk, which is packaged by its authors as attractively as possible and, of course, is never clearly labeled 'junk.' It's not that people are out to fool you. They may be just confused or mistaken. Or, more commonly, they are doing work that is correct, in the sense of not containing errors, but not useful for progress. The work is too speculative or too safe; it loses contact with reality or remains narrowly earthbound."

One of my favorite quotations, from Frank Wilczek and Betsy Devine's Longing For the Harmonies. It applies in many fields besides physics.

19. **Titus Weisstein**
August 10, 2014

The origin of quantum mechanics was stated by the old masters, like Werner Heisenberg and Niels Bohr: quantum mechanics follows from the existence of hbar. That is why Heisenberg used to wear a golden tie needle with hbar on it in the second half of his life. Just hbar.

Stating that quantum mechanics follows from string theory would mean that string theory can explain why hbar exists.

But string theory does nothing of the sort. Nor does loop quantum gravity. Therefore, proponents of both camps are just making wind.

Putting yourself above Heisenberg and Bohr in this issue puts you in a bad light…

20. **Lee Smolin**
August 10, 2014

Dear Titus,

No one is putting themselves above Bohr and Heisenberg, far from it, but that doesn't mean we can't make efforts to understand questions they left unresolved.

I agree that LQG does not explain either quantum theory or the value of hbar, it is entirely an application of conventional quantization to GR. But in the work I mention I give a derivation of QM from a particular limit of a matrix model for string theory, to be precise the bosonic part of the BFSS model. There, in hep-th/0201031 I do give an explicit formula for hbar in terms of the parameters of the matrix model, it is eq 109.

Thanks,

Lee

21. **Georg Zwiebel**
August 11, 2014

Lee,

formula 109 relates hbar to several other parameters that are unknown, unmeasured and whose existence is unconfirmed. And even time appears in it.

Speculations and fantasies are ok. But you are not really convinced that this is an explanation of hbar, or are you?

22. **Lee Smolin**
August 11, 2014

Dear Georg,

This is a model, not a theory. It serves as an existence proof of several ideas I think are worth exploring further. Among them 1) non-local hidden variable theories exist which reproduce Schrodinger quantum mechanics for an N body system, 2) there may be a simultaneous solution of the measurement problem of quantum foundations, unification and quantum gravity, and 3) hbar may indeed be a function of physical coupling constants and the number of degrees of freedom of that unified theory.

btw t is not time, it is temperature, scaled by a power of the size of the matrices and some coupling constants. See eq 47.

Thanks,

Lee

Fall QM Course

August 11, 2014
Categories: Quantum Mechanics

This year I'll be teaching a new version of the same course on quantum mechanics aimed at mathematicians that I taught during the 2012-3 academic year (there's a web-page here). During the last course I started writing up notes, and have spent a lot of the last academic year working on these, the current version will always be here. At this point I have a few of the last chapters to finish writing, as well as a long list of improvements to be made in the earlier ones. I'll be teaching the course based on these notes, and hope to improve them as I go along, partly based on seeing what topics students have trouble with, and what they would like to hear more about.

I've learned a lot while doing this, and it now seems like a good idea to write something on the blog discussing topics that don't seem to me to be dealt with well in the standard textbook treatments. Johan de Jong's Stacks Project has recently added a sloganerator (written by Pieter Belmans and Johan Commelin), which has inspired me to try to organize things as slogans. Slogan 0 to appear soon….

Comments

1. **QGravity**
   August 11, 2014

   It would be great if all of the problem sets be posted on the web page of the course so one can follow things better.

   Thank You!

2. **Per**
   August 11, 2014

   Being a physicist myself I always enjoy a clear and not to technical math description of fields in physics. I've read the first 15 pages or so and first impression is very good. Thanks for posting.

   Cheers, Per

3. **Peter Woit**
   August 11, 2014

   Per,
   Thanks. Feedback about how to make this document more readable by physicists is definitely welcome.

   QGravity,
   The problem sets from the previous course are at
   http://www.math.columbia.edu/~woit/QM/oldprobsets.pdf
   As I get problem sets for the new course written, they'll appear on the course page. Solutions won't ever appear there, since I want to be able to reuse the problems…

4. **ktahn**
   August 11, 2014

   Exciting stuff! Thanks for making it available. Would be looking out for notes and exercises.

5. **Daniel McLaury**
   August 12, 2014

   Prof. Woit,

   As someone who doesn't know a lot about physics, I've started reading the linked notes, but I've hit a snag a few pages in. In particular, at the bottom of page 4 I read

   "Given an observable O and states psi_1 and psi_2 that are eigenvectors of O with eigenvalues lambda_1 and lambda_2, the state

   c_1 psi_1 + c_2 psi_2

   may not have a well-defined value for the observable O. If one attempts to measure this observable, one will get either lambda_1 or lambda_2, with probabilities

   |c_1^2|/(|c_1^2| + |c_2^2|)

   and

   |c_2^2|/(|c_1^2| + |c_2^2|)"

   However, these numbers don't appear to be well-defined. For instance, if psi_1 is a lambda_1-eigenvector for O then 2 psi_1 is also a lambda_1-eigenvector for O, so we could regard the state

   2 psi_1 + psi_2

   as either

   2 (psi_1) + 1 (psi_2),

   giving us a 4/5 chance of observing lambda_1 and a 1/5 chance of observing lambda_2, or we could regard it as

   1 (2 psi_1) + 1 (psi_2),

   giving us a 50/50 chance of observing either lambda_1 or lambda_2. Are there perhaps some constraints on the psi_j here, say that they should be unit vectors with respect to the Hermitian form on H? (That interpretation seems to match up with something you wrote in the blog post above, assuming that this is the same thing as Born's rule.)

Or have I misinterpreted something?

And of course thanks for writing this up, making it publicly available, and taking the time to engage with your readers!

6. **woit**
   August 12, 2014

   Thanks Daniel,
   You're right, that's ambiguous. I was trying to be clever and write things in a normalization-independent way, but for what I wrote you need psi_1 and psi_2 to be normalized the same way, e.g. by having unit norm. I'll fix this right now.

7. **Charlie**
   August 13, 2014

   After scanning the first four chapters, I'm really excited about these notes. Many thanks for making them available!
   Could you record a version number and/or date last updated on the cover page?

8. **Peter Woit**
   August 13, 2014

   Charlie,
   There should be a date on the first page of the pdf.

What’s Hard to Understand is Classical Mechanics, Not Quantum Mechanics

August 11, 2014
Categories: Favorite Old Posts, Quantum Mechanics

For a zeroth slogan about quantum mechanics, I’ve chosen

> What’s hard to understand is classical mechanics, not quantum mechanics.

The slogan is labeled by zero because it’s preliminary to what I’ve been writing about here. It explains why I don’t intend to cover part of the standard story about quantum mechanics: it’s too hard, too poorly understood, and I’m not expert enough to do it justice.

While there’s a simple, beautiful and very deep mathematical structure for fundamental quantum mechanics, things get much more complicated when you try and use it to extract predictions for experiments involving macroscopic components. This is the subject of “measurement theory”, which gives probabilistic predictions about observables, with the basic statement the “Born rule”. This says that what one can observe are eigenvalues of certain operators, with probability of observation proportional to the norm-squared of the eigenvector. How this behavior of a macroscopic experimental apparatus described in classical terms emerges from the fundamental QM formalism is what is hard to understand, not the fundamental formalism itself. This is what the slogan is trying to point to.

When I first started studying quantum mechanics, I spent a lot of time reading about the “philosophy” of QM and about interpretational issues (e.g., what happens to Schrodinger’s famous cat?). After many years of this I finally lost interest, because these discussions never seemed to go anywhere, getting lost in a haze of complex attempts to relate the QM formalism to natural language and our intuitions about everyday physics. To this day, this is an active field, but one that to a large extent has been left by the way-side as a whole new area of physics has emerged that grapples with the real issues in a more concrete way.

The problem though is that I’m just knowledgeable enough about this area of physics to know that I’ve far too little expertise to do it justice. Instead of attempting this, let me just provide a random list of things to read that give some idea of what I’m trying to refer to.

- About 4000 articles a year are appearing on the arXiv at quant-ph.
- Maximilian Schlosshauer’s Decoherence and the Quantum-to-Classical Transition and The quantum-to-classical transition and decoherence.
- Wojciech Zurek’s Decoherence and the Transition from Quantum to Classical – Revisited, and Quantum Darwinism.
- David Lindley’s book Where Does the Weirdness Go?.
- Jess Riedel blogs about some of this here.

Other suggestions of where to learn more from those better informed than me are welcome.

I don’t think the point of view I take about this is at all unusual, maybe it’s even the mainstream view in physics. The state of a system is given by a vector in Hilbert space, evolving according to the Schrodinger equation. This remains true when you consider the system you are observing together with the experimental apparatus. But a typical macroscopic experimental apparatus is an absurdly complicated quantum system, making the analysis of what happens and how classical behavior emerges a very difficult problem. As our technology improves and we have better and better ways to create larger coherent quantum systems, thinking about such systems I suspect will lead to better insight into the old “interpretational” issues.

From what I can see of this though, the question of the fundamental mathematical formalism of QM decouples from these hard issues. I know others see things quite differently, but I personally just don’t see evidence that the problem of better understanding the fundamental formalism (how do you quantize the metric degrees of freedom? how do these unify with the degrees of freedom of the SM?) has anything to do with the difficult issues described above. So, for now I’m trying to understand the simple problem, and leave the hard one to others.

Update: There’s a relevant conference going on this week.

Update: I’ve been pointed to another article that addresses in detail the issues referred to here, the recent Physics Reports Understanding quantum measurement from the solution of dynamical models, by Allahverdyan, Balian and Nieuwenhuizen.

Comments

1. **Bernhard**
   August 11, 2014

   This is nice. Your slogan is more or less the same as my first supervisor’s when I was still in undergrad school. He was an expert in dynamic systems and just couldn’t accept people wanted to “understand quantum mechanics without understanding classical mechanics” – which according to him, was way much harder. The “measurement problem” was his favorite subject.

   And finally; “But a typical macroscopic experimental apparatus is an absurdly complicated quantum system, making the analysis of what happens and how classical behavior emerges a very difficult problem. ” Was the main reason I didn’t first want to get involved with experimental physics when I was young. How the heck people believed what were they measuring…. ? 🙂

2. **Peter Morgan**
   August 11, 2014

   “But a typical macroscopic experimental apparatus is an absurdly complicated quantum system, making the analysis of what happens and how classical behavior emerges a very difficult problem.” When we enter into experimental contexts in which QFT is necessary for accurate description of the statistics of recorded measurement events, the complexity becomes even more impossible. My related (admittedly idiosyncratic) worry is that for sufficiently detailed classical theories we would expect nonlinearity whenever we consider elaborate waveforms, whereas the Wightman axioms (and practice more generally) require linearity when we consider elaborate waveforms (because quantum fields are required to be operator-valued distributions, even though such a structure is not necessary for us to be able to construct a Hilbert space).

   One doesn’t need to understand classical trajectories to understand that quantum theory describes the statistics of observed and (computer-)recorded discrete measurement events, which may sometimes be obviously lined up but more generally are separated by far enough and mixed-up enough that we can’t be sure that they’re on trajectories at all (but nonetheless there *are* statistics of events).

3. **Matt Leifer**
   August 11, 2014

"About 4000 articles a year are appearing on the arXiv at quant-ph."

Come on dude! The vast, vast majority of those articles have nothing to do with the problem you are talking about. The largest portion are about quantum computing, quantum information et. al., there is a portion on the sorts of approach to quantum foundations that you have indicated you have no interest in, and then there is a bunch of people writing papers about how to solve the Schrodinger equation and related differential equations, i.e. basically doing traditional applied math sorts of things. That leaves probably not more than a dozen or so papers per year that you would actually judge as relevant.

More broadly though, I disagree with you, as you might imagine. The whole literature on decoherence mostly ignores the problem of ontology, i.e. what actually exists. It is one thing to get out of the formalism something that looks approximately like the Liouville or Hamilton equations, but quite another to be able to argue that this means that there are things that actually exist in the world that approximately look like classical particles and fields obeying these equations.

Further, there is the whole problem of interpreting experiments that are thoroughly within the quantum world, e.g. things like nonlocality and contextuality. You are not going to be able to understand those things just by looking at a classical limit. And we are actually making both conceptual and mathematical progress in studying these things. It is not just interminable debates that go round and round in circles, as it may have been when you last looked.

Even disregarding all of this, you are just flat out wrong that understanding the emergence of classicality has nothing to do with the structure of the theory. The same ideas about group representations and so forth that go into understanding how to quantize a classical theory show up in a dual role in decoherence and related subjects. Ignoring one whole side of this seems a bit strange. For the category theorists out there, it is like studying a functor without studying the adjoint functor.

4. **Tim Campion**
   August 11, 2014

   A couple of nitpicks: In this post, when you describe the Born rule, I think you mean to say that the probability of measuring observable \$O\$ of a state \$\psi\$ to have value \$\lambda\$ is given by the modulus-squared of the projection of \$\mid \psi \rangle\$ into the eigenspace of \$O\$ with eigenvalue \$\lambda\$.

   Also, I started flipping through your notes and I noticed that on the bottom of page 12, your definitions of irreducible and indecomposable representations are flipped around from what I learned in Fulton & Harris, or what you can find [on wikipedia.]

5. **Peter Woit**
   August 11, 2014

   Matt,
   I think you're missing the repeated parts of this posting where I explain that I'm well aware I know very little about what is going on in "quantum foundations" these days. The period I was referring to when I did make an attempt to follow this was back in the 1970s, quite likely before you were born. I was trying to be polite by not giving examples of what I meant by people going over the same kind of thing being discussed back then. To be slightly more specific I'll just say I was thinking of several physicists and philosophers with a high public profile who go on about these issues in this way. The positive reference to the huge quant-ph literature was meant to indicate that there seem to me to be a wide variety of different sorts of interesting things going on with some relation to the "interpretation" question, too many for me to hope to follow.

   I repeat that I honestly would like to hear good suggestions for things to read to get a grasp of what people are working on, e.g. review/expository pieces aimed at surveying the subject for non-experts.

   We all have our own intellectual prejudices, one of mine is that I've never gotten any insight into anything from a discussion about ontology, but maybe I just haven't read the right discussion.

   I don't doubt that ideas about group representations and other aspects of the fundamental mathematical structures of the subject I've been writing about show up for instance in the analysis of decoherence. I just haven't seen evidence though that thinking about decoherence feeds back into the kind of question I mentioned about fundamental structure. Again, quite possibly this is just because I don't know that much about this, and if so I'd love to be enlightened.

6. **Peter Woit**
   August 11, 2014

   Tim,
   Thanks. You're right, here in the blog posting what I wrote assumes all eigenvalues distinct.

   You're also right about the notes. I was trying to keep things as simple as possible dealing with the "reducible but not decomposable" phenomenon as something beginners were encouraged to skip, but I messed that up. Will rewrite, maybe right now…

7. **ScentOfViolets**
   August 11, 2014

   We all have our own intellectual prejudices, one of mine is that I've never gotten any insight into anything from a discussion about ontology, but maybe I just haven't read the right discussion.

   And I thought it was just me. Maybe 'Shut up and calculate' is subsumed in 'I am now very bored with these sorts of discussions'. Really. In fact, let's turn this around: has anybody ever gotten much in the way of insight in these types of discussions? Personally, I know a few people who have been motivated by them, but when pressed, they uniformly admit nothing much ever came out all the jaw-jaw.

8. **Chris W.**
   August 11, 2014

   I think Peter Morgan is touching on my nagging worry, which is properly understanding the complexities of the system being observed in conjunction with the experimental apparatus is, *on the face of it*, essential to making the observations, and properly drawing conclusions from them about the theory being tested.

   To put it another way, we don't want to quantum mechanics to be just a formalism. We want it to be the basis of an empirical science, and grappling with the absurd complexities of actual experimental apparatus as quantum systems would be seem to be an unavoidable part of this. Of course, generations of physicists have been somehow negotiating these complexities day in and day out with a fair degree of self-assurance. The question is how they can manage to do this without kidding themselves.

   Maybe the answer is that they *never* know for sure that they aren't kidding themselves. All they can do is think up and carry out various kinds of consistency and sanity checks, and design experiments that facilitate those checks. The basis of their confidence is the accumulated history of all that work. So the best policy is to attend to the specifics of such techniques and solutions, and avoid getting tangled up in "ultimate" questions that lead nowhere.

   Of course, this kind of practical response is great until it gets deeply stuck…

9. **Peter Woit**
August 11, 2014

ScentofViolets,
"Shut up and calculate" is not my perspective, which is all about trying to understand why certain kinds of calculations work. The point of the posting was to identify a specific aspect of QM calculations that works for reasons I don't understand (the Born rule). Understanding this seems to be a different sort of problem than that of those parts of theory where calculations work for reasons that I do think have a compelling explanation.

The comments in the posting were intended to indicate what seem to me the reasons this is hard and I don't understand it. I'm perfectly willing to believe others do though and could point me to something enlightening.

10. **abbyyorker**
August 11, 2014

I thought that the Everett long thesis was worth reading, even if you do not like many worlds.

11. **Als**
August 11, 2014

"I repeat that I honestly would like to hear good suggestions for things to read to get a grasp of what people are working on, e.g. review/expository pieces aimed at surveying the subject for non-experts."

If you like topos theory, you may want to read this paper explaining Isham's research program : http://arxiv.org/pdf/0803.0417v1.pdf

12. **Justin**
August 12, 2014

Woit,

Given your reaction to the comments on 't Hooft's foundations of quantum thread, I think this thread was a bit dangerous. Hopefully it doesn't turn into that thread all over again. I've browsed and known people who are interested in these foundational issues. I find a lot of them (not all by any means) dogmatically defend Einstein's views on quantum mechanics, and therefore these discussions always lead to advertising hidden variables, most notoriously Bohmian mechanics. If you're actually interested in getting into this stuff, I think you will have to work hard to distinguish serious attempts to understand what is going on versus countless papers containing large quantities of pseudoscience about 'photons telepathically mind-reading backwards in time' (that's almost an exact quote from one person I know working on this stuff). I wish you the best of luck.

13. **Bee**
August 12, 2014

I'm not sure your distinction is between classical and quantum, it seems to be between small and large systems. It is typically the intermediate regime that is difficult, the quantum – classical transition, the microscopic-macroscopic transition, the regime where N is neither small nor infinity that is difficult.

I change my mind on this every couple of weeks, but every once in a while I find it quite possible that our failure to find a theory of quantum gravity stems from us having missed something in the process of quantization, that maybe we're just doing it wrong and at very high energies it works differently. Then again I think the string is the thing and then again I think asymptotically safe gravity is it, and then I start it all over again. So recently I've been doing some reading on statistical mechanics and it's quite interesting how much progress has been made there in the 'difficult' regime of finite N, fluctuations and non-equilibrium and quantum things. Who knows, maybe that's where the answer will come from.

14. **Ralph B**
August 12, 2014

I was reading Griffiths "Consistent Quantum Theory" when Wilzcek pronounced it "bracing." I was delighted!

15. **Francois**
August 12, 2014

Dear Peter,

Like Ralph B, I also liked Griffiths' book, and along the same line of thougt, Roland Omnes' review in Review of Modern Physics is quite clear. I recently tackled Bernard d'Espagnat's book (Traité de Physique et de Philosophie… I gather from your postings you read french ?). It has two parts, one about physics (quantum physics really) one about philosophy of science. He has quite a philosophical approach throughout the whole book, but it is written in a pedagogical manner accessible to the non-philosopher.

16. **Jasper**
August 12, 2014

I've been studying the measurement problem for the past year, and I'm currently satisfied with decoherence as a solution to the preferred basis problem & "unobservability" of superpositions: how a measurement can only be performed in certain bases and why superpositions disappear in the macro setting. This is because of the form of possible interactions in physics. We cannot measure in the basis corresponding to non-local superpositions because all(/most) interactions are local, i.e., the environment becomes entangled with the local parts of a superposition. In the end this results in the fact that the predictions for the system while disregarding the environment are done using a diagonal density matrix in the basis defined by the system-environment interactions.
Most importantly the timescale on which this occurs can be calculated given the system and the interactions with the environment.

The unsolved problem of single well-defined outcomes: QM tells us the probabilities for the possible outcomes, but why does an experiment give a single outcome?
In my opinion this cannot be solved in QM, since the system becomes more and more entangled with the environment and the measurement apparatus and all the options within this whole state never disappear because of unitary evolution in QM. To me this is an interpretational problem of QM.

To me, it comes from wanting QM to describe single runs, which it seems it does not. Most importantly one has to think of what a pure state means. Experimentally these states are only defined by a class of preparation procedures and can only be obtained by first doing rigorous statistical testing of these procedures. Simply put, I think pure states should be regarded as representing an idealized(!) state of a system with the most definite property obtainable in quantum theory for some set of observables.

The assumptions that are necessary to identify a single quantum system with a pure state:

1. During the verification of the preparation procedure any possible deviation in the final measurement statistics due to a finite ensemble are neglected. Also, the final

measurement statistics are dependent on additional assumptions on the quality of the measurement apparatus, such as the efficiency, the number of false detections (dark counts), and the dead time.
2. The final selective measurement during the verification of the preparation procedure is assumed to be able to distinguish between all possible (non-degenerate) results perfectly.
3. The fundamental and experimental uncertainties in the preparation procedure are neglected.
4. The entanglement of the system with the preparation apparatus is neglected and the state of the system is characterized solely by the outcome indicated by the final measurement device instead of the full entangled state between preparation apparatus and system.
5. The produced state is not affected by the presence or absence of the final measurement apparatus.
6. The ubiquitous interactions between the system and the environment, which can also include the system's internal degrees of freedom, are neglected.

If the pure state is an idealization, it becomes quite difficult to talk about single runs, and it seems one can only talk about ensembles.

In my opinion, QM is consistent with the view that although there are uncertainties about the individual events, however, the statistical results of many numbers of events are robust with respect to small changes in the conditions of the experimental setting.

However, this begs the question if it is possible to devise a way to find out more about individual events.. To me this seems almost impossible.

17. **Xu Jia**
August 12, 2014

John Bell's work. We just don't have such people as him having a clear thinking nowadays. I think if he could live longer and got his Nobel Prize in time, many things would be different. In his words, there are simply no such things as apparatus and "boundary between classical and quantum world".

18. **Matt Leifer**
August 12, 2014

Peter,

OK then, let me recommend at least one thing. I think you might like this long review article by Klaas Landsman http://arxiv.org/abs/quant-ph/0506082 The thing I like about it is that he emphasises the dual role of quantization and the emergence of classicality.

19. **Peter Woit**
August 12, 2014

Jasper,
Thanks, that's great. You're addressing exactly the sort of question that I've been wondering about and have had trouble finding a clear discussion of. If you can recommend where to read more about these issues that would be very helpful.
As a crude summary though, do you think it's fair to say that the source of supposed probabilistic nature of QM is that we only have probabilistic information about our experimental apparatus? Or does this summary miss something very important?

20. **Too Distinguished**
August 12, 2014

I second Xu Jia's comment that John Bell had the clearest perspective on these issues. And I agree with what some others have said, that complicated-sounding questions about the quantum-classical transition and measurement all reduce to the basic question of what, exactly, exists. The mathematical formalism of quantum mechanics works great, but when it comes to describing reality, it seems obvious that there have to be non-local hidden variables.

21. **Peter Woit**
August 12, 2014

Matt,
Thanks a lot! I look forward to reading the Landsman review carefully, he's great in general on the issue of the mathematical structure of the theory, I'd forgotten that he's written also on this topic.

Als,

Thanks for the reference!

22. **Peter Woit**
August 12, 2014

All,
Justin has a good point that these discussions can quickly degenerate into unenlightening discussions of hidden variables. The 't Hooft posting was about a "hidden variable" theory, this isn't, so no more of this please.

23. **nick herbert**
August 12, 2014

JUJITSU UNIVERSE
We house-broke quantum reality
Taught Schrödinger's Cat to purr
Now daily life's more uncanny
Than atoms ever were.

24. **manfred Requardt**
August 12, 2014

By the way, there does exist an alternative to decoherence by environment. See for example arXiv:1009.1220. Instead of the environment a restricted set of commuting macrovariables is introduced. It is like coarse graining and is based on a beautiful old paper by v.Neumann.

25. **Curious Mayhem**
August 12, 2014

This is certainly a compelling way to look at quantum mechanics, instead of the convoluted path that starts with classical mechanics. The problem has always been what von Neumann called R, the reduction upon measurement, not U, the simple unitary transformation in the Hilbert space.

And the right approach to come from the other direction is the "decoherence" approach — it's completely physical and can, in part, be tested in a laboratory. That sweeps away the stifling metaphysical cud-chewing of "interpretations" of QM, like Copenhagen, many worlds, etc.

26. **vmarko**
August 12, 2014

Jasper,

"I've been studying the measurement problem for the past year, and I'm currently satisfied with decoherence as a solution to the preferred basis problem"

I'd second Peter's request — can you point us to a good review which discusses this? No matter how various people phrase decoherence as a solution to the preferred basis problem, I always fail to understand it, and always find holes in their arguments.

"This is because of the form of possible interactions in physics. We cannot measure in the basis corresponding to non-local superpositions because all(/most) interactions are local"

And this is one of the things where I disagree most. People must not ignore quantum gravity — locality of interactions is based on the assumption of a classical spacetime manifold, i.e. that gravity is not quantized. As soon as you try to quantize gravity, you must allow for quantum superpositions of different spacetimes, and as a consequence locality goes out the window. And then the pointer basis problem comes back at you in its full glory, all over again.

So whatever you do to fix the pointer basis, *do not* base your argument on locality!

I would really, really like to see a serious review which systematically discusses how a pointer basis comes about, especially if "induced" by decoherence. And then I would probably have additional questions for the author. Any references, please?

Best, 🙂
Marko

27. **James Cooper**
August 13, 2014

These are targeted at a layman, and the site does have a crackpotish design, but I found them very helpful in understanding decoherence and explaining why the world must behave quantum mechanically
http://www.ipod.org.uk/reality/reality_decoherence.asp
http://www.ipod.org.uk/reality/reality_is_relative.asp

28. **Jasper**
August 13, 2014

Before I respond to some of the questions, I just want to say I have the same problem as Sabine (Bee): "I change my mind on this every couple of weeks".

@Matt:
It's fun to see you recommend the article of a my supervisor 😉 I'm a postdoc working with Klaas Landsman.

@Peter:

"As a crude summary though, do you think it's fair to say that the source of supposed probabilistic nature of QM is that we only have probabilistic information about our experimental apparatus?"
I think that's about right, but I'd rather say that if you wish to define a state of a single quantum system we need first to obtain probabilistic information about the *preparation procedure* and about the *detection procedure*. It is the robustness of this probabilistic information against known and unknown "small" changes in the procedures why we can say anything about quantum phenomena. It is hard to say what the exact conditions are in each run and this is often not mentioned. In most considerations in QM it is common to automatically jump to pure states of single quantum systems, without considering the preparation and detection procedures, but does this description really reflect nature?

As an example of some of the above: experimenters in quantum optics mostly view the polarizer to be in a certain direction and that after all photons in a finite ensemble pass the polarizer they are verified by a subsequent detection procedure to always have a polarization in the direction of the polarizer. We can thus for other experiments use these photons and simply say they are in a pure state of the polarization after passing the polarizer. However, is/was the polarizer always truly in the specified definite direction? Is the polarization detection procedure always along this very precise axis? It would seem to be better to describe the photon as entangled with the polarizer which has some spread along the mostly known direction, but this means that when restricting the state to the photon we should actually describe it by a density matrix, not a pure state.

Also, Peter, if you would like to have a discussion on these topics, please feel free to contact me!

@vmarko:
Could you help me understand your problems with decoherence as a solution to the basis problem? I'm always interested in learning the views of others, especially to see if I missed something important..
I think your theoretical argument about quantum gravity is valid, although I believe I can for the purpose of understanding the experimentally observed nature of reality ignore it for the moment, as the decoherence solution seems natural and satisfactory within this context. In my foundational research I try to stay as close to experiment as possible to not lead me astray. I do not know at the moment what is physical and observable about "superpositions of space-time" and what questions I am allowed to ask or not, and thus I simply try to solve the problems at hand in the best way possible. This is my take on research, you cannot solve all problems at once, simply solve a problem in one arena then try later to expand to the other. By using only observed effects of QM, namely entanglement, I do not see what is wrong with using the fact that the interaction between electrons and the EM-field is at a single ("local") point to understand that if I have a superposition of the electron |x1>+|x2> that after coupling with the environment it will be |x1>|E1>+|x2>|E2>. In the end this will mean that if ~0 coherence disappears on the local level of the electron. The interaction "simply" dictates how the environment couples to the system, if for example it coupled differently and would be of the form (|x1>+|x2>)|E12>, it would not lead to loss of coherence.. As you now see, in this case the interaction determines the preferred basis, it is "localized" in position space, and not in, for example, the basis |x1>+|x2>.

As for references:
Unfortunately, there are no clear explanations, but the references you have are the best starting point.

Schlosshauer's book is very good and has had considerable influence on my thoughts. Also Schlosshauer's "Elegance and enigma", which is a book with interviews of researchers of all corners of the foundational arena and gives a clear overview of how people can think about QM.

Zurek is also good, but his vision of the capabilities of decoherence might often be too far extrapolated for my taste.
The second chapter of the book by Audretsch "Entangled Systems New Directions in Quantum Physics" just for the clear explanation of some of the hidden assumptions people sometimes skip when interpreting experiments.

I recommend the following article by De Raedt, Katsnelson, Michielsen:
http://www.sciencedirect.com/science/article/pii/S000349161400102X
This article was also important for my view of QM. It overlaps with my overall view of what experiments say about nature and what can be inferred from them. However, as with almost any article, I don't fully agree with it.

Allahverdyan, Balian, Nieuwenhuizen for the references and the discussion: http://www.sciencedirect.com/science/article/pii/S0370157312004085
Do not believe any claims on their part on "solving" the measurement problem, but their summary, discussions and references on the relevant quantities to this problem are quite clear. Simply do not read most of the main text, unless you want the nitty gritty details of how to understand their specific (but semi-realistic) model describing entanglement of a certain measuring device with a system, the selection of a basis of detection and the loss of coherence of the measuring device and of the system in this basis. They do not solve the single outcome problem, but merely show the decoherence solution to the superposition and basis problem.

29. **manfred Requardt**
August 13, 2014

Certainly entanglement is an important concept in QM. But in my view decoherence by environment is similar to friction in classical mechanics, i.e. it is of a rather contingent nature. I doubt that such an unsystematic interaction with the environment is really the solution to the quantum measurement process.
A cornerstone of the decoherence philosophy is the socalled basis ambiguity (for macro staes) which is then claimed to be solved via the selection of a pointer basis by decoherence.
In arXiv:1009.1220 we showed that the construction of a subalgebra of macro observables within the larger algebra of microscopic observables does exactly the same job. We then get mixtures in the classical world living above the microscopic quantum world in which the corresponding many-body state is still pure.

30. **vmarko**
August 13, 2014

Jasper,

"Could you help me understand your problems with decoherence as a solution to the basis problem?"

I'd be glad to discuss this with you, but I am afraid that Peter's blog might not be the best way to do it. How about we take the discussion privately, via e-mail? Peter, can you connect us?

The thing with quantum gravity revolves around the issue of fundamentality of QM. If nature really obeys QM at the fundamental level, then QM should be capable of describing even cosmology. I just don't see how the measurement problem can be solved by decoherence in that setup (no locality and no environment). And if QM is not fundamental but only an effective theory, then it is not really relevant, and calls for a construction of some other, more fundamental theory.

Btw, thanks for the references! I didn't get the chance to look them up yet, but I'll certainly do it soon.

Best, 🙂
Marko

31. **Jim**
August 14, 2014

Peter,
Nice post. I think part of the problem is that these issues of interpretation force us to make a simple choice. We either 'shut up and calculate' or we acknowledge that something has gone wrong somewhere or something's missing. Adopting the second position involves indulging our inner metaphysician and speculating about what we think reality should or might be like. Unfortunately, most papers on the foundations of quantum theory never get back across the line – they're stuck in a metaphysical no-man's land with no prospect of saying anything that might eventually lead to some kind of empirical test. The few that succeeded in getting back across the line – Bohm, Bell, Leggett to name a few – have inspired some awesome experiments which have deepened the mystery but which have also opened up new areas of research in quantum information.

There's actually been quite a bit of progress but the fundamental problems remain seemingly intractable: How should we interpret quantum probability? Quantum non-locality and the collapse of the wave function? 'Spooky' action-at-a-distance? All the different interpretational schemes are designed to circumvent some or all of these, including many worlds (which simply avoids them by invoking a multiplicity of different realities in which all possible measurement outcomes are realised).

All these issues were well known and formed the basis of the famous Bohr-Einstein debate in the late 1920s.. We've been living with these problems for nearly a century.

32. **Martin**
August 14, 2014

Re decoherence: something similar at http://arxiv.org/abs/cond-mat/0303290

Unitary time evolution of the density matrix does not change its eigenvalues, yet this is necessary for thermodynamic change. Similar to decoherence: an incomplete description of the system yields nonsense.

33. **Jan Reimers**
August 14, 2014

Two things that have always bothered be about most discussions of this topic:
1) Over emphasis on the term "measurement" which implies that some sentient being is watching, which then drags the conversation off into ridiculous directions. It is really just interactions which generate a macroscopic effect that creates the illusion of wave function collapse. This can happen in lab, but it mostly happens everywhere else, including half way between here and Andromeda for example. Is there any reason we must call it the "measurement problem" rather than the "interaction problem"?

2) Ignoring basis selection (einselection) as exemplified by the http://www.ipod.org.uk/reality/reality_decoherence.asp article. The density matrix goes diagonal in a particular basis,but why is that basis selected? Local interactions select a basis that localized in position space … how does that work and is it a universal rule?

Thanks
JR

34. **James Cooper**
    August 14, 2014

    > There’s actually been quite a bit of progress but the fundamental problems remain seemingly intractable: How should we interpret quantum probability? Quantum non-locality and the collapse of the wave function? ‘Spooky’ action-at-a-distance?

    Jim,
    Why would you say these problems are intractable? Of the last 3, one could say the decoherence papers has answered them. Heisenberg himself understood much of it in an albeit vague way. There is no “collapse” of the wave function – it just decoheres. Once it does so (ie. can no longer interfere with itself) that is what we call ‘reality’ which is just another word for the decoherence. Classical physics and classical intuition is an emergent phenomenon. Nature is not realist: your physical state is only an approximation and has no real meaning.
    As much as I hate to link to something Lubos wrote- the first 2 answers in this thread really sum up this way of thinking
    http://physics.stackexchange.com/questions/3158/why-quantum-entanglement-is-considered-to-be-active-link-between-particles

    I think the 1st question (on probablity) is the interesting one. Once we accept that nature is not deterministic (BTW, If it was, I couldn’t tell my fingers to type the keys they are typing!) and that quantum mechanics is complete, we can do away with Bohm and MWI and get back to the most interesting interpretation of all – Conciousness – What is it and how does it work

35. **Martin**
    August 14, 2014

    @Jan, point 2): this is specified by the observer. A system is typically described with a density matrix that corresponds to the energy of the interaction that yields the observation. E.g. at very low energies an atom can be described as a little ball without internal dynamics. At higher energy electronics transitions must be taken into account, at very much higher energy the internal dynamics of the atomic nucleus starts to play a role, etc. This is why classical theories such as the Navier-Stokes equation work at low excitation energies (long wavelengths) even though they ignore the molecular microstructure of the system (which enters the theory as parameter values such as viscosity).

36. **vmarko**
    August 14, 2014

    Folks, please stop telling everyone that decoherence solves the measurement problem. It doesn’t, until someone finds a solution to the preferred basis problem.

    Let me explain it a bit: suppose we are about to measure the spin of an electron, which is in the initial state |u>+|d> (I’ll be ignoring normalization factors of 1/sqrt(2) throughout). The “measurement” means to let it interact with the environment (i.e. the apparatus). After the interaction, we can write the state of the big system (electron plus environment) as |Psi> = |u>|Eu>+|d>|Ed>, where “Eu” stands for “environment has recorded the spin of the electron to be up”, and similarly for “Ed”.

    Now let me formulate the problem. I can introduce a different basis for the electron, |+> and |->, which is rotated by 45 degrees, as |+>=|u>+|d>, |->=|u>-|d>, and similarly |E+> and |E-> for the environment. Then I can rewrite the final state as |Psi>=|+>|E+>+|->|E-> (it’s the same state as above, this is just a change of variables). The problem is now that in experiment we always find the apparatus in states |Eu>,|Ed>, and never in |E+>,|E->. The former is somehow preferred over the latter by experiment, but not by theory (short of invoking the collapse postulate of Copenhagen interpretation). This is called the “preferred basis problem” or “pointer basis problem”.

    I simply don’t see how decoherence can be invoked in any way to favor one basis over the other (and I am not alone in this). Until you can provide a clean resolution to the pointer basis problem, please stop endlessly repeating that decoherence solves the measurement problem. It doesn’t.

    HTH, 🙂
    Marko

37. **Jim**
    August 14, 2014

    James Cooper
    vmarko answered your point on decoherence before I could respond, and I couldn’t have put it better. I haven’t looked at the literature for a while, but my understanding is that decoherence doesn’t solve what Roland Omnes called the ‘problem of objectification’. Roger Penrose called it the problem of objective reduction and there’s a nice quote from John Bell somewhere about decoherence being unable to explain how ‘and’ is replaced by ‘or’.

    I’d be wary of trying to resolve the problems by connecting measurement with consciousness. This has a long history (von Neumann – inspired by Wigner – credited consciousness as an explanation for his projection postulate – essentially the collapse of the wave function – in his famous book Mathematical Principles of Quantum Theory published in 1932). Trying to shed light on something we don’t understand by invoking something else we don’t understand has always seemed an odd strategy to me.

38. **Jasper**
    August 14, 2014

    Simple answer to Marco ‘s problem : there are no realistic coupling terms in quantum theory which couple the system and environment in the second basis, just as I gave in my original example for the spatial superposition. To measure the spin of an electron you need to use the electromagnetic coupling.. and work from there.
    If you want to understand this important point, study a realistic model with realistic coupling between spin, measuring device and an environment.. In the end the interaction terms determine the bases which are robust and coherence terms dissappear on the level of system and measuring apparatus.

    Also, as said, decoherence does not solve the single outcome problem aka the measurement problem, only basis and superposition problem.

39. **vmarko**
    August 14, 2014

    Jasper,

    “there are no realistic coupling terms in quantum theory which couple the system and environment in the second basis”

    I don’t understand this. The state |psi> that I wrote is the *final* state (the one describing the electron and the environment *after* the interaction), and I wrote the *same* state in two different bases. The properties of the interaction are important only when calculating the final state from the initial state. But once known, the same final state can be represented in two bases, and in experiments we always see only one of them and not the other. The interaction simply plays no role in this, as I see it.

    Can you explain your point more precisely?

Best, 🙂
Marko

40. **Jasper**
August 15, 2014

This will be my last post, because this discussion is time consuming. I recommend reading Schlosshauer since it is very clear.

The coupling picks which components of the environment couple to the which components of the system. The environment is thus correlated with specific information on the system, then if the distinct environmental states are almost orthogonal you can say that on the level of the system the coherence has almost disappeared in the components of the system picked by the coupling mechanism.

Example: An environment of light particles scattering with a massive particle, which is in a superposition, using contact interactions
(|x1>+|x2>)|E> -> |x1>|E1>+|x2>|E2>
The states |E1> and |E2> become more and more distinguishable as they encode many collisions of the light particles with the massive particle. The states become more and more orthogonal, thus coherence disappears between |x1> and |x2>. Any other states of the environment would not have this orthogonality. In fact, I can derive the timescale of this orthogonality…

Another example (hope this works, since I'm making it up as I go along): Qubit encoded in hyperfine levels of a atom with monitoring by stimulated photon emission

A qubit is encoded in two levels of an atom, where the state |u> corresponds to a higher energy level than |d>. Then we couple with an EM field which has exactly one photon in with exactly the energy difference between the levels. Stimulated emission means that the EM field (read: environment) becomes entangled with the qubit:
|u>|1> -> a|u>|1>+b|d>|2>
Here |1> means 1 photon with the specific energy and |2> two photons . Note that |1> and |2> are orthogonal and that they couple in a clear way in the basis |u> and |d>.

For the down state: |d>|1> -> c|d>|1>+d|u>|0>.
A superposition of the qubit thus behaves as
(|u>+|d>)|1>->a|u>|1>+b|d>|2>+c|d>|1>+d|u>|0>
Since |0>, |1> and |2> are orthogonal, when restricting to the qubit (read: trace over EM states of density matrix) we can see that coherence between |u> and |d> can become less (Note: depends on specific details of coupling, i.e. the coefficients a b c d).

Yes, after all this I can still write the density matrix in another way, but the orthogonal states of the EM field only couple in the above specific way to the atom,in the |u> and |d> basis which is where coherence is lost. In other words, using EM field we can only "observe" |u> or |d> of this atom, not a superposition |u>+|d>, and thus this basis is preferred.

Maybe not the most clear example and a bit too ideal, but the point is that for all realistic physical systems, with some thought, the preferred basis can be identified in which entanglement occurs which results in loss of coherence.

Anyway, hope you kind of see the idea.. It's better to read the book.

41. **manfred Requardt**
August 15, 2014

These problems how a pointer basis is selected, i.e. the basis ambiguity, is discussed and in my view solved outside the realm of decoherence in the paper I mentioned above. I think decoherence is a too cheap solution to be correct under all circumstances.

42. **Martin**
August 15, 2014

@vmarko "The state |psi> that I wrote is the *final* state (the one describing the electron and the environment *after* the interaction)"

No, the interaction is the measurement. After the measurement it is known whether the spin was up or down. What you write is that the measurement has taken place but nobody is aware of the outcome and your |Psi> indicates what the experimenter might guess about what has been recorded. Probability has to do with observation. "Probability" does not mean anything without conciousness. This is why QM probability is Bayesian. Like every probability. Forget Kolmogorov.

43. **vmarko**
August 16, 2014

Jasper,

"This will be my last post, because this discussion is time consuming."

At first I thought of giving up on the discussion, but then I changed my mind, since other people reading this blog might still be confused. I'm sorry that you don't want to continue.

In the first example, you said

"The states become more and more orthogonal, thus coherence disappears between |x1> and |x2>. Any other states of the environment would not have this orthogonality."

The last statement is just false. If by "orthogonality" you mean that the scalar product between |E1> and |E2> is zero, then the basis |E+>=|E1>+|E2> , |E->=|E1>-|E2> is also orthogonal. Feel free to check.

Orthogonality of the resulting basis plays no role in the discussion. The total Hamiltonian of system and environment is always self-adjoint, and its eigenvectors are always orthogonal. But there are also infinitely many other bases which are orthogonal as well, and I don't understand the importance of the basis |E1>,|E2> in this example.

Your second example is more complicated, due to the nonconservation of the number of photons, and because you didn't specify all interactions. You said

"Yes, after all this I can still write the density matrix in another way, but the orthogonal states of the EM field only couple in the above specific way to the atom,in the |u> and |d> basis which is where coherence is lost. In other words, using EM field we can only "observe" |u> or |d> of this atom, not a superposition |u>+|d>, and thus this basis is preferred."

Once you specify the final states that correspond to initial states |u>|2>, |u>|0>, |d>|2> and |d>|0>, I will be able to change from |u>,|d> to |+>,|-> basis for the atom, and from |0>,|1>,|2> basis to another basis for the EM field, which will be a linear combination of |0>,|1>,|2> which is orthogonal and which couples to |+>|-> in exactly the same way that |0>,|1>,|2> couples to |u>,|d>. So I still don't see why |u>,|d> basis is unique in any physical sense.

What I think you're trying to appeal to is the fact that the interaction Hamiltonian is generically always local in coordinate representatation, as opposed to say momentum representation (where it is nonlocal). And then you could say that the coordinate basis is somehow physically preferred because the interaction Hamiltonian is local *only* in that basis. This argument is ok, and it is good as long as you ignore quantum gravity. However, in QG the interaction Hamiltonian will become nonlocal even in the coordinate basis, and generically there will be no basis where H_int would be local. That's where the locality argument fails.

I'll make sure to read the Schlosshauer book.

Best, 🙂
Marko

44. **nick herbert**
August 16, 2014

Elliot Tammaro has a nice review of the various quantum interpretations at http://lanl.arxiv.org/abs/1408.2093. In Tammao's view none of the present-day interpretations fare very well.

45. **HDZ**
August 17, 2014

vmarko,

you are making a trivial but common mistake. Your example is valid only if the amplitudes of both components are exactly equal. (Feel free to check!) This ambiguity just reflects the fact that there is no unique diagonal representation of a density matrix in the case of degeneracy.

However, the point of decoherence is that macroscopic properties are "permanently" (repeatedly) and uncontrollably (irreversibly) measured by their environment with respect to a given basis. So even in the case of degeneracy, the "wrong" components would immediately be decohered again and again, while the "correct" ones (those characterized by the unavoidable environment) are "robust" under further decoherence (what Zurek later called the "pointer basis"). These components are thus dynamically "autonomous" after a measurement proper. If unitarity holds globally, it must necessarily lead to a superposition of many such autonomous "worlds".

If you want to avoid that consequence, you have to modify quantum theory (such as by a collapse). This is certainly not a matter of mathematical sophistication (as Peter Woit seems to believe). Formal limits of infinite numbers of particles or degrees of freedom have often be misused in order to hide an essential physical issue.

You may like to have a look at Sect. 3 of http://lanl.arxiv.org/abs/1402.5498

46. **Peter Woit**
August 17, 2014

HDZ,
I don't at all think that any of the issues discussed here are issues requiring mathematical sophistication, rather that they are complex issues of the physics taking place. My interest in more sophisticated mathematical frameworks for quantum mechanics is aimed in a very different direction (understanding aspects of the standard model that are still mysterious, as well as the question of unification with gravity).

2014 Fields Medals

August 12, 2014
Categories: Uncategorized

I thought this wasn't supposed to be announced until late this evening New York time, but the Fields Medal announcement is now online. The winners are:

- Artur Avila
- Manjul Bhargava
- Martin Hairer
- Maryam Mirzakhani

Mirzakhani is the first woman to win a Fields medal. Congratulations to all four.

I'm not at all knowledgeable about the work of this year's medalists, for this you can consult the press releases on the ICM page.

Update: Quanta magazine has profiles of the winners. Avila, Bhargava, Hairer, Mirzakhani.

Update: For ICM blogging, clearly the place to go is the blog of a Fields Medalist.

Update: According to Tim Gowers, the Fields Medal Committee was: Daubechies, Ambrosio, Eisenbud, Fukaya, Ghys, Dick Gross, Kirwan, Kollar, Kontsevich, Struwe, Zeitouni and Günter Ziegler.

Update: For two very different sorts of blog posts about the Fields Medal, see Terry Tao and Mathbabe.

Comments

1. **Bill**
   August 12, 2014

   Peter, it's Avila, not Avial. Do you know who was on the committee? Strange for them to announce it before the opening ceremony.

2. **Peter Woit**
   August 12, 2014

   Bill,
   Typo fixed. I haven't seen the committee list, don't know if it's public yet. Yes, it is strange for this to be posted before the opening ceremony. Also strange is that from the history it looks like this was posted on Wikipedia yesterday, even before the early ICM page went up.

3. **Als**
   August 12, 2014

   Great choice.

   I think some category theorists are going to be a bit disappointed for Lurie, but he did get a 3 million dollars consolation prize…

4. **Daniel McLaury**
   August 12, 2014

   Given that the page was written in the past tense ("the winners were announced"), this is pretty obviously an accident.

   What's interesting to me is that Quanta clearly had all those profiles ready to go in advance. Who else must they have written profiles for?

5. **Peter Woit**
   August 12, 2014

   Daniel,
   In cases like this journalists are often contacted in advance and told in confidence what will be announced, as long as they agree to "embargo" their stories and not release them publicly until a specified time. I'm sure this is how the Quanta stories came about.

   The odd thing here is that one would have guessed that things were embargoed until the public announcement in Seoul (10:30pm today here in New York). It's unclear whether it was intentional or not that the news got out earlier than this.

6. **Peter Woit**
   August 12, 2014

   Googling "Fields Medal" and "embargo" turns up various things, for instance this from the 2006 version
   http://www.icm2006.org/archivos/File/icm_2006/1%20August%202006%20note%20prensa%20rey%20Eng.pdf
   where news was given to the press on August 15, embargoed until the time of the ceremony on August 22.
   For the Seoul ICM, a cached journalist registration form shows up, which says that names of Fields Medalists are embargoed until the ceremony.

7. **David Roberts**
   August 12, 2014

   The official website was probably primed to go and the page released at something like midnight Seoul time. I got the first message about this 3.30 am local, and I'm in the same time zone as South Korea. It may have been a planned thing, or a small bungle on behalf of a programmer who set the day of release, but not the time. And then other news sources could follow suit, since the cat was out of the bag.

8. **Bill**
   August 12, 2014

Quanta profiles are way over the top for my taste, even though I liked some of the comments from the winners about how they focus on the problems. Another thought that came to mind – billiards everywhere (especially, if you include this year's Abel prize). No updates on the committee yet on the IMU website.

9. **Ethan**
August 12, 2014

Congratulations to all the winners, indeed!

One question that I have though is how relevant is these days the Fields Medal compared to other awards that exist right now in Mathematics.

Certainly, at the time the medal was introduced, there were no other high profile awards in Mathematics. These days, there are many other awards which do not have the insidious 40 years old limit.

The discrimination that these medals help perpetuate, ageism, is preposterous. To cite perhaps the most visible case -although there are others-, Andrew Wiles didn't get a Fields Medals for proving Fermat's last theorem, only because of the age limit.

The New York Times had an article about how the Fields Medal got to gain its status as the "Nobel Prize of Mathematics" http://www.nytimes.com/2014/08/10/opinion/sunday/how-math-got-its-nobel-.html?_r=0 . Not sure if the argument is valid though, because there are tales of how John Nash was disappointed when he didn't get one back in the 1950s.

Any thoughts?

10. **Peter Woit**
August 12, 2014

Ethan,
The combination of no Nobel prize and the age limit have always made the Fields medal kind of unusual. The prize was always well-known among mathematicians, especially after the fifties, but little-known outside mathematics. I don't think the Smale story referred to in the NYT had that big an impact.
So far the existence of other prizes doesn't seem to have changed much. The Abel prize is set up to be a Nobel equivalent, but it gets given to people late in their careers. The Fields medal always had a significant impact since it was identifying not the grand old men of the field, but the young stars. Unclear yet what the effect of the Milner/Zuckerberg prize with its big payday will be.

Over the past 20 years I see two conflicting trends: the first is that society and academia is ever more star-driven, so more and more attention gets paid to awards and those identified as stars.

The second though is that something has changed in math research, with a lot more research mathematicians working on more and more specialized problems. The first time I went to an ICM was Berkeley in 1986, and that year I don't remember much speculation earlier about the prize, just because the results of Donaldson, Faltings and Freedman were so dramatic it seemed pretty obvious who the prize winners would be (I also suspect there was much less effort to keep the news secret). To some extent this was also true in 1990 (with that year an interesting controversy about giving the prize to Witten, a non-mathematician).

From 1994 on though, my impression is that a few of the choices have been obvious, but while the rest go to very good people, they are choices that could easily have been different and no one would have been surprised. Instead of two-four people with results dramatically more significant than everyone else's, there may be just one such person. For example, this year I think people would have been surprised if Bhargava didn't get a prize, but not surprised if the other three winners had been, for example, Lurie, Ben Green, and Sophie Morel. Similarly, last time Ngo was a clear choice, but the other three names could easily have been different.

So, these days I see Fields medalists as getting a lot more attention from outside mathematics than in the old days. From within the community, the emphasis on academic stars mean they get more attention, while the second trend means they get less, since there's an awareness that there are equally good people in the same cohort without the prize.

11. **David Roberts**
August 12, 2014

Michael Nielsen commented on Terry Tao's post

https://plus.google.com/u/0/114134834346472219368/posts/huX13ZppDAf

that the announcement on the official site was removed, then reinstated. Seems like it could have been a mistake.

12. **Ethan**
August 12, 2014

Peter,

Thank you for the detailed explanations.

I have always loved mathematics. Although I got my PhD in engineering, my adviser was a mathematician and my thesis work involved applied mathematics, so I tend to follow the discipline closer than other people in my field.

I have not been around professionally for that long but it is definitely true that when I was in graduate school both trends you mention were in full force.

While I see it as a welcome development that society at large gives due recognition to scientific work, mathematics in particular, I find the "celebrity driven culture" a bit troublesome, both in academia and society at large, but particularly in academia where the pursuit of knowledge for its own sake should trump other issues.

Thanks for this interesting blog.

13. **John McAllison**
August 12, 2014

Iran, of all places in the world, manages to produce the first female Field's Medalist in Maryam Mirzakhani?

No script writer would have the imagination to think this story line up.

14. **Ethan**
August 12, 2014

John,

Up until I went to graduate school at Stanford, I was also ignorant about Iran's very strong tradition of cultivating intellectual elites both male and female.

It is perhaps Iran's best kept secret, although if you look at the winners of scientific Olympiads for high schoolers, you see Iranians at the top every year. Some of the good friends I made in graduate school went also to Sharif University, including several brilliant females.

The real tragedy for Iran is that it educates all these bright people only to expel them from Iran, "intellectually speaking", that is.

15. **John McAllison**
August 12, 2014

Ethan,

My understanding of Iran is that women's rights were severely eroded after the 1979 Islamic revolution, and only started to recover significantly around the start of the 90s. Even today, Iran still comes across as generally being very backward when it comes to gender equality compared to more secular Islamic majority nations like Turkey.

It'll be interesting to see how the Islamic Republic of Iran Broadcasting reports this, given that the Quran claims a man's testimony is worth twice that of a woman's in an Islamic court of law.

16. **Boon**
August 12, 2014

Any idea as to why Lurie didn't win?

17. **Ethan**
August 12, 2014

My experience with Iranian scholars contradicts the general perception :). In fact, there are many Iranian women educated after 1979 who hold faculty positions in America's top universities. As I said, knowing my share of Iranian Americans, or Iranians who have settled in America, the news of an Iranian woman settled in the US receiving the Fields Medal wasn't that surprising.

I think that many here in the West confuse giving females opportunities to become brilliant scholars with embracing Western feminism. I do not know personally Maryam Mirzakhani but I know people who know her. Most Iranians that I know -in fact I would say the overwhelming majority of them-, both male and female, hold what would be considered "socially conservative values" in the US when it comes to sex outside marriage, gay marriage, abortion, etc.

18. **Bill**
August 12, 2014

Boon, see Peter's comment above for possible answers. Or let's wait and see who was on the committee. If you watch Quanta videos, Hairer's main insight came from the theory of wavelets (Daubechies was the President of the IMU and the chair of the committee, also the first female chair). Avila and Mirzakhani's main work is on dynamical systems (billiards), so I wouldn't be surprised to see people from that area on the committee. All four were great choices, but clearly Jacob Lurie would also be a great choice.

19. **Als**
August 12, 2014

"Up until I went to graduate school at Stanford, I was also ignorant about Iran's very strong tradition of cultivating intellectual elites both male and female."

Exactly. People forget that Iran is a very old and great civilisation, with many great mathematicians (actually most of so-called "arab" mathematicians were actually Persians).

"Any idea as to why Lurie didn't win?"

There's no way to know, but it's easy to guess: he didn't prove any big theorem, all the four recipients did.

20. **Ethan**
August 12, 2014

Another quick question, is there a streaming link so we can watch the plenary sessions live? I haven't found any in the main page http://www.icm2014.org/

21. **Ethan**
August 12, 2014

OK, here it comes,

http://www.ebsmath.co.kr/cosCenter/seoul2014ICMOnAirPop?type=2

The pop up blocker was preventing me from seeing it :).

22. **A.J.**
August 12, 2014

Regarding Lurie: He's done some very notable work — his proof of the cobordism hypothesis is indeed a big theorem, likewise his geometric construction of tmf — but much of it hasn't gone through traditional peer review channels. I'd guess the selection commitee is waiting to see something more formal in print.

23. **Bourbaki**
August 12, 2014

@Ala
"Any idea as to why Lurie didn't win?"

There's no way to know, but it's easy to guess: he didn't prove any big theorem, all the four recipients did."

Jacob Lurie proved the Baez-Dolan Cobordism hypothesis. Along with Denis Gaitsgory, he proved Seigel-Mass formula for function fields. Saying that Jacob Lurie didn't prove any "big" theorem is like saying Grothendeick never proved any "big" theorem.

"Any idea as to why Lurie didn't win?"

Fields Medal these days is a function of whose is in the Prize committee.
Even then omission of Jacob Lurie in the list was quite surprising.

24. **Andy**
August 13, 2014

Bourbaki – A correction: he has *claimed* proofs of those results. However, he still has not managed to produce papers containing said proofs. Moreover, it's not clear to me that either of them is particularly central to mathematics (maybe the mass formula is, but the cobordism hypothesis is pretty technical and marginal).

Also, Grothendieck had plenty of deep results to his name early in his career (e.g. Grothendieck-Riemann-Roch).

25. **Als**
August 13, 2014

"Also, Grothendieck had plenty of deep results to his name early in his career (e.g. Grothendieck-Riemann-Roch)."

And don't forget his work in analysis, where there are some beautiful results.

26. **Urs Schreiber**
August 13, 2014

Saying that Jacob Lurie did not prove any big theorem is like lamenting the lack of a rock when faced with a mountain.

27. **Bourbaki**
August 13, 2014

@Andy

Jacob Lurie has already published a detailed sketch of the proof.

Also, Grothendieck never published his Reimann-Roch theorem, as he was never satisfied with the proof. And the reason why Grothendeick is Grothendeick is not because of his results in functional analysis.

28. **Andy**
August 13, 2014

Bourbaki – A detailed sketch is not the same thing as a proof. I don't care how smart Jacob is, he is not exempt from the ordinary rules of mathematical practice. Until a detailed proof appears (written by him or someone else), the theorems are not yet proved. Period.

And Grothendieck did publish a proof of GRR in SGA6 (which I guess was not peer-reviewed in the usual sense, but it's all there). Moreover, Borel-Serre published a detailed account of GRR in 1958, long before Grothendieck's Fields medal

29. **Bourbaki**
August 13, 2014

Most mathematicians working in Lurie's area actually believe the proof is complete and correct as well. And that's what matters.

As far Grothendeick is concerned, his important works were never went through the traditional peer-review process (for the the obvious reasons.)

And yes, Edward Witten got the Fields medal with ever proving any theorem.

30. **Als**
August 13, 2014

"Most mathematicians working in Lurie's area actually believe the proof is complete and correct as well. And that's what matters."

I agree that there are precedents (Witten, Perelman, Thurston), so I don't think it's the reason the committee didn't chose him.

31. **AJ**
August 13, 2014

Technically Lurie will be qualified for 2018 since he will just be 40.

32. **mkf**
August 13, 2014

Regarding, that the names were already in wikipedia on Monday, they accidentally put the names on the official ICM 2014 page on Monday morning (Middle European time) where I also saw them (and also the other prize winners, it seemed that they wrote the entries at that time since they appeared one after the other).

33. **Bill**
August 13, 2014

Lurie will turn 40 a few weeks before January 1, 2018, so this was his last chance.

34. **jjj**
August 13, 2014

Avila is the only one I happen to know, and whose work I know something of. He certainly deserves it. Everyone has known this for a long time, and he is just 35. It has been both scary and exciting to work in this general area in recent years
(interval exchange transformations, Teichmueller flow) where much deep and fascinating work had been done by many people (to avoid offending anyone I will just

mention the first greats: Keane, Veech and Masur) when Zorich appeared with lucid explanations, an exciting conjecture and his wonderful approach to math (generous, kind) and all of a sudden many superstars were involved: Yoccoz, Kontsevich, McMullen. I stop there mentioning only the Fields medalists, since it is unfair to mention one other name without a long historical discourse and many many names, explaining all the deep and wonderful contributions. You will note I didn't even mention Thurston, though I should have.

Avila has not worked in isolation; much the opposite, he has in a very egoless way
benefited and learned from everyone else and given back to them. Basically, he just loves math, and this is what is such an inspiration. But this shows the paradox of one amazing person being singled out for a prize, when it is the whole community they represent who should (and will) celebrate; we know we are part of it. "We" doesn't just mean dynamicists, it means mathematicians, or beyond that anyone who draws inspiration from the wonders of nature, and of its interaction with the human mind.

My one real talk with Artur was after an amazing lecture of his; at lunch I asked for further explanation of some point, and he talked (rapidly and clearly) for 5 or 10 minutes. I felt like the ideas were pouring into my head, which was on the verge of exploding.

Well, that's Artur. It is so great that there have been places and opportunities like
IMPA and the CNRS where someone like this has had the conditions to work and grow and to inspire, and let's hope that the powers can be can see that these working conditions need to be made more generally available; freedom and inspiration is how you get good math done, the opposite of the bureaucratic and business-style models that are forced over every part of human existence, more and more. To think, really to think and to create, and to encourage that in others, is the revolutionary antidote, revolutionary in that it turns the apparent world right on its head and says, "really?", and laughs.

So, congratulations to all of us, and let's use the inspiration to prove some theorems!

35. **Als**
August 13, 2014

@jjj

I completely agree concerning Avila and I think Peter is wrong when he says it could have been anyone along with Bhargava. Many people were expecting him to get it for quite some time.

36. **Ethan**
August 13, 2014

Now that most media have covered the news, the emphasis seems to be on,

– First woman to ever win a Fields Medal (less emphasis on her being Iranian).

– First Brazilian/South American to ever win a Fields Medal.

This, I think, is linked to Peter's observation that we are increasingly a star driven culture, which tends to favor shallow superficial things instead of content. You can also see this reflected in the number, and content, of the comments in the respective Quanta entries.

I think that this emphasis on matters not related to the research for which they (and the other two) received the medal is bad for Mathematics. It is also bad for the winners themselves because there will always be doubts whether somebody else could have been named winner, like the aforementioned Jacob Lurie, had it not been because of the "firsts". Gowers said http://gowers.wordpress.com/2014/08/13/icm2014-opening-ceremony/#more-5567

"I think that this time round there were an unusually large number of people who could easily have got medals, including other women. (This last point is important — one should think of Mirzakhani's medal as the new normal rather than as some freak event.) I have two words to say about them: Mikhail Gromov."

37. **JG**
August 13, 2014

Congratulations to all the winners. Iran's strong tradition in the Mathematics Olympiad is impressive, they usually have performed better than Israel, France, UK, Germany for example. It is sad that this is not matched with a strong publication record. I assume this is due to emigration, as in Mirzakhani's case, though she has said that there was a big emphasis on problem solving rather than theory, and she had to learn a lot of basic undergraduate mathematics when she went to Harvard – which makes her subsequent publication record all the more impressive.

For a humbling experience, see if you can match Mirzakhani's perfect score from the 1995 olympiad (problems) . Preferably within two days 🙂

38. **egan**
August 13, 2014

Question : is it possible for a mathematician to win two Fields medals? For instance one at the beginning of his career and a second one just before turning 40 after having proved a big theorem?
Os is it explicitly forbidden by the rules of the Fields committee?

39. **Ethan**
August 13, 2014

egan,

Unclear. The IMU's nominating rules say,

http://www.mathunion.org/general/prizes/nomination-guidelines/

"No winner of one of these Medals/Prizes is eligible for one of the others. "

Which clearly excludes a winner of one of the prizes/medals from being considered for the others -something that I find odd, particularly with respect to the Chern Medal- but I don't think it necessarily follows that somebody cannot win two Fields Medals.

40. **Yatima**
August 13, 2014

@John McAllison

It'll be interesting to see how the Islamic Republic of Iran Broadcasting reports this, given that the Quran claims a man's testimony is worth twice that of a woman's in an Islamic court of law.

Well … what do you expect? Iran ≠ ISIS.

IRIB world service: [Iranian woman wins Nobel Prize of mathematics]

41. **Peter Woit**
August 13, 2014

Enough about the Iranian government here, thanks.

42. **Mathematician**
August 13, 2014

I was surprised at this, here
http://www.simonsfoundation.org/quanta/20140812-in-mathematical-noise-one-who-heard-music/
"Hairer learned that he had won the Fields Medal during a visit to Columbia University in February."
I didn't realise they were informed so far in advance. That's a long time to have to keep a secret.

43. **Bourbaki**
August 13, 2014

"The second though is that something has changed in math research, with a lot more research mathematicians working on more and more specialized problems. "

I would be interested in knowing what caused this change. It seems mathematicians are now focused more specialized and more accessible problems than going for the big problems (of course, there are exceptions to this rule).

44. **Frederick**
August 13, 2014

"This, I think, is linked to Peter's observation that we are increasingly a star driven culture, which tends to favor shallow superficial things instead of content. You can also see this reflected in the number, and content, of the comments in the respective Quanta entries."

In fairness, I believe the Quanta entries were aimed at a lay audience. Very few people outside of the profession, no matter how smart, are going to have more than a cutaneous understanding of the work produced by Fields Medalists. I'm not sure what kind of analysis you were expecting. Even Terence Tao admitted that much of Bhargava's work is beyond his ken.

Personally, I'd rather see scientists, mathematicians, novelists, and other intellectuals turned into celebrities than actors and athletes. In many Asian cultures, the results of Science Olympiads are followed with the same enthusiasm that Europeans show for soccer.

45. **Ethan**
August 13, 2014

Frederick,

Please don't take me wrong.

I am not talking about the question of which type of celebrities I rather have, in which case, I agree with you, I rather see scientists and mathematicians celebrities than professional athletes, actors or, my absolute worse, people like Paris Hilton or Kim Kardashian .

What I am saying, and I think that this is what Peter was saying too, is that I do not see the fact that increasingly academia, or my field of engineering, are becoming star/celebrity driven as a good thing for either.

Here in Silicon Valley this star/fame driven culture caused a crash of gigantic proportions in the late 1990s that had reverberations in the larger economy. Some say that it is about to cause another one in the next 1-2 years. To a certain degree, the financial crash of 2007/2008 was driven by similar forces.

Some take, in my opinion, the wrong view that increasing the celebrity status of people working in these fields will attract more high quality interested students to them but in fact I contend that those who are more prone to make significant contributions to either field (academia, engineering) are going to be attracted to it no matter what (they have always been throughout history). What "celebrity" attracts is, well, those who are attracted to "celebrity/star status" to the detriment of everybody else. In a way, we have seen the same phenomenon happening in undergraduate admissions. For many years "reputation conscious" applicants have targeted universities based on reputations/ranking alone not which school is better for them. As a result, you get a lot of burnout with many seniors graduating from top schools picking "good paying", but uninteresting, careers instead of going to graduate school to apply their talents to scholarship.

Now, this is not to say that those working on science or engineering should resign themselves to be poor. On the contrary, I am a big advocate of paying scholars well so the choice is not between poor and middle class but between upper middle class and rich, which will cause a lot of people, the ones that matter anyway, to pick the "upper middle class" over wealth and continue to work as scholars or engineers.

The problem with star/celebrity driven academia is that the stars get a disproportionate share of resources. Just as the skyrocketing of the ratio CEO salary to average salary has only managed to alienate the workforce http://blogs.wsj.com/atwork/2013/06/11/the-state-of-the-american-workplace-is-meh/ , the same is likely to occur in academia long term. Star driven cultures, by definition, concentrate resources on a few stars (otherwise they would not be stars) causing the non stars to get the message that while they are necessary to maintain the discipline, there will be no reward for them.

In Silicon Valley, smart companies have a philosophy of sharing rewards among the rank and file as well, not only the executives. Those companies who focus the rewards on those at the top, do not end up doing very well in the long run although their executives all get wonderful golden parachutes.

46. **Ethan**
August 13, 2014

Typo

"Those companies who focus the rewards on those at the top"

should say

"Those companies WHICH focus the rewards on those at the top"

47. **David Roberts**
    August 13, 2014

    @Ethan:

    Or "Those companies THAT focus the rewards at that top" 🙂

48. **Ethan**
    August 13, 2014

    Forgot to comment (there is no edit button),

    "In many Asian cultures, the results of Science Olympiads are followed with the same enthusiasm that Europeans show for soccer."

    But then these countries do not produce top notch scientific results, precisely because the top people these countries produce are more interested in continuing being "stars" than in contributing to science at large.

    What sets American higher education apart from what I hear from Asian countries (China and India in particular) is the emphasis on ingenuity and original contributions combined, and this is important, with being very open regardless of people's background and age.

    In the US it is not unusual to hear people who did badly in high school, started in a community college and went on to become leaders of their respected fields (to the surprise of the "academic stars"). The more academia becomes "star driven", the more its doors will be closed to those with unusual backgrounds.

    Touching on the topic that gives this blog its name, I think that a very convincing case can be made that certain academic "stars" who shall not be named are probably responsible for the stagnation of physics nowadays. There are too many "stars" interested in keeping string theory alive to the detriment of alternative ways to get to a theory of everything.

49. **Ethan**
    August 13, 2014

    David,

    Oh well, the point remains, THAT I have elaborated in my other comment 🙂 .

50. **Harry**
    August 16, 2014

    Hi Peter,
    the link to T. Tao's blog post is broken,
    Best,
    Harry

51. **Peter Woit**
    August 16, 2014

    Harry,
    Thanks. Fixed.

52. **Shankar**
    August 18, 2014

    Bourbaki says – Fields Medal these days is a function of whose is in the Prize committee.
    'These days'?!

53. **shaghayegh maruphi**
    August 18, 2014

    I myself am an Iranian and I'm very proud that the first female winner of this prestigious medal is Iranian…..I hope my generation can pursue her work and win more medals….

    Just for mathematics 🙂

Smashing Physics

August 14, 2014
Categories: Book Reviews

I recently finally found a copy of Jon Butterworth's Smashing Physics, which came out in the UK a few months ago, but still hasn't made it to the US. As far as I know it's the first book about the Higgs written by someone actually involved in the LHC experiments. While there are several books already out there that do a good job of explaining the Higgs story (see for instance here) this one is a great read, giving a lively picture of what it was like to be involved in one of the experiments that made the discovery.

The book is structured as a bit of a travelogue, traveling through space to CERN and to various conferences, through time as the ATLAS LHC experiment unfolded, with physics explanations interspersed. A reviewer at New Scientist didn't like this, but I think the personal and idiosyncratic approach works well. We're given here not the highly processed take of a professional science writer, but a picture of what this sort of professional life is actually like for one specific scientist, from what happens in collaboration meetings, to an overnight spree on the Reeperbahn.

The perspective is definitely British (a lot of drinking goes on, with a scornful observation of American couples at a French bistro "drinking water"), and includes a fair amount of material about recent science funding problems in Britain. Butterworth's comments are often to the point, if sometimes impolitic. For instance, about the "God Particle" business, there's a footnote:

> Yes, I know Lederman claims he wanted to call it *The Goddamn Particle* and blames his publishers for the change. But my publishers wanted to call this book something really silly, and I managed to stop them.

For readers who know nothing about the physics involved, this book may not be the right place to start, with the well-known scientific story not getting a detailed treatment, and little in the way of graphics besides some Feynman diagrams. On the other hand, if you've read one of the other books about the Higgs, Butterworth takes you a lot deeper into the subject of LHC physics, including some extensive material on his work on boosted objects and jet substructure, which may lead to important results in future LHC analyses. If you like your science non-abstract and human, this is a great place to learn about the Higgs discovery story.

There's a quite positive review in the Guardian by Graham Farmelo, which describes the book well. That review though contains (like another review and like his wonderful book on Dirac) some odd material about string theory, in this case a long paragraph defending the theory, and telling us that "he and his fellow sceptics will be proved wrong in the long term." Actually there's very little about string theory in the book other than some sensible comments about being more interested in things experimentally testable. Like Tom Siegfried, it seems some science journalists are likely to always be unwilling to admit that they were sold goods that didn't turn out to work as advertised, and uncomprehending that most physicists, like Butterworth, never were buyers.

I gather the book may appear here in the US early next year, hope it gets some attention then.

Comments

1. **Marc C**
   August 14, 2014

   The book is already available on Audible. The narrator is English and it makes the stories and day-to-day living stuff a lot of fun. The technical stuff is hit or miss as a good portion of it takes a bit more contemplation (rereading, connecting it in your head). Still, I would recommend it. . . Just be ready to buy the book too so you can 'get' the details.
   The modeling and how that relates to the detector info was really insightful.
   Thanks for te review and the site Peter
   -Marc

2. **srp**
   August 14, 2014

   I just reread The Double Helix for the first time in a while, and Watson's personal point of view certainly enhanced that book. If you like good-natured (if slightly dated) international stereotyping between Brits, Americans, and even the French, it's good for that, too–but I think Watson had no trouble drinking along with his counterparts…

3. **Dom**
   August 15, 2014

   I first saw Butterworth in this series of films "Hunting the Higgs" where he comes over as a proper bloke. Hunting The Higgs

   I liked his statement in the Guardian recently
   "I didn't believe in the Higgs boson when I started work on the LHC, (as I describe near the start of Smashing Physics)" Guardian Link

4. **Timothy**
   August 15, 2014

   You can always buy the book used on Amazon from the UK, if you don't want to wait for its US debut. They also have "new" options on Amazon.

5. **Curious Mayhem**
   August 19, 2014

   Isn't it great that many — nay, most — physicists were not "buyers" of string theory? That's that wedge that string theory has driven between certain areas of theoretical physics and the rest of science. When you enter Stringworld, you suspend ordinary scientific standards.

   And I'm glad an experimentalist didn't believe in the Higgs boson before he started at LHC. Theorists believed in the Higgs boson, or something very like it, for very good reasons. But proper experimentalists try to see it for themselves, whatever the theoretical rationale. They don't sit at the edge of convoluted cultish rationalizations and think, "Well, I'm just too dumb to understand this."

Quantum Theory is Representation Theory

August 16, 2014
Categories: Favorite Old Posts, Quantum Mechanics

For a first slogan (see here for slogan zero) I've chosen:

> Quantum theory is representation theory.

One aspect of what I'm referring to is explained in detail in chapter 14 of these notes. Whenever you have a classical phase space (symplectic manifold to mathematicians), functions on the phase space give an infinite dimensional Lie algebra, with Poisson bracket the Lie bracket. Dirac's basic insight about quantization ("Poisson bracket goes to commutators") was just that a quantum theory is supposed to be a unitary representation of this Lie algebra.

For a general symplectic manifold, how to produce such a representation is a complicated story (see the theory of "geometric quantization"). For a finite-dimensional linear phase space, the story is given in detail in the notes: it turns out that there's only one interesting irreducible representation (Stone-von Neumann theorem), it's determined by how you quantize linear functions, and you can't extend it to functions beyond quadratic ones (Groenewold-van Hove no-go theorem). This is the basic story of canonical quantization.

For the infinite-dimensional linear phase spaces of quantum field theory, Stone-von Neumann is no longer true, and the fact that knowing the operator commutation relations no longer determines the state space is one source of the much greater complexity of QFT.

Something that isn't covered in the notes is how to go the other way: given a unitary representation, how do you get a symplectic manifold? This is part of the still somewhat mysterious "orbit method" story, which associates co-adjoint orbits to representations. The center of the universal enveloping algebra (the Casimir operators) acts as specific scalars on an irreducible representation. Going from the universal enveloping algebra to the polynomial algebra on the Lie algebra, fixing these scalars fixes the orbit.

Note that slogan one is somewhat in tension with slogan zero, since it claims that classical physics is basically about a Lie algebra (with quantum physics a representation of the Lie algebra). From the slogan zero point of view of classical physics as hard to understand emergent behavior from quantum physics, there seems no reason for the tight link between classical phase spaces and representations given by the orbit method.

For me, one aspect of the significance of this slogan is that it makes me suspicious of all attempts to derive quantum mechanics from some other supposedly more fundamental theory (see for instance here). In our modern understanding of mathematics, Lie groups and their representations are unifying, fundamental objects that occur throughout different parts of the subject. Absent some dramatic experimental evidence, the claim that quantum mechanics needs to be understood in terms of some very different objects and concepts seems to me plausible only if such concepts are as mathematically deep and powerful as Lie groups and their representations.

For more about this, wait for the next slogan, which I'll try and write about next week, when I'll be visiting the Bay area, partly on vacation, but partly to learn some more mathematics.

Comments

1. **Alex**
   August 16, 2014

   Peter,
   In your opinion, should physicists worry about the fact the Stone-von Neumann theorem is not true for infinite degrees of freedom, or is it purely a mathematical interest? I mean that one can always assume an arbitrarily high, finite number of degrees of freedom and get back a unique state space given the commutation relations.

2. **Matt Leifer**
   August 16, 2014

   "In our modern understanding of mathematics, Lie groups and their representations are unifying, fundamental objects that occur throughout different parts of the subject. Absent some dramatic experimental evidence, the claim that quantum mechanics needs to be understood in terms of some very different objects and concepts seems to me plausible only if such concepts are as mathematically deep and powerful as Lie groups and their representations."

   OK. but then why complex unitary representations? There are two possible answers I can think of to this.

   1. If you imagine that the basic structures of quantum theory are fixed in advance of considering symmetry tranformations, then they have to correspond to unitary representations.

   By the "basic structures", I mean that the observables are a von Neuman algebra, the "properties" are projection operators on a Hilbert space, or some other statement like that. Then, you can derive the state space structure from Gleason's theorem, and the structure of symmetry tansformations from Wigner's theorem. Specifically, once you have derived the probability rule from Gleason's theorem, you can show that the unitary transformations are the only automorphisms of the state space that preserve probabilities and are continuously connected to the identity.

   Presumably we want symmetry transformations to preserve probability, otherwise there would be observable violations of the symmetry, and we want continuous symmetries to be continuously connected to the identity. Discrete symmetries do correspond to antiuintary transformations of course, which is the other thing allowed by Wigner's theorem when you drop the continuity requirement.

   2. If you want to say that the basic structures of quantum theory themselves come from symmetry representations, then you haven't got a state space or probability rule fixed in advance. Presumably then you could investigate whether viable physical theories can be generated by varying the kind of group representation. You would then have to investigate whether a reasonable state space and probability rule could be defined for such structures.

   Now, if you go this route, then we already know that ordinary Hilbert space QM is not the end of the story. We can construct viable theories on Jordan algebras for example, but these seem not to be instantiated in nature. It seems to me that if you want to go the "everything is representation theory" route then you have quite a job to do explaining why such things are ruled out.

   Now, personally, I take the first point of view. This means that, for me, the ubiquity of unitary representations in quantum theory is not something particularly deep. That's just the only way it could possibly be given the basic structures of the theory. It's really the only possible way that symmetry transformations could be implemented.

   This means that, for me, "quantum theory is representation theory" is wrong. I might agree that "quantum physics is representation theory" because the fact that modern physics is based on symmetry is something that I regard as deep, so representation theory is the way to embed the physics, i.e. the physical interpretation of the operators, into an otherwise abstract probabilistic theory. But, the abstract probabilistic theory comes first, and needs its own justification independent of symmetry considerations.

3. **Peter Woit**
   August 16, 2014

   Alex,
   Yes, you can approach any QFT problem by cutting off the number of degrees of freedom, and then studying what happens as you make that number larger and larger. What makes things tricky (and interesting…) is that the limit is generally going to be singular, with the limiting theory having different properties. This is why renormalization theory is very non-trivial. Also, in condensed matter systems you get phenomena that only occur in the limit. I guess this is one example of "emergence".

   If you can bypass this, and directly mathematically formulate the limiting theory, that could be very powerful. The failure of Stone-von Neumann tells you that certain things that make life much simpler are no longer going to be true in the limiting theory.

4. **Peter Woit**
   August 16, 2014

   Matt,
   In representation theory in mathematics in general, one mostly works with complex, unitary representations. One could instead work with quaternionic or real representations, but this is most easily done by starting with a complex representation and asking for some extra structure. For many groups (e.g. finite or compact Lie groups), all representations are unitary (or, more precisely, unitarizable). Even for non-compact groups and infinite-dimensional irreducible representations, it's typically the unitary representations that are interesting.

   But non-unitary reps do occur in physics, for example the spinor rep of SL(2,C), and the state space of "ghosts" used in dealing with gauge symmetry. So, I'm just not convinced of the primary role of the probabilistic interpretation. Of course, sure, when you look at those parts of the theory where a probabilistic interpretation is going to connect the theory to experimental predictions, in those parts of the theory you'll have to have unitarity.

   In some sense the point of the slogan is to argue that there's more structure in fundamental quantum systems than just a von Neumann algebra. One is typically dealing with a universal enveloping algebra of a Lie algebra (not e.g. a Jordan algebra). The operators that generate the algebra of observables are given by the representation operators for a basis of a Lie algebra. From the point of view of the conventional formalism, it's very surprising that the Q,P operators that generate the operator algebra come from a Lie algebra and a group representation.

   Of course, this slogan has nothing to say about why certain Lie algebras occur, that remains a mystery.

5. **verissimo**
   August 16, 2014

   typo: univeral enveloping algebra

6. **Igor Khavkine**
   August 16, 2014

   Hi, Peter. Have you ever taken a look at these papers:

   > Bayen, F., Flato, M., Fronsdal, C., Lichnerowicz, A. & Sternheimer, D. Deformation theory and quantization. I. Deformations of symplectic structures. Annals of Physics **111**, 61-110 (1978),
   >
   > Bayen, F., Flato, M., Fronsdal, C., Lichnerowicz, A. & Sternheimer, D. Deformation theory and quantization. II. Physical applications. Annals of Physics **111**, 111-151 (1978),

   which were seminal in launching the idea of *quantization as deformation*, with representation theory playing a secondary role?

   The thing is that a classical Poisson algebra is more than just a Lie algebra, it also has a commutative product and a Leibniz rule for the bracket with respect to the product. Quantizing by simply representing the Lie algebra part of the structure can easily "break" the product structure and cause lots of problems, like Groenewold-type no-go results, the need to choose polarizations on the phase space, the need to choose the appropriate Lie sub-algebra to represent, deciding which of these choices lead to equivalent or inequivalent quantizations, deciding what constitutes a classical limit, etc.

   All of these issues are dealt with head on in deformation quantization. Moreover, once a classical Poisson algebra is deformed to a quantum non-commutative algebra, the non-commutative product defines the commutator bracket, allowing Lie algebras of symmetries to be represented in it. GNS type constructions give you representations of the quantum algebra on Hilbert spaces. Thus, unitary representations of Lie algebras of symmetries still appear.

   Of course, deformation quantization is not a construction or a prescription, rather it's a definition. On the other hand, the same can be said about quantization as Lie algebra representation. However, once given a definition, one can go and find constructive methods that satisfy it. This has actually been done with great success for deformation quantization.

7. **Peter Woit**
   August 16, 2014

   verissimo,
   Thanks, fixed.

   Igor,
   Thanks for the explanation, I've never completely understood how deformation quantization works. I gather the idea is to get a notion of quantization that gives something for any symplectic manifold. That's very interesting mathematically, but kind of an opposite direction to what I'm most interested in, understanding how quantization works in the very specific cases that occur in fundamental physics.

8. **Thomas Larsson**
   August 17, 2014

   Experience with CFT makes one expect that the infinite-dimensional groups of gauge transformations and diffeomorphisms should acquire an extension after quantization.

   The group of gauge transformations in 3D has two types of extensions: the central extension [Etingof-Frenkel 1994] and the Mickelsson-Faddeev extension [Pressley-Segal section 4.10]. Only the latter corresponds to gauge anomalies in QFT.

   The MF group is a "bad" extension in the sense that it lacks natural unitary representations [Pickrell 1989]. IMO this explains why gauge anomalies must cancel in the

SM: nature abhors groups without good unitary representations.

The central extension can hence not arise in QFT except in 1D. To realize it, a more general theory is necessary.

The situation is analogous for diffeomorphisms. There are no diff anomalies at all in QFT in 3+1D, but the diffeomorphism algebra has Virasoro-like extensions in every dimension. Hence these can only be realized in a more general theory than QFT.

When the diffeomorphism algebra acquires an extension, it ceases to be a gauge symmetry, and local observables become possible in QG.

And look, I managed to say that without any self-reference at all.

9. **Igor Khavkine**
August 17, 2014

Peter,
Those original papers are actually quite good in providing motivation for the deformation quantization (DQ) point of view even for the specific cases I think you have in mind (free particle, oscillator, hydrogen atom). Personally, I like to view specific cases as instances of something more general, and DQ does just that.

Here's an example of how it helps. In your own notes (Ch.14), you say that going beyond quadratic polynomials, the assignment of operators to classical observables is highly ambiguous/non-unique, referring to the well-known *operator ordering ambiguity*. So, just how much of this ambiguity is there, is any of it redundant (gives physically equivalent quantizations)? Turns out that this question is well posed in DQ and has even been answered. All inequivalent classes of quantizations are parametrized by the 2nd de Rham cohomology of the phase space. In particular, standard R^2n has a unique quantization. Note that this result is independent on the Stone-von Neumann theorem (no need to exponentiate observables), and even says something stronger, because it looks not only at the quantum algebra itself but also the specific choice of assignments of quantum operators to classical observables. I have not seen this question well formulated, let alone answered, in other points of view on quantization.

So, to answer the question of how DQ works, one could say that it's main benefit is to provide a good set of physically motivated definitions, sufficiently precise yet flexible for meaningfully answering questions such the one above.

10. **Jeff M**
August 17, 2014

Peter, sorry to be off topic, but have you seen Gordon Kane's letter to SciAm? Perhaps you might be willing to bet him that super partners won't be found in the next CERN run? He seems to be having trouble finding people willing to bet against it 🙂

11. **Reader297**
August 18, 2014

Peter,

I know this is a bit of a philosophical, unfalsifiable discussion, but I think I'm on Matt Leifer's side here.

It's not enough to say simply that quantum theory is representation theory. You also have to say that information is encoded in density matrices, or, at the very least, that you've got a theory of probabilities here, and then appeal to Gleason's theorem to argue that the probability measure must be encoded in density matrices.

The postulate of probabilities is a nontrivial additional postulate you have to make, and once you've made it, and have density matrices in hand to encode it, the necessity of unitary (or anti-unitary) transformations becomes inevitable from the Wigner representation theorem, or, more intuitively, to preserve information under symmetry transformations.

You say that "non-unitary reps do occur in physics, for example the spinor rep of SL(2,C), and the state space of "ghosts" used in dealing with gauge symmetry." I don't think I follow. On the physical Hilbert space, all representations must be unitary or anti-unitary. Ghosts are not physical states—they're vectors states put in to make the given system's Hilbert space more symmetric-looking and easier to work with. And non-unitary representations directly on fields are not fundamental features of quantum theory, but again are mere conveniences.

12. **Peter Woit**
August 18, 2014

Reader297,

After spending more time with Schlosshauer's book on decoherence while on a long plane trip, I'm more than ever suspicious that probability is not fundamental to QM, but just what you need to do when you break a quantum system up into two subsystems (e.g. a system under study and the apparatus/enviroment). But also spending time with his book "Elegance and Enigma" of interviews shows that opinions differ. Of the people in that book, I find myself most sympathetic with what Zurek has to say.

Given any structure, you can start with one end of it, and if it's rigid enough, get to the others. Besides not seeing the necessity or desirability of starting with probability, I also don't see how it gets you to any kind of explanation of the basic structures of QM: why are observables self-adjoint operators on a complex vector space? Why the Heisenberg commutation relations? Why Poisson bracket goes to commutator? Starting by taking as fundamental the mathematics that explains those structures seems to me more promising than starting with probability. I also suspect it is a more promising approach to QFT and the things we don't understand about unification than starting with probability and trying to add other axioms with "physical" motivation.

Of course you get unitary representations in those cases where your state space is going to have an interpretation that will require unitarity. The point of the comment was just that the fundamental mathematical structure involves lots of use of representation theory, including non-unitary representations. One person's "convenience" is another's deep and powerful insight…

13. **Peter Woit**
August 18, 2014

Jeff M,
Just saw that. Seems implausible that anyone would have trouble finding physicists who think odds are now no SUSY at the LHC. In any case, it should get interesting starting a year or so from now when results at higher energy start coming in. But, yet more discussion of this should wait for a more appropriate topic…

14. **Florin Moldoveanu**
August 19, 2014

SL(2,C) is another possible number system for QM besides reals, complex, and quaternionic numbers. SL(2,C) arises out of a subalgebra of SO(2,4) ~ SU(2,2) by eliminating the "ghosts" and it provides another decomposition of the d'Alembertian. In fact QM over SL(2,C) is mathematically equivalent with spinors and Dirac's equation. This gives rise not to a C* algebra but to a C*-Hilbert module. There is no QM over octonions because it can be proven that QM obeys a "dynamic correspondence" between observables and generators which rules out the only special Jordan algebra. This "dynamic correspondence" is related to Noether's theorem.

Deformation quantization is a well established mature domain which builds up recursively in powers of hbar an associative start product. There are inequivalent ways of building the star product (related to 2nd deRham cohomology) and this makes the area hard (the symmetric part of the star product forms a Jordan algebra, while the skew-symmetric part generates a Lie algebra). The typical examples are the Weyl quantization and Berezin quatization. Weyl quantization preserves symmetries but has convergence problems outside of flat R2n while Berezin preserves positivity. Berezin is best understood in terms of creation and annihilation operators which form z and zbar in complex numbers. Deformation quatization maps QM in phase space to QM in Hilbert space and is a transition in a larger sense from non-commutative to commutative geometry. All Kahler manifolds admit a Berezin quantization if positivity is present, but if one starts from a pure classical system, one uses geometric quantization which requires the Born-Sommerfeld quantization condition as a pre-requisite.

The topic of Poisson manifolds is an active research area due to symplectic reduction. Some classical systems admit both an odd-dimensional Poisson manifold description and an even-dimensional symplectic manifold description. Even better, some systems are bi-Hamiltonian (solitonic systems).

15. **manfred Requardt**
August 19, 2014

Well, in my view the structure of quantum theory is a low energy phenomenon (a little bit jokingly: perhaps only a small deformation of classical mechanics) whereas various modern frameworks seem to believe that it holds unaltered down to the Planck scale. But that is against our scientific experience. I would bet that quantum theory is emergent (perhaps even an epiphenomenon). There is a lot of deep mathematics around which may live on a microscopic scale with quantum theory emerging as some coarse-grained effective theory.

16. **Hendrik**
August 19, 2014

Dear Peter,

Three comments;-
1) On the Stone – Von Neumann Theorem.
This concerns only integrable representations, not any representation of the Heisenberg Lie algebra. This is physically justified as one can only perform finite measurements, not infinitesimal ones.
2) On the Groenewold – Van Hove theorem.
This also requires some integrability condition for some basic set of observables, see http://xxx.lanl.gov/abs/math-ph/9809015. Also, it has been generalized to other symplectic manifolds, see http://xxx.lanl.gov/abs/math-ph/9809011 or http://xxx.lanl.gov/abs/dg-ga/9605001
3) On the metaplectic algebra being a maximally quantizable Poisson subalgebra of the full Poisson algebra on R^{2n}. This is true, but it is not the only maximally quantizable Poisson algebra containing the q_i and p_i, see Sect. 5 of the first reference above.

17. **Reader297**
August 19, 2014

Hi Peter,

Let me address your points in turn.

You write:

"After spending more time with Schlosshauer's book on decoherence while on a long plane trip, I'm more than ever suspicious that probability is not fundamental to QM, but just what you need to do when you break a quantum system up into two subsystems (e.g. a system under study and the apparatus/enviro[n]ment)."

Suspicion is a wonderful and insufficiently appreciated sensibility. However, after decades on this idea, all we have are still only suspicions.

There have long been hopes of "deriving" probability ab initio from the bare mathematical formalism of quantum theory. Any such derivation, in particular, would represent a fundamental solution to the measurement problem without any a priori assumption of, say, the Born rule.

Advocates of the many-worlds interpretation have long argued that this goal is achievable, and much of their conviction derives from Zurek's envariance and decoherence arguments. (These arguments have led to the idea that "decoherence alone solves the measurement problem.")

However, no rigorous proof exists, and for good reason: Probability is more than just mathematics. The problem of understanding what probability actually means is a deep one in philosophy. Getting to probability from non-probability is hard a problem.

On the other hand, once you assume a priori something probability-like — namely, that we should be talking about a probability measure for vectors in a Hilbert space — then you can follow Zurek or Gleason (or even Hugh Everett, if you read his original papers) to show that the probability formula has to be the Born rule.

But simply starting from abstract vectors and somehow deriving the existence of probability from scratch has long been regarded as an impossible problem. On the other hand, if you've found a way to solve it, I'm sure we'd all love to see it!

You write: "Given any structure, you can start with one end of it, and if it's rigid enough, get to the others."

That's quite true. But not all ends are necessarily on the same footing. If one end is more general than the other ends, then the more general end is the safer starting place.

As I've said, starting without probability and getting to probability runs into some long-known and deep metaphysical obstructions. But if we do start with probability, then going the other way is easier and more general. If we start with the axiom that probability is encoded in density matrices through the Born rule Tr[(density matrix)(…)], then the necessity of unitary (or anti-unitary) representations for symmetries is a mathematical consequence. When decoherence diagonalizes the density matrix in a basis approximating coherent states, we get a structure that closely resembles classical phase space, and the commutator behaves algebraically similarly to Poisson brackets without having to assume any classical system a priori. (And that's important, because quantum theory appears to be more fundamental than classical physics. We shouldn't be starting with classical systems in the first place.)

You write: "Besides not seeing the necessity or desirability of starting with probability, I also don't see how it gets you to any kind of explanation of the basic structures of QM: why are observables self-adjoint operators on a complex vector space?"

Once you say that probabilities are encoded in density matrices through the Born rule, then reality of the probabilities implies that all observables must be self-adjoint.

That obviously still leaves the deeper question of why we have to work with complex numbers. Of course, that's a question whether one starts with probabilities-in-density-matrices or one starts with the quantum-theory-is-representation theory. We're not going to be able to "derive" quantum theory starting from just one axiom!

But there are definitely some simple cases that motivate the idea of working with complex numbers. If we consider, say, a two-state system (which, by the way, doesn't have Heisenberg commutation relations [q,p] = ih), then even if we write down a real density matrix, there are observables we can write down that always give real probabilities. That is, if we consider the most general matrices that combine with density matrices in the Born rule to give real probabilities, then those are self-adjoint complex matrices.

Moreover, as you know, there are certain symmetry transformations you can do that are generated by self-adjoint operators.

So the motivation is that it's a matter of generality. One could self-consistently consider just real-valued quantum theory (whether in the probabilities-in-density-matrices or the quantum-theory-as-representation-theory approaches), but it's not the most general structure that is consistent with real probabilities. At the end of the day, however, someone has to postulate that complex numbers are a part of the story, whichever point of view one takes.

After all, even if we declare that quantum theory is just representation theory, we could always imagine a finite-dimensional real vector space acted on by real orthogonal matrices. That's certainly not the most general thing we can do, but there's no logical reason why we have to consider complex numbers unless we postulate that we must do so.

Finally, you write: "Of course you get unitary representations in those cases where your state space is going to have an interpretation that will require unitarity. The point of the comment was just that the fundamental mathematical structure involves lots of use of representation theory, including non-unitary representations. One person's "convenience" is another's deep and powerful insight…"

That's true. But I suppose the issue here is that some things can be regarded as conveniences, whereas other things cannot be. And to a lot of folks, anything that cannot be regarded as a convenience is the deeper and more powerful insight.

18. **Zathras**
August 19, 2014

PW: "After spending more time with Schlosshauer's book on decoherence while on a long plane trip, I'm more than ever suspicious that probability is not fundamental to QM, but just what you need to do when you break a quantum system up into two subsystems (e.g. a system under study and the apparatus/enviroment). "

I don't get this distinction at all. Any observation requires (is?) a breakup of the quantum system. Discussing quantum mechanics without this means you are not doing anything empirical. I thought we were against that here.

19. **Peter Woit**
August 19, 2014

Hendrik,
Thanks. In comments here I'm often not even trying to be precise, am trying to do so in the notes I am writing, so that's helpful.

Reader297,
I wouldn't claim to get probability from nothing, but it seems to me to come into play only at the point when you try to deal with the difficult issue of how to extract predictions about experimental results, formulated in terms of idealized descriptions in classical terms of your experimental set-up. This just seems to me at the other end of things from the question of what the fundamental theory is, but to some extent that's a matter of perspective.

Experts I have been reading seem to agree that you need something beyond just decoherence to get the Born rule, but it's unclear to me what it is. Zurek claims to derive the Born rule, presumably making some kind of assumption about "interpretation", but also at other points claims to be agnostic about choice of interpretation. I'm having trouble putting my finger on where "probability" enters, and your comment referring to a deep problem in philosophy of course tempts me to see the question as one that might be on the other side of the nebulous philosophy science divide from what I feel I have to be concerned about.

By the way, for decoherence experts, I'm curious whether when you "derive" the Born rule you are making some assumptions that could be in principle violated. Is there even a thought experiment in which you would see something that could be interpreted as a violation of the Born rule (or even thought experiments where there is ambiguity about what the Born rule means or says)?

I don't think you answered my question about the Heisenberg commutation relations. Yes, for the two-state system, all you have is linear algebra of 2 by 2 matrices, so there's likely only one way to do things. For canonical quantization though (which is the basis of our understanding not just of single-particle QM, but of the quantization of linear fields), how does one explain where the Heisenberg relations come from, other than by invoking a Lie algebra representation (and one that is not a "symmetry") of the theory?

Zathras,
First of all, you're making the mistake many people make of thinking my complaints about string theory/SUSY etc are just based on lack of connection to experiment. That's just not the case, if they were mathematically compelling I'd be a fan. In the case of QM my main interest is not in the hard problems of its connection to experiment (everyone seems to agree there is no such thing as an experiment in contradiction with the theory, and such a thing seems unlikely). Physical theories are also mathematical objects, with deep connections to mathematical issues, and those are what I'm often most interested in (with one justification the lack of relevant experimental clues about how to improve the theory).

20. **Igor Khavkine**
August 19, 2014

It has always struck me that people seem to have much less trouble accepting how probability enters in classical mechanics. If you forget about phase space, then all you have is the Poisson algebra of observables. Probability distributions, aka *states*, are then (positive, normalized) linear functionals on this algebra, physically interpreted as "expectation values". A measure that is concentrated on a single point of the would-be phase space describes a given deterministic configuration of the system. Purely algebraically, such a state is an extremal (a boundary point) of the convex space of all allowed states and is called *pure*. Thus, a statistical classical theory completely supersedes deterministic classical theory. For practical purposes, all our models are really statistical, and pure state are only idealizations that could be achieved on special subsystems of the universe and only under very special circumstances (these are the preparation protocols for controlled experiments).

Now, turning to quantum mechanics, the whole preceding paragraph can be repeated word for word and carry the same exact meaning, with the exception that the algebra of observables is no longer Poisson, but is rather non-commutative. This changes also the condition of positivity. Pure states are still extremal points of the convex set of all allowed states, and are still only idealizations. Note that wave functions, Hilbert spaces or the Born rule need not be brought in at this fundamental level of description of quantum mechanics, yet one has enough machinery to make some calculations and associate them with experimental outcomes.

At this point, pure mathematics tells us that given a positive, normalized state on a possibly non-commutative algebra with conjugation (such as the quantum algebra of observables) always gives rise to a unitary representation of that algebra on a Hilbert space (this is the GNS theorem), such that application of the state is equal to the

(Hilbert space)-trace of this representation against some density matrix. Further, if the state is pure, then the representation is irreducible and the corresponding density matrix has rank one. It is in this way that pure states are associated with vectors in a Hilbert space (by factoring the corresponding rank-one density matrix) and it is in this way how the Born rule arises. Namely, if one associates pure states with vectors in a Hilbert space, as described above, then we already know that we need to `square' that vector to get a density matrix, which is then paired with observables inside a trace to get expectation values: the essence of the Born rule.

Granted, there was a notion of probability put into this discussion at the very beginning. So it does not address the desires of those who would like to see probabilities emerge from the formalism itself. However, given that probabilities are introduced in the same way as in classical theory, which I see very few people complain about, the Born rule follows automatically. To all those who are concerned by these issues, is this argument not already satisfactory to justify the Born rule?

21. **Reader297**
August 19, 2014

Hi Peter,

I like your response a lot. Let me address your comments in turn.

You say: "I wouldn't claim to get probability from nothing, but it seems to me to come into play only at the point when you try to deal with the difficult issue of how to extract predictions about experimental results, formulated in terms of idealized descriptions in classical terms of your experimental set-up. This just seems to me at the other end of things from the question of what the fundamental theory is, but to some extent that's a matter of perspective."

I happen to agree with you. I think a lot hinges on one's perspective.

That is, I think it all depends on whether one regards quantum theory from a purely instrumentalist standpoint, namely, as nothing more than a tool for extracting the probabilities for measurement outcomes obtained by abstract observers "outside" of quantum theory. In that case, I agree with you that we can put those experimental questions aside for the moment and just talk about the internal, bare (and sublime) mathematical theory absent any questions about observers, measurements, or probabilities. (However, then we don't have the complete quantum theory, because those probabilities ultimately need to get in there somehow!)

But there are other perspectives in which one tries to put observers into quantum theory as physical quantum systems in their own right, and then one runs directly into what it means to be an observer who is a quantum system in the Hilbert space. What does it mean to say that such an observer experiences probabilities, and why? How do bare mathematical state vectors turn into probabilities? Presumably, a quantum observer living in the Hilbert space has some state of being or experience — or maybe not! And is there some probability for an observer to be in a certain state of being? What does that even mean?

If one takes this more realist/physicalist perspective as I have just described it, rather than instead taking the instrumentalist perspective I mentioned above, then one cannot put the probability questions aside, because they are somehow entirely contained inside the bare mathematical theory at a very deep level.

You also write: "I'm having trouble putting my finger on where "probability" enters…" Me too. So far nobody has offered more than a hand-wavy "insert magic here" argument for deriving where probability exactly enters, which is why other people just bite the bullet and postulate probabilities as a fundamental axiom.

But, again, even if one takes an instrumentalist/bare-mathematical/representation-theory definition of quantum theory, one still has to deal with this step of getting probability into the theory somehow — axiomatically, I'd argue. And that means quantum theory is more than just representation theory. It's also a theory of probability, and that's something more than just representation theory per se. You need another axiom at least.

One can choose to ignore temporarily this additional probability axiom, of course, and focus on the mathematical of representation theory, but then one wouldn't be doing actual quantum theory, but an (again sublime) mathematical shadow of quantum theory.

Later, you write: "I don't think you answered my question about the Heisenberg commutation relations."

Woops! I apologize! Let me try to do it in the context of your next several comments.

You write: "Yes, for the two-state system, all you have is linear algebra of 2 by 2 matrices, so there's likely only one way to do things."

Right. There's no classical counterpart to this system, and that's part of the reason why I've argued that one shouldn't take classical systems as a fundamental starting point for quantum systems, nor regard canonical quantization as a physical thing, but merely a helpful mnemonic. Quantum systems appear to be more fundamental than classical descriptions, and so the arrow of reasoning goes from quantum to classical (in the appropriate decoherent/macroscopic/semiclassical limit).

Next, you write: "For canonical quantization though (which is the basis of our understanding not just of single-particle QM, but of the quantization of linear fields), how does one explain where the Heisenberg relations come from, other than by invoking a Lie algebra representation (and one that is not a "symmetry") of the theory?"

Here I think I would cite Steven Weinberg, who addresses precisely this question in the introduction to his new textbook on quantum theory. ("Lectures on Quantum Mechanics.") I think we can probably all agree that the guy who coined the term "Standard Model" knows a thing or two about quantum field theories (fallacy of appeal to authority—I'm guilty as charged!), and he has long believed (and vehemently argued) that the canonical-quantization procedure is nothing more than a convenient mnemonic that works at a practical level in certain situations, but is actually quite misleading especially in more general circumstances.

Instead, as I think you would enormously appreciate, Weinberg has argued that quantum systems should be defined by imposing symmetries on their Hilbert spaces. In particular, he argues in his QFT books that symmetries must be represented unitarily (or anti-unitarilty for certain discrete symmetries), and when you consider the unitary action of the Poincare group on states of a Hilbert space, the generators can be shown to satisfy the Poincare algebra (after much annoying work fixing phases, as Weinberg goes through in gory detail), and the P^0=H operator becomes what we identify as the system's Hamiltonian H. For a system whose H is local, Weinberg argues that the subspace spanned by low-energy eigenstates can always be approximated by a local QFT, and, as you no doubt know, he calls that idea effective field theory.

However, because the effective field theory is only to be regarded as an approximate description of the low-energy part of the full Hilbert space, the field operators are not fundamental entities, and the Heisenberg commutation relations satisfied by the field operators (at least in the bosonic case in which we have them at all) are not fundamental, let alone axiomatic.

In the non-relativistic single-particle limit, one can show that this Hamiltonian reduces to p^2/2m + V(x), where [x,p]=ih. In fact, one can even write down what x is in terms of K (the boost generators) and P (the momentum/translation generators). (Exercise!)

At no point does Weinberg invoke canonical quantization anywhere. And, indeed, there are quantum field theories that don't seem to have classical limits and/or Heisenberg-type commutation relations, such as fermionic field theories as well as the large classes of field theories that lack a Lagrangian description. But the Hilbert spaces don't care whether there's a nice classical limit.

I suppose one could argue that specifying the Poincare group is no better metaphysically speaking than specifying the Heisenberg commutation relations. We're still effectively postulating something to constrain and define our quantum system of interest, and the end result is a set of operators that satisfy a certain algebra.

But I think Weinberg's point is that this structure isn't a universal postulate or axiom of quantum theory, but just a matter of providing one way (among many) to define a particular quantum theory of interest. That is, it's just something we do out of convenience when we know a priori that a theory has to have certain symmetries. For other kinds of quantum systems, such as two-level systems, we don't have to take that approach. We can just postulate a Hamiltonian and go from there.

Igor Khavkine– Please see my earlier comments about instrumentalism.

In particular, the situation you describe for classical systems is what one would call classical instrumentalism. That is, we just regard the formalism as a recipe for computing expectation values and probabilities, and nothing more. In that sense, there is definitely a parallel to quantum instrumentalism, with some "simple" replacements. However, I think Peter would argue that it's not at all obvious why we replace classical observables with non-commuting quantum operators. And, again, I think most people would agree that once you say "probability," there's a unique prescription (the Born rule) for those probabilities.

But I'd like to focus on your prefatory statements: "It has always struck me that people seem to have much less trouble accepting how probability enters in classical mechanics. If you forget about phase space…"

I think your last clause is the key thing. In classical physics, you can certainly forget about phase space if you wish, but you don't have to. And that's important. You can always assume a phase space if you wish, in which case you're not doing instrumentalism anymore, but are assuming that there is a reality under those probability distributions. That provides people with a metaphysical security blanket.

In particular, in classical physics, even when you have a mixed states, you can always assume (again, if you wish) deterministic, definite states can be assumed to exist, even though they're not known with certainty.

But the weirdness of quantum theory is that this trick doesn't seem to be available anymore. What if we don't want to "forget about phase space" in the quantum case? Do we have the liberty to not forget it in quantum theory? Is there something there that we could choose not to forget, something analogous to classical phase space that we can choose to regard the Born rule as a probability of, apart from just the probability of a measurement outcome? If a system has a mixed state that isn't an extremal point of the convex set of allowed probability states, can we still assume that the system has a deterministic, definite state of being that's just not known with certainty, as we could choose to do in the classical case of a mixed state?

So when you write "However, given that probabilities are introduced in the same way as in classical theory…", I think that not everyone would agree that it's the same story in quantum theory. At the level of an instrumentalist perspective, perhaps yes, with my aforementioned provisos, but we have a much richer picture available to us in the classical case for understanding probabilities, and the absence of an obvious such picture in the quantum case makes quantum theory and quantum probabilities so much more mysterious and troubling.

22. **Peter Woit**
August 20, 2014

Reader297,
I really do disagree with Weinberg here. In general I'm no fan of the "your deep, beautiful mathematical structure is just an effective theory, a low energy approximation to some more fundamental theory, although I don't know what this is" argument. And in this particular case, the mathematics is very deep, and we don't have anything better. Part of the beauty of the story is that the fermionic case runs perfectly parallel, and a big part of what I wanted to write about in the QM notes is exactly this story. Among physicists there seems to be an attitude that these are just ad hoc rules of thumb, but I think an appreciation of the coherence of the mathematics gives strong evidence against this.

23. **manfred Requardt**
August 20, 2014

Dear Peter, well, there do exist quite a few approaches which try to derive for example quantum theory from a deeper level. See e.g. the work of 't Hooft or my recent arXiv:1205.1619 (using as a technical tool among other things the concept of cellular networks). I do not claim that these speculations are conclusive but we are convinced that something in this direction will ultimately work. After all, classical mechanics (symplectic geometry) is also beautiful from a mathematical point of view while quantum theory is a beautiful theory as well but is completely different.

24. **Thomas Larsson**
August 20, 2014

Manfred,

From the point of representation theory, irreps are the fundamental objects, and irreps behave like quantum theory (although not as quantum *field* theory except in low spacetime dimensions). So if the fundamenta of math and physics are the same, quantum theory is fundamental.

It was a while since I looked at 't Hooft's Planck-scale determinism, but iirc his motivation for looking at hidden variable theories was locality. Of this I approve, but it is possible to have locality in QG without abandoning quantum theory. Recall the standard argument why there are no local observables in QG:

1) An observable in quantum theory is an operator which commutes with all gauge transformations.
2) In GR all diffeomorphisms are gauge transformations.
3) Hence there can be no observables in QG that depend on spacetime coordinates.

The flaw in this argument is that it assumes that the gauge symmetries of classical and quantum gravity are the same. If the algebra of diffeomorphisms acquires an extension (to avoid self-referencing, see [the seminal paper of Rao and Moody 1994]), this is not true.

So does it ever happens that the classical and quantum theories have different gauge symmetries? Yes, one (or 25) such cases are described in [GSW 1988], section 2. The free string can be defined in any number of dimensions, and it has a conformal (or Weyl) symmetry that is a gauge symmetry. After quantization, we must distinguish between three cases:
1) The critical free string D = 26. There is no anomaly, the theory is consistent, and the conformal symmetry is gauge.
2) The supercritical free string. The theory is inconsistent, because there are ghosts (negative-norm states) in the physical spectrum, and we can forget about it.
3) The subcritical free string. The theory has a conformal gauge anomaly, but is nevertheless consistent because there are no ghosts in the physical spectrum. However, the conformal symmetry can no longer be gauge, due to the conformal anomaly.
So in the subcritical free string, the (both consistent) quantum and classical theory have different gauge symmetries. It does happen!

(And yes, I know that the subcritical interacting string is inconsistent, but that was not what I was talking about.)

25. **Reader297**
August 20, 2014

Hi Peter,

Those are fair points!

First, I wanted to give one more concrete example of where the Heisenberg commutation relations [x,p]=ih can come from without canonical quantization or an a priori classical system.

If you are given a QM system with an infinite basis that we can label with one continuous real label x, then we can define a unitary translation operator that changes |x> to |x+a> for any real a. The infinitesimal generator of that unitary translation operator is then just p, where it's then just a simple calculation to show that [x,p]=ih. From a Noether standpoint, we call the generator of translations the momentum by definition. So this is a self-contained way to introduce and define p and obtain [x,p]=ih without reference to an a priori classical system. What's lovely is that the eigenstates of z=x+ip then form a natural QM analogue to classical phase space, but have Gaussian wave functions with a size in x,p given by Planck's constant h, so one also finds phase-space quantization. So in the limit h->0, we get back a classical phase space.

Now to your more recent comments. You write: "I really do disagree with Weinberg here. In general I'm no fan of the "your deep, beautiful mathematical structure is just an effective theory, a low energy approximation to some more fundamental theory, although I don't know what this is" argument."

There's definitely room for philosophical disagreement here. But I think there are couple of things that need to be said about it.

One of Weinberg's motivations here comes from condensed-matter theory. In many of the kinds of systems they study, there exists a well-defined long-distance regime that looks just like a field theory (sometimes even a CFT), regardless of what the microscopic exact model actually is. You could have atoms on a lattice, you could have graphene, you could have a fluid, etc. — really qualitatively different microscopic models — and yet the long-distance description looks like a field theory.

So in this case, there isn't an "I don't know what this is." In lots of cases, we know what the microscopic model actually is. And the long-distance limit still looks like a field theory.

Now, what's really interesting about this phenomenon is that the mapping of microscopic models to field theories is many-to-one — many qualitatively different microscopic models can share the same long-distance field theory — and each equivalence class (all microscopic models that share the same long-distance field theory) is called a universality class.

The existence of universality classes is a double-edged sword. On the one hand, they imply that by studying a single field theory, you can make statements and derive properties and calculate things (all in the long-distance regime) that hold for a huge class of microscopic models. That's why it's called effective field theory — because it's really effective!

However, on the other hand, if you are simply handed the long-distance field theory, the many-to-one-ness of the mapping — that is, the large number of models in that universality class — implies that you'll never be able to guess the correct underlying microscopic model, unless of course you can do some sort of high-resolution experiment to see what's actually going on at short distances.

So we have a clear case study in which an elegant looking field theory really is just a long-distance limit of something that looks qualitatively different from a field theory — and, indeed, there's a huge class of qualitatively different microscopic models that look like the same field theory at long distances. This really does happen in physics!

The upshot is that when people like Weinberg look at the field theories of particle physics, such as the Standard Model, there is no longer the belief that people had back in, say, the 1940s that the field theory is necessarily fundamental, nor that the underlying microscopic model is itself a field theory.

This hunch is backed up by the fact — which Weinberg spends his field theory books explaining — that local, weakly-interacting, Poincare-invariant quantum systems canonically look at low energies/long distances like field theories. (Even GR works this way — see the myriad papers by John Donoghue on the arXiv.)

So the fact that you have a field theory at low energy doesn't tell you either way whether the microscopic model is a field theory or something truly qualitatively different.

One can certainly posit examples (like QCD or CFTs, or even the AdS/CFT correspondence) where the high-energy limit is still a field theory. But in a given practical case, that would be a huge assumption.

You write: "And in this particular case, the mathematics is very deep, and we don't have anything better. Part of the beauty of the story is that the fermionic case runs perfectly parallel… Among physicists there seems to be an attitude that these are just ad hoc rules of thumb, but I think an appreciation of the coherence of the mathematics gives strong evidence against this."

These lines seem suspiciously similar to what string theorists like to say!

And I think there's a good reason why.

At the end of the day, you are proposing that because nature looks like a field theory at long distances/low energies, it ought to look like a field theory at short distances/high energies, because field theories are somehow mathematically elegant and coherent. That's one proposal for what the microscopic models should be, and the string theorists propose a qualitatively different microscopic model that also looks like a field theory at long distances/low energies. The LQG people propose another. Etc.

What all these approaches have in common is (1) they are motivated in part by mathematical coherence and aesthetic appeal, (2) they look like field theories at long distances/low energies, as is inevitable (a la Weinbergian EFT reasoning) given that they are consistent with Poincare invariance, local interactions, and quantum theory, and (3) they are completely beyond current experimental confirmation.

Dare I say that — dum dum dum — they are ***not even wrong***?

26. **Igor Khavkine**
August 20, 2014

Reader297 wrote: "In classical physics, you can certainly forget about phase space if you wish, but you don't have to. And that's important."

Or is it? (Warning, rhetorical question!) This situation is, to me, a clear example of a case where there are two absolutely mathematically equivalent formulations of the same theory, though one is couched in terminology that leads to all sorts of philosophical difficulties (upon quantization, that is), while the other is not. What I will perhaps never understand is how some people, when faced with a choice between these mathematically equivalent formulations, consistently go for the one laden with philosophical difficulties. Unless, they are simply not aware of the alternative (though, in my experience, many of them are).

27. **Peter Woit**
August 20, 2014

Reader297,

I understand very well Weinberg's argument, just am not convinced. He also famously argued in the intro to his GR book that another kind of mathematics, geometry, was not needed for fundamental physics (he did this right before it became clear that gauge theory, a geometrical theory, was the way forward in particle theory). Yes, there too you can argue that geometry is just a low energy effective epiphenomenon if you want, but I don't think it's a convincing argument.

The argument against the "it's just a low energy approximation viewpoint" is the LHC data: qft and gauge theory work perfectly at the shortest distances we can test (which, by the way, is rather a different situation than string theory. I was trying to keep some of these postings free of tedious arguments about string theory, please help…)

28. **Peter Woit**
August 20, 2014

For those not aware of Weinberg's views on geometry, see
http://www.math.columbia.edu/~woit/wordpress/?p=529

I've deleted some comments trying to tediously argue about string theory. Please, unless you can seriously relate this to the topic of this posting and have something new to say, just stop.

29. **Reader297**
August 20, 2014

Hi Peter,

I'm agreed about avoiding string-theory discussions here, and keeping to the current topic.

You say "I understand very well Weinberg's argument, just am not convinced."

I think that's fair. And it represents a basic philosophical impasse: If one person says s/he simply isn't convinced, then that's that, at least until experimental data can tell us which point of view is the more accurate one.

But what everyone has to admit is that the opposite point of view is a legitimate one as well — namely, that field theory won't turn out to be fundamental — again until experiment can weigh in on the issue. People aren't crazy to assume QFT is fundamental, nor are they crazy to assume it isn't, because nature would look like a QFT at low energies either way. Perhaps the proper attitude about the high-energy regime is just to be agnostic at this point in time. People should work on what they like, and be tolerant of people who have a different theology about the presently unknown and unconstrained high-energy regime.

You also write: "He also famously argued in the intro to his GR book that another kind of mathematics, geometry, was not needed for fundamental physics (he did this right before it became clear that gauge theory, a geometrical theory, was the way forward in particle theory). Yes, there too you can argue that geometry is just a low energy effective epiphenomenon if you want, but I don't think it's a convincing argument."

Gravity is a great thing to bring up in this discussion, because it appears to be something qualitatively different from a local QFT, as it seems to involve a change (now properly acknowledged by Weinberg) in the structure of spacetime itself, which is not something local QFTs do.

We have a pretty good low-energy effective QFT for general relativity for long-wavelength perturbations of the metric around a fixed background (again, see, e.g., Donoghue's work), and we can even use it to extract some quantum corrections to, say, Newton's law of gravitation, but there are basic conceptual problems that arise when one allows the geometry of spacetime to fluctuate nonperturbatively and if one wants to probe short-distance physics.

So taking seriously the geometric interpretation of GR is precisely what makes it hard to describe it in terms of a local QFT.

When Weinberg wrote that textbook in 1972, there were justifiable hopes that GR might ultimately admit a reasonably straightforward fundamental QFT description based on spin-2 massless particles, rather than the spin-1 massless particles underlying QED/QCD/Standard Model. Indeed, Weinberg showed in some papers in the late 1960s that a massless spin-2 particle had to couple to mass-energy as its source, and had to satisfy the equivalence principle. (That's pretty amazing, when you think about it.)

If the QG-as-QFT approach had worked out, and we had one big QFT that contained all of basic physics, then people could well have argued that field theory was in fact fundamental after all, as you say it is.

But it turned out that GR did not obviously admit a straightforward description in terms of a QFT. That's precisely the reason why so many people looked elsewhere for the microscopic theory of quantum gravity, knowing that any such microscopic description would look like a local field theory at low energies anyway, so why not look for something different in the short-distance regime?

What's remarkable, of course, is that we now seem to have nontrivial conjectural examples of quantum gravity with certain asymptotic boundary conditions (e.g., AdS) in which there does appear to be a highly non-obvious description in terms of a local QFT. (For AdS, famously, it appears to be a CFT.) What's ironic is that it was precisely folks who were looking at a non-QFT microscopic description (string theory) who stumbled on this discovery!

In any event, it is precisely taking a non-Weinbergian viewpoint toward the geometric picture of GR that motivated lots of people to take the Weinbergian viewpoint about local QFTs not being fundamental. But with examples like AdS/CFT, we might in the end be able to be non-Weinbergian about both viewpoints.

What are your feelings about AdS/CFT, by the way? Do you feel it has partially vindicated your hopes that we can describe fundamental quantum gravity in certain cases as a field theory?

You also write: "The argument against the "it's just a low energy approximation viewpoint" is the LHC data: qft and gauge theory work perfectly at the shortest distances we can test."

Well, yes, except that the shortest distances we can test aren't very short in the grand scheme of things — they aren't very short compared to the scales that people are actually arguing about when they talk about the question of non-fundamentalness of local QFT — unless the Planck scale is for some reason exponentially closer to us than we think it is.

That is, most people think that QFT in some form or another will work quite well at far shorter scales than we have probed at the LHC. The place people have long thought things would break down is, as you know, some 10 or 11 orders of magnitude higher in energy/shorter in distance. So the LHC isn't telling us one way or another about the regime people are actually worried about. The fact that the LHC is consistent with gauge theories isn't relevant to those questions, unfortunately.

That is, we don't have anything close to the experimental data that needs to weigh in on the question of who's correct about the fundamentalness of local QFT. That's why I referred to any present-day speculation about this question as being "not even wrong."

Igor Khavkine–

Well, what you're saying is that if we take an instrumentalist point of view toward probability theory, then quantum probability theory is no worse than classical probability theory. And that's long been pretty much accepted as true. The orthodox or instrumentalist point of view is very old, and is the refuge most people turn to when they give up trying to make more sense of quantum theory.

But what troubles people is precisely the question of instrumentalism. Not everyone wants to be an instrumentalist, for basic philosophical reasons.

In the classical case, we have the option of being non-instrumentalist if we wish — some people really like to think they personally have a state of being, and that people and detectors and measurement apparatuses are just as much physical systems as the things they examine — whereas in the quantum case, we don't seem to have such a straightforward non-instrumentalist option.

You write: "What I will perhaps never understand is how some people, when faced with a choice between these mathematically equivalent formulations, consistently go for the one laden with philosophical difficulties."

What I take from this statement is that you have no personal philosophical problem with instrumentalism, and don't appreciate why some people might not like instrumentalism.

So I think you should go out and ask them why! If you don't know of any reasons to be troubled by instrumentalism, then you might find some answers by doing a little research on the question. 🙂

30. **Peter Woit**
August 21, 2014

Reader297,
Sorry, no interest at all in yet another empty time-wasting debate about QG, see:
http://www.math.columbia.edu/~woit/wordpress/?page_id=4338
I'm making a serious effort here to discuss something different and more interesting, please stop trying to turn this discussion towards the same tedious topics that no one has now had anything new to say about for years.

31. **Reader297**
August 21, 2014

Hi Peter,

Woops! My comment was a direct response to your own comments — it definitely isn't my intention to start a discussion over QG per se, and I'm not favoring any particular direction in that regard.

I was just responding to your statement that you believe QFT is fundamental, my response being that because anything satisfying some basic properties looks like a local QFT at the scales we can reach experimentally so far, and thus that one can just as legitimately argue the jury is out on whether QFT is fundamental. I was also conceding that AdS/CFT might favor your point of view.

My other point was that we'd need to wait for experiment to weigh in, but that the question about the fundamentality of QFT referred to energies far beyond the LHC, and so might not be settled empirically for a while.

These were intended to be directly related to your statements. I too am not interested in going into a deep discussion of QG here! The only reason I brought up QG at all was that when taking the geometric picture of gravity seriously, it seems to challenge the QFT-as-fundamental idea — but, again, there are possible ways around that issue.

By the way, thanks for the link to your FAQ. Which of the questions on your FAQ did you mean to point me to?

32. **manfred Requardt**
August 21, 2014

Dear Thomas, many thanks for your interesting remarks. As I understand it, it is argued that gravity and the presumed underlying quantum gravity have different diffeomorphism groups. It is a nice coincidence that recently (1206.0832; sorry for selfreferencing) I argued that diffeomorphism invariance is spontaneously broken due to the emergence of classical spacetime with the gravitons as Goldstone particles. This entails that observables need not be diff-invariant. More specifically, there exist two classes of observables, the Dirac ones and the broken ones.
I hope that your remarks are helpful in this context.

33. **Marty Tysanner**
August 21, 2014

Reader297,

It's a pleasure to read your comments, both their content and phrasing. Your discussions are very clear and well-reasoned, and I like the way you include ample context/background with your responses to explain why others may take positions different than Peter. It's certainly a quality of communication to aspire to! I assume your explanations are as complete as they are because you want to make your arguments clear to readers whose background is more limited than Peter's.

Peter,

I think this discussion is worthwhile to many of us in clarifying your apparent position that QFT is fundamental. I'm certainly finding it interesting at least. I hope you won't shut it down because Reader297 brought up QG… My impression was that he wasn't promoting an approach to QG — he was being responsive to your disagreement with some of Weinberg's viewpoints and the question of how to assess whether QFT is indeed fundamental. (In fact, I was surprised you interpreted his response as an attempt to steer the discussion toward QG or other non-topical issues.)

Anyway, I hope this discussion can productively continue!

34. **Reader297**
August 21, 2014

Hi Marty,

That's very kind of you to say. Thanks so much.

Peter,

I also wanted to add that I, too, have found your recent blog posting and subsequent discussions very enlightening.

I'm always happy to see blog postings here in which you articulate your own proposals and explain your reasoning for them, as well as open them up for conversation. Thanks!

35. **Peter Woit**
August 21, 2014

Marty/Reader297,
I'm afraid I have all too much experience with what happens here (and on many other blogs) once an argument over QG/AdS/CFT/string theory starts, so, trust me, if you want this comment section to be something worth reading (which I think it has been, thanks to Reader297 and others).

My sloganeering is intentionally challenging to what I think is some dubious received wisdom, and Reader297 has done a good job or laying out the standard point of view that I'd like to challenge. That's all well and good, but getting into the usual arguments over string theory/QG is something different and not going to lead anywhere interesting.

For the specific FAQ entry
http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=what-do-you-think-about-question-x-in-quantum-gravity

36. **Thomas Larsson**
August 21, 2014

I thought the comment about QG refered to me, because when I checked earlier today my last post seemed gone. However, before I had the time to get home and check the references for my scathing reply, the post was there again. So either I did not look hard enough, or the post reappeared. I plead guilty of having rehashed old arguments about QG in the past, but this time I think my comment was quite on topic.

Anyway, since I did spend quite a bit of time on this, let me say something else that Peter will also dislike: representation theory cannot be quantum **field** theory except in low dimensions.

Reps of finite-dimensional Lie groups is QM, and reps of infinite-dimensional Lie groups living on the circle (affine Kac-Moody and Virasoro algebras) are QFT in 2D, or really in one complex dimension. However, the corresponding representations in higher dimensions are not QFTs, although they are quantum theories.

For definiteness, consider Yang-Mills theory in 3+1D. As is well known, this theory has gauge anomalies proportional to the third Casimir, and in particular the anomaly vanishes if the gauge group is SU(N) with N != 3. However, as is clearly stated in [Pressley-Segal, Loop Groups, 1988, section 4.10], the corresponding current group admits two inequivalent extensions: the MF extension, which is also proportional to the third Casimir, and the central extension, which is proportional to the second Casimir. The second Casimir is non-zero for all gauge groups including SU(N) for all N, and hence cannot be related to the gauge anomalies arising in QFT.

Let me let this fact sink in, because very few physicists seem to appreciate it, including those who have studied the Pressley-Segal book for decades.

Not all extensions of the algebra of gauge transformations in 3+1D correspond to gauge anomalies in QFT. The others arise in – something else. Definitely not in QFT.

Worse, the MF algebra which does arise in QFT does not have any interesting quantum representations [Pickrell 1989]. Mickelsson tried to generalize the Pressley-Segal methods to higher dimensions, but after the Pickrell show-stopper he switched gears and looked for some kind of representations acting on a family of Hilbert spaces parametrized by a classical gauge field. This may be fine and dandy, but it is not really a true quantum theory. In a fully quantum theory, the gauge fields must be quantized as well, but then Pickrell's no-go theorem returns with a vengeance. So the extension that does arise in QFT does not have any good representations. This is of course in agreement with the fact that gauge anomalies cancel in the standard model.

In contrast, the central extension does not arise in QFT, but it does have representations, some or maybe all described somewhere in Pressley-Segal section 9.1. One example is the rep induced from an affine algebra living on some circle embedded in the higher-dimensional ambient space. They possibly show (I think I have seen such a claim but I cannot find it any more) that all irreps are achieved in this way. Since this result is somewhat boring from some people's point of view, it may be what Greg Moore refers to when he talk about discouraging no-go theorems [here, section 4.7]. Another reason why this extension cannot arise in QFT is that there is no privileged one-dimensional curve in QFT.

To summarize, there are three cases:

1) No extension. Then there is no non-trivial reps at all, by the same argument that the centerless affine algebra lacks reps.
2) MF extension. Arises as a gauge anomaly in QFT, but has no good representations. Besides, the gauge anomalies cancel in the standard model.
3) Central extension. Has unitary reps, but does not arise in QFT. Both because it has the wrong structure, and because the reps depend on a privileged 1D curve.

Since the extensions that arise in QFT don't have representations, and the extensions that have representations don't arise in QFT, I propose a better slogan:

*Representation theory is quantum theory, but not quantum **field** theory except in low dimensions.*

37. **Marty Tysanner**
August 21, 2014

Hi Peter,

Thank you for clarifying that you weren't specifically objecting to Reader297's mention of quantum gravity — you foresaw that bringing up QG would probably cause others to hijack the discussion with pointless, repetitive arguing. I agree.

Although it isn't your primary topic, you indicated in the comments that you believe the Standard Model may be fundamental. It would easier to understand this position if I knew specifically what it means (in your sense) to say that the Standard Model QFT is fundamental. Let me propose three alternatives; yours may be different.

1. The Standard Model is fundamental in the sense that its Lagrangian is inevitable. That is, whatever the ultraviolet completion of the SM might be, it will uniquely imply the SM as its low energy limit.

(Note: By an "ultraviolet completion" I include the possibility of explanatory microscopic models for which a Lagrangian can be specified.)

2. The SM is fundamental in the sense that it is the best description we can ever hope to empirically verify. Attempts at an ultraviolet completion may succeed in reproducing the SM at observable energies, but they will forever remain little more than "stories" we tell to reassure ourselves that we really can and do understand Nature "all the way down."

3. The SM is fundamental in that it already describes physics at all energy scales, or at least up to the Planck scale. This view doesn't preclude future extensions that explain the values of the various Yukawa couplings and gauge constants, provided those discoveries don't modify the SM Lagrangian in any basic way.

I won't argue with your definition since it stems from your own philosophical views.

My other question is about your statement,

> The argument against the “it’s just a low energy approximation viewpoint” is the LHC data: qft and gauge theory work perfectly at the shortest distances we can test.

Reader297 also asked about this. In what way do you disagree with the conventional view, i.e., that the LHC and future practical colliders can only probe “low” energies compared to the regime where the SM is expected to break down, so that collider data cannot help us determine whether the SM remains valid in the ultraviolet?

38. **Peter Woit**
August 22, 2014

Marty/Reader297,
This is getting rather far afield from the question of the relation of QM and representation theory, but a few comments:

I just don’t think that describing the highest energy you can study as “low energy” and thus irrelevant to fundamental questions is sensible, nor is basing arguments on highly speculative assumptions about what is taking place at 10^12 or whatever times the highest energies you know anything about. And on this I don’t think my point of view is unconventional. Sure, at low energies, lots of other things may look like QFT, because they have to for consistency reasons, but if you don’t actually have a non-QFT theory at high energies that has evidence for it or explains anything in a compelling way, the conjecture that it’s QFT all the way down is a viable one.

I don’t think the SM is a fully satisfactory model, since it doesn’t explain a list of things one would expect a fundamental theory to explain. But I have no idea what does explain those things and what a better theory than the SM is. My argument is just that given how well the SM works, it’s a good idea to take very seriously the mathematical framework it uses and try and understand where that might come from and whether there is some aspect of it we are missing, instead of dismissing it as “just a low energy approximation”, and looking for something completely different. Especially since conjectures about something completely different have gone nowhere.

39. **Reader297**
August 22, 2014

Hi Peter,

Again, fair remarks.

All I would say at this point is that we have enough physical examples now, e.g., from condensed matter, of systems that look like field theories at long distances but aren’t microscopically field theories that any speculations about fundamental physics being QFT “all the way down,” while perhaps viable, are not singled out by experiment. There’s no way to say that they’re even wrong.

And, again, it doesn’t help the situation that, as you note, “at low energies, lots of other things may look like QFT, because they have to for consistency reasons…”

What’s unfortunate is the dearth of hard experimental data high-energy physics has been suffering from for the past several decades. We’re flying blind. And it means that, at this moment in time, hypotheses about the fundamentality (or not) of QFT are mostly a matter of personal preference. Not that there’s anything wrong with that, but it does mean that people are going to argue a lot without changing each other’s minds.

40. **j.c.j.vandervelde**
August 22, 2014

Gentlemen,

you seem to agree that “at low energies, lots of other things look like QFT.”
Can you give some examples or references?

41. **Peter Morgan**
August 23, 2014

Peter: what’s your feeling about taking a signal processing approach to QM, instead of a classical mechanics approach? Hilbert spaces emerge naturally in signal processing through fourier transforms, and, through the elementary (and not very rigorous) mathematics of Leon Cohen’s Foundations of Physics 18, 983(1988), complex structure and incompatible operators are natural too when we take fourier transforms of probability distributions. Leon Cohen is at CUNY, so perhaps you know him?

Modern high energy physics seems to be more about feature detection in statistics of complex sets of signals than about particles.

42. **Reader297**
August 24, 2014

Peter (Morgan),

Working in an instrumentalist framework, Cohen seems to have been motivated by a theorem of Khinchin’s that a complex-valued function M(u) is the characteristic function for some probability distribution P(x) if and only if M(u) can be expressed as the integral over x of a product of the form g*(x)g(x+u) for a complex-valued function g(x) for which |g(x)|^2 integrates to 1. Cohen notes that this construction looks suspiciously like we have a wave function g(x).

But there isn’t anything surprising here. We could have skipped M(u) altogether and just noticed that we can express P(x) as g*(x)g(x) for any complex function g(x) for which |g(x)|^2 integrates to 1. Taking the extra step of going through M(u) doesn’t actually buy us anything. One can derive all of Cohen’s results just from expressing P(x) in terms of g(x).

Cohen notices that characteristic functions can be used to define joint probability distributions that are positive but not bilinear in wave functions. But he doesn’t address how to pick a canonical such distribution, nor how to write down the time evolution for them, nor how to handle matching his joint distributions onto the probability distributions of a larger system in the presence of nontrivial entanglement.

43. **Peter Morgan**
August 24, 2014

Reader297, certainly Cohen’s paper is no more than indicative, and certainly there could be other approaches. That’s a large part of why the paper I mention is in Foundations of Physics. Cohen’s academic output is more usually found in IEEE and other signal processing journals.

My only claim would be that a signal processing approach is more abstract (even while being closer to experimental data) and avoids having as much classical mechanics baggage, albeit I’m not sure whether that’s a good or bad thing (particularly for a new student of QM). To some extent, it’s taking Heisenberg without the Schrödinger.

Grand Unification of Mathematics and Physics

August 20, 2014
Categories: Favorite Old Posts, Quantum Mechanics, Uncategorized

For a second slogan about quantum mechanics I've chosen:

> Quantum mechanics is evidence of a grand unification of mathematics and physics.

I'm not sure whether this slogan is likely to annoy physicists or mathematicians more, but in any case Edward Frenkel deserves some of the blame for this, since he describes (see here) the Langlands program as a Grand Unified Theory of mathematics, which further is unified with gauge field theories similar to the Standard Model.

This week I'm in Berkeley and have been attending some talks at an MSRI workshop on New Geometric Methods in Number Theory and Automorphic forms. Number theory is normally thought of as a part of mathematics about as far away from physics as you can get, but I'm struck by the way the same mathematical structures appear in the representation theory point of view on quantum mechanics and in the modern point of view on number theory. For example, the lectures on Shimura varieties have taken as fundamental example the so-called Siegel upper-half space, which is the space Sp(2n,R)/U(n). Exactly the same space occurs in the quantization of the harmonic oscillator (see chapters 21 and 22 of my notes), where it parametrizes possible ground states. Different aspects of the structure play central roles in the math and the physics. In the simplest physics examples one works at a fixed point in this space, with Bogoliubov transformations taking one to other points, something which becomes significant in condensed matter applications. In number theory, one is interested not just in this space, but in the action of certain arithmetic groups on it, with the quotient by the arithmetic group giving the object of fundamental interest in the theory.

The workshop is the kick-off to a semester long program on this topic. It will run simultaneously with another program with deep connections to physics, on the topic of Geometric Representation Theory. This second program will deal with a range of topics relating quantum field theory and representation theory, with the geometric Langlands program a major part of the story, one that provides connections to the number theoretical Langlands program topics of this week's workshop. I've got to be in New York teaching this semester, so I'm jealous of those who will get to participate in the two related MSRI programs here in Berkeley. Few physicists seem to be involved in the programs, but these are topics with deep relations to physics. I do think there is a grand unified theory of some kind going on here, although of course one needs to remember that grand unified theories in physics so far haven't worked out very well. Maybe the problem is just that one hasn't been been ambitious enough, that one needs to unify not just the interactions of the standard model, but number theory as well…

Comments

1. **Urs Schreiber**
   August 22, 2014

   What underlies the striking "unification" of concepts of QFT with concepts found in number theory generally and in automorphic representation theory particularly is to a large extent attributable to the function field analogy. This together with the Weil uniformization theorem is what makes one guess geometric Langlands (and hence QFT phenomena) from number theoretic Langlands.

   However, to date this is really just that, educated guessing based on analogy. What is missing would be a theory that makes the "unification" here a systematic theorem.

   For instance at the Oxford meeting on the Langlands program that you mentioned a while back here, Sergey Oblezin had reported on his work with Gerasimov and Lebedev on topological sigma-model mirror symmetry computations related to geometric Langlands, and when asked afterwards if any of these insights could conceivably be carried over to the number theoretic Langlands correspondence, the answer was something like "We could, if only we really knew what it would mean to systematically consider p-adic sigma-models".

   This is open. It is probably somehow related to the phenomenon, maybe not widely appreciated, that also the construction of the refined Witten genus by Ando-Hopkins-Rezk passes all the way through arithmetic geometry (elliptic curves, hence string worldsheets, defined over Spec(Z); in fact complex worldsheets never even appear explicitly in the computation and only the elliptic curves in positive characteristic (the supersingular ones) contribute at the stringy height 2.)

   Last week at CUNY in New York I gave a talk (notes and video are here) on what I would modestly suggest might be a mathematical theory that would allow to turn the analogy here into a systematic theorem unifying automorphic mathematics with QFT.

2. **David Ben-Zvi**
   August 22, 2014

   Urs' notes look really exciting and suggestive! such unifying geometric structures are definitely needed to get a deeper understanding of these analogies, or to see the arithmetic world directly in the light of QFT.

   I would disagree though with the suggestion that the geometric Langlands analogy is only an educated guess to date (if it ever was that – starting from Drinfeld's original work). In fact one of the main themes of our current MSRI semester is the really exciting interactions going on between geometric Langlands and classical Langlands – for the first time in the last couple of years people (in particular Xinwen Zhu and Zhiwei Yun) are directly taking ideas and constructions from the geometric Langlands program and using them to prove (phenomenal) arithmetic results. (Ngo's proof of the Fundamental Lemma was arguably the beginning of this trend.) The fundamental structures of the Langlands program already span across fields of definition in a natural way.

3. **David Ben-Zvi**
   August 22, 2014

   The most exciting thing around these parts though is the shockingly futuristic sounding course announcement (http://math.berkeley.edu/courses/fall-2014-math-274-001-lec) by Peter Scholze, who seems to be able to work with number fields as if they were function fields:

   Syllabus: Originally defined by Drinfel'd, and then used extensively by L. Lafforgue and V. Lafforgue, moduli spaces of shtukas have proved to be a powerful tool in the study of the Langlands correspondence over function fields. It is an important question whether something similar could work over number fields.
   In this course, we want to sketch a strategy to define moduli spaces of local shtukas over mixed-characteristic fields such as Qp (leaving open the problem of assembling these spaces for varying primes p). These spaces should generalize Rapoport-Zink spaces, as well as the conjectural theory of local Shimura varieties that has recently been suggested by Rapoport-Viehmann. Notably, the spaces of local shtukas should overcome the 'minuscule' condition inherent in the theory of Shimura varieties, so that they can be seen as very general p-adic period domains (which exist even in situations where Griffiths transversality is a nontrivial condition). We wll start by reviewing the theory of (local and global) shtukas over function fields. Next, we will define shtukas in mixed characteristic in an absolute setup; this is closely related to the notion of Breuil-Kisin modules, and has been the subject of recent investigations of Fargues, some of whose results we will recall.
   To set things into perspective, we will spend some time detailing the case of p-divisible groups, explaining the equivalence between p-divisible groups and certain shtukas (due to Breuil, Kisin, and Fargues). We will then use the joint work with Weinstein to identify Rapoport-Zink spaces with moduli spaces of shtukas.

We will then be able to define general spaces of shtukas. They will turn out to be somewhat esoteric objects, called ‘diamonds’, so quite a bit of time will be spent on explaining the definition of diamonds. In particular, we will explain how they overcome the problem that a general space of shtukas should live over ‘a product of several copies of Spa Qp’ (taken over some absolute base ‘F1’), by giving a highly nontrivial definition of ‘a product of several copies of Spa Qp’ in a way that makes Drinfel’d’s lemma true.
If time permits, we will try to (conjecturally) understand the étale cohomology of these spaces, and their relation to the local Langlands correspondence.defined by Drinfel’d, and then used extensively by L. Lafforgue and V. Lafforgue, moduli spaces of shtukas have proved to be a powerful tool in the study of the Langlands correspondence over function fields. It is an important question whether something similar could work over number fields.
In this course, we want to sketch a strategy to define moduli spaces of local shtukas over mixed-characteristic fields such as Qp (leaving open the problem of assembling these spaces for varying primes p).

4. **David Corfield**
August 23, 2014

I wonder if philosophy might be somewhat to blame. Or is this to take on to much of a role for my discipline? Perhaps if we had at least pressed mathematicians over the past, say, 60 years to try make clearer sense to us their conceptions of space, it might have stimulated some thinking along these lines.

It’s notable in this respect that Weyl’s gauge theory grew out of a deep reading of Husserl. And if Urs’s approach were to flourish, a philosophically-oriented mathematician, William Lawvere, will be in line for the credit of devising the notion of ‘cohesion’.

5. **Urs Schreiber**
August 23, 2014

I used to just believe the advertisement that the statement of geometric Langlands duality follows, at the moment , by more than educated guesswork. It was an eye-opener when Robert Langlands himself publically expressed doubts recently. In this vein it pays to look at page 4 of

* Dima Arinkin, Dennis Gaitsgory, “Singular support of coherent sheaves, and the geometric Langlands conjecture” (arXiv:1201.6343)

The usual statement of geometric Langlands duality, as found in the standard reviews, they call a “naive guess”, pointing out that V. Lafforgue had explicitly shown it to be wrong in general. Then they discuss the “heuristic reason” for this failure. Check out some sample text from this page 4 to see the educated guesswork in action:

> “So, in the geometric theory one has been faced with the challenge of how to modify the Galois side […] with the hint being that the solution should come from […]. The general feeling, shared by many people who have looked at this problem, was that the sought-for modification has to do with the fact that […]. I.e., we need to modify […]. The goal of the present paper is to provide such a modification, and to formulate the appropriately modified version of the “best hope.”

Needless to say that I don’t find fault with educated guesswork, and that I find the work by all people involved here admirable. But where it involves educated guesswork one should call it such, for the sake of the subject. Speaking to actual number theorists reveals that a derivation of geometric Langlands and its QFT incarnation not involving handwaving would be much appreciated.

My modest point is that if one had a systematic “inter-geometric” theory which would work both for complex analytic as well as for arithmetic gemetry, then this kind of problem of wrong guesses might not occur.

For instance one central part of the analogy is that automorphic representations on the arithmetic side are supposed to be analogous to Hitchin connections on bundles of conformal blocks over a moduli stack of fields of a gauged WZW model.

Now if one had a truly inter-geometric theory, then it might be possible to axiomatize what the Hitchin connection on the bundle of conformal blocks over the moduli of fields of the WZW model would be (indeed, that is what the “cohesive” axioms are aiming at, we are almost there), and then interpret these axioms systematically in arithmetic geometry, such that one would obtain systematically the desired number-theoretic analog, together with the guarantee that it has all the relevant properties.

In other words, with a genuine inter-geometric theory one might be able to systematically say what Yang-Mills/Chern-Simons/Wess-Zumino-Witten QFT is in arithmetic geometry (something like — but much more comprehensive than — what is currently found under the headline “p-adic string theory”) and then systematically work out what the bundle of conformal blocks is when interpreted over some global field. If the inter-geometric theory works well, then it would guarantee that the arithmetic-geometry incarnation of this quantum theory would satisfy the same kind of general properties (e.g. the relevant arithmetic version of S-duality) that its complex-analytic cousin does.

Such a theory would be an actual “unification of interactions in physics and number theory” as in the thread above. Whether or not my humble suggestion for how to go about this works out, I believe such a theory would be as desireable as it is clearly missing at present, all the excitement about undeniable progress nonwithstanding.

6. **uair01**
August 23, 2014

… the way the same mathematical structures appear in the representation theory point of view on quantum mechanics and in the modern point of view on number theory …

Is there any way an interested outsider can get a feel for what you’re doing here? It sounds fascinating and beautiful, but also frustratingly unreachable. As an engineer I have some grasp of mathematics but this is no mathematics “as we know it”.

7. **David Ben-Zvi**
August 23, 2014

As I understand them, the issues that Arinkin-Gaitsgory discuss don’t in any way reveal a shakiness of the analogy or an indication of guesswork (I haven’t understood Langlands’ notes so can’t comment). Part of the problem is that the words “geometric Langlands” stand for several layers of statements. The traditional one, formulated by Drinfeld, has to do with individual cuspidal Hecke eigensheaves corresponding to irreducible local systems. This statement is the one that’s easiest to see in analogy with number theory (matching Hecke eigenforms with Galois representations) though even here one has to take account of a basic failure of naive dictionaries: the geometric analog of a number field or function field in finite characteristic should not be a Riemann surface, but roughly a surface bundle over the circle. This explains the “categorification” (need for a function-sheaf dictionary, which is the weak part of the analogy) that takes place in passing from classical to geometric Langlands — if you study the corresponding QFT on such three-manifolds, you get structures much closer to those of the classical Langlands correspondence.

In any case, this form of the geometric Langlands correspondence is now a theorem for GL_n. The categorical form that people now mean by GLC is an attempt to understand something more subtle, namely the functional analysis aspect of Langlands: we want to decompose the geometric analog of L^2 of the adelic double quotient. So it is very natural to expect that functional analytic issues emerge, i.e., you have to be very careful about setting up the correct function space. This has been clear to experts from the start, with the stated versions of the categorical correspondence being a guideline rather than a statement to be taken literally. But remarkably, despite the extra categorifications, the functional analytic issues are precisely analogous to those that arise in number theory (in the Langlands decomposition and Arthur-Selberg trace formula), and that goes for Arinkin-Gaitsgory's work as well. In fact their work is for me further justification of the robust nature of the analogy between number theory and QFT since even at this greater resolution both sides give rise to the same structures.

If anything it seems that the two sides keep getting closer – for example Xinwen Zhu's striking recent version of geometric Satake in mixed characteristic. His story doesn't have a direct number field analogue of factorization, the main geometric structure to come out of the geometric Langlands program, but rumor has it Scholze does! Likewise classical Langlands doesn't seem to have much use for many of the categorical structures of geometric Langlands, but apparently the thriving p-adic Langlands program is finding many of the same issues and structures as well. None of which should take anything away from the inter-geometric theory Urs is developing, which sounds very exciting, just to say the analogies and bridges between the areas are stronger than ever (and to my mind were never questionable, just only recently starting to really live up to their potential).

Nature is Fundamentally Conformally Invariant

August 20, 2014
Categories: Favorite Old Posts, Quantum Mechanics

For a third slogan I've chosen:

> Nature is fundamentally conformally invariant.

Note the weasel-word "fundamentally". We know that nature is not conformally invariant, but the kind of thing I have in mind is pure QCD, where the underlying classical theory is conformally invariant, with quantization dynamically breaking conformal invariance in a specific way.

The group of conformal symmetries, its representations, and what this has to do with physics are topics I haven't written about in the notes I've been working on. This is because I suspect we still haven't gotten to the bottom of these topics, and properly dealing with what is known would require a separate volume. Part of the story is the twistor geometry of four dimensions, which Roger Penrose pioneered the study of, and which recently has found important applications in the calculation of scattering amplitudes.

As a more advanced topic, this slogan would normally have been put off until later, but I wanted to point to a new article by Natalie Wolchover in Quanta magazine which deals with exactly this topic. It describes several different efforts by physicists to rethink the usual story about the hierarchy problem, taking a conformally invariant model as fundamental. For the latest example along these lines, see this arXiv preprint. The whole article is well-worth reading, and it includes a quote from Michael Dine (whose work I've been critical of in the past) that I found heart-warming:

> "We're not in a position where we can afford to be particularly arrogant about our understanding of what the laws of nature must look like," said Michael Dine, a professor of physics at the University of California, Santa Cruz, who has been following the new work on scale symmetry. "Things that I might have been skeptical about before, I'm willing to entertain."

Perhaps particle theorists are beginning to realize that the landscape is just a dead-end, and what is needed is a re-examination of the conventional wisdom that led to it.

Comments

1. **vmarko**
   August 20, 2014

   Peter,

   "We know that nature is not conformally invariant, but the kind of thing I have in mind is pure QCD, where the underlying classical theory is conformally invariant, with quantization dynamically breaking conformal invariance in a specific way."

   I am amazed by the number of people who keep repeating this "pure QCD" effect of dynamical breaking of conformal symmetry, with none of them ever actually trying to do the math.

   I've already made a whole series of comments on one of your previous posts about this — in pure QCD, the symmetry breaking **is not** a consequence of quantum corrections. It is a boundary condition that one plugs into the theory by hand. One can equally well choose a different boundary condition, and keep conformal invariance at the quantum level, starting from the very same "pure QCD". So there is nothing "dynamical" in "generating the scale" from quantum corrections, because the boundary condition is not a consequence of dynamics.

   I do understand and appreciate the importance and beauty of conformal symmetry. But this symmetry is broken in nature (much like supersymmetry must be). And symmetry breaking is never a consequence of dynamics — be it spontaneous or through quantum corrections — it is always due to a boundary condition, independent of any dynamics.

   The fundamental property of nature is the existence of the scale, not lack thereof.

   Best, 🙂
   Marko

2. **Peter Woit**
   August 20, 2014

   Marko,
   I just completely disagree with you here, but there is no point in having the same argument as last time. To stick to undeniable facts, the pure QCD Lagrangian is conformally invariant, the spectrum of the quantized theory (as calculated e.g. in the standard fashion via lattice gauge theory), is not.
   By the way, there is a counterexample to your "none of them ever actually trying to do the math". I started my career doing this kind of QCD calculation…

   Whatever you want to call it, the point of the slogan is that the other interactions should get with it and try to behave as nicely as pure QCD.

3. **vmarko**
   August 20, 2014

   Peter,

   "the pure QCD Lagrangian is conformally invariant, the spectrum of the quantized theory […] is not."

   Of course, I don't have a problem with the facts. I just have the problem with spinning this as a "dynamically generated" thing. It is a property that you can choose for the theory to have or not have — the spectrum of the quantized pure QCD can also be conformally invariant, if you want it to. This is usually overlooked because it is an uninteresting scenario. But to claim that the conformal symmetry *must* be broken in quantized pure QCD is just wrong.

   "Whatever you want to call it, the point of the slogan is that the other interactions should get with it and try to behave as nicely as pure QCD."

   I can appreciate this way of looking at things. But in whatever way you formulate interactions, sooner or later you need to break the conformal symmetry. And there is nothing in the theory (classical or quantum) that requires you to do so — you need to **postulate** that conformal symmetry is being broken (for example, via quantum corrections). IMHO, this somehow this goes against the statement of the slogan.

Best, 🙂
Marko

4. **Curious Mayhem**
August 20, 2014

Totally off subject, but really not:

https://www.southernnewengland.aaa.com/legacy/TravelDiscounts/discounts-details.php?PROD_ID=295

There's a new cruise liner in the Caribbean, Quantum of the Seas. No word if it sails through the Bermuda Triangle wormhole, or whatever it is.

Clearly, this quantum business is reaching new audiences.

5. **Nakanishi**
August 20, 2014

Pure QCD is badly infrared divergent; in contrast with QED, its infrared divergence survives even in the transition probability. Probably, it is impossible to avoid the appearance of infrared divergence without violating scale invariance. Indeed, in the 2-dimensional free massless scalar field theory, infrared divergence is very serious; scale invariance is spontaneously broken in the consistent quantization.

6. **Ramanujan**
August 21, 2014

QCD is classically conformally invariant. By asserting that it is fundamentally conformally invariant, you are claiming that classical physics is more fundamental than quantum physics. I do not agree.

7. **Aleksandar Mikovic**
August 21, 2014

Postulating conformal symmetry as a fundamental one (i.e. at short-distance scales) is the same idea as the fundamental supersymmetry: one wants to stabilize the quantum corrections to the Higgs mass or the cosmological constant by using the symmetry. However, in all these calculations the effect of quantum gravity is completely ignored, or it is assumed that it is not important. In a paper with M. Vojinovic (arxiv:1407.1394) we showed that the cosmological constant problem can be solved when a quantum gravity contributions are taken into account. I believe that the same could be done for the Higgs mass, so that one does not need fundamental supersymmetry or conformal symmetry. However, one needs a fundamental quantum gravity theory, and a good candidate is Regge quantum gravity (a modification of spin-foam models where the basic variables are the edge lengths and a spacetime triangulation).

8. **confused**
August 21, 2014

"Of course, I don't have a problem with the facts. I just have the problem with spinning this as a "dynamically generated" thing. It is a property that you can choose for the theory to have or not have — the spectrum of the quantized pure QCD can also be conformally invariant, if you want it to. This is usually overlooked because it is an uninteresting scenario. But to claim that the conformal symmetry *must* be broken in quantized pure QCD is just wrong."

I don't get that. The spectrum of pure QCD (SU(3) gauge fields only) consists of glueballs and you can compute their mass ratios in lattice QCD. I have done that. So there is no conformal symmetry in the quantum theory. What "property" of the theory can I choose to make these massive glueballs go away?

9. **vmarko**
August 21, 2014

"The spectrum of pure QCD (SU(3) gauge fields only) consists of glueballs and you can compute their mass ratios in lattice QCD."

Calculating mass ratios means nothing until you prove that at least one of those glueballs has nonzero mass. Otherwise all masses of all glueballs could be zero, and your mass ratios are satisfied trivially.

Btw, if you have managed to prove that a single glueball must have a nonzero mass, go claim the Millenium prize. 🙂

"What "property" of the theory can I choose to make these massive glueballs go away?"

Naively said, there is usually an implicit (and unproven!) assumption that at least one of the glueballs is massive, because only in that case calculating mass ratios is nontrivial.

More seriously, show me your calculation in full detail, and I'll probably be able to point you to such an a priori assumption. (Though it might be hard to find, if it is phrased in an obscure way or buried somewhere in the numerical calculations.)

Even more seriously, lattice QCD is an approximate rather than exact calculation method, and can introduce all sorts of artifacts (fermion doubling is the most popular example of this), some of which we may still not be aware of. These artifacts might lead you to a conformally broken result simply as a consequence of the approximation scheme, rather than being the property of the theory itself.

And really, in all these discussions one needs to pay special attention to distinguish the statement "mass gap *can* exist" from the statement "mass gap *must* exist", in every particular calculation. This can be done only by detailed step-by-step inspection on every paper that deals with conformal breaking in QCD. And it doesn't make sense to do this in blog comments.

I feel like I am going against the windmills with this. I've already stated my point as clearly as I can, and I see no point in further debate.

Best, 🙂
Marko

10. **confused**
August 21, 2014

"Calculating mass ratios means nothing until you prove that at least one of those glueballs has nonzero mass. Otherwise all masses of all glueballs could be zero, and your mass ratios are satisfied trivially."

That sounds a bit like demanding a proof that the International Prototype Kilogram has a non-zero mass.

11. **Peter Woit**
August 22, 2014

vmarko,
There is no fermion-doubling problem in pure QCD on the lattice. There is very strong numerical evidence for the conventional conjectures about the existence of a continuum limit of the lattice with a non-zero mass gap. Yes, there is no proof, and it's possible the usual conjectures are just wrong, but I know of no evidence pointing to that.

12. **vmarko**
August 22, 2014

Confused,

"That sounds a bit like demanding a proof that the International Prototype Kilogram has a non-zero mass."

Sigh… Are you trying to compare a glueball to a proton? One lives in pure QCD, while the other (arguably) lives in the Standard Model — where the conformal symmetry is explicitly broken by the Higgs mass, already at the classical level. Apples and oranges.

The proton mass can (in principle, ultimately) be expressed as the Higgs mass times a dimensionless coefficient. This coefficient is a complicated function of electroweak-, QCD- and Yukawa-couplings, some large and some small, combining into the renormalized grand-total of 1/125. While one can argue that the dominant contribution comes from QCD, it still ends up multiplying the Higgs mass (this is on purely dimensional grounds). On the other hand, in pure QCD there is no Higgs, and unless you *assume* the existence of some other nonzero mass parameter, the coefficient of 1/125 is multiplied by a zero, keeping all glueballs massless.

Peter,

"There is no fermion-doubling problem in pure QCD on the lattice."

Sure, but there might be other (unknown) artifacts of the lattice. Fermion doubling is just an example and a warning that such things may appear where you don't expect them a priori.

"There is very strong numerical evidence for the conventional conjectures about the existence of a continuum limit of the lattice with a non-zero mass gap. Yes, there is no proof, and it's possible the usual conjectures are just wrong, but I know of no evidence pointing to that."

That's because nobody is looking for such evidence — everyone is trying to calculate a nonzero mass gap. It's a social effect. Whenever I speak to QCD people, they always eventually say something on the lines of "Sure, you can keep conformal invariance, but why would you want that? It doesn't correspond to experiment.". And then people like Alessandro Strumia go on to claim that conformal invariance *must* be broken at the quantum level, because this effect was "proven" in QCD. But there is no proof. And there is no evidence to the contrary simply because QCD folks are just not interested in preserving conformal invariance, and they always implicitly discard that scenario as trivial.

That's why I have a problem with things like agravity and fundamental conformal invariance — they are blinded by a social effect, and have no solid basis.

Best, 🙂
Marko

13. **AlsoConfused**
August 22, 2014

Marko,
are you saying that in the end all mass is generated by the Higgs mechanism ?
But what if the Higgs mass is actually due to quantum corrections (Coleman-Weinberg mechanism) breaking the fundamental conformal symmetry of nature ?
http://arxiv.org/abs/1401.4185

14. **vmarko**
August 22, 2014

"are you saying that in the end all mass is generated by the Higgs mechanism ?"

Possibly. If not, you have to postulate other sources of mass. Mass is never automatically enforced by quantum corrections (or dynamics in general).

"But what if the Higgs mass is actually due to quantum corrections (Coleman-Weinberg mechanism) breaking the fundamental conformal symmetry of nature ?"

That always involves a postulate (a suitable boundary condition for the corresponding RGE). The CW mechanism does not *necessarily* break conformal symmetry — it does only if you choose appropriate boundary conditions. Alternatively, you can choose to keep the minimum of the Higgs potential at zero, with no spontaneous symmetry breaking at all.

Best, 🙂
Marko

15. **Peter Orland**
August 22, 2014

Marko,

1. It's not true that the mass of the proton mostly comes from the Higgs. It is the other way around – most of the mass comes from QCD. Most of the proton's mass is not from the quark masses (the latter does come from the Higgs, of course). Turning off the Higgs has little effect on the proton mass.

We used to call a fraction of the proton mass the "constituent" quark mass – but it is not really the quark mass – it comes from QCD.

2. "That's because nobody is looking for such evidence — everyone is trying to calculate a nonzero mass gap. It's a social effect." Not really. The numerical evidence has been done very carefully. As a matter of fact, it's been done carefully for decades.

There were serious analytic and numerical efforts by Patrasciou and Seiler to prove there was no confinement and gap, challenging the conventional wisdom. In the end, their program did not succeed, but they managed to do some interesting things in the attempt.

Also concerning 2.: there are a few people who do fool themselves into thinking they have demonstrated the existence of mass gap. They are generally not taken seriously (especially after the fiftieth paper). Most of us think that it will take a lot of creativity, hard work and (importantly) luck to understand the gap.

16. **Peter Orland**
August 22, 2014

Correction – almost all the constituent mass comes from QCD (flavor dependence comes from the Higgs).

17. **vmarko**
August 22, 2014

Peter Orland,

"Turning off the Higgs has little effect on the proton mass."

How can you know this? You can't turn it off in experiment, and you can't evaluate the proton mass analytically and take the limit, because we cannot solve SM QCD equations in the IR. Moreover, the argument that the two masses are proportional goes on purely dimensional grounds — besides the Higgs mass, there is no other fundamental dimensionful parameter in the SM that can be proportional to the proton mass. People often claim that effective QCD scale can serve for this purpose, but you should keep in mind that this scale is a phenomenological parameter, measured in the experiment, where the Higgs mass is always "turned on". So the QCD scale can also be considered proportional to the Higgs mass, and it can also go to zero if Higgs mass goes to zero.

As for numerical evidence, everything I ever saw were the calculations of mass ratios between hadrons. I've never seen an ab initio calculation from the fundamental SM parameters (if you know a reference for such an attempt, I'd like to see it). So numerical evidence basically says nothing about the nonzero-ness of hadron masses.

I am really frustrated by this being such an uphill struggle — everyone is just repeating the conventional wisdom, while nobody is able to provide serious proofs. And most often people assume that I don't know what I am talking about. I am really being tempted to write a serious paper with all the gory details of integrating beta-functions in various models, just to point out that a differential equation always comes with boundary conditions, and debunk the conventional wisdom about this.

Best, 🙂
Marko

18. **Hish**
August 22, 2014

Peter Orland, could you say a little more (or point to a reference) about the failure of Patrascioiu & Seiler's program? I only know a little of the story and that the question still appears to open on a (mathematically) rigorous level.

19. **MarkusM**
August 22, 2014

Peter,
"Nature is fundamentally conformally invariant."
Wouldn't that imply that "fundamentally" gravity is Weyl gravity (instead of Einstein gravity) as the Weyl tensor squared (more or less) uniquely leads to a conformally invariant gravitational action ? (Which wouldn't be all that far fetched, see http://www.ejtp.com/articles/ejtpv11i30p1.pdf)

20. **Peter Orland**
August 22, 2014

Hi Marko,

"How can you know this? You can't turn it off in experiment, and you can't evaluate the proton mass analytically and take the limit, because we cannot solve SM QCD equations in the IR. "

The experimental evidence is that the actual quark masses can be found through current algebra (pseudo-Goldstone Boson masses, for example). These are the masses given to the quarks by the Higgs. They are much smaller than the effective quark mass used to find other masses. You don't have to solve QCD to know this. This is fairly well-established physics. An experimentalist might laugh at our theories, but she/he takes this sort of thing very seriously.

Dimensional arguments don't work here. Any energy is proportional to any other energy. It doesn't mean both energies come from the same physics.

"I am really frustrated by this being such an uphill struggle — everyone is just repeating the conventional wisdom, while nobody is able to provide serious proofs. "

In the case, the conventional wisdom is clearly right. We really can say something about nature. The uphill struggle is due to QCD being a hard problem (hence no proofs), but it is wrong to say we don't know anything about it.

Hish,

I suggest you find P. and S.'s many papers at INSPIRE. There is far too much to summarize here.

Regards,
Peter O.

21. **Peter Orland**
August 22, 2014

Here is the link:

http://inspirehep.net/search?ln=en&ln=en&p=a+patrascioiu+and+seiler&of=hb&action_search=Search&sf=&so=d&rm=&rg=25&sc=0

22. **Anonyrat**
August 22, 2014

I can have conformal invariance in QCD even with the scale anomaly?

23. **AlsoConfused**
August 22, 2014

Marko,
"That always involves a postulate (a suitable boundary condition for the corresponding RGE). The CW mechanism does not *necessarily* break conformal symmetry — it does only if you choose appropriate boundary conditions. "
Aren't these boundary conditions coming from experiments, namely that one measures and fixes the relevant parameters at some scale and then "runs the RGE equations". Now, as experiments tell us that scale symmetry is (obviously) broken, we can exclude the case of radiative corrections maintaining it. Isn't this a peculiar/"fine tuned" case anyway ?
Moreover, doesn't conformal symmetry imply that one is in a fixed point ?
(P.S. If these questions are naive, I apologize; I only have a quite superficial understanding of this topic).

"Alternatively, you can choose to keep the minimum of the Higgs potential at zero, with no spontaneous symmetry breaking at all."
Never heard of this kind of scenario. Is there a good reference where I can read about it ?

24. **Peter Orland**
August 23, 2014

AC,

I am also also confused by those remarks.

Generally (but not always) conformal-invariant actions lead to breaking of conformal invariance. The scale appears, not because of some special boundary condition, but because of the LACK of a special boundary condition. Generic boundary conditions have broken conformal invariance. There is a conformal limit, obtained by taking the scale to zero (which is the special bc).

Anyway it's the conformal-invariant limit which is non-generic. It's best not to learn such things from remarks on comment threads (including my own).

25. **confused**
August 23, 2014

vmarko: "As for numerical evidence, everything I ever saw were the calculations of mass ratios between hadrons. "

What I recall, and it has been almost 30 years since I worked on this, is that in pure SU(3) YM on the lattice you compute some glueball masses as well as another "mass" which is related to a "string tension". Everything depends singularly on the lattice spacing but only mass-ratios are meaningful quantities (in ANY theory) so we report the glueball masses in units of the string tension "mass". The latter is now the new "dimensional" free parameter, instead of the original dimensionless coupling constant g ("dimensional transmutation"). We can measure the string tension related "mass" Ms, which means really we can measure the ratio of this "mass" and that standard block of mass in France which we call 1 kg, so this allows you to come up with actual predictions for the glueball masses.

All you seem to be saying is that you can set Ms=0, which is true but as trivial as saying you can set g=0 in the SU(3) theory and get 8 massless particles instead of the usual QCD.

Obviously no theory without mass parameters can predict a particle mass in kg, if only because the laws of nature don't know anything about that object in France which we call kg.

26. **walkloud**
August 23, 2014

vmarko,

"Even more seriously, lattice QCD is an approximate rather than exact calculation method, and can introduce all sorts of artifacts (fermion doubling is the most popular example of this), some of which we may still not be aware of. These artifacts might lead you to a conformally broken result simply as a consequence of the approximation scheme, rather than being the property of the theory itself."

I do not intend to pick upon you, rather the above statement. This is a rather naive viewpoint, especially today. Aside from the lack of proof of lattice QCD calculations belonging to the same universality class (a proof which is not special to QCD, but to any theory requiring a non-perturbative regulator), all approximations made in numerical lattice QCD calculations are well understood, and systematically improvable, meaning these approximations can be removed with refined calculations to, in principle, any desired level of precision. Fermion doubling is not one of these approximations. Fermion doubling is a well understood consequence of the "naive" implementation of fermions. This problem has been resolved for decades, with the original solution by Wilson himself, with a technique now known as "Wilson fermions". There are several other solutions that are all employed in today's calculations.

The three big systematics are 1) the continuum limit, 2) the infinite volume limit and 3) the limit of physical quark masses. All three of these systematics can be cast into the language of Effective Field Theory and hence, order by order corrections due to being away from these limits can be systematically described and parameterized in a model independent way, and then these corrections can be extrapolated away. The order in perturbation theory needed to control this extrapolation depends upon the desired level of precision of the final result. State of the art lattice calculations are now performed with the light (up,down) and strange quark masses basically at their physical values. Three or more lattice spacings are used and several volumes are used.

These calculations have demonstrated, for example, that ~95% of the mass of the proton comes from glue – meaning the calculations were performed at several values of the current quark masses (and lattice spacings and volumes), and then the proton mass was extrapolated to the point where these quark masses are zero, with still ~900 MeV for the mass of the proton. This is not a pen-and-paper proof that the mass of the proton comes from QCD (and not the Higgs vev), but this is a numerical proof. Taken alone, you may have your doubts, but lattice QCD calculations have now been used to make several predictions (not post-dictions) that have all been experimentally confirmed. Most of these lie in the realm of heavy quark physics. There is an extreme preponderance of numerical evidence now that QCD is the correct theory describing the IR realm of strong interactions (hadronic and nuclear physics).

Since this is already a bit long – look at any of the proceedings of the annual international lattice conference in the last few years. To come up with an alternate theory that shows all the properties observed in nature, can reproduce all the results of lattice QCD, yet does not poses "dimensionful transmutation" would really require a "conspiracy theory".

Regards,
André

27. **walkloud**

August 23, 2014

confused:

“Obviously no theory without mass parameters can predict a particle mass in kg, if only because the laws of nature don’t know anything about that object in France which we call kg.”

In fact, this is precisely the point. The QCD Lagrangian with all quark masses=zero (pure QCD? or is pure QCD just Yang Mills – I am unfamiliar with this jargon), is conformally invariant. But the dynamics of the theory generate a mass scale, Lambda_QCD. This length scale is approximately the confinement scale of the theory, and all states of this theory will have a mass proportional to Lambda_QCD – hence the generation of a mass scale. In physics, this was coined “dimensional transmutation”, I believe by Sidney Coleman. This mechanism, as noted above, is how QCD solves its IR problem. Without confining the gluons, QCD would be IR divergent, and thus not a sensible theory.

Now, relating this mass to something in kg requires more subtle questions involving atomic physics and units. But the point is that a theory with a conformally invariant Lagrangian can have a mass scale and be used to predict the mass of states of the theory. These predictions may require the aid of a computer, but they can still be made.

Regards,
André

28. **vmarko**
August 23, 2014

Oh, wow, it appears that we are finally getting somewhere!

AC,

“Aren’t these boundary conditions coming from experiments, namely that one measures and fixes the relevant parameters at some scale and then “runs the RGE equations”. Now, as experiments tell us that scale symmetry is (obviously) broken, we can exclude the case of radiative corrections maintaining it. Isn’t this a peculiar/” fine tuned” case anyway ?”

YES! You are RIGHT! It is experimental data that provides us with the correct boundary conditions. My only point is that the theory itself *doesn’t*! The theory *allows* a maintained conformal symmetry, but this is not interesting (or, as you say, a peculiar case) because in the real world conformal symmetry is broken! The theory doesn’t say it must be broken, the experiment does! And this is in direct contradiction with agravity approach (and the like) which claims that breaking of conformal symmetry is required by the theory. It isn’t — you have to postulate the breaking (in the theory), because experiment says it is broken. And this is in direct contradiction with Peter’s slogan. Thanks for noticing!

Peter Orland,

“Generic boundary conditions have broken conformal invariance. There is a conformal limit, obtained by taking the scale to zero (which is the special bc).”

Thank you too! We agree that the conformal limit can exist, and that it is not excluded by the theory itself (but rather by experimental data). As I explained above, this is in contradiction with the agravity approach and Peter’s slogan.

Confused,

“All you seem to be saying is that you can set Ms=0, which is true but as trivial as saying you can set g=0 in the SU(3) theory and get 8 massless particles instead of the usual QCD.”

And thanks to you as well! That is indeed all I am saying! The theory allows for the case Ms=0. Trivial or not, the theory itself does not exclude this possibility. The fact that Ms is not zero in nature is therefore not a consequence of the theory, but comes from experiment. Therefore, to make theory match experiment, one needs a theoretical postulate that Ms is not zero. This contradicts agravity approach and Peter’s slogan.

Walkloud,

“But the dynamics of the theory generate a mass scale, Lambda_QCD. This length scale is approximately the confinement scale of the theory, and all states of this theory will have a mass proportional to Lambda_QCD – hence the generation of a mass scale.”

My statement is that the theory (the theory alone) allows for the case where Lambda_QCD iz zero. This would correspond to to strong coupling asymptotically flowing to one as we run it further and further into the IR regime. As unusual as it might seem, the only thing I want to say is that there is nothing in the theory that forbids this case. It is rather an *experimental* fact that the value of Lambda_QCD is nonzero, and consequently we have a mass scale via dimensional transmutation. And as I explained above several times already, this is in contradiction with the point of view of Strumia, Woit and others, who claim that nature is in fact conformally invariant, but that this symmetry must be broken because the theory dictates it. My whole point throughout this long discussion is that the theory *does not* dictate it, rather the experiment does. Hence the contradiction with Peter’s slogan.

I can see now a small glimmer of hope that I am actually getting my point through to people here… 🙂

Best, 🙂
Marko

29. **confused**
August 23, 2014

Marko, I think everyone is well aware of what you’re saying but considers it too trivial to mention every time. By analogy, when someone states that QED explains the Coulomb force people usually leave out the obvious caveat that the coupling alpha=1/137 is not predicted by the theory and there is no a-priori reason why it could not be zero with no Coulomb force.

But maybe I’m not understanding something subtle. For example I am reading in the statement of the millennium problem (http://www.claymath.org/sites/default/files/yangmills.pdf):

“Prove that for any compact simple gauge group G, a non-trivial quantum Yang–Mills theory exists on R^4 and has a mass gap $\Delta > 0$”

I don’t understand the question. In what units is \Delta supposed to be expressed?
The only computable quantities are mass (or energy) ratios.

30. **walkloud**
August 23, 2014

Hello Marko and confused,

"My statement is that the theory (the theory alone) allows for the case where Lambda_QCD iz zero. This would correspond to to strong coupling asymptotically flowing to one as we run it further and further into the IR regime. As unusual as it might seem, the only thing I want to say is that there is nothing in the theory that forbids this case. It is rather an *experimental* fact that the value of Lambda_QCD is nonzero, and consequently we have a mass scale via dimensional transmutation."

This is not correct. You DO NOT need experiment to be able to know if the theory develops a mass gap (is Lambda_QCD > 0). You need to be able to solve the theory in the IR, which is what no one knows how to do with pen-and-paper. All SU(N>1) gauge theories are known in perturbation theory to exhibit properties indicative of dimensional transmutation – ie their beta functions are all negative, indicating they do develop a mass gap (Lambda_QCD > 0). Of course, this is perturbation theory, so not a proof in the IR regime.

This is where (numerical) lattice field theory enters. The lattice calculations are of course performed in terms of dimensionless variables (to confused's question – you can only compute ratios of energy scales without further input). What lattice calculations unambiguously show is that the theory does develop a mass gap, with a highly non-trivial spectrum. Add quarks to the calculation, and the spectrum becomes significantly richer. The calculations show the spectrum does not become trivial as the systematics are removed. This does not require experimental input.

Experimental input is needed to fix the absolute scale, so that the resulting dimensionless spectrum determined with the computer can be converted into units we are familiar with. In case one is concerned that somewhere between the scale at which the calculations are performed, and the UV, there is a non-trivial fixed point where the theory becomes conformal, the calculations can be, and are performed at scales where perturbation theory is reliable, so that analytic perturbative results can be matched to the non-perturbative numerical calculations, thus providing a smooth transition between the UV and IR. Yang-Mills SU(N) gauge theories, including those with small-enough number of dynamical fermions (such as QCD), do not have non-trivial fixed points where they become conformal. There is, however, an active area of research where people are adding more fermions to the theory, to see if they become nearly conformal.

The theory (Yang-Mills + small enough number of mass-less fermions), defined as the Lagrangian, does not tell you if Lambda_QCD is zero or non-zero. However, the solution to the theory does tell you Lambda_QCD is non-zero.

Regards,

André

31. **Peter Woit**
August 23, 2014

Andre and others,
Thanks for trying to get this straight, but I don't think anyone is going to convince vmarko on this question of our understanding of QCD. So, no more about QCD's breaking of conformal symmetry, that topic has been beaten to death.

32. **ConformallyCyclic**
August 24, 2014

I lately read part of Penrose's new book "Cycles of Time" which is also concerned with conformal invariance.
There is one thing that troubles me, which he addresses in a in a footnote, namely the role of the conformal anomaly. He states:
"There can, however, be an issue with regard to what is referred to as a conformal anomaly, according to which a symmetry of the classical fields (here the strict conformal invariance) may not hold exactly true in the quantum context. This will not be of relevance at the extremely high energies that we are concerned with here, though it could perhaps be playing a role in the way that conformal invariance 'dies off' as rest-mass begins to be introduced."
Unfortunately I do not understand his argument which seems crucial for thee whole scenario to work. Can anybody help me ?

33. **db**
August 24, 2014

Astonished that Dilbert of Sunday Aug 24 has not received a mention yet.

Quick Links

August 25, 2014
Categories: Uncategorized

Now back from the West Coast, here's a list of things I've run across that may be of interest:

- One piece of news from Berkeley is that Peter Scholze will be there this fall, giving a course describing new techniques for dealing with Langlands conjectures for number fields in an analogous manner to ones successfully used in the function field case. The course announcement (described by David Ben-Zvi as "shockingly futuristic") is here.

  Since it's never too early for Fields Medal predictions, I'm predicting an award for Scholze at the next ICM, in Rio, August 2018.

- It also wouldn't surprise me if Scholze gets one of the $3 million Milner/Zuckerberg breakthrough prizes in mathematics before then. I'm pleased to see that last week there was an announcement that some of the winners of this year's math prize have banded together to fund graduate math fellowships in developing countries.
- On the physics front, rumor from Peter Coles is that:

  > I have it on very good authority that Planck's analysis of the Galactic foregrounds in the BICEP2 region will be published (on the arXiv) on or around September 1st 2014.

  so next week we may (or may not…) find out if BICEP2 was seeing primordial gravitational waves or just dust.

- In the meantime, this week there's COSMO 2014, a cosmology conference going on in Chicago. The conference organizers have decided to have their public event mix artists and scientists, brought together around the topic of Multiverse: Fact, Fictions and Fantasies.
- Also in the Chicago area, Fermilab last week hosted a Nature Guiding Theory workshop, considering the question of what to make of the failure of the LHC to find SUSY or other physics predicted by the "naturalness" paradigm. Some of the discussion was of the conventional SUSY or multiverse variety. For instance, see here, or Raman Sundrum's Super-Natural vs. Other-Worldly in Fundamental Physics, which ends up arguing that:

  > Naturalness, anthropic selection, Multiverse are *Meta-theories*. The collection of naturalness-related experiments – LHC, flavor, axion searches, tests of Inflation (e.g. BICEP2, …), Dark Matter search, form a *Meta-experiment*.

  I'm not sure what a "Meta-experiment" or "Meta-theory" is, other than that it's not the conventional sort of science where you have the usual notions of how to make scientific progress.

  Some talks dealt with dumping the "naturalness" argument in favor of ideas about fundamental conformal invariance that I mentioned here. See for example here and here. By the way, Natalie Wolchover's excellent article on these ideas has been given by Wired the title Radical New Theory Could Kill the Multiverse Hypothesis.

- Going on at the same time as the cosmology conference in the Chicago area this week is a Fermilab workshop on Next Steps in the Energy Frontier. Today there were talks on future plans from Fermilab, CERN and the Chinese. The Chinese now have a Center for Future High Energy Physics directed by Nima Arkani-Hamed and are talking about a huge new machine that would start off as an electron-positron Higgs factory in 2028, then become a 50-90 TeV proton-proton collider in 2042. CERN is discussing similar plans, although they will have the high-luminosity LHC keeping them busy through 2035.

  On Thursday Arkani-Hamed will end the conference with a talk at 12:15 entitled "Go Big or Go Home…" (the US I guess already has decided to go home, with no plans for a big new machine). He really is ubiquitous at fundamental physics conferences in a variety of areas, since at 9-9:45 that morning he'll be addressing COSMO 2014 up in Chicago on the topic of "Cosmological Collider Physics". This Tuesday night he'll be at the other public event there, a showing of the movie Particle Fever, which stars him discussing the multiverse.

- There seems to be little scientific news anymore about string theory, but it is everywhere in popular culture. This past weekend it made Dilbert, it's playing a big role in DC superhero comics, and a few weeks ago some peculiar British TV show designed to torment young "child geniuses" had string theorist Brian Wecht bringing an 11-year old to tears with questions on the subject.
- John Horgan has put online an interesting interview with Carlo Rovelli, following up on one with George Ellis. Both Ellis and Rovelli criticize physicists for knocking philosophy. On the whole I'm quite sympathetic to what both Ellis and Rovelli have to say, although I think the problem with string theory is not really the "philosophical superficiality" that Rovelli sees as the problem. Jerry Coyne has a blog posting criticizing Rovelli as an "accomodationist". Note that if you want to argue about religion, please do it at Coyne's blog, not here.
- For those interested in the metaphysical end of philosophy as applied to physics, Oxford is hosting a conference in October on The Metaphysics of Quantum Mechanics. It's organized by the Power Structuralism in Ancient Ontologies and Metaphysics of Entanglement Projects. The first of these is funded by the European Research Council, the second by the Templeton World Charity Foundation (which I've never heard of before, and wonder about its relationship to the Templeton Foundation). Entanglement is the hot topic in fundamental physics these days. The Metaphysics of Entanglement project promises to bring quantum mechanics and theology together bu (Jerry Coyne is going to hate this…)

  > bringing the research results of the above investigation to bear on our understanding of questions regarding the metaphysics of the incarnation and of the Trinity in philosophy of religion.

- Anyone interested in making some easy money might want to contact Gordon Kane. He has a letter to the editor in the latest Scientific American arguing that string/M-theory predicts superpartners visible at the LHC and complains that:

  > Predictions based on such theories should be taken seriously. I would like to bet that some superpartners will be found at the LHC, but I have trouble finding people who will bet against that prediction.

Update: One more. The New York Times today has a very good story about IMPA, the math institute in Rio de Janeiro.

Comments

1. **arithmetica**
   August 26, 2014

   It seems more accurate to say that Scholze is working with mixed-characteristic local fields as if they were characteristic-p objects, which after all is his speciality. Number fields don't really seem to be in the picture yet – though I don't disagree with Ben-Zvi's assessment!

2. **ManuelM**

August 26, 2014

There is also a video, where the agravity idea is explained at the end:
http://cp3-origins.dk/video/10655

3. **ManuelM**
August 26, 2014

Sorry, it is actually this link:
https://www.youtube.com/watch?v=pW8LJlV9VNQ

4. **lun**
August 26, 2014

Among "curious" fundamental physics announcements, you might want to comment on this rather curious press release,
http://www.symmetrymagazine.org/article/august-2013/holographic-universe-experiment-begins
What does "the holographic universe" have to do with what is actually measured?

5. **Peter Woit**
August 26, 2014

lun,
Seemed best to ignore that. I did write about it a couple years ago, see

http://www.math.columbia.edu/~woit/wordpress/?p=4572

6. **Bill**
August 26, 2014

Peter, you criticized the trend of giving all the attention to a few chosen stars and yet you do it here yourself with Scholtze. If you read his course syllabus, he is building upon the ideas of many other people, including recent ideas. He looks like the real thing though, so I wish him well. Let's wait and see.

7. **Peter Woit**
August 26, 2014

Bill,
My criticism of the academic star system is the same criticism I have of the US economy in general, a belief that having outsized rewards going to a small number of people is not healthy. This is something quite different than the question of how much attention I'd advise people to pay to certain people's work. Sure, Scholze is building on lots of other people's work, but he is doing so more successfully than others in his field. So, he should be getting a lot of attention. On the other hand, I don't think the $3 million check he's likely to get at some point is a particularly good idea, and, whatever department he ends up in, if he's paid vastly more than his colleagues, that won't be particularly healthy either (in the sense of causing him or anyone else to do better mathematics).

8. **David Roberts**
August 27, 2014

From the Quanta piece

>the new models require a calculation technique that some experts consider mathematically dubious,

anyone care to elaborate? I'm a mathematician, and I know path integrals are already mathematically dubious, though from a physics POV ok – is there some extra-dubiousness going on?

9. **David Ben-Zvi**
August 30, 2014

For those interested, MSRI has arranged to videotape Scholze's lectures (though they are taking place down at the University) and make them available on their webpage – you can find the listings here for example.

10. **jd**
September 2, 2014

Big Planck conference 1-5 December. Draft program available. Just search on Google.

11. **Peter Woit**
September 2, 2014

jd,
For a long time the story has been Planck data release on polarization in October, the conference in December to discuss. For a while now though there have been rumors of an earlier data release, of data about the BICEP2 patch. This keeps getting pushed back, and now it's September 2, with the rumored September 1 date past. Waiting for the supposed arXiv paper, or new rumors…

Use the Moment Map, not Noether's Theorem

August 28, 2014
Categories: Favorite Old Posts, Quantum Mechanics

For a fourth provocative slogan about quantum mechanics I've chosen:

> Use the moment map, not Noether's Theorem.

Pretty much every physics textbook these days explains the way symmetry principles work as:

- Start with an action functional, invariant under a Lie group G.
- Use Noether's theorem to get a conserved charge (for each element of the Lie algebra of G).

There's a short (slightly mystifying) calculation always given to derive this. I'd like to argue that this is really not the best way to think about the implications of having a Lie group act on a physical system, that for this it's better to take the Hamiltonian point of view. There the way symmetry principles work is:

- For a function on phase space (or on a general symplectic manifold) you get a vector field. This is just Hamilton's equations, giving the vector field for time evolution corresponding to any Hamiltonian function.
- The infinitesimal action of G on phase space gives a vector field for each element of the Lie algebra of G. The moment map takes an element of the Lie algebra to a function on phase space (the one corresponding to the vector field).

I'm ignoring some subtleties here having to do with the relation between vector fields and functions not being quite one-to-one.

All of the basic examples of conservation laws in physics come about this way. The action of time translation gives the Hamiltonian function, space translation the momentum, rotations give the angular momentum, and phase transformations give charge. You can get these either as moment maps, or using Noether's theorem.

The moment map however gives you much more, with phase space providing structure that is not visible just from the action. A simple example is the harmonic oscillator in 3 variables. SO(3) rotations act on the configuration variables, preserving the action, so Noether's theorem gives you 3 conserved quantities, the angular momentum variables. The moment map point of view however gives you much more. The phase space is 6 dimensional (3 positions + 3 momenta) and the Lie group Sp(6,**R**) of linear symplectic transformations acts on it, with a subgroup U(3) preserving the Hamiltonian. The U(3) includes the SO(3) rotations as a subgroup, but it is much larger (9 dimensions vs. 3), so the moment map gives you many more conserved quantities. After quantization, you learn that energy eigenstates are U(3) representations, telling you much more about them than what angular momentum tells you.

The moment map point of view also gives you quantities corresponding to the directions in Sp(6,**R**) that are not in U(3). In the quantum theory these act on the full state space (not preserving energy eigenstates) and your state space is a representation of (a double cover of) this group.

For the simplest possible harmonic oscillator, in one-dimension, Noether's theorem doesn't really tell you anything. The moment map point of view says that there is an Sp(2,**R**) acting on phase space, with a U(1) subgroup preserving the Hamiltonian. The moment map is just the Hamiltonian itself. In the quantum theory you find that the harmonic oscillator state space is a representation of (a double cover of) Sp(2,**R**), with the U(1) action on states characterized by integers, which correspond to the energy. This integrality is the essence of the "quantum" in "quantum mechanics", and it's quite invisible to Noether's theorem, but a basic fact of the moment map point of view.

In some sense this is an argument for the Hamiltonian vs. Lagrangian point of view in general. The relation between the two is that, given a Lagrangian, one constructs a symplectic structure on the space of solutions of the variational problem, and thus a Hamiltonian formalism. Noether's conserved quantities are then examples of moment maps. The problem is that typically this requires the use of constraints and the quite tricky constrained Hamiltonian formalism.

The positive argument for the Lagrangian point of view is that it comes into its own in the relativistic setting, making Lorentz invariance easy to handle by the Noether's theorem method. This is quite true, with the standard version of the Hamiltonian formalism distinguishing the time direction and breaking Lorentz invariance. There is however a less well-known "covariant phase space" point of view, where one tries to work with the space of solutions of the equations of motion as one's phase space. Only if one identifies a solution with its initial data at a fixed time does one distinguish the time direction. I've recently enjoyed reading Igor Khavkine's review article, which in particular does a great job of explaining the history of this line of thinking.

The Lagrangian also comes with the extremely seductive point of view on quantization of the path integral. This point of view works very well for dealing with Yang-Mills theory, and I spent much of my early career convinced that all there was to quantization was figuring out how to make sense of integrating over the exponential of the action. I'm now much more aware of the advantages of the Hamiltonian point of view, especially in terms of understanding quantum theory as representation theory. In some sense what one really wants is to understand quantization in a way that takes advantage of both points of view, but the relationship between them is quite non-trivial.

The discussion here has been far too wordy for most people to make sense of. If you want to understand any of this, you need equations. Luckily, I've provided lots of them and many details here, see chapters ~~12 and 13~~ for the moment map, ~~chapter 19-22~~ for the harmonic oscillator.

Comments

1. **Antonio (AKA "Un físico")**
   August 30, 2014

   Hi Peter, I have just discovered your blog.
   Question: assuming that quantum information is conserved, what (Noether's) symmetry would be related to that conservation?
   Can the moment map point of view help in answering this?

2. **Ioannes P.**
   August 30, 2014

   > "My hope is that this level of presentation will […] be useful to mathematics students trying to learn something about both quantum mechanics and representation theory."

   It is indeed! —at least to this Feynman-naïf, lowly mathematics student. Yours plus the quantum information approach are the only ones so far that have made any remotely *intuitive* sense to me, without leaving nearly as much of the usual (and inevitable) "shut up and calculate" aftertaste.

3. **Peter Woit**
   August 31, 2014

Antonio,
I don't know exactly what "quantum information" is, or the conditions under which it is conserved. From the little I know, I don't see how either the moment map or Noether's theorem have anything to do with it since these come into play when you are "quantizing" a classical system, and "quantum information" doesn't seem to involve that.

To the extent "quantum information" is counting something using integers, typically there's a U(1) group action around somewhere, explaining why you are seeing integers.

4. **João Esteves**
August 31, 2014

Hi,

Quite interesting your post. One may speculate and say that also Quantum Field Theory should be related to the representation of some group, in the same way that Quantum Mechanics is related with the representation of the Heisenberg group and that also some moment maps should give some conserved quantities. But now this representation space would most probably be the space of distributions or generalized functions and not the nice Hilbert space of L^2 functions (in fact it is its closure). And what kind of group would act on such a space? And would it be easer to define a measure on it that it is on a separable Hilbert space?

5. **João Esteves**
August 31, 2014

…just to say that I just got into this:

http://arxiv.org/pdf/hep-th/0206135v1.pdf

Very nice. It will be very helpful, as it has many intersections to what I'm interested in and working on at the moment.

6. **Peter Woit**
August 31, 2014

At least for free fields (and, more generally, linear fields in an unquantized background background) qft is based on the Heisenberg group. The difference in QFT is that the dimension of the group is infinite.

The issue of distributions vs. L^2 or some other function space arises long before you get to QFT. Even for a free particle in 1d in QM, momentum eigenstates are not in L^2, and position eigenstates are not functions at all, but distributions. One way I'd like to improve the current version of the manuscript is by doing a better job of dealing with this issue. The problem is that standard ways of doing this (e.g. rigged Hilbert spaces) introduce a lot of complexity even in the very simple basic examples I'm trying to explain.

The new problem in QFT is that the field operators depend on functions (whereas in the QM analog, the position and momentum operators only depend on a finite set of indices).

7. **João Esteves**
August 31, 2014

Yes, but in QM I suppose the use of distributions can be avoided. I even think this was the main motivation of von Neumann in writing his treatise "Mathematical Foundations of Quantum Mechanics" as an alternative, more to the ground of what was known at the time, to Dirac's description using delta functions. But in QFT as it is a theory of scattering observables have a continuous spectrum, in contrast to QM which is mostly a theory of bounded states, and in that case the commutation relations between conjugate fields are distributional. My intuition is that this must taken into account if one hopes to have a rigorous treatment of QFT.

8. **Peter Woit**
August 31, 2014

Joao,
In QM you can also just look at scattering, that's not really where QFT is different.

The problem in QFT is that your analog of an operator Q_j in QM is a field operator \phi(f), where instead of an index j, the operator depends on a function f. In QM you can multiply and manipulate operators without a huge amount of trouble (although there are problems due to operators being unbounded). In QFT you can do the same, but the problem arises because you want local interactions, so need for example to define not just an operator \phi(x) (the field operator for f a delta-function), but higher-order products of this. That's where the serious problems come from.

To bring this back to the topic of the posting (or series of postings), my point of view on this is that defining arbitrary local products of field operators is inherently difficult and ridden with ambiguities, with renormalization theory our best wisdom about how to deal with this. If one looks not at arbitrary products, but at ones that correspond to a representation of a Lie algebra, then one gets a much more rigid setup and can perhaps hope to uniquely define such products, or at least better understand exactly what the inescapable ambiguities are.

Unfortunately, this philosophy only now really works for quadratic products, which I try and work out in detail in the notes (the finite-dim versions are complete, still working on the qft part of the notes). Examples like the \phi^4 interacting QFT have no known interpretation of the interaction terms in terms of a representation theory problem. On a more positive note, the SM Hamiltonian (or Lagrangian) only involves quadratic powers of the fermion fields, so, before quantization of gauge fields and the Higgs, the problem doesn't come up. For pure gauge theory, you do get cubic and quartic terms in the non-abelian case, but there one can perhaps exploit gauge symmetry in some way. For the fully quantized theory of fermi fields coupled to the Higgs and gauge fields, you need some new idea…

9. **Antonio (AKA "Un físico")**
August 31, 2014

Thanks Peter, I have to think about the question I asked you.
I keep your email and in case those moment maps show something interesting, I might contact you again.

10. **Bill**
August 31, 2014

What you said reminded me of the issue of defining a product of distributions that Martin Hairer had to deal with in his theory which he applied to Φ^4_3 Euclidean quantum field theory. It also involves some form of renormalization.

11. **Peter Woit**

August 31, 2014

Bill,
Yes, I think this is essentially the same problem Hairer is dealing with. I see that in his paper he writes
"the mathematical analysis of QFT was one of the main inspirations in the development of the techniques and notations presented in Sections 8 and 10."

12. **João Esteves**
August 31, 2014

As far as I know the major successes of Quantum Mechanics in describing the real world are the exact solutions of the Hydrogen atom and the Harmonic Oscillator, which are bound states and as so have a phase space that is compact. And when you consider differential operators on a compact manifold you get a discrete spectrum and a countable base of eigenfunctions and so a separable Hilbert space is natural. Of course, formally you can consider the eigenvalue equation for the momentum operator on R and this gives plane waves for eigenfuntions and continuous eigenvalues, but does this describe anything in Quantum Mechanics beside a free particle?
Maybe you have a different perspective, but in my view this is one of the major differences between QFT and QM, the fact that in QFT generalized functions are unavoidable.

13. **Peter Woit**
August 31, 2014

Joao,
A large part of QM is scattering theory, and to do this you have to handle the continuous spectrum (and distributions are a good way to do it).
Again, if you look at the hard problems of QFT, they're due to the nature of interactions as local products of quantum fields. Yes, part of this story is that of problems with defining products of distributions, but it's best to understand what the fundamental nature of the problem is.

14. **Igor Khavkine**
August 31, 2014

Hi, Peter. I'm glad you enjoyed the little historical overview in my article of the ideas leading to a covariant view of phase space. As with many deep ideas, its history is a much tangled web, which I find fascinating. Thanks for the mention, btw!

15. **João Esteves**
August 31, 2014

OK, Peter, it has been an interesting discussion. Thank you for replying.
Regards.

16. **Hendrik**
September 1, 2014

Dear Peter, regarding the best mathematical framework for QFT, I favour the C*-algebra point of view, which includes the group perspective you take,
i.e. is more general, and has a well controlled representation theory.
(1) Regarding free bosons;- here one takes the Weyl C*-algebra which is a twisted discrete group algebra of the underlying symplectic space. It gives you those representations of the associated Heisenberg group where the central element maps to the identity, which is what physics wants. The "momentum eigenstates" you mention, correspond to certain states which are nonregular (i.e. they are discontinuous on the underlying Heisenberg group), but which are quite well-defined (see Verbeure on plane waves). So rigged Hilbert spaces are unnecessary. Other C*-algebras are also possible, e.g. the C*-algebra generated by the resolvents of the smeared fields.
(2) For free fermions, you need to take the CAR-algebra, which does not fit well into your group perspective, but it is a very well-behaved C*-algebra. If you have physical symmetry groups acting on the fermions, you will take appropriate crossed products, to get covariant representations.
(3) Renormalization (at least in lattice C*-models) can be understood as a procedure which moves you out of one representation into another, which is why the perturbation series cannot converge. But at the C*-level it makes sense.
(4) I agree that pointwise products of the fields (Wick products) are hard to understand – though have been rigorously constructed in the free field case. These currently do not fit well into the C*-algebra picture.
Regards

17. **Peter Woit**
September 1, 2014

Hendrik,
Thanks. The C* algebra point of view is deeply related to representation theory. For fermions I try to make clear in the notes that there is a perfect parallelism between the symplectic/Heisenberg story for bosons and the orthogonal/Clifford story for fermions. To the extent C* algebras don't equally well handle either case, that's a problem

18. **Peter Woit**
September 1, 2014

Thanks Igor, your paper was quite enlightening. The question of the relation of the Hamiltonian and Lagrangian viewpoints seems to me surprisingly still not completely satisfactorily understood.

19. **Johan**
September 2, 2014

Hi Peter,

In symplectic geometry, people often require moment maps to be equivariant (wrt the co-adjoint action), which is needed for things like Kirwan convexity to hold. Non-equivariant moment maps are nevertheless also useful in some contexts. Could you comment on the role equivariance plays for your purposes?

20. **Peter Woit**
September 2, 2014

Hi Johan,

I'm generally assuming that if possible the moment map (which is only defined up to a constant) is chosen to be equivariant, so you have a Lie algebra homomorphism from the Lie algebra to functions on phase space, which becomes a Lie algebra representation when you quantize.

When the Lie algebra has non-trivial central extensions (H^2 non-zero), then you will have non-equivariant moment maps that can't be made equivariant. This is what physicists call the "anomaly". It's usually thought of as a purely quantum effect, but you do actually see it this way even at the classical level.

The finite-dimensional groups I'm writing about in the notes all have vanishing H^2, so one can take the moment map to be equivariant. There are some comments there about the situation in infinite-dimensions, but that's mostly beyond the scope of what I'm trying to write about there.

Not sure if this addresses what you're thinking about. Quite likely you know about some interesting examples of use of non-equivariant moment maps that I'm just unaware of.

21. **Urs Schreiber**
    September 3, 2014

    The kind of anomaly given by obstructions against lifts from actions by Hamiltonian vector field to the Poisson bracket Lie algebra is typically called a classical anomaly (e.g. Arnold's book, appendix 5.A).

22. **Thomas Larsson**
    September 3, 2014

    Every finite-dimensional Lie algebra that can be embedded into gl(N) for some N can be realized as a function on phase space. Obviously, since E^i_j = q^i p_j generate gl(N) under the Poisson bracket. Classically the same is true in infinite dimensions, but quantization leads to infinitites and is problematic.

    What is perhaps not so well-known, but obvious once you think about it, is that most infinite-dimensional Lie algebras of interest in physics can also be realized as functions on a *finite*-dimensional phase space. Namely, the functions f^i(q) p_i generate the algebra vect(N) of vector fields, so every algebra that can be embedded into that can be realized as functions over phase space. Since the phase space is finite-dimensional, quantization is not a problem.

    This is a kind of first-quantized approach and not directly relevant to physics, but a slight variation of this theme actually yields interesting representations of infinite-dimensional Lie algebras.

23. **Igor Khavkine**
    September 3, 2014

    Famously, the momentum map for the action of the Galilean group *G* on the phase space of a non-relativistic particle (or N such particles) cannot be chosen to be equivariant, as long as the action of *G* on its dual Lie algebra *g** is the usual coadjoint action. On the other hand, a group cocycle of *G* with coefficients in the coadjoint representation can be used to modify the action of *G* on *g** and make it equivariant (as discussed, for instance in these slides by Charles-Michel Marle). The same cocycle (or rather its infinitesimal version) shows up in the fact that the the boost and translation generators cannot be chosen such that their Poisson bracket is zero (even though their Hamiltonian vector fields commute). Instead, their Poisson bracket is a constant proportional to the total mass of the N-particle system. This is indeed a famous example of a classical anomaly.

24. **Derek Teaney**
    September 6, 2014

    Peter,

    This is a quick not very well thought out reply.
    The symmetries of that you are talking about are manifest if one uses the
    Schwinger-Keldysh formulation where the fields are doubled. This is
    useful for discussing real time physics at finite temperature.

    For instance the action of the harmonic oscillator is

    S \sim \int dt (\dot x_1)^2 – x_1^2 – ((dot x_2)^2 – x_2^2)

    You might want want to think about it in these terms. The Keldysh
    setup is most useful close to the classical limit. See for example,

    hep-ph/0212198

SUSY Bets

September 3, 2014
Categories: Uncategorized

The LHC long shutdown (LS1) seems to be progressing on schedule, with physics collisions at 13 TeV planned for early April 2015. I'd guess the earliest 13 TeV results might appear at the summer 2015 conferences. The long term plan is to accumulate up to about 50 fb^{-1} of data per year for about 3 years of data-taking, ending in mid-2018. There will then be a year and a half shutdown (LS2), followed by data-taking at 14 TeV from 2020-2022. The plan is to end up with about 300 fb^{-1} before a long shutdown (LS3) starting in 2023.

Hopefully there will be much learned about the Higgs, and some unexpected discoveries. One of the main targets will continue to be SUSY searches, despite the negative results found so far at 8 TeV (and 25 fb^{-1}). Something to watch will be how long it takes theorists heavily-invested in TeV-scale SUSY to give up and concede that this idea doesn't work. For this, one thing to keep in mind is what precise bets theorists have made in the past.

There's a new one this week. After Gordon Kane complained that he couldn't find anyone willing to bet against SUSY, Marcelo Gleiser decided to take him up on it, with stakes a bottle of 15 year old Macallan (which goes for about $100). Marcelo seems to think he has a bet that will get him his Macallan if no SUSY is found in the run ending in 2018, but I fear he has been had. Kane specifies:

> To have a meaningful bet the LHC has to work at an appropriate energy and luminosity. It is expected to take integrated luminosity of order 300 fb^{-1} at a total energy near 13 TeV in the next run, in less than two years after turning on in early 2015. Assuming those results, signals for gluinos and/or light neutralinos and/or charginos are expected, and that's the appropriate bet.

The only problem with this is that the current LHC schedule foresees maybe 100 fb^{-1} two years after first physics in 2015, not 300 fb^{-1}. For 300 fb^{-1} the schedule says the wait is likely to be until 2023, so Marcelo is going to have a very long wait for his fine Scotch.

Here's the status of the other SUSY bets I know about, and I'd be curious to hear about any other known ones:

- Back in 2000 some theorists at a conference in Copenhagen bet (stakes $50 cognac) about SUSY being found at the LHC by mid-2010. The losers ~~welshed~~ reneged on that bet, to be fair partly because the LHC was delayed, and didn't really get going until 2010, at half design energy. A new version of the bet was made in 2011, with stakes raised to $100 cognac and a cutoff date in June 2016.
- David Gross here announced back in 2012 that he had taken bets on SUSY, paying off once 50 fb^{-1} of data have been analyzed. This would likely at the earliest be in mid-2016, same time frame as the Copenhagen bet.
- Garrett Lisi announced on Twitter back in 2009 that:

  > Frank Wilczek just bet me $1000 that superparticles will be detected by July 8, 2015. Max Tegmark will arbitrate.

  At this point it seems that Wilczek is likely out $1000, since this date will only be 3 months into the run with results available for only a small amount of data if any.

- Wilczek also has a 2013 bet with Tord Ekelof that gauginos will be found by end 2019. This one is just for some chocolate coins.
- Jacques Distler made a $750 bet with Tommaso Dorigo based on the first 10 fb^{-1} of LHC data. This was more general, Jacques would win if either SUSY was found, or something else unexpected. Jacques paid up last year, see here.
- Many theorists were highly skeptical of SUSY long before the LHC turned on. Back in 2008 Adam Falkowski assigned a probability of .1% to a SUSY discovery at the LHC, and gave Lubos Motl 100 to 1 odds for a bet about SUSY after 30 fb^{-1} of LHC data. Lubos still has his $100 since the LHC didn't quite get to 30 fb^{-1}, but he should be out the money probably sometime mid-next year.

If there are any others of these out there, let me know…

Comments

1. **Jan de Goor**
   September 4, 2014

   Peter,

   it is worth mentioning that Gordon Kane has his susy predictions in a preprint (http://arxiv.org/pdf/1408.1961.pdf). Kane has not yet answered on the following bet proposal by a good friend of mine:

   GK statement: Some superpartner will be found at the LHC.

   My friend's statement: No deviation from the standard model of particle physics or from general relativity will be found; no new elementary particle (including additional quarks, additional gauge bosons, additional Higgs particles, axions, WIMPs or superpartners) will be found; no new interaction (such as technicolor), no new fundamental parameters and no new symmetry (such as GUTs or supersymmetry) of nature will be discovered; no additional dimensions of space will be detected; no evidence against dark matter as a mixture of ordinary matter and black holes will appear; no experiment will measure values exceeding the Planck force, the Planck momentum, the Planck power or any other Planck limit; no evidence against a decrease of the cosmological constant with time will be found.

   Timing for decision: End of 2017 – or some other date of Kane's liking.

   Prize:
   If GK wins (meaning that superpartners are found at the LHC OR that any of the above statements are found to be wrong) my friend will wear a T-shirt of GK's design, with a photo exchanged as proof.
   If my friend wins (meaning that superpartners are not found at the LHC AND that all of the above statements remain valid), GK will wear a T-shirt of my friend's design, with a photo exchanged as proof.

   Such a bet between "susy or anything else new" and "nothing new" seems heavily weighed in Kane's favour. Being in the first camp, but fearing that the second is right, I am as curious as anybody else about how things will turn out. All the best to you and your blog! Jan

2. **M**
   September 4, 2014

Even if SUSY-like new physics will be seen, more likely LHC will not give enough data to establish if it is SUSY or something else.

3. **piscator**
   September 4, 2014

   Jan:

   Your friend has already lost his bet. From the CMB we already know that there is more dark matter than baryons, so the old idea that 'dark matter is just small black holes formed from baryons' is already dead. And the Planck momentum isn't very big, any macroscopic experiment can exceed it.

4. **Clyde Davies**
   September 4, 2014

   In Wales, we *renege* on bets. Please watch your language in future.

5. **Krzysztof**
   September 4, 2014

   Slightly off-topic:

   "If you have seen the movie Particle Fever about the discovery of the Higgs boson, you have heard the theorists saying that the only choices today are between Super-symmetry and the Landscape. Don't believe them (…) The Landscape surrenders to perpetual ignorance (…) Perhaps only the theory phenomenologists should be allowed to publish in general readership journals or to comment in movies."

   B. Richer, http://arxiv.org/pdf/1409.1196v1.pdf

6. **Peter Woit**
   September 4, 2014

   Clyde,
   Fixed.

   M,
   Good point. Any deviation from the SM at the LHC is likely to be greeted as evidence for SUSY.

   Krzysztof,
   Saw that last night, well worth reading, will likely discuss in a separate posting.

7. **Too Distinguished**
   September 4, 2014

   Adam Falkowski is hilarious.

8. **Anony**
   September 4, 2014

   Adam Falkowski's bet is very commendable — he really risked $10K for a mere $100, and that too while betting against SUSY, which is like the holy grail. He did this based on the 1% fine-tuning needed in SUSY as his blog says. I think the rest of the bets, some made by much more famous physicists, pale in comparison. The Jester-Lubos bet is surely THE SUSY bet to beat all bets.

9. **E**
   September 4, 2014

   Why are the bets so small? Why not some serious money like a month's salary or a year's salary?

10. **markusM**
    September 4, 2014

    E,
    right, my point.
    I guess most people are just lacking true physical instinct. (Except for Garrett Lisi of course -:)).
    As far as I can tell betting against supersymmetry is one of the most easy ways to earn money.
    Where are the bets in favour of large extra dimensions and microscopic black holes by the way ?

11. **Clyde Davies**
    September 4, 2014

    Peter: thank you.

12. **Clyde Davies**
    September 4, 2014

    Sorry, that should have been 'diolch yn fawr'. 😉

13. **Garrett**
    September 5, 2014

    I admire Frank Wilczek a great deal, and it was big of him to take the SUSY bet I proposed during a conference. Frank chose the amount and the decision date. It does now appear he chose a decision date that will come before we can get new discoveries from the second run of the LHC. To me, this seems not completely fair, but he did choose the date, and the Pacific Science Institute could use a nice $1k whiteboard. Once the first bet is settled and paid I would be happy to place another similar bet with a date further out.

14. **Anon**
    September 7, 2014

Reading through the comments, it looks like it may still be awhile before Motl is forced to pay up:

"On the other hand, you're the winner after 30 inverse femtobarns (at 14 TeV, or more than 12 TeV) if no "discovery paper" of this kind is in the waiting line."

Unless I'm mistaken, the LHC has been running well beneath 12 TeV, so no data relevant to the bet has been collected yet.

15. **tulpoeid**
September 19, 2014

I'm surprised to read that Kane thinks he can't find anyone to bet against. Actually, I was thinking recently that it's high time to search for a serious relevant bet, as one doesn't hear about these things as often anymore 😉
I wonder if he (or anyone else, for that matter) has set something up on his blog already and if he's willing to bet against non-famous physicists…

16. **John Baez**
October 1, 2014

For some reason the physicist David Ring bet me case of scotch against a check for a penny that there would be 'strong evidence' for supersymmetry by 2010. Like a true gentleman, he offered to pay up on January 2nd, 2011:

> Hello John,
>
> I believe I owe you a case of scotch. I knew they'd go over schedule at the LHC, but not by this much! (we could always double the stakes and give them another 10 years. 🙂
>
> If you are indeed in Singapore, then that makes things a bit difficult. The selection available for delivery is limited, and the prices are exorbitant!
>
> I can send a case of something nice to Riverside, but at Singapore prices I can only afford something cheap. Rather than send something you would not enjoy, I could send a single bottle of something nicer, like Johnnie Walker Blue Label or Chivas Regal Salute. If you prefer single malts I can afford 3 bottles of Macallan 12 year.
>
> Please let me know your preferences, and send delivery instructions.
>
> Cheers,
> Dave Ring

He wound up getting me a case of Laphroaig, and I still have one bottle left.

17. **Peter Woit**
October 1, 2014

Hi John,
Thanks for letting us know that story. It's interesting that there seems to be some correlation between high enthusiasm for SUSY and unreasonable expectations about the operations of accelerators. Kane's expectation that the LHC will produce 300 inverse femtobarns in the next run seems to be an extreme example.

Quick Links

September 5, 2014
Categories: Langlands

Well worth reading is High Energy Colliding Beams; What Is Their Future, by Burton Richter. Richter is one of the pioneers of designing and building colliders, and he starts off by recounting some of the history. About proposals for a 100 TeV collider he comments on the challenges of doing this at high luminosity and the danger that the cost will be prohibitive (one thing I haven't seen in these discussions is cost estimates), and asks why there is no large-scale program to develop low-cost high-Tc superconducting magnets.

He's critical of the film Particle Fever on the same grounds discussed here (its portrayal of the only possibilities as being SUSY or the multiverse). About the multiverse, he writes:

> There are two problems with the landscape idea. The first is a logic one. You cannot prove a negative, so you cannot say that there is no more to learn. The second is practical. If it is all random there is no point in funding theorists, experimenters, or accelerator builders. We don't have to wait until we are priced out of the market, there is no reason to go on.

For some mathematics news, first there's the announcement from the Flyspeck project of the completion of a formal proof version of the proof of the Kepler Conjecture by Thomas Hales. Hales is in Berkeley this week talking about something unrelated (the Langlands program) at an introductory workshop for this semester's MSRI program on geometric representation theory. I've been watching some of the videos of the workshop talks, all of which have been quite good.

Also in Berkeley this semester is Peter Scholze's course, with video of the first lecture here, notes here.

In yet more Berkeley news, in December they'll host a mathematical physics workshop on Mathematical Aspects of Six-Dimensional QFTs. Better understanding the 6d N=(2,0) superconformal theory and its implications for various lower-dimensional phenomena is the main target here, a topic that will also be discussed here in the spring (where the 6d theory is called "Theory X").

Comments

1. **HL-LHC or HE-LHC**
   September 5, 2014

   High Energy Colliding Beams; What Is Their Future, by Burton Richter – what is a better investment, HL-LHC or HE-LHC? upgrading injectors or upgrading the magnets?

2. **Peter Orland**
   September 5, 2014

   I'm sure Richter understands this much better than I do – but I don't see how high-T_c magnets would help. Isn't a huge cryogenic facility needed to pump the tunnel down to a good vacuum? I thought cooling the magnets was relatively insignificant. An accelerator physicist would certainly know the answer.

   I also thought that there were some fundamental problems with making powerful high-T_c magnets anyway (friction of vortices moving through in the material? ceramic machining?). Anybody out there know?

3. **Peter Woit**
   September 5, 2014

   HL-LHC or HE-LHC,

   It seems highly likely CERN will go ahead with HL-LHC. My impression is that the cost of that is much less than an HE-LHC project. The latter would require 27km worth of new superconducting high-field magnets, I'd assume this is very expensive. Not as expensive though as digging a 100km tunnel and equipping that with magnets, which is why Richter is worried that a 100 TeV machine may carry an impossibly high cost.

4. **Casey Leedom**
   September 6, 2014

   Thanks for the reference to Burton Richter's paper. It was wonderfully readable and enjoyable for its long-term take on the history and future of beam colliders. I wish more papers like this were written by the "old men & women of science."

5. **Martin**
   September 6, 2014

   @Peter Orland: if superconducting magnets can be run at the desired amperage without having to use liquid Helium, then this would obviously save a lot of money. High-Tc materials that superconduct at liquid nitrogen temperature (77 K) are known, but building strong and large magnets with them is still a challenge, due to a too low critical current.

6. **Peter Orland**
   September 6, 2014

   Martin,

   Hi Martin,

   Yes, liquid nitrogen costs the same as milk, so it would save money. But isn't that a small fraction of the cost of the liquid helium already needed to produce the vacuum? My impression was that most of the cryogenics is needed after preliminary pumping, to properly evacuate the tunnel. If that's true, perhaps more money would be saved inventing a better high-vacuum method (if such a thing is conceivable. I got nuthin').

   I recall something similar about the low critical current of high-T_c superconductors (due to dissipation of moving vortices? I am not sure), but I thought another problem was machining. The materials are ceramics, so making coils would also be a problem.

7. **anonimous**

September 6, 2014

From LHC insiders and people involved in data analysis, I heard that a lot of persons are not happy about the HL program, given the negative results of run 1 in terms of new particles. If the could, they would push for HE, but they cannot stop the train anymore. Accelerator physicists worked a lot on HL and now it is impossible or impractical to change to HE. In their dreams, a lot of analysts wish to have HE.

Moreover, HL means challenging backgrounds at the limit of the detectors, triggers and analysis techniques which risk to damage new particle searches. PP collisions are already dirty enough…

A discovery in Run 1 would have been much better for this program and as we stand now, HL might turn in a nightmere. Anyway, I do not know the future and maybe they turn on LHC@8TeV and new states pop out easily…
Let's wait and see, but HE would have been much better.

8. **rss**
September 6, 2014

In the interests of procrastination, I looked into Peter Orland's question. I think the short answer is, the beam line indeed has to be maintained partly at cryogenic temperatures, to achieve the ultra-low vacuum required. But the volume of the beam lines is "only" 150 cubic meters, whereas the volume of the insulating vacuum for the cryomagnets is 9000 cubic meters. So if high Tc superconductors could be made practical for this purpose, the cost savings could be significant.

http://www.lhc-closer.es/1/4/15/0

9. **Peter Orland**
September 6, 2014

rss,

Thanks for looking into it. It sounds like a sensible answer…

10. **Anonyrat**
September 7, 2014

Ha, seems like the next great accelerator will be built on Pluto! (33-55K surface temperature).

11. **HL-LHC or HE-LHC**
September 7, 2014

since there's no sign of SUSY or DM @ 8TEV, I would think HE-LHC would be the better ROI over HL-LHC with limited funds

12. **Peter Woit**
September 7, 2014

HL-LHC or HE-LHC,
I think the point is that with limited funds one can't afford HE-LHC, at least not anytime soon (but I haven't seen any cost estimate for HE-LHC, this is a guess).

13. **J.F. Moore**
September 8, 2014

Peter Orland,

There are many technologies to achieve the ultrahigh vacuum required to reduce scattering to a negligible level for LHC or future colliders. LHC for example uses ion pumps and non-evaporable getters in the 'warm' non-cryo sections. CLIC is one proposal for a linear collider that uses normal conducting RF, so no need for cryo facilities at all. Modern turbomolecular pumps are more than capable of reaching the required vacuum, its just a question of scale and expense and system maintainability.

14. **srp**
September 8, 2014

Nice to have Burton Richter supporting my-oft made and nearly always ignored point that the only rational course of action on the energy frontier is to put major effort into advanced accelerator concepts such as wakefields. Peter even managed to skip that primary takeaway in his summary, but here is Richter's bottom line:

"I am both more optimistic and more pessimistic about e+e- colliders. More optimistic because accelerating gradients of more than 50 GeV per meter (50 TeV per kilometer sounds even more exciting) have already been demonstrated in plasma- wakefield acceleration and of several GeV per meter in laser acceleration, though both have now poor 6-dimensional phase space; more pessimistic because I don't see a push to develop these technologies for use in real machines. The e+e- colliders have two advantages over the proton colliders. The cross sections of interest are all of comparable orders of magnitude. The background of 10 billion or more uninteresting events for each interesting one, the problem of proton colliders, does not really exist for the electron colliders. There is a low transverse momentum fizz that is confined to small angle, but the interesting events are much easier to get at. In addition the equivalent mass reach in the electron colliders only requires 10% to 20% of the energy of the proton collider with the same mass reach. The 100-TeV p-p collider is matched by a 10- to 20-TeV electron collider. My challenge to the electron accelerator community is to produce a cost effective system with an acceleration gradient of at least 1-GeV per meter with reasonable transverse phase space and an energy spread of no more than 10% to 20%. Because of the parton distribution in the proton, the effective energy spread in p-p collisions is more like 100%. You have about 15 years to do it since that is the time to when HL- LHC will start to operate."

15. **plm**
September 13, 2014

About the multiverse/anthropic principle/landscape:

The electroweak standard model has parameters just like vacua in the string landscape are. Yet it makes lots of predictions/postdictions.

Picking a vacuum assuming the anthropic principle (or not) still leaves alot of predictions from string theory, independent of vacuum-picking -which have so far proved wrong, for the nonpostdiction part.

Assuming the anthropic principle is really more of philosophical attitude, empty of consequences. It has probably very little effect on how research is conducted. People (will continue to) try to understand better string theory or whichever theory they like, hoping to constrain more their predictions. Any sensible researcher liking the anthropic principle will not give up his research because of this as far as I can tell -I may be wrong, if you have examples please share.

16. **Ray**
September 13, 2014

Hi Peter, off-topic question but I think one in which you take an enormous interest – do you have any idea when the Planck polarization results are going to be published? I had heard a rumour that they were going to be published around September 1.

17. **Peter Woit**
September 14, 2014

Ray,
I don't know about "enormous", if so I might have better sources of info about this… From what I've seen, what's publicly known is that this conference

http://www.cieffeerre.it/Eventi/eventi-in-programmazione-nel-2014/planck-2014-the-microwave-sky-in-temperature-and-polarization/PLANCK-2014

to discuss the results has been scheduled for a long time for December 1-5. In many places I've seen "late October" mentioned for the release of the data that would be discussed at that conference.

Frank Wilczek on twitter and Peter Coles on his blog had very definite rumors about an early release of data specific to the question of the BICEP2 patch of sky. It's now long past the dates claimed in those rumors, and getting to the point that it would not seem to make sense to have two closely spaced releases, so at the moment, my uninformed guess is that "late October" is when we'll hear something.

18. **Shantanu**
September 17, 2014

Peter, something OT.
are you planning to attend David Gross's colloquium at NYU this week?
http://physics.nyu.edu/events.php?EventsPage=
If you do, let me know how it was.
Thanks

19. **Bob**
September 17, 2014

Nice to see about the formal proof. I've always wondered why this hasn't been done more, after all who knows how much of mathematics has errors that have gone undetected, it would hardly be the first time …

20. **Peter Woit**
September 17, 2014

Shantanu,
I hadn't noticed that was happening. Unfortunately I'll be teaching at that time, can't attend. Maybe someone else will report.

21. **Franck Nadaud**
September 19, 2014

Dear Professor Woit, greetings from Paris, France !

Here is a paper about high energy particle accelerators and log-periodicity:

http://cybergeo.revues.org/14173

What is interesting are the figures about costs, but the paper is in french althrough an english version may have been published in a geography review.

The paper was presented at a seminar in France about "scale relativity theory" of the french astrophysician Laurent Notale.

However, the figures relate energy levels of past accelerators and costs in current dollars I guess.

All the best regards.

Franck

Various and Sundry

September 17, 2014
Categories: Uncategorized

- The MacArthur Foundation today announced "Genius" grants of $625,000 to 21 people, including two mathematicians, Jacob Lurie and Yitang Zhang. While there was a time these awards often went to mathematicians and theoretical physicists (the 1987 winners included string theorists Dan Friedan, David Gross, John Schwarz and Steve Shenker as well as mathematicians Robert Coleman and David Mumford), that has been much less common in recent years.
  Zhang, now a professor at the University of New Hampshire, is a perfect candidate for the award, unrecognized by academia (he worked at a Subway for a while) while he was doing brilliant and important work in number theory. Lurie is undeniably a genius, but kind of the opposite of Zhang, someone whose talents and work have been very well-recognized and rewarded already. He's a Harvard professor and in November will be collecting a $3 million Milner-Zuckerberg Breakthrough Prize.
  The Wall Street Journal leads off their story about this with Lurie, characterizing him as "A mathematician offering his book free on the internet", implying that's what distinguished him for the award from the other possible genius candidates:

  > This year's winners span in age from 32 to 71 and include nine women and 12 men. A common thread: The winners reach their audiences in surprising places.
  >
  > "This year, we have several people who one might describe as being engaged to challenge the rest of us to be lifelong learners outside the traditional classroom," said Cecilia Conrad, who directs the fellows program as a vice president of the foundation. "It's new solutions to old problems."
  > …
  > Mathematician Jacob Lurie, who was honored for redefining models in algebraic geometry, negotiated with his publisher to make his book on math principles available for free download on his personal website. While academics sometimes place papers online free, putting a whole book online isn't yet standard practice, according to the 36-year-old Harvard University professor. "From my point of view, the benefit of writing a book is for people to look at it. I would like as many people as possible to look at it," he said.

  The book in question is the 2009 944 page Higher Topos Theory, available on Lurie's web-site here. He has just put up on his website an updated version of his second book, the 1178 page Higher Algebra. For those mathematicians worried that they might have trouble reading these because of a lack of physics background, Lurie himself reassures people here that

  > Since no knowledge of modern physics was required to write any of these books and papers, I can't imagine that you need any such knowledge to read them.

  In other Lurie news, he has also just put up on his web-site an important new paper, a first draft of joint work with Gaitsgory on the proof of Weil's Tamagawa number conjecture for function fields.

- Skepticism about string theory and the multiverse abounds these days. A wonderful New York Times profile of Peter Higgs ends with

  > This has led some theorists to propose that our universe is only one in an ensemble of universes, the multiverse, in which the value of things like the Higgs is random.
  >
  > Asked about that, Dr. Higgs lit up with a big grin. "I'm not a believer," he said.
  >
  > "It's hard enough to have a theory for one universe."

  In Scientific American, George Ellis has a piece entitled Why the Multiverse May Be the Most Dangerous Idea in Physics.

  Meanwhile, from a Templeton Foundation-financed conference on the Philosophy of Cosmology in the Canary Islands, Sean Carroll reports via Twitter that string theorist Tom Banks is arguing that "string theory has failed as a theory of our world."

- Commenter Shantanu pointed out something I hadn't realized, that David Gross will be here in New York this week, giving talks at NYU. Unfortunately I won't be able to attend, in particular I have to teach at the time of his colloquium on Tuesday. Perhaps someone who can attend will report what he has to say.
- In Grothendieck news, the English translation by Melissa Schneps of Winfried Scharlau's book on the later period of Grothendieck's life has started to appear, see some chapters here.

  A wonderful book of articles about Grothendieck's mathematics, Alexandre Grothendieck: A Mathematical Portrait, edited by Leila Schneps, has recently been published (with a version of the articles also available here).

Update: Vigorous back-tweeting (see here, here, here, here and here) now going on from Sean Carroll and Tom Banks. Admitting that string theory unification has failed is just not done. Revised and extended remarks from Tom Banks add praise for the greatness of string theory and avoid the word "failure":

> Without string theory we would never have been in a position to understand anything serious about quantum gravity, but without going beyond the present understanding of string theory we can make no further progress.

This still though reads as "string theory is at a dead end". Sean echoes the praise:

> conventional string theory has given us enormous guidance toward quantum gravity.

and dismisses the failure issue as something obvious and not worth mentioning:

> Need to go beyond is obvious.

I guess I should point out that it is not obvious that you need to go "beyond" string theory, in the sense of farther in the same direction. Might be that you need to abandon the direction that led you to a dead end, back-track, and try a different one.

Strange thing is that this discussion of a string theory dead end just seems to be about problems using string theory to do quantum gravity when there's a positive cosmological constant (otherwise, according to Carroll, "String theory is great"). The idea of string unification seems to be so dead it's not even worth mentioning.

Update: Shaun Hotchkiss has the latest news on Planck/BICEP2 and dust here. People are still scraping data off old Planck slides, with real Planck data on the BICEP2 patch rumored to be imminent for the past month or so:

> Any day now we are to expect Planck's paper revealing the non-conference-talk maps of the high frequency polarisation signal along BICEP2's line of sight. These will just be images though, not raw data. The word on the street/corridor is that a fully written draft exists and has clearance to be submitted and nobody I've spoken to knows why it hasn't been. The sort of phrases I've heard about what to expect from this is that "it will clarify a lot of things", but "it won't be conclusive".

Update: Two more. I've been avoiding writing about the AMS-02 announcement about the positron excess, waiting to hear something sensible about its significance. Resonaances is on the job, giving an interesting take on the data, and claiming this has nothing to do with dark matter.

There's a nice profile of Robbert Dijkgraaf here.

Comments

1. **george ellis**
   September 17, 2014

   Peter, you link to an article by me on multiverses that has appeared in SciAm with title "Why the Multiverse May Be the Most Dangerous Idea in Physics." This just shows the dangers of having subeditors assign titles to what you write, without consulting when they do so. I do not agree with that title, and disassociate myself from it. What is dangerous is weakening the criteria for what science is. Multiverses are only dangerous to science if they are used to motivate that move. String theory is of course another theory that has also been used to motivate that move. It is that move that is dangerous to science, not the theories that are defended in this way.

2. **Peter Woit**
   September 17, 2014

   George Ellis,
   Thanks for the clarification. My own version of this point would be that the problem with the multiverse is that it is being used to cheat, to evade having to acknowledge the failure of the idea of string theory unification.

3. **Jon Orloff**
   September 17, 2014

   I really dislike the term "genius awards," as the word genius is, I think, greatly overused. Aristotle was a genius. Newton was a genius. J.S. Bach was a genius. If you do something comparable to their work, you deserve to be called a genius.

4. **Michał Kotowski**
   September 17, 2014

   As for the Grothendieck news, I would like to stress that the fund-raising effort to collect money needed for the translation is still ongoing – see http://www.gofundme.com/7ldiwo (still almost $3000 to go…). Please contribute and spread the word!

5. **HL-LHC or HE-LHC**
   September 18, 2014

   "Sean Carroll reports via Twitter that string theorist Tom Banks is arguing that "string theory has failed as a theory of our world."

   can you clarify Tom Banks' view? is it that string theory is wrong, or is it not even wrong? what evidence or arguments persuade him? is he abandoning string theory for some other line of research? what is his preferred BSM and QG research?

6. **Casey Leedom**
   September 18, 2014

   George F. R. Ellis,

   Ignoring the editorial sub-title that was thrust upon your article, I'm confused about one sentence in it: "Astronomers are able to see out to a distance of about 42 billion light-years, our cosmic visual horizon." I thought we were only able to see backwards in time to about 13.7 billion years. Even if we double that for an edge-to-edge, 180° view we still only get 27.4 billion light years. Where's the "42" come from?

7. **S. Molnar**
   September 18, 2014

   Zhang recently gave a public lecture at IAS. I can't find the video on the IAS website, but here is a link on another website:
   http://video.ust.hk/Watch.aspx?Video=E05D054DB6D058F6

   Casey Leedom: I think we all know where the "42" comes from, but it's news to me that it has units of billions of light years.

8. **S. Molnar**
   September 18, 2014

   Correction: It must be a different IAS than the default, which confused me.

9. **Sesh Nadathur**
   September 18, 2014

   Casey: the distance we can see out to is not simply (age of the universe) x (speed of light). Perhaps this will help. (The precise numbers depend on the values of cosmological parameters, which have been updated since Planck though that page hasn't, hence the difference of 47 vs 42.)

10. **george ellis**
    September 18, 2014

    Casey Leedom: its basically because the expansion rate has not been constant over time. The universe was expanding faster at earlier times. You have to do an integral of 1/a(t) where a(t) is the scale factor to determine how far we can have seen. This gives a factor of about 3 extra over the present Hubble scale.

11. **george ellis**
    September 18, 2014

    The full details are here:
    http://www.scribd.com/doc/235971566/G-F-R-Ellis-T-Rothman-Lost-Horizons

12. **Peter Woit**
September 18, 2014

HL-LHC or HE-LHC,
Use of the "failure" word seems to have gotten Banks in trouble, see the update to the posting. Banks for a long time has had his own alternative to string theory "Holographic Space-Time", see for example
http://arxiv.org/abs/1109.2435

13. **HL-LHC or HE-LHC**
September 18, 2014

@Woit
yeah at the time of the comment i read the original post before the update.

It appears Tom Banks agrees with you on string theory unification.
I know this is outside your expertise, but is string theory a successful theory of QG, or has string theory provided us with a important insights into QG? Should string theory research continue on as a theory of QG – irrespective of its theory as GUT type unification?

14. **Geoff**
September 19, 2014

Is there some unspoken agreement that even the preface to a mathematical monograph has to begin with "Let…… " ?

15. **Peter Woit**
September 19, 2014

HL-LHC or HE-LHC,

My non-expert impression of string theory as QG research is that the last significant new idea was AdS/CFT 17 years ago, and that much of the work since then has been based on trying to figure out how to use gauge/gravity duality to think about QG. Both Banks and Carroll seem to be admitting that this doesn't work as hoped, at least for positive cosmological constant. The "firewall" business of recent years is based on the realization that it doesn't work to solve the black hole information paradox as hoped.

What's interesting about the Carroll tweets I think is that they show what happens when a realistic description of the situation slips out. This must quickly be remedied by larding it with hype.

People who have ideas about how to make progress on string theory as QG should do so. People who don't should consider working on something else.

16. **jd**
September 20, 2014

There is an article on AMS-02 on CNN. The publicity machine seems to be alive and well.

17. **Peter Woit**
September 20, 2014

jd,
Yes. Headlines like "AMS Space Experiment Sees Hints of Dark Matter Particles" can't be blamed on journalists. CERN put out a press release
http://press.web.cern.ch/press-releases/2014/09/latest-measurements-ams-experiment-unveil-new-territories-flux-cosmic-rays
claiming "tantalizingly consistent with dark matter particles with mass of the order of 1 TeV" and the APS summarizes the AMS-02 PRL as "More Dark Matter Hints from Cosmic Rays?"
http://physics.aps.org/synopsis-for/10.1103/PhysRevLett.113.121102

At Resonaances, Jester explains the actual situation:
"The dark matter explanation is unlikely for many reasons. On the theoretical side, the large annihilation cross section required is difficult to achieve, and it is difficult to produce a large flux of positrons without producing an excess of antiprotons at the same time. When theoretical obstacles are overcome by skillful model building, constraints from gamma ray and radio observations disfavor the relevant parameter space. Even if these constraints are dismissed due to large astrophysical uncertainties, the models poorly fit the shape the electron and positron spectrum observed by PAMELA, AMS, and FERMI (see the addendum of this paper for a recent discussion). "

Job Action at the Journal of K-theory

September 20, 2014
Categories: Uncategorized

Back in 2007 I wrote here several times (see for example here and here) about the story of the resignation of the editorial board of the Springer journal K-theory in favor of a journal published by Cambridge, called the Journal of K-theory. For a detailed history of this, see Eureka Journal Watch.

At first this story fit in with the narrative of a group of mathematicians banding together to do something about high journal prices, but the actual story was much murkier. There never seemed to be any evidence that anyone had tried to negotiate a lower price with Springer. The editorial board resigned in January 2007, but the managing editor Anthony Bak had stopped sending papers to Springer in April 2006, and the resignation wasn't made public until August 2007, a sequence of events that left some submitted and refereed papers in limbo.

The actual financial arrangements between Bak and Springer were never made public, and Bak was supposedly suing Springer for a significant amount of money, on grounds that also were never disclosed.

Wolfgang Lueck and Andrew Ranicki took over the task of dealing with the manuscripts in process at the Springer journal, and you can read Lueck's account of that here.

When the new "Journal of K-theory" was started, there was a statement from the editors that:

> The title of JKT is currently owned by a private company. This situation is only meant as a temporary solution to restart publication of K-Theory articles as soon as possible. It is the Board's intention to create a non-profit academic foundation and to transfer ownership of JKT to this foundation, as soon as possible, but no later than by the end of 2009, a delay justified by many practical considerations.

(a more detailed version is here).

The non-profit foundation did get created, it's the K-theory Foundation and one thing it does is sponsor conferences, and award every four years prizes for work by young mathematicians on K-theory, with the first two $1000 prizes awarded this year.

The latest news though is that there has been some sort of breakdown between management (the managing editor Bak), and the workers (much of the editorial board), leading to a strike (see news from Scott Morrison). The workers are demanding that the ownership of the means of production be transferred, as promised back in 2007, from Bak's company (ISOPP) to the K-theory Foundation.

Morrison has more details here, and in the comments quotes a claim that Bak's company has been been receiving 73-74,000 pounds per year, for services that Cambridge would normally pay 20-25,000 pounds per year for. So, this appears to not just be about the technicalities of ownership, but about significant sums of money coming in from publishing math papers. At Morrison's site, Andrew Ranicki advises "Follow the money."

It seems that removing control of the income thrown off by math journals from the clutches of Springer may not solve all problems. The editors on strike say that if Bak doesn't fold, they start yet another journal.

Update: The text of a recent talk by Tony Bak describing the history of the journal is here.

From Scott Morrison, news last week was that:

> As of a few hours ago, Tony Bak is no longer the President of the K-Theory Foundation, having been removed from the board by a unanimous (excepting abstentions) vote.
>
> Editors of the Journal of K-Theory have begun contacting the authors of submitted papers to give them the opportunity to withdraw their papers, or to wait and consider the option of transferring to a new journal.

Comments

1. **Narad**
   September 21, 2014

   I am reminded that Chandrasekhar almost single-handedly engineered the transfer of ownership of the *ApJ* to the AAS. *JKT* came in at just under 1200 pages in 2013. Unless there's some serious manuscript editing going on, including high-end LaTeX skills being brough to bear, that's barely an FTE.

2. **Mathematician**
   October 16, 2014

   So, are they starting a new journal?

Quantum Field Theory: Past, Present and Future

September 21, 2014
Categories: Uncategorized

Video is now available of David Gross's colloquium this past week at NYU, which had the title Quantum Field Theory: Past, Present and Future. It's quite interesting to compare his current point of view to that of ten years ago. The earliest substantive post on this blog was this one, which reported on a similar sort of talk by Gross, of similar length, also here in New York.

If you look at that blog post, you'll see that I found myself in strong disagreement with many of the main arguments Gross was making back in 2004. Remarkably, ten years later, there's relatively little I would disagree with in his NYU talk on much the same topic. Back in 2004 he was predicting the imminent discovery of supersymmetry at the LHC, in the current talk supersymmetry was not mentioned at all. I think the negative LHC results have had a very real effect on his thinking.

His 2004 point of view on string theory was that it was a better, more fundamental replacement for QFT. His arguments for this weren't very good then (see the old blog posting), and he seems to now have wisely abandoned them. Instead, the first hour of his talk was all about the story of our increasing understanding of the power of QFT. From there, he argued that there's some larger framework that we don't understand, which includes our current understanding of QFT, as well as things like quantum states that look like strings. He likes to refer to this conjectural new framework as QFT/string theory. Interestingly, there was no reference at all to "M-theory".

Gross's current vision of the future comes down to something close to mine: some yet undiscovered new ideas will tell us something new about the QFT framework, and this will show us how to make progress on quantum gravity and unification. I'd add something more specific, that previous progress came from understanding new ways of exploiting symmetries in QFT, so future progress may very well be of that same general nature. He pointed out that the story of past QFT progress was often that people had decided that something dramatically different was needed, but ended up realizing that they just needed to solve some very technical issues, not move to something very different (e.g. proper handling of renormalization and of gauge symmetry was needed, not new degrees of freedom).

In the question and answer period Gross made clear his distaste for the string theory landscape. About all he would say about anthropics was "Oy-vey", and that it's nothing but a cop-out. He characterized the supposedly finite number of "string vacua" with stabilized moduli and positive CC as likely irrelevant, since you don't know what theory they are a solution to, and there's an infinity of other solutions to the kinds of equations you're considering.

All in all, I found watching this quite encouraging. Seeing one of the great elder statesmen of the field stop promoting failed ideas, challenge dubious received wisdom, and move on to a more promising take on where the field should be heading is cause for optimism. I hope younger theorists will pay attention.

Comments

1. **Boaz**
   September 22, 2014

   There is a brief mention to M-theory around 1:30:30.

2. **theon**
   September 22, 2014

   Yes, some are sick and tired of claims such as "Theory of Everything" (i.e. everything except the standard model), "multiverse" (a coverup for the failure to explain the Universe) and so on. More prudently, string theory can be seen as a framework that gives certain insights in quantum field theories.

3. **Hanna**
   September 23, 2014

   Peter,

   listening to the talk, I get an impression that differs from yours. My impression is that Gross is still a full believer in string theory, but that he acknowledges that critics have a point. It seems to me that his retreat is only tactical; he avoids the issues that are easily criticized, while still being convinced to be correct on the large picture. String theory is still what he wants and thinks correct. He still believes in supersymmetry and higher dimensions.

   On the other hand, maybe you are right, and there is hope for improvement.

4. **Nathalie**
   September 28, 2014

   Gross is a good speaker and fun to listen to. However, nature couldn't care less about his beliefs. They are no more than far fetched speculations involving huge extrapolations.

   He talked about Feynman diagrams but said nothing concrete. It would have been more useful if he, for example, had said something about "Amplituhedron". Is it really a breakthrough in quantum field theory, as it is advertised to be?

Planck: It's Just Dust

September 21, 2014
Categories: Uncategorized

The Planck paper with results on dust in the BICEP2 patch of sky is now out, see here. I'm sure experts will weigh in soon and I'll link to such discussions, but my non-expert take is that Planck is saying that what BICEP2 saw is likely just dust. See section 6 of the paper, especially figure 9 which appears to show that BICEP2's claimed value of r=.2 is just what you'd expect from dust.

Update: More details from Natalie Wolchover and Sean Carroll.

Looks like Scientific American will have to pulp this month's magazine, with its Lawrence Krauss cover story about how BICEP2 is experimental evidence for quantum gravity and the multiverse.

Update: For more press coverage, see Nature, New Scientist, BBC News, The Guardian, the Washington Post, the Daily Mail and the New York Times.

The best explanation for all this that I've seen of course is from a blogger, Sesh Nadathur at Blank on the Map.

Update: Jester has a sensible take on this fiasco here. It now seems that release of the full Planck polarization results has been pushed back from October to "late November", just before the early December conference planned long ago to discuss the results. The joint analysis of BICEP2/Planck data that will show if there's any evidence of something besides dust is supposed to be released at the same time.

Comments

1. **Kent**
   September 21, 2014

   Sean Carroll weighs in here:
   http://www.preposterousuniverse.com/blog/

2. **dr. kansas**
   September 22, 2014

   I never quite grasped the full genius of the band known as Kansas, until now.

   https://www.youtube.com/watch?v=tH2w6Oxx0kQ

   "Dust In The Wind"

   I close my eyes only for a moment, and inflation's gone
   My Nobel dreams pass before my eyes, a curiosity

   Dust in the wind, all inflation is is dust in the wind

   Same old song, just a drop of hype in the CMB
   Inflation crumbles to the ground, though we refuse to see

   Dust in the wind, all inflation is is dust in the wind

   Now, don't hang on, nothing lasts forever but the earth and sky
   It slips away, and all your money won't another primordial wave buy

   Dust in the Linde, all inflation is is dust in the wind (all inflation is is dust in the wind)
   Dust in the wind (inflation is dust in the Linde), inflation is dust in the wind (the wind)

3. **Avattoir**
   September 22, 2014

   Verbatim reader comment at Carroll's blog, under the reader name George Efstathiou [who looks to my eye to be either the biggest cheese at Planck or close to it]:

   "As a member of the Planck Science Team, I would urge caution concerning the interpretation. What we are saying is that polarised [sic] dust emission in the BICEP2 field is high. But it may be that there is something left in the BICEP2 signal that can be attributed to gravitational waves. We need to cross-correlated [sic] the Planck maps with the BICEP2 maps. This analysis is underway."

   BICEP2 was definitely way out of line on a) the meaning and value of the Planck slide, b) using Planck data before Planck itself was ready to publish, c) extrapolating from something they didn't understand, d) the intensity of its readings, and e) the reliability of its assertions, and that seems to me like an awful lot of basic stuff to be wrong on. But it'd be wrong as well to leap entirely in the opposite direction.

   This may well follow the members of the BICEP2 team thru the remainder of their careers (for some, still new), which means it'd be nice for accuracy.

4. **chris**
   September 22, 2014

   Avattoir,

   ATLAS and CMS hated the preliminaty combination of their Higgs search results by outsiders. They frequently told everyone not to buy any of this until the full, cross-correlated analysis was ready. and when it was, the result was virtually indistinguishable from the preliminary combinations.

   I'm quite sure it will be the same here. Just imagine: would Planck announce now that the BICEP2 signal is dead for sure, what would they tell the taxpayer why they are doing the crosscorelation analysis anyway?

5. **AdamT**
September 22, 2014

Since this was trumpeted as a prediction of inflation the failure to observe this prediction would deal a huge blow to the theory, right?

Or is that not how science works anymore?

I also remember this being trumpeted as a victory for the multiverse…

6. **Peter Woit**
September 22, 2014

Avattoir,
I think it's completely accurate at this point to say that BICEP2 has provided zero evidence for primordial gravitational waves, instead is seeing pretty much exactly the expected dust signal.

This may change in the future, based on Planck data, new BICEP2 data, and a joint analysis of the two data sets (although seeing a significant signal this way doesn't appear very likely), but that's a separate issue. I don't think it's fair to use this possibility to try and evade the implications of the bad science that BICEP2 has done, promoted by press conference, and gotten on the front pages of prominent newspapers and magazines.

This is a perfectly good example of normal science: a group makes claims, they are checked and found to be incorrect. What's not normal is a massive publicity campaign for an incorrect result, and the open question is what those responsible will now do to inform the public of what has happened. "Science communicators" often are very interested in communicating over-hyped news of a supposed great advance in science, much less interested in explaining that this was a mistake. Some questions about what happens next:

1. Will the New York Times match their front page story "Space Ripples Reveal Big Bang's Smoking Gun" with a new front page story "Sorry, these guys had it completely wrong?" Or will they bury it in the specialized "Science" section tomorrow with some sort of mealy-mouthed headline like the BBC's today that BICEP just "underestimated" a problem?

2. Will Scientific American in the next few months put out a magazine cover saying "Our October magazine cover was nonsense"?

3. Will the BICEP2 team withdraw their PRL paper?

4. Will Linde/Guth/Starobinsky return their May Kavli Prize, which was awarded with the explanation
"More evidence was provided earlier this year by an experiment at the South Pole called BICEP2 which, however, awaits confirmation by independent data. BICEP2 detected swirls in the polarisation of the CMB that are believed to be caused by the gravity waves spawned during inflation, as predicted by Alexei Starobinsky." see

http://www.kavlifoundation.org/2014-astrophysics-prize-explanatory-notes

5. etc….

Maybe things are different in the experimental world, but based on a long career of watching hype about strings, susy, extra dimensions never matched by public explanations of what went wrong, I'm not so optimistic.

7. **Jeff M**
September 22, 2014

So, I'm curious, has anyone done some sort of investigation of when "science by press conference" exploded? And why? Is it the money involved? There's always been pretty big money in physics, no? As a mathematician, it's not something I'm exposed to really, big NSF grants in math wouldn't even cover lunch in most physics grants. Off the top of my head, the first big press conference announcement of an incorrect result I can think of was the cold fusion thing, but there must be some before that, no? Particle physics always got pretty good press coverage that I remember, but you wouldn't see announcements until things had at least been accepted for publication that I remember, or close.

8. **Peter Woit**
September 22, 2014

Jeff M,
The difference with math I think is that there's just a lot more public interest in claims to have discovered a limitless source of energy, or seen back to the first gizillionth of a second of the Big Bang than there is in, for example, having proved that zeroes of a certain function lie on a certain line in the complex plane.

Unlike some other people, I don't actually have any problem with the idea of holding a press conference when you publicly release results for which there's a lot of public interest. You'll need to deal with the press, and that's one way to do it. Keeping such a result under wraps as it goes through a refereeing process is not so easily done, and putting out a preprint on the arXiv is a good way to allow your colleagues to evaluate your claims. But when you do this, you'll have to deal with press interest.

In the BICEP2 case, there would have been no problem at all if they put out a paper and press conference saying "we have a solid observation of non-zero B-mode polarization, have to wait on Planck to know if it's dust or not". Instead they convinced themselves they'd made a huge discovery and decided to throw long, making dramatic claims (5 sigma effect!) to the press. Question now is how the mess they created by doing this will get cleaned up (or will it?).

9. **Sesh Nadathur**
September 22, 2014

Peter, just on your point 3 above: the actual PRL version of the BICEP paper contains rather more careful statements about the possibility of foreground contamination and the need for follow-up checks than the gung-ho pre-print version from March.

Re the other points: concluding at this stage that a measurable gravitational wave signal does not exist would be to make a similar mistake as BICEP but in the opposite direction. Of course this does appear to be the more likely outcome, but it isn't certain yet.

10. **Grad Student**
September 22, 2014

Dear Sesh,

You write, "concluding at this stage that a measurable gravitational wave signal does not exist would be to make a similar mistake as BICEP but in the opposite direction."

Were Peter to host a major hour-long press conference and hire a film crew and visit Andre Linde's house to announce the news of the failed BICEP2 results to him, and then film him crying instead of celebrating, then, perhaps, Peter would be doing the same thing in the opposite direction, but only if he were also scraping data from a powerpoint presentation to justify the results.

11. **Winther**
September 22, 2014

Peter:
I toally agree that we need a discussion regarding how such discoveries should be communicated to the public and that scientist needs to be careful when presenting their findings. However, I think you being unfair in the rest of the critique you make. I have some comments regarding the retorical questions you presented in the comment above:
***
Point 2): "Will Scientific American in the next few months put out a magazine cover saying "Our October magazine cover was nonsense"?"
***
If you would take time to read the first line of article you would see in the first paragraph that the author talkes about what this *would imply* if it was true and not claiming that inflationary B-modes are a fact: " *If* the recent discovery of gravitational waves emanating from the early universe holds up under scrutiny, it will…". What can possibly be wrong with this?
***
Point 3) "Will the BICEP2 team withdraw their PRL paper?".
***
Why should they retract the paper? This shows little understanding about how the experimental science works. If you would take time to read the paper (and the critique of it) then you would find that the main part of their analysis has stood the test of time. They do find a B-mode signal (and Planck confirms this, but it turns out that it's likely to be dust). They also use the best availiable information about polarized dust availiable at the time and this seemed to suggest that it's small. However, I do think that they should be critiqued for claiming a too significant (7sigma) detection when the dust issue was as uncertain as it was, but this is not enough to merit retraction in my opinion.
***
Point 4): Again, Kavli says *awaits confirmation* and note that they do not get the prize just for the B modes, but for the idea of inflationary cosmology. It is still a strong consensus that inflation (or something like it) have happened and we do have other evidence for it. B modes is just the missing piece of the puzzle that would settle this for good. People coming up with an idea that has stood the test of time (40 years) and whose work has lead to a lot of development in the field deserves a prize (with or without B modes).

12. **Sesh Nadathur**
September 22, 2014

The BICEP team arguably made two different mistakes: one to do with the actual science (the over-optimistic interpretation of foreground models) and one to do with the presentation of their result to the press. I was referring to the first one rather than the second — and in any case, I'm not convinced that having the scientific process played out in spotlights actually does the reputation of science (as opposed to the reputations of some individual scientists) much harm.

13. **Jeff M**
September 22, 2014

Peter,

Really, there is NOTHING more interesting that whether the zeros of that function have real part 1/2 🙂

14. **Not a physicist**
September 22, 2014

I don't understand the technicalities at all, but can some clearer conclusions be obtained by repeating the experiment at multiple frequencies to try to separate the dust signal from a purported primordial signal?

Does this type of experiment at least have the potential to exclude (or confirm) some versions of inflation?

15. **Peter Woit**
September 22, 2014

Sesh (and Winther),
Upon better consideration, I agree that the BICEP2 PRL as published probably doesn't need retraction. I do think though that in this case the referees likely saved them from themselves, because their paper as submitted would be a much better candidate for retraction.

I don't think anyone is claiming that now we know a measurable primordial signal doesn't exist, and that such a thing is not contributing to the BICEP2 data. However, we do know that if it is in their data, they now have nothing at all that would justify such a claim. Maybe Planck will help them out to get some such evidence, but that's speculation about the future (and I suspect most people would now give long odds against it).

Winther,
The SciAm cover has nothing about "if this holds up under scrutiny". More seriously, I think publishing in September a cover story about this, even with the "if this holds up under scrutiny" caveat, was irresponsible, given that since May it had become widely known that this was NOT holding up under scrutiny. To be fair, since it's behind a paywall, I haven't read the whole piece. Perhaps later on it explains the dust issue as a serious concern.
Do you really think that the award of the Kavli prize this particular year was not influenced by the BICEP2 claims to have experimentally vindicated inflation?

16. **Peter Woit**
September 22, 2014

Not a physicist,
Yes, that's what many experimental groups are now trying to do.

As discussed here at various times, the problem with inflation is that it's only a rather general idea, which you can implement in many ways, and its seems you can get any value for r that you want, from BICEP2's .2 to hopelessly small. Steinhardt argues vigorously that because of this it's not science, but that argument is best left for another time.

17. **Curious Mayhem**
September 22, 2014

Yes, inflation models can predict “r” values over a wide range, depending on model details and basic assumptions. If it is all just dust in the interstellar wind, it means only that inflation with such large values of “r” has been ruled out, again.

And again, inflation does NOT imply a multiverse, which is just one type of inflationary model, the Linde eternal inflating universe, with nucleating bubbles (of which we are in one). And this is NOT the same as the string “multiverse,” which is multiple disjoint realities, or something along those lines.

Inflation is an idea historically prior to and logically independent of string theory. The two have nothing to do with each other.

18. **Peter Woit**
September 22, 2014

Sesh and Winther,

A New York Times story by Dennis Overbye on this is now out
http://www.nytimes.com/2014/09/23/science/space/study-confirms-criticism-of-big-bang-finding.html
He quotes Paul Steinhardt as saying that the BICEP2 PRL should be retracted.

19. **Not a physicist**
September 22, 2014

To be more concrete, is it anticipated that in the next few years, some values of r could be experimentally excluded, at least painting inflation into a corner (of parameter space).

20. **Peter Woit**
September 22, 2014

Not a physicist,
In some sense the BICEP2 result that they see what is expected from dust already provides a limit of r <.2 or something. The joint Planck/BICEP2 result in a few months will likely put a more stringent limit. Planned other experiments will take the limit down further, I don't know what the ultimate limits are. None of this though I think could be described as painting r into a corner, since an appropriate metric is probably in terms of the exponent: there are various versions of inflationary theories with very small predictions for r.

21. **Winther**
September 22, 2014

Peter, there is no doubt that the prize was influenced by the BICEP2 results, but the work they got the prize for has been so important for modern cosmology that it hardly matters: they would/should have gotten it anyway at some point. You can compare it to Higgs getting a prestigious prize before the Higgs was found – nobody would protest (and imo inflation is to cosmology what the Higgs is to particle physics : we know something like it has to be there, the question is just what exact form it takes). Importantly: it’s not the Nobel prize – that would be something I too would protest against.
–
This is still not over, but if it turns out to be just dust then I would still not agree that it is as damaging as you think. As you write yourself, this is a good example on how science is supposed to work and cases like this will also make sure that future experiments will be much more careful with what they claim (and how they present it to the media) which is a good thing.

22. **Shantanu**
September 22, 2014

Peter,
IIRC if the Bicep-2 results were correct, it kills Starobinsky’s 1980 model.
But I agree in general that he and Linde as well as Guth were awarded prizes for their pioneering contributons to inflation. Its somewhat unfortunate that Demos Kazanas never gets any recognition for his work (even though the Bicep-2 team is one of the few experimental groups to have cited his 1980 paper).
Shantanu

23. **Cormac O'Raifeartaigh**
September 22, 2014

Hi Peter, I had a quick look at Larry’s article in Sci Am, and I don’t think any pulping or revisions will be required. The piece is laced with phrases such as “The observation, if confirmed”….and other sensible caveats.
I think it’s important to emphasize that the Planck result goes some way towards suggesting that the BICEP 2 observation may be dust – this is not at all the same as establishing that the presence of primordial grav. waves in the CMB is ruled out!

24. **Neil**
September 22, 2014

The Krauss cover story does indeed contain caveats such as “…the result (or observation), if confirmed.” Normally, this sort of caveat means “if the result is replicated, or produced by some other experiment.” But given that the background noise is now shown to be sufficient to swamp any possible CMB signal, it seems to me that there is as yet no BICEP observation or result to be confirmed. Rather there is a claim, which is now shown to be unwarranted.

25. **Neil**
September 22, 2014

I meant foreground noise.

26. **jd**
September 22, 2014

When I finish an article with a hefty dose of caveats, I am most often angry because my time has been wasted. I have never written such and I ask what is the motivation of those who do. Well, such an article is another publication on one’s record and if it is in a well-read magazine then that is all the better. It adds to the fifteen minutes of fame.

27. **Avattoir**
September 22, 2014

Professor Woit’s questions, answered (some of them, anyway; two):

1. a) No. And b) No. Instead, the NY Times published a fairly meaty, non-mealy-mouthed Denis Overbye story that, per the usual Grey Lady etiquette, appeared on top of page 1 of the SCIENCE section, and has moved down during the day with subsequent editions (The headline itself still appears, but it's fading like a tropical sunset.)

2. If there's anyone in the blogosphere who can speak authoritatively to SciAm's historical position on the unraveling of string theory, it'd be you. Has SciAm EVER backed down on string? Or on any cover page article? If the answer to both is 'No', I should think the answer would apply here.

I agree with you citing Seth Nadathur for 'best explanation'; but also maybe funniest. His take on the paper (not to leave out the paper itself, pretty mealy-mouthed in its own right), that the sheer size of this galaxy neuters BICEP2 technology for use on even larger scales, sparks the image of potential practical application in the opposite direction: detecting unusually high accumulations of dust at very small scales – useful for cleaners of our largest, most byzantine and ornate mansions and other large buildings (tho likely beyond the pocketbook of most in the middle class, who, like the poors, unable to afford the tens of millions in money and dozens of volunteer experts, will continue to be stuck with eye-balling it on their own).

28. **Peter Woit**
September 22, 2014

Avattoir,
My questions were kind of rhetorical… As expected, Dennis Overbye of the NYT wrote a good survey of the situation, but yes, I'm guessing it will appear tomorrow not on the front page, but in an inside section. Someone much better informed than me about the ways of the publicity world recently told me that, despite things moving online, positioning in the print paper is still considered the final say on what is important and what isn't.

SciAm has had critical articles about string theory and SUSY over the years. I've never seen anything though like this current situation though: the highest profile result in years in the highest profile subject is dramatically announced, within a couple months it's clear there are problems with it, a few months later SciAm decides to put this on its cover anyway. I don't see how sprinkling in a few "if confirmed" helps the matter.

29. **Mike Black**
September 22, 2014

Let's all calm down for a second.
(1) Dust does not explain the entire shape of the curve that the BICEP2 and KECK experiments measured. If you look at Fig2 of the new Planck paper, the slope of the polarization vs. multipole for dust looks nothing like the curve for gravitational waves for low values of multipole.
http://bicepkeck.org/B2_2014_i_figs/speccomp-thumb.png
(2) The main, indirect evidence for inflation comes from the shape of the density fluctuations vs. wavelength in the universe. (see https://www.astro.virginia.edu/class/whittle/astr553/Topic16/t16_galaxy_power_spectrum.gif)
Inflation can explain the slope of the density fluctuations at long wavelength as well as their Gaussian nature. I know of no other model that can accurately explain the shape of the curve at long wavelengths and the Gaussian nature of the fluctuations.

The theory of inflation is no better or worse off today than it was before the new Planck paper.

30. **Peter Woit**
September 22, 2014

Mike Black,
I think it's more accurate to say that inflation is no better or worse off today than in early March, before the BICEP2 announcement. The argument by BICEP2 that there was too little dust to account for their observations, so it had to be primordial is killed off by the Planck paper, returning things to the pre-BICEP2 situation.

31. **chris**
September 23, 2014

Peter,

I'm sure you'll love this take on the story then…

http://www.gizmodo.com.au/2014/09/physicists-are-backing-away-from-the-biggest-discovery-of-the-century/

32. **cthulhu**
September 23, 2014

The extensive caveats that are described in the SciAm paper remind me of reading papers in my field, control systems theory, in the early '90s. They would start off by saying that "control design technique X is well suited to guarantee stability and performance metrics in the face of nonlinearities, noise, time delays, structured and unstructured model uncertainties, etc." The next sentence would be "In this paper, we will restrict our focus to linear, time invariant single-input-single-output systems with no noise, no time delays, perfect sensors, perfect actuation, and no unmodeled uncertainties." As somebody else said, if you've caveated everything, what's the point anymore?

33. **West**
September 24, 2014

Cormac,
As far as I can tell, no one is arguing that this set of measurements amounts to a refutation of inflation as a modeling framework. What this data seems to indicate is that the dust+backgrounds (noise-only) model is a better model for the B-mode polarization BICEP2 saw than GW+dust+backgrounds (signal+noise). Sesh delineates this chain of inference really nicely at the bottom of his post Peter links to in update2.

This doesn't mean there isn't a signal buried in there somewhere, just that the simpler model is better at matching the data with fewer free parameters. At some point the measurements will get so precise that either the signal is found or cosmologist will have use more complex theoretical models, but as has been said ad-nauseum, we aren't at there yet.

34. **Antonio (AKA "Un físico")**
September 24, 2014

My opinion is that Bicep2 manipulated their simulations to gain worldwide popularity and in my opinion Bicep2 should apologize. 7 sigma is the maximized significance level of r different from zero when setting the dust to zero but, why Bicep2 had to invent a fictitious scenario by setting that dust to zero and publiciting their results with worldwide propaganda?.
Planck's preliminary results say that the r = 0.2 could be explained only by dust. Planck must refine their analysis; so let's see how this story ends.

PS: I have been discussing with Lubos Motl about this issue: he believes that I am an incoherent conspiracy theorist that enjoys attacking science. But he cannot explain why: from the r diffrerent from zero at 7 sigmas with Bicep2, we are now talking of a confidence of only 2.5 sigmas with Planck (and we will see which is the confidence in future analysis).

35. **david**
September 24, 2014

Today the joint analysis of BICEP2/Planck dust was done by a Chinese group arXiv:1409.7025. They got a new stringent limit of r (0.083).

36. **Art**
September 25, 2014

Maybe SciAm has taken action. Since upgrading to ios8, their ipad app doesn't work…

37. **Art Brown**
October 8, 2014

As a follow-up, the new version of the SciAm iPad app works with ios8, so I've finally been able to read the Krauss article. It actually mentions the Planck results and includes a supporting figure:

"Alas, as of this writing, the situation remains unsettled."

"Recently the Planck satellite revealed that such dust could be more prevalent than previously thought."

"The BICEP2 team stands by its estimates – but it now admits that it cannot rule out a dust explanation."

Maybe Krauss (or SciAm?) got some insider info ahead of Planck publication? Anyway, the article ends up as an uneasy hybrid of theory and experiment, but maybe stands as a report from "the front lines".

38. **Peter Woit**
October 8, 2014

Art,
The problems with the BICEP2 result were known back in May, and I'd guess the SciAm reference is to other results about dust that came out earlier this year that reinforced evidence of a problem. What's surprising here is that SciAm still thought it a good idea to put this kind of thing on the cover, several months after it became clear there was a problem with it.

39. **Edward**
October 8, 2014

Does PRL lose its credit?

Now the value of r significantly drops down and is consistent with Planck. However, there are several papers in which some theoretical models were supposed to solve the tension of BICEP2 and Planck. How will PRL deal with these papers?

40. **Art Brown**
October 10, 2014

Re SciAm, I concede…

Will you be participating in Festival Albertine's "exploration of mathematical styles"?

41. **Peter Woit**
October 10, 2014

Art Brown,
I just read about that in the NYT this morning, hope to attend.

Horgan Interview With Witten

September 22, 2014
Categories: Uncategorized

Back in 1996 John Horgan's *The End of Science* appeared, which included material from a fascinating 1991 interview of Edward Witten. I had mixed feelings when reading this. On the one hand, Horgan was doing something truly remarkable, challenging Witten in a way that no one else dared. This was 7 years after the "First Superstring Revolution", and it was starting to become clear that string theory was not working out as hoped. No journalist other than Horgan though was talking about this, or willing to confront someone of Witten's stature with difficult questions. Pretty much every other story in the press stuck to the simple narrative that Witten was a genius, and superstring theory a great success. On the other hand, Horgan did use his author's freedom to edit and frame the interview to make Witten look bad (today he admits the Witten profile was "pretty snarky"), so he was landing some low blows, against a rather gracious opponent.

This year Witten won the Kyoto Prize, and I was shocked to hear that Horgan was the person chosen to interview him. Witten rarely gives interviews and I would have thought that Horgan would be the last person in the world he'd agree to an interview with, given his past experience. The interview is now available here.

This time around Horgan avoids the snark, and asks some straightforward questions about whether Witten's views have changed since 1991, and what he now thinks about string theory, the multiverse, anthropics, etc. I have to admit that I find Witten's answers depressing, in contrast to Witten's advisor David Gross's current take on these issues (discussed here). About anthropics, Witten's "I don't like it, but may be the way to go" contrasts with Gross's "cop-out", and his insistence on string theory as the way forward contrasts to Gross's emphasis on the fertility of quantum field theory.

Back in 1996, after the appearance of Horgan's book, Gross and Witten wrote in to the Wall Street Journal (reproduced here) to argue that Horgan was wrong, since string theory would be tested by finding SUSY at the Tevatron, or, failing that, definitely at the LHC. We all know how well that has worked out, and Gross seems to have learned a lesson from this. Witten on the other hand has moved on to even more dubious testability claims (e.g. that the string theory landscape can be tested by "seeing a signature of a prior phase transition in the CMB"). From the 1996 claim that vindication would come "in the next decade", he now is talking about "200 years from now". His one point of close agreement with Gross is that both agree that not knowing what string theory is when time-dependent effects are large is a big problem, one that has seen no progress.

By a couple years from now, the idea of making progress in our lifetime by seeing SUSY at the LHC, then going on to use this to learn about string theory should be finally finished off. Already Gross seems to have evolved from the 1991 point of view to a more promising one, perhaps Witten at some point will start to do the same.

Update: The AMS has something similar, a Mathematical Moment with Witten. Pretty much everything said about string theory is exactly the same as thirty years ago, only change is that the story used to be that string theory would get some vindication at the LHC, now it's:

> The verification of superstring theory is probably a long way off, but could be found here on Earth, using particle accelerators (possibly much more powerful than those of today)

consistent with the "200 years" estimate from the Horgan interview.

Comments

1. **RM**
   September 22, 2014

   I am not sure why John Horgan's views on science deserve publicity. Prior to his assertions about physics, he wrote a book claiming that mathematics was coming yo an end because proofs were becoming too hard. In the 20 years since the publication of his opinions, there is little doubt that major breakthroughs have occurred in mathematics. Similarly, we have gained hard data in physics on many fronts. We have not been as successful theoretically. Still, these are matters that should be judged by professional scientists rather than a journalist.

2. **Peter Woit**
   September 22, 2014

   RM,
   You're referring not to a book, but to an article he wrote for Scientific American ("The Death of Proof"). I haven't looked at that in years, but I do remember that I disagreed significantly with what he had to say there. How mathematics keeps making progress despite getting harder and harder is an interesting question. I'm not so convinced though that in the long term math may also not find itself having reached a sort of end-point, with all of its fundamental structures and their relations discovered and laid bare. We're still quite a ways away though…

   I think if you read Horgan's take on the state of physics in the mid-nineties in the End of Physics, it stands up very well now. He's not a physicist and didn't always get things right, but he saw the problems fundamental physics was running into, in a way that most professional theorists either didn't see, or if they did, refused to admit publicly. Like any science journalist he operates not as an expert, but as someone who talks to experts, asks questions, tries to get a handle on what is going on, and then communicate it to others. He has often been much more willing to challenge experts than is common among journalists. Experts don't necessarily like this, but it sometimes is really needed.

3. **RM**
   September 22, 2014

   Yes, it "Death of Proof" was an article and not a book. My apologies. I think my point remains – his book "The End of Science" was a sweeping generalization to *all* of science. Not just particle physics. But also every other kind of physics, chemistry and biology. Do you really believe these claims are justified for *all* science?

   For example, major advancements in biology seem possible with the advent of techniques to image the interior of the human body. In the late 1990s, we also discovered that there was a non-zero cosmological constant which has obviously influenced the way we think about physics.

   Even in particle physics, ideas such as supersymmetry could have been discovered. I am not aware of any argument that could have proven that the predicted supersymmetric particles would not be discovered at colliders. When Horgan wrote his book, he could not have possibly known that these discoveries would not have happened.

4. **Peter Woit**
   September 22, 2014

   RM,

I agree that there’s a criticism to be made of the book that he’s sometimes trying too hard to fit different sciences in very different states into the same framework.

As for supersymmetry, it was clear by the 1990s that this wasn’t an idea that was answering many questions that we didn’t understand about the standard model. Many people were highly skeptical of it for very good reason. If you had the choice to get your news from the many journalists who talked to Gross and Witten, took down uncritically what they said, then wrote articles about what glorious ideas susy and string theory were, or from Horgan who approached them skeptically, you’d have done a lot better in terms of understanding the real state of particle theory to go with Horgan.

Sure, his skepticism could have been mistaken, SUSY and string theory could have turned into great successes. But his skepticism turned out to be justified, to his credit. Sometimes the world really does need skeptics…

5. **Dave Miller in Sacramento**
September 22, 2014

RM,

I doubt that anyone, including Horgan himself, thinks that he has gotten everything right in everything that he has written. But, as illustrated in the discussion here, he has managed to get a lot of people thinking and talking about significant issues. Isn’t that good enough? After all, no one takes him as an expert, he does not control funding, etc.

There is a place for a gadfly, so long as a the gadfly does not have pretensions of divinity. Horgan does not seem full of himself.

After all, even many of us who do not agree with Peter on everything feel he is provided a real service to physics by raising questions that needed to be raised about string theory but that were just being ignored.

Dave

6. **RM**
September 22, 2014

I am all in favor of reasoned skepticism. Horgan did not have the evidence back in the 1990s to make the strong claims that he did – in many cases, he failed spectacularly. In one particular instance, his poorly reasoned skepticism happened to be right about one particular theory. I don’t think that one example does much to restore his credibility.

In the early 1990s, there were many reasons to think supersymmetry was the right path – starting with the fact that the gauge coupling measurements suggested unification. It is only after the discovery of the cosmological constant and the strong constraints on SUSY from LEP that the motivations for SUSY began slipping. But these occurred after Horgan’s book was published in 1996. If he had expressed skepticism about SUSY in 2001, I would not have complained.

To sum up, poorly reasoned skepticism is about as valuable as war hawks braying for a fight at every opportunity.

7. **PAC**
September 22, 2014

Gross was much more bullish on string theory during his recent lecture at the Kavli Institute than he was at NYU. (Starting around 1:01:00.)

http://www.youtube.com/watch?v=jhYzbX6qavc

8. **Peter Woit**
September 22, 2014

PAC,
Thanks. I think though that that’s also at NYU a few days ago, but a talk for the public as opposed to a talk for physicists like the colloquium talk a day earlier. To the public, Gross is giving much the same story as always, it seems to be when speaking to physicists that he’s evolving, perhaps realizing that the old arguments for string theory are no longer going over so well among his peers.

9. **Hanna**
September 23, 2014

Peter,

Thank you for the post. I was wondering for some time what Witten was thinking, now that experiment and theory speak loud and clear against supersymmetry. He did not publish for a while on arxiv. I was hoping that he changed his mind, or that at least he developed some doubts. Now we know that he did not. Reading the interview, he reminded me of an old dictum:

Errare humanum est, perseverare diabolicum.

10. **chris**
September 23, 2014

RM,

your reasoning is very peculiar. you say that in the mid ’90s a sober inspection of the evidence should have led a person to believing in SUSY. and that skepticism was not reasonable at that moment.

i think you do not quite grasp what skepticism means. as i understand it, it means not believing in any phenomenon that is not established by firm evidence. and there was as little firm evidence back in the mid ’90s than there is now.

now that the skeptics of back then turn out to be right (although some of them out of academia while some of the uncritical minds of the time hold professorships now) you suddenly say that they were mislead back then into holding the correct opinion?

that is a very queer take on history.

11. **S. Molnar**
September 23, 2014

So, is it fair to say that when addressing the public Gross and Witten give much the same take as always, but when addressing physicists Gross is not so sanguine? If so, is there really a difference between the two? What, if anything, is Witten saying to physicists about the project?

12. **Dom**
September 23, 2014

RM, I'll second the point about your take on scepticism, there is no onus of proof on the sceptic, the sceptic is the spokesperson for the Null Hypothesis.

13. **Shantanu**
September 23, 2014

Peter or anyone else: do you know if there was an upsurge in hype about string theory
when evidence for non-0 neutrino mass was found in 1998. This was before pre-blogging days, don't remember too well.
shantanu

14. **Peter Woit**
September 23, 2014

Shantanu,
I don't recall ever seeing much in the way of claims that non-zero neutrino masses had any significance for string theory one way or another.

15. **imho**
September 23, 2014

Perhaps the larger issue is that public/private versions of this story exists at all. Everyone understands the need to preserve funding, and in every other field on earth, it's almost mandatory that people "bend the truth" to protect their interests or create the appropriate public image… Unfortunately, that's just the way the world works… But hard science is different. One of the core strengths of science is that it's correct. No superstition, no opinions, just fact. The public trusts us because we're impartial and we don't lie to them… so why do some continue to obfuscate the truth???

There are fields like Medicine, or Chemistry, or Cond Matt that can probably afford to falsely hype results. These fields produce concrete real-world results that directly benefit society. Yet at times, it seems these fields are the most subdued. So why is it that Fundamental Physics, which is mostly disconnected from society, feels the need to exaggerate results…. or maybe I just answered my own question?

16. **Als**
September 23, 2014

"There are fields like Medicine, or Chemistry, or Cond Matt that can probably afford to falsely hype results. These fields produce concrete real-world results that directly benefit society. Yet at times, it seems these fields are the most subdued."

The situation in medical science is far worse than physics. False claims, outright fraud and massive overhyping are depressingly common.

17. **Guido**
September 23, 2014

Slightly, but just slightly off-topic:

"Brian Cox: 'Multiverse' makes sense"
at http://www.bbc.com/news/science-environment-29321771

18. **AdamT**
September 23, 2014

FYI, the context of that article above is Many World's interpretation of quantum mechanics measurement problem. As opposed to the string theory landscape or eternal inflation's multiverse.

19. **RandomName**
September 23, 2014

I am very disappointed Horgan got the interview as he is just not a good interviewer for this kind of thing. And a *science* journalist claiming that we have come to the end of science/math is pathetic and sad.

20. **Douglas Natelson**
September 23, 2014

As I had said on my blog relatively recently, I think Horgan is a provocateur who carefully defines his terms to suit his agenda:
http://nanoscale.blogspot.com/2014/04/john-horgan-same-old-same-old.html

21. **Roger**
September 24, 2014

According to Horgan, Bill Thurston sandbagged him on "The Death of Proof".

22. **Raisonator**
September 26, 2014

Apropos Witten interview, there is an old one on the web which may not be so well known:
https://www.youtube.com/watch?v=zsyF2BAHg2o

23. **NonaMe**
September 26, 2014

"string theory would be tested by finding SUSY at the Tevatron, or, failing that, definitely at the LHC. We all know how well that has worked out" Well, no we don't. As far as I know the LHC is not over yet: it is barely starting to scratch the surface.

24. **HL-LHC or HE-LHC**
September 26, 2014

"His one point of close agreement with Gross is that both agree that not knowing what string theory is when time-dependent effects are large is a big problem, one that has seen no progress."

Can someone explain what are time-dependent effects on string theory and why is it a big problem? Does string theory have problems reproducing gravitational time dilation in GR?

25. **Eduardo Lira**
September 28, 2014

Dave Miller in Sacramento: "Horgan does not seem full of himself."

Not full of himself? The following statement by Horgan paints a different picture to me: "I don't accept that the evidence for inflation is "vastly greater" now than in 1996."

Ed Witten may be wrong at the end concerning ST, but taking on him the way Horgan does reminds me of Salieri and Mozart. Except that Salieri was a music composer himself.

26. **Peter Woit**
September 28, 2014

Eduardo Lira,
An interesting question is whether Witten is right or wrong about the evidence for inflation being "vastly greater now than in 1996". I suspect Paul Steinhardt might disagree with him about this. Of the lists of evidence for inflation I've seen, the most convincing to me are pre-1996. But, I'm no expert, and I'd love to hear from an expert what they think of this.

Horgan now has a good track record in his skepticism about Witten's claims. Witten's 1991 story that string theory would be vindicated at the Tevatron or LHC has not worked out, and in general Horgan's 1991 skeptical point of view has held up a lot better than Witten's.

What do you suggest science journalists do when reporting comments from someone like Witten that they are skeptical about? Should they ever even have any skepticism about what someone like Witten has to say?

27. **Tim Howells**
October 3, 2014

> Roger says:
> According to Horgan, Bill Thurston sandbagged him on "The Death of Proof".
> http://blogs.scientificamerican.com/cross-check/2012/08/24/how-william-thurston-rip-helped-bring-about-the-death-of-proof/

Thanks for that very interesting and relevant link. Also note that in a postscript to that article Horgan links to this blog.

Thirtieth Anniversary of the First Superstring Revolution

September 25, 2014
Categories: Uncategorized

Today's about the date that I'd pick for the 30th anniversary of the First Superstring Revolution. Witten's paper Some Properties of O(32) Superstrings arrived at the journal Physics Letters on September 28,1984, so presumably was finished and sent out around September 25.

The effect of this paper on the field was a bombshell. Witten was at the time far and away the most influential person in the field, regularly producing staggeringly original work that was having a huge impact. The arrival that fall of a preprint from him announcing that he had stopped work on everything else, and now had what looked like a viable, consistent unified theory of everything, one that he claimed was determined by a single parameter and made predictions ("It predicts axions and stable Nielsen-Olesen vortex lines") was the true First Superstring Revolution.

I wrote about this in some detail ten years ago, for the 20th anniversary, so won't repeat what is here and here, supplemented by comments from Larry Yaffe. For something more recent along the same lines, see here.

Ten years ago the 20th anniversary of the First Superstring Revolution was celebrated with a symposium at Aspen, but as far as I know, no one has organized a 30th anniversary celebration. There are now many, many known ways of trying to get unification out of strings, with the original 1984 hope that anomaly cancellation gave a more or less unique possibility long gone. As for unification itself, thirty years later Witten remains a true believer in the vision that came to him in September 1984 (see here and here), although he now seems to see little hope for vindication during his lifetime.

Comments

1. **AdamT**
   September 25, 2014

   How had Witten become so influential before the first superstring revolution? What had he done or worked on up to that point that made him so influential?

2. **george ellis**
   September 25, 2014

   For his extraordinary citation record, see here:
   http://scholar.google.co.uk/citations?user=30JcgzcAAAAJ&hl=en&oi=ao

3. **Peter Woit**
   September 25, 2014

   AdamT,
   In my book I tried to explain some of this, it's a long story. One aspect is the relation to mathematics, a lot of that is in the book, and much of it happened after 1984, with the development of Chern-Simons theory (which got Witten the Fields Medal) and later all sorts of topological quantum field theory ideas, leading to things like Seiberg-Witten, which revolutionized 4d topology. From the purely physics point of view, some pre-84 high points were Witten's work on "current algebra", leading to an effective theory of mesons and Baryons, using the Wess-Zumino-Witten term, his work on WZW conformal field theory, work on analyzing in very general terms SUSY breaking, and much, much more. That period of the early 80s was quite remarkable, and he was doing things far beyond what anyone else could.

4. **Shantanu**
   September 26, 2014

   Just out of curiosity (since I am not familiar with most of Witten's HEP papers), has any high energy (or astroparticle) prediction/model made by Ed Witten been confirmed/verified by any accelerators? I am guessing no, but I maybe wrong.

5. **Bill**
   September 26, 2014

   I don't understand Witten's logic. When it comes to landscape interpretation, he says that "universe wasn't made for our convenience", but somehow universe cares about "wonder, incredible consistency, remarkable elegance and beauty" of string theory?

6. **anon.**
   September 26, 2014

   Does a free PDF of "Some properties of O(32) superstrings" exist anywhere, or is it being kept behind firewalls by Mr Witten?

7. **Peter Woit**
   September 26, 2014

   Shantanu,
   Actually, Witten's first paper, his Ph.D. thesis, was a calculation of QCD effects on photon-photon scattering which I believe were later experimentally tested. Wikipedia has this
   http://en.wikipedia.org/wiki/Photon_structure_function
   but I'm sure there are much better references elsewhere.
   The work on Skyrmions and the effective low-energy theory for mesons that I mentioned is just an approximation, so doesn't make sharp predictions, but quite possibly there are experimental results best understood this way.

   As for BSM physics, you can't blame Witten much for not producing anything experimentally testable, since no one else has either.

8. **M**
   September 26, 2014

   Actually nobody could have given a correct prediction different from the Standard Model, because all experiments so far confirmed the Standard Model.

   The problem is that, while acritically following the naive string dream, many theorists were driven away to speculations so disconnected from any possible experiment, that institutions that once had excellent theoretical groups have now disappeared from the map of fundamental physics.

9. **Justin**
September 26, 2014

Woit,

The anomaly cancellation that you are referring to in this paper by Witten was actually done by Schwartz and Green after Witten had wrongly concluded that such a thing wasn't possible. So, contrary to what you write, it was not Witten's brilliance that started the 1st superstring revolution. Rather, the work of Schwartz and Green influenced Witten.

Also, what I wrote above suffices to undermine what you wrote about Witten doing things far beyond anyone else. In particular, the work of Schwartz and Green was a critical development in string theory which Witten had failed to discover.

10. **Peter Woit**
September 26, 2014

anon,
The original preprint is available here
http://ccdb5fs.kek.jp/cgi-bin/img_index?198412280

Justin,
If you read any of the earlier discussion I linked to, you'd see that I'm well aware of that argument, just disagree with it, for reasons explained in detail. You might also want to learn how to spell John Schwarz's name correctly…

11. **anon.**
September 26, 2014

Peter: thanks for the link to Witten's 1984 preprint. He proves SU(5) can be obtained as a subgroup to an O(32) supersymmetry theory. But wasn't SU(5) disproved by the failure of proton decay?

12. **Peter Woit**
September 26, 2014

anon.,
Not supersymmetric SU(5), where the proton lifetime is much longer. Also, the significance of that paper is not any specific model, but the claim of existence of a class of testable TOEs with a single adjustable parameter, and that Witten was working on them. Very quickly attention turned to more promising specific models.

13. **HL-LHC or HE-LHC**
September 26, 2014

where do you see string theory 10 years from now when all the results of LHC will be in and analyzed?

14. **HL-LHC or HE-LHC**
September 26, 2014

Peter,
Ed Witten agrees with you on the importance of precise prediction

http://blogs.scientificamerican.com/cross-check/2014/09/22/physics-titan-edward-witten-still-thinks-string-theory-on-the-right-track/

Horgan: Do you agree with Sean Carroll that falsifiability is overrated as a criterion for distinguishing science from pseudo-science?

Witten: Scientists aim to get as reliable and precise an understanding of nature as we can. The gold standard is a precise prediction that can be tested in a precise way in a laboratory experiment. Experiments that disprove theories are an important part of the scientific process.
^

but Witten believes string theory is predictive.

"I asked Witten how he responded to the claims of critics that superstring theory is not testable and therefore is not really physics at all. Witten replied that the theory had predicted gravity. "Even though it is, properly speaking, a post-prediction, in the sense that the experiment was made before the theory, the fact that gravity is a consequence of string theory, to me, is one of the greatest theoretical insights ever."

Witten also claims black hole entropy as a success of string theory.

15. **Peter Woit**
September 26, 2014

HL-LHC or HE-LHC,
I think the LHC will conclusively not see supersymmetry, and this + another ten years of no progress towards getting physics out of the idea of string theory unification will continue the trend towards it being a dead idea.

As for arguments about predictivity like the "predicts gravity" one, they've been discussed hundreds of times here. About that one in particular though, I think the best response was Lisa Randall's : "sure, string theory predicts gravity, ten-dimensional gravity."

16. **JG**
September 26, 2014

the best response was Lisa Randall's : "sure, string theory predicts gravity, ten-dimensional gravity."

Oh come on, String Theory predicts gravity exactly as we observe it in 3+1-dimensional macroscopic spacetime according to Einstein's General Relativity.

THAT is amazing.

In fact it was pretty amazing when Feynman first started telling everyone back in the 60s that gravitons described the same equation as GR – but no one ever got gravitons out of a theory that also predicted SM particles – until String Theory.

So, please bear in mind this great achievement of String Theory that NO OTHER THEORY HAS COME CLOSE TO.

btw: If a SUSY particle is discovered will you still be anti-string?

17. **JG**
September 26, 2014

A nicely formatted version of Witten's 1983 paper Some Properties of O(32) Superstrings

18. **Peter Woit**
September 26, 2014

JG,
I'll let you argue that one with Lisa Randall.

And, you're seriously abusing the word "prediction". String theory does not "predict SM particles", and even if Randall is mistaken, that's called a "retrodiction", not a prediction.

19. **Tom**
September 27, 2014

@JG – you are wrong. In D=4, superstring theory does not predict Einstein gravity, but Brans-Dicke scalar-tensor theory with an equal mix of spin 2 and spin 0 mediated gravitational forces. Experimentally it is known, to a very high accuracy, that gravity is Einstein's pure spin 2. This is one of these rare cases when string theory makes a solid prediction — it is clear that most of the landscape has massless dilatons, so if you are a multiverse fan, you are very lucky to be here.

20. **JG**
September 27, 2014

Ok, let's be a little more precise, String Theory can describe Standard Model particles *and* gravitons. No other theory comes close to achieving this.

@Tom

Yeah, ok, but I'm talking about the macroscopic low energy limit that we can currently observe – *that's* predicted by String Theory. (A recent publication has claimed that, *mathematically*, Black-Holes should not form because of quantum effects (hawking) during star collapse, this may just be an isolated pure maths result, unrelated to real-world physics, but there should be quite severe contraints for predictions from QG theories once we get to the really strong-field regimes and high energies. (If not, then we really will have to give up on physics))

(btw I incorrectly referred to Witten's "famous" 1984 paper as from 1983 above)

21. **Jeff M**
September 27, 2014

@JG – interesting paper, and also the arxiv followup. Always did find GR more aesthetically pleasing than QFT. Anyway, what I'm curious about is how one might account for what, to cosmologists, seems to be plenty of evidence for black holes? Is there some other mechanism that could account for what is seen? The papers imply if a star wants to collapse it will end up exploding. Gets rid of the information paradox, but what about the giant black hole at the center of our galaxy?

22. **theon**
September 27, 2014

My teacher Veltman was speaking about the "desert" in the late seventies and what do we see now? I agree that ST is a framework within which certain interesting questions can be answered (after enough pleasant assumptions such as SUSY, extra dimensions and their compactifications have been made). Indeed, a theory of everything except the standard model AND of the solid state except high Tcs, has the smells of a framework. The multiverse just avoids to explain our Universe. The many words interpretation is a poor-man's-physics denying that one has to bother about the apparatus – don't tell Bohr. But workable models exist and in their solution "many worlds" do not show up, I would say: not to the least. But Einstein's ensemble interpretation gets to the point. Quantum gravity I would like to see in the QFT on the "boundary", I do not care about the bulk. Concerning Witten, may I recall: It is the task of physicist to explain Nature.

23. **JG**
September 27, 2014

@Jeff M

There is suprisingly little observational evidence to support GR in the strong-field regime, the "black-hole" at the centre of our galaxy could just be an incredibly dense object – we don't have experimental resolution to conclude an actual event-horizon exists.

But, it's not so bad, we do have concrete evidence that *some* of the strong-field regime in GR is correct, especially from twin pulsars. See the Experimental tests of gravitational theory section from the Particle Data Group

To tie this in with the thread's historical overview of String Theory, note that this very same review of experimental tests of gravitational theory has been published for many decades – and only last year did they remove the reference to String Theory as a "promising" theory, which existed at least since the turn of the millenium (eg the last sentence of the introduction section of the 2006 version says " Superstring theory offers a promising avenue toward solving this challenge"

24. **JG**
September 27, 2014

The link above to the 2006 version of Experimental tests of gravitational theory didn't appear

25. **Peter Woit**
September 27, 2014

Please, enough discussion of claims about GR that have nothing to do with the topic of this posting.

26. **Chris Oakley**
September 28, 2014

theon,

I am hoping that your "many words interpretation" above was not a typo.

27. **Wavefunction**
September 29, 2014

Did Witten actually say that he had stopped work on everything else?

28. **Peter Woit**
September 29, 2014

Wavefunction,
I guess the paper didn't say this explicitly, but it made clear that he all of a sudden thought he saw the way clear to a TOE. And people I know who talked to him at the time told me this is what he told them (as well as that they should drop everything and work on string theory too). Over the next couple years he wrote a very large number of papers, basically all about string theory.

29. **Bill**
September 30, 2014

Peter, are you again not going to make the Nobel Prize prediction?

30. **Peter Woit**
September 30, 2014

Bill,
I retired from the Nobel Prize prediction business after this
http://www.math.columbia.edu/~woit/wordpress/?p=84
No idea who they'll pick this year. If the Swedes change their mind and let groups get the prize, a Higgs prize for CERN + ATLAS + CMS would be a good idea…

31. **lun**
October 1, 2014

I am not sure if this was already posted, apologies if it was, but here is a video-discussion with Witten from around the times:
https://www.youtube.com/watch?v=AmUI2qf9uyo#t=1008
It is actually amazing how many of the subjects discussed are still discussed in just about the same way

32. **Martin S.**
October 3, 2014

>> *and even if Randall is mistaken, that's called a "retrodiction", not a prediction.*

@PW Did not A.E. get NP for "retrodiction" of the photoelectric effect? PS Am not stringy fan (and my gut feeling is the ST does not predict/retrodict anything), 'm just disliking extremisms.

33. **Peter Woit**
October 3, 2014

Martin S.,
Of course a good retrodiction can be very strong evidence for a theory. If string theory had a convincing calculation of any of the SM parameters, that would be very impressive and give it high credibility. It doesn't though retrodict four-dimensional gravity. My point was just terminological: the word "prediction" is often seriously abused in this kind of argument. No one says Einstein "predicted" the photo-electric effect…

Quick Links

October 1, 2014
Categories: Uncategorized

- This month's Physics Today has a long article by Wojciech Zurek, Quantum Darwinism, classical reality, and the randomness of quantum jumps. I'm not sure if there's anything new there, but it's a very clear exposition of what seems to me the most penetrating point of view on the measurement problem in quantum mechanics, one that gets far too little attention in the press.

  I'd like to know what this makes me in terms of various ideologies of the interpretation of QM. Am I a quantum Darwinist, or maybe a Zurekian?

- At another extreme, getting lots of media attention while not saying anything substantive, there's the multiverse of the Many Worlds interpretation. The media campaign to promote this is still in high gear. Recent examples include Brian Cox: 'Multiverse' makes sense at BBC News, this week's New Scientist, which has a bunch of things including Multiverse me: Should I care about my other selves?, and an upcoming program here in New York that tells us that:

  > We may live in a multiverse in which every possibility happens and with each new possibility the universe branches off into another of many worlds.

  The New Scientist article has Don Page pointing out that this explains the problem of evil. God likes the idea of everything possible happening all the time so much he'd rather not be bothered to stop bad things from happening:

  > "God has values," he says. "He wants us to enjoy life, but he also wants to create an elegant universe." To God the importance of elegance comes before that of suffering, which, Page infers, is why bad things happen. "God won't collapse the wave function to cure people of cancer, or prevent earthquakes or whatever, because that would make the universe much more inelegant."
  >
  > For Page, that is an intellectually satisfying solution to the problem of evil. And what's more, many worlds may even take care of free will. Page doesn't actually believe we have free will, because he feels we live in a reality in which God determines everything, so it is impossible for humans to act independently. But in the many-worlds interpretation every possible action is actually taken. "It doesn't mean that it's fixed that I do one particular course of action. In the multiverse, I'm doing all of them," says Page.

- On the math front, I just noticed that Pieter Belmans has a blog. One of the many nice things there is his "atlas" for Spec **Z**.
- Over at Persiflage, anyone interested in how NSF grant applications in mathematics are evaluated can find an extensive and well-informed discussion.
- Videos from last week's Heidelberg Laureate Forum (which features Fields Medalists and others) are available here.

Comments

1. **Matt Leifer**
   October 1, 2014

   "I'd like to know what this makes me in terms of various ideologies of the interpretation of QM. Am I a quantum Darwinist, or maybe a Zurekian?"

   It means you are a closet Everettian in denial. Zurek is explicit that his work is a development of the Everettian program, although he does not necessarily endorse many-worlds. Still, the only way I can really make sense of Zurek's work is within a many-worlds context. Zurek would likely disagree, but I think he is working with an unusual definition of "objective".

2. **Peter Woit**
   October 1, 2014

   Matt,
   I wouldn't really deny being an Everettian, although one suffering from not being quite sure what an Everettian is. Of the things I've seen purporting to describe what such a point of view is, they seem to me to range from sensible statements, clarifying a bit how to think about this, to the completely empty, heavily larded with silliness. Surely there's a terminology to keep straight which sort of Everettian one is.

3. **Als**
   October 1, 2014

   "Over at Persiflage, anyone interested in how NSF grant applications in mathematics are evaluated can find an extensive and well-informed discussion"

   It was a depressing read. I had no idea there was so much PC-bullshit going on behind the scene.

4. **Peter Woit**
   October 1, 2014

   Als,
   Funny, my reaction was the opposite. I would have thought there was MORE PC-bullshit going on behind the scenes…

5. **Alex**
   October 1, 2014

   The description of the NSF evaluation in mathematics sounds very much like how it works in astronomy, modulo some obvious differences. I'm stunned that 30% of NSF mathematics proposals get funded though! It was more like 10% in astronomy before the financial crisis, and is surely worse now.

6. **Yatima**
   October 1, 2014

   I came here to read about math and all I got was further erosion of my dwindling hope that human brains can actually generate meaningful statements.

   "It doesn't mean that it's fixed that I do one particular course of action. In the multiverse, I'm doing all of them"

   What the hell is this I don't even.

   I think it was Danielewski who wrote that the writings of Heidegger prove the existence of crack back in the early twentieth century. This surely reaches that kind of level.

7. **JG**
October 1, 2014

“I’d like to know what this makes me in terms of various ideologies of the interpretation of QM. Am I a quantum Darwinist, or maybe a Zurekian?”

The paywall interpretation of Quantum Mechanics

8. **Anon**
October 1, 2014

Alex–it is true that Astronomy gives out 10-15%, and Math about 30%, but in Astronomy they tend to give people roughly what they ask for, whereas Math cuts budget requests substantially. High Energy also is around 30% and cuts budgets substantially. One difference in High Energy is that they use external reviewers (3-4 per proposal, typically) and the panel then looks at those reviews before ranking the proposals.

9. **Don Jennings**
October 1, 2014

Is there a place where I can look up all the grants awarded by the NSF (or DOE) to a particular person? I think it would be rather interesting to see how much various papers have cost the public.

10. **abbyyorker**
October 1, 2014

darwin is behind a paywall… bummer

11. **Peter Woit**
October 1, 2014

JG/abbyyorker,
For something not paywalled, there’s
http://arxiv.org/abs/0903.5082

Don Jennings,
Much information about NSF awards is searchable here
http://www.nsf.gov/awardsearch/advancedSearch.jsp

12. **CPV**
October 1, 2014

What the solution to the measurement problem is depends in large part on what you think the problem is, if it even exists at all. Why do you think it exists at all? Thinking that classically emergent behavior can’t occur at scales many, many orders of magnitude larger than quantum behavior seems like a strange prior. Look at turbulent flow. Look at gravity (probably).

13. **GoletaBeach**
October 2, 2014

Martin Perl has died.

http://www.mercurynews.com/science/ci_26646508/renowned-stanford-physicist-nobel-laureate-martin-perl-dies

14. **Igor Khavkine**
October 2, 2014

I’d love to hear about the ERC (european) version of how the NSF grant review process works.

15. **Peter Woit**
October 2, 2014

Goleta Beach,
Thanks, very sorry to hear that news.

Perl had a blog, see
http://martinperl.com/

16. **nick herbert**
October 2, 2014

One of the reasons Giordano Bruno was burned at the stake was his doctrine of multiple worlds. Bruno argued that the Church-supported doctrine of One World constituted an unjustified limit to God’s omnipotence and glory.

17. **Jeff M**
October 2, 2014

I don’t see how Everett solves the measurement problem. He just says that the wave function doesn’t collapse, you still have a measurement which changes things somehow. As I remember reading Everett, Schrodinger’s cat is still both alive and dead until you open the door.

18. **piscator**
October 3, 2014

nick: not relevant and not correct

The ERC procedure is (partially) as follows. The proposal initially goes off to three external referees (this is the first stage). If successful in the first stage, the proposal goes to an additional four external referees and the candidate is invited to interview. Once the process closes you get to see all the referee reports. People getting to the interview stage have a 40-50% chance of getting it, the overall success rate I think is around 7 – 10%. The interview panel is large (around 16 people, most of whom are not even really close to your field). e.g PE2 which is the main panel for high-energy theory also serves not just all experimental and theoretical particle and nuclear physics, but also AMO/ tabletop atomic experiments. The interview is about 30 minutes.

19. **Don**
October 3, 2014

Dear Professor Woit

I am wondering if you have looked at a possible connection between Multiverse mania today and Cantor's set theory of ~ 100 years ago. I have been studying the status of Cantor in mathematics and it is even today contentious. There seem to be parallel sociological situations. In phyiscs, string theory and multiverse mania represent the institutional and media mainstream, and in mathematics the axiomatic method of Hilbert, and its foundations in Cantor's set theory, seem to be the mainstream.

The idea of transfinite numbers seems to have much in common with the idea of multiverses. But as I learn more about transfinite numbers, they seem more and more like fairy tales, or "castles in the sky", similar to multiverse ideas. I increasingly sympathize with people that advocate finite-ist mathematics.

Anyway, it seems there should be a connection, maybe even a lineage between the glib treatment of infinity in mathematics and the current glib treatment of infinity in the multiverse ideas.

I apologize in advance if you have written about this in the past and I am being redundant, and would appreciate links to your thoughts about the matter.

Thank you sincerely,

Don DeGracia

20. **Peter Woit**
October 3, 2014

Don,
Sorry, but my knowledge of set theory and trans-finite numbers is minimal. So, I don't see a lot of parallels with the multiverse, beyond the fact that both have potential problems of "glib treatment of infinity".

21. **Chris W.**
October 3, 2014

CPV,
Thinking—or even routinely observing—that classically emergent behavior *can* occur at scales many, many orders of magnitude larger than quantum behavior is not the same as understanding why it *does* occur, given that the world is fundamentally quantum mechanical. That is the issue that Zurek, et al, have been grappling with.

22. **Don**
October 4, 2014

Dear Peter

Thank you for the reply. That's too bad. I was hoping you might have some insights to shed on the issue. I will continue to study the issue, which seems to involve the split of math into the different camps of formalists, logicits and constructivist. Again, there seems to be a sociological dimension of comparison with current physics.

For example, formalists accept Cantor's ideas of "magnitudes of infinity" (e.g. transfinite "numbers"), but constructivists do not, because one cannot construct examples of such "numbers". This seems analogous to the situation in physics, where some physicists are constructing in-principle untestable theories, vs. those who demand a strong grounding in empirical facts.

Again, Sir, thank you for the input.

Best wishes,

Don

23. **BooToo**
October 4, 2014

Don-
As a working mathematician, I use the ideas of orders of infinity
(countable, uncountable) and even the ordinal numbers, first uncountable ordinal, all the time; these are very well understood on a "practical" level, meaning we can prove everything rigorously and also have a complete understanding at an intuitive level of what is going on. This is Cantor's beautiful universe and seems to me not at all comparable with physics, altough some try to make that stretch. For instance physicists use Quantum Mechanics to do calculations despite not understanding the measurement problem, but my impression is that there is some very basic and important level at which they (still) do not understand things. Hence all the discussion
e.g. by Peter.

Now though I am not in logic or set theory, I have taught "naive set theory" and can attest that everything there, including at first strange concepts like the Axiom of Choice, Russell's Paradox and ordinal numbers, also non-well-founded sets…makes perfect sense. (And is fascinating: it is, certainly, a triumph of human thought).

Even, say, the idea of having say different models for the real numbers, as in nonstandard analysis, seems not really objectionable. These are "models", that is they reflect actual properties of the reals, but carry additional structure, which one may or may not want to consider at any given moment. Perhaps an analogy is that a differentiable map "carries" a lot of hidden extra structure: it defines actions on other spaces (the tangent bundle; the space of continuous functions; spaces of paths; groups of matrices; the collection of differential forms; the collection of measures; on homology and cohomology; some push forward, some pull back…) So you choose which models, and which axioms, suit your immediate purposes, and there is no contradiction. All is rigorous.

The multiverse stuff (including the Many Worlds interpretation) seems to me to
to be in a completely different world, not at all analogous. First of all it seems (to this outsider) to be completely non-rigorous mathematically. Evidence for this is that there are so many disagreements among experts. That doesn't happen in math.
More evidence is the fear leading to depression among experts that years of their lives have been lost writing preprints (8000 citations of Witten????) which may prove completely useless.
Again, this can't happen in math, where things are actually proven and hence intrinsically, and practically, worthwhile.

More evidence is that, despite the potential mathematical difficulties of say string theory or quantum field theory being far beyond what many of us mathematicians encounter in our own small efforts, sometimes the techniques being attempted on these enormously difficult problems are ridiculous in their simple-mindedness (I saw a paper on the multiverse measure problem, which it is not AT ALL clear makes ANY sense at all, based on a simple Markov chain model as taught at an undergraduate level- please give me a break).

As to constructivism etcetera, again this iscan be viewed as an issue of how much structure you want to carry along with you- for instance if you want something to be computable or verifiable and so on, but it is not really an issue of what is "right" or "wrong". That is not the case in physics, where Nature (i.e. experiment) is the ultimate arbiter.

So, I would answer your question more or less as follows: "Is there a useful analogy? No, not really".

24. **Bill**
October 4, 2014

How come this doesn't get more attention…

25. **Bill**
October 4, 2014

Better link.

26. **Peter Woit**
October 4, 2014

Bill,
These are not elementary particles, but quasi-particle excitations in a condensed matter system. This is interesting condensed matter physics, but has nothing to do with fundamental particle physics, or possible Majorana neutrinos, dark matter etc. If you compare the article you linked to to the Princeton press release it is derived from, you'll see that the Princeton document does its best to hype this by bringing in neutrinos and dark matter:

"In addition to their potential practical uses, the pursuit of Majoranas has broad implications for other areas of physics. Scientists believe, for example, that another sub-atomic particle called neutrinos, which also interact very weakly and are very hard to detect, could be a type of Majorana—a neutrino and anti-neutrino being the same particle. In addition, scientists regard Majoranas as possible candidates for dark matter, the mysterious substance that is thought to account for most matter in the universe, but which has not been directly observed because it also does not directly interact with other particles."

The idea that this has "broad implications" for neutrino physics or dark matter is nonsense, and you can see why the writer was taken in by this. Great physics, but not a shining moment for the communication of science to the public by my alma mater…

27. **Bill**
October 4, 2014

Indeed, very misleading. Does this mean that this "particle" is its own "antiparticle" only by analogy?

28. **Peter Shor**
October 4, 2014

@Bill: the "quasi-particle" is its own "anti-quasi-particle" in a very real sense, but these quasi-particles are not elementary particles, but are collective excitations which behave like particles, much like phonons.

29. **Don**
October 4, 2014

Dear BooToo

Thank you for such a detailed reply. I do not know if Prof. Woit's blog is an appropriate place to further develop this conversation. I have included my website in this reply. If it is possible for you to email me, perhaps, if you have time and do not mind, I can ask you some further questions about your comments. It would be most informative to speak to someone who actually uses these ideas in their work. Again, very much thanks for taking the time to explain the above. Best wishes, Don.

30. **Another mathematician**
October 4, 2014

Sorry, Don, but I'll be more blunt and say that the idea of a "a possible connection between Multiverse mania today and Cantor's set theory of ~ 100 years ago" is a complete non-starter. It's just not worth pursuing.

31. **Florin Moldoveanu**
October 6, 2014

I am a Zurekian myself and I am not "a closet Everettian in denail" as Matt put it. I do take to heart Everett's message: "let quantum be quantum" but the world split idea of MWI is just a very cheap shot to solving the measurement problem. Quantum evolution is only unitary and it looks like a unitary description of the collapse is a contradiction in terms, but it is not so. The solution is to use Grothendieck group construction which introduces the inverse operation to the tensor product (understood as a commutative monoid). The inverse operation reduces the dimensionality of the Hilbert space and corresponds to wavefunction collapse. To get to Grothendieck group construction one needs an additional ingredient, an equivalence relationship. QM naturally has an equivalence relationship, and Zurek discovered it: it is envariance: "what the system evolves unitarily over here can be undone by the environment evolving unitarily over there". There is only one additional open problem: spontaneously break the Grothendieck group. In certain cases this was done rigurously, but what is lacking is a proof of the universality of the mechanism. When this is achieved it will lead to the complete solution to the measurement problem. Proving it requires advanced math.

32. **GoletaBeach**
October 6, 2014

12 hours until the physics Nobel.. no buzz? 51 years since the prize was given to a woman… an award to 3 all female would be great.

33. **Don**
October 6, 2014

Hi Another mathematician

Thank you for the reply. Even if the thesis is wrong, it is always nice to see exactly why it is wrong. BooToo's explanation is perfectly cogent and suggests the parallel between mutiverses and set theory is incorrect at some very basic levels. But this is a side issue with respect to the things I am wondering about math in general, for which BooToo's reply is also helpful, but also provocative.

I have been reading up on the old controversies from Brouwer and other critics (Weyl, Poincare) of the formalist approach (e.g. of Cantor, Hilbert, Zermelo, etc) to mathematics. Are these different philosophies of math a case of people agreeing to disagree? Do formalists have reasons for rejecting constructivists and intuitionists and vise versa? How can such seemingly incompatible approaches to math co-exist?

These are really the issues I am trying to sort out. It is hard to ignore Poincare, for example, when he criticizes Cantor. My initial impression from what I have read is that the respective approaches could be considered "progressive" (formalists) and "conservative" (constructivists).

If such thinking is not too far off base, the tie in with multiverse mania was an analog at this level: e.g. some one like Dr. Woit is "conservative", insisting on traditional criteria of scientific acceptability (something I easily accept, being a bench scientist), and the multiverse people are more "progressive" in that they are willing to trash traditional scientific values, similar to how Cantor trashed the centuries long prohibition on treating infinity as a mathematical object.

But again, my main concern is to understand, with at least as much depth as I can appreciate not being a professional in this area, the status of the situation in math.

Again, thank you for your input.

Best wishes,

Don

34. **Peter Woit**
October 6, 2014

Don etc.
Please, enough here about Cantor, this is just far off topic.

Goleta Beach,
No idea who will get the Nobel this morning. However I do predict that on Nov. 9 your neighbor Joe Polchinski will be $3 million richer….

35. **Bill**
October 7, 2014

They should just rename it a Nobel prize in engineering…

36. **NP**
October 7, 2014

Let's not make foolish value judgments. Marconi won for the wireless, Jack Kilby won for the integrated circuit (Robert Noyce was dead), Ernst Ruska won for the electron microscope, there was a prize a few years ago for giant magnetoresistance (don't remember the details) … all were good physics. Was it 'engineering' for Don Glaser to invent the bubble chamber?

37. **Michael Gogins**
October 7, 2014

Here is a link to a freely available Los Alamos publication on Quantum Darwinism and randomness: http://permalink.lanl.gov/object/tr?what=info:lanl-repo/lareport/LA-UR-14-24063

Regards,
Mike

38. **Hoyuna**
October 7, 2014

Progress in experimental HEP has slowed down considerably, so there's a good chance that other branches of physics (especially applied physics) have struck 'Nobel gold'. Depending on one's level of optimism, one might predict no end for this trend.

What do you think, Peter?

39. **tt**
October 7, 2014

engineering ?
the arrogance of particle theory never ceases to dissapoint.

40. **Bill**
October 7, 2014

Sure, it is a great engineering invention by three engineers, so create a Nobel prize in Engineering. Don't call it a Nobel Prize in Physics. I might be wrong though, I am not a physicist. Are LED lights used in physics besides lighting the offices and burning DVDs (although this is already outdated)?

41. **Peter Woit**
October 7, 2014

All,
I personally know zero about blue leds, and don't think arguing about what's engineering and what's physics is very interesting, enough of that.

Yes, progress in HEP physics is much slower than in the past, with implications for physics Nobel prizes. More relevant though may be that if the Swedish Academy sticks to refusing to award prizes to experimental groups, that may be what will shut off HEP physics prizes. That the experimental discovery of the Higgs was not prize-worthy, and maybe never will be, shows there's now a problem even if all of a sudden lots of great experimental discoveries are made at big colliders.

42. **GoletaBeach**

October 7, 2014

Blue LEDs are used throughout those particle physics experiments which use photomultiplier tubes, like, say, most of the large neutrino experiments. The emission spectrum from the Blue LEDs matches the sensitivity of most PMTs better than the old Green LEDs. The Blue LEDs are used for monitoring and calibration.

I recall condensed matter theorists telling me in the 1970's that Blue LEDs were impossible due to some limitation or another. Now Ultraviolet LEDs are available. Nakamura was unrelenting in his empirical pursuit of an idea that was quite unpopular at the time. Combine that with the practical applications and it seems like a good prize to me. Only worry is that maybe the invention of the first LED of any type might have gone unrewarded.

It remains embarrassing that the last Nobel shared by a woman was in 1963.

From Goleta Beach it is easier to see the College of Engineering at UCSB than the KITP. The Engineering buildings are taller, and they surround the KITP.

43. **Niclas**
October 8, 2014

Alfred Nobel was an inventor and engineer. I agree that he would have liked the blue LEDs. Given that 20-25 of the world's energy is used for light then this is significant as the world needs to stop excessive energy consumption. Here is the motivation: http://www.nobelprize.org/nobel_prizes/physics/laureates/2014/advanced-physicsprize2014.pdf

44. **NP**
October 8, 2014

The 2014 Chemistry Prize is for 'optical nanoscopy'. They figured out how to beat the Abbe diffraction limit. Who is to say this is 'chemistry' not 'physics'? And it is the development of a useful tool, as opposed to a specific discovery of a phenomenon. Who is to say that is not 'engineering'? It is every bit worth a Nobel Prize, be it in Chemistry not Physics.

45. **Curious Mayhem**
October 10, 2014

Sorry to hear about Marty Perl.

When I first heard of the "many worlds" interpretation of quantum mechanics, I was baffled as to why it was considered science. (It isn't, of course.) It sounded like speculative metaphysics. Anyway, later I heard about Zurek, and his work struck me as the right approach. The issue had moved from metaphysics to physics, and you can measure it in a laboratory.

Bruno did not champion "many worlds" in the multiverse or Everett approach. He simply (and correctly) guessed that the fixed stars (in the same universe as ours) were other suns and could have planets, including planets with life. This left him with a theological problem, assuming orthodox Christian beliefs. Had he been a Buddhist, it wouldn't have been a problem, as "many worlds, many bodhisattvas" is an acceptable, if not common, view in Buddhism. Don't know about Islam. In Judaism, it was a view entertained by the Kabbalists, but definitely not a standard view.

46. **B'Rat**
October 11, 2014

nick herbert, Curious Mayhem,
this is not what happened with Bruno.
He was burned at stake, a barbarous act, but for his insistence in negating catholic dogmas like Trinity and so on, not because of his "many words" speculation. In fact, such idea had already been suggested more than a century before by Nicholas of Cusa, a papal legate who was made cardinal whom Bruno cited. The Aristotelian notion that there cannot be more than one World had already been condemned by the Bishop of Paris in 1277 on the basis that it limited God's omnipotence.

Since I do not want to start an off-topic discussion, I suggest you to add any ulterior comment on the argument in the following link, which discuss many misconceptions about Bruno's history:
http://thonyc.wordpress.com/2014/03/17/cartoons-and-fables-how-cosmos-got-the-story-of-bruno-wrong/

47. **Michael Gogins**
October 13, 2014

Regarding einselection, which appears to be required for quantum Darwinism to be a physically viable theory, what about this? http://philsci-archive.pitt.edu/10757/

Regards,
Mike

48. **anonymous**
October 14, 2014

Page's thought reminds me of the classical philosophy of occasionalism, perhaps most championed by Nicolas Malebranche, who though now widely disfavored was actually quite popular in his day.

Various News

October 15, 2014
Categories: Uncategorized

Various news about the usual topics:

- Natalie Wolchover at Quanta magazine keeps coming up with great, in-depth stories about interesting new topics in physics that are getting no attention elsewhere. Her latest is about the universality of the Tracy-Widom distribution.
- The LHC is cooling down, in preparation for a restart early next year. Nature has a good story about what is going on here. Latest status and plans are described here. The current plan is to start beam recommissioning next March, have 1 fb^{-1} by mid-June, in time to perhaps have some results to report at EPS-HEP2015 at the end of July. Another 10 fb^{-1} would be accumulated later on, before a heavy-ion run late in the year.

  In the long term, by 2023 there should be 300 fb^{-1} and many components of the machine and the experiments will start to become unusable due to radiation damage. Planning is going ahead for "Phase-II", or the HL-LHC, with Bertolucci's comment that "It is inconceivable under any reasonable scenario to stop the LHC program at that point".

- Nature has an editorial this week about What lessons can be learned from the presentation of the gravitational-waves story?, pointing to a planned discussion next week about Lessons in the communication of science from the BICEP2 story. I've already written extensively about this, but since the editorial refers to bloggers (and I know some people at Nature were unhappy with my blog entry about this, which was poorly worded), I'll take another opportunity to do so.

  From the purely scientific point of view, this is a pretty straightforward situation. The BICEP2 people fooled themselves into thinking that they had something much more exciting (primordial gravitational waves + evidence for inflation) than what they really had (a good measurement of B-mode polarization at one frequency). They then wrote a paper with over-optimistic claims, which later blew up in their face. This is perfectly normal science.

  What's not normal science is the behavior of a lot of theorists in response to the BICEP2 claims. The Stanford University Linde video and its 3 million downloads will live forever as an example of misguided PR for science. The comments from theorists about the significance of this for string theory that Nature quoted were an embarrassment for the field (why not just say that you could get any value of r out of string theory?), and even worse were the publicity campaigns from Linde, Guth and Carroll aiming to convince the public that this was evidence for the multiverse.

  What's the lesson for science journalists? Take a hard look at the behavior of some prominent theorists in this story, and draw the obvious conclusions for your future coverage of developments in this field of science.

- Just noticed that Sean Carroll is now trying to raise research funding online with a website devoted to attracting private funding. Will be interesting to see if that works, maybe it will become a model for how to fund this kind of research.
- One of the few things I'd change about my book written ten years ago would be the discussion of the philosophy of science "demarcation problem", that of deciding what is science and what isn't. Only after writing the book did I learn about the distinction between a "progressive" and "degenerating" research program due to Lakatos, which is a very good way of addressing the question of how to evaluate string theory. I also missed a paper that came out a few years ago by Johannson and Matsubara on String theory and general methodology. At one point they write that the string theory landscape business shows that:

  > String theory is a degenerative programme, according to Lakatos' criterion.

  There's a lot more in the paper, it's a good example of what I've seen too little of, philosophers of science engaging with the real issues here.

- For the latest on the string landscape, there was a conference last week on Fine-tuning, Anthropics and the String Landscape. See if you can find anything there like a plausible idea for how to get any testable physics, I couldn't. Alan Guth's introductory talk mainly explains why the measure problem means you can't predict anything, but then ends with a claim that physicists take the multiverse seriously anyway, quoting Weinberg from 2005 about Martin Rees's dog.
  Back in 2004-5, the expectation was that the string theory landscape could be used to predict whether SUSY breaking would take place at a high or low scale (see for instance here). That idea is long dead, and no other proposal for a prediction has replaced it. So, the string landscape is itself a degenerating research program. What do philosophers of science call it when a research program degenerates into something else, and that research program in turn degenerates. A (degenerating)^2 research program?
- The standard defence of string theory these days acknowledges that it can't explain particle physics, but claims it has had great success in quantum gravity. Next spring the KITP will have a program on quantum gravity foundations. The description of the program has a lot to say about "deep connections between quantum information theory and gravity", no mention of string theory. There seems to be a move away from string theory and a convergence between the KITP and the sort of alternative research favored by the Perimeter Institute.
- Speaking next month on Quantum Mechanics and Spacetime in the 21st Century at Perimeter will be Nima Arkani-Hamed, one of the organizers of the KITP program. Not clear what he'll be arguing for then, but he did just give a talk at an Oxford workshop on New geometric structures in scattering amplitudes, with the title "The Amplituhedron, Scattering Amplitudes, and the Wavefunction of the Universe". I'm curious to see how he gets the Wavefunction of the Universe, although I suppose one should keep in mind his comments here.
- Not announced yet what the price of tickets to Arkani-Hamed will be. For a real rock star of physics though, I think you want Brian Cox, who is on tour in Australia. Premium tickets there are about $175 US.

Update: The latest on the Journal of K-theory situation, from algtop-l

> Dear Colleagues,
>
> The time has come to advise your librarians to cancel the subscription to the Journal of K-Theory. The precious money could be better spent elsewhere.
>
> As you know the journal is going through a crisis. The most recent development is that the Bak family has written to Cambridge University Press informing them that they are under a contractual obligation to keep publishing the journal through the end of 2017, whether they like it or not. I haven't seen the contract in question, not have I seen the letter from the Bak family to Cambridge University Press, hence I cannot comment on the legal merits of the case. The Baks evidently feel confident, Tony Bak has accepted at least one paper for the 2015 edition of the journal without clearing it with any of the other editors.
>
> The Baks might be right, Cambridge University Press might have no choice but to continue publishing the journal. But the vast majority of the editors will be walking out and the scientific standards of the journal are bound to plummet. It would be a waste of money to continue subscribing.
>
> Yours, Amnon

Comments

1. **Sebastian Thaler**
   October 15, 2014

   Peter,

   Just curious if you've seen an advance copy of Lee Smolin's new book, being published next month by Cambridge. It sounds interesting and I'd love to hear your thoughts.

2. **Peter Woit**
   October 15, 2014

   Sebastian Thaler,
   I haven't seen the new book. From the synopsis, it sounds like it's largely along similar lines to Smolin's last book, which I disagreed a lot with, see here
   http://www.math.columbia.edu/~woit/wordpress/?p=5769
   He and I may agree about a lot of the problems with string theory, but we have completely different visions about how mathematics and physics relate to each other.

3. **Unemployed**
   October 16, 2014

   "…Sean Carroll is now trying to raise research funding online…"

   And I notice that his vehicle of choice, benefunder.org, says they'll take a flat 10% fee off all donations (or perhaps weaseled to >15% since they also claim researchers receive "nearly 85%".) Leaving aside the value of the actual research, doesn't a slapped-together website with 10% overheads seem like a bit of a scam?

4. **Shantanu**
   October 16, 2014

   Peter, I think PI is also gravitating more towards conventional physics in the last few years. You can get an idea by looking at their list of seminars and colloquia in the last few years compared to when they initially started.

5. **Peter Woit**
   October 16, 2014

   Shantanu,
   Will be funny if places like the KITP become the hosts for exotic speculation (eg. gravity is information), and PI becomes the place for staid, conventional research…

   Unemployed,
   My impression is that a 10-15% overhead expense for non-profit fundraising is not bad at all. And I thought the website (at least Carroll's part of it that I saw) was a well-put together professional effort.

   One danger of this kind of thing is not benefunder scamming the public, but the people advertising for funding doing it. When the NSF gives a scientist a grant, there are elaborate structures in place to make sure it is spent as intended, I wonder whether that's true in this set up. Is there really anything there to stop me from putting up a glitzy web-site advertising for funding for my investigations into the nature of reality and mathematics, but then using whatever comes in to finance my next vacation in Europe?

   This kind of funding raises a lot of interesting questions. Carroll's site makes all sorts of claims (just as a random example, that his "work is unique because he takes a big picture point of view that answers questions about the universe in the broadest possible terms") that would quickly get an NSF grant application put in the discard pile as not serious science. This is advertising aimed at the general public, something very different than making a scientific case to one's peers. The NSF is cutting back on support for HEP theory research, and a lot of new funding is coming in from private sources. So far, most of the private money has come from wealthy foundations, which often have a sophisticated staff to judge proposals and make sure the money is spent appropriately (as well as sometimes people like Jim Simons who can make scientifically informed judgements). That situation raises one set of issues, which I've discussed here often. Fundraising via direct advertising to the public, without oversight by foundation officers, is something I haven't seen before, I wonder how it will play out. What would theoretical physics look like 100 years from now if this becomes the model for how to fund it?

6. **Martin S.**
   October 16, 2014

   This kind of funding raises a lot of interesting questions. Carroll's site makes all sorts of claims …

   Hi, is something wrong about putting my own money directly to a research proposal, whatever vague it is? Does it mean that when a Joe gives a few bucks to Carroll, CERN looses some funding? According to my experience, research foundations tend to support current views and approaches with near and specific targets, thus effectively putting barriers on unbeknown ways. Any new source, especially when allowing more fundamental work (that usually needs a lot of relaxing), should be welcome, I would say.

7. **Peter Woit**
   October 16, 2014

   Martin S.,

   I'm not making any simplistic claims about what is bad or good. I just think that people should think about the significance of changes in how research is funded, not just assume that all new money is a good thing . At least in the US, it is a fact that government funding for theory research is being seriously cut back, and private funding is moving in to replace it.

   If the private model is a success, you'll likely see arguments made in Congress that this is a subject where government funding should be removed. You'll also see universities moving from hiring people who can get NSF grants to hiring people who can bring in money this way.

   The NSF peer-reviewed model for funding is not ideal, suffers from problems of favoring unambitious or conventional research. But there are also problems with funding research that makes grandiose claims about ambitiously revolutionizing the subject (I get a dozen of such claims in my email each week….). Is it better for people to have to convince their peers to fund them, or have to convince the general public/rich people (who don't actually understand anything about what they're funding). Both have problems…

8. **Baleen**
October 16, 2014

Doesn't the core of the anthropic argument, an "unnatural" lambda, basically render the entire landscape programme infeasible?

If lambda is unnatural then finding our vacuum state would involve working out a bunch of different quantities associated with some particular 10-dimensional CY manifold and then seeing if they all cancel each other to *120 decimal places*. No-one has even been able to use plain old lattice QCD in three dimensions to calculate the proton mass to *one* decimal place!

9. **Peter Woit**
October 16, 2014

Baleen,
Yes. And even if you could do that computation, there's this problem
http://arxiv.org/abs/hep-th/0602072

The whole string landscape business is very odd. There are all sorts of strong arguments that you can't get anywhere, have been for ten years. Everything people have done just makes clear these are insuperable problems, and yet, it somehow remains a subject people work on, and make claims to the media that this is a great insight into how physics works.

10. **Martin S.**
October 16, 2014

@PW: I understand that both of the approaches have issues, and that any change has two sides. Still, regarding peers: they are at conflict of interest, grouping into mafia structures, they (according to my experience) generally do *not* understand stuff slightly off their own work, and they tend to dislike anything that goes a different way than they do, and more it is relevant to their own work, more irrational they are. Thus I personally feel sympathies to this new public approach.

PS Is not your fear alike Anderson's fear about SSC?

11. **Peter Woit**
October 16, 2014

Martin S.,
Anderson was worrying about the allocation of money between fields of physics, I think this is kind of different.

Maybe one source of my worry about this has to do with seeing how US universities have changed during my lifetime. They're now a lot more market-driven and PR-driven, and this is not necessarily a good thing for scholarship. And, for whatever reason, I seem to have an aversion to PR as opposed to serious discussion.

12. **Martin S.**
October 16, 2014

@PW
I seem to have an aversion to PR as opposed to serious discussion.
That's the point: one day you can discuss with the public on a blog, the other day you can try to convince your peers within academia. Or it can actually go like: today a nonsensical PR, tomorrow a power abuse within the institution. Thus it seems to be about the general approach, not about a currently followed path.

Whatever way it will go I wish you good luck. And I guess that this blog puts you into a good position regarding a reasonable communication with the public.

13. **Daniel Fischer**
October 16, 2014

Re. BICEP2 Lawrence Krauss made an interesting case today – in a talk called "Cosmology for Philosophers" at Bonn University in Germany – that there could well be a cosmological signal in the data after all (with a lesser r, I suppose, but good enough to test inflation). And that we should all wait until the end of this year when the *joint* BICEP/Planck paper will come out. So I will.

14. **Peter Woit**
October 16, 2014

Daniel Fischer,

Sure, there could be an observable non-dust signal, we don't know yet, but back in March all BICEP2 had was an observation of B-modes at a level consistent with that expected from dust. Claiming a "five sigma" observation of primordial gravitational waves was just completely wrong and unjustified.

Any real evidence of such a signal will be based on joint work of the two experiments, or maybe on Planck's all-sky results on their own. Supposedly we'll know end of next month. Whatever happens, doesn't change the fact that the March headlines and PR were an embarrassment and a fiasco, a good example to various people of how not to behave.

15. **Ramanujan**
October 17, 2014

In the Linde video, the experimentalists showed up at his door unannounced, and claimed a discovery that was not supported by the evidence. How do you transmute this into a statement about "the behavior of theorists" ?

16. **Lee Smolin**
October 17, 2014

Dear Sebastian Thaler and Peter,

My new book, The Singular Universe and the Reality of Time is written with Roberto Mangabeira Unger, a Brazilian philosopher who is Professor of Law at Harvard, and is thus a very different kind of book than my previous books. I hope you will find more to agree with in it, or at least take it as a challenge to argue with. It supports the conclusions of Time Reborn, but it is more radical and goes deeper.

In the end, you and I may disagree about the nature of mathematics and its proper role in physics (about which there is much more in the new book) but I suspect we agree that what really matters is to develop theories that make testable predictions for doable experiments. The new book lays out a program for doing that and presents some early steps.

Best wishes,

Lee

17. **Peter Woit**
October 17, 2014

Lee,
Thanks, I look forward to seeing it.

Ramanujan,
Yes, you're right, the experimentalists have most of the blame for that video. Linde's claims about "a smoking gun" were in the Stanford press release, not that video, see
http://www.math.columbia.edu/~woit/wordpress/?p=6958

Linde could have pointed out to the guy who showed up at his door that while r=.2 was all well, and good, he had versions of inflation for any value of r.

18. **Don Murphy**
October 18, 2014

Peter wrote:
Not announced yet what the price of tickets to Arkani-Hamed will be. For a real rock star of physics though, I think you want Brian Cox, who is on tour in Australia. Premium tickets there are about $175 US.

The Tickets for the Arkani-Hamed talk are free.

19. **Nathalie**
October 18, 2014

The reason why tickets to Brian Cox show cost $175 and to Arkani-Hamed talk nothing is simple to understand.
Having attended talks by both I would say that Cox knows much less physics but is much better as entertainer, for people who don't know the subject.

20. **Yatima**
October 19, 2014

I didn't know who Brian Cox is, but he's apparently [also writing books](#).

21. **anonymist**
October 20, 2014

From Alan Guth's presentation:

> * For every real Alan Guth, there will be an infinite number of BB[]s who will think they are Alan Guth, sharing exactly all my memories and all my thought processes.
>
> * With overwhelming probability, the BB[]s who share my memories will see the world rapidly disappear, since for them it never existed. Thus, with overwhelming probability, beings who think they are me will see the world rapidly disappear.
>
> * Conclusion: our continued observation of a coherent world gives overwhelming evidence that we do *not* live in a world with an infinite (or huge) ratio of BB[]s to normal observers. Hence a naive measure based on our own pocket universe is unacceptable.

I'm no physicist or expert of any kind, but doesn't point 1 completely undercut the conclusion reached in point 3 here? At any given point in time, Alan Guth's observation that he has continued to observe a coherent world over the previous five seconds, or two weeks, or 62+ years, can only be based on what he can recall of that stretch of past time. But if what he remembers, or thinks he remembers, is a function only of what his present brain-state is … and if (by point 1) that brain-state is (barring some extra constraint) more likely a product of fluke than the kind of environment and life history which his brain state tells him that he has … then what reason does he have to trust his recollection of experiencing a normal world over the past however-long? His recollection doesn't let him conclude a suitable extra constraint exists, because his recollection itself requires the existence of such an extra constraint to be a reliable witness to that effect (or to almost any effect).
And it obviously doesn't work for him to say "aha, I'll settle this through experiment, by waiting five seconds to see if I still exist and life continues as normal during that time". At the beginning of the five seconds the results of the experiment are of course not in yet. At the *end* of the five seconds, the results may be in but now he's facing the equally uncertain question of whether his last several seconds of remembered past, including the decision to test his Boltzmann-brainness by waiting five seconds, were fictitious or not!

22. **Peter Woit**
October 20, 2014

anonymist,
The "Boltzmann brain" business was a hot topic in certain circles six years ago, see for instance
http://www.math.columbia.edu/~woit/wordpress/?p=836

I don't think anything worthwhile ever came out of that debate, and it seems to have died out for very good reason. I have no idea why Guth is reviving it.

All: please don't try and revive this debate here. The probability of anyone having anything interesting to say about it at this point seems to me to be about the same as that of a Boltzmann brain popping into existence.

23. **Anon**
October 20, 2014

http://people.maths.ox.ac.uk/lmason/NGSA14/Films/
http://people.maths.ox.ac.uk/lmason/NGSA14/Slides/

24. **paddy**
October 20, 2014

Thank you PW.
I have enjoyed very much over the last several days (1) delving into where the Natalie Wolchove Tracey-Widom article have lead me and (2) reading of Lakatos' ideas in philososphy of science (was pretty much ignorant of them before this).

Yet More News

October 24, 2014
Categories: Uncategorized

- Charlie Munger, the billionaire business associate of Warren Buffett, has donated $65 million to the KITP at UCSB for the construction of a residence for visitors. For more on this, see a UCSB story, a New York Times article, and for some background, 90-year old Munger's explanation that "I won't need it where I'm going".
- On the other coast, today and tomorrow at Princeton there will be a workshop on string cosmology and inflation. They have a list of questions to be addressed, including

  > Are there any plausible alternatives to string/M-theory as a fundamental theory of physics?
  > …
  > Does string theory make any cosmological predictions? Does it exclude anything?

  As far as I can tell, there's an odd consensus set of answers to these two questions among string theorists. No, string theory makes no predictions about cosmology, but also no, there are no alternatives.

- For an interesting discussion of the problems raised by this sort of "no possible predictions, but no alternatives" situation, see this debate involving John Horgan, David Tong and Tara Shears. Horgan does a good job of pointing out the problem. Tong's defense of string theory relies heavily on claiming that it is highly mathematically rigid, so mathematical consistency is what can give us faith in it. One problem with this is that the whole string theory landscape picture is an extremely ill-defined conjectural framework, the opposite of mathematically rigid. Yes, there are parts of string theory that seem to be mathematically consistent and lead to interesting results. The problem is that those have nothing to do with what is observed about fundamental physics.
- Jim Gates has an article about Sticking with SUSY, despite no evidence from the LHC. He explains that the thing he finds most convincing about SUSY is the cancellation in divergent vacuum energies between fermions and bosons (or at least that's how I interpret his comments). I'm actually somewhat in sympathy with this. One thing I've been writing about in my quantum mechanics notes is the beautiful parallelism between "bosonic" and "fermionic" quantization. A fundamental theory needs both, and likely has some super-algebra of symmetries acting on it. I just don't though see a good argument for the realization of this general idea in terms of the standard kinds of extensions of the Poincaré algebra to a superalgebra. These don't appear to tell us anything about physics we know about, and predict physics we don't see.
- I was hoping to have time last Sunday to see a discussion at the French Embassy between John Nash and Cedric Villani, part of their Festival Albertine. Unfortunately I ran out of time to do this, but luckily for you and me, video is available here.

Comments

1. **Lee Smolin**
   October 24, 2014

   Dear Peter,

   If the conferees want an answer to the question "Are there any plausible alternatives to string/M-theory as a fundamental theory of physics?" they ought to invite some of the people contributing to the development of such alternatives and have an honest examination of the question. They have plenty to choose from, they could start with the speakers at Loops 13, which 200 people attended: http://www.pirsa.org/C13029.

   Fifteen or twenty years ago it was possible to say that string theory was more promising than its alternatives, and indeed 15 years ago I switched my research from LQG to M theory. But I think that any objective evaluation of the evidence has to credit much more substantial progress has taken place since concerning the alternatives; especially spin foam models, but also other background independent approaches including shape dynamics, CDT, group field theory and tensor models, etc.

   To support this I would point to the fact that much more is known from spin foam models about the challenges it faced 15 years ago, including substantial recent results on the emergence of GR in the semiclassical limit, the entropy and temperature of generic black holes, the elimination of cosmological and black hole singularities, finiteness, etc.

   So, I would urge my string theory friends who think they know the answer to the question to have another look. There is a whole generation of brilliant young theorists working on alternatives to string theory you should meet!

   Thanks,

   Lee

2. **Marty Tysanner**
   October 24, 2014

   The Horgan/Shears/Tong debate was interesting, but I found the some of the reasoning (especially by Tong) to be wanting. A few examples:

   1. Mathematical consistency as a pillar of science (experiment being the other): Tong argued the internal consistency of string theory should be taken as significant evidence for it even without experimental evidence. The obvious problem he ignored is that mathematical/logical inference is only useful in physics if the starting *physical* assumptions are correct. Even if string theory were completely consistent mathematically, to justify his argument Tong would need to show that its underlying assumptions are uniquely implied by observations, a task that isn't humanly possible. (One can't exhaust all possible alternatives — that requires unlimited creativity and time to propose and carefully work out all possible theories and their consequences.) Experimental evidence of deduced consequences of his favored theory is *mandatory* to help validate its initial assumptions — "pure thought" doesn't cut it.

   2. Tong argues for the proposal of atoms in the 18th century as an example of "pure thought" that only later was shown true experimentally. Atoms were proposed as a possible way of explaining concrete observations — the idea was driven by unexplained observations, not "pure thought." Is Tong suggesting that any newly proposed model that attempts to explain new phenomena, but only later is supported observationally, supports a conjecture that his favorite quantum gravity theory will someday have concrete observational evidence specific to that theory?

   3. Dark energy. The moderator tried to point out the distinction between excellent *observational* evidence of accelerated expansion of the Universe, and the *theoretical* nature of conclusions that it is due to dark energy. Tong insisted the acceleration must be caused by dark energy because GR predicts the acceleration if dark energy is inserted (i.e., as a cosmological constant in Einstein's equations); therefore "dark energy" is not just a theoretical conclusion. He overlooks that, while observations are *consistent* with a c.c., they don't demonstrate (yet, at least) that the acceleration is uniform everywhere and that therefore dark energy is the *only* plausible cause. The Lambda-CDM "concordance model" hasn't yet attained an observational status that makes it unassailable…

3. **Art Brown**
   October 24, 2014

Thanks much for the link to the Festival Albertine video. I had hoped that the topic of “style” might form a bridge across the chasm between hi-level math and the rest of us, but that organizing principle damped out in the first half hour or so (to re-emerge, it is true, in the final question on the math “lab” vs solo work).

I was surprised that Dr. Villani appeared to downplay the role of intuition in guiding his work; I had thought that quite important, and look forward to his book to learn more. (I suppose that also qualifies as “style”, so maybe the topic was more successful that I at first thought.)

Two trivial notes:
1) Was that a large spider lapel pin I glimpsed from time to time on Dr. Villani’s jacket?
2) Microphone technology seems stuck in the 20th century: if you aren’t within licking distance, it’s useless.

4. **Yatima**
October 25, 2014

Unrelated and Classical: In the area of “Amazingly Correct Pop Science”, Kip Thorne has been tasked by Warner Brothers to come up with a visually correct Black Hole. Disregard the default “the feels” music:

Interstellar – Building A Black Hole – Official Warner Bros.

5. **amused**
October 25, 2014

I expect that alchemy had a good degree of `mathematical consistency’ too, back in the day when heavyweights such as Isaac Newton and Robert Boyle were working intensely on it.

6. **David**
October 25, 2014

Dear Peter,

Can we find the slides or videos of the workshop of string cosmology and inflation at Princeton?

7. **Peter Woit**
October 25, 2014

David,
Sometimes someone posts slides and/or videos of talks at these PCTS workshops after they’re over, sometimes not. It’s still going on, so, wait and see. Most likely they would be posted at the workshop website
http://pcts.princeton.edu/pcts/StringOpen2014/StringOpen2014.html

8. **Jesper**
October 25, 2014

Dear Lee

could you provide links/references to the recent results you mention on emergence of GR in a semi-classical limit?

Thanks

9. **Lee Smolin**
October 25, 2014

Dear Jester,

There are many papers which discuss the emergence of GR from spin foam models; here is a small selection of them, in no particular order.

http://arxiv.org/abs/1108.2258, http://arxiv.org/abs/1304.5626, http://arxiv.org/abs/1109.6538, http://arxiv.org/abs/1308.4063, http://arxiv.org/abs/0809.2280, http://arxiv.org/abs/0902.1170

Thanks, Lee

10. **vmarko**
October 26, 2014

Jesper, Lee,

Just for completeness, there are also some criticisms regarding the way GR emerges from spin foam models (and ways to get around that). See for example

arXiv:1307.5352, arXiv:1104.1384, arXiv:1111.2865, arXiv:1201.2187.

11. **Bob**
October 26, 2014

Dear Lee,

Thanks for those paper references. They look interesting. I have a question for you. LQG, spin foams, etc., are characterized as a quantum theory of pure gravity, without matter and matter interactions. Can you discuss the ways in which Loop Quantum Gravity addresses the criticisms that quantum gravity needs to unify gravity with the other interactions because gravity cannot be decoupled at Planck scales from all other interactions (if there are any) at Standard Model, and higher, energy scales? Thanks very much!

12. **Oregonator**
October 26, 2014

I can’t make sense out of Nash’s 4th order gravity equation. The Lagrangian is that of conformal gravity except that he has replaced the 1/3 by a 1/2. Why ?

13. **Lee Smolin**

October 26, 2014

Dear Bob,

Actually there is a moderate sized literature on matter coupling and unification within LQG. To summarize, let me emphasize that there are several different questions.

1) It is just not true that "LQG, spin foams, etc., are characterized as a quantum theory of pure gravity, without matter and matter interactions." From very early in its development, it was worked out in detail how to include gauge fields, fermions and scalars in LQG. For example, see Thomas Thiemann's book.

2) Extension to supergravity is also straightforward, at least for N=1, including D=11. See papers by Yi Ling and myself as well as recent papers by Thiemann and collaborators,

3) Hence, even if LQG doesn't constrain the matter or gauge field content of what it is coupled to, LQG provides a framework in which matter gauge fields and gravity are all coupled together.

4) There are issues that need to be addressed with fermion doubling which are the subject of work in progress with Jacob Barnett.

5) You can ask in addition whether, "quantum gravity needs to unify gravity with the other interactions". This from a LQG point of view is an open question.

6) But you can ask if LQG gives a compelling framework for unifying the different interactions? Here the answer appears to be yes. The simplest possible extension of the theory, gotten by extending the gauge group of gravity, which is the Lorentz group, to a larger group, G, yields a natural extension of Einstein-Yang Mills. This comes about from a built in spontaneous breaking of the gauge group G to the product of the Lorentz group with a compact factor H, which becomes the Yang-Mills gauge group. See arXiv:0712.0977, older papers by Peldan and later papers by Krasnov et al. See also arXiv:1212.5246 which explores implications for a unification of the electroweak interactions with gravity.

7) Going beyond all these solid results are indications that the chiral fermions of the standard model may emerge from topological excitations of spin network states, arXiv:hep-th/0603022.

Clearly there is much still to do in this direction, but these are sufficient to assure us that LQG provides a framework within which to describe matter and gauge fields coupled to quantum gravity which can suggest hypotheses as to further unification.

Lee

14. **Noboru Nakanishi**
October 26, 2014

Dear Peter,

SUSY is an unnatural symmetry when gravity is taken into account, because the local version of translation group already include Lorentz group. Indeed, in the Einstein gravity, the local Lorentz group is completely disconnected from general coordinate transformation group. This situation cannot be resolved by considering supergravity, in which the Poincare symmetry becomes internal symmetry and therefore quantum supergravity necessarily becomes inconsistent with SUSY in the sense that the anticommutator between two supersymmetry generators cannot yield the translation generator.
I think it more natural to supersymmetrize the local Lorentz symmetry alone. Since its Lie algebra is sl(2, C), its natural "super"extension is the orthosymplectic superalgebra osp(N,2; C).

15. **Marc**
October 27, 2014

Kaku still keeps on spreading the message (to the public, unfortunately) that string theory is the only game in town.
https://www.youtube.com/watch?v=fV1eAAvAQVc (4:50)

16. **Aleksandar Mikovic**
October 27, 2014

Dear all,
The exact statement regarding the classical limit of a spin-foam model is that it is given by the area-Regge theory (see arxiv:1104.1384) whose geometry is different from the the usual Regge discretization of General Relativity (these geometries are known as the twisted geometries, see arxiv:1308.0040). A natural way to solve this problem, as well as the problem of coupling of fermionic matter to a spin-foam model, is to introduce explicitly the edge lengths into a spin-foam model, which can be done generalizing spin-foam models by replacing the symmetry group (Lorentz group) with a 2-group (Poincare 2-group), see arxiv:1110.4694 and arxiv:1302.5564.

17. **Bob Teller**
October 27, 2014

Kaku discussing the LHC and its latest discovery, "The Higgs boson is included in string theory."

18. **Shantanu**
October 27, 2014

Peter, something OT
A talk by Tom Kibble on history of EW symmetry breaking
https://cds.cern.ch/record/1956423?ln=en
First time I have hard a talk by Kibble

19. **David Metzler**
October 27, 2014

Roughly on-topic: I notice that my recent Phi Beta Kappa newsletter has a quote from member Brian Greene: "The boldness of asking deep questions may require unforeseen flexibility if we are to accept the answers." One may reasonably ask just how much flexibility is appropriate, however.

20. **Peter Woit**
October 27, 2014

David Metzler,
Google tells me that quote is from Brian's 1990s "The Elegant Universe", and in context refers to the unintuitive nature of quantum mechanics and relativity. The

"unforeseen flexibility" there isn't the dubious flexibility about what's science and what isn't currently in vogue among some string theorists.

21. **Cormac**
    October 28, 2014

    Hi Peter, I too enjoyed the Gates article, thought he hit the nail on the head.
    How did you manage to give a link to a PW article without password issues? I had an article in the September issue of PW myself, but couldn't show it to colleagues in the US, am I missing something?
    Regards, Cormac

22. **QG**
    October 28, 2014

    Yet More News
    Posted on October 24, 2014 by woit
    On the other coast, today and tomorrow at Princeton there will be a workshop on string cosmology and inflation. They have a list of questions to be addressed, including

    Are there any plausible alternatives to string/M-theory as a fundamental theory of physics?
    …
    Does string theory make any cosmological predictions? Does it exclude anything?
    ^

    it's October 28, 2014 and the workshop was Oct 24.

    has anyone seen this Princeton workshop on string cosmology and inflation? what is this workshop's answer to the question

    Are there any plausible alternatives to string/M-theory as a fundamental theory of physics?
    …
    Does string theory make any cosmological predictions? Does it exclude anything?

    regards

23. **Peter Woit**
    October 28, 2014

    Cormac,

    Not sure where I saw a link to the Gates article. I couldn't actually find it on Physics World. No special knowledge about linking to them…

24. **David Metzler**
    October 29, 2014

    Peter—I appreciate the clarification on the Greene quotation, it sounds much more reasonable now. It's a shame that it is so hard to convey to the general public the difference between the rigorously tested but counterintuitive notions of quantum mechanics and relativity, on the one hand, and the dubious claims of (some) string theorists, on the other.

25. **Hendrik**
    November 3, 2014

    Some recent hype that may be worth noticing:
    http://www.eurekalert.org/pub_releases/2014-11/uosc-sft110314.php

26. **Peter Woit**
    November 3, 2014

    Thanks Hendrik,
    That required its own blog posting…

Breakthrough Prize Ceremony

October 29, 2014
Categories: Uncategorized

There's a story in Variety this afternoon announcing that Seth MacFarlane will be the host this year for the ceremony in Silicon Valley announcing the 2015 Breakthrough Prizes. MacFarlane was the host of the 2013 Oscars. Other celebrities there to award prizes will include Kate Beckinsale, Benedict Cumberbatch, Cameron Diaz, Jon Hamm and Eddie Redmayne. The ceremony will be televised, not live, but November 15 at 6pm on the Discovery Channel.

The announcements that evening will include awards of up to 6 $3 million prizes in the life sciences. The physics prizes this year, funded by Yuri Milner, will include a $3 million prize and one or more $100,000 prizes for young researchers. The past practice of awarding $300,000 to semi-finalists for the $3 million seems to have been stopped, after Joe Polchinski collected a couple of these. Polchinski seems to be the odds-on favorite for the $3 million this year. Another possibility would be Strominger and Vafa, also semi-finalists last year. I suppose there's an outside chance that the committee making the choice, which is dominated by string theorists, will decide that a non-string theorist is worth recognizing.

The Mathematics prize is funded by Milner and Mark Zuckerberg. The winners there are already known, see here.

The next day there is an announced symposium scheduled to be held at Stanford to honor the prize winners in the life sciences (see here). Last year there was a similar symposium in physics right after the ceremony, so one could guess that such things might be planned for physics and math as well.

Update: There will be separate math and physics symposia on Monday, and an evening lecture from the Physics winner. A little more detail here. The math symposium will be at Stanford, live-streamed to Berkeley, Stanford details and RSVP here.

Comments

1. **Per**
   October 29, 2014

   These prices are such nonsense. Why give it to some dinosaur who's been faculty for well over a decade and already won a bunch of prices? Better to focus on the young and upcoming and help them focus solely on research without any economic concerns.

2. **Michael Hutchings**
   October 29, 2014

   There will be a math symposium at Stanford on Nov 10. However I can't find a website for it.

3. **abbyyorker**
   October 29, 2014

   Quite a break from the past. The 1938 Breakthrough prizes were hosted by Seth Niedermayer.

4. **Low Math, Meekly Interacting**
   October 29, 2014

   I'll never complain (much) about scientists getting more money, but I can't for the life of me understand how larding up the event with a bunch of celebrity eye candy is supposed to draw the kind of public interest anyone would want.

5. **martenvandijk**
   October 30, 2014

   Breakthrough Prize 2015? Breakthrough Credit 2015 if you ask me.

6. **Yaakov Baruch**
   October 30, 2014

   Sorry – I don't want to hijack the thread with this totally unrelated link:
   http://www.realclearscience.com/blog/2014/10/lhc_outdone_by_tabletop_electron_experiment_108918.html
   I'm only posting it in the hope that Peter, and others, may comment on this subject at some future time within an appropriate post.

7. **Peter Woit**
   October 30, 2014

   Yaakov,
   That's not really news, so I don't want to start a discussion of it here. Note that the graphic in that story shows that "naive SUSY" was already ruled out by this kind of experiment back in 2002, long before the LHC. The fact that these kind of precision experiments rule out popular SUSY models has always been ignored by SUSY enthusiasts, and I suspect will continue to be ignored by them, no matter how good the bounds get.

8. **Als**
   October 30, 2014

   Tao is going to appear on Colbert according to this article in the Atlantic : http://www.theatlantic.com/education/archive/2014/10/when-your-teacher-is-a-celebrity/382029/

9. **CPV**
   October 30, 2014

   What would have a higher effect – 3 mm to one awardee, or 100k to 30? I think very, very few people who make large contributions to math and science go in it for the money. These prizes aren't going to improve the level theoretical science occurs at. Progress hasn't stalled because theorists aren't working hard enough or there aren't enough of them or they can't make 3mm a year. It might be nice to boost salaries to 100k /yr for postdocs but I bet that wouldn't make much difference either. Progress has stalled because of scale ratios like 10^30.

10. **Jim**
    October 31, 2014

    Seems an odd choice of celebrities. I could understand a Natalie Portman (with a finite Erdos number) or even a James Franco as a presenter but I can’t understand what Cameron Diaz has to do with science.

11. **Simple biologist**
    October 31, 2014

    @Jim

    Cameron Diaz was at least one of the quests on that evangelical science+atheism film about Lawrence Krauss & Richard Dawkins. Quick googling also reveals that she describes herself as ‘science nerd’.

    Propably easy picking (as far celebrities are concerned) for the organisers.

12. **Jonathan Miller**
    October 31, 2014

    I work about 20 hours less per week than I would prefer, in part due to low pay. I expect that there are others like me, more pay being available for young physicists might improve physics productivity.

News, Interesting and Otherwise

November 3, 2014
Categories: Experimental HEP News, Multiverse Mania, Uncategorized

On the side of interesting good news, I just heard that yet another Fields Medalist has a blog. This time it's David Mumford, who is blogging here, with his latest posting about path integrals.

His website contains a wealth of other very worthwhile material, including copies of pretty much all of his papers, some of which had been quite hard to find. Much of Mumford's career has been in the field of algebraic geometry, where he is a towering figure for mathematicians working during the past few decades. This is not just due to his ideas, but also to his expository talents, which have made many of his monographs and papers the standard place young mathematicians have gone to learn parts of the subject.

Michael Schmitt's Collider Blog is not new, but it's great to see that after a period of relative quiet he's been very active there. His postings from the last couple months give some great detailed explanations of recent news from HEP experimental analyses.

The news from Mochizuki is that there will be a workshop in March on his work, with proceedings to be published. Go Yamashita will be giving two weeks of lectures there. One can hope that this is good news, in that it promises the possibility of an exposition of Mochizuki's claimed proof of the abc conjecture that will allow other mathematicians to finally understand it well enough to evaluate it.

On the much less interesting news front, multiverse mania continues. Much of this mania seems to have to do with people's fascination with the idea of different copies of themselves doing somewhat different things an infinite number of times elsewhere. I fear that in my case the multiverse is just causing me to do the same thing an infinite number of times in this universe, which is really tedious. In any case, latest developments are:

- Nathalie Wolchover and Peter Byrne have a new piece at Quanta: In a Multiverse, What Are the Odds?, which leads with the news that "the multiverse camp is growing", while headlining the obvious "measure" problem that you can't calculate anything with the idea. Paul Steinhardt is quoted as saying

  > The multiverse idea is baroque, unnatural, untestable and, in the end, dangerous to science and society.

  which is about right, but the rest of the article is mostly dubious claims from the multiverse promotion crowd. More to come next week, looks like the usual bubble collision business.

- Ars Technica has a report on last week's debate in Brooklyn about the multiverse. Tegmark was on the pro-multiverse side, Wilczek on the anti-side, Janna Levin in the middle. According to the reporter

  > Overall, Wilczek seemed to get the better of this part of the debate.

  Much of the debate seemed to be about the "Many Worlds" interpretation, with Wilczek describing this as an empty idea: "metaphysical baggage added on", and Tegmark rather enthusiastic about this kind of thing. On the cosmological multiverse, I gather Wilczek's attitude wasn't so much negative as that it was too speculative to be interesting.

- What started this multiverse obsession among prominent theorists was the work by KKLT and others supposedly showing that you could get the right cosmological constant in string theory, but that when you did so you ended up with an exponentially large number of possibilities and a likely loss of any predictivity. At the time I thought that was the end of that line of thought in string theory, but instead it turned out to be the beginning of the bizarre period of multiverse mania we now live in.
  The latest news is that KKLT doesn't actually work, that you can't get stable string vacua that way. I don't think though that this will have any effect on multiverse mania and its use as an excuse for the failure of string theory unification. It seems to me that we're now ten years down the road from the point when discussion revolved around actual models and people thought maybe they could calculate something. As far as this stuff goes, we're now not only at John Horgan's "End of Science", but gone past it already and deep into something different.

Comments

1. **Amused**
   November 3, 2014

   "The latest news is that KKLT doesn?t actually work, that you can?t get stable string vacua that way."

   Are the results of that paper generic, or do they only apply to a specific set of models? Just reading the abstract makes it seem like it's the latter, though I'm definitely no expert on this stuff.

2. **Amused**
   November 4, 2014

   Peter,
   "I don't know" is an acceptable answer.

3. **Peter Woit**
   November 4, 2014

   Amused,
   Sure, I'm no expert and don't know. But I mentioned this because I heard from someone much better informed than me about it, and they believed it was significant (i.e. it's not that there are other known ways of doing this). I'm hoping someone better informed will comment here about this. One reason I haven't looked into this more is that it seems to me that virtually no string theorists any longer care about the technical issue involved. Maybe some commenters will show up to prove me wrong…

4. **Peter Woit**
   November 4, 2014

   Amused,
   If you want something amusing about this, see Lubos's blog, where he picked this up from me. An expert on the problems with KKLT has written in to complain about "the lack of scientific attitude of some members of the string pheno community". It seems that string theorists trying to examine the problems of KKLT get something like the response to anyone pointing out problems with string theory. In this subject now, as I suspected, it's all about the politics of defending an ideology, no longer about

science.

5. **lazyman**
   November 4, 2014

   "The latest news is that KKLT doesn't actually work, that you can't get stable string vacua that way."
   Well, not in this universe, but there are infinitely many other universes where it does work! 😉

6. **Curious Mayhem**
   November 4, 2014

   The multiverse idea is baroque, unnatural, untestable and, in the end, dangerous to science and society.

   It may also rot your teeth. Being unnatural, it's undoubtedly outlawed in some states.

7. **bombom**
   November 5, 2014

   A side benefit of "multiverse mania" is that the ironic phrase "In what universe?" seems to have entered the lexicon. (Thanks, perhaps, to Sheldon??)

   Peter, I think the real reason for the popularity of the multiverse is for people to imagine that in at least one of them, string theory is right.

   (And that in at least one other, "they" received the Milner prize…)

This Week’s Hype

November 3, 2014
Categories: Quantum Mechanics, This Week's Hype

From commenter Hendrik, there’s the news that USC has put out a press release claiming that String Theory Could Be the Foundation of Quantum Mechanics. These claims are based on this paper, which argues that finding the Heisenberg commutation relations in a string field theory calculation means string field theory can be the foundation of quantum mechanics.

In my quantum mechanics course this semester, I’m now up to around chapter 13 or so of the notes available here. Last class I was pointing out that one already sees the Heisenberg commutation relations in classical Hamiltonian mechanics. The functions on phase space are a Lie algebra, satisfying commutation relations given by the Poisson bracket. These relations are determined by knowing what happens on linear functions, together with the Leibniz rule. On linear functions, the commutation relations are Heisenberg’s.

So, I think the discovery out of USC is an even greater one: string field theory can explain not only quantum mechanics, but classical mechanics too.

Update: I should have realized that this thing already had one wave of hype earlier this year, courtesy of Tom Siegfried, which I wrote about here. This new wave comes courtesy of Physics Letters, which thought this worth publishing, and USC, which thought a press release was a good idea.

Comments

1. **Bob Jones**
   November 3, 2014

   You know, string theory actually is quite important for understanding the (mathematical) foundations of quantum mechanics:

   http://arxiv.org/abs/0809.0305

   http://arxiv.org/abs/q-alg/9709040

   It’s a shame that serious ideas like this hardly ever get discussed on this blog.

2. **Peter Woit**
   November 3, 2014

   Bob Jones,
   There has been some blogging here about the mathematical foundations of quantum mechanics. I’ve been teaching and writing about the topic, expect more blogging, this spring a finished draft of a book, and more to come after that. Sorry, but claiming that those two papers you reference show that string theory is important for understanding the foundations of QM is utter hype. I don’t believe the authors of those papers would agree with that claim.

3. **Bob Jones**
   November 4, 2014

   Peter,

   I’m glad to hear that more serious content is on the way.

   Regarding the papers I mentioned, the formality theorem proved in the second one is really one of the most fundamental results in the mathematical theory of quantization. I don’t think I’m exaggerating when I say that string theory is important here; in the second paragraph, Kontsevich himself writes, “The solution presented here uses, in an essential way, ideas of string theory”.

4. **Peter Woit**
   November 4, 2014

   Bob Jones,
   Actually, it seems to me that all Kontsevich is really saying is that in order to prove a certain technical conjecture, he used calculational techniques developed in studying topological string theory (which has nothing to do with physical string theory). He then writes the proof without using any string theory. Quite interesting work and great mathematics, but, sorry, “string theory actually is quite important for understanding the (mathematical) foundations of quantum mechanics” is just hype.

5. **Bob Jones**
   November 4, 2014

   Ah, so mathematical work on two-dimensional sigma models and topological string theory doesn’t count as string theory. Got it.

Last Week’s Hype

November 3, 2014
Categories: This Week's Hype

When looking at the nonsense spread around the media by this week’s university press-release-driven hype about string field theory “explaining quantum mechanics”, I realized that maybe I shouldn’t have ignored last week’s university press-release-driven hype, which was about the multiverse “explaining quantum mechanics”. For that one the press release is [New quantum theory is out of this parallel world] and the paper is [here]. It has generated all sorts of press stories, with a typical example [Parallel Universes Exist – And Could Explain All Physics, Says Griffith University Study].

Sharing the credit or blame for this with the Griffith University press office is the APS and its Physical Review X, which published the paper [here]. The APS Editor in Chief explains [here] that

> In recent years, however, we have seen a strong need of some researchers to have their best scientific contributions published in highly selective and small journals that can disseminate those contributions broadly and offer them high visibility.

The idea seems to be that if you want “high visibility”, and you’ve got $1700 to pay for it, Physical Review X is there to get you into the media. They seem to have realized though that maybe the “parallel worlds explain quantum mechanics” might be seen as going too far, so have put out an [editorial justifying its publication].

Comments

1. **Matt Leifer**
   November 3, 2014

   Your criticism of PRX seems a little bit harsh to me. The APS have decided to adopt PRL-like standards for PRX. This has resulted in PRX getting a higher impact factor than PRL in the last couple of years. The 2014 numbers are:

   PRX: 8.385
   PRL: 7.728

   To me, it can only be a good thing that the top ranked journal in physics is no longer one with a ridiculous page limit. Hopefully, the most important papers in physics will now be easier to read and better explained.

   Of course, the APS does media promotion for articles published in all of its journals, and it should be no surprise that they put more effort into this for their top titles like PRX and PRL. Science journalists are also aware that these are the top journals, so they pay more attention to stuff that appears in them. It should also be no surprise that they are liable to pick up on anything that mentions parallel words or the multiverse. You can’t blame the APS for the fact that this paper got more attention than other things that appeared in the same journal.

   PRX is not a journal that the APS have set up just to be a fast track into the media for a fee. It is simply a that adopts PRL-like standards, and hence gets more media attention, that also happens to be gold open-access. We can argue about the merits of gold, author-pays, open access, and we can argue about whether this particular paper ought to have been published in PRX. We might even find ourselves agreeing on these issues to some extent, but to lambast PRX over this one paper seems a bit of a stretch. The impace factors speak for themselves. Physicists are paying attention to what appears in this journal, and I don’t think they are being swayed by what gets media attention.

2. **Peter Woit**
   November 3, 2014

   Matt,
   Thanks. Perhaps you’re right that I’m being somewhat unfair. I do think though this shows the danger of where you can end up if you emphasize “high visibility” and have an “institution pays” model. If I were an author who publishes in PRX I’d be kind of unhappy to see this stunt, and I wonder if the editor is hearing about it.

3. **Neil**
   November 4, 2014

   I love that they are invoking Feynman. I can well imagine what he would think of this.

4. **Maurice Carid**
   November 4, 2014

   Hi Peter,
   implicitly you call their paper “nonsense”,
   can u tell us why u think so?
   You yourself expressed unease with the
   foundations of QM, so what’s wrong when these guys
   make a proposal to clarify them on the level
   of new physics rather than philosophy?
   I found the paper’s arguments quite sound
   on a technical level, and
   their theory clearly can be tested
   against experiment (for a finite
   number of worlds). Also, what exactly
   is wrong on a technical level with the press
   releases? To me it seems that they give
   a fair popular account of the paper and make
   no other exaggerated claims.
   cheers Maurice

5. **Bernd**
   November 4, 2014

I’ve heard that PRX is burning money at a crazy rate because they’re not publishing enough papers to meet their costs, so expanding their mission to fringier stuff might be a step to generate more income.

And if the editorial tells us anything, it’s that they’re not following PRL standards as PRL would not publish a manuscript that merely contained “new ways of looking at things”.

6. **Als**
November 4, 2014

I think you’re being unfair. The press misleading titles are annoying, but the article itself is fine.

7. **Peter Woit**
November 4, 2014

Maurice/Als,
I’ve already wasted more time on this than is wise.

8. **Shantanu**
November 4, 2014

I am afraid standards of PRL have also gone down. I don’t understand
why PRL still publishes upper limit papers (for experimental papers).

9. **Narad**
November 4, 2014

> I’ve heard that PRX is burning money at a crazy rate because they’re not publishing enough papers to meet their costs, so expanding their mission to fringier stuff might be a step to generate more income.

It is unclear to me how well such a strategy would advance the stated goal of “diversify[ing] the Journals’ revenue stream” in the long run.

10. **upper limit**
November 4, 2014

Why shouldn’t PRL publish papers establishing upper limits, for example for the neutron electric dipole moment?

11. **Peter Woit**
November 5, 2014

Narad,
That document makes clear the issue here. It points out
“APS rejects ~18,000 articles each year. At $1500 each, this is a tempting potential revenue stream of $27,000,000”
It goes to on to emphasize commitment to high standards. The problem is though that from the APS point of view accepting rejected papers and collecting $1500 for them IS tempting, and the collision of hard cash and intellectual standards isn’t always going to leave intellectual standards coming out on top.

12. **Narad**
November 6, 2014

> The problem is though that from the APS point of view accepting rejected papers and collecting $1500 for them IS tempting, and the collision of hard cash and intellectual standards isn’t always going to leave intellectual standards coming out on top.

Sure, but I don’t think that turning into, say, *Medical Hypotheses* would work in the long run, especially with a shiny 8.463 IF at stake. It doesn’t even seem to be a case of the “portable peer review” that Springer was using to auto-shunt rejections into its OA operation, even if that was an idea for *PRX* in the early stage (as well as perhaps competing with *NJP*, an idea I’ve seen floated).

Then again, there’s no accepted date on Hall et al. (and that $1700 isn’t buying competent typesetting, given Eq. [25] in the PDF, but perhaps *PRL* is just as bad). There *is* the October 9 “PRX Takes on a New Role” announcement, though. Time will tell.

13. **srp**
November 6, 2014

The strategy problem for an author-pays journal with high initial prestige is analogous to that facing luxury product brands such as Hermes or Fendi. Such a brand is usually prestigious partly because it is somewhat exclusive. But a successful brand of this type always faces the temptation to make a short-term killing by surging production and reaping large margins on those sales until the market realizes that exclusivity has disappeared. After making this killing, though, the brand’s prestige drops very low and it may have to lie fallow for a long time before it can be rebuilt. That’s pretty much the story of Cartier. (Actually this can happen in the mid-market, too. In the late 1970s and early 1980s Izod became a cultural phenomenon with the publication of The Preppy Handbook, and they responded by putting that crocodile logo on a flood of product that cascaded into down-market channels.)

Could APS play this game?

Hollywood Theoretical Physics Week

November 5, 2014
Categories: Film Reviews

Just got back from an opening night showing of the new sci-fi film Interstellar at the Ziegfeld theater here in New York. If you want some idea of what the film is about, trailers are here and here. Warning: spoilers in next paragraph, skip that if you care.

To me the big plot surprise was that the human race is saved by the theoretical physicist. An elderly theoretical physicist has been trying to solve some equation for gravity his entire career. If he solves it this will somehow save the human race (which has just about ruined its planet). Turns out, he wasn't being honest, he knew how to solve the equation, but to save the planet, you need to reconcile quantum theory and gravity. Only way to do this is to go into a black hole and get the "quantum data". Dad (Matthew McConaughey) does this, then manages to transmit the "quantum data" via Morse code to his daughter, a theoretical physicist who has taken over from the old guy, who has died. She uses the "quantum data" to write something on the blackboard that flashes by (maybe a 10d gravitational action), this somehow saves the human race. Before we get to this point, lots of plot involving going through a wormhole (looks kind of like what going through a wormhole always looks like), and various time spent on exotic planets orbiting a black hole.

The black hole portrayal is one that great effort went into on the accuracy front, with Kip Thorne involved. He and the film's director have a book coming out Friday, The Science of Interstellar, and there's a TV documentary about the science behind the movie. Evidently this film has been in the works for a while: John Preskill tells the story here of a 2006 meeting with Steven Spielberg to discuss the film, also attended by Andrei Linde, Lisa Randall, Savas Dimopoulos, Mark Wise and Thorne.

Anyway, I enjoyed the film, even though I'm not usually a big fan of sci-fi films.

Opening in a few days is another major Hollywood effort centered around theoretical physics, quantum gravity and black holes: the Hawking biopic, The Theory of Everything. A trailer is here. I'll probably see that this weekend and will report back.

For yet a third Hollywood-type event opening this week and featuring a theoretical physicist, quantum gravity and black holes, see this trailer. You can watch this Thursday night, more info here.

Finally, it's not really Hollywood unless you have an awards ceremony featuring Hollywood stars. This Sunday night, some lucky string theorist will get a $3 million check for his work on quantum gravity and black holes. Seth MacFarlane is hosting, more details here. From what I recall, the argument for setting up this huge prize for theoretical physicists was that they don't get enough public attention…

Update: I hadn't noticed that there's another film opening this week featuring theoretical physicists, The Principle, with trailer here. I wrote a bit about this early this year here.

Update: Phil Plait really doesn't like the film, finding much of the plot scientifically implausible. Of course he's right about that, but it makes one wonder if he has seen many sci-fi films. From my limited experience, this one is about average on the implausibility meter.

Comments

1. **Tim Howells**
   November 5, 2014

   Re Realism and black holes: I was struck by a question made to Leonard Susskind after his talk on the great "Black Hole War" he had with Hawking.

   http://www.youtube.com/watch?v=KR3Msi1YeXQ

   Susskind had elaborated at length on information theoretic properties of black holes – how all the information within a BH is projected as a holographic representation onto its spherical outer surface; How a paradoxical interpretation allows that somehow information both can and cannot escape from a BH (or something like that). etc etc etc.

   Then someone asked "How confident are you that black holes exist?" Great Question! Susskind hemmed and hawed and finally said, "Well most great physicists believe they exist, and very few do not believe, so It would be really surprising to find out that they did not exist …".

   My interested layman's take-away was that this is just more madness.

2. **dr. kansas**
   November 5, 2014

   perhaps the most remarkable thing of the film was not one mention of string theory not the multiverse nor inflation.

   i guess they wanted to make it as plausible as possible, and thus the filmmakers were operating under greater constraints than academics.

3. **Grad Student**
   November 5, 2014

   yes indeed. imagine if one of the characters would have invoked the multiverse or the stringy landscape or parallel worlds or the many-worlds theory.

   Matthew McConaughhey: The world is going to end.

   Anne Hathaway: Do not worry, as it is just our world. There are infinitely more worlds that are not going to end, in which the crops are doing just fine. For you see, everybody splits into multiple independent worlds with every decision.

   Matthew McConaughhey: Oh OK.

   THE END

   Thus while, the movie would have ended there, in our world the multiverse movie has no end in sight.

4. **Yatima**
   November 5, 2014

It’s 2014, but then!

“How confident are you that black holes exist?”

This is on the same level as “How confident are you that the world is not 3000 years old” or “How confident are you that P is not NP?” though.

Pretty confident.

Classical GR never lets you down.

If there was any hemming and hawing, it was because of the scientist’s customary bashfulness. An individual with Neil Armstrong tendencies would just have facepunched the questioner, then gone on to the next question.

5. **CFT**
November 5, 2014

Once upon a time fissile nuclear radiation was the rage. It caused ants and angry women to grow fifty feet tall, and nonplussed fifty story tall lizards to eat Tokyo, and gave nerds glowing eyes and superpowers in comic books and b-rated movies.
*
Now we have black holes and worm holes. They allow cosmic space babies to pop out of creepy dark monoliths, empower red satanic floating cyclopean robots to go crazy, and provide nerds with FTL interstellar travel in comic books and b-rated movies.
*
While the special effects and CGI eye candy for the plot devices have improved dramatically,
The scientific aspects of the stories are as tenuous and mediocre as ever.

6. **Oldster**
November 5, 2014

Yatima: I realize it’s a trivial point, but wasn’t it Buzz Aldrin who facepunched somebody, not Neil Armstrong?

7. **Grad Student**
November 5, 2014

Serious question here.

Has a black hole every been directly observed? I am fairy certain a wormhole has never been directly observed.

I just googled “black hole observation” and found numerous press releases and PR claiming their existence and direct observation thereof.

I also read the recent news that a UNC Professor has proven that black holes cannot exist, which also got a lot of press.

Well, if they have been observed for sure, as thousands of sites state, then how can thousands of other sites state that a UNC Professor has proven that they do not exist?

Does anyone have any more insight on this?

Have black holes been observed?

Are there photos from the hubble?

8. **Peter Woit**
November 5, 2014

Grad Student,

It seems to be uncontroversial that there’s a black hole at the center of the galaxy. You can observe orbits of objects nearby, and there’s something there exerting a gravitational force of million of solar masses, while at the same time not itself radiating, so if that’s not a black hole, it’s something very much like one. To the extent there are controversies over black holes, I think it would be more accurate to describe the controversy as about exactly what their properties are.

As for believing something because internet sites quote a professor saying it, surely you know better…

9. **Igor Khavkine**
November 5, 2014

Clearly, the new biopic about Hawking spans a larger time period than this older one from BBC, as well as having a much larger budget. I wonder if it will do anything much differently. The BBC one actually contained some math and physics, in a decently correct way. On the other hand, I doubt that either one nails all the personalities involved.

10. **Gus Bici**
November 5, 2014

I’m looking forward to seeing Larry & Max & Michio in this movie:
http://pressreleases.religionnews.com/2014/10/30/highly-anticipated-documentary-principle-opens-chicago-multiple-sold-screenings/

Cool trailer, Larry with eerie green backlighting.

11. **Peter Woit**
November 5, 2014

Gus,
Thanks for the news, I’ll add that one to the list for this exciting week. Hope it will make it to NYC…

12. **vmarko**
November 5, 2014

Peter,

I'm curious about some more spoiler details, if you care to share. Namely, one of the talking points about the movie was that it aimed at scientific accuracy more than usual, in particular wrt. to wormholes etc.

So my question is — which of these do you think has been depicted in the movie:

(a) the Schwarzschild BH solution — which does not feature a wormhole, or
(b) the Einstein-Rosen solution — which does not feature a traversable wormhole since it is inside the BH horizon, or
(c) the Morris-Thorne solution with exotic matter — which does feature a traversable wormhole but doesn't have any properties that even remotely resemble a black hole (nothing black, no horizon, etc.)?

I mean, one cannot just draw pretty pictures of an ordinary black hole, simultaneously claim that there's a traversable wormhole inside it, and then go on to say that all this can be scientifically accurate. There was an interesting discussion regarding this over at Sean Carroll's blog, so I'm just wondering now what actually happened in the movie? 🙂

13. **Peter Woit**
November 5, 2014

(Spoiler alert, you may not want to read…)
vmarko,
In the plot, the wormhole and black hole are two different things. The wormhole is small, near Saturn. The black hole is big, it's what the planetary systems in another galaxy rotate around. The wormhole is pretty much a black spot, and when they enter it, it's exactly the same graphics as always: you're rushing down a tube-like region, random stuff going by fast on the sides, then you come out to regular space.

It's the black hole the planets rotate around that is what is supposedly more accurate than usual. Basically though, it's the picture you see here
http://www.wired.com/2014/10/astrophysics-interstellar-black-hole/
I gather this is the image that is supposed to be scientifically accurate. When our hero goes into the black hole to get the "quantum data", then it's back to the usual visual silliness, flying through various collections of luminous stuff in the dark.

14. **vmarko**
November 5, 2014

Peter,

Thanks for the info, now the whole thing makes more sense. Btw, the rendering of the black hole's accretion disc is actually not fully accurate — they have dropped the (very prominent) Doppler effects [1], because they make the resulting image too confusing for the audience. I guess that the actual realistic black hole is not what can be seen in the movie, after all. 🙂

[1] See Jean-Pierre Luminet's comments (and a fully realistic rendering of a black hole) here: https://www.facebook.com/sabine.hossenfelder/posts/10152939381329574

15. **Chris Oakley**
November 5, 2014

Re: the original topic (Interstellar), Christopher Nolan always gets my vote. I will look forward to seeing it. And if he forbade mentions of String Theory and multiple universes, it was probably on the grounds of lack of scientific evidence. What kind of crazy world are we living in when a film director takes more trouble about such things than many leading scientists?

16. **Gus Bici**
November 5, 2014

Peter, in your review of "The Principle" trailer you wrote:

"As near as I can tell from all this, without having yet seen the full film, it appears that what probably happened is the following. Sungenis decided that the anthropic principle business in cosmology supported his views, so he went and got physicists like Kaku, Krauss and Tegmark to say silly things on camera, then edited this to suit his case. Maybe the trailer is misleading, and these people actually make a cogent case against Sungenis's nonsense and for solid science, we'll see…"

Is the Earth's supposed alignment with the CMB radiation really anthropic? I mean it does not HAVE to sit on this "axis of evil". If this alignment does exist, there will have to be a non-anthropic explanation. I think "coincidence" is the current explanation?

17. **Peter Woit**
November 5, 2014

Gus,
My interest in discussing the details of the scientific arguments in "The Principle" is about the same as in discussing the details of what the stuff on the side of the tube of the wormhole in Interstellar was. Before saying more, I'll wait till I actually see "The Principle". From the trailer it looks like a hoot.

18. **martibal**
November 5, 2014

Honor to the precursor !
Black Hole in Hollywood is an old story. Anybody remembers the Disney movie "The Black Hole" in 79 ? Was quite impressive at the time
http://en.wikipedia.org/wiki/The_Black_Hole

19. **Gus Bici**
November 5, 2014

Point taken, Peter. I'll go back now to happy lurker on your fine blog:)

20. **Jeff M**
November 5, 2014

RE the science in "Interstellar" you might want to check out http://www.theguardian.com/film/filmblog/2014/nov/05/interstellar-astrophysics-does-space-science-work-out

21. **CFT**
November 5, 2014

martibal,
My comment above mentioning how black holes now "empower red satanic floating cyclopean robots to go crazy, " is homage to Maximillian, the big red floating evil robot with meat grinder arms and one uni-brow eye in Disney's movie "The Black Hole". It goes crazy, kills a bunch of people, and then literally goes to hell, flames, pitchforks, screaming souls and all, no joke. No one believes me how over the top strange this robot character actually is until they see the movie. The creative director of the movie was definitely on something psychotropic at the time.

22. **MB**
November 6, 2014

The link to your earlier article on The Princple is broken. There's just got an extra "http://www.math.colum" at the beginning.

23. **Peter Woit**
November 6, 2014

MB,
Thanks, fixed.

24. **Narad**
November 6, 2014

Martibal, the NBC "Special Treat" (similar to the better remembered ABC "Afterschool Special") did *The Day after Tomorrow* back in 1975. The traversing-the-black-hole effects, as I recall, were of the crew moving in slow motion in skewed and stretched video frames.

25. **Wavefunction**
November 6, 2014

There's also "The Imitation Game", the movie about Turing based on Andrew Hodges's biography that's coming out later this month. This year's turning out to be a good year for science-based movies.

26. **martibal**
November 6, 2014

Narad: cannot remember to have heard about it, maybe it did not cross the Channel. Looks funny.

CFT: I should have recognized the terrible description of Maximilian, indeed !

27. **martibal**
November 7, 2014

A nerd post (sorry):

Which one would be next ? String theory awakes, Susy awakes, gauge theory awakes ? I would prefer noncommutative geometry awakes, but not sure Disney would buy it 🙂

28. **Narad**
November 7, 2014

It turns out that there *is* a multiverse movie.

29. **Patrick Harris**
November 8, 2014

Just saw Interstellar and really enjoyed it (I'm a big sci-fi fan). Didn't have too many eye-rolling lines as these type of films can have and the quality portrayal of the other planets and space scenes was appreciated.

Glad to hear The Principle is coming out, hopefully my eyes won't roll too much that they fall out ;). Looking forward to your comments!

30. **cl7281**
November 10, 2014

Just seen (and enjoyed) it, and while I agree no multiverse musings I'm not so sure string theory didn't creep in by the back door. There was much talk of extra dimensions and gravity leaking across them. Admittedly this was 5d not 10d so maybe Chris Nolan is a big Kaluza-Klein fan…

Quantum Mechanics and Spacetime in the 21st Century

November 6, 2014
Categories: Uncategorized

This evening's Hollywood-style entertainment came from the Perimeter Institute, where they had a big public event, live-streamed to the world, featuring Nima Arkani-Hamed speaking on Quantum Mechanics and Spacetime in the 21st Century. You should be able to watch the thing soon from the Perimeter site, should be posted at some point here.

The talk was pretty much the same as many other such Arkani-Hamed talks, quite close to the one at the IAS nearly four years ago, discussed here. In this format he can't go on forever, was cut off by around an hour and a half, so said he couldn't get to the third part of the talk, which might have been the 21st century part (amplituhedron?). As in the IAS talk, what he did cover was first mostly the 1960s sort of arguments that Weinberg describes in the first volume of his textbook about the constraints on consistent relativistic QFTs. Then an advertisement for the Veneziano model and string theory. The last part of the talk was a long advertisement for SUSY, ending with an acknowledgement that it wasn't showing up at the LHC. He's now giving 2018 as the date for when we'll know about LHC-scale SUSY, which is moved up from the 2020 of the IAS talk of 2011, but still very different than the "year or so after LHC startup" he was saying in 2005. The current plan is for maybe 10 inverse-fb next year, 50 in 2016. If nothing shows up then, I don't see that the next 50-100 supposedly coming by 2018 have any real chance of finding SUSY.

One thing that struck me about the talk was its odd combination of over-the-top enthusiasm ("this is the greatest time ever!") and intense defensiveness. He kept emphasizing the claim that theorists, even without experiment to keep them honest, were working with highly constrained rules, that it was very hard to do anything not obviously wrong. He denied sociology had to do with what unsuccessful ideas people decide to pursue. He didn't address at all the "not even wrong" problem: what about the things like the landscape, baroque constructions that evade being wrong by being empty? I like a lot this picture and quote from Perimeter:

> If you manage to find one idea that's not obviously wrong, it's a big accomplishment. Now, that's not to say it's right. But not obviously being wrong is already a huge accomplishment in this field.

I think the defensiveness here that's coming through is very personal. He's gotten a $3 million award and a reputation as a leader of the field for ideas that haven't worked out, but which he can defend as "not obviously wrong" and thus a "huge accomplishment".

All in all, the talk was very backward looking, recapitulating the SUSY/string theory ideology that has led us to where we are. It looks like he's planning on hanging in until 2018 with the same story, only then maybe admitting failure (and possibly going for "the multiverse did it, we never had a chance" cop-out). He did end with an upbeat claim that the SUSY picture being all wrong would be very exciting, opening up the field by showing we need something completely new. The obvious question for him is "you pretty clearly wouldn't now bet $10 on SUSY at the LHC, so why wait?" Why not stop giving promotional talks about SUSY? One thing he could have done that would have generated some excitement in the field would be to have pitched out the two-thirds of the talk he did give, publicly saying these ideas aren't working, and talked about something from this century, amplitudes or whatever, the part of the talk he never got to.

Well, maybe in 2018…

Update: You can watch the talk here, along with some online commentary in a chat box that was rolling during the talk.

Update: There was also a more technical talk at Perimeter by Arkani-Hamed, earlier in the day, on Cosmological Collider Physics. The video doesn't seem to be available yet though.

Comments

1. **Neville**
   November 7, 2014

   Off topic: For a real hoot, listen to Barry Simon's story starting at 1:05:41 of http://www.youtube.com/watch?v=GIQXE_2a3KI

   The punch line is at 1:07:02.

2. **blue**
   November 7, 2014

   I have a PhD in mathematical physics and I am desperately looking for a postdoc position of around K$40/y with NO hope, and he just won a M$3 prize for "not obviously being wrong"!!! Tyrion Lannister: "If you want justice, you've come to the wrong place".

3. **manfred Requardt**
   November 7, 2014

   Dear Peter, I am always fond of reading your commentaries about such strange kind of physics talk. Such talks always get a lot of attention but there is almost no physical progress being conveyed.

4. **Hamish**
   November 7, 2014

   "Not obviously wrong"…sounds like a good name for a physics blog!

5. **Dimitrelis**
   November 7, 2014

   @Peter:
   By negation of the statement "not obviously wrong" one gets "obviously not wrong", i.e. "obviously right"…
   You have to be obviously right, in order not be considered obviously wrong.
   So what does he mean with "Now, that's not to say it's right"?

   …hmmm, I must be "obviously" wrong.

6. **Shantanu**
   November 7, 2014

Peter at some point the problem also lies with the hosts . I am pretty sure they must have been aware of similar hype he must have mentioned in other talks(many of which can be easily found from the web). Were there hard questions(similar in spirit to what you wrote in this article)? Its somewhat sad that people like blue and others are having
a hard time finding jobs and here …

7. **Peter Woit**
November 7, 2014

The Arkani-Hamed show really is quite a performance. His colleagues and the public do seem to just eat it up uncritically. In this case, since he went on so long, they had no time for questions.

8. **Shantanu**
November 7, 2014

Peter, the technical talk is now available.

9. **Peter Woit**
November 7, 2014

Shantanu,
I'm still getting an error message if I try to play the video. Audio and a pdf of images is available.

10. **Shantanu**
November 7, 2014

Peter you may need to contact the help desk (assuming you haven't tried other web browsers etc). I had a technical problem in viewing
many PI talks recently(not this one) and I contacted them, and since then I don't see a problem. (although I don't know what was the problem and what was the fix done by PI folks)

11. **Grad Student**
November 7, 2014

Listening to Arkani-Hamed speak, I am reminded of the famous poem by W.B. Yeats:

"The best lack all conviction, while the worst
Are full of passionate intensity."
– http://en.wikipedia.org/wiki/The_Second_Coming_(poem)

12. **First Last**
November 7, 2014

Hi Peter, if I understand Arkani-Hamed correctly, the only graviton-graviton scattering amplitude consistent with relativity and qm (unitarity) that has been found is the Veneziano amplitude. Is that correct? That would seem to be a strong indication it is the one to pursue, isn't it?

13. **Peter Woit**
November 7, 2014

First Last,
Sure, that's the main motivation for string theory. But Arkani-Hamed neglects to mention that to get a consistent amplitude, you need to be in flat space-time 10d, which is, well, obviously wrong. To get the 4d Veneziano amplitude he was advertising, you have to get to get rid of 6 dimensions, and that is why the program has failed.

Another point that struck me is that he's convinced space-time is "doomed", and if so, amplitudes at short distances are irrelevant, since there is no space-time at these distances. There's a huge inconsistency between "we must do string theory because it has well-behaved amplitudes at short distances" and "we know there is no space-time at short distances".

14. **Nobody**
November 8, 2014

"Not obviously wrong" approaches "not even wrong".

Progress!

15. **NEOW**
November 8, 2014

But now there's an even lower rung:
"not even obviously wrong"

16. **Peter Woit**
November 8, 2014

All,
If you have something interesting to say about the Arkani-Hamed talk, please do. On the other hand, if you just want to repeat content-free slogans about quantum gravity, please do that elsewhere.

17. **Gus Bici**
November 11, 2014

Listening to the "more technical talk" (MP3) by Arkani-Hamed and it ended abruptly before Nima could finish "what really bothered him". He said it might not bother others, but it really really bothered him. Can anyone summarize Nima's bother?

Short Items

November 7, 2014
Categories: Uncategorized

- On Monday there will be symposia at Stanford featuring the Breakthrough Prize winners, with streaming video available. For the morning program, with dignitaries and such, see here. The Mathematics symposium will run 11-5, the program is here, streaming video here. The Physics symposium is also 11-5, no program yet, but streaming video will be here. If you're in Berkeley they have an event to watch the videos at International House, see here.
- Tomorrow in Paris will be the Seminaire Bourbaki, supposedly you can watch the talks online here.
- Latest news on the Journal of K-theory front (for background, see here) is that the Editorial Board has resigned and is starting up a new journal, to be called Annals of K-theory and published by MSP. The story seems to be that the Journal of K-theory was very profitable, but the profits were going personally to the managing editor, Anthony Bak. Evidently he refused to agree to demands to change this arrangement, so was removed from the K-theory Foundation set up to use funds from the journal, and the other editors (except one) resigned. They are encouraging university libraries to consider canceling subscriptions to the Journal of K-theory, but it's not clear this is possible, since such subscriptions are now often part of bundles.
- If you'd like to see what the theory group at CERN is up to, take a look at presentations at their retreat, which ends today.

Update: The program for Monday's Breakthrough Prize physics symposium is available. It reveals that there will be 3 joint winners of this year's $3 million. I'm betting Polchinski and two others, most likely Strominger and Vafa. The physics speakers get 15 minutes to give a talk, 5 minutes for questions. Mathematicians get twice as long, a total of 40 minutes/talk.

Update: It looks like mathematicians too are getting in on the Hollywood thing. Next week, sandwiched in between Diane von Furstenberg and Jennifer Lawrence, Terry Tao will appear on the Colbert show.

Update: The Terry Tao Colbert show segment is here.

Comments

1. **S. Chan**
   November 8, 2014

   What is with the Bourbaki video? When I browse the web page, a message comes up saying, "This is a private video. Please log in to verify that you may see it."

2. **Ramanujan**
   November 8, 2014

   I'd go with Bekenstein, 't Hooft, Susskind. Though the CM triplet Kane, Molenkamp, Zhang might be a safer bet.

3. **Avattoir**
   November 9, 2014

   N.E.W. going men's mag Risqué with the Lawrence/Tao/von Fürstenberg snack imagery? E. in H.E.P. now = Esquire?

4. **Don Jennings**
   November 9, 2014

   Interesting. Does this indicate that Mr. Milner is coming around to the not uncommon sentiment that doling out $3m per already-quite-wealthy-physicist is perhaps not the smartest way to spend money?

5. **David Roberts**
   November 10, 2014

   Amusingly, the Colbert Report profile neglects to mention the Fields Medal, probably the most impressive prize Terry has received.

6. **Whoops**
   November 13, 2014

   David- it was mentioned at 0:30, check again (and called the "Nobel of Math").

7. **David Roberts**
   November 13, 2014

   @Whoops

   I can't watch the video clip from my Vegemite-munching country, but the Fields was and still is missing from the long list of prizes here:
   http://thecolbertreport.cc.com/guests/terence-tao

The Theory of Everything

November 8, 2014
Categories: Uncategorized

Hollywood theoretical physics week, focusing on quantum gravity and black holes, continues with the opening this weekend of The Theory of Everything, a Stephen Hawking biopic. It's quite good, although a bit too heart-warming for my taste. The focus is on the relationship between Hawking and his wife Jane, and there's quite a bit more emphasis on religion than can really be justified. I wouldn't be at all surprised if Eddie Redmayne gets an Oscar for his portrayal of Hawking. It's very impressively well-done, and the sort of inspirational material the Academy Awards people love.

There are things you could complain about in the film's portrayal of the science (and Dennis Overbye does so here), but this was handled better than I expected, with some reasonable relationship to reality, given the constraints of this kind of movie. In every way, a better film than Interstellar, the other Hollywood theoretical physics movie of the week.

Watching the film did remind me of days long past. When I was a graduate student in Princeton I remember Hawking coming there to give a talk (or talks?), this would have been around 1980. He was talking about Euclidean quantum gravity, and at the time was still able to speak, but his speech was so indistinct that someone who worked with him translated, repeating what he said so everyone could understand. At the time, the general feeling was something like "great physicist, too bad the guy only has a year or two to live" (he did come close to passing away in 1985). I'm absolutely sure that no one then would have believed it possible that he'd go on to become a huge celebrity, make it through two failed marriages, sell 10 million books about physics, and still be with us and active deep into retirement age. Personally I thought a lot of his last book was misguided (see here) but his is an amazing story and he's got a lot better excuse than his able-bodied colleagues for giving up and going for the multiverse.

Comments

1. **Spelling**
   November 9, 2014

   "Stephen" rather than "Steven"?

2. **Katy**
   November 9, 2014

   "….but his is an amazing story and he's got a lot better excuse than his able-bodied colleagues for giving up and going for the multiverse."

   Not a fan multiverse fan, thankful for this blog, disappointed with this concluding sentence. What on earth doe Hawking's disabilities have to do with his belief in the multiverse?

3. **Katy**
   November 9, 2014

   Seriously, if Hawking believes in the multiverse, it's not because he "gave up." Say what you will about the man- his disability has never caused him to lose interest in scientific advancement.

4. **Known As Drew**
   November 9, 2014

   Don't know if you've seen the 2004 BBC biopic "Hawking" starring a young(er) Benedict Cumberbatch. Managed to avoid being sentimental or mawkish. Recommended.

5. **Peter Woit**
   November 9, 2014

   Spelling,
   Thanks. Fixed. I always make that mistake. I blame it on my brother, Steve.

   Katy,
   Sorry, but I do think that the M-theory/multiverse argument is very much what people do who are giving up on really understanding unification (and the movie paints Hawking's main early scientific motivation as understanding unification). "The Grand Design" really is a book that does serious damage to science, by blithely claiming that the M-theory multiverse is a solution to the unification problem. Hawking should know better, just like a lot of other prominent theorists should know better.

6. **Jon**
   November 10, 2014

   I, too, was disappointed to read the last sentence of this post. Sure, you can say he gave up, but your response to Katy does nothing to clear up why you conflate his physical condition with his position on these theoretical issues. Total cheap shot.

7. **Peter Woit**
   November 10, 2014

   Jon,
   I didn't conflate his physical condition with his position on the multiverse. My comment was more a cheap shot at a long list of his colleagues, who don't have the excuse of having to work under extremely challenging conditions to explain why they have taken the easy way out and given up. I don't claim that Hawking's disability is why he has chosen to give up, just point out that at least he has an excuse for doing so, unlike others.

   But, make no mistake about it: Hawking, like others, has chosen to give up. This is a great shame, given his huge influence.

8. **disgruntled**
   November 10, 2014

   Peter – non-scientist, read your blog, find it hugely interesting.

I’ve always thought your arguments carry far more weight and are more likely to influence when they are devoid of sly comments, blithe ‘humour’, cheap shots or the like. This is a case in point.
Your last sentence is very mis-guided, and your responses to Jon and Katy actually make the situation worse – you actually seem to suggest that you used Hawking’s physical condition to make a cheap shot at others……..

9. **Cosmonut**
November 10, 2014

IMO, Hawking’s huge influence is mainly due to his physical condition and his grandiose claims about wanting “complete understanding of the Universe”, “knowing the mind of God” and so on.
His big discovery about black hole radiation happened 40 years ago and while certainly excellent work, it doesn’t really merit comparison to Einstein (whom the media love to compare Hawking to).

But while media hype is one thing, what annoys me is that Hawking does disingenuous PR for himself as in his “Grand Design” book where he claims that the Theory of Everything has already been discovered and hence, he has already achieved his goal of “understanding it all”, which is quite false.

Even worse is his recent autobiography where he presents his highly speculative hypotheses as established scientific facts. For example, he explicitly claims that “the No Boundary Principle is the reason why anything exists at all”.

This is not just hype, its outright dishonesty to boost his own image.

10. **Peter Woit**
November 10, 2014

disgruntled,
Sorry, but this blog is chock-full of sly comments and blithe “humour” that not everyone finds funny. Always has been and always will be. I do try and keep the cheap shots to a minimum, the reference to my making them here was in the blithe humor category.

A certain amount of humor in confronting the massive PR campaign by Hawking, Susskind, Linde, Guth, Polchinski, Arkani-Hamed, Carroll and a raft of others to promote pseudo-science seems to me a healthy way to deal with an otherwise thoroughly depressing and intellectually empty subject. I intend to keep at it.

11. **jd**
November 11, 2014

Amen, Peter Woit. I find that my sense of humor, dark though it is, maintains my sanity. You hang in there.

12. **Jim**
November 12, 2014

I will join the chorus saying the last line is pretty bad and if you are worried PR then you ought to remove it (as well as this comment when you’re done).

The point is that it reads more or less as “Hawking’s in a wheelchair, so we don’t have to expect too much from him. These other guys aren’t, what’s their excuse?!” A bit mean-spirited, wouldn’t you say? And that’s only if we accept your premise that accepting the multiverse is tantamount to giving up. We are therefore left with the conclusion that either accepting the multiverse (as such and so famous physicists do) is NOT giving up or the opponents of the multiverse are arguing from a mean-spirited position. Or both. Whatever the case, it’s not a good look.

13. **Peter Woit**
November 12, 2014

Jim,
I’ve repeatedly clarified exactly what I meant, and I actually don’t think there was anything at all unclear about what I originally wrote. If you or others just ignore that and want to argue about something else, I don’t see why that’s a good reason for me to delete things because of PR concerns.

One additional perhaps clarifying remark: there is no humor here, this is about two tragedies. The first is Hawking’s medical condition, which has forced him to try and survive and work under an awful set of constraints. The second is the tragedy of the descent of a subject with a great history into pseudo-science, led on this path by Hawking and many others. Why each of these scientists have given up on the tough job that initially inspired them is a complex issue that will keep historians of science busy in the future. One aspect of this issue is a moral one, and all I’m pointing out is that Hawking’s situation on that front is different than that of others.

14. **Katy**
November 25, 2014

If you believe that Hawking has given up intellectually, fine, but don’t tie his disabilities into that decision. (You are, ultimately, speculating why he has bought into the multiverse nonsense). Read up on ableism and how language, conflation and projection have real ramifications on disabled people. It’s not a matter of what you meant, it’s what you said.

2015 Breakthrough Prize in Fundamental Physics

November 9, 2014
Categories: Uncategorized

The prize was awarded (by the actor who played Stephen Hawking), in a Hollywood-style awards ceremony (see here) to the 51 members of the two teams responsible for the supernova data showing that the universe is accelerating, with the 2011 Nobel Prize Winners (Perlmutter, Schmidt and Riess) specifically cited as the leaders. I gather the 51 people split the $3 million, so each get around $60K. This is interestingly different than the previous prizes, which mostly went to a small number of string theorists for research that hasn't worked out very well (my prediction of an award to Polchinski, the runner-up for the past two years, was quite wrong). I'm quite curious what caused the change of policy here. The only previous prize for experimental work in physics was a special award for the Higgs discovery, and that went to the experiment spokespersons, not to all the physicists involved (which was controversial at the time).

Anyway, quite interesting and surprising, kind of an about face from theory to experiment, and from rewarding just leaders to recognizing full collaborations.

Update: More here. It seems that the $3 million is not split equally among everyone involved, but that half goes to each of the two teams, and for each team, one third of their winnings goes to their leaders (all to Perlmutter in one case, split equally by Riess and Schmidt in the other).

Video from the ceremony here.

Update: For more details about the ceremony, there's Vanity Fair. I had heard that relatively few actual scientists were getting invited (and no one really wanted to hear from the mathematicians…). It does seem that a big motivation here is to bring Silicon Valley guys and Hollywood/music biz women together for a party:

> Christina Aguilera, who performed during the event, also noticed a difference between tech types and her entertainment-industry colleagues: "Through Yuri, I've been hanging out with the Google guys, Facebook guys. I find them all to be so down to earth. It's really refreshing."
>
> Unlikely duos chatted over a dinner of lasagna and chicken by the French Laundry's Thomas Keller. Aguilera conversed with Twitter C.E.O. Dick Costolo. Elon Musk and Kate Beckinsale were instantly alight in each other's company.

Comments

1. **Navneeth**
   November 10, 2014

   Typo in the title.

2. **Peter Woit**
   November 10, 2014

   Navneeth,
   Thanks, fixed. I hope the topics in 2105 will be different.

3. **Bernhard**
   November 10, 2014

   Since the prize was not split between the collaboration, in what sense has the prize been given to "to the 51 members of the two teams"? It seems to be the same BS of awarding the spokespersons, just as the Nobel.

4. **Peter Woit**
   November 10, 2014

   Bernhard,
   My understanding is that all the members of the two teams are officially recipients and do get a share of the money (each team splits whatever is left of half, after a third goes to their leaders). Only the three leaders though I think get to dress up and meet the Hollywood stars who hand out the prize.

5. **Bernhard**
   November 10, 2014

   Hi Peter,

   Ah OK, I jumped the gun. So, around 40k each. Not bad.

6. **Kuas**
   November 10, 2014

   I wonder how the vote associated with the prize will be split up. Obviously having 51 experimentalist laureates voting on the next winner could cause some problems if they were not de-weighted.

7. **Peter Woit**
   November 10, 2014

   Kuas,
   Already if you look at the Selection Committee for this year, of the 7 LHC experimentalists given a prize last year, only three are now on the selection committee, so there's already some selection effect against experimentalists. In this case, I assume they just allow the three leaders to be on the committee, the rest don't qualify.

   I'm quite curious though what caused the big change in policy this year in the type of award. Seems unlikely it was just the 3/15 experimentalists, but who knows. It seems quite possible that either Milner or the string theorists themselves finally realized that a prize of this kind always going to string theorists was not good for its credibility. Whoever you are, if you're giving out prizes, you want to award prizes to people who will increase your own credibility, not reduce it…

8. **Don Jennings**
   November 10, 2014

Just speculating, but I've been pretty surprised that the $3m recipients seem to have used only a negligible fraction of their new wealth to support anybody else doing physics. Perhaps if Milner too had been expecting to see some secondary philanthropy, he's now decided to just cut out the disappointingly greedy middle-men.

9. **Bernhard**
November 10, 2014

Don Jennings,

Yes, and we should specially quote some spokespersons who said would their money to finance the poor postdocs and students but never did. I guess in the end mortgage is more important than the Higgs.

10. **Bernhard**
November 10, 2014

that would use their money

11. **JD M**
November 10, 2014

On an unrelated piece of news, George Zweig won this year's J. J. Sakurai prize:
<a href="http://www.aps.org/programs/honors/prizes/prizerecipient.cfm?last_nm=Zweig&first_nm=George&year=2015&quot; title="2015 J.J. Sakurai Prize for Theoretical Particle Physics Recipient"

12. **mateo**
November 11, 2014

Eagerly awaiting your take on the latest Higgs hubbub!

13. **Curious**
November 11, 2014

Can you elaborate on "and no one really wanted to hear from the mathematicians…"?

14. **Peter Woit**
November 11, 2014

mateo,
Best ignored, seems to be non-news. Maybe someday there will be exciting news about the Higgs if the LHC measurements of its properties show something unexpected. Unfortunately, that hasn't happened.

15. **Eric Weinstein**
November 11, 2014

Well, I can assure you that even mathematicians in the heart of Silicon Valley are not on the radar. I wouldn't read too much into that. Give it time and see what happens. They are finding their way. This is a particular crowd (so far), but they are trying to do some good. It would be very surprising if they got it nailed down straight out of the gate.

16. **Nicholas Suntzeff**
November 11, 2014

Although the teams were officially recognized in the Breakthrough Award, we were not invited to the ceremony. In the case of the Gruber Prize, the team was generically awarded but our names were not listed individually (except for Saul and Brian). However, we were invited to a simple and elegant ceremony hosted by Martin Rees at Trinity College Cambridge, which was wonderful for the teams. For the Breakthrough Prize, our names are listed but we were not invited. I know that Brian Schmidt made a number of appeals to the organizers that it is wrong not to have the teams there, but they did not agree. It is not as if there was not space – it was in a blimp hanger after all!

Brian and I co-founded the High Z Team in 1994, after the Calan/Tololo Survey had proven that Type Ia supernovae could be calibrated to 6% distances. He and I made two rules: (1) each six months a new subgroup based on university or observatory location of us would get the recent data, and the whole group would work in support of that subgroup, and (2) the person who was the intellectual force behind any particular paper would be first author. With those rules we had no problems of authorship and responsibility, and not surprisingly almost all the papers were first authored by a grad student or postdoc. So the University of Washington (Stubbs, Reiss, and Diercks) had the data the semester before UC Berkeley with Riess and Filippenko got new data.

What these prizes don't seem to understand that most science is done by teams, and at least in astronomy, the teams are often creative anarchies. Brian was elected our leader after one year of both of us co-leading, and deserves to be recognized as our leader. Adam was first author on the discovery paper, and without his dedication and brilliant work, our paper would not have been published in time with the SCP. But, if you took away many of the single members of our team, we would not have made this discovery as quickly as we did, if at all. Thus, the team needs to recognized as a team. Both the Gruber and the Breakthrough Prize went partially in this direction, but not completely.

My hope is that as the Breakthrough Prize evolves, the idea of a team of equals with different talents working together, becomes more understood by those who have no idea of what doing science is.

17. **CPV**
November 11, 2014

Rich people generally believe that, upon reflection, what made them rich is their own unique gift. Team prizes don't fit that narrative. When they give back it's almost always in a reflected glory sense. There are exceptions, but not many. So, the giving will be in ways to find or assist those with unique gifts like their own. I think you could argue that people with immense gifts don't really need anyone's help on average, and that one should look down the ladder for people to help. Gates has done a good job with this, mostly.

18. **emkajot**
November 12, 2014

@CPV
I very much recommend reading Gladwell's "The Outliers" for a completely different perspective on how some people with immense gifts succeed (and many others

don’t). Gates is actually discussed as an example.

19. **vzn**
November 12, 2014

lol “nobody wanted to hear from the mathematicians”…. :'(
you gave a link for the televised stanford event featuring the mathematicians.
wonder if that is archived anywhere? really hope it is. has anyone seen it?
more commentary on breakthrough prizes 2014 (last yr)

20. **Peter Woit**
November 12, 2014

vzn,
While I saw some of the streamed talks, it looks like they are not now available. Presumably they will at some point appear on the breakthroughprize.org site. When that happens I’ll probably take a look at some of what I missed, and link to them from here.

21. **Bernhard**
November 15, 2014

Nicholas Suntzeff,

I very much agree with you. What is troubling about these prizes is that they are supposed to be a channel to make scientists be recognized by the rest of the society. People working in big science are very much aware how a huge collaborative work each paper is, but outside this bubble this is alien culture. What these prizes do when crowning the leaders is to actually diminish the group’s efforts even more, IMO. People will acknowledge what they can see – if they see the leaders these are the one’s who will get ALL the attention from the outside world no matter how many footnotes people write saying the work was actually a collaborative effort,

22. **Davide Castelvecchi**
November 16, 2014

This whole thing reminds me of the party at Peter Gregory’s mansion in the Silicon Valley sitcom 🙂
https://www.youtube.com/watch?v=KLoYVjDTYsM

Alexander Grothendieck 1928-2014

November 13, 2014
Categories: Obituaries

I just heard that Alexander Grothendieck passed away today, at the age of 86, in Saint-Girons. For a French news story, see here.

Grothendieck's story was one of the great romantic stories of modern mathematics, and many would consider him the greatest mathematician of the twentieth century. For some blog entries about him here, see for example this and this. I'll add other links as I see them or think of them.

Update: For some blog entries about Grothendieck's recent life, you could start here.

One of the best places to learn about Grothendieck is from his friend Pierre Cartier, in an article that can be found here, among other places.

Le Monde now has an obituary.

Steve Landsburg has a blog post.

Update: The news about Grothendieck came out in the French press a day ago, but at this point the only things I've seen in the English-language press are an AP wire story, and this at the Independent. Come on science journalists, if any story about mathematics and mathematicians is worth writing about, this one is.

Update: There are now obituaries at the New York Times and the Telegraph. The IHES has a page at their website.

Comments

1. **MathPhys**
   November 13, 2014

   A great mathematician and a great man. Too sad.

2. **Felipe Zaldivar**
   November 13, 2014

   Adieu Shourik!

3. **Ryszard Kostecki**
   November 13, 2014

   This are sad news. He had very unique personality, and was a great master of structure, intuition, poetry, and anarchy. But maybe his death and surrounding publicity can, ironically, stimulate translation of his works to English? In particular of *Recoltes et semailles* (only some small part of it was translated). There is an ongoing crowdfunding project of translation of his biography from German to English: http://www.gofundme.com/7ldiwo. Maybe a translation of Recoltes could be organised in the same way?

4. **SusanA**
   November 14, 2014

   One of the greatest–if not the greatest–mathematicians of the 20th century has died, yet the English media has not picked up on it.

5. **Winfried Scharlau**
   November 14, 2014

   Grothendieck has passed away, a great man and a great human being. I think it is a matter of respect and a matter of honesty to be very careful with statements about him. The internet is full with wrong, half-true, incomplete, sensational and misleading information about him. Do not believe everyting you read and check everything carefully.

   Winfried Scharlau

6. **CIP**
   November 14, 2014

   Thanks for this nice post Peter.

   Note though, that, as Cartier mentions, Grothendieck insisted on the spelling "Alexander" for his first name. A bit of political rebellion against the oppressively intrusive French State?

7. **Peter Woit**
   November 14, 2014

   CIP,
   Thanks. Given Cartier's comment, I did change the spelling.

   I was somewhat tempted to leave the French version though, since it's quite sad to see that so far virtually the only new coverage of his death is coming from the French press, where it's a big story.

8. **XXM2212**
   November 14, 2014

   The point is that only a few mathematicians can really connect to his work .
   He was the purest form of mathematicians ,the best mathematician in the 20th century no doubt about it .
   It would take world leading mathematicians to talk about his maths,for every mathematician it would be the same as some child talking about seashells ,fantasmagoric divination.

The truth is that what distinguishes him is that he answers the Why questions , mathematicians live in a universe which is bounded in the same manner the physical world at some era ,R.Feynman talked about this impossibility in physics. But in maths it is possible … the why is generally untouchable
His maths reflect the universe .

9. **laboussoleermonpays**
November 14, 2014

The last forty years of Grothendieck's life were a long goodbye to mathematics but his "broken dream […] to develop a theory of motives" (to quote P. Cartier in his famous article "A mad day's work…") seems to me a silent hello to physics thanks to the works of Kontsevitch and Connes…

And here is his forecast about a unification theory :
<blockquote cite=
[…] Toujours est-il que de trouver un modèle "satisfaisant" […], que celui-ci soit "continu", "discret" ou de nature "mixte" — un tel travail mettra en jeu sûrement une grande imagination conceptuelle, et un art consommé pour appréhender et mettre à jour des structures mathématiques de type nouveau. Ce genre d'imagination ou de "flair" me semble chose rare, non seulement parmi les physiciens […], mais même parmi les mathématiciens (et là je parle en pleine connaissance de cause). Pour résumer, *je prévois que le renouvellement attendu (s'il doit encore venir…) viendra plutôt d'un mathématicien dans l'âme ; bien informé des grands problèmes de la physique, que d'un physicien* . Mais surtout, il y faudra un homme ayant "l'ouverture philosophique" pour saisir le nœud du problème. Celui-ci n'est nullement de nature technique, mais bien un problème fondamental de "philosophie de la nature
in *Récoltes et semailles*
(Chapitre 2. Promenade à travers une œuvre ou l'Enfant et la Mère. § 2.20. Coup d'œil chez les voisins d'en face, p. 80 (transcription d'Yves Pocchiola))

10. **laboussoleermonpays**
November 14, 2014

English translation* of the last Grothendieck's quote from "Harvests and Seeds"
<blockquote cite=
It nevertheless remains true that the finding of a 'satisfactory' model […] – whether this model was 'continuous', 'discrete' or of a 'mixed' nature – would require a great conceptual imagination, and a consummate art for apprehending and updating mathematical structures of a new type. This kind of imagination or 'flair' is rare indeed, not only amongst physicists (Einstein and Schrödinger seem to be notable exceptions), but even amongst mathematicians (and there I am speaking in full knowledge of the facts).
To sum up, I predict that the long-awaited renewal (if it is still coming…) will come from a born mathematician well-informed about the big questions of physics rather than from a physicist. But above all, we will need a man with the kind of 'philosophical openness' necessary to take hold of the heart of the problem. This problem is by no means a technical one, but is rather a fundamental question of 'natural philosophy'.
*http://www.ens.fr/IMG/file/internationale/2007/Sciences/Epreuve_ecrite_anonyme_commentaire.pdf

11. **A. Weil**
November 14, 2014

Here is a wonderful biography of Grothendieck by Pierre Cartier:
http://inference-review.com/article/a-country-known-only-by-name/

12. **MysteriousFunctor**
November 14, 2014

black fire on white fire…salut

13. **Amir Aczel**
November 14, 2014

Winfried Scharlau, I am impressed that you were able to find him some years ago. He must have told you some interesting things; he wasn't communicating with many people by that time. I hope he had a pleasant "retirement."

14. **Florian Robl**
November 14, 2014

RIP

15. **Neil**
November 14, 2014

Sad. A truly great, great mathematician. I hope Grothendieck did not burn all his papers as, I believe, he one time threatened to do. What a loss that would be.

16. **Bill**
November 14, 2014

http://www.spiegel.de/fotostrecke/alexander-grothendieck-fotostrecke-121125.html

17. **Bill**
November 14, 2014

Alexandre Grothendieck in his own handwriting.

18. **Als**
November 14, 2014

French television went to Lassere, the village where Grothendieck spend the last decades of his life:

http://api.dmcloud.net/player/pubpage/4f3d114d94a6f66945000325/546646c306361d4aaa9d01d8/7ce02452b5f14406aa33ef24cbf849ec?wmode=transparent&chromeless=0&autoplay=1

I'm bit shocked by the complete lack of reaction in the US media.

19. **chiz**
November 15, 2014

From the NY times.

20. **Reza**
November 15, 2014

Dear Als

Thanks for putting the address of this video in your comment.
Grothendieck wanted to be alone and I think he recived his gift.

21. **Thomas**
November 15, 2014

Peter, unsolicited advice: why don't you contribute to cover this yourself in the US media, by writing a piece, for e.g. the Wall Street Journal ?

22. **David Appell**
November 15, 2014

It is not so easy trying to explain modern mathematics to the public. Several years ago I tried to cover the 2002 Field Medals for Salon.com. I was so frustrated I ended up taking a completely different angle:

"Math = beauty + truth / (really hard)," Salon, September 5, 2002.
http://www.salon.com/2002/09/05/math_prizes/

Several people have told me they like it, including an editor at Forbes, and one persobn saying it was the best article about mathematics she'd ever read. (But she wasn't a mathematician.)

23. **Richard**
November 15, 2014

Grothendieck

Brilliance and bizarrerie —
Inextricably intertwined:
No subject that he plumbed
Took the measure of his mind.
A soul forever questing
For the Great Beyond —
A heart forever testing
The limits of despond.

24. **suzanna**
November 15, 2014

I was surprsied that the BBC did not pick up on this and still hasn't! The only English press that has finally covered this is the Telegraph on 14th Nov. I myself only heard of Alexander Grothendiek through one of Peter Woit's recent blogs (i work in a completely different scientific field). I got intrigued by the fact that a genius can abandon his amazing career. it became clear to me that this guy was not only a pure mathematecian but also carried his purity of reasoning to other areas of life from scientific funding issues to the environment and the human condition. ok, maybe some describe his reaction to scientific corruption and social injustices as insanity but actually it's totally admirable and inspiring to all kinds of scientists.

25. **Mathematician**
November 15, 2014

Are there any mathematical notes or manuscripts that can now become available to be examined by experts?

Breakthrough Prize on TV

November 15, 2014
Categories: Uncategorized

You can watch the recent Breakthrough Prize awards ceremony on TV tonight, 6 pm on the Science and Discovery channels. The Science Channel has a [site with videos of highlights of the evening](), the complete list of which is:

- Christina Aguilera singing "Beautiful".
- Larry Page of Google talking about himself and Google.
- Mark Zuckerberg talking about the universe and the "masters of string theory", which is "our best hope for one explanation of reality".
- Michael C. Hall talking about his own cancer, then bringing on Jimmy Wales to talk about a cancer researcher.
- Sergey Brin of Google talking about himself.
- Sergey Brin's wife Anne Wojicki talking about herself, and about her husband's DNA (which supposedly indicates an increased risk for Parkinson's, so research on curing that is important).
- Lana Del Rey singing "Video Games".

I've heard a rumor that one mathematician was actually allowed to say something, for 30 seconds. Will have to wait for the show tonight to see if that was true…

Update: Just saw the show. There was a nice video shown of the mathematicians saying some things about math in general. Unlike the rest of the scientists, they weren't given their award by a star or starlet, but were brought on stage together already holding their awards, and Richard Taylor said something for 20-25 seconds on everyone's behalf. I also hadn't realized that a sizable part of the show was two promotional segments for Hollywood movies (the ones about Hawking and Turing).

Comments

1. **amirpouyan**
   November 15, 2014

   Science and Propaganda* Channel Presents!

   *Propaganda is a form of communication aimed towards influencing the attitude of a population toward some cause or position

2. **David Appell**
   November 15, 2014

   How many explanations of reality does Mark Zuckerberg need?

3. **Peter Woit**
   November 15, 2014

   amirpouyan,
   The Science channel does carry a lot of propaganda for science, and that's not necessarily such a bad thing. The Breakthrough Prize business does fit into that, one of its themes is that science is valuable, deserves more recognition. Another theme though is that Silicon Valley billionaires belong up there with Hollywood and music-world celebrities on TV. They're paying for it, so I guess they should get what they want.

4. **Bernhard**
   November 15, 2014

   This is great material for a South Park episode.

5. **Nobody**
   November 16, 2014

   Peter,

   "… not necessarily such a bad thing."

   Are you sure? Where would the multiverse and other such corruptions be without all the money associated without the publicity? The politicized nature of show biz is powerful. Look at what politics did to climate science. I'd say that physics is playing Russian roulette with this 'star' business. Some money might be generated, but you're losing your soul.

   Run the publicity seekers out of the business and turn down the big star prize money (if you can resist the corrupting lure).

6. **Nobody**
   November 16, 2014

   "without the publicity"

   Cut and paste error: "with the publicity"

7. **Peter Woit**
   November 16, 2014

   Nobody,

   I don't really think that money has much to do with what's causing the multiverse problem (and its predecessor, the string theory unification problem). In general, I don't think the people driving this are doing it for the money. They're doing this because doing something real is hard and frustrating, while this is much easier, and it's easy to fool oneself. As always, the people you see on TV are going to be the ones who are most interested in being on TV. But in a world with no TV science channels, no large book advances, and no billionaires giving prizes, I think we'd have the same problem (although the public would be less aware of it).

   What traditionally keeps a lid on this kind of nonsense is that other scientists, especially ones in your own department, will stop taking you seriously if you start doing this kind of thing, start ignoring your opinions about who to hire, start discouraging students from working with you, etc. The real money danger here is that a colleague spouting pseudo-science is one thing, a colleague spouting pseudo-science who has just won a respected $3 million prize, has a large grant from the Templeton Foundation, is bringing large donors to the university, and attracts students because of his/her TV appearances is a more challenging problem. And even if your colleagues know not to take you seriously, that may be less true for university administrators.

8. **anon.**
   November 18, 2014

   Peter. Are multiverse TV adverts simply a symptom of the problem that alternative ideas are *less interesting* to viewers? In other words, there are people talking about multiverse ideas to lay persons in an exciting way, but the guys with really interesting work don't go on TV to issue adverts, partly because they're busy actually doing stuff, and partly because it's not trivial but requires complex math to explain.

Advertisements for the Multiverse

November 15, 2014
Categories: Multiverse Mania, Uncategorized

After watching the Breakthrough Prize awards tonight, tomorrow night on the Science Channel you can watch a program that actually features physicists rather than Hollywood/Silicon Valley celebrities. There's an hour long infomercial for the Multiverse, entitled "Which Universe Are We In?". You get to hear from

- Max Tegmark starting and ending the show with a generic promotional spiel about how wonderful the multiverse is.
- Seth Lloyd about how weird QM is, and that it and cosmology provide strong experimental support for the multiverse.
- Anthony Aguirre explaining about seeing collisions of other universes in the sky, and about how evidence for the multiverse has now been seen (BICEP2), providing a huge leap forward for the multiverse.
- Laura Mersini-Houghton about the string landscape and how she has used it to make predictions, which are now becoming accepted.

The program ends kind of like a car commercial, with beautiful scenery and swelling music. A voice over mentions un-named fuddy-duddy critics, mainly to say that BICEP2's "great support for the theory of the multiverse" has "given then something to think about". It suggests that the answer to the question raised by all these different kinds of multiverse ("which one is true?") can be answered by believing all multiverse models at once, no need to choose.

No mention of tedious things like dust. This multiverse is all new and shiny, slices, dices, provides every reality you could possibly want.

On a somewhat higher level, Quanta magazine followed up last week's multiverse piece with a new one this past week, Multiverse Collisions May Dot the Sky from Jennifer Ouellette. Aguirre appears here too, working with collaborators on analyzing possibly observable consequences of bubble collisions. One of them is Hiranya Peiris, who explains that multiverse theory is like the theory of evolution:

> Peiris acknowledges that this argument has its critics. "It can predict anything, and therefore it's not valid," Peiris said of the reasoning typically used to dismiss the notion of a multiverse as a tautology, rather than a true scientific theory. "But I think that's the wrong way to think about it." The theory of evolution, Peiris argues, also resembles a tautology in certain respects — "an organism exists because it survived" — yet it holds tremendous explanatory power. It is a simple model that requires little initial input to produce the vast diversity of species we see today.
>
> A multiverse model tied to eternal inflation could have the same kind of explanatory power. In this case, the bubble universes function much like speciation. Those universes that happen to have the right laws of physics will eventually "succeed" — that is, they will become home to conscious observers like ourselves. If our universe is one of many in a much larger multiverse, our existence seems less unlikely.

The problem of course with bubble collision "predictions" are that they're not falsifiable. As far as they're concerned, you can only win: seeing nothing doesn't disprove the multiverse. The most recent attempt to look for evidence in the CMB that I'm aware of is this, which found nothing in the WMAP-7 data. I haven't seen anything using Planck data released so far. Presumably when new data is released later this month some kind of search for bubble collision evidence will be done, and Quanta magazine isn't likely to report the likely outcome.

The Quanta piece isn't an infomercial like the TV program, it does explain some of the problems with this whole endeavor, including this from Erick Weinberg:

> "My own feeling is you need to adjust the numbers rather finely to get it to work," Weinberg said. The rate of formation of the bubble universes is key. If they had formed slowly, collisions would not have been possible because space would have expanded and driven the bubbles apart long before any collision could take place. Alternatively, if the bubbles had formed too quickly, they would have merged before space could expand sufficiently to form disconnected pockets. Somewhere in between is the Goldilocks rate, the "just right" rate at which the bubbles would have had to form for a collision to be possible.
>
> Researchers also worry about finding a false positive. Even if such a collision did happen and evidence was imprinted on the CMB, spotting the telltale pattern would not necessarily constitute evidence of a multiverse. "You can get an effect and say it will be consistent with the calculated predictions for these collisions," Weinberg said. "But it might well be consistent with lots of other things." For instance, a distorted CMB might be evidence of theoretical entities called cosmic strings. These are like the cracks that form in the ice when a lake freezes over, except here the ice is the fabric of space-time. Magnetic monopoles are another hypothetical defect that could affect the CMB, as could knots or twists in space-time called textures.
>
> Weinberg isn't sure it would even be possible to tell the difference between these different possibilities, especially because many models of eternal inflation exist. Without knowing the precise details of the theory, trying to make a positive identification of the multiverse would be like trying to distinguish between the composition of two meteorites that hit the roof of a house solely by the sound of the impacts, without knowing how the house is constructed and with what materials.

There's also the problem that even if you did see something, it really would tell you pretty much nothing about the supposed other universe:

> Should a signature for a bubble collision be confirmed, Peiris doesn't see a way to study another bubble universe any further because by now it would be entirely out of causal contact with ours. But it would be a stunning validation that the notion of a multiverse deserves a seat at the testable physics table.

Update: One problem with arguing that the multiverse is like the theory of evolution that physicists should keep in mind: creationists love it.

Comments

1. **Phil Fogle**
   November 15, 2014

   Peter, after following your blog for several years, I finally got round to reading your book, thank you – an exhilarating read!

   I can't help thinking of Copernicus adding epicycles to his heliocentric model of the solar system to avoid the 'paradigm shift' of Keplerian orbits.

   Multiverse -> epicycles?

2. **Peter Woit**
   November 15, 2014

   Phil Fogle,

   Not sure it's such a good analogy, with epicycles you at least have a rather well-defined, testable framework. Part of the problem with the multiverse is that you don't even have a well-defined theory you can calculate anything with.

Glad you enjoyed the book!

3. **tulpoeid**
November 15, 2014

If I can put a name on what concerns me with "this kind" of theories (strings, multiverse, parallel universes *inter alia*) is their theism.

Their fans glorify the Impossibility of Knowing, and they bask in it.

They are glad that the scientific method has finally found the position such a slut deserves (back to the kitchen) and they feast on the infidels.

Have we already past the point where enough is enough and their marks are already left on science for a couple of centuries? I don't know. But hopefully in the end what will be remembered won't be the silence of the friends.

4. **Michael**
November 15, 2014

Peter,
Is there any chance of sanity and the scientific method coming back to physics? With billionaires giving away money to theorists who have no testable results, it doesnt seem as though there is any feedback to prevent this thing from spiraling out of control. Any ideas how we might see theoretical physics become a science again?

5. **Andrew Foland**
November 15, 2014

On falsifiability: "Fossil rabbits in the Precambrian."

6. **Peter Woit**
November 15, 2014

Michael,
The problem isn't billionaires. The choices of mathematicians were perfectly reasonable, and this year the physics prize went to an important experimental result, which I think had to do with a realization that the physics prize choices of theorists were becoming an embarrassment.

Many parts of theoretical physics are perfectly healthy, but the multiverse mania problem really is just getting worse, and it's a problem generated by physicists themselves, not by the press and not by the billionaires. I have no idea what might slow it down or reverse the trend, keep thinking people will just get tired of listening to the same nonsense. Doesn't seem to be happening though.

7. **Robert Arnold**
November 16, 2014

It is more than evident to any fair minded observer that this pop science multiverse mania harms fundamental progress in physics. It reminds me of the attitude of the of the faculty of the university I attended in the seventies that deterred me from continuing to be a math major.

8. **Martin**
November 16, 2014

This comparison with evolution theory is nonsense. From the fossil record (e.g. the faunal succession) and DNA it is evident that the biological species have not all come into existence at the same time as the Genesis fairy tale would have it, but in temporal succession. And that the fittest (in terms of adaptation to the environment) survive is seen all the time. To suggest that evolutionary theory is a tautology is BS. The theory of evolution is not at all like this multiverse nonsense for which there is no evidence at all. Peiris must be desperate to come with this "comparison".

9. **Low Math, Meekly Interacting**
November 16, 2014

I suppose I must accept some of the blame myself. Subjects like unification and universal origins are intrinsically fascinating to me, and I keep going back to those subjects again and again in hopes of learning about the true answers to the deepest questions. This is in spite of the fact that there's virtually no hope of any relevant prediction making contact with experiment.

Meanwhile robust and vital physics is being done in areas like calculating QCD phenomena from first principles, untangling the mysteries of high-temperature superconductivity, using quantum mechanics to speed up computation without worrying about all the dead cat nonsense. I.e. doing research that is constantly confronted with the unforgiving standard of nature as we know it exists. Such work is old, somewhat prosaic, fiendishly difficult, tends to yield only incremental progress over the span of careers and does almost nothing to satisfy the primeval desire to somehow understand "what it all means".

Unfortunately, the multiverse is the only realm in which such spiritually satisfying pursuits are likely to flourish while I am alive. Maybe a bit like a habitual parishioner finally losing his faith completely, and facing the loss of the sense of certainty and wonder that faith once promised, I'm mourning. I used to love reading these gripping stories about strings and loops quantum proliferation. But it's dead to me now, and the storytellers are not sages; they're human just smarter-than-average beings who went far down the wrong path in pursuit of some standard of beauty they'll never realize, and likely doesn't even exist.

I'll look forward to the LHC telling us a bit more about things like QCD matter and the properties of the Higgs. I'll try to be more appreciative of the real science being done, and give the time and consideration people doing such difficult and important work are due. I'll once-and-for-all tell multiversalists where they can shove it.

10. **Simple biologist**
November 16, 2014

The comment about evolution is so ignorant that I can't even be bothered to de-construct it. Just about the only good thing left to say about these stringy multiuniversalists is that they serve as an example and warning of the pathology that happens to the natural sciences when they get separated from nature and observation.

I just wish that they wouldn't try take to take biology, chemistry, condensed matter physics and other forms of science still following scientific method down to their path into pseudo-science and metaphysics.

11. **Peter Woit**
November 16, 2014

LMMI,

I don’t think it’s at all necessary to only pay attention to ideas that are experimentally testable. Actually, testability in itself is not the real problem with the multiverse. The problem with the multiverse is that it’s inherently an empty idea, devoid of explanatory power, being invoked as an excuse for failure. In fundamental physics, there are a small number of questions we don’t understand, and there’s nothing wrong with speculative work with a long-term goal of understanding those, but now far away from useful contact with experiment. As an example: there’s a huge amount we don’t understand about QFT, and trying to improve that situation is very worthwhile, even if dealing with questions about QFT not relevant to experimental tests.

The problem is that this kind of hard work has trouble competing with hucksters who claim that they already have the answers. Such hucksters are always going to get on TV shows, the disturbing trend is that they’re now starting to get real traction in the scientific community itself. Dealing with this I fear is not a problem for the public, but a problem for scientists themselves.

12. **Steven**
November 16, 2014

Thr multiverse interpretation I think is a primary example of an empty or bad explanation like the Ancient Greek Myth of Persephone as an explanation for the seasons. It was specific/testable, but not truly falsifiable because you could ways move the goalpost or alter something without questioning the main idea.

13. **Neil**
November 16, 2014

Actually, Persephone was falsifiable, with a trip to the southern hemisphere where it is summer in january. The multiverse has lots of explanatory power. It can “explain” everything, and therefore nothing.

14. **chris**
November 17, 2014

Peiris’ take on evolutionary theory serves to show exactly two things: his utter ignorance and arrogance. unfortunately, physicists are derided as arrogant by many of the practitioners of the so-percieved “less hard sciences” and this is a perfect example why.

I wonder whether she realizes how much harm she does to the credibility of science with statements like these. Or if she even cares, for that matter.

15. **Visitor**
November 17, 2014

The tautology that Hiranya Peiris has in mind is “the survival of the fittest”. I.e. “they survived because they were fit and we know that they were fit because they survived”. In the context of evolution that is a tautology but it is not quite the same as Peiris’ garbled “an organism exists because it survived” .

“One problem with arguing that the multiverse is like the theory of evolution that physicists should keep in mind: creationists love it. ”

Let’s just keep them out of the argument. Saying that this or that theory is unacceptable because creationists like it is no better than saying that the multiverse is true because is supports atheism (which is Susskind’s position.)

See http://en.wikipedia.org/wiki/Association_fallacy

16. **Mike Sharples**
November 17, 2014

The “epicycles” analogy is an interesting one. However I think it is an analogy that applies to the Standard Model rather than the Multiverse. The arbitrary terms of the SM could be considered similarly to planetary epicycles, motivating one to look for a deeper model underneath which may explain why the terms are what they are.

Unfortunately the Multiverse says you can have any “epicycles” you like. No further explanation necessary!

17. **Stuart**
November 17, 2014

There is only one hope out of this multiverse/dimensions mania. Let the real falsifiable quantum theory of gravity show up.

18. **Mike Sharples**
November 17, 2014

“Let’s just keep them out of the argument. Saying that this or that theory is unacceptable because creationists like it is no better than saying that the multiverse is true because is supports atheism”

I am not so sure I agree. I think it is very important to understand why creationists like the Multiverse so much. It is not that they believe in it, they like it because it so seriously discredits the anthropic principle.

19. **Peter Woit**
November 17, 2014

Mike Sharples,
It’s not that creationists like the multiverse or care about the anthropic principle. What they (or at least some of them) do like is when prominent physicists announce that the theory of evolution and an obviously pseudo-scientific theory have the same status.

20. **B'Rat**
November 17, 2014

Just wondering… who exactly is investing the big money on such infomercials?

21. **Peter Woit**
November 17, 2014

B’Rat,
I don’t think filming a physicist talking in an exotic location and throwing in a few special effects is really all that expensive. So, this infomercial probably didn’t cost a lot

to produce, and it's being shown repeatedly on cable channels, funded by commercials and your cable fees. So, no big money needed, it's just the usual sort of project that TV production companies do all the time.

22. **Jeff M**
November 17, 2014

Actually the "tautology" argument really is just silly, and shows up Peiris as at best completely uninformed. Biologists have of course known about that from the beginning, Stephen Gould talks about it quite a lot. "Survival of the fittest" is a tautology, but that's not what evolutionary biologists talk about. They talk about natural selection, and how it operates on various traits. Simple Biologist would no doubt be able to give a much better explanation than I can, as a lowly mathematician, but it's very clear that evolutionary biologists don't use any sort of tautological arguments. It's depressing that a high end physicist thinks they do.

23. **Nobody**
November 17, 2014

Jeff M,

To me. the TOE _is_ tautological in the sense that it is so good that, if it were not true, that fact would require an explanation – that is, in the absence of experimental evidence directed for or against the TOE, one would expect it to be true. This would not, of course, be a true tautology but rather, it seems to be a prediction or consequence of already known science. The amazing discovery would be that the TOE was _not_ true, and we would have to search for an explanation.

OTOH, the falsity of the multiverse would require no explanation. One does not expect it to be true on the basis of uncontroversial science,

This would make the TOE completely dissimilar to the "gee whiz" multiverse, which is just an escape from the hard problem of understanding and explaining, in a verifiable way, the particulars of this physical reality which we have come to know and love, and for which we have more concrete evidence than that we can imagine it.

24. **Peter Orland**
November 17, 2014

"To me. the TOE _is_ tautological in the sense that it is so good that, if it were not true, that fact would require an explanation – that is, in the absence of experimental evidence directed for or against the TOE, one would expect it to be true."

You must be joking. This is not a scientific argument. It reminds me of arguments for the existence of god – or communism – or scientology.

25. **Peter Woit**
November 17, 2014

All,
Not much point in arguing about the possibility of a self-evidently true TOE, all that's clear now is that no one has a candidate for such a thing. And if there were such a thing, I don't think it would be "tautological", but rather some sort of opposite of tautological, not telling us nothing new, but telling us everything.

26. **Supernaut**
November 17, 2014

"I haven't seen anything using Planck data released so far." Lloyd Knox of UC Davis gave a popular talk last week about the Cosmic Microwave Background as observed by the Planck spacecraft. At the end of the talk someone asked him about this (multiverse collisions) and he stated that they had seen no evidence to support this.

27. **Theo Nieuwenhuizen**
November 17, 2014

Dear Peter, this multiverse nonsense is more than I can bear. How can serious people throw our trade in the garbage bin on selling nonsensical emptiness? They also make me look like a fool, I am in the same trade, called physics.
But one thing is clear already: the many worlds idea about quantum mechanics is a mistaken, nonsensical issue. I spent 15 years on studying what goes on in quantum measurements, the only point of contact between the quantum framework and the reality of tests in detectors. We have good results and a good picture of what goes on. What is very clear: the many worlds idea comes in nowhere and it has nothings to to with the whole subject. It is a lot of talking about a misconceived structure of the theory. You better adopt the ensemble interpretation, and for those who don't like that, let me add: Einstein worked on it, even in his last year.

28. **David**
November 17, 2014

Mike Sharples,
"Unfortunately the Multiverse says you can have any "epicycles" you like. No further explanation necessary!"
In a sense this is true. But I'm guessing the folks in favor of the multiverse argue that what makes the multiverse explanations for things possible is string theory with its myriad of ways the extra dimensions can compactify. Presumable, string theorists are hoping that eventually we will understand string theory much better, and be able to understand the mechanism by which a multiverse is generated as well as what makes each "universe" take on the specific set of laws it takes on.

But perhaps in the distant future, some theory (be it string theory, or something completely different) will come along and do a superb job at explaining everything we see around us, unifying gravity and quantum mechanics, and all the particles and interactions. Perhaps it will be validated by many experiments. But if it predicts a multiverse, then that would be very strong (indirect) evidence in favor of a multiverse. Perhaps this theory will rely on the multiverse explanation for at least one parameter that we observe (eg, dark energy). If the multiverse exists, then I suppose that's the closest we can come to showing it exists.

29. **Peter Woit**
November 17, 2014

David,
Sure, if you have a well-tested theory that implies a multiverse, that would be a good reason to believe it. With string theory though, this is being used the other way around, with the multiverse given as the reason for not being able to test it.

Theo,
I pretty much agree with you about the many worlds interpretation. But I hope people who want to argue about that will wait for a post where it's the topic, which I'll get around to some day…

30. **Nobody**
November 17, 2014

Peter Orland,

“You must be joking. This is not a scientific argument. It reminds me of arguments for the existence of god – or communism – or scientology.”

It’s not a scientific argument for what?

I said that one would *expect* the TOE to be true and that if it turned out not to be (obviously implying experiments) one would need to know why. That is not an argument either …

Wait a minute, I know what’s wrong. I used “the TOE” to mean “theory of evolution” and you (and Peter W) took it to mean “the theory of everything”.

(slapping forehead)

I was trying to outline a substantial difference between the type of theory that was the theory of evolution, before it was established through research, and these theories of everything that are making physics look like a carnival side show. The reason I am doing it is that the comparison was made in the article referenced above.

31. **Peter Orland**
November 17, 2014

Sorry about the misunderstanding, Nobody.

32. **Peter Woit**
November 18, 2014

Nobody,

Funny. I thought the particle physicist’s had a trademark on “TOE”…

33. **nb**
November 18, 2014

“One problem with arguing that the multiverse is like the theory of evolution that physicists should keep in mind: creationists love it.”

Not just they love it. The cited argument is the so called ‘tautology argument’.

http://www.talkorigins.org/faqs/evolphil/tautology.html

Can’t find the right words.

34. **Tammie Lee de Cortez Haynes**
November 18, 2014

Dear Dr Woit
.
You really shouldn’t tell us what Creationists think.
Unless you like telling us stuff that’s wrong.
.
I’m a Creationist.
I like the multiverse for this reason:
If the multiverse is the best case that Atheists can offer, Creationism is in the catbird seat.

35. **scottrileywilson**
November 18, 2014

Peter,
As bad as you think these “theorists” are I don’t even think you fully realize the negative impact they have and are making. Science has become religion. Peer review has become the new authority…etc

36. **Curious Mayhem**
November 18, 2014

Umm … there’s no evidence for any multiverse in the BICEP2 data.

If they mean evidence for inflation, inflation doesn’t require a multiverse. Even the Linde eternal inflation idea is not the modern multiverse concept, which comes from trying to save string theory from itself.

Meanwhile, while there’s no string cosmology, “string-inspired” cosmology has a hard time predicting inflation. If the next round of precision CMB measurements do confirm anything like what BICEP2 reported, string theory will have suffered another mortal blow.

How this is related to Darwin’s natural and other mechanisms of selection and evolution is beyond me. Unobservable multiverses, mutually disconnected, don’t “compete” in the same ecosystem and have nothing to adapt to. From a mathematical point of view, there’s no defined space of possibilities, or measure on that space. How do you compute probabilities?

I know, I know — I just don’t appreciate the poetry of it.

37. **Monty**
November 19, 2014

Peter, you wrote: “The problem of course with bubble collision “predictions” are that they’re not falsifiable. As far as they’re concerned, you can only win: seeing nothing doesn’t disprove the multiverse.” There’s not really anything wrong with this kind of search for corroborating evidence though, really. I mean, what’s the difference between that and, say, someone hunting for exoplanets in 1960? It was theorized that other stars should have planets, and until we had better equipment no one would have suggested in 1960 that not finding any was proof that there were none out there. But if there *happened* to be one big enough and near enough that a telescope in the 60s could see it, well great. The hypothesis that there were exoplanets was not falsifiable in 1960, but someone could have proved it if they’d got lucky. By analogy, some people think we live in a bubble universe that could in the past have intersected with another bubble, leaving a trace. If we find such a trace, that will be a pretty good data point. If we don't–well you're right, we don't learn anything from that, because maybe the hypothesis is wrong, or maybe the traces are too far in our past, or maybe we're not looking right, or maybe our bubble is still a "virgin". But does that really mean no one should be allowed to look for the traces in the first place? There are probably better examples than exoplanets… counterexamples to the Riemann hypothesis? If we look and don't find one, we haven't learned anything. Should we not bother looking, then, because it's a search that can only result in winning or stalemate, but never losing? Or SETI signals–no one will ever be convinced no one else is out there just because SETI doesn't find any signals. Does that mean they shouldn't even look? etc. etc.

I mean I agree there's no evidence at all to believe that our bubble ever intersected another, or even that there were ever any other bubbles coexisting with ours in the first place. But you can't also blame people exploring these ideas for actually going and looking for evidence – surely that can't be a bad thing?

38. **anon.**
November 19, 2014

Monty: a better analogy is philosopher Auguste Comte (1798-1857) who (prior to the discovery of emission and absorption line spectra from various elements in sunlight) claimed it impossible to ever know the composition of the sun, because it's too hot to visit and sample. This is typical of defective "no go theorems," which only get "accepted" without scrutiny because nobody has the time or inclination to argue over them. Woit and others are not, however, claiming to disprove and discredit all research on certain topics that are currently unproductive; rather they are trying to deflate the hype from populist speculations that are way overblown in the media.

39. **Dom**
November 19, 2014

Monty. As I understand it, the problem is not speculation and trying to find things out, it is partly presenting science fiction as science fact and partly suggesting that anything we don't have an answer for is "Because Multiverse".

40. **Nobody**
November 19, 2014

"There's not really anything wrong with this kind of search for corroborating evidence though, really."

There's nothing wrong with looking to see what's there – in fact it's necessary, but since you put it that way ("corroborating evidence"), what is it that you are trying to "corroborate"? Do you have any falsifiable predictions about the signal or is it "anything surprising will do"? If you can't make a falsifiable prediction then you can't use unpredicted results as a "corroboration" of an existing scientific theory.

So what is the theory and what observations does it predict?

How would an unexpected, unpredicted, structure in the sky "corroborate" an existing theory that makes no predictions any more than the existence of specific constants in our familiar world, which constants have never been derived from an over-arching theory that survives Occam's razor, "corroborates" the multiverse? How would your multiverse theory without predictions be any better than "God set it up that way"?

A large scale, unpredicted, structure is nothing. Were such a thing found, you would have to write a theory simpler than the result and then verify it through its predictions. You aren't at the first step of that. You haven't even begun. You haven't found something unknown to explain.

You multiverse lovers are already involved in overwrought promotion of fantasy as fact in the popular press. I dread the impact of an actual discovery on that existing, shameless performance, but _I_ predict that, should that happen, there will be an overnight cobbling up of some BS theory which would magically be much easier for our geniuses to arrive at than a theory explaining the parameters of our known laws, especially since multiverses are so fascinating to the gullible. Thinking will come later.

Oh wait, I forgot – the parameters of our laws have already been explained by the multiverse theory, as a consequence of its 3rd law – "whatever is, is".

Kaku's a good at this TV stuff. Perhaps you can arrange, beforehand, for him to explain how the multiverse is proven the very evening after something unknown is discovered or even better, he could explain it the day before as he already is.

As for exoplanets, their existence is a straightforward expectation from known theory. The form of an exoplanet's signal was predictable and easily separable from spurious results. The discovery of their signal confirmed what was predicted and is providing fine tuning detail.

41. **Peter Woit**
November 19, 2014

Monty,
Of course I'm not arguing one shouldn't look, but just pointing out this is not a falsifiable test of the theory, and when, as everyone expects, nothing is found, no one will report on this or pay any attention.

42. **Fred P**
November 19, 2014

"The theory of evolution, Peiris argues, also resembles a tautology in certain respects — "an organism exists because it survived" — yet it holds tremendous explanatory power."

"an organism exists because it survived" is an observation, not a theory. It is an inaccurate re-statement of survival of the fittest (inaccurate in part because survival of the fittest works on populations over generations – it doesn't describe an individual organism), which is a somewhat misleading phrase describing the process of natural selection. Natural selection is quite testable and verifiable, even if the biochemical mechanisms behind natural selection were not known for a long time after On The Origin of Species was published; indeed, On The Origin of Species itself has a lot of evidence for natural selection. On The Origin of Species, 1st edition 5th edition, with a definition of "Survival of the Fittest"

Finally, natural selection itself is only a subset of the modern theory of evolution. Mutation bias (some mutations are biochemically more likely to occur than others), as one example, is also part of the theory of evolution which is not part of natural selection. Sample source on Mutation bias)

So Peiris is arguing using an inaccurate re-statement of a misleading description of a portion of the theory he's claiming that he's describing to attempt to buttress a particular theory.

43. **Katy**
November 25, 2014

I personally never found the concept of the multiverse to be beautiful, though I was willing to accept it as a grim reality. We should be relieved if the multiverse were universally accepted as bunk. There is plenty left to ponder and strive for without chalking up every event as "bound to happen somewhere in the multiverse."

Planck News?

November 19, 2014
Categories: Uncategorized

The timing for release of long-awaited Planck polarization data keeps getting pushed back. At one point it was supposed to be earlier this year, most recently it was supposed to be this month, with that timing forced by a conference devoted to discussion of the results planned for December 1-5. The website for that conference now says:

> The 2014 Planck public release of data products and papers will actually take place a few weeks after this conference. This conference is therefore the first occasion to preview the Planck 2014 data products and discuss their scientific impact. The presentations will be videocast online. After the conference, the presentation slides will be made available.

Another conference scheduled assuming the data will have been released is this one in Paris December 15-19.

The Planck website now reports:

> – The data products and scientific results will be presented at a public conference in Ferrara. The presentations will be videocast during the conference and slides will be made available after the end of the conference.
>
> – It is planned to release all major data products and scientific papers to the public before the end of 2014. A few of the derived products (e.g. the Likelihood code) will need a little more time to be readied for release, but will be made public within the month of January 2015.

David Spergel on Twitter reports December 22 as the date for release of papers and data.

It will be interesting to see how the cosmology community deals with the situation of no papers or data, just videocast and slides, from December 1 on. From similar situations in the past, some people have highly developed technology for scraping data off slides, presumably that will be in high demand.

Comments

1. **piscator**
   November 19, 2014

   Given that one informally hears from people in the collaboration that it is a desperate rush to get these papers written in time, the question arises as to how trustworthy and nailed-on the results will be. e.g even the WMAP-9 paper, with much more time on a well-understood instrument, had in its original version a significant error in the headline numbers in the abstract.

2. **JG**
   November 19, 2014

   Oh dear, so it looks like the 700 million euro Planck project won’t be able to rule out BICEP2’s conclusions after all. Christmas time release of disappointing news is an old trick.

3. **West**
   November 20, 2014

   Everyone expects there to be a mad dash for whatever sort of data that can be gleamed from presentation slides. I am more curious what the Planck collaboration is doing to try to manage the free-for-all.

   With seemingly reliable techniques to estimate the uncertainties inherent in screen-scraped maps, I hope we won’t see a replay of previous silliness. If Planck doesn’t come out with its own preliminary parameter estimates, someone else definitely will (poorly done or not) using their now public data. So I expect some actual numbers from the first December conference and not just a flashing of maps on the screen for a second or two.

   Or the entire presentation could be devoid of images, which would be a hilarious tease to the model building theorists.

4. **Ben Gold**
   November 20, 2014

   I think with Planck’s high resolution screen-scraping is less valuable, and the current excitement is around B-modes which you will never get in any believable way by screen-scraping a polarization map. I imagine the main delay of releasing maps is precisely because they know full well that the instant they’re out people will be running their own analysis code and publishing power spectra and cosmological parameter estimation.

   In any case, it sounds like their plan is to release the maps and basic parameter results at the same time. Likelihood code gets released a little later mostly because that’s a pretty big chunk of software & data that has to be cleaned up once the analysis chain has settled down.

5. **Sesh Nadathur**
   November 24, 2014

   Ben Gold: it doesn’t seem to make much sense releasing parameter results and model constraints if the likelihood code is not settled. Probably only those non-controversial results will be released where a later change in the likelihood code is unlikely to result in a significant change to parameter fits. Which means there won’t be much new to learn from Ferrara.

6. **Ben Gold**
   November 25, 2014

   Sesh: I meant more that the likelihood code release usually happens after running -all- chains, including the weird stuff like simultaneously allowing tensor running, variable neutrino number, isocurvature modes, etc. Some of those chains can take a long time to converge, and sometimes involve changes to the code which are insignificant unless you’re one of the three people worldwide interested in that particular model. But they’re totally unnecessary for a “basic results” paper.

   That doesn’t mean only non-controversial stuff can be released before then, since there could be something new that’s demonstrably robust against other weird parameters (say, n_S running) without requiring lengthy chains of every possible parameter combination.

7. **Martin**
   November 28, 2014

   Peter, as always thank you very much for the comprehensive coverage! Interestingly, the following statement is now missing on the conference page: “The presentations will be videocast online. After the conference, the presentation slides will be made available.”
   Maybe they changed their mind to prevent the problems you alluded to (screen-scraping etc) ??

The Imitation Game

November 19, 2014
Categories: Film Reviews

This season's Hollywood math/physics extravaganza is starting to come to an end. For coverage of the Breakthrough Prize ceremony, I enthusiastically recommend Michael Harris's new piece at Slate which just appeared.

The final high profile production, one promoted at the Silicon Valley ceremony, should be The Imitation Game, a film based on the life of Alan Turing, to be released on November 28th. I had the chance to attend a preview screening last night, featuring a Q and A with the film's screenwriter. The short version of a review is: go to see this is you like watching Benedict Cumberbatch and Keira Knightley perform, but if you want to know anything about Turing, avoid the film and spend your money instead on a copy of the new edition of Alan Turing: The Enigma by Andrew Hodges.

Turing's story was little known until 1983, when Hodges published his biography, which is just fantastically good. Hodges (see his web-site here) is a mathematical physicist who began working with Penrose back in the 1970s on twistor diagrams, work that has recently played a prominent role in the hot topic of new methods for computing scattering amplitudes. The Hodges book made Turing a famous figure, partly for his code-breaking role, partly as a martyr for gay rights given the horrific story of the way he was treated because of his sexual orientation. By 1986 the biography had inspired a play, Breaking the Code, that ran in London and New York, and then became a 1996 movie. There have been other film treatments of the story since, including the 2011 Codebreaker.

Other than a few general facts, the part of the film set at Bletchley Park has little relationship to reality, with almost none of what is portrayed actually having happened. As just one example of the sort of thing that was made up out of whole cloth, the film has Turing discovering a Soviet spy, who uses his homosexuality to blackmail him into silence. Cumberbatch plays a compelling character, but one much like his Sherlock Holmes on TV, not like the Turing of the Hodges book, or like any other mathematically talented person I've ever known.

It often mystifies me why people who make movies based on fascinating real stories sometimes just ignore what really happened and instead make up a much less interesting plot. In this case, hearing from the screenwriter after the film made the problem clear. He seems convinced that Turing is a little known figure, and that it is his job to reveal this unknown story to the public, unaware that this was done much better back when he was in pre-school. From his comments, he never bothered to understand anything about what Turing actually did during the war, in particular he is convinced that Turing's big breakthrough was to realize that to break codes it was helpful to know some phrases that were likely to be in the message (e.g. "Heil Hitler"). He explained that he was sure that Turing saw himself as a figure in a thriller, and that informed how he wrote the film. All in all, he had a very simplistic agenda (to reveal the unknown fact that a gay man had won World War II) which completely overwhelmed any interest in the details of what actually happened.

The contrast with the recent Stephen Hawking biopic is striking. That film took some dramatic license, and simplified some complex people and situations, but it didn't just completely make things up, and the star's portrayal of Hawking was convincingly true to life. The memory of Alan Turing would have been much better served by a similar degree of respect for reality.

Update: The Guardian has a review, which explains some of what the film gets wrong. For something with more detail, see this.

Comments

1. **S**
   November 19, 2014

   This is very discouraging to hear. "Breaking the Code" is a fine play — it's too bad, even mystifying, that they didn't trace that back to its source (which would have been easy).

   I second your mystification point. Years ago, I was involved in a society devoted to the work of a writer (who had an exceptionally interesting life) and we were contacted by a group of filmmakers who were making a fairly high-profile film about him and wanted help locating an example of his handwriting so that they could get every detail perfect. We obliged, of course, but imagine our surprise when they had completely substituted his life with far less interesting Hollywood boilerplate in the film.

   Some things never change I guess.

2. **Jeff M**
   November 19, 2014

   Peter,

   Not to pick nits, but describing Turing as "mathematically talented" is kind of like saying that Micky Mantle was a pretty good ballplayer. 🙂

   I am not a logician, nor do I play one on television, but Turing's proof of Godel's theorem is one of the most beautiful things I've ever seen. And he did much much much more than that.

3. **Peter Woit**
   November 19, 2014

   S,
   The Hodges book is credited as "inspiration" for the film, and some of what's in the early part of the book about Turing's experiences in school and his first love for another boy is used in the film. I suspect though that once the story moved to Turing's actual career and the work done during the war, this became technical material the screenwriter had no understanding of or interest in. From then on, supposedly more dramatic fiction replaces non-fiction.

   The Soviet spy business is a weird example, since it's not only a fantasy the writer dreamed up, but exactly the fantasy that later motivated Turing's homophobic persecutors (i.e. that his homosexuality made him someone the Soviets could blackmail).

4. **bugannoyer**
   November 19, 2014

   In case you want to read a more entertaining fantasy involving imagined Turing activities during WW II, I recommend Neal Stephenson's "Cryptonomicon". Turing is a secondary character.

5. **Sammy**

November 19, 2014

If anyone has any doubts about reading the Hodges biography, then I should pass on that in 1983 I loaned my copy of the 1st edition to a couple who had actually worked with Turing. They both said it was very faithful to the man they knew.

6. **Yatima**
November 19, 2014

Seconding "buannoyer" on "Cryptonomicon". It's a fun but sometimes overly loquacious story. A notable book of 1999.

It is an interesting twist of history that Alan Turing, inventor of the concept of the Universal Turing Machine, designed the specialized "Bombe" to break the Enigma cipher in Hut 8, while the world's first fully programmable electronic digital computer, the Colossus, was designed by Tommy Flowers just a hut or two away to break the Lorenz cipher.

P.S. @Jeff M.: "Turing's proof of Godel's theorem". I suppose you are talking about the negative answer to Hilbert's "Entscheidungsproblem": Given a formula in First-Order Logic, is there a procedure that can say whether the formula is a tautology or not? Gödel's completeness theorem proves that if the answer is "yes", this will eventually be proved (which practically may take longer than the universe's lifetime though). Turing showed that if the answer is "no", the procedure may well never find out. Alonzo Church proved the same a bit earlier using a different approach.

7. **Jeff M**
November 19, 2014

@Yatima,

Yes, but I was keeping it simple for the physics types 🙂

8. **anon.**
November 20, 2014

There's a nice extract from Turing's popular article, "Solvable and Unsolvable Problems", published by Science News in 1954, here:
http://www.ens.fr/IMG/file/internationale/2009/Sciences/Sujets_commentaire_SIS09_en.pdf

9. **Em Comments**
November 20, 2014

"Turing's story was little known until 1983"

Yes exactly. That's why the Turing Award (http://en.wikipedia.org/wiki/Turing_Award) which is described as 'the "highest distinction in Computer science" and "Nobel Prize of computing"' was created in 1966 as he was unknown until 17 years later.

10. **dpb**
November 20, 2014

@em comments

Turing was known as a pioneering computer scientist and mathematician in the 50s and 60s, but very few people had any idea what he was up to during the war. People from Bletchley Park tended not to talk.

11. **Visitor**
November 21, 2014

No one else seems to care but I am profoundly offended by the idea that Turing "won the war".

12. **Dom**
November 21, 2014

Visitor, you may be British and I may have the wrong idea, I don't know but speaking as a British person, the oft-repeated snippet from the trailer where the character says something about "Breaking an unbreakable code and winning the war" is to my ears typical British humour where we will matter of factly put together two difficult and possibly unrelated things as if it was a formality.
Having said this and having read Hodges book when it was first published as my late father had a great interest in Enigma, the film looks like a missed opportunity.

13. **srp**
November 21, 2014

Turing's work gets what sounds like better treatment than John Nash's did in A Beautiful Mind. How hard would it have been to get the Nash equilibrium concept right instead of mangling it completely? It's not that abstruse an idea.

14. **Michael Shain**
November 21, 2014

Our local cinema, The Phoenix, in East Finchley, London had a special showing of The Imitation Game followed by a question and answer session with a panel a member of which included a very lively Ruth Bourne well into her eighties.
Ruth was a Royal Navy Servicewoman who was a Bombe operator and was asked about war time romances at Bletchley Park. The entire cinema audience collapsed when she said "the odds were good but the goods were odd".
There is a wonderful interview on YouTube recorded in 1992 "From Code Breaking to Computing: Remembrances of Bletchley Park 50 Years Later"
http://www.youtube.com/watch?v=6p3mhkNgRXs
where Jack Good, who worked with Turing, and Donald Michie are interview by David Kahn. To quote from the blurb:
"In this 1992 interview Jack Good and Donald Michie discuss their cryptanalytical work during World War II at Britain's Bletchley Park. The technical aspects of Germany's Enigma and Lorenz Geheimschreiber, the code-breaking Bombe, Heath Robinson, and Colossus machines, and the personal contributions of Max Newman, Alan Turing, and Tommy Flowers are explored. Donald Michie, in the final part of the interview, offers some details on the origins of artificial intelligence research."

15. **Dom**
November 21, 2014

Bill Tutte and his reverse engineering of the Lorenz machine configuration just from the ciphertext always deserves a mention I think.
Bill Tutte Wikipedia

16. **Visitor**
November 22, 2014

"The contrast with the recent Stephen Hawking biopic is striking. "

…along with a few other striking contrasts. For example, Hawking is still alive whereas Turing is dead and couldn't object to anything that the film makers wanted to do. There is no doubt that had Hawking died before the picture about him was made, the contrast between it and the Turing picture would be rather less striking…

Remember: a story "based on a true story" is false story.

17. **Joe Prokop**
November 22, 2014

Hi,
This link was posted on a film review website for the Imitation Game,
http://en.wikipedia.org/wiki/Marian_Rejewski.
It would appear that a joint effort by Polish and French Mathematicians including Unercover activities broke the Enigma Cipher in 1932. Mathematical Techniques developed by Prof. Rejewski and colleagues solved the Cipher. It is interesting how this has never been recognized in the popular accounts about code breaking during WW II.

18. **Chris W.**
November 22, 2014

Michael Shain mentioned Tommy Flowers. See this article, published about a week ago on IDGConnect.com:

> As contemporaries explained in a short film made by Google, once war was over the orders were to smash everything to pieces. According to these testimonials some Colossus machines were even dumped down coal mines. This was extremely galling for the people who had worked hard, for years, creating and nurturing these machines.
>
> It also pushed back the development of computing. "Tommy Flowers held various master documents in a safe concerned with Colossus and he had been instructed that these were to be destroyed," described one individual. "And he went down to the workshop and destroyed them. Put them on the fire. That was the end of them."

19. **dom**
November 23, 2014

The Poles have been given credit for their work in the detailed accounts I have read.

20. **Dom**
November 24, 2014

As a follow-up to my comment about the Poles, here is a timely bit of correspondence in The Guardian Engineers, linguists and other heroes

21. **Peter Shor**
November 29, 2014

@Joe Prokop: As I understand it, the Poles and the French broke the 1932 Enigma cipher and got the techniques to the British. But by 1939, the Germans had changed the Enigma machines and made the cipher harder to break. So Turing and Bletchley Park actually needed to do more than just use the Polish techniques.

22. **R.K.**
December 1, 2014

"not like the Turing of the Hodges book, or like any other mathematically talented person I've ever known"

one could remark that it is usually interesting to see how (male) mathematicians and physicists tend to define 'themselves' by certain 'habits', certain behavioural traits that they obviously think they somehow learned already during infancy, or even before and not while conformizing each other during the eras of shapening their career. Instead of taking the above dicscussed film, I didn't see it and will not ever make any attempt to see it, as a starting point to discuss stereotyping of and *among* mathematicians and how these patterns could maybe possibly be overcome in the distant future, one takes the 'mis-match' of the film with a possible stereotype as a further proof that 'it doesn't show reality'. To remind the reader also of that: art is not about reproducing reality, even if a film or a piece of 'art' (I don't know if I wouldn't call this film art, but it could be close) is based on real characters or events it is usually judged as a poor endeavour for any artist to merely reproduce 'reality' that i.e. the cinema misleadingly often tries to imitate. Instead, such films possibly have to be viewed as documents how society deals with science and/or mathematics and such endeavours can be shallow, but they will always lead to interesting insights, if more on a meta-level (i.e. discussions like these here).

23. **Curious Mayhem**
December 3, 2014

One of the striking things about the real story of Turing is that it is unlikely that he died a suicide. His mother, for one thing, emphatically denied it. It seems that Turing had a penchant for experiments with poisonous chemicals, which is probably what led to that deadly apple bite. See *Turing: Pioneer of the information age*, by B. Jack Copeland.

For Your Viewing Pleasure

November 29, 2014
Categories: Uncategorized

If you've already seen the various new math/physics films coming out of Hollywood, this week you might be interested in watching some of the real thing, including the following:

- The hot ticket this week will be Monday's Planck session at the conference in Ferrara. They've now edited their website to remove references to a promised webcast and slides. So, the only way to get images suitable for scraping may be to get yourself into the lecture hall at Ferrara and bring a camera. Press release here, conference program here.
- Also on Monday, if you're in Cambridge (MA), there's Steven Weinberg's Lee Historical Lecture in Physics, topic Glimpses of a World Within. The only blurb is

  > "Since the 1970s the evidence has accumulated that the structures appearing in the laws of nature at a really fundamental level are vastly smaller than anything we encounter in our high energy laboratories."

  which I don't think I'd personally agree with, quite curious to see what case he makes. These lectures often are later made available here.

- For another historical lecture, there will be a webcast of this event at CERN on Tuesday. It features film of interviews with Roy Glauber, characterized as "the last living scientist from the Theory Division of the Manhattan Project". Glauber taught the first quantum field theory course I ever took, at Harvard in 1976-77, almost forty years ago (I thought he was pretty ancient at the time). The year earlier I had taken quantum mechanics with Norman Ramsey, here shown signing Fat Man. At the time it seemed perfectly normal that all my instructors had gotten their start designing weapons.
- If you're in Berkeley this week, you could attend a Langlands-related conference at MSRI, which in addition is honoring Michael Harris. Program is here, videos to appear soon after the talks.
- Recently concluded at MSRI was a workshop on geometric representation theory, with lots of interesting talks, videos available here.

Update: There is supposed to be video from Ferrara here, but not working. A press conference was held, but the only thing I see from Planck online is in French here. Nothing about primordial gravitational waves, just confirmation of the standard cosmological model and of 3 neutrinos.

Update: No public release of any numbers from Planck that I can see, although they are being discussed in Ferrara. People are pointing out that the authoritative source for the best values of cosmological parameters at the moment is Twitter.

Update: Peter Coles has this take on the Ferrara Planck results: "a bit of a farce." Among the many oddities here, it seems that only the French component of Planck is putting out any news to the public.

Update: Adrian Cho at Science has a report about the latest Planck results here. Speculation is that Planck data alone and their joint analysis with BICEP2 will not see a gravitational wave signal, just set an upper limit. Planck is supposed to release papers Dec. 22, unclear if this will include the analysis with BICEP2.

Comments

1. **Arkadaş**
   November 29, 2014

   On the Loeb & Lee Lectures page that you link to, there is also a link to a talk by Glauber titled Recollections of Los Alamos and the Nuclear Era.

2. **David Appell**
   November 29, 2014

   Hi Peter. I'm just curious — how much did you learn in your first QFT course by Glauber, compared to what you know today?

3. **Peter Woit**
   November 29, 2014

   David,
   Not very much, for one thing the class was way above my head (I was a sophomore…) so while I learned a lot in it, I missed even more. Given Glauber's interests, the course was aimed not at the hot topics of that moment (gauge theory, the standard model), but at other applications of qft, including condensed matter and quantum optics. I then went on to take QFT courses from Coleman and Weinberg, learning about Yang-Mills theory and the path integral method. It seemed to me that the lesson of the Standard Model was that path integrals were the way to go, much simpler conceptually than canonical field theory methods that Glauber was using (just pick a Lagrangian and start turning the crank…). I think I kind of dismissed much of the material of Glauber's course as old-fashioned and unnecessary.

   I've kept learning more and more about QFT over the years, and I've in recent years grown to appreciate canonical methods, that's mostly what's in the course I'm teaching. These provide a connection to representation theory that is much harder to see in the path integral. Nowadays my point of view is that we don't truly understand QFT, with the standard story in all the textbooks just one slice of much wider topic.

   So, taking Glauber's course was just a first step, there's a huge amount to learn from what we do about QFT, far beyond what is in typical courses and textbooks, and a huge amount we still don't know.

4. **Mark Hillery**
   November 30, 2014

   I never took a course from Roy Glauber, but, since I have worked in quantum optics, I have made extensive use of the techniques he developed. He is one of the founders of the field, and by determining which quantum states of the electromagnetic field correspond to its classical description, he was able to show when distinctly quantum effects come into play. His papers are masterpieces. Just this last summer at a conference three of us, Pierre Meystre, the editor of Physical Review Letters, Carl Caves, who has made numerous highly significant contributions to quantum optics and quantum information, and I were discussing how elegant Roy's papers are and how they can serve as models for younger physicists. Those of us who have worked in the field were delighted when Roy's contributions were recognized by his sharing the 2005 Nobel Prize in Physics.

5. **Doug McDonald**
   November 30, 2014

I have no idea if Glauber was at Harvard when I was (1966-1970) (in the Chem. dept) but I did meet Ramsey a couple of times and visited his lab. My housemate worked for Klemperer, who did molecular beam electric resonance while Ramsey was a magnetic beam resonance type. The big deal (i.e. envy) was that Ramsey had a PDP-8 before Klemperer got an HP machine, and only slighter later did I get a PDP8-e for our beam scattering experiments. All those computers would be big big ticket memorabilia today.

But the physics story was that I sat in on (alone among chemists!) Schwinger's quantum mechanics class (not field theory). He delivered exceptionally lucid lectures that I, even from the start, even as a chemist (but I had already had the corresponding chemistry class, once at Rice, once at Harvard), had detected to be the "Chinese Lunch" of exposition. He could make you think, from the reasoning he delivered, that if YOU had been there 1920-1940, YOU could have replaced Schrodinger or Dirac (not so much Heisenberg) … in class … but five minutes later and little bit of thought, it all fell apart. The physicists actually taking the class eventually realized this too.

I'd love to find out if any people who later became famous were in that room.

A tidbit question: what did Glashow's license plate read?

6. **Brathmore**
   November 30, 2014

   Peter,

   You took QFT as a sophomore, and QM as a freshman (along, presumably, with Math 55)? I'm astounded. I suppose you must have taken several college level courses while in high school to pull that off! Wow.

7. **Peter Woit**
   November 30, 2014

   Brathmore,
   I'd studied multivariable calculus and physics on my own in high school, so QM and Math 55 were courses I was prepared for (and did fine in). Taking QFT as a sophomore was something a responsible advisor would probably have stopped me from doing. Luckily I was assigned Glashow…
   But, for just about anybody, I'd argue two QFT classes at least are what it takes to get any handle at all on the subject.

8. **Aaron**
   December 1, 2014

   Do you remember what text you used for the multivariable calculus?

9. **Peter Woit**
   December 1, 2014

   Aaron,
   Funny, but I realized I still have that book, it is a Calculus book by Hocking
   http://www.amazon.com/Calculus-introduction-linear-algebra-Hocking/dp/0030779103
   I don't remember how I got it, I don't think there was any good reason for that book in particular, and I don't recommend it, there are lots of similar and better books. Looking at it now I see it doesn't include some vector calculus (e.g. Stokes theorem), which I guess I must have learned elsewhere (E and M?).

10. **paddy**
    December 1, 2014

    Peter,
    As best as I can remember, I must have had Coleman 75-76 ish for QFT immediately before Glauber took over for your years. I do appreciate your point: at that young age the more times I was exposed to the same subject alternately constructed the more I learned.

11. **Bob**
    December 2, 2014

    Does anyone know if Roy Glauber is related to Robert Glauber, currently at Harvard's Kennedy School?

12. **Bernhard**
    December 4, 2014

    The video session (which was really good) with Roy Glauber is now available here:

    https://cdsweb.cern.ch/record/1973615

    I actually managed to download to a mp4 file (no link available, but if you know how to do it for youtube, it's the same principle here).

13. **Surjit Singh**
    December 8, 2014

    Does anyone here have any opinion on this controversy here?
    http://en.wikipedia.org/wiki/E._C._George_Sudarshan#Controversy_regarding_Nobel_Prize

14. **Shantanu**
    December 9, 2014

    Surjit: Sudarshan's case was discussed on this blog a while back.
    http://www.math.columbia.edu/~woit/wordpress/?p=311&cpage=2
    Not sure what became afterwards of these petitions etc. There are very few people who read this blog who have expertise in this field to confirm or refute this.

The Multiverse in a Nutshell

December 1, 2014
Categories: Multiverse Mania

The Guardian has a podcast up today featuring Robert Trotta and David Wallace called The Multiverse in a Nutshell. It's largely more of the usual uncritical multiverse hype that has been flooding the public expositions of fundamental physics for years now. Trotta gives the usual promotion of the cosmological multiverse, with no indication there is any problem with it. He assures us that this is being tested (by looking for "bruises" in CMB collisions). As far as I can tell, the Planck results released today, like all CMB data, show no evidence for anything like this. It appears that the Planck people don't even think this is worth mentioning. The public channels used for this hype will never report the fact that there's nothing there, instead they will just endlessly talk about this as something "scientists are looking for."

Wallace talks about something completely different, many-worlds, with nobody telling listeners that this has nothing at all to do with the cosmological material. Instead we're told that it's all related, because "most theories" "tell us there must be a multiverse". When challenged about the splitting universe business in QM, Wallace admits that at the fundamental level there is no splitting, there's just one theory and one universe, that "many worlds" is just a way of talking about the emergent behavior of the classical approximation. His book about this, The Emergent Multiverse, is quite good and makes clear what the "Multiverse" there really is. It's a real shame that he chooses to involve himself in this kind of attempt to muddy the waters and promote pseudo-science to the public.

Thankfully at least the physics community has one physicist trying to do something about this nonsense: Paul Steinhardt. In an interview with John Horgan, here's his "Multiverse in a Nutshell":

> Unfortunately, what has happened since is that all attempts to resolve the multiverse problem have failed and, in the process, it has become clear that the problem is much stickier than originally imagined. In fact, at this point, some proponents of inflation have suggested that there can be no solution. We should cease bothering to look for one. Instead, we should simply take inflation and the multiverse as fact and accept the notion that the features of the observable universe are accidental: consequences of living in this particular region of the multiverse rather than another.
>
> To me, the accidental universe idea is scientifically meaningless because it explains nothing and predicts nothing. Also, it misses the most salient fact we have learned about large-scale structure of the universe: its extraordinary simplicity when averaged over large scales. In order to explain the one simple universe we can see, the inflationary multiverse and accidental universe hypotheses posit an infinite variety of universes with arbitrary amounts of complexity that we cannot see. Variations on the accidental universe, such as those employing the anthropic principle, do nothing to help the situation.
>
> Scientific ideas should be simple, explanatory, predictive. The inflationary multiverse as currently understood appears to have none of those properties.

Comments

1. **Travis**
   December 1, 2014

   I don't understand this type of objection to the multiverse. It's entirely plausible that many features of the universe are in fact accidental, in exactly the same way that many features of earth and our solar system are accidental. Imagine for a moment that our solar system were completely surrounded by dust which made it completely impossible to see any other stars (for the purpose of this thought experiment, the dust is some kind of super dust which is opaque to all wavelengths). Would we be warranted, based on our knowledge of physics, in concluding that it is extremely likely that there are other stars out there, with planets arranged around them in different orbits from those in our solar system? Or would we say that such a model of the universe "explains nothing and predicts nothing" and keep searching for some deep reason why the orbits of the planets in our solar system are exactly the way that they are? The reason we would go with the "many stars" model of the universe is that theories which makes testable predictions, like the theories of gravity and nuclear reactions, also predict that similar processes should happen elsewhere in the universe. Analogously, inflationary theory which makes testable predictions like B modes in the CMB also predicts that similar processes should happen elsewhere in the larger universe. Even if inflationary theory in particular turns out to be wrong, the idea that many features of our universe are accidental is no more unscientific than the idea that the exact positions of the planetary orbits are accidental.

2. **Peter Woit**
   December 1, 2014

   Travis,
   Sure, it's plausible that some features of what we think of as fundamental physics are historical accidents. It's also plausible that these are not accidents but choices of an all-powerful deity. Neither conjecture is science though until you come up with a conventional scientific test of the idea. Until then, no matter how much hype proponents put out, Steinhardt is right that the "idea is scientifically meaningless because it explains nothing and predicts nothing".

3. **Katya**
   December 1, 2014

   Peter-
   You wondered if Paul Steinhardt agreed with Edward Witten about the "vast amount of evidence for inflation in the last 20 years." Now you have your answer:

   "…none of the magnificent observations made over the last 30 years can be viewed as supporting inflation."
   and…
   "As just explained, it is not possible to find evidence to support or refute inflation because an inflationary multiverse includes patches with cosmic gravitational waves and without them."

   No one more credible on this topic.

4. **Justin**
   December 1, 2014

   Woit,

   If I'm not mistaken, don't you consider yourself a believer in the many worlds interpretation of quantum mechanics. If you really believe that the universe splits off into undetectable many worlds when a quantum measurement takes place, how are multiverse ideas more nonsensical?

5. **Peter Woit**
   December 1, 2014

Justin,
No, I'm not a believer in many worlds. I've written a bit about QM interpretations, someday should write more about many worlds. As I tried to refer to in the posting, I think I mostly agree with Wallace: fundamentally there's only one world, no splitting, and quantum mechanics is exact. Classical behavior emerges in some limits from quantum, and if you try and say what is happening in classical terms you can do this using the "many worlds" language if you want. But this is in many ways a misleading thing to do, it just completely ignores the actual problem (how is classical behavior emerging from quantum mechanics?). If you try and take it seriously you get all sorts of problems.

So, I'm just not particularly opposed to people using "many worlds" language if they want for some purposes, but they need to make clear what they're doing when they do this, that there really is just one universe and fundamentally no splitting. Unfortunately many worlds fans mislead people about this, and then, even worse, go on to promote this misleading version of many worlds as related to empty pseudo-science about the string landscape, etc.

6. **Justin**
December 1, 2014

@ Woit,

Thanks for clarifying your viewpoint on quantum mechanics. Indeed the "language" of many worlds can get confusing. I highly recommend you take a look at the book Consistent Quantum Theory by Robert Griffiths. It's available for free in pdf format easily found online. It's very much in tune with your views on quantum mechanics.

Keep up the good work on this multiverse nonsense!

7. **GM**
December 1, 2014

@ Travis

That analogy is not really very good. The features of a particular planetary system may be "accidental", but they are not completely random, they are what they are because of simple and understood laws of physics, even if the process is extremely complicated . And it's worth remembering that historically we actually made a lot of progress towards understanding those laws by studying such accidents.

8. **amirpouyan**
December 2, 2014

I have two questions about many-worlds interpretation:

1)when one uses an ensemble of a system to study it in statistical mechanics, there are two ways he can interpret his work:

a) he can claim that every single picture in the ensemble(with millions of different scenarios) actually exists, or
b) he can say that the different scenarios in ensemble only exists in "his mind"

I think the question of importance is that is there any "experimental" distinction between these two views? (somehow i guess that the -b- interpretation sounds better!)

2) doesn't path integral formulation of QM support the idea that the different universes are somehow real? (because it seems that they interfere with each other in the integration process!)

I also found the "many-worlds interpretation" section of this post by R. F. Streater very interesting,

9. **David Metzler**
December 2, 2014

I second Justin's comment about Griffiths' book. Not wanting to push you too hard, Peter, to explain in depth your views on interpretation of QM, but I would be curious about your take on the Consistent Histories approach. I find it intriguing.

10. **howie**
December 2, 2014

The problem Steinhardt has i think is that he needs to construct an alternative to inflation if his attack is going to be fruitful.
But his alternatives are basically cyclic universes, in the case of the Ekpyrotic model embeded in a higher dimensional space. so its hard to see how they are not just multiverses in time rather than in space.
Interestingly other string cosmologies seem like they might be simpler, have you seen them discussed in this video? some claims of potential tests as well:

http://www.youtube.com/watch?v=Do4HGjmjobw

11. **Mr MonoPole**
December 2, 2014

"Science and hypothesis", by Henri Poincare, foreword by Larmor. Some good definitions of what science is and what is not. As one may expect – common sense and relation to the doable experiments characterize Poincare views. He does not mention universe at all.

12. **Peter Woit**
December 2, 2014

David,
I haven't paid much attention to the "Consistent Histories" business. From the time I've spent thinking about this, the whole "interpretation" issue increasingly seems to me besides the point. The interesting question is that of really understanding what happens in a "measurement", how classical emerges from quantum, and what the limitations of that approximation (classical, isolated system) are. Without engaging with those questions I don't see how one addresses the question of what is really paradoxical (as opposed to an artifact of one's over-simplified model for measurement, one's insistence on trying to isolate a system from apparatus and environment), and what if anything is missing from the standard formalism of states and unitary time evolution in order to understand measurements.

13. **Neil Bates**
December 2, 2014

Peter, first let's try to get MWI proponents' point right, then I'll say why I don't agree with them. They say that the wave equation continues to evolve. That means, literally no reduction to exclusive outcomes. Yes they say *that* all "happens in one world", but we can only see the events relative to our own entangled branch. In that sense, the unobserved possible outcomes are literally “other worlds”. Yet the branching structure of the continued superpositions contradicts the observed Born probabilities. The attempts to get actual correct frequentist proportions of events to fit the BPs, are basically circular or contrived arguments. IMHO it just doesn’t work. (See the many critical comments at e.g. Sean Carroll’s blog posts on the subject.)

OTOH if we reject that, we are stuck with a special “collapse” happening to the wavefunction. I myself think that interactions can make that happen, it’s not about “observers”, but no one can really resolve that yet. The “quantum measurement paradox” is still unsolved.

14. **Unemployed**
December 2, 2014

@DavidMetzler, @Justin:
I was about to follow your recommendations and download Griffiths’ book, but then I saw he’s also written stuff about QM&Theology. Unfortunately, time is finite, and I don’t want to waste it on god talk. Is his book that you recommended pure science, or is there any religious stuff hidden in it?
Thanks.

15. **David**
December 2, 2014

Unemployed,
It’s always possible that Griffiths’ book has stuff in there that you won’t agree with (some kind of philosophy you won’t agree with, or some kind of religious stuff you won’t agree with), but just skip over it. What’s wrong with that? Are you worried that you won’t be able to tell what’s science and what’s religion or “god talk”? It’s a free pdf, so just skip over the stuff you don’t like.

16. **Tim May**
December 3, 2014

Unemployed,
There’s nothing mystical or religious in the Griffiths book. The philosophy mentioned is not central and is mostly of the nature of mathematical theories versus actual physical reality. (Think of the point that planets and people don’t move by computing differential equations, yet Nature does indeed follow the results quite closely. Then extend this point to Hilbert spaces, tensor projections, and all that stuff. Griffiths does conclude that just as Newtonian mechanics appears to be much closer to reality than pre-Newtonian mechanics and the quantum mechanics is even closer to reality, from vast numbers of experiments carried out to very high precision.

Griffiths does a good job of gathering together the arguments of many in this area.

No religion that I could find, except for one line thanking his parents.

BTW, very little said about Many Worlds, and what there was was uncontroversial.

–Tim May

17. **David Metzler**
December 3, 2014

Peter, I agree with you on what the main problem is. However I do think that some interpretations of QM (e.g., for me, the Copenhagen interpretation) make it harder, psychologically, to think in productive ways about measurement, decoherence, etc. So I see an alternative interpretation as a psychological device that may possibly help get to the bottom of the key problem you mention, namely how classical emerges from quantum. The Consistent Histories approach seems particularly well suited for that purpose in some ways. (For me, MWI does not help at all, though likely that’s my problem.)

Unemployed, as Tim May says, there’s no religious material in Griffiths’ book. And if you skim some of the detailed examples on first reading, you can get a sense of the CH approach pretty quickly.

18. **Curious Mayhem**
December 3, 2014

We should be grateful to the all-powerful deity–or whatever arranged things to be just so–for Paul Steinhardt and the few others like him, like Peter.

If primordial gravitational waves are detected–and they could be soon–then inflationary theories move into firmly scientific territory. But the multiverse proper will remain a fever swamp of insanity.

19. **al**
December 3, 2014

Opening the new Smolin-Unger book, on finds it on page2 of the preface, in a nutshell:
” There is only one universe at a time, with the qualifications that we discuss. The most important thing about the natural world is that it is what it is and not something else. This idea contradicts the notion of a multiverse – of a plurality of simultaneously existing universes – which has sometimes been used to disguise certain explanatory failures of contemporary physics as explanatory successes.”

20. **Mark Thomas**
December 3, 2014

Marco Gleiser has a short commetary titled ‘A Quest for the Unattainable Unification of Knowledge” on E. O. Wilson’s new book “The Meaning of Human Existence” see link http://radio.wpsu.org/post/quest-unattainable-unification-knowledge
It is good to know that there are some who still believe in the faith of Einstein that there are Unities of knowledge and explanatory power in such unifications (in the Sciences). I hope we do not look at the likes of E. O. Wilson someday and say he was a relic of the 20th century and what he has to say was or is wishful thinking and that such is quaint. By God I hope it does not come to that.

21. **Katya**
December 5, 2014

Curious Mayhem-
Re detection of primordial gravitational waves; Professor Steinhardt’s comments are beautifully simple:

“…it is not possible to find evidence to support or refute inflation because an inflationary multiverse includes patches with cosmic gravitational waves and without them.”
and therefore,

"…even if we detect primordial gravitational waves, we should not rush to the conclusion that they are due to inflation. Better theories may come along that avoid the pitfalls of inflation and that nevertheless predict gravitational waves."

22. **amirpouyan**
December 6, 2014

"A different way of conceiving of this unimaginably immense proliferation (Multiverse idea) is to locate it as happening not externally to the cosmos but internally to the mind/brain states of observers. Making that move is a turn from a manyworlds interpretation to a many-minds interpretation, but this scarcely serves to mitigate the prodigality of the proposal … for many of us it still remains a metaphysical steam hammer brought in to crack an admittedly tough quantum nut" John Polkinghorne, Quantum Theory: A Very Short Introduction,p:53,

note that the book is highly praised by Chris Isham ,

23. **Andrew**
December 6, 2014

Katya

Steinhardt criticizes inflation, because if this theory is confirmed, then his own work on the cyclic universe will remain useless. It looks like a conflict of interest.

24. **Tom**
December 6, 2014

@Andrew,
When the initial Bicep2 (now apparently faulty) results were released, I recall some interview w/ Steinhardt where he seeemed very accepting of the possibility that his cyclic universe hypothesis could have been falsified. He wasnt trying to handwave and make a torturous, convoluted defense.
While he probably feels vindicated in light of Bicep2 non-results, had the Bicep2 results been corroborated, I didnt sense that he would go to his grave trying to defend cyclic cosmologies.

I dont know the detailed chronological sequence, but was Steinhardt critical of inflation before he started advocating cyclic cosmology?

25. **S**
December 7, 2014

Correct me if I'm wrong, but Steinhardt might well stand to win a Nobel prize if inflation is verified. Before he became a major critic of it, he helped develop the theory.

In any event, it doesn't seem fair to me to call it a "conflict of interest" when a scientist with one theory critiques a competing theory. Presumably his criticisms might be part of what motivated him to develop another theory in the first place. Conflicts might arise in refereeing or the like, but I don't think the accusation should be raised just for simple public criticism.

26. **Andrew**
December 7, 2014

No, if inflation is confirmed (I'm pretty sure) Steinhardt did not receive the Nobel Prize. I mean, Steinhardt and Turok gloatingly respond to failure of Bicep2 team. It's No Good. P.S. Sorry for my English.

27. **howie**
December 8, 2014

The Nobel is limited to three persons.
If inflation were to be recognised , Guth is the stand out name. But you are right the theory was developed by others as well. Linde, Starobinsky, Albrecht and Steinhardt would be the next in line to be considered.
Steinhardt would be just as entitled to share the prize as any of the others but given his vocal opposition its unlikely he would be picked , although it would be very amusing.
However without a red tilted gravity wave spectrum i doubt inflaiton will be handed any nobels.
Steinhardt developed the cyclic cosmology as an alternative to inflation as I understand becuase he was not happy with the implications of inflation (i.e a multiverse). i dont think that's a conflict of interest, scientists have to follow what they think is the most promising leads.

28. **Abraham**
December 8, 2014

This week's Guardian Science has a decent counterpoint and discussion by Ungar and Smolin:

http://www.theguardian.com/science/audio/2014/dec/08/cosmology-robert-mangabeira-unger-universe-time

29. **Curious Mayhem**
December 17, 2014

Katya,

The point you make about there being other explanations for cosmic gravitational waves is a good one. Their detection alone doesn't prove inflation.

But they are a distinctive predictive of inflation, and along with the classic problems that inflation solves, their detection would be strong evidence in favor of it.

Very Short Items

December 9, 2014
Categories: Uncategorized

- Long-awaited results from the Planck experiment were unveiled last week, with a new model for how to do this: hold a conference with no videos, no slides released, no wifi in the lecture hall, and put out a press release in French. They did release an amazing image that looks like a Van Gogh, but if you want numbers, you have to search Twitter. Stories at Nature and the New York Times indicate not much new, data relevant to primordial gravitational waves still to come (Dec. 22?, next year?).
- News from MIT is that, after an investigation of charges of sexual harassment of one or more students online, the university has revoked physics professor Walter Lewin's emeritus status and is removing his lecture videos and course material from their online course sites.
- Today's Wall Street Journal has a sensible piece by Ira Rothstein on The Perils of Romanticizing Physics.
- I took a look at Kip Thorne's *The Science of Interstellar* in a local bookstore. It gives a detailed explanation of the "science" behind the film, explaining what a lot of the highly confusing later plot of the film was supposedly about. It seems it's all based on the "large extra dimension" business of 15 years ago, the dimensions that were supposed to show up at the LHC. If you want to see all the equations, go here and look for the pictures of the blackboards.
- The LHC magnets are now getting trained for 6.5 TeV operation, as well as inspiring fashion designers.
- I mentioned last year's Gelfand Centennial conference at MIT here, thought that there were no videos. Luckily I was wrong, quite a few talks well worth watching are now available here.
- Videos from the Breakthrough Prize symposia recently held at Stanford are now available. For the physics talks, see here. For the math talks, see here, here, here, here, here and here.
- If you need a change of pace, and can't get enough of the string theory/LQG debate, this is for you.
- For talks about the implications of not seeing new physics at the LHC, there's Naturalness 2014. Nima Arkani-Hamed kicked it off with "Hopefully My Last Ever Talk On This!" (seems unlikely…). He argues for the idea that it's the Multiverse that did it, and we should keep looking for Split SUSY, disses "conformality" approaches. Matt Strassler on the other hand points out that Arkani-Hamed's use of the multiverse explanation doesn't make sense (there's no anthropic reason for the highly non-generic SM).
- There's a workshop this week at Caltech on scattering amplitudes and the Grassmanian.
- Finally, a HEPAP meeting this week. Budget news for HEP theory doesn't look good, but on the other hand the US now seems to be functioning without a budget, so it's kind of hard to be sure…

Update: Slides from the Planck conference are now available.

It seems the US does have a budget now, some info here. DOE HEP and Cosmic Frontier $766 Million, down very slightly from last year's $775 million, higher than the White House requeset of $744 million.

Update: Scott Aaronson on Walter Lewin here.

Comments

1. **Too Distinguished**
   December 9, 2014

   Peter,
   Can you unpack Matt Strassler's argument a bit?
   Thanks.

2. **Peter Woit**
   December 9, 2014

   Too distinguished,
   I'm referring to the last few slides of the set that I linked to, which are Strassler's. I don't want to do more to put words in his mouth, but he explicitly refers to Arkani-Hamed (and Dimopoulos), and seems to just be making the obvious argument that the SM is highly non-generic, in all sorts of ways that have nothing to do with the possibility of life. So, it seems to me that arguing that the Multiverse solves the naturalness problem is nonsense since that argument says that we live in some generic universe, just constrained by anthropics, and that isn't the case.

3. **Too Distinguished**
   December 9, 2014

   Thanks Peter. It seems like there are two ways to think about this. In one picture of the multiverse, the Higgs mass is the only tunable parameter in an otherwise fixed symmetric structure (our non-generic Standard Model), whereas in the picture Strassler seems to be considering, literally anything could happen. Is that a fair assessment, and if so, do you know of any good arguments made by NAH or anyone else in favor of the first picture?

4. **Peter Woit**
   December 9, 2014

   Too distinguished,
   I don't know of any arguments from Arkani-Hamed or anyone else that would justify starting by assuming that the multiverse gives a highly non-generic thing like the SM. You can do this, but it seems to me (and I think this is Strassler's point) that this is just internally inconsistent.

5. **martibal**
   December 9, 2014

   What's wrong with a press release in french ? 🙂
   Actually there is at least one number, an upper bound on the sum of the masses of the neutrinos: 0,23eV

6. **Peter Woit**
   December 9, 2014

   martibal,
   Who said there was anything wrong with just putting out press releases in French? Not me.

7. **Ivan**
December 9, 2014

“The LHC magnets are now getting trained for 6.5 GeV operation”.
Isn’t it TeV?

8. **Peter Woit**
December 9, 2014

Ivan,
Thanks. Fixed.

9. **Yatima**
December 10, 2014

News from MIT is that, after an investigation of charges of sexual harassment of one or more students online, the university has revoked physics professor Walter Lewin’s emeritus status and is removing his lecture videos and course material from their online course sites.

I can’t think of many regions in the Multiverse parameter space where such a reaction makes any sense, except the ones affected with dangerously high masses of Policial Correctness and Nether Regional Covering. As the press release says, “[Lewin] last taught an online MITx course in fall 2013.” Are the videos going to harass students all by themselves? Will the course material morph into sexually explicit material, burning the minds of the young ones forever? Sadly, MIT has not been very great in the backbone department this decade.

10. **Wayne**
December 10, 2014

Yatima — one presumes that the OCW videos and associated course material (textbook readings, problem assignments and solutions, and so forth) contained well-hidden subliminal messages of evil intent, only now discovered.

Were that not been the case it would be impossible to conceive of a justification MIT could have used as a basis to purge everything associated with Walter Lewin’s immensely popular 8.01, 8.02 and 8.03 courses — the first of which was recorded in 1999! — on elementary physics.

I suppose we should be relieved that high-school and college students, and legions of adult learners will now be protected from .. well, it’s hard to imagine from *what* exactly, but no doubt MIT should be encouraged in its campaign to make Lewin not merely a non-emeritus former professor, but a non-person as well.

11. **Too Distinguished**
December 10, 2014

Peter,
One more question if you would be so kind.

It seems to me that a critic of the multiverse idea would want to argue that the SM is generic, in the sense that it “couldn’t be any other way.” If it could be other ways — if there is nothing fundamental or inevitable about its symmetries — then one is led inexorably to the question of why it is this way, and from there, to anthropic selection in a landscape. By calling the SM non-generic, doesn’t a multiverse opponent get him- or herself in a bind?

Thanks again.

12. **Peter Woit**
December 10, 2014

Too Distinguished,
By “generic” one usually means indistinguishable from others, the opposite of “it couldn’t be any other way.
Yes, one is led inexorably to the question of why the SM is the way it is, but “because anthropics” is a non-answer.

13. **Shantanu**
December 11, 2014

Peter, something OT.
Watched this CFA talk on fast radio bursts by AVi Loeb
https://www.youtube.com/watch?v=aHVx6FCHsCg
and towards 1:10:43 (near the end) he shows an email from Freeman Dyson.
shantanu

14. **Peter Woit**
December 11, 2014

Thanks Shantanu,
To save others some trouble, here’s a quote from the e-mail from Dyson that Loeb was showing:
“I was lucky to grow up at a time when students had no respect for elder statesmen. The elder statesmen at that time, Heisenberg and Dirac and Born and Schrodinger and Yukawa and Einstein, were all pursuing fantasies that were obviously going nowhere. So we ignored the elder statesmen and went ahead using our own judgment. The students today should be doing that too.”

Maybe off-topic here, on-topic for the next posting…

15. **Arthur Reader**
December 21, 2014

The treatment of Walter Lewin by MIT is an academic disgrace and a clear assault on basic freedoms. No opportunity was afforded Lewin to defend himself in court before a judge and jury and no opportunity was given to face his accuser or cross-examine.

With spinelessness like that, who will dare teach women STEM knowing full well that any personal comment in any context misinterpreted by anonymous women with borderline personalities will be grounds for peremptory dismissal, removal of all titles and your teaching career consigned to the memory hole?

Weinberg on the Desert, Seiberg on QFT

December 9, 2014
Categories: Uncategorized

Last week Steven Weinberg gave a Lee Historical Lecture at Harvard, entitled Glimpses of a World Within. There's a report on the talk at the Harvard Gazette.

In essence, Weinberg argues in the talk for an idea that first started to dominate thinking among HEP theorists nearly forty years ago, one that is sometimes called the "Desert Hypothesis". The idea is that by looking at what we know of the SM and gravity, you can find indications that the next level of unification takes place around the Planck scale, with no new physics over the many orders of magnitude between the scales we can observe and that scale, at least no new physics that will affect running of coupling constants for instance. The evidence Weinberg gives for this is three-fold (and very old by now):

- He describes listening to Politzer's first talk on asymptotic freedom in 1973, and quickly realizing that if the strong coupling decreases at short distances, at some scale it would become similar to the coupling for the other fundamental forces. In a 1974 paper with Georgi and Quinn this was made explicit, and he argues this is evidence for a GUT scale a bit below or around the Planck scale.
- He explains about the Planck scale, where gravity should be of similar strength to the other interactions. This idea is even older, well-known in the fifties I would guess.
- He refers to arguments (which he attributes to himself, Wilczek and Zee in 1977) for a Majorana neutrino mass that invoke a non-renormalizable term in the Lagrangian that would come from the GUT scale.

Weinberg sees these three hints as "strongly suggesting" that there is a fundamental GUT/Planck scale, and that's what will explain unification. Personally though, I don't see how three weak arguments add up to anything other than a weak argument. GUTs are now a forty-year old idea that never explained very much to start with, with their best feature that they were testable since they generally predicted observable proton decay (which we haven't seen). We know nothing at all about the source of particle masses and mixing angles, or the reason for their very different scales, and there seems to be zero evidence for the mechanism Weinberg likes for getting small neutrino masses (including zero evidence that the masses are even Majorana). As for quantum gravity and the Planck scale, again, we really have no evidence at all. I just don't think he has any significant evidence for a desert up to a Planck unification scale, and this is now a very old idea, one that has been unfruitful in the extreme.

Weinberg ended his talk with another very old idea, that cosmology will somehow give us evidence about unification and GUT-scale physics. That also hasn't worked out, but Weinberg quotes the BICEP2 value of r as providing yet more evidence for the GUT scale (he gives it a 50/50 chance of being correct). Again though, one more weak piece of evidence, even if it holds up (which I'd give less than 50/50 odds for at this point…), is still weak evidence.

For a much more encouraging vision talk, I recommend listening to Nati Seiberg at the recent Breakthrough Prize symposium. Seiberg's talk was entitled What is QFT?, and to the claim that QFT is something understood, he responds "I really, really disagree". His point of view is that we are missing some fundamental insights into the subject, that QFT likely needs to be reformulated, that there exists some better and more insightful way of thinking about it than our current conventional wisdom. In particular, there seems to be more to QFT than just picking a Lagrangian and applying standard techniques (for one thing, there are QFTs with no known Lagrangian). Seiberg takes the fact that mathematicians (who he describes a "much smarter than most quantum field theorists"…) have not been able to come up with a satisfactory rigorous version of QFT to indicate not that this is a boring technical problem, but that we don't have the right definition to work with.

To make things more specific, he describes joint recent work (for another version of this see here) on "Generalized Global Symmetries" that works with global symmetries associated to higher co-dimension spaces than the usual codimension one case of Noether symmetries and Lagrangian field theory. Evidently there's a forthcoming paper with more details. I'm in complete agreement with him that there must be better ways of thinking about QFT, and I think these will involve some deeper insights into the role of symmetries in the subject.

Update: The paper Seiberg mentions is now available here.

Comments

1. **West**
   December 9, 2014

   I continue to be amazed at the comments of those who hope-beyond-hope that the BICEP2 claim of r=0.2 will somehow survive the full results from Planck. I'd gladly take those 1:1 betting odds. It really requires some serious prior confidence in "large r" being true to get that odds in the face of a serious galactic dust problem.

   Or maybe 50/50 is just a "pull a ratio out of the air to still seem confident/hopeful but not overly so." Maybe someone should ask where that conjectured odds comes from next time.

2. **Nobody**
   December 9, 2014

   " Maybe someone should ask where that conjectured odds comes from next time."

   50/50 means the same thing as "I haven't got a clue".

3. **Peter Morgan**
   December 9, 2014

   I don't see any discussion of regularization/renormalization in Seiberg's talk, the Strings14 powerpoint of his that you link to, nor in the one paper I've looked at, arXiv:1401.0740v2 [hep-th] (cited on the first page of the powerpoint). Is there some magic way to use his methods to construct a mathematically well-defined interacting QFT that is so obvious that renormalization doesn't need to be discussed?

4. **Peter Woit**
   December 9, 2014

   Peter Morgan,
   Seiberg isn't claiming to solve the problem of rigorously constructing interacting QFTs. My take is he's making the claim that physicists understand less about QFTs than they think. One aspect of this is his suggestion that the reason for not having a rigorous construction of 4d interacting QFTs is that we're missing some important understanding of the problem. But you should take his general claims about QFT as an argument about our ignorance, that we don't know what QFT is, not as a claim that he knows what it is while others don't. These general arguments are given just as a motivation for the very specific research program he then describes, investigating new ideas about symmetries.

5. **West**

December 9, 2014

Nobody,
At least in probability theory, one does not quantify the large uncertainty in a parameter r ∈ [0, ∞) with the invocation of a coin flip. Thats what posterior distributions and credible intervals are for. But if we want to compare nested models with an odds ratio, you still need to specify your chosen prior odds ratio and compute a ratio of marginal likelihoods.

So if a physicist has no clue what a reasonable estimate of the true value for a parameter is, he or she should just say so in colloquial language. On the other hand, if that person wishes to invoke a probability or an odds ratio as a meaningful probabilistic statement, I think demanding the reasoning as to how that value was reached is perfectly acceptable.

6. **Nobody**
December 10, 2014

"What are the chances that the BICEP2 value of r is correct?"

"50/50"

You can't get any more uncertain than that.

It is colloquial language. It just sounds slightly better than "I don't know", invoking a sense of calculation "to still seem confident/hopeful but not overly so" (as you put it).

You don't think he calculated it do you? Was a range specified? If not, it's meaningless, except as a big "I don't know".

7. **Peter Woit**
December 10, 2014

Enough about Weinberg's "50/50". Listened to in context, he wasn't saying "no idea", but indicating a reasonable chance that BICEP2 was seeing something other than just dust, r some sizable fraction of the .2 initially claimed. He would like to claim this as yet more evidence for the GUT scale. We'll see what Planck says.

One thing I do know though: if BICEP2 is all dust, Weinberg won't count that as evidence against a GUT scale…

8. **Flavio**
December 10, 2014

Peter,

in your post you seem to make several different, but related points:

1) There is no evidence for a desert up to a high (GUT/Planck) scale.
2) There is no evidence that unification takes place at the GUT scale, and such a scale probably does not exist, because GUTs disagree with experiments.
3) There is no evidence that unification takes place at the Planck scale.
4) There is no evidence that there is a Planck scale at all.

Did I summarize your view correctly ? If not, which points do you disagree with?

9. **vmarko**
December 10, 2014

Flavio,

"4) There is no evidence that there is a Planck scale at all."

I'd say this is a misnomer. Planck scale exists by definition (Planck length, time, mass, etc.). What you probably wanted to say is something like "that there is no evidence for any scale where gravity has strength comparable to other forces", a hypothetical scale which may or may not coincide with the Planck scale.

HTH, 🙂
Marko

10. **Ru**
December 10, 2014

On thing I've never quite understood – why should the fact that coupling constants are of similar strength in a certain regime be anything more than coincidence? particularly as, as I understand it, with the unadorned SM the constants don't all cross over at the same point.

11. **chris**
December 10, 2014

Ru,

if there is an underlying symmetry group that is broken at a certain scale, then the remnants start out with the same coupling at that scale.

and the crossing becomes more precise if you have TeV scale superpartners (not that i am advocating them – just stating the standard rationale).

12. **Shantanu**
December 10, 2014

Peter, did someone in the audience ask the obvious questions you alluded to such as how come no proton decay has been seen and so on?
Or are people so brainwashed that they cannot see the emperor is not wearing clothes?

13. **Peter Morgan**
December 10, 2014

Peter, *if* one takes the ill-definedness as a starting point, as Seiberg seems to, then what about the Wightman axioms and/or the Haag-Kastler axioms might reasonably and usefully be changed seems IMO the question to ask, not the deep dive into more-exotic-but-apparently-just-ill-defined. It seems as if Seiberg is not really starting from where he thinks he is. Hey ho.

14. **Peter Woit**
December 10, 2014

Flavio,
Change all the "There is no evidence" to "There is only very weak to weak evidence" and I'd agree with all those statements. The problem here isn't that Weinberg has no argument or evidence, it's that the arguments are quite tenuous and weak. Given the lack of good ideas to pursue, it's reasonable to pursue weak arguments and see where they lead. The problem is that people have been doing this for forty years now with no success, which is a good reason to be suspicious of the arguments and well aware of why they are weak, instead of trying to push them the way Weinberg is doing, implying that they are stronger than they really are.

One problem with tenuous arguments is that it's then easy to explain away any failure of their predictions. While claiming vindication from BICEP2, if its result disappears, that's no problem. Another tenuous argument that Weinberg might have brought up 5 years ago if he was giving the same talk would have been that GUTs + no proton decay are evidence for SUSY (this is how Weinberg could have answered Shantanu's question if it had been asked), and that plus the "naturalness argument" implies LHC-scale SUSY breaking. Now that that hasn't worked out, this isn't considered a serious problem for the whole scenario, because the argument was always tenuous.

15. **Peter Woit**
December 10, 2014

Peter Morgan,
We've known for a long time that axiom sets like Wightman or Haag/Kastler aren't good enough (gauge symmetry, gravity, etc, etc). The problem is what is the right thing to replace them, and I think it's highly likely new ideas are needed.

16. **David**
December 10, 2014

Peter,

If, at a certain energy scale, the forces (except gravity) all have the same coupling strength, does it necessarily follow that they are all "unified" under one mathematical theory, as in the electroweak theory? Is there some kind of proof of this idea? Or is it possible that they can all have the same strength, yet still exist separately as the strong and electroweak forces? Is this discussed in QFT textbooks?
Thanks!

17. **Peter Woit**
December 10, 2014

David,
The fact that running coupling constants have the same strength at some energy in and of itself says nothing about unification. The argument is that it's evidence for unification, because in some GUT unification schemes you break the symmetry with Higgs fields, and the way this is done, there is an energy at which this breaking occurs and at which the coupling constants need to be the same. This actually depends on a mass of details, including how you define the coupling, how the Higgs breaking takes place, whether you choose a supersymmetric theory, what the scale of SUSY breaking is, other threshold effects, etc. etc. I wrote a little bit about this question in my book "Not Even Wrong". If you want to know exactly what is going on, you have to get into the details of how SUSY GUTs work.

This is one of the two main pieces of evidence for SUSY GUTs (the other is that the particles in a generation fit well into the spinor rep of SO(10)). Again, the problem is just that it's a weak piece of evidence.

18. **Flavio**
December 10, 2014

Peter,

thank you for your clarifications, which makes your point of view very interesting and different from many others. What then is your present opinion about the following statements:

5) Unification surely occurs between LHC scales and at the latest some distance scale larger / some energy scale lower than the Planck scale.

6) Something about our present understanding of QFT has to change between LHC scales and some distance scale larger / some energy scale lower than the Planck scale.

7) Speaking about distance scales smaller than the Planck scale (or about energy scales for single particles higher than the Planck scale) makes no sense.

Do you tend to agree or disagree?

19. **Peter Morgan**
December 10, 2014

> We've known for a long time that axiom sets like Wightman or Haag/Kastler aren't good enough (gauge symmetry, gravity, etc, etc). The problem is what is the right thing to replace them, and I think it's highly likely new ideas are needed.

I agree that we need some new idea(s), but there are many possible weakenings either of Wightman or of Haag/Kastler. We can consider relinquishing microcausality or Lorentz invariance (both often relinquished at small scales, including for string theory and noncommutative geometry approaches, but fairly constrained), introduce non-associativity (limited C*-algebraic/Hilbert space structure, problematic for interpretation, I can't see how it can work but maybe it's not impossible if there's non-associativity only at time-like separation), or introduce nonlinear structure instead of using distributions (for Haag/Kastler, no weak additivity, which is fairly often relinquished, but it's new insofar as it's not been concretely considered in the Wightman context, AFAIK, no definite constraints yet), or some other new modification. Do you just mean not a field theory and not an algebra of observables, which more-or-less cuts Wightman and Haag/Kastler off at their roots?

20. **Peter Woit**
December 10, 2014

Flavio,
I don't agree with any of those statements, because I don't agree with "surely occurs", "has to change", "makes no sense". All one can solidly say about these issues is that we don't actually have any solid arguments or evidence, so should keep an open mind and not convince ourselves that our weak arguments imply that we must follow their

logic.

21. **Peter Woit**
December 10, 2014

Peter Morgan,
I don't have specific alternate axioms in mind and I doubt Seiberg does either. I wish I did. If you want some vague personal speculation, a major motivation for the book I've been writing about QM (and some free QFT) is to see how far I can get recasting QM and QFT in terms of representation theory (for instance taking the enveloping algebra of a Lie algebra as one's algebra of operators). It's very clear though that this is insufficient, not capturing a lot of the mathematical structure one wants. It's very unclear what the right general framework is to capture the structures one finds in the Standard Model. So, to me at least, better axioms are a long range goal, not something for the near future. Before worrying about axioms I think you first need to find a deeper understanding of the mathematics behind the Standard Model, and that question still holds many mysteries.

22. **Flavio**
December 10, 2014

Peter,

thank you for your answer. I am intrigued that you do not agree with the statements 5, 6, and 7, as I have rarely heard about people not agreeing with them. Disagreeing with them means that you are open for the existence of scales smaller than the Planck length. Maybe that is indeed the way to go, advancing against the flow of the more usual arguments. All the best for your endeavours, and please let us know about your progress!

23. **guest123**
December 10, 2014

"there must be better ways of thinking about QFT" I agree. Someone compared QFT to a Rube Goldberg machine. One promising approach is the Amplituhedron. Scattering amplitudes are volumes of a geometric figure.

24. **Minimal action**
December 10, 2014

WHat exactly is a "QFT without a lagrangian"?
It is true that under certain symmetries (like conformal symmetry) and boundary conditions, you can calculate some or even all correlators without any lagrangian being necessary.

That said, if it also turns out that, whatever lagrangian you try in the classical level with this symmetry, the symmetry is broken by quantum corrections (CFT again!), it is not clear that you are talking about a "theory without a lagrangian" rather than "a symmetry impossible to realize in a quantum theory".

I guess one has to wait until the paper, but Some of the discussion around QFT seems a little bit vacuus.

25. **Peter Woit**
December 10, 2014

Minimal action,
The 6d N=(2,0) superconformal theory that is a hot topic this days is often given as an example of a theory with no Lagrangian.

26. **A String Theorist**
December 10, 2014

Minimal action,

Any "isolated" quantum field theory—that means, one with no continuous parameters—cannot admit a Lagrangian description. To understand why, look at the converse statement: any time you have a QFT with Lagrangian L, meaning that the correlation functions may be computed by the path integral of exp(i L), then nothing stops you from considering the correlation functions computed by exp(2 i L), or exp(g i L) more generally. Doing so will give you a family of correlation functions depending on the parameter g (except in the trivial case when L is free; then g can be trivially absorbed into the path integral measure and has no relevance). The (2,0) theory, and many other QFTs, have no continuous parameters, so their correlation functions cannot possibly be computed by the path integral of any Lagrangian.

27. **Martin**
December 11, 2014

Physicists like theories that work. That is hard enough. Mathematical rigor comes second, and is often useless anyway. E.g. sloppy use of Dirac delta functions often works well enough. You only improve the math if the theory does not work well enough, which is why AQFT is of no interest to most physicists. That is just pragmatism. Data are needed to show that current QFT theories are not good enough, only then progress may be made. Speculating about the Planck scale without having relevant data is for retired physicists.

28. **tristes_tigres**
December 11, 2014

What is the state of understanding of far less advanced theory – QED ? It was a long time ago that I took the class on it, and it may not have been very up-to-date at that time, so my question may be obsolete. But isn't renormalization in QED a bit of a crutch to compensate for lack of consistent theory?

29. **Thomas Larsson**
December 11, 2014

A String Theorist:
Is this correct? To me, a quantum theory (not necessarily a field theory) is fully defined by its set of all correlation functions, but couldn't there be a Lagrangian formulation anyway. E.g., the Ising model, i.e. the CFT with c=1/2, is an isolated theory, since the closest theories have c=0 or c=7/10. Nevertheless, it has a lattice formulation with a Hamiltonian (the real Ising model that Ising thought about), and can also be reached with epsilon expansion from phi^4 theory in 4D.

30. **A String Theorist**
December 11, 2014

Thomas,

The Ising model is isolated only as a CFT, not as a QFT. You can perturb the theory by adding an operator to the Lagrangian. It will no longer be a CFT, but its a fine QFT. And voila, the coefficient of that operator you've added is the continuous parameter.

31. **minimal action**
December 11, 2014

More to T.Larrsonś comment, if you know the topology and all correlation functions, you should be able to reconstruct Ln(Z), and hence the lagrangian.
The issue is, not all symmetries "imposed" are stable against quantum correction.
Naively I would say the same applies to the remark by `string theorist` about "isolated"theories, if any lagrangian changes the parameters continuisly, why do you talk about a "theory without a lagrangian" rather than just an inconsistent theory?

32. **Thomas Larsson**
December 11, 2014

String Theorist,
Now I feel stupid. The Ising model at a non-critical temperature is of course related to phi^4 theory without being conformal.

33. **Giotis**
December 11, 2014

You can find a strong reason why the 6d (2,0) SCFT cannot have an action, on the first two paragraphs of page 10 of the following paper by Witten:

http://arxiv.org/abs/0712.0157

There are also no go theorems saying that you can't have a deformation of a free Abelian 2-form gauge theory to a non Abelian one but the assumption is that you have a local action

See e.g.

http://arxiv.org/abs/hep-th/9909094

34. **GBrown**
December 13, 2014

"It's very unclear what the right general framework is to capture the structures one finds in the Standard Model."
Doesn't Connes' noncommutative geometry capture the structures satisfactorily ?

35. **Yatima**
December 13, 2014

@Martin

Physicists like theories that work. That is hard enough. Mathematical rigor comes second, and is often useless anyway.

I agree that rigor comes second, but it being useless is an inflammatory statement in the class of "I don't know what's going on but I can fib my way through, and I don't really care whether any of this makes sense."

Working on a mathematical object that is shown to imply ⊥ after a decade or so in a footnote that no-one reads? I'm against that. That is not a theory that "works" in any sense, shape or form.

36. **Peter Woit**
December 13, 2014

GBrown,
No. Noncommutative geometry ideas have been used to give a different formulation of QFT, but I don't think the results at this point convincingly explain any of the things we don't understand about the standard model.

See a later comment for more about this.

37. **Martin**
December 13, 2014

@Yatima: OK, I withdraw "useless". Sorry.

I meant: if a theory is approximate (effective) then mathematical rigor does not make it any better. See e.g. algebraic QFT – did this yield anything of use for actual particle physics? I have nothing against math, honest.

38. **Peter Woit**
December 13, 2014

Martin,
Seiberg's point was not that rigor matters, but that the failure of mathematicians to develop rigorous versions is a hint that something is missing, something that could lead to important new insights if you found it.

One thing you learn when doing mathematics is that sometimes when you can't find a proof of a statement that you think you understand well and has to be true, it's not because you're not clever enough to find the proof, it's because you were misunderstanding the situation, and the statement was actually not true.

39. **Martin**
December 13, 2014

Yes, Peter, I see that.

On the other hand, mathematical rigor does not imply physical correctness. I think that in some areas of theoretical physics (not in all!) math has overtaken physics, which is a sign of a crisis, at least from the physics point of view.

40. **Martin S.**
December 14, 2014

"*One thing you learn when doing mathematics …*"

@Peter: I seem to have a different experience (being a mathematician myself), at least when dealing with structures where you already have found an amount of stuff. When in such cases I encounter a situation of not being able to find a proof, I usually have to dig more deep into those structures. And then one reveals more precise properties of the studied structures, usually along with that searched proof (and why the proof could not have been found without taking into account those more precise properties).

41. **Martin S.**
December 14, 2014

"*I think that in some areas of theoretical physics (not in all!) math has overtaken physics, which is a sign of a crisis, at least from the physics point of view.*"

@Martin: Physics is, for sure, ultimately ruled by experiments. Still (if mathematics does exist underneath as I strongly expect), you want to have some theory that either directly describes the physical stuff, or at least it approximates that stuff. And you want to understand that math too, to not get lost in translation (as it apparently happened to those overtaken by the parts of mathematics that are irrelevant to physics).

42. **Peter Woit**
December 14, 2014

To expand a bit on my perhaps overly dismissive comment about NCG approaches to QFT and the Standard Model, a better way to describe my impression of the subject is that I'm intrigued, but not convinced. I should make clear that this is an "impression", based upon periodic attempts to learn a bit about the subject, which have not at all gone far enough to really understand what is going on. Such efforts have taken me just far enough to see what look like some interesting ideas, while not seeing something that, to me, convincingly explains what seem to me the big mysteries of unification.

Among the many things I haven't had a chance to look at, it has been pointed out to me that there are new papers out by Connes and Collaborators, see
http://arxiv.org/abs/1409.2471
and
http://arxiv.org/abs/1411.0977
as well as a blog posting about this by Connes here
http://noncommutativegeometry.blogspot.com/2014/11/particles-in-quantum-gravity.html
which anyone interested in the subject should take a look at.

I should also point out that while I'm not convinced by this, I'm quite convinced that standard GUT approaches don't work, better alternatives are few and far between, so this is one that deserves to be taken seriously.

43. **Lee Smolin**
December 14, 2014

Dear Peter,

You might look at a recent reformulation of the NCG formulation of the standard model by Boyle and Farnsworth: http://arxiv.org/abs/1408.5367.

Thanks,

Lee

44. **Peter Shor**
December 14, 2014

You say *"zero evidence that the [neutrino] masses are even Majorana"*. There's also zero evidence that the masses are Dirac. The only real evidence we have that the neutrino masses are Dirac is that the other fermions we've seen are Dirac.

And (since I don't think you can count the different generations as separate pieces of evidence) we've seen exactly three elementary Dirac fermions: the electron, the up quark, and the down quark. I don't think this is strong evidence that the neutrino isn't Majorana.

45. **martibal**
December 15, 2014

"Noncommutative geometry ideas have been used to give a different formulation of QFT, but I don't think the results at this point convincingly explain any of the things we don't understand about the standard model."

I would have rather permuted "QFT" and "Standard Model" in the above sentence:

NCG (in Connes sense) does not explain so much about QFT, in the sense that once obtained the Lagrangian of the Standard Model, in order to do physics one uses the usual tools of QFT (in particular the equation of the renormalization group, coming from usual approach to the SM). On the contrary, it gives explanations on several aspects of the SM that are rather ad-hoc otherwise:
– the Lagrangian, with the correct representations of particles;

– the need for a scalar field (which comes out on the same footing as the other gauge fields, as the "noncommutative" part of the connection 1-form);

– a constraint on the number of particles per generation (which should be 2 x (2 x n)^2 for n integer. The SM is n=2, that is (6 colored quarks + electron + neutrino) x 2 (left, right) x 2 (antiparticles) = 32;

– a relation between masses.

The gauge group itself has been "almost" derived from geometrical principle, up to an ad-hoc symplectic hypothesis. And it seems that in the last work of Connes and al you mention, this ad-hoc hypothesis can be removed.

46. **martibal**

December 15, 2014

One may say think this does not explain anything and this is just a way to rewrite in a complicated mathematics language things that one already knows. But one could say the same about GR (Weinberg does it in his book, at least an older version of it): “all the tensors with their funny way of transforming under Lorentz transformation can be given a geometrical interpretation, like curvature, but this is not relevant for physics”. [the quote is not exactly Weinberg, but there is a chapter in his book which says something like that].

In my opinion all the interest of GR is precisely its geometrical interpretation, which makes it much more that just a collection of rules of tensor transformations. If NCG manages to turn the SM into a geometrical theory, I would say it is quite relevant for physics.

Of course, there are other aspects of the SM one should explain which are not addressed in NCG, or not in a sufficiently convincing way. Peter, could you be more explicit on that point: in your opinion, what are the most important things we do not understand about the standard model ?

47. **Peter Woit**
December 15, 2014

martibal,
I wrote a chapter in my book (chapter 8) specifically about this, don’t want to reproduce it here, in any case it’s pretty obvious to anyone who spends much time studying QFT and the Standard Model what the list is of things it doesn’t explain.

As for why I don’t find the list you give of answers provided by NCG convincing, part of the problem is that these are post-dictions, so that raises the bar for how compelling they need to be.

48. **Jesper**
December 15, 2014

“… I think you first need to find a deeper understanding of the mathematics behind the Standard Model… ”

“As for why I don’t find the list you give of answers provided by NCG convincing, part of the problem is that these are post-dictions … ”

Well, if you find a deep mathematical understanding of the standard model it will by default be in the form of post-dictions.

49. **anon.**
December 15, 2014

“Well, if you find a deep mathematical understanding of the standard model it will by default be in the form of post-dictions.”

That’s like vacuously arguing that Schwinger’s prediction of the electron’s magnetic moment was a post-diction, because it had already been measured. A better mathematical understanding of the SM might allow a precise prediction of neutrino masses, the strong coupling, the Weinberg mixing angle, or the CKM weak interaction mixing angles matrix. Thus it could be a prediction fully testable by future inprovements in SM parameter measurements, not just an ad hoc post-diction.

50. **Peter Woit**
December 15, 2014

Jesper,
Sure. Nothing wrong with a good postdiction, they can be very convincing. But the bar is higher for such claims.

51. **Will**
December 15, 2014

I know nothing at all about non-commutative geometry, but martibal’s post seems to imply at least one prediction, which is the existence of three right-handed neutrinos. If the “relation between masses” include predictions of the neutrino masses (sterile or active), that would be even better. Does it make these predictions?

52. **Nobody**
December 15, 2014

If the new mathematics does not simplify, I would not call it “deep”, but “obtuse”. If the mathematics simplifies, it should postdict everything the old theory did, but why would it need to predict anything new to be considered a step in the right direction?

53. **martibal**
December 15, 2014

@Will: there was an older version of the model with massless neutrinos. NCG seems to be dead when neutrinos were discovered to have a mass. Some years after, it came out that some flexibility can be introduced by making the distinction between two notions of dimensions (the metric dimension, and the so called KO-dimension). In the commutative case, i.e. for a Riemannian manifold, both notions coincide with the usual dimension of the manifold. It was implicitly assumed that the same should be true in the noncommutative case.

At the same time, Connes and Barrett noticed independently that there was no reason to identify these two dimensions in the noncommutative case. By fixing the KO-dimension (which can take value from 0 to 7) to 6, one solves at the same time two problems: 1. The number of particles/generation is 2(2 x a)^2, hence the possibility to incorporate a massive neutrino; 2. One solves a problem of over-counting degrees of freedom in the fermonic action (“fermion doubling”).

The point is that NCG does not say anything about the number of generations. So with some contorsion one might built a model where the three generations do not have the same KO-dimension, meaning that one could have massless neutrinos (i.e. KO-dimension 0, as in the older version) for some generations, and massive neutrinos for other (i.e KO-dimension 6) for others. However I do not know if such weird possibilities have been checked.

54. **martibal**
December 15, 2014

The relation is between the sum of the mass of the fermions ans the mass of the W. Since the top is much heavier than the other fermions, in pratical the relation is between the mass of the top and the W.

55. **martibal**
December 15, 2014

Peter & Anon: a postdiction that the gauge group of the SM is U(1) x SU(2) x SU(3) would be a high level postdiction, or there already exists some justification to it ?

56. **Peter Woit**
December 16, 2014

Martibal,
SU(3)xSU(2)xU(1) is already a very simple mathematical structure. If you predicted this before it was known in terms of some other simple mathematical structure, that would be compelling. If you start already knowing this, claiming to derive it from some other "simpler" structure is going to be convincing only if the simpler structure is quite a bit simpler, and what I have understood of the NCG explanations doesn't to me convincingly do that. In any case, the real test I think is whether you can explain the more complicated aspects of the standard model in terms of the simpler structure you are advocating, it is that which would be convincing.

57. **martibal**
December 16, 2014

Peter,

to push a little bit the argument: the U(1) x SU(2) x SU(3) structure is simple, but in usual QFT nothing explains (or am I wrong ?) why it should stop here: why not … x SU(4) x SU(5) x… ?

The whole point of NCG is that (Euclidean) GR + SM are obtained from one single action, which is geometrical. In other terms the SM is viewed as "gravity" on the noncommutative part of space (time). So a postdiction would be that – assuming the geometrical structure of space(time) is not a manifold, but a manifold x noncommutative part – then there are only four interactions: gravity on the commutative part, the SM which has to be a U(1) x SU(2) x SU(3) gauge theory on the noncommutative part. But I agree this is not the most highly convincing postdiction.

Before I stop monopolizing the discussion: I do not think anybody working in the field is pretending that this is fully convincing. But as you said earlier, none of the other approaches to unification or quantum gravity seems much more convincing. Looking at the ratio produced results/attention received, I think NCG scores not too bad.

58. **Hans-Peter**
December 16, 2014

Martibal,

some months ago I have searched through arxiv, google scholar and the SCI about this topic; I found just one postdiction of U(1)xSU(2)xSU(3) from a simpler structure. And that postdiction is not taken seriously by anybody in the high energy physics community. Many have tried to relate the three gauge groups to the complex numbers, for U(1), the quaternions, for SU(2) and the octonions, for SU(3), but without success. Many other attempts failed and were not even published – ask your favourite high energy physicist. So the present situation is: There is no justification, as you call it, of the interaction gauge groups from a simpler structure.

Many are trying to deduce them uniquely from other, more general structures, dropping the requirement of simplicity, but still without success. (Uniqueness is the key here, of course.) Also Peter's work can be seen as an attempt in this direction, I guess. But no such postdiction or justification is available yet. Finding a postdiction seems to be easy at first sight, since the result is known, but in fact it is a surprisingly hard problem.

59. **Peter Woit**
December 16, 2014

martibal,

I definitely agree that NCG ideas are worthy of attention, especially because of the lack of good ideas about unification.

In your description though of unification by NCG, the problem is with the claim of deriving SU(3)xSU(2)xU(1) by just doing geometry on the "non-commutative part". The danger is that among many possibilities for the "non-commutative part", you have just picked the one that gives SU(3)xSU(2)xU(1). When I've been trying to read the NCG literature, this is a point at which I've found things unconvincing. Possibly I'm not reading the right explanation…

60. **David**
December 16, 2014

Didn't I read somewhere that NCG predicted the wrong mass for the Higgs? Did they do something about that?

61. **martibal**
December 17, 2014

Peter, well the hope is that precisely, on the noncommutative part, one does not have so much way to do geometry but the one that gives you the standard model. All depends on what you call "doing geometry" of course.

The starting point is the observation that all the information of a compact spin manifold M is encoded within a triple given by the commutative algebra C^infty(M) of smooth functions on M, the Hilbert space of spinors and the Dirac operator. Conversely, Connes worked out the conditions that must satisfy a commutative algebra A, an Hilbert space H and an operator D such that – given any triple (A, H, D) with A commutative – then there exists a spin manifold such that A = C^infty(M). Then one drops out the commutativity requirement, and define a "noncommutative geometry" as a spectral triple (A, H, D) where A does not need to be commutative.
Now the result is, assuming three things:
– the algebra is the product of C^infty(M) by a finite dimensional algebra A_F,
– the KO-dimension (see a post above) of the finite dimensional part is 6,
– there is an ad-hoc "symplectic hypothesis" to define a real form on A_F,
then A_F = C + H + M_3(C) (complex, quaternion, 3×3 matrices), which is precisely the algebra the spectral triple of the SM is built on. All this is in the paper "Why the standard model" by Chamseddine and Connes (arXiv: 0706.3688 [hep-th]). In their most recent work you have already mentionned, it seems they can get rid of the ad-hoc symplectic hypothesis.

So once you buy the definition of a spectral triple (which has a strong justification, since in the commutative case this is equivalent to the definition of a Riemannian spin manifold) one basically deduces the gauge group of the SM from the first two assumptions above.

62. **martibal**
December 17, 2014

Hans-Peter: thanks for the information.

David: from the beginning of the model (early 90′) there was a prediction for the Higgs mass around 170GeV. It has been ruled out in 2008 by Tevatron. After the discovery of the Higgs, Chamseddine and Connes noticed that if one takes into account an extra-scalar field (proposed in a completely different context by particle physicist to avoid the instability of the electroweak vacuum due to the “low mass” of the Higgs), then one can obtain the correct Higgs mass.

The question is then: why this extra field was not present before, since the claim is that the SM is almost uniquely derived from the NCG principles ?

It turns out that to justify this new field inside the NCG framework, one has to allow a little but of flexibility, regarding one of the conditions that defines a spectral triple (namely the one that tells you the operator D is a first-order differential operator). The present state of the art is that the extra-field (which violates the first-order condition) can be viewed as a small excitation around a vacuum (which does satisfies the first-order condition).

63. **Hans-Peter**
December 17, 2014

Martibal,

the key assumption in what you write about NCG is the KO-dimension 6 of the finite dimensional part. It indeed implies the three gauge groups – but does not imply them uniquely: if you took 7 or 8 or 9 as an assumption, the argument dissolves. Besides, the assumption is not simpler than the result, as required; it is just a hidden way to introduce the result in the argument right from the start. So it should not be counted as a justification of the standard model.

64. **Curious Mayhem**
December 17, 2014

Seiberg’s argument is very interesting and deserves closer attention. We don’t understand QFT and only have a “practical” or pragmatic understanding of how to calculate with it in certain cases. That’s not satisfactory. The situation becomes acute when gravity is brought into the picture and the spacetime background moves to the front of the stage and becomes part of the show. We need something background-independent — indeed, spacetime-independent — to make progress. Higher groups? Maybe. My pet cause is starting with field fluxes as fundamental. You can do that already, although it’s merely a curiosity in QFT as we know it now.

65. **martibal**
December 17, 2014

Hans Peter: as I said in a precedent post, the KO-dimension 6 also has a justification regarding the fermonic action. In fact it is justified in this way in Connes and al paper. Then one “noticed” that it allows to have massive neutrino. In Barret paper the KO-dimension 6 is justified because it corresponds to the Dirac operator with Minkovski signature. So the choice is far from arbitrary.

Even if you consider it as a free parameter, that one single parameter with only 8 possible values (from 0 to 7), fixes the gauge group of the SM, the number of particles per generation, their representation, the form of the Bosonic Lagrangian – including the Higgs – obtained together with Einstein-Hilbert action… well, I would say this is quite an interesting step towards an understanding of the mathematical structure of the SM.

66. **Hans-Peter**
December 21, 2014

Martibal,

Connes indeed achieved an interesting step. He discovered a structure that allows to tie the gauge groups together in a different way from that of GUTs. He showed that the three gauge groups are deeply related somehow. On the other hand, he also states that these ideas do not explain most parameters of the standard model, nor the number of generations. So we have to take the truth in these ideas and continue the search. But in which direction …?

67. **martibal**
December 21, 2014

Well, recently there have been several works by various groups on a new scalar field who comes out by relaxing the first-order condition (see a precedent post). This opens some possibility towards some Pati-Salam models, whose SM would be the vacuum. Hopefully some phenomenology could be done there.

At more fundamental level, there is also the idea to get rid of the assumption that space (time) is described by the product of a manifold by a finite dimensional space. In Connes and all last work there are very interesting things in that direction. The idea would be NOT to postulate from the beginning that there is a manifold (or say differently, NOT to start with an algebra with an infinite dimensional center and only a finite dimensional noncommutative part) but to be able to reconstruct everything from some “abstract” noncommutative relations.

The problem is not to find research projects, the problem is to have the possibility to work on them with a little bit of stability (meaning contracts longer than 1 or 2 years). But that is another story.

Defend the Integrity of Physics

December 17, 2014
Categories: Multiverse Mania

This week's Nature features a call to arms from George Ellis and Joe Silk, entitled [Scientific method: Defend the integrity of physics](). I'm very glad to see well-known physicists highlighting the serious problem for the credibility of science raised by the string theory multiverse and the associated ongoing campaign to justify the failures of string theory by attacking the scientific method. Acknowledging evidence that an idea you cherished doesn't work is at the core of what science is and physics now has a major problem with prominent theorists refusing to abide by this principle. Ellis and Silk do a great job of identifying and characterizing an important challenge the scientific community is facing.

The issue is however complicated, and while the Nature piece carefully and clearly addresses some of the complexities, there are places where things get over-simplified. In particular, the introduction frames the issue as whether a theory being "sufficiently elegant and explanatory" allows it to not need experimental testing. The problem with the string theory multiverse though is not this, since such a theory is the antithesis of "elegant and explanatory". There's just about nothing in science as inelegant as the various attempts (e.g. the KKLT mechanism) to make string theory fit with known physics, and "the multiverse did it" is no more an actual explanation of anything than "a big omnipotent turtle did it".

Trying to cut through the complexities, Ellis and Silk write:

> In our view, the issue boils down to clarifying one question: what potential observational or experimental evidence is there that would persuade you that the theory is wrong and lead you to abandoning it? If there is none, it is not a scientific theory.

This is at the heart of the matter, but there are subtleties. A common recent move among some prominent string theorists has been to argue that string theory is falsifiable: it is based on quantum mechanics, so if experiments falsify quantum mechanics, they falsify string theory. This just makes clear that the question of falsifiability can be slippery. Philosophers of science are experts at the intricacies of such questions and Ellis and Silk are right to call for help from them.

They also make the interesting call for the convening of a conference to address these issues. How such a thing would work and how it might be helpful seem well worth thinking about. As for one of their other recommendations though:

> In the meantime, journal editors and publishers could assign speculative work to other research categories — such as mathematical rather than physical cosmology — according to its potential testability.

I'm leery of the impulse among physicists to solve their problem of how to deal with bad physics by calling it mathematics. Yes, there is good mathematics that has come out of untestable ideas about string theory, but no, this doesn't include the string landscape/multiverse cop-out, which physicists need to face up to themselves.

For the specific arguments from Sean Carroll and Richard Dawid that Ellis and Silk address, I've written about them elsewhere, see for instance [here](), where I discussed in some detail Dawid's arguments.

Update: Sabine Hossenfelder has commentary on this [here]().

Update: Taking the opposite side of the argument in January's Smithsonian magazine is by colleague Brian Greene, with an article entitled [Is String Theory About to Unravel?](). As you might expect, Brian's answer is "No", and he gives a good account of the point of view Ellis and Silk are warning against. He mentions the possibility of encouraging news for string theory from the next LHC run, but says that "I now hold only modest hope that the theory will confront data during my lifetime."

Update: Sean Carroll responds to the criticism from Ellis and Silk with [a tweet]() characterizing them as belonging to the "falsifiability police":

> My real problem with the falsifiability police is: we don't get to demand ahead of time what kind of theory correctly describes the world.

Update: Gordon Kane joins the fight in a [comment at Nature](), claiming that, before the LHC, string theory predicted a gluino mass of 1.5 TeV.

> The literature contains clear and easily understood predictions published before LHC from compactified string theories that gluinos, for example, should have been too heavy to find in Run 1 but will be found in Run 2 (gluino mass of about 1.5 TeV).

As far as I can tell, this is utter nonsense, with Kane publicly claiming string theory predictions of a gluino mass of around 600 GeV (see page 22 of [this]()) back in 2011, then moving the "prediction" up as Run 1 data falsified his earlier predictions. Kane at least makes falsifiable predictions, the problem with him only comes when they get falsified…

Update: Chad Orzel has his take [here]().

Update: Adam Frank has an essay on this [here]().

Comments

1. **Bee**
   December 17, 2014

   I quite like the idea to rename it, this is an important issue when it comes to funding. It would make it abundantly clear what this is an what it isn't. This really is my biggest frustration when it comes to high energy theory, cosmology and quantum gravity today, that in my book much of it simply isn't physics. Jeez, have you recently looked at the hep-th arxiv? Most of it is in the best case mathematical physics, in the worst case philosophy, or maybe art. I kinda wish it was at least science fiction, then it would be more interesting.

   If you think that the mathematicians or philosophers don't want it either, then call it something else, but please don't let them block money and jobs that funding agencies mistakenly believe go into theoretical physics. (Yes, that's right, what I actually mean is give me a job and boot some string theorists.)

   Needless to say, I have made myself very unpopular among my colleagues with this opinion 😉

   I don't want anybody who really wants to work on string theory to stop – if it makes them happy, please. But don't sell it as something it is not. And I say this as somebody who has a paper in physics.hist-ph, so don't get me wrong, I do think philosophy is relevant and we'd even be better off paying more attention to what the philosophers are trying to tell us – sometimes at least.

2. **Peter Morgan**
   December 17, 2014

As always, thanks for the link and for your discussion. I commented there.

3. **Peter Woit**
December 17, 2014

Bee,
One of the odd sociological things I've noticed about the arxiv hep-th category is that it increasingly has large numbers of papers about quantum gravity/inflation/cosmology. What's odd is that I would have thought that the "Quantum Cosmology" in gr-qc was precisely intended to provide the right home for this topic. One interpretation is that people coming out of a string theory background don't want to be associated with those coming out a GR background…

4. **Douglas Sweetser**
December 17, 2014

I make it a practice to always refer to "work on strings" instead of the far, far more common label. One of the greatest disservices to the scientific community was the wide use of Theory label. The word Theory should be reserved for only be used for the best science has to offer: Newtonian mechanics, special relativity, general relativity, quantum mechanics, and the standard model are all physics theories. They each are logically coherent and make many predictions. I think we may have to replace some of these theories with better theories, but any upgrade must be coherent and have predictions.

Same number of characters, one more space: "work on strings" is my small way to defend the integrity of physics, and science in general.

5. **Bee**
December 17, 2014

Peter: Not sure about this. Annoyingly, almost all of these topics are cross-linked now. It's annoying because more often than not I have the same paper 3 times in my feed. At this point I myself am so confused which arxiv category to best post in, that I normally do the same – after somebody once asked me why I post in qg-qc "which nobody reads".

In any case, I have a longer comment on the Nature piece at Starts With A Bang. Thinking that maybe it's of interest for some of your readers, the link is here:

https://medium.com/starts-with-a-bang/does-the-scientific-method-need-revision-d7514e2598f3

6. **Bee**
December 17, 2014

gr-qc I meant, sorry.

7. **Peter Woit**
December 17, 2014

Bee,
Thanks for the link, I'll add that to the text of the posting.

I am looking at arXiv postings at the arXiv site, so the cross-posted ones come later. I do mean that what's odd is that articles on inflation, string cosmology, etc. are getting posted with hep-th as the primary category, cross-posted to gr-qc, not the reverse, which is what one would have naively expected.

8. **Martin**
December 17, 2014

"A common recent move among some prominent string theorists has been to argue that string theory is falsifiable: it is based on quantum mechanics, so if experiments falsify quantum mechanics, they falsify string theory".

This is not how science works. In real life there must be a reason to take a theory or model seriously, in particular non-trivial (good enough) confirmation by experiment. Science is about inferencing in the Bayesian way. The 1930 theme was that some statements cannot be verified whereas they can (in principle) be falsified. E.g. a statement like "there are no pink elefants" is not confirmed by never seeing one, it can be falsified by seeing one. That sort of logic.

In normal physics a theory is never deemed credible merely because it is potentially falsifiable. That may be a necessary quality of a theory, but it obviously has nothing to do with making the theory credible. A credible theory is a theory that WORKS. Why don't these "theorists" understand this? We could discuss what is meant with "work", but to pretend that everyting is fine as long as the possibility of falsifiability cannot be proved to be identically zero is a joke. I have never seen this type of argument in real physics such as solid state physics and theory, or in relativity theory, or in quantum mechanics. Why was quantum mechanics with the probability interpretation accepted? Because it worked. Why was special relativity accepted? Because it worked. Why does physics work with theories that are evidently not totally correct – but only approximate or effective? Because these theories work. In real physics nobody presents a theory without showing that it has at least some correspondance with experiment or observation. I do not understand why so many seem to believe that a mere theoretical possibility of falsification should make a theory credible to a reasonable extent. Falsifiability is merely a negative criterion: a model that cannot possibly be falsified cannot be scientific. Turning it into a positive criterion makes no sense.

String theory does not work. That is all. People come with "theoretical falsifiability" only because they have nothing else.

9. **Martin S.**
December 17, 2014

@Bee: I guess that the original source of one of the images at your commentary is AbstruseGoose. Sorry for putting it here, I am not social enough to leave message there.

10. **amirpouyan**
December 17, 2014

In his "The Road to Reality" , Penrose argues that supersymmetry has an unfalsifiable(un-Popperian) character because you can always argue that the superpartners can be detected with higher energies, then he asks:
"Does the 'un-Popperian' character of such models (like supersymmetry) make them unacceptable as scientific theories? I think that such a stringent Popperian judgement would be definitely too harsh. For an intriguing example, recall Dirac's argument that the mere existence of a single magnetic monople somewhere in the cosmos could provide an explanation for the fact that each particle in the universe has an electric charge that is an integral multiple of some fixed value (as is indeed observed). The theory which asserts that such a monopole exists somewhere is distinctly un-Popperian. That theory could be established by the discovery of such a particle, but it appears not to be refutable, as Popper's criterion would require; for, if the theory is wrong, no matter how long experimenters search in vain, their inability to find a monopole would not disprove the theory! Yet the theory is certainly a scientific one, well worthy of serious consideration."
R.Penrose, The Road To reality, ch:34,

I have added “(like supersymmetry)” to the text because Penrose has mentioned it as an example of un-Popperian idea a few lines earlier,

As you see, being falsifiable is not necessary for the basic assumptions of a scientific theory (like existence of a magnetic monopole somewhere in universe), but it is necessary for the predictions of such theory (like the fact that electric charge is an integral multiple of some fixed value). In other words, we may have a theory based on some unfalsifiable assumptions, but the theory can be scientific if it makes falsifiable predictions. And it can become “credible” if the predictions are verified.

A more complete version of what martin said, can be:”a model that its predictions cannot possibly be falsified cannot be scientific. Turning it into a positive criterion makes no sense.”

In the string theory case, it seems that neither the basic assumptions (like replacing particles with tiny strings) nor the predictions (if any!) are falsifiable and it makes the situation very difficult.

11. **Mesa**
December 17, 2014

Maybe these things can take a page from mathematics and be described as conjectures instead of theories. IE string conjecture, multiverse conjecture, inflation conjecture, quantum gravity conjecture. Maybe the burdens on conjectures are less stringent. Along the road to theories from conjectures might lie testable hypotheses.

12. **Bee**
December 18, 2014

Hi Peter,

I see. I just don’t notice this in my reader – I have hep-th, hep-ph and gr-qc in the same feed and (that includes all the cross-links), so I often don’t even know which category a paper is posted in.

Anyway, I think the main reason people post papers in one arxiv and not in another is not the name of the arxiv but the people who they know read it. Another issue with the arxiv categories is of course all the AdS/CFT stuff, much of which goes under high energy physics, though there really isn’t much high energy physics in it.

A simple way to avoid these issues would be to allow people to tag their papers with certain keywords.

13. **Bee**
December 18, 2014

Martin,

Thanks for this. I vaguely recalled that I had seen this elsewhere before, but couldn’t recall where. I’ll update the reference.

14. **Chris Oakley**
December 18, 2014

Brian Greene writes well as always, but I cannot help feeling that he would have been better off switching fields as advised in the autumn of 1984. I cannot say that the unification of General Relativity and Quantum Field Theory was high on my own list of priorities, though I was aware of the messianic zeal of those, like him, who latched on to the new band wagon in the succeeding months. My problem was, and is, that there is no point in trying to unify anything with Quantum Field Theory until we actually have one.

15. **David Bailey**
December 18, 2014

I feel that theoretical physics is not unique in science in having been infected with non-science of a variety of forms. I daren’t quote any examples because I’d be off topic, but the causes are not restricted to this area – people trying to publish as much as possible, the relentless push of money, and the way the media will hype very preliminary research reports that don’t need to be right, just exciting.

I don’t know what the answer is, but it might help if people from different disciplines realised how widespread these problems are.

16. **Simeon Every**
December 18, 2014

Penrose’s monopole example is I think only unfalsifiable if the existence of a single monopole produces “… the fact that each particle in the universe has an electric charge that is an integral multiple of some fixed value…” but also has no other consequences that are not more easily falsified by observation than the detection of the single monopole. Also, the theory would need to specify that there was in fact only one monopole, or some other practically undetectable number of them.

17. **Jerzy Kowalski-Glikman**
December 18, 2014

I think that the utilitarian character of science (predictions of the results of experiments and the background on which engineering can be developed), physics in particular, to which indeed Popperian argument applies, is only one side of the coin. The second, and not less important is to tell a story about the world. Such story might be full of dragons, fairies and multiverses -nothing wrong with it. Whats might be wrong would be to say that your story is the only one, but I do not think even hard core string theorists say that nowadays.

18. **Chris W.**
December 18, 2014

Jerzy,

The predictions of the results of experiments is not simply part of the utilitarian character of science, because the experiments themselves generally lack a utilitarian motivation. Rather, they are designed and performed specifically to *test theories*. There may or may not be a utilitarian interest in those theories’ predictions, and in any case that interest is largely beside the point.

People have been telling a stories about the world for millennia. What matters about science is its specific, focused and serious efforts to find out whether or not those stories are false, or by elimination *might* be true. The only reason we can seriously contemplate in science that there might only be one true story about the world is because of the objective elimination of candidates through such a process of elimination, leaving just a few standing. Of course we can never know for sure that we have found that one true story, even when “a few” becomes one.

For these reasons the testability of theories is of critical importance. Sean Carroll deplores demanding “ahead of time what kind of theory correctly describes the world.” Nevertheless we have no alternative but to require—ahead of time—that scientific theories be testable. This may very well be a non-trivial constraint on what the world can be like, but we’ve always known that this is implicit in the effort to do science. A world that is sufficiently capricious—a world in which reproducible results can never be sustained—would not be a world in which science can be done. It would be strictly the domain of gods, demons, and fairies, or something even more chaotic. Indeed, for most of human history the world appeared to most people to be more or less just this.

(PS: The use of the word “story” in this context is problematic, but I’ll have to leave that alone for now.)

19. **Bernhard**
December 18, 2014

I really had a big laugh with Gordon Kane. I wonder if anybody still takes him seriously, it’s so ridiculous.

20. **Flavio**
December 18, 2014

The discussion mixes up two arguments.

(1) The string idea does not work.
(2) The string idea is not falsifiable.

Argument 1 is tested by checking with experiment. For example, we can check whether supersymmetry exists. Discussing argument 2 is not smart. The argument would imply that the whole string approach is nonsense. Therefore all string workers get angry when falsifiability is discussed; they know that they are not doing nonsense. A fair and honorable discussion should only focus on argument 1; the rest is gratuitous hostility and leads to hostile reactions.

Stating that an idea is wrong is different from stating that it is nonsense. Also Ellis and Silk mix up the two arguments, and thus elicit a lot of hostile reactions. As expected.

This situation leads to my personal point of view on unification research:

(a) The unified theory we are all so desperately looking for has not yet been found because feedback, criticism, responses from reviewers and responses from funding agencies always slip from argument 1 to argument 2, thus discouraging anybody attempting to work on the issue.

(b) The unified theory will only be found by a researcher with no need for funding and who works in hiding. (Like for Fermat’s last theorem.)

Let us create a climate free of hostility – a climate that allows loners to work on their own. Then unification will appear.

21. **Don**
December 18, 2014

I am currently reading Hermann Weyl. Since its a real book and not a PDF, I can’t find the exact quote and am only paraphrasing from memory, but he says something to the effect: Philosophers wish to think a thought and immediately get the truth, but science takes a very long time and philosophers are just impatient.

Increasingly, as I become familiar with this whole debate, Weyl’s sentiment seems a wise attitude to take.

Thanks,

Don

22. **Jerzy Kowalski-Glikman**
December 19, 2014

Chris W.,

I probably should write “utilitarian”, by which I mean “to be able to develop a technology i.e., to produce artifacts (not only material, but also, say, medical procedures) that behave according to the designer’s plans”. As for the story, I precisely said what I meant, and therefore I do not agree with you. In my view, the only requirement is that the story is consistent, in particular consistent with other stories that we consider “correct”

23. **Peter Shor**
December 19, 2014

I finally got around to reading the *Nature* article, and came across this line:

> In our opinion, this is moving the goalposts. Instead of belief in a scientific theory increasing when observational evidence arises to support it, he suggests that theoretical discoveries bolster belief.

This is clearly taking things too far in the opposite direction. The theoretical discovery of color confinement and asymptotic freedom clearly increased belief in the quark model, and there was even a Nobel Prize given out in connection with it.

But that’s because this theoretical discovery improved the alignment of theory and experiment, which is the test that theories need to pass.

24. **manfred Requardt**
December 19, 2014

I think the work of Gerald Holton is helpful in this context of what is science and what not (Thematic Origins of Scientific Thought). It shows how important mental dispositions are.

25. **Kevin Henderson**
December 19, 2014

As an experimentalist, having physicists making claims that are, in principle, not falsifiable does not appear bad. You can think of it like this:

I have just thought of something and I think it explains some physical phenomena. If it never gets published (arXiv) no one will know about it. It might have a flaw that others can point out. It may be useful in peripheral ways that help others connect ideas of their own. It may, of course, be of no benefit to anyone, but what harm has come?

In my field, the reverse tends to be true. People do not publish enough of their mistakes to let others know not to try a specific method that is doomed to fail. Science is becoming more about not doing, but deciding what not to do. I think we all have to get used to that.

26. **Nobody**
December 20, 2014

"It may, of course, be of no benefit to anyone, but what harm has come?"

You'd be right if funding was unlimited. Since it isn't, there has to be fights over whether or not various lines of thought are crap.

27. **arsenic&lace**
December 20, 2014

It makes perfect sense to describe modern string theory as a form of mathematics. Mathematicians may not view their work as speculative* but that's really what it is; conjuring rules to describe an imaginary system, and then deducing the consequences of these rules. String theorists are only different in that they sometimes eschew the levels of rigor employed by mathematicians.

At any rate, this whole debacle is just what is guaranteed to occur when experiment, which is the driving force behind physics and all science, atrophies due to lack of reasonable targets. Abandoning string theory won't save HEP; the only thing that can do that is some actual experimental results.

*Some mathematicians view their work as an investigation on equal footing with say, chemistry: much as the chemist investigates the properties of an object found in reality, so too does the mathematician. Different algebras are as real to them as different polymers, but this is a ludicrous position. Mathematicians just don't hope necessarily that their work has actual relevance to reality.

28. **Peter Woit**
December 20, 2014

I think that virtually no mathematician would say that "string theory is mathematics" and that people claiming this are not mathematicians (Ellis and Silk certainly aren't). What mathematicians do is work to create new mathematics. Applying known mathematics doesn't make you a mathematician any more than my using the laptop I'm typing into makes me a computer scientist. "String theory" now covers a wide variety of different things, some of which try to use mathematics that is not well-understood, and can very well lead to new mathematics. Some of that activity certainly can be seen as mathematics research (a lot of Witten's work falls into this area for instance). Other parts of string theory, for instance those trying to put together complicated models of "string phenomenology" or "string cosmology", are just using well-known mathematical techniques and are in no sense mathematics research. They're straightforwardly physics, sometimes using mathematics that just doesn't happen to be part of the usual physics curriculum. The failure to connect successfully to reality doesn't make this work mathematics, it makes it failed physics.

29. **Colin**
December 20, 2014

The pure mathematician's view on string theorists seems to be roughly: "They believe in a lot of conjectures, some of which turn out to be very insightful, but their confidence in these beliefs is difficult to comprehend, as is their obsessive focus in certain specific classes of examples." Genuine and interesting new mathematics, such as 'monstrous moonshine', has been inspired by developments in theoretical physics, even if we don't trust what the physicists claim as 'theorems'. It's reminiscent of the story of the Italian school of algebraic geometry in the early 20th century: some people working in this area became increasingly cavalier with standards of rigour, and for a long time 'got away with it', in that they obtained a number of important results that were ultimately proved correct by more rigorous standards, but eventually the contradictions started to pile up and nobody had any confidence any more about which results were correct.

30. **tulpoeid**
December 21, 2014

"It is based on quantum mechanics, so if experiments falsify quantum mechanics, they falsify string theory. This just makes clear that the question of falsifiability can be slippery."

No, excuse me, this is not some deep smart logical paradox, don't let these people lower everybody's standards. It's a failure to grasp ridiculously basic logical concepts to a degree horrifying for someone who gets public funds (directly, instead of as a pupil or a patient). "Send someone to fetch a child of five."

31. **Hilton Ratcliffe**
December 22, 2014

I recently retired after 40 years in astrophysics, during which time I succeeded in making of myself something of a pariah, although all I wanted to do was practice physics, physically. I am South African, partially educated at the University of Cape Town, George Ellis's academic home. At this stage of my life I can say what I like without jeopardising my meagre pension. And what I say is this: thank heavens for George Ellis, Peter Woit, the late Geoff Burbidge, and those few others who had the courage of their convictions and stood up to the corruption of science. My swansong, and indeed also my magnum opus, is my third book, Stephen Hawking Smoked My Socks, a treatment of the influence of belief in the formulation of our opinions, scientific or otherwise. In it, I acknowledge the courage of Ellis, Burbidge, and you, Peter. I salute you, Sir.

32. **Hontas Farmer**
December 22, 2014

A point of view which is quite interesting was published here by Johannes Koelman which states that string theory, M-Theory and other quantum gravity models can claim a degree of verification by post-diction. M-Theory in particular reproduces the Standard Model of particle Physics (many of them actually) and General Relativity. By predicting the same low energy behavior as those models theories gain their verification.

What is your take on such an argument?

Personally while I find this line of thought interesting I am not totally convinced. Postdiction of known physics only means a model is plausible. What needs verification are the unique features of a hypothesis. *What needs verification are the predictions that go beyond known physics*.

> I'm leery of the impulse among physicists to solve their problem of how to deal with bad physics by calling it mathematics. Yes, there is good mathematics that has come out of untestable ideas about string theory, but no, this doesn't include the string landscape/multiverse cop-out, which physicists need to face up to themselves.

Let us hope this kind of thinking does not gain traction. Who would they have make that call? What "wonderful group" would get to decide that this model which deals with physical phenomena is physics and this other model over there isn't?

33. **Nobody**
December 22, 2014

“Who would they have make that call? What “wonderful group” would get to decide that this model which deals with physical phenomena is physics and this other model over there isn’t?”

Nobody can stop anybody from claiming that some idea is physics. But people do make decisions about what research will be funded, who will be hired, and what papers will be published in reputable journals.

It is how the world works.

34. **Curious Mayhem**
December 23, 2014

@Douglas Sweetser – I like this “work on strings.” The overuse of “theory” was perhaps lifted from the follies of the humanities of recent decades.

As for “predicting quantum mechanics,” this is as absurd as “predicting the Standard Model.” In both cases, the theories in question pre-existed “work on strings” — as did supersymmetry, higher dimensions, and inflation — and depend in no way on “work on strings.”

What amazes me is how long this desperation has lasted and how many who might have done some science have been driven from the field by the “work on strings” monomania.

Quick Links

December 19, 2014
Categories: Uncategorized

- The Planck data release has been delayed yet again. December 22, is now off the table, the latest plan is "before the end of January 15", see here. Some peeks at their results are in slides from the Ferrara conference, available here. The fact that the slides for the "Planck low-ell CMB power spectra" talk are unavailable correlates with the rumor I've heard that they have recently found serious problems with that part of their data analysis, which would explain why the data release keeps getting pushed back.

  This week there's a conference in Paris, no slides yet. Streaming video has been available, which I took a look at for a while. Just managed to catch the tail end of questions about what the state of their analysis is relevant to the crucial B-mode business. Not enough to get the bottom line of what the state of affairs is. Perhaps someone who was there or who watched the whole thing can report. About the best source of information on cosmology these days seems to be Twitter, hashtag #planck2014. Something else of interest at the Paris conference was a debate about inflation featuring Steinhardt, Mukhanov, Linde and Brandenberger. Maybe video will be available someday, along with the slides.

- Scott Aaronson has more here about the problems with the recent movie about Turing that I mentioned here. Despite (or maybe because of…) having little relation to reality, the screenplay of the film has been nominated for a Golden Globe award.
- David Mumford and John Tate wrote a biographical sketch of Grothendieck for Nature. Unfortunately it seems that it won't be published there because of being too technical. It is however available at Mumford's blog.
- There's an interesting interview with Nikita Nekrasov at the artist Marina Abramovic's MAI site.

Update: Shantanu points out that the Paris talk videos are available here. Looking a bit, I didn't see anything from the Planck people about when they will release direct B-mode polarization results (next month? later?). Steinhardt gave a powerful talk arguing in detail that inflation does not predict anything, and that the usual claims for it are untenable. For the Steinhardt, Mukhanov, Linde, Brandenberger debate, see here.

Update: For yet another explanation of the problems with the Turing movie, set this at the New York Review of Books.

Comments

1. **Shantanu**
   December 19, 2014

   Peter the videos of the Paris conference are archived
   http://webcast.in2p3.fr/videos-lcdm_extension

2. **George Jones**
   December 21, 2014

   In the video for the Steinhardt, Mukhanov, Linde and Brandenberger debate, who has the mic from about 32:45 to 35:00?

3. **Peter Woit**
   December 21, 2014

   George Jones,
   That's Eva Silverstein.

4. **Katya**
   December 21, 2014

   Peter – Thanks for the link. I also enjoyed Slava Mukhanov, who gave a rollicking presentation. He pointedly disagreed with Alan Guth, who now claims that science does not depend upon falsifiability, but instead progresses in an "arena of competing ideas." Mukhanov slammed that as theology or philosophy, but not physics. He was also dismissive of the multiverse, because if he cannot check to see if it's there or not, "who cares." Science is built on experimentation, not theoretical arguments. "If something cannot be ruled out, it is not science." Bravo.

Dualities

December 19, 2014
Categories: Multiverse Mania

There's a very interesting new paper on the arXiv by Joe Polchinski, a survey article for Studies in History and Philosophy of Modern Physics, entitled just Dualities. It's an unusually lucid summary of the story of dualities in quantum field theory and string theory. This is a very complex subject which has been a central one in theoretical physics for the last few decades, but most expository writing on the subject has tended to be either superficial promotional material or mired in technical detail obscuring fundamental issues.

One reason for this is that, as Polchinski does an admirable job of making clear, in a very real sense we still do not understand at all the fundamental issues raised by these dualities. He notes that "we are still missing some big idea", and points to the same comments from Nati Seiberg last month that I blogged about here. For most of the dualities at issue, our current standard technology for dealing with QFTs (the Lagrangian and the path integral over classical fields) is capable of capturing the two QFTs that are in some sense "dual", but we lack a viable larger framework that would give the two QFTs in two different limits and explain the duality relationship.

For an example of the problem, probably the oldest and most well-studied case where we are missing something is Montonen-Olive duality, a non-abelian duality between electric and magnetic charges and fields. A currently popular idea is to find the explanation of this in "Theory X", a 6d superconformal QFT, with duality coming from compactifying the theory on a torus (for more about this, see talks last week in Berkeley). The problem with this is that we don't have a definition of the "Theory X".

Polchinski places this problem in the context of a conjectural "M-theory" with various string theory limits. This has been the dominant idea in the subject for nearly 20 years now, but we seem no closer now to finding an actual realization of this conjectural picture than we were back in the mid-90s. Twenty years and thousands of papers have just given better understanding that various possible ideas about this don't work.

One place where I think Polchinski's survey is weak is in the treatment of this conjecture, where at times he takes as solid result something highly conjectural. For instance he starts off at one point with:

> String-string dualities imply that there is a unique string/M-theory.

and moves on to the conjecture that

> In this sense it may be that every QFT can be understood as a vacuum state of string/M-theory.

The problem here is that he's built a speculative view of the unification of physics, constructed on an assumption about a "unique" theory, when we don't know at all that such a thing exists. One basic lesson of mathematical research is that you need to keep very clear the distinction between what you really understand and what is speculation, because your speculation is often wrong and if so will lead you in the wrong direction. I think particle theory of recent decades likely suffers from people forgetting that some ideas are speculative, not firmly grounded, and may be pointing in the wrong direction.

One wrong direction this takes Polchinski is to the non-predictive, pseudo-scientific landscape of supposed string theory solutions and the multiverse, which he blithely invokes as our best fundamental explanation of physics. Tellingly, unlike the clear explanations of other topics, here he makes no attempt to describe these ideas other than to note that

> they rest on multiple approximations and no exact theory.

In a final section, Polchinski addresses the question of what all this tells us about what is "fundamental" and what is the role of symmetries. This is the crucial question, and I'd argue that our lack of understanding of where these dualities come from likely is due to our missing some understanding of how symmetries are realized in QFT or string theory. This has been the lesson of history, with the Standard Model only coming into being when people better understood how symmetries, especially gauge symmetries, could act in QFT. Polchinski largely takes the opposite point of view, arguing that the fundamental theory maybe has no symmetries, local or global. He quotes Susskind as suggesting that symmetries have nothing to do with fundamental equations, are just calculational tools for finding solutions. I think this is completely misguided, that a strong case can be made (and I do it here) that "symmetry" (in the sense of the mathematics of groups and their representations) lies at the very foundation of quantum mechanics, and thus any quantum mechanical theory, even string/M-theory, whatever it might be.

Wondering whether there will be an arXiv trackback to this, and whether Polchinski has something to say about it…

Update: The arXiv Monday evening has a large collection of excellent review articles entitled "Exact results on N=2 supersymmetric gauge theories", edited by J.Teschner (first is arXiv:1412.7118, last arXiv:1412.7145). Some of the results reviewed are based on deriving implications of the existence of the 6d (2,0) models discussed here and in the comment section.

Update: I've put this blog posting in the Multiverse Mania category, not because of the posting content, but because of comments in the comment section from Polchinski and Bousso.

Comments

1. **CIP**
   December 19, 2014

   Polchinski notes the analogy between S duality and the duality of descriptions in the Fourier transform. Such dualities of description have seemingly become ubiquitous in math, exposing deep connections between algebra, arithmetic, and geometry.

   Even the wave-particle duality that so confounded early quantum mechanics fits the pattern. Despite Susskind, these things are probably all quite deep.

2. **Giotis**
   December 19, 2014

   Well you don't have to resort to 6d SCFT to give a geometric origin of S-duality; as it is described in the paper the Montonen-Olive duality is explained by IIB self-duality.

   Moreover IIB self-duality on a circle is geometrically realized by compactifying M-theory on a 2-Torus (or similarly from F-theory) via modular invariance (i.e. the diffeomorphism invariance of the Torus).

   So overall the very existence of M-theory explains S-duality.

3. **Joe Polchinski**
December 19, 2014

Thank you for your review. Indeed, the landscape discussion was a bit of an aside and perhaps should have been omitted as a distraction. My remarks on symmetries are very much outside the orthodoxy and I was glad for the opportunity to present them.

About trackbacks, you seem to think that am responsible. I once stated my opinion, and someone, I don't know who, acted in response. I stand by my opinion, and believe that whoever acted did so correctly. It should be noted that your blog has no actual science content. There is science news, science opinion, science criticism, science sociology, occasional science invective, but no actual science. (Lubos, who also seems not to be tracked, does have science content but also has so much that is inappropriate that his signal/noise is miniscule). There is a primary literature of science, and a secondary literature of science criticism etc. It is great that the secondary literature exists and links the primary literature, but this is one-way, the primary literature should strive for a maximum signal/noise.

p.s. Regarding "they rest on multiple approximations and no exact theory," you might note that the preceding arXiv paper was directed at examining these approximations and making them more precise.

4. **Peter Woit**
December 19, 2014

Joe,
Thanks for the comments and thanks for writing the thought-provoking article. Your comments about symmetries struck me as quite similar to ones I've seen from Arkani-Hamed, especially about gauge symmetry, I think they're less outside current orthodoxy than you think. As for the symmetries "a calculational tool, not fundamental" point of view, Howard Georgi was warning students like me taking his Lie algebras class back in 1978 along much the same lines (it's not original with Susskind)

About your "no actual science" claim I'll have to respectfully disagree. There are lots of different things on this blog, but it's not unusual for it to contain more actual science than a typical arXiv paper on the landscape…

In any case, "actual science" by your definition doesn't seem to be the criterion that the arXiv is using to bar trackbacks to my blog, since they now allow trackbacks to a wide range of journalistic sources (checking at random those from yesterday, one is to the Atlantic magazine). Despite putting a lot of effort into this, I haven't been able to get an answer from the arXiv to the question of what criterion they are using to bar this blog. If you happen to find out, please let me know. At this point the only information I have about this is that you've called for such a bar and that has been very influential.

5. **Flavio**
December 20, 2014

Peter,

do not get worried about Joe's remarks about your blog. How can you two get along? Each of you two tells the other, politely, that he is not doing science. And you both believe this assessment of the other to be true, with every cell in your body.

There are only for possibilities: either of you can be right or wrong. But all four possibilities are of no help. But maybe I am wrong; maybe you see it differently.

Progress can only come by getting back doing science, by returning to the search for a solution to fundamental physics.

6. **M**
December 20, 2014

Recognising promising and failed lines of research is a key part of science. "Not Even Wrong" should have been on arXiv

7. **Peter Woit**
December 20, 2014

Flavio,
I don't at all think he's not doing science. The great majority of the article was about important and interesting scientific work. Better understanding these dualities is a great scientific problem, with both physical and mathematical aspects. As for the string landscape, for lots of it I'm willing to admit it's science, just really bad science…

8. **Mike Duff**
December 20, 2014

Giotis,

I agree with your remark "So overall the very existence of M-theory explains S-duality''. S-duality in D=4 follows from string-string duality in D=6 (hep-th/9501030) which in turn follows from M-theory in D=11 (hep-th/9506126). (Don't usually respond to blogs, but here I made an exception since M-theory is taking a bashing and this work somehow got overlooked in Joe's review.)

9. **Peter Woit**
December 20, 2014

Mike Duff/Giotis,
The problem with the "M-theory explains S-duality" argument is that, as Polchinski is careful to point out, you don't understand what M-theory is. The reason I mentioned the explanation in terms of the 6d superconformal QFT is that that seems to be something one has a chance of understanding independently of a full understanding of M-theory.
I seriously do think people need to keep clear what it is that they have a solid understanding of, and what is conjecture. Kudos to Polchinski for doing this somewhat better than usual.

10. **Giotis**
December 20, 2014

Yes of course professor Duff, what you mention is another classic derivation of S-duality from M-theory that was not included in the paper.

Peter not sure what you mean; we know enough to make that claim with confidence.

The reason why people often focus on the 6d SCFT (e.g. for the Langlands correspondence) and not on the Stringy/M-theoretic explanation of S-duality mentioned by professor Duff, is explained in page 5 of the seminal paper by Witten "Some Comments On String Dynamics" (http://arxiv.org/abs/hep-th/9507121).

Anyway the 6d SCFT is a prediction of IIB or M-theory.

11. **Peter Woit**
December 20, 2014

Giotis,
What I mean is that you can't claim to have explained something by showing that it is an implication of something else that is just as poorly understood. You're just relating one conjecture to another conjecture. If M-theory was much better understood than 4d qft that sort of relation might have explanatory power, but the opposite seems to be true.

12. **Mike Duff**
December 20, 2014

Giotis

Re S-duality from M-theory: Thanks for pointing out page 5 of the seminal paper by Witten, "Some Comments On String Dynamics" (hep-th/9507121). On this topic, see also page 33 in another seminal paper by Witten, "String Theory Dynamics in Various Dimensions" (hep-th/9503124).

13. **Peter Woit**
December 20, 2014

Giotis/Mike Duff,

I just took a look at the page recommended in hep-th/9507121, and I was amused to notice that Witten makes a version of the point I've been trying to make:
"Thus, if one asks, "How can the S-duality of N= 4 Yang-Mills theory be made
obvious?" one answer is that this can be done by embedding N=4 supersymmetric
Yang-Mills theory in the heterotic string and then mapping to a Type IIA theory by using string-string duality. The weakness of this answer is that it embeds the gauge theory in a problem with many other features – such as gravity – that may not be material. One would like to "flow to the infrared," eliminating as many degrees of freedom as possible, and obtaining the minimal theory in which the S-duality is still manifest. The self-dual string in six dimensions may be the answer to this question. The self-dual string in six dimensions does not look easier than the Type IIB model that we started with; certainly we understand it less. Nevertheless, it might be the right structure for understanding the four-dimensional field theory."

Witten talks here about the self-dual non-critical 6d string, these days it seems that attention is now on the simpler 6d superconformal QFT, presumably easier to understand. But the point is the same: embedding something you don't understand in something much more complicated you don't understand isn't a great explanation. Much better is to embed in the simplest thing that has the properties needed to explain what you want by this mechanism. What you want here follows from the 6d superconformal QFT, which is hard enough to understand. Embedding this in string theory or M-theory just puts you in a more complicated context, while it's the 6d properties that have some explanatory value.

14. **Mike Duff**
December 20, 2014

Peter Woit

Re your view that "The reason I mentioned the explanation in terms of the 6d superconformal QFT is that that seems to be something one has a chance of understanding independently of a full understanding of M-theory. I seriously do think people need to keep clear what it is that they have a solid understanding of, and what is conjecture."

Here is what Greg Moore says (http://www.physics.rutgers.edu/~gmoore/Strings2011FinalPDF.pdf).

"Important characteristics of the six- dimensional (2,0) theories S[g]: These theories have not been constructed – even by physical standards - but some characteristic properties of these hypothetical theories can be deduced from their relation to string theory and M-theory."

15. **David Ben-Zvi**
December 20, 2014

While it's certainly true that there is no mathematical definition or construction of the 6d SCFT (or even physical, as Mike Duff quotes from Greg Moore), the kind of object it is is clear mathematically, and one can formulate precise conjectures/axiomatics for its properties. Of course the main evidence for any of these constructions comes from string theory and M-theory, but on the other hand there are mathematical theories very close to it (eg its dimensional reductions or its Coulomb branch deformations) which can be defined and understood using current mathematical techniques.

For example in the workshop Peter links to Kevin Costello presented a mathematical definition of an approximation to theory X (the (2,0) theory) — roughly speaking it's the part of the theory seen by its local operators and surface defects, but ignorant of the 4-dimensional defects, and not enough to explain S-duality. This part makes sense as a perturbative quantum field theory and agrees well with many predictions in physics.

At least as far as mathematics goes, this is one of the great appeals of theory X – it's extremely useful to have a conjectural structure with this powerful predictive power (eg Langlands duality) that's close to things we understand or at least can formulate precisely; while we'd love to "go to 11" and see indescribably further through M-theory, this seems very far from the capabilities of contemporary math.

16. **Giotis**
December 21, 2014

Exactly professor Duff. Thanks for pointing out this excerpt.

@Peter some remarks:

As I have mentioned above 6d SCFT is a prediction/achievement of the second superstring revolution.

The best way we have to describe the theory so far is via AdS/CFT. Indeed the theory is dual to M-theory on AdS7xS4.

Although the theory sits in a conformal fixed point we should not forget that even in six dimensions it is the low energy limit of 6d LST (Little String Theory). Moreover the theory itself is known as the theory of tensionless strings in six dimensions (though of a solitonic nature).

Finally the discussion on whether we have to resort to the full machinery of String/M-theory is far from closed. Witten just made an argument at that point in time without expressing any certainties. Check for example his remark in chapter 4.2(page 15) of his paper "Geometric Langlands From Six Dimensions". This statement is similar (but stronger) to Moore's comment referenced by professor Duff above. Moreover Meng-Chwan Tan reproduced (http://arxiv.org/abs/0807.1107) the field theoretic results of the aforementioned paper regarding Langlands correspondence using the dualities and machinery of M-theory.

From another stringy point of view S-duality reduces to T-duality as it was demonstrated by Vafa in his classic paper "Geometric Origin of Montonen-Olive Duality" (http://arxiv.org/abs/hep-th/9707131).

Finally we should not forget that S-duality and T-duality are just parts of a bigger U-duality group which can only be understood within the general context of M-theory.

17. **Chris Austin**
December 21, 2014

On the issue of whether or not the 6d interacting superconformal field theories have Lagrangians: on pages 10 and 14-15 of 0712.0157, (cited by Giotis in the post on Weinberg and Seiberg), Witten seems to argue that an obstruction to these theories having Lagrangians in 6 dimensions is that when you compactify them on an S^1 of radius R to 5 dimensions, R occurs in the denominator of the resulting d = 5 Yang-Mills action, (so that the Yang-Mills coupling constant is proportional to \sqrt{R}), rather than in the numerator, as would normally be the case for a KK reduction. But it seems to me that a qualitative counter-example to this argument is provided by Witten's own work on obtaining type IIA superstring theory by compactifying d = 11 supergravity on an S^1 of radius R, on page 4 et seq of hep-th/9503124: the resulting superstring coupling constant is proportional to R^(3/2). The reason for this is that the type IIA superstrings result from solitonic d = 11 2-branes that wrap the S^1, (the 2-branes are the classical solutions of d = 11 supergravity found by Duff and Stelle). Could it not be possible that the 6d (2,0) theories have Lagrangians that admit classical 1-brane solutions, and on compactification on an S^1, the d = 5 Yang-Mills fields arise from 1-brane configurations that wrap the S^1?

18. **Urs Schreiber**
December 21, 2014

To amplify again what Giotis mentions above: there is an actual definition of the 6d (2,0)-CFT is via AdS7/CFT6 holography and it translates to a good mathematical definition in a suitable limit.

This deserve a reminder from time to time. In that same String2014 "vision talk" already mentioned above, "Physical Mathematics and the Future", Greg Moore in section 5.1 lists some ideas of how to capture the 6d (2,0)-SCFT, only to conclude (on p. 28) that:

> As stressed to me by Edward Witten, thanks to the AdS/CFT correspondence, we do have an understanding of the complete solution of the theory in the large rank limit for su(N) theories

In the limit in which the Chern-Simons terms in the dual 7d theory dominate, this even becomes essentially a mathematical precise definition, where the partition function of the 6d theory is obtained from the geometric quantization of the 7d Chern-Simons theory in direct higher analogy to how the 2d WZW model arises from 3d Chern-Simons theory (CS3/WZW2 correspondence). And this is the way that in the abelian case and for just the 2-form sector Witten has studied the 6d theory since

– E. Witten, "Five-Brane Effective Action In M-Theory", J. Geom. Phys.22:103-133,1997 (arXiv:hep-th/9610234)

– E. Witten, "AdS/CFT Correspondence And Topological Field Theory" JHEP 9812:012,1998 (arXiv:hep-th/9812012)

19. **Peter Woit**
December 21, 2014

Urs,
I'm a bit dubious about your argument that at large N, AdS7/CFT6 is nothing but the phenomenon we understand in terms of geometric quantization for AdS3/CFT2. Wouldn't the same hold true for the well-known AdS5/CFT4, and is that really all there is to that in large N? I've never heard anyone make that claim before.

In any case, the initial point remains that you're trying to explain one conjecture (electro-magnetic duality) in terms of others that are equally if not more mysterious (AdS/CFT + M-theory).

20. **Urs Schreiber**
December 21, 2014

In the limit where the CS terms dominate. That's the content of

— E. Witten, "AdS/CFT Correspondence And Topological Field Theory" JHEP 9812:012,1998 (arXiv:hep-th/9812012):

> This flux [in 4d SYM] is encoded in the AdS/CFT correspondence in terms of a five-dimensional topological field theory with Chern-Simons action. A similar topological field theory in seven dimensions governs the space of "conformal blocks" of the six-dimensional (0,2) conformal field theory.

For just the abelien self-dual 2-form theory in 6d, it's very construction in

— E. Witten, "Five-Brane Effective Action In M-Theory", J. Geom. Phys.22:103-133,1997 (arXiv:hep-th/9610234)

is via abelian 7d Chern-Simons theory in direct analogy to CS3/WZW2. This statement later became the main theorem (i.e. was made mathematically rigorous) in

— M. Hopkins, I. Singer, "Quadratic Functions in Geometry, Topology, and M-Theory" J. Differential Geom. Volume 70, Number 3 (2005), 329-452. (arXiv:math.AT/0211216)

21. **Peter Woit**
December 21, 2014

Urs,
You're just talking about a topological limit or sector of the theory. I don't see how that gives you electro-magnetic duality of the 4d theory, that seems to me to require behavior under conformal transformations that you can't see in the TQFT limit, or am I missing something?

22. **Urs Schreiber**
December 21, 2014

The 7d CS theory is topological, not the 6d theory that it encodes holographically. Just as for CS3/WZW2. The conformal structure in 6d arises as the polarization structure for the geometric quantization of the 7d theory.

23. **Peter Woit**
December 21, 2014

Urs,
OK, but based on what I do understand here (the subtleties of getting from 3d Chern-Simons to a 2d CFT), I'm suspicious that to do what I don't understand here (get from 7d Chern-Simons to a 6d CFT) you need to make some conjectural assumptions at least as strong as electro-magnetic duality.

In any case, what you seem to be claiming is that one just needs to understand 7d Chern-Simons, not the full M-theory, to get non-abelian duality.

24. **Peter Woit**
December 21, 2014

And I'm still wondering about AdS5/CFT4. 5d Chern-Simons is enough to give you 4d large N CFT??

25. **Urs Schreiber**
December 21, 2014

No. Check out the articles that I pointed to for the statement.

26. **Joe Polchinski**
December 22, 2014

Peter,
I don't speak for the arXiv, but clearly this is a judgement call, a question of signal/noise. Let me explain what I mean by `no science.' Your most recent post on the landscape:
"The latest news (arXiv:1410.7776) is that KKLT doesn't actually work, that you can't get stable string vacua that way. I don't think though that this will have any effect on multiverse mania and its use as an excuse for the failure of string theory unification…"

First, you seem to just assume that this paper is correct. An authority on the process of science might be expected to recognize that just because a paper appears on the arXiv, it cannot be assumed to be correct. Second, there is absolutely no discussion of the content of the paper, or what are the scientific issues involved. Rather, you just use it as an excuse to launch into your standard uncharitable assessment of a group of scientists, and to indulge in some of your favorite charged words ("mania", "failure"). Third, you get both the science and the sociology wrong, see arXiv:1412.5702.

I see zero signal here, and a lot of noise. And this is a very representative example. So no, I do not think that the arXiv should link to this.

p.s. Thanks to Mike Duff and others for the discussion of references.

27. **Maurice Carid**
December 22, 2014

Hi Peter,
with all due respect, but I think Dr. Polchinski has a valid point here.
For a string-theory/landscape unrelated example: in your entry
from Nov 3, 2014 "Last week's hype" you slandered an arXiv entry
as "university press-release-driven hype" and the press releases themselves as
"nonsense spread around the media". When Als and I – both after having read the paper – challenged you to explain what's wrong with the paper and the press releases, all you contributed was "Maurice/Als,
I've already wasted more time on this than is wise."
What useful information would a trackback to your blog entry have
conveyed to the readers of that paper?
cheers & no hard feelings!
maurice

28. **Peter Woit**
December 22, 2014

Maurice Carid,
A statement is not slander if it is accurate. I think my characterization of that paper is pretty much the same as the way most physicists would describe it.

29. **Peter Woit**
December 22, 2014

Joe,
You're avoiding the question of what the arXiv policy is under which I am banned. What is it? If you don't know what it is, don't you think you really should ask someone before publicly defending it? I'm having a lot of trouble taking seriously your claims that you don't have any responsibility for this or know who does. This disagrees with what I've been told, which is that people at the arXiv point to you to justify this decision.

You seem to indicate that the arXiv policy is somehow based on a "signal/noise" evaluation, one that you have performed and found this blog wanting, largely because it regularly refers to the research program you are heavily invested in as a failure. The obvious problem is that others see what is signal and what is noise quite differently than you do. I suspect that my characterization of the anthropic string landscape program as pseudo-science driven by refusal to acknowledge failure is one that a sizable fraction of the scientific community would agree with (see the Ellis/Silk piece in Nature, although you likely dismiss that too as "noise"). You are very obviously using the "signal/noise" business as an excuse to ban a legitimate scientific point of view that makes you uncomfortable.

As for the mention here of arXiv:1410.7776, the background there is that I was contacted by someone expert on that topic who felt that that paper and the research program it was part of posed a serious challenge to widespread assumptions among theorists about KKLT moduli stabilization, that this was not getting sufficient attention from other theorists, so they wanted me to make this issue more widely known. My initial reaction was not to get involved on the grounds that the string landscape business, as serious science, is what some would accurately characterize as noise, not signal. In the end I decided to briefly mention it as one of six short items. Rereading what I wrote, I still think it's precisely on target as regards the sociology and the evaluation of that line of research.

As for your claim that I assumed the paper was correct and that I'm too dimwitted to realize that papers on the arXiv are sometimes not correct, that's just ridiculous. It's interesting to note that my brief mention here did have the effect my correspondent wanted, causing extensive discussion at another blog, and perhaps this even had some influence on arXiv:1412.5702. If what I write here is noise, it's noise that a lot of people in the field take seriously, and that's the problem that seems to be bothering you.

30. **Raphael Bousso**
December 22, 2014

Peter,

You write that you are not too "dimwitted" to recognize that an arXiv paper might be incorrect. We should accept that.

There are two remaining possibilities. One is that you analyzed arXiv:1410.7776 carefully and convinced yourself that the technical issues it raises support your claim that "KKLT doesn't actually work." If so, I'm sure you will have an equally specific response to arXiv:1412.5702, which does not reach this conclusion.

I would normally recommend that you post your analysis on the arXiv. On the other hand, by posting it here you could silence those who say that your blog contains no science. Anyway, you should post it somewhere, since the technical questions at issue are of scientific interest quite independently of any applications to KKLT and the landscape (which you may feel would not by themselves merit the effort of writing up your analysis).

The only remaining possibility is that you knowingly choose to advertise as "news" any paper that you perceive as fitting your narrative, without having any idea whether it is correct. I suspect that this wouldn't bother your clientele, so you might as well let us know right away whether a detailed analysis will be forthcoming from you.

31. **Dom**
December 22, 2014

As an interested lay person, I enjoy reading Peter's blog, I have tried all of the blogs he links to and although some interest me, only this one I refer to on a daily basis. I'm not actually that concerned (for example) whether the string approach is correct or not, I prefer to read lots of views. I would say one thing though as an outsider, the more confident I am about my approach to the problems I have to solve professionally, the more I welcome sceptical/hostile enquiry. When people with presumably enormous intellects get so upset that people criticise their ideas I can only conclude that there is some underlying and considerable internal lack of confidence. Einstein's reported reply to the book "100 Authors against Einstein" – namely "If I were wrong, one would be enough." strikes me as how a confident person deals with it.

32. **Bill**
December 22, 2014

Took a look at recent trackbacks; certainly, this blog would not decrease signal/noise ratio. Was surprised to see how few trackback there are on an average day. Don't see why anyone would make such a big deal out of (or even have a strong opinion about) this.

@Raphael Bousso: You can insult Peter all you want, but why such animosity toward his "clientele"?

33. **Bernhard**
December 22, 2014

It's pretty clear the not linking of the arXiv to NEW has absolutely nothing to do with science given other completely non scientific material they link to. This censorship that reminds us of inquisition times. If the policy of the arXiv was to link only to highly technical and (as best as possible), unbiased material, that is one thing. But since there is no such thing (Motl, really?), the sober conclusion is that the ban is pivoted by string theory critic. By the way, professor Polchinski, if you only "stated your opinion" to someone that happened to have veto powers on the arXiv, that is the same as making the ban yourself, as you are aware of your own influence and power on the field (that you conquered with talent and hard work, but I'm afraid still not justifiable).

34. **Doug McDonald**
December 22, 2014

As to signal/noise, in the blog business, especially blogs like this,
its not a dichotomy, its a trichotomy:

signal/metasignal/noise

I see this blog as being intended as metasignal

35. **Physics postdoc**
December 22, 2014

As a physics postdoc who does GR but not string theory, I've learned a lot about dualities in string theory and their relation to the 6D theory from the discussion in this comment section and I've picked up some great references. This is typical of my experience with Peter's blog, and has kept me reading along for about 6+ years. So I disagree with Joe and Raphael. My experience is that this blog has a very high signal-to-noise, and I think this very comment section is an example. Peter deserves all the credit for this, I don't know anyone who does a better job or puts more energy into moderating great discussions on a physics blog.

36. **Peter Woit**
December 22, 2014

Raphael,
A few explanations of things you don't seem to understand:

About my "clientele". A sizable part of it is string theorists, with the one who contacted me repeatedly to suggest I mention the line of work in question just one of many. When you insult people who find the information on this blog worthwhile, you're insulting a large number of your colleagues.

You have it completely backwards if you think that "KKLT doesn't work" fits my narrative, quite the opposite. I'm a big fan of Bousso-Polchinski, KKLT, and Susskind's argument for anthropics and the string theory landscape. If you, Joe, or others can convince your colleagues that string theory is a theoretical framework with essentially zero predictive power, that fits my narrative perfectly and I'm happy to support you. I've been saying this ever since the landscape business started, for an example from the earliest days of this blog, see
http://www.math.columbia.edu/~woit/wordpress/?p=13&cpage=1#comment-111
I mentioned arXiv:1410.7776, despite it going against my favored "narrative" because I thought some people would find it interesting, and it seems they did (I'm still wondering whether it motivated Joe to write that paper…).

About the technical details in question, of course I'm not going to devote time to them and anyone reading the "KKLT doesn't actually work" clause of that sentence in context would recognize that I wasn't vouching for it. I've ad nauseam made the case that this kind of construction is not science, but pseudo-science: it's designed not to give a plausible way of reliably calculating something that can be compared to experiment, but instead to provide an elaborate excuse for not being able to do this.
I don't see any point to taking seriously the details of this elaborate excuse. My non-expert take on the details is that Joe likely has it right with his comment that these arguments "rest on multiple approximations and no exact theory." Maybe you can find an obscure deep corner of parameter space with stabilized moduli and hopelessly complicated solutions, maybe you can't, but in either case this has nothing to do with serious science (in Joe's lingo, it's just "noise").

37. **Anonymous**
December 22, 2014

Raphael

Since when is “you must not discuss a disputed preprint, unless you yourself have posted a preprint on the subject” how science is done? Your attitude seems to be that those outside of your club should keep their mouths shut about your field. Regardless of whether the preprint in question is correct, this is deeply deeply unscientific.

38. **Peter Woit**
December 22, 2014

Anonymous,

People really should take a look at the posting that so upset Bousso and Polchinski, it’s
http://www.math.columbia.edu/~woit/wordpress/?p=7266

The short sentence
“The latest news is that KKLT doesn’t actually work, that you can’t get stable string vacua that way.”
with “latest news” a link to arXiv:1410.777 is the full and complete discussion of the paper in the posting. I don’t see how it’s not obvious that that was nothing more than a pointer to something people might be interested in, just giving the gist of the paper’s claim.

So, Bousso isn’t objecting to my discussion of a paper, he’s objecting to my mentioning a paper. This is really, really odd.

39. **Maurice Carid**
December 23, 2014

Hi Peter,
Because you gave us no clue why you think that
http://arxiv.org/abs/1402.6144 is “university press-release-driven hype”
I cannot be sure. But in this case the suspicion of Dr. Bousso that you
“advertise as “news” any paper that you perceive as fitting your narrative, without having any idea whether it is correct” is correct, suggests itself strongly (I’d say “even in case you have no idea…”, though.).
The above paper is not about the landscape or string theory
but strictly only about the many worlds of quantum mechanics. You lauded the work of David Wallace on this idea on your post from Dec 1, 2014. The above paper is a much more mathematical and concrete elaboration on this subject than Wallace’s more philosophical musings.
It thus seems to be excluded that you dismiss the paper in principle because of
its subject matter.
There seem to be two remaining possibilities. Either Dr. Bousso’s quote above is correct for this case: u just took a glance at the title, thought it’s about the landscape or “string theory explaining QM” and slandered it.
Or you actually read it and found some technical flaw in the paper. If the latter is correct I challenge u to briefly reveal the flaw. (B.t.w. I have no connection to the authors of the above paper, I just read it carefully.)

40. **Joe Polchinski**
December 23, 2014

Just for the record (and my memory is not perfect here, it’s been a long time), I believe that my original comment about trackbacks, which had whatever influence it had, was made publicly on this blog around 2006, and that it was motivated by a discussion, initiated by Peter, about whether Steven Weinberg was senile. It’s true that Peter concluded that he was not, but you chose to publish the original speculation. Note that Weinberg has written at least three books and thirty research papers since that time. Now you’ve since toned down `senile’ to `mania’, `pseudoscience’, but it’s still not scientific discourse.

You know Peter, you repeat the same rants so often, if the arXiv did link to you they would have to include an arXiv warning “this post has significant text overlap with 20 previous posts by the same author.” 🙂

41. **AReader**
December 23, 2014

Hi Peter,

I do like reading your blog, because I think it is important we’re looking for alternative theories …

I was just confused about this bit from you: “I’m a big fan of Bousso-Polchinski, KKLT, and Susskind’s argument for anthropics and the string theory landscape”. I was always under the impression you felt that string theory and multiverse are “wrong” – or have I misunderstood you, and you think they are quite likely to be correct theories?

Thanks.

42. **Peter Woit**
December 23, 2014

Joe,

I’d been told by a physicist who talked to you a few years ago that you were slandering me, telling him and other people that you didn’t read my blog because I had accused Weinberg of being senile. This physicist told me that he had to explain to you that what you were saying was not true. Your behavior then and now was and is completely unprofessional and you should be ashamed of yourself.

Some facts, with documentation:

1. I have never thought that Weinberg is senile or said anything like this, privately or publicly. Instead I have said exactly the opposite. The blog entry in question is this one
http://www.math.columbia.edu/~woit/wordpress/?p=289
in which I discussed in detail Weinberg’s Living in the Multiverse article, hep-th/0511037.

The morning that I was writing this, a colleague came into my office and I showed him the Weinberg paper. He reacted with a comment that Weinberg must be senile if he believed this was science. I argued with him that no, unfortunately for physics that wasn’t it, that Weinberg was as sharp as ever, but somehow this was becoming a popular point of view among physicists.

In the comment section of the blog that afternoon, responding to a comment from Thomas Larsson that "the heroes of your youth have become pathetic old men" I wrote, in full
"Hi Thomas,

One of my colleagues this morning, after being shown the Weinberg article, commented that Weinberg must just be senile. Unfortunately I don't think that's what's going on. Weinberg wants to be part of whatever the hot topic in particle theory is, and the landscape is the hot topic these days. It's being driven mainly by younger people, not by seventy-year-olds, and you can't put their behavior down to senility."
Yes, one of the "younger people" I had specifically in mind was Polchinski.

I think my meaning in that comment was perfectly clear: I was disagreeing with Thomas and arguing against any attempt to attribute senility to Weinberg.

2. Polchinski's argument that trackbacks should not be allowed to my blog was made at this blog posting:
https://golem.ph.utexas.edu/~distler/blog/archives/000760.html
see this comment
https://golem.ph.utexas.edu/~distler/blog/archives/000760.html#c003357
Neither he nor anyone else at the time mentioned my supposed accusations about Weinberg. He was personally outraged about this blog entry of mine
http://www.math.columbia.edu/~woit/wordpress/?p=73
which discussed in unflattering terms an article by him and Bousso in Scientific American promoting the multiverse, anthropics, etc. This was one of the first such articles in a major publication by well-known physicists, perhaps the first that made clear the extent of the problem in the physics community. When I saw his comment, I didn't recognize the text he was complaining about as mine, realized this by googling it. My reaction was that perhaps that was over the top, so in a comment responding to him I apologized for it. Many people then contacted me to tell me they thought I was wrong to do so, that what I had written was perfectly justifiable. Given Polchinski's later behavior, they were right, and I was wrong.

One odd thing about this was that Polchinski's example of what was wrong with my blog was not a discussion of a scientific paper on the arXiv, but of a popular promotional piece in a magazine. It was unclear to me then and remains unclear to me now why he thought that was a good argument against allowing links to discussions of arXiv preprints on my blog.

43. **Peter Woit**
December 23, 2014

AReader,
No, I don't think string theory and the multiverse are "wrong", I think they are "not even wrong": they don't predict anything, so can't be "wrong". Interpreting the Bousso-Polchinski argument as "if string theory make sense and implies anything, it implies a landscape with just about every possibility", that's perfectly in agreement with my point of view. We just disagree about where that argument leads you. I (and I think most physicists) believe it leads you outside the bounds of science, into another sort of activity.

44. **Hamburger**
December 23, 2014

I have been reading Peter's blog for about a decade now without much active participation in the comments. I am now chiming in in support of him.

I find the scientific discussions in this blog very valuable with a high signal to noise ratio. Other blogs that I reed are Tommaso Dorigo, Scott Aaronson, Sean Carroll, and Tim Gowers. I feel that all these blogs have similar signal to noise ratio. (But I do not know if these arXiv trackbacks to these blogs.) In any case, exclusion of Peter looks like at best a childish behavior and at worst a corrupt way of behaving at people one disagrees with.

45. **Curious Mayhem**
December 23, 2014

If non-scientific sites like The Atlantic are tracked-back on arXiv, then Not Even Wrong should certainly be tracked-back on arXiv as well.

The trichotomy of signal/metasignal/noise is germane here. Not Even Wrong is metasignal. It isn't and doesn't pretend to be publishing original research. It does follow and comment on original research, in a scientifically informed way that is aware of multiple papers, ideas, lines of research, etc., at once. The idea that it's noise is nonsense.

46. **Joe Polchinski**
December 24, 2014

Peter, Yes, you repeated an anonymous suggestion that Weinberg is senile and disagreed with it, so you got to put the idea out there without anyone taking responsibility for it. And even the rest, your ridiculous attempts to psychoanalyze Steve Weinberg: the idea that he was just following a hot topic, when he had actually created that topic twenty years earlier, it was the young people who were following him. It's fine if you want to discuss this on your blog, but I still don't think it's arXiv material. The arXiv is for science.

47. **Yatima**
December 24, 2014

The arXiv is for Science.

No disrespect intended, but I can only read that in the voice of GlaDOS, continually pursuing Science.

This day is off to a good start.

48. **M**
December 24, 2014

While inventing the multiverse, Weinberg had enough free time for writing a few papers about the physics of our little universe. What a pity that string theory is so deep that younger generations no longer have enough free time.

49. **anon**
December 24, 2014

In case readers of this blog want to see the other side of this argument, let me give some support to Joe in this.

Though I don't have any first hand knowledge of trackback policy, I think it's pretty obvious to everyone that in Peter's case there was no policy. Someone just decided not to allow trackbacks to Peter's blog.

This might seem unfair, but to be honest there is no constitutional right to have your blog linked to the arXiv. Though the arXiv is not quite a peer-reviewed journal, it does by necessity exclude certain kinds of material which might be judged to be unsuitable.

Peter's blog is unsuitable for two reasons:

1) Peter has a controversial and highly negative view of a significant fraction of current theoretical research. Therefore many (though perhaps not all) scientists would be upset to have Peter's commentary linked to their arXiv preprints. Since the arXiv serves the scientists who upload their papers, I don't think administrators really had a choice but to exclude links to Peter's blog.

2) One might still argue that relevant scientific criticism of a paper should be easily accessible through the paper's arXiv preprint, whether the authors like it or not. This is where Joe's point is crucial: Peter's criticism is usually not relevant to the scientific issues under consideration. A relevant scientific critique must provide new knowledge, in particular one of the following: a) an argument that the analysis of a paper is erroneous or in contradiction with accepted facts, or b), a suggestion of a concrete alternative approach which is arguably more promising. But Peter's critiques are far more fundamental than this. He just doesn't think that the technical issues people are grappling with are worthwhile. This is Peter's scientific judgement and is ultimately his opinion, it is not a technical criticism relevant to any specific paper on the arXiv. The thing is, all scientists come to their own conclusions on the relative merit of various lines of scientific research, and find much of it wanting. No scientist really needs Peter's opinion on these matters, especially since his views are pretty simple and not extremely deep. I should emphasize that Peter has no scientific argument that string theory does not describe our universe. There are, however, scientific (albeit distressingly theoretical) reasons for believing that it does. However, I think Peter does make a convincing argument that we will probably never know for sure whether string theory is right. I hope this is not the case.

I like Peter's blog a lot. It's fun to read and disagree with. He runs a tight ship, and links a lot of interesting stuff. But I think it plays a role quite distinct from papers that appear on the arXiv, and see no need to connect them.

50. **Peter Woit**
December 24, 2014

Joe,
Anyone who follows the link I provided can read the 2006 discussion about arXiv trackbacks for themselves, and see that it was not, as you claimed here, about Weinberg. It was about your thin-skinned reaction to my accurate characterization of your Scientific American article as pseudo-science.

I note that you don't deny doing what I have been told you have been doing, lying to people in private for years about this issue, for the purpose of attacking my reputation. This is not just sleazy and unprofessional, but actually illegal.

51. **Peter Woit**
December 24, 2014

anon,
I disagree with many of your claims, e.g. that I have no scientific argument that string theory does not work, but the issue at hand is the arXiv policy. You don't seem to know what it is any more than anyone else. I think if you follow most trackbacks to the arXiv you'll find that very few of them are to the kind of technical discussion that I'm supposedly not providing.

I think you do though get at the reason the arXiv is doing what it is doing, and Polchinski's behavior also makes this clear. People don't like criticism, especially accurate criticism, and a decision has been made to ban trackbacks to my blog because of its critical point of view about a certain scientific research program.

52. **Sesh**
December 24, 2014

I am a working physicist, and I write a blog that refers to arXiv papers. Trackbacks from my blog do appear on the arXiv (not that it makes much of a difference to me whether they do or not, my blogging software sends the trackback links automatically). With that context out of the way, I have the following observations:

1. The arXiv includes trackbacks from a number of "journalistic" sites that contain no serious technical discussion, such as the Atlantic (clearly a departure from the trackback policy as stated in 2006).

2. The argument that to be included in trackback lists blogs should contain "actual science content" is not just obviously not current arXiv policy, it also appears unworkable. Are there *any* blogs that do this? If I have something that constitutes "actual science content" (i.e., an original research paper), I put it on the arXiv direct, not on my blog. Surely blogs invariably contain only the "meta signal" of science discussion, criticism, opinions, news etc. irrespective of their technical level or whether the author is an active researcher or not.

3. Bearing the above point in mind, when I follow trackback links from the arXiv – which I only rarely do, and only for papers outside my field of expertise – I do so precisely because I want to read a less technical (or even non-technical) discussion about the paper in question, including possible general criticisms. The primary literature is already there on the arXiv in the form of the actual papers, when I follow trackback links I am specifically looking for secondary literature. To me this seems to be the main point of trackbacks.

4. The signal/noise ratio of any blog is also a function of the quality of the comment threads. This blog seems to have a very high readership from among practising physicists, and therefore a far higher standard of discussion in the comments (whether or not they agree with the original posting) than almost any other science blog I know of.

53. **NumCracker**
December 24, 2014

Dear Dr. Woit,

AFAIK string theory was able to derive Black Hole area/entropy relations from first principles and, from a numerical perspective (at equivalent rigor level as taken by lattice QCD theorists) it nonperturbatively implies on a 3+1 world like ours (http://arxiv.org/abs/1108.1540). So in my humble perspective, it is not the theory that is "Not Even Wrong" … but the ability of modern physicists to analyse that from the numerical side that is still too naive.
Regards

Num.

54. **Peter Woit**
December 24, 2014

NumCracker,
While the general issue of "is string theory not even wrong?" is off-topic here, the specific model you mention is discussed by Polchinski in his "Dualities" paper. There he writes about the sort of Matrix theory you mention

"One challenge that remains here is that if one compactifies some of the dimensions (to get down to the four noncompact dimensions of our vacuum), Matrix theory becomes complicated, and if more than three dimensions are compactified its form is not known."

About the specific matrix model you discuss, he writes in a footnote:
"There is a covariant form of Matrix theory, based on ten Matrices X^u, which is supposed to describe IIB string theory [46]. However its full interpretation is not clear. One issue is that time, which is one of the ten matrices, must be Euclidean."

I don't believe that that it's accurate to say that the theory is as well defined as lattice QCD, or that the issue here is just not big enough computers.

55. **Raphael Bousso**
December 24, 2014

Thanks for the various responses to my earlier comment. Some readers took offense at my suggestion that folks don't read this blog because Peter's opinions are founded in deep technical expertise; yet the very same posters went on to explain that indeed, they come here for different reasons. I, too, visit regularly; the site is well curated and has useful links. (I even recall taking advantage of a funding opportunity that I might have overlooked had it not been condemned on these pages.) But let's not mistake the services and the discussion offered here with scientific research, as Peter does in asserting that his blog "contains science".

Doing science at any robust level does require a high degree of technical competence, along with other skills. This is what makes progress slow and hard-earned, much unlike the opinions voiced on blogs. The two papers referred to earlier illustrate this well. They engage in the presentation and analysis of a technical question that is of some interest beyond the string landscape, so no excuse for reflectively dismissing them. The question also bears on the validity of the KKLT construction.

Peter admits he has not made an effort to follow. Yet, as he notes, the validity of some construction of this type is central to his criticism of string theory on the grounds that it has many vacua. This is only the starting point of his many confident declarations about the subject; yet he concedes he has no real understanding of the case for or against its validity.

Rigorous criticism is what drives science; but simply jumping to a poorly founded conclusion and holding onto it independently of any developments is not rigorous criticism. After evidently taking our claim that string theory has many vacua on hear-say, Woit goes on another leap to conclude that therefore the theory is unpredictive and hence unscientific.

I know much less than Peter thinks he knows, so I have no pat conclusions to offer. One thing I know, from actually trying, is that it's a lot harder (but not impossible) to work out predictions in string theory than it was for the Standard Model, which also has a lot of solutions. (This is largely because we can test the Standard Model directly at the energy scales where it is simple—we don't have to infer the Higgs mass from an eV scale experiment. In quantum gravity, we expect the theory to be simple at the Planck scale, and if someone could access that scale there would be plenty of predictions to check.) But Peter isn't merely saying that progress is too slow, and the connection with experiment too difficult to establish, for quantum gravity research to be worth anyone's time—a legitimate if radically defeatist position. He simply knows that string theory is not science. Maybe he knows more than I do, but his response to my comment does not give me great confidence.

56. **Peter Woit**
December 24, 2014

Raphael,

A few quick comments before I'm off to holiday celebrations:

1. I'm glad you've found information here useful!

2. About KKLT, etc. No, I'm no expert on validity of the approximations used. I have though paid close attention (and often written about on this blog) to papers which try to get actual predictions out of such scenarios. As an example, attempts to decide whether this predicts high or weak-scale SUSY breaking. This effort convinced me that there was no plausible vision for this working out. Others may disagree, and they should keep working at this if they want, but I think it's a fair assessment that the past ten years have not been kind to such efforts. In recent years I see few serious attempts to make predictions, replaced by a lot of experts instead just assuming that, if these things work, they have very little predictive value and then, instead of abandoning the ideas, saying that, well, nothing we can do, that's how nature works. This seems to me a serious problem for the field.

2. I've never said "string theory is not science". String theory is a huge subject, with all sorts of science going on, and some mathematics. Even KKLT is science, but it's failed science. Once you come to the point where it becomes clear that your favored idea about nature has led you into something very complicated, with no positive evidence for it, and lots of evidence that the idea is unpredictive, the scientific method says you give up and do something else. Refusing to do this and instead asking for a change in the scientific method is the problem, and the point where the subject turns from science into pseudo-science.

3. Sure, maybe my evaluation is wrong, and people will find some way to turn this back into science. From everything I've seen, that's not happening though, with things headed in the other direction.

I think it should be clear to all where we disagree and are making different judgments, and future developments may make clear whose were better. So far, when I look back at what I was writing about this a decade or more ago, I think that writing holds up well.

Anyway, off to holiday dinner, best wishes to all, whatever their views on KKLT and the multiverse….

57. **NumCracker**
December 24, 2014

Dear Dr. Woit,

I am not telling that such matrix models are at the same foot as QCD, actually they have no fermionic sign problem on the lattice, what I am asserting is that the really interesting physics of them may most likely dwell on a nonperturbative sector. QCD bound states (e.g. are there tetra-quarks?) is a similar example in QFT that equally can not be understood by any advanced kind of math available nowadays, except by computer methods and hard numerical processing: does it make it a "Not Even Wrong" theory? NO! Maybe It just proves that most brilliant humans are just stupid mathematicians when faced to Nature's mysteries, but this also shows us that our specie is still very skilled in finding right answers by brute (numerical) force. BTW if IKKT is not a consesus around string theorists, BFSS model is, and you can see it is as (or more) suitable to numerical attack as QCD on deep infrared here: http://arxiv.org/abs/0707.4454
You have a beautiful blog here, which is read by imminent physicists, maybe more of them will find it encouraging to try to find realistic nonperturbative ST/Matrix vacua, by aforementioned battle-tested methods from QFT, maybe dualities would be even part of the predictive game (e.g. in the spirit of hep-lat/0208020). Maybe even Matrix models will be for ST what Wilsonian latticization was for QFT.

Merry Christmas

Num.

58. **CWJ**
December 24, 2014

Anon argues again trackbacks from arXiv to this blog .

First, “Peter has a controversial and highly negative view of a significant fraction of current theoretical research.”

Well, no. It’s controversial to string theorists. It’s not terribly controversial to physicists outside that subfield. Many will agree or disagree at some level, but it’s neither a radical nor a marginal position.

Also, hello! string theory is not a “significant fraction of current theoretical research.” It’s a significant fraction of high energy theory. But that itself is only a small portion of theoretical physics. (If you look at NSF funding, it’s about 0.5% of all of physics, or maybe about 4-5% of all of theory; these numbers vary from year to year, of course.) It’s exactly this kind of egocentrism (theory = high energy theory) that Peter is arguing against.

Second, Anon says ” many (though perhaps not all) scientists would be upset” to be linked to Peter’s criticism. My God, are you a physicist or a kindergarten teacher? Have you never received a referee report? Never been on a tenure committee? In a faculty meeting? Didn’t you have to defend a Ph.D thesis? Peter’s criticism is calm and measured compared to the vicious nastiness we face every day as working physicists. The idea that this blog will hurt the poor wittle feelings of those poor wittle posters on arXiv is raving nonsense.

59. **Chris Oakley**
December 24, 2014

Peter,

Raphael Bousso appears to be a landscaper, and not the kind that can do something useful in your garden. Your blog enabled him to find another source of funding.

You are therefore failing in your basic mission to stamp this kind of thing out.

A careful re-evaluation may be called for.

60. **Lee Smolin**
December 25, 2014

Dear Joe and Peter,

I agree with Joe that global symmetries cannot be fundamental and have made this point in both recent books. The reason is that global symmetries are forbidden in a relational framework such as general relativity. The principle that all observables represent dynamically evolving relationships implies that any two physically distinct events must be distinguishable by their values of local observables. Hence no exact global symmetries.

This goes back to Leibniz’s principle of the identity of the indiscernible. Indeed, general relativity with spatially compact boundary conditions has no global symmetries, ie GR has no killing fields on its configuration space, as was proven by Kuchar in the 1970s.

Symmetries feature in theories that describe subsystems of the universe and represent transformations of an external frame of reference relative to the subsystem modeled. Thus GR with externally imposed boundary conditions can have global symmetries which represent translations of their asymptotic regions relative to an isolated system. This is btw why a framework with asymptotic regions such as AdS cannot be a fundamental description of a physical theory.

Penrose used to make a similar argument, and proposed long ago that all the global discrete symmetries, including time reversal symmetry symmetry and parity, should break down in a fundamental theory.

This does not apply to gauge invariances, which reflect the presence of relational observables. Nonetheless, for other reasons I also agree with Joe that many fingered time may be emergent. It is indeed dual to a three dimensional local conformal gauge invariance, as is shown by shape dynamics.

Best wishes for the holidays,

Lee

Winter Break

December 23, 2014
Categories: Uncategorized

Blogging here should be light to non-existent for a while, with family holiday celebrations tomorrow and departure for a trip to Europe the day after. Travel plans still in flux, but the general idea is to head south after arriving in Paris, spend a couple weeks on the road and mostly in Italy, end up back in Paris January 6, back to New York on the 11th.

I somehow seem to have caused in the last posting (see the comment section there) an eruption of an even odder version of the kinds of attacks from string theorists that were common in 2006, a period known to aficionados as the "String Wars". The new version is more like the "Multiverse Wars". From past experience I know that involvement in such things is not a good way to spend your vacation, so I think when I head to the airport I'll likely shut off comments. In the meantime, I hope the holiday spirit will reign…

Update: Off on vacation, comments will be off. Some last minute links that may be of interest:

- A paper on the hot topic of black holes and quantum gravity that at first glance looks quite interesting (also this). If this is new, surely by the time I get back experts will have been heard from.
- Mochizuki on efforts to understand his proof of abc.

Comments

1. **Jeff M**
   December 23, 2014

   Peter – have a wonderful trip! I'm off to Italy with family on Thursday, so perhaps I'll see you in Rome, or Florence 🙂

2. **Patrick Harris**
   December 24, 2014

   Wish ya the best, Dr. Woit! I've enjoyed your blog immensely, and support your future endeavors in the struggle against "multiverse mania." Happy Holidays!

3. **Bee**
   December 24, 2014

   Wishing you happy holidays and a good trip!

4. **Kevin NYC**
   December 24, 2014

   "Yes dahhhhling I will be traveling to the south of France….". Nice work to go soewhere and have fun…

5. **MathPhys**
   December 25, 2014

   Best wishes on the Holiday Season, Peter.

6. **Art**
   January 19, 2015

   So have any experts weighed in on the Nomura papers?

Back

January 13, 2015
Categories: abc Conjecture, Uncategorized

Now back from vacation, and as far as I can tell, not much happened while I was away. Here are a few things I've seen that may be of interest:

- Mochizuki has posted a long progress report on "activities devoted to the verification of IUTeich." New Scientist has an article about this here, which quotes Minhyong Kim making comments I think most experts would agree with:

  > Some mathematicians say Mochizuki must do more to explain his work, like simplifying his notes or lecturing abroad. "I sympathise with his sense of frustration but I also sympathise with other people who don't understand why he's not doing things in a more standard way," says Kim. It isn't really sustainable for Mochizuki to teach people one-on-one, he adds, and any journal would probably require independent reviewers who have not studied under Mochizuki to verify the proof.

  Lieven Le Bruyn has a less charitable take (see here and here):

  > If you are a professional mathematician, you know all too well that the verification of a proof is a shared responsability of the author and the mathematical community. We all received a referee report once complaining that a certain proof was 'unclear' or even 'opaque'?
  >
  > The usual response to this is to rewrite the proof, make it crystal-clear, and resubmit it.
  >
  > Few people would suggest the referee to spend a couple of years reading up on all their previous papers, and at the same time, complain to the editor that the referee is unqualified to deliver a verdict before (s)he has done so.
  >
  > Mochizuki is one of these people.
  >
  > His latest Progress Report reads more like a sectarian newsletter.
  >
  > There's no shortage of extremely clever people working in arithmetic geometry. Mochizuki should reach out to them and provide explanations in a language they are used to.

  Mochizuki's progress report strikes me as quite an odd document, especially in its insistence that experts need:

  > to *deactivate the thought patterns* that they have installed in their brains and taken for granted for so many years and then to start afresh, that is to say, to *revert to a mindset* that relies only on *primitive logical reasoning*, in the style of a student or a novice to a subject.

  He at times seems to be arguing that his ideas are nearly disconnected from the rest of known mathematics, and the only way to understand why the abc conjecture is true. This is highly implausible, since the great beauty and strength of mathematics is the way in which deep ideas are interconnected, with many paths from one place to another. If he wants to convince people that he really has what he claims, the best way to do it would be to follow the conventional route: write himself a document giving an exposition of a proof of abc, in as clear and simple terms as possible.

  Unfortunately, that doesn't seem to be what he has planned, with his efforts devoted to getting others to start from the beginning and master his long series of papers. If this works, at some point there will be others able to write up a proof of abc using his ideas, and when that happens, experts may have something they can work with. This looks now like a story that is going to go on for a long time…

- The last couple weeks in Jerusalem there was a Winter School on General Relativity. It included a final session (video here) largely devoted to defending string theory as the one true path to quantum gravity. This included a panel discussion where Carlo Rovelli held his own in a battle of the LQG/string wars, with him ganged up on by Gross and Arkani-Hamed. Mostly I don't think there were any new arguments, just a rehash of the tediously familiar. Gross did give an enthusiastic call for all students to read the Dawid book discussed here.
  For yet another promotional effort about strings, one that seems like it could have been written exactly the same way twenty years ago, see here.
- One new argument from the Rovelli side was to point out that "Nature talks", and what it has said at the LHC so far is that SUSY is not there, blowing a big hole in the expectations of the superstring theory community. The Economist has a piece about how the upcoming LHC run at 13 TeV will be:

  > the last throw of the dice for the theory, at least in its conventional form.

  As often the case though, the article misrepresents the strength of arguments for SUSY:

  > But, though the Standard Model works, it depends on many arbitrary mathematical assumptions. The conundrum is why these assumptions have the values they do. But the need for a lot of those assumptions would disappear if the known particles had heavier partner particles: their supersymmetric twins.

  This is pretty much complete nonsense, since the problem with SUSY has always been that it doesn't actually explain why the SM model parameters take the values that they do, and this has always been the best reason to be skeptical about it.

  On the other hand, the Economist and Rovelli do get the basic story right: Nature talks, and if what it says in LHC Run 2 is that the theoretical physics community has been barking up the wrong tree for the last forty years, it will be interesting to see if theorists are still willing to listen.

Comments

1. **acTeVist**
   January 13, 2015

   Peter, happy new year.

   On a light-hearted note, an interesting poster has been seen around CERN after the winter break…
   http://i57.tinypic.com/2yvs8b6.jpg

2. **Bob**
   January 13, 2015

   While it may be implausible that there aren't connections between the rest of mathematics and IUTech, that doesn't mean that Mochizuki or anyone else knows what they are … it might take some time indeed to find them … and it seems there's still a question of whether it will ever be studied enough to be.

Mochizuki does seem enamoured with his work and he may well want to believe that it is truly isolated, as that would add substantially to the degree of his achievement. Given this, he might not be looking too hard.

Definitely a good candidate for automated proof checking as I saw someone mentioning on one of those linked blogs, perhaps a task for one of his acolytes. That would make people stand up and take notice. I know that tends to take years, but in this situation the alternative could take longer …

I do wonder if some of his acolytes are giving too much deference to Mochizuki in not pushing for more rework of the papers.

And there would also seem to be lots of reasons a person might not want to ever travel (unrelated to mathematics) and most they wouldn't necessarily want to talk about. I'm aware Mochizuki has been abroad before but things change …

3. **Douglas Natelson**
January 13, 2015

acTeVist – that's great. I love how 200 years from now one of the focus topics is how condensed matter physicists still don't "get it".

4. **Peter Woit**
January 13, 2015

acTeVist,

Thanks! Although I'm not sure how funny it is….

5. **Als**
January 14, 2015

"Mochizuki has posted a long progress report "

I found Mochizuki's remarks on the Langlands program quite interesting.

"it will be interesting to see if theorists are still willing to listen"

They have been willing to listen for at least a decade, the problem is that nobody can find an alternative.

6. **Bernd**
January 14, 2015

Nature has already spoken, the electron EDM measurement of the ACME collaboration essentially rules out SUSY partner masses up to 10 TeV, i.e., higher than what the LHC Run 2 will achieve.

7. **Andrew**
January 14, 2015

Efstathiou: Paper testing BICEP2 might already be submitted, but have to wait for arXiv submission in ~2 weeks to learn more. He also said "it is pretty easy to infer what the answer will be". It's Just Dust. The Story is Over((

8. **Shantanu**
January 14, 2015

Peter, a discussion between Freese, Tegmark and Gleiser on grand unified theories.
(although lot of the discussion was about origin of consciousness etc)

https://www.youtube.com/watch?v=hoTgId9inb8

9. **ptmalloy**
January 14, 2015

The brief comment by Mochizuki. His comment about reverting the mindset of a novice sounds a lot like a call to "beginners mind."(http://en.wikipedia.org/wiki/Shoshin
).

Mochizuki is a family name associated with the samurai class and the samurai were among the patrons of Zen.

Not that this necessarily means much, other than to suggest Mochizuki is using a familiar cultural idiom to describe how he expects people to approach his work.

10. **Andreas**
January 14, 2015

There is a 3-weeks seminar in March about IUTT at RIMS, talks by his students Yamashita and Hoshi and Minhyong Kim is there giving a talk aswell according to the program. In addition, Yamashita will publish a 200-300pages proof in the proceedings of the seminar, so i guess soon afterwards.

If the seminar and especially yamashita's writeup is good, the situation could clear up much faster than your pessimistic thought of "a story that is going to go on for a long time…" – don't you think so?

11. **Peter Woit**
January 14, 2015

Andreas,
Maybe Yamashita will produce the kind of document needed, that seems the most likely way for this to happen in the near future. We'll see.

I did note though that Mochizuki writes
"This led me to pose the following question to Hoshi: If one were to assign to the level of understanding of IUTeich that he achieved as a result of reading through the papers on IUTeich at least five times a score of "100", then how would he rate the level of understanding of IUTeich that he achieved as a result of attending Yamashita's seminar? The answer that he gave me was a score of roughly "10 to 15"".

and describes Yamashita as writing a “survey” of IUTeich (rather than a proof of abc). Yamashita on his website does label the document as “A proof of abc conjecture after Mochizuki” which is more promising.

12. **Cormac O'Raifeartaigh**
January 14, 2015

Hi Peter, the conference in Israel was a few days, not a couple of weeks! The poster suggests that it was mainly focused on the history and philosophy of general relativity to commemorate the centenary, looks very interesting
http://www.vanleer.org.il/sites/files/SpaceTimeConf_3.pdf

I had hoped to go, but couldn’t really justify the trip as there was no session on cosmology. This was a bit disappointing from my partisan viewpoint; our group contend that Einstein’s cosmic models of 1917, 1931 and 1932 (and his abandoned steady-state model) offer quite a lot of insights into his thoughts on space-time. Ah well, another time…

13. **Cormac O'Raifeartaigh**
January 14, 2015

Sorry Peter, I get it now. What you called a winter school on GR was actually the 29th Jerusalem winter school on theoretical physics – the conference I mentioned was a GR centenary conference in Jerusalem on Jan 5-8, that coincided with the winter school for a few days…which explains some of the talks you referred to!

14. **JG**
January 14, 2015

Wonder if I can get an accumulator (combo bet) on no primordial gravitational waves observed, no SUSY particles observed and no proof of the abc conjecture observed for 2015

15. **Shai Hulud**
January 15, 2015

Bob,
Given the vast length of Mochizuki’s proof and the complexity of even the normal and understood machinery of algebraic geometry that he is building on, I think it’s fair to say that this is *not* something proof assistants are likely to be able to handle any time soon. (Even if someone was willing to do the work of formalizing hundreds and hundreds of pages of proofs.)

The same could be said for Wiles’ proof of FLT, Deligne’s of the Weil conjectures or Falting’s of the Mordell conjecture (which is what Mochizuki is comparing his work to) and these three cases are (and were at the time) a lot less weird than Mochizuki’s work. In fact, it isn’t even clear if Mochizuki is claiming to have a proof of abc in ZF, the foundational paper on IUTeich being littered with references to grothendieck universes.

16. **Justin**
January 15, 2015

Glad you’re back Peter with everything okay. If I’m not mistaken, you were in or around Paris at the time of the terror. Glad you were not involved.

17. **Peter Woit**
January 15, 2015

Justin,
Thanks. Yes, I was in Paris during all of that. A very sad situation, but I was impressed by the way the French dealt with it.

18. **Arkadas**
January 15, 2015

Le Bruyn’s comment on his screenshot of Mochizuki’s note comes off as a bit of a cheap shot.

Had not noticed this page before.

19. **Kevin NYC**
January 15, 2015

I thought of comments Prof Woit has made here when I watched the season premier of PBS NOVA and they had a very good episode about the LHC and the Higgs,.and the restart.

But they had Jim Gates on to describe SUSY and he said that as a young man he found it to be very satisfying as a theory because it really could explain everything… then he looked wistful and said that after the restart they did find some evidence.. because he did not want to be sbown that he has wasted 20 years of his life on a completely flawed theory.. which actually explained nothing because nature did not work like that.

A sad but telling moment that string theorists will confront as they get older. Compare Higgs in his old age to.. Witten?

20. **electron EDM**
January 16, 2015

@Benard
Nature has already spoken, the electron EDM measurement of the ACME collaboration essentially rules out SUSY partner masses up to 10 TeV, i.e., higher than what the LHC Run 2 will achieve.
^

how solid is this result? what is a % confidence interval. Could a sufficiently sensitive EDM measurement of the ACME collaboration essentially rules out SUSY partner masses up to the planck scale, i.e lower bound for EDM measurement under 10^-38

21. **Bernd**
January 16, 2015

@electron EDM:

The current upper bound is 8.7 × 10^(–29) e·cm with 90% confidence. Going all the way down to the Planck scale will be difficult because the SM has a nonzero electron EDM around 10^(-38) e·cm, from where you'll be measuring against a nonzero background.

22. **Klaus**
January 16, 2015

Bernd, the Planck scale is just 10^-33 cm, so there is good chance to achieve it in EDM measurements, whatever the noise at 10^-38 cm may be …

23. **Bernd**
January 16, 2015

@Klaus:
Generically, new CP-violating terms couple to the electron EDM proportional to the inverse of the mass scale squared. Even the most drastic assumption of the dipole moment being proportional to the electroweak scale only gives you corrections from the Planck scale at a sensitivity of 10^(-53) e·cm, i.e., way below the SM value.

24. **neo**
January 16, 2015

Bernd

are there experiments underway to lower that bound?

25. **Noah Smith**
January 17, 2015

How about people stop picking on Mochizuki? If no one ever understands his theory, who gets hurt? He's not hurting anyone, no matter how sane or insane he turns out to be. Give the guy a break, jeez.

26. **Bernd**
January 17, 2015

@neo:
ACME is upgrading the experiment, expecting another order of magnitude improvement in sensitivity. The group of Ed Hinds at Imperial is working on laser cooling of YbF molecules that should give another order of magnitude, so we're talking of probing a scale of 100 TeV here.

27. **Aleksandar Mikovic**
January 17, 2015

The discussion on string theory vs alternative theories by Gross, Rovelli, Arkani-Hamed and Polchinski was very revealing. The string camp arguments are: (1) string theory is the only extension of GR and QM which generalizes the graviton scattering amplitudes and (2) string teory is the only game in town. According to (1) we do not need to worry about the experimental discrepancies, like the absence of supersymmetric particles in LHC, while (2) explains why so many young people are still attracted to string theory. Rovelli was quite right to say that we should "listen to Nature", because the statements like (1) are based on certain assumptions,
which we know that are true for a certain range of the length scale, but we have no idea what happens outside of this range. That is why we need a hint from an observation or from an experiment.

The second statement, emphasized by Gross, is referring to the fact that it is very difficult to extract the semi-classical results for LQG (the loop quantum cosmology results, strictly speaking, do not count, because loop QC is a truncation of the full theory). However, in the past four years have been made important advances as far as the semi-classical limit of the spin foam models is concerned. SF models can be considered as the lattice formulation of LQG, so that in the strict sense they are not the same as LQG, but it is expected that they will give LQG in the continuum limit. Anyway, by using the effective action approach from QFT, one can obtain the classical limit for a SF model and calculate the quantum corrections. The classical limit of the standard SF models is the area-Regge theory, which is almost, but not quite, the GR. If the Lorentz group of a SF model is replaced by a categorical generalization
(2-Poincare group) one obtains a spin-cube model, and those have the desired classical limit, which is the Regge formulation of GR.

28. **JG**
January 17, 2015

@Noah Smith

He wouldn't get the flak if he hadn't claimed such a big result. source

Why not post something like "I have some original approaches to mathematics and I might be able to develop a proof of some long-standing conjectures in this new framework. They might not conform to the standard ZF axioms or such, but I think it's an interesting new approach"

29. **neo**
January 17, 2015

@bernd

based on the current ACME results, what is the % likelihood that SUSY will show up in LHC run 2? If SUSY does show up in LHC run 2, does this mean there is a problem with ACME?

30. **Bernd**
January 19, 2015

@neo:
If the interpretation of the ACME result in terms of sufficiently heavy superpartners is correct, then the likelihood is certainly less than 10% for a detection during LHC Run 2. Probably much less, depending how large the mass scale that the LHC can probe is ultimately going to be (which is much less than 13 TeV).
In case the LHC should see something, this most likely means that there is some fine-tuning going on to make the electron EDM sufficiently small. I'm sure it's possible to cook up a variant of SUSY that produces such a fine tuning, but in terms of probabilities you should use your own discretion how much credibility you want to attach to such a theory.

31. **Doong**
January 19, 2015

Hi,

From any of these two articles from PhysicsWorld,
1> “Measuring (almost) zero” by Chad Orzel, or
2>“Search for electron’s electric dipole moment narrows”

or simply from the Wikipedia entry on ‘Electron electric dipole moment’, and references therein like, ‘Order of Magnitude Smaller Limit on the Electric Dipole Moment of the Electron’- http://arxiv.org/abs/1310.7534

It maybe noted that –
1>Within the standard model of elementary particle physics, Electric Dipole Moment of the Electron is predicted to be non-zero but very small, at most 10–38 e•cm or about about 10–39 e•cm
2> Present experimental range for EDM covers less than or upto,- 8.7 × 10–29 e cm

As such the naïve question (for me at least going by the numbers) is-

Why couldn’t any super-symmetric particles or some modified version of Standard Model via SUSY be lurking in the gap, or in other words,

if EDM is found positive within the still unexplored range why can’t we consider it’s existence owing to Super-symmetric particles among other explanations?

Else can we say that if ACME results closes the gap with a negative result, and the nearer it is as such to the SM limit, the lesser is the probability of detecting a super-partner, till none, if the gap is fully closed with a null result?

Any pointers will be much appreciated.
Thank you.

32. **neo**
January 25, 2015

@Bernd

“The current upper bound is 8.7 × 10^(−29) e·cm with 90% confidence”

doesn’t this upper bound already eliminate SUSY?

Supersymmetric models predict that |de| > 10−26 e·cm (ref below)

SUSY predicted effect on Electron electric dipole moment |de| > 10−26 e·cm is >> > 8.7 × 10^(−29) e·cm by 3 to 4 orders of magnitude

improving the upper bound to 10e-30 or -31 e·cm would only increase the % confidence.

not only will LHC2 run not see SUSY but no upgrade at any scale will, either.

ref: Arnowitt, R.; Dutta, B.; Santoso, Y. (2001). “Supersymmetric phases, the electron electric dipole moment and the muon magnetic moment”. Physical Review D 64 (11): 113010. arXiv:hep-ph/0106089

Short Items

January 19, 2015
Categories: Uncategorized

- The latest issue of the New York Review of Books has an article about the new Turing film, explaining in detail how it gets pretty much everything completely wrong about Turing and his story (see my review here). In related news, this week it was announced that the film is one of the final Oscar nominees for Best Adapted Screenplay.
- The DESY research magazine femto has a sequence of articles about the LHC, SUSY and BSM physics.
- The Swedish Research Council has just announced a ten-year grant of $60 million SEK (about $7 million) to bring Frank Wilczek to Stockholm University.
- Mike Duff has some complaints about the Dean Rickles "A Brief History of String Theory" (for mine, see here.)
- Jim Stewart, a mathematician who became wealthy based on his popular Calculus book (which we use here at Columbia) passed away last month at the age of 73. For more about him, see here and here. I had the pleasure of meeting him a couple times, with one occasion including a tour of his remarkable home in Toronto, Integral House.
- For a new book about a certain mathematical point of view on QFT, see Factorization algebras in quantum field theory, by Kevin Costello and Owen Gwilliam.
- Quanta magazine has a nice article by Kevin Hartnett on Ciprian Manolescu's work on the triangulation conjecture.

Update: One more. The Yale Art Gallery now has an exhibition of prints based on equations chosen and drawn by well-known mathematicians and physicists. It's called The Art of the Equation, and impresario of the project Dan Rockmore will be discussing it there at 5:30 on Thursday January 22.

Comments

1. **editors**
   January 19, 2015

   Dr. Woit,

   We've been running a website that excerpts one old paper in the mathematical sciences per day. About a quarter of the posts are related to physics. Unfortunately, the physics posts don't seem to get much attention from the physics community. Understandably old physics is only of interest to a minority of the physics community, but if you know anyone who is interested in that sort of the thing please feel free to share the following:

   http://www.mathmarauder.com/archives/category/field/physics

2. **Cormac O'Raifeartaigh**
   January 19, 2015

   Wow! That maths marauder website is great.
   Amazing to see relativity discussed like this in 1904, and from this perspective. I think the difference is that AE started from the opposite end; ff you assume with a universal speed of light as a principle, what does that imply about space and time?
   Great website..

3. **Anonymous**
   January 19, 2015

   Hi Peter,

   Do you have an opinion about the approach used in Costello and Gwilliam's work?

4. **Peter Woit**
   January 19, 2015

   Anonymous,
   My opinion is that I wish I understood it better…

5. **CHRIS**
   January 20, 2015

   Anyone can comment/explain the difference between the axiomatic QFT approach (nets of observables/C* algebra etc..) which is actively pursued by some european universities now and factorization algebra approach pursued by Owen,Grady,Costello ? I been reading AQFT (Haag) on my own and I didn't feel comfortable with the axiomatic approach at all and the math there. BTW , i am not bias.
   Thanks

6. **gossip**
   January 20, 2015

   Do you have some more details on the Wilczek item? Will he be relocating to Stockholm full-time? Or only a few months per year?

7. **Peter Woit**
   January 20, 2015

   gossip,

   From this
   https://twitter.com/FrankWilczek/status/557145157993132032
   where he refers to "visiting" Stockholm in June I assumed it isn't a full-time move, but I don't know.

8. **Douglas Natelson**
   January 20, 2015

   I'd love to look at the mathmarauder page, but it has been unreachable since yesterday (and I have been too lazy to use the web archive/wayback machine). I hope that page is going to continue – it sounds fun and informative.

9. **Anonymous**
January 20, 2015

Chris,

Perhaps the biggest difference is that the axiomatic treatments you're referring to are in Lorentzian signature, whereas Costello-Gwilliam are in Euclidean. As a consequence, the algebra of observables in these two situations satisfy different properties.

10. **Abdelmalek Abdesselam**
January 21, 2015

@Chris and Anonymous

I think the main difference is that the Costello-Gwilliam approach is in the setting of formal power series in "h bar". The axiomatic/algebraic approach a priori is non-perturbative although recently it has developed a sub-area addressing pertubative renormalization etc. ; see for instance the recent work of Katarzyna Rejzner and references therein. The difference between Lorentzian and Euclidean signatures is not that big a deal, especially because the precise connection is given by the Osterwalder-Schrader Theorem which is a result from axiomatic QFT.

11. **timothy212**
January 21, 2015

Dear all
Can anyone illustrate on the prospect of various constructive quantum field theory like
1.) Axiomatic approach
2.) Owen Costello approach
3) topological qft
4) Balaban approach
5) others ..
and how far each one is from constructing the ultimate 4 dimension interacting qft and also the mass gap problem
One interesting thing is it seems like the
Axiomatic approach is mainly pursued
By European universities and almost no
US researchers doing that .
Do you guys know why ?
Thanks

Tim

12. **Igor Khavkine**
January 21, 2015

@Abemalek,

Just a quick note. The difference between Lorentzian and Euclidean **is** a big deal if the background you are interested in is not Minkowski space (or, more generally does not have a nice timelike Killing vector). In the absence of such time translation symmetry there is no known analog of the Osterwalder-Schrader Theorem or even of the Euclidean formalism. And even just having a Killing vector is not enough (I believe the spacetime has to be static, and not just stationary), e.g., AFAIK QFT on Kerr does not have a Euclidean version.

13. **Abdelmalek Abdesselam**
January 21, 2015

@ Igr,

I didn't make it explicit but I was talking about flat space.

14. **Igor Khavkine**
January 21, 2015

@Abdemalek,

I'm well aware and my comment was aimed at the larger audience. And that's fine if all that you are interested in is Minkowski space. However, it's worth reminding people of the fragility of the link between the QFT formalisms in these different signatures, because the attitude of "Why bother with Lorentzian signature at all?" is troublingly common.

While I'm on the subject, another point that's often overlooked is the relevance of this issue for non-perturbative QFT. Quite a number of people are happy to accept that lattice gauge theory provides a non-perturbative construction of QFT (or at least a path eventually leading to one). However, that method is contingent on the Euclidean formalism and hence breaks in non-static backgrounds, for which then there's not even a suggestion of a method that could lead to a non-perturbative construction.

15. **Bernhard**
January 21, 2015

6M SEK per year for a grant including a Nobel Laureate is actually reasonable. Because of the large overhead costs at Swedish Universities, about 2 M SEK per year are needed just to cover his salary (given that you need about 1M just to cover a random postdoc). Given that he will need students and travel money for his team, 60M SEK end up not being extravagant.

16. **Dean Rickles**
January 24, 2015

Hi Peter,

I just read the Michael Duff review. Since his complaints make no sense at all and get things entirely the wrong way around, this is probably a good place to vent:

(1) He complains that I don't explain how M-theory came to be. Yet I very explicitly noted in the preface that I was dealing with the earliest phases of string theory, and offer only a glimpse of M-theory, with considerably less detail involved.
(2) He says I "echo the Wikipedia version of the History of String Theory, according to which research on branes began only in 1995". Nothing could be further from the truth. There is section after section, and footnote after footnote where I point out that most of the D-brane apparatus was discovered much earlier, going back to the late 1980s – including Duff's work. I'm particularly annoyed at this one, since it was one of the key myths I wanted to correct. (Ditto his claim that I buy into the talk of "superstring revolutions" – another myth I explicitly argued against in the book)
(3) He says I only mention "membranes" in a "derisory" way – I've no idea where he gets this from. There is nothing in the least derisory about anything I say about them!
(4) Worst of all, he complains that I "belittle the role of supergravity" by referring to a "decade of darkness" in the 70s and 80s. But this is an ironic title! The whole point is to show that the 'dark days of string theory' is also a myth, and supergravity is identified as the central player in this story. Yet apparently I "downgrade supergravity" with "zeal". That is a stunning statement given the pains I went to to tell exactly the opposite story.

Incidentally, my final paragraph contains the remark: "The lesson I think emerges from this is that, while the mythological presentations of 'revolutions' and 'dark years' and so on, make for a good story, a more accurate depiction reveals a somewhat less turbulent life story, though no less interesting for it."

So I'm truly flummoxed by this review of Duff's…

Best,
Dean

17. **Urs Schreiber**
January 24, 2015

@Abdelmalek Abdesselam,

even on Minkowski spacetime/Euclidean space, it is not a priori clear how to apply Osterwalder-Schrader or any other result in Haag-Kastler-style AQFT to those factorization algebras. Because, while the conceptual idea is similar in both cases — to encode a QFT by its system of assignments of collection of observables to regions of spacetime (Heisenberg picture!, dual to the Schrödinger picture of say the cobordism hypothesis) — the technical implementation is quite a bit different. In those factorization algebras the "collection" of observables assigned to a spacetime region is not only not C*, but is not even an algebra at all, it is just a chain complex (a quantum deformed BV-complex) without a product operation. The whole system of these chain complexes as the space region varies does inherit an operadic homotopy-algebra structure sort of globally by way of inclusion induced by spacetime regions into each other, but there is a priori no "net of algebras", not even in Fredenhagen's perturbative relaxation of the Haag-Kastler axioms.

There might be a good translation between these two formalizations of Heisenberg-picture stype QFT, and maybe once it's there it would allow to transport results such as Osterwalder-Schrader from Haag-Kastler-style axiomatics to factorization algebras. But for the moment this is wide open, as far as I am aware.

18. **Peter Woit**
January 24, 2015

Hi Dean,
Glad to provide a venue for you to respond!

19. **Tim Nguyen**
January 29, 2015

@timothy212

Each of the approaches you mention are addressed at different aspects of QFT (topological, perturbative, constructive). Each have their own goals and limitations and as such are not as comparable as your question might suggest. For the axiomatic QFT you are asking about, the main goal seems to be to overcome the limitations of formulations QFT formulated on flat space, since our universe is curved not flat. Moreover, there is a [Haag's theorem](#) which roughly says the usual interaction picture of QFT's doesn't exist, hence the emphasis of the axiomatic school on algebras of observables rather than their representations.

As for why it hasn't caught on broadly, my guess is that it's because 1) it's still perturbative 2) it's not relevant to the majority of interesting physics already taking place on flat space or on (compact) Euclidean manifolds.

But your question raises a good point: it's clearly a sign that there is much to learn and much work to do in QFT when there are many disparate schools of QFT that don't actively communicate. Any particular school will have its own problems internal to its own (e.g. if you are interested in TQFT, you can go off and study category theory and leave physics behind). On the other hand, I think real progress will be made once more emphasis is put on solving important framework-independent problems in QFT, e.g. mass gap, making sense of many of Witten's remarkable results (which did not come about by adhering to any particular formalism).

20. **Tim Nguyen**
January 29, 2015

@Igor

If I understand correctly, couldn't one just simulate QCD on a curved background by modifying Wilson's lattice action to be plaquette dependent to account for a spatially varying metric?

21. **Peter Woit**
January 29, 2015

Tim,
Yes, you could do QCD on a curved Euclidean signature background as you suggest. I think the problem Igor is concerned about is that you lose the usual argument connecting the theory to a physical Minkowski signature theory.

For something related, I was going to give a link to a video of a lecture by Graeme Segal about Wick rotation

https://www.youtube.com/watch?v=vTvXHL6ZJik

22. **JG**
January 29, 2015

Not sure if this is a good place to post this, but latest rumours are that the BICEP2/Planck analysis is out next week and there is no B-mode signal due to gravitational waves.

eg according to Peter Coles

> Hearing that the #Planck/#Bicep2 cross-correlation results will be out next week (and the only B mode in CMB is from lensing, not GWs)

23. **Peter Woit**
January 29, 2015

More specific rumor I've seen is that this will be in an arXiv paper out Monday night.

The NSA, NIST and the AMS, Part II

January 19, 2015
Categories: Favorite Old Posts, Uncategorized

Last summer I wrote here about an article in the AMS Notices which appeared to make misleading claims about the NSA's involvement in putting a backdoor in an NIST cryptography standard known as DUAL_EC_DRBG. The article by Richard George, a mathematician who worked at the NSA, addressed the issue of the NSA doing this kind of thing by discussing an example of past history when they were accused of doing this, but were really actually strengthening the standard. He then went on to claim that:

> I have never heard of any proven weakness in a cryptographic algorithm that's linked to NSA; just innuendo.

This appears to be a denial of an NSA backdoor in the standard, while not saying so explicitly. If there is a backdoor, as most experts believe and the Snowden documents indicate, this was a fairly outrageous use of the AMS to mislead the math community and the public. At the time I argued with some at the AMS that they should insist that George address explicitly the question of the existence of the backdoor, but didn't get anywhere with that. One of their arguments was that George was speaking for himself, not the NSA.

The question of fact here is a very simple and straightforward mathematical one: how was the choice used in the standard of points P and Q on an elliptic curve made? There is a known way to do this that provides a backdoor. Did the NSA use this method, or some other one for which no backdoor is known? The NSA refused to cooperate with the NIST investigation into this question. The only record of what happened when the NIST asked about how P and Q were chosen early on in the development of the standard is this, which indicates that people were told by the NSA that they were not allowed to publicly discuss the question.

Remarkably, the latest AMS Notices has a new article with an extensive discussion of the DUAL_EC_DRBG issue, written by mathematician Michael Wertheimer, the NSA Director of Research. At first glance, Wertheimer appears to claim that the NSA was unaware of the possibility of a backdoor:

> With hindsight, NSA should have ceased supporting the dual EC_DRBG algorithm immediately after security researchers discovered the potential for a trapdoor. In truth, I can think of no better way to describe our failure to drop support for the Dual_EC_DRBG algorithm as anything other than regrettable.

On close reading though, one realizes that Wertheimer does not address at all the basic question: how were P and Q chosen? His language does not contain any actual denial that P and Q have a backdoor.

For a careful examination of the Wertheimer piece by an expert, see this from Matthew Green. Green concludes that

> … it troubles me to see such confusing statements in a publication of the AMS. As a record of history, Dr. Wertheimer's letter leaves much to be desired, and could easily lead people to the wrong understanding.

In a recent podcast on the subject Green states

> I think it's still going on… I think that the NSA has really adopted a policy of tampering with cryptographic products and they're not going to give that up. I don't think that this is a time that they want to go out admitting what they did in this particular case as a result of that.

Given that this is now the only official NSA statement about the DUAL_EC_DRBG issue, the Notices article has drawn a lot of attention, see for instance here. The Register summarizes the story with the headline NSA: So sorry we backed that borked crypto even after you spotted the backdoor.

The publication of the George and Wertheimer pieces by the AMS has created a situation where there are just two possibilities:

- Despite what experts believe and Snowden documents indicate, the NSA chose P and Q by a method that did not introduce a backdoor. For some reason though they are unwilling to state publicly that this is the case.
- P and Q were chosen with a backdoor, and the AMS has now repeatedly been used to try and mislead the mathematics community about this issue.

I've contacted someone at the AMS to try and find out whether the question of a backdoor in P and Q was addressed in the refereeing process of the article, but been told that they won't discuss this. I think this is an issue that now needs to be addressed by the AMS leadership, specifically by demanding assurances from Wertheimer that the NSA did not choose a backdoored P and Q. If this is the case I can see no reason why such assurances cannot be provided. If the NSA and Wertheimer won't provide this, I think the AMS needs to immediately cut off its cooperative programs with the agency. There may be different opinions about the advisability of such programs, but I don't think there can be any argument about the significance of the AMS being used by the NSA to mislead the mathematics community.

Update: There's an Ars Technica story here, with a peculiar update of its own:

> An NSA spokesperson emailed Ars on Friday to say Wertheimer retired in the fall of 2014 and submitted the article after he left his position. The *Notices* article made no mention of his retirement.

Another odd thing about the Wertheimer piece is that in a different part of it he seems to reveal what I would have thought the NSA considered a closely held piece of information about Taliban communication methods (see here). If he can discuss that publicly, why can't he say whether P and Q were backdoored?

Update: This is getting international attention, with le Monde reporting the AMS Notices piece as an admission by the NSA that they backdoored DUAL_EC_DRBG.

Update: The NIST has put out a revised draft on its cryptographics standards process and asked for comments. On the NSA problem, it says that no changes have been made to the NSA-NIST Memorandum of Understanding, and that

> cooperation with NIST is governed by an MOU between the two agencies and technical staff meet monthly to discuss ongoing collaborative work and future priorities.

It seems (see the NIST VCAT report) that, despite its obligations under the MOU, the NSA has refused to explain what it did with regards to compromising the DUAL_EC_DRBG standard, and experts believe (see above) that the NSA is committed to continuing to tamper with cryptographic products. Under these circumstances I don't see how the NIST can expect anyone to not be suspicious of their standards.

A promise is made to identify NSA contributions to standards, but a footnote says that names of some NSA staff cannot be revealed and that documents involving NIST-NSA collaboration provided in response to FOIA requests may be redacted. I don't see anything here that would keep the NSA from misleading or corrupting NIST staff to produce a backdoored standard, while keeping their input out of any record available to the public.

Comments

1. **Roger**
   January 19, 2015

   You admit that the AMS articles do not deny that the NSA could have a backdoor to DUAL_EC_DRBG. So maybe the NSA does have a backdoor, but the info is classified. The backdoor would not be a proven weakness unless the NSA leaked the data, or someone got it by solving a discrete log problem.

   Why are you blaming the AMS? It is just reporting the different views on this subject. If you do not believe that we should have a military intelligence agency doing work like this, then complain to President Obama, not the AMS.

2. **Peter Woit**
   January 19, 2015

   Roger,
   I think all the evidence is that there is an NSA backdoor to this standard, and that Matthew Green is right: they're not admitting this because they intend to keep compromising standards. If so, the two Notices pieces are intentionally misleading and the AMS is being used by the NSA to mislead the math community. This is not about "views on the subject", it's about what the facts of the matter are, and these are not letters to the editor, but refereed articles.

   Even those who have no trouble with the NSA backdooring NIST standards should not be happy with the AMS being used this way.

3. **guest**
   January 20, 2015

   "I think this is an issue that now needs to be addressed by the AMS leadership, specifically by demanding assurances from Wertheimer that the NSA did not choose a backdoored P and Q".

   Well, I think that if and whenever such assurances will be given, you should not trust them very much.

4. **Anonymous**
   January 20, 2015

   I don't understand what you consider misleading about Wertheimer's statement. Is it that you feel the use of "discovered" in the clause "after security researchers discovered the potential for a trapdoor" suggests that this was the first discovery of that potential? To me it doesn't suggest that. (For comparison, in his Notices article on this subject Hales refers to "the back door algorithm discovered by Ferguson and Shumow," and he certainly doesn't intend to suggest that NSA had previously been unaware of it. It would be overly fastidious to reply "No, NSA discovered it, while Ferguson and Shumow merely re-discovered it.")

   As I read it, Wertheimer's statement seems clear and unambiguous. He says it was a mistake for NSA to continue supporting the algorithm once the public knew it had the potential for a back door. That may be a cynical or distasteful statement, but I'm not convinced it was intended to mislead anyone.

5. **Peter Woit**
   January 20, 2015

   guest,
   Maybe I'm not cynical enough, but I suspect that (assuming the backdoor) the reason Wertheimer does not address the P,Q issue is that an out and out bald-faced lie to the math community is beyond what he is willing to stomach (and that he has plenty of mathematician colleagues who also would not stomach this if it was done by someone speaking for them). Besides that, the Snowden (or other) documents may very well contain the details of what happened and this may someday come out, providing some incentive not to put your name to a public lie.

   Anonymous,

   Taken as a whole, the article basically claims to address in detail the controversial issue of the accusation that they backdoored a standard, while cynically evading ever actually addressing the issue. I don't see any way to characterize this as other than misleading, and think it was a huge mistake by the AMS to allow this to appear in the Notices without requiring that the issue be addressed (even if by explicitly saying that the NSA refuses to answer the question).

   Beyond the evasion of the central issue, there's also a string of other misleading statements about the history of this issue, for the details of those, see Matthew Green's account (his conclusion "could easily lead people to the wrong understanding" is a synonym for "misleading").

   Did you read the le Monde article? If the Wertheimer statement was not misleading, why are they reporting that the Wertheimer article admits the NSA backdoored the algorithm (when it carefully does no such thing?)

   I still think that the way Wertheimer's statement is written, he encourages the "we should have stopped this once we found out there was a problem" reading rather than the cynical "we should have stopped this once we were caught" reading (the French are cynics..) . The ambiguity is intentional, misleading and the AMS should not have allowed this to happen.

6. **Michael**
   January 20, 2015

   Why should anyone be surprised? If they are willing to backdoor to begin with, why would they not continue to try to mislead the public? The whole point of backdooring is to be able to monitor communications without people knowing it. Surely their opinions on this haven't changed retroactively just because they got caught. So big surprise, he's being shifty and evasive. But unintentionally he is just making things clearer than before. If he had nothing to hide, he'd lay the truth out.

7. **Anonymous**
   January 20, 2015

   The CIA has a long and well known history of infiltrating, directing, and manipulating media sources for their own ends. One might expect the editors of AMS Notices would be aware of this history and be cautious about allowing themselves to be used as a conduit for such disinformation.

   http://en.wikipedia.org/wiki/Operation_Mockingbird
   After 1953, the network was overseen by CIA Director Allen Dulles, by which time Operation Mockingbird had major influence over 25 newspapers and wire agencies. The usual methodology was placing reports developed from intelligence provided by the CIA to witting or unwitting reporters. Those reports would then be repeated or cited by the preceding reporters which in turn would then be cited throughout the media wire services. These networks were run by people with well-known liberal but pro-American big business and anti-Soviet views such as William S. Paley (CBS), Henry Luce (Time and Life Magazine), Arthur Hays Sulzberger (New York Times), Alfred Friendly (managing editor of the Washington Post), Jerry O'Leary (Washington Star), Hal Hendrix (Miami News), Barry Bingham, Sr. (Louisville Courier-Journal), James Copley (Copley News Services) and Joseph Harrison (Christian Science Monitor).[6]

http://en.wikipedia.org/wiki/CIA_influence_on_public_opinion
In 2012, Tricia Jenkins released a book, The CIA in Hollywood: How the Agency Shapes Film and Television, which further documents the CIA's efforts at manipulating its public image through entertainment media from the 1990s to the present. The book explains that the CIA has used motion pictures to boost recruitment, mitigate public affairs disasters (like Aldrich Ames), bolster its own image, and even intimidate terrorists through disinformation campaigns. That same year CIA was portrayed by Aidan Gillen in the third installment of the Christopher Nolan's Batman film series.
USA 2014
The CIA worked with prominent national security reporter Ken Dilanian while he published articles for the L.A. Times to secure positively written stories informed by CIA narrative. [36] Specifically, Dilanian sent full stories to the CIA before they were published on at least one occasion and on at least one other radically rewrote a story on the CIA's urging. The CIA also encouraged a publication indicating few collateral deaths were associated with a strike from its controversial drone strike program in contradiction to eye witness testimony recorded by an Amnesty International investigation [37] and which also contradicts a documents obtained by a FOIA request indicating that acceptable numbers of collateral damage are pre-calculated for drone strike targets and while typically valued at around 10 innocent deaths can sometimes be significantly more. [38]

http://monthlyreview.org/1999/11/01/the-cia-and-the-cultural-cold-war-revisited/
U.S. and European anticommunist publications receiving direct or indirect funding included Partisan Review, Kenyon Review, New Leader, Encounter and many others. Among the intellectuals who were funded and promoted by the CIA were Irving Kristol, Melvin Lasky, Isaiah Berlin, Stephen Spender, Sidney Hook, Daniel Bell, Dwight MacDonald, Robert Lowell, Hannah Arendt, Mary McCarthy, and numerous others in the United States and Europe. In Europe, the CIA was particularly interested in and promoted the Democratic Left and ex-leftists, including Ignacio Silone, Stephen Spender, Arthur Koestler, Raymond Aron, Anthony Crosland, Michael Josselson, and George Orwell.

8. **Shannon**
January 20, 2015

Just to be clear, you say you "think the AMS needs to immediately cut off its cooperative programs with the agency," by this you mean that the funding program of the NSA/AMS
(for personal grants and optionally supporting a recipient's graduate student) should be discontinued, right? It is probably unusual for the AMS to say that they want to stop a funding opportunity offered by a government office, turning down money on moral grounds.

9. **Peter Woit**
January 20, 2015

Anonymous,
I don't think any of this is relevant here (and I really don't want to get involved in general discussion of the history of the activities of the CIA and NSA). In this case what is coming from the NSA is being published with their name on it, and to the extent it's misleading, it is so not surreptitiously, but out in the open.

The relevant historical analogy though may be that in the DUAL_EC_DRBG case the NSA took advantage of the NIST and its cryptographers to put out bad crypto, here they're taking advantage of the AMS and its editors to put out misleading information about the earlier NIST story. In the NIST case the problem was that NIST cryptographers didn't push back when being used by the NSA, here it seems the editors didn't push back.

10. **Peter Woit**
January 20, 2015

Shannon,
If the NSA really is using the AMS to mislead the math community, all I'm saying is that an appropriate response would be for the AMS to withdraw from cooperation with them. This doesn't mean shutting off the NSA grant system, it means the AMS should tell the NSA it doesn't want to lend its name to this or to any longer play a role in their administration. As far as I can tell, currently the main AMS involvement in the grants is in choosing review panels, so de facto the main change would be that the AMS would tell the NSA they had to do this themselves (like the NSF and every other agency that provides research grants to mathematicians).

11. **Fred P**
January 21, 2015

Presentation on the issue from 2007: "WHAT WE ARE SAYING: The prediction resistance of this PRNG (as presented in NIST SP800-90) is dependent on solving one instance of the elliptic curve discrete log problem. (And we do not know if the algorithm designer knew this before hand.)"

Blog post from a security blogger at the time: Even if no one knows the secret numbers, the fact that the backdoor is present makes Dual_EC_DRBG very fragile. If someone were to solve just one instance of the algorithm's elliptic-curve problem, he would effectively have the keys to the kingdom. He could then use it for whatever nefarious purpose he wanted. Or he could publish his result, and render every implementation of the random-number generator completely insecure.

12. **David Roberts**
January 23, 2015

The *Notices* article now says at the top of the first page:

> POST-PUBLICATION EDITOR'S NOTE: This article is a part of the ongoing series "Mathematicians Discuss the Snowden Revelations". At the time of the writing of this piece Michael Wertheimer was the Director of Research at the NSA; he recently retired from that position. He can be reached at nsapao@nsa.gov.

13. **Punished Gamer**
January 23, 2015

> P and Q were chosen with a backdoor, and the AMS has now repeatedly been used to try and mislead the mathematics community about this issue.

It's psyops from the start. Deny, evade, use the spiral of silence and make sure all affiliated publications stay on message. The goal is to outlast first outrage, then interest, and finally memory. When you see articles proclaiming things like "This NSA backdoor conspiracy again", you'll know they've moved onto the final stage: Mockery, painting anyone still concerned as a tinfoil hatter to discourage inquiry.

Psyops is not about argument. It's about "feels over reals". In the absence of clear evidence, and open debate, its easy to turn people away or off with distraction, confusion, and ridicule. Over time, and in the absence of any kind of investigative media, it will eventually work.

Elliptic Curve cyptography is done. No-one can trust the NIST standards, possibly their research, and more importantly no-one can trust **everyone** to not be using those standards. This isn't about trusting the mathematics, it's about trusting the implementations. Thos have been compromised and short of a total worldwide agreement to adopt new, compromise-exclusionary ones, ECC is not going to be able to be trusted by anyone.

And we can't have the agreement while the psy-ops campaign to shut down debate is in effect. RiP ECC.

14. **Carey**
January 23, 2015

I don't understand; or, rather, I think I do. This is a clash of cultures where there is an expectation that a mathematician is a mathematician is a mathematician, upholding certain ideals, wherever they happen to be sitting. This is an unreasonable expectation. The NSAs sole mandate is to intercept communications and decrypt them into plain text if needed. By definition it is their responsibility to facilitate, create, and guard backdoors an trapdoors in crypto when the opportunity arises. They are under no obligation to discuss it. People involved will be given National Security Letters (NSLs) and they will not be permitted to speak out it. However, it was total naivete to involve them from the get-go. Haranguing them for a mea culpa is a wast of time and is about as naive as the farmer wondering why that nice boy didn't just east as much corn as he wanted.

15. **tt**
January 23, 2015

Actually that is not their "sole mandate". They are supposed to also be protecting
US government communications. By introducing a backdoor (and assuming noone else would figure it out) they have weakened their own communications.

16. **Peter Woit**
January 23, 2015

Carey,
Like Wertheimer, you're ignoring the main point here. It's one thing for the NSA to act secretly and refuse to discuss it, that's what they do. It's quite another to instead go public, and do this by putting out misleading information via the the AMS.

I do think the AMS was naive here, with their conception that they are just hosting an exchange of people's viewpoints, and thus trying to get the view of someone on the NSA side was important. The problem is that the Wertheimer article is instead essentially an official response from the US government about a matter of fact, the only time it has been willing to address this matter of fact. Presented with such a document, the AMS should have realized it was in danger of being used, and insisted that the Wertheimer article address clearly the question of the backdoor instead of misleading about it.

17. **Carey**
January 23, 2015

@tt: good point, they do have precisely that dual mandate. I misspoke.

@ Peter: I don't think I am ignoring it. This was my point about the clash of cultures. Retired or not from NSA at the time of writing the piece, Wertheimer is a mathematician and, if you read some of his bio, pretty far from the stereotypical apparatchik. Maybe he just feels like discussing the issue with his peers. But, even if it was an intentional attempt to mislead the public, per your suggestion, so what? It seems to me that the issue was involving the NSA to begin with. I suppose I am not debating a fact but a matter of opinion. It doesn't bother me that they aren't coming clean or that they may be deliberately misleading the public. I expect that given their jobs, right or wrong. I do agree with you that the AMS has put itself in a funny spot by being a medium for the discussion.

18. **Peter Woit**
January 23, 2015

Carey,
I think it wasn't a mistake, but perfectly reasonable for the AMS to try and get a response from the NSA to the accusations about the backdoor. It's surprising they did get a response (and no, given NSA security, I don't think this came about because some guy just felt like talking about it, this was a policy decision, probably made at a very high level).

I seem to be less cynical than most people in that I'm surprised that someone at the NSA, given the chance to just say "no comment", would instead decide to write an intentionally misleading public statement. My suspicion is that that's not the way Wertheimer sees it, but that he's so used to the point of view that outsiders have no right to know anything that he doesn't realize what he is doing. It really was the job of the AMS to point it out to him and ask for either a real answer to the question or a clear statement that he wasn't allowed to provide one.

19. **Roger**
January 23, 2015

Peter, even if Green is right that the NSA plans to continue nefarious spy schemes, I don't see anyone being misled. As Fred points out, it has been known since 2007 that anyone with a solution to the EC discrete log problem can create a backdoor to the pseudorandom number generator, and that you can eliminate the problem by generating your own P and Q. The NSA is not saying whether it has that discrete log value or used it to spy on anyone.

I am not sure why it matters that the NSA spell this out for you. If you are concerned that the NSA is spying on your random numbers, then I suggest using your own P and Q or another generator, regardless of what the NSA says. You seem to think that the AMS needs to get some assurances from the NSA for you, but I do not see how any such assurances would do you any good.

20. **Peter Woit**
January 24, 2015

Roger,
The record shows that the people at NIST were misled about this (see the report of their investigation). The assurances I think the AMS now needs from the NSA are about whether they've been used to mislead the math community. To all appearances, this is what has happened, so the NSA owes both the AMS and the math community an explanation.

Snowpocalypse 2015

January 27, 2015
Categories: Uncategorized

For the last few days the media in New York have been filled with continuous frantic warnings of the deadly storm of the century bearing down on the city. Grocery stores have been emptied, with long lines of desperate people trying to stock up on supplies.

Midday yesterday Columbia announced that classes were canceled starting at 3pm, Barnard went one hour better, canceling classes starting at 2pm. The city announced that it would be illegal to be in the parks after 6pm (a snow-covered branch might fall on you), the transit system would start shutting down at 7pm and by 11pm there would be no public transit, and all roadways in the entire tri-state area would be closed to non-emergency traffic. The mayor's office warned people not to try and order takeout delivery since it would be illegal for the delivery people to travel on the streets to deliver it.

By late afternoon the university was deserted, and stores on Broadway had signs announcing early closing due to the impending disaster. Weather reports the day before had said the storm would start at 1pm Monday, but by early evening there hadn't been much more than snow flurries, with maybe an inch or two total accumulation. When I went to sleep around midnight, the city was completely locked down, with the TV news channels filled with blaring warnings of the two to three feet of snow about to arrive, interspersed with press conferences from public officials telling people to barricade themselves in their homes and not go outside.

The strange thing about this was that if you actually looked at the weather report, they were now forecasting 3-5 inches of snow overnight. Waking up in the morning and looking out the window, all that was visible were more flurries, and a total accumulation of 2-3 inches, with the streets clear. Turning on the TV news, the huge "Blizzard of 2015" logos were still up, and camera crews seem to have been sent out to search the region (mostly unsuccessfully) for a snow drift to put a reporter in front of. The contrast between looking out the window and watching TV was pretty dramatic.

Anyway, my class today is canceled, so students will have to wait until Thursday to hear more about the mathematics of quantization of the harmonic oscillator (complex structures, squeezed states, coherent states). Lecture notes still being worked on, but this is chapter 21 of the current notes.

Columbia never used to shut down at all, New York City never used to shut down the transit system, and the states never used to shut down all roadways. Until the past decade or so people tried to go about their business here in the winter, taking action to shut things down only once snow had arrived and was causing a problem. The US has now become a nation of hysterics, with media-driven hype frightening everyone about everything, and public officials desperately taking action to protect the citizenry from imaginary threats.

Luckily for us all, people cowering in their homes do have the internet and can still learn quantum mechanics. MIT has just announced that edX will have an online version of their quantum course, Mastering Quantum Mechanics, which looks quite good and will start February 10. The instructor will be Barton Zwiebach, and I'm glad to see that one of the topics covered will be squeezed and coherent states of the harmonic oscillator.

Comments

1. **ghassan**
   January 27, 2015

   Peter,
   The hysteric reaction to announced hypothetic threats is not a US-Only thing, You know France very well, and it is the same thing over here. The working principles of weather forecasters, politicians, … is that they will not be blamed for taking measures to protect against something that never happens, but they will be sued for not doing anything if the threat materialises. The much fabled "precautionary principle" (now part of the French constitution).

   rgds

2. **Joel Rice**
   January 27, 2015

   wow did you get that right. North of the city we got a few inches of snow, and no traffic outside. Nothing like some storms in the sixties with drifts blocking doors so we could not get out of the house ! There was some 'political' change at the weather channel, and they decided to go for this over-the-top style of 'reporting'.

3. **Shecky R**
   January 27, 2015

   Well, let's be blunt: generations today are "softer" than they used to be when it comes to putting up with temporary hardship! But, in fairness to government and politicians, I don't blame them for erring on the side of caution, given the absolute thrashing they will suffer whenever a true emergency or disaster arises, and lives are lost and well-being threatened. Our entire medical system is built on the same ongoing scheme, of outrageous costs, in order to curtail death and suffering whenever technically possible, instead of accepting it as part of life.

4. **Bee**
   January 27, 2015

   Meanwhile in Germany the hot topic this morning was the facebook outage…

5. **Jeff M**
   January 27, 2015

   Well, up here in CT we got dumped, almost 2 feet and still coming down. Right you are about hysteria though, up here everyone goes nuts in the grocery store. I'm like "it's New England, it snows 2 feet at least every couple of years." We got 3 feet two years ago. It really didn't used to be like this, even in the city. I remember the Lindsay blizzard, the blizzards in '78, the one in '96 when there were people skiing down Lorimer Street and my terrier mix kept disappearing in the snow in Lindsay Park. Subways didn't shut. Key Food was open. My wife works for a NYC company, which is of course shut. Her main office person in the city actually spent the night at the office, woke up to 6″ and was like WTF? My wife is of course working on the computer, I will light a fire later…

6. **Chris W.**
   January 27, 2015

   Peter, that URL bounces to MIT's main online course page. This link goes directly to *Mastering Quantum Mechanics*.

7. **Peter Woit**
   January 27, 2015

   Chris W.,
   Thanks, was missing an x, fixed.

   Jeff M.,
   I've heard the reports of 6″ here in the city. Always hard to tell with blowing snow, but up near Columbia I'd say it's more like 2″, and that seems typical for reports from areas north and west of here. The governor's decision to shut down the subway system (first time in history this was ever done because of snow) seems especially odd in Manhattan, where virtually all of it is underground. The subway has problems not when the snow falls, but when it melts.

   Bee,
   Mercifully the US seems to have escaped that disaster (not that I would notice…).

8. **Thomas Larsson**
   January 27, 2015

   Weren't some Italian scientists convicted of manslaughter because they didn't warn the public about some earthquake in advance? Better safe than sorry.

9. **David Appell**
   January 27, 2015

   "NYC Mayor: 'Reconcile Yourselves With Your God, For All Will Perish In The Tempest'"

   http://www.theonion.com/articles/nyc-mayor-reconcile-yourselves-with-your-god-for-a,37850/

10. **Jeff M**
    January 27, 2015

    Yeah, that Onion bit was great 🙂 Peter, I would believe N and W was less, I think some of the Central Island got a bunch of snow, my wife's company is in Greenpoint so more than 5 miles from Columbia. It is completely nuts they closed the subway in Manhattan, really any of the underground lines. They NEVER used to close the whole subway, though of course at times with say tons of snow the above ground lines would stop running for a while, or with flooding some of the underground lines would stop.

11. **CIP**
    January 27, 2015

    And when I was a kid we had to walk six miles to school through six feet of snow….

12. **Carey**
    January 27, 2015

    Onion is great re the blizzard…

    Blizzard Survival Tips

13. **Yaakov Baruch**
    January 27, 2015

    Awesome post! Similarly, I grew up playing with caustic soda, sulfuric and nitric acid, and lots of other lively chemicals, which I could buy at my local pharmacy. Sadly nowadays kids can only read (or watch videos) about this stuff. How sad (and not one iota safer).

14. **Harlequin**
    January 27, 2015

    I can see some benefit to shutting down before a storm, because there are situations that are less dangerous if people have already gotten themselves to shelter before the snow begins. (I'm thinking of the big blizzard in Chicago a few years ago where order a thousand cars ended up snowed in on Lake Shore Drive–the storm hit at the end of the workday, and the city chose to keep Lake Shore open instead of letting those ~1000 cars get snowed in on the surface streets where they'd block emergency vehicles. If there'd been an earlier snow emergency called, maybe more of those people would've gotten home; as it was, many stayed with their cars all night.) That doesn't explain why the emergency management team hadn't figured out by midnight that they didn't need to stay closed for the morning, though. Nor does it answer why the underground trains shut down, unless it was more incentive for people to stay inside…

15. **Peter Woit**
    January 27, 2015

    Harlequin,
    Yes, it would be less dangerous if everything shut down, everyone got off the streets and stayed home whenever a snow storm was coming. Since there are snow storms pretty regularly in this area from December through March, best way to avoid any danger would be to implement this December 1, let people come back out of their homes only on April 1….

16. **Neil**
    January 27, 2015

    "let people come back out of their homes only on April 1…."

    Just in time for April Fool's day. We don't need the LHC to find WIMPS.

17. **sandy**
    January 27, 2015

    It's easy to criticize and claim 'hype' but the memories of Superstorm Sandy are probably still fresh in the minds of the city leaders (that includes the police and other emergency workers). They're not taking any chances. They are wise not to.

18. **Peter Woit**

January 27, 2015

sandy,
“Don’t take any chances” does seem to be the new motto at work here (by the way, it’s also the motto at work in the NSA’s plan to collect all information about everyone, all the time).

It might have been a good idea though for someone to explain to the Governor the difference (re flooding) between a snowstorm and a hurricane before letting him shut down the subway system.

19. **sandy**
January 27, 2015

According to CNN Headline News, coastal flooding is now a problem (admittedly in Massachusetts, not NYC). I believe not all of the coastal structures in NY+NJ which suffered flooding damage due to Sandy have been repaired. I recall Chris Christie (last year?) complaining about the attitudes of some of the residents in those regions. Depending on circumstances, the flooding situation in NY/NJ could have been very different.

20. **Peter Woit**
January 27, 2015

sandy,

Among all the hysterical weather warnings for Manhattan the past couple days, I seem to have missed the ones about how the island was going to flood.

21. **sandy**
January 27, 2015

I have to deal with 2 feet of snow in my driveway. At least I didn’t lose heat and power, unlike back in 2012. Back in the good old days that everyone hankers for, there was no social media and blogs, indeed not even the internet. Today, the smallest incident (or mishap) can or will be posted online and possibly go viral. The city leaders, and those in other regions also, have to operate under that scenario.

22. **Harlequin**
January 27, 2015

We can (and apparently do) disagree on what level of danger is necessary before steps are taken. But there’s a time lag in getting large groups of people to do stuff, like evacuate or get home, and so you can’t always wait until the storm hits before you start asking people to get off the roads. It’s mostly that point that I was responding to.

It’s about a balance of risks; I think a foot-plus of snow dumped on the city in a short time would have been dangerous, and taking precautions in that very rare case (even though it didn’t end up happening) isn’t unreasonable. Again, the subway, possibly unreasonable–as I also agreed in my first comment. And I wasn’t putting danger before everything else, what with my “they should have turned everything back on once they realized the storm wasn’t so bad” bit at the end. Obviously, it’s your blog and you can say what you like, but it seems a bit weird to take my point to an extreme illogical end that’s been refuted by the stuff I already wrote.

(And my perspective may also be informed by having grown in up the Midwest, where many of the highway ramps have gates that can be closed in case of snow emergency, and schools build in an extra week or two to the calendar because the expectation of snow days is so high–your statement that “the states never used to shut down all roadways” is totally foreign to me. It wasn’t common, but it did happen. That said, the university in my hometown has closed a few times in the last few years. It never used to, even when everything else shut down.)

23. **Peter Woit**
January 27, 2015

Harlequin,
Apologies for uncharitable comments. Looking out my window at the beautiful day, snowless streets, and locked up University may be making me kind of crazy…

My understanding is that announcing a closing of all roads over the tri-state area, many hours in advance, at a time when there was no snow, was completely unprecedented for this area. This wasn’t a case of prudent closing of highways when a blizzard starts.

Manhattan really is a different kind of environment than the rest of the US. People don’t have cars, and even when a foot of snow falls (not uncommon), sidewalks quickly get shoveled so it’s not hard to get around by foot and public transit. There are only a few arteries buses travel on, and those get plowed quickly. Shutting down the subway and bus system immediately brings the place to its knees: most people can no longer get to work or conduct their business, and everything has to shut down. This has never been done before in the history of the city because of a snow storm, I think what happened is just completely outrageous. The media and politicians however are busy telling themselves what a great job they did, how this had to be done in order to “not take chances”, and I fear the obvious lesson is not going to be learned.

24. **Peter Shor**
January 27, 2015

On the other hand, here in Massachusetts, I easily have two feet of snow in my yard. The city of Framingham, not very far away, had 2.5 feet early this afternoon, and I think nearly six inches have fallen since then. So the total snowfall amounts here are comparable to the Blizzard of ’78, but the aftermath is going to be much less costly because everything was shut down.

25. **CIP**
January 27, 2015

The only thing that will inspire more whining in New Yorkers than a real disaster is narrowly missing a predicted one. This was a really big snowstorm, and NY was lucky enough to only catch the edge of it. And Gloria would have been a really disastrous hurricane for NY if it had deviated a degree or two from the trajectory it actually took.

26. **sandy**
January 27, 2015

Long Island is not so far from NYC (Queens, anyone?), and some parts received substantial snowfall. A small difference in the track of the storm could have made a substantial difference to NYC. You may need a subscription to read this, but anyway there was quite a bit of snow at MacArthur airport on LI.

http://www.newsday.com/news/weather/travel-ban-continues-for-east-end-towns-as-snow-continues-to-pile-up-1.9853573

The National Weather Service reported that as of 1 p.m. 24.8 inches of snow fell at Long Island MacArthur Airport. That is second only to the 27.8 inches that fell there Feb. 8 and 9, 2013, according to records the weather service has kept since 1984.

27. **srp**
January 27, 2015

I am glad that our host has recognized the excessive risk aversion that has infiltrated almost every aspect of public policy over the last thirty years. You could fill a large volume with scientists expressing laments along the lines of Yaakov above. You could easily do the same with even late baby boomers and early Gen Xers marveling over the new terror for children going about and playing without adult supervision, along with amazing horror stories of parents being pulled in by police because their ten-year-old was playing alone in a park or even outside their house.

That's without even getting into scientific-regulatory issues about radiation exposure, GMOs, drug approvals, etc. (I remember reading Crease and Mann's The Second Creation about the history of particle physics where a then-contemporary physicist had a good laugh at their expense when they proposed recreating Rutherford's experiment that discovered the nucleus–it would have been regulatorily impossible to do that experiment in New York City in the 1980s.)

Anyway, not trying to start a big woolly political debate of the kind Peter hates, just expressing some sympathy for his frustration with the snow-phobia.

28. **Nobody**
January 27, 2015

It's a step in the asinine infantilization of adults, which we seem to like, because we keep electing would be mommies and daddies. I can only shake my head at those here who are actually justifying it.

29. **Peter Woit**
January 27, 2015

srp/Nobody,
I do detest ideological discussions, and in this case I just don't see this as a right/left issue. Closing down the area was a joint Christie/Cuomo effort and, as a general rule, conservatives are every bit as willing to become hysterically fearful about things as liberals. This unfortunately seems to be a bipartisan trend. Not sure who to blame, one can always blame the media…

Peter Shor/sandy/CIP,
I've been in New York several times when it got two feet of snow, and honestly it just wasn't a big deal, no "disaster" at all. There were some subway delays, streets in the outer boroughs took a while to clear. Nowhere near the impact this two inches has had.

And, as I pointed out in the posting, at the point the hysterics shut everything down in the city last night, weather.com was predicting 3-5 inches of snow….

30. **Michael**
January 27, 2015

If the storm's track were 50 miles to the west of it's actual track, the snowpocalypse would have happened. They simply don't have the ability to predict storm paths with extreme accuracy with current technology. All they knew is that there was a chance of a major snowstorm. Given the problems that would arise if the storm did hit New York City, it was prudent to prepare for the storm as people did.

31. **Anonyrat**
January 28, 2015

The Weather Channel was quite clear about the differing predictions of three major weather models.

And this is what happened in February 2014:
http://www.cnn.com/2014/02/13/us/nyc-deblasio-schools/

> New York Mayor Bill de Blasio, who took office 44 days ago promising to bring together residents of a divided city, **was widely vilified Thursday for keeping the nation's largest school system open during a brutal storm expected to leave up to 14 inches of snow in some areas.**
>
> While millions of children in the region were given the day off, New York City public schools — with 1.1 million students — remained open, triggering an avalanche of anger from many students, parents and even one well-known weather anchor.
>
> "It's always a tough decision based on imperfect information," de Blasio told reporters late Thursday morning.
>
> The mayor said the National Weather Service reported as little as 3 inches of snow on the ground at the start of the school day, with warmer conditions than in previous storms. Since 1978, he said, New York City schools have closed due to snow just 11 times.
>
> "At the time," he said, "we thought our children would be able to get to school safely."

32. **Anonyrat**
January 28, 2015

See above. So my response to "The US has now become a nation of hysterics, with media-driven hype frightening everyone about everything, and public officials desperately taking action to protect the citizenry from imaginary threats" is that it is the people who have become hysterics, the media is merely a reflection of the people, and public officials are responding to brutal criticism in the past.

33. **Rehbock**
January 28, 2015

I have sympathy for public officials. Damned if they do and don't. But you have it right that the nation has become hysterical over imaginary threats.

34. **PaulS**
January 28, 2015

I'll go the December 1 – to – April 1 thing one better.

If the snowstorm *had* materialized, it's at least conceivable that somewhere, somehow, at least one person might have managed to kill him- or herself. It would have been an accomplishment, since the city is chock full of buildings and suchlike, making it hard, for example, to get well and truly lost in any weather conditions whatsoever – unlike the Midwest where I now live, which has very large open or wooded areas.

But never mind. After all, we're freaking out over vague hypotheticals here, solely for the sake of Nielsen ratings, and the careers of braying, self-aggrandizing hucksters and drama queens like Andrew Cuomo (who takes after his father in that respect.) Remember too that back when the City *didn't* close down completely, going out was nonetheless *voluntary*. Basic liberty has been another casualty of mindless hysteria, and not just with snowstorms.

Anyway, so we're worried out of our minds over just one purely hypothetical (and foolish) person. So let's not forget – an *actual, non-hypothetical* person is murdered in the City, and another dies in traffic, nearly every single day. It follows inexorably that only valid "precautionary" solution consistent with the actions of the politicians on Monday and Tuesday is simply to lock the City down from December 1 to December 1 rather than April 1, i.e. permanently. Only that way can the mindless hysterics be truly "safe".

Come to think of it, permanent lockdown doesn't seem too far from what those bossy "precautionary" hysterics are really after.

35. **Kevin NYC**
    January 28, 2015

    I blame the media! And, in a way, nature itself. Only a small group is in favour of blizzards and hurricanes and tornados. Most people don't want the bother or the risk, and so if you attack these acts of nature, they have no constituency to protect them. This is great for a politician, because usually whatever he does he will.have active resistance.

    Now we have a backlash because nothing bad happened. This is preferred to the other kind of backlash, when a politician is blamed for something bad.

    Oh well.. the funny part is that Coumo closed the Subways without even talking to DeBlasio. 15 minute heads up… haha..

36. **Visitor**
    January 29, 2015

    Living in Manhattan, I find the fact that the subways were closed to be a source of personal embarrassment.

37. **Bozo**
    January 30, 2015

    Growing up in Urbana, Illinois, every year we'd put chains on the car for at least a week, usually two. Nowadays I am pretty sure most people in Urbana don't even own a set of chains. And don't have snow tires either. Yet everyone survives, except for those in SUVs who have watched too many commercials and think they can fly through the air, and can round corners at high speed without rolling, just because they have 4-wheel drive.

    Is it because the winters are so much milder (which is in general certainly true- a snowman built this year lasted only a week, and when I was a kid we'd have snow piles until April sometimes), because of front-wheel drive, or better tires, or because
    the road maintenance is more effective?

    Right you are, Peter, about the over-protection in all things. The same goes for
    speech, where you can't risk offending anyone. And yet there is something schizophrenic in American culture: at the same time, Bush et al revelled in making it ok to say "kill", and delighted in starting unnecessary wars and in implementing actual gulags complete with torture spas for the visiting tourists. At least presumably they weren't being called names while undergoing waterboarding.

    To resolve this dichotomy in the future, I suggest that next time mayor pay Chris Kyle to station himself on top of the Dakota and take headshots at anyone caught cross-country skiing in Central Park. A very non-wimpy way of protecting the citizenry from nature!

38. **Alan K**
    January 31, 2015

    I lived practically all my life in Wisconsin (I am 44). The last few years I have lived in North Jersey. It was comical the other day when I went to the grocery store to pick up a single item, I could not believe the hysteria of people clearing off the shelves. I just shook my head. Mob mentality.

    When I attended high school in the eighties in Wisconsin, in the winter it was just a given to wake up a bit early and go out and shovel the snow (often well over a foot of snow). Then, yes, I actually walked to school in the snow. And weeks on end it would be below zero. It was the norm. It was life. School was rarely canceled for snow and never canceled for cold. They have these things in New Jersey out here called "delayed openings" for school. HAHA. Things have really changed over the decades.

    Alan

39. **srp**
    February 3, 2015

    Apparently Chicagoans still know how to deal with mega-blizzards. They didn't shut down their public transit system despite this:
    http://www.huffingtonpost.com/2015/02/02/midwest-blizzard-2015-photos_n_6596072.html

Mathematics Without Apologies

January 27, 2015
Categories: Book Reviews

If you'd asked me ten years ago to describe a book I'd love to read that could be characterized as part of an "incredibly unlikely trend in books about math for the general public", I might have chosen "brilliant meditations on the practice of mathematics and on mathematics at the deepest level, from first-rate mathematicians, focusing on the Langlands program, with expert-level discussion of the subject." And yet, here we are, not much more than a year after Edward Frenkel's Love and Math, with the publication last week of another very different but equally fascinating example of exactly this trend: Michael Harris's Mathematics Without Apologies. If you are interested at all in what mathematics really is and what the best mathematicians really do (and you're up for an intellectual challenge) I highly recommend that you get a copy and set some time aside for delving into this unusual book.

While Harris shares many of Frenkel's themes and concerns, his style is very different, favoring density, indirectness, the post or post-post-modern, and deep engagement with history, philosophy and sociology. Only one of these two authors assumes a familiarity with Max Weber. Where Frenkel is ever guileless and straight-forward, Harris has a whole chapter on the "trickster", taking some pride in being known for "Harris's tensor product trick." While reading, more than once one wonders whether one is really supposed to take something seriously (for instance, there's quite a long bit about Thomas Pynchon's novels and conic sections…).

Normally when I'm reading a book I want to later write about, my practice is to fold down the corners of pages that contain something new, unexpected, especially insightful, or something I'd really like to argue with. Then I can start writing by reviewing those pages. My problem with this book is that I ended up folding down the corners of a large fraction of the pages, so when I sat down to write, my usual method would force me to reread pretty much the entire book. Not a bad idea, since I'm convinced I missed a lot the first time through, but other tasks beckon and it's not a quick read.

I'm not sure I can do much better here than randomly list a few of the themes of the book: the pleasures of doing mathematics, the role of pure mathematicians in society (Wall Street!) and many forms of art and culture, how best to explain number theory to an insightful actress, the philosophy of mathematics and philosophy of Mathematics (two different things), Indian Metaphysics, n-categories, the yoga of motives, Voevodsky's univalent foundations, the life and thought of Alexander Grothendieck and Robert Langlands, etc., etc. There's also serious doses of sex (including an extensive discussion of Frenkel's film), drugs (from Erdos to Andreas Floer to late nights at Oberwolfach) and rock and roll (from the "Math Rock" genre which I'd never heard of before to the IAS house band "Do Not Erase").

Harris manages to move back and forth between the deepest ideas about mathematics at the frontiers of the subject, insightful takes on the sociology of mathematical research, and a variety of topics pursued in a sometimes gonzo version of post-modern academic style. You will surely sometimes be baffled, but definitely will come away knowing about many things you'd never heard of before, and with a lot of new ideas to think about.

For some more about the book, including some early versions of some chapters, see Harris's website here.

Update: Princeton University Press now has a Q and A with Harris about the book up here.

Update: The book now has a blog.

Comments

1. **Shecky R**
   January 27, 2015

   Good to hear… I just saw this volume in a bookstore last weekend (having seen no pre-publicity for it?) and, leafing through it, thought it looked darn good… and different. Hope to start reading it next week, and now even more anxious to do so!

2. **Ninguem**
   January 28, 2015

   I was never sure what was going on with this book. It is serious or a parody (like in the Pynchon chapter you mention) and we're being made fun of? Does he dislike people who make tons of money or is Jim Simons OK? What about the quants, are they cogs in the machine or morally responsible? And what about Frenkel? I don't think he likes Frenkel.

3. **Peter Woit**
   January 28, 2015

   Ninguem,
   I don't think it's ever a parody, mostly it's completely serious, but not always. Harris is not a black and white thinker, and has a well-developed sense of humor. I can't speak for him, but I can assure you it's possible to have a more complicated view of Jim Simons and the money coming into math research from the finance industry than just "it's bad" or "it's good". I don't see any evidence at all that he dislikes Frenkel. In terms of his mathematical research I know he's quite interested in geometric Langlands and the things Frenkel has worked on.

4. **Peter Woit**
   January 29, 2015

   Michael Harris has told me that a series of questions and answers about the book will soon appear at the Princeton University Press blog site. One of them addresses Ninguem's question:

   3. The text refers to any number of controversies and polemics, historical or contemporary. But the author doesn't come down clearly in favor of a solid position on anything. Is this a "postmodern" book? Or does the author just not care?

   I am certainly opinionated about a great many things, and it is my considered opinion that most of the sharpest controversies — like platonism vs. nominalism, or positions on what Wigner called "the unreasonable effectiveness of mathematics" — miss the features that make it really interesting to be a mathematician. To avoid distracting the reader with pointless polemics, I consciously chose to present those features with a minimum of ideological adornment, and to allude to controversies only obliquely. I'm told there's a risk that some will find it disorienting to read a book about mathematics that doesn't tell them what to think; but it's a risk I'm willing to take. [Regarding the controversy over the role of mathematics in finance: the allusions in the book are admittedly hardly oblique, but I tried to limit myself to reporting positions that have already been expressed in the press. I have my own opinions, of course, but why should anyone care what I think?]

5. **Art Brown**

January 31, 2015

It appears Harris' title is referencing (refuting?) Hardy's famous essay. Is there more to the relationship than the cover?

6. **Peter Woit**
   January 31, 2015

   Art,
   Yes, it's definitely a reference to the Hardy book, and there's quite a lot of discussion of the Hardy book in Harris's. In some sense, the Hardy book (while very different) is the closest historical analog of this one, in the sense of having many of the same themes: why mathematicians do mathematics, the role of mathematics in the wider culture, etc.

7. **Art Brown**
   February 1, 2015

   Thanks very much. It's even nicer to have two treats in store. (I never got around to reading Hardy; I see it's now free.) Although with your notes and the MIT QM course life will be busy…

8. **Pete**
   February 4, 2015

   Thanks again for reviewing another popular math book.

   Love and Math is one of the best books on the subject I've read in several years, especially since Frenkel touches on ideas of mathematical realism and maths amazing ability to uncover the deepest structures of our universe. Now that you say this one is on par with it, I'll definitely have to make a stop at the local bookstore.

It Really Is Just Dust

January 29, 2015
Categories: Uncategorized

The Planck collaboration has an inimitable way of releasing important new results, they like to do it in French (see here for instance). Tonight a French Planck website contains the long-awaited news of the results from the BICEP2/Keck/Planck collaboration to reanalyze the BICEP2 data on polarized B-modes, in a way that allows proper estimation of the contribution of dust. The bottom line is that the BICEP2 claims of seeing a primordial r=.16-.20 that got a huge amount of media attention last year have been shot down. The new analysis says that r is less than .13. I don't see a paper yet, rumor is that the paper will be on the arXiv Monday night.

My understanding is that full polarization results from Planck's own data are still not quite ready, as has been the case for quite a while now.

Hat tip for this news to Steve (retired LLNL physicist). Thanks Steve!

Update: The Planck collaboration now has information available about this (in English!). An NSF version is here, NBC News here. It still looks like we'll have to wait until next week for the paper, which has been submitted to PRL.

Update: The paper is now available here.

Comments

1. **martibal**
   January 29, 2015

   We are not so far from the 200th anniversary of the Waterloo's battle. This is a mark of resistance against the linguistic domination of the Perfide Albion !

2. **JG**
   January 29, 2015

   Google Chrome browser it offers automatic translation

   > It turns out that the part of the dust had been significantly underestimated.
   >
   > Once the part of the galactic emission subtracted correctly, there is always an excess but it is too small for this to be considered a detection and could be the result of simple changes associated with experimental sounds.

   Not a perfect translation, but you get the gist

3. **Sakura-chan**
   January 29, 2015

   Off-topic, but pretty big piece on Zhang in the New Yorker.

   http://www.newyorker.com/magazine/2015/02/02/pursuit-beauty

4. **BMc**
   January 29, 2015

   " D'après les modèles à leur disposition, la collaboration BICEP estimait que cette part était négligeable, tout au plus très faible. Avec une seule fréquence d'observation, ils n'avaient pas d'autre possibilité pour évaluer le signal d'origine galactique…"

   A tiny bit of sarcasme there?

   I'm trying hard to laugh about this whole situation. But I can't.

5. **Peter Woit**
   January 29, 2015

   My own translation of the last part:

   > The spatial distribution of the Planck 353 GHz signal shows up in the 150 Ghz map of BICEP2 and of KECK: these last two thus contain a non-negligeable contribution from galactic dust. Once the galactic dust emission is correctly subtracted there is still an excess, but at the present time too small to count as a detection and could be the result of simple fluctuations due to experimental noise.
   >
   > The parameter r measures the size of the signal produced by primordial gravitational waves. Today's limit is a bit less strong because there is a small excess: too small to be a measurement of anything, but sufficient to weaken the limit [from r=.11 without the BICEP2/KECK data to the new r=.13]

6. **West**
   January 29, 2015

   Looking at the likelihood(?) plots at the bottom of the page, the K+P and B+P curves have some pretty dramatic separation. As the Keck array was supposed to be noticeably more sensitive than BICEP2, I would go put money on a small value of r as suggested by the red curve.

7. **Peter Woit**
   January 29, 2015

   Just noticed that I hadn't clicked on the "Expert" level to see the plots that West is referring to. One needs to be sure to do that.

8. **JG**
   January 29, 2015

The “expert” page makes it clear that we haven’t really got that much more certainty on this than what was known since last year.

> What is certain today is that the primordial origin signal is not at the level previously estimated – at most about half. Given the instrumental uncertainties (remember that the signal is incredibly weak!), We can not say more today. It will take further measures of extreme sensitivity at other frequencies to better understand the question …

So 700 million euros wasn’t enough for the Planck mission to decide this one – maybe that’s why they prefer to release updates on obscure french wep pages rather than youtube and a harvard-like press conferences

Results from POLARBEAR, Spider, BICEP3 etc will decide this I guess

9. **Physicsphile**
January 29, 2015

JG,

Planck may not have been able to significantly constrain primordial gravitational waves but they have had some other very useful results. For example they have shown that the non-gaussianity parameter is very small which rules out many proposed inflation models.

10. **AW**
January 30, 2015

Dear Peter,

I think the heading of your blog post is a bit strong:

“It really is just dust”

You yourself just quoted from the Planck website in your own translation:

“Once the galactic dust emission is correctly subtracted there is still an excess, but at the present time too small to count as a detection and could be the result of simple fluctuations due to experimental noise.

The parameter r measures the size of the signal produced by primordial gravitational waves. Today’s limit is a bit less strong because there is a small excess: too small to be a measurement of anything, but sufficient to weaken the limit [from r=.11 without the BICEP2/KECK data to the new r=.13]”

This statement is inconsistent with saying “it really is just dust”. For that I would require the analysis to show the absence of any residual excess at a level of $r < 0.01$ with a least 3 sigma significance.

This not the case. On the contrary, the statement on the Planck website might very well be construed to imply a residual excess (with definitely $r > 0.01$ given their current sensitivity) at 1-2 sigma level (this would fit with their notion, that the excess, while there, might still very well be a fluke). So I think your title is misstating their results and hence too strong.

11. **Anthony Reynolds**
January 30, 2015

I went to a colloquium on Wednesday given by Paolo de Bernardis at LMU-Munich, and Slava Mukhanov worked hard to get him to say that BICEP2 was wrong. He finally did. But he also said that Planck would publish something in February (he didn’t say when in February), and he wouldn’t say what those conclusions would be. I guess that might be the arXiv paper “due out” on Monday.

http://www.physik.uni-muenchen.de/aus_der_fakultaet/kolloquien/asc_kolloquium/archiv_wise14/bernardis/index.html

12. **chris**
January 30, 2015

The link points to an empty page. Have they taken the results down again?

13. **peppe**
January 30, 2015

The cached page https://webcache.googleusercontent.com/search?q=cache:http://public.planck.fr/resultats/253-la-reponse-de-bicep2-keck-planck

14. **Sesh Nadathur**
January 30, 2015

Planck people I spoke to this morning were surprised to hear that the results had been announced already. I suspect it was unauthorised jumping of the gun, which is why they have been taken down. The paper has already been submitted to a journal a couple of weeks ago, and will be on the arXiv early next week, followed a few days after by the Planck 2014/2015 release papers (or so I’m told).

15. **stringph**
January 30, 2015

So if ‘It’ = ‘what caused the BICEP collaboration to claim a detection’, then the new results show ‘it’ was exactly dust. If the dust had not been there (or had been accurately subtracted) the measurements would be a lot smaller and they could not possibly have claimed detection.

16. **martibal**
January 30, 2015

According to a cosmologist friend, it seems that someone has given to people outside the collaboration the “secret” URL of the non-yet public and still in preparation Planck page on the Planck/BICEP analysis, in violation of all ethical rules. Unless the page has been cracked. In any case it seems that the page was not “searchable” and the URL complicated enough not to be randomly found. This friend ask people not to sprea
d the google cache page.

17. **Peter Woit**

January 30, 2015

AW,
I don't think it's misleading. It's a null result, definitively showing that the original BICEP2 claims were wrong, and that's the obvious meaning of the title.

Your .01 criterion is randomly chosen. Of course all one can do here is put bounds (unless you have a real observation). You can't claim evidence for something based on 1-2 sigma, especially something extraordinary ("extraordinary claims require extraordinary evidence"). Claims from BICEP2 that "maybe we really did see something and weren't wrong" are what would be misleading.

18. **Peter Woit**
January 30, 2015

martibal,
One of many odd things here is that if this was a page not intended for the public you would normally make that clear by putting something at the top indicating that. It looked exactly like an intended official announcement, and that site is exactly the one used last year by Planck as the only place for official news from the collaboration.

If it had been clear that was not meant to be public, I would have not linked to it (although I might have mentioned "rumors about r <.13"…..)

19. **AS**
January 30, 2015

I have a paper comparing my predictions for r and ns to the global fit of Planck, Bicep and Keck. Should I cite a web page disappeared even from webcache or wait?

20. **Peter Woit**
January 30, 2015

AS,
You could cite various tweets on Twitter, a BBC news story, or maybe this blog…

21. **Sasha**
January 30, 2015

Peter

What other projects associated with the primordial gravitational waves will produce results this year?

22. **JG**
January 30, 2015

AW

[ESA seems to support the wording in Peter's headline]()

23. **Peter Woit**
January 30, 2015

Sasha,
I know little about the current state of other CMB experiments, perhaps someone better informed can comment about what to expect.

24. **JG**
January 30, 2015

[Paper released]()

(and to clarfiy my above comment, I meant that the ESA public announcement implies that the entire signal can be attributed to dust – which is what I interpreted Peter's headline to mean – since obviously he isn't making a prediction about the entire future of observations in this field)

25. **West**
January 30, 2015

@Sasha: SPIDER just finished its flight down on Antarctica. The collaboration has a nice set of blog entries about the balloon flight. https://spiderontheice.wordpress.com/

26. **Mark**
January 30, 2015

Has there been any commentary from the BICEP2 team about their flawed initial analysis? In other words, is anyone eating crow and acknowledging the mistake in their – shall we say "over exuberant" – analysis?

27. **West**
January 30, 2015

@Mark: At least from the quotation from Kovac given to Nature News, not really. http://www.nature.com/news/gravitational-waves-discovery-now-officially-dead-1.16830

I find the quotation "These [original dust maps] seemed to indicate that the region of the sky chosen for our observations had dust polarisation much lower than the detected signal" rather problematic. After the March announcement there were a number of papers showing that a careful analysis using the preliminary dust maps (Planck's?) meant there was a sizable microwave foreground from dust. Did anyone go back and dig through the other noise models and quantify their actual uncertainty?

28. **B**
January 30, 2015

The New York Times now has an article "A Speck of Interstellar Dust Rebuts a Big Bang Theory" quoting Dr. Clem Pryke of BICEP "We can't say with any certainty whether any gravity wave signals remain… Obviously, we're not exactly thrilled, but we are scientists and our job is to try and uncover the truth. In the scientific process, the truth will emerge."

[http://www.nytimes.com/2015/01/31/us/a-speck-of-interstellar-dust-rebuts-a-big-bang-theory.html?hp&action=click&pgtype=Homepage&module=first-column-region&region=top-news&WT.nav=top-news]

29. **Sasha**
January 31, 2015

West,
I know about SPIDER. But I wonder when the results will be available? Anyone heard the news or rumors about Bicep3? Will there be more results from Keck and Polarbear?And whether or not to hope for a positive result?

30. **Jim Akerlund**
February 3, 2015

Here is the arXiv version 1502.00612.

Short Items

January 29, 2015
Categories: Uncategorized

- Science magazine this week has an article and a podcast about the NSA and the AMS. AMS president David Vogan is portrayed as outraged at the NSA's misuse of mathematics, but without much support for doing anything about it:

  > But after all was said and done, no action was taken. Vogan describes a meeting about the matter last year with an AMS governing committee as "terrible," revealing little interest among the rest of the society's leadership in making a public statement about NSA's ethics, let alone cutting ties. Ordinary AMS members, by and large, feel the same way, adds Vogan, who this week is handing over the presidency to Robert Bryant, a mathematician at Duke University in Durham, North Carolina. For now, U.S. mathematicians aren't willing to disown their shadowy but steadfast benefactor.

  Two odd things from the piece and the podcast:

  1. The NSA budget is highly classified. Essentially nothing is known about it, with estimates of its total ranging from \$8 billion to \$25 billion/year (by the way, can anyone tell me why that number is a secret?). Precisely one line item in their budget is publicly reported: the \$4 million to the AMS-administered grant program. The AMS seems to be the only organization in the world that the NSA has a publicly disclosed relationship with.
  2. The reporter said he tried but was unable to get in contact with Richard George, the ex-NSA person who published a piece in the Notices claiming the NSA backdoor was "just innuendo".
- On a much more positive note, this week the New Yorker has a really wonderful piece about Yitang Zhang.
- There's an interview at the Huffington Post with Lenny Susskind about The Future of Physics. It looks like his point of view is that there is no known alternative, no matter what happens at the LHC, that fine-tuning is evidence for the multiverse is evidence for string theory. The only alternative now: hope for an unforseen surprise.

Update: Paul Frampton tells me that he has published a book telling the story of what happened to him. It's now available on Amazon.

Comments

1. **Als**
   January 29, 2015

   It seems that Susskind's interview is from 2006.

2. **Peter Woit**
   January 29, 2015

   Als,
   Thanks Als, I should have seen that. Interesting (or sad…) that nine years later it seems as current as then. In 2006 I guess there was more reason to "expect the unexpected" at the LHC.

3. **Richard**
   January 29, 2015

   David Vogan was a truly decent and admirable person (as well as being a tremendously talented and generous mathematician) when I knew him at MIT in the 1980s and it seems that some things do not change.

4. **Yatima**
   January 29, 2015

   by the way, can anyone tell me why that number is a secret?

   Because knowing the actual budget of a bureaucratic outfit is like knowing someone's True Name: It gives you power of it. That cannot be allowed to happen. So it's somewhere in this 52 billion lump sum (extra expenses are, I suppose, excluded because this sounds suspiciously low to me).

   Related reading: A psychological history of the NSA

5. **Anon**
   January 30, 2015

   Here's almost everything you wanted to know about the intelligence community budget. The justification for keeping it such a safeguarded secret is so that weaknesses aren't found and exploited in various programs.

   http://cryptome.org/2013/08/spy-budget-fy13.pdf

6. **Anon**
   January 30, 2015

   The relevant slides appear to be p. 84 for a budget of around 12 billion and page 159 with a listing for cryptologic math and IDA research

7. **Oldster**
   January 30, 2015

   The following exchange is part of that Huffington Post interview with Susskind in 2006:

   Odenwald: If string theory loses its experimental support at the LHC, wouldn't it be far worse than merely going back to cosmology circa 1975 or even 1965? We would have to question the very mathematical tools we have been using for the last 50 years!

   Susskind: I agree with your analysis, except that I would add: Expect the unexpected. Unforseen surprises are the rule in science, not the exception. Remember: Stuff happens.

   That "50 years" comment jumped out at me, and I notice Susskind seemed to agree with it. I wonder if they were both thinking that quantum field theory itself could face new doubts in that case. I'm old enough to remember when Dirac stated doubts about QFT …

8. **tt**
January 30, 2015

Frampton missed an opportunity. He should have called it
"Frampton comes online"

9. **Michael Hutchings**
January 30, 2015

I question the statement "Ordinary AMS members, by and large, feel the same way" (about not making statements about the ethics of the NSA, let alone cutting ties). How about a survey or a poll? I imagine that those who receive financially support from the NSA would like to continue to do so, but this is probably a small fraction of academic mathematicians, and I would expect many of the rest to have serious ethical concerns (if they are paying attention at all).

10. **Grad Student**
January 30, 2015

Yes, the fact that Susskind's remarks are basically the same today show how physics/string theory has stalled.

Susskind, Frampton, et al. definitely had a lot more hope of "Seeing something unexpected" a few decades back:

https://www.youtube.com/watch?v=y7rFYbMhcG8

🙂

11. **Peter Woit**
January 30, 2015

Michael,
I have no data at all to support this, but my guess from talking to people is that if you remove the "I don't care" majority, sentiment among mathematicians who do care is much more heavily on the anti-NSA side. Even among those with NSA grants you might find some who don't see why the AMS is involved in this (as far as I know there's no good reason for that program to not just be run by the NSA, leaving the AMS out of it).

12. **jsm**
January 30, 2015

Of course, NSA funnels the money through the AMS for the prestige and influence it buys them — if it simply wanted to help math in the US, it could give the money to NSF. The shocking thing is how little the AMS has been willing to sell itself out for.

Vogan surely knows more about the mood among the AMS membership than the rest of us — after all, he's served on many committees. Judging by the Elsevier boycott, I'd guess that a petition to sever the AMS's ties with NSA would get about 2000 signatures (the same 2000).

13. **Martin S.**
January 31, 2015

Regarding a poll,
it seems that those who hope for a safe employment and/or do not know a freer world, like them.

14. **Peter Woit**
January 31, 2015

Martin S.,
I doubt those numbers are very meaningful, with most respondents probably not having any idea what the NSA is or does (not everyone in the US carefully follows the Snowden revelations…). The pattern of NSA favorability falling sharply with age may just reflect that as people get older they are more likely to have figured out what the NSA is.

15. **Visitor**
February 1, 2015

"The pattern of NSA favorability falling sharply with age may just reflect that as people get older they are more likely to have figured out what the NSA is."

Or maybe the older people are the less likely they are to have grown up in the "let's put our whole lives online via Facebook, Twitter, and the rest of that" world.

Of course, that doesn't include the ideological baggage that your "figured out what the NSA is" carries, but it might be more accurate for lacking it.

16. **NoGo**
February 3, 2015

Hello Mr Woit!
Like your blog very much.
I thought you may find this article interesting, and am very curious what you think of it:

For some reason it makes a bit of a splash in Russian mainstream media…
Thanks!

17. **Peter Woit**
February 3, 2015

NoGo,
Sorry, but understanding exactly what those authors have shown that is new is beyond my expertise. Personally I'm happy to characterize the wavefunction as "real", to the extent that anything is "real", but that's just words, capturing likely not at all whatever the actual issue addressed by the paper might be.

18. **Shantanu**

February 4, 2015

Peter, something OT.
Another brilliant paper by Avi about hiring
http://arxiv.org/abs/1502.00709

19. **abbyyorker**
February 7, 2015

I read Frampton's e-book and I wish him all the best. That bikini model was hot. But it's hard to believe he was THAT naive, especially reading the nyt transcript of his joke messaging from the airport. Anyway, he did his time and good luck to him.

20. **Jean D**
February 28, 2015

«by the way, can anyone tell me why that number is a secret?»

In the 6th Century, Sun Tzu put it this way: «Appear strong where you are weak and weak where you are strong.» This way the other guy can't tell how many «chariots» you can really field.

Even though everyone knows what they personally would do with an X37-B, no one knows for sure yet what it's all about. A lower figure would indeed point to the Air force only testing a concept, the higher figure may put a sat killer laser like on the Argleigh Burke class of ships on its next flight. All good engineers can tell you everything is feasable, and will also tell you exactly how much it will cost.

Surely you must know we can infer many things with the proper and true data. You PhDs do the same with the Universe.

A Letter to the AMS

February 3, 2015
Categories: Uncategorized

Leonid Reyzin at Boston University has drafted a letter in response to the recent article published in the Notices by Michael Wertheimer of the NSA (discussed here). He's collecting signatures, and if you're a member of the AMS I urge you to consider contacting him and adding yours. If you know others who might be interested in signing, please forward the link to them.

Comments

1. **Steve Huntsman**
   February 3, 2015

   NSA strengthened DES against the then-classified technique of differential cryptanalysis by improving the S-boxes. It is not fair to say that NSA weakened DES by reducing its key length without looking at this.

2. **Peter Woit**
   February 3, 2015

   Steve Huntsman,
   Yes, suspicions about the DES S-boxes turned out to be unfounded (and the NSA via Richard George has used the AMS Notices to suggest that the same is true for DUAL_EC_DRBG). But that this suspicion was unfounded has nothing to do with the DES key length issue mentioned in the letter, where there seems to be no question that the NSA pushed for a shorter key length so that they could break such encryption. Even they haven't tried to claim that shortening key length was a way to strengthen DES.

3. **Roger**
   February 3, 2015

   It says "blacklisting an inventor of DES from other cryptography jobs". Who was that?

   Wikipedia:
   The team at IBM involved in cipher design and analysis included Feistel, Walter Tuchman, Don Coppersmith, Alan Konheim, Carl Meyer, Mike Matyas, Roy Adler, Edna Grossman, Bill Notz, Lynn Smith, and Bryant Tuckerman.
   http://en.wikipedia.org/wiki/Data_Encryption_Standard

   You say: "there seems to be no question that the NSA pushed for a shorter key length so that they could break such encryption."

   There is some question about that. For details, see this AMS Notices article that only says:
   "There have been persistent rumors that NSA had pressed for the shorter key length."
   http://www.ams.org/notices/200003/fea-landau.pdf

   Yes, there were rumors, but I do not see those rumors confirmed anywhere.

4. **tt**
   February 3, 2015

   details and a citation on the blacklist here (Horst Feistel is the guy)
   http://ethanheilman.tumblr.com/post/108115952435/a-response-to-wertheimers-encryption-and-the-nsa

5. **Peter Woit**
   February 3, 2015

   Roger,
   "NSA tried to convince IBM to reduce the length of the key from 64 to 48 bits." is not a "rumor", it's based on the declassified, sanitized version of the NSA's own history. See the reference at the Wikipedia page.

6. **Michael Hutchings**
   February 3, 2015

   I would like to sign something, but I don't feel qualified to sign this particular letter, because it refers to a lot of history which I don't really know anything about. I think one could get more signatures with a letter referring to broader principles, something along the lines of "The AMS should convene a task force to consider reducing or eliminating ties with the NSA, due to serious ethical concerns about this relationship [references]."

7. **Ninguem**
   February 4, 2015

   Is it only for members of the AMS?

8. **Ethan Heilman**
   February 4, 2015

   In regards to the history of the DES key length it is from the NSA's internal history, "Book III: Retrenchment and Reform" which can be found here at cryptome.

   > "NSA worked closely with IBM to strengthen the algorithm against all except brute force attacks and to strengthen substitution tables, called S-boxes. Conversely, NSA tried to convince IBM to reduce the length of the key from 64 to 48 bits. Ultimately, [*] they compromised on a 56-bit key."

   Also see NSA attempting to clamp down on feedback register techniques:

> “In 1977, a patent controversy stirred the already-choppy waters. George Davida, a University of Wisconsin professor, applied for a patent on a cryptographic device using advanced mathematics techniques and [— — — — —-] shift registers. The COMSEC organization was unruffled, but DDO, fearing the spread of shift register techniques that would give the SIGINT side problems, recommended a secrecy order, which was duly put in place by the Patent Office. The inevitable public debate turned on the issue of academic freedom. NSA answered that if Davida had published the technique in an academic journal he would have been protected, but since he had instead applied for a patent, it appeared that he was in it for the money and thus lacked First Amendment protection. This was incontrovertible logic but bad politics, and once again NSA was forced to back down. The Davida patent was reinstated.”

And ITAR restrictions which was used to threaten IEEE authors with censorship.

> “NSA hunted diligently for a way to stop cryptography from going public. One proposal was to use the International Traffic in Arms Regulation (lTAR) to put a stop to the publication of cryptographic material. [..] The Institute of Electrical and Electronics Engineers would be holding a symposium on cryptography in Ithaca, New York. Concerned about the potential hemorrhage of cryptographic information Meyer sent a letter to E. K. Gannet, staff secretary of the IEEE publications board, pointing out that cryptographic systems were covered by ITAR and contending that prior government approval would be necessary for the publication of many of the papers.”

When threats didn’t work they attempted to slow down academics via other means.

> “It was essential, then, to slow the rate of academic understanding of these techniques in order for NSA to stay ahead of the game. (There was general recognition that academia could not be stopped, only slowed.) ”

9. **Peter Woit**
February 4, 2015

Ninguem,
I don’t think signers necessarily need to be members of the AMS, you should contact Leonid Reyzin if you’d like to add your name.

Michael Hutchings,
The letter is specifically intended as a response to the Wertheimer article, but I completely agree that a letter of the sort you indicate would be a great idea (do you want to organize it?).

My impression is that when all this started post-Snowden, there was little enthusiasm at the AMS for taking any action to cut ties with the NSA. The decision was instead to encourage discussion, and that’s the goal of the Notices articles and letters. Interestingly, I haven’t seen much in the way of practical discussion of exactly what the AMS ties to the NSA are, and exactly what steps might be possible to change them. However, the two articles from NSA people it seems to me have helped make clear what the fundamental problem with that organization is: it operates outside the usual constraints of the bill of rights and democracy. As a result, people working there like George and Wertheimer find it natural to respond to the basic question (did the NSA backdoor DUAL_EC_DRBG?) by writing an evasive and misleading piece in the Notices. Their attitude appears to be that the public and the math community have no right to ask this kind of question, and deserve to be misled if they try.
This may be having an effect at the AMS, making people more open to a discussion about cutting ties with the NSA. I don’t know if a letter or other action is the best way to move this forward. As of this weekend the AMS has a new president (Robert Bryant), who may bring a different perspective to the question.

10. **Michael Hutchings**
February 4, 2015

I was hoping for a letter that lots of people could sign to express their concern without having to know the detailed history. However the person who organizes this letter should probably still know what they are talking about, so I wouldn’t be the best person for the job. I could still do it though if no one else will. Maybe we should first see if Robert Bryant will do something.

Planck Data Out

February 5, 2015
Categories: Uncategorized

Long awaited data from the Planck satellite was released today, papers available here. The accompanying press release leads with results about the timing of the first stars, 500 million years or so after the big bang, with little mention of the very early universe. This is also the main topic of BBC News coverage.

This paper reports a bound on r of .08-.09, exactly what Shaun Hotchkiss was predicting earlier this week here. This appears to be pretty much the end of the line for hopes that Planck would see primordial gravitational waves, with the paper seemingly pointing to other experiments being necessary to get below r=.05 (see page 35).

The BBC News story also characterizes these bounds as ruling out the simplest inflationary models, requiring they be supplemented by "exotic physics".

> What is clear from the Planck investigation is that the simplest models for how that super-rapid expansion worked are probably no longer tenable, suggesting some exotic physics will eventually be needed to explain it.
>
> "We're now being pushed into a parameter space we didn't expect to be in," said collaboration scientist Dr Andrew Jaffe from Imperial College, UK. "That's OK. We like interesting physics; that's why we're physicists, so there's no problem with that. It's just we had this naïve expectation that the simplest answer would be right, and sometimes it just isn't."

For about as long as I can remember, string theorists and multiverse fans have been pointing to Planck data as the test of their ideas. For cosmic strings, the last Planck data release had a paper ruling them out. I don't see a paper on this topic out or projected for the new data, it seems that this is now something not even worth looking for.

We've also been hearing for years that Planck will test supposed evidence of bubble collisions indicating other universes, see for instance this article about this paper, where the article states that

> Data from the Planck telescope should resolve the question once and for all.

I don't see anything in the new data even looking for this. Has it already been ruled out, without any publicity, or did the Planck people think it was something not worth even looking for?

Comments

1. **Ben Gold**
   February 5, 2015

   The bubble-collision paper you reference is looking for the signature in temperature only. I think I've seen some calculation of polarization signatures for specific multiverse models, but I'm unaware of a general "bubble collision"-type search that uses polarization. So I think the answer to your question is "mostly ruled out without publicity", but also that even defining a generic polarization signature for such models is hard, so there could be room for something clever.

2. **Peter Woit**
   February 5, 2015

   Thanks Ben,
   What about this one
   http://arxiv.org/abs/1109.3473
   which claims a polarization signal?
   For the Planck data, is this in the category of already ruled out, Planck thinks not worth looking for, or results buried somewhere I didn't see?

3. **Ben Gold**
   February 5, 2015

   OK, the punch line of that paper (which is looking basically at the E-mode generated by the temperature gradient) seems to be that small spots would be detected first in temperature, and "very large spots [angular radius > 25°] could be detected almost as easily in polarization as temperature".

   There are some particulars of the signature that might make it stand out a bit more, but basically the upshot is that you'd be looking for large-scale E modes (and TE correlations), where there's plenty of ordinary signal (unlike B modes) and where Planck has been having trouble this whole time.

   Side note – while it looks like Planck has released a lot of data, it notably does not include polarization power spectra, or in fact any polarized maps except for the LFI frequencies (where foregrounds are larger) and, for some reason, at 353 GHz. Not the 100/143 that I'd think to use for CMB analysis.

4. **Peter Woit**
   February 5, 2015

   Thanks Ben,
   OK, so for that one I guess it's still wait and see…

5. **Jeff**
   February 5, 2015

   I just watched this on BBC and it was bizarre. After three minutes of talking about the earliest stars they cut to an expert who completely changed the subject (or they cut the segue) and babbled "Multiverse, multiverse, multiverse."

6. **Bernhard**
   February 5, 2015

   This whole story resembles SUSY so much…

7. **David**
   February 6, 2015

How does this reconcile with the earlier BICEP/Planck joint paper?

8. **Peter Woit**
February 6, 2015

David,
They get a somewhat stronger bound on r from their data. As Ben Gold explains, they still haven't finished the full analysis of their polarization data, presumably will do even better when that is done.

9. **Urs Schreiber**
February 6, 2015

It deserves to be highlighted more explicitly: the model of inflation strongly preferred by the Planck2015 data is — as was already the case after the Planck2013 analysis — the Starobinsky model.

10. **Anonymous**
February 6, 2015

Urs, there is no such preference. The data is not sufficient make such strong distinctions in any case, but with the new r distribution it is not true that the Starobinski model is preferred. More will be known in the coming years, no need to jump the gun.

11. **Peter Woit**
February 6, 2015

Urs/Anonymous,
You really can't claim strong support from experiment for your model when your model's prediction is that experiment won't see anything. All sorts of early universe models don't have observable primordial gravitational waves.

I just looked out the window, saw no large monster green turtles. This provides strong support for my theory that the universe is controlled by a large monster green turtle, one that lives on the other side of the planet….

12. **Urs Schreiber**
February 6, 2015

The Starobinsky model sits right in the center of the parameter range preferred by the Planck data, the famous figure 1 in arXiv:1303.5082, which is now confimed as figure 22 of arXiv:1502.01589.

Quoting Jörg Paul Rachen, member of the Planck collaboration, from his IMAPP talk just yesterday:

> The Starobinsky model is the model with the highest Bayesian evidence as it is right in the center of the likelihood peak and at the same time has the lowest number of free parameters.

13. **Anonymous**
February 6, 2015

The central value of r is nonzero (.03-05), but with (in)famously low significance. There is no statistically significant distinction among the many models in the allowed range, including Starobinski, but the latter is not at the central value. There's more data on the way, so anyway there's no need to guess right now.

14. **Urs Schreiber**
February 6, 2015

I am not guessing, I am quoting public statements made by experts with an identifiable name attached to them.

15. **Eduardo Lira**
February 6, 2015

Be it that SUSY doesn't show up at the LHC or that Planck sees only dust, it all leads over and over again to the ultimate objection critics make to ST: that it can't be falsified.
However, doesn't the history of physics record other circumstances where falsifiability was also absent and sometimes so for many years, but only to turn out different in the longer term? Consider for example the question about the chemical composition of stars that scientists asked themselves in the first half of the 19th century. You couldn't then (nor can today!) bridge the immense distances to the stars, so it seemed impossible to perform analysis of any kind to find out what they are made of. Among others, French philosopher Auguste Comte properly (or so it seemed then) presented the case as an example of a class of facts that were to stay forever hidden from human knowledge.
Any theory aimed at describing what stars are composed of that had been proposed those days wouldn't have been falsifiable, and should then have been classified as "not scientific". But of course later discovery of the absorption spectra phenomenon ultimately led astronomers and physicists to determine the precise composition of stars, never mind if you can't go measure it in situ.
Back to ST, can we discard the possibility of coming someday across the likes of spectra absorption lines that allow us to reveal the precise shape of Planck-scale space-time, and the inner structure (if there is any) of elementary particles? If you ask people like George Ellis and Joe Silk -to mention a recent reference in this blog- the answer would be that we'll never find out. In their own words, "the higher dimensions are wound so tightly that they are too small to observe at energies accessible through collisions in any practicable future particle detector" (Nature, Dec. 16, 2014).
But what if you replace the idea of "too small tightly wound higher dimensions, too small to observe" with "too large distances to the stars, too far to analyze", and also the one of "any practicable future particle detector" with "any practicable future means to reach a star"?
Could a future discovery nobody anticipates now take ST out of the not-even-wrong limbo, as it would have been the case for any "not scientific" (i.e. unfalsifiable) theory which happened to be proposed 150 years ago to try to predict the composition of stars, and which became falsifiable against all odds when spectra absorption was discovered and explained? As improbable as it may look today (and as it seemed in 1840 that we would stumble upon a means of finding out what stars are made of), if such a breakthrough comes to occur (whether this happens in the near future or 200 years from now is fundamentally irrelevant), confrontation of ST with the newly acquired way to probe reality will allow physicists to determine whether it is plainly wrong or just right, the latter if only in the broad sense of pointing in the right general direction for further construction and refinement.
A much less important consequence of this last hypothetical outcome is of course that the category where Auguste Comte belongs in history (whatever it may be) would be joined by numerous new members.

16. **Peter Woit**
February 6, 2015

Eduardo Lira,
Please, this is off-topic, has nothing to do with yesterday's Planck results. I will leave it though, just so I can point to a relevant FAQ entry (the problem with string theory is not that its predictions are unobservable, but that there aren't any)

http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=doesnt-string-theory-make-predictions-at-very-high-energy

I will however delete further off-topic comments that have nothing new and interesting to add.

17. **West**
February 6, 2015

@Urs: Did you happen to catch how much better the marginal likelihood (hate using "evidence" for this) is for the Starobinsky model than its competitors?

18. **West**
February 7, 2015

Am actually able to answer my own question now that the Planck collaboration's paper about constraints on inflationary models is out.

Table 6 (p.18) is a nice summary of the model comparisons using only Planck data and as Urs suggested yesterday, the Starobinsky model comes out on top. Mind you not by much, and if one includes the BKP cross-correlation results, some other models do just as well at fitting the data. The results lend credence to models with small r values but no single one has truly distinguished itself from the pack.

So in the end, we have a 95% upper limit on r at 0.08, no evidence for spectral running and an uncertainty interval (68%) for n_s at the sub-1% of the peak posterior value. Impressive work all around.

IPMU Conversation with Edward Witten

February 6, 2015
Categories: Langlands

I recently heard from Hirosi Ooguri that a transcript of a long conversation with Witten held at the time of his Kyoto Prize award has just appeared in the Kavli IPMU Newsletter. It's a truly fascinating document, giving some great insights into Witten's work at the boundary of math and physics and how he sees the state of ideas in this area. It's wonderful that he was induced to give such a thoughtful and extensive explanation of both the history and significance of these various topics.

Just to pick out a couple examples, the discussion of geometric Langlands describes a lot of detailed history that I was unaware of. I had noticed that in their first massive (still unpublished) paper that started the subject, Beilinson and Drinfeld credit Witten with "the main idea" (I wrote about this in detail here). But there was nothing in what Witten has written that corresponds to what they did, and experts I talked to didn't see how this came from Witten. Witten tells the true story this way:

> Actually, the very little bit of what Beilinson and Drinfeld were saying that I could understand made me wonder if the work of Nigel Hitchin would be relevant to them, so I pointed out to them Hitchin's paper in which he had constructed commuting differential operators on the moduli space of bundles on a curve. Differently put, Hitchin had in a certain sense quantized the classical integrable system that he had constructed a few years before. Although I understood scarcely anything of what Beilinson and Drinfeld were saying, I did put them in touch with Hitchin's work, and actually, in their very long, unpublished foundational paper on geometric Langlands that you can find on the web, Beilinson and Drinfeld acknowledged me very generously, far overestimating how much I had understood. All that had really happened was that based on a guess, I told them about Hitchin's work, and then I think that made all kinds of things obvious to them. Maybe they felt I knew some of those things, but I didn't. But anyway, there were ample reasons in those years to think that geometric Langlands had something to do with physics, but as you can see I still couldn t make any sense out of it.

He also describes how he came to the idea of interpreting geometric Langlands as a form of mirror symmetry, inspired by things he learned from David Ben-Zvi at lectures about the Langlands program bringing together mathematicians and physicists at the IAS.

He contrasts his work in recent years relating Khovanov homology and gauge theory with the geometric Langlands work, saying that he thinks the Khovanov homology ideas are in a form such that mathematicians are more likely to be able to appreciate their roots in gauge theory:

> I think it s actually very difficult to see what advance in the near term could make the gauge theory interpretation of geometric Langlands accessible for mathematicians. That's actually one reason why I m excited about Khovanov homology. My approaches to Khovanov homology and to geometric Langlands use many of the same ingredients, but in the case of Khovanov homology, I think it is quite feasible that mathematicians could understand this approach in the near future if they get excited about it. I believe it will be more accessible. If I had to bet, I think I have a decent chance to live to see gauge theory and Khovanov homology recognized and appreciated by mathematicians, and I think I'd have to be lucky to see that in the case of gauge theory and the geometric Langlands correspondence – just a personal guess

About the geometric Langlands story, he thinks there is still much to be understood, including its connection to conformal field theory:

> In fact, part of the original work of Beilinson and Drinfeld on geometric Langlands has still not been understood to my satisfaction. Here I have in mind the use of conformal field theory at what they call the critical level (level -h, where h is the dual Coxeter number) to construct the A-model dual of certain B-branes (the ones that are associated to opers, in the language of Beilinson and Drinfeld). Davide Gaiotto and I obtained a few years ago a reasonable understanding of what electric-magnetic duality does to the variety of opers, but I still do not really feel I understand its relation to conformal field theory. However, in the last few years physicists working on supersymmetric gauge theories in four dimensions and their cousins in six dimensions have made several discoveries involving the role of conformal field theory at the critical level, so the time may well be right to resolve this point.

Among the many other highly interesting comments, one was Witten's take on the possible connection of quantum field theory to number theory. He has a long history with this, going back to conversations with Atiyah in 1977 in which Atiyah suggested some connection between Langlands and Montonen-Olive. Witten writes

> I was skeptical about Montonen-Olive duality, I didn't seriously try to relate it to Langlands duality and I didn't try to learn what Langlands duality was. I did not learn anything more about these matters until the late 1980s. Then I learned just superficially about the Langlands correspondence. If one knows even a little bit about the Langlands correspondence and a little bit about conformal field theory on a Riemann surface, one can see an analogy between them. I wrote a paper that was motivated by that but then I realized that my understanding was too superficial to lead to anything deep, so I abandoned the matter for a number of years.

Later of course, he followed work on geometric Langlands and ultimately found the connection to gauge theory he worked out with Kapustin. As far as current prospects for connections to number theory, he has quite a few comments, but thinks the subject is still a dream that is not ripe:

> For me personally–it's a dream that eventually number theory would make contact with physics some time, but I doubt it will be soon. There are all kinds of areas where specific number theory formulas appear in physics, and these may be clues that the dream will come true one day. But to really get me excited, somehow the number theory would have to enter the physics in a more structural way. I m not that interested in a specific formula that comes out of a physics calculation in a more or less ad hoc fashion. Number theory would have to be more integrated with the physics to get me excited, and I don t see that happening soon. In my work, I concentrated on the geometric form of the Langlands correspondence because I could see that there was hope to really understand it in the context of the physics-based tools that were at hand. There might be something like that one day for the Langlands correspondence of number theory, but probably a lot is missing and we do not know what has to happen first.

This just gives a taste of the conversation, there is lots, lots more there, on a wide range of topics. Highly recommended reading for anyone with an interest in this area, I've never seen anything like it.

Comments

1. **Als**
   February 6, 2015

   Thanks for the link.

   Are there also interviews of the previous winners? I tried to find them but without success…

2. **Paul Frampton**
   February 7, 2015

   Peter, Your links are always useful but none more so than today's link to the December 2014 issue of the IPMU News with its interview about physics and mathematics at IPMU of Edward Witten by Hirosi Ooguri. I would not have read this otherwise and could not put it down before reading all seventeen pages. Edward always speaks perspicuously and Hirosi cleverly steers the interview. The distinguished theoretician Hitoshi Murayama put mathematicians and physicists in adjacent offices at IPMU.

3. **Giotis**
February 8, 2015

What can you say about this interview? Just amazing; it has it all and it's of historical importance.

Maybe the only thing that is missing is a reference to the quest for the identification of the underlying symmetry of String theory responsible for its rigid structure that would further restrict the theory. Finding this symmetry is of immense importance since it will shed light to its underlying structure (potentially with phenomenological implications).

A glimpse of this symmetry can be seen in the Higher Spin algebra of Vasiliev's theory in AdS which is expected to be a sector of String theory in its tensionless limit (where this symmetry would be unbroken).

Witten himself was one of the first people that motivated this in the context of Higher_Spin/ Free_CFT correspondence in his 2001 talk at the John Schwarz 60th birthday symposium.

http://theory.caltech.edu/jhs60/witten/1.html

4. **Tim Nguyen**
February 8, 2015

Thanks for the link Peter and your positive reaction to the interview parallels mine. I'm sure you must have been asked this before, but I'm curious how you reconcile your appreciation of the fruitful ideas coming out of string theory with the otherwise polemical views you have of string theory as a science.

5. **Peter Woit**
February 8, 2015

Tim Nguyen,
I really don't want this to turn into yet another of the same tedious discussions about math and string theory that have often taken place here. One interesting thing about Witten's commentary is that he makes very clear that no one really understands the origin of a lot of the fascinating structures he's talking about or what the right way to think about them is. As he points out, these things make clear that there is much we don't understand about QFT, much less string theory (which is becoming a very ill-defined term). To a large extent, the mindset of theorists for twenty years has been dominated by the conjecture that the big explanation is some mysterious 11d theory, and that if understood it would give a unified theory. I don't see any progress on this, or actually any good reason to think that 11d is the way to a unified theory (quite the opposite…). Thinking about these mysteries with more of an open mind about what the new structures are that we're looking for seems like a good idea, and I think Witten at his best (as is on display here) is often doing that.

6. **Tim Nguyen**
February 8, 2015

Peter,

Apologies if these are repetitive comments (perhaps you could link me to relevant past discussions if you so wish), but it seems to me that string theory being on the one hand unsatisfactory as a science and on the other, being a rich source for mathematics, can be seen as independent issues. The same tree can produce both good fruit and bad fruit and we reap the benefits so long as we know which is which. Perhaps this was implicit in your remarks.

7. **Ian Welland**
February 9, 2015

What applications have arisen from the mathematics invented by string theorists? If there aren't any, how can it justifiably be called good mathematics?

8. **Peter Woit**
February 9, 2015

Ian Welland,
Most mathematicians don't think "good mathematics = applied mathematics", and if you fired mathematics faculty whose research in recent years has not had significant applications, many of the best research universities in the world wouldn't have much of a math department anymore. Even if you believe "good=applied", pure math research often only finds applications very much later, even centuries later.

I'll delete any more comments of this kind though. If you're not interested in what Witten is talking about that's fine, just ignore it.

9. **Giotis**
February 9, 2015

Another thing that I would definitely expect to hear in a discussion like this and it was skipped for some reason is a reference to the Monstrous moonshine. This can also be linked to Witten's pioneered work on AdS_3_QG/Monstrous_CFT_2 correspondence.

Three-Dimensional Gravity Revisited (http://arxiv.org/abs/0706.3359)

10. **Justin**
February 10, 2015

Woit, Excellent interview with Witten.

But, didn't you find his outlook, while honest, a bit discouraging? e.g. gauge theory and Langlands won't be appreciated by mathematicians anytime soon, number theory and physics won't have any unification in any foreseeable future.

11. **Peter Woit**
February 10, 2015

Justin,
Not at all, I found his comments realistic and actually encouraging. The relationship of mathematicians to gauge theory is a complicated and long story. It never has been the case that mathematicians are able to adopt physicist's techniques for dealing with quantum Yang-Mills above 2d, for geometric Langlands or anything else. What mathematicians have gotten out of this is new ideas and inspiration about how to pursue their own methods (an old example was Seiberg-Witten, a nice new example is in

Witten's comments about Kevin Costello's work). Ultimately I think new ideas will come here from each side doing what it does best, not one side adopting the other's machinery.

About number theory and physics, I see lots of fascinating possibilities and connections, but agree with Witten that there is still no compelling picture of exactly what is going on here. I'd have been quite surprised if he claimed that major progress was imminent (few people work on this). What would have been discouraging would have been if he said he felt there was nothing there, that this couldn't lead anywhere, but that's quite different than what he was saying.

12. **Don Murphy**
February 10, 2015

I believe that Beilinson and Drinfeld gave Witten so much credit for helping them because they recognize that true genius is often the ability to see connections where most people do not. Witten has demonstrated this genius on many occassions.

13. **Jeff M**
February 10, 2015

Pardon my ignorance, but I'm kind of curious at the hero worship of Witten in the physics community. Clearly a great mathematician, they don't give you a Fields for nothing. My orals in grad school were presenting a JDG paper of his, brilliant bit of work. Took reading a whole book by John Roe to actually understand what he was doing. But physics? I understand that a big chunk of what people in physics work on, and have been working on for several decades, comes from Witten's work, but what of it has actually been shown to be true? What has he worked on that has been experimentally verified? I've been to some neat talks on topological quantum field theory, fun stuff, but any known relation to the real world? Just curious…

14. **Peter Woit**
February 10, 2015

Jeff M,
There really have been no major advances in theoretical particle physics that have been experimentally verified in now over 40 years. If you look at Nobel prizes, there have been no Nobel prizes for theoretical particle physics work done post-1973. So, yes Witten has not done anything at that level, but to be fair, neither has anyone else (he started grad school around 1973).

But he really has revolutionized how we think about quantum field theory. TQFT is mostly his doing. Anyone who looks at the history of this I think has to be in awe of how much he has done, with much of it way beyond what anyone else in the field was doing or capable of doing. The outcome of this so far has worked out much better for mathematics (his Fields medal is well-deserved, and that was awarded before Seiberg-Witten and much else) than for physics. But it's entirely possible that when progress finally comes in the future, his ideas will be part of it. There really is a history of overlap between good physics and good mathematics, so the mathematical qft successes could very well be matched by success on the physics front.

He's been I think overly enthusiastic about string theory unification, and slow to acknowledge that that idea hasn't worked out, but at least you don't hear him going on about the multiverse…

15. **MathPhys**
February 11, 2015

Peter,

"There really have been no major advances in theoretical particle physics that have been experimentally verified in now over 40 years."

Exact solutions in 2D statistical mechanics [e.g. Baxter's hard hexagon model] and their critical limits, the 2D conformal field theories [e.g. the c=1/2 theory that describes the critical limit of the Ising model] are, in my opinion, major advances in theoretical physics, that are less than 40 years old and examples of these theories were verified experimentally in the early 1980's in experimental studies of surface critical phenomena [my recollection is that these experiments were post Baxter's work, but before Belavin et al.].

This is the only corner of string theory [in the broad sense of the word] that I'm aware of that has been experimentally verified. Non-canonical critical exponents were measured in the lab, and the theory was right on the money.

16. **Peter Woit**
February 11, 2015

MathPhys,
My comment was intended to just be about theoretical HEP physics. Advances in QFT of course have many other applications. I don't think Witten though has ever shown a lot of interest in condensed matter/stat mech physics, although some of his work has applications there and I'm sure he knows quite a bit about the subject. He doesn't seem to be joining the current wholesale move of a lot of the string theory community into condensed matter.

17. **Justin**
February 11, 2015

Woit wrote,

"TQFT is mostly his doing."

Witten did lots for the subject, and deeply deserved his fields medal. But, let's not forget the importance of Atiyah who really suggested to Witten that knots might have a physical interpretation. Also, Floer's results were critical as well. And Seiberg-Witten does have two names in the title.

18. **Peter Woit**
February 11, 2015

Justin,
I wrote in detail about that history in my book, it's chapter 10 there. I stand by the claim that "TQFT is mostly his doing", although of course certainly other people were crucially involved.

19. **A.J.**
February 11, 2015

TQFT gets a lot of attention from mathematicians, but it's not what won Witten the respect of his fellow physicists. If I understand the history correctly, he was already famous by the time he started thinking about that stuff, for his work on 1/N expansions, on current algebra, on anomalies, on instantons, on chiral matter in supergravity, on positive energy in GR, and on SUSY breaking.

What has he worked on that has been experimentally verified?

He actually has worked on a few things which have been checked experimentally, but it's not what made him famous. People listen to him because he's done a lot to clarify the way people think about quantum field theories. It's too bad that he hasn't managed to predict any new resonances, but it's a bit blinkered to think that's the only worthwhile thing theoretical particle physicists can do. I'd say, for example, that Dirac's synthesis of Heisenberg & Schrodinger's work on quantum mechanics was far more important than his prediction of the positron. The former is critical for understanding quantum physics, whereas the latter was so dodgy that explaining it usually only confuses people who're trying to learn the subject.

20. **MathPhys**
    February 12, 2015

    He doesn't seem to be joining the current wholesale move of a lot of the string theory community into condensed matter.

    He's a wise man. But my point is that conformal field theories, which are the building blocks of string theory, have verified experimentally, on a theory by theory basis. These theories with predictions based on Virasoro algebra, affine Lie algebras, expectation values of vertex operators, etc, make physical sense.

21. **kashyap vasavada**
    February 12, 2015

    @MathPhys:
    Can you elaborate or give references for your statement about experimental verification?

22. **Thomas Larsson**
    February 12, 2015

    There are many experimentally realized system which behave like the Ising model or its cousins when they undergo second-order phase transitions. A monolayer of an inert gas on a graphite substrate is often quoted as a particularly clean system. Such systems were studied for many years before CFT – Ernst Ising got his PhD in 1925. It turned out, both empirically and by theoretical considerations, that similar statistical systems have identical and not just similar critical exponents, or completely different ones. This is called universality, and CFT yields an exhaustive classification of universality classes in 2D systems, and the RSOS models of Andrews-Baxter-Forrester gave concreate realizations of all classes as lattice models.

    This is absolutely wonderful work – basically the only experimentally verified theoretical discovery during my entire non-career – and I have argued for 25 years that BPZ deserve the next Nobel prize for this. However, from the statphys point of view it is also clear that this is not a ToE, not even in statphys. CFT explains universality in 2D (the string world sheet is two-dimensional), but it has nothing to say about the experimentally more interesting 3D case. And we know that universality is also present in 3D, empirically and theoretically (epsilon expansion), so one could ponder if the explanation could be similar to 2D.

23. **MathPhys**
    February 14, 2015

    The experiments that I know of were performed at the U of Washington, Seattle, in the early 80's. Theorists who were involved in the analysis of the results usually included M Schick. The critical exponents that were measured were non canonical, hence non trivial, and always agreed precisely with predictions based on 2D conformal field theories, which indeed came right after.

    I only wish to say that the mathematics of string theory, that is the Virasoro algebra with a central extension, vertex operator algebras, etc, is more than recreational mathematics, and more than an abstraction in a platonic plane. It makes non trivial predictions that can be measured experimentally.

    The fact that this mathematics is not sufficient to lead to a theory of everything is a different matter.

Short Items, and a Quick Book Review

February 13, 2015
Categories: Book Reviews, Multiverse Mania

- Peter Orland has a new blog, Ensnared in Vacuum, where he's writing about some non-perturbative QFT questions.
- Physics Today this month has book reviews of two books about theology and the multiverse (one of which I wrote about here). There was a time when I would have thought that discussions of theology wouldn't be what Physics Today covers, but evidently that's no longer the case.
- On a related topic, Kate Becker at The Nature of Reality has an article entitled Does Science Need Falsifiability? It's about the campaign by physicists like Sean Carroll and Lenny Susskind against the Popperazi who keep pointing out that giving up on falsifiability puts physics in danger of becoming, well, theology. Frank Wilczek has a very sensible take on the subject:

  > "I think falsifiability is not a perfect criterion, but it's much less pernicious than what's being served up by the 'post-empirical' faction," says Frank Wilczek, a physicist at MIT. "Falsifiability is too impatient, in some sense," putting immediate demands on theories that are not yet mature enough to meet them. "It's an important discipline, but if it is applied too rigorously and too early, it can be stifling."

  On Twitter, the usually mild-mannered Wilczek makes clear his feeling about this

  > Not often I refer to a "pernicious" "faction", but appropriate here.

  Mysteriously, he has a new website, for a company "Wolfcub Vision, Inc".

- Frank Close has a new book out, Half-Life, which is essentially a biography of the physicist Bruno Pontecorvo. It's also a gripping spy story, investigating the question of exactly why Pontecorvo fled with his family to the Soviet Union in 1950. There's no smoking gun found, but all the evidence Close lays out makes the case that it is quite likely that Pontecorvo had been spying for the Soviets, fleeing when warned that he was in danger of being exposed.

  Freeman Dyson has a much better review of the book in the New York Review of Books than I could ever write. He argues that Pontecorvo made a mistake by fleeing to enforced isolation in Russia, that in the worst case if caught he would have spent a few years in jail, then could have resumed his career. That things would go this way would not however have been clear to Pontecorvo: the Rosenbergs were arrested just before he fled, and things didn't work out so well for them.

  Besides the fascinating spy story, there's also a lot of history of nuclear physics during the 30s, 40s and 50s, much of which I wasn't aware of, as well as quite a bit about Pontecorvo's later work on neutrinos. If you're interested in the history of 20th century physics, this is something you'll find well worth reading.

Update: For another new book, Steven Weinberg's To Explain the World, I fear that I don't have the time to read it and write a review. However, here are two interesting reviews, pro and con.

Update: For two hints about "Wolfcub Vision", a commenter points out that Wolf cub=Wilczek in Polish, and a correspondent points me here.

Update: David Mumford has a posting Is it Art? at his blog, motivated by my friend Dan Rockmore's equations project. An article about the recent panel discussion of this at Yale is here.

Comments

1. **Peter Orland**
   February 13, 2015

   Hi Peter,

   Thanks for letting people know.

2. **Brandon B**
   February 13, 2015

   I'm confused, maybe ignorant, but what is Wilczek implying?

3. **Peter Woit**
   February 13, 2015

   My guess is that Wilczek is just expressing the same opinion as most scientists hold towards those who, like Carroll and Dawid for instance, are trying to claim that their favored theories are still science even if unfalsifiable: they're a minority (a "faction") and they're doing something blameworthy ("pernicious").

4. **martibal**
   February 14, 2015

   A bit off-topic but since the post is also about history of physics: I do not know if you have heard of that, but in Italy there is some "official announcement" (from a prosecutor – "procuratore" in Italian) that Majorana was alive after he disappeared, and was in Venezuela between 1955 and 1959. The elements do not seem so convincing at first sight (a postcard, a "physionometric" analysis of a picture) but enough for a prosecutor to make this claim.

5. **Krzysztof**
   February 14, 2015

   Wilczek means in Polish a wolf cub…

6. **Mesa**
   February 14, 2015

   So, the falsifiability argument is really about theories with different confidence intervals. Tight confidence intervals tend to come from the ability to do repeated, controlled experiments. There are plenty of interesting questions out there where you might want to develop theories but have very limited data and no ability to do repeated, controlled experiments. Climate studies, macro-economics, Planck-scale physics and cosmology are among them. It's fine to develop theories (or really hypotheses) in these disciplines that explain whatever data you do have, but the extreme limitations of such an enterprise need to be understood from the beginning. Lumping activities with wildly varying experimental confidence intervals and ability to do controlled experiments together all as "doing science" is where the trouble begins. They are really different categories of activities. The famous puzzle of the man hanging from a noose in his house with a puddle of water under him is a good example. N=1; can't do the experiment again. Plenty of interesting hypotheses. The smartest guy in the room comes up with the ice block hypothesis. It's elegant and explains the limited facts. Is it "true"? We will never know. Is it worth working on more elegant explanations for these kinds of puzzles? That is a matter of personal taste and available resources. Perhaps the work itself will help techniques that lead to solutions to other puzzles with more verifiable solutions.

7. **Peter Woit**
   February 14, 2015

   Mesa,
   I don't think that's the problem here, what you refer to is a conventional well-understood issue that never led to people arguing that falsifiability should be abandoned. The motivation of people like Carroll, Susskind, Dawid who are now arguing against falsifiability is very clear: they have specific untestable theories that they want to promote, of exactly the sort of thing that conventionally are not considered to be science, and that Popper's criterion was specifically designed to rule out as pseudo-science (multiverse explanations of the arrow of time for Carroll, multiverse explanation of why string theory says nothing about particle physics for Susskind, lack of any predictions by string theory for Dawid).

   Most physicists immediately see where this kind of argument leads and want nothing to do with it. What's amazing is how much traction those pushing this have gotten despite this.

8. **Ian Welland**
   February 14, 2015

   Well Wilzcek has an office at Arizona State University's biophysics floor right down the hall from mine, so that should give a hint as to what he's been up to. Maybe if I work up the courage I'll ask him, I see him wandering about from time to time.

9. **paddy**
   February 14, 2015

   I must object to Mesa's implied inclusion of climatologists (and even economists) in Carroll's looking glass world. I suggest he review what empiricism means.

10. **Mesa**
    February 14, 2015

    PW

    I think we a largely in agreement here. I am just pointing out that there are large swaths of this activity going on in other spheres as well, and these activities should be regarded as different in type from normal science. Whether they are worth doing or not can be debated, but it should be clear they shouldn't be the main line of inquiry. To me, lack of falsifiability is the same thing as small number of observations . Non repeatable small number of observation problems can't really be solved with any confidence, just discussed. However, that may not mean that they are not worth discussing.

    paddy

    I lumped together a group of activities where the data was limited (one way for data to be limited is to have only one history, ie small number of observations) and controlled experiments were not possible. Both climate science and macro-economics fit that description exactly, as well being highly complex systems too boot. I did not claim there was no data. One way to easily identify these types of fields is that there is a lot of emotional debate about fundamental issues, even among skilled practitioners. Again, macro-economics and climatology fit that description exactly. The debates then turn to arguments over extremely simplistic models and politics.

11. **Visitor**
    February 15, 2015

    " He argues that […] in the worst case if caught he would have spent a few years in jail, then could have resumed his career. That things would go this way would not however have been clear to Pontecorvo: the Rosenbergs were arrested just before he fled, and things didn't work out so well for them."

    As the Rosenbergs were executed after Pontecorvo fled to Russia, it is difficult to understand how their execution could have effected his decision to flee.

12. **Peter Woit**
    February 15, 2015

    Visitor,
    My point was that Pontecorvo had no reason to believe that the worst-case scenario if he stayed was a few years in prison for him. The later execution of Rosenberg and his wife shows that he quite rationally could have been worried about this. I don't know if it was known soon after Rosenberg's arrest that he would be charged with crimes carrying a death penalty, but it's possible that was the case.

13. **sm**
    February 15, 2015

    The 'con' review of Weinberg's new book erroneously states (unfortunately, before the pay wall where it can be seen):

    "As his active involvement in research declined, he has become a prolific contributor to what's called the "public understanding of science" "

    The reviewer clearly has little or no idea of Weinberg's recent research. Off the cuff, in the last 10 years Weinberg has made important research contributions to cosmology (and in the process written what could be the standard monograph in the subject), the interpretation of quantum mechanics, and particle physics (conformal symmetry). Indeed, in the last year or so he even pointed out that contrary to reasoning of one of his illustrious former colleagues, accepted by almost everyone, tetraquark states may indeed be 'visible at leading order in 1/N. I would guess > 99% of theorists would be happy to have Weinberg's research in the last ten years as their career achievement!

    Not that Weinberg really needs any defending here. We know he is in the same league as Bethe when it comes to research longevity. What boggles the mind is that a journalist could have the nerve to make such a judgement. In Weinberg's case it may have more than a little to do with the fact he seems, according to the journalist, to critically assess the received wisdom of Enlightenment 101!

    Dirac sometimes suffers similarly, with the ill-informed jibe 'he did little after the 1930s' or something to that effect.

14. **Too Distinguished**
    February 17, 2015

"Our current best understanding of science shows no evidence for a multiverse, so anyone who wants to posit one needs to come up with some significant evidence for one, experimental or theoretical, and I haven't seen that happening. … the question of multiple universes is well worth ignoring…"

It seems to me that significant theoretical arguments for the multiverse do exist, which is why you aren't ignoring them.

15. **Peter Woit**
    February 17, 2015

    Too distinguished,
    I'll just point out that you changed my "significant evidence" to "significant theoretical arguments" which is a different thing…

16. **Anonymous**
    February 17, 2015

    Frequently repeated is not the same thing as significant. Outside of a fairly small enclave (largely consisting of those with a vested interest), these arguments are seen as a total joke.

The Singular Universe and the Reality of Time

February 14, 2015
Categories: Book Reviews

Lee Smolin has a new book out last month, co-written with philosopher Roberto Unger, entitled The Singular Universe and the Reality of Time. To get some idea of what he's up to, there's a review by Bryan Appleyard at The Sunday Times (non paywalled version here), another Bryan Appleyard piece here, and interviews with John Horgan and at Scientia Salon. In other news about Smolin, he's one of the winners of this year's first Buchalter Prize in cosmology.

The book is written in a rather unusual style, with the first two thirds or so by Unger, the rest a shorter contribution from Smolin, together with a section discussing where they disagree. It's neither a popular science book, nor a technical work of philosophy, but something somewhere in between, best perhaps compared to something one rarely now sees, a work of "Natural Philosophy". I found the long section by Unger rather hard going and not very rewarding, and realized that I have a fundamental problem with this sort of writing. Arguments about physics and mathematics made in natural language leave me often unable to figure out exactly what is being claimed. Sometimes this is because I'm not familiar enough with a philosphical tradition being invoked and its associated use of terms, sometimes I suspect it's because natural language is just too imprecise and ambiguous.

The Smolin section is shorter and written with more precision, making it easier to get an idea of what he's trying to claim. To seriously address all his arguments would be a large project I'm not able to undertake, but here is a list of "hypotheses" or "principles" that he arrives at:

- The uniqueness of the universe.
- The reality of time.
- Mathematics as the study of evoked relationships, inspired by observations of nature.

For the first of these I don't really disagree. Smolin takes this as an hypothesis of no "multiverse", an hypothesis that science may be able to confirm or disconfirm. Our current best understanding of science shows no evidence for a multiverse, so anyone who wants to posit one needs to come up with some significant evidence for one, experimental or theoretical, and I haven't seen that happening. It's entirely possible that a compelling theory may emerge that naturally implies a multiverse, but that's not currently the case. Unlike Smolin, I wouldn't take this as an hypothesis, more just would say that the question of multiple universes is well worth ignoring until someone comes up with a good reason to pay attention.

For the second, one problem is that I'm not exactly sure what it means. I guess that when I hear the word "real" I'm always rather suspicious that a meaningless distinction is being invoked (i.e. is the wave-function "real"?), and start trying to remember what it was I once understood about ontological commitments from reading Quine long ago. Part of what Smolin is referring to I think I'm sympathetic with: the nature of time remains mysterious in a way that space isn't. While relativity treats them on an equal footing, in quantum theory this is not so clear. My suspicions about this mystery though tend to focus on the analytic continuation between Minkowski and Euclidean signature, which I'd guess is quite different than Smolin's concerns (see hypothesis three…)

What Smolin seems to have in mind here is the hypothesis that physical laws are not "timeless", but can evolve in time, with an example the ideas about "Cosmological Natural Selection" he has worked on. One problem with this is that the question then becomes "what law describes the evolution of physical laws?", with an answer re-introducing "timeless" laws. Smolin refer to this as the "meta-law dilemma" and devotes a chapter to it, but I don't think he has a convincing solution.

On the third hypothesis, about the nature of mathematics and its relationship to physics, I just fundamentally and radically disagree. For a shorter version of Smolin's argument, see this essay, which he has recently submitted to the FQXI essay contest. I've been writing something about how I see the topic, will blog about it here very soon. What I'm writing isn't a response to Smolin's arguments, but a positive argument for the unity of math and physics at the deepest level.

My problems with Smolin's point of view aren't especially about his arguments concerning Platonism and whether mathematical objects are "real" (see earlier comments about what's "real"), they're about arguments like this one, where he argues that the explanation for the "unreasonable effectiveness of mathematics" in physics is not some deep unity, but just

> mathematics is a powerful tool for modelling data and discovering approximate and ultimately temporary regularities which emerge from large amalgamations of elementary unique events.

The argument essentially is that mathematics is nothing more than a calculational tool that just happens to be useful sometimes in physics. This is a common opinion among physicists, and a big problem for me is that here Smolin is not taking a provocative minority point of view, but just reinforcing the strong recent intellectual trend amongst the majority of physicists that the "trouble with physics" is too much mathematics. As I've often pointed out, the failures of recent theoretical physics are failures of a wrong physical idea, rather than due to too much mathematics, with the multiverse just an endpoint of where you end up if you throw away all non-trivial mathematical structure in pursuit of a bad idea.

In his essay, Smolin gives a discussion of mathematics itself which I think few mathematicians would recognize, defining it as "the study of systems of evoked relationships inspired by observations of nature", and consisting in bulk just of elaborations of the concepts of number, geometry, algebra and logic. I started my career in physics departments, and I'm well aware of how mathematics looks from that perspective (even if you have a lot of interest in math, like I did). My experience of moving to work in math departments made clear to me that the typical ideas of physicists about what mathematics is and what mathematicians do are highly naive, with Smolin's a good example.

I'll end with just one example of what I see is wrong about the conventional physics view that Smolin represents. A big application of mathematics to physics is the use of the rotation group SO(3). In that case it's true that many of the applications can be thought of as concerning approximate aspects of complicated physical systems, and derived from working out precisely the implications of our experience dealing with the 3d physical world. But, besides the chapter on angular momentum operators (and thus SO(3) representation theory) in every quantum mechanics textbook, there's an earlier chapter where the Heisenberg commutation relations are given as fundamental postulates of the theory. A concise way of stating this postulate is that quantization is based on a specific unitary representation of a Lie algebra (the Heisenberg Lie algebra). This is not approximate, but the fundamental definition of what we mean by quantum theory. The structure here is very deep mathematics (appearing for instance in number theory, the theory of theta functions and of Abelian varieties), and is far removed from the kinds of mathematics that one runs into as typical approximate calculational tools when studying physical problems. This is just an example, but there are many others. I don't think that if you look at them you can sustain the argument that deep mathematics and deep physics are not close cousins with a unity we only partially understand.

Anyway, more detail to come about this…

Comments

1. **al**
   February 15, 2015

   " is the wave-function "real"? " of course it is but probabilities are not.
   A (verbal) description might look as if it is complete without being so, e.g. Russell's barber; in the real world you go there and check if the barber shaves himself.

Einstein's positivistic ghost is still much too "real".

2. **Peter Morgan**
February 15, 2015

Given that any set of formal rules could be chosen, and the consequences within that set of formal rules worked through, and called mathematics, then the question would seem to be how we decide which sets of formal rules are "interesting". Additionally, physicists seem often happy to work with a mixture of formal rules and slightly less formal rules, what perhaps might be called "engineering" rules and related to what Lakatos calls Bridge Principles.
Perhaps mathematics should be thought of as something other than or more than just formal rules, but if any set of formal rules is a putative mathematics (which I take to be a large set, and that people over millenia have tried many possibilities), from which we choose whichever sets of formal rules prove to be interesting, or, if we are physicists, "useful" with a judicious addition of engineering rules, then it would seem that evolution would ensure that mathematics will be effective.
I have repeatedly thought that I might put this into an FQXi contest entry, but I have no wish to read the literature I would have to read to put this in academic context, and anyway I don't much care whether this is what mathematics is or not. Tautology is wonderful: insofar as it continues to be useful, we will continue using it.

3. **Cormac O'Raifeartaigh**
February 16, 2015

Hi Peter, the first Appleyard piece you cited is protected by a paywall. The second will probably sound very impressive to ST readers but is actually quite poor – the argument is terribly muddled by the fact that the author repeatedly conflates theories that are backed by strong empirical evidence with ones that are not. Indeed, the author seems something of cosmology skeptic, which is a different sort of argument

4. **Peter Woit**
February 16, 2015

Cormac,
The Sunday Times link is paywalled, but the next link is to the same text, available for free at Appleyard's site.

5. **raisonator**
February 17, 2015

"Part of what Smolin is referring to I think I'm sympathetic with: the nature of time remains mysterious in a way that space isn't."
I see this exactly the opposite way. Time comes from Von Neumann algebras, something that Alain Connes has been preaching for quite a while, but which has been little recognized, I guess.
But where does space come from? Everyone who pretends to know and claims that space is not so mysterious has to answer the question why it comes with three dimensions.

6. **Cormac O'Raifeartaigh**
February 17, 2015

Apologies Peter, I misread the sentence. However, I remain unconvinced by Appleyard's arguments, I think he's making a very different point to (say) Ellis and Silk, mainly because he hasn't really understood the material, what do you think?

7. **Peter Woit**
February 17, 2015

Cormac,
I haven't paid much attention to Appleyard's views, but I seem to generally disagree with his point of view. For instance, in the review he writes

"In the late 1980s, when physicists were insisting they were on the verge of a "theory of everything", I found myself using some of the arguments that now appear in this book — I was, primarily, suspicious of the claim that the universe was made of maths."

as you can see from my essay, my point of view is diametrically opposite.

8. **Nathalie**
February 17, 2015

"Difficult though it is, you should buy it and, whether you read it or not, look after it. This might be one of the most important books of our time. Or not. Right or wrong, like Thomas Piketty's Capital in the Twenty-First Century, it is an event." So says Appleyard.

I don't see why I should spend my time on reading wild speculations which don't lead anywhere. Wouldn't it be much better to follow instead the advice "shut up and calculate" and thereby perhaps have a chance to contribute to the advancement of real knowledge?

9. **fermi**
February 17, 2015

I disagree with Appleyard and Woit both, especially Appleyard. The discussion of whether Mathematics has (or should have) an unfair and undue influence on physical theories is irrelevant: We don't choose what's to be relevant, the Nature does! If we can learn more about Nature by learning more about Math, I say the more power to Math then.

If there was another (equally predictive) alternative to Math for forming theories of Nature, I'd welcome it. But I don't know what it is, if indeed such an alternative exists. The philosophy certainly does not cut it. Of course religion explains everything, but it predicts absolutely nothing, and it is useless for a Scientific theory. What else is left but Math that Physics can rely on?

10. **JSi**
February 18, 2015

Mathematics being "a […] tool for modelling data and discovering approximate and ultimately temporary regularities" automatically follows if one denies the platonic status of any laws of nature. That mathematics is a "powerful tool" is an acknowledgment.

The quote just says (1) what we see is temporary, including the laws, and that (2) we have only an approximate description of what we see, and that (3) mathematics is powerful for describing what we see. All these sound quite fair to me, and also ultimately empirical.

If nature is somehow simple, as it may be because a simple structure is more probable (on some bayesian or complexity theory sense), then a branch of research studying formal structures and aiming for generality easily has structures similar to nature. That the coincidences are sometimes surprising may be because our intuition does not quite grasp the entire thing we have built (mathematics), or its relationship with structures of nature. That the structures are beautiful has more to do with the aspirations of the mathematician than with anything outside.

SO(3) in quantum mechanics puts mathematics in a position more like a language. But of course the whole theory and all the concepts, including three dimensions, invariances implied by the Lie group, etc., may all be approximations. That we have an expressive and elegant language to define a theory still does not give the theory any metaphysical privileges.

(I haven't read Smolin's book, which is one reason I might be wrong. Also I'm not a mathematician or a physicist.)

11. **Anonyrat**
February 18, 2015

If I ask "why are coordinate systems unreasonably effective in enabling computations about the physical world?" virtually everybody would reply that coordinate systems are not real, they are constructs we impose to enable measurement and calculation.

The relationship between mathematics and physics may be like that of coordinates to measurement and calculation.

12. **Bob Finney**
February 21, 2015

Roberto Unger also submitted an essay in the FQXI contest. Some of his observations are pertinent to this discussion.

"According to the first family of ideas, mathematics is discovery. It is the progressive (or recollected) discovery of truths that exist in a domain of mathematical facts uncomplicated by the vicissitudes and variations of the manifest world.

According to the second family of ideas, mathematics is invention: the free development of a series of conventions of quantitative and spatial reasoning. This conventional practice of analysis maybe rule-guided or even rule bound, but the rules themselves are inventions. There is no closed list of motives for this inventive practice. Some have little or nothing to do with the deployment of mathematical analysis in natural science. Others take this deployment as their goal." – Unger

Unger's central point:
"Mathematics is an understanding of nature emptying it out of particularity and temporality: a view of nature without either individual phenomena or time. It empties nature out of them to better focus on one aspect of reality: the recurrence of certain ways in which pieces of the world connect with other pieces. Its subject matter are the structured wholes and bundles of relations that outside mathematics we see embodied only in the time-bound particulars of the manifest world"

As a committed Platonist his essay has given me much to think about.

13. **Gva**
February 27, 2015

Smolin's writing on the subject of the "reality" of time rubs me the wrong way. There's an unnervingly moralizing tone to a lot of it that recalls creationists railing about the theory evolution damaging public virtue (other even less savory parallels come to mind). That this tone starts creeping into his work around the time he joins FQXi hardly calms my nerves.

Nebulous concerns about the effect that concepts in fundamental physics might have on society at large should be regarded as out of bounds in an intellectually honest debate, as it's ludicrously parochial to expect nature to shape itself to conform to human preferences.

Towards a Grand Unified Theory of Mathematics and Physics

February 14, 2015
Categories: Uncategorized

A draft of an essay I've written, with plans to submit it to the FQXI essay contest, is available here. Constructive comments welcome…

People who have a take on the subject that has nothing to do with what I'm writing about are encouraged to submit their own essays to FQXI, but not to post them here.

Update: Thanks to all commenters for often helpful comments. I've revised the essay a bit, mostly by adding some material at the end, material that to some extent addresses important issues raised by some commenters.

Update: The essay has been submitted and is posted here.

Comments

1. **Joel Rice**
   February 14, 2015

   Nice essay. But I don't see what is so geometrical about the CKM mixing, and suspect that it is Perfectly Reasonable Effectiveness of Mathematics.

2. **Anderson**
   February 14, 2015

   I don't know, but I always felt that saying "it seems unreasonable that mathematics, which is a creation of the human mind, should be effective in understanding the world" is kind of the same as a bird saying "it seems unreasonable that my wings, which were created by my body, should be effective in helping me navigate through space"

3. **Jeff M**
   February 14, 2015

   I've never understood why anyone thinks mathematics being effective is surprising. I mean, basic math is the real world. Animals count. When my dog is chasing a rabbit, it's doing all sorts of fancy computations, but it's just nature. Same as when I'm catching a baseball. No one says "wow, how surprising, English (or fill in your favorite other language) is so surprisingly effective at describing the real world." Math is a language. It's got more uncompromising rules, but still.

4. **CIP**
   February 14, 2015

   Peter,

   I like the way you showed how the development of mathematics and physics since Wigner has continued develop new and more powerful links between them – but I doubt that Wigner would have been surprised. I'm not sure though, that the aspired to unification is any closer than it was in Newton's day.

   Mathematics continues to proliferate worlds in in endless profusion, but physics (I hope) still seems to be pretty singular, despite the MW movement.

   PS – Since you referenced it, I looked at your QT, Groups, and Representations book. The part I read looks very promising, and a nicely chosen level. Publication date?

5. **Garrett**
   February 14, 2015

   That's an excellent piece. It could, though, use a minor copyedit–mostly some minor wording adjustments and a sprinkling of commas. You might also add to the abstract the point you are making that not only has mathematics been "surprisingly" useful in describing nature, but that advances in our description of nature have been "surprisingly" leading to new and deep mathematics. If I were to add something else, it would be that the success of mathematics, and specifically the ubiquity of Lie groups, representation theory, and differential geometry, in our description of nature strongly suggests that our world is, fundamentally, some rich and beautiful mathematical structure, come to life.

6. **Peter Woit**
   February 14, 2015

   Thanks Garrett!

   CIP,
   Thanks! I'm working on the last half of the book this semester while teaching a course based on this, hope is to have something fairly complete by the end of the semester in May. For a completely finished product, sometime next year…

7. **Metatron**
   February 15, 2015

   @Anderson and Jeff M

   The mathematics at hand is far from basic mathematics. For example, Einstein's theory of General Relativity makes use of Riemann's mathematical creation, differential geometry. I'm pretty certain in 1853, when Gauss asked Riemann to prepare a Habilitationsschrift thesis on non-Euclidean geometry, nobody could foresee higher dimensional differential geometry being applied in 1907 by Einstein to describe gravity as curvature in a four-dimensional spacetime. Moreover, the theory of Riemann surfaces is central to the study of worldsheet dynamics in string theory, a theory of quantum gravity. And let's not get started on Riemann's celebrated zeta function, as its generalization, L-functions play a pivotal role in the theory of Motives, which arises in the Langland's program.

8. **Marc Nardmann**
   February 15, 2015

   I would argue that the "intriguing pattern of U(1) hypercharges" (§4.2) is to a large extent understood: Instead of formulating the Standard Model with the group U(1) x

SU(2) x SU(3), one should — essentially equivalently — use S(U(2) x U(3)), which has U(1) x SU(2) x SU(3) as a sixfold covering. This reveals that the Standard Model fermion representation has a rather simple form: It is the direct sum of two parts, each of which is the restriction of a representation of the identity component of Spin(3,1) x U(2) x U(3). The first part is the tensor product of [A] the right-handed Dirac representation of the identity component of Spin(3,1); [B] the representation Lambda^even of U(2) on the exterior product Lambda^even(W), where W = C^2 is a 2-dimensional unitary complex vector space corresponding to weak interactions; [C] the representation Lambda^odd of U(3) on the exterior product Lambda^odd(S), where S = C^3 is a 3-dimensional unitary complex vector space corresponding to strong interactions; and [D] the trivial representation on a 3-dimensional unitary complex vector space C^3 corresponding to particle generations. The second part is the same up to replacing the right-handed with the left-handed Dirac representation and replacing the representation Lambda^even of U(2) with Lambda^odd.

This description encodes in particular the U(1) hypercharges. It is not mysterious that Lambda^odd(S) occurs here instead of Lambda^even(S): switching between these two exchanges particles with antiparticles. But the fact that the representation Lambda^* of U(2) — which has no obvious geometric origin — is coupled with the — clearly geometric — Dirac representation hints at a geometric origin at least of the weak interaction. In this sense I agree that we are indeed "missing some piece of geometrical structure", as you (PW) write. But while we do not yet have a good explanation of the Standard Model fermion representation, we should not make things even more mysterious than they are by expressing them in terms of U(1) x SU(2) x SU(3) instead of S(U(2) x U(3)).

By the way: "Wigners" -> "Wigner's", "solutons" -> "solutions", "or details" -> "for details".

9. **rfp**
February 15, 2015

great essay! Minor typo: in second bullet of Sec 8, it should be "for details" (not "or details").

10. **TonyK**
February 15, 2015

Also "underly" and "solutons".

11. **al**
February 15, 2015

Ivor Grattan-Guinness (who passed away last december) wrote in 2008 a paper that I would like to see quoted just as often as Wigner's essay:

Grattan-Guinness, I., "Solving Wigner's mystery: the reasonable (though perhaps limited) effectiveness of mathematics in the natural sciences." The mathematical intelligencer 30.3 (2008): 7-17.

12. **andrew**
February 15, 2015

how similar is wigner's thought concerning the "unreasonable effectiveness of mathematics" to putnam's no-miracles argument for scientifc realism? putnam thought that if theories weren't "real," confirmation of their novel predictions was miraclulous.

13. **Paul Levy**
February 15, 2015

While this article is far more coherent and persuasive than Lee Smolin's, it has one glaring weakness in common with Smolin's – it doesn't anywhere say what mathematics is. I don't think it's tenable to argue for a "grand unified theory of mathematics and physics" unless you answer this question.
For background, I'm a mathematician with a recreational interest in the philosophy of the subject. I read Wigner's article some time ago and wasn't particularly convinced by it, not least because the same wonder he feels about applications of mathematics to physics is often felt by mathematicians about applications of one field of mathematics to another – but this sense of wonder doesn't stop Platonism from being an obsolete philosophical framework.
As a specific comment, I think the quote you give from Frenkel about the Langlands programme is hyperbole. (However, I see the book can be downloaded online, so perhaps I'll be convinced by it…)

14. **Martin**
February 15, 2015

Einstein's starting point for the general theory was the equivalence principle:

http://en.wikipedia.org/wiki/Equivalence_principle

heavy mass is inertial mass.

Einstein thus tried to formulate a theory in which this distinction is not made anymore. Only after that did the mathematics come into view.

Another thing is that physical models are only effective, and thus not everyting derived mathematically from a useful model is necessarily true. See vacuum fluctuations. The idea that the vector potential A can be thought of as being a connection field only came after the Maxwell equations had been formulated, and these derived from the older descriptions of electricity and magnetism. Maxwell's idea to include the displacement current D was a physical insight.

The idea that mathematics can lift the veil of Nature in a post-empiricist manner is a form of Idealism.

15. **PFD**
February 15, 2015

One last typo (because the devil is in the details): " (…) it's difficult to impossible to predict (…)". Difficult or impossible? 😉
Also "it's" -> "it is"

Thanks for sharing. Good luck.

16. **JG**
February 15, 2015

I liked your essay, it is a bold suggestion that String Theory could be replaced one day by mathematical structures coming from ideas in the Langlands program.

I wonder how you decide what type of mathematics is going to be useful for describing nature – for example, is a rigorous measure theoretic Hilbert Space formulation of QM required? If not, why is measure theory not as applicable as, for example, number theory?

17. **Peter Woit**
February 15, 2015

Thanks to all who pointed out typos, those at least are fixed.

JG (and others),
I'm not making an argument that one can just look to mathematics to decide how to describe nature. One needs to first look at our empirically most successful theories (e.g. the SM and general relativity), and then try and understand, amongst the many ways of mathematically formalizing them, which of these ways relates them to the deepest known mathematical structures. If there really is a unity there, the known connections between successful physical ideas and successful mathematical ideas will suggest other such connections worth looking into. This may lead to progress, on either the mathematical or physics end.

18. **Peter Woit**
February 15, 2015

Paul Levy,

Yes, the unreasonable and unexpected effectiveness of one field of mathematics in another is common and I'd argue sometimes evidence for unity in mathematics that we don't yet understand. The argument for unity with fundamental physics is an argument that there is something going on here at the deepest levels that we don't understand, and from that point of view we likely don't yet know the correct best definition of "mathematics". There are lots of definitions one can now come up with for "mathematics", from looking at what mathematicians do and coming up with some generality that covers it all, but I don't think that leads to anything particularly interesting or truly gets to the heart of what the deepest mathematical ideas are telling us about.

19. **hopffiber**
February 15, 2015

A bit funny, but totally unsurprising I guess, how you write about all these nice subjects and ideas and totally avoid mentioning the words string theory even once, despite it being so intimately connected both to the geometric Langlands and the (2,0) theory. It's ok I guess, but somehow feels a bit bordering on intellectual dishonesty: string theory is really quite an important part of how people came to these ideas, so my advice would be to add a bit about how the Langlands duality is really a S-duality of string theory, how (2,0) is the world-volume theory of M5 branes and so on. Of course that would go totally against the narrative this blog is trying to sell, so as I said, it's really not surprising.

20. **Peter Woit**
February 15, 2015

hopffiber,
I don't see how adding discussion of the complicated story of often heavily over-hyped claims about string theory and the Langlands program would add anything worthwhile to the essay. If you want to discuss the ideas that are in the essay, that's fine, but if you or others just want to argue about "string theory", you'll have to find another posting where it's relevant.

21. **tomate**
February 15, 2015

I think the essay has lots of problems and it has no chance to win the contest:
– It's obviously written in a great hurry
– Very low-profile, but if that's your style that's OK
– There is no effort in being pedagogical, soon you go into rings and Lie groups, bundles and whatever. A basic understanding of what kind of physics is going on is completely missing.

My suggestion is, if you are serious about this, you should try to tell a story that people smart enough and with a background in physics, but not your own field, should be able to follow. Nevertheless, I do agree on the general ideas. For some quite different thoughts on the connection between mathematics and physics, you might want to take a look at the words of V.I. Arnold,

http://pauli.uni-muenster.de/~munsteg/arnold.html

Typos: "underly"!!!, "Milss"

22. **Peter Woit**
February 15, 2015

tomate,
Thanks. Actually I did put quite a bit of time into thinking about the essay, wrote a couple initial versions that I discarded as being unworkable. It is true that I put very little time into proof-reading, thanks to you and others for helping me there.

The main problem with the initial versions is that I started out trying to explain in detail more of the mathematics and physics, but then realized that I was starting to write something that would be at least 90 pages, not 9. To really explain Weil's use of the Heisenberg algebra and the adelic point of view on theta functions, what an automorphic representation is, topological quantum field theory, etc., etc. is a major project. The rules of the contest say you can add a page or two of technical material, but that wouldn't help significantly. So, I finally decided the best I could do was just lay out in the space given an outline of the ideas I'm referring to. Some experts will recognize what I'm talking about, others I hope will find something intriguing that encourages them to go out and learn more (I should try and find some more, better expository references). Trying to write about these topics without referring to rings or Lie groups, or that level of basic tools in mathematical physics I don't think is really possible without producing something content-free.

In any case, trying to win a contest is not why I decided to do this. This was just a good opportunity to put together these thoughts and get something written that perhaps others will find of interest.

23. **Mayer A. Landau**
February 15, 2015

In the bibliography, the publication date for Clifford Algebras and Lie Theory by Meinrenken is 2013 not 2003.

24. **Qwertz**
February 16, 2015

Refs. 1,2: bold type goes too far.
Refs. 2,4,8: "Birkhauser" -> "Birkhäuser".

Ref. 5: “A., and” -> “A. and”, comma after “Program” missing.
Ref. 14: “Grassmanians” -> “Grassmannians”, “curves.” -> “curves,”, “113(1982)” -> “113 (1988)”.
Ref. 15: “3(1988)” -> “117 (1988)”.
Ref. 16: Comma after “E.” missing.

25. **Art**
February 16, 2015

Maybe add a ref for Atiyah and Singer? and some discrimination of your conclusion from the MUH? You cover a lot of ground…

26. **lun**
February 16, 2015

One comment:

I think the relationship between mathematics and physics is more of an N to 1 correspondence than “unity”. It is true that the laws of physics are elegantly encoded in formal structures provided by mathematics (a school of mathematics known as constructivism posits this is in a sense a tautology because
we construct such formal structures in terms of what we know).
However, at least as far as we know, most formal structures thought up by mathematicians, or even theoretical physicists, have nothing whatsoever to do with the real world, and assuming they do because they are formal could mean a false lead.
For instance, in the speculation section of your paper you mention six-dimensional superconforman N=(2,0), which is indeed being studied for its formal properties. But I never saw any hint that such a theory has anything to do with the real world.
Likewise, the objects being touted as being relevant to the number theory connections you mention in section 6 are nothing like realistic quantum field theories, for pretty fundamental reasons.

Perhaps this way of thinking is too superficial and reductive, but on the other hand perhaps there are good reasons why such theories are unrealistic, and focusing on their “elegant properties” connected to mathematics will make us miss them: In the 19th century the leading minds of mathematics studied potential inviscid flow without asking themselves why this tended to be such a poor model for “real” fluids, especially low viscosity ones where such methods should apply best, because potential flow was so elegant and so connected to vector calculus. It turns out that real fluids are much more elegant and complicated and fascinating, for reasons that you would never understand focusing on potential flow.

27. **Peter Woit**
February 16, 2015

Mayer A. Landau, Qwertz, Art,

Thanks, typos fixed, some references added. Not sure whether it’s worth trying to refer to other points of view and draw distinctions, that’s a long story. For the MUH, I’d say the difference is that it also posits an identity of math=physics, but that all mathematical structures are equally good. My interest is in the question of why certain very specific mathematical structures appear in this context.

lun,
Yes, there are lots of mathematical structures and one real world. But the evidence is that there are certain more fundamental structures, and the puzzle is that these show significant relations to fundamental physics. The example of the N=(0,2) 6d theory was given mainly to illustrate the fact that it is becoming widely accepted that we need new ways to define qfts (beyond picking fields and a Lagrangian). Finding new such techniques might give added insight into the fundamental physical theory.

28. **Radioactive**
February 16, 2015

I mostly agree with tomate, and your excuses will really not fly. A good essay makes advanced subjects feel accessible, this one doesn’t. And further the thesis seems to be: The mysteries in physics are linked to some even more mysterious mysteries in mathemathics. The smart mathematicians, without any physical insight; knowing any facts about nature, or doing experiments, thought or otherwise, will be able to solve it all for us. You attack string theory for much less.

29. **Peter Woit**
February 16, 2015

Radioactive,

I wish I could do better at explaining some of these concepts in a few pages, but am not so sure it’s possible. There is no royal road….

Most of what I’ve actually written here is a long list of examples of things we DO understand, linking deep ideas about fundamental mathematics and deep ideas about physics. Yes, I expect more of this to come, illuminating mysteries both of mathematics and physics. You’re very defensive and skeptical at the idea that help from mathematics will solve problems in physics, but I can assure you that the reaction of many of my mathematician colleagues about my views is to be equally defensive and skeptical about the idea that anything coming from this kind of physics will help solve problems in mathematics. Personally I believe information flow in either direction is about equally likely.

As for the idea that all physicists need is “physical insight”, I don’t think this has historically worked very well in the absence of any experimental results to provide data for physical insight to operate on. If nothing new turns up at the LHC, the problem of no data is going to be a very significant one going forward, and I don’t think physicists should refuse to consider that what mathematicians know might be of help. And, hey, even if that doesn’t work out, they might find themselves solving the Riemann hypothesis problem and getting a million dollars…

30. **Ben**
February 16, 2015

Peter,

Regarding the remarks of tomate and radioactive, anyone who seriously follows fundamental physics generally, rather than just a particular approach, will understand the main import of most of your essay, and can easily look up the details. Nor is it inappropriate for the context; an essay about exotic smoothness structures by Torsten Asselmeyer-Maluga in the 2012 contest was probably much steeper from the perspective of the typical non-expert, but was nevertheless one of the winners, and now appears as one of the chapters of the resulting Springer book.

A quibble I have is the generality implied by "mathematics." No one will argue against the centrality of number theory, geometry, and representation theory, but mathematics is vastly broader than that, and I don't think this approach has any unique claim to represent the essence of mathematics, or to pair it with physics in a way that is necessarily superior to other approaches. However, some degree of hyperbole in advertising shouldn't offend anyone.

Typos: group of \mathbf{F}_p, move commas and periods inside quotes (illogical, I know!)

31. **Paul Florijn**
February 17, 2015

Peter,

IMHO section 8 should have an additional point of the following kind:

\item (or \bullet) If an unusual space-time structure, such as quantum foam, occurs around the Planck length or some other very small length scale, completely new mathematical structures could appear in QFT.

32. **Als**
February 17, 2015

I think that you're overcomplicating things.

The reason there are so many links between current math/physics research is the probable existence of a "universal geometry" which is the true geometry of space-time and the source of all the geometries encountered by mathematicians. I expect Grothendieck's favorite toys (topos, motives) to play a significant role in its eventual discovery.

33. **Faustino**
February 17, 2015

You say: 'I wish I could do better at explaining some of these concepts in a few pages, but am not so sure it's possible. There is no royal road….'
So is it not possible for a not specialist to undertendimg anything about the question why it seems unreasonable that mathematics, which is a creation of the human mind, should be effective in understanding the world?

34. **Dom**
February 17, 2015

Faustino:
As a non-specialist in these areas I would say that analogies can only take you so far, sometimes off in the wrong direction – e.g. the Higgs Field/British Prime Minister moving through a room one.

35. **Maurice Carid**
February 17, 2015

Hi Peter,

From the contest guidelines:
"Foremost, the intellectual content of the essay must push forward understanding of the topic in a fresh way or with new perspective. While the essay may or may not constitute original research, if the core ideas are largely contained in published works, those works should be the author's. At the same time, the entry should differ substantially from any previously published piece by the author. "

What is new in your essay?
Isn't it meant to be a review?

36. **verruckte**
February 17, 2015

I've never been surprised at the effectiveness of mathematics in describing and predicting the 'real world', either. I take it that they both are a description, or reflection, or condensation, of the same thing. I imagine there is some kind of of 'Ur'-reality that is not accessible to us. That's not very useful I suppose from a technical aspect, but as a layman that's how I think of it.

37. **Peter Woit**
February 17, 2015

Ben,
I agree that what I'm writing about is just one particular aspect of mathematics, just as I'm also writing about just one particular aspect of physics (which I awkwardly call "fundamental", with no corresponding word for mathematics). In the case of both physics and mathematics, the great majority of the subject and the questions people are addressing aren't about "fundamentals", and that's a good thing. It's only because people think about specific "non-fundamental" problems and discover new phenomena in math and physics that we find out about unexpected ideas about fundamentals. Just sitting around thinking about fundamentals themselves is typically an activity that goes nowhere.

38. **Peter Woit**
February 17, 2015

Faustino,
I think non-specialists can certainly can understand the general picture I'm trying to advocate. Understanding the specifics of the aspects of quantum field theory and

number theory I'm writing about does just require a major investment in time, you're not going to understand these things by reading an 8 page popular essay.

39. **Peter Woit**
February 17, 2015

Maurice Carid,

One comment would be that no ideas are ever completely new, with just about every sensible thing anyone comes up with having appeared in some form earlier.

I think some of the ideas about the relationship of QFT and number theory are new (they're new to me, I didn't understand them a few years ago, and don't know anywhere else they're written down). There are very few details in what I wrote, I hope to write something with specifics later this year.

I suppose the "grand unified theory of math and physics" claim isn't new, since I get claims of that sort in my e-mail or mailbox every few days. But maybe a non-crackpot such claim would be new…

40. **Radioactive**
February 17, 2015

There may be no royal road to geometry, but someone who has been down it can usually give a brief travel guide. You have undoubtedly read John Baez's and Terrence Tao's writings on very advanced topics that are neither trivial nor alienating.

A flow of ideas from mathemathics to physics and vice versa is welcome and, dare I say, necessary. But historically significant breakthroughs have come from experimental results, or in the case of GR thought experimental, from thinkers with incredible insight into the physics and the mathemathics involved. The historical precedent makes me very skeptical that progress in physics will come from what is basically pure math, but of course doesn't preclude it. However there is no argument that there is a 'lack of experimental results' when there are exciting developments from Plank and others coming quite regularly. The pessimism generated by the LHC's failure to drown us in new particles is also quite misguided. Null results have had an important place in physics. It is the lack of new ideas in light of it that is most disappointing.

As for the unreasonable effectivness of mathemathics: in math, where ideas are generalised as much as possible and no more, it is not surprising to me that connections can be found between different broad categories. Nor that complicated objects (like gauge theories) may have different aspects illuminated more or less by looking at them in different lights, using different, possibly very complicated, mathematical tools.

41. **Peter Woit**
February 17, 2015

Radioactive,
Tao and Baez are great expositors, but I think you'll find that their expositions are great because they write at the necessary length. I had a 9 page limit, not 90, so was not doing exposition. If you want exposition, try the book I'm writing, which seriously tries to do that (although surely not as well as Tao or Baez). It's at 450 pages and more to come…

Sorry, but claims that all is fine with fundamental theoretical physics research because of "exciting developments from Plank and others coming quite regularly." is just hype. There's a reason for little to no progress beyond the standard model over the last 40 years, I don't think physicists are doing themselves any favors by being in denial about it.

42. **Toby Young**
February 18, 2015

Peter,

your essay looks like a winner to me, definitely in the spirit of past winners such as: It From Bit or Bit From It 2013.

Personally, I would have liked to have seen more on the extraordinary interplay in the 80s, starting with Instantons from QFT leading to Donaldson theory, Donaldson invariants, simplified using Seiberg–Witten gauge theory, but I loved what you put in all the same.

43. **Maurice Carid**
February 18, 2015

Peter:

> But maybe a non-crackpot such (Grand Unified) claim would be new…

Yeah, but this hasn't this claim already been made by Frenkel in the
quote of your paper? He calls it "grand unified theory
of mathematics" but he includes quantum field theory so
I guess he means the same?

> I think some of the ideas about the relationship of QFT and number theory are new

You mean the three sentences after Frenkel's quote dealing with
adele rings and the sentence
"In a hazy analogy…" further down the page?
I hold a PhD in physics but never heard about "adele rings". I just asked
a fellow mathematician and he said "yeah that's something from number
theory, but I'd had to look it up".
I fear this doesn't cut the following requirement from the contest guidelines:

"Accessible to a diverse, well-educated but non-specialist audience…"

What is new in your essay seems to be accessible only to specialists
in number theory.

44. **S**
February 18, 2015

Hi Peter,

I enjoyed the essay and look forward to its final form. One thing I would like to see better addressed is just what a "grand unifying theory of mathematics and physics" would even look like. Not in its specifics, but what such a beast even IS. For example, Tegmark might say that his "Multiverse IV" is just such a theory. I imagine you would not find it so, but I don't think your article is clear enough about what the words even mean to say why not (or, perhaps, why). It focuses mostly on specific mathematical and physical objects or theories.

Another question that comes to mind comes from the fact that mathematics is far larger than those things that turn out to be important in physics. SO(3) is an important Lie group in physics, for example; if SO(73) is, I'm not aware of it, but nobody would be too shocked if SO(73) turned out to have some at least moderately interesting mathematical properties (I'm thinking along the lines of 1729 / Ramanujan), or even to be really important in some far-future mathematical theory. Physics thus seems irreducibly more concrete, and any "grand unified theory" of physics and math seems (to me, right now) like it will have to fall quite a bit short of explaining a lot of interesting features of mathematics just by virtue of its being at most coequal with physics.

I also fear you may come close to dancing with a category error; but it's a speculative essay about a very interesting and fun thing to think about, one that might be fruitful, and it doesn't claim to be rigorous at all; so I think this is pretty forgivable.

Thanks for posting.

45. **verruckte**
February 18, 2015

One of the questions that I've always been fascinated with is : Is there 'more' in mathematics than in physics, in the following sense — Is there a mapping between everything provable in mathematics and some facet of the 'real' world of physical laws. Whether there is or is not such a mapping seems to me to be a key feature of a unified theory.
I don't know whether it would be more mysterious if there weren't such mapping or if there were, but either way it seems fundamental to me.

46. **Jim Beyer**
February 18, 2015

Interesting Essay.

FWIW, I am not a physicist or a mathematician and found many of the pronouncements descriptive and believable, but over my head. I don't really know what the target audience is for this, but how many outside the area really even know what a Lie Group is?

Two points of interest (to me anyway) arose from this essay. The first is the assertion that experimental evidence has been elusive for the past few decades, so we really need either mathematical path finding or perhaps, some GR-type "deep thinking" to get off the plateau we are stuck on. The essay seems to argue for the former.

Which brings me to my second point. I risk getting into the String Theory issue here, but isn't that precisely an example of mathematical path finding NOT working? Hasn't String Theory been driven/seduced by the beauty and elegance of the math? Even if one is more supportive of the String Theory issue, one can see the risk of exploring an area of mathematics in the hopes of furthering our understanding of physics, but which has nothing to do with how our world works or functions at all. String Theory has been wandering in a box canyon for 30+ years, due in some part to periodic revelations of mathematical elegance.

So, to me, the essay has pushed me to believe the next step forward will be from some kind of "deep thought" insight, like which Einstein achieved to give us GR. Maybe mathematics will get lucky, but there are obviously risks involved.

Finally, it would seem to be that mathematics is completely abstract but limited (by our own intellects) whereas the physical world is somewhat less abstract and likely somewhat less limited. So neither field encompasses the other. We seem to be near some kind of boundary that we can't quite surmount. My gut tells me the deep thinker will best the mathematical explorer.

I also couldn't figure out the HTML stuff…..

47. **Peter Woit**
February 18, 2015

Jim Beyer,

The "string theory is elegant math" claim is endlessly repeated by everyone, and it seems to be hopeless to get most people to understand the problem with it. "string theory" encompasses a wide range of different things, some of which are elegant math and some of which very much aren't. The efforts to use string theory to unify physics are now based on postulating an infinity of ugly possibilities, the antithesis of elegant math.

There's lots of elegant math that has no relationship to fundamental physics, I'm not arguing at all that one just needs to look for elegant math. One is looking for elegant math that has explanatory power about physics. The essay points out past examples of this in the past and argues there will be more in the future. String theory just isn't especially relevant.

48. **Aaron Sheldon**
February 18, 2015

There is a typo in the first sentence of section 8.

Overall a pleasant summary of very beautiful topic, the study of symmetries.

In the introduction it would be helpful to clarify the problem that is meant in the statement "grand unified theory of physics and mathematics".

Do you mean strictly looking for a theory that has both QM and GR as the limiting cases?

Or is this a more ambitious program of looking for a single theory from which the results of any experiment can be derived at least in principle?

The former being a reasonable expectation of mathematics and physics, while the latter might be fundamentally forbidden within the context of mathematical logic (i.e. could the latter goal be shown to be equivalent to the halting problem? If you had a theory that predicted everything then could it predict when any algorithm halts?).

49. **Radioactive**
February 19, 2015

So you think actual experiments like Planck/Bicep/LHC2 are just hype but some fuzzy hope that number theory is going to help with fundamental physics is a solid path?

I don't deny that there are some problems with fundamental theoretical physics research, part of the reason I left the field and the reason I read this blog, but not knowing enough math and paying too much attention to experimental results are not problems I ever observed. Quite the opposite in fact.

The essay is supposed to be about the surprising unity of math and physics but reads more like an essay on the surprising unity of math and math.

50. **Erik**
February 19, 2015

Dear Peter,

You might be interested in checking out this contribution to the essay contest: http://arxiv.org/pdf/1502.04794.pdf

51. **Yatima**
February 19, 2015

@Jim Beyer

"Finally, it would seem to be that mathematics is completely abstract but limited (by our own intellects) whereas the physical world is somewhat less abstract and likely somewhat less limited."

I would say it is the other way around. In Mathematics, you 1) Select objects that allow some form of arrangement 2) Posit invariants over these arrangements 3) Select a Logic (itself a subject and part of of Mathematics) 4) See where the Logic leads you, trying to steer clear of breakdowns (logical contradictions)

Given that humans explore the above structure, exploration is necessarily limited to symbolic computation by Turing machines. So far, this has not been an area of concern or we can work around the hang-ups nicely (Uncomputable number found? Just call it \Omega and press on)

The physical world seems to implement a particular structure that is amenable to explorations of the above kind. The universe can be described by a finite substructure of itself, which is nice! Why is this so and whether this description has some remarkable property (e.g. it may an "island in the space of consistent theories", not need any "arbitrary values" or may be the only use in need of some "highest order structure") are of course open questions.

I sure hope it has remarkable properties. Getting some arbitrary ooze out of all the work would be a let-down.

52. **Stuart**
February 19, 2015

Most mathematicians are of the opinion that deep down at the fundamental level of nature there is just numbers nothing else but numbers.This is totally mistaken since mathematics by definition is an empirical science. It born out of observations and experiments on reality. Differential geometry and its other forms are born from the properties of space-time. By making more profound studies of the physical properties of space-time new mathematics arises.This approach led Newton to discover calculus. If one on the other hand tries to start with abstract mathematics and hopes to explain reality one ends up with String Theory. A theory with no predictive power nor a connection with reality.

53. **Igor**
February 19, 2015

Stuart, can you cite a modern and respectable mathematician who expressed the view that "deep down at the fundamental level of nature there is just numbers nothing else but numbers"? Numbers are only elements of a very particular mathematical structure, that of a field, and there are much more algebraic structures which are richer our even prettier than "numbers". Why should nature be in a fundamental level a particular mathematical structure, be it a field or anything else? Also, you can construct alternative mathematics using alternative logic (fuzzy or quantum logics for example). What kind of mathematics is nature made off if any at all?

Now, the view that "mathematics by definition is an empirical science. It born out of observations and experiments on reality" is a very naive and simplistic one of mathematics. It may be true that historically mathematics grew out from empirical observations, but mathematical truth is completely independent from physical experiments, and you do not even need to be a platonist to see this. I do not think that Grothendieck ever needed directly an experimental result to produce any discovery in algebraic geometry.

54. **Igor**
February 19, 2015

This idea of a unified theory of mathematics and physics seem very presumptuous to me, to say the least. Even if a better understanding of the mathematical structure of the Standard Model, as discussed by Peter in his text, or of a theory of quantum gravity based on string theory, as defended by Michael Rio in his own essay (1502.04794v1), can produce new non-trivial applications of sophisticated pieces of mathematics, motivate new conjectures, new insights to the Langlands program &c., there will still exists many other mathematical theories outside such 'unification' that, nevertheless, mathematicians still regards as interesting. So is this a *really* unification of mathematics?

For example, the Greeks could very well consider the theory of statics of Archimedes as an unification of physics and mathematics (indeed, they not even separated physics, mathematics and philosophy at all), since it is an application of Euclidean geometry, which is basically the whole of Greek mathematics, to describe physical configurations.

I think that the most interesting part of Peter's essay is the relation of number theory and QFT, but which could very well be much more developed. Well, maybe in another opportunity!

PS: All this talk remembers me of Gregory W. Moore's http://www.physics.rutgers.edu/~gmoore/PhysicalMathematicsAndFuture.pdf

55. **Peter Woit**
February 19, 2015

Stuart,
"If one on the other hand tries to start with abstract mathematics and hopes to explain reality one ends up with String Theory."
You really can't blame string theory on the mathematicians. It was invented by physicists in the late sixties who had little interest in mathematics, pursued by physicists during the seventies and early eighties who had little interest in mathematics, became popular when someone influential decided it was a promising physical idea about unification (yes, he happens to also have done work with great mathematical significance), and string theory unification is now based on a physical idea (the landscape) which is completely mathematically empty.

Erik,

I saw that. It's a good example of what I don't have in mind: the idea that some piece of abstract mathematics applied to a conjectural framework with no known connection to fundamental physics is the way forward. I try to make clear that what I have in mind is mathematics applied to a deeper understanding of the specific QFT that has been successful. I don't think the idea that applying some abstract math will tell you what M-theory is and show how to do unification is at all a promising one. Lots of effort has gone in that direction in the last twenty years, with nothing to show for it.

Radioactive,
Actually, as far as telling us anything about fundamental physics (as opposed to dust), BICEP2 IS just hype. Despite the endless claims of the last ten years about how Planck data would tell us about fundamental physics, I'm having trouble seeing how it has changed anything in our understanding of physical laws. About LHC2, one can hope for the best, but despite the hype, I think the most physicists feel that the odds now are that it will not see anything unexpected.

To put the reason for interest in number theory in a language that physicists might find more palatable, the claim is just that progress in fundamental physics has often come in the past from a better understnding of symmetries and how to exploit them. Mathematicians know a lot about symmetries that has not yet found application in physics (representation theory is a large field), and the Langlands program is currently one of the most active areas in mathematics where new work is being done on the exploitation of symmetries.

56. **Peter Woit**
February 19, 2015

Igor,

If you haven't looked at the latest version of the essay, see material I've added at the beginning of the concluding section which addresses the "unification of mathematics" question. But yes, the essay is intended to be somewhat presumptuous and provocative.

I do hope to someday get something serious written about qft and number theory, but that's a big project, and I think our understanding is still very fragmentary.

57. **verruckte**
February 19, 2015

"The universe can be described by a finite substructure of itself, which is nice!"

I don't think that's been determined yet, without the word 'partially' in there before 'described'. Can it be, though? I think that it's a very interesting question. I personally think the answer is 'No, it can't be'. I suppose if physical reality is actually infinite in extent, then it sort of follows that it could contain another arbitrarily large subset, but I still wonder. What does it mean to describe something completely? Don't you essentially have to make an actual copy, and thus break the no-cloning quantum theory constraint?

It is interesting to note, though, that what we think of as mathematics is being performed by a bunch of ionic charges skittering around in a lump of water and protein. If math really is 'larger' than physical reality, which I gather most of you think, it makes me doubtful that we would ever be able to apprehend it fully with our lumps of protein.

58. **Radioactive**
February 20, 2015

I suppose you and I have different outlooks. I am still willing to wait for the results of experiments, overhyped or not, null or not, because they provide grist to the mill of high quality theoretical physics. Whatever the right theory of fundamental physics at high energy scales, there is no guarantee that it will be mathematically elegant but it certainly will predict the results of experiments.

Speaking of things that have been overhyped, I have heard uncountable times how advanced representation theory/topology/number theory/something else under the mathematical sun is going to lead to a new understanding of fundamental physics. This essay is speculation that the Langlands program is going to help fundamental physics because it will help us unify everything back to the one holy mathematical Object. It sounds like ancient philosophy instead of natural science. The "unreasonable effectiveness of mathematics" itself is overhyped and you are helping to build it.

59. **Peter Woit**
February 20, 2015

Radioactive,

If theorists want to sit on their hands and wait for new experimental results that may be a long time coming (as in, after they're dead…) they can do that. One problem with this tactic is that their colleagues may decide to stop hiring more theorists, and there is some data indicating that is happening, see

http://www.physics.utoronto.ca/~poppitz/Jobs94-08.pdf

60. **Radioactive**
February 20, 2015

If there isn't any good reason to hire theorists then why hire them? It isn't a choice between becoming a hep-th postdoc and working in a coal mine. Perhaps fewer garbage papers on the arxiv per day would be good for the field.

Since Anglo-Saxon studies and Abstract Topology departements continue to exist in universities I doubt we'll ever see a complete extinction of mathemathical physicists, though there may be fewer of them, which is not a great loss.

61. **failafail with extra hubris**
February 20, 2015

"Speaking of things that have been overhyped, I have heard uncountable times how advanced representation theory/topology/number theory/something else under the mathematical sun is going to lead to a new understanding of fundamental physics."

Yes, Herr Stark, I agree! The work of Hermann Weyl belongs more to representation theory, topology, and number theory than physics. I mean, really- a gauge theory? Pauli has already shown these structures to be as unphysical as Koenig's spin structure idea.

We should focus on physics of immediate impact in defense and industry- Indeed, Herr Stark, any new physics will manifest through your famous shifts in ultracold atoms. As to whatever Bardeen, Einstein, Oppenheimer, Noether and Weyl are erestwhile up to right now, I'm sure none of it will ever pan out. I mean, really, Herr Stark, who cares about understanding the fractional quantum hall effect? Topological order, edge effects?

What we need is still more experiment. Physics should just be all experimentalists, really- no need to even come up with models to verify! Who needs models, let's just run experiments! Then, something actually interesting comes up in those experiments, we can start developing the mathematical physics required to understand the new physical phenomena. We will simply call some of the mathematical physicists back from the coal mines.

62. **Jim Beyer**
February 24, 2015

Well, I like the new essay better.

It is probably human nature to think one's own post had something to do with your changes, I have no idea if that's the case, but I like that you'd subtly distanced yourself from the String Theory antics of the past 30+ years. That's probably a good thing.

Your point about Einstein's insight on GR being helped by Riemannian
geometry was interesting as well.

I sort of wish I could see the old essay, but that might be a chore. I will suffer my personal hallucinations as to what was actually changed…

63. **Abdelmalek Abdesselam**
February 27, 2015

@Peter

Nice essay! I also think there are interesting things to discover by combining number theory and QFT. The connection between the function field (over C) case of the Langlands program and QFT, is by now well established. However for number fields the situation seems rather unclear. There are some intriguing observations though regarding this hypothetical NT/QFT connection. For instance the Weil distribution whose positivity implies the Riemann Hypothesis looks a lot like the (multiplicatively translation-invariant) two point function of a Euclidean QFT. This was pointed out in a paper by Burnol: http://arxiv.org/abs/math/9809119
Also, Connes' trace formula in http://arxiv.org/abs/math/9811068
involves taking a limit of removing both UV and IR cut-offs just as in the usual approach for constructing a QFT model. Clearly the Weil distribution has to do with the action of rescaling group R^*, but the question is: on what?
A possible connection to such an action on QFTs, namely the renormalization group, was considered in this article by Leichtnam: http://arxiv.org/abs/math/0603576
Having studied Euclidean QFT over both R which is the usual setting, and over Q_p, I can say that the definitions and basic properties are very natural in both of these settings and they parallel each other in a very nice way. For instance, a good theory of probability measures on spaces of distributions require a nuclear topological vector space. Over R it is S(R^d) which is countably Hilbert. Over it is S(Q_p^d)
which is not metrizable. These two spaces are in some sense at the opposite ends of the spectrum ofthe general notion of nuclear space due to Grothendieck. All the good theorems like Bochner-Minlos (for constructing free boson models) or the Levy continuity, hold for these two extreme examples. In fact the theory over p-adics
is simpler than over R and therefore offers a useful testing ground of ideas for constructing QFTs rigorously. I explained that in my short "essay"
http://arxiv.org/abs/1311.4897

64. **Peter Woit**
February 27, 2015

Abdelmalek,
Thanks for the interesting comments!

This Week’s Hype

February 20, 2015
Categories: This Week's Hype

A quarter-century or so ago, one of common arguments for string theory research was that it was “the only game in town”, in the sense that it was the only possible way to get a unified theory. For instance, back in 1987 David Gross had this to say:

> So I think the real reason why people have got attracted to it is because there is no other game in town. All other approaches of constructing grand unified theories, which were more conservative to begin with, and only gradually became more and more radical, have failed, and this game hasn’t failed yet.

As years went on and string theory unification went nowhere, this often was replaced by a new “only game in town” argument, that string theory was the only possible quantum theory of gravity. This argument got strong disagreement from people pursuing Loop Quantum Gravity or any number of other ideas.

This week, Quanta magazine has a new version of the argument, reporting that “Researchers are demonstrating that, in certain contexts, string theory is the only consistent theory of quantum gravity. Might this make it true?” The new argument (based on this and this) seems to be that string theory is the only possible theory of quantum gravity because if you look at a certain class of CFTs (based on orbifolds by permutation groups) and invoke the AdS/CFT conjecture for AdS3/CFT2, the 3d gravity theory in the large N limit would have a density of states more characteristic of string theory than a conventional particle theory.

The most obvious problem here is that it is in 3 space-time dimensions, where there are no physical gravitational degrees of freedom. The S-matrix of quantum gravity is exactly calculable in flat 3d space: it’s zero. There’s a very long history of studying 3d quantum gravity, as a toy model without gravitons, but with just topological degrees of freedom. For more about this, see for instance Steve Carlip’s 1998 book on 3d quantum gravity, which works out a large number of different ways of quantizing 3d gravity (not including string theory). One problem with the argument that string theory is the only way to quantize gravity because it is the only way that works in 3d is that, as Carlip shows, there’s a long list of other completely different ways to do this (all arguably not that relevant to the problem since none have gravitons). This is also quite different than the usual argument that string theory is needed to quantize gravity, which is based on the occurrence of a spin 2 graviton in the spectrum of the string theory.

Ignoring the obvious problem of no gravitons and being in the wrong dimension, there are other problems with the argument, for instance the claim that looking at permutation orbifolds tells you about all CFTs, or the claim that a large density of states at high energy means you have to have a string theory. The article quotes Matt Strassler about this:

> But these aren’t really proofs; these are arguments. They are calculations, but there are weasel words in certain places… And just finding a stringy density of states — I don’t know if there’s a proof in that … This is just one property.

Carlo Rovelli sums up the issue with using this to hype string theory and excuse its failures:

> They should try to solve the problems of their theory, which are many, instead of trying to score points by preaching around that they are ‘the only game in town.’

I haven’t followed closely work on AdS3/CFT2, but it is a quite interesting topic, although not because it promises a proof of the “universality” of string theory. Chern-Simons theory is based on a very similar relation between a topological 3d qft and 2d CFTs, and there we have some idea what is going on, although many fascinating questions remain. One might hope that AdS3/CFT2 provides a context where one could understand things using some ideas from the Chern-Simons context. This is what Witten did back in 2007 in his paper Three-Dimensional Gravity Revisited (I wrote about this before the paper here). My understanding is that problems with Witten’s proposal later surfaced, I’d be curious to hear from an expert on the latest state of that (perhaps Witten can write a “Three-Dimensional Gravity Revisited Revisited” paper).

There are a lot of wonderful questions still not understood about this story, but I don’t see that using it to argue that string theory is the “only game in town” does anything other than throw one more thing on the pile of outrageous hype generated by string theory partisans over the last 30 years.

Update: There’s been a change to the Quanta article, adding to the quote from Lee Smolin, who is making much the same point I was making in this posting:

> “And even in that case , there have existed for a long time counterexamples to the string universality conjecture, in the form of completely worked out formulations of quantum gravity which have nothing to do with string theory.” (String theorists argue that these particular 2+1 gravity theories differ from quantum gravity in the real world in an important way.)

This whole thing really is very strange: on the one hand string theorists are arguing that only string theory can give you quantum gravity, based on an argument in 2+1 d. When you point out to them that there are well-known counterexamples to their argument in 2+1 d, they say “well, things are different in 2+1d than in other dimensions”. Just bizarre…

Comments

1. **Natalie Wolchover**
   February 20, 2015

   Peter,

   Thanks for responding to my piece with your hallmark spirit. 🙂 A couple of minor points: I’m not sure whether you’re calling the article itself “hype” or claims presented therein, but I would like to note that most of the points you raise about the limitations of this research are explored in the piece. I would also note that you’ve curated your excerpts a tad misleadingly. Strassler’s quote began “It’s clear that these papers are an interesting attempt.” Carlip, whom you mentioned, also had a mildly positive take. Most experts I spoke to (even some LQG theorists) consider these papers worth taking seriously. No one is anywhere near claiming that string universality is proven, of course. But it seems reasonable to call these consistency conditions “evidence” — a new kind of evidence, possibly, and certainly not of the smoking gun variety.

   I also think it’s worth pointing out that there are a couple of papers discussed in the story that go beyond 3D gravity. The most significant is Maldacena and colleagues’ from last summer: http://arxiv.org/pdf/1407.5597.pdf.

   Always a pleasure.

   Natalie

2. **andrew**
   February 20, 2015

   Peter, what exactly is the “only game in town” claim? Is it something like:

.
If I begin with a set of desiderata including
* Low-energy limit is general relativity in 3+1
* Low-energy limit is QFT/QM
* Lorentz invariance
* Unitarity
I would (if I could do the math etc) find a unique solution: string theory. What is the full list of desiderata?
.
If it were proven, it would be quite compelling, but not the final word (eg Lorentz could be broken at a small level in nature).

3. **Peter Woit**
February 20, 2015

Hi Natalie,
Sorry not to make it clear, but the "hype" reference was to the claims physicists are making, not the article. You do quote appropriately skeptical voices. Just thought I'd add some context though about how 3d is different….

About ignoring the Maldacena et al 4d reference, that just seemed to be a completely different topic and sort of calculation, one I'm not familiar with and don't have time to become well-enough informed to write about it. Taking a quick look I see that even they don't make any strong claims for their result:

"We should mention the grand dream of deriving the most general weakly coupled consistent theory of gravity. It is quite likely that the only such theory is a string-like theory, broadly defined. We are certainly very far away from this dream, but hopefully our simple observation about three-point functions could be useful."

4. **Peter Woit**
February 20, 2015

Andrew,
Some string theorists would like to try this style of argument, there are a variety of sets of hypotheses you could imagine. See my previous quote though, where Maldacena et al acknowledge "we are certainly very far away from this dream" and I think that's accurate.

The problem with No-Go theorems (here string theorists are hoping for a No-Go theorem about alternatives to string theory) is that very often someone finds a way around them. They can be very helpful at focusing attention on precisely what assumptions lead to an obstruction to doing something, giving ideas about what needs to be done to get around the obstruction. In this case though, I think one is so far away from anything one could reasonably call a No-Go theorem that it's just hype to make claims about one. In particular, arguments about 3d quantum gravity are just pretty much irrelevant to the topic.

5. **guest**
February 20, 2015

The strength of the proof is inverse to how many assumptions they make.

6. **Anonyrat**
February 20, 2015

While we don't live in a multiverse, we do live in a world with many towns, and a lot of league sports.

7. **Giotis**
February 21, 2015

Natalie Wolchover,

Maybe you can explain why the Maldacena, Zhiboedov 2011 paper provides "evidence that string theories are the only quantum gravity theories with a particular feature that reproduce general relativity at large scales." as the article claims?

On the contrary this paper as I see it provides evidence of Vasiliev's HS theories by confirming the famous HS_AdS$_4$/Free_O(N)_vector_model correspondence of Klebanov-Polyakov (http://arxiv.org/abs/hep-th/0210114).

But maybe I'm missing something.

On the other hand if your aim was to claim that a Supersymmetric version of HS_AdS4$_4$ is a limit of String theory you should have mentioned in addition the accompanying papers that make this claim i.e. in this case http://arxiv.org/abs/1207.4485. Otherwise the above statement in your article is meaningless and confusing.

But again maybe I'm missing something so I would be grateful if you can elaborate on this statement of your article.

8. **Carl**
February 21, 2015

The quote "the only game in town" is exceptionally apt. It's most associated with con man "Canada Bill" Jones, master of the Three Card Monte, who ironically was himself addicted to gambling and lost his money to better professionals as fast as he took from marks. Supposedly, on being advised by a friend that the Faro game he was losing money at was rigged, he replied "I know, but it's the only game in town!".

9. **Peter Woit**
February 21, 2015

Carl,
The weird thing is that many string theory proponents are well aware of the origin of "only game in town", but use this anyway. See for instance

http://www.math.columbia.edu/~woit/wordpress/?p=1917

10. **BMc**
February 22, 2015

For many years, it was regarded as a hallmark of a successful theory of quantum gravity that it should automatically exclude spacetimes that are known, on general grounds, to be unphysical. For example, it simply should not work on Goedel's famous spacetime. Indeed, if somebody came up with a theory of quantum gravity that *demanded* that spacetime must be globally hyperbolic, as all realistic spacetimes are, that would be counted as strong evidence in its favour. String theorists used to argue like this: in the heady days of 1985, some of us thought that [at least] the internal manifold could have only a tiny number of geometries, and that was [rightly] hailed as strong evidence in favour of string theory.

If the correct theory of quantum gravity only works on globally hyperbolic spacetimes, then it won't work at all on AdS. Conversely, a theory that works spectacularly well on unphysical spacetimes like AdS, indeed that is the only game in town there….. well, that is pretty powerful evidence that it is *not* the correct theory of quantum gravity. Yes, I know that things like gauge theories work quite well on AdS, but we don't look to gauge theories to select our spacetimes for us.

So the conclusion here should be precisely the opposite of the one being drawn.

Well, I don't know whether I really believe all that. But it shows how tricky, and perhaps how futile, this sort of argument can be……

11. **Aleksandar Mikovic**
February 23, 2015

Andrew,
Even with a short list of desiderata for a QG theory (well-defined theory, semiclassical limit is GR + QFT), string theory is not a unique solution, because the spin-cube models (categorical generalizations of spin-foam models) also satisfy the list. However, the complete list of conditions must include a generalization of the QM formalism to the whole universe (no external observes, the meaning of the measurement and the wave-function collapse in quantum cosmology, the role of time).

12. **Natalie Wolchover**
February 23, 2015

Giotis,

There's no doubt that the sentence in question is very vague and probably confusing to an expert. As the paper was explained to me, Zhiboedov and Maldacena considered a class of AdS4 theories with higher spin symmetry; when they deformed these theories in a way that made the higher spin states heavy, the only theories that ended up looking like GR in the right limits were string theories. So the "particular feature" would be heavy higher spin states, as opposed to massless higher spin states, which of course our universe does not have. Hope that helps.

13. **Giotis**
February 23, 2015

Natalie Wolchover,

Thanks for the answer.

I think I understand now what have happened.

Are you sure you have referenced the correct paper?

From your description I understand that the correct paper is

http://arxiv.org/abs/1204.3882

Check the motivational introduction in appendix G and the relevant conjecture.

14. **Robert Delbourgo**
February 24, 2015

I have another game, but nobody wants to play it! Oh well, that is the fate of alternative approaches against the string/brane/susy juggernaut and one must just accept it. That is life.

15. **smtx**
February 24, 2015

The S-matrix cannot be zero, it is a unitary matrix, it must be the identity. When Einstein invented special relativity, SR survived on its own merits. It didn't matter if anyone wanted to play Einstein's game, or stick to the juggernaut of the luminiferous ether. Whatever new game anyone wishes to offer today will have to prove itself on its own merits.

16. **si**
March 1, 2015

Regarding experimental "confirmation" of the String theory, there will be a Gordon Research Conference in June. Its title is "String Theory & Cosmology (New Ideas Meet New Experimental Data)". I'm rather interested to see what the new experimental data are.

17. **Peter Woit**
March 1, 2015

si,
Thanks. More about this here
http://www.grc.org/programs.aspx?id=16938
I see the participants are going to explain about
"Testing String Theory Through Observational Cosmology"
Wonder what new "test of string theory" they've got?

Once they're finished in Hong Kong, many of them will fly to Aspen to discuss the same topics
http://www.aspenphys.org/physicists/summer/program/currentworkshops.html

Quick Links

February 25, 2015
Categories: Uncategorized

- The LHC is getting close to the point where it can be restarted with a 6.5 TeV beam energy. Latest news here, schedule here. Plan is for a sector test late next week (beam in part of the machine), beam in the whole machine March 23. First physics run May 18th.
- Next week there will be a conference in Venice devoted to neutrinos, blogging going on here.
- There may be some progress on the Mochizuki/abc front. Ivan Fesenko has written up some notes that try and put Mochizuki's ideas in context with some other more conventional parts of mathematics. The week after next will see a workshop in Kyoto, with lectures from Go Yamashita on the abc proof. Another recent survey talk by Mochizuki is here.
- The latest AMS Notices has a series of articles about the life and work of Arthur Wightman, one of the main figures in the effort to make rigorous sense of quantum field theory.
- In my essay about math and physics, I mentioned the Atiyah-Bott work on the moduli space of solutions to the Yang-Mills equation in the case of Riemann surfaces. This has an intriguing analog to the function field case, which was discussed already by Atiyah and Bott. Dennis Gaitsgory has a new paper out that touches on this in the context of his proof (with Jacob Lurie) of the Weil Tamagawa number 1 conjecture for function fields (see here). The new paper has the footnote: "The contents of this paper are joint work with J. Lurie, who chose not to sign it as author."
- Returning to physics, Princeton University Press announced that Frank Wilczek will edit a Princeton Companion to Physics, modeled on the wonderful Princeton Companion to Mathematics, which was edited by Tim Gowers. Publication is planned for 2018.
- Caltech hosted a workshop the past couple days, inaugurating the Walter Burke Institute for Theoretical Physics. Hopefully they're well-funded enough to put videos or slides online. John Preskill's remarks at a celebration of the event are available here. He gives some principles for doing science that I very much agree with. In recent years I've become especially aware of the importance of his first principle: "We learn by teaching", since I've been learning a lot that way. As the trend grows towards institutes modeled on the IAS and prestigious positions that involve no teaching, I think this needs to be kept in mind.

  I also agree with his last principle: "Nature is subtle", and found very interesting his comments on the holographic principle:

  > Perhaps there is no greater illustration of Nature's subtlety than what we call the holographic principle. This principle says that, in a sense, all the information that is stored in this room, or any room, is really encoded entirely and with perfect accuracy on the boundary of the room, on its walls, ceiling and floor. Things just don't seem that way, and if we underestimate the subtlety of Nature we'll conclude that it can't possibly be true. But unless our current ideas about the quantum theory of gravity are on the wrong track, it really is true. It's just that the holographic encoding of information on the boundary of the room is extremely complex and we don't really understand in detail how to decode it. At least not yet.
  >
  > This holographic principle, arguably the deepest idea about physics to emerge in my lifetime, is still mysterious. How can we make progress toward understanding it well enough to explain it to freshmen?

  From what I can tell, the problem is not that it can't be explained to freshmen, but that it can't be explained precisely to anyone, since it is very poorly understood. The AdS/CFT conjecture is now older than some of my current students, with a literature of more than 10,000 papers, but taking a look recently (see here) at what should be a toy model case (AdS3/CFT2) reminded me just how little seems to be truly understood. This is a quite odd and I think historically unprecedented situation.

- Somewhat related to the holography question, for anyone interested in condensed matter physics, I recommend taking a look at Ross McKenzie's blog Condensed Concepts. He discusses some of the issues related to attempts to use holography in condensed matter. He also has a recent paper (with Nandan Pakhira) showing a violation of a bound suggested by holographic arguments.

Update: On the multiverse mania front, tomorrow Science Friday is hosting Sean Carroll to continue his war against falsifiability and the conventional understanding of science, joined by Seth Lloyd to help promote the multiverse. Perhaps it should be "Pseudo-science Friday"?

Update: Michael Harris's book now has a blog, which promises to discuss topics that didn't make it into the book.

Update (March 7): Beam is back in the LHC (well, at least in a part of it). There was a successful test today, sending beam into one sector in one direction, two in the other, see here.

Comments

1. **gadfly**
   February 25, 2015

   I'm very empirically driven and not an expert at all on quantum gravity, can anybody summarize or point me to a good summary of why the holographic principle, in spite of having no apparent empirical justification, is held in such high regard?

   This is not intended to be hostile in any way, I'm just genuinely curious.

2. **Peter Woit**
   February 25, 2015

   gadfly,
   I think there are two main sources of physical interest in this, since it (adS/CFT) seems to non-trivially relate two different quantum theories:

   1. strong coupling on one side gets related to weak coupling on the other. So, for systems where you don't understand the strong coupling theory (for instance string theory on the adS side, QCD or some condensed matter system on the CFT side), this promises to turn your problem into a weakly coupled one you can understand (e.g. a gauge theory on the CFT side, a weakly coupled gravity theory on the adS side). If this really works, it becomes a powerful calculational method for theories we otherwise know little about.

   2. Gravity theories on the adS side get related to more conventional field theories on the CFT side. This promises to make sense of quantum gravity.

   You'll need an expert to explain exactly what the state is of understanding about when this works and how this works exactly (i.e. what exactly are the two dual theories, and what exactly is the mapping between them). One problem for the non-experts is to sort out what's hype and what's a solid result (I'm guessing Preskill's "all information in the room is in the walls" business isn't on the "solid result" side….). My recently quick attempts to understand what was known in the AdS3/CFT2 case didn't get me far, which surprised me since we know a lot in that case about both sides of the correspondence (2d CFTs, 3d gravity theory).

3. **Ben R**

February 25, 2015

The idea of gravitational holography originally came from the Bekenstein entropy bound, which is convincing because it doesn't depend on any particular theory of quantum gravity but only on the same sort of quasiclassical argument that gave us Hawking radiation. Take a look at gr-qc/9310026 and hep-th/9409089 (which predate AdS/CFT). The latter even has pictures showing how the projection onto the boundary is supposed to work.

4. **Art Brown**
February 25, 2015

Re "We learn by teaching", it's notable that the three examples Dr. Preskill referenced, Einstein, Schockley, and Bardeen, did not. Teach, that is. RIP, Bell Labs.

5. **Douglas Natelson**
February 25, 2015

Art Brown: Well, Bardeen did at UIUC after Bell Labs, but your point is taken.

6. **RogerP**
February 26, 2015

"From what I can tell, the problem is not that it can't be explained to freshmen, but that it can't be explained precisely to anyone, since it is very poorly understood."

"What can be said at all can be said clearly." – Ludwig Wittgenstein –

7. **Alex**
February 26, 2015

On the topic of the holographic principle being held in such high regard, I have a naive question. What is the difference between the holographic principle and specifying the physics via boundary conditions? "all information in the room is in the walls" seems like an obvious quote given that the fundamental field equations are second order and hence are uniquely specified by giving the values of the fields on the boundary of the region?

8. **Peter Woit**
February 26, 2015

Alex,
These are quantum field theories, not classical field theories. And, again, the real interest here is in the fact that strong coupling in the bulk theory is getting related to weak coupling in the boundary theory, and vice versa, allowing access to information about strongly coupled theories.

9. **Alex**
February 26, 2015

Peter,

I acknowledge that relating strongly coupled theories to weakly coupled ones definitely seems like a result but now I'm confused by your statement: "These are quantum field theories, not classical field theories". Are you suggesting there's some sort of fundamental difference between solving a quantum field theory by specifying boundary conditions as opposed to an analogous classical field theory?

10. **Peter Woit**
February 26, 2015

Alex,
Yes, the whole question of boundary conditions is very different in QFT, and the AdS/CFT story does not seem to be the conventional one about specifying boundary conditions. But I'd love to hear from someone expert who could tell us more about exactly what AdS/CFT does say (or at least provide references).

11. **gadfly**
February 26, 2015

Peter,

Thanks, that was informative. I've only heard bad results about actual applications (for instance, I heard that AdS/CFT or something closely related can be used to get a theory of quark gluon plasmas, but with the fine print being that it is the worst such theory), have there been any cases where it has been exploited with undeniable success?

12. **Bernhard**
February 26, 2015

I read Sean Carrol's piece again. Nothing I haven't read before but still deeply disturbing. I suppose one should just ignore this sort of thing. It could be laughable it it weren't so sad:

"The truth is the opposite. Whether or not we can observe them directly, the entities involved in these theories are either real or they are not. Refusing to contemplate their possible existence on the grounds of some a-priori principle, even though they might play a crucial role in how the world works, is as non-scientific as it gets."

Is someone really refusing "to contemplate the possible existence" of a multiverse (who?) or is and others selling this as a serious scientific idea that deserves acceptance (and applause) based on nothing? He is completely misleading people here so to appear that people are being stubborn and blind to this great revolution whereas he and other geniuses are being so open minded. It reminds me that Feynman once said: "with an open mind I do not mean an empty mind!"

13. **Peter Woit**
February 26, 2015

gadfly,
I'm really the wrong person to answer that question. From what I've seen, the most detailed understanding is available in the case of AdS5/CFT4, relating N=4 SUSY Yang-Mills to supergravity and superstring theory. The interest there is that weakly coupled strings and gravity, which you can compute with, tell you about strongly coupled SUSY Yang-Mills. The applications to the quark gluon plasma involve making this work for the non-SUSY case, and it is there that I think the trouble arises and you no longer have a controlled approximation method.

Bernhard,
Sean keeps attacking the straw man who "refuses to contemplate" the multiverse, not acknowledging that serious multiverse critics do contemplate it, just don't see any conventional scientific evidence for it, or any plausible path to getting any. If there's no way to get such conventional scientific evidence, then contemplating the multiverse is much like contemplating the existence of angels and supreme beings. Nothing wrong with that, but it's just not science.

14. **Anonyrat**
February 26, 2015

" The AdS/CFT conjecture is now older than some of my current students….This is a quite odd and I think historically unprecedented situation."

The closest recent thing I can think of is renormalization theory – from Steukelberg (1943), Schwinger, Feynman (1948-49) – to Wilson (1971) — 28 years, enough interval for the birth to PhD of a physicist.

There is a big difference with AdS/CFT, of course, that even when ill-understood, renormalization theory was known to work in the physical world.

15. **gadfly**
February 26, 2015

Why is Sean Carroll so committed to the multiverse?

16. **Peter Woit**
February 26, 2015

gadfly,
He long ago wrote a paper about how the multiverse would explain the arrow of time problem, and that had a lot to do with his first book. I have no idea how he got into that, it was at a time that the multiverse started to be a hot topic among string theorists.

It has always mystified me why he's running simultaneous campaigns for the multiverse and against religion. I would have thought that if you were intensely devoted to upholding science and rationality against religion, you'd be inclined to stay away from the parts of science that, at best, skate on the edge of what is science.

17. **student**
February 26, 2015

The introduction of arXiv:0912.0959 provides a fairly concise introduction to basic aspects of the AdS_3/CFT_2 correspondence and references to earlier reviews. Holography in this case is the correspondence between type IIB string theory on AdS_3 x S^3 x M and the CFT with target space Sym^N M where M = K3 or T^4 and N is taken to be large. Early checks of the duality included comparisons of the moduli spaces, (BPS/protected) spectra, and symmetries. More recent progress (circa 1999) is the matching of 3-point functions of BPS operators between the CFT and supergravity dual. The equality of the 3-point functions is explained by a very non-trivial non-renormalization theorem.
One of the difficulties is that the dual string theory is not at the symmetric product orbifold point in the CFT moduli space. That is why many of the early checks compute quantities that are (covariantly) constant over the CFT moduli space. Going beyond protected quantities, the "toy model case (AdS3/CFT2)" is perhaps less developed than some of its higher dimensional cousins. For example the anomalous dimension of the non-BPS Konishi operator in N=4 SYM in the large N limit can be computed for all values of the t'Hooft coupling using spin chains and integrability.

18. **Jon**
February 27, 2015

Peter,

I think there is a link between Sean Carroll's commitment to the multiverse and his campaign against religion. The multiverse is often seen as a counter argument to the idea that the fine tuning of physical constants provide evidence for God.

I'm pretty sure I've heard him use this argument in the past in debates against theists.

19. **Peter Woit**
February 27, 2015

student,
What I'm wondering about is not the kind of very specific duality conjecture you mention, but the philosophy that this duality phenomenon is general, that (for instance) to every CFT2 you'll get a gravity theory on AdS3. That's what is being discussed in the Quanta article, or in Preskill's claim about the boundary of a real room. What's the exact conjecture there, what's the evidence for it, and what's the state of understanding of why it might be true?

Also, how much recent progress is there? You refer to recent progress from 1999, but that's now a very long time ago…

Jon,
Yes, but my point was more about his war against falsifiability, since that's conventionally the first argument you make against the hypothesis of a deity (it's unfalsifiable). The problem with "the multiverse did it" as an explanation of fine-tuning is again falsifiability. If you don't have conventional scientific evidence for it, it's the same sort of explanation as "the big guy upstairs did it".

20. **Jim Harbough**
February 27, 2015

I read his blog (as well as many others). Sean loves Everett's MWI and is almost fanatical about it. It strikes me that he is now focused on cosmological philosophical questions and less on being a physicist.

21. **Bernd**
February 27, 2015

The violation of the holographic duality bound is based on DMFT calculations, which is a bit like string theory for strongly correlated fermions in the sense that it is somtimes sold as "the only game in town". Nobody knows how accurate these methods really are.

22. **Bill**
February 27, 2015

Speaking about duality: http://arxiv.org/abs/1502.07719

23. **Chris W.**
February 27, 2015

Both the multiverse adherents and the theists would have something if they could say that their favored explanation "explains fine-tuning [or whatever] in precisely this way [insert details here], and if you don't believe me *you can check it using these methods [insert details here]*."

Of course, they can't do that. The theists don't really care, because their agenda is apologetics for a faith, which is a nearly 2000-year-old practice originally directed at Roman overlords. The multiverse adherents have a somewhat different problem. They're asserting that certain features of the observed universe are simply accidental, with no explanation in physical laws. The trouble is that they can't point to *actually existing* alternative instances (of a universe) where those features differ or are missing. All they can do is conjecture that such instances exist, and then resort to the last refuge of scoundrels: "You can't prove that they don't exist!" The argument thus degenerates into another form of apologetics.

24. **Peter Woit**
February 27, 2015

Bill,
What that paper is doing looks like the attempts to get "string theory" out of the strong coupling expansion in lattice gauge theory that were very popular when I was a grad student in the early 80s (work of people like Polyakov and Migdal). The problem with this is two-fold:

1. You get some sum over a lattice version of surfaces, with certain weights, and you can call this "string theory" if you want, but it has no known relationship to what you normally call "string theory", the continuum quantization of the vibrating string.

2. To recover continuum gauge theory, which is asymptotically free, you want weakly coupled gauge fields at short distances. Here, you've got a strong coupling expansion, which is about not g=0, but g=infinity, with no way of getting to the physical g=0.

Anyway, I haven't read the whole paper, but I don't see any new idea that gets around those old problems (or even any claim that what the author does gets around those problems). Unless I'm missing something the claim by the author to have the first explicit gauge-string duality is completely misleading, since this sort of relation between gauge theory and sums over surface has been well known since the late 70s, and is also well-known to not give you the kind of duality between gauge theory and a physical string theory that you are looking for.

25. **hopffiber**
February 27, 2015

Peter,
>Also, how much recent progress is there? You refer to recent progress from 1999, but that's now a very long time ago…

I'm sure you know some of this, but every rational 2d CFT is proven to have a holographic dual theory of CS type, which can also be described as a topological string theory. This is thought of as a simpler sector of the full AdS3/CFT2 duality, where non-rational 2d CFTs are thought to be dual to (non-topological) string theories in AdS3. There is plenty of observations supporting this, but no real proof, but this understanding is for sure more recent than 1999.

And of course there is continuously a lot of evidence for higher-dimensional specific cases of AdS/CFT produced all the time, through techniques like integrability, localization, entanglement entropy and so on. Of course the general case is very hard to study, so investigating "simple" examples is the best we can do. But the fact that a lot of examples do work in non-trivial ways do point to some degree of generality, at least to me.

26. **Mark**
February 27, 2015

Peter, here are two references that do what you want. They provide evidence that large-N CFTs with a gap in the spectrum of operator dimensions is dual to semiclassical gravity in AdS:
http://arxiv.org/pdf/0907.0151.pdf
http://arxiv.org/pdf/1403.6829v2.pdf

27. **Giotis**
February 28, 2015

Especially for AdS_3 I'm not sure I understand the point of the discussion.

The fact that every consistent theory of QG in AdS_3 is a CFT_2 is ancient history. It was proven back in 1986 by Brown-Henneaux in the celebrated paper

"Central Charges in the Canonical Realization of Asymptotical Symmetries: An Example from Three-Dimensional Gravity".

Specifically they showed that in AdS_3 the asymptotic symmetry is generated by a Virasoro algebra.

28. **Shantanu**
February 28, 2015

Peter and others to me, looking at the agenda of previous neutrino telescopes in venice workshops. experimental neutrino physics seems to be like experimental incarnation of string theory. Just as string theory makes no contact with experiment, experimental neutrino physics seem makes no contact with theory or any BSM physics, despite bold claims that it is first evidence for physics beyond standard model.

29. **Jeff M**
February 28, 2015

Pardon the ignorance of a mathematician, but I'm kind of curious about the whole AdS/CFT thing. I mean AdS has negative cosmological constant, and the universe has positive cosmological constant, no? So whatever the proposed duality is (and it is conjectured, not proved), what the hell does it have to do with the actual universe? Pretty, yes, if someone can actually prove it, but physics?

30. **Peter Woit**
February 28, 2015

Jeff M,
There's no claim that AdS/CFT itself has anything to say about the physical case of our universe. There is a hope that it might inspire finding a similar duality that would apply (see "dS/CFT duality"), but, like for many questions about generalizations of AdS/CFT, I've never understood exactly what the state is of the search for such a theory.

31. **bitboy**
March 1, 2015

Some thoughts on the arguments/comments by Sean Carroll and Seth Lloyd in the NPR segment:
– Comments on falsifiability of string theory were disingenuous to say the least:
– Contrary to what SC and SL stated, most folks that claim that string theory is not falsifiable don't do so because the experiments are out of reach(Do they really think so many people that use the word "falsifiable" are so ignorant about such things?) but because there are _no_ real proposed experiments that can validate or invalidate string theory("validate" in the sense of an experiment that can confirm some unique prediction of ST).
– Sorry, "GR falls out of string theory" is not a valid argument.
– They both seem to think that scientists should do science and leave philosophy to the philosophers and that physicists use "science should be falsifiable" as a catchphrase. Apparently these two physicists are the exception to this rule and are sophisticated enough in the philosophy of science to have the right to admonish other physicists in their use of philosophical ideas.
– Sean Carroll states that "The lesson here is scientists should respect the field of the philosophy of science". I guess they should do this by trashing certain parts of that field, like the idea of falsifiability. 🙂

32. **martibal**
March 2, 2015

Peter, a bit off topic but since you mention it and this is something I always wonder: is it possible in a few words to explain why there is no dS/CFT theory ? Is it just a technical difficulty, or there is a deep reason for which the A in AdS is important ?

33. **Peter Woit**
March 2, 2015

martibal,
Maybe someone else has a good answer. My problem is that I don't really understand why AdS/CFT, so no idea about why no dS/CFT. Many of the general arguments about the ubiquity of holography seem to apply to either dS or AdS.

34. **fg**
March 2, 2015

A rather intriguing statement I have heard about the AdS5/CFT4 correspondence is that the fifth dimension in AdS5 in a sense encodes the virtuality/off-shellness. In the CFT in 4 dimensions, there are loop corrections that correspond to the possibility of having virtual excitations (e.g. a quark that fluctuates momentarily into a quark+gluon state). In the AdS5 side of the correspondence, weak coupling corresponds to a purely classical theory that has no quantum fluctuations. The statement was that, for some specific observables, one can relate rather precisely the 5th coordinatein AdS5 to the virtuality of the modes that would be running in the loops in the QFT side of things.

35. **MathPhys**
March 2, 2015

On a different matter, I have just realized that Tullio Regge, one of the most original theoretical physicists in the 20th century, has passed away in October 2014. He was 83, and I don't think that this was widely announced outside the Italian Physics community.

His observation in 1957, at the age of 26, that scattering amplitudes in non-relativistic quantum mechanics are controlled by poles in the complex angular momentum plane led in 1967 to the Veneziano amplitude and from there to almost everything discussed in this blog.

36. **Peter Woit**
March 2, 2015

fg,
I've also heard it claimed that one could think of the 5th dimension in terms of the renormalization group flow. Again, I've never seen a precise statement.

37. **S**
March 2, 2015

@martibal,

I'm certainly no expert on AdS/CFT, but as I understand it, the geometric setting for the theory is an asymptotically hyperbolic manifold: take a compact manifold-with-boundary \bar{M} and put a metric g on the interior (M) such that g blows up to second-order. Then for any defining function f of the boundary (f is zero precisely on the boundary and df is nonvanishing there), f^2g is a metric on the whole manifold-with-boundary. We can choose different functions f, and so restricting f^2g to the boundary, we induce a conformal class of metrics there (but not a well-defined Riemannian metric). Hence the *conformal* in the CFT. The interior metric can then be expanded in a well-defined way in powers of the defining function, where the coefficients are determined by the conformal geometry of the boundary, say N.

The problem is that the curvature of the interior metric is asymptotically -|df|^2, where this norm is taken in the metric f^2g. Thus, we can have negative-curvature metrics on the interior, or maybe asymptotically flat ones, but we can't have positive-curvature ones.

This I *think* is the reason, but I don't claim to know a lot about Lorentzian geometry, or (goodness knows) the AdS/CFT conjecture.

38. **S**
March 3, 2015

(I should have noted that in order to have the above-mentioned expansion, the interior metric g must be Einstein — hardly a small point).

39. **matthew foster**
March 3, 2015

My recollection is that the idea behind the connection with AdS and the RG picture is that AdS encodes the conformal group geometrically. I.e. the conformal group for the boundary theory is generated by SO(d+1,1) (for a Euclidean theory), and these appear as Killing vector fields which encode isometries of the AdS metric. Then it is natural to view the RG as a propagation through the space(time), say by following a one-parameter family of diffeomorphisms generated by the dilation operator, as you move away from the boundary theory. But I'm not sure if this is precisely how it works in practice, i.e. N = 4 SUSY Yang-Mills. In this (hand-waving) version it's a very pretty idea that isn't obviously tied to string theory.

40. **Sam**
March 5, 2015

Peter,

Why does nobody say anything about the almost one year delay in the announcement of the Lucasian chair occupant? It is a rather curious situation for such a prestigious professorship.

Sam

41. **Ross McKenzie**
March 6, 2015

Bernd commented

“The violation of the holographic duality bound is based on DMFT calculations, which is a bit like string theory for strongly correlated fermions in the sense that it is somtimes sold as “the only game in town”. Nobody knows how accurate these methods really are.”

I disagree. My detailed response is here.

GR100

March 7, 2015
Categories: Uncategorized

I hadn't thought until recently about the fact that this year is the 100th anniversary of Einstein's discovery of the field equations of general relativity, so there will be quite a few events taking place commemorating this (for a list of some, see here). This week's Science magazine has a special issue on the topic. It includes news stories about LIGO and gravitational waves, new tests of the equivalence principle, and possible tests of GR from observations of the black hole at the center of our galaxy.

There's also a review of a book from a few years ago about Einstein's search for a unified theory, Einstein's Unification by Jeroen van Dongen. The review addresses something I mentioned in my recent essay about mathematics and physics, that the development of GR provides a good example of a successful theory coming out of not just experiment and "physical intuition", but motivated also by the serious use of deep mathematical ideas. According to the review:

> Einstein employed two strategies in this search : either starting from a mathematically attractive candidate and then checking the physics or starting from a physically sensible candidate and then checking the mathematics. Although Einstein scholars disagree about which of these two strategies brought the decisive breakthrough of November 1915, they all acknowledge that both played an essential role in the work leading up to it. In hindsight, however, Einstein maintained that his success with general relativity had been due solely to the mathematical strategy. It is no coincidence that this is the approach he adopted in his search for a unified field theory.

Besides the fact that Einstein said so, other evidence for the primacy of the mathematical strategy in this case is the simultaneously successful work by mathematician David Hilbert, who was definitely pursuing the mathematical strategy.

While I think there's an excellent argument that a mathematical approach was crucial in Einstein's discovery of the field equations, the later history this book deals with also shows the dangers this can lead to. Einstein spent much of the rest of his life on a fruitless attempt to get a unified theory by pursuing the same mathematics he had so much success with in the case of GR. It's a good idea to keep in mind both examples. On the one hand, trying out some new deep mathematical ideas can lead to success, on a time scale of a few years. On the other, if you've spent 30 years pursuing a mathematical framework that has gone nowhere, maybe you should do something else. A lesson that Einstein's successors at the IAS might want to keep in mind…

The story about new tests of the equivalence principle contains the usual nonsense about testing "string theory predictions":

> Using beryllium and titanium, they found gravitational and inertial mass equal to one part in 10 trillion, as they reported in Physical Review Letters in 2008. That's not quite precise enough to test string theory predictions.

That "string theory predicts violations of the equivalence principle" is what used to be called a "factoid", something not true repeated so often that it becomes a fact. It seems though that usage has changed, with "factoid" now often being used to refer to something true. A new word is needed.

Update: See here for an article by Michel Janssen and Jurgen Renn discussing in detail the question of the "mathematical" versus "physical" strategies in Einstein's discovery of the GR field equations.

Comments

1. **M**
   March 7, 2015

   Experimentalist: "how can I use BRST cohomology to proof that the AdS/CFT duality prediction of genus one string correction to beryllium inertial masses is not an artefact of gauge fixing the superconformal symmetry of the D5-BPS black hole background?"

   String theorist: "What is beryllium?"

2. **Chris W.**
   March 7, 2015

   I wonder how much of the Science special issue will remain available online as full text. It deserves to remain available that way indefinitely.

3. **Chris W.**
   March 7, 2015

   Based on the above excerpt, I find it interesting that the book review seems to gloss over a historical viewpoint which I thought was more or less standard, namely that Einstein was struggling to find a precise formulation of geodesic deviation, presumably without knowing at the start what that notion was.

   To put it another way, if one senses that gravity in some sense breaks the rigid metric structure of spacetime as understood in special relativity, but that that metric structure must be preserved as a limiting case, one needs an appropriate mathematical framework to consider such things. Riemannian geometry turned out to be precisely that framework. The *new* physical realization that objects moving solely under the influence of gravity were following essentially inertial trajectories drove this search for a framework.

   The problem later on was that Einstein was unable to identify such a *key* physical realization that could drive and guide a similar search for the mathematical structure of a unified theory. We are arguably in a similar position today, despite having vastly more mathematical resources at our disposal. Some would argue that the age of such key physical realizations has past, and so we can only grope furiously through the mathematical possibilities with some past successes to guide us (we hope).

4. **Oldster**
   March 7, 2015

   Perhaps what Einstein lacked during that last 30 years was a contact with a physical intuition in his mathematics that was as fruitful as the Equivalence Principle was for GR, eventually leading to Riemannian Geometry as the framework for GR. That may also be why Banesh Hoffman lamented the failure of Weyl's original 1918 unified field theory to succeed at unifying gravitation and electromagnetism, because gauge invariance is such a seemingly reasonable idea, and leads to beautiful theories as well …

5. **Chris W.**
   March 7, 2015

PS on my last comment: I really should have said "…*especially with* vastly more mathematical resources at our disposal.". The increased size of the search space for appropriate mathematical structures makes the need for the right kind of physical guidance all the more crucial.

(Note, by the way, that the obtuse identification of mathematically formulated physical theory with mathematical theory doesn't help matters at all. This is about more than just the question of relative mathematical rigor.)

6. **lun**
   March 8, 2015

   In general, string theory does predict a violation of the equivalence principle in some regime, since Newton's constant couples to a scalar field (the dilaton), which generically mixes with other scalar fields.
   If Galileo threw such scalar fields off the tower of pisa with a normal weight, they would not fall at the same rate.
   The magnitude of this violation is of course completely undetermined (and things like Moduli Stabilization are supposed to make it as tiny as possible), but it is worth keeping in mind.

7. **Peter Woit**
   March 8, 2015

   lun,
   If the magnitude is completely undetermined, there is no prediction. It's also true that some string theorists, when pressed for an example of a prediction of string theory, have argued for "no violation of the equivalence principle" as a prediction, see for example
   http://www.math.columbia.edu/~woit/wordpress/?p=151
   http://www.math.columbia.edu/~woit/wordpress/?p=559

   This is a typical example of a "string theory prediction", no actual prediction, with claims from string theorists of evidence for string theory if you see something or if you don't.

8. **lun**
   March 8, 2015

   Peter, the "reference" you give in the second link is, to put it very mildly, not up to standard of scientific literature, and the second link has very little to do with the equivalence principle (It is relatively easy to incorporate the variation of the fine structure constant into GR, just add a scalar field coupled to the photon with a VEV varying in time in the FRW metric).

   Of course what you are saying about the lack of a quantitative prediction is right, especially since string theorists have invented really ingenious ways of making this prediction irrelevant for any conceivable experiment.
   Nevertheless, since the driving principle which drove Einstein to develop his mathematics is the EP, it is worth keeping in mind that in string theory the geometrization of gravity is most likely a property of the EFT in certan vacua, not a fundamental principle of the full theory. Quite a few string theorists never even thought about it.

9. **Martin S.**
   March 9, 2015

   > A new word is needed.
   mystification

10. **EFT**
    March 9, 2015

    @ lun,

    "…in string theory the geometrization of gravity is most likely a property of the EFT in certain vacua"
    What does EFT stand for ?
    Thanks!

11. **Peter Woit**
    March 9, 2015

    "EFT" is a conventional acronym for "Effective Field Theory".

12. **Zathras**
    March 9, 2015

    "The review addresses something I mentioned in my recent essay about mathematics and physics, that the development of GR provides a good example of a successful theory coming out of not just experiment and "physical intuition", but motivated also by the serious use of deep mathematical ideas."

    Some would say it is the only such example.

13. **John**
    March 9, 2015

    I read the article about LIGO and GR waves. It was filled with words like 'certainty'. Reminds me of SUS and the LHC back in the mid 2000's. So what happens if nor waves are found by LIGO?

14. **Peter Woit**
    March 9, 2015

    John,
    The SUSY story is quite different. There's strong indirect experimental evidence for gravitational waves (the pulsars), none for SUSY. The theoretical arguments for gravitational waves are much stronger also (it's hard to come up with a theory without them, whereas the simplest theories are ones without SUSY).

    It will be interesting if nothing is seen. I'd guess that at first the suspicion would fall on the astrophysics, that the modeling of production of gravitational waves is wrong, or the frequency of events that generate gravitational wave was wrong. But sooner or later it would become clear if there was a major problem with GR, and that would be quite revolutionary.

15. **Yatima**
March 9, 2015

> “Some would say it is the only such example.”

Regrettably, “some” got a bad case of Gruppenpest and were never heard of again.

16. **srp**
March 9, 2015

I was hoping somebody would use the anniversary to officially retire the “ball rolling on a rubber sheet” pseudo-explanation which is extremely confusing if you try to think about it at all.

17. **Peter Orland**
March 9, 2015

srp,

The usual TIME-LIFE picture of a ball on a curved static rubber sheet is not even a pseudo-explanation. The main effect of gravity is that the rubber sheet is being sucked into energy (mass). The curvature of the rubber sheet is a small correction.

18. **Shantanu**
March 10, 2015

John, there are no guaranteed astrophysical sources in the frequency range of advanced-LIGO/VIRGO. So if nothing is seen, the blame will be on astrophysical sources. Note that CMB polarization experiments also now on hot on the heal of this
and could be lucky if nature chooses a value of r within sensitivity range of upcoming experiments.

19. **WLM**
March 11, 2015

A couple of useful references for a modern study of the fundamental mathematics of General Relativity (as opposed to various black-hole solutions and AdS correspondences):

David B. Malament, Topics in the Foundations of General Relativity and Newtonian Gravitational Theory. University of Chicago Press, 2012

R. K. Sachs and H. Wu, General Relativity for Mathematicians. Springer-Verlag, 1977

These are (copyrighted) books, not papers or summaries. Although versions can be found online, they are worth their purchase price. (Although I was a student of David Malament at UCI, I have no financial ties to any of the above authors).

Spring Break

March 11, 2015
Categories: Uncategorized

I'm heading off soon on spring break, planning on traveling to Scandinavia and hoping to see a solar eclipse. There hasn't been much news recently from the math and physics worlds, and it's unlikely I'll be blogging until I get back (around the 24th), so will turn off comments while away.

Comments

1. **Johan**
   March 12, 2015

   Svalbard or Faroe islands? In any case, enjoy your trip!

2. **Peter Woit**
   March 12, 2015

   Hi Johan,
   Thanks! Will be going to the Faroe islands. Would love someday to get to Svalbard, but not this time…

Back from Break

March 24, 2015
Categories: Multiverse Mania

Clouds cleared about 15 minutes too late at Torshavn in the Faroe Islands, so totality was behind a cloud, but still an impressive sight. And the Faroe Islands are quite a remarkable place to visit. Some recent news:

- The plan has been to inject a beam into the LHC this week, leading to a news item in the UK Daily Express about how Scientists at Large Hadron Collider hope to make contact with PARALLEL UNIVERSE in days. This nonsense comes to us courtesy of this paper published in Physics Letters B.
- Unfortunately the machine checkout going on at the LHC has identified a problem that may delay contact with the PARALLEL UNIVERSE for a little while. Looks like no beam this week, for details see this from CERN. Some news is put out here, details of discussions of the problem here.
- Also on the parallel universe front, Quanta magazine has an interview with Weinberg. About the multiverse, he repeats some of the arguments for it, but also says:

  > I am not a proponent of the idea that our Big Bang universe is just part of a larger multiverse.

  About string theory, the LHC and SUSY, the exchange went:

  > **If the LHC finds no evidence for supersymmetry, what happens to string theory?**
  > Damned if I know!

  Weinberg went on to respond to the issue of the testability of string theory by discussing the possible measurement of primordial B-modes, without mentioning that string theory makes no predictions at all about this.

- Quanta magazine keeps putting out some of the best coverage of math and physics available. See for instance Natalie Wolchover on penguins (although also read Tommaso Dorigo and Adam Falkowski) and Erica Klarreich on moonshine.
- Jess Riedel has a wonderful blog posting about the subtleties of the classical limit in quantum mechanics. Textbooks like to claim this is explained by just taking the hbar goes to zero limit of a path integral, but that doesn't really provide an explanation, for reasons clearly laid out by Riedel.
- Nominations are open for this year's Breakthrough Prizes, see here. There will be $3 million prizes in physics and mathematics, as well as $100,000 "New Horizons Prize" for younger researchers, up to 3 each in both math and physics. For more, see here.

Update: Sabine Hossenfelder performs the public service of reading the "PARALLEL UNIVERSES" paper and explaining what is going on here.

Update: This year's Abel Prize went to John Nash and Louis Nirenberg. Nature News has a story here. The award to Nash was for his work on PDEs and the Nash embedding theorem. He already has an Economics Nobel, for his work on game theory. This surely makes him the first person to win not-quite-Nobels in two completely different fields.

Update: Also at Nature, news about the LHC problem.

Comments

1. **Justin**
   March 24, 2015

   You beat me to posting about the moonshine Quanta article. I was going to link it to you as soon as you got back, as I knew it would be in line with your interests. Are you as excited about this research as I am?

2. **David Roberts**
   March 25, 2015

   The arXiv version of the Phys. Lett. B paper is here

3. **Bee**
   March 25, 2015

   Hi Peter,

   I just posted a rant about the parallel nonsense: http://backreaction.blogspot.com/2015/03/no-lhc-will-not-make-contact-with.html

   Best,

   Sabine

4. **momerathe**
   March 25, 2015

   I wouldn't get too wound up about the Daily Express – it's not really considered a serious source of news; more a soapbox for whatever the proprietor's latest racist or paranoid conspiracy theory is.

5. **Anna**
   March 25, 2015

   The parallel universe hype has been all over the media here in the UK – not just the Daily Express, even reaching the Metro. Another example of 'Churnalism' (the article appears to originate with physics.org) and poor scientific reporting. Unfortunately, it is being reported as being CERN's primary objective – with the inevitable reaction from the public ('money wasted by those delusional boffins').

6. **piscator**
   March 25, 2015

   The comments on the Quanta magazine article are depressing, as people take turns to explain how Weinberg doesn't know what science is.

7. **Peter Woit**
March 25, 2015

Justin,

In my case, the right word is more "intrigued". This is one topic I wish I knew more about, once I catch up on other things, hope to learn some more about it.

8. **SteveB**
March 25, 2015

Is it just me or does everyone see that Tommaso Dorrigo's articles and everyone else's articles on Science 2.0 are newly blocked? Firefox tells me "Access denied". The site appears to want me to login as if I were an author of a blog — which I am not. I have enjoyed Tommaso's articles for some years now and would miss them.

9. **Peter Lund**
March 25, 2015

> This surely makes him the first person to win not-quite-Nobels in two completely different fields.

http://en.wikipedia.org/wiki/Herbert_A._Simon

10. **Peter Shor**
March 25, 2015

Peter Lund: Awfully close, but doesn't a not-quite-Nobel prize have to be awarded in a Scandinavian country?

11. **Narad**
March 25, 2015

> This nonsense comes to us courtesy of this paper published in Physics Letters B.

As a brief aside from the substantive comments, I can't help but be dismayed that – at this late date – Elsevier is completely incompetent at generating HTML from such material, even though they're obviously throwing an algorithm at it, which appears to specialize in *hopelessly breaking things* (e.g., Eq. [1]; the rendering of the $\sim 10^{19}$ GeV" in the original in the introduction is something to behold, as someone or something had to explicitly change the font of the "GeV," which was missed in Table 1).

I generally have to go to campus to snarf papers (this one is OA), so I'm used to the amateurish typesetting in the PDFs (Eqs. [25] and [28] are in close competition), but this takes the cake. And PDF is so Not Mobilly Enough.

Gah. I hope the rant won't be received too poorly; I'm back to reading, where I belong.

12. **Thomas Larsson**
March 26, 2015

Linus Pauling won two unrelated quite-Nobel prizes, chemistry and peace.

13. **MB**
March 26, 2015

"The mass of this remnant is found to be greater than the energy scale at which experiments were performed at the LHC. We propose this as a possible explanation for the absence of black holes at the LHC."

Hmm let me see here…

P(BH | not seen at LHC) = P(not seen at LHC | BH) P(BH prior) / (P(not seen at LHC | BH) P(BH prior) + P(not seen at LHC | no BH) P(no BH prior))
~ 1.0 * P(BH prior) / (1.0 * P(BH prior) + 1.0 * P(no BH prior))
= P(BH prior).

Yep, completely useless.

14. **Geoff**
March 26, 2015

In the Freeman Dyson spirit of "It's better to be wrong than vague", I've read seemingly serious proposals from supposedly sober physicists that quantum computations would be done in parallel universes. Perhaps it is best that alleged parallel universes are discovered at the LHC. I don't think The Onion is creative enough to come up with a headline reading "Parallel Universes Discovered at NSA".

15. **Adw**
March 26, 2015

Re: the earlier GR100 post—

If it should happen that, even with more sensitive detectors, no gravitational waves are observed, would some sort of graviton extinction occurring in interstellar space be a possible explanation—or is it basically a given that nothing could plausibly scatter gravitational waves at classical length scales enough to cause a null result?

16. **paddy**
March 26, 2015

To Adw (and please pitch in folk who know better than I):
(a) do not confuse graviton detection with gravitational wave detection, and
(b) unless said energy is beam-like, scattering is not extinction.

17. **Nathalie**
March 27, 2015

There is no Nobel Prize in economy. If someone says that there is, you should tell him or her: sorry, you can't reverse time – Nobel died already in 1896 and the "Bank of Sweden Prize" in economy was created around 1969! Scientists should make accurate statements and not get carried away by incorrect statements in the press.

18. **imho**
March 27, 2015

Hmmm…

What are the ramifications of this article that claims to rule out extensions to the standard model that require significant interation between dark matter particles. Isn't this contrary to the whole "dark matter particles are supersymmetric partners" idea?

19. **Shantanu**
March 28, 2015

Peter, check out Cliff Burgess recent talk
https://cds.cern.ch/record/2002541?ln=en

20. **Dom**
March 28, 2015

On the subject of the LHC, I hope that this isn't something that has been linked many times before LHC Configurable Monitors

21. **M**
March 30, 2015

The End (of string theory) is Near:
http://arxiv.org/pdf/1503.08130.pdf

22. **Peter Woit**
March 30, 2015

M,
Quite the opposite, that paper shows that string theory research should be the highest priority for mankind, crucial for the survival of the species (as long as possible…).

23. **bugannoyer**
March 30, 2015

From Dr Sen's paper, the solution to the vacuum-instability apocalypse is to send spaceships out to 10^10 light years, where they will be carried out of causal connection with the nucleating vacuum:

"In our universe the horizon size is of the order of 1010 light years. This means that by sending out space-ships we can reach and establish civilizations on dierent worlds situated within a radius of about 10^10 light years from us today"

And:

"The cost of this endeavour is clearly going to be high"

And concludes that we need to research the vacuum parameters to best allocate resources:

"In the context of string theory this means that we need to identify the correct minimum of the potential that describes the phase in which we live and then compute the probability of decay of this phase by standard techniques."

Perhaps he could help us get a start on funding by donating a modest portion of the Milner prize? Or is it too much to hope that this is just all an early start on Wednesday?

24. **Radioactive**
March 31, 2015

Wilczek has an article about physics in the next 100 years, http://arxiv.org/abs/1503.07735 , much of which could have probably been written 30 years ago. But he still has 70 to go.

25. **Thomas Larsson**
March 31, 2015

Radioactive, it was written 14 years ago:
http://arxiv.org/abs/hep-ph/0101187

26. **Thomas Larsson**
March 31, 2015

"5.5.Produce the New Particles!
Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new R-odd particles. Even in the focus point scenario, there must be several accessible to the LHC."

– From Wilczek's future summary above.

27. **Radioactive**
March 31, 2015

Well in that he says "I expect that in ten to fifteen years we will know a lot more", and now's he's revising his estimate from O(10) to O(100) years. As far as parameter tuning BSM papers I've seen a lot worse 😉

28. **CU Phil**
March 31, 2015

Peter,

Thought you might be interested in this special issue of Philosophia Mathematica on "Mathematical Depth." It came out of a conference on the topic involving mathematicians, philosophers & historians of mathematics, and a physicist or two. Links to videos of the talks are here: https://www.youtube.com/playlist?list=PLQw7KTnzkpXfGo93vo3kQk7_jA_HgwbnC

and the foreword to the special issue itself here: http://philmat.oxfordjournals.org/content/early/2015/02/27/philmat.nkv003.full

29. **Peter Woit**
March 31, 2015

CUPhil,

Thanks! That is very interesting. It's very hard to figure out exactly what "Mathematical Depth" is, but I think it's a very real and important characteristic of some ideas.

30. **Chris W.**
April 2, 2015

On that Weinberg interview, some might find it interesting to compare his responses with his essay (book chapter) "Against Philosophy" in *Dreams of a Final Theory* (1993). I found this PDF online.

Also see this review of his new book, *To Explain the World*.

31. **Socrates**
April 3, 2015

There has been so much interest in the lay press regarding Fuwa, Wiseman et. al's papers regarding nonlocal wavefunction collapse and EPR steering, but very little discussion in the online physics community.

http://arxiv.org/abs/1412.8178
http://arxiv.org/abs/1412.7790
http://www.nature.com/ncomms/2015/150324/ncomms7665/full/ncomms7665.html

Off-topic, but interesting?

Thanks

32. **Yatima**
April 4, 2015

Off-topic, but interesting?

As I see it, yes: "QM works exactly as expected."

Various News

March 31, 2015
Categories: Uncategorized

Some news from all over:

- The problem with a short in the LHC seems to have been resolved (one can follow progress here), looks like they'll be ready to inject a beam in a few days. Also looks like they're not likely to spend their Easter Sunday doing this, so, maybe it will be next Monday?
- Cambridge has finally gotten around to choosing a new Lucasian professor (the last two were Michael Green and Stephen Hawking). Michael Cates will take the position July 1.
- Grothendieck's death last year was sad to hear about, but a positive result is that the Grothendieck Circle is back in the business of making available resources concerning his work. There's a comment at the top of the website that

  > With the agreement of Grothendieck's family, the work of the Circle to bring Grothendieck's unique story and writings to the public has resumed.

- There was a workshop this past month devoted to Mochizuki's work, but I haven't found anyone who knows what happened there. Minhyong Kim has taken to trying to write about Mochizuki's ideas on MathOverflow, see here.
- The Toronto Star has a long article about Langlands.
- At the KITP this week a new program on quantum gravity is starting. This month's Scientific American has a Joe Polchinski cover story on Burning Rings of Fire. Maybe some of the KITP talks will be enlightening, but the small amount of time I've spent trying to follow the past two years of debate on this has just left me mystified, struggling to see how the very general framework people seem to be working in can possibly lead to a resolution of the questions they're concerned with.
- Frank Wilczek has a speculative article about Physics in 100 years. A commenter here suggests comparing it to Wilczek's version of nearly fifteen years ago. The last fifteen years have not been kind to Wilczek's hopes for vindication of SUSY or SUSY GUTs, but he's not giving up yet. It will be interesting to see what his reaction will be if the next fifteen years are equally discouraging.

  I do very much like one thing in the new version, the section about possible unification of ideas of quantization and of symmetry, where he speculates:

  > Quantization and fundamental symmetry will not appear as separate principles, but as two aspects of a deeper unity.

  That's pretty much one of the main motivations of the book I'm writing (see here).

Update: There are rumors going around tonight that there's been a hoax perpetrated on the arXiv, something like the Sokal hoax. This has to do with an hep-th posting entitled Riding Gravity Away from Doomsday, which has appeared under the name of a very prominent string theorist, Ashoke Sen, winner of the $3 million Milner Fundamental Physics Prize. What I'm hearing is that no one can believe that Sen could possibly have seriously written something this silly, so it must be some sort of hoax. Speculation is that the hoax could have been carried out to make the hep-th moderators look bad, by showing that they'll agree to anything, no matter how absurd, if it invokes the Landscape and the multiverse. Some think that Sen's account must have been hacked and then used to post the nonsense paper, others think that Sen himself is behind the hoax, having had enough of the Landscape business. I'll update this as more information becomes available.

Update: At least some papers on the arXiv still are serious.

Update: Beams are back in the LHC, successfully circulated at 450 GeV on Easter Sunday (live blog here). Next step, ramp up to 6.5 TeV.

Comments

1. **George**
   March 31, 2015

   I think maybe Tuesday. Monday is a big holiday in Europe – Easter Monday.

2. **MB**
   April 1, 2015

   Yaay for the LHC getting fixed so easily!

   My opinion on the Sen piece FWIW: Honestly I don't get the hoopla about it. I'm not sure it's anything like Sokal. First of all it's an "essay" for some random competition, not a journal paper. Second, supposing the typical eternally inflating multiverse paradigm is true I can't spot any obvious flaw in Sen's (or the author's if it really wasn't Sen) reasoning, which follows as an almost trivial consequence of the assumptions. Please correct me if I'm wrong. The essential message is to spread the human race out so as to avoid vacuum decay bubbles as much as possible. He does some very crude dimensional analysis in support of this that doesn't go very far in terms of building a concrete model. If there is silliness in this it seems to stem from the starting assumptions, not anything in the essay itself.

   So: silly premise? Yes, if you think the eternally inflating multiverse is silly (as I do).
   Absurd title? Definitely. Pure, shameless click-bait.
   Sokal-esque hoax? Not convinced.
   Worth getting worked up about? Nah. Nothing to see here. Move along. If he tries to get it published in Nature or promoted as some major breakthrough then maybe start an outcry…

3. **petpanther**
   April 1, 2015

   April Fools' Day?

4. **anon**
   April 1, 2015

   Seconding MB…

   If Sen's account was hacked I doubt this would last more than a minute on the arXiv. He is not a sarcastic person and tends to be kind and differential to other's work, even when he doesn't find it so useful. So I doubt this is a hoax. My guess is that it is what it appears to be: a lightweight, fun article intended for general readers.

Why he decided this would be appropriate for posting on the arXiv I don't know. Perhaps he just wanted it to be available to general readers on the internet.

5. **Bobito**
   April 1, 2015

   Text such as: "The situation looks pretty bleak, particularly since we have no control on when and where such a doomsday bubble may form! Nevertheless there is a course of action that could save some of our descendants from this catastrophe. The essential idea is simple; we must spread out as fast as possible, establishing civilizations on dfferent worlds in different parts of the universe, so that even if some of us are hit by the catastrophe, the others may survive."

   is either a joke, or the worst kind of crackpot crap. Let's not pretend that it's respectable to write things like this. It isn't. No real physicist would write this. As far as know, travel to "other worlds" is a physical impossibility, and always will be. It's all fine and good to oversimplify, but it is dishonest and irresponsible, especially in a popular essay, to claim as a solution something simply ridiculous.

   I think it has to be a joke. The joke is to see if he wins the contest submitting complete garbage.

6. **Aquiles**
   April 1, 2015

   To understand Sen's paper, use Occam's razor !

   There is a trivial interpretation: today is April 1st ….

7. **Maurice Carid**
   April 1, 2015

   Ha, ha. All you commentators were successfully
   sent into April by Peter.
   Truly funny April fools' day joke, Peter ;-).

8. **Radioactive**
   April 1, 2015

   Sen's article is in hep-th instead of pop-ph, I'm sure it will be moved. It's just some kind of popular, quasi sci-fi essay (not paper) for a general audience. Lighten up.

9. **Anonymous**
   April 1, 2015

   https://indico.cern.ch/category/6386/ is not open anymore.

10. **Peter Woit**
    April 1, 2015

    Anonymous,
    Too bad. Public news is still available here:
    http://lhc-commissioning.web.cern.ch/lhc-commissioning/news-2015/LHC-latest-news.html

11. **AcademicLurker**
    April 1, 2015

    Radioactive,

    I think that the article grates because, as our host frequently points out, hep theory is becoming subject to Poe's law. I suspect the same sort of paper would have been considered all in good fun in, say, 1995.

12. **N.**
    April 1, 2015

    On Sen: April 1st?

13. **Peter Woit**
    April 1, 2015

    Yes, the update about Sen was written very late last evening, I've just restored comments that pointed this out. Unfortunately all evidence seems to be that this isn't actually a hoax…

14. **Bernhard**
    April 1, 2015

    I had a great laugh with that article. Ashoke Sen could make some money writing sci-fi plots for TV series.

15. **Thomas Larsson**
    April 2, 2015

    Am I missing something here? The arxiv lists Sen's paper as Submitted on 27 Mar 2015. Surely that's not sufficently close to April 1st.

16. **Zathras**
    April 2, 2015

    So Peter,

    Are you Statler or Waldorf? 🙂

    http://resonaances.blogspot.com/2015/04/what-if-part-1.html

17. **David**
    April 2, 2015

    Peter,

    What do you make of this paper?

    http://arxiv.org/pdf/1504.00056v1.pdf

18. **Peter Woit**
    April 2, 2015

    David,
    I gather the claim is that in some approximation, in some class of string vacua, you can't get a positive CC 4d spacetime. The subject of "string vacua" is an infinitely complex one, I don't see that this changes the situation much. Their conclusion I guess is that many arguments about string vacua "should be viewed with caution", and on this I can't disagree with them…

19. **z**
    April 2, 2015

    It's interesting you have Langlands right after Mochizuki, who was quite fed up with Langlands program.

20. **srp**
    April 3, 2015

    The Star article on Langlands is pretty entertaining. He sounds like quite an interesting character.

21. **physics grad**
    April 5, 2015

    6.5 GeV is unlikely to be interesting… I believe you mean 6.5 TeV! 🙂

22. **Peter Woit**
    April 5, 2015

    Thanks physics grad, fixed.
    I keep making that mistake…

23. **Manjil Saikia**
    April 6, 2015

    Prof. Sen has given an update on his website about this: http://www.hri.res.in/~sen/

24. **Peter Woit**
    April 6, 2015

    Manjil Sikia,

    Thanks! It's interesting to see that Sen denies any connection to eternal inflation and the multiverse, and yet devotes the concluding section to arguing for investigation of the string theory landscape. I guess this is logically consistent, but it gives up on "statistical predictions", and, going forward, I don't see any plausible argument that you could ever use the string theory landscape to predict anything.

25. **jonnie**
    April 7, 2015

    The Sen paper seems to mirror the plot of Schild's Ladder by Greg Egan, some of the hardest of hard sci-fi.

    http://en.wikipedia.org/wiki/Schild%27s_Ladder

    I loved it, but actual physicists may be angered by it, I don't know 🙂

26. **MB**
    April 7, 2015

    jonnie,

    Actual physicist here who thinks Schild's Ladder was pretty darn good. Much better, in fact, than the vast majority of sci-fi (confession: I do like Star Trek, though for sentimental reasons more than anything). I'll grant it's certainly not to everyone's taste, but I think you'd have to be stubbornly, boringly hard-nosed to be unwilling to suspend disbelief over Egan's departures from reality as we know it (or speculate it to be). He at least goes to extraordinary lengths to ensure the internal consistency of his fictional universes.

27. **Mitchell Porter**
    April 7, 2015

    I wonder if he's serious in proposing that this scheme to delay extinction via vacuum decay, through a crash program of cosmic fecundity and settlement, offers an important incentive to study the string landscape. That is the sort of thought you see in impassioned Internet discussions by transhumanists at their most extreme, who often don't understand why our whole civilization isn't already mobilized around some Promethean agenda like "ending death". I have personally had at least one discussion with someone anxious to know if string theoretic cosmology could allow "information processing" to continue forever.

28. **Chris W.**
    April 7, 2015

    Such speculations (and obsessions) make the plot of the film *Interstellar* seem conservative by comparison, to put it politely.

29. **MB**
April 7, 2015

Mitchell Porter,

My reading was that you only need a doubling time just faster than the vacuum decay rate, so in ordinary terms you don't need anything like a "crash program of cosmic fecundity and settlement" (though I love the phrase — thanks!).

Fun speculations aside, I don't understand how anyone can really worry about these things. I mean, even if we were all very confident string theory was a complete, consistent and unique theory of physical phenomena *and* we had the whole landscape mapped out, who's to say that one of our fundamental assumptions isn't slightly wrong? Maybe nature doesn't *admit* a complete, consistent and unique theory. Maybe in a hundred billion years God comes in, says "nice experiment that one, moving on" and everything vanishes in a puff of thought. Who's to say? In the infinite future there is an infinite amount of time for things to go wrong. You have to assign literally zero prior probability to anything you want to avoid. Not good Bayesianity.

Chris W.,

Nah. String landscape/multiverse doesn't predict anything so exotic as closed timelike curves. Only random configurations of chaotic events which, on casual inspection, *look* like closed timelike curves. 😉

30. **Zimriel**
April 8, 2015

Not that I want to start a flamewar here but "travel to other worlds" isn't impossible. It's *impractical*. Those are different adjectives.

31. **Parth**
April 9, 2015

The essay posted under the name Ashoke Sen is actually by Ashoke Sen.
He clarifies his essay in his website:
http://www.hri.res.in/~sen/

32. **JG**
April 9, 2015

haha, I was amazed at that anyone could make a comparison to the "Sokal Hoax" for this innocent and fun little essay.

Come on Peter Woit – you have not too bad standards, the update about the Sen publication was well below what is expected

News of the Multiverse

April 8, 2015
Categories: Multiverse Mania

Just about ten years ago, my April 1 posting here was a fantasy about the Stanford ITP getting major funding from the Templeton Foundation, using it to fund a program on the multiverse, and renaming themselves the Stanford Templeton Research Institute for Nature, God and Science. The last part hasn't yet come true yet, but I just noticed the announcement last year of a $878K Inflation, the Multiverse, and Holography grant from Templeton to the SITP, the third part of "A three component Templeton Initiative at the Stanford Institute for Theoretical Physics."

To get some idea of the scale of this funding, note that the entire NSF budget for theoretical HEP is about $12 million (the DOE spends about $50-60 million, but that supports groups at the labs, as well as computational hardware, and is decreasing). The Templeton Foundation has an endowment of over $3 billion (growing rapidly), and pays out over $100 million in grants/year (also growing rapidly). I don't think my skills as a fantasist are good enough to imagine what this means for ten years from now in the future.

In other multiverse news, the Literary Review of Canada has published a review by David Orrell of the recent Unger/Smolin book, and an exchange of letters between him and Matthew Kleban. I wrote something about the book here, and I'm in many ways not very sympathetic to the point of view of Orrell and Unger/Smolin, especially about the role of mathematics in physics.

I'm more on Kleban's side about mathematics, but the way he paints multiverse studies as the latest scientific descendant of the mathematics-driven successes of physics of the past is highly problematic. While this is a point of view favored at Stanford and at Templeton (Kleban has a $175,000 grant from them), I don't think it's a defensible one. Kleban's arguments are

- More to the point is the string landscape, a relatively concrete structure believed to follow from the mathematics of string theory.

Here "relatively" is a weasel word (relative to what?), masking the fact that we don't at all know what the structure of the string landscape is.

- contrary to Unger and Smolin's assertions, recent work indicates that current or near-future cosmological observations – specifically, the detection of positive spatial curvature – would falsify the landscape (if it is false).

The situation with the measurement of spatial curvature is that recent Planck results give |Omega_K| less than 0.005 and the expectation is that it is zero to a much higher accuracy than that, way beyond anything measurable (this is considered one of the main arguments for inflation). This "prediction" isn't "recent". Susskind's book on the multiverse ten years ago gave this one bit of sign information as the only prediction of the multiverse (see here). Shortly thereafter some authors were arguing that you could get positive curvature from the string landscape (see here). I have no idea if they're right, but in a recent paper Kleban himself writes about this:

> Positive curvature would probably not completely end discussion about a multiverse but it would be very bad news for the eternal inflation/CDL bubble nucleation framework.

and I think Orrell has it right that

> I would be interested to see if the detection of positive spatial curvature actually falsified the theory – wouldn't it just adapt?

- Furthermore, the theory can be used to predict the signatures of cosmic bubble collisions: violent events where two previously separate "universes" collide.

There's no evidence at all for such "signatures", and I don't think there's any plausible argument for why they'll appear in new data given that they haven't been seen yet (I wrote here about Kleban's Columbia talk about this). Final data from Planck on polarization are expected soon, but this is so implausible that I'm not sure Planck will even bother to look.

The problem with this kind of "testable prediction" is that it's much like my claiming that my theory that the universe is controlled by a giant turtle is testable and predictive, since if you saw a big picture of a turtle in the CMB, that would be strong evidence for my theory. There was a reason Popper went on about falsifiability…

- the standard model of particle physics combined with Einstein's theory of general relativity – two of the most well-established theories in physics – predict a large landscape quite similar to that of string theory.

This one brings back the "string wars" era, since I haven't heard anyone trying to use it (based on this) since 2007. Whenever people make a "string theory is just like the standard model" argument I'm never sure what to respond. How do you argue with someone trying to claim that the most successful physical theory ever, by far, is "quite similar" to a theory that has had zero success? It's kind of like trying to argue with someone who wants to tell you that black is white, because they're both kinds of grey. Surely they're not serious?

In this case, sure, if you put the standard model on a complicated space-time background, added lots of fluxes, etc. to the background, maybe you could turn it into as useless a theory as string theory. This doesn't mean it's "quite similar".

Update: Just noticed another recent essay about the multiverse, Marcelo Gleiser's examination of whether Fairies live in the multiverse.

Comments

1. **JG**
   April 8, 2015

   I think you should give the Templeton Foundation a break, they wouldn't need to fund bright young scientists if democracy worked better – it doesn't, the majority of the population don't care that bright young scientists have to go into IT, Finance etc rather than pursue a career in academic research.

   These Templeton funded students/researchers are not stupid people, they must learn a lot of advanced physics to come up to expert level on the multiverse question – and I am sure they are allowed to conclude it is not a workable idea, and I am sure the Templeton Foundation would be delighted if their funding was responsible for rigorous arguments explaining why the multiverse is a bad idea, just as much as they would be delighted with strong arguments supporting the idea.

   Are you worried that these people aren't doing "proper" physics?

2. **Peter Woit**
   April 8, 2015

JG,

If you pay closer attention, you'll notice that I'm not criticizing the Templeton Foundation, I'm just reporting what is going on (by the way, I've attended events sponsored by them, and recently wrote an essay for an essay competition they fund). They're free to do what they want with their money, but I'm also free to comment (if I had $3 billion I'd stop blogging and instead spend my time giving grants to people with my point of view). There are definitely problems with DOE/NSF funding and how their panels work, as well as the problem of Templeton's fondness for mixing religion and science. No source of money is problem-free. In some cases (e.g. the Simons Foundation) I actually agree more with their choices of how to spend money than I do with the DOE/NSF peer-reviewed choices.

But I think people need to be aware of the changes going on. The shift from public funding to funding by wealthy donors is very real and there should be awareness that this is happening, who the donors are, and what their agenda is. When you see lots of activity going on in the field related to the multiverse, you should keep in mind that there may be more to the story of why it is happening than "physicists are excited about the multiverse" .

3. **JG**
April 8, 2015

Ah, my bad understanding of the situation.

Templeton are giving the money to established academics, NOT young students and researchers.

Ok, yes you should call them out, and continue to do it, it is a very valuable service.

Sorry I must have been thinking about some other private funding agencies who actually are doing something useful.

4. **nicola**
April 9, 2015

Fairies surely live in the multiverse although they are most likely higher dimensional – my guess is M5 branes.

5. **Jim Baggott**
April 9, 2015

Peter,
I have a favour to ask. I think it's time that opponents of what I call 'fairy-tale' physics dropped arguments based on Popper's criterion of falsifiability. Popper published these ideas in his book *The Logic of Scientific Discovery* in 1934, and I think it's fair to say that the literature on the philosophy of science has moved on since then. Few contemporary philosophers have many good words to say about it.

I've just posted an article on this theme on my new website (!) – see http://www.jimbaggott.com/articles/against-falsifiability/

6. **Steve Bergman**
April 9, 2015

I thought the Planck polarization results were finally released a couple of months ago, on Feb 5. Am I in error?

7. **Lee Smolin**
April 9, 2015

Dear Peter,

I am glad to see the issues raised by Mangabeira Unger and myself getting discussed. But re Kleban's remarks, "contrary to Unger and Smolin's assertions…", let me note that the prediction of eternal inflation of slightly negative spatial curvature is discussed in our book on page 460. I explain why this is a genuine prediction of eternal inflation but conclude, "However, this will be difficult to confirm or falsify with near-future observations because it will require a great deal of precision to distinguish this from vanishing curvature."

Thanks,

Lee

8. **Bernhard**
April 9, 2015

Is there any comprehensive document where all these alleged multiverse "predictions" can be found? These bubble collisions are what keeps popping up, but there seems to be little hope there. Anyway, not that I believe the proponents have anything compelling, but it would be educational to see a list.

9. **Peter Woit**
April 9, 2015

Steve Bergman,
Some polarization results were released, especially ones related to the BICEP2 claims. But if you look at the public Planck results page, you'll see that of the 28 2105 results papers listed, 8 are still "in preparation". The page says "Additional 2015 products will be released near the end of March 2015", yet another deadline they haven't made, don't know what the latest is. As far as I know there's nothing in the 20 released papers about even looking for bubble collision signatures, I have no ideas if that will be somewhere in the 8 still to come.

Lee,
Thanks for pointing that out. I think you're being too kind though. It seems to me that the generic prediction of inflationary scenarios, including any bubble universe ones, is that there is no conceivably measurable spatial curvature, and thus that spatial curvature observations will never tell you anything about bubble universes.

Bernhard,
I think Kleban's list is pretty comprehensive, which is why I bothered to write about it. As far as I know, no one has put together a list of all the claims made about "bubble collisions". Claims about the possibility of seeing such things get lots of play in the media, with never any followup when someone looks at the data and sees nothing.

10. **David Orrell**
April 9, 2015

Interesting that multiverse theory is being funded by money from investment firms. Not sure if they will turn out to be as good at picking scientific winners as they are at picking stocks. I have posted an article on this:
https://futureofeverything.wordpress.com/2015/04/09/funding-other-universes/

11. **bhr**
April 9, 2015

Correct me if I'm wrong but the $878K is spread out over three years according to the link to the project description. This might make the comparison with the NSF/DOE numbers a bit less scary.

That comes out to just under $300K/year. I'm a bit confused about where it's going. According to the link, this will fund one postdoc, 2-3 peoples' summer salaries, and a workshop. I would have guessed that only adds up to about half the total award.

12. **Peter Woit**
April 9, 2015

bhr,
I think the numbers roughly add up, you may be underestimating what people get paid. I'd guess very roughly $100 K would go to summer salaries, $100 K for postdoc salary + benefits +overhead, $100 K for visitors + workshops + public lecture.

Note that this is supposedly only one of three Templeton grants to the SITP. One of the others is this
http://www.templeton.org/what-we-fund/grants/quantum-gravity-frontiers
which is for $600K, so $200K this year. I don't know what the third grant is, but
there's at least $500K this year. For comparison, the same people also have an NSF grant
http://www.nsf.gov/awardsearch/showAward?AWD_ID=1316699
$1 million for ~~3 years~~ (*correction, I'm told this is for two years*), which presumably covers another month of summer salary, more postdocs and grad students.

It seems likely that they're now taking in ~~significantly more~~ *about the same* from Templeton than from the NSF.

It also seems to me that some of the newer Templeton grants like this one are structured to be very much like NSF grants, with the standard summer salary/postdoc package (although it looks like they're not now supporting students). The Stanford Templeton postdoc is a conventional condensed matter theory postdoc, indistinguishable from NSF ones, see the ad here
http://web.stanford.edu/group/sitp/CondensedMatter/postdocpositions.html

Templeton has always favored giving grants to the most prestigious universities around. It may be that in the future the most prominent theory research groups will be getting their funding more from Templeton than from the NSF/DOE.

13. **Missleaded layman**
April 9, 2015

Is a a big picture of a turtle in the CMB a requirement for the giant turtle controlled universe theory, I mean, if no such picture is found in the CMB data, would that rule the theory out?

14. **Peter Woit**
April 9, 2015

Missleaded layman,

Certainly not! My giant turtle theory works equally well with a turtle who likes his image spread over the sky, and one who doesn't.
But the point is that I can answer the criticism that my theory is empty and makes no distinctive predictions. There is a distinctive prediction: "if the image of a giant animal shows up in the CMB, it will be the image of a turtle". And I think it's undeniable that if and when a convincing turtle image shows up in the CMB, that will give good reason for taking the turtle theory seriously.

15. **Missleaded layman**
April 9, 2015

Thank you. I started to search the web for multiverse related topics after reading an unsatisfactory book about multiverse theory targeted at laymen earlier this year, and eventually came across your blog. And for sure I'll be back seeking your point of view frequently!

16. **vmarko**
April 9, 2015

"it's undeniable that if and when a convincing turtle image shows up in the CMB, that will give good reason for taking the turtle theory seriously."

I think we should take the turtle theory seriously already now. Namely, in addition to the prediction of turtle image in the CMB, there are theoretical reasons as well — it naturally explains why there are no unicorns in the observable universe, and why the world is flat. Not to mention the explanation-through-randomness for the values of the fundamental coupling constants, which is equally good as in inflation+antropic principle+string theory.

17. **emile**
April 9, 2015

Ironically, in the multiverse, there must be a few universes out there with a picture of a Giant Turtle in their CMB.

18. **paddy**
April 9, 2015

…and one in which it is turtles all the way down.

19. **N**
April 9, 2015

I like the Turtle Approach.
As many turtles, so many universes.
And everybody happy, even the Templetones.

Amen.

20. **JG**
April 9, 2015

For people waiting for Planck's final say on bubble universe collisions and turtles I think the paper to watch for is

"Planck 2015 results. XVI. Isotropy and statistics of the CMB"

Still "In prep" as of this posting

21. **DrDave**
April 9, 2015

Nice acronym.

22. **former mathematician**
April 9, 2015

From Rebecca Goldstein's "Plato at the Googleplex": "It's not turtles all the way down, but rather reasons, logoi, all the way down."

23. **ateixeira**
April 10, 2015

@Peter why do you say that the standard model is "the most successful physical theory ever". In terms of sucess I think that GR is at least as good as the SM. Or is it because the range of application of the standard model (either be it theoretical or practical) is far greater than the range of application of GR?

Now that I think of it I think it is safe to say that Condensed Matter Physics may be even more successful than the SM and GR together (again in a theoretical and practical level)

More in line with the main focus of your post I'd say that this situation of research funding being more and more dependent from private donors is kind a return to the old days of gentleman science where patrons were responsible for providing the means of subsistence of influential scientists. This of course has two problems in my view. In the first place it may happen, as you already alluded, that these modern day patrons pay for the kind of research they want to see and lastly young researchers can be drawn to this highly visible and profitable fields of research hindering progress in other worthy areas (like condensed matter for instance).

24. **Low Math, Meekly Interacting**
April 10, 2015

I'm really encouraged to hear that someone, even in the guise of reductio ad absurdum, is putting the Fairy Question out there in the public discourse. My daughter is captivated by and obsessed with all things Tinkerbell. I don't know how to tell her Tink isn't real. I dread that conversation more than the one about the Easter Bunny, or even Santa. Now it occurs to me I don't have to disabuse her of anything. Thank you, Multiverse! It's also comforting to realize there's a copy of me somewhere that's totally pumped to have The Talk about sex and menstruation. The gifts you bestow are truly infinite!

25. **Shantanu**
April 11, 2015

atexeira, Unfortunately even these private foundations such as templeton (or Fox)
also mainly fund fad topics (and probably topics already funded by NSF/DOE etc)
There is still a fundamental problem that people who work on non-mainstream ideas
or independent research find it very hard to get funding or jobs. (Again I don't want
to go into which topics are these or examples of such people in this post.)

26. **Anthony Reynolds**
April 11, 2015

In Orrel's comments, he states at least twice that "if we take time seriously", then follows some conclusion. I don't know what it means to take time seriously, or to take it lightly, for that matter. To be taken seriously is not a possible attribute of time. I would say that statement is "not even wrong." I haven't read Unger and Smolin's book, but some of Smolin's writing that I have read (not all of it) suffers from the same problem. At least his more wild speculations.

27. **Anthony Reynolds**
April 12, 2015

Jim Baggot,

Regarding your wish to get rid of "falsifiability." In Chapter 1 of your book "Farewell to Reality," you put forward the "Testability Principle" as part of the demarcation problem. But it seems that this principle includes both 'verifiability' and 'falsifiability' at some level. Neither of these works all the time, but together, with some notions of how to interpret the tests gets you what you want. Indeed, you state

> While this is not quite falsifiability, it certainly has some of its flavor. So, while perhaps Popper's falsifiability notion should not be our sole criteria, it certainly should be an element of the solution to demarcation.

28. **Anthony Reynolds**
April 12, 2015

Sorry, my cut and paste failed me. It should be:

Indeed you state

> A theory that continually fails repeated tests is a failed theory.

While this is not quite falsifiability, it certainly has some of its flavor. So, while perhaps Popper's falsifiability notion should not be our sole criteria, it certainly should be an element of the solution to demarcation.

29. **Jim Baggott**

April 14, 2015

Anthony Reynolds,
Peter's blog is probably not the best place to have a discussion about this. Have a look at the article I posted on my website (the link is in my earlier post). I have it on authority from a highly respected academic philosopher that I am, at heart, a Popperian. That's OK by me, but it doesn't make my point any less valid. We need to accept that the business of accepting or rejecting theories is all rather fuzzy, and doesn't lend itself to easy, black-or-white demarcation. To continue to construct arguments based on falsifiability alone simply plays into the hands of the fairy-tale physicists, I think.

An approach to theory development based on mathematical (rather than physical) logic and intuition is perfectly valid and has been enormously successful in the past. Obvious examples are general relativity, Dirac's relativistic wave equation for the electron, and even the Higgs mechanism. But, whether by accident or design, all these examples predicted or at least hinted at effects that might be discernible in empirical reality: the perihelion of Mercury and the bending of starlight, the positron, the masses of the W/Z bosons and the existence of the Higgs boson.

The criticism of 'fairy-tale' physics is that in many cases there are simply no empirical references; no tests. And where 'predictions' are made, these are so loose, adjustable or negotiable that taking the trouble to establish the empirical facts takes us no further forward. Some philosophers use the term 'progressive' to describe a theory that allows us to make scientific progress in this sense and, it seems to me, 'testability' is an adequate criterion to help us to understand what it means for a theory to be progressive.

For sure, a theory that is 'testable' is also in principle 'falsifiable', but these two terms are not synonymous. Is Newtonian mechanics false? Arguably no, it's not – it still holds good within its domain of applicability, which is why we still teach it in schools and colleges. Falsifying a theory can be really difficult – philosophers reference something called the Duhem-Quine thesis: when a prediction is not upheld by empirical data this could mean the theory is false or it could mean that any one of a number of approximations or 'auxiliary hypotheses' required to make the prediction are invalid, but the data don't tell us which. A test is a test.

30. **David Orrell**
April 14, 2015

Anthony,
You say that "To be taken seriously is not a possible attribute of time" so to assert that it can be taken seriously or not, as Unger and Smolin do in their book, and I do in the review, makes no sense.
Being taken seriously is not a fundamental attribute of time, any more than it is a fundamental attribute of the multiverse. It refers to the attitudes of people. One of the main arguments in Unger and Smolin's book is that some scientific approaches take time as a given, while others invest more energy in exploring its attributes and see it as playing a more active role – in the authors' words, they take it more seriously. It would be a shame to write off their approach based on what looks like a case of semantic confusion.

31. **gadfly**
April 14, 2015

"I'm in many ways not very sympathetic to the point of view of Orrell and Unger/Smolin, especially about the role of mathematics in physics. "

I think spending time with complex systems (plasmas, proteins, shockwaves in materials, glasses, ecological and economic systems) would rapidly disabuse you of the notion that mathematics is anything more than the scientific equivalent of democracy: that is to say, a mediocre and tragically limited way of doing things, but vastly superior to any of its competitors. There was a very good article in IEEE about the reasonable ineffectiveness of mathematics recently which establishes this point pretty well.

32. **David Orrell**
April 15, 2015

Further to gadfly's comment, apparently there is a NOVA show on this evening about this topic of the effectiveness of math: http://www.pbs.org/wgbh/nova/physics/great-math-mystery.html

33. **gadfly**
April 15, 2015

That NOVA looks amazing, thanks for sharing!

34. **Lee Smolin**
April 16, 2015

Dear Anthony,

"Taking time seriously" is of course just a slogan , which is shorthand for "investigate and test scientific hypothesis which incorporate a notion of physical time which has one or more of the following attributes":

-Laws are not timeless, but evolve, according to some specific dynamical mechanism, which has testible consequences.

-The arrows of time have their origin in a fundamental irreversibility or time assymmetry, of the fundamental theory, out of which the standard, time irreversible laws are emergent.

-The fundamental laws refer to an objective distinction between the past, present and future, not available within a block universe interpretation of GR.

-At the fundamental or quantum gravity level, the refoliation invariance of GR is replaced by dependence on a global slicing as in shape dynamics.

The conceptual and philosophical analyses of my recent book with Mangabeira Unger have value to the extent that they suggests new hypotheses like these which may be developed and tested-because they make falsifiable (or if you prefer, testable) predictions.

Here are some papers which propose and develop such ideas:

–Marina Cortes, Henriques Gomes and Lee Smolin, Time asymmetric extensions of general relativity, gr-qc. arXiv:1503.06085.

–Marina Cortês, Lee Smolin, Quantum Energetic Causal Sets, arXiv:1308.2206, Physical Review D, volume 90, eid 044035.

–Marina Cortês, Lee Smolin, The Universe as a Process of Unique Events, arXiv:1307.6167, Phys. Rev. D 90, 084007 (2014).

–Marina Cortês, Lee Smolin, Spin foam models as energetic causal sets. arXiv:1407.0032

–Henrique Gomes, Sean Gryb, Tim Koslowski, Flavio Mercati, Lee Smolin, Why gravity codes the renormalization of conformal field theories, arXiv:1305.6315 [hep-th], published as “A Shape Dynamical Approach to Holographic Renormalization, “European Physical Journal C, January 2015, 75:3,

–Lee Smolin, Precedence and freedom in quantum physics, arXiv:1205.3707. International Journal of Quantum Foundations 1 (2015) 44-56.

–Lee Smolin, A perspective on the landscape problem, Invited contribution for a special issue of Foundations of Physics titled: Forty Years Of String Theory: Reflecting On the Foundations, DOI: 10.1007/s10701-012-9652-x arXiv:1202.3373

–Lee Smolin, Unification of the state with the dynamical law, arXiv:1201.2632. Foundations of Physics: Volume 45, Issue 1 (2015), Page 1-10

–Lee Smolin, A real ensemble interpretation of quantum mechanics, Foundations of Physics, 2012, DOI 10.1007/s10701-012-9666-4 arXiv:1104.2822

–Mohammad H. Ansari and Lee Smolin, Self-organized criticality in quantum gravity, hep-th/0412307.

–Julian Barbour and Lee Smolin, Variety, complexity and cosmology, hep-th/9203041.

–Lee Smolin, Did the universe evolve? Class. and Quantum Gravity 9 (1992) 173-191.

Thanks,

Lee

LHC News

April 9, 2015
Categories: Uncategorized

The LHC has just ramped up for the first time to 6.5 TeV, and has a probe beam circulating in one direction, the highest energy protons humans have ever accelerated. You can follow what's happening here.

The BBC has gotten very excited about this whole LHC thing.

Update: Now it's two beams at 6.5 TeV. They just need to be careful to avoid beam collisions until the press event is organized…

Update: Maybe they weren't careful enough. The Monday morning beam commissioning reports "Possibly first collisions". No confirmation of this from the experiments, or officially from CERN.

Comments

1. **Gus Bici**
   April 9, 2015

   6.5 is large. 13 is gross. LHC to be renamed GHC?

2. **Peter Woit**
   April 9, 2015

   Gus,
   Luckily, it's only 6.5/beam, 13 for two of them.

3. **Los Ranchos**
   April 9, 2015

   I think its wrong not to be excited about the LHC, whatever motivations or theories got it advertised, sold and done.

4. **Bernhard**
   April 10, 2015

   Los Ranchos,

   Sure, but not that much. Kids are watching, you know…

5. **Low Math, Meekly Interacting**
   April 10, 2015

   Think about it: there's a pocket universe in which it makes perfect sense.

6. **EFT**
   April 10, 2015

   Could this level of energy provide a definitive test for the simplest MSSM model?
   Thanks

7. **Peter Woit**
   April 10, 2015

   EFT,
   Depends what you mean by simplest. In the simplest pictures of what SUSY models are supposed to do, evidence should have turned up at LEP and the Tevatron.

   I've already written a huge amount about SUSY and the LHC, and that will be the big story during the coming year so will be discussed extensively here. Right now though, no news, so it's off-topic for this posting.

8. **EDBM**
   April 11, 2015

   A rare caption of an excitation of the BBC field! I suppose it has long since decayed to a ground state…

9. **srp**
   April 11, 2015

   The real-time LHC data page is oddly compelling. They have a nice documentation page to help you understand each element, too. On three visits to the page at widely separated times, the same beam always had the higher intensity of the two–wonder if that's a design spec, or just the way it turned out due to normal manufacturing or operating variance.

10. **Justin**
    April 12, 2015

    Woit,

    I would like to clarify exactly your view on what one might expect from the LHC. If I'm not mistaken, you do not expect to see any fundamental deviation from the standard model because the standard model may turn out to be a basic fundamental description, one that we need to understand better. Is that more or less correct?

11. **Thomas Larsson**
April 13, 2015

Justin,
The most striking results from LHC 1 are clearly that the Higgs mass is at the brink of instability and that no BSM physics has shown up. As emphasized e.g. in arxiv:1307.3536 or arxiv:1407.2122, the near-criticality of the Higgs mass means there is really no need for any new physics at all until close to the Planck scale. This may even be thought of as a principle – the principle of living dangerously.

12. **Michael Shain**
April 13, 2015

The authors of arxiv:1307.3536 (Investigating the near-criticality of the Higgs boson)
in their conclusion seem to favour the multiverse :
"an explanation of near-criticality almost necessarily requires the existence of an underlying statistical system. This drives us towards the multiverse as the most convincing framework in which one can address the issue."

So is the discovery of the mass of the Higgs proof of the existence of the Multiverse?

13. **Thomas Larsson**
April 13, 2015

Personally I don't believe in the multiverse, but would rather expect that near-criticality is a reflection of a symmetry principle. This is in analogy with the situation in statistical physics, where critical exponents satisfy certain thermodynamic inequalities as equalities, due to an underlying scale symmetry.

Be that as it is. The striking experimental facts are that all kinds of BSM physics have been disfavored by the LHC, and that the Higgs is balancing at the edge of instability. It must mean something.

14. **Peter Woit**
April 13, 2015

All,
Please, there are too many postings on this blog about the multiverse, no need to infect the ones that aren't with the topic.

Justin,
I expect that by far the most likely result from the LHC will be continued confirmation of the Standard Model (and I suspect that's also most people's expectation). Personally I think the most likely place to look for deviations from the Standard Model is the Higgs sector, and the LHC has a long career ahead of it studying the Higgs. It would be wonderful if the LHC sees something that violates the Standard Model, we desperately need a hint from experiment about how to do better. I just don't know of any proposed new physics to be found at the LHC that has any compelling theoretical or experimental evidence to support it.

15. **srp**
April 16, 2015

"Beam lost due to losses in the usual position…" 4/17 03:41:29

Is that normal ramp-down or a repeated teething problem? It says there was a "protection dump" and lists the data as part of a "post mortem" which sounds less than propitious. I'm sure they'll get the bugs worked out but it must get nerve-wracking over there.

16. **KenW**
May 4, 2015

Your reply to Justin is disheartening. If a long career exploring the Higgs is what awaits the LHC, I fear that there will never be a more capable collider. People – governments – want significant new physics. Of course, results are results. But the high costs associated with these machines make it difficult to go further by this means, at least without spectacular news. Are there truly no lower cost options?

17. **Peter Woit**
May 4, 2015

KenW,

It was always clear that one problem with overselling the LHC, as a machine that would discover extra dimensions, black holes, supersymmetry, etc., was that there would be a price to be paid in terms of credibility. If the LHC turns up nothing beyond the SM, that is going to make the case for a higher energy machine more difficult to make. If the LHC turns up unusual Higgs behavior, and the case for a higher energy machine to study it is made difficult because the public and decision makers think "but what about those extra dimensions, black holes, etc?" that would be a shame.

My impression is that current studies of a larger machine than the LHC are still at a preliminary stage, with nobody talking about realistic cost estimates, so it's unclear what the financial feasibility of these ideas is. If a larger machine can mostly be financed at current CERN budget levels, it's a realistic possibility. If much higher budget levels are necessary, that seems unlikely. While the US, Japan or China could do such a project, there's zero probability in the US, and I have no idea about Japan or China.

I'm surprised there hasn't been more discussion of the HE-LHC idea (reusing the LHC tunnel for a machine with twice the energy), since I would have thought the cost of that would be much more likely to be affordable than the more ambitious VHE-LHC ideas which would require a new, much larger, tunnel and infrastructure.

I should make it clear that personally I think that, if technologically realistic, any of these higher energy accelerator ideas are worth the money and I hope at least one will be pursued. These are expensive machines, but affordable on the scale of the resources available to the countries involved, and more worthwhile than a lot of other things that are being funded.

The Lady Gaga of French Mathematicians Comes Stateside, and Other News

April 15, 2015
Categories: Uncategorized

Cédric Villani is in town today, giving a talk at the French consulate. He'll discuss his book, recently translated into English (I wrote a bit about it here). Yesterday, despite the lack of suitable bread and cheese, he was in Princeton, where he gave a public lecture at the IAS. The New Yorker has a story about him by Thomas Lin, entitled The Lady Gaga of French Mathematicians Comes Stateside.

If you're not listening to Villani tonight, you could be watching a PBS Nova program on mathematics, The Great Math Mystery. Among the mathematicians interviewed will be my colleague Dusa McDuff. As for the question on the PBS site:

> Is math a human invention or the discovery of the language of the universe?

the answer is the latter.

What some mathematicians might consider the "Great Math Mystery" is whether Mochizuki really has a proof of the abc conjecture. There finally will be the topic of a workshop involving experts in the field, to be held this December in Oxford. Still no paper from Go Yamashita about this, but here you can find some photographs of the boards from his talks in Kyoto last month. Mochizuki himself has a new paper, inspired by conversations with Fesenko.

Also in New York this week, Bjorn Poonen will be speaking on Thursday. His topic is a heuristic argument that there is a finite bound on the rank of elliptic curves. For notes from a talk of his about this last year, see here.

Update: The Villani IAS talk is available here.

Update: At David Mumford's blog he has a long and very interesting posting about the state of mathematical research publishing.

Update: One more piece of math news. Dan Rockmore has set up a public version of his Concinnitas Project, which lets people post, with explanation, a picture of their choice of a "most beautiful mathematical expression". See here for details.

Comments

1. **Interested**
   April 15, 2015

   Will the Villani IAS lecture be posted on their website afterwards?

2. **PointedRemark**
   April 15, 2015

   Fesenko's impressive survey of IUT,
   https://www.maths.nottingham.ac.uk/personal/ibf/notesoniut.pdf
   cited in the last line of the "here" link above, ends with this pointed and invaluable remark:

   An opinion of R. Langlands on current trends about supporting long-term fundamental research work can be heard during the 52nd minute of his video lecture [20]. Some roots of the decline of support to long-term fundamental work, such as the shortsighted race to higher number of publications and higher citation index, which often results in pressure to produce short-term work that consists essentially of minor improvements to known results, originate from causes external to the mathematical community. To do well in their academic career, young researchers are very often pushed to go along this path which typically implies a very narrow specialisation. The latter leads to the emphasise on technical perfection as opposite to innovation and on presentation rather than the substance of work. Following this path eventually makes it more difficult to think broader, to learn new areas, to develop in more directions. Lack of inventiveness, more widely spread imitation, fear to stand alone in scientific endeavour, fear to look too far away are associated issues. Some roots, such as the unnecessarily strong emphasis on concrete applications, originate from within the mathematical community. Changes are needed.

   There is an issue about attitudes of number theorists towards the study of IUT and their unusually sluggish response. Reasons for this are related to the topics discussed in the third paragraph of 3.3 and in the previous paragraph. It seems that the number theory community is suffering from the problems listed there even more than other mathematical communities.

3. **Interested**
   April 15, 2015

   Never mind, Villani's talk has already been posted:
   https://video.ias.edu/villani-publiclecture-2015

4. **Peter Woit**
   April 15, 2015

   PointedRemark,
   I think Langland's point is excellent and quite important, but I'm not convinced it's relevant to the IUT/Mochizuki story. Here the question is not why people aren't being inventive and developing their own long-term research projects, but why they're not signing on to Mochizuki's, which is different. This has a very high profile in the math research community, everyone in the field knows about it and has looked at it. My interpretation of what has happened is that a lot of experts have just decided that this claimed proof is not yet in a state where it can be evaluated by the usual methods and the methods used are not obviously convincing enough to be worth the time needed to try and figure out how to use them. Given this, it's quite reasonable to decide to not spend one's time on IUT, but to instead work on ones own ideas and wait until either Mochizuki/Yamashita/someone else puts out a comprehensible version of the arguments. Mochizuki's own reports on what is being done to check the proof aren't inspiring confidence.

   I think sooner or later though, enough people will put enough time into this for it to become clear what the value of the ideas is. Some day we'll know whether or not there's really a proof of abc and powerful new methods there, but in the meantime I don't think people can be criticized for not finding this convincing and not wanting to abandon work on their own ideas in favor of working on Mochizuki's.

5. **vmarko**
   April 15, 2015

   Hi Peter,

   "Is math a human invention or the discovery of the language of the universe? [T]he answer is the latter."

   Risking that this might be off-topic, I nevertheless need to ask something about this. It seems that you have a very strong opinion, and I am curious why? For example, how do you rate, say, the axiom of choice — is it a part of the language of the universe, or not? Note that if you say "yes" I'll ask you further about the Banach-Tarski paradox (which seems anything but to hold in our universe), while if you say "no" I'll ask you about the lack of power of the ZF without C, and the fact that most mathematicians today prefer ZFC over ZF, despite it not being the language of the universe (by assumption).

   Or as another example, take the distributive law (of propositional logic) and note that it fails in the context of quantum mechanics (as discussed, say, in the Wikipedia article on quantum logic). If math is the discovered language of the universe, does distributive law hold or not? If not, how many theorems in standard math would go down the drain because their proofs rely on it?

   There is also the question regarding Brouwer's intuitionistic logic versus Hilbert's formalist logic (discussed here) — which one is the correct language of the universe? I as suspicious that it can't be both, so…

   It seems to me that these examples suggest that math (being mostly built on top of some particular logic and some paricular set theory) is a language that sometimes nicely describes the real world, and sometimes less so, depending on the adopted axioms. This makes it more a human invention than a discovery of some property of nature, since otherwise issues like Brouwer-Hilbert controversy could be resolved experimentally. I am really curious to hear how do you deal with these issues, from your POV.

   Best, 🙂
   Marko

6. **Peter Woit**
   April 15, 2015

   Marko,
   Sorry, this really is off-topic. In my essay I tried to explain my point of view. The question is a completely ill-defined one, and people can use it for inspiration to discuss all sorts of topics. Questions about the fundamentals of mathematics (or physics) in terms of logic happen to be ones that have always left me cold, so I've neither the knowledge nor the interest to carry on my half of an intelligible debate about the issues you raise.

7. **Douglas J. Keenan**
   April 16, 2015

   Last month, *The Observer* (which is the Sunday version of *The Guardian*) had a 2000-word article "Cédric Villani: 'Mathematics is about progress and adventure and emotion' ". The article focuses on Villani's book *Birth of a Theorem*.

8. **gadfly**
   April 16, 2015

   Speaking of the research climate for fundamental mathematics, what is the research climate like for pure mathematics in general? Obviously they've never needed huge grants (in general anyway) but I've always wondered how hard it is to become a research professor in a subject* which is so controversial.

   *I do realize this probably varies from subtopic to subtopic within pure mathematics.

9. **Peter Woit**
   April 16, 2015

   gadfly,
   I don't think pure math is controversial, quite the opposite. Few people are interested or care one way or the other about it.

   Academic math departments have a mix of pure and applied, and the pure component is in some sense mostly funded by the teaching mission: there are lots of students out there who need to learn calculus, etc. This is what most pure mathematicians are being mostly funded to do. The number of purely research positions, e.g. at research institutes, is a small part of the overall number of positions.

   Like most of academia and the rest of society, there's been a trend to a star system where some people do very well, most people not so much. So, people with permanent positions at major research universities are doing quite well and have good environments for their research, but such positions are hard to get (and, like Langlands one might worry this makes people risk-averse). Not so hard to get some kind of position, but often these are poorly paid adjunct positions, in which carrying on research can be quite difficult.

10. **Jeff M**
    April 16, 2015

    Peter and gadfly,

    As an academic mathematician I'll put my two cents in. Peter has it basically right. Math (pure or applied) is significantly easier than physics, in that lots of students need calculus, and not many need basic physics. At my university, we have 32 full time mathematicians (this includes math ed and statistics, if you take just math folks it's 16). There are 5 physics professors. That said, getting an academic position in math is hard. When we have an opening, we'll get 3 or 4 hundred applicants, most over qualified. I'm not at a research one school, you're expected to do research but you teach 3 classes a semester. Our last hire has a Ph.D. from Cornell and a research postdoc at Rochester, and he was happy to get the job. Most of our adjuncts are honestly not Ph.D. mathematicians, that end of the job market actually gets covered in large part by jobs at community colleges and such, brutal, but at least with job security and vaguely reasonable pay. Not that there aren't some Ph.D. adjuncts in math, just nothing as bad as physics, or art. And math, like physics, has an out, you can go work on wall street and make 10 times what I do.

11. **EFT**
    April 17, 2015

    I must be missing something, but I would very much like to know why Cédric Villani is called "the lady gaga of french mathematicians".
    Thanks.

12. **Peter Woit**

April 17, 2015

EFT,
Probably like most people, the little I know about Lady Gaga is that she is a celebrity who favors flamboyant dress. I'd guess that's the analogy in someone's mind that the article's author is referring to.

13. **Zimriel**
April 17, 2015

"a heuristic argument that there is a finite bound on the rank of elliptic curves"

Oh my. Is he gunning for the Birch and Swinnerton-Dyer Conjecture?

14. **Peter Woit**
April 17, 2015

Zimriel,
I don't think Poonen's heuristic argument really addresses Birch Swinnerton-Dyer. Even if you prove no elliptic curves of arbitrarily high rank, you still have to prove BSD for curves up to some finite rank, and that's completely open.

15. **David Roberts**
April 17, 2015

@Zimriel,

there are still infinitely many curves of rank greater than, say 18, (and conjecturally, only finitely many above 22 or so) so there's no hope of just enumerating the rank greater than 1 curves, and using Bhargava-style results on density (100% curves blah blah) to get BS-D.

16. **Art Brown**
April 19, 2015

Columbia was certainly well-represented on the Nova program (I counted 3 reps), even if Dr. Tegmark got the bulk of the airtime. Does someone there know somebody at PBS?

Flashy graphics and hip engineers didn't rescue this program from boredom, imho. One bet they missed: instead of rhapsodizing about the universality of gravity, explain that most folks are currently doubling down on the theory, invoking unobserved dark matter to explain discrepant galactic motions, in direct analogy to the Uranus orbit discrepancy leading to the discovery of Neptune. (Rather fewer are betting that the correct analogy is to the Mercury orbit discrepancy.)

17. **Obs**
April 19, 2015

EFT, the New Yorker article just says "Villani has been called the Lady Gaga of French mathematicians" which it probably got from one of the many earlier articles that says the same thing. However, a bit of Googling indicates that it comes from Villani himself in an interview with Télérama in 2011, where he comments on the attention in French media after he won the Fields metal; a lot of focus on his clothing style, little on what he actually did or on fellow French winner Ngô Bảo Châu: "Je suis un peu la Lady Gaga des mathématiques."

18. **EFT**
April 20, 2015

Peter & Obs,
Thank you very much for the clarifications.

19. **gadfly**
April 20, 2015

I finally watched the nova program. The Tegmark section caused me to laugh out loud. The point where he implied that nature was contained in its entirety in the standard model and that this somehow furnished his argument that math and nature are one and the same reconfirmed my view of him as a nigh unparalleled munchkin.

It is obvious that the standard model doesn't actually describe the vast majority of reality… right?

20. **Peter Woit**
April 20, 2015

gadfly,
I haven't watched the Nova program yet (it's on the DVR…) but while I disagree with Tegmark about a lot of things, it sounds like this isn't one of them. The Standard Model + GR is an amazingly successful and comprehensive fundamental theory, and the connections between these theories and mathematics are very deep.

There is a big difference though between being a successful fundamental theory and "describing reality". A fundamental theory is just not relevant for understanding most of science, not to mention things besides science.

21. **gadfly**
April 20, 2015

Peter,

Intriguing. The SM+GR does indeed have a far more mathematical character than say, the statistical mechanics of classical liquids. But I have always heard that GR and QFT contain a relatively small amount of pure mathematics; a dash of continuous group theory in the latter and a bit more than a dash of differential geometry in the former. Is this true?

What do you make of the alternative viewpoint that more exotic mathematics is a result of poor intuition? We have a poor intuition of subatomic, high energy systems, and a poor intuition of black holes and cosmological scales, so we need a more subtle and complex language to describe them; in some regards, it is a form of packaging ignorance. You can find sophisticated mathematics elsewhere in physics where there is a breakdown of intuition; people have tried to use topological techniques to study the configuration spaces of biomolecular systems, for instance.

22. **Chris W.**
April 20, 2015

gadfly,
Speaking of reliance on intuition versus mathematical formalization, see [this fortuitous post] from Steve Hsu.

One can argue that the breakdown of intuition is inevitable; science can't help but advance into realms where intuition is not helpful, and worse yet, can never really be developed. Extreme combinatorial complexity might be one such realm. In such realms the human mind needs powerful aids.

23. **Peter Woit**
April 21, 2015

gadfly,
No, it's simply not true at all. SM + GR contain a huge amount of deep, non-trivial mathematics.
Mathematics is a language that can be of great use in situations where our intuition fails us, but typically it is not sophisticated mathematics that is of use, but more basic mathematical ideas. What's amazing about SM + GR is that deep mathematical ideas are precisely what is needed to state the theory.

24. **Tim May**
April 21, 2015

Peter, I learned of your site after finding John Baez's site (or series of postings), around 2003-4. I was pretty down on "string theory" at the time, having been unconvinced by the semi-popular version that all particles are some kind of violin strings. So your site, and the books by you and Smolin were refreshing. (I am less down on ST now, despite the untestable part, but at least things have moved beyond the guitar and violin metaphors. Also, ST no longer seems to be the 800-lb gorilla. Perhaps I am wrong in terms of who is being hired, but certainly the shine has come off it a bit in the past 15 years. The AdS-QFT thing is really intriguing, and two of the most interesting talks I've been to at Stanford have been by Lenny Susskind!)

It seems to me that the category/topos and representation theory aspects of physics are central, though not always necessary for "shut up and calculate" work, and that these involve some of the deepest aspects of work done by Grothendieck, Serre, Mac Lane, and others. (Maybe my connection to computers and programming theory have influenced my interest in type theory, category theory, and "topology via logic" points of view, which are along the lines of "algebraic geometry" a la Grothendieck, Lawvere, Abramsky, and others.)

I'm just a retired physicist who did some fun work for Intel in the 70s and 80s and who took Jim Hartle's GR class in 1973. (Wish I'd learned more…seems black hole horizons are a lot more confusing than I seemed to think back then! As the current slogan goes, it's not the singularity at the center that is interesting, but the event horizon.)

BTW, some of the most invigorating stuff back then was in analysis and point set topology (mostly class notes, but Kelly and Dugundji were often used). Utterly unconnected with physics, to me at that time, but I keep coming back to it again and again. And it of course relates to quantum theory in interesting ways–lattices, von Neumann algebras, even intuitionistic logic.

Thank you for your site.

–Tim May, California

25. **Kevin NYC**
April 24, 2015

LadyGaga is famous for her outrageous costumes, esp her shoes, but she seems to have tamed it down some now that she is engaged/married. She does have a very good singing voice and is trained.

Her greatest accomplishment is getting everyone to call her 'Lady Gaga' and be known across the globe.

Things Have Changed

April 24, 2015
Categories: Uncategorized

I've been busy with other things, but after taking a look today at various new things related to quantum gravity, I was struck by how much things have changed sociologically in that subject over the last few years. Back in the days of the "string wars", debates about quantum gravity were fiercely polarized. Oversimplifying and caricaturing the situation a bit, the two sides of the quantum gravity debate were:

- Those interested in loop quantum gravity as well as other more exotic attempts to reformulate the problem of quantum gravity. These people just considered pure quantum gravity and devoted a lot of effort to analyzing the deep conceptual issues that arise. They sometimes considered highly speculative hypotheses, trying out abandoning the usual basic axioms, for instance replacing fundamental axioms of quantum mechanics. Lee Smolin was an influential figure, and the Perimeter Institute a major center for this research.
- String theorists, who argued that the appearance of spin-two massless mode in the quantized string spectrum showed that string theory was the only way to understand quantum gravity. They claimed that they had a single, very specific and highly technical mathematical structure to study, which obeyed the conventional quantum theory axioms. Their efforts were devoted to specific computations in this theory, and they seemed to regard the other side of the debate as woolly thinkers, caught up in meaningless ill-defined philosophical speculation. The KITP at Santa Barbara, led by David Gross and Joe Polchinski, was a major center for this side of the debate.

These days, things have changed. If you're at Perimeter, prominent activities include:

- This week's conference on a very technical issue in string theory, superstring perturbation theory.
- This month's course of lectures on Explorations in String Theory.
- The next public lecture will feature Amanda Peet promoting string theory. Peet has been one of the more ferocious partisans of the string wars. The text advertising her public talk a few years back at the Center for Inquiry in Toronto warned attendees who might consider "parrotting of critical views by outsiders like Lee Smolin."

On the other hand, it you're in Santa Barbara these days, you might be participating in a KITP conference on Quantum Gravity Foundations. This is featuring very little about the technical issues in superstring theory being discussed at Perimeter, but a lot of discussion of deep conceptual issues in quantum gravity. There's also a lot of willingness to throw out standard axioms of physics, maybe even quantum mechanics. They're even letting Carlo Rovelli talk.

The sort of speculation going on at the KITP is featured on the cover of this month's Scientific American, and this week Quanta magazine will be publishing a series of pieces on something related, the "ER=EPR" conjecture. There's debate whether anyone really understands this and whether it is consistent with standard quantum mechanics. It also features a diagram that people call the "octopus" diagram. Back in the day it was Lee Smolin who was getting grief for an "octopus" diagram (see here), yet another way in which things have changed.

For a more balanced view of quantum gravity issues, you might want to spend your time in France, where the IHES recently hosted an interesting series of surveys of the subject (see here), and the Quantum Gravity in Paris conference featured more specialized talks. In the category of quantum gravity topics I wish I had more time to learn about, Kirill Krasnov's talk was presumably related to this recent work, which looks intriguing.

Comments

1. **Noboru Nakanishi**
   April 24, 2015

   I believe that the most natural unification of Einstein gravity and quantum field theory is the (non-perturbative) operator formalism of quantum Einstein gravity. I don't understand why people wish to neglect this fact.

2. **Dave Miller in Sacramento**
   April 25, 2015

   Peter,

   Over the decades, I've had similar musings to the firewall and ER=EPR ideas: for example, I remember around 1980 a discussion with Subhash Gupta when we were both at SLAC about the fact that black holes never actually quite form (classically, it takes an infinite amount of coordinate time, but in that amount of coordinate time, Hawking radiation wipes out the black hole); I always assumed something like the firewall had to exist.

   But I (and as far as I know Subhash) never got beyond such musings.

   So, I hope all this leads to wild successes.

   But, my "gut feeling," alas, is that you have to be Einstein for such qualitative musings to lead to a revolution in understanding.

   So, what's your gut feeling about all this — the firewall, ER=EPR, etc.?

   Dave Miller in Sacramento

3. **Peter Woit**
   April 25, 2015

   Dave Miller,
   I've always been dubious that very general speculation about gravity/space/time/black holes, etc., is going to go anywhere. I tried to give the example of Krasnov's work as the kind of thing that seems more promising. The big problem of quantum gravity to me seems to be the relation of space-time degrees of freedom and the internal degrees of freedom of the standard model. The best argument for string theory vs. LQG was always that string theory would explain this relation.

   These days, prominent string theorists seem to have just given up on the problem, and adopted a version of Smolin's call for "seers", but on steroids. The world has always been full of people with ideas about how they are going to revolutionize physics,
   ideas which are far too vague and speculative to ever go anywhere. What's weird is that some of the most prominent figures in the theoretical physics community are now headed down this route (one example of recent years is Verlinde and "entropic" gravity). I'm loathe to specifically criticize "firewalls" or "entangled particles = wormholes" too much since I don't know exactly what these people are doing (I've spent enough time looking at it though to decide that my time was better spent on other things).

One thing that is clear though is that, unless it actually achieves something, putting this on the cover of Scientific American, or otherwise giving it a lot of publicity, is not a great idea. More hype is not what this subject needs.

4. **Dave Miller in Sacramento**
April 25, 2015

Peter,

Thanks — I suspect you're right: perhaps a lot of physicists (and I'm not excluding myself) are overly beguiled by the thought of being Einstein thinking deep thoughts while working in the Swiss Patent Office.

You wrote:
>The big problem of quantum gravity to me seems to be the relation of space-time degrees of freedom and the internal degrees of freedom of the standard model.

Are you acquainted with the "problem of time" in quantum gravity? It seems to me the solution probably lies in an integrated theory where the standard-model degrees of freedom act as "clocks" to make time determinate: of course, I have no idea how to actually do this, alas.

I'll look into Krasnov's work as you suggest. Like you, I am old enough to remember when "real" physics meant doing calculations that had, in principle, experimentally testable predictions.

I suppose the best counter-argument to your and my pessimism is that, if enough people muse over various oddities and loose threads in existing theories, perhaps one of them actually will end up being Einstein sitting in the Patent Office.

Anyway, it has been interesting watching the "string wars" and all the rest, even if it is hard to see how it has advanced physics. Thanks for keeping us up to date on what is going on.

Dave

5. **Neil**
April 25, 2015

I am glad to see Asymptotic Safety first in the list of alternatives in Nicolai's survey, rather than at the bottom where it usually is. But perhaps that was just alphabetic. I thought after the remarkable prediction of the Higgs mass by AS, it would have gotten far more attention. Perhaps it is just that non-perturbative theory is so difficult.

6. **Tom**
April 25, 2015

The type of ideas discussed by Krasnov, that is GR as YM. have been around for more than 30 years. The classic one is McDowell-Mansouri SO(4,1) gravity, PRL 38, 739 (1977), reformulated by Stelle and West in PRD 21, 1466 (1980) in a way that connects YM with Einstein-Cartan and (anti)de Sitter. Very interesting, but did not lead to anything better than good old GR except for giving a moral raison d'etre for the cosmological constant…

7. **Peter Shor**
April 25, 2015

I think that a symptom of what is wrong with string theory right now is that nobody is saying "ER=EPR makes absolutely no sense. It isn't compatible with our understanding of general relativity, and it isn't compatible with our understanding of quantum mechanics. Please explain to me how this makes any sense."

Compare this with the mathematicians' treatment of Mochizuki's claim of a proof of the ABC conjecture. Or, if you want a mathematician of the stature of "EP=EPR", the treatment of Connes' idea about how to prove the Riemann hypothesis. In these two cases, we get a healthy amount of skepticism.

8. **David Roberts**
April 25, 2015

> Einstein thinking deep thoughts while working in the Swiss Patent Office.

and then (fast-forwarding a few years) he had to go and learn the then-hardcore-pure-mathematics now known as semi-Riemannian geometry to properly formulate a working theory to get gravity in the picture, with actual predictive power. Having a nice idea like the equivalence principle is not enough.

9. **jd**
April 26, 2015

I hope to hear opinions on the paper by Saini and Stojkovic, Radiation from a Collapsing Object is Manifestly Unitary, PRL114, 111301(2015).

10. **Dave Miller in Sacramento**
April 26, 2015

jd,

Bee Hossenfelder has a critical discussion (including some heated back-and-forth in the comments with one of the authors) of the paper.

The bottom line seems to be that:
a) The paper was over-hyped in the media
b) If you assume no singularity forms, the paper proves no information loss occurs
c) Everyone already knew b) anyway

In any case, as I recall, Hawking actually agreed that no information loss occurred some time ago (vide Lenny Susskind's book), though of course that does not necessarily settle the issue.

My own suspicion is that the horizon never technically forms because of Hawking evaporation (technically, you can only be sure of the horizon at standard coordinate time equals infinity); however, quantum effects should "smear out" the horizon and… well, I'm unclear on how that quantum smearing affects everything.

Dave

11. **martibal**

April 26, 2015

@Franck: well, since Connes name has been mentioned in the discussion (in another context though), let me answer that if there is at least one approach where the relation between the space-time degrees of freedom and the standard model degrees of freedom are addressed, this is precisely Connes noncommutative geometry. It can be summarized as saying: the standard model is a gravity theory, but gravity on a slightly non-commutative version of space-time. This is off topic and I already had a long exchange on that topic in the comments of a previous post, so I do not want to discuss it here, just recall that even though it is not hyped in the hep-th community, it exists and has been through recent interesting developments.

12. **Lee Smolin**
April 26, 2015

Dear Tom,

I partly agree, but there are three important aspects of gravity expressed in connection variables that MacDowell and Mansouri and Kelle and West missed. These are the fact that general relativity in 3+1 dimensions can be understood as a constrained topological field theory and that when doing so there is a redundancy in the equations of motion that can be removed by making the theory depend just on the chiral half of the spacetime connection. That is you can write a topological field theory for the chiral left handed SU(2) left space time connection, and constrain the action in the simplest possible way and find that general relativity emerges.

The third fact is that the action is most directly expressed as a function, not of the metric, and not of the frame fields, but of a self-dual two form. (The constraints that I mentioned yield the frame field as an integration constant.)

When one combines these three insights one has the Plebanski action, (whose Hamiltonian formulation was discovered by Ashtekar), which was also rediscovered by Capovilla, Dell and Jacobson. This action is not just polynomial, it is cubic in the fields, making it the simplest possible action that GR can have. The important work of Kirill Krasnov extends and deepens these insights.

These insights are also at the heart of loop quantum gravity and spin foam models. Non-trivial results are possible for a non-perturbative quantization because the action is that of a diffeomorphism invariant gauge theory with a cubic action, closely related to topological field theory.

Macdowell-Mansouri and Stelle-West build on a different insight, that (in the modern language) you can relate GR to a broken topological field theory of the deSitter or anti-deSitter group. This is compatible with the Plebanski formulation and can be incorporated into it.

Indeed by combining these insights there emerged also a connection with Chern-Simons theory induced in 3 dimensional boundaries such as horizons; and this led to an understanding of the role of the cosmological constant in quantum gravity, as an infrared cutoff that is imposed as a quantum deformation of the chiral SU(2).

Thanks,

Lee

13. **Mark**
April 26, 2015

Peter Shor,

I'd be really curious why you think "ER=EPR makes absolutely no sense". My understanding is that it gives a geometric description of states in QG (very entangled black holes) that had no prior understanding. It is an extension of the AdS/CFT description of the eternal BH/thermofield double, and you can argue for it starting from Ryu-Takanayagi (Section 1 of http://arxiv.org/pdf/1412.8483.pdf). These may not be overwhelming evidence, but why is the idea nonsense?

14. **vmarko**
April 26, 2015

Dear Lee and Tom,

The Plebanski action is indeed polynomial and cubic, but the action proposed by Krasnov is neither, due to the presence of the square root in the action (2) (also obvious in (20) in his paper). I don't really see the benefit of this for the spinfoam models. In particular, the absence of the tetrad fields makes it hard to couple fermionic matter later on, just like in the Plebanski case.

There is also the approach based on the Poincare 2-group (I'm shamelessly advertising myself here… see arXiv:1110.4694), with the constrained BFCG action — also polynomial and cubic, similar in structure to Plebanski in that it has a topological sector plus the simplicity constraint. Its advantage is that tetrad fields are explicitly present in the topological sector, which allows for the straightforward coupling of fermions. Also, the constraint has a much more transparent geometric interpretation. Finally, if one adds the cosmological constant term, one can show that the MacDowell-Mansouri action can be recovered as a second-order theory from this action (by substituting one of the algebraic equations of motion back into the action).

IMO, there are many reformulations of GR action in terms of various variables, from the historic ones (Palatini, Einstein-Cartan) to these modern ones (including the teleparallel gravity, both historic models and the recent Baez-Wise version). Each reformulation has certain appeal and benefits, as well as drawbacks. Krasnov's approach is certainly interesting, but I honestly don't see why it deserves so much hype. What problem does it solve, that other proposed actions don't? And is that solution worth the price of the nonpolynomial simplicity constraint?

Best, 🙂
Marko

15. **Guest**
April 26, 2015

Well said, Peter Shor. What I find quite entertaining is that Lenny Susskind – who jointly proposed this ER=EPR is obviously completely unconvinced by it and he doesn't try to hide that very much. I kind of expect him at any moment to say "Oh, I can't do this anymore. We all know it's not right."

16. **EFT**
April 26, 2015

Dear Peter and Lee,

I have read with great joy Peter's book "Not Even Wrong" as well as Lee's book "The Trouble with Physics".
Do you think that this change is a sign that theoretical physics is slowly becoming healthy again (using Lee's words) and that this will take physics out of its present crisis

and lead it back to the exciting and glorious years of the past centuries?

Thanks,

17. **Peter Woit**
April 26, 2015

EFT,
Personally I don't think theoretical physics is getting healthier. The changes in recent years have been a mixed bag. One positive development is that there's a lot more skepticism in the physics community about string theory, with the issues that Lee and I were raising moving from a marginalized point of view to a rather conventional one among physicists. This potentially creates space for people to work on other ideas. Unfortunately, there has been a backlash against sophisticated mathematical approaches, with too many physicists believing that it was mathematics that led string theorists astray. As a counterpoint, such mathematical research remains alive, partly due to significant funding from Jim Simons.

Among string theorists themselves, a sizable number have basically given up on conventional science, using the multiverse as an excuse to make string theory immune to challenge from experiment. Many if not most have moved on to studying other topics, from condensed matter physics to the kind of quantum gravity going on at the KITP. I have no expertise on the condensed matter stuff, but as indicated here I'm skeptical about the quantum gravity ideas and how they are being pursued. This seems to bring together the worst of the string theory fondness for hype and the worst of the long tradition of empty quasi-philosophical speculation.

18. **Peter Shor**
April 26, 2015

@Mark: Why does ER=EPR make no sense to me? Because general relativity is a well-defined theory of gravity without entanglement, and wormholes are created by curved space (possibly with negative energy density), not by information. And because quantum mechanics is a perfectly well-defined theory without general relativity, and entanglement is perfectly well-behaved without invoking wormholes.

19. **Mark**
April 26, 2015

@Peter Shor: Thanks for your answer. I don't think they wanted to explain entanglement or ER bridges, which are well understood. They just say that in QG there are some states (e.g. two very entangled BHs) that can be described in another (dual) way involving an ER bridge. While the original description is certainly fine (the wave function of the two BHs is entangled), the other description they are proposing is more geometrical and may help in calculations/thought experiments.

I'm at risk of repeating myself, but take the most concrete manifestation of EPR=ER: 1. Two CFTs in a thermofield double state. There's no gravity, but there's entanglement. 2. Take the eternal BH in AdS, the two asymptotic regions are connected by an ER bridge. By CFT/AdS 1. and 2. are equivalent descriptions of the same thing, so EPR=ER. (I acknowledge that this isn't a perfect example, because 1. was not a QG state, but I wanted to give a setup, where there are calculations that support the picture.)

20. **Peter Shor**
April 27, 2015

@Mark

Let me quote from Maldacena and Susskind's paper:

> Suppose that we take a large number of particles, entangled
> into separate Bell pairs, and separate them in the same way as the mini-black holes. When we collapse each side to form two distant black holes, the two black holes will be entangled. We make the conjecture that they will also be connected by an Einstein-Rosen bridge.

And I repeat: with our current understanding of entanglement (which I think is very good), it cannot generate ER bridges, even in the interior of black holes where it's not falsifiable. You might as well conjecture that unicorns live in the interior of black holes. That's not falsifiable, either.

21. **Peter Woit**
April 27, 2015

To add my own less well-informed take on this to Peter Shor's, perhaps someone can point me to something I'm missing here (which I surely am):

I understand very well exactly what an entangled pair of particles is, both mathematically and physically. I also understand (less well and less exactly) the AdS/CFT duality conjecture, as a conjecture relating two well-defined theories (supergravity on AdS and N=4 SUSY on the boundary). When people however announce that they are conjecturing some duality between the simple, well-understood entangled particle system and a complicated gravitational system (e.g. a wormhole), I immediately get lost, because I can't figure out exactly what the gravitational system is or how the mapping is supposed to go. When I try and read about this, all I find are vague general statements about holography and such, so I soon give up.

It's possible one can get somewhere interesting by following this kind of highly speculative line of thinking, but I don't see this happening. It seems to me that people are going to credulous people in the press like K.C. Cole to promote vague speculation, long before getting to the point of having a well-defined idea that can be evaluated sensibly in some way or other (of course all of this is completely unmoored from experiment, but it would be nice if it weren't also unmoored from the parts of theoretical physics we understand).

22. **Mark**
April 27, 2015

@Peter Shor

It's true that using particle scattering you can't see behind the horizon, so there could well be unicorns there. But people have thought of other (more subtle) observables that probe the interior. Take this paper: http://arxiv.org/pdf/1303.1080.pdf

The time evolution of entanglement entropy after a quantum quench (known in 2d CFT from the work of Calabrese-Cardy) is reproduced by the Ryu-Takanayagi surface going through the ER bridge. (It's a very special situation, but also concrete. Maybe the same results can be produced without an ER bridge, but the ER bridge is consistent with the field theory knowledge.)

Hong Liu from the CTP also worked on probing the geometry behind the horizon. You can ask him, if you're unhappy with my imperfect explanations.

23. **Guest**
April 27, 2015

The best bit about ER=EPR is that apparently now “a particle is a black hole”. Honestly! Lenny Susskind said it. If a crank had said it, it would have been dismissed.

24. **Matthew Foster**
April 27, 2015

I can’t say much for the quantum gravity side of things, but I would certainly advocate against a backlash to sophisticated mathematical methods (esp those developed alongside, entangled with the first phase of string theory ending in the 80s) . 1+1-D Conformal field theory has been enormously important in statistical physics and condensed matter; much of this structure grew out of quantum gravity and string-related research. E.g., key results on the structure of affine Lie algebras by Gepner and Witten. 1+1-D CFT underlies the deepest formulations of the fractional quantum Hall effect via Moore and Read’s construction of many-body wavefunctions from conformal blocks, and the understanding of edge states as CFTs (Wen). That’s the clearest application of WZW/Chern-Simons holography to a real well-studied experimental system (5/2 state, which may well harbor non-abelian Ising anyons).

If you are a field theorist working anywhere in quantum physics except unification and quantum gravity, I think you can profitably view much of string theory (1st quantized) as an elaborate self-consistent construction exercised within the framework of conventional quantum field theory. Much of the mathematics that we are now employing in condensed matter has already been field-tested there, and partially translated from the language of mathematicians into a form that one can use to compute things with. I.e. I’m happy to use results from conformal embedding theory, but I’m glad I didn’t have to prove their validity! If string theorists decide to stop doing this job of importing mathematics, I’m afraid us condensed matter theorists will have to do it ourselves. That means more time spent not working on calculations not “immediately” to experiment, or even perhaps concrete models. I’m not sure our funding agencies will approve…

25. **Shantanu**
April 27, 2015

Peter, this is also the first time Ed witten has given a conference (or any technical talk)
at PI. Earlier he has given a public lecture there a decade back

26. **gadfly**
April 27, 2015

EFT,

Theoretical physics as a whole is pretty healthy, but the subfields concentrating on gravity, high energy particles, and cosmology will remain in their current languid circumstances until more experiments probe those regions, since theorists are impotent and helpless without actual empirical data. This isn’t a cultural problem, since pragmatism will beat idealism if there are good experiments, sooner or later.

27. **Peter Woit**
April 27, 2015

Matthew Foster,
Thanks. 1+1 d CFT is an amazing subject, both in terms of mathematics and physics. It’s not really string theory, but string theory research brought a lot of attention and progress to the subject. Unfortunately the CFT in AdS/CFT is a different one, and the concentration of the subject on that topic hasn’t led to the same kind of progress.

gadfly,
Cosmology has lots of data, so does HEP, the problem is that theorists are fond of trying to answer questions not addressed by any data, and often such that it is unclear whether there ever will be data. It’s worth noticing that pure mathematics is a subject that has managed to keep making great progress, even in the absence of any data. Theorists should pay attention to how mathematicians do it. One thing they don’t do is promote ill-defined ideas…

28. **Chris W.**
April 27, 2015

> It’s worth noticing that pure mathematics is a subject that has managed to keep making great progress, **even in the absence of any data**.

Isn’t that possibility inherent in pure mathematics? It’s not about the physical world and in a strict sense isn’t empirical. The only “data” it might need, if any, are the results of calculations, enumeration of cases, etc., that raise interesting mathematical questions.

29. **Peter Woit**
April 27, 2015

Chris W.,
Yes, pure mathematics has always been pursued mostly independent of any data. My point is that mathematicians have over the centuries developed effective ways of making progress based purely on the internal logic of the problems they pursue. This is what theoretical physicists are also trying to do in areas like quantum gravity.

One of the most basic parts of the method of pure mathematics is an insistence that you need to pay close attention to exactly what it is you understand, and exactly what claims you are trying to make. If you don’t, you’ll get lost very quickly. I’ve never had any luck arguing with physicists that if they’re not going to have any data, they need to behave more like mathematicians. In general they’re convinced that making precise statements is a waste of time that will just slow them down, but I think they should give this a try. Couldn’t hurt…

30. **Justin**
April 27, 2015

Peter,

I know it is off topic, but April is almost over and I was hoping that you would write a blog entry commemorating the 100th anniversary of Noether’s Theorem which was discovered in April 1915.

Best Wishes

31. **Peter Woit**
April 27, 2015

Justin,
Sorry, but I’ve just spent much of the last three years writing a book about how to do symmetry arguments in quantum theory without using Noether’s theorem, for a blog entry about this, see
http://www.math.columbia.edu/~woit/wordpress/?p=7146
So, I’m the wrong person for this.

Noether was a great mathematician, and Noether's theorem is a great result, central to understanding symmetry in the Lagrangian formalism. But, these days I'm quite wrapped up in the Hamiltonian formalism…

32. **Dave Miller in Sacramento**
April 28, 2015

On the issue of quantum gravity/information loss in black holes/firewalls, etc., I just ran across this preprint by Hawking from early last year in which he argues that no event horizon ever actually forms and that therefore there is no need for a "firewall." The paper has no math at all (!); nonetheless, I am unsure what to make of it.

Peter, I cannot find that you have mentioned this: do you (or anyone else) know what to make of it?

Dave

33. **chethan krishnan**
April 28, 2015

@ Matthew Foster

Great to know that there is real appreciation for the work of string theorists from a working Condensed Matter physicist. I have seen ego-based nonsense from both sides, but this is rare.

Cool, awesome, etc.

34. **Thomas Larsson**
April 28, 2015

Matthew Foster,

Nobody has denied that the application of CFT to statistical systems is a huge success, and I have argued for 25 years that BPZ deserve a Nobel prize for this. At the same time, the limitation is very obvious in this context.

Since the string world sheet is a unobservable target manifold, in HEP you might argue that infinite conformal symmetry is so special that it must be Nature's choice. In statphys it is the base manifold that is 2D, and we know that there are 3D systems as well, and that these are both more experimentally relevant and much more complex.

When I was a postdoc I heard a joke about Polyakov's response to "Good morning": "Yes, it is a good morning, nobody has solved the 3D Ising model yet". I have no idea how much truth there is to this joke, though.

35. **Peter Shor**
April 28, 2015

@Dave
I expect that even Hawking sees this paper as not an actual resolution of the black hole paradox, but as a proposal for a high-level description of what such a resolution should look like.

36. **Jeff M**
April 28, 2015

Pardon my ignorance, as a mathematician I'm kind of confused by Hawking's paper. Not the paper, I understand his argument, more or less. And off the top of my head, I'm guessing he's right about firewalls not existing. But why exactly is ADS/CFT supposed to be useful? I mean ADS/CFT applies in universes with negative cosmological constant, which is not the case in our universe. And it's only a conjecture. I certainly understand why ADS/CFT is an interesting conjecture, but it seems to me that in order to make it relevant at the very least someone should come up with a version that applies in the actual universe, even if it's still only a conjecture. Am I missing something?

37. **Peter Woit**
April 28, 2015

Jeff M,
This whole story is based on the idea that some version of an AdS/CFT type holography is supposed to explain the black hole information paradox. Ten years ago the people involved in this were claiming that holography solved the black hole information paradox, now they've changed their mind, and their arguing about whether/how it does. Part of the problem here is that you don't know why AdS/CFT holography works, much less how something analogous is supposed to work in this different setup.

38. **Jeff M**
April 28, 2015

Thanks Peter. Personally, I was never really convinced losing unitarity was that big a deal. Not a big fan of determinism 🙂

39. **Peter Shor**
April 28, 2015

@Jeff M: I think the big problem with losing unitarity is that we have no idea how to generalize QFT to a reasonable non-unitary theory.

So assuming the real theory is unitary is kind of like looking for your keys under the streetlight. You suspect that the keys aren't under it, but you also know that if they're not, you are *never* going to find them.

40. **Vladimir**
April 28, 2015

Dr. Woit,

I've heard Susskind claim that not only does ER=EPR for (maximally entangled?) black holes, but also for elementary particles. Do you know what he means by this? How can wormholes connect two entangled particles if the entangled particles are prepared at low energies?

Thanks.

41. **Matthew Foster**
April 28, 2015

@ Jeff M

AdS/CFT might be useful and interesting if it helps us say things about CFTs. That's the context in which there is non-string theory interest in high energy physics, since you could view the gravity dual of N=4 SUSY Yang-Mills as a calculational tool for studying the strong-coupling regime of this theory. How you get from this very special theory to non-conformally invariant standard model physics is not obvious, since the question is how much infrared physics comes in. (And that's assuming the conjecture works for this special theory). But similar questions arise in connecting lattice models to CFTs and massive integrable QFTs, which do indeed work in suitable contexts. E.g. the E8 spectrum of the massive transverse field Ising model.

Some condensed matter physicists are also interested in AdS/CFT, if it might shed light on CFTs in more than 1 spatial dimension or stat physics models in more than 2D. Without studying the current state of the subject I can't say much, but my impression is that the precise correspondence is not well-established for other (e.g. non-gauge) theories. Nevertheless, one might hope that some general results about CFTs that do not follow simply from global conformal invariance could be obtained this way, such as the Ryu/Takayanagi formula for entanglement entropy.

One curious aspect of AdS/CFT is that the gravity side at weak coupling is classical and generically non-integrable, meaning you get dissipation (and presumably, chaos and thermalization). One expects these to emerge in an isolated quantum many particle theory when driven far from equilibrium (e.g. following a global quantum quench), so long as the system is not integrable. But in general we don't know a way to derive such "non-unitarity" from QFT. For disordered quantum systems such as the problem of Anderson (de)localization, we know how to obtain effective non-unitary theories that correctly capture the "chaotic" aspects of such systems, e.g. wavefunction multifractality. These can be non-unitary CFTs, which admittedly are harder to find and make sense of (since they are beyond Peter Shor's streetlight), but there are a few well-understood examples.

42. **Peter Woit**
April 28, 2015

Vladimir,
I have no idea. One problem with this whole subject is that listening to people like Susskind, it's sometimes very hard to tell the difference between when they have a reasonably well-defined idea and when they don't, so you don't know what to ignore and what to pay attention to.

43. **Jeff m**
April 28, 2015

@peter shor. Thanks, nice to know my intuition isn't completely crazy 😉

44. **chethan krishnan**
April 28, 2015

@JeffM

In AdS, black holes *still* have the same theoretical problems as in flat space. So it is conceivable that we are not throwing the baby out with the bathwater by working with AdS instead of flat space, even though AdS is possibly substantially more tractable, due to AdS/CFT.

Note also that since the universe is accelerating, real world black holes are not actually flat space black holes either! For astrophysical questions, this doesn't matter. AdS/CFT deals with fundamental (as opposed to astrophysical) aspects of black holes, and needs the asymptotic boundary in a crucial way, but I think it is still instructive, at least if black hole physics is dominated by the (parametrically at least) near horizon region.

45. **Moshe**
April 28, 2015

Matthew Foster: Your last paragraph got my attention, could you please provide an entry point to the literature on these non-unitary CFTs relevant to (many-body?) localization?

Thanks,

Moshe Rozali

46. **matthew foster**
April 29, 2015

@ Moshe

The non-unitary CFTs of which I speak describe critical delocalization, as occurs at a non-interacting Anderson metal-insulator transition. There are different examples under different levels of control. E.g.,

1) "Exactly solved" 2D critical states: Dirac fermions coupled to random abelian or non-abelian vector potentials. Critical properties like wavefunction multifractal exponents are known exactly, from 1+1-D CFT. (Non-interacting, so energy is a parameter and the post-disorder-averaged theory is 2+0-D). These are affine Lie algebras, e.g. Sp(2n)_k, U(n)_k, SO(2n)_k. For positive integer n,k these are unitary, but one needs to take the limit n->0 at the end to get results relevant to disorder physics ("replica trick"). There's a better-defined version (the SUSY trick), but you get the same results. These have also been checked numerically. First papers were by Ludwig, Fisher, Shankar, Grinstein and by Nersesyan, Tsvelik, Wenger in 94. We review these in our recent papers on 3D topological superconductors, since these models turn out to be relevant to surface states.

2) Anderson metal-insulator transition in 2+\epsilon. Perturbative RG results via the epsilon expansion. Same kinds of data can be extracted.

3) Plateau transition of the integer quantum Hall effect. Extensive numerics spanning 20 years, and many indications that the underlying theory is conformally invariant, but no one knows what that theory is yet analytically (as far as I know). Martin Zirnbauer and Ilya Gruzberg are among the people who've worked extensively on this problem.

Both 2) and 3) are reviewed in Evers and Mirlin, Anderson Transitions, RMP 2008.

As for the interacting problem (e.g. MBL), obviously I wouldn't expect space-time conformal invariance because the disorder is non-dynamical. In some situations, however, you can graft interactions into the non-interacting theory and determine stability, and perhaps even identify nearby interacting, critically delocalized fixed points (scale invariant separately both time and space.) This happens e.g. in the unitary metal-insulator transition with long-ranged Coulomb interactions in d = 2 + \epsilon, and also in class AIII topological superconductor surface states in a certain regime (e.g. here).

47. **Moshe**
April 29, 2015

Thanks Matthew!

48. **S**
May 3, 2015

[comment moved from inappropriate post to more appropriate one]

Not strictly on topic, Peter, so delete if you wish, but consider it a request for the future — do you have any thoughts on this stuff? It's hard for me to tell if it's the same as the ER=EPR material that was discussed in part I, and which you already addressed in an earlier post. Thanks!

https://www.quantamagazine.org/20150428-how-quantum-pairs-stitch-space-time/

49. **Peter Shor**
May 3, 2015

@S: No, that's not about ER=EPR. It's about research which is much more plausible than that (that's not setting a very high bar …).

50. **Peter Woit**
May 3, 2015

S,
I moved your comment to here. That topic is very relevant to the general theme of the posting: 15 years ago this kind of attempt to explain quantum gravity as "information" would have been the kind of thing the string theory community would have little time for. Things have changed.

I don't understand though exactly what these people are doing so won't comment on it specifically. In general I'm skeptical about this sort of thing. There's a long tradition of attempts to derive fundamental physics from simple-minded models of 0s and 1s, it's a favorite idea of a wide range of people from crackpots to great scientists. As far as I can tell it has never gone anywhere, and the mathematical ideas that have been successful in fundamental physics are ones of a very different nature. On the other hand, an infinite dimensional Clifford algebra acting on qubits maybe is closer to fundamental physics.

One thing to note about the Ouellette Quanta article is that I don't think she mentions that her husband has just been coauthor of a paper on the precise topic she is writing about, see
http://arxiv.org/abs/1504.06632

51. **matthew foster**
May 6, 2015

From my limited knowledge, my impression is that one fundamental limitation of AdS/CFT schemes seems to be the lack of an algebraic structure that can determine the "data" of the CFT. In 1+1-D CFT, the differential equations satisfied by correlation functions derive from specializing the generic Verma module to descendants of an appropriate primary field. At least for a rational CFT the spectrum is determined by the algebra. Of course the conformal group is finite in d > 2 and it's not clear to me what if any connection one has between local QFTs and "central extensions" of higher dimensional loop algebras (analogs of Kac-Moody).

If the CFT is in some general sense "non-integrable," then it makes sense that the crucial data (scaling dimensions, OPE coefficients) cannot be exactly computed via some machine, algebraic or holographic. But one might hope that correlation functions beyond the trivial ones (four point and higher) are determined, if one can input the necessary data obtained elsewhere (epsilon expansion, numerics…). Do we know if this is the case? From conversations I've had with experts, I have the impression that these correlators might be less universal than in 1+1-D CFT. Then one needs to understand how to classify different holographic duals. Naively I would expect that has something to do with the additional space used for compactification on the gravity side, which is usually not discussed in applications to condensed matter…

52. **Thomas Larsson**
May 6, 2015

Matthew,

The higher-dimensional analogs of affine algebras can not arise in QFT. Gauge anomalies in Yang-Mills theory in 3+1D are proportional to the third Casimir, but the Kac-Moody extension is proportional to the second. Hence the multi-dimensional affine extension requires that we go beyond QFT.

As you noted, the conformal group is finite-dimensional when d \geq 3, but the Virasoro algebra also arises as an extension of the diffeomorphism algebra on the circle. As such it has a direct generalization to higher dimensions, but the extension is not central except when d=1.

At least in statistical physics there is strong reasons to expect universality to hold, both from experiments and from theoretical considerations (4-epsilon expansion).

53. **Matthew Foster**
May 6, 2015

@ Thomas Larsson,

Thanks–I'm not an expert on the anomaly structure in QCD so your comment is very helpful. But I wonder if there might be connections between affine algebras and non-gauge theories. A trivial observation is that you get a loop algebra as the commutation relations of spins on any lattice, when expressed in terms of momentum modes. Obviously that doesn't help you understand anything about a particular Hamiltonian. But I wonder if non-trivial extensions can give rise to interesting representations that might "exactly solve" some the low-energy sector of some (artificial but non-trivial) quantum spin models. Locality would be the hard thing to achieve I would guess, but there are useful non-local spin models that arise as mean field approximations in condensed matter or in cavity QED type schemes.

54. **Moshe**
May 6, 2015

Matthew, there is a large body of work on the questions you are raising, and this is probably not the best place for this discussion. Briefly, it is true that the higher dimensional CFTs in AdS/CFT are not integtable, but one perspective is that this is what makes them interesting. For example there is recent effort to quantify quantum chaos using this framework, for which genericity arguments are more powerful than algebraic structures. As you expect from a quantum chaotic system, for generic observables there isn't a closed form expression, or a convergent series expansion. Another comment is that AdS/CFT is no longer a good name for the subject, since most of the work has to do with massive deformations of CFTs and other non-CFTs, so this is really duality relating continuum QFT to gravitational theories.

55. **Matthew Foster**
May 7, 2015

@ Moshe

Now I'm intrigued! Can you give a reference or two for the application to quantum chaos?

Best,
matt

56. **moshe**
May 7, 2015

Look at recent papers by Douglas Stanford and Steve Shenker, especially a very recent one with Juan Maldacena. This is pretty technical and not completely digested yet, but hopefully some of it will be useful.

57. **moshe**
May 7, 2015

I should also mention that Alexi Kitaev has been working on very similar ideas. There are various talks on the Internet (one in KITP in their ongoing entanglement program), but none of it is published as far as I know.

58. **Matthew Foster**
May 11, 2015

Thanks Moshe!

Dreams of a Final (or Better) Theory

May 1, 2015
Categories: Multiverse Mania

John Horgan has an interesting interview with Steven Weinberg here. Weinberg isn't very optimistic about possible progress these days:

> **Horgan**: In 1995 you told me that it's a "terrible time for particle physics." Are you feeling any better about your field now? Are there any particular advances that give you hope?
>
> **Weinberg**: I'm not much more cheerful.
> …
> **Horgan**: Do you still believe in the attainability of a "final theory" of physics, one that ends what you called "the ancient search for those principles that cannot be explained in terms of deeper principles"?
>
> **Weinberg**: I still expect there to be a final theory, but I'm less confident that humans will discover it in this century.

When asked about when string theory should be abandoned as a dead end he ignores that part of the question:

> **Horgan**: In your new book, To Explain the World, you write that "scientific theories cannot be deduced by purely mathematical reasoning." Doesn't that principle apply to string theory? At what point, if ever, should string theory be abandoned as a dead end?
>
> **Weinberg**: String theory may be inspired by mathematical reasoning, but not deduced, and certainly not confirmed.

He defends the multiverse with

> Further, if we find some future theory that does make successful predictions about a lot of things, which turn out to be true rather than false, and if that theory also predicts the existence of a multiverse, then we should take that prediction seriously even though it can't be tested directly.

which is true enough, but doesn't address the fact that there is no such theory. The string theory landscape "prediction" of a multiverse is exactly the opposite sort of thing, not a corollary of successful predictions, but something being invoked as an excuse for failure to make predictions about anything at all.

For something more substantive, I recommend Alessandro Strumia's theory summary for Moriond 2015. It has a lot of interesting commentary about a range of phenomenological topics. On the multiverse and anthropics he takes a quite different point of view than Weinberg:

> Nobody talked about anthropics at Moriond 2015. This has an anthropic interpretation: Moriond is not in California. Clearly, social factors are playing a role, as always when experiments cannot set the issue. On one side, `having discovered the multiverse' is physically indistinguishable from `having pursued a failed unification program', but sounds much better. On the other side, future physicists could consider us as crazy for not having immediately accepted anthropic arguments.

He discusses "naturalness" extensively, including explaining why anthropics is no solution, since it doesn't explain an unnaturally small Higgs mass.

Comments

1. **Bernhard**
   May 1, 2015

   "Further, if we find some future theory that does make successful predictions about a lot of things, which turn out to be true rather than false, and if that theory also predicts the existence of whatever" , than yes, I would also take "whatever" seriously.

   About Moriond, I think there is also another reason. This is one of the most important conferences for HEP experimentalists. I guess talking about this embarrassing multiverse businesses is something better left for more stringy-focused conferences or at least for a conference where not too many people doing real science are around.

2. **JG**
   May 1, 2015

   "Weinberg isn't very optimistic about possible progress these days:"

   What about you?

3. **gadfly**
   May 1, 2015

   No good data -> No progress. I think we can safely say that the limits of what we can know about so called "fundamental" theories have been reached for the foreseeable future.

4. **Jacques**
   May 2, 2015

   Humans will discover the final theory this century. There is a good probability that the final theory is a theory that explains why there is only gravity and the standard model.

   The research options in this segment are not exhausted yet. There is an anthropic reason for it (-: this research segment is not attractive for researchers, as it promises few – or even no – novel particles or effects.

   The scenario might be the following: once the LHC will have shown to everybody that there is no BSM physics, the search for a final theory will be rapid, because on the one hand, the research options are not exhausted, and on the other hand, the options are not legion.

5. **Yatima**
   May 2, 2015

In other news Quanta Magazine has the second installment of the "looking at entanglement while thinking about spacetime" series: How Quantum Pairs Stitch Space-Time, which is hard to understand but which leads to Arxiv 1306.2164: "A Practical Introduction to Tensor Networks: Matrix Product States and Projected Entangled Pair States".

6. **TOE**
May 2, 2015

"The string theory landscape "prediction" of a multiverse is exactly the opposite sort of thing, not a corollary of successful predictions, but something being invoked as an excuse for failure to make predictions about anything at all."
Right, this is highly suggestive of the "sickness" of a theory. In fact many different theories of quantum gravity presently produce a multiverse. See https://indico.math.cnrs.fr/getFile.py/access?resId=4&materialId=slides&confId=782
page 21. (Very importantly, this list puts the string landscape into perspective).

7. **Bee**
May 2, 2015

It isn't true that finding a multiverse is the same as having failed at unification. It means you haven't succeeded in finding sufficiently many assumptions (axioms/constraints) that give you a unique unification. That isn't the same thing. String theory can well have the one unification they are looking for, just that they haven't found it.

I've explaint that in great detail elsewhere, but it really isn't so difficult to see that every theory has a multiverse if it's underspecified for exactly the reason that Weinberg points out: You cannot deduce a theory on purely mathematical reasoning. You need a set of postulates, and if you don't have sufficiently many, you get an ambiguous result, that's it. In the end, all these postulates must be emprically motivated (not counting mathematical consistency itself).

8. **Eduardo Lira**
May 2, 2015

How much did human beings know about SR in the last years of the 19th century? Zero, right? And about GR? Zero again. What about QM, not to mention QFT? Or the SM? All ridiculous questions, aren't they?

Well, considering such antecedents, and with another 85 years to go before this century's end it's quite absurd that someone (be it on this side of ST and the MV or the other) dares to assert that humankind has hit a dead end in the foreseeable future concerning the search for a "final theory" of physics. Nature's complexity is fully independent of human impatience. Or a physicist's wish to find definite answers during his or her lifetime.

9. **Art Brown**
May 2, 2015

So we're running out of water, Brooklyn is moving here *en masse* , and our physics is getting dis'ed by old Europe. Just another perfect day…

10. **JG**
May 2, 2015

My question wasn't rhetorical

What do think about possible progress?

11. **Peter Woit**
May 2, 2015

JG,
I'm not particularly optimistic, in some ways now even more pessimistic than ten years ago. Back then I would never have believed that multiverse nonsense would be so influential, and I hoped the LHC would discover something unexpected about the Higgs mechanism.

On the other hand, there's a much wider understanding that the string theory/SUSY paradigm hasn't worked out, and this may lead to more openness to looking for other ideas. There's a wide variety of activity in mathematics related to physics, and this may lead somewhere. I have no idea though on what time scale…

12. **Mesmar Djehha, alias EFT**
May 3, 2015

Dear Peter,
When you say "There's a wide variety of activity in mathematics related to physics, and this may lead somewhere. I have no idea though on what time scale…"
what if the time scale becomes longer than the one already taken by string theory, would you still believe that advanced mathematics would lead to a breakthrough in physics?
Thank you.

13. **Peter Woit**
May 3, 2015

Mesmar Djehha,
I don't think the problem with string theory is the time scale for progress, but the sign of the derivative. As more and more has been learned about string theory, it has become more and more clear that ideas for how to get unification out of it and connect to the standard model don't work (i.e. are empty, they answer no questions about the standard model).

On the other hand, my own personal experience is that as I have learned more and more mathematics, I see more evidence for deep connections between physics and mathematics. Maybe this is a mirage that will dissipate as more is learned about these subjects, maybe not.

Progress doesn't happen linearly, but in jumps when people have good ideas. The question is evaluating the prospects for good ideas solving certain problems. I'd claim the probability of finding a compelling string theory explanation for the SM has been decreasing for a long time, with the "landscape" just the claim that it has hit zero, but that's alright. I don't see any evidence of a comparable phenomenon if you try and evaluate prospects for the use of advanced mathematics in physics.

14. **Theo Nieuwenhuizen**
May 3, 2015

String theory is a consistent framework to consider questions about quantum field theory. Various insights are gained from it. It has drawbacks due to its ad-hoc

assumptions like the string nature of particles, extra dimensions etc. It has failed to recover, let alone to improve, the standard model. So the program is a failure for physics, but it can be a subject for mathematical physics.
The multiverse is a poor way of escaping to admit the failure, it is a clear sort of cheating since its claims lie out of the domain for which the theory was formulated. Incredible that people believe this fallacy.
Moreover, if we accept the multiverse, we must give up on falsifiability, but why then still do any science at all? We do have to keep separating science from religion. It is amazing to see that so many people who don't accept God do accept the multiverse. Humans do need religion, but why such an obviously failed one?

15. **Steve**
May 4, 2015

When I read Weinberg's comment about taking multiverses seriously I thought of his surprisingly sympathetic treatment of many worlds in his text "Lectures on Quantum Mechanics". He certainly doesn't endorse many worlds, but he gives it several pages of careful thought. Maybe he's thinking that kind of multiverse?

16. **Chris W.**
May 4, 2015

Steve,
I think Weinberg certainly appreciates the distinction, but he is trying to give careful consideration to both. In *Dreams of a Final Theory* he warns against dismissing a possible way forward in physics on the grounds that it seems unacceptable on *a priori* philosophical grounds.

Of course, that begs the question of why the multiverse should still be considered a possible way forward…

17. **MikeS**
May 5, 2015

A final theory this century?

A final theory, one assumes, has to be so logically compelling and self-consistent that things could be no other way. I would suggest we have a difficult conceiving how such a theory might even be framed, which rather gives serious difficulty in finding what it actually is.

Something looking similar to the Standard Model is far too arbitrary to fit the bill. General Relativity, with its geometric elegance, may give a mere glimmer of what such a theory might look like – but then again maybe not.

18. **JFD**
May 5, 2015

Peter,
I find it hard to believe that you recommend Strummia's comments as "more substantive". I'll bet that it is only because the comments reinforce your preconceived biases. He clearly says "In my opinion.." and then, amazingly, "The most likely outcome should have been…" as if he has special insight into the laws of Nature. There is not much of substance there.
There is a demonstrable need for the weak scale (quark and gauge boson masses) and the QCD scale (the nucleon mass) to essentially overlap – this produces the complex nuclei and atoms that we see. It is a valid scientific question whether this is a consequence of Nature having a unique ground state which luckily has this property, or a multitude of ground states sufficient to invoke anthropic selection. This may be difficult to settle any time soon, but it will not be decided by mere opinions.
I enjoy your BS detector, which is great for puncturing a lot of the hype that is out there. But "Naturalness" has been the dominant form of hype for decades, and has made clear predictions that have so far failed. Lets hope that the LHC offers us a reasonably definitive answer – finding "natural" new physics would be awesome. But if naturalness fails, it provides extra motivation to continue to explore theories with multiple ground states.

19. **Peter Woit**
May 5, 2015

JFD,

Not sure if it qualifies as more than an opinion, but to me it seems there are no solid arguments about what Higgs mass is likely or unlikely, and that seems to put me in agreement with Strumia. This is often stated as a "measure problem", but it seems to me worse than that: you don't even know what space the measure you can't calculate is a measure on (what's the space of theories you are putting a measure on?)

Sure, maybe a better theory has many vacua with different physics, but this postulate isn't worth anything unless it manages to explain something, not just provide an excuse. I think what Strumia was reacting against was the point of view pushed by Arkani-Hamed that the only two possible outcomes from the LHC are vindicating one of two ideas ("naturalness" or "multiverse"), both of which are popular, but neither of which actually explains much at all.
Glad you enjoy the BS detector!

20. **Just Another Reader**
May 11, 2015

"… if we find some future theory that does make successful predictions about a lot of things, which turn out to be true rather than false, and if that theory also predicts the existence of a multiverse…"
You go on to say that he "doesn't address the fact that there is no such theory". But would you not agree that your criticism arises as a result of a misread of what Weinberg said. He said "*if* we find…"—a conditional that surely concedes there is no theory at present. So perhaps your criticism of the Weinberg is, in this case, unfair. Or rather unneeded.

21. **Peter Woit**
May 11, 2015

Just Another Reader,

I don't think it's a misreading, note that I describe what Weinberg says as "true enough". The conditional "if" though evades addressing the difficult question, which Horgan was explicitly asking, of evaluating claims being made about the "string landscape", in favor of saying something obvious about a hypothetical circumstance that is not the case.

I'd be very curious to hear Weinberg address explicitly the "dead end" question: does our current best research indicate that the string landscape is an indication of failure and that the subject is at a dead end, or are there serious indications it's a plausible picture of reality that is our "best hope for a unified theory" as string theorists often claim these days?

Einstein’s Dice and Schrödinger’s Cat

May 2, 2015
Categories: Book Reviews

I’ve written a review for the Wall Street Journal of Paul Halpern’s new book
Einstein’s Dice and Schrödinger’s Cat (It’s here, unfortunately now behind a paywall).
I liked the book quite a bit, and learned many things about some history I already thought I knew well. The most dramatic section of the book is the story of the 1947 trouble between Schrodinger and Einstein caused by Schrödinger’s publicity campaign for a supposed breakthrough in the search for a unified theory, and Halpern writes about that here
.

The title of the book emphasizes their misgivings about the probabilistic interpretation of quantum mechanics, and describes how Schrödinger’s cat arose out of discussions with Einstein. More of the book though is actually about their efforts to generalize GR and find a unified geometrical theory of gravity and electromagnetism. This began almost as soon as the field equations for GR were in place (1915). The book’s stories of media hype for bad ideas, involving physicists given rock-star academic positions at institutes set up for them make clear that some contemporary problems go back much further than I’d ever realized.

Einstein’s later work is, for good reason, dismissed as misguided, since it ignored quantum theory. He and Schrödinger did however have good reasons for skepticism about the Copenhagen interpretation of quantum mechanics. The measurement problem has turned out to be a very subtle one, with the cat experiment a very good way of making clear the problem. Their enthusiasm for ideas about unification that weren’t working was also way ahead of their time…

Update: For some other reviews of the book, see Jennifer Ouellette in the New York Times, and Denis Weaire in Physics World.

Comments

1. **cthulhu**
   May 2, 2015

   Have to add this one to my list…FYI, if you search with Google for the exact title of a WSJ article, you get a link that bypasses the paywall.

2. **Peter Woit**
   May 2, 2015

   cthulhu,
   Thanks. It does seem that the Google link behaves differently.

3. **Richard Gaylord**
   May 3, 2015

   this is IMO a really terrible book unless you’re into reading about the randiness of Einstein and Schrodinger. the scientific content is close to zero. it was a major dissappointment to me since i thought his “The Great Beyond” was quite good.

4. **Peter Woit**
   May 3, 2015

   Richard Gaylord,

   I strongly disagree. There’s relatively little in the book about Einstein’s romantic life, and Schrodinger’s is a significant part of the story, since one of the main reasons he ended up in Dublin instead of Oxford or Princeton is that he essentially had two wives.

   It’s true there’s little in the book about the details of the generalizations of GR that Einstein and Schrodinger were working on. That’s a very technical subject, and those ideas don’t seem to go anywhere, so it’s not an unreasonable choice to not focus on it.

5. **john McAllison**
   May 3, 2015

   Peter,

   in your review you say:

   “While most of their colleagues were moving on to the study of such quantum-field theories, the two of them resisted, feeling to different degrees that such theories were likely inherently flawed.”

   I can’t help feeling that this sounds a little like you with your attitude towards String Theory 😉 Nowadays, any respectable graduate course in HEP will include this.

6. **Peter Woit**
   May 3, 2015

   John McAllison,
   I think there are a few obvious differences between me and Einstein/Schrodinger (as well as between QED and string theory…).

7. **Kenneth Vatz**
   May 3, 2015

   First I read the review in today’s Times Book Review by the “popular science” writer, Jennifer Ouellette, who says there is little that’s new in the book. Then I read your review in the WSJ (by Googling it, as described), and decided I would get it.

   But what really influenced me, as a lay reader, to ignore Ouellette altogether was this excerpt from the end of her review:

   “Mainstream physics left [Einstein] behind as the Standard Model of particle physics took shape, and the mathematical approaches once explored by Einstein and Schrödinger have long since given way to string theory and loop quantum gravity, two of the most promising candidates for quantum gravity.”

I’m not sure if Ouellette reads your blog.

8. **Peter Woit**
May 3, 2015

Kenneth Vatz,
I’ve added a link to the New York Times review and to one at Physics World.

I don’t actually disagree all that much with Ouellette’s review. It’s a fact that much of the story of Einstein and Schrodinger is “well-traveled historical ground”, and it’s very hard for anyone to come up with a new take on it. Halpern’s decision to focus on their interactions in their later years is a new take, and that’s where he has new material. For the rest of the story, there are many other better sources, ones it’s hard for anyone to compete with.

I’m pretty sure Ouellette has heard of my point of view on string theory (maybe she was even in the audience when Sean Carroll and I debated this once..). Saying that the failed ideas of Einstein and Schrodinger have been replaced by string theory etc. doesn’t necessarily make much of a positive claim for string theory, even with the “promising”. If you read Halpern’s book, you’ll see that he also probably has a more positive take on string theory than I do.

9. **Anonymous**
May 4, 2015

My understanding of Einstein’s motivations (which might well be wrong) is that he was trying to obtain a theory from which quantum mechanics would naturally emerge, and so “quantization” would not be the starting point of such a theory.

From the standard biography by Pais :

“He was looking for a unified field theory, but to him that concept meant something different from what it meant and means to everyone else. He demanded that the theory shall be strictly causal that it shall unify gravitation and electromagnetism, that the particles of physics shall emerge as special solutions of the general field equations, and that the quantum postulates shall be a consequence of the general field quations.”
[p. 465 in the OUP edition]

In addition, he had a favoured mathematical idea for reaching this end : overdetermination of systems of partial differential equations [mentioned in Pais].

From another (excellent) articulation of Einstein’s position :

https://www.marxists.org/reference/archive/einstein/works/1940s/reply.htm

“Assuming the success of efforts to accomplish a complete physical description, the statistical quantum theory would, within the framework of future physics, take an approximately analogous position to the statistical mechanics within the framework of classical mechanics. I am rather firmly convinced that the development of theoretical physics will be of this type; but the path will be lengthy and difficult.”

So he had a fairly clear and not entirely unreasonable notion of what he was looking for, and how he would go about it. It’s just that he didn’t succeed.

And neither String Theory, nor Loop Quantum Gravity are – in this sense – attempts towards fulfilling Einstein’s “dream”.

I can stand corrected on anything I’ve said here.

10. **Phil Harmsworth**
May 4, 2015

Richard Gaylord,
If you’re interested in the technical details, the Pais biography of Einstein provides an overview.

Pais also explains that, far from ‘ignoring quantum theory’, as Peter stated, Einstein initially thought it was possible to derive its features without invoking probabilities at a fundamental level. As Pais makes clear, to some extent he wavered in this conviction towards the end of his life.

11. **Richard Gaylord**
May 4, 2015

sorry for sendingg so many comments but i wanted to add the following:

Einstein quote: “I consider it quite possible that physics cannot be based on the field concept, that is, on continuous structures. Then nothing remains of my entire castle in the sky, including the theory of gravitation, but also nothing of the rest of modern physics.” (In a letter to a friend in 1954, the year before he died.)

and to get a good understanding of Einstein’s research program in his post GR period, see the book “Einstein’s Unification”.

Also, recently there have been a number of very interesting articles on arXiv (history and philosphy of physics) describing Einstein’s methodology in seeking a cosmological model based on GR.

finally, i’d like to read a discussion of the causal net approach to spacetime proposed by Raphael Sorkin and taken up (without attribution) and modified by Stephen Wolfram who considers the concept of spacetime itself to be misguided.

12. **Michael Gogins**
May 4, 2015

I found the Peter Woit review absorbing and it interested me in the book.

Regards,
Mike

13. **Oldster**
May 4, 2015

Anonymous: I think you covered your points pretty well. I was never sure what Einstein thought overdetermined PDE’s would quantize however. Somehow, I got the impression he might have been thinking more about quantization of charge or such there, but that’s merely my speculation, not something I recall him writing down. But the quote you gave from Pais may be consistent with that …

14. **Cormac**
May 7, 2015

Good review Peter, well written and interesting. Given their kerfuffle concerning the possible theft of ideas in the 1940s, I find it surprising that Einstein never pointed out that the Schroedinger cat paradox was an extension of his gunpowder argument, outlined in a letter to Schroedinger…does the author touch on this?

I hope the author mentions that Schrodeinger wrote two excellent early books on GR and cosmology, this is not widely known.
Fun fact. Did you know that Schroedinger published a short paper on relativistic cosmology way back in 1918, asking this: why not put the cosmic constant on the RHS of the field equations? And why a constant, might it not be a variable? [Yes, he really did suggest this. Einstein's reply was that, if constant, putting the cc on the RHS made no difference (not quite true): if not constant, one would have to hypothesize a time variation and he had "no wish to enter this thicket of hypotheses" !
Such little titbits make historical books interesting..

First Collisions of Run 2

May 5, 2015
Categories: Experimental HEP News

This morning in Geneva saw the first collisions in the revamped LHC, there's an event display from ATLAS here, CMS here.

These collisions are just at the injection energy of 450 GeV/beam, but over the past few weeks beams have been successfully ramped up (without intentional collisions, although see page 23 here) to the planned 6.5 TeV/beam. 6.5 TeV beams have been accelerated not just with probe intensity, but with nominal bunch intensity and squeeze. They are roughly halfway through a planned 8 week beam commissioning process and on schedule, with stable beams for physics collisions still planned for around June 1. For quite a while now, 6.5 TeV/beam collisions have been possible, but no word yet on when this is planned for, I assume there will be some sort of media event at that time.

Update: Something about this from CERN here.

No Comments

The Admiral of the String Theory Wars

May 7, 2015
Categories: Uncategorized

The science magazine *Nautilus* this month has a profile of me, under the title [The Admiral of the String Theory Wars](). The writer, Bob Henderson, spent a lot of time talking to me and other people around here, including attending my class. I think he did a very good job of handling the complex topic of the debate over string theory, not so easily done in the context of this kind of personal story. Then again, I guess I would think that since he was mainly hearing my point of view, others may see this very differently…

Talking to Henderson brought back memories of a lot of the very odd things that happened during this period, now nearly ten years ago. I also recently realized that this week is the 10th anniversary of the date on which I sent off the final version of "Not Even Wrong" to the publisher (it then went to copy editing, was published a year later). Looking back at it now, I don't see much that I'd change. The earliest version of the manuscript written around 2003 didn't discuss the landscape and multiverse business, which only really got going in 2004. By 2005 I did add a chapter on this, but at the time thought doing this might be a bit unfair to string theorists. Surely they were all aware this was obvious nonsense and would quickly themselves put a stop to it. I was very wrong about that and, oddly, it's over this argument (where I would have thought my point of view was uncontroversial) that I've gotten the most grief from certain prominent string theorists (e.g. Polchinski).

For the latest in the string wars, you can watch last night's Perimeter Institute [public lecture by Amanda Peet](). It was a promotional effort that reminded me a lot of the kind of thing you see on late night TV, with an inspirational speaker selling a product to a rapt studio audience. The main selling points for string theory were that it "would blow your mind", that branes explain black hole entropy (the Strominger/Vafa calculations of 20 years ago), and that the world is a hologram (Maldacena from nearly 20 years ago).

After the talk the first question read to her was the obvious one about testing these ideas experimentally. Remarkably, she claimed that string theory was testable, that published work of 10 years ago showed that it could be tested at accelerators, and so far it had passed the tests. What she was referring to was one of the weirder stories of the string wars. At the time Jacques Distler and collaborators wrote a paper entitled *Falsifying String Theory Through WW Scattering*, which I discussed [here](). They also wrote to the Wall Street Journal about this (see [here]()). A few months later their paper was accepted by Physical Review Letters, with their claims about string theory removed, I assume at the insistence of a referee (see [here]()). In a spectacular display of chutzpah, when the PRL paper appeared, the authors had their institutions put out press releases claiming that "Physicists Develop Test for String Theory" (see [here]()), exactly the claim that PRL would not let them make in print. It's this claim that Peet is now using to mislead the public at Perimeter. I think it's their responsibility to look into this and issue a public correction. Or do they really feel that it is all right for their public lecture series to be used to mislead the public about science?

Update: I see that the Nautilus piece is part of [an issue on "Error"](), which also includes interesting articles about the [OPERA superluminal neutrino story]() and about [Vladimir Voevodsky]().

Update: I probably should have included the relevant question and answer exchange with Peet, here it is:

> **Q**: Any predictions or comments on how and when string theory might be able to be proven experimentally?
>
> **A**:… there are experiments that can be done, this is published work that has been around for almost ten years now, that you can do experiments in nature in the lab without having to invent equipment that you haven't invented yet that would test whether or not the assumptions of string theory are true or false. So far none of those experiments has shown a red flag that says string theory is wrong.

Update: Ethan Siegel has a [blog entry]() and [live-blogging about the Peet talk](). He was hoping that Peet would discuss some sort of connection between string theory and observable physics, didn't hear any (he seems to have missed the bogus claim about the LHC). Siegel gives an argument that string theory is in principle falsifiable, since it predicts space-time supersymmetry superpartners, although with no prediction for the mass scale. I've never heard this before, and I don't know of any argument that such superpartners must appear in the spectrum of a string theory.

Comments

1. **nicola**
   May 7, 2015

   About "It's this claim that Peet is now using to mislead the public at Perimeter".

   To be honest, at the question session at 1.07.50, she says that "the **assumptions** of ST are falsifiable".

   That statement is in fact true because we can in principle test the assumptions of any theory if only we had enough energy to test the Planck scale. So, that statement while true is completely useless. The point is not about assumptions but about any falsifiable predictions (that was the question from somebody from twitter). In particular the LHC-predictions (I suppose). I often hear/read from string theorists that they confuse assumptions with predictions. Maybe one should make it clear once and for all that the assumption that particles are extended objects is not a prediction?

   She then refers to that PRL paper but still talks about the ST assumptions. In fact she is not answering the question.

   Too bad there wasn't, in the audience, any expert in the field to point out that she is in fact manipulating/misleading the public. Because indeed her answer sounds as if so far ST fulfilled all the experimental tests. But in fact there were no experimental tests of ST so far – to do that you need a prediction first. She "forgot" to add that so far ST made no predictions but merely succeeded in fitting the already obtained experimental results. She also forgot to add that there are so many compactification schemes in ST that they can fit (almost) whatever they want.

2. **perry**
   May 7, 2015

   Adm. Woit "it's a trap"

3. **zzz**
   May 7, 2015

   I am the monarch of the sea,
   A critic of the theory stringy,
   Whose praise Nautilus loudly chants…

4. **Tom**
May 7, 2015

Be very wary when advocates claim the "science is settled" or that "xx% of the 'experts' have 'consensus' "

re the Nautilus interview in which
"He [Woit] is called an 'incompetent, power-thirsty … moron' and a 'stuttering crackpot-in-chief ' " ,
Falling back onto ad hominem attacks is a sure sign of scoundrels who have no data to justify their beliefs or opinions. You'll find this behavior in almost every human endeavor (public policy, economics, science, medicine, etc).

5. **KenW**
May 7, 2015

I've known and worked for a lot of admirals – some good and some not. Henderson seems pretty fair, so I wonder if this is a case where the editor wrote the title rather than the author. It seems like a dose of God Particle sensationalism. As for Dr. Peet, she is a gifted lecturer for a lay audience. She sounds very confident about her subject. She sounds as though she is presenting settled history.

6. **Anon**
May 7, 2015

Peter, You have been mentioned on page 8 of the pdf (slides) of the talk Where is Susy? by Howard Baer at Pheno 2015.

Where's Where is SUSY? — https://indico.cern.ch/event/364031/session/35/contribution/182

7. **Peter Woit**
May 7, 2015

Anon,
Thanks. The arguments he's objecting to though (about how massive gluinos and squarks can be and not violate naturalness) are from Arkani-Hamed and others, I was just reporting them…

8. **Bernhard**
May 7, 2015

nicola,

If we could reach the Planck scale there would still be no unambiguous way to test ST because non-perturbative ST is unknown (see the FAQ). In any case, this is an really old discussion, one that I'm sure Peter is not exactly dying to moderate…

9. **Peter Woit**
May 7, 2015

nicola,

Besides the point Bernhard makes about this, if you look at the links where I discussed the Distler et al paper the main point was that the assumptions they claim to test are standard QFT assumptions, not assumptions that have anything to do with strings or string theory. That's why PRL made them remove the claims about string theory from the paper.

That paper was clearly written exactly to provide someone like Peet the argument she made. But it's a fact that PRL would not allow that argument to be published, and her claim that it is in a published paper is provably false. Again, I think it's the responsibility of Perimeter to take some action to correct the false information put out in their public lecture.

10. **CIP**
May 7, 2015

Admiral or the admirable kid who pointed out the Emperor's unclothed state?

11. **Thomas Larsson**
May 8, 2015

String theory is not completely void of predictive power. In fact, I have used string theory to make a [falsifiable but confirmed prediction about the LHC](). I'm particularly proud that I already in 2007 could predict that poor Lubos would lose his experimental-susy-by-2006 bet.

12. **Mesmar Djehha**
May 8, 2015

In my opinion, Bob Henderson's article could be a beautiful epilogue to any new edition of Peter's book "' Not Even Wrong".

13. **nicola**
May 8, 2015

Bernhard,

at Planck scale you would surely test the ST basic assumptions e.g. if particles are indeed stringy. Testing the dynamics and its consequences is another thing.
I am trying to return to old discussions.

Peter,

I perfectly understand what you say. However she seems to refer to the assumptions, not predictions. Maybe she means predictions all the time but says assumptions or maybe she does it on purpose.

14. **Bernhard**
May 8, 2015

nicola,

The point is falsifiability. Of course, if would see stringy behavior at the Planck scale this would certainly be strong evidence for string theory. But since you don't know the theory non-perturbative behavior not observing it does NOT falsify the theory because stringy behavior is not a necessity. The point is that string theory would still be immune to this test, just as it is immune to the non-SUSY at the LHC test.

"I am trying to return to old discussions. "
A time-machine to travel to 10 years ago is probably your best option.

15. **Peter Woit**
May 8, 2015

nicola,
She is making the same argument as the Distler et al paper which claims to derive "predictions" of string theory testable at the LHC based on "string theory assumptions" about high energy behavior, and explicitly referring to the paper, vetted as a "published" paper. Again, the problem here is that this claim about string theory was removed in the published version since it the "assumptions" are qft assumptions not string theory assumptions.

About the "string theory predicts you see strings at high energy" argument, it's not the one she is making. If you look at the Nautilus article, you'll see that Kleban tries to make it, but Michael Dine, a string theorist expert on this, disagrees.

16. **Anonyrat**
May 8, 2015

The Admiral that sank the ~~Spanish~~ String Theory Armada!

17. **nicola**
May 9, 2015

sorry I meant "I am not trying…", interesting mistake.

18. **Chris Oakley**
May 9, 2015

Sir (I suppose I have to address you like that now you're an Admiral),

Mr. Henderson needs to be congratulated on not mentioning he-who-shall-not-be-named by name, although I see the name crops up in the comment section. Speaking as one of the early followers of the blog (a select group of – according to he-who-shall-not-named – anti-science crackpot losers), I would say that the article is pretty fair, and I am glad that he pointed out the considerable mission creep that has resulted in your blog being one of the prime sources of fundamental physics news irrespective of your views on String Theory. I suppose that there are lot of String Theorists out there who do not appreciate the considerable amount of unpaid work that goes into it, but I do.

19. **Edgar**
May 9, 2015

"A few months later their paper was accepted by Physical Review Letters, with their claims about string theory removed, I assume at the insistence of a referee."

It is pretty obvious the change in title refers to a large class of possible 'new physics' scenarios at the LHC, not a request from a referee. (This also had to do with the re-doing of the analysis.) The beauty of the relationship discussed in the paper (originally studied by Adams et al. and others in the context of QCD) is rooted in the UV/IR connection: bounds on EFT coefs. that may be derived assuming basic properties of the high-energy theory. For instance, claims have appeared in the literature, e.g. breaking of Lorentz invariance and quantum gravity, which may be then confronted with data (even if somehow hidden in direct low-energy experiments, e.g. due to new symmetries such as SUSY). The existence of a light Higgs changes the tone of the paper, but the premises remain.
The search for UV/IR connections is key to relate measurements we can perform to theories for which we may not have direct access to. This is even more transparent in cosmology, e.g. 1502.07304. Unfortunately you seem to have failed to appreciate the basic idea(s).

20. **Peter Woit**
May 9, 2015

"Edgar",
So, your claim is that the change in title and wording of the paper was because the reaction from the journal was that the "we have found a way to test string theory at the LHC" was too modest? That's very funny. And your claim that the problem here is that I'm just too ignorant to appreciate the significance of this work does bring back the glory days of the string theory wars. As does the hiding behind anonymity to make arguments you're too embarrassed to attach your name to…

It seems your point of view is that the real problem here is my raising this issue, not Amanda Peet's telling the public that Distler et al shows that the LHC can test string theory (as well as claiming that it already has done this, successfully). Do you have any problem with her doing that?

21. **Guest from Norway**
May 9, 2015

Thanks to Woit for his blog. I have read the blog for many years now. Here is a voice from Norway that you may find nice😊

https://m.youtube.com/watch?v=eCuE7UOP9gw

22. **Edgar**
May 9, 2015

listen to what she says regarding this point, carefully:

http://www.youtube.com/watch?v=MlDd2HtFfPU&t=68m0s

"experiments […] that would test whether or not the assumptions of string theory are true or false"

This is factually correct. The case of Lorentz invariance for example has been raised many times when the subject of Quantum Gravity has been brought upon, e.g.1106.1417 .
The type of sum rules derived by Distler et al. (and others) does this precisely.

Again, there are measurements (in cosmology) of various kinds which may open a window to physics at high energies, e.g. 1109.0292.

I think this is a more fruitful discussion that nitpicking some specific wording. Unfortunately you stop when the fun/physics begins!

Didn't occur to you the paper by Distler et al. may have been submitted for the first time with its given title already?

23. **Peter Woit**
May 9, 2015

Edgar,

For more context, here's the question that Peet was asked, followed by the relevant extract from her answer. She is clearly referring to the Distler et al claims that string theory can be tested at the LHC, and claiming that so far it has passed the tests.

Q: Any predictions or comments on how and when string theory might be able to be proven experimentally?

A:… there are experiments that can be done, this is published work that has been around for almost ten years now, that you can do experiments in nature in the lab without having to invent equipment that you haven't invented yet that would test whether or not the assumptions of string theory are true or false. So far none of those experiments has shown a red flag that says string theory is wrong.

Do you really think that is an honest answer to the question? If it isn't, why are you writing here to criticize my complaint about it, rather than criticizing her behavior. Is it honest to tell the public that the LHC has successfully tested string theory?

"Factually correct" is dishonest weaseling. It would be factually correct to say that the LHC has tested the "assumptions" of string theory, having in mind that string theory assumes usual mathematical axioms. That was the problem with the Distler et al paper (and press releases). Their assumptions were standard QFT assumptions, having nothing to do with string theory (the day someone finds a violation of Lorentz invariance, do you really believe the reaction of string theorists will be to abandon string theory rather than look at string theory models that can accomodate this?).

You're correct that I don't know the details of the publication process of that paper. It sounds like you do and maybe you can enlighten us on what happened between the May and October versions, although your suggestion that the authors changed the title in order to strengthen the claims of the paper doesn't really pass the laugh test. All I do know is that the preprint carried the bogus title, which I wrote about, the final published version had it removed, then the authors put out press releases using it.

24. **Dundee**
May 9, 2015

You seem to be too focused on small players in string theory while sparing the big names. In addition you do not report on positive developments such as the workshop that took place at the Simons center this week with diverse topics. One should keep a positive attitude and not to be critical most of the time.

25. **Peter Woit**
May 9, 2015

Dundee,
I think I've done more than my part as far as offending big names in string theory…

Happy when there's something positive to report, I'm afraid I can't agree about the Simons Center talks (other than the Seiberg one, which was much the same as similar talks of his I've discussed here before).

26. **Dundee**
May 10, 2015

Peter: To spend half a post about a talk by Amanda Peet is pointless. Her work for the last ten years have little impact on the string community and as such her talk does not represent them. It is clear that you did not listen to the Simons meeting talks, but only glanced at the titles. Some of the talks like those of Damour and Sunayev, among others, show that there are still serious people who do excellent work without making noise.

27. **Peter Woit**
May 10, 2015

Dundee,
I wasn't discussing Amanda Peet's own work, but her high profile public talk. You say her public talk doesn't represent the string theory community, but she was specifically chosen by Perimeter for this purpose (presumably with the participation of the string theorists at Perimeter and who advise Perimeter). Will any string theorists join me in complaining to the people at Perimeter who made this choice, and asking that the institution post a correction concerning the bogus claims she made in her talk? I've been considering doing this, but it shouldn't be me, it is string theorists who should be contacting them.

There's by now a long tradition of fanatics from the string theory community getting a lot of attention for ridiculous claims for the theory. I can't think of one example of a string theorist ever publicly raising an objection to any of this.

The Simons Center talks you mention weren't ones I found compelling, but, tastes differ.

28. **Tammie Lee Haynes**
May 11, 2015

Dear Dr Woit.

The String Theorists at Perimeter may be unaware of what Dr. Peet stated.
Even if they were in attendance, their attention may have been distracted at the time she made her remarkable claims.

In fairness to them, I suggest you let them know of the problem, before criticizing them for not correcting it.

29. **Yi-Zen Chu**
May 11, 2015

Thank you for linking to the article on Vladimir Voevodsky, Peter.

This resonated with me because it is a scientific issue I've always been concerned about — how careful theoretical physicists are in their own work, and how much effort is actually put into checking each other's work. As with most human behavior, I believe the incentives need to be there to encourage this sort of scrupulousness. However, the terrible academic job market, declining government support for fundamental research, pressure to publish O[10] papers every year, and the (usually) sub-par refereeing process in journals, all point towards the lack of such an incentive structure.

Voevodsky himself has responded by advocating the use of a "proof assistant". I'm curious, how else has the Mathematics community at large responded? Are there any adjustments being made?

What about theoretical physics? I often wonder if any established theorists share my concern? (I am currently a mere postdoc and have no power whatsoever.) Much more importantly, is there any concrete effort exerted at all to maintain a high standard of intellectual inquiry in theoretical physics?

30. **Peter Woit**
May 11, 2015

Yi-Zen Chu,
I don't personally know anyone in mathematics now using a "proof assistant" in their work (or at least if they are I'm not aware that they're doing this). It is a huge effort to put a proof in a form that a computer can check, and I'm not even sure that it is possible for lots of areas of mathematics research. One question that came to mind when reading the Voevodsky article was whether the work of his that had an error was actually of the sort that a "proof assistant" could have been used to catch it.

Questions of the reliability of the theoretical physics literature I think are quite different, since there rarely is anything with the level of rigor of a "proof", so even in principle a "proof assistant" would be of no use.

31. **Peter Woit**
May 11, 2015

Tammie Lee Haynes,

The string theorists at Perimeter know Amanda Peet well, so I doubt are surprised she would do this kind of thing. If I thought there was any chance they or Perimeter would do anything about this, I would pursue the matter with them, but there are limits to how much time I want to waste on this, and writing the blog entry probably already went over the limit.

This blog is very widely read by string theorists, so quite a few now know about the Peet issue. If they care about the credibility of their field, they are the ones who should be doing something.

32. **Jeff M**
May 11, 2015

Yi Zen and Peter,

Let me put my 2 cents in as a mathematician on the proof/refereeing question. First, Peter is right most proofs are essentially impossible to check via computer, and I'm guessing they will be for quite some time. There are exceptions, like Hales proof of the Kepler conjecture. But that's a very special case, and the proof would be impossible without computers, it's nothing resembling a "normal" proof. As for refereeing, I think it varies, a lot. I think most people take it seriously, but it's easy enough to miss things. A recent paper of mine had a mistake, not in any of the important parts, just a minor calculation at the end. My co-author and I missed it, basically out of lazy stupidity, we were done with all the big stuff and weren't paying attention. I'm guessing the referee missed it for the same reason. In that case it didn't matter, just meant that our estimate for the shortest geodesic was 2.3 and it should have been 2.4. But bigger mistakes have happened. Theres a well known case from the 70's, where a big time paper was published proving that the bottom of the spectrum of the Laplacian on surfaces was 1/4 (big big deal) – only thing was, it's not true. Caught pretty quickly. But it happens. And I know of people taking advantage of this. I remember a conversation with Peter Sarnak about some people who were claiming a big result, and he read the preprint and explained to them it was incorrect. So did several other people. They still claimed it was right, and kept sending it to journals until someone finally published it.

33. **Kevin NYC**
May 11, 2015

I am the monarch of the sea,
A critic of the theory stringy,
Whose praise Nautilus loudly chants…

I rule over all the landscapes
that give multiple answers freely,
And find them most unseemly..

34. **Yi-Zen Chu**
May 11, 2015

Thanks for the response, Peter.

My questions were meant to be of a more general nature, not just specifically about whether mathematicians are now using a "proof assistant"/computer to check their logic. Perhaps let me give 2 basic examples, to illustrate what I was getting at. It's very common, nowadays, for theoretical physics papers to contain multiple authors. Most journals do not require authors to describe who contributed what to the research itself. The question here is, do the authors of a given paper even cross-check each other's work? When physicists write "reviews" of a particular sub-field, they probably do not check — and perhaps do not even read thoroughly! — the papers they cite. How reliable then is the body of work we call theoretical physics, especially given the current climate of an over-saturated job market?

I believe Voevodsky himself, in the article you linked, raised issues of similar spirit — the temptation of publishing a sloppy proof because of the fear of competition, and the consequence of having students learn from their advisers that doing so is alright, etc. I thought this latter point regarding students learning what is OK to do is quite a good one; academia is strongly driven by internal social dynamics and what students assimilate as the "norm" is going to set the climate, and therefore the intellectual standards, for the next generation of scientists.

Jeff M — I was told referees in Mathematics actually step through the proofs in a given paper, so once it passes peer review there is high chance the results are correct? This is one of the reasons why I've always held mathematicians in high intellectual regard. Your final story has tarnished that image slightly…

35. **Jeff M**
May 11, 2015

Sorry to tarnish the image, but it does happen. Not often. If you referee something you are supposed to go through it carefully, and if you cannot confirm it you should ask the editor to get the author(s) to explain. Most things in any good math journal are essentially correct, as in the major results are correct. And anything that isn't correct is discovered quite quickly, unless it's very obscure. So you can assume most everything you see in good math journals is OK. Physics is different, "correct" should mean "agrees with experiment" and "is mathematically consistent" which is the best you'll ever do. Peter's issue, and Lee Smolin's, and all sorts of other people at this point, is that "correct" no longer has anything to do with the first, and often not the second either. Well, perhaps that's not quite fair to string theory, it "agrees with experiment" in the sense that some version of it agrees with any experiment you care to do, in any universe you happen to live in 🙂

36. **Peter Woit**
May 11, 2015

Yi-Zen (and others),
Sorry, but enough about this. It seems you want to discuss an immense topic (problems with the physics literature and scientific literature in general) that has nothing at all to do with the topic of this posting.

37. **paddy**
May 11, 2015

Tho the discussion was off topic….twas also interesting to us listeners.

38. **Peter Woit**
May 11, 2015

paddy,
Unfortunately not interesting enough this late this evening to the person who would have to sensibly moderate such a discussion…

39. **Peter**
May 12, 2015

@Yi-Zen Chu
The "sloppiness" in higher category theory is of conceptually different nature than the one in theoretical physics. For an example of the kind of things done by a "proof assistant" you can check Carlos Simpson's paper math/0506471 ("Explaining Gabriel-Zisman localization to the computer"). But the topic of "stability" of results in math and how deep you have to go into someone else's proof, etc. is huge.

40. **Jeff M**
May 12, 2015

Sorry to go off topic Peter, will be a good boy from now on…

About That Smoking Gun…

May 13, 2015
Categories: Multiverse Mania

Aeon magazine has just published a long piece on the current state of cosmology by Ross Andersen. One focus is on Paul Steinhardt and his claims that the popular multiverse/eternal inflation scenario doesn't explain what it is supposed to, and is compatible with almost any experimental result. The BICEP2 fiasco, where multiverse proponents first claimed a "smoking gun" vindication from B-modes, then went on to claim that no B-modes was just as good for their theory once they disappeared, is the main topic of the article (for a previous posting about this, see here).

> That's why Steinhardt was surprised to see inflationary theorists clinking glasses when BICEP2 announced a high swirls figure. 'They declared victory,' he told me. 'They said it was smoking-gun proof! Just what they expected!'
>
> But then a few months passed and BICEP2's interpretation started to look wobbly. In June, Linde told New Scientist that he didn't like the way BICEP2's swirls were being treated as a smoking gun for inflation. In July, Guth made similar statements to the Washington Post. Steinhardt was furious. He thought it was flip-flopping. He began to wonder if any data would disturb the serene certainty of inflationary theorists. 'It was Andre Linde who used the "smoking gun" language in the first place,' he told me. 'Now he says it doesn't make a difference what BICEP2 says. How can it be that not seeing gravitational waves is fine, and then seeing them is a smoking gun, and then not seeing them is fine again?'

Steinhardt explains that the underlying problem is that the underlying problem is an inherently untestable paradigm, compatible with anything:

> The theory's weaknesses can be explained away with the same glib shrug that accompanies the retort: 'God just made it that way.'
>
> A dominant, infinitely flexible multiverse theory could make it easy not to strain for the next leap forward. It could lead to a chilling effect on new ideas in cosmology, or worse, a creative crisis. Steinhardt thinks we're already there. 'Andre Linde has become associated with eternal inflation because he thinks the multiverse is a good idea,' he told me. 'But I invented it, too, and I think it's a horrible idea. It's an emperor's new clothes story. Except in that story, it's a child who points out that the Emperor has no clothes. In this case, it's the tailors themselves telling us that the theory is not testable. It's Guth and Linde.'

His hope for how the subject will get saved from itself is with help from philosophers:

> 'The outside community isn't recognising the problem,' he said. 'This whole BICEP2 thing has made some people more aware of it. It's been nice to have that aired out. But most people give us too much respect. They think we know what we're doing. They take too seriously these voices that say inflation is established theory.'
>
> I asked him who might help. What cavalry was he calling for?
>
> 'I wish the philosophers would get involved,' he said.

That might help, but I think other ideas are needed…

Comments

1. **Theo Nieuwenhuizen**
   May 13, 2015

   I can't help for longing for the 1930's, even if it was before my time. People were very cautious with new claims. Pauli felt bad for proposing a new particle – the neutrino.
   Nowadays, if you don't propose a whole new sector of the standard model (SuSy, mirror matter, …) you won't be taken serious. Better proclaim smoking-gun inflation or even multiverses (= undecipherable verses).

   What do we really have? Just the standard model particles, with the stable ones being protons, electrons, photons, neutrinos. In the 1930s one would have tried to construct a Universe from that. And one would have hunted the bears-on-the-road like neutrino free-streaming, that seems to be a road block (but is not).

   Great times. I bet we will be forced to return to this level of modesty. And have to wonder how we could get it so wrong.

2. **Jim Given**
   May 13, 2015

   The philosophers of science have already told us everything we need to know:
   Every domain of Big Science needs a "governing narrative" to provide a basis for discussion and to evaluate research priorities. Such a narrative, a Big Theory, is constructed so as to be non-falsifiable. to provide continuity. So attempts to directly overturn Big Theories, whether Freud's in psychology, Chomsky's in linguistics, or inflationary cosmology in this case; are futile. What must happen is for funders and research directors to conclude the dominant Big Theory is no longer a productive source of interesting research directions; and for bright young theorists to work on other paradigms and find success. Perhaps future historians of science will conclude we are already well into this phase. But Big Theories never fail; those with a lifelong vested interest in them retire or die and are replaced by theorists with a different perspective. (This is all vintage Imre Lakatos.)
   *** May you live in interesting times. ***

3. **Michael Gogins**
   May 13, 2015

   Jim Given: I think it's a little more complex, and a great deal more interesting, than that. Think about Einstein and entanglement. He hated its implication of irreducible indeterminacy because it conflicted with what you are calling a "governing narrative," which for Einstein was the idea that natural law is deterministic and local. To fight this spooky implication, Einstein, Podolsky, and Rosen wrote a paper which has become extremely influential and productive, even though it is wrong.

   I guess there's a saying of some sort lurking in there: "wrong enough to be useful" or something like that.

   Anyway, scientists of different views are, in my judgment, at least the good ones, involved in something more like a dialectic than a die-off.

   Also, I think the final arbiters of how this all works are the scientists themselves, not philosophers. The scientists have implicit philosophies, which I call "the philosophy of scientists" as opposed to "the philosophy of science," which are usually pretty vague and not formulated to the standards of philosophical papers, but on the other hand, the empirical successes of science warrant that there is something important going on in "the philosophy of scientists."

4. **Syksy Räsänen**
May 13, 2015

Steinhardt is disingeniously conflating eternal inflation and inflation. Inflation is not necessarily eternal (many models, including ones consistent with the data, such as Higgs inflation, do not lead to eternal inflation), and does not in any way imply the multiverse. It's true that some inflationary theorists are promoting the chain of inference inflation -> eternal inflation -> multiverse. But the criticism should be directed towards the theorists, not inflationary theory. Instead, Steinhardt is adopting this false chain of reasoning to promote his own competing ideas.

PS. The first person to suggest inflation as a solution to the horizon problem was not Guth, but Kazanas: http://adsabs.harvard.edu/doi/10.1086/183361

5. **Thomas Larsson**
May 13, 2015

Theo, I think the situation today may be quite similar to the 1930s. At that time people had discovered that QM was incompatible with electromagnetics, and many people apparantly thought that the only way to solve that problem was to postulate new physics – I have seen claims that Oppenheimer thought so. Alas, we all know that the solution was not new physics, but a new way to look at old physics – renormalization. Perhaps there is a lesson there for our time, when everything but SM + GR looks increasingly unlikely.

6. **JG**
May 13, 2015

The role of philosophers belongs to pre-1950s and even much earlier when not many scientists took cosmological models serious.

Since then we have a remarkably good *scientific* cosmological model – people should (re-)read Weinberg's The First Three Minutes from 1977(!) to remind just how good a scientific theory we have even without Inflation, Dark Matter/Energy and other more recent theoretical suggestions. (Weinberg updated the book in 1993, before the accelerating expansion had been discovered but he discussed many of the other newer ideas)

None of these additions since the late 1970s are of a philosophical nature, they are deep mathematically constructed ideas that require a high degree of understanding of a lot of known physics.

Philosophers can only muddy the waters, we need brilliant mathematical/physical insights building on a solid understanding of the existing theory. We may even already have discovered the true model.

7. **Art Brown**
May 13, 2015

It's a pity the article didn't include any responses from Linde and/or Guth, given Steinhardt's statements about them.

8. **Dave Miller in Sacramento**
May 13, 2015

Perhaps someone more knowledgeable than I can let me know if my impression of inflation is close to the mark:

My impression is that inflation is an interesting idea, which might explain some interesting features of the universe (large-scale homogeneity, near flatness, etc.), which is consistent with existing cosmological data, and which perhaps has a little bit of observational data that supports it but that is inconclusive.

Is that pretty much it?

Incidentally, I was at SLAC when Guth did his initial inflation work there, and I went to hear one of Alan's early talks on the subject: it sounded interesting and still sounds interesting (the Big Bang as the ultimate beginning was always a bit dodgy), but I am curious as to whether it can ever be confirmed or disproven.

Dave Miller in Sacramento.

9. **Dave Miller in Sacramento**
May 13, 2015

Jim Given wrote:

> The philosophers of science have already told us everything we need to know: Every domain of Big Science needs a "governing narrative" to provide a basis for discussion and to evaluate research priorities. Such a narrative, a Big Theory, is constructed so as to be non-falsifiable. to provide continuity.

Except… a lot of those "governing narratives" *have* been falsified: the geocentric theory, the immutability of species, the absoluteness of space and time, the immovability of the continents, etc.

So… the philosophical theory you mention has in fact itself been falsified!

Dave

10. **Neil**
May 13, 2015

Send in the philosophers? I doubt that would help from what I have read so far (see Richard Dawid's book on "post-empiricism".) The multiverse, like free will, offers philosophers a dream subject–something that can be debated endlessly without fear of ever reaching resolution.

11. **Aleksandar Mikovic**
May 14, 2015

The obvious solution to the multiverse problem is to find a quantum gravity theory different from string theory, such that it allows inflation. One example is loop quantum cosmology.

12. **Syksy Räsänen**
May 14, 2015

Dave Miller in Sacramento:

The major success of inflation is not explaining the spatial homogeneity, isotropy and flatness of the background (although the last was not obvious at the time inflation was proposed), but accounting in detail for the perturbations in terms of quantum fluctuations, first discussed by Mukhanov and Chibisov.

The perturbation calculation is non-trivial, and it correctly predicted that the primordial perturbations are a) close to scale-invariant, b) dominantly scalar, c) adiabatic, d) statistically homogeneous and isotropic and e) Gaussian. These are real predictions, made before the observations were in. No other proposal has been able to account for them as well as inflation, even after the fact.

So there is a lot of support for inflation. Whether one regards inflation as proven is a matter of taste. There are hundreds of inflationary models (many of which differ from one or more of predictions given above), and I would prefer to have more understanding of the physics, or observation of the wavelength dependence of the gravitational waves (see below), before saying that we know that inflation occurred.

As for the smoking gun, the BICEP2 results were misrepresented from the beginning as "first direct evidence of cosmic inflation", because there was a lot of evidence already, and gravitational waves are not more direct than scalar perturbations. The simplest models of inflation predict a relationship between the amplitude of the scalar perturbations, amplitude of the gravitational waves and the wavelength-dependence of the gravitational wave amplitude. I think that could be called a 'smoking gun', in the sense that if it was measured, I think we could say that inflation happened, even without further understanding of the models.

Please note that even if something is a smoking gun (definite evidence for A), not finding it does not imply that A is not correct.

13. **Alex**
May 14, 2015

@Syksy Räsänen

"The simplest models of inflation predict a relationship between the amplitude of the scalar perturbations, amplitude of the gravitational waves and the wavelength-dependence of the gravitational wave amplitude. I think that could be called a 'smoking gun', in the sense that if it was measured, I think we could say that inflation happened, even without further understanding of the models."

But a parameter was measured, and it was found to conflict with the simplest models of inflation. Yet, all this measurement has done was to constrain which models of inflation could possibly work. Therefore I really don't think this could reasonably be called a smoking gun. Again, all it did was to rule out models of inflation that could not possibly be true.

14. **Alex**
May 14, 2015

@Syksy Räsänen

I've put a very rough argument together to put this idea that this observation would be a "smoking gun" on a slightly more objective footing.

I think its reasonable to assume that an observation being a "smoking gun" means that this observation must carry some kind of information about the theory we are trying to evaluate. Namely whether it be true or false. The key parameter we're measuring here would be the scalar-to-tensor ratio denoted by r. The measurement of r should tell us whether our theory is true or false. Denote these outcomes by \omega_r. \omega_r = 0 or 1 for false and true. They key question here is what is the probability that our theory (inflation) is false given some observation of r. I think its quite reasonable to assume (given the above discussion) that P(\omega_r = 0) ~ \epsilon, where \epsilon is a very small number, approaching zero. This quantifying the fact that no matter what value of r we observe, inflation is correct. Hence, P(\omega_r = 1) ~ 1 – \epsilon.

What is the information given by this measurement which you characterize as being of the "smoking gun" nature. The information entropy is E(I(\omega_r)) = -\sum_{\omega_r}(P(\omega_r)log(P(\omega_r))) = -\epsilon*log(\epsilon) – (1-\epsilon)*log(1-\epsilon) ~ 0.

Hence, no real information is conveyed given you characterization of this experiment. Hence we do not have a smoking gun.

15. **Syksy Räsänen**
May 14, 2015

Alex:

1. BICEP2 only claimed to have measured the amplitude of gravitational waves, not the dependence of the amplitude on wavenumber. So even if correct, their results would not have been what I called a smoking gun above.

2. The BICEP2 results, if correct, would not have been in conflict with the simplest models of inflation, though there was some tension. See http://arxiv.org/abs/1405.1390 (though their accounting of the tension is a bit simplistic).

3. Let us not confuse the concepts "smoking gun" and "test". A is a test for X, if X implies A. Proving A does not prove X, but disproving A disproves X.

A is a smoking gun for X, if A implies X. Proving A proves X, but disproving A does not disprove X.

Seeing gravitational waves that satisfy the consistency condition I referred to above can, in my view, be regarded as a smoking gun for inflation, because it relates three independent, non-trivial, quantities in a way that no other theory has (as far as I know) has been able to reproduce.

However, it cannot be regarded as a test, because even if the relation holds, the gravitational wave amplitude depends on the model (and can be below the sensitivity of near-future experiments), and there are many models of inflation in which the relation does not hold.

16. **Alex**
May 14, 2015

Syksy Räsänen:

I'm sorry I think I read part of your comment too quickly. Yes, I agree with point 1) above.

But as for this part:

"However, it cannot be regarded as a test, […] and there are many models of inflation in which the relation does not hold."

Am I reading this right? You're saying that one of the reasons it can't be regarded as a test is *because* there are many models of inflation in which this relationship between wave number and amplitude doesn't hold anyway?

17. **RM**
May 14, 2015

I think philosophers would be of help to Steinhardt – one of the basic lessons one learns in philosophy is that if A implies B, then Not A does not imply Not B.

18. **Alex**
May 14, 2015

Syksy Räsänen:

To sum up:

The alternative test you are proposing seems to be on the same footing as the amplitude measured by BICEP2 in terms of its ability to determine the validity of inflationary theory. If there is a specific relationship between wave number and amplitude, inflation is right. If there isn't this specific relationship, inflation is right. The problem isn't with your test, its with inflationary theory itself, its not uniquely defined.

Also, I disagree with your description of the "smoking gun" term. If an experiment is going to provide me with conclusive evidence that a theory is correct, there ought to be an experimental outcome that would prove the theory incorrect. In other words, the experimental outcome should give actual information (see my above post).

I think we're probably going to have to agree to disagree.

Cheers

19. **Syksy Räsänen**
May 14, 2015

Unlike Steinhardt claims, there's nothing extraordinary about the situation with regard to gravitational waves. This distinction of test vs. smoking gun is common to all cases where there are several models whose predictions overlap in the area where predictions have been made, but which have different predictions outside.

For example, consider direct detection experiments of dark matter, sensitive to a certain range of mass and interaction cross section. If events are seen that are consistent with dark matter (so that they are consistent with other observations, and backgrounds, systematics etc. are ruled out), this can be regarded as a smoking gun. If they are not seen, this does not rule out dark matter, because its mass and cross section may be out of the range probed by the experiment. This does not mean that the experiment does not give us information about dark matter (it narrows the possibilities).

20. **Andre**
May 14, 2015

Steinhardt criticizes inflation, because if this theory is confirmed, then his own work on the cyclic universe will remain useless. All simple.

21. **Peter Woit**
May 14, 2015

Andre,
Surely his own work is a motivation, but I think it's not unreasonable for him to argue that it's not a fair scientific competition between ideas when one side gets to claim victory no matter what the experiments say. If you take a look at the reaction after the BICEP2 results were announced, see
http://www.nature.com/news/gravitational-wave-finding-causes-spring-cleaning-in-physics-1.14910
you had
1. "Paul Steinhardt, a theoretical physicist at Princeton University in New Jersey and an originator of the cyclic theory, agrees that — if the BICEP2 findings are confirmed — his theory is now dead"
although he did say he would look for a cyclic model that would agree with BICEP2.
2. Linde on the other hand, reacting to incompatibility of BICEP2 with popular string inflation schemes: "There is no need to discard string theory, it is just a normal process of learning which versions of the theory are better,"

22. **Alex**
May 14, 2015

Syksy Räsänen:

IF you are already convinced inflation (or dark matter) indeed happened then yes these experiments provide information in the form of narrowing down which specific parameter values in the theory are true.

But I maintain that these tests provide no information as to whether the theory of inflation itself (for any parameter value) is true. There will always be a parameter value to fit ANY observation. So we can't "say that inflation happened" based on the results of BICEP2 or the experiment you proposed. But if we assume inflation happened then we can say which version of it happened based on the results of these experiments.

Ok, the prosecution rests.

23. **Dave Miller in Sacramento**
May 14, 2015

Alex and Syksy,

Thanks for your informative back-and-forth on inflation.

Sysksy, you wrote:

> The major success of inflation is… accounting in detail for the perturbations in terms of quantum fluctuations, first discussed by Mukhanov and Chibisov.
>
> …it correctly predicted that the primordial perturbations are a) close to scale-invariant, b) dominantly scalar, c) adiabatic, d) statistically homogeneous and isotropic and e) Gaussian….
>
> … There are hundreds of inflationary models (many of which differ from one or more of predictions given above)

That was sort of my impression, and it strikes me as suggestive, but very, very far from conclusive. After all,, isotropy and Gaussian distributions are, for example, rather common features of phenomena in the real world!

Having known Alan when he started working on inflation, I have a vested interest in hoping that it is eventually proven to be true: kinda cool to be able to say "I was there when…" (Similarly, I knew Schwarz, Polchinski, and Susskind when I was a student, so it would be kinda cool for me if string theory works out.)

But… when there are, as you say, "hundreds of inflationary models" that give different results… well, if the theories of Planck, Rutherford, Bohr, et al. had been that rubbery, I think they would have been a good deal less convincing.

Thanks again for the back-and-forth.

Dave

24. **srp**
May 14, 2015

Clearly a smoking gun and a crucial test are not the same things, as Syksy points out. A smoking gun in the suspect's hand would be powerful evidence of the suspect's guilt, but the absence of the smoking gun would not be enough to exonerate him. It is perfectly logical to posit such "one-way" tests that are not crucial but could still clinch the truth of a theory if they were passed. (In the same way, we might rule out a suspect if a test turned out one way but not convict him if it turned out the other way, e.g. if we see him far away from the victim on a video time-stamped at the time the crime occurred he's in the clear, but failing to find such a video wouldn't make him guilty.)

On the other hand, if we theorized that the murderer must come from a vaguely defined set of over one million people and claimed that our theory was useful only because a smoking gun might turn up in one of their hands, we might not be hired back as consulting detectives.

25. **Art Brown**
May 17, 2015

Syksy Räsänen,
Thanks for the reference to the illuminating Audren et al. analysis of the tension between BICEP2's original paper and Planck. I realize this topic is now Overtaken By Events, but:
1) Can you elaborate a bit on your comment that "their accounting of the tension was a bit simplistic"?
2) What am I to make of the conclusion in BICEP2's original release (1403.3985v1, 2, & 3): "These high values of r are in apparent tension with previous indirect limits based on temperature limits …"?
Thanks again.

26. **Calvin Marshall**
May 19, 2015

Dr. Woit,

As I understand it from Vilenkin's book 'Many Worlds in One', the mechanism for eternal inflation is a scalar field: areas of false vacuum decaying into areas of true vacuum. But as Alexander Vilenkin notes in 'Many Worlds in One', there is no direct evidence for the existence of scalar fields in nature: "Another important question is whether or not such scalar fields really exist in nature. Unfortunately, we don't know. There is no direct evidence for their existence." (p. 61). Reading this as a layman, this is what I take away: no scalar fields, no eternal inflation. There's no direct evidence for scalar fields, therefore there's no direct evidence eternal inflation. Is this a correct understanding?

Thank you,
Calvin Marshall

27. **Peter Woit**
May 19, 2015

Calvin Marshall,
We do now have evidence for a scalar field, the Higgs field. It's very unclear though whether the Higgs field can be used to give you inflation. The models cosmologists mostly work with use a postulated scalar field to drive inflation, one which has no other observable effects.

28. **Syksy Räsänen**
May 20, 2015

Art Brown:

1) In the paper, the authors find the confidence levels for Planck and BICEP2 separately and look at their overlap. They say that since the overlap is in the region between one and two sigma, there is no significant tension.

It would be more appropriate to do one or more of the following: 1) ask the Bayesian question of what is the likelihood of the BICEP2 observations given the Planck observations (assuming the simplest inflation model), 2) consider the goodness-of-fit of the model to the full dataset, or 3) see how much the fit is improved by the addition of new parameters.

All this, of course, assuming the (wrong) interpretation of the B-modes as being due to gravitational waves.

2) The part you quote is correct: the results were "in apparent tension" (which is different from conflict). For caveats in that BICEP2 statement of tension, see the abstract of the Audren et al paper.

29. **Syksy Räsänen**
May 20, 2015

Art Brown:

I realised now that your question is probably related to the title of the Audren et al paper, "BICEP2 and Planck are not in tension". I would say that their analysis shows that the tension is not as large as some thought (with the caveats above), not as saying that there is no tension, but this is a matter of taste.

30. **Art Brown**
May 20, 2015

Syksy Räsänen, Thank you very much for the additional explanation. On a separate note, I see you have updated your cosmology notes and look forward to having a go at them.

31. **Syksy Räsänen**
May 21, 2015

Art Brown:

Let me know of any typos!

32. **Hank Bolden**
June 6, 2015

In the New York Times dated June 5, 2015:

"Crisis at the Edge of Physics" DO physicists need empirical evidence to confirm their theories?

http://www.nytimes.com/2015/06/07/opinion/a-crisis-at-the-edge-of-physics.html

Contested Boundaries

May 13, 2015
Categories: Uncategorized

Apologies for too much recent posting here about the tired topic of the string wars. I hope to soon make amends by writing about something new I learned about geometric Langlands.

The summer 2015 issue of *Perspectives in Science* has an excellent article about the debate over string theory entitled [Contested Boundaries: The String Theory Debates and Ideologies of Science]() (also available [here]()), by Sophie Ritson and Kristian Camilleri. It deals with the debate from a point of view bringing together aspects of history, philosophy and sociology, while staying away from the technical aspects of the controversy.

The article has an interesting take on the debate, not taking sides as to who is right or wrong, but examining what the central issues have been and the ways people involved have chosen to make their case. One point made that I'd never thought of is that while this debate concerns an issue that comes up often, that of the boundary of what is science and what isn't, here the usual roles are reversed:

> In most scientific controversies in which we find scientists engaging in boundary work, the boundary dispute is generally over whether an unorthodox or minority view or approach should be regarded as science, pseudoscience, or pathological science. UFOology, parapsychology, intelligent design, and cold fusion all represent cases of this sort. The "ideological attempts to define science," as Gieryn explains, are largely motivated by the desire "to justify and protect the authority of science by offering principled demarcations from poachers or impostors" (Gieryn 1999, p. 26). However, in the case of string theory, it is the dominant research program in a well-established field of science that has been forced to defend its credentials as "scientific" (Taylor 1996, pp. 177–9).
>
> This presents an intriguing departure from most studied episodes of boundary work. String theory currently enjoys a privileged status by virtue of being the dominant paradigm within theoretical physics. Yet string theorists have found themselves forced to defend the scientific legitimacy of their research against charges that it has degenerated into a form of "metaphysics," "non-science," or "bad science." In doing so, string theorists have attempted to "loosen" the methodological definition of science, while critics try to impose a stricter definition.

The emphasis of the article is on the string theory debate, not the multiverse, and I'm (accurately) quoted as defending string theory as "scientific". When I wrote about this in my book back in 2002-3, I had no idea that multiverse pseudo-science would take hold among prominent theorists, a situation that raises issues I never thought would come up (the sort of thing Steinhardt is hoping for help with from the philosophers, see the last posting). The parts of the book about the "is it science?" question are ones I would write differently today, based on both recent history and new things I've learned about the philosophy of science.

Comments

1. **Andrew Norris**
   May 13, 2015

   Mention of boundaries brings to mind Turing's view on them:
   "Science is a differential equation. Religion is a boundary condition"

2. **Jim Beyer**
   May 14, 2015

   This was covered at a basic level on NPR's Science Friday:

   [Should These Scientific Ideas Be Retired?]()

   I think it is significant (and troubling) because even Ufologists, etc. don't dispute the validity of the scientific method; they just report results/findings which are not reproducible. I find John Mack more credible (as a researcher) than some string theorists….

3. **Marc**
   May 14, 2015

   Peter– you are quoted as saying "At the moment [string theory] is a theory which cannot be falsified by any conceivable experimental result." Suppose the 100 TeV collider discovers extra dimensions — ten of them. Would the knowledge that space-time is at least fourteen-dimensional rule out superstring theory? Or, perhaps more reasonably, suppose extra Z's are discovered that show that the gauge group of nature has at least rank 10 (or some large number). Would this rule out heterotic string theory? Yeah, these are extreme, and rather ridiculous, but they are "conceivable"…

4. **Peter Woit**
   May 14, 2015

   Marc,
   "conceivable" is perhaps an overly strong word, I think with that statement I was trying to be provocative, not precise. A better wording might have been "plausibly conceivable". And I wasn't just referring to heterotic string models, but to more general possibilities.

   But, honestly, it is extremely difficult to come up with actual experimental results that one couldn't somehow explain by a complicated string theory model. First of all, the examples you give assume a certain theoretical interpretation of some experimental result, with experiment not directly measuring number of dimensions or rank of gauge group. Trying to think of experimental results that would have that interpretation, I suspect alternate explanations would be possible. Even given airtight evidence for one of those theoretical interpretations, it's unclear to me that one really can't find some kind of string model with those properties. For a while there was an active research program of trying to characterize the "swampland", qft theories that could not come from string theory. Typically though, claims to identify such a QFT were quickly met by someone coming up with a string theory construction, see for example
   https://golem.ph.utexas.edu/~distler/blog/archives/000651.html

   So, the claim I had in mind was really: think of anything you can plausibly imagine turning up in a plot from an LHC experiment in the next few years and find an example that would cause string theorists to abandon string theory. I can't think of anything like that.

5. **Peter Woit**
   May 14, 2015

   After more thought, a quite precise version of the claim I had in mind that Marc refers to would be:

“No one working on an LHC experiment has ever run a Monte-Carlo for some scenario, such that if data matched the Monte-Carlo, that would cause string theorists to abandon string theory.”

6. **JG**
May 14, 2015

article available at mitpressjournals.org for non-subscribers

7. **Peter Woit**
May 14, 2015

JG,
Thanks. I’ve now put both links in the posting, hopefully people will be able to get access to one or the other of them.

8. **Anonyrat**
May 14, 2015

“No one working on an LHC experiment has ever run a Monte-Carlo for some scenario, such that if data matched the Monte-Carlo, that would cause string theorists to abandon string theory.”

Much sharper, but could use further sharpening in my opinion. For instance, the string theorists’ riposte could be – “Nor have they run a Monte-Carlo that would cause physicists to abandon QFT. Abandon SUSY, the Standard Model, etc., yes, but not QFT”.

I know you’ve previously dealt with this kind of objection, so just saying….

9. **Peter Woit**
May 14, 2015

Anonyrat,

I’m sure many if not most theorists would happily abandon the Standard Model given a proven violation of it, and much of the LHC effort is looking for that, surely with Monte-Carlo simulation of possibilities. In some sense LHC research is mainly an effort to falsify the SM, and my point was just that no one is even trying to falsify string theory.

Better to not reopen the question of the appropriate definition of falsifiability though, it’s a thorny subject discussed here often, and it seems highly unlikely anyone will have anything new and insightful on the topic.

10. **al**
May 15, 2015

It’s a pity that Roger Penrose (Roads to Reality) is not mentioned.
Also a glance towards the financial aspects might have been useful in discussing sociology: doing theoretical physics is the cheapest way to do (natural) science, and it’s a bonus if the work is never refuted. Advertising nowadays often promotes ‘prestige’ that is not to be judged by grossly material standards.

Next Week's Hype

May 14, 2015
Categories: This Week's Hype

I know I should be coming up with material on different topics here, but the multiverse stuff sometimes is just too hard to ignore.

Next week's Comicpalooza in Houston will feature string theorist Gerald Cleaver. His blurb tells us that:

> His EUCOS team conducts long-term systematic computer-based studies of global phenomenology of parameter spaces of the string landscape of around 10,500 possible string-derived universes and its theorized multiverse realization.

A local paper has a news story: Physicist to discuss multiverse theory at comic convention. According to the article:

> Cleaver is a physicist and early universe cosmologist at Baylor University whose area of expertise is string theories, or the concept that there are not one but multiple universes in existence…
>
> Cleaver said when the multiverse theory was first investigated scientifically in the 1980s, the formulas made it look like a string, hence the name, but was later revised to be more spherical or bubble-like. Some call the universes "bubbleverses" as a result….
>
> Cleaver said there are four levels of universes with ours being in the first and most simple level. The second level has its forces and can contain that of the first. The third has its own and can contain the first two and likewise with the fourth level.
>
> String theory remains just that – a theory. Cleaver, however, feels scientists are close to proving the theory. It could be a matter of a few days or a few years but he and others like him press on with the belief that reality is bigger and much stranger than fiction.

According to his website, besides the string landscape,

> Cleaver is also interested in the general concept of multiverse, not just the string/M-derived class. In his spare time he is writing a book on philosophical and theological implications of a multiverse and has contributed related chapters in associated books…
>
> Cleaver is also a member of the XP4 division of Icarus Interstellar, a non-profit organization created by his Ph.D. graduate Dr. Richard Obousy. Members of XP4 are exploring advanced propulsion systems and energy generation concepts for interstellar spacecraft, including possible string/M-theory realization of the Alcubierre effect.

For Cleaver's views on theology and the multiverse, see his articles here. Last fall he gave a talk to students at Baylor explaining that

> a multiverse is the likely natural mechanism through which the God of infinitudes grants inherent freedom to a spatially and/or temporally infinite creation. In other words, the multiverse is God's means of indeterminacy in action.

Update: Keeping up with the hype is getting to be beyond my powers. Just this morning there's:

- The idea that "string theory allows time travel at the LHC" has been revived, reported on here, and Morgan Freeman has it on Through the Wormhole. This first appeared back in 2011, discussed here then.
  By the way, the same site has convincing evidence for time travel, reporting earlier this year that Large Hadron Collider Predecessor May Be Four Times Larger, Much More Powerful.
- Nova has something about yet another paper describing how we're going to observe the multiverse through bubble collisions. The paper at least admits the obvious, that if such things were observable, almost surely we would have already seen them:

  > a certain amount of tuning must be applied in order to construct models that produce signatures that are possibly observable but not yet ruled out by data.

Comments

1. **DV82XL**
   May 14, 2015

   Can you think of a better venue for this sort of flight-of-fancy?

2. **N**
   May 15, 2015

   10,500? Not 10^500?
   What a relief!

3. **phil fogle**
   May 15, 2015

   Ok , we know there are nutters in the world, but can someone please come up with a counterexample???

4. **Guest**
   May 15, 2015

   They wrote: "String theory remains just that – a theory". They should have written: "String theory remains just that – a hypothesis".q

5. **S**
   May 15, 2015

   This seems like a piece that might interest you, Peter, especially toward the end (although the whole article is interesting). It's unclear what the attitude of the reviewer or writer toward what they're saying is:

   http://www.firstthings.com/article/2015/06/a-new-era

"Increasingly, one finds science lapping over its seawalls. Indeed, in some areas, the boundary between science and speculation has been entirely washed away. Science began with philosophical speculation twenty-five centuries ago, and it seems likely that it will end in the same place."

6. **Richard E.**
   May 15, 2015

   You have to sign a statement of faith to work at Baylor, so presumably the religious spin is less of a surprise in this case.

7. **Theo Nieuwenhuizen**
   May 15, 2015

   String theory is no more than a consistent quantum model. I don't understand why we should believe any of this, knowing that string theory does not reproduce the standard model of elementary particles. This hyping is just a cover up for its complete failure to connect to the things that do exist. I don't mind string theologists. But they should choose words like: "As a string theologist I am convinced that …"

8. **Jon Awbrey**
   May 15, 2015

   G-strings?

   Where "G" is for *Genesis*, of course.

9. **Peter Woit**
   May 15, 2015

   Theo,
   I think you're being very unfair to theologians (or theologists), by trying to associate them with Cleaver's string landscape stuff.

   Richard E.
   Seems to me that the theology is the part of Cleaver's work that actually makes some sort of sense. If God is going to be in the business of making universes, why shouldn't he make lots of them? On the other hand, I do think Baylor should be concerned about the landscape pseudo-science and public promotion of it…

   By the way, this reminded me of Amanda Peet's Perimeter lecture, which probably could have been improved by adding some God into it. There was a live blog during that lecture which included some Perimeter physicists going on about how wonderful it was, one of whom (Tibra Ali) was a Cleaver postdoc. Perhaps Cleaver can be Perimeter's next public event.

10. **Nobody**
    May 15, 2015

    According to Aquinas, God knows all possible universes, so maybe Tegmark et al _should_ be considered theologians.

11. **Theo Nieuwenhuizen**
    May 15, 2015

    Dear Peter, if string theologists declare themselves as such, this will not much improve my respect for them. For others it would clarify why they make the claims they make.

12. **Mr. Roarke**
    May 15, 2015

    String Theory, Multiverse, God… the Neapolitan ice cream of Fantasy Island.

13. **KenW**
    May 15, 2015

    Comicpalooza looks like quite the ticket. Jackie Earle Haley alone is worth the price of admission. But you get Henry Winkler, too.

14. **Noboru Nakanishi**
    May 15, 2015

    String theory is a theory of everything except for the truth.

15. **Al**
    May 15, 2015

    I don't know what we gain by drawing attention to seeming crackpots like Cleaver with his theological justifications for multiverse. There are plenty of proper scientists working on string theory landscape who deserve to be criticized on scientific grounds.

16. **Narad**
    May 16, 2015

    > If [G-d] is going to be in the business of making universes, why shouldn't he make lots of them?

    I suspect that *that* phrasing instantiates the heresy of G-d as demiurge, but somebody would have to check with Guy Consolmagno.

17. **Yatima**
    May 16, 2015

    Cleaver said there are four levels of universes with ours being in the first and most simple level. The second level has its forces and can contain that of the first. The third has its own and can contain the first two and likewise with the fourth level.

    More evidence that the War on Drugs is an utter failure.

18. **adrian**

May 16, 2015

dear all,
i believe it is a bit unfair to associate the work of Dr. Cleaver with the work of the rest of the strings theorists. These things just catch the media attention, but this is NOT what string theorist do. As you all know, we have this website called arXiv.org, where one can see what string and other theorist work on.
Also, we have this website called inspire.net, where you can check that the work of Dr. Clever in the last few years and on the string landscape, have not had a big impact in the community. Again, it is my impression that what the media says or reflects is not the reality of the string theorists.

Also, it is, clearly a bit unfair to write opinions about the talk and its contents–before–the talk happened.

Just wanted to get the thing straight. This is not the reality, nor the main interest of most of the string theorist, who, by the way, are working on different aspects of quantum field theory.

thanks

19. **Peter Woit**
May 16, 2015

Adrian,
The research program Cleaver is pursuing is the sort of thing lots of prominent string theorists were promoting as the "String Vacuum Project", see
http://www.physics.rutgers.edu/~mrd/SVP-v2.ps
The only difference is that most of them seem to have realized (but never admitted publicly) that this was nonsense and given up on it. It's still as far as I know a respected part of the "string phenomenology" business.

It is true that most good string theorists have long ago given up on "string phenomenology" as hopeless, but they still publicly often promote the claim that "string theory is our best hope for unification". And Cleaver's pseudo-science about the multiverse and string theory is now firmly mainstream.

As far as outreach efforts go, I don't see a lot of difference between what Cleaver is doing, the Amanda Peet/Perimeter "Lego" performance, and any number of books by prominent string theorists about how wonderful the multiverse is.

20. **adrian**
May 16, 2015

dear Peter,
thanks for your reply. As you may have guessed, I work on string theory. Allow me to disagree with some of the things you wrote and agree with others.

–I heard about the project you mentioned lead by Douglas and Kane. When I heard about it via some european physicists, my reaction was positive. I see the whole idea as a 'taxonomic' effort. A classificatory effort—in this case of Calabi-Yau 3-folds. In my experience, these efforts are worthwhile.

We learnt a lot of geometry [even useful to mathematicians sometimes] thanks to projects based on pure taxonomy. Good examples are papers lead by Gauntlett, Waldram, Martelli, Sparks, classifying AdS-vacua of type II and eleven dimensional supergravities. More recent works by Tomassiello and collaborators, Papadopoulos and collaborators, etc.

These are just examples, but you and your readers are probably aware that this is precise and correct science. Some people like me have used these papers to learn about QFTs. Now, will the same go for the project you linked? Obviously, i do not know.
But is not a bad idea to classify geometries. Do you agree?

I may be a bit allergic to the wording about the 'landscape and particle Physics" but any educated physicists knows that there is not much of Physics—for the moment– in there.
I see it as a project in geometry, with the hope that it might illuminate some Physics beyond the Standard Model….but that is a hope, close to the opinion.

–I do not see "string phenomenology" as a waste of time. I see it as a way people learn about geometry. See papers, for example, by Schaffer-Nameki and collaborators. This is serious and correct science. Will it have impact or find a place in Physics beyond the Standard Model? I do not know. I am a bit pessimistic, but that is an 'opinion'.

I agree with you that some statements have been over claimed in the past [and probably in the present]. But that is not Physics or science. It is the press together with some not-so-careful attitude of scientist. One should not forget, they are humans, so a bit of ego is expected to appear. I think that for example, all the physicists i mentioned above, are doing correct, not-overclaimed, precise studies. I think you will agree.

–About the talk of Amanda Peet in Perimeter and your comparison with Cleaver's forthcoming one. I cannot give you my opinion, I did not hear Cleaver's talk. I do not think I will be able to hear it [will it be recorded and linked?].

In any case. Let me tell you what I saw in Peet's talk: She talked–in a popular style– about topics that are well established. Mostly, black holes and the calculation of their entropies. These are very precise computations in CFT and in gravity. She said nothing that was technically wrong. Same goes for her description of AdS/CFT. One may or may not like her style, her choice of topics and the wording she used. But I do not think she said anything technically wrong. You feel it was a bit over claimed? me too. But is hard in a talk of those characteristics to be completely clear about what we observe and what we do not observe. It is hard—and this I know for experience– not to talk as if we are describing "the truth". I honestly do not think she gave a bad talk, I do not think she was dishonest. One can of course pick three or four phrases where the 'stringer in her' does not clarify "this is not observed in Nature". In summary, I believe that Peet's talk was correct.

–Finally and to close the disagreements; you wrote
"And Cleaver's pseudo-science about the multiverse and string theory is now firmly mainstream."

But, it is mainstream in the press, not in the ArxiV. Do you agree?

Physics is decided in the ArXiv [i mean, the papers posted there], these seminars, the labs and on what physicists work on. I understand and share your worry about all this press attention. I do agree it is not beneficial for young people, I see it in my early courses, talented kids that want to learn about M-theory before learning the oscillator. One just needs to call their attention to some obvious facts, they understand.

All right, too long comment, your readers are already gone, your attention cannot be bothered any more. Apologies

21. **Klaus**
May 16, 2015

@adrian, @Peter,

since about 8 years, every morning, the first thing I look at is http://arxiv.org/list/hep-th/recent . You are right to distinguish what happens in the press from what happens on arxiv. The press mainstream is about "multiverse", "unfied theory" etc. The term multiverse appears in 151 abstracts in hep-th; many well-known physicists appear as authors, but this is indeed a small number.

The arxiv/hep-th mainstream differs from the press mainstream in many other topics: it is rarely about "unfied theory", rarely about "theory of everything", rarely about "comparison with experiment", rarely about the "standard model". And when hep-th is about these topics, it is usually about approaches that are NOT from string theorists. Try by yourself.

Not only is the reality of string theory in the arxiv different from in the press; in a sense, hep-th is even more disappointing: string theorists in the arxiv do not seem to work on the topics they tell about in the press or in popular talks.

Hept-th shows clearly that people working on string theory have given up both on unification and on comparison with experiments. Also impressive is the lack of string theorists working on the foundations or on the principles of string theory. As a group, string theorists have effectively given up determining the spectrum of elementary particles, given up deducing the forces of nature, and given up calculating fundamental constants.

I might be wrong, and I am open to be corrected; but since many years, hep-th does NOT provide good advertizing or encouragement for string theory as a theory of everything.

22. **adrian**
May 17, 2015

dear Klaus, dear readers,
I must have lots of free time, that I am responding to comments. I will try to stop, as the thing is addictive. First of all, I wish to thank Klaus and Peter for the non-agressive character of their comments. I would immediately retreat to the usual silence if people started being aggressive pointlessly. In this way of discussing, one can may be learn something from other people. Let me take some minutes of your attention to give my opinion or views on Klaus' comments.
I will copy Klaus' comment above and try to elaborate on my view on it.

Klaus

"The term multiverse appears in 151 abstracts in hep-th; many well-known physicists appear as authors, but this is indeed a small number."

Yes, the number is small. I think the topic is immature—needs many developments in the mathematics to occur. It is unclear if it will ever give something useful for Physics.
These reasons, I think, scare away most stringers. Some people, of course, decide to explore the landscape of string theory. Please, appreciate that this is an interesting geometrical problem.

Klaus

" string theorists in the arxiv do not seem to work on the topics they tell about in the press or in popular talks."

This, I believe is due to the fact that the topic one is presently woking on is not yet 'mature' in our minds. It is unclear what its implications are. It would be incorrect that I give a popular talk discussing my latest paper dealing with geometries dual to 2-d CFTs. The significance of the paper and the topic is still unclear in my broad picture of Physics. I cannot tell these things in a talk for people without mathematical instruction. This is for my technical seminars. Do we agree? I believe this is the main reason behind for this correct observation by Klaus.

Klaus

"Hept-th shows clearly that people working on string theory have given up both on unification and on comparison with experiments. Also impressive is the lack of string theorists working on the foundations or on the principles of string theory. As a group, string theorists have effectively given up determining the spectrum of elementary particles, given up deducing the forces of nature, and given up calculating fundamental constants.
"
Many comments to make. I believe that what is described above is nearly correct.
Some people will disagree and with reason, since they attempt to make contact with experiments [for example, those working on the quark gluon plasma, cosmology, some beyond the standard model]. But, may I suggest that this disconnection with the experiment is due to the fact that, may be, the string theory is not yet ready to calculate some of these things Klaus wants to calculate. You may suggest that it will never be. I do not know for sure, but you do not know that either. I just think—and this is clear to me–that the theory has the potential to calculate those things. But certainly is not at the level of technical development to do so. For example, it is unclear how to compute the precise values of certain constants in Nature. But you will surely agree that it is not an in-principle impossibility.

To be fair, some other things have been calculated, like spectra and correlators in different QFTs [not in the standard model] that can be checked as soon as the Lattice field theory progresses a bit on the technical side. I think, given the interest that practising physicists show on this, the topic should not be disregarded.

What Klaus is asking, would be like asking a stringer of the early 1990's to deal with the strong coupling regime in Yang-Mills field theory. It is 2015 and we can with good confidence say sure things about field theories that are related to YM after many [MANY] deformations of the theory. Still, we believe that the path covered is on the correct track. Since we saw similar deformations in Lattice field theory [there, it goes under the name of 'strong coupling expansion']. All these developments thanks to the remarkable proposal by Maldacena in 1997.

Sorry that I digressed. My point is that sometimes the theory is not ready to produce what the physicist want it to produce. Sometimes a theory cannot produce it. People who have studied the theory of strings, surely feel the remarkable beauty and power of the theory. This gives some confidence on the above projects. Of course, this is a feeling. Might be wrong. Another feeling, due to the amazing consistency of the theory is that, may be, it is actually a deep branch of mathematics that we are working on.

Finally, Klaus wrote:
"I might be wrong, and I am open to be corrected; but since many years, hep-th does NOT provide good advertizing or encouragement for string theory as a theory of everything.
"
You are correct, in my view. There are people who would disagree. People who seriously work on string phenomenology, either in Spain, Germany and many other places. Once again, let us make a distinction between what physicists say or want or wish and what the theory of strings actually gives. The arXiv is a good thermometer, what people work on is what the theory is actually giving. As you know this should be averaged over time. Apologies that I insist, but I am pretty sure that the theory of strings have taught physicists a LOT about the general structure of quantum field theory, illuminating aspects that were completely obscure in theories like QCD, the Electroweak theory, etc. So, in my opinion, it is worth to have an open mind. One may learn lot thanks to the theory of strings.

To close this comment. Some reader may want to investigate some of the claims in this post–those related to the useful role of string theory to learn about QFT. I suggest to read the different sets of lectures that Matthew Strassler gave in Trieste 2000, TASI 2001, TASI 2005. Of course, there are many more, I can write upon request, but these are good places where to start.

Thanks again for your patience, apologies for the unreadable post.

23. **Peter Woit**
May 17, 2015

Adrian and Klaus,
Thanks to both of you for the thoughtful comments.

My perception I think is consistent with both of yours. Theorists have mostly abandoned work on connecting string theory unification to experiment, as well as on the problem of really understanding "what string theory is" at a fundamental level. They've done this for the excellent reason that there are no good ideas around about these topics, so no way to make progress. Better then to do other things, which is what they are doing.

The problem I see though is that instead of acknowledging what doesn't work, many if not most string theorists just repeat the same failed ideology, and allow that to govern what research topics are pursued. The first part of the ideology is that there is some sort of single 11-dimensonal M-theory that will solve the problem of fundamentals and explain known facts about dualities. The second part of the ideology is that string theory explains unification but actually checking this is hopeless, because of the string landscape. Most string theorists don't give public promotional talks, but they do invoke this ideology if asked, and those that do give public talks heavily promote the two ideologies. The Amanda Peet lecture was a good example: it was a sales pitch, not a scientific talk, and when someone asked the obvious question about the product not working, she answered by making completely untrue claims for the product: "tested at the LHC!!".

The hep-th literature is depressing to look at these days, I think for the reason that theorists still insist on following the "hot topic", and the choice by leaders of the field of "hot topic" remains constrained by the requirement that it be consistent with the governing ideologies. Maybe some day this will lead to something new and some real progress, but I fear this will take a very long time.

24. **These Binary Trees**
May 17, 2015

Physics will always be philosophy's subservient niece-turned-mistress, messily slurring her strange words like Edison's talking dolls.

https://www.youtube.com/watch?v=VMiAOTnAZ5I

25. **Bernhard**
May 18, 2015

" … at comic convention. "

At least at an appropriate place.

26. **adrian**
May 20, 2015

Dear Peter,
I would like to comment on something you wrote a couple of days ago, in the comments o your post. You wrote:

"The hep-th literature is depressing to look at these days, I think for the reason that theorists still insist on following the "hot topic", and the choice by leaders of the field of "hot topic" remains constrained by the requirement that it be consistent with the governing ideologies. Maybe some day this will lead to something new and some real progress, but I fear this will take a very long time
"
My comment is the following; it is easy from the outside to criticize. Just like the person who watches football criticizes the ones in the field, sometimes with acidity, many times in very unfair ways.
I think that your comment goes on a similar line. I would add, if you believe it is so depressive, may be you should start publishing papers, not following the hot topics, with good ideas that young people will follow and things will improve over all.

Both of us know that it is not easy. We both know that people do the very best they can. In the case of the string theorist—that I know close enough, they belong to the most 'pure' scientist you can find. They do not cheat, they act honestly, they calculate and write doing their very best effort. We all know that this is not necessarily the case in all other areas of knowledge.
So, to finish, I believe that comments like the one above are quite unhappy and unfair.
Do not take this as a personal attack. It is not. I do know that what you wrote is not a personal attack to any stringer in particular. But I believe that comments in this tone create on lay-people the impression of a dishonest community. The stringers might be misguided [I do not think it is the case, but that an opinion], but certainly, they are not lazy or dishonest or anything like that. Quite the opposite I would say.
thanks

27. **Peter Woit**
May 20, 2015

Adrian,

I wasn't accusing string theorists of being lazy and dishonest, in any case I would never generalize about qualities of groups of people, that's not something I do. About dishonesty, I'm afraid I have to say that over the years I have definitely run into some dishonest string theorists, although they're a small minority of the field.

The criticism of the faddishness of particle theory was meant seriously, but it's a sociological rather than personal criticism. I've written about it extensively, on the blog and in my book, and I think it's a problem that many people are aware of and would agree with me about. Faddishness is an aspect of any human endeavor, but I've had enough experience in two different but related academic fields (math and physics) to see that the problem is a lot more serious in particle theory, as well as to see from the situation in mathematics that it doesn't have to be that way.

I completely understand the appeal of working on topics where there is a lot of activity going on, and that there are a wide range of reasons to do this, some good, some less so. I just think it really is a fact that the range of topics discussed on hep-th at any time is relatively narrow, something quite understandable when major progress is in the works, something discouraging to watch when it isn't.

Personally, most of my recent effort has gone into the book I've been writing, which is almost done. One aspect of writing the book is that it has made even more clear to me where there are some basic questions that are not well understood. I have a list of ideas about such things to work on once I finish the book, hopefully some day one or more of those will bear fruit and I'll write about it. But even if they don't, that doesn't change the actual nature of the current state of hep-th, which I think can only be honestly described as discouraging.

Lurie and Categorifying the Fourier Transform

May 18, 2015
Categories: Langlands

Before I turn to the main topic of this posting, a lecture by Jacob Lurie, I'd like to point to something else involving him, a comment and posting at Mathematics Without Apologies, a blog you should be following anyway. On the topic of the usefulness of "proof assistants", I liked Lurie's point that a major problem with this is:

> Working in a formal system, more or less by definition, means that you can't ignore steps which are routine and focus attention on the ones that contain the fundamental ideas.

But if you want to discuss this, it should be over there, the topic of this posting is something very different.

Last week I noticed that Lurie had given a talk at Harvard on "Categorifying Fourier Theory", which is available here. I enjoyed watching it, ending up quite intrigued by the abstract picture he was painting, but rather discouraged by the lack of any example that would give insight into what it might be useful for. Neglecting to mention the example that explains why an abstract theorem is useful is unfortunately all-too-common practice among mathematicians. Perhaps in this case with Dick Gross, Jean-Pierre Serre and John Tate in the front row, he felt it unnecessary. Luckily though, he gave the same talk recently in Arizona, and there (in the question session) did give a fascinating motivating example.

His starting point was the Fourier transform, which one can generalize to any abelian group G, and think of as identifying complex functions on G with complex functions on the dual (or character) group G^. The standard Fourier transform is the case G=G^=**R**, Fourier series are the case G=U(1), G^=**Z**. He then went on to discuss two levels of abstraction, or categorification of this. The first identifies a representation of G on a vector space V with a function from G^ to vector spaces (the isotypic decomposition of the representation). The second identifies representations of G on categories with representations of G^ on categories.

It was this equivalence of representations on categories that was his main result, for which in Arizona he gave the example of G the group of invertible Laurent series. The idea is that this group can be identified with its dual group G^ (in some sense as algebraic groups), using the Weil symbol (for a definition and context, see here). Lurie's claim that was new to me was that the equivalence in this case is essentially the GL(1) version of the general local geometric Langlands conjecture, which is supposed to be an equivalence of two representations on categories, for more general (non-abelian) groups G.

At least for me, understanding of some sophisticated mathematical phenomenon really starts when I understand the simplest example of the phenomenon. For the number field case of Langlands theory, my initial efforts to understand the subject didn't lead anywhere until I realized that maybe it was best to first think about the local version, which was a statement about representation theory that I could make some sense of. I was hopeful that thinking about the simplest case of that, the abelian case, would give great insight, found though that the Abelian case is already quite non-trivial (local class field theory). For the geometric Langlands case, I found that the discussion of the local version in Edward Frenkel's book was very helpful, but I always wondered about the abelian case. Now I'm hopeful that the abelian story is something that although I've never seen it, is well-understood, and that a helpful reader will point me to a reference.

Another reason for being interested in this particular topic is that it has some connection to the relationship between Langlands theory and QFT that first got me interested in all of this. Back in 1987 Witten wrote some fascinating papers giving an abstract formulation of free fermion theories on Riemann surfaces (see here and here) with tantalizing connections to what later became geometric Langlands. In this work the group of invertible Laurent series and the Weil symbol play a central role. There was also later work by Takhtajan on this, see here and here. I wonder why the most recent version of the last reference deletes the material on the multiplicative group case, which is the one Lurie mentions.

Comments

1. **Marty Weissman**
   May 19, 2015

   The abelian case of geometric Langlands is geometric class field theory. Serre's text on algebraic groups and class fields is the standard reference. But there are many expositions! See http://mathoverflow.net/questions/73054/a-reference-for-geometric-class-field-theory for links.

2. **Peter Woit**
   May 19, 2015

   Thanks Marty,
   What I'm curious about is something that should be much simpler, the local case, and just for the group GL(1,C). Lurie's argument seems to indicate that this case follows from his very general, very simple construction. Also, he's claiming an equivalence of group actions on categories, and I'd like to know more about what that means here (as opposed to just a usual Langlands correspondence of representations).

3. **Justin**
   May 19, 2015

   Peter,

   I've never understood what this langlands QFT connection is supposed to have anything to do with new physics. Is this purely a mathematical interest, or is there potential for a new stimulating insight into fundamental physics?

4. **Peter Woit**
   May 19, 2015

   Justin,
   Personally I believe the connections between qft and the representation theory point of view on number theory (e.g. Langlands theory) indicate a deep unity that if better understood may lead to new insights in fundamental physics, for more, see
   ww.math.columbia.edu/~woit/mathphys.pdf

   But that's a far off goal, and the question I'm asking here is a purely mathematical one, I was just pointing out the tantalizing connection to QFT.

5. **David Ben-Zvi**
   May 19, 2015

   Thanks for the links Peter! I haven't yet watched the videos, which sound great, but here's some perspective on the story. First of all the local geometric Langlands conjecture is (roughly speaking) an equivalence of two-categories, one associated to G and one associated to the dual group G^. This is completely natural from the physics — a four-dimensional topological field theory (such as twisted N=4 super Yang Mills) should attach a 2-category to a closed 1-manifold, in this case the circle, so that an equivalence of field theories (S-duality) gives an equivalence of 2-categories. Roughly speaking, following Frenkel-Gaitsgory, one side is the 2-category of categorical representations of the loop group, and the other is the 2-category of sheaves of categories over the stack of Langlands parameters — very roughly, modules over the tensor category of quasicoherent sheaves on the space of Langlands paramters. In the abelian case, geometric Langlands is captured by Cartier duality (algebraic Pontrjagin duality, i.e. Fourier transform) — this goes back at least to Laumon's work on Fourier transforms and that of Rothstein. There they work in the global setting, using the self-duality of the Jacobian, but this has a well-known local origin, the Contou-Carrere Cartier self-duality of invertible Laurent series (or of its quotient by invertible Taylor series, i.e., the abelian affine Grassmannian). This is what Jacob is generalizing I believe, though I need to find out what the precise statement is. Sadly as with most things in this subject it's hard to find introductory references.

6. **David Ben-Zvi**
   May 19, 2015

   In any case let's work out the GL_1 case in the simpler Betti form (the Betti Geometric Langlands conjecture is a new modified form of the usual de Rham one which, unlike its predecessor, really is a TFT, an algebraic model of Kapustin-Witten's – to appear). On both sides we're not dealing with any old two-categories, but with monoidal categories (the 2-categories being the collection of their module categories). On the automorphic side this is the "categorical group algebra" of the group K^* of Laurent series — i.e. the monoidal category built so that its module categories are simply categories with an action of the group. This means (in the Betti version) local systems on K^* with convolution (in the de Rham case this is replaced with D-modules). But K^* splits as a product of two contractible pieces (the infinite dimensional vector space of monic Taylor series and its dual, the formal group of K/O) which don't affect the category, and then Z (counting degree of leading term) and C^* (the coefficient of the leading term). So we need to describe local systems on Z x C^*, with convolution. Sheaves on Z are the same as representations of the dual multiplicative group C^*, ie quasicoherent sheaves on pt/C^*. The Mellin transform (multiplicative Fourier transform) identifies local systems on C^* (ie reps of pi_1 = Z) with quasicoherent sheaves on the dual torus C^* (labeling monodromies). So we have an equivalence of monoidal categories betweek Loc(K^*) with convolution and QCoh(C^* x pt/C^*) with tensor product, hence of their 2-categories of module categories. Now note the latter is C^*/C^*= C^* local systems on the punctured disc — i.e., the space of Langlands parameters (it's fun to work this out for general tori to not get confused between the group and its dual). This is the (Betti) local geometric class field theory (the de Rham version involves the stack of rank one connections on the punctured disc, which is more painful).

7. **David Ben-Zvi**
   May 19, 2015

   As an illustration of Betti vs de Rham by the way it's useful to look at how we can think of sheaves on C^*. In the Betti version, local systems=reps of pi_1=modules for group algebra of pi_1=quasicoherent sheaves on the dual C^*. In the de Rham case, D-modules on C^*= modules for C[z,z^-1] adjoined z d/dz = (by Mellin transform) modules for C[s] (s corresponding to z d/dz) adjoined t, t^-1 (corresponding to z) which acts on s as shift by integers. In other words we have difference modules on the dual C, which are the same as Z-equivariant quasicoherent sheaves on C. ANALYTICALLY these would be the same as sheaves on the quotient C/Z= C^*, but algebraically the two categories are very different. However, they both contain the same "basic objects of interest", the "eigensheaves" (rank one local systems with various monodromies) and things finitely constructed out of them (finite rank local systems=flat connections). Thus the two theories are different ways to "integrate" the same basic objects. The claim is the kind of objects seen by Kapustin-Witten are closer to the former than the latter.

8. **Peter Woit**
   May 20, 2015

   Thanks David!

   That's very helpful. It would be great if someone would write this sort of thing up in detail. I'm still quite curious how one might understand the picture you're explaining in terms of Lurie's "categorification" of Pontryagin duality.

9. **John Baez**
   May 20, 2015

   Peter wrote: " I'm still quite curious how one might understand the picture you're explaining in terms of Lurie's "categorification" of Pontryagin duality."

   I can't tell what level of understanding you're at and what you're wondering about, so I'll assume this remark means Ben-Zvi's story is not screaming out *categorification* to you, as it is to me.

   The point is that sheaves are like functions, but functions on a space form a set, while sheaves form a category. It's good to draw up a big "analogy chart" with two columns, at left listing things you can do with the set of complex-valued functions on a space (or abelian group), at right the corresponding things you can do with the category of sheaves over that space. Most fundamentally, you can add and multiply functions on a space, and you can convolve functions on a group. All these operations have analogues for sheaves! Think about the analogue of the "group algebra" over in the right column. The fun starts when you get to the Fourier transform.

10. **Peter Woit**
    May 20, 2015

    John,
    Thanks for the further comments elucidating Ben-Zvi. I do see the general "categorification" going on here, I'm afraid I wasn't very clear about what I still don't see. It's really about this specific example, how the identification given by the Weil pairing and the general story about group actions on categories told by Lurie ends up giving the equivalence between the things David describes. I suppose I just need to think carefully about the hints he provides…

Various News

May 19, 2015
Categories: Uncategorized

- First test collisions at 6.5 TeV/beam at the LHC are tentatively scheduled for Thursday morning.
- At CERN today there's a workshop about the Higgs Machine Learning Challenge.
- Also on the topic of LHC data analysis news, Tommaso Dorigo announces the award of a grant for the AMVA4NewPhysics project.
- Sabine Hossenfelder has a review, slideshow and discussion of the Dawid book on "String theory and the scientific method" (which I wrote about here and here). Much of the discussion is about the "No Alternatives" argument, but at this point I don't even see how it applies here. The Landscape shows that string theory unification is a failed program, which rules it out. As for whether "gravitation is due to the spin two massless mode of a superstring" is the only alternative, these days my impression is that many prominent theorists are pursuing alternatives, that gravity is supposed to be an "emergent" phenomenon coming from something else.
- For those sticking to the 1984 point of view though, there's a workshop on some interesting mathematics that's part of that story (super geometry, super moduli spaces) going on at the Simons Center.
- Last week on Jeopardy (see here), no one got this question:

  > Nima Arkani-Hamed is using this number dimension, the next one beyond time, to rock the physics world.

  I wouldn't have either…

- For a bit of mathematics history, you might want to read Beilinson on Gelfand's seminar.
- Natalie Wolchover at Quanta keeps on coming up with interesting physics stories not seen anywhere else, last week covering news about ultra-high energy cosmic rays.

Update: Successful test with first collisions at 13 TeV this morning at the LHC, see here.

Comments

1. **Bee**
   May 19, 2015

   Thanks for the link. I found the no alternatives argument to be the least convincing one. Anyway, I think it's good some philosopher is trying to make sense of this mess 🙂

2. **Egon**
   May 19, 2015

   It seems that the Jeopardy answer must be referring to the following paper:
   http://arxiv.org/abs/hep-th/0104005

   The paper is from 2001, though, so it's hard to see how it could still be 'rocking the physics world.'

3. **Chillin**
   May 19, 2015

   I'm staying with the 1984 viewpoint.

4. **Tommaso Dorigo**
   May 20, 2015

   Hello Peter,

   thank you for highlighting my grant. I literally spent four months of my life on writing it, but the chances of success with this kind of EU projects are very small, so I must have been lucky anyway.

   Since no good deed goes unpunished, I will ask you the courtesy to advertise the PhD positions we will open, in a few months 😉

   Cheers,
   T.

5. **PAC**
   May 20, 2015

   Goodness, the Soviet Union as portrayed by Beilinson sounds like sheer heaven, from today's perspective. No soulless consumerism, no politics-as-spectacle, no globalized noise. And respect for the austere beauty of mathematics and art.

   Very far from a complete picture obviously, but a much-needed one.

6. **AHS**
   May 21, 2015

   Thanks for Beilinson's little write-up. Some very nice insight. Cheers

7. **Nick M.**
   May 21, 2015

   > **Egon** says:
   > *May 19, 2015 at 12:06 pm*
   >
   > It seems that the Jeopardy answer must be referring to the following paper:
   > http://arxiv.org/abs/hep-th/0104005

> The paper is from 2001, though, so it's hard to see how it could still be 'rocking the physics world.'

Egon,

As opposed to thinking about what was "'*rocking*' the physics world" back in 2001, the Jeopardy production staff was instead hoping that you would be drawn to thinking about what was "'*rocking*' the music world" back in 1966 to 1975; that is, to associate the word '*rock*' (or, more precisely, the phrase '*rock and roll*'), with the word '*dimension*' so as to bring you to thinking about the R&B/Pop/Soul vocal group "The 5th Dimension". I know you're given perhaps all of three seconds to try to put this all together, but it's a perfect example of the wacky way that Jeopardy likes to bury clues within 'the answer' so as to guide you to the correct 'question' response on their game show.

-Nick

8. **Narad**
May 21, 2015

> so as to bring you to thinking about the R&B/Pop/Soul vocal group "The 5th Dimension"

Yah, I figured that out, but it seems very oddly placed as the only third question in as bland a category as "physical science." True obscurantism in this vein would have realized that The 5th Dimension made a lot of bread by covering Laura Nyro songs, including two from *Eli and the* ***Thirteenth*** *Confession*.

9. **Natalie W.**
May 22, 2015

Thank you, Peter!

10. **N**
May 23, 2015

OT:
There is an interesting debate going on over at Bee's site.
Lubos at his best!
🙂

11. **paddy**
May 23, 2015

Thanks N. Interesting is perhaps an understatement.

12. **Robert Barton**
May 27, 2015

Jeopardy is pretty straight forward, and its questions are written to be answered by non-specialists. Time is the fourth dimension(common knowledge). So one more. It's meant to be that simple. You don't have to know who Arkani is and 99.9% of people don't. His name is a kind of red herring. They could have substituted a whole bunch of names for his. Jeopardy does not reference physics papers. The music group idea is probably correct because they use "rocked" which seems a weird verb choice otherwise. So if you got it by the music group that's impressive lateral thinking but it would only serve to reinforce the easy answer.

John Nash 1928-2015

May 24, 2015
Categories: Obituaries

I was sorry to hear this morning that John Nash and his wife Alicia died yesterday in a car crash (news story here). They were in a taxi on the New Jersey Turnpike, heading home from the airport after a trip to Norway where Nash was awarded the Abel Prize.

Nash's mathematical career was cut short by the onset of mental illness, which he then struggled with for many years. Sylvia Nasar's A Beautiful Mind is a wonderful biography, doing a great job of accurately portraying Nash's life, including the role of the mathematics community in its various parts. The movie version is another story, especially in the way it shows Nash's mathematical achievements as somehow being due to his delusions, when what really happened is that the onset of delusional thinking is what made it no longer possible for him to continue doing research at the highest level.

During the years I was a graduate student in Princeton, Nash was often to be seen, especially in the mathematics/physics library, and I talked to him a few times. The first time was when he stopped me one day, told me he had seen my name on the physics department picture board, and was curious about the origin of my last name. While I had heard stories about Nash, that he was mentally ill, spent his time writing delusional things on the hallway blackboards, he seemed fine to me. This was a period (early 1980s) when he had stopped writing on the blackboards and was successfully dealing with the illness. I was very glad to see how later on he was able to lead a more normal life and enjoy the recognition he deserved.

Update: The New York Times has an excellent long obituary of Nash this morning, presumably mostly prepared before his death.

Comments

1. **Bill**
   May 24, 2015

   This tragedy reminds me that we are all human, even though we all tend to lose touch with reality and forget what it means.

2. **Neil**
   May 24, 2015

   Are we sure that his blackboard scribblings were delusional? I mean his real ones, not the ones in the movie. Did anyone write them down?

3. **Peter Woit**
   May 24, 2015

   Neil,
   The blackboard writings were mostly before my time (I got there in 1979). I'm sure people read them and tried to make sense of them, never heard of anyone saying that they had been insightful as opposed to delusional. Nash himself may be the best source about this, I recall reading somewhere comments from him about what was going on in his mind during those years and what he thought about it from his later, non-delusional point of view.
   There's no question though that he was ill during this period, and the effects of this disease in terms of delusional thinking are well-known.

4. **Dan**
   May 24, 2015

   Sad to hear about their passing. On a happier tangent, regarding the NYT obituaries, here is an interesting article where one of their writing staff explains the somewhat weird process by which they write "advance" obits, including interviewing the "pre-dead" subjects…

5. **David Derbes**
   May 25, 2015

   Nash and obituaries.
   I was an undergraduate 1970-74 at Princeton and knew Nash a very little, again mostly from extremely brief interactions with him at Fine-Jadwin (the math and physics buildings are connected, and the library is on the ground floor/basement with holdings for both.) Back then one could smoke in the building, and I would occasionally bum a light or a smoke from him (never the other way.) The writings on the blackboard were typically done overnight and usually involved very large numbers raised to very large exponents. I wish I'd taken pictures of them. I had a few courses at 8:00 and was greeted by new results frequently. Now and then in the middle of the night my lab partner Mark Johnston and I would see him at the Computer Center (it was the only time we could get on; Mark wrote the Fortran code to analyze our lab data.) My friend Danny Rohrlich, now at the Technion I think, the nephew of Fritz Rohrlich and himself now a physicist, double majored in physics and psychology. He had, I think, pretty normal conversations with Nash when nobody else could. I had two very brief and memorable interactions with Nash after graduation. I went to my 10th reunion in 1984, and leaving Monday morning to go visit a friend in New York, got off the dinky to wait for the train, suddenly realized that I didn't know when the next train was. I went into the station to look for a schedule, and saw a man with his back to me. I walked up to him and said, Pardon me, do you know when the next train to New York is? He turned around; it was Nash! "No," he said. Four years later I was on a NSF sponsored teacher summer school at Princeton, when walking up the stairs towards Blair Arch, there was Nash! I said, "Hello, Prof. Nash." He turned and smiled. "Hello," he said. He was clearly much, much better. I said, truthfully, "I used to see you around Fine when I was an undergraduate. You look great!" And he said, "I'm feeling good." It was really wonderful. Something happened between 1984 and 1988. I don't know what, but the difference was night and day. Nash always liked libraries (Fine-Jadwin, and the main library, Firestone in particular) and trains. My classmates referred to him as the Ghost or the Library Crazy Man. He always wore red Keds.

   Obituaries: Every big newspaper keeps a large file cabinet (or probably now, its electronic equivalent) called the morgue. Newsworthy people get a file and interesting articles and other snippets go into the morgue, to be pulled out when the time comes to write their obituary. That's why they could put Nash's obit together rapidly. Feynman's NYT obit is really good, and appeared the next day. (I learned about this from a friend whose family owned the New Orleans newspaper. His father offered me the use of the morgue to do some high school research into a politician running for governor. Pre-internet.)

6. **Matt Grayson**
   May 25, 2015

   In 1979, on a Fine Hall blackboard, I saw a list of towns and a claim that 2^(zip code)-1 was prime… He was a ghost then – Russell Crowe perfectly captured the distant look in his eyes. I visited Princeton in 1987, and it was wonderful to see Nash's improvement.

   I'd forgotten the Keds!

7. **David Derbes**
May 25, 2015

I forgot to mention that Nash's late night notes were often in the form of a letter, typically from Moses to Brezhnev, or other unlikely world leaders. I think Kruschev also figured in the correspondence, as did famous mathematicians, many of whom I had not then heard of.

8. **paddy**
May 25, 2015

Ave atque vale.

9. **Jeff M**
May 26, 2015

My Nash story, told to me by one of my professors in grad school. He was an undergrad at MIT when Nash was there, Paul Cohen was there at the same time. They would get into pissing contests in the math lounge, each trying to prove he was smarter than the other. So my prof Al would hang around since they would start discussing something and scribbling away on blackboards and Al said he learned a lot watching them. In any case Nash knew Al to say hi to, and one day he comes up to him and hands him an offprint of one of his papers. Al takes it, says thank you, and then opens it up. On the inside of the cover was a very elaborately drawn and calligraphed "interstellar drivers license" valid everywhere in the galaxy, made out to Al Vasquez, and marked as being "valid in perpetuity." Al was kind of confused, this was before Nash had his public break. Later, when Nash was hospitalized, he contacted Al through one of the MIT profs to let him know that despite the fact that the license he gave him was good forever, Nash was going to have to revoke it. Needless to say Al still has the license.

10. **Cormac O'Raifeartaigh**
May 26, 2015

My Dad met him a few times at IAS and always said that "he didn't seem any stranger than anyone else there"!

11. **Nathalie**
May 26, 2015

What kind of Nobel Prize did Nash get?

There is no Nobel Prize in mathematics and the one in economy is the The State Bank of Sweden Prize created 3/4 of century after Nobel had passed away.

12. **Casey Leedom**
May 26, 2015

Nathalie, "… he shared the 1994 Nobel Memorial Prize in Economic Sciences with game theorists Reinhard Selten and John Harsanyi."

http://en.wikipedia.org/wiki/John_Forbes_Nash,_Jr.

13. **Peter Woit**
May 27, 2015

Casey Leedom,
Nathalie is just referring to, as Wikipedia notes, the fact that the prize Nash got is technically the "Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, commonly (and misleadingly) referred to as the Nobel Prize in Economics". The Abel prize is also not a Nobel prize, although set up to be in some ways equivalent. At the time Nash got the Abel, I commented here
http://www.math.columbia.edu/~woit/wordpress/?p=7613
that "This surely makes him the first person to win not-quite-Nobels in two completely different fields.", but that was challenged here
http://www.math.columbia.edu/~woit/wordpress/?p=7613&cpage=1#comment-217775

14. **Kieron**
May 27, 2015

[Something I shared with my friends when I heard, on Facebook. – kieron]
A profound loss, but also a source of some hope….
— — — — — — — —
John Nash was a great mathematician. He, tragically, was involved in a traffic accident in a taxi on his way back from the airport, with his wife Alicia, after receiving the (Norwegian) Abel Prize for his work on non-linear differential equations and geometric analysis (with Louis Nirenberg). Both John & Alicia were killed, on Saturday, and one might hope they are together now.
Although he is best known for the "Nash equilibrium" (eventually sharing the Nobel prize in Economics as a result) which extends game theory to non-zero-sum contexts and paved the way to real-world economic use, he did mathematically more profound work later, in different fields — after, thankfully, returning to sanity. That range and depth is rare in modern mathematics, which has been increasingly specialized. (I can only think of Terry Tao as someone as wide ranging and recognised, in modern maths.) He was a prodigious talent and, despite Hollywood, his illness was clearly a handicap, not a help or inspiration.
The story of his return from illness, and rebuilding his professional and personal life is touching and heartwarming too. A story that gives us all hope that our troubles will pass and that a bright future exists and is attainable and that help, companionship and support are so important — an indispensable part of humanity.
I met him once, at a colloquium at Princeton in the very early '90's when I was doing work on X Windows at MIT. He seemed sane enough then, and I barely understood the differential geometry he was talking about. 🙂
Check out this rather good obituary at the NYT:
http://www.nytimes.com/…/john-nash-a-beautiful-mind-subject…
Truly A Beautiful Mind. RIP.
– kieron

15. **S. Molnar**
May 27, 2015

I'm not sure what to make of this, but since both Nash and NSA are recurring subjects here, it might be of interest: http://econospeak.blogspot.com/2015/05/john-nash-as-cryptanalyst.html. The blogger is the son of a well-known logician. Speaking of logicians, the Guardian obituary, which is a disappointment, claims Nash was being funny when he said that among mathematicians it is only the logicians who tend not to be sane; the obituarist apparently does not realize that this is a commonplace view.

Not quite OT, since you mentioned it, do we get the Woit name etymology? I have no idea whether you came by it from a long lineage of Woits or whether it's a corruption of an entirely different (Polish?) name.

16. **Peter Woit**
May 27, 2015

S. Molnar,
The name is Latvian (my father was born in Riga), in Latvian, Voits was the family name, spelling changed when they moved to Germany, to agree with German pronunciation.

17. **A. Seibert**
May 28, 2015

I haven't seen the movie recently, but I don't think it attributes Nash's mathematical achievements to his delusions. Granted, it inaccurately shows him as delusional during his mathematical peak, and it links his brilliance and his madness by using the same flashing-numbers special effect for each. But unless I've forgotten scenes where Ed Harris and the other apparitions whisper successful proof strategies in Nash's ear, this merely suggests that both Nash's brilliance and his delusions may stem from his mind's rare openness to unusual ideas and appetite for patterns–which was basically the view expressed by post-recovery Nash and by Nasar's book. That's quite distinct from saying the delusions caused the mathematics. The only mathematics I recall the movie attributing to the delusions was fictitious "code breaking" stuffed into abandoned mailboxes.

18. **NumCracker**
May 29, 2015

What a stupid way to die … it's a pity!

19. **Fawaz Isaac**
June 1, 2015

His story taught me a lot, there is no words could describe how his Bio influenced lots of people.
At the end nothing could be said except, good bye legend, good bye ghost of Fine Hall.

REST IN PEACE BEAUTIFUL MIND!!!

This Week’s Hype

May 27, 2015
Categories: This Week's Hype

I’m busy with other things, so no possible way I can keep up with the claims about string theory flooding the media for some reason these days. It’s hard enough to find the time to read all of this, much less write something thoughtful about it… One obvious point to make though is that none of it acknowledges the obvious: the widely promoted idea that we can get a unified theory and explain the Standard Model by using a theory of strings has turned out to be an empty one. The result of tens of thousands of papers and more than 30 years of work is that all the evidence is that if you can get something this way that looks at all like the Standard Model, you can get anything. Normally when that happens you simply acknowledge the problem and give up, but for some reason that hasn’t happened. Instead of a description of this straightforward situation, the public gets the following:

- Frank Close describes the situation as

  > In recent years, however, many physicists have developed theories of great mathematical elegance, but which are beyond the reach of empirical falsification, even in principle. The uncomfortable question that arises is whether they can still be regarded as science. Some scientists are proposing that the definition of what is “scientific” be loosened, while others fear that to do so could open the door for pseudo-scientists or charlatans to mislead the public and claim equal space for their views…
  >
  > Is physics moving towards an era in which elegance will suffice and into the domain of theories that are beyond the reach of experimental proof? Or will empirical evidence remain the arbiter of science?

  Close correctly identifies the problematic nature of multiverse pseudo-science, but misses the basic facts about the string theory landscape. This is not a theory of “great mathematical elegance”, quite the opposite, and there is no such thing developed “in recent years”. If you go back 30 years, there were then claims of “elegant” string theory models, but those never worked out. KKLT is the opposite of “elegant”.

- Clifford Johnson takes the Close piece as a starting point to explain his own view. He lacks interest in string unification, thinks that string theory should be thought of as a “method” for solving problems. He doesn’t really explain though why it is a “method” that deserves so much more attention than any number of other methods used in physics. He also doesn’t acknowledge that, besides the huge amount of TOE hype (which he and other string theorists often appear on TV to promote), the hype problem for the string theory “method” may be just as bad. For example, he was quite proud of his efforts to promote string theory as the method to understand heavy-ion physics (see here and here), but that’s something that really hasn’t worked out very well.
- Over at Starts With a Bang, by Sabine Hossenfelder, there’s Will the LHC be able to test String Theory: Definitely maybe. The article actually does a very good job of explaining why the answer is “no”, so I have no idea why the misleading headline. The fact that the AdS/CFT heavy ion predictions haven’t worked out is explained, with the comment that

  > The LHC, thus, has already tested string theory!

  and that this failed, which might be a more accurate headline.

- There’s a long interview with Sean Carroll at the Edge website. He’s quite defensive about the multiverse, claims it’s a prediction of our best theories, and gives his usual characterization of multiverse critics as zealots unable to understand the idea of an indirect test

  > But certain zealous colleagues of mine are saying that because you can’t see the other universes in the multiverse or because you can’t see the little super strings moving around, these theories are not falsifiable and, therefore, should not count as science.

  He’s also defensive about string theory, there the argument is

  > Either we will bring it down to earth and connect it to the world we see or people will lose interest. People cannot maintain this optimistic idea that we’re going to get the right theory of quantum gravity, the theory of everything, if it’s literally decades and decades of people writing down equations and never predicting the experimental outcome of anything. But we’re not there yet. It would be a terrible shame if we gave up on string theory when maybe next year someone will figure out how to bring it in connection to observations, or maybe ten years from now it will happen. This is how science works, and this is it at work.

  The problem here is that it is “literally decades and decades of people writing down equations and never predicting the experimental outcome of anything”, and no matter how many decades of this go on, someone can always argue that “maybe next year”. He’s avoiding the very real issue at the center of things: why hasn’t string theory been held to account for its failures the way any normal speculative scientific idea is supposed to?
  As far as his current interests go, from what he says, he seems to be losing interest in the multiverse, which soon may no longer be a hot topic, and moving into research into complexity and consciousness.

Comments

1. **Calvin Marshall**
   May 27, 2015

   Dr. Woit – Thank you for continuing to write such interesting, informative articles. Always look forward to the latest showing up in my Twitter feed.

2. **John McVirgo**
   May 27, 2015

   Peter,

   surely you need to be following more closely what top young post docs have to say about String Theory, rather than focusing on the opinions of theoretical physicists much older and possibly ignorant in comparison?

   For example, this is what David Simmons-Duffin has to say:
   http://www.quora.com/Is-it-true-that-many-physicists-are-abandoning-String-Theory

   “Here’s my personal favorite at the moment (Warning: technical jargon — feel free to skip this paragraph). Physicists have recently been able to compute operator dimensions and scattering amplitudes in planar N=4 Super Yang Mills theory nonperturbatively at all values of the ‘t Hooft coupling. These quantities interpolate between 5-loop Feynman diagram calculations at weak coupling (note, these are calculations in QFT, a theory no physicist — even Woit or Smolin — disputes the correctness of) and 10-dimensional supergravity calculations at strong coupling, with full String Theory calculations in between. This is spectacular evidence that N=4 SYM (a close cousin of the theory of quarks) is both a Quantum Field Theory and a String Theory (as conjectured by Maldacena), in a way that can be made quantitatively precise.

I don't know what high energy theorist isn't impressed with 5-loop Feynman diagrams. And if you're impressed with that, you'd better be impressed with all-loop results that agree with 5-loops when Taylor expanded to 5th order.

That kind of result is so impressive that I am convinced without a shadow of a doubt about the merit of studying String Theory. It also shows why String Theory is an inevitable and inextricable part of physics. Gauge theory is real and correct: it describes electrons and photons and W-bosons and quarks and has been verified innumerable times by a variety of experiments. The above evidence shows that some gauge theories literally are String Theories. Given that many scientists would like to understand gauge theory better, it would be totally crazy to abandon String Theory!"

3. **Yatima**
May 27, 2015

and moving into research into complexity and consciousness

This does not bode well as there lies a fertile savannah where the tenure buffaloos of crackpottery congregate, free of the restraints of realistic physics and the theory of computation. At night, the ghostly moonlight of fantasist attributes ascribed to the human mind illuminates their ruminations of ethereal qualia.

4. **vmarko**
May 27, 2015

John McVirgo,

Young post-docs are not a very good opinion gauge, since they are typically still bedazzled by the theories they study (I'm also struggling with that, so…). The first signal of becoming a mature scientist is when one recognizes all the *problems* that the theory has, and starts paying attention to those rather than saying "Oh wow, this can be evaluated to five loops! Wow!".

The serious scientist focuses on what is wrong with their own pet theory, and emphasizes that in public and among peers, so that others may also attack the problems. Putting one's head in the sand doesn't make the problems go away, but only adds to confusion. Senior scientists are aware of these problems (at least they should be), while junior scientists somewhat lack the experience to recognize them as such. Btw, that is why mentoring is so important in academia.

As for the concrete example you quoted, the "planar N=4 Super Yang Mills theory" has a very big symmetry, and nobody should be surprised that something like that can be integrated in some cases. But this is very very far from, say, Standard Model, or any other realistic theory, where a large portion of that symmetry is broken. Saying that it is a "a close cousin of the theory of quarks" is just naive. I often compare such statements to a man who wants to go to the Moon, and who after climbing a tree says "I've made a first step". It's just a lack of perspective.

HTH, 🙂
Marko

5. **Jeff M**
May 27, 2015

Ha, I will refrain from listing all the reasons I think consciousness research is a dead end 🙂 As for "let's study string theory since some gauge theories are string theories" I will point out that generalization is not always a good thing. Also, the fact that you can compute something does not make it interesting, or useful. If only – one of the joys of being a mathematician is working on some problem, realizing you can compute something related to what you want, and at the same time realizing it won't do you a lick of good…

6. **Bee**
May 28, 2015

Peter,

Ethan, who edits the collection, asked me to write a piece on the question. (He also picked the title and changed a few paragraphs here and there.) My first impuls was to say, well, the answer is obviously no. On second thought, I managed to write "no" in 1600 words 😉 To be fair, I find the AdS/CFT approach to strongly coupled matter very interesting, though the applications to strange metals seem better motivated. Besides, I'm trying to remain open-minded. As the saying goes, predictions are difficult, esp about the future. (And I've been told it's wrongly attributed to Niels Bohr, so I won't.)

7. **Peter Woit**
May 28, 2015

Thanks Bee,

I should have made clearer that I think it was a very well-done article, except maybe for the title….

8. **Peter Woit**
May 28, 2015

John McVirgo,
I did write something at that Quora question. Best if people who want to discuss that do it there, although I fear participating in discussions at sites like that is not something I have the time for.

9. **Koray**
May 28, 2015

I think Mr. Carroll's problem is more with Mr. Occam than with Mr. Popper. If I come up with a scientific model that fits the existing data, but inevitably also suggests that there are unicorns that I can never see, should I take the unicorns seriously, or should I doubt that perhaps there's an equally good, yet to be found model that doesn't suggest unicorns?

Unless logicians demonstrate that this is the only mathematical model we can ever find, I'm not comfortable with taking unicorns seriously. Even then, my next move would be to question the foundations of math or its use in physics itself.

10. **Hilbert**

May 28, 2015

I think if there's a theory that fits the data, then successfully predicts the observation of something new, AND also contains a mechanism within itself that generates unicorns that can never been seen, but the mechanism follows from the mathematics and from the principles of the theory, THEN we should take those unobservable unicorns seriously.

11. **Peter Woit**
May 28, 2015

Koray/Hilbert,
I think that what Carroll is doing is trying to shift the argument from actual theories we know about (where the unicorns are a dubious feature) to some hypothetical unknown theory (where the unicorns are a sure thing). At least this zealot isn't very interested in such hypotheticals, happy to admit there's logically some unicorn theory out there which I'd sign on to.
This is a standard move in string theory defenses, to try and discuss some completely successful unknown string theory that string theorists wish existed, not the one that actually does.

12. **Nobody**
May 29, 2015

From Frank Close quotation:
"Some scientists are proposing that the definition of what is "scientific" be loosened… towards an era in which elegance will suffice and into the domain of theories that are beyond the reach of experimental proof? "

Reminds me of the talk about new methods of valuating corporations at the top of a market boom, like the dot.com boom, when "elegant" corporations that had never made a cent were sky high. Could this mean the career ending bust is near?

From Sean Carroll quotation:
"It would be a terrible shame if we gave up on string theory when maybe next year someone will figure out how to bring it in connection to observations, or maybe ten years from now it will happen."

Definition of insanity: doing the same thing over and over again and expecting different results. – Albert Einstein

"moving into research into complexity and consciousness."

Oh no!

13. **zzz**
May 29, 2015

"the next six months will be critical for string theory"

– Tom Friedman

14. **David Metzler**
June 2, 2015

I thought this was interesting, from the "not the only game in town" department:

https://www.quantamagazine.org/20150527-a-new-theory-to-explain-the-higgs-mass/

Also of possible interest to you, Peter, though OT for this posting:

https://now.dartmouth.edu/2015/05/major-byrne-gift-launches-new-program-mathematics

15. **Matthew Foster**
June 3, 2015

Hi Peter Woit and John McVirgo,

It seems to me that you are talking apples and oranges here. The David Simmons-Duffin quote sums up some of the very strange and remarkable results now available for large-n, N=4 Super Yang Mills theory. That's some exotic conformally invariant, in some sense "integrable" QFT that keeps leading to new surprises, from AdS/CFT to the Amplituhedron. It's possible that one can learn some deep aspects of quantum field theory from studying this "exactly solvable" (?) model. The same can be said about many aspects of quantum gravity and string research over the years; sometimes seemingly completely unrealistic models of fundamental physics end up having surprising and testable consequences for effective condensed matter systems–for example, 1+1-D Liouville field theory, which has connections to membrane and glass physics as well as being a toy model of quantum gravity.

Irrespective of the "hype" they use to sell their work, I view the efforts of string theorists from a pragmatic perspective, similar to my view of manned space exploration. Absent the race to put weapons platforms in space that was a big cold war motivation, one might ask what the compelling scientific purpose of manned exploration has been or will be in the near future, beyond useful near-earth orbit missions to deploy and service scientific instruments, satellites, etc. Advocates stress the indirect benefits; perhaps if string theorists were better-acquainted with the mathematics and physics "spin-offs" that have paid off in condensed matter, one could make a better argument than the one to abandon scientific falsifiability. I'm afraid that the latter will just isolate high-energy theory from the rest of science, and probably have a chilling effect on funding.

At the same time, if you acknowledge that unexpected byproducts are a main benefit of a large line of research, one has to evaluate the value of those compared to the investment. I'm curious where you come down on that one, Peter, given your interest in Langlands, CFT, and the mathematical aspects of QFT more generally. Are these less impressive or important than the benefits of further accelerator research, assuming no sparticles or other non-SM physics is found at LHC?

16. **Peter Woit**
June 3, 2015

Matthew Foster,
I don't see any useful way to compare accelerators and pure theory research, they're just two very different things. The HEP experiment people have a very tough job ahead figuring out how to go beyond the LHC. There isn't an obvious best choice, and all choices are expensive. Theorists are not expensive at all on that scale. I don't think moving money from one side to the other can help: diverted theory funding would be too small to solve the problem of the experimenters, diverted experimental

funding I don't think will solve the problems of the theorists (I don't think they are now due to insufficient funding).

I'm obviously all in favor of mathematically based QFT research, which I think one can justify either by "spin-offs" or simply by the argument that deeper understanding is inherently of value. What I see as problematic is selling one narrow part of this research (that related to string theory) by bogus claims that string theory will unify physics, when that's a failed idea. One part of the problem is that if you justify your field by an overhyped idea that doesn't work, as it becomes clear the idea doesn't work, people will pull the plug on your field (already happening: in the US right now very few young theorists can get a job in this area).

The other part of the problem is that this leads to a focus of research into certain narrow areas based purely on the fact that they are connected to the failed program. Sure, N=4 SYM is an interesting model, but there are a huge number of other poorly understood questions at the intersection of qft and math. The Simmons-Duffin quote doesn't seem to recognize this is not the only model out there with a fascinating and not completely understood structure. Instead of more people working on AdS/CFT, justifying themselves by the failed idea of string theory unification, I think we'd be better off if questions not related to AdS/CFT could also get attention. These might seem harder to justify than just invoking string theory and the theory of everything, but making a more honest case is likely to work better in the long run anyway.

17. **Totally off topic**
June 4, 2015

"Liouville field theory, which has connections to membrane and glass physics as well as being a toy model of quantum gravity"

This sounds fascinating. Could you give me a reference on applications of Liouville theory to membrane and glass physics?

18. **Matthew Foster**
June 4, 2015

Hi Totally off topic,

There is a generalized version of Derrida's Random Energy Model (a supersimplified, exactly solvable model of the glass transition) in which many-body energies have logarithmic correlations, instead of being independent as in the original. The partition function can alternatively be thought of as that for a classical particle in a log-correlated random potential. At high temperatures, the particle is "delocalized" throughout the volume, while it tends to get stuck in one of a "few" minima below a freezing transition at which the extensive component of the entropy vanishes. There is yet another interpretation of the model in terms multifractal wavefunctions of disordered Dirac fermions in 2D.

The connection to Liouville is as follows. The partition function is a random variable, and exhibits log-normal fluctuations at low temperatures. This is in contrast to the free energy, which is non-fluctuating (self-averaging) in the thermodynamic limit. You can write a generating functional to calculate moments of the partition function, averaging over potential configurations. The result has the form of Liouville field theory.

In the high temperature phase, you can calculate statistics using scaling dimensions of local (vertex) operators. This fails at low temperatures, however, because the OPE generates higher moment operators from lower ones.
There is a mapping to the KPP equation for non-linear diffusion, in which one keeps the entire infinite tower of moment operators. In the end, the glass transition (or transition for the moments) is encoded in the "velocity selection" of the front solution to KPP, and this allows an exact solution. This part is well-understood and has been checked numerically in various contexts.

The part that I don't understand that well is how to interpret the results in terms of LFT. There are some interesting ideas that the fusion of this infinite tower of operators somehow produces new "macroscopic" operators, and it is the dimensions of these that one is computing with KPP.

This whole business is due to Carpentier and Le Doussal, although the result was actually conjectured earlier by Chamon, Mudry, and Wen, using a schematic mapping to the problem of directed polymers on the Cayley tree.

Try:
"Glass transition of a particle in a random potential, front selection in nonlinear renormalization group, and entropic phenomena in Liouville and sinh-Gordon models"

Carpentier and Le Doussal, PHYSICAL REVIEW E, VOLUME 63, 026110

19. **Totally off topic**
June 4, 2015

Thanks so much, Matthew Foster!

20. **Sebastian Thaler**
June 7, 2015

Interesting piece in June 7th New York Times on this topic:

http://www.nytimes.com/2015/06/07/opinion/a-crisis-at-the-edge-of-physics.html?ref=todayspaper&_r=0

21. **tk**
June 8, 2015

In case you haven't seen it, there is a comment in the major local Berlin newspaper: http://www.tagesspiegel.de/wissen/mathematik-ein-universum-aus-zahlen/11770478.html

The Nash Musings

May 28, 2015
Categories: Uncategorized

Since the news of the tragic death recently of John Nash and his wife in an automobile accident last weekend, some of those who were at Princeton during the same time I was (late 70s, early 80s) have been exchanging emails of their memories of Nash from that time. It turns out that two of them, Mark Schneider and Steven Bottone, had transcribed some of Nash's blackboard writings in a notebook. Here's Steve's account of this:

> Back in May 1979, just after our general examination, Mark Schneider and I decided we would write down some of the writings that Nash left on the blackboards around Jadwin. Mark sat in a wheeled desk chair with a steno notebook in hand and I wheeled him from board to board as he transcribed the musings. I believe if we had done this even six months earlier we would have had a lot more interesting samples of Nash's writings, but I think he was already starting to tail off on his output by May 1979. As far as I know, we were the only Princetonians who thought of compiling any of this. I have made a PDF out of the musings.

Since I suspect quite a few people are curious about these blackboard writings (including me, they mostly had ended by the time I got to Princeton in late 1979), Mark and Steve have allowed me to make the pdf available, see the link above. Mark points out that they didn't actually see Nash writing these, so his authorship is an assumption. Steve notes that the handwriting was distinctive, and that the general assumption was that Nash was the author.

Update: Princeton has now made available publicly Nash's graduate school records, see here.

Comments

1. **Luca Signorelli**
   May 28, 2015

   Wow. It's stuff out of a Philip K. Dick late novel, like "Valis".

2. **andrew**
   May 28, 2015

   I'm not sure I'm comfortable reading his blackboard notes – I presume he wrote them when he was unwell. The notes might gratify a crass curiosity about his mental illness (e.g. "just how nuts was he?"), but what can we actually learn about Nash or mental illness from them?

3. **Peter Woit**
   May 28, 2015

   Andrew,
   Nash wrote them to communicate with others, I don't think his privacy is being invaded by reproducing them. Yes, he was ill, and they are symptoms of his illness, which they give some insight into. One might legitimately take the attitude this is none of our business, or also legitimately I think, instead want to understand more about what he was going through.

4. **andrew**
   May 28, 2015

   Peter. In a thorough biographical piece about Nash, the notes might help to form a complete understanding of his state of mind, but without that context they aren't particularly valuable, and, in my opinion, are in slighlty bad taste.

5. **Luca Signorelli**
   May 28, 2015

   Andrew I strongly disagree about the bad taste. As Peter said, they're attempts to communicate with the public, not private writings. And the biography and ilness of Nash are well known to the public.

6. **Peter Woit**
   May 28, 2015

   andrew,
   I would think that anyone interested enough to read these things would have some interest in understanding Nash and his state of mind. I do recommend again to anyone with such an interest to read the Sylvia Nasar biography, which is quite good (and evidently includes some other samples of this kind of writing by Nash).

7. **andrew**
   May 28, 2015

   Peter, we can agree to disagree. I will continue to enjoy reading your blog. All the best.

8. **S. Molnar**
   May 28, 2015

   I believe the Nasar biography contains not just other examples of the blackboard writings, but some from this set. As far as I know, Nash did not object to this, and it's my understanding that he and his wife were active to some degree in publicizing mental health issues (someone can correct me if I'm mistaken on either count). It seems to me that there are two reasons for suppressing this material: general privacy concerns, which I believe Nash himself did not see as a problem, and a sense that there is something shameful about them, which I feel is akin to blaming the victim. I don't doubt Andrew's sincerity and good intentions, but I'm not convinced his prescription is applicable here. I freely admit to being less than well-informed on this particular case, but I don't see the difference between hiding the public writings of someone who is mentally ill and, say, hiding the evidence of partial paralysis of someone, possibly a president, who suffered from polio.

9. **Peter Woit**
   May 28, 2015

   S. Molnar,
   I believe these were one of the sources Sylvia Nasar had access to, so that's why some of them are in her book.

10. **Jim Conant**
May 28, 2015

I don't think these are in bad taste. If they were accompanied by snarky or derisive commentary, that would be a different story. Not only do these give some insight into Nash's condition, but I also think they read like found poetry.

11. **Oyster Boy**
May 29, 2015

Let me just put it this way… I wish there were a better record of a period in my life when my perceptions were not shared by others who were in a better position to define "reality". And I wish there was less stigma surrounding talking about it openly. To sweep the unsettling bits under the rug is more a disservice than to allow them broader display in the light of day.

For those who knew me before and after, respect was never diminished. And I later achieved an even higher level of general public respect. It was only the strangers, and the social protocol of not talking about it that made me realize how real the stigma was. Do not hide it away. Promote candid conversation and awareness.

12. **Roger**
May 29, 2015

If you are concerned about Nash's privacy, then you must surely object to Nasar's unauthorized biography of him. It had a lot of dubious and privacy-invading allegations against Nash.

13. **Mark Schneider**
May 30, 2015

There has been enough conversation about the appropriateness of posting this material that I thought it might be helpful if I gave my view on this, since I not only assented to the posting, but was originally motivated to record them.

Let me first relate a parallel story. Both my mother and her best friend, in close sequence, suffered from and ultimately succumbed to dementia, probably Alzheimer's disease. As their rationality was stripped away, there were other aspects of their personalities that remained, made even more visible in the absence of cognitive abilities that we use to mask our true emotions. My "aunt's" deep and authentic kindness remained in her actions and comments, and my mother's ability to be simultaneously gentle but firm remained, expressed in frustrations with my father that I had not really understood when she was well, but which were later confirmed by a close relation after my mother's death. And yes, both of these women exhibited these traits in behaviors that were comical as well as tragic; being able to laugh a bit eased the pain of seeing loved ones slowly leave us, and never diminished our respect for them.

Why did we want to record these writings that were almost certainly the work of John Nash? I won't claim to speak for Steve, but let me go beyond my slim quotation in Sylvia Nasar's book. Yes, many of these writings were funny. But at the same time there was an exceptional ingenuity to them, and perhaps some insights into Nash's personality. It would be easy to look at his brief PhD dissertation and picture him as a brilliant individual who just tossed it off in a couple weeks work in a break from playing Hex. But I suspect the detail work that was evident in his board writings (even in his careful handwriting and layout, which I attempted to reproduce faithfully) was characteristic of the meticulous, time-consuming effort that he put into all of his work, regardless of his health. I recall times when a curious graduate student would erase a word or two of a board writing, only to find it scrupulously restored shortly thereafter. I don't think it is too much of a stretch to argue these are personality insights that are more difficult to obtain from a healthy individual, who is concerned with maintaining public perceptions of casual brilliance. Maybe one could even infer from the persistent theme of world leaders that Nash hoped that his insights could be useful in bettering the globe.

And finally, for those of you who have also followed some of the comments on the NY Times article, there was considerable criticism of the Nobel committee for being slow and hesitant to award the prize to Nash, seeing this as unjustified discrimination against him for an illness over which he had no control. Someone unfamiliar with Nash might think that his condition was akin to folks we may know who suffer from depression, or bipolar disorder, but are largely able to function. But the Nobel committee, I'm sure, worried about the highly public nature of this award, probably the highest intellectual honor in the world. Would having Nash at the ceremony be so destructive as to unfairly detract from the honor for the rest of the awardees? Moreover, would such an award to an individual who has lost a grip on reality be a public relations nightmare for the Nobel prize? I am not suggesting I know the right answer to these questions, but I respect them as being very thorny questions. The content of these board writings might help people understand the difficulties faced both by John Nash and the Nobel committee.

14. **David Derbes**
May 30, 2015

@Roger,

Sylvia Nasar had, I believe, the complete cooperation of Alicia Nash (who comes across in Nasar's biography as a woman of great kindness and strength) and at the least was not opposed by John Nash, who at the time of the biography, seems to have been, to a first or better approximation, his healthy, pre-psychotic self. (I should mention that I responded to a request in the Princeton Alumni Weekly from Nasar and wrote her a letter about my extremely limited knowledge of Nash in the early 1970's. Some of that appears in her book.)

When Nash wrote his blackboards, they were clearly intended to be seen. And after he returned to health, he could certainly have let it be known that he would prefer them to be forgotten. I don't think there is either an invasion of privacy or bad taste here. If we don't know what the phenomenon is, we have no hope of understanding it, perhaps with a hope of lessening its effects on others.

I don't want to name names, but there is (or was; I do not know if he is still alive) in my neighborhood a near-clone of Nash's case, a brilliant and highly respected professor at the University of Chicago who seems to have succumbed to schizophrenia. For years he wandered the streets of Hyde Park aimlessly with an upright umbrella and an increasingly filthy handkerchief held in front of him, singing opera. I think Princeton did enormously better by John Nash than Chicago by its former biology professor. Probably the signal difference was Alicia Nash. I do not know if the Chicago professor had as stalwart a wife (or any wife) as John Nash did. With luck some psychiatrists will learn a little more about this terrifying disease from the Nash record, including the blackboards, and maybe begin to understand it a little better.

15. **John Lowery**
May 30, 2015

Waiting for more information on Nash's reformulation of Einstein's Theory of Relativity. Anyone know more about this as seen in the Daily Mail(UK):
'….The 'Beautiful Mind' mathematician John Forbes Nash Jr. (pictured), who died last week, had told a friend he had discovered a replacement equation for Einstein's theory of relativity. Award-winning mathematician Cédric Villani said Nash had explained the work on Einstein's theory to him three days before his death. Nash and his wife Alicia, 82, were killed in a taxi crash in New Jersey in the US last week…'

16. **Thomas Hannagan**
May 30, 2015

See here for Villani's talk at the Hey Festival:

https://www.hayfestival.com/p-9799-cedric-villani.aspx

The Nash discussion starts at about 45′.
Looks like Nash discussed an equation bearing on general relativity, which he had apparently showed to Einstein in his time, but Villani does in no way mention any "replacement for Einstein's theory of relativity"…

17. **Andy P.**
May 30, 2015

David Derbes : I remember the guy you're talking about from Hyde Park. Over the five years I was in graduate school at Chicago, we frequently encountered him on the street. Twice he freaked out and started screaming obscenities at my children. I remember being very surprised to learn (from a fellow student, later confirmed via internet searches) that he was a professor and not a random homeless person. I have never heard stories about Nash behaving aggressively like that, which is perhaps one of the reasons that Princeton tolerated him more than Chicago tolerates their former faculty member.

18. **Steven Bottone**
May 31, 2015

Hopefully I will only have to use this term once, and I will put it in quotes: I will not conjecture on "mental illness". When (presumably) Nash placed these writings on the blackboards in Jadwin and Fine Halls, he most certainly did so for others to read. For those not familiar with these buildings on the Princeton campus, let me point out these writings appeared on the blackboards that lined the hallways, stretching from floor to ceiling, and in full public view. It is a fair assumption to make that whoever wrote these messages meant for them to be read by all passing by.

As Mark pointed out in a previous comment, these writings were meticulously composed and quite carefully formatted to fit neatly, yet artistically, into the space available. The handwriting was so distinctive that it would be nearly impossible mistake these notes for those of another author. Indeed, we would occasionally see attempts at imitating these writings, but there was no mistaking which of these were authored by the person who has been described as "The Phantom of Fine Hall".

I would say that most of our fellow graduated students were genuinely intrigued by these writings and it was always the case that if I walked by a new contribution I would stop to read it and if I were with others they too would stop to read it. Comments were often of the form as to the meaning of these messages and even if on the surface it appeared that the comment was one of ridicule, there was nevertheless invariably an undertone of awe.

I prefer to look at these writings in the spirit that I think Nash posted them. I think he wrote them for his own amusement, and for the amusement of the rest of us. They are meant to be satirical, usually political, commentary. They are funny. I ask those of our literary friends out there to comment on the content of these writings based on their own merit. Do they show great wit? Is this the work of a great satirist, who actually has an almost unique breadth of knowledge of political history, popular culture, and mathematics? This is how I prefer to view these fascinating musings.

19. **Neil**
June 1, 2015

I find these musings most interesting, but baffling. One cannot dismiss them as ramblings without some context, and Nash did not provide any. But one can do some interpretation based on the time they were written. For example, lon nol almost certainly refers to the Khmer president. It would have been witty, and characteristic, had Nash added the comment "ceci n'est pas un palindrome", so he was clearly not in his best form.

I was disappointed so many comments were political and social commentary, rather than mathematics. I was hoping for the bones of some brilliant conjecture.

20. **Curious Wavefunction**
June 1, 2015

To be honest, that statement by Nash about Ford and Nixon is one of the most insightful statements about American politics of the last thirty years that I have seen.

21. **SeekingSanity**
June 3, 2015

Invading privacy? Come on. These notes were written on public blackboards.

Lets have some perspective here. Yes you can talk about privacy, but these notes hardly cross the line. Especially since the person in question had an biography and indeed a major motion picture on their very private life released to the general public.

There's also a clear public interest element in publishing these. Someone who wrote these was a tenured professor at the institution in question for many years. This can be taken in many ways; 1) It was dangerous to do so, 2) it shows that it was not dangerous, but either way publishing these allows people do debate based on something concrete.

Personally I think it shows just how far academia can tolerate eccentricity, and that's a good thing within reason. Especially as we enter an age of increased careerism and corporatism in higher education. The ability to have a professor like could represent a strength of the traditional academy — though I'm sure many would now be happy to spin it as a weakness.

LHC Run 2

June 2, 2015
Categories: Experimental HEP News

Run 2 of the LHC is about to start with first stable beams scheduled for Wednesday morning, Geneva time). If you're up (I'll be asleep) you can watch [a live webcast], or watch what is going on [here]. The current plan is 3 bunches/beam Wednesday, 13 bunches/beam Friday, and 48 bunches/beam over the weekend.

Tomorrow will also be an [LHCC meeting], which you can also watch live. It will include reports from the experiments, and a status report about the machine which should give the latest details about the planned schedule for ramping up the intensity over the next couple months.

For the best advice about what to look for in coming months, see Jester's summary [here]. First new results may well be about gluinos.

This week there's a workshop going on at Nordita. On Thursday Gordon Kane will explain how [string theory predicts that the LHC will see superpartners soon]. I gather his claim is that gluinos are at 1.5 TeV, just above the Run I limits of around 1.4 TeV, so a sure thing for Run II. Of course, [back in 1997] he was claiming they were at around 250 GeV, just above Run I limits, a sure thing for Run II (but that was the Tevatron…).

Update: Kane has very specific [string theory predictions for Run 2]: gluinos at 1.5 TeV, winos at 620 GeV (+/- 10%). So, I guess string theory is going to finally be tested by the LHC over the next year or so…

Comments

1. **SRV**
   June 2, 2015

   Why does Gordon Kane keep making irresponsible 'predictions' that aren't firm??

2. **Peter Woit**
   June 2, 2015

   SRV,

   Because he can? A more to the point question might be why people keep inviting him to do this. For example, long after publishing the failed Tevatron predictions, Physics Today invited him to do it again, see here
   http://www.math.columbia.edu/~woit/wordpress/?p=3236

3. **gluinos**
   June 2, 2015

   what are the implications to HEP if LHC run 2 finds no gluinos?

4. **Peter Woit**
   June 2, 2015

   gluinos,
   That's a very interesting question that I think we're going to find out the answer to. Gluinos are both easy to see (since strongly interacting), and standard arguments for SUSY (eg "naturalness") don't allow you to push their mass up too high. So, they're the most likely ones to be seen, given standard expectations about SUSY. If they're not seen, likely there will be no superpartners seen.

   Kane is an extreme case, it's quite clear what his reaction is going to be ("gluino mass is just above 2 TeV, will be seen at the HL-LHC"). Much more interesting will be how others react to this situation. Will they just say they believe in SUSY no matter what (John Ellis seems to be planning to go that route), or will the negative experimental results change their attitude about SUSY? Some of them will have to pay off bets they have made about this, perhaps that will have some effect.

5. **Eduardo Lira**
   June 2, 2015

   "First new results may well be about gluinos".

   Dr. Woit,
   I think I remember you wrote in your blog not too long ago that no matter what energy they revved up the LHC to no superpartners would ever appear. Am I missing something?

6. **Mimoune**
   June 2, 2015

   Peter

   I thought you may be like to read this

   https://www.quantamagazine.org/20150527-a-new-theory-to-explain-the-higgs-mass/

7. **Peter Woit**
   June 2, 2015

   Eduardo Lira,
   What I meant is just what Jester was discussing: first new results are likely to be stronger limits on gluinos than the limits from Run 1.

   Mimoune,
   I did see that. I think I agree with Arkani-Hamed: "far-fetched"…

8. **you'll_never_walk_alone**

June 3, 2015

Because of the hierarchy problem, until we find them or reach the highest energy scales, the most probable place for sparticles is always just around the corner. That might be frustrating, but it's how it is.

LHC results should barely impact your preference for SUSY over the SM alone – even with all the LHC results, the simplest SUSY models are much more probable explanations for the smallness of the weak scale than the SM.

Kane's remarks are easy to ridicule, but they're not actually that foolish or silly.

9. **David Roberts**
June 3, 2015

And stable beams are up, with collisions going ahead!

10. **Peter Woit**
June 3, 2015

YNWA,

So, SUSY is a theory for which there is no evidence, which always predicts that it will be vindicated "right around the corner", and when it isn't, that has no negative impact on the idea at all. Somehow Kane has always neglected to explain this.

If you want to make particle theory an object of public ridicule, that argument seems to me an excellent way to do it.

11. **you'll_never_walk_alone**
June 3, 2015

There is enormous evidence for SUSY versus the SM. The SM's generic prediction for the weak-scale is totally wrong, $M_Z \sim M_P$ (of course it can be tuned to get it right though). The LHC results should decrease your belief in SUSY versus the SM, but only by a jot. In order to make SUSY as bad at predicting the weak scale as the SM, you'd have to exclude sparticles up to near the Planck scale. That's simply a reflection of how bad the SM is at predicting the weak scale.

12. **Peter Woit**
June 3, 2015

The SM doesn't predict the weak scale at all, and, by your argument, neither does SUSY.
This reminds me of the argument that, since the SM doesn't predict the CC, but SUSY does, SUSY is much better, even though the prediction is off by some exponentially large factor. To my mind that doesn't count as "enormous evidence" for SUSY vs. the SM, but the opposite.

13. **you'll_never_walk_alone**
June 3, 2015

The SM (interpreted as an effective theory valid up to around the Planck scale) does predict the weak scale – its generic prediction is totally wrong $M_Z \sim M_P$. That's the essence of the hierarchy/fine-tuning/naturalness problem in the SM.

A SUSY model also predicts the weak scale and its generic prediction is much better (its prediction is that $M_Z \sim M_SUSY$, where M_SUSY could be anthing up to the Planck scale.

This comes from a place of respect – you must see that there are good reasons for believing that sparticles are around the corner, even if you don't find them compelling.

14. **Peter Woit**
June 3, 2015

YNWA,
I don't think the SM has anything to do with the Planck scale. You can get a bad prediction by making various assumptions about what is happening at the Planck scale and interpreting the SM in terms of those, but that's a problem for your assumptions about the Planck scale, not for the SM.

I also don't think the arguments for SUSY were good pre-LHC (and wrote a chapter about this in my book many years ago). For fans of these bad arguments, the LHC gave some hope of vindication anyway. That's now pretty much gone, and best that be admitted, not replaced by new "right around the corner" claims.

15. **M**
June 3, 2015

If I remember correctly, around 2000 Gordy was saying that he would have abandoned SUSY if it was not discovered at Tevatron run II.

16. **you'll_never_walk_alone**
June 3, 2015

All right, let's leave it at that. I'll try to read your book sometime.

17. **Anonymous**
June 3, 2015

"A SUSY model also predicts the weak scale and its generic prediction is much better (its prediction is that $M_Z \sim M_SUSY$, where M_SUSY could be anthing up to the Planck scale."

Is there actually a hard argument that it cannot be above the Planck scale (other than that all bets are off at the Planck scale)? If not, your "prediction" is simply that the weak scale is a scale. I find the fact you are trumpeting this as some kind of success hilarious.

The hierarchy problem is a case of one free parameter, when extrapolated over 16 orders of magnitude in energy, taking on a value we do not understand. The Standard Model also has 25 other free parameters with values we do not understand the reasons for, some of which also look suspicious. Increasing the number of free parameters by 100+ in order to sort of account for the value of one parameter does not look like progress.

The SUSY may be always just over the horizon to those who love her truly, but increasingly the rest of us simply won't care. This certainly looks like it will be the case if it doesn't show up in the next couple of years.

18. **gluinos**
June 3, 2015

Peter,

What is the upper limit of Gluino masses that is allowed under naturalness/weak scale hiearchy argument? What is the upper limit of gluino masses LHC Run 2 13TEV can produce and detect? How soon will these results be announced, given LHC Run 2 starts today and its projected luminosity? thanks regards

19. **Peter Woit**
June 3, 2015

gluinos,
The talk by Mike Lamont today said 5-10 inverse fb this year, 1 inverse fb during the initial 50 ns part of the run (early July). So, won't match the amount of 8 GeV data until sometime next year. 1 inverse fb should be about enough to match the previous bounds (1.4 Gev), so that may happen by the end of the summer. I'd guess by sometime next year you'll see 2 GeV bounds.

The problem is that there is no upper limit. If you believed the naturalness argument, we were supposed to have seen these things long ago, at the Tevatron. The people who didn't give up after the Tevatron/LEP and insisted that LHC energies were needed are now all gearing up to argue that nothing at the LHC is still no reason to give up, that what is really needed is a 100 TeV machine.

20. **Tim**
June 3, 2015

Here are my predictions:

September 2015 – Z' boson not discovered at LHC
March 2015 – no sign of gluinos at LHC
Summer 2016 – still no axions seen at LHC
2017-2018 – strong evidence that dark matter particles, whatever they are, are not produced at LHC
2019 – "Theory of Everything" fever finally collapses, Nobel Prize awarded for "Theory of Nothing", which explains why there is probably nothing to see between 1-100 TeV.

21. **you'll_never_walk_alone**
June 3, 2015

To my anonymous critic. You want to know what happens if we permit the SUSY scale, and presumably also the cut-off in the SM, to be greater than the Planck scale (let me call this high scale Λ). In the comparison of SUSY versus the SM, not much changes – the SM predicts that $M_Z \sim \Lambda$, whereas SUSY predicts that $M_Z \sim M_SUSY$, where M_SUSY is anything less than Λ.

The point isn't that SUSY predicts the weak scale spot-on – it doesn't. The point is that a SUSY model's generic prediction is more compatible with what we observe than the SM prediction. The smallness of the weak scale is much more probable in a SUSY theory.

You compare the dim-2 Higgs coupling with the SM's many other unexplained parameters, such as the Yukawas. If we can't explain the Yukawas, who cares if we can't explain the dim-2 coupling? But we don't know any theories that make concrete, correct predictions for the measured Yukawas (without parameter fitting). If we knew of such theories, I'd definietly favor them over the SM (especially if they were supersymmetric).

22. **SRV**
June 3, 2015

Wouldn't you say that supersymmetry is a 'prediction' of string theory?

23. **Peter Woit**
June 3, 2015

SRV,
Personally I have been consistent in claiming that string theory makes no predictions. In the past string theorists have often responded to the "no predictions" claim by saying "string theory predicts supersymmetry". See for example
http://www.math.columbia.edu/~woit/wordpress/?p=3904

Ever since results from the LHC started to come in, you hear this claim a lot less often…

24. **Alex**
June 3, 2015

YNWA,

What experimentally verified predictions does SUSY give that the SM doesn't?

25. **Thomas Larsson**
June 4, 2015

I have realized that I am probably the most successful string phenomenologist on this planet. Not famous or prominent, but successful in the sense that used to count as success in pre-post-modern physics: agreement with experiment. After all, who else has managed to use string theory to make a falsifiable but confirmed prediction about the LHC?

26. **Mesmar Djehha**
June 4, 2015

Given the role supersymmetry plays in modern mathematics, would the nonexistence of supersymmetry modify our view on the relation between physics and mathematics?
Thanks

27. **Anonymous Critic**
June 4, 2015

"The point isn't that SUSY predicts the weak scale spot-on – it doesn't. The point is that a SUSY model's generic prediction is more compatible with what we observe than the SM prediction. The smallness of the weak scale is much more probable in a SUSY theory."

My issue initially was that you described the postdiction that a number is between zero and infinity as being a successful prediction. The fact is that this model is set up entirely to solve this "problem" – the fact that it can accommodate a solution is not then evidence that this model is in any way correct.

I agree with you that there aren't any known models that successfully reduce the number of free parameters from the SM – but this does not mean that such models are not realized in nature. The fact that models designed to solve the hierarchy problem alone continue to not show up in nature should maybe be taken as a sign that what we are doing now is not the right approach. If we continue to make the "just over the horizon" argument, and learn absolutely nothing from experimental results, it will ultimately kill the field.

28. **Peter Woit**
June 4, 2015

Mesmar,
No. The specific supersymmetry algebra that has been conjectured to be relevant for particle physics, to some extent being tested by the LHC, is only one very special example of the general phenomenon of supersymmetry and supersymmetric QFTs. That this specific example isn't relevant to the real world doesn't affect the mathematically interesting aspects of supersymmetry (this example was never especially interesting mathematically). It might even have a positive effect, encouraging people to pay more attention to aspects of supersymmetry that are mathematically interesting, rather than this particular one which just had a failed idea about physics associated with it.

29. **gluinos**
June 5, 2015

Update: Kane has very specific string theory predictions for Run 2: gluinos at 1.5 TeV, winos at 620 GeV (+/- 10%). So, I guess string theory is going to finally be tested by the LHC over the next year or so…

^

do you agree with Kane? What happens Kane's prediction if no gluinos or no winos.

30. **Peter Woit**
June 5, 2015

gluinos,
I can predict with 100% certainty what will happen once Kane's prediction doesn't work out: he'll come up with another prediction. A couple years from now he'll be predicting gluinos just above 2 TeV, too heavy for Run 2, but a sure thing for the HL-LHC.

31. **Egon**
June 5, 2015

If no gluinos or winos, Kane revises his model and makes another prediction. In his paper on G_2-MSSM, he says that the entire sparticle spectrum is determined once you take into account the electroweak symmetry-breaking scale, the Higgs mass, and the gravitino mass (which he calculates approximately), but the details depend on the discrete choice of the 7-dimensional compactification manifold, which changes the hidden sector gauge group and the parameters in the formula for the gravitino mass: http://arxiv.org/abs/1408.1961

At best, the prediction of a 1.5 TeV gluino mass could be considered a test of the very specific string-inspired model that Kane is promoting, which is essentially a particular instantiation of SUSY SU(5), but even within his G_2-MSSM model there is room for adjustment.

32. **John Baez**
June 5, 2015

I've become increasingly suspicious of "naturalness" or "no fine-tuning" arguments. They often seem to amount to saying that dimensionless constants are more likely to be near 1 than enormously large or enormously small.

This might be a fine heuristic when we're completely clueless and desperately need any heuristic we can grab ahold of – but is there any reason to believe it's *true?* Can someone point to a reference where someone enthusiastically and intelligently defends this sort of argument? Or alternatively, attacks it?

33. **David Roberts**
June 5, 2015

The thing I find sad is when people say "look! No free parameters!", when they have chosen a geometry on which to dimensionally reduce (or similar: a choice of non-compact space, a choice of structure group,…). Do they really believe that a moduli space of Calbi-Yau manifolds is different to a parameter space of coupling constants? That a discrete choice of homotopy type of manifold, of isomorphism class of a Lie group is different from a choice of real number?

34. **srp**
June 5, 2015

As a geometry guy don't you realize that geometry always seems more natural than real analysis? Visualization!

35. **Urs Schreiber**
June 6, 2015

@John, regarding naturalness: I thought the discussion by Strassler here wasn't too bad: http://profmattstrassler.com/articles-and-posts/particle-physics-basics/the-hierarchy-problem/naturalness/

@David, regarding free parameters: of course moduli are also parameters, but they are not free parameters, instead they are dynamical fields. While you may start out selecting moduli in string theory just as you select free parameters in field theory, afterwards you are required to check that the values you chose constitute a solution to the theory (background equations of motion+quantum corrections+anomaly cancellation). Notably if you want to assume that the moduli won't evolve away from the values you'd like to consider, then you need to check that they sit in their potential well, hence that they are stabilized. Discussion of moduli stabilization is a huge topic (landscape and all). Beware that the full constraints for consistent choices of moduli in string theory are strong, hence solving them is hard, and accordingly it is in practical approximation rather than in fundamental meaning that glancing through the literature you might get the impression that some people choose their moduli as if they were free parameters.

36. **Urs Schreiber**
June 6, 2015

@John, sorry, forgot to add: a pronounced criticism of the traditional naturalness argument was voiced by Wilson, see p. 10 here http://arxiv.org/abs/hep-lat/0412043

37. **Peter Woit**
June 6, 2015

Urs,
You're neglecting to mention that there's a minor problem with the "moduli are dynamically determined" argument: you don't know what the dynamics are. The "moduli stabilization" story is a very complicated one, and not because you know what the underlying theory is and are having a hard time solving it…

38. **David Roberts**
June 8, 2015

@Urs

Mentioning 'moduli' was a little risky, but I mean the moduli space of *all* CY manifolds. I had hoped mentioning homotopy types makes it clear. Unless changing homotopy type is part of the dynamics (which it may be, who knows) I don't see how picking a homotopy type of manifold (and then of course complex structure etc, which can evolve over time) is not a choice.

39. **Urs Schreiber**
June 9, 2015

@David, yes change of homotopy type (aka topology change) of spacetime is part of the dynamics of string backgrounds, see e.g. the references here: http://ncatlab.org/nlab/show/flop+transition .

But this is tangent to your quarrel about the common usage of the term "free parameters". Consider plain GR. It's free parameters are the gravitational coupling constant, the cosmological constant and the prefactors in front of all higher curvature corrections. What is not called a free parameter of GR is the homotopy type of spacetime (what physicists call "the topology"). Instead, this is part of what it means to have a solution to the theory.

In string theory all those prefactors in the effective Lagrangian are fixed, in M-theory also the global coupling is fixed, you may not choose these parameters freely, their values are fixed by a more fundamental principle. Instead, there is now a richer space of solutions of the theory, which in suitable limits look like some effective field theory with free paraneters. But now all these parameters are actually parameters of the solution space of the UV-completing string theory (its moduli) and hence are dynamical fields.

See also the string theory FAQ http://ncatlab.org/nlab/show/string+theory+FAQ#IsStringTheoryTestable

40. **David Roberts**
June 10, 2015

@Urs

thanks for the clarification. I guess GR is qualitatively different at this point in time in that we have (local) solutions that correspond to measured reality, and one can calculate a solution then go measure how accurate it is.

We aren't yet at the point with string theory that Eddington was at when he measured the effect of gravity on light (if we even get there!) in the sense that it was a new prediction that could make or break GR. I guess it's more like when people predicted the Omega^- based on what we now know as representation theory of the structure group of the gauge bundle, but at the time would have been (for mathematicians) very shaky justification. At that time, Gell-Mann and Ne'eman got lucky, in that they had sniffed out what was going on, and the details filled in later. Now, on the other hand, people have no single idea (rather a giant space of field theories-worth) what specific geometric structure should give what we see at the LHC (the analogue of SU(3), or the Schwarzschild metric), if there even is one, and so are clutching at whatever they can calculate.

Time will tell, perhaps.

41. **Urs Schreiber**
June 10, 2015

@David, your issue about the technical meaning of the technical term "free parameter" has nothing to do with phenomenology, it applies to, say, the Ising model as well as to any other realistic or nonrealistic model, and as such is not qualitatively different in gravity, no, on the contrary, it is precisely the free parameters of Einstein-Yang-Mills-Dirac-Higgs Lagrangians that are meant when people say that string theory has no such. This is a mathematical statement about certain functionals which is entirely independent of any phenomenology. While I doubt here the right place here for the discussion we are having, it's good that you voiced your confusion about this common terminology, because without such basics sorted out, there is no educated assessment of the theories under discussion.

42. **David Roberts**
June 10, 2015

@Urs thanks again for clarifications. I'm not sure my issue is only with 'free parameters', but as you say, perhaps this is better discussed elsewhere.

43. **Tommaso Dorigo**
June 12, 2015

Here's two predictions that Gordy Kane made in August 2011 to me privately (I then blogged about it):

———
I thought it was worthwhile to comment a little on recent LHC searches, since they have led to a number of surprising statements. First consider gluino searches. The results of a search for gluinos are very sensitive to squark masses. Theoretically the only well motivated values for squark masses are very large, tens of TeV, because they are generically predicted in compactified string/M-theories when the associated moduli satisfy cosmological restrictions. Then (a) the gluino production rates are considerably reduced, and (b) the decays or gluinos to 3rd family final states dominate. Existing gluino searches cover this region poorly. The current limit on gluino masses is not above 500 GeV. Whether the squarks are indeed so heavy is not the issue, the point is that if they are the limits on gluino masses are smaller than is often stated. I and others excpect this decay to tops and bottoms is the signature by which gluinos will be found, with masses well below a TeV.

Second, when squarks are heavy the two doublet Higgs sector is an effective single doublet since the heavy partners decouple. There is a single light Higgs boson observable. If the gauge group of the theory is the MSSM one then the Higgs mass is between about 115 and 128 GeV (essentially a function of the parameter tan(beta)). It will not be above that range. It has the SM production rate. The LHC searches are not yet sensitive to this region, and should not yet have seen a signal, so not seeing a signal does not allow any meaningful conclusions about Standard Model or MSSM Higgs bosons.
———

As you see he got one wrong and one right 😉

Cheers,
T.

The West Coast Metric is the Wrong One

June 5, 2015
Categories: Favorite Old Posts

I'm trying to get back to blogging about quantum mechanics slogans, this one is about relativistic quantum mechanics. I'm hoping it will stir up more trouble than [my last East vs. West one](#).

If you're doing calculations in relativistic quantum field theory, you typically handle the Minkowski nature of space-time by introducing an indefinite signature metric, and there are two possible choices:

- Mostly minus signs (for the spatial components), positive sign for the time direction. This is commonly known as the "West Coast metric", I'm guessing because Feynman used it.
- Mostly plus signs (for the spatial components), negative sign for the time direction. This is commonly known as the "East Coast metric", I'm guessing because Schwinger used it.

While I was educated on the East Coast, most courses I took and textbooks I used favored the West Coast metric, and that's very much true of more recent textbooks (I don't know of a recent one using the East Coast convention). When I started on [this project](#) I used the West Coast convention. After a while though, I finally found this conceptually more and more confusing, and switched to the East Coast convention. As time has gone on, I've become more and more convinced that this is the right convention to choose, that the West Coast convention is just a mistake, and a source of conceptual confusion in the subject.

Here are some reasons:

- With the East Coast convention, the treatment of spatial coordinates is just like in the non-relativistic case. In the West Coast convention, as far as space goes, you have decided to work with a negative definite metric, which is a quite misguided thing to do for obvious reasons.
- With the East Coast convention, if you do what you always do to make a QFT well-defined, analytically continue to imaginary time, you end up working with the standard Euclidean metric in four dimensions. In the West Coast version, you end up with a negative definite metric, again a bad idea (thanks to Peter Orland for emphasizing this to me). You could instead do your analytic continuation by analytically continuing all three of the space coordinates, also a really bad idea.
- With the East Coast convention, the Clifford algebra Cliff(3,1) is the algebra of real four by four matrices. In other words, you can choose your gamma-matrices to be real matrices and work with a real spinor representation (the Majorana representation). Going with the West Coast, Cliff(1,3) is the algebra of two by two quaternionic matrices, a confusing thing to work with. Ignoring that, what physicists end up doing is working with gamma matrices that are pure imaginary, which is highly confusing (odd powers of gamma matrices are pure imaginary, even ones real). [According to Figueroa-O'Farrill](#), in this case you are working with:

  > pseudo-Majorana spinors – a nebulous concept best kept undisturbed.

  To be fair, for this problem you can do what he does, and just change the sign in your definition of the Clifford algebra.

- Weinberg's quantum field theory textbook uses the East Coast convention.

One reason this issue came to mind is that I've been trying to understand (not very successfully…) Schwinger's old papers on Euclidean quantum field theory, where he makes some quite interesting claims. Schwinger used the East Coast convention for this, and as explained above, it's only with this convention that you get something sensible after analytic continuation in time. There is very little literature following up on Schwinger's arguments, I'm suspecting partly because in the West Coast convention following Schwinger's arguments becomes virtually impossible.

The problem here is part of a more general problem, that I think most physicists don't appreciate the mathematical concept of "real structure" or complexification. Given any formulas, they're happy to just start using complex numbers, even when the quantities involved are real, with the idea that only at the end, when you get observable numbers, do you need to impose some reality condition. For an example of this, one often finds in qft texts claims that sound very strange to mathematicians, I have in mind especially things like

> The mathematically sophisticated say that the algebra SO(3,1) is isomorphic to SU(2) X SU(2) (Tony Zee, QFT in a nutshell, page 113 first edition).

One problem here is that Zee is using the standard math notation for the Lie group to denote the Lie algebra, an unfortunately common practice. But the real problem is that the two Lie algebras are only isomorphic if you complexify, as real Lie algebras they are quite different. If you have always from the beginning complexified everything, this distinction doesn't make sense to you, but it is often an important one if you want to really understand what is going on in some calculation involving spinors. For another example, there's

> the 3D rotation algebra, which has multiple names so(3)=sl(2,R)=so(1,1)=su(2), due to multiple Lie groups having the same algebra. So we have shown that so(3,1)=su(2) + su(2) (Matthew Schwartz, QFT and the Standard Model, Page 162).

Schwartz properly distinguishes by notation between the group and the Lie algebra, so is in better shape than Zee, but, again, so(3,1)=su(2) + su(2) is only true for complexified Lie algebras. The statement "so(3)=sl(2,R)=so(1,1)=su(2)" suffers from a typo (so(1,1) should be so(2,1)), but again an identification is being made that only makes sense if you have complexified everything. What's really true here is that you have SL(2,R) a double cover of SO(2,1), and sl(2,R)=so(2,1), as well as SU(2) a double cover of SO(3), with su(2)=so(3). But SU(2) is a very different Lie group than SL(2,R) (although they share the complexification SL(2,C)).

So, my modest proposal is that the HEP community should just admit that the West Coast convention was a mistake, and rewrite all the textbooks (Weinberg doesn't have to…).

Update: A commenter tells me there is at least one recent textbook with the right convention, Srednicki's.

Checking some books, I remembered one other intriguing recent choice, that of Michael Dine, who wrote the first half of his book (the QFT part) in the West Coast metric, but the second half (the string theory part) in the East Coast metric.

Update: For those interested in how to translate back and forth between Coasts in the two-spinor notation, I noticed that Dreiner, Haber and Martin have written review papers, with a line in the tex that lets you choose which Coast. See [here](#) and [here](#).

Comments

1. **auxsvr**
   June 5, 2015

   One very common mistake physicists make is that the ladder operators taught in quantum mechanics are elements of su(2), but they're elements of its complexification, as su(2) is a real algebra. This distinction is essential if one needs to study group theory in depth for quantum gravity etc.

2. **Peter Woit**
June 5, 2015

auxsvr,
Yes, that's the same issue I'm talking about. Raising and lowering operators occur in representations of the complexification sl(2,C), not of su(2).

3. **Peter Morgan**
June 5, 2015

The invariant distinction for 4-vectors (in flat Minkowski space) is between space-like definite, light-like, and time-like definite (or semi-definite or indefinite, and with similar distinctions for bivectors and for spinors), so I suppose it's better to formulate geometrical argument in these terms rather than in terms of +0- (or -0+). For the algebra, this is just one of very many sign and factor conventions (anyone for a five-minute argument about 2\pi?); we have no choice but to be clear which conventions we are using and to keep perfect track of them all. But I think signs and factors are the least of my worries when trying to understand other people's notations and formalisms (as you note above, people are not as clear as could be about whether they are working over the real or complex field).

4. **Sudip Paul**
June 5, 2015

Mark Srednicki also uses the mostly plus metric in his QFT textbook. Also I was always confused by this East Coast/West Coast nomenclature. Why can't people simply say mostly plus/mostly minus?

5. **Peter Woit**
June 5, 2015

Sudip Paul,
Thanks, Srednicki's book is one of the ones I don't have, I suppose I should get a copy. Glad to know that there's at least one recent example of the right convention.

Instead of East/West, or mostly plus/mostly minus, how about right/wrong?

6. **Alex**
June 5, 2015

While I agree with your conclusion, I think I should point you to this [preprint] indicating the Pin groups may cause physically-meaningful phenomena…depending on which signature you use…well, neutrinoless beta decay can *only* be described in the East coast metric.

7. **Gandalf**
June 5, 2015

Someone has to defend the "West coast metric" here, it seems. For in that metric, energies p^2 are positive, as they should be! Ok, you have to accept that spatial distances x^2 are negative .. you can't have it all. But who wants to impose a negative energy condition? I rather travel -1000 miles 😉

8. **Al**
June 5, 2015

Interestingly I've never heard of this "West/East Coast" nomenclature. Instead when I studied it, we had "the one that particle physicists use" (mostly minus) and "the one that general relativists use" (mostly plus). If I remember correctly, the mostly plus one stems from Minkowski's geometric formulation of special relativity (introducing imaginary time coordinate), so it's the one in use by relativists.

9. **Al**
June 5, 2015

By the way, that might explain Michael Dine's choice. When string theorists want to emphasize string theory's potential as TOE and connect it to gravity, they use mostly plus ("East Coast") one.

10. **John Baez**
June 5, 2015

I agree that it's better to take spacelike vectors to have v · v > 0, because this convention makes spacetime geometry backwards-compatible with the usual way of describing the geometry of space. This is how I do things in my book *Gauge Fields, Knots and Gravity*.

However, the convention for Clifford algebras requires another decision, as you note. And this decision is lot more interesting to me. Should a vector with v · v = 1 give a square root of 1 in the Clifford algebra, or a square root of -1? Thinking about the relation between Clifford algebras and division algebras, I decided that square roots of -1 were more natural here. You can see the thought process hiding [in here]: starting from the idea of a normed division algebra, you can quickly see that an n-dimensional normed division algebra is the same thing as an n-dimensional representation of a Clifford algebra generated by n-1 anticommuting square roots of -1. Another way to put it is that a skew-adjoint orthogonal matrix – that is, a matrix lying both in the Lie group O(n) and the Lie algebra o(n) – must be a square root of -1.

By contrast, your argument that 2×2 matrices of quaternions and purely imaginary gamma matrices are "confusing" seems based on taste rather than theorems.

(My argument that spacelike vectors should have v · v > 0 is also based on taste – or rather, history: I'm saying this is nice because it's the convention we're used to from ordinary Euclidean and Riemannian geometry. But my argument that square roots of -1 are better than square roots of 1 is based on theorems. The simplest of these theorems, of course, is that taking the real numbers and adjoining a square root of -1 gives a field, the complex numbers, while adjoining a square root of 1 does not.)

11. **Noboru Nakanishi**
June 5, 2015

The use of the words, "East Coast Metric"and "West Coast Metric", is unwelcome; high-energy physicists are not necessarily Americans. I once used the words, "Space-favored Metric" and "Time-favored Metric". The former is more convenient than the latter except in the momentum-space consideration. But it must be emphasized that this is the matter of convenience but not the matter of the right/wrong business.

As was noted by Schwinger (and independently by Nakano), the x of a field operator has no intrinsic metric signature in quantum field theory. People just introduce the Minkowski metric by hand. Similarly, in general relativity, the Einstein equation contains no information about metric signature. One just introduces the Lorenzian metric signature by hand as the boundary condition. From these facts, I proposed that the quantity x should be something living in the space which has no metric; concretely, the space of x should be an affine space. Time should be embedded into this space. I formulated quantum Einstein gravity on this standpoint. One of the consequences of this formulation is that SUSY is a wrong symmetry!

12. **Sudip Paul**
June 5, 2015

Just remembered, Polchinski's two volumes of String Theory are completely in the mostly plus convention.

Btw, you might wanna change the title of this post to "West Coast Metric Considered Harmful." 🙂

13. **Magnema**
June 5, 2015

Personally, I prefer the -+++ convention. However, I do see the following advantage in the +— convention: obviously, when it makes squared spacial intervals negative, it makes squared time intervals positive. Practically speaking, the only paths one cares (that I've seen in my studies of classical GR) are the geodesics of particles, which are timelike curves, thus why would would prefer timelike curves to have positive length.

14. **Peter Woit**
June 6, 2015

John,
I agree that the question of the convention for Clifford algebras is a much more interesting one. I've used both, when I first was teaching representation theory I used the negative one, see
http://www.math.columbia.edu/~woit/notes17.pdf
for pretty much the same reason you mention (that Clifford algebras are generalizations of C, or H, where the generators have square -1). More recently I changed to the positive convention, I forget exactly why. Bourbaki does use the positive convention, and this is the kind of thing they usually put a lot of thought into.

I do think it's not just a matter of taste that if you want to work over the reals with an algebra that is isomorphic to an algebra of real matrices, you should use real matrices, and not introduce an "i" into your calculations (e.g. when thinking about Majorana fermions using a Clifford algebra isomorphic to M(4,R)). It is a very interesting question though whether the right Clifford algebra to think about for Minkowski space is M(4, R) or M(2,H) (which is equivalent to the question of which sign to use), they are two different real Clifford algebras (and as Alex points out, the Pin groups are different). My understanding of Schwinger's argument was that he had a "Euclidean principle", that what you see in Minkowski space is only things that analytically continue from Euclidean space, and this explains why you don't see Majorana fermions (a real spinor representation). I don't know if his argument would change if he was using the opposite convention and the Clifford algebra was M(2,H).

15. **Robert Delbourgo**
June 6, 2015

I guess the metric you choose depends on how you were brought up, which then gets ingrained in your brain. I was brought up as a particle person so use the +— choice. It really does not matter all that much although I do appreciate Peter's arguments for the other choice.

16. **John Fredsted**
June 6, 2015

To mind comes the following passage from the monograph *Clifford Algebras and Spinors*, by Pertti Lounesto:

> Physicists might want to observe that the Clifford algebras Cl(3,1) = Mat(4,R) and Cl(1,3) = Mat(2,H) are not isomorphic as associative algebras, even though both of them have the same complexification M(4,C) …

17. **Ralph**
June 6, 2015

Much as I'm tempted to agree with you, the only really convincing point there is Weinberg.

18. **Mike Duff**
June 6, 2015

Type IIB supergravity is an example of a "bi-coastal" theory: its equations of motion are the same in either east or west conventions. This is because it exhibits a metric reversal symmetry g_ab goes to minus g_ab.

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19. **manfred Requardt**
June 6, 2015

I think, the dividing line runs typically between high energy physicists and general relativists. In QFT one usually likes to have timelike orbits of particles to have positive length, while in GR one likes to have the spatial metric to be positive.

20. **Anon**
June 6, 2015

Well, on the "west coast" (mostly minus) side, I'd like to note that even the venerable Wald switches to (+,-,-,-) when discussing spinors in curved spacetime (see the discussion under equation 13.1.18).

21. **Theo Nieuwenhuizen**
June 6, 2015

Einstein was thinking physically rather than mathematically. His Lagrangian has a positive kinetic term with a plus in front (west coast convention, being on the east coast). It is indeed annoying that the physical choice is connected with mathematical inconvenience. But "imaginary time" is indeed imaginary, it has nothing whatsoever to do with physics, even though Penrose may discover "physics" in this mathematical game. So the right choice depends on what you are. Physicists should go to the west coast, other can do what they want. (PS I started out on the east, but realized the betray.)

22. **tt**
June 6, 2015

imaginary time is used in stat mech.
http://en.wikipedia.org/wiki/Imaginary_time

23. **Radioactive**
June 6, 2015

Much as I hate to agree with Lubos, and I have the emotional intelligence to identify tongue-in-cheek remarks, but the choice is completely irrelevant and quibbling about sign conventions is best left to pedants who need to feel important at lunchtime talks.

24. **Peter Woit**
June 6, 2015

Radioactive,
Don't worry, I'll soon move on from technical pedantry and get back to blogging about less childish topics like theories of everything, the multiverse, etc….

25. **vmarko**
June 6, 2015

I've also never heard of the West/East coast terminology, and I think most people in Europe haven't either. There was always just a particle-physicist signature (+,-,-,-) and the gravity-physicist signature (-,+,+,+).

I was also first educated in the particle-signature fashion, but after started doing research (on GR and QG), I switched to gravity-signature, and never ever looked back. It's much more convenient, for all the reasons Peter mentioned, and then some (raising and lowering indices, discussing spatial hypersurfaces and the Cauchy problem, working with Kaluca-Klein scenarios, canonical quantization, Dirac's analysis of constrained systems, etc).

As for books, note also that the "GR-phonebook" (Misner, Thorne, Wheeler) uses the (-,+,+,+) signature throughout.

As for the energy, I don't quite understand what is the problem — energy can be (and should be chosen to be) positive in either metric, AFAIK. The overall sign of the action is what fixes the sign of energy, and this can be chosen independently of the metric signature. One chooses the ground state to be of minimum potential energy, i.e. the action to have a maximum, which is done by multiplying any given action with -1 if needed. This has nothing to do with the choice of the metric.

26. **Peter Woit**
June 6, 2015

Anon,
Thanks for pointing that out. Looking into it, it seems that Wald is doing this to try and follow Penrose, who I hadn't realized is in the West Coast camp, despite being a relativist. It's striking to see multiple instances of writers changing convention in the middle of a book, something that normally one would very much try hard to avoid. I suspect it is partly because the sign issues get very tricky when dealing with spinors, so authors decide it's just too hard to figure out how to change from one to the other.

27. **Kartik Prabhu**
June 6, 2015

Peter,
* The link to Figueroa-O'Farrill's notes is broken. You need a www. instead of ww. in the URL.
* I think the choice of mostly minus or mostly plus metric when not dealing with spinors, is largely an issue of convenience, nothing to do with positivity of energy or action terms.
* For spinors, the relevant issue is the relative sign choice in the metric and the definition of the Clifford algebra. Different relative signs would give inequivalent spinor representations as already pointed out. I don't know of a good physical argument for choosing one over the other, unless physics some how only cares about the representation of the complexified group in which case either convention is fine.
* As for Wald's switch in metric signature. That is done to use the 2-spinor notation where one wants to identify an abstract index "a" on the tangent space to a pair of spinor indices "AA′". In particular, one wants the metric \eta_ab -> \epsilon_AB \epsilon_A′B′ instead of with a minus sign where \epsilon is a real symplectic 2×2 matrix. Though I am sure this again has to do with a choice of sign for the defining equation of the Clifford algebra.
* But overall as pointed out in the notes by Figueroa-O'Farrill "There is no difference between 3+1 and 1+3—there never is at the level of Spin group" so maybe this point is moot!?

original post

28. **Peter Woit**
June 6, 2015

Kartik,
Thanks, fixed the link.

Ignoring spinors, for standard flat space calculations, clearly you can use either convention if you want to. I still think though that working with a negative definite metric (for space or analytically continued space-time) is always going to be at best confusing, if not worse, and that's what the West Coast convention makes you do. The issue about analytic continuation in QFT seems to me a real one, there's far too little clear discussion of the issue (non-perturbatively), and maybe none in the West Coast convention.

I would have thought you could do two component spinor notation with either convention, and a bit of googling shows that it has been done. See
http://www.niu.edu/spmartin/spinors/
and
http://www.niu.edu/spmartin/TASI11/
for details. These papers evidently include a tex file where you can change a single variable, and get the paper in either convention (I'll add this info to the posting).

Yes, for the spin groups, doesn't matter. The physical significance of the two different Clifford algebras and different Pin groups is still obscure to me.

To say that "physics doesn't depend on the real structure, just the complexification" I'd interpret as a claim that the physical picture for the physics of spinor fields in different real structures is related by analytic continuation. This may or may not be true, it's exactly what I'd like to understand better, but have been finding very little in the literature that addresses this.

29. **Kartik Prabhu**

June 6, 2015

Peter,

Being from a GR background, I agree that it is very convenient to work in the mostly plus signature and being in Chicago I'll skip the East/West dichotomy! 🙂

* I am not sure what significance one should attach to "Wick rotation" or analytic continuation procedures emplyed in textbook QFT. Firstly, those work only in analytic and static/stationary spacetimes, which again from a GR perspective is highly restrictive.

* Yes, one can do 2-component spinor notation in either signature if one wants to carry around the "sigma matrices". Penrose's notation gets rid of the sigma matrices entirely (See: Spinors and Spacetime Vol1 — Penrose & Rindler) and one can write equations like g_ab = \epsilon_AB \epsilon_A′B′ which works only in the mostly minus signature. If using a mostly plus signature this would have a minus sign which is very inconvenient. So this notation by Penrose sacrifices the goodness of the mostly plus metric for the goodness of skipping sigma matrices entirely, which is also the convention Wald uses. I have also seen Bob Geroch use this notation in his lectures. In the http://www.niu.edu/spmartin/spinors/ link that you cite this corresponds to their Eq.2.48 which does have a sign change when changing the metric signature.

* I still don't understand the physical significance (if any) of pinors. As far as I have seen, physics theories only use spinors and the Spin group in which case this sign choice is irrelevant.

* Again, I am pretty skeptical of the importance of analytic continuation to physics in general.

original post

30. **Peter Woit**
June 6, 2015

Kartik,
Thanks for the comments on the two-component formalism.

About analytic continuation, I realize this doesn't work in arbitrary backgrounds, but I do think it's a deep issue in QFT. As far as I've ever been able to tell, there really is no such thing as a rigorous, non-perturbative formulation of QFT that does not use the analytic continuation. Even in the simplest considerations of the free field propagator, you have to add some extra piece of information (how do you integrate through the poles) that is very nicely expressed by the analytic continuation requirement.

Anyway, analytic continuation issues are somewhat of a different topic, but the point here is just that they seem to not have been sorted out for spinors, with the general assumption being one of "only depends on complexification", in the sense of assuming the analytic continuation can be done. Your comment led me to take a look at Penrose (at least to the extent of picking up "Road to Reality"), and I was intrigued to learn that he does use the West Coast metric, but that he also clearly thinks of the question in terms of complexified Minkowski space and analytic continuation. Quite possibly the right way to think about these questions is in terms of twistors.

31. **Kartik Prabhu**
June 6, 2015

* I also think that twistors are in some sense capturing the some part of this issue. I would highly recommend both volumes of Spinors and Spacetime — Penrose & Rindler. They present the formalism in a very clear style.

* Another related annoyance wrt complex vs real forms of groups is that we physicists do not distinguish these in Yang-Mills theory. I have had the hardest time finding a good reference on the issues of group structure, complex vs real forms, Cartan subalgebras… as it relates to Yang-Mills theories.

original post

32. **John Baez**
June 6, 2015

Kartik wrote: "I still don't understand the physical significance (if any) of pinors."

One place they can be important is when you've got a theory where spacetime is non-orientable. Then you need to know how your "spinors" transform under orientation-reversing symmetries – so what you really need is pinors, which are a representation of a double cover of O(p,q) rather than merely SO(p,q).

Here I will not enter into the fact that O(p,q) has up to 8 different double covers! After choosing a sign convention for your Clifford algebras, the Clifford algebra will contain a group Pin(p,q) which is a specific double cover of O(p,q). We can hope that nature will be kind enough to use this one.

I saw Cécile DeWitt-Morette give a talk about this stuff. Once she and her husband Bryce DeWitt both did computations for a free fermion field on a nonorientable spacetime. They got physically different answers! And it turned out the reason was that one was using a pinor representation of Pin(1,n), while the other was using a pinor representation of Pin(n,1).

They wrote a paper about this.

33. **Kartik Prabhu**
June 6, 2015

John,

Thanks for pointing that out and for the link to the DeWitts' paper. I was completely ignoring orientability issues.

In this case, wouldn't one want the universal cover of O(p,q) rather than merely a double cover? If I am remembering correctly for d > 3 the Spin(p,q) group is also the universal cover of the oriented part of SO(p.q). For instance in d=3, I thought the universal cover gave rise to representations like anyons.

original post

34. **Marc Nardmann**
June 6, 2015

More information related to John Baez's remark about double covers of O(p,q) can be found in the following two articles:

1.) Trautman, Andrzej: Double covers of pseudo-orthogonal groups. Clifford analysis and its applications, NATO Science Series 25 (2001), 377-388.

(http://link.springer.com/chapter/10.1007%2F978-94-010-0862-4_32)

For p,q>0, eight inequivalent double covers of O(p,q) are constructed in that article; see §7.2. Trautman says there are precisely 16 inequivalent double covers of O(3,1), but he does not construct explicitly the other 8.

2.) Trautman, Andrzej: On eight kinds of spinors. Acta Physica Polonica B 36 (2005), 121-130. (go to http://www.actaphys.uj.edu.pl/_cur/pl/acta_physica_polonica_b, then browse)

This article ends with the following conclusion: "Physicists have been wise to restrict their attention to the groups Pin(1,3) and Pin(3,1) as these are the only double covers of [the full Lorentz group] that admit faithful irreducible spinorial representations and allow the construction of real invariants and covariant currents."

35. **Lowell**
June 7, 2015

Well, my book "Quantum Field Theory" (Cambridge Univ. Press) has the proper
(-,+,+,+) metric.

36. **Lowell Brown**
June 7, 2015

Might as well give my full name.

37. **Tony Smith**
June 7, 2015

John Baez said in his week156 web page (17 Sep 2000):
"… any self-dual irreducible unitary group representation H must admit an antiunitary intertwiner J: H → H with either J^2 = 1 or J^2 = -1.
In the first case H comes from a real representation;
in the second case it comes from a quaternionic representation
…
in Volume 1 of Weinberg's "Quantum Field Theory" …
the CPT operator on the Hilbert space of a spin-j representation of the Poincare group is an antiunitary operator with (CPT)^2 = -1^(2j).
So indeed we do have (CPT)^2 = 1 in the bosonic case, making these representations real,
and (CPT)^2 = -1 in the fermionic case, making these representations quaternionic. …".

Tony Smith

38. **John Baez**
June 8, 2015

Kartik wrote:

> In this case, wouldn't one want the universal cover of O(p,q) rather than merely a double cover?

You're right that we need to carefully think about all these issues rather than merely grabbing a double cover and using that. But part of the problem is that the concept of "universal cover" applies to *connected* Lie groups, and O(p,q) typically has 4 connected components. So, for example, O(3,1) has 4 connected components that contain the elements 1, P, T, and PT respectively. If we restrict attention to the connected component containing the identity, its fundamental group is Z/2 so its universal cover is its double cover, isomorphic to SL(2,C). But things get more complicated when we consider all of O(3,1). It has *different* double covers. All of them become isomorphic when we restrict them to obtain covers of the connected component containing the identity. That is, all restrict to give universal covers of this connected component! I think what's happening is that they are different central extensions of SL(2,C) by the group {1,P,T,PT} = Z/2 × Z/2.

You're right that O(2,1) is very different: here the fundamental group of the connected component containing the identity is Z, not Z/2, so we get anyons.

39. **Mesmar Djehha**
June 8, 2015

I always imagined funnily (I am an autodidact in physics and mathematics from north Africa) that the terminology West Coast/East Coast came from the fact that when we look down at the metric matrix in front of us, we see the + (representing sunrise) coming from the west wtr to the matrix in West-Coast convention, whereas we see the sunrise coming from the east in the Est-Coast convention.
Thanks a lot Peter for this posting. We really learn a lot about mathematics and physics from this blog.

40. **Martin**
June 8, 2015

Srednicki kindly provides a free draft copy plus errata:

http://web.physics.ucsb.edu/~mark/qft.html

41. **Fabien Besnard**
June 8, 2015

Here is a discussion at mathoverflow about the related issue of the convention used in defining Clifford algebras. Note that I don't claim to understand the first (and meta) answer there…

42. **Justin**
June 8, 2015

Love this blog post. Do not worry about the detractors. I do feel that this blog is not quite as good as it could be because it focuses way too much on multiverse nonsense. I'd like to see more postings about trying to understand quantum field theory better, and this post is right on target.

43. **Geoff**
June 8, 2015

I have only skimmed the comments so I apologize if I'm repeating something already mentioned more succinctly above. I was under the impression that the most obvious reason for using the mostly plus convention was that at some point one is forced to do violence to General Relativity and cast it into Hamiltonian form thru the ADM decomposition. Although Ted Jacobsen has been voicing valid reasons for not quantizing the metric, it seems that the vast majority of successful quantization schemes have passed thru the Hamiltonian formulation.

A wonderful resource that I've just come across that doesn't appear to be on the ArXiv is an article about spinors by Geroch in the Springer Handbook of Spacetime where he uses the mostly minus convention. I've heard a lecture in which he expressed bewilderment that spinors which were originally used to describe electrons make the proof the Positive Energy Theorem in GR substantially easier. And if every tensor and tensor operation has a counterpart in the spinor formulation, it certainly seems worth learning. It does seem from my quite limited viewpoint that something profound is happening when spinors show up in such an unexpected manner.

Professor Baez – I had always skimmed the descriptions of non-orientable spacetimes as it seemed that they lead to violations in causality. Are there non-orientable spacetimes which preserve a global causal structure or are they merely interesting from a theoretical standpoint?

Professor Lowell Brown – I've quite enjoyed having your textbook as welcome addition to my shelf and was hoping you'd get around to someday writing the second half of the book.

I have always been fascinated by stochastic quantization as it too seems entirely unexpected. For those who know, can you point me in the direction of a reference that might explain its physical relevance or is it simply a trick which also involves complexification?

Does anyone happen to know if Schwinger changed conventions when he left Harvard for UCLA?

Thanks in advance for help anyone can provide.

44. **Lowell Brown**
June 9, 2015

Nope, Schwinger stayed East Coast when he moved to the West Coast.

45. **John Baez**
June 9, 2015

I wrote:

> I think what's happening is that they are different central extensions of SL(2,C) by the group {1,P,T,PT} = Z/2 × Z/2.

Sorry, on second thought that doesn't sound right: even in O(3,1) the transformations T and P don't lie in the center, only 1 and PT are. So, I think we should delete the word "central" here.

46. **Peter Woit**
June 9, 2015

Justin,
Don't worry, I'm quite good at ignoring detractors who I don't think have a case. And I do know when a topic is substantive, attracts an intelligent discussion, and I learn something, will do my best to try to make that happen more often. Sorry about the next posting….

47. **cedric bardot**
June 9, 2015

> …most physicists don't appreciate the mathematical concept of "real structure"

Well, may be one will have to wait for the experimental discovery of a fundamental Majorana neutrino to expect better awareness about clifford algebras, pin groups and real structures (cf I. Todorov arxiv.org/abs/1106.3197)
On the theoretical side people like A. Connes *et al* and John .W. Barrett may have already paved the way to improve our geometric understanding of the standard model in order to orientate us in the possibly already nontrivial dimensional structure of zeptospace (arxiv.org/abs/hep-th/0608226 and arxiv.org/abs/hep-th/0608221).

48. **nicola**
June 11, 2015

The many-minus convention is far better because the mass-shell constraint has the clear form p^2=m^2 (while in the other you get the awkward minus sign). Moreover the Dirac equation in the many-plus convention would get an additional factor of i – either in front of m or in front of Dirac matrices – which is even more awkward.

That Weinberg uses many-plus convention – well, Landau uses the other one.

Wick rotation is just some other mathematical operation used in calculations. There is no deep Physics behind it. To choose a convention in Physics because Wick rotation looks nice, is beyond me.

Backwards-compatibility is the only reasonable argument. If it weren't for the mass-shell constraint and the Dirac equation it would probably convince me. On the other hand the creator of Python language considers backwards-compatibility as misguided since newer ideas/solutions are better then the older ones and so why constraining yourself with the wrong conventions/habits invented before? Many new languages invented are not backwards-compatible anymore.

49. **Peter Woit**
June 11, 2015

nicola,
1. I don't really see why p^2=-m^2 is a problem.
2. I don't see any i's in the Dirac equation in East coast convention, either in Weinberg (equation 1.1.19), or my current notes (section 45.1, that chapter is a mess now, but I think the signs, factors of i are correct).
3. I don't think Wick rotation is just "some other mathematical operation". QFTs really only are well-defined in imaginary time, it's a fundamental fact about them.

50. **Adam Helfer**
June 11, 2015

As a relativist, I'd like to make some points in favor of the mostly-minus convention. Of course, one really can use either convention — one just has to include minus signs or factors of i in different places. And for certain specific applications (e.g. the ADM formalism or some QFT arguments) it may be technically easier to use the mostly-plus convention. But as a matter of principle — of trying to make the mathematics match most directly the physical foundations — in relativity, I think the mostly-minus convention is to be preferred.

The reason is that proper intervals between timelike-separated events are directly measurable: you just let a clock run along the trajectory whose proper time you want. On the other hand, spacelike proper intervals are by definition acausal concepts. They cannot be measured directly, and working them indirectly tends to be involved.

To some degree, of course, one can measure space-like intervals by means of "rigid rulers," but notice that this is not at all a simple thing: the rulers really serve to define a coordinate system and what one reads off from them are coordinate differences, not invariant intervals. Alternatively, one can infer spacelike separations from systems of timelike measurements. (Note that such measurements are needed to calibrate the system of rigid rulers anyway.)

Also the idealizations involved — and the question of how to correct for deviations from them for real measurements — are much more severe for rulers and space-like measurements than they are for clocks and time-like ones. A clock simply has to be small enough and stable enough that its mechanism is not significantly affected by curvature over its run-time. On the other hand, if one wants to extract physics from a ruler extending over a large enough region that space-time curvature is significant, in general one needs to solve the coupled system of the ruler's internal dynamics and gravity (and one cannot really treat the ruler as rigid).

(As an example, note that GPS works primarily by making time-measurements. Some reference to rulers — Earth-based reference frames — is needed, but the main measurements are temporal. Also interferometers like LIGO essentially measure time-signals, as they compare phase differences for light of a fixed frequency along different space-time paths; the spatial separations are inferred.)

It is true that the mostly-plus convention seems to be the friendlier, less radical departure from Euclidean space. But really the lesson of relativity is that three-space is, except in restricted circumstances, a rather indirect concept of limited physical significance. (I hasten to add that in many circumstances, especially when relativity is not important, it certainly is useful!) However, the everyday concept which most directly generalizes to relativity is not space but time. So my feeling is that the friendliness of the mostly-plus convention is meretricious (as far as the foundations of relativity go), and we get a deeper sense of the geometry of space-time by embracing the perspective implicit in the mostly-minus one.

51. **Adam Helfer**
June 12, 2015

I recently sorted out the relation between the two-spinors of Penrose and Rindler and the usual QFT gamma-matrix/four-spinor formalism. The Penrose and Rindler formalism is entirely self-consistent and does not use gamma-matrices, although they do give a conversion in the notes. However, this conversion does not mesh with the usual QFT conventions; it is better to use another one.

For explicit formulas, see the appendix of http://arxiv.org/abs/1408.2729.

52. **nicola**
June 12, 2015

Peter,

1. If p^2=-m^2 does not look awkward to you then fine. It looks awkward however to many other people.

2. Weinberg's equation 1.1.19 is missing a factor of "-i" next too kinetic term(compared to other textbooks). That factor was incorporated into the definition in 1.1.20 so that the metric in 1.1.21 is as you prefer. However standard textbooks like Landau, Bjorken-Drell or Itzykson-Zuber, leave the -i factor next to the derivative and so the Dirac operator is
D= -i\gamma^{\mu}\partial_{\mu} + m (see e.g. eqn 2-9 in I-Z)
while in Weinberg it is
D= \gamma^{\mu}\partial_{\mu} + m
because the gammas are redefined.

3. Come on Peter, it's just a mathematical trick from complex analysis. That no-one so far has found a better way of defining QFT, well it's still just a trick.

Moreover the Wick rotation plays a role only in QFT. It doesn't play a role in QM or classical theory. Unlike the p^2=-m^2 which appears everywhere where particle dynamics is considered. To me it looks awkward as well as the Dirac equation (without the -i factor).

53. **Peter Woit**
June 12, 2015

nicola,
Changing the space-time signature changes the definition of the gammas. As I tried to point out, one virtue of the conventions I advocate is that you can choose the gammas real. As for the Dirac equation, I think you've just provided another argument for my conventions: Weinberg's Dirac equation with no gammas is simpler than one where you have to use i (and allows you to describe Majorana fields more simply).

Sorry, but I don't think analytic continuation in QFT is just a "trick". I'd claim it's a fundamental insight into the nature of time (and the existence of anti-particles). Yes, you only need this in relativistic quantum field theory, but that's the fundamental theory.

54. **Thomas Larsson**
June 13, 2015

Am I the only one who thinks that the topic of this post qualifies as Not Even Wrong? Much more so than string theory, which by any reasonable metric is just wrong.

Sign conventions are like interpretations of QM – if no experiment can tell them apart, they are all equally right or wrong. But of course it is a bad idea to use several conventions in the same calculation, as the people who claimed to have found a muon anomaly 15 years ago did.

55. **nicola**
June 13, 2015

Peter,

Arguments that you find in favor of many+ convention I find actually against it. Well, I guess that's why we call it a convention.

Many people would prefer to leave that -i factor because together with the derivative it gives the momentum operator and so the Dirac operator admits a very nice form D=gamma p + m. Using Weinberg's approach we have D=i gamma p +m with an unnecessary factor of i only because you want p^2=-m^2.

By the way, another argument in favor of many- convention is the form of the action of the point particle. In many-, the Lagrangian is a square-root of (4-velocity)^2, something manifestly positive. In many+ you have a square-root of -(4-velocity)^2, with an awkward minus.

56. **David**
June 15, 2015

Just as a curiosity,

there is a famous writer of hard science fiction (the one which relies heavily on the science) called Greg Egan. He is a mathematician and is famous because his writings are the hardest science fiction ever.

In a trilogy of books (Orthogonal) he esspeculates with a universe with a Riemmanian metric instead of the Lorentzian of our universe. This creates very strange things, such as in that universe the speed of light varies with the frequency and that the "twin paradox" is upside down (time goes by faster with the twin that remains at home), among many many other things. I have not read the book, this is what I have discovered investigating about it.

This a detailed discussion in Egan's web page, in case you are interested http://gregegan.customer.netspace.net.au/ORTHOGONAL/ORTHOGONAL.html#CC

57. **Jack Morava**
June 21, 2015

I'm sorry to come late to this topic, but I learned only recently
of (IMHO profound) joint work of Graeme Segal and Maxim Kontsevich
on Wick rotation, discussed in a talk

https://www.youtube.com/watch?v=vTvXHL6ZJik

by GS at a birthday conference for MK last June. The key technical
point occurs around 25:00 in the video; in particular it seems
to me to provide convincing evidence that the East Coast convention
is NATURAL in the technical sense of the term.

58. **Peter Woit**
June 21, 2015

Jack,
Thanks a lot. I agree that Segal's work on Wick rotation (which as far as I know he has lectured about, but not really written up), is the deepest thinking about the question around. As you note, for what he's saying to make sense, you need the East Coast metric (he actually very quickly but explicitly says that having 3 minus signs won't work: another way of saying that analytically continuing three directions of space instead of one of time is a bad idea).

59. **Alma**
June 25, 2015

Hi Peter and thanks for a great article!
I'm having an argument with a friend over what Zee did and I'm hoping you can clarify it. Did Zee complexify the SO(3,1) in the book such that his statement is correct contextually, or is the excerpt you are showing simply wrong mathematically and not deducible from the introduction? Thank you very much!

60. **Peter Woit**
June 25, 2015

Alma,
What Zee says would be correct if you complexify the two Lie algebras: what is true is that the complexified Lie algebra of SO(3,1) is the same as the complexified Lie algebra of the product of two copies of SU(2). This is not true for real Lie algebras, which is what the notation he is using refers to (if you ignore the fact that he's using the notation for Lie groups to refer to Lie algebras…) My point was just that for him and Schwartz (and a lot of physicists), there is no difference between a real Lie algebra and its complexification.

The complexification of su(2) is sl(2, C), and of so(3,1) is so(4,C). So, what the "mathematically sophisticated" would really say is
so(4,C)=sl(2,C) + sl(2,C)
When you complexify a real Lie algebra so(p,q), you get something that doesn't depend on the signature (so(q+p,C)), since once you have complexified you can just appropriately put in factors of i to make your inner product the standard one.

A Crisis at the (Western) Edge of Physics

June 9, 2015
Categories: Multiverse Mania

The New York Times had an op-ed piece this weekend by Adam Frank and Marcelo Gleiser, entitled A Crisis at the Edge of Physics. They make some of the usual criticisms of string theory and the multiverse, ending with

> Are superstrings and the multiverse, painstakingly theorized by hundreds of brilliant scientists, anything more than modern-day epicycles?

I mostly agree, although I don't think they make clear what the real problem is, that these theories predict nothing and explain nothing. In contrast, epicycles were a quite useful, well tested model that was highly predictive and approximately correct. If we had modern day epicycles, that would be a huge advance…

Last Friday in his concluding talk at a Nordita conference on particle physics and cosmology, Michael Turner gave his take on the multiverse:

> Most important discovery since Copernicus?
> Is it science? (not testable)
> Many true believers (left coast) and not enough doubters.

He makes clear his opinion on these questions with this graphic:

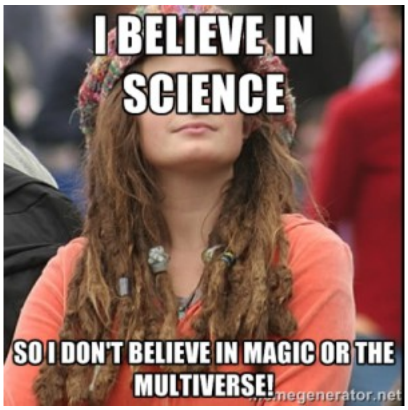

and I think this expresses well the majority opinion of the physics community. A major question here is whether the problem of pseudo-scientific multiverse mania is one of the "edge" of physics (the "left edge", as Turner notices and was discussed here), or whether it has infected the center. Some days I'm quite discouraged to see how widespread this is, other days it seems to me that we may finally be getting over this. There's only so long you can get media attention for your empty but easy to understand new "Copernican revolution" before people lose interest and move on to something else. Perhaps we're getting to that point. I think this was the first year that the World Science Festival here in New York didn't have a program promoting the multiverse, and maybe that's a sign of change.

For quite a few years now, there have been few scientific talks trying to use a multiverse to do calculations at serious string theory conferences (see for instance this week's String Pheno 2015, or Strings 2015 later this month), with the multiverse mainly appearing in promotional talks to the public. Maybe the public is finally getting bored and starting to adopt the point of view that Turner's graphic suggests (and that I think the physics community should get behind).

Update: Physics Today has an opinion piece entitled Could the evolution of theoretical physics harm public trust in science? This addresses an issue I don't think some theorists realize the seriousness of. If you start arguing that conventional notions of testability don't matter, this can be a very dangerous thing to do in an environment where public trust in science is an issue. Put differently, if physicists publicly promote the pursuit of speculative ideas in an ideological framework that can never be falsified, they create a real danger of a public perception that science is just one more ideology.

Comments

1. **Jeff M**
   June 9, 2015

   Honestly, epicycles were not "predictive" in any meaningful way. They were just a fancy geometric way of building a table of planetary positions based on past positions. The geometry was there simply because it was clear there had to be some geometric explanation for the positions. But they made no predictions, in the sense that if someone found a new plant, epicycles would tell you nothing about where it should be. Of course, if you want you can consider epicycles to be the correct theory of the solar system, since it's just a change of coordinates.

2. **gadfly**
   June 9, 2015

   I would say homeopathy is worse than multiverse mania, but at least the placebo effect and therapeutic "consultations" actually help people.

3. **Eric Habegger**
   June 9, 2015

   Seriously, is it really necessary to characterize the multiverse mania as a West Coast phenomena? I seem to recall that there is a prominent faculty member at Columbia who has written many books that popularized the whole multiverse idea. The idea of one coast being the fount of misinformation does a disservice to all interested parties and also promotes a "new" source of prejudice that will cause problems down the line. I'm from the West Coast and I've never entertained the multiverse as anything serious.

4. **Peter Woit**
   June 9, 2015

Eric,
Good point about Columbia. There's also someone at MIT… Still though, I think there's a statistically significant effect, was interested to see that Turner shares that perception.

5. **Jeff M**
June 9, 2015

Oh, and BTW, there's a previously unprintable word missing between "I" and "believe" in that great sign 🙂

6. **Jan Galkowski**
June 9, 2015

@Jeff M: "Epicycles". Half in jest, presumably the AIC (or its variants, including Watanabe's WAIC) would penalize the epicycles in comparison to an ellipse for the extra parameters required. An interesting question is whether or not the ellipse would lose in favor of epicycles because of poor observational accuracy, assuming those observational errors were not, at the time, well understood or estimated.

K. P. Burnham, D. R. Anderson, *Model Section and Multimodel Inference*, 2nd edition, Springer, 2002.

S. Konishi, G. Kitagawa, *Information Criteria and Statistical Modeling*, Springer, 2008.

7. **David Roberts**
June 9, 2015

Aren't epicycles really just a Fourier expansion of the apparent motion in the earth's frame of reference? The issue is then a poor choice of coordinates making life hard 🙂

8. **Bee**
June 10, 2015

I'm not sure that this push towards phenomenology is useful at all, and I say that as someone who normally preaches the value of phenomenology. Look, on the one hand there's people who couldn't care less whether their theories are testable at all. That's bad of course. But on the other hand (should I say other coast?) are people who cook up models just because they can be tested with the next experiment, and I can't see there's more value in that. Lisa Randall's talk (at the same conference) was an explicit example for that. That's searching under the lamp post. Does any one really believe a model becomes more plausible because it can soon be testable? No. So why do they do it? Because it will get published and when it doesn't get found one twiddles some parameters and moves the "phenomenology" to next decade's experiments. That's equally sick if you ask me.

9. **chris**
June 10, 2015

Those of you who think epicycles were not predictive should read Keplers Astronomia nova and look at how he got to the Ellipse shape. Peter Woit is spot on with his analysis.

Oh, and I don't believe in science. I wonder who does. I simply accept proven statements about nature as being true. Pictures as the one above are a real disservice to science, putting it on the same footing as religion.

10. **Dom**
June 10, 2015

The word "believe" is natural to use but highly problematic, when challenged I once said I believed in facts and evidence, this was dismissed by the other person as simply "another belief system" as if the alternatives were all equally valid.

11. **Peter**
June 10, 2015

@JeffM
Epicycles are essentially a Fourier expansion.

12. **Peter Woit**
June 10, 2015

Bee,
At least in particle theory, my impression is that the hiring prejudice in favor of phenomenology is now changing, as departments decide that the LHC is not going to provide a lot of new physics. The pressure on "string theorists" to produce something "testable" has now been there for a long time, being met by claims that AdS/CFT is going to solve non-HEP problems. Unclear to me where this is going…

13. **Peter Erwin**
June 10, 2015

Of course the Ptolemaic model was predictive — that was one of its main features, and one of the reasons it stayed popular for centuries: you could use it to predict the future positions of the planets. The idea that a model of the heavens should not just provide a handwaving, semi-metaphorical picture of what was going on, but should make numerical predictions for future observations, was a key achievement.

These weren't terribly *accurate* predictions, and for a long time people weren't very concerned with their inaccuracy. And, of course, the model failed to *explain* a lot of things, and failed to predict things that the Copernican model did (e.g., the phases of Venus).

(It may be worth remembering that Copernicus's heliocentric model *also* had epicycles — indeed, there was one variant that had secondary epicycles attached to the primary epicycles.)

The problem in thinking about Ptolemaic (and Copernican) epicycles as "Fourier expansions" and the like is that they were constrained to have uniform circular motion. If you allow the epicycles to be *elliptical*, with non-uniform motion, then, yes, you can think of them as approximations to actual planetary motion.

14. **al**
June 10, 2015

Copernic's book was published with a preface by Andreas Osiander who took pains to tell everybody that actually heliocentrism si just another way of 'saving appearances.' It was presented as a mathematical hypothesis, a way of doing calculations, not as physics. But in medieval universities two courses were taught: one with the Aristotelian-physical model which was deemed 'true' but known to be descriptively inexact; the other 'untrue' but exact (by the way Ptolemy had the 'equant' (punctum equans) , a point inside an orbit where non uniform movement appeared as uniform).

15. **phil fogle**
June 10, 2015

As a member of the 'public', I constantly see evidence of the undermining of the principle of falsifiability, and HEP seems to be playing a major role.

Almost all my friends have heard about the Higgs field; not one has any idea of its significance. Their minds are full of black holes, holograms and multiverses that they believe are proven facts.

@Gadfly
…actually, I see homeopathy (sorry for being OT) as extremely insideous, as it hijacks the placebo function. Medics should be honest about placebos.

16. **Brian**
June 10, 2015

Funny thing is about the update section, I thought thats what the fuss was about. Science is useful because it allows us to make predictions and test them. It lets us either discover how the world works or at least create a narrative that works, but either way its consistent, rigorous, objective, and sometimes has practical applications. I'm not arguing against religion, superstitions, or odd beliefs, if thats physiologically helpful to you thats all fine, so long as we have an objective way of knowing how the world works that we can use to acquire knowledge free from the subjective views and interests of others. Thats why I found it so unsettling, about a year or two ago when I read a popular science article by someone related to the field. they where saying that it was time to move on to "post empirical science " (what ever that is). Another talking about getting rid of Popper's falsification, the bottom line is the creation of a "science" that does not depend on experiment or verification. It is troubling to hear the words you would expect from a new age woo mister or some incomprehensible post-modernist professor coming from scientists or philosophers of science.

Now to me that seems like giving up what science is in order to save a few careers. The whole argument depends on a lack of imagination, "string theory is right because there is no other game in town" (dubious). I have to ask what makes string theory the only game in town other than it looks fancy and some scientists/mathematicians like it? If science is simply a series of narratives that work, made more plausible by experiments, than any narrative would do in string theories place, since no experiment can be done to confirm it. Even fantastic ones like how the Greeks used to use mythological figures to explain the way the world worked, after all they have the same amount of experimental evidence. In the history of science these mythologies became unsustainable do to the scientific revolution, we could do experiments to come up with better stories, scientific theories. As far as the public is concerned, this is where the power of science comes from, the fact that it explains how the world works and can be demonstrated. Scientists would shot themselves in the foot by arguing that testability needs to go because its not their pet theory that benefits, its the snake oil salesman.

anyway, I say its better to have no theory and science than have a theory and no science.

17. **al**
June 11, 2015

'Postempirical science' is an attempt to shift the burden of proof. Standard science was and still is positivistic: if you make claims you have to point to some evidence. Positivism did not admit any ontological 'known unknowns'. Postempiricists try to argue that you should give them credit as long as they have not been proved wrong. There is an evident shifting of power here as outsiders cannot beat experts at their game.
(actually it was mathematicians in the 19 c. who first claimed the right to consider whatever they pleased as long as it was not obviously self-contradictory; physicists had to point to some external support, but 'mathematical' or 'theoretical' physics tended to erase the distinction)

18. **Nobody**
June 11, 2015

@Peter

"Epicycles are essentially a Fourier expansion."

I think I originated that idea on the Compuserve SciMath forum about 20 years ago and I also believe that it is not true.

But I'm not that smart, so maybe you or someone else can show me how you can get a Keplerian ellipse from a harmonic series of constant speed circles.

19. **Peter Woit**
June 11, 2015

Enough about epicycles please. One problem is that I really know nothing much about them, so can't do a proper moderator's job (e.g. delete misinformation…).

20. **Nathalie**
June 11, 2015

Dear Bee,

You are being unfair to Lisa Randall.
As the great Wolfgang Pauli used to say, when you don't have a theory (as is currently the case for beyond the standard model) you should make "simple" models that can be tested and thus may provide some insight. And that is what Lisa Randall is doing.

Esoteric movements, by not being testable, tend to lead nowhere.

Once you have a theory which starts looking promising it is worth to work on polishing it, in order to understand better its structure.

21. **John Baez**
June 12, 2015

Jeff M wrote:

> Honestly, epicycles were not "predictive" in any meaningful way.

Sure they were! It's true they couldn't predict what would happen to the Solar System if you added a new planet, but Newton's laws couldn't predict anything about chemistry and people don't complain that he didn't invent quantum mechanics.

Anyone who mocks the epicycles Ptolemy's *Almagest* probably hasn't thought about how much better it was than previous models, and how long it took for heliocentric models to exceed its predictive power. Check out Ptolemy's *Almagest*: Fact and Fiction by Richard Fitzpatrick of the physics department of U.T. Austin. Some quotes:

> The standard popular modern criticisms of Ptolemy's model of the solar system are as follows. First, it is generally thought that Ptolemy's thinking was shackled by accepted truths in ancient Greek philosophy (mostly due to Aristotle), which held, amongst other things, that the Earth was stationary, and that celestial bodies were constrained to move uniformly around circular orbits. Second, it is supposed that these mental shackles directly lead Ptolemy to introduce the concept of an epicycle as a sort of kludge to explain the observed retrograde motion of the superior planets without having to admit that this phenomenon was caused by the Earth's motion. Third, it is generally held that Ptolemy's model of the solar system was not particularly accurate, leading later Arabic and medieval European astronomers to add more and more epicycles in order to get better agreement with observations. The final version of the model is alleged to have contained an absurd number of epicycles, and to have essentially collapsed under its own weight, leaving the field clear for Copernicus and his, supposedly, much simpler, and much more accurate, heliocentric model of the solar system.
>
> Needless to say, the popular criticisms of the Almagest that I have just outlined are almost entirely wrong. What I want to do in this talk is to describe what Ptolemy actually did in the Almagest, and to contrast this with the mistaken popular view of what he did.

and:

> The *Almagest* model of the geocentric solar orbit is surprisingly accurate. It can predict the position of the Sun, relative to the stars, to an accuracy of about 1 arc minute. That is, about 1/60 th of a degree.

22. **Kevin Henderson**
June 22, 2015

The general public in America favors ideologies of all sorts. In particular, the majority are in favor of things which scientists either believe or know to be magic (i.e., false). It is unfavorable, economically, at least, at this time to condemn all fantastic ideas based on unrealistic physics. People like to think science fiction is closer than it is.

A View from an Ex-String Theorist

June 9, 2015
Categories: Uncategorized

Every so often I've taken a look at something about theoretical physics on Reddit, generally ending up not spending much time there. One reason was that I realize I'm already spending more of my life than is healthy arguing with people about string theory and the like, so better to avoid a new venue for that. The temptation to respond is strong when one sees someone mischaracterizing one's opinions, but I've generally been able to resist temptation at that site.

Today I happened to come across a really wonderful discussion there though, and wanted to draw attention to it, even though it's from a year ago. It's entitled A View from an Ex-String Theorist and consists of a long piece by someone who has recently left string theory, as well as some answers to questions asked by others. If you want to understand what string theory looks like these days to good theorists who are working on it, read what "No_More_Strings" has to say.

No_More_Strings explains very well the difficult job situation in the field, and the effects this has. With a lot of very smart people and almost no jobs, postdocs are in no position to take the time to try and learn something new that isn't a "hot topic", or try and work on an unpopular idea that might take years to go anywhere. This is a huge part of the story of why this field is in trouble, and the situation seems to have just gotten worse since I wrote about it in my book over 10 years ago.

The suggestion that "string theorists" should stop calling what they do "string theory" is an excellent one. No_More_Strings explains how smart people in the field are not working on string unification, but have moved on to different things with little relation to quantized strings. Giving up the name would be a good first step to allowing people to think of what they are doing in a less narrow way. If you didn't have to start every grant application by explaining that you're motivated by "our best hope for a theory of everything", you might find it easier to work on something quite different, with no relation at all to quantized strings.

I can't quite resist correcting a couple things mentioning me. No, I don't think string theorists are stupid. No, I don't think that Witten "singlehandedly destroyed the study of "real" physics" (the last one isn't the fault of No_More_Strings).

Comments

1. **Amirpouyan**
   June 9, 2015

   If they are not stupid, then why on earth they are insisting on a program that they know (better than anyone else!) has no promising future?!,

2. **Peter Woit**
   June 9, 2015

   Amirpouyan,
   You don't seem to have read or paid any attention to the piece, which explains pretty well that what most "string theorists" are doing (at least the smart ones…) is working on a wide range of topics, of varying degrees of promise for various purposes, most of which have little to do with strings or the supposed use of strings to get a unified theory.

   And, even those who haven't given up on string unification are staying with it not out of stupidity, but out of an honest belief that it's the best idea about unification that they know of, no matter what its problems. Anyone who thinks Witten is stupid is a fool…

3. **Vlad**
   June 9, 2015

   I'm sure we'll hear Lubos' long reply about this tomorrow. Thanks a lot, Peter.

4. **M**
   June 10, 2015

   Gia Dvali has an interesting new understanding of black holes as condensates of gravitons with energy 1/R. If true, this would also mean that black holes are insensitive to UV physics (Einstein general relativity is enough); so computations of black hole entropy in terms of string micro-states would just be way a misunderstanding black holes.

5. **chris**
   June 10, 2015

   I am consistently staggered by some of the insinuations that surface in rants like the one on reddit that you linked to of failed researchers (be it that they didn't land a permanent job or that their research program collapsed).

   .) Why do they always need the collective shoulder-patting of "we are the smartest"?

   .) Why do they believe that their small subfield is so very special in terms of emotional, intellectual and time commitment?

   .) Why do they believe that their particular area is starved of academic positions?

   .) Why do they think they are too "smart" for the "dumb" homework problems or laundry tasks?

   .) And finally, why do they assume that reading fiction loosely related to the theme of research qualify you for anything but a job as librarian?

   I am always reminded of the students that are "bored by classical physics" and the like and jump into quantum field theory in year 2 or so. Never saw a single one of them being successful.

   It's a simple truth that research as a career is highly risky, extremely time consuming, very competitive and very often frustrating, quite independent of the specialization. I would have thought that – especially in good institutions – you get this message very early on and consistently.

6. **Radioactive**
   June 10, 2015

   It's funny that he repeats how smart he is while complaining that all he has ever done is `laundry'. The academic system is designed to select precisely those people who are the best at `laundry' and this guy seems to have indeed been very good at it. But there are many other necessities, besides technical skill, to be a really good scientist like creativity, courage, perseverance and luck.

The canonical example being Einstein, but even the originators of string theory had to toil in obscurity for a decade or so. If you don't have the courage to just do what you're interested in because you are addicted to the praise of your supervisor, you are afraid of losing your position or god forbid, not getting hired at a Top Ten university, or you don't have any ideas that you truly believe in and can make progress on then you are lacking the tools.

It's a shame that the system doesn't allow everyone to just study whatever they want to for as long as they need to (though would that really lead to more insights or just more time for bad ideas?) but as it is this guy had found his level: good technically, not a lot of insight. I have never met a tenured professor who, if you had a really good idea would stop you from working on it, even if it slowed down your output. University is supposed to teach you technical skills that are necessary, but curiosity and creativity are going to make you a great scientist who is doing interesting things. If you don't have those, and are also unwilling to take risks, then working on obscure technical problems is probably the best place for you.

7. **Klaus**
   June 10, 2015

   Sorry, there is no reason to criticise the guy in question. Leave him alone! Yes, you have to take risks to do good research. But you also have to feed your body and later your family. The story of the young man is still happier than that of Lubos Motl, who also left string theory, and now earns his bread writing rants on his blog, for his own fan club. And probably the story is much happier than those of Seiberg or Arkani Hamed, who lure people into the field telling falsities. Everybody takes his choices in his life. Also Motl's decision is a choice. Even the IAS people luring others to join them have taken a choice. Are they happy? They have to decide.

   IMHO, we reduce the talk about human choices, but instead talk about unification, and on how to achieve it.

8. **Radioactive**
   June 10, 2015

   I am not criticizing his decision to leave physics, rather the unsaid assumption that because he is `really smart' boring technical work is beneath him. It's a shame about the job situation but doing the exciting, creative research that he seems to want to do instead of `laundry' would have landed him in exactly the same position. So he didn't have either the guts or the ability.

9. **Peter Woit**
   June 10, 2015

   chris/Radioactive,
   I think you're being too hard on "No_More_Strings". I didn't read his (seems highly likely it's a "he"….) piece as complaining at all. It provides a very accurate take on what the grad student/postdoc system is like in that particular academic field. People who choose to enter it likely have a fairly good idea what they're getting involved in when they start, and reasonably good career prospects for doing something else when they leave (and this is what will happen to almost all of them). More interesting is the significance for the field itself: is this organization of research, this particular set of incentives and pressures, a good way to encourage progress or not?

10. **Radioactive**
    June 10, 2015

    Maybe I am being too harsh but his attitude about `laundry' problems and his repeated assertions about how smart he is are galling. If you don't like it you either quit, as he wisely did, or take a risk and do something interesting. Most people (including me) don't have what it takes to succeed at the latter.

    Regarding the organization of research, have a look at http://www.nature.com/news/the-future-of-the-postdoc-1.17253 . From personal experience I have seen an institute hire half a dozen permanent staff and even more postdocs without increasing the quality of its output. The 'publish or perish' system is often maligned but what is the alternative? Don't publish but keep your job forever? The truth is that boring technical laundry is all that most people can do and the various (also maligned) metrics like citation tracking and impact factors do a reasonable job of measuring quality.

    If there is a problem it is minting too many PhDs like this guy, with technical skill but an inability to drive research themselves who are unlikely to become good scientists. The blame for that is principally on PIs and university administrators who make hiring PhDs and postdocs appealingly cheap. Progress will happen and is happening, it is just harder to distinguish from background noise and perhaps a few bright people are lost to the banking industry.

11. **Jeff M**
    June 10, 2015

    It's fascinating as a mathematician to see this discussion. Math and physics are close, but the job markets have always been very different, since math is a service department and hence universities need a lot more mathematicians. It's not that the job market doesn't suck, it does, but nowhere near as badly as in physics. If you go back to when I was in grad school (late 80s), everyone got an academic job when they got their doctorate. Most of them weren't serious research jobs of course, but you could do some research in most of them. And almost all the jobs were tenure track. It started falling off a cliff the year I got my Ph.D. (great timing on my part), but even so, most of the people I know got jobs, tenure track, just later than they would have, and not in places as good as they were hoping for. My thesis was published in a very very good journal, and when it got seen I actually got asked to apply to a top program – didn't get it, but I did have a shot. If I hadn't have been married, and settled in NYC, and if I could have moved anywhere, I might well have been able to move up to a pretty serious research program. As it is, I have tenure, I enjoy my job, I've done research, and it's been supported. From what I know this is extremely unlike physics.
    Part of the problem is pretty obviously physics should produce many fewer Ph.D.s They're just setting people up for 5 or 10 years of stressful postdocs with almost no chance of a permanent job, and very little opportunity to work on what interests them. Most of them end up doing the laundry, and eventually hating it. An alternative I guess would be not to drop the number of doctorates, but instead just let postdocs do whatever they want. Most still wouldn't get jobs, but they would enjoy what they were doing, and maybe it would drive research in interesting directions.
    One other interesting thing is I really think there is much less laundry in math. Not sure why, maybe it all got done in the 17th Century. No one wants postdocs to sit and do boring stuff for them, it wouldn't help the senior people or the junior people.

12. **Peter Woit**
    June 10, 2015

    Jeff M,
    I also found the contrasts to math quite interesting, including the comments about this and the comments from a mathematician there.
    One thing that struck me is that my experience as a grad student/post doc didn't at all involve a senior person making me work all the time on endless computations. My main memory of the time was of how difficult it was to get a senior person to take any interest at all in what I was doing. I suppose this is just evidence I wasn't doing it right: unless you're an obvious genius, the way to the small chance of a successful career there may very well be finding a senior person who needs this kind of work done, under time pressure, and get it done.
    I also don't see that happening very much in math, where the pressure on young people seems to be more to come up with something oneself, to show that one can do math research oneself at a high level. Part of what is different is the very different job situation.

I often advise young people trying to decide which field to go into that at this point they're more likely to be able to have a career doing work related to deep issues in physics in a math department than in a physics department, and this piece provides more evidence of that. The author's attitude of "I don't do proofs, I do do laundry" I don't really understand.

13. **Jeff M**
June 10, 2015

    Peter,

    You're right about the fact that for young folk in math the thing to do is to show you can do something yourself. Even your thesis. And you never know what it will be. The thing in my thesis that actually got attention was a technical lemma, not the actual result.

14. **Richard E**
June 10, 2015

    The first commenter is spot-on though – "I think the fact that you never really enjoyed learning quantum, E&M, etc was a big warning sign." This person says they didn't much enjoy most of physics, so why am I surprised that their career in physics didn't work out.

    When I am admitting people to grad school a really useful "rule of thumb" is whether the person is more excited about the *idea* of being a physicist than with physics itself — since that is a red flag.

15. **Justin**
June 10, 2015

    Woit,

    I was interested in your comments about advising students. If you're a young student about to enter graduate school with interests in mathematics and physics, and you're having a tough time picking between the two, do you have any advice on how to choose?

16. **Peter Woit**
June 10, 2015

    Justin,
    If you're interested in an academic career after a Ph.D., and find math and physics equally appealing, go to graduate school in math. You're just much more likely to have a chance at a reasonable academic job, and math is intellectually a very healthy subject. If you're interested in the experimental side of physics, that's very different than what mathematicians do. The only way to pursue that is in a physics (or maybe engineering) department. If you're not set on an academic career, but interested in getting a Ph.D and then doing something other than teaching at the college level, then there's no particular reason to necessarily favor math over physics.

17. **condensedmatter**
June 11, 2015

    Peter
    I may add that there is also condensed matter physics, which is a quite healthy subject. There are many interesting problems in theoretical and computational condensed matter physics (and in theoretical chemistry). These problems have usually straightforward connections with experiments. A PhD in these fields can be quite satisfactory for students with interest in the theory side. The job market does not seem terrible, at least in Europe, so that academic or research career is a difficult but still reasonable option.
    P.S. Peter, your blog is a fantastic source of information. I have been following it for several years but it is the first time that I post a comment.

18. **John Baez**
June 11, 2015

    Justin wrote:

    > If you're a young student about to enter graduate school with interests in mathematics and physics, and you're having a tough time picking between the two, do you have any advice on how to choose?

    You didn't ask me, but people often ask me this, so my page of advice to the young scientist includes a section "Math or Physics?"

    This is not about the job situation: it's about the big differences in research in these two fields, which may not be apparent to students who like courses in both subjects.

19. **Chris Oakley**
June 11, 2015

    John,

    I had not read your "advice …" Good stuff. I wonder where "No More Strings"'s "raw naive curiosity" went. Maybe his/her lack of much of this is why he/she decided to get out. What you say about talks is pretty apposite, and the one thing that ought to have persuaded me that I had no future in physics was that I thought 95% of seminars were a waste of time. The most common fault was simply that they assumed that you, too, had been working on a similar problem for the last 6 months. After the first slide nothing much was comprehensible, but no-one in the audience, including me would dare admit that for fear of being thought ignorant. God knows how many hours of my life (and other audience members) went down the toilet as a result. Personally, I would rather outrage the audience than bore them. At least they will remember it.

20. **Tim Campion**
June 11, 2015

    For me, one of the more sobering aspects of this reddit story comes in the comments, where reddit user Valeen discusses his move from string theory to the oil and gas industry. I think there's a big problem if the pipeline of talented young string theorists is being diverted into figuring out ways to burn _more_ of the fossil fuels in the earth.

21. **Peter Woit**
June 11, 2015

    condensed matter and John,
    Thanks for the help with advice to Justin. I should have noted that I'm woefully uninformed about the job situation in condensed matter theory, even more when it comes to Europe or other non-US areas.

Tim Campion,
You might want to keep in mind that probably a lot more ex-HEP theorists end up working in the financial industry and for the NSA or other "Homeland Security" businesses than work for energy companies…

22. **gadfly**
June 11, 2015

Doesn't the existence of the math department hinge more on the intro calculus sequence, remedial courses, differential equations and linear algebra though? Their actual research output is generally of little interest to the outside world short of what goes on in the applied math department. The abundance of math positions then would seem to be furnished by the fact that you can teach intro calculus and ponder the Goldbach conjecture without needing to find a grant to fund your lab equipment or personnel.

In other words, mathematics only possesses superior job opportunities if you are comfortable with being irrelevant; otherwise you're an applied mathematician competing for supercomputing grants like everyone else.

23. **Peter Woit**
June 11, 2015

gadfly,
Sure, the job situation in math is reasonable because of the needed teaching (if you despise the idea of teaching students math at that level, then you should try another field). Not being dependent on grant money has definite advantages. Even on the applied side of math though, while grants are important, there's nowhere near the same imbalance between the number of talented people and the number of jobs as in areas like string theory/HEP theory.

I had in mind contrasting HEP theory vs. mathematics, and was commenting on the job situation. If you instead want to debate who is more irrelevant and unconnected to the real world, it's not at all clear to me that these days mathematics can win that one…

24. **gadfly**
June 11, 2015

Haha fair enough, HEP and much of pure mathematics seem to have considerable trouble relating to the mundane.

I wonder if part of the problem with the modern HEP research paradigm is that it attracts individuals with an attachment to exotic physics/mathematics. Not that such things may not be necessary, thus far no one has formulated GR or QFT without the exotic physics/mathematics, but rather that one seems to get a lot of individuals with an emotional attachment to such things, and if there is a better way to interfere with a scientist's good judgment than to let him/her develop an emotional attachment to an idea I cannot think of it.

25. **CWJ**
June 11, 2015

Most of the students at my not-really-prestigious university who want to be string theorists are swayed by the perceived glamour of the field. It's quite clear talking to them that they are not interested in actually doing physics–they want to come up with that Next Big Idea. Not coincidentally, these are usually not very talented students either. The good students (in our population–the situation is probably different at "better" universities) are more generally interested in challenging problems no matter the subfield. They usually go on to get some sort of good job, if not in academia than at a national lab. (My latest student, who is defending today, has competitive offers from 4 labs.) My own field, nuclear physics, is one that many string theorist may sneer at, but I have steady funding, I work on problems I find interesting and satisfying, I have fun and interesting colleagues from around the world, I get to go to conferences around the world. And the Department of Energy actively encourages universities to hire in my field, as support for their experimental facilities. The competition for jobs is still tough, of course–the grass is no more greener here than elsewhere. True, it's not as glamorous as string theory or particle physics in general–but if and when we actually detect non-baryonic dark matter, or lepton nonconservation in neutrinoless double-beta decay, or many other experimental tests for beyond the standard model, you're going to need nuclear physics, and good, reliable nuclear physics, to make sense of those experiments. Maybe that's "laundry" physics, but it is still fun and satisfying.

26. **gadfly**
June 11, 2015

CWJ:
Sadly I must admit that I used to be just such a student and was quite caught up in the massive hype surrounding HEP. I think part of the problem is the publicity campaign; I was interested originally excited about more "mundane topics", especially polymer physics and biology, but when I heard individuals like Brian Greene wax lyrical about the grandeur of theoretical HEP I had a hard time justifying an interest in something as comparatively "unimportant" as protein folding or percolation. Luckily for me I accidentally stumbled back into my interests and never looked back; a pity, I think, that others are not so fortunate.

27. **Djehha**
June 11, 2015

Dear Commentators,
The picture I get from your comments is not really encouraging for someone who wishes to contribute to theoretical physics.
If theoretical physics seems to cost a lot of money and yet produce very little these days, why not just stop paying people to do research and pay them only for teaching, just as it was done in the past centuries. Good ideas will then certainly come form here and there every now and then. But, since students are supposed to write something before being awarded a degree, why not just let them investigate those ideas: if they don't find anything useful about them, they just report it and they get there degree. If they find something useful, they describe it, they get their degree, and they go on teaching with their diploma and continue working on the idea on their own without getting payed for it. This way, nobody will get payed for doing research.
Is this way of thinking too naive?
I also wonder if the same grey picture is seen elsewhere in some sub-field of other sciences.
Thank you.

28. **CWJ**
June 11, 2015

Djehha,

Why do you say theoretical physics produces very little? There is tremendous vibrancy in theoretical physics, especially if you don't make the mistake of thinking theoretical physics = HEP (or worse yet, = string theory only*). But there is wonderful work being done in nuclear physics, astrophysics, cosmology, biophysics and polymer physics, condensed matter, complex systems…and in HEP, just not only HEP. The job market is tight, but it has been tight since the 1970s or before.

*PS — I am not against string theory, I am against "string theory and nothing else."

29. **Djehha**
June 11, 2015

CWJ,

Sorry, by theoretical physics I meant fundamental physics.

30. **CWJ**
June 11, 2015

Djehha,

So, in other words, you make the mistaken equality. Okay, very well. I don't agree, but let's go with it. Are you suggesting that because "fundamental theoretical physics" is in a bit of lull, that all students of theoretical physics, including "non-fundamental" subfields of theoretical physics that are in fact quite productive shouldn't be paid either?

Also, when you say that the endeavor in "fundamental physics" (I refuse to accept the equality with theoretical physics) costs a lot of money, are you talking about just theory or experiment? As far as theory, only a tiny amount is spent on it. A few years ago the NSF budget for high-energy theory was about $1 million / year, at least for ordinary grants, with about half going to string theory. Maybe about the same from the Department of Energy. But a lot more money goes into experimental physics, and certainly a lot more goes into even other fields of theory such as condensed matter theory. So, relatively few students get supported to pursue theoretical "fundamental physics," and many more get supported to pursue other subfields of theory and even more to do experiment.* So, I think your scenario _is_ naive, or at least not really reflective of how physics is pursued and funded in the US.

*For what it's worth, even in these fields money is very tight and very competitive and I'm not arguing there are just buckets of money lying around. But anyone who thinks string theory is sucking up all the money in the room and failing to produce a final theory for that, is mistaken.

31. **Vlad**
June 11, 2015

"Are you suggesting that because "fundamental theoretical physics" is in a bit of lull, that all students of theoretical physics, including "non-fundamental" subfields of theoretical physics that are in fact quite productive shouldn't be paid either?"

No, he means fundamental theoretical physics only. I thought that was clear from his most recent comment. He's making no statements about non-fundamental theoretical physics.

32. **Peter Woit**
June 12, 2015

Djeha/CWJ,
Theory is relatively cheap compared to experimental physics (although I think the HEP theory NSF budget is more like $10-15 million/year, DOE maybe $50 million/year, compare to HEP experiment of maybe $800 million/year). Much of what this pays for is postdocs, the real problem is the small number of permanent jobs compared to the number of people going into the field. Most permanent jobs are funded not so much by grants, but by university budgets, so funded now out of tuition revenue.

Given university budgets, there's no way you're going to see a big expansion in full-time reasonably paid permanent teaching positions. In principle physics department could decide to hire more in HEP theory, less in other fields, but the trend is the opposite.

33. **CWJ**
June 12, 2015

I agree budgets have been static for a long while so no chance of big expansions. DOE likes to fund theory relevant to their experiments, which nowadays means RHIC, J-lab, and the under-construction FRIB and others. They even offer carrots in the form of "bridge funding" to encourage universities to hire faculty in a certain area. But even that amount is relatively small, and it corresponds only to a mild swelling distributed over many universities, and the competition for these few additional spots–one, maybe two new ones in a very good year–is very tough.

34. **Mesmar Djehha**
June 12, 2015

Dear CWJ, Vlad, and Peter,

I thank you very much for all your comments. Vlad had exactly my point.
Indeed, all your comments give me a more accurate picture of how physics is pursued and funded in the US.

35. **bw**
June 12, 2015

I don't think today's physics job market environment is that different from when I was a undergraduate physics major in the early 1970's with dreams of eventually making a big discovery in theoretical particle physics. (I was at MIT during the charm discovery of the J/Psi – November 1974 Revolution no less!). Fortunately or unfortunately, my travails in the required MIT undergraduate quantum mechanics course convinced me to move out of physics into electrical engineering, and then on to finance.
Over the last 40 years, I have always heard and read about the employment problems of newly graduating PhD physicists, even as I thought about what could have been, as my physics interests continued from afar.

36. **Bernd**
June 19, 2015

Djehha,

there's also a lot of fundamental physics to be explored outside of high energy physics. All sorts of collective quantum phenomena that emerge within interacting many-body systems are no less fundamental than, say, quantum gravity. Or quantum information, which is even more fundamental than physics itself! The techniques people use in these fields are often quite similar to those used in HEP, with the main difference being that there are actual experiments that you can compare your results to.

CMB@50

June 14, 2015
Categories: Multiverse Mania

Last week Princeton hosted what seems to have been a fascinating conference, celebrating the 50th anniversary of studies of the CMB. Hopefully videos and slides will be posted, but one can get some idea of the highlights of the talks from live tweeting that was going on, that is gathered together here. The third day of the conference featured a panel where sparks flew on the topics of inflation and the multiverse, including the following:

> Neil Turok: "even from the beginning, inflation looked like a kluge to me… I rapidly formed the opinion that these guys were just making it up as they went along… Today inflation is the junk food of theoretical physics… Inflation isn't radical enough – it's too much a patchwork. It all rests on rare initial conditions… Akin to solving electron stability with springs… all we have is proof of expansion, not that the driving force is inflation… "because the alternatives are bad you must believe it" isn't an option that I ascribe to, and one that is prevalent now… inflation is pretty but we should encourage young to think about its problems & be creative (not just do designer inflation)
>
> David Spergel: papers on anthropics don't teach us anything – which is why it isn't useful.. sometimes we need to surrender (to anthropics) but that time is not yet now.
>
> Slava Mukhanov: inflation is defined as exponential expansion (physics) + non-necessary metaphysics (Boltzmann brains etc)… we should separate inflation from the landscape… exponential inflation is very useful, the rest is not for scientific discussion… In most papers on initial conditions on inflation, people dig a hole, jump in, and then don't succeed in getting out… unfortunately now we have three new indistinguishable inflation models a day – who cares?
>
> Paul Steinhardt: inflation is a compelling story, it's just not clear it is right… I'd appreciate that astronomers presented results as what they are (scale invariant etc) rather than 'inflationary'… Everyone on this panel thinks multiverse is a disaster.
>
> Roger Penrose: inflation isn't falsifiable, it's falsified… BICEP did a wonderful service by bringing all the Inflation-ists out of their shell, and giving them a black eye.
>
> Marc Davis: astronomers don't care about what you guys are speculating about at all (mulitiverses, pre-big bang, etc).

I was encouraged by Steinhardt's claim that "Everyone on this panel thinks multiverse is a disaster." (although I think he wasn't including moderator Brian Greene). Perhaps as time goes on the fact that "the multiverse did it" is empty as science is becoming more and more obvious to everyone.

Comments

1. **Geoff**
   June 14, 2015

   Although I'm not particularly a fan of inflation and certainly not the multiverse, I can't help but wonder if any theory of the early universe runs into the inevitable problem of initial and/or boundary conditions. It seems that the best one can do in physics is write down a physical law, but there really doesn't seem to be a way to derive the initial / boundary conditions from first principles. Is there any consensus that they are somehow outside the purview of physics?

2. **Mesmar Djehha**
   June 15, 2015

   Geoff,

   From what I understand about the Ekpyrotic scenario, it just claims to do so: eliminate the need for initial/boundary conditions by removing any beginning for the Universe and replacing it with a cyclic history.

3. **Peter Woit**
   June 15, 2015

   Geoff,
   I think Steinhardt's argument is against claims that inflation solves fine-tuning problems (why is the universe so flat?), that all inflation models with a scalar field do is move the fine-tuning problem from one place to another.

4. **Tim Howells**
   June 15, 2015

   As a naive outsider those quotes really surprised me. It looks like something radical has happened since Leonard Susskind was claiming victory in a cafeteria food-fight over the multiverse. That was in 2005, so ten years ago. Was there a turning point, or is this the turning point? I was particularly suprised that they questioned inflation so strongly. Any pointers would be appreciated. Genuinely curious – not flame-bait!

5. **Anonymous**
   June 15, 2015

   This panel is full of (in)famous critics with various agendas. Not very representative of the field, in contrast to the observational talks which looked amazing.

6. **Peter Woit**
   June 15, 2015

   Tim Howells/Anonymous,
   The panel did over-represent critics of inflation, but I think the reason for multiverse skepticism is that string theorists were not on the panel, just cosmologists (except for the moderator, who seems to have been the only one defending the multiverse). Susskind is not a cosmologist…

7. **Douglas Natelson**
   June 15, 2015

   I'm trying to parse Penrose's remarks. Does Penrose think that the BICEP2 results have somehow falsified inflationary cosmology? I don't see how a lack of a statistically significant signal above the dust background is a strong statement one way or the other. Is there some context here that I'm missing?

8. **Student**
June 15, 2015

Peter or anyone,

Where can we watch the videos?

Are they available online?

Thanks!

9. **Peter Woit**
June 15, 2015

Student,
As far as I know, the talks were livestreamed, so in principle there is video. However, it does not seem to be available now, don't know if there are plans to make it available in the future.

Doug Natelson,
I have no idea what Penrose had in mind, perhaps someone else does.

10. **Student**
June 15, 2015

Ed Witten defends inflation:

Ed Witten: the criticisms of inflation are too a priori. Inflation is an incredibly simple theory. The beauty: it's weakly coupled. #CMBat50

Ed Witten: the same (simple, weakly coupled) cannot be said about the competing theories (to inflation) #CMBat50

Why no mention of this Peter? Perhaps you missed it?

11. **Peter Woit**
June 15, 2015

Student,
The extracts I gave were only a small fraction of the discussion, the "sparks flew" ones where people were saying something controversial. There were many other interesting comments, including Witten's, anyone interested should read the whole thing. I don't think it's surprising that Witten (who is not a cosmologist, by the way…) and many others would defend inflation, I'm much more curious whether he'd be willing to defend the multiverse…

12. **Cormac**
June 15, 2015

I think Mukhanov has a point. The word 'inflation' is used in several different ways, and much of the criticism isn't really aimed at the basic notion of an exponential-expansion in the early moments of the cosmos (a hypothesis that gives a very 'natural' and hard-to-rule-out explanation for several features of the observable universe). Most criticisms of inflation are leveled at the apparent consequences of various models of inflation, from the problem of fine-tuned initial conditions to the notion of the multiverse – but if a successful model of inflation ever emerges, these predictions may not part of it. In the meantime, there are few theories that can model current observations the way the hypothesis of an exponential expansion can…

13. **milkshaken**
June 15, 2015

As a naive outsider, I believe Penrose comments (about bringing out all hidden inflation partisans and giving them black eye) was about the premature celebrations of B-mode polarization result that soon turned out to be analysis error…

14. **chris**
June 16, 2015

"The beauty: it's weakly coupled." … speaks volumes. translation: the beauty is that we can use perturbation theory.

this is an unfortunate "search by the lamppost" bias that has influenced physics for far too long now.

15. **Andrew Tho as**
June 16, 2015

Very true, Chris.

"Ed Witten: the same (simple, weakly coupled) cannot be said about the competing theories (to inflation)"

That's no argument in favour of the existence of inflation!

Various Links

June 16, 2015
Categories: Uncategorized

Ordered from the more to less serious…

- On the geometric Langlands front, there's video posted today of this talk by Dennis Gaitsgory. Michael Harris has already commented here about the local geometric Langlands conjecture in terms of 2-categories that Gaitsgory discusses. Today's arXiv listings have a new paper on geometric Langlands by Witten, which begins with his version of a formulation relating the categorical point of view and quantum field theory.
  I wrote recently here about a talk by Jacob Lurie that alluded to an explanation of this categorical equivalence for the simplest case of G invertible complex numbers (rather than the case of G a general semi-simple Lie group that Gaitsgory and Witten are discussing). I'm still far from being enlightened concerning that simplest case, but have learned a lot from Alexander Polishchuk's Abelian varieties, theta functions and the Fourier transform which I take as treating the global versions of this equivalence.
- Witten has been busy, with yesterday's arXiv listings including a 429 page paper with Gaiotto and Moore (does anyone know of a longer hep-th paper that's not a review article?). There's also a companion shorter summary paper. The motivation here is again a categorical picture expressed in terms of quantum field theory models, but for more insight you'll need to find someone more expert than me on this topic.
- Strings 2015 will be held in Bangalore in a couple weeks. Talks titles are now available, so you can see what the hot topics in the "string theory" community are. Of Gaiotto, Moore and Witten, only Witten will be speaking. Perhaps his talk on An Overview of Worldsheet and Brane Anomalies will shed some light on the 429 page paper.
- On the Mochizuki/abc front, hopes rest on a planned workshop at Oxford this December. Fesenko has circulated a letter about the workshop, which gives suggestions about how to approach the subject.
- Via Chandan Dalawat's Google+ page I found out about this autobiographical piece by Misha Gromov. One thing it made clear to me is why I couldn't get anything out of the couple times I've heard him lecture.

  > Being trivial is our most dreaded pitfall: you say stupid things, not original things, outrageously wrong things – all will be forgotten when the dust settles down. But if you pompously call a+b=c "Theorem" in your paper, you will be forever remembered as "this a+b guy", no matter you prove bloody good theorems afterwords…
  > I was introduced to the idea on September 1st 1960 at the then Leningrad University when our analysis professor Boris Mikhailovich Makarov said to me after our first calculus class – he expressed this in somewhat metaphorical terms – that I should've kept my mouth shut unless I had something non-trivial to say.
  > Further encouraged by my teachers and fellow students, I tried to follow his advice and, apparently, have succeeded – I hear nothing disrespectful about my mouth for the last 10-20 years. Strangely, this does not make me feel a lot happier.
  > "Trivial" is relative. Anything grasped as long as two minutes ago seems trivial to a working mathematician.

  Another thought this raises is that I've just spent the last 3 years of my life writing something trivial…

- I really like Jordan Ellenberg's suggestion of Cold Topics Workshops.
- Mathematics makes it into the Guardian with an article about mathematical modeling.
- With the LHC inactive, some LHC physicists have had to spend their time studying plots (trigger warning, and NSFW).

Update: One more, far more serious than anything above. Sabine Hossenfelder brings up an important and rarely discussed topic here.

Comments

1. **M**
   June 16, 2015

   I guess that most readers live in d=4, N=0 and will stop reading at the first line "This paper is devoted to the study of massive two-dimensional theories with (2,2) supersymmetry" unless somebody explains what is the physical relevance of the next 429 pages

2. **Drew Day**
   June 16, 2015

   I understand your long-standing opposition to string theory. I genuinely appreciate your many elucidations on the validity of the subject — especially as an outsider who has no skin in the game (apart from a large part of my physics education coming from Leonard Susskind, who obviously has his now-somewhat baggy skin in).

   However: Not withstanding this sentence, it is grammatically improper to put phrase "string theory" in quotes. It's clear that you (and everyone involved) agree that string theory actually exists, that people label themselves string theorists, and that those opposed to the topic know a string theorist when they see one. So it's unnecessarily derisive to act like the phrase itself is in dispute. Even if all of these fulfilled criteria weren't true, it is **still** incorrect to put jargon in quotes.

   You seem to have plenty of ammunition to attack the content of string theory and those who practice it. So concentrate your fire where it is best applied: the content of your opponents' beliefs and not the name of their community.

3. **Peter Woit**
   June 16, 2015

   Drew Day,
   The use of quotes wasn't in any way intended as an attack on string theory or the string theory community, it was just a quick way of referring to the widely acknowledged fact (see the recent posting "A view from an ex-string theorist") that the name "string theory" is now misleading since most "string theorists" aren't working on strings (and avoiding making the tedious point that most talks at Strings 2XXX aren't about strings). The name really is extremely misleading at this point, but of course I'll keep using it until a better convention comes along. But I don't see a good argument against sometimes adding the quotes in a context where noting that it's no longer an accurate name is appropriate.

   Note added: please, interesting comments about any of the different topics of the posting are encouraged, string theory wasn't actually one of them.

4. **CU Phil**
   June 16, 2015

   Hi Peter,

On the (very serious) topic of Bee's posting, I strongly encourage everybody to read the linked lecture below. It's by a distinguished philosopher, Peter Railton, and was delivered at a meeting of the American Philosophical Association. It's a very personal recounting of many things, but most relevant here is his discussion of his personal battles with depression and the continuing stigma attached to it in academia (and outside academia). He discusses depression starting on p.12 — the section titled "A fourth transition" — but the whole lecture is worth reading.

http://leiterreports.typepad.com/files/dewey-lecture-drs-rev1.pdf

5. **Peter Woit**
June 16, 2015

CUPhil,
Thanks, but I'd rather not encourage a discussion here of the difficult topic of depression, outside of the context of the postdoc system that Sabine puts it in. And, in any case, better that people discuss the topic at her blog, since she has done an excellent job of raising the topic and addressing it.

6. **SeekingSanity**
June 17, 2015

> Being trivial is our most dreaded pitfall: you say stupid things, not original things, outrageously wrong things – all will be forgotten when the dust settles down. But if you pompously call a+b=c "Theorem" in your paper, you will be forever remembered as "this a+b guy", no matter you prove bloody good theorems afterwords…

What a disgusting attitude. I hope he was joking.

7. **Bernhard**
June 17, 2015

I have noticed that Witten has sort of a habit of writing really long "papers" that are actually books. Does anyone read them? I mean, if he wasn't Witten and would bet nobody would. However, being the most influential leader of his field, perhaps there are crazy people who take the challenge? And who judges the usefulness of this kind of paper for the field? Or maybe I'm just being completely naive, so would really like to read an informed opinion to correct my ignorance…

8. **SRV**
June 17, 2015

I agree entirely with M. Peter, is there a way you could tell us what Witten is up to in graduate terms or Susskind lecture level stuff? What is the advance? What did he accomplish this time? Thanks.

9. **jon**
June 18, 2015

Dr Woit linked the summary paper, which is beautifully written and 45 pages long. They state applications on page 2. Treat yourself, don't cheat yourself.

10. **Peter Woit**
June 18, 2015

Bernhard,
The length of the papers isn't really problematic. These are very complex topics, and if you're going to work out details, papers become very long. A shorter paper might be much worse, making it much harder for anyone interested to follow what is going on. There are a relatively small number of people I would guess following the details of many of these things. The time is long gone when just the fact that Witten was working on something brought huge numbers of people into a subject.

Another question is whether the motivation for such technical work is getting enough attention. In the case of geometric Langlands, Witten and others have over the years written up quite a bit of expository material. For this latest 429 page paper, the motivation to me is still somewhat obscure, maybe just because I haven't had time to look more carefully at the summary paper. If there is good motivation for this work, I'm sure in the future we'll see more papers explaining this motivation.

11. **jon**
June 18, 2015

Perhaps 2d N=2 LG models and knot homology as motivations somehow leave the reader cold. The 2d/4d correspondence should not.

12. **Eduardo Lira**
June 19, 2015

One of the most reputed scientists of our time writes a paper but all you have to say about it is that it's too long? Now that's as much trivial as one can get.

13. **MathPhys**
June 19, 2015

I knew that F Dolan died at a young age a number of years ago. I did not know that it was this way. The Hell with the post-doctoral system and the rest of it. It's just not worth it. The abuse that's described in commentaries on the same page is all too real. This is madness.

14. **Peter Woit**
June 19, 2015

Eduardo Lira,
You don't seem to have read the little I did say about it. I'm not of the opinion it's too long, actually wish it were slightly longer, with a bit more explanation of the motivation of the work. If I understood that better I might have more to say (or maybe not, this was a posting intentionally of short items and links).

15. **M**
June 20, 2015

Dear jon, when Einstein started avoiding quantum mechanics, when de Broglie and Heisenberg started avoided quantum field theory, when Dirac started avoided renormalization, physicists thought that they were misguided and moved on ignoring them.

Now, page 2 of the short paper starts: «Let X be a Kahler manifold, and W : X → C a holomorphic Morse function. To this data physicists associate a "Landau-Ginzburg model." It is closely related to the Fukaya-Seidel (FS) category» and concludes «Two of the motivations for the detailed construction of interfaces are the nonabelianization map of Hitchin systems that arises in theories of class S, and the application of supersymmetric gauge theory to knot homology».

Seriously: I don’t know what to think about this. Do such two-dimensional SUSY systems have relevance for physics? If yes, how? Or are they a way of avoiding physics?

16. **Bernhard**
June 20, 2015

Hi Peter,

Thanks, I got it.

Multiversal Journeys

June 17, 2015
Categories: Multiverse Mania

One of the many efforts to promote the Multiverse to the public way back when (2005-2007, some of their advertisements are here) was an organization called Multiversal Journeys. Back in 2006 they got $77,000 in funding from the Templeton Foundation (via FQXi). Since 2007 they seemed to have disappeared, with no more scheduled public events as far as I could tell.

Now they're back though, with an upcoming event defensively called Clarifying theoretical physics and cosmology misconceptions for SF Bay Area journalists. It's unclear exactly what they think the misconceptions are that such journalists need to have clarified. One of the three announced speakers (John Terning) might actually be able to do some clarification, but the other two seem more devoted to the spreading of misconceptions.

Yasunori Nomura likes to promote the idea that the landscape and the many-worlds interpretation of quantum mechanics are one and the same thing (see here and here), a claim I believe few serious physicists have ever been able to make sense of. He also claims that the multiverse can be used to make predictions about physics, and back in 2009 used the multiverse to predict that the Higgs mass would be 141 +/- 2 GeV (see here). This prediction played a central role in the film Particle Fever, which featured David Kaplan and Nima Arkani-Hamed during the period leading up to the Higgs discovery explaining how a 140 GeV Higgs would mean the multiverse was right. Will Nomura clarify for the journalists any misconceptions they might have about what scientists are supposed to do when their theory's well-publicized prediction is tested and shown to be wrong?

The other speaker is Texas Tech chemist William Poirier, who has some sort of "Many Interacting Worlds" theory, which supposedly shows that the observed behavior of particles indicates the existence of parallel universes (see here). Again, I'm curious what misconceptions about physics he plans to clarify to journalists.

Update: I see that this event will be funded by at $5750 grant from FQXi to Multiversal Journeys (Fall 2014 Mini-grant).

Comments

1. **Filux**
   June 17, 2015

   Ah, so the multiverse does make predictions and the prediction was wrong. So, the multiverse is falsifiable. Therefore, you were wrong, Peter.

2. **KJ**
   June 17, 2015

   Filux,
   You misunderstand the nature of the multiverse theory. All Nomura's failed "prediction" means is that the particular flavor of multiverse theory he was espousing in 2009 is false. It does not mean that multiverse theory itself is false. In some other version of multiverse theory, the Higgs mass takes precisely the value that was found at the LHC. In fact, since he is still promoting multiverse theory, he clearly still believes in it despite his failed prediction. And that is precisely Peter's point: Every possible outcome can be "predicted" by some flavor of multiverse theory, thus it is not falsifiable.

3. **Martin Kochanski**
   June 17, 2015

   I have been meaning to ask for some time whether your observations of multiverse mania have got you anywhere near an answer to the question "What is the multiverse for?". To offer meaningful criticism of *any* project it is first necessary to know what the project is trying to do. In this case the doers of the project often don't seem to know, or even to want to know, so it seems to be up to you.

   I dont't even find it easy to discern whether the multiverse is meant to encompass universes with all possible values for the constants of nature, or only of some of them. Or, for that matter, whether we are talking about one common set of laws of nature into which the constants are plugged (raising the question "Why?" all over again but about laws this time rather than constants), or whether there are universes for every possible set of laws as well.

   Does the multiverse contain a Newtonian universe? If not, why not?

4. **Peter Woit**
   June 18, 2015

   All,
   Just deleted a long thread of arguments about math/physics/AdS/CFT, which really had nothing to do with this.

   Martin,
   The goal is to justify failure. The multiverse did it, so the fact that string theory/susy/your favorite theory hasn't worked out despite 40 years of effort is not the fault of your favorite theory and you can keep on working on it, despite the failure of the idea.

5. **Patrick Dennis**
   June 18, 2015

   Peter, have you done a column elaborating your own take on many -worlds?

6. **Peter Woit**
   June 18, 2015

   Patrick Dennis,
   I've probably commented on this somewhere, should someday write more. I don't want to start a discussion here (it's a complex topic). In brief though, my point of view is that you can consistently talk about decohered states as different "worlds" in some contexts if you feel like it, but these are just empty words, and the people using them often do so to evade the interesting questions about QM (how does classical behavior emerge? Why certain observables? why the Born rule? etc.).

   Adding this empty "interpretation" to an empty claim of a multiverse with different laws of physics in each universe seems to me to be emptiness^2, and as far as I can tell that's what Nomura is trying to sell.

7. **Cormac**

June 19, 2015

Hi Peter, I have a different answer to the question "what is the multiverse for?".
Excuse the armchair philosophy, but my understanding is that the hypothesis of a multiverse was not originally proposed "for " anything, i.e., was not proposed as an answer to any particular puzzle. Instead, it emerged uninvited as an unattractive consequence of most models of inflation. While the hypothesis of inflation gives predictions that are nicely consistent with observation, the downside is that it has proved impossible (so far) to rule out the prospect of inflation happening at different rates in different regions of the infant universe…so the multiverse is what you might call a 'bastard' theory.
The fact that some have jumped on the idea to fit various other proposals should not cause us to lose sight of how the idea initially arose..

8. **vmarko**
June 19, 2015

Cormac,

As far as I could tell, what came from inflation was the most benign form of the multiverse — a bunch of bubbles in the ever-expanding spacetime, all of which have identical coupling constants. Such a thing doesn't even really deserve the name "multiverse", as it is a single spacetime continuum with a single set of laws of physics (and with a somewhat complicated structure), i.e. a "universe".

The first real problem started when string theory got mixed with this scenario, with its 10^500 vacuua, so that low-energy physics could in principle be different (and random) for each bubble. Although this is arguably also still just a single spacetime continuum, one could justify the name "multiverse" in some way.

The second real problem started when people started making identifications of different bubbles with different wavefunction branches in the many-worlds interpretation of QM. This is where things really got messy and the "multiverse" idea got promoted to cargo-cult levels… 🙂

9. **Paul Benoit**
June 20, 2015

By the strength of your conviction, I assume you have a solid rebuttal to the Many Interacting Worlds approach? Seems to me that if he can get the same precise predictions with a vast degree less 'work' then he's on to something. You appear to be opposed to his view but have proven yet to provide anything at odds. I don't want to believe in any form of parallel universe, it's messy, unknowable, and puts an insurmountable barrier far to close in the future for comfort.

I'm disappointed in your lack of informed reposte to any of it thus far.

10. **Peter Woit**
June 20, 2015

Paul Benoit,

Look, there are literally thousands of people out there who are convinced that they have some revolutionary new way of replacing the fundamental ideas of quantum mechanics by something they find more appealing. If one paid close attention to even a small fraction of these (such as the ones who get a press release issued and get in the media) and tried to carefully explain what was misguided about what they are claiming, that would be a full-time job. I've got better ways to spend my time, already waste far too much of it trying to argue against bad ideas being pushed by very prominent theorists, so don't feel the need to branch out into arguing against bad ideas that virtually no one takes seriously.

In this case I see no evidence at all that getting rid of wave-functions and the Schrodinger equation in favor of "Many Interacting Worlds" is any sort of improvement of the conventional theory. The claims about "same predictions with less work" are about certain specific approximations to very complex quantum systems and maybe are useful there. As far as I can tell, this idea makes simple quantum systems far more complicated and hard to analyze, so at a fundamental level, it's an idea for "(maybe) same predictions with a lot more work".

11. **Neil**
June 20, 2015

It is not any of the multiple multiverses that I object to. It is the idea that coupled with the anthropic principle, the multiverse has explanatory power.

12. **marten**
June 21, 2015

It should make no difference whether the multiverse concept is coupled with the anthropic principle or with the dinosauric principle.

13. **Andrew Thomas**
June 21, 2015

The MWI doesn't attempt to "get rid of wave-functions and the Shrodinger equation" – it is merely an interpretation. Which means it takes the results you already have, and the equations you already have, and then says "Oh, this appears to indicate the existence of an infinity of parallel universes". But, apart from that it brings nothing new, or changes any of the equations you already have, or else it would be testable.

14. **Andrew Thomas**
June 21, 2015

Paul Benoit says: "Seems to me that if he can get the same precise predictions with a vast degree less 'work' then he's on to something."

Yeah, it would be, but he can't.

15. **Andrew Thomas**
June 21, 2015

Can I point out that absolutely ANY interpretation of quantum mechanics you might want to dream up – no matter how insanely crazy – would be 100% equally valid as the MWI (as long as it fitted the results). Which is why we have so many interpretations of quantum mechanics – all on exactly the same level of validity.

16. **Nobody**
June 21, 2015

The beloved many worlds interpretation is supposed to be simpler because "infinity is simpler than 1" (no symmetry breaking). But is it? Which transfinite cardinal number are we talking about? If the 1st, why is that simpler than 1? Anyone with an IQ over 80 can learn to count starting with '1'. Can you teach him to understand transfinite cardinals?

If we have a continuous distribution of probability, the countable infinity will not do anyway. How does the continuum hypothesis fit into this "infinity is simpler than 1" justification for calling many-worlds simple.

17. **Magnema**
June 21, 2015

@Nobody: Saying "all" is far easier than saying "choose this one" – and I'm pretty sure that holds up from an information-theoretic standpoint, although I don't know too much about that myself. It's not the "1" that's the problem; it's the "that one" that is the problem.

Of course, it's not quite "all" (unless we're talking Lewis' modal realism), it's "all with XYZ properties," but it still stands that "all with XYZ properties" is simpler than "this one with XYZ properties"…

@Peter Woit: So your problem with MWI (setting aside the many people who pretend to understand quantum mechanics but don't) is not so much that it's a bad theory, or even that it shouldn't be preferred fundamentally, but rather that it'll end up being a lot more work for (at best) the same results? So, seeing as we should use collapse mechanics for all practical computations anyway, it's simply not worth studying, so to speak?

18. **Andrew Thomas**
June 21, 2015

Magnema: " It's not the "1" that's the problem; it's the "that one" that is the problem."

Essentially, this is the age-old battle against indeterminism which goes all the way back to Einstein and Bohr: "God does not play dice". Why should it be "that one?"

I suspect people will always try to fit quantum mechanics into a deterministic, classical framework. But that's purely through human bias – not through cold analysis of data. The MWI is merely another step in an attempt to remove indeterminism. I'm sure it won't be the last.

19. **Peter Woit**
June 21, 2015

Enough about MWI. I think there's now plenty of evidence for my point of view that discussion of this topic is just a complete waste of time.

20. **paddy**
June 21, 2015

At the risk of being trivial and other things, I want to note that sometime in the last week or so Jeopardy had a category/question to which I (on my couch) and the ringing-in contestant answered "multiverse". He was told he was wrong and the "right answer" was "parallel worlds". Well..some time later Alex awarded the contestant his lost points saying that the referees judged his answer to have been equally correct. Not sure if this is a good or a bad thing….tho it did amuse me 🙂

21. **tt**
June 21, 2015

just want to point out 'MWI' = many worlds interpretation,
is not exactly the same as 'MIW' = many interacting worlds

22. **Peter Woit**
June 21, 2015

tt,
Thanks, my last comment should have read "MWI or MIW".

23. **Gus Bici**
June 23, 2015

Sean Carroll compares Multiverse to Australia:
https://mobile.twitter.com/seanmcarroll/status/613043720795992066
Crikey.

Strings 2015

June 21, 2015
Categories: Strings 2XXX

Strings 2015 gets underway in Bangalore tomorrow, and so far the press coverage I've seen consists just of two articles that aren't what I'd guess string theorists would like to see:

- In The theory that may have been stringing scientists along for years Robert Matthews starts with

  > Over the next few days, the southern Indian city of Bangalore will be playing host to an exclusive group of people.
  >
  > No amount of money can buy you membership and it doesn't matter how well connected you are. But rumour has it that it helps if you have a brain the size of a planet.

  but soon moves on to

  > There's just one problem: there's not a shred of evidence to support it. And that is now leading to awkward questions about just what all these very smart people have been doing with their time and funding.

  He then goes on to discuss the recent New York Times Op-Ed, as well as a Physics Today piece

  > The American Institute of Physics has now gone further, with an editorial in Physics Today asking if the insouciance of string theorists over the issue of testability may harm public faith in science.

  Finally, there's an accurate discussion of the relation of string theory to the search for superpartners in Run 2 at the LHC

  > The real fun and games will start if the particles are not found. That is because, even after all these years, string theory is not tied down very tightly.
  >
  > As a result, theorists can tweak their equations to explain away the failure.
  >
  > Such flexibility is a classic symptom of pseudo-science, and it is what particularly irks physicists about string theory – along with the arrogance of some of its practitioners, who insist it is the only way to complete Einstein's quest.
  >
  > But there is another source of resentment, and one with which we can all sympathise: that an entire generation of brilliant minds may have been lost to an esoteric mind-game.

- The other string theory news story is at The Telegraph, a Calcutta paper, which has Physicists too can have fun. The article is about Ashoke Sen's bizarre hep-th article Riding Away from Doomsday, which was the topic of an April 1 posting here. Since then, Sen has shot down any idea that this was a hoax, and has turned it into a major research program, with a technical hep-th article and a talk scheduled for the opening session of the conference tomorrow. The article argues for the importance of computing the decay rate of the universe since it

  > is important for planning our future course of action if we are to adapt this strategy for increasing the life expectancy of the human race.

  String theory is supposed to provide this calculation, but Sen characterizes the status of that calculation with

  > it is probably fair to say that we do not yet have a definite result based on which we can plan our future course of action.

  which shows that, despite what some people say, he definitely has a sense of humor.

Given press coverage debating whether string landscape research is pseudo-science or a joke, I wonder what prominent string theorists gathered in Bangalore now think of the string theory community's decision over the last ten years to mostly go along with the idea of the string landscape as an excuse for why string theory predicts nothing. This all started about ten years ago, and it's interesting to take a look again at the panel discussion at Strings 2005, where the panelists were in favor of the landscape, but the audience not so much. The last ten years have shown conclusively that any hopes for predictive science coming out of the landscape were misguided, but will that or bad press cause anyone to re-evaluate their stand on this?

Update: Slides of the talks are appearing here. One of them is so interesting I'll devote a posting to it…

Comments

1. **Bernhard**
   June 21, 2015

   " Since then, Sen has shot down any idea that this was a hoax." Wow, it seems one get away with really anything these days in theoretical physics once you're on top enough. HEP-TH, the official laughing stock of science.

2. **South Park is brilliant**
   June 21, 2015

   It's just not true that "There's just one problem: there's not a shred of evidence to support it." It is a consistent unification of quantum mechanics and gravity. That in itself is evidence that it is on the right track.

3. **Dave Miller in Sacramento**
   June 21, 2015

   SPib wrote:

   > It is a consistent unification of quantum mechanics and gravity. That in itself is evidence that it is on the right track.

   But does the theory actually exist? Can you actually write down any non-trivial solutions?

You know the widely accepted claims that phi-to-the-fourth theory does not actually exist in 4D (i.e., except as the trivial free theory)? Do you have any evidence that something of the sort is not true for superstring theory?

For that matter, can you even write down the equations for the mysterious M-the0ry that supposedly is the reality behind strings?

These are not rhetorical questions: I am not as critical of string theory as Peter is. If nothing else, solely as a matter of mathematical curiosity, I would like to know the answers to these questions.

Can you answer any of them?

Dave Miller in Sacramento

4. **Peter Woit**
June 21, 2015

All,
Please resist trying to have the same tired arguments about string theory. At this point, anyone who argues that "string theory is a consistent theory" has long ago heard that the problem "what is string theory?" has not yet been solved, but decided that there's no contradiction between something not being well-defined and its being consistent.
20 years ago, this kind of thing was the problem with arguments for string theory. Things are different and much worse now, with the move to the string landscape pseudoscience/joke.

5. **Jonas**
June 22, 2015

It is an experience of life that whenever persons start talking or writing about the end of the world/universe/civilisation – like Sen is doing – it is because they are feeling their own end approaching. One should be kind and understanding to the person, and at the same time ignore the topic and arguments.

After all, previous experience can be seen to imply that the world/universe/civilisation survives the death of single humans.

But who knows; maybe the person is right this time?

6. **Visitor**
June 22, 2015

"The American Institute of Physics has now gone further, [. . .] asking if the insouciance of string theorists over the issue of testability may harm public faith in science."

Speaking strictly for myself, as a non-scientist, I can say that their concern in misplaced – slightly. My attitude towards science has not changed. My faith in scientists, however, is greatly diminished. (Although this is not solely because of string theory and multiverse mania; the last decade and a half has seen accelerated trends that, along with the growth of "science of a new type", compel me to realize that scientists are as venal and corrupt as the rest of modern society.)

7. **Dave Miller in Sacramento**
June 22, 2015

Although, Peter, maybe someday someone actually will come up with a non-trivial solution to string theory, or a set of definite equations for M-theory or whatever! And, I (and I suspect you yourself) would find that event quite interesting.

Dave

8. **Giotis**
June 22, 2015

Special attention to Gaberdiel talk.

There is a modest revolution going on for some time now regarding the underlying symmetries of String theory.

This revolution encompasses AdS/CFT and Higher Spin HS/CFT dualities.
Roughly the picture is like this:

As it is well known String theory besides its massless sector has an infinity number of massive higher spin fields. These fields are distributed in various trajectories (so called Regge trajectories) based on the relation between the mass and the spin of each field. The leading Regge trajectory is the one with the lowest mass for a given Spin. At its tensionless limits these massive fields become massless and it is generally expected that at this limit String theory will possess a huge underlying symmetry.

Now Higher Spin theories on AdS spaces pioneered by Vasiliev have an infinity number of massless Higher Spin fields too and thus posses a Higher Spin gauge Symmetry. This higher Spin gauge theory includes the spin 2 graviton and generalizes pure gravity in AdS. This basically means that the underlying space possesses a higher space time symmetry which by gauging it gives rise to the higher spin gauge theory much like gauging Poincare symmetry gives rise to pure gravity.
The general expectation is that this higher spin symmetry is a closed subsector of the bigger underlying symmetry of String theory at its Tensionless limit. The higher spin fields of Vasiliev's HS gravity theory should correspond to the higher spin fields of the leading Regge trajectory of String theory.

Now via a Higgs like phenomenon this huge symmetry of String theory is broken and Tension is introduced, as a consequence the higher spin string fields become massive.

The goal is to verify these general expectations and analyze the higher underlying symmetry of string theory using these facts.

For this you can use AdS/CFT by embedding HS/CFT dualities to Stringy AdS/CFT dualities.

Generally speaking an interacting Vasiliev's HS theory is dual to a free CFT on the boundary where the 't Hooft coupling is zero but N is large.

The conserved HS currents of the free CFT correspond to higher spin gauge fields in the bulk.

For example in AdS_4/CFT_3 Klebanov-Polyakov famously conjectured (and later verified by others) that there is a duality between a vector like CFT (and not a Matrix like in Yang Mills theories) and Vasiliev's HS theory in the AdS_4 bulk. Then you can try embedding this HS duality in String theory dualities.

The famous ABJ triality paper for AdS4/CFT3 and Gopakumar Gaberdiel work on AdS3/CFT2 are very important in that respect.

Check:
ABJ Triality: from Higher Spin Fields to Strings:
http://arxiv.org/abs/1207.4485

Higher Spins & Strings:
http://arxiv.org/abs/1406.6103

and subsequent work

9. **nicola**
June 22, 2015

The title of Witten's talk is really something "What Every Physicist Should Know About String Theory". So we learn that every physicist should actually know string theory. That's really funny!

10. **Peter Donnelly**
June 22, 2015

I had to look up "insouciance". It means "casual indifference".

Very apt.

11. **paddy**
June 22, 2015

Sorry but todays featured article on Wikipedia is…(drum roll) …M-theory. Thus it must be defined….nicht wahr? 🙂

12. **Peter Woit**
June 22, 2015

paddy,
I notice in that very long article, no space at all is found to discuss what the problems with the idea of M-theory as the TOE are. All I see is one sentence saying that "some" criticize the idea. At least the single source given in the footnote is one I can't argue with…

13. **Jan Reimers**
June 23, 2015

Nima's slides appear to be full of SUSY optimism: here

– LHC run #2:
1 SUSY even per minute,
100 times the particle production of run 1 (because E^4 in cross section),
100 fb^-1 by 2017, which is enough to discover a <=1.8TeV gluiono ot <=1TeV stop.
– Q: Should we be worried/depressed about prospects for low-E susy after run #1?
A: Not much more worried that we should have been (were!) after LEP II.
– There is a "Why no SUSY @ LEP" slide that I cannot understand.
– Naturalness: Not problems – Opportunities (Thankfully no mention of multiverse!)
– He mentions something called "Compulsory Natural SUSY" which apparently gives better unification of the (3) coupling constants than does "Natrual SUSY".
– He is pushing for a 100TeV ring and a Higgs Factory. We need these to map out the Higgs potential.

Of course no mention of strings.

14. **Steamy Ray Vaughn**
June 23, 2015

Peter, just some information that may be of interest:

At Strings 2015 Nima Arkani-Hamed gave a talk 'Collider Physics at the LHC and Beyond' and said, about finding supersymmetry: "we should always be looking for it" 59:28, and "we could just be unlucky and miss it" [at the LHC] 59:49.

https://strings2015.icts.res.in/talkTitles.php

I take this to mean he believes there is >50% chance we'll find something at the LHC.

If I understood correctly, not finding supersymmetry at the LHC would only mean adjusting parameters by 1%.

15. **Peter Woit**
June 23, 2015

SRV
(moved your comment to more appropriate posting).

I took a look at the Arkani-Hamed talk, didn't see anywhere he said he believes better than 50% chance of SUSY at the LHC. He says the multiverse is still the best explanation around, and has nothing new about SUSY: according to "naturalness" arguments you were supposed to see it at the Tevatron or LEP. By the same arguments you could say there was still one chance in 10 that it was at higher energy, so would show up at the LHC. All he's saying is that if you don't see it at the LHC, you can keep playing the same game as for the last 20 years and say there's one chance in 100 (1% tuning) that it is at higher energy. He's spent much of his life on SUSY models, so thinks looking for SUSY is a good argument for a 100 TeV accelerator. I don't think so, instead the LHC will have provided extremely strong (99 to 1) proof that a not very good idea that didn't explain much really doesn't do what you wanted it to.

The more interesting things about the talk I thought were:
1. Despite being at a string theory conference, no mention of strings. For a 100 TeV machine he's not going to even try the "maybe we'll see strings and extra dimensions" argument that many used for the LHC.
2. The serious question for a next generation machine is about what it would tell us about the Higgs, and that was much of his talk.

16. **nicola**
June 29, 2015

Is there any way to see Witten's 2nd talk. It's not here
https://strings2015.icts.res.in/speakerProfile.php?sId=70

I didn't know that every physicist should know anything about ST so I'm curious what he meant.

Convergence

June 22, 2015
Categories: Uncategorized

Another conference that starts today is the Convergence conference at the Perimeter Institute. The concept is explained by Neil Turok here. His point of view is one I'm very much in sympathy with:

> Turok explains that the "large bandwagon" of the last 30 years has not found experimental support. The bandwagon in question is the Standard Model of particle physics established in the 1970s, which, he says, people have been elaborating ever since. "Grand unified theories, supersymmetry, string theory, M-theory, multiverse theory," he lists. "Each is not particularly radical, but is becoming ever more complex and arbitrary."
>
> To illustrate the lack of experimental support for these ideas, Turok describes how many people were hoping string theory would represent a radical development; but since string theory – as currently interpreted – leads to the multiverse, Turok describes it as the "least predictive theory ever".
>
> Indeed, experimental support has not been found for other extensions of the Standard Model either. "We have discovered the Higgs and nothing else," says Turok, "yet the vast majority of theorists had been confidently predicting WIMPS (weakly interacting massive particles) and supersymmetric particles…Theorists are walking around in a bit of a stunned silence." He adds that it could turn out to be right that all sorts of other particles are needed along with the Higgs – but that thought seems to be misguided.
>
> "My view is that this has been a kind of catastrophe – we've lost our way," he says. "What we need are ideas as simple and radical as in the start of the 20th century with quantum mechanics."
>
> So what might these ideas look like? Turok explains how observations have shown that the universe is simpler than we ever expected – in contrast to our theories, which are becoming ever more complex.

It's great to see this, very encouraging to think that a more reality-based point of view on fundamental physical theory may finally be emerging. My only criticism is that the program doesn't seem to include any mathematics or mathematicians (except perhaps in the context of history and Emmy Noether). If you have a structure you don't understand, which is unusually simple, with surprising explanatory power, you might want to ask mathematicians about it…

The schedule is here, there's a blog here.

Update: You can follow a lot of what is going on at this conference on Twitter, here. For example, I was glad to hear about this comment from Dimopoulos

> There is no difference that we know right now … between the story of divine intervention and the multiverse.

It's great to see a conference on fundamental physics where the multiverse is coming in for some appropriate skepticism.

Update: One thing to say about the multiverse, it does provide an excuse for an endless number of popular articles mulling it over. Just today, there's Caleb Scharf and George Johnson.

Comments

1. **Mesmar Djehha**
   June 22, 2015

   Dear Peter,
   I would like to know please what structure do you have in mind when you say "If you have a structure you don't understand, which is unusually simple, with surprising explanatory power, you might want to ask mathematicians about it…"
   Isn't risky to ask mathematicians about the subject because all they will want to do is first put your question into context by inventing all sorts of axioms and, hence, complicate the subject further and drive it farther away from reality without any deeper insight on the physics?
   Thank you.

2. **Andrew Thomas**
   June 22, 2015

   I am most impressed with Neil Turok. I like the fact he is in charge of such an influential institution but he doesn't "play it safe" by going along with the mainstream – which must be the temptation. He is prepared to go against the flow and take some risks.

   I also suspect he is correct – which always helps.

3. **Peter Woit**
   June 22, 2015

   Mesmar Djeha,
   You haven't talked to many mathematicians… They come in all kinds, with all kinds of interest and expertise. Some might behave as you suggest, all you have to do then is ignore them, you haven't risked much.

   I was of course oversimplifying the situation. There is already a very active area of interaction between mathematicians and physicists both interested in fundamental physics. My suggestion was just that Turok and Perimeter might want to encourage that by bringing some of it into this sort of event. You can take the attitude that you don't think mathematicians know anything that could be of use, but given how stuck physicists are, I don't think they should be so quick to convince themselves this isn't a potential route to progress.

   If you want an example of a mathematician who might play a useful role at a conference like this, one would be Graeme Segal. Recently on this blog, see
   http://www.math.columbia.edu/~woit/wordpress/?p=7773
   there was some discussion of a talk by him on Wick rotation. He's a mathematician who has thought very deeply about this, and it's not at all impossible that this could give an insight into the nature of time that physicists would not otherwise come up with. He knows a great deal about quantum field theory, and is not at all someone who is going to make things more complicated rather than less.

4. **AcademicLurker**

June 22, 2015

Peter,

If reconsidering our basic ideas is going to be necessary to move forward, to what extent do you think this will include some of the fundamentals of quantum field theory itself, as opposed to just the standard model?

5. **Radioactive**
June 22, 2015

Peter, I never understand this frequent wish you make for mathematicians to come in and sort out physics. One of the most damning criticisms of string theory (and other "Standard BSM" approaches) is the lack of experimental support and the movement towards anthropics, landscapes, multiverses and the like.

Do you think mathematicians, not well known for their solid grounding in reality, are likely to bring physics back to earth?

6. **Peter Woit**
June 22, 2015

Academic Lurker,
Yes, I think new ideas not just specifically about the Standard Model, but about the kind of quantum field theory (gauge +spinor fields) used in the SM may be needed. More specifically, ways to think about this kind of quantum field theory as constructing representations of certain infinite dim symmetry groups (or whatever the right generalization of a representation is…). In physicists language: new ways of exploiting symmetries in QFT.

Radioactive,
Anthropics, landscapes, the multiverse etc. are the kind of thing physicists come up with, not mathematicians. What mathematicians are very good at is knowing the difference between what they understand and what they don't, and not fooling themselves about it. In addition, there undeniably are some very sophisticated mathematical ideas being used in the Standard Model. While physicists are to some extent trained to know something about these, there are people who are actual experts on them, and they tend to work in math departments.

7. **Radioactive**
June 22, 2015

The historical parallels aren't exact of course (and never are) but it seems like you are waiting for the 21st century equivalents of Jacobi, Poisson and Hamilton to come and formalise existing physical theories and link them to existing mathematical structures. I would prefer the 21st century Plancks, Bohrs and Heisenbergs to bring new physical theories, rooted in experiment, to explain old and new inconsistencies. Perhaps you think the current experimental state of the art isn't leaving enough clues for prospective Planks but, in my opinion, the list of known unknowns now is at least as rich as it was in 1900.

Eventually of course the 19th century formalism turned out to be useful for the 20th century revolution and I certainly wouldn't want to claim mathematicians interested in physics are useless, but I don't think it's debatable that the real progress came from physicists and would have happened with or without the previous century's mathematical developments. Mathematicians are likely to have good ideas about the mathematics of Quantum Field Theory but progress in physics (and that is what we care about right?) is more likely to come from people who understand error bars and know the values of some fundamental constants.

8. **Peter Woit**
June 22, 2015

Radioactive,
Yes, I think the lack of clues in available data is the main reason conventional paths to progress are not working in physics. In particular I disagree that there are now known unknowns anything like what was available in 1900. At that time there was already a huge amount of spectroscopic data about the energy levels of atomic systems, and it was this data that made possible Bohr and Heisenberg's advances.

9. **Radioactive**
June 22, 2015

The combination of cosmological data (CMB, dark matter, dark energy), particle data (neutrino oscillations, old and forthcoming LHC data), theoretical questions (colour confinement, strong CP, black hole information) and null results (proton decay, horizon problem) seems as strong a list as was available at the beginning of the 20th century. I am sure at the time it looked equally impossible to discover anything useful from a hodgepodge of spectra. Perhaps I am just more optimistic than you.

10. **Tammie Lee Haynes**
June 22, 2015

Dear Dr Woit

I was astonished that you were pleased by the absurd statement made by Dr Dimopoulos. "There is no difference that we know right now … between the story of divine intervention and the multiverse."

I appreciate you wish to avoid theological discussion, but my point is more about evidence than anything else. Thus I remind you that a large number of independent witnesses have reported occurrences of Divine Intervention, in the New Testament, at Fatima in 1917, and elsewhere.

Of course, you may find the accounts of these, (or any other witnesses) to be non-credible, But of course, the accounts have been found to be credible by billions of people. And more importantly, the accounts of witnesses is direct empirical evidence, which is a lot more than your Multiverse promoting colleagues can offer.

Sincerely
Tammie Lee Haynes

11. **Peter Woit**
June 22, 2015

Tammie,
I think Dimopoulos was referring to questions of the origins of the universe or of the laws of physics, not of human experience. Sure, in terms of the latter, there's more evidence of divine intervention than of other universes with different physics.

That said, any further theological discussion is strongly discouraged…

12. **David Nataf**

June 22, 2015

"We have discovered the Higgs and nothing else," – Turok
That's incorrect, I don't know why people say that.

Dark energy has also been discovered in the past thirty years. Dark matter and massive neutrinos may have as well, depending on where you set the discovery date.

13. **Peter Woit**
June 22, 2015

David,
Turok doesn't say "We have discovered the Higgs and nothing else in the past 30 years", you're putting together two things a couple paragraphs apart, taking them out of the context of what he is saying. The "30 years" is a reference to the history of speculation about GUTs, SUSY, string theory, M-theory (40 years is actually more like it at this point). The "and nothing else" comment about the Higgs isn't associated with a date, and refers to those speculative ideas. Put differently, I don't see him saying that dark matter and dark energy are not discoveries, but that they are not ones of the sort that were predicted by GUTs, SUSY, string theory, M-theory, the multiverse (unlike nonexistent LHC SUSY WIMPs).

14. **M**
June 23, 2015

dear Radioactive, the situation is different from ≈100 years ago. Then, matter was not understood and a lot of data was accumulating. Today, only Dark Matter remains not understood, and so far we see Dark Matter only trough its gravity.
Then, mathematical physicists had grasped some aspects of classical mechanics later relevant for quantum mechanics, but Heisenberg and Schrodinger invented quantum mechanics from data, without knowing these mathematical instruments.
One similarity is maybe that 100 years ago most physicists believed in aether and tried to insist on it despite negative experimental results…

15. **Thomas Larsson**
June 23, 2015

Apart from the absense of BSM physics, one striking fact has been learnt in recent years: the Higgs mass appears to sit exactly at the border of stability. 1 GeV less and the SM vacuum would be unstable, 2 GeV more and the SM would be boringly safe in the stable region. The authors of http://arxiv.org/abs/1307.3536 calls this the principle of living dangerously. I think this may be an important hint, and in any case it is pretty much the only hint that experiments has provided.

16. **Radioactive**
June 23, 2015

M, I strongly disagree. Kelvin's famous statement "There is nothing new to be discovered in physics now. All that remains is more and more precise measurement." was made in 1900. They had a pretty good grasp of nature and the spectacular successes of Maxwell's electromagnetism, thermodynamics and the kinetic theory gave them good grounds for confidence.

I would draw parallels between the discovery of the Higgs and Einstein's Brownian motion papers. That is, difficult to detect particles, already used with great predictive success, whose existence was believed by most physicists and awaiting experimental confirmation. Or perhaps between blackbody radiation/cosmic expansion – experimental anomalies which fit awkwardly with our current theories (EM+thermodynamics/GR+QFT), whose resolution seems like a bit of a kludge Planck quantization/inflation.

Of course the analogy is not exact in every sense. The problems nowadays are certainly harder because we know more, so there are more constraints and we have picked most of the low hanging fruit already. But my point is that Kelvin had a pretty deep mathematical understanding of classical mechanics which turned out to be completely useless for making progress in clearing up the one or two 'loose ends' of 19th century physics. Moreover these loose ends (dark matter, dark energy, strong CP and the others I listed above) turned out the unravel the whole of physics.

17. **Peter Woit**
June 23, 2015

Radioactive/Andrew Thomas,
I agree with M that the situation then was the atomic scale was a huge unexplored territory, with physicists having a lot of tools at hand to learn more about it. One can have a different point of view about this, but in any case I think maybe physicists should just get over the idea that huge revolutionary progress like that of the period 1900-1925 is just around the corner. This is a stock claim at every conference like the Perimeter one, has been my entire career. One should be an optimist that progress is possible, but maybe it's a naive idea that all that is needed is a new bright young Einstein/Bohr/Heisenberg to take a fresh look at the data and get some insight from that that will change everything and throw our current theories in the trash bin.

18. **Radioactive**
June 23, 2015

Peter, huge revolutionary progress would be nice, or any progress at all. Though what we think of as progress is probably very different. I would prefer a 'Bohr atom' to an elaborate and beautiful theory linking QFT, geometric Langlands and the Monster group but which doesn't explain any of the currently known experimental anomalies. Fortunately these things often come hand in hand.

19. **Jeff M**
June 23, 2015

It's always fascinated me why anyone thinks the Fermi paradox is a paradox. Fermi especially. I mean he clearly knew that the speed of light is a barrier to civilizations finding each other? There could be 100 advanced civilizations within a 100 light year ball around us, and we would never know. It's a pity Mars wasn't just a little bigger, we might have had obvious life right next to us.

20. **Douglas J. Keenan**
June 23, 2015

@ Radioactive, 3:48 am

Regarding the quote attributed to Lord Kelvin, the attribution is disputed, and the earliest known claim for the quote is dated 1988, at least according to Wikipedia:
https://en.wikiquote.org/wiki/William_Thomson#Disputed

21. **Mol**
June 23, 2015

Radioactive, besides Kelvin's famous remark about the future of physics, many theoretical physicists were well aware that something was not quite right about classical physics. Lorentz and Poincaré, for example, were hard at work trying to reconcile the dynamics of the then recently discovered electron to the microscopic Maxwell-Lorentz electrodynamics, long before Einstein's 1905 paper on special relativity. (Give a read on Poincaré's "Sur la Dynamique du Electron" on the Internet that you will see how close he came to discover relativity.) Or the Balmer formula, known since 1885 but that would have to wait for 28 years to be derived by Bohr! J. J. Thomson tried to invent a lot of classical models in order to describe the Hydrogen atom using only classical physics and his beloved electron.

By the way, Einstein's hypothesis of the light quanta and his relativistic energy-momentum relations would be established experimentally only in 1923 with the Compton effect. Even Bohr expressed doubts about the reality of photons before that. So if something like photons or E=mc^2 took almost a decade in order to be proved experimentally, it seems reasonable that SUSY and related ideas should take even a longer period.

About the usefulness of pure mathematicians in guessing new laws, I am inclined to agree with Peter. Remember that pure mathematicians like Gauss, Riemann, Ricci and Levi-Civita created the mathematical structures that Einstein would use later to realize his ideas on gravitation and discover GR. Indeed, I read somewhere that both Einstein and Marcel Grossman studied Levi-Civita's "Absolute Calculus" carefully around 1910.

22. **Don**
June 24, 2015

"What we need are ideas as simple and radical as in the start of the 20th century with quantum mechanics."

I wonder if this is a fair statement. There were completely unexpected empirical discoveries that preceded the "simple and radical" ideas, no? Discreetness was forced on Planck by the data and by the complete lack of fit with classical theory. It's well known he didn't like this "radical" revision of physics, and even resisted when Einstein took his 's ideas further.

Then, when the next steps were taken, again based on new data coming from atomic spectroscopy and the very early particle physics, the people that came up with the "simple and radical ideas", Heisenberg et al suffered much resistance from the determinists like Einstein and Schrödinger, that continues today with all the debates about "interpretations" of quantum mechanics.

What really new radical discoveries have their been? The expansion of the Universe and the accelerated expansion come to mind. But most things after the 1920s generally fit the frameworks that emerged by that point.

Maybe if physics turned to other phenomenon it doesn't currently include in its scope, similar progress could be made. For example: how is living matter different from nonliving matter? What the heck is mind and consciousness? In spite of the tons of smoke and obscurity surrounding these topics, they are "low hanging fruit" for a radical revision of our understanding of the universe. These phenomenon are part of our experience of the physical universe, but right now they are completely outside the bounds of physics.

Thanks for allowing me to express my views.

Best wishes,

Don

23. **Aleksandar Mikovic**
June 24, 2015

It is clear that we need a more predictive theory than string theory. As far as the quantization of gravity is concerned, there are already simpler theories which achieve this, for example, spin-foam and spin-cube models, and these models do not need supersymmetry in order to construct a finite quantum gravity theory. As far as the explanation of the elementary particle spectrum, I believe that one must find a generalization of the concept of symmetry, which means to generalize groups and supergroups. Such generalizations already exist, for example, quantum groups and categorical generalization of groups, called 2-groups, and the goal is to see whether these structures suffice or we need something new. This will also require to make an appropriate generalization of the space-time structure (i.e. to generalize a smooth-manifold).

24. **gadfly**
June 24, 2015

I definitely agree with Don, although as a biophysics student I'm a bit biased.

The fact of the matter is that many regimes of nature stubbornly resist an organized theoretical description. The magnitude of the problem is highlighted by how little we need to move away from typical physics to find systems where textbook physics becomes surprisingly impotent; just look at the semi-classical mess that is a membrane protein to find something which, while technically a physics problem, demands numerous novel methods to study.

More importantly information theory is slowly becoming a more generally accepted framework for thinking about and tackling hard problems. In many regards the problems with protein dynamics are problems of dimensionality reduction; what is the most minimalistic way to describe the entire problem? Working on a variety of problems could spur developments in information theory which could have kickbacks for more traditional challenges (although I'm an outsider so I don't know how deeply related information theory is to fundamental physics, although the observer/reality relationship and cutting edge concepts like the black hole information paradox suggest that it could be).

25. **Radioactive**
June 25, 2015

I came across this talk of Feynman (argument from authority!) with his ideas about exactly these issues – mathematics in physics and how to make discoveries.

https://www.youtube.com/watch?v=obCjODeoLVw

26. **gadfly**
June 25, 2015

He makes some interesting points; in particular, I think that the most optimistic take on the relationship between mathematicians and physicists is that mathematicians are typically useful after the fact. I can think of only one major exception and it's an outlier: general relativity. Otherwise the process of pioneering physics is only encumbered by attentiveness to axioms and extreme rigor. Once you've figured out the rules organizing and packaging them as the mathematicians like to do becomes more imperative.

27. **Peter Woit**
June 25, 2015

gadfly/Radioactive,
Actually I thought it was kind of embarrassing to watch Feynman pontificate about something he clearly knows nothing about. The idea that what mathematicians do all day is engage in extremely rigorous discussion of axioms, in very general cases, with no interest in special cases, no idea of the "meaning" of what they are doing, is about the same as the idea that physicists all spend their time wearing white lab coats and standing in front of a bench with a piece of experimental apparatus on it.

28. **gadfly**
June 25, 2015

I wonder if you are reacting to some of the idiosyncrasies of Feynman's communication. For instance I don't think he was saying that something like a ring has no meaning, but was actually referring to levels of abstraction.

To him I would guess the equation x + 5 = 7 has no "meaning" until it is specified that x represents the number of dollars one must add to 5 to get 7, even though the equation still means something in the abstract sense.

It's probably not too productive to argue about what Feynman thought but I suspect it is unlikely he was unaware of the fact that mathematicians work on specific cases considering he worked with people like Kac on occasion as well as more mathematically oriented physicists. At any rate Peter is it not true that a substantial amount of modern mathematics research remains interested in theory building from axioms and high levels of rigor? If we take it for granted that he conflated these individuals with all mathematicians, we can still discuss whether or not this mathematical subculture is relevant to physics I think.

29. **Peter Woit**
June 25, 2015

gadfly,
Like physics, mathematics is a huge activity with a wide range of people doing a wide range of things, with many of those things having various relations to physics. I don't see the point of debating stupid caricatures of what mathematics is, what mathematicians do, and what the relations to physics are, even if they're coming out of the mouth of Feynman.

30. **Radioactive**
June 26, 2015

One of the reasons I posted the video was Feynman's assertion that physicists have to care about 'blocks of copper and glass'. One of my problems with SUSY, strings or multiverses is the failure of the community to abandon or drastically modify these theories in the face of failed predictions or failure to make predictions. Peter, your desire for more mathematicians in physics seems to run opposite to this. There are a great deal of different types of mathematicians but they are as a group, it's fair to say, not concerned with blocks of copper and glass and not likely to care very much if something beautiful, like string theory, has no connection to reality. And this has been your lament about physics for the last 10 years.

31. **Peter Woit**
June 26, 2015

Radioactive,
Unlike a lot of people, my criticism of string theory has never been that too much attention is paid to mathematical beauty and not enough to experimental results, and I think the physics community is making a big mistake if they decide that is the lesson of the string theory fiasco. The current explanation for how string theory works, the landscape, is something of unparalleled ugliness, which only a physicist, not a mathematician, would find appealing.

The underlying problem is the fact that there are very few hints from experiment about how to do better than the Standard Model. It just is not true that there are lots of such hints and they are not being paid attention to. It has been clear now for a long time, long before the LHC results, that string theory unification predicts nothing. The problem is a failure to acknowledge that the internal problems of the theory are deadly, instead creating a bogus pseudo-scientific connection to experiment (the landscape).

What mathematicians are expert at is understanding exactly what the properties of a mathematical framework are, what works and what doesn't. They have a strong culture of abandoning ideas that don't work. Working in a situation where you don't have experimental clues, but have to rely on the coherence of the ideas and models you're studying is exactly what they are experts at, and I think this could be helpful. I don't see the argument that they're going to do worse than the "physical" path that has brought us to the multiverse.

32. **Anonyrat**
June 26, 2015

Contra @Mol, I thought that Einstein's hypothesis of the light quanta was established by Robert Millikan's 1914 experiment on the photoelectric effect.

33. **David Derbes**
June 27, 2015

@Douglas J. Keenan,

I have before me Robert L. Weber's delightful anthology *A Random Walk in Science*, copyright 1973. On page 191, is a selection entitled "Clouds, 1900" from Lord Kelvin: "The beauty and clearness of the dynamical theory, which asserts heat and light to be modes of motion, is at present obscured by two clouds.
I. The first involves the question, How could the earth move through an elastic solid, such as essentially is the luminiferous ether?
II. The second is the Maxwell-Boltzmann doctrine regarding the equipartition of energy."
The citation is "Slightly condensed from *Philosophical Magazine* (6) **2** 1 (1901)." The editor adds: "Kelvin could certainly recognize the important clouds. One needed relativity, the other quantum theory, to blow it away."

Anomalies Revisited

June 22, 2015
Categories: Uncategorized

Some of the talks at Strings 2015 have now appeared online, and one of them I found quite fascinating, Witten's Anomalies Revisited. Some of his motivation comes from string perturbation theory and M-theory, but the questions he addresses are fundamental, deep questions about quantum field theory (and not just any quantum field theory, but exactly the sort of qft that appears in the SM, spinors chirally-coupled to gauge-fields/metrics).

The fundamental issue is that these are theories where the path integral does not determine the phase of the partition function. Part of story here is the well-known story of anomalies, perturbative and global. One interesting point Witten makes is that vanishing of these anomalies is not sufficient to be able to consistently choose the phase of the partition function, and he gives a conjecture for a necessary condition that is stronger than anomaly cancellation.

The standard story of QFT textbooks is that once a Lagrangian is chosen, the corresponding QFT is well-defined. But quantization really is a lot more subtle than that, and the anomaly phenomenon is just one indication of the problem. In earlier parts of my career I spent a lot of time thinking about anomalies; the connections to some of the deepest mathematics around (K-theory, index theory and much more) are truly remarkable. In recent years I've been thinking about other things, but Witten's talk is a strong encouragement to go back and revisit the anomaly story (some of his best work has "revisited" in the title…)

In particular, Witten emphasizes a particular case I'd never paid much attention to: the 3d massless Majorana fermion. One would think that this is among the simplest quantum field theories around, but Witten explains how this is an example of a theory with a potential inconsistency (no way to consistently choose the sign of the partition function), even when the anomaly vanishes. This theory also appears in a hot topic in condensed matter physics, the theory of topological superconductors. One reference Witten gives to related work is to this recent paper (there's a typo in his reference, should be 1406.7329 not 1407.7329).

Witten ends with:

> I hope I have at least succeeded today in giving an overview of the tools that are needed to study the subtle fermion integrals that frequently arise in string/M-theory. A detailed analysis of a specific problem would really require a different lecture. Write-ups of some of the problems I've mentioned – and some similar ones – will appear soon.

I look forward to those write-ups, with the theories he's talking about of a lot more interest than just their role in string/M-theory calculations.

Comments

1. **Dave Miller in Sacramento**
   June 23, 2015

   Peter,

   From time to time, I have seen claims that the anomaly problem amounts to nothing more than the breaking of conformal invariance by renormalization. Can you tell me if this is true, false, part of the story, or not even wrong?

   And, what is the simplest (but correct) exposition of the issues that you can recommend (i.e., for someone who took QFT a *long* time ago)?

   Thanks.

   Dave

2. **Peter Woit**
   June 23, 2015

   Dave,
   That's just a small part of the story. More generally, the anomaly refers to a subtlety in quantization: a symmetry of the classical theory does not work in the expected way in the quantum theory. You already see this in the phenomenon of the one-half energy of the ground state in the harmonic oscillator. You can get rid of this by redefining the Hamiltonian, but that changes how the symmetries of the classical system are implemented in the quantum system. For a finite number of degrees of freedom, you can work with either Hamiltonian, but in QFT, with an infinite number of degrees of freedom, you don't have a finite shift and this causes the anomaly.

   Put differently, the anomaly is due to the fact that normal-ordering is needed in QFT, and this sometimes changes how classical symmetries appear after quantization. Maybe the simplest example is the U(1) global chiral symmetry in the theory of 1+1 d fermions. I wrote something about the general phenomenon in the book I'm writing which is online, would like to add a section working out the 1+1 d fermion story, but that may end up on the list of things I have to drop for lack of time/space.

3. **Chris Austin**
   June 23, 2015

   Witten argues that a partition function or functional integral can sometimes have an undefined phase factor exp(i \alpha), where \alpha is a constant. But in quantum field theory, we always in practice calculate a ratio such as Z[J]/Z[0], where J are some sources or background fields. This is necessary in order to cancel the vacuum bubble diagrams, (or equivalently, if the interactions are turned on and off adiabatically, as in the Gell-Mann – Low derivation of the Feynman diagram expansion, it is necessary in order to get a stable limit as the adiabatic switching is removed).

   Witten's constant phase factor will always cancel between the numerator and denominator of Z[J]/Z[0] if the manifolds and gauge fields in the numerator and denominator are topologically equivalent, so does it really matter? From pages 83 and 84, he is concerned about consistency of his phase factor between different manifolds. Does this mean it is now possible to have topologically distinct manifolds or gauge fields in the numerator and denominator?

   This seems to raise the issue of when one should or should not sum over some topologies in Z[0], which Witten doesn't address. We have to sum over Yang-Mills instantons, so do we also have to sum over all compactification topologies in the M-theory Z[0]?

   Witten must presumably know that the condition he proposes on page 89 is satisfied for D = 11 Majorana gravitinos, gravitons, and 3-forms on all 12-manifolds Y, (so that the condition doesn't say that M-theory is inconsistent), but I can't find anywhere in the talk where he says so. (Nor in Freed and Moore, which Witten cites on page 124.)

   Since spinors are involved, should it be all *spin* D + 1-manifolds, at least in the sense that w_2 vanishes? Page 101 seems to suggest yes. On page 144, Witten invokes RP^4, which is Pin^+ in the sense that w_2 vanishes although w_1 doesn't, (according to pages 45 to 46 of Milnor and Stasheff). What about D + 1-manifolds that are

Pin^-, in the sense that w_2 + w_1^2 vanishes, although w_2 doesn't, for example RP^2, RP^6, RP^10? What about D + 1-manifolds for which w_1 vanishes but w_2 doesn't, for example RP^5, RP^9?

4. **Tim Nguyen**
   June 24, 2015

   Peter,

   "You can get rid of this by redefining the Hamiltonian, but that changes how the symmetries of the classical system are implemented in the quantum system."

   I don't immediately understand what this means. Could you please elaborate? My impression is that you meant something involving Poisson brackets (classical) or commutators (quantum), both of which annihilate constants.

5. **Peter Woit**
   June 24, 2015

   Tim,
   It"s an intricate story, but here's quite a lot of detail about this in
   http://www.math.columbia.edu/~woit/QM/qmbook.pdf

   One basic point is that, even classically, if you have a symplectic action of a Lie group G on a phases space, there's an ambiguity of a constant in how you define the moment map, and that's an origin of the problem. In some sense what you have is a central extension of G that is acting, and it may be non-trivial.

6. **Anon**
   June 24, 2015

   Chris,

   The phase doesn't cancel for all physically relevant operators in the QFT. The typical example is the energy-momentum tensor in classically conformally invariant theories. In this case the overall phase factor comes from the definition of the path integral measure, which no matter how you cut it introduces an unavoidable extra dependence on the metric (besides the explicit occurrence of the metric in the Lagrangian), which may then cause the trace of the EM-tensor (which is the generator of conformal transformations) to be nonzero in the quantum theory.

7. **Alex**
   June 24, 2015

   Anon,
   I'm confused by your comment. The example you gave seems to indicate that the phase factor depends on the metric, hence is not a constant. In the example in Witten's lecture the partition function can be determined up to an undetermined phase factor exp(i\alpha) where \alpha is a constant. Please explain.

8. **Anon**
   June 25, 2015

   Alex,

   In the case I'm most familiar with (two-dimensional conformal theories) the coefficient alpha is a real number that can be expressed as an integral over the manifold of a density depending on the metric (something like alpha = ∫sqrt(g) R, where R is the curvature and the integral is over the whole manifold). This is a constant for fixed background, but can vary if you vary the background metric (which is what you do to determine the EM-tensor, and contributes something nontrivial even to the EM-tensor on a fixed flat background).

9. **Sakura-chan**
   June 28, 2015

   Off-topic to this thread but not to the blog…

   http://www.nist.gov/itl/csd/random-number-generation.cfm

   Looks like NIST has formally removed the Dual_EC_DRBG from its recommended methods for generating pseudorandom numbers.

Summer Break

June 29, 2015
Categories: Uncategorized

Heading out soon for summer vacation travels, this time to Ireland and France, back in about two weeks. While away comments will be shut off.

My vacation will spare readers commentary on the rest of the Strings 2015 talks, which at some point will appear here. It seems that the conference included the usual publicity campaign for string theory, with Ashoke Sen continuing to demonstrate a sense of humor by telling the press that

> In string theory, we are essentially following in the same steps

as the 50 year story of the Higgs mechanism, the Standard Model and the discovery of the Higgs particle. It does put modern HEP research in perspective once one realizes that its greatest success (the Standard Model) and greatest failure (string theory unification) are essentially the same…

Comments

1. **Mesmar Djehha**
   June 29, 2015

   This is a nice but incomplete future prediction for the field.
   50 years will then also be the lapse of time before string theory predictions could be tested, but what would be the lapse of time before the appearance of those predictions?

   Happy vacations Peter.

2. **Peter Woit**
   June 29, 2015

   Mesmar Djeha,
   I think part of Sen's joke is that since the Higgs mechanism was 1964 and the vindicating discovery was 48 years later (2012), taking the birth of string theory as the Veneziano model in 1968, we're on track for experimental vindication of string theory next year…

My Summer Vacation

July 15, 2015
Categories: Uncategorized

When I was young, I recall that a standard assignment when restarting school was an essay on "what I did on my summer vacation". Now that I'm back in the office after a vacation, here's a version of that, covering the physics/math aspects:

- In Paris I visited the Palais de la Découverte, the science museum in the heart of the city. This was the site of some of my earliest memories of getting interested in science, back in the late 1960s. I started out by visiting the physics section, some of which looked like it hadn't changed much since those days. One addition was a video screen with some seating nearby. It was running Particle Fever, and surely someone trying to annoy me had timed things so that I got there just as the part promoting SUSY finished, and the part featuring Nima Arkani-Hamed promoting the multiverse got underway.

  Besides revisiting my youth, this made me wonder what the effect of trying to impress the young with glitzy pseudo-science will turn out to be. Will it turn off youngsters looking for something intellectually serious? What would my 12 year old self have made of this?

  I soon made my way though to a big LHC exhibit, which cheered me up immediately. This was quite well-done, giving a good idea of what a machine like the LHC really is and what the scientists working there are doing. I quite enjoyed the last part of the exhibit, a recreation of typical grimy offices at CERN that people work in. I'd like to think that I'd have appreciated that, with its unspoken message that these are people who care not about appearances but just about the fascinating work they are doing.

- Now that scientific bookstores are gone in New York, checking out the ones in Paris is a high-priority when visiting there. This time I bought a few books in French, one of which was a new biography of Grothendieck, by Georges Bringuier. I've read widely among the many sources emphasizing Grothendieck's mathematics, this is one that instead focuses on his life, including quite a bit about the years after he left the IHES in 1969. This is an amazing story, and there's much in the book that I didn't know, especially about Grothendieck's mystical views.

  The last years of his life he was a hermit, just about completely isolated and perhaps paranoid, but still supposedly writing (the biography explains his view that one shouldn't read mathematics, but write it). Perhaps someday we'll find out what he was writing about, for some information about this, see here. While in the earlier part of his career Grothendieck wanted nothing to do with physics, associating it with the Bomb, evidently at the end he had physics books in his home rather than mathematics ones.

  One person Grothendieck was in communication with in the early 1990s was Robert Thomason, see a letter available here. Thomason died in Paris in 1995, his notebooks have just become available at this site.

- Another book I found in Paris was from a few years back, about the "unification of mathematics" by Parrochia, Micali and Anglès. The topics are Clifford algebras, abstract algebraic geometry (a la Grothendieck), and the Langlands program, wrapped up in an argument that these provide a unified framework for mathematics. The point of view is the very French one emphasizing the "philosophy" of a subject, and much of the argument is that philosophers of science should be paying close attention to the "Langlands philosophy" and its significance as a unifying set of ideas. They also argue that physics should fit naturally into this sort of unification, an idea I'm very fond of, but they don't seem to be very aware of the actual connections between physics and the Langlands story (they refer only to the classification of branes by twisted K-theory).

Comments

1. **RandomPaddy**
   July 15, 2015

   Oi! What did you do in Ireland?

2. **Peter Woit**
   July 15, 2015

   RandomPaddy,
   Nothing math/physics related. Seven days driving around the country, first to a place in East Cork, then two nights in Kerry, three up in Connemara, finally back to Dublin for two nights. Weather was very pleasant, greatly enjoyed getting to know a bit about the country (before this I had just spent a couple days in Dublin).

3. **nc**
   July 15, 2015

   A miracle. Pleasant weather in Eire!

4. **Sudip Paul**
   July 15, 2015

   http://arxiv.org/abs/1507.03414
   Thoughts?

5. **Reader**
   July 16, 2015

   I was reading AG's autobio online soon after he died. The key idea I thought was most interesting was his following paraphrased comment.

   To really understand a subject you have to mentally live outside its room.

   That is, every subject has bounds of thinking in language and concepts. To live outside it, you have to step into the unknown and invent your own language and concepts.

6. **Peter Woit**
   July 16, 2015

   Sudip Paul,
   I don't know much about pentaquarks. It seems quite plausible that QCD allows such states, but as far as I know there isn't a reliable calculational method available.

7. **sdf**
July 16, 2015

“This was the site of some of my earliest memories of getting interested in science, back in the late 1960s”

Somewhat similarly, my own earliest memories of same, were also from Paris but instead from the Cité des Sciences et de l’Industrie in the 19th arr. from the late 1990s. I think I can even remember most of the exhibits…

8. **Richard**
July 16, 2015

Reader,

Understanding presupposes alienation — and profound understanding presupposes profound alienation.

9. **Michael Hutchings**
July 16, 2015

Where do you find scientific books in Paris? (French OK)

10. **Peter Woit**
July 16, 2015

Michael,
Best places I’ve found are Eyrolles on Bld. St. Germain (downstairs) and Gibert Joseph on Bld. St. Michel (top floor). Maybe there are others I’m not aware of…

11. **Thomas**
July 16, 2015

@Michael: There is a small but interesting scientific library which often has hard-to-find second-hand books, rue Dante, between the Collège de France and Notre-Dame. I’ve seen books both in English and French there (although apparently you read both).

@Peter: Wonderful that you are mentioning the Palais de la Découverte, for anyone who hasn’t been yet it is indeed worth a visit. Both for the building and what is inside. Very family friendly; I don’t know what the 12 yo Peter would have thought about Physics, but I can report that my 4 yo loved every moment of it (especially the biology section).

It is not well known that the Palais was on the brink of disappearing a few years ago, threatened as it was by the much bigger and more modern Cité des Sciences. A petition circulated in the french scientific community to save this institution, and was successful. As a compromise now the Palais is semi-private, not entirely state-owned anymore, probably not a bad deal.

12. **Michael Hutchings**
July 16, 2015

OK, thanks for the tips. I’m one of those weird tourists who likes bookstores and not just museums. I previously managed to find Gibert Joseph (impressive place!) but didn’t know about the others.

13. **paddy**
July 16, 2015

Quite a bit off topic: To nc, The weather in Ireland is always fair and “soft”. Depends on ones perspective.

14. **James MC**
July 17, 2015

Hope you had a nice holiday Peter especially the Cork part that’s where I live 🙂

15. **Low Math, Meekly Interacting**
July 17, 2015

OT, but Yoichiro Nambu passed away. It struck me that perhaps the last couple generations of particle physicists to see their fundamental predictions verified is rapidly dying off.

16. **Tommaso Dorigo**
July 20, 2015

We followed a similar trajectory – the museum in Paris is the place where my father first described a synchrotron to me. But it was well into the seventies for me.

Cheers,
T.

GenCyber

July 18, 2015
Categories: Uncategorized

Over the years the NSF has financed various summer camps for high school students, designed to get them interested in mathematics or other areas of science. This summer they've teamed up with the NSA to deal with the problem of bad press due to the Snowden revelations by organizing a massive new program of quite different summer camps. The program is called GenCyber, and the New York Times today has an article about it here. This year the NSA/NSF is funding 43 camps (for a list, see here), with 1400 youngsters attending them, the plan is to expand to 200 camps over the next few years.

The NSA official in charge, Steven LaFountain explains how the PR aspect works:

> Mr. LaFountain said the agency would not make sales pitches to campers, but hoped that the work of the agency would be enough to lure them into the field.
>
> "We're not trying to make these camps something to make people pro-N.S.A. or to try to make ourselves look good," he said. "I think we'll look good naturally just because we're doing something that I think will benefit a lot of students and eventually the country as a whole."

According to the New York Times, one sort of thing being taught is how to crack password files:

> "We basically tried a dictionary attack," Ben Winiger, 16, of Johnson City, Tenn., said as he typed a new command into John The Ripper, a software tool that helps test and break passwords. "Now we're trying a brute-force attack."
>
> Others in the room stumbled through the exercise more slowly, getting help from faculty instructors who had prepped them with a lecture on the ethics of hacking. In other words, they were effectively told, do not try this at home.
>
> "Now, I don't want anybody getting in trouble now that you know how to use this puppy," Darrell Andrews, one of the camp's instructors, warned loudly. "Right? Right?" he added with emphasis.

Teaching thousands of kids how to crack password files? What could go wrong with that?

The program at Marymount features indoctrination visits to the NSA together with the hacking instruction, and one of the instructors seems to realize part of the problem:

> And here at Marymount University, where campers are staying in dorms for their two-week program, visits to the N.S.A. and a security operations center break up classroom time.
>
> The idea — and the challenge — of the camp, according to its head, Diana Murphy, a professor of information technology at Marymount, is to first teach students how to hack, so they can understand and defend against attackers they might encounter in cyberspace.
>
> "It's a fine balance for me as a teacher, because you have to teach them some of the hacking techniques, and layer that in with an ethical discussion," Ms. Murphy said in an interview before camp began.
>
> "They are most interested in the attacking things."

Update: CNN has an article up today about this here.

Comments

1. **telemurk**
   July 18, 2015

   Thank you for posting this. It's the first I've seen about it.

   What an insane waste of resources with a high potential to create all sorts of disastrous blowback down the road.
   I can't imagine what possible legitimate role the NSF can play here. Are they now so corrupted by NSA money and pressure that all common sense and all moral standards have been erased by excuses and equivocation?

   A slight suggestion would be to instead spend government money on math camps, run by the "old" NSF. That might lead to just as many future NSA recruits eventually, but also to engineers, climate scientists, and, yes, physicists and mathematicians-but with no potential downside and no compromising of the ideals
   of academe and of science. And, not incidentally, with the result of providing a much better background for the students.

   As for the NSA itself, instead of wasting money on this, they might still pull Thomas Drake's Thin Thread approach out of the dustbin and thereby have a chance at catching real threats like the Tennessee
   shooter, before it happens. To avoid offending the corporate lobbyists, that could even be run in parallel to the currently in vogue and 1000 times more expensive "create an infinite haystack" approach.

2. **Peter Woit**
   July 18, 2015

   telemurk,
   It seems to me that the NSA tactic of making grants through the NSF here is much the same as the way they use the AMS to make grants. Involving these two organizations allows them to take advantage of their high credibility and reputation. I know of no evidence that both organizations aren't willing sellers.

3. **Chris W.**
   July 18, 2015

   Concerning the reference in telemurk's comment, Wikipedia has an informative article on the NSA's ThinThread project (active during the 90s), for those who aren't familiar with it.

4. **Michael Weiss**
   July 18, 2015

Readers interested in the original generation of post-Sputnik NSF-supported summer science camps should check out "The Summer Science Program," now in its 57th year and supported by its alumni. Designed for rising high-school seniors with a passion for physics and mathematics, SSP has focused on celestial mechanics (from foundations to observational astronomy) and was originally supported by Caltech faculty, including Richard Feynman and Maarten Schmidt in the 1960s and 1970s. SSP currently has two campuses (Boulder, CO and Socorro, NM), with a third campus planned in the life sciences. See link: http://www.summerscience.org and Wiki page. SSP is international in its outreach and remarkable for its mentorship of young women. The extraordinary alumni of SSP exemplify the wisdom of inspiring the next generation.

5. **wereatheist**
July 18, 2015

> Teaching thousands of kids how to crack password files? What could go wrong with that?

They teachthem cracking *password files*. But the interesting thing is to get these password files 🙂

6. **Peter Woit**
July 18, 2015

wereatheist,
Presumably breaking into systems to get the password file was in one of the earlier lesson plans…

7. **Jeff M**
July 18, 2015

It's really upsetting that the NSF and the AMS get involved with the NSA, but it certainly goes way back, at least with the AMS. The NSA always has a big booth at the yearly AMS meetings, talking about how they hire more mathematicians than anyone else. Used to give me a good laugh in graduate school, since there is no way in hell I could ever have gotten security clearance, not that I ever wanted to work for the NSA 🙂

8. **Richard Séguin**
July 19, 2015

Is this displacing other types of summer math and science programs? I hope not. In the 1960s I attended two 6-7 week NSF sponsored math programs for high school students at universities, although at that time we did not call them "camps." They were fabulous experiences, and they were not polluted by politics, dubious ethics, and indoctrination, which impressionable kids should not be exposed to.

I'm sure that the NSA could have directly sponsored these things by themselves, but using the NSF as a front softens and confuses apparent intentions.

9. **Peter Shor**
July 19, 2015

The NSA is the by far largest employer of mathematicians in the country. Clearly, the AMS needs to have *some* relationship with them. (Just like the American Physical Society needs to have some relationship with the Department of Energy, one of whose objectives is the building of bigger and better atomic bombs.) But maybe they need to think about this relationship more carefully than they have in the past.

10. **Peter Woit**
July 19, 2015

Peter Shor,
The problem is that, unlike the DOE weapons labs, the NSA deceives the public about what it is doing, to the extent for instance of having the National Intelligence Director lie to Congress under oath about this. Without Snowden, none of this would be known. What most bothers me about the AMS is the way they allowed themselves to be used by the NSA to mislead the math community in this way (about the back-doored elliptic curves). These summer camps seem to me to have a goal again of misleading the public, in this case kids and their parents, by whitewashing the story of what the NSA actually does.

Besides this, there's the other issue of whether training kids in running tools to break into computer systems is a good idea. The DOE analog would be summer camps for kids on how to design nuclear weapons….

11. **KenW**
July 19, 2015

We ought to be careful about getting all morally indignant. The nation needs its next generation of code-breakers and it starts with puzzle-solving, which is much enjoyed by kids with the right temperament They should be encouraged and, eventually, recruited as one would recruit talent for any government job.

Recruiting them at this young age makes me nervous, however. Mathematical talent surfaces at an early age, and they can handle that. But the ethical issues require a more mature mind. The schools should certainly do a better job of teaching math, including tricky stuff like puzzle-solving

But I'm uncomfortable about this NSA program.

12. **Richard Séguin**
July 19, 2015

"Teaching thousands of kids how to crack password files? What could go wrong with that?"

Of course this could backfire! Once these young kids learn from the government that hacking is a cool problem from all angles, how long will it be before many of these curious, internet savvy, kids discover and connect with the underground hacker bandits and all their cool tools.

13. **Peter Woit**
July 19, 2015

KenW,
The NSF and the NSA have for a long time been sponsoring activities for schoolchildren and undergrads promoting problem solving, and hoping to get students interested in this, partly to provide a workforce for the NSA for the future. This program is however something new and different. Looking at the various websites for the different camps, there's a heavy emphasis on:
1. scaring children about the supposed cyberthreats to the US homeland.
2. promising them a well-paid career working for the NSA or someone else defending against such threats.

14. **telemurk**
July 19, 2015

This is just getting worse and worse:
” This program is however something new and different. Looking at the various websites for the different camps, there’s a heavy emphasis on:
1. scaring children about the supposed cyberthreats to the US homeland.
2. promising them a well-paid career working for the NSA or someone else defending against such threats.”

As the father of a young child, I think (1.) is not only ill-considered but is borderline criminal. Just because China and Russia and North Korea might employ such tactics doesn’t mean we should necessarily rush to emulate them. A background in trust and idealism like that I picked up in part from Boy Scouts, inspiring and admirable math and history and English teachers in public school in a University town (ok I was lucky), would arguably serve not only the children, the world but also the nation much better than this. They will have their whole later lives to learn cynicism and fear of the other.

The reason the whistleblowers’ revelations (Snowden, Binney, Drake and so on) are so disturbing is not only because of the
facts revealed, but because of the subtext: the people involved, at least at the higher levels, are willing to lie to Congress under oath, to intentionally manipulate public opinion to get what they want, all the while making dubious but self-serving policy decisions like this. That is, the programs not only have the potential for being abused but that is already happening, on multiple fronts. If the foxes are in charge of the hen house, what should we expect- and revelations like these can only increase our suspicion that the very wrong people are in charge. A wrong-headed program will select for bad leadership as those who are more reflective and perceptive are shunted aside or worse. “Trust us”, they say? And why, exactly, if you keep on proving the opposite?

15. **Ben R**
July 20, 2015

I don’t think you should be upset about kids learning to use password crackers in summer camp. The idea that this would make them more likely to hack websites is like the idea that playing violent video games predisposes kids to real-world violence. I think there’s no evidence that that’s true, and some evidence that it actually has the opposite effect, maybe by removing some of the forbidden-fruit aspect of it, or by providing a safe outlet for their curiosity. Anyone who protects any data with a password, which is anyone at all these days, needs a basic understanding of how dictionary attacks work so that they pick passwords that aren’t vulnerable to dictionary attacks. We also need kids who are interested in computer security and study it in college so that they can build more secure systems and keep everyone safe. You could just as well discourage kids from learning about any other subject that they might turn to evil. Like chemistry. Sure, let’s teach teenagers how to synthesize chemical compounds—what could possibly go wrong? And don’t get me started about physics.

16. **Axl**
July 20, 2015

I totally agree with Ben R. Plus, I think these teenagers know about the events of the past couple of years with regards to Snowden and that the NSA might be trying to pull a PR stunt and “groom” the next generation (though I don’t think it’s true). These kids aren’t stupid. They’re there because they’re interested and they know full well what the NSA has done and what they are capable of.

17. **Peter Woit**
July 20, 2015

Axl,
I’m glad to hear that US 13 year old children are a lot more sophisticated about the NSA than many of the research mathematicians I’ve talked to, not to mention the general adult population. Impressive…

18. **Fred P**
July 20, 2015

They are learning password cracking. Note that password cracking software tools and instructions on how to use it are both freely available on the internet, and I’d expect any high-schooler interested in the subject to be able to access (and use) it easily on their own.

If your passwords aren’t secure against dictionary and/or brute-force attacks, they aren’t secure (which is, I’d assume, the point of the exercise). When possible, my passwords are 128 random characters; as I use PasswordSafe, I never see them. This doesn’t mean that my passwords are secure; just that they are as resistant as I can make them against several classes of attacks. Password length/complexity is, of course, not a defense against rainbow tables.

19. **Axl**
July 20, 2015

Peter,

You never watched or read the news when you were 13? When you were 13, did you think that everybody was good and nobody had ulterior motives and everyone was being genuine? Impressive…

20. **zzz**
July 20, 2015

“they know full well what the NSA has done”

Really ?? because I don’t and I am 40 something years old.

21. **Axl**
July 20, 2015

zzz,

I meant that they know they deceived the public and that they have spied on American citizens and collect their data. Of course, nobody (except for the NSA) knows full well what the NSA has done. I’m sure these tech savvy teenagers know about Snowden, etc.

Anyway, the camp actually looks interesting and fun, regardless of what the NSA hopes to gain from this. These kids have heard of the Snowden revelations, and that we can’t really trust the NSA. And the NSA isn’t putting a gun to their heads to work for them after they graduate. So I’m not sure what people are so upset about. I would participate in this camp if I was a teenager and had the time.

22. **Low Math, Meekly Interacting**
July 20, 2015

I'm not about to sing the praises of the NSA. But I look at what happened to the OPM recently, and it becomes harder to argue against those with the attittude that, yeah, they're mendacious, amoral spooks, but they're OUR mendacious, amoral spooks. To posess superior cybersecurity is to have the ability to circumvent someone else's inferior security, generally. I'm not sure whether I'd be more disturbed that the NSA deliberately strong-armed NIST into breaking Dual_EC_DRGB if I found out Chinese hackers were so much more clever that they wouldn't have to to gain an edge. We should aspire to promote higher ethical standards in our government, obviously, but I wouldn't want enlightened democracies to find themselves highly vulnerable to cyberattack because we couldn't tolerate our youth learning how to hack. At least it's somewhat out in the open, and if we are to be secure, then we need young people to learn these skills and be passionate about them.

I'm quite sympathetic to your stance, Peter, and it's admirably high-minded and scrupulous. But it's not a high-minded, scrupulous world we inhabit. If this approach is inappropriate (and I'm not saying it isn't), how better to stimulate interest among young people in cybersecurity expertise, which is an incredibly valuable asset we simply must promote?

23. **Peter Woit**
July 20, 2015

LMMI,
Computer and network security is not a new problem, and there's a huge ($100 billion/year order of magnitude) industry already in place devoted to dealing with it. Any problems with recruiting good people to enter the cybersecurity industry can be dealt with in the usual capitalist manner (pay them well and provide good working conditions and training).

I just don't see the argument that the solution to failures of the security setup at a government agency is to have the NSA setting up summer camps for children. It does though look a lot like a solution for the NSA to the PR problem created for them by Snowden…

24. **G.S.**
July 20, 2015

Peter,

By that logic, we shouldn't ever try to engage a teenager's interest or teach him/her a skill. If it's important enough, just pay them well as an adult and the good ones will be motivated purely by money to learn what's necessary on the job. I'd like to see academic departments try that strategy to lure grad students.

25. **Peter Woit**
July 20, 2015

G.S.,
I just don't see any evidence that an insufficient number of people are interested in hacking or computer security. No matter what the skill, companies always complain that it's hard to hire good people, and by that they always mean that it's hard to get good people to work for what they would prefer to pay.

The only problem with hiring in this field that I've heard of is that, post-Snowden, the NSA itself has found it harder to get good people to work for them (for obvious reasons). I don't think these summer camps are motivated by solving any actual problem except the NSA's PR one, and their attempt to do this by going after children is pretty problematic.

26. **G.S.**
July 20, 2015

It's not your main point, but I've been working at software companies for several years now. Some of my responsibilities have included recruiting at job fairs, reading resumes, and conducting interviews for a number of positions, ranging from software/algorithm engineer to IT and computer security. What I mean when I complain that it's hard to find good people is that it really is hard to find good people. There are a lot of applicants who think they can write professional code or work IT but who are woefully inadequate for the jobs. There are some who are qualified technically, but are completely inept at dealing with people in a professional environment — I think everyone has experienced at least one IT guy like that. The true talents are needles in a haystack.

27. **Igor Khavkine**
July 20, 2015

They are literally creating script kiddies.

28. **Low Math, Meekly Interacting**
July 20, 2015

Well, the private sector is good at competition, but it wants to sell to everyone, friend or foe of liberty and democracy. If loyalty to something other than shareholder interests is desired, I'd hope for a trustworthy national agency. Not that the NSA has demonstrated much ability to provide that, of course, but the kids in that article were getting some ethics classes along with learning how to crack bad passwords. The irony isn't lost on me, but maybe it's a start.

29. **Anonymous**
July 21, 2015

It's clear that the NSA realizes it has an image problem and is actively working to change that impression. They have been actively recruiting undergraduates at the University of Utah for their summer program going so far as to invite a recent Utah alumnus / NSA hire to do the recruiting. Although NSA representatives claim analysts must work out of Ft. Meade many expect the situation to change fairly soon due to a recent addition here in the state:

http://www.wired.com/2012/03/ff_nsadatacenter/

There is also rampant speculation that part of the motivation for the Utah location is to not only draw from computer science programs, but also to tap into an extraordinary source for future linguists needed in critical languages (e.g. Mandarin, Russian, etc.)

http://www.npr.org/2014/06/07/319805068/lessons-from-the-language-boot-camp-for-mormon-missionaries

"The approach has also gained traction in the U.S. military. In fact, the ties between the U.S. military and the MTC run pretty deep. The Army's Intelligence Brigade, made up of linguists, is based in Utah and draws on former missionaries to fill its ranks."

30. **Michael Hutchings**
July 21, 2015

I keep hearing that the NSA is the largest US employer of mathematicians, but does anyone have any actual numbers or estimates? (I was rather surprised by the large scale of this summer camp program.)

31. **David J. Littleboy**
July 21, 2015

In addition to the "what could possibly go wrong" problem, the intellectual vacuity of the program is horrific. Couldn't they think of something of intellectual value to teach? Sheesh. There is a bit of math (theory of computation, cryptography) and engineering (programming languages, operating systems) in the computer area that has some intellectual content. But this is so stupid it sounds like a parody…

32. **Fred P**
July 21, 2015

@Peter Woit –
Here's some evidence that there aren't enough people in one sub-area of computer security:

CISOs are "almost impossible to find these days," she said. "It's a bit like musical chairs; there's a finite number of CISOs and they tend to go from job to job in similar industries."

"It's extremely hard to find good people right now," says Tim Eades, CEO of vArmour, a security firm.

Myself, I think the camps are probably both advertisement for the field and advertisement for the NSA. Both help the NSA in the long term.

@Michael Hutchings-

I found a dated (2001) article claiming "The NSA hires about 50 mathematicians a year, about two-thirds of them PhDs. "This is a very significant percentage of US citizen maths PhDs, and we are incredibly worried about the shrinking pool," says Schatz."

I found a less-dated article (2013) claiming: "The agency declares that it is the United States' largest maths employer, and Samuel Rankin, director of the Washington DC office of the American Mathematical Society, estimates that the agency hires 30–40 mathematicians every year."

33. **Peter Woit**
July 21, 2015

Fred P.,
That supposed "shortage" is of top cybersecurity executives, sounds to me like just the usual "shortage" of "stars" in every field (although as G.S. points out, really good people are a minority, but I think this is true in most fields).

Pretty much everything about the NSA is highly classified including their budget, number of employees, and presumably also the number of mathematicians working there (although maybe they told the kids being given tours of the place as part of their summer camp experience). I'd be a little bit suspicious about that "30-40" 2013 number, since I would have guessed that the NSA expanded quite a bit post-9/11/2001, so would have thought the recent numbers would be higher than the 2001 number. Maybe one issue is how you count who is a "mathematician", since a lot of the work they do is applied work on the borders with other fields.

34. **RandomPaddy**
July 21, 2015

The profession of "cyptologist" needs to be terminologically separated from "mathematician" in much the same way as "theoretical physicist" and "computer scientist" were seperated in the post war/Manhatten Project era. It's fine for cryptologists to wander in and out of math conferences and vice versa, but this effective domination of the profession — certainly in terms of youth recruitment programmes at least — is not healthy or desireable in the long term. It's leading to politicisation and increasing ructions. My own fear is that it will lead to nationalist/ideological splits in mathematics, a feat even the cold war did not achieve.

These NSF camps should be "cryptography" camps, designed not to generate interest in "mathematics and science" generally, but codebreaking specifically. De-Facto this is the case, but their de-jure designation as "science" camps is leading to somewhat justified worries about an effective nationalisation of the mathematical profession by its now largest employer. This could be avoided completely, and turned in a win-win for everyone, if the distinction was clearly made between "cryprography" and "mathematics".

The NSA, its policies, and uses of mathematics lies very much in the temporal realm of politics. A profession of "cryptolgy" could exist alongside this realm; Mathematics cannot. It's a subtle, indeed soft redefinition, but it should start happenning sooner rather than later before the NSA embarks on any more such gaucheries.

35. **telemurk**
July 22, 2015

It's an interesting discussion. But I think the idea that the NSA is actually "protecting" us is questionable given the many revelations of Binney, Drake, Kiriakou, Snowden and so on. Listening to some of the interviews on YouTube with these very intelligent, certainly courageous and (at least to their own way of thinking) very patriotic folks is an enlightening experience.

Two small further points:
-an IT security guy I met on an airplane told me that the speculation is that it was actually the Russians and not the Chinese who stole the personnel data; he said that was much more their style, and that perhaps for US political reasons it was more interesting to pressure the Chinese;
-No doubt the kids who do go to these camps – especially the good ones-will forever be on the radar of the NSA, whether or not they end up working there, perhaps diminishing the odds they will use any hacking skills for (non-government-sanctioned) nefarious purposes.

36. **Joshua Rubin**
July 29, 2015

I am writing in defense of teaching about password cracking, and about understanding the tools bad guys are using.

Passwords are one way of establishing the rights that should be granted by a computer system. Others exist too — something you know, something you have, or a physical characteristic. Sometimes two of those are used. Sometimes two people must cooperate. Designers must understand the tradeoffs.

A good system must protect against realistic threats. It is important to be able to spell out those threats. The bad guys might make an educated guess about one person's password . The bad guys might have a complete copy of everything on the computer system, but not have any password. The bad guys might want the password of any account, not a specific one. The bad guys might be monitoring everything the computer does, in real time. The bad guys might have resources that far exceed your own. The bad guys might know far more than you know. The bad guy might pretend to be a user who forgot their password. The bad guy might be the system administrator. The bad guy might be your love interest.

Just as physics students need to play with air tables, future security folks need to play with cracking tools. Doing uses a different part of the brain than reading or listening.

37. **Tom Leinster**
July 31, 2015

Michael Hutchings asked “I keep hearing that the NSA is the largest US employer of mathematicians, but does anyone have any actual numbers or estimates?”

In testimony to the Senate Judiciary Committee on 2 October 2013, Keith Alexander (then director of the NSA) stated that the NSA employed 1013 mathematicians and that they were the largest employer of mathematicians in the USA. He didn’t elaborate on what he meant by “mathematician”. Link here.

Random Notes

July 21, 2015
Categories: Uncategorized

- The LHC has now finished the first part of its physics run at 13 TeV, with intensity ramping up more slowly than hoped. Total luminosity/experiment so far is about .1 inverse fb (see here), about a tenth of some earlier projections (see here), not enough for any likely new physics results this summer (see here).

  According to the latest schedule physics will begin again the second week of August, with beam intensity increasing during the month. Most data-taking is planned for September and October, with a target of 10 inverse fb.

- Yoichiro Nambu died recently, at the age of 94. He was one of the most influential figures in theoretical physics, for his work on many topics, but especially on spontaneous symmetry breaking in quantum field theory. Unfortunately I never got to meet him, it sounds like he was one of the nicest people in the field. There are lots of stories now out about him and his work, I especially liked this one from one of his students.
- Massimo Pigliucci's Scientia Salon project now has a book of essays out, with the title Scientistic Chronicles.
- There's a wonderful article out by Rick Jardine on Grothendieck's great work on homological algebra, known to mathematicians as "the Tohoku paper". It was written while Grothendieck was in Kansas, and immediately had a huge influence on the subject. Jardine explains not only the background of the article and what's in it, but some of the later developments that have come out of it.

Update: Via David Goss, news that the FBI file for Paul Erdos is now available.

Comments

1. **Noboru Nakanishi**
   July 21, 2015

   I would like to express my sincere condolence for the death of Professor Y. Nambu, who had been my most respectable acquaintance since 1961.

2. **fg**
   July 22, 2015

   Another recent loss is that of Raymond Stora, the S of BRST, who died recently at the age of 85.

3. **gevtev**
   July 22, 2015

   GeV –> TeV?

4. **Peter Woit**
   July 22, 2015

   fg,
   Thanks for letting us know, I'm sorry to hear that.

   gevtev,
   Thanks, I seem to make that mistake regularly, someday I'll get used to this TeV scale business…

5. **Noboru Nakanishi**
   July 22, 2015

   fg
   Thank you for your information. Professor R. Stora was also my old friend.

   I have also heard that Professor S. Okubo of Rochester University died at the age of 83 on July 20. He is known by Gell-Mann-Okubo mass formula, etc.

6. **Pt**
   July 23, 2015

   The inference magazine that Jardine's article is published in seems to have been created to promote anti-evolution views. It's too bad because their other articles are interesting!

7. **anonymous**
   July 23, 2015

   Don't know if you've seen this, Peter.

   How likely is it that they're seeing a genuinely new resonance ? A super-partner ?

   http://www.symmetrymagazine.org/article/july-2015/something-goes-bump-in-the-data

   "If this bump matures into a sharp peak during the second run of the LHC, it could indicate the existence of a new heavy particle with 2000 times the mass of a proton."

   "Even though this bump is far too small to signify a discovery and presents no predictable pattern, its presence across multiple different analyses from both CMS and ATLAS is intriguing and suspicious. "

8. **Peter Woit**
   July 23, 2015

   anonymous,
   There are good postings about this from Tommaso Dorigo

http://www.science20.com/a_quantum_diaries_survivor/those_three_higgs_events_piling_up_at_18_tev-156030
and Jester
http://resonaances.blogspot.com/2015/06/on-lhc-diboson-excess.html

I think Jester has it right ("there's little reason to get excited yet.") There have recently been lots and lots of papers "explaining" the diboson excess (there's a string theory explanation for this, just as there's a string theory explanation for anything…).
There may be enough data collected in Sept-Oct. to know by the winter conferences in early 2016 whether there's anything there. So, just wait a bit, but I don't think the odds are great that this is something real.

9. **vkrishna**
July 23, 2015

I found the personal remembrance of Prof. Nambu that you linked to, to be very nice. He seems to have been a really decent man in addition to being a great physicist. I have
Thanks.

A Beautiful Question

July 24, 2015
Categories: Book Reviews

Frank Wilczek's new book, A Beautiful Question, is now out and if you're at all interested in issues about beauty and the deep structure of reality, you should find a copy and spend some time with it. As he explains at the very beginning:

> This book is a long meditation on a single question:
>
> **Does the world embody beautiful ideas?**

To me (and I think to Wilczek), the answer to the question has always been an unambiguous "Yes". The more difficult question is "what does such a claim about beauty and the world mean?" and that's the central concern of the book.

Early chapters are of an historical nature, searching out the roots of such ideas, going back to the Pythagoreans and their beliefs about number and harmony, and then on to Plato and Platonism. The discussion of physics begins seriously with Kepler and Newton, and emphasizes very much the nature of light and color, topics about which Wilczek is currently actively pursuing new ideas. Maxwell then appears as the foundation of our modern understanding of light and electromagnetism.

Notions of symmetry (gauge symmetry) then begin to appear, as well as the surprising notion of quantization of wave motion. The basic ideas behind the Standard Model are extensively developed, emphasizing the connections to symmetry and beauty. Wilczek tries to make some changes in the conventional terminology, calling the Standard Model the Core Model, fields "fluids", etc., in an attempt to bring the subject closer to a conventional language that might give the average person some feel for what is going on. He also brings in some of his personal experience: he is of course most well-known as one of those responsible for the final stage of the development of the Core Model.

Wilczek has been traveling a lot lecturing about this recently. You can for instance watch or hear him speak here, here and here, read extracts from the book here or here, find reviews here and here. The Wall Street Journal has "a week in the life" here. I very much like Wilczek's comments in an essay here which explain how the book came about and who it was written for. His conclusion that he had written it to speak to himself as a child or adolescent very much resonates with my own experience writing a popular book.

The later parts of the book deal with more speculative questions about particle physics, and here I find that truly difficult issues appear: can we hope to agree on what beauty is in this context? Wilczek has never been a fan of string theory, and the various problematic claims about its "beauty" really don't come up. He is however a fan of supersymmetric GUTs, and there issues of beauty become problematic. Yes, the fact that a family of SM fermions can be organized into a spinor representation of SO(10) is a beautiful fact, and likely indication of something deep about the world. But there's a serious problem with getting unification by putting things into larger symmetry groups, without at the same time having a compelling idea for what breaks the larger symmetry to the smaller one we see. Just postulating a new set of GUT Higgs is not so pretty.

Similarly, the general idea of supersymmetry has beautiful aspects, but its implementation as a specific extension of the Poincaré group starts to become seriously unbeautiful once one introduces structures needed to break the supersymmetry to correspond to observation. I think one reason for Wilczek's fondness for this idea is his involvement in the first calculation of coupling constant unification in such theories. A parent always thinks their child is unusually beautiful, but may not always be a very good judge of the matter…

Wilczek has made some bets that SUSY will appear at the LHC, but I think he's going to be losing them. Garrett Lisi has an account here of one such bet, which had a problematic time limit. By next summer I think there will be enough data to start making a judgment about whether SUSY is there at LHC accessible energies. I'm quite curious to see how Wilczek and others of his generation that had so long invested their hope in this will react to a negative result. Will SUSY start looking a lot less beautiful?

Bonus item: In today's Wall Street Journal there's another profile of a particle physicist, of George Zweig, a co-discoverer of the quark theory which ultimately was vindicated by the later discovery of asymptotic freedom by Wilczek and others. Zweig is now starting a hedge fund, I hadn't realized that he worked for quite a while at Renaissance, the Jim Simons hedge fund.

Update: This does seem to be the week for profiles of mathematicians and physicists in the media. There's a wonderful one about Terry Tao in the Sunday New York Times magazine.

Comments

1. **Bernhard**
   July 24, 2015

   I haven't read the book, but I suppose Wilczek discuss at the beginning of the book (or somewhere) what he means by "beauty". I remember many years ago reading Weinberg's "Dreams of a Final Theory" that the analogy for him of beauty was with a "beautiful horse" and that "a beautiful horse wins races". So this was a subjective idea that had an objective way to be tested. The same would hold for physics theories (beautiful theories are the ones like QED that are also objectively successful).

   This is interesting as beauty has been one of the big arguments for SUSY (I recall John Ellis' "why I love SUSY" in 2011) but SUSY has been very unsuccessful in the way Weinberg discussed. I don't get how at this point SUSY can still be beautiful but I suppose I need to read the book.

2. **Noboru Nakanishi**
   July 24, 2015

   Peter,
   I agree with your opinion on SUSY. But I would like to claim more definitely: Spontaneously broken SUSY is totally ugly; it can never been a truth.

3. **Walter**
   July 24, 2015

   Just because the result of a broken symmetry is very ugly, doesn't mean the underlying symmetry is less likely to be true. I'm talking about SUSY here. If SUSY is so beautiful, does it matter if breaking SUSY leads to an ugly-looking theory? I thought the general trend was that as you go higher in energy, things look more beautiful. So if you go at high enough energies, SUSY is restored, and everything is nice and beautiful again. So why is the ugliness of broken SUSY a reason for thinking SUSY isn't true?

4. **Sebastian Thaler**
   July 24, 2015

   There is a fascinating profile of Terry Tao in this Sunday's NY Times Magazine:

http://www.nytimes.com/2015/07/26/magazine/the-singular-mind-of-terry-tao.html?hpw

5. **Yatima**
July 25, 2015

Does Frank Wilzeck mention "low Kolmogorov complexity" as one of the driving criteria of "beauty" as mentioned for example here? The postulate being that:

Among several patterns classified as "comparable" by some subjective observer, the subjectively most beautiful is the one with the simplest (shortest) description, given the observer's particular method for encoding and memorizing it.

This sounds very correct. Theories with a low-complexity "core description" and no extraneous "ifs", "thens" and "buts" and randomly tucked-on stuff, all of which would increase the KC (however given) by a factor of 2 at least are indeed what one likes to see.

6. **Peter Woit**
July 25, 2015

Walter,
The argument that "my beautiful idea only applies at high energies where it can't be tested; at energies where we can do experiments it doesn't apply and doesn't say anything " has some obvious problems…

7. **unification**
July 25, 2015

in your opinion are GUT theories of unification like SO(10) promising? is there any GUT you find promising? what do you think is a promising avenue for unification? are there any well motivated theories of GUT, unification, as well as no unification of strong and electroweak forces? thanks

8. **Peter Woit**
July 25, 2015

Unification,
There's a chapter in my book about GUTs, it's a complicated story, but I don't think anything much has changed since I was writing that chapter a dozen years ago.

What isn't really discussed in the book is the construction that Wilczek describes in detail in his, how to make a generation of fermions out of the spinor representation for SO(10). This is "beautiful", there's something important going on there, but I think we're missing some important ideas about what this means.

You can think of the SO(10) spinor representation as what you get from a collection of 5 fermionic oscillators, this is described in detail in my notes at
http://www.math.columbia.edu/~woit/QM/qmbook.pdf
For a good discussion of the relation to GUTs, see the article by Baez and Huerta
http://arxiv.org/abs/0904.1556

9. **Bill**
July 25, 2015

That NYT article was great – not because it was about Tao but because it contained some lovely thoughts about mathematics in general. The article would be more appropriate if Tao actually solved the twin prime conjecture or Navier-Stokes.

10. **Chris Oakley**
July 25, 2015

A general point here. One problem with using "beauty" as a criterion for scientific theories is the lack of a precise definition. In the case of female beauty, though, there is a well-defined unit called the Helen (H), named after Helen of Troy. As this is a large unit, people prefer to work with the millihelen (mH), which is defined as the beauty required to launch a single ship.

11. **Anonyrat**
July 25, 2015

If Ptolemy's epicycles worked, would we consider them to be beautiful?

12. **Bernd**
July 25, 2015

Isn't SU(5) also very beautiful?

13. **PeterH**
July 25, 2015

Carl Sagan in an episode from the Cosmos series stated that the Universe need not be in harmony with human ambition. The human ambition to find a 'beautiful' theory of the laws of nature is meaningless since 'beauty' is subjective.

14. **Peter Woit**
July 25, 2015

Bernd,
The SU(5) vs. SO(10) question in GUTs is a good example of where relative magnitudes of beauty are clear. To describe one generation of fermions using SU(5) you need two irreducible representations, of dimension 5 and 10. Using SO(1o) (more accurately, Spin(10)), you just need one representation, and it's a special distinguished representation, the 16 dim spinor representation. So, the SO(10) case is more beautiful…

One has 16=10 +5 +1, so SO(10) also includes a singlet, which is just what one expects for a right-handed neutrino.

15. **Noboru Nakanishi**
July 25, 2015

Walter,
Spontaneously broken SUSY implies the existence of Nambu-Goldstone fermion, which was experimentally excluded. If one appeals to super-Higgs mechanism by using

supergravity, it implies that quantum-gravity effect cannot be neglected in the energy region of the Standard Theory. Furthermore, quantum supergravity cannot be global-SUSY invariant, because the anticommutator of supersymmetry generators in quantum supergravity cannot have the spacetime index of the translation generator.

16. **Walter**
July 26, 2015

Peter,

I understand your point, but my question was meant to address your thoughts on SUSY leading to the ugliness of having a lot of extra particles. I understand how that can be viewed as ugly. But if the underlying, fundamental theory, valid at the Planck scale (whatever it is, no assumptions here) is "beautiful", even though it leads to a million fundamental particles from the TeV scale to the Planck scale, why should these million particles negate this? That's all I was asking.

17. **manfred Requardt**
July 26, 2015

I have problems with beauty as a scientific criterion. This would imply that our anthropocentric point of view is the same as the view how nature works in its deep structure. Keplers mysterium cosmographicum was a concept derived from beauty but was totally beside the point . I lectured about SUSY several times but while being perhaps a mathematically beautiful framework I never found it beautiful from a physical point of view. The same holds in my view as to string theory.
It is important to understand the biological and psychological roots of a concept like beauty before one can decide if beauty is a useful scientific concept.

18. **Peter Woit**
July 26, 2015

Walter,
The issue isn't number of fundamental particles, it's whether they're tightly constrained by a symmetry argument. The simplest SUSY extensions of the SM have a new particle for each of the ones we know about, which is twice as many particles, but SUSY tells you their properties. You can argue whether this is more beautiful than the SM (more symmetry, but a symmetry that tells you only about unobserved particles, nothing about the observed SM particles), I happen to think not.

The big problem with SUSY though is that these simplest extensions don't work (since they predict superpartners we don't see). So, you need to make the theory more complicated by adding degrees of freedom that will cause SUSY breaking. This is where it gets completely ugly, and it's at the level of the fundamental theory.

19. **David Bailey**
July 26, 2015

I remember as a kid learning the gas laws PV=nRT. I thought that was incredibly beautiful – so much information packed into one equation – and then I learned that this equation is inexact, and that a more accurate equation devised by van der Waals was more accurate, but it had different coefficients for each type of gas!

That made me rather cautious about appeals to beauty in physics.

Maybe every appeal to beauty implicitly assumes there is no deeper structure to mess things up (the quantum structure of molecules in the above example, which give molecules finite size and intermolecular forces).

20. **manfred Requardt**
July 26, 2015

I agree totally with what David Bailey says. The problem is that most of our theories are only effective theories being approximately correct on a certain level of resolution. In a sense there is no real reason that such theories should be beautiful. On the other hand, in the realm of mathematics there is ample room for beauty. This is the platonic point of view uttered by many great mathematicians (recently by e.g. Allan Connes). Nevertheless even there this philosphy is a little bit vague.

21. **Magnema**
July 26, 2015

@Peter Woit: So the real issues come not from SUSY itself, but from the modifications to the simplest ideas of SUSY in order to have it be compatible with observation?

Would these extra degrees of freedom you refer to be yet another (undetermined by SUSY itself) particle (something like the Higgs, given their role in symmetry breaking)?

22. **Thomas**
July 26, 2015

manfred Requardt:
"It is important to understand the biological and psychological roots of a concept like beauty before one can decide if beauty is a useful scientific concept."

I think this is a very important point. And there is a nascent field about that, neuroaesthetics.
http://old.unipr.it/arpa/mirror/pubs/pdffiles/Gallese/2009/DiDio_COP2009.pdf

Even more on topic, you may want to look at this fMRI study co-authered by Atiyah:
http://journal.frontiersin.org/article/10.3389/fnhum.2014.00068/abstract
I wonder whether Wilczek mentions that in his book.

23. **geometriclanglander**
July 26, 2015

The article on Terence Tao doesn't really say anything new, I'm not sure why you consider it 'wonderful'. Tao certainly deserves all the plaudits and praise bestowed upon him, but I'm not sure that such hagiographies give any particular insight. It might be good for scientific outreach I guess.

In any case, on another Tao related matter, the comments on this post
https://terrytao.wordpress.com/2015/07/20/a-nonstandard-analysis-proof-of-szemeredis-theorem/
make interesting reading from a viewpoint of scientific sociology.

24. **manfred Requardt**
July 27, 2015

Thomas, many thanks for your helpful hints and remarks! By the way, there is a related discussion concerning the conventionalism in science as being introduced by e.g.

Poincare (see the nice biography by Jeremy Gray). This is a purely pragmatic criterion without aesthetic qualities (apart perhaps from simplicity and economy of thinking). But, nevertheless, it is obvious that there is some peculiar structure underlying our universe.

25. **Beauty in Mind**
July 27, 2015

I have recently come across Weber electrodynamics. In my view this is an extremely beautiful theory, but it seems that it just doesn't work. If so, this would be a nice example of beauty being deluding, and probably it is not an exceptional case in the history of physics.

"The great tragedy of science – the slaying of a beautiful hypothesis by an ugly fact." – Thomas Henry Huxley –

26. **Peter Woit**
July 27, 2015

Magnema,
There's an infinite variety of ways to get SUSY breaking and explain away the non-observation of superpartners. But all involve postulating additional, otherwise unmotivated, physics beyond a simple SUSY extension of the SM.

27. **Jim Eshelman**
July 27, 2015

It seems to me that "beauty" is largely being used where simplicity is really meant in much of this discussion. IMHO beauty is way too subjective to enter into any discussion in physics, whereas simplicity is much easier to agree on — PV = nRT is simple, the van der Waals equation is certainly less so. Few would disagree. The Standard Model is far from simple. That's what all the fuss is really about—find the overarching simple set of concepts that unify and simplify that mismash of fields and adhoc "constants". Only the means differ.

28. **perry**
July 27, 2015

SU(5) was beautiful till the protons refused to die

29. **RandomPaddy**
July 27, 2015

A 78 year old is starting a hedge fund. I think we've hit the top.

30. **Chris W.**
July 28, 2015

Speaking of Zweig, the Wikipedia profile of him points to this CERN colloquium talk he gave in November 2013, which is a history of particle physics leading to the development of the concrete quark model. (It starts with a review of the stage-setting prior to World War II. In 1947 Zweig was 10 years old.)

The talk has probably been widely read, but I had never come across it before. It's extremely readable, with many fascinating anecdotes about the wrangling that led to the concrete quark model. (It also indicates that Zweig turned to neurobiology by the end of the 60s—earlier than I had thought.)

31. **manfred Requardt**
July 29, 2015

As Jim Eshelman says, simplicity is certainly an important aspect in this field. The concept of symmetry in all of modern physics is an example. On the other hand, even this quality is not entirely clear. Einsteins field equation are looking simple but threy are in fact very complicated if one tries to solve them (non-linar partial diff.-equations). Sometimes only the mathematical representation of an actually complex situation may appear simple.

32. **Jim Beyer**
July 30, 2015

I tend not to like the terms 'beauty' (or especially 'elegance') when describing scientific endeavors. In my opinion, these phrasings seem to be an effort to persuade one towards the author's belief system absent grounded technical evidence.

33. **GoletaBeach**
August 2, 2015

Ground Loops. Fadeev-Popov ghosts. Eventually, there is messiness underpinning beautiful experiments and theories.

34. **Peter Woit**
August 3, 2015

GoletaBeach,
"Fadeev-Popov ghosts"
Exactly. This is one part of the Standard Model that isn't beautiful, and isn't completely satisfactory non-perturbatively. I think that's a strong argument that it should be getting a lot of attention as an aspect of the SM that we don't properly understand, for which there is a more beautiful, more compelling explanation, one that would teach us something about the fundamental theory.

35. **anon**
August 3, 2015

Faddeev-Popov ghosts and BRST are not a fundamental part of the Standard Model, are they? Since there are other ways of fixing the gauge that don't involve ghosts, aren't they mostly a calculational device? But, for what it is worth, I personally find Faddeev-Popov ghosts and BRST to be a technically quite beautiful and elegant calculational device, since they magically automate a lot of the gauge fixing while allowing you to largely do calculations in a gauge-invariant set of variables, as opposed to an awkward and arbitrary choice of reduced variables. I use "magically" on purpose, since the idea of "negative" degrees of freedom embodies in ghosts still seem magical to me to this day.

36. **Peter Woit**
August 3, 2015

anon,
Faddeev-Popov/BRST is the only covariant gauge-fixing procedure I know of (for the non-abelian case). Non-covariant gauge-fixing introduces a different set of technical problems and uglinesses. The only way of dealing with gauge symmetry that I know of no technical problems with (perhaps just because of ignorance…) is what the lattice people do: don't fix the gauge, integrate over all gauge choices. But you can't get the perturbation series this way.

I agree that the introduction of anti-commuting variables to deal with gauge symmetry is not ugly, but a quite beautiful idea. It's closely related to very deep mathematics: homological methods in representation theory. The fact that the same "trick" (introducing anti-commuting variables, a complex, and looking at the cohomology of the complex) occurs throughout modern mathematics is a good reason to believe that it's the right way to handle the problem, and well deserves to be thought of as beautiful.

37. **GoletaBeach**
August 3, 2015

Thanks Peter, I hope a beautiful solution to the ghosts emerges.

As the old joke goes, when Fermi asked God how to solve Ground Loops, God simply replied `Never could solve that problem.'

This Week's Hype

July 28, 2015
Categories: Multiverse Mania, This Week's Hype

Symmetry, the FNAL/SLAC run online magazine funded by the DOE, today is running a piece of multiverse mania entitled [Is this the only universe?](). It's a rather standard example of the pseudo-scientific hype that has flooded the popular scientific media for the last 10-15 years.

Besides the usual anthropic argument for the size of the CC, the evidence for the multiverse is string theory:

> For further evidence of a multiverse, just look to string theory, which posits that the fundamental laws of physics have their own phases, just like matter can exist as a solid, liquid or gas. If that's correct, there should be other universes where the laws are in different phases from our own—which would affect seemingly fundamental values that we observe here in our universe, like the cosmological constant. "In that situation you'll have a patchwork of regions, some in this phase, some in others," says Matthew Kleban, a theoretical physicist at New York University.

No mention is made of the fact that there is no evidence for string theory, with the multiverse given as the usual argument for why this is so.

Kleban also claims the theory is testable in this way:

> it may be possible to experimentally induce a phase change—an ultra-high-energy version of coaxing water into vapor by boiling it on the stove. You could effectively prove our universe is not the only one if you could produce phase-transitioned energy, though you would run the risk of it expanding out of control and destroying the Earth. "If those phases do exist—if they can be brought into being by some kind of experiment—then they certainly exist somewhere in the universe," Kleban says.

No word on how to do that.

Nomura says the way to test the idea is by looking at the universe's spatial curvature (which, according to Planck is zero within the experimental uncertainties). According to Nomura, the implications for the multiverse of possible more accurate measurements of the spatial curvature are

- If it remains zero, that's consistent with the multiverse.
- If it is negative, that's "strong evidence of a multiverse".
- If it is positive, that's a problem for some multiverse models, but not evidence against the multiverse, because you can't have evidence against the multiverse:

  > a positively curved universe would show that there's something wrong with our current theory of the multiverse, while not necessarily proving there's only one. (Proving that is a next-to-impossible task. If there are other universes out there that don't interact with ours in any sense, we can't prove whether they exist.)

Kleban and Nomura are quite excited by this, because the multiverse has the wonderful implication that we can all give up on trying to find a better fundamental theory and do something more useful with our lives:

> If there were different universes with different phases of laws, we might not need to seek fundamental explanations for some of the properties our universe exhibits.

Comments

1. **Scott Church**
   July 28, 2015

   Peter, I find it interesting that Kleban says, "a positively curved universe would show that there's something wrong with our current theory of the multiverse, while not necessarily proving there's only one…" His *own* research has shown that positive curvature at the 10^-4 level would rule out slow roll and false vacuum eternal inflation, and negative curvature on the same scale would rule out slow roll eternal inflation (http://arxiv.org/abs/1202.5037). Guth and Nomura reported similar findings (http://journals.aps.org/prd/abstract/10.1103/PhysRevD.86.023534). Current Planck results give a flat universe to a confidence interval of only +/- 10^-3 so more refined observational data could verify either possibility.

   It seems to me that eternal inflation is… forgive the expression… "the only game in town" for generating a multiverse, and the vacua of string/M-theory are needed if it's to have the widely varying physical constants required by anthropic constraints. The only alternatives I see are Tegmark's Level-4 multiverse or Many Worlds QM (both of which to me seem downright bizarre), or purely speculative new physics of some sort for which there isn't a shred of evidence. So in the absence of this sort of wishful thinking/special pleading, if future refinements in observed curvature do rule out eternal inflation this way, how exactly have we failed to prove that there's only one universe? Am I missing something here?

2. **Peter Woit**
   July 28, 2015

   Scott Church,
   The thing you quote I think comes from Nomura, not Kleban. I've seen the Guth/Nomura paper you mention, but I've never been convinced that you can't get positive curvature out of this kind of scenario. In particular, look at the conclusion of this paper
   http://arxiv.org/pdf/hep-th/0610231v2.pdf
   which I've never seen refuted.

   It seemed to me that all that Nomura was doing was stating the obvious: multiverse proponents will claim victory if the curvature is negative, come up with a different model if it is positive. You'd have to ask him though about this, it's quite possible the writer mangled some subtlety of what he was trying to say.

3. **Scott Church**
   July 28, 2015

   Thanks Peter! I've downloaded a copy of that paper and will give it a read asap. Have you seen the Kleban & Schillo paper as well (the first link)? It was published the same year as Guth and Nomura's, and appears to have taken a somewhat different path to the same results. In any event I imagine you're right… whether eternal inflation is ever falsified or not, multiverse proponents will find ways to keep it alive. They have 10^500 vacua to play with… and who couldn't get what they want out of that? 🙂

4. **Peter Woit**
   July 28, 2015

Scott Church,
I've seen it, but haven't looked at it carefully, beyond noticing that they don't even refer to the earlier paper I mentioned that reaches opposite conclusions. I just don't see how the sort of arguments they're making can possibly be strong enough to rule out all mechanisms for making extra universes.

5. **Peter Woit**
July 28, 2015

Maybe another comment I should have made in the posting is that, as anyone who watched "Particle Fever" knows, Nomura was saying that the multiverse predicts a 140 GeV Higgs. When that was falsified, there seems to have been no impact on his promotion of the multiverse (rather the opposite…), so I see no reason to believe that falsification by discovery of a positive spatial curvature would cause him to abandon the idea. It was interesting in this article that he seems to be explicitly acknowledging that what he is arguing for cannot be falsified.

6. **Scott Church**
July 28, 2015

Yeah… so I saw from some of your previous posts about LHC's failures to date to turn up a gluino. Now that LHC Run 2 is under way it'll be interesting to see how he (and others) respond if no superpartners are discovered by next summer.

Btw, on another note, I just finished your book yesterday, and I cannot tell you how much I enjoyed it! Sadly, I'm in a bit of withdrawal now… what to read next that could be as enjoyable and informative…? 🙂

7. **verruckte**
July 28, 2015

Well, I'm disappointed in Symmetry for running that piece. Their stuff is usually better than that.

8. **Low Math, Meekly Interacting**
July 28, 2015

It's not all bad. Quanta has an interesting recent piece on Navier-Stokes that might help with pop-sci-induced depression.

9. **nicola**
July 29, 2015

Peter,

that 140GeV Higgs was obviously meant for some other Universe, not ours ;).

10. **Chris Kennedy**
July 29, 2015

The magnitude of multiverse nonsense that continues to be perpetuated is really unfortunate. I saw part 1 of the Uranium documentary on PBS last night. Sure it was very basic for a general audience but when you look at what real scientific discoveries and accomplishments were decades ago it shows by comparison just how tragically hollow the community's current goals and purpose have become.

11. **Anonyrat**
July 29, 2015

"…we might not need to seek fundamental explanations for some of the properties our universe exhibits."

IMO, given planetary orbit sizes, orbital periods, masses, planetary diameters, rotational periods, etc., theory eventually tells us that the sizes of orbits and orbital periods can be related easily and follow as a consequence of a fundamental law, the other things do not. To be useful, multiverse theory has to tell us what features of the Standard Model follow readily as a consequence of some fundamental theory, and which do not.

12. **Chris W.**
July 29, 2015

> "…we might not need to seek fundamental explanations for some of the properties our universe exhibits."

Too bad nobody suggested the idea of a multiverse to Isaac Newton. It might have saved him and his successors a lot of work. Never mind that the work actually bore fruit… 🙂

13. **Giotis**
July 30, 2015

Well on theoretical level and since String theory is currently the only reliable theory of everything and Quantum gravity to work with someone must answer first the following question:

What is the correct vacuum of String theory?

To answer this question you must have a background independent, non perturbative definition of the theory and (besides AdS/CFT) we are still lacking of such definition. String Field Theory for example if further developed has the potential to answer such deep questions in a background independent manner (Ashoke Sen's conjectures in OSFT is a clear indication that it can do so).

What we have so far in the String landscape are the low energy effective vacua derived from the perturbative/semiclassical String theory but combining these with eternal inflation and the seemingly (so far at least) unnaturalness of the SM and of the Cosmological Constant the case of the multiverse becomes quite appealing.

But again these are mostly bottom up considerations, if we don't have a complete Cosmology provided by String theory via its background independent, non perturbative definition I don't think we can give a definitive answer. I know for example that David Gross strongly advocates this PoV.

The point to take away is that String theory has not given yet the final OK for the Multiverse picture.

14. **al**

July 30, 2015

Hypothetically true = Real
This is the philosophical equation of strings-cum-multiverse theories.
The hassle is who should bear the burden of proof when rejecting the substitution
noncontradictoriness= evidence.

15. **Peter Woit**
July 30, 2015

Giotis,
"the case of the multiverse becomes quite appealing"

As far as I can tell, the only thing "appealing" about it is that it allows string theorists to explain away the failure of the idea of string theory unification.

16. **Richard Noth**
July 31, 2015

re. Giotis,

I'm sure I'm not the only reader here who is having trouble wrapping their head around something being 'the only reliable theory of everything' whilst simultaneously sufficiently lacking in definition to be able to make any concrete statements about any of the 'everything' around us!

Math and Physics Summer Camps

August 1, 2015
Categories: Uncategorized

With the kids shipped off to NSA summer camp, now is the time for mathematicians and physicists to head off to their own summer camp experiences. Some of these have websites where the rest of us can participate a bit virtually. These include:

- Out at Stony Brook, in mathematical physics there's the Simons Center Workshop going on now. This features lectures at the nearby Smith Point beach.
- Down in Princeton, the summer PiTP program for this year has just ended, with the topic New Insights Into Quantum Matter. In recent years many HEP theorists have given up on applying duality arguments to string theory unification and have gotten interested in condensed matter physics. Links on this schedule will take you to lectures that include three by Witten on Fermions and Topological Phases.
- Utah has been the center of the algebraic geometry world for the past few weeks, with the AMS Summer Institute in Algebraic Geometry held in Salt Lake City ending today. This is one in a series of big events held every ten years and as usual was preceded by a bootcamp for graduate students. Also as usual, the NSA is helping out with the funding.

  Overlapping with this, some algebraic geometers were at camp up in the mountains nearby, at the Zermatt Resort, for the PCMI Summer Session, this year the topic was Geometry of moduli spaces and representation theory.

- Later this month, SLAC will host the 43rd SLAC Summer Institute, with topic this year The Universe of Neutrinos.
- For Spanish-speaking mathematicians, around the same time the place to be will be Cusco for AGRA 2015. Michael Harris has notes for his lectures and has started blogging in the appropriate language.

Comments

1. **RandomPaddy**
   August 2, 2015

   What happens at academic workshops anyway? Are they more for graduate students or do professors learn a few tricks as well? Or are they really just another hazily defined part of the workshop/colloquim/seminar/symposium/conference spectrum?

2. **Peter Woit**
   August 3, 2015

   RandomPaddy,
   Typically these things have some mix of graduate students, postdocs and senior people, as well as some mix of non-professional activities to keep them entertained. A few of them have separate components aimed at graduate students, or even undergrads (PCMI). One reason for linking to them is that there are a lot of good expository lectures, and some of the materials from them are available online.

Multiverses: Science or Science Fiction?

August 3, 2015
Categories: Multiverse Mania

The September issue of Astronomy magazine is now out, with a cover story on Multiverses: Science or Science Fiction? The author Bob Berman does a good job of explaining both the arguments for various Multiverses, as well as the reasons for skepticism about some of these arguments. After quoting Max Tegmark as defending multiverse theory as science since it is a prediction of an "arguably testable" theory (inflation), Berman ends the piece in a way I have to agree with:

> Given the current multiverse infatuation, it may be fairest to give the last word to a prominent skeptic. Columbia University mathematical physicist Peter Woit, who maintains the popular multiverse-critical blog Not Even Wrong, pulls no punches.
>
> "Physicists had huge success in coming up with powerful compelling fundamental theories during the 20th century," he explains, "but the last 40 years or so have been difficult, with little progress. Unfortunately, some prominent theorists have now basically given up and decided to take an easy way out. The multiverse is invoked as an all-purpose, untestable excuse. They allow theoretical ideas like string theory that have turned out to be empty and consistent with anything to be kept alive instead of abandoned. It's a depressing possibility that this is where physics ends up. But I still hope this is a fad that will soon die out. Finding a better, deeper understanding of the laws of physics is incredibly challenging, but it's within our capability as humans, as long as the effort is not overwhelmed by those selling a non-answer to the problem."
>
> Whoa, intense. We've got to toss the multiverse if we care about physics!
>
> Of course, if an infinite multiverse does exist, some other Woit is out there saying the exact opposite.

The same issue of Astronomy has a "Web Extra" entitled What happens if string theory is wrong? It mentions the 2013 poll of theorists discussed here, which had a large majority (73%) answering the question "Do you think that String Theory will eventually be the ultimate unified theory?" with a "No", then goes on to link to a 2007 article by Sten Odenwald. Some of that article includes quotes from an interview with Lenny Susskind, which Odenwald recently included here. It will be interesting to have an update on that material in a year or so once 13 TeV LHC results on supersymmetry are in.

Bonus material: Quanta magazine has a great interactive map of "Theories of everything".

Comments

1. **Bernhard**
   August 3, 2015

   Does Tegmark mention why inflation predicts a multiverse? I suppose the argument is that some models of inflation predict a multiverse (I'm not even sure what that really means, but anyway) ? In the case of string theory is clear what happens, the theory predicts nothing, so the multiverse is used as an excuse for this failure and it also become the main prediction of the theory. But inflation is supposed to make a few predictions (right?). I'm sure one can have inflation without a multiverse, so the whole connection is rather unclear to me. As I'm ignorant in this whole thing I just looked at Wikipedia, where I read "All models of eternal inflation produce an infinite multiverse, typically a fractal." Whatever that means.

2. **Peter Woit**
   August 3, 2015

   Bernhard,
   I'm no expert, but from what I've seen of work on inflationary models, Steinhardt is right that you can get pretty much whatever you want from them, and I assume that means a multiverse or no multiverse. One expert is Katie Freese, and here
   https://www.quantamagazine.org/20140722-in-search-of-dark-stars/
   she comments on the BICEP2 data (which turned out to be wrong)
   "If you take the BICEP data literally, which I'm not saying you should, you never have eternal inflation. So you don't have to have eternal inflation, if you ask me. I was very happy about that."
   See her paper with Will Kinney about this
   http://xxx.lanl.gov/pdf/1404.4614.pdf
   where the abstract says
   "even tiny running of the scalar spectral index is suffcient to prevent eternal inflation from occurring…"
   I have seen multiverse enthusiasts claiming "inflation implies multiverse", but since many of their other arguments are full of holes, I see no reason to believe this one isn't too.

3. **Curious Mayhem**
   August 3, 2015

   It's still hard to understand why people try to connect "multiverses" (in the string sense) with inflation. The two logically have nothing to do with each other. Inflation can give rise to different parts of spacetime having different elementary physics or "eternal" inflation, but they're all part of one meta-universe, or whatever you want to call it. And neither has anything to do with the many-worlds interpretation of QM, which is not a theory, but a particular spin on quantum mechanics, if you'll pardon the pun. (Arguably, it's an obsolete interpretation, superseded by a better approach, decoherence, which is actually testable.)

   Inflation could be proved tomorrow and have no direct bearing at all on string theory. There is an indirect bearing: string theory has a very hard time accommodating inflation, except for some special models with a peculiar symmetry.

   (Interesting lookback: I just saw the old "East/West Coast metric" post. The history of the West Coast metric is simple: Bjorken & Drell's 1964 textbook, before Feynman ever arrived at SLAC. The convenience for particle physicists is straightforward. Time-like and causal spacetime intervals have a positive square-interval, and most importantly, the momentum 4-vector has positive norm = +m^2. In a particle's rest frame, this convention fits naturally with just a time-like component being non-zero. And the phase of a positive wave starts with +omega*t (or E*t in natural units), again matching nicely with the rest frame.

   (The East Coast metric does have advantages if you're working with special or general relativity and need to make contact with the non-relativistic limit. But particle physicists mainly live in momentum space, not real space. Which might explain something about them ….)

4. **Peter Woit**
   August 3, 2015

Curious Mayhem,
I learned QFT from Bjorken and Drell, my invocation of Feynman was just a guess as to where they got their choice of metric from (they were writing in the early sixties, at a time there were already quite a few QFT books around). After I later checked a lot more QFT books I realized that many of the 1950s-era ones, for instance Schweber, or Bogoliubov and Shirkov, were already using the West Coast metric long before Bjorken and Drell. I'm still curious about the history, never managed to find out who first started using the West Coast convention. Interestingly, the earliest sources I looked at treated special relativity by using imaginary time (and thus the East Coast metric), which is very natural from the point of view of modern Euclidean methods.

I don't want to start up that discussion again, but would be interested if someone knew the 30s-50s history of this.

5. **Robert Delbourgo**
   August 3, 2015

   We were brought up on the West Coast metric at Imperial College in the late fifties/early sixties but that may have been the influence of Schweber's book. Bogoliubov and Shirkov was also out then. Again that may also be due to the fact that we were being instructed in particle physics; general relativity took more of a back seat.

6. **Low Math, Meekly Interacting**
   August 3, 2015

   I'll never get over my bafflement at the attraction to the anthropic multiverse. It's not so much wrong as a decision to commit suicide, from a scientific perspective. What's the point of a prediction that can never be tested? Even if one believed in the multiverse utterly, it's still a trust fall into the abyss. Is it really worth redefining science to keep? Isn't human dishonesty enough of a problem for science without having to dispense with the means to combat it?

7. **unification**
   August 4, 2015

   Inflation could be proved tomorrow and have no direct bearing at all on string theory. There is an indirect bearing: string theory has a very hard time accommodating inflation, except for some special models with a peculiar symmetry.
   –
   " string theory has a very hard time accommodating inflation" why is this so? there is a field string cosmology

8. **Bee**
   August 4, 2015

   Did you mean 13 TeV?

9. **Peter Woit**
   August 4, 2015

   Bee,
   Thanks, fixed. I need to figure out some way to stop always making that mistake..

10. **MB**
    August 4, 2015

    "Of course, if an infinite multiverse does exist, some other Woit is out there saying the exact opposite."

    Well that's an entertaining notion at least.

11. **anon**
    August 4, 2015

    Curious Mayhem, I am similarly puzzled by the seeming widespread confusion of multiverses (or phase regions in a single inflationary space-time) and many-worlds, which are completely unrelated concepts. I am also puzzled by the oft-repeated claim among otherwise serious people that every phase and every history that is possible in an infinite (possibly eternally inflationary or not) space-time is therefore *mandatory*. Maybe I am missing some ergodicity assumptions that they never state, but to me that sounds like like saying that because infinity is a lot, therefore any infinite sequence of integers includes every integer (obviously false).

    As for decoherence, though, since most people who study decoherence for good reason don't claim that it is an *interpretation* of Quantum Mechanics, I disagree that it can supersede any candidate interpretation of QM, including many-worlds (which I personally disbelieve, by the way) but this may be veering off-topic.

12. **Wayne**
    August 4, 2015

    Peter,

    I've always wondered why multiverse proponents always say that the laws of physics and the values of fundamental constants vary in each universe. If there is a multiverse (or if eternal inflation is correct), why must this be so? Why can't each instance of inflation somewhere just produce the same laws and constants?

    Also, I don't understand why string theorists say that string theory predicts a multiverse. Do they know how string theory describes the making of these different universes? As far as I know, they just have different solutions, but no mechanism by which these universes can get made. Or perhaps I'm completely wrong.

13. **Peter Woit**
    August 4, 2015

    anon,
    Likewise, I've never understood the "anything that can happen will happen" argument, it seems to be based on an assumption that the underlying theory is mostly vacuous (other than providing some constraints on what can't happen). In particular, in any sensible theory, no Peter Woit would be defending the multiverse…

    Wayne,

As usual in this kind of "string theory", you can come up with "string vacua" that give what ever you want. I'm sure you can come up with a string inflation model that leads to a multiverse with each universe having the same physics, as well as models with each universe having different physics. The appeal of the second is that you can use it as an excuse for not being able to predict anything, the first has the defect of being predictive.

14. **Scott Church**
August 4, 2015

Bernhard/Curious Mayhem/anon,

I'm no expert either, but I believe the confusion over the term "multiverse" is that it's used in different ways. Eternal inflation leads to one in that each of the "bubbles" that have reheated from the larger inflating "fractal" space-time (or "metaverse") are being called "universes." This is what Tegmark referred to as a "Level I" multiverse (Many Worlds "parallel" universes he calls "Level II"). Eternal inflation, if real, would lead to a Level I multiverse.

These days I see a lot of folks doing two things: First, they treat inflation as synonymous with *eternal* inflation, which it isn't (it is true that most inflationary models are likely to be eternal, but not all are, and those that aren't seem to get summarily dismissed). Second, they treat any inflationary multiverse as one based on the string landscape even though, as you said Curious Mayhem, the two logically have nothing to do with each other.

It seems to me this is happening mostly because in the absence of a truly workable theory, the eternal inflation/string landscape show is the only way to generate the widely varying physical parameters/laws one needs to anthropically sidestep fine-tuning issues. Without the string landscape, inflation (eternal or otherwise) only gives us a universe/s with the same physical laws as our own, and that doesn't do much to keep the fine-tuning demons at bay. A lot of people these days are terrified of fine-tuning… and whether they'll admit it or not, for reasons that seem to be more metaphysical than scientific (hence, the ease with which they accept post-empirical "beauty" as more scientific than direct observational verification). Over the last 30 years or so this is the best they've been able to come up with and they're sticking to it… because admitting defeat and facing the alternatives is just too damn scary. 🙂

15. **Magnema**
August 4, 2015

@ Scott: You're off-by-one on your numbers there, with respect to Tegmark.

Level 3 is QM MWI. A note on this: anon is correct in stating that decoherence is a phenomenon, not an interpretation. It's when a thermal system interacts with a quantum one, which effectively wipes away the quantum one – but this occurs in both MWI and Copenhagen-style work.

Level 4, known in the philosophical community as David Lewis' Modal Realism, is "everything possible exists" (up to some possible limitations). This has the strength of working from more-or-less no axioms (and therefore is supremely "simple"), but has the weakness that it's virtually impossible to get anything out of it. I think this is the epitome of "simple theories over understandable predictions," and is much more grounded in aesthetic metaphysics (and plausibly epistemology) than anything physicists (with the exception, as usual, of Tegmark) want to deal with.

Finally, there's the issue of the multiverse as it's most often discussed on this blog and similar sources, which Tegmark splits in half. You start out by realizing that if we have a really big universe, then there's a lot of parts that we haven't seen to date, and – given dark energy – we might never see, and therefore we can never observe. Inflation, in itself, gives you nothing more than what a really big universe gives you (and I believe this is true for both eternal and non-eternal theories). There's the issue of how seriously to take these other possible universes, but if they're all virtually identical then there isn't much of an issue here. (This is the kind of multiverse which, in the most straightforward sense, a positive curvature would rule out.)

The distinction here between "inflationary universe" and "infinitely large ordinary universe" only occurs if you add some theory – any theory – which has multiple low-energy limits, such as string theory. Then, you can get the anthropics on parameters, which is the whole point of people talking about the multiverse in the first place. This is where your numbering mismatch with Tegmark comes from – he calls the former, a really big universe, the level 1, and the latter, where you can get anthropics in physical laws, the level 2.

@Wayne: The (simplified) idea is that different parts of our universe would have no causal contact, and therefore, while they could be the same, there's no reason to be – and if there's infinitely many causally-independent regions, then with probability 1 they will have a representative from every non-probability-zero subset.

16. **Magnema**
August 4, 2015

Insofar as predictions and testability are concerned: I think it would be fair to say that a single-universe perspective makes the prediction that life should be a reasonably generic property of possible universes, given that the one that exists has life (barring both incredible luck and any deific-style selector on life existence). Any multiverse makes no such prediction, although the absence is a prediction itself relative to a single-universe perspective.

These predictions are highly unconventional in that, unlike typical science, these start with the experimental result (life being observed) and make conjectures about the theory, because they make guesses about the space of possible worlds based on our observed one. It's analogous to a physicist making a mathematical conjecture based on a particular model of an observed physical phenomenon, and then (when mathematicians prove the physicist's conjecture) taking it to be evidence that his model of the phenomenon is correct.

It's odd, to be certain, and brings to bear a question in the philosophy of science, namely: what are predictions allowed to be about? Are you allowed to conjecture something based on your theory which is logically either true or false before you conjecture it, then treat that as a prediction? In other words, should you count predicting math with physics as a testable prediction? Personally, I think it would be fair to say yes, but I'm willing to grant that it's not clear…

A mostly-related remark: I started following this blog because I thought it would be a good thing for me to have both pro-multiverse and anti-multiverse resources on my reading list. I'm glad I did that, because my positions have shifted significantly by the exposure to more perspectives… while I maintain a MWI-view of QM (with some QB sympathies), and maintain an acceptance of the multiverse, it's led me to question my views a bit more, as I've seen some legitimate criticism (as opposed to the flawed counterarguments I've heard most often).

In particular, whereas before I took the above test to be "single universe predicts no fine tuning" (which I still do) and "fine tuning exists" (which logically leads to the conclusion "single-universe has been falsified"), this blog has convinced me that we don't understand how to apply anthropics (either in "what life is" or in the prior probability of worlds) well enough to conclude the latter point. I'm still inclined to sympathize, but every time people try to use anthropics and make wrong predictions, I become significantly less certain that they can get the logic correct when they already know the answers they should get…

(Sorry for the longwinded double-comment, but I figured since I was talking about two different things I should split it.)

17. **Peter Woit**
August 4, 2015

Magnema,

I don't see any argument at all from "causally-independent" to "with probability 1 they will have a representative from every non-probability-zero subset" (and I think this is the point anon was raising). Just because you know nothing at all about causally disconnected universes doesn't mean there are an infinite number of them out there uniformly filling out some probability space.

To actually say anything about these other universes you need a theory of where they came from (and no, I'm not convinced such a theory will only produce negative curvature). The problem with all discussions of the multiverse is that they're empty of actual substance: no one has a viable parametrization of the space of possibilities or of the dynamics leading to these possibilities. What people do then is just make an empty statement about things not understood being equally likely, and their "multiverse theory" is indistinguishable from my "I have no idea what is going on" theory.

18. **Scott Church**
August 4, 2015

Magnema, thanks for the sanity check! I actually meant to preface that statement with *"If memory serves me…"* but inadvertently dropped it in my edits. Apparently, memory didn't serve me all that well. 🙂 Anyway, thanks for all your other comments too… very informative and thought-provoking.

19. **adrian**
August 5, 2015

Dear Peter,
I am not fan of taking a quote and deconstruct it, as it may loose the intended meaning, but what you wrote above caught my attention. Indeed, the quote in your article (did I misunderstand? is it you saying this?)

"They allow theoretical ideas like string theory that have turned out to be empty and consistent with anything to be kept alive instead of abandoned. "

Seems to suggest that you believe that

–Sting Theory is an empty idea.
–that String Theory is consistent with anything.

Is it the case that you believe on these two statements above?
Could you please qualify them? what do you mean by 'anything'?
what do you mean by 'empty'?
Are you suggesting that all the work done in String Theory since 1969 is empty of content? Could you please explain? I can think of some counter-examples to you two statements above.

20. **Magnema**
August 5, 2015

@ Peter Woit: Statistically, I believe it is fair to say that, for some distribution, in an infinite number of trials, you will, with probability 1, get at least one point (even infinitely many points) every non-zero measure subset, correct? If the subset has total probability p, then the probability of missing it N times is (1-p)^N, which goes to zero as N goes to infinity, meaning you will almost certainly hit it eventually.

Now, you can question three things that I can see. Firstly is whether or not they are randomly selected at all, which I take to be anon's point. I think the most plausible counterargument would be "if not randomly, then what?" In general, I don't see a way this is distinguished from any other statistical mechanical problem, where you have probabilities arising from a deterministic problem based on one's own ignorance. Of course, there's foundational questions in statistical mechanics, too, but at the very least I don't think this is a different question.

Secondly, there's the more technical issue of where the probability measure comes from. I accept this as an issue, both theoretically and (as evidenced by the repeated failures of anthropics) practically. In fact, to take it a step further, this even assumes a probability measure at all, which would be tricky to derive from first principles. I think this is more your protest, and it's certainly well-grounded as a concern.

However, in order to make assertions in general, we don't really need to know the exact probability measure so long as we know the measure-zero sets, which one can at least make some reasonable-sounding assumptions about. You don't need a particular distribution for anthropic explanation of some sort of finite fine-tuning to come out if the universe were infinite. Of course, you do need a measure to effectively apply anthropics as a predictive tool more generally, which I think is where the greater issue comes in, but I think that's a slightly different problem.

Thirdly, of course, is the issue of whether the universe is infinitely large at all, which would be the point where curvature would come in, as well as some other assumptions (e.g., cosmological principle) which may or may not be justified…

21. **Peter Woit**
August 5, 2015

adrian,
The discussion is about string theory as a TOE, and my claim is that, as such it is empty, predicting nothing.

Magnema,
I just don't see that "pick randomly, I can't tell you why" has any more content than "I have no idea what's going on here, pick randomly if you feel like it".

22. **Neil**
August 5, 2015

"Of course, if an infinite multiverse does exist, some other Woit is out there saying the exact opposite."

Ah, so there is one invariant in the multiverse. All Woits are contrarians.

23. **Peter Woit**
August 5, 2015

Neil,
I don't think skepticism about whether the multiverse is science is contrarian, but rather pretty boringly mainstream and conventional…

24. **John**
August 5, 2015

Scott Church mentions that one possible reason for people supporting the multiverse is to defend against fine-tuning. Peter and others are clearly against the idea of the multiverse. So does that mean Peter is not afraid of fine-tuning, supports it or has another explanation?

25. **Peter Woit**
August 5, 2015

John,
It bears repeating that I'm not "against the idea of the multiverse". Maybe there is a multiverse, I have no idea. My claim is rather that current multiverse theories have no significant scientific content.

As for "fine-tuning", I just don't think there's a real problem there. I have no idea what is responsible for the CC, so no idea how to calculate it. People who think they have a unified TOE, in which the CC is calculable, may have a problem with it generically being determined by the Planck scale and thus way too large. They have a fine-tuning problem.

In practice though, such people don't actually have a viable TOE with a calculable CC, so what they do is say "my TOE has a huge landscape, so I can't actually calculate the CC, instead I'm just going to assume it's equally likely to be anything". When they do that, if they assume things are calculable in terms of the Planck scale, they then have a fine-tuning problem (which they claim to solve with a multiverse/anthropic argument).

26. **Scott Church**
August 5, 2015

John,

When I said that many folks embrace string landscape/eternal inflation (SL/EI) out of a fear of fine-tuning, I didn't mean to imply the opposite–that those who are skeptical of it don't consider fine-tuning to be an issue. My only point was that many people have latched onto it unquestioningly for reasons that are ultimately less than scientific. Few physicists dispute the seriousness of fine-tuning issues, but many think that attempts to account for them via SL/EI have been at best unproductive, and at worst disingenuous. Too many people today just default to them because they're fluid enough to explain *away* what we see rather than explain it, and inappropriately blur the distinction between physics and metaphysics in the process. What's needed are models that are specific and robust enough to make predictions we can verify, and our search for them must be philosophically as well as scientifically honest. In this Brave New Post-Empirical world we're in grave danger of losing sight of that.

Magnema,

Regarding multiverse probability measures, one of the best treatments of the technical issues I've seen was given by [Ellis, Stoeger, and Kirchner (2008)](). But regardless of how we handle these issues we still have at least two fundamental problems. First, there are serious issues with an infinite universe, philosophical and observational, and even if there weren't I don't see how one could ever be verified (even in the case of negative curvature we'd only have infinite spacio-temporal extent if the universe/multiverse was simply connected topologically–an untestable premise). Second, the entire question of multiverse probabilities is moot until there's a demonstrable physical method for actually *producing* one. To that end, the only game in town is eternal inflation, which brings us right back to the whole too-fluid-to-be-useful problem. It seems to me that until there's a way around these conundrums SL/EI is more metaphysical belief than genuine science… turtles all the way down. There's nothing wrong with that per se, but if that is one's view then one should be honest about it, and many today aren't. 🙂

27. **Hank Schraeder**
August 6, 2015

Peter,

All this stuff is horseshit and I wish you'd stop reporting on this nonsense. You're preaching to the choir, so find something else to write about. I wish these so-called researchers stopped doing this garbage. They're trying too hard to justify their overly big paychecks. What nonsense.

28. **Layman 2000**
August 6, 2015

@Hank Schraeder:

You are wrong. Woit is not preaching to the choir. As a layman interested in physics I hear a lot about multiverse these days and even bought and read a book about it which turned out to be totally unsatisfactory. So I went out to look for arguments and counterarguments, and this site is an important source of information and informed opinion.

29. **Simeon Every**
August 6, 2015

The question of whether multiverse theories are scientific depends on what 'science' is taken to mean.
I always assumed that science meant 'that which can be known using scientific methodology', but I sense that some subscribe to a canonical interpretation of science, whereby a theory that invokes entities consistent with established entities that have been accepted as scientific is by default a scientific theory.
I have no good philosophical argument against the canonical interpretation but can see that it is of no obvious practical value.

30. **Peter Woit**
August 6, 2015

Simeon Every,
I don't think "invoking entities consistent with established entities" gets you into the "canonical interpretation of science". For example, few agree that my favorite theory of pre-big bang physics (a big green turtle did it) is science even though it satisfies your consistency criterion.

A more common "canonical interpretation of science" is that it's whatever scientists do. Susskind in particular argues for this. It's also why it's important that scientists speak up and challenge those in their ranks who are on a campaign to get acceptance for pseudo-science. I agree there's a good argument that there's too much on this blog about this, but an equally good argument that someone needs to be doing this, and at the moment far too few physicists are stepping up.

31. **AcademicLurker**
August 6, 2015

"Of course, if an infinite multiverse does exist, some other Woit is out there saying the exact opposite."

This sounds like the motivation for a Woit based research program, in which physicists postulate different string vacua and then try to predict what Peter Woit would think of the multiverse in the resulting universe…

32. **Code Ferret**
August 6, 2015

@AcademicLurker: That gave me a hearty chuckle this morning. Thanks

33. **Anonyrat**
August 6, 2015

In no branch of the multiverse does multiverse theory have significant scientific content, and therefore in all branches, the Peter Woits there will be saying this. That is what is invariant.

34. **Magnema**
August 6, 2015

@Peter Woit: I agree with your issue; however, I think the same question you raise can be applied to classical statistical mechanics – in a purely deterministic system, from whence the statistics? As I recall, that's a serious problem in the philosophy of physics as well – but I'd guess they're one-and-the-same problem, namely, how do you get from ignorance (either of the microstate of a system or the self-locating ignorance in the multiverse) to probabilities? Yet we do like to think we have an idea of what's going on in statistical mechanics.

@Scott Church: For the first question (infinite universe), I admit, I have no response, although I would say that (as a Bayesian) the longer we go without observing topological complications, the higher we may want to raise the probability of a simple topology… but that depends on your priors over topologies, which depends again on a measure over possible universes. I'm also curious about your "problems with an infinite universe" – aside from being untestable and the open question of how to derive probabilities in the infinite case (which I'm not sure is a "real problem" in the same sense as what I said to Peter – it's no different than the usual foundational Stat Mech difficulties).

For the second, I would have two responses. Firstly, "argument without a particular mechanism" can be valid sometimes – after all, it is the entire principle behind (ordinary) inflation. If it works there, one shouldn't necessarily reject the multiverse idea on that ground (although I admit that it weakens the hypothesis, just as it does inflation). Secondly, as I said before, I think anthropic argument may work even when you don't understand the probability measure well enough to make accurate numerical predictions, although the latter would be a key element that would turn you from "a plausible-sounding metaphysical idea, which resolves a particular problem" into "a proper physical theory."

35. **Simeon Every**
August 7, 2015

The most contemporary developments in philosophy of science (that I am aware of) characterise research paradigms in terms of the extent to which theories in given paradigms produce testable predictions. This seems to be favouring methodological issues over canonical issues. Unfortunately there seems to be a prevailing mood amongst theoretical physicists (at least among those with the most public prominence) to declare philosophy irrelevant.

36. **anon**
August 7, 2015

Magnema: "…how do you get from ignorance (either of the microstate of a system or the self-locating ignorance in the multiverse) to probabilities? Yet we do like to think we have an idea of what's going on in statistical mechanics."

I submit that we have less of an idea of what is going on in Statistical Mechanics than we sometimes pretend to. Regarding the "Everything that is Allowed is Mandatory" problem, much of classical Statistical Mechanics take some form of ergodic hypothesis as a postulate, but this is mostly unproved and is often technically false (ergodicity often only holds only modulo a coarse-graining assumption). There are some rather interesting and complicated (even chaotic) classical systems that are not ergodic. See for example the Fermi–Pasta–Ulam problem. These may be the rule rather than the exception.

37. **Low Math, Meekly Interacting**
August 7, 2015

Simeon: I don't have much of an opinion on the relevance of philosophers, except to say a robust research program doesn't need them. I would furthermore posit if your research program could stand to benefit from an infusion of philosophical rigor, that's a sign of deep trouble. History ought to be an adequate guide, and it tells us science disconnected from observation is a dead-end path. There are precisely zero examples of true success in the absence of empirical evidence. There is precisely zero reason to think true success can be found in the absence of empirical evidence in the future. If quantum gravity and/or unification research cannot make contact with experiment, then they have no future as science, or should not, in my opinion. That's why the anthropic multiverse is anathema. It's an attempt at creating a loophole for a line of inquiry that may very well be hopeless. It's an excuse to keep working on it.

To my silly little mind, it really looks about that simple. I'm sure a philosopher could frame the position much better, but is there much added value? It's a question of whether to stop and focus on things one can ever test, or redefine "science" for oneself to avoid that requirement. What more is there to it, in actuality? Very little of true substance, it would appear.

38. **Scott Church**
August 7, 2015

Hi Magnema,

Beyond the untestability issues, the serious problems I was referring to were; 1) The philosophical problems with instantiating *actual* (as opposed to conceptual) infinite universes in the real world (e.g. "Hilbert's hotel", etc.); and 2) Physical difficulties with the formation of spacio-temporally infinite child universes in eternal inflationary scenarios (for instance, see Ellis & Stoeger (2009)). The former in particular is a hotly debated issue and both are off-topic here. My intention was simply to note them in passing.

As for inflation, I'm with you in that I also think its current fluid state, by itself, shouldn't lead us to reject Level I multiverses outright. I must admit, I have something of a love/hate relationship with inflation. On the one hand, it's a beautiful paradigm that solves multiple seemingly disparate fine-tuning issues in one fell swoop (e.g. flatness, CMB isotropy, etc.), and it is testable in the sense that specific characteristics of the CMB and model-specific predictions from tunneling and/or reheating scenarios allow us to rule out many inflationary models. It also seems to be the only viable option for bringing at least some multiverse scenarios into the realm of observational testability… *in principle,* that is. However, in practice it only pushes the fine-tuning problems back a level. And in the absence of any clear inflaton candidates it leaves us free to dream up any scalar field scenario we like (hence the fluidity). Yes, a conceptually valid physical mechanism is involved and those models can be tested, but the end result is little more than whack-a-mole. And to many folks the real appeal of Level I multiverses is that they open the door to possible anthropic solutions to fine-tuning problems. But I don't see how they can do this without the string landscape–all of which is true whether we're able to define valid probability measures on the outcome space or not…

Which sadly, brings us right back to square one… untestability. I must say, I was one of those who eagerly awaited the Planck results hoping for the "smoking gun" of primordial B-mode CMB polarizations. I was also one of those rejoicing when the BICEP2 results were first announced. But once those results started to unravel, it was disconcerting as hell to watch Linde and others furiously backpedal on B-modes as a smoking gun. "Geez," I remember thinking. "Steinhardt is right… they really *can* get whatever they damn well please out of this framework, can't they…?" I'm no expert on these matters, but I suspect that we aren't going to find a viable inflaton candidate or be able to meaningfully evaluate string theories without probing much closer to Planck scale energies. I don't see how this can ever be done without an LHC the diameter of the solar system (that has all the attendant luminosity problems solved as well). Even if it were possible to build and operate such a device, imagine what it would take to get anyone to fund it… especially if Republicans control Congress. (Sorry… I couldn't resist. 🙂)

It may not sound like it so far, but my real complaint isn't with inflation, multiverses, or even string/M-theory per se. It's with all the high priests of "post-empiricism" who expect us to flippantly dismiss all these concerns and line up behind them so they can redefine science as a beautiful, Utopian fairy tale land… with verdant landscapes and lush theoretical gumdrop trees… where gossamer winged math fairies grant every wish… where 72-virgin review committees publish every paper and dry every tear… and nothing as pedestrian as real-world *facts* are allowed to spoil anyone's party. Unfortunately, the trouble with fairy tales is that sooner or later one wakes up from them… and more often than not, with one helluva hangover and someone pressing charges. 😀

Best.

39. **Carlos Looney**
August 7, 2015

Neil: whether there are other universes out there in a multiverse with other "Peter Woits" saying different things depends upon your definition of a "Peter Woit." Do we mean just the DNA structure, what about environmentally induced alterations, mental content, or other variables?

According to some Physicists of the MWI persuasion, each "Peter Woit" is constantly splitting into two Peter Woits in two different worlds everytime a measurement is performed. Should we distinguish between these Peter Woits and what criteria? Are the "Peter Woits" we do not encounter real or unreal? Notice that the Peter Woits in the inflation scenario are real Peter Woits. But with MWI it is an open and probably never resolvable question.

40. **Peter Woit**
August 7, 2015

Actually I think the multiverse is an opportunity for philosophers of science to show that their subject is relevant and useful. There's a lot of material there to apply their best thinking about the demarcation problem to. Unfortunately I haven't seen much of this happening.

41. **Low Math, Meekly Interacting**
August 8, 2015

Peter: I'm probably too dismissive or pat in my thinking, but sometimes professional philosophizing seems like a very convoluted path to the obvious point. Book-length treatises which amount to the substance of clever aphorisms like "beautiful horses win races."

But you're probably right about programs that can keep moving the goalposts of testability due to an abundance of flexibility. It's one thing when a model's predictions are beyond the grasp of mere mortals to test. But what about those that could be, but also could be testable at, say, any energy between what the LHC can probe and the Plank scale? One can call it ugly, but if beautiful horses win races, proponents seem initially to be game for the challenge of science, and I can think of no iron-clad argument for denying their legitimacy at the outset. Of course, if the race is allowed to go on forever, that's a different story.

I.e., there are research programs that start out being arguably scientific, but evolve through repeated failures into something else. My challenge to the philosophers would be to give us a clearer answer to the demarcation problem, one that can tell us the right time to say enough's enough, and have everyone agree.

42. **Magnema**
August 8, 2015

@anon: Agreed on the lack of understanding of Stat Mech – the more I learn, the less I feel like I know about how it emerges. My point was more to make the analogy between established science (with, admittedly, poorly understood foundations) and multiverse-type arguments. I suppose I am assuming something analogous to the ergodicity in the multiverse case as well.

@Scott Church: I agree with everything you just said.

However, I note an issue here on Bayesian vs. Popperian science. In the former, which is usually regarded as the stronger theory, the CMB tests were taken as evidence, rather than a total disproof. It would seem that those individuals place a very high prior probability of inflation/string theory/related hypotheses, which means that, while the test should have been sufficient to bump an "uncertain" person into a "very certain" position, the converse is insufficient to bump a "very certain" position into anything less than a "moderately less certain" position. (E.g., suppose that they think: 100% chance of positive results meaning inflation, 99% negative results do; then, while they should reduce their confidence in their theory from, say, 99.999%, it's not going to be reduced too dramatically.) Now, whether or not their utter confidence in their theory is justified (I tend to think not) is a different concern, but it can still be scientific seeing as the prior probabilities have no scientific way (to my knowledge) of being determined…

43. **Jason**
August 9, 2015

Hey Peter,

I'm a former physicist; I got my PhD working on CMS before the LHC turned on, finishing in 2006. Although my research was focused on rare signature events, specifically Z' searches for indications of Super Symmetry, most of my colleagues and I thought Super Symmetry was probably made up non-sense. We all knew the reasons the theorists wanted it, but from an experimental perspective, it seemed a big stretch that there would be twice the number of particles, and not a single bit of evidence that they impact any Standard Model (SM) measurement. Wouldn't it be difficult to exclude the idea that those extra SM particles would have changed the result of any SM measurement going on already? There are many high precision measurements of the SM, surely something would have shown up by now, not just in the high-energy experiments?

I have been enjoying your blog for a while now. I don't really have a strong opinion one way or another about the multiverse. However, I do think your arguments are a bit more persuasive for me that what I've heard on the other side. I'm just wondering, if it were for the practice of debate, what would be your best defense of the multiverse? Or in other words, what would be your best arguments against yourself?

Thanks Again!

44. **adrian**
August 9, 2015

Dear Peter,
I have followed the discussion about the topic of the landscape in this blog post. Before, I asked you to qualify a statement you were quoted on—see message above, you did it. I probably disagree about the total lack of value of string theory as a theory of Everything, but the topic in itself [I mean TOE] interests me very little.

I wanted to comment something, probably a bit influenced by my daily read of the hep-th arXiv. As you may guess, I do work on string theory, publish regularly and never worked on 'the landscape'. I may be a bit unfair in the comment that follows.

It is my impression that most physicists or most string theorists are actually NOT working on landscape related topics. So, when people refer to the 'multi-verse mania', I find hard to understand what they are talking about. It seems to be something that most goes on the press than on the actual work of physicists/string theorists.

Let me take as an example the work of two physicists I know personally and have discussed at length with them: Matt Kleban and Ben Freivogel. They are both solid and serious physicists. It is a pleasure to discuss any particular problem with them, they will always make an interesting observation–probably also a correct one– and one always leaves enriched after a conversation about Physics with them.

Having written this, we open the spires page for their work. I will take them as representatives of physicists working on landscape related topics. Again, may be there are other representatives, but these two, at least, I know for a fact are serious and will not lie in a paper. As you can see, their work on the topic is not excessively cited by colleagues. It is just cited like any other set of papers of other colleagues. I mean, papers cited order 60 times since 2008 is not reflecting a real rush of activity, many of my more recent papers have more citations than that. Notice, this is NOT an evaluation of the quality of the papers of these two physicists. I -know-the papers are very good. This is just a reflection that there is not a lot of activity. It is not the case that large amounts of money in funding are deviated into that topic.

It is certainly NOT my intention in this comment to offend anyone, much less Kleban or Freivogel. I am just taking them as representative of a line of work [actually as serious physicists working on that line]. I do know that a very cited paper does not mean it is a good paper, but it reflects the level of activity on the topic, sometimes, it also reveals the paper is very good, but is not always the case. I know many papers that are very good and almost not cited.

Could you please let me know if my view of this topic is misguided? Let us also observe that of Susskind or Linde say 'multiverse' many times, this does not mean the topic has wide acceptance among the majority of working physicists. Does not mean either that big amounts of funding are assigned to that area. I may be wrong, could you please correct me? I do not have good data.

Having written this, I believe the criticism you voice on the topic is healthy.
I have the impression that you sometimes get carried away and involve the whole area of String Theory under the same criticism. This is pernicious. But it is your blog and are your opinions.

Thanks

45. **Peter Woit**
August 9, 2015

Jason,
I think your argument about why to not expect SUSY at the LHC (zero evidence, direct or indirect, for a single SUSY state, out of a large expected number) is one a lot of physicists shared. It's too bad that it rarely got made publicly.

I'm not much of a debater… The best argument the multiverse people have is the one for the CC. As for the landscape, you can try and argue that research should continue, absent a successful idea. All in all though, I think arguments for the multiverse are even weaker than arguments for string theory.

46. **Peter Woit**
August 9, 2015

Adrian,
You're right that most string theorists don't work on the multiverse, and pretty much do their best to try and ignore it. Many of them do however invoke it as an excuse when challenged about the failure of string theory TOE ideas to work out. Many of those promoting the multiverse are not string theorists (for example Carroll and Tegmark), but physicists who want to address "big ideas", and end up falling for something empty which seems to do this.

It is a fact though that the string theory landscape and the multiverse get a huge amount of public attention. Good string theorists may not take them seriously, but they're also not willing to go on record saying anything critical about the subject (there's a strong ethos of "string theory is under attack, so thou shall not criticize anything about string theory or the arguments being used to justify it"). By letting this go on and doing nothing, they are allowing some serious damage to be done to their field. There are a lot of people out there that the press and the public rightly turn to, and they're not stepping up to the job (put differently, why aren't there more like David Gross?). I confess to a certain level of annoyance about this, that I'm doing something unpleasant because the people whose job it is to deal with it don't want to.

47. **adrian**
August 10, 2015

Dear Peter,
I would like to briefly comment on your answer above. You stated
'Good string theorists may not take them seriously, but they're also not willing to go on record saying anything critical about the subject (there's a strong ethos of "string theory is under attack, so thou shall not criticize anything about string theory or the arguments being used to justify it") '

I have two observations:

1–that many string theorist do not go 'on record' with their opinion about the landscape and anthropic arguments because they have not worked on the area actively. It is difficult to have an opinion and record it somewhere if one have not contributed or deeply thought about the thing. Do you agree? In my case, I see anthropic arguments with some fascination-rejection (for example, I find the book by Barrow and Tipler beautiful to read). I see the landscape as a VERY interesting geometrical problem. But since I wrote no paper on any of those areas, all I can contribute are 'feelings'. Why should I go on record or on the press to say my 'feelings'?

2–The area of String Theory is, indeed and unfortunately, under attack. The attack that people who are NOT working in the area are expressing is sometimes short sighted, sometimes ill-intentioned and sometimes based on misconceptions. Sometimes it is fair criticism. This is a type of 'high-profile' attack that other areas of Physics [QCD, Collider Physics, Condensed Matter] do not suffer from–I mean press articles, books, blogs. Believe me that the most interesting criticism of the topic, I have heard from physicists working in the area. Indeed, I find that the fairest criticism is made by those who are actively working on the topic, in seminars and papers. Of course, some other people write press-articles "in favour" of String Theory that are pathetic to read.
But as I wrote some months ago in your blog's comments—that time about Amanda Peet's talk at Perimeter: the science is made in papers, seminars and conferences, not in blogs, not in press articles nor in popular talks.

thanks for the attention

48. **Lucy M**
August 10, 2015

Adrian "It is difficult to have an opinion and record it somewhere if one have not contributed or deeply thought about the thing. Do you agree?"

Yes…it's a fact of life that the people immersed in a theory understand by far the most. Would you agree that this is the same for every theory that has ever existed?

A serious problem of this, is that while people on the inside of a theory may be totally impartial, sometimes they are not. This is a serious problem that bedevilled knowledge before and outside the scientific revolution. Knowledge inevitably gave power to those who preached it.
Prediction is there among other reasons to prevent scientific incumbent knowledge from having to adopt new theories on the word of its insiders, and they from becoming philosopher-kings in the event their theory is adopted.

Can I just say that it is very clear that String theorists have had 'group shared experiences' that leave them with a shared sense of sureness Strings are the right way forward.
The problem with group experiences like this, is that the sense of sureness has no skeptic on the inside, at all. This means there is nothing to check excesses. The attempt to re-define Science without predictions, and with a set of traits exactly matching what string theorists think they can demonstrate, is an astonishing example of what can happen.
But also the quality of argument from string theorists….without internal criticism for standards, these arguments have become really superficial and liced with fallacious debating devices. Your most senior theorists – founders even – have come out with the most specious and frankly dishonest arguments.
I believe this is a direct consequence of a group of people succumbing to group shared experiences instead of treating such things as par for the course. Because they are. Everyone has them in the realm of their own theory.

49. **Lucy M**
August 10, 2015

Adrian "It is my impression that most physicists or most string theorists are actually NOT working on landscape related topics."

Yes but the REASON is that there literally are zero research directions, the multiverse is perfectly sterile.
What it doesn't mean is that most string theorists BELIEVE or SUPPORT the multiverse.
If they won't come out and make a firm break with their colleagues that do support the multiverse, then – again – we're being asked to trust the word of insiders bonded to each other by nebulas group shared 'experiences'.
Why aren't you giving your full details? Can you post a link to a website set up by dissenting string theory insiders?
I notice you use the subtly modified general vocabularly that is the hallmark of the 'shared group experiences'. For example the word 'beautiful' gets used rather a lot.
Another hallmark is more than a whiff of ego involving ideation not unlike those who have had religious experience.
Another hallmark is avoidance of fundamental criticism, and of the tactics of own side.
Apart from that you seem a very sweet person :O)

50. **Lucy M**
August 10, 2015

Peter Woit "Actually I think the multiverse is an opportunity for philosophers of science to show that their subject is relevant and useful. There's a lot of material there to apply their best thinking about the demarcation problem to. Unfortunately I haven't seen much of this happening."

the problem with deploying philosophy is that the very enactment of that effectively answers the central question that has been there from the start and still remains unanswered to this day. Is science unique, or is it a continuation of pre-scientific philosophy.

If science were unique philosophy would not be able to see that uniqueness. All it would see would be the elements in which science is like philosophy.

This happens to always be the conclusion of philosophy. The productivity gets played down, as well as every other unique feature. The special meaning of a whole range of words within science, are rejected out of hand by philosophers because they don't understand the word in the first place. Words become like they are outside science; just instances of more general effects. Nothing can be a special case because nothing ever was before science. Prediction is just a special case of criticism. Or one of many ways to determine the truth value of a theory. Consensus is about dogmatic authority. And so on. Just exactly what those words and the effects they describe intractably always were before science.
p.s. Popper on science that it had to be 'falsifiable' is a widespread misconception of the thrust of his work. In fact Popper thought science was an unprivileged domain of philosophy and Conjecture&Refutation would suffice there as everywhere else. In other words rational criticism replacing prediction.

51. **Peter Woit**
August 10, 2015

adrian,
I don't think the problems with the landscape require being an active researcher in the field to understand. The more general issue of what the problems are with string theory as a TOE and whether it is accurate to describe the situation there as "failure" are much more complex. I've wasted a lot of my life following that story since its beginnings when I was a graduate student, others quite sensibly may want to just ignore it.

If you make that decision though, I think you forfeit both the right to complain about people criticizing this failure, as well as the right to justify your research as related to a promising path to a TOE. I do see a lot of string theorists these days claiming the TOE problems have nothing to do with what they do, but then their grant proposals start with a reference to string theory as a TOE. They're also often unhappy about people criticizing string theory's failures, while at the same time unconcerned about dubious TOE claims being made by their colleagues.

52. **student**
August 11, 2015

One of the general "landscape" goals is to find "top-down" constraints on effective field theories (EFTs). These give constraints on the landscape. Quite remarkably string theory predicts constraints on EFTs that aren't immediately apparent from a purely EFT perspective. One example where string theory has provided insight into the structure of effective actions is dissipative relativistic fluid mechanics.

Another example where "top-down" constraints were found first from string theory is the classification of 6d (1,0) SCFTs. Recently the "landscape" of 6d (1,0) SCFTs has been nearly tamed into a complete classification. Of course this is a far ways away from the landscape problem being discussed here, but it seems quite plausible that many of the general lessons will apply.

The work on the landscape might be interesting or not, but without serious effort it would be difficult to say anything intelligent about a narrow sub-field. It is perfectly fine if this blog doesn't care to maintain such a standard, but it is completely unfair to criticize others for not going "on record" about papers they haven't studied in depth.

Hopefully the media will eventually follow the example of Quanta Magazine and there will be a much richer and diverse discussion of contemporary research.

53. **Peter Woit**
August 11, 2015

student,
I've no idea whether, as you claim, string theory provides insight into fluid mechanics. I do know though that the classification of 6d (1,0) models provides nothing at all in terms of the basic problem of getting a prediction about observable physics out of string theory.

This is a very good example of the problem: the question of what is known about supposed "string vacua" is an infinitely complex one, so people can attack anyone who points out the disastrous nature of the landscape problem as just insufficiently expert on the technicalities of "string vacua" research. By invoking irrelevant calculations you're just misleading people, claiming "progress" that isn't there, and doing your best to obfuscate the failure of the whole research program.

I agree that many of the writers at Quanta are doing a great job of covering diverse topics of current research, in particular by avoiding what the science media has done far too much of for 30 years, which is promoting misleading claims about string theory as a TOE (like the one you're trying to make…).

54. **student**
August 11, 2015

Peter,
The only claim that I made is that without serious effort, one can't say anything intelligent about a research program. I think your post illustrates this nicely. The classification of 6d (1,0) SCFTs might pave the way for classifying 4d N=1 SCFTs and eventually give constraints on the possible 4d SUSY quantum field theories (or their SUSY-breaking cousins) or it might not. Without hard work, it is hard to say if this approach will succeed or fail. Independently if this is interesting for the landscape or string theory as a TOE (which I am agnostic about contrary to claims otherwise) it is certainly interesting as a vast generalization of the geometric Langlands program.

55. **Peter Woit**
August 11, 2015

student,
I have no idea who you are or if you actually are a student, but if you are I'll just point out that you probably lack the experience to know what "serious effort" is relevant to understanding the issues behind getting the SM out of string theory. This is a problem that a huge number of smart people have worked on for over thirty years, and almost all of those people have decided to work on something else. Back in 1985 there were specific proposals of how to compactify string theory and get the SM. After much effort, there are now well-understood reasons why those proposals don't work. The history of the subject is that of continually finding new possible ways to construct "string vacua", getting farther and farther away from the goal of a predictive framework. The fundamental problem with "string phenomenology" is not just the current state of the subject (which is dismal), but the derivative: things just get worse and worse every year and this has been going on now for three decades. This is both at the theory and experimental level (no SUSY, no evidence for GUTs). If you had been following the subject since 1984 and paying attention to the results being claimed (as opposed to the technicalities of how they were achieved), I think you would understand a lot better the current situation, as well as how ridiculous are the claims you are making about classifying possible string vacua using 6d superconformal field theories. Far more promising ideas than that have foundered long ago on basic problems that such superconformal theories have nothing to say about. You can do anything you want and claim that "maybe a miracle will happen and those problems will go away", but that's wishful thinking, not science.

56. **student**
August 11, 2015

Peter,
Your reply has almost nothing to do with what I wrote. The only relevant statement you make

> how ridiculous are the claims you are making about classifying possible string vacua > using 6d superconformal field theories.

is manifestly false. The statement is that the classification of specific string vacua also classifies ALL 6d superconformal field theories. Again, I'm not saying anything about "string phenomenology" besides the fact that not having studied it, I can't say anything meaningful about it.

Do you still do research anymore? In thinking about prospective research topics, it seems that there are many fascinating areas of string theory that you completely ignore. However, despite your criticisms, you don't really offer any program or vision that a student could work on besides some notes summarizing some basic mathematical results from the 70's.

57. **Peter Woit**
August 11, 2015

student,
"I'm not saying anything about "string phenomenology" besides the fact that not having studied it, I can't say anything meaningful about it."
OK, this makes things clear. Obviously I was discussing string vacua possibly relevant to the real world, and you have no interest in that.
"some basic mathematical results from the 70s"
You're changing the subject, but that too makes some other things clear.

58. **Curious Mayhem**
August 14, 2015

It's scary to think about an infinite number of universes, each with a copy of Peter Woit saying something slightly different.

It's comforting to realize that this universe seems to instantiate at least a local maximum of verificationism in our incarnation of Peter.

Short Items

August 12, 2015
Categories: Multiverse Mania

A few short items:

- The New Yorker has its own coverage here of the NSA GenCyber summer camp program for children that was discussed here.
- The LHC is about to start doing physics again at 13 TeV, with beam intensity slowly ramping up in coming days and weeks. You can follow what's happening here.
- Some filmmakers are planning an IMAX film about the LHC, more information available here.
- Online media stories with skepticism about the multiverse continue to appear. The latest one is by Shannon Hall at Nautilus, with the title Is it Time to Embrace Unverified Theories? (I think it's a general rule that the answer to all questions in titles is No). I like one of the comments on the piece, arguing that some speculative physics is best thought of not as science or religion, but as a game.
- It's behind a paywall and I haven't seen the full story, but this week's New Scientist has a piece entitled What if .. Most of reality is hidden? A large amount of theoretical activity in recent decades has gone towards ways of figuring out how to hide new physics from any possible interaction with experiment. It seems this is another way of characterizing the problem discussed in the Nautilus article of unfalsifiable theories. Again, since it's the title of an article, the answer should be No.

Comments

1. **Stephen Olsen**
   August 12, 2015

   "(I think it's a general rule that the answer to all questions in titles is No.)"

   see: http://ccdb5fs.kek.jp/cgi-bin/img/allpdf?199007095

2. **Ben R**
   August 12, 2015

   The existence of that paper ("Is Hinchliffe's rule true?" by Boris Peon) does mean that Hinchliffe's rule (as defined in the abstract) can't be true, but the abstract is wrong to say "Hinchliffe's assertion is false […] only if it is true". It's false whether or not it's true.

3. **unification**
   August 13, 2015

   what about theories that could be verified in principal but the technology to verify them do not exist or are too expensive or too difficult ? i.e GUT scale physics

4. **Peter Woit**
   August 13, 2015

   unification,
   For the multiverse and string theory, the problem is not being falsifiable even in principle. For things like GUTs which are much more well-defined, there may be legitimate predictions, in principle testable (SUSY GUTs typically make proton decay predictions that could be feasibly testable).

   There is an obvious problem though with theories that claim some new physics at untestably high energy scales, when such theories don't explain anything about observable physics. My theory that the universe is made up of very small green turtles, turtles which appear to be pointlike at distance scales below the Planck scale, is in principle testable. That doesn't mean you should take it seriously as science…

5. **Michael**
   August 13, 2015

   In media, the principle is known as Betteridge's law:

   https://en.wikipedia.org/wiki/Betteridge%27s_law_of_headlines

   and a few amusing Twitter accounts apply it to the day's news:

   https://twitter.com/yourtitlesucks
   https://twitter.com/betteridgeslaw

6. **Jesper**
   August 13, 2015

   Perhaps I don't get it, but is it not a problem to assign a color to your turtles, when they are that small?

7. **Low Math, Meekly Interacting**
   August 13, 2015

   It's a metaphor for a new force, quantum chromophorodynamics.

8. **bb**
   August 13, 2015

   I was under the impression that GR received experimental validation well before the 60s (Mercury's perihelion, Eddington's measurement of the bending of light, …).

9. **Carl**
   August 13, 2015

   The New Scientist piece is a mix of stuff. The reference to string theory is around extra dimensions wrapped too tightly to be probed with foreseeable technologies, rather than any "landscape" stuff.

There's also some talk about bubble universes beyond our horizon, and the usual quantum Many Worlds stuff. It's all very muddled (as, in fact, is most of the larger article that this is a part of.)

10. **CU Phil**
August 13, 2015

Jasper,

The color is confined at longer distances for "in-shell" final states.

11. **Peter Woit**
August 13, 2015

Jesper,
The physical laws at the turtle scale are quite different, and if you create a macroscopic state of many turtles, it should interact with visible light such as to have a greenish appearance. Note that this theory is quite falsifiable: if you get a lot of these turtles together, shine light on them, and it looks blue, the theory is falsified.

Your comment does make clear that there remains lots of research to be done on turtle-TOE theory, it's a wide-open field…

12. **Nobody**
August 13, 2015

"turtle-TOE theory"

Call the media!

13. **Jon Lennox**
August 13, 2015

If a headline asks a question that isn't yes-or-no, the answer is ¯_(ツ)_/¯.

14. **Scott Church**
August 13, 2015

This and other threads of late have gotten me to thinking… It's no secret that we cannot test string/multiverse theory and will not be able to anytime soon. It's doubtful we'll be able to in our lifetimes. But… *what if we could?* Would anything be different? Suppose we actually *did* have a working solar system-sized collider or similar device that allowed us to probe Planck-scale distances and energies. This is all speculative of course, but it's reasonable to assume we'd find new particles and/or symmetries, a viable inflaton candidate perhaps, and more. What would we do with these discoveries? It seems to me that even if all this comes to pass we'll still be faced with a fundamental problem.

Historically, successful theories have been able to make predictions because they offered fundamental paradigm shifts that led to them via their formalism. General relativity predicted things like gravitational lensing and the perihelion shift of Mercury's orbit because it postulated a fundamentally different sort of space-time than classical mechanics did, and new field dynamics to go with it. The SM proposed actual underlying symmetries in the universe unlike those of its predecessors, and it was those that led to predictions of otherwise unexpected new particles, including the Higgs. All viable theories come in two parts: 1) A paradigm that proposes some new physical entity, state, or behavior; and 2) A mathematical framework that describes it. Successful theories are validated not by their ability to mathematically describe observation, but by *their ability to verify their underlying paradigms* within the frameworks they propose.

So here we are… our amazing super-duper-Planck-collider has scraped the universe right up to Planck scale energies and filled our databases with new particles, fields, etc. I suspect that *regardless of what we find,* all of that data will fit quite nicely with at least one possible string vacuum state. We will only have discovered which of the the 10^500 theoretically possible string vacua describes our universe. This raises a dilemma. For string/M-theory, the new paradigm is string/brane objects embedded in extra compactified dimensions. The mathematical framework that formalizes it has proven to be powerful but as we've seen, fluid enough to describe anything, including virtually anything our super-duper-Planck-collider manages to find. So the question is… *how does the paradigm get verified here?* How do we know that our "theory" of reality is an *explanation* of new physics in our universe and not simply an arcane but beautiful mathematical description of it?

The only way around this dilemma I can imagine would be to verify a multiverse… that is, to somehow directly observe other regions with different string vacuum states. But that *is* impossible, in principle as well as in practice. Unless I'm missing something, it seems to me that regardless of how elegant or workable the string/M-theory formalism is or how well it describes our observations, *if it does not provide a way to verify its underlying paradigm it is nothing more than an arcane but lovely mathematical framework*. It describes everything, but explains nothing… even if we do have Planck-scale observations.

Thoughts?

15. **David Metzler**
August 13, 2015

Peter, regarding the LHC, do you know of an easily accessible primer and/or glossary for the bewildering variety of acronyms and specialized accelerator physics notions that are tossed around, say at the morning meetings? I've become a bit of an LHC junky but there's a lot that goes on that's still over my head.

16. **Peter Woit**
August 13, 2015

David Metzler,
Sorry, maybe someone else can help. Some of this one can figure out with a bit of googling, but for some of it you probably need an expert to explain the significance of what they're doing. At least the bottom line is clear: they're back in business as of last night, now doing physics with 86 bunches in the beam. They will be increasing the number of bunches in the beam, trying to get up to something like 2800 bunches.

17. **Bill K**
August 13, 2015

"Peter, regarding the LHC, do you know of an easily accessible primer and/or glossary for the bewildering variety of acronyms and specialized accelerator physics notions that are tossed around, say at the morning meetings?"

Try http://lhccwg.web.cern.ch/lhccwg/Bibliography/UsefulAcronyms.htm

18. **Chris W.**

August 13, 2015

Scott Church,

That sounds like a very clear characterization of what Peter has always considered the problem to be—at least, leaving aside the practical computational difficulties of matching one or more vacuum states with the data.

The vacuum states become surrogates or “fall guys” for the underlying theory. This or that vacuum state may fail to match the data, but another will eventually work. The theory is never threatened, i.e., *by construction*, no data can contradict it.

Some people may thoughtlessly consider that to be a virtue—the ultimate hope for any theory…

19. **srp**
August 13, 2015

Now for a thought experiment: A string theorist, before the Ultra-Hyper-Mega accelerator goes online, comes up with a particular vacuum configuration (call it Turtle Conformal Theory) that produces the SM and also makes predictions for the Planck-scale phenomena. So long as that configuration were a) provably unique in producing the SM and b) made unique predictions at the Planck scale, then the UHM would provide a real test and would tend to confirm belief in TCT if its new particles and fields matched what the theory predicted. But if either a) or b) were not true, then the theory would probably still have lots of doubters.

20. **Chris W.**
August 14, 2015

srp,
The UHM would provide a real test of TCT, i.e., the *particular* vacuum configuration. It wouldn’t provide anything close to a test of the underlying theory, unless that theory implied that TCT was the only configuration that might work, i.e., that wasn’t already ruled out by established tests of the Standard Model.

If the latter was the case one could view the TCT as string theory combined with a correspondence principle, which eliminates the other (10^500?) conceivable vacuum configurations. On that basis one could consider the TCT as *the physical theory* being proposed, with string theory relegated to the status of a mathematical framework used in identifying this viable candidate. Of course, given the uniqueness of the TCT, if it fails the mathematical framework fails as the basis for as a fundamental theory. (It may still have many other uses in mathematical physics.)

Of course we’re nowhere close to arriving at this milestone. Also note that the idea of multiverse becomes basically irrelevant—just so much untestable metaphysical window dressing.

Aside from this, note that the initial arbitrariness of the vacuum configuration—before applying the correspondence principle—sounds a lot like the empirically determined and otherwise freely adjustable elements of the Standard Model we were hoping to eliminate. Achieving the milestone and passing the subsequent tests could be taken as evidence that the given arbitrariness is unavoidable, and reflects the choice of vacuum configuration.

That would be very interesting, but again, we’re nowhere close to that point, and we don’t really have a good idea how to get there.

Genius at Play

August 15, 2015
Categories: Book Reviews

A month or two ago I read the new biography of John Conway, Genius at Play, by Siobhan Roberts (whose book about Coxeter I reviewed here). Since then, writing about it has been on my to-do list, but I wasn't at all sure what to say. In today's Wall Street Journal Jordan Ellenberg has done a better job of this than I ever could, so I have a place to start: read Jordan's review.

Probably the first thing to say about the book is that it's an excellent portrayal of its subject, who is an unusual and well-known figure in the math community. Roberts spent a great deal of time with Conway, traveling with him and getting to know him rather well personally, then very ably turning that experience into a quite readable and enjoyable book. It's hard to imagine that a better biography of Conway would be possible.

In his review, Jordan crystallized precisely for me why I was having trouble writing about Conway and the book:

> Will you like this book? Here's a simple test. What's the rule that produces the sequence 1, 11, 21, 1211, 111221, 312211 . . . ?
>
> This is Mr. Conway's "look-and-say" sequence, so called because each number (after the first) is what you get when you look at the previous number and say it aloud: "one one; two ones; one two, one one; one one, one two, two ones . . ."
>
> If that makes you laugh with surprise, as it did me, you'll like Mr. Conway, and you'll like "Genius at Play." If not, you might want to quit here and go read something improving about the Greek debt crisis.

I'm afraid this didn't make me laugh with surprise; it seems that I'm immune to the charm of this sort of thing. While there was a lot of Conway's story I found interesting and which kept me avidly reading, his mathematical interests are very different than mine. Mathematical games make up a big part of his life and career, but the only aspect of this I've at any time found appealing was back in high school, when I remember writing a computer program to run the game of Life. I learned from the book that this is Conway's most famous creation, a fact he's not entirely happy with.

I also learned that my one personal experience with Conway is widely shared: at lunch with a group here at Columbia he mostly spent the time explaining how to calculate in one's head what day of the week any date is. Unfortunately I just didn't enjoy the idea of spending time on this then, and still don't.

Conway is one of the main figures responsible for an important piece of mathematics, discovering and working out the properties of some of the sporadic finite groups. This isn't something I've ever known much about, and I was quite interested to learn from the book some more about the subject and the history of how it came about.

I can't think of any other biography that I've read that gives such a vivid impression of its subject. In Conway's case this is somewhat of a mixed bag. He can be a very entertaining character, but his personal flaws are also apparent, with a suicide attempt and several failed marriages testifying to some real problems. Whenever books like this appear, I think one reaction of some mathematicians (not me…) is "is this good for the public portrayal of mathematics and mathematicians?". Conway's mixture of genius, highly accessible mathematical discoveries often related to games, and serious issues dealing with the outside world fit rather well with a certain caricature of what mathematicians are like. In my experience with great mathematicians, very few of them other than Conway fit the caricature. While any book about him would likely reinforce the caricature, at least this one gives a very well-written and comprehensive view of its subject.

Comments

1. **JSE**
   August 15, 2015

   It totally makes me laugh with surprise! I often teach that sequence to undergrads in discrete math; it really emphasizes the point that a sequence is an arbitrary list of numbers, generated however you please, not "a button on your calculator." On the other hand, I find the idea of computing days of week totally boring.

   Anyway, thanks for kind words. One thing I'm sad about is that I addressed the point you make: "Conway in many ways fits a public stereotype about mathematicians, but many of the qualities that match the stereotype actually make him highly ATYPICAL among real mathematicians" more explicitly, but some of it got cut for length, so I hope the point is still somewhat clear.

2. **KJ**
   August 15, 2015

   I wouldn't worry too much about this book propagating stereotypes. The set of people who 1) are not already interested in math 2) are the sort that is influenced by stereotypes and 3) decide to read this book has to be vanishingly small.

3. **Puffin**
   August 15, 2015

   Peter,
   What is your opinion of Conway & Kochen's Free Will Theorem (which I believe he was alternately referring to as his "Free Whim Theorem" a while back.) This seemed to me to be saying something surprisingly deep about the relationship of observers with the principles of QM and relativity. It's almost like having a theoretical bound on Bells inequalities if you believe observers can change their minds. I was surprised it didn't receive more attention.

4. **Richard Séguin**
   August 16, 2015

   Damn, JSE's review is behind a pay wall. It would have been nice to settle now with just that since I'm already off on a (ultra-)marathon read of something else and probably won't get to the book for quite some time. I, for one, found the answer to the sequence problem amusing.

5. **Chris Lott**
   August 16, 2015

   Richard, google "wsj a fellow of infinite jest" and click on the first link.

6. **Ben R**
   August 16, 2015

Puffin, I'm not PW but the "free will theorem" seems to be just a rebranded variant of Bell's theorem. Bell's setup was pretty awkward and I think Conway & Kochen's is better, but I don't think it says anything about free will that Bell didn't understand in 1964. Scott Aaronson seems to feel similarly (http://www.scottaaronson.com/democritus/lec18.html).

7. **Jeff M**
   August 16, 2015

   See and say is wonderful, even more so since Conway proved things about the sequence despite the fact that it has no "mathematical" definition. When I was in grad school Conway came and gave a talk about surreal numbers, which included his doing rope tricks to demonstrate the relation with knots. He's much more than games. As for Free Will, it's related to Bell's, but they are different as best I understand. One can explain Bell in various ways, some of which have nothing to do with free will. The free will theorem, as best I remember, says that if you want to throw out free will, you have to throw out lots of other stuff too.

8. **KenW**
   August 16, 2015

   That sequence reminds me of sudoku. Although I realize that you could play sudoku with mahjong tiles.

9. **Richard Gaylord**
   August 16, 2015

   i read the book, was not amused by the opening test and felt that while it might be an accurate portayal of Conway, it in no way, represents the way serious mathematicians work. I personally came away with the feeling that "it's too bad that an obviously brilliant person would fritter away his professional life working on amusing but trivial problems". i thought his own explanation of the history of the game of life which is referred to in the book (http://blog.tanyakhovanova.com/2010/07/the-sexual-side-of-life/) was fascinating. i can understand his conflicted attitude to GoL since he would prefer to be known for his mathematical work rather than for GoL, the profound implications of which in regards to emergent phenomena, he apparently missed, but according to his biographer, Conway would prefer to be known for anything than for nothing at all so he's come to terms with this notoriety . As for his free will theorem, it is vacuous and shows a basic lack of understanding of the philosophical nature of the issue (much as Wolfram's free will argument does). physicists and mathematicians trying to do philosophy are always amusing (we see this in the work of Tegmark, Deutsh, Dawid and others you have mentioned in your blog postings).

10. **John Baez**
    August 16, 2015

    What made me "laugh with surprise" is not that Conway *invented* the look-and-say sequence, it's that he showed the ratios of successive terms approach a constant 1.303577269034… which is the unique positive root of a polynomial of degree 71, and that he proved even deeper results about this idea.

    Anyone who thinks Conway has frittered away his time on trivial problems must not know the *Atlas of Finite Groups*, the book *Sphere Packings, Lattices and Groups*, and Conway's profound analysis of the Leech lattice and its symmetry groups. I think anyone who did those things has done enough hard work – and if they want to goof off and have some fun, that's fine with me.

11. **John Rennie**
    August 16, 2015

    If you're interested in a mildly technical history of the finite group work then I recommend Mark Ronan's book Symmetry and the Monster. This is a popular book not a maths book, but Ronan frequently slips and injects enough real maths to whet the appetite.

    Actually I have an idea I heard about the book on this site, though searching your site doesn't find anything. My apologies if I'm telling you something you already know.

12. **Richard Séguin**
    August 16, 2015

    Chris Lott: It doesn't depend on how you get to that page. If you're not a subscriber, WSJ throws up a paywall. At this point, it might already be too late to find this issue on a newstand somewhere.

13. **JSE**
    August 16, 2015

    Richard, I promise, it depends how you get to the page. I'm not a subscriber and when I get there through a Google search, I can see the whole thing. Otherwise, just the intro.

14. **Montague**
    August 16, 2015

    Man, Ellenberg really missed the mark with his account of the Look-and-say sequence. The definition of the sequence is a trifle–I mean it can be amusing that a young child might be able to guess how it works where a Fields medalist would be stuck, but that is not the beauty of the thing. The beauty of the thing is how Conway takes this trivial-seeming property which does not promise any mathematics at all, and spins from it a whole theory of "audioactive decay" involving a startling analogy with the periodic table. The mind that is moved to construct such things from a child's toything, alongside his deeper contributions to mathematics (see the Atlas of Finite Groups and the Sphere-packing book with Sloane) is a mind that I want to know more about!

15. **Anonyrat**
    August 16, 2015

    @John Baez, per Wiki, if L_n is the number of digits in the nth item in Conway's sequence, then Limit n -> infinity of L_n+1 / L_n = 1.303577269034….

    Also, was his proof lost?
    Proof of Conway's lost cosmological theorem
    Authors: Shalosh B. Ekhad and Doron Zeilberger
    Abstract: John Horton Conway's Cosmological Theorem about sequences like 1, 11, 21, 1211, 111221, 312211,…, for which no extant proof existed, is given a new proof, this time hopefully for good. – See more at: http://www.ams.org/journals/era/1997-03-11/S1079-6762-97-00026-7/home.html#sthash.txtWMezQ.dpuf

16. **Peter Lund**
    August 16, 2015

    Séguin: yes it does. Following the link gave me the paywall, going via google gave me the review with no paywall. This is my standard experience with WSJ.

17. **Richard Séguin**
August 16, 2015

Chris and Peter: Thanks! I originally did the search with DuckDuckGo, and I got the paywall. I switched to Google and got through. I'll remember this trick in the future.

18. **Cosmonut**
August 16, 2015

Must agree with the comment about caricature mathematician.

I've often found that while theoretical physicists are seen as "Brilliant minds unlocking the secrets of the Universe", the perception of mathematicians is "Borderline nutcases obsessing over arcane stuff".

19. **John Baez**
August 16, 2015

Anonyrat wrote: "if L_n is the number of digits in the nth item in Conway's sequence, then Limit n -> infinity of L_n+1 / L_n = 1.303577269034…."

Right, sorry: I said "ratios of successive terms", but it's "ratios of successive numbers of digits".

"Also, was his proof lost?"

Not of this fact, but of a stronger fact of which this is a corollary: the Cosmological Theorem. The paper you mention is quite fun, and it adds to the reputation of mathematicians as borderline nutcases obsessing over arcane stuff. First, one of the authors, Shalosh B. Ekhad, is actually a computer. Second, the abstract says:

Abstract: John Horton Conway's Cosmological Theorem about sequences like 1, 11, 21, 1211, 111221, 312211,…, for which no extant proof existed, is given a new proof, this time hopefully for good.

Third, it ends with a rant on the four-color theorem, titled "On *a posteriori* trivial theorems: The ultimate proof of the Four-Color Theorem should emulate our proof".

It explains the lost-and-found story:

> **The Cosmological Theorem.** There exists an integer N such that every string decays in at most N days to a compound of common and transuranic elements.
>
> Conway stated that two independent proofs *used* to exist, one by himself and Richard Parker (that only proved that N existed), and another one by Mike Guy (that actually proved that one may take N = 24, and that it was best possible). Unfortunately both proofs were lost. Here we announce a new proof (which establishes that one may take N = 29; with more computations one should be able to rederive (or else refute) Guy's sharp N = 24).

20. **David Roberts**
August 17, 2015

@John

DZ goes on to write:

"eventually one should be able to type Prove4CT();, and the truth of the theorem should be implied by the halting of the program. In order to check the validity, the checker would not need to see any specific configuration. Everything should be done internally and silently by the computer. All that the checker would have to do is check the program."

and this is now the case! Gonthier and Werner have of course completely formalised the proof. (for those who may not know: http://www.ams.org/notices/200811/tx081101382p.pdf)

21. **Peter Woit**
August 17, 2015

It does seem that the WSJ prefers Google to my blog: if you access the same URL via Google as the one in the link in the posting you get the full review.

Puffin,
I confess to have pretty much paid no attention to the "free-will theorem". It starts from assumptions that ignore anything interesting about the measurement problem, and claims to reach conclusions about something that to me is a question about consciousness and how brains work. As far as I can tell, Conway knows little about either physics or neurobiology. Maybe there's something interesting going on in this "theorem", but I'm happy to let others be the ones to spend their time on it.

22. **Martin S.**
August 17, 2015

Can anyone hint on the experimental setup of the spin-component-square measurements of the Free will theorem? I am into the basic physics they use, not the overall theorem herein.
Regarding their spin axiom (on measuring squares of spin components), they cite several possibilities for such measurements at their article, endnote 1, page 27.
I have looked at the Kochen/Specker work that is one of those cited possibilities. It seems that it is based on spectra measurements (of systems where Hamiltonian contains spin-based parts that alter the base Hamiltonian); see part 4 there, pages 13-16 (71-74 in journal page numbering).
If I get it correctly, the Free will theorem takes entangled systems and looks at corresponding measurements. Does it mean that measurements of spectra on one of those entangled components can affect whether the other (entangled) component has (or does not have) its base Hamiltonian altered by the spin-based interactions?
May be it is different for molecular beams that they cite as another possibility for the measurements. Yet when I looked at work of Erwin Wrede (that is cited by them), I have not seen anything on Hamiltonians or spin there; not being surprised, since that work of Wrede is from 1927.
They mention interferometry with crafted coherent beams (see the endnote 1 at their article) too, but they do not go into details there.

23. **Douglas Natelson**
August 17, 2015

About "lost" proofs: This is the second "lost" proof story I've heard about Conway. Manjul Bhargava told a story at a talk I'd attended that Conway had also lost his original proof of the 15 Theorem. . Are "lost" proofs common among practicing mathematicians? Is Conway particularly disorganized?

24. **Peter O.**
August 18, 2015

Yes, common. If a proof reduces to checking six hundred trivial cases, one can sit down and check them. At the end one has proved to oneself that a certain statement is true, and can tell it everybody else. The proof is an unpublishable pile of waste paper, but others will find nicer proofs, or program a computer to check these cases.

25. **Shantanu**
August 18, 2015

Peter, OT to this, but some sad news, in case you were unaware.
Jacob Bekenstein passed away yesterday.

26. **Chris W.**
August 18, 2015

Concerning Jacob Bekenstein, Jennifer Ouellette posted [this scientific obituary] on her SciAm blog yesterday, and Scott Aaronson [posted a personal tribute] today.

27. **Jeff M**
August 18, 2015

Douglas

Yes, Conway is particularly disorganized. And I don't believe his lost proofs were the "check 600 cases" types, though those proofs certainly exist. Mathematicians prove all sorts of things, and sometimes it isn't even that the proofs are lost, it's just that the result is not interesting. BTW, I know Conway is disorganized since when he was giving the talk on surreal numbers I mentioned before, someone had to drive to Princeton from NYC to make sure he would show up, and he had in fact completely forgotten he was supposed to give a talk 🙂

28. **Erraticon**
August 19, 2015

Interesting how people easily forgive a famous person his/her unpleasant excesses clearly caused by a personality disorder (most likely bordeline in this case).

29. **Neil**
August 19, 2015

Well, that did make me smile, if not laugh. I plugged it into the online encyclopedia of integer sequences (https://oeis.org) and sure enough it came up, along with a few interesting tidbits (no integer larger than 3 appears, and 333 or longer does not appear, although 222 does). But the cosmological thing–I don't know.

30. **Joseph O'Rourke**
August 21, 2015

One minor correction that may be implied by John Baez's post ("…Conway invented the look-and-say sequence…"): Conway did not invent the sequence. The biography says it originated in a Math Olympiad in Belgrade.

Lepton Photon Interactions at High Energies and the Multiverse

August 22, 2015
Categories: Experimental HEP News, Multiverse Mania

This past week the large biennial "Lepton-Photon" (International Symposium on Lepton Photon Interactions at High Energies) conference has been taking place in Ljubljana. These have been going on since 1965, now alternating years with the ICHEP ("Rochester") conference. It's been quite a while since the main topic of the conference was lepton-photon interactions, these days it covers the entire field of HEP, with a format of plenary survey talks. Taking a look at the slides will give you an excellent survey of what is going on in HEP, both in experiment and theory.

One newsworthy talk was Mike Lamont's on the current state of the LHC Run 2. The new LHC run is at 13 TeV collision energy, with 25ns bunch spacing (as opposed to 8 TeV, 50ns in Run 1). No showstopper problems so far, but increasing the luminosity (by increasing the number of bunches) has been a slow process. As a result, Lamont expects only about 3.4 inverse fb luminosity this year, down from early hopes for a number more like 10 inverse fb. The plan is for 30 inverse fb next year.

The summary talk by John Ellis featured undimmed enthusiasm for SUSY. What will be very interesting to see will be the treatment of this subject at the next Lepton-Photon conference.

One subject that was not mentioned at all in Ellis's talk, and, as far as I could tell, not in any of the other ones either, was the multiverse. The organizers and speakers seem to all realize that there's no scientific content to this idea worth discussing, so best to ignore it. I'm completely mystified though by the decision to have as public outreach a promotional talk about the multiverse by Alan Guth. Why anybody in HEP thinks it's a good idea to make pseudo-science the public face of the subject just baffles me. If you want to see one reason why this kind of thing is really a bad idea and doing great damage to the public perception and understanding of the subject, take a look at this.

Away from serious scientific conferences, the multiverse continues to dominate the media's coverage of fundamental physics. The usually sober publication The Economist features both an article and a video this week promoting multiverse pseudo-science. As usual in such pieces, no skeptical voices are to be heard. Susskind deals straightforwardly with the lack of scientific evidence problem by simply saying things that aren't true:

> This idea of a multiverse is not gratuitous speculation. No, it really comes out of both experiment or observational physics about the universe and the current theories as best we understand them.

He doesn't explain what the experimental evidence for the multiverse is. The BICEP2 observation of primordial gravitational waves perhaps?

Comments

1. **andrew**
   August 22, 2015

   Useful links. I still believe in SUSY and that naturalness is important. I concede though that an alternative narrative – that naturalness and SUSY were follies, followed by a generation of physicists but scorned by nature in experiments for years – is gaining traction.

   Does that make you hopeful regarding the perception of ST? Could the dominant narrative – the world's geniuses following in the footsteps of legendary physicists, uncovering bizarre secrets of the multiverse at elite institutions – come under pressure? Is it plausible that, like SUSY and naturalness, we'll come to see ST as smart people getting it wrong?

   The thing with narratives is that isn't events per se but our perception of events that matter. Although LHC says nothing about ST, one perception is that theoretical speculation was wrong and that we need new ideas. Ultimately that might shift our feelings about theoretical physics including ST in the last 30 years or so. Do you think we're any closer to that point?

   I'm aware that this a contentious view on the development of scientific ideas, but I'm not looking for a discussion on that.

2. **Ilya**
   August 23, 2015

   I don't think it's fair to say that Susskind lies, just from that quote. He doesn't claim that evidences exists, he just writes "comes out of", which is more like "is hinted by". He doesn't write that it's not speculation (it is), he says that it's not *gratuitous* speculation – that's debatable but not plainly false.

3. **Peter Woit**
   August 23, 2015

   Ilya,
   I don't think that
   "This idea of a multiverse … really comes out of … experiment or observational physics."
   can be characterized otherwise than as a statement that is not true. The weasel-word reformulation you suggest (a "hint" can mean any degree of evidence, no matter how negligible) is different.

   Note that I was careful to say that Susskind was saying something untrue, but did not say that he was lying, which would imply that he was intentionally saying something he knew to be untrue. Quite likely he believes what he said. I don't think multiverse-maniacs are liars. I think that they have for various reasons convinced themselves of bad arguments, which they unfortunately are very actively trying to promote to their colleagues and the public.

4. **Cosmonut**
   August 23, 2015

   Peter,
   I've read your criticisms of multiverse mania for a while now and I agree with you.

   But is there any push back within the scientific community against this rubbish ?

   Or is the pro-multiverse camp winning by virtue of constant repetition ?

   It seems every time I read a popular article about fundamental physics in the media, its the same old bilge about string theory, inflation and multiverse – or even worse, the "Tegmarkian hierarchy of multiverses".

No genuine new understanding of fundamental physics and cosmology over the last 10 years or so, as far as I can tell.

5. **phil fogle**
August 24, 2015

I agree that Susskind is (probably) sincere but (likely) delusional. It's not at all uncommon for good scientists to 'believe' in some of their speculations – subjectively confusing 'sufficient' for 'necessary'.

The Spectator review is in another category (parallel universe?) all together, and is an utter disgrace. But it's the entertainment industry, on the level of mesmerism and radium therapies, and will always be there as a background pollutant.

6. **verruckte**
August 24, 2015

The reason that the multiverse narrative keeps getting pushed is that it's what the public wants to hear. Media picks up on it, because it sounds mystical and exciting. Saying "no, nothing new here yet, we are still just stuck with the standard model" is NOT mystical and exciting. It sells no books, attracts no eyeballs to advertising, and is basically just boring.
The intoxication of popular interest and the financial opportunities thereof are tempting. The fact that the subject matter is not able to be tested is *not* a disadvantage, it is a huge advantage, for the simple reason that no one can ever actually show you are wrong. This plus the mantle of authority given by being an ACTUAL SCIENTIST are the perfect recipe to sell books and headlines and expensive talks. I believe, as Peter seems to, that most proponents have probably convinced themselves first of all. The capacity of humans to adjust reality to match desires can not be overestimated.

7. **Shantanu**
August 24, 2015

Peter, I think the problem lies with the organizers and also the audience.
Hope at least some brave member asked some provocative question.

Peter: an OT question prompted by the recent passing away of Bekenstein. When you were an undergrad student at Harvard, were you impressed by Hawking's (and related work by others) 1974 work on the black holes have a temperature and radiate? Was this extensively discussed in the hallways at Harvard?

Or is it that in Harvard at that time, no one cared about GR and you don't recall any discussions of this

8. **Peter Woit**
August 24, 2015

Shantanu,
I've never met Bekenstein. When I was at Harvard (1975-79), the focus was on the SM, few people were thinking about quantum gravity. At Princeton (1979-84) there was a lot of interest in quantum gravity, especially in Hawking's semiclassical Euclidean methods, which were closely related to the semiclassical gauge theory methods a lot of people there had been working on. Malcolm Perry, Hawking's student, was a young faculty member and Hawking visited and gave lectures.

9. **Dave Miller in Sacramento**
August 24, 2015

Peter,

Alan Guth is, of course, one of the inventors of inflation: I knew Alan, very slightly, when he did his initial work on inflation at SLAC back around 1980. So, doesn't the inflationary multiverse predate the "landscape scenario" by a couple decades or so? Is there any reason for the two to be connected?

I know there is an ongoing confusion among the "many-worlds" interpretation of quantum mechanics, the landscape scenario, and the inflationary multiverse. Is there any "accepted" connection among these, or is it just a free-for-all open to anyone's speculation?

By the way, I would interpret Lenny's "comes out of" as just meaning that Lenny thinks the ideas are suggested or motivated by observation or experiment, not that there is any firm connection.

All the best,

Dave

10. **Dave Miller in Sacramento**
August 24, 2015

Shantanu,

To elaborate on your question to Peter: I was an undergrad at Caltech when Hawking gave a talk on his recent work on black-hole evaporation — I think it was in '74: I had enough sense to keep my copy of the typewritten paper that was handed out to the audience (although I looked at the paper a few months ago and still cannot understand his notation). I do recall that we all understood enough to grasp that Hawking was saying that black holes evaporate and therefore do not last forever; however, even when I took Kip Thorne's class on GR a year or so later, I do not recall any discussion of Hawking evaporation in class (Kip was obsessed with accretion disks around black holes at the time — we students were not so obsessed).

At SLAC around 1980, I remember a casual discussion with a post-doc, Subhash Gupta, about the issue: we were specifically interested in the point that, prima facie, Hawking evaporation means the event horizon never actually forms. Nothing came of that discussion and no one else at SLAC seemed interested. (If Subhash and I had only known, we could have ignited the discussion about the "firewall" thirty years earlier!)

Hope this gives you a bit more info as to how it was viewed back then.

Dave

11. **Peter Woit**
August 24, 2015

Dave,
I think claiming that the multiverse is "suggested or motivated by observation or experiment" is also an untrue statement.

A small number of people (Susskind among them) have been known to speculate about a connection between MWI and the inflationary multiverse. I've yet to see a comprehensible explanation of how that is supposed to work. Popular press articles often conflate the two, as far as I can tell purely because the writer doesn't understand what they're writing about.

Multiverse fans often claim that "typical" inflationary models imply eternal inflation and thus a multiverse (while not mentioning that in such models physics will be the same in all universes). Others claim that there are plenty of inflationary models that don't imply eternal inflation. The big problem with inflation right now (and I think this is a point Steinhardt emphasizes) is that you can get pretty much whatever you want out some model in the framework, making it of dubious testability.

12. **Dave Miller in Sacramento**
    August 25, 2015

    Peter,

    You wrote:
    >Multiverse fans often claim that "typical" inflationary models imply eternal inflation and thus a multiverse (while not mentioning that in such models physics will be the same in all universes).

    I recall that Alan claimed he had a proof that inflation that was eternal in the past direction was not possible, though I myself could never understood the details: I think I ran across a reference to it in Alan's book.

    In fact, this paper (http://arxiv.org/pdf/hep-th/0702178.pdf) seems to make that point (from the abstract: "Although inflation is generically eternal into the future, it is not eternal into the past: it can be proven under reasonable assumptions that the inflating region must be incomplete in past directions, so some physics other than inflation is needed to describe the past boundary of the inflating region"), though I do not believe this is where I first saw it.

    The paper also does have a brief discussion of the landscape (p.9-11), but it is frustratingly vague: Alan indicates a bit of skepticism, but, while he suggests that you can get different physics in different universes (at least different cosmological constants), no details are given of how that could work.

    Anyone know where anyone has actually talked about that in any detail? I fear I am still confused.

    It will be interesting to hear what Alan has to say in his talk.

    Dave

13. **Shantanu**
    August 25, 2015

    Thanks, Dave. However Caltech had a strong GR group then (in fact still does).
    and 70s was the golden age of GR. As Peter mentioned, very few people in Harvard were interested in quantum gravity.

14. **Chris W.**
    August 25, 2015

    Speaking of Hawking and black hole evaporation, he is in the news again with another (?) take on the problem:

    > The [Wall Street Journal] notes that Gerard 't Hooft of Utrecht University, winner of the 1999 Nobel Prize in physics who was present at the conference, had presented a similar idea in 1996.
    >
    > He said the problem was that his math did not add up.
    >
    > "I claim he is now where I was 20 years ago," he told the Journal. "If he announces this as a new idea, I won't be thrilled."

15. **Peter Woit**
    August 25, 2015

    Chris W.,
    I'm not convinced that what the world needs is more unclear speculative claims about the black hole information paradox, even if they're coming from Hawking. I have no idea what's going on here, urge those interested to try Sabine Hossenfelder, who wrote about the talk here:
    http://backreaction.blogspot.com/2015/08/hawking-proposes-new-idea-for-how.html

    Personally I'll wait for the paper with Perry promised for next month and see if I can make any sense of that. Maybe before then the experts on this will have come to some consensus on whether it's worth paying attention to.

16. **Chris W.**
    August 26, 2015

    I'm not convinced of that either, which is partly why I excerpted the comment from 't Hooft. Pushing out preliminary conclusions and arguments to make a conference deadline doesn't help.

17. **Curious Mayhem**
    August 27, 2015

    Even if primordial gravitational waves are detected (which I think will happen soon), it's no evidence for the multiverse. The multiverse theory is constructed in such a way as to not require such evidence, or any evidence for that matter.

    >(:-P

A Singularly Unfeminine Profession

August 23, 2015
Categories: Book Reviews

Phenomenologist Mary K. Gaillard has recently published an autobiographical memoir, with the title A Singularly Unfeminine Profession, and last week's Nature has a detailed review.

Gaillard is a very distinguished HEP phenomenologist, with a career that began in the 1960s, taking her in 1981 to a professorship at Berkeley, from which she is now retired. She has been married to two other physicists, Jean-Marc Gaillard and Bruno Zumino.

One highlight of her career is her work on charmed particles, which included an accurate prediction of the charmed quark mass (1.5 GeV, in this paper with Ben Lee, see page 905). The prediction came in a paper published in mid-1974, months before the discovery of the J/Psi in November. Unfortunately she and Lee didn't have the courage to put the prediction in the abstract, which just said "the average mass of charmed pseudoscalar states lies below 10 GeV".

Wikipedia also credits her (with Chanowitz and Ellis) with a prediction of the b-quark mass. Maybe I'm missing something here, but this appears to be much less justifiable, since the paper was based on an SU(5) model which is known not to work. It's also a much vaguer prediction, and appears in the abstract in a mistaken form. In the book, Gaillard tells the story:

> We were correcting the proofs for the published version of the paper in July, at around the same time I went to pick up Leon Lederman at the Geneva airport, and, through a screen near the baggage claim gate, he handed me a beautiful histogram showing clear evidence for a b-bbar spin-one bound state – named Upsilon by its discoverers – with a mass of about 10 billion electron volts, in other words, evidence for a bottom quark with a mass of about five billion electron volts. John quickly penciled in a correction to the abstract with our more precise prediction, but his handwriting was so bad the "to" was read as "60", and our prediction came out in print as
>
> $m_b/m_\tau=2605$
>
> implying a b-quark mass of over 5000 billion electron volts.

The upsilon discovery was announced publicly at a press conference only later, in August. I can't help noticing that it seems that back in 1977 discussing results of an HEP experiment before the press conference wasn't unusual. It is only more recently that one hears that to do this is to subvert the scientific process.

Among the many other things I learned from the book was the origin of John Hagelin and the Maharishi's posters explaining that N=8 supergravity was the TOE fitting together with the Maharishi's ideas. Hagelin was dating Gaillard's cousin and learned about the N=8 story from Gaillard.

The latter part of her career focused first on supergravity, then in 1985 on superstring phenomenology. Thirty years later she's sill working on much the same idea as in 1985 (see here). The book explains the idea of string theory unification using a compactification, but doesn't reflect on the question of when or whether it might be a good idea to finally give up on this.

A major theme of the book is that of how her gender has affected her career, including more discussion of the details of her employment and job offers than would be usual in a book of this kind. It's a complex story, with the details of it well worth paying attention to for anyone interested in the problems women encounter in science. Gaillard started out her career facing serious obstacles as a woman, but later on achieved a large degree of professional success. She has a lot to say about the attitudes and remarks she ran into from men along the way, often from ones who were close friends.

She is most critical of the CERN theory group, which she left in 1981 after being turned down for a senior staff position (at a time she had job offers from Berkeley and Femilab). To this day, as far as I can tell, CERN-TH has no women as permanent scientific staff, and only one (out of 19) female staff members. Perhaps things will change with the incoming CERN director…

Comments

1. **David Nataf**
   August 24, 2015

   I'm just posting to second the comment made by Peter: the book review at Nature.com is very good, very informative and well-written.

2. **Shantanu**
   August 24, 2015

   Peter, who is the new CERN director?
   (sorry I don't know)

3. **Peter Woit**
   August 24, 2015

   Shantanu,
   Fabiola Gianotti will become CERN DG on Jan. 1.

4. **Jr**
   August 25, 2015

   There seems to be a word or two missing in the following sentence.

   "The upsilon discovery was announced publicly at a press conference in August, and I can't help noticing that it seems that back in 1977 the LHC-era idea that saying anything about results to those outside the experiment pre-press conference hadn't yet taken hold."

5. **Peter Woit**
   August 25, 2015

   Jr.,
   I think the sentence is correct, if not very well worded, I'll do some editing.

One problem with it is that I was implicitly referring to something many people might not be aware of, the criticism of this blog for discussing the Higgs discovery results before the press conference (I was "subverting the scientific process"), see
http://profmattstrassler.com/2012/06/18/new-higgs-rumors-have-arrived/

6. **sm**
August 25, 2015

jr,

Perhaps also missing a comma to help with the parsing.

Peter,

Who did get senior staff Cern positions in and around 1981? This might help clarify whether or not the innuendo is justified.

7. **Peter Woit**
August 25, 2015

sm,
I personally know very little about the people involved, or what was going on at CERN-TH around that time. Sergio Ferrara's online bio says "In 1981, he joined as a staff member the Theory Division at CERN." Maybe he was chosen over Gaillard. (*Note added: this is not the case. Ferrara was hired in 1981 to a junior position that had nothing to do with the Gaillard story*). In any case, I think only those actually involved in the decision likely know what considerations were behind it and why they did not choose Gaillard.

One interesting thing is that, looking at the CERN-TH archive page here
http://library.web.cern.ch/archives/CERN_archive/guide/theory/isath
it seems that this was so long ago (more than 30 years) that all the records can be consulted. If anyone at CERN wants to go take a look, perhaps they can report back…

What is clear is that Gaillard is still pretty unhappy about this. She had been there for quite a while, was professionally very successful, and had job offers from Berkeley and Fermilab. Turning someone down in such a situation is pretty much tantamount to telling them you'd be happy if they left. I find it hard to believe that losing both her and Zumino is not something they came to regret.

There's always far more in such decisions than the gender issue, but to me Gaillard's discussion of how it affected her professional life seems like a reasonable one, well worth reading. She doesn't seem like someone quick to take offense at the inconsequential.

8. **sm**
August 25, 2015

Peter,
What a remarkable data source you cite. Thank you. Have you thought of sharing your insights into data mining with that community?

You're surely right that when it comes to job offers at places like Cern there is more to the decision than gender (or spousal relations for that matter – poor Zumino, Cern forcing him to choose between his wife and a deep collaborator!). This is evidenced by the Cornell job offer to the non-publishing Wilson, which could have seemed outrageous at the time, even though he presumably conformed to the gender stereotype of those days.

9. **Piscator**
August 25, 2015

Could it be as simple as the fact that Mary K is not European? It has certainly been said that CERN has, at least informally, nationality quotas for the staff positions in the theory group, based on the nations that fund it. I don't know anything definite about the politics as far back as 1981, but I wouldn't be at all surprised to learn that there was a political expectation that the permanent staff be from the CERN member states.

10. **Peter Woit**
August 25, 2015

Piscator,
I doubt that was it. Gaillard got her degree from Orsay, and by 1981 had been in France, married to a Frenchman, for nearly 20 years. I don't see how that could have been the problem, and I don't think she mentions it as an issue in the book.

11. **Radioactive**
August 26, 2015

It's a bit tedious to wade through all this 35 year old gossip, and I'm sure there was and still is sexism at CERN, but it sounds just as likely that whoever was in charge didn't want to have to employ this woman as well as her ex- and new husband, which would be quite disruptive.

12. **Peter Woit**
September 3, 2015

It turns out that Ferrara was hired as a junior staff member in 1981, which had nothing to do with the issue of a senior appointment that year that Gaillard discusses in her book. I've thus deleted several comments that discuss him in this context.

This Week's Hype

August 28, 2015
Categories: This Week's Hype

Bogus media stories about how "physicists finally find a way to test string theory" have now been with us for decades, with a large number of them documented here. Recently this phenomenon seemed to finally be dying down, with such stories the province of more obscure media outlets and the press offices of not very well-known institutions. Yesterday though saw a new example of the genre, coming to us from IAS faculty and the Fermilab/SLAC publication *Symmetry*, which announces that Theorists from the Institute for Advanced Study have proposed a way forward in the quest to test string theory.

The source of all this is the Arkani-Hamed/Maldacena paper Cosmological Collider Physics from earlier this year. As usual with a lot of these bogus stories about "testing string theory", the work in question actually has nothing to do with string theory. It's about possible ways to look for particle physics effects in subtle effects in non-Gaussianities in the CMB. This is a theoretically interesting topic, but suffers from the obvious problem that, experimentally, there are no non-Gaussianities in the CMB. The limits on non-Gaussianity from Planck and other CMB experiments are quite strong.

The connection to string theory is given in the article as:

> But scientists are working out ways that experiments could at least begin to test parts of string theory. One prediction that string theory makes is the existence of particles with a unique property: a spin of greater than two.

This is of course complete nonsense, since there are plenty of known particles of spin greater than two. String theory arose as an attempt to explain some of these, but it turned out that it didn't work, the actual explanation was QCD, a quantum field theory. The author seems to have gotten this argument from the following statement in the Arkani-Hamed/Maldacena paper:

> Of course, if we were to detect the contribution of a spin 4 state in the non-gaussianity, it would be a strong indication of string theory during inflation, since we suspect that a structure like string theory follows when we have weakly interacting particles with spin s greater than 2.

Knowing that there is a spin 4 state up at the inflation or Planck scale would of course be quite interesting, but I don't see any reason to believe that effective field theory would apply to it or that this would "of course" "be a strong indication of string theory". This argument would actually make better sense at lower energy. I suppose you could claim that lots of work being done at the LHC is "a way forward in the quest to test string theory", since any day it could lead to evidence for a new weakly interacting spin 4 state. That would of course be pretty silly, but less silly than this article.

Combined with the bogus "test", the article includes a large helping of the usual promotional material, ending with a section on "The value of strings". We're told that

> Witten and others believe that such successes in other fields indicate that string theory actually underlies all other theories at some deeper level.

"All other theories"???

Update: I should make clear that my comment about the strength of the limits on non-Gaussianities was about the quality of the experimental results, and my impression that there are not near-term prospects for doing much better. Depending on what models one is talking about, such results are often not strong constraints. A correspondent suggest this source for more information about all of this.

Comments

1. **M**
   August 28, 2015

   I discovered a spin 4 particle:
   http://pdg.lbl.gov/2014/listings/rpp2014-list-a4-2040.pdf

2. **Nathalie**
   August 28, 2015

   We are indeed living in the golden age of bogus speculations presented as facts.
   Here is another example from a recent talk by Hawking, taken from

   https://www.kth.se/blogs/stockholmtech/2015/08/hawking-proposes-way-for-information-to-escape-destruction-in-black-hole/

   "The existence of alternative histories with black holes suggests this might be possible," Hawking said. "The hole would need to be large and if it was rotating it might have a passage to another universe. But you couldn't come back to our universe."

3. **Shantanu**
   August 28, 2015

   Peter nice article. I sincerely hope the author of this article reads your critique
   and wish journalists for such magazines are more responsible.

4. **KJ**
   August 28, 2015

   Note that the author's comment that string theory "underlies all other theories" does not qualify that he is talking about correct, or even approximately correct, theories. Thus, string theory underlies even all false theories. If there are really 10^500 versions of string theory, this may very well be true!

5. **Avattoir**
   August 28, 2015

   KJ, then that's even more exciting, given there's no limits at all, not even theoretically, on the number of false theories.

6. **George Ellis**
   August 29, 2015

"There are plenty of known particles of spin greater than two." – Theoretically proposed, or experimentally verified? What are they?

7. **TS**
August 29, 2015

The particle data group lists 17 states of spin 4 or above. Now the PDG is partly a political entity and one should be a bit careful about these listings (something LHCb's now-famous pentaquark analysis wasn't), but e.g. the a_4(2040) is a spin-4 resonance established beyond all doubt. Actually, the next edition of the PDG listings will include my measurement of its properties — and I can assure you that the data are unambiguous about its spin being 4.

8. **vmarko**
August 29, 2015

Folks,

I think that Arkani-Hamed and Maldacena are talking about spin-4 fundamental (i.e. elementary) particles, as opposed to composite particles which can be of any spin (some atomic nuclei can have spin 9/2 or such). The fundamental particles with spin higher than 2 are sort-of forbidden in QFT due to a no-go theorem (Coleman-Mandula, Weinberg, Weinberg-Witten — see for example 1007.0435 for a review). But apparently they are not forbidden in string theory, so if (say) a spin-4 elementary particle has some signature in the CMB, string theory would be able to describe it while ordinary QFT wouldn't. Or something along those lines.

Of course, observation of spin-4 fields is not a *prediction* of string theory (AFAIK). IOW, the existence of such particles is *allowed* by ST, but not *required* by it.

HTH, 🙂
Marko

9. **Peter Woit**
August 29, 2015

vmarko,
The problem is that how do you tell if a spin 4 state up at that scale is "elementary"? Composite particles look elementary unless you can probe them at some higher scale. This is why they specify "weakly interacting" instead. It's extremely unlikely that you can see evidence at all that such a state exists, much more so that you can study its interactions. And all of this is going to be taking place up near the Planck scale, where all bets based on effective field theory arguments are off.

10. **vmarko**
August 29, 2015

Peter,

I fully agree with you. It's just that I saw some confusion in the comments regarding the question whether spin-4 particles have already been detected in nature or not. So I wanted to clarify that the existence of composite spin-4 particles is not controversial, while the existence of elementary spin-4 particles is. Of course, establishing (experimentally) that a given particle is indeed elementary is a very hard task even at the LHC scales, let alone near the Planck scale.

🙂
Marko

SUSY 2015

August 29, 2015
Categories: Multiverse Mania, Uncategorized

SUSY 2015, this year's version of the big annual conference on supersymmetry, has been going on for the past week at Lake Tahoe. Joe Lykken began his summary talk by explaining how as a kid he was a big fan of the Bonanza TV show, about a ranch along the shores of Lake Tahoe. He always wanted to travel to Lake Tahoe and visit the Ponderosa ranch, was bitterly disappointed when he finally got to Lake Tahoe and found that the Ponderosa did not exist. The relevance of the story to his talk is "left as an exercise for the audience", with a hint in the next slide, which gives the executive summary of the search for SUSY at the LHC: "We haven't found it." He ends the summary talk with this now well-known prediction for the SUSY 2215 conference.

In his discussion of "naturalness" (see slide 25), he makes what to me has always been an obvious point, but I haven't seen it made ever at a conference like this:

> **Is the Standard Model (almost) all there is?**
>
> Maybe the naturalness argument applied to the Higgs is just wrong (well, it was apparently wrong for the vacuum energy too)
>
> The Standard Model plus some TeV scale renormalizable additions (like dark matter) might be all there is
>
> The Standard Model itself, or with such modest additions, is completely natural (EW scale is not a prediction)
>
> Usual counterargument involves the putative Planck scale and unification thresholds, but this is speculative
>
> An unsatisfying scenario, leaving many questions unanswered, but has a certain minimalist appeal

Some of the other talks included:

- Nima Arkani-Hamed's slides and title aren't available, but the Twitter coverage of the conference included a picture of one of his slides. This tweet reports that he announced that he was "rather annoyed" and "sick of thinking" about naturalness. I guess if you've spent most of your career arguing that the LHC would "resolve the naturalness problem" (because it would move fine-tuning from the Tevatron 10% to 1% if it saw nothing), gearing up now for an argument that a 100 TeV collider is needed to "resolve the naturalness problem" (by going from 1% to .1%) would be kind of annoying… In the slide he makes the same argument as in Particle Fever: no SUSY means particle theory should commit suicide and embrace the multiverse.
- In Gordon Kane's talk he gave his latest string theory-based predictions for the masses of superpartners. For the last twenty years or so, his gluino mass prediction has always been the same: just a little bit higher than the current bounds. These bounds however keep moving up, so his prediction has moved from 250 GeV in 1997 to 1.5 TeV today ("Detectable soon"!). I think one very solid prediction that one could make is that Kane's SUSY 2016 gluino mass prediction will be 2 TeV.
- Joe Polchinski's talk defended moduli stabilization schemes such as the KKLT one that came out of his original work with Bousso, ending with the claim that

  > The KKLT construction has been thoroughly vetted, and it seems to me has survived robustly.
  >
  > The de Sitter vacua are still there, as is the landscape.

  Besides his pre-KKLT role in moduli stabilization, Polchinski is one of the most prominent exponents of the idea that particle theorists should just give up, using the KKLT moduli-stabilized string vacua as a reason for why string theory can never be tested, but we should believe it anyway. He's been promoting this heavily since 2004, when he got Scientific American to publish this article with Raphael Bousso. My take on that article seems to have upset him greatly, and had a lot to do with the arXiv policy of not allowing trackbacks to this blog (which continues to this day).

  This blog does seem to have played an odd role in the topic of Polchinski's talk. Back in early 2014 someone wrote to me to suggest that I might want to discuss a series of papers by Bena, Grana et al. which pointed out problems with KKLT. I responded that I didn't think this was a good idea: while I was no expert, it seemed to me that the KKLT construction was an absurdly complex one involving poorly controlled approximations (thus hard to conclusively decide if it was "right" or "wrong"), and it had entered the realm of ideology, as a bedrock for explaining why string theory could never predict anything. What I didn't say was that I'm a fan of arguments showing string theory can never predict anything, so why should I publicize work challenging them?

  Later in 2014, the same person wrote to me again to suggest that I change my mind, that there was a new preprint about this. I weakened and mentioned it here. A couple months later Polchinski published this, which mounts a defense of KKLT against criticisms like those of Bena, Grana, et al. I didn't know about this, but did write a long posting about another arXiv preprint he had posted the same day, which was about "dualities" in general, and which I found quite interesting. Only after he and Bousso appeared in the comment section to criticize me about KKLT did I realize that they saw me as responsible for promoting anti-KKLT views, not realizing that I'm actually a KKLT fan. Some strange things have happened over the years at this blog, that Christmas Eve discussion was right up there.

  In any case, just to make my views clear: I'm a big fan of KKLT, glad to hear that it now has been "thoroughly vetted". Back when I first heard about it in 2003, I thought "this is great! now that string theorists have proved to themselves that their theory isn't predictive, they'll move on to something else". I've been surprised though about how long that is taking to happen…

Comments

1. **CIP**
   August 29, 2015

   Very droll!

2. **Thomas Larsson**
   August 30, 2015

   Ah, Bonanza. Or the Cartwright brothers, as the show was called on Swedish television. The intro music was very catchy.

   It is nice to see that Lykken mentions what I see as the single most striking result from LHC Run I – that the SM seems to be sitting precisely at the edge of stability. If we elevate this observation to a principle – the principle of living dangerously – we can immediately make a prediction, both for LHC Run II and future accelerators: there will be no new physics which moves the SM away from the stability boundary. New physics which stays on the stability boundary is not affected, of course.

3. **Gianleo**
   August 30, 2015

Peter,

one reason that people have difficulties to believe that the standard model is final might be the following. When the electromagnetic and the weak interaction were made consistent by Salam, Weinberg and Glashow, physicists did not call it electroweak mixing, but called it electroweak unification. Voices of dissent (Feynman above all, in a fit of desperation, shouted that the standard model is made up of "three independent theories!") were ignored.

This misuse of the term `unification' put almost everybody on the wrong track for several decades. Everybody started looking for similar kinds of `unification'. And of course, they had no success, because GSW did not unify anything in the first place.

Even today, this is a minority view, but maybe it can explain part of the story.

4. **Peter Woit**
August 30, 2015

CIP,
Thanks. I think the comic aspects of some of what is going on here are greatly underappreciated…

Gianleo,
"unification" is just a word, one that I think can justifiably be used for the electroweak theory. I don't think though the SM is a "final theory", it leaves unanswered questions that should have an answer, including that of the relation of the three different gauge theories in it. One reason I've always been skeptical about SUSY extensions of the SM is that they don't answer such questions, just introduce more.

5. **Gianleo**
August 30, 2015

Peter,

you are right, `final' is not correct for the standard model ( I meant `almost all there is'). But electroweak `unification' did not reduce the number of parameters. So in what sense is it a unification? In what sense is it correct to speak of electroweak interaction as just a single interaction? The weak interaction remains separate from electromagnetism, or doesn't it?

6. **bw**
August 30, 2015

Peggy Lee had a good song for this https://www.youtube.com/watch?v=LCRZZC-DH7M

7. **Peter Woit**
August 30, 2015

Gianleo,
I think that "unification" is an appropriate word because the theory describes weak and EM forces in terms of the same sort of QFT (gauge theory), so a "unification" of framework. Then there's the Weinberg angle: this is not just a simple product of two gauge theories. But "unification" is just a word and there can be more and less of it. The question of why the two different couplings, why the different charges, why the Higgs, are still awaiting a good idea about further unification.

8. **Curious Mayhem**
August 31, 2015

Takes a Lykken. He has a dry sense of humor, as befits his Scandinavian ancestors. I was a fan of Bonanza when I was a kid as well, and I too was disappointed when I finally made it to Lake Tahoe and saw no Cartwright brothers.

Particle physics will commit suicide *by* embracing the landscape. The Planck scale isn't speculative, but the unification scale of the other three forces is. There are surely alternative "naturalness" problems that arise from other unification schemes, and alternative ways to solve those "naturalness" problems. The landscape is surely and thoroughly unnatural — illegal in 38 states, or it should be.

There's likely to be something to SUSY, but she's probably not dressed in a convenient, easy-to-spot, quasi-perturbative outfit. Particle physicists have wanted to believe in this implementation because it looks like, and lies close to, the Standard Model they know already.

Someone needs more imagination. Too much particle theory of the last 25 years has been virtuoso performances on a few, already established, strings whose tunes might have already been exhausted in, say, 1990. Young folks have had to dance to these worn-out tunes if they wanted to get jobs. The field has been dominated by a small number of established senior people pushing strings for too long.

9. **Shantanu**
August 31, 2015

Gianleo, do you have a reference to the Feynmann quote about standard model?
First time, I am hearing something like this.

10. **Low Math, Meekly Interacting**
August 31, 2015

"Genius: The Life and Science of Richard Feynman" – by James Gleick

https://books.google.com/books?id=LYEFPnMxXQUC&lpg=PA433&ots=UOox-G1Qw3&dq=%22the%20standard%20model%20feynman%20repeated%20dubiously%22&pg=PA433#v=onepage&q=%22the%20standard%20model%20feynman%20rep

11. **Matt Grayson**
August 31, 2015

Interesting. The same interview is described in The Second Creation by Crease and Mann. I guess they were the recipients of Feynman's ire.

12. **Anon**
September 1, 2015

I think Feynman was being precise….. that nature may not have unification like in grand unified theories, where the SU(3), SU(2), U(1) of standard model are unified into one force such as SU(5) or SU(10).

13. **Low Math, Meekly Interacting**
September 1, 2015

I hope this isn't too OT, but I myself have wondered for a long time what Feynman really meant by that. My limited understanding aside, it seems to me quite legitimate to say there is an electroweak force with a broken symmetry, with some fairly compelling experimental evidence even by the time of that interview. Certainly the case for a Grand Unified Force is much more tenuous to this day, experimentally. Feynman seems to be rejecting even the case for there being even a convincing unification of the electromagnetic and weak forces, which makes him sound more like a contrarian than a skeptic on that particular subject. Am I in error?

That said, I've admired the unfashionable integrity of this view overall. Like his many comments on a host of other subjects, he was often remarkably prescient.

14. **Art Brown**
September 1, 2015

Matt Grayson: Nice! Only connect!

LMMI: I once heard Feynman comment after a colloquium: "You have to explain why the weak force is so much weaker than EM", with emphasis on "why". Or words to that effect. Or so my memory insists…

15. **Low Math, Meekly Interacting**
September 1, 2015

I'm probably confused, but I never had the impression that electroweak unification aspired to answer the "why" kinds of questions that people find so unsatisfactory about the Standard Model. But is this limitation a good reason to suspect that unification may somehow be illusory? I just can't tell if Feynman is being overly scrupulous here, because I can't figure out exactly what he's being scrupulous about.

16. **Anon**
September 1, 2015

If we drop the U(1)_Y group from the Standard model, there will only be the weak and strong interactions and no photon. The photon owes its existence entirely to the U(1)_Y group. In std model there are 3 forces and 3 gauge groups. There is electro-weak physics, but the words "electroweak unification" are probably just to say it is the unified framework of QFT rather than unification of forces like in grand unified or even Pati-Salam (partially unified) models.

And of course there is no evidence so far for GUTs or Pati-Salam and a lot of parameter space is being ruled out. If there is no such unification of forces in nature (which appears to be more and more the way experiments are pointing) then we will have just have several forces and several gauge groups.

17. **Gianleo**
September 2, 2015

Yes, the story is from "Genius" by Gleick. Search for "three theories".

(I was wrong to add "different", but if you read the text, you will see that Feynman meant it.)

18. **sm**
September 2, 2015

Anon: Your ideas are very close to those of Veltman, who expresses them in a colourful and forthright way in his 1999 Nobel prize interview. Veltman stresses that nature could just as well be non-unified, so discussions in the popular literature of unification are a form of (annoying and possibly stupid) propaganda for that area of research. I recommend the whole 30 min interview for those who have not already seen it – it is informative as well as entertaining. In the interview he also gives a 'Veltman centric', (and on the face of it correct) explanation for why the LHC was built but the SCC was not!

19. **Anonyrat**
September 2, 2015

^^^ 6:39 of Veltman's interview here: http://www.nobelprize.org/nobel_prizes/physics/laureates/1999/veltman-interview.html

20. **Anonyrat**
September 2, 2015

Veltman's Nobel lecture also makes the point, http://www.nobelprize.org/nobel_prizes/physics/laureates/1999/veltman-lecture.pdf (PDF file)

> "To what extent are weak and electromagnetic interactions unified? The symmetry used to describe both is SU2 x U1, and that already shows that there is really no unificaton at all.
>
> True unification, as in Maxwell's theory, leads to a reduction of parameters; for example, in Maxwell's theory the propagation velocities of magnetic and electric fields are the same, equal also to the speed of light.
>
> In the electroweak theory there is no such reduction of parameters: the mixing angle can be whatever, and that makes the electric coupling constant e = g sin(θ) a free parameter. If the Higgs sector is not specified, then the Zo mass and the photon mass are also free parametrs. There is really no unification (apart from the fact that the isovector part of the photon is i the same multiplet as the vector bosons).
>
> However, if one specifies the simplest possible Higgs system then the number of free parameters diminishes. The Zo mass is fixed if the weak mixing angle and the charged vector-boson mass are fixed, and the photon mass is necessarily zero. So here there is some unification going on. It seems to me, however, utterly ridiculous to speak of "electroweak unification" when choosing the simplest possible Higgs system.

21. **anti**
September 2, 2015

"Usual counterargument involves the putative Planck scale and unification thresholds, but this is speculative"

is there a hierarchy problem if there is no planck scale or unification threshold?

22. **Peter Woit**

September 2, 2015

anti,
No. The hierarchy problem is that of why the electroweak mass scale is so much smaller (and thus "fine-tuned") than the GUT or Planck scale. If you give up the assumption that the SM is just some effective theory for some other theory with those mass scales, than the "fine-tuning" problem disappears.

23. **anti**
September 2, 2015

peter

what are the physical consequences and predictions if you give up the assumption that the SM is just some effective theory for some other theory with those mass scales?

24. **Peter Woit**
September 2, 2015

anti,
Giving up such assumptions about unification leaves you with just the SM, it doesn't make the SM itself more predictive, and the origin of the electroweak scale remains a mystery. It does get rid of certain "problems" that appear generically when you make those unification assumptions (e.g. the hierarchy problem).

25. **anti**
September 2, 2015

is that a serious proposal among particle physicists? what is your own personal view?

26. **Peter Woit**
September 2, 2015

anti,
It's not a "serious proposal", it's just a reminder that the "hierarchy problem" is based on certain assumptions that we do not know are correct. I've often made this point here, and I think it's understood by most people in the field. I was just interested to see that Lykken was making the same reminder in his survey talk, since it is not often brought up explicitly in such talks.

27. **Anon**
September 2, 2015

sm, Anonyrat — thanks for the pointer to Veltman – that is interesting.

If we do not have unification of course we can still have new physics such as due to a higher theory based on parity (P) symmetry, or another way to proceed could be horizontal gauge symmetry between generations, and see what regions of their parameter space can be tested. Non-unification would motivate more work in such directions, I think.

Peter — Normally I agree with you on most things you write and am a regular visitor to your blog. However on Hierarchy problem, I think it does not matter whether there is or there isnt new physics at an intermediate scale such as GUT scale — we have the hierarchy problem even if it is standard model all the way up to the Planck scale — or if we argue that is a non-problem in this case, then I think an argument can also be made that it is a non-problem even if there are intermediate GUT scales.

This is because the Planck scale still exists even if there are intermediate scales — and the divergence due to Planck scale can be fine tuned to give any mass just as if it were the divergence in pure standard model from Planck scale.

Anyway there are more direct ways to see that not having intermediate scales is not a solution to the hierarchy problem.

I do think that experiments are showing that hierarchy problem is not solved in nature and I feel it maybe that its a non-problem — whether or not there are intermediate scales.

28. **Peter Woit**
September 2, 2015

Anon,
The point is the same for the Planck scale. You're making an assumption that there is some different physics that acts as a cutoff for the SM, and that this different physics takes place at the Planck scale. But we actually know nothing at all about what is happening at the Planck scale (this is why Lykken refers to it as the "putative Planck scale").

The standard argument for quantum gravity coming into play at the Planck scale is dimensional analysis. We know Newton's constant, it's a dimensional number, and if we assume it can be calculated as some number of order one times the scales in the problem, we get the Planck scale. But (whatever string theorists might tell you), we don't have a viable theory in which to do such a calculation. How do you know that if we could do this calculation, we won't get some exponentially small number, not a number of order one (for example, some "induced gravity" theory, where the Einstein-Hilbert action is an effective action, caused by some exponentially small effect)? Of course I don't have a viable quantum gravity theory of this kind. But I think it's important to be reminded of what assumptions one is making, and to remember that we don't have any evidence for assumptions about what is happening at the Planck scale. All we do know is that the cut-off SM is quadratically sensitive to what is happening at the cut-off. This is undoubtedly a clue, but only a "problem for the theory" if you make some assumptions such as a high quantum gravitational scale acting as a cutoff.

29. **vmarko**
September 3, 2015

Regarding the electroweak unification — I often like to present it as "half-unification". Namely, the only thing that makes it more unified than taking the simple product of weak and electromagnetic forces is the fact that SU(2)xU(1) is broken down to the electromagnetic U(1) subgroup which is *not* the second factor in the product (i.e. charge and hypercharge are not the same thing). Other than that, there is really nothing that unifies electromagnetic and weak forces in the way that doesn't also unify the strong interactions in SU(3)xSU(2)xU(1). Crucially, the number of free parameters is not reduced, which should be the main feature of any proper unification. So while there is something nontrivial in the electroweak unification, it is still not the proper real thing. Therefore half-unification. 🙂

Regarding the hierarchy problem — in the appropriate classical limit, we observe three scales in nature: the Planck scale (i.e. the Newton's gravitational constant), the electroweak scale (i.e. the Higgs mass) and the cosmological scale (i.e. the cosmological constant). The huge discrepancies between these three scales are called the hierarchy problem (ratio of Higgs to Planck scale is around 10^{-34}) and the cosmological constant problem (ratio of the CC to Planck scale is around 10^{-122}).

At the fundamental quantum level, it may happen that the ratios of these scales are of order one (case A). If that is the case, the hierarchy and CC problems become the problems of explaining the differences in the scales when taking the semiclassical limit of the theory. This has to be quite nontrivial, since we need to obtain some extremely small numbers by solving equations which feature only numbers of order one. It is likely a nonperturbative effect, and given some putative theory of everything, it is ultimately a mathematical problem.

OTOH, it may also happen that at the fundamental quantum level the ratios of the three scales remain basically the same as in the classical regime (with only small quantum corrections). In that case (B), the hierarchy and CC problems become the unanswered "why" questions for the fundamental theory. Then it is a matter of taste whether or not they are considered problems (a failure in our understanding of nature) or just facts of the real world (either by God-did-it design or by multiverse-did-it randomness). While the design/randomness resolutions are metaphysical, I prefer to think of the hierarchy and CC problems as our failure to understand something, which maintains them as physical problems, and means that our putative theory of everything is still not fundamental enough.

To sum up, case (A) requires a proper fundamental theory and a proper mathematical resolution of both problems. Case (B) either reduces to case (A), or denies the hierarchy and CC problems at the metaphysical level.

HTH, 🙂
Marko

30. **Low Math, Meekly Interacting**
September 3, 2015

Very interesting. Thanks all, learned a lot (with awareness, of course, that deeper understanding of most of the relevant facts are beyond me). There just isn't much discussion in popsci literature about what nature might look like in the absence of a "unified" TOE of the sort you are all familiar with. These subtleties are glossed over to the point it becomes difficult for the interested layperson to even imagine how or why it might not be so, or what HEP might focus on profitably in place of the putative new broken symmetry at the TeV scale…or any scale.

31. **Anon**
September 3, 2015

Peter and Marko,

Thanks for your comments. What I feel is that there is a procedure of doing calculations that gives finite and meaningful answers — regularization and renormalization — that works both for Standard model as well as for GUTs and non-GUT extensions of the standard model (that have more mass scales than the standard model).

I think therefore if hierarchy problem is a non-problem in Standard model, it is also a non-problem in any renormalizable extension of the standard model that may have additional mass scales.

With neutrino masses and mixing we are already going beyond the standard model. Any theory that takes them into account will be consistent and will only lead to a different set of "why" questions.

— if we just add 3 neutrinos and give them small Dirac masses so as not to introduce any new scale, then the why questions would be: why are the Dirac Yukawa couplings so small ~10^-12 (esplly nagging for the third generation)? Why is there an ungauged global B-L symmetry? Why is electric charge quantized if there are no Majorana mass terms? Why is the Majorana mass term absent?

— if we add 3 neutrinos to Std Model and add a Majorana mass term this will lead to a new mass scale (lets say much greater then SM scale, example 10^14 GeV), and all the above why questions disappear. A new why question would be: why is the seesaw scale 10^14 GeV. The hierarchy problem (/ hierarchy non-problem) which was anyway there in SM remains as a hierarchy problem (/hierarchy non-problem) in this case.

In both the above cases we can do QFT, renormalization and get finite answers for measurable quantities.

32. **Peter Woit**
September 3, 2015

Anon,
This has now wandered far away from the topic of the posting. Questions such as the ones you bring up about neutrino masses are important, but a very different topic, and I have to remind everyone that I'm not able/willing to run this blog as a general discussion forum.

33. **Oliver**
September 4, 2015

Hi Peter,

I don't understand what you mean by "the assumption that the SM is just some effective theory". The U(1) and scalar sectors of the standard model are renormalizable only within perturbation theory; non-perturbatively they suffer from the triviality problem. As such, the standard model as it stands strictly only makes sense if defined with a cutoff.

34. **Peter Woit**
September 4, 2015

Oliver,
Yes, all indications are that the SM is not a complete theory, and that something new has to come into play at shorter distance scales. My comment was very over-simplified. One point it intended to refer to is that we know that appropriate gauge field + fermion theories are asymptotically free and make perfectly good sense at all distance scales, not just as an effective theory. This doesn't work for the U(1) factor in the SM, but one can assume for instance some GUT scenario where at short distances there is no U(1) factor (it is part of a larger gauge symmetry)

The real problem are the scalars, with that part of the theory only seeming to make sense with a cutoff. One should keep in mind though that at this point we have limited experimental information about the scalar sector of the theory. While we know much more since the observation of the Higgs and many of its decays, still nothing is known experimentally about scalar field self-interactions. It is the scalar sector and its Yukawas that is responsible for most of the parameters in the SM that we don't know how to calculate. It also has the odd property that to a good approximation the self-coupling is running to zero.

So, my argument is more that most of the ingredients of the SM make sense at arbitrarily short distances, with the main exception the scalar sector, which is the least understood and most problematic. One can imagine a deeper insight into the scalar sector which would change its high-energy behavior to make the triviality problem disappear (not that I have the vaguest idea how to do that).

35. **Louise Riofrio**
September 5, 2015

The Ponderosa Ranch location was near Lake Tahoe. It was open as a tourist attraction until 2004. Those of us too young for the show (Lorne Greene was on a TV show before Battlestar Galactica? Who's Lorne Greene?) still enjoyed visiting the ranch.

36. **GoletaBeach**
September 6, 2015

LM,MI …. thanks so much for the link to the story of Feynman and SU(3)xSU(2)xU(1). Made my day. But there is something to the concept of the Conserved Vector Current. Perhaps Veltman nailed it. Seems to me once there was a mini-industry of testing from the ground up whether the electroweak Lagrangian was really right, but everything was so consistent that eventually everyone caved. Well, there are still some STUck on similar concepts. Good.

This Week and Next Week’s Hype

September 3, 2015
Categories: Multiverse Mania, This Week's Hype

This week’s hype comes to us from Discover Magazine, which has Is Our Universe One of Many? Here’s How We Can Find Out. Needless to say, the author doesn’t actually tell us how we can find out, just repeats the usual “maybe we’ll see bubble collisions” argument often discussed here. We’re also told that

> It is important to keep in mind that the multiverse view is not actually a theory, it is rather a consequence of our current understanding of theoretical physics.

It seems that our “current understanding of theoretical physics” is the string theory landscape. If you ask what the evidence is for string theory, you’ll be given the usual circular reasoning that we don’t have evidence because of the multiverse.

For next week’s hype, there will be a promotional public talk in Paris next Tuesday entitled String Theory: results, challenges and magic. Presumably this will be much the same as the speaker’s String theory: results, magic and doubts from two years ago. The argument for string theory seems to be that it is “magic”, and somehow in the past two years the “doubts” have turned into “challenges”. The abstract describes string theory poetically as having

> soured and captured the imagination of a generation of high energy physicists.

which I would say is a deep truth, if probably not one the speaker intended.

Comments

1. **5371**
   September 4, 2015

   The word “soured” is rather hard to emend. I incline toward “spurred” rather than “soared”, but neither is really satisfactory.

2. **jjg**
   September 4, 2015

   Perhaps from “faire tourner” which can mean rotate, revolve, churn or sour.

3. **DrDave**
   September 4, 2015

   Soured…how very odd…soared, perhaps, with a split clause.

4. **Chris W.**
   September 4, 2015

   The author of the Discover Magazine piece (Crux blog post) also flirts yet again with conflating the multiverse with the Many Worlds interpretation of quantum mechanics, although he seems to back away from that in the last section (“Testing the Theory”).

5. **a l**
   September 4, 2015

   Carlo Rovelli posted recently a refreshing essay which demolishes most of ‘our mathematical universe’ and its incarnation in the multiverse.

   “…science can be said to be nothing else than the denotation of a subset of David Lewis’s possible worlds: those respecting certain laws we have found. But Lewis’s totality of all possible world is not science, because the value of science is in the restriction, not in the totality.”

   Rovelli C., Michelangelo’s Stone: arXiv:1508.00001 [math.HO]

6. **Patrick Dennis**
   September 4, 2015

   My layman’s understanding of the concept of multiverse as “… a consequence of our current understanding of theoretical physics,” is that it would be a consequence of inflation, not string theory (and that one does not depend upon the other.) Wrong?

7. **David**
   September 4, 2015

   perhaps (in a French-speaking context referring to imagination)
   ‘soured’ = ‘piqued’ ?

8. **Peter Woit**
   September 4, 2015

   Patrick Dennis,
   The “inflation implies multiverse” claim is a common one, and I’ve often written about the problems with it. This particular author though is not talking about inflation, but writes
   “Instead the idea that the universe is perhaps one of infinitely many is derived from current theories like quantum mechanics and string theory.”
   putting string theory and QM on a par.

9. **Yatima**
   September 5, 2015

   Thanks, “a l”, for the link to Carlo Rovelli’s Michelangelo’s Stone: an Argument against Platonism in Mathematics. It’s an interesting read (and I will read it again), but it’s not very convincingly written.

Not a believer in hard-core Platonism myself. I like to consider Mathematical Structures as computers (syntactic machines) with no limits imposed by computational complexity, whose actual (entirely imagined) behaviour one wants to unravel. Very much linked to the real world.

However, Carlo Rovelli's examples seem rather ill-chosen and trying too hard. In particular, they are free of constraints that the real world would impose. Alien intelligences that don't develop counting? Nice for Asimov's Magazine of Science Fiction but I can only say good look in the real-world predation game with THAT. Marvin Minsky handled this rather better in 1985 in [Communication with Alien Intelligence](). Develop Riemannian geometry before Euclidean one? Possible, but general ideas (highly compressed and with book-length contextual baggage) do not spring fully-formed out of someone's real-world brain machinery, it's a building-up process from simple ideas. Always. Linear algebra not so universal? I really don't think so, and the fact that it was formulated rather recently is not an argument against its universality. We don't program a computer to slowly unravel the platonic world's theorems because we want to find the interesting ones only? Of course not! It's because no-one is mad enough to use computers on problems clearly far, far, far outside of the polynomial complexity problem set (itself a platonic concept, actually), Gödel's completeness theorems notwithstanding. I suspect Carlo Rovelli might profit from a course in the Theory of Computation.

It made me think of Hilary Putnam's, The Logic of Quantum Mechanics (Originally, "Is Logic Empirical", Boston Studies in the Philosophy of Science Vol. V, 1969), wherein the (entirely unsurprising) claim is made that classical logic is actually not universal and that this can be illustrated by the necessity of having other logics to describe QM for example (no mention is made of Linear Logic for this, still in the future). It's like "Dude! No need to crash in. That door was open. Take a seat, I'll buy you a drink."

But this is getting rather off-topic.

10. **Peter Woit**
September 5, 2015

a1/Yatima,
"But this is getting rather off-topic."
Yes, I left the link to Rovelli because people might be interested, but it has little to do with the topic of the posting, and this is not a great place to discuss it.

11. **a l**
September 5, 2015

Reading Rovelli's piece might give you a somewhat different perspective on the multiverse topic. Otherwise it's once again "not testable, blah blah". If our knowledge of mathematics is contingent, it looks less convincing that something is real just because there is a theory for it.

12. **Neil**
September 5, 2015

Rabinovici is not the first to invoke magic. Did not Witten himself suggest that the M in M-theory could stand for "magic"?

13. **Vincent**
September 6, 2015

What struck me (a mathematician with very limited knowledge of physics) in the Discover article is the claim that 'The universes predicted by string theory and inflation live in the same physical space (unlike the many universes of quantum mechanics which live in a mathematical space), meaning they can overlap or collide.'

This seems a huge improvement over multiverses where the universes are truly disjoint and cannot be detected from within one another. But it also left me with a very basic question: what is the definition of a universe? This 'same physical space' sounds pretty much like what I would naively call the universe. What do I miss?

14. **Peter Woit**
September 6, 2015

Vincent,
The cosmological multiverse and the QM many-worlds multiverse are two completely different things. In both cases the "universes" are part of a larger structure. In the many-worlds case there is one overall state space, with different subsectors that "decohere", so can effectively be treated as independent. In the cosmological case, there is one space-time manifold, with different "bubble universes" which are causally independent. The "bubble collision" business is about the possibility in some multiverse models of a violation of causal independence, an interaction of the bubbles. However there is no evidence this is happening in our "bubble", and no reason at all to expect it to.

15. **Ronan**
September 6, 2015

BBC's popular science documentary series "Horizon" aired an episode about the multiverse last week – featuring Max Tegmark, Seth Lloyd …..

Available to watch here (I think only in UK)

http://www.bbc.co.uk/programmes/b0695t56

16. **Peter Woit**
September 6, 2015

Ronan,
Here in the US we got to enjoy that last fall:
http://www.math.columbia.edu/~woit/wordpress/?p=7352

17. **Narad**
September 7, 2015

> perhaps (in a French-speaking context referring to imagination)
> 'soured' = 'piqued' ?

I was thinking that Hebrew would be a better angle to approach this one from than French, but that wasn't particularly promising, either, without acrobatics (m.m. *Sauerstoff*).

I'm inclined toward a typo in an odd construction using "sourced"; it doesn't quite seem to be at the mystery level of "homologous recombinaltion *tiniker*."

18. **Neil**

September 8, 2015

I vote for roused. Somehow the r and s got exchanged.

19. **Anon**
September 9, 2015

"Soared" would be consistent with Discovery writing style, so I suspect a typo, maybe Freudian. I have to say, though, that despite the occasional exaggerations, Discovery magazine has become better that Scientific Amercian, in my opinion, but maybe only because SciAm has become so awful.

20. **marten**
September 10, 2015

If it is a typo, "sourced" seems more likely to me..

21. **Lindsay Berge**
September 11, 2015

I would guess 'soured' should be 'stirred' as in the collocation 'stirred the imagination'. So the phrase would be 'stirred and captured the imagination of a generation of high energy physicists.'

22. **Low Math, Meekly Interacting**
September 11, 2015

"The universes predicted by string theory and inflation..can overlap or collide. Indeed, they INEVITABLY MUST COLLIDE…" (caps mine)

Oh really? Am I wrong, or is there no consensus on this, even among Lanscapers and Eternal Inflators?

23. **Scott Church**
September 11, 2015

Peter or anyone, I have a quick question which hopefully isn't off-topic here… In eternal inflation, SM, Landscape or otherwise, bubble universes will nucleate amidst larger inflating regions and reheat in the usual manner. Presumably this means that each nucleating bubble "neighborhood" is surrounded by isotropic inflating regions out of which other bubble universes are nucleating. So… how can bubble universe collisions occur between bubbles that are all inflating away from each other in full afterburner? Am I missing something…?

24. **Marty Tysanner**
September 12, 2015

Scott,

I don't know your background so I'll give some detail. You're getting somewhat more than you asked for!

To do its job, inflation must smooth out and/or dilute preexisting inhomogeneities of the pre-cosmological space by rapidly expanding the space, and then it must stop. General relativity offers a way: a space that contains a constant, positive energy density will expand exponentially as *exp(Ht)* in all directions. The energy density determines the rate *H* ("Hubble parameter"); *t* is the time.

In the simplest models, the vacuum is presumed to be filled with a *scalar field* whose associated potential energy curve has special characteristics; more complicated models may employ more than one field. The curve generally has a (nearly) flat part or/and a metastable "bowl," both at a high energy where inflation does most of its job; this is followed by a steep drop to a stable (or at least metastable) minimum where reheating and subsequent cosmological evolution occur. The scalar field and its potential energy curve exist to cause inflation: they are imposed by *fiat*, not by anything in the Standard Model. Beyond general characteristics, the particular assumed shape of the potential depends on the model. Since there is no *a priori* choice that is dictated by a fundamental theory there is substantial freedom to choose the potential to give you what you want in terms of consistency with observational evidence.

The scalar field must somehow get into the state of high energy density for inflation to begin; an actual inflation *theory* would explain this, ideally. Once it starts, inflation will continue as long as the scalar field state remains there, e.g. while the "inflaton" "rolls" down the gently sloping part of the potential (the Hubble parameter decreases slowly as the inflaton rolls down). It ends when the inflaton rolls down the steeply falling region and reaches the stable/metastable lower energy state.

(You can think of inflaton motion at some point "x" in the space as the time-dependent change of energy density of the scalar field at x. Note that we're talking about energy *density* — the energy density can have different values at different points, and different values correspond to different places on the potential energy curve.)

In some models the potential curve has a metastable high energy "bowl" where the inflaton remains trapped for a time. Since the energy density remains stuck there with a constant value, the Hubble parameter *H* is constant so that the exponential expansion *exp(Ht)* produces a de Sitter (hyperbolic) space. This is probably the kind of scenario you are referring to. Being a quantum mechanical entity, the inflaton can probabilistically tunnel through the "bowl wall" onto a gently sloping/ steeply sloping/ stable minimum region of the assumed potential, depending on the model. Spatially, in the bubble universe scenario the tunneling event corresponds to formation of a spherical region of finite radius; this region has the energy density given by the tunneled-to part of the potential curve, and subsequent evolution inside continues from there down the curve to the minimum energy.

In this scenario, the newly formed spherical region has a lower energy density than the ambient de Sitter vacuum (maybe even zero), and thus a domain wall forms to separate the "inside" region from the ambient vacuum. This is the bubble wall, initially stationary with respect to the bubble center. Being of lower energy than the ambient vacuum, the wall continually "eats" energy of the surrounding high-energy vacuum and thereby adds to its kinetic energy, causing the wall to rapidly accelerate outward into the de Sitter space. If the tunneled-to region of the assumed potential is gently sloping, the bubble interior will meanwhile undergo a secondary but less rapid inflationary period until it drops into the stable "true vacuum" state.

Of course, the de Sitter vacuum outside the bubble continues its exponential expansion all the while: the de Sitter space expands faster than the space inside the bubble, so compared to the exponentially increasing volume of a "box" that contains the bubble, the exponentially increasing bubble volume occupies a rapidly decreasing fraction of the (expanding) box volume over time. Inflation is eternal in this scenario (as in nearly all inflation models) because, even though inflation will eventually end "almost surely" at every point in the de Sitter space via tunneling, the expansion of space occurs faster than tunneling can stop that expansion.

(Some people get confused by exponential expansion, since it implies that an object far enough away will be moving away faster than the speed of light. The bubble wall speed increases exponentially from the center, but "speed of light" in an expanding space is only meaningful if measured locally: at any given moment the wall speed is with respect to a *fixed* point infinitesimally distant from the wall at that moment, whereas the wall can move faster than the speed of light relative to a point a *finite* distance from the wall.)

Having a quantum mechanical origin, bubble nucleation is a probabilistic event. Nucleation events are independent, so two bubbles will sometimes form nearby. Both bubbles expand and their walls accelerate toward each other. Now to answer your specific question (finally!): If two bubbles form *sufficiently closely*, the acceleration of their respective walls toward each other can "outrun" the accelerated expansion of the ambient de Sitter vacuum between them, and they will collide.

This is all hypothetical of course… In principle, the aftermath of a bubble collision could be detected today if it had occurred under very optimistic/ideal (and probably very unlikely) conditions. Such an event would presumably have a marked effect on the energy density in the collision region which would ultimately manifest as a strong, circularly symmetric inhomogeneity of the cosmic microwave background. But no such inhomogeneity has convincingly shown up, yet at least. Importantly, only collisions that occurred very soon after nucleation would cover a significantly large region of the sky to be observable; later collisions, if their effect could reach us at all, would subtend too small an angle to be statistically discernible.

Sorry Scott (and Peter!) for such a long comment!

25. **Marty Tysanner**
September 12, 2015

As a side comment to my previous one, the kind of eternal inflation that arises from single field inflation models should lead to all bubbles having essentially the same particle content and laws of physics. A scalar field has no obvious properties that should affect what kinds of particles appear during the reheating phase that immediately follows the inflationary phase, nor do they indicate why fundamental constants should be bubble-dependent (to me, anyway). That seems to make the kind of multiverse indicated by most inflation models less than WOW!-worthy, but that's just my opinion…

26. **Peter Woit**
September 12, 2015

Marty,
I think the question that always comes up is whether inflation implies endless new bubble universes, which must "inevitably" collide. Some experts seem to say no (e.g. Katie Freese), Lim and others say yes, although often in the context of popular articles like this one that are chock-full of other misinformation, so hard to take seriously. I'm not expert enough on the details of all inflationary models to judge who is right here, and, honestly, there seem to be much better ways to spend ones time than trying to sort this one out. If anyone knows of a reliable source with a definitive answer, please do tell.

27. **Low Math, Meekly Interacting**
September 12, 2015

Marty,

That was magnificent. Thank you.

28. **Scott Church**
September 14, 2015

Marty, sorry for the delayed response… I just got back from a week in Alaska and am finally settled in again. Thanks so much for your thoughtful and informative response! I do have a physics background (MS level) and was aware of much of that already. But the key piece I was missing–which you so graciously provided–was that domain walls will acquire kinetic energy from their immediate inflating deSitter neighborhoods at a rate that can allow bubbles that are suitably near each other to grow faster than the underlying inflation can separate them, thus leading to collisions. Now it makes sense.

Of course, as Peter points out this still leaves us not only with the question of whether there is evidence of such collisions occurring (the answer to date being a resounding No), but also whether they're inevitable within the extant chaotic inflationary framework, with or without M-theory. The question is important, I think, if for no other reason because should it turn out that such collisions *are* inevitable within those frameworks, the fact that they aren't observed raises some serious issues with chaotic inflation. I too would be interested in finding reliable sources with a definitive answer.

Best.

Various and Sundry

September 14, 2015
Categories: Multiverse Mania, Uncategorized

The semester here is finally underway, and I'm getting back to work on my quantum mechanics and mathematics book (latest version available here). Current plan is to have a final version by next spring, with publication by Springer late next year. This semester I'm teaching Calculus II, a subject where there's only one thing I really dislike about pretty much all textbooks, their refusal to use Euler's formula. Since I couldn't find an online source I was completely happy with, I spent some of the last couple days writing up some notes for the students on Euler's Formula and Trigonometry, which maybe someone else will find useful. In other news:

- Nima Arkani-Hamed was here today, giving a talk on a new model he calls "NNaturalness". The basic idea is to consider something like N copies of the Standard Model, with N a large number. Large N fixes the technical naturalness problem, with something like $N=10^4$ fixing the MSSM's current naturalness problem, and $N=10^{16}$ fixing the non-supersymmetric problem. He makes clear that he's well aware that this is a pretty contrived thing to do, but argues that it's interesting one can do this while evading dramatic disagreement with experiment, and coming up with potential CMB signatures soon observable (e.g. the effective number of relativistic degrees of freedom).

  He has a nice description of the naturalness problem as "in any theory where we can compute the mass of the Higgs it has a fine-tuning problem". Probably there are people out there who think they have a way to compute the Higgs mass who would disagree with him. To me the problem is that the theories he's talking about (GUTs, string landscape) don't actually explain anything about the underlying physics of electroweak symmetry breaking (where does the Higgs field come from and why does it have those couplings?). Given this, it's unclear why one should worry about the fine-tuning.

  He describes the landscape and the multiverse as "like democracy, the worst idea except for everything else", and gives a defensive argument for why one should study alternatives like "NNaturalness", even if they're not as good as the multiverse (which he finds "simple and deep"). To him it's worth thinking about alternatives to the multiverse (as a "foil") not because the multiverse is untestable pseudo-science, but because maybe one shouldn't just give up. So, it seems that at this point he's not quite signing up with the intellectual suicide of multiverse mania, although he sees it as the most attractive path available.

  In other Arkani-Hamed news, the IAS has an article about his activities promoting a next generation collider here.

- The KITP has a newsletter here, including a description by Graham Farmelo of his visit there. Oddly, no matter what he writes about, Farmelo almost always includes an unconvincing defense of string theory and/or the current activities of string theorists (for examples, see here, here, here and here). In this case he assures us that the KITP theorists are not given to "mathematical adventurism". I think he's right, but that's the problem…
- Someone pointed me recently to Olivia Caramello's web-page on Unifying theory and her arguments with fellow category theorists. I had a youthful infatuation with category theory, but ultimately came to the conclusion that there's a real danger in that kind of "unification" of going too far in the direction of saying less and less about more and more. Many of the ideas involved are powerful and attractive, but the remarkable thing about mathematics is that, even for the lover of grand ideas, less generality is sometimes even more so.

Update: One more. If you're in the Bay Area next week, you might want to head up to MSRI for a series of elementary talks on the Langlands program by Edward Frenkel.

Comments

1. **Unemployed**
   September 14, 2015

   Surprised to hear Euler's formula is absent from introductory texts. After discovering it as a youth, I used it just like this, and found it a wonderful alternative to the pain that my classmates were suffering without it. But I always assumed its absence from our curriculum was merely one more manifestation of our crap teachers and textbooks. Kudos for re-introducing it to the world.

2. **mitch**
   September 15, 2015

   Peter,

   You have a typo in your exponential notes on page 9.

   Where you are evaluating the integral of cos(ax)cos(bx), on the second line, the second term in the integrand should be exp[i(b-a)x].

   See, I'm looking out for you.

3. **Veríssimo**
   September 15, 2015

   What's up, Peter. Nice post, but I was expecting something about the latest hype as well:
   http://www.theguardian.com/science/life-and-physics/2015/sep/14/straws-in-the-lhc-wind-lepton-universality-and-an-update-on-that-bump

4. **Aleksandar Mikovic**
   September 15, 2015

   As far as the category theory is concerned, the way to obtain some interesting results is to avoid generalizing too much. For example, the Turaeev-Viro 3-manifold invariants were obtained by using special tensor categories (associated to representations of quantum groups at roots of unity) while the invariants of 4-manifolds can be associated
   with 2-categories. Moreover, the 2-categories also appear as 2-groups or crossed modules, which generalize our notion of symmetry (which is based on the notion of a group). This is useful for constructing new TQFT's and quantum gravity theories.

5. **Ru**
   September 15, 2015

   Isn't Euler's Formula and Trigonometry taught at grade 12?

6. **Peter**
   September 15, 2015

"An engineer / scientist … starts out knowing a great deal about very few things but over time learns more and more about less and less until eventually s/he knows everything about almost nothing".

"A philosopher / architect … starts out knowing a little about a large number of things but over time learns less and less about more and more until eventually s/he knows almost nothing about everything"

7. **andrew**
September 15, 2015

Can you give a few more details about N-Naturalness? I cant find any of Arkani-Hamed's slides are on the web, but I've heard about his idea anecdotally from a few sources.

8. **Olivia Caramello**
September 15, 2015

Thank you for your pointer to my website. I should clarify that the kind of unification that I pursue in my work is not through (Lawvere-style) category-theoretic generalization/axiomatization, but rather through transfer of suitable invariants. While category theory can be regarded as a unifying language since its concepts are sufficiently general to specialize to many important 'concrete' notions in specific fields of Mathematics, Grothendieck toposes provide a substantially different way for relating different mathematical theories with each other, which is based on the possibility of transferring invariants across their multiple representations. The kind of insight that these novel topos-theoretic methods can bring is therefore much deeper than the "static unification" through generalization achieved by category theory (the distinction between these two different kinds of unification is explained at this page): they allow, for instance, to translate a difficult-to-check property in the context of one mathematical theory into a completely different-looking one which can be much easier to tackle in another theory, and to generate concrete results in different mathematical areas which might be hard to prove using alternative methods (a list of examples is available on my website).

9. **sm**
September 15, 2015

"By the end of this course, we will see that the exponential function can be represented as a power series"

Peter, why leave them 'dangling in expectation' to the end of the course? The power series follows in two lines – satisfies the stated equation and meets the initial condition, both only requiring calculus 1if I am not mistaken?

You would then have a 100% 'do it all yourself' physicist's exposition!

10. **Peter Woit**
September 15, 2015

Ru,
In the US Euler's formula is not part of the standard secondary school curriculum. Even worse, it's not part of the calculus curriculum at the college level. I hope this is not true in most other countries.

One fundamental problem (besides that of not having a crucial, powerful tool) is that without using Euler's formula, there's no sensible way of understanding why the addition formulas (and thus most trig identities) are true. As a result, students are led to believe that mathematics is about some list of formulas you should memorize without understanding why they are true.

Another way of getting the same thing (cos + isin\theta is a rotation by \theta), while avoiding complex numbers would have been to use two by two rotation matrices, but that requires introducing matrix multiplication.

I don't really understand why the US math curriculum makes this choice not to use Euler's formula. It seems to be that the logic is "to understand Euler's formula you need to understand complex functions of a complex argument, and we can't talk about that until a complex variables course"

sm,
The difference between a math and a physics course is that in the math course you're supposed to pay attention to whether the power series converges, and whether what it converges to is the function you have in mind. Much of the later part of the course is devoted to understanding series and convergence issues, so, yes, I'm leaving them dangling…

11. **Peter Woit**
September 15, 2015

Andrew,
Sorry, it's a slightly complicated picture, and I don't think I'd be doing anything very helpful by explaining badly what I understood of it here. He says a paper is underway, collaborators are Cohen, D'Angelo, Hook, Kim, Pinner, maybe you can find out more from one of them.

12. **Peter Woit**
September 15, 2015

Verissimo,
The Butterworth article isn't hype at all, it does an excellent job of explaining the story.

13. **anon**
September 15, 2015

It is unfortunate to see promising young researchers potentially destroying their own careers by being so clueless with regards to social skills, as Olivia Caramello seems so bent on doing right now. I hope someone would give her better advice, and that she would follow it.

14. **Peter Woit**
September 15, 2015

anon,
I don't know her field and I didn't look that carefully at all the details of her exchanges with other mathematicians. But some of them did seem to be giving her good advice, and I hope she'll make use of it. The issue of "folklore theorems" in mathematics is a very tricky one for a lot of reasons. Her choice to make all this debate quite public is very unusual. To the extent it makes the situation with the mathematics and its history clearer, that may be a good thing. On the other hand, to the extent it gets people worked up and annoyed about priority claims, not a good thing at all.

15. **Peter Woit**
September 15, 2015

mitch,
Thanks. Fixed.

16. **tt**
September 15, 2015

I think Dr. Caramello comes off well in the exchange.
Besides the accusers admitting to writing letter of reference that insulted her, one accuser defends himself with this statement.

"You'll recall that, when you first told me about the `de Morganization' construction (the largest dense de Morgan subtopos of an arbitrary topos) I was very surprised — not so much by the fact that this construction existed, but by the fact that no-one had spotted it before.

However, a little later I discovered that I had found the construction myself,…"

that is just absurd.

17. **Jeff M**
September 15, 2015

Alex Heller, who taught me topology my first year in graduate school, was a student of Eilenberg in the 40s and around while category theory was being developed. Used to drive me crazy in class, every proof was "the diagram commutes." That said, though Alex was a category theorist in some meaningful sense, even he used to call it "general abstract nonsense" which I believe was a quote from Eilenberg. It is possible to get to general. Then again, what Caramello is doing is somewhat different, Grothendieck had a very interesting take on things. Not that I'm an expert on that sort of stuff. I have crossed paths with Grothendieck in his work on dessins des enfants.

18. **Tony Smith**
September 15, 2015

As to "… Calculus II … textbooks … refusal to use Euler's formula …"
years ago when I was at Georgia Tech a physics PhD candidate friend of mine
was teaching a comparable calculus course using a book with no Euler's Formula
and
he took it upon himself to show how Euler's Formula made calculations easy and natural.
The students rebelled (it is not in the book – we don't want to know about complex numbers – complex number theory is not a prerequisite – …. etc …)
and complained to the head of the department.
My friend was admonished and told to apologize to the students
and to avoid all use of the devil's tool of complex numbers in the course.

Maybe that was not the isolated incident that I thought it was
and
maybe that is why publishers even today keep Euler's Formula out of such textbooks.

Tony

19. **Peter Woit**
September 15, 2015

Tony,
Hopefully that won't be the reaction of the students this time. The odd thing is that complex numbers are a topic in high school math, so is trigonometry. But explaining to students the relation between the two subjects is somehow off limits.

20. **Low Math, Meekly Interacting**
September 15, 2015

I was first introduced to Euler's formula in pre-calc in high school. It's about the only thing I remember clearly from that class, actually, because I thought my teacher had found God or something. Very few things that I can recall floored me like that did. It was also the first time my lack of innate skill in mathematics made me a little depressed.

21. **si**
September 15, 2015

I think that for more adventures students, specially if they have a computer software background, an introduction to the Chebyshev polynomials and their relationship to the trigonometry and de Moivre's formula may be interesting (this is actually how computers calculate the trigonometric and many other functions).

22. **cthulhu**
September 15, 2015

My high school (in the late '70s in the rural Midwest) had only trig as the highest math, and we barely got into complex numbers. Then in college (engineering school) I did math through PDEs, but we didn't talk much about complex numbers; I got more about complex numbers in my control systems class (Laplace transforms) than the math classes. But being something of a math and science nerd, some years ago I found Paul Nagin's book "An Imaginary Tale: The Story of sqrt(-1)", from Princeton Science Library Press; it is ostensibly a history of complex numbers, and quite entertaining at that, but it also has a lot of semi-rigorous derivations of things like Euler's formula, de Moivre's formula, and a lot more. Might be worth recommending to your students as a resource.

23. **Douglas J. Keenan**
September 16, 2015

"Euler's Formula and Trigonometry" notes that e^($i\pi$) = −1 and then says that the equation "relates three fundamental constants of mathematics".

I like the discussion in the Wikipedia article [Euler's identity](), which is based on the equation e^($i\pi$) + 1 = 0.

> Three of the basic arithmetic operations occur exactly once each: addition, multiplication, and exponentiation. The identity also links five fundamental mathematical constants: 0, 1, π, e, i. Furthermore, the equation is given in the form of an expression set equal to zero, which is common practice in several

areas of mathematics.

24. **srp**
September 16, 2015

I got the formula from Eric Temple Bell's Men of Mathematics, who rhapsodized over its astonishing combination of all the fundamental constants in math. But I never was exposed to it as something useful.

25. **Jeff M**
September 16, 2015

The question of exposure to the complex numbers is an interesting one, from what I can tell they have been largely disappeared from the curriculum, even for math majors. At my university, there isn't complex variable course, and realistically you could get through a whole major without really seeing them, except peripherally. I just checked, and Stewart's precalc book (a standard) does mention them, about 5 pages pretty early, then a few more when it does polar coordinates. It mentions DeMoivre. So students will probably see them then, for a minute, and might not again until grad school. I saw them first in high school, not because we got taught them, but because all the math nerds did it on their own. And when I took Calc in high school, we did do Euler. I also of course did complex as an undergrad (out of a very interesting book by Polya).

26. **AcademicLurker**
September 16, 2015

Huh. I don't know about Euler's formula, but we certainly learned about complex numbers, basic operations with them, and the idea of the complex plane in high school. Has all of that disappeared from the curriculum?

27. **Peter Woit**
September 16, 2015

Academic Lurker,
Complex numbers are taught in high school, generally in the context of roots of polynomials. Euler's formula is about the exponential function, and essentially says that you can extend it from the real numbers to the complex numbers, preserving the property that exponents add when you multiply. The remarkable, beautiful fact is that when you do this you get trig functions: the exponential of an imaginary number is a complex number with real part the cosine, imaginary part the sine.

28. **Low Math, Meekly Interacting**
September 16, 2015

Where I went to school, i was all about factoring, as I recall, and that was all we were tested on. I found the exercises chugging through complex conjugates, etc., very tedious. Euler's formula was just an aside, alas. We were wrapping up the complex numbers unit, and he pulled it out of his hat, so to speak, during the last lecture. We never had time to learn what it was "good for". I think we were just meant to be amazed at how raising e to the i*pi power gives you -1, of all things, and he walked us through the proof to hammer it home. I was duly amazed.

29. **MB**
September 16, 2015

Nice notes on Euler's formula. There's another typo at bottom of page 9, in the last point where you're deriving the orthogonality of the exponential functions: you need minus signs in various places. Either you want n -> -n in the exponentials or in the conditionals and the text below.

30. **Peter Woit**
September 16, 2015

MB,
Thanks! Fixed.

31. **G.S.**
September 16, 2015

Peter,

You say "One fundamental problem (besides that of not having a crucial, powerful tool) is that without using Euler's formula, there's no sensible way of understanding why the addition formulas (and thus most trig identities) are true."

What's wrong with drawing the picture? You can quickly derive both addition formulas by drawing a couple of right triangles and using the definitions of cosine and sine. https://en.wikipedia.org/wiki/Proofs_of_trigonometric_identities

In my experience teaching college math and physics, the students are already struggling to remember the definitions of basic trig functions (hence SOH-CAH-TOA). Introducing complex numbers, rules for multiplying and conjugating them, and another formula (Euler's) for memorization is probably going to overwhelm them.

Then again, the better students in the class will probably love it. I know I was amazed when I first learned about Euler's formula. It was an episode of the Simpsons. It flew by Homer after he crawled through a portal to the "hypothetical" third dimension.

32. **AcademicLurker**
September 17, 2015

@Peter: I meant that while I remember complex numbers being covered in high school, I can't recall whether Euler's formula specifically was covered. I suspect that, as LMMI mentions, it might have been mentioned as an aside and so didn't stick.

My first clear memory of Euler's formula being presented in a way that made clear how useful and cool it is was in the Vibrations and Waves section of one of my early university physics courses.

33. **Peter Woit**
September 17, 2015

G.S.,
Would any mathematician who needed to derive the addition formula do it using that triangle argument, or would they use Euler's formula (as the page you link to also does)? Why use a non-obvious argument about triangles when there's an obvious one using exponentials?

As Academic Lurker points out, the other reason students should know Euler's formula is that it's crucial for solving ODEs, including the basic ones used in physics to describe wave motion. If it's needed for a second year physics course, it should be taught early on in the math curriculum.

I was wondering how our ODE textbook deals with this and just took a look at the book for our basic math ODE class. They use Euler's formula, don't assume students know it, put the explanation for why it's true in a footnote. The idea that that's the way math students should encounter this subject is kind of appalling.

34. **Dato**
September 17, 2015

So I specifically went to the bookshop to take a look at the book for 12th grade ("première enseignment secondaire classique"):

The formula

It's a bit short, in a "just learn this" kind of way, but it's there. I do remember that our *professeur* ~30 years ago enthusiastically showed us that it was indeed an extension of the exponential over the reals and we went on from there.

35. **Jeff M**
September 17, 2015

Well, really, the best way to teach the various trig identities is to use the Taylor series 🙂
Peter is right, it's depressing that this has been dropped from the math curriculum (in large part), especially since calc books have added so much useless garbage. I learned calculus first out of Sherwood Taylor, my dad's book from City College in the early 50's, it was 5 x 9 and maybe an inch and a half thick, missing most of what's in current calculus books, but somehow they included stuff like Euler, and actual proofs.

36. **vmarko**
September 17, 2015

I'm a little surprised by this whole discussion about Euler's formula. The way I learned math in the first-year calculus course at my University (as a physicist) is more or less the way math students learned it as well — one introduces the exp function in a complex plane via its power series, prove convergence, basic identities etc, and then go on to introduce both trigonometric and hyperbolic functions (also over the full complex plane) by splitting exp into real and imaginary part, and evaluating the corresponding series on the real line, imaginary line, etc. The Euler formula is then actually a part of the definition of sin and cos.

The interesting question then is what do these functions and their power series have to do with triangles and planar geometry. This connection is then being made in a nontrivial manner, by studying the relationship between the complex plane and the real plane, and the map between U(1) and SO(2)…

But I was generally under the impression that everyone learns math in this way in a university course. The "elementary" trigonometry was something one learns in high school, but that any serious calculus course (university level) would introduce all elementary functions more rigorously, as a suitable power series in a complex plane, etc. So I am surprised that this is not the curriculum at every university out there.

Also, the Euler formula, despite its elegance, is not universally powerful in proving identities in trigonometry. For example, the famous identity

A sin x + B cos x = \sqrt(A^2 + B^2) sin (x + arctan B/A),

is a bit more tough nut to crack using Euler formula. 🙂

HTH, 🙂
Marko

37. **sm**
September 17, 2015

vmarko:
Depends what you mean by "using Euler's formula". Simply by considering (Asin(x)+Bcos(x))/sqrt(A^2+B^2) we have (as the mathematicians say) reduced to problem to the case(s) already proved by Euler's formula.

38. **Low Math, Meekly Interacting**
September 17, 2015

I did the same first year calc in college that all the other science majors had to take, including the physics and math majors (if they didn't place out, which was rare, of course). Complex numbers barely came into it. Neither Euler's formula nor the famous Identity were even mentioned, if I recall (and I would have, since the lattered bordered on mystical to me).

Well, to me the argument for including it early is simply that it's amazing. I know, I know, it's also terribly useful. But is it gauche to simply marvel at it? Generate a little more excitement for a subject that many find intimidating without having to resort to gimmicks, like they do in my niece's high school? Sorry, OT probably. I'll shut up.

39. **G.S.**
September 17, 2015

Peter,

Every mathematician before Euler used arguments about triangles to prove those trig identities. While it's true that I use Euler's identity today to quickly derive trig identities and perform integrals involving trig functions, that method is only "obvious" to me because I've spent the last 20 years using complex numbers. I agree that they should learn it, but I question whether introducing it in Calc 2 is wise. I think you'll end up confusing more students that you end up helping.

This whole thing actually reminds me of my own experience TA-ing a lower level physics class in grad school. I was always appalled that physics books simply state the solution to the simple harmonic oscillator differential equation as a linear combination of sine and cosine (or as complex exponentials). You can prove that it works, but there's never an explanation for how that solution was guessed in the first place. It's usually chalked up to "experience" that the students don't have.

The only method they had seen up until then for solving a differential equation was to try a power series. So, away I went. I spent a discussion session showing that the power series solution that you get ends up being a linear combination of the power series for sine and cosine (which they had memorized from a previous class). Then I showed that this can be equivalently written as a linear combination of the power series for complex exponentials with opposite sign (by proving Euler's identity from the power series).

The A students liked it, but most of the B and below students (the ones who could most use the discussion session) stopped attending after that.

40. **Peter Woit**
September 17, 2015

G.S.,
I've taught Calculus II several times before, and often tried saying something about complex numbers and Euler's formula. Some students get it, some don't. The big problem is not that this is too hard for them, but that it's not in the book, so they don't have a good source outside of class to learn from. This year I tried to do a bit better on that with the notes I wrote, but that can't replace the book.

The problem with saying "don't teach this in Calculus II" is that there's no where else we do teach this (I checked, it's not explained in the ODE book). So, many students either leave here with several years of university mathematics education, but not knowing this, or they have to pick it up in the gutter (their physics class on wave phenomena….)

41. **Miki**
September 17, 2015

Quick application and mastering Euler's formula is taught to every sophmore electrical engineer, as the practical method to represent AC currents, and more generally Narrowband Stochastic processes Every cellular modem works on the "I and Q" representataion of the received Signal + Noise.
It is so common like using a calculator, that probably nobody remembers that Euler invented it more than 300 years ago…..

42. **vmarko**
September 17, 2015

sm,

The standard way to prove the formula I quoted is to use addition formula for the sine on the right-hand side, and then use the identities expressing sin and cos in terms of tan, reaching the left-hand side after a little algebra. That is the standard trigonometric proof.

My point was that there is no simpler way to prove it, say by using Euler formula but without invoking addition formula and sin-to-tan, cos-to-tan identities. So Euler formula is not an all-powerful magic bullet for everything in trigonometry. You can use it to simplify proofs of *some* theorems, but by no means *all* of them. Some things are just equally hard, whether you use Euler formula or not. 🙂

That said, I'm still completely perplexed to hear that it is not the main part of the standard curriculum at universities. I learned it as a central piece of complex analysis — second year undergraduate calculus course (both for math and physics majors). Somehow it seems odd that the University of Belgrade, Serbia, has a stronger undergraduate calculus course than Columbia University, New York. If that's really the case, I'll never suggest to any future student to ever go study at CUNY, since its math curriculum is just sub-par.

Best, 🙂
Marko

43. **Anonyrat**
September 17, 2015

Math 1a is the basic mathematics course for all Caltech freshmen.
The curriculum is here:
http://www.math.caltech.edu/~2014-15/1term/ma001a/
Lecture 23 is complex numbers (lecture notes PDF: http://math.caltech.edu/~nets/lecture23.pdf)

There was a major revision in undergraduate mathematics requirements in 2012, the detailed outline is here (PDF): http://cue.caltech.edu/documents/22-core_with_tables_appendix.pdf

e.g.,

> Changes for students entering in fall 2013:
> 1.Reduce the requirement in mathematics to 27 units from the current 45 units.

You may want to read through the document, e.g., later on:

> The transition from high school to college presents problems for all students, but for some students it is particularly challenging. At Caltech, many newly admitted students lack the background in mathematics that is necessary to succeed in Ma1a. Unfortunately, few of them are even aware that their background in mathematics is deficient. This is not their fault. The mathematics curriculum in high schools is less rigorous than it was even a few decades ago.
>
> In conversations with Caltech students who have struggled with freshman mathematics, most report that they were star math students in high school, which of course is a major reason why they were offered admission to Caltech in the first place.
> Many of them, however, have never seen mathematics as it is taught at Caltech.

The specific problems have been identified and are made up with a at-home online course: Transition to Mathematical Proof (TMP) course, for incoming students.

The TMP outcome is supposed to be:

> Incoming freshmen who successfully complete TMP will be able to:
>
> * Write simple but logically correct proofs that utilize appropriate terminology and notation.
> * Understand various proof methodologies, including direct proof, proof by contradiction, proof
> by contrapositive, and induction.
> * Manipulate sets using the various set-theoretic operations and theorems.
> * Compute the cardinality of various classes of sets.
> * Prove when a function between sets is injective, surjective, and bijective.
> * Prove or disprove that a real-valued function is continuous.
> * Use the definition of the derivative to prove various differentiation properties.
> * Use various differentiability and continuity theorems in proofs beyond calculus.
> * Prove when a sequence converges or diverges.

44. **Jeff M**
September 17, 2015

Perhaps a good way to get a handle on what has happened as far as student's preparation in high school is to consider Spivak's calculus book. As I understand it (one of my professors in college was good friends with Spivak at Princeton, so I got various stories) he wrote it basically from his teaching notes for honors calc at Brandeis in the mid 60s. You would have trouble using it in a junior level analysis course now. He proves e is transcendental! It's one of the prettiest books ever written, but no one is close to ready for it, certainly not as freshmen. Hell, in Calculus on Manifolds Spivak says all a student needs is one year of calc and one semester of linear algebra. In 22 years I've had one student who could handle that book as an undergraduate, and she's a math professor at Carleton now.

45. **Ru**
September 17, 2015

I learned about Complex number in grade 12.

We used something called cis(x) = cos(x) + isin(x)

you get the idea. We just didn't call it Euler's formula.

It made things much easier.

46. **Thomas**
September 18, 2015

Dear Peter,

The idea of your book seems very similar (although, of course, completely different in style) to this book http://www.springer.com/us/book/9783319192000, also published by Springer. Have you already read it? If yes, would you recommend reading it before or after reading your book (or not at all)?

47. **sm**
September 18, 2015

vmmarko:

"My point was that there is no simpler way to prove it, say by using Euler formula but without invoking addition formula and sin-to-tan, cos-to-tan identities."

And my point was that it is just a rewriting of the addition formula, which has already been elegantly proved by Euler.

Are you complaining that it is cheating to use cos(arctan(B/A))=A/sqrt(A^2+B^2), which follows using scaling to set sqrt(A^2+B^2)=1 and Peter's unit circle – no more really than was already put into Euler (from the geometrical point of view) in the first place?

Long live Euler!

48. **Paul Vetter**
September 18, 2015

Dear Peter,

Longtime lurker —
Thanks for the interesting notes on Euler's formula — I will share with my children, who are in geom/trig and calculus in school. One topic that this always raises for me, though, is WHY e and pi are transcendental. There seems to be little material available for interested laypersons. As I understand it, pi is transcendental because e is, and they are both transcendental because they have something to do with what we mean about having multidimensional space with directions orthogonal and how it is we rotate. Can you suggest good reading about why pi must be transcendental and why it is that it has the value of about 3, (order 1) rather than 0.1 or 1000?

49. **AcademicLurker**
September 18, 2015

they have to pick it up in the gutter (their physics class on wave phenomena….)

It wasn't easy learning Euler's formula on the mean streets, but it toughened me up.

50. **Narad**
September 18, 2015

> Perhaps a good way to get a handle on what has happened as far as student's preparation in high school is to consider Spivak's calculus book.… You would have trouble using it in a junior level analysis course now. He proves e is transcendental! It's one of the prettiest books ever written, but no one is close to ready for it, certainly not as freshmen.

I almost mentioned this earlier, but I don't think it invokes Euler's formula. My copy (second edition) is around here somewhere, but I was lazy and just looked what was online.

And yes, we used it as freshmen ('84). My high school didn't offer calculus, so I took it by correspondence course from the University of Wisconsin Extension. This was enough put me in the honors calc tier and do it all over from scratch. (Nonetheless, I didn't qualify for honors analysis with a certain eyepatch-wearing fellow the next year; the substitute was not pretty. I'm not sure what honors analysis actually used, but I tried working my way through Apostol on my own.)

51. **steve newman**
September 19, 2015

re-"why pi must be transcendental and why it is that it has the value of about 3, (order 1) rather than 0.1 or 1000?"
the value of about 3 comes easily from the definition of pi as the ratio of the circumference to the diameter of a circle. The perimeter of a square circumscribing the circle is 4 x diameter and the perimeter of an inscribed square is 2 x square root of 2
x diameter. The circumference of the circle is greater than 2.83 and less than 4 times
the diameter which puts pi somewhere near 3. (if you look at inscribed and circumscri bed hexagons you get pi between 3 and 3.46).
why pi must be transcendental is not so easy.

52. **herman claus**
September 19, 2015

maybe already said : of the "Euler's Formula and Trigonometry, " pdf : page 5 on top, 6th line (3th line of the expansion) is wrong : you forgot a pair of brackets ;
Nice PDF, thanks for that

53. **Peter Woit**
September 19, 2015

Ru,
Yes, one can define "cis" that way and show that it has all the properties of an extension of the exponential to pure imaginary arguments, and that's a common strategy. Still, once one has done that, I don't see why one shouldn't just drop the "cis" notation and use the exponential notation, which will be much simpler to work with.

Thomas,
Thanks I hadn't seen that book. It does have some similar goals to what I'm writing, and there's a significant amount of overlap. I'd describe my version as going for more mathematical depth, which is why it's about twice as long. Also, that book sticks to the fairly conventional Lagrangian symmetry story of most physics textbooks, while I'm trying to emphasize parts of the story that aren't in such books, including paying much more attention to the Hamiltonian point of view.

herman claus,

Thanks. fixed.

54. **Richard**
September 19, 2015

Anonyrat wrote

> Math 1a is the basic mathematics course for all Caltech freshmen. …
>
> There was a major revision in undergraduate mathematics requirements in 2012 …
>
> The transition from high school to college presents problems for all students, but for some students it is particularly challenging. At Caltech, many newly admitted students lack the background in mathematics that is necessary to succeed in Ma1a. …

As somebody who was accepted to Caltech as an undergraduate from overseas in the 1980s, this sounds incredible. At the time I could scarcely believe the type of remedial-level mathematics (and physics, and chemistry, for that matter) on which the best and brightest from the finest US high schools wasted a year and more of tertiary education. The Euler Formula that Peter Woit's students are encountering for the first time should surely have been year 9 or 10 high school material for any student destined for a tertiary education in any quantitative field.

That the situation might be even worse today is both hard to understand and dispiriting to consider.

Are things as bad outside the USA? Are "four year" tertiary degrees also becoming two year programs on top of two years (or worse) of remedial secondary school material elsewhere in the industrialized world?

55. **Peter Woit**
September 19, 2015

Richard,
The situation with the level of math and science instruction in US secondary schools is bad, but I don't think any worse now than in the 1980s, and in some ways better. As far as I can tell, the US high school math curriculum right now is more or less identical to what I experienced forty years ago in the early 1970s (and so is the set curriculum for calculus classes, whether offered at the end of high school, or beginning of college). On the positive side, many more US high school students are taking calculus classes in high school.

But, with Euler's formula not even in the calculus textbooks, US students are still highly unlikely to see that in high school.

56. **Doug McDonald**
September 20, 2015

I looked at my own freshman math book, from 1962 "Thomas", which apparently still has nominal successors in print. Euler's formula is there, quite prominently, though near the end. There is an online version of a near-recent successor, and its still there.

I taught for many years junior level quantum mechanics to chemists, and we used that formula a lot; the students were not bothered by it. What they were bothered by was the idea that they had to generate even an obvious three line derivation or proof. We never of course asked for mathematical rigor.

57. **Oiler**
September 21, 2015

It's great that you passed on your Euler notes; that kind of commitment to teaching is too often missing!

I teach at a university somewhere outside the US, and I always work in either Euler's formula (almost exactly as you present it!) or the 2×2 matrix version or both as soon as possible. The matrix version allows me to make the linear transformation/matrix link. This is in Calc 1,2,or 3: because it is guaranteed they haven't seen it either in high school or with any earlier prof.

I use the power series definition for exp, but heuristically (and if they are interested,
I will explain the convergence). I hand out a little resume of the material to them since no book has it. I also draw a graph of the function e^(it): \R\to\C
and explain that looking at this "Slinky" from the side gives you sin and cos.
And to draw sin, I set my arm in "harmonic motion" and walk along with time. And: there is always some mention of music synthesis, as a motivation.

This is just one example of similar missing material. As a student I had many moments of frustration/fury when I realized some obvious explanation had been left out. almost as if the secrets were being kept within the guild.

Somebody mentioned Spivak and old editions of Thomas. Both I have warm memories of for completely different reasons. Spivak is simply beautiful and is a great backup for the prof, but is only good for that rare student who wants to really know what is going on and is ready to learn on their own. Old Thomas has some great word problems as I recall, although it is too dry for self-study.

Back to Euler, when I teach power series (calc level) I use that to treat Fourier series
-proving orthonormality, and writing the formulas, is so much easier than with sin/cos-and then I make the Fourier series- Taylor/Laurent series link. This is done not too rigorously: mainly just to blow their minds- as it did for me when I realized that–in grad school!!!–In other words, it gives them a good advertisement for what is to come, and a taste of why math is at once so cool and so powerful and so simple.

58. **Richard E**
September 21, 2015

Does anyone have links to talks on the Nima's N-naturalness? Am curious to understands its implications for the thermal history of the early universe.

59. **Dave Miller in Sacramento**
September 28, 2015

Peter,

Maor suggests that the idea of the exponential function first arose from the concept of compound interest and the question of what would happen if the compounding became instantaneous.

I was a young kid when the banks started decreasing their compounding periods back in the '60s, and I remember hoping that I would get incredibly rich as they reduced their compounding periods to zero!

Of course, my hopes were dashed because the limit of (1 + x/n) to the nth is just exp (n), which is, alas, a finite limit for any real x.

You can in fact easily derive the power series for exp(x) from this by just taking the binomial expansion of (1 + x/n) to the nth and taking the limit n -> infinity. This easily shows that the limit is finite.

If you then plug in an imaginary number, you get the limit as n goes to infinity of (1 + i x/n). For n very large, it is easy to see that one is taking a small angle close to x/n and "compounding" it n times to get to the angle x. (A bit of thought shows that in the limit n goes to infinity, the radius remains at 1: e.g., look at the binomial expansion for (1 + i x /n) times its complex conjugate all to the nth..) Using the binomial expansion to get the power series of course works in exactly the same way as in the real case.

All this does not meet the mathematician's standards of rigor (although I suspect it can be "cleaned up" as easily as any other approach). However, if Maor is right, it may be close to the historical development. And, multiplying (1 + i x/n) by itself n times,. is a somewhat intuitive way of reaching the angle x (in the limit n goes to infinity of course) on the unit circle.

Dave

60. **Michael Mueger**
September 30, 2015

This is a comment to your notes on Euler's formula.

Do you know the book `Numbers' (multiple authors, edited by J.H. Ewing), Springer GTM 123? Among many other things, it has a very nice treatment of complex numbers, exponential function, trigonometric functions and, of course, pi. One might even find that treatment a bit too comprehensive.

61. **Peter Woit**
September 30, 2015

Michael Mueger,
I know the book, hadn't looked at it recently. It is quite wonderful, and the chapter on "pi" has an excellent treatment of the issues.

62. **Step Maths**
October 10, 2015

I've just taught Euler's theorem and De Moivre's theorem to my A-level Maths class and I wanted one of my brightest students to do some independent study on how to use it to integrate and differentiate products and powers of trigonometric functions (as extra reading) – I'm going to share your document with her. Thanks 🙂 @Olier – do you know of any resources / books which describe how to use Eulers formula for Fourier Series? That might be good extension material too!

Connes on the Riemann Hypothesis

September 21, 2015
Categories: Uncategorized

There's a fascinating new preprint out from Alain Connes, called An essay on the Riemann Hypothesis, written for a volume on "Open Problems in Mathematics". Evidently the late John Nash is an editor, and responsible for commissioning this piece.

Connes is a mathematician of the first rank, and a very original one at that. He has now struggled with the Riemann hypothesis for many years, and his account of various approaches to the problem and the state of efforts to pursue them is a remarkable document of a sort that too rarely gets written.

Much of what he is concerned with is the question of how to find a proof along lines related to those used to prove the analog of the Riemann hypothesis in the case of function fields (this was successfully carried out by Deligne in the early 1970s). James Milne has a wonderful expository piece on the topic of this proof, going into details of the history and the mathematics. It provides a great supplement to the more speculative article by Connes.

For something much more concrete about the Riemann hypothesis, there's a new book by Barry Mazur and William Stein, Prime Numbers and the Riemann Hypothesis. Among a long list of attempts to relate this to physics, there's an interesting relatively recent discussion of one idea from John Baez.

Comments

1. **John Fredsted**
   September 22, 2015

   Thanks for the link to the book by Barry Mazur and William Stein. Browsing the available PDF of the book, it strongly seems to me that this is a book I just have to buy when it arrives in the stores. What I like about it is its, at least apparently, accessibility for non-mathematicians (I am myself a physicist) and people not experts in the field of the Riemann Hypothesis.

2. **a l**
   September 22, 2015

   I can't resist to quote:
   Physics of the Riemann Hypothesis arXiv:1101.3116

3. **Abdelmalek Abdesselam**
   September 22, 2015

   A little less known (e.g. not mentioned in reference by a 1) connection to physics and in particular QFT is that the Riemann Hypothesis is equivalent to the existence of a 1d QFT with specified translation invariant 2-point function. See page 6 of "The explicit formula in simple terms" by Jean-Francois Burnol
   http://arxiv.org/abs/math/9810169
   or the earlier paper http://arxiv.org/abs/math/9809119
   Here it is QFT in the Euclidean formulation and the main requirement is not unitary or Osterwalder-Schrader positivity but rather Nelson-Symanzik positivity which makes the model an honest probability distribution on D'(R) or D'((0,infinity)) if one wants to use the log or not.

Visions of Future Physics

September 22, 2015
Categories: Uncategorized

There's a great profile of Nima Arkani-Hamed by Natalie Wolchover just out at Quanta magazine, under the title Visions of Future Physics. I recently linked to another profile of him from the IAS, which covers some similar ground.

He's often been a topic of postings here, and the profile explains why, with his colleagues describing him as the "messiah", "Pied Piper" and "impresario" of high energy physics.

> "He keeps coming up with the goods, and his persuasiveness is hypnotic," said Raman Sundrum, a theoretical physicist at the University of Maryland in College Park, "so a lot of people follow where he leads."

I've often marveled at his performances, with his talks sometimes a unique mixture of brilliance, insight, and over-the-top outrageous indefensible claims (his talk here last week was uncharacteristically restrained). As an example of the genre, the IAS profile includes:

> "It is extremely interesting to think about getting sophomores up to the speed of a second-year graduate student. I think it is possible," says Arkani- Hamed.

which is simultaneously quite inspirational and, well, nuts.

A couple years ago I was struck by a talk of his in which he showed a lot of self-knowledge, describing himself as an "ideolog" (see here). There's more about this in the Quanta profile:

> "It's important for me while I'm working on something to be very ideological about it. And then, of course, it's also important after you are done to forget the ideology and move on to another one."

The ideologies on display this time include a very speculative picture of a future union of mathematics and theoretical physics:

> Ultimately, he said, anywhere from 10 to 500 years from now, the amplituhedron and these cosmological patterns will merge and become part of a single, spectacular mathematical structure that describes the entire past, present and future of everything "in some timeless, autonomous way."…
>
> There is a mathematical proof, Arkani-Hamed observed, that all algebraic numbers can be derived from configurations of a finite whole number of intersecting points and lines. And with that, he expressed a final conjecture, at the end of a long, cerebral day, before everyone else went home to bed and Arkani-Hamed headed to the airport: Everything — irrational numbers, along with particle interactions and the correlations between stars — ultimately arises from possible combinatorial arrangements of whole numbers: 1, 2, 3 and so on. They exist, he said, and so must everything else.

Personally, I don't think this is going to work out, but he's right that people need a vision to pursue, to drive them forward in finding new things. Unless he gets a lot further with it, I don't think this one is going to get so much interest as to drive out other ideas, especially from mathematicians interested in physics, who have other competing visions.

Where Arkani-Hamed has become a really problematic ideolog, one endangering the health of the subject, is in his insistence on "naturalness" as the central question of HEP theory at the TeV scale, coupled with the ideology that if the LHC doesn't see new "natural" physics at the TeV scale, then the intellectual suicide of the multiverse is all HEP theory has to look forward to. He's been pushing this ideology, hard, for quite a while now, and I think it's long past time for him to take his own advice and "forget the ideology and move on to another one."

Much of the article is about his efforts to push forward a Chinese plan to build a next-generation collider. Perhaps his great enthusiasm will help move this project along (a book about it by Yau and Nadis, From the Great Wall to the Great Collider, will soon come out). It raises all sorts of difficult issues for the future of experimental HEP, including that of the future of CERN, issues that will play out over many years (timelines for things like this are generally wildly over-optimistic, and here people are talking about 2042). Framing the case for a 100 TeV machine as "1% fine-tuning evidence for the multiverse from the LHC wasn't convincing, even though we said it would be, so we need a bigger machine to get .1% fine-tuning evidence" is something that I think isn't going to fly, no matter how enthusiastically presented. In the article Kyle Cranmer makes the point that .1% tuning versus 1% tuning means little:

> "I am very sympathetic to the idea that this is a critical point in the field and that naturalness/fine-tuning is a deep issue," he wrote in an email. "However, I'm not convinced that if we built a 100-TeV collider and saw nothing that it would be conclusive evidence that nature is fine-tuned." There would remain the nagging possibility that a natural completion of the Standard Model exists that a collider simply can't access.

and Jester

> argues that if no new particles are found at 100 TeV, this will leave physicists exactly where they are now in their search for a more complete theory of nature — clueless.

I think David Gross has it right:

> Gross, who considers naturalness a murky concept, simply wants a last-ditch search for new physics. "We need more hints from nature," he said. "She's got to tell us where to go."

The case for mankind to embark on a new project to push forward the boundaries of science is the same as it has always been: even though it's expensive and difficult, we should do it because we'll see how the world works at an even smaller distance scale. Just possibly we'll learn enough to understand how to improve the Standard Model and achieve an even deeper insight into the physical universe.

Update: Gross and Witten have an editorial today in the Wall Street Journal (as usual with the WSJ, try Googling the article to get around their paywall), supporting the idea of a Chinese Great Collider project.

Comments

1. **Magnema**
   September 22, 2015

   I mean, technically, last year I was a sophomore and was taking a class that was typical of a second-year math grad student at my institution (namely, manifolds). It was difficult (although a part of that is because I'm more of a physics person than a math person), and I was an unusual case to boot. However, with more background support structure, I would think it intellectually plausible on behalf of (smart) students, albeit that the investment into education is likely larger than what the general population is willing to commit…

2. **Peter Woit**
   September 22, 2015

   As a sophomore in college I also took a second-year level graduate course (quantum field theory). Then, and even more so now, I'd describe the suggestion that I (or any other similarly ambitious undergraduate) was operating at the level of a typical second year Harvard grad student as, well, nuts.

   To be honest, I have no idea what Arkani-Hamed is actually talking about here. Presumably he has some more specific idea in mind about certain skills needed in a 100 TeV collider project, but I have no idea what they are. I do strongly suspect though that, whatever it is he has in mind, inspirational as well as delusional likely is an accurate way of characterizing it.

3. **Peter Woit**
   September 22, 2015

   And, by the way, I don't think he'd necessarily disagree. "Inspirational and delusional" characterizes what it is to be an ideolog (at least a good one), and he's well aware that's part of his nature.

4. **chris**
   September 23, 2015

   I've had a sophomore in the QFT class I was teaching last year and I can confirm that it is nuts. What I fear Arkani-Hamed is going for is a "physics" education that skips the actual physics and goes right for the stringy multiverse thing. Classical mechanics and wave guide exercises in E+M are for the old generation and totally useless – we start with an arbitrary dimensional supersymmetric membrane…

   Also, I find the political lobbying for the next hadron collider immoral. Come on, we all know that the chances of a factor 5-10 in energy getting us anywhere are slim. That's what a skeptical look at the current situation tells us. Should we sink billions into an almost hopeless endeavor for political reasons or rather pause a moment and use that money for finding other ways of accessing much larger energy scales?

5. **Bee**
   September 23, 2015

   It's like watching a public case study on chronic caffeine overdose 😉

6. **srp**
   September 23, 2015

   There is another moral and practical issue for the collider project. China is an unfree country that is unfortunately becoming less free in recent months as Xi imposes his new security law (that makes science, culture, and journalism emphatically security concerns–basically everything is security) and cracks down on Western ideas (to the extent of trying to get rid of foreign textbooks in universities, etc.). Moreover, there is evidence that the Party is very concerned about long-term stability and its ability to keep control over society. A long-term transfer of the center of gravity of particle physics into such an environment does not seem wise to me. This project isn't like putting on an Olympics, where journalists mostly have to be contained for just a couple of weeks.

   One could, of course, argue that swarming the country with cosmopolitan international physicists would exert a salutary influence on the political/cultural situation. To me the downside risk seems more compelling than the upside opportunity but I'd be happy to be convinced otherwise.

7. **Michael Hutchings**
   September 23, 2015

   As a naive outsider, I would think that a machine with 10 times the energy of previous machines would have a good chance of seeing something new. I don't know why chris writes that the chances are slim. But the enormous resources that this would require are probably better spent on something else.

8. **Frank Quednau**
   September 23, 2015

   "There is a mathematical proof, Arkani-Hamed observed, that all algebraic numbers can be derived from configurations of a finite whole number of intersecting points and lines"

   May I ask what this refers to and how I can find more information about it?

9. **Scott Church**
   September 23, 2015

   It's only a hunch… who can say…? But unless I'm missing something it seems to me that while increasing collider energies of 100 Tev or more may reveal some new physics, as things stand today vis 'a vis the SM and/or M-theory we have no real reason to expect any until we can probe at something on the order of the Planck scale. That will require either radically new detector technology (and I mean *radically* as in unlike anything we can currently conceive)… or a collider roughly the diameter of the solar system (with all the attendant luminosity and detection issues overcome). Anyone care to speculate how we're going to get *that* funded and built? 😉

   And if not… are the billions we're liable to spend trying to bridge the intervening gap really anything more than a Hail Mary? It seems to me we need some new ideas… *New* ideas.

10. **John**
    September 23, 2015

    Isn't being an ideologue about the same as being a true believer? You position is not swayed by facts but instead you search and cherry pick results you like to support your position. To me this is the exact opposite of what a scientist is defined as.

11. **Jan Reimers**
    September 23, 2015

    Can someone comment on calculating backgrounds between 10 and 100TeV. In particular
    1) Is the data useless if we can't calculate the standard model background between 10-100TeV?

2) Can we reasonably expect to calculate the high particle count (i.e. 2 in –> N out diagrams where N>6) cross sections given that the Black Hat team came to rescue with 2->4, 2->5 gluon cross sections at the last minute before the LHC started.
3) Or am I miss-understanding the history?

Thanks
JR

12. **M**
September 23, 2015

«Whatever the answers are, here's one monkey that's going to keep on climbing, and looking around him to see what he can see, as long as the tree holds out». 99.9% of the world annual GDP is a byproduct of scientific progress, and a 100 TeV collider costs only 0.01% of it

13. **Peter Woit**
September 23, 2015

Michael Hutchings,
The 100 TeV collider proposal is different than the LHC in the sense that with the LHC we knew that it had to see the Higgs or something even more interesting. There's no similar argument for the next order of magnitude in energy. So the case for the next jump is harder to make. I think though the case can be made, the fact that the LHC case was a slam-dunk and this one isn't shouldn't matter.

If we do go to 100 TeV, maybe we'll see what the SM predicts, maybe we'll see something new and unexpected. The only way to find out is to look, and I think it would be a shame if humans decided it wasn't worth looking, better to spend the money on some aircraft carriers. You could reasonably argue that we should save the money, and theorists can try and figure it out by pure thought. So far, trying to get beyond the SM by pure thought hasn't worked out well.

The standard arguments about this are usually I think very naive, with those in favor saying "better than weaponry" (like my argument above…), those against saying "cure cancer instead" (or, essentially, "spend the money on me and what I do"). One thing to keep in mind is that while this is a very expensive project, it's also one with a very long time scale, so the budget needed per year may not be out of scale with what is already being spent. We're talking about modest spending shifts on the scale of budgets like the US,EU, China. It's not at all clear what would get crowded out by a Chinese decision to devote part of its economy to a big project like this, instead of what people would otherwise be doing. Fewer aircraft carriers is a real possibility…

John,
I think Arkani-Hamed is making the case that you need to be a true believer to devote your life to this kind of work. There's something to that, but it becomes problematic if you go beyond convincing yourself to keep going on a project that may not be working, and start convincing other people that something is working when it really isn't. The question of when and how you decide to admit that something doesn't work and move on is a huge problem for HEP theory now, and I don't think he's addressing that one, even while he acknowledges that you have to do this at some point.

Jan Reimers,
I assume that those kinds of questions are just what Arkani-Hamed's Chinese institute is supposed to be addressing. As mentioned above, the time scale for such a machine is very long, lots of time for improvements in background calculations. Especially with all those sophomores doing the work of graduate students…

14. **Michael Hutchings**
September 23, 2015

I don't know what the budget is like for the 100 TeV machine; the book should be interesting. Anyway I would hope that we could build it sometime, and probe as far as we can, since it is our nature to do so. My argument against building it for now, call this naive if you will, is that we have some urgent problems to solve in order to ensure the long term survival of human civilization, and the latter seems to be a prerequisite for really big science projects. For example, there is a lot of activity in China to develop "green", "sustainable" technologies (out of acute necessity), and this is something that might get crowded out.

15. **paddy**
September 23, 2015

At the risk of sounding like a Philistine: When does advocating for a 100 Tev collider become also a "spend the money on me and what I do"?

16. **Daniel Tung**
September 24, 2015

"Personally, I don't think this is going to work out…"
May I know why?

17. **Yatima**
September 24, 2015

When does advocating for a 100 TeV collider become also a "spend the money on me and what I do"?

When it becomes a private venture? "Shut up and take my dollars" applies. Of course, one can still apply for subsidies at *L'Etat providence*.

But I would like to see a [muon collider] and the [International Linear Collider] first.

18. **Radioactive**
September 24, 2015

We should build a collider orbiting the earth if it would add a tenth of a percent to the sum of human knowledge. Worrying about budgets is for accountants. I could give a damn what is politically feasible at any given moment as long as it's physically feasible.

19. **chris**
September 24, 2015

Michael Hutchings,

it is essentially as Peter explained: there is no "slam-dunk" case for anything popping up below the GUT or Planck scales, which are at least a further factor 10^10 away. Note that Peter, contrary to all that his string theory enemies say, is a real "optimist" here. A "pessimist", like myself, sees the standard model as a complete theory up to essentially the GUT scale, because all current evidence (GUT scale, right handed neutrino scale, instability scale of the Higgs potential and Planck scale) points at that.

It's a good question what conclusion to draw from this. As Peter pointed out, it is very unlikely that money not spend on a future hadron collider will be used in any more sensible way. And while we might not find new physics, the simple benefit of keeping the technological knowledge alive might very well be worth the effort. What I am worried about though is that hyping this collider for more than it will probably be is very dangerous. Scientists should state clearly that the chances to find new physics are at best unknown and it would not be a surprise if such a machine just pushed limits. In this respect, one could think of lobbying rather for either building another collider (e.g. CLIC or a muon collider) or for investing the money into accellerator technology research or alternative experiments.

20. **JohnB**
September 24, 2015

Given the enormously successful history of particle accelerator discoveries, right up to the LHC, isn't the right approach to experimentation, to just keep on building bigger and bigger accelerators (lets say 10x as powerful as the previous one, if there are no solid predictions for new particles), until a project finally fails and nothing is discovered?

If the available beyond-SM theories are inadequate, all you've got left is experimentation – and once experimentation fails (still doing more than well enough there, to justify a new generation of accelerators…), the primary focus will have to shift back onto applied-physics/material-development, aimed at developing accelerator tech, that is a couple/few orders of magnitude better than the last failed experiment (likely a multi-generational effort – unless theory advances and gives some new solid predictions).

21. **Scott Church**
September 24, 2015

Chris, I agree… those are the points I was trying to make earlier. This isn't a question of what is, and is not worth funding–I think we all agree that the search for new physics up to and including GUT scale, is. The real questions are;

1) Is it possible to get there from here with the technical means available to us today and for the foreseeable future?

2) Are we pursuing said new physics down the right paths, with the right technologies, and on a potentially fruitful theoretical basis?

3) Given 1) and 2), is funding for such endeavors possible, and who will do so?

The answers to 1) and 2) are anything but clear. Indeed, as Chris said, given what we know of the SM today and the lack of fruitfulness the whole M-theory framework has returned to date, render it likely that we *aren't* going to find anything this side of GUT/Planck scales. And that leaves us with 3)… which is a sorry business because we're left with trying to sell funding for ever larger collider projects to governments that are already strapped, and rightly or wrongly, not in a mood to invest in projects for which we can't guarantee big returns.

Radioactive, I feel your pain. I agree that budgets are for accountants, and in my heart I don't give a damn about political feasibility either. Unfortunately, unless our personal checking accounts are larger than the GDP of a middle-sized nation–a claim few of us can make–we don't have the luxury of indulging such a stance. Like it or not, we're going to end up dealing with accountants… Treasury Dept. accountants… who report to world leaders indentured to their electoral base and already up to their necks in failing social security, a collapsing Greek economy, etc. etc. etc. It sucks, I know. But the sad reality is that we aren't going to get donuts delivered to the CERN lobby next week, much less funding for colliders and other projects, unless we convince those folks to agree to signing the checks. How do we deal with that? I have no idea… but we certainly don't have the luxury of pretending that we can continue to move forward without doing so.

While these are important concerns, I think it's just as important for us to continue exploring other ways we might push back the boundaries. For instance, pursuing advances in cosmological observation and detection technologies that might allow us to peer deeper into primordial events where the energies we can't reach today with colliders were the rule; Or exploring other theoretical avenues that might bear more testable fruit than M-theory. Regardless of what we can, or cannot achieve with ever larger and more costly colliders, these are things that *are* within our reach today… and perhaps, more likely to convince those who sign checks.

22. **Dom**
September 24, 2015

On the subject of the output of colliders, Jon Butterworth's latest article in the Guardian is quite nice [Guardian Higgs Article]

23. **Peter Woit**
September 25, 2015

Frank Quednau,
I don't know, but two people who might are Goncharov, who was involved in the "amplituhedron" business, and Deligne, who I hear was talking to him about this stuff, and knows this kind of mathematics.

Daniel Tung,
I just don't see any evidence at all that you can explain anything about the standard model this way. Much of our understanding of the standard model is based on its symmetries and I suspect that a deeper understanding will come from a deeper understanding of these symmetries. Arkani-Hamed's speculation ignores these completely (he's been known to say very unkind things about gauge symmetry…).

24. **srp**
September 25, 2015

It's encouraging to hear a couple of breaths of rationality vis a vis the obvious next step of investing in the exploration of advanced accelerator technologies. These could end up finding paths to ultra-high energies that cost much less, plus they are more likely to create new spin-off benefits than simply scaling up the existing designs. It would be irrational to commit to spending $15 billion on a the Big Dumb Accelerator without first investing a few hundred million bucks to find out if there were a better and cheaper way.

25. **Peter Woit**
September 25, 2015

srp,

Spending a few hundred million dollars to make the problems of why going to higher energies is so expensive go away isn't an idea that's been ignored. As far as I know, there just is no idea around such that throwing money at it is going to solve the problems anytime soon. Such ideas are more the sort of thing that will take many decades to see if they will work at all (and certainly should be investigated and funded). But if you want to start building something ten to twenty years out from now, to have it ready when the LHC has been fully exploited, your choices of plausible technology are limited, and all expensive.

26. **Tony Smith**
September 25, 2015

As to Nima's "… efforts to push forward a Chinese plan to build a next-generation collider …" and "… Gross and Witten have and editorial in the Wall Street Journal … supporting the idea of a Chinese Geat Collider project …"
Gross and Witten say in WSJ:
"… China would leap to a leadership position in an important frontier area of basic science. More practically, to build such a massive collider, China would need to develop frontier technology in many fields, from superconducting magnets to high-speed electronic detectors, attracting many of the world's top scientists and technologists …".

Since the USA cancelled the SSC Collider project it has relied on Europe and the LHC for fundamental collider physics, seeing the center of the physics world shift from the USA to Europe.
Now Nima, Gross and Witten advocate that China, with its increasing wealth and industrial dominance, become the new center of the physics world.

Why can't the new center be the USA itself, so that the USA would be the developer of frontier technology and be the center of attraction of the world's top scientists and technologists?

Some (maybe including Nima, Gross and Witten) might say that the USA cannot afford such an expensive project, and under current USA policies that might be true, but Citigroup has recently released a note entitled "Cold Fusion" advocating a way to transform the currently "ineffective monetary policy" of the USA into "effective fiscal policy … via central bank monetization … essentially monetizing … spending".

The Citigroup plan would allow the USA to undertake many-billion dollar programs (such as the new Great Collider) just as easily as it has undertaken Quantitative Easing to aid its financial sector,
with the USA reaping the benefits of advancing science and technology.

As to why Citigroup entitled its note "Cold Fusion", the note says:
"… Essentially you are combining Paul Krugman fiscal with Republican tax and Bernanke 2002 … monetary.
…
a (very cold) fusion of Krugman macro, Republican tax and Bernanke (2002) monetary policies …".

Tony

27. **Marc**
September 26, 2015

Does going to higher energies really help to understand the standard model?

This is only true if we discover something new. But will we discover something new?

We will discover something new if nature has more degrees of freedom at higher energy than at low energy. But is this the case?

Quantum theory usually does not confirm this. Does nature get more complex at higher energy?

Usually not. After all, higher energy means lower distance. But it seems obvious from quantum theory that nature will get simpler and simpler at low scale.

It might well be that the hope to find new things at smaller and smaller scales is ill-founded. In any case, the unification attempts discussed at present (new dimensions, new particles, new symmetries) do not seem to get simpler at small scales, but more complex. We should look for simpler attempts, not for more complicated ones.

28. **Peter Woit**
September 26, 2015

All,
Attempts to turn this comment thread into a tedious political debate about US monetary and fiscal policy will be ruthlessly suppressed.

29. **Neil**
September 26, 2015

Why do Gross and Witten think there is a question about the Higgs mass?

In any case, the Great Collider would probably cost less than the ISS and produce more science.

30. **Peter Woit**
September 26, 2015

Neil,
I'd guess that's a badly done edit, presumably what they meant is that we have no idea why the Higgs mass is 125 GeV.

One of the main goals of a new collider would be to measure the interactions of the Higgs with itself, something that may be inaccessible at the LHC.

31. **Anon**
September 26, 2015

PW re srp,

Laser-Driven Particle Accelerators?

32. **Peter Woit**
September 26, 2015

Anon,
Exactly what I was thinking of as an example of a technology not ready for use anytime soon.

For a circular machine the fundamental limits are due to the magnets for a proton machine (and there seem to be no prospects of dramatically higher field strengths), or synchrotron radiation for an electron-positron machine (a muon collider would solve this, but the short lifetime of the muon is a huge problem). For a linear machine, to build something much cheaper you need something like such a laser acceleration scheme. Maybe someday this will be doable, but it seems that much more work is needed before one will have a usable technology.

33. **srp**
September 26, 2015

The point is that “much more work” is not being funded. DOE keeps these alternative concepts barely ticking over as theoretical exercises, but there’s not much money or community attention devoted to actually building prototype components, etc., and there is no organized program to figure out the specific roadblocks of the main alternatives, investigate each one to see if there are any showstoppers or to develop solutions, etc. Saying “it’s a lot of work” while systematically preventing the community from doing the work is silly. If you drew out a decision tree for the next-generation accelerator at the energy frontier, there’s no way you would rationally end up spending even a billion dollars on preparatory planning for a scaled-up traditional machine without first spending a fraction of that to try to rule out the practicality of wakefields over the relevant time horizon. I know that Burton Richter has written than some kind of wakefield design is the sensible next step at the energy frontier, but I guess when people reach a certain age in physics its hard to get anyone to listen no matter one’s track record.

34. **Anon**
September 27, 2015

Look, if I had 10-100G dollar I’d like to spend on a scientific project, and you’d manage to somehow convince me that spending on high energy physic is presently more pressing than finding earthlike planets, or quantum computing, or robotic exploration of Titan, or proteomics, or human on mars, or artificial intelligence, or permanent facility on the moon (to name a few project I'm not working on), then you’d still have to convince me it’s not smarter to put my money on laser driven technology first. No one want to build a 100G dollar accelerator, and by the time it’s ready you can use another technology to reach the same target for 1/1000 of the price.

35. **Peter Woit**
September 27, 2015

Anon,
The question of “why not build a laser (or plasma wakefield, or whatever)-driven linear accelerator to get to the 10 TeV energy scale, instead of the proposed Great Collider”? is a legitimate one. But my impression is that at this point the cost of such a thing is not 1/1000 less, but infinitely more, since no one knows how to do it, at any price. srp is likely right that this kind of research deserves more support, but I’d like to see some sort of realistic time frame + cost estimates for a machine using such a technology. If anyone can point to a convincing document about how such a thing can be designed and built in my lifetime (I figure I’ve got maybe another 25 years or so..), at dramatically less cost, I’d like to see it. And, by the way, claims of “1/1000 the cost” are not confidence inspiring…

If it’s not at all clear that this can be done, and the timescale for finding out is many decades, then the problem is that a decision to put all the marbles on such a speculative idea is that it may be tantamount to a decision to shut down energy frontier physics for good. The clock is ticking, twenty years or so from now it will not make sense to keep running the LHC, and if there is no viable next generation project on the horizon the field will have to shut down, with trained people changing fields to something else, and no new people being trained.

I should make clear that I don’t think the case for a 100 TeV machine is at all a slam-dunk one. HEP is facing a very difficult future and some tough decisions. As part of this, I don’t think unfounded claims that a speculative new technology is going to make the problem go away are helpful.

36. **Anon**
September 27, 2015

It’s not confidence inspiring to suggest 1/1000 the cost in the next three decades? Please remember of the human genome project: 3 G dollars in 1990, and 30 years later it would cost not 1/1000, but 1/1000000 the price.

But on retrospect it’s not fair of me to make this argument, because I’d not support the Great Collider for the next two decades even if you could garantee that the price won’t drop. The main reason? Simply because I don’t see any garantee that it’d significantly increase our knowledge.
Actually, from reading your blog one could have expected that you thought the same. How then would you justify priorising high energy physics over the many inspiring projects I mentionned previously? Or you think we could all fund them in the next 20 years?

37. **Dave Miller in Sacramento**
September 28, 2015

Peter,

You wrote:

> a decision to put all the marbles on such a speculative idea is that it may be tantamount to a decision to shut down energy frontier physics for good. The clock is ticking, twenty years or so from now it will not make sense to keep running the LHC, and if there is no viable next generation project on the horizon the field will have to shut down, with trained people changing fields to something else, and no new people being trained.

Well… for better or worse, I suspect that your statement will turn out to be an accurate prediction of the rest of the century in “high-energy” physics.

Things have not worked out the way you and I had hoped when we were doctoral students way back when.

Dave

38. **Radioactive**
September 28, 2015

Anon: “I’d not support the Great Collider for the next two decades … Simply because I don’t see any garantee that it’d significantly increase our knowledge.”

So you’d like to defund all speculative research then?

39. **Peter Woit**
September 28, 2015

Anon,
My view on prioritizing funding is that you have to look at the situation in each field, get a realistic idea of what funds are needed to significantly move forward, decide whether such a level of funding is feasible, then deal with the politics of competing for funding.

Some fields (like quantum computing) are already hot topics and well-financed, I’m suspicious that there aren’t good ideas out there that aren’t already getting funded. Other fields you mention (e.g. manned space exploration) I personally don’t see as worthwhile at the costs it takes to do them. One way to argue against manned space exploration would be to say “let’s wait to do it until the costs come down by a factor of 1/1000”, but that would be effectively the same as saying “it’s not worth it, so shut the field down”. Better to deal with the difficult actual argument, not evade it, for manned space exploration as for high energy frontier experiment.

40. **Jeff M**
September 28, 2015

Well, I've stayed out of this, since I'm a mathematician and we don't have to worry about funding really. But everyone seems to be dancing around the real issue, which is WHY you fund research. You can do it for the sake of advancing knowledge, or else to advance the well being of humanity/the planet via technology. AFAICT HEP funding is solely based on the first, it's not like it has any technological implications. Neither does QFT research that I can tell (correct me if I'm wrong). Basic QM, sure, solid state of course, fusion research and nuclear physics in general of course. If you want to argue someone should fund a collider, be honest, the only reason to do it is abstract. Once you do that, you can argue about whether even then it's worth it, since it's very unclear that you're going to get anything from a much bigger collider. Personally, I don't think you've gotten much from the LHC, I mean did anyone expect them not to find the Higgs eventually? And given that nothing else has shown up (and I'm willing to bet you actual money nothing else will) where are you now?

41. **Peter Woit**
September 28, 2015

Jeff,
Actually, QFT research does have a lot of practical applications. Solid state physics is based on it, since the field effectively deals with an infinite number of degrees of freedom, not the finite number of simple QM.

You need to, when possible, experimentally test an idea like the Higgs mechanism, not just believe it since it seems to be the most plausible idea. Often ideas you think are the most plausible turn out to be wrong (or only part of the story). Besides verifying conjectures about the Higgs mechanism, the LHC has also shot down huge areas of conjecture about TeV scale physics (there's an interesting ongoing question of when many theorists will acknowledge this reality). With no LHC we'd forever hear that SUSY is the most plausible idea about TeV scale physics and should just be believed.

42. **Scott Church**
September 28, 2015

The Oct. issue of Scientific American has an intriguing article called [How Big is Science?] From the opening… "Mammoth instruments such as CERN's Large Hadron Collider are often held up as symbols of the human commitment to decoding the world. But how highly does humanity as a whole actually regard science? How big *is* science? This is not an easy question to answer, but by gathering what credible data exist, we can approximate an answer."

Haven't had a chance to read it yet, but among other things it discusses some of the issues surrounding funding of large physics projects like the LHC, and how public perceptions impact that. The graphic on the first page alone is enlightening.

43. **Jeff M**
September 28, 2015

Peter

Had no clue solid state used QFT's thanks for correcting me 🙂 As for the Higgs, my point wasn't that it wasn't interesting, just that if you were thinking practically it wasn't going to get you anywhere. As far as the LHC chucking supersymmetry etc. out the window, has that really happened? Aren't all the SUSY people just running around saying "wait for the next bigger accelerator?" I'm not holding my breath

44. **Anon**
September 28, 2015

PW,
Your opinion on how to prioritize funding is perhaps unclear (one way to read it is you're happy to offload this on political games, but hopefully this is not what you intended to say). I happen to agree on manned mission (except most children trill, and the more they trill the more will turn scientists…), disagree that quantum computing is funded at an appropriate level (I even suspect that, in the long run, progresses in high energy physic will depend on first making a quantum computer work), disagree that not adding 10G USD to a field is the same as shutting the field down.
Radioactive,
No, I'd glad to support the next collider if the cost was 10-100 millions USD.
Scott Church,
Great link! This kind of graphic is arguably the best support for the GC, e.g. maybe a new collider "just to check there's really nothing to see" would be fine after all, if only expanditures on Science were enough to make the F35 program looks cheap.

45. **abby yorker**
September 28, 2015

The article says 10 billion for 100 Tev. LHC cost 13 billion(Forbes). I recommend that we go for a Planck energy machine, to minimize costs.

46. **Peter Woit**
September 28, 2015

Jeff M,
It's been a long time since HEP people had a good case that what they were learning about fundamental physics would have practical implications. If those who argued that the LHC would see SUSY try to follow the Gordon Kane model, and try to use it as an argument for the Great Collider, I think they will run into serious questions about their credibility.

Anon,
I think it's the job of physicists to come up with the cheapest way of successfully investigating higher energy scales, and if the budget numbers are plausibly affordable, make their best case to the politicians who will have to sign off on budgetary decisions. I understand the US budgetary and political situation, and it's clear that there's no way this kind of project is now doable in the US. I understand nothing about the analogous issues in China, but don't see an argument why physicists shouldn't make their case for this.

abby yorker,
I think 13 billion is a maximalist way of accounting for LHC expenditures, 10 billion a minimalist estimate. I don't understand the economics of this well enough to understand the cost differences between doing this kind of thing in Geneva (one of the most expensive places in the world) vs. China.

47. **srp**
September 28, 2015

As between moving the center of experimental HEP to China v. exploring advanced concepts, I'm not sure which one is the riskier bet of all the marbles. ISTM that the technological and economic facts have eliminated the bet-one-marble actions from the strategy space on the energy frontier. One advantage of the advanced concepts is that one cannot be so definitively sure about the lack of technological spinoffs as one is for the scaled-up conventional machines. Work on dielectric concepts, for example, involves building structures similar to the sort handled by the semiconductor industry.

Moreover, the time lags for such machines (if they turned out to be feasible) would be more concentrated on R&D than on construction because they would be much, much smaller. While a Manhattan Project-style organization chasing down two or three alternative technologies and the relevant engineering would be no joke to manage, the construction, operation, and maintenance of the machine itself ought to be much less of a superhuman feat of coordination than the LHC or the proposed Chinese Gargantuan Collider. In fact, if these time savings were not possible on the back end of the project then that would mean that the entire concept offered no cost savings at all.

48. **Hiro**
September 29, 2015

http://arxiv.org/abs/hep-ex/0005008

Linear 1 PeV Muon Collider, will it become a reality or a delusion?

The Free Particle

September 29, 2015
Categories: Quantum Mechanics

Following on my notes about Euler's formula, I've finally finished some work on another piece of elementary exposition, a discussion of the free quantum particle, which can be found as chapters 10, 11 and 12 of the book I'm working on.

These chapters are a complete rewrite and major expansion of what used to be there, a rather slap-dash single chapter on the subject. The excuse for this in my mind had been that it's a topic treated in detail in every quantum mechanics textbook, so best if I passed over it quickly and moved on to things that weren't so well treated elsewhere. Another reason for this was that my understanding of analysis has never been what it should be, and it seemed best if I not make that too obvious by how I handled the mathematics of this subject.

This summer I started rewriting the book from the beginning, and once I hit the chapter on the free particle it became very clear that it needed improvement, both for its own sake and for how the material was needed in later chapters. I spent some time doing some remedial study in analysis, and after a while got to a point such that I felt capable of writing something that captured more of the relevant mathematics. Finally, today I got to the point where these three chapters are in decent shape, and soon I'll move on to later ones.

One thing that I'd never thought much about before, but that struck me while rewriting these chapters, is the quite peculiar nature of a position eigenstate in quantum mechanics. Normally one only thinks about this in relativistic quantum field theory, where the problems associated with localizing a relativistic particle motivate the move to a quantum field theory. Of course a position eigenstate is just a delta-function, but what is peculiar is the dynamics, what happens if you take that as an initial condition. See the end of chapter 12 for what I'm talking about (be sure you have the latest version, today's date on the front), I won't try and reproduce that here. Part of this story is the tricky nature of the free-particle propagator in real time, as opposed to its much better behavior in imaginary time. The issue of analytic continuation in time continues to fascinate me, including the quite non-trivial nature of what happens even for the supposedly trivial case of a free particle in one spatial dimension.

Comments

1. **vskrin**
   September 30, 2015

   Hi,

   a small typo on the pg.146:

   "At any time T >0, no matter now small, this will be a phase factor with
   constant amplitude extending out to infnity in position space"
   in that quote: n->h

   as someone with a physics background, but with interest in more mathematical topics I think I'll start with your book when I try to refresh my memory of the nonrelativistic QM.

   thanks for making the drafts public!

2. **vmarko**
   September 30, 2015

   Peter,

   Page 145, equation for U(T,q_0,q_T). You say "to make sense of such an integral, on can…" [define it using Wick rotation, basically]. This is not really correct, and is a common misconception. The integral is perfectly well-defined as it stands, and can be evaluated with no additional analytic continuation, Wick rotation or otherwise. It's called the Fresnel integral (there is a Wikipedia article about it), and it can be solved by using the solution of the Gauss integral on the real line, the Cauchy residuum theorem, and a creative choice of the integration contour.

   The Wick rotation is not necessary to define that integral. Rather, it has to do with the fact that the Dalambertian operator is hyperbolic, and does not have an inverse. Then one uses the Wick rotation to transform the Dalambertian to a Laplacian (which is elliptic and therefore has an inverse), and then define the inverse of the Dalambertian by analytic continuation of the inverse of the Laplacian.

   HTH, 🙂
   Marko

3. **Peter Woit**
   September 30, 2015

   vmarko,
   "creative choice of integration contour"
   I should look more closely, but I'm still not convinced that's not a definition that uses analytic continuation.

4. **Peter Morgan**
   September 30, 2015

   Agreeing with vmarko about page 145. For another treatment, for example see Bogoliubov&Shirkov, page 147ff.

5. **Peter Woit**
   September 30, 2015

   vskrin,
   Thanks. Fixed.

   Peter Morgan,
   That's the relativistic propagator, which is trickier. As far as I can tell Bogoliubov and Shirkov use the +i\epsilon method to deal with the issue I'm discussing. Doing this

defines the integral as the boundary value of something well-defined in a complex half-plane, away from the boundary. I don't think it's inaccurate to characterize this as analytic continuation definition.

6. **Peter Morgan**
September 30, 2015

Seeing your response to vmarko, perhaps all that's necessary is to replace "To make sense of such an integral …" by something like "To derive an analytic expression for such an integral …".

7. **vmarko**
September 30, 2015

The propagator is more complicated, precisely because of the distinction between hyperbolic and elliptic operators, i.e. Minkowski vs. Euclidean spacetime. For that story you really need the Wick rotation. But for the integral discussed in Peter's book, this is unnecessary, because the integral can be evaluated without any analytic continuation.

The integration contour is drawn in the Wikipedia article on [the evaluation of the Fresnel integral](), and the rough steps of the evaluation are explained briefly. It's a simple exercise in complex analysis. Moreover, the function under the integral is the exponential, and since it doesn't have any poles the calculation does not contain any undefined quantities or such, so no analytic continuation involved.

HTH, 🙂
Marko

8. **Peter Woit**
September 30, 2015

I took a look again at Brian Hall's "Quantum Theory for Mathematicians", and it has an excellent treatment of the free particle (see chapter 4), as well as much more about the subtleties of the Heisenberg uncertainty relation (chapter 12).

In exercise 2 of chapter 4 he derives the integral discussed here in a manner that doesn't initially seem to be the usual boundary of a holomorphic function derivation. I'll have to look more carefully at that…

9. **Peter**
October 1, 2015

Dear Peter,
to show the convergence of the Fresnel integrals (without complex analysis) one can use the Dirichlet test. Evaluation is more painful though, but is in most classical analysis books. For a newer exposition, you can check this book by S.Popescu:

http://civile.utcb.ro/cmat/cursrt/ma2e.pdf

The convergence is in Example 50, p.122 (of the text), the evaluation is Example 60, p. 164.

10. **Dave Miller in Sacramento**
October 1, 2015

Peter,

The "tricky nature of the free-particle propagator in real time" is part of the Feynman path-integral approach as presented in Feynman and Hibbs, if I recall correctly. And, of course, if you convolve the kernel against any reasonable function, it works as it should.

Back in the mid-'70s, I had a conversation with Weinberg about how this is different in the QFT case due to the pesky 1/E factor (1/sqrt(E) in terms of the definition of the quantum field) required by relativistic invariance of the volume element in momentum space. Steve said he had never thought of the fact that that this messed up localizing the particle at a point.

Of course, there is another way of looking at it — you can view the vacuum as having non-trivial correlations at short distances (revealed in the non-zero VEVs), but Steve likes looking at particles as primary and the fields as just a way of describing interactions among the particles.

I'm still bothered as to how this works out in the fermion case — I know the math, but I am not sure how to view it physically, especially if one tries to think in terms of fields rather than the infinite number of particles of the Dirac sea. Alice Rogers has had some things to say relevant to this, but I'm not sure I understand her.

Dave

11. **Anon**
October 1, 2015

It is not hard to see that he Fresnel integral exists as an improper Riemann integral without any need for analytic continuation or deformation of integration contour away from the real line. As you take the integration limits to infinity, the function oscillates more and more rapidly (cancelling itself out more and more) so the contribution of any interval of given size at larger and larger distances becomes less and less. You can show the limit exists.

This is not true, however, for the moments of the Fresnel integral we often need to calculate, e.g., \int dx x^n e^{- alpha x^2}.

These can be calculated, though, without analytic continuation also. A nice source on how to do this is
Albeverio et al., J. Funct. Anal. 113, 177

Basically they do this by the generalization of the e^{-epsilon x^2} convergence factor we often add in Physics, do the integral, and take the limit epsilon -> 0. They generalize this to a general class of test functions and show that the limit is unique and independent of the test function.

We do something very similar to calculate Fourier transforms of tempered distributions

12. **Bob**
October 1, 2015

Hi Peter,

At the end of chapter 12, you've given the expression for a time-evolved position eigenstate as a Gaussian that broadens over time. What do you mean when you describe it as "a phase factor with constant amplitude extending out to infinity"?

13. **Andrew**
October 1, 2015

Can you tell us more about the process of writing a book? How did it come about? Was it commissioned by a publisher? Or did you begin a draft and later contact a publisher?

14. **Anon**
October 1, 2015

By the way, while the Fresnel integral straightforwardly converges as a improper Riemann integral, I believe it doesn't exist as a Lebesgue integral. To define it in the sense of Lebesgue, you do need the kind of convergence factor trick as in the Albeverio paper I mentioned above. Since you need that trick anyway as soon as you want to calculate moments, that might as well be your starting point. Still, no analytic continuation is ever necessary in either case.

15. **Peter Woit**
October 1, 2015

Many thanks to all who provided references about the question of convergence of this integral. I wasn't aware of ones not using some sort of i\epsilon argument (or equivalently a -\epsilon^2 convergence factor. I still think though the highly singular nature of what you get when you try and localize a non-relativistic particle (as opposed to the good behavior in imaginary time) is remarkable.

Bob,
It's only a Gaussian in imaginary time, in real time it's a phase factor.

Andrew,
The book came about because I started teaching a quantum mechanics course for mathematicians. There was no appropriate book covering a lot of what I wanted to do (e.g. the representation theory point of view, for other topics there are good books, for instance the Brian Hall book is great on the analysis side of the story). So I started writing notes for the class, at some point it became clear these were turning into a book. This project has now though gone on much longer than I wanted, I hope to finish soon.

I heard from several different publishers interested in what I was writing, earlier this year signed a contract with Springer. No, in case you're wondering, there's no money to be made writing this kind of thing, the motivation is purely the feeling that someone should do it, and that one learns a lot by working on something like this.

16. **Bob**
October 1, 2015

Hi Peter

So it is. You're right, it is strange.

If you take a normalisable but very sharply peaked wavefunction at t=0, you can presumably convolute it with that object and get the corresponding wavefunction at t=T. The further you go spatially from the original peak, the more rapidly that phase changes with distance, so beyond some distance the contribution it makes to the wavefunction at time T will be arbitrarily small, provided the original function has a finite gradient everywhere.

I see that it works, but the way that it works is certainly strange.

17. **John Baez**
October 2, 2015

Anonymous wrote:

> By the way, while the Fresnel integral straightforwardly converges as a improper Riemann integral, I believe it doesn't exist as a Lebesgue integral.

That's right, functions like sin(x^2) or cos(x^2) or exp(ix^2) are not Lebesgue integrable, because Lebesgue integration can't handle the "infinite cancellation" required to get a finite answer. But you can use any reasonable way of first imposing a cutoff or forcing the function to decay for large x and then removing this; they all give the same answer. Analytic continuation is essentially one of these methods.

This goes to show that the Lebesgue theory is not the last word on integration, and not nearly good enough to do path integrals in real time.

The Henstock-Kurzweil integral is able to handle this particular integral.

> In general, every Henstock–Kurzweil integrable function is measurable, and f is Lebesgue integrable if and only if both f and |f| are Henstock–Kurzweil integrable. This means that the Henstock–Kurzweil integral can be thought of as a "non-absolutely convergent version of Lebesgue integral"…

18. **Peter Woit**
October 3, 2015

Thanks John,
The lack of Lebesgue integrability makes precise the problem with the definition I was trying to put my finger on.

19. **Anon**
October 6, 2015

Bob, these propagators are certainly strange objects. It may help sometimes to remind oneself that propagators are not generally functions. Rather, they are distributions (generalized functions), which generally don't even need to have any representation as pointwise functions. Even when they do have such a representation on part of their range (as happens to be the case with the t>0 free particle propagator), you can do all kinds of things with them (e.g. Fourier transforms, moments, etc.) involving "integrals" that are divergent in the Riemann or Lebesgue sense. I promise that for the most part all this makes intuitive sense once one familiarizes oneself sufficiently with generalized functions.

The canonical source for generalized functions is the brilliant multi-volume:

I.M Gel'fand and G.E. Shilov, Generalized functions

Oddities

October 2, 2015
Categories: Uncategorized

A few odd things that I've run into recently:

- The IAS has a weekly meeting to discuss current topics in HEP theory. From their events calendar, next week's meeting will be devoted to "The Cosmological Constant and the String Landscape", with suggested readings the 28 year old papers by Weinberg on anthropics and the CC, as well as the KKLT paper (I'm a fan, see here) and its Bousso/Polchinski predecessor from about 15 years ago. I would have thought this was a very well-worn topic. The one recent reference is a defense by Polchinski of KKLT against some technical challenges, but if the intent is to discuss those, it's unclear why they're not in the references (and they have nothing to do with Weinberg/CC/anthropics).
- Mochizuki has two new things on his website. One is a long discussion of the use of the words "anabelioid" and "Frobenioid", the other is an animated video of a diagram explaining a theorem (see near bottom of here).
- A couple years ago there was a controversy over the proof of the so-called "Yau-Tian-Donaldson" conjecture (see here), with Donaldson and his collaborators publicly complaining about Tian's paper claiming credit for proof of this conjecture. The Tian paper was submitted to the Courant journal *Communications on Pure and Applied Mathematics* in February 2013, was published there in the July 2015 issue (a technical corrigendum appeared a couple weeks ago). More expertise than I have would be needed to see if the published version addresses the Donaldson et al. concerns, a quick look doesn't indicate evidence of that.

  At the time I wrote:

  > On a more positive note, perhaps this controversy will not interfere much with future progress in this area, as Donaldson and Tian are jointly organizing a Spring 2016 workshop on this topic at MSRI.

  The MSRI directory for this year though lists Tian as visiting, but not Donaldson.

- Among the many oddities associated with string theory is the decision of a group in Philadelphia to name a group of charter schools there the String Theory Schools. I don't think they teach string theory, just liked the name. The financing of these schools is now attracting controversy. It seems they are running into some financial trouble, involving huge real estate deals and tax-exempt bond financing. For the details of the story, see here. For some analysis, see a Naked Capitalism piece: Private Equity Asset-Stripping Strategy Meets Charter Schools to Produce Even Better Looting.
- Presidential candidate Ben Carson has been widely (and quite appropriately…) criticized for some of his odd and non-sensical views about science. In USA Today's factcheck piece about this, we're told

  > Carson went on to claim that the presence of stars and planets is related to the existence of multiple Big Bangs that eventually might produce an ordered universe:
  >
  > Carson: And then they go to the probability theory, and they say "but if there's enough big bangs over a long enough period of time, one of them will be the perfect big bang and everything will be perfectly organized." And I said, so you're telling me if I blow a hurricane through a junkyard enough times over a long enough period of time after one of them there will be a 747 fully formed and ready to fly?
  >
  > That is not an accurate reflection of the Big Bang theory. Though some theories of the origin of the universe suggest that the Big Bang was only one of many such explosions, these theories do not state that the currently ordered existence is a spontaneous result of one of these repeated Big Bangs.

  He's getting it somewhat wrong, but this does sound a lot like Carson has been reading about the string theory multiverse…

Update: Please do not use the reference to Carson as an excuse to post your thoughts on US politics and the ongoing political campaign. I think everyone would appreciate not having to be subjected to that topic here.

Comments

1. **s t schools**
   October 2, 2015

   Let's not be quick to blame string theory for everything. They could have been G-String Theory Schools, and the curriculum may have included (*ahem*) … but the name was shortened to avoid unwanted attention from the Tea Party.

2. **Benny Oid**
   October 3, 2015

   The word is "Frobenioid".

3. **Peter Woit**
   October 3, 2015

   Benny Oid,
   Thanks, fixed.

4. **prize**
   October 3, 2015

   http://www.nytimes.com/2015/10/03/opinion/the-folly-of-big-science-awards.html

   Nobels coming up. NY Times editorial "The folly of big science awards". Many other prizes pay more, and the recipients are indeed in the least need of funding, but the Nobels are the best known and the most prestigious.

5. **Shantanu**
   October 4, 2015

   Peter any rumors of this year's nobel prize?
   Here is my guesses

Either neutrino physics and it goes to Yoichiro Suzuki and Art Mcdonald
Or in astrophysics/GR goes to Vera Rubin and Irwin Shapiro
Shantanu

6. **Peter Woit**
October 4, 2015

The physics Nobel announcement is scheduled for Tuesday morning. I have no idea who it will go to, seems unlikely to go to my choice, which would be to make up for their 2013 mistake of only rewarding theorists, not experimenters, for the Higgs discovery (so, a prize to ATLAS, CMS and CERN).

At $3 million, the big prize now is the Breakthrough Prize, which will be awarded Nov. 8. For that one also, I have no idea.

7. **Bernhard**
October 4, 2015

Shantanu,

I also have no idea, but wouldn't be really fair to give a prize to Rubin and forget W. Kent Ford. The work for which Rubin is most recognized for is co-authored by him and in this particular case the Nobel committee wouldn't be able to use the excuse of just acknowledging the group leader like they did with Powell. But anyway, not much to discuss, maybe the prize this year will in the end go to solid state physics or alike.

8. **Thomas Larsson**
October 6, 2015

Kajita instead of Suzuki, otherwise you were right, Shantanu.

9. **Shantanu**
October 6, 2015

Yes Suzuki was the spokesperson of Super-K from 2002 until recently. Kajita was the co-chair of the atmospheric neutrinos and proton decay group which announced 10 sigma evidence for non-0 neutrino mass.
It should have been awarded to Totsuka who initiated SK, but unfortunately the committee was late as he passed away in 2008.

That said, its still not clear to me what non-0 neutrino mass has told about physics beyond standard model, string theory etc.

10. **Bernhard**
October 6, 2015

The Nobel committee keeps making these kind of mistakes. Why not give the prize to the whole Super-K collaboration? It's a huge disservice to science to create these false myths around one person and give the public a wrong idea about how science works. And that should be the point of these prizes. It's so typical of Swedish mentality this leadership/boss thing (no matter how much they will deny this till death). Peter, sorry for the off-topic comments, last one from me.

11. **AcademicLurker**
October 6, 2015

I was predicting Rubin and Shapiro. Good thing I didn't bet any money. My reasoning was that since they're 87 and 85, respectively, the prize for dark matter should be awarded soon if it's going to be awarded at all.

12. **Regretacles**
October 6, 2015

Sno and super kamiokande had some systematics which actually left the question open. Kamland nailed it. I wish Stuart was still around to help me see how to find this all amusing

13. **David Nataf**
October 7, 2015

It's not clear if dark matter should ever get a prize. What's "proven" is that the gravitational mass of matter in the universe , assuming GR or even Newtonian gravity, is ~5x higher than what one would get from baryons.

The exact meaning of that is debatable until WIMPs are confirmed.

14. **Low Math, Meekly Interacting**
October 7, 2015

I've seen a lot of grudging approval of the latest Physics Nobel. While some show the usual unbridled enthusiasm for the pick, there's a significant amount of chatter on the interwebs expressing the opinion that while the prize is probably deserved, it's also one of the most boring, anticlimactic subjects ever to be so honored. There are no deep concepts here about BSM physics. The math is fairly trivial. We haven't actually learned anything interesting (Dirac or Majorana?) about neutrino mass, and the mere fact that it exists is hardly surprising. And so on.
Me, I'm just glad someone still thinks experimental verification in particle physics is extremely important.

15. **Peter Woit**
October 7, 2015

LMMI,

I think the attitude that it's not interesting to study the neutrino sector is quite wrong-headed. It's not just a copy of the quark mass matrix, mixing-angle story. The possibility of Majorana mass terms, and the fact that a right-handed neutrino state would be something quite different than anything else, neutral under all the SM forces, mean that there is definitely something new to study. Maybe there's a hint lurking there of a new insight into the SM, it would be absurd not to look. And, obviously, the discovery and measurement of neutrino mass terms is a significant advance in our understanding, well deserving of a Nobel prize.

16. **NeapTide**
October 7, 2015

I think Peter Woit is right on target. The theory of neutrino mass/mixing may not be challenging, but the experiments are quite challenging. In the end physics needs experimental/empirical proof. And then the theory of what causes neutrino mass/mixing is a `tail of the dragon' just as CP violation is… the dragon is BSM physics.

17. **Neil**
    October 7, 2015

    Boring and anti-climatic subject? Remember the solar neutrino problem, which was considered so puzzling that there was even a serious (or maybe not so serious) suggestion that the sun's fusion engine had shut down temporarily.

18. **Low Math, Meekly Interacting**
    October 7, 2015

    Hi, Peter & NeapTide,
    Well, yeah! There's obviously been considerable letdown in the wake of the Higgs discovery, and related anxiety about the future of collider physics, given the lack of hints about what energy scale is now necessary to probe to have a good chance of discovering something new. What better prospects are there, experimentally, than in neutrino physics? And besides, the work done on improving these detectors to help explore foundational questions appears to be accelerating progress in the field of neutrino astronomy, with obvious positive side-effects for astro- and nuclear physicists. Seems to me there's tons to love about neutrino physics, and the "meh" the latest Prize has gotten in some quarters is pretty surprising to me.

19. **Low Math, Meekly Interacting**
    October 7, 2015

    Sorry, should have written "fundamental", not "foundational"…

This and That

October 7, 2015
Categories: Uncategorized

- The Perimeter Institute's public lecture series tonight will feature Neil Turok on The Astonishing Simplicity of Everything. I think Turok is one of the few theorists speaking to the general public who has got the story of the current situation right: the LHC and CMB results point to the Standard Model + simple model of cosmology, ruling out many of the complicated models that have enthralled theorists for decades. By all rights, this should change the behavior and attitudes of theorists, I hope talks like his will have an effect. In any case, it should be vastly better than the last one of these, devoted to a misleading sales job for a failed theory.
- Nature magazine has a very good piece by Davide Castelvecchi on Shinichi Mochizuki and his impenetrable proof. It gives an accurate picture of the current situation: still virtually no experts have been able to understand Mochizuki's claims well enough to evaluate whether he has a valid proof. One counter-example is Ivan Fesenko, who is organizing a December workshop that may clarify the situation.
- The big news this week is the physics Nobel for the discovery of neutrino masses. I haven't written about this partly because I'm pretty ignorant about the history of the experiments awarded the prize (or, more accurately should have gotten the prize, not just a single person from the experiment), and the web is full of well-written coverage of this. The issue of the theory of neutrino masses is a fascinating, but quite intricate one, and some day I hope to write a bit about it, but lack the time right now.
- Tommaso Dorigo has a posting here about the great Italian theorist Guido Altarelli, who passed away last week.
- At some point I came across a list of the top donors to US political campaigns and was a little bit surprised to see that Jim Simons was on it, at number 7. I also noticed that someone else at his hedge fund spends even more on politicians than Simons does: Robert Mercer, a computer science Ph.D., is at number 4 (better informed people have pointed out to me that these numbers only include some publicly reported categories of donations). The Washington Post has a profile on Mercer today, which explains that he's one of the people we have to thank for Ted Cruz (Mercer is the top donor in the US to 2016 presidential campaigns, according to this).

  In terms of funding politicians and science, increasingly it's the hedge fund guy's world, we just live in it. I'm a big fan of much of what the Simons Foundation does, less so of Mercer's science funding, which goes to the Oregon Institute of Science and Medicine. The people there seem to be interested in climate denialism, surviving nuclear attack, and getting ahold of your urine.

  Physicists and mathematicians don't always have huge success in the hedge fund business. Robert Stock, a physics Ph.D from Carnegie-Mellon, seems to have gone from trying to blow up missiles with lasers to instead having more success blowing up a hedge fund, Spruce Alpha.

Update: According to today's New York Times, for the 2016 presidential election cycle, Mercer is the number two donor in the US, at 11.3 million. Simons is way, way down the list at 550,000. Their list of donors is heavily dominated by the financial industry (64 individuals or families), with the next largest number from the oil and gas industry (17 individuals or families).

Comments

1. **martibal**
   October 8, 2015

   Well, sentence like "We stand on the threshold of breakthroughs, both theoretical and experimental, which could change our picture of the world and the development of our society" doesn'it sound like pure propaganda ? It does not seem to me as the most honest attitude from a theorist. What in the present situation of physics allows to claim that we are close to a major breakthrough that will change the development of our society ? This permanent claim to the next-to-come major breakthrough is very much in the spirit of PI, but what is the benefit for physics at the end ? Why do physicist have to behave like teenagers so sure that what they are doing is the coolest thing ever ? I know this is how people like Arkani-Ahmed needs to behave, but it seems that mathematician are more modest. Remember years ago Connes about Riemann hypothesis saying that what he just explained was an epsilon in the good direction, but infinity minus epsilon is still infinity.

2. **Peter Woit**
   October 8, 2015

   martibal,
   I'm sympathetic to your point, but I do think that Turok has somewhat more of an excuse for this sort of thing than most people. As director of Perimeter, he has to find funding for the place. Being a salesman making the best pitch he can is part of his job description…

3. **NeapTide**
   October 8, 2015

   On the Nobel this year… just from memory, without a refresh… the neutrino oscillation evident in atmospheric neutrinos was `glimpsed' in a variety of experiments… old ones long forgotten under the Alps, in the IMB, and the original Kamiokande. Seems to me Kajita led the definitive analysis of this effect with the (then) new Super-Kamiokande, and proved it was a new neutrino oscillation beyond all reasonable doubt.

   Most of these experiments started as proton-decay detectors, and found nothing. I still remember them showing their low-energy events (in the early 1980's) and saying they were `boring low energy neutrino physics'. Turns out there was a Nobel discovery in the `boring low energy neutrino physics'. A nice turn of serendipity.

   However, the atmospheric neutrino effect was also noted in Koshiba's 2002 prize citation. If there is an oddity here, it is that Kajita's work overlaps quite a bit with Koshiba's.

   As for SNO, it really showed that the total solar neutrino flux, independent of neutrino flavor, agreed with the solar models of Bahcall. SNO is just a phenomenal experiment, a tour de force. On the other hand, it provided a verification that all the neutrino flux was really present, while the Ray Davis experiment, which measured just electron-flavored neutrinos, found a deficit. So you could say SNO really proved that the Davis deficit truly was oscillation of the nu_e's into other nu_x's.

   Qualitatively, though, the KAMLAND experiment did it a bit more convincingly. KAMLAND was a bit quicker and dirtier than the spectacular experimental care of SNO. But KAMLAND saw the neutrino oscillation independent of all sorts of solar uncertainties. Hardcore experimentalists like the control present in KAMLAND.

   You see where this is heading, I suppose…. MacDonald + (KAMLAND) might have made a bit more sense than MacDonald + Kajita. But Kajita really led the team that nailed atmospheric neutrino oscillations, which were not probe by KAMLAND, but have been verified subsequently by K2K, T2K, and MINOS, so Kajita is definitely deserving. It's just that Koshiba kind of got rewarded for that already.

   Possibly KAMLAND will get worked into yet another prize. Possibly with Daya Bay and/or RENO, for theta_13.

   Also, there are several major experiments underway or planned in the neutrino world: KATRIN (neutrino mass), DUNE (CP violation in neutrino mixing), Majorana, CUORE, NEXO (double beta decay), Project 8 (clever neutrino mass advance in sensitivity), LSST (astrophysical neutrino mass measurement).

And there is kind of a fault line between the LHC experimentalists and the neutrino experimentalists. The natural fault that results from questioning why continue the LHC if nothing seems to be there, and from the LHC the questioning of the more modest discovery potential of the neutrino program…. if you measure all the neutrino stuff, it implicitly points to BSM physics, but doesn't help identify what that physics really is. Passing a threshold at the LHC does do that.

4. **Peter Woit**
October 8, 2015

Surely no one who regularly reads this blog could possibly believe that I'm going to tolerate the usual tedious arguments about climate change. Please just stop.

5. **srp**
October 8, 2015

According to the linked article, Stock had long (several months) departed Spruce when the fund collapse occurred. It's hard to see how the blowup of a strategy using short-term movements in ETFs could be retroactively pinned on him.

6. **Peter Woit**
October 8, 2015

srp,
Yes, the article says Stock left 4 months before the blow-up. It also says that he had been there ten years and designed the strategy they were using of taking money from ETFs by "by identifying daily-resetting, highly-levered ETFs experiencing volatility decay, and shorting them in bull and bear pairs," and that this strategy blew up in August due to the usual sort of "fat tail", a "seven-sigma" event that was not supposed to happen. They attribute the collapse to the strategy they were using, which seems to be the one designed by Stock, with the fault not theirs, but "unprecedented market events that led to similar strategies losing money".

In his defense he says that he doesn't know how the strategy was implemented after he left. Maybe his successor changed things, maybe if he had stayed there he would have seen the seven-sigma tsunami coming and gotten out in time. And, maybe he's just another one of many quants who designed a strategy that makes you money reliably for a while, until the day it wipes you out, after which you whine about "unprecedented market events".

7. **Chris Oakley**
October 9, 2015

To the above I might add that although the financial markets generally had huge respect for people with mathematics or related Ph.D.s, I doubt that majority of the ones I knew made money overall (and personally, not wanting to involve myself in things over which I had no control, I kept clear of trading). One blew up a hedge fund c. 1998. Another one, a loner who combined the roles of quantitative analyst, programmer and trader, had, like Stock, a strategy that made small amounts of money slowly and then lost all of it and more quickly when market conditions no longer suited.

And then of course there was the LTCM debacle. The so-called "smartest guys in the room" simply turned out to be the most reckless. The melt-down they caused was a nice little practise run for the one that occurred ten years later. Shame that no-one seemed to learn anything from it.

8. **Shantanu**
October 9, 2015

NeapTide, nice summary. Just to add that Kajita was the co-analysis coordinator
of the atmospheric neutrinos and proton decay group at Super-K (the other co-chair
was yours truly's thesis supervisor). But Kajita also did the similar analysis in Kamiokande and had given the talk at Neutrino 98. Also Koshiba's nobel prize was for neutrino astrophysics.

However, its still not clear to me as to what exactly we have learnt about BSM physics
from neutrino data (and not just hints).
shantanu

9. **martibal**
October 9, 2015

Peter,
yes you are right, I do not know Turok but being Director of a prestigious private institute can justify a bit of propaganda for fundraising purposes 🙂

10. **Art Brown**
October 9, 2015

From the wallpaper behind Turok it looks like Perimeter favors the West Coast metric.

11. **Yatima**
October 9, 2015

NeapTide says: *Also, there are several major experiments underway or planned in the neutrino world*

I didn't know so many neutrino experiments where in the pipe. Excellent. Still, I suppose the first results will be in a decade or two? I still remember reading in a popular science magazine about Kamiokande II obtaining data from Supernova 1987A, back when Z80-based home computers were a thing.

12. **srp**
October 9, 2015

It is true that there are a number of sucker strategies that involve the risk equivalent of writing out-of-the-money options, allowing you to make "free money" for a while before being ruined by the occasional "fat tail" event. Whether physicists are more arrogant than finance/econ types or more prone to fool themselves that their strategies are foolproof would be an interesting empirical question.

13. **Bane**
October 10, 2015

Srp: I believe there's a saying in finance: Losing money when everyone is losing money is seen as not good but understandable. Not making money when everyone else is making money will get you fired.

In other words, it really doesn't pay (literally) to concentrate too much on strategies which take measures to avoid extreme downsides, since doing so will pretty much have to reduce your "normal" profitability and that's what the employer cares about.

14. **sm**
October 10, 2015

"I have never denied that the most profitable investment for a wealthy person in Russia is an investment in politics" – Boris Berezovsky.

He learnt this from the Americans who 'helped' (themselves to) Russia in the early 1990s.

I would add that that if you have been accused on Capitol Hill of tax avoidance to the tune of six billion dollars, Berezovsky's observation is spot on.

15. **Jeff M**
October 10, 2015

The coverage of Mochizuki's supposed proof of abc is really strange. First, everyone seems to compare him to Perelman, which is silly. Perelman (who proved the Thurston geometrization conjecture, a much bigger deal than just the Poincare conjecture) was working in a well known field, with a well known idea, which went back to Thurston. His student Hamilton had worked on it for a while, but got stuck, and couldn't get unstuck. People in the area, which is pretty close to mine, always thought Hamilton's approach was maybe the way to go, it was just that no one could get past some problems. Perelman did. I was at a conference not long after it was announced, and various big shots were reading his stuff and trying to work out if it was OK. Even then, the feeling was it probably was fine. Mochizuki, no one as far as I can tell, has a clue. Grothendieck wasn't like that either, he made enormous strides, but people in the field knew quickly they were enormous strides. Maybe he's right, or close, but he's going about it very very weirdly. In math, people are usually very very good about sharing their work, and explaining it. Perelman's papers were understandable, they just took some normal refereeing, more or less. Mochizuki no one knows anything, as far as I can tell. And it's not like it's not people who know what they're talking about. Faltings? Really? On top of everything else that's his thesis advisor. When de Branges claimed to have the Riemann hypothesis, I remember talking to Peter Sarnak, who told me in no uncertain terms it couldn't be, other people had tried the same technique, and realized it couldn't work. I have the same feeling about this – could be wrong, but I wouldn't bet on it.

16. **Peter Woit**
October 11, 2015

Jeff M,
I think you're right that this situation is not similar to the Perelman one, but I don't think many people see it as similar, especially now that it has been three years, and experts still don't understand the purported proof. What Perelman did was post basically an outline of a proof, far from something refereeable as a proof. But it was quite comprehensible to experts, who could then do the work to check details and write up a refereeable proof.

Mochizuki's papers purport to contain a refereeable proof, but it seems that almost no experts have been able to understand these papers and their argument well enough to judge whether they contain a valid proof. It's a very unusual situation…

By the way, Hamilton wasn't Thurston's student, he's older, got his degree in the 60s.

17. **captain obvious**
October 11, 2015

Perelman's paper had a key advance at the very beginning of the work that could be checked as a routine (for experts) calculus exercise, i.e., that differentiating his entropy functional along the Ricci flow gave a positive quantity, so it was increasing. And then he gave a quantity whose gradient was Ricci flow!! Incredible stuff that could be checked by direct calculations assignable to a graduate student. No big new terminology getting in the way, and immediate visible easily checked insights that clearly had the potential to do what he claimed.

Wiles also had quick, if not immediate, spinoffs from the main argument such as neat lemmas in commutative algebras.

18. **Jeff M**
October 12, 2015

Peter,

About Hamilton, thanks, I can't remember why I thought he was a student of Thurston, maybe just confusing him with Anderson. And you're right about what Perelman posted, but as captain obvious points out, Perelman's outline was pretty easy to check, and from what I remember it was in fact pretty clear that what he said was right, and very very clever, and fit right in with a bunch of previous work. Filling in the details took some time, but everyone I was talking to was very confident from the get go it would work. Wiles was a bit more interesting, it was clear he was on to something, again he was working with well known ideas, plenty of people had a feeling about Fermat once Taniyama-Shimura hit. The hole in Wile's proof took serious work to plug, but again the experts I talked to always thought it was doable.

19. **Bill Daniels**
October 12, 2015

Didn't Terry Tao solve an Erdos posed problem recently? Didn't see anything on this blog about it. But more interesting than Turok's TED talk. A nice contrast to the ABC situation.

20. **RandomPaddy**
October 13, 2015

A proof means your agument has to convince another person. If you can't do that then you haven't proved anything. The theoreom is either right or wrong to begin with but your proof of it is an entirely seperate thing.

21. **Anon**
October 14, 2015

RandomPaddy, mathematical statements are not necessarily right or wrong to begin with. See, e.g., the Continuum Hypothesis, which is unprovable in our usual foundational systems for Set Theory. We can either assume its truth or its falsity as an axiom and get a consistent theory (assuming ZF set theory is consistent to begin with).

22. **Peter Woit**
October 14, 2015

All, please resist the temptation to use off-hand remarks in comments to start off-topic discussions. I promise to try and find time later today to come up with some new material. Anything to head off a discussion of the foundations of set theory….

23. **Low Math, Meekly Interacting**
October 14, 2015

I liked most of Turok's lecture. He's one of those rare critics whose style is so genteel and sweet he can make a body blow feel like a warm hug. I've worked with a couple people like that. They have the gift of making you feel unburdened when you give up on a stupid idea, rather than just stupid for having pursued it. He left me pretty cold when he started pitching his own highly speculative ideas without adequate disclaimers, though. I find it persuasive that the apparent simplicity of the universe on vastly different scales may be an important clue, and scale invariance of one form or another may be a fruitful avenue of investigation. I'm happy to be shown wrong, but if I understand his and Steinhardt's work correctly, what largely distinguishes their cyclic universe models from inflation, as far as we can currently observe, is the lack of measurable B-modes in the CMB. Maybe inflation's ability to give you any B-mode prediction you want disqualifies it as science, but the fact remains you can't use the absence of a signal to distinguish between the two. I really hope gravity wave detectors advance as much as Turok predicts, but the jury still seems very much out on that one. It's also not clear to me how "cyclic universe" models aren't just multiverse models with a different topology. Lastly, the philosophical point he makes during the Q&A sounds perilously like positivism to me. "More of the same" beyond our cosmic horizon seems the most parsimonious hypothesis. Would some other possibility not simultaneously deviate from the Cosmological Principle and invoke a landscape of some sort? I know we can't say either way with absolute certainty, but the actual evidence seems more supportive of mediocrity, and, with a few caveats, it's a fair stance to have based on current knowledge, isn't it?

I'm not trying to debate these points, just offering my view that Turok's gently devastating critique of stringy anthropism in its current guises, as valuable as it is, may not make up for enthusiastically popularizing ideas that perhaps themselves are open to serious philosophical question.

24. **not ed**
October 14, 2015

Another case of prizes going to those in least need of funding? "Exceptional Achievement in Research" etc

Comment edited, I believe the included link was meant to send people here:
http://www.aps.org/programs/honors/prizes/apsmedal.cfm

25. **Leonardo**
October 15, 2015

Go Yamashita was writing "a proof of abc conjecture after Mochizuki" which would supposedly clear up Shinichi's work and put it on a more familiar ground to other mathematicians. Yamashita is known to this kind of stuff, as evidenced in some of his past papers, such as "a simple proof of convolution identities of Bernoulli numbers", in which he took the work of T. Agoh and K. Dilcher, whose proof was complicated calculations in more than 10 pages, and simplified it to just one formula on a new kind of generating function. It was expected that Go's work would severely lower the barrier for others to understand IUTeich (although I'm not sure he would actually cover all of it).

However, there's been no news on this front for a long time.

Does anybody know if Yamashita is still working on this? Should we expect it to be ready on time for December's workshop? I'm pretty confident this is the refreshing breath IUTeich needs.

26. **Davide Castelvecchi**
October 17, 2015

Thank you very much Peter, I am glad you liked the article!

Jeff: I hope I made it abundantly clear in my profile of Mochizuki that his case is different than Perelman 🙂

Leonardo: My understanding is that Yamashita has now finished writing up his notes in Japanese and that they are currently being translated into English.

27. **Leonardo**
October 17, 2015

Davide, those are great news. Are you aware of the scope of his notes? I hope they are a self-contained proof of the abc conjecture through IUTeich and not just a survey as was Fesenko's. Of course, both are pretty helpful and welcome, but I'm of the opinion that a full double-check, independent, paper would be far more important. Well, let's wait and see. The Oxford Workshop is just around the corner, many news should come from it.

28. **Curious Mayhem**
October 27, 2015

Here's a nice summary of the Noble Prize for neutrino masses and mixing:

http://physics.aps.org/articles/v8/97

The page has links to the key original papers.

Prizes and Other Stuff

October 15, 2015
Categories: Uncategorized

- There will be an awards ceremony November 8 for the 2016 Breakthrough Prizes, hosted by Seth MacFarlane, and airing live on the National Geographic Channel, later to run on FOX. The next day at Berkeley there will be a Breakthrough Prize Symposium, featuring talks by the winners and others.

  In physics the prizes to be awarded include the big $3 million prize and up to three $100,000 prizes for young researchers. The symposium physics schedule lists as speakers "2016 Breakthrough Prize Laureates", with the plural perhaps a hint that more than one person will be sharing the $3 million. The first three rounds of these mostly went to string theorists but there seems to have been some sort of policy change last year (the award went to Supernova observations indicating an accelerating cosmology). I have no idea at all who they'll choose this year. The theorist speakers discussing the future of the subject at the symposium are Arkani-Hamed, Hall and Bousso, a clean sweep for multiverse mania.

  On the mathematics side, there's also a $3 million prize and up to three $100,000 prizes. The symposium schedule lists "2016 Breakthrough Prize Laureate", so maybe a hint there's just one in math.

- In other news on the prize front, the APS will now be awarding a Medal for Exceptional Achievement in Research, with the first one going to Edward Witten.
- The Wall Street Journal has published a response to the Gross/Witten piece advocating a Chinese "Great Collider". Physicist Jonathan Katz describes particle physics as a "dying", "moribund" subject. He argues that research funding should go to "tabletop experiments", like for instance the ones he does.
- The new AMS Notices is out, with a piece by Loring Tu on the origin of the theorem due to Atiyah-Bott often known as the Woods Hole Fixed Point Theorem. Tu does a great job of explaining this beautiful mathematics, as well as the details of the controversy over Shimura's role in sparking this by a conjecture. He doesn't much discuss Shimura's memoir, which I wrote about here, which includes the claim that Serre attacked him out of jealousy about Shimura's conjecture. I've never really understood Shimura's point of view on this, since usually mathematicians value theorems over conjectures, and it is clearly Atiyah-Bott who had the theorem here.
- From David Mumford's blog I learned about an experimental neurobiology paper co-authored by Atiyah. It is quite interesting, but even more interesting is the blog entry by Mumford that it inspired. Mumford is one of the greats of algebraic geometry, and he gives a fascinating characterization of the different sorts of ways in which mathematicians pursue research. Mathematics done at the highest level involves strikingly different personalities and research strategies, which Mumford characterizes as four different tribes: "explorers", "alchemists", "wrestlers" and "detectives". If you're at all interested in how mathematics research is done, this is highly recommended reading.
- Also recommended, if you're interested in the overlap of physics and philosophy, is a new piece by Massimo Pigliucci on String Theory vs. the Popperazi.

Comments

1. **si**
   October 15, 2015

   The part of the Atiyah's paper I found interesting is the ranking of a bunch of formulas as beautiful or ugly by mathematicians. Not surprisingly, the Euler formula ranked consistently as the most beautiful. But the one ranked as the "ugliest" was the Hardy-Ramnanujan formula for calculation of pi. I know it has strange, seemingly random constants, but I consider these as the essence of its beauty, not ugliness. I think one cannot talk about mathematical beauty without also considering what is ugly. My guess is that there is a wider range and variation in the ugly side.

2. **zzz**
   October 15, 2015

   Table top experiments, yeah that sounds like a good idea.
   Google 'Jonathon Katz physicist' to see what he is doing.

   yikes

   http://www.riverfronttimes.com/newsblog/2010/05/19/controversial-wash-u-prof-jonathan-katz-cut-loose-from-gulf-oil-spill-relief

3. **Bill**
   October 15, 2015

   It says on the website you linked to: "Each of the Breakthrough Prize laureates receives a $3 million prize, and a total of seven prizes will be awarded."

   First of all this means that the laureates don't share one prize, but also gives more than a hint that there will be multiple winners.

4. **Peter Woit**
   October 15, 2015

   Bill,
   I think that info is for the first round of these awards several years ago. After those awards, now the plan is for a single $3 million award/year (one in physics, one in math).

5. **Bill**
   October 15, 2015

   My guess is that the prize in Mathematics will go to Yitang Zhang.

6. **Davide Castelvecchi**
   October 16, 2015

   The "beauty of math" paper actually came out last year
   http://www.nature.com/news/equations-are-art-inside-a-mathematician-s-brain-1.14825

7. **op ed**
   October 16, 2015

   About WSJ and Katz and the Chinese Great Collider, look up what Phil Anderson had to say about the SSC. For example this http://www.the-scientist.com/?articles.view/articleNo/8688/title/The-Case-Against-the-SSC/

or this
http://www.math.columbia.edu/~woit/wordpress/?p=6419

See also if you can find Rustum Roy's numerous articles/speeches against the SSC.
Ultimately the SSC projected cost ballooned to about $8.4 B and Congress pulled the plug. But that pot of money did not flow into the hands of condensed matter physicists. Instead the consequence of the SSC's termination was a general cut in funding across all of science (or maybe just physics). Essentially, "if the HEP community can absorb an 8B loss of funding, then other areas of physics can also learn to make do with less".

Science funding is not a zero sum game, where if money is cut from one area, it goes to another.

8. **srp**
October 16, 2015

Regarding the Great Collider of China and other science mega-projects, I recommend Freeman Dyson's 1988 essay "Six Cautionary Tales for Scientists" in his collection From Eros to Gaia. He there describes how the internal ecology of a field can be ruined by a too-large unripe Great Leap Forward project. It isn't a question of sending the money from HEP to condensed-matter; it's a question of distorted resource allocation within HEP itself. The essay is also a beautiful and fascinating read.

9. **leap**
October 16, 2015

The key word/concept is "unripe". I suppose one can say the SSC was unripe and too big of a leap forward. Who knows. It was certainly jingoistic, and that rubbed many people the wrong way. But Freeman Dyson was also unhappy about HST, advocating smaller telescopes or whatever. (See if you can find cartoons from the 1980s about the Large Space Binoculars.) Who is to determine what is too big of a leap forward? Usually only hindsight decides that.

Freeman Dyson is a good author (so is his son George). Read "Disturbing the Universe" it's another fascinating read.

10. **Bernd**
October 19, 2015

Op ed,

you're right about science funding not being a zero-sum game*, but then building such a gigaproject with no clear vision other than "umm, maybe nature will unexpectedly throw something at us" is going to result in a backlash against all science funding in the long run.

* Although to a large part, it is. And, of course, a lot of science funding does not go to scientists. How much of the LHC budget went to construction companies?

11. **srp**
October 19, 2015

The HST turned out to be a great thing, but the opportunity cost of it for astronomy was pretty high, especially given all the problems it had leading to extra delay and expense. Once a project like that is up and running, the incremental cost per discovery is quite low–most of the costs are sunk upfront. So it makes good sense to support these projects' continuing operation ex post, as then they are the cheap option. But one should not allow this post-investment optimality to obscure the potential pre-investment suboptimality of a project that crowds out all others in its field.

12. **Low Math, Meekly Interacting**
October 20, 2015

It's probably unfair to characterize much of modern functional neuroimaging as "the new phrenology", but studies on the vanguard of subjective experience and their voxelated correlates need to be viewed with a great deal of skepticism. It's only very recently that we're approaching being able to use more advanced neuroimaging to reliably diagnose devastating neurodegenerative diseases like Alzheimer's. For subtler (but not terribly subtle, really) conditions like major depression, bipolar disorder, and schizophrenia, these techniques have essentially no value as diagnostic tools, and it's only with considerably larger sample sizes and impeccable statistics have reproducible correlations generally survived much beyond the first reported observation.

The paper is remarkable, though. A Fields medalist senior-authoring a neuroscience article in an open-source journal? That's an encouraging development, if nothing else. And the language…I assume Atiyah wrote the stuff that doesn't read at all like a bioscience manuscript. I mean, it's loads of second-hand rhapsody slathered on a base coat of anecdote, but very lovely all the same, and a damn sight more enjoyable to consume than most of what I have to slog through on a regular basis.

13. **Paul D.**
October 27, 2015

The WSJ has also published an essay that is down on basic science in general.

http://www.wsj.com/articles/the-myth-of-basic-science-1445613954

14. **Yuri**
November 3, 2015

"The Breakthrough Prize today announced that ten-time Grammy Award winner Pharrell Williams will perform at its third annual Breakthrough Prize ceremony, held to honor the world's top scientists and mathematicians, on Sunday, November 8 in Silicon Valley.

Williams will be joined at the exclusive ceremony by a star-studded lineup of presenters, including Academy Award winner Russell Crowe, Academy Award winner Hilary Swank, Lily Collins, and Thomas Middleditch and Martin Starr of HBO's Silicon Valley. As previously announced, Emmy-nominated Cosmos executive producer and Family Guy creator Seth MacFarlane will serve as the ceremony's host."

The Latest Information on Information

October 17, 2015
Categories: Uncategorized

The field of hep-th has always been quite faddish, with many of the fads easily recognizable just from looking at the buzzwords appearing in paper titles. In recent years "entanglement" is a buzzword that has been all the rage, and John Preskill has some data here (slide 3) on how many hep-th papers have it in their title. Extrapolating from 62 in 2011, 119 in 2013 and a projected 220 this year, long before we see a new accelerator, all hep-th papers will have "entanglement" in their titles. Another very visible trend is that an increasingly large fraction of these and other papers in hep-th (which used to mean high-energy particle physics) are now about low-energy non-particle physics topics.

I make periodic attempts to listen to talks or read papers explaining the hot topics at issue, but have to confess that I tend to lose interest, not seeing anything relevant to the standard model or unification, or to the kind of deep mathematics that in the past has provided insights into those topics. Suggestions of what to read to follow these latest fads are welcome, when I have more free time I'll look into them. In the meantime, I'm just reporting a trend, will leave it to others to decide what it all means.

This brave new world of hep-th is generating a lot of activity. The week before last saw a big "The Information Universe" conference in the Netherlands, that addressed questions such as

– Is the universe one big information processing machine?
– Is there a deeper layer in quantum mechanics?
– Is the universe a hologram?
– Is there a deeper physical description of the world based on information?

Last week was the kick-off meeting of the It from Qubit collaboration, which is supposed to bring together quantum information theory and fundamental physics. This is an extremely large effort, very well-funded by the Simons Foundation. They just announced that they're planning on hiring a dozen or so postdocs this year. If you get one of those jobs, there's a warning that you'll have a "significant burden" of having to travel to collaboration meetings in places like Bariloche, but at least on long flights you'll be flying business class.

For a detailed explanation of the plans of It from Qubit, see here.

After the Simons-funded meeting a few days ago at Stanford, this weekend there's yet another quantum information/entanglement/HEP meeting at Stanford, this one funded by the Templeton Foundation. The program for the Templeton Program meeting is here.

Update: It seems that this page has been edited this weekend to remove reference to the It from Qubit collaboration travel policy, however you can still find it here.

Update: For popular expositions of these ideas, there's a new article in Science News, and a book coming out by George Musser, reviewed today by Sabine Hossenfelder. I haven't seen the book, but the Science News article doesn't help me understand much: all I get out of it and the papers its links to are some very vague conjectures about understanding quantum gravity via AdS/CFT. Standard facts about quantum mechanics on the CFT side become more exciting sounding conjectures about gravity on the AdS side, but it remains unclear to me exactly how any of this is supposed to really work. For one thing, we don't live in AdS space.

Comments

1. **Steamy Ray Vaughn**
   October 17, 2015

   I saw Leonard Susskind give a lecture as long ago as 2013. I vaguely remember, he said something like '… by the way, we're undergoing some kind revolution in entanglement… every day [week?] there's a new paper out…'

2. **Matt W**
   October 17, 2015

   Modelling the fadishness of HEP-TH titles might actually be an interesting little project. Am wondering if each fad (information, firewalls, multiverse, etc) follows the same initial geometric increase followed by a plateau and decay.

3. **Anonyrat**
   October 18, 2015

   The fadishness of technology has been well-captured in the Gartner Hype Cycle.
   http://www.gartner.com/technology/research/methodologies/hype-cycle.jsp
   It is likely that the curve they draw is of validity for scientific topics as well.

4. **anon**
   October 20, 2015

   I like it when they have these conferences at kitp, because then all of the videos are available and promptly posted. Thanks for pointing this meeting out.

5. **Jeff M**
   October 25, 2015

   Well, Peter seems to be being finicky with posts about this, but here's a try. I read the it from qubit writeup pretty carefully, and found it quite scary. Does that really pass for science now? It reads like a press release trying to sell a new piece of software. Conjectures are theorems. Completely open problems are discussed as if there is some sort of established theory about them. Rigor is completely absent. There is no expectation that anyone will have to prove anything, ever, mathematically or via experiment. HEP has jumped the shark.

6. **Peter Woit**
   October 25, 2015

   Jeff M,
   Physicists have never been much interested in rigor or proof, so you can't really criticize this project on those grounds. Some parts of what they are doing though seem to be completely divorced from any contact with experiment, so a real question arises as to how you evaluate work like this if the usual standards of neither mathematics nor physics apply. That their proposal leads off with some pretty extreme hype ("A rapid change is taking place in our thinking about fundamental physics that amounts to a

nascent paradigm shift.") and also makes an argument based on the exponential increase in use of a buzzword, isn't really confidence inspiring.

Personally I'm dubious that quantum information theory is going to revolutionize our understanding of fundamental physics (eg. the SM and gravity), or our understanding of QFT (it seems more likely that our understanding of QFT may revolutionize quantum information theory). I see not a hint here of anything addressing the problems of the SM or unification, and the problem with claims to have a quantum theory of gravity has always been that, in isolation from the rest of physics, quantum gravity theories seem to be untestable. But, that's just me, in a few years we'll see what emerges from this activity, and maybe it will be clear if there's anything to the revolutionary claims. The danger is that the claims will just get a lot more exposure, with the problem of evaluating whether this is a useful paradigm shift never solved.

While I don't think it's going to be a revolution in physics, quite possibly some interesting things will come out of this research. Hopefully part of what the grant will fund will be some readable expository literature, such that one can learn what people have found without having to wade through mountains of hype.

7. **Neil**
October 25, 2015

Musser has a Cliff's Notes summary of his book in the latest SA. I read it twice and still got nothing out of it. I am a bit reluctant to invest my time reading his book until someone whose opinion I respect, like Peter, tells me it is worth my time.

On it from bit, I don't understand why this is a promising path other than the fact that Shannon's theorem is a transformation of the Second Law.

8. **Jim Akerlund**
October 26, 2015

Off Topic.

Nautilus magazine has a good article on Sloane's encyclopedia of number sequences.

http://nautil.us/issue/29/scaling/how-to-build-a-search-engine-for-mathematics

9. **Jeff M**
October 27, 2015

Peter

I completely appreciate that physicists don't do proof (it's a running joke among us mathematicians) but I had hopes for rigor. Perhaps I'm misremembering, but when I worked through the field equations of GR or Dirac's equation for the relativistic electron I seem to remember a lot of rigor. Ah well, perhaps it's good you're in the math department 🙂 As for Jim Akerlund and Sloane's Encyclopedia of Integer Sequences, it's an amazing resource. I was working on something and we got this sequence, we plotted it and stared at it, and knew it had to be something, but no one we talked to had ever seen it. Plugged it in to EIS, found out it was Stern's sequence, discovered in the 1840's, used to help make watches, more or less lost by the 20th century (though used to construct the rationals). Lots of fun to reference a paper written in the middle of the 1800's…

10. **Dave Miller in Sacramento**
October 27, 2015

Neil wrote:
>On it from bit, I don't understand why this is a promising path other than the fact that Shannon's theorem is a transformation of the Second Law.

I think even that is an over-statement: I've done some work involving information theory (I hold some patents on implementations of error-detection-and-correction systems). The way I'd put it is that anything that involves multiplicative counting (you know m choices followed by n choices, etc.) lends itself to taking logarithms, and then it inevitably looks a little bit like entropy. Of course, Shannon chose to use the word "entropy," hence he encouraged this sort of over-statement. But, I don't think Shannon's theorem really is a transformation of the Second Law.

By the way, I am not attacking you personally: I know that your statement is a widely accepted view. I'm just pointing out that, in my experience working in both physics and information theory, that widely accepted view is misleading.

However, like Peter, I am open to being shown to be wrong: I've always been curious about "It from Bit" (I went to a lecture by Wheeler pushing the idea way back in the mid-'70s), even though I'm skeptical.

Dave.

11. **Neil**
October 27, 2015

Dave,

Yes, I misspoke. I meant Shannon's equation is a variant of Boltzmann's entropy equation. But, as you point out, that could just reflect that they both deal with combinatorics.

12. **Chris W.**
October 27, 2015

Dave and Neil,
Wikipedia has [a pretty good discussion](#) of this very topic. By now it has a long history.

13. **Baruch Garcia**
October 27, 2015

If you wish to learn more about ¨It from Bit¨ from Wheeler´s perspective, I suggest reading his autobiography – Geons, Black Holes, and Quantum Foam. His work is also mentioned in Amanda Gefter´s book – Trespassing on Einstein´s Lawn. An archive of his papers can be found at jawarchive.wordpress.com.

14. **Peter Woit**
October 27, 2015

Please, enough about "It from Bit", which, as far as I can tell, has little or nothing to do with "It from Qubit". I would be interested in hearing more about this second topic from someone who understands recent developments. Personally I've never seen anything interesting coming out of the first topic, but replacing bits by qubits does seem more promising.

15. **Dave Miller in Sacramento**
October 28, 2015

Peter,

Most of the "It from Bit" presentations going way back that I remember seeing did in fact invoke quantum mechanics — certainly Wheeler did so, and I remember even old sci-fi novels that did so.

And, the idea of tying in wormholes to quantum entanglement also has a long history, at least going back to Dave Deutsch's famous Phys. Rev. article, which tied his theory of quantum computation into wormholes, time travel paradoxes, and parallel universes: certainly the most interesting case for many-universe theory I've ever seen, even if it was a bit, let us say, speculative (I always wondered how he managed to get the Phys. Rev. editors to approve it for publication!).

Dave

16. **Will Nelson**
October 28, 2015

I feel like the entanglement and more generally "information" angles are sort of under-delivering. It's not clear to me what, if anything, has been gained by focusing on the "information" of a quantum system or a putative analogies with computation (Lloyd: "the universe is a quantum computer, computing itself"…gee thanks for that!) With entanglement the goal is to see spacetime "emerge" from specific forms of entanglement, but we already see this kind of entanglement in the states of a standard QFT, and it doesn't seem to be particularly helpful (so far) to think of this is as creating the spactime.

Well I think it's unwise to ignore the fads or hot topics of hep-th since plenty of smart people obviously think they are promising, but I do think these two may be not as promising as hoped.

17. **Anonyrat**
October 28, 2015

An interview with Abhay Ashtekar of general interest.
http://thewire.in/2015/10/29/good-scientists-solve-problems-but-great-scientists-know-whats-worth-solving-14279/

A little clip about string theory:

There's even been talk of "post-empirical science".

Yes. There was another Strings conference in India at TIFR, in 2001. I happened to be in India at that time – people had just discovered that the universe is going through accelerated expansion, so that the cosmological constant may be positive. And I saw in newspapers that Tom Banks and Edward Witten had said that, no, the cosmological constant cannot be positive because it is incompatible with string theory. It has to be negative, they said. And that these observations are premature. They were completely wrong. The fact is that nobody goes back to these things and says, well, let us be a little more modest about it.

It's like shifting the goal post.

Exactly! There is nothing wrong with making the statement. But then ignoring completely that you made that statement – that is wrong. And then to say that this is the only theory. It has not had hard experimental/observational success, and it has not made that much progress in quantum gravity.

18. **Mitchell Porter**
November 3, 2015

Abhay Ashtekar supposedly said

"And I saw in newspapers that Tom Banks and Edward Witten had said that, no, the cosmological constant cannot be positive because it is incompatible with string theory. It has to be negative, they said. And that these observations are premature. They were completely wrong."

This comment makes no sense. Witten's talk – available at http://arxiv.org/abs/hep-th/0106109 – was about quantum gravity in de Sitter space. He discusses theoretical difficulties but certainly does not conclude that it's impossible. As for Tom Banks, I don't see him in the Strings 2001 program but the claims make even less sense in his case, since he was already writing papers specifically about adapting string theory to do Sitter space.

19. **Anonyrat**
November 3, 2015

Well, Jan 1, 2002, Dennis Overbye wrote in the NYTimes:
http://www.nytimes.com/2002/01/01/science/the-end-of-everything.html

> In 1998 the two teams announced that instead of the expected slowing, the galaxies actually seem to have speeded up over the last five or six billion years, as if some "dark energy" was pushing them outward.
>
> "It's definitely the strangest experimental finding since I've been in physics," Dr. Witten said. "People find it difficult to accept. I've stopped expecting that the finding will be proved wrong, but it's an extremely uncomfortable result."

which means that at sometime, per the newspapers, Witten thought that the result must be wrong, etc.

Anyway, Ashtekar (elsewhere in the interview) remarks on S. Chandrasekhar's powers of memory, and disclaims having any such.

20. **Peter Woit**
November 4, 2015

Mitchell Porter,

Witten had this to say at the time
"This means that there is no classical way to get de Sitter space from string theory or M-theory… In fact, classical or not, I don't know any clear-cut way to get de Sitter space from string theory or M-theory. This last statement is not very surprising given the classical no go theorem. For, in view of the usual problems in stabilizing moduli,

it is hard to get de Sitter space in a reliable fashion at the quantum level given that it does not arise classically".

Ashtekar may not be quite accurate, but do you have any doubt that if the data showed the universe to be AdS, this would now be heavily advertised as evidence for string theory?

21. **Anonymous**
    November 5, 2015

    The It from Qbit hype is certainly over the top, partly due to the personalities involved, some of whom have a habit of declaring their own work revolutionary based on very little that is solid. There have been various fads over the years proclaiming new theories of spacetime, including non-commutative geometry, deconstruction, two dimensional gravity, et cetera. They have each been interesting in some ways, but none really lived up to the claims of proponents.

Russian Math-Physics Film News

October 27, 2015
Categories: Uncategorized

I'm pressed for time, heading out tomorrow for a short vacation in San Francisco, but I did want to write a little bit here before leaving. Last year around now, a theme was Hollywood blockbuster films with physics/math themes, this year there seem to be none of those, instead I'm hearing about some Russian films with such themes.

This evening here at Columbia there was a showing of Ekaterina Eremenko's [Colors of Math](), an exploration of the sensual nature of mathematics, pairing research mathematicians with the five senses. Part of it is [available on Youtube](). Eremenko went to graduate school in mathematics in Moscow, then later on to a career in modeling and TV, and in recent years has been making films (see [here]()). Her current project is called [The Discrete Charm of Geometry](), trailer is [here]().

Somehow I'd missed until today hearing about a truly fantastic Russian film project which has been going on for years, showing no signs yet of reaching an endpoint. The topic is the life of the great Russian theoretical physicist Lev Landau, and the London Review of Books has a column by James Meek about the film [here](). It seems that the director, Ilya Khrzhanovsky, has been working on this for a decade, creating a huge set in Kharkov, where Landau worked during the 1930s, and doing his best to recreate the time as accurately as possible. Meek describes the shooting as follows:

> For more than two years, between 2009 and 2011, hundreds of volunteers, few of them professional actors, were filmed living, sleeping, eating, gossiping, working, loving, betraying each other and being punished in character, in costume, with nothing by way of a script, on the Kharkiv set, their clothes and possessions altered, fake decade by fake decade, to represent the privileged, cloistered life of the Soviet scientific elite between 1938 and 1968.

Among those brought in to participate in all of this were well-known physicists and mathematicians Dmitry Kaledin, Nikita Nekrasov, Andrey Losev, Carlo Rovelli, David Gross, Shing-Tung Yau and Alexander Midgal.

Post-production is now going on in London, in a huge five-story building in Mayfair, financed by someone who doesn't want to be identified. It's unclear what will emerge from the seven hundred hours of film shot in Kharkiv, perhaps "a dozen or more movies, a TV series, and a user-directed internet narrative system." Whatever it is, I'm very curious to see the result.

Comments

1. **SimpleBiologist**
   October 28, 2015

   Eremenko also did short films about Nash and Nirenberg for Abel price foundation.

   http://www.abelprisen.no/artikkel/vis.html?tid=63683

2. **Visitor**
   October 28, 2015

   "For more than two years, hundreds of volunteers, few of them professional actors, were filmed living, sleeping, eating, gossiping, working, loving, betraying each other and being punished in character with nothing by way of a script, on the Kharkiv set…"

   Because if you put enough amateurs together, and leave them to their own devices, the results are bound to be professional. Or maybe not.

   Don't expect too much in the way of a watchable movie.

3. **Boris**
   October 28, 2015

   I don't think the youtube piece mentions the name of Anatoly Fomenko (starting at 5:00), you can find more of his drawings on the internet. Also, it is unfortunate that Perelman wasn't living on that set in Kharkiv.

4. **Peter Woit**
   October 28, 2015

   Visitor,
   I agree there may be a "watchability" problem with whatever emerges, but if you compare to last year's example of a film portrayal by professional actors of mathematicians gathered in a research institute of the period (Bletchley Park), I'd much rather watch real mathematicians dressed up doing who-knows-what than the Hollywood completely bogus version of what supposedly happened.

5. **Chris Oakley**
   October 28, 2015

   Re: Hollywood's interpretation of Bletchley Park, i.e. *The Imitation Game:* it should have been listed under comedy. It seems that having built the enormous and complex bombe – entirely without the help of any technicians, it seems – Turing still does not realise that its purpose was to search for cribs until someone says something in a pub about the possibility. So I suppose that he was building the machine for the sheer fun of it up to that point. He was apparently also a traitor.

   I really do not mind professional actors reciting technical jargon with obviously no understanding, but I do object to needless blackening of peoples' names in the name of "drama".

6. **Chris W.**
   October 28, 2015

   So perhaps the lesson from *The Imitation Game* is that no script might be better than the deliberate hash that a few professional scriptwriters could make of the actual events, especially with producers breathing down their necks. There is nothing stopping those volunteers, especially if many of them are mathematicians, from studying the original history and the personalities involved and improvising a story that bears a decent resemblance to what actually happened. A smart director who cares about the history might help a lot.

   Anyway, we'll see. The Moscow News has [this story]() about the making of *Colors of Math*. (There is an NYU connection.)

7. **Luca Signorelli**
   October 29, 2015

   Hi Peter, on a completely different topic, have you seen this?

   http://thewire.in/2015/10/29/good-scientists-solve-problems-but-great-scientists-know-whats-worth-solving-14279/

8. **Chris W.**
   October 29, 2015

   Luca, thanks for pointing to that wonderful interview. It deserves to be widely read.

9. **Chris W.**
   October 30, 2015

   PS: On that interview, see Anonyrat's comment on the previous post, where it is more directly relevant. (Post responses there, not here.)

10. **theoreticalminimum**
    October 31, 2015

    Regarding Landau, it's probably worth mentioning the excellent 2013 collection, by physicist Mikhail Shifman, of memoirs and reviews on the scientific activities of some the most prominent theoretical physicists belonging to the Landau School, Under the Spell of Landau: When Theoretical Physics was Shaping Destinies (World Scientific).

11. **AcademicLurker**
    November 2, 2015

    Is there a decent biography of Landau in English? I haven't seen one.

    Feynman and Landau are widely agreed to be the 2 greats of the first post-quantum generation; the contrast between the huge amount of Feynmanalia and the relative lack of material on Landau is striking. Blame the cold war, I guess.

    I do recall how at Caltech, the bookstore had one whole shelf filled with copies of the Feynman lectures and another shelf filled with Landau and Lifshitz.

12. **Richard**
    November 2, 2015

    > Is there a decent biography of Landau in English? I haven't seen one.

    Neither have I, and I'd certainly seek out and read such.

    Much simpler than that: "Landau's List" is a nice piece of non-apocryphal folklore, but (aside from the few top Newton, Einstein, Dirac entries) its contents seem to be relatively obscure. Any leads?

The Elephants in the Room – What every physicist should know about string theory

November 3, 2015
Categories: Uncategorized

The string wars seem to still be going on, with the latest salvos coming from Ashtekar and Witten. In a very interesting recent interview, at the end Ashtekar has some comments about string theory and how it is being pursued. About claims that string theory is the only possible way to get quantum gravity he says:

> I don't know why science needs such statements; indeed, scientists should not make such statements. Let the evidence prove that it's the only theory. Let the evidence prove that it is better than other theories or let its predictions be reproduced more than those of others. Science should not become theology. And, somehow such statements have a strong smell of theology, which I don't like.

About AdS/CFT and the current state of its relation to quantum gravity:

> We seem to be using these gravity ideas in other domains of physics rather than solving quantum gravity problems. I don't think that the quantum gravity problems have been solved. And I have said this explicitly in conferences with panels – in which Joe Polchinski, Juan Maldacena and I were panellists – that, in my view, this is very powerful and these are good things. However, the AdS/CFT conjecture is the only definition of non-perturbative string theory one has – and it's a definition, it's not a proof of anything. It talks about duality, but there's no proof of duality. To have a duality, A should be well defined, B should be well defined and then you say that A is dual to B. Since we don't have another definition of string theory, we cannot hope to prove that string theory is dual to its conformal field theory. You can define string theory to be the conformal field theory. You have to construct a dictionary relating string theory in the bulk and conformal field theory on the boundary. That dictionary has not been constructed in complete detail.
> Again, nobody is taking anything away from the successes that the AdS/CFT duality has had; but there is a big gap between the successes and the rhetoric. The rhetoric is at a much higher level than the successes. So, for example, in this conjecture, first of all the space-time is 10 dimensional. The physical space-time is supposed to be asymptotically anti-de Sitter, which has a negative cosmological constant. But we look around us, and we find a positive cosmological constant. Secondly, the internal dimensions in the conjecture, or this definition, are macroscopic. The Kaluza-Klein idea is that there are higher dimensions but because they are all wrapped up and microscopic, say, at Planck scale, we don't see them. That's plausible. But here, in AdS/CFT duality, they need the radius of the internal dimensions to be the same as the cosmological radius. If so, if I try to look up I should see these ten dimensions; I don't. So, it can't have much to do with the real world that we actually live in. These are elephants in the room which are not being addressed.
> … there are these obvious issues and practitioners just pretend that they don't exist. And that to me is unconscionable; I feel that that's not good science. I don't mean to say string theory is not good science, but publicizing it the way it's done is not good science. I think one should say what it has done, rather than this hyperbole.

A good example of the problems Ashtekar is concerned about is provided by an article in the latest Physics Today by Witten with the title What Every Physicist Should Know about String theory. It's devoted to a simple argument that string theory doesn't have the UV problems of quantum field theory, one that I've seen made by Witten and others in talks and expository articles many times over the last 30 years. This latest version takes ignoring the elephants in the room to an extreme, saying absolutely nothing about the problems with the idea of getting physics this way, even going so far as to not mention the first and most obvious problem, that of the necessity of ten dimensions.

The title of the article is the most disturbing thing about it. Why should every physicist know a heuristic argument for a very speculative idea about unification and quantum gravity, without at the same time knowing what the problems with it are and why it hasn't worked out? This seems to me to carry the "strong smell of theology" that Ashtekar notices in the way the subject is being pursued.

Witten is a great physicist and a very lucid expositor, and the technical story he explains in the article is a very interesting one, with the idea that most physicists might want to hear about it a reasonable one. But the problems with the story also need to be acknowledged and explained, otherwise the whole thing is highly misleading.
Besides the obvious problems of the ten dimensions, supersymmetry, compactifications, the string landscape, etc. that afflict attempts to connect this story to actual physics, there are a couple basic problems with the story itself. The first is that what Witten is explaining as a problematic framework to be generalized by string theory is not quantum field theory, but a first-quantized particle theory, with interactions put in by hand. This can be used to produce the perturbation series of a scalar field theory, but this is something very different than the SM quantum field theory, which has as fundamental objects fields, not particles, with interactions largely fixed by gauge symmetry, not put in by hand. For such QFTs, there is no necessary problem in the UV: QCD provides an example of such a theory with no ultraviolet problem at all, due to its property of asymptotic freedom.

Another huge elephant in the room ignored by Witten's story motivating string theory as a natural two-dimensional generalization of one-dimensional theories is that the one-dimensional theories he discusses are known to be a bad starting point, for reasons that go far beyond UV problems. A much better starting point is provided by quantized gauge fields and spinor fields coupled to them, which have a very different fundamental structure than that of the terms of a perturbation series of a scalar field theory. A virtue of Witten's story is that it makes very clear (while not mentioning it) what the problem is with this motivation for string theory. All one gets out of it is an analog of something that is the wrong thing in the simpler one-dimensional case. The fundamental issue since the earliest days of string theory has always been "what is non-perturbative string theory?", meaning "what is the theory that has the same relation to strings that QFT has to Witten's one-dimensional story?" After 30 years of intense effort, there is still no known answer to this question. Given the thirty years of heavily oversold publicity for string theory, it is this and the other elephants in the room that every physicist should know about.

Update: For another take on string theory that I meant to point out, there's an article quoting Michael Turner:

> Turner described string theory as an "empty vessel," and added: "the great thing about an empty vessel is that we can put our hopes and dreams in it."

The problem is that the empty vessel is of a rather specific shape, so only certain people's hopes and dreams will fit…

Update: Many commenters have written in to point out this article, but I don't think it has anything at all to do with the topic of this posting. There are lots of highly speculative ideas about quantum gravity out there, most of which I don't have the time or interest to learn more about and discuss sensibly here.

Update: It is interesting to contrast the current Witten Physics Today article with a very similar one that appeared by him in the same publication nearly 20 years ago, entitled Reflections on the Fate of Spacetime. This makes almost the same argument as the new one, but does also explain one of the elephants in the room (lack of a non-perturbative string theory). It also includes an explanation of the T-duality idea that there is a "minimal length" in string, an explanation I was referring to in the comment section when describing what I don't understand about his current argument.

Comments

1. **manfred Requardt**
   November 4, 2015

   What Abhay Ashtekar is saying about String Theory and ADS/CFT is exactly to the point. I think it is a fair description of its scientific status.

2. **Warren**

November 4, 2015

You are being too positive.
The real problem is that all approaches to high-energy theoretical physics are seriously flawed.
The Standard Model is not asymptotically free.
Even QCD is not saved by asymptotic freedom, which has the problem of renormalons.
Experience with rigorous approaches, as constructive quantum field theory or lattice gauge theory, has shown us that nonperturbative approaches do not solve perturbative problems.
Loop quantum gravity has problems even with the continuum limit, and doesn't solve renormalizability of gravity, only regularizes it.

But string field theory does exist, & people have addressed nonperturbative problems with it.
So rather than denouncing or over-promoting the various alternatives, a more useful attitude would be to just admit that there is no well-defined approach to this problem yet, and recognize the practicality of complementary formulations that are all incomplete.

3. **martibal**
November 4, 2015

Is there a free link to Witten paper ?

4. **Bori**
November 4, 2015

Is this just an American phenomenon because of the culture of self-promotion and exaggeration resulting in a natural counter-reaction? Speaking to physicists in Europe, they seem to be more pragmatic about string theory – a certain proportion of positions goes to string theorists and they do their job, but no hype and no bad feelings.

5. **Tammie Lee Haynes**
November 4, 2015

Dera Dr Woit

String Theory has "a smell of theology"?
As a Christian, I am astonished that you would make such a statement.

Christianity's claims to being true are based on the accounts of witnesses to certain events such as the miracles of Jesus. The account of what a witness saw is the very definition of empirical evidence. (Of course, from time to time all of us find witness accounts to be non-credible, which is why you are not a Christian)
But the point is this. Before String Theory can claim to have "a smell of theology", it will need to have support from empirical evidence.

Very truly yours
Tammie Lee Haynes

6. **Charles Day**
November 4, 2015

Martibal, click again on the link to the article. You'll find that it's now free.

7. **anonymous**
November 4, 2015

"motivating string theory as a natural two-dimensional generalization of one-dimensional theories is that the one-dimensional theories he discusses are known to be a bad starting point"

Loop quantum gravity itself has a roughly comparable foundational issue : we are assuming that the 3-metric tensor of gravity – or canonically transformed analogues – can be subject to a "quantization" procedure analogous to those developed for fields on Minkowski space-time treated as assemblies of harmonic oscillators : themselves quantized by techniques developed for non-relativistic finite degrees of freedom.

8. **Peter Woit**
November 4, 2015

Warren,
I think there's a difference between elephants in the room (we don't know how to connect string theory to known 4d physics, with or without going to a string field theory) and something much smaller (mice? cockroaches?), such as the renormalon problems or the Landau pole at exponentially large energies.

Charles,
Thanks for making sure that Witten's article is available.

Tammie Lee Haynes,
Perhaps theology is the wrong word. Maybe a better one for some string theory promotional activities would be "evangelical", dedicated to spreading the good word. But, all, please resist temptation to discuss religion here.

Bori,
I don't think there's that much difference between string theorists in the US and Europe. There is a bigger market here for a small number that want to evangelize. However, one thing to say about both Ashtekar and Witten is that they are the sorts who would usually much prefer to stick to the technical details of the science, and not engage in public battles. I can see why Ashtekar might have had enough of the hype over AdS/CFT, I'm not so sure why Witten feels it necessary to make this well-worn claim at this point.

Anonymous,
Sure, a basic problem of quantum gravity is that we don't know what fundamental variables to work with and/or what the correct quantization procedure is. Ashtekar is responsible ("Ashtekar variables") for what seems to me the most intriguing such choice of variables.

9. **Tom**
November 4, 2015

There's a new book coming out in December, published by CRC press, called "Why String Theory" by a young theoretical physicist named Joseph Conlon (found on Amazon). Though the description seems to indicated it is pro-string theory, does anyone have advance knowledge of its main premise? Just another semi-religious screed, or does it turn on any critical spotlights? (Admittedly, I wasn't interested enough to do a thorough web search.)

10. **garcol euphrates**
    November 4, 2015

    A link from the interview in the Wire references one of Abhay Ashtekar's formative books on cosmology, Gamow's 1 2 3 … Infinity. The link referenced contains a reading list of science books (some of my favorites) compiled by a fellow named Robert Anton Wilson, a writer of some stature, who is quoted as:
    "Wilson also criticized scientific types with overly rigid belief systems, equating them with religious fundamentalists in their fanaticism."

    Full circle!

11. **Peter Woit**
    November 4, 2015

    Tom,
    That book does look like mainly an advertising effort, an expansion of the web-site of the same name:
    http://whystringtheory.com/about/
    I notice there's a chapter on "Direct Experimental Evidence for String Theory". No page numbers, but I'm betting that one's rather short…

12. **martibal**
    November 5, 2015

    Thanks Charles !

13. **Dave Miller in Sacramento**
    November 5, 2015

    Peter,

    You wrote:
    >The first is that what Witten is explaining as a problematic framework to be generalized by string theory is not quantum field theory, but a first-quantized particle theory, with interactions put in by hand.

    This is something that has bothered me for decades, but that I rarely see discussed.

    Isn't it the case that string theory does not even rise to the level of a non-relativistic first-quantized theory? It seems to me that it is the 2-D equivalent of Klein-Gordon, which, of course, does not give an acceptable version of probability (until you second-quantize it).

    Specifically, does free string theory , even in principle, actually give a probability for a string to have a probability (probability density function, of course) to actually be in a specific configuration in spacetime? If so, how you do numerically calculate this?

    Everyone: I am honestly not trying to score rhetorical points here against string theory. These are sincere questions, and if I am ignorant of work that answers these questions, please point to that work.

    Dave

14. **Dave Miller in Sacramento**
    November 5, 2015

    Warren,

    You wrote:
    >But string field theory does exist, & people have addressed nonperturbative problems with it.

    I have followed enough of your work to know that you have seriously worked on the subject: I truly appreciate that someone seems to take these issues seriously.

    Could you point us to the best source with the most concrete discussion of the existence of string field theory, if possible a source that addresses basic issues such as the state space, actual calculation of probabilities for at least the free string case, etc.?

    Thanks.

    Dave

15. **marten**
    November 5, 2015

    Does an empty vessel stop making most noise once filled only with hopes and dreams?

16. **Warren**
    November 5, 2015

    Peter:

    Renormalons are a low energy problem in QCD (strong coupling), not large, because of asymptotic freedom. They make QCD nonperturbatively nonrenormalizable. A related problem is nobody knows how to calculate parton distribution functions. In particular, nobody can do higher-twist corrections, because they require experimental input of more PDF's, ad infinitum — more nonrenormalizability (in the sense of lack of predictability). Of course, you could calculate PDF's if you could calculate confinement, but you can't.

    Dave:

    Free string field theory is the same as string quantum mechanics, by the correspondence principle. Work has been done on nonperturbative vacua (due to the tachyon) for the bosonic string: The basic result is that the bosonic string is crap. So you need supersymmetry, but little has been done in SFT there yet. A similar situation exists in QFT in D=4, where you need supersymmetry to eliminate renormalons, by making the theory finite.

17. **Peter Orland**
November 5, 2015

Warren,

QCD is far from a finished problem, but claiming renormalons ruin its properties is not an accurate statement.

It is not known what the implications of renormalons are. They are special terms in perturbation theory, which (like instantons) ruin Borel resummability (all of which I'm sure you know). But there is no evidence that they render theories nonrenormalizable. There are models with perturbative renormalons which suffer from no such problem (I have worked on one of these). You may be familiar with the idea of resurgence, which does seem to deal well with Borel singularities in quantum mechanics (though not yet relativistic QFT).

In any case, the lattice gives pretty good evidence that QCD is fine at large distance scales. There is even work under way to find pdf's. The real problem is getting to extremely weak coupling (and string-inspired models do even worse).

I am not disagreeing that there is a lot of work to be done in QFT (confinement, mass gaps, and a lot more), but it is not a failure.

18. **Warren**
November 5, 2015

Peter Orland:

The only models I know of that solve the renormalon problem are quantum mechanical, not QFT. "Nonrenormalizability" comes from arbitrary coefficients of each ambiguity in the inverse Borel transform, corresponding to VeV's of an ∞ # of composite color-singlet operators. This is quite analogous to higher-derivative terms in nonrenormalizable field theory (the higher-twist problem even more so). This problem is seen also in constructive quantum field theory, as well as an analysis by 't Hooft in the complex coupling constant plane. In lattice QCD, this shows up as nonuniversality @ the origin in coupling constant space.

I did not mean to imply QCD was a failure. Only that it has problems, just as string theory does. But Peter Woit was implying that QFT was OK, in contrast to string theory being a complete failure. I would say string theory is the only theory that shows Regge behavior & spectrum of the type corresponding to confinement. Lattice QCD, on the other hand, has managed to calculate only ground state properties, & can't deal with even rotational invariance, so can't calculate cross sections.

19. **Bernhard**
November 5, 2015

" A should be well defined, B should be well defined and then you say that A is dual to B. Since we don't have another definition of string theory, we cannot hope to prove that string theory is dual to its conformal field theory."

I'm pretty sure I read this somewhere before…

20. **Peter Orland**
November 5, 2015

Warren,

Whatever problems of consistency QFT has, there is no comparison to those of string theory. I'm not claiming this makes it better than strings, just that it is a much better-developed field.

As to your first paragraph, one can do better than quantum mechanics. Asymptotically-free models in 1+1 dimensions have renormalon singularities in perturbation theory, yet there is good analytic evidence of sensible non-perturbative solutions. The exact S matrices are known, and there is even information about correlation functions.

Concerning your second paragraph; there is no serious controversy whether lattice QCD (or any lattice QFT) will have rotational invariance, in the continuum limit, assuming that limit exists. Approximate rotation invariance is seen on the lattice, although a weaker coupling would be better. There is also some numerical evidence of Regge behavior (although this may not have been studied much in more than three dimensions). A lot more is known than ground state properties. I do not mean there are theorems or analytic solutions (yet), but the computer is a good guide to what the theory predicts.

You are right to say that QFT has problems (which is why people work on it), but string theory is not as far along.

21. **ronab**
November 5, 2015

What are PDF's? Googling the term just gets swamped by the other sort of PDF's. Thanks.

22. **Peter Woit**
November 5, 2015

Warren,
My comments were about UV problems, which is what Witten was writing about, and claiming that string theory solves, unlike QFT. I'm still quite surprised that Witten thought such a claim was what "every physicist should know about string theory". I'd have expected that title to be used these days to make an argument for string theory as a way to handle strong-coupling problems, via its relation to AdS/CFT.

From the talks of his I've seen, Witten likes to claim that in string perturbation theory the only problems are infrared problems, not UV problems. That's never seemed completely convincing, since conformal invariance can swap UV and IR. My attempts to understand exactly what the situation is by asking experts have just left me thinking, "it's complicated".

The problems with QCD are IR problems, and I'm certainly willing to believe they can best be addressed by finding some form of string theory dual to QCD. As far as I know, there's no well-defined candidate for such a theory (i.e. that matches QCD in the UV, behaves like we think QCD should in the IR). QCD has the virtue that it works beautifully in the UV, and has a conjectural definition in the IR (via the lattice), even if it is hard to calculate things. I'm not claiming this is satisfactory, just that it's completely inconsistent with the story Witten is trying to tell about string theory being necessary to solve QFT UV problems.

23. **Peter Woit**
November 5, 2015

ronab,

PDFs = Parton distribution functions

24. **Warren**
November 5, 2015

Orland:

I wasn't disagreeing, only that string theory is by far better than alternatives to its problems. If you want to compare apples to oranges, pQCD is by far worse than QED. S-matrices in 2D massless theories isn't much better, since only backward & forward scattering, so just numbers, not functions (& usually reduces to just 2->2). Proof of rotational invariance & being able to calculate anything with angles are 2 quite different things. Similar remarks apply to Regge behavior & getting an actual Regge trajectory. And properties vs. numbers, which you really get from lattice QCD basically for ground states.

Woit:

I hope you're not confusing conformal invariance on the worldsheet with that in spacetime. As far as solving UV problems, I'm pretty sure Witten means in quantum gravity. String theory seems to solve that. No alternative does (although some people have a conjecture for 4D N=8 supergravity).

25. **andy norris**
November 5, 2015

"empty vessels make the loudest sound"

26. **Peter Woit**
November 5, 2015

Warren,

The problem with Witten's article is that his story about what goes wrong in QFT that doesn't in string theory is about the small proper-time behavior of loops in perturbation theory, applying equally well to QCD and GR. Given the fact that QCD has no UV problem, it seems to me that the story he's telling is likely irrelevant to the GR divergences problem.
My earlier comment was about worldsheet conformal invariance, specifically the action of the modular group. Witten's argument for no UV problem, for the torus case, is that the analog of proper-time in loop integrals takes values in the fundamental domain in the upper half plane, and this is bounded away from zero, so no small-time problem. There are lots of potential technical problems to worry about (you need this argument to work for arbitrary genus, in super-moduli space, etc), but what I was wondering about was the following: the modular group acts on this domain, taking it in particular to another domain that isn't bounded away from zero. From another related angle, discussions of T-duality in string theory often claim that the right picture is that of some "minimum length", below which you should go to a T-dual picture (acting by the modular group). But, if there are potential IR problems, how do I know that those IR problems in the T-dual picture don't appear now as I go to the UV?
To be clear, I believe there is likely some answer to this, cleanly separating out what's UV and what's IR, but it's not there in the Physics Today piece as far as I can tell.

27. **Peter Orland**
November 6, 2015

Warren,

"I wasn't disagreeing, only that string theory is by far better than alternatives to its problems. "

I am not sure what you mean by this. It seems you are saying to say that string theory is on better theoretical footing than QFT. Such a position is simply not tenable. Calculations with QED and QCD aren't perfect, but there are many reasons to trust them, not least of which is comparison with experiments. We agree that there are problems in calculating with QFT, but these do not invalidate its successes.

And lattice measurements tell you more than vacuum properties (as I said earlier).

28. **Peter Orland**
November 6, 2015

I meant "trying to say", not "saying to say."

29. **Dave Miller in Sacramento**
November 6, 2015

Warren,

You said:
>Free string field theory is the same as string quantum mechanics, by the correspondence principle.

Doesn't string quantum mechanics suffer from the same sort of problem as "first-quantized" Klein-Gordon, i.e., no sensible meaning for probability? Can you suggest any place you can point me to where I can see how this works in detail?

A possibly related problem that bothers me is how you actually get a concrete number for the "probability" (really the probability density) in first-quantized string theory given the use of the Gupta-Bleuler constraints. In QED, you can always drop Gupta-Bleuler and just go to the Coulomb gauge: You then are stuck with an apparently instantaneous Coulomb interaction, but at least probabilities more or less make sense (positive-definite Hilbert space inner product, etc.).

Can you point me to any detailed discussion of how the analog of all this works in string theory? E.g., I am tempted to think there must be some analog of the instantaneous Coulomb interaction but have never seen this mentioned.

Thanks.

Dave

30. **Warren**
November 6, 2015

Woit:

There are technical difficulties with higher-loop amplitudes in string theory, still being investigated. (A paper out today seems to claim to solve them.) But there are good arguments for finiteness that hold up to as many loops as have been evaluated. (Maybe they are proofs; I'm not enough of an expert to tell.) In any case, the arguments seem better then those for the next best alternative, 4D N=8 supergravity. (& for loop quantum gravity arguments indicate the opposite.) But you're not going to get all the details into a Physics Today article.

Orland:

I meant to say that string theory is on more solid ground than any other quantum gravity theory, just as pQCD is for strongly interacting particles @ large transverse momenta. String theory clearly is not of as good standing as QCD, nor QCD as QED ("apples vs. oranges"). I am not aware of any calculations in lattice QCD that give the masses of radially excited hadrons as falling on linearly rising Regge trajectories, nor any that give low-energy scattering amplitudes of the sort described by nonlinear sigma models.

Dave:

@ the free level, strings are just a reducible, unitary representation of the Poincaré group. You can write the single-string Hilbert space as a direct sum of those for the "usual" particles. So if you understand free particles, you understand free strings. The free part of the string field theory action can be decomposed into the sum of field theory actions for particles of different spins. Nowadays quantization (for particles or strings) is better treated by BRST, which deals with unitarity in any gauge, particularly in the Feynman gauge, which is more practical than Coulomb gauge. Similar remarks apply to first-quantization: Stückelberg & Feynman solved the problem of the Klein paradox in that context by identifying "negative energy" particles as antiparticles. This interpretation can be applied either in classical mechanics, or in quantum mechanics in terms of the first-quantized path integral giving the Stückelberg-Feynman propagator.

31. **Warren**
November 6, 2015

Orland:

P.S. By "ground states" I meant in the quantum mechanical sense (like the hydrogen atom), i.e., masses (& some couplings) of the π,ϱ,N,Δ,…

32. **Peter Orland**
November 6, 2015

Warren,

I think you are overly pessimistic.

It's true that most of the work on Regge trajectories in the lattice literature is only on glueballs, but there is no fundamental obstacle to asking the same question for mesons and baryons (it is more difficult, of course).

There are attempts to work out scattering amplitudes. For example, the QCD corrections to light-by-light scattering are being studied on the lattice.

I don't work along these lines myself, but I don't see them as impossible lines of inquiry.

33. **chris**
November 6, 2015

Warren,

here is a recent review of excited lattice spectroscopy:
arXiv:1411.0405
And here is just one recent example of lattice calculations of scattering amplitudes:
arXiv:1504.01717

It is rather strange that you have singled out these two topics as unsolved by lattice, since there are major efforts going on in the US, Europe and Japan to address exactly these questions. There are of course things to be cleaned up (especially mapping out resonances in a finite box is a tricky thing) but there is no doubt whatsoever that the lattice will solve this in the coming years given enough computer power and algorithmic improvements.

By the way, for us lattice people it is funny to see claims that "QCD is nonperturbatively nonrenormalizable" and such. You know, because of asymptotic freedom the continuum limit of the lattice regularization scheme is well defined and that is QCD. In N-flavor QCD you define the physical point by N+1 experimental measurments, you go there and out pop all other observables. We don't care about perturbative approximations or models – if there are problems in them, they are not problems of QCD. QCD is perfectly well defined and consistent and we know how to do the path integral.

Oh, and one more thing. Your claim
"As far as solving UV problems, I'm pretty sure Witten means in quantum gravity. String theory seems to solve that. No alternative does (although some people have a conjecture for 4D N=8 supergravity)."
is a rather gross misstatement. There is plenty of evidence that gravity is asymptotically safe and no UV problem exists to begin with. For a recent review, see e.g. arXiv:1202.2274

34. **TS**
November 6, 2015

Can someone explain to me what renormalization in a non-perturbative context means? Obviously, the meaning must be different than what we mean when we apply renormalization in perturbative calculations, namely the task of relating measured quantities to the quantities that appear in the calculation. Since the measured quantities involve all orders of perturbation theory, it is not trivial to see how the measured quantities can be related to the ones used in the calculation. In a non-perturbative setting OTOH I would expect that the objects entering the calculation are the ones we observe in the lab without any further requirements on the theory: say, two pions in -> machinery -> two pions out, where all pions have a mass of 140 MeV, spin 0 and charge = ±1 (and, except for the choice of units, these would come out of a sufficiently well-developed theory).

A web search leaves me a bit puzzled: "non-perturbative renormalization" in this context appears to be not more than the setting of a numerical value for a mass (or several), which is more or less in line with what I would expect one needs for a non-perturbative computation, but why would one call this "renormalization" instead of "choice of units"?

35. **Peter Woit**
November 6, 2015

TS,
This is starting to get a bit far afield from the topic of the posting. But I think the simple answer to your question is that whatever your definition of qft is, it appears to need a cutoff, and the cutoff qft will be characterized by certain parameters ("bare parameters"). You hope to compute observable numbers in terms of such bare parameters (in perturbation theory, by Feynman diagrams, non-perturbatively by some other method such as a lattice Monte-Carlo). The hope is to remove the cutoff, varying bare parameters with the cutoff in such a way as to get a well-defined limit for physical observables. This is what I'd call "renormalization" in general, perturbative or non-perturbative.

A problem with Witten's argument is that it's purely about problems with renormalization in perturbation theory. If the short distance behavior of the theory is not governed by perturbation theory, such arguments will be irrelevant.

36. **Warren**
November 6, 2015

Chris:

Thanks for the references; I'll look them up. In fact, there is no proof that the continuum limit of QCD is universal, for the reasons I gave, & it's exactly because of asymptotic freedom. By "nonrenormalizable", I mean an infinite number of parameters must be introduced, because there are an infinite of renormalons, corresponding to an infinite number of color-singlet operators whose VEV's must be determined. "The lattice will solve this in the coming years given enough computer power and algorithmic improvements" is something I've been hearing for 40 years; given Moore's law you'd think it would've been done by now. So, yes, I have plenty of doubt. The only work I've seen on asymptotic safety in gravity is based on doing an ε epsilon from D=2, & I'm not satisfied by arguments that treat 2 as ≪ 1. In fact, any renormalization group arguments for finite changes in the coupling constant are dubious due to the arbitrariness of the β function past 2 loops.

Orland:

The only calculations I've seen on Regge trajectories in lattice gauge theories gives the result only near argument 0: the intercept & the slope @ 0. As to not seeing impossible lines of inquiry, I would say the same for string theory. Advances have been made in both in the last 40 years, both are hard problems, & both leave much to be desired.

37. **TS**
November 6, 2015

Thanks, Peter! The terminology makes a lot more sense to me now.

38. **Warren**
November 6, 2015

Chris:

Looked @ 1 of your papers: Uses HAL QCD approach, which calculates a 2-body potential from the lattice, then plugs that into a (continuum) Schrödinger equation to find the results. Not sure how much advantage that has to the old quark model stuff that began with linear (for confinement) plus Coulomb terms. But I'll keep reading…

But the asymptotic safety reference is the same old thing. Hard to calculate in an ∞ dimensional coupling space.

39. **Attila**
November 7, 2015

While it is true that universality of the continuum limit in QCD has not been
proven, if the continuum limit was not universal, I would not expect the
continuum extrapolations of different observables, taken with different discretizations, to agree so well. Even if the low lying hadron spectrum and the equation of state at mu_B=0 was the only thing the lattice could calculate, which I think is not true, that would already be quite impressive, since no other method can do even that. I have to add that I am a lattice practicioner, so if I did not believe in lattice results in general, that would be somewhat strange.
_
"The problems with QCD are IR problems, and I'm certainly willing to believe they can best be addressed by finding some form of string theory dual to QCD."
I think that is optimistic. As far as I know, one does have dual theories to
gauge theories closer to QCD than N=4 SYM, the problem is that the dual theory
gets crazier as you get closer to the real thing, and the whole approach loses the
computational advantage it had. An example of a computation in a theory closer
to the real thing (N=2* SYM) is: hep-th/1108.2053

40. **Warren**
November 9, 2015

Attila:

It's all about optimism, isn't it? People are optimistic about their own areas of research, & impressed with the results so far obtained, and dubious about claims in other areas. So lattice QCD can make good predictions for low energy (where renormalon contributions can be neglected, & plugging potentials into 1st-quantized calculations can be pretty good), with nothing but hopes for high energy, while the converse is true for string theory. Similar remarks apply to pQCD, which is good for large transverse momenta, where log corrections can be calculated, but power corrections (especially ones from higher twist) must be ignored.

So we have a bunch of competing approaches staking out different "countries" in momentum space, each saying their country is better, & threatening to invade each other's territory in the future. But history has taught us that these "threats" tend to be hollow, & people tend to ignore the diminishing returns from their own approaches. & each ignores the fact their own predictability problems prevent them from extending into the other territories, whether due to renormalons, twist, or compactification.

41. **Attila**
November 9, 2015

Warren:

While I fundamentally agree, it seems I am more optimistic in general,
and not only in the case of my own area. 🙂

For example, in my own field (finite temperature field theory) there are actual examples where the applicability region of resummed perturbation theory and the lattice simulations overlap. In QCD (and even more in pure gauge theories) there are several observables, where high temperature lattice calculations can be compared to perturbative calculations, with good agreement. This includes the equation of state and fluctuations and correlations of conserved charges, for example.

I do not think one approach needs to “rule them all”, it is not a realistic hope. A more realistic one would be to make contact, and find an overlap in the applicability of approaches.

I know almost nothing about quantum gravity, but as an outsider, it does not seem completely hopeless to me that even if the UV completion is some kind of string theory, other approaches such as the asymptotic safety program with the functional renormalization group or CDT can be used as a low energy approximation of that. I could easilty be completely wrong on this one, but I don’t think I am wrong on QCD.

Sorry for the off topic posts, I will stop now.

Various News

November 8, 2015
Categories: Uncategorized

Later tonight will be the 2016 Breakthrough Prize ceremony, broadcast live on the National Geographic channel. While mathematicians and physicists are getting their popcorn ready, waiting to find out which of their colleagues will be $3 million richer, they might want to check out Mathematics Without Apologies, where Michael Harris is writing about his experience on the red carpet at last year's ceremony. In other news:

- A few miles down the road from the event tonight at the NASA Ames Center, the Stanford Institute for Theoretical Physics has a new website. They have various videos you can watch, as well as this account of the history of the SITP:

  > The gauge hierarchy problem was first addressed in the earliest days of SITP by Susskind and Dimopoulos, whose ideas eventually led to the introduction of supersymmetry into particle physics. The gauge hierarchy problem and the discovery experimental discovery of supersymmetry were principle reasons for the building of the Large Hadron Collider, but the puzzle remains. To this day Dimopoulos and his band of young postdocs and students have offered the most exciting proposals for discovering new physics in this area.

  Personally, I thought the Higgs was the principal reason for the LHC, but perhaps I was misinformed.

- Several people wrote to tell me about a USA Today article reporting Study may have found evidence of alternate, parallel universes, noting that this needed a new installment of This Week's Hype. I'm very pleased to see that the excellent science journalist Jennifer Ouellette has been on the case, debunking this much better than I ever could. The blame for this kind of thing is jointly shared by physicists and journalists, glad to see that at least some journalists are taking action to deal with the problem.
- There have also been several suggestions that I write about Leo Kadanoff, the great theoretical physicist who passed away a little while ago at the age of 78. Unfortunately I never met him, and only had a general acquaintance with his work. A very good obituary by Kenneth Chang did appear in the New York Times.
- I've also heard from lots of people with more stories about the Khrzhanovsky film about Landau described here. It seems that I'm the only one who didn't know about this. In other performing arts/physics news, Lee Smolin discusses here a project he has been involved in.
- The LHC has finished its 2015 run colliding protons at 13 TeV, will now turn to heavy ion physics. Integrated luminosity recorded by ATLAS and CMS is about 4 inverse femtobarns. Results of the analysis of this data may start to be available publicly around the time of Moriond in March. Consulting Jester to see what to expect, it looks like better limits on or evidence for stops, Z-primes and gluinos should be available. In particular, we'll finally see a conclusive test of string theory (Gordon Kane argues that string theory predicts a 1.5 TeV gluino, see here).
- In other news from the LHC, it seems that humanity narrowly missed a major problem back in August, when Simon Parkes, a Labour town councillor for Whitby in North Yorkshire, foiled a sinister plot by the Illuminati to "use the LHC to open an evil portal that would allow them to become more powerful."

Update: Awards ceremony hasn't started, but names are out: Ian Agol for mathematics, 5 teams of 1300 people for neutrino oscillations (with 7 specifically identified, including the 2 winners of this year's physics Nobel).

Junior recipients in math are Larry Guth and Andre Neves, in physics Bogdan Bernevig, Liang Fu, Xiao-Liang Qi, Raphael Flauger, Leonardo Senatore, and Yuji Tachikawa.

Update: The big news, via Michael Harris, is that Peter Scholze (to my mind a better mathematician than any of those who got prizes) turned down a $100,000 prize. Good for him.

Comments

1. **John**
   November 8, 2015

   That Illuminati link is a bit scary. I would suggest a global meditation session to defeat them ASAP. Just hope they don't turn it on at 3am again like last time, or we're screwed.

2. **Bill**
   November 8, 2015

   Whoever guesses Breakthrough Prize laureates before 10/9c gets a gift certificate from Peter worth 10 off-topic comments.

3. **Peter Woit**
   November 8, 2015

   Bill,
   Sorry, no such offer. If you are not guessing but do know the winners, if you let us know early, maybe I'll go for one off-topic comment (unless I get legal advice that that's problematic as a valuable bribe…).

4. **Bill**
   November 8, 2015

   It is possible that I've met some of the winners in real life but I don't know who the winners are at this moment, so would have to guess: Yitang Zhang for math, and Maxim Kontsevich for life sciences.

   It seems like Michael Harris knows something. From his blog: "And something tells me (just this morning) that this year's ceremony will be more interesting than I had anticipated."

5. **MSZ**
   November 8, 2015

   https://breakthroughprize.org/News/29

   No need to guess …

   The 2016 Breakthrough Prize in Mathematics to be Awarded to Ian Agol; The 2016 Breakthrough Prize in Life Sciences to be Awarded to Five Individual Recipients: Edward S. Boyden, Karl Deisseroth, John Hardy, Helen Hobbs, and Svante Pääbo; The 2016 Breakthrough Prize in Fundamental Physics to be Awarded to Seven Leaders and 1370 Members of Five Experiments Investigating Neutrino Oscillation: Daya Bay (China); KamLAND (Japan); K2K / T2K (Japan); Sudbury Neutrino Observatory (Canada); and Super-Kamiokande (Japan)

6. **gg**
November 8, 2015

Peter: I'm not a particle physicist and was a young graduate student at the time SSC was cancelled and LHC moved forward. My (vague) recollection is that the attitude at the time was that the Higgs would still show up at the Tevatron, and that SSC/LHC would fry bigger fish.

7. **Peter Woit**
November 8, 2015

gg,
The Tevatron never had much of a chance at the Higgs. Depending on mass, it had a chance at barely seeing evidence for it, not much more. From the beginning, the Higgs was the main aim of the SSC and LHC. The text I quoted gives some insight into how some theorists at Stanford see the world.

8. **Danny Calegari**
November 8, 2015

Your comment that Peter Scholze is "a better mathematician than any of those who got prizes" seems churlish, whatever your views on the value of the Breakthrough Prizes. Mathematicians have many different styles. For example, some are theory builders, others are problem-solvers. Ian Agol, as well as being a close personal friend, is one of the most gifted problem-solvers I've ever met. I'm delighted to see his wonderful contributions to mathematics made more visible to the wider public as a result of this award.

9. **Peter Woit**
November 8, 2015

Danny,
Yes, there are many different kinds of mathematicians with many different styles, and no linear ordering makes any sense. No criticism of Agol or the other winners was intended. I know none of their work very well, they all seem to be quite good at what they do. It's also true though that I suspect one could have identified a hundred other mathematicians just as talented and accomplished as those three and I don't see why the math community should take these prizes seriously.

I've edited what I wrote slightly to make it clear that I was expressing my own opinion and personal idiosyncratic judgement. Before this, based on what I understood of Scholze's work I had a high opinion of him as a mathematician. With his decision to turn down the money and, I hear, basically make no public comment, ignore these people and go about his work, I now have a very high opinion of him as a human being. Instead of the usual congratulations and singing of the virtues of the prize winners and their new-found wealth, I think the math community tonight would do well to instead congratulate Peter Scholze for embodying its best qualities.

10. **Mateo**
November 8, 2015

60 minutes had a piece tonight (new I think) on the LHC. Sounded to me like the potential for extra dimension and dark matter findings were being oversold in terms of the LHC but what do I know. Thought I'd provide the link in case Peter would like to comment:

http://www.cbsnews.com/news/extra-dimensions-dark-matter-a-more-powerful-collider-hunts-for-clues/

11. **John Baldwin**
November 8, 2015

I haven't yet decided how I feel about these awards, but I'd be interested to hear Scholze talk about his decision to refuse the award rather than to, say, use it to support struggling mathematicians in need of funding, or help a public library on the verge of closing, or donate it to a charity whose mission is to help relieve actual human suffering. I'd like a glimpse into Scholze's moral calculus.

12. **math_lambda**
November 9, 2015

@ John Baldwin : on paper what you say might make sense, but on the other hand the celebrity circus is such an insane thing, coming there would be condoning it, and apparently Scholze didn't want to. He's a tenured professor at Bonn when he could have a very high salary in the US, and you could make the same "moral calculus" remark for that. Never met him, but my guess is money and fame are the least of his worries, a respectable choice indeed.

13. **mh**
November 9, 2015

@John Baldwin.
Scholze is a mathematician. He has no moral obligation to become a philanthropist and he has no moral obligation to justify his refusal. He is doing science a great service by just ignoring this circus.

14. **Bill**
November 9, 2015

First of all, I think it will be difficult to identify 100 or even 10 mathematicians whose work was at the level of Ian Agol in the last 10 years. No matter what one thinks about these awards, he is not a bad choice.

As for Peter Scholze, would he have rejected the main prize? I feel (as he might have felt) that his work is worth more than junior prize, so he did the right thing. I am sure he will accept the Fields medal in 3 years.

15. **iv**
November 9, 2015

There appears to be a clear political agenda behind the Breakthrough Prize in Physics this year. Nima, Witten and Gross etc. want to push the Great Collider project in China led by Wang Yifan — the leader of the Daya Bay neutrino experiment. But Wang needs the appropriate status to convince the Chinese government to invest in this extremely expensive project. The rule for the Breakthrough Prize is to have the past winners to nominate the future ones, so Nima et al. can get this strange combination of neutrino experiments to share this prize so that Wang can get the international recognition that he needs. ~1000 people to cover this political move, wow!

16. **Peter Woit**

November 9, 2015

Bill,
I’m not saying at all that Agol was a bad choice, just that there would be very many equally good choices, for both prizes.

I doubt that Scholze’s reason for turning down the prize was that it wasn’t good enough. Michael Harris on his blog points out that by doing this Scholze removes himself from likely consideration for the \$3 million, which he otherwise would be a very strong candidate for. So, in effect, he’s turning down \$3.1 million.

I just watched some of the taped awards show, and it’s very clear why one might feel it was worth at least $100K to not participate. The Fields Medal is something rather different.

17. **Bill**
November 9, 2015

Peter,

On second thought I agree with you about Peter Scholze. The format of these awards treats scientists as expensive escorts to a costume party so, whatever his thought process was, he deserves a lot of respect. I wonder if previous winners have regrets now that the prize lost “some of its luster and legitimacy” (in the words of Michael Harris).

18. **David Ben-Zvi**
November 9, 2015

Independently of the hoopla, it seems clear Agol’s work deserves all the recognition it’s getting — I recommend Danny’s blog
for a discussion of one of the most spectacular breakthroughs.

19. **rc123**
November 9, 2015

Even though you seem to understand that no classificatory move is possible, it seem (at least to me) very rude to dismiss brilliant mathematicians who have gone on to do remarkable work as somewhat “worse than Scholze”. As you have said so, you don’t comprehend Agol’s work in topology or Nevé’s work in differential geometry, so why bother making such remark knowing it can be offensive (I’m not saying that was your intent)?

20. **Christopher Herzog**
November 9, 2015

My objection to these prizes is that the biggest winners are the U.S. and other federal governments. The prize winners, assuming they don’t turn around and donate their winnings to charity, will all have to pay a sizable chunk of their winnings to the IRS (or its equivalent in other countries). Why not take the same money and do what for example Simons or Howard Hughes do? Set up a foundation. Involve some scientists to do peer review. Issue grants that will fund your favorite kind of research, research you presumably already think that the government is not doing enough to support?

21. **Peter Woit**
November 9, 2015

rc123,
On the whole I think that “this mathematician or physicist is better (or worse) than that one” is a judgment best avoided, for the obvious reasons that different talents, accomplishments, and different areas of mathematics are not really easily comparable this way, and that of course, anyone’s knowledge is limited, so humility is in order. I think if you look at the thousands of pages I’ve written on this blog, you will find that such judgments are expressed extremely rarely (although I’m not unwilling to write something because some may find it offensive).

In this case though, I thought it important to point out that, of the four people awarded prizes, Scholze was not a random one, but widely considered by those who know him and his work to be the most talented young mathematician to come around in quite a while. His refusal of the money seems to have gotten zero attention in the press, partly probably because they know nothing about who he is.

I don’t know Agol personally, but I’d be surprised if he was particularly upset and offended that Peter Woit thought Peter Scholze was more talented than him. And if he does feel that way, he has $3 million to make himself feel better…

22. **Shantanu**
November 9, 2015

Peter and others,
Yours truly is one of the 1300 winners (although they have spelled my first name wrong) on the website. let’s see though how much share of the prize I get 🙂

23. **Peter Woit**
November 9, 2015

Shantanu,

Congratulations!

24. **Rompety Rabbit**
November 9, 2015

Interesting that this “hoopla” will be on the National Geographic channel, even while
a news item from today is about the impending destruction of that once great organization:
http://reverbpress.com/business/rupert-murdoch-national-geographic-layoffs/
(cited on http://www.truthdig.com)

Surely the new National Geographic will want to reach this demographic:
https://www.youtube.com/watch?v=mKKKgua7wQk

Christopher Hertzog makes a comment which must be far too sensible in the age of Trump:
“Why not take the same money and do what for example Simons or Howard Hughes do? Set up a foundation. Involve some scientists to do peer review. Issue grants that

will fund your favorite kind of research, research you presumably already think that the government is not doing enough to support?"

Why not? Maybe because that would take actual intelligence and wisdom?

25. **Peter Woit**
November 9, 2015

Please direct further comments about the Breakthrough Prize to the next posting
http://www.math.columbia.edu/~woit/wordpress/?p=8088

I've moved the latest comment there.

26. **gadfly**
November 10, 2015

I feel like Scholze should have accepted the prize and given it to charity or something…

27. **Avattoir**
November 10, 2015

Or, Gadfly, at least to the next posting.

There's much to admire about Professor Woit, including his periodic posting of drafts of his text-in-progress. There's an unexpected, for me, example from this thread. When several commenters attacked him for, as one called it, appearing (I think worse than that was implied, at least by some others.) "churlish". Yet Woit didn't react with anything remotely like the peevishness that usually accompanies churlishness, instead showing restraint, indulgence & patience in making his points again & in various ways. Soon at least a few of those who'd attacked him indicated they'd turned & had come to agree with him. One doesn't see that in many blogger-monitored websites. However, since this comment is "off topic", admonishment should follow, correct? It's like a test.

28. **Peter Woit**
November 10, 2015

Avattoir,
Thanks, I'm not completely immune to flattery, so, no admonishment.

The questions at issue about the mathematics prize I think are quite interesting, and I hope I can have some effect in getting people to think about them. Danny Calegari's comment I didn't take as an "attack", he was expressing a reasonable view which I can understand and I'm sure is widely shared. Whenever prizes like this are announced, in private people may express "so and so is better" judgments, but there are good reasons not to do this in public, since such judgments often are, well, "churlish" is a good word.

And yes, this discussion should be in the next posting, but I don't want to encourage more of it in either location.

2016 Breakthrough Prizes

November 9, 2015
Categories: Uncategorized

The 2016 Breakthrough Prizes were announced last night, discussed a bit in the last posting. Today there are programs going on at Berkeley, livestreams available here.

One thing that strikes me about these things is that the situation with the physics prize has changed dramatically since the first three years, when they went mostly to string theorists. Having a heavily promoted much larger cash prize than the Nobel, given largely to theorists for ideas many of which haven't worked out, raised obvious questions about the wisdom of the whole thing. The last two years have seen a 180 degree turn, with the prizes going to experimentalists for successful experimental results. Even better, there has been an unusual emphasis on making an award to entire experimental collaborations, not just a small number of "great men" identified as collaboration leaders or spokespersons. I don't know of any other major prizes that do this. The lack of an experimental Nobel for the Higgs discovery is one reflection of that problem, it's great that the Breakthrough Prize people are doing something about it.

This is now just the second year of the math prize, which has never been as problematic as the early physics prize. However, the institution of cash prizes of this size, promoted in a Hollywood style, is something I don't think anyone in the math community ever asked for, and it's not at all clear it's a good thing, or in keeping with some of the best values of the math research community. This year I think Peter Scholze set a remarkable example by turning down a prize, a move which unfortunately has gotten little attention in the media. I hope his action causes people to take a closer look at this gift horse. Instead of just celebrating the shower of cash and attention, research mathematicians may want to consider whether, just as they changed direction with the physics prize, Milner and Zuckerberg perhaps should be encouraged to listen to Scholze and move in a different direction.

Update: I just watched some of the talks at the afternoon symposia. Arkani-Hamed made the case for a Great Collider, mostly quite sensibly in terms of the desirability of better understanding the Higgs: is it pointlike? how does it self-interact? The argument is that addressing these questions goes beyond what the LHC can do, can be done by a large new collider.

On the math side, David Nadler gave a beautiful talk about Langlands/geometric Langlands, ending with a prediction for the future that a central role will be played by Peter Scholze's work, including recent ideas on what Nadler calls "Arithmetic conformal field theory". He suggested that 50 years from now, Hartshorne and other graduate textbooks on algebraic geometry will be replaced with new ones based on Scholze's perfectoid spaces. Maybe if they hadn't offered Scholze money they could have gotten him to talk about this stuff…

Update: The New York Times Science section has an article today about the goal of the Breakthrough prizes, to turn scientists into celebrities. Yuri Milner is quoted:

> "We are at the very beginning of this journey," he said, noting that if you were to look at a list of the top 100 celebrities in American society, there would not be a single scientist on the list.
>
> "The question is why?" he added.

The question this raises, which may have something to do with Peter Scholze's refusal to participate, is "what if good scientists don't want to be celebrities?" The impulse to do science and mathematics at the highest level and the impulse to be a celebrity may just be two very different, incompatible things.

The New York Times article doesn't mention the Scholze story, but it does discuss the young student, Ryan Chester, who was given a $400,000 award for making a film about special relativity. It turns out that doing a great job of making such a film has a lot more to do with interest in being a filmmaker than interest in being a scientist:

> Mr. Chester, however, said in an interview that he was not planning to study science in college, but instead will probably study film, hopefully at a school like the University of Southern California or New York University.

Update: Videos from the symposia and panel discussions at Berkeley are available here.

Update: In other large-check news of the day, the IAS has announced a $20 million donation from Robert Rubenstein, CEO of the Carlyle Group, a top private equity firm. This completes a $212 million capital fundraising campaign.

Comments

1. **Jeffrey M**
   November 9, 2015

   Peter,

   Agree about Sholze, but worth noting that he is following in the footsteps of Perelman, who turned down $1 million from Clay after he proved the Poincare Conjecture.

2. **Anonyrat**
   November 9, 2015

   We are witnessing the attempted transformation of mathematics and physics research into a "winners-take-all" culture. Instead of making life a bit easier for the many graduate students, post-docs and people struggling in tenure-track positions, money is being given to the stars. So it is becoming like professional sports, acting, etc., where the top becomes extremely wealthy; the next-to-the-top just make a living, and everyone else is waiting tables while trying to make a mark in the field.

3. **Bill**
   November 9, 2015

   Maybe, we shouldn't put words in Peter Scholze's mouth… Maybe, Quanta magazine can interview him.

   Is it just a coincidence that all mathematics prizes were in geometry?

4. **KJ**
   November 9, 2015

   "Milner and Zuckerberg perhaps should be encouraged to listen to Scholze and move in a different direction."

   In what direction is Scholze suggesting they move?

5. **Peter Woit**
November 9, 2015

KJ,
That may be a bit badly worded. I have no reason to believe Scholze has any particular suggestions for them, and no idea what his thinking about any of this is.

My point was rather that if arguably the best young mathematical researcher around turns down your effort to encourage mathematics research by giving him a 100K check, effectively saying he wants no part of what you are doing, you might want to reexamine whether what you are doing really is the best way to encourage math research. I sincerely doubt that if Milner and Zuckerberg went around to the best mathematicians in the world and asked them "what can we do to encourage mathematics research?", they would hear back "set up a Hollywood-style award ceremony and hand out $3 million checks".

6. **Baixiao**
November 10, 2015

Maybe this is a bit off topic here, but I find striking that Milner's prize has been awarded to just a few condensed matter Physicists so far. It is embarrassing to see all those Silicon Valley millionaires giving away their money to mainly high energy and cosmology Scholars. I am sure they are all perfectly aware that their fortunes largely come from technologies that heavily rely on the work of many (unfortunately, rather quiet) condensed matter Physicists.

I know this may not be the forum to air this kind of frustration. However, I find this neglect particularly painful these days after the recent loss of Leo Kadanoff, whose contributions, which were instrumental for Ken Wilson's Nobel winning work, were largely unrecognized by various "big prize" committees.

7. **Andrew**
November 10, 2015

Can you elaborate on why a winner turned down his cash prize? Misgivings about the way the math competition is organised? He thought other mathematicians were more worthy? He lives an ascetic or solitary life and didn't want such a change or attention?

8. **M**
November 10, 2015

Some recognition should also go to people, like John Bahcall (now deceased), who computed the flux of solar neutrinos claiming an anomaly with respect to measured rates. After all, the SNO neutral current measurement confirmed that they were right. And neutrino oscillation discoveries started from this anomaly.

9. **math_lambda**
November 10, 2015

Looks like a long joint paper by Cariaini and Scholze was posted to the arxiv precisely on sunday : http://arxiv.org/abs/1511.02418 Now that's funny!

10. **Peter Woit**
November 10, 2015

Andrew,
I don't know what Scholze's thinking was about this. I hope he'll comment publicly at some point. He's not someone like Perelman who leads an unusual solitary and ascetic life, so I don't think that has anything to do with it.

11. **Bernhard**
November 10, 2015

The case for a new collider to study Higgs properties is actually a sound one. Even with the Super LHC we won't know the Higgs width accuratly enough. Around 10% of the total branching ratio would stil be open for it to decay to exotic stuff.

12. **Thomas Larsson**
November 10, 2015

Baixiao, I may misremember, but I think Wilson has publicly stated that he thought his Nobel should have been shared with Kadanoff and Mike Fisher.

The status of the Nobel prize does not only come from the fact that it is big and old (although I know of no other prize that beats Nobel on both size and age), but also from the fact that the Nobel committees (in the sciences) have never made any serious blunders. This is bound to make the Nobel committees very conservative institutions, that are unlikely the change their practice anytime soon.

13. **Bernhard**
November 10, 2015

"from the fact that the Nobel committees (in the sciences) have never made any serious blunders."

Serious blunder maybe not, but the Nobel prize has a rather regular history of "forgetting" several people and in some cases, in an inexcusable way, like the fact that S. Nath Bose didn't get the prize.

The argument that they keep committing these mistakes and will keep making them out of tradition simply doesn't stick. They have the power to change the status quo and they should. Acknowledging for example that big science is done by large teams should not in any sense interfere with the rigorousness of the process.

14. **LK2**
November 10, 2015

Nobel prizes on Peace has few blunders. I would not say so for the other ones.
Milner prizes are a disgrace for sciences and now I really wonder why Scholze turned it down.

15. **Peter Woit**
November 10, 2015

All, please, enough with off-topic discussion of Nobel mistakes, real or imagined. That has nothing to do with the topic of this posting.

16. **rc123**
November 10, 2015

OK, my comment will be off topic, but it won't be about de Nobel: considering I'm a layman in mathematics, you all think of Scholze as a "problem solver" or a "theory builder", if you even can categorize him in such a binary mode?

17. **Peter Woit**
November 10, 2015

rc123,
He's definitely on the "theory builder" end of things.

18. **ronab**
November 10, 2015

Peter:
"… Even better, there has been an unusual emphasis on making an award to entire experimental collaborations…"

On the contrary, it seems to me that this approach makes the entire breakthrough prize utterly pointless. Why on earth does anyone care whether the Facebook founder's friend decides that some actress should say nice things about one particular 1000 person physics collaboration over another, and to give it a check that amounts to a few hours or days of its ongoing budget? If they give Witten a check for $3 million, at least it makes some difference to Witten, but relatively tiny awards by non-scientists to vast collaborations make absolutely zero difference to anybody.

19. **Peter Woit**
November 10, 2015

ronab,
I agree there's a real question "Why on earth does anyone care whether the Facebook founder's friend decides that some actress should say nice things " about any particular scientist, mathematician, or group of such. But, given that some people seem to care about these things (prizes, awards), giving yet another one to people who already have a long list of them on their CV already, and generally no need for the money, seems a way to make no difference. As far as I know, there are no other prizes given to such groups, so this means 1300 people now have an impressive sounding prize to put on their CV, which could be quite significant to them. If they're getting an evenly distributed cut of the money, that would be over $2000 each, and for many of them such a check might have more impact on their well-being than the multi-million dollar checks going to the well-off.

20. **ronab**
November 10, 2015

Peter,
"… an impressive sounding prize to put on their CV, which could be quite significant to them."

That's a very alarming thought. The physics hiring process is random enough as it is. Is it now to be further corrupted by random low-level researchers strutting around with Nobel-prize-lite's on their resumes simply because they happened to join the right large collaboration at the right time, ie shortly before the subject caught the fleeting fancy of a passing website entrepreneur? I'd been assuming that anyone with hiring influence would be smart enough to ignore such specious claims to fame. But if you're right and it might actually be significant to them, do you really think this would be a good thing?

21. **Peter Woit**
November 10, 2015

ronab,
The point of the prize is to reward people who have done work the funder thinks is admirable. If 1300 people have one, I don't think having this on one's CV is going to be a major factor in any hiring, but it allows people to point out "I worked on a successful, important experiment". Milner's goal of getting more recognition for scientists I think is fine, and this seems a good example of how to do it right (as opposed to trying to turn a particular scientist into a celebrity by giving him a large check and getting him to dress up and walk a red carpet).

22. **Jim Akerlund**
November 11, 2015

Watching the videos on the Breakthrough Prize Math Horizons II. At the 31 minute mark is a fire alarm that interrupts Ian's talk. The talk resumes at the 54 minute mark. I mention this for the potential viewers. I was viewer # 371. The video being on Youtube means that you can skip the offending section without too much trouble. The fire alarm did effect the talks in that the question and answers at the end of the talks were cancelled, and it seems some of the talks were shortened.

23. **Tom DeLillo**
November 11, 2015

As chair of a department at a midsize state university with budget shortages and students in debt, I have trouble taking much interest in all of these prizes. I can rarely hire new assistant professors to replace retirees. Even a funded three-year postdoc would be nice to give some young person a chance to stay in the profession.

24. **Bernd**
November 11, 2015

Peter,
I agree with you that when asking the best mathematicians how to encourage people to do research, they probably won't come up with large cash prizes, but why would you want to ask them in the first place? I'm not so sure that they are the most authoritative source on why young people decide to pursue one interest or another.

On the other hand, if one judges the effectiveness of the prizes by the coverage they have received in media geared at young audiences (Buzzfeed, Vice, reddit), then one has to say that these awards have been an utter failure so far.

25. **Peter Woit**
November 11, 2015

Tom DeLillo,
Thanks, I think you identify a crucial issue here. For many years middle-class academic jobs have been disappearing in the US, replaced by poorly paid adjunct positions, and a small number of well-paid positions for "stars". Prizes like the Milner-Zuckerberg one seem intent on making this worse, their idea is that what the field needs is more money and attention for the "stars".

26. **Math.Phys**
November 11, 2015

Why Scholze turned down the breakthrough prize? Probably because he's young and still remembers the reason why he decided to be a research mathematician and thinks that these people are doing damage to the subject.

27. **Peter Woit**
November 11, 2015

Bernd,
I think good mathematicians are the best source of advice about how to encourage good mathematics research, and that there is likely a strong consensus amongst them that turning mathematicians into wealthy celebrities isn't the way to do it (and that there's even a serious danger that will negatively impact the research community).

Considering the number of jobs available doing mathematics research, there are more than enough young people interested. Young people are exposed to mathematics on a daily basis in their schools and either the subject itself or an inspiring teacher convince many that they love the subject and want to learn more about it. If one sees a problem there, a much better way to spend money would be on getting better math teachers by paying them better. That would have a lot more positive impact than pictures of Maxim Kontsevich getting a big check and a glitzy orb from a starlet.

28. **Cynicism**
November 11, 2015

I think a comment in Michael Harris' blog is pertinent. Scholze used to play in a rock band himself. Now he is a professional mathematician. Robert Schneider of Apples in Stereo became famous as the frontman for a rock group. Now he is a graduate student under Ken Ono. If you look at mathematical conference photos, you will often see at least a few people dressed in a way that wouldn't be out of place at a metal concert. I can't speculate on the experiences of these two or anyone else, but I also played casually in a rock group and personally found the profession wanting.

I distinctly remember an instance where I worked extremely hard to sell a middling amount of tickets to a show and although I didn't get paid, the friends I brought along enjoyed it. Later on that week I ran a tutoring session and although it wasn't terribly fascinating stuff that I was teaching, I felt a whole lot better about seeing the "Oh, I get it now" look in someone's eyes and doing something that could actually make someone's life better than killing myself to play for free to an auditorium full of half-interested people. Plus I got paid for the tutoring! I think I played one or two more shows after that, and each time I couldn't help feeling that music was the sucker's bet and I was just wasting my time there. Perhaps this was a function of the times and the music industry collapsing, but I would certainly be sad to see mathematics become as susceptible to market forces.

29. **Curious Wavefunction**
November 11, 2015

"Milner and Zuckerberg perhaps should be encouraged to listen to Scholze and move in a different direction."

Just curious: What kind of direction would you think about here?

30. **Peter Woit**
November 11, 2015

Curious Wavefunction,

As indicated in my previous comments, I think it would be better to not spend money on those already doing very well, but on positions important to the profession that are not paid well. Two examples are
1. High school math teachers. If anyone deserves more recognition and support, it's them.
2. Permanent university teaching positions with a livable salary and with low enough teaching load to allow for a healthy research program. Such positions are disappearing in many universities in the US (see the comment from Tom DeLillo above). Endowing a bunch of these each year would have an impact, encouraging more people to think of math research as a viable career, not just something that only "stars" have a shot at.

If one wants to directly support math research, along the lines traditionally done by the NSF, I think the Simons Foundation does a good job of this, others could follow their lead.

31. **Jeff M**
November 11, 2015

Amen Peter to all the points. One thing in math, we have always prided ourselves (most of us anyway) on not having a Nobel or any sort of star system really. We basically all had the choice after grad school to go work on wall street for a ton of money. When I got my doctorate, the year the academic market in math collapsed, no one I know had a job offer until May. None of us had given up to go make a ton of money, we were all trying desperately to get academic jobs which honestly paid almost nothing, my first job, in 1992, paid $32,600. I worked at that salary for 3 years due to a contract dispute. If I had just gone to Wall Street I could have easily started at 5 times as much, with a big bonus. I would be retired now 🙂

32. **Yes, please.**
November 11, 2015

Did anyone else find the talk after Nadler's – the one by George Lakoff – strange? A lot of the things he describes as original finds of cognitive science, such as looking at multiplication as rotation, are just standard fare that one finds in the explanatory passages of college textbooks. I was so surprised by the triviality of the whole talk that I can't even mention it non-anonymously (nonymously?) for fear of being accused of being stupid or worse, mean-spirited. What am I missing?

33. **Chris W.**
November 11, 2015

I was wondering why Lakoff was even giving a talk at such an event. I guess the work and writings summarized in this section of Wikipedia's profile partly explain the invitation.

34. **Low Math, Meekly Interacting**
November 12, 2015

Lakoff appears to be arguing with people like Max Tegmark about the "reality" of maths. I assume whether or not one considers this a worthwhile discussion to have in the first place will determine their opinion about the appropriateness of Lakoff's lecture for the occasion.

Put me squarely in the "not" column.

35. **Jim Baggott**
November 12, 2015

There’s a long history of wealthy businesspeople distributing largesse for the benefit of science and scientists, all the way from Nobel, Rockefeller, the Carlsberg Foundation (which funded Niels Bohr’s institute and research programmes in Copenhagen), the Bambergers (who founded the Institute for Advanced Study in Princeton, bringing Einstein, von Neumann, Oppenheimer, Godel and Weyl to the faculty) to more recent examples including John Simons, John Templeton, Fred Kavli, and now Yuri Milner and Mark Zuckerberg.

Much of this is good, old-fashioned philanthropy. There are nearly always agendas at work beneath the funding, and in some cases these may be more visible. But good philanthropy is not PR – yes, the odd institute or professorial chair is named for the benefactor but often little else. In some cases, the names of the benefactors are not widely known or advertised.

I can’t help but wonder if the Hollywood, Oscar-style Breakthrough Prizes are saying
more about the benefactors in this case than the individuals who are being rewarded for their efforts.

36. **Chris W.**
November 12, 2015

Jim Baggott,
I assume you mean James (Jim) Simons.

37. **Bill**
November 12, 2015

Another problem is that universities will do anything to raise and keep their rankings high, and rankings usually put high weight on faculty with important awards. Quite often there are quite a few good candidates for any given award but only one gets it, often based on random factors. If later you compare the salaries of award winners and other equally good researchers, the difference can be quite dramatic.

38. **Truth**
November 13, 2015

Actually I think Neil deGrass Tyson would be among the top 100 most famous celebrities in the US. Maybe even Bill Nye too.

39. **Peter Woit**
November 13, 2015

Truth,
Depending on how you measure celebrity fame, I think there might be several scientists in the top 100. The problem is that the way you acquire celebrity fame is by getting on TV, and the people you mention are famous for their TV shows, not for their scientific accomplishments. I don’t think the Breakthrough prizes have created any celebrities, and as long as they reward scientific accomplishment and not talent as a TV personality, they likely won’t. I liked Michael Harris’s point in his latest blog entry:

“whatever the Silicon Valley celebrities may think, the deep motivations that make someone want to be a mathematician are hopelessly, comically, incompatible with the deep motivations that make someone want to be a rock star.”

40. **Justin**
November 13, 2015

Woit,

Can you explain why it is important to have funding to do math research? So long as one is earning a livable wage, I don’t see what difference it makes. A full time professor at a community college I would think would be in just as good a position to do math research as a professor at Harvard. Of course their outputs aren’t expected to be the same, but that’s because the professor at the community college is presumably less talented than the Harvard professor.

41. **Peter Woit**
November 13, 2015

Justin,

Math research is much less dependent on research funding than experimental science fields, where funding is needed for equipment, etc. However, it still does play an important role. A lot of funding from the NSF and private foundations goes to paying the salaries of postdocs and stipends of graduate students. Without such funding, there would be many fewer young research mathematicians at work. This kind of funding also supports conferences, workshops and travel, making it possible for people to get together with others working on the same topic, which is very helpful for making progress. It also sometimes pays for people to take time off from teaching responsibilities and devote full-time to research, including traveling somewhere to work with others.

Research funding also does go to mathematicians with permanent jobs to supplement their salaries, and one could question that (but I don’t want to start that debate here). Universities often take a cut of this, and as a result may reduce the amount of teaching such a person needs to do, providing them with more time to work on their research.

It is quite common for mathematics faculty without grants, at places with little research funding, to do excellent research. A notable example would be Yitang Zhang and his very successful work. But there is a huge difference between teaching at a community college and teaching at Harvard. At a community college one likely will be teaching something like 5 different classes/semester, which is quite a lot of work, leaving the average person with little energy for other projects. At Harvard right now, it’s more like one class/semester, allowing one to spend most of one’s time on research (whether or not one has a grant or any sort of funding).

42. **Bill**
November 13, 2015

Speaking about Yitang Zhang, I am still very surprised he didn’t get the Breakthrough prize. He was an obvious candidate in the sense that he clearly deserved it and that the prize could be seen as a payback for all lost wages. I thought he would be the most apolitical choice, but clearly there was some politics involved.

43. **Bill**
November 15, 2015

I know it’s maybe not a good time for comments, but I thought this might cheer people up: a new breakthrough theorem by Peter Scholze.

44. **Chris W.**
November 16, 2015

It was mentioned above that Peter Scholze used to play in a rock band. Manjul Bhargava (in that photo talking with Scholze) is an accomplished tabla player, according to Wikipedia.

45. **pindaroi**
November 17, 2015

On the subject of Mathematics creeping into popular awareness:
Some of you may be familiar with the series Lewis from ITV in the UK, which is the successor to the Inspector Morse series.
The scene of the opening crime in the case presented by the latest episodes 5 and 6 of Series 9 is the Andrew Wiles building in Oxford's Math department. and the solution depends on the representation of the structure of a specific knot in knot theory.

46. **rc123**
November 17, 2015

Also, about mathematics going popular, in the TV show Elementary, I just realized three mathematicians (all in separate episodes!) have gone on to be murderers…

47. **been there**
November 20, 2015

Why is everyone assuming that Scholze turned down the prize because he does not like the money? Has it crossed anyone's mind that he may have simply not liked the bother to have to cross the Atlantic, dress up in a tuxedo, and in general act as you are told by the organizers and not as you please? Four days of your life for a $100,000 check (minus taxes). If you don't show up, no prize.

48. **Peter Woit**
November 20, 2015

been there,
Maybe I'm wrong, but as far as I know, winners are not required to dress up and perform to get the check (especially the junior ones who I don't think appeared on TV), he could have accepted and politely declined to attend the ceremony. I hope some day we'll hear from him about his thinking about this, I haven't heard anything about him feeling that his motivations have been mischaracterized.

49. **been there**
November 21, 2015

some years back you were required to. as I said – been there.

50. **Pierre**
November 29, 2015

https://www.math.univ-toulouse.fr/spip.php?article645&lang=fr

51. **Peter Woit**
November 29, 2015

Pierre,
Thanks. Sounds like he'll accept that one, and presumably also the Fields medal when he gets in it 2018…

This Week's Hype

November 17, 2015
Categories: This Week's Hype

This week's string theory hype comes to us from USC physicists Clifford Johnson and Nick Warner, courtesy of the USC press office (see here and here). It's garden variety hype of this kind, exactly the same claims about strings and extra dimensions that were being made thirty years ago. There's no acknowledgement these haven't gone anywhere, instead we're "closer than ever to an answer".

When the question of testability comes up, the multiverse is not invoked as an excuse. Instead, it seems that dark matter is going to provide the test:

> Observations show that dark matter and energy constitute more than 95 percent of the universe. Scientists have established that they are new forms of matter and energy, but so far their precise nature is unknown. They may hold the key to confirming the veracity of string theory, Johnson said.
>
> "It's really kind of amazing — and humbling. There are forms of matter that seem to show up naturally in string theory that could well be good candidates to be dark matter," he said. "People are hoping that this could be a key to making contact between theory and nature."

Hard to know what Johnson has in mind for his "show up naturally in string theory" claim. Presumably he's thinking of the ancient "we'll test string theory by finding superpartners" claim, somehow neglecting to mention that this hasn't worked out.

I'm especially impressed by the description of string theory's power to explain dark matter as "amazing – and humbling", deftly pairing outrageous over-the-top hype with an invocation of the selfless humility of the research scientist.

Comments

1. **David Nataf**
   November 18, 2015

   Peter,

   What are your preferred theoretical approach to the fundamental physics problems of dark matter and dark energy?

2. **vmarko**
   November 18, 2015

   "The coolest thing about string theory is it's the *only* theory out there that reconciles quantum mechanics and general relativity," said Warner

   What is not so cool is Warner's complete ignorance of all non-string theories out there, that are at least equally as successful in this reconciling.

   "There's always the possibility that the framework is incomplete, or just plain wrong," Johnson said. "We need a way of getting measurable predictions from the theory that we can go away and test — a key step in any scientific endeavor."

   At least Johnson can be ack-ed for not subscribing to ST-does-not-need-experimental-verification crowd. 🙂

   Best, 🙂
   Marko

3. **Peter Woit**
   November 18, 2015

   David Nataf,

   About dark energy, I have no idea.
   One thing I've never completely understood is the status of attempts to explain dark matter using right-handed neutrino fields. Such fields have no SM interactions, but there is a complicated story about neutrino masses, and it's not clear to me you can't get something massive that would explain the astrophysical observations, but would not show up in experiments designed to look for a very weak non-gravitational interaction. If anyone knows of a good source explaining this particular point, I'd like to hear about it. But I don't want to try and moderate a discussion board about these topics.

   vmarko,
   Johnson pays lip service to experimental verification, but my experience in discussions with him is that his attitude is de facto the same as claiming this doesn't matter at all. Back in the days of the string wars he wrote a long series of postings on his blog denouncing my book, and I tried then discussing with him the problems with "string phenomenology". After getting some odd responses I finally realized his position was that such things were of no interest and didn't matter (he also didn't see anything incompatible between denouncing my book and Lee Smolin's, and refusing to read them). From his latest, it seems to me his attitude hasn't changed, as he deals with the connection to reality problem by repeating very stale hype that no one knowledgeable about the subject would take seriously.

4. **Patrick Harris**
   November 18, 2015

   Dr. Woit,

   The article begins with the oft used (exhausted?) analogy of a wire (or straw) to describe how extra dimensions might be elusively small. While I can begin to wrap my head around extra dimensions from a mathematical perspective, I've never understood what physicists hope to "see" probing finer and finer distances. Would they actually observe these dimensions? Are they looking for violations of conservation laws? Or would they observe motion in 3 spatial dimensions that can only be explained by invoking more?

   I realize this question betrays my naivete and I don't want to contribute noise to your blog, but any pointers would be appreciated :).

   Thanks,
   Patrick

5. **PeterH**
November 18, 2015

With reference to the Dark matter test of String theory:
They may hold the key to confirming the veracity of string theory, Johnson said
How is this claim justified without definite predictions from String theory?

6. **Will Nelson**
November 18, 2015

I am generally far from your viewpoint but when I see someone describe some success as "humbling" the way Johnson does, that really irks me…and all the more so when it's not even a success yet. What he should really find humbling is the number of typos in his book on D-branes.

7. **Peter Woit**
November 18, 2015

Patrick Harris,
What you expect to see depends on the details of what physics is governing your extra dimensions and how this interacts with conventional physics. A simplest example would be one extra dimension, a circle of some size with its geometry fixed. If you Fourier analyze, you expect momentum components in that direction to be discretized in units proportional to the inverse size. So, for very small circles, you expect to not see anything, until you get to energy levels that excite modes with those large momentum components. Then you will have a different energy-momentum relation, one involving those components.

But, there's an infinite array of many other possible things you can do with extra dimensions to make them so far invisible, with new physics if you could see them. Many popular string theory books should have some more about this in the string theory context.

This is kind of off topic, note that even Johnson doesn't think it's plausible to claim we'll see such things.

PeterH,
There are no string theory predictions about dark matter. One might think that Johnson was well aware of this, and thus being intentionally misleading. From my experience trying to discuss such things with him, I think it's also plausible he just knows nothing about this beyond very old claims that string theory implies susy, and susy theories can have a dark matter candidate, hasn't paid attention to the fact these haven't worked out (or thinks that's irrelevant and one is free to keep telling the public that some day they might).

8. **Peter Woit**
November 18, 2015

To be clear. I've no idea what specifically Johnson is referring to as string theory "good candidates" for dark matter. Maybe he's thinking of axions. I've never heard any other string theorist making this claim, though, so the whole thing is rather odd.

9. **Syksy Räsänen**
November 18, 2015

Peter:

For sterile neutrino dark matter, see http://arxiv.org/abs/1204.5379 . I find nuMSSM particularly appealing, as you can also do baryogenesis with the right-handed neutrinos, all without introducing a new higher energy scale.

10. **Peter Woit**
November 18, 2015

Thanks Syksy!

11. **G.S.**
November 18, 2015

Patrick Harris,

When I was touring graduate schools in the early 2000s I remember researchers at University of Washington working on a (more or less) tabletop experiment to test the 1/r^2 law of gravity at short distances. One of the motivators was that if there were extra dimensions and if they were large (much bigger than the Planck scale, but still much much smaller than the distances being tested) then there should be some deviation from 1/r^2. I never followed up, but I'm guessing they never found a significant deviation.

12. **RandomPaddy**
November 18, 2015

> There's no acknowledgement these haven't gone anywhere, instead we're "closer than ever to an answer".

I don't think Theoretical Physics is going to be able to get out of this Tailspin. Particle physics anyway. There's no acknowledgement that the field isn't even going anywhere, having been stuck in more or less a hamster wheel theoretically for, is it three or four decades now? The generation of people now *retiring* can hardly remember a time when the field wasn't all about wild speculation and unsubstantiated results. I don't think this is a situation a community or disipline can just "snap out of".

The lack of progress is also pretty stark when you compare it to other fields in science and technology, which have in an understandable way made massive advances over the same period. At some point, especially if the LHC doesn't make further discoveries, or a successor accellerator never gets built, I think this contrast may become too big to ignore and particle physics funding will dry up. Large colliders may go the way of the Apollo program, but unlike NASA will theoretical physics have enough wider support (or credibility) to be able to "keep the lights on" for a few lean decades. A lot of knowledge and expierience could be lost because the public got tired of hearing too much "fairytale physics". Maybe this kind of "die-off" is actually what needs to happen to break the cycle.

Sorry for being so negative.

13. **Peter Woit**
November 18, 2015

RandomPaddy,

This kind of public relations effort from the USC string theorists does seem to be a response to the problem of people getting a clue that theoretical speculation (and especially string theory) has not been going anywhere for a long time. Unfortunately the idea seems to be to just have friendly media put out stories claiming all is well. This is of limited value in dealing with the NSF and other physicists, who may recognize empty hype for what it is. Perhaps the hope is that the public in general, and philanthropic billionaires in particular are not so savvy…

The experimental HEP situation seems to me quite different, with the LHC and the Higgs discovery definite recent progress. There the difficulty of getting to higher energy and the lack of new discoveries besides the Higgs are a looming problem, but a different one than that of having spent 30 years training students to work on unsuccessful speculative ideas.

14. **phil fogle**
November 18, 2015

Although it's decades since I was an active physicist (optics) I am amazed and delighted be the progess in experimental physics, particularly HEP.

I understand the frustration than has come from the stagnation over the past generation in the apparent lack of productive theoretical physics. However, it's a false premise to try to keep it alive by hyping failed theories…

15. **TS**
November 19, 2015

RandomPaddy,

I would add that also theoretical particle physics has been progressing in large strides, even if we ignore neutrinos, dark matter and other non-accelerator-related topics: calculational techniques have progressed hugely in the past few years: multi-loop calculations, many-body final states, model-independent approaches, automated calculations at NLO to name a few topics. The reality check the LHC provided has drawn brain capacity back from model-building (or model-concocting) to the tough work of doing actual calculations, and it's showing.

Now you can claim that unlike a successful calculation in solid-state physics there are no applications, but then you are back to the question of why one should do fundamental research at all.

16. **Thomas Larsson**
November 19, 2015

RandomPaddy,
There are some ways to rephrase your observations in more quantitative ways:
1. There has been no Nobel prize to any discovery in theoretical HEP made after 1973.
2. When Frank Wilczek becomes 65 in 2018, there will be no active (below normal retirement age) "fundamental" theorist with a Nobel prize, for the first time since H.A. Lorentz won the prize in 1902.
It is of course possible that HEP theorists or even string theorists win a Nobel prize for contributions outside HEP; BPZ are my favorite candidates for their contributions to statistical physics. But with the possible exception of Zamolodchikov, they are also beyond retirement age.

17. **Paul D.**
November 19, 2015

G.S.,

See the most recent publications from the Eöt-Wash group. No detections of new physics.

http://www.npl.washington.edu/eotwash/

18. **Douglas Crumfield**
November 19, 2015

I wish Clifford Johnson (or his supporters) would defend his views here. As a layman, it's hard to know whether I should believe Clifford or Dr. Woit.

19. **Peter Woit**
November 19, 2015

Douglas Crumfield,
For whatever reason, there have been no submitted comments here arguing that there's a case for Johnson's claims. He has his own blog, and way back when put out a long series of postings about criticism of string theory (which he characterizes as a Storm in a Teacup), see
http://asymptotia.com/more-scenes-from-the-storm-in-a-teacup/
Many of those postings have long and substantive arguments in the comments section.

Unless I've missed something though, I've never before heard this claim from him or anyone else about an "amazing" explanation of dark matter from string theory. It seems to be something he never used as an argument back then when other physicists were involved, but does think it's a good argument for a popular promotional article.

20. **piscator**
November 20, 2015

Having just looked at this briefly, your description seems pretty unfair. It seems there is a write-up by a journalist of an interview? a chat? some kind of talk with Clifford Johnson in a cafe. It is a good thing that scientists tell other people what they are doing, but there is commonly some loss of transmission when filtered through a journalist with possibly no science background.

`Hype' is putting out misleading press releases, not making genuine efforts to explain to a journalist aspects of a difficult and technical subject that are far removed from everyday life, and then finding that the non-expert journalist doesn't quite get everything right.

21. **Peter Woit**
November 20, 2015

piscator,
This is basically a press release. The author is a "Public Communications Manager" working at USC. I think characterizing it as hype is quite fair, with the dark matter business especially egregious. It doesn't even bother to try and explain what the USC faculty (Johnson and Warner) are currently doing.

I see no evidence that the writer is misunderstanding anything or getting anything wrong, rather that he is accurately transcribing the hype he was fed. For this kind of PR effort by a university, generally the faculty involved would be asked to look over the article before publication and make sure it is accurate.

Langlands Items

November 20, 2015
Categories: Langlands

There's an interesting development in the math-physics overlap, with a significant number of physicists getting interested in the theory of automorphic forms, often motivated by the problem of computing string scattering amplitudes. This has led to a group of them writing up a very long and quite good expository treatment of Eisenstein series and automorphic representations, which recently appeared on the arXiv. The emphasis is not on the physics applications (which an introduction explains come about when one is dealing with systems with discrete symmetries like the modular group or higher dimensional generalizations), but on the calculational details of the mathematics. There are quite a few expositions of this material in the mathematics literature but many (mathematicians included), may find the detailed treatment here very helpful.

Another aspect of this area is some overlap with the interesting of mathematicians studying Eisenstein series in the context of Kac-Moody groups. There's a conference this week bringing together mathematicians and physicists around this topic.

Turning to recent developments in the Langlands correspondence itself, which relates automorphic forms to Galois representations, when I discussed David Nadler's talk at the Breakthrough Prize symposium (the video is available here), I forgot to mention one thing he talked about that was new to me, the Fargues-Fontaine curve. Nadler explained that Fargues has recently conjectured that the local Langlands correspondence can be understood in terms of ideas from the geometric Langlands correspondence, using the Fargues-Fontaine curve. For more about this from Fargues himself, see materials at his website, which include lecture notes, links to videos of talks at the IAS and MSRI, and this recent survey article. Also informative is some explanation from David Ben-Zvi at MathOverflow.

In April there will be a workshop in Oberwolfach on geometric Langlands that will include this topic, for details of the planned discussions, see here.

Fargues was here today at Columbia, and gave a talk on "p-adic twistors". Nothing much about Langlands, this was about the question of what the analog is for the Fargues-Fontaine curve when you take the real numbers as your field (the use of "twistors" is that of Simpson's, see here, not the common use in physics, which is quite different).

I won't display my extremely limited understanding of this subject by trying to provide my own explanations here. A big problem is that this is mainly about the p-adic Langlands correspondence, something I've never been able to understand much about. After making a renewed attempt the last few days, I at least started to get some idea of what are the biggest problematic holes in my knowledge of the math background. Interestingly, it seems many if not most of them have Tate's name attached (Hodge-Tate, Lubin-Tate, etc, etc…). One pleasant discovery I made is that there are now some excellent expository pieces on this material available, often courtesy of some talented graduate students. One wonderful source I ran into is Alex Youcis's blog Hard Arithmetic, which has given me some hope that with his help I might soon make a little progress on learning more about this kind of mathematics. I don't know what's in the water at Berkeley, but something there keeps producing high-quality blogging by mathematics students, another example is here.

Comments

1. **zzz**
   November 20, 2015

   "what's in the water at Berkeley"

   what is this "water" that you speak of ?

2. **Peter Woit**
   November 20, 2015

   zzz,
   Maybe that's it, with the drought they're drinking something else that would explain the phenomenon…

3. **DrDave**
   November 23, 2015

   I can tell you that the bottom of the reservoir does not taste good right now.

4. **Marty**
   November 24, 2015

   Can I ask a naive question here, though in the spirit of the blog? As a mathematician working on and around the Langlands program for over a decade now, I'm sometimes asked about applications to physics. I can mumble "conformal field theory" and "partition function" and "BPS state". I even talked to a physicist asking about automorphic forms on exceptional groups a few times. But I don't really know enough to know where this fits in physics.

   So really… what's the shortest path from an honest *theorem/conjecture* in the Langlands program (geometric Langlands is fine) to an honest *physical observation/experiment*? Analogies don't count. Also… just because "conformal field theory" is full of physics words/motivations… mentioning it doesn't count on its own.

   Is the motivation of physicists to understand the Langlands program limited to the string theorists?

5. **Peter Woit**
   November 24, 2015

   Marty,

   No, the motivation to understand the Langlands program is not limited to string theorists (I'm at least one counterexample…)

   Maybe one should first differentiate between what mathematicians often mean when they say the "Langlands program" (relating arithmetic questions about Galois groups to automorphic representations) and just one side of that (the automorphic side). One example of quantum field theory that relates both sides is the Witten et al. stuff dealing with the geometric Langlands case, in terms of a mirror symmetry, with its origin in the modular invariance of a twisted N=4 supersymmetric 4d qft (with that explained in terms of a superconformally invariant 6d qft).

   As far as I know, the connections to string theory are just on the automorphic side, with one source the modular invariance of string theory amplitudes, explaining why you expect to get automorphic forms.

My own interest (and I think this has also motivated a lot of other work) is in the analogy between automorphic representations and conformal field theory on a Riemann surface, which Witten first pointed out back in 1988 (in "Quantum field theory, Grassmanians, and algebraic curves"). As I've learned more and more about the function field and arithmetic cases, the way automorphic representations appear there seems to me to indicate a profound unification between those subjects and those parts of QFT which we are able to understand purely in terms of representation theory. I think there's a lot more to discover there, but, maybe that's just me…

Besides the later connection to 4d SSYM, the geometric Langlands stuff from the beginning involved a crucial use of conformal field theory (see for example Frenkel's 1994 "Affine algebras, Langlands duality and the Bethe ansatz"). As far as I know, the connection between this appearance of QFT in geometric Langlands and the later 4d stuff remains poorly understood.

I keep seeing all sorts of interesting suggested ideas and analogies in this area, the above doesn't do justice to the range of such ideas. Conformal field theory plays a central role and I would describe that as 2d qft, not string theory (which to me means you try to integrate over the moduli of curves), but that's an argument over words that is best discouraged (I'll delete any efforts to carry it on here).

6. **David Ben-Zvi**
   November 27, 2015

   Marty – from my point of view it seems like the flow of information is predominantly in the other direction, from physics to the Langlands program, though perhaps the great breakthrough was the realization that [geometric] Langlands duality and electric-magnetic duality are both part of the same big picture. The geometric Langlands program is amazingly well explained from the perspective of physics — it fits perfectly into the structure of four dimensional gauge theory and even better into that of six dimensional conformal field theory (the relations with 2d CFT are AFAIK subsumed inside these bigger structures). This picture is subtle enough that it appears to know all of the bells and whistles and complications people have discovered in geometric Langlands — at least if you ask it the right questions. Further it suggests a richer structure behind geometric Langlands than one would have imagined before.
   From the point of view of physics geometric Langlands allows one to probe electric-magnetic duality on various kinds of defects in certain highly supersymmetric quantum field theories. Some of the structures in the math remain somewhat esoteric from a physics perspective, but others don't — for example, the appearance of W-algebras in the work of Feigin-Frenkel and Beilinson-Drinfeld (the link to CFT that Peter mentions) is directly tied to one of the hottest topics in gauge theory in recent years, the AGT conjecture. W-algebras themselves are an aspect of the theory of Whittaker vectors, a fundamental theme throughout the Langlands program, which in physicists' hands becomes something very geometric about "cigars" (making 2 of your 6 dimensions look like a cylinder capped off with a disc at one end). The relation of geometric Langlands with this 6d CFT more broadly puts in right at the heart of many questions in supersymmetric gauge theories, since maybe half of the so-called N=2 SUSY gauge theories (the subject of Seiberg-Witten theory etc) come from compactifications of this one theory, so that one finds the same structures scattered throughout the subject.

7. **sdf**
   November 28, 2015

   By far the clearest introduction to the FF-curve is the guest blog post that Jared Weinstein gave on Persiflage blog—link below

   https://galoisrepresentations.wordpress.com/2013/07/23/the-fundamental-curve-of-p-adic-hodge-theory-or-how-to-un-tilt-a-tilted-field/

8. **Peter Woit**
   November 29, 2015

   Thanks David and sdf,

   I had forgotten to include a link to that Jared Weinstein blog entry, which is a great example of an expository blog article. There's a second part to it at
   https://galoisrepresentations.wordpress.com/2013/08/12/the-fundamental-curve-of-p-adic-hodge-theory-part-ii/

This Week's Non-Hype

November 23, 2015
Categories: Uncategorized

Since I often post here complaints about articles produced by the press offices of various institutions that hype in a misleading way physicist's theoretical work, I thought it a good idea to make up for this by noting a positive example of how it should be done. The SLAC press office this week has a Q and A with Lance Dixon, with the title [SLAC Theorist Lance Dixon Explains Quantum Gravity]() which is quite good.

Dixon gives an informative explanation at a basic level of what the quantum gravity problem is. He includes an even-handed description of the string theory approach to the problem, and explains a little bit about the alternative that he and collaborators have been pursuing, one that has gotten much less attention than it deserves. This is a very technical subject, so there's a limit to how much he can explain, but he gives the general idea, and includes a [link to his most recent work]() in this area.

Many promotional efforts for string theory begin by making claims that quantum field theory cannot be used to understand quantum gravity, due to the divergences in the perturbation series. This has been repeated so often, for so many years, that it is an argument most people believe. The situation however is quite a bit more complicated than this, with one interesting aspect of the story the discovery in relatively recent times that long-held assumptions about divergences in perturbative quantum gravity calculations were just wrong. Such calculations turn out to have extra unexpected structure, and thus unexpected cancellations, making naive arguments about divergences incorrect. Continuing progress has come about as Dixon and others have developed new techniques for actually computing amplitudes, uncovering unexpected new symmetries and cancellations.

For a good summary of the current situation, see this [talk by Zvi Bern](), especially page 7, where Bern details how, going back to 1982, "So far, every prediction of divergences in pure supergravity has either been wrong or missed crucial details". For N=8 supergravity, current arguments say that a divergence should show up if you could calculate 7 loop amplitudes, but Bern warns against betting on this. In that talk he also explains the recent work with Dixon and others that gets mentioned in the SLAC piece, about the surprising nature of the divergence in pure gravity at two-loops, making its physical significance and whether it really ruins the theory not so clear.

I was interested to read Dixon's account of his thinking back in the mid-80s:

> I began to be concerned that there may be actually too many options for string theory to ever be predictive, when I studied the subject as a graduate student at Princeton in the mid-1980s. About 10 years ago, the number of possible solutions was already on the order of 10^{500}. For comparison, there are less than 10^{10} people on Earth and less than 10^{12} stars in the Milky Way. So how will we ever find the theory that accurately describes our universe?

Although this never made it into media stories, I think that by a couple years after the initial enthusiasm for string unification in 1984, many theorists had already started to notice that the idea likely had fundamental problems, with a serious danger that it would turn out to be an empty idea. This now has become clear, but the idea lives on, with "QFT must have divergences" the main argument for continuing to take it seriously. Now that argument isn't looking so solid…

Update: A good [explanation of the situation from 4 gravitons]() who, thankfully, is not overly worried that he might be giving succor to the Woits of the world…

Comments

1. **Christopher Herzog**
   November 23, 2015

   I think it's interesting that one of the main tools that Dixon et al. use to compute scattering amplitudes in gravity is a relation between gravity and gauge theory amplitudes that in my opinion is simplest to understand from a string theory perspective — the Kawai-Lewellyn-Tye relations (Nuclear Physics B, 269 (1986) p 1-23).

2. **Peter Woit**
   November 23, 2015

   Christopher Herzog,
   Sure, that's one of several techniques they use. But if you look at Bern's slides string theory doesn't seem to be the source of the new phenomena that they've been discovering in recent years. It looks a lot more like they are uncovering new basic structures in perturbatively quantized gravity quantum field theories, and I don't see evidence that string theory is the way to understand them.

3. **Umesh**
   November 23, 2015

   Perturbative divergences aren't the only problem plaguing QFT-like treatments of gravity – this is a very retro, 80's way of stating it, and it's no fault of string theorists that it keeps getting reported/ repeated this way. The much more profound aspects of non-QFT like behaviour of gravity involve the area like scaling of entropy, for which there seems no sensible explanation from a local QFT point of view. The only known way is AdS/ CFT, which is string theory again (of course other examples of explicit calculations are Strominger/ Vafa etc, which involves string theory in a crucial way.). This isn't to downplay any of the the above work on perturbative supergravity amplitudes, but to make use of the above to attack string theory by cherry picking one aspect of perturbative quantum gravity is wrong.

4. **Peter Woit**
   November 23, 2015

   Umesh,
   This isn't an "attack on string theory", just reporting the fact that one of the standard arguments used to claim qft cannot describe quantum gravity shows signs of being in the process of being shown to be wrong.

   As for entropy scaling with the area not being possible in qft, recall that Hawking radiation is a semi-classical qft calculation.

   Going on about how poorly understood relations to AdS/CFT are the only way to understand quantum gravity in 4d dS is just pure hype, and trying to claim that such arguments show qft can't be used in this context is absurd.

5. **Umesh**
   November 23, 2015

   Hawking's calculation is indeed an 'semi-classical QFT computation', but the entropy in question is of the black hole background – which needs a quantum theory of gravity to be understood. QFT, with it's local degrees of freedom, can never produce an area extensive scaling. Of course effective QFT in a black hole background is sufficient to highlight this point, but the conclusion is about gravity (semi-classical in this case), not about QFT. To repeat, Hawking's calculation doesn't say anything about the QFT. Also, nowhere in my comment have I claimed that AdS/ CFT can be directly (or in any obvious way) used to understand dS quantum gravity. I have used it to highlight the conceptual issues about quantum gravity, the area scaling of entropy being one of them.

6. **Peter Woit**
   November 23, 2015

   Umesh,
   The posting was about conventional claims that you could never get quantum gravity out of qft because of its perturbative short-distance behavior now looking like they may be wrong. I don't think your claims about qft and area law behavior are any more solid. The history of qft contains a long list of claims that qft couldn't reproduce X (see the history of the Higgs mechanism and QCD) which turned out to be nonsense. Often this was because of the subtleties of gauge invariance and gauge degrees of freedom. The subtleties of geometrical degrees of freedom and diffeomorphism invariance are even greater.

7. **Umesh**
   November 23, 2015

   It is true that QFT has overcome many issues claimed to be beyond its reach in surprising ways in the past. The claim that QFT cannot reproduce the area extensive entropy are much more solid and quantitative – references are numerous – contrary to what you claim. Of course one cannot predict anything about the future, but perturbative finiteness doesn't mean non-perturbative consistency (which is where the trouble with quantum gravity lies, and the area extensive scaling is a hallmark of a non-perturbative effect in quantum gravity – leads to issues like loss of unitarity and so on). Barring some miracle (with local theories like QFT), it is in no way clear as to how to get an area law for something as universal as the entropy, at least in any obvious way. I'm also quite sure that nobody claimed 'you could never get quantum gravity out of qft because of its perturbative short-distance behavior' – it surely was one problem with making a conventional QFT of gravity for starters. If one thinks he/she has an answer to that, there's a huge list of issues waiting to be answered – area extensive entropy is next on that list.

8. **Peter Woit**
   November 23, 2015

   Umesh,
   Sure, there are problems with any theory of quantum gravity that go beyond the heavily advertised perturbative short-distance problems. I'm just pointing out there's a problem with the central heavily advertised claim here (do you deny this?).

   I personally don't see any point to entering into ideological arguments of the "my non-existent supposedly consistent but inherently untestable non-perturbative theory of quantum gravity sucks less than your non-existent supposedly consistent but inherently untestable non-perturbative theory of quantum gravity" sort. It seems clear to me that the bottom line is we don't yet know what the right fundamental degrees of freedom are to describe space-time geometry, or what the right way to go about quantization of these is. In such a situation, vague arguments about "entropy" and "locality" are no serious reason not to pursue any particular line of research.

9. **Noboru Nakanishi**
   November 24, 2015

   Two decades ago, I showed that the perturbative approach to quantum gravity is nonsense, because it is based on the wrong assumption that the zeroth-order approximation of the gravitational field is a c-number (see N. Nakanishi, Gen. Rel Grav. 27 (1995) 65). Quantum Einstein gravity can be formulated quite satisfactory and beautifully without using perturbation theory (see N. Nakanishi and I. Ojima, Covariant Operator Formalism of Gauge Theory and Quantum Gravity (World Scientific, 1990)). The method for solving qft in the Heisenberg picture is reviewed in N. Nakanishi, Prog. Theor. Phys. 111(2004), 301.

10. **Ralph**
    November 24, 2015

    I'm confused about the emphasis on the very-large-number of discrete string theory vacua.

    With atoms, we tend to make continuous approximations for collections of millions or just thousands of atoms. Why are solutions to string theories different?

    With 1e500 string theory vacuosities, why should one count discrete solutions, rather than making continuous approximations to the set of solutions, and using less precise but more informative notions such as dimension and measure?

    E.g. 1e500 solutions would be no big deal if they were all spread along a single "line" characterized by a single parameter. One could treat the parameter as a continuous variable until/if we ever managed to constrain the parameter sufficiently to reduce to a smallish number of physically realistic solutions, and then when justified, switch to the more precise discrete picture?

11. **vmarko**
    November 24, 2015

    Ralph,

    You should be careful to make a distinction between 10^500 solutions of a single theory and 10^500 different theories. The latter is the landscape problem in ST. It is the problem of 10^500 inequivalent ways to define one particular theory, before ever looking for any of its solutions.

    And no, you cannot parametrize those 10^500 theories with a single parameter (or any reasonable number of parameters), since there is no unique order relation between them (you cannot tell which vacuum would come "before" and which "later" as you increase the parameter). If such a technique were possible, people would be already using it. 🙂

12. **Umesh**
    November 24, 2015

    '..there's a problem with the central heavily advertised claim here (do you deny this?).'
    This particular claim is more complicated according to me to be responded to in simple confirm/ deny (or agree/ disagree) terms (also my denial is of no value in these intensely technical matters). As far as specialists are concerned, I think it is true that highly supersymmetric theories of gravity (as in {\cal N} = 8 SUGRA in 4d) might be perturbatively finite to high orders. That said, to claim that the whole issue of short distance divergence of gravity (the 'heavily advertised claim', as you put it) has started to seem wrong is false. In my limited view of the subject, it is still reasonably accurate to say that most garden variety field theory versions of gravity (even with less SUSY) are divergent in the normal sense. I don't know the full status of the field, but think it fair to claim that even experts would agree that the perturbative divergence issue in gravity as a field theory is far from settled. Again, let me emphasize the SUGRA community has done an amazing job with these intensely difficult calculations (not that my emphasis matters).

    "my non-existent supposedly consistent but inherently untestable non-perturbative theory of quantum gravity sucks less than your non-existent supposedly consistent but

inherently untestable non-perturbative theory of quantum gravity"
I cannot speak for you precisely, but I presume the aspect of the 'ideological debate' you refer to (in this case) is the case for gravity as a field theory (and its perturbative finiteness) vs. string theory as a quantum theory of gravity. As an aside, I should mention that there is a post somewhere on this blog that has a similar discussion, there the case has been about the 'asymptotic safety' scenarios for gravity (also to with gravity as a field theory) vs. string theory, and the same issues remain. Fundamental degrees of freedom describing spacetime geometry are well known in certain special geometries – again I've to refer you to many thousands of papers about the AdS/ CFT correspondence. Therefore, that we 'don't know the fundamental degrees of freedom of spacetime geometry' is a blanket statement that is wrong. Further, you say, and I quote '..vague arguments about "entropy" and "locality"..' . I must emphasize that the basic notions of 'entropy' and 'locality' in QFT and QFTs as applied to gravity are far from 'vague'. Right from Hawking's seminal paper full of quantitative calculations of entropy to other attempts at getting the area law and thousands of follow up papers (to Strominger/ Vafa's calculation) are very much counter to your claim.

13. **Peter Woit**
November 24, 2015

Umesh,
This discussion shows exactly why I generally make it a policy to avoid debates about quantum gravity. It's an extremely complex subject, with debates like this immediately degenerating into empty ideologically-driven sloganeering, with endless irrelevancies ("asymptotic safety"???) and dubious claims thrown in to make sure that no substantive question gets accurately addressed. This is just a complete waste of time.

Dixon is doing the opposite, addressing a very specific, well-defined technical issue with the theory and making progress at better understanding it. Given his success at this and his background doing important work in string theory, his opinion that looking at alternatives to string theory as a theory of quantum gravity I think is well worth listening to.

14. **Umesh**
November 24, 2015

I don't quite understand what sloganeering and dubious claims you're referring to. I mentioned that there was a blog post here relating to the asymptotic safety scenarios for gravity as a field theory, and as similar discussion ensued. I'm sure Lance Dixon is a great physicist and has much important to say about string theory (surely more than me), but on the other hand, I could invoke equally competent authority to claim that 'alternatives to string theory as quantum gravity' are quite futile. That wouldn't suit you, or help the discussion, would it? Just to repeat again, to make sure there is sufficient accuracy, here's where I stand:

The claim that gravity is finite in perturbation theory in no way makes the 'non-renormalizability of gravity' claims completely go away. It is true that highly SUSic theories of gravity are perturbatively finite. This by itself in no way dilutes the claim for string theory as correctly addressing the issues which remain once perturbation theory is accounted for.

Also, said blog post relating to asymptotic safety:

http://www.math.columbia.edu/~woit/wordpress/?p=2199

15. **Peter Woit**
November 24, 2015

Umesh,
Following that link to my old blog posting, the only content there relevant to this discussion is a sentence from a commenter pointing to a long rant by Lubos Motl, and I see Lubos has a new equally long one today. I find it hard to think of any bigger waste of time than trying to engage with those.

16. **Umesh**
November 24, 2015

Sure, you're free to decide whether or not to spend time on Lubos Motl's blog. There's more relevance than just the commenter's pointing to Lubos Motl's blog. Whether or not his post is driven by ideology is subjective and surely irrelevant. The only important thing is about the reference to black hole thermodynamics (surely independent of any ideology), which is discussed in the comment thread. I'm just pointing out the similarity to the discussion above. Let me copy-paste your reply to V_NO:

V_NO,

What Weinberg is talking about is the standard claim for string theory that it is needed to deal with the perturbative renormalizability problems of quantum gravity. That may not be true, either because of asymptotic safety, or also because of possible perturbative finiteness of some supergravity theories.

Non-perturbatively, you don't really even know what string theory is, I don't see how one can claim it's the only possible way to deal with non-perturbative problems in quantum gravity. The problem is more complicated than just invoking black hole thermodynamics, which was originally discovered as a semi-classical phenomenon in QFT.

If I understand your above reply correctly, you seem to be ambiguous regarding string theory being the only way to resolve non-perturbative issues of quantum gravity, one manifestation of which is black hole thermodynamics (please note similarity to above discussion, which is literally the same thing). If the problem is 'more complicated than just invoking black hole thermodynamics', then it stands to reason that one must solve that issue immediately if one thinks that he/ she has overcome the 'renormalizability problem of gravity' one way or other, before proceeding to 'more complicated' aspects you seem to refer to. If the same remains your stance pertaining to the above discussion, I've nothing more to say.

17. **Peter Woit**
November 24, 2015

Umesh,
If characterizing Motl as "driven by ideology" is "subjective", so is everything… Sorry, I've wasted enough time on this.

18. **Umesh**
November 24, 2015

It just means that Lubos Motl's blog posting is irrelevant to this this discussion. What is surely not ideology is black hole thermodynamics, and that's the bulk of my last comment. The question of whether or not when one solves 'perturbative issues in quantum gravity' one can indeed solve issues related to black hole thermodynamics is a sharp physical question, independent of Lubos Motl's (or yours, or mine or anyone else's, for that matter) ideology. If you can concentrate on this aspect, we can make progress about the issue at hand.

19. **chris**
November 24, 2015

Umesh,

why is pointing out progress in a direction that progress has been thought to be impossible 10 years ago so offensive to you?

Dixon&Bern have really done great work in the last decade. They picked up a task that 't Hooft gave up and made progress. Whether or not you like it, that does change the perspective on perturbative gravity for some of us.

And about that area-entropy relation: all you have to do is cut a QFT at the stretched horizon. Or use Ashtekar variables. Yes, holography is an interesting feature – but how that motivates string theory is beyond me. and even how that relates to the topic discussed here other than a distraction from it is even less clear to me.

20. **Umesh**
    November 24, 2015

    As I've made clear in my comments, the work of Dixon-Bern is far from 'offensive' to me, as you claim. I've even made it clear that Dixon is a great physicist (and achieved string theorist as well). Therefore, I don't understand why you feel the achievement of above physicists (which also I characterize as great, not that it matters) is 'offensive' to me. I should possibly be very clear that high order perturbation theory calculations in SUGRA are great. I have nothing against it, and am surely not advocating abandoning it or some such idiocy. Apart from that, because of my ignorance, I can't quite comprehend your statements about black hole entropy, maybe some paper/ reference would help. I don't think I've mentioned anything anywhere in my comments that imply 'holography motivates string theory'. If you are saying that there're other ways to get black hole entropy apart from strings, great, I don't know of them. What I don't quite follow is how impressive work on the finiteness of perturbative SUGRA (or asymptotic safety in gravity) would possibly imply that it is non-perturbatively consistent – without evidence coming from black hole thermodynamics and so on. Apart from that, lemme say again, tour de force work.

21. **vmarko**
    November 24, 2015

    Umesh,

    I agree with you that advances in perturbative QG are not going to teach us much about nonperturbative problems such as the BH entropy.

    But I am quite perplexed by your statement "If you are saying that there're other ways to get black hole entropy apart from strings, great, I don't know of them.". Wow, is it really possible that you haven't heard of, say, loop quantum gravity? The calculation of BH entropy in LQG doesn't involve AdS/CFT, doesn't involve SUSY, doesn't require 10 or 11 dimensions, doesn't require string-anything, but certainly does get the entropy calculation right. And the calculation works for an ordinary Schwarzschild black hole, as opposed to the extreme Kerr black holes that are doable in ST. And you haven't even heard about any of this?! Wow!

    If you honestly really didn't know that there exists a LQG calculation of BH entropy, start for example here,

    https://en.wikipedia.org/wiki/Black_hole_thermodynamics

    and look up the LQG-related references cited in there.

    Best, 🙂
    Marko

22. **Peter Woit**
    November 24, 2015

    Sorry folks, I'll delete any more attempts to carry on this argument about black hole entropy calculations. It really has nothing to do with the Dixon article, and anyone who wants to read an LQG vs. string theory argument about these calculations can find it a hundred other places.

23. **Chris W.**
    November 24, 2015

    In reading the SLAC Q & A, which is obviously intended for non-specialists and the general public, it is apparent that Dixon avoids discussion of the dynamics of spacetime itself. That is, the focus is on how far one can get a conventional QFT for a field that has the characteristics expected of the gravitational interaction in flat spacetime with a fixed metric.

    The upshot is that one can get considerably farther than was initially thought after early attempts. This decouples two issues that seem normally to be conflated; (1) can perturbative finiteness of a quantum theory of the gravitational field be achieved, and (2) can gravity be understood as an interaction in flat spacetime, for which calculated effects normally thought of as indicative of spacetime curvature can be given an operational interpretation in flat 4-dimensional spacetime that works for most or all "practical" purposes.

    Notice that (2) assumes we can *do* calculations, i.e., that (1) has been answered in the affirmative. The question then becomes, what do we make of this? Does it fly in the face of the lessons we thought we had learned from general relativity? Does it make the program implied by (1) essentially pointless, even if it is successful on its own terms?

    One possible response—perhaps more or less what motivates workers in this area—is that the effort is far from pointless. It would give a well-defined quantum theory of gravity in which one can do sensible calculations with results that can be compared with observation. If those comparisons show disagreements (as one might expect), then the theory has problems, but they are empirical problems, not internal problems that render the theory inherently nonviable. One will have to consider where to go next, but an extremely important milestone will have been achieved, which contrasts sharply with the current predicament of string theory (and the putative M-theory).

24. **oneloop**
    November 24, 2015

    @vmarko

    Please read this paper

    http://arxiv.org/abs/1205.0971

    and this Wikipedia article

    https://en.wikipedia.org/wiki/Immirzi_parameter

25. **vmarko**

November 24, 2015

While I admire the attempts of Dixon and others to push and see how far one can get with the idea of perturbative QG, and the quality of the SLAC Q&A being an exquisite example of non-hype (as Peter says), I have to say that I am somewhat skeptical regarding the whole program of QG as a spin-two field in flat Minkowski spacetime. Chris has put it in a form of a question:

"can gravity be understood as an interaction in flat spacetime […] Does it fly in the face of the lessons we thought we had learned from general relativity?"

My answers would be that gravity is (1) unlikely to be understood like that, and (2) it certainly does fly in the face of GR.

Treating gravity as a spin-two field in flat spacetime is just like treating planet trajectories using epicycles — it can certainly be done, provided that you introduce enough free parameters, but it is unlikely to give you any real geometrical insight into what is going on, and it is ugly beyond anybody's taste. One of the major points of GR was that spacetime itself is participating in physical events, as opposed to being a "box" in which physics happen. This is called "background independence", and the approach to gravity as a spin-two field in flat spacetime (infamously endorsed and advocated by Feynman) simply ignores this lesson of GR. Besides, the background geometry is unobservable, so insisting on it is like insisting on aether in electrodynamics. You can have it, but to what purpose?

Even if one accepts the background-dependent approach, the question is which background is better, Minkowski or de Sitter? Given that we observe a positive cosmological constant, one can argue that de Sitter background would be more "correct" than the Minkowski background, rendering void most of perturbative QG in flat spacetime.

So while Dixon's research is valiant in its technical ingenuity, skill and very nontrivial analysis, I somehow doubt that it can have any fundamental impact on the problem of quantum gravity.

[attempt to discuss black hole entropy with oneloop edited out]

Best, 🙂
Marko

26. **Peter Woit**
November 24, 2015

Marko,
Perturbation theory isn't analogous to epicycles in celestial mechanics, it's analogous to, well, perturbation theory in celestial mechanics (where it has a very long history).

It's not unreasonable to expect that, for a weakly coupled theory, a perturbation expansion about flat spacetime based on gravitons might give an extremely good approximation, given that we live in a very nearly flat spacetime. If your perturbation expansion runs into trouble, you can argue that there's a conceptual problem with the whole idea. One advantage the string theory quantum gravity program always had was that they had an argument that they didn't run into this problem, but qft would (that argument now seems to have a hole).

What I think is interesting about the Dixon et al. stuff is that they are unearthing new structure in these supergravity theories. Once this is better understood, it could be either not very interesting (a complicated mathematical artifact of the specific calculation), or very interesting (a deep new symmetry, one saying something about both fermionic and bosonic degrees of freedom).

Dismissing what they are doing as useless because it's not "background independent" or doesn't reproduce an argument about black hole entropy, or isn't holographic, or whatever seems to me foolish. Quantum gravity research programs that just study pure gravity, with whatever features you find attractive, are clearly missing a huge part of the fundamental structure of the world. Personally I'm extremely dubious that you can understand quantum gravity purely independently of matter degrees of freedom, and if you do, you'll never know whether you're right since you can't test pure quantum gravity theories.

And, please, endless arguments about "background independence" are even more stale and tedious than the black hole entropy ones. Just stop.

27. **vmarko**
November 24, 2015

Peter,

"a perturbation expansion about flat spacetime based on gravitons might give an extremely good approximation, given that we live in a very nearly flat spacetime"

As an approximation yes, I agree, but promoting perturbation expansion into a fundamental theory is a different ballgame. And we already know that there are high-curvature regions of spacetime (say, inside black holes) where the perturbative expansion around flat background just isn't a reasonable thing to do. Promoting this approach to a fundamental theory of QG seems ill-conceived IMO.

"a deep new symmetry, one saying something about both fermionic and bosonic degrees of freedom"

I am not trying to dismiss Dixon's work, and people should certainly be encouraged to keep studying it. But extraordinary claims require extraordinary evidence. If it really turns out that such a deep new symmetry is found and understood, I'll probably become a believer. But until that happens, I'm simply very skeptical about the benefits of the whole perturbative approach.

And regarding matter fields, I completely agree that studying only pure gravity won't cut it. But the matter sector is also in a very poorly understood state. The Standard Model is complicated, reasons for particle families and symmetries unknown, and vast sectors not explored enough (dark matter, neutrinos…). It is questionable whether such a complicated structure can actually help in figuring out the properties of QG itself. IMO, QG should be studied together with matter fields, but only in a generic framework where the number and type of matter fields are not fixed in any way. Who knows what might come up next in LHC, or what dark matter is made of? One cannot really rely on any details of the matter sector when discussing QG — just generic properties and the type of coupling (equivalence principle, alternatives, etc…).

Best, 🙂
Marko

28. **Peter Woit**
November 24, 2015

Marko,
No one is claiming that a perturbative expansion is a fundamental theory. However, one might optimistically hope that if it succeeds, it will give evidence that one has identified a good set of fundamental fields and their symmetries. If one understands path integrals well enough (a big if..), one might then even be able to write down a fundamental theory.

"the matter sector is also in a very poorly understood state"
As opposed to the quantized geometry sector??? Here I disagree completely. The lesson of the last few decades in HEP is that we understand the matter + gauge fields sector depressingly well. And if your theory says nothing at all about this, and only deals with likely unobservable effects, you'll have no way to convince others you have the right one, other than endlessly trying to claim yours is prettier than other people's.

29. **Noboru Nakanishi**
November 24, 2015

Peter,
As I stated above, the perturbative approach to quantum gravity has a conceptual difficulty. In order to set up the interaction picture, one has to choose not only a particular space-time structure such as flat but also a particular coordinate system such as Minkowski metric by hand (Indeed, if one adopts polar coordinate system, perturbation theory will become completely different from the conventional one.). This procedure explicitly violates general-coodinate invariance (more precisely speaking, BRS invariance) at the operator level, that is, pertubation theory explicitly violates BRS invariance in each order.
The introduction of a particular c-number metric as the zeroth-order approximation of the quantum gravitational field is indeed wrong. The zeroth-order quantum gravitational field is a q-number; its explicit form was already given in terms of a complete set of Wightman functions.

30. **Peter Woit**
November 24, 2015

Noboru Nakanishi,
I certainly don't claim that I know how to set up such a perturbation expansion in a sensible way that properly handles local space-time symmetry via BRS. This is a question for Dixon et al. They have some calculational method which I believe they claim gives consistent, sensible results, and a lot of calculations of specific amplitudes to back this up. You'll have to ask them how they handle the problem you identify.

31. **Dave Miller in Sacramento**
November 25, 2015

Noboru,

You said:
>As I stated above, the perturbative approach to quantum gravity has a conceptual difficulty. In order to set up the interaction picture, one has to choose not only a particular space-time structure such as flat but also a particular coordinate system such as Minkowski metric by hand.

You're right, of course.

However, when I was a doctoral student at SLAC back in the early '80s, I did a bit of work on lattice theories: when I first heard about lattice theories, I objected that they violated Lorentz invariance in a *huge* way, and therefore could not be right.

I trust that everyone here will agree that my initial impression was missing the point: i.e.,. working on lattice theories can be useful, and lattice theories can (sometimes) indeed recover Lorentz invariance in the limit that the lattice spacing goes to zero.

Around the same time (circa 1980), I went to a seminar (I think it may have been by Dan Freedman, though I'm not sure — it has been a long time!) discussing early attempts at perturbative quantum gravity, and I made the same sort of objections that have been made here: the inability to deal with non-perturbative effects such as black holes, the breaking of general covariance, etc. The speaker tried to be polite, but clearly thought that I was an ignorant grad student who did not understand that you had to walk before you could run.

Perhaps the speaker was right: I continue to be intrigued by the issues I tried to raise way back when, but, after all, neither I nor anyone else has solved those issues. Perhaps it is best to try to do something where one can actually calculate?

Dave

P.S. Peter, I think discussion like this can help many of your readers think through these issues even if the discussion itself does not reach closure. After all, someday, some bright young physicist who is mulling over such conundrums may actually come up with something that will create a major breakthrough.

32. **vmarko**
November 25, 2015

Peter,

"No one is claiming that a perturbative expansion is a fundamental theory."

Wait, are you saying that Dixon is not trying to construct a fundamental theory of QG? So he is talking about the prospects of quantization and renormalization of GR that will in the end be an approximate theory of some other fundamental QG model? Given that quantization is a nonunique process, why would you even expect that two different quantization schemes (the perturbative one and the "fundamental" one) would be compatible in any way at all? If Dixon is **not** trying to construct a fundamental QG model, then in what way is his work relevant for QG at all?

Again, if he discovers some unexpected structure that can be used to construct a fundamental QG model, that's great, and I'll gladly change my opinion. Otherwise, I just don't see what is the benefit of his approach, aside from developing mathematical techniques to perform complicated multi-loop calculations.

""the matter sector is also in a very poorly understood state"
As opposed to the quantized geometry sector??? Here I disagree completely."

I said that the matter sector is **also** in a very poorly understood state. Namely, not as opposed to QG, but rather just like QG.

"The lesson of the last few decades in HEP is that we understand the matter + gauge fields sector depressingly well."

Really? Why do we have three families of particles? Are neutrinos of Dirac or Majorana type? What principle fixes the choice of SU(3)xSU(2)xU(1) gauge group? Can you be certain that in the future LHC will not find the fifth interaction, extending the gauge group by an additional SU(4) term, for example? What is dark matter made of? What is the cause of matter/antimatter symmetry violation? I'd say that our understanding of these things is not depressingly good, but depressingly bad.

The existence of the dark matter is a proof that the SM must be incomplete. So if we already know that, we have to expect that various properties of the SM are likely to change in the future (just look at what happened to the neutrino sector). It is a transitory theory, one that will keep being improved/substituted with various BSM models, as we learn new facts about matter fields from experiments.

So when discussing properties of QG, one cannot rely on the details of the SM, simply because they are likely to be modified as we collect more data about matter in the UV and IR regimes.

Best, 🙂
Marko

33. **Peter Woit**
November 25, 2015

Marko,
One could hope that the relation between perturbative QG and fundamental QG might be much the same as for other QFTs one understands (e.g. QCD). Arguing that it's not worth investigating the perturbative version is kind of like telling Feynman and Schwinger back in the late 40s "why are you wasting your time on those approximate calculations? Everyone knows the full QED theory is completely different, has a Landau pole, and that's the real problem that needs to be solved".

I don't know what to say about the claim that the state of QG is just like the state of the SM. Maybe there really are alternate universes and we're in different ones…

34. **vmarko**
November 25, 2015

Peter,

"Arguing that it's not worth investigating the perturbative version is kind of like telling Feynman and Schwinger back in the late 40s "why are you wasting your time on those approximate calculations? Everyone knows the full QED theory is completely different, has a Landau pole, and that's the real problem that needs to be solved"."

Well, the difference is that in QED we have access to experimental data that is described by the approximations that Feynman and Schwinger were developing, so they were obviously useful. But conceptually, this hypothetical criticism of Feynman and Schwinger is actually entirely valid — both QED and SM are **effective** field theories (i.e. approximations at a given scale), while the fundamental theory cannot be a QFT, precisely because of things like the Landau pole and such.

In case of QED, perturbative calculations make sense because we can perform experiments, and compare the "true" experimental result to the approximate theory. When we see agreement, we use it as a confirmation that the approximation scheme is good enough, whatever the fundamental theory may be.

In the case of gravity, however, we have no experiments, and there is no way to test if perturbative QG is anywhere near being a valid approximation to reality. So it's a bit of an academic exercise. Putting one's faith into it, then, is ideologically as biased as believing in the "truth" of string theory, LQG, or {insert your favorite QG model}.

Again, I am not saying that perturbative approach is worthless or a waste of time. But if people are to take it seriously, its proponents must make a strong convincing case why they think that nonperturbative effects can be ignored in the quantization procedure. In QED you have strong agreement with experiment to make that case. In QG you don't.

Best, 🙂
Marko

Quick Items

November 25, 2015
Categories: Uncategorized

A few quick items before the holiday:

- I hear that Luis Alvarez-Gaumé will be the next Director of the Simons Center, starting next Fall, taking over from John Morgan, the founding Director. My understanding is that the hope was to have the directorship alternate between mathematicians and physicists, and with the hire of Alvarez-Gaumé, they've managed to achieve this. His early work on supersymmetric path integrals and the index theorem (see here) was characteristically lucid and this remains one of the great points of intersection between modern mathematics and the quantum theory. One of the best relatively short introductions to QFT is this one (with a shorter arXiv version here). I think he's an excellent choice.
- In physics blogger news, Tommaso Dorigo reports that he has found a publisher for the book he has been writing: Anomaly! – Scientific Discoveries and the Quest for the Unknown, and it should appear next year. I'm very much looking forward to seeing a copy. His insightful but irreverent take on experimental HEP I'm sure will make this a fascinating read for anyone interested in the subject.
- Matt Strassler's blog has been dormant for a while, but he has now been heard from. After a couple year visiting position at Harvard, he says he's now "employed outside of science", but working on a book about particle physics for non-experts.
- Jim Holt has a review in the latest New York Review of Books of my colleague Michael Harris's Mathematics Without Apologies.
- By some accounting, today is the 100th anniversary of Einstein's GR field equations, which he presented November 25, 1915 at a lecture in ~~Gottingen~~ Berlin. This anniversary has been celebrated in many places, in many ways this year, so there's not any need for me to chime in. Among many excellent treatments of the topic, there's also an unfortunate tendency of some to use Einstein to grind their particular axes. Sean Carroll I suspect has Einstein spinning in his grave, using the PBS NewsHour to enlist Einstein as a multiverse fan:

  > The ability for seemingly constant things to evolve and change is an important aspect of Einstein's legacy. If space and time can change, little else is sacred. Modern cosmologists like to contemplate an extreme version of this idea: a multiverse in which the very laws of physics themselves can change from place to place and time to time. Such changes, if they do in fact exist, wouldn't be arbitrary; like spacetime in general relativity, they would obey very specific equations.

  Perhaps Carroll could enlighten the public by writing down these "very specific equations" he's advertising, for comparison to the Einstein field equations.

  If I were to grind my own ax here, it would be to note that Einstein's great breakthrough came about through close collaboration with some of the best pure mathematicians around, adopting difficult but deep ideas about geometry. Without the mathematicians, I'd guess that the theory of general relativity would have taken many more decades to come to fruition. Maybe there's a lesson there…

Comments

1. **Warren**
   November 25, 2015

   `Perhaps Carroll could enlighten the public by writing down these "very specific equations" he's advertising, for comparison to the Einstein field equations.'

   As if the public (as in "PBS") were enlightened even by Einstein's equations.

2. **Peter Woit**
   November 25, 2015

   Warren,
   It seems I was being too indirect. The point is that there are no "very specific equations" for the landscape, so Carroll is moving beyond axe-grinding into being intentionally misleading here. At this point no one knows what the space of configurations of a full string landscape theory might be, or what dynamical equations on this space might be the right ones. What exists are just some conjectured approximations in some very specific Rube Goldberg-esque constructions (eg KKLT), and even those are very complicated.

3. **Low Math, Meekly Interacting**
   November 25, 2015

   I think even the unsophisticated can get the basic gist out of the simplified expressions of Einstein's field equations one typically sees in popular treatments (or on tee-shirts). I'll never know the full power of the tensor maths embedded therein, but the "matter tells space how to curve, space tells matter how to move" aspect, and the impact of the modern inclusion (as well as Einstein's mistaken one) of lambda has, is appreciable on a superficial level. Not sure if I would be able to form sound judgments if one stuck the universe of Einstein next to a multiverse, expressed in equations. I am wise enough to know how useless the latter is to science unless it makes some testable predictions, which it hasn't, and very likely won't, ever. Juxtapose that fact with the successes of GR, and I should hope the depressing reality multiverse mania would be obvious to any interested layperson. What's sad is the lack of interest many in the media show in being hard-nosed about the facts, which are simply GR is a spectacularly successful theory, and the anthropic multiverse is utterly bereft of value until it makes even one testable prediction that isn't utterly trivial or tautological. Which it hasn't. And likely never will.

4. **cedric bardot**
   November 25, 2015

   In the words of Einstein :
   "Nobody who really grasped [the general theory of relativity] can escape from its charm, because it signifies a real triumph of the general differential calculus as founded by GAUSS, RIEMANN, CHRISTOFFEL, RICCI AND LEVI-CIVITA"
   "On the General Theory of Relativity" (submitted 4 November 1915, published 11 November 1915) from http://einsteinpapers.press.princeton.edu/vol6-trans/110.

5. **Emil Martinec**
   November 25, 2015

   `If I were to grind my own ax here, it would be to note that Einstein's great breakthrough came about through close collaboration with some of the best pure mathematicians around, adopting difficult but deep ideas about geometry. Without the mathematicians, I'd guess that the theory of general relativity would have taken many more decades to come to fruition. Maybe there's a lesson there…'

   Funny how it is that some of the best pure mathematicians around HAVE BEEN and ARE collaborating with physicists about deep and difficult ideas about geometry, topology, etc. They work on mathematics related to string theory (Kontsevich, Yau, Morrison, Donagi, Morgan, Freed, Donaldson,…).

The deepest ideas in mathematics and the deepest ideas in theoretical physics have been intertwined for centuries (Newtonian dynamics/calculus, Maxwell theory/fiber bundles, Einstein gravity/Riemannian geometry,…).

String theory has been the source of some rather deep results in mathematics over recent decades (monstrous moonshine, elliptic genera, homological mirror symmetry, Seiberg-Witten theory, …). The flow of ideas in both directions has been stimulating for both fields.

The cognitive dissonance of your love of mathematics and your antipathy toward string theory is simply breathtaking … and telling. Maybe there's a lesson there…

6. **zzz**
November 25, 2015

Nature has a story this week about his collaborations

http://www.nature.com/news/history-einstein-was-no-lone-genius-1.18793

7. **Shantanu**
November 25, 2015

Peter, I thought Matt is a tenured prof at Rutgers.
shantanu

8. **Peter Woit**
November 25, 2015

Emil,
My criticisms of string theory have never been about its effects on mathematics. That's an extremely interesting subject, but one of great complexity, beginning with the fact that "string theory" is now a pretty ill-defined term. Eliminating actual content and complexity by claiming all sorts of very different things as "related to string theory" is just a tedious ideological debating point.

If you're interested in what I think, and not in a dumb string theory rules/sucks argument, you could start by noticing that my complaints have always been about the effect of "string theory" on physics, not on mathematics. You write about collaboration between mathematicians and physicists on "deep and difficult ideas about geometry, topology, etc.", which is quite accurate. What you don't mention is such collaboration on deep and difficult ideas about physics. The string theory unification picture of the world has degenerated into a hideous pseudo-scientific story about the "Landscape" (do you think there is deep new mathematics there?) and wishful thinking that the LHC will see some evidence for SUSY (any collaboration with great mathematicians on making LHC predictions about properties of superpartners?).

There's a wide array of wonderful deep mathematical ideas behind fundamental physics and lots of very good mathematicians that would like to think about them. If physicists would admit that string theory unification has been a failure and abandon empty, dead ideas, there would be even more opportunities for new interesting overlaps of the two subjects.

9. **Peter Woit**
November 25, 2015

Shantanu,
Strassler resigned his position at Rutgers a couple years back.

10. **Jeffrey M**
November 25, 2015

Peter,

Did Strassler explain why he resigned? I would think Rutgers was a very good position, low teaching load, don't know how good the department is in physics (it's very good in math) but you are certainly near plenty of great people in Princeton and the city. Was he just sick of physics? Not if he took a visiting job at Harvard, I wouldn't think. Did he think a few years at Harvard would get him a better gig?

11. **Peter Woit**
November 25, 2015

Jeffrey M,
I have no idea why Strassler left Rutgers (which has a very good physics department), you'd have to ask him. He has continued to be active in physics research, no evidence this was because he'd lost interest in that.

12. **martibal**
November 25, 2015

Emil, there are also great mathematicians working at the interplay between geometry and physics, and not interested that much in string theory, e.g. Connes. There might also be a lesson here.

13. **Peter Woit**
November 25, 2015

martibal,
On the topic of Connes and string theory, it may not be 100 years, but 10 years ago I posted this
http://www.math.columbia.edu/~woit/wordpress/?p=313
The story about string theory and Chicago is rather funny, perhaps Emil knows who it refers to.

All, actually there was nothing at all about string theory in this posting. I'd like to encourage discussion of any other topic than that.

14. **ronab**
November 25, 2015

Peter,
In providing context for his failed grant applications, Strassler says: "U.S. government cuts to theoretical high-energy physics groups have been 25% to 50% in the last

couple of years”. Is this correct? I had no idea.

15. **Chris W.**
November 25, 2015

Einstein sought guidance from good mathematicians on how to mathematically formulate his ideas about how to solve certain problems in physics. Again, the initial ideas were *about physics*, and they were his ideas, with some inspiration from people like Ernst Mach.

S. S. Chern saw this clearly, and discussed it in a an edited collection of symposium talks published following the Einstein centennial year of 1979. (See pages 271-287.)

As of now we lack physical ideas of comparable fruitfulness in confronting the current problems in fundamental physics. We have no lack of mathematical ideas, but we don’t know how to select which ones we need, or clearly identify the kind of new mathematics that might be required. (I’m inclined to believe that the problem is primarily one of selection and reformulation; the array of available mathematical ideas is already vast.)

16. **Justin**
November 25, 2015

Woit,

Could you elaborate on Einstein working with great mathematicians? I’m not an historian on the subject, but I believe Einstein worked a little bit with Marcel Grossmann to understand some Riemannian geometry, but that except for this collaboration, he developed his theory quite independently.

17. **Chris W.**
November 25, 2015

PS: Chern’s essay (originally a talk) may have been republished elsewhere as well, perhaps in a volume of his collected papers.

18. **Peter Woit**
November 25, 2015

ronab,
That’s somewhat of an exaggeration. The DOE HEP theory budget numbers recently are
FY2013 66.3 million
FY2014 64.3 million
FY 2015 59.2 million
FY 2016 60.3 million (proposed)
Given inflation, and some of this an increase in Lattice QCD, there definitely has been a decision to decrease the amounts going to theory groups. Because of the details of how these grants work, some of the decreases have been large. For some discussion of the subject see
http://www.math.columbia.edu/~woit/wordpress/?p=6701
and this
http://www.usparticlephysics.org/sites/default/files/webform/p5/calamity.pdf

Justin,
Grossman was crucial in introducing him to modern geometry and the techniques he needed to formulate the theory. Around the time he announced the field equations, Hilbert found the variational form of GR, and other very well-known mathematicians (for example Noether and Weyl) made contributions to the understanding of the theory.

19. **D R Lunsford**
November 26, 2015

It seems a little inaccurate to mention Weyl as an afterthought or to put him alongside Noether, who I am not dissing here – it’s just that Weyl was one of three people who really got it right off the bat – with Eddington and Einstein. Noether’s theorem did not play a role in early geometrical ideas about gravitation and was not published until 1918. It was Weyl who first understood the physical problem of the GR action and conservation laws stemming from it, which work also came in 1918. I am pretty sure that Noether and Weyl did not meet until Weyl was at Goettingen, about 10 years later. So her work likely had no influence on Weyl when he was working on his generalization of Riemannian geometry. (Weyl of course had found special cases of Noether’s theorem. Nevertheless his intent was physical and he wondered if conservation laws could be found at all. I could be wrong. See “The Dawning of Gauge Theory” by O’Raifeartaigh. And certainly, he was greatly influenced by her in his later work on electron theory.)

-drl

20. **nicola**
November 26, 2015

Do these “very specific equations” also change from point to point by some yet another set of “very very specific equations”? . Maybe there is even more general set of “very very very specific equations” that govern the “very very specific equations”? Maybe there is an infinite sequence of equations like that? And then there may be equations that govern the whole infinite sequence.

That somebody takes ideas like that seriously is beyond me.

21. **chris**
November 27, 2015

Einstein did his greatest work when he had solid experimental (or observational) evidence and followed it to the end. Yes, GR is a triumph in its mathematical sophistication, but he had the Perihelion precession of Mercury as a guidance.

In later years Einstein followed beautiful mathematical ideas and deep connections – you all know what the outcome was.

22. **geoff**
November 27, 2015

I would hope that those who know the particulars of this history will chime in and correct or expand as needed, but I think part of the physical argument arose in the context of whether or not the energy momentum tensor was a true tensor versus pseudo tensor. Also, I believe Einstein spoke of the equations for gravitational waves as being nonlinear several years before formulating the field equations themselves, and it was the task of separating gauge degrees of freedom from physical ones that led Hilbert to enlist Noether’s help. (“With Einstein’s theory, one of the many paradoxical consequences of this failure of energy conservation was that an object could speed up as it lost energy by emitting gravity waves, whereas clearly it should slow down.)

http://arstechnica.com/science/2015/05/the-female-mathematician-who-changed-the-course-of-physics-but-couldnt-get-a-job/

I've never been sure as to whether or not Mach's principle is a philosophical argument, a physical argument, or an attempt to impose boundary conditions that was never really fulfilled. Also, I'll second the motion that Weyl is grossly under appreciated by physicists today.

23. **Dimitrelis**
November 27, 2015

@Chris:
Exactly, and just to add that before GR, the anomalous perihelion of Mercury was attributed to … unseen matter- the planet Vulcan.
But I guess unseen (or dark) matter was always a very convenient solution…

@Peter:
I think Einstein has no grave 🙂

24. **David Brown**
November 27, 2015

"… Einstein spinning in his grave …" Einstein does not have a grave. His body was cremated and his brain was removed at autopsy and preserved in formalin.
https://en.wikipedia.org/wiki/Albert_Einstein%27s_brain

25. **Emil Martinec**
November 27, 2015

Peter,

I wouldn't say that string theory (removing the scare quotes) is an ill-defined term; rather that it is a somewhat inapt designation for a circle of ideas about fundamental physics, given that the primacy of strings is now understood as an artifact of perturbation theory. And the fact that all sorts of very different things (from finite simple groups to K-theory, to name but two examples) are related to string theory is not so much a 'tedious ideological debating point' as it is an indication that this circle of ideas has the depth and richness that one would hope for in a fundamental theory.

If your notion of deep and difficult ideas about physics is restricted to a narrow set of issues in particle physics, then no, mathematicians are not who one should run to for help. But fundamental physics is so much more than that. Einstein taught us the deep interconnection between geometry and gravitation, a notion which has been extended by the central role of gauge theory in the other fundamental forces. A useful role for mathematicians in this enterprise has been and remains to help develop the geometrical/topological/algebraic ideas that might be of use in constructing a physical theory. Sometimes those ideas are already at hand when the time is ripe for an advance on the physics side (e.g. Riemannian geometry was available to Einstein), sometimes mathematics is the beneficiary of developments in physics (e.g. quantum mechanics leading to developments in algebra and representation theory). String theory has been both beneficiary and benefactor of mathematics, for example K-theory was available to help understand brane dynamics, while properties of string worldsheet dynamics have led to developments in algebraic geometry such as mirror symmetry and quantum cohomology.

As for the so-called landscape, I regard the whole discussion as premature. A certain segment of the community is piling speculation upon speculation, and not really getting anywhere. It is useful to recognize that there is always going to be an issue with quantum cosmology — that the universe we inhabit had substantial quantum fluctuations early on, and we are now in one decohered branch of the wavefunction; and so what to make of all the other branches out there, and the question of how many are there. To make progress on a problem, one needs the tools to do so. Most of the analysis of the landscape is based on an analysis of effective field theory and the seeming existence of a plethora of solutions to the effective field equations. People spun their wheels for twenty years trying to solve the Hawking paradox using the same methods, without success.

We now understand that black holes are consistent with quantum mechanics in specific examples in string theory, yet we cannot point to the dynamical process which supersedes Hawking's analysis of horizon dynamics in effective field theory. I suspect that making progress on cosmology (i.e. global dynamics) will have to await a better understanding of such local dynamical questions. One of the least understood aspects of string theory is dynamics.

As for your desire for physicists to "admit that string theory unification has been a failure and abandon empty, dead ideas", good luck with that. People actually working in the field will pursue the directions they deem most promising, regardless of your little diatribes here.

26. **martibal**
November 27, 2015

Peter: thanks to the link to your previous posting. The quote of Connes is still pretty accurate ! Regarding the french CNRS that offers stable job at young people, there would be things to say (in my opinion, it has turned now into a "there is only a short interval of time after the PhD in which one has a chance to get a stable job, at PhD+ 5 one is already out", which has many perverse effects), but that is another story 🙂

27. **Peter Woit**
November 27, 2015

Emil,
One thing that strikes me is that you decided to comment here in response to an item I wrote, completely ignoring what that item was about. I'm curious: do you really think it's all right for physicists to go on PBS and tell the public that the multiverse is "Einstein's legacy" and that there are "very specific equations" describing it? If it isn't, who should be saying something about this? If not me and my "little diatribes" (that's what this one was about, not string theory), who is or should be taking on this job?

The argument for string theory that, while we don't know what it is (other than a "circle of ideas") various different kinds of mathematics have turned out to be useful to analyze some of the complicated structures that have turned up, isn't a very convincing one. Yes, this circle of ideas that grew out of string theory has led people to wander around in some mathematically rich areas, raising questions and finding interesting things that would not otherwise have turned up. That's great, and a perfectly good argument for pursuing this circle of ideas if you can't think of anything else to do. It's not an argument though for the 10d superstring (or related circle of ideas) as having anything to do with fundamental physics.

Back in 1985, at the beginning of your (and my, we were educated in exactly the same era) career, the idea that something like the heterotic string could unify physics was a reasonable one to try, but it's now thirty years later. The idea that you could predict something, anything, this way has collapsed, giving us multiverse pseudo-science. The idea that higher energy colliders would find SUSY, giving experimental clues pointing towards this scenario, has also pretty much collapsed, with the final part of that story to happen over the next year or two. Given this, I think there's a very strong argument that you and others need to face facts as scientists, admit failure, and move on.

You can dismiss this as a “little diatribe”, dismiss any evaluation of the current state of string unification efforts as “premature” (with the right time to consider admitting failure only when you’re no longer around) and try and prop up a failed idea by hyped connections to mathematics, but I don’t think you’re doing the subject any favors this way.

28. **Anonymous**
November 27, 2015

Ronab, Peter,

While the total DOE high-energy physics budget hasn’t gone down by much percentage-wise, the cuts haven’t been distributed equally. Some large collaborations at certain universities received much larger percentage cuts than this average (I don’t know of any place that received anywhere near a 50% cut, but possibly Strassler does.)

29. **Emil Martinec**
November 27, 2015

I was reacting to the last paragraph of your original post (which I quoted in my initial response). So my response was a comment about the interaction of mathematics and physics, and that some of the most fruitful interactions of this sort in recent times have arisen in the context of string theory. I don’t think that wholly inappropriate, given the general content of this blog.

You propose that “general relativity would have taken many more decades to come to fruition” if the appropriate mathematics weren’t already developed. Yet you seem remarkably impatient with string theory not being in final form thirty years on from its inception as a candidate for a fundamental theory, given that there is no ready-made theory of quantum geometry for us to read up on. (BTW, I would rather have said we are forty years on from the work of Scherk-Schwarz/Yoneya showing that string theory is a quantum theory of gravity.)

As for contact with experiment, indeed that may be hard to come by given the large disparity between the electroweak scale and the Planck scale. If the scale of extra dimensions and the string tension scale are close to the Planck scale, then string theory looks remarkably like a 4d theory of quantum gravity coupled to matter — ordinary particle physics up to near the Planck scale, a handful of odd resonances that we’ll never see, and then a spectrum of black hole states. But absence of evidence is not evidence of absence. String theory could easily be correct and not testable by humans — in fact that could well be true of any theory of quantum gravity given the remoteness of the Planck scale.

In such a situation, internal consistency is one of the few routes to progress. Einstein was initially motivated to resolve the apparent inconsistency between Newtonian gravity and special relativity. Today, we have the apparent inconsistency between locality, causality, and quantum unitarity manifested in the black hole information paradox. String theory has made undeniable progress in this direction, but not a complete resolution.

As for particle physics unification, while it would be nice for there to be some rigid structure and a limited possibility to arrive at anything other than the Standard Model at low energies, it has so far not emerged, and looks increasingly unlikely. And the number of clues we are likely to get from experiment seems increasingly limited. The situation is unfortunate, but again nothing here says string theory is wrong (rather than simply not useful in this particular context).

Which brings us back to the issue of multiverse speculation. It seems to me a legitimate scientific question whether the structure and parameters of the standard model of particle physics and cosmology are (1) predetermined, or (2) properties of the part of the state we have access to, and therefore environmental. I suppose (1) splits into subcases of being (a) calculable in principle, or (b) fixed metadata. Maybe we will know enough someday to say definitively one way or the other. What questions are worth pursuing differ depending on the answer. Again, this is an issue for any theory of quantum gravity, not just string theory.

At the moment, I think we are in no position to answer, and trumpeting one particular possibility in the media is not particularly helpful, and tarnishes the subject with a patina of unseriousness. We simply don’t know enough to say one way or the other. I cringe when I hear statements to the effect that a multiverse is a “prediction” of string theory. At best, it’s a possibility.

Such speculations are putting the cart way before the horse. Let’s get a quantum theory of gravity first, and learn how to calculate with it; only then might we hope to address such questions. I suppose it’s human nature to want to jump to the final answer, and fill in the details later. But what if the nature of the final answer depends crucially on the details?

I suppose you’ll dismiss this as some dodge that it’s premature to judge string theory (again the scare quotes). Your mind was made up long ago and I’m sure I won’t change it. I will simply conclude by stating that I still think the subject is making interesting progress on important questions; they’re just not the questions we were initially asking 30 (or 40) years ago about unification of particles and forces, and instead have more to do with the nature of quantum gravity.

30. **Peter Woit**
November 27, 2015

Emil,
So, it seems that your position is that string theory is unlikely to ever be testable by humans, but that showing it is an internally consistent untestable theory is the future of the subject. Since you don’t have an actual theory (what is M-theory again? everyone seems to have given up on even trying to answer that), what you’re actually talking about is just showing that a “circle of ideas” is consistent, while being completely empty of content. This is really nothing but a set of excuses for giving up on doing science. It’s not surprising that people doing this don’t clearly explain to the public the state of their subject, but instead produce such huge amounts of promotional hype.

31. **Emil Martinec**
November 28, 2015

My how you love to twist people’s words! I said that for ANY theory of quantum gravity that, because of the remoteness of the Planck scale, experimental or observational tests will be hard to come by, and it is a distinct possibility that such tests will be beyond our reach. Does that mean we should give up trying to reconcile the fundamental incompatibility of quantum theory and general relativity, two theories that we have abundant evidence for? Some (you, apparently) might say yes, others (me, for sure) continue to be intrigued. Having at least one example of a consistent theory that combines the two is still a useful exercise IMO. At the moment we don’t have any.

I wouldn’t say that people have given up on the question of what M-theory is, rather the focus has moved beyond the aspects of that question easiest to answer using existing tools such as perturbation theory around simple backgrounds, and effective field theory.

One outstanding issue is what becomes of the equivalence principle in a quantum theory of gravity. Accelerated frames are related to thermal effects in the quantum theory, and related notions of density matrices and quantum entanglement; so these are likely to be some of the ingredients, and they also tie into the information paradox. A conceptual framework that ties all these notions together is currently lacking, but is certainly a major topic of current research. I could give other examples.

The first-principles exact calculation of black hole entropy, absorption/emission amplitudes, and so on, in particular controlled examples in string/M theory, was a tour de force of theoretical physics. The “circle of ideas” (oh those scare quotes again) in which those calculations took place is hardly devoid of content, or unscientific.

32. **Peter Woit**
November 28, 2015

Emil,
I don't think I'm twisting your words to say that you have given up on explaining anything about observable physics, by giving up on unification and any of the open questions of the standard model. Worse than that, string theorists have constructed a scenario with no evidence for it, designed to avoid admitting failure, and are aggressively selling this in the media, to students and colleagues, ensuring that future generations won't try and attack these problems, since it is "well-known" that they can't be solved.

It is possible to pursue a question purely by the criterion of internal consistency, with no checks from the real world. Mathematicians do it all the time. But if you're going to do this, the lesson mathematicians all know is that you need to be quite precise about what you are doing, very clear in your arguments, and very honest with yourself when something doesn't work. As far as I can tell, the way quantum gravity is being pursued by "string theorists" (I think the scare quotes are important, at this point, any actual theory involving strings seems to have little to do with this) involves none of this discipline at all. It's pretty clear where this kind of activity ends up, and its not with any reliable understanding of anything.

33. **random reader**
November 28, 2015

The current Martinec/Woit discussion is one of the most incisive I have seen on this blog, and is getting to some core issues that previous string theorist commenters have denied or ignored (or evaded). Thanks to Prof Martinec for the candid and to-the-point remarks; I hope he continues the conversation rather than exiting at the crucial point as Distler, Motl, Polchinski et al have done in the past.

34. **Neil**
November 28, 2015

Yes, this exchange between Peter and Emil was tremendous, and I would love to read more.

But at the end of the day, I can't help thinking it can be reduced to Peter saying "Show me the money" and Emil arguing about what is meant by "show" and "money".

35. **Emil Martinec**
November 29, 2015

@random reader,
I'm not sure what you mean by "the crucial point"; the major points have largely been made above. If I discontinue, it's because at this juncture there is not much more to say without repeating myself.

@Peter,
As I have said, I am not a fan of the landscape/anthropics, precisely because it implies that much if not all of the structure/parameters of particle physics/cosmology are environmental and not fundamentally determined and so the calculational power of the most fundamental level of physics would be rather limited.

But just because I don't like it doesn't mean it's not a legitimate scientific issue. IF the theory of quantum gravity plus gauge theory and matter has a sufficiently complicated structure of metastable states and IF transitions among them are allowed and IF one can understand enough about how early universe cosmology works, then the part of the universe we inhabit is the outcome of a variety of quantum processes, and the post-big bang universe we experience is but one of many discrete possibilities. If a quantum theory of gravity and matter can be constructed which has these properties, it is worth taking seriously and trying to understand better.

However, not all the string theory community agrees with all the premises. For a rather thoughtful technical critique of the assumptions (which I largely agree with), see section 4 of Tom Banks' 2010 TASI lectures (arXiv:1007.4001). The landscape is not currently a settled consequence of string theory.

As for all the "media hype" (my scare quotes this time), at the risk of repeating myself, I think it's unfortunate, but I don't think there's much to be done. It is not my job (or any of my colleagues) to police the field. There's plenty of bad popularization of science. Maybe journalists should do a better job of researching their stories. Like any science idea, either it will have power and find support when we understand quantum gravity better or it will not.

Every problem has a time when it is ripe to be solved, and often premature attempts to solve it don't help. It's always a judgment call where to invest one's precious time for research; even unsuccessful attempts often bring up new ideas or move the field forward incrementally. But at some point people keep rehashing the same old ideas, and at that point it is prudent to wait until new tools arrive. I think the black hole evaporation problem is a quintessential example. There were suggestions that black hole radiance violates quantum mechanics; that the information is stored in microscopic remnants; that it disappears into a baby universe; and on and on. The problem wasn't ready to be solved until gauge/gravity duality came along, we understood much better what quantum gravity is about, and we could see that all of these suggestions were on the wrong track (at least in the examples where quantum gravity has a presentation as an ordinary quantum field theory).

36. **Curious Wavefunction**
November 29, 2015

I look forward to Dorigo's book. What do you think of Randall's book which was favorably reviewed in the NYT today?

37. **vmarko**
November 29, 2015

Emil,

"IF the theory of quantum gravity plus gauge theory and matter has a sufficiently complicated structure of metastable states and IF transitions among them are allowed and IF one can understand enough about how early universe cosmology works, then the part of the universe we inhabit is the outcome of a variety of quantum processes, and the post-big bang universe we experience is but one of many discrete possibilities."

All three IF-s that you name are theory-dependent, and experimentally very hard (if not impossible even in principle) to verify. It is one thing to study a theory which has all three properties, but it is quite another to teach students that there is no alternative.

It would only be fair to acknowledge the opposite point as well: IF the theory of quantum gravity plus gauge theory and matter has a sufficiently simple structure of metastable states, or IF transitions among them are not allowed, or IF one can understand early universe cosmology without inflaton fields, then the physics describing our local patch of the universe also describes the totality of it, and the post-big bang universe we experience is pretty much unique. The values of fundamental coupling constants then ask for an explanation, and it is quite legitimate to try to improve the theory by trying to calculate some of them from some first principles, as opposed to claiming that those values are an environmental accident.

And of course, yes, there are theories out there that satisfy either of those IF-s, and even those that actually attempt to reduce the number of fundamental coupling constants (say, the NCSM by Connes, Chamseddine et al. at least tries to improve/reduce the number of SM free parameters).

The actual problem is that landscape/anthropics is an ideology, and the fact that it is represented to younger scientists as the only possibility, with students in most major universities being actively discouraged from thinking about alternatives (no PhD advisors, no funding, etc). I've heard the sentence "string theory is the only way to quantize gravity" way too many times, on serious conferences, from leading scientists in the field. Even if we accept landscape/anthropics as a scientific possibility (for the sake of the argument), it is by no means the only one, or even the most plausible one. People involved need to be more honest and more humble about what is speculative and what is done-and-dusted, when communicating science to students and outsiders. This lack of this honesty is where the problem is.

Best, 🙂
Marko

38. **Peter Woit**
November 29, 2015

Curious Wavefunction,
I took a quick look at it in the bookstore, which convinced me that its content is pretty much orthogonal to my own knowledge and interests. I've never learned much about dinosaurs, the Oort cloud, relevant astrophysics of our galaxy, and for whatever reason, they're just not high on my list of things I'd like to spend time learning more about. So, for an informed take on that book, you'll have to look elsewhere.

39. **Peter Woit**
November 29, 2015

random reader,
I agree with Emil that such discussions as this tend to end when people feel they have nothing new to say. I'm approaching that point and I'm sure he is too. I doubt either of us will convince the other of much, but the usefulness of this is likely to be that of giving a clear expression of differing points of view on these topics, I hope more informative than the usual caricaturizations. I think the point of view he is explaining is a rather mainstream one among sensible string theorists, many thanks to him for providing it.

40. **Peter Woit**
November 29, 2015

Emil,

The common reference to "not liking" the landscape scenario I think completely misses the point. If string theory implied that string vacua almost always came with a low energy SU(3)xSU(2)xU(1) gauge symmetry and three generations of matter particles, but the Yukawas were all different in some exponentially large set of different vacua, then I'd say that it's a scientific theory that has to be taken very seriously even though I didn't like the implications. The problem though is that there is zero significant evidence for such a scenario. What's dangerous here is not that people are pushing an idea that goes against traditional optimism about what we can hope to explain, but that they are pushing as science an idea with no scientific evidence behind it (and with a rather obvious non-scientific motivation of wanting to avoid admitting failure of a cherished idea).

I don't think media hype for this or other dubious ideas can be blamed on journalists. In my experience they of course miss subtleties and complexities, but they do a pretty good job of giving an account of the story they're hearing from well-credentialed physicists. Quite a few prominent and distinguished theorists have decided it's a good idea to publicly promote the anthropic landscape (sometimes claiming they "don't like" the idea). That very few of their colleagues are willing to publicly disagree with this is a big part of the problem.

Time will tell whether the latest gauge/gravity inspired approaches to the black hole information paradox lead anywhere. I don't think any evidence for string/M-theory is going to show up this way, but it's not implausible such work will lead to more promising ideas about quantum gravity (as a fan of representation theory, the appearance of an interesting group in what Strominger and other have been up to looks to me intriguing), or maybe have significant spinoffs in other very different fields (condensed matter, quantum information). But still, I see a two-fold problem, the usual faddish concentration on one particular idea, together with really bad arguments that others are unlikely to lead anywhere. Way back when, I think the point of view of mainstream theorists on the quantum gravity research community was often that such research wasn't going anywhere, since it was divorced from the parts of fundamental physics that we had evidence for (with the argument for string theory that it captured both the SM and quantum gravity). It might be a good idea for present day string theorists to recall their previous point of view.

41. **random reader**
November 30, 2015

The "crucial" new elements of this discussion compared to others seem to me to include the following contributions from Prof. Martinec.

1. The clear statement that "At the moment we don't have any [example of a consistent theory that combines quantum theory and general relativity]." This is very different from the usual idea that the formal consistency checks on string theory are overwhelming and have essentially decided the issue, and of course also different from the idea that any consistent unification has to closely resemble string/M theory or be essentially equivalent to it.

2. The comment that "If the scale of extra dimensions and the string tension scale are close to the Planck scale, then string theory looks remarkably like a 4d theory of quantum gravity coupled to matter — ordinary particle physics up to near the Planck scale, a handful of odd resonances that we'll never see, and then a spectrum of black hole states. ". This is more specific than the usual "Planck scale remote implies quantum gravity hard to observe" and raises a couple of interesting possibilities. One, that there will not be a lot more interaction between string theory and 4d quantum gravity questions beyond the picture of stringy microstates for (some) black holes. Another, that there may be a more general and robust class of theories, perhaps falling short of a full theory of quantum gravity, that generate the Standard Model + "handful of odd resonances" + "spectrum of black hole states". In that case the stringy calculations are a particular and somewhat elaborate realization of a more general pattern that one might as well find and study on its own.

42. **Emil Martinec**
December 1, 2015

random reader,
Let me clarify on your point (1), as you seem to be eager to extract a takeaway message somewhat at odds with the point of view I was intending to convey. IMO the formal consistency checks on string theory ARE overwhelming evidence that there is a theory combining quantum mechanics and gravity, whose solutions include the ones we have found through string perturbation theory and effective field theory — at least the ones where additional structure such as supersymmetry guarantees the existence of such vacua. Which is not to say that non-supersymmetric vacua do not exist; we can simply say less about them, because our calculational tools are more limited.

Nevertheless, being convinced of the existence of such a theory and having it in hand are two different things. We are in a rather unprecedented situation that the part of the theory we know consists of a set of methods to construct a collection of examples of solutions, around which we can describe perturbation theory in terms of strings and branes. The perturbative limits, together with a set of duality conjectures for which there is strong evidence, piece together a complete picture of the solution space (again in examples with enough supersymmetry) which is conjectural but in the best sense — having myriad checks, eg involving elaborate mathematical identities that one conjectures based on the duality hypothesis and then verifies by independent means. But we don't have in hand the organizing principle, an underlying idea such as the equivalence principle from which all these results flow as consequences, and which would enable us to formulate the theory beyond these various limits and especially in non-supersymmetric and time-dependent backgrounds.

As to whether this edifice is unique and inevitable, I am an optimist that time will tell, and that the answer is yes. For now let me mention the example of N=8 supergravity in 3+1 dimensions, whose perturbation theory was the subject of a recent discussion thread. There is no limit of string theory that decouples all the "extra stuff" of string theory — the branes and extra dimensions — and leaves only the 3+1 dimensional quantum field theory of N=8 supergravity. So that's great, you might say, maybe there's a theory without all this extra baggage, that doesn't involve strings and is perfectly consistent on its own. Here is where one needs to think carefully about the spectrum. N=8 supergravity has 28 gauge fields. There are black hole solutions in supergravity carrying electric and/or magnetic charges sourcing these gauge fields. The extremal limits of these solutions are in fact the objects we tried to throw away — the strings, branes, and Kaluza-Klein modes — moving within or wrapped around the extra dimensions; or bound states thereof. We discover this by considering the spectrum as a function of the 70 moduli of N=8 supergravity; in asymptotic limits of that moduli space, the black holes become perturbative objects in string theory. So if there is a consistent theory with only N=8 supergravity in 3+1d without all the extra structure of string theory, it has to come with a reason why ALL the charged black hole solutions, which seem perfectly benign and not all that different from the uncharged black hole solutions, in fact are excluded from the spectrum of the theory. If they are not, then we are led back to the full structure of string theory compactified on a torus. One could go further and think about why these objects indeed cannot be excluded, because they will be pair created in external fields with some small but finite probability, etc, but anyway perhaps enough said.

As for your point (2), indeed the stringy structure in the sort of situation I described naively seems confined to understanding the black hole spectrum, but that is by no means assured. It's the regime that's hardest to analyze, because there are no small dimensionless parameters in which to do perturbation theory. Because black holes teach us that local field theory breaks down at some level, there may be subtle nonlocal effects that influence low-energy physics in ways we do not currently understand and should be on the lookout for (but that's a hope, not an expectation).

I share the sentiment that current string technology is a bit of a Rube Goldberg device when it comes to explaining black hole structure, each example having different particulars but always yielding the same Bekenstein-Hawking area law in the end. This is precisely why people are searching for a more universal explanation. That doesn't mean strings and branes and extra dimensions will disappear from the final formulation, see the discussion of N=8 supergravity above. They will still be present in the spectrum of excitations in solutions that do have sufficiently large extra dimensions, or weak string coupling, etc. Peter is often complaining that people seem to have stopped doing string theory. I don't think it's true, rather I would say people are trying to absorb and abstract the lessons that string theory has taught us in the known examples, into some basic organizing principles from which the rest follows.

43. **Peter Woit**
December 1, 2015

"Peter is often complaining that people seem to have stopped doing string theory. "

I often note that string theorists have stopped doing string theory, but that's an observation not a complaint (I'm glad to see that they're doing something more promising). To the extent there's a complaint, it's that such people are often still insisting on centrality of the original speculation about strings that they started with, long after it has become clear that didn't work out well, with other ideas now the center of attention. Explicitly or not, there's often an argument being made that "string theory is the source of all good ideas", discouraging work on ideas that don't fit into the line of thinking that led from string theory to currently fashionable topics.

44. **random reader**
December 7, 2015

Thanks again to Prof Martinec for the long replies, which are clarifying and refreshingly detailed compared to most previous online discussions of these matters.

Regarding (1), the strength of consistency checks (versus actual construction of the nonperturbative theory) as evidence for a well-defined theory: the evidence actually looks, from this outsider (mathematician's) perspective, to be mixed, both in its strength and in where that evidence is pointing.

It is already the experience of the past 3 decades that stringy subjects that are mathematized, or have strong overlap with things that can be constructed as mathematics — things like mirror symmetry, geometric Langlands, CFT/TQFT, loop groups, integrable systems, matrix models, Kac-Moody algebras, deformation quantization (etc) — do have a lot of interrelations, and may well be moving toward a unification. Maybe a topological M-theory, or a "mathematicians' fragment of string theory". But this seems very far from string theory as physics relatable, in principle, to observation. Rather, there may be a unification all the mathematical discoveries and coincidences from string theory, but it is likely to be in a generic framework that does not have unique relation to weird critical dimensions like 10 or 26, or big supersymmetry algebras.

Quantization is an example that looks stringy perturbatively (deformation quantization a la Kontsevich) but does not show stringy characteristics non-perturbatively, as far as anyone knows. Mirror symmetry looked amazing, and continues to lead to amazing things, but the physicists' curve counts were wrong starting around genus 10.

None of these situations where the mathematical framework can be constructed, shows any sign of the very special features of the 10, 26 or 11 dimensional string/M (or SUSY) theories. It looks like the critical string theories are a non-generic singular point in "theory space" where several submanifolds intersect, with the multiple overconstrained descriptions leading to the extra relations (dualities). In such a picture, the fact that (at least perturbatively) there is an intersection at all is miraculous and needs to be explained, but it may also be that a generic part of the picture is enough for describing nature, and strings are too special, but we cannot see this until enough math is developed.

The appearance of string- or brane-like structures in N=8 SUGRA or other theories does not seem all that surprising. The scarcity of accessible methods (by the standards of earlier unifications) to unify GR and QFT suggests that the new ingredients in different theories that even come close to success are going to be strongly related to each other, otherwise there would have been more freedom to find the new stuff in the first place. This is true of strings and noncommutative geometry, and I have read claims that loop/Ashtekar quantum gravity models (of which I know nothing) also embed into string theory. At any rate, string theory seems to be the most universal known model, so it is expected that all the present contenders will look as though large parts of them can be built from strings, contain stringy structures, or both. But it can also be that once an idea comes along that explains how to get a mathematically rigorous unification (or whatever unification-like thing is the ultimate output), strings will appear as only a universal solution of a specific constraint not tied to what is needed for unification — a special/sporadic/symmetric point in a much larger universe of consistent usable theories that do not all reduce to strings. Sure, if structures such as extremal black holes have a stringy description then whatever else describes them will also have a strong overlap with the string description. But if the "only game in town" argument is really only the "currently most universal object in the category of unification candidate-theories" argument, then finding a principle describing what the theory is about may also be exactly the thing that eventually renders the theory superfluous.

Of course, strings can be unique and consistent because of some universality phenomenon where some large class of underlying models looks like strings as some parameters or number of degrees of freedom get large. But in that case one could model quantum gravity by those underlying degrees of freedom, too.

In all, the quasi-mathematical "consistency checks and coincidences are unbelievably compelling" seems very strong, but not so much as an argument for string theory. The same evidence can point just as compellingly in one or more other directions that are surely related, but are not necessarily an indication that string/M theory will be the lesson to emerge from the consistent mathematics as it is developed. It seems to me that the evidence for the lesson being something non-stringy is stronger, since strings are not fitting any known pattern of what consistent (mathematical) theories end up looking like, and strings' subtheories that are mathematical have not provided any indication that this historical pattern is wrong.

45. **Peter Woit**
December 7, 2015

random reader,
Many thanks for your very thoughtful and well-informed take on this subject. This is the sort of discussion I'd love to see a lot more of. Unfortunately this is an extremely complex subject, with most mathematicians involved not knowing much about the issues of how the objects under study connect (or don't) to a hoped-for unified theory, and most physicists involved not knowing how the objects connect (or don't) to the rest of mathematics.

One of my original main reasons for skepticism about the proposed 1984 string theory unification proposal was that the direction one needed to go to connect string theory to the real world was kind of orthogonal to the directions in which I could see beautiful and deep connections to the rest of mathematics. In more recent years, this has become a much more complex topic, with different sorts of connections to possible unified theories via connections to 4d qfts coming into play. Looking at this whole subject for clues as to what is fruitful and what isn't, without trying to fit things into the original speculative framework of 10 dimensions and strings, seems to me a very valuable thing to do, although few are equipped to do so at the moment.

Super and Great Colliders

November 30, 2015
Categories: Book Reviews, Experimental HEP News

I've recently finished reading two new books on huge collider projects, which make an interesting contrast.

The first is From the Great Wall to the Great Collider, by Steve Nadis and Shing-Tung Yau. It's a very well-informed and topical book, a bit of a political document, designed to make the case for a Chinese "Great Collider". This is a proposed machine of up to 100km in circumference, that would operate first as an electron-positron collider, designed to be a "Higgs factory", allowing precision study of the Higgs. In a second stage the same tunnel would be used for a proton-proton machine with collision energies up to 100 TeV. This would be designed to explore the energy range above a TeV, in much the same way as the LHC, but with seven times the energy, thus a much higher energy reach. This energy would also allow study of Higgs self-interactions.

The Nadis-Yau book is an unusual document in many ways. Yau is a great geometer, but a main concern of the book is something completely different, the question of how one might construct such a huge physics and engineering project. There is a great deal of information in the book about the history and current state of experimental HEP, but from an unusual angle, that of the many Chinese contributions to the subject. I've read many histories of HEP, but learned a lot of new things from this one, with its very different emphasis.

This is a short, rather than encyclopedic, book, with about 130 pages of text. It functions well in explaining the case for a large new collider to anyone interested, but has a distinct focus on arguments for the proposal to do this in China. The Chinese government and people in coming years will be deciding whether to go ahead with this, and this book is the perfect place for them to read a serious account of what this proposal is and why it deserves to be taken seriously.

The current state of affairs is that an initial conceptual design has been completed recently, which was reported here. This gave rise to some mistaken reports like this one that the Chinese government had given its approval to the project. There's still quite a ways to go before that happens, with a final conceptual design not due until next year, and even if there is a go-ahead, construction only starting in 2020-25.

For a detailed look at the physics to be done by such a collider, see this new review article. I was interested to see (page 32) that the previous description by one of the authors of the current situation as leading to only two possibilities ("natural" SUSY or some such, or the multiverse and the end of hope for explaining things) has been expanded to now include a more interesting third possibility: "correlation between the physics of the deep UV and IR".

Just after finishing the Nadis-Yau book, I got a copy of a new history of the SSC project, Tunnel Visions, by Riordan, Hoddeson and Kolb. This book has been in the works for a long time, with the authors starting to gather material back in the 80s, before the project was cancelled in 1993. I've been hearing about the book for quite a while, glad to see that it has finally appeared.

The cancellation of the SSC had a disastrous effect on the US experimental HEP program, moving the center of research conclusively to CERN and its LHC project. A central concern of any book of this kind has to be the "what went wrong?" question. The conclusions drawn are similar to ones I remember often hearing back then in the wake of the disaster: the SSC was a juicy target for a Congress intent on budget-cutting, easily portrayed as out of control (its budget kept increasing from $3 billion early on, to maybe $12 billion at the end), with little support from non-Texas representatives. In some sense the surprising part of the story is that the project got as far as it did before being terminated by an overwhelming Congressional vote.

One part of the story I had never understood was that as the SSC budget expanded it was coming into direct conflict with the plan to keep funding the other HEP labs (Fermilab, SLAC, Cornell, Brookhaven), and that was part of the story of the politics of this within the scientific community. I also hadn't appreciated the way the challenges of a project of this scale required bringing in companies and other parts of the US military-industrial complex, making it take on some of the aspects of a large defense spending project. A major topic of the book is that of the problematic interaction between this and the standard ways that physicists were used to doing business.

Unlike Nadis-Yau, *Tunnel Visions* is more of an academic book, with notes and references to a huge number of extensive interviews making up a large part of the text. It's not at all an inspirational story, nor is there all that much physics discussed. At the same time, it's the definitive work on a crucial part of the history of high energy physics in the US. One group of people who should definitely be reading it are those planning the Chinese project. Some of the difficulties they will face if they go ahead will be similar: the SSC was an 87km ring, of similar scale to the new proposal. That this almost got off the ground 20 years ago here in the US is a good argument that it is something that could be pulled off in China over the next 20 years if they want to do it.

Based on the fact that the SSC might have worked with more international support, the authors end with the conclusion

> Despite the added difficulty of organizing and managing them, pure-science projects at the multibillion-dollar scale should henceforth be attempted only as international enterprises involving interested nations from the outset as essentially equal partners. Nations that attempt to go it alone on such immense projects are probably doomed to failure like the Superconducting Super Collider.

The Chinese proposal is still in its infancy, but there's reason to expect it might be a "go it alone" project. Given the way the US budget operates, at this point no country is likely to look to the US as a reliable source of sizable funding. CERN has its own proposal for a 100 TeV collider, but it seems hard to believe that both projects will go ahead, although also hard to see the Europeans agree to give up energy frontier physics to China. Many of the lessons of the SSC funding debacle are rather specific to the US and the way budgets are done here. I have no idea what the considerations in China are for projects like this, I guess we'll start to find out in coming years.

Comments

1. **srp**
   November 30, 2015

   The SSC was a classic "Plan B" approach, as described by Freeman Dyson, that overwhelmed the ecology of the field (e.g. destroying funding for the rest of the accelerator community). It is also one of the few instances I know of where the sunk cost fallacy was not allowed to operate in the public sector ("we've already spent X on it, we can't give up now [even though the budget keeps exploding and the completion horizon keeps receding]).

2. **Lamont**
   November 30, 2015

   As an incredibly naive first-order guess, doesn't it seem fairly likely that China could pull it off as a "go it alone" project? All they'd have to do is sell themselves the idea that it would be a point of national pride to do what America was not capable of doing…

3. **Thomas**
   November 30, 2015

I'm glad to see that the Riordan et al. book is out. I'm very interested in the story of the SSC. I feel that the usual story that the SSC was killed by shortsighted politicians is greatly oversimplified. It was clearly a mistake to try a giant leap forward at an entirely new laboratory while there was a healthy program going at Fermilab (and SLAC).

It also not clear to me whether the SSC really would have worked as advertised (this is obviously impossible to know, and I am certainly not an expert). The LHC was a natural evolution of earlier machines, and had significant teething problems. Also, in accounts of the Higgs discovery it is always emphasized how important modern detector technology and computing was in making this possible. I'm not sure that the Higgs could have been discovered almost 20 years ago.

4. **TS**
December 1, 2015

In terms of scale, ITER is probably the project to compare to. It had its share of organisational trouble, many of which could be traced to a structure where ITER itself has (had?) no funds, but for all I can tell, it seems to be moving forward steadily, in spite of cost overruns, delays and all the usual troubles.

Yet, the comparison falls short in a very important way: ITER is applied science insofar as its aim is — to phrase it boldly — to replace a multi-billion dollar industry, namely that of electricity production. So the motivations for it and the case to be made for it are entirely different. There is also the question whether ITER really is the only game in town, like the chinese accelerator (or the FCC) would be. Wendelstein 7X is supposed to see plasma in 2015.

5. **Matt**
December 1, 2015

Anyone who followed the news or has seen the pictures of impenetrable smog in Beijing from the last few days probably realizes that China's least problem is the construction of a new multi-billion dollar particle collider. With an economy in severe trouble and an environment at the brink of collapse I strongly doubt if Chinese politics will ponder about paying for a new toy for particle physicists. Sometimes I feel like the HEP community is like the band aboard the Titanic, keep fiddling away while the ship is sinking. But hey, who knows?

6. **jd**
December 1, 2015

The most recent Science magazine (end of November) indicates that the timeline for ITER will slip 6 years with a resulting 2B increase in cost. Best not to compare any future accelerator project to fusion.

7. **iHEP**
December 1, 2015

Matt: A lot of Chinese strategy is based on looking good in front of the world and "catching up" with the US. China also has some fine scientists. Throw in a quasi-dictatorial milieu and I am not surprised at all that they would pick particle accelerators over environmental pollution mitigation.

8. **Dave Miller in Sacramento**
December 1, 2015

Peter,

I have a fondness for e+/e- colliders, having been involved in some of the early work on the PEP machine at SLAC (I wrote part of the software to model the drift chambers, before I went into theory): this seems to me the cleanest way to study details of the Higgs, and wringing out all the details of the Higgs seems to me the logical next step experimentally.

Do you agree? And, can you tell us the cost estimate for the first-stage (e+/e- collisions) of the "Big Collider"?

On the Riordan book: do they go into details about the physicists who managed the SSC project? I knew the two top guys, but was never clear whether they were part of the problem or just in the wrong place at the wrong time.

You wrote:
>One part of the story I had never understood was that as the SSC budget expanded it was coming into direct conflict with the plan to keep funding the other HEP labs (Fermilab, SLAC, Cornell, Brookhaven), and that was part of the story of the politics of this within the scientific community.

A friend of mine with ties in DC told me at the time that one of the top guys from SLAC was powwowing with Senate staffers around the time the SSC was cancelled: my friend, a former HEP experimentalist who had gone into defense work, was curious about a possible connection but had no inside details as to what was discussed. Does the book go into this? Were people from the four other labs out to get the SSC or was it just that their support was lukewarm or that they merely expressed concern for their own budgets?

Too bad that HEP has become a bit of a political circus, but it is interesting for those of us with a background in the field.

All the best,

Dave

9. **Peter Woit**
December 1, 2015

Dave,

I do think there's a good case that an e+/e- machine of sufficient energy to be a "Higgs factory" would be useful to study many aspects of the Higgs. It's not clear to me though whether you could go to high enough energies this way to study the Higgs self-interaction, which may be the most promising place for something new.

As far as I know, at this point no one is talking about cost estimates for these large colliders, whether in China, at CERN or elsewhere. For one thing, it depends a lot on how you count, but the numbers I would guess will be very large, $10 billion and up.

There's a lot in the book about the people running the project, including some indication of a feeling that it was a mistake that Maury Tigner, the head of the original design team for the SSC, was shunted aside in favor of Roy Schwitters, who became the director. There's some argument that the fact that Schwitters was not an accelerator physicist, and also not experienced dealing with a project of this scale, were both sources of problems.

I think the problem with the other labs was that they needed to fund their own futures. Doing this at the same time that the SSC spending was ramping up made the budget situation for non-HEP physics much tighter and didn't help with support in the wider physics community. Once discussions started about the cancellation, I'm sure the other labs were closely involved in figuring out how to protect themselves, as well as finding something to do with the large number of people who had moved to the SSC project, and all of a sudden were out of a job.

10. **dsatkas**
December 2, 2015

Correct me if i'm wrong but the reason the LHC uses baryons (protons, lead etc.) instead of leptons (electrons-positrons etc.) is that it's circular and there is much more radiation emitted with lighter particles (i think it's m^-4). So if that is correct how will the chinese use electrons-positrons in 100 km facility efficiently?

11. **Peter Woit**
December 2, 2015

dsatkas,
The point is that LEP (in the LHC ring), was just a bit too low in energy (209 GeV) to be able to study the Higgs. Now that we know what the mass is, it turns out you don't have to go that much higher in energy to do so. Even with a much bigger ring, the plan is to go just to 240-250 GeV, but that's enough.

12. **srp**
December 2, 2015

In more-hopeful news, there's this:

http://www.symmetrymagazine.org/article/shrinking-the-accelerator

Probably worth spending maybe $100 million over the next few years to tell if there is or is not a better way going forward.

13. **Curious Mayhem**
December 28, 2015

The biggest failure of the SSC was not getting international involvement earlier on. The Japanese were riding high at that point, toward the end of the "bubble economy," and certainly could have spared some money for it. (In 1991, they basically wrote a check to pay for a large part of the 1990 Persian Gulf war to eject Saddam from Iraq.) When the SSC was first proposed in the mid-80s, the project was sold at the political level with a 110%-Americanism — with allusions to Apollo and the Manhattan Project — that was overdone and imposed a cost a few years later, when it became clear that foreign money would be needed. Absurd claims were also made by some political figures; for example, the SSC would cure male pattern baldness (seriously!), which I'm sure made it more attractive to certain politicians.

The SSC was protected until 1993 by the alliance between Bush Sr., who was from Texas, and the Louisiana congressional delegation, which was rewarded with a piece of the LIGO experiment, I believe. But that same period was dominated by deficit politics and the 1990-91 recession. It led Bush Sr. to go back on his promise to not raise taxes and to Perot's 1992 presidential campaign, in which the interrelated federal and trade deficits were big issues. After Clinton became president in 1993, the SSC rapidly lost its political protection and became a target for budget cutting; it was canceled in the fall of 1993, if memory serves me. There were a couple earlier attempts to kill it, but the Bush Sr.-Texas-LA alliance saved it each time.

The cost overruns were also significant and dramatically stimulated strong reaction from other scientists in other fields against the SSC. No one may remember him now, but the late Rustum Roy of Penn State led the charge against the SSC for the condensed matter folks and was ecstatic when it was canceled; he believed that high-energy physics was too large of the US science budget and produced an oversupply of PhDs.

There's a 2013 Scientific American article about the SSC that's worth reading. I can't vouch for the accuracy of the quotes; e.g., I don't remember anyone proposing "Gippertron" as a name. But "Desertron" was certainly a contender.

I visited the SSC remains in 1998, and it was a sad sight (or site). Many of us no longer in high-energy physics know that the SSC cancellation was a big reason.

—–

The case against e+/e- is straightforward. For rings, the radiation goes inversely with the mass to the 4th power (see comment above), and that certainly makes hadron colliders more attractive, in the circular case, for energies higher than ~ 10 GeV. That's one reason why SLAC's collider for the Z0 (about 90 GeV) was a linear collider, with partial circular arcs at the end, and not circular. The LEP energy was limited to ~ 200 GeV for the same reason. This wasn't an issue for LEP I, at the Z0, but became a noticeable limitation for LEP II, just above the W+W- pair threshold.

The other problem with e+/e=, not mentioned above that I can see, has to do with incoming fluxes and cross-sections. With proper focusing, the current density from e+/e- is smaller than for hadrons. Then the cross sections kill your statistics: for point-like particles, they fall like 1/s, as opposed to hadronic cross sections that rise like s. This latter problem is an issue for either linear or circular colliders.

Not that I'm prejudiced against e+/e-. I did my doctoral thesis at such a collider and spent a couple years on an ILC (International Linear Collider) task force in the 1990s. The clean-ness of e+/e- was a compelling argument in favor. See various Web articles about the ILC, which has been floating around in proposal form for about 25 years.

Next Week’s Hype

December 2, 2015
Categories: Favorite Old Posts, This Week's Hype, Uncategorized

Next week there will be a workshop in Munich with the title Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics. It’s organized by Richard Dawid, to discuss his ideas about “non-empirical theory confirmation”, developed to defend string theory research against accusations that its failure to make any testable predictions about anything make it a failure as a research program.

I guess the idea of such a workshop is to bring together string theory proponents and critics to sort things out, but looking at the program and talk abstracts, this doesn’t look promising, with the central issues to be evaded, and speakers likely to just talk past each other.

While the workshop title refers to “Modern Physics” in general, the talks are mostly focused on one very narrow part of the subject: quantum gravity. String theory is supposed to be something much more than a quantum gravity theory, explaining the Standard Model and low energy physics. This has been a complete failure, and the plan at the workshop seems to be to deal with this elephant in the room by ignoring it, or worse, claiming it isn’t there.

From the talk abstracts, about the only person discussing particle physics will be Gordon Kane (Quevedo may mention it, although his main interest is cosmology). Kane will be claiming that string theory makes testable predictions about particle physics, ones about to be tested. The problem is that he has been making the same claims for thirty years, arguing back in the 90s in the pages of Physics Today and a book that string theory would be tested at LEP and the Tevatron (by finding superpartners). As his predictions have been conclusively falsified, he just refuses to acknowledge this and starts advertising new ones. Perhaps the most outrageous case of this is his latest book (discussed here), which is a reissue of the old one, with falsified predictions simply deleted and replaced, without any acknowledgement of what happened. I don’t think this behavior raises any philosophical issues about theory confirmation, just the sociological issue of why the physics community tolerates this, or why he’s the one person invited to this workshop to address the largest problem of the subject.

Another central problem here is the hype problem. If you give up on testability, and allow theory confirmation based on claims that “my theory is just better” by some ill-defined metric, you open up the obvious problem of how to deal with people’s natural human inclination to praise the wonderful characteristics of their intellectual children. At the workshop, Joe Polchinski’s talk is entitled “String Theory to the Rescue”. I see nothing in the program about any planned examination of the significant string theory hype problem, or even any acknowledgement that it exists.

I’m actually in a way more sympathetic than most people to the idea that “non-empirical” evaluation of a theory is an important and worthwhile topic. Fundamental physics theory is facing a huge problem due to the overwhelming success of the Standard Model and the increasing difficulty of exploring higher energy scales. If it is to continue to make progress there is a real need to do a better job of evaluating theoretical ideas without help from experiment. There is a group of scientists who have a lot of experience with this problem, and have a well-developed culture designed to deal with it. They’re called “mathematicians”. Despite the fact that this workshop is hosted by the Munich Center for Mathematical Philosophy, the organizers don’t seem to have thought it worthwhile to invite any mathematicians or mathematical physicists to participate, missing out on a perspective that would be quite valuable.

Update: It’s now “this week”, not “next week”. Some tweeting from the conference going on, you can try the hashtag #WhyTrustATheory. Massimo Pigliucci comments from the Q and A session about a problem with this kind of thing: some people “very very much like the sound of their own damn voice”. I hear that David Gross claimed to have 20 possible observations that would invalidate string theory, but didn’t say what they were.

Update: Massimo Pigliucci is blogging a detailed account of the conference, see here (he’s also a speaker, slides here).

Update: I don’t think I’d noticed before that Lee Smolin has a very much to the point review of the Dawid book here.

Comments

1. **RandomPaddy**
   December 2, 2015

   > If it is to continue to make progress there is a real need to do a better job of evaluating theoretical ideas without help from experiment.

   I would go in the opposite direction. What about really scrutinising the theories we already have, against the data we already have. The LHC is producing and has produced presumably petabytes of data. I presume Fermilab’s results and many others are also on file. Who among theorists is looking at this data, testing it against newer theories? Has all value really been squeezed out of these? Given the plethora of untested theories, my guess is that theorists really aren’t in the habit of looking, and/or experimenters aren’t in the habit of showing.

   Could a lot of overly speculative theories be avoided if the speaker was required to say “I checked this against XYZ data sets and they didn’t immediately invalidate most of it”.

2. **Peter Woit**
   December 2, 2015

   RandomPaddy,

   As far as I can tell, phenomenological theorists and the experimentalists themselves are already trying the best they can to do what you suggest. Every HEP experimentalist I’ve talked to tells me its their fervent hope to discover something that disagrees with the Standard Model and that this is the main focus of their research.

   The problem with speculative theories is not that no one is trying to confront them with experiment, but that they’re too ill-defined to do so, or carefully constructed to evade the possibility of such a confrontation.

3. **Jim Baggott**
   December 2, 2015

   Peter,
   I don’t think it’s quite true to imply that Dawid has organised this conference on his own. I believe he’s also had inputs from George Ellis and Joe Silk, the authors of the short note in Nature last December which triggered the decision to hold this conference. That was a strongly-worded piece (aside from calling for a conference, they suggested that journal editors and publishers assign speculative work to research categories other than ‘physics’ and that ‘the domination of some physics departments and institutes by such activities should be rethought’).
   I think it’s highly likely that the protagonists in this debate will tend to talk past each other, but Ellis and Silk will be joined by Carlo Rovelli, Massimo Pigliucci, Helge Kragh and Sabine Hossenfelder, all of whom have been critical of the string theory enterprise, the community’s tendency to hype and distort its promise and the threat it

poses to the integrity of the scientific method and the public understanding of science.
I think there will be plenty of opportunity for argument, at least.
I don't have high expectations, but I'm hoping that the conference will help to foster a better understanding of the role that the philosophy of science can play in all this. This should be all about seeking to define an acceptable approach to theory development and selection in high-energy physics and cosmology, against the background of a relatively slow evolution of experimental and observational science (satellites take decades to plan, build and put into orbit; colliders take decades to plan, build and put into operation). I'm sure that much could indeed be learned from mathematicians, as you suggest, but it may be that the first step is get some kind of agreement that there is a problem here, before working out how we might try to resolve it.

4. **Foster Boondoggle**
December 2, 2015

"There is a group of scientists who have a lot of experience with this problem, and have a well-developed culture designed to deal with it. They're called "mathematicians"."

Haha! Nice try. Back when I was in grad school, the mathematicians were still trying to prove that interacting quantum field theory exists. W. Arveson, Berkeley math prof 17 years ago: "Here is a good problem for 21st century mathematicians: show that this program for quantizing nonlinear PDEs like [phi^4 theory] can be carried out in four spacetime dimensions. In other words, show that Quantum Field Theory exists in rigorous mathematical terms." (Have they succeeded yet? Does QED, by far the most accurately confirmed physical theory ever, exist?)

If we wait for the mathematicians to baptize physical theories as acceptable we'll be waiting a very long time. It took them 25 years to figure out how to formalize something as simple as the Dirac delta function.

Anyway, aren't there a bunch of mathematicians out there who absolutely adore string theory? As an ex-physicist, my difficulty with string theory was always that it seemed to be motivated mainly by the beauty of the math (and its offer of a way out of some of the more formal difficulties of field theory) – much closer stylistically to Einstein than to Feynman. (Think of Einstein's quote about how he would have reacted if the eclipse observations hadn't borne out his theory – "Then I would have felt sorry for the dear Lord. The theory is correct." vs. the Feynman quote about not being able to fool nature.) I don't mean that as a criticism of the field – it's more about my own limitations – but looking to mathematicians to bail fundamental physics out of its current difficulties seems a bit perverse.

5. **Bee**
December 2, 2015

I will do my best to be the voice of reason 😉

6. **Peter Woit**
December 2, 2015

Thanks Jim,
I do see that string theory critics will be represented, but they're mostly ones concerned about other issues than the ones I mentioned (with Rovelli somewhat of an exception, although not an HEP theorist, and Sabine well aware of the hype problem). The Ellis/Silk challenge to Dawid was mainly on the multiverse front, and there I'm interested to see that it's the multiverse proponents who don't seem to be well represented.

Foster Boondoggle,
My point was not about mathematical physics and whether it's the answer to any problems in physics, rather that mathematicians understand well the issues that arise when you try and make progress having only things like consistency as your guide.

Bee,
Good luck!

7. **Jon Forrest**
December 2, 2015

"If you give up on testability, and allow theory confirmation based on claims that "my theory is just better" by some ill-defined metric, you open up the obvious problem of how to deal with people's natural human inclination to praise the wonderful characteristics of their intellectual children."

It's been a long time, and I'm not sure what's happened since, but in the early 1970s the issue you mention was a problem in the field of Linguistics, specifically with the theories of generative syntax and generative semantics.

8. **Douglas Natelson**
December 2, 2015

After 16 years as a professor I still bristle at the implication that the only "real" modern physics out there is high energy physics.

9. **Low Math, Meekly Interacting**
December 2, 2015

What am I missing about Dr. Kane? I can think of no equivalent in another scientific discipline. He'd maybe have to be a paleoanthropologist who not only helped convince the majority of the field that Sasquatch exists, but also convinced them that the only plausible explanation for not bagging a Sasquatch by now is that the beast has evolved, through a complex process of phyletic dwarfism, into a nanobacterium-sized extremophilic autotroph that lives in old lava tubes 5km below the Cascade Range. Until core samples reveal no such creature, in which case the estimate is revised to 5.5 km below the Cascades.

I'm not really trying to be insulting, I'm genuinely mystified by this character.

10. **Peter Woit**
December 2, 2015

LMMI,
I don't think he's convincing anyone of this, with very few people in the field now taking his claims seriously. The real question is why he's given a public forum to make them. I think that's clearly because he's making a case for string theory. There are different standards for claims about string theory than for claims about other ideas…

11. **Low Math, Meekly Interacting**
December 2, 2015

It's a pretty darn weird thing to observe, whatever the standards.

I should add I'm using "Sasquatch" as an analogy for the kind of SUSY that "naturally" solves the hierarchy problem and gives us dark matter, etc., i.e. the kind of beast only cryptozoologists take seriously these days.

12. **Martin S.**
December 2, 2015

@Peter,
as a mathematician and having an experience of talks with other mathematicians about what directions of study are reasonable, the way according to common mathematicians would be: who is higher in hierarchy or generally in position of power.

PS Some mathematicians enjoy to pretend that their work has connections to empirical sciences, so that they can get more funding. It may explain why some mathematicians love the string theory…

@Jon Forrest,
decades ago I had seen a lot of ridiculing on linguistics. By now it seems that they solved a lot of issues by the big-data approach. If only particle physicists were able to do it alike.

13. **Shantanu**
December 2, 2015

Peter, I am also going to attend this meeting. If you have specific questions to specific speakers, email me.
shantanu

14. **Peter Woit**
December 2, 2015

Thanks Shantanu, I look forward to hearing what you think of the event. By the way, I don't have an e-mail address for you that works…

15. **vmarko**
December 3, 2015

Peter,

I'll be attending the conference as well (and my e-mail address should work, if you feel like compiling a list of questions… 🙂).

Regarding Kane's book, my feeling is that few of the actual experts in the field have actually bothered to read it, let alone both issues. So they are probably unaware of his "outrageous" behavior, or are reluctant to criticize him without investing some time to seriously read and compare what he said in the two issues of the book. For one, I am in that crowd, and I certainly have better things to do than read two almost-identical books on string theory hype.

Regarding mathematicians, I agree with you that they are underrepresented here, and it could be better. But on the other hand, my impression (based on working with and talking to mathematicians) is that they tend to shrug at any type of problem which cannot be formulated as a theorem. Or at least they are at a loss what to do with such a problem. Granted, I may have talked to wrong people or lousy mathematicians, but as far as I have seen, philosophers are much more adept to providing a clear formulation of a problem, especially when it is too vague to make rigorous sense.

Best, 🙂
Marko

16. **Jesper**
December 3, 2015

I agree very much with the notion that we can learn from mathematicians on how to evaluate ideas and structures, which are not directly empirically verifiable, and also that mathematicians should be involved in the development of such structures. Collaborations of this kind do exist. One example is Chamseddine & Connes and there are others 'out there'.

17. **andrew**
December 3, 2015

I didn't find Dawid's work particularly convincing. A question for the conference (or anyone interested): what's the relationship between non-empirical theory confirmation (NETC) and Bayesian confirmation theory (BCT)?

It struck me somewhat that many of the NETC arguments could be interpreted as reasons for having a high prior belief in a particular theory in BCT, e.g. few or no viable alternative theories. The "scientific" aspect of BCT – updating a prior with experimental data – is of course absent in non-empirical confirmation.

Also, NECT seems to invoke induction – we're told the fact that something qualitatively similar to a theory worked in the past helps to confirm that theory. How does this overcome Hume's problem of induction (w/o BCT)?

18. **Peter Woit**
December 3, 2015

vmarko,
I doubt experts paid attention to the books, but they've surely aware of his claims in papers and talks that he can use string theory to make predictions, as well as the fact that these predictions keep getting falsified, and he keeps making new ones.

I don't think mathematicians will only discuss rigorous theorems. It is true that their reaction to an attempt to discuss with them a topic in current quantum gravity research would likely be "I'm not understanding, could you more precisely explain your assumptions and what the question is you are trying to address?" Wouldn't hurt most physicists to try and engage in a discussion like this.

I hope to hear what you think of the conference, don't have a lot of questions for the speakers myself. For better or worse, I think I have a pretty good idea how most of them would respond.

19. **vmarko**
December 3, 2015

Peter,

Whenever I have tried to discuss quantum gravity with mathematicians, they always reacted in precisely the way you described.

But then if I try to engage them in a discussion, I end up realizing that they don't have a good grasp on QFT, in particular the renormalization procedure. Once I even gave a three-lecture course on renormalization, tailored to mathematicians who do not want to invest a lot of time into learning QFT in all gory detail (a writeup of the resulting lecture notes is here). I don't know if I did a good job or not, but I became painfully aware that even discussing QFT is way too remote from what mathematicians are used to, and we never reached the point where I could even formulate the problem of QG. And this experience was with the audience whose research is mostly related to TQFT, category theory and algebraic topology, which are arguably the areas of math closest to QG.

My point is that mathematicians live in a world of fully rigorous math (like TQFT), which is quite removed from what physicists deal with (QFT), already at the point of terminology, let alone any further common ground. They do tend to have good will and motivation to engage in QG, but when faced with a learning curve that involves a lot of things which are nowhere near as rigorous compared to the things they are used to, very few actually invest enough effort to swallow all that non-rigorous stuff in order to understand the basic concepts that QG deals with.

Note that I am not trying to criticize them, but only to point out that there is a huge divide in the background knowledge and a way of thinking of a mathematician versus a physicist. It's just a very big hurdle to cross, in both directions. So engaging them in any discussion related to QG usually stops at the point of that divide, unfortunately.

Best, 🙂
Marko

20. **Peter Woit**
December 3, 2015

vmarko,
What I was thinking of more was some of the discussion of new ideas of quantum gravity, where statements are being made like "ER=EPR". Maybe my mind has been deformed from too long in math departments, but my reaction to a lot of current discussion about quantum gravity is that I can't follow it not because there is some technical thing I don't know, but because it's very hard to figure out what the precise claim being made is. There's some web of conjectures being invoked, but it's often hard to understand what's being assumed about what.

21. **vmarko**
December 3, 2015

Peter,

Oh, now I understand what you mean, and I completely agree. Mathematicians are experts on stating explicitly all the assumptions of a certain claim or conjecture, and can provide nontrivial input into what is consistent with what, and how various conjectures do or do not fit together. That would certainly bring a lot more clarity into QG physics, and would certainly be a very good thing.

Best, 🙂
Marko

22. **Low Math, Meekly Interacting**
December 3, 2015

The histories I've read seem to indicate that mathematicians were helpful to Einstein because he was asking precisely the right kinds of questions. The equivalence principle he relied upon made the problem to be solved very clear. I look at current debates about, say, the firewall paradox, and there appears to be a distinct lack of clarity, namely because there is a lack of consensus that a "paradox" follows from what is understood about black hole physics. It could either the most important insight of the day, or an irrelevance built on faulty premises. It's just an example, but the field of quantum gravity apparently has a disturbingly large number of arguments about basic premises, even among adherents of the same theoretical framework. Is it reasonable to expect mathematicians could help if QG physicists aren't asking the right questions?

23. **Peter Woit**
December 3, 2015

LMMI,
The point I'm trying to make (this is much like my comment to Foster Boondoggle) is that the culture of mathematicians might be helpful here, as opposed to the subject itself (which may or may not contain helpful knowledge). I think you're right that a big problem is "distinct lack of clarity", and mathematicians are trained to recognize that problem and treat it as one of central importance. Physics has a different culture, with doing an explicit calculation more at the center of things, and an inclination that it doesn't matter much if the logic of the calculation is murky, since that's something that can be cleaned up later if the result of the calculation is interesting and reproduces the real world. If your calculations can't be checked against the real world, and all you have to go by is consistency, my argument is that you might have some things to learn from the way mathematicians operate.

24. **jim**
December 4, 2015

Non-empirical physics really sounds like an oxymoron. Why bother? I was under the impression that the purpose of physical science was to explain the reality that we can see and examine. Perhaps if theoretical physics can come up with an system which specifically predicts the Standard Model and gravity and within which there is a proof that no other such theory can exist we could be happy without empirical evidence. This doesn't seem likely. My concern is that the exuberance of theoretical imagination unchecked by any empirical examination over the last 35 years is exactly what has left us with the impasse of string theory/KKLT/landscape stuff where we now stand. Why should the application of more mathematical rigour and surety of internal consistency to any fervent ideas that may be forthcoming really take us any closer to understanding reality? This doesn't seem to have happened yet.

25. **sm**
December 4, 2015

vmarko:
In the early 90s I was involved in a similar effort with our maths department. A mathematician (a collaborator of Shelah – if I had known of the latter then I should have been warned!) in 10 lectures derived Weiner measure because we all knew the common lore of its equivalence to Feynman's path integral. In return I in 5 lectures was able to derive (using this term flippantly) in the usual physics manner not only Feynman's PI for the non relativistic case, but also the PI for the Dirac equation (the locally susy version with its grassman variables etc) to bout. All this revealed to me the chasm between physics and the 'equivalent maths'. For the first time I was exposed to a mass of new (to me then and probably even to most field theorists today – unless they are genning up to become financial engineers!) ideas such as sigma algebras,

measure theory, filtered probability spaces, martingales, large deviation theory … the list goes on, all needed just to define and compute Feynman's PI (more accurately its imaginary time version as I think I remember reading that the naive Feynman's real time version is known not to exist mathematically, even for non relativistic quantum mechanics – a sobering thought). If all this infrastructure is required just to properly formulate plain old non-relativistic QM, what hope do we have that mathematicians will help with QFT in the near term, let alone with 'string theory'? A highlight (if that is the correct word!) of my lectures was to justify why all my paths were differentiable (so the action could be computed for each path) when previously the mathematician had shown with complete rigour that with probability unity all paths contributing were continuous but nowhere differentiable! At that point I mumbled something about the renormalisation group!

26. **Urs Schreiber**
December 4, 2015

sm, for that reason one eventually needs a formalization of physics that captures the core mathematical structures of relevance "synthetically", i.e. without breaking them up into a heap of constituents, but capturing the intended properties right away. There was a talk about such an system at IHES last week. A video recording is linked to here.

27. **Interested Layperson-Not a Player**
December 4, 2015

The January 2016 issue of Astronomy Magazine has a one-page article that is very pro-multiverse. You are mentioned in the article, entitled "Not science fiction" and subtitled "Three cheers for multiverses!". The author, Jeff Hester, writes that if you have read his previous columns you know falsifiability is a big deal for him (I have not read all of his previous columns). In his current column he asks first if we can observe multiverses. He goes on to say that that is the wrong question. He says

"the right question is whether theories that rely on multiverses are more or less successful than theories that do not. Putting it differently, the statement 'multiverse theories will make more interesting and correct predictions than theories without multiverses' is itself a testable prediction."

He mentions David Deutsch's work on quantum information and that Deutsch himself says his work depends on Hugh Everitt's "many-worlds interpretation" of quantum mechanics.

If I have violated any copyright issues by quoting the author then just delete my post, but if you can check out his article and offer any comment here please do. On to the other hand you may feel it's just not worth the time responding to every popup supporting multiverses.

28. **Chris W.**
December 4, 2015

LMMI: *Exactly*. That is very much the point of Chern's talk for an Einstein Centennial conference (1981) that I mentioned in a comment on another recent post of Peter's. I got the impression that mathematicians are, for the most part, not particularly interested in groping their way to a clear formulation of a problem, when there are already plenty of clearly stated problems waiting to be investigated (and which show promise of being non-trivial and mathematically fruitful). Einstein was motivated to do this because he felt *as a physicist* that it was important and necessary. Most mathematicians would be content to say: "Good luck with that. Let me know how it turns out, and if so, whether you would like to consult me about anything."

I think there is a lot of self-selection working against us here. If a mathematician had the cast of mind to take a serious interest in these problems at these difficult early stages then they probably wouldn't be in a math department, or they would have already done some serious research bearing on it, despite being professionally identified as a mathematician. Such people are pretty rare, or at least their ideas on physics haven't been seen as worthy of attention.

29. **Couvent**
December 4, 2015

I'm a bit suspicious about the way the relation between physics and mathematics is described here. I spent a brief moment in a Theoretical Physics department and some parts of had a close relationship with the mathematicians at the university. This was certainly true for the Statistical Physics group. Some mathematicians even came to that group to get their PHD (an went back to the Mathematics department, got tenure etc.)

Isn't the problem you are talking about here mainly typical for theoretical HEP, String Theory etc.? My pet theory is that renormalisation is the root cause of it. Mathematically dubious, but it worked – suggesting that a bit of sloppiness and mathematically ill-posed problems are, well, no problem. It worked! String Theory etc. doesn't seem to "work" in that way, but the attitude is still there.

30. **Peter Woit**
December 4, 2015

All,
Please, enough unfocused generic discussion of the uses of mathematics in physics. The issue I was trying to raise in this posting really is something different, the question of what mathematicians might contribute to the topic of this conference (how do you evaluate theories without contact with experiment?). This is about methodology, not the substance of either field. My impression is that most physicists understand so poorly how mathematicians conduct research that they can't see the issue here (which would explain why no one seems to have invited any to participate).

31. **Peter Woit**
December 4, 2015

Thanks Interested Layperson,

A bit of the article is at
http://www.astronomy.com/magazine/jeff-hester/2015/11/not-science-fiction
I haven't seen the whole thing, but I don't have a problem with the part quoted. That there are no interesting and correct predictions from the string landscape multiverse is the problem, and thus according to Hester, this sort of multiverse has already been falsified.

By the way, if people want a detailed defense of the string theory multiverse, together with a detailed documentation of priority claims that it was basically his idea, Andrei Linde has a preprint at
http://arxiv.org/abs/1512.01203
I'm somewhat curious why he or others with a similar point of view (Susskind, Vilenkin, Guth, etc. etc.) are not speaking at this conference next week.

32. **sm**
December 4, 2015

Peter: "My impression is that most physicists understand so poorly how mathematicians conduct research that they can't see the issue here".

That is the point of my (perhaps simplistic) example – even questioning the differentiability (let alone continuity, which is true only for a narrow class of continuous time processes – as has been learnt the hard way by those have sought in vain to generalise Black Scholes to the real world!) of Feynman paths did not come naturally to physicists! The mathematicians were invaluable here.

I think it is obvious that the mathematicians' specific deep understanding of relevant concepts to physics is what makes them special. If you just need a very good critical listener that does not let you get away with any 'sleight of hand' (no names mentioned!), and these are just as important in the general scheme of things, then a mathematician is not necessary – a physicist will do. Sydney Brenner in mock disgust commented that Francis Crick (a former physicist of course) 'did not let him get away with anything'!

33. **Chris W.**
December 4, 2015

Peter,
The issue (isn't it?) is how to evaluate *physical* theories without contact with experiment (at least provisionally). Do pure mathematicians ever have to worry about this? They evaluate their own theories without contact with experiment, but that's because those theories don't refer to the outer "observed" world, i.e., they aren't empirical to start with. Their initial assumptions don't even need to have a physical interpretation. The end game is proof that an assertion follows from certain assumptions. What happens when the empirical soundness—and not just the consistency—of the assumptions is a central issue, perhaps *the* central issue?

Applied mathematicians presumably operate differently, perhaps most of the time. Is that who you're talking about?

34. **sm**
December 5, 2015

Peter: (sorry for my verbosity on this topic!). I looked at your link above to Linde's recent (no doubt justified) 'Lindecentric' historical account of inflation/multiverse. Maybe my measure theory example above (differentiability of Feynman paths) where mathematicians helped enormously is not as simplistic as it seems.

Linde gives the usual (a la, Guth, Susskind, Page, …) argument for the multiverse 'measure problem showing by example how you can easily get yourself in a twist trying to define probabilities when your set ('sample space') is infinite. The problem with this oft used example is that its 'measure' (the Cesaro or counting density – note mathematicians don't call it a measure) is not a measure. It does not satisfy all the criteria to be one – hence one easily (or naively, as in the case of philosphers of the past) produces a contradiction. What bothers me is that the reader is not warned,or at least I missed it. Is this to shield the poor reader from the horrors of measure theory or something else? Relevant mathematicians surely have something to say, if only because for 200 years they too have had all the confusion of an ill defined 'theory' without a foundation – probability theory. Amusingly, access to abundant empirical data did not help, if anything it hindered. Instead an rich array of logical problems associated with 'thought experiments' played an important role leading to Kolmorgorov's final synthesis and satisfactory definition of probability theory – one that now looks so obvious that one wonders wny it took 200 years to find. Depending on ones personal conviction, one may hope (and in much less than 200 years, but as they say, 'don't hold your breath') for a similar outcome with one or more of QFT, StringTheory, inflation theory … . Skim reading the abstracts of the Munich conference, I unfortunately don't see any obvious reference to this interesting, and to my mind, obviously relevant history (relevant even at the technical level) of mathematics, perhaps reflecting your point about the absence of relevant mathematicians. Note also that is well known that philosophers did not always have a positive role in creating a solid probability theory.

35. **Peter Woit**
December 5, 2015

Chris W.,
If your theory makes no contact with experiment, "empirical soundness" isn't really a meaningful criterion, For example, the people doing quantum gravity these days like to announce that they are showing how to get rid of space (and maybe time too). If their replacement for space makes no testable connection to reality, the only way to evaluate it is by whether it is well-defined, internally consistent, consistent with some other specified assumptions based on how one hopes to connect the idea to reality. Methodologically, this is the kind of thing mathematicians are trained to do (this has nothing to do with applied mathematics). By the way, mathematicians also have a huge amount of expertise about different ways to think of generalizations of the classical notion of space (i.e. a differentiable manifold), but that's another topic.

sm,
It does seem to me that mathematicians and philosophers of math would have something to contribute to a discussion of the supposed "measure problem" (my point of view though is that this isn't the problem, the problem is more fundamental, that you don't know the space your measure is supposed to be on). Again, I find it odd that there seems to be little discussion of the multiverse planned at this workshop. Perhaps this is because if proposed "non-empirical confirmation" is invoked as an ally by the multiverse maniacs, that would seriously discredit the whole idea in many people's view.

36. **vmarko**
December 5, 2015

Peter,

"I'm somewhat curious why he or others with a similar point of view (Susskind, Vilenkin, Guth, etc. etc.) are not speaking at this conference next week."

I'm curious about that too. All speakers have been invited by the organizers, and there is no option for contributed talks. So the list of speakers reflects solely the choices of the organizers.

Best, 🙂
Marko

37. **martibal**
December 6, 2015

A curiosity tangent to the topic: here is the text of the "Samy Maroun center for Space, Time and the Quantum" whose Rovelli (one of the speaker of the Munich conference) is president (see http://www.centresamymaroun.com): "[…] Institutional agencies supporting theoretical physics research focus on the development of mathematical tools, but research cannot confine itself to these tools, neglecting direct physical intuition. Physical intuition based on experimental results and the qualitative content of the successful physical has always represented, historically, the essential source of our understanding of the physical world. The main objective of the Samy Maroun Center is to promote fundamental research on Time, Space and Quanta, grounded in the first place on the capacity of intuition to imagine and describe the world."
I was quite surprised to read that mathematical physics is somehow over founded by institutional agencies !

38. **sm**
December 6, 2015

Peter,

"my point of view though is that this isn't the problem, the problem is more fundamental, that you don't know the space your measure is supposed to be on".

In the case of "probability theory" (as it was before Kolmogorov, or more accurately, before the early 1900s when the French mathematicians especially started to 'get a grip') the problem was even more fundamental than that – you didn't even know you had to use a measure in the first place. Despite this, the "theory" had great calculational success (hence the reason to find a proper foundation), rather like some, but not all, of the "theories" we have now.

As I am sure you know, the history of the development of probability theory over 200 years is very rich and with the benefit of hindsight, counterintuitive, in places even comical, involving very colourful characters (to say the least!). The history our modern "theories" so far, to my mind, has great parallels with all this and we still have a lot to learn from Kolmogorov and ancestors (and maybe even his descendents – although at the moment it seems to be going other way as far as probability is concerned), possibly not only at the meta level.

39. **Peter**
December 7, 2015

Peter Woit: 'my point of view though is that this isn't the problem, the problem is more fundamental, that you don't know the space your measure is supposed to be on'

This is supposed to be one of the good things about measure theory: you don't need to know that. You just need to know a containing space, and that can be much bigger, then ask where the support of your measure is. Here is an example.

Suppose you are interested in (Lebesgue-) measurable functions from $[0,1]^2$ to $[0,1]$. You only care about these up to equivalence on sets of measure zero (meaning: you can do what you like to a function on a set of measure zero, and I will say you didn't change it). Think of such a function as $f(x,y)$, and suppose you are interested in what functions show up as $f(x_0,y)$ for a given f and any fixed x_0, called 'points' (I know this is a strange name! There is a reason for this construction, it is a combinatorial graph limit, but you don't need to care). Now in principle such functions could be any function at all from $[0,1]$ to $[0,1]$, not necessarily even measurable, because f is defined up to equivalence on a set of measure zero and the line (x_0,y) has measure zero. So you only care about `typical points', meaning that (in some sense which should be rigorous) you want to get rid of that kind of silly example.

To be more concrete, if you are given $f(x,y)$ is 0 if $x\le y$ and 1 otherwise, then the 'typical points' $f(x_0,y)$ are a one-parameter family, so you should get the structure of a line out of this.

Here is how you do it. You take any given f, and any measurable set X in $[0,1]$. Now you generate a measure on $L_1[(0,1)]$ by setting the measure of $\{f(x_0,y):x_0\in X\}$ equal to the measure of X. Next, you declare the 'typical points' of f to be the support of your measure.

The thing to note here is that $L_1([0,1])$ is a truly huge space, and you are looking for low-dimensional manifolds in it, but nevertheless the measure theory happily lets you do this. And because you had a measure to work with, you could do this construction rigorously.

40. **Shantanu**
December 7, 2015

Peter,
Sabine asked the question about the observations. He pointed out just one of them
saying if that there is evidence for a long range force, or a vectorial/tensorial force
or evidence for 5th force it would invalidate the string framework.

41. **Peter Woit**
December 7, 2015

Shantanu,
I wonder what Gross is thinking of. I don't see any reason you can't have another long range force like the electromagnetic force, coupling to a charge. Presumably he means coupling to mass. Often one hears the exact opposite claim, that observation of a fifth force would be evidence for string theory, see for example here
http://www.math.columbia.edu/~woit/wordpress/?p=6561
In string theory you have potential long range forces due to moduli fields, in some sense the big problem of the theory is why you don't see these (how do you stabilize moduli?). So, I'm guessing he meant non-scalar forces that violate the equivalence principle, coupling to mass, violating various principles of GR + QFT, not just string theory. It's easy to come up with things that don't fit fundamental principles of QM or QFT, and say those would falsify string theory. The real question though is whether you can falsify the characteristic properties of string theory that are different than QFT.

I would bet good money though that if experimentalists discover a fifth force, the headlines will be "string theorists claim evidence for string theory", and if it's not scalar, some "string vacuum" will be constructed to reproduce whatever is observed…

42. **emile**
December 7, 2015

Shantanu: will the slides be put online?

43. **Shantanu**
December 7, 2015

I am not on the organizing committee. But I will check tomorrow, although I will pretty sure they will be at some point. Also they are videotaping the whole thing, so the videos will also be put online. However as a physicist, I didn't really get much from the philosophy talks
and the main talks I paid attention to were by Gross, Rovelli and also Helge Kragh
who mentioned some very interesting things about Eddington's fundamental theory, which I didn't know.

44. **vmarko**
December 7, 2015

Gross didn't really explain why he thinks that a new long range force would invalidate ST, and I think that at that point the majority of people in the audience were very grateful for that. Some people just don't distinguish between asking a question and giving a lecture, and don't know how to give back the microphone…

In other news, the majority of the discussion centered around Dawid's three-rule non-empirical theory confirmation criterion, and there was a big fuss about whether it's a misnomer, because it's more about theory assesement than confirmation (the latter being too loaded with different meanings).

Other talks were also interesting, but not controversial in any way. Overall it was nice today, we'll see how tomorrow goes.

Best, 🙂

Marko

45. **Alessio**
December 8, 2015

Douglas: I totally agree with you…in fact there is no paradigma shift, nor any crisis whatsoever in science, but, maybe, in some fields.

46. **Jeffrey M**
December 8, 2015

It's fascinating, there really is some sort of deep misunderstanding of mathematics, among both physicists and philosophers. From Pigliucci's blog – "Theorists may give up, or they may play with extrapolation, or toy models (i.e., thought experiments). They could also adopt strategies from other fields, like mathematics, where beauty is a criterion for success. [Uhm, that's pretty dangerous territory…]" But beauty is neither a necessary nor a sufficient condition for success in math. The primary criterion for success is of course proof, unless you can prove something correct it will not succeed. If you can prove something, there are various reasons why it might be a success. One is certainly beauty, mathematicians love beautiful results. Another is usefulness, mathematicians love plenty of not beautiful things because they are very useful. Physicists of course don't have proof really, even experiment isn't quite "proof," and this makes the use of the other secondary criteria a serious problem.

47. **Shantanu**
December 8, 2015

emile. The slides will be put online.
Today's highlights was the fireworks between Gross/others and Kane, when Kane mentioned that M-theory predicted Higgs boson mass of 126 GeV. Probably Marko or Sabine can/will add more either here or on Sabine's blog.

48. **Chris W.**
December 8, 2015

Massimo Pigliucci's post for today ("Why Trust a Theory? — part I") described another tense exchange (brief) on a less substantive matter.

49. **vmarko**
December 8, 2015

Ok, day two. 🙂 Arguably, the only thing more fun than a duel between a ST proponent and a LQG proponent is a duel between two disagreeing ST proponents… When Gordon Kane explicitly put on his slides "in 2011 M-theory predicted the Higgs mass at 126 GeV" (and he even gave the error bars of that prediction), David Gross had to intervene, and the fight was on! 😉 After some philosophers protested at the interruption and demanded that Kane continues on with the talk, Gross said (I'm paraphrasing from memory) "oh, we physicists have these discussions in the middle of a lecture all the time — you know, it's called the 'scientific method'…"

Eventually other talks went on, with some interesting but less controversial topics.

But the main party was in the afternoon, stolen by Slava Mukhanov. The incredibly funny yet lucid lecture spanned topics from inflation to dangers of falsifiability in the former USSR, including the equation which will probably become a classic:

inflation – theology = exp (-aH),

with a comment "even Lemaitre knew this"… You had to be there.

The party continued during the discussion session where Mukhanov played the role of "voice of reason", telling everybody that if a theory cannot be tested it is not physics, and redefining falsifiability away isn't going to change that. I'll just paraphrase an exchange regarding eternal inflation and the multiverse:

Gross: Multiverse is an important problem where philosophers can help, and it's a good topic to collaborate on.
Mukhanov: Maybe we should also invite theologists?

Really, you just had to be there… 🙂 I can only guess about tomorrow, when the multiverse will become the main topic.

Best, 🙂
Marko

50. **Tammie Lee Haynes**
December 8, 2015

Dear Dr Marko

I'm glad you were amused by Dr Mukhanov's comment on the Multiverse:
"Maybe we should also invite theologists?"

But as a Creationist, I ask this: Why would theolgists attend?
Christian Theology is based on the accounts of witnesses to the Miracles of Jesus.
Accounts of witnesses are "empirical data."
And as you note, the Multiverse Proponents like Dr Gross cant offer any to the theologists.

very truly
Tammie Lee Haynes

51. **Shantanu**
December 9, 2015

Something else to add about Slava's talk. Apparently he mentioned that he knew
the bicep-2 results were wrong, the day they came out. After talking to him, he mentioned that he said the same to Alan and Andrei, but they wouldn't listen to him.

Today's Sabine's talk was also awesome. She asked David(representing Polchinski)
if he was referering to Peter Woit's blog, when he mentioned "blogs attacking string theory" and David said no.
shantanu

Today’s Hype

December 9, 2015
Categories: Multiverse Mania, This Week's Hype

Joe Polchinski’s contribution to the ongoing Munich meeting has now appeared on the arXiv, with the title [String Theory to the Rescue](). Evidently he’s not actually to be at the meeting, I’m not sure how his paper will be presented.

It’s pretty much the usual hype about the string theory and the multiverse, with untestable ideas about quantum gravity the only topic. The one innovation is that it contains a calculation: the probability of a multiverse is 94%:

> To conclude this section, I will make a quasi-Bayesian estimate of the likelihood that there is a multiverse. To establish a prior, I note that a multiverse is easy to make: it requires quantum mechanics and general relativity, and it requires that the building blocks of spacetime can exist in many metastable states. We do not know if this last is true. It is true for the building blocks of ordinary matter, and it seems to be a natural corollary to getting physics from geometry. So I will start with a prior of 50%. I will first update this with the fact that the observed cosmological constant is small. Now, if I consider only known theories, this pushes the odds of a multiverse close to 100%. But I have to allow for the possibility that the correct theory is still undiscovered, so I will be conservative and reduce the no-multiverse probability by a factor of two, to 25%. The second update is that the vacuum energy is nonzero. By the same (conservative) logic, I reduce the no-multiverse probability to 12%. The final update is the fact that our outstanding candidate for a theory of quantum gravity, string theory, most likely predicts a multiverse. But again I will be conservative and take only a factor of two. So this is my estimate for the likelihood that the multiverse exists: 94%.
> This is not to say that the multiverse is on the same footing as the Higgs, or the Big Bang. Probability 94% is two sigma; two sigma effects do go away (though I factored in the look-elsewhere effect, else I would get a number much closer to 1). The standard for the Higgs discovery was five sigma, 99.9999%.

My problem with a lot of the West Coast theorists is that I don’t seem to have the same sense of humor, so often I have trouble telling when they’re joking (does anyone know when Andrei Linde is joking?). Here though it seems quite clear that Polchinski is pulling the leg of the philosophers gathered to hear him speak. My calculations show that the chance the above text could be a serious contribution to a philosophy of science conference cannot be above .1%.

The other evidence that something comical is going on here comes from some of the over-the-top claims about the virtues of string theory. In particular, we’re told

> A remarkable feature of string theory is that the dynamics, the equation of motion, is completely fixed by general principle. This is consistent with the overall direction of fundamental theory, describing the vast range of phenomena that we see in terms of fewer and fewer underlying principles. Uniqueness would seem to be the natural endpoint to this process, but such theories are truly rare…
> Indeed, when I assert that the equations of string theory are fully determined by general principle, I must admit that we do not yet know the full form of the equations, or the ultimate principle.

Polchinski is quite right that it is “remarkable” to know that you have a unique theory, with unique equations, fixed by a general, ultimate principle, but you don’t know what the theory is, what the equations are, or what the principle is. I’m curious to hear from people at the conference what gets the bigger laughs: this or Kane’s argument that the unknown theory predicts a 1.5 TeV gluino (unless it’s not found, in which case it doesn’t).

Update: See [here]() for the latest coverage of the meeting from Massimo Pigliucci, who comments at one point:

Update: For more on Polchinski’s paper there’s Sean Carroll on Twitter, [here]() and [here](), who tells us

> So strange how the public perception of string theory has been warped by a few contrarian voices. Good topic for some future PhD thesis.

and that Polchinski’s new paper

> lays out the case for string theory, and how unexpectedly successful it’s been.

I’m assuming by “contrarian voices” he’s referring to Polchinski. That string theory has been “unexpectedly successful” and the multiverse is a 94% sure thing is a highly contrarian point of view among physicists.

Update: More coverage of the workshop is available from [Sabine Hossenfelder]() and [Massimo Pigliucc]()i.

Comments

1. **Bee**
   December 9, 2015

   We are told Joe’s paper will be presented by David Gross instead.

2. **Shantanu**
   December 9, 2015

   Peter, David is presenting his talk. But he said he will disagree with Polchinski.

3. **Massimo**
   December 9, 2015

   Peter,

   the above comments are correct, David Gross will present Polchinski’s intervention (apparently, he is sick). This may turn out to be an unfortunate choice, for two reasons: i) Gross has had (far) more than his fair share of commentary already at the conference (he rarely actually asks questions of other speakers, he just gives his extended opinion on whatever they said); ii) it is going to be difficult for us to tell how much Gross will present Polchinski’s actual paper and how much editorializing he will do. I’ll report on today’s session at my platofootnote.org blog tomorrow, while my report on yesterday’s session will come out in about three hours. Cheers.

4. **Chris Oakley**
   December 9, 2015

   For some reason Polchinski’s words remind me of Cumrun Vafa’s argument (footnote 2 here: [http://arxiv.org/pdf/physics/0308078v4.pdf]()) that the reason we do not see hyper-advanced aliens around is that they will have migrated to a more happening part of the multiverse. This in a supposedly-serious scientific paper. I wonder whether the whole String Theory phenomenon is just a huge wind-up. I suppose that I have always assumed that it was, but still …

5. **M**
   December 9, 2015

   Polchinski emphasises the string understanding of black hole issues. At the conference Dvali presented his ideas about black hole issues, which are sensible and make any UV understanding (such as strings) irrelevant. What was the reaction by string theorists?

6. **AH**
   December 9, 2015

   Polchinski gave a talk which looks very similar to that arXiv post last week at the Einstein Centennial Conference in Berlin. He made the same comment about have unique equations following from a unique principle. He meant it seriously, although he also told us we were "allowed to laugh." Ashtekar pressed him on what it might mean, and the response was essentially that this was a property of certain approximate results which he expected to persist in the full theory (whatever that might be).

   We were told later in the conference that Polchinski was taken to the hospital with what sounded like rather serious symptoms. I don't know any details, but this would explain why he's not giving his talk in Munich.

   I've deleted some later comments about Polchinski's health. I hope that his health problems are not serious.

7. **vmarko**
   December 9, 2015

   Gross presented Polchinski's talk, basically read it from the paper, with very few comments of his own. No questions were asked since Joe is not here to answer (and because we were overtime). The 94% estimate for the multiverse produced a good laugh in the audience. 🙂

   Best, 🙂
   Marko

8. **andrew**
   December 9, 2015

   Peter,

   In your last post on this conference, I suggested Bayesian confirmation theory ought to be discussed, especially its relation to post-empirical confirmation (PEC), as I suspect PEC amounts to arguments for high prior probability in a particular theory.

   How sad I am read Polchinki's utterly fatous "calculation" in Bayesian probability.

9. **Bee**
   December 9, 2015

   David did a pretty good job. (Except for running badly over time.) I'll have a summary of the workshop at Starts With a Bang, but not sure how quickly Ethan will be able to get it out, today or tomorrow.

10. **Nobody**
    December 9, 2015

    Lies, damned lies and Bayesian statistics.

11. **Peter Woit**
    December 9, 2015

    Sounds like there was also some comedy from David Gross, who today was reading Polchinski's paper about the 94% probability of the multiverse, and string theory's ultimate unknown unique principle, but then explained that
    "The public is confused because there are a host of ppl who write blogs or books who attack string theory"
    https://twitter.com/skdh/status/674560758701801472

12. **Jim Baggott**
    December 9, 2015

    Peter,
    Under further probing by Sabine at the beginning of her presentation on cognitive and social bias in theory assessment (which was excellent btw) Gross said he wasn't referring to you. Heaven knows who he was referring to then…

13. **Staffan**
    December 9, 2015

    Polchinski's parody of Bayesian argumentation is interesting not least because Dawid's main argument for the "no alternatives argument" is Bayesian. One wonders if the parody is aimed at Dawid himself, which would be curious coming from someone like Polchinski.

14. **Chris W.**
    December 9, 2015

    Staffan,
    I'd guess that the intent of Polchinski's parody is to say, in effect, "we don't need no stinkin' Bayesian argumentation".

    The spurious Bayesian argumentation aside, Polchinski seems to be reiterating what has been the standard line for at least a decade. Remember when Susskind was declaring that the grumbling was to no avail, and it was time to stop worrying and love the Landscape?

    Indeed, Polchinski is arguing (yet again) that based on very general considerations about the prevalence of metastable states in more familiar physical contexts (accessible to experiment) we should have expected to end up here. That sounds to me rather like asserting in 1890 that the existence of an aether is inevitable, given everything we know about classical mechanics and electromagnetism, but it will sound convincing to many until a shift in perspective makes it seem otherwise, regardless of the problems of aether theory.

15. **Calvin Marshall**
December 9, 2015

Dr. Woit – Sean Carroll seems to be on the offensive with String Theory today. Check out his Facebook page: https://www.facebook.com/seanmcarroll?fref=nf

"So strange how the public perception of string theory has been warped by a few contrarian voices. Good topic for some future PhD thesis."

Earlier in the day he posted: "Joe Polchinski lays out the case for string theory, and how unexpectedly successful it's been." http://arxiv.org/abs/1512.02477

Wondering if he caught wind of your recent post about Einstein spinning in his grave after the PBS NewsHour interview?

16. **Peter Woit**
December 9, 2015

Calvin,
I saw those on twitter, added a mention as an update to the posting.

The Polchinski piece itself is clearly part of a PR offensive for string theory, and Carroll seems to feel the need to join in on the side of his tribe. I get the impression string theorists are feeling somewhat embattled these days, as the LHC no-SUSY results shut off the last hope for vindication from experiment, and the public perception that the theory has not been successful hardens. Not sure that going on twitter and saying it's all the fault of some blogger who has "warped" public perceptions is really going to do anything other than convince people that you're a sore loser.

It's funny to think of the situation 10 years ago when the argument was that string theory critics such as Lee Smolin and myself were just losers in the marketplace of ideas. Now that things haven't been going so well for them in this marketplace, they start complaining about market distortions…

17. **vmarko**
December 9, 2015

Maybe they feel embattled in part because of this conference. Despite the dominant presence of ST-related people, and the supportive opinions on ST by some philosophers, the unanimous conclusion of the participants today was that multiverse is nonsense. And yes, I mean unanimous, unless somebody silently lied when Gross initiated an opinion vote/poll.

The day started with an excellent lecture by George Ellis, who did a thorough analysis and the rebuttal of virtually all versions of the multiverse (save for the many-worlds interpretation of QM which is a different ballgame and was never really even mentioned seriously). There was no bashing or anything — Ellis presented in turn each multiverse idea as a legitimate scientific hypothesis, tried his best to take it seriously and interpret it charitably, and then mounted a fatal counterargument, rendering it either false or irrational. A textbook example of serious and proper scientific argument.

After Gross read/presented Polchinski's paper (and IMO did a fairly good job at that), there were several very nice lectures both on philosophy and physics. The panel discussion was "regular", basically what you would expect of any normal panel discussion, with a nice Q&A session at the end. All in all, a very nice conclusion to the event, if somewhat less vigorous than previous days. No serious controversies, only a couple of friction sparks here and there that are maybe better left unmentioned.

In the end, everybody I asked had a very positive overall opinion of the whole three-day event, myself included. Despite a few lectures that included some ST hype (the Polchinski lecture being by far the worst in that regard), most of the people expressed a very reasonable overall opinion both on ST itself and on the related philosophical issues. The conference was actually educational in the positive sense, IMO.

Best, 🙂
Marko

18. **Peter Woit**
December 9, 2015

Thanks Marko,
Too bad Polchinski wasn't there to be the lone voice for the multiverse. It really is an odd situation now, especially in the US. Here some of the most prominent theorists (either not invited to Munich or not willing to travel there), and many of the ones who most often appear in the media, act as if the multiverse is a done (or 94% done) deal. On the other hand, from your account of the conference, and my impressions from people I talk to, an overwhelming proportion of professional physicists don't think it's science (or, not successful science).

19. **Scott Church**
December 9, 2015

Peter,

I agree that the situation in the U.S. is particularly odd. If an overwhelming proportion of professional physicists don't think ST/multiverse passes for legitimate science, then the fact that some prominent theorists in America would go so far as to consider it a "done deal" demands explanation. It's one thing to see a wide range of opinions regarding some theory or idea. It's another thing altogether to see opinions sharply divided between strongly convinced, and strongly unconvinced… especially if one of these camps is a clear minority. It seems to me that in cases like this something other than science is driving things. If ST/multiverse really is a "done deal" why aren't there more than just a few American theorists who think so, no matter how prominent they are? I think it's fair to ask whether personal beliefs, agendas, or anything else having nothing to do with science might be contributing to the certainty these folks have. Thoughts?

20. **John**
December 9, 2015

Scott, I think all the multiverse love comes from a desire to think that the ultimate theory had been discovered in their lifetime. We all want to know the answer to life, the universe, and everything – we just don't want the answer to be found after we are dead.

21. **Peter Woit**
December 9, 2015

Scott,
First of all, I think it's too bad there wasn't a wider range of view represented at this conference, the deck was kind of stacked in favor of string theory and against the

multiverse, reflecting the point of view of the organizers. So, even if Marko is right and the consensus there was that the multiverse is nonsense, that doesn't necessarily reflect what is going on in theory groups, especially in the US.

My view is certainly that the side that sees the multiverse as not science has by far the better scientific case, so no need to invoke other reasons. As for what is driving multiverse mania among some theorists, I think it's a combination of factors:
1. If your interest is in having a high media profile, communicating to the public an inspiring tale of current scientific progress, then the multiverse works well: it's easy to explain, it sounds impressive, and it explains away uncomfortable questions about why the ideas you were promoting a few years back didn't work out. This is going to work much better on TV than "we're kind of stuck, haven't been making much progress…".
2. The string theory unification really has conclusively failed, and this puts people who have invested a lot of their lives in the subject in an uncomfortable position. Most of them (except for a few outliers like Kane) have given up on explaining anything about particle physics via string theory, and are concentrating on quantum gravity or connections to condensed matter, or quantum information theory, or anything else. Adopting the multiverse as an excuse in this situation is tempting: it gives you an argument for why the problem was insoluble. People then struggle with this temptation vs. knowing that this is an abandonment of science.
3. I think John has a point. Those who convince themselves of the multiverse can feel that, while they may not see a final theory, the framework they have devoted their life to working in is close to one.

22. **Bee**
December 10, 2015

(Rumor about Polchinski's health removed).

David said he wasn't referring to you, but not who he had in mind. Either way, as one of the few bloggers who write about quantum gravity I felt offended enough to defend the integrity of science bloggers. It is debatable who is to blame for much of the hype in the media, whether it's the scientists themselves, institutional press offices, or science writers. But the problem clearly doesn't have its origin in the bloggers, who most of the time just try to sort out fact from fiction. (I suspect David doesn't actually read blogs.)

23. **Jim Baggott**
December 10, 2015

I came away thinking that the conference had been a helpful meeting of minds, a chance for philosophers to set the record straight on the use of falsifiability criteria and help get past the slogans. But it was also a missed opportunity. Ellis and Silk made points in their Nature article last year that were simply not addressed. Instead, the conference was broadly configured as a way to defend string theory against the charge that it is untestable and so unscientific. Despite Kane's attempt to shoot the counsel for the defence in the foot with his absurd claims, I suspect the string theorists will be broadly satisfied with the outcome. Shifting attention to the multiverse was a clever ploy, as even astrology would look vaguely scientific in comparison (and it was indeed a great pity that Polchinski couldn't attend).

This was just an opening skirmish, I think. The success of a series of best-selling popular science books, a continuing stream of popular science articles and television documentaries, and the award of prestigious "breakthrough" prizes valued at $3 million have all helped to create the impression in the public consciousness that string theory is a valid or even "true" description of nature. This is surely not a good thing.

I was really struck by the consensus in the room that string theory is NOT confirmed or validated. In his opening presentation Gross explained that it's actually not even a theory, of the kind that can be put into a single equation and printed on a t-shirt. It is rather a "framework", a set of ideas, concepts and mathematical relationships. To become a theory it has to be set up in the right way. Gross admitted that we can test theories but it's really hard to test a framework. The trouble is that string theorists don't yet know how to set up the string framework in precisely the right way.

Setting aside the question of whether they will ever be able to figure this out, it seems to me that it would be really refreshing to see one or two leading string theorists make an honest appraisal of the status of the theory in a couple of popular science vehicles, much as Gross did at this conference. I think this would really help to set the record straight. People have all sorts of different reasons for wanting to invest belief in stuff like this, but making the position on string theory clear would in my view restore some sense of integrity.

24. **Shantanu**
December 10, 2015

For those who attended the meeting, does anyone know the name of the gentleman
who in the opening talk by David Gross mentioned that "my theory can be written on a t-shirt" after Gross said that you cannot write down the equations of string theory on a T-shirt. one thing no one brought up in the meeting is the disproportionate amount of funding given to string theorists and those who work on landscape and its very hard for people who don't work on this or LQG to earn a living. I was too tired by the panel debate,
but would have raised this issue, if I was there.
shantanu

25. **AcademicLurker**
December 10, 2015

After reading the last 2 comments, I'm wondering whether agencies that fund HEP theory should change their application process. "Send us a T-shirt with your proposed theory written on it…"

26. **Paul D.**
December 10, 2015

Report of the meeting from Sabine Hossenfelder (in SWaB):

http://www.forbes.com/sites/startswithabang/2015/12/10/why-trust-a-theory-physicists-and-philosophers-debate-the-scientific-method/

Near the end:

'But Gross has his worries: "The public is confused because there are a host of people who write blogs or books who attack string theory." As one of the few bloggers who regularity writes about quantum gravity, this remark offends me. The reality is that the biggest task of science bloggers – like Peter Woit, Ethan Siegel and myself – has become to clean up after sloppy science journalism. Hype is a real problem. But it's not the bloggers who are to blame for this.'

27. **Urs Schreiber**
December 10, 2015

In case some of the commenters above really don't know: the reason that there is not an equation defining perturbative string theory is that it is an S-matrix theory, so it isn't defined by an equation that constrains states, but by a prescription for how to compute the S-matrix elements: the string perturbation series.

28. **Joseph Conlon**
December 10, 2015

Jim: Regarding your last paragraph, I feel obliged to point out I have just written a book Why String Theory? on precisely this topic, with 250 pages on what string theory is today in 2015, what it means in 2015, and what are the various reasons people actually spend time working on it 😉 And indeed, it was published all of 2 weeks ago.

29. **Peter Woit**
December 10, 2015

Urs,
You might want to explain that point about not needing equations because it's S-matrix theory to David Gross. He was a student of Chew's doing S-matrix theory in the mid-60s. For his account of his 1966 realization that this "was less of a theory than a tautology", see here
http://link.springer.com/content/pdf/10.1007/978-1-4613-1147-8_7.pdf
I suspect he'd say the same about your claim to explain non-perturbative string theory this way.

Upon re-reading Urs's comment I realized I misread it as about non-perturbative string theory. As he wrote it, I really don't understand at all what he means. You can write down equations for perturbative string theory as a sum over worldsheets, and this isn't an S-matrix theory (or, you can compute the S-matrix, but not just the S-matrix).

30. **Peter Woit**
December 10, 2015

Joseph Conlon,

Looking forward to seeing your book. If it addresses the criticisms of string theory that for some reason got very little attention at the Munich workshop, and gives an honest appraisal of what the state of the subject is (not just hype a la Polchinski), that would be really great.

31. **Jim Baggott**
December 10, 2015

Joseph Conlon,

You'll be pleased to know your book was mentioned several times (by Fernando Quevedo during his presentation and personally to me by Graham Farmelo), and it's on my Christmas wish list. However, I'd still like to see some of the leading figures in the string theory community – Gross himself, perhaps, or Brian Greene or Lenny Susskind – make more effort to provide an honest appraisal of the current status of the theory, in much the way that Gross did at this conference.

32. **Kartik Prabhu**
December 16, 2015

@PeterWoit Some coverage of the Munich Conference: https://www.quantamagazine.org/20151216-physicists-and-philosophers-debate-the-boundaries-of-science/ You seem to have gotten some press too!

33. **David Minor**
December 22, 2015

In the version of the multi-verse I live in the probability is only 0.001%

34. **John**
December 23, 2015

Is there a good Bayesian estimate of the probability that Bayesian inference is an adequate replacement for the empirical verification of scientific theories?

35. **Jim Akerlund**
December 31, 2015

David H. Bailey & Jonathon M. Borwein weigh in on the Munich meeting in this Huffington Post article, http://www.huffingtonpost.com/david-h-bailey/data-vs-theory-the-mathem_b_8886292.html. Most of the article is a rehash of what has been said here, and the article quotes you, Peter. It is the last paragraph that stands out. *"This does not mean that all research in string theory and the multiverse must stop. But the practitioners of these fields should recognize that the chips are down: they cannot exist much longer as science if they cannot at least establish some crisp, testable connections with the real world of scientific data and analysis. They should not be given a free pass for all time."*

White Smoke Over Oxford?

December 10, 2015
Categories: abc Conjecture

I've stolen the title of this posting from Michael Harris, see his posting for a discussion of the same topic.

A big topic of discussion among mathematicians this week is the ongoing workshop at Oxford devoted to Mochizuki's claimed proof of the abc conjecture. For some background, see here. I first wrote about this when news arrived more than three years ago, with a comment that has turned out to be more accurate than I expected "it may take a very long time to see if this is really a proof."

While waiting for news from Oxford, I thought it might be a good idea to explain a bit how this looks to mathematicians, since I think few people outside the field really understand what goes on when a new breakthrough happens in mathematics. It should be made clear from the beginning that I am extremely far from expert in any of this mathematics. These are very general comments, informed a bit by some conversations with those much more expert.

What I'm very sure is not going to happen this week is "white smoke" in the sense of the gathered experts there announcing that Mochizuki's proof is correct. Before this can happen a laborious process of experts going through the proof looking for subtle problems in the details needs to take place, and that won't be quick.

The problem so far has been that experts in this area haven't been able to get off the ground, taking the first step needed. Given a paper claiming a proof of some well-known conjecture that no one has been able to prove, an expert is not going to carefully read from the beginning, checking each step, but instead will skim the paper looking for something new. If no new idea is visible, the tentative conclusion is likely to be that the proof is unlikely to work (in which case, depending on circumstances, spending more time on the paper may or may not be worthwhile). If there is a new idea, the next step is to try and understand its implications, how it fits in with everything else known about the subject, and how it may change our best understanding of the subject. After going through this process it generally becomes clear whether a proof will likely be possible or not, and how to approach the laborious process of checking a proof (i.e. which parts will be routine, which parts much harder).

Mochizuki's papers have presented a very unusual challenge. They take up a large number of pages, and develop an argument using very different techniques than people are used to. Experts who try and skim them end up quickly unable to see their way through a huge forest of unrecognizable features. There definitely are new ideas there, but the problem is connecting them to known mathematics to see if they say something new about that. The worry is that what Mochizuki has done is create a new formalism with all sorts of new internal features, but no connection to the rest of mathematics deep enough and powerful enough to tell us something new about that.

Part of the problem has been Mochizuki's own choices about how to explain his work to the outside world. He feels that he has created a new and different way of looking at the subject, and that those who want to understand it need to start from the beginning and work their way through the details. But experts who try this have generally given up, frustrated at not being able to identify a new idea powerful enough in its implications for what they know about to make the effort worthwhile. Mochizuki hasn't made things easier, with his decision not to travel to talk to other experts, and with most of the activity of others talking to him and trying to understand his work taking place locally in Japan in Japanese, with little coming out of this in a form accessible to others.

It's hard to emphasize how incredibly complex, abstract and difficult this subject is. The number of experts is very small and most mathematicians have no hope of doing anything useful here. What's happening in Oxford now is that a significant number of experts are devoting the week to their best effort to jointly see if they can understand Mochizuki's work well enough to identify a new idea, and together start to explore its implications. The thing to look for when this is over is not a consensus that there's a proof, but a consensus that there's a new idea that people have now understood, one potentially powerful enough to solve the problem.

About this, I'm hearing mixed reports, but I can say that some of what I'm hearing is unexpectedly positive. It now seems quite possible that what will emerge will be some significant understanding among experts of a new idea. And that will be the moment of a real breakthrough in the subject.

Update: Turns out the "unexpectedly positive" was a reaction to day 3, which covered pre-IUT material. Today, when things turned to the IUT stuff, it did not go well at all. See the link in the comments from lieven le bruyn to a report from Felipe Voloch. Unfortunately it now looks quite possible that the end result of this workshop will be a consensus that the IUT part of this story is just hopelessly impenetrable.

Update: Brian Conrad has posted here a long and extremely valuable discussion of the Oxford workshop and the state of attempts to understand Mochizuki's work. He makes clear where the fundamental problem has been with communication to other mathematicians, and why this problem still remains even after the workshop. The challenge going forward is to find a way to address it.

Comments

1. **Eric Weinstein**
   December 10, 2015

   Hi Peter,

   Thanks for this post. I think what is so confusing about this situation actually comes from how many mathematicians portray the field (at least to the outside world). In this fantasy description, mathematics is a uniquely objective subject and the only thing that matters is the proof. To my mind, Mochizuki is taking this portrayal quiet literally. In so he is revealing to the wider world what many Mathematicians are reluctant to admit to the public if not themselves.

   In fact, the mathematical literature is very human in that it is filled with ambiguous statements, logical gaps, wrong proofs, attribution errors. With that said, there is *something* objective about it. In general, when well regarded(1) people from well known research institutions(2) with little history of making big false claims(3) are willing to write up something explicit(4) which is not too long(5) in roughly familiar terminology(6) in English/French/Russian or other major languages(7) and talk about their work to experts in seminars(8), they trigger a process that produces a level of scrutiny which is usually equal to the task of validating the claim in a matter of days to months. There is also the perverse requirement that the author not develop too much deeply original thinking(0) on her or his way to a proof if this process is to complete quickly.

   Now, the problem here is that Mochizuki is pushing enough on various parts of 0-8 that it exposes what mathematicians know but are not always happy to admit: that the field, if it is objective, is not objective in the simple way conveyed to the world. Rather there is an economics of mathematics, a politics of math, and numerous failure modes of math which dominate the short run. In fact many senior mathematicians are of two minds where they know that the field is somehow completely objective and non-objective.

   As a former insider turned outsider, I personally think this situation is terrific. In one simple stroke, Mochizuki has showed us that the field is driven by incentives, culture, and human failing. I hope that this proof is correct so that in the second act, he can show us that after the politics, recriminations, and resistance, the field usually ends up with something pretty close to objective truth. Over the long run things are much better, at least as far as the proofs are concerned (if not always the narrative and attribution.)

2. **lieven le bruyn**
   December 10, 2015

   The mood appears to have changed today. Felipe Voloch's daily update is just in :
   ABC day 4 : https://plus.google.com/106680226131440966362/posts/LLHPN3QLoqX

3. **z**
   December 10, 2015

   Something opposite – Math Quartet Joins Forces on Unified Theory at Quanta: https://www.quantamagazine.org/20151208-four-mathematicians/

4. **Jeffrey M**
   December 10, 2015

   My two cents, as a mathematician, Peter has the process for this kind of thing exactly right. I watched it happen with Perelman's proof of Thurston's geometrization conjecture. I was at a conference maybe a week after it was released, and various experts in the field were there (my field is pretty close) – they had already been working on it feverishly the whole time. And they already were saying it looked likely to hold up. Of course that was a very different set up, since Perelman was doing work which followed in the footsteps of Hamilton (who teaches with Peter at Columbia), and basically what he did was to figure out a way to get through the wall which Hamilton hit. This seems way different, unprecedented more or less. I'm trying to think of a case where there was a completely new way of approaching something, and no one could understand, and I can't. Plenty of times someone comes up with a really new way to look at things, but it's always the case that people catch on pretty quickly, at least the examples I can think of.Usually, if a bunch of very good mathematicians look at something, and tell you it doesn't make any sense, it actually doesn't make any sense.

5. **Rubbernecker**
   December 11, 2015

   Given that

   1) Its basically in an entirely new sub field and its incredibly rare for someone to move from one area to another
   2) At least four people already understand a relatively young theory
   3) No mathematician understands the vast majority of extant proofs

   Is it maybe not that big a deal if experts working in other areas never understand this proof?

6. **Peter Woit**
   December 11, 2015

   Rubbernecker,
   This is supposed to be part of arithmetic algebraic geometry, and say new things about that subject. Many of the people at the workshop are among the best arithmetic algebraic geometers in the world.

   What went wrong yesterday is that two of the four people (Mok, Hoshi) who supposedly understand IUT were unable to explain it to anyone else. A third (Yamashita) will try today, but his talks were supposed to assume material from the other two.

   Yes, few people understand the details of many complicated proofs. As I was trying to explain here, that's not what's going on. What the experts are trying to find is a new idea that says something about arithmetic algebraic geometry.

   The danger here is that IUT is a subject disconnected from the rest of mathematics. If you spend more time studying it, you will learn lots of new definitions, be able to prove lots of theorems relating them, but learn nothing new about other mathematics.

7. **Chris W.**
   December 11, 2015

   z,
   Opposite indeed. That's an inspiring story—another great article from *Quanta*.

8. **RandomPaddy**
   December 11, 2015

   The Scottish legal system has a verdict which may be applicable in this case: Not Proven.

   The stark contrast between this conjecture and the recent, say, Polymath projects is glaring.

9. **Michael Hutchings**
   December 11, 2015

   Eric Weinstein: Your comment seems to imply that there is resistance to Mochizuki's work due to non-objective factors. There are of course various heuristics (usually with good reasons) which mathematicians use in deciding whether it is worthwhile for them to invest large amounts of time trying to understand something new. In Mochizuki's case, we have a conference of leading experts, including more than one fields medalist, getting together for a week to do their best to understand his work. One can't really say that his work isn't getting a fair hearing.

10. **Eric Weinstein**
    December 11, 2015

    Hi Michael,

    Thanks for this. I do not mean to imply that Mochizuki is running into something untoward. I mean to imply that we do a disservice to mathematics when we confuse the objective nature of the underlying subject matter with the way in which humans attempt to do mathematics. I don't mind the heuristics if they are recognized not to be fundamental truths.

    Of course there is a resistance here. He is (to the best of my understanding) not bending over backward to decrease the burden on the proof checkers. I would find that annoying were I an expert in this field. But I would never pretend that math is uniquely objective as a profession. Proofs may be objective, but proof checking is not.

    Let me make an analogy. In computer software, there is a difference between a code review done by a subjective human doing their best and a code review done by the compiler. What I find at turns amusing and annoying is when mathematicians pretend to be compilers. The compiler doesn't care about commenting code properly. It doesn't see whether a style guide has been adhered to or whether readability mattered to the developer. It just compiles or it fails.

Mochizuki seems not to care particularly for decreasing the burden on his proof checkers. For the subset of those proof checkers who are open about how prone to error and delay this process is, I believe he is acting sub-optimally and should be pushed to be more helpful. But, for those mathematicians who pretend that the profession is blind to all but the objective truth….I think this is a splendid reveal. No compiler takes this long.

Warmly,

Eric

11. **Peter Woit**
December 11, 2015

Eric,
What I was trying to get at in my posting is that "proof checking" isn't really what this is about (at some later date "proof checking" comes into play, but other things have to happen first: you can't sensibly check a proof you don't understand). What mathematicians actually do is something much less mechanical, trying to individually embody understanding of mathematical ideas, and share this understanding as a community. The problem here is that Mochizuki is not successfully communicating mathematical ideas to the rest of the community. "What we've got here is failure to communicate…" Why this is happening is a fascinating question, involving both the standard ways the community operates, as well as some very unusual special features of this case. I don't actually know of any other comparable example, and also it's not at all clear where this is going to end up.

12. **sdf**
December 11, 2015

Mochizuki has made minimal effort to engage with the community at large. In that case, workshops of this sort set a dangerous precedent. Simply the fact that he has already demonstrated that he is a first rate mathematician means that we should take this thing seriously without a real and proper involvement is silly. Until he begins making more of an effort it should be ignored. And the description given by Veloch of Kuehne's talk on day 2 shows, I think, that this thing is an embarrassment to the organisers and to Oxford by proxy.

13. **jsm**
December 12, 2015

There appears to be a black hole: if someone understands Mochizuki, then *he* can't be understood by anyone else.

14. **Gavrilov**
December 12, 2015

"The danger here is that IUT is a subject disconnected from the rest of mathematics."

This is not unprecedented. In 1968, P. S. Novikov and Adian published a
(negative) solution of the famous Burnside problem. The proof was more then 300 pages long, which is impressive but not unheard of even in 1960s. Its peculiarity is that it does not use any standard methods; the authors pretty much developed a whole new theory from scratch.

Not being an expert, I can't say to what degree this theory (extremely hard to master)
is detached from the rest of mathematics. But it looks like this far it has only been used in a very narrow area of research, more or less close to the original problem.

Admittedly, the "Bernside theory" is built on a rather elementary basis, while Mochizuki started from the height of arithmetic geometry, but I would not call it a radical difference.

If the analogy, and the proof itself, is correct, then eventually we will see half a dozen of experts in this theory who understand it, while the rest of mathematicians, even from close fields, won't bother. Not terribly promising, but we do not always have what we dream of, in life or in mathematics.

15. **Chris Austin**
December 12, 2015

Under an earlier post, a commenter said that Mochizuki is a family name associated with the samurai class.

A Scientific American article, "Japanese Temple Geometry," by Tony Rothman, with assistance from Hidetoshi Fukagawa, May 1, 1998, says that during Japan's period of national seclusion, (1639 – 1854), there was a tradition in which samurai and others would prove mathematical theorems, usually about Euclidean geometry, and inscribe them on delicately colored wooden tablets called sangaku, and hang them under the roofs of temples, as an offering to the ancestors. Perhaps Mochizuki views his work in this spirit.

16. **Timothy Gowers**
December 12, 2015

I am a complete outsider to this area, but it seems to me that a very important thing that is lacking is some kind of story that explains why Mochizuki's machinery might be appropriate for proving the ABC conjecture. If I contrast it with another area I don't understand — Grothendieck-style algebraic geometry — the latter comes out far more favourably (in this one respect — I'm not talking about the fact that it has obviously been checked by thousands of mathematicians), because there are all sorts of accounts of how the more abstract way of looking at varieties is a fruitful thing to do. I know that if I did want to learn it, I wouldn't be told that I had to become comfortable with a vast array of complicated definitions before any benefits fed back into what I already know about.

The case with Perelman was again very different: I don't understand his proof at all, but I did understand accounts for the non-expert that explained about Ricci flow and what it was supposed to achieve.

What I would want to see from Mochizuki and his followers is a baby result that can be proved by his methods, that points the way towards more complicated ones.

17. **math_lambda**
December 12, 2015

On this Mathoverflow page Minhyong Kim, who is one of the organizers, has just written the following :

Update (12 December, 2015): I've written a brief summary of the Oxford workshop on IUTT rather rapidly, so as to save people the trouble of circulating rumours. This seemed to be a reasonable place to put it. All errors in it are my own: http://people.maths.ox.ac.uk/kimm/papers/iutt=clay.pdf

18. **vmarko**
December 12, 2015

That link seems to be broken (also on the mathoverflow page)?

Best, 🙂
Marko

19. **Peter Shor**
December 12, 2015

The link is probably only temporarily broken … Kim explains it in MathOverflow; he took it down while getting permission to quote Mochizuki.

20. **Gavrilov**
December 12, 2015

Gowers,

There are cases when it is impossible. The Novikov-Adian theory I mentioned
is an example of a huge machinery designed specifically for an extremely
difficult but very narrow target, which is difficult to use for anything else.
No baby results there.

As I know nothing about IUT I can't say if it is the case, but it may be
a possibility.

21. **Timothy Gowers**
December 13, 2015

Gavrilov, that's very interesting, but it raises an obvious question. If the only way to hit a narrow target is via a huge, elaborate, and seemingly irrelevant theoretical apparatus that has no other applications, then how does anybody discover that apparatus in the first place? There must be some story. The least Mochizuki could do is tell us that story. What is the story in the Novikov-Adian case? It cannot be that, just for fun, they developed an incredibly complicated theory and then observed to their great surprise that it solved precisely one interesting problem. Sometimes the story is that a theory is developed for another purpose but turns out to be useful for the given problem, but you imply that that is not the case for the the Novikov-Adian theory.

I realize that your point is that there aren't baby results along the way. Maybe I should generalize my requirement and say that there should be a path from not understanding the theory at all to understanding it completely that does not require huge leaps of faith that there is some point to what one is learning.

22. **Klas M.**
December 13, 2015

I'm far from an expert on the Burnside problem but describing the proof as isolated and without baby results does not seem correct to me. As I have understood it Novikov and Aidan set out to understand and classify periodic words and cancellation in groups and as a result of that work could prove that there are infinite Burnside groups. So before they got to their famous result they also had relevant "smaller" results.

23. **Gavrilov**
December 13, 2015

Gowers, you ask questions I would like to know the answers myself.

Apparently, the idea of a solution first came to Novikov in 1950s, but
I do not know how far it was from the final theory, and what was the *story*.
Much less what is the path "from not understanding the theory at all to
understanding it completely" in this case.

These are interesting questions, but probably they could only be answered by an expert.

24. **Gavrilov**
December 13, 2015

For those who are interested, I have found a nice piece about the solution
of the general Burnside problem on Mathoverflow, by Mark Sapir.
http://mathoverflow.net/questions/48184/a-synopsis-of-adyan-s-solution-to-the-general-burnside-problem
In particular, it is said that there is no "short description" of Novikov-Adian work.

25. **Yatima**
December 14, 2015

Thank you Chris Austin for the sangaku reference. That was an interesting read:

"Many of the problems are elementary and can be solved in a few lines; they are not the kind of work a professional mathematician would publish. Fukagawa has found a tablet from Mie Prefecture inscribed with the name of a merchant. Others have names of women and children—12 to 14 years of age. Most, according to Fukagawa, were created by the members of the highly educated samurai class. A few were probably done by farmers; Fukagawa recalls how about 10 years ago he visited the former cottage of mathematician Sen Sakuma (1819–1896), who taught wasan (native Japanese mathematics) to the farmers in nearby villages in Fukushima Prefecture. Sakuma had about 2,000 students….

The best answer, then, to the question of who created temple geometry seems to be: everybody. On learning of the sangaku, Fukagawa came to understand that, in those days, many of the Japanese loved and enjoyed math, as well as poetry and other art forms."

Sorry Peter for the off-topic posting. Please remove if inappropriate.

26. **Gavrilov**
December 14, 2015

Gowers, using your terminology from “Two cultures”,
the work of Novikov and Adian is much closer in spirit to problem solving then
to theory building. This is why there is, apparently, no way to describe it in two words.
There are other examples of this sort, although less extreme, such as the Feit-Thompson theorem.
(But probably there are reasons to call this work a new theory, albeit an odd one,
and not a huge collection of tricks. It may be systematic in its own way.)

My point is that when you are focused on a single extremely difficult problem,
then no matter where you started, you have an (unfortunate) chance of producing something as incomprehencible as the Novikov-Adian proof.

27. **David Roberts**
December 14, 2015

@Tim Gowers

I think Mochizuki *has* been telling the story of how he came to think of his ideas, and how one should think of his work as approaching the solution: by analogy with other theorems and so on. The problem is that this other, existing, body of work requires something that doesn’t exist in the arithmo-geometric world, and this is what his theory is designed to give, at the expense of catapulting out of the usual techniques and objects.

The problem is, there’s too much analogy (“alien arithmetic structures” and so on) and less middle-ground explanation before one gets to pages and pages of definitions. I think Lieven le Bruyn did a great job of working through what a Frobenioid is, in a simple and known case. Such unpacking is something Mochizuki didn’t do; clearly somebody or some collection of somebodies needs to go back to the precursor papers and fill in all the worked examples that are absent. This is for me the clearest way forward, and how to approach the massive wall of definitions with something like a climbing strategy.

28. **Rh L**
December 14, 2015

@Tim Gowers

I think Mochizuki has attempted to tell the story of how he came to develop his ideas in the slightly more expository piece, A Panoramic Overview of Inter-universal Teichmuller Theory, available on his website:

http://www.kurims.kyoto-u.ac.jp/~motizuki/Panoramic%20Overview%20of%20Inter-universal%20Teichmuller%20Theory.pdf

As far as I can tell, and this is with the caveat that I could be very wrong in such a brief space, this grew from Mochizuki’s proof of a conjecture of Grothendieck in anabelian geometry. One of the first things he seemed to have done, after proving Grothendieck’s conjecture, was to build an analogue of Hodge theory for Arakelov geometry, which he called Hodge-Arakelov theory and wrote about in papers in 1999 and 2002.

In the introduction to Panorama, he characterised the current theory as the result of trying to overcome the difficulties of applying scheme-theoretic Hodge-Arakelov theory to diophantine geometry. The resulting theory appears to be (in part) a theory of non-scheme-theoretic deformations, i.e. a theory that presumably involves geometric structures that go beyond schemes.

29. **random reader**
December 14, 2015

(Rh L) “[he built] an analogue of Hodge theory for Arakelov geometry, which he called Hodge-Arakelov theory and wrote about in papers in 1999 and 2002. ”

That’s strange, because it would have been more important than proving ABC if he had succeeded. Building a working machinery that unifies Hodge theory with its number theoretic analogue (etale cohomology and site, l-adic Galois representations) has been a central goal in algebraic geometry for the past 50 years. It would have been the news of the millenium had somebody done this.

The more limited goal of building a more adelic Arakelov geometry, has been around since the late 1970’s, and also considered very important. It is a fearsomely technical subject in ways that run in a different and much less algebraic direction than anything Mochizuki is known to have published or studied. If he had surmounted the difficulties (1) at all, and even better, (2) using his methods that don’t rely on heavy doses of modern differential geometry and analysis, that would be considered a titanic achievement. It would have been noticed and recognized in the past 15 years.

That comes to one of the weird points about the Mochizuki ABC papers: where in those documents is there any of the hardcore analysis that one would expect, relating the very general algebra to the analytic number theory problem that is ABC? One would expect at least a page or two (or fifty) of grungy estimates and hard analysis at least for getting a weak form of ABC that might be boosted to full ABC by more algebraic arguments. Mochizuki does seem to use the latter, but there isn’t much sign of hard analysis in his papers, and one should be able to find it just by skimming if it’s there. This is a question that must have come up in various forms at the Oxford conference — where is the hard work being done? — and it would be a big confidence builder if the believers would just point to the locations in Mochizuki’s papers where the analytic-number-theory part of the action takes place. The idea that it can be black-boxed into a few lines of analysis and 500 pages of algebra doesn’t sound right.

Having said that, I think the sociological concerns about not leaving Japan to explain the proof, the papers not having been refereed, etc, have been overplayed. The papers contain plenty of motivation and exposition that is illuminating apart from the stated goal of proving ABC. They are quite discursive compared to anything else in the field of arithmetic geometry, which has more than its share of long dense papers, and are not in the impenetrable dense theorem-proof style.

(can’t get the formatting to work when posting with firefox, sorry about that.)

30. **lieven le bruyn**
December 14, 2015

@David Roberts, thanks for the nice words. I only “checked” one paper as a non-specialist, got stuck and then discovered the wealth hidden in the Arakelov bit. Just the same, if a student would hand in Fobenioids1 as a paper, she’d have to rewrite it seriously.

Sad to see that some refer to my blog as criticising Mochizuki (or even calling his work ‘nonsense’), most recently at “Todeszone der Mathematik”. I’m just getting tired of his lack of interest in reaching out.

Also sad to see that Minhyong Kim did not (yet) put his report on last week’s Mochizuki-Fest in Oxford back online. I learned a lot from it. Anyway, luckily there’s always the mysterious @math_jin Twitter account to repost images of ‘lost’ files.

As my own ‘lost’ blog is getting more hits recently I’ve put a little story online about the Log Lady and the Frobenioid of Z. It’s about the Arakelov bit, but probably only digestible if you did see Twin Peaks, the log lady, and Norma at the Double R Diner, way back then…

31. **Brian Conrad**
December 14, 2015

@random reader: The “hard analysis” you’re looking for is contained entirely in the known equivalence (from several decades back) between Szpiro’s Conjecture for elliptic curves and the ABC Conjecture when each is formulated over general number fields (proof going via consideration of Frey curves associated to an ABC triple and a robust variation of the ground field).

Everything Mochizuki is doing is focused on proving Szpiro’s Conjecture for all elliptic curves over all number fields. Moreover, it is in the nature of the method that his main work is in the case of elliptic curves satisfying certain auxiliary local and global properties that necessitate working over a somewhat large number field (and the general case is then deduced by a very short argument); in particular, his method does not work directly over Q in the main parts.

For the same reason, one cannot get some insight into Mochizuki’s methods by trying to unravel what they are saying in the context of some of the other known concrete consequences, since his entire proof takes place in the setting of Szpiro’s Conjecture (whose link to the known consequences via ABC goes through long-known arguments which treat Szpiro’s Conjecture as a black box).

It is somewhat akin to the fact that Wiles’ proof of Fermat’s Last Theorem works not with the Fermat equation, nor even with elliptic curves over Q (for which general modularity in the semistable case was sufficient to apply to hypothetical Frey curves), but rather with Galois representations and modular forms, which in turn admit powerful operations having no interpretation in terms of elliptic curves (let alone the Fermat equation).

That is, one cannot get insight into Wiles’ method by thinking solely about the more concrete framework of elliptic curves (because spaces of weight-2 modular forms with a given level generally have Hecke eigenvalues that are not rational, so not all eigenforms in the space are related to elliptic curves).

32. **random reader**
December 14, 2015

Brian Conrad,

thanks very much for the comment. Good to see the big guns weighing in here.

I don’t see how Szpiro and ABC differ here, or how the lack of visible hard analysis is comparable to Wiles’ proof of Fermat’s Last Theorem.

In Wiles’ work on FLT, the analytic objects he was showing to exist were known to have a rigid algebraic description with rich properties, and conjectured to satisfy a relatively precise (Langlands) equivalence between the algebraic and analytic sides of the coin. Wiles made a breakthough on the Galois (algebraic) side and consequences on the automorphic (analytic) one flowed, but this transfer of results was not itself the novelty of his work. The relations to analysis and the automorphic side of Langlands philosophy were, if vague memory serves, encapsulated in the use of some results from Tunnell’s work on icosahedral(?) representations. Correct me if that’s wrong, it is surely in your line of expertise and not mine. But the point is that a sufficiently precise translation to the non-analytic setting was already known and Wiles used it, maybe with some new twists, but the main action was in the algebraic theory, deformation of Galois representations, the commutative algebra of Hecke rings, and the patching argument with auxiliary primes.

It is a reasonable and important question to understand what Wiles’ proof does at the level of concrete objects such as coefficients of modular forms, since he is ultimately proving an existence theorem for concrete objects and it is a bit outre if there is no way to describe in principle how the machinery unpacks to some sort of complicated manipulation of those objects. Asking the equivalent about Mochizuki’s work does not strike me as a form of confusion or category error, but a basic conceptual point that (if answered) would clarify what is happening in his papers.

Returning to Mochizuki’s proof and the absence of visible hard analysis there:

Both the Szpiro conjecture and ABC are analytic conjectures, one about numerical invariants of elliptic curves and the other about invariants of pairs of integers (or infinite families of either type of object, and the extension to number fields). In both cases one would expect a proof to include some sort of nontrivial estimation process involving inequalities, real/complex/harmonic analysis, L-functions, differential equations, etc to take place in order to obtain conclusions. Mochizuki works with some version of theta functions, which (maybe in a different setting) were known for a long time to have a more algebraic description, so to an extent there is an algebraization of the things to be proved, but I am not aware of any purely algebraic statement that is known to imply ABC or Szpiro or Vojta conjectures. His papers do involve some estimates, but rather short ones that make up very very little of the content of the papers. This is not the distribution of labor one (or this one commenter) would expect in a paper that purports to accomplish an amazing feat in what is ultimately analytic number theory.

In brief, even taking Szpiro/ABC equivalence as a black box, which might or might not contain a lot of hard analysis, it’s hard to see how the Szpiro part can then be proved without lots of additional hard analysis. Perhaps Mochizuki shows that there is so much uniformity in the way the elliptic curve invariants vary, that easy estimates will do. But even this would require some strong results interconnecting the algebra and the analysis and at some point estimates seem likely to intervene.

33. **Brian Conrad**
December 14, 2015

@random reader:

Perhaps I was unclear in the analogy I was trying to make. The aspect of the proof of FLT I was alluding to was just that even though the statement of most primary interest is about showing that a specific equation has no Q-solution or that a specific q-series with Q-coefficients is in fact a modular form, the actual context for the argument must take place (as you are aware) with Hecke operators acting on spaces with eigenvalues outside of Q and with Galois deformation rings, neither of which can be “interpreted” entirely in terms of an initial more concrete structure of interest (such as a Diophantine equation or a specific q-series or a specific elliptic curve over Q).

That is, it was just meant as an illustration of the well-known fact that to prove a theorem of interest about a concrete thing we may need to enlarge the scope of the problem and then could lose the ability to “interpret” the core ideas of the proof in terms of operations involving just the original concrete thing. An expert in analytic number theory asked me recently how to unravel Mochizuki’s arguments in the context of some other concrete consequences, and I had given a related explanation for why that couldn’t be done and that this isn’t a danger sign at all, and so I tried to import the same explanation for my original reading of your question: I had mistakenly thought you were specifically asking about trying to see where in his arguments he is getting his hands dirty with estimates involving ABC-triples. You won’t find it in that form because he never works with ABC-triples.

In the context of Szpiro’s conjecture, he also doesn’t apply IUT to any old elliptic curve, but has to assume several local and global properties which are always attained after a finite extension of the ground field with controlled degree. So it is an essential feature of his technique that he is permitting rather general ground fields, and in fact the conditions he needs can’t ever be fulfilled over Q.

In the end he is going to aim to prove Szpiro (for a given epsilon, with elliptic curves satisfying some specific local and global properties) with a constant depending on the ground field only through its Q-degree, so it is kosher for him to making extensions of controlled degree in the middle of the argument. There is a separate “short” argument using Belyi maps that reduces the general case of Szpiro to the ones he actually handles in the IUT machinery, but this latter argument is a clever proof by contradiction that makes things ineffective roughly as in Roth’s theorem.

To tell you where the “analysis” yielding an inequality should be found, I need to say something about what is going on in his method (for which I only have an impressionistic awareness based on some lectures at the Oxford workshop last week). He uses serious algebro-geometric constructions with p-adic theta functions to make cohomological constructions that encode some local numerical invariants arising in Szpiro’s conjecture (for an E satisfying the local and global hypotheses alluded to above) in terms of a special kind of fibered category (arising from E – {0}) called a Frobenioid. This encoding involves a controlled ambiguity after accounting for variation of choices made in the construction (i.e., what is intrinsic is not a specific cohomology class, but rather a certain coset by a controlled subgroup of an ambient cohomology group on a certain fundamental group). The full force of Mochizuki’s work on the anabelian properties of hyperbolic curves is used to show that everything which just happened can be expressed in terms entirely intrinsic to a Frobenioid without any reference to the original elliptic curve.

The purpose of encoding number-theoretic data (with controlled error) in terms of Frobenioids appears to be that Frobeniods admit additional intrinsic operations (such as a weak version of “Frobenius maps”) which one can’t express in terms of the original geometric objects (such as punctured elliptic curves). By analyzing how the cohomological constructions interact with those operations (this involves introducing yet more abstract notions, such as “Hodge theaters”) eventually after a lot of work he arrives at two bounded domains in an R-vector space, one domain inside the other, and comparing their volumes (which have to be computed!) gives the desired logarithmic form of the inequality with an “error term” (arising from various ambiguities in the constructions) that is the desired uniform constant if one can exert sufficiently precise control on it uniformly in the original elliptic curve. So it is in this final step of computing volumes with an “error term” that your sought-after “hard analysis” should be found.

But the constant obtained in that way isn’t the one for Szpiro’s Conjecture (for a given epsilon) for all elliptic curves over number fields of controlled degree! It is only suitable for elliptic curves satisfying a specific list of local and global properties. To bootstrap this back to general elliptic curves over an original number field of interest, one needs to go through a proof by contradiction as mentioned above, so in the end the final constant is ineffective (but depends on just epsilon and the Q-degree of the original ground field). So one recovers Mordell but not effective Mordell.

34. **Rh L**
December 15, 2015

Wow, I didn’t expect my comment to have sparked off such an excellent discussion.

@Brian Conrad: Thank you for the illuminating comments and for the excellent notes on the Oxford IUT Workshop you’ve posted here (via the “mysterious” @math_jin):

http://mathbabe.org/2015/12/15/notes-on-the-oxford-iut-workshop-by-brian-conrad/

@Lieven le Bruyn: I’ve also very much appreciated your expository work on Frobenioids. Thank you for pointing out the @math_jin account, it would seem to be very useful at this stage.

@random reader: I was going to chime in on your comment to my remark on Hodge-Arakelov theory, but I think Brian Conrad has addressed that in his notes, i.e. that what was produced in the earlier papers would not have lived up to your expectation of what a “Hodge-Arakelov theory” would be. I think Mochizuki had said as much in the introduction that I was paraphrasing.

35. **Peter Woit**
December 15, 2015

For those following comments here, but not updates, you should be reading Brian Conrad’s report on the workshop here
http://mathbabe.org/2015/12/15/notes-on-the-oxford-iut-workshop-by-brian-conrad/

36. **David Roberts**
February 9, 2016

Commenting because I can, and because it’s not true that people don’t read old comment threads[1]: they are an important source of a) historical information b) inside information. The number of times I’ve searched for things mathematical and found blog discussions on it that I found excellent…

I also wanted to comment on the mathbabe thread after it was closed, to prove up-to-date links for people trawling the interwebs for the history of this interesting episode.

[1] Sure it was a generalisation: I had assumed though that old comment threads were closed to prevent spam. The suggestion of starting a new blog to continue the discussion baffles me.

37. **Peter Woit**
February 9, 2016

David Roberts,
The main reason is because of spam, but more specifically because after a certain period the ratio of non-spam/spam comments becomes quite small, and the great majority of traffic to the postings is spambots trying to break in.

LHC Run 2 First Results

December 14, 2015
Categories: Experimental HEP News

First results using the full data from Run 2 at 13 TeV will be presented tomorrow at CERN at 15:00 Geneva time, with a live webcast available here. For some relevant commentary, see Tommaso Dorigo and Matt Strassler.

Among relatively reliable rumor sources, Jester is tweeting about a supposed excess at 750 GeV in the diphoton spectrum. We'll see tomorrow, but the problem with this is that it would be hard to understand why such a thing didn't show up in Run 1 at 8 TeV. Tommaso explains why it is only at higher masses that one expects the Run 2 data to be competitive with that from Run 1, and suggests that what to look for is a 2 TeV excess in the dijet spectrum, since there were already hints of such a thing in the Run 1 data.

Matt describes 13 TeV results recently published by the ATLAS and CMS groups looking for exotic behavior at very high mass (predicted for instance by various theories of extra dimensions). Nothing there.

One other thing to look for is whether Gordon Kane will get his Nobel Prize. He's predicting a 1.5 TeV gluino, with current limits around 1.4 TeV, and this year's Run 2 data perhaps enough to push those up a bit. It may though be that such analyses will be among those that take longer, not appearing until the Moriond conference in March.

Update: CMS went first, results now publicly available here. Tommaso was pulling our leg, the 2 GeV Run 1 excesses have gone away. There is a diphoton excess at 766 GeV, but an unimpressive one (2.6 sigma locally, 1.2 sigma with look elsewhere effect).

Gluino mass limits have moved up, some as high as 1.7 TeV. Presumably Kane is now at work on new string theory predictions.

Bottom line: nothing beyond the SM so far.

Update: ATLAS next. No gluinos up to 1.8 TeV. 2.2 sigma for the 2 TeV excess that CMS doesn't see.

They also see an excess in diphotons around 750 GeV, 3.6 sigma local significance, 1.9 sigma with look-elsewhere. So, starts to look interesting if combined with CMS, the rumor was right. They also reanalyzed the Run 1 data, nothing there at 750 GeV, no combination of Run 1 and Run 2.

Results available here.

Bottom line: only thing interesting is the possible 750 GeV diphoton excess. One can predict a flood of theory papers with models predicting such a thing. Will have to wait until at least next summer though to see if this gets confirmed or goes away.

Commentary from Matt Strassler here.

Update: As expected, best explanation and discussion of the implications of the diphoton excess is from Jester, see here.

Reasons to be excited: naively combining CMS and ATLAS gives something of 4 sigma significance, people are making the analogy with the early Higgs signal. Reasons to be less excited: in the case of the early Higgs signal, the tentative signal was what was expected from the Higgs, and we had very good reasons to believe there was a Higgs roughly in that mass range. Here I know of no well-motivated models that predict this: extraordinary claims require extraordinary evidence, and this is not that.

Comments

1. **Tom Weidig**
   December 15, 2015

   Lubos is offering bets. May be you should take it if you believe in a non-discovery.

   http://motls.blogspot.lu/2015/12/a-new-750-gev-boson-is-very-likely-there.html?utm_source=feedburner&utm_medium=feed&utm_campaign=Feed:+LuboMotlsReferenceFrame+(Lubos+Motl%27s+reference+frame)

2. **Peter Woit**
   December 15, 2015

   Tom Weidig,
   Not interested in betting against this (especially not at 10-to-1 odds). I do think the likelihood is now stronger that this will go away than that it will survive, but it would be fantastic if this were true: the non-standard model physics we've been waiting to see for 40 years, at an energy where the LHC can start to study it.

3. **vmarko**
   December 15, 2015

   If the 750 GeV event eventually turns out to be really a new particle, I am sure that Gordon Kane will easily modify his ST model such that it… hmm… will have had predicted… precisely that particle, several years ago. 🙂

   And if you ask him how is that even possible, he'll give the (by now famous) answer: "go and read my paper, it's all written there."

   Best, 🙂
   Marko

4. **NeapTide**
   December 15, 2015

   Nice, hope it is really true.

   LUX (the dark matter experiment) also published a nice paper this week. They seem to have calibrated way better than ever before, and aren't seeing the low-mass WIMPs that were claimed.

5. **Peter Woit**

December 15, 2015

Marko,
I don't think Kane has shown interest in "predicting" unexpected things like this. So far his claims have been about string theory "predicting" the Higgs, and "predicting" that the Higgs will behave like the standard model says, as well as a long history of "predictions" of superpartner masses.

A couple interesting things to watch for over the next year:
1. Once Kane moves his "prediction" of the gluino mass up again in coming months, who will invite him to give a talk explaining why string theory "predicts" a 2 TeV gluino, and not mentioning any of the earlier string theory "predictions" of the gluino mass that marched up from a few 100 GeV years ago to 1.5 TeV in Munich recently? Will the organizers of the Munich conference publish in their proceedings the news of the falsification of string theory? Will Physics Today publish an update on the "supertestability" of string theory?

2. Any reasonable hope for vindication of SUSY at the LHC is rapidly disappearing, at what point will all those besides Kane who advertised SUSY at the LHC as a "test of string theory" admit failure and face up to the implications of this failure?

6. **Ralph Dratman**
December 15, 2015

"One can predict a flood of theory papers with models predicting such a thing."

Does your theory of theory paper flooding give any quantitative predictions? There might be some surprisingly energetic papers in flight right now, from a University far, far away.

7. **Cosmonut**
December 15, 2015

"Gluino mass limits have moved up, some as high as 1.7 TeV. Presumably Kane is now at work on new string theory predictions."

In an universe where string theorists are not continually revising their predictions, they do not exist because nobody was willing to sponsor a program of research which promised a theory of everything and delivered nothing after 40 years.

This is a demonstration of how useful the Anthropic Principle is for explaining properties of the universe that we observe. 🙂 🙂

8. **RandomPaddy**
December 15, 2015

How many times has a maybe-maybe-not bump like this appeared before? Are there examples where it did and didn't result in a new particle being ultimately confirmed?

9. **Peter Woit**
December 15, 2015

Random Paddy,
Jester refers in his posting to early hints of a Higgs that disappeared
"It is a very large effect, but we have already seen this large fluctuations at the LHC that vanished into thin air (remember 145 GeV Higgs?)."
I think one can probably come up with quite a few such examples, there's a reason that people in this field have adopted the "5 sigma" standard. In this case it's tricky how to take into account the two different experiments (they haven't combined their data), as well as the fact that you're looking at lots of different channels, so increasing the probability of seeing a fluctuation in one of them. I doubt that at this point the statistical significance of this is near 5 sigma. In addition, the fact that this doesn't correspond to anything theoretically well motivated argues for added caution (and also, added excitement if it works out).

Run 2 and SUSY

December 15, 2015
Categories: This Week's Hype, Uncategorized

What surprised me most about today's Run 2 results (see here) was that CMS and ATLAS were able to already significantly push up limits on superpartner masses, especially the gluino mass. Limits on the gluino mass went from 1.3-1.4 TeV in Run 1 to something like 1.6-1.8 TeV in the new Run 2 data (this depends on exactly what channels one is looking at). This not only kills off Gordon Kane's string theory prediction of a 1.5 TeV gluino, but it also removes a large chunk of the remaining possible mass region that the LHC will be able to access. And it wasn't just the gluino: ATLAS quoted limits on sbottom masses moving up from 650 GeV in Run 1 to 850 GeV today. Whatever you thought the remaining probability was for SUSY after the negative Run 1 results, it's significantly smaller today.

Almost all the news has been about the possible diphoton excess, ignoring the quite significant story about SUSY. Davide Castelvecchi at Nature though today talked to Michael Peskin, who has been one of the more consistent proponents of SUSY over the years, and this was part of his story:

> Meanwhile, searches for particles predicted by supersymmetry, physicists' favourite extension of the standard model, continue to come up empty-handed. To theoretical physicist Michael Peskin of the SLAC National Accelerator Laboratory in Menlo Park, California, the most relevant part of the talks concerned the failure to find a supersymmetric particle called the gluino in the range of possible masses up to 1,600 GeV (much farther than the 1,300-GeV limit of Run 1). This pushes supersymmetry closer to the point where many physicists might give up on it, Peskin says.

I had thought that the "physicists give up on SUSY" story wouldn't get going until next year, but maybe it's already started.

Update: In just a few hours after the announcement already 10 papers on hep-ph devoted to explaining the diphoton resonance. SUSY explanations not among the popular ones.

Update: Another eight or so papers explaining the diphotons. And the press has the obvious explanation: string theory:

The idea seems to be that since people were looking for Randall-Sundrum gravitons (which somehow counts as string theory) then if they find something in the diphoton spectrum it could be a graviton. I'm no expert, but none of the dozens of hep-th papers seem to discuss this possibility, and the papers about searches for Randall-Sundrum gravitons (like this one) set limits way above a TeV. On the other hand, I don't doubt that some "string vacuum" can be found that will explain the diphotons, and that we'll hear more about it in the press.

Comments

1. **Noboru Nakanishi**
   December 15, 2015

   SUSY cannot be a (spontaneously broken) symmetry of the elmentary-particle physics. This proposition is a logical consequence of quantum Einstein gravity. It is quite surprising that there are still many people who do not want to give up SUSY!

2. **Brandon Enright**
   December 16, 2015

   Is there any chance at all that this 750 GeV bump could be the boson super partner to a SM fermion? I take it diphoton decay is not the expected SUSY signal?

3. **Peter Woit**
   December 16, 2015

   Brandon,
   I'm sure one could come up with some kind of SUSY model to explain this, but it's not what's expected in simpler such models.

4. **Bernhard**
   December 16, 2015

   Peter,

   But isn't electroweak SUSY production still not the last hope? I remember in these searches the gluino mass is assumed very high and "decouples" from the LHC phenomenology. The cross section for electroweak production is lower, so I would consider these scenarios to be still possible (not that I personally believe on them). I know ATLAS and CMS have produced limits there too, but should not be competitive with strong production.

5. **Peter Woit**
   December 16, 2015

   Bernhard,

   Sure, the easiest thing to see is strongly interacting superpartners, and it's only those that the LHC is now putting new bounds on. Maybe all the strongly interacting ones are very massive, but there are electroweak ones you can detect with enough luminosity. Very much worth looking for, especially because you might find something else interesting. But, I don't know of any serious motivation for the idea that strongly interacting superpartners are very massive and decouple, other than a desire to avoid conflict with what experiments are now telling us.

   I suppose I should look at materials from people promoting the ILC, I think that looking for such things is one of the arguments for that kind of machine.

6. **David**
   December 16, 2015

   I believe that the SUSYers can "explain" the diphoton excess in the next few days. 🙂

7. **Peter Woit**
   December 16, 2015

   David
   Already done, see

http://arxiv.org/abs/1512.05333

Also, it seems that string theory can explain this, see
http://arstechnica.co.uk/science/2015/12/first-high-energy-lhc-results-supersymmetry-still-dead-watch-for-gravitons/

8. **Luka**
December 17, 2015

Brandon Enright: *Is there any chance at all that this 750 GeV bump could be the boson super partner to a SM fermion?*

But wouldn't this violate the R-parity?

9. **Douglas Natelson**
December 17, 2015

I've heard grumbling (Strassler alludes to this) that ATLAS didn't combine with their Run 1 results (which would have lowered the significance considerably, since there seems to be no hump in Run 1 near 750 GeV). Any word on the reasoning here, since it's presumably not naive cherry-picking?

10. **Peter Woit**
December 17, 2015

Douglas Natelson,
I'm no expert, but I've heard two problems with such a combination are
1. Backgrounds are different at 8 and 13 TeV.
2. Even if there is a new 750 GeV state, you don't know what produced it, can imagine that production rates are quite different at 8 and 13 TeV. One way this could happen is if this new state is the result of a decay of something even heavier, heavy enough so that there's a big difference in production cross-section.

11. **AS**
December 18, 2015

The problem with RS gravitons is that they predict
sigma(pp->gamma gamma) = sigma(pp-> e+e- + mu+ mu-).
But no peak is observed in dileptons. See page 9 of
http://arxiv.org/pdf/1512.04933v1

12. **Peter Woit**
December 18, 2015

AS,
Thanks for explaining that, and thanks for the useful reference.

13. **mfb**
December 19, 2015

@Douglas Natelson: I'm surprised that CMS did some combination, and apparently just for the backup slide in the presentation – I have no idea how they did it and they didn't publish it. How to combine 8 and 13 TeV? The cross-section ratio depends on the production process, which is unknown.

14. **Howard Baer**
March 5, 2016

The question of when to give up on weak scale supersymmetry is addressed in a recent paper arXiv:1509.02929 (PRD93 (2016) 035016 entitled

Upper bounds on sparticle masses from naturalness or how to disprove weak scale supersymmetry

Gluino masses can range up to about 4 TeV before much fine-tuning sets in.
In this case, LHC has explored only a fraction of the allowed SUSY parameter space.
The Higgsinos have the most direct connection to naturalness, but these particles are exceedingly difficult to see at LHC. They are better searched for at an e+e- collider such as the proposed ILC.

15. **Peter Woit**
March 5, 2016

Howard Baer,

I find it hard to reconcile your present claims with the many colloquia about SUSY I've sat through during the past 20 years (Peskin and Arkani-Hamed are among the examples that come to mind), in which an explicit claim was made that if SUSY was the mechanism to explain fine-tuning, it would definitely be seen at the LHC. For another example of this, there's your such 2009 talk

http://arxiv.org/abs/0908.2785

where you state
"On the theory side, quadratic divergences associated with the scalar sector require new physics at or around the electroweak scale. Also, we have no guidance as to the origin of the generations, the quark and lepton masses and mixings or the origin of the gauge symmetries. We also need a consistent merging of the SM with a quantum mechanical theory of gravity.
On the positive side, data from existing experiments are already pointing the way to a new, more elegant paradigm for the laws of physics as we know them. In addition, data from experiments soon to operate, especially from the CERN LHC, should clinch the deal."
and
"These data, matched against SUSY theory, seem to point to the Minimal Supersymmetric Standard Model (MSSM) (or MSSM plus gauge singlets) as being the correct effective theory of nature between the weak and GUT scales. The truth will be revealed by experiment as the LHC era gets under way, since weak scale supersymmetry predicts a host of new matter states, many of which should be accessible to LHC searches."

Given this history, I don't see why one should take seriously claims that this idea isn't dead because the LHC was never going to be able to resolve the issue.

Updates

December 16, 2015
Categories: Multiverse Mania

Some new items mostly updating older ones:

- Natalie Wolchover has a very good article at Quanta, entitled A Fight for the Soul of Science, reporting on the recent Munich conference discussed here. David Gross sounds a little bit like John Horgan, emphasizing the problem of HEP physics getting too difficult, with an "End of Science" danger. I think he has the problem right:

  > "The issue in confronting the next step," said Gross, "is not one of ideology but strategy: What is the most useful way of doing science?"

  I hadn't realized quite how radical Dawid is. He seems to have moved on from discussing theory "assessment" to theory "confirmation". Even the most devoted string theorists like Gross may be unwilling to sign on to this, comfortable with the idea that string theory deserves a positive assessment, as a promising idea still worth working on, much less so with claims from Dawid that one can sensibly discuss string theory as a "confirmed" theory, one that belongs in our school textbooks.

  There was much discussion evidently of Bayesian confirmation theory, and Gross was enthusiastic about this

  > Gross concurred, saying that, upon learning about Bayesian confirmation theory from Dawid's book, he felt "somewhat like the Molière character who said, 'Oh my God, I've been talking prose all my life!'"

  He may have become a bit less enthusiastic later when faced with Joe Polchinski's Bayesian calculation showing a 94% probability confirming the multiverse.

  Sabine Hossenfelder and Carlo Rovelli both explained well the danger of such claims of non-empirical Bayesian confirmation of one's ideas:

  > The German physicist Sabine Hossenfelder, in her talk, argued that progress in fundamental physics very often comes from abandoning cherished prejudices (such as, perhaps, the assumption that the forces of nature must be unified). Echoing this point, Rovelli said "Dawid's idea of non-empirical confirmation an obstacle to this possibility of progress, because it bases our credence on our own previous credences." It "takes away one of the tools — maybe the soul itself — of scientific thinking," he continued, "which is 'do not trust your own thinking.'"

- For more on the "non-empirical science" front, see Alan Lightman's long piece in Harper's on "Quantum Cosmologists" Sean Carroll, Alexander Vilenkin, James Hartle and Don Page. Lightman waxes poetic on the importance of "The need to ask the really big questions", but unless I missed it, there's nothing there about the need to provide any evidence for the answers that one comes up with. At least one of the four quantum cosmologists, Don Page, seems to see no particular distinction between theology and science.
- On the Mochizuki front, there's this report in Nature about the Oxford conference. On the question of what went wrong with the later talks

  > But Conrad and many other participants say they found the later lectures indigestible. Kim counters that part of the difficulty lay in cultural differences: Japanese mathematicians have a more formal style of lecturing than do those in the West and they are not as used to being questioned by a testy audience, he says.

  I don't think the "Japanese culture" explanation of the problem holds water. Of the three problematic speakers, one (Mok) is not Japanese at all (from Hong Kong), and the other two certainly understand that explaining mathematics to someone is about more than reading a lecture to an audience. It's not plausible that the reason they didn't have satisfactory answers to questions from the audience is their Japanese cultural background. For the case of Mochizuki himself, he grew up here in New York, went to prep school, undergraduate and graduate school in the US. The question of why following him is so difficult is a fascinating one, but I don't think the answer to it has anything to do with his choice to move to Japan.

  A must-read detailed report on the situation is Brian Conrad's, available here.

Update: For some other commentary on the Munich workshop and relevant issues, see Sabine Hossenfelder and my Columbia colleague Andrew Gelman (who I think, unlike anyone in the theoretical physics community, actually knows something about Bayesian methods).

Update: Fesenko has a comment at the Nature article, where he makes the claim: "There are no questions about the theory which are left unanswered." I agree with my Columbia colleague David Hansen's response to this, that this seems to be an absurd statement.

Update: There's a very good report on the abc conjecture workshop from Kevin Hartnett at Quanta. His take on what happened agrees with others:

> Kedlaya's exposition of Frobenioids had provided the assembled mathematicians with their first real sense of how Mochizuki's techniques might circle back to the original formulation of Szpiro's conjecture. The next step was the essential one — to show how the reformulation in terms of Frobenioids made it possible to bring genuinely new and powerful techniques to bear on a potential proof. These techniques appear in Mochizuki's four IUT theory papers, which were the subject of the last two days of the conference. The job of explaining those papers fell to Chung Pang Mok of Purdue University and Yuichiro Hoshi and Go Yamashita, both colleagues of Mochizuki's at the Research Institute for Mathematical Sciences at Kyoto University. The three are among a small handful of people who have devoted intense effort to understanding Mochizuki's IUT theory. By all accounts, their talks were impossible to follow.

There's also a report now from Fesenko, available here. His take on this is that the problem wasn't the talks, it was the audience:

> Labor omnia vincit. Progress in understanding the talks correlated with preparation efforts for the workshop. There were participants who came unprepared but were active in asking questions, many of which were already answered in recommended surveys and some of which were rather puerile.

Unclear what the point of such remarks is, unless the goal is to make sure that many of the experts who attended the workshop won't come to any more of them.

Update: The paragraph from Fesenko's report on the workshop quoted about has been removed, replaced by

> Без труда не выловишь и рыбку из пруда. Progress in understanding the talks correlated with preparation efforts for the workshop. Lack of reading of non-classical papers of the theory often correlated with the depth of asked questions.

Update: Nature covers the Munich conference as Feuding physicists turn to philosophy for help.

Update: The Fesenko report has been further edited, with the paragraph mentioned above now reading

> Без труда не выловишь и рыбку из пруда. According to the feedback, progress in understanding the talks and quality of questions often correlated with preparation efforts for the workshop and reading of non-classical papers of the theory.

Update: Michael Harris's take on the press coverage of the Oxford conference is here.

Comments

1. **Low Math, Meekly Interacting**
   December 16, 2015

   Well…I was recently in a video conference with some potential Japanese collaborators, and it was impossible to get answers to all but the most straightforward and anodyne questions. Incredibly awkward. The translator sometimes would just sit there, fidgeting slightly, apparently so abashed by what we were saying she couldn't speak it aloud in Japanese. You could tell how well the men (all men) around the table could understand English, though, by how red their faces were getting with whatever mix of mortification and rage they were attempting to conceal beneath stone-faced stoicism. I've worked with loads of Japanese researchers who've done a lot of post-graduate or post-doctoral work in the West and were fairly acclimated. Never had a problem. But perhaps without some amount of pre-exposure, there can be substantial cultural barriers to useful dialog, and a lot still gets "lost in translation". Or not translated at all!

2. **Staffan Angere**
   December 16, 2015

   Bayesian confirmation theory is very, very popular in philosophy right now, especially in epistemology. My guess is that this is mainly because (i) it is simple and often lots of fun to play around with, (ii) it is mathematical and therefore rigorous in at least one way, and (iii) you can use it to prove more or less anything.

   Dawid's treatment of the no-alternatives argument (published in a paper in the British Journal for the Philosophy of Science this year, written together with Hartmann and Sprenger) is a good example of (iii). Given the model (or "framing") they use, and their assumptions about the priors, the argument is certainly valid. But the model is just one possibility out of countless, and while most of their assumptions sound prima facie reasonable in that specific model, other models would give quite different recommendations.

3. **Scott Church**
   December 16, 2015

   Peter,

   Regarding Lightman's piece, I got the same impression you did–he seems to think that when asking "the really big questions" one needn't bother with meaningful searches for evidence for the answers that one comes up with. But that said, I don't think Don Page is blurring science and theology. As a Christian, he believes the hand of God is manifested in the workings of nature and that fills him with wonder. But rest assured he is quite clear that this is a *metaphysical* stance well outside the realm of his science. It certainly hasn't kept him from contributing to quantum cosmology research–he's a leader in the field.

   I believe his comments are in response to Sean Carroll and Lawrence Krauss (also mentioned in the essay), both of whom are outspoken atheists who actually *have* made a point of publicly blurring the distinction between science and theology (or "a-theology" if you will), and on many occasions. Page is simply setting the record straight by pointing out that his faith coexists quite nicely with his quantum cosmology research–that science and theology are separate but complementary realms that are neither incompatible, nor mutually exclusive, and they shouldn't be encroaching on each other's turf. If anything, it is *he* who is who is clear about this rather than the others.

4. **Peter Woit**
   December 16, 2015

   Scott Church,
   I don't think Page always so cleanly separates his science from his theology, see for instance
   http://arxiv.org/abs/1412.7544
   I see that Carroll has just finished writing a book: "ON THE ORIGINS OF LIFE, MEANING, AND THE UNIVERSE ITSELF", which starts out with the fundamental nature of reality and ends up with a code of ethics. This strikes me as designed to compete with the great religious texts, although presumably in the parts not quantum cosmology he is often invoking well-grounded scientific facts.

5. **Scott Church**
   December 16, 2015

   Thanks Peter! I hadn't seen that paper of Page's, but I plan on reading it more closely soon as I get a chance. At first blush it looks as though he's attempting to argue via inference to the best explanation in both scientific and philosophical areas, invoking (among other things) well-grounded facts wherever they seem relevant. I haven't seen Carroll's book yet, but based on what you said I get the impression he's trying to do the same.

   Part of the problem with this sort of thing is that fields like quantum cosmology unavoidably take us into realms where the line between physics and metaphysics is easily blurred and it becomes increasingly difficult keep the two separate, and all the more important that we do so. This is a problem because (in my experience at least) not many physicists are well-trained in philosophy–particularly philosophy of science and religion which is what's really needed for that. Some folks do cross the line from time to time, but I don't think that necessarily means they aren't doing their best to appropriately honor it. More than one physicist of late has suggested that perhaps it's time for more dialog between physicists and philosophers, and I for one, agree. Speaking of which… I find it telling that Krauss–who runs rough-shod over that line every chance he gets–is openly contemptuous of philosophy, including the philosophy of science, and has been called out for it by many commentators including Carroll himself.

6. **vmarko**
   December 16, 2015

   Peter,

   Dawid's usage of the term "confirmation" was a matter of vigorous analysis and debate during the conference.

   As Dawid explained during the Q&A session, he uses the term confirmation as synonymous to "increasing the Bayesian probability that for the viability of the given theory", or something to that effect (there was also a big discussion what "viability" means in this context, but that's another story). Here the word "confirmation" is being used as a highly specialized technical term, defined in the context of Bayesian analysis. Dawid's point was that, according to latest understanding in philosophy of epistemology, the term "confirmation" can actually have no other meaning except the above Bayesian one.

   Despite the fact that he is probably right about latest results in epistemology, he was still heavily criticized for using the word confirmation, from two sides. First, if you ask a random person on the street what does it mean that a theory is confirmed, they are not going to start talking about Bayesian epistemology etc. Common meaning of the word "confirmation" in everyday usage is waaay different than the specialized technical meaning that Dawid uses. Of course, the common meaning is ill-defined, does not stand up to epistemological scrutiny, and eventually boils down to the technical Bayesian meaning when one thinks about it really hard. But despite this, ordinary people still understand the word in that common everyday sense ("being confirmed means that it's true"), just like a whole bunch of other ill-defined terms (like truth, proof, reality, etc…). So this is bound to introduce confusion.

Second, George Ellis mounted a very strong criticism of Bayesian analysis itself, distinguishing between adjusting the prior in light of new data, versus adjusting the prior in light of new theories. The former is useful and good, the latter does not really make sense. In short, Bayesian analysis is legitimately applied to information. However, theory is not information, and Bayesian analysis cannot be applied to theories.

My impression is that Dawid acknowledged these criticisms. The biggest part of the problem (although not the only one) turned out to be just the poor choice of terminology.

Also, another thing that became overwhelmingly obvious was the misinterpretation due to ignoring the proper context. When you hear the main single-sentence claim by a philosopher, it can sound scandalous or overstated. But when you listen to their lecture which explains the claim in more detail, you become aware of all the footnotes, side-remarks and technical definitions that reinterpret the scandalous main claim into something much more reasonable, well-measured and uncontroversial. It's like in journalism — the title of the newspaper article produces a "Wow!!! Seriously?!" reaction, but when you read the whole article, your reaction reduces to "Oh, of course, it's obvious and trivial, nothing to see here really (yawn)…".

So Dawid's statement is much less controversial when one understands the appropriate context and technical definitions of the terminology being used. The only criticism beyond that is basically the one given by George Ellis.

Best, 🙂
Marko

7. **Cam**
December 16, 2015

Brian Conrad's notes on the IUT workshop are absolutely outstanding for both lucidity and informativeness and I can't recommend them strongly enough.

8. **Bee**
December 17, 2015

Peter, You might have seen it already, but just in case, I have a summary here

http://www.forbes.com/sites/startswithabang/2015/12/10/why-trust-a-theory-physicists-and-philosophers-debate-the-scientific-method/

that also has a few words on the assessment vs confirmation issue.

9. **M**
December 17, 2015

If the new scalar at 750 GeV really exists and if it will turn out to be there for no natural reason, it would make a direct contradiction with anthropic selection arguments

10. **Ali**
December 17, 2015

As the pragmatist (and radical empiricist) C.S. Peirce already argued convincingly over a century ago, Bayesian inference has no place in valid scientific inference (or in inference of any sort, for that matter, e.g. in business decision making). That Bayesian inference now plays an increasingly prominent role in particle physics, astrophysics/cosmology, and now biology is unfortunate and personally very depressing (I have watched this develop over my time working in these three different communities). This is what happens when scientists hand over the keys to statisticians and philosophers of science (don't expect the philosophers to come to the rescue either…I strongly dispute Dawid's claim that Bayesian inference is the only solution to the problem of confirmation, as reported above by Marko). It would probably help if there were more dialogue among these communities. What would be useful for many would be to simply revisit Peirce's writings on scientific inference, which were way ahead of his time (and still ahead of our own time apparently…he would have been sorely disappointed with the Munich conference). The enormous amount of nonsense written in the intervening century (in both statistics and in the philosophy of science, and in their intersection, e.g. Popper) may still prevent most from appreciating how fundamental and relevant his writings remain.
****

"The relative probability of this or that arrangement of Nature is something which we should have a right to talk about if universes were as plenty as blackberries, if we could put a quantity of them in a bag, shake them well up, draw out a sample, and examine them to see what proportion of them had one arrangement and what proportion another. But, even in that case, a higher universe would contain us, in regard to whose arrangements the conception of probability could have no applicability." (Peirce 2.684, i.e., volume 2, section 684 of his collected papers)

****
"Every argument or inference professes to conform to a general method or type of reasoning, which method, it is held, has one kind of virtue or another in producing truth. In order to be valid the argument or inference must really pursue the method it professes to pursue, and furthermore, that method must have the kind of truth-producing virtue which it is supposed to have. For example, an induction may conform to the formula of induction; but it may be conceived, and often is conceived, that induction lends a probability to its conclusion. Now that is not the way in which induction leads to the truth. It lends no definite probability to its conclusion. It is nonsense to talk of the probability of a law, as if we could pick universes out of a grab-bag and find in what proportion of them the law held good. Therefore, such an induction is not valid; for it does not do what it professes to do, namely, to make its conclusion probable. But yet if it had only professed to do what induction does (namely, to commence a proceeding which must in the long run approximate to the truth), which is infinitely more to the purpose than what it professes, it would have been valid." (Peirce 2.780)
****

For those curious about the further details of this history, you can refer to my book draft. In this draft, I also present a novel and optimal approach to general statistical inference, one that is strictly frequentist in nature and in complete accord with Peirce's guidelines, providing a concrete example of just how relevant his ideas continue to be:
http://arxiv.org/abs/1301.5186

(Apologies to Peter for the self-promotion, but this history and perspective is unfortunately not presented anywhere else in the literature.)

11. **Chris W.**
December 17, 2015

Ali,
In the excerpt from your comment below, are you indicating that Popper was the source of some of that nonsense? (I should note that he was a great admirer of Peirce's contributions.)

The enormous amount of nonsense written in the intervening century (in both statistics and in the philosophy of science, and in their intersection, e.g. **Popper**) may still prevent most from appreciating how fundamental and relevant his writings remain.

12. **Chris W.**
December 17, 2015

Andrew Gelman's post (the pingback above) is highly relevant.

13. **Peter Woit**
December 17, 2015

Bee,
Thanks, I did see that, strongly recommend that those interested in this read your article, so added another link to it (and to the Gelman posting).

14. **Ali**
December 17, 2015

Chris,

Peirce of course considered falsification an important aspect of inference, but he also recognized the narrowness of this purely "negative" view of inference (which Popper never properly conceded). What is missing in Popper is the "positive" basis of the process of scientific inference on Occam's razor. Peirce recognized the importance of Occam's razor, but even he didn't go as far as I would in stressing its essential nature. There are really just two aspects to scientific inference (and epistemology, period), the statistical frequentist notion of goodness-of-fit or what I more generally refer to as "concordance" (and which Peirce alludes to in the above quotes) and Occam's simplicity. Without Occam, we are lost in a sea of possible theories; we need simple theories to make the fastest growth in our knowledge of the universe (see Peirce's discussion of the game of twenty questions, which I also write about in my book draft). Without goodness-of-fit applied to real-world observations (e.g., most famously through Pearson's chi-squared test, but even more fundamentally through the statistical approach that I develop in my book draft), we are slaves to arbitrary conventions (Bayesian "flat priors"). But the latter is a deeper argument, the details of which you will find in my book draft.

I have long thought that Peter's view of string theory as "not simple" is indeed the most important criticism of the entire endeavor. That it is "not even wrong", i.e. not testable (easily), is also of course important, but its lack of simplicity is more critical. These two critiques embody well the dual competing aspects of scientific inference, simplicity and concordance.

15. **Ali**
December 17, 2015

For more details on Popper's views, I recommend Deborah Mayo's book "Error and the Growth of Experimental Knowledge". While she also admires Peirce, she is an avowed Neyman-Pearsonian ("confidence intervals" are unfortunately merely a "confidence trick" as Arthur L. Bowley noted long ago and Edwin B. Wilson, Gibbs's sole student, did his best to inform us of). While I disagree strongly with her on her defense of Neyman-Pearson (see my book draft for further details), her presentation of Popper's views is very interesting.

I need not point out that it's of course better to be a Karl-Pearsonian or Fisherian or Neyman-Pearsonian or Popperian frequentist than to be a Bayesian.

But even better is to be a Peircean frequentist.

16. **srp**
December 17, 2015

Dogmatic frequentists and Bayesian comments read to the outsider much more like religious than scientific claims. Both are inveterate proselytizers of the unchurched.

17. **Cormac O'Raifeartaigh**
December 17, 2015

That's a very nice summary of the Munich meeting from Bee in Forbes magazine, Bee.
However, there is one aspect of the debate that still puzzles me: one lesson from the history of theoretical physics is that many 'sleeping beauty' theories showed little sign of connecting with experiment at first, but did eventually throw up predictions that could be tested. It seems to me that we should not assume that the difficulties in connecting string theory to the real world may one day be overcome…the real question is, how long along the road does one travel before losing heart?

18. **Cormac O'Raifeartaigh**
December 17, 2015

P.S. Indeed, one can draw a useful link between the Munich conference and the recent conference on the history of GR in Berlin as follows:
After the initial excitement following Eddington's observations of the bending of light in 1919, GR went through something of a 'low watermark' period. It was not until the observation of evidence for exotic phenomena such as quasars, pulsars and black holes in the 1960s that GR became a topic of major interest to the community. So it took 50 years for a theory of gravity to connect with the macroscopic world – how long should we allow for a theory concerning the sub-atomic world to mature such that it can make predictions about that world? Who can know that such predictions will not be rendered possible through some unimaginable mathematical breakthroughs down the road?

19. **Jeffrey M**
December 17, 2015

Cormac

I think trying to compare string theory and GR is pushing it. After Eddington GR was essentially universally acknowledged to be the theory of gravity. It wasn't necessarily such a big deal for a long time because no one thought there was much to do with it. I mean Newton works just fine for almost anything anyway, and there weren't any big problems out there that anyone thought GR would apply to. And there were very good people working on it, coming up with all sorts of solutions of the field equations. And it was THE theory. String theory is almost the opposite, there are plenty of big question out there people hoped it would apply to, but of course there has not ever been any way to test any of it. Beyond what is going on at the LHC now, where SUSY is NOT showing up, which as best I understand it would be a real problem for ST.

20. **Scott Church**
December 17, 2015

Cormac,

It is true that "sleeping beauty" theories sometimes surprise us by turning out to be more testable than we thought. There certainly are times when deciding how far to travel a road before giving up isn't easy. But that said, there are at least two differences between string theory and your GR analogy.

First, Eddington's observation of gravitational light bending *was* in fact an unambiguous test of GR. For that matter, so was the perihelion precession of Mercury. In both cases GR made specific predictions that could be accurately measured, and they were verified. These two observations alone were reason enough to embrace GR no matter how long a low-watermark period ensued. Despite 40+ years of effort no such thing has occurred with string theory.

Second, as already noted, GR makes clear predictions–the kind that can be tested and falsified if they're wrong. String theory doesn't. The problem isn't just that testing it is likely to be out of reach for the foreseeable future–the larger problem is that it's fluid enough to predict literally *anything* we might observe. Hence, the title of Peter's book and this blog: *Not Even Wrong*. GR *could* have been wrong… and it wasn't. As it stands today string theory can't be, and that situation isn't likely to change anytime soon.

IMHO, these problems are more than enough reason to justify turning down a different road.

21. **Peter Woit**
December 17, 2015

I'd rather if Cormac's comment not be the excuse for rehashing rather old arguments about string theory, and GR isn't a great analogy, for many reasons.

I don't know how much it got discussed at the Munich workshop, but the "when do you give up and try something else" question is obviously a central one for string theory, as well as for the philosophy of science in general. I've noticed in recent years a change in how string theorists respond to the "when can it be tested" question, from "by finding SUSY at the LHC, by observing cosmic strings, by Planck CMB results, etc." to "maybe something unexpected will appear". While unexpected progress can't be ruled out, and is a good argument that some people who think they see a way forward should keep trying, the shift to this argument is an indication of a problem. How do you keep this argument from being used to justify sticking with a failed research program, since, no matter how bad things look, it's always the case that maybe a miracle will happen?

22. **Chris W.**
December 17, 2015

Scott Church's reply to Cormac is exactly what I was thinking of saying. The "something unexpected" that is needed is a large reduction in the flexibility of string theory to which he refers. That does not refer to relevant observations or the lack thereof. It refers to how the theory *responds* to observations.

Ali,
In my mind this flexibility is what the lack of simplicity of string theory is really about. Simplicity is essential to making a theory *meaningfully* testable, and not merely capable of cranking out quantitative assertions that one might try to compare with observation. If GR's predictions of gravitational light bending and the perihelion precession of Mercury had been conditioned on hidden structure that could be freely adjusted to achieve agreement with observation, then GR would have been viewed in a different light—maybe a signpost on the road to a good theory, but not satisfactory as it stood. And that inadequacy would have nothing to do with an inability to produce numbers that could be compared with experiment, either in theory or in practice.

PS: Another way to put this is to say that ability to generate assertions about observable objects or events is a prerequisite for testability of a theory, but by itself falls well short of testability in the usual sense.

23. **G.S.**
December 17, 2015

This is probably just semantics, but is the perihelion precession of Mercury considered a "prediction" of GR? It adequately explains the deviation from Newtonian gravitation but the effect had been known for quite a while. I would think that any new theory of gravitation which failed to explain the precession would have failed to gain attention. In that sense, it's more of a "post-diction". The deflection of light and gravitational redshifting of light strike me as the true "predictions".

24. **vmarko**
December 17, 2015

Peter,

"I don't know how much it got discussed at the Munich workshop, but the "when do you give up and try something else" question is obviously a central one for string theory, as well as for the philosophy of science in general."

It did get discussed, that was one of the main topics actually. David Gross kept repeating the word "strategy" all too often, there was a big debate what does it mean to say that a theory is "viable", etc. If Gross and Rovelli agreed on anything at all, it was the operational definition of "viability". A theory is considered viable "if I want to keep working on it", Gross said, and Carlo shouted "Yes! Yes! That's it!". 🙂

And in the absence of experiments, what criteria do we have to estimate the viability of the theory? The whole motivation for the conference was Dawid's proposed answer to that question, condensed in his three criteria: No Alternatives Argument (NAA), Meta-Inductive Hypothesis (MEH) and Unexpected Explanations Argument (UEA). The big debate was whether NAA, MEH and UEA taken together can be considered a good or useful criterion for viability of a given scientific approach (they obviously don't work when taken separately one by one, but together they are more robust).

I did not see any sort of consensus being reached on the issue during the conference. And the issue is certainly important, not just for string theory, but for all other approaches to quantum gravity as well — some due to practical problems, some due to conceptual problems, but none of the QG theories can actually be tested. So the issue is wider than just ST.

If anything, the Munich conference raised awareness among people (philosophers and physicists alike) that this is an important question to answer.

Best, 🙂
Marko

25. **Ali**
December 18, 2015

srp,

There are three modern (and major) viewpoints in statistics as far as I can tell: (1) Frequentism is the "only game in town" (e.g. Deborah Mayo), (2) Bayesian approaches are the "only game in town" (there are many that claim this), (3) Frequentism and Bayesianism have been reconciled. You will find that all three views have their dogmatic defenders. Which camp do you fall into? I suppose category #3, in order to acknowledge the "worth" of camps #1 and #2 (they did work on their ideas for such a long time…it would be a shame if this were all for nothing)? But this is an equally untenable dogmatism. As statistics now increasingly impacts how science is done (how it is reported and even planned…see biology), this is an important fight. You can either participate in it or you can watch from the sidelines. I actually fall into a subcategory of camp #1, as I dispute the worth of Neyman-Pearson confidence intervals (as did many frequentists before me before, until dogmatism and simple inertia allowed confidence intervals to supplant other approaches and the search for better ideas…now Bayesianism is doing the same…sound familiar?).

The main reason I don't acknowledge Bayesian approaches is that they are based on an obvious logical fallacy, that we can construct a "flat" prior (in one coordinate system) and use it to construct a meaningful posterior probability distribution on which we can be confident actually applies to our universe. But this "flat" prior will be "flat" in only one coordinate system. As physicists, it is obvious that a simple coordinate change will completely alter what is meant by "flat" and therefore what one then gets as a "posterior" distribution. The whole deep desire to construct probability distributions on data is the really the problem (equally a problem with frequentist "confidence intervals"). As Edwin B. Wilson tried to tell us, all we really have is the p-value for certain statistics (e.g. Pearson's chi-squared/p-test, which is the physicists' favorite tool, for good reason). This is the only thing we can all agree on. It should be noted that Wilson is often reported as the discoverer of "confidence intervals" (by Neyman himself), a false characterization of his work that he disputed his entire life. So essentially, Neyman was lumping Wilson in with Neyman-Pearson. I guess you could say Wilson was being "dogmatic" about this. I think it's the lack of people willing to be "dogmatic" that has led to the current complete disarray and confusion within the field of statistics and certainly to outsiders (no one criticizes any more, at least not like they did in the past). So here's to dogmatism (especially when it's in the defense of science)!

Chris,

I was a bit glib in the above discussion of "not even wrong" and "not simple" criticisms of string theory. These claims cannot be so easily separated, as simplicity is what ultimately allows for testability.

Chris and G.S.,

The prediction of light bending vs. the postdiction of Mercury's perihelion represent an important focal point in the field of the philosophy of science (it comes up everywhere). Basically, the prediction of light bending was not really seen as convincing as the postdiction of the perihelion. An interesting mental exercise: Imagine if there were no historically/currently testable predictions from G.R., only the postdiction of the perihelion of Mercury. G.R. would still be heralded as a fantastic triumph, as it is simple and gives exactly the right postdicted result for the perihelion. If all string theory did was postdict the mass spectrum with either zero or even only a handful of free parameters (less than the number of masses, but not 10^100), then it would also be celebrated (and not just by those working on it!). It would need to make no new predictions. That's also, by the way, why Popper's views are restricted. They don't acknowledge the importance of the simplicity of certain theories that postdict successfully.

26. **Manfred Requardt**
December 18, 2015

The ongoing discussion about the theory of confirmation of scientific theories is certainly very important. However, concerning the work of Don Page I would like to remark, that he wrote quite a few fundamental papers which I carefully studied and I never had the slightest impression that he was mixing up theological with scientific reeasoning whatever his own beliefs are.

27. **Natalie Wolchover**
December 18, 2015

Peter, many thanks for linking to my article and facilitating a great discussion.

Following up on vmarko's last comment, I wanted to clarify that "viability" as "something worth working on" was not the definition used in Munich, at least not by the philosophers in the room. Dawid said repeatedly that what he means by the "viability" of a theory is its ability to account for observations at the scales it intends to describe. Newton's theory is "viable" as a theory of gravity at low energies and speeds, for example. "Viable" is basically a substitute for "true," since a viable theory will ultimately turn out to be encompassed by a larger theory and is not therefore true in a universal sense.

So, when Dawid argues that the NAA, MEA, and UEA arguments increase the Bayesian confirmation that string theory is viable, he's not merely saying that they increase the probability that you should work on string theory, he's saying that these arguments increase the Bayesian confirmation that string theory correctly describes nature at the Planck scale. It's a stronger claim.

The reasoning is that these three non-empirical arguments impose limits on underdetermination; they decrease the likelihood that there are other theories out there that are also able to describe nature at the Planck scale, which in turn increases the likelihood that string theory is viable. Dawid doesn't claim that these arguments can ever completely kill underdetermination or (in other words) drive the confirmation of string theory's viability up to 100 percent, but he argues that they can get us pretty far — far enough to account for the trust/confidence in string theory exhibited by many of its proponents.
Thanks again!
-Natalie

28. **Low Math, Meekly Interacting**
December 18, 2015

In my field, we have to contend with greedy sociopaths trying to circumvent scientific scrutiny by suppressing the evidence. From a philosophical point of view, that threat pales in comparison to an open-ended proposition to eschew testable hypotheses so long as something strikes enough people as beautiful and/or unique. Liars get caught, eventually, if the evidence exists. In science, it always should, from the greatest physicist to the lowliest stamp collector. How can it possibly be otherwise? I've been reading about this conference for days, and it just seems like…madness! Bayesian epistemology in place of evidence, built around a "framework" of theory generation of nearly infinite malleability? For decades? With no end in sight? How is that not unthinkable? A days-long dialogue between some of the brightest minds in the world devoted to…this! I'm grateful for all the wonderful coverage, but I still can't quite believe it.

29. **Peter Woit**
December 18, 2015

LMMI,
A lot of this doesn't make much sense until you realize that Dawid, Gross and others are taking as starting proposition a well-entrenched ideology about string theory, then working backwards to find a philosophical framework to justify it as science. I believe Dawid is sometimes even quite explicit about this: he's just writing down the arguments string theorists use to justify what they do in the face of conventional failure as science.

30. **NeapTide**
December 18, 2015

There are `sleeping experiments' too… superconductivity was discovered in 1911 but wasn't well described by theory until 1957. CP violation was discovered in 1964 but didn't have a good theory until 1973.

You don't always need a theory to make experimental progress. Maybe, if we finally get the mass scale of the neutrinos established, we'll see a pattern that still isn't evident. Ditto with the CP violating phase in neutrinos.

Dark Matter is a long, long sleeping experimental discovery, one that has had lots of phenomenological impact but still doesn't have a good theory.

Maybe science is a process, and maybe theories aren't things defined by falsifiability or degree of Bayesian probability. Maybe theory is just a repository for the discussion in a sort of standardized format. And maybe the highest calling is to evade the problem of the most charismatic or insistent human defining right from wrong, but be guided by something bigger than a human (at least ideally) like experiments, observations, and some sort of durable beauty in the theory.

31. **Laurence Lurio**
December 18, 2015

I'm a little surprised that in this discussion no one has taken issue with Wolchover's analogy that no one has observed an atom e.g. " modern scientific theories typically make claims far beyond what can be directly observed — no one has ever seen an atom — and so today's theories often resist a falsified-unfalsified dichotomy".. Individual atoms have been pretty clearly observed. The most dramatic example being STM images of Silicon, but x-ray diffraction, observation of fluorescence from a single atom in an atomic trap, even radioactive decay of a single nucleus in a Geiger counter, all observe single atoms pretty clearly. The observability of atoms seems a remarkably poor example of how theories can't be falsified.

32. **vmarko**
December 18, 2015

Laurence,

"I'm a little surprised that in this discussion no one has taken issue with Wolchover's analogy that no one has observed an atom […] Individual atoms have been pretty clearly observed."

I guess what Natalie meant was that atoms have not been *directly* observed back at the beginning of 20th century, when the modern atomic theory was first introduced. There were things like Brownian motion and such, but these were all *indirect* observations of (various effects of) atoms, and I guess back then nobody was even contemplating any possibility of direct observation of individual atoms. Direct observations (via X-diffraction etc.) came quite some time later.

Best, 🙂
Marko

33. **Magnema**
December 19, 2015

@LMMI: I think "Bayesian epistemology in place of evidence" is misphrasing the point. Rather, it would be "Bayesian epistemology as a generalization of hypothetico-deductive evidence." After all, evidence is taken into account in the updating rule… the question is what to do with evidence which is not set up from an experiment.

I'd like to note that "flat priors" – while usually used (as the principle of indifference) for naive guesses – are generally not taken as acceptable, for the reasons Ali mentions (flat priors are no less special than any other set for a different problem). I think a lot of the issues that physicists run into occur when they try and apply really naive methods (such as the principle of indifference) and try and get results… it would be akin to saying "well, we don't know what our error is, so we'll say 5%… and then look, this result is statistically significant!" You make one statistically irresponsible decision and get nonsense results. This isn't necessarily a problem with a Bayesian interpretation itself, but rather with trying to apply Bayesian methods with a fast-and-loose approach.

Accordingly, the retrospective alterations that Peter and LMMI refer to, I would guess (although I don't know the community well enough to say for certain anything about the particular of the individuals), come from the way they choose their priors and their probabilities to fit their conclusion rather than their choice of BCT as a theory of philosophy of science in its entirely. I.e., classical overconfidence in their probability determinations rather than reconstructing their entire philosophy of science.

As another error physicists make: as noted in the "Gathering of philosophers and physicists…" article above: Bayesian methods are designed for when one can explore the complete parameter space. When one only has access to a selection of parameters in consideration, there's no guarantee you'll get accurate results, and physicists are (generally) trying to apply those fast-and-loose results to what is already a limited parameter space.

34. **Ali**
December 19, 2015

Magnema,

To construct the posterior probability distribution, you need to assume a prior probability distribution (which can be updated with whatever data are at hand). The posterior is however completely set by the prior (which is not unique). There is no way around this in Bayesian statistics. This is the definition. The only argument from Bayesians that I have ever seen against this is that we should not take the posterior distribution so seriously, but then what's the point?

The desire to construct the posterior probability distribution is of course strong (not only in Bayesian statistics, but also in traditional frequentist approaches), but it's not logically sound, as Peirce points out above. Bayesians have largely won over a sizable chunk of the scientific community because they give scientists what scientists they think they want (you can even find many Bayesians who admit this). But scientists are also no experts at understanding the subtleties of the probabilistic assessment of data. Again, as both Peirce and Wilson pointed out, the only thing we really have is the p-value for particular statistics based on the cumulative distribution (which is invariant to changes in the underlying coordinate system). In my manuscript I develop an entire framework for optimal statistical inference based on this idea and using a particular and remarkable statistic that I call the "fidelity" (a specific interpretation of the Gibbs entropy). I show that it is superior to maximum likelihood (the previous "gold standard") for parameter estimation and superior to the chi-squared/p-test (the previous "gold standard") for goodness-of-fit assessment. I also generalize Student's t-test to completely arbitrary distributions, with now an automatic means for goodness-of-fit assessment. Additionally, I straightforwardly extend the notion of the "fidelity" to non-parameteric testing, developing a test that is superior to the KS test (also considered the "gold standard" for such testing). That all of this is possible in a way that is coordinate-system invariant is remarkable. Again, the entire system is based on the p-value of the fidelity, a statistic constructed from the coordinate-independent cumulative distribution. My approach avoids the thorny problems that characterize both Bayesian and traditional frequentist approaches. For example, my approach is the only solution that I'm aware of to Neyman's paradox, which led Neyman to believe that there is no fundamental approach to statistical inference and that we should only use good judgement or "good behavior" in assessing data in order to not be misled. But Neyman's paradox (a bit of mathematical sleight-of-hand by Neyman) is simply resolved by sticking to the cumulative distribution and respecting the topological ordering of the coordinate system.

I only give the above details to show that there are other possible ways of performing statistical inference. But, if you are a Bayesian, you are done. You already have a complete system for doing any kind of inference (one that is of course fatally flawed). The only work to be done is to find better applied math techniques and algorithms to more efficiently perform the necessary computations (which was the real barrier to applying Bayesian approaches in the past…now we have cheap and much more powerful computers).

35. **Peter Woit**

December 19, 2015

Please, stop the repetitive rants about Bayesian inference. I'm deleting them and any more on the topic. This has little to do with the topic of the posting and drives away people with relevant comments.

36. **Visitor**
December 19, 2015

" I believe Dawid is sometimes even quite explicit about this: he's just writing down the arguments string theorists use to justify what they do in the face of conventional failure as science."

Does Dawid anywhere explain the reasons underlying his apparently naive and uncritical acceptance of such arguments? That's a question potentially more interesting than the theoretical apparatus he's constructed. I would hope that his reasons amount to more than a simple appeal to authority.

37. **vmarko**
December 19, 2015

Visitor,

"Does Dawid anywhere explain the reasons underlying his apparently naive and uncritical acceptance of such arguments?"

I think it's naive to call his arguments naive and uncritical. Despite the fact that I disagree with everything he said in his lecture, I have to admit that the arguments and reasoning presented in his lecture (and I guess his book as well) is very thought-through, and anything but naive and uncritical.

What he did was to look at what string theorists were actually doing, provide an abstract description of it in terms of the NAA, MIH and UEA, and then promote this description into a normative statement ("this" is what they are actually doing, and it is also what they should be doing). And he was very up-front with all that, while including a great deal of analysis of the interactions between NAA, MIH and UEA, assumptions, pitfalls, caveats, characterizations, etc…

You are welcome to disagree with his proposals (as many people do, myself included), but it is just plain wrong to call them naive, uncritical or appealing to authority.

Best, 🙂
Marko

38. **Peter Woit**
December 19, 2015

Marko,
I think what "Visitor" means by "simple appeal to authority" is much the same as what you describe as "promote this description into a normative statement". As far as I can tell, what Dawid is doing simply ignores the main question: isn't all this just an attempt to prop up a degenerating research program? The tactic seems to be to forgo that obvious interpretation of the situation in conventional terms, in favor of a point of view taking string theorists as authorities, with the job just to justify their arguments.

39. **Natalie Wolchover**
December 19, 2015

Visitor, vmarko, Peter,
Dawid says there is one way in which his arguments are normative, or at least, following conventional policies of how science should work. Any of his three criteria, he argues, could disconfirm a hypothesis, just as easily as it might confirm it. This makes each one a sort of test.

Also, to the question of what I meant by "no one has ever seen an atom," the STM images are extremely impressive, but aren't we still interpreting data? Fantastic interpretation, I agree – hence the near-100 percent confirmation of atomic theory. In the late 20th century, physicists worried they would never manage to rule out all possible alternative interpretations of the indirect evidence they possessed in favor of the existence of atoms. Atomic theory was "undetermined." But eventually, everyone decided that the indirect effects of atoms (like the way dust flits through the air) should count as empirical data. Almost everyone, that is — Ernst Mach refused to believe in "the reality of atoms" to his dying day, in 1916.

-Natalie

40. **Peter Woit**
December 19, 2015

Natalie and Marko,
Looking again at Dawid's three criteria, I think that they might more accurately be characterized as a typology of arguments for a degenerating research program:

1. No Alternatives: no matter how bad things look for our ideas, they're better than anyone else's.
2. Meta-inductive: we had a good reason to start looking at these ideas, since this kind of thing had worked before. That reason is still good, no matter had bad things turned out and look now.
3. Unexpected explanatory coherence: the more we study our ideas, the more devoted to them we get, so there must be something to them.

41. **couvent**
December 19, 2015

I'm not an active physicist anymore, but these three criteria seem to be unconvincing.

"No alternatives" might have a sociological explanation. "Unexpected explanatory coherence" depends on what it is that you explain – black hole entropy etc. seems, to me at least, fairly unconvincing. But the weakest criterium is "meta-inductive". What do people mean when they say this kind of thing worked before? What is "this kind of thing"? The graveyard of physics is littered with things that didn't work. I bet most physicists have had more ideas that don't work than ideas that do work. If you only look at the published ideas that did work, you're suffering from a serious case of confirmation bias.

42. **vmarko**
December 19, 2015

Peter,

"I think what "Visitor" means by "simple appeal to authority" is much the same as what you describe as "promote this description into a normative statement"."

When listening to Dawid's lecture, I didn't get that kind of impression (despite the fact that I was expecting to hear precisely something like that). Dawid doesn't promote descriptive to normative because string theory physicists are very smart, but because he believes that his three arguments are the only sensible way one can assess a theory absent any observational evidence. So he's not appealing to authority, but rather saying there's nothing else we can do.

Of course, I disagree with him on that, and yes his arguments do indeed support a degenerating research program. But his arguments were not designed so that they would support string theory, that is somehow a side-effect. The arguments were designed to capture what we can say about the quality of a theory when we have no access to experiment. And of course, I believe they are very very wrong, but I failed to see any appeal to authority there.

This is funny, btw. I heavily disagree with Dawid's points, but somehow I keep sounding like I'm defending his ideas here. That's certainly not my intention.

Natalie,

"Also, to the question of what I meant by "no one has ever seen an atom," the STM images are extremely impressive, but aren't we still interpreting data?"

If you put it that way, then no one has ever seen a chair either — all we see is light coming to our eyes, and then we're interpreting that data. Interpreting the data from X-ray experiments is really no worse in any way (neither qualitatively nor quantitatively) than interpreting the data when we look at bacteria, red blood cells, chairs and tables, or other galaxies.

Best, 🙂
Marko

43. **atoms are real**
December 19, 2015

You can see an individual trapped barium ion glowing with the naked eye.

44. **RandomPaddy**
December 20, 2015

They should do more of these conferences for unresolved problems/speculative theories in physics. Everything from Multiverse to dark matter all the way down to quantum foundations. I suspect if you allowed physicists to talk about things in such a setting you'd get to hear more dissatisfaction that you would suspect.

That said, from the coverage of this conference things might just boil over altogether into a furious series of shouting matches. Shamefully, I must confess this prospect would make me even more likely to buy a ticket.

45. **Radioactive**
December 20, 2015

When you win a Nobel prize you earn the right to fly to nice cities and have conferences about the meaning of simple English words like "test", "verify" and "true". I am sure the coffee break pastries were fantastic, but I really don't think there was anything there to inspire so much hand-wringing debate. The outcomes were completely banal.

In stark contrast to the abc conference which is scientifically and sociologically fascinating on many levels.

46. **Manfred Requardt**
December 20, 2015

Today I read the Consilience section in the recent Sci.Am. (Dec. 2015) and I think there are some useful remarks in it concerning the ongoing debate about scientific verification. Shermer mentions the 19-th century philosopher William Whewell and his notion of "consilience by induction´´. He argues that for a theory to be accepted it must be based on more than one induction or a single generalization drawn from a specific fact. It must have multiple induction that converge on one another , independently but in conjunction (Book: The Philosophy of inductive Science).

47. **Low Math, Meekly Interacting**
December 20, 2015

Magnema: I tend to think of "evidence" used to update priors in science as something other than a human judgment about, say, the beautiful unifying properties of a theoretical framework. Rather "evidence" is the result of an objective measurement of a natural phenomenon. Of course, when one has a functionally infinite landscape of possibilities to choose from, I suppose one can exclude some of those from the newly-inferred probability distribution (say, when a particular model predicts sparticles of a certain mass that are not observed). But that process seems to have made little dent in the posterior assessment. "Evidence" in this new paradigm seems to mean something quite different than what is healthy. The ability to calculate probabilities using Bayes' shouldn't obscure this. If, functionally, this epistemic paradigm allows so many lines of "evidence" but cannot assign to them a proper weight, perhaps it is suspect.

48. **George Ellis**
December 20, 2015

You comment "…Andrew Gelman (who I think, unlike anyone in the theoretical physics community, actually knows something about Bayesian methods)." If you and Golman bothered to read the article, you'd see this was indeed debated at the meeting by both physicists and philosophers. This kind of snide remark undermines your attempt to provide useful comments on what is going on.

49. **Peter Woit**
December 20, 2015

George Ellis,
Apologies for the snideness. I wasn't there so don't know all that was discussed. Note that my remark was directed at theoretical physicists, not philosophers or others. From reading Wolchover's report and others, my impression was that the main invocation of Bayesian confirmation from theorists was coming from Gross and Polchinski, using it in ways that I think are extremely hard to take seriously. While I've often seen a sophisticated use of ideas about probability, including Bayesian ones, in several areas of physics in the analysis of experimental data, I haven't seen this at all in theorist's work divorced from experiment. Quite possibly there are cosmologists or those who study quantum measurement theory who are capable of this, I don't think the same is true of string theorists.

50. **Peter Woit**
December 20, 2015

Marko,
You say that Dawid claims “his three arguments are the only sensible way one can assess a theory absent any observational evidence… there’s nothing else we can do.”

I didn’t hear the talk, but I did read his book, and I don’t recall seeing a “no alternatives” to “no alternatives + two others” argument anywhere there. Rather it seemed to me that he was taking as his starting point a reading of arguments made by string theorists, trying to come up with a classification of those arguments. It seems to me that he was starting from the conclusion he wanted. If you instead started from the general question “what are good, justifiable ways of evaluating theoretical progress”, I think you’d end up with a different list.

An obvious alternative to Dawid’s three that immediately comes to mind would be the criterion for progress “are these ideas moving away or towards successful contact with experiment as they develop”. This seems to me the most common way of evaluating theoretical ideas, which mostly start life unable to make contact with experiment, then either move towards being able to do so, or get abandoned as failures. In the case of string theory unification, the ideas started life with a program for contact with experiment, claims that they were nearly there. The history of the last thirty years has been one of moving in the wrong direction, headed towards zero possibility of any contact with experiment.

Did this sort of thing come up at the workshop? I’d argue that there’s a reason this kind of criterion for confirmation is not being put forward by Dawid: it makes string theory look bad…

51. **vmarko**
December 20, 2015

Peter,

I listened to the lecture, but didn’t read the book (and have no intention of doing so), so information that I have is kind of complementary to yours. 🙂

Basically I agree with everything you said above, and it’s a subset of the reasons I have for disagreeing with Dawid’s criteria. And no, during the conference nobody has discussed the criterion of moving towards or away from experiment or any similar idea (I like your proposal, provided that you can give some operational way of estimating the notions “moving towards” and “moving away”).

I also agree that Dawid was probably looking at what string theorists were doing when he was constructing his arguments. But Dawid’s position does *not* strike me as the one where he is deliberately cheering for ST. I agree that he was looking at a very skewed and biased sample of scientists, but I don’t think that he cherry-picked them intentionally. If anything, he didn’t take a good enough sample of HEP and QG theorists during his research, so one can blame him for constructing his criteria based on an unrepresentative sample of scientists. And I do think that is the case. But I also believe it was an error out of ignorance, not that he did it on purpose.

Of course, I might be wrong, but that was the impression I got after listening to his lecture and talking to him in person. Maybe I’d have a different impression if I had read his book, I don’t know.

Best, 🙂
Marko

52. **Armin**
December 20, 2015

Peter,

Just a quick observation: your apology makes it sound like you consider George Ellis a “philosopher or other”. Also, I think that referring to theoretical physicists in your remark without qualification is far too strong a claim.

53. **Peter Woit**
December 20, 2015

Armin,
I think you’re misreading my comment here. My intent was purely to acknowledge that a snide remark wasn’t a great idea and to instead try to make clearer the thought behind it (which has nothing at all to do with Ellis).

54. **Laarbi**
December 21, 2015

Can anybody provide us with a link to the video of the Munich conference?

Thank you!

55. **Peter Woit**
December 21, 2015

Laarbi,
My understanding is that the talks were recorded, at some point (not yet) will be made available here
http://www.whytrustatheory2015.philosophie.uni-muenchen.de/media/index.html

56. **Kartik Prabhu**
December 21, 2015

@PeterWoit
A more optimistic article about the abc conference: https://www.quantamagazine.org/20151221-hope-rekindled-for-abc-proof/

57. **Peter Woit**
December 21, 2015

Kartik Prabhu,
Thanks! That article is quite good, but I don’t think the bottom line is any different than other reports from the conference:

“The next step was the essential one — to show how the reformulation in terms of Frobenioids made it possible to bring genuinely new and powerful techniques to bear on a potential proof.These techniques appear in Mochizuki’s four IUT theory papers, which were the subject of the last two days of the conference. The job of explaining those papers fell to Chung Pang Mok of Purdue University and Yuichiro Hoshi and Go Yamashita, both colleagues of Mochizuki’s at the Research Institute for

Mathematical Sciences at Kyoto University. The three are among a small handful of people who have devoted intense effort to understanding Mochizuki's IUT theory. By all accounts, their talks were impossible to follow."

I'll add a link to this to the posting. There is also a report on the conference from Fesenko,

https://www.maths.nottingham.ac.uk/personal/ibf/files/iut-i-rep.html

which blames the audience for what happened:

"Labor omnia vincit. Progress in understanding the talks correlated with preparation efforts for the workshop. There were participants who came unprepared but were active in asking questions, many of which were already answered in recommended surveys and some of which were rather puerile."

I'm not sure what the point of such a comment from Fesenko is, unless he's trying to ensure that most of the experts who showed up at this workshop won't attend future ones.

58. **Daniel**
December 21, 2015

I wonder why people like Weinberg, t'Hooft and Wilczek were not invited…

59. **lun**
December 22, 2015

George Ellis – the deluge of hep-ph papers about the "750 GeV bump" shows the snarkiness is justified by quite a few sigmas since the hypothesis of knowledge by theoretical physicists of Bayesian probability has quite a low p-value. 🙂

60. **vmarko**
December 22, 2015

Daniel,

"I wonder why people like Weinberg, t'Hooft and Wilczek were not invited"

Actually, this question was asked during the conference (not for those three names in particular, but for someone else, I can't remember now for who), and the response from the organizers (Dawid in particular) was that actually they were invited, but were unable or unwilling to appear. Again, I don't know about Weinberg, t' Hooft and Wilczek in particular — but apparently many more renowned scientists and philosophers were invited, but for one reason or another, didn't show up. Unfortunately (but understandably) no names were spelled out, so…

Best, 🙂
Marko

61. **Daniel**
December 22, 2015

vmarko,

Thanks for the info!

62. **DrDave**
December 23, 2015

" This "no-alternatives" argument, colloquially known as "string theory is the only game in town," boosts theorists' confidence that few or no other possible unifications of the four fundamental forces exist, making it more likely that string theory is the right approach."

So, the only way to disprove string theory would be to come up with a a fake alternative which could not be tested and makes no predictions. How strange is that?

63. **William M Briggs**
December 23, 2015

Bayes's theorem doesn't work to say a theory has this-and-such probability is true. Giving any theory a probability only makes sense comparatively. Why? Well, here's a longer argument:

http://wmbriggs.com/post/17544/

64. **Rh L**
December 28, 2015

Peter,

I think you'd love the newest edition of Fesenko's comment (ca. December 27, 2015):

"Без труда не выловишь и рыбку из пруда. Occasionally, a baby-like attitude to attend the workshop without reading any surveys or non-classical papers of the theory affected the quality of asked questions and progress in understanding the talks."

65. **Peter Woit**
December 28, 2015

Rh L,
It seems that Fesenko has been editing that text every day or so. I no longer see the paragraph you quote, but he has added this complaint:

" These papers contain a large variety of new ideas and concepts (listed in the letter to participants) and it is a rather naive question to ask to single out the most important idea/concept."

I think that's a misunderstanding of the attitude of many of the experts there, who were trying to find a new, powerful, idea in Mochizuki's formalism they could understand, because that is what would give them the insight needed to make some progress on understanding the proof. It’s not that there was an expectation that there is a single important new idea here, quite likely several are needed, but one has to start by finding and understanding a first one.

66. **David Hansen**
December 30, 2015

@Peter: And now that comment is gone in turn. What a joke.

67. **Paul D.**
January 9, 2016

Tom Hartsfield comes down against attempts to abandon empiricism for the sake of string theory:

http://www.realclearscience.com/blog/2016/01/string_theory_has_failed_as_a_scientific_theory.html

Why String Theory?

December 25, 2015
Categories: Book Reviews

I recently got a copy of Joseph Conlon's new book Why String Theory? and was pleasantly surprised to find that it's quite good. Conlon is a lively, entertaining writer, generally sensible about the scientific issues involved, and I think does a great job of explaining the point of view of typical physicists now working on string theory. He also very ably explains the "sociology" of the field, the different kinds of people who work in this area and their varying sorts of goals and motivations.

The book is explicitly motivated by the desire to answer a lot of the criticism of string theory that has become rather widespread in recent years (wasn't always so…). For a typical example from the last few days, see Why String Theory is Not a Scientific Theory at Starts With a Bang. I have mixed feelings about this sort of thing. It gets the main point quite right, that string theory unification is untestable, having failed to make any predictions, and by the conventional understanding of the scientific method, it's past the time at which most theorists should have abandoned it and moved on. On the other hand, I don't see at all the point to arguing about the term "scientific theory". Sure, it's a scientific theory, a failed one. I've personally never noticed any consistent usage by physicists of terms like "theory", "model" and "hypothesis" in ways that accurately indicate degree of experimental support, don't see why some writers insist that there is one. I also very strongly object to the article's standard move of trying to make a failed theory a "mathematical theory". Mathematics is about well-defined ideas, and there is currently no such mathematical construct as "string theory". The problems with string theory have nothing do with mathematics, rather have to do with a physical idea that didn't work out.

To a large extent the problems Conlon is struggling with are ones that the community of string theorists has inflicted on itself. The great majority of writing for the public by string theorists is characterized by large amounts of outrageous hype. For a very recent example, see Daniel Harlow here, who seems to think string theory is a huge success at explaining the standard model

> although it hasn't quite managed to reproduce the complete standard model of particle physics, it comes very close and the obstructions seem more or less technical.
> I want to emphasize that postdictions are just as good formally as predictions for testing a theory; the distinction is purely sociological.

and that it is also much more (did you know that string theory is what explains the existence of black holes?)

> the main reason to work on (or be inspired by) string theory from a scientific point of view is that it may provide explanations of phenomenon that have ALREADY been observed: the existence of black holes, the small positive cosmological constant, and the evidence for an inflationary phase of the early universe.

As for the problem with the multiverse making no predictions, that's just wrong. We just don't know what the theory is, when we figure it out, surely it will make predictions:

> the issue is not that it doesn't make predictions. The issue is instead that we do not yet understand it well enough theoretically to know what the predictions are!

I've always found reading this kind of thing quite puzzling. My impression of most string theorists is that they're smart and rather sensible, well aware of the difference between ridiculous hype and an actual scientific argument. Unfortunately such sensible string theorists also have seen no point in trying to write for the public until now, and I'm glad that Conlon's book finally changes that.

If you followed the reports from the recent Munich conference, you likely heard that the assembled philosophers and physicists nearly unanimously found the anthropic multiverse point of view Harlow advertises to not be legitimate science. Conlon expresses his opinion in this way, and I think it's the majority one among string theorists, whatever you might have heard:

> The most serious problem with the anthropic landscape is that it provides a cheap and lazy explanation that does not come from hard calculation and also has no clear experimental test. It sounds exciting, but does not offer lasting sustenance, and may even act as a deterrent against necessary hard work developing new calculational tools.
> Of course, this does no mean that the anthropic approach is necessarily wrong. However the triumph of science has been not because it contains ideas that are not necessarily wrong, but because it contains ideas that are, in some important sense, known to be true: ideas which have either passed experimental test or are glued together by calculation. The anthropic landscape is neither of these. It represents incontinence of speculation joined to constipation of experiment.

Instead of Harlow's claims that string theory makes lots of postdictions, coming very close to reproducing the complete standard model, modulo some technical issues, Conlon deals with the situation in a much more honest and straightforward fashion. Of the fourteen chapters of the book, chapter 7 is entitled "Direct Experimental Evidence for String Theory." Here's the entire content of chapter 7:

> There is no direct experimental evidence for string theory.

Conlon's point of view is different than that of the majority of string theorists in one way, which he explains in detail.

> My interest in string theory is in what it can offer to physics that can be probed by experiment.
> This view is far from universal. It may seem odd, but most of those who work on string theory are essentially uninterested in any connections with experiment, any public claims that they may make to the contrary notwithstanding.

He backs this up by the observation that less than 10% of talks at recents Strings 20XX conferences have any connection to observable physics.

Here, I'm again in the majority, with his colleagues, who I think have made an accurate evaluation that connecting current string theory to experiment is a failed and hopeless project (I differ with them on prospects for this changing). Conlon has a research program to investigate potentially observable effects of moduli fields, something his colleagues are skeptical about. While I'm also skeptical about this, it does seem like a reasonable thing to investigate, especially since such things may be generic to all theories with extra dimensions, not just string theory. The chapter of the book describing this research is one with material you won't find in other popular books.

Many of his colleagues have adopted the attitude that, while connecting string theory to experiment is hopeless, it deserves investigation purely as an idea about quantum gravity. While Conlon devotes a fair amount of space to the arguments about quantum gravity and string theory claims about them (including some criticism of loop quantum gravity) he avoids much of the usual hype, and also makes it clear that he himself isn't interested in pursuing this because of the lack of any hope of ever testing one's ideas. In some sense I think he and I agree here: it is only if one's idea for quantizing space-time degree of freedom connects up somehow to our successful theories of other quantized degrees of freedom that one will have any hope of ever knowing whether one has the right theory of quantum gravity. Absent a connection of this kind, one is doomed to become just another cog in an endless fruitless ideological argument about whose quantum theory of gravity is better (or at least, whose sucks less).

Conlon claims that at this point, most string theorists are interested in string theory not as a theory of quantum gravity, but because of applications of ideas that have emerged from string theory to other fields (e.g. AdS/CFT). Here he gives a reasonable account of attempts to use AdS/CFT to say something about condensed matter physics. One place in the book where he, unusually, descends to conventional incantations of hype is his account of applications of AdS/CFT to heavy-ion physics, where he says nothing about the fact that this doesn't work very well, just repeating some rather stale hype.

There's a lot else to like in the book, for instance a chapter of highly perceptive descriptions of the different kinds of theorists and the different ways they work, including some rather amusing and mostly friendly caricatures of common behavior. For an example of the kind of thing you'll read here but not in any other popular string theory book, he notes that certain persons have recently received multi-million dollar prizes based upon model-building ideas that didn't work out.

There's a lot more in the book than I have time to discuss, some of which I agree with, some of which I don't. Obviously I have a different point of view than Conlon's, but his at least I find to be one with serious arguments behind it, unlike all too much of the popular string theory literature. One thing I found rather discouraging after my book came out ten years or so ago was what seemed to me a lack any serious response from sensible string theorists. Quite a few years later, it's great to see that Conlon has written such a thing, and I recommend it highly to anyone who cares about these issues.

And, Happy Holidays!

Update: Sabine Hossenfelder has a posting with a similar take on the Siegel piece. I also like her description of the Munich workshop:

> There was, however, not much feud at the workshop, because it was mainly populated by string theory proponents and multiverse opponents, who nodded to each other's talks. The main feud, as always, will be carried out in the blogosphere…

I haven't seen the full piece, but New Scientist now seems to be covering the multiverse as theology, which is about right.

Update: Over on Facebook Dan Harlow explains the "technological problems not relevant for questions of principle" needed to get string theory predictions

> the idea is that in order to view string theory as a theory of nature, we need to view it as providing a unique probability measure on the space of low energy theories. This would be computed by understanding both the structure of the landscape and the dynamics of eternal inflation. We can then compare our observations to the predictions of this measure, and if they are atypical the theory is ruled out. We are far from doing this though, except for the imprecise cartoon that seems to more or less work for the cosmological constant. This seems just as scientific to me as quantum mechanics, except that we don't yet know how to compute the probabilities.

I see a bunch of problems of principle here, starting with not knowing the underlying non-perturbative theory and going on from there. Some commenters over there think "It's hard to even begin to imagine how one can even take Woit seriously.", but it looks like they take seriously Harlow's claims that this "seems just as scientific to me as quantum mechanics", with the minor difference that you can't calculate anything.

Comments

1. **Bee**
   December 26, 2015

   I preordered the book, but it hasn't yet been shipped. Looking forward to reading it.

   As to experimental evidence. This isn't an issue specific to string theory. Actually I think that string theorists are more interested in phenomenology than those working on other approaches to quantum gravity, where (aside from lip confessions) the interest is a grand total of zero.

   My point of view has always been that quantum gravity isn't science as long as one doesn't at least try to find experimental evidence. The only other thing worth doing is to prove that one particular theory is the only possible one given certain assumptions, and there doesn't seem to be much effort devoted to this either. (And why would they? We'd end up in a situation where we would argue about which assumptions are the right ones.)

   Why are there so few people working on finding experimental evidence for quantum gravity? I can tell you it's not for lack of interest, at least not among the young people, it's for lack of funding. I get a lot of requests from students and postdocs who want to work with me, but I have to turn then away because I have no funding. I barely managed to get funding for my own research (kudos to FQXi). And lest you think it's because I am just a mediocre scientist, let me add the following twist.

   At some point I concluded that quantum gravity phenomenology is a bad, bad field to work on because it flops through peer review, which is almost certainly conducted by people who in the majority think that funding should go into further mathematical speculations. Seeing that my colleagues who work on AdS/CFT have funds thrown after them, I applied for funding in that field too. Nevermind that I have basically zero prior experience and the field is entirely overpopulated already. I got on first try funding for two postdocs positions (or actually for my own position, a complication that I still have to sort out).

   Then I have to listen to certain Nobelprize winners who insist that the reason scientists work preferably in one field and not another has nothing to do with funding, which eventually is allocated by peer opinion, leading to a rich-get-richer trend. No, scientists are totally unaffected by this and only follow their personal interests, in a completely objective and unbiased manner. So now I work on AdS/CFT (or rather, my postdocs do). Completely unbiased by funding opportunities.

   The problem is that the people at top institutions, the ones with the big names and the many awards and the big prizes, are the ones who we read about in the press. And they have by and large pretty much no contact to the reality of research life.

   Why don't journalists for a change call up some dozen string theory postdocs and ask them how they came to work on what they work on and why they work on what they work on? I have asked this question to a lot of postdocs over the years and I can tell you what the replies would most likely be. "I read Brian Greene's book," is a reply I got to hear a lot. But besides this it is mostly "I wanted to work on quantum gravity and string theory was the only option." And most young students don't realize that they are very likely to get stuck on the topic of their PhD thesis, unless they switch fields very quickly afterwards. And so it happens. These are the stories the public does not get to hear about.

   Sorry for the rant, I find it all very frustrating, seeing that so little has changed in the last ten years. And happy holidays to you too 🙂

2. **Giotis**
   December 26, 2015

   I don't see a contradiction or a distinction here.

   By now it is more than clear, even to its critics I believe, that String theory and the generic Stringy framework with all of its tools is more than relevant for Nature; it has been proven again and again that it can be used to help us answer some (if not all) of the deepest questions about the fundamental nature of reality and in the process it raises new ones which is equally important.

   The theory is at the center of a web of deep ideas binding them together; but the picture is far from being complete or clear; in that respect people have no choice but to follow the theory and see where it leads. To do so they must explore all of its streets and highways and this is what people do I guess; phenomenology is one aspect of this endeavor.

3. **Warren**
   December 26, 2015

One thing even string people seem to forget is the reason string theory got started in the first place was the experimental fact that both the spectrum and small-angle scattering of hadrons follows linear Regge trajectories that are predicted only by string theory. The facts that the known string theories predicted the wrong intercepts and spacetime dimension turned phenomenologists off to this approach.

4. **Jesper**
December 26, 2015

@ Bee, I would suggest that one reason why Quantum Gravity phenomenology is not very popular to work on might be the obvious one that we don't yet have a credible theory of quantum gravity that produces predictions – and without that we don't know what experimental evidence to look for. Am I wrong?

In all this debate about quantum gravity, experimental evidence and whether we should change the notion of science I would say that a credible test of a 'final theory' is that it should essentially postdict the entire standard model – or at least most of its essential structure such as number of generations, gauge groups etc. Nothing less. And until we have that – and the wait might be infinite – then the question of a final theory should simply be left as 'undecided'.

Also, I can report that the "No, scientists are totally unaffected by this and only follow their personal interests,…" is not correct. I think that we all have a responsibility and obligation not to give in to the funding pressure and insist on following our own interest – essentially at all cost. Because if we don't we become corrupt, we become part of the problem.

Of course the system sucks – but systems often do! And nothing can prevent someone from just doing whatever he/she wants if he/she insists – there are other ways to fund research than through research grants and university positions.

5. **Peter Woit**
December 26, 2015

Bee,
Thanks for the comments. The AdS/CFT phenomenon is quite remarkable and deserving of its own book. I see that the Maldacena paper is up above 10,000 citations, way off scale anything that has ever happened in the history of physics. What I've heard time and again from young theorists is that they go into AdS/CFT because it's something that seems to be not understood, seems to be both a new deep idea about quantum gravity, and to have other applications, and, of course, it's where most of the jobs are. The coverage of AdS/CFT in Conlon's book reflects this, with hype level a bit less than the usual, still somewhat uncritical to my taste. An accurate take on this story awaits another book, one by someone who has mastered some fraction of those 10,000 papers and is able to cut through the hype and seriously evaluate what is known, what isn't, what works and what doesn't. Unfortunately I don't think we'll see this soon.

6. **Peter Woit**
December 26, 2015

Warren,
Some things about the Conlon book I didn't mention is that he's rather skeptical about how successful AdS/CFT has been as a technique to study QCD. He writes somewhat wittily:
"This research, aiming to reproduce the properties of the actual strong force, has now been carried on for over a decade, with more or less degree of rigour and less or more degree of success."

One place where I strongly disagree with him is that he argues essentially that it's not worth working on strongly-coupled QCD, that this is no longer an important frontier problem in physics, so does not deserve to be funded.

7. **Conrad**
December 26, 2015

Bee, Peter,

quantum gravity is a tough field to work in for two reasons. First of all, people honestly are looking for observable effects. This prevents them from going to far into fantasyland. Secondly, many models predict the lack of such observable effects.

Even the simple prediction of an observable effect would make a researcher famous. But no prediction has ever been formulated that stood up to the test of the community.

My personal impression, following the field in the past decade, is that no observable quantum gravity effect exist (except the cosmological constant, of course). Of course, this conclusion is tough to swallow. But if the impression is correct, it is a wise move to change field!

We can also ask the opposite question: Bee, why are you convinced that other observable quantum gravity effects should exist in nature? There must be some reason(s) for your gut feeling and for your – admirable! – tenacity. Can you try to formulate it?

8. **Shantanu**
December 26, 2015

Peter: Is Conlon a physicist? What does he critic about LQG? Does he say anything about asymptotic safety or CDT?

Bee, Peter : Yes unfortunately there is a lot of hype about certain trendy fields (ADS/CFT been one). Another one been is calculating the GW signal from black hole/neutron star binary where gravity division of NSF has thrown in lots of money.
OTOH no funds for QG phenomenology and also various other interesting topics
in GR and alternate GR theories. However weren't institutes like Perimeter institute
and other such places set up to give freedom to people to work on non-mainstream ideas? It looks like they are also following fad trends.

9. **Bee**
December 26, 2015

@Jesper,

The whole point of phenomenological models is that you work them out to test for "new physics" even if you don't know the complete underlying theory. In quantum gravity, the most common examples are space-time fluctuations (eg decoherence caused by), defects in the structure of space-time (deviations from GR), violations or deformations of Lorentz-symmetry, consequences of the Planck length setting a limit to the resolution of shortest distances, and so on. All these are possible consequences of quantum gravity that can be searched for by help of phenomenological models.

In particle physics there is a whole industry behind producing phenomenological models that quantify "beyond the standard model physics" so that data can be analyzed for traces of it. We've spent the last 3 decades hunting for particles that aren't there. I think it's more than time to look for something more promising.

Incidentally, you are wrong in saying that it "isn't popular". It is in fact very popular, which has its own downsides. Every time someone writes a paper that supposedly tests something quantum gravity, it is all over the news. Unfortunately, much of this research is rather shallow and low quality, an issue that would be remedied by better funding.

And your remark "of course the system sucks" is one of the most common excuses I hear. So common you will find it mentioned on the slides of a recent talk that I gave as the main cause of the problem. There isn't any "system" other than us.

10. **Bee**
December 26, 2015

@Conrad,

My main point is really the following: If you don't think that quantum gravity has observable consequences, then it isn't science and no science funding should go into it. If you invest into quantum gravity as research in a scientific discipline, you also must invest into studying its potentially observable effects. Where are the dozens of research groups dedicated to quantum gravity phenomenology?

As to my "conviction". In brief I'd like to quote the "principle of finite imagination," that says just because you can't imagine something doesn't mean it's impossible. Over the history of science, it has happened over and over again that something once deemed impossible became possible. I haven't come across one single convincing argument that no quantum gravitational effects can be detected. Why are you so convinced that it isn't possible? And just to be clear, I don't necessarily mean direct effects.

To speak of my personal opinion, presently the most promising areas seem to me CMB entanglement and massive quantum oscillators. Both of these only test the perturbative end, but at least that would move quantum gravity into the realm of being an actual science. Imagine how awesome this would be. You'd think that hundreds of people work on this. Instead it's maybe 10 and I'm being generous on that count.

The non-perturbative end is of course much more difficult. You would want to look at relics from the early universe, and to solve the inverse problem one would almost certainly have to combine different observables.

Another possibility are naked singularities. It has been claimed some years ago that naked singularities (contrary to expectation) might actually be created in gravitational collapse without requiring very special initial conditions. This would mean that we potentially could have uncensored view of strong curvature regions. Remains the question how would you tell a naked singularity from a black hole and what could we learn from this?

You might not agree that these are interesting experiments. But getting an answer for which experimental avenue is the most promising is exactly why this field needs funding.

11. **Jesper**
December 26, 2015

@ Bee

perhaps we have misunderstood each other slightly. I read your first comment as a complaint about the system, which eventually lead you to apply for funding in a field, which you are not interested in, namely AdS/CFT – and that you are now working there (or your postdocs). My reaction is that I find this a wrong choice of action. I do not like "bread-and-butter" type research, where people do research in order to survive and progress in the system as it is now. There is way too much of that. I think that funding should not play a role when choosing a research subject.

And I completely agree with you when you write that there is no system except us – and I would love to see more people try to rebel against it.

To change a system takes sacrifice, it is always easier to work with it. In my own case I have now worked for more than four years without research funding – essentially because I refuse(d) to work on subjects, which might have secured me a career. I don't see any law of Nature that dictates that you can only do research on university funding.

As to phenomenology I am certainly no expert. There is of course a difference between BSM physics and quantum gravity phenomenology – and my take on the latter is very much aligned with what Conrad wrote in the above. I suspect that the only observable effect of quantum gravity might be the standard model itself.

12. **Lee Smolin**
December 26, 2015

Dear Bee,

I agree with your main points but would disagree when you assert that "those working on other approaches to quantum gravity, where (aside from lip confessions) the interest (in phenomenology) is a grand total of zero." Here is a list of potential quantum gravity phenomena which have been proposed and studied over the years by different people.

-Lorentz symmetry breaking.
-Deformation of Poincare invariance.
-Dispersion by Planck scale discreteness of quantum geometry.
-CPT violation
-parity violation connected to lorentz symmetry breaking.
-parity violation in gravitational wave production by inflation leading to detectable effects in tensor modes.
-Corrections to Hawking radiation, coming from planck scale geometry, observable in patterns of lines superimposed on thermal Hawking spectrum, potentially observable in bursts coming from evaporating primordial black holes.
-Planck star models of black holes as sources of fast radio bursts.
-Effects at low l modifying the CMB spectrum coming from a bounce replacing the initial singularity.
-Diffusion in momenta of quanta propagating on causal sets.
-Dimensional reduction, ie modifications in the spectral dimensions of quanta propagating on quantum geometries.
-Gravitationally induced non-linearities in quantum evolution (Penrose, D'iosi etc).

Apart from pure quantum gravity phenomenologists, these proposals have come from people studying LQG, spin foam models, causal sets. asymptotic safety, causal dynamical triangulations, emergent gravity. As you know well, because you are an organizer of it, there are enough people working on quantum gravity phenomenology to populate a 80-100 person meeting every year or two.

I agree that so far no novel phenomena have been observed, but we do have impressive limits on some of these, some of these above the Planck scale.

Thanks,

Lee

13. **Gregor**
December 26, 2015

@Peter:

10000 citations is really a lot. But the statement that this is "way off scale anything that has ever happened in the history of physics" is exaggerated. It is well known that the largest fraction of physicists work in the field of condensed matter physics. In this context electronic structure methods are the theoretical workhorse. Therefore it is natural that a search for the most highly cited articles in APS journals mainly reveals articles related to electronic structure theory. These articles are more often cited than Maldacena's paper. Here is a (very incomplete) list:

Physical Review 136 (3B) B864; Physical Review 140 (4A), A1133; Physical review letters 77 (18), 3865; Physical Review B 45 (23), 13244; Physical Review B 23 (10), 5048; Physical Review B 46 (11), 6671; Physical Review B 41 (11), 7892; Physical Review B 59 (3), 1758; Physical Review B 13 (12) 5188; Physical Review B 54 (16) 11169

Of course, this only tells us that there are more people working in this other subfield of physics or, perhaps, that the citation manners are different in these two areas. If Maldacena's paper is a basis for the usage of certain mathematical structures in different areas of physics then an explanation for the high citation numbers might be that it is interesting for a much larger crowd of physicists than only the string theory community.

14. **4gravitons**
December 26, 2015

Interpreting Harlow's post as arguing that the standard model is a postdiction of string theory seems fairly disingenuous, as the clarifications that he added to the post should show. String theory does get quite close to reproducing the standard model, and the obstructions there do indeed appear to be technical. That doesn't mean it uniquely reproduces the standard model as the only possible stable state, or even that the standard model is the most likely such state, just that the standard model appears to be one possible configuration. Meanwhile, as Harlow clarifies, the "postdictions" he's referring to are things like the existence of gravity, not the standard model.

15. **Neil**
December 26, 2015

"It represents incontinence of speculation joined to constipation of experiment."

Very quotable.

16. **Peter Woit**
December 26, 2015

Shantanu,
Conlon is a well-known young string theorist. His criticism of LQG is fairly long and I can't reproduce it here (and am not interested in engaging in that particular argument). I don't recall anything about asymptotic safety or CDT, and as I noted I don't think Conlon is fundamentally that interested in the quantum gravity issues, for reasons he explains.

4gravitons,
What Harlow is referring to by "it comes very close [to completely reproducing the standard model] and the obstructions seem more or less technical." are arguments that you can get essentially any low energy physics out of "string vacua". I think he's found the most misleading way possible of saying that string theory is a completely empty idea as far as particle physics goes. Any one who wants to can read what Harlow wrote, read what I wrote, and decide who it is who is being disingenuous here. The added material you mention is mainly his response to Scott Aaronson's pointing out to him that his claim of no real distinction between postdictions and predictions is nonsense.

I did go take another look at the Facebook posting, noticed that after writing that long piece of hype, Harlow's attitude is that he's now too busy doing serious science to respond to any criticism of it. I would like to see him respond to Conlon, who explains clearly what the problem with Harlow's claims about the multiverse are.

I also see that writing a posting mainly saying nice things about a book defending string theory and wishing everyone happy holidays draws comments from string theory ideologues about what an embittered person I am….

17. **Peter Woit**
December 27, 2015

All,
Please, if it's not about the Conlon book or the Siegel or Harlow pieces, please resist the temptation to submit comments, especially those about how you feel about quantum gravity…

18. **Bee**
December 27, 2015

@Jesper,

Yes, I think you misunderstood this, sorry. I am still working on qg pheno. But I am feeling rather stupid about it to be honest. Not because I think it's not a good research area, but because I am willingly sabotaging my future prospects of continuing to work in academia at all. (Actually, I wanted to leave, but then the money came through. So now I have two years to figure out what to do next.)

Your idea that scientists 'should not' be affected by funding opportunities is just totally past reality. People go where money goes. Show me any reason why I should believe this isn't so in science.

The people who 'rebel' against the system are the ones who are forced to leave. That's reality. Consequently the ones who stay are the ones who are, by and large, fine with what's happening because they themselves benefit from it.

19. **Bee**
December 27, 2015

Dear Lee,

There are enough people to populate an 80-100 person meeting, yes. The next one of which, incidentally, is due next year and doesn't have any funding whatsoever. As I said above, it's not that there is no interest in the topic. This isn't the problem, especially not among the young people. The problem is that there's no funding and no positions.

The vast majority of people working on these topics do it as an occasional on-the-side paper because it's not research that one can actually live from.

How many people can you list who have a position that pays full time for qg pheno? I could come up with maybe ten. How many positions have there been in the last decade searching for candidates in qg pheno? I know of maybe three. How many research groups on qg pheno are there in the world? Two? How many positions have there been for LQG and string theory in comparison? How many departments have string theory or LQG groups, or the occasional other theoretical (mathematical?) approach to quantum gravity. Right. Do you really believe that this is a good balance between theory development and phenomenology?

Best,

B.

20. **Low Math, Meekly Interacting**
December 27, 2015

I didn't think Siegel's piece was so bad. A bit extremist perhaps, but maybe more people need to stick their necks out a little and advocate for staunchly conservative point of view, even if it's a bit mythical. I'll take that over "post-empirical science" any day. I've said more-or-less the same thing before, but in just about every other branch field of physics (or any other branch of science, for that matter), there's no need for these philosophical debates. Contact with experiment is taken for granted, and difficulties that are generally down to instrument sensitivity or funding. If quantum gravity and unification research could be sufficiently compartmentalized, I doubt anyone but a few esoterists would care about the debate, such as it is. But it can't be compartmentalized. Many scientists from all disciplines and millions of non-scientists alike idolize people like Einstein and Hawking. It's human nature to be fascinated, even spiritually moved, by the search for an "ultimate theory". So when the very people who are perceived as holding this knowledge go around re-defining science to fit a "post-empirical" paradigm, the effect is disproportionately influential on public perception, and, in my opinion, highly damaging. Maybe Siegel missed some nuances. There's so much nuance in the discussion already veritable geniuses can't agree on the most basic level about what "science" is anymore. I'll take Siegel's perspective over that alternative any day.

21. **Jesper**
December 27, 2015

@ Bee

"Your idea that scientists 'should not' be affected by funding opportunities is just totally past reality. People go where money goes. Show me any reason why I should believe this isn't so in science.

The people who 'rebel' against the system are the ones who are forced to leave. That's reality. Consequently the ones who stay are the ones who are, by and large, fine with what's happening because they themselves benefit from it."

I actually agree with you – but I don't think it has to be that way and I don't agree that people, who are forced to leave, have to stop doing research. Why is it a black and white picture, where either you are 'in' and doing research or you are 'out' and not doing it?

Imagine if you applied the same logic to artists – do artists stop producing art when they have no money? No, of course not, they eat porridge for a while, live on the dole, find a patron, whatever. I think that science and art are related in this respect, its a question of passion, idealism –

– the only example I can give you (but I'm sure there are others) is myself. I was forced to leave some years ago – my research is way off mainstream, but I publish in top journals – and I have continued my work and have manage to get funding elsewhere.

I see very little reason to stop doing what I love just because the flow of cash stops – I just have to adjust and push on, and I'll do that as long as I believe in what I do. This is the type of rebellion I would wish to see more of. If the funding system sucks, well, then find your money elsewhere! If you really believe in what you do, then you'll find a way – I don't agree that being a scientist is a profession – to me its a lifestyle, a passion, a vocation.

22. **Shantanu**
December 27, 2015

Jesper : if you don't mind, could you list the sources from where you get funding, since I am sure that would be of interest to many people. FWIW, I know another person, who had a hard time getting a job (or invitations for conferences/seminars) for about 8 years, but still published papers in top quality journals by working in evenings and weekends and taking a day job in biophysics to pay bills. eventually he did get a job in a teaching university with time available for research. But I agree with Bee that probably such cases are almost non-existent. Maybe you and him are the only current examples.

Lee : once upon a time PI had an option where anyone could propose to organize conferences as long as the LOC/SOC involved one PI resident faculty and it would provide logistic support. Is such a service no longer available? At any rate I do hope that PI would organize the next incarnation of the QG phenomenology conference to keep the tradition.

23. **Jesper**
December 27, 2015

@ Shantanu,

I know that there are very few examples of people who work without research funding. My point is that I don't see why it need to be that way. An example: Schwarzschild did his important work while fighting in WWI. History is full of people who didn't stop just because conditions were a little (or very) challenging. But it appears that today people just stop when they don't get paid to do research. This I don't really understand. I think that the analogy with artists is relevant.

As to your question: I don't want to expose my private finances here, I can just say that at the moment I finance my research out of my own pocket.

Perhaps one can turn the question around: isn't it conceivable that one can get better research conditions outside the universities, where so much time is now taken up with things that has nothing to do with research?

24. **Peter Woit**
December 27, 2015

Jesper/Shantanu,
This has gotten completely off-topic. Whatever the prospects are for financing one's research oneself (easy for some people, impossible for many others), efforts like Sabine's to hold the current system to account are extremely important. Conlon's book discusses funding issues extensively, and his efforts, like those of Harlow, have a

lot to do with fighting to preserve funding for string theory in the face of bad PR like Siegel's article. None of them are planning on giving up this fight in favor of doing string theory in their spare time.

25. **4gravitons**
December 27, 2015

@Woit,

I do appreciate your perspective on Conlon's book, but I wasn't intending to gloat about it. 😉

(More seriously, I suspect you'll see more pieces like that as time goes on. Science communication seems to work by establishing tropes and building on them, and the sort of tropes Conlon is working with are a newer invention, the result of a younger string theory community finding its voice.)

I just think you're being more than a little unfair to Harlow. Yes, he's using older tropes…but in the context of a (non-Sean-Carroll) facebook post. Most of his expected audience understands what he's referring to, or at least are people like Scott Aaronson who are likely to ask for clarification. I doubt Harlow expected his post to go viral.

26. **Peter Woit**
December 27, 2015

4gravitons,

I don't see Harlow as using older tropes, but actually newer ones, albeit highly extremist ones. Does anyone not trained at Stanford believe claims like his that the reason we don't currently have testable statistical string landscape predictions is "a purely practical problem, not one of principle"? I don't think there's any way to construe that statement as anything other than a piece of outrageous hype. Sure, if you wanted to, you could find some way of construing some of his other statements so that they weren't obvious nonsense (e.g. by making his "postdictions" claim not apply to the SM, although he makes it in that context and doesn't explicitly say it doesn't apply there). I think he honestly believes what he's saying, but I also think most physicists recognize it as not science, and if voices like his dominate those like Conlon's, you're going to see a lot more, accurate, "String theory is not science" articles in the future.

One amusing thing about his post. According to his definition, everything I teach and think about all day is "string theory". I hadn't realized that….

27. **Gil Kalai**
December 28, 2015

I liked Harlow's piece. It is clear and short and it makes quite a few interesting points some of which are new for me. I liked especially the following points:

"3) Just because string theory reproduces known physics, that of course doesn't mean that we should declare victory for it. But it does mean that we should take it seriously. In particular one should note that none of its "competitors" have done this.

4) Except for one: the effective field theory of the standard model coupled perturbatively to general relativity (EFT for short). This theory also accounts for almost everything we have observed (except dark matter, neutrino masses, perhaps inflation). EFT is the real nemesis of string theory: what would convince us to prefer string theory to it?

5) I would say that there are two basic reasons by which we can prefer one physical theory to another. One is of course if one theory correctly accounts for observed phenomenon that the other doesn't. The other is if one theory explains the same observations using fewer parameters.

6) As far as we can tell, string theory may eventually beat EFT on both of these criteria…"

So Daniel's point is that (at present) the main scientific alternative ("nemesis") of string theory is not the even more speculative and much more partial suggestions for quantum gravity (the "competitors" as Daniel referred to them), but a rather straightforward coupling (EFT) of the standard model with general relativity. (I suppose that like string theory also EFT should not be thought as a single possibility but is quite board. Perhaps not quite as board as ST itself.)

Daniel expects (or hopes) that eventually some variants of ST will beat all variants of EFT (which is a perfectly fine), and, taking for granted the ST framework (which is also perfectly fine), he even regards some major remaining difficulties as of "technological" nature irrelevant to "matters of principle."

There were other interesting points (like the value of postdictions, the distinction between practical matters and "matters of principle" and a few more) in this piece, so I was quite happy to read it.

Regarding the issue of "science". Since I regard mathematics as science the debate regarding this term is little moot for me (interesting questions arising from philosophy of science notwithstanding.)

28. **Peter Woit**
December 28, 2015

Gil Kalai,

Saying "string theory reproduces known physics" is extremely misleading, when what you mean is that it can reproduce not just known physics, but basically anything. As for the idea that string theory reproduces the standard model with less parameters, that's simply untrue. If he wants to hope that string theory will beat EFT on these criteria, he's welcome to do so, but, after 30 years, there's zero evidence of this working. There's a fundamental problem with the way he and other string ideologs argue, which is basically: "First, assume everything we would like to be true is true, then…" As for the claim that the problems are just "technological", not "matters of principle", I think that is not just hype, but absurd hype.

29. **vmarko**
December 28, 2015

Regarding the "technological problems" of ST, it would be really good if people would remember the extremely sarcastic way in which Pauli was mocking some of Heisenberg's similar statements, a century ago. Namely, he took an empty canvas, put it in a frame, and said "This proves that I am as good a painter as Titian. Only some technical details are missing."

Also, I often hear the off-the-cuff statement that general relativity can be combined with the Standard Model in an EFT. This isn't really true — one can only combine them classically, i.e. take the classical limit of the SM and plug it into Einstein's equations. There is no such thing as an "effective quantum theory of gravity", be it GR+SM or otherwise. Looking at low-energy behavior (i.e. putting a cutoff at some scale) isn't enough — one needs to define the path-integral measure for the effective gravitational degrees of freedom (if one is to have a quantum theory at all), and this is an unsolved problem even in the context of EFT.

This is in contrast to the SM in flat spacetime, which certainly can be regarded as a low-energy EFT of some more fundamental theory. The measure is there, the dof's are known, everything is well-defined. But for gravity none of this works, because we don't know what are the dof's, and because of measure issues — and this *despite* the presence of a cutoff.

So I'd say that neither ST nor EFT can describe all observed experiments. ST has trouble with the matter sector, EFT has trouble with the gravitational sector. Lose-lose.

Best, 🙂
Marko

30. **Anonyrat**
December 28, 2015

Marko,
What is the experimental result or observation that EFT has trouble with?
Thanks in advance!

31. **vmarko**
December 28, 2015

Anonyrat,

The trouble with EFT is not experimental, but theoretical: there is no theory! Nobody is able to write down the equations for an effective low-energy quantum theory of gravity. The measure of the gravitational path integral is not well-defined, so the theory does not exist, let alone experimental predictions. This was known back in the '60s and '70s, when DeWitt was studying the measure for the gravitational path integral. Already then it was obvious (to the well-informed) that any discussion of QG requires one to formulate a UV completion, precisely because an effective field theory doesn't exist. If a QG EFT were possible, by now people would have long forgot about strings, LQG and other Planck-scale theories, and be content with an effective low-energy theory.

Best, 🙂
Marko

32. **Peter Woit**
December 29, 2015

Sorry, but I have to keep pointing out that this is not a quantum gravity discussion board. If people want to discuss Marko's comment with him, please contact him directly.

33. **amused**
December 29, 2015

Since Conlon apparently discusses sociological aspects, did he explain why, in competition for jobs in particle theory, N single-author publications in Phys.Rev.Lett. by a non-string researcher (N being any number between 1 and infinity) count for flat zero when weighed against just one article in, say, PRD by a string researcher where he/she was one of eight authors on the paper with the others all being more senior people?

34. **Peter Woit**
December 29, 2015

amused,
No mention of that…
More generally, I didn't notice much awareness of the problem of faddishness, of pressure to work on certain designated "hot" topics if one wants a career in the business.

35. **srp**
December 29, 2015

I just read Maguejio's 2003 book on VSL (found it used). Minus his profanity and ageism, the same points about U.S. theorists all working on the same trendy stuff and getting funded only for doing it is in there. It would be nice to see some systematic evidence about this funding issue.

36. **Peter Woit**
December 29, 2015

srp,
Quantifying the grant issue in the US would be a bit tricky, for one thing many grants are to groups. As many US HEP theorists will tell you, grant amounts have been going down, as far as I know, independently of area people work in. It would be interesting though if anyone has any data.

One somewhat more promising way to quantify faddishness and which fields are hot would be to look at the postdoc hiring data here
https://sites.google.com/site/postdocrumor/
You'd have to figure out how to categorize people's research fields, which would not be so easy.

In Conlon's book, one thing he emphasizes is that he has had to convince committees of non-string theorists to give him a grant (and has been successful). He emphasizes quite a lot that connection to experimental testability is an important part of his work, and seems well aware that that argument is now an important one to be able to make if you're a string theorist and want a positive evaluation from other physicists. That may not actually be a good thing, if you believe that the current state of string theory cannot successfully make contact with experiment, that the best hope for the future is more formal work that would uncover something new about the underlying theory, something with a better chance at connecting to experiment.

37. **Scott Church**
December 29, 2015

Peter or Anyone,

Regarding grant requests and/or postdoc hiring for string theory research emphasizing "[connections] to experimental testability," how many of these proposals are based on postdictions rather than predictions? Can such proposals be taken as a sign that things are trending away from [Texas sharpshooting](), or what might be called the Kane Effect? If so, how many of these proposals seem to show real promise?

38. **Peter Woit**
December 29, 2015

Scott Church,
I think most people hoping to get a "test of string theory" are looking for predictions, would be happy though to get a postdiction, and this hasn't changed. As I've documented extensively on this blog, the story of "tests" of string theory is that they're never tests that can falsify string theory. They refer to some possible observable effect in some model, a model supposed to have something to do with string theory (sometimes just "string-inspired"). The problem is that if such an effect is not observed, that just means you change to a different model. Kane's "predictions" are of this kind, and Conlon's work on observable effects of moduli is also of this kind (Kane is different in the way he markets such "predictions", in the much stronger claims for them he makes). I don't think any of these show significant promise, since the models being invoked don't actually plausibly explain anything we know about.

Conlon I think does explain this fairly clearly. The motivation of his search for models with observable effects of moduli is not that string theory implies such specific models are likely. It's more that string theory provides the inspiration for looking at such classes of models, which give new ideas for unexpected things to look for. While such things are unlikely to show up, if one did see them, they would indicate dramatic new physics of some kind.

39. **life of brian**
December 30, 2015

[Low Math Meekly "Interacting] I didn't think Siegel's piece was so bad…[]…maybe more people need to stick their necks out a little and advocate for staunchly conservative point of view"

It's certainly one possibility…that Siegel has taken a courageous stance for deeply held principle. Another possibility is the severe to terminal String Theory critique in media has lately begun to fizz.
Finger in the wind or principled and potentially self-sacrificial harsh criticism? In the end you'd have to decide what else you'd expect to see given one, or the other respectively.
Evidence of serious long term consideration perhaps. One way to get an indication on that might be to ask what are the hackneyed positions out there. If the space is starting to fizz, then chances are the ill thought through, pointless or point-missing, components of criticism are the ones becoming commonplace so hackneyed, so the ones showing up disproportionately right now. How's he doing so far?

40. **Peter Woit**
December 30, 2015

life of brian,
More or less identical arguments over string theory have now been going on for a decade or so, without a lot of change. The only thing that I see having changed in the last couple years is that the LHC results are shutting off the last hope for some observable new physics relevant to string theory (pre-LHC a common argument argument from string theorists was that connection to experiment would be made through the discovery of SUSY at LHC energies).

The naive "it predicts nothing observable so it's not science" critique of string theory is now more powerful than it used to be, as more and more string theorists adopt the attitude that nothing observable is not a problem for the theory, attracting a reaction like that of Ellis/Silk. My impression is that Siegel is well-aware of some of the subtleties of the arguments over string theory, also aware of the fact that the naive critique has gained force from recent developments, so he thought it was worth making now (popular science articles in Forbes aren't a place you can really do subtlety).

41. **Greg**
January 1, 2016

Peter,

is string theory probable at all? I ask for the following (humorous) reason. A supersymmetric theory has about 105 free parameters. Each parameter can have at least 10 million possible values (a rough guess). That makes 10^735 possible parameter combinations. But the landscape/multiverse has only 10^500 options. I would deduce that there is only a probability of 10^-235 that standard model plus gravity is part of the landscape.

What is wrong with the reasoning? 🙂 Happy holidays to you too!

42. **Ignatz Ratzkywatzky**
January 3, 2016

Going through John Baez's Crackpot Index, I note that the last and highest scoring item is #37:

"50 points for claiming you have a revolutionary theory but giving no concrete testable predictions."

Hmmm.

End of Year Links

December 30, 2015
Categories: Uncategorized

A collection of links to round out the year:

- The Seminaire Bourbaki talks this January look unusually interesting. Luckily I'll be in Paris at that time.
- For an end of year present, Jacob Lurie has posted a version of his unfinished next book, Spectral Algebraic Geometry. It's advertised as much more user-friendly than previous versions of the same material and that's quite true after reading the first chapter.
- If 850 pages or so of this sort of thing isn't enough to keep you busy during the break between terms, try Lurie's Harvard colleague Dennis Gaitsgory's A study in derived algebraic geometry, a book project with Rozenblyum, also in a preliminary version (around 1100 pages), with more to come. I'm hoping for the more user-friendly version of this one…
- Also from Harvard, videos of last month's Current Developments in Mathematics talks are now available here. At least the first of Peter Scholze's talks is rather user-friendly.
- Very, very user-friendly (especially if you read Japanese) are the Japanese television versions of Edward Frenkel's talks earlier this year at MSRI, available here.
- If you just can't get enough of the new 750 GeV particle, you probably should read Tommaso Dorigo's take on it.
- Back when I was writing about the AMS's role as a mouthpiece for the NSA in its attempts to mislead people about their role in backdooring an NIST crypto standard (see here and here), one thought I kept in mind was that since this standard supposedly was never used in anything important, maybe one shouldn't get so upset. Recent news (see Matthew Green for an explanation) is that this bad crypto actually was used in something quite important: widely used firewall/VPN hardware from Juniper Networks. Quite likely this was used by the NSA to get access to much of the traffic on a wide variety of networks.

  The story is actually much more complicated than one can believe, with a still unclear sequence of changes in the software indicating that others, possibly a foreign government, took advantage of the NSA backdoor to compromise these systems. Green points out that this makes very clear the problem with government-mandated backdoors: even if you trust the government, they make it much easier for others to take advantage of the security problems they have introduced:

  > One of the most serious concerns we raise during these meetings is the possibility that encryption backdoors could be subverted. Specifically, that a backdoor intended for law enforcement could somehow become a backdoor for people who we don't trust to read our messages. Normally when we talk about this, we're concerned about failures in storage of things like escrow keys. What this Juniper vulnerability illustrates is that the danger is much broader and more serious than that.
  >
  > The problem with cryptographic backdoors isn't that they're the only way that an attacker can break into our cryptographic systems. It's merely that they're one of the best. They take care of the hard work, the laying of plumbing and electrical wiring, so attackers can simply walk in and change the drapes.

- If you just can't get enough of my and other people's views on string theory, Ben Winterhalter has a piece on the Jstor blog, telling the story of his attempts to figure out what's going on with extra dimensions.
- Among the many great articles at Quanta, I can recommend this one, which features my Columbia colleague Wei Zhang.

Happy New Year!

Comments

1. **Thomas**
   December 31, 2015

   Thanks! Have a nice change of year too!

2. **John Fredsted**
   December 31, 2015

   Thanks for all the nice blog posts this year, always interesting to read. I am looking forward to new ones in the year to come. Happy New Year!

3. **uair01**
   December 31, 2015

   Agree with the others: many thanks for the blog and have a good New Year.

4. **Tim May**
   December 31, 2015

   Many thanks for your blog over the years. I especially appreciate the links to mathematics work. It's through this blog that I first heard about the Langlands project. And it does appear that the most interesting "fallout" of the string theory effort has been in the AdS/CFT work.

   I arrived on your blog some years ago, maybe around 2004 or so, just before your book, but possibly just after Smolin's book. Not sure. I know I had read his "Three Roads" book.

   I'd been unpersuaded by a string theory popular book I read around 1987 or so, from before the later changes, and then still unpersuaded by the Brian Greene book. And so I was pretty skeptical of the hype.

   However, I think things have gotten a lot healthier in recent years. Some of the hype of the "we now know that everything is vibrating strings, a symphony of different chords" has settled down.

   I've seen several talks by Lenny Susskind and am much impressed. (Not a lot of talk about string theory, though.)

   Anyway, Dr. Woit, I have enjoyed your blog as a breath of fresh air. And I appreciate the many links to modern mathematics. (I am still amazed that even pure mathematicians like Gaitsgory refer to physics!)

   I hope to be reading your blog ten years from now.

5. **geometriclanglander**

December 31, 2015

Frenkel's MSRI lectures were meant to be aimed at undergraduates, but watching the first two, they actually seem to be more appropriate for middle schoolers.

Thanks for the CDM links in particular. Scholze's lectures are truly beautiful. Donaldson's review of Kahler-Einstein metrics for Fano manifolds is also a fantastic overview. The subtlety (and comedy timing) with which he trolls Gang Tian with the '4 different proofs now exist' at the beginning of his second lecture is quite amusing.

6. **Ramsey Glissadevil**
   December 31, 2015

   Happy New Year Peter!

7. **Neil**
   December 31, 2015

   Happy New Year! Looking forward to 2016 and news from LHC. May the 750 GeV bump be with us.

8. **paddy**
   December 31, 2015

   Like the rest above, I send wishes for a Happy New Year and a thanks for all the columns and links.

9. **MathPhys**
   January 1, 2016

   Best wishes for 2016!

10. **Happy New Year**
    January 1, 2016

    For me this is the greatest blog on physics and mathematics. It's serves students, researchers, and the general public.
    Happy New Year Peter and everyone who has posted a comment or simply read this blog.

11. **Peter Shor**
    January 1, 2016

    Happy New Year!

12. **srp**
    January 1, 2016

    Happy 2016!

13. **Dave Miller in Sacramento**
    January 2, 2016

    Peter,

    Thanks for your ongoing coverage of the NSA backdoor cryptography story. It is interesting to note that a news story that broke towards the end of the year is that NSA intercepted communications between Netanyahu and members of Congress and passed them on to the White House. Congress is not pleased… should be an interesting story as it unravels.

    Thanks for all your work on this blog, and Happy New Year.

    Dave Miller in Sacramento

14. **Mario**
    January 3, 2016

    I was searching for this info, but haven't found anything related serios: In 2016, LHC should provide ~10times the number of collisions compared to 2015, which means the 750GeV boson riddle will be resolved. But when will we get trustful information about it? I assume that there will be some high-energy conferences in 2016, where it is expected that ATLAS and CMS will provide updates. Can somebody point to dates and names of such conferences? Or will we find out in other ways (at this blog, like for the Higgs 3.5years ago 😉)? Thanks!

15. **Peter Woit**
    January 3, 2016

    Mario,
    The current schedule has a first period for physics data-taking April 25 – End May and if that goes well they should have more data than from the 2015 run, enough to see if the bumps persist. My impression is that two of the major summer conferences are June 13-18
    https://indico.cern.ch/event/442390/
    and August 3-10
    http://www.ichep2016.org/
    My totally uninformed guess is that June 13-18 is too early to have analyses ready (but I may be wrong, I was surprised how much was done by Mid-December this year, only 6 weeks after the run was over). So, August 3-10 might be the dates to keep in mind. Also, they seem to now like to announce important things at CERN instead of at regularly scheduled conferences, so the dates of the conferences may be irrelevant.

    Post-Higgs, I've to some extent retired from putting much effort into tracking down reliable rumors, but if I hear something interesting, it likely will be a topic here…

16. **Low Math, Meekly Interacting**
    January 5, 2016

    Despite your retirement from the rumor mill, here's hoping something shows up that's worth reentry.

Best wishes for 2016!

17. **Low Math, Meekly Interacting**
January 5, 2016

Nice layman's article on the use/abuse of Bayes' Theorem by John Horgan @ SciAm. Hopefully relevant and useful to your less statistically-savvy readers, given the recent meeting in Munich.

http://blogs.scientificamerican.com/cross-check/bayes-s-theorem-what-s-the-big-deal/

18. **Peter Woit**
January 5, 2016

All,
I second the recommendation about John Horgan's discussion of the Bayes business, but please discuss this over there on his blog. My earlier experience with discussions about this here convinced me that the world is full of people who want to argue about irrelevant aspects of this, and that Joe Polchinski's Munich contribution conclusively proved that the idea of evaluating string theory/multiverse research this way is laughable.

19. **Low Math, Meekly Interacting**
January 6, 2016

OK, sorry! Glad it was worthy of note in your latest post, at least.

Various and Sundry

January 6, 2016
Categories: Uncategorized

Some short items on a wide variety of topics:

- The Hawking/Perry/Strominger paper on a new idea about the black hole information paradox (see here for an early discussion) based on BMS supertranslation symmetries has now appeared on the arXiv. I'm no expert on the intricate arguments about this paradox, so have no idea what the implications of this paper for that really are. However, it does seem to be a very interesting approach to quantum gravity questions (although the paper mostly deals with simpler gauge theory calculations). The ideas are squarely in the mainstream of what has been the most successful way of making progress in fundamental theory: identifying new implications of symmetries that are at the center of our core theories (the standard model and GR). Such a new understanding looks like a far more promising way forward than much of what is currently popular in the subject.
- For an example of what is currently popular, the KITP is hosting a workshop this week of the the It from Qubit Simons Collaboration, on Quantum Error Correction and Tensor Networks. I gather this is supposed to somehow explain AdS/CFT, but I've never understood how this is supposed to come about. Evidently I'm not the only one wondering about this. John Presskill reports that, in his talk leading off a series of lectures on this, Patrick Hayden commented that

  > I'm unsure what we are trying to learn from these tensor network models of holography.

- Tonight PBS will be showing the film Particle Fever, which I wrote about here. It's a great film, highly recommended, despite the larding with comical nonsense about the multiverse (if you believe the theorists in the film, the multiverse is supposed to be tested by its prediction of a mass of 140 GeV for the Higgs). The capsule summary in the New York Times TV listing this morning for the film is "Scientists recreate conditions from the big-bang theory". While the LHC has nothing to do with the big-bang theory, maybe this summary refers to the comedy of the theorists and another well-known TV show, in which case viewers may be a bit disappointed.
- In other LHC related news, the AMVA4NewPhysics project now has a blog, latest posting explains the basics of b-tagging.
- I've never been able to really make sense of many of the arguments about "Bayes's Theorem", and the recent attempts to justify string theory using this just seemed bizarre. John Horgan has a great explanation of what is going on here, including this take on the Bayes/string theory/multiverse business:

  > In many cases, estimating the prior is just guesswork, allowing subjective factors to creep into your calculations. You might be guessing the probability of something that–unlike cancer—does not even exist, such as strings, multiverses, inflation or God. You might then cite dubious evidence to support your dubious belief. In this way, Bayes' theorem can promote pseudoscience and superstition as well as reason.
  >
  > Embedded in Bayes' theorem is a moral message: If you aren't scrupulous in seeking alternative explanations for your evidence, the evidence will just confirm what you already believe. Scientists often fail to heed this dictum, which helps explains why so many scientific claims turn out to be erroneous. Bayesians claim that their methods can help scientists overcome confirmation bias and produce more reliable results, but I have my doubts.
  >
  > And as I mentioned above, some string and multiverse enthusiasts are embracing Bayesian analysis. Why? Because the enthusiasts are tired of hearing that string and multiverse theories are unfalsifiable and hence unscientific, and Bayes' theorem allows them to present the theories in a more favorable light. In this case, Bayes' theorem, far from counteracting confirmation bias, enables it.

- The recent Munich conference trying to justify string theory by Bayesian methods wasn't the only example of European funding for philosophers to weigh in on the latest in fundamental physics. Another just announced European LHC-related project is a 2.5 million Euro research unit aiming to investigate the LHC "from an integrated philosophical, historical and sociological perspective."
- I just ran across a recent paper by Kristian Camilleri and Sophie Ritson on The role of heuristic appraisal in conflicting assessments of string theory. It is very good, unlike almost every other discussion of this topic, I think it gets right the central serious argument of the "string wars": how does one evaluate the prospects for the string unification idea? There is no simple answer to this, you need to understand what the state of efforts to connect a hoped for unified string theory to reality really are, how they have evolved, and try to make a sensible judgment about whether this is a failed idea or whether there is hope left. I highly recommend reading this for those who are not completely tired of this subject.
- In the same journal I noticed another quite good article, by Porter Williams on naturalness. He carefully explains the different incarnations of "naturalness" and I think comes to the right conclusion that it is best thought of as the idea that physical behavior at widely different distance scales should not be correlated. By the way, the name "naturalness" for this is a bit of marketing genius (how could "nature" not be "natural"?).
- In geometric representation theory news, the Simons Center is running a program on the topic this month, videos here. Here at Columbia Roman Bezrukavnikov will be the Spring 2016 Eilenberg lecturer, with his topic "Geometric categorification in representation theory". I believe talks will be Thursdays at 2:40, watch the Columbia math department website for more news.
- Personally, I'm about to head out tomorrow night on vacation, so expect minimal blogging and possibly even shutting off of comments. When I get back, I'll be teaching our spring semester graduate course on groups and representations, see here. Also trying to finish my book on quantum theory and representation theory. Current state (see here, comments always welcome) is that I've gone over and rewritten the first 34 chapters (except the introduction), planning on rewriting and adding material to the rest of the manuscript this semester. This better be done by this summer, partly because that's when it is supposed to be delivered to Springer, partly because I'm already quite tired of this project and want to work on other things…

Update: Any mention of Bayesianism seems to attract a large number of people who want to discuss it, especially aspects that have nothing to do with the string theory/multiverse business. Please discuss this topic with John Horgan at his blog.

Update: Sabine Hossenfelder has more on the Hawking/Perry/Strominger paper here.

Update: Scientific American has an interesting interview with Strominger, who explains some of the ideas behind Hawking/Perry/Strominger. Jacques Distler has come out of retirement at Musings to object that this work violates two central ideological tenets: one should not pay attention to gauge invariance, and the answer to all questions should be string theory or AdS/CFT.

Comments

1. **Jeffrey M**
   January 6, 2016

   Could someone please educate a mathematician about how physicists get to say this in a paper – "Recently such an a priori reason for doubt has emerged from new discoveries about the infrared structure of quantum gravity in asymptotically flat spacetimes." Infrared structure of quantum gravity? Quantum gravity? Did someone come up with a correct theory for quantum gravity while I was sleeping? Is the implication somehow that ANY theory of quantum gravity will have the exact same infrared structure in asymptotically flat spacetimes? Really?

2. **Peter Woit**

January 6, 2016

Jeffrey M,
The famous problems with quantum gravity are ultraviolet problems, so shouldn't be relevant for the infrared structure. And the infrared structure should be largely determined by the fact that you want GR in the classical limit.

All,
Those who want to argue about Bayesianism and Horgan's views should do it at his site.

3. **Kartik Prabhu**
January 6, 2016

@Jeffrey M
Any theory of quantum gravity should reproduce General Relativity in the far asymptotic region ("infrared"). The infrared structure you quote comes purely from studying the symmetries of asymptotically flat spacetimes in General Relativity.

So even if we don't have a full quantum theory of gravity, _any_ such theory better reproduce this structure to be physically viable.

(original post: https://kartikprabhu.com/notes/re-quantum-gravity-infrared-structure)

4. **Jeffrey M**
January 6, 2016

Thanks for the answers, that makes sense. Now I'm only confused about "new discoveries" since they can't mean new discoveries about GR, right? Or do they mean new implications about possible theories which have the right classical limit? Have there been limits found in that direction? Personally, I think Hawking was right in the first place about black hole information loss, and you just don't actually have unitarity, but what do I know.

5. **vmarko**
January 6, 2016

Peter,

Is there a draft or something of the Porter Williams paper that is not behind the SD paywall? I looked it up on the arXiv, and failed. Yet from your description it seems a very interesting article to read, so…

Best, 🙂
Marko

6. **Peter Woit**
January 6, 2016

Marko,
Try the philosophy of science version of the arXiv
http://philsci-archive.pitt.edu/11529/

7. **Kartik Prabhu**
January 6, 2016

@JeffreyM for some context:

The infrared structure here refers to the symmetries of asymptotic flat spacetimes given by the BMS group. This has been known for a long time in classical GR. Some recent work by Strominger relates these symmetries to the so-called "soft theorems" in scattering calculations (involving gravitons) in perturbative QFT. In essence, what it says is, since the BMS group is infinite-dimensional, there is an infinite-dimensional degeneracy in what one would like to call the "asymptotic ground state".

How this shakes out for the information "paradox" remains to be seen.

(original post: https://kartikprabhu.com/notes/re-quantum-gravity-infrared-structure2)

8. **Narad**
January 6, 2016

> I just ran across a recent paper by Kristian Camilleri and Sophie Ritson on The role of heuristic appraisal in conflicting assessments of string theory.

I haven't read it yet, but this is the first time that I've seen an editorial office include a boxed notice of this sort: "When citing this paper, please use the full journal title *Studies in History and Philosophy of Modern Physics*."

Neither CASSI nor Crossref has an entry, and "Stud. Hist. Philos. Mod. Phys." doesn't appear to have much competition on the abbreviations front.

9. **vmarko**
January 7, 2016

Peter,

Thanks for the link! 🙂

The Williams article is an excellent read, cutting straight through the naturalness ideology that has been dominating particle physics for too long, IMO. In a more general context, it is refreshing to see that QFT ideology is on the retreat — first a step back from fundamental QFT to EFT, then the serious criticism (even failure) of naturalness as a guiding principle. The next in line will be renormalization, once enough people gain enough awareness that gravity is nonrenormalizable (which puts a wrench into the gear of renormalizability as a guiding principle, so to say), and demote it to simply a useful technique of studying EFTs. Williams also hints in this direction in the article, but doesn't want to stray too much from his main topic…

The era of QFT dominance is fading. With the latest results from the LHC (mainly evidence of Higgs and no evidence of SUSY), serious cracks are beginning to show in the QFT paradigm. This is important, because people really need to get disenchanted of QFT as a set of ideas that are universally valid. Once that happens, the younger generation of string theorists will gain awareness and courage to step back and ask "wait, what are we really doing here when constructing ST?", and start thinking outside the box.

The Williams article is one of the first, but extremely important steps in this direction.

Best, 🙂
Marko

10. **Anonyrat**
January 8, 2016

…physical behavior at widely different distance scales should not be correlated…

If AdS/CFT relies on the asymptotic spacetime being AdS, but claims to describe string-scale physics, isn't that a correlation of physical behavior at widely different distance scales?

Thanks in advance!

11. **vmarko**
January 8, 2016

Anonyrat,

No, not really — AdS/CFT is a duality between two theories, rather than between two different scales of the same theory. For example, UV range of CFT maps into IR range of AdS, which by itself doesn't tell you anything about the IR range of CFT. And vice versa.

An example that actually does violate naturalness would be the UV/IR mixing in noncommutative geometry models.

Best, 🙂
Marko

12. **Anonyrat**
January 8, 2016

Thanks, Marko!
—
Porter Williams lists some work in progress:
http://porterdwilliams.com/research/
including ""The explanatory failures of the multiverse".

Looking forward to the results of that research!

13. **AdamT**
January 9, 2016

Peter, vmarko,

I am struggling to understand what a physical theory that is highly correlated at different scales would look like? Are we talking about a "fractal" theory here?

Any explanations that would inform intuitions on what these theories might look like from a big picture sense would be appreciated!

Adam

14. **Peter Woit**
January 9, 2016

Adam T,
Simple examples would be free field theories or conformally invariant theories. In some sense what's surprising is the "natural" behavior, that the behavior at long distances is so independent of how you define the theory at the cutoff scale.

15. **vmarko**
January 10, 2016

Adam,

In addition to what Peter said, it is interesting to imagine what nature would look like if Standard Model were not "natural" as much as it is. For example, imagine that all quark and lepton flavors/generations had substantially long lifetime. In such a scenario, despite the fact that the top quark is very heavy and contributes only on TeV scales, the low-energy behavior of the theory (say, chemistry) would be completely different (read: much richer) than it is.

So "naturalness" is a statement that for example chemistry has properties based on up/down quarks and the electron, regardless of what other particles may exist at higher energies. This behavior is, as Peter said, actually very surprising. In an "unnatural" SM chemistry would highly depend not only on these three particles, but on all possible particles that can exist in nature (and I mean *all*, up to Planck scale, not just the particles we have discovered so far). That's what "mixing of the scales" would mean.

IMO, the word "naturalness" is a complete misnomer for the phenomenon.

Best, 🙂
Marko

16. **Krzysztof**
January 10, 2016

Back in late 1940s no "naturalness" was expected – by "common" opinion the QED should die at 100 MeV, or so.

17. **Chris W.**
January 10, 2016

One could argue that "naturalness" (or whatever else one might call it) is extremely important for the development of science. Given "mixing of the scales," the progressive, staged development of physics and chemistry would be far more difficult, if not altogether impossible. (I'm reminded of the importance of "separation of concerns" in computer science and software engineering.)

18. **Thomas Larsson**
January 11, 2016

Naturalness and symmetry are in some sense opposites. In quantum theory we expect parameters to renormalize to generic values, even if the bare parameters vanish, unless:
1) some symmetry principle forbids it.
2) the bare parameters are finetuned.
Thus symmetry and finetuning result in the same experimental signature: non-generic values for parameters. That the Higgs mass seems to sit at the only a priori non-generic value – the boundary of the stability region – is rather striking from this pow.

19. **M**
January 12, 2016

Schellekens posted an interesting paper (Big Numbers in String Theory, http://arxiv.org/pdf/1601.02462.pdf) with a funny joke about landscape scanning:
«We were hit directly by the Big Number problem because the computer program experienced some mysterious segmentation faults each time it was starting to explore a very large hidden sector. We finally discovered that this was due to a message stating something like "remaining time …. years; aborting". The number of years, expressed as an unlimited size integer, was so large that it overflowed the text buffer allocated for it.»

20. **zzz**
January 12, 2016

lots of good bits in that paper

from the abstract:

"…I correct a huge but inconsequential error, …"

21. **Zathras**
January 13, 2016

So Peter have you heard anything more about the LIGO rumors which have been going like crazy over the last few days?

22. **Chris W.**
January 13, 2016

Another: "There are no negative results in science, just bad expectations." 🙂

23. **RM**
January 13, 2016

Hi,

Loved that statement about the moral message in Bayes theorem. As an ex-particle physicist on Wall Street, I would say to my former colleagues that you have no idea how good you have it! Abuse of Bayes theorem and statistics is rampant in the business world to the point that correct usage is almost a cause for suspicion.

Best,

RM

24. **Larry Lurio**
January 17, 2016

On the subject of statistical bias and LIGO there is a very nice book by Harry Collins called "Gravity's Shadow". He looks at statistical bias more from a philosophical point of view than a mathematical one. The book focuses on experimental bias in the search for gravity waves. One very interesting story that comes out of this is how the LIGO collaboration eventually rejected a test signal injected into the data to simulate a gravity wave.

25. **angry Jonny**
January 20, 2016

"Jacques Distler has come out of retirement at Musings to object that this work violates two central ideological tenets: one should not pay attention to gauge invariance, and the answer to all questions should be string theory or AdS/CFT."

You're kidding, right? Just in case there are actually some smart young people reading this blog who can't tell you're kidding, you should clarify that you were kidding, that Distler acknowledged that gauge degrees of freedom become dynamical at boundaries, only it wasn't clear why this is appropriate for horizons of decaying, non-eternal black holes. I mean, I get it- "don't read that, Distler is a knee jerk ideologue" is your code for "read Distler's blog, there's a discussion thread of 25 or so posts by people who've actually published in this topic." But not everyone's hip to your satire, the way you criticize the internet's rancorous politicization of physics by pretending to be so focused on the politics you've lost track of the physics- and I wouldn't want the kids to miss out on a cool discussion.

26. **Peter Woit**
January 20, 2016

angry Jonny,

When I wrote that, the substantive discussion thread over there hadn't really started, I'm glad to see it did. I was being somewhat humorous, but certainly didn't say "don't read that". If I want people to not read something I ignore it, don't advertise and discuss it here.

As far as the youngsters being misled, I think the misleading thing would be to point them in Distler's direction without warning them he's a knee jerk ideologue and what the ideology is.

Back at Work

January 21, 2016
Categories: Uncategorized

It's been a while since the last posting here, mostly because I've been away on vacation, but also because I haven't seen anything that newsworthy. But, since I'm back in the office and there have been complaints, here are a few items:

- For the first time in a very large number of years, a new volume has appeared in the series of Bourbaki treatises, dealing with algebraic topology (table of contents here). From the table of contents, it appears to be a rather modern treatment mostly of the fundamental group, but still in the Bourbaki style of exhaustive coverage and abstract point of view (I don't see any mention there of actually computing the fundamental group of anything…).
- While in Paris I attended some of the Seminaire Bourbaki talks. You too can watch via Youtube, or read the written versions.
- Far from mathematics and physics, one thing I did in Paris was stop by a store selling Breton products, and had a discussion with the owner about Kouign Amanns. He had a short hand-written list of a few places they could be had in the US. When I got back here, the next morning I went out to my local bakery (Silver Moon, at Broadway and 105th), and found that while I was away they had started selling them.
- On the Mochizuki front, there's a new paper by Vesselin Dimitrov, claiming that if Mochizuki's argument is correct, it implies something even stronger than Mochizuki claims, an effective version of the abc conjecture. The next workshop about this will be in Kyoto in July. One mathematician who has gotten interested in this and is listed as planning to attend is Edward Frenkel.
- If you can't get enough of the "Is HEP physics dead or what?" debate, see John Horgan on How Physics Lost its Fizz.
- Among the things going on here at Columbia this semester, there are Eilenberg Lectures on geometric representation theory (starting in a few minutes…) by Roman Bezrukavnikov, a course by Michael Harris on Lafforgue's recent work on the Langlands correspondence for function fields (also the topic of one of the Seminaire Bourbaki talks), and a conference celebrating Dusa McDuff's 70th birthday.

Better leave now to get a seat at the talk…

Comments

1. **Bryan**
   January 21, 2016

   Kouign-amanns are fairly common now at San Francisco bakeries and coffee shops, and yes they are delicious.

2. **David Roberts**
   January 21, 2016

   Ronnie Brown would be happy that the van Kampen theorem is proved for fundamental groupoids and not just for groups, and has a thorough treatment of groupoids generally.

3. **Ramsey Glissadevil**
   January 22, 2016

   Bryan…Multiple layers of sweet resplendency.

4. **Chris Kennedy**
   January 22, 2016

   I see that Trader Joe's sells frozen pack of four that you have to proof before baking. Has anyone tried those?

5. **John**
   January 22, 2016

   What is your take on the big rumor that GW's were found at the advanced LIGO run?

6. **Peter Woit**
   January 22, 2016

   John,
   I know nothing about that rumor. That advanced LIGO sees something has always been the expectation, so a detection would in some sense not be surprising (although a great breakthrough in opening up a new kind of astronomy).

   As for whether putting out the rumor on Twitter was a good idea, my own prejudice is that one should only put out rumors that one has a very good reason to believe, at a point where there's no good argument for keeping information secret. I think the history of rumors about LHC results so far has been that they've been pretty accurate, something that those in other fields should try and emulate…

7. **Yatima**
   January 22, 2016

   "Kouign-amann is a speciality of the town Douarnenez in Finistère, Brittany, where it originated in around 1860. The strict recipe of Douarnenez requires a ratio of 40 percent dough, 30 percent butter and 30 percent sugar."

   Solid fare for the morning, indeed.

8. **Bernd**
   January 25, 2016

   John,

   it's known that LIGO people sometimes deliberately put in fake signals and only reveal it when the publication announcing the discovery is about to be submitted. It's called "blind injection", and they do this as a test of the detection procedures: http://www.ligo.org/news/blind-injection.php

9. **Anonyrat**
January 31, 2016

Slide 16 of Carlo Rovelli's talk is worth using as a banner on this website:
https://videoonline.edu.lmu.de/en/node/7477

Hitler doesn't get a postdoc in High Energy Theory

January 23, 2016
Categories: Uncategorized

I recognize that this is a genre that is a bit tired, and arguably in poor taste, but the commentary on the HEP theory postdoc job market in the video [Hitler doesn't get a postdoc in High Energy Theory] is insightful. As far as I can tell the HEP Theory postdoc/junior faculty market has been the same for the last 45 years or so: far more people than jobs, and if you want one you better be working on one of a small number of "hot" topics. One might speculate that this correlates with the lack of progress in the field during this time. I'm a bit better informed about the mathematics job market for fresh PhDs, which is much healthier, as is the intellectual state of the field.

A recent trend does seem to be fewer jobs in the US, more in Europe. Anyone with better information about what is going on is encouraged to comment here (and, condolences if this is because you're on the market).

Comments

1. **Jeffrey M**
   January 23, 2016

   Peter,

   You are certainly correct that the job market in math is much better than in HEP, but just so no one gets the wrong idea, the job market in math sucks. It's sucked for 24 years. I managed to get my Ph.D. the first year it collapsed, and while I got a job, and so did most people I know, they weren't what we were expecting, and it got worse. Got better for a bit with some new NSF postdocs, but now it's much worse again. It's no picnic, even with a good advisor and a good thesis. My thesis was published in Mathematische Annalen, and I was lucky to get a job with a 4 course teaching load.

2. **RhoPhi**
   January 24, 2016

   Does anybody have data on post-doc jobs offered, faculty jobs offered and drop-outs every year? can think of a couple of ways to measure these from rumor sites and arXiv.org but I was wondering if data exists.

3. **Manfred Requardt**
   January 24, 2016

   I have just seen the video Hitler doesn`t get a postdoc in HEP and I think, it is a brilliant piece of modern art.

4. **DR**
   January 24, 2016

   > My thesis was published in Mathematische Annalen, and I was lucky to get a job with a 4 course teaching load.

   Damn.

5. **John**
   January 24, 2016

   One can argue that the tight job market in the USA should be no problem for people, because they can just move to Europe, right? Or China for that matter. Well, the problem for people to move to another place is often a very personal one.

   For example, my wife and I live in Europe. I have a postdoc position, my wife has a good (non-academic) job and we have a young child. This means my academic career will soon end because I cannot just move with my family to pursue some other postdoc position somewhere. My wife would have to quit her job. She already did it once for me, but this cannot continue.

   Because scientists are supposed to do various postdoc positions at various places before entering a tenure-track position (with not having job security even then), my question is:
   Are there scientists out there who have a partner with some professional ambitions? If yes, how do you manage?

   I think it is very old-fashioned for one partner to work and the other to stay at home, but this seems to be the effect of the postdoc-system.

6. **Ramsey Glissadevil**
   January 24, 2016

   Enjoyed the Hitler video. Yet, I'm sadden so many bright and dedicated physicists leave the their fiancé for finance.

7. **Chris Oakley**
   January 24, 2016

   God knows how many sets of fake subtitles I have seen for that clip from *Der Untergang* … the original one was about Hitler being banned from Xbox Live, but there has also been his problems with installing printer drivers, problems with booking a skiing holiday and many others. What distinguishes this one, though, is the lack of spelling errors. So maybe that it the thing, out-of-work or soon-to-be-out-of-work particle theorists: if you cannot get a job compactifying fluxes in 11 dimensions maybe you should consider teaching English.

8. **anonymous**
   January 24, 2016

   @ John:

   That's a tough situation and I sympathize. My wife and I are both academics, so our situation is a little different (worse in some ways, better in others). We married right around the time we graduated and spent the next five years apart jumping through all the academic hoops. We eventually found dual positions, but it was challenging and we both sacrificed better positions to be together.

Someone remarked to me that it's easier to find a spouse than an academic job. Given the competitiveness of the job market, I think this is unfortunately true. Are aspiring academics supposed to remain single and unattached until they get a TT position? Or find a partner who is willing to relocate every couple years? Ugh. It's a nightmare.

The majority of academics I know have a spouse who fits the mold of "stay-at-home". The exceptions are older couples who were on the job market 10-15 years ago, or those with a spouse who travels a lot for work or can work from home.

I wish you well in your situation.

9. **Ignatz Ratzkywatzky**
January 24, 2016

I recall an apocryphal story my supervisor told me.

After a lot of applications, talks, and networking, a clever HEP theory postdoc at a national lab was offered an assistant prof position at a major university.

However, his wife had a well established career with a high paying job in the nearby large city. Her job was not very transportable.

After quite a bit of soul searching, the postdoc told his colleagues that he was going to turn down the offer.

They reacted,

"Are you nuts?
You may never get another such wonderful offer.
You can always get another wife."

10. **Tim May**
January 24, 2016

All, it's hard to post an optimistic personal account here that doesn't appear to be boastful. But I think sometimes the negative stories need to at least get some counter-response.

In 1970 I was accepted at MIT, Stanford, and Berkeley for college. I transferred my acceptance and Regents Scholarship from Berkeley to UC Santa Barbara. A lesser school compared to Berkeley, on overall grounds, but a more interesting fit to my interests. (College of Creative Studies, with many advantages.)

By around 1972 it was clear the Big Drought was unfolding. Tales of Ph.D.s driving taxi cabs, professors advising that the odds of the then-current Ph.D. candidates getting a real position were dwindling. (Besides the overall downsizing of HEP and other physics funding, there was a glut of physics professors who had been hired in the post-Sputnik boom era….and they were still 30 years or more from retirement.)

Fast forwarding, I decided to not apply to grad school and instead join a small semiconductor company. There, I worked on a bunch of "engineering physics" probems. Because we were the leaders in dynamic RAM memory, I had exposure to some intersting problems. One of them was the mysterious issue of bits sometimes being flipped, but not permanently. In fact, the bit flips were apparently random and occurred only once (or at least close to only once…).

My physics background served me well, as I knew about the physics of how the devices worked (more so than a lot of the EE folks, who thought in terms of circuits), and I knew some geology. I had a brain storm that maybe low levels of uranium or thorium or the like in our ceramic and glass packages were causing the problem. Some experiments confirmed this. And all of the physics calculations about charged particle tracks in silicon matched. A lot of stuff I don't have the space here to describe.

So my career was launched. Lots of papers on this "soft error" phenomenon. (Oh, and the cosmic ray corrollary was indeed obvious: but in 1978 when the first paper was presented, it was insignificant as a source as compared to alpha particles.)

Instead of spending until 1980-82 doing a Ph.D. and then 4-8 years or more as a post-doc, I had some fun and retired from Intel in 1986.

I've been pleasantly able to pursue whatever interested me ever since.

So, there are also some alternatives to going to a hedge fund (not an option in the 1970s, early 80s, of course). And some of these may even let you do some fun physics and make some good money.

I'm not saying the options are the same today. But it shows that the pure academic track is not the sinecure it was in the WW II to around 1920 period. The poor prospects for good employment are sort of a reversion to the earlier situation, I think.

I have no guesses on what some current options might be. But I'm pretty sure most HEP grad students and post-docs will be facing some tough choices.

–Tim May

11. **Richard Séguin**
January 25, 2016

Tim:

I remember that bleak 1972 period. Where I was at the time, mathematics grad students were urged to take some computer science or statistics courses as fall backs. It was very discouraging, especially if you were not particularly interested in those topics.

12. **Ross McKenzie**
January 25, 2016

RhoPhi asked about data on "attrition". I don't know it for math or physics. However, there is hard data for biology, summarised in a nice graphic.

http://www.ascb.org/where-will-a-biology-phd-take-you/

It is pretty depressing. In particular, less than 8 per cent of entering Ph.D students become tenure track faculty.

13. **Anonymous**
January 25, 2016

I don't know if postdoc employment situation is really better in Europe, but I can say that getting a permanent university position is definitely just as difficult here as it is in the US. Maybe you can get a postdoc position or two after your PhD, but that is just postponing the inevitable; in the end you have to think of something else because there are just not enough permanent positions in the academia for everyone. If you want to save yourself a couple of years of moving from country to country and a lot of stress, you might as well quit academia after PhD (ever even after masters).

I don't believe that this system even chooses the best people for the professorship positions. It merely chooses people who have the mental fortitude to withstand all this bullshit for a decade. Most people (no matter whether they are good physicists or not) see the bleakness of their future prospects already during their PhD or first postdoc and leave.

14. **Tim May**
January 25, 2016

Yes. Richard Séguin, 1972 (plus or minus a year or two) was a terrible awakening.

An awakening about how many graduating physicist would find no jobs.

I don't recall panicking, just realizing that any thought of being a university physicist was probably a fantasy.

By around 1973 I think I was ready to move into industry. For various reasons, my small group hosted Feynman. I was the designated grillmaster, so I ended up cooking a steak for Feynman. As he sat on a couch at our place, chewing on his probably-overcooked steak, we grilled him (ah hem) about the future of physics.

He opined then, in the spring of 1973, that he thought if he were then starting out, he'd go into computer science. (He did a lot of fundamental work in CS not long after.)

He influenced me a lot. About a year later I joined Intel. For about a year I thought I was not figuring out the fundamental laws of the universe, but then I came to like what I was doing and realized that only about a couple of people per 30 years really do much about figuring out the real basics.

I appreciate guys like Nima, whether he is right or wrong, but most of us will not be Nimas.

(Not a big fan of string theory, but I like Nima's talks.)

I sometimes think Physics–the industry–has gone too far in emphasizing the Hero, the Conquerer, the World-Changer.

–TM

15. **Bernd**
January 25, 2016

I don't work in HEP, I work on more condensed-mattery stuff, but I sometimes get applications from HEP people. My impression is that the postdoc situation is a bit easier in Europe because all the hotshot postdocs go to the US, so the competition in Europe is less harsh. For real jobs, it's a different matter because 1) there are way less permanent positions than postdocs and 2) many of the people who went to the US for a postdoc are trying to come back.

16. **John**
January 25, 2016

@Ignatz Ratzkywatzky
Your anecdote nicely illustrates how sick the community is.

Let us not forget what it means to be in science: you have a parttime teaching job with most of the rest of the time supervising students, writing grant proposals, dealing with institute evaluations, meetings etc. There is no such thing as independent research because you need have impact, so you are stuck with the hot topics. You are crazy to choose this over your family. The only reason some people still do it seems to be status. A lot of tenured staff are in it for the status, and do not really care about science.

But, if you want to do real independent research and do not care about status, then take, say, a parttime job as a webdesigner and spend your spare time on research.

@anonymous (January 24, 2016 at 4:14 pm)
Yes, it's a nightmare. And especially so for women. For a woman, if you wait until you have a tenured position before choosing a partner, then you lost maybe all of your childbearing years. Women tend to be attracted to someone she can look up to. So, choosing a jobless partner who stays at home all day may not be attractive. Governments ask why there are so few women in science. Well this is your answer, right there.

17. **chris**
January 25, 2016

At least in Germany we are flooded with postdoc money. It is difficult to get a decent postdoc because there are so many open positions. And it is especially difficult as you have to tell them that after 6 years there is close to 0 chance they will land a permanent job.

And yeah, basically all my tenured colleagues have spouses that gave up their career or are single.

18. **Confused**
January 25, 2016

Dear All,

I liked the video, but I am left confused. Could you please explain to me why in the video Hitler is angry for not having any academic job offer but He's saying "… you want me to spend another year teaching undergrads…" Is that not an academic job? Or am I not understanding the american academic system?

Thank you.

19. **Richard Séguin**
January 25, 2016

Tim:

"But, if you want to do real independent research and do not care about status, then take, say, a parttime job as a webdesigner and spend your spare time on research."

You realize, though, that dropping out of official research environments (academia, research institutes, and the less common structures in private industry) makes actively participating in the research community more difficult. It's not impossible, but you have far less day to day contact with others of like mind, it's generally harder to network, and unless you've previously established a reputation, you have the sniff of a crank unless proved otherwise.

20. **AcademicLurker**
January 25, 2016

Confused:

My understanding is that most postdocs don't teach undergraduate classes. Perhaps they made a special exception for Hitler.

There's such a thing as a "teaching postdoc" that combines research with undergraduate teaching, and I think is designed for people who plan to apply for jobs primarily at small liberal arts colleges.

21. **Peter Woit**
January 25, 2016

Confused,
You need to understand that much of the US system is now a two-tiered one, with poorly paid adjuncts doing much of the undergraduate teaching, full-time tenured faculty doing relatively little undergrad teaching, more grad teaching, research, supervision of graduate students. Postdocs generally don't teach (although in some cases may pick up extra funding by adjunct teaching). I took the reference you quote as being to the prospect of having to try and support oneself as an adjunct if no further postdoc position was to come through.

22. **Tim May**
January 25, 2016

Richard Séguin,

Just to be clear, I didn't write the paragraph about being a part-time Web designer and doing research on the side.

I've already said enough in this thread. But I agree with you that doing physics on the side, or as in independent, is tough. Math may be easier to do this way.

23. **Michael Hutchings**
January 25, 2016

With a couple of spectacular exceptions (Perelman, Zhang,…), being isolated from the community is generally fatal for serious mathematics research.

24. **Jeffrey M**
January 25, 2016

@Michael Hutchings,

Well, Perelman had positions at Courant, Stony Brook, and Berkeley, and then at Steklov. And Zhang, who was out of math for a number of years after grad school, did get a job as a lecturer at UNH. Neither of them were really isolated, though Zhang certainly didn't have the usual teaching load for a high level researcher. I would agree isolation is generally fatal, I can't offhand think of any real result coming from outside the academic community in recent years.

25. **Michael Hutchings**
January 25, 2016

Jeffrey M:

Yes, but my understanding is that Perelman proved the Poincare conjecture while hiding away for a long time in the woods in Russia. And I suspect that Zhang didn't have much contact with the number theory community while doing his famous work. I think that even for professional mathematicians with solid training and credentials, going into isolation is usually very bad. In this regard it will be very interesting to see what happens with the abc conjecture…

26. **a job market survivor**
January 25, 2016

@Confused, @Peter, most graduate students in HEP theory are supported on TAships in the US (as NSF, DOE funding has been cut so much that even rich institutions can't support theory students on RAs anymore). Hitler is saying he's not willing to postpone his graduation for another year in the hopes that he'll have better luck on the postdoc market next year.

@John, while the whole issue of childbearing, and more broadly speaking, the likelihood of facing long-distance relationships during the grueling postdoctoral phase of an academic career is a big structural problem that badly, badly needs addressing, I can assure you that the issue of underrepresentation of women in the sciences starts with the systematic discouragement of girls from pursuing science and math in grade school, and good old-fashioned bias, both implicit and explicit, is still a very real factor.

27. **Peter Woit**
January 25, 2016

Thanks a job market survivor,
I should have realized that, somehow was thinking of Hitler as a current postdoc, not a student…

28. **Peter Woit**
January 26, 2016

All,
Please resist the temptation to use an interesting comment about the effect of the postdoc system on women's careers as an excuse to start exactly the same argument about why women don't go into science that one can see a million other places. Surely you don't want to read those arguments again, so why do you think it is a good idea to start them?

29. **Anonyrat**
January 26, 2016

How is a healthy, sustainable particle physics community of active researchers possible in these circumstances? When the winnowing out of the field is this brutal, the formation of a monoculture becomes much more likely.

30. **Confused**
January 27, 2016

@AcademicLurker, @Peter, @a job market survivor,

Thank you very much for your answers.
So as I understand you, it is more difficult to find postdoc positions.
But are teaching undergrads easily found in the US after one's PhD is completed (like here in north africa)? I mean, can people who have not done any postdoc in HEP find a permanent undergraduate teaching position, so that one supports oneself while doing independent research without funding?

Thank you very much again.

31. **AcademicLurker**
January 27, 2016

@Confused,

Low paying adjunct positions teaching undergraduates are reasonably easy to find post PhD. These have no job stability and are renewed each semester.

Permanent teaching positions are much more difficult to get. Even bloodthirsty fascist dictators have trouble securing them.

32. **Anonymous**
January 27, 2016

"I recognize that this is a genre that is a bit tired…" Noooooooo! I will never tire of Hitler Downfall parodies.

Now if someone would figure out how to combine it with Rickrolling…

33. **twistor**
January 28, 2016

@ RhoPhi:

Rough computation: assume the number of jobs is constant. Assume each professor has about ~10 phds / academic lifetime on average. Then 10% of phds will get a permanent position…

Certainly in Europe the # of jobs in t-HEP is a constant, perhaps slowly declining if any.

34. **Alex K**
January 28, 2016

Did anyone else notice that they replaced "Stalin" by "Nima"

I don't think he's *that* bad 😛

35. **Low Math, Meekly Interacting**
January 28, 2016

@Anonymous

This particular Unterganger left me conflicted. The final comment about the pants was so poignant…the resulting wave of sympathy for the Führer was a tad unsettling.

36. **gadfly**
January 28, 2016

"How is a healthy, sustainable particle physics community of active researchers possible in these circumstances? When the winnowing out of the field is this brutal, the formation of a monoculture becomes much more likely."

Well, HEP stuff is not currently insightful to the rest of science, and evolved out of a hyper-reductionistic paradigm that is dead outside of physics. Moreover, it's pretty obvious that without actual data to play with, you can't learn more about how the world works. So why manufacture pointless, grandiose sounding philosophical questions when there are so many things that we don't understand that we can research instead?

The hiring situation is getting worse in CS/quantitative biology disciplines (biophysics, bioinformatics) where I moved after my physics B.S., but it's nowhere near as bad as HEP or pure math. Engineering and C.S. PhD's I meet get TT positions without even doing a post doc.

When the fact that your field is antiquated and anachronistic has become obvious to everyone else, why not simply move on and work on other perfectly interesting problems for which there is plenty of good data and a will in the lay public to fund it?

–Just a gadfly

37. **Shantanu**
January 30, 2016

Peter and others. OT to this, but the videos of the LMU workshop on why trust a theory are online
http://www.whytrustatheory2015.philosophie.uni-muenchen.de/program/index.html

38. **Peter Woit**
January 30, 2016

All,
Please, the HEP theory job market is on-topic, whether HEP theory is dead isn't.

More From Polchinski

January 28, 2016
Categories: Multiverse Mania

Joe Polchinski has a rather odd preprint on hep-th, more of a blog posting than a paper, summing up his views on string theory and the multiverse. This is a revised version, wisely dropping a really unfortunate section. The material previously here explaining the background to what was in that section has been moved elsewhere, and I've renamed the posting.

I've never personally met Polchinski, and from those who know him I've heard that he's a nice guy. I've also recently heard that he's ill, wish him the best.

Update: It seems that I misunderstood why Polchinski removed the section about me from his arXiv article. He's now claiming that it was just because, since he'd gotten trackbacks to my blog banned, it would be unfair that there would be no trackback to his article (true enough…). This whole situation is a level of bizarre beyond the heights reached way back when during the string wars.

For those reading the version of Polchinski's article on his website rather than the current one on the arXiv, to understand what this is about, please read the material here.

No Comments

Some News

January 31, 2016
Categories: Uncategorized

Not much time for blogging at the moment, with one reason that I'll be giving a talk at Rutgers on Wednesday, and need to get that prepared. A few quick items:

- As some commenters have mentioned here, talks from the recent Munich conference (discussed here) are now available. From the little time I've found to look at them, I think Rovelli's is the talk that makes the point about all of this most worth making, with Massimo Pigliucci good at explaining the wider implications.
  While interesting comments on the talks are encouraged, for reasons that I can't explain publicly, discussion here of the Polchinski contribution is not welcome.
- Besides watching Gordon Kane in Munich on string theory predictions, he also has a paper about this out now.
- Congratulations to Bert Kostant on the award of the 2016 Wigner Medal. Kostant has been one of the major figures over the years in developing many deep ideas about the intersection of mathematics and physics, as well as a leading figure in the algebraic approach to Lie algebras and their representations.
- A lot of mathematicians and physicists want you to use TurboTax.
- Steven Weinberg's sensible opposition to guns in UT Austin classrooms has gotten a lot of media attention (for instance here). Of the many obvious reasons why this is a bad idea, he correctly points out that it may well make it difficult for UT to recruit faculty.

Update: A commenter points out that more videos from the Munich conference are available here.

Update: John Horgan has a wonderful interview with the remarkable and ubiquitous Sabine Hossenfelder. Highly recommended.

Update: For news from the LHC, see last week's Chamonix LHC performance Workshop. From the summary, the goal is about 30 inverse fb of pp collisions this year.

Comments

1. **Vladimir Kalitvianski**
   January 31, 2016

   This place contains more videos from the conference: https://videoonline.edu.lmu.de/en/wintersemester-2015-2016/7475

2. **nicola**
   January 31, 2016

   I went through Kane's talk.

   It is not the first time he mentions Newton's law or the Schroedinger equation and then claims that their predictions are analogous (in methodology) to string theory (slides 9 and 10). He says that in both cases (Newton or Schroedinger) you have to propose the force or the Hamiltonian and then see if they work with the experiment. In ST one provides the compactification scheme and then see if it works too – and so they are analogous in methodology.

   I think Kane is missing a very simple fact that disqualifies his analogy. The point is that for both Newton and Schroedinger theories we know what are the elementary constituents interacting and we know what is their interaction (whether it is electromagnetic, gravitational or any other know interaction). In principle we could write corresponding potentials for all the interacting constituents and so there is no guess work about the Hamiltonian to which Kane is referring. In Newton or Schroedinger equations there is in principle no room for proposing the potentials unless one is considering some effective potential due to complexity of the system considered. However exact potential are solutions of concrete equations (e.g. Maxwell) which have been tested experimentally many times.

   On the other hand in ST one starts with guesses about the compactification schemes and ends up in theories that has never been verified. There is no analogy here, I'm surprised he is bringing this up.

   Other thing that surprises me in his talk is slide 17 where he claims that "Gravity, YM theories, gauge fields,…. – all are prediction of and evidence for ST". He is certainly confusing a prediction with the postdiction – you cannot predict something that has been already discovered, but I hear many STheorists making this error.

3. **Narad**
   January 31, 2016

   > A lot of mathematicians and physicists want you to use TurboTax.

   I'm afraid that they've failed to adequately explore the parameter space – the free version last year started concealing the information that it won't do self-employment tax (without buying the premium version) until late in the exercise.

4. **David Roberts**
   January 31, 2016

   I would have appreciated Kane putting references for all the claims in his paper. If it was Wikipedia, there would have been a good many [citation needed] tags. Also, are any of the 'incorrect predictions' he refers to due to himself?

5. **Frank**
   January 31, 2016

   Kane did mention Lee Smolin and Peter at the 6 minute mark in the presentation. Sounded like he has some major disagreements with them.

6. **Peter Woit**
   January 31, 2016

   I was really surprised the organizers chose Kane as a speaker. His point of view doesn't really have anything to do with the "post-empirical" topic of the conference. His idea seems to be to do conventional empirical science, making distinctive falsifiable predictions. The innovation is to just ignore it when they get falsified. He just puts out a new paper or new edition of a book, replacing the falsified ones with new ones, repeating as necessary.

7. **andrew**
   February 1, 2016

Looking forward to reading your perspective on your book 10 years on (if slides are available). Will there be e.g. a second edition with a new preface?

I guess the book has aged reasonably well; whilst a lot of things have changed in the last decade, a lot of things have stayed the same.

8. **Peter Woit**
February 1, 2016

andrew,
I think the argument of the book has aged very well, with evidence for it now stronger than when it was written (most dramatically, no SUSY at the LHC). Main developments since then have been the discovery of the Higgs (expected, but I was hopeful that it might turn out to behave in an unexpected way), multiverse-mania, and, personally, I've learned a lot more about mathematics, which would change how I'd write some of the book if I were to do it again.

I'm trying to finish my textbook the next few months, and then want to pursue a bunch of ideas that have been on hold while I work on the textbook. Spending time on popular writing isn't something I want to do much of, but perhaps I should write some kind of piece about the "string wars", ten years later.

Slides from the Rutgers talk will be available.

9. **Balazs Vagvolgyi**
February 1, 2016

https://videoonline.edu.lmu.de/en/node/7494

In her talk Sabine Hossenfelder has a brilliant interpretation of how the Multiverse theories are born.
It starts at 8:22 in the video.

She basically argues that in any physical reality with any set of physical laws where intelligent beings get to think about their physical laws, there will always be unobservable/unmeasurable factors that will prevent them from coming up with a complete set of axioms to represent the theory of everything.
Then eventually their physicists, after long struggle, will always come up with a theory that will group all the unobservables/unmeasurables into a framework that will just be a version of the Multiverse.

10. **phil fogle**
February 1, 2016

OK, this isn't physics, so delete if necessary; but I applaud Weinberg's stance at UT; the mere thought of a disgruntled student in a classroom is very disturbing for the lecturer; would he be breaking the law if he refused to teach after an anonymous threat, for instance?

This has to be on the outer fringes of state insanity.

11. **Kevin S. Van Horn**
February 1, 2016

I'm not a physicist myself, but it seems to me that the No Alternative Argument would be more appropriately named Proof By Lack of Imagination.

12. **TS**
February 2, 2016

The idea of grading people who might want to discuss their grades while carrying a weapon is fairly scary. It shouldn't take a Nobel laureate to see that.

On a more delightful / depressing note: Kane writes that Witten discovered M-Theory in 1995. How amazing! I was only aware of him conjecturing the existence of an M-Theory in the same year, but I totally missed that he also made the next step! And in such timely fashion!

13. **John**
February 2, 2016

"The idea of grading people who might want to discuss their grades while carrying a weapon is fairly scary. It shouldn't take a Nobel laureate to see that."

So how do you know they don't carry an illegal gun now? To get a CCW permit you must go through training, background checks and other obstacles. Seems to me these students are more vetted then others which you know nothing about.

But then most people are scared of what they don't understand.

14. **Peter Woit**
February 2, 2016

Sorry, but I'll cut off further debate about the advisability of having armed students in one's classroom. Nothing anyone says here is likely to affect most people's opinion that this is obviously crazy. I am curious to know if it will have an effect on UTs ability to hire faculty.

15. **Miki**
February 2, 2016

Peter,
I read Kane's paper, but I am not an expert in ST and cann't follow his arguments and claims.
Supeficially, the paper looks "scientific" and sound – what is your opinions about it?

16. **Peter Woit**
February 3, 2016

Miki,
Kane makes a complicated argument, involving a lot of different assumptions. As explained above, he has a long history of doing this, making "predictions" which are falsified time and time again. I don't see any point to spending time understanding the details of the latest ones.

17. **SteveB**
February 3, 2016

Thanks for the LHC news links.

Can anyone explain the long technical stops that coincide with year's end?

Why would the world's most expensive machine be turned off and then have to go through 4 weeks of recommissioning every year? I realize some maintenance is required but it seems such a long time to be idle.
start silly comment
I am hoping that the answer is not that the people working there want long vacations, but if that is the case I wonder if the many people on this site that complain about finding HEP jobs, might be willing to replace those people at 80% of their salary and keep the machine up.
end silly comment

18. **zzz**
February 3, 2016

its really easy to accelerate particles from your keyboard, a lot harder from
an actual accelerator

19. **Nilay**
February 3, 2016

Off-topic, but perhaps of interest, is a New Yorker piece by Lawrence Krauss on the waste of money that is the Breakthrough prizes:

http://www.newyorker.com/tech/elements/do-the-new-big-money-science-prizes-work

20. **Anonyrat**
February 3, 2016

I hope that your Rutgers talk becomes available!

21. **30 inverse fb of pp collisions this year**
February 3, 2016

– is 30 inverse fb of pp collisions this year enough data to
1- decide on 750GEV diphoton excess
2-determine if natural SUSY exists or rule it out
3-incease bounds on gluino masses
4-determine if LSP neutralino dark matter exists ?

22. **Fred P**
February 3, 2016

@SteveB-

Here's a rundown of what they did for commissioning last year: http://lhc-commissioning.web.cern.ch/lhc-commissioning/2015/2015-comm-plan-schedule.htm

This doesn't look like an excuse for a vacation to me.

23. **downtime**
February 3, 2016

Part of the answer is electric utility rates, especially during peak holiday periods. A better answer is from Fred P. It's typically a mad scramble to get everything installed on schedule. The correct question to ask is "how do they get so much done with so little down time?" But the best answer is "ask someone at CERN (or SLAC, Fermilab, etc.) how do they manage to do as much as they do within the limited downtime available?"

24. **A.J.**
February 3, 2016

SteveB:

It's the world's most complicated machine. Four weeks a year of maintenance would be a miracle. However, the period you're talking about, the 'recommissioning period', isn't so much maintenance as 'the process of turning the LHC on'. The LHC doesn't run during the winter season because fuel costs are higher then and the budget is limited. During this time, the physicists and engineers do maintenance and upgrades. Then in the spring, they turn on and tune all the subsystems in their proper order so that they can have collisions in the summer and fall.

* I wonder if the many people on this site that complain about finding HEP jobs, might be willing to replace those people at 80% of their salary and keep the machine up.*

Those are theory jobs they were talking about. Putting theorists in charge of LHC maintenance would be a bit like giving a dentist a scalpel and asking him to perform brain surgery. Maybe you'll get a discount, but you probably won't get the desired effect.

25. **chris**
February 4, 2016

Steve B,

the LHC being the "world's most expensive machine" is off by at least an order of magnitude.

The total cost of the LHC is about $9 billion.

Just for reference: The total cost of the ISS is $150 billion. The cost of a single CVN-78 Class Aircraft Carrier is close to $10 billion. (And the R&D cost for the F35 plane was a staggering $320 billion).

Even looking at physics alone, the LHC is not the most expensive machine. The cost estimate for ITER is >$15 billion and the current projection for the Webb telescope is $8.8 billion.

26. **Peter Woit**
February 4, 2016

Nilay,
Thanks for pointing that out. I pretty much agree with Krauss, although I think the standard "instead support young people who are just entering the field" alternative he suggests is problematic. The problem I see (especially in theoretical physics) is not that good young people can't get postdocs, but that there are so few permanent jobs for them. It would be a great idea for the math/physics community to try and convince Milner and Zuckerberg to stop giving these large personal awards, and instead endow permanent positions in the name of the honoree.

30 inverse fb

1. yes
2. no (or, maybe, yes, since it is already ruled out).
3. yes
4. no (you can always push the mass up higher).

27. **Alex**
February 5, 2016

In case anyone is interested in some technical details, here is a fairly good short summary of the model Kane and his collaborators have been working on where you can read about some of the assumptions and how much connection to M-theory it actually has: https://ncatlab.org/nlab/show/G2-MSSM

28. **30 inverse fb of pp collisions this year**
February 8, 2016

2. no (or, maybe, yes, since it is already ruled out).

are you saying the result of first run @ 8 TEV is enough to rule out TEV-scale SUSY?

29. **Peter Woit**
February 8, 2016

30 inverse fb,
If susy is supposed to be "natural", it really is supposed to show up at the weak scale (100 GeV), and the current bounds on superpartners rule that out. You can claim if you want that there is just some fine-tuning, so the superpartners are at higher mass, even push them up above the point the LHC will see them in this run.

As for "TeV-scale susy", depends exactly what that means. There are different bounds on different superpartners, best bounds on the strongly interacting ones, which already have to be above a TeV, but if you think a few TeV is "TeV-scale", that's not yet ruled out.

30. **Anonyrat**
February 12, 2016

Clara Moskowitz, on page 14 of the February 2016 Scientific American, in an article on XENON1T tells us "WIMPs are a prediction of superstring theory". Little did I know!

31. **Peter Woit**
February 12, 2016

Anonyrat,

She just doesn't mention that no WIMPs are also a prediction of superstring theory…

Rutgers Talk

February 4, 2016
Categories: Uncategorized

Slides from my talk at Rutgers are now available here. The idea was just to advertise to physicists there the point of view that is all too familiar to regular readers here. The final speculative comments about relations to mathematics shouldn't be taken too seriously, these are things I hope to work on and write about much more in a few months once my current book project is completed.

Update: Interestingly, my Princeton advisor Curt Callan yesterday gave a talk at the KITP with a bit of a similar theme, starting off by arguing that the success of the standard model made future progress in HEP very difficult. His answer to the problem is quite different than mine (his involves trying to make contributions to biology). The first question at the end (from David Gross) is about the relation to new mathematics.

Comments

1. **Ian Agol**
   February 4, 2016

   If you use \documentclass[handout]{beamer}, it should shrink your pdf document from 125 to 32 pages (eliminating the strip-tease feature).

2. **Peter Woit**
   February 4, 2016

   Thanks Ian,
   That's a useful fact I didn't realize. Will change that in the morning.

3. **G.S.**
   February 4, 2016

   I like pacing through the talk bullet by bullet. It makes the material easier to digest.

4. **Bernd**
   February 5, 2016

   I'm not so sure that the current obstacles in experimental HEP mean an end of being able to conduct experiments at all. Rather, I have the impression that only the paradigm of the particle accelerator is coming to an end. Instead, we have to think about how to conduct new types of experiments that allow us to probe effect occurring far beyond the electroweak scale. For instance, some people are working on probing Planck-scale physics with optomechanical oscillators. This is actually not that crazy, at least far less crazy than expecting the Chinese to build yet another accelerator and hoping for a miracle to show up there.

5. **Peter Woit**
   February 5, 2016

   I've changed the pdf file to one with single-pages.
   G.S.,
   For you and anyone else who likes the pauses, that version is at
   http://www.math.columbia.edu/~woit/rutgers-withpauses.pdf

6. **Peter Woit**
   February 5, 2016

   Bernd,
   I wasn't intending to claim that there are no possibilities for non-accelerator based experimental progress (or, lower energy accelerator-based things like neutrino experiments). Such things should certainly be pursued, my point was just to provoke thought about what can be done in the absence of new data.

7. **lun**
   February 5, 2016

   Did you get any interesting questions?

8. **Peter Woit**
   February 5, 2016

   lun,
   Some good questions, mostly people who sensibly wanted to challenge my provocatively negative take on prospects for progress (for instance: what about cosmology? what about neutrino masses? what about previous fallow periods in the history of the subject?). No string theorists there who wanted to argue about that.

9. **LK2**
   February 5, 2016

   There are also possibilities for accelerator-based progress.
   Magnets technology is progressing fast. Superconducting cavities: same thing.
   A lot of progress in wakefield plasma accelerator with a lot of R&D at SLAC and CERN.
   Do not forget the possibility of ILC and a muon collider.
   Sure: very expensive projects but all this could push the end of collider physics decades in the future.
   Accelerators are needed anyway for all the precision physics measurements. Just to name a few: mu2e, g-2 , pi to 3e, B-factories…
   Anyway: Peter I liked your perspective in the talk.

10. **LK2**
    February 5, 2016

As for the neutrinos, there are powerful experimental efforts. The Fermilab's DUNE project will clear up a lot of things: hierarchy (non and inverted) and CP phase in the lepton sector. The Japanese are progressing towards Hyper-K with similar goals. This stuff will be also useful for cosmic neutrinos and proton decay.
I do not see a grey future for HEP right now. Maybe in >50 years.

11. **Jake**
February 5, 2016

Reading thru now, got to page 15 "There is one science that does not rely on empirical testing to make progress: mathematics." and aren't those fighting words at an institution that employs Doron Zeilberger?

12. **Gregor**
February 5, 2016

@Peter:

I also scrolled through the slides and found a very basic statement (not central to your talk) that is not so clear to me: On slide 24 you write "Observables correspond to self-adjoint linear operators on H". This depends a lot on your definition of observables. I think it does not cover the Berry phase which also gives you a measurable quantity of quantum mechanical systems. Is this not considered to be an observable?

13. **paddy**
February 5, 2016

Jake: (At the risk of being OT.) After refreshing myself on DZ's opinions, yes they are rather amusing in juxtaposition to PW's talk. Thank you.

14. **Peter Orland**
February 6, 2016

Gregor,

The Berry phase is a matrix element of an observable. The operator, not the matrix element is the observable. This is not in conflict with the idea that self-adjoint operators are observables.

15. **Yatima**
February 6, 2016

Doron Zeilberger

Never heard of him before. Damn! [I suppose he is using Coq now]

There is one science that does not rely on empirical testing to make progress: mathematics.

The judgment of whether something is "empirical" or "à priori" (I am at a loss to find the best contextually relevant antonym of "empirical") is really on a sliding scale. *"Do you judge this statement to be a theorem of this hopefully self-consistent system"* is a call performed by resource-constrained, imperfect judges that moreover have to rely on external memory like paper and state-tracking systems like blackboards. Such is life…

16. **Michael Gogins**
February 6, 2016

Many thanks for the slides of your talk. I'm not a scientist, but I found this a very helpful distillation of your blog and point of view that gives me more of a feeling that I am getting a glimpse of the issues. I also think I agree that going over the details of the mathematical physics with mathematical insight and rigor is likely to be very helpful, but what is the role of "physical intuition" (like Einstein's thought experiments e.g. about the equivalence principle) in this?

Hasn't mathematics itself gone through several episodes in just the past century of redefining what exactly is a proof (Russell's Paradox/incompleteness, computer proofs, categorification), and what kind of mathematical tools and objects can or should be used in proofs?

Even more, is there any danger that "mathematical proof" can become so difficult that progress slows or halts, by analogy with the increasing difficulty of high-energy physics experiments? Is there a "human intelligence limit" here somewhere? How much of the increasing difficulty, assuming it is real, is due to mere complexity, and how much is due to increasing abstraction/sophistication? Can computer-assisted proof help with this?

I'm a mathematical Platonist like Godel, but I'm not sure I'm a radical Platonist in your sense. How does a radical Platonist learn which mathematical entities give the most insight into physical reality without experimental decision?

17. **Peter Woit**
February 6, 2016

Gregor,
Peter Orland is right that this is still an operator on states. This kind of operator is very interesting though, not of the same sort as local fields, which have an interpretation in terms of a representation of a Heisenberg Lie algebra (in physics terms, canonical commutation relations). Things like Wilson loop and 't Hooft loop operators indicate that fields may naturally be quantized in a quite different way than matter fields.

18. **Peter Woit**
February 6, 2016

Michael Gogins,
I don't think the basic idea of what a proof is has changed in quite a long time. It's fundamentally just the idea that you need to make your assumptions and claimed logical implications completely explicit.

The remarkable thing about mathematics is that it continues to progress, despite the use of ever more difficult to absorb formalisms. I find this really striking and well worth thinking about it. Naively one would expect progress to have ground to a halt, as the whole thing became too difficult for humans to master, but this hasn't happened.

I don't think you can understand the deep connections between physics and math by just looking at one of the two subjects. As for whether you can find new relations between math and physics of the sort I expect, the point is that you already know a lot about the physics side (the SM), and the argument is that you should focus on that. All experiment has been telling us for many years is "pay better attention to the SM". Would be great if new experiments told us something more than that, but this may not happen.

19. **Douglas Natelson**
February 7, 2016

Very nice. I am going to have to try to go through your book at some point, though I worry that I'm not sufficiently math-literate, in the sense that I don't really know how enough of the vocabulary to think intelligently about phrases like "the tangent space at the identity of the group". I know separately what a tangent space, an identity, and a group are, but mathematicians have a way of speaking that I haven't learned. If you have any advice on a "how to read about modern math for non-mathematicians", I'd greatly appreciate it.

20. **Peter Woit**
February 7, 2016

The book I'm writing does try to convey exactly what you're asking about, at least the first half of it, which tries to explain basic examples of Lie algebras, groups, representations in a concrete way, while also taking somewhat of a mathematician's point of view and language. The second half of the book things aren't so simple, I'm trying to work out basic examples of QFT using some different mathematics than usual. This I suspect both mathematicians and physicists will have to struggle a bit to follow.

21. **David**
February 7, 2016

I'm working through your book notes (haven't gotten to QFT parts, yet) and am enjoying it, but I must say it's a struggle. Biologist here, with some physics background. If a reader hasn't prepared with a couple books on lie algebras, group theory, and the basic math of QM (linear algebra, hilbert space, operators) before trying your book, I'd think he/she would get lost quickly.

22. **Low Math, Meekly Interacting**
February 7, 2016

Is calling mathematics a "non-empirical science" also intentionally provocative? There are quite a few learned people who would consider maths a separate field and feel mathematicians have a different métier from that of scientists. Furthermore, they would say maths and the natural sciences are all the better for this differentiation, while acknowledging the former's "unreasonable effectiveness". I'm on the fence about this distinction myself, and just wonder why you favor looking at it as a "science" vs….well, something else.

23. **Peter Woit**
February 7, 2016

LMMI,
Yes, that's intentionally a bit provocative, I'm well aware that many if not most people have a different view of mathematics. But from the "radical Platonist" point of view, mathematics is not different than other sciences, it is the science of the study of certain kinds of objects and their relations, objects with a deep connection to the physical world. The only thing different is the role of empirical experiment (arguably there are also "experiments" in math, e.g. numerical calculations checking examples of a number theory conjecture, but these are "non-empirical" in the sense of not measuring something about the physical world).

Vaguely relevant is this comic, putting the sciences on a scale together…
https://xkcd.com/435/

24. **vmarko**
February 7, 2016

Peter,

You *are* aware of the popup text (when you hover the mouse over the comic) in that particular xkcd, right? 🙂

Best, 🙂
Marko

25. **Peter Woit**
February 7, 2016

vmarko,

Yes. Gell-Mann liked to say that. Until a mathematician explained to him about SU(3)…

26. **Eli Rabett**
February 7, 2016

Your take on mathematics continuing to progress w/o empirical data, of course raises the question of what is progress. Is it finding additional answers to ad hoc puzzles, or finding new understanding through the solution of puzzles?

27. **Peter Woit**
February 8, 2016

Eli Rabett,

Mathematics progresses by discovering new interesting mathematical objects, and better understanding the ones we know about. Solving specific problems (like the Poincare conjecture or Fermat's last theorem) are in some sense just evidence that one has better understood the relevant objects.

28. **Will**
February 8, 2016

I know you're not a fan of Tegmark's multiverse enthusiasm, but your "radical Platonism" sounds something like Tegmark's Level 4 multiverse (the mathematical multiverse). Am I misunderstanding one of you?

29. **Jeffrey M**

February 8, 2016

As a mathematician I would make the argument that math is NOT an empirical science. Even if one is a Platonist, since the Platonic objects are not accessible via anything but math. Now that I'm thinking about it perhaps mathematics is really the multiverse, if it is Platonic. In the Platonic universe of mathematical objects there would have to be contradictory ones, which were internally self consistent. Say, the universe where the continuum hypothesis is true and the one where it isn't. Two different universes, with different rules. Not a problem for us mathematicians, you just have to decide which one you want to live in. Kind of a big problem for physicists, who are supposed to describe the universe we do actually live in 🙂

30. **Peter Woit**
February 8, 2016

Will,
In one sense I'm on-board with Tegmark, identifying the physical world with a mathematical structure. In another sense, our visions are opposite. He sees a multiverse of different physical laws corresponding to the infinity of all possible mathematical constructs (which to me is an empty idea). I see ultimately one specific set of physical laws unifying physics (something not too far from the SM + GR) corresponding to the fact that mathematics is a highly structured subject, with a unifying fundamental set of ideas (which we don't yet completely understand).

31. **Curious Wavefunction**
February 8, 2016

Excellent set of slides. I think your lessons from math on slide 17 could well be applied to other disciplines where models are gradually claiming to replace hard experiments and simulation is becoming an impressive if misleading source of hype and funding.

32. **random reader**
February 9, 2016

[Peter Woit wrote:] "mathematics … continues to progress, despite the use of ever more difficult to absorb formalisms. … Naively one would expect progress to have ground to a halt, as the whole thing became too difficult for humans to master, but this hasn't happened."

I don't think the naive expectations are necessarily so different from what is happening. The training time to get to the frontier and make meaningful contributions is, as expected, getting longer and longer. More and more giant papers appear that very few people understand, and take years for the community to digest.

Total progress has accelerated as the number of PhD's has exploded, but progress per mathematician may be slowing down as higher complexity and diminishing returns set in. This is masked somewhat by the availability of ever more powerful technology and the new growth areas that creates within mathematics. The increases in productivity from LaTeX and email and Skype and github (or similar tools) are one-time jumps so that one expects a slowdown to set in at some point, unless funding is pumped in to increase the number of trained specialists. If there is a salvation through AI coming online that would also be a kind of validation of the Horgan thesis, and would probably only be temporary.

Voevodsky's program of proof formalization is based exactly on the "incomprehensible complexity" thesis and he gives some examples from his own research experience.

33. **random reader**
February 9, 2016

p.s. Peter, I had meant to write a long reply to Brian Conrad at the Mochizuki thread, but did not get there before you closed the comments? Is there any possibility of reopening it? The thread on ABC at Cathy O'Neil's blog is also closed, so no ability to continue there. I guess I can start a blog if necessary but was hoping to answer under the blogpost where Conrad's comments appeared.

34. **Peter Woit**
February 9, 2016

random reader,

It's true that the trend is for mathematics research to get more specialized, with new developments understood by smaller fractions of the community (the number of people who understand the proof of FLT is pretty small). And I'm somewhat mystified how students manage to get to the frontiers of the subject and make a contributions in a finite amount of time (then again, as I get older, the ability of some of the young to quickly absorb difficult ideas never ceases to amaze). But it's undeniable that significant progress keeps happening, of a sort that hep-th isn't seeing. We'll see if this continues, I'm not optimistic that more Ph.D. students or proof formalization are that helpful.

I've changed the parameter that sets the date for closing old comments, so you can still comment on that old posting. One problem with this is that typically no one looks at old comment threads, so an ongoing discussion there is not very likely. I would encourage you in the idea of setting up your own blog and thus having a place you can easily write more extensively. I promise to link to it!

35. **random reader**
February 9, 2016

Mathematical progress includes the development of ever-improving language for efficiently organizing the mathematics, so that conceptual complexity is reduced in some ways at the same time as overall difficulty and amount of knowledge increases. Scheme theory, derived geometry, homological/homotopical algebra, and perfectoid spaces all are complicated (and interrelated) formalisms with heavy upfront investment for learners, but they systematize and simplify what was done before, allowing the same human cognitive capacity to achieve more in finite time.

Other than that, however, the different impression of progress rates between math and physics (or math and any other field with well understood benchmarks of progress, such as nuclear fusion, AI, space travel, lifespan extension etc) is largely because of a cultural difference in standards of progress. In math one can always formulate and explore new problems that look solvable and define solutions of those as progress. Every once in a while an old and difficult benchmark problem falls to new tools but that's a rarity. Theoretical physics has this to a degree, but a much lesser one because there is a much smaller set of problems considered fundamental. "Have you unified GR and QM yet" (or predicted new particles, or understood dark matter, etc) is a very hard standard to meet.

In math there is also a tendency to accept weak notions of "solve", such as qualitative or nonconstructive existence proofs, or proofs of concept that don't necessarily blossom into a working tool after finding some in-principle solution to a problem. In
physics, particularly experimental physics, it is hard to accept something that claims to be predictive of some particle mass but does not actually lead to a precise number that can be checked against experiment. In math the equivalent is often considered a full solution.

36. **Radioactive**
February 9, 2016

You will be aware I'm sure that Lubos has gone through your slides point by point. Less the hysterics, some of what he says is valid. Have a look at any random day of hep-th and ignore the popular press. The field has moved on quite a bit from the pair of pants diagram and the Randall-Sundrum model.

You have also no doubt heard rumours of gravitational waves detected by LIGO. On the verge of this massive experimental discovery you're announcing that empirical confirmation of new physics models is impossible and we should instead turn to noodling around with group theory and see what happens with the massive web of conjectures comprising the geometric Langlands program. Certainly these things are interesting mathematics but does this honestly seem like a promising direction for physicists? What should physicists do about it? Most are far less mathematically inclined than you and care about predicting the results of real experiments – check any day on hep-ph. What kind of career do you propose for the workaday phenomenologist?

37. **Peter Woit**
February 9, 2016

Radioactive,
I think it should be obvious there's little point to me responding to Lubos's ranting, his agenda is just to misinterpret and misrepresent what I have to say. Obviously the few words that fit on a slide can't convey the actual arguments I'm trying to make (much fuller versions are available of course in many places, for instance my take on the hype problem is http://www.math.columbia.edu/~woit/wordpress/?cat=8, not just the two particular examples in the talk). As I pointed out at the beginning, the intent of the talk was to point to where the actual arguments were, and to be provocative, not make a bunch of carefully worded, fully hedged and defended claims.

There's no argument in my talk that experiment is dead. Obviously whatever experimental avenues are available should be explored, and I've no expertise to advise experimentalists about what they should be doing. The question I was raising was that of, if you're a theorist interested in certain questions, with no experimental help in sight, what can you do? The Munich conference I was referring to was based on the assumption that a significant part of theoretical activity is now "post-empirical". A perfectly defensible attitude towards this is that it's a mistake, that theorists should should stick to thinking about things with some reasonable connection to experiment. A lot of people read my book as making that argument, but that's not really what I think, and the talk was an attempt to explain why. I do think it is possible to make progress by trying to better understand the internal logic and mathematical structure of a theory, even without help from experiment telling one if one is on the right track. And if you try and do this, there's a lot you could learn from mathematicians about how to make progress.

38. **random reader**
February 9, 2016

"see what happens with the massive web of conjectures comprising the geometric Langlands program"

That is not quite accurate. Geometric Langlands is not like some neighboring subjects where there are vast pyramids of conjectures that go far beyond any proven theorems and with no hope for an imminent proof. There is a massive web of both conjectures and theorems, and the weight of the latter is considerable. Lots of the conjectural picture, although still developing, has been proved, and the time span between conjecture and proof has been far shorter for geometric Langlands than classical arithmetic Langlands (because the geometric theory is easier and more structured). The subject is in flux but in a very healthy way where more and more of the expected picture is falling into place, leading to new refinements of that picture, and further cycles of conjecture-proof-application.

39. **Radioactive**
February 10, 2016

Physics unlike mathematics has both logical and experimental tests to pass. Working on mathematical structure is what string theorists do and have done for years. You are advocating a more rigorous mode of working – axiom, conjecture, theorem, proof, consequences – instead of trying to leap over steps as physicists are wont to do. But it seems if people actually followed your approach then we would be more or less in the same place.

If everything was formulated nice and consistently what then? Streater and Wightman's axioms had very little effect on the important developments of the 20th century. Do you think that the mental labour involved in proving rigorous theorems about string theory would stop people from hyping it in the popular press?

40. **Peter Woit**
February 10, 2016

Radioactive,
I'm not advocating that physicists adopt mathematician's standards of rigorous proof, just that, especially when they can't make contact with experiment, they devote more attention to making clear what they understand and what they don't. Sure, leap over steps and see what happens (little known fact: mathematicians do this too), but be aware that you're doing it and make that clear to others. Would this not make any difference? Maybe. But my experience with string theory has been that the extreme difficulty in figuring out exactly where the subject is, what is understood and what isn't, has not only made the public hype problem worse, but makes it difficult to make progress (with the progress needed sometimes just achieving clarity on what doesn't work).

As for QFT, again, I'm not arguing for applying full rigor to what is well understood. An example of what I have in mind is the question of whether the electroweak theory (chiral gauge theory) has a well-defined formulation outside of perturbation theory. Can you even in principle consistently do non-perturbative calculations? This appears to be irrelevant to contact with experiment, and the general assumption is that this is an uninteresting technical issue, but maybe there's something to be learned there. For people to be motivated to work on it though, there has to be some perception that there is a problem there, and some idea of what would count as progress.

41. **Andre**
February 11, 2016

Peter,
Interesting you should mention chiral gauge theories – there is a recent article by D M Grabowska and D B Kaplan
http://arxiv.org/abs/1511.03649
which combines two nice ideas in lattice field theory in a proposal to non-perturbatively regulate chiral gauge theories.

42. **Nick M.**
February 11, 2016

random reader wrote:

> **"**In math there is also a tendency to accept weak notions of "solve", such as qualitative or nonconstructive existence proofs, or proofs of concept that don't necessarily blossom into a working tool after finding some in-principle solution to a problem.**"**

In physics we call this working on M-Theory. 😉

43. **the ghost of jon**
February 14, 2016

“Streater and Wightman’s axioms had very little effect on the important developments of the 20th century.”

Is this attitude fair? Bisognano and Wichmann took those axioms as hypothesis and published their theorem a year before Unruh published his result. Also, the irony of this comment appearing on this blog in particular is, Streater and Wightman published their book more or less motivated by their disappointment with a talk by Geoff Chew.

As to what concerns workaday physicists, sure, e.g. Lawrence’s collider experiment paradigm is about studying effective field theories at different scales of contractable minkowski space. Ultra cold physics cares about field theories on stratified manifolds and non-abelian braid statistics. Many important insights there have genealogies traceable to VFR Jones and to Mandelstam’s (Kadanoff-Ceva inspired?) string localized excitations studied via DHR theory of superselection sectors in the Haag-Kastler axioms (Frohlich credits D.Friedan’s talks as inspiration for his paper on non-abelian statistics- I should mention, BTW, it takes a lot of chutzpah to walk into Greg Moore’s physics department and tell them they need to do more math). Shoot, over and above Jones’ huge influence via Witten, there’s a paper Jones presented at a conference where he asked for something like the Wegner’s self-dual lattice models but with Hopf Algebras from finite non-abelian groups. So along came this guy named Kitaev, and…

It sometimes feels like we say the phrase “contact with experiment” as if there’s some static, God given collection of experiments, faithfully streaming new phenomena at physicists who would then be surely remiss if they didn’t spend all their time, like Beatrice in the Paradisio, staring at this God given stream of Data. I think we say it this way because it’s an honest reflection of a workaday phenomenologist’s experience- new kinds of experiments can sometimes take an entire career to bare fruit. You must publish or perish, and attempts at rigorizing interacting field theories is, notoriously, a graveyard of ideas. Sure, you can listen to the siren song of mathematical physics but only after your men have tied you to the mast. True, an individual who dives off his boat in pursuit of their song will surely perish. But, also, someone eventually realized those were wales and made an industry out of fueling lamps with the stuff. If there were high priests of lay-internet discussion of sirens back then, maybe no one ever would have.

44. **Daniel**
February 17, 2016

Hi, maybe not unrelated to the content of your talk, Smolin in a recent paper (http://arxiv.org/pdf/1512.07551) commented on the role of Mathematics in HEP, and what lesson should we draw from Einstein’s General Relativity.

Stacks Project Party

February 7, 2016
Categories: Uncategorized

Last night I got to attend a major event of the Manhattan social season, a party celebrating the fact that [the Stacks Project]() has [reached the milestone of 5000 pages](). As far as anyone knows, no one has ever printed out the whole thing, but to give an idea of scale, the party featured a large stack of reams of paper totaling about 5000 pages.

I was going to include a party report, describing the various celebrities there, their outfits and conversations, but one of them (Mathematics Without Apologies) has its own blog, so I'll just refer you there.

For some background about this amazing project, from when it was a mere 4000 pages, see here.

No Comments

Gravitational Wave Predictions

February 10, 2016
Categories: Uncategorized

I think I can confidently predict that tomorrow morning either one of two things will happen:

- The first observation of gravitational waves will be reported by the LIGO experiment.
- A large fraction of the scientific community will be really, really angry at members of the LIGO collaboration.

I'm betting on the first of these two alternatives, and like everyone else will be watching to see what happens tomorrow. If you want some informed commentary on what it all means though, this isn't the place (what I know about gravitational radiation is basically the little that I learned in a GR course about 40 years ago…), so for now I'll leave comments closed.

One place advertising a live feed is Nature. I'll be happy to list better possibilities here if people let me know about them.

Update: Another place to try for the webcast is here.

Update: Big event here at Columbia. Roone Arledge auditorium packed.

Update: Quite amazing, just as predicted, observation of two black holes coalescing, a historic discovery. That stuff I was taught 40 years ago really works. More details many places as the embargo is lifted, with some good examples Natalie Wolchover at Quanta, Dennis Overbye at the New York Times, and Davide Castelvecchi and Alexandra Witze at Nature. The paper has been refereed and is here at PRL.

Update: Better info about the waves is available elsewhere, but I can report here on something pretty amazing: my graduate school roommate's gravitational wave soup bowl.

No Comments

This Week’s Hype

February 13, 2016
Categories: Multiverse Mania, This Week's Hype

This week’s dramatic announcement of the discovery of gravitational waves was a major milestone for the fields of physics and astrophysics. The LIGO observation validates a lot of previously untested aspects of our understanding of general relativity, and promises the imminent opening up of a new field of observational astronomy, as LIGO sees other astrophysical sources of gravitational waves. Watching the announcement, the lead up to it, and the press stories that came out, many immediately as the embargo was lifted, I was struck by the general high quality of the stories in the press (I linked to a few of them in the last posting, but there are many more). Congratulations to whoever organized this, and to all the science writers who have done a great job producing enthusiastic but generally hype-free coverage of the story.

Unfortunately, those physicists brought in by major news organizations to tell the public what the significance of this is often can’t resist the temptation to indulge in the usual hype. At the Wall Street Journal today, Michio Kaku’s commentary is labeled Riding Gravity Waves to the Big Bang and Beyond, and subtitled “Once again, Einstein’s theory of relativity is confirmed by scientists. Next stop: Creation.”

There’s nothing in his piece about what else LIGO might observe and what we might learn from it about the universe. Instead, it’s all about the big bang, Creation, and before the big bang, things which as far as I can tell, LIGO data is highly unlikely to tell us anything about:

> Now we are witnessing the third great revolution in telescopes, the use of gravity waves to open a new chapter in astronomy. For the first time, waves from the very instant of creation might be observed, giving us “baby pictures” of the universe as it was born. High-school textbooks may have to be rewritten to incorporate the new discoveries coming from this third generation of telescopes.
>
> This may also have philosophical implications. Right now the big-bang theory doesn’t tell us what banged, why it banged, and what caused it to bang. It only tells us that there was a bang. But if space-based gravity-wave detectors similar to LIGO’s detectors can measure the radiation emitted an instant after the big bang, then, using mathematics, one can run the equations backward to determine what set off the big bang in the first place, in effect answering the biggest question of all: What banged and why?
>
> When Einstein postulated gravity waves a century ago, he not only opened up an entirely new chapter in astronomy, he also opened the door to answering the most important philosophical questions of all time, including the creation of the universe.

Over at the New York Times, in the Sunday Review, Lawrence Krauss has a more sensible piece, entitled Finding Beauty in the Darkness. Multiverse mania seems though to be irresistible, as he ends up with this summary of the physics significance:

> Ultimately, by exploring processes near the event horizon, or by observing gravitational waves from the early universe, we may learn more about the beginning of the universe itself, or even the possible existence of other universes.

Comments

1. **Sesh Nadathur**
   February 13, 2016

   To be fair, he does mention “this third generation of telescopes”, which might refer to gravitational wave detectors in general, including those such as (e)LISA and others, which might possibly detect gravitational waves from the early universe. Though these are at best far in the future whereas LIGO is here and now.

   On the topic of good science reporting, I agree that the standard was generally very high, though an amusing failure appears in The Independent:
   http://www.independent.co.uk/news/science/gravitational-waves-discovery-could-let-scientists-build-a-time-machine-to-look-into-the-universe-s-a6867466.html

2. **Peter Woit**
   February 13, 2016

   Seth,
   Thanks, that mainly looks like a headline writer failure. On next generation experiments, the piece does have eLISA launching in 2020, whereas it seems it’s going to be much later than that. This 2014 presentation says 2034

   https://astro.uni-bonn.de/~sambaran/DS2014/Modest14_Talks/Benacquista.pdf

   and it looks like the idea that even this later generation of detectors is going to tell you something about before the big bang (“Creation”) or the multiverse is, well, just hype.

3. **Syksy Räsänen**
   February 13, 2016

   Sesh, referring to any detectors that look for signals from the early universe, or that just probe physics that is relevant for understanding the early universe, as time machines has (sadly) become standard. Both Planck and the LHC have been called time machines. In the former case, this was even done by ESA itself:
   http://www.esa.int/Our_Activities/Space_Science/Planck/ESA_s_time_machine

4. **Peter Shor**
   February 13, 2016

   One thing I haven’t seen any mention of is what this means for the supporters of MOND—Modified Newtonian Dynamics? Do they just give up and admit general relativity is correct, or do they continue supporting their theory?

   (Maybe this is off-topic, but there are certain similarities to one of the main topics of this blog.)

5. **anon**
   February 13, 2016

   eLISA is definitely not launching in 2020. As far as I know, eLISA hasn’t even been selected for launch yet. ESA has chosen gravitational waves as the science theme for the third large-class mission slot (L3), planned for 2034. But unless I’ve missed the news, the precise mission concept has not been decided, so the eventual gravitational wave telescope may not resemble eLISA plans.

6. **Shantanu**
February 13, 2016

Peter, MOND is not a relativistic theory But any relativistic version should behave same as GR in the strong field and should produce tensorial fluctuations like in GR. Right now the signal cannot rule out scalar-tensor theories.

7. **Peter Woit**
February 13, 2016

All,
As I feared with the last posting, there's a large number of people wanting to discuss the significance of this signal for GR and black hole physics. This doesn't work very well with my lack of willingness to moderate such a discussion, given that I know nothing much about the subject, and suspect that half of those wanting to make claims about it know as little as I do. So, unless someone has something more on topic, like an explanation of how LIGO is going to provide evidence for the multiverse, please try and find a better venue, hosted by someone who knows more about this. I'll be very happy to advertise that here.

8. **Tim May**
February 13, 2016

This discovery is refreshing in so many ways.

But the connection to this blog theme, I think, is that _experimental_ physics is again vindicated. Actually _seeing_ the waveforms, without elaborate processing and appeals to 1.5 sigma or 2.1 sigma probabilities, is refreshing. And this is far from the only case–the astronomical discoveries of dark energy (or whatever is causing an accelerating expansion of the universe) and of non-luminous or dark matter (the galaxies passing through each other), and all the results with the CMB….these need no hype by journalists or scientists themselves.

Maybe it's time to slow down on the theories that may be centuries (or more) from any hope of testing. (To be sure, the best mathematicians and theorists will continue to work in many areas–and may even have breakthroughs. But maybe it's time to not have most physics departments dominated by one particular area.)

This is an obvious theme, the costs and timelines for the MeV energy range, up through synchrotrons and linear colliders and rings past the GeV range and up to TeV and beyond. The pickings seem to be getting slimmer despite huge increases in cost. With no "surprises" to suggest new physics. A next generation collider may be the last one for a long, long time. When the next big jump in energy requires GNP-level spending, it just won't get funding.

Sometimes it may just be best to switch efforts to a more fruitful field. As I have been reading, the LIGO and Advanced LIGO cost well under a billion dollars. Money well spent!

And how can "multiverse" hype compete for enticing young minds–or spending by governments– with the now dead certain detection of black holes a billion light years away spiraling in and radiating away massive amounts of energy?

Personally, I think this puts a cork in the hype bottle for a long time.

9. **upstate_cyclist**
February 14, 2016

There was one bit of particle related news buried within the detection paper and one of the supplements. Using a simple Yukawa-correction, they put a bound 90% credible limit on the graviton mass at 1.2 x 10^-22 eV/c^2. This is the best dynamical bound to date but still not as "good" as the some of the others from galaxy clusters & weak lensing. But still, its another experimental bound necessary for a theory of quantum gravity, which I thought was what these folks were trying to sort out in the first place.

And at some point, with enough sources, one is able to do some cosmology with these standard sirens, but how to get there never seems to arrive on the page. Also, space-based detectors are sensitive to super-massive black hole and white dwarf binaries, not the primordial waves (if they even exist) from the Big Bang. It looks like both Krauss and Kaku are in some wishful thinking, i.e. "if only the BICEP2 signal hadn't been dust, then … MULTIVERSE".

They are perfectly welcome to do that on their own time and not muddle an article difficult subject to discuss with the public with their fatuous navel gazing.

10. **Mark Hannam**
February 14, 2016

The prime targets for Advanced LIGO are, as you say, astrophysical sources of gravitational waves, especially binary mergers — they are expected to produce the strongest signals.

LISA isn't due to be launched until after 2030, so there will hopefully be many impressive observations and discoveries long before we get anywhere near observing GWs from the big bang.

I think one of the coolest things about this observation was that it is the closest we have ever come to measuring a signal *from* a black hole.

I wrote about what we saw here,

http://fictionalaether.blogspot.com/2016/02/gravitational-waves.html

and a bit about the experience of the last few months and the final announcement here,

http://fictionalaether.blogspot.com/2016/02/what-it-feels-like-to-detect.html

11. **vmarko**
February 14, 2016

Folks, there seems to be some confusion about which kind of gw-detector would be able to look for CGB (the gravitational analogue of CMB), so just a short note: neither ground-based detectors (such as LIGO or the most advanced idea of ET), nor the space-based detectors (such as LISA or the most advanced idea of BBO) would be able to see anything even remotely in the range of CGB frequencies (and this despite the name of the BBO).

The way we *can* look for CGB signals is using the pulsar timing arrays (IPTA and SKA). Only those types of gw-detectors would be useful to "look at Creation" (pardon the language), or at least somewhat close to it. 🙂

A nice map of what kind of detector can see what can be found here:

http://rhcole.com/apps/GWplotter/

The map is interactive, so you can choose which sources, which detectors and which observable will be plotted. The CGB (aka the "stochastic background") goes from 10^(-7) Hz and downwards. So there is no chance for any Solar-system sized detector to catch this signal, aside from the nature-provided detection with the PTA.

GW community has so far been very good at steering clear from hype, so we should help them keep it clean in the future as well.

Best, 🙂
Marko

12. **HarrisTeter**
February 14, 2016

Hello all,

Starting around 37 minutes into the NSF video here: https://www.youtube.com/watch?v=aEPIwEJmZyE, Kip Thorne states that LIGO will see gravitational waves from cosmic strings–listen to the LIGO video and you will hear: "Giant Strings that reach across the universe. They're thought to have been created by the inflationary expansion of the fundamental strings that are the building blocks of all matter that expanded through inflation at the beginning of the universe."

This probably has a greater influence on the perception and direction of things than Kaku and Krauss.

Also, might anyone know when a second signal might be detected by LIGO? Thanks! 🙂

13. **Anonymous Mathematician**
February 14, 2016

@HarrisTeter:

The rumors are that a second signal has already been detected by LIGO (although nowhere near as strong as the first signal). The press conference said they expected at least a handful each year.

14. **Davide Castelvecchi**
February 15, 2016

Thibault Damour, one of the people who predicted gravitational waves from cosmic strings (in work with Vilenkin), told me that even if such signals are discovered by LIGO/Virgo, we wouldn't know if these strings were generated by the fundamental strings of string theory or some other mechanism.

15. **Peter Woit**
February 15, 2016

Davide,
Hype about this has been going on forever, and I've written several times about it on the blog. For an early example, see

http://www.math.columbia.edu/~woit/wordpress/?p=37

which was about a 2004 press release from UCSB claiming that LIGO would within two years "test" string theory and possibly confirm it, by seeing evidence of cosmic strings.

What's going on here is that there is a long story about one-dimensional configurations in the Higgs fields of certain GUT models, which you might expect to be created in some early universe models. At some point certain string theorists started pointing to some fundamental string theories that also had such solutions and started up a hype campaign about them. As always, the story about string theory "predictions" is that you can get any "prediction" you want out of string theory: no cosmic strings, cosmic GUT strings, fundamental strings that look like cosmic GUT strings, or whatever.

I've been glad to see that the press coverage of the LIGO discovery has generally ignored this nonsense.

16. **John**
February 15, 2016

As an interested "lay person" I was disappointed in many of the headlines and quotes. I read that this was the "Holy Grail of physics" and that the discovery "fundamentally changed our perception of the universe". Hype isn't limited to the multiverse it seems. It's a great achievement in experimentation but it verified not changed our perception of the universe.

I do have what I think is an interesting question. Right now we measure distance using the light from "standard candles". Light is subject to lensing effects but I read that the gravitational waves are not "hindered" in any way. They also based the distance to the 2 black holes strictly on the properties of the gravitational waves.

So do you think that gravitational waves can or will be used to verify distance to far off objects? Or at least be used to verify if the "standard candle" method is accurate?

17. **MM87**
February 15, 2016

On Scientific American we learn that "WIMPs are a prediction of superstring theory."

http://www.scientificamerican.com/article/last-call-will-wimps-show-their-faces-in-the-latest-dark-matter-experiment/?wt.mc=SA_Twitter-Share

18. **Chris W.**
February 15, 2016

John,
Gravitational waves should be subject to lensing effects. For example, see arXiv:1309.5731.

19. **upstate_cyclist**
February 16, 2016

@Peter — While there was a search for string cusps in the previous science run before the upgrade began (http://arxiv.org/abs/1310.2384), I expect that cluster resources are going to be allocated such that a repeat of this effort is very far down the priority list. So at least from the instrumental/search side of things, the best bet is continued silence.

Though if primordial B-modes truly recede into the background, one may see another flare-up of interest in this source as theorists grasp for old-new straws as aLIGO reaches design sensitivity and new machines come online.

20. **Davide Castelvecchi**
February 16, 2016

I'm guessing that anything that travels along geodesics should be subject to lensing effects. What's more surprising to me is that the positive cosmological constant might produce dispersion (with no need for gravitons to be massive) and thus chromatic aberrations in the gravitational waves — in other words different wavelengths refracting at different angles. This is apparently one consequence of the work by Ashtekar's team
http://arxiv.org/abs/1510.04990

21. **Ethan**
February 16, 2016

Peter,

Isn't the LIGO result at least some sort of refutation of the Munich symposium on the necessity of experimentation to validate scientific theories? String theory might not have been able to come up with falsifiable experiments of its own, but the detection of gravitational waves one hundred years after being predicted shows that what Richard Feynman said about what science is remains true: https://www.youtube.com/watch?v=EYPapE-3FRw , namely, it doesn't matter how beautiful a theory is or how smart is the guy who proposed it. Unless you can correctly predict outcomes in falsifiable experiments, your theory is not scientific.

22. **Navneeth**
February 16, 2016

For what it's work, Rainer Weiss though the NYT article was bad. 🙂
https://www.youtube.com/watch?v=8_brgtd-zY4
(Don't remember the exact moment he said that.)

23. **Daniele V**
February 16, 2016

Personally, what I find really upsetting is the sentence "High-school textbooks may have to be rewritten": new discoveries in science add chapters to textbooks, do not throw away anything (of well-established theories, of course). Classical mechanics has continued to be normally taught in schools and universities after Relativity and Quantum revolutions, nobody uses Einstein's Equations to make an airplane fly. It may seem just a fussy detail, but in this way people are driven to think that scientific knowledge is not trustworthy at all, because "today scientists assert something, next century the opposite" (heard from a friend of mine) as it were a matter of pure opinions.

P.S.: it's my first post here, I thank the blog master for his hospitality and his interesting blog.

24. **Peter Woit**
February 16, 2016

Ethan,
I don't think the gravitational wave result has much to do with the problems of string theory, or of that part of theoretical physics in general which suffers from not having any prospect of getting relevant data. It's a wonderful example of the canonical scientific method in action, but doesn't change the problems that have led people down roads divorced from experiment (and I need to keep pointing out that there's not anything necessarily wrong with that, the question is how you go about it).

Daniele V.,
Thanks! I think you're right that that's a problem. Possibly what people writing something like that have in mind is something more sensible (that the LIGO experiment will make in into the textbooks as a confirmation of GR, beginning of new kind of astronomy), can't quite help themselves from embracing the trope of a "revolution" overturning the old.

25. **Shantanu**
February 17, 2016

Peter Shor: Let me add something to your MOND question. Eight years ago we proposed that if the relativistic MOND theories are correct then the galactic Shapiro delay of GWs should be much smaller than that of photons/neutrinos (see arxiv:0804.3804). So one acid test
would be comparison of relative arrival time between GWs and photons/neutrinos.
If the Fermi GBM detection at 0.5 seconds is confirmed to be due to this GW signal, then such theories are dead.
Another way is to look for vector modes of GWs. This was suggested by Eva Sagi (See
arxiv:1001.1555) and references therein. As mentioned in the LIGO tests of GR paper, right now they cannot rule out extra polarization states. of course these are just one class of relativistic MOND theories. If MOND is due to modified inertia or something, then at its still an open question.

26. **Venp**
February 18, 2016

Most impressive discovery based on a single event since Omega-minus some 50 years ago.

27. **Shantanu**
February 20, 2016

Peter: something else about this result directly connected to particle physics.
if the Fermi GBM signal seen 0.4 second after the LIGO event is confirmed to be due to
this GW signal, then brane world models are ruled out (as they predict GWs arrive much earlier.) See Fig 1 of
http://arxiv.org/pdf/gr-qc/0105114.pdf

28. **Nathalie**
February 21, 2016

On LISA:

It is sad to see how LISA has been treated up to now, both by NASA and ESA. Much work was done on the project already in 1990s, see for example
http://pubman.mpdl.mpg.de/pubman/item/escidoc:52079/component/escidoc:52080/schutz_60668.pdf
and now I see that the plan is to launch it in 2034. I know that many scientists have devoted their scientific lives to it. Can someone tell me what has been going on?

29. **bnewsbd**
March 16, 2016

Nice Post. Visit another newssite Bnewsbd.com–
Letest and Largest bangla online newspaper in Bangladesh.

30. **Michael R. Sabino**
March 26, 2016

Yes, I think you're right that LIGO isn't the appropriate experiment to detect gravitatational relic radiation. There are several experiments looking at higher, more thermal frequencies for that. Some of them use tiny resonant masses, others like the holography detector at Fermilab use lasers.

Yet More About Grothendieck

February 16, 2016
Categories: Uncategorized

Since Grothendieck's death somewhat more than a year ago, quite a lot of new material about him and his mathematics has become available. Visit the Grothendieck Circle to find a lot of this, with just one example some new chapters of the English translation of the third volume of Scharlau's biography.

This month's AMS Notices has the first of two parts of a long article with contributions from many mathematicians discussing Grothendieck's work and their memories of him and his influence on their careers. Colin McLarty has an excellent expository article, maybe the best of attempts I've seen to explain some of the themes of Grothendieck's mathematics in a relatively accessible manner.

While you're there, this latest issue of the Notices has quite a bit else worth reading, from my colleague Ivan Corwin on KPZ universality to Beilinson on Gelfand's seminar, and an amusing attempt by Jeremy Gray to guess not the next Fields medalist, but who would have gotten one in 1866.

Comments

1. **Anonymous Eric**
   February 17, 2016

   Peter, your math posts may not generate as many comments or interesting discussions, but they are much appreciated. Today's post is a perfect example. The Mochizuki/ABC posts have been very nice also.

   Keep them coming. Thanks.

2. **Anonymous Adam**
   February 17, 2016

   @Anonymous Eric Amen.

3. **MathPhys**
   February 18, 2016

   I definitely agree with this.

Various and Sundry

February 23, 2016
Categories: Uncategorized

- The online magazine Smashpipe has the first part of a two-part article written by Gerald Alper, who recently came up here to Columbia to talk to me about string theory/etc. It was an interesting conversation, so I'm curious to see what he makes of the more substantive part, which is in part two, planned for next week.
- If instead you'd like to read about a conversation with my colleague Brian Greene, there's a piece at Cosmos Magazine. Brian is taking his World Science Festival to Australia next month and will be on tour there.
- In other Columbia news, LHC experimentalist Emlyn Hughes has evidently baffled the students in Frontiers of Science again. Three years ago he undressed for the students, this year the performance somehow involved a student mistress (see here and here). No, I don't understand any of this either.
- As a last Columbia story, this semester in the physics department Bill Zajc is teaching a string theory course for undergraduates, Physics W4012, based on the Zwiebach book. While Zajc isn't a string theorist, he is a frequent commenter at Lubos Motl's blog.
- There's a new issue out of Inference, which has some interesting articles, including an essay by Pierre Schapira on category theory (French version here). Also there's Jean-Pierre Luminet on holography.

  Inference is a bit of a mystery, unclear who is editing it (some speculation here). Whoever it is though, it's quite worth paying attention to.

- The HEP Postdoc Project is collecting anonymously information aimed at helping potential postdocs (or even Ph.D students) find out more about what it's like to work with various senior HEP theorists. No, like Inference, I have no idea who is behind this.

Comments

1. **Harald**
   February 23, 2016

   Inference, oh my? Whoever Michael Fumento is, he does support my theory that no journalist ever will get the difference between watts and watt-hours right.

2. **jsm**
   February 23, 2016

   There are some interesting articles in Inference (but did they have the authors' permissions to publish?).

   However, a supposedly scientific article (Fumento, The Nuclear Reaction) that quotes the Cato Institute in its first paragraph arouses deep suspicions among those of us who regard the Cato Institute as a far-right propaganda mill.

3. **Scott Church**
   February 23, 2016

   Peter,

   Michael Fumento, who also contributed to this issue, is a long-time Far-Right PR hack. He's been a key player in the anti-global warming propaganda machine for nearly 20 years as well as other under-the-table antienvironmental lobbying. At one time or another he's been affiliated with the Cato Institute, the Hudson Institute, the Competitive Enterprise Institute, and a number of other industry-funded astroturf fronts. A quick overview of his background is available at ExxonSecrets, and Tim Lambert has been tracking his antics for some time now at Deltoid. I'm not advocating poisoning the well here, but from long experience I can tell you that any forum he's associated with isn't likely to be a good source of science.

4. **Peter Woit**
   February 23, 2016

   jsm/Scott Church,
   The question of who is editing Inference is still a mystery to me. I linked to Michael Harris's speculation about David Berlinski as the only serious guess I've seen, and I wouldn't be surprised if he and/or people connected to him had something to do with it. This doesn't change the fact that whoever is behind this has gotten some intelligent people to write some interesting things worth reading.

   For a very small number of publications, based on long experience I trust the editors completely and start reading the articles they edited assuming I'm reading something reliable. For most though, if I don't know about the author I don't start out assuming reliability. About 30 seconds of googling "Michael Fumento" gave some indication of where he's coming from, and it wouldn't occur to me to expect an even-handed treatment of his subject.

   My own main scientific interests (mathematics and theoretical physics) mercifully are topics that don't line up ideologically in any of the conventional left-right ways. So I find that I can get something out of articles on these topics written by people whose political and cultural views I strongly disagree with, while at the same time finding appalling some articles by people I'm otherwise on the same side with (e.g., the multiverse…).

   I appreciate the point people make about Fumento, but, all, it wasn't an article that seemed worth paying attention to. Please help me out by keeping the usual left-right fights out of this comment section. I'm generally a partisan of the left, but that doesn't mean that I have any interest in reading more of my side's take on the usual hot button issues, and I suspect I'm not the only one.

5. **srp**
   February 23, 2016

   Mr. Alper's first part seems a tad self-indulgent, but I'm hoping part 2 gets out of his head a bit more.

6. **crazy_horse**
   February 24, 2016

   I suspect Waddingtons are behind the HEP Post-doc Project – it's obviously a ruse to collect data for a new Top Trump cards game about famous physicists..

   I play Susskind, Degree of expertise in general physics 10

7. **adrian**
February 25, 2016

Dear Peter,
the las intention of this comment-post is to be aggressive or offensive. Please, read it with that in mind.

Periodically, you write an article "This week hype" where, in my opinion, you correctly criticise some online or newspaper article/TV show, etc. I believe it is a healthy exercise to alert people that an exaggeration, deformation of truth, misrepresentation of reality, etc, is being made.

I read the first part of the article by Mr. Alper in the website "Smashpipe". While I found charming the fascination he has with academia and its environment, you will probably agree with me that phrases like

–'all semblance of ordinariness falls away and you quickly realize you are in the presence of a remarkable mind'

–'he is putting the finishing touches on a major textbook'

— ' the famous theoretical physicist who had written the controversial, radical book, Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law.'

–'only a little bit louder than the click heard around the world on February 11, 2016, signaling the detection and verification for the first time in history of of Einstein's prediction of gravitational waves'

[this last one, is just imprecision with poetic license]

–'Top flight mathematicians, I can see, think twice as fast as the ordinary person'

–'He seems to be concentrating fiercely, in a private self-space into which only he can slip at will and where no one is allowed. It's the place where lonely geniuses like Gary Kasparov […] '

[these last two, poetic ways of describing you]

Perhaps you will agree that these are also forms of 'hype'.
As I mentioned, I find the article 'endearing' (I am professional physicist and know the environment, Columbia, etc), but you can imagine the impact this may have on a young person.

So, may be you could also criticise that article for its 'hype' content. Also for being (this part one) contentless for a blog like yours—that characteristically contains interesting links and information. You wrote a line about that. I await for part II.

The article about Brian Greene's visit to Australia is also misrepresenting his place among his theoretical Physics peers. One may understand, these are journalist licenses. But let me insist, they can play badly on the young person, who could believe, is in presence of a *great* scientist.

thanks

8. **a l**
February 25, 2016

Inferences publishes high quality work and obviously there is money behind it. The Editors offer a mix of popular science with (right wing) political views and one understands why they prefer to remain anonymous. Just for fun one can check some authors and look what pops up, e.g.:
Henri Lepage, Mont Pelerin society
William Kininmonth, climat sceptic,
Michael Denton, anti-Darwinist

9. **Peter Woit**
February 25, 2016

adrian,

My concern about "hype" has always been about misrepresentation of scientific ideas, trying to convince the public that ideas that don't work are actually successful science. The world is full of other kinds of hype that are more harmless and that people themselves should be able to easily recognize, not needing my help to point out. Most profiles of people in magazines make them out to be more accomplished, more significant and more interesting than they really are, and I don't think I should need to explain that to people. The young will sooner or later figure this out for themselves…

10. **Jeffrey M**
February 25, 2016

I will point out, for those who aren't mathematicians, that in the math community there is quite a lot of argument about category theory. Interesting obviously, anything Grothendieck worked on is bound to be interesting, but there are issues. One of my professors in grad school, who was a student of Eilenberg's at Columbia when Eilenberg was busy developing category theory, used to call it "general abstract nonsense." And he liked it and used it all the time. I swear every proof for an entire year of topology was "the diagram commutes."

11. **Radioactive**
February 25, 2016

Peter, if you read back your last comment and don't see the slightest bit of irony I don't know what to do for you.

12. **Peter Woit**
February 25, 2016

Radioactive,
Well, no more irony in that comment than in most of mine, so I guess I'm beyond help. I do try my best to stick to criticizing (hype about) ideas rather than people, can't think of an example of my writing about an article and criticizing it along the lines of "this says X is so great, but he's not". That people, young or old, might believe incorrect claims about science made in usually reputable information sources seems to me to make challenging such claims worth the effort. Not so with claims about personalities…

13. **adrian**
February 25, 2016

Dear Peter,
your answer to my comment and that of the reader 'radioactive' is satisfactory in my opinion. Indeed, I do notice that when you entitle "This week's hype" you mostly discuss a mis-represented idea, experiment, or a misrepresentation of its consequences.

I agree with you, the hype is to be criticised. The public must be correctly informed. Many times, it is not the fault of the scientist, but of the journalist (or in most cases, the journalist's editor).

There is one point I wish to comment with you—not sharply related to the one we discussed above, but that is "around that".

The public should also know, or be explained, informed and appreciate the fact that technical issues about science are to be decided by the practising scientists, in the usual forum. Let me take an example, close to your heart: the validity of the theory of strings (or a subset of it) as a physical description of reality. This technical issue is NOT going to be decided in a blog. Neither yours, not Motl's nor anyone else's. It will certainly NOT be decided in a newspaper's hardcopy or online article, by some enthusiastic or critic journalist.
It will be decided in the arXiv, the regular seminars, the conferences.
I mention this, because in various popular presentations I gave recently, I noticed how people—specially young people–confuse 'science' with 'what they read in the blogs'.

This is (I think/hope it is clear!) not an accusation in any form. But a suggestions, that, from time to time, it would be good if you can prevent your readers, specially those who are not professional physicists, that some of the topics discussed are NOT "free debate" and that the people who work on those things are not idiots awaiting their illuminating comments. In this sense, I find Strassler's blog, the one that best delivers this idea (and others related).

I do appreciate the role of the blogs. The discussion is nice, sometimes it is interesting.
Some issues can be talked about and your blog, specially, is in some cases an important source of links and information.

But please, consider this comment. I started to feel, since some years ago, this influence of the blogs on lay people. Most worryingly, in young people. On the one hand, it is good as people are interested, on the other hand, it is making them confuse what the thing is actually about.

thanks

14. **Peter Woit**
February 25, 2016

adrian,
I just don't see the problem. Before there were blogs, non-scientists got their information about string theory from magazine articles and popular books, which generally were full of misleading, over-enthusiastic claims about string theory (often put forward not by journalists, but by scientists themselves). To do something about this I wrote a book, and some fraction of the entries on this blog. For people who know nothing about the subject, all this does is tell them that there is a disagreement about the issue. For those who know more about the subject, they can read both sides, think for themselves, follow links and learn more, and see if they can make their minds up about who has the better arguments.

I think what's really bothering you (and bothers Matt Strassler, and a lot of people who are not happy with my blogging), is that some of the arguments made on my blog discredit conventional sources of authority in the subject. My response is that unfortunately this is a very unusual situation in which certain such authorities are, because of human unwillingness to admit failure, making a serious mistake. Their behavior threatens to discredit their own subject, which is one I care about. The worst of this is the string theory multiverse, which is a way to avoid admitting failure, at the cost of abandoning conventional standards of what is science and what isn't. The whole point of this abandonment is to ensure that the issue of failure cannot be adjudicated in the usual ways, by careful argument in the scientific literature. There is no way to show that the string theory multiverse is "wrong" (it's "not even wrong"…).

For those unhappy about this situation, I think the proper thing to do is to take your complaints to the people who have led the subject into this mess, not complain that I'm pointing to the problem.

15. **Erik**
February 26, 2016

I looked around a bit on the HEP Postdoc Project website. I am not a high energy physicist, but I think it is an interesting project. Unfortunately, it struck me that there are already a few hateful/insensitive/harsh 'reviews' on there. How hard can it be to give an honest review of somebody's teaching/supervising skills without bashing them completely? Somehow I had hoped that physicists would prove to be better than the general audience, which also has a tendency to do this.

16. **Ned**
February 26, 2016

Dear Mr. Woit,

sorry for butting in – just in regard to your 7:44 pm comment to adrian I wanted to say as a grateful longtime reader of your blog that I've never yet read such a succint and concise statement about the point you're coming from.
If you ever should put up something like a FAQ or a "mission statement", these 2 paragraphs maybe would have a place there.

17. **Scott Church**
February 26, 2016

Peter, I second Ned's comment. Great summary of the service you're providing to the physics and lay communities here. 🙂

18. **Yatima**
March 5, 2016

[Attempt to carry on a tedious left/right argument deleted. Please don't do this. No sensible person wants to read such a thing.]

Anyway, I finally got around to reading Pierre Schapira's article on Category Theory, which references this nice Scientific American blogpost about computer-assisted theorem proving which I would like to point out:

[Voevodsky's Mathematical Revolution]

We red:

The same week as the Heidelberg Laureate Forum was a conference in Barcelona on univalent foundations, which Voevodsky skipped in order to be with us. A special issue of a journal (perhaps Automated Reasoning — Voevodsky couldn't quite remember) will come out of the conference, with papers written by the participants. Almost all of them will be submitted together with the formalized proof in Coq. That's most likely the first time such a thing has ever happened.

Some of those computer verifications rely on a library of verified proofs that Voevodsky himself has created, so Voevodsky decided to submit his library to ArXiv. He imagined a one-page description of what the library is, along with all of his Coq files. It turns out, however, that ArXiv isn't yet up to the task — while it can accept attached files, they can't have any directory structure. Voevodsky plans on pestering the folks who run ArXiv until they make it possible.

Said blogpost references a no-longer existent lecture video, which however can be found on YouTube:

Vladimir Voevodsky: Univalent Foundations: New Foundations of Mathematics

Beyond Experiment: Why the scientific method may be old hat

February 27, 2016
Categories: Multiverse Mania

This week's New Scientist has an article by Jim Baggott and Daniel Cossins entitled Beyond Experiment: Why the scientific method may be old hat, which deals with the recent controversy over attempts to excuse the failure of string theory by invoking the multiverse. The article (unfortunately behind a paywall) does a good job of describing the nature of the controversy: what do you do when it becomes clear your theory can't be tested? Do you follow the conventional scientific norms, give up on it and work on something else, or do you try and find some kind of excuse, even if it means abandoning those norms?

Much of the article deals with the issues raised at the recent Munich conference (discussed here). Two of those quoted (Dawid and Gross) are not multiverse partisans, instead argue that the motivations that got people interested in string unification more than 30 years ago are good enough to justify indefinitely pursuing the theory, no matter how bad things look for prospects of connection to experiment. On the other hand:

> Their enthusiasm is far from universal, and some physicists are downright alarmed. Woit warns that the need for empirical vindication could be pushed so far into the background as to be invisible. Carlo Rovelli, a theorist at the University of Aix-Marseille in France, believes that this scenario has already come to pass. Rovelli … argues that the last thing we need is a system that legitimises failed theories. "A theory is interesting when it teaches us something new about the real world," he says. "Not when it becomes a house of cards that delivers nothing but university positions."

On the question of the string theory multiverse as science, those gathered at the Munich conference were pretty uniformly hostile. As a proponent of this, the article quotes only one person, who wasn't there:

> Sean Carroll, a theorist at the Caltech Institute of Technology at Pasadena and a leading advocate of the multiverse, insists that if anyone is being unscientific, it is those physicists who seek to enforce outmoded philosophical principles and impossibly high standards. "People support these theories because they offer the best chance of providing a useful account of the data we actually do collect here in our universe."

I'm not sure how the string theory multiverse provides an account of data we have collected that is "useful", except in the sense of "useful to those who don't want to give up on string theory."

Carroll has explained his views in more detail here, arguing that falsifiability is an idea that needs to be retired, to be replaced by "empiricism". "Empiricism" seems to mean "ability to account for the data", with "the multiverse did it" an acceptable way to account for data, even if not falsifiable. He'll be giving a talk on this at the American Astronomical Society meeting in San Diego this summer, with abstract:

> A number of theories in contemporary physics and cosmology place an emphasis on features that are hard, and arguably impossible, to test. These include the cosmological multiverse as well as some approaches to quantum gravity. Worries have been raised that these models attempt to sidestep the purportedly crucial principle of falsifiability. Proponents of these theories sometimes suggest that we are seeing a new approach to science, while opponents fear that we are abandoning science altogether. I will argue that in fact these theories are straightforwardly scientific and can be evaluated in absolutely conventional ways, based on empiricism, abduction (inference to the best explanation), and Bayesian reasoning. The integrity of science remains intact.

Carroll's argument seems to be that the conventional understanding of how science works that we teach students and use to explain the power of science has always been wrong. Falsifiability by experiment isn't necessary, instead, what is the "absolutely conventional" way to do science is "empiricism, abduction (inference to the best explanation), and Bayesian reasoning". I'd never heard of abduction as a basis of science before. If you believe Wikipedia, this goes back to Charles Sanders Peirce, whose view in later years was:

> Abduction is guessing. It is "very little hampered" by rules of logic. Even a well-prepared mind's individual guesses are more frequently wrong than right. But the success of our guesses far exceeds that of random luck and seems born of attunement to nature by instinct (some speak of intuition in such contexts).

As for "Bayesian reasoning", I would have thought that Polchinski's Bayesian calculation of an "94% chance" of a multiverse would have conclusively shown the absurdity of that.

Comments

1. **Carl**
   February 27, 2016

   The fundamental problem with trying to apply Bayesian reasoning to the multiverse is that it requires you to presume a multiverse to begin with. If there's only actually one universe, it's meaningless to talk probabilistically about the properties of that universe.

2. **Jon Awbrey**
   February 27, 2016

   (sp) Peirce

3. **Jon Awbrey**
   February 27, 2016

   Peirce is simply describing the process by which we seize on an initial hypothesis or model. That will in practice be constrained by all sorts of previous experiences with the phenomenon in question but in principle our choice can be very wild indeed, revealed in a dream or stepping off a bus or whatever it may be. The only real test of the hypothesis or model comes by way of the deductive consequences that follow from it and the inductive confirmations or falsifications that follow those. The word *abduction* is a clumsy translation of Aristotle's *apagoge* from Prior Analytics 2.25.

4. **Felipe Pait**
   February 27, 2016

   What exactly would one be able to learn about the real world using a theory that cannot make predictions that are falsifiable?

5. **Peter Woit**
   February 27, 2016

   Jon Awbrey,

Thanks, fixed.

6. **Peter Woit**
February 27, 2016

Felipe Pait,

By Carroll's notion of science, I think he can claim to be learning about the real world, but I don't think he can answer the question "How will you know if you are wrong?" Empiricism, abduction, Bayesian reasoning, etc. will get you to a model that seems to you to describe the real world, but how will you know if you're mistaken? It is this ability of experiment to shoot down a model that you are fond of that seems to me to be the most distinctive part of science, and I don't see that anywhere in Carroll's conception.

7. **john lowery**
February 27, 2016

Because Darwin wasn't there when his Evolution occurred– he used abduction (inference to the best explanation) to formulate his theories.

8. **Jon Awbrey**
February 27, 2016

There has always been a lot of misunderstanding about the requirement of falsifiability. At root it is simply the idea that an empirical law is not a logical tautology. I don't see any reason to dispense with that just yet. In practice the principle affords us leverage only when we have two or more competing theories for the same domain.

9. **Harry Dale Huffman**
February 27, 2016

When you have to change the definition of "science" (decades into your research, especially), it means you are no longer a good, honest scientist. Period. (It's much the same as changing the data to fit the theory.) Throw those criminals out of science.

10. **Bayes_or_bust**
February 27, 2016

I don't think it's fair to judge Bayesianism by Polchinski's text, which was indeed absurd.

I think you're right, though, to be wary of scientists invoking Bayes as a get out of jail free card for their ideas. Bayesianism provides a strict framework for updating degrees of belief in light of data by a so-called Bayesian evidence. Polchinski did not use that framework – he just pulled numbers from thin air.

It seems that many wish to claim their arguments are Bayesian or that their theories are supported by Bayes, but actual calculations seem beneath them. If they don't make the calculations showing their theories to be more plausible than alternatives, they shouldn't be allowed any refuge under the cover of Bayes.

11. **Peter Morgan**
February 27, 2016

A tangential note that Carlo Rovelli has just become the editor of Foundations of Physics, http://www.springer.com/physics/journal/10701, replacing Gerard 't Hooft. It will be interesting to see how the journal changes.

12. **Jon Awbrey**
February 27, 2016

Another thing that needs to be understood is that Bayes reasoning or anything that involves probabilities has anything to do with the initial abduction, which takes us from a state of unquantifiable uncertainty to the first hypothesis of a model category or reference class. It is only after these choices are made that speaking of probabilities becomes possible.

13. **Jon Awbrey**
February 27, 2016

Correction: has nothing to do with

14. **Nick M.**
February 27, 2016

Peter,

I think you meant to quote "Polchinski's Bayesain calculation" as a "94% chance" of a multiverse; just makes it sound all the more absurd.

15. **Peter Woit**
February 27, 2016

Nick M.,
Thanks, fixed.

16. **Jon Awbrey**
February 28, 2016

I probably shouldn't judge a talk by its abstract, but the more I read Carroll's the more Wikipedisch it sounds. The phrase "inference to the best explanation" was coined by Gilbert Harman in his attempt to explain abductive inference but it conveys the wrong impression if anyone takes it as a substitute for the whole course of inquiry rather than just its starting point. Peirce himself was always very clear about this.

17. **M**
February 28, 2016

150 years ago the dominant theory was that particles are vortices in the aether. What a pity that the scientific method forced us to replace this empirically superior understanding with quantum mechanics and relativity.

18. **sm**
    February 28, 2016

    Carl:

    "The fundamental problem with trying to apply Bayesian reasoning to the multiverse is that it requires you to presume a multiverse to begin with"

    No. It only requires a non-zero 'probability' for a multiverse to begin the algorithm, and it can be as small as you like (e.g., 10^{-100} but make sure you are using double double precision in your computer program!). Here 'probability' is a number between 0 and 1 representing your subjective 'belief' in the multiverse (calling it a probability just because it takes values between 0 and 1 is a mistake because it muddles subjective belief with frequentist probability). Bayesian analysis tells us we can have confidence in the existence of a multiverse if the flow of all relevant information into the Bayes algorithm has driven this 'probability' sufficiently close to 1 that everybody agrees there is a multiverse (0.94 is clearly no where near close enough for readers of this blog!). If someone still rejects the multiverse they should state just how close to 1 they need for 'belief' and if the belief reaches that value at some point in the future then accept the multiverse. On the other hand if the belief is driven sufficiently close to 0 then the multiverse hypothesis can be rejected in the same spirit. Or they can reject the whole idea of Bayesian analysis (which by the way is at the root of a major part of modern technology nowadays).

19. **vmarko**
    February 28, 2016

    Sm,

    I would begin the algorithm with exactly zero initial probability for the multiverse, since it reflects my belief that there is no multiverse to begin with. So how do you continue the calculation from there? Is there any hard data regarding the multiverse that can move the zero prior even to 0.5, let alone 0.94? I don't think so.

    🙂
    Marko

20. **a l**
    February 28, 2016

    Falsificationism came to replace verificationism as it became obvious that general statements, e.g. "all electrons in the universe are identical", are not really verifiable. Thus the burden of proof is shifted: it is the opponents/deniers who have to provide evidence. And evidence can be disputed, e.g. do muons disprove the general statement or are they something else?

    The existence of a multiverse is obviously not a general statement but an existential one: its supporters are supposed to provide evidence and not just beliefs.

    We can see Science devolving: originally it was 'How the world is'; that became 'How do we know it' and next 'Why do we believe it'. The Bayesian argument ultimately reduces knowledge to the best one among of our beliefs – which is no more than to say 'Just now we are unable to imagine something better'.

21. **Bayes_or_bust**
    February 28, 2016

    a 1, that's right: Bayes tells you to believe the most plausable theory you can find and provides a recipe for evaluating the plausibility of a theory. What more do you want? Do you want Bayes' theorem to invent new hypotheses for you or prove no-go theorems that preclude viable hypotheses?

    I really think you're all barking up the wrong tree by criticising Bayes itself. I'd advise scrutinising the Bayesian multiverse arguments – do they actually calculate anything? Can you express their arguments as equations in terms of probabilities? – so far the arguments strike me as being rather specious and not calculations within the framework of BCT.

    In other words, accept the use of Bayes, and ask does Bayes actually give support to eg the multiverse? My feeling is that there are difficulties, to say the least. I'd like to see what eg Caroll has actually calculed when he says these things.

22. **sm**
    February 28, 2016

    vmarko:

    'I would begin the algorithm with exactly zero initial probability for the multiverse,'

    Not only an incorrect application of the algorithm, but also a pretty religious and dogmatic position you adopt, if I may say so!

    Wouldn't a more scientific approach be to take a small prior (not really necessary though) and watch the information flow in the fullness of time drive that prior to zero, thus confirming your initial belief, if in fact that happens? Also, I wonder what would have happened to a large fraction of the Atlas/CMS higgs analyses had they taken a zero prior for the existence of the Higgs!?

    Perhaps a more thoughtful criticism would be that the multiverse proponents have not (yet?) focused on a definite method for calculating the likelihoods. Unless I am mistaken, this is the point Peter is always making.

23. **Jon Awbrey**
    February 28, 2016

    I think that is correct. Bayes' theorem is a deductive identity that adds no information to the situation, nor is that its job. It does not add rows or columns to the contingency matrix or make the observations that populate its cells. Those are jobs for the independent capacities of abductive and inductive reasoning.

24. **Peter Shor**
    February 28, 2016

    If you have enough evidence for your hypothesis, Bayesian reasoning and ordinary statistics will give the same results.

    So applying logic, this means that the only time you need to use Bayesian reasoning is when you don't have enough evidence for your hypothesis. It seems to me that this statement also has quite a bit of empirical evidence supporting it.

25. **Anonyrat**
    February 28, 2016

The problem is not just with the tiny but non-zero subjective belief in the multiverse. Any feature of the world that occurs only in only some branches of the multiverse does not drive the subjective probability of the multiverse any higher, because of the very large number of branches of the multiverse. Features of the world (say, Lorentz invariance) that are true for almost all branches of the multiverse (e.g., in string theory) do increase the Bayesian subjective belief of a multiverse; but also make the multiverse scientifically unnecessary.

26. **vmarko**
February 28, 2016

Sm,

I choose the zero prior because the multiverse idea isn't testable experimentally, by definition. It's like with religion, an atheist chooses zero prior for the existence of God not because he wants to apply Bayesian analysis to God, but because Bayesian analysis isn't applicable (no new data can ever be provided for or against the existence of God, by definition).

In contrast, things like the Higgs hypothesis, the GW hypothesis and low-energy SUSY hypothesis are all highly testable, and the new data (respectively observed, observed, not observed) can change the prior using Bayesian analysis.

The point is that new *data* can change the prior, while new *beliefs* cannot. If we were to use beliefs as data-points, the existence of God would be very supported by Bayesian analysis. Ditto for the multiverse. So one needs to be careful to apply Bayes only to testable hypotheses.

🙂
Marko

27. **steven johnson**
February 28, 2016

Karl Popper knew something about falsificationism. And he knew that by its standards Darwin wasn't a scientist and natural selection etc. wasn't science. Somebody managed at last to cobble up some minor experiments, or maybe hypothetical experiments, so that he finally agreed to retract this rather embarrassing conclusion.

As an historical science, evolutionary biology isn't very big on the properly falsificationist experiments. It tends to be big news when someone actually conducts a controlled experiment in evolution. In historical sciences like evolutionary biology, geology and cosmology, experiments tend to make assumptions about how laboratory experiments demonstrate physical principles at work. Of course, this evades the problem of induction: How do we know that an experiment on the behavior of perovskite in a lab really provides a valid generalization? Here too falsificationism condemns the historical sciences as unscientific. (Popper wanted to condemn all history as unscientific because he disliked Marxism, being an extreme right winger, one of the founders of the notorious Mont Pelerin Society.)

On the other hand, it is routine to find perfectly good falsificationist experiments in the works of such scientists as J.B. Rhine and Michel Gauquelin. Their tradition lives on in whole fields, like parapsychology and evolutionary psychology.

So, I'm sorry but until it's possible to clarify some of the peculiarities in the notion of "falsification" as exemplified in its great champion, the assumption that it's enough to cry "Unfalsifiable!" isn't quite acceptable to everyone. It really does seem to me the shoe is on the other foot. After all, by falsificationist perspectives, how can the widespread use of models be justified? When is adjusting a parameter pursuing a failed theory?

As I understood it, abduction is probabilistic reasoning. Since it seems that learning how things really are (which is what science seems to be so far as I can tell,) instead of proving a logically necessary a priori by deduction, that doesn't seem to be a bad thing.

When I understand Bayesian reasoning (as opposed to Bayes' theorem,) I'll decide what I think about that.

28. **Ben**
February 28, 2016

sm, I think you must not have read Polchinski's argument (which is in arXiv:1512.02477). He calls it "quasi-Bayesian", but the actual argument is "I'll start with a 1/2 prior probability, and multiply it by 1/2 for each of these three reasons to believe in a multiverse".

If you actually wanted to use "Bayesian" techniques to figure out the odds of a multiverse, I suppose you'd start with

$P(M|E) : P(\neg M|E) = P(E|M)\, P(M) : P(E|\neg M)\, P(\neg M)$

where M is "there's a multiverse" and E is the evidence and : is a ratio representing relative odds. He takes $P(M) = P(\neg M) = 1/2$ as his prior. I guess his three reasons to believe in a multiverse with a 1/2 probability assigned to each means he wants $P(E|\neg M) = 1/8$. If you take $P(E|M) = 1$, the odds for a multiverse are 8:1 ≈ 89%. Other values of $P(E|M)$ give lower odds.

Setting aside that Polchinski did the math wrong and showed no sign of understanding what Bayesianism even is, the main problem is that $P(E|\neg M)$ is made up out of nowhere. It's similar to what happens with the Drake equation, where the argument boils down to "it's clearly true that $a/z = (a/b)(b/c)\ldots(x/y)(y/z)$, and we know some of those factors accurately, and others approximately, and the rest not at all so I'll make up some values, and plugging in all of that we get that a/z is high (or low depending on my preconceptions), and you can trust it because it came out of this self-evidently correct equation, and I totally didn't tweak my guesses until I got the answer I wanted".

There's obviously a well-defined sense in which the Bayes equation is correct. When people disagree with what you get out of it, they're disagreeing with what you put into it.

vmarko, a prior of 0 in Bayesian terms means you're impervious to any evidence whatsoever, even the end of the world happening exactly as prophesied in Revelation. sm is right to make fun of you for that.

29. **ID**
February 28, 2016

re: Sean Carroll, is there a reason to prefer multiverse to intelligent design? can Bayesian reasoning provide a probability for ID?

30. **Peter Woit**
February 28, 2016

steven johnson,
"the assumption that it's enough to cry "Unfalsifiable!" isn't quite acceptable to everyone"

This is a straw man argument. No one, Popper included, claims that falsifiability is all there is to this issue. I've written extensively about this, including a chapter in my book, and endless discussions here.

About bringing evolution into this, do you really want to go there? Carroll's argument that the string theory multiverse is "straightforwardly scientific and can be evaluated in absolutely conventional ways" is in danger of being interpreted as a claim that untestable multiverse theories are of the same nature as the testable theory of evolution (some other string theorists have explicitly made this argument). The ID people love this…

31. **srp**
February 28, 2016

Economists have grappled some with the applicability of formal probabilistic decision theory (especially Bayesian expected utility maximization) to different types of questions. A distinction often cited by anti-mathematization advocates is one Frank Knight made between "risk" (where probabilities can be reliably assigned to relevant events) and "uncertainty" (where they cannot). The usual Bayesian riposte is that a probability distribution of probabilities still yields a probability distribution.

More fundamentally, along the lines of what I think Jon Awbrey is getting at, Ken Binmore pointed out that even the leading developer of subjective expected utility theory, Leonard Savage, in 1951 made a distinction between "small world" questions where it made sense to assign probabilities to events and "large world" questions where it did not. The multiverse question seems to me to be the largest large-world question possible.
http://else.econ.ucl.ac.uk/papers/uploaded/266.pdf

32. **Justin**
February 28, 2016

I think some people don't realize just how destructive the multiverse enterprise is. In my case, it literally turns me away from physics. I feel depressed that the greatest joy in my life has become a complete joke.

33. **Jon Awbrey**
February 28, 2016

I have no horse in this race (cat in this box?) as far as multiverses and polycosmoi go — I will limit myself to clearing up popular confusions about Peirce's concept of abductive reasoning. Analytic philosophy swayed many people into thinking that science could be reduced to purely deductive reasoning, eliminating induction and ignoring abduction, but Peirce was a practicing scientist who worked outside that warp. In his model of the inquiry process abduction is at root logically prior to any discussion of probabilities, however true it may be that all three modes of inference work in tandem to advance any moderately complex investigation. There's more information on the history and function of abductive inference in the following article:

• Functional Logic : Inquiry and Analogy

See especially:
• Section 1.2. Types of Reasoning in C.S. Peirce
• Section 1.4. Aristotle's "Apagogy" : Abductive Reasoning as Problem Reduction

34. **vmarko**
February 28, 2016

Ben,

I am fully aware what a zero prior means, I picked it up deliberately. But I guess the point I was trying to make didn't really get accross. I assign zero prior to the existence of multiverse, of unicorns, of the Loch Ness monster, and of UFO implants in people's necks. The reason why I do that is because none of these are any part of testable and falsifiable science, so whether one believes in those is a matter of personal choice, and only priors of 0 and 1 make sense. That is, one either believes in this (uncritically) or one does not (also uncritically). Either way, it is not a subject of Bayesian analysis.

Some people are trying to spin the multiverse idea as something scientific, or at least motivated by well-established science. This is misleading – what string theorists study is a conjecture on top of a generalization on top of a conjecture on top of an extrapolation on top of a generalization of existing established well-tested theories of physics. And when this whole edifice of theoretical construction falls short of their initial expectations (i.e. the landscape problem), they invent yet another layer of extrapolation which provides a catch-all panacea-like answer to any remaining unanswered question: "multiverse did it". And when I refuse to assign a nonzero prior to that, people think that I don't know what a zero prior means…

🙂
Marko

35. **David Nataf**
February 29, 2016

Justin,

It's not sensible for the multiverse research to turn you away from physics. The vast majority of physics research out there has nothing to do with the multiverse.

36. **Peter Woit**
February 29, 2016

All,
I'm deleting further attempts to debate vmarko's point, which are going nowhere.

37. **Butters**
February 29, 2016

You are doing a disservice by convincing gullible people that confirmatory experiments are the only way to make progress in science.

38. **Peter Woit**
February 29, 2016

Butters,
Another straw man argument that I've never made.

39. **Ali**
February 29, 2016

Peirce is spinning in his grave regarding Carroll's conflation of Peirce's notion of abduction and Bayesian inference. Peirce spent much of his life fighting the "inverse probabilists". His collected writings still represent the most compelling, systematic, and sustained arguments against Bayesianism. His critiques (made over decades) also happen to be among the earliest criticisms of inverse probability (along with his contemporary Venn, but Peirce's views were even more systematic).

40. **Veríssimo**
February 29, 2016

How is abduction any different from belief bias ou confirmation bias?

41. **Scott Church**
February 29, 2016

I think the issue here is what constitutes "proof". The idea behind abduction, or "inference to the best explanation", is problematic only if we think of it in these terms rather than likelihoods. Given two or more inconclusive theories offered as explanations of some body of evidence, testable or not, we cannot claim to have *proven* any of them via abduction or Bayes reasoning. But we can use such reasoning to claim that one or more of them are more likely to be true given what we know of the world so far. And the more evidence we gather that favors one over the others, the more reason we have to be confident in it. If the body of evidence becomes so large that all but a handful of cranks would favor another alternative, then perhaps we could start calling it fact, but not before. In any event we shouldn't be calling it science until it's at least testable in principle.

The key is to treat abduction as a measure of *confidence* rather than proof, and Bayes reasoning formalizes this approach well. Problems only arise when we try to apply it numerically to situations where the background and probability spaces involved (particularly the probability of the evidence obtaining if the hypothesis isn't true) are ambiguous and difficult or impossible to quantify. But even if we can't meaningfully apply numbers, we might still be able to frame a general argument in Bayesian terms. For instance, I don't see how one could calculate the probability that NASA faked the moon landings on a sound stage at Norton Air Force Base in San Bernadino, but I doubt anyone would dispute that the likelihood of the evidence given the background alone is small enough to merit rejecting that theory.

In the case of the multiverse, the issue isn't Bayesian methods per se. It's the fact that in the absence of a rigorously understood mechanism for actually producing one it's all but impossible to pin down the sample space involved, or even to define a meaningful probabilistic measure when we can only observe our universe. The same is true with the string landscape. With Bayesian analysis everything depends on how one defines the *reference classes* and how they're discriminated from the background. The biggest problem with Carroll's "empiricist" approach is that within a Bayesian framework, he wants to treat theoretical "elegance", or "beauty" as *evidence* that infers to the best explanation. Stunningly gorgeous (in his opinion) would passes for proven fact. On this logic, "my wife is hotter than yours!" would pass for "science". Really…? Even if he were married to Miss July, and I to a roller derby queen who lost a head-butting contest with an East African rhino, how exactly does one formally demonstrate such a claim? History is strewn with the wreckage of heart-achingly beautiful theories that ran afoul of the real world, and as Edmund Burke once said, those who are ignorant of the past are doomed to repeat it.

42. **Jon Awbrey**
February 29, 2016

Let me just say again that abduction is not "inference to the best explanation" (ITBE). That gloss derives from a later attempt to rationalize Peirce's idea and it has led to a whole literature of misconception. Abduction is more like "inference to *any* explanation" (ITAE) — or maybe following Kant's phrase, "conceiving a concept that reduces the manifold to a unity". The most difficult part of its labor is delivering a term, very often new or unnoticed, that can serve as a middle term in grasping the structure of an object domain.

43. **Ignatz Ratzkywatzky**
February 29, 2016

Justin says:
February 28, 2016 at 9:47 pm

"I think some people don't realize just how destructive the multiverse enterprise is. In my case, it literally turns me away from physics. I feel depressed that the greatest joy in my life has become a complete joke."

How odd.

Aside from the tiny part of physics phase space that are the completely speculative parts of astrophysics and HEP, the rest of physics has never been healthier.

No lack of interesting questions to work on in condensed matter, optics, soft matter, biophysics, experimental astrophysics, etc. all of which still adhere to the only way to do science – the scientific method.

I'd leave theses "angels on a pin" arguments to others and find a more productive and rewarding way to spend my life.

44. **Scott Church**
February 29, 2016

Jon Awbrey,

Perhaps "inference to the best explanation" wasn't what Peirce had in mind, but for better or worse that seems to be how the term "abduction" is most commonly used today. The Wikipedia page on Abductive Reasoning even defines it as such. In any event, I'm not particularly attached to that label so if it isn't the best one feel free to toss it. 🙂 My only intent was to draw the distinction between *confidence* and *proof* in how we adjudicate between theories and what passes for science.

45. **David Levitt**
February 29, 2016

Justin: "I think some people don't realize just how destructive the multiverse enterprise is. In my case, it literally turns me away from physics. I feel depressed that the greatest joy in my life has become a complete joke."

For me, a 73 year old professional scientist that still remembers my fascination reading about physics and physicists in Scientific American 60 years ago, it is more of an "Emperor has no clothes" moment. I now realize that theoretical physicists are no smarter, honest or perceptive than many of the scientists in my own much more pedestrian field.

46. **Bernhard**

February 29, 2016

Sorry if this has been discussed already. Is Carroll still advocating Weinberg's "prediction" of the cosmological constant as a pro-argument to the multiverse? I forgot about this argument but now reading it again it's pretty clear to me I have no idea what this argument has to do with multiverse. I get the environmental argument, but that's it. Specifically:

"If the universe we see around us is the only one there is, the vacuum energy is a unique constant of nature, and we are faced with the problem of explaining it. If, on the other hand, we live in a multiverse, the vacuum energy could be completely different in different regions, and an explanation suggests itself immediately: in regions where the vacuum energy is much larger, conditions are inhospitable to the existence of life. There is therefore a selection effect, and we should predict a small value of the vacuum energy. Indeed, using this precise reasoning, Steven Weinberg did predict the value of the vacuum energy, long before the acceleration of the universe was discovered."

The argument that a small cosmological constant means necessarily a multiverse beats me…

47. **Unifier**
February 29, 2016

Bernard,

The reason can be found in this talk given by Nima Arkani-Hamed,
entitled "Why is There a Macroscopic Universe?"

https://www.youtube.com/watch?v=F2Fxt_yCrcc

Peter,

In that same talk, Arkani-Hamed said that people who are quick to
dismiss the multiverse as a possible solution to the CC problem have
never actually worked on the problem. What do you think about this?

48. **Peter Woit**
February 29, 2016

Unifier,
I don't dismiss the multiverse as a possible solution to the CC problem. Maybe that's the way the world is. The problem is that "the multiverse did it" is a scientifically empty argument. The worst part of what is going on here is that this kind of empty argument is being made in order to justify refusal to admit failure.

But, ignoring that, what I mean when I say it's empty is this: the string theory landscape does not allow you to calculate a real probability distribution of CCs, so what people do is choose a flat probability distribution over the anthropically allowed range, and then congratulate themselves that the observed CC is not probabilistically extremely unlikely (although how unlikely it is depends on all sorts of assumptions). I agree with Arkani-Hamed that there's no good idea about what sets the CC, so I propose the Peter Woit CC theory, which is "I have no frigging idea why the CC is one thing rather than the other". What are the observational predictions of the Peter Woit CC theory? Well, since there's no reason for the CC to be one thing or another, my observational prediction is that it will be in the anthropic range (this is a tautology), with a flat probability distribution. Funny, but this is exactly the same prediction that you get from the string theory landscape. Now do you see why I say "the multiverse did it" explanation of the CC is empty of conventional scientific content?

49. **Shantanu**
March 1, 2016

Peter , OT. But from a recent preprint by ellis, it seems Lev Okun is no more. RIP.

50. **Mike Sharples**
March 1, 2016

In the rarefied atmosphere of Theoretical Physics I suspect that the elite 2% sometime forget that when Bayesian statistics or arguments on the principle of falsification are invoked, then this is an area where many can meet them on completely equal terms.

There is no intellectual argument that is so esoteric that it can transcend the more prosaic principles that these simple concepts demand.

So when an argument looks circular to the "ordinary guy" there is a pretty good chance that it is. You have to be able to justify yourself in ordinary terms. If you cannot then it is certain that your arguments are too weak, not too complex.

51. **Jr**
March 2, 2016

"Carroll's argument seems to be that the conventional understanding of how science works that we teach students and use to explain the power of science has always been wrong. "

Well, I think that philosophers would argue that conventional descriptions of science taught to students in science courses vastly oversimplify how science works, or should work. A lot of science is not really about trying to falsify theories.

52. **Peter Woit**
March 2, 2016

Jr,
I don't recall as a student being taught any rigid notion of what was science and what wasn't, other than the importance of being able to find out if your were wrong by doing an experiment. This is what Carroll is now campaigning against, and the goal is to have multiverse pseudo-science written into the textbooks. Do you really think that teaching students about the multiverse and explaining to them that this is "absolutely conventional" science is a good idea?

53. **Jr**
March 3, 2016

No, I do not think teaching the multiverse theory seems like a good idea and it does strike me as seriously misguided. But independently of that, it may be a good idea to make the textbooks at little more nuanced on the topic of falsification. I do not want to go into to much discussion on the point since anything I could say has been said much better by philosophers already.

54. **Jon Awbrey**
March 4, 2016

Re: Scott Church

Names are not important of course, except for the purpose of communication. The important thing is for us to distinguish hypothesis formation from hypothesis evaluation. Now, there happens to be a long tradition of using the word *abduction* to distinguish that former, most incipient stage of inquiry and I think it serves communication to preserve that tradition.

Concepts, hypotheses, and theories have to be formed, logically speaking, before they can be evaluated. In complex inquiries extending over long periods of time, formation, evaluation, and re-formation will of course proceed in cascades of parallel and series operations, but the analytic distinction between elements and mixtures is still worth its salt.

The role of ab-, de-, in-duction in the cycle of inquiry is discussed a bit further in the following article:

☞ [InterSciWiki • Inquiry]

55. **Masood-ul-Alam**
March 7, 2016

Your mention of "Bayesian reasoning" in reference to Polchinski's Bayesian calculation of an "94% chance" is worrisome.
There are books on gravitational waves or astrostatistics that include discussions on the so-called Bayesian and frequentist "viewpoints." It is one thing to say which "mathematical formula" is applicable in which
situation. It is another thing to discuss philosophical issues which generate potential source of esoteric matter and confusion in physics teaching. It think it will be a great service to science and students of science if authorities sit together and set the meaning of the phrase "Bayesian" in gravitational physics and drop it if it is unnecessary.

56. **LM**
March 7, 2016

Would "testable prediction" be the same as "falsifiable"? I think next to everybody can agree that a theory with no testable predictions is pure speculation. Now, what would be the distance between testability and falsifiability?

57. **Peter Woit**
March 7, 2016

LM,
I don't think the issue of how you test a scientific idea can be summarized in any one word, whether its "testable" or "falsifiable". This is a complex question, and you have to look at exactly what is being claimed as a "test".

The relevance of falsifiability is that there's obviously something suspicious about a "test" that you can't fail. But also obviously, that can be meaningless. String theorists are known to claim string theory is falsifiable because it is based on quantum mechanics and quantum mechanics is falsifiable, but claiming "my theory is falsifiable, because it says that "absurd thing X won't happen" is obviously problematic. Any time spent thinking about this leads to the conclusion that this is a complicated issue, with bogus "tests" easy to come up with, meaningful ones not hard to recognize as such. My claim is just that string theory is not capable of giving a meaningful one, and to evaluate that claim you need to look at what supposed string theory "tests" are.

58. **LM**
March 7, 2016

I agree that the distinction doesn't answer all questions but the point is that no one would argue that to be science a theory must be tested. So it's a matter of test. There has to be a definite and defined test, it has to be reproduceable, and yield definite results. It has to be new – in the sense that a new theory cannot be based exclusively on previous tests (it wouldn't add any new knowledge – if I'm not mistaken, the Occam's razor). And as long as theory aims to describe real world the test has to be empirical i.e based on observable reality. So wouldn't these discussions better served defining testability – the notion that may indeed be evolved and extended with time?

59. **Peter Woit**
March 7, 2016

LM,
The problem is that of defining what a good "test" is. You've given some criteria, but they don't address the relevant problems that come up. For instance: some physicists like to claim that their multiverse theory is testable, because it is conceivable within their framework that certain distinctive patterns would appear in the CMB. So, there's a test: does this pattern appear or not in the CMB? If it did, that would definitely be evidence for their theory. The problem with non-falsifiable tests appears though: if the pattern doesn't appear, that isn't evidence against their theory. If you look into their theory, you see that it's so poorly defined that it says nothing at all about the likelihood of such patterns appearing. There's obviously a problem here with claiming "my theory is testable", when there is no possibility of failure. This is the main issue with "tests" of string theory or the multiverse.

60. **LM**
March 7, 2016

Peter, I understand your concerns but maybe we are living in a different world today? For example, the Net is full of stuff of all kinds from obvious garbage to brilliant ideas and insights. It's all there, nobody can possibly falsify it all because new stuff just comes out faster than any capacity for policing.
Where's the answer? How do we deal with it? With openness and collective memory (e.g. rating). As long as test is clearly declared, and it fits the clear and precise scientific criteria a theory must declare its results, openly for everybody to see and make their own judgement. A theory with a hundred unconfirmed tests (and these need to be new tests, specific to purported new knowledge) would still have a rating of zero, and no talks and speculation could change that for anybody with a capacity for rational judgement. Could it be enough – I don't know just thinking of some way forward. And for already mentioned reason, I wouldn't even call an idea, concept, hypothesis a "theory" until it comes up with a set of tests that fit the criteria of science.

61. **Scott Church**
March 7, 2016

Few people would dispute that science needs to be testable in the sense of accounting for observation. That's not the problem here. The real question is… *to what degree* do they do so? It's not enough to come up with a model that explains things (what Sean Carroll and others have dubbed the new "Empiricism"), nor is it enough for it to be "elegant" or "beautiful"… the "my wife is hotter than yours" approach. To be testable in any sense that matters scientifically a theory must account for observation *specifically,* and *uniquely*.

General relativity predicted the bending of starlight in the gravitational field of the sun, and the perihelion precession of Mercury. These predictions were scientific because they were specific enough to be tested practically to great accuracy, but more importantly, because they were consequences of GR alone. Newtonian physics did not predict either, and could only account for them with ad-hoc contrivances ripe for Occam's razor. It is true that for a while Mercury's perihelion precession could be explained by the gravitation of an unseen planet called "Vulcan", but once the solar system had been mapped thoroughly that explanation went away. There's also the fact that the solar system actually *could* be mapped accurately enough to rule out the Vulcan hypothesis.

String theory and the multiverse are only "testable" in the weak sense of being able to loosely (*very* loosely) account for observation… the way the Vulcan hypothesis once did. What makes them unscientific is that their "predictions" are neither specific enough, nor unique enough to render them potentially favorable over their competitors by any foreseeable test. The ability to hit a barn at 5 paces with a sawed-off shotgun does not make one a marksman, nor does it earn one a sharpshooter medal if a hundred other folks can do the same thing blindfolded.

Who’s Winning the String Wars and Why Should You Care?

March 1, 2016
Categories: Uncategorized

Part two of Gerald Alper’s piece at Smashpipe is now available there, with the title Who’s Winning the String Wars and Why Should You Care?, and some more substantive material than in part one. One of the great things about having a blog is that whenever anyone writes anything about you that you think might not be 100% correct, you can blog about it, and explain yourself ad nauseam. So, here are a few clarifications for readers of that article:

- About the “horrible sentence”

> The Hilbert space of the Wess-Zumino-Witten model is a representation not only of the Kac-Moody group, but the group of conformal ~~representations~~ as well.

I don’t think it’s a bad sentence, it succinctly conveys the main point about the close relationship of the WZW QFT to representation theory. Like a certain number of things in the book though, it’s not intended for everyone. There were certain things I wanted to explain, and the way I went about this was to try to as clearly write them down as possible, in a way accessible to as many people as possible, but well aware that not everyone would understand everything. Unlike writing “the WZW model is related to mathematics like X is to Y”, where X and Y are things most people would recognize, you’re not going to get fooled into believing you understand something you don’t by what I was writing. Those who do understand the sentence will understand a real idea.

- I AM VERY CONFIDENT that I AM RIGHT. No one has ever critiqued string theory with the level of detail that I have.

Not sure exactly how I said this, but I suspect the “no one has ever” wasn’t intended to convey that this was a good thing. I’ve clearly spent too much of my life thinking about this. I also should specify that what I’m confident about is that current “string vacua” models don’t correspond to reality. They’re complicated, ugly, and don’t explain anything. My suspicion is that even Witten might not completely disagree with this, acknowledging that at our current understanding of string theory, there is no convincing model. I think a more accurate way of characterizing where Witten and I disagree here is with how promising it is to pursue this particular vision of unification. I am not at all confident that Witten or someone else pursuing it might not come up with something really new and successful some day. I just think it’s a relatively unpromising route to keep heading down, although I acknowledge it’s possible it might lead to finding a more interesting path. Doubtless Witten feels the same way about things I find more promising.

Comments

1. **Bee**
   March 1, 2016

   I’ve summed up Witten’s thoughts here. 😉

2. **Peter Woit**
   March 1, 2016

   Thanks Bee,
   That’s really wonderful, hadn’t seen it.

   And congratulations on ten years of Backreaction, as well as its new sponsor!

3. **paddy**
   March 1, 2016

   Peter,
   There’s your problem. You lack Ms. Hossenfelders rhythm. And thank you for the link.

4. **paddy**
   March 1, 2016

   Peter,
   Sorry for two posts. After reading the Alber’s article it is my opinion you come off quite well. No need for caveats.

5. **Low Math, Meekly Interacting**
   March 1, 2016

   I hold you blameless, but I hated that “interview”. I learned ten times more about the interviewer than his subject, most of which was irritating. I learned precious little else about science or other scientists, beyond the obsequiousness their high IQs induce in Mr. Alper. Allegedly there were three-plus hours of fascinating conversation. I wish the fascinating parts had actually made it into print.

6. **MC**
   March 2, 2016

   Is there a part three? Because I see no interview in part two.

7. **jim**
   March 2, 2016

   Personally, I read your blog at least partly because you write clearly and honestly.

8. **Chris Oakley**
   March 2, 2016

   I second LMMI’s comment above.

   In regard to the “horrible sentence”, I think the point is just that Not Even Wrong (the book) is not really written for a general audience. It is more a book for people like me who already have a D.Phil. (or Ph.D.) in Elementary Particle Physics. I loved it, but admit that, not knowing anything about the WSW model, I cannot claim to have found the “horrible sentence” much more comprehensible than Alper or Holt did.

Which reminds me: you need to update the non-paywall link to the Robert Matthews review of your book to this: http://www.cgoakley.org/qft/RM%20Strings%20FT.html

9. **Low Math, Meekly Interacting**
March 2, 2016

I’m also mystified by the critiques of your writing (both your own and others’) in this piece. OK, not a lot of flowery prose, but I tend to like it when authors resist the urge to sacrifice clarity and directness to show off their facility with adjectives and metaphors. Better still if they apparently lack the urge altogether. The visual presentation of the blog very much reflects the content, and I think that’s great: uncluttered, efficient, clear. Nothing remotely wrong with that. Nothing “rough-hewn” about it, either. Not being showy or self-indulgent doesn’t make one unrefined, for heavens sakes. It’s just different, and far too rare in online content, IMO.

10. **Bernhard**
March 2, 2016

LMMI,

I second your last comment very much. Peter, many of us that come here like your clear argumentation and sober conscious between what you know and what you do not. Don’t ever change it! 🙂

There is also another thing that’s irritating in this interview which is a an obvious reverence to authority. The whole part on “Witten is the greatest string theorist who ever lived” makes that clear, just to give an example.

Very little content in this interview and too pompous to my own taste.

11. **KJ**
March 2, 2016

The interview and this discussion of it nicely illustrate the difference between a mathematician/scientist and a journalist. When the former communicates, the primary goal is the efficient transfer of meaningful information. To the latter, the story and how it is told take precedence. It is no surprise that most readers of this blog prefer the former.

12. **Chris W.**
March 2, 2016

I was going to say some things about Alper’s style, but others have already expressed my reactions well. I also heartily endorse LMMI’s comments and the follow-ups on them.

13. **Scott Church**
March 2, 2016

I’m currently reading Lee Smolin’s book *The Trouble With Physics* and enjoying it very much. Regarding references to authority (e.g. “Witten is the greatest string theorist who ever lived…”) he tells a story about a talk Alain Connes gave at a symposium in Chicago in 1996. A little ways into the talk a prominent unnamed string theorist got up and left. A couple years later Connes gave the talk again at the Rutherford Laboratory and the same guy was there, but this time he sat through the whole thing with rapt attention and praised it later in his own talk. Shortly thereafter Connes ended up sitting next to him on a bus. He asked him point-blank… “How can it be that you attended the same talk in Chicago and you left before the end, and now you really liked it?” The guy’s answer was… “Witten was seen reading your book in the library at Princeton…!”

According to Smolin, this sort of thing is not uncommon and he had many similar anecdotes. As an outsider I’m in no position to comment–certainly not as well as Peter and many others here. But if he’s right, the string theory community is on the verge of becoming a mega-church with Witten and one or two others as it’s high priests. To be fair, Smolin also said that this sort of thing is staring to change and he’s starting to see string theorists willing to admit that their field is “in crisis”. But even so, this strikes me as beyond troubling.

14. **Peter Woit**
March 2, 2016

Scott Church,
That story about Witten is well-known, but a couple comments

1. There are far worse reasons for getting interested in something than finding out that Witten was interested. Paying some attention to what the best people find interesting isn’t a bad idea at all.

2. Things have changed quite a lot, and it has been a long time since Witten’s enthusiasm for something has had much of an effect. The fact that Witten was working on geometric Langlands for quite a while didn’t seem to cause many physicists to get interested. Arguably these days physicists aren’t paying enough attention to his enthusiasms…

15. **Jeffrey M**
March 2, 2016

Scott and Peter,

Well, first, I think Peter is right, it’s a good idea to pay attention to what the best people in your field are interested in. That said, I’ve never had a satisfactory explanation of why physicists are so enthralled with Witten. He’s clearly a brilliant mathematician, there’s a reason he won the Fields. My orals in grad school were to present a paper of his which was published in JDG. I spent several weeks trying desperately to understand it, finally went to one of my committee members, who pointed me to a book by John Roe which was essentially the paper, explained in much more detail. Beautiful beautiful stuff. But I’ve never figured out what physics result, confirmed by experiment, is Witten’s. He can’t be loved so much just for string theory? I think he was a postdoc at Princeton when I was there in the late 70’s, what was he doing then? I remember people talking about him (I was in physics back then, ended up in math after I transferred to Hampshire).

16. **Scott Church**
March 2, 2016

Thanks Peter… that’s good news! Smolin did make a point of emphasizing what a great physicist Witten is and how much he’s brought to the field (as though anyone would question that). I guess what worried me was that this wasn’t the only such anecdote he told, and the overall picture he paints is one of the string community treating

Witten's interests not as a matter of expertise, but as authoritative… Witten is interested, so it must be *true*. If that's changed somewhat since the book came out (2007), wonderful! We're not going to get very far if we start treating scientists as authorities rather than experts. 🙂

17. **Peter Woit**
March 2, 2016

Jeffrey M,
Witten came to Princeton in 1980 (he had been a grad student there mid-70s). His reputation in physics isn't really built on string theory: he was by far the most respected figure in the field in 1984, before string theory hit (his decision to take it up was influential for that reason). My popular book has a chapter where I try to explain what he was doing pre-string theory, why it was important both as math and physics.

Witten really was just born too late. Basically there have been no Nobel prizes for theoretical developments post his grad student days, so you can't hold it against him that he doesn't have one.

18. **Jeff M**
March 2, 2016

Peter

I wasn't holding against him the lack of a Nobel prize. We math types disdain the Nobel 🙂 I just never had anyone talk about what he did in physics. Anyway, as I said, he's certainly a brilliant mathematician, or he was. Funny I thought he was at Princeton when I was there, maybe I remember people talking about him from when he was a grad student and how great he was. By 1980 I was at Hampshire…

19. **curious**
March 2, 2016

My suspicion is that even Witten might not completely disagree with this, acknowledging that at our current understanding of string theory, there is no convincing model. I think a more accurate way of characterizing where Witten and I disagree here is with how promising it is to pursue this particular vision of unification. I am not at all confident that Witten or someone else pursuing it might not come up with something really new and successful some day. I just think it's a relatively unpromising route to keep heading down, although I acknowledge it's possible it might lead to finding a more interesting path. Doubtless Witten feels the same way about things I find more promising.
^

So uh have you ever spoken to Ed Witten about this?

20. **Peter Woit**
March 2, 2016

curious,

No. But I think there's a reason he doesn't these days work on "string phenomenology" models. If he thought there was a convincing model, I think he would be working on that.

21. **Justin**
March 2, 2016

"Doubtless Witten feels the same way about things I find more promising."

By this you mean he "does" or "does not" feel that these other things are promising?

22. **Luca Signorelli**
March 3, 2016

Peter,
I'm no scientists, but a former professional journalist and editor of a major magazine in Italy. And this interview is dreadful. Unprofessional, badly written, confused and tedious. As others have noted, the author is so enamored of himself he forgets he was supposed to write about you. A hack job if I've ever seen one, which I've wouldn't ever allowed to print had I've been the editor. Sad (albeit hardly your fault)

23. **Thomas Larsson**
March 3, 2016

Actually, I think the sentence is a bit horrible, but for a different reason and because I have struggled a lot to formulate sentences like that myself. I would say that the Hilbert space carries a representation, or that the representation acts on the Hilbert space, or that the Hilbert space is a module for the Kac-Moody group, thinking of a representation as matrices and of a module as the vector space on which they act.

24. **Peter Woit**
March 3, 2016

Justin,
My guess is that he doesn't find promising things that I do, again, with the best evidence that he doesn't work on them.

25. **Urs Schreiber**
March 3, 2016

Or he thinks he has done the math-phys that was his job to do, setting up the right theory, and others are to work out the phenomenology.

In the recent words on Gordon Kane,

at 21:23 in https://videoonline.edu.lmu.de/en/node/7485 and referring to Acharya-Witten, Witten-Atiya and Witten's work on [M-theory on G2-manifolds](#):

> Witten sort of took the attitude: well, the whole thing is set up, we are done, this is the right answer. He went off and did mathematics after that. But this is where we began.

26. **Peter Woit**
March 3, 2016

Urs,
In the same talk Gross took the unusual step of intervening and pointing out that almost no one in the theory community believes Kane's claims about using this to make predictions. I strongly suspect Witten would agree with him.

Witten's Ph.D. thesis was on phenomenology and he's written many papers over the years on phenomenology, not mathematics. I see no reason to believe that he believes in the kinds of claims Kane is making, but just doesn't want to work on any of that himself, preferring to stick to things like Khovanov homology.

27. **John Baez**
March 7, 2016

"The Hilbert space of the Wess-Zumino-Witten model is a representation not only of the Kac-Moody group, but the group of conformal representations as well."

This sentence would be less horrible if it said "group of conformal transformations". That, at least, is a thing!

I see that in your book you indeed said "transformations", not "representations". Whew!

So where did the typo first creep in? I see it's in Alper's *description* of Holt's complaint.

28. **Peter Woit**
March 7, 2016

Thanks for pointing that out John!

I'm a bit disturbed that I read that and repeated it on the blog without noticing the problem…

Checking, it looks like the typo is Alper's. I realize that I wrote about it here
http://www.math.columbia.edu/~woit/wordpress/?p=464
and way back then the typo wasn't there.

29. **lucy m**
March 8, 2016

I read because I see a man with integrity and a cause I believe in. The fact he's stuck to it, all these years through good times and bad. It was never going to win friends or boost his career, much nearer the reverse than that. I just feel so damn rare in our time. Also he virtually never publishes my comments and Groucho Marx yes sir!

30. **Daniel Kagan**
March 23, 2016

Wow is this interview bad. Why does the author spend so much time talking about a review of Peter's book, rather than the book itself? This article illustrates the differences between bad journalism and good journalism, rather than the difference between scientists and journalists.

Michael Atiyah's Imaginative State of Mind

March 3, 2016
Categories: Uncategorized

Quanta magazine has an intriguing article by Siobhan Roberts out about Michael Atiyah, and what he's up to these days. It mentions some new ideas about twistor theory I hadn't heard about, that emerged from a conversation with Penrose, which Penrose wrote up as Palatial twistor theory and the twistor googly problem. Penrose explains the name:

> The majestic ambiance of the unusual location (Buckingham Palace) of a brief discussion with Atiyah, no doubt provided inspiration for the initial thought that non-commutative twistor algebra should be the key to those subsequent developments described in this paper.

The Quanta article explains:

> One day in the spring of 2013, for instance, as he sat in the Queen's Gallery at Buckingham Palace awaiting the annual Order of Merit luncheon with Elizabeth II, Sir Michael made a match for his lifelong friend and colleague, Sir Roger Penrose, the great mathematical physicist.
>
> Penrose had been trying to develop his "twistor" theory, a path toward quantum gravity that's been in the works for nearly 50 years. "I had a way of doing it which meant going out to infinity," Penrose said, "and trying to solve a problem out there, and then coming back again." He thought there must be a simpler way. And right then and there Atiyah put his finger on it, suggesting Penrose make use of a type of "noncommutative algebra."
>
> "I thought, 'Oh, my God,'" Penrose said. "Because I knew there was this noncommutative algebra which had been sitting there all this time in twistor theory. But I hadn't thought of using it in this particular way. Some people might have just said, 'That won't work.' But Michael could immediately see that there was a way in which you could make it work, and exactly the right thing to do." Given the venue where Atiyah made the suggestion, Penrose dubbed his improved idea "palatial twistor theory."

The article links to this recent talk about the role of beauty in mathematics, and describes some very speculative ideas he's been working on, which I guess correspond to for instance this paper.

About this kind of work he has this to say:

> If you try to direct science, you only get people going in the direction you told them to go. All of science comes from people noticing interesting side paths. You've got to have a very flexible approach to exploration and allow different people to try different things. Which is difficult, because unless you jump on the bandwagon, you don't get a job.
>
> Worrying about your future, you have to stay in line. That's the worst thing about modern science. Fortunately, when you get to my age, you don't need to bother about that. I can say what I like.

When asked if he's risking his reputation this way, he has this sensible response:

> My reputation is established as a mathematician. If I make a mess of it now, people will say, "All right, he was a good mathematician, but at the end of his life he lost his marbles."
>
> A friend of mine, John Polkinghorne, left physics just as I was going in; he went into the church and became a theologian. We had a discussion on my 80th birthday and he said to me, "You've got nothing to lose; you just go ahead and think what you think." And that's what I've been doing. I've got all the medals I need. What could I lose? So that's why I'm prepared to take a gamble that a young researcher wouldn't be prepared to take.

Update: For an alternate source of information about "palatial twistor theory", see slides here, video here.

Comments

1. **wam**
   March 4, 2016

   Whenever possible, could you link to non paywalled
   versions of articles. Some of your readers may not have
   an academic affiliation that allows them access.

2. **Peter Woit**
   March 4, 2016

   wam,
   I generally do try to do that. In this case, I don't know of any other source for Penrose's "palatial twistor" paper, if anyone else does, please let us know.

3. **Dom**
   March 4, 2016

   Is this it? Or a similar paper "Towards an Objective Physics of Bell Non-Locality: Palatial Twistor Theory"
   Downloads the PDF from http://www.ijqf.org

4. **Peter Woit**
   March 4, 2016

   Dom,
   Thanks. That's similar, not the same, refers for details to the other one.

5. **David Roberts**
   March 6, 2016

   There is no non-paywall version that I could find. Heck, my library, I found, only subscribes to the Proceedings of the RS via a two-year moving paywall, which means I will only be able to read this article in 2017!

6. **Peter Woit**
March 6, 2016

David Roberts (and others),

I've added a couple links, to a set of slides and a video of Penrose lecturing on this. The drawings in the slides may in any case be more helpful than the graphic-free paper.

7. **wam**
March 8, 2016

Thanks for links. 'In person' is invariably better than 'on paper'. In RP's case it makes a big difference – how else to understand his prelapsarian graphics. How he fits it all onto a transparency using magic markers is somewhat miraculous. For other, possibly more primitive examples look into the back issues of Twistor Newsletter.

8. **anon**
March 9, 2016

Can you actually make some critical comments about "twister theory" as you do "string theory", please? Just quoting Penrose on twistors is like blogging quotes from Witten on string theory.

I realise that the amount of existing hype for each is vastly different, so that it is justified to treat twistors as news, rather than hype, but what about the connections of the 2 spinors in twistors with the usual standard model spinors?

9. **Peter Woit**
March 9, 2016

anon,
"twistor theory" isn't a subject that has led to any specific well-defined physical theory, but I think it's very interesting in the way that it shows that one can study 4d geometry in a way that builds in spinors and conformal invariance from the beginning. It has always seemed to me that you need some new ideas before building qfts based on this kind of geometry. Arkani-Hamed several years back was claiming a new approach to qft based on these variables, but what has come out of that (theories based on amplitudes, defined in terms of volumes of geometrical objects) leads to interesting things, but nothing that seems to me to answer questions about the SM and its relation to gravity.

The reason for bringing this up in the blog post was the claim by Atiyah and Penrose that there's a new idea here. I've spent a little time looking at it, still don't understand well enough to see if it goes somewhere. The sort of quantization Penrose is trying to do always has confused me, because it seems to be purely quantum mechanical, whereas I'd like to see what this does for QFT. Thinking more deeply about this (and whether the "palatial twistors" help) seems worthwhile, I haven't had the time recently to do so.

10. **manyoso**
March 9, 2016

Peter,

Speaking of Arkani-Hamed and his Amplituhedron work, if I remember correctly that was tied rather explicitly to N=4 supersymmetric Yang-Mills theory. I gather that it also borrowed heavily from Penrose's twistor theory, but I wonder whether these new "palatial" ideas either implicitly or explicitly depend upon supersymmetric theories?

Also, given the rather disheartening news for SS theories now that the LHC has significantly constrained the parameter space, is Arkani-Hamed working on something else? All I can find from him lately seems to be "motivational" talks and papers on the way forward for particle theory and under consideration next generation colliders.

11. **Abbyyorker**
March 9, 2016

Very interesting article on M.Atiyah. He says he is going back reading Einstein and Dirac and finding stuff people missed. It would be great to see what he has found.

12. **Peter Woit**
March 9, 2016

Manyoso,
For something very recent from him, see
https://www.youtube.com/watch?v=1HRdqNcgOOo
Some motivational stuff about colliders, etc, but also a long section on the latest on amplitudes. There's the usual story about motivating work on amplitudes by hopes for finding a common origin of spacetime and QM, but I don't see any particular progress towards that.

Three may be one, but I don't know of any relation of the "palatial" stuff to SUSY.

13. **David Roberts**
March 9, 2016

If people want to skip to the 'new' material in Penrose's talk, for instance if they are familiar with twistors, then jump to 45 minutes in (yes, I had to watch all that to find the actual point). He then says that palatial twistor theory is not any sort of quantum gravity, but then proposes another deformation of the algebra to make more commutators nonzero—he explicitly calls it a 'crazy new proposal'—to maybe be able to do something. The comments Penrose makes in the question time are some of the best bits.

Multiverse Observed, South of Glasgow

March 4, 2016
Categories: Multiverse Mania

It turns out the multiverse does exist, just off the A76, 25 miles north of Dumfries in Scotland. It's called the Crawick Multiverse and is now open 10 am to 4 pm. Admission is 5 pounds, but parking is free.

For more details, see this story, which explains about the huge mounds, and that

> There's also a mound where mudstone slabs trace a spiral path up to the top that represents the multiverse. Along the way, some of the slabs are carved out to symbolise other potential universes where different physical laws apply.

The idea seems to be that

> the park is a modern take on Neolithic monuments such as Stonehenge, which paid tribute to the movements of the Solar System – but this time the focus is on the latest advances in physics, such as chaos theory and the idea of parallel universes.

so while Stonehenge was designed to last forever and show that a primitive people understood about the solar system, this is going to show our descendants that 21st century humans knew about the multiverse.

Since it is well known that the LHC is our best chance of figuring out if multiverses exist, it seems that the people at CERN have agreed to have the same person build something like this there. This should ensure that when any future generations excavate the LHC site, they will get some idea of its purpose, and marvel at how much progress the human race made after the Neolithic era.

Update: Another observation of a multiverse, this one in Trieste.

Comments

1. **Chris Oakley**
   March 4, 2016

   Stonehenge is obviously the remnants of a neolithic gas holder and has nothing to do with astronomy. The metal has long since rusted away, but one can see that the raised stone ring would have supported an enormous tank, supplying, I expect, most of the south of England (in this universe, at least: I cannot speak for other universes).

2. **Jon Awbrey**
   March 4, 2016

   No one expects the entanglish exposition.

3. **John**
   March 4, 2016

   PT Barnum had it right oh so many years ago.

4. **Bayes_or_bust**
   March 4, 2016

   I know you're mocking it, but I actually like the look of it! I like massive land art and sculpture parks. It goes without saying that a multiverse would profoundly affect the way we think about ourselves, and it's cool to explore that through art. This is one spin-off from the multiverse we can enjoy.

5. **Bernhard**
   March 5, 2016

   Bayes_or_bust,

   Maybe we should be even more radical and try to explore the multiverse exclusively through art.

6. **David Bailey**
   March 5, 2016

   I think that when science starts to ditch experimental proof, and rely on beauty, it merges with art and becomes understood more as a metaphor than a statement about reality.

   I hope this enterprise does well and even features on cosmology TV programs, because (probably unintentionally) it might force science to contemplate just where it is heading.

   I don't think the rot in science is confined to HEP.

7. **Staffan Angere**
   March 5, 2016

   Benhard,

   that is probably the most reasonable way to proceed. After all, the word "multiverse", with roughly its contemporary meaning, was coined by Michael Moorcock in his fantasy and science fiction stories (the word appears in William James, but with a very different meaning).

8. **vmarko**
   March 5, 2016

   Now we just need to propose the idea of a "nulliverse" – a situation where only zero universes exist, and we will have a complete offer for any metaphysical ideology:

nulli-, uni- and multi-verses. So far people who align with solipsism were underrepresented in modern cosmology, but nulliverse could change all that for the better.

Also, aside from the fact that it doesn't need any experimental evidence whatsoever (whatever the argument others offer, just claim it's all your imagination and the real world doesn't actually exist), it would be trivially cheap and easy to build a theme park for the nulliverse. Just so that future generations know that we didn't leave solipsists out in the cold in modern cosmology…

🙂
Marko

9. **Anon**
March 5, 2016

As reality goes
which one is most terse:
a nulli-, a uni-, or a multi-verse?

Or why not a poly-
If that be our curse
For a poly-multi-verse can't be much worse…

10. **Lucy M**
March 5, 2016

Hi Anon, I quite like the idea of the multiverse, megaverse, multi-level-multiverse bubbleverse, and so on, all as merely parochial regions of the perverse

11. **Yatima**
March 5, 2016

The satanic multiverses cannot be far off.

12. **Jon Awbrey**
March 5, 2016

I have done the calculations and compute that the probability of a universe with bagpipes is vanishingly small, so we are clearly living in an outlier.

13. **paddy**
March 6, 2016

So why, Jon Awbrey, are Glaswegians seen typically as aliens?

14. **MC**
March 7, 2016

Everybody knows that the real universe is the one with the City of Amber at its center. The others are just shadows…

15. **Jim Given**
March 8, 2016

"Believe me, if you've seen one multiverse, you've seen them all."

16. **Jon Awbrey**
March 8, 2016

Re: Paddy

I suspect it has something to do with alien abduction.

17. **paddy**
March 8, 2016

Jon Awbrey, 🙂 though tread carefully should ever visit Glasgow.

This and That

March 9, 2016
Categories: Uncategorized

A few short items:

- Nature has an editorial this week summarizing the situation with the 750 GeV possible diphoton bump. It mentions a new paper analyzing related data (the number of theory papers on this as a function of time). The paper is called A Theory of Ambulance Chasing, and claims that looking at a large collection of similar fads producing theory papers, the high-level behavior of the HEP theory community can be well summarized by a model that requires only two parameters to fit the data.
- If you're at Stanford tomorrow and a fan of multiverse mania, you can go hear Alexander Vilenkin talk about The Universes Beyond the Horizon. According to the Stanford PR for this

  > Despite the similarities between Vilenkin's theory and the Wikipedia summary of the film Interstellar, many scientists have hope for the multiverse theory.

  Stanford physics faculty members seem to have innovative ideas about the scientific method, with one of them quoted as claiming

  > Once a reasonable idea comes, you can never say it's wrong.

  which I guess could be taken as some sort of motto for research into string theory and the multiverse.

- CERN is running a series of articles about the Theory group there, first one is here.
- Norbert Bodendorfer has a nice new blog about loop quantum gravity and related topics.
- The Templeton Foundation has mercifully stopped giving huge financial prizes to people for dubious attempts to bring religion and science together. I hadn't even realized they had already given out this year's Templeton Prize, which went to a British Rabbi.

  Among the many things they fund is a recent $1.1 million grant for this project on the philosophical implications of quantum gravity. They will hold a summer school this year (with Amanda Peet and Carlo Rovelli, that should be fun), and there's a Youtube channel.

- Chris Quigg has an interesting overview of the future of HEP physics. I particularly like his emphasis on the questions

  > How are we prisoners of conventional thinking?

  and

  > Might we have misunderstood the hierarchy problem, and so need to reframe it? Perhaps it is time to ask whether the unreasonable effectiveness of the standard model (to borrow a turn of phrase from Eugene Wigner) is itself a deep clue to what lies beyond.

Comments

1. **M**
   March 10, 2016

   An initial burst of interest followed by a decline is typical real discoveries (like the Higgs and neutrino oscillations) as well as of anomalies that go away. So I don't see any real content in the chasing-ambulance paper, apart for the provocation. Its author would become famous if the 750 GeV anomaly will turn our to be real.

2. **CM**
   March 10, 2016

   The Nature editorial was worth reading. However, there was one sentence that is hyperbole at best
   "Still, people at CERN, the European particle-physics lab that hosts the LHC, have scarcely talked about anything else since. "
   Although I am not personally at CERN but work remotely on one of the two large experiments, all the people I converse with have been intently focused on getting ready for the start of data taking in a few weeks. Discussions about the 750 GeV diphoton bump might constitute a pleasant diversion during work breaks, but there are usually more important or interesting things to occupy even break time.

3. **Bayes_or_bust**
   March 10, 2016

   @M The goal of The Theory of Ambulance Chasing is modelling the behaviour of physicists (specifically the number of papers they make) after an anomaly is announced. I don't think there's any suggestion (in the paper or elsewhere) that one should attempt to infer whether an anomaly is genuine or a fluctuation from any such analysis of the dynamics of the number of papers written about it.

4. **Low Math, Meekly Interacting**
   March 11, 2016

   "Many of these universes collapsed and formed black holes, Vilenkin said. If the black holes are big enough, they may have inflating universes inside of them, and these expanding universes would be connected to the visible universe by wormholes."

   …and? I take another hit off the bong and get my mind blown, or we observe some consequence of this?

5. **my milkshake brings all the boys to the yard**
   March 14, 2016

   Hi Peter. well, it seems Neil Turok agrees with you about string theory: "Instead it's given us a huge collection of theories where, if you like, there's no overarching theory to tell which particular version of string theory is the one that describes the world. It's almost self-destructed…" etc. etc. (Though I disagree myself.)

   Turok also asserts (in the video at least) that the universe is simple. That is either an article of shear faith or ignorance of our current theories (e.g. QCD). He also asserts (in the text) "Our most secure knowledge about the world, about the natural world, is physics." That is hopelessly naive and flat out wrong: has he never experienced the greenness of green (i.e. qualia?) He doesn't give me much confidence that he knows what he's talking about at all.

   Then (in the text) he talks about the founding principles. Well, okay.

http://www.cbc.ca/radio/ideas/the-ultimate-simplicity-of-everything-1.3490322

6. **Peter Woit**
   March 14, 2016

   Thanks MMBATBTTY, I hadn't seen that. It deserves a separate posting…

   At this point I don't think my point of view about string theory is an unusual one at all, not surprised to hear what Turok has to say. He obviously is not ignorant about QCD, which, yes, is from a fundamental point of view extremely simple (in the limit of massless quarks, no free parameters at all (except maybe the number of colors).

7. **James Mc**
   March 16, 2016

   Hi Peter you might be interested to know Andrew Wiles was awarded to the 2016 Abel Prize

The Ultimate Simplicity of Everything

March 14, 2016
Categories: Uncategorized

There's a wonderful interview with Perimeter Institute director Neil Turok here, entitled *The Ultimate Simplicity of Everything*, and done for a Canadian radio program.

Turok discusses his point of view on whether we're at "the end of physics", and I'm very much in agreement with what he has to say:

> I think what people are sort of expressing is that we haven't had a big revolution in physics. String theory was hoped for to be that revolution in the 1980s but it hasn't really panned out in the sense that it hasn't given a single prediction. Instead it's given us a huge collection of theories where, if you like, there's no overarching theory to tell which particular version of string theory is the one that describes the world. It's almost self-destructed, I would say because it turned out to be not just one theory but this vast collection of theories which could all give different descriptions of the world.
>
> So I think that sort of theoretical catastrophe, as I view it — meaning the logical pursuit of quantum mechanics and relativity over a hundred years was tremendously successful at some level but finding its own successor theory, it hasn't been successful. I think that is also laying the ground for some sort of revolutionary change in the sense that we basically will have to go back to the founding principles. It looks like the founding principles of modern physics — quantum theory and relativity — have played out and they have not given us the answers we need. And so we have to go back and question those founding principles and find whatever it is, whatever new principle will replace them. So matching these great puzzles posed by the observations are equally great puzzles in our fundamental theories. And so that is just a wonderful thing to contemplate in itself. I mean, partly people become very pessimistic and say, oh my god, I've devoted 50 years of my life to studying this incredibly technical and difficult theory and now I find it's blown up in my face, it's not giving any predictions at all… and so some people talk about the multiverse where the universe would be wild and chaotic on large scales and almost anything you could imagine would actually exist somewhere in the universe. I mean, this is literally a scenario which became very popular among a category of physicists, that there is a multiverse out there. Yet the evidence is exactly the opposite. That, as we look around us, things could not be simpler. There's no evidence for chaos on large scales in the universe. It's totally the opposite. It's pristine, elegance, minimalism is all we see. So, I think this is a very, very exciting time to be doing theory. The challenge is enormous. The clues are enormous. We're waiting and we're preparing and we're encouraging people to take radical leaps.

Comments

1. **anon.**
   March 15, 2016

   "The chance is high that the truth lies in the fashionable direction. But, on the off chance that it is in another direction – a direction obvious from an unfashionable view of field theory – who will find it? Only someone who has sacrificed himself by teaching himself quantum electrodynamics from a peculiar and unfashionable point of view; one that he may have to invent for himself." – Feynman's Nobel prize lecture, 11 December 1965.

2. **Giotis**
   March 15, 2016

   "It's almost self-destructed, I would say because it turned out to be not just one theory but this vast collection of theories which could all give different descriptions of the world."

   Obviously Turok as most of String theory denialists is mixing the notions of a theory and the solutions (or vacua if you like) of a theory.

   The theory is one and unique, the solutions are maybe many.

3. **vmarko**
   March 15, 2016

   Giotis,

   "Obviously Turok as most of String theory denialists is mixing the notions of a theory and the solutions (or vacua if you like) of a theory."

   So you would be comfortable putting David Gross and Gordon Kane in the ST-denialists category? Because they also say that ST is a framework, and only once you choose a particular vacuum you fix one particular theory. In that theory you can then fix various boundary conditions that specify one particular solution or another.

   The way I understood ST is that the choice of the vacuum is not a boundary condition within a theory, but the choice of the theory within a framework, just like you can choose the gauge group SU(3)xSU(2)xU(1) within the QFT framework to get one particular theory (the SM).

   Btw, this isn't my analogy. David Gross was very explicit about this during the Munich conference, and I've also heard a bunch of ST experts make similar statements for years now. I don't think Turok has anything mixed up here.

   🙂
   Marko

4. **Peter Morgan**
   March 15, 2016

   "There's no evidence for chaos on large scales in the universe." OK, but if Physics is only interested in scales where there is simplicity, the "large" ones, what's all this other stuff? Fortunately for those who are willing to get their hands dirty there is condensed matter physics, biophysics, etc. http://www.forbes.com/sites/chadorzel/2016/03/13/why-isnt-the-biggest-conference-in-physics-more-popular/#6d10d6f77594 makes a worthwhile case at least for the CM.

   Without detracting from the utility and intellectual interest of effective simple models, it's not necessarily that the world is simple, it can equally well be that we sift the world for simplicity; it might be inadvisable to be in denial about the dross.

5. **Andrew Thonad**
   March 15, 2016

   Giotis: "String theory denialists". You make people who disagree with string theory sound like the flat Earth society. People who disagree with string theory aren't denialists because it is not a proven theory. I'm afraid you're the one who comes across with a head-in-the-sand worldview.

Peter Morgan, yes, Neil Turok says we live in the "messy middle". So there's plenty of complexity yet to be understood in the middle scales. In fact, that's probably going to be more of a challenge in the long run.

6. **Cormac O'Raifeartaigh**
   March 15, 2016

   The statement "Yet the evidence is exactly the opposite. That, as we look around us, things could not be simpler. There's no evidence for chaos on large scales in the universe" doesn't feel quite right to me.
   My understanding is that the one point that hypotheses such as the multiverse bring to the table is to highlight what may be an unjustifed assumption – i.e., we cannot at this point be sure that what we measure locally is an accurate representation of what may exist on larger scales.
   I hope that ssomeone will one day prove that cosmic inflation happened to all of the universe, or none. But until that time, it seems it is logical to leave open the possibility of disconnected regions…however distasteful we find the idea …

7. **Chris W.**
   March 15, 2016

   Cormac,
   Frankly, I think it's about time we made some unjustified assumptions *that can be tested*. In science, hedging bets can be toxic.

   Why don't we try assuming that certain things are simpler than we think they have any right to be, and then—at worst—find out exactly how and why that turns out to be wrong? If the multiverse forces itself on our attention because of unexpected *observations* that would be a tremendous improvement over the current situation.

   Of course, for that to happen we will almost certainly need a much better formulation of the idea than we have now.

8. **vskrin**
   March 17, 2016

   Apropos the above discussion, and Turok's statement,

   "…and so some people talk about the multiverse where the universe would be wild and chaotic on large scales and almost anything you could imagine would actually exist somewhere in the universe. […] There's no evidence for chaos on large scales in the universe. It's totally the opposite. It's pristine, elegance, minimalism is all we see.",

   I believe that this part of a Feynman's lecture is very relevant: https://youtu.be/-Km7-6-J81k?t=18m20s .

   Feynman speaks about a slightly different issue, also related to anthropics, but the way I see it his argument is equivalent to Turok's argument against the multiverse.

   Cheers!

9. **Jim Akerlund**
   March 18, 2016

   vrskin,

   Thanks for the the link to the Youtube Feynman lecture videos. Wow.

10. **Low Math, Meekly Interacting**
    March 19, 2016

    We may get some new sense of how messy the middle really is by summer. Jester tells us the 750 GeV bump is…not dead yet. In fact…no, I don't want to get my hopes up!

Number Theory News

March 16, 2016
Categories: Uncategorized

- Andrew Wiles is the recipient of this year's Abel Prize. I have to confess that I found this surprising, since I assumed he'd already won this. His work in general and specifically the work that led to the proof of Fermat's Last Theorem is on any reasonable list of the top few achievements in mathematics in recent decades.

  If you haven't seen the documentary about the FLT proof, you really should, it was a BBC Horizon show in the UK, Nova here in the US, transcript here.

  I'd heard and Nature confirms that Wiles has for quite a while now been working quietly on the BSD Conjecture, maybe some day there will be another very dramatic moment in the subject, and another documentary.

- Erica Klarreich at Quanta has the story of a surprising new result about prime numbers from Kannan Soundararajan and Rober Lemke Oliver. They have found that, given a prime number with a certain last digit, there are different probability for the last digit of the next one (among the various possibilities). This violates usual assumptions that such things are in some sense "random", indicating just how subtle this "randomness" is.

  For more details, there's an excellent blog post from Terry Tao. This might be a good time to point out that people sometimes complain about the quality of coverage of scientific advances aimed at non-experts. From what I've seen in recent years, the coverage of mathematics advances has been of extremely high quality, with this story a good example.

- April 29 is the release date for The Man Who Knew Infinity, a film about the life of Ramanujan. It's based on a great biography and a fascinating story. I hope this turns out better than the similar situation with the film about Turing.

Update: It turns out that an astronomer, Chung-Ming Ko, had already a while ago done some calculations showing non-randomness in the last digits of primes, see here. The new paper has been updated to refer to that.

Reports about the Ramanujan film are that they took great pains to get the mathematics right, with Ken Ono and Manjul Bhargava working extensively on the film.

Comments

1. **Jeff M**
   March 16, 2016

   It should be noted that the result on primes is suggestive and not a proof. Essentially they noticed something numerically, and came up with heuristic arguments, which depend on a conjecture of Hardy and Littlewood, to explain the numerical results. So, might be true, might not be. I'm perfectly willing to believe Terrence Tao, who thinks it makes sense – he's a much better number theorist than I am 🙂

2. **Roger**
   March 16, 2016

   The Abel Prize only started in 2003, so it had a backlog of deserving recipients.

3. **Peter Woit**
   March 16, 2016

   Roger,
   Sure, he's only the 17th recipient ever of the prize. My surprise was just that I would have guessed he'd have been much higher on the list ordered from the beginning of people awarded the prize.

4. **anon**
   March 16, 2016

   I think he's the youngest person to receive the prize thus far. Maybe the committee has prioritized older people when they've started clearing the backlog. Many recipients were in their late 70s or 80s, so they wouldn't have necessarily had that many years left (though I think all but Nash are still alive).

5. **Peter Woit**
   March 16, 2016

   anon,
   I think that's right. I hadn't realized they were still so heavily concentrating on much older people and hadn't yet gotten down to those in their sixties.

6. **geometriclanglander**
   March 16, 2016

   I do feel it's a great shame that Israel Gelfand wasn't awarded the Abel Prize before he died.

7. **Jeffrey M**
   March 17, 2016

   In addition to the age issue, the Abel hasn't been for a single result, it's been for people who've made multiple high level contributions to math in diverse areas. It's not like the Nobel. Wiles was too old for a Fields, but he was given a special citation at the ICM the year he would have won the Fields.

8. **Daryl**
   March 17, 2016

   Hasn't Wiles already proven a special case of BSD conjecture?

9. **Davide Castelvecchi**
   March 18, 2016

I'd like to point to Evelyn Lamb's article as another example of excellent coverage 🙂
http://www.nature.com/news/peculiar-pattern-found-in-random-prime-numbers-1.19550

And I'm glad I'm not the only one who thought that Gelfand should have gotten the Abel.

10. **Mário da Silva**
March 18, 2016

Not only Gel'fand… They probably thought Arnol'd would have lived longer (he died too young for nowadays standards). He did not receive a Fields Medal for mysterious reasons. At least he was awarded the Wolf Prize.

Anyway, there is now that "Breakthrough Prize in Mathematics", but with such a dumb name I doubt it will catch the others in prestige.

11. **Michael Weiss**
March 18, 2016

It is wonderful when the Abel Prize goes to an extraordinary senior mathematician who in his or her younger years did not receive the Fields Medal. A joyous example was John Tate, whose generosity and deep insights were seldom accompanied by a drive to publish. Despite the legendary status of his PhD Thesis at Princeton, this work was not formally published for almost 20 years. Yet scores of landmark publications by others thanked Tate for sharing key insights and relied on methods that he pioneered and conveyed in meetings. Tate's methods also contributed as an essential foundation to Wile's proof of FLT. As an aside, generations of Harvard undergraduates found Tate to be inspiring and approachable, not only in formal courses but also in the dining halls of Adams House and later Dunster House — and even on the basketball court! The prestige of the Abel Prize was enhanced by John Tate's recognition.

12. **JSM**
March 19, 2016

Much as Wiles deserved the prize, this appears to confirm that they are not going to give it to Langlands. If they were going to, this should have been the year.

13. **Bob**
March 20, 2016

JSM, that's just a Langlands conjecture …

14. **Mario**
March 21, 2016

Peter – is there some reason that you did not cover the update of the 750GeV diphoton resonance? You were my source #1 for rumors on the Higgs in 2012. Can't you use some of your connections? 🙂

15. **Peter Woit**
March 21, 2016

Mario,
I just don't have any information or thoughts about that that aren't much better discussed elsewhere. I can only keep telling people to read Jester at Resonaances so often….

I don't think the recent update materially changes the situation. The evidence is marginal, and we should know for sure one way or another later this year (July if you're an optimist). If there really is a state at 750 GeV it will revolutionize the field, which will be quite exciting.

Maybe reliable rumors will appear here, but at the moment it seems others have better sources, and you should look to them for the latest on this.

16. **geometriclanglander**
March 22, 2016

The paper by Ko was published in Chaos, Solitons and Fractals… it's unfortunate really, given that journal's rather chequered reputation. One can't really be too surprised if it wasn't taken seriously.

17. **IM**
March 28, 2016

Peter,
Off-topic, but probably you should mention in a future posting that the second part
of Grothendieck's obituary has come out in the AMS Notices.

18. **Peter Woit**
March 28, 2016

IM,
Thanks for pointing that out. I noticed that the last page of the Grothendieck obituary material in the AMS Notices included a large recruitment ad for the NSA. May be some spinning in his grave going on…

Two Book Reviews

March 28, 2016
Categories: Book Reviews

Blogging has been light here, trying to finish a complete draft of the book I'm working on, this should be done very soon. Here are a couple all-too-short reviews of books with some relation to math or physics.

A Doubter's Almanac

The main character of Ethan Canin's new novel A Doubter's Almanac is a mathematician, one who solves a great problem early on in his career (as a graduate student in Berkeley, then a faculty member at Princeton). It's a beautifully written work, with a remarkably convincing portrayal of a talented young mathematician struggling with a difficult problem and making his way through life. I wouldn't have guessed that anyone who hadn't lived and worked in this kind of environment would be able to describe it so realistically.There are only a couple false notes in the many details of the part of the story set in academia. In particular, I don't think anyone would consider a "subchairmanship" to be much of an inducement, even at Princeton, and they don't give Abel Prizes to young geniuses. Besides getting the details right, the characters come up with some quite insightful remarks about mathematics, including some that deal with the way talent and immersion in a mathematical problem may alienate one from the rest of the humanity.

While I greatly enjoyed the first half of the book, I have to admit that the later part held less interest, turning away from academia to a long story of family relations and the ravages of alcoholism. Not at all an upbeat book, if that's what you're looking for, but I can't think of another novel as good that so deeply engages with some aspects of mathematics and the mathematical life.

Black Hole Blues

Janna Levin's Black Hole Blues has just been published, with excellent timing for anyone who wants to know more about the story of LIGO and its first observation of gravitational waves. The main strength of the book derives from her interviews with some of the people crucial to building LIGO (in particular Kip Thorne and Rainer Weiss). Together with research and other interviews she has put together a rich version of the history of the project and the roles of the three physicists (Drever, Thorne and Weiss) whose vision and dedication made it successful. LIGO has been a very long term project, with its beginnings going back 40 years. It's remarkable that it didn't get abandoned or defunded at some point, with the NSF playing a very important role in supporting the project over many years.

Drever, Thorne and Weiss will likely soon be the recipients of all sorts of well-deserved honors and prizes (I'd bet on this year's Breakthrough Prize and probably the Nobel too). I was sad to learn that this is coming too late for one of them, Ronald Drever, who is ill and suffering from dementia. The physics of LIGO has a bright future, it's great to have the story told of the people who made it happen.

Comments

1. **Richard Séguin**
   March 28, 2016

   Janna Levin was on Wisconsin Public Radio today for an hour. Podcast available here:

   http://www.wpr.org/shows/quest-hear-universe

   I have a copy of A Doubter's Almanac, but found that the smell of the ink or paper made me sneeze, so I'm not sure when I'll get to it. I've never had that problem before.

2. **Richard Gaylord**
   March 29, 2016

   just wanted to note that Levin's book was not published 'with excellent timing' by happenstance but rather because the publisher decided to rush the book into print now rather than at its previously announced Summer or Fall (i forget which) publication date after the detection of gravitational waves was announced. a very good (and an unexpectedly intelligent one, based on my dealings with publishers on four scientific programming books) move by the publisher. also, i recommend Levin's first book which is a nice little scientific memoir but i found her second book, a novel, dealing with Turing and Godel to be unreadable.

3. **tulpoeid**
   March 29, 2016

   Just for the record, have you read "Uncle Petros and Goldbach's Conjecture"? Not tremendously well written, but quite amazing in talking about "the way talent and immersion in a mathematical problem may alienate one from the rest of the humanity" and in engaging "with some aspects of mathematics and the mathematical life".

4. **Dragster**
   March 29, 2016

   I heard Ethan Canin at a book reading. It turns out he has no real background in math or mathematicians at all. After completing the original draft of the book he happened across a local topologist who gave him the math jargon and sociological insights, and he went back and wove these into his text. It then took multiple drafts to get it to sound authentic enough, but not so technically intimidating that the publisher would fear frightening away the general public.

5. **Scott Church**
   March 29, 2016

   The LIGO story has a personal connection for me. My thesis adviser left the University of Washington in 1988 to accept a position with LIGO at Caltech. The last thing he did before leaving was oversee my defense. After handshakes and well-wishes, he hopped in the car with his family and headed off to Pasadena. Today he is the director of LIGO Hanford and his name might appear in Levin's book. 🙂

6. **Sebastian Thaler**
   March 29, 2016

   Thanks for these reviews, Peter. I hope we can look forward to a review from you later this year of Roger Penrose's new book, "Fashion, Faith and Fantasy…", which offers a critique of string theory among other areas of science.

7. **Shantanu**
April 4, 2016

Peter, was sufficient credit also given to Russell Hulse and Joe Taylor in this book
for their discovery of H-T binary pulsar and subsequent observations? After all, its the decay of the orbit of PSR1913B+16 over more than a 20 year period which convinced skeptics that GWs carry away energy and also helped buttress the case for funding of LIGO. After the LIGO announcement, I was surprised to see not even a single interview of Joe Taylor or Russell Hulse.

8. **Peter Woit**
April 4, 2016

Shantanu,
There are a couple pages in the book about Hulse/Taylor and the binary pulsar story.

Short Items

March 30, 2016
Categories: Uncategorized

A few short items:

- Beams are back in the LHC. You can follow what is going on here real-time, or here for details of this year's beam commissioning. Physics runs scheduled to start last week of April.
- There's a wonderful interview with John Baez in two parts, here and here.
- For news of US HEP, take a look at the HEPAP presentations as they appear here. Reports from the LHC experiments are scheduled for April Fool's Day.
- In the last month the omnipresent Nima Arkani-Hamed was giving talks at the David Gross Fest (video here, nice comments about Gross at the beginning, presentation here), a series of lectures in Trieste (see here), and a colloquium talk at Fermilab.

  In the FNAL talk Arkani-Hamed advertises a "Modern S-matrix program", based on recent work on amplitudes. He's a reliable source for what the conventional wisdom is among the most influential people in the field, and he has this to say about the current situation (right at the end of the talk):

  > String theory killed QFT, then QFT killed string theory back, now QFT is king. We're in a situation where most people think QFT is king and string theory a derivative thing in some limits.

  His own opinion is that we need "something else", neither QFT nor string theory, but he doesn't know what it is.

- I seem to have missed this paper on String theory and general methodology (arxiv version here) when it came out quite a few years ago. At the time the authors felt that

  > the majority has not been convinced and they continue to believe that string theory is the right way to go.

  I'm not sure if the authors had any data to back that up, curious if anyone would still make this claim now. Arkani-Hamed seems to think the majority opinion has changed, with QFT killing string theory.

- For some interesting talks at one of the few conferences not featuring Arkani-Hamed, see the Nambu Memorial Symposium.
- I was sorry to hear recently that Rudolf Haag passed away back in January. For a short biography, see here. For an earlier posting linking to an autobiographical piece, see here. His book Local Quantum Physics is well worth reading. For a discussion of perhaps his most famous result, Haag's theorem, see here.
- Multiverse mania shows no signs of slowing down, with a long BBC article here. Despite the length of the article, the author doesn't seem to have been able to locate anyone who could add a note of skepticism amidst the usual thick layers of hype.

Update: A lot of data on recent DOE funding trends is available here and here. From FY 13 – FY 17, theory funding is down %20 at universities, 2% at labs. A bit over $20 million/year is now being spent by the DOE on HEP theory and computational research. For the most recent round of reviews, 23 groups were funded, and of these two were ones not previously funded, with two previously funded turned down.

Comments

1. **edmeasure**
   March 30, 2016

   I really appreciate these short item updates you provide.

2. **Anon**
   March 30, 2016

   It is really impressive the comment of Haag on String Theory: people like Witten can spend time on such stuff but is a pity if an entire young generation (generations?) do it.
   I always thought something like this but this is the first time I read it from a famous physicist. I see also now people whom was hired years ago for building string theory groups and now they do not really do any research of any impact. I think that the hiring departments made "unlucky" bets..

3. **anon**
   March 31, 2016

   These two consecutive sentences in an otherwise very interesting interview with John Baez (see Part 2) sound perhaps a bit ironic:

   "But I'm trying to travel less, because it's bad for the planet.

   You've gained some fame for your "crackpot index".

4. **John Fredsted**
   April 1, 2016

   To me, the two sentences

   1.) "So, I felt the need to alert people and try to dream up strategies to do something. That's why in 2010 I quit work on n-categories and started the Azimuth Project."

   and

   2.) "What I really like is getting out of the US and seeing the rest of the world."

   in Part 2 of the Baez-interview sounds like cognitive dissonance on the part of Baez, although "But I'm trying to travel less, because it's bad for the planet" softens it up a little bit.

   In my opinion, climate change is not a technological challenge, it is first and foremost a mental challenge: without a fundamental change in the very sentiments of our species, our civilisation is already doomed; we have to learn to abstain from opportunities. Although, a lot of words are uttered from people on behalf of the climate and the planet, the actual actions of those very seem people far too often speaks volumes to the opposite.

5. **John Fredsted**
   April 1, 2016

   seem -> same

6. **John Baez**
   April 1, 2016

   John Fredsted – I’m well aware of the cognitive dissonance there. I agree that if we don’t change our habits we’re in trouble. I also have incipient diabetes and still enjoy doughnuts.

7. **John Fredsted**
   April 1, 2016

   John Baez: Maybe I was being too harsh in my criticism; if so, please forgive me. In any case, perhaps it was unreasonable of me to criticise you in third-person. Luckily, though, you were aware of this thread.

8. **John Vastola**
   April 4, 2016

   Professor Arkani-Hamed’s comment is very interesting, but I’m not sure that I completely understand. I’ve heard that, in some sense, different versions of string theory are dual to quantum field theories; if this is true, then understanding string theory can in principle be reduced to understanding quantum field theory. Is this what he is talking about? Or is he simply saying that QFT has been a successful framework for actual (experimentally accessible) physics, whereas string theory has not? Or neither? Clarification from someone who actually knows what they are talking about would be greatly appreciated!

9. **Peter Woit**
   April 4, 2016

   John Vastola,
   I think he’s clearly referring to the duality business, to AdS/CFT. The big problem with string theory has always been “what is string theory, non-perturbatively?” AdS/CFT says that the answer is given by a QFT. So, one lesson would be that what’s fundamental is QFT, which can be defined perturbatively or non-perturbatively. In other words, QFT is more fundamental, because you can define it without reference to string theory (although in a strong coupling limit, string theory may be a good approximate calculational technique), whereas, outside of a perturbation expansion, string theory is defined in terms of a QFT.

Is String Theory Scientific?

April 1, 2016
Categories: Multiverse Mania, Uncategorized

Among the various April Fool's things on the web, the most subtle one I've found is by the people at James Madison University, who are advertising an April 1 event discussing the question of Is String Theory Scientific?

Part of the joke surely is Betteridge's Law or Hinchcliffe's Rule, which assure us that that answer to the question is "No".

Update: Among today's other April Fool's efforts, Kyle Cranmer updates an oldie but goodie, supersplit supersymmetry.

Update: Another April 1 effort, this one an essay on the multiverse by Robert Lawrence Kuhn. Kuhn claims that the majority of cosmologists disagree with George Ellis about the problems with the multiverse, and that Andrei Linde (with Steve Weinberg agreeing with him) represents the consensus viewpoint. Very funny.

Update: INTO THE MULTIVERSE: God's Voice in String Theory is labeled March 31, but surely it too is an April 1 effort.

Comments

1. **Davide Castelvecchi**
   April 1, 2016

   Hi Peter, did you know that supersplit supersymmetry was back?
   http://theoryandpractice.org/2016/04/Supersplit-750/#.Vv3wNGOgq5g

2. **Peter Woit**
   April 1, 2016

   Hi Davide,
   You seem to have missed my update…

3. **Narad**
   April 1, 2016

   JMU did miss an opportunity to site the event in "upstairs room 500" rather than a real meeting venue, although that would play better if there weren't an actual fifth floor (the ballroom).

4. **anon**
   April 2, 2016

   The whole YouTube channel that you linked to in your third update looks like an April 1 effort…

5. **Jim Akerlund**
   April 2, 2016

   It looks like the third update is a serious thing, not in the vane of April Fool's. There is a certain group that does lots of pingbacks to this blog that will be interested in this, if they don't already know about it. The video looks like a new morph of Intelligent Design into Intelligent String Theory. A quote from the video; "Evolution itself, I'm not down with…", occurs at the 23:09 point.

6. **John Baez**
   April 3, 2016

   I reported a new convex polyhedron with regular faces.

   I also reported a proof that there's a Turing machine that computes uncomputable functions. But this is actually *true*, once you insert the fine print (which I have not done here).

Local Debates

April 4, 2016
Categories: Multiverse Mania

I noticed that tomorrow (Tuesday, April 5) evening here in New York City there will be not one, but two debates involving theoretical physicists:

- At 7 pm the American Museum of Natural History will host the 2016 Asimov Debate, with this year the topic Is the Universe a Simulation?. You can watch a livestream at that site.

  I confess that if this were a few days earlier, I would be convinced it was definitely a joke. But, it seems not, that instead this “has become a serious line of theoretical and experimental investigation among physicists, astrophysicists, and philosophers” and that it’s a “provocative and revolutionary idea”. One thing this is not is new. Nearly nine years ago it got a lot of media attention, and I wrote about it here (and here, where quite possibly my Message to Our Overlords kept them from turning us off). Sadly, the “blink” feature of html no longer seems to be supported, so the red text there won’t blink. Maybe it annoyed the overlords and they had it turned off.

- Much further downtown, at the New York Academy of Sciences, at the same time there will be a panel discussion on a much more sensible and interesting topic What Does the Future Hold for Physics: Is There a Limit to Human Knowledge?. Also at 7 pm, livestream here.

  If I’d been asked (actually I was asked, and then unasked, a rather mystifying situation) for my views on this, I’d make the point that there’s no way to know what the limits will be to human understanding of physical laws. It has however become all too clear what the danger is of what will happen when we reach those limits. Instead of prominent theorists frankly admitting “we don’t know”, there will be an attempt to sell the story to the public that theorists have a wonderful, successful theory which describes everything, which sadly has the unfortunate feature of not making any falsifiable predictions. The string landscape/multiverse scenario now is being very aggressively sold as exactly this kind of endpoint to physics, to a large degree by people unwilling to admit the failure of string theory-based unification. There’s a very real danger that this will enter the textbooks, and that we will in our lifetimes see the end of fundamental physics as a human endeavor. The limit we will have hit will be due not to the nature of our minds, but instead the nature of our sociology.

  I suppose one other way of seeing if we’ve reached the end of physics would be if physicists started spending their time debating things like whether we live in a simulation. Oh, wait…

Update: At the NYAS evidently there was some discussion of the multiverse, with the audience told “The multiverse hypothesis is no more speculative than the universe hypothesis”.

Update: Clara Moskowitz at Scientific American has a report from the AMNH debate. At least there is one participant I agree with:

> And the statistical argument that most minds in the future will turn out to be artificial rather than biological is also not a given, said Lisa Randall, a theoretical physicist at Harvard University. “It’s just not based on well-defined probabilities. The argument says you’d have lots of things that want to simulate us. I actually have a problem with that. We mostly are interested in ourselves. I don’t know why this higher species would want to simulate us.” Randall admitted she did not quite understand why other scientists were even entertaining the notion that the universe is a simulation. “I actually am very interested in why so many people think it’s an interesting question.” She rated the chances that this idea turns out to be true “effectively zero.”

One thing I’ve noticed about these kinds of things: they often feature physicists going on about mathematics, but mathematicians are never invited…

Update: The Asimov debate is available here, the NYAS one here.

Comments

1. **Peter**
   April 4, 2016

   Asimov was a serious scientist. I don’t think he’d like it.

2. **phil fogle**
   April 4, 2016

   I can’t understand people who have a block about saying “I don’t know”. To me, it’s the very foundation of our civilization, and kudos to those who say it, and are prepared to go out and actually do something about it!

   Sadly, it’s humans, not the overlords, who are addicted to entertainment and fantasy.

3. **Bee**
   April 5, 2016

   John Barrow wrote a whole book about the 2nd question “Impossibility: The Limits of Science and the Science of Limits”. The summary is basically: there either is a limit or there isn’t. We’ll either reach it or we won’t. And that, I think is pretty much all that sanely can be said. (In case someone is interested, it’s a good book, I reviewed it here).

4. **John Fredsted**
   April 5, 2016

   The first topic, that on simulation, made me immediately think of the science fiction crime thriller ‘The Thirteenth Floor’. I must admit that it made quite an impression on me, unscientifically of course :-).

5. **adrian**
   April 5, 2016

   Dear Peter,

   You wrote:
   ”Instead of prominent theorists frankly admitting “we don’t know”, there will be an attempt to sell the story to the public that theorists have a wonderful, successful theory

which describes everything, which sadly has the unfortunate feature of not making any falsifiable predictions
”

Given that the debate/conversation between Balasubramanian, Silverstein and Weiner did not happen yet, I believe it would be good not to have a prediction of what will be discussed (that ”we do not know” will not be said, or to write that it will not be a useful debate).

The three mentioned above are very good physicists, with a very strong record of research papers. Certainly able to give their views on the topic ”what is the future of Physics”. Of course, nobody knows the answer and they will voice ”opinions” or ”feelings” for how they see the things. But the three of them have the authority to voice these opinions.

thanks

6. **Richard J. Gaylord**
April 5, 2016

“we will in our lifetimes see the end of fundamental physics as a human endeavor.” while i agree with your comments and dissatisfaction with the current state of theoretical physics, i disagree with your apparent view of what is fundamental (perhaps it should be called foundational). There are a great many (perhaps endless) still unexplained phenomena that physics has yet to solve. As Nobel prize winner P.W. Anderson has observed (see his article “More in Different”
http://robotics.cs.tamu.edu/dshell/cs689/papers/anderson72more_is_different.pdf
“The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. At each new level of complexity entirely new properties appear. The understanding of the new behaviors requires research which is as fundamental in its nature as any other. It requires entirely new laws, concepts and generalizations that necessitate inspiration and creativity to just as great a degree as in the less complex one.”

7. **oliver knill**
April 5, 2016

Wittgenstein once said it adequately: “Wovon man nicht sprechen kann, darueber muss man schweigen.” (“Whereof one can not speak about, thereof one better must be silent”).
While such debates could also stimulate the interest in science. A bit more than 200 years ago, 1812, Michael Faraday got tickets for public lectures of Humphry Davy and shortly after revolutionized physics. But what a difference it is: at the time of Faraday, public lectures for which tickets for the public were sold, actually featured live experiments about new findings. But maybe we compare wrong things: today, the arena for new findings is much larger and the entire world notices and follows great experiments in our time: instruments on Mars finding evidence for water, accelerators detecting the Higgs Boson or detectors finding gravitational waves or neutrino oscillations, telescopes seeing further and further back in time, the discovery of exciting new materials like graphene or nano technology advances etc. We actually live in a very successful and exciting time in physics. Such discussion events could help to wake interest in science. Maybe a young “Faraday” will attend and leave with an important insight: “mumbling about the end of human knowledge is not what science is about”.

8. **Peter Woit**
April 5, 2016

Adrian,
You misread what I wrote. I was discussing my own view of what the danger is of how the “end of physics” will happen, not predicting anything at all about what the panelists will have to say.

9. **anon.**
April 5, 2016

“Instead of prominent theorists frankly admitting “we don’t know”, there will be an attempt to sell the story to the public that theorists have a wonderful, successful theory which describes everything, which sadly has the unfortunate feature of not making any falsifiable predictions.”

Nature abhores a vacuum, as does politics: “Le roi est mort, vive le roi!” Any admission of ignorance is used by “critics” of modern physics as an excuse to ignore the whole subject as being incomplete. Please also avoid tying yourself to Popper, who ignores the very sensible idea of doing experiments *before* formulating a theory. Popper’s basic premise is that theories should be speculative but falsifiable. That’s not science. Really useful theories begin with empirical evidence and make some predictions that are wrong, that are labelled anomalies or issues, requiring further work. Most real world theories are modified or expanded, not falsified, after tests.

Superstring theory is supposed to be doing just that in three ways; 1. by achieving unification by incorporating spin-2 gravitons into a framework that might also include a standard model, 2. by making extrapolations of *empirical*running couplings meet at a common value near the Planck energy, and finally 3. by possibly allowing for the observed positive dark energy with the immense anthropic landscape of metastable vacua, and dark matter possibly with sparticles. I personally think that all of these pro-string arguments are simplistic and hype as you argue, but I don’t like the Popperian argument about “falsifiability” that you keep bringing into this blog! There is so much experimental data waiting for theoretical explanation that it’s a red-herring or strawman to ask for more experiments. The key problem is that superstring theory doesn’t really *quantitatively interrelate existing experimental data very well for the number of ad hoc assumptions and extra parameters it introduces,* not that we need falsifiability. We need theories that better correlate existing data. Before you are annoyed at this, please realise that it is not just your blog when it’s the leading opposition to string. If you use weak arguments, the blog caters to mainstream dogma.

10. **Dom**
April 5, 2016

anon.
I don’t recognise the person you describe as the views I read of Peter’s here. It would be interesting however to see a theory in the true sense of the word that meets your criteria but is not falsifiable.

11. **Peter Woit**
April 5, 2016

anon,
You’re arguing against things I’m not saying (where did I “ask for more experiments”?).

My argument is with the string landscape/multiverse “theory” which is now in the process of being institutionalized as canonical textbook science. The problem with this “theory” is that there is no plausible way to ever show that it is wrong. It’s pseudo-science with exactly the problem that “lack of falsifiability” conventionally refers to. If you have a better term that I can use in sentences where I refer to the problem I’m open to suggestions, but obviously I can’t everytime I mention this issue engage in a long disquisition on the subtleties of the demarcation problem.

12. **Apostolos Syropoulos**
April 5, 2016

A few years ago I attended a workshop somewhere in England. In a discussion, I dared (!) to say that I find stupid the idea that we actually live in a computer simulation and we are not real. A "prominent" English professor of Computer Science told me that I am solipsist…

13. **Confused**
April 5, 2016

Only if we do live in a multiverse will I be able to watch the two debates simultaneously, for then I just have to watch the 2016 Isaac Asimov debate while my other copie will be watching the live stream from the New York academy of sciences 🙂

14. **Chris W.**
April 5, 2016

Apostolos,
I would have loved to hear what that professor thought a solipsist was. What a ridiculous response…

15. **paddy**
April 5, 2016

I am a wee bit concerned by the NYAS discussion as its description invokes the "God of the gaps" and it is funded by (drum roll please)…

16. **Peter Woit**
April 5, 2016

paddy,
I suspect the NYAS panelists won't be that interested in discussing the limits of what we can know about physics in terms of "divine intervention", even if Templeton is helping fund this. Less sure what they think about "the multiverse did it", which is functionally similar.

Actually, I would have thought that funding the AMNH "Is the Universe a Simulation?" discussion would be more the thing to do if you want to bring religion into science. I don't see much of a difference between prominent physicists sitting around discussing "are we the creations of some superhuman beings, running a mysterious computer program that governs everything we do", or "are we the creations of some divine being(s), who control us and our universe by some unknown mechanism for some unknown purpose". Having the AMNH explicitly holding theology debates between physicists would at least have the virtue of novelty.

17. **Chris W.**
April 5, 2016

In these "Is the Universe a Simulation?" discussions, how often do people address the "it's turtles all the way down" infinite regress that such a notion implies? Given how problematic the idea is in other ways I would except to start there and ask why we should even bother with more extended consideration of the notion.

18. **anon.**
April 5, 2016

Sorry Peter, but did appear to effectively ask for more experiments, simply by your demand for falsifiability. How can a theory be falsified without more experiments?

"The problem with this "theory" is that there is no plausible way to ever show that it is wrong."

That's dangerous again! There are 10^500 versions of superstring to be falsified before we give up. You seem to be simply repeating Popper's argument, which was based mainly on the Michelson-Morley experiment which falsified one version of Maxwell's light-carrying aether. After that, Lorentz and FitzGerald modified the theory a bit so that moving objects are contracted in the direction they go. So if experience is anything to go by, the way this sorry saga will eventually end is that Witten or someone will eventually come up with some falsifiable prediction, which will be falsified by experiment, and then he'll just modify the theory a bit. Popper's falsification ignores examples of theories that were simply modified to agree with new results. The reason old theories disappear from textbooks is not because they are falsified, but because something better replaces them.

Can you think of one example of a mainstream theory that was debunked by experiment, without a new rival theory already being present to take over? (Don't say Michelson-Morley 1887 because in the gap until 1905 there was FitzGerald and Lorentz's ad hoc modification.) I don't know of any example of falsification really working. It's always been invoked only after another, better theory has taken over.

19. **Peter Woit**
April 5, 2016

anon,
I'm afraid you're completely ignoring the argument I actually make in favor of discussing different issues about how science works that you are concerned with. All I'm doing is repeating myself at this point, but, again, the simple point is that the problem with the string theory/multiverse "theory", "scenario", "framework", or whatever you want to call it is that there is no plausible way to ever show that it is wrong (or even that it needs to be modified). It's in the same category as empty ideas like, well, the theory that we're a "simulation".

Again, I think referring to this as a falsifiability problem seems to me an accurate use of language. What I seem to keep seeing is that there's some sort of political correctness language problem here, that anyone using the word "falsifiability" gets immediately accused of being a naive "Popperazi". If someone will tell me what the politically correct word is for referring to the problem, I'll consider using it instead.

20. **Another Anon**
April 5, 2016

If string theory is becoming canonical textbook material then it seems to me that that part of physics is become more like philosophy: thousands of papers, self-perpetuating, self-referential, but plenty of material there for a university course. And just as few people with philosophy degrees go on to become philosophers, I suspect most of the physicists will just take their excellent mathematics training from string theory and go into finance.

Physics then is not so much interested in explaining the universe. It's more interested in generating publications, and training for future careers elsewhere.

However, I think you're misjudging the situation. I see the tide has turned in a big way against string theory over the last five years. It's perceived as a theory which has not come up with the goods. If something else big comes along soon, I think there'll be mass desertion from string theory overnight.

21. **Peter Woit**
April 5, 2016

Another Anon,
It's been true for a long time that if a promising idea about unification comes along, string theory unification will become a lot less popular. The problem I see though is that young people potentially interested in the problem are being fed now from a young age a diet of conventional wisdom that there's no point in even trying. We're assured that a wonderful new discovery has been made, a new, better decentering of our place in the universe, showing that the answer to such questions is just "the multiverse did it". This is actually independent of string theory.

Unfortunately I see this point of view becoming more and more popular, with skepticism about it all too uncommon. Making progress on unification is an extremely hard problem, discouraging people from ever trying is really easy.

22. **Robert**
April 5, 2016

If the arc of physics has led most theorists into a wasteland in the last decades, it's mind boggling to think that a whole field can be so wrong! Further, it's scary to think how defensive people get to preserve such a state. How does one correct a whole field that has gone off track? How do you council young aspiring theorists to think independently? Professional peer pressure seems insurmountable.

Also, I have a very curious eight year old physics loving nephew who is in love with the idea of string theory. He's enamored with all the hype and feel I'll just confuse him by knocking it down. Any thoughts anyone? Thanks.

23. **anon.**
April 5, 2016

"… there is no plausible way to ever show that it is wrong (or even that it needs to be modified)."

I simply don't understand why you want a theory than can be shown to be wrong. The standard model is a minimalist theory based on three observed field theories, with very little speculation, and thus very little possibility for falsification. Suppose the Higgs boson had not been discovered, do you really believe the standard model would have been totally falsified? Surely it's better to look at positives in evaluating theories, instead of requiring a way to "prove them wrong". Superstring's "positives" all relate to things that have not really been observed. Nobody has seen a spin-2 graviton, a heavy sparticle, or a whether couplings are equal at the Planck scale. If these things had all been seen, then surely you'd accept superstring theory as a good working theory (akin to the standard model), regardless of whether it is possible to disprove it.

24. **Peter Woit**
April 5, 2016

Robert,
The strange thing is that I think most professional theorists are well aware of the emptiness of the multiverse/string landscape business. What's unfortunate is that the ones who like to be on TV and make good copy are the ones willing to promote an easy to understand, grandiose sounding, but scientifically worthless set of ideas. It's up to others who know better to speak up if they care about their field. There is no problem of professional peer pressure to go on about the multiverse, instead one reason for the evangelical behavior of multiverse proponents is that they're well aware of how skeptical their colleagues are.

The outrageous and damaging hype aimed at the public and the young I think already has had a couple decades effect of driving sensible young people away, and attracting those susceptible to impressive sounding but empty ideas. As for your nephew, I think confusing him would be a good idea…

25. **Peter Shor**
April 5, 2016

The *really nice* (???) aspect to the theory that the universe is a simulation is that you don't actually have to reconcile quantum mechanics and general relativity. You could just postulate that the code is buggy, and that the universe will crash when the first black hole evaporates 10^68 years from now.

26. **Peter Woit**
April 5, 2016

anon,
I complete disagree that the standard model cannot be shown to be wrong, and likely with some aspects of what appear to be your views on complex issues of what science is, but that's irrelevant. Again, you want to argue the subtleties of theory confirmation/falsification and I'm talking about something completely different, the problem of people promoting pseudo-scientific empty ideas.

27. **Another Anon**
April 5, 2016

Anon: "I simply don't understand why you want a theory than can be shown to be wrong. The standard model is a minimalist theory based on three observed field theories, with very little speculation, and thus very little possibility for falsification."

There's all the difference in the world between a theory with little possibility for falsification because the theory gets it right every time and complies with all experiments (the standard model) and a theory with little possibility for falsification because the theory suggests eleven dimensions of space time which we can never see or infinite parallel universes where we can never go. All the difference in the world.

28. **A.J.**
April 5, 2016

I don't think it's a good idea to burst an 8 year old's bubble. Let him be excited. But don't hesitate to point him towards less speculative reading materials. The history of astrophysics and particle physics is full of wonders.

29. **Low Math, Meekly Interacting**
April 5, 2016

Forgive this naive non-physicist, but isn't the "Standard Model" in some sense already "falsified", i.e. it's certainly incomplete, and neutrino masses already exist in tension with the theory without even considering dark matter and dark energy.

The "best" unifying candidate gives us a framework for tying up these loose ends, but a virtually infinite number of ways to do so, while the other favored "alternatives" don't incorporate matter, and may not even be compatible with special relativity at energies we already know it holds.

Sounds pretty awful overall to this outsider, and also like a huge opportunity for a young upshot to shake the foundations of physics, if enough young upshots are given the opportunity to do so. Ditching empiricism strikes me as too radical a remedy just yet, and that does appear to be the only means for the status quo to remain viable.

30. **Peter Woit**
April 5, 2016

All,
I don't want to turn this into a discussion of the Standard Model. No, the Standard Model is not like the string theory multiverse. At all. Questions about unsatisfactoriness of the SM, with respect to for instance neutrino masses or dark matter, are very interesting, but they're well-defined problems of conventional science, have nothing to do with the problems of multiverse theories. Trying to discuss these things together will shed light on neither.

31. **vmarko**
April 5, 2016

There are two kinds of people in the world — those who debate whether the Universe is a simulation or not, and those that have learned and understood quantum mechanics (especially the implications of Bell inequalities) well enough to know that even if the Universe were a simulation, it would be a very weird one, namely one that is not being executed by an algorithm (given that fundamental quantum randomness is uncomputable).

Also, there are two kinds of people in the world — those who debate whether there is a limit to human knowledge or not, and those who have learned and understood mathematical logic (especially the implications of Goedel's incompleteness theorems) well enough to know that the obtaining new knowledge is a process that does not converge, while being simultaneously aware that there will always exist an infinite amount of facts about the Universe that we fail to understand.

Finally,

"There's a very real danger that this will enter the textbooks, and that we will in our lifetimes see the end of fundamental physics as a human endeavor. The limit we will have hit will be due not to the nature of our minds, but instead the nature of our sociology."

Very well said. However, being an optimist, I believe that fundamental physics will not end, but will rather be reabsorbed into math. The drought of experimental data in quantum gravity is already pushing it from physics departments into math departments. Mathematicians are much more adept at developing theories with no contact to experiment, and I hope that fundamental physics will not die but be preserved within math departments, until technology catches up and enables us (or our descendants) to perform new experiments.

Best, 🙂
Marko

P.S. Robert, by all means do burst your nephew's bubble. Have him learn real physics, teach him to question everything, teach him not to trust any authority figure. Teach him the difference between enthusiasm and hype — not just in science, but in life in general.

32. **Dom**
April 6, 2016

Anon
"I simply don't understand why you want a theory than can be shown to be wrong. "

That is not really how I understand falsifiability, it is the concept that there is in principle a test that can be performed that would show it to be incorrect. The alternative is indistinguishable from magic.

33. **John**
April 6, 2016

So Tegmark said there was a 17 percent chance that we are in a simulation. I couldn't tell if he was serious or how he arrived at 17%. Chalmers said that you can't prove that we are not in one because any proof could be simulated.

Not sure how this got classified as a science debate. It was a philosophical debate pure and simple.

34. **Michael Weiss**
April 6, 2016

The discussion of whether we live in an actual universe or within a simulation reminds me of a wry solution to the Shakespeare mystery: these magnificent plays and sonnets of the Elizabethan era were not in fact written by William Shakespeare (wrong social class and educational background), but instead by another Londoner named William Shakespeare. We stand enlightened.

35. **Robert**
April 6, 2016

Peter (and Marko),

Thanks for the advice about my nephew. I can 'teach the controversy' but still want to
very be careful that I don't turn him off to physics by giving him a big disappointment.
Of course at only 8, the physics of ordinary cotton strings is more relevant anyway.

Peter, I am currently enjoying reading your book. The first parts bring back memories of my physics student days at University of Illinois in the mid to late 70's.

36. **Jeff M**
April 6, 2016

John

Just a guess, but maybe Tegmark did the Hampshire summer studies in math, and that's where the 17 comes from. Hampshire recently changed all the speed limit signs on campus to be 17mph in Kelly's honor.

YP17

37. **Tom Andersen**
April 6, 2016

I watched 80% of the NYAS discussion. The first thing I noted was that all three panelists were essentially cloned as far as their outlook on physics was concerned.

I submit the not very popular viewpoint that the internet for all the good it has done us has basically wiped out individual viewpoints in physics. Case in point: its not unusual or weird to discuss the multiverse or 11 dimensions as they are part of the current accepted legend, while truly new 'obviously wrong' ideas are not tolerated. 'Physics' (being the entire community) will not admit that it failed to predict dark matter, dark energy, high Tc superconductivity, etc. The list is embarrassing and getting longer every year. Meanwhile astronomers and condensed matter workers keep finding new data. Its ok to be wrong.

38. **A**
April 6, 2016

arXiv seems to be doing a survey – asked in the comments for trackbacks from this blog to be allowed again, other readers might want to do the same.

39. **Another Anon**
April 7, 2016

At the NYAS evidently there was some discussion of the multiverse, with the audience told "The multiverse hypothesis is no more speculative than the universe hypothesis".

Good grief. I've never heard of the "universe hypothesis" before – probably because it's not a hypothesis. The universe is defined as "all of time and space and it's contents"(thank you Wikipedia). The universe therefore exists. Does the multiverse exist? There is no evidence that it does. Therefore the multiverse hypothesis is more speculative than the "universe hypothesis".

I think what he means is that you can't prove that something doesn't exist. OK, but that still does not raise the multiverse hypothesis to the same level as the "universe hypothesis", for which we have unambiguous evidence. In fact, his suggestion seems to be that the multiverse is clearly separated from the "universe", in which case if the universe is defined as the set of all the things which exist, then the multiverse does not exist – by definition.

40. **A third Anon**
April 7, 2016

As someone who listened to the whole thing, I think the explanation for that comment was that both hypotheses refer to the region beyond the visible horizon. Presuming everything is the same even everywhere where we cannot see is as presumptuous as assuming the conditions are variable. Neither is visible, so both equally speculative. Anyway most of the dialogue was not about that.

41. **Another Anon**
April 7, 2016

Yeah, but the string theory multiverse proposes different laws of physics in different universes. That's a whole step beyond "presuming everything is the same everywhere".

42. **Peter Woit**
April 7, 2016

A third Anon,
Sure, there's some sense in which that comment is true. But it's also worded not so as to enlighten anyone but anything, but to instead score a propagandistic point. I'm curious whether anyone at the NYAS event made any attempt to explain the problems with the multiverse, as opposed to trying to justify and sell it.

43. **A third Anon**
April 7, 2016

Hi Peter. Most of it was not about that. If I have it right, this was just the extreme end of a more general discussion of how to deal with the distinction between complicated versus simple/symmetric explanations of things. One speaker pointed out that in dark matter, from the point of view of the dark sector our visible particle physics is more complicated than Occam's razor would justify. So these parts of physics we don't measure yet could either be complicated or simple as far as we know, and it's not clear how to judge that.
I guess the multiverse/universe thing is about that kind of question but beyond the horizon. I wouldn't know what of it was propaganda but it didn't seem like that, just some interesting musings on this topic.

44. **anon.**
April 7, 2016

Hi Peter. Of course tested theories like the SM have evidence. The problem you know is that certain superstring theorists have – since the 80s – argued that coupling unification and spin-2 gravitons are a kind of evidence. There are two differences. First, time ordering. In other words, the Pauli-Fierz 1939 spin-2 graviton idea predates the string theory framework that incorporates it. I don't understand why the historical order is so important here. Physicists in some other galaxy might have come up with a spin-2 superstring gravity framework before deducing that spin from universal attraction of similar charge sign in quantum gravity! Secondly, the number of such postdictions is far, far smaller in superstring, than it is in the SM. This is the real problem to me, not whether the model is contrived to fit empirical data, or is making way out predictions of hitherto unknown phenomena. A danger in your approach may be that, if some supersymmetric particles seem to turn up at the LHC, you'll be debunked, even though it won't be a proof that any particular existing supersymmetry model is right. The anthropic landscape is so big it will be claimed it covers whatever turns up…

45. **Peter Woit**
April 7, 2016

anon.,
You're still just ignoring everything I write and insisting on arguing about the complexities of a completely different topic and what my "approach" to that is, even though you have no idea what my views are on the topic you insist on discussing or what my approach to that topic is (if you really must know, short version: it's complicated.). Sorry, but this has become a complete waste of time.

46. **TCS**
April 7, 2016

In the age of steam, when engineering was developing increasingly precise machines, a prevailing view was of a beautifully precise mechanical Universe.

In the age of computing, when computers are simulating increasingly complex systems, an emerging view is of a beautifully complex simulated Universe.

Plus ca change?

47. **Sirius**

April 7, 2016

Will everyone just please stop simulating themselves. Some of us have work to do!

48. **Shantanu**
April 8, 2016

Can someone point me to links of videos of these talks if they are archived?
Thanks
shantanu

49. **Confused**
April 8, 2016

Shantanu,

Here is the archive for the NYAS video:
http://livestream.com/newyorkacademyofsciences/physics1

There is nothing archived yet for the 2016 Isaac Asimov debate.

PS: I managed to watch the two debates, not because we are living in a multiverse as I hoped, but thanks to the fact that we live in a computer simulation. I just had to go throw the program twice, once branching towards the AMNH and once towards NYAS. I am looking forward to read in this blog an expression like the "Computer Simulation Mania".

50. **Confused**
April 8, 2016

Sorry for the typos:
I meant "….to go through the program…" not "….to go throw the program…".
There must be a virus in my simulation, or else that it should be upgraded. 🙂

51. **Zoviyer**
April 8, 2016

Peter, related to the last sentence in the second update. Kontsevich in the 2015 breakthrough prize Math Panel (you can watch this around minute 19 of the video that is publicly available in YouTube) said he believes we're in a simulation but in this case because he doesn´t have a good explanation of why the mathematics of quantum mechanics work.

52. **Peter Woit**
April 8, 2016

Zoviyer,
Thanks. That may just be Kontsevich's sense of humor. At least I hope that's what it is…

53. **Jim**
April 8, 2016

It is kinda odd because saying we are living in a simulation is really identical to saying that God exists, but none of the participants wanted to go in that direction. It wasn't even brought up. I guess some physicists are more comfortable with master computer programmer than they are with a God.

54. **zzz**
April 8, 2016

yeah, really, really strange, its like they think "The Matrix" is a plausible historical documentary

55. **Fred P**
April 8, 2016

The main problem I have with Universe as a simulation conjectures is that the hardware requirements would be enormous (i.e. ludicrous).

Let us suppose 1 x 10^80 particles in the "simulated" universe. The memory requirements alone to track each particle would be at least the same number of particles in the "real" universe. Furthermore, these particles would have to be organized in a way that communication and processing can occur in finite times between many of these particles, and so that they are not dense enough to prevent organized motion. I guess I could do the math directly, but this is plainly infeasible; we are proposing creating a computer with more mass than the observable universe into a space far smaller than a galaxy. It would quickly collapse under its own gravity.

One could argue that one has an algorithm that reduces these memory requirements by, say, 10 billion or so (which is highly infeasible). We are still talking about trying to create memory for a computer that far exceeds a galaxy in size, and needs to be compressed into a relatively tiny space (compared to a galaxy) to function, without having gravitational collapse.

Even if the memory requirements were met by, say, having a constellations of close galaxies somehow communicating with each other, each having their own local processing of information, that just makes the problem even worse – since the cost of the Turing-like machine is then replicated in many different places, each of which requires significant mass, and additional communication problems.

I don't see this working without positing (for no reason other than to support this conjecture) wildly different physical laws, and deity-like powers on the simulators, in which case why would they be simulating our wildly different universe?

56. **Chris Oakley**
April 8, 2016

Re: "The Matrix" – how could the machines who created this simulated world for humans while simultaneously harvesting their body heat and brainwaves be sure that some other even more advanced race was not doing the same thing to them?

57. **Mitchell Porter**

April 8, 2016

Robert – you can tell him about testing a theory. Particles have masses and charges, and in string theory those properties come from the shape and size of the extra dimensions, etc. The crowning achievement of string theory would be to explain the specific mass, charge, etc. of the particles we actually see, but that hasn't happened yet.

58. **G.S.**
April 9, 2016

Fred P.,

From the 2012 paper by Beane, et. al (http://arxiv.org/pdf/1210.1847v2.pdf):

"... the discovery of the string landscape, and the current inability of string theory to provide a useful predictive framework which would post-dict the fundamental parameters of the Standard Model, provides the simulators (future string theorists?) with a purpose: to systematically explore the landscape of vacua through numerical simulation. If it is indeed the case that the fundamental equations of nature allow on the order of 10^500 solutions, then perhaps the most profound quest that can be undertaken by a sentient being is the exploration of the landscape through universe simulation."

So, evidence that we are inside a simulation would be evidence in favor of string theory. However, lack of evidence that we are inside a simulation has no repercussions for string theory.

59. **vmarko**
April 9, 2016

Now I'm getting confused — is the multiverse inside a computer simulation, or is the computer simulation part of the multiverse? Or is it somehow both at the same time? Oh string theorists, please help me understand this deep physics!

😀
Marko

60. **Low Math, Meekly Interacting**
April 9, 2016

That's what so mind-blowingly awesome about about it: you can have a simulated multiverse embedded in a real multiverse, or another simulated one! I think infinity x infinity makes anthropics a googolplex times more plausible, don't you?

I don't get all the hate for the simulation hypothesis, though. What if the simulators are advanced humans? They might be interested in us because they like to simulate alternate versions of their past. Maybe they can simulate a quantum mechanical universe and solve problems associated with the embedded lattice QCD or whatever because they've coupled the simulation to a "real" quantum mechanical experiment or quantum computer. Anyway, there are probably ways to deal with any of the sundry criticisms and perceived implausibilities. In my mind it puts the simulation hypothesis on firmer ground, scientifically. There must be a non-zero probability the simulators will eventually plaster "Oh, all right, you got us; yes, you're a simulated brain in a fancy jar" across the heavens before rebooting. There's zero probability we will observe other bubble universes beyond our causal horizon. Which is where they all must be, apparently.

Not that either is more useful for solving current problems with physics, nor more or less ridiculous to debate professionally.

61. **Peter Woit**
April 9, 2016

All,
I'm now deleting any more comments explaining why the simulation argument is stupid. Yes, it is. Comments about the panels are fine, what's intriguing here I think is why very smart people are involved in such a dumb argument, not the argument itself.

62. **anon-e-mouse**
April 10, 2016

Having programmed for most of my life the temptation to see the world through the eyes of coding is irresistible ("What the heck is the state machine for this flower?"). I get that, it makes sense, it is my suspicion that all immersive disciplines experience their own unique filters on the world. However, every programmer takes shortcuts, and bugs are only bugs if users find them. So consider the downside. The universe may not have been QA approved for shipping yet…it may not even be a fully functional alpha release…

63. **BittenByBits**
April 10, 2016

Seriously, I think the only reason for this "the universe is a simulation" nonsense is because people who spent too much time coding instead of doing real math or physics
(1) have experienced a self-induced delusion where they start thinking everything is really 0's and 1's
and/or:
(2) want to believe their time hasn't been wasted learning arbitrary rules, and that they are really onto something much more profound, thus wasting everyone else's time with "serious" speculations which are amusing only when turned into science fiction by a talented author, and which were always tongue-in-cheek in the first place, except possibly in the case of Philip K. Dick, who was not only an amazingly creative writer but battled very real paranoia and schizophrenia.

Exhibit A: Wolfram.

64. **Another Anon**
April 11, 2016

" why very smart people are involved in such a dumb argument"

Physics has always been influenced by the technological advances of the day. As an example, steam power influencing the development of thermodynamics.

65. **Tammie Lee Haynes**
April 11, 2016

Dear Dr Woit

In re “the Universe is a simulation”
Have you ever heard of anything more creationist, in the literal sense?

Starshot

April 12, 2016
Categories: Uncategorized

I don't know what this is about, but Stephen Hawking and Yuri Milner are here in New York today, with a press conference downtown scheduled at noon, supposedly to announce a mysterious new space exploration initiative, to be called Starshot. There's also evidently some sort of Columbia connection, with a reception scheduled up here. More later, when we find out what this is all about. Last year there was a Hawking/Milner announcement of Breakthrough Listen and Breakthrough Message, projects related to communication with alien civilizations.

Update: Livestream will be available here.

Update: According to this source (and Google translate…), this is a project to send a small space probe to Alpha Centauri.

Update: "Breakthrough Starshot" is a $100 million research program, hoping to develop (ultimate cost of order $10 billion) very small probes attached to light-sails, pushed towards Alpha Centauri by ground-based lasers (a "Silicon Valley approach to spaceflight"). The claim is that such things could travel at a significant fraction of the speed of light, get there in 20 years or so. One thing I'm not seeing is how you get a signal back to earth.

Update: They have a website here, there's a story at the New York Times here, at Scientific American here.

One thought about this is that if you really could accelerate probes this way with lasers, sending them out to solar system planets in days would seem to be a more interesting application.

Update: A more detailed story is here.

Comments

1. **S**
   April 12, 2016

   That is the genius part. The probes will be loaded up with literature about the multiverse. When the Alpha Centaurians find out about the scientific virtuosity of earthlings, they will come up with their own way to send a signal back to earth.

2. **Art**
   April 12, 2016

   Light sails accelerate slowly, but they accelerate constantly. A mission to a solar system planet might not be much faster than conventional rockets because it would take so long to get up to that 0.2c speed. Once it gets there though… zoom.

   I'm sure communication will be one of the significant engineering challenges. One of the things I'd investigate is if you could have the sail change its shape to modify the reflection of the driving laser and still see the reflection back on Earth.

3. **Peter Woit**
   April 12, 2016

   Art,
   The plan seems to be to accelerate very quickly (days?) with very high powered earth based lasers, using the fact these things are so light.

   Another problem I saw someone pointing out: can you decelerate at Alpha Centauri?

4. **Low Math, Meekly Interacting**
   April 12, 2016

   If the thing is small, then I would imagine there's no way at all to decelerate (short of a collision), unless the Centaurians have some powerful lasers of their own.

5. **NoGo**
   April 12, 2016

   Some details there:
   http://arstechnica.com/science/2016/04/breakthrough-starshot-announces-plans-to-send-ship-to-alpha-centauri/

   On acceleration, for example:
   "Propulsion will be outsourced to a facility on Earth. The small spacecraft will be equipped with a light sail, and a phased array of lasers in the 100GW range will provide the sail with enough push to get the craft moving at roughly 20 percent the speed of light in just a matter of minutes."

6. **anon**
   April 12, 2016

   What about all those poor birds and monarch butterflies flying into the 100 GW laser beam? Pretty soon they will need bird traffic controllers…

7. **cedric bardot**
   April 12, 2016

   The roadmap for interstellar laser driven spacecrafts has been thoroughly outlined by Philip Lubin in http://arxiv.org/abs/1604.01356

8. **Robert**
   April 12, 2016

   This concept is the engineering equivalent of String Theory. You end up with a lot of PR but nothing useful in the long run.

9. **NoGo**

April 12, 2016

List of challenges:
http://breakthroughinitiatives.org/Challenges/3

10. **Jakob**
April 12, 2016

The deceleration part seems like a big problem and I would be very interested in how this could be achieved safely (a very robus parachute maybe?). In the roadmap Cedric linked to the possibility to accelerate larger spaceships with the same method sounds like a really intriguing next step. Although I'm quite sceptical at the moment, I think it's awesome that such a bold, alternative idea gets funded.

11. **AcademicLurker**
April 12, 2016

Another problem I saw someone pointing out: can you decelerate at Alpha Centauri?

Important consideration. In case there's an advanced alien civilization in Alpha Centauri, it would be pretty rude to send even a small space craft slamming into them at 0.2c.

12. **Low Math, Meekly Interacting**
April 12, 2016

Per cedric's reference:

"Thus a 1 kg spacecraft going at 0.3 c will have an effective "yield" of 1 MT or roughly that of a large strategic thermonuclear weapon."

No way in Hell that thing is slowing down, even if it's a ship-on-a-chip.

Note the word "deceleration" doesn't even appear in the article.

13. **Dale C.**
April 12, 2016

There is no need for deceleration. At solar system scale, even at 0.2c there is plenty of time to snap a few pictures when you fly past. I am curious about communication back to earth. The communication appears to be laser-based, but how can that be detected at a huge distance to earth from a device weighing a few grams?

14. **Low Math, Meekly Interacting**
April 12, 2016

Assuming it's a wafer craft, what kind of detail could you resolve with optics that tiny (presumably on the order of what my iPhone can boast)? It would traverse the Earth-Moon system in roughly five seconds. How close does it have to get to something to improve on the space telescopes we'll have when that thing finally arrives? If you needed to be a few million kilometers away for a productive flyby of a planet, is it possible to aim that precisely over those distance and time scales without being able to adjust trajectory? I guess they must be thinking of these sorts of things, but tall order indeed.

15. **lzp**
April 12, 2016

Can a laser beam be focused to achieve such an incredibly small divergence at such distances? A related question: how large is the best focused laser dot on the Moon–I kind of recall ca. 1 km in diameter?

16. **Steve Huntsman**
April 12, 2016

Quoting from the Economist story on this: "At its destination it would beam back pictures of the star's planets with its on-board laser. No current observatory could possibly pick up such a signal—but the kilometre-wide launch array should be able to. The optical systems used to meld the output of the lasers could be used in reverse as a vast and sensitive telescope."

17. **JackLothian**
April 12, 2016

One of my favourite low-brain activities is reading science fiction. I will have to read it & see how it compares with Larry Niven novels.

18. **M**
April 13, 2016

A photo taken at $v \approx c$ would be an interesting demonstration of relativity, even without going to Alpha Centauri

19. **Hans**
April 13, 2016

Of coarse anybody is free to think/believe that there are 'aliens' out there and fantasy about technology to contact 'these things' – but actually doing so is not to decide to the likes as 'some nutty professors' and/or 'some wealthy Santa Clauses' – this is first and fore all a serious political issue that concerns us all and should be dealt with at the UN or so.

20. **Avattoir**
April 13, 2016

[What's with all these negative waves?]()

Sure, maybe there's no way to decelerate the little wafer ships, or retrieve and relay back here pictures or data of any kind, and maybe all that ends up happening is firing off the equivalent of an armada of thermonuclear devices that'll vaporize anything any of them strike. But why isn't anyone posting how beautiful this all is, something righteous and hopeful for a change?

21. **Daniele V**
April 13, 2016

At 0.2c even a very small dust particle could destroy the probe if they clash, this is also a big problem.

22. **bugannoyer**
April 13, 2016

I'm sure their engineers have thought of this, but since I didn't see it addressed in the articles, I'd wondered how they intended to deal with the likelihood of radiation damage from ambient particles impacting the electronics at ~.2C.

23. **Low Math, Meekly Interacting**
April 13, 2016

This multi-kilometer mirror array…it can theoretically receive a data stream from something like a laser pointer 4.4 light years distant. If you made two of these things half as big each and used it as an interferometer, what could you resolve at the same distance? I'm just not getting why one would spend that kind of money to send a downsized 2MP phone camera to the closest star system under the proposed conditions when much of the required technology would apparently yield an instrument with such sensitivity to light in the (I assume) visible spectrum. It's a ginormous laser cannon, but it's also the most amazing light collector anyone has dreamed of building. Avattoir has a very good point: we can strafe Alpha Centauri with a hail of kiloton kinetic energy ordnance, or, you know, maybe just look at it.

24. **Ben**
April 13, 2016

The likelihood of them getting destroyed is why they launch so many.

As far as the damage the probes themselves could do… even at 0.2c, a few grams is more in the kiloton range, and detonations that large (from meteorites) happen in our upper atmosphere reasonably often, sometimes without anybody on the ground noticing (without specialized instruments).

I think "phoning home" is really the biggest problem. It's not just a question of increasing sensitivity, because if the signal is that faint than other sources and noise will be present as well.

25. **Herr Weh**
April 13, 2016

At this point it seems more like a research project for interstellar travel in general rather than an already fixed proposal with predetermined details. What counts – and I think that is very interesting – is that somebody came up with real money to actually get it on. Definitely better use for his money than some of the other projects Milner has funded. For those interested in interstellar travel I recommend following Paul Gilster's excellent Centauri Dreams blog. Here's his take on the Starshot project: http://www.centauri-dreams.org/?p=35402

26. **Low Math, Meekly Interacting**
April 13, 2016

Looks like it'll be a 1 watt laser sending bursts of telemetry, so relatively much more powerful (during a burst) than a laser pointer at least should be. The array will reportedly receive 10-14 of the photons transmitted, assuming it can be aimed, because the beam would only be about 1000m wide, if the sail can focus it well enough.

This is all obviously way, way beyond me.

27. **paddy**
April 13, 2016

What makes me think that long before these cell phone probes arrive for their flyby, solar system based technology will have learned all about Alpha Centauri?

28. **vmarko**
April 13, 2016

I have a feeling that the Starshot project is similar in many aspects to the Mars One project. In short, it's a nice-sounding idea that attracts a lot of attention and wishful thinking, sounds plausible when looked at naively, but is ultimately burdened with some insurmountable engineering problems which eventually render it fail. The devil is in the details, as usual.

Hopefully they'll prove me wrong. 🙂

🙂
Marko

29. **Ken Wrona**
April 13, 2016

The probe can most certainly be decelerated. If aimed squarely at the host star it will decelerate from photon pressure. The question is whether it will be destroyed before it reaches a sufficiently slow speed. The Lubin roadmap isn't sanguine about this but it is discussed. There are a lot of variables.

Anyway, this is an interesting engineering project that should rightly be undertaken with private money. It's likely that a lot of interesting things would be learned.

30. **Jesper**
April 13, 2016

I think that the best solution to the "phone home" problem would be to kindly ask the Alpha Centaurians to make the call for us. I mean, if you live in the Alpha Centauri system you must have some super-cool powerful equipment and surely you won't mind making a collect call for a crazy flyby probe that hurtles through your system at near light speed

31. **SunJammer**
April 13, 2016

From Wikipedia article, "Sunjammer":

“Sunjammer” is a science fiction short story by Arthur C. Clarke, originally published in 1963,[1] and included in the March 1964 issue of Boys’ Life. The story has also been published under the title “The Wind from the Sun” in Clarke’s 1972 collection of short stories with this title. It depicts a yacht race between solar sail spacecraft.

Plot Summary:
John Merton, a spaceship designer, develops and promotes a lightweight spacecraft with a large area of solar sail, to be powered entirely by radiation pressure—the so-called wind from the sun. The sun-yachts start their journey in Earth’s orbit, and, pushed simply by sunlight, can achieve a speed of two thousand miles an hour within a day.

The concept leads to the development of the sport of sun-yacht racing, and after several years of refining his ideas, Merton competes in what will be his final race. His hopes for victory rest on the low mass of his craft which he has made possible through advances in automation enabling him to fly it solo.

Soon, all but two of the competitors have dropped out, mainly due to damaged craft, and it is a straight race between Merton’s craft and Lebedev, entered by a Russian crew from the University of Astrograd. Although the Lebedev is lagging Merton’s yacht, its senior pilot delivers a surprise blow by announcing that he plans to jettison his co-pilot in an escape capsule now that the earlier, navigationally intensive part of the race has finished.

Merton responds by recalculating his expected margin of victory and realises that the race is now going to be neck-and-neck at the finish line. At this point news arrives of a massive, and potentially deadly, solar flare. The race has to be abandoned, and there is no winner, though Merton abandons his craft with its sail still fully extended in order to ensure that it will be blown into interstellar space.

Planned 2014 solar sail mission

NASA planned to launch a solar sail technology demonstration mission titled ‘Sunjammer’. The title is a reference to the story.[3] The mission was cancelled in October 2014.[4]
…………
Comment: of course the nano-idea and use of lasers is a new twist on this idea.
Don’t know if Clarke was the first to think of it, but my memory was jogged by this
item and I remembered some great story by him, so thanks to Google, in one second I had found it!

32. **Ralph**
April 13, 2016

Re comms back to earth, another problem: receiving a very weak signal requires very fine tuning. But the tuning will have large uncertainties…

The signal from the probe will have a large doppler shift. In principal you just change the tuning of your receive to compensate but…. Any residual drag from radiation pressure etc will cause a drift in the doppler shift, destroying the tuning.
The latter tune drift is proportional to frequency, so using lasers not radios for communications makes this orders of magnitude worse.

Multi kilometer phased array operating at very-precise-but-constantly-changing optical frequencies coping with both 100GW and sub attowatt power levels?

Also, what will be the heat load from interplanetary / interstellar gas at 0.2c? Changing temperatures will cause further frequency drift on the source making the comms even more challenging.

33. **In Hell's Kitchen (NYC)**
April 13, 2016

Let’s hope the Centaurians don’t mistake Yuri’s armada for a relativistic weapon and retaliate: https://en.wikipedia.org/wiki/The_Killing_Star

34. **ZZZ**
April 13, 2016

A related idea is to use ground based lasers to heat propellants in a spaceship to achieve much higher specific thrust than possible with on-board chemical energy. Some national governments have been pursuing this for a while.

35. **MikeS**
April 14, 2016

Whatever the engineering merits of this project, it is clearly intended in the realm of space exploration rather than SETI.

If we really though there was intelligent life at Alpha Centauri then it would make more sense to just send the laser beams – with some suitably encoded data – and wait for the Centaurans to fire back.

36. **anon**
April 14, 2016

In principle you can actively decelerate a craft like this the same way you accelerated it – incorporate a detachable mirror that reflects the laser beam backwards onto the main craft, whose light sail is turned around once it has detached from the mirror – the mirror would of course be rapidly accelerated away from the main craft. In practice, this would need the ability to maintain a near-earth-based laser focused over interstellar distances. I have no idea if that is practicable.

37. **Paul D.**
April 26, 2016

This idea requires multiple engineering “miracles”, many of which would be very useful for other applications by themselves, even at lower levels of performance. So I’m not sure why he’s funding this instead of going after the pieces individually.

Example: he imagines reduces the cost of the continuous lasers from \$10/W to \$0.10/W. At that cost it becomes feasible to power aircraft via lasers in orbit (no CO2 emission, unlimited range, no fuel weight.)

As for sending spacecraft elsewhere in our solar system: the likely application there would be beamed power for plasma engines, like Hall thrusters or VASIMR, with an exhaust speed of perhaps 100 km/s. The power and energy requirements would be orders of magnitude lower than if one used light pressure for thrust.

Various and Sundry

April 13, 2016
Categories: Uncategorized

Some quick links:

- Via my Columbia colleague at Mathematics Without Apologies, a documentary about Perelman that I was unaware of.
- I learned something yesterday about another math department colleague, Mikhail Khovanov: he has games called Ringsanity and Ringiana available as apps.
- Took a quick look at a Stephon Alexander's new book The Jazz of Physics, which is now in bookstores. While I enjoyed reading some of his account of his career in theoretical physics, I'm afraid that his two main topics, jazz and models of the big bang, both are things that pretty much leave me cold. For those with more interest in either topic, you probably should take a closer look.
- I'm sorry to see that there's some sort of fight developing over Grothendieck's papers. A shame.
- Previous attempts to figure out what "ER=EPR" is supposed to mean have left me baffled. Trying to read Susskind's write up of his IAS lectures on the topic hasn't helped, I'm afraid.

Update: Mikhail Khovanov tells me that Ringiana for Android can be found here. It was developed jointly with Nikolay Gromov.

He also comments that "Thompson groups V and T act on the states of Ringiana and Ringsanity, correspondingly. These groups were the first two infinite, finitely-presented, simple groups discovered by mathematicians."

Comments

1. **Can Hatipoglu**
   April 14, 2016

   I am looking forward to see some content in English about the fight over Grothendieck's papers.

2. **Yatima**
   April 14, 2016

   Reste à savoir quelle est la valeur de ces archives. "Difficile à dire, tant que l'université refuse tout échange avec nous", poursuit l'avocate, "mais on peut imaginer que des grandes universités américaines avec beaucoup de moyens seraient prêtes à payer le prix fort."

   The relatives' lawyer says that the value of those documents (apparently hidden somewhere in Montpellier in a secret vault and being scanned to the tune of 45KEUR but with no intention of publishing them – indeed it seems Grothendieck did not want them published in the first place) is difficult to judge as the university refuses to engage but that big monied american universities would be willing to lay down large wads of cash.

   It sounds all pretty sordid.

   That article has a weird last paragraph saying that Einstein was a major factor of "The Bomb" (oh really?) and that no-one knows what might come out of Grothendieck's stash of unpublished notes. Huh.

3. **ptmalloy**
   April 14, 2016

   Are there Android versions of the two games? Just looked for Ringsanity, but could not find it in the Google Play store.

4. **Anonymous**
   April 14, 2016

   ptmalloy,
   "Ringiana is available for both iPhone and android, while Ringsanity you can get for iPhone, with android version in the works."
   http://ringsanity.blogspot.com/2016/03/mathematics-of-ringiana-and-ringsanity.html

   It can be downloaded from here:
   https://play.google.com/store/apps/details?id=com.strongcoupling.vanbaalon.cantor

5. **ptmalloy**
   April 15, 2016

   Anonymous,

   Thank you very much! I will look for it.

   ptm

6. **Bill**
   April 16, 2016

   I am curious what was Mikhail motivation to create those apps…

7. **Abel Molina**
   April 17, 2016

   Wow, Ringsanity looks really cool. Thanks for posting the links, gonna get it for sure!

8. **Davide Castelvecchi**
   April 20, 2016

What I find interesting about the documentary on Perelman is that there is no mention whatsoever of his Jewish heritage — or of the antisemitism he had to fight during his life as a student in St. Petersburg. Masha Gessen gives the full story in Perfect Rigor, her superb biography of the man.

The Man Who Knew Infinity

April 16, 2016
Categories: Film Reviews

Last night I went to see a showing at the Tribeca Film Festival of the new movie about Ramanujan, The Man Who Knew Infinity. It was extremely good, infinitely better than the most recent high profile film about a mathematician, the one about Turing (see here).

The story of Ramanujan is one of the great romantic stories of mathematics, with a large part played in it by the Cambridge mathematician G. H. Hardy. The filmmaker was inspired by Robert Kanigel's excellent 1991 biography of the same name (he says his mother gave it to him to read, she had it through her book club). The book is an excellent source for the story of Ramanujan's life, and Hardy's A Mathematician's Apology is something everyone should read (for one thing, it's short). For some more about the film from an expert on Ramanujan's work, the AMS Notices have this from George Andrews.

Some dramatic license was taken, for instance in having Jeremy Irons play Hardy as a much older man than he actually was when he met Ramanujan. After the film there was a panel discussion, with filmmaker and screenwriter Matt Brown explaining that it took 10 years to get the film made, largely because of the difficulty of financing it. He claimed that he could have gotten the financing much earlier if he had been willing to go along with certain plot changes the financiers wanted: in particular they wanted the story to revolve around a love affair of Ramanujan with a (white) nurse, to be played by a high-profile starlet.

Also at the panel discussion were two mathematicians: Princeton's Manjul Bhargava and my Columbia colleague Ina Petkova. One reason the film is so true to the real story of the mathematics and mathematicians involved in it is the involvement of Ken Ono and Bhargava. Ono was heavily involved in the filming (and he has a memoir from Springer, My Search for Ramanujan, about to appear). Bhargava was involved in the editing, in particular in helping choose among the many takes of the actors acting out a mathematical discussion those which seemed true to life.

The film is supposed to be released here in the US on April 29, I can't recommend it enough.

Update: Scott Aaronson has a far better review of the film than mine here.

Comments

1. **Hugh Osborn**
   April 16, 2016

   The film skates over that Ramanujan married his wife when she was 10 (arranged by
   his mother) and left here in India when she was 15, very different from the impression
   in the film.

2. **Peter Woit**
   April 16, 2016

   Hugh Osborn,
   Thanks. Unlike the mathematics, the part of the movie about his wife didn't ring true, but I had forgotten about the real story there (read the biography over 20 years ago). I guess what went into the movie was a half-way compromise between the true story and the steamy romance with the British nurse.

3. **Fernando**
   April 16, 2016

   The other concession to cinematic tastes is that the actor playing Ramanujan is quite thin. As the photos show, Ramanujan was a little chubby until he got sick.

4. **Peter Woit**
   April 16, 2016

   Fernando,
   Yes, and the film starts out with a shirtless hot-bodied Ramanujan. Not clear if mathematicians should strenuously object to this kind of taking of cinematic liberties.

5. **Dav**
   April 16, 2016

   If I had the time & means, I'd love to one day make a movie about the mathematician Évariste Galois.

6. **Tim May**
   April 18, 2016

   I finally caught the Turing movie. About like the Hawking movie. A lot of mawkishness, not much of the science. (I thought about 2 minutes of "A Beautiful Mind' captures some of the mathematics of Nash..the rest was garbage.)

   I hold out little hope for films about major mathematicians or physicists.

   It wasn't very different 50 years ago–I was there– and we survived it.

7. **Daniel**
   April 18, 2016

   I wonder why no one has ever made a serious biopic of Einstein.

8. **Visitor**
   April 18, 2016

   My guess is that Einstein because lived a fairly public life it would therefore be rather more difficult to present the tissue of lies, falsehoods, misrepresentations, and distortions necessary to make the kind of "dramatic" and "absorbing" movie that has any hope of being financed.

   (And there might be the secondary problem of Einstein's appearance being too familiar to people: any actor portraying Einstein would look unlike him, and suspension of belief become rather difficult: the movie's psychological mechanisms wouldn't work. As an extreme example of a different sort, portraying Hitler seems an almost hopeless task; the only good movie portrayal is by Ganz, and even then it seems to take viewers quite a while to accept him. Chaplin's portrayal is also good but required a fundamentally different sort of movie in which to work.)

9. **Robert Matthews**
   April 18, 2016

   Must admit I was somewhat put off going to see The Man Who Knew…by the trailer, which shows Ramanujan bearing the brunt of racism. As far as I can tell, my battered copy of Kanigel's bio contains no evidence, so have put the film in the "Reality Just Isn't Interesting Enough" category (along with the recent Turing and Hawking bios).

   Maybe I should reconsider ?

10. **Parth**
    April 18, 2016

    Thanks for this positive review. I'm wary of such films, but having Bhargava and Ono on board brings optimism. Now I'm looking forward to the screening here in Sydney Australia (proper release date here is 5th May but going for advanced screening).

11. **Peter Woit**
    April 18, 2016

    Robert Matthews,

    It's likely the film overplays the extent of overt racism encountered by Ramanujan. It's not a documentary, takes lots of liberties with the true story. But what I think is admirable is that it does a good job of getting the core of the story right, the mathematics and the relationship of Hardy and Ramanujan.

12. **Jim**
    April 18, 2016

    The worst cultural assault that poor Ramanujan suffered was the horrible food he was forced to consume in Britain, which most likely led to his early demise. World War I war-time Britain wasn't so accommodating to vegans.

13. **Edward Friedman**
    April 18, 2016

    Actually, there are several dramatizations of Einstein. Check on YouTube

14. **Avattoir**
    April 19, 2016

    Prof Woit, at first I misread your second comment in this thread as, effectively, 'this kind take on mathematicians'.

    Hardy's notorious fondness for cricket, including in setting matches into which he led teams of mathematicians against teams of other schools, and most especially the photo of him leading a team of English mathematicians "against [mathematicians from the rest of] the world", was dadsplained by my late father – who was in the business (of maths & physics, tho also an amateur thespian, cricketer, footballer and knew several of these characters, served to inspire [this bit of comedic art.]

15. **Tim May**
    April 19, 2016

    About Ramanujan dying from having only British food to eat, I am skeptical.

    I think I read this about 50 years ago, probably in Bell's "Men of Mathematics."

    Having eaten some British food, having lived on a variety of various fast foods, having cooked as a bachelor, I think R. must have had some other issues, or some disease, etc.

    I think the meme that R. died from eating British food is just too facile, too much part of some popular meme. I haven't seen the new film, but I hope it doesn't perpetuate this meme (unless it justifies it).

16. **Peter Woit**
    April 19, 2016

    Tim May,
    "The Brits killed Ramanujan with their food" is a suspiciously provocatively entertaining meme. I think though the film portrayal of this is quite plausible: he is shown trying to eat in the college, being unable to get vegetarian food not cooked in lard. He's also shown having a lot of trouble trying to do his own cooking in his room. It's all too plausible that he had grown up without learning to cook, and that this together with difficulty in getting foods that fit his dietary restrictions led to health problems. His death is often attributed to tuberculosis, but whatever it was, it wasn't just malnourishment, because his health deteriorated and he died after returning to India, where presumably he was getting an appropriate diet.

17. **Avattoir**
    April 19, 2016

    Tim May, this seems to capture the consensus from the mid-1990s:

    "hepatic amoebiasis … that can result in dysentery [if] not treated properly", supported generally by a lot of evidence of Ramanujan suffering from chronic poor health since childhood, and most particularly by "two bouts of dysentery before he left for England", resulting in vulnerability to "lifestyle changes" capable of aggravating the living amoebae to the point capable of killing the host.

    FWIW my two English grandparents lived to 85 and 101 respectively, and the only one of their children who remained in England lived to 87 – a decade beyond any who moved elsewhere, including here.

18. **dom**
    April 19, 2016

Jim
As a native of these islands, I can confirm that vegetables do indeed grow in the ground and fruit trees work too. Additionally in wartime a much greater effort was made in growing these. They may have been different fruits and vegetables to southern India I grant you.

19. **srp**
April 19, 2016

This legend is as good as the one about Descartes being killed by having to get up early in cold weather because of the whims of Danish royalty.

20. **Stephane**
April 20, 2016

Linked to this film, see: https://www.youtube.com/watch?v=yvHrn-41XMU

"Where does mathematical genius originate and what does it tell us about the human mind? Join us for "The Infinite Mind: Exploring Mathematical Genius," as we explore these and related themes with mathematicians Manjul Bhargava and Steven Strogatz, together with Matt Brown, director of The Man Who Knew Infinity, a new film about Ramanujan to be released in April, starring Jeremy Irons and Dev Patel."

21. **Neeti Sinha**
April 22, 2016

Appreciate the inside information, have been waiting for it.. and look forward to seeing it. Although from the trailer, I did think too that the racism part was overdone, as that tends to happen in such historical takes, and might not be the truest picture.

22. **J. Pooh**
April 27, 2016

Recommended: 'N is a Number', a biopic film about Paul Erdös.

Nearly impossible to buy (unavailable new?, used DVD overpriced), but easy to find online. YouTube downloader app is useful for keeping a copy.

23. **Peter Lund**
April 28, 2016

Re. Descartes: Swedish royalty. The reigning Queen, actually. (Who was incidentally officially called "King" in Swedish because she actually ruled.)

https://en.wikipedia.org/wiki/Christina,_Queen_of_Sweden#Visit_from_scholars.2C_musicians_and_Descartes

24. **srp**
May 1, 2016

Saw "The Man Who Knew Infinity." Enjoyed by both my spouse and I.

Jeremy Irons's Hardy, whether accurate or not, seems like a real person you could meet on the street–excellent performance. They really overdid the ethnic prejudice angle, I think, and they skated over the fact that Ramanujan had actually published a couple of things in Indian journals before sending his letter to Hardy. But by traditional Hollywood biopic standards (e.g. black-and-white studio classics like the Paul Ehrlich and Marie Curie films) this movie was true to the main lines of its subject's life and caught the spirit of mathematical discovery pretty well, as far as I can tell from reading some history of the field.

This Week's Hype

April 19, 2016
Categories: This Week's Hype

It occurred today that the past year or so there haven't been as many editions here of This Week's Hype, with in particular the previously common "Scientists finally find a way to test string theory!" stories now less common than they used to be. What is often replacing bogus claims about testability though is just unvarnished hype about string theory, minus any claims that it can ever be tested. Examples I've run into today include Cumrun Vafa's Fundamental Lessons From String Theory presentation at the April APS Meeting, which tells us that

> String theory is leading to a revolutionary revision of many fundamental and long held principles of physics

despite a lack of any connection to experiment, either now or in the future.

In the local bookstore I took a look at Christophe Galfard's The Universe in Your Hand, which builds up to a final chapter with some sort of rather incomprehensible voyage with a robot to the string theory multiverse. Nothing anywhere to be seen there about whether this might be science or fantasy. Jennifer Ouellette has a review in the New York Times here.

While mulling over these thoughts about the new prediction-free environment for string theory, I noticed that an article has just appeared that seemed to contradict such thoughts, Natalie Wolchover's Physicists Hunt for the Big Bang's Triangles, with a headline claiming that "evidence for string theory" could be found in the sky. It's by far the best popular piece I've seen about "string cosmology", giving an excellent idea of what people in that field are up to these days (which includes large amounts of hype, coming from the scientists, not the journalist).

In summary, here's what we learn about current string cosmology. One of the main targets is a "prediction" of the level of non-gaussianity in the CMB, something which all observations so far have shown to be unobservably small:

- Matthew Kleban and Eva Silverstein are described as "cosmological clocksmiths", working out the non-Gaussianity "predictions" of a large range of string cosmology models. It seems that you can get any number you want this way by an appropriately complicated model. Kleban likes unwinding inflation and we're told:

  > "I think it's pretty plausible that some version of this happens," he said.
  >
  > Though Kleban acknowledges that it's too soon to tell whether he or anyone else is on to something, plans are under way to find out.

  Silverstein has "many string inflationary models" of all sorts, so can get you one with any amount of non-gaussianity you might want. As far as she is concerned, having plenty of complicated models with zero evidence for any of them, such that, no matter what you see or don't see, there's always lots more and more "predictions", is a perfectly traditional kind of science. Her reaction to people pointing out the problems with this?

  > I find it surreal, because we are currently doing some traditional science with string theory.

- Her Stanford colleagues Andre Linde and Renate Kallosh are taking a different approach, promoting theories with no observable non-gaussianity, and, it seems, no observable effects at all:

  > Linde isn't bothered by this. In supporting the alpha-attractor models, he and Kallosh are staking a position in favor of simplicity and theoretical beauty, at the expense of ever knowing for sure whether their cosmological origin story is correct. An alpha-attractor universe, Linde said, is like one of the happy families in the famous opening line of Anna Karenina. As he paraphrased Tolstoy: "Any happy family, well, they look in a sense alike. But all unhappy families — they're unhappy for different reasons."

- On the East Coast, there's Arkani-Hamed and Maldacena, with a paper last year on Cosmological Collider Physics. It described observable signatures of new physics at the inflationary scale, but the only comment I could find about how you would actually observe such things was

  > In terms of measurability, if the couplings are Planck suppressed, then it seems impossible to measure this through the CMB or large scale structure. (See e.g. for a discussion of measuring these effects via large scale structure.) But it might be possible using the 21cm tomography .

  Arkani-Hamed explains to Wolchover that there's a "cosmic variance" problem making things inherently unobservable, but he removed most discussion of this from the paper, hoping to get around it by changing the laws of quantum mechanics:

  > In his paper with Maldacena, Arkani-Hamed initially included a discussion of this issue, but he removed most of it. He finds the possibility of a limit to knowledge "tremendously disturbing" and sees it as evidence that quantum mechanics must be extended. One possible way to do this is suggested by his work on the amplituhedron, which casts quantum mechanical probabilities (and with them, unitarity) as emergent consequences of an underlying geometry. He plans to discuss this possibility in a forthcoming paper that will relate an analogue of the amplituhedron to non-Gaussianities in the sky.

  There has been further work on this by Kamionkowski and collaborators, and we're told:

  > Observing the signals predicted by Arkani-Hamed, Maldacena and Kamionkowski would be like striking gold, but the gold is buried deep: Their strength is probably near the gravitational floor and will require at least 1,000 times the sensitivity of current equipment to detect.

  No word on prospects for more than 1,000 times more sensitive experiments.

For about as long as I can remember, string cosmologists have been promising that their ideas would be tested by the Planck experiment. Now that negative results are in from that, we're told to instead look forward to a new generation of experiments. The ones mentioned in the article are SPHEREx and the LSST, with results next decade. SPHEREx claims that they may be able to push down bounds on non-gaussianity by a factor of 10, which would be an impressive result. This would rule out lots of string cosmology models, but of course there would still be plenty more.

The next experiment all involved are talking about is looking for signals in the 21cm hydrogen line, see Sabine Hossenfelder here for more about that. No time estimates on that one. As far as I can tell, the plan for this starts off with "First, build a base on the other side of the moon…"

Comments

1. **Bee**
   April 20, 2016

Peter,

The radio telescope on the moon is a dream experiment that, if it comes into being at all, will take decades of planning and construction. But there are already experiments measuring the 21cm line – like LOFAR and MWA – which I mention in my article (which you can btw read without the ad clutter here). So there will probably be first data in a few years.

And on the issue of experimental tests, I am (again) organizing a conference on quantum gravity phenomenology. (Which hasn't been announced yet, we only just got the website up.)

2. **M**
April 20, 2016

String models tend to make one "prediction": field excursions have sub-Planckian size. Unfortunately this implies that it's difficult to get inflation: the Lyth bound demands super-Planckian field excursions if the tensor/scalar ratio is large enough to be detectable. That's why most research in "string inflation" is inventing ad-hoc models that avoid the "prediction" and can give unusual features. Anyhow, the works that aim at reconstructing more primordial fluctuations from large-scale structures are interesting.

3. **Bayes_or_bust**
April 20, 2016

Peter,

What are your thoughts about Kane's preprint

The lightest visible-sector supersymmetric particle is likely to be unstable

Bobby S. Acharya, Sebastian A. R. Ellis, Gordon L. Kane, Brent D. Nelson, Malcolm J. Perry
(Submitted on 18 Apr 2016)

http://arxiv.org/abs/1604.05320

a generic prediction of ST: DM will be uncharged under SM gauge groups. The reasoning is actually simple, and doesn't require any technical details of ST. I'm not sure why no one's emphasised this point before.

4. **Shantanu**
April 20, 2016

Sabine: congratulations. I am happy you were able to procure funding for this meeting.
Peter: Jerome Martin wrote ~ 400 page review paper doing a laundry list of ALL models of inflation. Almost every BSM /QG theory has some model of inflation associated with it. Even I have written a paper on model of inflation arxiv:1510.08834 🙂 So I wish journalists take such string theory inspired models of inflation with a grain of salt.

5. **Peter Woit**
April 20, 2016

Bayes_or_bust,

This looks like the usual Kane way of doing science: for many years X is advertised as a "generic prediction" of string theory, about to be tested, then when X is not found, the discovery is made that "not X" is a "generic prediction" of string theory. The latest effort fits into the theme of this posting, with the new "generic prediction" of string theory that there won't be observable effects of string theory.

6. **AcademicLurker**
April 20, 2016

Slightly OT, so feel free to delete, but as a break from string theory hype, Matthew Buckley over at the Boston Review is doing a series of articles trying to explain quantum field theory to a general audience: http://www.bostonreview.net/books-ideas/matthew-buckley-search-new-physics-cern-part-2.

I'd be interested to hear Peter Woit's take on it.

7. **Peter Woit**
April 20, 2016

AcademicLurker,

Thanks, I had seen that, seems I forgot to mention it here. It's an interesting multipart series, with the idea to end later this year at the point when we find out if the 750 GeV bump is real, with the significance having been explained by the article. I'm glad to see somewhat of a trend towards trying to get some quantum field theory into popular articles, which is no easy task…

8. **Fred**
April 23, 2016

A question about the cosmological triangles sought (mentioned in Wolchover's article): since any trio of galaxies forms a triangle, is it more precisely *equilateral* triangles that will give a clue about inflation if they are found in greater number than predicted by chance?

For Your Reading Pleasure

April 21, 2016
Categories: Multiverse Mania

Since I just spent some of the morning not doing what I should have been doing, but reading about other things, in case you also want to do this, here are some options:

- I'm very excited to see an article at Smithsonian Magazine with the title Can Physicists Ever Prove the Multiverse is Real? (remember, answers always no to headlines). Unlike just about every other effort of this kind, the author (Sarah Scoles) brings up the obvious problems, quoting Carlo Rovelli:

> Some theoretical physicists say their field needs more cold, hard evidence and worry about where the lack of proof leads. "It is easy to write theories," says Carlo Rovelli of the Center for Theoretical Physics in Luminy, France. Here, Rovelli is using the word colloquially, to talk about hypothetical explanations of how the universe, fundamentally, works. "It is hard to write theories that survive the proof of reality," he continues. "Few survive. By means of this filter, we have been able to develop modern science, a technological society, to cure illness, to feed billions. All this works thanks to a simple idea: Do not trust your fancies. Keep only the ideas that can be tested. If we stop doing so, we go back to the style of thinking of the Middle Ages."

- John Horgan has a wonderful, very long, interview with Scott Aaronson. Highly recommended as a way to avoid work and learn all sort of interesting things from and about Scott, whose blog you should be reading anyway. If you want to discuss this, you likely can do so with the man himself here.
- If you just can't get enough of the multiverse, there's something else quite long available, a podcast of Sam Harris in conversation with Max Tegmark.

Comments

1. **Jeffrey M**
April 21, 2016

So in the Scoles piece Polchinski and Linde seem to argue that since the probability that everything comes out exactly the way it did in the universe, which makes life possible, is so low, there must be an infinite number of universes out there. Do they really not understand basic probability theory? Probability doesn't apply to single instances. Not that way. Why exactly doesn't anyone call them on this? Suppose you saw a random number generator, and you knew it generated numbers from 1 to 1000000. You see a 1 on it, does that tell you anything? No. It certainly doesn't tell you that there must be millions of random number generators out there since you happen to live with the one which is showing a 1.

2. **Peter Woit**
April 21, 2016

Jeffrey M.,
This is by now an ancient argument, I don't see any possibility of anything new being said about it, so would like to discourage anyone who wants to carry it on. You need a larger framework than our current physics to make sense of any of these issues of what is likely and what isn't. Linde et al. have one and are promoting it, the problem is that it's an empty, untestable one. Rovelli is right that this is going back to pre-scientific styles of thinking.

3. **John Fredsted**
April 22, 2016

A tiny correction: I guess 'Every' in the link to the article at Smithsonian Magazine should read 'Ever'.

4. **L**
April 22, 2016

Now, if we could stop addressing the "Middle Ages" like an intellectual wasteland, that would be great.

5. **Peter Woit**
April 22, 2016

John Fredsted,
Thanks, fixed.

L,
Good point. Whatever the failings of medieval theologians, I suspect they would have recognized the problems with multiverse explanations.

6. **Phil Koop**
April 22, 2016

I don't think that multiverse speculation is "an intellectual wasteland" a consequently I don't agree that comparing the speculators to medieval thinkers implies that the pre-modern era was an intellectual wasteland. One needs only a nodding acquaintance with history to be aware that medieval thought was rich, varied, and intricate. Indeed, the usual complaint of the student is that it was all too intricate.

But the fact remains that there is a basic qualitative difference between scientific and non-scientific modes of thought, and that the multiverse falls on the non-scientific side of the line. This is not to say that pre-modern modes of thought have disappeared, nor that scientific thought has no medieval or indeed classical antecedents. But today we (ideally) prefer to make decisions with a mode of thought that was alien to our forbears.

I really like Ada Palmer's treatment of this intellectual history in her "Sketched of a History of Skepticism" series (first post here: http://www.exurbe.com/?p=2725.)

> Science has not replaced religion–they coexist happily, productively, even symbiotically within many arenas, places and individuals, even as they chafe and vie in others. But in the modern West, the Scientific Method has largely displaced older systems for guiding daily micro-decision-making which were more closely tied to religion. We now use science-based reasoning a hundred times a day when we are called upon to make decisions. Whether making a sandwich, buying a new teapot or evaluating an argument, we think about data from past experiences, bring in what facts and hypotheses we have accumulated from educated and informed living, consider the credibility of sources, ask ourselves questions about plausability, probability, evidence and counterargument, speculate about the range of possible errors and outcomes …
>
> For my purposes today, the most important part of what I just described is that the belief or disbelief we extend to the politician (or to our teapot) is provisional …

7. **L**
April 23, 2016

The fact is that, despite the common conception, there's more in the Middle Ages than theologians. Universities were born, with its basic curriculum centring on logic in the trivium and four mathematical disciplines on four in the quadrivium (arithmetic, astronomy, geometry and music, seen as applied proportions); people studied, even empirically, every area of "natural philosophy"; in physics we have medieval thinkers derive the basics of dynamics and kinematics, discovering results often attributed to Galileo and his peers, and inquire the possible motion of the Earth, in addition to refining astronomical models; and in general, these people laid the basis of the scientific method.

A good short introduction on the matter can be read here.

8. **Denis Boers**
April 25, 2016

Peter,

I am a long time reader of yours, both your blog and the book,
and I am delighted to see your insistence on intellectual responsibility
is gaining traction. See for instance : https://arxiv.org/pdf/1604.06773

Keep up the good work, for science's sake !

Denis

Group Theory in a Nutshell for Physicists

April 22, 2016
Categories: Book Reviews

There's a new book out in the Princeton "Nutshell" series, Tony Zee's Group Theory in a Nutshell for Physicists. I liked his Quantum Field Theory in a Nutshell quite a lot, it's packed with all sorts of insights into that subject. Both books are written in a very light, chatty and entertaining style, full of various sorts of worthwhile digressions. Long ago I tried to use the QFT book in a course I was teaching, and there found that it functions better as a supplement to a standard QFT book. Zee's treatment is just too short, covering too much, to provide the level of detail most students need to learn the subject for the first time. For people who already have gone through a standard book or course, Zee's QFT book is great as a follow-on, likely to explain a lot of things they found confusing the first time around.

The new book on group theory has a length much better matched to the amount of material (it's longer than the QFT book, and the material covered is much less complicated). The level of detail for most topics should be a good amount for students encountering the subject for the first time. The main topics covered are:

- Finite groups and their representations.
- Unitary and orthogonal groups, their representations, and applications to quantum mechanics.
- Classification of simple Lie algebras.
- The Lorentz and Poincare groups and their representations, with a discussion of the Dirac equation and Weyl and Majorana spinors.
- A grab-bag of some other topics, including a little bit about conformal symmetry and grand unified theories.

While for each of these topics there are other good textbooks out there, this is a great selection for an advanced undergraduate/graduate physics course. I expect this to justifiably become a popular choice for such courses.

While I liked a lot about the book, I have to confess that there were things about it that did put me off. Some of this likely has to do with the fact that I've been working for the last few years on a book (see here) that covers some of the same topics, so I'm hyper-aware of both the technicalities involved, and the issues that arise of how best to approach these subjects. In addition, much of these topics is standard core mathematics, but Zee seems to have consulted few if any mathematicians (at least I didn't recognize any in his acknowledgements). Unlike some others, this is a subject where mathematicians and physicists really can communicate and teach each other a lot.

Some of the choices Zee makes that I don't think are good ones are things that input from mathematicians probably would have helped with. Maybe the most egregious is his decision to use the same notation for a Lie group and its Lie algebra, on the grounds that physicists sometimes do this, and to notationally distinguish the two in the usual way (upper vs. lowercase letters) is "rather fussy looking". Using the same notation for two very different things is just asking for confusion, and I remember struggling with this as a student. Zee is well aware of the problem, on page 79 having his interlocutor "Confusio" say:

> When I first studied group theory I did not clearly distinguish between Lie group and Lie algebra. That they allow totally different operations did not sink in. I was multiplying the Js together and couldn't make sense of what I got.

Please, if you're using this book to teach students about this subject, discourage them from following Zee in this choice.

~~There are places in the text where Zee gets things wrong in a way that just about any mathematician could likely have saved him from. One minor example is a footnote saying "Mathematicians have listed all possible finite groups up to impressively large values of n" (actually, they're classified for ALL values of n)~~ *(my mistake, I misread and wasn't looking at the finite group chapters carefully enough. Zee does get this right).*

One place Zee gets things wrong is when he writes down the Heisenberg commutation relations, and says this is an "other type of algebra", off-topic "since this is a textbook on group theory, I talk mostly about Lie algebras". Actually those are the commutation relations of a Lie algebra, the Heisenberg Lie algebra, and there's a group too, the Heisenberg group.

This gets into my own prejudices about the subject, with the story of the Heisenberg group to me (and I think to most mathematicians), a central part of the story of quantum mechanics, something little appreciated by most physicists. Another place where I think Zee goes wrong due to current physics prejudices is in ignoring Hamiltonian mechanics in favor of Lagrangian mechanics. As a result, instead of being able to tell the beautiful story of the Lie algebra of functions on phase space and what it has to do with conservation laws, he just mentions that Noether's theorem leads to conservation laws and refers elsewhere for a discussion. The connection between symmetry and conservation laws is one of the central parts of the connection between Lie groups and physics, and deserves a lot more attention in the context of a course like this.

So, in summary, the book is highly recommended, with the caveats that you absolutely shouldn't use the same notation for Lie groups and Lie algebras, and you should supplement Zee's treatment with that of a certain more mathematically-minded blogger…

Comments

1. **edmeasure**
   April 22, 2016

   So when (and where) will we see Peter Woit's version in print?

2. **Peter Woit**
   April 22, 2016

   With a little bit of luck, late this year or early next year from Springer. The current version available online is mostly complete, except for ongoing work on a few of the last chapters.

3. **Fernando**
   April 22, 2016

   All finite groups? No, only all finite simple groups.

4. **Peter Woit**
   April 22, 2016

   Fernando,
   Thanks. My mistake, I didn't look carefully enough, he does get this right. Will fix.

5. **anon**

April 22, 2016

At least as of year 2008, the number of (non-isomorphic) groups of order 2048 was not precisely known (the number is known for all smaller orders): see http://www.math.auckland.ac.nz/~obrien/research/gnu.pdf

6. **Peter Woit**
April 22, 2016

anon,
Thanks. I was being quite unfair to Zee about this, my misreading, due to not looking all that carefully at the finite group chapters. This is a topic he covers in quite a bit of detail, in a manner closest to the way mathematicians teach the subject.

7. **ronab**
April 22, 2016

I actually found Zee's QFT book extremely hard to read: Repeatedly, I'd be sinking my teeth into some interesting physics, when I'd be suddenly distracted as I hit upon yet another of his pointless anecdotes about some physicist's personal habits, completely breaking my concentration. These are scattered everywhere so you really can't avoid them. If this had been an online version I'd have suggested that at least a "Show Bullshit / Hide Bullshit" button might have helped.

8. **Peter Woit**
April 22, 2016

ronab,
There's a fair amount of that in the new book also. You can however miss a lot of it by ignoring the footnotes…

9. **theoreticalminimum**
April 23, 2016

I saw that this is Zee's 3rd contribution to PUP's "Nutshell" series, his 2nd being a book on general relativity, wherein he even has a small intro to twistors. I think what's generally interesting about reading Zee's books is the beautiful subtle insights he provides on different topics that one has been exposed to via standard courses supported by standard textbooks in graduate school. All this to say that I am eagerly waiting for his group theory book to show up at my doorstep soon!

10. **Ryan**
April 23, 2016

Although there are already several very good books on group theory for physicists like Robinson's "Symmetry and the Standard Model" or Schwichtenberg's "Physics From Symmetry", I think, it's great that Zee took the time to write this book. However, as with his QFT book, I think, it is better suited as a complementary book. In my humble opinion, he covers too many advanced topics, which aren't essential for beginners. Especially many beginners may feel the need to understand all these advanced topics before reading on and thus give up before they read about the most important topics, such as spinors etc. .

That said, after reading a quicker introduction to the essentials, it's great to read Zee's special perspective and his enjoyable explanations of advanced topics. As you already noted, exactly as the other books mentioned above, it lacks mathematical rigor and that's why I'm eagerly waiting for your book Peter, to fill this gap!

11. **simplicio**
April 23, 2016

Anyone have thoughts on Zee's Relativity book? I used his QFT book in conjunction with my class text (Peskin and Schroder) for my QFT class and found that it made the going a lot easier. I was thinking I might try and go through his Relativity book this summer, since I never had a class on the topic and wanted to learn.

12. **David**
April 23, 2016

His relativity book is excellent. I've tackled several other intro books on the subject (Hartle, Carroll, Schutz), and I'm not a physicist, just an amateur. Zee takes a unique approach, building up the mathematics of curvature in conjunction with variational principles and just enough group theory. The sections in which he derives classical mechanics from variational principles and symmetry considerations helps consolidate your math skills before diving into curved spacetime. He gives an outstanding exposition of how the field equations follow from the Einstein-Hilbert action. Overall, this book added a lot to my level of understanding.

Some caveats: the modern trend is to develop GR using index-free forms; Zee's book is almost entirely based on index notation. That's both a strength and a weakness. The last quarter of the book is a less-than-rigorous survey of some frontier topics that I found to be pretty worthless. If you don't like the dialogues with Confusio, you will find minor annoyances scattered throughout (though they do serve the purpose of letting the author point out common misconceptions and their antidotes).

13. **David Metzler**
April 23, 2016

A bit OT: your readers might be interested to know that the LHC achieved its first stable beams of the year yesterday, albeit with a tiny intensity.

https://indico.cern.ch/event/506406/

It seems that they are more or less on schedule, if I'm reading things correctly. Peter, do you know if last year's problem with the CMS detector is still around and likely to seriously degrade its performance, or not?

14. **Peter Woit**
April 24, 2016

David Metzler,
As far as I know, CMS has successfully dealt with that problem, see a relatively recent report at http://science.energy.gov/~/media/hep/hepap/pdf/201603/Olsen_CMS_HEPAP_April_1.pdf

15. **Frank Wilhoit**
April 24, 2016

This review is a prime example of the kind of thing that reviews ought to do, but in my view the conclusioon that the book is "highly recommended" is in conflict with the bulk of the review.

Zee is a good writer — and good writers need to be *extra* careful to get things right, because what they say is more memorable and more persuasive.

In my experience, teaching at the undergraduate level in a different field, it is *not* at all straightforward for the classroom instructor to try to correct the book. Patently bad books make this easier. Books that are well written and well packaged gain, thereby, an increment of credibility and auctoritas. For all these reasons, this sounds like a book I would stay away from.

16. **John McAllison**
April 24, 2016

Personally, I love the way Tony Zee adds colour to his texts via his interesting and sometimes hilarious anecdotes which creates a link to what's being learned. This is from his General Relativity book concerning the teaching of tensors:

Long ago, an undergrad who later became a distinguished condensed matter physicist came to me after a class on group theory and asked me:

'What exactly is a tensor?'
I told him that a tensor is something that transforms like a tensor.

When I ran into him many years later, he regaled me with the following story. At his graduation, his father, perhaps still smarting from the hefty sum he had paid to the prestigious private university his son attended, asked him what was the most memorable piece of knowledge he acquired during his four years in college. He replied:

"A tensor is something that transforms like a tensor."

17. **Peter Woit**
April 24, 2016

John McAllison,
The same story is in the new book.
One of the things that does grate about the book to a mathematician's sensibilities is that Zee uses the tensor product symbol for just about every kind of product, except the tensor product. Given the kind of confusion indicated by the story he likes, I don't see why he (and other physicists), don't just define the tensor product.

18. **Ryan**
April 25, 2016

@Peter Do you have a reference where the correct usage of the tensor product $\otimes$ vs. the direct product $\times$ is explained for physicists? Or maybe some quick example, where Zee gets this wrong and should use another symbol instead?

19. **Peter Woit**
April 25, 2016

The tensor product is a product of vector spaces, the things Zee does that I was referring to are
1. use the tensor product symbol for a product of groups (SU(3) times SU(2) times U(1))
2. use the tensor product symbol for the cross product of vectors (angular momentum is x times p). This is an unusual usage.

This distinction between types of product can become pedantic. The more substantive question in my mind is that of why he avoids use of the tensor product. I can see why physicists don't want to use the abstract, coordinate invariant definition, but the definition in terms of a basis is straightforward.

20. **Johnny**
April 25, 2016

I think that the fact that mathematically minded readers disagree with the choices he makes would only please Zee. He writes for physicists and is proud of the way physicists deal with pedantic issues.

21. **Peter Woit**
April 25, 2016

Johnny,
I'm sympathetic with Zee on avoiding pedantry, but things like distinguishing a Lie group from a Lie algebra are not pedantic distinctions, and one really confuses students by acting like they are by using the same notation for both.

22. **Johnny**
April 25, 2016

I haven't seen the book so I can't really judge, but I would be very surprised if Zee doesn't explain clearly that they are separate things, adding that it should be clear from context which is which in every case he discusses.

23. **Peter Woit**
April 25, 2016

Johnny,
That's exactly what he says. Of course he's aware that they are different things. This doesn't change the fact that using the same notation to mean quite different things and expecting your reader to figure out for themselves which one you're talking about is a bad idea. Especially when you're dealing with students trying to learn the subject for the first time.

24. **Jeff M**
April 25, 2016

He really uses the same notation for the Lie algebra and the Lie group? Pedantic mathematician that I am, I am appalled…

25. **A.J.**
April 25, 2016

Et's leke useng the same symbol for 'i' and 'e' when spelleng Englesh words. Weth suffeceent effort, your readers can fegure out what you meant. But there's no benefet to doeng et, et makes errors more lekely, and et makes you sound leke an edeot…

26. **Conductor**
April 26, 2016

"Et's leke useng the same symbol for 'i' and 'e' when spelleng Englesh words. … et makes you sound leke an edeot…"

Only if you're reading aloud.

27. **Justin**
April 26, 2016

I'm a layman, with no physics. I understand algebra at the level of Fraleigh from my undergraduate days. I'm interested in learning the representation connection to quantum mechanics, and was wondering how I could go about studying the prerequisites to Zee's book.

28. **cthulhu**
April 27, 2016

Anybody have opinions on how Zee's books compare to Susskind's "Theoretical Minimum" books? As an engineer who agonized between majoring in physics vs aerospace engineering in college, I am still quite interested in physics, and have enjoyed the Susskind books (although I'm not all the way through the quantum mechanics book, and there's no GR book yet). So I'm always on the lookout for stuff that can be understood by an engineer who made through tensor calculus and calculus of variations, even if I'm a bit rusty 🙁

29. **Johnny**
April 27, 2016

A.J. I don't think your example is good (it's not even funny). A closer example (not perfect though) would be to use the same symbol "x" to multiply numbers and vectors. Truly appalling, ha?

30. **Peter Woit**
April 27, 2016

Justin,
Zee's book is aimed a people with a significant background in physics, only real math background needed is linear algebra (which he reviews at the beginning). I don't think it's that useful if you don't already have a course in quantum mechanics.

Cthulhu,

Zee's is quite a bit more advanced that Susskind's, with Susskind explicitly trying to write for people with minimal background, Zee's book developed out of a course for advanced undergrads and beginning graduate students, and is more at that level.

Various Items

April 29, 2016
Categories: Uncategorized

A few short items:

- Things had been going quite well at the LHC, they were ahead of schedule, starting to ramp up intensity for the new run. Then at 5:30 this morning a weasel decided to visit a 66kV transformer, which did not end well either for the weasel or for the LHC power grid. The machine and a lot of its cryogenics lost power, and recovery is going to take a week or so.
- For some commentary on the excitement about the new run building up (pre-weasel), see Tommaso Dorigo (at least I'm guessing he's the author) here. He points to the twitter #MoarCollisions hashtag.
- In various Breakthrough Prize related news, first there's an announcement from Terry Tao about the new IMU Graduate Breakout Fellowships, funded by him and some of the other math prize winners.

  On the physics front, Caltech has Glitz and Qubits, about Alexei Kitaev and John Schwarz's experience with the prize. Schwarz still hopes for vindication of his string theory prize by a discovery of SUSY at the LHC, assigning a much higher probability to this than I think most other people would these days:

  > I would say the probability is on the order of 50 percent or so that it will show up.

  As for the glitz:

  > At the 2014 award ceremony, Schwarz says, he and his wife, Patricia, were "both struck by the fact that the Hollywood types showed no interest in mingling with scientists." And the media coverage also seemed to focus on the movie stars rather than the award winners.

  Kitaev points out a major positive effect of the $3 million: more respect from one's family:

  > But for Kitaev, the biggest impact of awards like the Breakthrough Prize in Fundamental Physics is on his family. "They don't really understand what I'm working on," he says. But thanks to these awards, they at least realize his research is a pretty big deal. "It helps me do more work because they have more respect for it," he says. "My wife is really proud of me."

- In other big money news the Perimeter Institute and the Stavros Niarchos Foundation have announced a new professorship for Asimina Arvanitaki, funded by $8 million. More about her here.
- At Nautilus you can read an interview with IAS director, theoretical physicist Robbert Dijkgraaf.
- At Edge, there's a conversation with Frank Wilczek. I'm quite curious what the following is about, have no idea:

  > What I've been thinking about today specifically is something of a potential breakthrough in understanding our fundamental theories of physics. We have something called a standard model, but its foundations are kind of scandalous. We have not known how to define an important part of it mathematically rigorously, but I think I have figured out how to do that, and it's very pretty. I'm in the middle of calculations to check it out.

Update: Two more:

- Jim Baggott has a post on Status Anxiety, with more thoughts about the Munich conference and the use of the term "theory".
- Discover magazine has an article in the upcoming June issue on The Fall and Rise of String Theory. The story seems to be that String theory for some reason ran into a little trouble in 2006, but now it's back, because Strominger has done some "string-inspired" black hole calculations, and some people are claiming inspiration from AdS/CFT for an approximate calculational method in some condensed matter models. The idea now seems to be that, starting from this, string theory is on its way to again finding a theory of everything. No comment on its hype problem.

Update: A special 2016 physics Breakthrough Prize has been awarded to the LIGO people. \$1 million split by Drever, Thorne and Weiss, \$2 million for the rest of the collaboration.

Comments

1. **John Sirois**
   April 29, 2016

   From context below that quote, particularly:
   "Who does this falsify? It's a funny situation where the theory of electroweak or weak interactions has been successful when you calculate up to a certain approximation, but if you try to push it too far, it falls apart. Some people have thought that would require fundamental changes in the theory, and have tried to modify the theory so as to remove the apparent difficulty. What I've shown is that the difficulty is only a surface difficulty…."

   A guess would be a "solution" for Landau Poles.

2. **tommaso dorigo**
   April 29, 2016

   Hi Peter,

   yes, the post at the AMVA4NewPhysics site was mine – good guess! And thanks for the link.

   About the weasel: it will cause a few days of delay, but by now we've gotten accustomed to the fact that our big machine can be stopped by improbable causes such as saving on resistors, bombing it with baguettes, and now munching on wire cables. I think the cause of the next technical stop is hard to predict, but I am a betting person so I offer the following:

   – sewage water flooding of the ATLAS cavern, 10:1
   – growth of fungus on the inner walls of the beampipe, 20:1
   – hijacking of the LHC control room by Corsican independentists, 15:1

   Cheers,

T.

3. **Anonyrat**
April 29, 2016

Further context from below the Wilczek quote:

> "**This thing that I mentioned before about what I was thinking about and am excited about is making the foundations of the standard model more secure.** This problem that we're addressing has been a worm in the rose for decades that has been worrying people. Most people don't want to think about it; they think it's somehow going to resolve itself.
>
> It looks very technical, but it's been there and it's been annoying for those of us who care about logical consistency."

So I think John Sirois' guess is a very good one.

4. **Low Math, Meekly Interacting**
April 29, 2016

I read somewhere the baguette thing is apocryphal. I, of course, want desperately for the the story to be true. Is it?

On the subject of animal attacks, some early pioneer of electronic music, Allen Strange, had a band, The Electric Weasel Ensemble. Perhaps this hapless mustelid was a fan.

5. **Abbyyorker**
April 29, 2016

Landau pole is a u(1) thing as I recall. I would guess that that would not be a fundamental enough problem to motivate Wilczek, assuming he thinks it is possible that it is contained in a GUT. But maybe he doesn't.

6. **Anonyrat**
April 30, 2016

Well, it is the QFT problems with a fundamental scalar like a Higgs, whether you call it Landau poles or quantum triviality.

7. **Nick M.**
April 30, 2016

If Frank Wilczek's idea pans out, it might mean a sort of *"mini retreat"* for certain sectors of the multiverse camp as well:

> "Who does this falsify?…….It falsifies speculative theories that have been trying to cure a problem that doesn't exist. It's things like certain kinds of brane-world models, in which people set up parallel universes where that parallel universe's reason for being was to cancel off difficulties in our universe—we don't need it."

8. **Mitchell Porter**
May 1, 2016

I think Wilczek is talking about naturalness and Randall-Sundrum.

9. **Stephane**
May 1, 2016

There is also a good article by Wilczek about "Entanglement Made Simple" in Quanta Magazine : https://www.quantamagazine.org/20160428-entanglement-made-simple/

10. **Gaucho**
May 1, 2016

Nothing about what Wilczek says really points to Landau poles or the multiverse or naturalness. Rather, it sounds like he is referring to the lack of a nonperturbative regulator for chiral gauge theories in d=4. The regularization of theories with global chiral symmetries currently involves domain wall fermions ("parallel universes where that parallel universe's reason for being was to cancel off difficulties in our universe"), and Grabowska and Kaplan have recently made a proposal to extend these techniques to gauge theories using gradient flow. The lack of a nonperturbative regulator for chiral gauge theories is certainly a situation where it was "not known how to define an important part of it mathematically rigorously." I would guess that this has motivated Wilczek to revisit the problem. Curious to see what he comes up with.

11. **Peter Woit**
May 1, 2016

Gaucho,
Thanks, your guess sounds quite plausible. I've never understood why this problem gets so little attention.

12. **srp**
May 1, 2016

How many times do English-speaking physicists complain that their progress is being held up by weasels? Funny that this time it is literal.

13. **Low Math, Meekly Interacting**
May 2, 2016

My sick sense of humor notwithstanding, it looks as if things ended quite poorly indeed for the sleekit saboteur. There may not have been enough of it left to determine whether it was a weasel or a marten. Perhaps also of interest: apparently the Tevatron suffered a raccoon raid in 2006, which was bravely repelled by Fermilab operators. Casualties were reportedly low, and the machine was unscathed.

http://www.npr.org/sections/thetwo-way/2016/04/29/476154494/weasel-shuts-down-world-s-most-powerful-particle-collider

14. **Visitor**
May 2, 2016

Raccoons have a very high incidence of rabies and are the single largest vector for rabies in human beings:

"Wild animals accounted for 92% of reported cases of rabies in 2013. Raccoons continued to be the most frequently reported rabid wildlife species (32.3% of all animal cases during 2013), followed by bats (27.2%), skunks (24.6%), and foxes (5.9%)." (from http://www.cdc.gov/rabies/location/usa/surveillance/)

15. **Other**
    May 2, 2016

    The baguette story is indeed true. On a related note, two beer bottles were previously found in the LEP beam line.

    http://blogs.nature.com/news/2009/11/exclusive_interview_baguette_b.html
    http://blogs.nature.com/news/2009/11/_a_tale_of_two_beer_bottles_1.html

16. **phillipe**
    May 3, 2016

    Jim Baggott link doesn't work.

    Scientific American article is behind a paywall.

17. **Peter Woit**
    May 3, 2016

    phillipe,
    There seems to be some temporary problem with Jim Baggott's web site, presumably it will get fixed, so try later. The Discover magazine article is mostly behind a paywall. Columbia provides electronic access via our library to Discovery magazine content, unfortunately I don't know of other options.

18. **Shantanu**
    May 3, 2016

    Peter and others: Those of you who have electronic access to Discovery magazine, please write to the author of this article to point out about the hype. Else such non-sense articles will continue.
    PS: have you seen any article/press release/statement arguing that LIGO results are evidence for string theory?

19. **Peter Woit**
    May 3, 2016

    Shantanu,
    The Nadis article comes pretty close to making the LIGO string theory connection, here's some of it (fair use…)

    "Using similar techniques originally inspired by string theory, Strominger's group has computed the spectrum of gravitational waves emitted when compact objects like stars fall into giant black holes — predictions that could be verified by the future Evolved Laser Interferometer Space Antenna, planned to launch in two decades (or maybe sooner). Strominger also believes that evidence of conformal symmetry might emerge from the Laser Interferometer Gravitational-Wave Observatory, which spotted gravitational waves for the first time earlier this year. Soon, he says, astronomers may be drowning in data that they cannot fully interpret. "We'd like to use ideas from string theory to shed some light on corners of this."

    At this point, I guess any use of conformal symmetry from now on will be described as an "idea from string theory".

20. **Peter Woit**
    May 3, 2016

    Shantanu,
    After taking another look at the article, I decided it was worth it's own "This Week's Hype" post, see the next posting.

This Week’s Hype

May 3, 2016
Categories: This Week's Hype

The June issue of Discover magazine has an article entitled The Fall and Rise of String Theory (sorry it’s behind a paywall).

I had added this as an update to the last posting, but just looked at it more carefully and realized that it’s squarely in the “string theory predictions” tradition covered by editions of “This Week’s Hype”. In particular, the article claims:

> The upcoming Evolved Laser Interferometer Space Antenna could help verify string theory’s predictions of gravity waves. Three spacecraft will orbit around the sun and measure tiny ripples in space-time via sensitive lasers.

The article starts off by explaining the history of string theory this way:

> String theory was once the hottest thing in physics…
>
> Strominger knew, even in the euphoric ’80s, that such assertions were overblown. And, sure enough, skepticism has seeped in over the years. No one has yet conceived of an experiment that could definitively verify or refute string theory. The backlash may have peaked in 2006, when several high-profile books and articles attacked the theory. But while string theory has receded from the spotlight, it has not gone away. “The theory is still evolving and getting better — and better understood,” maintains Juan Maldacena of the Institute for Advanced Study at Princeton University….
>
> Emerging from this diverse work is a new consensus: String theory may not be the fabled theory of everything, Strominger says, “but it is definitely a theory of something.”

The article’s main selling point for string theory is that it led Strominger to think about something else, ideas about the conformal symmetry of black hole solutions.

> Strominger subsequently realized that the presence of this symmetry, which hadn’t been recognized before, could be used to support a range of predictions. For example, he and his collaborators are currently trying to calculate the intensity of electromagnetic radiation emanating from the vicinity of a black hole. In a few years, Strominger says, once the worldwide network known as the Event Horizon Telescope comes online, astronomers can test those radiation estimates through direct measurements.
>
> Using similar techniques originally inspired by string theory, Strominger’s group has computed the spectrum of gravitational waves emitted when compact objects like stars fall into giant black holes — predictions that could be verified by the future Evolved Laser Interferometer Space Antenna, planned to launch in two decades (or maybe sooner). Strominger also believes that evidence of conformal symmetry might emerge from the Laser Interferometer Gravitational-Wave Observatory, which spotted gravitational waves for the first time earlier this year. Soon, he says, astronomers may be drowning in data that they cannot fully interpret. “We’d like to use ideas from string theory to shed some light on corners of this.”

From now on, I guess use of conformal symmetry in physics now likely to be sold as a “prediction of string theory”.

The article goes on to a different area of string theory hype, that of AdS/CMT. According to Andrew Green of University College, London, string theory is “the new calculus”, and according to the piece’s author “Strominger agrees.” It’s hard to come up with appropriate words to characterize this level of hype.

Comments

1. **Physics Professor**
   May 3, 2016

   Good news! String theory thusly predicts another twenty years for this blog to have meaning and purpose!

The Big Picture

May 3, 2016
Categories: Book Reviews

I'm about to head out on vacation to commune with nature in the Pacific Northwest, so there's likely to be no more blogging here until after May 17th. That's just one reason I'll leave comments closed here, another is that I'm sure you can find other, better, places on the internet to discuss the issues raised in this posting.

A little while ago I bought a copy of Sean Carroll's new book The Big Picture, which is now reaching the bookstores. This posting is not really a review of the substance of the book, but more a reaction to its basic conception. The first point to make is that I mostly agree with what Carroll has to say, to the extent that one reason for not writing a more usual sort of review is that I didn't bother to do more than skim a lot of the chapters, since the theme seemed both so familiar and so unobjectionable. One exception would be a small number of pages about the multiverse, which he contrasts with religion, ending with (referring to religion)

> This is the problem with theories that are not well-defined.

He's got the problem right, but doesn't notice that it applies equally well to this particularly dubious bit of "science".

The largest part of the book (from my rather quick read) is a very conventional argument for science as opposed to religion, of a sort that has existed for centuries, been common since the 19th century, and very common in recent years as part of the "New Atheism". One reason I can't focus on this is that I just don't see any evidence that science needs this sort of defense against religion, it seems to me to be doing extremely well without it. Our culture valorizes science and scientists very highly these days (much more so than ministers or theologians), and I just don't see what some other people see as a need for books arguing the case for science.

The really striking thing about this particular book though is that Carroll has a much more unusual and ambitious goal than just arguing for science. He wants to promote what he calls "poetic naturalism", which as far as I can tell is a term of his own invention ("naturalism" by itself is now a conventional term for the "science, not religion" viewpoint). Beyond the "science instead of religion" idea though, "poetic naturalism" seems to me to simultaneously lack any real content, while claiming to address the deepest human questions of meaning and morality. Asked in this interview the question

Your book, The Big Picture, roams far beyond cosmology and physics, into consciousness, philosophy and the meaning of life. What do you hope to achieve?

he answers

Well, this is the book that should accompany the Gideons Bible in all hotel rooms in the world – that would be a nice achievement!

I'm not sure what Carroll might have had to do with this, but poetic naturalism is now listed on Facebook as a possible choice for one's religion.

Perhaps the strangest thing in the book is a chapter devoted to Carroll's replacement for the Ten Commandments, which he calls the "Ten Considerations", since they're not commandments. They are:

- Life Isn't Forever.
- Desire Is Built Into Life.
- What Matters Is What Matters To People.
- We Can Always Do Better.
- It Pays to Listen.
- There Is No Natural Way to Be.
- It Takes All Kinds.
- The Universe Is in Our Hands.
- We Can Do Better Than Happiness.
- Reality Guides Us.

It's hard to argue with such sentiments, but also hard to understand what they have to do with the author's expertise as a theoretical physicist.

The last chapter of the book begins with a description of Carroll's early experiences in the Episcopal church, which he was quite fond of. I also had such experiences (I was an altar boy for several years at an Episcopal church, the American Cathedral in Paris). Unlike Carroll, I was never a believer, but just figured this was one of quite a few mystifying things that adults got up to, and that it seemed I had to go along with it until I got older. Thinking back to those days, I was struck by the realization that I recognized the tone and a lot of the content of Carroll's writing. It very much sounds like a sermon, one evangelizing the good news not of Jesus, but of science, and is aiming for much the same effect: "I want to shiver with awe and wonder at the universe".

My own point of view on all of this is that I just don't think theoretical physicists have anything useful to tell the average person about meaning and morality, other than that it's a mistake to search for it in our discoveries about physics. I don't understand why we're increasingly seeing texts promoting physics as inspiration for how to live (for another recent example among many see here). I'm with Steven Weinberg, and his famous line

> The more the universe seems comprehensible, the more it also seems pointless.

Given that, the best advice to people who come to physicists looking for the meaning of life seems to me to politely tell them that they're looking in the wrong place and asking the wrong person.

While I deeply value and respect my many friends and colleagues among physicists and mathematicians, I can't imagine why anyone would think they have any unusual insight into the great questions of meaning and morality. I'm afraid that to some extent the opposite is true. My background, career and circumstances are quite similar to Carroll's, and when I think about them my main thought these days is that I lead an extremely lucky and privileged (and not just in the white/Anglo/male/hetero sense) life, well-isolated from many of the challenges that most people have to deal with. There are a lot of beautiful, wonderful, and useful things one can learn from physicists and mathematicians, but our expertise is in something very far-removed from the question of how to live a good life in the face of significant challenges.

It seems likely that one motivation for books with this defensive attitude about science is the current ugly environment of our politics and culture. This ugliness I believe is driven by the economic disaster that has been inflicted on a significant fraction of our citizenry over the last few decades by the privileged and well-educated, both Democrats and Republicans. While Carroll and I were enjoying our respective times at Harvard and similar places, and have ended up turning our upbringing and Ivy League experiences into a very pleasant and cosseted lifestyle in the wealthy enclaves along either coast, things have not been going so well for many others. They're now in bitter rebellion against what has happened to them, with an anger sadly turned against other racial groups, but even more so against self-satisfied elites. I don't think a book like this has much hope of speaking to such people, to their view of science or their experience of religion. Scientists who want more respect should stick to what they know, and avoid the temptation of "science-splaining" to the public. In particular they should avoid preaching about meaning, morality, and other issues that they know no more about than anyone else.

Update: Robert Crease has a review of the book in Nature. He also finds odd the "greeting-card-like homilies" that appear in the book.

No Comments

Back

May 18, 2016
Categories: This Week's Hype, Uncategorized

Now back after a satisfying vacation amidst very large trees. Here are some things of note from the past couple weeks:

- For those fascinated by the arguments over string theory, you might want to look at a document sent to me by Ilyas Khan, The People vs. String Theory. It's also available in a free Kindle version, here. Some claim characters in this are recognizable to those well-versed in the subject.
- One thing that has always annoyed me about popular accounts of string theory is that they often claim that known particles are just like vibrational modes of a physical string, bringing music into it, as an argument for the beauty of string theory. No one ever mentions that the analogs of physical string vibrational modes have nothing to do with observed particles. If they exist at all, they're some sort of Planck-scale states. Known particles are modeled typically by zero modes, with the classical analog not playing your guitar strings, but picking up the guitar and carrying it around, a much less musical activity.

  I don't remember ever bothering to make that argument publicly, because it seemed likely to lead nowhere but to silly arguments from string theorists. I'm now glad to see that 4gravitons has taken up the issue with a blog entry Particles Aren't Vibrations. And, yes, check the comments for the expected response.

- Kudos to John Horgan for his talk at a recent Science and Skepticism conference here in New York. I've never quite understood why conferences like this seem devoted to a defense of ideas about science that are pretty much mainstream, especially in a place like New York, while ignoring pseudo-science when it comes from people considered members of the pro-science tribe. Horgan has some discussion of reaction to the talk here.
- Maybe this should have its own entry for This Week's Hype, but I'll just mention here that the June Scientific American has The Collider That Could Save Physics. It seems that SUSY is needed to "save physics". Way back when it was LEP that was going to "save physics" by finding SUSY, then it was to be the LHC. This year's LHC run should put the final nails in that coffin (data is now starting to be collected, see for instance here). Unfortunately the reaction of many SUSY partisans is not to follow the usual norms for how science is supposed to work and give up on the idea, but instead to claim that the LHC results aren't conclusive, and a new machine is needed. In the SciAm article the ILC is advertised for this task. This electron-positron machine would have a much lower center of mass energy than the LHC, but one can find obscure SUSY models specially designed to have states that would be hard to see at the LHC, but could be seen at the ILC. I hope this isn't the best argument for the $10 billion ILC…
- The L-functions and Modular Form Database is up and running now, providing a wealth of data about a central part of modern mathematics. Persiflage has an expert's take on the significance of the project, including some criticism of the hype surrounding its launch (non-zero, but quite small on any scale used to measure theoretical physics hype). Other experts weigh in in the comment section, so don't miss that.

Update: One more I forgot to add. Some people at Rutgers have decided to show what can go wrong when you have the Templeton Foundation funding "philosophy of physics". They've scheduled a two-day Rutgers Mini-Conference on Multiverse, Theodicy, and Fine-Tuning, during which the speakers will consider the following two topics:

- **Everettian Quantum Mechanics and Evil**

  > The problem of evil has been around for a long time: How can an all-powerful and all-good God allow evil of the sorts we see in the world? If the Everettian interpretation of quantum mechanics is correct, though, then there is a lot more evil in the world than what we see. This suggest a second problem of evil: If Everettianism is true, how can an all-powerful and all-good God allow evil of the sort we don't see?

- **A Probability Problem in the Fine-Tuning Argument**

  > According to the fine-tuning argument: (i) the probability of a life-permitting universe, conditional on the non-existence of God, is low; and (ii) the probability of a life-permitting universe, conditional on the existence of God, is high. I demonstrate that these two claims cannot be simultaneously justified.

Update: One more, from CERN-TH, Is theoretical physics in crisis?. Nothing really new, but don't miss the photo of John Ellis's office…

Comments

1. **4gravitons**
   May 18, 2016

   For the record, your endorsement is probably not going to help the discussion-in-the-comments situation, but thanks for the plug nonetheless.

2. **4gravitons**
   May 18, 2016

   Also you got the title wrong.

3. **Peter Woit**
   May 18, 2016

   4gravitons,
   Sorry about the mistake with the title, I'm not fully awake today. Fixed now.

   Also sorry if my linking to this makes your problem with string theory fanatics worse. Taking a look at that brought back memories of ten years ago for me. I think you're getting a good idea of what I was contending with.

4. **Magnema**
   May 18, 2016

   Minor typo, first paragraph: sent to *me

5. **Peter Woit**
   May 18, 2016

   Magnema,
   Thanks! fixed.

6. **Jon Forrest**

May 18, 2016

Another minor typo:

“Maybe this should have it’s own entry” ->
“Maybe this should have its own entry”

7. **Peter Woit**
May 18, 2016

Jon Forrest,
Thanks! I suppose I should proofread these things more carefully before posting…

8. **Chris W.**
May 18, 2016

It should be noted that there is really only one string theory fanatic replying to 4gravitons’ post and subsequent comments. Some of you can probably guess who it is.

9. **Anonyrat**
May 18, 2016

A draft of a probability problem in the fine-tuning problem argument is here:
http://philsci-archive.pitt.edu/11004/1/fine-tuning-anon.pdf
A probability problem in the fine-tuning argument
Halvorson, Hans (2014) A probability problem in the fine-tuning argument. [Preprint]

10. **Low Math, Meekly Interacting**
May 19, 2016

I pretty much agree with everything John Horgan said. But I don’t agree that defense and promotion of skepticism and science literacy isn’t vitally important, even in New York. I see every reason to be concerned about a surge of pre-enlightenment thinking, and an increasing willingness in society to view the failures of individual scientists as a failure of science and secularism in general. Horgan is entirely right about what he criticizes, and everyone can use a good ombudsman. But if they can cease contributing to the problem, there’s a great deal of good a more self-aware movement of skeptics can do. The battle against willful ignorance is far from won, IMHO.

11. **Ptiede**
May 19, 2016

I would have to disagree with what Horgan said. Often what he complained about was already material the skeptical societies have written extensively about. His complaints seemed to be .ore with mainstream media than with skeptical society itself. For example his discussion about cancer rates and mammograms is completely off base. Lots skeptics have had the discussion about this. They didn’t need Horgan telling them about it. The same goes for the multimeter and string theory.

12. **Peter Woit**
May 19, 2016

LMMI/Ptiede,
I don’t want to get involved in discussions over what Skeptic organizations are up to, largely because I’m mostly ignorant about this. I would also assume that, like any large diverse group, you can easily find among their many activities some evidence for whatever argument you feel like making.

I should say though, that the reason I’m mostly ignorant about such organizations is that from the little I’ve seen they seem to engage a lot in a kind of self-righteous preaching to the converted that I’m allergic to. The one aspect of their activities I’ve followed a bit has been the way they deal with bad mainstream theoretical physics (e.g. multiverse/string theory), and there they seem more often willing to embrace it than to challenge it (a point that Horgan was making).

13. **Low Math, Meekly Interacting**
May 19, 2016

The echo chamber of self-righteousness (as well as the incivility) is precisely what turns me off to the capital-S-Skeptics as well. I’d rather my daughter go to church camp than Camp Quest. I’m too old-fashioned to get my head around kids singing campfire songs about Charles Darwin, regardless of my complete atheism. And Horgan is 100% right on about quasi-religious nature of the anthropic multiverse. As suggested, it may be no accident that some of the loudest proponents of Capital-S-Skepticism are prone to cooking up their own untestable hypotheses (memes, bubble universes). There is an uncomfortable similarity between the pot and the kettle.

That said, I do still believe very strongly that secularism and empiricism are in need of an organized defense. Too much of the third world is falling completely under the thrall of medieval religious fundamentalists, and American politics is flirting ever more dangerously with a newly-invigorated strain Eurocentric racialism and neo-fascism. It’s not enough for the John Horgans of the world to sneer, I’m afraid.

All right, I’ve said all I will say about that. Promise.

14. **Igor Khavkine**
May 19, 2016

A small note about skeptical organizations. Peter, I would agree that they are “more often willing to embrace it than to challenge” bad theoretical physics. But that is one failing among many positive things that they do: opposing the push of creationism/intelligent design into US public schools; publicly supporting the scientific consensus on climate change, vaccines and GMOs; informing the public about shams like astrologers, psychics, faith healers and the dangers of quack medical treatments; rationally opposing conspiracy theories; informing consumers about predatory businesses that base their products on scams or pseudo-science; generally getting people informed and excited about science. The list goes on. I would ascribe the above failing regarding bad physics to the fact that their priorities are simply oriented toward other battles, arguably more relevant to a large number of people. Of course, this is just all the more reason for people who are better informed and more narrowly motivated to keep pointing out the problems with bad physics.

Lastly, I think it’s no surprise that no movement, including the skeptical movement, is immune to the pull of preaching to gatherings of like-minded people. However, besides being their own reward, skeptical conferences also aim at giving their attendees some tools and arguments that they could use to stem the tide of pseudo-science in their own way, and that might as well be done through a bit of preaching. 🙂

15. **John Smith**
May 19, 2016

Wonderful and much-needed talk by Horgan, all his points are spot on. There is a large base among skeptics who support (or are quiet on) war mongering, which turns out to have a large impetus on the rise of fundamentalism in other societies. Thus not only do they lack proper focus, their efforts are in large part self-defeating as well.

16. **Peter Woit**
May 19, 2016

Igor Khavkine,
I'm sympathetic to the goals you mention, and hope these organizations are having a positive effect. It's quite difficult to tell from here in New York, where what they are advocating for is what is already the mainstream, consensus viewpoint. I hope they're also taking this message to parts of the country where it isn't, and figuring out how to change minds there.

17. **diphton**
May 19, 2016

If LHC finds 5 sigma evidence of a new 750 gev particle, SUSY along with technicolor and extra dimensions are all possible explanations, with SUSY and KK gravitons being evidence of string theory

regards
750 diphoton excess

18. **Peter Woit**
May 20, 2016

diphton,
Yes, since string theory can predict anything, anything is "evidence of string theory".

More on topic, it's interesting to consider the effect of seeing a 750 GeV state on the ILC project. I assume the current 500 GeV proposal would be dead. Depending on what this is, perhaps a new lepton collider proposal would emerge.

19. **Dave Miller**
May 20, 2016

Peter,

You wrote:
>I should say though, that the reason I'm mostly ignorant about such [skeptical, secularist] organizations is that from the little I've seen they seem to engage a lot in a kind of self-righteous preaching to the converted that I'm allergic to.

Peter, having followed some of these groups for some years (and occasionally considered getting involved with them), I would say it is worse than that. They seem repeatedly to get into strange internal sectarian squabbles resembling "political correctness" (although untangling who is "politically correct" can even be confusing). It reminds me of some political groupings I have observed (and occasionally been involved in).

There are actual scholars, such as NT scholar Bart Ehrman, who write books, give lectures, etc. about subjects they really are experts on: I think that is a good thing.

Otherwise… well, let's just say that some people need to be part of a "movement," and I wish them well.

Dave Miller in Sacramento

20. **Cormac O'Raifeartaigh**
May 20, 2016

That's an interesting article by John Horgan and I think his concept of 'hard targets' vs soft targets' is quite useful. However, it seems to me that Horgan misses an essential difference between the two :
One may question whether 'hard targets' like string theory or the multiverse are real science, whether they are merely over-hyped speculation, and wonder at those who suggest we move the goalposts of testability.
But such theories do not deny well-established science. Unlike 'soft targets' like astrology or AGW skepticism, the 'hard targets do not blithely ignore well-established facts and assert that the moon is made of blue cheese.
That 'soft target' is a very different sort of madness (and btw there is nothing 'soft' about trying to deal with climate skepticism in the media).
I think it's quite dangerous to draw a rough equation between these two phenomena.

21. **Narad**
May 20, 2016

> and btw there is nothing 'soft' about trying to deal with climate skepticism in the media

Or attempts by activists to undermine public health by sowing mistrust in vaccinations, not to mention the plethora of predatory cancer quacks. I could go on.

22. **astringtheorist**
May 21, 2016

hi peter,
as someone from the community, who often defends your views, I am surprised to find that you are genuinely confused about some of the basics of the subject that you have spent a lot of time analyzing.

1) first of all, the usual massless particles that we see are not just zero modes. If you look at the state-operator map, you will notice that the gauge boson, for example, (see Polchinski 2.28) is given by acting with a creation operator on the vacuum of the open string; similarly, the graviton, involves a for example, is given by a left *and* a right creation operator on the closed string. So the massless particles that we see do correspond to vibrational modes.
The zero-modes you are talking about are tachyonic states, which are projected out in the superstring.

2) second, the excited states are not at Planck scale; they are at string scale. In fact, perturbative string theory only works because it cuts off the running of the gravitational coupling constant at a string scale, which is far below the Planck scale. The ratio of these two is controlled by the string coupling constant. So, it would be good to be careful about this terminology.

23. **Peter Woit**

May 21, 2016

astringtheorist,

No, I'm not confused about these issues. Thinking back on it, I remembered that the main reason I never wrote about this earlier is exactly to avoid a long tedious discussion of your issue 1. The problem here is that the description of the ground state of a superstring with the SM as a low energy limit is a very complicated story, with no real relation to the classical string and its vibrational modes (and this is where the public is being misled). If you really want to make a non-misleading analogy between this system, with its zero-energy ground state and higher-energy tower of states, I think 4 graviton's point, which I agree with, is that the zero energy state corresponds to the classical non-vibrating string. Given the complexity of the ground state, you can if you want make other analogies, but if you don't want to mislead people, you should explain the issue raised here (and this is never done).

As for 2., of course I'm aware that the string scale can be anything you want (which is part of the problem of the theory's lack of predictivity). My reference was just to the general picture of the class of compactification models most often advertised to the public.

24. **David hurn**
May 21, 2016

Is it me or do the words following the

Everettian Quantum Mechanics and Evil

Mean absolutely nothing.

25. **Dave Miller**
May 22, 2016

Peter,

You wrote:
>The problem here is that the description of the ground state of a superstring with the SM as a low energy limit is a very complicated story, with no real relation to the classical string and its vibrational modes (and this is where the public is being misled).

Can you (or anyone else) suggest a reference that goes into detail on the supposed physics of this?

I have no rhetorical ax to grind on this — I am genuinely curious as to what is going on in the math/physics and have come to realize that the vague ideas I had are probably mistaken.

Thanks.

Dave

26. **4gravitons**
May 22, 2016

Peter,

I'm actually not making the point that the zero energy state of the superstring corresponds to the the classical non-vibrating string. There's a double meaning in the title: the particles (that "you" are thinking of) aren't the vibrations (that "you" are thinking of). They're still vibrations (at least, to the extent that the metaphor works at all with tachyons in the mix, which admittedly is fuzzy).

The misunderstanding I'm trying to clear up is a much more basic one. I've run into plenty of people who've seen a trailer for the Elegant Universe or the like, and think that there's some sort of one-to-one map between higher and higher states of the free string and more and more massive SM particles. (Someone in the comments on Lubos's post proposes a pretty clean example, of someone who thinks that muons and taus are just higher excitations of electrons.) The particles that they are thinking of are certainly not the vibrations that they are thinking of.

I don't think this misunderstanding is Brian Greene's fault, since he covers the important details later. Any work of science popularization is going to be misunderstood (mine being an instructive example!), especially by people who only get a partial story. It's the duty of the rest of us as science writers to clear up those misunderstandings when we find them. That's why I'm in favor of more people writing about string theory, not fewer.

I think both you and Lubos (but yes especially Lubos) occasionally need to take a breath and realize that not everything written on string theory is a salvo in the string wars. Some people just genuinely want to explain something.

27. **Peter Woit**
May 22, 2016

Dave Miller,
This is just about the standard story of the quantized relativistic superstring, which is described in standard textbooks (Green-Schwarz-Witten, Polchinski). It is a quite complicated story: even before introducing fermionic variables, you're trying to quantize a theory that not only has the complexities of relativistic quantum field theory (infinite number of degrees of theorem, indefinite Minkowski metric), but also has an infinite-dimensional group of reparametrization symmetries that you need to deal with in order to get a unitary state space. Popular books about string theory avoid discussing any of this, often leaving a misleading impression that there is a close connection of this kind of theory to vibrating physical strings.

28. **Peter Woit**
May 22, 2016

4gravitons,
I did understand exactly the point you were trying to make, and it is part of what I had thought of writing about in the past, but hadn't. I don't think Brian or most other string theory popularizers intend to mislead anyone about this, but that people will be misled in this way by the kind of thing they choose to write is unavoidable. There's an unfortunate lack of interest among string theory popularizers in dealing with this sort of problem. I was glad to see that you were doing this and thought it worth pointing to, sorry if you think that's a "salvo in the string wars".

I've had the misfortune of acquiring a huge amount of experience with the kind of reaction you've gotten to trying to say something sensible. One of my least favorite aspects of this are the invariable accusations that one doesn't know what one is talking about whenever one tries to say anything about a complex issue. This is a good example: there is a huge disjunction between talk of "vibrations" and the construction of the states of the superstring that are supposed to correspond to physical particles.

This leads on the one hand to popularizations seriously misleading people, on the other hand to people accusing one of not knowing what one is talking about if one tries in a blog entry to address what is misleading.

29. **Dave Miller**
May 22, 2016

Peter replied to me:

>It is a quite complicated story: even before introducing fermionic variables, you're trying to quantize a theory that not only has the complexities of relativistic quantum field theory (infinite number of degrees of theorem, indefinite Minkowski metric), but also has an infinite-dimensional group of reparametrization symmetries that you need to deal with in order to get a unitary state space.

Hmmm, yeah, I have both GSW and Polchinski: I think both sets of authors would claim that what they are doing is a natural generalization of, say, Gputa-Bleuler. I'll admit it gets so convoluted that I am very unsure that I am left with any real grasp of the physics, but I am never sure if that is just my own shortcomings or if they are pushing the formalism way beyond any reasonable limits.

I take it, though, that the first-order (maybe zeroth order!) approximation to the SM does lie in the basic stuff in GSW rather than in arcane details of, say, Calabi-Yau compactification? I know that something like Calabi-Yau would be needed (and there is the KKLT flux stuff, and so on) to actually get a real model, but if I actually understand the GSW stuff in their first few chapters, will I know what is relevant to this debate?

What bothers me is, quite aside from any actual ties to experiment, does string theory actually make sense in its own terms? Again, I honestly do not know, so I am not taking a position, just curious as to what I need to grasp to be able to judge.

Of course, maybe this is the fundamental point at issue between proponents and critics of string theory.

Dave

30. **Peter Woit**
May 22, 2016

Dave Miller,

Yes the levels of complexity I was referring to are just the beginning, then you have compactification and moduli stabilization to deal with. I don't think those though are relevant to the points that either I or 4gravitons were making, and this isn't the place for yet another discussion of them.

31. **Anosel**
May 22, 2016

The draft Anonyrat linked sums up the argument thus:

> An omnipotent God would get to choose the laws of nature, and hence the standard measure on initial configurations of the universe. Therefore, if the standard measure says that the chances of life are practically nil, then a theist who believes contemporary physics should think that the chances of life are practically nil, as a result of the laws that God chose. But then the theist and the atheist are in exactly the same epistemic situation: both are puzzled by the fact that we exist.

The whole thing seems to hinge on the measure over possible constants being a matter of physics rather than of metaphysics.

But I doubt that fine-tuning advocates really cast their argument that way so it seems like a strawman.

32. **Low Math, Meekly Interacting**
May 23, 2016

It's not clear to me what conclusions, if any, one can draw from the photo of Ellis' office. Looks to me like the blackboard etc. may be littered with nothing more than satirical graffiti that hasn't been removed because of its humor value. Gallows humor? Poking fun at the "critics"?

Killer Asteroids

May 24, 2016
Categories: Uncategorized

During my recent vacation I visited my old friend Nathan Myhrvold, and got a tour of his company's lab near Bellevue. At that time he told me about what he had been working on recently, which has now appeared on the arXiv here, and is the subject of news stories today at the New York Times and Science magazine.

I confess I've never worried much about killer asteroids, but am glad that someone is doing this. Nathan has always pursued a wide range of different interests, and killer asteroids has evidently been one of them. I first heard from him a year or two ago about how he had gotten interested in the question of how to model the observability of such objects. Such modeling affects choices to be made about how to optimally search for these things (space-based or earth-based telescopes? what kind?). He wrote a paper last year about this, which was published in March.

What Nathan told me when I saw him was that he had found significant problems with the modeling done by the NEOWISE/WISE group at NASA, and you can now judge for yourself by reading his paper. I'm very far from being able to understand the details of this story well enough to judge who's right here. I do know Nathan well enough to know that his work on this deserves to be taken very seriously, and would bet that he has identified real problems. As noted in the comments there, the reaction from one of the ~~NASA~~ WISE people quoted at the end of the Science article wasn't exactly confidence inspiring.

Update: There's a press release about this out from NASA today, pretty much devoted to attacking Nathan's work.

Update: For some specific criticisms of Nathan's work, see the comment thread here. For a response to some of this from Nathan, see here.

Update: Scientific American has an article about this here.

Update: As pointed out by Wayt Gibbs in a comment, those interested in some discussion of the main point at issue might want to read the exchange here.

Comments

1. **Ethan Vishniac**
   May 24, 2016

   Ned Wright is a professor at UCLA, not a NASA person. I urge anyone interested to read the papers rather than judge by your opinion of Ned Wright or Nathan Myhrvold. For those without the patience to do so, I note that Ned has been usually acerbic, and usually correct, throughout a long career in science.

2. **Peter Woit**
   May 24, 2016

   Ethan Vishniac,
   Thanks, fixed the affiliation, I think.

   I know nothing about Wright, but I do have a lot of experience with what happens when you tell smart scientists they're wrong about something. If you're the one who is wrong, you quickly learn immediately why. If it's them, you get this kind of thing (for which "juvenile" is more accurate than "acerbic").

3. **Henry Lichtenstein**
   May 24, 2016

   If triple "l" "Killler" is not merely a typo, please explain what you mean by it. Thanx.

4. **edmeasure**
   May 24, 2016

   It's entirely possible that some significant threats among the smaller asteroids could be missed, whether for these reasons or others, including ones that could flatten a city or a small country. The big ones, extinction level event size, not likely. That's not the case for comets. Giants could be out there coming and we wouldn't see them until it was entirely too late.

5. **Peter Woit**
   May 24, 2016

   Henry Lichtenstein,
   I do think the triple "l" looks quite good there, but I fear it is a typo that I'll fix.

6. **Henry Lichtenstein**
   May 24, 2016

   Whew! Glad it wasn't meant to imply another Chicxulub impactor was heading our way.

   🙂

7. **Dwayne Day**
   May 24, 2016

   "I do have a lot of experience with what happens when you tell smart scientists they're wrong about something."
   .
   I note that while you complain about the reaction of one of the WISE scientists, you neglect to address a very serious issue here: Myhrvold did NOT bring up these issues in a peer-reviewed paper, but by posting it to a blog and then having his public relations people notify various science journalists and bloggers about it. That's bypassing normal (and proper) scientific procedures. It's not really telling "smart scientists they're wrong about something,"–the way you do that is by publishing with peer review. This is unprofessional behavior.
   .
   In addition, Myhrvold also interjected himself into an ongoing NASA mission selection competition in an obvious effort to influence that selection. That too is unprofessional. He's not a knight in shining armor, he's a spoiler.

8. **Peter Woit**
May 24, 2016

Dwayne Day,
Nathan did not "post the paper to a blog". He posted it on the arxiv and submitted it (if you believe the NYT) to the journal Icarus. Among the fields I'm familiar with, posting a preprint to the arXiv at the same time as submitting it to a journal is the standard behavior.

I have no idea whether he would like to influence the NASA selection, but don't see why, if he's convinced there's a problem with the NEOWISE numbers, it is "unprofessional" for him to raise the issue. Put differently, if you knew there was a problem with the numbers being used in such a selection process, it seems to me that you would have some duty to raise the issue. He's an unusual case of someone who has deeply immersed himself in the relevant technical issues, while not being affiliated with any of the groups seeking funding (even more unusual in that not only is he not seeking funding, others have been seeking it from him).

9. **Henry Lichtenstein**
May 24, 2016

@ Dwayne Day

"… –the way you do that is by publishing with peer review."
.
That is certainly one way, especially if one is at leisure to do so. But it is not the only way. I've been to a few conferences in my day, and I can vouch for there being many other ways of telling people they are wrong.

10. **Henry Lichtenstein**
May 24, 2016

"… , while not being affiliated with any of the groups seeking funding"
.
A laudable way to be, indeed. Beware of geeks seeking funding, like some folks at East Anglia University.

11. **Jeffrey M**
May 24, 2016

Peter,

While posting to arxiv while you submit is totally standard, in math at least that's not what you would do if you thought you found an error in someone's paper. In my experience, the person who thinks they found the error would email the author, and let them know so they can fix it. This literally just happened to me, someone had emailed a while ago about a paper I wrote 15 years ago, and my co-author and I finally had the time to look at what they said. I spent all day emailing back and forth with her today, and we came to the conclusion that we were right in the first place, so we're emailing the person who thought we had an error to see what they say. If we had decided instead they were right, we would have posted a correction to arxiv, and to the original journal, acknowledging who found the error. Seems like a much nicer way to do things.

12. **Dwayne Day**
May 24, 2016

"Submitted" is not the same as "published." He's made his paper available online–prior to any peer review–and then had his flacks publicize it (see the comments over at NASAWatch).

And he not only raised the issue of NEOWISE data, but injected himself into the Discovery selection process re NEOCam. He's playing spoiler.

The scientists quoted in the article have pointed out that he should go through peer review instead of engaging in a public relations campaign bypassing the scientific process and going straight to the press. If his analysis is correct it will survive peer review.

13. **AdamT**
May 25, 2016

Jeffery M,

What makes you believe that Myrhvold didn't contact them in private beforehand? My reading of the article indicates they were talking before he published.

14. **Jeffrey M**
May 25, 2016

AdamT

Where do you see that? They clearly talked, but from what I read it looked like it was after his paper was on ArXiv, when errors in Myrhvold's paper were pointed out to him. I see nothing to indicate he wanted to give anyone time to fix anything, he pretty clearly wanted to make a splash himself.

15. **Peter Woit**
May 25, 2016

Jeffrey M,
As far as I know, there was discussion of this between the parties before the preprint appeared. Would of course have been better if that led to agreement on what was right and what was wrong. Resolving such an issue tends to be more straightforward in math than in other fields.

16. **G. S.**
May 25, 2016

There are a few things which bother me about this story:

– Normally, when a single author writes a 111 page article criticizing a large scientific collaboration's data analysis, I am a bit suspect. It doesn't mean that the person is wrong, but I assign a high prior belief to the author being in error. Regardless of the author's institutional affiliation, I am unlikely to give much credence to the claims until after they have survived peer review.

– According to the Science article, it sounds like there was quite a bit of back and forth between Myrhvold and NEOWISE. NEOWISE disagreed with his analysis and suggested that he submit the paper to peer review. That seems like the proper course of action to settle a scientific disagreement. I am troubled by the additional media blitz (initiated by Myrhvold?) where the author claims the scientific establishment is staying the course because they are worried about their NEOCam proposal. This is similar to the argument constantly used by climate change deniers for why climate scientists repeatedly publish papers confirming global warming. It's possible that Myrhvold is correct, but these tactics are moving my belief even further out of his favor.

– The media seem to love these stories of "lone non-expert proves scientific establishment wrong". It seems there is a new one that gets promoted every week. Almost all of them eventually get debunked (e.g., the Canadian teenager who found a lost Mayan city … that turned out to be an old cornfield). Unfortunately, the public never reads the debunking article and they are left with the mistaken belief that the scientific community is constantly being proven wrong by non-experts. This makes it extremely difficult for the scientific community to convince the public that its findings are a solid foundation for public policy.

17. **Peter Woit**
May 25, 2016

G.S.,
I think this is a rather unusual situation and cast of characters, so I don't think trying to figure out who is right by analogies of the kinds you are making is sensible. In addition, trying to paint Nathan as analogous to a climate change denier is pretty offensive.

And no, as usual, I'm not going to allow comments here from people who want to argue about climate change.

18. **G. S.**
May 25, 2016

I certainly did not intend to cause offence. I merely intended to point out which aspects of this story bias me a particular way and why. In my experience, my line of reasoning has biased me toward the eventual correct conclusion much more often than not. However I realize that my line of reasoning is not proof, which is why I fully encourage peer review from (hopefully) initially unbiased experts in the field.

19. **Henry L.**
May 25, 2016

@ G. S.
"…, I am a bit suspect."
—
Did you really mean to say that you are not to be trusted?

20. **G. S.**
May 25, 2016

@Henry L.

Ha ha, no. I always thought that "suspect" could be used in this way but the dictionary claims otherwise. I learn something every day.

Interestingly, if I did say that I was not to be trusted, would you believe me?

21. **Henry L.**
May 25, 2016

G. S.,
—
Thank you for taking my remark in the spirit that I intended it to be taken.
—
I would believe you but I wouldn't entirely trust you. 🙂

22. **Scott Church**
May 25, 2016

Peter, I'm not familiar with this issue, but it seems to me that your friend is entirely within legitimate bounds. Dwayne Day says,

"'Submitted' is not the same as 'published.' He's made his paper available online–prior to any peer review–and then had his flacks publicize it (see the comments over at NASAWatch)…"

I'll grant that submitted doesn't equate to published, but the Arxiv is well-known as a *preprint* forum where researchers routinely post work while it's in review. In addition to the journal/s it's been submitted to, this gets it in front of a larger audience where it can receive whatever expansion and/or correction it may need that much faster. Everyone does this… we're all the better off for it, and we don't accuse anyone of duplicity for doing so.

The press routinely trolls the Arxiv for anything that might fuel PM-Edition potboiler stories and regardless of what anyone says or publishes, "Killer Asteroids Threaten Humanity!" is bound to be like tossing a 10 lb bloody pot roast into a shark tank. It's little wonder they were all over it, and whether Myrhvold spoke to them first or not, you can bet they would've sought him and others out for comment soon enough anyway. If he, or anyone else chose to respond that hardly makes them "flacks" or "spoilers." I don't recall anyone labeling Stephen Hawking as either when he publicly bet Gordon Kane that the LHC wouldn't find the Higgs, only to turn around and proclaim that it could destroy the universe at any moment when it was.

As for NASAWatch, I've seen countless such *****Watch sites over the years. In my experience all were agenda-driven forums run by "activists" who invariably saw ***** as a cabal of nefarious evil doers against whom they had been anointed to be "whistle blowers" delivering "the truth" to the masses. Not surprisingly, NASAWatch is owned by SpaceRef–a self-described "new media company focused on the space sector" that among other things, has an "Intelligence Unit." According to the NASAWatch website header, "This is not a NASA Website. You might learn something. It's YOUR space agency. Get involved. Take it back. Make it work – for YOU." [sic]. The only references I was able to turn up to their content were at conspiracy websites. The words *objective* and *trustworthy* don't exactly leap to mind here.

23. **Henry L.**
May 25, 2016

@ Scott Church
—
I agree that Hawking's bets and proclamations do not make him a spoiler, but I fail to see how the outcome of his bet with Kane has anything to do with his proclamation about the Higgs possibly destroying the universe. The discovery of the Higgs at the LHC did not create the possibility that it could destroy the universe.

—
Perhaps I am misreading your comment?

24. **Scott Church**
May 25, 2016

Henry L.,

It didn't… I cited the two stories together only because both involved the Higgs, and both struck me as examples of situations similar to that involving Peter's friend that didn't lead to anyone being accused of being a flack or a spoiler. Other than that, you're correct–there is no similarity between the bet and the doomsday comment. Best.

25. **JeanTate**
May 26, 2016

I'm a bit surprised no one has yet commented on the actual content of the Myhrvold paper (arxiv preprint version anyway). There are sites where this is discussed, and which include posts by actual astronomers, professional and amateur, e.g. Challenge to Asteriod Size Estimates, at Yahoo Groups.

It seems that, among other things, Myhrvold did not do a good job of checking for consistency, both internal (e.g. radius vs diameter), and external (e.g. independent estimates of the sizes of asteroids, from studies independent of WISE, occultations, etc; there are quite a few such independent datasets, available publicly, for free)."

As far as I know, these sorts of simple, if sometimes rather tedious – astronomers tend to use rather a lot of different systems, and conventions, not all immediately obvious (surprisingly, radius vs diameter is one!) – consistency checks peer reviewers expect authors to do, before submitting to arXiv. Sure, if you're an outsider, you'll likely miss some, but I find it strange that a trained physicist would trip up over radius vs diameter, no matter how deeply you'd have to dig to get complete certainty ("size" clearly doesn't cut it).

26. **Anonyrat**
May 26, 2016

"Amy Mainzer, principal investigator for NEOWISE, pointed out that many of her projects' measurements have in fact been reproduced and confirmed. Japan recently operated an infrared telescope called Akari that was similar to WISE, and it measured many of the same asteroids."

https://www.washingtonpost.com/news/speaking-of-science/wp/2016/05/25/that-study-critiquing-nasas-bad-science-on-asteroids-is-pretty-bad-science/

27. **Anonyrat**
May 26, 2016

http://arxiv.org/abs/1403.7854
A Comparative Study of Infrared Asteroid Surveys: IRAS, AKARI, and WISE
Fumihiko Usui, Sunao Hasegawa, Masateru Ishiguro, Thomas G. Mueller, Takafumi Ootsubo

28. **Anonyrat**
May 26, 2016

From the WaPo link:

He {Myhrvold} added that he tried to get WISE scientists to comment on his work for months, but that they'd refused to respond until he made his work public. Mainzer disputes this, saying she has offered corrections to the paper that Myhrvold has ignored, including an issue that he sees as a possible sign of fraud.

"In more than 100 cases, the reported asteroid sizes they listed were an exact match to prior papers, that's within the meter. That's just not possible to have happen accidentally," he said.
…
""We've tried to explain this many times," Mainzer said. Those measurements, she continued, are intentionally pulled from other sources — they're used to calibrate the measurements made using the spacecraft with previously observed data. It's not the case of a mistake or fraud, but a standard procedure."

"She also takes issue with the underlying thesis of the critique, which is that the NEOWISE calculations ignore Kirchhoff's law of thermal radiation."

""Of course this model doesn't perfectly conserve energy, and no one has ever said it does," Mainzer said. The model, she explains, sweeps away some of the complexities of an asteroid's heat distribution to make up for the fact that asteroids have incredibly complex surfaces. Scientists using these models don't have every data point they would need to do a perfect calculation, and that's the whole point of using the model in the first place.

"The paper is basically useless in the sense that he's complaining about a model which it has always been clear does not satisfy physical laws," Ned Wright told The Post. "So then the question is, does it work, is it a good approximation anyway, and the answer to that seems to be generally yes.""

https://www.washingtonpost.com/news/speaking-of-science/wp/2016/05/25/that-study-critiquing-nasas-bad-science-on-asteroids-is-pretty-bad-science/

29. **Anonyrat**
May 26, 2016

On a yahoo egroup, Joseph Masiero, Ph.D. Scientist, NASA JPL/Caltech, points out a NASA-JPL response (provides no link to that response)

https://groups.yahoo.com/neo/groups/mpml/conversations/messages/32026

Quote:

A sampling of the errors include:

— Equation 3 (page 7), equation 26 (page 20) and equation 33 (page 21) are all wrong by a factor of 4. This is because of a fundamental error confusing diameter and radius. It hasn't been possible to review the actual code used by the author(s) in implementing the thermal model, but if it follows these equations it will consistently get incorrect results for diameters, albedos and predicted fluxes. One example of this can be seen in Figure 21 (page 68), which shows a diameter for asteroid 295 of about 660 kilometers. Such a size would make this object bigger than asteroid Vesta, the second largest object in the asteroid belt. The actual diameter of asteroid 295 is estimated at about 30 kilometers from the Infrared Astronomical Satellite (IRAS), Japan's AKARI satellite, and WISE data.

— The paper mischaracterizes the use of the radar/occultation/flyby diameters. In Mainzer et al. 2011 ApJ 736, 100, the NEOWISE team uses these diameters to compute model brightnesses and compare them to the measured brightnesses for the objects. They are in excellent agreement. The radar/occultation/flyby sources are cited in this paper; later papers reference this calibration paper.

— The paper erroneously states that the NEOWISE data are not reproducible. On the contrary, in Usui et al. 2014 the authors find that IRAS, AKARI, and WISE are all in agreement to within +/-10% 1-sigma for the ~2000 asteroid diameters measured by all three observatories. This point is given in the abstract (See: http://arxiv.org/abs/1403.7854). These measurements were independently taken over a 30-year span beginning with the joint UK-US-Dutch IRAS mission and Japan's AKARI mission, as has been documented in peer reviewed papers. While Usui et al. 2014 is mentioned in the new report (on page 2), the central point of its finding was omitted.

–The paper has changed the standard asteroid thermal model (Harris 1998) that is widely used and has been extensively peer reviewed, but the paper doesn't show that the proposed model works better than the standard model at replicating the radar diameters.

— The paper mischaracterizes the NEOWISE use of visible brightness H; it is an observational constraint and not a free parameter (page 48.)

— In equation 32 (page 21), the brightness of the Sun is computed as being of order 10E-11 Jy. This is off by more than 20 orders of magnitude as currently written.

Data from the mission is publicly available. A quick guide to the NEOWISE data release, data access instructions and supporting documentation is available at http://wise2.ipac.caltech.edu/docs/release/neowise/. Access to the NEOWISE data products is available via the on-line and API services of the NASA/IPAC Infrared Science Archive."

30. **Anonyrat**
May 26, 2016

The above comments, including page numbers, from Joseph Masiero are in reference to
http://arxiv.org/abs/1605.06490
"Asteroid thermal modeling in the presence of reflected sunlight with an application to WISE/NEOWISE observational data"

31. **Anonyrat**
May 26, 2016

Dave Herald of IOTA –
https://groups.yahoo.com/neo/groups/mpml/conversations/messages/32032
Quote:

There is a world-wide group of (mainly) amateurs who regularly observe occultations of stars by asteroids The main groups are located in Nth America, Australia/New Zealand, Europe, & Japan. These groups collectively observe over 200 actual occultations per year (and have high hopes for this number to increase significantly as Gaia data becomes available over the next few years). Their observations and results are annually archived at NASA's Planetary data System, Small Bodies node, Asteroid/Dust Archive – at http://sbn.psi.edu/pds/resource/occ.html.

The observed length of a single occultation chord routinely has a precision of about 1 km for a main belt asteroid. That is, we are measuring the outline of an asteroid at a 1km precision, at which level surface irregularities (let alone gross shape) are evident. To obtain a reasonable estimate of the asteroid size, you generally need at least 3 occultation chords spread across the asteroid. On occasions we have many 10's of chords located across the asteroid – with the shape and irregularities of the asteroid profile being blindingly obvious. We generally fit an ellipse to the observations – as asteroids are rarely spherical. And this raises the issue of the rotational orientation of the asteroid – as the profile is continually changing as the asteroid rotates. A consequence is that we do not get (or expect) a 'single' diameter measurement for an asteroid. On the other hand the observations can (and are) linked to light-curve inversion models to provide a scale (as well as validation) for those models. It seems to me that any study of the accuracy of asteroid diameter determinations from missions like NEOWISE is entirely academic unless it taps into this publically available data. The failure of Myhrvold to undertake this in the course of his criticism of the work of others is (IMHO) bordering on the inexcusable.

Turning now to a specific critique of Myhrvold's paper (which I find extremely tedious reading…) Fig 23 (on page 72!) is (from my perusal) the first (only?) point at which he presents diameters derived by his approach. It lists just three asteroids, and interestingly we have a single reasonably-well-determined occultation diameter for each of them. Importantly, for these three asteroids we have a measured diameter two compare against the two 'inferred' diameters, with the obvious ability to assess which inferred diameter is best in each case, and whether there is any consistency across different asteroids. To summarise the various results:

Asteroid # 208 306 757
NEOWISE 45.0km 51.6km 36.7km
Fig 23 146.5km 83.8km 6.6km
Occultations 48 x 42km 61 x 44 km 39 x 34km

Clearly the occultation results align extremely well with NEOWISE. In contrast there is major disagreement with the results of the author's 'bootstrap' solution – with the strong implication that his bootstrap methodology is seriously flawed. IMHO the consequence of this on the paper as a whole doesn't need to be stated…

A final comment. We (in the occultation community) have not undertaken a formal comparison of our occultation results with those from NEOWISE/IRAS etc. However I believe it is a fair comment to assert that the NEOWISE/IRAS diameters are 'about right' – and certainly more than reliable enough for our current prediction purposes. Furthermore I cannot think of any instance where the diameter determined from a well-observed asteroidal occultation was greatly different from a NEOWISE.IRAS diameter – especially when a shape model is available such that we can assess the effects of the shape model on the expected diameters.

32. **Dwayne Day**
May 26, 2016

NASA Response to Recent Paper on NEOWISE Asteroid Size Results
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Press Release From: NASA HQ
Posted: Wednesday, May 25, 2016
.
A paper posted Sunday by Nathan Myhrvold to ArXiv.org and described in an article by reporter Ken Chang in the May 23 New York Times discusses interpretations of data on asteroids from NASA's NEOWISE mission. The paper was posted before undergoing the essential scientific peer-review process to catch and remove significant errors.
.
Examination of the paper by members of the science community studying near-Earth objects has found several fundamental errors in Myhrvold's approach and analysis—mistakes that an independent peer review process is designed to catch. The errors in the paper lead to results that are easily refuted, such as sizes for well-known asteroids that are significantly larger or smaller than their already-verified sizes. While critique and re-examination of published results are essential to the scientific process, it is important that any paper undergo peer review by an independent journal before it can be seriously considered. This completes a necessary step to ensure science results are independently validated, reproducible, and of value to the science community.
.

All of the published NEOWISE team papers providing their results have endured the peer-review process. NASA is confident that the processes and analyses performed by the NEOWISE team are valid and verified and stands by its data and scientific findings.
.
Data from the NEOWISE mission is available on a website for the public and scientific community to use. A guide to the NEOWISE data release, data access instructions and supporting documentation is available at http://wise2.ipac.caltech.edu/docs/release/neowise/. Access to the NEOWISE data products is available via the on-line and API services of the NASA/IPAC Infrared Science Archive. A list of peer-reviewed papers using the NEOWISE data is available at http://neowise.ipac.caltech.edu/publications.html

// end //

33. **south**
May 26, 2016

Having read the comments here and some testimonies online, Myhrvold comes across as an individual more interested in seeing his name and ideas talked about in the media than contributing to science. Similar to some of the individuals and their over-reaching ideas in the string theory that receive some criticism on this blog.

—

Also, all I see in the NASA statement is a dispassionate assertion that (1) the peer-review process is the proper hurdle to overcome, and that (2) experts have identified several errors in Myhrvold's work. I don't consider it an "attack" to state that someone's calculations are wrong — in fact, this is what Myhrvold himself has done to NASA. But he did so publicly and these exchanges should carried out professionally through a peer-review process. Since Myhrvold eschewed this process and was first to blast to the media about NASA's errors, I think it's necessary and responsible for NASA to defend their reputation by publicly stating their disagreement.

34. **Dwayne Day**
May 26, 2016

From the Washington Post article:
.
"Some of the things I've heard from third parties are, 'wow, we'll see if it makes it through peer review,' which makes me think they'll ambush it in peer review," Myhrvold said.
.
So he had his people contact the NY Times, Science, and apparently other websites pushing his claims before peer review.
.
https://www.washingtonpost.com/news/speaking-of-science/wp/2016/05/25/that-study-critiquing-nasas-bad-science-on-asteroids-is-pretty-bad-science/

35. **Anonyrat**
May 26, 2016

Myhrvold has evidently put in a lot of work, and it would be tragic if none of it is of value. What I don't get is that, from all accounts, he has resources at his command that I can only dream of, and he certainly could have hired a couple of smart students or even professionals to vet his work before publishing it. It is not as though someone was going to scoop him. If this turns out to be a fiasco, anything important that he has to say in the future will get little attention.

36. **Jeffrey M**
May 26, 2016

I think this makes clear that arXiv is now treated in a way it really shouldn't be. arXiv is for preprints. The odds that there are errors in things posted on arxiv are noticeably higher than for things published in reputable journals. I think a lot of people have forgotten that, they treat arXiv like a journal, which it's not. If Myhrvold tried to get the authors to fix mistakes he thought they made, and they didn't (either because they couldn't, or because they didn't feel they were mistakes), he could have gone to the journal editors where it was published. If even after that he got nowhere, he could put something on arXiv, and send it to a journal, but to make a big deal out of it is really out of bounds, no one has checked his work carefully, and someone did check the original work. The original referee might have screwed up, but Myhrvold hasn't been refereed yet. This shouldn't be happening in public. Myhrvold and NASA should just be waiting on the referee report on his paper, and if the referee says junk then no story, if the referee thinks Myhrvold is right, then it's a story.

37. **JeanTate**
May 26, 2016

I would be interested to hear how mathematics arXiv submissions are different from astronomy/astrophysics ones.

In the latter field, most authors clearly indicate whether the document has been submitted to a peer-reviewed journal, when/whether it has been accepted for publication, etc. How timely, or accurate, this info is is left up to the reader to decide. Some post the published version (modulo formatting etc), but I think that's rare. There are, almost always, significant differences between the "submitted to", and the "accepted for publication in" versions; sometimes these can be quite major. There are also quite a few arXiv documents that have, apparently, never been submitted to a journal, and some which apparently were submitted many years ago, but are still not published (in a peer-reviewed) journal.

In almost any published paper, you'll find "errors". Mostly these are trivial – a misplaced comma, say – but occasionally you'll find some howlers. A figure's axes that are mislabeled, say, or an equation that omits a key term. I've asked astronomers about some of these, and most seem quite unconcerned; the mistakes are "minor" enough that a fellow professional, working in the same sub-sub-field can work out what was intended; indeed, I've had responses where mislabeled axes, say, we're not even noticed!

So, how bad are the – many – "errors" in the Myrhvold preprint? Of the ones already noted, and copied here, some would require just minor tweaks, and the main conclusions would be unaffected. Others, not so clear (the misunderstanding/misrepresentation of H, for example). The basic physics, and statistical analyses seem OK, but their application seems flawed, in several places. Also, one of the central themes – picked up by the popsci articles, i.e. the "unphysical" modeling – is misplaced (as Mainzer notes).

38. **Ignatz Ratzkywatzky**
May 26, 2016

"Anonyrat says:"

Thank you for posting the only comments worth reading about this controversy.
The ones with astronomical content.

As for the rest

“What dire Offence from astro’mous Causes springs,
What mighty Contests rise from trivial Things,”

with apologies to Pope.

39. **Jeffrey M**
May 26, 2016

JeanTate

In math, from my experience, many postings on arXiv are done before something is submitted somewhere, but not long before. People are often trying to work out where to send something once they’re confident it’s right, so they’ll put in on arXiv while they’re trying to figure that out. People usually repost published versions, and corrected versions before publication. There are things on arXiv that have never been published, there are various reasons for that. I have a paper on arXiv which never appeared anywhere, I am sure it is correct, but it’s in a field not mine, and my co-author and I could never get our language to where the referees in that area would deal with the paper, so we just gave up. As you note, plenty of published papers have errors, referees miss things all the time. Usually not a big deal, but not always. There’s a famous example in my area, a paper of McKean where he claimed to prove that 1/4 is the bottom of the Laplace spectrum on surfaces. This was a big big deal, which unfortunately isn’t true.

40. **Anonyrat**
May 26, 2016

NASA pointed out that Myhrvold estimates the diameter of asteroid 295 to be 660 kilometers – this would be larger than the asteroid Vesta which is supposedly the second largest object in the asteroid belt. The diameter estimated by WISE, etc., is around 30 kilometers.

41. **Anonymous**
May 26, 2016

I would hardly characterize the NASA response as an attack. It simply states that peer review is appropriate for such claims. That’s pretty mild compared with what they could have said.

I’m frankly startled, given your critiques of the meddling of billionaires in the physics and math worlds, and of the rockstar culture in physics, that you are embracing Myhrvold’s science-by-public relations efforts.

42. **Peter Woit**
May 26, 2016

I’ve added as an update a link to something from Nathan explaining his point of view on this controversy.

While I have no expertise or ability to figure out who is right and who is wrong on the technical issues being raised on both sides here, I do have some expertise on the question of Nathan’s motivations, based on observations conducted over the long-term and over the shorter term of the last couple years. Some commenters seem to think this is a usual story of a wealthy person who wants to see his name in the papers, throw his money around and “meddle”in the world of science. They should note that devoting a huge amount of time and energy to rather tedious and complex issues of data analysis in a highly-obscure subject, and writing 60 page (successfully peer-reviewed) and new 110 page (out for peer-review) papers about this data analysis, are a funny way to go about this. Nathan has gotten a lot of media attention for various of his activities, and to whatever extent he might want more, he has lots of much easier ways to go about this. Among all the scientists I’ve met over the years, he’s one with an extremely unusual degree of enthusiasm and love for learning as much as possible about the technical details of a wide variety of scientific subjects, coupled with a brilliant mind and an insane amount of energy. Those criticizing his motivations seem to me to be missing the obvious: the guy loves real science with a passion and engages with it very seriously.

43. **Narad**
May 26, 2016

> The errors in the paper lead to results that are easily refuted, such as sizes for well-known asteroids that are significantly larger or smaller than their already-verified sizes. While critique and re-examination of published results are essential to the scientific process, it is important that any paper undergo peer review by an independent journal before it can be seriously considered.

This seems to be a very priggish approach coming from NASA. I know that the context is different (and I also worked on the make-the-final-product side of the AAS journals for over a decade a while ago and prevented more than a few errata), but virologist Vincent Racaniello recently was the subject of similar complaints.

Is there some sort of crucial difference between postpublication peer review and the prepublication version? I see no particular reason to delay the “more eyeballs” part until after solemnization.

44. **No Math, Meekly Interacting**
May 26, 2016

“But the most egregious problems with the NEOWISE project are dead simple to explain; indeed, this might be a good project for a middle-school science class.”

Ooooh-kay then! No mistaking the size of Dr. Myhrvold’s stones!

45. **Henry L.**
May 26, 2016

“No mistaking the size of Dr. Myhrvold’s stones!”

Would that be based on using the thermal model?

46. **Anonymous**
May 27, 2016

While I still think Myhrvold’s pre-publication publicity efforts are inappropriate, and still loath him as a billionaire patent troll, I’ve now looked at the paper and withdraw my comment about meddling. His argument (about the copied data, I can’t evaluate the thermal modeling) is pretty damning. The NEOWISE people have some explaining to do.

I do have some sympathy for their disinclination to engage with him, whatever the truth of the matter. Myhrvold hurts his case by his tone, which comes across as too similar to that of the many cranks that everyone at NASA or funded by NASA has to deal with on a daily basis.

47. **Anonyrat**
May 27, 2016

http://arxiv.org/pdf/1604.03444v1.pdf
MODELING THE PERFORMANCE OF THE LSST IN SURVEYING THE NEAR-EARTH OBJECT POPULATION

cites Myhrvold, if only to disagree with him. But they weren't ignoring him.

"The limiting magnitudes and solar elongation coverage presented here are at odds with Myhrvold (2016); his Figure 7 shows much fainter limiting magnitudes than presented here. While Myhrvold (2016) cites the possibility and capability of LSST observing at much smaller solar elongations, this is inconsistent with the published cadences provided by the LSST project and would potentially interfere with
its other science goals. Additionally, Myhrvold (2016) incorrectly assumes that an object need only be detected once for it to be counted as discovered, cataloged, and tracked"

48. **Anonyrat**
May 27, 2016

In https://arxiv.org/ftp/arxiv/papers/1605/1605.06490.pdf, Myhrvold writes about the NEOWISE claims of high accuracy, and that all the caveats are not included.

"This implies that the correspondence between NEATM model estimates and those from radar or other means has been calculated yielding a quantified numerical answer ≤±10%. In the passage above there is a caveat that the error estimate applies only with certain pre-conditions, but those caveats are frequently dropped from other references (as in the quote from (Masiero et al., 2011) in section 1 above.)"

A look at Masiero et al. 2011 is NEATM applied to main belt asteroids, not near-earth asteroids. NEATM is "Near Earth Asteroid Thermal Model". It was developed in 1998. This abstract (doi:10.1006/icar.1997.5865) essentially says that the thermal models for large main-belt asteroids were wrongly being applied to Near Earth Asteroids, that have very different properties, and that is the motivation for the NEATM model.

But it seems after that, NEATM was found to be much more widely applicable, giving good results with main-belt asteroids. Therefore, when Myhrvold complains that Masiero et. al. write "Using a NEATM thermal model fitting routine we compute diameters for over 100,000 Main Belt asteroids from their IR thermal flux, with errors better than 10%." (this is in the abstract) it is possible that NEATM model accuracies for main-belt asteroids can indeed by 10% while not being that accurate for actual NEOs. It is not clear to me.

Most of the Mainz, Masiero, etc. papers that are cited have preprints on arxiv. You can look at them and see if Myhrvold's criticisms are justified.

49. **Low Math, Meekly Interacting**
May 27, 2016

"Would that be based on using the thermal model?"

Indeed. There's a lot of heat here, clearly. We'll see if there's light. Tenured professors openly cutting rivals to the quick is hardly surprising, based on what I've observed in my career, and barely warrants a shrug. I normally do expect similar behavior from an "outsider" to be indicative of a crackpot at work. Obviously, Dr. Myhrvold doesn't fit that description. But on the subject of stones, his status as a "billionaire patent troll" of considerable renown does mean that if he's wrong, it's going to be a very, very satisfying experience for some people to point that out, given the current sociopolitical climate. I doubt they will resist the opportunity to gloat in a most public fashion.

50. **JeanTate**
May 27, 2016

Thanks for posting the link to Nathan's response, Peter.

He makes a strong case for some apparent, strange inconsistencies in the published NEOWISE diameter estimates of some asteroids, and at first blush it seems pretty sound. However, despite Anonyrat pointing to many papers being freely available, it's not clear how different the preprint versions are from the actual, published in peer-reviewed journals … some at least are behind paywalls (this points to yet another barrier to outsiders doing front-line, independent research in astronomy; unless you are rich, or have a backdoor, simply obtaining the necessary papers, not preprints, can be prohibitively expensive. Ironic really, as taxpayers, you pay for almost all the published research, yet you have to pay, yet again, to actually read what your dollars resulted in).

I'm a little confused, however; my read of Nathan's paper is that the NEOWISE diameter inconsistencies are a relatively minor part. The main part has to do with the models which are used to produce diameter estimates, and in this regard, it seems that Nathan is doing a dodge of his own.

Finally, Nathan claims that the shortcomings in his equations (etc) are minor, easily fixed, and do not significantly change his main conclusions. That may well be so; however, until he makes the changes, publishes them, and we can all evaluate them for ourselves, his claim has no legs, IMHO.

51. **Scott Church**
May 27, 2016

For what it's worth, this was published today in Slate:
http://www.slate.com/blogs/bad_astronomy/2016/05/27/nathan_myhrvold_claims_nasa_scientists_asteroid_calculations_are_all_wrong.html. Haven't had a chance to read it closely yet, but at first blush it seems thorough, and presents both sides of the debate.

52. **Confused**
May 27, 2016

From the slate paper:
"This is the model Myhrvold claims is wrong. However, the asteroid diameters found by the NEOWISE team agree very well with previous satellite measurements. NEOWISE looked at many of the same asteroids as an earlier mission called IRAS—a couple of thousand of the same asteroids—and found that the diameters calculated for those asteroids matched the measurements using IRAS to about 10 percent. Not only that, measurements using a Japanese satellite called Akari also yielded similar results, and all three agree well with the radar and occultation measurements."
http://www.slate.com/blogs/bad_astronomy/2016/05/27/nathan_myhrvold_claims_nasa_scientists_asteroid_calculations_are_all_wrong.html
—
They cite this paper in support of that claim:
"The mean values of the relative differences are 2.8%, 1.7%, and 7.5% for IRAS, AKARI, and WISE, respectively, and the standard deviations for each are 12–13 %.We found that the size derived by AKARI is closer to that derived by IRAS or WISE. This is not a surprising result, as the beaming parameter adopted in the thermal model calculation in the AKARI catalog is calibrated with well-studied main belt asteroids larger than 90 km, whose size, shape, rotational properties, and albedo are known

from different measurements, as mentioned above. In this respect, the diameters obtained by radiometric measurements based on I–A–W are reliable in a statistical sense, which are smoothed out and averaged over a limited number of observations, even though the sizes obtained by radiometric and other measurements can be discrepant by up to 30%."
http://arxiv.org/pdf/1403.7854.pdf
—
So the 1-sigma uncertainty is >10%, and from figure 7 it looks like the (more usual to report) 2-sigma uncertainty is ~25%. I don't see how Myhrvold's point that claiming "+/- 10% accuracy " is way overconfident is at all controversial from this data.
—
Also, this is apparently for the same asteroids that were used to calibrate this thermal model… Am I understanding that last part correctly? Don't they need to estimate out of sample error in order to assess the skill of the model?

53. **Henry L.**
May 27, 2016

LM MI,
"There's a lot of heat here, clearly."
—
Heat, for sure; clarity, not so much.
🙂

54. **Anonyrat**
May 27, 2016

Since LSST and Ivezic have been brought into the mix by the SciAm article, the "NASA" team have this from April 12: http://arxiv.org/abs/1604.03444 Modeling the Performance of the LSST in Surveying the Near-Earth Object Population
which is from before this controversy hit the airwaves.

Emphasis added:

"We have performed a detailed survey simulation of the LSST performance with regards to near-Earth objects (NEOs) using the project's current baseline cadence. The survey shows that if the project is able to reliably generate linked sets of positions and times (a so-called "tracklet") using two detections of a given object per night and can link these tracklets into a track with a minimum of 3 tracklets covering more than a ~12 day length-of-arc, they would be able to discover 62% of the potentially hazardous asteroids (PHAs) larger than 140 m in its projected 10 year survey lifetime. This completeness would be reduced to 58% if the project is unable to implement a pipeline using the two detection cadence and has to adopt the four detection cadence more commonly used by existing NEO surveys. When including the estimated performance from the current operating surveys, assuming these would continue running until the start of LSST and perhaps beyond, the completeness fraction for PHAs larger than 140m would be 73% for the baseline cadence and 71% for the four detection cadence. **This result is a lower than the estimate of Ivezic et al. (2007,2014); however it is comparable to that of Jones et al. (2016) who show completeness ~70$%.** We also show that the traditional method of using absolute magnitude H < 22 mag as a proxy for the population with diameters larger than 140m results in completeness values that are too high by ~5%. Our simulation makes use of the most recent models of the physical and orbital properties of the NEO populations, as well as simulated cadences and telescope performance estimates provided by LSST. **We further show that while neither LSST nor a space-based IR platform like NEOCam individually can complete the survey for 140m diameter NEOs, the combination of these systems can achieve that goal after a decade of observation.** "

55. **Low Math, Meekly Interacting**
May 27, 2016

One thing that is quite clear: Myhrvold has gone very publicly on the record as stating the NOEOWISE team, et al. should perhaps someday be discussed in classrooms the way we discuss the Piltdown Man. Only the fraud is more obvious. Chutzpah doesn't begin to describe it. If he's wrong he's going to find out what being an 8.4 Tesla shit magnet feels like, and may even deserve it.

56. **Anonyrat**
May 27, 2016

The record of space telescopes operating in the infra-red for detecting asteroids seems unsurpassed. LSST, if it operates per specs, will also be awesome. If I were worried about asteroids that might collide with Earth, I'd want both instruments. If I had to choose one or the other — that is a very hard decision. I hope this goes back to being science and engineering and less the media-circus that it has become. I hope that Myhrvold gets some collaborators and he makes the breakthrough in NEO asteroid detection that he is looking for. Bye-bye for this thread.

57. **Henry L.**
May 27, 2016

LM, MI

I wasn't aware he was on record to have made such a (perhaps) reckless pronouncement. But I can't imagine that he was unaware of the consequences (if he was wrong). So he is either confident that he is not wrong, or he may not care about the consequences. I hear that ~650 mega-bucks can buy a lot of protection against all sorts of consequences.
—
I think that mega-bucks are the keys to clarification for these sorts of controversies. Chercher les mega-bucks if you have a dog in this fight. I don't.

58. **Astronomical Cowardice**
May 28, 2016

I have looked at three of the papers — Myhrvold's arxiv preprint, and (as referenced by Myhrvold) Masiero et al., 2011, and Mainzer et al., 2011c — with some care, and I must say that I find Myhrvold's criticism of the portrayal in Masiero et al. of "direct" diameter measurements (what Myhrvold calls ROS) as the result of the thermal model to be fully legitimate. (Myhrvold reiterates this point in his note, "A Simple Guide to NEOWISE Data Problems," that Peter links to in one of his updates.) (Note, to avoid confusion, that Masiero et al. reference as Mainzer et al., 2011b that which Myhrvold references as Mainzer et al., 2011c.)

I haven't read any of these papers in complete detail, but I have done more than skim them, and I have read through some sections quite closely. Myhrvold complains that 123 of the thermal-model diameter estimates (117 in Masiero et al., 2011) are actually ROS diameters from other sources. On the surface, including such "foreign" diameter measurements in a table of thermal-model estimates is bad science. It would be simply wrong of them to have replaced their model estimates with others' measurements. It would be at a minimum questionable to have somehow baked these benchmark values into their analysis so that their thermal model reproduced those exact values.

Note that Mainzer et al., 2011c analyze thermal models for 50 objects specifically for which ROS diameters are known, and make explicit that they use and report these

ROS diameters. Masiero et al. cite this paper, as I discuss below.

Masiero et al. do cite various ROS papers, but only in the introduction. I find no mention in their paper that they have used these ROS values in any way in their analysis. I find no mention that any of the diameter values in their Table 1 ("Example of Electronic Table of the Thermal Model Fits"), or in the electronic table of which it is an extract, are produced in any way differently than others of the same class. The ONLY clue I see anywhere in the paper itself that those which Myhrvold has identified as ROS values are different is that they are round kilometer values.

Masiero et al. cite a number of other papers, and I have only looked at the few of them that seemed potentially relevant to this issue. Of these, only Mainzer et al., 2011c proved relevant, discussing the explicit use of ROS diameters. Masiero et al. cite Mainzer et al. a number of times in different contexts. The most relevant citation is:

"We obtained our data used for fitting in a method identical to the one described in Mainzer et al. (2011b) and A. K. Mainzer et al. (2011, in preparation), though tuned for MBAs. Specifically …"

In their elaboration of how they collected their data (the "Specifically …" part) Masiero et al. say nothing about requiring any of the objects they analyze to have independent ROS diameters nor do they mention collection any ROS diameters. Although Mainzer et al. could be viewed as a hint that Masiero et al. use ROS diameters, nothing in my reading of Masiero et al. itself states or implies that they use ROS diameters, and in my reading of Masiero et al. and Mainzer et al. together, only the coincidence of the values of a subset of the diameters in Masiero et al. with those in Mainzer et al. indicate that Masiero et al. use ROS diameters.

There are a number of places in the paper, particularly in Sections 3 and 10, where the authors described differing treatment of different classes of object. But, to reiterate what I said above, in no place to I see a statement, or even a hint, that objects for which ROS diameters are available were treated differently.

Table 1 gives thermal-model diameter estimates together with error bars. From text elsewhere in the paper, it would seem that these error bars have been generated with a model-driven Monte Carlo process (but I have not found a description of this process in the paper). However, comparing the error bars for the ROS diameters with, for example, those in Mainzer et al., we see that those error bars are, as are the diameters themselves, input values, rather than results of the model. Again, there is no suggestion in Masiero et al. that these error bars have been put in by hand.

(Myhrvold also complains that the right approach would have been to run the analysis completely blind to the ROS values, and then use the ROS values to measure the errors due to the thermal modelling. I think he's absolutely right about this. Not to have done so is, in my view, poor science.)

Let me note that I have no dog in this fight. Also, you don't have to trust that I'm not making stuff up here or spinning things in some artful way. Look at the papers yourselves. For these ROS values to be presented as results of the thermal modelling with no discussion — or even hint — of where they actually came from is either willfully misleading or incredibly sloppy.

59. **Anonyrat**
May 28, 2016

Sorry, I intrude again. I have to disagree with Astronomical Cowardice.
http://arxiv.org/pdf/1105.0975.pdf
Table 1 has the caption: "Table 1. Spherical NEATM models were created for 50 objects ranging from NEOs to irregular satellites in order to characterize the accuracy of diameter and albedo errors derived from NEOWISE data. The diameters and H
values used to fit each object from the respective source data (either radar, spacecraft imaging, or occultation) are given…..".

Column #1 of the table is the object ID.
Column #2 of the table is the diameter in km and Column #3 is the H value from the (radar, spacecraft imaging or occultation data).
Column #8 is a pointer to the reference to the source of column #2 and #3.

Thus Object #47, with a diameter of 138 +/- 13 km and H of 7.8 comes from reference d, which is Shevchenko, V., & Tedesco, E., 2006 Icarus, 184, 211. Via scholar.google.com one can find this on researchgate.net, where you find Object #147 is the asteroid Aglaja, and it is ascribed a diameter of 138.0 km. You can read the paper and see the details of how that result of 138.0 km was reached (starting with an occultation measurement in 1984.)

It is very clear from the text of the preprint that this preprint describes a calibration of NEOWISE, and that after calibration but with Monte Carlo of measurement uncertainties, NEOWISE gives the diameter of 138 +/- 13 km.

Perhaps the preprint authors should have had two columns, one with the 138.0 km from reference d, and the second with the NEOWISE answer of 138 +/- 13 km. On the other hand, presumably anyone intimately familiar with the literature (or anyone who bothered to read the preprint carefully enough) would know what the condensed table meant.

Yes, the format the authors used will trip up the unwary. On the other hand, it allows their table to neatly fit the width of the page.

My apologies for breaking my promise to exit this thread. I have no stake in these games; I am not a professional scientist, nor affiliated with any science research institution (I am an employee of a telecom company.).

60. **Igor Khavkine**
May 28, 2016

@Anonyrat, your commentary has been very insightful and helpful. Thank you. It seems that the main obstacle to resolving the controversy here is the communication barrier between the scientists who share a common knowledge base related to asteroid astronomy and those who don't, despite being technically competent in other ways. Anyone who has worked in a narrow field would probably agree that such communication barriers are ubiquitous. It's often simply impossible to include all the details of this background information when writing an isolated paper. In fact, including such details can sometimes be seen as tedious repetition by the intended expert audience.

61. **Henry L.**
May 28, 2016

@ Igor Khavkine
—
You make the excellent relevant point "… that the main obstacle to resolving the controversy here is the communication barrier between the scientists who share a common knowledge base related to asteroid astronomy and those who don't, despite being technically competent in other ways." I belong to the latter category, my competency being nuclear physics (LANL, retired). This is why, I believe, such a heated controversy here (in the comments section of the personal blog of an expert in "Quantum Theory, Groups and Representations") seems out of place.

62. **Astronomical Cowardice**
May 28, 2016

In response to Anonyrat's comment, Anonyrat has mixed up two of the papers I discussed.

I criticize Masiero et al.; Anonyrat defends Mainzer et al., the paper I did not criticize.

I apologize for leaving the door open to confusion. I should have been more detailed with paper titles and links in my original comment. Let me correct that now.

I discuss three papers (whose lead authors all begin with the letter 'M'):

The first I refer to as "Myhrvold's arxiv preprint." Its title is:

"Asteroid thermal modeling in the presence of reflected sunlight with an application to WISE/NEOWISE observational data"

and it can be found here:

https://arxiv.org/ftp/arxiv/papers/1605/1605.06490.pdf

The second I refer to as "Masiero et al., 2011" (and in abbreviated form as "Masiero et al."). Its title is:

"MAIN BELT ASTEROIDS WITH WISE/NEOWISE. I. PRELIMINARY ALBEDOS AND DIAMETERS"

and it can be found here:

http://iopscience.iop.org/article/10.1088/0004-637X/741/2/68/pdf

This is the paper I criticize. For completeness, it is cited in Myhrvold's arxiv preprint as:

"Masiero, J.R., Mainzer, A., Grav, T., Bauer, J.M., Cutri, R.M., Dailey, J., Eisenhardt, P.R.M., McMillan, R.S., Spahr, T.B., Skrutskie, M.F., Tholen, D., Walker, R.G., Wright, E.L., DeBaun, E., Elsbury, D., Gautier IV, T., Gomillion, S., Wilkins, A., 2011. Main Belt Asteroids with WISE/NEOWISE. I. Preliminary Albedos and Diameters. Astrophys. J. 741, 68. doi:10.1088/0004-637X/741/2/68"

The third I refer to as "Mainzer et al., 2011c" (and in abbreviated form as "Mainzer et al."). Its title is:

"THERMAL MODEL CALIBRATION FOR MINOR PLANETS OBSERVED WITH WIDE-FIELD INFRARED SURVEY EXPLORER/NEOWISE"
and it can be found here:

http://iopscience.iop.org/article/10.1088/0004-637X/736/2/100/pdf

I did not criticize this paper. Anonyrat defends its arxiv preprint version (see below). For completeness, this paper is cited in Myhrvold's arxiv preprint as:

"Mainzer, A., Grav, T., Masiero, J., Bauer, J.M., Wright, E.L., Cutri, R.M., McMillan, R.S., Cohen, M., Ressler, M., Eisenhardt, P.R.M., 2011c. Thermal Model Calibration for Minor Planets Observed With Wide-Field Infrared Survey Explorer/Neowise. Astrophys. J. 736, 100. doi:10.1088/0004-637X/736/2/100"

and in Masiero et al. as:

"Mainzer, A. K., Grav, T., Masiero, J., et al. 2011b, ApJ, 736, 100"

Anonyrat discusses in his comment:

http://arxiv.org/pdf/1105.0975.pdf

This paper is titled

"Thermal Model Calibration for Minor Planets Observed with WISE/NEOWISE"

and, as indicated above, is the arxiv preprint version of Mainzer et al.

In my original comment I say of this paper:

"Note that Mainzer et al., 2011c analyze thermal models for 50 objects specifically for which ROS diameters are known, and make explicit that they use and report these ROS diameters. Masiero et al. cite this paper, as I discuss below."

That is, I make a point of acknowledging that I find nothing misleading about the use and reporting of ROS diameters in Mainzer et al.

Sorry to be so pedantic about all of these references, but once burnt, twice shy.

To recapitulate, I criticize Masiero et al., and not Mainzer et al. Anonyrat has mixed the two up. I stand by my original conclusion that Masiero et al. "is either willfully misleading or incredibly sloppy."

63. **tt**
May 28, 2016

but they cite the Mainzer articles. this is such a non-issue.
they were not hiding that these values were from other sources.
they are identical to the other sources. Alls it takes is asking the author
and they replied yes its from other sources.

64. **Anonyrat**
May 28, 2016

My apologies if I've confused between papers.

IMO, having seen this kind of thing before, the problem is the lack of trust on either side. We can imagine what each side thought of the other, but let's not got there.

Now that the dispute has gone to the press, attitudes would likely have hardened, and it is going to be difficult to restore trust. In the interest of science, I urge both sides to stop talking to the press, give it a month's break to cool tempers down, and then dedicate a couple of days of face-to-face conference to walk through all the material. Perhaps a mutually respected person should be present, not to decide technical issues but to keep the proceedings parliamentary. Surely doing something like this is within the resource budgets of both sides.

65. **JeanTate**
May 29, 2016

I too have read the key papers Myhrvold cites, in his "A Simple Guide to NEOWISE Data Problems".

"Main Belt Asteroids with WISE/NEOWISE. I. Preliminary Albedos and Diameters" (Masiero+ 2011; doi:10.1088/0004-637X/741/2/68) is not as clear as it could be, re the extent to which the diameters in Table 1 are derived/sourced entirely/solely from "thermal model fits". On its face this seems to support what both Myhrvold and Astronomical Cowardice write (more later).

"NEOWISE Observations of Near Earth Objects: Preliminary Results" (Mainzer+ 2011; doi:10.1088/0004-637X/743/2/156) is quite different. As Anonyrat points out. Myhrvold: "There is no explanation in those papers that diameters were copied, let alone a justification for why it was done." That may be so for doi:10.1088/0004-637X/741/2/68; it is clearly not, for doi:10.1088/0004-637X/743/2/156. Here is an extract from the caption to Table 1, of the latter: "Two calibration papers (Mainzer et al. 2011b, 2011c) discuss the absolute calibration of the WISE data for small solar system bodies and should be consulted before
comparing with data derived from other sources." Note that "Mainzer et al. 2011b" is "Thermal Model Calibration for Minor Planets Observed with Wide-Field Infrared Survey Explorer/NEOWISE", doi:10.1088/0004-637X/736/2/100

Yes, it may be true that, as Myhrvold writes, "What Mainzer and coworkers mean by "calibration" is debatable, but they appear to mean more in the sense of validation — that when they calculate their color correction (to adjust to the properties of the WISE sensor), they get roughly the same observed IR flux from the test objects using the ROS diameters as they see from the asteroids they represent."

However, as I have noted before, astronomers often use conventions, terms, methods, etc which *seem* to be the same as those in other fields of science – and indeed they often are – but may not be (they are sometimes not even consistent, among themselves; "flux" and "extinction" or "attenuation" are notorious examples). Naturally, this can create confusion and misunderstanding when an outsider tries to fully understand a published astronomy paper. Of the three alternatives Myhrvold gives (colossal error, fraud, something else), I think the last is by far the most likely … he simply didn't fully understand what he read.

Returning to "Main Belt Asteroids with WISE/NEOWISE. I. Preliminary Albedos and Diameters". Astronomical Cowardice writes "For these ROS values to be presented as results of the thermal modelling with no discussion — or even hint — of where they actually came from is either willfully misleading or incredibly sloppy." Yes, to a complete outsider this seems a reasonable conclusion. Sadly, I think it's neither; this sort of thing can, IMHO, be found in thousands of published astronomy papers. And many a peer reviewer would not even notice.

I'll end with a hobby-horse: early in his "Simple Guide", Myhrvold cites "Combining asteroid models derived by lightcurve inversion with asteroidal occultation silhouettes" (Ďurech+ 2011) and "Asteroid albedos deduced from stellar occultations" (Shevchenko&Tedesco 2006). Unless you have a cool $71.90 to spare (or a backdoor), you cannot read either. If one wants to get outraged by anything, high up on the list, surely, is why so many Icarus papers are behind paywalls? Personally, I think it's particularly outrageous in these two papers: without the, freely given, observations of many amateur astronomers, neither paper could have been written.

66. **Anonyrat**
June 1, 2016

http://arxiv.org/pdf/1104.4227.pdf
Combining asteroid models derived by lightcurve inversion with asteroidal occultation silhouettes

67. **Anonyrat**
June 1, 2016

https://www.researchgate.net/profile/V_Shevchenko/publication/238579042_Asteroid_albedos_deduced_from_stellar_occultations/links/5405eefb0cf2bba34c1df0c1.pdf
Asteroid albedos deduced from stellar occultations

68. **JeanTate**
June 3, 2016

Thanks for digging up these two, Anonyrat.

The second is (almost) certainly what was actually published in Icarus; the first not quite so. In the arXiv abstract (http://arxiv.org/abs/1104.4227), the Comment is "33 pages, 45 figures, 4 tables, accepted for publication in Icarus", which strongly implies it's the same as what (later) was published. However, this is caveat lector; no one is charged with checking that it's accurate, and while authors almost invariably have good intentions, they do sometimes make mistakes. Further, the PDF contains this, at the bottom of the first page: "Preprint submitted to Icarus", making the ambiguity worse.

69. **Peter Erwin**
June 4, 2016

Further, the PDF contains this, at the bottom of the first page: "Preprint submitted to Icarus", making the ambiguity worse.

That's just boilerplate text generated by the LaTeX style file, not something the authors explicitly wrote themselves.

70. **JeanTate**
June 4, 2016

@Peter Erwin thanks for that! Question: how could an outsider ever learn that? Presumably it's something unique to style sheets used by authors thinking of submitting to Icarus, I guess.

71. **Low Math, Meekly Interacting**
June 5, 2016

The divergent accounts of Dr. Myhrvold and the NASA-affiliated investigators make it extremely difficult to figure out who, if anyone, has portrayed the situation with sufficient accuracy to draw firm conclusions from an outside perspective. If there are any uncontroversial statements that can be made, it would appear that Myhrvold's attempts to build a model using basic physics principles is still outperformed by NASA's empirical model, at least in some key instances, though that situation may change with further refinement. Also, the NASA teams' data reporting lacks sufficient clarity to satisfy an outsider that they've been anything but sloppy, at best, and at worst malfeasant.

I am fully in agreement with Phil Plait's article on at least one point: No matter which (if any) of Myhrvold's accusations proves to be correct, they are all quite troubling (with varying degrees of severity), and some publicly-available and well-vetted account of the dispute's resolution is very much in order. I fear that won't happen, given the attention span of modern media, despite the great importance of this story. We have on the one hand an argument for the need for transparency and public accountability of govt.-funded agencies and experiments, and on the other the legitimacy (real or perceived) of the “outsider” scientist as a watchdog. The outcome won’t resolve that age-old tension by any means, but I think it could prove to be a critical bit of history informing the perennial clash of public-vs.-private, insider-vs.-outsider, peer/expert-vs.-well-informed-amateur.

Most unfortunate is the tone, e.g. Dr. Wright’s gratuitous insults. Even worse are the allegations by Dr. Myhrvold that he was compelled to “go public” because of concerns about his work getting fair treatment during peer review. This could legitimately be characterized as “poisoning the well”, and I think he should have waited to see if his suspicions proved justifiable.

To echo Phil Plait, I do very much hope all concerned are as willing to be as vocal and thorough as they have been leading up to peer review of the Icarus article once that process is finished.

72. **Wayt Gibbs**
June 9, 2016

Myhrvold published of the paper to arXiv on June 1: http://arxiv.org/abs/1605.06490

He posted a lay-language explanation of why he finds the NEOWISE group’s use of observations in place of thermal model computations for a few selected asteroids so troubling to Medium on May 25: https://medium.com/@nathanmyhrvold/a-simple-guide-to-neowise-data-problems-a93f41e3bdb4#.kwe9iujo8

He has elaborated in three posts (so far) to the Minor Planets Mailing List on Yahoo on why some of the complaints about his paper in NASA’s press release were off base and has also replied to Herald’s suggestions about using occultation observations: https://groups.yahoo.com/neo/groups/mpml/conversations/topics/32025

He recently gave a talk at the Code Conference on asteroids that briefly touched on the controversy over his paper: https://www.youtube.com/watch?v=CH4k4kNBpN8

73. **Jan Reimers**
June 10, 2016

Maybe someone has already said this, but there seems to be a number of orthogonal issues here
1) Myhrvold’s pre-print has some errors in it. Perhaps already corrected in fourth revision.
2) Myrvold’s bootstrap model predicts some erroneous diameters.
3) Myrvold’s is alleged to have gone running to the press before peer review. Or did the press run to Myrvold?
4) Myrvold makes some harsh accusations about integrity and professionalism.
5) The NEOWISE team do not show what their model predicts for the asteroids for which ROS data are available. Instead they jumble the ROS numbers in same table as the IR model predictions. So we get no indication of how well the NEOWISE IR model works.

As far as I can see the status 1,2,3,4 has absolutely nothing to with whether or not #5 is correct but NASA is using 1,2,3,4 as a smoke screen to deflect attention from #5.

I think a good lesson from this is if you want people to pay attention to something like #5, then don’t do 1,2,3,4.

JR

74. **Wayt Gibbs**
June 11, 2016

Myhrvold put a corrected revision of his article on arXiv on June 1: http://arxiv.org/abs/1605.06490

He posted a plain-English explanation of the reasons that the accuracy of the NEOWISE estimates cannot be determined from their published papers on Medium on May 25: https://medium.com/@nathanmyhrvold/a-simple-guide-to-neowise-data-problems-a93f41e3bdb4#.kb12omv42

He has responded to Dave Herald’s suggestions and some of the misleading statements in the NASA/JPL press release on the Minor Planets Mailing List on Yahoo: https://groups.yahoo.com/neo/groups/mpml/conversations/topics/32025

A story by Scientific American on the debate on May 27 provides some perspectives from astronomers not directly involved WISE or NEOWISE: http://www.scientificamerican.com/article/for-asteroid-hunting-astronomers-nathan-myhrvold-says-the-sky-is-falling1/

Myhrvold gave a short talk at the recent Code Conference on asteroids, and briefly mentioned the debate over asteroid sizes: https://youtu.be/CH4k4kNBpN8

Summing It Up

May 29, 2016
Categories: Book Reviews

There's a new popular book out about number theory by the team of Avner Ash and Robert Gross, entitled [Summing It Up: From one plus one to modern number theory](). This is the third such book that they have written, and I'm embarrassed that it seems that I never reviewed the other two here. All three are highly recommended for anyone who wants a popular book-level introduction to some of the central topics of modern number theory.

- *Summing It Up* is the latest of the three, but it's also the most elementary. It's an introduction to the subject of modular forms, starting at the very beginning. The first half of the book covers in detail some basic ideas about number theory that can be understood in elementary terms, including things like the problems of counting the ways an integer can be a sum of squares or higher powers, or partitioned as a sum of smaller integers. The second half of the book tries to explain in as simple and concrete terms as possible what a "modular form" is, and what some of the properties of such objects are.

  I've just finished teaching a graduate course on representation theory, and ended up the course with a short discussion of the representations of the group SL(2,**R**) (two by two real matrices of determinant one), and what this had to do with modular forms. The relation of modular forms to representation theory (not discussed in the book) is roughly the following. The action of SL(2,**R**) on itself by left multiplication induces an action on functions on the quotient space SL(2,R)/SL(2,**Z**) (elements of SL(2,**Z**) are matrices with integer entries) and one can ask how this representation decomposes into irreducible representations, with modular forms providing part of the answer. How this works is quite basic to our modern understanding of how representation theory and number theory are related.

  In the last few chapters the authors try to explain how modular forms answer concrete counting questions raised in the first half of the book. A problem with trying to do this kind of thing is that while the questions may be straightforward to state, and the basic definitions of modular forms relatively accessible, connecting the two requires invoking some subtleties (half-integral modular forms). The authors several times apologize for not being able to explain exactly what is going on. I've always been quite fascinated by these particular subtleties, since they appear in the basic relationship of representation theory and quantum mechanics. The half-integrality here is related to the half-integer that appears in the ground-state energy of the harmonic oscillator. For quite a bit about how this is related to representation theory, see the [book I've been working on](). I'd love some day to write about the relationship of the ideas that appear in quantum mechanics to the ones that appear in modular forms, but first of all I need to understand myself much better what is going on.

- The first of the three books by Ash and Gross was [Fearless Symmetry: Exposing the Hidden Patterns of Numbers](), published in 2006. It's perhaps the best place to start for a popular introduction to the Langlands program and what it says about the relationship of number theory and representation theory. Modular forms make a brief appearance in that book, where an explanation of them had to be skipped over due to a lack of space. The newest book makes up for that omission.
- The second of the three books was the 2012 [Elliptic Tales: Curves, Counting, and Number Theory](). It covers in detail the topic of elliptic curves and their role in number theory, aiming at a description of one of the main open problems of the subject, the conjecture due to Birch and Swinnerton-Dyer. This is a very active subject of current research, with significant progress being made. If one had to guess which of the Millenium Problems will be the next to fall, this might be a good bet.

While there's a long tradition of popular books about number theory, these typically emphasize elementary methods for solving problems, ones that can be understood without a lot of the modern machinery. The first two Ash-Gross books do a good job of trying to give some insight into this modern machinery, even though a popular book can only do this in a very limited way (just as popular physics books can only give a very limited explanation of quantum field theory). The new one is different in that it makes a serious effort to explain exactly what is really going on, although following this path means that the book can only cover the first steps in the direction of modern techniques for understanding number theory.

Comments

1. **Justin**
   May 29, 2016

   Can someone explain why the Riemann hypothesis isn't related to Langlands? The hypothesis is one of the deepest in number theory, and it just seems to me that Langlands should have something deep to say about it.

2. **John Fredsted**
   May 30, 2016

   Once again your blog has provided me with inspiration as to what book to buy/read next. This time the (third) book by Ash and Gross. Hopefully, a physicist like I will be able to digest it; if not, then no hard feelings, of course. Thanks for your blog.

3. **Andrei**
   May 30, 2016

   "If one had to guess which of the Millenium Problems will be the next to fall, this might be a good bet."

   Funnily, my master's thesis' advisor told me that BSD was like a 27th century-level conjecture or something.

4. **uair01**
   May 30, 2016

   I have the first two books and I find them fascinating. I have studied electrical engineering, so I have some practical math skills, but the books are still challenging. I must applaud the authors for their audacious approach. It is not a light read (like "Love and Math" by Frenkel), but neither is it an academic book (like "Essential topology" by Crossley). But the books showed me some real modern mathematics that I never tasted in university. I will buy the third book when I've finished the second.

5. **uair01**
   May 30, 2016

   Note: If you buy the books you should consider buying the paper version. The Kindle version is not very good with the formulas. The Kindle text is ASCII but the formulas are pictures, so they don't scale with the text. The Kindle version is still readable, but the paper version is much more comfortable.

6. **John Fredsted**
   May 31, 2016

   @uair01: For reading pleasure, there is nothing like a book of paper in your hands. Books should always be read this way, I think, not looking on some electronic screen. But perhaps it is just me that is old-fashioned, or getting old, for that matter :-).

7. **Henry L.**
May 31, 2016

@ John Fredsted
—

Perhaps you are old-fashioned. As for getting old, there is no doubt 🙂

8. **Martin H Krieger**
May 31, 2016

Question: Why is counting up related to scaling symmetry?

One of the recurrent curious phenomena in mathematics and in physics is that counting problems, where we seem to be adding up one-by-one, packaged in a suitable function (such as Riemann's zeta), are then shown to be related (through that packaging function) to a scale invariant function (here the theta functions, which are modular). The Langlands Program is connects a counting function (in a Galois representation) to an automorphic form and representation, meeting in their respective L-functions.
Similarly, the 2d Ising model, solved in many ways by adding up one-by-one the interactions, that is, exp -beta Ei, and packaging them together in the partition function (whose log is the negative of the chemists' free energy), can also be solved by means of renormalization (scaling up from 2 by 2, to 4 by 4, …), as done by Kadanoff and Wilson, and also by Hilhorst, Schick, and van Leuven. And, as pointed out by J-M Maillard, the basic symmetries of that Ising model are embodied in two quadratic forms in the Boltzmann factors (see Baxter's book, chapter 10), whose intersection is an elliptic curve.

9. **former mathematician**
May 31, 2016

It is a happy coincidence that this is published so soon after the Ramanujan biopic opened.

10. **Henry L.**
May 31, 2016

@ former mathematician

I had not heard about the film, "The Man Who Knew Infinity", the Ramanujan biopic. Thanx for mentioning it.

11. **Shantanu**
June 3, 2016

Peter, sad news. Tom Kibble passed away.
Maybe would be nice to hear some tributes from you on him and his impact on particle physics

12. **Peter Woit**
June 3, 2016

Shantanu,
Sorry to hear that. Unfortunately I never met Kibble and am not well-informed about him and his work, so best I leave this to others.

13. **Shantanu**
June 3, 2016

Not even in EW symmetry breaking and his role in discovering the Higgs mechanism?
Around 2012, Frank Close was backing him to win the Nobel for his role in Higgs discovery.

This Week’s Hype

June 4, 2016
Categories: Multiverse Mania, This Week's Hype

One possible reaction to the phenomenon of hype in fundamental physics is to not worry much, figuring that it should be a self-limiting process. While there’s a huge appetite in the media and elsewhere for the “exciting new idea”, overhyped “new” ideas sooner or later should pass into the category of no longer “new”, and less capable of producing “excitement”. The problem is that this doesn’t seem to be happening: favored physics hype keeps getting promoted as “new” and “exciting”, no matter how old it is.

In the case of multiverse hype, Andrei Linde was promoting the idea 34 years ago, back in 1982. That hasn’t stopped many people from heavily promoting it as “new” for quite a few years now. Taking this to a new level, a [talk by Martin Rees] this past week at the Hay Festival advertised the multiverse as not just exciting, but so new as to be one of the main developments in physics of the past year:

> The astronomer will share his excitement about recent cosmic ideas and discoveries. Since last festival there have been new searches for life (even intelligent life) in space. One of Einstein’s greatest predictions has been confirmed with the detection of gravitational waves from colliding back holes. Images of Pluto have surprised us, and astronomers have discovered thousands of planets orbiting other stars, some resembling Earth. And there is speculation that physical reality encompasses more than the aftermath of our big bang: we may inhabit a multiverse.

Lord Rees explains in more detail [in the Telegraph] how exciting this is. It seems that he has been excited about this for more than a quarter century, with a [book on the subject back in 1989]. Since at least 2003 and a Templeton-funded Stanford conference on the multiverse, he has been publicly expressing willingness to bet his dog’s life on the existence of the multiverse, and he repeats that in the Telegraph article (should someone contact the RSPCA?). Luckily for the Rees family pets, there’s no way to ever resolve this issue, so the last couple generations have survived, and so will further ones.

Update: Also this past week, in the category of hype that will never die, Scientific American has [Gravitational Waves Could Finally Help Us Prove String Theory]. This particular hype campaign goes back at least a dozen years. See [here] for a 2004 blog post about a [UCSB press release] featuring claims that LIGO might produce evidence for string theory in 2005 or 2006.

Comments

1. **Peter Woit**
   June 4, 2016

   physicsphile,

   I think it was Weinberg who came up with that, here
   https://arxiv.org/abs/hep-th/0511037
   I never knew what to make of that, not knowing how Weinberg felt about either dogs or Andrei Linde…

2. **Neil**
   June 4, 2016

   Gravitational waves, Pluto pics, and exo-planets. Ergo, the multiverse?

3. **Nick M.**
   June 5, 2016

   How strange! It would appear that some physicists like wagering on a class of ***“Not Even Wrong”*** ideas, producing what can only be described as ***“Not Even A Gamble”***. 😉

4. **Krzysztof**
   June 6, 2016

   The Big Bang term, coined by Hoyle, was used to ridicule Lemaître’s idea.

   It seems that the most illustrious British astronomers keep a habit of ridiculing themselves.

5. **John**
   June 6, 2016

   Will anyone take this bet? Give me \$100 now and when the multiverse is proved I will pay you \$1 million.

6. **Scott Church**
   June 6, 2016

   One small problem John… You may encounter some resistance regarding what constitutes “proof.” Next thing you know you’ll find yourself in a math “beauty” pageant, and if book deals and grants are involved Miss Multiverse’s lawyers might be pretty damn good. 😉

7. **Justin**
   June 6, 2016

   On a related but more positive note, I think your readers will enjoy the latest at Backreaction. http://backreaction.blogspot.com/2016/06/dear-dr-b-why-not-string-theory.html

8. **Harrison**
   June 6, 2016

   Justin, thanks for pointing out that write-up, and site for that matter. Some great stuff.

Short Items

June 8, 2016
Categories: Uncategorized

- String theory continues to make progress. Today the news is from Megan Fox:

  > "Sometimes I just know things," she explains. "I accidentally tap into stuff sometimes. I used to do it as a kid, and I do it as an adult. I crossed over and saw a future string."
  >
  > String, as in string theory. Fox is into stuff like that. She's also spiritual. On her Instagram profile, she describes herself thusly: "Child of the Cherokee Tribe … forest nymph … Lunar Leo mother goddess to 2 bohemian revolutionaries-my kamikaze free spirit & my peaceful warrior."

  A few months ago it was Jaden Smith moving the subject forward:

  > Jaden sees himself as a modern-day prophet and is working on a collection of essays," a pal says in the new issue of Us Weekly. "They're new takes on string theory and chaos theory, but more mystical."
  >
  > After all, he's getting an out-of-this-world assist with the tome. Explains the source, "Jaden thinks he has spiritual ties to people in other dimensions and galaxies, and they are helping him write."

- At some sort of other extreme, Sabine Hossenfelder has very sensible things to say about the string theory phenomenon here.
- If you read Physical Review Letters or the Financial Times you might think that a "key to an unseen portion of the universe" had been found. Luckily for you, Natalie Wolchover is on the case, uncovering the story of why you might not want to take that new fifth force seriously quite yet.
- If you're interested at all in the story of the superluminal neutrinos, you might want to read Gianfranco D'Anna's fictionalized account of the story, 60.7 Nanoseconds, which has just appeared in English

Update: This string theory story is so bizarre I don't know what to make of it:

> While working on String Theory, Kaku, discovered what he sees as evidence that the universe is created by an intelligence, rather than merely formed by random forces. He suggests he can explain it by what he calls, "primitive semi-radius tachyons." We do not yet have a succinct explanation of this idea from Kaku, other than he's referring to tachyons, which are theoretical particles that unbind particles from one another.
>
> Without getting into physics itself, Kaku concludes that we live in a Matrix-style universe, created by an intelligence.
>
> "I have concluded that we are in a world made by rules created by an intelligence", he said. "Believe me, everything that we call chance today won't make sense anymore. To me it is clear that we exists in a plan which is governed by rules that were created, shaped by a universal intelligence and not by chance."

Comments

1. **Bee**
   June 8, 2016

   The other day there was a poster hanging in our local supermarket, offering a weekend-seminar on M-theory based quantum healing. It was amazingly costly. Thanks for the link 🙂

2. **Another Anon**
   June 8, 2016

   So, the M stands for "massage". Now we know.

3. **Veríssimo**
   June 8, 2016

   They should invite Megan Fox to host the next Milner prizes. Not only because of her looks. It also seems like that she, unlike Kevin Spacey, would be interested in mingling with the scientists.

4. **Harrison**
   June 8, 2016

   I listen to peoples' apparent first-hand experiences with "other dimensions" and treat them like a Poe short story. Vivid, but at the end of the day it's all jewelry. Comically aslant and thus ineffective.

5. **Michael**
   June 8, 2016

   Re: Fifth force. Let's not forget Nature!

   http://www.nature.com/news/has-a-hungarian-physics-lab-found-a-fifth-force-of-nature-1.19957

6. **David Hansen**
   June 8, 2016

   That first item is effing amazing.

7. **student**
   June 8, 2016

   Dr. Hossenfelder's following remarks are quite too the point:

“So far, string theory has scored in two areas. First, it has proved interesting for mathematicians. … Second, string theory has shown to be useful to push ahead with the lesser understood aspects of quantum field theories. This seems a fruitful avenue and is certainly something to continue.”

The part of the argument that is harder to follow is that other approaches to QG will have a more significant relevance to applications.

“And since that theory underlies all modern technology, this is research which bears relevance for applications.”

Just a casual glance at the arXiv today shows

[arXiv:1606.01893] “Particle-Vortex Duality from 3d Bosonization”
[arXiv:1606.01989] “A Duality Web in 2+1 Dimensions and Condensed Matter Physics”

which is an example of how the rich interplay between ideas in the condensed matter theory, high energy theory, string theory, and even mathematics via mirror symmetry can be applied to problems in condensed matter. This hopefully is an example of the “fruitful avenue” Dr. Hossenfelder suggested and will have an impact on applications faster than “Not in ten years and not in 50 years, but maybe in 100 or 500 years.”

8. **Shantanu**
June 8, 2016

Peter, ongoing workshops on status of supersymmetry and LHC searches at KITP
http://online.kitp.ucsb.edu/online/experlhc16/
and
http://online.kitp.ucsb.edu/online/lhc16/

9. **Peter Woit**
June 9, 2016

student,
The problem is that the papers you quote are interesting work on QFT, but don’t really have any significant connection to string theory, and certainly not to the main topic Sabine was writing about, string theory as a theory of quantum gravity.

10. **Bernhard**
June 9, 2016

Whatever Kaku has been smoking must be really strong stuff. How someone get away with saying these absurdities and still be taken seriously by anyone (assuming that’s the case) is a beyond me.

11. **Dom**
June 9, 2016

Can I offer the suggestion that Michio Kaku has a calendar fault as this was clealy intended to be an April Fool article.

12. **Peter Woit**
June 9, 2016

Bernhard/Dom,
I’m pretty mystified by the Kaku piece. Quite possibly some of the odd stuff is due to the writer not Kaku (“primitive semi-radius tachyons”???). It’s also true that lots of prominent theorists now sit around publicly debating whether we’re a simulation, which may be what Kaku is talking about, see for instance
http://www.math.columbia.edu/~woit/wordpress/?p=8392

13. **Northern Jock**
June 9, 2016

Just an opinion but I think what you do is important on this blog and that you have good judgement generally speaking, but that it’s a bad idea to go down the road of mocking. It’s important stick with the problems associated with String Theory, it’s theorists, and advocacy with some connection to the community or where there some direct responsibility on the part of the community.
Otherwise…well I mean you could go onto YouTube and find stupendous interpretations and claims about anything you like. And I think that’s an argument that could be thrown back at you, that would have some influences with some people, for a limited interval of time.

14. **David Levitt**
June 9, 2016

I teach an upper level course on medical devices and I show Michio Kaku’s Daily Show interview (pushing his book “The Future of the Mind”) as a classic example of the nonsense that is in this field. Almost every statement of Kaku’s is factually wrong. I suppose I am old fashioned, but I worry about everything I say in lecture, and I am a chagrined if something is not quite right. And here, Kaku can go out before millions of viewers and just spew gobbledygook???

15. **Bernhard**
June 9, 2016

Peter,

I honestly don’t understand the purpose of these debates and to me they are a symptom of how sick our field became. We have plenty of interesting, difficult, down to earth problems to solve and work with.

16. **Peter Woit**
June 9, 2016

Northern Jock,
I understand your point, but the string theory hype machine is still active and I think pointing to the nonsense that it leads to is justifiable. Mostly I do just point without mocking, sometimes I can’t resist temptation.

17. **Northern Jock**
June 9, 2016

Peter Woit – That's entirely understandable and you are very entertaining as well as informative.

18. **Bernhard**
June 9, 2016

Northern Jock,
I wold also add to what Peter is said is that Kaku is not your typical random crank that you would find on youtube. I remember when I was in undergraduate school some people were studying QFT with his book. Not to mention he has a prestigious position as a professor of theoretical physics, so his voice is taken much more seriously by the media and, even worse, by young unwary minds.

19. **Henry L.**
June 9, 2016

Kaku also appears in a Samsung Galaxy S7 Edge commercial, so he is cashing in on some of his notoriety.

20. **zzz**
June 9, 2016

Kaku appears on the morning news shows for anything sciencey.
He doesn't study hurricanes or tornados, but i have heard him speak about them

21. **Northern Jock**
June 9, 2016

Bernard – I wasn't talking about Kaku, who is fair game if he wants to carry on as he does. I was talking about the minor celebrity and the teenage son of a celebrity. I think advocacy of that kind tends to happen randomly wrt the merits of the subject.

22. **Neil**
June 9, 2016

Uh, oh. If Kaku is right, our programmers have been caught out, and we are all in for a reboot.

23. **Rabbit**
June 9, 2016

…"even worse, by young unwary minds." There's no helping unwary minds, young or old.

24. **A**
June 9, 2016

Peter

There are countless crackpots out there who have their own take on relativity, but that does not mean that relativity is wrong. So I don't regard the musings of such people to be any kind of negative statement on strings. Do you?

Regarding Kaku, I couldn't get the video to play. Does he really say that stuff? The organization that published this page doesn' automatically have credibility of course.

25. **Dr. Michelson Moreley**
June 9, 2016

A,

There are countless crackpots out there who have their own take on unicorns, but that does not mean that unicorns exist. So I don't regard the musings of such people to be any kind of positive statement on strings. Do you?

26. **Peter Woit**
June 10, 2016

A,
Of course the views of crackpots about a theory have nothing to do with its value. The tradition though is that celebrity crackpots latch on to successful physics theories that everyone has heard about (e.g. relativity and quantum theory), but the innovation is that now they're latching on to an unsuccessful theory that everyone has heard about, not knowing the difference.

27. **A**
June 10, 2016

@dmm
I don't fully understand your post but I'm going to go with "No".

@peter
Lol.

28. **Narad**
June 10, 2016

> Today the news is from Megan Fox

Oh, dear:

"She and costar Shia LaBeouf [*of Walgreens fame*] were given a tour of of the Great Pyramid of Giza by the Ministry of Antiquities and someone 'high-ranking in that field — I will not say who' told the actors that the pyramid was never actually a tomb.

"'They presume they may have been some type of energy plant at some point,' says Fox."

Even [Phil Lesh](#) was more sophisticated, and I'll bet the shows (which weren't very good aside from Hamza, IMO) at least produced more inquiry into the nature of the observer and the observed, as well as a fair amount of energy.

29. **Richard J. Gaylord**
June 10, 2016

""I have concluded that we are in a world made by rules created by an intelligence", he said. "Believe me, everything that we call chance today won't make sense anymore. To me it is clear that we exists in a plan which is governed by rules that were created, shaped by a universal intelligence and not by chance.". while i admire Kaku's hair much more than i do his books, i think we need to be careful here because this is a quote attributed to Kaku, and many academicians i know have been either misquoted or had their ideas distorted by others with their own agenda. For example, Einstein has often been said to believe in God but for Einstein, God was simply shorthand for nature (or nature's laws). i expect that Kaku realizes that invoking a "universal intelligence" is not a useful scientific explanation of any phenomenon.

30. **Shantanu**
June 10, 2016

Peter: sorry for the OT comment. But looking forward to your report on talks at the LHC conference at KITP, esp. the supersymmetry ones.

31. **Jim Given**
June 10, 2016

Kaku actually says, "a Matrix-style universe"?? Is this a reference to some sort of matrix-valued field theory? Is it a reference to M theory? A reference to the movie, "The Matrix"?

32. **Yatima**
June 10, 2016

When Meedja say "Matrix-style" what they mean is "Matrix-style, like in that Warner Brothers movie from before the War on Stuff with raining green glyphs and Neo and stuff". Using "Matrix" in a technical sense is prohibited as per editor rules (this is an empirical observation though).

Moreover, "M-theory" is not a shorthand for "Matrix theory". My hunch is that the "M" is a quantifier symbol derived from the mirrored "W" as in "Witten".

33. **Parth**
June 12, 2016

Funny string articles..I also think Kaku is referring to living in a simulation by an intelligent being as the 'Matrix-style universe' seems to refer to the common phrase 'living in a Matrix' i.e. living in a simulation like in the movie The Matrix.

34. **Peter Woit**
June 12, 2016

Shantanu,

Sorry, but, just skimming some of the talks, I don't see any real news there. The overwhelming issue right now is the possible 750 GeV state. Total luminosity for this year's run is now at about the level of last year's run (above 3 inverse fb) so soon ATLAS and CMS should have the analysis done to see if a bump is still there in the diphotons. If it is, that's going to completely dominate the subject for the forseeable future (and I'll predict a high-profile event at CERN).

If the bump is gone, then maybe no event at CERN, but talks at ICHEP in Chicago. If there's any remaining ambiguity, it should soon be removed by the data coming in over the next few months.

If no bump, then things should return to the situation things have been in for a long time: overwhelming evidence against heavily oversold SUSY scenarios, with a sector of the physics community refusing to move on and acknowledge what has happened. One conference to wait for might be the one in late September labeled "Is SUSY alive and well?", check out their website with a graphic indicating that SUSY is not only dead, but is haunting the living…

https://workshops.ift.uam-csic.es/susyaaw

35. **Tim**
June 14, 2016

Would it be unwise to read the complete lack of leaked rumors this close before ICHEP as a sign that the bump is most likely dead? Either physicists have gotten several orders of magnitude better at keeping their mouths shut, or this year's run hasn't produced anything interesting …

36. **Peter Woit**
June 14, 2016

Tim,
I think it's just too early. It's only very recently that the experiments have accumulated an amount of data comparable to last year's, and analysis of that data takes a while. I don't know if these are blinded analyses. If they are, it will be a while before they are unblinded.

37. **anon**
June 15, 2016

Regarding result announcements: LIGO collaboration will have a press conference in a couple of hours at the 228th AAS meeting and it looks like they're going to announce new detections of gravitational waves.

38. **Provider of Truth and Factcheck**
June 18, 2016

The story is probably a hoax, at least according to this guy:

http://blog.drwile.com/?p=14864

Kaku never claimed any of these things about "primitive semi radius tachyons".

Some History of Science

June 13, 2016
Categories: Uncategorized

The period of the "String Wars" has now receded far enough into the past that it has become a topic of interest to historians of science. I learned today from Sabine Hossenfelder's round-up of various articles addressing the history and sociology of string theory that Sophie Ritson has published an article on the 2006 "trackback" controversy . It's a fairly straight-forward account of that story, based on publicly available sources, emphasizing the interesting issues raised about science blogging.

While the article deals with the 2006 history, what has happened since then sheds some light on the topic, for example making clear that the "active researcher" business was always a red herring. Within a couple years after 2006 I noticed that arXiv trackbacks were appearing to all sorts of sources obviously not "active researchers" (for example, Slashdot articles). I tried to find out what the new arXiv policy was, but got nowhere. At one point I decided to do some experimental work, setting up a fanboy string theory site, trashing string theory critics and enthusing over the multiverse. An arXiv moderator took a quick look, and decided the anonymous author qualified (see discussion here). I realize this was obnoxious behavior, but thought it at least had a chance of goading the arXiv moderators into revealing their current policy. No dice. Every so often I've tried again to contact someone associated with the arXiv to ask what their policy is, but this has never led anywhere. Sabine describes the current arXiv trackback policy as "one of the arXiv's best-kept secrets". If you look at recent arXiv trackbacks you'll see that the list is dominated by links from the excellent MathOverflow site, but also includes links from a wide variety of other sources that clearly are not "active researchers" (for instance: New York Times stories, press releases on Phys.org, MIT Technology Review weekly lists of arXiv papers, and Quanta magazine stories).

Besides the secret nature of the current policy, the odd way in which the "active researcher" policy came to light is rather remarkable. This all started back in August 2005 (see here) and at that point trackbacks pointing to this blog were appearing. A few months later that stopped and, wondering why, I wasted a lot of time trying to contact people associated with the arXiv to find out what was going on. I finally heard from a Cornell administrator that links to my blog were not being allowed for an undisclosed reason, and I wrote about that here. Sean Carroll picked up the story here, and a former member of the arXiv editorial board revealed the "active researcher" policy in a comment at that blog entry. This I gather forced Jacques Distler into a public discussion of the policy here, which I commented on here. The Ritson article covers this part of the story in some detail.

So, bringing 2006 history up to date, I have no idea what the current arXiv trackback policy is, other than that they've found some new criterion other than the "active researcher" one to justify blocking trackbacks from Not Even Wrong. I guess this will remain "one of the arXiv's best-kept secrets", at least until someone accidentally reveals all in a blog comment somewhere…

Comments

1. **MathPhys**
   June 13, 2016

   It's scary to realize that 'active researchers' can engage in clandestine behavior such as described. The varnish of decency is thin.

2. **Harry Dale Huffman**
   June 13, 2016

   Inevitably, when a "policy" remains strangely a secret, it turns out to be the arbitrary judgment (i.e., personal "likes" and "dislikes") of a single or a few individuals. It means you can't trust the system behind the enterprise.

3. **CIP**
   June 13, 2016

   Using a combination of intuition and certain ESP-like effects inherent in String Theory, I have deduced the arXiv trackback policy: " No trackbacks for Peter Woit."

   That is all

4. **Henry L.**
   June 13, 2016

   Peter,

   Do you have any suspicions about their reason for blocking your blog, which you would be willing to share?

5. **KJ**
   June 13, 2016

   It seems to me that someone determined enough to find out the arXiv trackback policy could do so by exploiting the various sources of public funding they receive and open-records laws. Cornell itself is a sort of public-private hybrid. Several public institutions are "members" and get a seat at the decision-making table. Donations to arXiv are tax-deductible, which makes it subject to certain sunshine laws. Surely at least one source of their money makes them subject to an open-records law that could be used to out the trackback policy if one was willing to put enough time into figuring out which one. I am certainly not such a person, but a science journalist or open-records activist interested in the subject might be.

6. **Peter Woit**
   June 13, 2016

   Henry L.,
   The two people who seem to have something to do with this are Jacques Distler and Joe Polchinski. I've never met either of them personally, but they've both made clear that they are deeply upset and offended by my views about string theory and the multiverse. As far as I can tell, that's the reason behind this. As for what excuse they are now using for the trackback business, now that the "active researcher" one seems no longer being used, I honestly have not the slightest idea. And I think that's intentional, since it means that I can't argue against it…

7. **Peter Woit**
   June 13, 2016

   KJ,
   Personally I've already wasted too much time on this. I'm in the odd position of having spent a lot of time back in 2005-6 in order to find out the "policy", it's hard to get motivated to go through that again, in order to find out the new bogus reason I am unworthy.

8. **Henry L.**
June 13, 2016

I was once proud of Cornell (my undergraduate school). That was half a century ago. Sigh.

9. **KJ**
June 14, 2016

Peter,
I didn't really think you would want to waste any more time. But maybe a science journalist interested in the string theory wars would want to pursue it. I am thinking someone who does that kind of thing for a living could do it much faster than you or I anyway, since we are not familiar with the relevant laws and procedures.

10. **Donald R. Pherson**
June 14, 2016

Hearing the annals of this "trackback" crisis is quite disturbing to me. It seems the "powers that be" at arxiv want no part in the criticism of string theory. String theory has become, at least by the mid-naughtes, more of a way to publish papers, bring in university funds, and maintain faculty than actual legitimate research. To put it differently, it's a racket. There are institutional interests at work that want to maintain the strings status quo. Thankfully the field is drying up however.

Nonetheless, it seems the string theory "wars" are still continuing:

https://www.reddit.com/r/Physics/comments/4o1rk9/im_sick_and_tired_of_the_antistring_theory_bs/

11. **Peter Woit**
June 14, 2016

All,
"Donald R. Pherson= Anonymous Postdoc" is a troll, please ignore. I and others were taken in for a while, leading to a comment thread that I'm afraid you'll never see unless you have been religiously refreshing comments here recently…

12. **Curious cat**
June 15, 2016

You are lucky, the wayback machine didn't catch it up for us 🙂

http://web.archive.org/web/*/https://www.math.columbia.edu/~woit/wordpress/?p=8578

13. **Lindsay Berge**
June 15, 2016

Back when I was involved is Physics research, I would occasionally get referees saying, in effect, "this area is not interesting because I would be working on it if it was". This was no doubt an honest appraisal but unhelpful. Generally another referee could be found who was more open minded or objective.
It would seem the attitude of some String Theorists is that "this area must be vitally important as I would not be working on if it was not". This elevates any fundamental criticism to a personal attack. I also suspect that any questions of legitimacy are deeply threatening after years and years of failure to actually deliver on the original heavily publicised promises (Theory of Everything). Any means possible would seem justified to silence those who would undermine the great and noble undertaking and imperil the reputations, careers, and support of the researchers. A truly productive program (like the original Standard Model) would not be subject to the same fearfulness.

14. **Peter Woit**
June 15, 2016

Lindsay Berge,
People do take criticism of ideas they are invested in personally, I don't think that's really avoidable. What is supposed to happen is that rational criticism of ideas should be met by a rational defense of them, leading to a clarification of what is right and what is wrong. This works very well in mathematics, where it generally is not hard to reach agreement on what is right and what is wrong.

The problem in physics is all about what to do in the "not even wrong" case. Defenders of the multiverse like Polchinski argue that I should not be doing what I'm doing (and thus not linked to by trackbacks), that instead I should be writing scientific papers proving that they are wrong. But the problem with "not even wrong" ideas is that one can never do this, all one can do is point out that their ideas come with no way to ever show that they are wrong. One could try and dress this up as a scientific paper, but it would be a very unusual one, with no equations, not that different from what I write on the blog, and I think people like Polchinski would be no happier with that than with my blog postings.

15. **David Levitt**
June 15, 2016

I am a biophysicist with some background in physics and an amateur's interest in high energy theoretical physics. I find blog sites such as this one extremely valuable in gaining insight into current controversies. What makes these blogs far superior to the general review article or typical newspaper or magazine survey is the presence of the blog comments. For example, if a post by Peter about some string theory argument was factually incorrect, I would expect this to be clearly pointed out by a string expert in the blog comments. (As far as I can tell, Peter does not discriminate against reasonable critics of his posts). The general lack of such responses is, for me, the strongest argument for the correctness of Peter's post. This is the unique strength of these forums. The fact that one or two egotistical physicists controlling the arXiv can deprive the arXiv readers of this valuable link is disgraceful and should illicit a strong response from the physics community.

16. **Brathmore**
June 16, 2016

Wait a minute! If this post is about string theory, and string theory isn't science, then how can this be part of the "history of science?" Shouldn't it be labeled as part of the "history of pseudo-science?" Doesn't the inclusion of non-science in a "history of science" diminish one and elevate the other, or tacitly acknowledge that the non-science actually is science? Me thinks we need some better terminology here.

17. **Peter Woit**
June 16, 2016

Brathmore,

Just to clarify: string theory is science, just happens to be science that didn't work out. The multiverse is pseudo-science. Hope that helps…

18. **Anonymous**
    June 17, 2016

    There is nothing wrong with the ArXiv deciding which sources they want to give the state of a trusted site.
    Neither you nor anybody else has a granted right to get trackbacks approved by the ArXiv administrators.
    I really dont understand why you get worked up about this, as if they were denying you a human right.

19. **Peter Woit**
    June 17, 2016

    Anonymous,
    Sure, it's up to the arXiv to decide this. I'm just trying to find out on what basis they made their decision.

Quick Items

June 15, 2016
Categories: Uncategorized

- In a couple hours, at 1:15 pm New York time, there will be a press conference at the AAS meeting where LIGO and Virgo scientists will discuss “ongoing research” (webcast here). The general assumption is that there will be observations of new gravitational wave sources announced.
- At some other extreme in the space of science talks, the AAS meeting also featured a talk yesterday by Sean Carroll on “Normal Science in a Multiverse”. There’s some discussion of the talk here, and twitter has this. Some counterpoint from Joseph Silk here.

  It seems that Carroll was arguing that the multiverse shows that we need to change our thinking about what science is, adopting his favored “abduction” and “Bayesian reasoning” framework, getting rid of falsifiability. Using this method he arrives at a probability of the multiverse as “about 50%” (funny, but that’s the same number I’d use, as for any binary option where you know nothing). So, from the Bayesians we now have the following for multiverse probability estimates:

  1. Carroll: “About 50%”
  2. Polchinski: “94%”
  3. Rees: “Kill my dog if it’s not true”
  4. Linde: “Kill me if it’s not true”
  5. Weinberg: “Kill Linde and Rees’s dog if it’s not true”

  Not quite sure how one explains this when arguing with people convinced that science is just opinion.

- Among the many summer conferences one might want to take a look at, there’s last week’s Workshop on String Theory and Gender, and this week’s LHCP in Lund. Wilczek will be giving the “Theory Vision” talk at the end on Saturday.
- Today’s Wall Street Journal has an interesting article on the not so great job market for Ph.Ds (to avoid paywall, try Googling, e.g. “Job-Seeking Ph.D. Holders Look to Life Outside School”). I find the claim that the median income for Math/Physics mid-career Ph.Ds dropped 6% over the past 3 years highly remarkable (if true). Yes, US middle class incomes have been tanking, but I that number is pretty extreme, especially since this has been a period of modest economic expansion.

  One other bit of news I learned from the article was what universities (including mine) are doing to help with the situation:

  > at Columbia University, Ph.D.s are taking classes in using Twitter to better communicate their work to nonacademic audiences.

Update: The LIGO news was a second black hole inspiral. I’m sure you can find good coverage of this elsewhere.

I hadn’t realized that the AAS is sponsoring a whole Multiverse Mania Fest, bringing in to promote a new definition of science not just Carroll, but Richard Dawid. Lenny Susskind this afternoon gave a talk (see here) that seems to argue that the Multiverse is a great idea, even though it won’t ever be testable. No news on what his Bayesian percentage is, or whether he’s willing to bet the lives of helpless pets. Sean Carroll made his usual straw man attack on the “Popperazi” (who, despite what he thinks, understand what indirect evidence is), see here.

Update: Another odd multiverse-related item. Laura Mersini-Houghton and collaborators have made well-publicized claims that they have testable predictions based on the string theory landscape. I’ve written about these several times here on the blog, see here, and this posting for one example that includes a response from Mersini-Houghton and Richard Holman.

Their claims are based on two 2006 papers, see here and here. Very recently Will Kinney posted this paper on the arXiv, which has in the abstract:

> we compute limits on these entanglement effects from the Planck CMB data combined with the BICEP/Keck polarization measurement, and find no evidence for observable modulations to the power spectrum from landscape entanglement, and no sourcing of observable CMB anomalies. The originally proposed model with an exponential potential is ruled out to high significance.

See the conclusions section of the paper for the details.

This isn’t particularly surprising or odd, although one wonders if the Kinney paper will get a fraction of the attention that the original claims have gotten. What is odd is that I hear that Mersini-Houghton is asking to have the paper removed from the arXiv, on grounds that have something to do with the fact that she was originally collaborating on the project with Kinney, but is not listed as an author (although he offered to put her name on it). I can’t think of another example of this kind of thing ever happening before, perhaps others are aware of similar controversies.

Update: Much more detail at Backreaction, including comments from Will Kinney explaining the issue with the arXiv.

Comments

1. **KJ**
   June 15, 2016

   I do think that multiverse proponents have abducted the scientific method. But I don’t think that is what Carroll has in mind.

2. **Another Anon**
   June 15, 2016

   Carroll: “About 50%”
   Polchinski: “94%”

   It’s just an absolute nonsense these probability guessing games, aren’t they? I have no idea why highly-regarded physicists are doing this.

3. **Neil**
   June 15, 2016

   When a weather forecaster tells me the probability of rain tomorrow is 50%, I translate it as “I don’t know.” With a greater than 50%, I hear “There is more reason to think it will rain than it won’t” and vice versa with less than 50%. Given Sean Carroll’s past championing of the multiverse hypothesis as an explanation for fine tuning, I’d have thought he’d assign a somewhat higher probability.

4. **Tim**
June 15, 2016

I'm surprised there's so much hostility to these percentage based confidence levels.

Because I'm 99% sure these guys are full of sh*t.

5. **paddy**
June 15, 2016

I do not like to see the very important LIGO results juxtaposed with nonsense (a wee bit more polite Tim, no?).

6. **Peter Woit**
June 15, 2016

I agree with paddy that more politeness would be a good idea. Not good to emulate the tone of a certain other blog.

On the other hand, I'm somewhat fascinated by the juxtaposition of real scientific progress (LIGO) and pseudo-science (the Multiverse) at the AAS meeting. Given the difficult situation of HEP theory, I can understand why string theory meetings sometimes feature Multiverse stuff, but if your field is healthy with serious new advances like this going on, why would you schedule all sorts of pseudo-scientific talks of this kind? The contrast must be quite jarring.

7. **David Nataf**
June 15, 2016

I like to believe that this interest in the multiverse is not permanent. It's likely that a lot of people got now-defunct theoretical training, so this is what they can do. They may have trouble acquiring and training new generations of elite graduate students, who will be more interested in pursuing more dynamic research programs, such as gravitational wave modelling and LHC phenomenology.

8. **Bee**
June 16, 2016

Do you have a link/reference for point 4. and 5. regarding multiverse (Weinberg/Linde)? I hadn't heard of these before.

9. **Jim Baggott**
June 16, 2016

Arguably, Bayesian decision theory (or a subjective, qualitative version of its logic) plays a legitimate role in the scientific method when there are empirical data on which to base prior and posterior judgements. As far as I can tell, the multiverse is really nothing more than an idea or a hunch – I wouldn't even accord it the status of a hypothesis (see http://www.jimbaggott.com/articles/status-anxiety-all-theories-are-not-the-same/). In this regard, Carroll's assertion is surely no more scientific than Pascal's wager.

10. **Bee**
June 16, 2016

Jim,

The problem with using Bayesian reasoning for "theoretical" evidence (as Sean suggests) isn't the priors. The problem is that the supposed evidence isn't about the theory itself, but rather about scientists' beliefs about the theory. Hence, if you use Bayesian inference to take into account non-empirical facts, you are changing the question from "How likely is X to be a correct description of Nature?" to "How likely do theorists think X is a correct description of Nature?" The latter is a quantifiable probability (and not even an entirely uninteresting one) but not actually the one you wanted to know.

This situation can get rather murky in theoretical physics because it's an empirical fact that mathematics works very well. So if you use logical reasoning based on mathematics, that could count as an empirical argument. (Philosophers would disagree on calling it that way, but let's not fight about words.) The problem is though that no proof is ever better than its assumptions. And if you look at what it is that enters these probability estimates, it's primarily scientists' opinion on how likely certain assumptions hold far beyond the regime where they've been tested.

11. **Another Anon**
June 16, 2016

The crux of what you are saying, Bee, is that "How likely is X to be a correct description of Nature? is not a scientific question. "Likely" has no place in science.

12. **Peter Woit**
June 16, 2016

Bee,
See the last paragraph of
https://arxiv.org/abs/hep-th/0511037

13. **Another Anon**
June 16, 2016

I really mean "opinions" have no scientific value.

14. **Bee**
June 16, 2016

Peter,

Thanks!

Another Anon,

I don't know if you did this on purpose or whether it's a copy-and paste mistake, but this isn't what I wrote. "How likely is X to be true?" can be a scientific statement (depending on what X is about), and in fact I would argue that all scientific statements are essentially statements of likelihood (you can never prove any statement about reality to be true). What I said was that statements about likelihood of beliefs of some people aren't normally what scientists are interested in.

(You might actually want to read Sean Carroll's recent book, which I reviewed here. Whether or not you like his opinion about the multiverse, he does a pretty good job explaining Bayesian reasoning and its relevance for science.)

15. **RandomPaddy**
June 16, 2016

> I find the claim that the median income for Math/Physics mid-career Ph.Ds dropped 6% over the past 3 years highly remarkable (if true).

Totally unsurprising. The Great Recession has taken its toll and a lot of these people have been hopping from temp job to temp job over the last 8 years. People wonder why the economy won't pick up, and yet we're talking about prime spending age people who don't have the careers to support anything of the kind. Welcome to Japanification.

16. **Jim Baggott**
June 16, 2016

Bee,
I've actually never been a fan of Bayesianism, because of the difficulty of establishing priors even when there is an abundance of empirical evidence and because I believe it is virtually impossible to be objective. My point is really that all the proponents of Bayesianism that I've read argue for its utility in theory assessment in the light of evidence. I think (I confess I haven't researched it properly) that Dawid and his colleagues are the only philosophers of science advocating Bayesianism in non-empirical theory assessment (see 'String Theory and the Scientific Method', pp. 68-72). Of course, this is a gift to theorists who want to dress opinion in the language of probability. Hence my remark about Pascal's wager.

Whilst I'm mostly in agreement with your view, I do think it's important to avoid what I think is a critical category error. In 'Farewell to Reality' I tried to distinguish between 'correspondence truth' and 'coherence truth'. In science we take something to be true if it corresponds to the empirical facts – for this reason I'd regard general relativity to be 'true', on the understanding that it may one day be superseded by an even more general theory which establishes correspondence with even more facts.

But I can also establish the 'truth' of a network of mathematical relationships which may (or may not) have any relation with empirical facts. Now, establishing the coherence of a set of mathematical structures or a set of logical assertions might give me some confidence that these structures or assertions have something useful to say, but this DOES NOT COUNT as empirical evidence (sorry, this IS about words and I think it's worth fighting for). I would go so far as to suggest establishing coherence truth is no real guide at all to correspondence truth – for this we need predictions (I'll even settle for predictions of existing facts) and so far we have none. The history of science is littered with coherent structures that have nothing whatsoever to say about physical reality.

Another point that these (few) theorists seem to overlook is that new theories are adopted by the community only when they are shown to work better than the established theories they're meant to replace. This isn't even about making testable predictions – it's about making testable predictions which demonstrate that the new theory (string theory or the multiverse) goes further or has greater explanatory power than the standard model of particle physics or big bang cosmology. My opinion (I can put a probability on it, if you want) is that any prediction, for example of some subtle effect in the cosmic background radiation, is likely to be accommodated by tweaking one or more of the many approximations theorists have to make in order to apply the established theories, and Occam's razor will tend to favour the conservatives. In this view the proponents of new theories will have a real uphill struggle to make a convincing case, even if they're ever able to make testable predictions. I'm afraid there's just too much we don't know and so too much flexibility, irrespective of how well the mathematics work.

17. **anonymous**
June 16, 2016

"..and at Columbia University, Ph.D.s are taking classes in using Twitter to better communicate their work to nonacademic audiences.."

I think that courses teaching specific tools used in the prospective industries would be far more useful. What employers seek is practical knowledge and ability to solve problems using the tools they know. They usually don't look for a person who would make for them goundbreaking discoveries.

18. **Peter Woit**
June 16, 2016

Jim/Bee,
I appreciate the serious discussion of the issues of assigning probabilities, but maybe more relevant in this case is that the particular question here seems to me to not pass the laugh test (average expert on the issue thinks the idea is a joke). I take Weinberg's comment about Rees/Linde not as a serious attempt to characterize the "probability there is a multiverse", but as a joke making fun of the whole concept of doing this.

The serious issue is more that of, once you have Carroll, Dawid, Susskind giving prominent talks at this kind of conference, who is the joke on?

19. **adamt**
June 16, 2016

Sean Carroll believes the Multiverse idea is a middling obscure phenomena that indirect evidence could reveal.

Peter Woit and others (myself included) believe the Multiverse is a completely obscure hypothesized phenomena that no evidence (indirect or otherwise) can illuminate at all.

Peter, have you gone through Sean's indirect evidence point-by-point to see why he believes this to illuminate in some way the Multiverse idea?

20. **Peter Woit**
June 16, 2016

adamt,

I don't know what specific point-by-point list of indirect evidence there is, but the arguments for the multiverse have been addressed I think exhaustively and ad nauseam here for more than a decade. Some of this is in the site FAQ, lots more if you poke around in the 125 postings here
http://www.math.columbia.edu/~woit/wordpress/?cat=10
I suppose I should put together a single location of serious arguments about this, but it's hard to get motivated to do this, since my experience has been that the policy of Multiverse proponents is to steadfastly ignore the serious problems with what they are claiming, and instead attack straw men.

21. **Anon**
June 16, 2016

Modest economic expansion can happen while many industries (and perhaps the majority of the middle class) are tanking. The mean can become very different from the median. As conversations most of my long-suffering colleagues, would-be colleagues, and especially students (current and ex-) will attest, the reports of the end of the Great Recession have been greatly exaggerated.

22. **Peter Woit**
June 16, 2016

Anon/RandomPaddy,

I'm not especially surprised by a negative median income trend, it's just that the 6% change seems high over such a short period of time. I did a quick check and couldn't find the real source of those numbers, may take a better look when I find a moment.

23. **Flavio Botelho**
June 16, 2016

I also think Multiverse is a good theory… Just a philosophical one, not a scientific one. Trying to push it as if it were science is a disservice for humanity.

24. **Chris W.**
June 16, 2016

RE: decline in median income for Math/Physics mid-career Ph.Ds

That would be consistent with what's happening to many other white-color occupations. This economy is being driven by the values and priorities of professional investors and Wall Street. They don't want to sacrifice a portion of their returns to compensate well-educated, highly skilled workers any more than anyone else, if they can find ways to avoid it. That attitude is spilling over into areas beyond business, including the administration of colleges and universities.

25. **Curious Wavefunction**
June 16, 2016

Wouldn't the puzzlement at this kind of discussion in conferences go away if we assume that such conferences have two parts: one in a serious scientific vein and one in a lighter, slightly jocular vein? I am not sure Sean Carroll for instance treats the multiverse with the same kind of seriousness he treats LIGO. I think it's ok to put numbers on probabilities like this as long as you make it clear that you are engaging in philosophy and even humor and not science.

26. **Peter Woit**
June 16, 2016

Chris W.,

I'd describe the trends I see differently, with some highly-skilled people doing well, but also fewer mid-range jobs, so more people ending up in poorly paid jobs. At universities, faculty at places like Columbia are doing well, and if you go into university administration your pay has soared dramatically. On the other hand, nationwide there are more teaching jobs being filled by poorly-paid adjuncts. From what I hear of the Silicon Valley world, Google is offering head-spinning sums to some people, and that's probably true of other large successful technology companies. But a few such companies have gobbled up a large part of the economy, with not so much left over for those not working in these few places.

All in all, the pattern in industry after industry seems to be one of "winner take all", with those doing well doing very well (and this is not just Wall street people), but fewer middle-class jobs. More and more cases are visible of middle-aged people who used to have a reasonably well-paid job which they lost as their industry was "disrupted", and now are unemployed or working for much less than they used to.

This trend though isn't new, it has been going on for years, and I'm still suspicious of the size of the claimed effect over 3 years.

27. **Peter Woit**
June 16, 2016

Curious Wavefunction,

I'm sure Carroll knows the difference between serious experimental results like LIGO and speculative stuff like the multiverse, but I've never seen evidence that he isn't quite serious about the multiverse, or has any particular sense of humor about it. When I first heard the Polchinski argument about "94%", I was sure it was a joke, but it seems that he and others take this seriously. What I'm afraid is that conference organizers conceptualize this kind of speculation as, yes, different than solid science, but think of it as "inspirational", as opposed to possibly silly.

28. **Anonyrat**
June 16, 2016

I think the NSF survey on PhDs that you're looking for is here:
https://ncsesdata.nsf.gov/doctoratework/2013/

Median salaries are here (table 53):
https://ncsesdata.nsf.gov/doctoratework/2013/html/SDR2013_DST53.html

To take a pair of data points (2013)

Mathematics/ statistics (11-15 years since PhD) median income: 109,000 standard error 6,000

Physics (11-15 years since PhD) median income 110,000 standard error 4,000

To look for the decline one would have to go to a previous version of this survey.

Here's the 2010 survey:
https://ncsesdata.nsf.gov/doctoratework/2010/html/SDR2010_DST53.html

The 2010 pair of data points reads:

Mathematics/ statistics (11-15 years since PhD) median income: 100,000 standard error 4,500

Physics (11-15 years since PhD) median income 114,000 standard error 7,000

I'm sure other commenters will better be able to slice and dice this data. But on the face of it, it is physics PhDs, not mathematics/statistics PhDs that have had a decline in median income, for this range of experience.

29. **Anonyrat**
June 16, 2016

This article presents some caveats in interpreting the NSF survey of PhDs for newly minted PhDs:
http://www.sciencemag.org/careers/2016/05/employment-crisis-new-phds-illusion

30. **Peter Woit**
June 16, 2016

Anonyrat,
Thanks! Those numbers by age cohort have large "standard errors", not clear whether they're useful. The numbers for all ages are

Math
2003 80,000
2006 81,800
2008 95,000
2010 97,000
2013 98,000

Physics
2003 94,000
2006 99,900
2008 108,ooo
2010 110,000
2013 111,000

These aren't inflation adjusted. Using a CPI inflation calculator I get, in 2013 dollars

Math
2003 101,290
2006 97,570
2008 102,790
2010 103,630
2013 98,000

Physics
2003 119,000
2006 115,440
2008 116,860
2010 117,520
2013 111,000

These do both both show a huge drop 2010-2013, when US median household income was going back up a bit after the recession, see here
https://en.wikipedia.org/wiki/File:US_GDP_per_capita_vs_median_household_income.png
An odd thing about these numbers though is that they show incomes going up significantly from 2006-8 to 2010, but this was the worst of the recession, with median US household income going down quite a bit. So, I'm still suspicious about these numbers, the 2010 ones are unexpectedly high, the 2013 ones unexpectedly low.

31. **paddy**
June 16, 2016

Not to rain on anyone's parade, but those salaries (like mine) are quite a bit over median US incomes. And I enjoy what I do. I would rather complain about the ratio of the salaries of my plumber to that of my banker.

32. **MikeS**
June 17, 2016

Is it possible that the multiverse is indeed string theory playing its end-game? The failure to make useful predictions about our own Universe has been turned into a strong indicator for a Multiverse. Perhaps even going so far, through this odd use of Bayesian statistics, as some sort of proof of a Multiverse.

But where to from here? String theory is no more likely to be able to say anything useful about these alternative realities than it is about our own, and as these alternates will almost certainly remain beyond any hope of our perceiving them then any conjectures will forever remain as conjectures.

Okay, a few more years tinkering with the probabilities of a Multiverse, but that's about it. Pats on the back all around. Multiverse proved. But now what?

The only game in town is playing its final hand. And guess what, it's all aces! Indeed 10^500 of them!

33. **tulpoeid**
June 17, 2016

Peter,
Only indirectly related to today's post, but would you have any article (either yours or others') to recommend on the similarities between strings/multiverse and religion? And I don't mean it as a parody.

34. **Anonyrat**
June 17, 2016

An outsized influx of young PhDs, or retirement of elderly PhDs would have the effect of driving the median salary of (all ages) down.

But yes, it looks like we have 1-sigma results only for 2010-2013.

35. **Peter Woit**
June 17, 2016

tulpoeid,
I can't recall ever seeing any such article and I haven't tried to write such a thing. Religion is a very complex social phenomenon, and so is string theory/multiverse research. It might be possible to say some interesting things about the similarities and differences, but I haven't seen that done.

Sean Carroll is an interesting figure here, he's rather explicit about wanting to replace a religious world-view with one based on theoretical physics, with the multiverse playing a role perhaps closer to theological speculation than to conventional science.

36. **Neil**
June 17, 2016

Re God, Carroll and the multiverse. Carroll addresses some of these questions in The Big Picture, in particular Chapter 36 (Are We the Point?), where he posits his 50-50 ("perhaps") credence figure, and to a lesser extent in Chapter 18 (Abducting God). In Chapter 36 he states that the multiverse is not a theory but a prediction of other scientific theories (string theory and inflation, which he admits are "speculative"), whereas God is not a prediction of any scientific theory.

37. **curious**
June 17, 2016

has there been any discussion that instead of a multiverse, God did it, in terms of Bayesian statistics?

38. **Neil**
June 17, 2016

Curious

In Chapter 18, Carroll discusses whether there is any evidence to update our priors about divine creation (such as, why is there evil then?) and concludes that the concept of God is too imprecise and variable to apply Bayesian updating, leaving us just with our priors. I thought that was obvious all along. Peter is right in thinking that this sort of debate is a monumental waste of time outside of its entertainment value.

39. **Narad**
June 18, 2016

> When a weather forecaster tells me the probability of rain tomorrow is 50%, I translate it as "I don't know."

Not to try to drag things off topic, but my understanding is that such statements denote that rain is likely for 50% of the covered forecast area.

40. **Another Anon**
June 18, 2016

Narad, that works out to the same thing.

https://en.wikipedia.org/wiki/Probability_of_precipitation

41. **Matt Grayson**
June 18, 2016

I understand it's a human compulsion, but why do we persist in thinking that our theories, which are essentially calculation methods to produce numbers that (one hopes) agree with experiments, have ANYTHING to do with reality? Go through history: every time we have thought, "well this theory(n-1) worked well enough at the time, but it was later seen to have nothing to do with the reality we *now* know due to theory(n)." Yet here we are with GR and the Standard Model, and whatever framework is supposed to succeed them, seriously talking about the probability that their implications exist. Well, a Frequentist actually has data, and that probability is Zero.

(I'm resisting the urge to claim proof by induction, but Raoul Bott defined induction as "You work out n=1, you work out n=2, you keep going until you're bored. Then you have a proof." I think the historical record satisfies his criterion..)

Perhaps I misunderstand their claim. Multiverse proponents are NOT, as far as I can tell, claiming probabilities for results of experiments. Or even probabilities that theory(N), for some large N, will make concrete predictions and involve multiverses, are they? But even then, theory(N+1) will show how silly they were to believe such a thing.

42. **Dave**
June 18, 2016

"Argument from ignorance." Have none of these physicists heard of this fallacy? If you don't know something, then all you can conclude from this is that you don't know. So why the assenine guessing games about probabilities? Why the philosophical debates about accepting the reality of something without falsifiability? I don't know, you don't know, point.

43. **Bee**
June 18, 2016

Hi Peter,

Coindidentally I was about to write a blogpost about the Kinney paper, which is now online here.

44. **Bee**
June 18, 2016

@Jim Baggot

I think I agree with what you say. I should check out your book though to get my vocabulary right 🙂

45. **Anonyrat**
June 18, 2016

The American Mathematical Society has some statistics.

The mathematics PhDs awarded by year 2003-2013 in the US are here:
http://www.ams.org/profession/data/annual-survey/2013Survey-NewDoctorates-Supp-TableD3-4.pdf
From there:
2008-09 : 1605
2009-10 : 1632
2010-11 : 1654
2011-12 : 1798
2012-13 : 1843

Median starting salaries for new PhDs 2000-13 in Teaching & Research, Academic Research Only, Government, and Business & Industry categories are available here:
http://www.ams.org/profession/data/annual-survey/2013Survey-StartSal.pdf

The median numbers look like this

Academic teaching & research (11-12 month)
2008: 56.0K
2009: 60.0
2010: 57.0
2011: 55.0
2012: 60.0
2013: 60.0

Research (11-12 months)
2008: 50.0K
2009: 50.0
2010: 51.5
2011: 55.0
2012: 55.0
2013: 54.0

Government:
2008: 81.5
2009: 82.5
2010: 80.0
2011: 70.0
2012: 82.0
2013: 87.0

Business:
2008: 90.0
2009: 90.0
2010: 90.0
2011: 94.3
2012: 95.0
2013: 100.0

I suppose 2011 was the year of government shutdown?

46. **Jeffrey M**
June 18, 2016

Anonyrat,

Wow, I haven't looked at those numbers in a while. I can't believe how many PhD's there are now! I think the year I got mine ('92) there were around 800. And the job market sucked then.

47. **Narad**
June 18, 2016

@Another Anon:

> Narad, that works out to the same thing.

Thanks for the reference. Perhaps I misinterpreted Neil's original comment, but it's neither here nor there in this thread, so I'll just pipe down.

48. **Bayes_or_bust**
June 19, 2016

Regarding the probabilities of the multiverse, you are right that the 'average expert on the issue thinks the idea is a joke'.
.
I've never seen even a sketch of a calculation for e.g. a Bayes-factor for the multiverse vs. universe (or even enough detail to begin a calculation), and suspect that such a calculation would prove deeply problematic.
.
Caroll et al simply seek refuge from accusations that their work isn't scientific in Bayesianism, but their probabilities are just guesses.
.
However, whilst I'm glad you're ridiculing their numbers, I hope it's clear to your audience that there are many important legitimate applications of Bayesian statistics in science.

49. **a l**
June 20, 2016

Jerome Ravetz in the Guardian, June 8
How should we treat science's growing pains?
https://www.theguardian.com/science/political-science/2016/jun/08/how-should-we-treat-sciences-growing-pains

String Sociology

June 20, 2016
Categories: Strings 2XXX

If you're interested in the various sorts of internal divisions these days among people doing what gets called "string theory", you might want to take a look at [this blog entry]() and the discussion there with string phenomenologist Joseph Conlon.

Back in 2002 or so when I started writing my popular book, it was a lot clearer what the term "string theory" meant and who counted as a "string theorist". If I were writing about this today, there would be a much more confusing situation to try and explain. There's still a conventional "string theory" story about a supposed theory of everything based on quantized strings often told to the public, but it no longer corresponds much to what researchers who call themselves "string theorists" are actually doing.

To get some better picture of this, it might be a good idea to take a look at the big string theory summer conferences. The biggest is Strings XXXX, [this year in Beijing](), about six weeks away. No talk titles available yet, but in recent years one clear pattern has been that most of the talks have little if anything to do with the "string theory" of the textbooks (4gravitons tries to categorize things [here]()) . These conferences have been going on for over 20 years.

Since 2002, there has been a breakaway conference, String Phenomenology 20XX, which I think Conlon characterizes accurately as follows:

> one reason the String Pheno conference was founded was because people working on pheno topics weren't getting a look-in at the Strings conference and so set up their own conference. I think the Strings conference is most accurately regarded as the Princeton view of the world (broadly, every year it reflects subjects popular at the IAS and a couple of other similar places).
>
> Generally, the ratios vary with place. A small fraction do string pheno in the US, a significant number in Europe, almost none in India, quite a few in Korea…

This year's version of this conference has just gotten underway in Greece, you can follow the talks [here](). One big topic this year is the possible 750 GeV diphoton excess. Around the time of Strings 2016 we should hear whether this is real or not. If it is, String Phenomenology 2017 will likely be completely dominated by the topic, if not, it will have vanished without a trace.

Finally, at the other end of the spectrum is String-Math 20XX, which has been going on since 2011, and this year starts [next week in Paris](). Quite a few first-rate mathematicians are involved this year. As with Strings 20XX, most of the talks don't actually have anything at all to do with the theory of a quantized string, and this is more of a "QFT-Math" than "String-Math" conference at this point.

One can read the blog comments mentioned to get some idea of the arguments going on about "phenomenology" vs. "mathematics". The "phenomenologists" argue that they are the ones doing physics and engaging with data, but don't really point out that the models they work with have no known (i.e. not purely speculative) connection to any known physical phenomenon. They're hopeful someday things will be different, but there's no evidence at all of any progress in that direction.

Phenomenologists like Conlon do battle with their Strings 20XX brethren by accusing them of doing "mathematics", not "physics", of in essence really being just an offshoot of the String-Math 20XX crowd. There's an implicit argument that such people don't deserve jobs in a physics department, but should move to a math department. I can report though, that while much of the String-Math 20XX research is more than welcome in the math community, that's not true of most of what goes on at Strings 20XX (a conference that very few mathematicians ever attend).

If you find the current situation confusing, rest assured that you're not the only one…

Comments

1. **Harrison**
   June 20, 2016

   I seem to discern, more or less, an aspirant class of tragicomedy now up-and-running that would completely elude the Greeks. Aristotle more than anyone.

2. **Thomas Müller**
   June 21, 2016

   Peter, are these writers implying that the definition of string theory is "String theory is whatever is done (since 20 years) at the IAS in Princeton"?

3. **Peter Woit**
   June 21, 2016

   Thomas Muller,
   I think what's being made is an even stronger claim, that for now and the future, whatever people at the IAS do, they'll call it string theory. Recall
   https://www.theguardian.com/science/2005/jan/20/science.research

   "Most string theorists are very arrogant," says Seiberg with a smile. "If there is something [beyond string theory], we will call it string theory."

4. **Tim's Mom**
   June 21, 2016

   I've been thinking about this. I think somebody should do a systematic study of exactly how the sociology of a field of science is *different* from the sociology of a non-science field.

   (Also, my hunch is that the field of Mathematics would land on the 'science' side.)

5. **Michael Barany**
   June 22, 2016

   Tim's Mom, a good book to start with is Gordin's recent Pseudoscience Wars http://press.uchicago.edu/ucp/books/book/chicago/P/bo10672063.html
   There is a lively debate among some sociologists of science and more philosophers of science, but the consensus in the former field at least seems to be that you can mainly tell a scientist by looking at who acknowledges her as a scientist and that there are not really robust criteria beyond that.

6. **Tim's Mom**

June 22, 2016

This is what I'm talking about. Compare the sociology of mathematicians, physicists, string theorists and geologists to the sociology of astrologers, art historians, politicians and volleyball players. Even in art history there is a consensus about some things so it's not a priori obvious what's going on. Somebody needs to work on this.

7. **Jonny 5Brane**
June 23, 2016

An early version of the Tim's Mom's abject nihilism surfaced after Kuhn's work first came out. I am happy to observe Woit *is not* taking that stance – Conlon has 45 publications and yet his ressentiment is palpable. I feel like I can see the face he makes when he says "Princeton". Strings-Math 2016 does indeed look awesome! Looks like I'll be hosting all night watch parties next week. Enjoy!

Rumor Mongering

June 21, 2016
Categories: Experimental HEP News

Since I don't see why Resonaances should have all the fun, I guess I'll post something here about the big upcoming news of the summer: is the 750 GeV diphoton bump still there in the 2016 LHC data? We're very soon about to hit a major fork in the road for high energy physics: if the bump is there, the field will be revolutionized and dominated by this for years, ft it's not, we're back to the usual frustrating grind.

Last year's tentative signal was based upon 3.2 inverse fb of 2015 data, and as of today the experiments have over 6 inverse fb of new 2016 data. One can guess that within ATLAS and CMS, plots have started to circulate of preliminary analyses of some sizable fraction of the new data, and some number of people now know which fork we're headed down, a number that will grow to 6000 or so in coming weeks.

If you're not one of those, you could try accessing this data indirectly, using a model of how the CERN administration works. According to this presentation at LHCP this past weekend

> the next major update of physics results from the LHC is at the ICHEP 2016 Conference, August 2016, Chicago

and

> CERN management is in regular contacts with the experiments' Spokespersons. It is agreed that any significant (i.e. discovery-like) result (such as the 750 GeV bump becoming a signal or other) has first to be announced in a seminar at CERN.

No detailed schedule yet for ICHEP, but the first day of plenary talks there is scheduled for Monday August 8. My CERN-modeling suggests that the two forks in the road will correspond to the following two possibilities

- A "special seminar" in Geneva mid to late July, where a "discovery-like result" will be announced jointly by the CERN DG and the two experiments.
- A pair of seminars in Chicago on August 8 or shortly thereafter showing off bump-less plots. Much of the drama will be gone by then, since not all 6000 physicists will have kept quiet, and everyone will realize that if "discovery-like" was going to happen, it would have happened earlier.

It may become clear which fork we're taking relatively soon, since the "special seminar" route takes some planning and will get announced in advance. If there's no such news by mid-July I think it will be clear we're headed down the boring fork in the road.

Unfortunately, life being what it is, that's the most likely one anyway. To supplement this CERN-administration-modeling, you can watch the comment section at Resonaances, where rumors of no bump have already started to appear…

Update: Now, it's an official rumor (since it's on Twitter). I can add to this that, of the rumors I have heard, there have been no rumors that this official rumor is not an accurate rumor.

Comments

1. **Another Anon**
   June 21, 2016

   Yeah, I was going to say the rumour is that the bump has gone.

2. **anonymous**
   June 21, 2016

   Jester has hinted in the comments on his blog that the bump is indeed going away:

   "Many papers, including some written by the winners, made interesting contributions that will live longer than this particular bump. So this post is self-irony through the tears, rather than slut shaming as interpreted by some."

   On the bright side, the LHC is coasting along magnificently, churning out 2/fb/week despite the dump leak in the SPS. ICHEP could be presenting 15/fb of data in total, 30/fb in total by the end of 2016 assuming no more curious fouines.

3. **Henry L.**
   June 21, 2016

   Peter,

   I am interested in how your "CERN-modeling" works. Would you be willing to share some details, or are your algorithms proprietary?

4. **Peter Woit**
   June 21, 2016

   The model is quite simple: it just says that if there's a "discovery", they will reveal this at a special seminar at CERN, announced a week or so in advance. If there's a let-down non-discovery, they will announce at a previously scheduled conference outside CERN. This modeling is based on past observations and basic understanding of how a good PR operation works. I do think it's a pretty powerful model: once you know the announcement plan, you know the result.

   Of course my confidence level in the model may depend on proprietary algorithms, although these are more relevant to evaluating rumors…

5. **anon**
   June 21, 2016

   "One can guess that within ATLAS and CMS, plots have started to circulate of preliminary analyses" – do we even know reliably whether the data has been unblinded?

6. **Neil**
   June 22, 2016

Dismaying for BSM hopes, but still just a rumor. Perhaps disseminated to put the bloodhounds off the scent trail?

7. **M**
   June 22, 2016

   anon, ATLAS had a meeting on June 16 at 13:00 and CMS on June 20 at 17:00: it’s impossible to keep information private among 3000+3000 persons, especially when not everybody agrees on a selective policy for announcing data. They risk that somebody could go beyond a tweet and post the digamma spectra on a blog. Anyhow, another rumor is that a few years ago, in one analysis, higgs to gamma gamma disappeared for a while, until it reappeared with more data. Diphoton data are growing fast, already now they have twice the data analysed so far.

8. **anon**
   June 22, 2016

   @M, should I take that as confirmation that the data have been unblinded from someone inside an experimental collaboration?

9. **Dan D.**
   June 22, 2016

   In regards to M’s comment above, I, too, seem to recall that at one point, the Higgs digamma signal in CMS fluctuated downward quite a bit in one batch of data. I can’t remember if this was before or after the 2012 discovery announcement.

10. **Alex C**
    June 22, 2016

    My little bird in CERN told me the bump has gone in 2016 data

11. **Confused**
    June 22, 2016

    I am wondering if it is commonly understood amongst particle physicists that this is the expected behavior of p-values. If there is no signal, the p-value should not be expected to converge onto any specific value as more data is collected. It will random walk between 0 and 1; ie if the null hypothesis is true the p-value is a sample from a uniform distribution. See for example the simple simulation output in figure 2 here (you can easily run your own simulations as well):
    http://pss.sagepub.com/content/22/11/1359.full

12. **Peter Woit**
    June 22, 2016

    Confused,
    Particle physicists do have a quite sophisticated understanding of these issues, this is a central part of what they do, you’re mistaken if you think they’re not quite aware of this. The problem is that they do this a lot: a huge number of very different analyses get performed, so it is tricky to characterize probabilities of seeing a certain size fluctuation at a certain location. Then there are two independent experiments, if they see something at roughly the same location, how do you take into account the significance of that?

    The bottom line here is that, however you quantified it, last year’s data contained a potentially significant signal, although one not large enough to be convincing. With this year’s data, the situation is just about binary: with pretty high probability,if there’s something there, the signal should get larger and become convincing, if not its significance should recede. The rumor is that the latter has happened, with details still to come.

13. **Bayes_or_bust**
    June 22, 2016

    I’d like to have seen a Bayesian analysis of the combined data. My feeling is that the complicated statistical analyses (local vs global and LEE, and the lack of a reliable combination of ATLAS and CMS data) contributed to the hype and confusion about a new particle. I think this mess would have been partially avoided with e.g. Bayes-factors for a diphoton toy model vs the SM.

14. **Peter Woit**
    June 22, 2016

    Bayes_or_bust,

    I don’t think a Bayesian or other analysis would have changed anything, other than giving people yet more different inconclusive ways of characterizing the significance of the bump.

    What has surprised me about the LHC results has been how rare things like this are. If you look back at the history of HEP experiments you can find a long list of interesting looking, but not quite convincing effects that have appeared and then disappeared. This is just part of how this kind of science works. Given the huge number of different analyses going on at the LHC, I’d have expected more such stories. The experiments have done an unusually excellent job of trying to be rigorous about their statistical methods.

    The reaction of the theory community is a different story, see the discussion at Resonaances. There the way the field is intensely starved for anything new led to this getting an unusual (and, it seems, unjustified) amount of attention.

15. **db**
    June 22, 2016

    @Peter Woit

    Of course the real question is whether or not you would be prepared to kill someones dog if your CERN PR model was falsified. 🙂

16. **G. S.**
    June 22, 2016

    Bayes_or_bust,

    Before calculating the Bayes-factor for a diphoton toy model, you have to decide which toy model(s) to consider. How would you do this?

I would think that you would want to use one that existed and was deemed "reasonable" prior to observation of the diphoton excess. That would be to avoid models that have since been tuned or selected to fit the observation. However, I'm not sure any such models existed.

17. **Sal Rappoccio**
June 22, 2016

The statistical methods we've developed are robust to all of this… that oft-decried 5-sigma standard of evidence is a pragmatic way to get around the trickiness of the 3-sigma jumps and bumps that should happen occasionally when performing hundreds and thousands of analyses. We just need to stick to the decision! Instead, we collectively lost our minds over a tantalizing hint, and will subsequently pay the price if it is falsified.

It's like playing Texas Hold'Em. You've got a pocket pair of aces. Yes, it's a good hand pre-flop. But, it's not enough to bet your house on, there are 5 cards to play. At least wait until the turn! In science, we should really wait all the way until the river in order to call the game ;).

18. **S**
June 22, 2016

Actually, Sal, you kind of want to push people off those pocket twos before the flop, just in case, which does justify a pretty aggressive bet pre-flop.

Maybe this explains string theory PR?

19. **Bayes_or_bust**
June 22, 2016

@GS I think a toy model could have been made. @Peter it's not a lack of rigour that I dislike. It's the complicated frequentist techniques and lack of an official combination of ATLAS and CMS data.

Even now @jester's blog you can see confusion about this. No one knows the combined global significance for that search. And they worry they've been fooled by the sheer number of analyses.

Suppose we knew the Bayes-factor and it said: diphoton toy model 5 times more plausible after 2015 data. Don't you think that would alleviate confusion? No global/local/LEE to contend with.

That said, the decisions people make to to e.g. make a paper or get excited, are based on many factors beyond the plausibility of the new physics model.

20. **Peter Woit**
June 22, 2016

Bayes_or_bust,
No, "diphoton toy model 5 times more plausible after 2015 data" would add to, not reduce confusion, by introducing a host of new complications (which toy model? how do you get the "5 times"?).

The experiments I think do a good job of quantifying what can be quantified. Beyond that, it's a matter of judgment and experience, the importance of which can't be eliminated by more sophisticated statistical analysis. My impression is that a typical expert reaction to the diphoton plots and to the kind of numbers quoted for statistical significance is something like "yes, looks like there could very well be something there, but I can think of several cases in the past when bumps like that went away with more data". You can assign all sorts of numbers to this situation, but the bottom line is that the 2015 data was not convincing evidence, especially not convincing evidence for an extraordinary claim (completely new, never before seen physics not part of the standard model). Extraordinary claims require extraordinary evidence, and that wasn't there.

What's interesting about this situation is that the new data should with high probability resolve the issue. If there really is a state with that kind of cross-section, we should see convincing evidence soon. If there isn't, the new data should sizably reduce the significance of the bump. Landing in the middle zone isn't especially likely.

21. **Bayes_or_bust**
June 23, 2016

The Bayes-factor (I said 5 times for an example) would be calculated in the usual way. I agree there would be questions about which toy-model was tested against the SM, but even in the frequentist log-likelihood ratio methods currently used for the diphoton analyses, one has to make an alternative model to test. So I don't see it as problematic. But we'll have to agree to disagree, I think.

22. **anonymous**
June 23, 2016

Peter and others,

Is there any known plausible theoretical reason for the existence of a heavy resonant state at 750 GeV ? That would certainly make it a-priori more likely to exist.

After all, the Higgs discovery was extremely well-motivated theoretically around *half a century* before it was discovered.

23. **M**
June 23, 2016

Dear anonymous, the Theory of Anything univocally predicts 10^500 different vacua. About 10^350 of them are indistinguishable from the Standard Model, and about 10^345 of them have a diphoton resonance at 750 GeV.

24. **Another Anon**
June 23, 2016

One thing that bugs me a bit is why all the particle physicists got so thrilled by the news of the bump. How can it be good to discover something we don't understand? Effectively, that increases our ignorance about the world. Surely the aim is to decrease our ignorance, by making sense of the data we already know. A new particle just pushes us further from that goal. I appreciate a new particle would have given them something to work on, but that's not the point – there's already plenty of stuff to work on: go solve the hierarchy problem, or dark matter if you're short of something to do,

25. **MikeS**
June 23, 2016

Another Anon

Unexpected findings do not increase our ignorance they reveal our ignorance. Which is the first step in reducing our ignorance.

26. **Another Anon**
June 23, 2016

Good point. But I don't think I'd be so happy to discover I was ignorant.

27. **Martin S.**
June 23, 2016

@ Another Anon: Then you apparently do not have a soul of scientist.

PS Thinking about new unexpected stuff may help you to find clues to other difficult phenomena.

28. **Low Math, Meekly Interacting**
June 23, 2016

At one time, wouldn't something like 2/3 of the particles we already observe have been completely unexpected? Of course, I suppose today we have much better constraints on where something unforeseen might be lurking. Still, I'm surprised and chagrined at how risible some in the blogosphere seem to think the activity and excitement around the 750 GeV bump has been. It's not like HEP theorists have had much else that was obviously better to do lately.

29. **Peter Woit**
June 23, 2016

LMMI,
The main blog making fun of this has been Resonaances, whose author himself has papers on two different models explaining the bump. There's a reason he calls himself "Jester", and I think a sense of humor about this whole situation is not out of place…

30. **Low Math, Meekly Interacting**
June 23, 2016

I do get the humor, sort of. Given the abject starvation for new data theorists are experiencing, it's certainly better for one's sanity to laugh than to cry. But there's also some accusatory chatter about schlock being dumped into the arXiv to drive up citation counts, people ruining their own Christmas, etc., and all that seems kind of cruel to me. I mean, HEP theorists may be treading a path into the driest desert imaginable. The way things are going, there may literally be nothing for the next Feynman or Gell-Mann or Weinberg or Wilczek to sink his or her teeth into…EVER. So, for pity's sake, why look askance at people burning a few hours and electrons on a potentially vanishing dream. At least they're theorizing about an actual result. Spurious result or not, it's no anthropic landscape.

31. **DrDave**
June 23, 2016

"I do think it's a pretty powerful model: once you know the announcement plan, you know the result."
So you are saying that you can make predictions based on observation, and that the results are falsifiable.

32. **chris**
June 24, 2016

this aligns nicely with the rumors i have been hearing.

33. **Peter Shor**
June 24, 2016

I think this can all be explained with sociology/game theory.

If I remember properly, before the LHC came on, physics departments increased the number of phenomenologists they were hiring, because they thought there was a substantial probability that with the LHC, this field would become more important.

So now we have all these untenured phenomenologists, all of whom are desperate to write a significant paper in the next few years. It would be foolish of them *not to* publish explanations for any possibly significant bump discovered at the LHC, on the off-chance that the bump is real and their paper is correct, winning them the tenure lottery.

34. **wb**
June 24, 2016

>> all these untenured phenomenologists

In seems that the 'worst case scenario' is becoming reality: The LHC saw the Higgs but nothing else.
So what now?

35. **John Baez**
June 24, 2016

Another Anon wrote:

> But I don't think I'd be so happy to discover I was ignorant.

Opinions may vary, but I already feel extremely ignorant about particle physics. There are lots of mysteries in this subject, from why such a complicated theory as the Standard Model seems to work so well, to what's the explanation of dark matter (a new particle not in the Standard Model?), inflation (another new particle?), dark energy, etc. etc. Since I'm completely stuck on trying to figure these things out, any further clues would be great.

36. **Jonathan Tooker**
June 26, 2016

When will they announce the spin of the Higgs-like particle that got announced in this way in 2012?

37. **Karibe**
June 27, 2016

I was hoping the bump won’t go away, but grow into a signal.

Some Ancient History

June 27, 2016
Categories: Uncategorized

A couple months ago there was a session at an APS meeting with the topic Sidney Coleman Remembered. Slides are available for talks by Coleman's student Erick Weinberg and colleague Howard Georgi. Georgi has recently posted a written version of the talk here. He also a few years ago wrote this biographical memoir about Coleman for the National Academy of Sciences.

David Derbes and collaborators are putting together a book version of Coleman's famous lectures on quantum field theory, hope to be finished with this by the end of the summer. I've helped out in a very small way by sending them a scan of my notes from when I took Coleman's course very long ago.

This will be a great resource for anyone learning QFT and, in the meantime, if you don't have a copy of Coleman's Erice lectures, Aspects of Symmetry, you should get one. The period of these lectures spans the late sixties and seventies, and at the time they were required reading for everyone, giving every couple years a lucid explanation of the most important new ideas in the field. The last (1979) lectures are about the 1/N expansion, and I notice that Coleman extensively credits Witten, who was a postdoc at Harvard at the time. A couple years later, Witten in some sense took over from Coleman, lecturing about supersymmetry (a topic Coleman never warmed to) at Erice in 1981.

Last Friday at CERN there was an event devoted to the 40th anniversary of supergravity. Coleman makes a couple appearances, with Sergio Ferrara claiming he was responsible for the name "gravitino" and Peter van Nieuwenhuizen quoting him as saying

> I am uninterested in gravity, and superuninterested in supergravity.

One reason for this was surely the ferociously technical difficulties of constructing supergravity, with Coleman not interested in difficult calculations. Another was likely a lack of interest in topics with no known relation to experiment, which likely had to do with his comments about both gravity and supergravity. At the CERN event, Albert De Roecke's presentation, Desperately Seeking SUSY, reviews the long story of the failure of SUSY and supergravity to make contact with experiment, including a New York Times 1993 article about the failure to find SUSY at the Tevatron. It has extensive detail about the unsuccessful searches at the LHC.

De Roeck also includes a copy of David Gross's 1994 bet with Ken Lane that SUSY will appear at the LHC (when at least 50 inverse fb have been accumulated). Gross will likely have to pay this off next year, but another such bet just came due on June 16th, so a group of theorists should by now be buying expensive cognac for their more prescient colleagues.

Comments

1. **Tony Smith**
   June 27, 2016

   How would the Derbes et al version of Sidney Coleman's lectures on quantum field theory compare with the 1999 Cambridge University Press book by Robin Ticciati Quantum Field Theory for Mathematicians in which the preface says that Ticciati
   "… had audited Sidney Coleman's outstanding Harvard lectures
   and had taken very good notes …[and]… had Robert Brandenburger's official solutions to all the homework sets. …".

   Tony

2. **Peter Woit**
   June 27, 2016

   Tony Smith,

   Ticciati's book is quite different, although in places one can certainly see Coleman's influence. Probably quite a few modern QFT books were influenced, directly or indirectly, by Coleman's course. Picking up a few at random, I see that in their introductions Tony Zee and Michael Peskin mention learning QFT from Coleman.

3. **Jeff M**
   June 27, 2016

   I notice that Peter Orland is a NO on the linked bet, which should be paid off now. Enjoy the cognac Peter 🙂

4. **susy**
   June 27, 2016

   how many inverse fb is needed before any conclusions about SUSY can be made at LHC energies?

5. **David Derbes**
   June 27, 2016

   Hi, Peter.

   Thank you very much for the plug, and also for the xeroxed notes, which have been invaluable. But I want to be sure that my colleagues' names are front and center. First and foremost, none of this would have happened without Brian Hill's notes. Brian was a friend of Sidney's and a grader of Sidney's QFT course for three years. His handwritten notes were the closest thing to a textbook that the course had for at least fifteen years. About three years ago, Bryan Ging-Ge Chen and Yuan-Sen Ting LaTeX'd Brian's notes for the arXiv (and you can get them today, if you like). I got in touch with Brian, Bryan and Yuan-Sen and asked about the second semester. They hadn't any notes for that. I emailed a former high school student of mine (I'm a high school teacher, but I have a PhD in theoretical physics), Matt Headrick, who had a set of second semester notes by a grad student who prefers to remain anonymous. Yuan-Sen made the initial suggestion that we try to turn the notes, the videos and all the rest into a book. Yuan-Sen suggested I get in touch with Richard Sohn, who was very helpful to him and Bryan when they were LaTeX'ing Brian's notes. Richard has taken over the job of the second semester, I'm about done with the first. Richard has done more work on the book than anyone. I mentioned this project to Peter about two years ago, and he sent me his notes. As most of your readers know, Harvard videotaped Coleman's lectures in 1975-76, and these are on the web. Richard and I have transcribed nearly all of them. In addition, we hope to have a foreword by David Kaiser, a former Coleman grad student (though he finished with Alan Guth), who has also edited a big chunk of the first semester. He also got copies of Sidney's own notes from Diana Coleman, Sidney's widow. When Richard and I had been at it for a few months, David Griffiths (of textbook fame, and also a Coleman PhD student) emailed Yuan-Sen to ask about the second semester. Yuan-Sen told him to get in touch with me. David has provided unbelievably wonderful criticism, great suggestions and rewording, and generally improved the manuscript's quality by a factor of at least 2 and probably 3. There ought to be at least seven names on the cover: Brian Hill, Yuan-Sen Ting, Bryan Ging-Ge Chen, Richard Sohn, David Griffiths, David Kaiser and me. (I would also include the anonymous grad student, but that's not in keeping with his wishes.) At present only Bryan's, Yuan-Sen's, Richard's and my names are on it (tentatively) (very probably David K will be credited on the cover for the foreword), but I'm going to ask Brian, David G and the anonymous grad student again once we're nearly done. I should also add that all proceeds are going to Diana Coleman. We're all doing this work for free, out of affection and respect for Sidney Coleman. Harvard's IP people are OK with it, nobody asked for royalties (not even Harvard), and I hope the community will be reasonably happy with the result. It isn't going to be the book Sidney would have written, no one should be fooled, but we hope it will be a reasonable approximation, and we think Sidney's voice is clearly heard. Finally, we're also including problem sets and solutions; these were supplied by Matt Headrick, Peter Woit, Brian Hill, and the graders Nathan Salwen and David Lee. At present we're not including final exams and answers, but maybe we should. This is a big book, perhaps 900 pp. We hope to deliver the finished manuscript (LaTeX source and illustrations) to World Scientific by September 1.

6. **Peter Woit**
   June 27, 2016

   susy,
   To get some rough numbers, Tevatron gluino mass bounds were something like 350 GeV. Naively scaling to the LHC at a factor of 6.5 higher in energy, one would expect to be able to reach up to something like 2300 GeV. Results from 3 inverse fb reported late last year give (see De Roeck) something like 1700 GeV bounds. Measured in logarithmic terms, there's not that much space left between 1700 and 2300 GeV, most of the increased reach expected from the LHC has already happened. I don't think David Gross would be making his bet with Ken Lane today…

   There are of course lots of different SUSY particles one can look for, and higher luminosity is more relevant to states that are not strongly interacting. There will be a very long story of slowly rising bounds of different sorts, best to leave more discussion of this to August, when we'll see what gets announced at ICHEP (and whether that starts to cause SUSY enthusiasts to finally give up…)

7. **Peter Woit**
   June 27, 2016

   Thanks David for the details, I think this is a great project, congratulations to all of you on getting near the end!

8. **Lee Smolin**
   June 27, 2016

   Dear Peter,

   I was told that "I am uninterested in gravity, and super-uninterested in supergravity…but…" was most of Sydney's letter of recommendation for me. Perhaps he used it for others as well.

   Lee

9. **Peter Woit**
   June 27, 2016

   Thanks Lee,
   That's consistent with another version of the talk by Peter van Nieuwenhuizen, see here
   media.scgp.stonybrook.edu/presentations/20140109_vanNieuwenhuizen.pdf
   where he tells the story this way:

   I remember a recommendation letter from Sydney Coleman that began as
   follows: "I am uninterested in gravity, and superuninterested in supergravity".

10. **Harrison**
    June 28, 2016

    "If you tell yourself something over and over again, right or wrong, it becomes intuitive."-Sidney Coleman

    Thanks very much for that biographic item on him.

11. **Peter Orland**
    June 28, 2016

    Jeff M,

    Thanks for the good wishes. For some reason, an earlier bet (from 2000) was extended, without consulting most of the signers.

    I drink milliliters of alcohol per year, but I look forward to getting a contact high from other winners (if the bet is ever paid off).

12. **David Appell**
    June 28, 2016

    A few years ago I wrote an article about the discovery of supergravity, including the night Peter van Nieuwenhuizen verified by computer that a prominent term vanished:

    "When supergravity was born," Physics World, September 2012, pp 32-36.
    http://www.davidappell.com/articles/PWSep12appell-supergravity.pdf

    He told me that right afterwards he was depressed for days. And he told me that he got emotional while reading my article about it, 36 years later.

13. **Alan**
    June 29, 2016

    In the De Roeck presentation, he suggested 1500GeV was a limit to gluino mass for SUSY being natural. If mass of gluino is already constrained for >1700GeV, does it mean that naturalness has already be lost?

14. **Peter Woit**
    June 29, 2016

    Alan,

The problem is that there is no well-defined definition of what "naturalness" precisely means. It is now widely recognized though that the LHC results are in stark contrast to pre-LHC expectations based on such "naturalness" arguments.

15. **lzp**
June 29, 2016

Ouch, these are strong words, quoted from Peter van Nieuwenhuizen's presentation linked above–but in line with the feelings/beliefs/preferences of the majority of the readers of this blog:

"The reader may ask why in this book [Facts and Mysteries of Elementary Particles, 2003] string theory and supersymmetry have not been discussed. . . The fact is that this book is about physics and this implies that theoretical ideas must be supported by experimental facts. Neither supersymmetry nor string theory satisfy this criterion. They are figments of the theoretical mind. To quote Pauli, they are not even wrong. They have no place here. . ." (M. Veltman)

16. **Alan**
June 29, 2016

Ok, more precisely, when will 84% -1 sigma- of pMSSM models be discarded, taking the prior as experimental constraints which where in 1977 -when MSSM was created-.

17. **Peter Woit**
June 29, 2016

Alan,
The problem with that formulation is that there's no obvious way to choose a measure on the space of pMSSM models. You want "naturalness" to mean that this measure falls off at higher masses, but how to do this is ill-defined.

18. **GoletaBeach**
June 30, 2016

Perhaps the most successful endeavor in broader particle physics in the past 20-or-so years was… the LHC detector collaborations argued for very challenging and expensive calorimeters… far better than D0 or CDF… at a time when Higgs to gamma gamma was largely disbelieved for two reasons…. 1)the bias of experimentalists to believe new particles are usually very heavy, particularly a fundamental scalar (with the well known theoretical support for the Higgs)…. that was why the no-lose Higgs theorems were attractive for SSC experimental advocates…. 2)Higgs to gamma gamma is just too darned hard, experimentally speaking.

ATLAS and CMS pushed for fantastically expensive calorimeters anyway, and they were right. Of course Higgs to Z0 Z0 is excellent too. But once again, seeing that is another testament to the quality of the detectors… good coverage, excellent systems, etc. All the while, ferocious downward pressure on budgets, and ATLAS and CMS came through.

Not much connection with Sidney Coleman, who was a delightful guy… in fact ATLAS and CMS (and LIGO, and maybe LSST, Planck, LZ, etc) are collectives where single great personalities aren't particularly effective… endless institutional reviews and internal assessments drive the modern big science collaborations. Celebrating that rather boring process isn't as fun as the celebrating the 40th anniversary of supergravity, and a good party is always good, even if it celebrates 25 years of not seeing dark matter or SUSY.

Big science collaborations are sometimes called the last refuge of communism. Nobody wants to laud that. Maybe we should.

19. **George Pennington**
July 6, 2016

Peter,

Having recently struggled to review some of Karl Popper's works on the philosophical problem of induction, I'm totally in favor of placing a deadline on proving a theory. The bet, be it a bottle of cognac or a set of Encyclopedia Britannica, puts the unfalsifiable theory to rest so that science can continue to advance along other fronts. I have great respect for the theorist who is willing to set an end date and a bottle of France's finest on the table to either connect their theory with empirical experiment or set it aside in favor of others willing to take the same challenge.

20. **zzz**
July 7, 2016

they have bet like that, what they need is a bookie to enforce it.

21. **Andrew Porter**
July 9, 2016

I knew him as someone I saw at science fiction conventions, often at in the Boston area, but also at the annual World Science Fiction Conventions, held at various places around the world. His help for SF writers struggling to put real and logical physics into their stories and novels was legendary.
.
In the SF field, he was long known as "the best physicist to have never won a Nobel Prize," as detailed here:
.
http://fancyclopedia.org/sidney-coleman

22. **Richard A. Lupoff**
July 9, 2016

As a onetime science fiction fan and sometime science fiction writer, I knew Sidney Coleman in another, and non-technical, context. When I was grappling with a scientific concept that was vital to a story I was writing, I would ask Sid for help. He was endlessly patient and generous in this regard. Importantly, he was utterly lucid, explaining concepts in terms that I–very much a layman!–could understand. Sid is fondly remembered in the literary field, and it is gratifying to see the attention he is now receiving among his former colleagues. He deserves to receive great credit and to be fondly and warmly remembered!

23. **Gregory Benford**
July 9, 2016

I'm a physicist at UCI and wrote a memoir about Sid in 2012 that focuses on the man, an old friend:
http://www.gregorybenford.com/uncategorized/remembering-sid/

24. **Adam Whitman**
July 9, 2016

I've made numerous video tributes to Sidney Coleman. Here they are.

https://www.youtube.com/watch?v=nEn64S67VDc

https://www.youtube.com/watch?v=WLS2oUjA2qs

https://www.youtube.com/watch?v=S9qbZSVSFAM

https://www.youtube.com/watch?v=P2SL1GquC4A

Short Items

June 30, 2016
Categories: Uncategorized

- Erica Klarreich at Quanta magazine has a wonderful profile of Peter Scholze. Scholze has been busy revolutionizing various parts of arithmetic geometry in recent years, and the article does a good job of giving some of the flavor of this. I noticed this morning that Scholze has a new preprint out, about a q-deformation of de Rham cohomology, so that may be the latest, hottest news in the subject.

  The new paper was written for the occasion of his acceptance of the 2015 Fermat Prize. Another Quanta piece makes the obvious point that we already know who one winner of the 2018 Fields Medal will be. While Scholze has been awarded a fair number of prizes already, it's interesting that he's not universally in favor of the prize phenomenon: see here for some discussion of his decision last year to turn down one of the 2016 Breakthrough Prizes.

- In addition to being a great mathematician, Scholze also seems to be a fine human being. The AMS Notices this week has an interview with another such mathematician, Robert Bryant, who is now the AMS President, recently head of MSRI. Unlike Scholze, I've had the pleasure of getting to know Bryant a little bit, since he was a visitor one semester here at Columbia. The Notices article explains his interesting background, which is somewhat unusual for an academic mathematician. The math community is lucky to have leading figures like him combining mathematical talent and excellent personal qualities.

  I first heard about him back when I was a post-doc at Stony Brook, trying to learn more about mathematics and the math community. At some point I asked Claude LeBrun (a young geometer then, now an older one, with a conference in his honor next week in Montreal) who he thought the best young geometers were. He told me about Robert Bryant, and when I asked "why him?", his answer was "He's read and understood all of Cartan". I wasn't sure whether to take that seriously, but from the AMS interview, he was quite right about that.

- For one last piece of mathematics news, fans of geometric Langlands may want to take a look at the new preprint by David Ben-Zvi and David Nadler on a Betti form of geometric Langlands.
- Turning to physics, last week UCLA announced a $11 million donation to fund a Mani L. Bhaumik Institute for Theoretical Physics at UCLA. As NSF funding for theoretical physics stays flat or declines, at least in the US it is private funding like this that is becoming much more important.
- At CERN the LHC has reached design luminosity, and is breaking records with a fast pace of new collisions. This may have something to do with the report that the LHC is also about to tear open a portal to another dimension. Not clear why people are worrying about the 750 GeV state with this going on.

Soon heading North for a week-long vacation, blogging likely slim to non-existent.

Update: For geometric Langlands fans, this and this on the arXiv from Dennis Gaitsgory tonight.

Comments

1. **Heiko242**
   June 30, 2016

   I sometimes wonder if there is an upper limit for mathematical prodigy. Hardy gave himself a score of 25, Littlewood 30, Hilbert 80 and Ramanujan 100. Are people like Scholz, Bryant or Tao close to 80? Maybe even close to 100? Better?

2. **Peter Woit**
   June 30, 2016

   Heiko242,
   I don't think mathematicians can be usefully ranked like that. If one looks at the best work of mathematicians like Scholze, Bryant and Tao, I think one sees both different, incommensurable, technical talents, as well as different backgrounds that come into play. Bryant's story about his engagement with the works of Cartan is a good example. If he hadn't pursued that, but had gone in some other direction, would he have accomplished as much? Scholze's story about starting with the Wiles proof and working backwards is another example of a very special way of entering mathematics that worked out for him. It's possible that if Scholze had devoted himself to reading Cartan, and Bryant to trying to understand the Wiles proof, neither would have had anything like the same success.

   Great new mathematics comes out of a complicated mixture of having learned the right things, having the right talents, being in the right place to talk to people who can help, having the time and dedication to focus on something, and choosing the right thing to focus on. As mathematics progresses, in some sense easier things get done and what is left is harder. On the other hand, the way the community is organized and the way mathematicians are trained also evolves, making it possible for them to effectively specialize and attack problems beyond what their predecessors were able to deal with.

   To the extent that some isolated notion of raw talent is part of the story, I don't know of any reason to believe that the best mathematicians of today are particularly different than those of other eras.

3. **David Hansen**
   June 30, 2016

   I'm just writing to strongly endorse Peter's response to Heiko. Like all human endeavors, math has context, and any simplistic ranking of mathematicians is essentially meaningless.

4. **Bill**
   July 27, 2016

   http://www.dw.com/en/dont-call-me-a-prodigy-the-rising-stars-of-european-mathematics/a-19421389

Physics and Math News

July 11, 2016
Categories: Uncategorized

Now back from vacation, here's the latest on revolutionary developments in physics and mathematics:

- On the high energy physics front, the good news is that the LHC is performing remarkably well, with already over 13 inverse fb of luminosity, far above that expected at this time, on track to end up with a lot more than the targeted 25 inverse fb for the year. The bad news however is that new reliable rumors (together with the non-observation of any sign of a "special seminar" at CERN, see here) confirm non-existence of the 750 GeV state that would have killed the Standard Model and revolutionized the field. As far as I know, the plan is still to present these results publicly the first week of August at ICHEP, it looks like this will be on August 5.

  For some interesting discussion of the statistical analysis issues that come up when trying to quantify how significant the 2015 evidence is for the supposed 750 GeV state, see the comment section of this blog entry. These subtleties it seems will be made irrelevant by the arrival of new data.

- In mathematics there has also been an unconfirmed claim of something revolutionary, but the problem is that there's nothing analogous to new data coming in to help decide the issue. This is the claim first made four years ago by Mochizuki to have a proof of the abc conjecture, using new methods he calls "Inter-Universal Teichmuller Theory". The current situation is an extremely unusual one, with experts still unable to understand and evaluate the purported proof. For the best summary of the situation, see Brian Conrad's detailed explanation from last December here.

  Not much seems to have happened since then, but one very recent development has been the appearance of a new survey of the theory. Unfortunately, my guess is that this is not likely to address the issues raised by Conrad and provide what he and other experts are looking for: precise checkable arguments. Instead the new survey is another attempt by Mochizuki to communicate his general high-level vision, often in very metaphorical terms. The last section of the survey is a remarkable attempt to position his ideas in the landscape of modern mathematics, which includes setting these ideas in opposition to those of the dominant research program (and thus of great value if they work out).

  What's really odd here is the way that usual mechanisms for transmitting understanding have failed. Mochizuki has worked to transmit understanding of his ideas to a small number of others, but the transmission has stopped there, with understanding of the abc proof not moving from them to others. For a while the hope was that Go Yamashita would be the one to move this forward, but he has not produced a promised document, or succeeded in communicating by his talks. More recently, last year Yuichiro Hoshi produced a document that is supposed to explain crucial ideas, but it is in Japanese, so inaccessible to most experts. Why this has not been translated remains very unclear.

  Next week in Kyoto there will be another workshop trying to further understanding of the IUT theory. I hope this works out better than the last one. There's a preparatory document here which to me seems to ignore the fundamental problem of figuring out what has gone wrong so far. In particular, its last point appears to be explicitly aimed at discouraging anyone in the audience from confronting speakers that are not successfully communicating ideas and insisting that they try to do better. It would be more fruitful to encourage this instead.

Comments

1. **CERN Physicist**
   July 11, 2016

   Latest Rumor : ATLAS shows gamma-gamma bump at 975 GEV, confirmed at 2.7 Sigma. Will update.

2. **Urs Schreiber**
   July 12, 2016

   Elsewhere this had been crypto-rumoured for a while already:

   https://twitter.com/dorigo/status/745528592927338498
   https://twitter.com/dorigo/status/750266675526832128

3. **Another Anon**
   July 12, 2016

   These sigma significance levels need reevaluation. 2.5 sigma supposedly means a result is correct over 99% of the time – in other words, hardly ever wrong. But we know that's nowhere near the reality.

4. **Peter Woit**
   July 12, 2016

   Another Anon,
   That's the topic being discussed at Tommaso Dorigo's blog (which would be a better place to discuss this than here). The subtleties of going from such a "local" number to a number reflecting the actual probability of something new are well-known in HEP physics (and the reason behind the very high "5 sigma" standard). This is nothing new, but has always been part of this subject.

5. **sdf**
   July 12, 2016

   Is this on anyone's radar? http://www.ihes.fr/~lafforgue/math/NoriMotivesInformation.pdf

   Re. Y. Hoshi, he gave ~4 hours of lectures in Paris on his mono-anabelian ideas, he has superb English so that is not a reason for lack of translation. One frustrating thing he does is give a new name to everything e.g. say A is an 'isomorph' of B if A is isomorphic to B; say that a morphism is 'multiradial' if it is not injective… etc etc. this sort of thing seemed to cause a lot of raised eyebrows in the theatre of IHP—this appears to be the m.o. in Mochizuki's writings too.

6. **Chris Austin**
   July 12, 2016

   On page 3 of the new overview, ("Alien Copies"), Mochizuki says, (slightly paraphrased):

"Let N be a fixed natural number > 1. Then the issue of bounding a given nonnegative real number h \ge 0 may be understood as the issue of showing that N h is roughly equal to h."

This is an absurd statement, because h factors out of the approximate equality. It is not a typo, because he then repeats it in a formula.

7. **Peter Woit**
July 12, 2016

Chris Austin,
In the text you mention Mochizuki is explicitly pointing to sections 2.3 and 2.4 for an explanation of what this is supposed to mean (i.e. what does "roughly equal", or "=" mean), so you need to go there to interpret what he is writing. The problem is not that he is making trivial, easily identifiable mistakes.

This does however give some idea of the problem with this survey and with similar things he writes. All sorts of things are in quotes (or analogies, not equalities) and even when they're not (e.g. roughly equal), it's often hard to track down a precise definition, or figure out what precise claims are being made. This then makes it impossible not only to check for flaws in the argument, but even to understand exactly what the argument is.

The 2012 IUT papers are supposed to contain the precise statements, but these are 600 pages long and have resisted the efforts of experts to understand them. Mochizuki claims that four mathematicians have, with his help, gone through these and checked them. Normally what should have happened is that, armed with a detailed understanding, these mathematicians should have been able to transmit this to others, and to write up an independent version, one that would help others understand the papers. Besides the possible example of the Hoshi document in Japanese, this has not happened.

8. **Michael**
July 12, 2016

Chris Austin,

A clearer version of that statement appears at the bottom of page 11: The point is that 'roughly equal' is in the sense of the 'absolute error' not the 'relative error'. For instance, if h and 101h differ by at most 1, then h can't exceed 0.01. (By contrast, the relative error between Nh and h is fixed by N and, as you assert, would tell us nothing about h itself.)

9. **Chris Austin**
July 12, 2016

Peter and Michael, thank you for the clarifications.

10. **anon**
July 12, 2016

Regarding Hoshi's document, Brian Conrad wrote in January in a comment on the same page that has his summary of the December workshop that Hoshi is hoping to make an English translation of his notes available around the time of summer workshop.

11. **Peter Woit**
July 12, 2016

anon,
I've heard that such a translation is under way, but one of the mysteries of the subject is why this is taking so long. With the workshop less than a week away, it's already pretty much too late to be useful to people preparing for the workshop.

12. **Peter Zbornik**
July 12, 2016

Peter, you write that "Mochizuki has worked to transmit understanding of his ideas to a small number of others, but the transmission has stopped there, with understanding of the abc proof not moving from them to others."

I would like to comment on that. There actually seems to be substantial "transmission" of IUT "to others" from Taylor Dupuy. Dupuy is the guy that Felipe Voloch "felt" "was the one that made the most progress learning the stuff" on the Oxford workshop (https://plus.google.com/106680226131440966362/posts/UHoetkZ7XXK).

You will find Dupuy's has published several series of IUT-related videos on his vlog:
https://www.youtube.com/channel/UCHWnZ1NtJ4WvE5AHmNVXziw/playlists

The playlist "IUT strategy" seems to be a good starting point:
https://www.youtube.com/playlist?list=PLJmfLfPx1OednuaMHywSDgaDzRa0MiaOf

Then seems to be other relevant playlists too, like:
The geometry of Frobenioids, Anabelian Geometry, Etale Theta, Hodge Theatres, etc.

It seems that Taylor Dupuy is making an effort to "transmit" Mochizuki's work to a wider audience.

Dupuy will be a speaker at the RIMS conference. It seems that the conference is going to be recorded on video, although maybe not streamed.
https://twitter.com/DupuyTaylor/status/744999080229765120
https://twitter.com/DupuyTaylor/status/744999335092436992

Otherwise, part of the confusion around this proof seems to come from the fact, that Mochizuki has worked on IUT for a long time and few professionals had done their homework before the Oxford workshop. Imagine you attend a class in some branch of maths you haven't studied before and that you have ignored studying the required prequisite literature. I think that's what happened at Oxford and that is really not very surprising.

I for sure experienced this quite some times during my math studies, when I had skipped my homework before class. The worst thing I could do in this situation would be to ask impertinent and not-so-very-clever questions.

The second issue worh mentioning is the fact that Mochizuki is Japanese and seems to be value that highly (someone wrote he is from a samuraj family).

Thus, when Mochizuki asks people to do their homework, to be polite and not to interrupt if it is not absolutely necessary, then this might be an other way to ask that the visitors to respect the "the Japanese language-based mathematical culture that exists" "at RIMS".
http://www.kurims.kyoto-u.ac.jp/~motizuki/students-english.html

To quote Mochizuki in full:
Especially with regard to (1), the prospective applicant should be aware that Japanese
is the official language in which mathematical and administrative affairs are conducted here at RIMS. Some individuals may tend to regard this state of affairs as a sort of "unpleasant obstacle". On the other hand, at the present time, there is no shortage of institutions throughout the world at which one may obtain a quality graduate education in mathematics in English. By contrast, the Japanese language-based mathematical culture that exists here at RIMS [and indeed at other Japanese universities] is, in my opinion, a precious cultural asset, both for Japan and for the world.

There has been more written about this:
"Despite mathematics being a universal language, culture clash could be getting in the way, says Kim. "In Japan people are pretty used to long, technical discussions by the lecturer that require a lot of concentration," he says. "In America or England we expect much more interaction, pointed questions coming from the audience, at least some level of heated debate."
https://www.newscientist.com/article/dn28682-mathematicians-left-baffled-after-three-year-struggle-over-proof/

I am certainly not an expert in Japanese culture, but I know so much, that they tend to put more emphasis on politeness and respect, than in the West.

Well in any case it seems that Dupuy has done his homework and is actually "transmitting" IUT to the West.

If Dupuy's videos on IUT would get wider publicity, that might actually constructively contribute to transmitting IUT to a wider audience and thus to further mathematical knowledge and development :).

13. **Dan**
July 12, 2016

As a physics person, it's hard to imagine that people are so invested in even trying to read Mochizuki's paper. Given this kind of difficulty, it seems like a similar document in physics would have been abandoned and forgotten after a day or two, let alone years (Verlinde's entropic gravity paper comes to mind). Maybe it just shows how different the goals are: here one can still hope to extract precise technical statements/proofs, whereas in a physics paper that would often be, at most, a secondary goal. Still, it seems strange… I guess Mochizuki must have some serious cred/social capital in his field?

14. **Peter Woit**
July 12, 2016

Dan,

I don't think there really could be a physics analog of this, since what's at issue is whether a very complex set of new ideas is powerful enough to provide a proof, a kind of question physicists don't deal with. Mochizuki is a well-known, accomplished and talented mathematician, and the fascination of this is that he clearly has new ideas, but the complexity of what he is doing and the way he has chosen to explain them has left experts unsure of how much can be gotten out of them.

The "entropic gravity" business is quite different. There was nothing at all complicated about that. It was a simple, but vague idea (not much different than ones other people had discussed in the past). Experts could immediately read Verlinde's paper and see what he had. Different people would have different judgments about whether pursuing that kind of vague idea would ever lead anywhere, but that's a different question.

15. **Dan**
July 12, 2016

I guess that's part of my confusion- how vague/non-vague is Mochizuki's work? Trying to parse the intro of his new paper is impossible for me; it sounds very vague, but obviously it's above my pay grade. Maybe I can ask a sharper question: do people seem to agree that his paper is technically sound, but they just aren't sure how it connects to standard literature? Or is there any possibility that he is "wrong"/any of this is jibberish?

16. **Peter Woit**
July 12, 2016

Peter Zbornik,

I'm sorry, but I really don't buy at all the "Japanese culture" explanation for the problem. Mochizuki grew up here in New York City (from age 5), went to school at Exeter, then got his undergraduate and graduate degrees at Princeton. His upbringing is more American than mine (I spent 5 years as a kid in France).

I also don't buy the "didn't do their homework" explanation for what happened in Oxford. Brian Conrad's article I think does an excellent job of explaining the problems with the talks, and looking at the slides confirms what he has to say. He and others who were there are the best in the business, and claiming that the problem was that they were asking stupid uninformed questions is just ridiculous.

From what I can tell, Taylor Dupuy's efforts are very enthusiastic (I've looked at some of them), but at a similar level of vagueness to Mochizuki's surveys (or even much more so), and suffer from the same problems of being disconnected from the rest of solid mathematics, and being too imprecise to be checkable. Maybe I'm wrong, but as far as I know, he's not claiming to fully understand the details and have checked them.

17. **Peter Woit**
July 12, 2016

Dan,
The problem is twofold: the shorter "survey" papers are very vague, the 600 page 4-part IUT paper series more precise but bafflingly complex and invoking a completely new universe of mathematical objects. A lot of that complexity is likely irrelevant to the heart of the supposed proof. What's needed is something in between: a detailed outline of the proof, with precise, checkable statements. The mystery here is why, four years out, such a thing seems to still not exist.

This kind of highly complex proof is inherently difficult to check: the mathematical objects involved can have subtle, often unintuitive behavior. To check such a proof, you need an expert, one who has some pretty deep insight into the behavior of the mathematical objects being manipulated. My impression is that we're still unfortunately far from anyone (other than Mochizuki), being able to carry this out with confidence in the result.

18. **Zoviyer**
July 12, 2016

I think is clear that the way of transmitting IUT has not been efficient. But on the other hand it may be that this theory is enough original and has been developed for such a long time that it would be as if Grothendieck presented to the world the EGA or SGA volumes in one stroke and thinking that in three years we could understand his proof of the first parts of the Weil conjectures.

19. **Peter Woit**

July 12, 2016

Zoviyer,

Sure, that's in principle possible. My guess though is that in such a hypothetical case it would take people a significant amount of time to understand the whole thing, but you would see progress taking place as this happened, and you wouldn't see the odd phenomenon of people close to Grothendieck supposedly mastering the ideas, but unable to write up their own version and explain it to others. This is a really peculiar situation that I know of no historical analog for.

20. **Jeffrey M**
July 12, 2016

Zoviyer,

Thing is, while it took a bit for people to catch on to what Grothendieck was doing, they did, and in a few years. With the IUT the best people just have no clue. When Perelman proved Thurston's geometrization conjecture, I was at a conference with many of the best geometric analysts in the world. Perelman had posted some work, and everyone was reading it, and within weeks they had a very good idea of what he had done. When Wiles proved Fermat, the same thing. The hole in his original argument was picked up quickly, took him a year to fix. What's going on here is you have someone claiming to have essentially invented an entirely new branch of math, with little connection to other branches. It's like Newton and Leibniz with calculus, more or less. People were using calculus almost immediately to do all sorts of amazing things, though of course it took more than a hundred years to put it on a good foundation. But people understood it, and used it, lots and lots of people. No one really seems to understand IUT, at least well enough to explain it in any fashion to very very smart people. No one has used it for anything. It could be correct, but I somehow doubt it. 600 pages of heavy duty math has probably tens of thousands of little things that could be wrong, and any one of them would likely blow the whole thing up.

21. **Zoviyer**
July 12, 2016

Peter,

I agree with your points. From the outside looks as if both Hoshi and Yamashita had no internalized the crucial ideas and how they relate as steps of the proof, and that's why it has not been communicated efficiently in the workshop or afterwards. The document that has not been translated to English is "just" an Introduction to IUT

22. **kbot**
July 13, 2016

With regards to the statistics issue – another point is the so called "look elsewhere" effect. The LHC experiments do hundreds of searches looking at a large number of final states. It is not just the probability of this particular search one has to consider but also what the entire experiment sees as a whole to properly compute the probability of having these results. This is why it often appears that results that taken on their own in isolation would indeed be perplexing if there were 3 sigma deviations coming and going all the time. However, what is neglected in this discussion is that people forget that there are hundreds of searches that land spot on the SM. Just like one expects that if one has a bunch of experimental points that you expect to fall on a line you don't expect all of them to be within one error bar of the line. You expect a certain fraction of them to be 1 sigma, 2 sigma, 3 sigma, etc to be off just from the statistical nature of the measurement process. Unfortunately statistics and p-values are often misunderstood or misrepresented so people can easily get the wrong impression of what the real probability of seeing deviations is…

23. **Peter Zbornik**
July 14, 2016

Peter, I agree with you, there is no proof published in a peer-reviewed journal.

But then we have a unique situation, where there is a claim, that Mochizuki has changed the foundations of mathematics.

Quoting the Nature article:
"Fesenko has studied Mochizuki's work in detail over the past year, visited him at RIMS again in the autumn of 2014 and says that he has now verified the proof. (The other three mathematicians who say they have corroborated it have also spent considerable time working alongside Mochizuki in Japan.) The overarching theme of inter-universal geometry, as Fesenko describes it, is that one must look at whole numbers in a different light — leaving addition aside and seeing the multiplication structure as something malleable and deformable. Standard multiplication would then be just one particular case of a family of structures, just as a circle is a special case of an ellipse. Fesenko says that Mochizuki compares himself to the mathematical giant Grothendieck — and it is no immodest claim. "We had mathematics before Mochizuki's work — and now we have mathematics after Mochizuki's work," Fesenko says.
http://www.nature.com/news/the-biggest-mystery-in-mathematics-shinichi-mochizuki-and-the-impenetrable-proof-1.18509

Talking about the lack of examples: refering to Fesenko above, it would be interesting to get an example, where in "non-standard multiplication" 2*3=5

I would not agree with progress not being made at Oxford, quoting Conrad in his posed referred in your post:
"Despite the difficulties and general audience frustration that emerged towards the end of the week, overall the workshop was valuable for several reasons. It improved awareness of some of the key ingredients and notions. Moreover, in addition to providing an illuminating discussion of ideas around the vast pre-IUT background, it also gave a clearer sense of a more efficient route into IUT (i.e., how to navigate around a lot of unnecessary material in prior papers). The workshop also clarified the effectivity issues and highlighted a crucial cohomological construction and some relevant notions concerning Frobenioids.

Another memorable feature of the meeting was seeing the expertise of Y. Hoshi on full display. He could always immediately correct any errors by speakers and made sincere attempts to give answers to many audience questions (which were often passed over to him when a speaker did not know the answer or did not explain it to the satisfaction of the audience)."

The main problem for the participants in Oxford was, that they did not understand the big picture and thus it was not easy to dig into the details. Conrad wrote in the post you refered above, that: "Many are willing to work hard to understand what must be very deep and powerful ideas, but they need a clearer sense of the landscape before beginning their journey." Dupuy and Mochizuki have now laid out the land whith their vlog resp. last paper. I believe this deserves some recognition.
There are several more papers with surveys ot IUT in the conference programme: https://www.maths.nottingham.ac.uk/personal/ibf/files/iut-kyoto-pr.html
At the conference there seem to also be some "transmission made".

The end of your post was not very polite nor correct:
"There's a preparatory document here which to me seems to ignore the fundamental problem of figuring out what has gone wrong so far. In particular, its last point appears to be explicitly aimed at discouraging anyone in the audience from confronting speakers that are not successfully communicating ideas and insisting that they try to do better."

The problem at the Oxie conference was that the interruptions made things worse, "party crashing-style", quoting Conrad:
"The fact that the audience was interrupting with so many basic questions caused the lectures to fall behind schedule, which caused some talks to go even faster to try to catch up with the intended schedule, leading to a feedback loop of even more audience confusion, but it was the initial "too much information" problem that caused the

many basic questions to arise in the first place. Lectures should be aimed at the audience that is present."

As for the doing the homework before the conference at Oxie. It really seems that all these esteemed experts did not study the IUT and other papers before the conference. Specifically: Voloch wrote in the last comment of his blog post, that:
"I prepared some but did not put 100s of hours as some had suggested but neither did most people I talked to."
Conrad: "It was reasonable that participants with advanced expertise in arithmetic geometry should get something out of the meeting even without reading any IUT-related material in advance, as none of us were expecting to emerge as experts (just seeking basic enlightenment)."
Contrast this with Fesenko and Mochizuki in the nature article:
"Mochizuki has estimated that it would take a maths graduate student about 10 years to be able to understand his work, and Fesenko believes that it would take even an expert in arithmetic geometry some 500 hours. So far, only four mathematicians say that they have been able to read the entire proof."
Contrast this with Conrads post, quote:
"At multiple times during the workshop we were shown lists of how many hours were invested by those who have already learned the theory"
Well, sad to say, "basic enlightenment" is seldom effortless. I think we can agree on that :o)

Yes, these guys were experts in their field, but they were faced with an other new field of maths and in this case, like it or not, you have to do your homework, because they cannot by definition be experts in this new field.

Now we have some Japanese and a Russian having spent considerable time with Mochizuki. Maybe some of the English-speaking "expert" community would also venture to do the same? That would certainly prove fruitful.

But then there definitely is a culture barrier here, undeniably, since Hoshi's IUT introduction has not been translated to English and evidently the Japanese do not consider it to be a priority to translate it to the English-speaking mathematicians :o)

Please allow me a personal reflection. I believe we have been stuck with an outdated number system based addition and multiplication. At the same time there really has been almost no work on alternative construcions of the natural numbers or on generalizing multiplication (the only serious one I know of are the surreals) since Hilbert's program. So from that point of view, for me, it is difficult not to see the work of Mochizuki as a possible "light in the tunnel". There is no work on "un-ordered" "numbers" (whose cardinality exceed that of the largest possible ordered field). On the other hand we have all those wierd stuff with connections to quantum mechanicsa around the Riemann hypothesiss, which all point to the need to widen the notion of basic concepts like "number", "addition", "multiplication" and "order".

Now let's see if the new workshop will bring "order out of chaos" :0). It seems to be invitation-only, where the applicant has to state his or her "knowledge of IUT" (https://www.maths.nottingham.ac.uk/personal/ibf/files/iut-rims16.html), so party-crashers may be stopped at the gates. It is organised at Mochizuki's home turf (he has stopped travelling outside of Kyoto, last years) by Mochizuki, Fesenko and Taguchi. So it seems that the scene is set for some "transmission" of knowledge, provided that the participants have done their homework, will be polite and will not crash the lectures.

Somewhere I read, that Mochizuki estimated it would take like until, I believe, 2020 before IUT was understood and accepted.

Maybe in 2020 we will have the abc proof and some less rigid foundations of mathematics to go with it too, if Mochizuki doesn't go Perelman playing ping pong with himself after all the hate thrown at him in the mean time.

Maybe it would be suitable to end this comment by quoting some "laws" from Fesenko's website:
"Everything takes longer than you think [including learning IUT – comment by PZ]."
"When all else fails, read the instructions [of required reading for the conference – comment by PZ]."
"Abrams's Advice: When eating an elephant, take one bite at a time [i.e. learning IUT takes time, there is no short-cuts – comment by PZ]."
https://www.maths.nottingham.ac.uk/personal/ibf/some.html

24. **Peter Woit**
July 14, 2016

Peter Zbornik,
I think you're selectively quoting Conrad, who was being exceedingly polite (despite being repeatedly insulted as a rude party-crasher who hadn't done his homework). I hope the organizers of the Kyoto workshop think carefully about his analysis of what went wrong at Oxford and why Mochizuki's work is not getting understood by experts. That they think the problem was experts asking questions and that they are trying to discourage questions is not confidence inspiring. When no one in the audience of a talk is understanding the speaker, it is much better for everyone if someone interrupts with questions than if the speaker is allowed to continue unimpeded.

25. **Narad**
July 14, 2016

> Brian Conrad's article I think does an excellent job of explaining the problems with the talks….

Speaking as layperson, but with at least pretty rigorous undergraduate training, I also found it invaluable in understanding things well beyond just "drama + mystery proof." Thanks for leading me to it.

26. **hmm**
July 19, 2016

IUT Kyoto Summit seems is progressing constructive discussions.
Christelle Vinsent (@ xl772) 's tweets is very convenient.
and this site is very nice. https://storify.com/xl772

27. **Peter Zbornik**
July 23, 2016

Peter, for completeness, here is Fesenko's take on the proceedings of the Oxford workshop and on the reasons for the relatively slower pace of knowledge transfer of IUT to a wider international audience: https://www.maths.nottingham.ac.uk/personal/ibf/files/iut-i-rep.html

28. **Peter Woit**
July 23, 2016

Peter Zbornik,
I wrote about that at the time here:
http://www.math.columbia.edu/~woit/wordpress/?p=8195
Fesenko kept changing the part of it blaming Brian Conrad (which I think is absurd), I see the latest version is

” Some participants who had ignored recommendations to study the theory before the workshop did not always behave considerately towards the speakers, interrupted the flow of talks by asking shallow questions and felt free to exhibit negative emotions without any concern to the other participants. ”

There’s an ongoing problem with how these ideas are being communicated, and instead of addressing it, Fesenko here tries to shoot the messenger. This isn’t helpful.

29. **anon**
July 25, 2016

This whole saga just sounds unprofessional on the part of the principal actor. Blaming it on some inherent problem with an idea of Japanese culture derived from orientalist Hollywood minstrel shows seems insulting to Japanese culture.

30. **marshall Flax**
July 25, 2016

@anon Don’t think it is fair to call anyone “unprofessional”. Simpler explanation is that it is *very very very hard* to simplify the work of a decade or two into digestible pieces. Not only is it hard work, but it is work that might require the surrounding community to help. Perhaps the struggle we’re observing is actually the only way to clarify things. Perhaps this really is the best of all possible worlds.

This might actually be the most efficient way to actually share a breakthrough, even if it is painful for all those directly involved?

31. **Peter Woit**
July 25, 2016

Marshall Flax,
This is an unusually difficult situation, both for Mochizuki and for those trying to understand his ideas. We’ll see what emerges from the Kyoto workshop, but reports I’ve heard are not very encouraging, with the same obstructions to progress obvious at the Oxford workshop still there. I strongly disagree that this is the best of all possible worlds, in particular, better possible worlds exist in which
1. Hoshi’s survey exists in English
2. Fesenko took to heart criticisms of what happened in Oxford, encouraging people to demand better explanations of material no one is understanding, instead of attacking people for doing this.
3. Mochizuki paid attention to explanations from experts of what was needed for them to follow him, and wrote up a version of the proof addressing their concerns.

I agree with anon that blaming problems on Japanese culture makes no sense. From what I can see, part of the story here is something not that uncommon in mathematics: an author writes up his ideas in a way that is impossible for others to follow. What is supposed to happen then is that journals tell the author to rewrite. The author may not agree and be willing to do so, and then others need to step in, figure out what is going on, and write something readable themselves. What is highly unusual here is that we haven’t seen that happen, and the central question is why that is.

32. **Snowman**
July 26, 2016

I think it’s pretty obvious what’s going on with Mochizuki. He wants to sell his theory, but others want to buy the proof for Vojta/Szpiro/ABC only. I think he sees the proof more as a motivational example for his theory, thus he has little interest to publish a more compact version that cuts out all unnecessary generality. I suspect in reading his papers in reverse chronology, concentrating only on the parts relevant to the proof, one would find more success than in trying to digest his published works of the last 10 years or so.

33. **marshall Flax**
July 26, 2016

@Snowman … don’t think that’s precisely true either … at least some of the participants in the seminar are imagining that the theory might be extendable one day to address even deeper questions like BSD or Riemann.

34. **Peter Zborník**
July 27, 2016

Some assorted tidbits on this mesmerizing topic:
—
1) The publication of the proof might be on the way:
It seems, that Mochizuki’s ABC papers are being refereed, some say that probably by a Japanese journal:
“These papers are currently being refereed, and, although they have not yet been
officially accepted for publication, the refereeing process is proceeding in an orderly,
constructive, and positive manner.”
http://www.kurims.kyoto-u.ac.jp/~motizuki/Questions%20and%20Comments%20on%20IUT.pdf
—
2) Replacement for Japanese IUT introduction:
Maybe Hoshi’s Introduction to IUT, might in large be replaced by his lecture notes from the Oxie and RIMS workshops? Someone mentioned they were good.
—
3) Intellectual theft in the mathematical community:
Fear of stealing the proof “Perelman”-wise, and of confronting frustrated, impertinent mathematicions, might also play a role in the slower pace of knowledge transfer. After all, the Mathematical community is a jungle, according to Grothendieck, with whom Mochizuki compares himself (see the article in nature above) and who just couldn’t care less for his work to go through standard peer reviewing.

This is what Wikipedia has to say on Grothendieck: (https://en.wikipedia.org/wiki/Alexander_Grothendieck):

“While not publishing mathematical research in conventional ways during the 1980s, he [Grothendieck] produced several influential manuscripts with limited distribution, with both mathematical and biographical content.”
…
“In the 1,000-page autobiographical manuscript Récoltes et semailles (1986) Grothendieck describes his approach to mathematics and his experiences in the mathematical community, a community that initially accepted him in an open and welcoming manner but which he progressively perceived to be governed by competition and status. He complains about what he saw as the “burial” of his work and betrayal by his former students and colleagues after he had left the community.[13] Récoltes et semailles work is now available on the internet in the French original,[36] and an English translation is underway. Parts of Récoltes et semailles have been translated into Spanish[37] and into Russian and published in Moscow.[38]”
…
“He […] criticized what he saw as the declining ethics of the scientific community, characterized by outright scientific theft that, according to him, had become

commonplace and tolerated."
https://en.wikipedia.org/wiki/Alexander_Grothendieck

—

4) Speculation:
We might also speculate, that Mochizuki might considers other things more important, than making "PR" for his theory and fraternizing with the mathematical community, like for instance, using IUT to prove Riemann's hypothesis. What then is a decade of slower acceptance of IUT and ABC compared to proving Riemann's hypothesis?

—

5) Cultural speculation
For some reason, I believe that the Japanese might not mind all that much, that IUT becomes a Japan-dominated field of study, centered at RIMS lead by Hoshi and guru Mochizuki :o) What these guys mind is not PR and marketing, but doing some fine new maths. Someone good at PR and marketing in the mathematics community should help them.

—

6) My own take:
Personally I am more interested in the mathematics, that comes with the proof, than the proof itself, so therefore I see as the biggest problem the unvillingness of the experts to invest the 500 hours or more to study the problem.

35. **e. ehrenweist**
July 28, 2016

Peter writes: "reports I've heard are not very encouraging, with the same obstructions to progress obvious at the Oxford workshop still there".

Taylor Dupuy wrote on twitter: "Great conference, we got way way farther than Oxford".

36. **e. ehrenweist**
July 28, 2016

Following up on my last message re @DupuyTaylor 's comments, some more twitter feedback on the IUT Summit: Artur Jackson (@arturj) wrote "I am impressed with … the depth of the discussions. Better results than Oxford". Ivan Fesenko wrote "A successful and rewarding workshop (see @math_jin feed; n.b. of course Ivan is an organizer) . Christelle Vincent (@xl772) has a number of tweets on the conference as well, see generally the hashtag #iutsummit . The tone of the feedback I have read appears generally, contra Peter's comment, to be positive and encouraging than at Oxford.

37. **Peter Woit**
July 28, 2016

e.ehrenweist,

I hope we'll get some serious public reports from experts who were there, now that the workshop is over. The twitter traffic made clear that that's not a good way of transmitting tricky arguments in arithmetic algebraic geometry. That things went better than Oxford (maybe due to Mochizuki himself playing a major role) is not inconsistent with the same problems still being there. Fesenko thought Oxford went great, so one might take that into consideration in reading his take on Kyoto.

Will likely write more about this once there's more serious information available.

38. **Peter Zbornik**
July 28, 2016

First report in by Nature: http://www.nature.com/news/monumental-proof-to-torment-mathematicians-for-years-to-come-1.20342

39. **Peter Zbornik**
July 28, 2016

Christelle Vincent sums up the "mood at the conference" in a series of replies to a tweet here:
https://twitter.com/maths1Bsummer16/status/758179530817220608

Ghostbusters

July 14, 2016
Categories: Film Reviews

Last night I went to a preview screening of the new Ghostbusters film. This isn't a review, all I'll say is that if you liked the first one, you'd probably like this one too.

In the first film, an early scene was set here at Columbia University, with Bill Murray an experimental psychology professor (you can watch it here). Academia doesn't come off too well… In the new film, again an early scene is set at Columbia, but now the protagonist is a theoretical physicist played by Kristen Wiig. She first appears in a lecture hall with a huge blackboard filled with equations relevant to GUTs and supergravity. For some explanation of how that came about from Lindley Winslow, who provided this and other advice, see here.

Theoretical physics comes off better in this version of the film than experimental psychology did in the first version. Academia is still made fun of though. The chair of the Columbia physics department is portrayed as telling Wiig's character that if she wants to get tenure she needs to do better than to have a letter from Princeton, since that department is well known to no longer be what it once was.

Update: For more about the physics background, see here. The Lindley Winslow piece doesn't mention that Janet Conrad took over from her when she had a baby, and it was Conrad's stuff that went into the Kristen Wiig character's office. I'd somehow missed that the bad guy had a string theory paper:

> Meanwhile, an antagonist named Rowan North got a string theory paper on Feynman ghost diagrams, which offered the opportunity for a little interdisciplinary ribbing. "Of course we made the woman a neutrino theorist and the bad guy a string theorist," Conrad says.

String theorists really do get no respect these days…

Update: More here, including

> Conrad made Wiig's character a neutrino physicist. She decided the bad guy would probably be into string theory. There's just something sinister about the theory's famous lack of verifiable predictions, Winslow says.
>
> String theorists can also be lovely people, though, Conrad says, and "I wanted to make as evil as possible." In the scientific paper she wrote for his desk, "he doesn't acknowledge anyone. He just says 'The author is supported by the Royal Society of Fellows,' and that's it."
>
> Also, she wrote for him "an evil letter where he's turning someone down for tenure."

Update: There's a profile here of Kate McKinnon, the most entertaining of the new Ghostbusters, emphasizing her interest in physics.

Comments

1. **Douglas Natelson**
   July 14, 2016

   I think you really wanted this clip for commentary about academia: https://www.youtube.com/watch?v=RjzC1Dgh17A

2. **Peter Woit**
   July 14, 2016

   Douglas Natelson,
   Thanks! I had forgotten about that part of the earlier film. Lovely sentiments and footage of the campus here (the new film has no such footage, just a quick exterior shot).

3. **Peter Woit**
   July 14, 2016

   For the new film, I don't think there was any actual filming here at Columbia, and in general most of the places supposedly in New York were actually locations in and around Boston.

4. **Mark Callaghan**
   July 15, 2016

   I liked the first one – but i was 15 at the time! , I used to find ' Carry on' films funny , but now they make me cringe. Thanks for the clip though – very funny.

5. **Low Math, Meekly Interacting**
   July 15, 2016

   I have no plans to ever see this movie.

   That said, I'm curious enough to ask: Are these amazing details something any moviegoer (expert or otherwise) is likely to be able to apprehend, or would it require one's nose be pressed against their 60″ flat-screen while their itchy trigger finger hovers over the Pause button?

6. **Peter Woit**
   July 15, 2016

   LMMI,
   A huge blackboard is the background for an early scene. No pause button, but I could recognize the standard graph of GUT coupling constant unfication, as well as some SUGRA equations.

   The scene in the office with Conrad's stuff lasts a little while, but I wasn't paying attention to the books or other things in the office.

   You need more of an experimentalist than me to recognize things in the lab scenes.

   As for the string theory paper mentioned, I didn't see that, quite possibly it ended up on the cutting room floor.

7. **Doug McDonald**
July 15, 2016

I'm a chemical physicist. I very strongly recommend the film Real Genius. It too has pertinent equations on the blackboard. The lab laser is a real one, and would actually burn holes in (very thin) ceramic plates. I'm told that a larger version of the same ideas has burned a hole through the Pacific Coast range (comment from a very very drunk ex-student in a position to know, and apparently too drunk to worry about classified information) and is admittedly now militarily operational.
And even better, the main professor protagonist is the very perfect image of a very real Professor of Electrical Engineering here at the time, who worked in the "Gaseous Electronics Lab" building (I'm not making up the building name.) This guy ran the exact same kind of jokes for real.

8. **Low Math, Meekly Interacting**
July 15, 2016

Thanks for the summary! Amazing the amount of work that must have gone into those minute details, and too bad some of them didn't make the final cut. Also somewhat ironic, given the premise.

9. **srp**
July 16, 2016

A second to the motion for Real Genius.

10. **String postdoc**
July 17, 2016

last night I saw ghostbusters with some string theory postdocs here at Princeton. Two of them are women and found the film hilarious. One remarked that Lubos has no place now that physics departments hire men based on looks. 😀😀

11. **Chris W.**
July 18, 2016

From what I've seen of Kate McKinnon's performances in other things, including SNL skits, she seems smart as hell and naturally inquisitive about a lot of things.

That said, it sounds like her interest in science and engineering was almost still-born. Maybe she should take a sabbatical from acting and comedy sometime and make a serious run at it, at least as a self-taught amateur. Her lack of a mathematical background will require a very serious commitment to work through, however.

12. **Andreas**
July 20, 2016

Hey Peter! There is the IUTT Mochizuki workshop going on since monday (https://www.maths.nottingham.ac.uk/personal/ibf/files/kyoto.iut.html). Did you hear some news from there yet? I can't find any sources – usually your blog is a pretty good one 🙂

13. **Peter Woit**
July 20, 2016

Andreas,
Yes, I know, see the previous posting. I have been hearing some news from there, will likely write something after its over. I do hope a well-informed take on the workshop will emerge from one or more of the participants, as happened with Brian Conrad's summary of the last one.

14. **Chris W.**
August 6, 2016

The New York Times has another profile of Kate McKinnon:

> **Did you consult with any real-life scientists?**
>
> I've been a big astrophysics nut since I was 12. I have always had a real soft spot for the bizarreness of quantum mechanics. But I gave up on being a scientist in high school — I'm just not that good at math. So that ticks me off, that I've limited myself and my life choices in that way. I will never be a theoretical physicist.

WIMPs on Death Row

July 21, 2016
Categories: Experimental HEP News

One of the main arguments given for the idea of supersymmetric extensions of the standard model has been what SUSY enthusiasts call the "WIMP Miracle" (WIMP=Weakly Interacting Massive Particle). This is the claim that such SUSY models include a stable very massive weakly interacting particle that could provide an explanation for dark matter.

According to the "WIMP Miracle", evidence for such a particle is supposed to show up as they get produced at the LHC, and at underground detectors designed to look for ones traveling through the earth. Like all other predicted SUSY particles, no evidence for such a thing has appeared at the LHC. A sequence of more and more sensitive underground experiments has also come up empty.

One of the latest of these, LUX, announced results today, see here, press release here. These are the results from the final 20 month run of the LUX detector and they are conclusively negative: no candidate events were seen, putting four times smaller bounds on the cross-section for any such particle. New Scientist has it right I think, with a story headlined Dark matter no-show puts favoured particles on death row. Ethan Siegel has a very good article, Dark Matter May Be Completely Invisible, Concludes World's Most Sensitive Search, which includes:

> The null detection is incredible, with a fantastic slew of implications:
>
> 1. Dark matter is most likely not made up, 100%, of the most commonly thought-of WIMP candidates.
> 2. It is highly unlikely that whatever dark matter is, in light of the LUX results, will be produced at the LHC.
> 3. And it is quite likely that dark matter lies outside of the standard mass range, either much lower (as with axions or sterile neutrinos) or much higher (as with WIMPzillas).

Enthusiasts are not likely to give up so easily though, with Sean Carroll tweeting that the news is only "we're not seeing it yet, stay tuned." Not sure what one is supposed to stay tuned to, this is pretty much a final result from LUX. There will be a next generation experiment, LZ, but that's for after 2020. There are other competing experiments now operating, including Xenon1T, now being commissioned, which will be somewhat more sensitive. There seems to be no serious reason though to expect WIMPs to appear at somewhat lower cross-sections if they haven't appeared yet.

With SUSY and the "WIMP miracle" now dead ideas, perhaps that will lead to focus on more promising ones. There is still a great deal that we don't understand about neutrinos. A few days ago I saw this intriguing news about the PTOLEMY project, which I hadn't heard about before.

Comments

1. **M**
July 21, 2016

   "WIMPs on Death Row" is hype, altought negative. What gets more in trouble is WIMP interacting with nucleons at tree-level and in s-wave. WIMP that interacts through electroweak loops is ok. WIMP with a p-wave non-relativistic suppression is ok.

2. **Peter Woit**
July 21, 2016

   M,
Thanks. But is there any motivation for such models, at a level visible with another order of magnitude sensitivity, or would such things always be likely invisible to this kind of experiment?

3. **Douglas Natelson**
July 21, 2016

   I will ask this because I'm too lazy to read extensively to figure out the answer: Is the LUX result directly incompatible w/ the DAMA claims?

4. **Peter Woit**
July 21, 2016

   Douglas Natelson,

   Even the earlier LUX results were incompatible with DAMA, see for instance
http://arxiv.org/abs/1406.5200

5. **Jeffrey M**
July 21, 2016

   OK, I'm confused. DAMA observed changes in dark matter flux based on the earth's orbit, right? But LUX says no such thing could have happened? Or only that it couldn't be WIMP's?

6. **Peter Woit**
July 21, 2016

   Jeffrey M,
If you believe DAMA's interpretation of the periodic effect in their data as due to a WIMP, that implies a WIMP cross-section (probability of interacting with a nucleon) so large that LUX should be seeing a sizable signal. In this sense, LUX rules out the idea that DAMA is seeing WIMPs.

7. **Marc**
July 21, 2016

   Peter–Often the DM experimental results show, in the plot of cross-section vs mass, a large gray area in the lower right which corresponds to the predictions of typical SUSY models. While this experiment takes out a big chunk of it, it only looks like about 1/2 of the gray area. So doesn't Death Row seem a tad bit premature?

8. **Peter Woit**
July 21, 2016

Marc,
The problem is that the meaning of "typical SUSY model" and the associated grey blobs are ill-defined and move as the experiments get more sensitive. For example, a few minutes research turned up some 2001 SUSY "predictions", see
http://arxiv.org/abs/hep-ph/0106148
where the conclusion is
"When CDMS is moved to the Soudan mine, its sensitivity will drop to between 10^-8 and 10^-7 pb and GENIUS claims to be able to reach 10^-9. At those levels, direct detection experiments will either discover supersymmetric dark matter or impose serious constraints on supersymmetric models.

The LUX result is below 10^{-9} pb over a very large range, down to 2.2 x10^-10 at 50 GeV. So, it is very much "imposing serious constraints" on SUSY, but of course there will always be SUSY models with smaller cross sections. I don't think there's a sensible way to put a measure on such things.

9. **Jeffrey M**
July 21, 2016

Thanks Peter. So DAMA was explicitly claiming WIMPS, which LUX rules out. Got it. Should we expect a surge of papers explaining DAMA in some different way?

10. **Peter Woit**
July 21, 2016

Jeffrey M,
The interpretation of the DAMA signal as being due to dark matter was already ruled out by earlier experiments, including early data from LUX. So, there's nothing new about that here. I haven't followed the latest in attempts to explain the DAMA signal, but, since it is just an annual modulation, one can imagine all sorts of non-fundamental sources due to a subtle seasonal variation in the environment of the experiment.

11. **M**
July 22, 2016

One example of an alive&healthy WIMP is (in SUSY langauge) an umixed Wino. Or, more generically, a weak triplet with zero hypercharge. It interacts only with heavy electroweak vectors, giving a loop-level direct detection cross section below present bounds and a little above the neutrino background.

12. **Tom Andersen**
July 22, 2016

Peter,

Theorists should not diminish experimental results they don't understand, based solely on the fact that they can't predict them.

Don't you think that the DAMA team has used various techniques to improve any seasonal environment variations? Despite all efforts at this the signal amplitude remains the same. Is it a WIMP dark matter signal? No. Is it real? Looks likely.

There are now a few direct reproductions of the DAMA experiment. Let the experimenters decide this one.

13. **Peter Woit**
July 22, 2016

Tom Andersen,

You're reading things into what I wrote that aren't there. Of course I'm aware that the DAMA people (and others) have done their best to identify environmental sources of the signal. They are seeing something, but my understanding is that the results from other experiments show that it's not the hoped for WIMP signal, and that currently no one knows what it is. Given this, an environmental source remains a possibility, as well as something much more interesting. I have no idea what relative probabilities to assign to those two alternatives, was not implying anything about that issue at all.

14. **Chris Austin**
July 22, 2016

M,

"One example of an alive&healthy WIMP is (in SUSY langauge) an umixed Wino. Or, more generically, a weak triplet with zero hypercharge."

Do you mean just the electrically neutral component of the triplet?

15. **Peter Woit**
July 22, 2016

Chris Austin,
If it's not electrically neutral, it's not a WIMP…

16. **Bernhard**
July 22, 2016

If not DM directly, there is still hope to find evidence on the existence of other stuff from the dark sector at the LHC (like dark photons).

17. **piscator**
July 22, 2016

Tom: talk off-line to experimenters involved in other WIMP DM searches and ask them their opinion of DAMA's claims. It is not just theorists who are sceptical about DAMA's results.

18. **paddy**
July 22, 2016

Peter,
My reading of Chris Austin's comment is that he was pointing out exactly what you said. Perhaps he could phrased it better: "Do[n't] you mean just the electrically neutral component of the triplet?"

19. **M**
July 23, 2016

Chris, yes. Its direct detection cross section is ≈ 0.3 10^-46 cm^2

20. **tom joad**
July 23, 2016

Interesting news, but in what sense are SUSY or WIMP DM dead ideas? There exists parameter space that wasn't excluded by any experiments. They are alive.

I suppose you mean that they're less plausible in light of LUX latest results? Can you elaborate on your reasoning? In the past you dismissed inductive (Bayesian) reasoning in science, but now you suggest that the plausibility of particular models has changed in light of data.

Have you changed your mind? If so, very good. If not, OK, but don't argue that the status of SUSY DM or WIMP was affected – it has the same status as before, unfalsified.

21. **Yatima**
July 23, 2016

it has the same status as before, unfalsified

The correct adjective here is (still) "unjustified".

That being said here is report on the possibility that Dark Matter is mainly formed by a large population of primordial black holes (thus, a galactic halo of black hole "gas"). I am not sure whether this fits the (AFAIK) sparse observations of lensing events.

22. **Peter Woit**
July 23, 2016

tom joad,
The last thing in the world I want to get into is a debate over Bayesian reasoning, but I'll point out that my criticisms and mocking of it had to do with its misuse (things like Polchinski's calculation of probability of a multiverse), not with its use to characterize the significance of experimental results.

Obviously SUSY is unfalsifiable due to its huge parameter space,, always has been, always will be, and John Ellis I think has stated that he'll never abandon it. Equally obviously the "WIMP miracle" is not working out, and in that sense the idea is on its way to the graveyard of failed ideas.

23. **tom joad**
July 23, 2016

Fair enough, that makes sense to me. These DM results are pretty cool, and I enjoy reading your blog. Enjoy the weekend.

24. **DP**
July 23, 2016

Hi Peter, you might be interested in the new announcement from the PandaX Dark Matter Experiment.

25. **Andreas**
July 24, 2016

Hey Peter! Thanks for this news, i haven't seen it yet. Concerning dark matter, what did you think about Adam Riess' recent investigation:
http://journals.aps.org/prl/abstract/10.1103/PhysRevLett.116.201301 ?

26. **Peter Woit**
July 24, 2016

Andreas,
Sorry, but you really need an astronomer to evaluate that idea, not me.

DP,
Thanks! I took a quick look at
http://pandax.physics.sjtu.edu.cn/files/pandax_ii_idm.pdf
and their new result seems to be quite similar to the LUX result (best limit 2.7 x 10^{-10}pb at 40 GeV, vs 2.2 at 50 GeV). Not sure why LUX is getting all the atttention. Perhaps combining the two results one might get even more stringent exclusion limits.

27. **MikeS**
July 25, 2016

Until we have some better explanation of Dark Matter, WIMPs of some form must remain a contender. We either have to establish a theory that Dark Matter does not exist, or we have to find where it is hiding. And in the case of the latter, there are precious few theories.

I suspect it is simply the hitching of WIMPs to the SUSY bandwagon, and the rather circular logic whereby one seems to promote the probability of the other that people object to.

28. **Doug McDonald**
July 25, 2016

There are no pointers to papers above, and a quick search didn't find what I want.
Question from an experimentalist: I read the Panda Powerpoint. It quotes a
maximum cross section versus energy.

BUT … such an experiment measures the PRODUCT of a cross section times a flux.

There is no measurement of a flux! They must be assuming something … is it something astronomical?

29. **Syksy Räsänen**
July 25, 2016

Doug McDonald:

The experiments measure events per time in each energy bin. A simple discussion of WIMP direct detection can be found on pages 116-117 of my lecture notes.

http://www.courses.physics.helsinki.fi/teor/cos1/cosmo2015_07.pdf

30. **Tim**
July 26, 2016

Doug,

The "WIMP Miracle" mentioned in the post is that if you assume a certain cross section and mass that are compatible with a particle that interacts via interactions at the "weak" scale, then the standard model + big bang nucleosynthesis produces it in the right amounts and with the right energies to explain both the amount and spatial distribution of observed dark matter. You can calculate the expected flux from that.

Once the expected flux * expected cross section is not observed, it's still possible that dark matter exists with other smaller values, but the connection to a particle (potentially supersymmetric) interacting at the weak scale is lost.

So there you go. The assumptions come from theoretical predictions of formerly popular models that seemed to explain our dark matter observations quite nicely. Unfortunately this + LHC bounds now suggest that the right answer now seems to be something other than a weak scale lightest supersymmetric particle.

31. **GoletaBeach**
July 26, 2016

Peter, wow. Just within the realm of SUSY models, plenty, plenty of models predict WIMPs with cross sections lower than LUX, XENON1T, or even LZ sensitivity.

Some questions only have empirical answers, and thank goodness that particle experimenters have traditionally only lightly paid attention to theorists' hype.

There was tremendous disrespect for Ray Davis and his neutrino experiment from theorists… I remember it well. There was disrespect for all the many, many searches for neutrino mass/mixing, because, there was no good theoretical prediction for delta-m-squareds. Or the mixing angles. Thank goodness experimentalists soldier on.

I even remember when the weak interaction was agreed by all theorists, Fermi too (who died in 1954) to be S+T. It turned out to be V-A with no theoretical prediction of that, although Lee and Yang eventually followed up on Ernest Lawrence's admonition, `Who the he** cares about parity violation, this is experiment!' in response to the tau-theta puzzle.

Declaring the `WIMP miracle' dead is so obviously premature that you have just joined the scammers and hype-meisters that you often criticize. I guess you are jealous of them, that their scamming gets publicity and your doesn't.

32. **Chris Kennedy**
July 26, 2016

So how could neutrino mass be a possible explanation for dark matter? Seems to me that since galaxies come in a variety of baryonic mass distributions from center to edge, the neutrino mass distribution would always (and conveniently) have to be such that it is arranged to produce near-flat rotation curves for all varieties of galaxy. In my opinion, the odds of that are off the chart.

33. **Peter Woit**
July 26, 2016

Goleta Beach,
No disrespect to these experiments intended. They're doing an impressive job of repeatedly pushing down limits by large factors. It's great that people are doing them, they're looking somewhere no one has ever looked, and if they find something it will revolutionize physics. Yes, such experiments should be done, no matter what theorists say about the likelihood of them seeing something.

That said, I still think it's important to admit that these huge improvements on the limits carry implications for certain theoretical ideas. I just googled for more info about the GENIUS proposal that Ellis et al. mention. The 1998 proposal, see
http://arxiv.org/abs/nucl-ex/9801004
states in the abstract:
"The entire MSSM parameter space for prediction of neutralinos as dark matter particles could be covered already in a first step of the full experiment ". If I'm reading figure 3 properly, the entire region of points plotted as such "predictions" is now ruled out by LUX. For twenty years I've been seeing plots of this kind and claims of various kinds that SUSY dark matter models imply a WIMP in this region. Now that such things are ruled out, acknowledgement of previous "predictions" that have been falsified would be a good idea, before moving on to new models and new "predictions".

The "miracle" in the SUSY "WIMP miracle" was always hype, but one reason it got attention was that it appeared to make a specific claim that something would be seen at the LHC and in experiments with sensitivity like that of LUX. Sure, of course WIMP dark matter isn't dead, maybe it's there at lower cross-sections. But it seems to me the "miracle" is dead, and it should be given a proper public burial.

Chris Kennedy,

My understanding is that right-handed (sterile) neutrino models can readily provide dark matter.

34. **GoletaBeach**
July 26, 2016

The `miracle' was that weak interaction order-of-magnitude cross sections + thermal equilibrium in the early universe easily accommodate the observed dark matter density… noticed by Ben Lee and Steven Weinberg in the 1970's. Has not a thing to do with SUSY, and lasting interest in the WIMP being a right-handed neutrino (or left-handed antineutrinos) is traceable to that `miracle'. Maybe couplings to the right-handed sector were appreciable in the weak interaction in the early universe.

Your opinion, if based on anything related to SUSY, won't influence any serious scientists' opinion about the WIMP miracle. Maybe you'll help grow some anti-WIMP feelings among popular science hobbyists, though.

As for the SUSY connection, there is and will always be hope that SUSY is related to the weak interaction, and so that is the connection that seems to mainly be in your crosshairs.

Referencing H.V. Klapdor-Kleingrothaus doesn't raise the level of the discussion. He discovered neutrinoless double beta decay, although nobody else agrees with his discovery.

Perhaps take a look at https://arxiv.org/abs/1604.07336 . Figures 2 and 5, and, the paper seems to indicate that scalar and gaugino masses were only scanned up to 2 TeV. It may be that scanning to 100 TeV or more yields a lot more low-cross section neutralino likelihood. Those plots are for the fewest free parameter CMSSM.

The 2 TeV limits? Motivated by only scanning where the LHC can see stuff. It is entirely possible that SUSY is there, unobservable a the LHC, but detectable by direct detection experiments. Or, detectable only by indirect detection (Veritas, CTA, Pingu, etc).

More parameters than the CMSSM? No problem, figure 3 of https://arxiv.org/abs/1503.00599, in the 9-parameter MSSM.

Of course we all want to see revisions after Run-2 LHC results are presented.

35. **Peter Woit**
July 26, 2016

GoletaBeach,
Maybe this is my lack of education, but all the public discussions of the term "WIMP miracle" that I remember seeing claim as a big part of the "miracle" the LSP in SUSY, I had assumed that when people referred to the "miracle", they had in mind the "miracle" of SUSY predicting a weak-scale stable particle. For an example of what I have in mind, see
http://www.symmetrymagazine.org/article/july-2015/miraculous-wimps

"When researchers use the properties of the lightest supersymmetric particle to calculate how many of them would still be around today, they end up with a number that matches closely the amount of dark matter experimentally observed—a link referred to as the "WIMP miracle.""

I'm curious if you know who first used this term, did Ben Lee or Steve Weinberg call it that way back when? I always assumed it was part of the SUSY/String theory hype (who else calls a very rough coincidence good only within several orders of magnitude a "miracle"?)

I don't doubt that there's plenty of room in SUSY parameter space and that you can find recent plots with susy models "predicting" some region to the lower right of the latest exclusion. What I'm objecting to is ignoring all the similar plots I've seen over the years, put forward by people promoting SUSY. If you Google "wimp miracle", the first two things that turn up are
https://en.wikipedia.org/wiki/Weakly_interacting_massive_particles
which has a 2004 plot of the area SUSY is supposed to show up at that is now disallowed, and then this from 2009
http://www.ps.uci.edu/~jlf/research/presentations/0912upenn.pdf
which (page 9) has a plot claiming a "universal prediction" of a 10^{-44} cm^2 cross-section (also now completely disallowed).

As for what this does to the public perception of science, I don't think 20 years of heavily hyped "predictions" based on a "miracle", followed by "oh, just forget those, we've got new ones" when they're conclusively falsified is a healthy situation.

Theorists and experimentalists who want to communicate with the public about the implications of these results (together with those from the LHC) for theory it seems to me should tell a straightforward and accurate story: experimental results have not shown what was expected, so these ideas are pretty much dead. This is what scientific progress looks like. Yes, there are always more models, and very good reasons to keep looking, but the general ideas behind SUSY extensions of the SM are no longer a particularly plausible motivation for anything. Abandoning these and moving on to other more promising ideas (let's hear more about right-handed neutrinos, less about neutralinos) would be a good idea.

36. **GoletaBeach**
July 26, 2016

I'm not an etymologist… appears that the earliest (1985) reference to WIMP, has no mention I see of SUSY…
http://ac.els-cdn.com/0550321385905371/1-s2.0-0550321385905371-main.pdf?_tid=4ae66064-538d-11e6-a255-00000aab0f26&acdnat=1469577879_0f1061aceb68f51962bd19520d599e70

And earliest WIMP Miracle (2007, possibly from Feng alone) is on the first 4 slides of:
http://theory.fnal.gov/jetp/talks/feng.pdf

Note that the SUSY stuff in that 2007 talk is *after *the WIMP miracle.

The logic is (thermal equilibrium) + (weak interaction) = WIMP miracle.

SUSY only chases that ambulance. If some people get it wrong, that is their mistake.

As experimental data accumulates, simple scientific principles require any well-defined phenomenology to adapt to respect the new experimental data. And so it goes with SUSY phenomenology… the very simplest SUSY phenomenology, the CMSSM, still has plenty of room for an LSP that is the WIMP, even after all known data (through LHC run 1) is accommodated. But even if someday experimenters doing good science eliminate all viable CMSSM parameter space, all they will do is prove the CMSSM LSP is not the WIMP. The WIMP miracle will remain untouched.

Of course the MSSM has less simple parameterizations than the CMSSM. As I pointed out, in those less simple parameterizations, there is even more room for an LSP that is the WIMP.

The above is just: SCIENCE!!! SUSY has loads of parameters. Ruling it out is a lot of work. Just as proving global warming was man-driven was a lot of work. Welcome to real science.

The LUX results are entirely consistent with what is expected under both SUSY and the separate and distinct WIMP miracle. There is no "conclusively falsified" anything here.

In the talk you link, and the plot you refer to, Baltz and Gondolo's predictions went off the bottom of the plot. Feng took a specific category of models and pointed at 10^(-44) cm^2. Obviously other models weren't covered by his pointing. And Feng ain't a consensus Snowmass or NAS report. If you don't like Feng's hype, take it up with him. Tainting a whole field with one (possible) hypester is not good. And Wikipedia? Seriously?

Real science is a complicated, messy thing. It clearly frustrates you that simple stories you have decided are true are not. What frustrates me is that good experimental science effort is likely to be impeded by your cartoonish simplifications.

37. **Peter Woit**
July 26, 2016

GoletaBeach,
Interesting that the terminology is that recent, I had no idea. I would have thought it went back to SUSY enthusiasts of long ago. Whoever it's due to though, I think it's a bad idea, misleading hype. Would be better to find another one.

While doing a bit of looking at old papers/slides to see where the "miracle" came from, just about every one of them has a plot of susy predictions that are now conclusively falsified (often the range of the LUX results isn't even plotted). This isn't just Feng.

Look, experimentalists should do what they can do: design and build the best experiments to look where no one has looked before. I don't see why they should get into questions about complicated BSM models that have no good motivation. Theorists should be the ones evaluating such models, and they are the ones I'm claiming need to look at the long, sorry tale of SUSY extensions of the SM, draw the conclusion it's no longer a promising idea, and not continue down the rabbit hole of promoting ever less motivated corners of SUSY parameter space.

I'm well aware this is a complicated story (actually I wrote a whole chapter in my book long ago about SUSY, precisely because it is a complicated subject). I should have realized that I didn't write about the "miracle", because it wasn't called such back when I was writing (2002-3). But, despite all the complexities, there are now a lot of very good reasons to call SUSY extensions of the SM a failed idea. I wrote about a bunch back in 2002-3, the LHC has given us a bunch more, and so have experiments like LUX. No, you can't ever completely rule out something like the SUSY framework, but you can get to the point where its original motivations haven't worked out and it has become highly implausible. We're there now.

38. **GoletaBeach**
July 27, 2016

The original (thermal equilibrium) + (weak interaction) = currently observed relic density is very compelling, and independent of SUSY. That is the `hype' that most serious speakers point to. Feng named it the WIMP miracle. Weinberg and Lee pointed it out in the 1970's, and in the 1980's that connection lead to the first WIMP searches… when the big idea was to test whether the WIMP had V couplings like heavy Dirac neutrinos… in the 1980's that was ruled out. No-one called it the WIMP miracle but everybody knew what was going on. SUSY didn't matter at that time, although the attraction of the LSP (then called the photino) being the WIMP started to be discussed.

Only recently has WIMP sensitivity of experiments achieved the expectation for A couplings like Majorana neutrinos. People in the thick of things know this, you don't, you keep projecting SUSY BS onto the field.

And guess what… long before you were around the usual experimental introduction to anything w/r to SUSY was… `There is no experimental evidence that supports SUSY. But SUSY has withstood the test of time.' It is sardonic.

That SUSY might be restored at a mass scale a bit higher than accelerator folks want is not important for a possible LSP/WIMP connection. The cancellations that SUSY provides to regulate infinities work just fine even if a 500 TeV collider would see absolutely no evidence of SUSY, so the original motivation for SUSY is intact.

39. **Peter Woit**
July 27, 2016

GoletaBeach,
Thanks, I'd love to hear more about the possible role of Majorana neutrinos here. I think those working in this area would do well to discuss such topics when describing publicly motivation for these experiments, rather than trying to emphasize SUSY motivations that have not worked out.

40. **GoletaBeach**
July 27, 2016

Most experienced experimentalists are fairly ecumenical about SUSY. It is another good reason to look for stuff, but not the only reason. Phenomenological SUSY provides serious numerical modeling and serious model building. Lots of phenomenologists work hard on scanning its super-big range of parameters. That is all good science…

Sure, people oversell SUSY. One of those people is you, who want to portray the testing of SUSY as a straight up and down test, like the Michaelson Morley experiment. It is not.

It is perfectly possible that a peculiar corner of SUSY parameter space out of reach of LHC or the Great Collider or Veritas or CTA or LZ is what Nature chose. If eventually some experiment 500 years from now figures that out, I'm sure theorists 500 years from now will say that Nature's solution is beautiful and natural.

But that is not a serious argument to cease experimentation and returning to theological debates over naturalness or the anthropic principle or the nature of transubstantiation.
Doing the best experiments we can afford remains a better use of our time. Sometimes we do the wrong experiments and sometimes indelicate hype gets used to justify the correct experiment. I wish we were all perfect but we are not.

41. **GoletaBeach**
July 27, 2016

A/Majorana… something published is Fig. 1 in:
https://arxiv.org/pdf/astro-ph/0509269v2.pdf shows where the Majorana line was.

Latest LUX among others blew through that line over much of the mass range
https://arxiv.org/pdf/1602.03489v3.pdf

42. **Peter Woit**
July 27, 2016

GoletaBeach,

You're missing the context of what I write about SUSY here. I'm very aware of the complexities, have been following the story since the very early 80s (when I was a graduate student at Princeton, Witten was telling students they should learn about supersymmetry, Julius Wess was a visitor, giving a series of lectures that ended up as the book with John Bagger). When I was a postdoc at Stony Brook during the mid 80s, SUSY/supergravity was something hard to ignore.

From the beginning though, I found that SUSY didn't provide any convincing explanations about the SM. Over the years it has become a hugely oversold subject, partly by making claims (e.g. "SUSY solves the hierarchy problem, so superpartners will be seen at LEP, Tevatron, LHC, etc..") that have always failed. My impression of the way most theorists felt about SUSY a few years ago was increasing skepticism, with the LHC results to come the final and last hope. Now that those have come in, I think people need to draw the appropriate conclusions, not go on for the rest of all our lives saying "still a great idea, just have to go to higher masses"

So, sure, you're going to see me keep arguing that new LHC results are nails in the coffin (I think we'll see more in a week or two at ICHEP) of a bad idea that never worked. The long sad story of how we got to this point though is too complicated to keep retelling.

43. **GoletaBeach**
July 27, 2016

Well, the very high energy cancellations above the SUSY restoration scale are the bedrock prediction of the SUSY endeavor, and Nature doesn't check in with any of us as to where to place that restoration scale. Nature also doesn't care about the LHC or experimental budgets.

Burt Richter has always been right: theoretical physics is by and large a sociological activity, only a little different than theology or philosophy. You are making sociological observations. Only when incontrovertible data from experimentalists or observers test the theoretical guesses do theorists take notice, and often theorists find one of N wild papers they published and assert that they predicted it all. We're going through an episode with Gordon Kane etc and everyone who went out on a limb about SUSY at the LHC. Oh well, the human drama.

As to your experience in the 1980s, well, you move in theoretical circles. You got sucked into the groupthink. Experimentalists were never anything but skeptics about SUSY… poor Mohammad Mohammadi and his SUSY discovery in the 1980's at UA1.

Experiment also has sociological aspects, of course, and the drive to higher energy colliders has by and large been based on planning with a rear view mirror. Higher energies have worked before, so why not try again? There are of course other ideas… WIMP searches and indirect WIMP searches among them.

SUSY will always remain a reasonable motivation for all of them, forever.

As a field (including astrophysics) we've been interrogating dark matter since Opik in 1915 or so. Again, Nature doesn't give a darn about human life scales. Secrets will be revealed when they are. Sociological sniping based on impatience is irrelevant.

As to nails in the coffin: it is a very large coffin and your nails don't begin to cover its perimeters. You're free to get bored with it, of course, but others may be more durable than you.

And on several occasions the search for neutrino mass/mixing was declared worthless and `over' too. Thank goodness solid scientists carried on.

44. **Chris Kennedy**
July 28, 2016

I had this friend who called me a few months ago and said: "I ran some calculations and I'm pretty sure I have a ghost in my house." Of course I thought he was kidding but he continued: "In exactly two weeks, I'm going to start hearing noises, and several items are going to get knocked onto the floor when no one is in the room." So I went over to his house, we did a stakeout and we never heard a sound. He then said: "Well next week for sure" and this went on for three more occasions after that.

Even though he was the one who gave me the dates and times of all of the predicted supernatural "events" he then started saying things like: "Look, it's a ghost, it's going to come out when it feels like it – it's certainly not obligated to our timetable." I then asked him: "Did you ever consider the possibility that your house is not haunted after all?"

45. **Low Math, Meekly Interacting**
July 28, 2016

Forgive the naïve outsider, but the story of the search for neutrino masses doesn't seem to me to be remotely like the story of SUSY. When you have a particle that demonstrably exists, a third of those particles emitted by the Sun are completely missing, there's no particularly deep reason for it to be massless, and mass solves the missing neutrino problem, you've got some serious "motivation". SUSY solves naturalness problems that might not even be problems, at the cost of an extravagant number of new particles that we've never seen, and which can remain forever hidden by breaking the symmetry at whatever scale remains conveniently unprobed. Where is the analogy?

46. **Tim**
July 28, 2016

I don't know that a doubling is extravagant. In fact I'd personally say it's more likely than a handful of new particles. Nature just sort of likes to do that.

Remember that antimatter doubled the number of known particles; generations tripled them for no obvious reason. Any broken symmetry is inevitably going to result in "… and this is the other half of the universe you didn't previously know about."

47. **paddy**
July 28, 2016

I can't be the only who remembers Glashow's answer to the likes of Tim's argument. To paraphrase: SUSY must be right…after all we have already found half the particles it predicts.

48. **Peter Woit**
July 28, 2016

Tim,
What's really suspicious here is that the theory says "all particles" will come in pairs of a specific kind, but if you look at the complicated structure of the many known particles, there are no pairs this kind. Not one. Another way of saying this is that the theory claims a new symmetry of nature. Symmetries tell us something by relating different states. SUSY provides no relations between any known states, it acts trivially on the space of known states (supposedly taking them to a different space).

And, unlike antimatter, introducing SUSY breaking to make the partners invisible means you're not just postulating a doubling of the state space, with a Z2 symmetry, you're also adding in a huge extra number of random parameters to deal with the SUSY breaking.

49. **GoletaBeach**
August 1, 2016

to Chris Kennedy… the only technique that guarantees certainty is to not look for new physics. They you are absolutely sure of the result… that nothing new will ever be discovered. If you want something new, there will always be risk of coming up emptyhanded.

to Low Math… the parameter space that describes possible neutrino masses and mixing angles is vast. No theorist had any prediction of which parameters Nature would pick. The solar neutrino experiment didn't start as a neutrino oscillation experiment, it started because a clever experimentalist devised a technique with sufficient sensitivity to observe solar neutrinos. Most of the theoretical community didn't support the experiment, and when there was a deficit, most of the theoretical community thought that poor solar models, not neutrino oscillation, was the explanation. A similar sequence played out with atmospheric neutrino oscillations. The heart of the matter is… experimentalists are generally at least as good and usually better at finding new physics than theorists. A good reason to keep doing WIMP and other dark matter experiments.

To Tim: yes, the tripling into three generations is an entirely unpredicted phenomenon, little considered. It often feels like there is a pattern of excitations underlying that one, but no substructure theories have ever been devised that help describe it. The usual contemporary attitude is that quantum field theory masks any underlying pattern so completely that it is pointless to look for a pattern. A good argument to complete the one pattern that is incomplete… the neutrino mass template. Maybe an interesting pattern will show up.

50. **Low Math, Meekly Interacting**
August 1, 2016

Hi, Goleta Beach,

Thanks for your reply. I'm far from expert in the matter, nor am I as knowledgeable as you about the history, by my understanding is that the first experiments back in the 1960s were designed to observed solar neutrinos because that's an interesting thing to do. Beyond the related nuclear physics, theory didn't really need to come into it. but they found the discrepancy (should have said 2/3, not 1/3 of the electron neutrinos missing), and that got the ball rolling in the right direction in short order. Of course people first suspected an astrophysical explanation. My understanding is that those speculations were dispatched without much difficulty, perhaps ironically by the temperature of solar neutrinos, which, among other results, showed that you couldn't explain away the deficit through poor solar modeling. Reactor experiments, etc. gave a pretty tight upper bound on neutrino masses, the lower bound was assumed, so by the time people were looking seriously at neutrino mass, there were already very tight constraints.

Again, an experimentally verified particle, an experimentally verified and sizable deviation from theory, experimental elimination of the alternatives, and fairly inevitable experimental validation of the best explanation for the discrepancy. Which, perhaps to no surprise, has fostered advances in the field of neutrino physics that may provide the best hope for BSM theorists.

SUSY, rather, looks to me like the theoretical tail wagging the dog to death, and then continuing to wag the mutilated corpse in hopes flailing will resuscitate it. There's no experimentally-validated anything to show unequivocally that it's necessary, just a lack of understanding of the hierarchy problem. Which, as many smarter people than me continue to point out, we can't say for certain is even a problem. And there's no upper bound on the scale. There likely would have come a time when we could have said that the neutrino mass is so small that, if it exists, it couldn't explain the solar neutrino problem. No such luck with SUSY. It's already failed the "naturalness" test, LSP WIMPs are looking ever more implausible, etc., etc. It can live comfortably forever in the anthropic multiverse, which great for SUSY, I suppose, but hopeless scientifically. I just don't see any comparison to neutrino masses at all.

51. **GoletaBeach**
August 5, 2016

To Low Math, well, not quite.

SUSY has a vast parameter space of possibilities that all are, (IMO) "Natural". Naturalness is a completely subjective judgement, and I have no doubt that should SUSY show up experimentally *anywhere*, theorists will eventually proclaim the situation "Natural". What else can they do? Call *Nature* un-*Natural*??

The space of possible neutrino parameters was and is also vast. That *any* experiment happened on the true neutrino parameters is a minor miracle.

That is the analogy: 2 vast parameter space with really no guidance as to where nature is hiding.

The resistance to the Ray Davis Homestake result was much, much stiffer than you portray. By far the strongest resistance was: people thought his experiment was just flat wrong. It was a radiochemical experiment more akin to Rutherford & Soddy than to 1960's particle physics. The experimental community were just utterly skeptical… thought the `missing neutrinos' were just a bad calibration. And then lots of theorists referred back to the vast parameter space, and shrugged. A few took Davis seriously…. John Bahcall… then the MSW effect showed up in the 1980's, and Davis got more respect. Turns out the MSW effect is not the origin of the Davis effect, but, still, that MSW can stimulate neutrino transitions really turned peoples heads at the time, and helped stimulate new experiments that verified Davis.

There is an excellent, excellent argument for SUSY… stabilizing quantum field theory at very high energies. How high do you need to go to see SUSY restored? Anybody's guess. There will never really be a serious argument that any scale is `natural' until we have empirically discovered it.

Sure, we all wish that a very clever $10,000 experiment will discover SUSY in some incredible series of unlikely events. Believe me, the experimental community probes ideas like that all the friggin' time. And lots have something to do with Dark Matter. No matter what, though, WIMPs ain't dead. They don't need SUSY to still be right.

Quantum Theory and Representation Theory, the Book

July 26, 2016
Categories: Quantum Theory: The Book

For the last few years most of my time has been spent working on writing a textbook, with the current title Quantum Theory, Groups and Representations: An Introduction. The book is based on a year-long course that I've taught twice, based on the concept of starting out assuming little but calculus and linear algebra, and developing simultaneously basic ideas about quantum mechanics and representation theory. The first half of the course stuck to basic non-relativistic quantum mechanics, while the second introduced free quantum field theories and the relativistic case. By the end, the idea is to bring the reader to the point of having some appreciation of the main elements of the Standard Model, from a perspective emphasizing the representation theory structures that appear.

The discussion of quantum field theory I think is rather different than that of other textbooks, taking a Hamiltonian point of view, rather than the Lagrangian/path integral one in which most physicists are now trained (myself included). One basic idea was to try and work out very carefully the quantization of a finite-dimensional phase space in all its representation-theoretic glory, with the idea that free quantum field theories could then be developed as a straightforward extension to the case of taking solutions of a field equation as phase space. While this point of view on quantum field theory is fairly well-known, writing up the details turned out to be a lot more challenging than I expected.

As part of this, the book attempts to carefully distinguish mathematical objects that usually get identified by physicist's calculational methods. In particular, phase space and its dual space are distinguished, and the role of complex numbers and complexification of real vector spaces receives a lot of attention.

At the same time, the book is based on a relatively simple philosophical take on what the fundamental structures are and how they are grounded in representation theory. From this point of view, free relativistic quantum field theories are based on starting with an identification of irreducible representations of the space-time symmetry group using the Casimir operator to get a wave-equation. This provides a single-particle theory, with the quantum field theory then appearing as its "second quantization", which is a metaplectic (bosonic case) or spinor (fermionic case) representation. These are some of the specifics behind the grandiose point of view on how mathematics and physics are related that I described here. For some indications of further ideas needed to capture other aspects of the Standard Model, there's this that I wrote long ago, but which now seems to me hopelessly naive, in need of a complete rethinking in light of much of what I've learned since then.

The manuscript is still not quite finished, and comments are extremely welcome. While several people have already been very helpful with this, few have been willing to face the latter chapters, which I fear are quite challenging and in need of advice about how to make them less so. The current state of things is that what remains to be done is

- A bit more work on the last chapters.
- A rereading from the start, bringing earlier chapters in line with choices that I made later, and addressing a long list of comments that a few people have given me.
- An old list of problems (see here) needs to be edited, with more problems added.
- I need to find someone to make professional drawings (there are some funds for this).
- An index is needed.

Optimistically I'm hoping to have this mostly done by the end of the summer. Springer will be publishing the book (my contract with them specifies that I can make a draft version of the book freely available on my web-site), and I assume it will appear next year. To be honest, I'm getting very tired of this project, and looking forward to pursuing new ideas and thinking about something different this fall.

Comments

1. **tulpoeid**
   July 26, 2016

   Impressive. Congratulations and may you pursue new ideas very soon 🙂

2. **Marcus**
   July 27, 2016

   Hi Peter,

   The content of your books look really good. Congrats for (almost) finishing!

   I'm interested in what you think is the unique difference of your book to existing books that seem to cover similar ground (e.g. Zee's new Group Theory book, Schwichtenberg's Physics from Symmetry or Robinson's Symmetry and the Standard Model Reference)? At a first glance I would guess maybe mathematical rigor?

   Best wishes,
   Marcus

3. **Asaint**
   July 27, 2016

   Any disclosures as to what those "new ideas" are?

4. **StrangeRep**
   July 27, 2016

   Hi Peter,

   1) On p240 you seem to say that Kepler's 3rd law is associated with the LRL vector. I believe this is not correct. Rather, the 3rd law arises from a dilation-like symmetry. In coordinate form, it is t -> kt, q -> k^{2/3} q. See section 10.2 of Stephani, i.e.,

   H. Stephani, "Differential Equations…", CUP 1989C, ISBN 0-521-36689-5

   (I find this extra dilation-like symmetry interesting because it is not associated with a conserved quantity. Not sure how/whether it's relevant in the quantized version.)

   2) What units are you using in the Hamiltonian and LRL vector on p240? Your expressions seem dimensionally invalid, at least superficially.

   3) Your expression W on p241 for the quantized LRL vector seems to have a typo. Your denominator |Q|^2 should be just |Q|, shouldn't it?Also, you don't seem to mention that the main reason why Pauli chose that modified version was in order to have an Hermitian operator. The GvH thm is less of an issue here, since one just looks

for Hermitian versions of the classical quantities.

And btw, do you want further comments delivered here on your blog, or would email be more convenient?

5. **fg**
July 27, 2016

To follow up on the point 1 in StrangeRep's comment:

The existence of the LRL conserved vector is responsible for the fact that all the bounded orbits a 1/r potential are closed. The conservation of LRL is specific to this potential, and is not related to the usual geometrical symmetries (such as angular momentum, from rotational invariance). It may (but I am not sure) be in fact related to the scaling symmetry pointed out in StrangeRep's comment.

At the quantum level, I think it is related to the degeneracy of the energy levels of the hydrogen atom (all states with the same n and different l have the same energy).

The only other rotationnally invariant potential whose orbits are all closed is the harmonic oscillator. Again, this property can be attributed to the existence of an extra conserved quantity, besides the geometrical ones.

6. **Peter Orland**
July 27, 2016

Hi Peter and Commenters,

After the comments above, I took a look at page 240. It is never actually stated which of Kepler's laws follows from which property of the Coulomb problem. It probably would be good to clarify this.

It's nice to have an explicit solution of the hydrogen atom done this (Pauli's) way in a textbook. I always preferred Fock's method of stereographic projections (it's somewhat more elegant and quicker, esp. for Coulomb scattering), but what you are presenting is probably more suitable for a course at this level.

7. **Peter Woit**
July 27, 2016

StrangeRep,
Thanks! Such comments are greatly appreciated, in general better to send them to me via email.

Thanks to others for comment about the Coulomb potential calculation. One thing one realizes when writing something like this is that even quite a few hundreds of pages is not enough to do more than scratch the surface of the topics one wants to explain. The main goal in this case was the Lie algebra calculation of the hydrogen spectrum, which is both very important physics and a great demonstration of representation theory techniques.

Marcus,
I think there's a detailed answer to your question in the introduction. I'm trying to cover different material not so readily available elsewhere, and also to develop a certain unifying point of view about the relationship of QM and representation theory.

The level of rigor is higher than typical physics books, less than that of typical math textbooks. I've described it jokingly as a level of rigor of the kind one sometimes sees in Russian math textbooks. Unlike many math textbooks, the aim is generally to develop specific central examples, rather than to develop theory and make general statements. The goal is to simultaneously be precise enough to keep mathematicians happy, while still writing something that physicists might be willing to read and try to follow

Asaint,
One project is to get back to work on Dirac cohomology. The book actually develops a lot of the basic elements needed for going farther with that.

8. **Justin**
July 27, 2016

Woit,

What exactly are the prerequisites for working through your book? I understand linear algebra from Serge Lang's famous text, but it does not mention anything about Hilbert spaces and I've never studied them. I also know some algebra from Fraleigh's classic text. I have a very minimal amount of physics just now learning some quantum mechanics from a text geared towards freshman undergraduates so it's below the level of Griffith's text.

9. **Peter Woit**
July 28, 2016

Justin,
You should be able to read the book. It avoids getting into the serious analysis of Hilbert space theory. A warning to anyone though is that more mathematics and more physics come into it as the book goes on, while the treatment gets sketchier, so the later parts will be much more challenging.

10. **D R Lunsford**
July 30, 2016

Book looks like a valuable addition to the canon, thanks for the effort.

-drl

11. **Chris Oakley**
July 30, 2016

Roger Penrose writes the wave equation for a free helicity *s* particle as ∂^{AA'}Φ_{AB…} where there are 2*s* LH SL(2,C) indices on the field. It may have been obvious to him why this followed from Wigner's irreducible reps of the Poincare group, but as it was not obvious to me, I did the analysis in section 3.3 here: maybe there's a quicker way. This equation is the massless Dirac equation for spin 1/2, source-free Maxwell's equation for helicity 1 and linearised GR for helicity 2. So it gets you from your chapter 42 to chapter 46 without needing to invoke pre-quantum physics, and I would have thought, especially if you are aiming the book at the mathematical end of the physicist market, it would be a welcome addition.

12. **Peter Woit**

July 30, 2016

Chris,
Thanks for the pointer to the notes. I wanted though to do something different than the SL(2,C) spinor formalism, for various reasons (one is to keep separate the different ways complex numbers occur). As in a lot of other places in the book, I also wanted to focus on the structure of specific examples, as opposed to a more general formalism. So, while the SL(2,C) formalism is of great generality, covering all spins, I wanted to instead concentrate on the specific cases of field equations for fields occurring in the Standard Model.

13. **Auntiegrav**
August 5, 2016

Do you have a notification list for when the book is published? I look forward to it. Sounds like just what this old nutjob needs.

Monumental Proof to Torment Mathematicians for Years to Come

July 28, 2016
Categories: abc Conjecture

Davide Castelvecchi at Nature has talked to some of the mathematicians at the recent Kyoto workshop on Mochizuki's proposed proof of the abc conjecture, and written up a summary under the appropriate title [Monumental proof to torment mathematicians for years to come](). Here's the part that summarizes the opinions of some of the experts there:

> Mochizuki is "less isolated than he was before the process got started", says Kiran Kedlaya, a number theorist at the University of California, San Diego. Although at first Mochizuki's papers, which stretch over more than 500 pages1–4, seemed like an impenetrable jungle of formulae, experts have slowly discerned a strategy in the proof that the papers describe, and have been able to zero in on particular passages that seem crucial, he says.
>
> Jeffrey Lagarias, a number theorist at the University of Michigan in Ann Arbor, says that he got far enough to see that Mochizuki's work is worth the effort. "It has some revolutionary new ideas," he says.
>
> Still, Kedlaya says that the more he delves into the proof, the longer he thinks it will take to reach a consensus on whether it is correct. He used to think that the issue would be resolved perhaps by 2017. "Now I'm thinking at least three years from now."
>
> Others are even less optimistic. "The constructions are generally clear, and many of the arguments could be followed to some extent, but the overarching strategy remains totally elusive for me," says mathematician Vesselin Dimitrov of Yale University in New Haven, Connecticut. "Add to this the heavy, unprecedentedly indigestible notation: these papers are unlike anything that has ever appeared in the mathematical literature."

Kedlaya's opinion is the one likely to carry most weight in the math community, since he's a prominent and well-respected expert in this field. Lagarias has a background in somewhat different areas, not in arithmetic algebraic geometry, and Dimitrov I believe is still a Ph.D. student (at Yale, with Goncharov as thesis advisor).

My impression based on this and from what I've heard elsewhere is that the Kyoto workshop was more successful than last year's one at Oxford, perhaps largely because of Mochizuki's direct participation. Unfortunately it seems that we're still not at the point where others besides Mochizuki have enough understanding of his ideas to convincingly check them, with Kedlaya's "at least three years" justifying well the title of the Nature piece.

Organizer Ivan Fesenko has a much more upbeat take [here](), although I wonder about the Vojta quote "now the theorem proved by someone in the audience" and whether that refers to Mochizuki's IUT proof of the Vojta conjecture over number fields (which implies abc), or the Vojta conjecture over complex function fields (such as in Theorem 9 of the 2004 paper http://www.kurims.kyoto-u.ac.jp/preprint/file/RIMS1413.pdf), or something else. The reference to Dimitrov as discussing "applications of IUT" might be better worded as "would-be applications of IUT".

There will be a [conference at the University of Vermont]() in September, billed as "An introduction to concepts involved in Mochizuki's work on the ABC conjecture, intended for non-experts."

Update: Fesenko has updated his report on the conference (see [here]()) to include a more accurate characterization of talks by Vojta and Dimitrov (you can see changes to that report [here]()). Between this and the Nature quotes, there seems to be a consensus among the experts quoted (Kedlaya, Dimitrov, Vojta, Lagarias) that they still don't understand the IUT material well enough to judge whether it will provide a proof of abc or not. Unfortunately it still seems that Mochizuki is the one person with a detailed grasp of the proof and how it works. I hope people will continue to encourage him to write this up in a way that will help these experts follow the details and see if they can come to a conclusion about the proof, in less than Kedlaya's "at least three years".

Update: New Scientist has [a piece about this]() which, as in its typical physics coverage, distinguishes itself from *Nature* by throwing caution to the wind. It quotes Fesenko as follows:

> I expect that at least 100 of the most important open problems in number theory will be solved using Mochizuki's theory and further development.

Fesenko also claims that "At least 10 people now understand the theory in detail", although no word who they are (besides Mochizuki) and why if they understand the theory in detail they are having such trouble explaining it to others, such as the experts quoted in the *Nature* article. He also claims that

> the IUT papers have almost passed peer review so should be officially published in a journal in the next year or so. That will likely change the attitude of people who have previously been hostile towards Mochizuki's work, says Fesenko. "Mathematicians are very conservative people, and they follow the traditions. When papers are published, that's it."

I think Fesenko here seriously misrepresents the way mathematics works. It's not that mathematicians are very conservative and devoted to following tradition. The ethos of the field is that it's not a proof until it's written down (or presented in a talk or less formal discussion) in such a way that, if you have the proper background, you can read it for yourself, follow the argument, and understand why the claim is true. Unfortunately this is not yet the case, as experts have not been able to completely follow the argument.

If it is true that a Japanese journal will publish the IUT papers as is, with Mochizuki and Fesenko then demanding that the math community must accept that this is a correct argument, even though experts don't understand it, that will create a truly unfortunate situation. Refereeing is usually conducted anonymously, shielding that process from any examination. Lagarias gives some indication of the problem:

> It is likely that the IUT papers will be published in a Japanese journal, says Fesenko, as Mochizuki's previous work has been. That may affect its reception by the wider community. "Certainly which journal they are published in will have something to do with how the math community reacts," says Lagarias.

While refereeing of typical math papers can be rather slipshod, standards have traditionally been higher for results of great importance like this one. A good example is the Wiles proof of Fermat, which was submitted to Annals of Mathematics, after which a team of experts went to work on it. One of these experts, Nick Katz, finally identified a subtle flaw in the argument (the proof was later completed with the help of Richard Taylor). Is the refereeing by the Japanese journal being done at this level of competence, one that would identify the sort of flaw that Katz found? That's the question people will be asking.

In some sense the refereeing process for these papers has already been problematic. A paper is supposed to be not just free of mistakes, but also written in a way that others can understand. Arguably any referee of these papers should have begun by insisting that the author rewrite them first to address the expository problems experts have identified.

Update: Fesenko is not happy with the Nature article, see his comment [here]().

Comments

1. **Scott Lange**
   July 28, 2016

Can anyone give some insight as to how someone can create something in one lifetime that is so complex it cannot be explained to other experts in that field in less than three years? From a non-mathematician's perspective, it is incredibly hard to imagine such a situation. If I start from the same knowledge base as someone else, and then spend a day or two creating something new on top of it, I can't imagine it taking more than 20 or 30 minutes to explain to that other person- a creation-to-explanation ratio of perhaps 50:1. I would also think that ratio would increase with scope of the creation, due to the various dead-ends that a larger project would have to have encountered that then would not need to be explained.

Thanks in advance for any insight anyone can offer!

2. **Peter Woit**
July 28, 2016

Scott Lange,
Well, Mochizuki hasn't spent all his time since the proof came out explaining to experts, and they haven't spent all that time listening. On the other hand, it is four years, not three… This really is an extremely unusual situation, I know of no other like it. One thing that I think is fascinating about it is that by understanding what has gone wrong, you get insight into the complexities of how things work normally, when things go right, and understanding gets transmitted.

3. **eggcrook**
July 28, 2016

It sounds like the plot of the Taming of the Shrew where the father (Mochizuki) won't allow any of the suitors of his popular daughter (ABC proof) to marry her until they find a husband for his unpopular and difficult daughter (IUT theory). If he wants as many people to put the effort into learning IUT as possible, a reduction of the ABC proof to existing mathematics would be a disaster for him.

4. **Thomas**
July 29, 2016

Two properties of Mochizuki's texts repel people: First, his too frequent and too often repulsive terminology, much of which is about things one usually does not need new words for – mathematicians usually have a better sense for words and take better care on them (esp. Grothendieck and his school). Then, a lack of expository structure – usually one gives an overview what is done where and with which ideas, so that readers can decide which parts they read in which order with which way of reading.

5. **Davide Castelvecchi**
July 29, 2016

I think that Mochizuki's case has some (distant) similarities with Louis de Branges' claim to have proved the Riemann hypothesis. He also had a track record of solving important problems. and he too developed his own, nonstandard mathematical language that other mathematicians found indigestible. Although contrary to de Branges' case, people are taking Mochizuki's proof seriously and some are putting a lot of effort into reading it. See this story from several years ago:

http://www.lrb.co.uk/v26/n14/karl-sabbagh/the-strange-case-of-louis-de-branges

In the past, there have been cases of theories that (even though much less grandiose and elaborate) took a long time for people to understand — and eventually had to be reformulated in a different language. When I wrote my profile of Mochizuki back in October, some of my sources mentioned Newton's Principia as an example. And just the other day, one source told me: "It was mentioned at the conference that sometimes things take a long time to be accepted. Two examples mentioned at the conference were Galois theory and Class Field Theory, each of which took about 40 years."

6. **e. ehrenweist**
July 29, 2016

Cohen's proof of the independence of the continuum hypothesis using forcing seemed at first nearly incomprehensible, and alien, to most mathematicians. It took several years before its ideas were distilled and made accessible. (I still find forcing to be somewhat magical for that matter.)

7. **mahmoud**
July 29, 2016

ehrenweist,

Cohen's proof was quickly judged as correct by Gödel (as well as other prominent logicians in the US) and in a few years after Cohen's breakthrough there was an avalanche of new applications of forcing. In fact Shoenfield's 1967 textbook contains an exposition of forcing, just 3 years after Cohen's papers, so how is this in any way similar to Mochizuki's case where experts can't understand even the overarching strategy after 4 years?

8. **Aatu Koskensilta**
July 29, 2016

ehrenweist,

as mahmoud says, Cohen's proof was understood very quickly, and what confusion and misunderstanding there was, mostly had to do with the fact that it was so unexpectedly "mathematical", involving, in its more natural, non-syntactic form, surprisingly little particularly logical machinery in any substantial sense. It took a while, but not very long, before set theorists and logicians sorted out what was essential, what was just idiosyncratic technical scaffolding, and so on, resulting in the now familiar unramified partial order and Boolean valued formulations. There were plenty of easy cases to try forcing on, and a vast array of immediately apparent possibilities for further application and refinement — collapsing cardinals, iterated forcing, class forcing, and so on, and so on — so that people were able to quickly develop a sense for the sort of mathematics that the new stage of set theory would turn out to be. As Kreisel (who apparently was somewhat miffed he hadn't come up with forcing himself, and even, somewhat implausibly, suggested it was nothing new, really, pointing to all manner of in hindsight analogous technical whatsits in constructive and intuitionistic mathematics) and others were at pains to point out, the basic ideas and proofs were very simple and straightforward by any mathematical standards, even if they did at points give out a slight whiff of magical out-of-hat-pullery, and there certainly wasn't years worth of new terminology, visions, grand theory, to digest. Indeed, it was all positively pedestrian in comparison to, say, what proof theorists back in the day were up to.

9. **Peter Woit**
July 29, 2016

All,
Please, no more comments using this as an excuse to discuss a completely unrelated topic.

10. **sdf**
July 29, 2016

The thing stopping me from going near his papers are the language and notation as highlighted by Dimitrov in the quote you have. Faltings said that he didn't think that Mochizuki's work would be taken seriously until "he wrote a readable paper"—recall that Faltings was Mochizuki's advisor.

11. **Peter Zbornik**
July 29, 2016

There might Be a need for a "Rosetta stone" or a dictionary in order to speed up IUT learning. Fortunately such a stone sesms to be around. Mochizuki writes the following in his paper "Bogomolov's proof of the geometric version of the Szpiro conjecture from the point of view of inter-universal Teichmüller theory" (Res. Math. Sci. 3(2016), 3:6), that:
"
aspects of inter-universal Teichmüller theory may be thought of as arithmetic analogues of the geometric theory surrounding Bogomolov's proof. Alternatively, Bogomolov's proof may be thought of as a sort of useful elementary guide, or blueprint [perhaps even a sort of Rosetta stone!], for understanding substantial portions of inter-universal Teichmuller theory."

http://www.kurims.kyoto-u.ac.jp/%7Emotizuki/Bogomolov%20from%20the%20Point%20of%20View%20of%20Inter-universal%20Teichmuller%20Theory.pdf

12. **mahmoud**
July 29, 2016

I think Castelvecchi is quite right in his comparison to de Branges, what he did not mention is de Branges earlier and correct(!) proof of Bieberbach's conjecture, which also was presented in form of a lengthy and mostly indigestible manuscript that most mathematicians dismissed at the time.

So the Mochizuki affair could be summarised by asking: are we dealing with another case of de Branges' proof of the Riemann hypothesis, or of his proof of Bieberbach's conjecture?

13. **Radioactive**
July 29, 2016

For the interested non-mathematician – what even are the objects or concepts involved in IUT? What are the universes that are supposedly inter-connected by Teichmüller theory? I suppose a more basic question would be about Teichmüller theory itself… but has anyone constructed any examples of these spaces and the relations between them in terms of more familiar things?

14. **Michael Barany**
July 29, 2016

Davide's comparisons to Newton and Galois seem spot-on from my perspective as a historian and sociologist of mathematics (though it will take a very long time indeed to say whether IUT has anything approaching the importance of the calculus or Galois theory). Some historical references for those who are interested: on Galois, if you read French you should definitely track down the recent work of Caroline Ehrhardt, who has come up with some real breakthroughs in how historians understand the interpretive history of Galois's very confusing texts. On Newton, Niccolo Guicciardini's Reading the Principia is an accessible and insightful place to look. A now-canonical comparison-point for sociologists is the classification of finite simple groups, though that was a large collective project rather than a proposal from an individual; see Alma Steingart's important article "A Group Theory of Group Theory."

15. **Peter Woit**
July 29, 2016

Radioactive,
The problem here is that such questions as "what are the basic new objects and concepts" have baffled experts, so the non-expert doesn't have a chance. Knowing anything about the usual Teichmuller theory I don't think helps.

16. **Dale C.**
July 29, 2016

Apparently the 500+ page introductory material is too complex and time-consuming to digest. Is the only way out that someone (or a group of people) digest the material and re-write it to more closely follow mainstream mathematics? And should they re-write just the parts essential to the ABC proof, or the whole material? In the beginning, there was not enough motivation to do that, as no outsider knew if the re-write was worth the effort. It appears that the attitude might be changing.

17. **Peter Woit**
July 29, 2016

Dale C.,
I'm not sure what "introductory material" you mean. The problem I think is basically that
1. Mochizuki refuses to rewrite material, on the grounds that this should not be necessary, people should just spend the time it takes to understand what he has written.

2. The small number of people who were supposed to have digested this material, and rewrite it in this manner (e.g. Go Yamashita), have not been able to complete this task.

3. Others have been unable to digest the material. If you can't understand yourself why a proof works, you're not going to be able to rewrite it for others to understand.

That, four years later, no one else has been able to write up their own version of the proof is the central mystery here.

18. **Dale C.**
July 29, 2016

With the "introductory material" I was referring to "Mochizuki, S. Inter-universal Teichmüller Theory I-IV", that apparently the ABC proof is based on. It looks very much like no outsider understands the material despite 4 years since the release of the claimed proof.

I suppose Mochizuki re-writing material has its own problems, as probably according to his understanding the material is complete already. If this is the case, then somebody else has to invest time to make the translation to a more mainstream formulation. The problem might be that the material is so large and so far away from mainstream mathematics that it will take years to understand and re-write it.

19. **Bernhard**
July 29, 2016

Peter,

What do you think about Lagarias' comment that Mochizuki's work has "some revolutionary new ideas," ? This seems really odd. Aren't they still trying to figure out if the alleged proof is right or wrong still? How do they than separate a possible collection of crackpotish ideas from "revolutionary" ones? Is this a case where no matter what the outcome the tools he invited (which?) are already useful?

Also shouldn't be on Mochizuki's shoulder the burden to try to prove his work is worthy? Why aren't these guys reacting like "You have a proof? Make it readable" His refusal to do so, sounds a bit he's hiding behind formalism.

It's not like they can do an experiment to verify it so…

20. **Peter Woit**
July 29, 2016

Dale C.,
Those papers are where the details of the proof are supposed to be, and it's exactly those that everyone is having trouble with.

Bernhard,
There are definitely lots of "new ideas", the problem is understanding them and whether they really are powerful enough to give a proof. They are not being just dismissed, partly because Mochizuki has a serious track record, partly because as people work on them, they are not finding that they are wrong.
I think there is a strong feeling among a lot of people in the field that it is Mochizuki's responsibility to do a better job of communicating his ideas, so they're not going to spend more time on this now.

21. **Peter Zbornik**
July 30, 2016

Here is what Mochizuki thinks of the situation, December 2014:

"Indeed, I have been participating for over 20 years now, as author, referee, editor, and editor-in-chief, in the refereeing of countless papers for math- ematical journals, and, as far as I can see, the verification activities on the part of the three researchers discussed above already exceed, by a quite substantial margin — i.e., in their content, thoroughness, and meticulousness — the usual level of refereeing for a mathematical journal. Moreover, although I have received com- ments not only from the "core three" researchers, but also from other researchers as well, concerning numerous superficial technical oversights that may be repaired immediately (i.e., a routine aspect of the refereeing process),
I have yet to hear of even a single problem that relates to the essential thrust or validity of the theory."
…
"My understanding, at present, concerning the verification of IUTeich is that
at least with regard to the substantive mathematical aspects of such a verification, the verification of IUTeich is, for all practical purposes, com- plete; nevertheless, as a precautionary measure, in light of the importance of the theory and the novelty of the techniques that underlie the theory, it seems appropriate that a bit more time be allowed to elapse before a final official declaration of the completion of the verification of IUTeich is made.
On the other hand, I should also state that, although such precautionary measures may serve a meaningful role for a limited amount of time, I am not of the opinion that such precautionary measures should be maintained for periods of, say, the order of 20 ~ 30 years. That is to say, although there are perhaps numerous approaches to the issue of computing an appropriate length of duration for such precautionary measures, my current sense is that the length of duration of such precautionary measures should not exceed 10 years, i.e., counting from the time of the first oral presentation of the theory (i.e., in October 2010) and the posting of the series of papers on the theory (i.e., in August 2012). Put another way, my current sense is that some date during the latter half of the 2010's would be an appropriate time for the termination of such precautionary measures."

http://www.kurims.kyoto-u.ac.jp/~motizuki/IUTeich%20Verification%20Report%202014-12.pdf

22. **Radioactive**
July 31, 2016

From reading that it does sound like, if he hadn't claimed to solve a famous problem, the refereeing process would have been done and dusted a long time ago. And it does seem that he has made quite a bit of effort to disseminate and teach the theory to people and recognizes the need for more exposition. Thus it must be frustrating to him to see such statements as the title of this article.

I suppose the proliferation of specialties in math can lead to situations like this – especially when important proofs come from unexpected sources – and rather than exposing some fault of Mochizuki it exposes some weaknesses in the mathematical community. With everyone an expert in their own subdomain, and with no time to carefully examine new approaches, if the one with a revolutionary result is, understandably, not willing to drop everything and publicize then a state of confusion will persist.

Though I suppose someone with a good expository style will eventually learn what a Frobenoid is and write a textbook.

23. **David Roberts**
July 31, 2016

Peter,

Is there likely to be a high-level blog post from someone who was there this time around? Or perhaps an interview with such a person with a serious mathematical audience in mind? (i.e. not merely educated layperson-level coverage, nice though that is)

24. **Peter Woit**
July 31, 2016

David Roberts,

I agree that that would be a great service and really helpful for the field and for the math community in general. I hope it will happen (and hear rumors it might…).

25. **Timothy Chow**
August 1, 2016

I am curious about your comment that Mochizuki is "the one person with a detailed grasp of the proof and how it works." What about Hoshi and Yamashita? Admittedly they have not produced expositions that satisfy others, but then again neither has Mochizuki.

You also said, "That, four years later, no one else has been able to write up their own version of the proof is the central mystery here." This does not seem so mysterious. Writing up your own version requires a lot of talent in its own right. Perhaps Hoshi and Yamashita don't have that particular talent. As for others, few if any made any concerted attempt to understand the papers for the first three years.

26. **Joe Yang**
August 1, 2016

As someone who has left the filed of mathematical research many years ago, I only recently started following the ABC conjecture/Mochizuki saga and found it to be absolutely fascinating. Given that several mathematicians have claimed to have gone through the IUT theory thoroughly and Mochizuki's reputation of being a detailed and bright mathematician, it is unlikely that IUT theory is complete rubbish. Of course, it is still possible that there are technical mistakes in the proof of the ABC conjecture that have not been caught due to the massive amount of new ideas and technique/concept/terminology.

One interesting point I have seen raised in another thread of discussion is the lack of HARD analytical number theory type of argument in Mochizuki's claimed proof. This seems to be addressed in the most recent survey (though 100 pages long) article by SM:

http://www.kurims.kyoto-u.ac.jp/~motizuki/Alien%20Copies,%20Gaussians,%20and%20Inter-universal%20Teichmuller%20Theory.pdf

Section 1 is accessible to anyone with basic calculus background and makes for very interesting reading as SM explains how to do Gaussian integral to a fictitious high school student . Through section 1, one can get some cursory sense of what SM is trying to do with IUT and perhaps also understand his frustration. The following passage on page 7 in particular is clearly meant to address some of the criticism pointedly:

"the idea that meaningful progress could be made in the computation of
such an exceedingly difficult integral simply by considering two identical
copies of the integral — i.e., as opposed to a single copy — struck the
student as being utterly ludicrous.
Put another way, the suggestion of Step 3 was simply not the sort of suggestion that
the student wanted to hear. Rather, the student was keenly interested in seeing some
sort of clever partial integration or change of coordinates involving "sin(–)",
"cos(–)", "tan(–)", "exp(–)", " 1
1+x2 ", etc., i.e., of the sort that the student was
used to seeing in familiar expositions of one-variable calculus."

27. **Peter Woit**
August 1, 2016

Timothy Chow,
I disagree that writing up a proof requires unusual talent, this is what mathematicians are trained to do. This is what we do with students: try and teach them why something is true, then see if they understand it by asking them to give the argument in their own words. Until they can do this we don't recognize them as someone who understands the argument.

To apply this to the current situation is of course oversimplifying and being rather harsh on Yoshi and Yamashita. The situation with this proof is quite unusual due to its complexity and use of unfamiliar concepts. But, it seems to be a fact that they have not been able to either write out a version of the proof that others can understand, or give lectures that transmit understanding. Why this is is a big mystery, I just don't believe that the problem lies with laziness of the audience.

28. **Peter Woit**
August 1, 2016

Joe Yang,

The Gaussian integral business is I think a good indication of the problem with Mochizuki's survey (the latest one, and similar early documents). It is advertised as an explanation to other mathematicians of his ideas, but the Gaussian integral explains nothing at all about them since it's just a very vague analogy. Bringing in the high school student makes explicit that what this is is a parable, and in the parable experts in the subject are being compared to ignorant, unsophisticated high school students. I think experts trying to get something out of this document find this beginning part of it not only useless, but insulting, it doesn't exactly encourage them to read on.

29. **Joe Yang**
August 1, 2016

Peter,

Yes, the tone used in the high school student analogy can certainly be taken as insult to some. On the other hand, I don't see anything wrong with using Gaussian integral as a motivating analogy (even if a "vague" one as you put it) in a survey paper to explain the concept behind "mutually alien copies". It should not be "useless" if indeed the idea behind mutually alien copies is similar to taking two identical copies of integrals. Having met / worked with"experts" in various mathematical fields, I find it common occurrence that many who are brilliant in understanding ideas close to their own research to be not necessarily so fast at grasping "alien" concepts. More often than not, the "experts" would like to see an analogy from concepts that they are familiar with and as such a simple analogy can often be a good starting point.

30. **Peter Woit**
August 1, 2016

Joe Yang,
I'm actually normally a big fan of mathematicians including more motivational material in what they write, including even vague analogies like this one. Too much of the math literature is only readable by experts, including no help to the nonexpert about how to get oriented, how to get the general picture of what is happening. The problem here is a very unusual one, that there's plenty of such motivational material, but the more precise material experts need to understand exactly what is going on is missing from the survey, and in the long IUT documents buried in a huge array of new definitions and formalism, often depending on earlier papers. There's somewhat of a standoff here, with Mochizuki's attitude: "it's all there, you just have to work harder", a lot of experts saying: "we've tried that, couldn't get anywhere, you need to write this up in a more conventional form, an outline of the proof with precise, checkable statements". I can't recommend Brian Conrad's blog post highly enough as a serious explanation of what the problem is.

The workshops at Oxford and Kyoto have been the best attempt so far to overcome this standoff. This is a difficult situation and I think lots of people are honestly working hard to understand the potential new mathematics here. I do think though that it would help if Mochizuki (or those close to him) made more of an effort to write something that would address the concerns experts have pointed out. Responding to these with even implicit insults is not helpful.

31. **Timothy Chow**
August 1, 2016

Peter, I may have misunderstood what you meant by "writing up their own version of the proof." Hoshi and Yamashita have, after all, given lectures and produced written text that is not simply a verbatim reproduction of Mochizuki's writings. I took you to mean producing a fundamentally new exposition that meets the popular demand for a high-level sketch that conveys the main ideas of the proof in more conventional language that the experts are already accustomed to. That sort of thing requires talent and doesn't always happen within a few years. Consider, for example, the account of etale cohomology in SGA4, or Hironaka's paper on resolution of singularities. These have a reputation for being formidable and inaccessible texts, and it took a very long time for alternative expositions to appear.

32. **Peter Woit**
August 1, 2016

Timothy Chow,
As far as I know the only English-language written texts available from Hoshi and Yamashita are sketchy slides from presentations (Yamashita is supposed to be writing a conventional document but it hasn't emerged, Hoshi has some notes on IUT in Japanese which I gather he is translating into English). Among those slides, I don't see anything like an outline of the abc proof, they appear to be addressing more specialized topics that are somehow part of the proof. They're meant to complement lectures, undoubtedly those in attendance at the lectures could get more out of them. But the bottom line seems to be that no one in attendance at those lectures, or readers of those slides, has come away saying "now I understand the proof, at least in outline".

I wasn't asking for the kind of thing you describe, readable by many mathematicians. That would be great, and hopefully someday we'll have such a thing, but, sure, that's the result of often a lot of time boiling down the initial version. What doesn't seem to exist is something readable by experts.

SGA4 is maybe a good example. When it came out (1972?)[*actually much earlier, see Brian Conrad's comment*] it was already the product of not just one person, but multiple experts who had absorbed Grothendieck's ideas.
A more readable version SGA 4.5, did take 5 years to appear, and something like that for IUT is still a long ways away. By the way, my guess is that if we ever do have a version of Mochizuki's ideas, integrated with the rest of mathematics, he may be as unhappy with the way they are written up as Grothendieck was with SGA 4.5…

33. **Brian Conrad**
August 2, 2016

Peter:

Roughly speaking, SGAn was the seminar in 196n (so SGA1 in 1961, etc.). The Springer-Verlag edition of the SGA4 volumes was widely published in the early 1970's, but the original copies were distributed to a more limited set of math departments in the mid-1960's (e.g., in the Harvard math library the original SGA's are massive volumes in yellow binding consisting of typewriter-generated text on paper that must by now be very brittle; laid end to end, these volumes occupy much of an entire shelf and constitute a very daunting sight!)

But your main point about others in geographical vicinity to Grothendieck picking up the ideas and pushing them into new terrain is apt: in those days one had to be in one of a handful of places (e.g., Paris [Serre/Grothendieck/Deligne/etc.], Moscow [Shafarevich/Manin], Princeton [Katz], Bonn [Harder], or Boston [Artin/Mazur/Mumford/Tate]) to acquire a "working knowledge" of the new concepts by talking with local experts, that being much more efficient than trying to read on one's own, and things really spread that way. (I have been told by a top expert from those days that much was learned through seminars and talking with people, rather than by direct study of EGA on one's own, etc.)

For example, already by the late 1960's the students of Mike Artin, Barry Mazur, David Mumford, and John Tate were making very creative use of the ideas of SGA4 (e.g., Knutson's 1968 PhD thesis on algebraic spaces, Jim Milne's 1967 thesis using etale and crystalline methods to prove BSD for constant abelian varieties, Tadao Oda's 1967 thesis on Dieudonne theory for abelian varieties, Larry Roberts' 1968 thesis on flat cohomology of finite group schemes, Friedlander's 1970 thesis on etale homotopy theory). And that's just the PhD students in Boston, to say nothing of what was being done by the senior faculty there (Deligne-Mumford, Artin-Verdier, Artin-Mazur, Artin-Tate, etc.) and by both PhD students and senior faculty elsewhere.

The publication of more accessible references such as SGA 4.5 in 1977 (let alone Hartshorne's textbook at a more basic level in the same year, the impact of which on the explosion of the subject probably cannot be overestimated) helped tremendously in spreading the understanding of these ideas for those who weren't able to attend lectures by Katz, Deligne, etc. But well before the huge increase in accessibility of the subject through written alternatives to the foundational references, within a handful of years after SGA4 a growing number of people were picking up the principles through the oral tradition and producing high-level research based on these ideas.

In more recent times one can see something similar happening with perfectoid spaces (which were introduced only in 2012 and have already led to tremendous advances in many directions never anticipated at the inception), thanks in no small part to the creator of the subject traveling early on to numerous places to give substantive lectures and producing insightful surveys on the basis of which (along with original papers) seminars have been run all over the world. Of course, the available options for dissemination of ideas are far greater now than in the 1960's.

34. **sdf**
August 2, 2016

New Scientist article here reports I. Fesenko as having made the absurdly ambitious statement (2nd paragraph from end):

"I expect that at least 100 of the most important open problems in number theory will be solved using Mochizuki's theory and further development."

35. **mahmoud**
August 2, 2016

Unless New Scientist has fabricated the "100 problems"-quote by Fesenko it seems high time to put him in the crackpot camp together with de Branges. Also, didn't Mochizuki himself say before that IUT is likely to be a one hit wonder without any major applications besides abc?

36. **Brian Conrad**
August 2, 2016

mahmoud: It is correct that when an audience member asked Mochizuki during one of the Q&A Skype sessions at the Oxford workshop whether he was aware of other applications of his IUT work to other problems in mathematics, his reply was a forthright "No" (which is of course quite fine).

He also explained in very reasonable terms why he had explored trying to use it to attack RH. The point is that his notion of Frobenioid was inspired by the search for a replacement for Frobenius maps that is useful in characteristic 0, and the IUT work provided a context for applying that notion in some specific instances. Hence, it seemed quite natural to follow up the IUT work by considering if Frobenioids could be applied to characteristic-0 versions of problems for which Frobenius maps have been very fruitful in characteristic p, such as RH.

He was very straightforward in then saying that after trying this for some time (maybe a couple of years?) and not making much progress, he wasn't looking into that approach to RH anymore.

37. **Alan**

August 2, 2016

On Go Yamashita's page (http://www.kurims.kyoto-u.ac.jp/ja/list/yamashita.html) you can find this:

近年は,望月新一氏による宇宙際幾何学のさらなる発展の方向性で同氏と共同研究をしている.望月新一氏の計算においてabc予想の誤差項にRiemannゼータ関数との関連性を示唆する1/2が現れる.一方,同氏の宇宙際Teichmüller理論においてテータ関数が中心的役割を果たすのであるが,テータ関数はMellin変換によってRiemannゼータ関数と関係する.さらに,宇宙際Teichmüller理論において宇宙際Fourier変換の現象が起きている.これらのことから,長期的な計画であるが"宇宙際Mellin変換" の理論ができればRiemannゼータ関数と関係させることができるのではないかと期待して共同研究を進めている.

I have a translation:

Recently, I am researching in collaboration with Shinichi Mochizuki, the perspective of future development of Inter-Universal Geometry, of his authorship. According to Mochizuki's calculations, on the abc conjecture "error" there appears a 1/2 as an indicator of the relationship that can exist with the Riemann zeta function. On the other hand, on Mochizuki's Inter-Universal Teichmüller Theory, the theta function presents a fundamental role, that by its turn is related to the Riemann zeta function by the Mellin Transform. Moreover, in the Inter-Universal Teichmüller Theory appears the occurrence of Fourier Transform. Based on this facts, although it's a long term project, I am advancing the collaborative research in expectation that if the "Inter-Universal Mellin Transform" is completed, then the relationship with the Riemann zeta function can be proved.

38. **David Roberts**
August 3, 2016

Sadly the second link to what I gather is Fesenko's Facebook feed (in the sentence "you can see changes to that report [here]") requires a FB account. Can anyone with access let us know what it says?

39. **Peter Woit**
August 3, 2016

David Roberts,
That's not very important, just the edit history of that post, showing the half-dozen or so different versions as the author edited and added some material, most substantively clarifying what Vojta and Dimitrov had to say (I assume you can see the latest version).

40. **mahmoud**
August 3, 2016

Conrad and Alan,

Glancing at Mochizuki's papers the connection to RH is actually discussed at some length in IV: Log-volume Computations and Set-theoretic Foundations: Remark 2.2.1 on page 47 is essentially an elaboration of what Alan translated from Yamashita. Despite his answer over skype that Conrad reported it appears he hasn't given up on that approach yet (it isn't clear when that remark was written, he has added new remarks to the paper in June 2016 so 2.2.1 might be a new addition).

Anyway, this suggests a way forward for experts in number theory who are unwilling to go through the whole terminological mayhem around IUT: study the computations (especially the "log-volume estimate" 1.10) and take Mochizuki on faith that it should apply to (arbitrary Dedekind) zeta functions; if anything emerges that contradicts known bounds on these functions this would throw serious cold water on the reliability of (this application of) IUT.

41. **Brian Conrad**
August 3, 2016

mahmoud:

Thanks for the clarification; perhaps something changed between December and June. Due to discussions I've had with some who have looked in detail at the computations (including around IV.1.10), it seems that those computations are not going to shed much light in the direction you mentioned. Hopefully there will eventually appear an essay by an audience participant at the Kyoto meeting who can address what mathematical insights from the IUT papers were communicated to the audience.

In case you may have wondered, the question and answer at http://mathoverflow.net/questions/245438/mochizukis-gaussian-integral-analogy has essentially no mathematical content and does not address statements which are precise. Nothing helpful is likely to come out of Math Overflow Q&A's at this stage (not that one should have expected otherwise, given the circumstances).

42. **marshall Flax**
August 3, 2016

@Brian Contrad, perhaps what's changed is the last two slides from https://www.maths.nottingham.ac.uk/personal/ibf/files/dimitrov.pdf ?

43. **Brian Conrad**
August 3, 2016

marshall flax: No, that definitely has nothing to do with it (and I recommend not to spend time wondering what someone else may or may not achieve in the direction of RH). The appearance of an L-function there is related to Siegel zeros, and the connection between a suitable formulation of ABC and Siegel zeros has been known for some time since work of Granville & Stark (as Dimitrov notes on his slides too), well before 2012. Those slides reflect considerations of Dimitrov done on his own, treating the methods of IUT as a black box (as one can see from how the slides are written); the core arguments in his slides are applications of the standard methods of Diophantine analysis.

44. **Gowers**
August 4, 2016

I'm not the first to say this by any means, but I sometimes wonder whether a change in culture is needed, a change particularly highlighted by this example. Proofs have two functions: to certify the truth of mathematical statements, and to give insights into why they are true. If one mathematician provides a long and almost incomprehensible, but correct, proof of a statement (perhaps checkable by only a handful of experts), we tend to say that that mathematician has solved the problem, but if another mathematician comes along and finds a shorter proof that everybody understands, that is often much more valuable. I think there's a case for saying that Mochizuki hasn't really solved the abc conjecture even if his proof is correct. However, if someone uses his ideas to produce a comprehensible proof, then obviously Mochizuki will have played a major — probably *the* major (but that depends on the details of how the comprehensible proof is found) — role in the solution.

45. **Jeffrey M**
August 4, 2016

Just a quick comment, Fresenko is wrong about publication and what it means. Plenty of things have been published which no one in the field treats seriously. It depends on where it appears, which essentially has to do with how good the refereeing is. Annals for Wiles is the classic example, but there are plenty of others. Incorrect stuff gets published plenty, but experts usually know it's wrong and just ignore it. The fact that no one can (yet) explain this to people who should be able to understand it is a giant red flag, in my opinion. Nothing has ever been like that, ever. Grothendieck confused plenty of people, but as brian Conrad points out, quite a few people were using his stuff to generate all sorts of amazing results within a short period.

46. **Peter Woit**
August 5, 2016

I'm closing comments on this for now, since I'm on vacation and don't have time to moderate them. In addition, this has become kind of depressing, with some people seemingly intent on choosing pro or anti Fesenko or Mochizuki positions and arguing them. I'd urge everyone to stop this and stick to the question of whether or not this is a proof. I've seen what can happen to your field when, lacking such a standard, people start choosing sides and arguing. It's not pretty, and mathematics should not go there.

HEP Physics News

July 31, 2016
Categories: Strings 2XXX, Uncategorized

- ICHEP 2016 starts in Chicago this week. Talks about the new diphoton results are scheduled for 9am (Chicago time) Friday. There will also be talks later in the day at CERN (5pm Geneva time), scheduled as part of this summer's TH institute. Consulting my prediction here (although I had the plenary vs. parallel wrong), I think it's very clear that these will be negative results for the supposed 750 GeV bump. See Resonaances for discussion of the significance of this.
- Last year I heard Nima Arkani-Hamed talk here about "Nnaturalness". The paper is now out.
- There's a very interesting profile in Nautilus of Fotini Markopolou, who left theoretical physics to work on a startup in England.
- At Quanta magazine, Natalie Wolchover has a nice article about intriguing new neutrino results. With nothing unexpected showing up at the energy frontier being explored by the LHC, in coming years it may very well be the neutrino sector, which can be explored at much lower energies, where one should look for something new.
- Strings 2016 is starting in Beijing in a few hours. Schedule here, talk titles here.

Update: Videos of the Strings 2016 talks are becoming available here. The talks of the first day featured little string theory (except for a historical talk by John Schwarz). A major theme was 3d quantum field theory, with Witten and Costello talking about the same new ideas starting with Chern-Simons theory, and Seiberg talking about dualities in 3d qft.

Update: Slides are here. Tuesday started off with entanglement entropy and tensor networks. I confess that not only do I not see what this has to do with string theory, but have trouble seeing what it has to do with anything. The connection with quantum gravity using tensor networks seems vaguely to recall the much more well-motivated spin networks of LQG, which string theorists always denounced as not exhibiting a Lorentz invariant ground state. Now it's the string theorists promoting this kind of network structure, noticing the same problem they used to denounce as convincing evidence their competitors had it all wrong. The world is a strange place.

Update: New Scientist has a quote from me saying the obvious thing about Nnaturalness. Blogging likely light to nonexistent for the next week, I'll be traveling, listening to music in Nashville, as well as up in the mountains of Virginia/Tennessee,

Comments

1. **David**
   July 31, 2016

   Will the Strings 2016 talks be available for streaming anywhere?

2. **Peter Woit**
   July 31, 2016

   David,
   I assume so, they always have been in the past. Presumably links to videos will appear at some point on the Strings 2016 website.

3. **Tim**
   August 1, 2016

   I am amazed Strings 2016 and ICHEP happen at the same time. I guess they're working under the assumption that no one who is interested in string theory is also interested in experimental results?

4. **Peter Woit**
   August 1, 2016

   Tim,
   The main part of ICHEP is next week (the only overlap with Strings 2016 is the first two days, Thursday and Friday this week), and it is largely an experimental physics conference, with experimentalists usually not going to theory conferences, and vice versa. The Strings schedule leaves plenty of time for participants to head to Chicago on Friday and catch most of ICHEP (although I don't think a lot of that will be going on).

5. **Szilard**
   August 1, 2016

   Natalie Wolchover is one of the best science journalists out there, IMO.

6. **vmarko**
   August 3, 2016

   Peter,

   The tensor networks are not really being used in the similar context to spin networks of LQG. String theorists are studying tensor networks in order to apply them to holography-related ideas. AFAICS, a lot of ST has now mutated into the study of holography and AdS/CFT, so I am not really surprised that tensor networks are so prominent at the conference.

   Also, the entanglement entropy is tied into the issue of black hole information problem, which is of course a very popular toy-topic for ST folks. So no surprise there either.

   Of course, one can always challenge the relevance of holography and BHI problem to quantum gravity (and I often like to do it), but that's a different issue.

   🙂
   Marko

7. **Peter Woit**
   August 3, 2016

   vmarko,
   I understand that this work is motivated by issues to do with holography, I'm just having trouble seeing a plausible scenario for how you get from this sort of thing to any

new insight about fundamental physics (as opposed to the hundredth claim to “solve the black hole information paradox”). To some extent the idea seems to be that this is going to lead to some completely new understanding of QFT, or even of quantum theory itself, but I just don’t see it.

Yes, these are different networks in a different context, the similarity is just in the idea that one is going to construct quantum gravity states using some discrete combinatorial gadget, and it’s amusing to see that this encounters the same problem as the (to my mind much better motivated) LQG attempt. I’m curious what Gross and Witten think of this. They were quite critical of such things when motivated by LQG, wonder if they think this is anything better.

8. **haxi**
August 3, 2016

@David, Talks from the Strings 2016 can be viewed at here: http://ymsc.tsinghua.edu.cn:8090/strings/?page_id=706

9. **zingo**
August 3, 2016

@haxi
Not really. The video doesn’t play. I wonder whether these people have heard of YouTube.

10. **haxi**
August 4, 2016

@zingo, you don’t really know China, do you? 🙂

11. **Ricardo**
August 4, 2016

No signs of SUSY yet

https://twitter.com/srrappoccio/status/761214871488823296

Sean Carroll says some rethinking might be needed, or not

https://twitter.com/seanmcarroll/status/761224126040150016

12. **MB**
August 4, 2016

750 GeV bump not seen in CMS data https://cds.cern.ch/record/2205245

13. **Andreas**
August 6, 2016

Is there any update on the global 2.5sigma excess of WZ bosons at 2 TeV (http://arxiv.org/abs/1506.00962), that has been seen by ATLAS last year?

14. **cma**
August 6, 2016

Anyone knew where I can listen to the “vision lecture” and the powerpoint given by David Gross at String 2016?
Thanks

15. **Gilbert Gibbs**
August 8, 2016

Peter,

I vaguely recall some purported connection between entanglement in tensor networks and AdS/CFT, whereby spacetime itself could be constructed. I believe, this was the piece Nature did on it last year: http://www.nature.com/news/the-quantum-source-of-space-time-1.18797

With regards to spin networks, I found Roger Penrose’ original papers on them, quite insightful, but markedly different wrt the above. Incidentally, today is Penrose’ 85th birthday, so congratulations to him.

16. **Yatima**
August 9, 2016

The summer of null results continues:

Sterile Neutrinos not an element of this universe with p > 0.99: via the red-top press

17. **martibal**
August 9, 2016

Does this rule out only a fourth generation of neutrinos, or also right-handed neutrinos for the known generations ?

18. **Low Math, Meekly Interacting**
August 9, 2016

Despite the frustratingly vague wording in many of these popular articles, it appears that the IceCube collaboration largely ruled out low-mass sterile neutrinos of the 4th kind. If it has anything to do with chirality, type of fermion, see-saw mechanism, etc. that’s nowhere made explicit in what I’ve seen.

19. **Yatima**
August 10, 2016

Natalie Wolchover does her magic at Quanta Mag: What No New Particles Means for Physics tl;dr Just keep on trucking! (Although nothing is said about the significance of “the desert” for the prospect of financing a next generation colliders)

The Nightmare Scenario

August 10, 2016
Categories: Uncategorized

Now back from a short vacation, and there seems to have been a lot happening on the debate over fundamental physics front. From the experimentalists, news that the Standard Model continues to resist falsification:

- At ICHEP, as expected, new data from ATLAS and CMS ruled out the supposed 750 GeV state that would have indicated new physics.
- Also at ICHEP, significantly stronger bounds on SUSY: gluinos ruled out up to 1.9 TeV, stops up to 900 GeV.

Recall also the recent results from LUX and PandaX (discussed here) putting stronger bounds on the sort of WIMP dark matter supposedly a feature of SUSY models.

In addition, there was news from IceCube ruling out the possibility of certain models of light sterile neutrinos (paper here, a Nature news story here).

For what it all means, you should of course consult Resonaances (and read a profile of Jester, The Rogue Blogger Who Keeps Spoiling Physics' Biggest News) as well as Tommaso Dorigo (and some coverage from Physics World featuring him).

From an even wider perspective, see Sabine Hossenfelder for The LHC "nightmare scenario" has come true and Natalie Wolchover's piece on the "nightmare scenario", What No New Particles Means for Physics (by the way, congratulations to Wolchover on some well-deserved awards, including this one). My own views on this are well known: this was in some sense a major theme of my book, and among other places I've written about this, see for example a 2013 Edge essay.

My perspective on this is in some ways similar to Hossenfelder's, but I draw very different conclusions, strongly disagreeing with her criticism of "reliance on gauge symmetry", and "trust in beauty and simplicity". This hasn't been what theorists have been doing for 30 years. The string theory unification ideology has led to an emphasis on extremely complex and ugly models, with gauge symmetry not a fundamental feature at all. Yes, she's right to point to "A failure of particle physicists to uncover a more powerful mathematical framework to improve upon the theories we already have", but I'd argue that that failure is due to an insistence on looking in the wrong place.

Wolchover's piece captures some of the current angst well, for instance quoting Maria Spiropolu about SUSY as follows:

> "We had figured it all out," said Maria Spiropulu, a particle physicist at the California Institute of Technology and a member of CMS. "If you ask people of my generation, we were almost taught that supersymmetry is there even if we haven't discovered it. We believed it."

Arkani-Hamed is as quotable as ever, saying the lesson of all this failure is

> "There are many theorists, myself included, who feel that we're in a totally unique time, where the questions on the table are the really huge, structural ones, not the details of the next particle. We're very lucky to get to live in a period like this — even if there may not be major, verified progress in our lifetimes."

I suspect that there are quite a few physicists, of my generation and later, who don't necessarily feel "very lucky" to get to live in a period of no significant progress in the past 40 years, with the prospect of none in our lifetimes. It would be great if the "huge, structural" questions really were on the table, but I've seen little interest among theorists in such questions, beyond whatever the fad of the day related to AdS/CFT might be.

Also finishing up while I was away was the Strings 2016 conference, and for the state of the subject you might want to watch David Gross's "Vision" talk (video here, I had to download the whole thing to watch it, streaming was unusable). I think Gross was right in pointing to work on the so-called "Sachdev-Ye-Kitaev" model as perhaps the most interesting thing being discussed at the conference. This is an exactly solvable large-N quantum mechanical model exhibiting features of holography. A good place to start learning about it is Kitaev's KITP lectures here and here.

Gross also went over some history, noting that some current topics in string theory echo back to 1967 work of Mandelstam (and that Gross himself had written a paper at that time on this material, so next year will be a 50th anniversary of his engagement with the subject). As for the current state of the theory, his take is much the same as that he has talked about at many earlier Strings conferences: string theory is now a "framework" encompassing QFT and most of the rest of fundamental physics, but we don't really know "what string theory is". To me it's very unclear why one is supposed to believe so strongly in the overarching role of a set of ideas that aren't well-defined and have been an utter failure at explaining anything about particle physics. Soon he'll have to pay off his SUSY bets, but this doesn't seem to have changed his conviction that superstring theory unification is on the right track. He ended with his usual sign-off "The best is yet to come", but this time added a parenthetical "I hope it comes quick".

Another new Wolchover piece well worth reading is about Miranda Cheng and her work on modular forms and string theory on K3 surfaces. I think this sort of work on the boundary of mathematics and physics makes clear the problem with string theory and mathematics: things are complicated and ugly if you try and make the theory look like the real world. You get beautiful ideas and great mathematics when you ignore the supposed connection to the real world and go in another direction. The problem is that often, instead of pursuing good mathematical ideas where they lead, people doing this feel the need to stick to some connection to the failed idea. Here's what Cheng has to say:

> I personally always have the real world at the back of my mind — but really, really, really back. I use it as sort of an inspiration for determining roughly the big directions I'm going in. But my day-to-day research is not aimed at solving the real world. I see it as differences in taste and style and personal capabilities. New ideas are needed in fundamental high-energy physics, and it's hard to say where those new ideas will come from. Understanding the basic, fundamental structures of string theory is needed and helpful. You've got to start somewhere where you can compute things, and that leads, often, to very mathematical corners. The payoff to understanding the real world might be really long term, but that's necessary at this stage.

For another physicist still enthusiastic about string theory, see this interview with Brian Greene, somehow motivated by a sci-fi horror series, Netflix's "Stranger Things".

Comments

1. **Justin**
   August 10, 2016

   As the most influential string theorists like Witten approach retirement, should we expect to see a whole new generation of independent research ideas coming from the young theorists? Or will the influence of strings carry over into the next generation preventing research in other ideas? And how are we supposed to ever make progress if we are limited in what we can work on? Also, is this a problem unique to the US or do strings have a stranglehold in Europe as well?

2. **Bob**
   August 11, 2016

   To me, that fact that people are disagreeing about what should be fundamental and what we have too much of really means we need a period where everything is challenged and everything is explored.

Here's hoping we move into a period where wild speculation and exploration of new ideas is actually encouraged rather than sticking to paradigms that may be leading us nowhere fast.

When you reach a dead end in a maze, you have to take a few steps back to find another branch, no matter how gold-gilded the walls are.

3. **Bee**
   August 11, 2016

   Peter,

   I suspect the reason you are disagreeing with me is that I am not referring to string theory, but primarily to all the hep-ph clutter you find on the arXiv. If the diphoton story has demonstrated on thing it's that you can cook up hundreds of models to fit any bump that comes up as a fluke. This type of construction is hence arguably seriously underdetermined.

4. **Thomas Larsson**
   August 11, 2016

   Has anybody considered the possibility that the reason why the LHC found no trace of BSM physics might be that there is no BSM physics? Except for gravity, which is a separate story. With the Higgs mass right at the edge of the stability region, there seems to be no compelling theoretical reason why there has to be anything beyond the SM. On the contrary, the fact that the SM is living on the edge makes it more compelling to me than if it were deep inside the stability region.

   The standard counter-argument is that visible matter only makes up 5% of the universe's mass. However, recall that medieval astronomers knew that the universe was a clockwork with at least thirteen epicycles. Experimental data are always interpreted within some framework, be it epicycles or QFT+GR. QFT and GR are of course on much stronger footing than epicycle theory ever was, but we know that their marriage is an unhappy one, and if they break down anywhere , the large-scale structure of the universe is as likely a place as any.

5. **Bee**
   August 11, 2016

   Thomas,

   The problem with your idea is the statement that the "unhappy marriage" of QFT and GR is as likely to break down on (cosmologically) large scales as anywhere. On cosmologically large scales joining GR and QFT works just fine. The problems only become noticeable at very short distances, or high energies respectively. This doesn't mean that a combination of GR and QFT does *not* lead to long-distance modifications, but at least it isn't presently clear how or even why that should be so. And in any case, that would also constitute new physics.

   Having said that, of course it's been considered that there isn't any new physics until the Planck scale, it's called the "big desert". But you don't read much about it in the pop sci media because there isn't much to be said about it.

6. **Dave Miller**
   August 11, 2016

   Peter,

   It sounds as if you agree with Miranda Cheng's comment?

   Personally, it sounds sensible to me: the basic ideas of string theory are sort of intriguing, and it will be interesting to see them explored and fleshed out. Alas, connecting them to experimental physics seems to be a project for the distant future.

   Maybe the "string wars" will end in a truce along the lines Cheng suggests.

   Dave Miller in Sacramento

7. **Navneeth**
   August 11, 2016

   > **Thomas Larsson**: Has anybody considered the possibility that the reason why the LHC found no trace of BSM physics might be that there is no BSM physics?

   Why are so many people so quick to dismiss BSM physics so soon, despite the experimentalists' repeating that we have so far gathered only a fraction of the data the LHC was designed to generate?

8. **Maurice Carid**
   August 11, 2016

   The crucial question is "what do we learn from the unholy mess that the particle physics community got itself into?".
   I agree with Peter that I cannot comprehend the lessons Sabine suggests.
   Neither do I comprehend her answer to Peter:

   >I suspect the reason you are disagreeing with me is that I am not referring to string theory, but primarily to all the hep-ph clutter you find on the arXiv.

   Sabine, do you consider the "hep-ph clutter" you're referring to, to rely overly on gauge symmetry or to be beautiful and/or simple?! If not, it is no example for cases of too much "reliance on gauge symmetry" or "trust in beauty and simplicity" in the last decades, that Peter challenges you to cite.

9. **Jan Reimers**
   August 11, 2016

   Hi Peter, I am curious if you think the recent progress on amplitudes is a counter example to pessimistic statements like:

   Yes, she's [Sabine's] right to point to "A failure of particle physicists to uncover a more powerful mathematical framework to improve upon the theories we already have", but I'd argue that that failure is due to an insistence on looking in the wrong place.

   It would be great if the "huge, structural" questions really were on the table, but I've seen little interest among theorists in such questions,
   beyond whatever the fad of the day related to AdS/CFT might be.

In particular we can now calculate complex gluon tree amplitudes using a starting point (Grassmanian + twistor variables etc) that has no locality or unitarity and we see these features emerge in the final answer. At least for tree level we can't just wave this off as only applying to a toy model (planer N=4 SYM). For me this really does seem to be uncovering a more powerful mathematical framework to improve upon the theories we already have. At least it is a definitive step in that direction which can be rigorously checked against known results. Does this work also not count as a step towards the huge structural questions (emergent space-time and emergent QM)?

Thanks
JR

10. **Low Math, Meekly Interacting**
August 11, 2016

Hi, Naveenth,
Based on my read of the popular literature on the subject, while the quoted experimentalists are maintaining an admirably agnostic stance, the theoretical picture is pretty grim. Since the LHC has turned on (and arguably well before), the favored theory for BSM physics, SUSY, has been shown to be all-but-irrelevant to the major problems it was purported to solve. If SUSY kept the Higgs mass "natural" and supplied a candidate for WIMPs, at the very worst a credible bump should have appeared somewhere by now. Instead there's NOTHING. If anything like SUSY exists in nature, it either has to be horribly ugly to meet its original demands, or it's fine-tuned. The former seems unlikely, and the latter just adds exponentially to the mystery of nature's apparent "unnaturalness". There's little reason to think that SUSY will appear at energies accessible to any conceivable experiment. At best, there's no reason to think that feasible experiments are probing nature at energies that are in any way special. We would simply have to be extremely, incredibly, ludicrously lucky to live in a universe where BSM physics is accessible.
Presently, there's simply no compelling reason to think the LHC will see anything deviating from the SM. Maybe it will, but why should it? And if we are to build another, more powerful collider, what would it be for? Perhaps all we can hope to do is learn more about the SM Higgs. Is that enough to justify the tens or hundreds of billions of dollars that will be needed to build this machine? Can a vibrant field of theoretical and experimental particle physics be sustained by such an endeavor? I guess these are the major, and deeply troubling questions that need to be faced even now. Unless, again, we are very, very, very, VERY lucky.
Do I have this right?

11. **Radioactive**
August 11, 2016

Thomas – there is all that Dark Matter floating around in space…

Peter – Ads/CFT is a very long lived fad, going strong since 1997. And I am not sure what you expect theorists to do when there's a popular and promising direction, other than to pursue it and then give talks about it at conferences.

12. **Anonyrat**
August 11, 2016

IMO, we have to figure out "beautiful and simple" rather than "arbitrary and underdetermined" symmetry-breaking, if the fundamental theory is built with symmetries and dimensions that aren't apparent in our world.

13. **Peter Woit**
August 11, 2016

Justin,
There are now many generations of string theorists of all ages, the problem isn't Witten (whose influence is not what it used to be). Actually, Witten and those of his generation had a much wider training than many younger theorists, who often ended up with a rather narrow perspective.

These days, even "string theorists" mostly don't work on actual string theory. The faddishness of the field remains an ongoing problem, even as the fads evolve. The nature of the fads and prospects for doing other things vary a lot from place to place, hard to generalize much about "US vs. Europe" for instance.

14. **Peter Woit**
August 11, 2016

Bee,
We agree about hep-ph. I think a problem may be that lots of hep-ph work starts with a motivational claim about how beautiful SUSY is, how beautiful GUTs are, but then goes on to extremely complicated and ugly constructions (because the simple, beautiful ones don't look at all like nature).

15. **Peter Woit**
August 11, 2016

Dave Miller,
I don't agree or disagree with Miranda Cheng's comment, I quoted it as a good example of how string theorists working in that area think about their work. They realize there is no hope of doing something that connects with experiments (which is good, keeping them from working on failed ideas), but at the same time, the goal of getting such a connection to string theory motivates their research direction. The danger here is that this keeps them from pursuing directions that are mathematically promising (because they clearly don't lead to anything related to physics), while at the same time encouraging research directions that are mathematically unpromising, but promise some sort of physics connection (although I think these are mirages).

A good example of what I mean is Cheng's recent talk at the Strings 2016 conference, where one of her topics was counting IIB flux vacua, a subject motivated by dubious physics, and leading into a thicket of complicated mathematics.

16. **Peter Woit**
August 11, 2016

Jan Reimers,
Some of the amplitudes ideas you mention are an example of new structural ideas, and I'm sure that's something Arkani-Hamed had in mind. So, yes, that's a good example of an interesting research program. At the same time, there's a huge disjunction between what is actually going on in that subject, and claims that one is going to get something like "emergent spacetime and QM" out of it. Arkani-Hamed himself acknowledges that the long hype-filled motivational intros to his talks about this are something he does to keep himself from getting discouraged, more than actually grounded in reality. At the latest Strings, his technical talk was "Towards deriving string theory as the weakly coupled UV completion of gravity", and, like the Cheng example above, I suspect this is an example of devotion to string ideology sending someone in a direction quite different than where the actually interesting things being learned about amplitudes might be pointing.

17. **Peter Woit**
August 11, 2016

Radioactive,
I don't know if "fad" is the right word for something going on for nearly 20 years (or, even more so, something like SUSY extensions of the SM, at over 40 years). What I meant to indicate is a huge overemphasis on one idea, partly because everyone else is working on it. After 20 or 40 years, claims about "promising" are highly problematic, arguably one might instead take the point of view that once tens of thousands of papers and decades have been devoted to an idea, if its initial promise hasn't worked out, it never will.

18. **John**
August 11, 2016

The real life reason it's called the nightmare scenario is because the funding dollars will start to dry up and jobs will be lost. Governments and taxpayers will start to question why we need to keep funding the LHC if there is nothing else to find.

On the bright side some funding should free up for new concepts and ideas that are different from the direction that has been pushed for 20+ years.

19. **Cormac O'Raifeartaigh**
August 11, 2016

Hi Peter,
as I said elsewhere, I remember Dad used to worry quite a bit about the possibility of a 'desert' in accelerator physics after the discovery of the Higgs (unlike his colleague Julius Wess, who felt that the first SUSY discovery was just around the corner).
The frustration is understandable from a theorist's point of view but the situation is a little different for the experimentalist. After all, it's reasonable to explore the next energy frontier and see what shows up, at least for a while. As my old supervisor used to say, experimental physics works primarily by ruling things out.
Of course, it's possible particle physics may find itself in the same situation as GR in the 40s and the 50s for some years. After all, we can't expect experiment to always track faithfully, there will always be periods of drought. And maybe it won't be such a bad thing if more theoreticians and experimentalists focus on other areas such as condensed matter physics for a while. Many important breakthroughs were made there before, as you know.
My feeling is that the next big discoveries may come in observational cosmology – hopefully these will show the way for the next generation of particles
Regards, Cormac

20. **WLM**
August 12, 2016

It would seem that one aspect of this "ultimate catastrophe" is that, as recently as a couple of years ago, almost everybody in hep-ph thought that finding the proverbial needles in the haystack would be easy: you just had to sit down on that metaphorical haystack (the energies at the LHC) and the needles (new particles) would poke you in the ass. Turns out that was wrong. Super-symmetry isn't just harder to find, it may not even exist. So BSM physics got much harder to identify; if the LHC is going to expose anything, it will probably be fairly subtle, maybe not at all obvious. One of the things that David Gross said recently applies here (paraphrasing): If we are now going to find maybe only a single needle hidden within a single straw somewhere within that haystack, we need to understand the haystack very, very well. That's what the LHC can help to provide, including the Alice experiment, not just CMS and ATLAS.

On the other hand, the push to investigate N=4 super-symmetric, non-interacting, infinite color charge, conformal versions of QCD, as something that's sort of close to the real world if one could only somehow insert quarks into it and break super-symmetry to get it back to reality, seems like the "more of the same" that both you and Bee are referring to. The main reason that this approach is "interesting" and "shows promise" seems to be that it looks like it may someday have some connection to a version of String Theory, not that it is actually about physics. Seems like what an old federal judge friend of mine used to refer to as "mental masturbation" — something that used to appear to be more common in the law and in philosophy than in physics :-).

21. **Radioactive**
August 12, 2016

I guess I would agree with that Peter. However if you dislike hep-ph and hep-th 'clutter' then the academic environment is much more at fault than any perverse desire on the part of the average researcher to doggedly pursue failed ideas. The non-stop treadmill of competition for new PhDs, new postdocs and new staff with very little flow out the top and the pressure to publish 2-3 papers a year and get cited means no time to start whole new research directions or take risky decisions. With a healthy experimental situation – eg. the 70s in particle physics, right now in gene editing – this environment probably works quite well for making rapid progress.

Regarding switching from elementary particle theory to condensed matter. Many people, young and old, I have interacted with seem to view theories about fundamental particles as not just intellectually, but morally superior. I think the continued existence and even growth of apparently moribund ideas will continue as long as this view persists.

22. **anon**
August 12, 2016

I wouldn't consider the situation in life sciences (e.g., gene editing) as quite that good. There's a huge oversupply of recent PhDs who are slaving in low-paid (usually a paid a lot less than in physics) postdoc positions for years. And there's no shortage of of bad (inconsequential, plain wrong or in worst cases downright fraudulent) papers.

23. **Mitchell Porter**
August 12, 2016

WLM, see e.g. arxiv:1607.02843 for fruits of N=4 research.

24. **Thomas Larsson**
August 12, 2016

Navneeth:
Yes, the LHC have only gathered a fraction of the data at full energy, but IIRC statistical errors only decrease with the square root of the integrated luminocity, so error bars will only go down with one order of magnitude or so. Besides, there are other experiments, like PEDM and proton decay, which also point at no BSM physics.

Radioactive:
As I said, 95% of the universe's mass is dark, *if* you interpret the observations within the framework of QFT+GR. But my impression is that most plausible models of dark matter lead to BSM physics at LHC scales, so there is a cognitive dissonance with the null results from LHC. But a modification of the QFT+GR framework would of course also be a kind of BSM physics.

Bee:
People talked about the Big Desert back when I was a student, but nobody believed in it then. And it is incompatible with the standard interpretation of dark matter and energy, right?

My argument against BSM physics, apart from the obvious fact that it is disfavored at the LHC, was the criticality of the Higgs mass. This is not my idea – the authors of arXiv:1307.3536 call it the principle of living dangerously.

25. **Jonas**
August 12, 2016

Thomas Larsson,
As far as I understand things the non-zero neutrino mass is BSM, so we already have such data. It's just not related to SUSY or similar things.

26. **NoGo**
August 12, 2016

Perhaps one more direction where BSM physics might come from (apart from colliders, comsology, and neutrno detection):

http://arstechnica.com/science/2016/08/researchers-orbit-a-muon-around-an-atom-confirm-physics-is-broken/

27. **Shantanu**
August 12, 2016

Thomas, forget dark matter. But don't neutrino oscillations already tell us there must be BSM physics? After all that's why last year's nobel was given

28. **Peter Woit**
August 12, 2016

All,
The neutrino mass story is complicated, I don't think "BSM/non-BSM" is very useful. One project planned for when I have some time is to get unconfused about some aspects of that story, maybe will write about it here if I manage that.

NoGo,
For, this, there's the ever reliable and topical Natalie Wolchover, with
https://www.quantamagazine.org/20160811-new-measurement-deepens-proton-radius-puzzle/
which includes

"Still, Pohl is highly skeptical that the puzzle is evidence of new fundamental physics."

29. **Anonymous**
August 12, 2016

"I suspect this is an example of devotion to string ideology sending someone in a direction quite different than where the actually interesting things being learned about amplitudes might be pointing."

What is the direction that the actually interesting things being learned about amplitudes might be pointing? Is there any technical reason for this comment?

30. **Peter Woit**
August 12, 2016

Anonymous,
I'm no expert on amplitudes, so,it would be silly of me to claim to know technically what people studying amplitudes should be doing. It seems to be a rich subject with many possibilities. The point of the comment was just that using the subject to try and find a way to make a dubious nogo argument for string theory is the kind of thing you get into if you are enthralled by a dubious ideology rather than the logic of the subject itself.

31. **tommaso dorigo**
August 15, 2016

Hi Peter,

thanks for the link. Maybe something that other commenters have not pointed out here is the importance of the present status of things for the next forty years. We are in a difficult situation as we need to plan the next big machine now, and we do not know what is the right thing to do. Since experiments are not giving any hint of what direction to take, it seems natural to go for a precision machine that will measure the Higgs boson properties in detail – an e+e- collider reaching several hundred GeV but not more. That thing will improve our knowledge of what we know already, but will most likely not discover anything new, like the much glorified LEP and LEP-II.

If no investment is made in the near future in a big new hadron collider (which should have one order of magniture larger energy to make any sense) or an entirely new-concept muon collider (which is very challenging and probably as of yet unmotivated), we risk that humanity remains stuck for a long while; in the meantime, we will lose a whole generation of machine developers and detector builders, without a chance of transferring their know-how to the new generations.

(I must say that regardless of the above threat, I remain of the opinion that no choice should be made now, as we should rather keep waiting for new developments in the neutrino sector and in other related fields of experimental physics before committing to building some new big toy that might not be the right thing).

In summary, the situation may look good to NAH, but experimentalists have little to rejoice here.

Cheers,
T.

32. **vmarko**
August 15, 2016

Hi Tommaso,

"Since experiments are not giving any hint of what direction to take, it seems natural to go for a precision machine that will measure the Higgs boson properties in detail"

It seems to me that the experimental results from the LHC are actually giving a very strong hint which direction to take next — study IR physics, maybe also Higgs precision measurements, and abandon high energies for now. Certainly, it's a gloomy prospect, but a precision-measurements machine can maintain the knowledge base and the craft of building particle colliders until there is a development suggesting there is something to be found at higher energies.

In my opinion, we are better off developing methods to study the high energy frontier by catching cosmic radiation. Only there we stand a chance to find out that there is something interesting to look for, and at which energy scale to expect it. And only once we find that out, it makes sense to build another collider, at the appropriate energy scale to study it. I know, it's tough, luminosity from cosmic rays sucks, but the no-new-physics situation at the LHC leaves us little choice, IMO.

Best, 🙂
Marko

33. **Peter Woit**
August 15, 2016

vmarko,

My impression is that it's not just that the luminosity from cosmic rays sucks, but that, at center of mass energies above the LHC scale, it's useless for since you're highly unlikely to see any events probing very short distances.

To get more precise information about the Higgs, one needs a new high-energy machine. A lepton collider that could produce and study Higgs particles would have lower beam energy than the LHC, but still would be higher than LEP, extending collider technology to higher energies.

34. **NoGo**
August 16, 2016

Peter,

Thanks for the link to Natalie Wolchover's piece. Indeed, it is very informative, and is far more level-headed than other coverage of these experiments I've seen.

Still I wonder, perhaps naively, if some significant progress towards BSM can be made by improving sensitivity of detectors, and increasing calculation power of computers used, rather than "smashing things harder". (I confess that the sensitivity achieved by LIGO still blows my mind… Of course it's a different area of research, but demonstrates how far one can go). Isn't it true that the effect of "heavier" quantum fields can be in principle detected even if colliders lack the energy to actually create particles of such fields?

I admit that what made me jump up and down about muonic deuterium experiment is that, if it turned out BSM physic, it might shed some light on generation problem. As a non-physicist, again perhaps naively, I find the generations to be the most unsatisfactory part of SM, evidence for something else. I can in principle buy (with cringed nose) anthropic explanation for the rest of SM feathers, but not that one…

35. **Peter Woit**
August 16, 2016

NoGo,
In essence what these experiments are doing is creating a highly excited state (by colliding protons), then looking at the long-lived states this decays to, and using this to study the physics at a high energy scale (in principle one might indirectly even get information about scales higher than the accelerator is producing). The detectors are quite sensitive in the sense of identifying the decay products, more sensitivity of this kind I don't think would help much. The problem is the large number and complexity of the decay products, making it very challenging to extract useful information about the processes that produced them. So, the sensitivity challenge is that of extracting a small interesting signal from a much larger uninteresting one. This is what HEP experimental analyses are all about, and their methods are highly sophisticated, using as effectively as they can a huge amount of computational power.

Yes, the generations are a great mystery and in principle a different behavior than expected of muons could shed light on it. But it's very hard to think of what could plausibly explain such different behavior, at low energy scales that we think we understand well. Thus the skepticism about a non-Standard Model explanation of this.

36. **GoletaBeach**
August 17, 2016

Nature is natural by definition, and if there are no thresholds of new physics crossed at the LHC, that must be perfectly natural.

It is perfectly fine for the neutralino of SUSY to be far lighter than all the other superpartners. The photon, mostly the neutralino superpartner, is far lighter than all the normal partners.

A 1 TeV neutralino is perfectly fine for even the absolutely simplest SUSY phenomenologies, with all other superpartners out of reach of buildable colliders. That neutralino might be seen in the direct dark matter experiments or the indirect ones.. no problem.

https://idm2016.shef.ac.uk/indico/event/0/contribution/32

Or it might not. Whichever it is, all perfectly natural, because natural always is what nature chooses.

37. **TS**
August 17, 2016

I am reading the exchange to say that no one around believes that B-factory experiments will discover anything worthwhile? Well, make that singular, as there's only going to be Belle II. Anyway, did the biggest new particle accelerator experiment slip everybody's mind?

I can't help wondering what B-physics could achieve if as much brain power was devoted to it on the theory side as is to high-energy phenomenology or string theory.

38. **Francis**
August 18, 2016

David Gross video at Strings 2016 is now available in youtube
https://www.youtube.com/watch?v=abnu7ZbzvLQ

39. **Douglas Natelson**
August 19, 2016

I know that high energy folks are desperate for some new physics, but let's hold off on the ritual sacrifices for now, ok?
https://www.theguardian.com/science/2016/aug/18/fake-human-sacrifice-filmed-at-cern-with-pranking-scientists-suspected

Various News

August 19, 2016
Categories: Uncategorized

Before turning to other topics, congratulations to my Columbia colleague Wei Zhang, who was awarded the Gold Medal at the recent ICCM in Beijing.

On the HEP physics front, some news is:

- On Monday at 1:30pm Danish time, at the conference on Current Themes in High Energy Physics and Cosmology that is part of the Simons Program at the Niels Bohr International Academy, there will be an event adjudicating the bet on SUSY first made in 2000 (described here, the wager is here).

  Nima Arkani-Hamed will act as referee, a bit unconventional since he's on one side of the bet. On the other hand, it's clearly the losing one and I have no doubt he'll concede graciously. Others who may be heard from as part of the event include David Gross and Gerard 't Hooft.

  Video streaming should be available at this site, I guess the time is 7:30am New York time, maybe I'll be up and watch during breakfast…

- In related news, Frank Wilczek has conceded loss on a similar bet he made back in 2009 with Garrett Lisi, see here.
- As for other similar bets I'm aware of, Lubos Motl is about to lose his bet with Adam Falkowski, and David Gross should be losing his with Ken Lane within the next year (perhaps he'll comment on this on Monday). The interesting story to watch here will be whether this changes anyone's behavior. Will Gross, Wilczek and others continue to point to SUSY extensions of the Standard Model as the promising future of the field, or will they acknowledge that this is an idea that hasn't worked out?
- In other HEP news, the "nightmare scenario" seems to have driven physicists at CERN to try human sacrifice to propitiate the angry Gods who are tormenting them in this manner.
- There's a conference going on in Banff organized by FQXI, and you can follow it to some extent on Twitter. I'm somewhat of a skeptic about claims to have deep new insights into physics based on what seem like simple, vague natural language arguments. Not clear if the 140 character limit of Twitter is a good or bad thing in trying to capture such arguments.
- Another of the vague sort of claims I'm dubious about is the "ER=EPR" one described here. On the other hand, this at least appears to have important applications.
- Also on the dubious HEP news front, there have been lots of news articles recently about a supposed new "Fifth Force", generated by a press release from UC Irvine promoting a PRL paper by UCI theorists. This story had been debunked a couple months ago by Natalie Wolchover at Quanta (see here).

  The usual hit on science journalism is that the work of scientists is hyped and misrepresented by journalists, but I think this shows an all too common example of the real problem: hype from physicists and their institutions (egged on by PRL), with journalists trying to hold the line against it (and not always succeeding).

- Finally, there's a wonderful interview with Edward Witten (part of a TV program in Dutch) that I would guess was filmed in ~~1999-2000~~ and is available here. Witten is there quite optimistic about the prospects of string theory, and of course I'm curious whether and how the intervening years have changed his point of view. On other topics the interview is quite fascinating, doing an unusually good job of getting the normally reticent Witten to talk a bit about life and wider issues (as well as demonstrating an admirable refusal to get provoked into pontificating about various questions he's not expert in).

Update: For a detailed explanation from one of the theorists working on the supposed "fifth force", see here.

Comments

1. **NoGo**
   August 19, 2016

   Well, if I correctly understand Natalie Wolchover's article, the fifth force "discovery" is not completely dead yet:
   [[Their history "is reason for pause," Thaler said, but "I still think follow-up studies on this intriguing anomaly should definitely be performed."]]
   Although given the history of Atomki group as described in the article, I am not holding my breath…

2. **Peter Woit**
   August 19, 2016

   NoGo,
   I think if one reads the full article, there are some devastating comments from experts, with "Their history "is reason for pause," " a polite version of "this is almost certainly nonsense".

3. **Peter Woit**
   August 19, 2016

   NoGo,
   By the way, at least one of the authors has a history with this kind of press release, see
   http://www.math.columbia.edu/~woit/wordpress/?p=335

4. **Shecky R**
   August 19, 2016

   FWIW, John Horgan noted in a recent column that Witten had told him in a 2014 interview that he still thought string theory was "right" and would be validated in time.

5. **Peter Woit**
   August 19, 2016

   Scheky R,
   I know Witten is still optimistic that string theory will in some sense work out, but I'm curious what effect the negative results of the last nearly 20 years have had on how he sees this. The Dutch interviewer did a great job of getting him to talk about various things, maybe he's willing to go back to the IAS for an update…

6. **Robert Garisto**
   August 19, 2016

   > egged on by PRL

• The experimental paper is subtitled "Possible Indication of a Light, Neutral Boson," not evidence or observation. That is a weak claim.
• The abstract says that it "could possibly be due to nuclear reaction interference effects or might indicate that, in an intermediate step, a neutral isoscalar particle".
• The first phrase is not present in the preprint for the paper…
• We did not highlight either paper or issue a press release on them.
• Editors get to, in the end, accept or reject.
• We do try to be sure claims match the evidence, as in this case. Did they discover a new boson? Possibly, but probably not. The paper does not claim discovery of a new boson.

7. **Tim M.P. Tait**
August 19, 2016

Peter,

In addition to Robert Garisto's comments, which are all accurate and fair, as far as I can tell, I am deeply disturbed by the way you choose to portray the experimental results by Krasznahorkay et al which appeared there or our theoretical interpretation. Both are suitably cautious and do not claim a discovery. I defy you to find a quote or interview by any of the scientists involved in which the need for verification is not stressed. So where is the hype? It's not coming from the scientists.

You are grossly mischaracterizing the article by Natalie Walchover in Quanta magazine, which, while it does contain several sobering quotes from experts, by no means "debunks" anything. All but one had nothing more to say than that there were earlier reports at a different mass. Are those earlier results inconsistent? We don't even know because no one has mentioned the uncertainty on the earlier determination. Particle and nuclear experiments recalibrate their energy measurements all of the time. One of the experts she quoted from the Michigan State cyclotron said much stronger things, but backed them up with no specific detailed criticism. That's hardly convincing.

Extraordinary claims require extraordinary evidence. No one has made such a claim, and the scientists involved have all agreed that more evidence is needed. Before you start declaring it all to be hype and assigning blame, it would be more convincing if you actually had a clear idea of who has said what and presented the situation as it is, not as you think it should be.

8. **Peter Woit**
August 19, 2016

Tim Tait,
I strongly disagree with you about the Wolchover article, which I think is an excellent piece of reporting and makes a devastating case that this is an experimental result that one should be extremely skeptical about. I won't go through what I think you have wrong, but here's one specific example: you say "no one has mentioned the uncertainty on the earlier determination" but if you look at the paper it says 13.45(30) MeV (compare to the latest claim of 16.70 +/-.35+/-.5 MeV).

Given such skepticism, experts can argue about whether PRL should have published the experimental result, but I don't think anyone can seriously argue that the stories all over the press about a new fifth force of nature are anything other than highly misleading. And they're all due to the "UCI physicists confirm possible discovery of fifth force of nature" press release. Do you really think that press release with that headline was a good idea?

Robert Garisto,
Maybe I'm wrong, but I was under the impression that PRL encourages authors to have their institutions issue press releases like the UCI one when a paper is published. If I'm wrong I apologize. If I'm right, what do you think PRL should do about misleading press releases about PRL papers? What if this is a repeated phenomenon? I'm referring to the Northeastern press release discussed here:
http://www.math.columbia.edu/~woit/wordpress/?p=335

9. **Tim M.P. Tait**
August 20, 2016

Peter,

To be clear, I do not have any problem with the Wolchover article, just with your mischaracterization of what it says.

Furthermore, the claimed uncertainty on the curren mismeaurement is not what is relevant here. What is relevant is the claimed uncertainty on the previous claim.

Finally, again, if you wish to claim that the scientists are the "bad guys" here, point to one place where they overhyped the situation. If you have no example, then concede that this part of your statement is unfounded.

10. **Peter Woit**
August 20, 2016

Tim,
The most egregious and problematic example of hype here is UCI putting out a press release with the headline "UCI physicists confirm possible discovery of fifth force of nature". It seems to me that scientists have some responsibility for this kind of thing. In particular, given the long history in this field of such overhyped press releases, anyone dealing with their institution's press office should be well aware of the potential problem and take steps to make sure this doesn't happen.

11. **Frans Langelaan**
August 20, 2016

A big thank you for the link to the Witten interview, which in turn lead me to a DVD box of "Van de schoonheid en de troost" (dutch for "Of beauty and consolation) which consists of 14 dvd's with interviews (each appx. 1.5 hours long, by Wim Kayzer, the same who interviewed Edward Witten) with Wole Soyinka, Roger Scruton, Jane Goodall, George Steiner, Vladimir Ashkenazy, Steven Weinberg, Martha Nussbaum, Karel Appel, Edward Witten, Elizabeth Loftus, Rutger Kopland, Gary Lynch, Stephen Jay Gould, Dubravka Ugresic, Simon Schama, Catherine Bott, John M. Coetzee, Richard Dufallo, Leon Lederman, Rudi Fuchs, Tatjana Tolstaja, Freeman Dyson, Richard Rorty, György Konrád, Germaine Greer, Yehudi Menuhin.

It must have aired here in the Netherlands some years ago, but I suspect, as is often the case with programs of this quality, at a time when I was fast asleep in bed. I wasn't aware of the existence of the show, but Wim Kayzer has a pretty terrific reputation and I could not resist ordering the box right away, looking forward to long winter nights…

12. **Anon**
August 20, 2016

If you listened carefully to –all of– the interview with Witten you'd have immediately concluded this *could not* possibly be 99-00, but *significantly* earlier. It is amusing to see his consistency regarding consciousness…

13. **Frans Langelaan**

August 20, 2016

The series of which the Witten interview was part of dates to 2000.

14. **Peter Woit**
August 20, 2016

Anon,
The video was broadcast in 2000. Witten at some point refers to the current situation as three years or so after a big breakthrough, with understanding of that now consolidating, (I'm paraphrasing from memory). At first I thought he might be referring to AdS/CFT, but more likely he was referring to M-theory and 1995. So, my best guess would be 1998 or 1999, but if anyone has a source for the actual date, that would be interesting.

15. **Anon**
August 20, 2016

I wonder if Kane will also be giving conceding defeat on SUSY?

He wrote on Tomasso's blog in December 2015:
http://www.science20.com/a_quantum_diaries_survivor/supersymmetry_is_about_to_be_discovered_kane_says-160367

"Now that we can predict the gluino mass from compactified M theory we know that superpartners should not have been expected in LHC Run 1 because the gluino mass is about 1.5 TeV, and it will appear in Run 2 once the luminosity gets over 15-20 fb-1."

16. **A Nony Mouse**
August 20, 2016

I didn't know that the death of a model was dependent on bets.

17. **Anon**
August 20, 2016

"So, my best guess would be 1998 or 1999, but if anyone has a source for the actual date, that would be interesting."

It was probably around 1996 judging by his discussion of the CC problem –clearly pre-1998– and the age of his children…

18. **Michael Shain**
August 20, 2016

Regarding the CC, in the interview Whitten says "suspecting the answer is zero guides a little bit my choice of attempts to solve problems". I wonder how approach to problem solving changed after 1998?

19. **Nope**
August 20, 2016

In the same video series there one with Freeman Dyson
https://www.youtube.com/watch?v=rBSnCfAcaek
and shortly after the 30 minutes mark it is stated that this interview happened in 1997. It seems reasonable to assume Witten's interview is from the same year.

20. **anon**
August 21, 2016

I found the interview with Witten very embarrassing to watch at the end: Witten would give a complete answer, the interviewer couldn't think of anything further to immediately to ask, long awkward silence with the camera still focused on Witten, Witten squirming with embarrassment.

The interviewer then got very personal by asking Witten about whether the holocaust affected him, which it obviously did looking at how uncomfortable he was with even thinking about the question. Again, awkward pause from interviewer.

21. **Peter Woit**
August 21, 2016

anon,

I think you're misunderstanding what the interviewer was doing. Witten is often quite reticent in conversation and the interviewer was trying to push him to say more. The interviewer's tactics made for some awkwardness, but I think he did get Witten to say more than he usually would have on some topics, which was interesting to hear.

22. **Anonymous Coward**
August 21, 2016

According to this webpage of the Netherlands Institute for Sound and Vision, the air date of the Witten interview is Feb 27th 2000.

http://zoeken.beeldengeluid.nl/internet/index.aspx?chapterid=1164&filterid=974&contentid=7&searchID=2169206&columnorderid=-1&orderby=1&itemsOnPage=10&defsortcol=12&defsortby=2&pvname=personen&pis=

23. **MathPhys**
August 21, 2016

I agree with anon that watching the interview was embarrassing in the sense that I felt that Witten was less than well treated. Witten made it perfectly clear that he has nothing to add regarding the Holocaust. Then there were these long pauses during which I'm fairly confident that Witten expected either a change of topic, or a cut in the filming. Instead, the filming crew pressed on. They were no doubt expecting to be able to film him get emotional. Witten's performance was impeccable. As a scientist, he was obviously happiest whenever he could steer the discussion away from personal matters and manage to talk about the universe which is "a wonderful place" that filled him with "awe", etc.

24. **Robert Garisto**
August 22, 2016

Peter:

The most important thing we do is to try and ensure that the claims in the published PRL itself are responsible and reflect the evidence presented. The paper is the version of record, not anything in the press.

There is a generic statement in our acceptance letter to authors suggesting they contact their press people. After all, dissemination of physics is one of our mission goals. It goes without saying that we do not encourage public statements that over-state results. When we are involved in the publicity ourselves, either through our outlets such as Physics (physics.aps.org) or through collaborating with the authors' press offices, we do our part to ensure that the message is scientifically accurate and not overhyped. Of course, we do not have control on the message when it does not involve us.

I did try to combat this particular instance: https://twitter.com/RobertGaristo/status/766331929348145152

25. **Tim M.P. Tait**
August 22, 2016

Peter,

To be honest, I hadn't noticed the title of the UCI press release before now. (I was asked to look over a draft of text before it was posted, which I think is fine as it is, but not the title, and I overlooked it when it appeared on line). Even in the era of click-bait titles running rampant, I am not in favor of that phrasing. I can ask for it to be changed, but that article (like any piece of scientific journalism) is the property of its author, and I have no standing to demand anything of it.

I still think your blog entry does a disservice by accusing many people of many things which they are not responsible. Robert has addressed PRL's role as far as the press is concerned, and has only touched on the fact that the articles which appear in PRL are subject to peer review involving at least two anonymous referees. That process is hardly fool-proof, but compare the track record of PRL with something like Science or Nature, and you'll find that over-statements leading to retractions are not the norm in PRL articles. Similarly, the scientists themselves have been measured and cautious in every communication that I have seen, and you have not provided an example to the contrary.

Finally, what I consider the most serious problem with what you wrote is in regard to the work of the Hungarian group led by Krasznahorkay et al. The article by Wolchover (which apparently both of us thought was well done) does not "debunk" anything. You saying that it does is not fair to her, nor to its subject matter. It contains one very negative comment (and several cautious ones), but that comment itself does not offer any informed indication as to why one should discount Krasznahorkay et al.'s work. So either it is his uninformed opinion, or somehow the reasoning was not included in the text of the article. I have no way to know which it was, but given the over-all thoroughness I see in other parts of that article, I suspect the former. If there are any serious flaws in the work of Krasznahorkay et al., someone should point them out. Taking random potshots at them without being informed of what they did or did not do wrong does nothing to advance scientific understanding.

So while I concede that there is a certain amount of hype in the title of the UCI press release, I would suggest that by over-stating the evidence against the result, you are engaging in a different kind of hype. I would be in favor of reducing over-statements on all sides.

26. **Peter Woit**
August 22, 2016

Tim,
It was not my intention to criticize here the PRL papers (in particular I have zero competence with regards to evaluating the experimental one). I think we're just going to have to disagree about the implications of Wolchover's story on one's evaluation of the credibility of the experimental result. Yes "debunk" is strong, but as a short characterization of the situation I think it's defensible.

Obviously what I wrote about this is motivated by a pet peeve, due to seeing way too many examples over the years of highly misleading press stories like this latest series. These things almost always come from the same mechanism: PRL paper generates problematic press release generates misleading stories. For HEP theory PRL papers that generate press releases, it seems to me the majority lead to misleading stories (unless they're completely ignored). This is not good at all for the public understanding of science. One can argue that the fault is that of the journalists who write the misleading stories, which is part of the problem, but I do think the phenomenon of bad press releases is a big part of the problem, and both PRL and physicists involved need to be aware of this and think about what they can do about it.

One starting point would be to first think twice about whether a press release is really a good idea. PR people have an agenda, and it's not caution and scientific accuracy. Once a piece is written, quite likely an editor is going to read it quickly and put the most attention-getting headline on it that they can think of, this is their job. Some non-expert journalists getting the press release are going to pay more attention to the headline than to the content, and are going to miss carefully hedged caveats. It seems to me that this case shows what goes wrong pretty clearly, would be a good idea if people involved, from PRL on, think about what can be done to improve the situation.

Not Ever Wrong

August 22, 2016
Categories: Uncategorized

The "SUSY Bet" event in Copenhagen took place today, with video available for a while at this site. It appears to be gone for the moment, will put up a better link if it becomes available. An expensive bottle of cognac was presented by Nima Arkani-Hamed to Poul Damgaard, conceding loss of the bet. On the larger question of the significance of the negative LHC results, a recorded statement by Gerard 't Hooft (who had bet against SUSY), and a statement by Stephen Hawking (not in on the bet, but in the audience) claimed that if arguments for SUSY were correct, the LHC should have seen something, so they think nature has spoken and there's something wrong with the idea.

The losers of the bet who spoke, (Arkani-Hamed, David Gross and David Shih) demonstrated the lesson about science that supersymmetry and superstring theory have taught us: particle theorists backing these ideas won't give up on them, no matter what. They all took the position that they still weren't giving up on SUSY, despite losing the bet. In more detail:

- Arkani-Hamed was not a signatory of the original bet in 2000, but signed on to the later 2011 version. He explained today that at the time he thought chances of SUSY visible early on at the LHC were just 50/50 (with his 2004 work on split SUSY motivated by realizing that pre-LHC the conventional picture of SUSY at the electroweak scale was already ruled out). He attributed his decision to take the pro-SUSY side of a 50/50 bet to "optimism", implying that this took place at a conference dinner where there may have been too much to drink. In his split-SUSY scenario, SUSY may yet show up at the LHC, or it could even be invisible there, requiring a higher energy accelerator. So, he's not giving up on SUSY based on LHC results.
- David Gross also is not giving up, arguing that fine tuning of a factor of 100 or 1000 is not a problem (invoking the large ratios that appear in the fundamental Yukawa couplings). He did say that young people might want to take this as reason to look for new ideas, but, for himself, felt "I'm too old for that".
- David Shih isn't giving up either, arguing that there still was lots of data to come, plenty of room for SUSY to appear at the LHC, still believes we'll discover SUSY, at the LHC or elsewhere.

One piece of misinformation promoted by several of the speakers was the idea that "everyone" back around 2000 believed in SUSY as the next new physics to be found. In my book (written in 2002-3) I wrote a long section about the evidence against SUSY, and, of course, if you look at the bet under discussion, in 2000 many more people (16 vs. 7) were taking the anti-SUSY vs. pro-SUSY side (at least in Copenhagen, but I think this reflects the general range of opinions).

No one today asked the obvious question "Is there any forseeable experimental data that would cause you to decide that SUSY was an idea that should be abandoned?". I'm now not seeing any prospect in my lifetime of anything that would cause these or other SUSY proponents to give up (John Ellis has also announced that no matter what the LHC says, he's not giving up). Unfortunately "Not Ever Wrong" is clearly the slogan of the (minority) segment of the particle theory community that long ago signed up for the vision of fundamental physics in which SUSY plays a critical part.

Update: There's a blog entry from Natalie Wolchover about this. She has more detail about the final remarks from Gross that I mentioned:

> "In the absence of any positive experimental evidence for supersymmetry," Gross said, "it's a good time to scare the hell out of the young people in the audience and tell them: 'Don't follow your elders. … Go out and look for something new and crazy and powerful and different. Different, especially.' That's definitely a good lesson. But I'm too old for that."

Update: Video of the Copenhagen event is available here.

Update: I happened to be looking at Michael Dine's 2007 Physics Today article on string theory and the LHC, noticed the side remark that "The Large Hadron Collider will either make a spectacular discovery or rule out supersymmetry entirely." I wonder if he still thinks this, and whether we'll ever see Physics Today publishing something updating its readers.

Update: Yet another news story about this, from Science News.

Comments

1. **GoletaBeach**
   August 22, 2016

   So… what experiments should experimentalists do, Peter? Should we fold up shop and give up because some theorists lose bets?

   I think there has never been doubt that going to the energy frontier is always the best experiment to perform. Mel Schwartz used to emphasize this point; he did lots of clever experiments, from being the prime mover in the nu_mu discovery to (pi mu) atoms to very clever exploitations of the SLAC facility in doing K0L physics.

   The argument for the energy frontier among experimentalists never depended on some theoretical bets… primarily it depended on discovery potential for something completely new. To the extent theorists have enabled marshalling resources through hype to get the machines built, they are useful. If they are wet blankets they should be ignored.

   If they overhype… well… James Watson's rule kicks in… if you overhype and are wrong, the only penalty is embarrassment, and there is always a chance your wrong hype will stimulate the right idea. If you underhype you risk 1)the right idea might never germinate and 2)the needed experimental work might very well never get performed.

   How hilarious it has been to read "Tunnel Visions" and read some of the rationale in 1993 for killing the SSC. The need to focus on economically remunerative research was a big argument. Meanwhile, the single biggest economic force since 1993… the world wide web… was invented at CERN to assist particle physics. Irony factorial.

   The non-signals at the LHC tell us that a proton collider should be the next energy frontier machine, not a lepton machine. Precision Higgs studies are the role of e+e- in the future. Prepare, Peter, for all the hype from people you find frustrating to get those projects done. It will take decades, but the allure of the energy frontier will never die.

   Meanwhile lots of clever low energy experiments are underway. Likely, though, that there will be the usual fights to cease basic research and spend all scarce research funds on a new Facebook or something. Perhaps that is what the result of your negativity might be, Peter.

2. **Urs Schreiber**
   August 22, 2016

   It is good to remember the difference between the variants of "low" energy susy checked at LHC, and high energy susy, namely supergravity. No known mechanism exists which would force a supergravity theory to settle in a vacuum that preserves a global supersymmetry (which is what the LHC could see), while there are excellent theoretical arguments for supergravity itself, i.e. for high energy susy: Besides sugra being a prediction of spinning strings (the spectrum of the spinning string miraculously exhibits spacetime supersymmetry) there are some strong general mathematical arguments, such as Deligne's Tannakian theorem on tensor categories and its implication for supersymmetry.

3. **Low Math, Meekly Interacting**
   August 22, 2016

   Hi, Goleta Beach,

   I don't think you're being realistic here, and Watson's nonsense about overhyping is directly refuted by the example of the SSC. In retrospect, it actually took outrageous hype to defend the thing, and it still got killed. By the standards of any other field in science, SUSY has completely and utterly failed as a "natural" explanation for the Higgs mass, and it has failed equally to supply a realistic WIMP. It's only in HEP, apparently, that a theory can crater so dramatically and still be tweaked in such a way as to receive endless and unconditional support. One never encounters anywhere else such boundless trust in "beauty", as judged by a miniscule fraction of the overall physics community.

   They may be right. The universe may indeed be supersymmetric. But at what energies should it be sought for, if not the TeV scale? The LHC got funded because there was such a high degree of confidence that the Higgs (be it fundamental or composite) would be discovered at accessible energies. And there was a ton of hype about SUSY to ice that cake. Now the Higgs has been discovered, and it's not supersymmetric. Rather, it's super-standard. If SUSY exists, it's fine-tuned to at least some degree, we now know that much. But by how much? How can anyone answer with confidence anymore, except to say it could be any value up to energies we will never be able to probe? Has anything of the scale of the next-generation collider been funded based on such uncertainty of discovery?

   I personally would gladly vote to build such a machine anyway, just in case. Sadly, people like me aren't in charge, and the example of the SSC tells me HEP researchers have a vertically-steep uphill battle ahead of them to convince those who hold the purse strings to commit to such an expense. And they will need a better argument than "well, you never know…" The overhyping of SUSY will likely factor directly and powerfully into the skepticism of the funders those future researchers will face, and I'm inclined to predict it will be the field's undoing unless a much better idea is discovered, and soon.

4. **Peter Woit**
   August 22, 2016

   Goleta Beach,
   I think what experimentalists should do is pretty much ignore unconvincing beyond the SM models, and go about the business of exploring the energy frontier as best they can. Such models may have some useful role in motivating ideas about what searches to do, but, especially given how they haven't worked out, people should be careful to not let them have too much influence over what searches get done and what don't.

   As for the next collider, I do think there should be one, but that considerations of what to build should not be significantly influenced by arguments like "this machine is better because it will have better reach for superpartner X". In terms of selling a machine to governments, some hype is unavoidable, but if you're going to engage in over the top hype, you should avoid blowback to your own community, not engage in it at technical conferences for instance. The main problem I'm concerned about is theorists believing their own hype.

   The politics of a new collider are going to be very tricky. All that's clear to me is that this is hopeless in the US right now, there's no way that is going to happen, no matter how much hype you engage in. I have zero understanding of what the Japanese and Chinese situation is. In Japan, with negative interest rates, you might think that building a very expensive machine would make money, not cost money… As for CERN, I also don't know much, but if I had to guess, I'd guess any project that could fit in the envelope of the current level of funding should be salable, anything that would cost a lot more will likely be hard to sell (no matter how much hype…). More specifically, I'd guess that a new 100km ring is going to be hard to get, but anything you can imagine putting in the LHC ring (ep machine, HE-LHC) much more plausible.

5. **GoletaBeach**
   August 22, 2016

   To Low Math… well, "Tunnel Visions" tells a quite different story of the demise of the SSC. A bit more realistic about how things actually work, as opposed to a theoretical guess as to how US funding works.

   The theorists (as serious as Steven Weinberg) were hardly listened too… meaning, overhype didn't kill the SSC. Much stronger budget winds, the demise of the Soviet Union, and the reconciliation of HEP v. billion dollar project management styles in the US forces were all way more important than the hype was. Basically the theory hype has to be there but in politicians minds the difference between Steven Weinberg saying "This is important" and him saying "If you fund this you will get the Starship Enterprise built before Russia does" is squat. Both arguments saturate the "science good" meter.

   As for SUSY being unnatural, I disagree, as I said earlier:
   "A 1 TeV neutralino is perfectly fine for even the absolutely simplest SUSY phenomenologies, with all other superpartners out of reach of buildable colliders. That neutralino might be seen in the direct dark matter experiments or the indirect ones.. no problem.

   https://idm2016.shef.ac.uk/indico/event/0/contribution/32

   Or it might not. Whichever it is, all perfectly natural, because natural always is what nature chooses."

   My decades in physics have taught me how right Watson was. His view is not nonsense at all… the reason being, the path to truly important discoveries is usually circuitous and not linear at all. Thinking that it is easy to predict the serendipity of discovery the real nonsense.

   Increasing beam energy is always the best route to a fundamentally new discovery, whether it is SUSY or not. Integrated luminosity at a hadron machine does help because in essence (the parton distribution functions) it does slowly increase the available energy. But slowly. Getting more energetic beams in the long run is needed, no matter what.

   Thanks Peter. Probably the kind of thinking that is needed… if the field can think of a way to get, say, 3X the energy for the same cost as the LHC, that should trigger a new collider. Innovation is essential to achieve that. That the direct work product of collider physics has economic value has always been a ruse.

   On the other hand, the indirect work product, from every light source in the world to the world wide web, are rather powerful arguments to keep the endeavor going.

6. **Lino D'Ischia**
   August 22, 2016

   Remember what Planck said.
   "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

It was true a hundred years ago; and, it is true now. David Gross is onto something with the remark you attribute to him.

7. **srp**
August 22, 2016

Goleta Beach:
1) The U.S. got the benefits of the web without tossing the additional billions that were still needed into the SSC.
2) Glad you may be seeing the light on the need for accelerator innovation. It would be encouraging to this non-physics taxpayer if the particle community would stop trying to build scaled-up versions of what they already know how to do and invest at least a few hundred million dollars in advanced accelerator concepts, if only to rule them out. Both rational and sales-oriented considerations make it desirable to pursue new technologies offering the potential to drastically cut the cost of the energy frontier.

8. **Low Math, Meekly Interacting**
August 22, 2016

Hi, Goleta Beach,

I'm pretty familiar with the sad story of the SSC, though I'm sure not as much as yourself. What I meant by "in retrospect" is that anyone looking back will think themselves justified in ignoring the eggheads with their expensive toys and killing the thing even when, back then, the scientific argument looked substantially better. Because those eggheads got it rather wrong anyway.

As for the question of naturalness, I'm also looking at history and seeing decades of vehement arguments in favor of SUSY appearing long, long ago, and ever since. Clearly there's always a place where "natural" SUSY can hide, and, if I understand correctly, that will be true forever, given how many knobs SUSY has to twist. I imagine it must be possible to craft a SUSY theory in which SUSY is perfectly natural and 100% undetectable by the LHC, just by having an extreme version of the model in the Quanta article. But why, then, were so many incredibly smart people (including several Nobelists) so thoroughly convinced that the LSP simply had to appear in the LEP? Then the Tevatron. Then in the first run of the LHC? Then they talk about one-part-in-a-thousand fine tuning as being perfectly OK, obviously. And, hey, what does it really mean to be "natural" anyhow? Etc. This has been going on literally for decades. I know of no parallel in science, anywhere. I've seen wrong ideas. I've seen failures. I've seen outright fraud (embarrassing amounts in my own field, in case anyone thinks I lack any perspective at all). But the failure-proofing of the SUSY paradigm seems outlandish. I'm perfectly aware of my own limited intellect; that's why I rely on the testimony of those much smarter people. But their story keeps changing, and my ability to trust their judgment is simply gone.

I don't think it's reasonable to expect continued financial support based on an infinite regression of models "explaining" why the sure bet wasn't, and why we should look just a little bit harder, over and over again. Much better to use your other argument, which is simply "push the frontier".

That said, if I understand correctly, an order-of-magnitude increase in energy will be staggeringly expensive even by the standards of "big science", barring some revolutionary technological breakthroughs (I fantasize about the muon collider, which I'm told is likely a pipe dream). I think without a much more compelling argument for a new discovery than the SSC had going for it back in the day, I'm with Peter: Zero chance in the USA of it passing the laugh test in Congress. The next generation hadron collier will probably have to be in China, mostly for nationalistic reasons. So let's hope the PRC stays stable over the next 40 years or so.

9. **Peter Woit**
August 22, 2016

Lino D'Ischia,
One problem here is that SUSY enthusiasm isn't just something common in Gross's generation. Several generations of theorists since have been trained from their student days in this ideology. Of the comments from the bet losers, it was David Shih who seemed most unconcerned about not seeing SUSY so far, and he is the youngest of them.

10. **Giotis**
August 22, 2016

Jesus, the same story again and again.

SUSY is there; it is alive, well and unbroken at some high energy scale. It's almost a necessity of Nature to be there for strong *fundamental* theoretical reasons.

It is just that along the way people thought that the presence of the Symmetry (if appropriately broken at certain scale) can be justified also from effective field theory point of view using some bottom-up arguments based on naturalness.

So what people are trying to see in LHC now is how much it is broken using bottom-up low energy effective field theoretic arguments based on naturalness.

It is naturalness that is failing here (to the extend that it is failing at the specific LHC scales), but naturalness is just a general strategy used to guess at which scale new Physics is expected to appear; it is unrelated to SUSY per se.

The fact that no new Physics (not just SUSY) seems to appear at LHC just means that the effective field theory doesn't have a clue why Higgs is so light and it's a question for the fundamental theory to answer (this also depends on how much fine tuning someone is willing to accept).

So be it, all it's nice; SUSY should not carry the burden to justify the usage of naturalness by Physicists as a working hypothesis to construct BSM phenomenological models.

I'm sure this is what Gross had in mind and reporters must stop misleading the public with similar eye catching hype.

11. **Peter Woit**
August 22, 2016

Giotis,
"SUSY is there; it is alive, well and unbroken at some high energy scale."
The problem is that such a statement is a religious one, not a scientific one. Gross and the other losers of the Copenhagen bet were betting that SUSY was science, with particular testable experimental implications. Unfortunately for that point of view, they lost. This really is now the question for the future of HEP theory: is it really all right if the field becomes based on a series of untestable claims like yours (which, by the way, I strongly disagree with)?

I think you will find that, post LHC results, your religious views are now those of a small minority of the field.

12. **Michał Pawlak**
August 22, 2016

In 1986 I attended a summer school of physics, where John Ellis was one of the tutors, and had a lecture on SUSY. Asked about experimental prospects of finding it he responded, quote “I will not commit suicide if SUSY is not found at LEP. But if it is not found at the SSC then maybe…”. OK, LHC is lower energy than the SSC, but I’m a bit afraid…

13. **Giotis**
August 22, 2016

I feel I repeating my self so for the last time I will state the obvious.

SUSY as a fundamental symmetry doesn’t make any top down low energy BSM predictions or the scale at which it is broken.

It is naturalness that makes these predictions and not just for SUSY but for any potential new Physics that can be used to explain the Higgs lightness.

These are two completely unrelated issues.

14. **Anon**
August 22, 2016

On a more mundane level, we can at least test David Shih’s optimistic claim that by spring, the LHC will have up to five times the data it had at ICHEP giving 12×5 = 60\fb. Personally, I believe David’s being overly optimistic as with SUSY based upon how things are going right now, and it will be a rather more modest 35\fb.

Also, despite the optimism of Nima and David Gross about this being a great opportunity for newly minted PhDs to come up with new ideas to challenge SUSY, the main problem remains: No new physics has shown up at the LHC to show the way and we need to wait until then.

15. **GoletaBeach**
August 22, 2016

srb… of course no-one predicted the web would come from the expenditures justified for CERN between 1954 and 1993….

What no-one knows is… what innovation did we miss out on by *not* pursuing the SSC… and would it have been as lucrative as the web was? The point is…. serendipity.

Of course much of the condensed matter community was vociferously opposed to the SSC as impractical, focused on wasteful intellectual curiosity that would never lead anywhere. Not one of those SSC antagonists credit high energy physics with inventing the web, though.

There have been intense efforts in accelerator innovation for at least 30 years. In fact, the LHC did benefit from some of the very practical magnet innovations developed for the SSC…. and the LHC ignored some other of the SSC lessons; had they paid attention to them the magnet problems the LHC experienced might have been avoided.

But accelerator physicists are often prevented by practical matters from innovation… they must deliver the beam and aren’t always given enough freedom and support to play with instruments like the LHC and benefit from some serendipity.

Low Math… doesn’t the argument for the SSC look even stronger today? At first glance, the LHC didn’t have sufficient energy to discover new physics. The SSC might have had sufficient energy.

As to why Nobelists predicted evidence for SUSY at PEP-I, SppS, KEK, LEP, Tevatron, LHC… they are optimists. Optimists have a huge evolutionary advantage over pessimists, because optimists make things happen. And sure, we’ll need innovations to keep future collider costs within a reasonable envelope. I’m optimistic that we’ll get them.

As for fine tuning… the universe is already amazingly fine tuned… see
https://en.wikipedia.org/wiki/Fine-tuned_Universe

which omits the surprising fine-tuning of G_fermi, which controls the rate of pp fusion in the Sun.

The only sensible definition of naturalness is: if nature chooses it, it is natural.

16. **wimp**
August 22, 2016

@GoletaBeach, if dark matter consists of 1 TEV neutralinos, wouldn’t LHC, Lux, PandaX, or upcoming Xenon1T detect it?

@Peter Woit – what about neutron and electron EDM measurements? it is conceivable they can find a EDM consistent with SUSY or rule out EDM to SM values, ruling out SUSY, and do not require multibillion dollar colliders.

best

17. **Peter Woit**
August 22, 2016

wimp,
The problem is that these are just two numbers, and if you manage to measure a value too large to be the SM value, you have found evidence for BSM physics, but very little information about what it is, in particular no evidence that it is SUSY.

On the other hand (and this is where we are now), when you find a lower bound on an EDM inconsistent with “generic” SUSY models, SUSY proponents will just say that this doesn’t rule out SUSY, only puts a single constraint on the large parameter space of SUSY models. These EDM numbers have been inconsistent with generic SUSY models with TeV-scale superpartners for a while, but that has had little effect on getting anyone to give up on looking for SUSY at the LHC (although it was one of the motivations for split SUSY).

This is the general problem with low energy experiments that give very indirect access to high mass scale physics. So far all they give is a small number of complicated constraints on BSM models, and if you observe something non-SM you may justifiably get a Nobel prize, but the only information you’ll have about BSM physics is one number, a complicated function of the huge parameter space of BSM models.

18. **wimp**
August 22, 2016

Peter Woit,

i understand what you are saying, but what about an exclusion? finding EDM does not prove SUSY, but a sufficiently low EDM might be able to falsify it.

is it possible in principle that a sufficiently low measured value of electron/neutron EDM – both- on the order of 10^-32 or less, can rule out SUSY, since SUSY generically predict values of EDM much higher than this? i.e if electron, neutron, proton EDM – all three- are measured to less than 10^-32, would that falsify SUSY (assuming no other evidence of SUSY in LHC, rare decays, dark matter searches)?

regards

19. **Peter Woit**
August 22, 2016

wimp,
The problem is that you are falsifying a particular notion of "generic" SUSY model. Given the kind of EDM limits you mention, SUSY enthusiasts could just say
1. SUSY might be "generic", at high enough energy scale.
2. SUSY might be at lower energy scales but "non-generic", or "fine-tuned", just happening to be such that the EDMs are much lower than expected by dimensional analysis.

In any case, the main point I was trying to make is that ruling out SUSY models at higher mass scales might be something you can do with a new collider, but it's not a good motivation for building one. We already know all sorts of problems with such models, and they don't really explain anything, so ruling them out is not an interesting goal. Understanding what physics looks like at multi-TeV scales (and whether it is SM or non-SM) is a good motivation.

20. **Lino D'Ischia**
August 23, 2016

Peter, your response only points out the wisdom of Planck's statement; i.e., only now, after two unsuccessful attempts to see anything of SUSY, will the younger generation begin to distrust this approach. And it will have to be the generation behind the present one. This might take some time. OTOH, I believe that at least some of the youngest generation will begin to look in unchartered areas, and should something prove promising, who knows,maybe the whole field will latch onto that. I think the problem in modern-day science is that opposing viewpoints are so quickly suppressed.

But I can't help feel that all of this should be part of some learning curve for scientists.

21. **Claudio Merati**
August 23, 2016

Urs,
you say that supergravity might be the way to go. How many parameters (instead of the about 20 of the standard model) does it have? Are they fewer than the 105 of usual supersymmetry?

22. **MathPhys**
August 23, 2016

Nature didn't read the paper of Wess and Zumino. Too bad.

23. **Peter Woit**
August 23, 2016

Claudio Merati,
The SUSY models under discussion are generally assumed to be supergravity models, this aspect of them doesn't solve the fundamental problem Urs and Giotis want to ignore: the supposed beauties of the models are wrecked by the necessity of breaking SUSY since you see no evidence of it in the observed particle spectrum.

24. **M**
August 23, 2016

There are good motivations for SUSY. But the main motivation, that weak-scale SUSY makes the weak scale naturally small, is dead. If future data will show evidence for sparticles, it would be like discovering a dead body.

25. **chris**
August 23, 2016

I am a HEP theorist myself and about 30% of my published work deals with new physics searches.

If I had to decide, I would not fund a new collider today.

I am surprised that many of my colleagues still would. They seem to be blinded by some strong observational bias or put sociology over interest in nature. Because if the LHC has told us one thing it is that everything points towards a "dessert" (i put the word into quotation marks as I am again quite surprised that the working of our theories over many orders of magnitudes should be labeled by a derogatory term).

So, let us look at the big picture. Aside from all speculative thoughts there was one big argument why the LHC had to be built: In its energy range there had to be the Higgs or otherwise S-matrix unitarity would be violated. And remember, when we started out the upper mass bound for the Higgs (from triviality) was ~600-800GeV. That is a solid reason to sink O(10) billion.

What has happened now? The Higgs is found and its mass indicates that our vacuum is metastable and something will happen at ~10^12GeV. So again we are in a situation where there is a new energy where we have to see some sort of new physics, just like before. Only this time around it happens to be 9 or so orders of magnitude beyond our current technical capabilities.

Leaving egos, careers, technical expertise and all these aside, what can one conclude from that? I think the logical decision would be that accelerator physics should start confronting the challenge of bridging the gap of 9 orders of magnitude in energy and while they are busy doing that the big money should go to other areas of fundamental physics where progress is just happening at breakneck speed like e.g. cosmology.

As individuals – especially those stuck in the field – we might not like it, but that seems to be the only logical conclusion. Some people seem to prefer to hype their specific model and want to convince the public to commit substantial resources towards investigating if their ideas are correct. But in all honesty, a scientist should know better and clearly separate established fact (like someting new has to show up at around 10^12 GeV) from fantasy (like "SUSY is around the corner").

26. **MathPhys**
August 23, 2016

AGT tells that Nekrasov's instanton partition functions in 4D N=2 SYM can be used to compute conformal blocks in non-supersymmetric 2D critical systems, including the humble Ising model.

These surface critical systems are studied since decades in many different ways, using various analytic methods including 2D conformal field theory, numerical methods, as well as experimentally. The AGT correspondence, which originates in very stringy ideas, leads to results that are in perfect agreement with everything that we know. 4D SUSY is absolutely necessary to obtain these results in 2D non-SUSY systems, and no SUSY-breaking or any approximation of any type is involved in these computations.

If only for this reason, 4D supersymmetry cannot be a totally wacko idea. However, SUSY simply does not appear in high energy physics.

27. **Peter Woit**
August 23, 2016

chris,
Arguments like "we know there's no BSM physics up to 10^12 GeV" from theorists are just as worthless as the "we know from naturalness there has to be BSM physics at the TeV scale" ones. I hope experimentalists have the sense to ignore both of them.

Mathphys,
I agree that d=4 N=2 SUSY is a fascinating theory, (besides the AGT stuff you mention, this is the theory that gives Donaldson invariants and a very deep connection between 4d geometry and topology and physics). People should study SUSY theories like this, while noting that the ones leading to beautiful mathematics are different than the ones used in the failed program to connect SUSY to experiment (e.g. N=2, twisted, vs N=1). I wouldn't be at all surprised if thinking about SUSY and QFT led to some new classes of theories providing new insights into the world, I just think it's clear that the particular class used so far doesn't work.

28. **anon**
August 23, 2016

As someone who worked on SUSY for years, and even did my Ph.D. on a SUSY model, I have to admit that I never was able to see anything particularly compelling or beautiful about the whole idea. I know, I know, the square root of the Poincaré group and all that, but as beautiful and inevitable as I find the mathematics of gravity or gauge theories, SUSY always seemed ugly to me. Personal esthetics, I guess.

29. **anon**
August 23, 2016

Chris, though it would be very nice for this glutton if everything pointed to a dessert, I think you meant to say desert.

30. **Low Math, Meekly Interacting**
August 23, 2016

With SUSY, you really can have your cake and eat it too.

31. **GoletaBeach**
August 23, 2016

To chris… I don't really agree that cosmology is proceeding at breakneck speed. A simple cosmological constant fits the dark energy just fine… B-modes promise to be a long hard slog. Maybe we'll get the average neutrino mass out, that will be good. But hardly breakneck speed.

The arguments you mention about the Higgs & LHC were really arguments for the Higgs & SSC. The LHC got interesting when precision electroweak indicated a light Higgs. The no-lose theorem never applied to the LHC and a heavy higgs.

What you miss is the exploratory aspect of high energy physics. A prominent motivation for experimentalists and machine builders is the unexpected. No theorist predicted the muon, parity violation (actually seen in the 1930's, it is a wonderful story), strangeness, CP violation, neutrino flavor, the bottom quark, the tau lepton… well, the charm quark, the Z0, the W, the top quark, and CP violation in the B system, the Higgs and its mass (roughly)… they did get predicted by theorists.

A requirement of theoretical prediction would, in the past, have resulted in missing about 1/2 of particle physics discovery. Luis Alvarez wrote a wonderful essay in the early 1970's about the necessity to politely and congenially disregard theory regularly (but not always).

Sure, $10 billion ain't easy. But accelerator builders haven't figured out how to get factors of 10 or 10,000 in beam energy because they are lazy. Nobody has figured out economical flying cars either in the past 50 years. Some technological problems are rightfully hard.

There has been however a very nice adiabatic continuous improvement in accelerator abilities. The field needs to support machine guys and not burden them with ideas of flying cars… and don't ever think that innovation is not intensely pursued in the accelerator business. It is, but often theorists are a big ignorant about it.

32. **anon2.**
August 23, 2016

Just a quick question about your book. Are you going to keep the latest free PDF version of the final draft online, even after it is printed? Would the publisher permit that, since it might just reduce sales? (On the other hand, seeing what you're getting like this may be a way to promote and popularise the book, just as browsing in bookstores works.)

33. **Peter Woit**
August 23, 2016

anon2,
My contract with Springer specifies that I retain copyright, as well as the right to make the final draft version available on my web site, which I intend to do (I did learn something from the experience of my first book…)

Right now rereading from beginning, fixing minor things and making various improvements based on comments and suggestions from readers. Pretty thoroughly sick of working on this, hope it's over soon.

34. **Peter Woit**
August 23, 2016

All,
I'm deleting further tired general discussion of topics like particle accelerators and their funding, and hoping for a return to the topic of the posting.

35. **Low Math, Meekly Interacting**
August 23, 2016

I actually was curious about what the current state of SUSY and general BSM physics betting might be, in light of the current LHC results. Maybe the dust hasn't settled enough yet, but it was quite entertaining a few years ago to see all the personal odds estimates being thrown around, as well attempts at generating aggregates for different outcomes. Useless scientifically, of course, but a very interesting sociological exercise.

36. **GoletaBeach**
August 23, 2016

Well, the accelerator discussion (as well as EDMs) was targeted to the topic of "in your lifetime". But it is your blog and if it is tiresome to you, that is alright.

To stay on topic: "Not Ever Wrong" and "Not in Your Lifetime" are inequivalent. Ever is forever. Your lifetime may be forever for you, but it is not forever for everyone else.

Not Ever Wrong makes current particle theorists sound like theologists. I don't think they are. I think that their belief is that eventually empirical evidence will decide whether SUSY is wrong or right, it is just that the time frame may be 1000's of years, or however long it takes to do a complete set of experiments up to the Planck Mass. Speculation about how long it will take to perform that set of experiments is… well… tiresome to you.

The current particle theorists view is not the same as theology. In theology the concept is that the theology is correct independent of empirical evidence.

37. **johnnythelowery**
August 23, 2016

Congratulations to you too on this in recognition of you standing up to the Zeitgeist of the day. Bravo!!

38. **Jonathan Miller**
August 24, 2016

As an experimentalist in HEP, I think the community should focus on testing the SM and not on testing unmotivated theories. It is entirely natural that sometimes theory needs to wait for some hint from experiment and this is the current case.

And there is no reason to be pessimistic about the field, there are a lot of obvious areas to test the SM such as neutrino masses, CP violation in the lepton sector, proton decay, confinement and muon g-2 to name just a few. These don't require a bigger collider, but that doesn't make them any less important or likely to provide the necessary hint.

And it would be best if theorists create theories to fit these new hints, not only look for some version of an old, no longer motivated, model that is not ruled out by the new 'constraint'.

If all the obvious places to test the SM result in trivial extensions and no hint about future physics, then it might be time to become pessimistic.

39. **WLM**
August 24, 2016

I had a slightly different take on David Gross's comments, following on those of 't Hooft, than Peter. I understood him to be saying that the emphasis on SUSY as a central issue is actually misplaced [except perhaps for some String theorists], and a distraction from dealing with our lack of understanding of some true fundamentals of particle physics and of space-time in general. (From 't Hooft: The resolution of whether super-symmetry exists or not isn't just about whether some new particles show up at a particular energy in a particular experiment, but rather about the nature of space and time: is space-time super symmetric?) To summarize:

– The underlying issue is one of "naturalness", or the lack thereof, in the heirarchy of masses and their couplings with the Higgs field.

– There have been a whole series of issues with "big numbers" and "small numbers" during the development of the Standard Model, all of which appeared to be completely "unnatural" at the time. Some have been dealt with in the theory, in QCD, and are calculatble (if you understand the dynamics properly) and some have just been accepted as "just the way it is" — such as the heaviest quarks having 50,000 times the mass of the quarks that make up the proton. There's no fundamental explanation for that – just measurement. In 1960, that would have been considered insanity. Today, it's "So what?"

– We should not try to apply the same methods to other, perhaps totally unrelated "problems of large numbers" just because they worked before. Every such problem is its own separate case and will probably require its own particular solution, as we better understand the underlying physics. E.g., the Cosmological Constant problem is probably unrelated to the hierarchy problem, and will need its own separate solution, to be able to calculate anything.

– Super symmetry offers the possibility of handling the hierarchy problems by turning them into logarithmic effects, just like we learned from QCD that couplings vary logarithmically with energy, not linearly. Some type of super symmetry still seems like the best hope for doing that.

– So super symmetry doesn't show up at the LHC, or only shows up very tentatively over the next few years? That's probably just a sign that there are small numbers involved, just like there were in the quark-lepton mass spectrum. So what? It's likely that we don't understand the parameters of super symmetry buried in the couplings with the Higgs just like we don't understand other paramters of the Standard Model: we are missing something fundamental.

– This doesn't mean that it's right, just that it's the only explanation we have come up with so far that addresses this particular large number issue. So he's sticking with it, as something that will need to be considered in more fundamental explanations.

– What we really should be worrying about is identifying the underlying dynamics of more fundamental physics, that better explains those unexplained parameters in the Standard Model and lets us do actual calculations in regimes where we currently cannot. And the younger generation should not be shy about throwing out the preconceptions of the prior generation of physicists in order to do so.

While I can't claim to agree with "believing in" SUSY just because it's the only game in town, this seems like a much more nuanced, rational approach than Peter suggests.

40. **Another Anon**
August 24, 2016

Yeah, as johnnythelowery says, congratulations. This is probably the nearest you're ever going to get to official confirmation that SUSY is dead.

41. **Andreas**
August 24, 2016

Don't worry Peter, that's exactly what Thomas Kuhn descriped. Some theory only dies when their participants die. It would be worrying if many new people join SUSY/Strings, but i guess that doesn't happen. At our universities, string theoretists are not taken serios anymore, and the groups mainly consist of a few old guys, and senior-postdocs which cant find permanent positions. no PhD students mainly.

I guess thats a good sign. 🙂

42. **Bernd**
August 24, 2016

Peter,
I strongly disagree with the statement that arguing for the desert is the same as arguing for low-energy BSM physics on grounds of naturalness. First of all, the desert is not a thing, but actually the absence of a thing (BSM physics below the GUT scale). Hence, the default assumption should be to stick with the SM and put the burden of proof on anyone who claims there are violations just round the corner.

43. **Peter Woit**
August 24, 2016

johnnythelowery and anon,

Thanks, but in this case I don't think my skepticism about SUSY at the LHC is a "standing up" to anything, since I think most HEP theorists have always shared this skepticism (as I keep pointing out, it's the losers of this bet who were in the minority). So, on this one I'm in the majority, now kicking the losers while they are down…

WLM,
I didn't discuss Gross's comments about SUSY/naturalness getting too much attention, while our complete lack of understanding of the SM parameters at all gets very little. I completely agree with those comments.

I do though also completely disagree with his argument that one should have faith in SUSY, because it promises a solution to the naturalness problem, even if you have to fine tune things somewhat. This argument claims to solve a conceptual "fine-tuning problem" by extending the SM by something complicated, adding in over a hundred new parameters. And then you find it doesn't really solve the problem, you still need some fine-tuning. I've always been of the opinion that this never was a solution to the problem (adding a huge number of new parameters is not a plausible explanation of a puzzle about one parameter), but the LHC could have vindicated this. It didn't, so now the disturbing situation is that proponents of this have decided to stick to a bad "solution to a problem" and keep promoting it, without any hope now of experiment resolving the issue.

44. **Mr Anon Anonymous**
August 24, 2016

The bet I'm really looking forward to is the Lubos-Jester one. Boy, am I looking forward to that one!

The LHC has recorded ~20\fb making ~30\fb by mid-October a realistic possibility. Make the blog post a good one, Peter 😉

45. **Peter Woit**
August 24, 2016

Mr. Anon Anonymous,
I think that bet was a good indication that skepticism about SUSY at the LHC was always very widespread (Jester offered 100 to 1 odds, betting it wouldn't show up).

I hear Lubos is doing his best to refuse to admit defeat, now claiming he won't pay up until all analyses in all channels by CMS/ATLAS based on 30 inverse fb are completed, which could be quite a while.

One reason I never got interested in searching out such a bet was that I suspected I'd never see the money if I won. I would have predicted that some sort of anomaly would show up in LHC data that SUSY enthusiasts could point to as a possible SUSY signal, have been surprised that that hasn't happened.

46. **Ming the Merciless**
August 24, 2016

Opinion: Nature has spoken on more than one level here, for example on the matter of which speaks the most for human naturel, neo-philosophical notions of Critical Rationalism as exemplified by Karl Popper and currently David Deutsch on Karl Popper; or is it the more traditional mixture of common experience with a peppering of self-honesty and awareness of the facts of history.
We don't change our minds. It may not even be possible to change our mind. Not about core hard-won whole-life insights. The reason this is not apparent to us is because it's a relatively tiny subset of the fulsome summary of all the real or possible things we think we have (or had, or would've).
Everyone is [usually] more-than-happy to abandon, or switch sides, on the vast majority of our opinions because we attach little real value to them. So it looks like changing our minds is something humans do all the time, when in reality we do not, ever.
Science on the theoretical side is almost always about the hard-won insights and whole-life lessons. It cannot, and has never, depended on this sort of notion. What has changed relatively recently is the level in scientific affairs at which positions are decided. Everywhere else the positions that really matter are attributes of groups not individuals. Science uniquely managed to heave this trait in the direction of the individual far enough that the consensus and ultimate course became on-balance, and ultimately settled objectively by objective reality, via measurements.
That has now changed, it has reverted back to the base-energy state of human nature in which it is relative power that has the final word, not truth. Not because humans are cynical or attach little value to the virtue of objective truth, but because that is the objective truth in human affairs.

47. **Anonymous**
August 24, 2016

I assume people are aware of this: at collaborations like ATLAS, the limits on BSM physics can move to publication quickly; however, unexpected/expected excess could take much longer to go through the internal review and politics. This is just the reality of the life in big collaborations. With this in mind, you might want to hold your bet for a little longer and it is very sensible to me that you do want to see majority of the results with full 2016 LHC data.

48. **anon**
August 24, 2016

Dull discussion accompanying the bet. Just a load of big shots (who in this case were proven categorically wrong) exchanging platitudes about new physics and naturalness. The final speaker literally agreed with everything said previously. With respect, what was he there for? I wanted something more adversarial. Unfortunately, these kinds of discussions are somewhat ceromonial and reverential, and don't challenge the ideas or the logic of their special esteemed guests. As a result, we hear the same old things and nothing changes. I suppose at least they had the video message from 't Hooft.

49. **Peter Woit**
August 24, 2016

anon,
I think the idea was to just have as panelists those who had lost the bet, with the idea that they might have something interesting to say about how losing the bet had changed their point of view (winners of the bet were likely to not have much to say except "told you so"). This didn't really work out, since the attitude of the losers was pretty uniformly "So what? I still believe SUSY rules, no matter what experiment says".

50. **NoGo**
August 24, 2016

Can you explain this attachment to SUSY? What is so compelling about it?

51. **WLM**
August 24, 2016

Peter,
I completely agree with you re. the "ugliness" of super symmetry as a solution to a hypothetical "naturalness problem", and that we should not just have faith that it will ultimately somehow turn out to be correct after all (despite needing fine tuning to reduce fine tuning). [One should note, however, that "ugliness", just like "beauty", is not a valid criterion for deciding the correctness of a theory. :)]

I was just giving Gross the benefit of the doubt in his statement that we don't understand the parameters of super symmetry buried in the couplings with the Higgs, yet, just like we don't have a clue about other open issues with the SM itself. And we need to.

My interpretation is that he hopes/wishes/fantasizes [take your pick] that, once we understand what is really going on, those 100+ parameters will not just be explained, but will most likely disappear. His analogy was that, just like the morass of hadrons and their constituents was clarified once we understood gauge theory and asymptotic freedom in QCD, a more basic understanding of the underlying dynamics of Higgs interactions is what is required to clear up what is really going on in the morass that is super symmetry and the naturalness "problem" — and perhaps other SM issues as well. And that will require understanding those Higgs couplings much better.

Of course, a fundamental difference is that the issues in the 1960s with hadrons and quark confinement were the results of multiple experiments. The issues thrown up by super symmetry are strictly theoretical, solving a theoretical problem, with essentially no experimental guidance or even confirmation of the problems themselves, other than negative ones. But my interpretation of his current approach is to say, OK, we don't know what's going on, but let's start by assuming some kind of super symmetry effectively exists and see if that can lead us to the deeper understanding where we can explain it away.

That's obviously just my interpretation from several of Gross's talks. It's very different from just "believing in SUSY" (like some of those panelists apparently still do). And it could be considered increasingly foolhardy as decades of experiments accumulate against that hope. It would be interesting to hear his actual thoughts spelled out.

52. **TS**
August 25, 2016

I had a stint with theoretical particle physics a long time back, I did some supersymmetrical calculations, then switched to experimental particle physics. To my understanding, the best argument for SUSY was that if supersymmetry were realized in nature, there would be a Nobel for my university (well, until Julius Wess's untimely passing). The second best argument was the pure ingenuity of QFT that forces you to discover supersymmetry if you are creative and study its mathematics hard enough. I always felt that these arguments were more convincing than "it gives you dark matter (once you assume an additional symmetry)" and "it cures a mathematical-aesthetical problem that you have when you extrapolate to arbitrarily large energies assuming that there are no unknowns on the way there." This skeptical point of view may have been reinforced by my very pragmatic advisor, who never bought the hype, and who is still at the forefront of pure standard model calculation, but who at the time wanted to do a few SUSY calculations in order to learn the formalism himself. (BTW I would like to remind everyone that recent progress in SM calculations has been tremendous, so it's not at all like theoretical particle physics is not making progress, and the progress is actually very much related to better understanding of the mathematical structures even though generalized polylogarithms and numerical-analytical methods probably aren't as sexy as amplituhedra.)

Anyway, what I originally wanted to point out is that talking about hundreds of parameters as a deficit of SUSY is not attacking one of its weak points. The idea, as I understood it then, was that we need all these parameters because we don't understand how supersymmetry is broken, so they parameterize our ignorance and if we keep them, we avoid prejudice about the unknown: if you assume that SUSY is a fundamental symmetry, you also assume that, say, the mass of the electron and the selectron are related by the same mechanism that relates the mass of the muon and the smuon. Thus nobody who believes in SUSY believes that the masses of selectron and smuon are independent parameters (provided of course that the SM parameters are known inputs). The large number of parameters is a convenience, and allows very general experimental exploration, exactly because it doesn't require to specifiy the mechanism of SUSY breaking.

This seems to imply that someone should keep track of which models of SUSY breaking have been excluded, but I don't think I've seen anything like that. Maybe even with SUSY breaking there's more freedom left than I could possibly imagine?

53. **Peter Woit**
August 25, 2016

TS,
It's true that the huge number of new parameters problem is not fundamental, in the sense that one can always claim that "all that is needed is to figure out the mechanism of supersymmetry breaking". On the other hand, despite 40 years of effort, no one has come up with any compelling such mechanism (i.e. one that explains something about known physics, or does anything other than multiplying entities and adding complexity on a scale larger than the number of phenomena one is trying to explain). The extra parameters issue is a quick way of referring to that problem, since examining in detail all the possible mechanisms for susy breaking and their problems leads to absurd complexity (such that this is an issue that theorists appear to have pretty much abandoned, as far as I can tell, few people still work on it). And, these mechanisms completely torpedo claims to have a "beautiful" or "elegant" theory.

54. **Robert Matthews**
August 25, 2016

Seems like 't Hooft and Peter W should have placed side-bets on the "We lost, but we could still win" reaction from the usual SUSYpects.

The debacle also seems to leave Popper's idea of refutation looking pretty underpowered when it comes to theories like SUSY, with so many tunable parameters.

Maybe this week we've seen the emergence of a new criterion for such theories: "aleatory refutation" – aka Death by Wager.

55. **MathPhys**
August 26, 2016

" John Ellis has also announced that no matter what the LHC says, he's not giving up "

A man has declared his mind closed. I have respect for self-knowledge.

56. **Emil**
August 26, 2016

There were always many arguments against low energy supersymmetry, both empirical and aesthetic. The searches for superpartners at the LHC coming up empty is only the latest, strongest empirical argument against low energy supersymmetry. The aesthetic is as soon as one took the beautiful idea of supersymmetry and tried to match it to nature, it rapidly became ugly. It is a solution in search of a problem, like a cave man finding by chance a shiny MacPowerBook and after puzzling over what to do with it, uses it as a hammer.

That doesn't mean that supersymmetry has no place in physics, any more than that the laptop has no use good use. Solid mathematics has a way of finding a realization in physics. It's that aesthetic appeal of supersymmetry that leads to its advocates not giving up on it. It's just that we haven't yet found that proper place, and in the meantime we should probably give up viewing it as a hammer.

More seriously, the most compelling argument against low energy supersymmetry was always that although it was advocated on the basis of 'naturalness' to cancel quadratic cutoff dependence and explain the light Higgs vs. the Planck mass, it provided and still provides no answer to the biggest naturalness problem of all–namely the smallness of the vacuum energy in Planck units. By itself this is telling us with certainty that supersymmetry cannot solve all naturalness problems associated with the Planck scale, which will require a much better understanding of how quantum theory and Einstein's gravity fit together.

57. **Anonyrat**
August 26, 2016

Uses of supersymmetry – am under the impression that the BRST formalism relies on supersymmetry? So, mathematically, not physically, supersymmetry is realized in every non-abelian gauge theory?

58. **Peter Woit**
August 26, 2016

Anonyrat,

It depends what you mean by "supersymmetry". If you mean generically use of a Lie superalgebra, then BRST is an example. But usually "supersymmetry" in this context means a specific Lie superalgebra, a non-trivial extension of the Poincare group. BRST treatment of gauge symmetry doesn't involve space-time symmetries.

I agree with Emil that probably what's going on here is that "supersymmetry" has some different role in fundamental physics. I'd guess that you want to use Lie superalgebras in the context of space-time symmetries, but the particular extension of the Poincare group and the representations of this that people have been trying to connect to reality don't work (and I think the problem is not just a low energy one). Perhaps you want something more like the way superalgebras appear in BRST.

59. **anon**
August 26, 2016

Anonyrat, BRST and SUSY are very different.

In BRST y0u have a single Grassmann-odd operator that squares to zero and is used to conveniently encode and impose the gauge constraints on the state space. The constraints can generally be imposed in other ways that are more complicated in practice but don't require the BRST formalism. BRST doesn't add any physical states that are not in the original gauge theory. The whole point of BRST is that the "ghosts" we introduce should factor out of the state space and in addition cancel out gauge-redundant degrees of freedom. So BRST reduces the state space as opposed to increasing it.

In SUSY you have a set of Grassmann-odd operators that, when "squared," give spacetime translations (not zero as in BRST). SUSY can thus be seen as an extension of spacetime, adding Grassmann-odd "directions" besides the usual 3+1. Field vibration modes in the new odd directions provide superpartners to the usual particles in ordinary spacetime. So, in contrast to BRST, adding SUSY to a model provides extra physical states.

60. **Cormac O'Raifeartaigh**
August 26, 2016

NoGo: I think the main attraction of SUSY is that it offered a way around certain problems in the construction of a consistent unified field theory of all four interactions. Plus, as the last possible symmetry of the Poincare group, it would seem a pity that Nature might not avail of it!
Re the zeitgeist, it seems to me that many commentators such as Johnnythelowery have things the wrong way around; the current zeitgeist is SUSY skepticism, not SUSY optimism (at least if judged by blogs, media articles and pop science). Which is strange because only low-energy SUSY has been ruled out so far.
Indeed, it seems to me that most commentators have misunderstood the nature of the bet – it was whether SUSY particles would show up by a certain energy range. A silly bet to be sure, but to interpret the negative result as 'SUSY dead' rather than 'SUSY looking less likely' is simplistic..

61. **Peter Woit**
August 26, 2016

anon,
That's a good explanation of the usual point of view. I find it interesting though that there are contexts where these things are not so different, with one of them Witten's N=2 (twisted) SUSY YM theory that gives Donaldson invariants. In that case, among the supercharges is one that can be interpreted as a BRST differential

Much more generally, the interesting mathematical applications of SUSY theories often just look at "BPS states", using a construction closer to the BRST one. It may be that the right perspective on SUSY theories is that most of the states are more like "ghosts", not physical. And at the same time, I'm suspicious that BRST is more fundamental than people think, really needed to properly understand what is going on with gauge symmetry.

62. **Low Math, Meekly Interacting**
August 26, 2016

Hi, Cormac,

I think many of us outsiders understand (to the extent our brains allow) that there many ways to look at SUSY and what it might do. As an outsider, I've been surprised at the relative lack of discussion in the popular literature about the unique ability of SUSY to combine space-time and internal symmetries. I'd love to see more popular exposition about that aspect of the theory and why it is or isn't compelling on its own. Then again, the relevant energy for that idea (in isolation) appears to be around the Planck scale, so maybe that's why it's not so interesting.

The other idea, the one where the superpartners that come with this unification have anything to do with the Higgs mass or a viable dark matter candidate? That idea seems to be bad shape, given the empirical evidence. It's not clear to me what optimism or pessimism has to do with it anymore. Experiment has told us that if SUSY is relevant to these aspects of nature, it's reportedly "very cleverly hidden". That seems to be a nice way of saying it's no longer plausible, if it ever was.

63. **Peter Woit**
August 26, 2016

LMMI,
SUSY doesn't really combine space-time and internal symmetries, it's an extension of the space-time symmetries, commutes with the internal ones.

64. **Low Math, Meekly Interacting**
August 26, 2016

Hi, Peter,

Yes, thanks for correcting my imprecise description. I'm pretty much directly quoting another source directly, which delves into just what you said in detail that rapidly starts flying over my head (I'm not at all qualified to even get into the Coleman-Mandula no-go theorem and the nitty-gritty of why SUSY is the one-and-only non-trivial way to circumvent it, except to say I recognize this is important and interesting in its own right).

65. **Nakanishi**
August 26, 2016

I would like to comment that there is a supersymmetry other than SUSY or BRS, but yet consistent with the unitarity of the physical S-matrix. I believe that it is natural to supersymmetrize only the local Lorentz symmetry of the Einstein gravity, because it commutes with the spacetime symmetry. The Lorentz symmetry SO(3,1) is extended to the orthosymplectic superalgebra OSp(N, 2; C). In this extension, {Q, Q} and {Q^(bar), Q^(bar}} are identified with M_(mu, nu), but {Q, Q^(bar)}=0.

66. **John Baez**
August 27, 2016

From a mathematician's viewpoint, spacetime supersymmetry is just one aspect of "supermathematics", namely the replacement of vector spaces, manifolds, Lie groups, Lie algebras, commutative algebras and so on by their Z/2-graded versions. This is a remarkably potent idea – for example, it provides new improved proofs of the Atiyah-Singer index theorem, the positive mass theorem in general relativity, and a new outlook on differential forms.

Moreover, supermathematics is not an *arbitrary* generalization of ordinary mathematics: there are plenty of theorems explaining how this Z/2-graded generalization is uniquely singled out. For an example, I recommend Urs Schreiber's post on Deligne's theorem, but can't resist mentioning the closely related result that I proved earlier.

I'm sure all this is somewhere in the back of the minds of physicists who think supersymmetry is "the only game in town": there are few other principles with such wide-ranging connections to beautiful mathematics – except of course for those that have already been widely adopted in physics, such as gauge invariance, diffeomorphism-invariance, or the principle of least action.

The problem, of course, is that spacetime symmetry focuses our attention on a collection of staggeringly beautiful physical theories – such as 10-dimensional superstring theory and 11-dimensional supergravity – which seem to describe worlds *utterly unlike our universe*. And when people try to tweak these theories to make them physically realistic, they – so far – become ugly and complicated.

So, back when I was interested in figuring out the fundamental laws of nature, I avoided having anything to do with supersymmetry. Only after I quit that and took up pure math for a while did I come to appreciate its beauty! I especially enjoy how superstrings and supergravity are connected to n-categories and normed division algebras, with the octonions being closely connected to 10- and 11-dimensional spacetime supersymmetry.

I think it's possible to take a balanced attitude and enjoy the beautiful mathematics of supersymmetry while taking an agnostic or even jaundiced attitude to whether it's relevant to particle physics. But this is, of course, nearly impossible for people who hae staked their careers on it.

67. **Marc Nardmann**
August 27, 2016

John Baez,

when you say that supersymmetry is a remarkably potent idea that provides for example a new improved proof of the positive mass theorem in general relativity, are you just referring to Witten's 1981 article "A new proof of the positive energy theorem"? Its fourth section starts as follows: "In this section a few speculative remarks will be made about the not altogether clear relation between the previous argument and supergravity." Has this become clearer later?

Independently of the answer (which I'm really interested in), my point is this. There are many examples in differential geometry where spinors are important to solve problems whose statements do not already involve spinors. The positive mass theorem is certainly such an example. But is it an example for the usefulness of supersymmetry? Would a Z_2 grading on geometric objects make the spinorial proof easier or more elegant in any way? Knowing the analytic details of the proof quite well, I doubt that. So, while I agree with you that one should "enjoy the beautiful mathematics of supersymmetry", I think there is a danger of overhyping supersymmetry as a panacea already in pure mathematics.

I see an analogy to the discussion of physics we are having here. Obviously fermions (i.e., spinors) are needed to describe the universe we live in. But do we need supersymmetry to describe the universe? Even if we ignore (absence of) experimental evidence, would a Z_2 grading that puts bosons and fermions on the same footing really make our description of fundamental physics easier or more elegant? Everyone has to judge for themselves, but I doubt this one, too. From a purely geometric and representation-theoretic viewpoint, I find the Standard Model quite elegant the way it is; and the few aspects that lack elegance would not be remedied by supersymmetry.

68. **Peter Woit**
August 27, 2016

Marc Nardmann/John Baez,
Once you expand the term "supersymmetry" to include all uses of Z2-graded mathematical structures, it doesn't make sense to argue about whether it is related to physics. Of course it is, very fundamentally (the Dirac operator is the supercharge of a supersymmetric QM theory). The problem though is "which Z2-graded mathematical structure"? There a huge universe of them, as big or bigger than the universe of all more conventional mathematical structures. It becomes impossible to say anything true but non-trivial about such a universe.

A more useful discussion is to distinguish between what parts of this general Z2-graded universe reflect something deep about physics and/or mathematics (certain SUSY

QM models) and what parts don't seem to (SUSY extensions of the SM)

69. **Marc Nardmann**
August 27, 2016

Peter Woit,

you said: "A more useful discussion is to distinguish between what parts of this general Z2-graded universe reflect something deep about physics and/or mathematics […]" That is closely related to the point I wanted to make: I agree that there are many places in differential geometry where spinors and Dirac operators can be used in "deep" ways, in contexts that do not a priori involve spinors. Does this already justify — probably by definition — to say that "supersymmetry" is used there in a nontrivial way? If so, then I will not argue with that definition. If not, then I argue that the positive mass theorem is not a good example of a deep application of supersymmetry. This is roughly analogous to the fact that we would not call the plain Standard Model (which involves spinors and Dirac operators) a "supersymmetric" theory.

70. **John Baez**
August 27, 2016

Marc Nardmann wrote:

> When you say that supersymmetry is a remarkably potent idea that provides for example a new improved proof of the positive mass theorem in general relativity…

It's late at night here in Singapore, so I'll try to answer your questions later, but for now let me just note that I didn't say that. I tried to be quite careful in how I expressed myself, saying:

> From a mathematician's viewpoint, spacetime supersymmetry is just one aspect of "supermathematics", namely the replacement of vector spaces, manifolds, Lie groups, Lie algebras, commutative algebras and so on by their Z/2-graded versions. This is a remarkably potent idea…

Maybe it wasn't clear, but "this" refers to "supermathematics", not supersymmetry.

71. **Marc Nardmann**
August 27, 2016

John Baez wrote: "Maybe it wasn't clear, but 'this' refers to 'supermathematics', not supersymmetry."

That was indeed not clear to me. I apologise for mis-paraphrasing. So, which Z_2-graded objects were you alluding to in the context of the positive mass theorem?

You mentioned the application of "super" concepts in proofs of (versions of) the Atiyah-Singer index theorem. This seems to me to be a good example of the potency of "super" ideas. I still do not see how the positive mass theorem is an example. To me, it is just an example of the usefulness of spinors in differential geometry — which is profound, but not "super" in a technical sense.

72. **Urs Schreiber**
August 28, 2016

The fact that the worline theory of an ordinary spinning particle (such as the electron) is 1d supersymmetric (see here for details and references) is interesting mostly because it is a precursor of the fact that the worldsheet theory of the "spinning string" (as it was originally called) just so happens to be 2d supersymmetric (and called the "superstring" only once this was realized), which in turn is interesting because it miraculously implies that the effective spacetime theory of which these strings are quata is locally 10d supersymmetric. This is interesting because, while the automatic worldline supersymmetry of the electron does not imply that the spacetime theory is supersymmetric, just assuming that the electron is the point particle limit of a string (and nothing else) does imply local spacetime supersymmetry. This is a key prediction of the assumption of strings:

strings & fermioms => supergravity

This of course does not prove that local spacetime supersymmetry (supergravity) is realistic, but it is one of the theorems that miraculously imply local spacetime supersymmetry and thus make it a plausible possibility. Of course this particular argument does require the assumption that particles are limits of strings, which might be wrong, and if so then the conclusion wouldn't follow, of course.

On the other hand Deligne's theorem needs no assumption beyond established principles of quantum physics (that elementary particles are labled by irreducible linear representations of the local spacetime symmetry group), but as a payoff it does not imply that the local spacetime symmetry group is the super-Poincare group, it only implies that among all imaginable modifications of local spacetime supersymmetry (e.g. non-commutative groups, aka quantum groups or more bizarre possbilitities) all except precisely the class of super-symmetry groups are excluded on mathematical grounds.

Combined with the observavation made in 1922, that the world does contain fermions, and the realization, a little later, that hence the phase space of every realistic physical theory (such as the standard model) is a supermanifold (the fermions constituting the odd-graded coordinates) it makes it rather plausible that the local symmetry group acting on this supermanifold in general mixed bosons and fermions, hence be a supersymmetry group. Of course this is not logically implied and could well be wrong, but the point to notice is that given the above circumstances, it would almost seem to require _more_ explanation why it shouldn't, than the other way around: experiment shows that phase space is a supermanifold, and Deligne's theorem says that the most general local spacetime symmetry group may be a supergroup, so why on earth would Nature then choose to constrain this possibility and have an ordinary group act on the supermanifold, if it could choose a supergroup.

Again, this does of course not prove local spacetime supersymmetry, but it serves to show that there are good reasons to expect that it might be: Because conversely, if the world were not locally supersymmetric, then there would be reason to ask: why this constraint? Why, if provided with the possibility to choose from the class of all super-groups, and with phase space of verified physic already being a supermanifold, why would Nature choose a purely bosonic symmetry group? It's possible, but it shows that it is not insane to suspect that fundamentally (i.e. at high energy) it might be different.

That all said, recall again that all this is about high energy local supersymmetry (supergravity). There is no comparable argument from first-principles which would force that a theory of supergavity should settle in a vacuum where at the weak scale there is a global supersymmetry left (which is the kind of supersymmetry that is invoked for those arguments about naturalness, gauge coupling unification, dark matter etc.). In contrast, a global supersymmetry (a global Killing spinor) in a solution to the equations of motion of supergravity is about is unlikely as a global translation symmetry (of which the supersymmetry is a "square root"). We don't expect the universe to be globally translation invariant in any one direction, even though locally it is, and analogously it would be surprising to find a global supersymmetry, even if locally everything would be supersymmetric.

In conclusion, the main question of interest seems to be a question rarely discussed at the moment:

Theoretically, the real question is: given a theory of supergravity (or some UV-completion thereof) what are the compactification mechanisms, what can one say about its effective "low" energy (say weak scale) vacua. Is there any actual mechanism, besides prejudice, that would prefer Calabi-Yau or G2 compactifications? Or maybe better: at which scale would such these be effective?

Experimentally, a good question would be: what would be experimental checks not of low energy supersymmetry, but of supergravity. For instance [these authors here](#) argue that plateau inflation (such as Starobisnky-inflation) — which is the model of inflation presently preferred by experiemtal data — gives a still better fit to experimental data after embedding into supergravity. Now, that particular argument will have its loopholes, but generally more experimental arguments for or against high energy supersymmetry (supergravity) would be interesting to have.

73. **Peter Woit**
August 28, 2016

Urs,
You are trying to derive from an extremely general abstract theorem (that Tannaka duality works for not just groups but also Z2-graded groups) an argument for a very specific supergroup, a rather ugly one with no experimental evidence at all for it. I just don't see any argument at all for this.

"All groups" covers almost all of mathematics, and adding in Z2-graded groups makes this even more general. I'm a big fan of the idea that quantum mechanics is fundamentally representation theory, and (see the book I've been writing) I think there's a huge amount to say about how highly non-trivial and specific basic structures in representation theory govern quantum theory. But, you can't get something from nothing: an extremely general piece of abstraction applying to almost the entire mathematical universe cannot possibly do the job of distinguishing the very specific mathematical structure that seems to govern the physical universe.

74. **Marc Nardmann**
August 28, 2016

Urs Schreiber wrote: "[T]his does of course not prove local spacetime supersymmetry, but it serves to show that there are good reasons to expect that it might be: Because conversely, if the world were not locally supersymmetric, then there would be reason to ask: why this constraint?"

That's a good question, and I cannot resist to offer a thought. Most people (including me) assume that the Standard Model (or whatever BSM model you want to replace it with) is an effective theory at large length scales of some underlying theory U. This underlying theory does not even have to be a quantum field theory. The bosonic and fermionic fields in the effective theory are effective degrees of freedom that arise in a possibly complicated way from the degrees of freedom of U. In this situation, should we expect that the effective theory allows local spacetime supersymmetry transformations that can mix the effective bosons and fermions?

I think there are essentially two possibilities we have to consider: Either there exist *many* theories U that induce effective theories with bosons and fermions that look roughly like the Standard Model; or there is essentially only one possible U with this property.

In the second case, not having any a priori information about U, I would assign a 50% prior probability (or much less, but I'll be generous here) to the statement that the effective theory allows local spacetime supersymmetry. I.e., I would answer Urs' question with a counterquestion: Why should this constraint *not* exist?

In the first case, however, the situation is different. If bosons and fermions can arise in many different ways from an underlying theory, we should expect that the absence of transformations that can mix them is a pretty generic property in the class of possible theories U. After all, the bosonic fields could appear effectively by a mechanism quite different from the mechanism that produces the fermions. Then we would not expect a symmetry that can mix them, because they are just quite different objects, like apples and orangutans. (In the Standard Model, the bosons and fermions do not look similar at all. I take that as a hint that they arise indeed in quite different ways from the underlying theory, whatever it may be.)

Hence, no matter whether we are in the first or (as I hope) second case or something in between, I do not find it particularly puzzling that the effective theory that describes our universe shows no sign of spacetime supersymmetry. If we would see supersymmetry, that would be an extremely strong clue about the underlying theory: it would have to induce fermions and bosons as effective degrees of freedom in very similar ways, so that the effective theory could mix them. But we do not see it. Why should anyone be surprised? For the stated reasons, I even find it very likely that supersymmetry does not occur at *any* length scale at which the effective theory is valid. And in the underlying theory, the question "Is there spacetime supersymmetry?" might conceivably not even be well-defined.

Maybe SUSY enthusiasts do not like this idea — that the bosons and fermions in our universe arise effectively from an underlying theory U in completely different ways — because they assume that then U would have to be an inelegant, complicated theory? I do not see any argument that would support this assumption.

75. **John Baez**
August 29, 2016

Peter wrote:

> You [Urs Schreiber] are trying to derive from an extremely general abstract theorem (that Tannaka duality works for not just groups but also Z/2-graded groups) an argument for a very specific supergroup, a rather ugly one with no experimental evidence at all for it.

I don't think he's doing that. For starters, he didn't mention any specific supergroup, much less a specific "rather ugly one". Which supergroup are you talking about, anyway? Whatever it is, Urs never mentioned it.

I think the only reasonable attitude is to realize that we are missing many pieces of the puzzle, both experimental pieces and conceptual pieces: that is, ideas we need, that we don't have yet. So, people motivated primarily by mathematical elegance should not expect their work to make contact with experiment in the next few decades, or indeed at all. The time is not ripe for that. They should instead try to do good math. Good math can be recognized independent of any applications to physics, and it has its own inherent value.

I think Urs is doing good math. He's not spending his days working on the minimal supersymmetric extension of the Standard Model or any other specific ugly attempt to ram our current theoretical thinking down the throat of the experimental data we have now. He's instead inventing fundamental new ideas in geometry, topology and algebra – indeed, so fundamental that breaking math into these separate subjects doesn't do justice to his work. Some of his work is on particular theories that string theorist like (showing how they fit into a single structure, the "brane bouquet"), but a lot of his work is more general than that, and thus of more general interest. I'm talking about things like infinity-categories, infinity-topoi, and how these permit a new much more general approach to old topics like Lie groups, Lie algebras, gauge theory, prequantization and so on. These are worth studying *regardless* of any application to physics. In fact, trying to connect them prematurely to particle physics runs the risk of screwing up their natural development, by making us focus on the theories we know and not on the ones we don't.

76. **John Baez**
August 29, 2016

Marc Nardmann wrote:

> I still do not see how the positive mass theorem is an example. To me, it is just an example of the usefulness of spinors in differential geometry — which is profound, but not "super" in a technical sense.

Okay – I was trying to give examples of "supermathematics" (Z/2-graded mathematics), not supersymmetry, but I guess this was a bad example. I was in grad school in the early 1980s when Witten was starting to wow mathematicians with his new arguments for known results. His "physics proofs" of the Atiyah-Singer index theorem and positive mass theorem came out then. Both involved the Dirac operator in ways that surprised mathematicians, so I tend to mentally lump them together.

But you're right, his argument for the positive mass theorem doesn't use Z/2-graded ideas, at least not explicitly. Wikipedia says his argument was "inspired by positive energy theorems in the context of supergravity", and the fourth section of [Witten's paper](#) discusses the connection to supergravity. But he says the connection is "not altogether clear". I don't know if anyone ever nailed it down!

A much better example, from around the same time, would be [Witten's approach to Morse theory](#). I took a course on that with Raoul Bott, and at one point he said, eyebrows wagging mischievously: "So we think about this, and we get stuck. So we need to *super*think!"

Around this time I also took a course from Quillen on Witten's proof of the Atiyah-Singer index theorem. Quillen was trying to make it rigorous using "high-school calculus" and lot of elementary superalgebra. He got scooped by Getzler.

So, I think of this as the era when mathematicians realized the importance of supermathematics.

77. **Peter Woit**
August 29, 2016

John,
I think we agree about strategy: step back and look for new mathematical insights that may later find applications in fundamental physics. Even if you don't get what you want for physics, you'll learn more about deep mathematics, which is all to the good. And sure, Z2-graded mathematics may very well be part of those insights. Now that I'm wrapping up work on the book, I'm looking forward to going back to doing precisely that, thinking about Dirac cohomology.

My problem with Urs is that while he's often doing this sort of thing, at the same time he finds it necessary to try to use this to defend the central failed research program that has dominated (and done a huge amount of damage to) theoretical physics for over 30 years. His argument starting with Z2-graded Tannaka duality ends up with the specific endpoint of an argument for supergravity, in ten dimensions (whatever you want to call the local supergroup there, that's the one I'm referring to). I don't think there's a serious argument there. You can't get to that kind of specific theory from general ideas about the relation of QM, representation theory and Tannaka duality. When you try and do it, you're just adding in lots of unexamined assumptions and eliding distinctions that are exactly the ones you need to be looking at to figure out where this train of inference goes wrong.

Defending 10d superstring theory and supergravity as the fundamental theory while arguing that any possible actual experiments are irrelevant is very dangerous, the "Not Ever Wrong" danger I'm trying to point out. Bringing very abstract not relevant mathematical statements in to help do this is a really bad idea. I think in this year we're going to finally see the collapse of any hope that supersymmetric extensions of the standard model will ever see a test or get experimental support. I hope the community reacts to this by challenging the assumptions that led to enthusiasm for these models, not by permanently seeking refuge in excuses ("only visible at high energy") and dubious invocations of abstract mathematics.

78. **anon**
August 29, 2016

Peter Woit wrote: "I'm suspicious that BRST is more fundamental than people think, really needed to properly understand what is going on with gauge symmetry."

Yes, I remember BRST seeming very ad hoc to me, until I realized I could think of the Faddeev-Popov ghosts as differential forms in disguise, with Q being essentially a version the exterior differentiation d constrained to the gauge directions. It's been a while since I worked on this but I seem to remember convincing myself that the path integral over the ghost action expresses a coordinate-invariant volume form on the gauge surfaces like we would normally do with differential forms. Since then I've thought of BRST as about as fundamental and well-motivates as differential forms are.

I know there are cases where BRST gets a little more complicated and I am not sure how far the correspondence with differential forms can be taken.

79. **Jeff M**
August 29, 2016

John,

Wow, you're giving me memories of grad school 🙂 That Witten paper on Morse theory was the basis of my orals, I had to present it. Honestly, could never work out what Witten was talking about until I read Roe's book on it.

80. **M. Nix**
August 29, 2016

This is nothing new. Kuhn's Theory on the Structure of Scientific Revolutions predicts exactly this sort of response. Individual scientists rarely change deeply-held views, no matter what evidence they're faced with (as with most humans). Rather, as the evidence mounts to discount old theories and promote new ones, new scientists adhere to the newer theories while the adherents of the old theories eventually retire and leave the field. Thus, while individual scientists don't change their views, the outlook of the field as a whole shifts over time.

81. **DrDave**
August 29, 2016

Since everyone spells these Dutch names wrong, Gerard has put up a handy web page to explain:
http://www.staff.science.uu.nl/~hooft101/ap.html
I suppose Gerard probably holds the record for having his name spelled wrong. Don't think he doesn't notice!

And speaking of "wrong", wouldn't it have been nice to see someone in the video just say "I was wrong"?

82. **John Baez**
August 30, 2016

Peter wrote:

> I think we agree about strategy: step back and look for new mathematical insights that may later find applications in fundamental physics. Even if you don't get what you want for physics, you'll learn more about deep mathematics, which is all to the good.

Agreed.

> And sure, Z/2-graded mathematics may very well be part of those insights.

Not "may very well be" – *is*. Let's face it, the realization that particles with half-integer spin obey canonical *anti*commutation relations, and that more generally you need to insert minus signs whenever you switch two odd objects, is precisely Z/2-graded mathematics. Z/2-gradings permeate mathematics, from Clifford algebras and K-theory on up. It's crucial to understand them.

That's a different question from whether particle physicists should care about supergroups that mix bosonic and fermionic physical degrees of freedom. In my opinion there's no sign from Nature that they should.

> My problem with Urs is that while he's often doing this sort of thing, at the same time he finds it necessary to try to use this to defend the central failed research program that has dominated (and done a huge amount of damage to) theoretical physics for over 30 years.

I fought hard against string theory for quite a while. I only started working on it after I gave up on fundamental physics and decided to enjoy some of the math for its own sake. So I'm a weird case. But I completely sympathize with how tough it is for people who work in the string theory community to break free of its core beliefs. I know because I broke free of loop quantum gravity.

Once you spend a few years working with people, pursuing the same majestic dream, going to conferences with them, becoming friends with them, it really feels tragic to throw in the towel and admit they're probably on the wrong track. For a while you feel life is pointless – because nothing else feels as exciting as the dream you once had. And then you have to make a decision: you can either stay in close contact with your former colleagues and spend your time arguing with them, casting doubt on their dreams, or – as I did – you can leave them behind, staying friendly but not talking much about your new work.

So we should never expect string theorists to admit in a public announcement that they lost a bet because they were wrong. They will either succeed someday… or gradually fade away.

> … dubious invocations of abstract mathematics.

On a wholly separate note, not related to this supersymmetry business, I wish people would stop saying the phrase "abstract mathematics" in a critical tone of voice. Mathematics is abstract, and it's been so at least since 1600 BC when Babylonians were solving some cubic equations. Abstraction is fundamental to mathematics. It's good. If people said "mathematics I don't understand", I'd be perfectly sympathetic and try to help.

I think your real complaint has nothing to do with Deligne's theorem being "abstract": it's that after applying it to see particles should form a representation of some supergroup, Urs goes the extra mile and throws in a plausibility argument that maybe, despite the experimental evidence, this supergroup should not be a mere group:

> Again, this does of course not prove local spacetime supersymmetry, but it serves to show that there are good reasons to expect that it might be: Because conversely, if the world were not locally supersymmetric, then there would be reason to ask: why this constraint? Why, if provided with the possibility to choose from the class of all super-groups, and with phase space of verified physics already being a supermanifold, why would Nature choose a purely bosonic symmetry group? It's possible, but it shows that it is not insane to suspect that fundamentally (i.e. at high energy) it might be different.

The reasoning here is hand-wavy, but the conclusion – that it's *"not insane to suspect"* the existence of supersymmetries – is so weak that I couldn't possibly argue against it. If nobody were working on supersymmetry, I'd say we should definitely put some people on it. The problem, in my mind, starts when a research community devotes a huge amount of time to studying this possibility and not as much to studying the alternatives.

83. **Urs Schreiber**
August 30, 2016

Peter, for one, it's not "Z/2-graded Tannaka duality" in Deligne's theorem. It's plain Tannaka duality for tensor categories in characteristic zero. That's the force of the theorem and the point for the present discussion: Starting with something god-given that does not mention super-symmetry, the theorem says that it secretly is governed by super-symmetry.

I'll leave it at that here. If anyone feels like discussing aspects of what I explained, let's do so in it's own comment thread here.

84. **JE**
August 30, 2016

Urs said, "so why on earth would Nature then choose to constrain this possibility and have an ordinary group act on the supermanifold, if it could choose a supergroup".

Because she is whimsical enough to dislike susy?

As forceful and mathematically appealing as it may seem, the fact that susy is now known to be nonexistent or totally out of experimental reach should suffice to abandon the idea that it (in its present form and unless miraculously proved otherwise in some foreseeably distant future) plays a key role in fundamental physics.

85. **Urs Schreiber**
August 30, 2016

High energy supersymmetry (supergravity) need not necessarily be experimentally out of reach. The Starobinsky model of cosmic inflation, preferred by the PLANCK satellite data, is argued to work even better after embedding into supergravity. See here for more discussion.

86. **Marc Nardmann**
August 30, 2016

John Baez wrote: "A much better example, from around the same time, would be Witten's approach to Morse theory."

I agree, that's another good example.

John: "[Witten's] argument for the positive mass theorem doesn't use Z/2-graded ideas, at least not explicitly."

They do not even occur implicitly. Not even the Z_2-grading of the Clifford algebra (i.e., its superalgebra structure) is used in the proof.

It's amusing that Witten's motivation for the spinorial proof of the positive mass theorem was a "not altogether clear relation" to supergravity. There would have been a much more natural and quite obvious motivation: the 1963 work of André Lichnerowicz about obstructions to nonnegative scalar curvature on closed manifolds. (Witten cites this work in this article, but only in passing, as an example that "spinors have been used in the past to prove various results that are at least roughly related to the positive energy theorem".) The path from Lichnerowicz' work to the spinorial proof of the positive mass theorem is straight and seems in hindsight hard to overlook. I could explain it here in a few paragraphs, but that might be off-topic. At least from the late 1960s on, there must have been people who knew both, Lichnerowicz' work and the (then open) positive mass conjecture. No outburst of ingenuity would have been necessary to find the spinorial proof. It's remarkable that nobody discovered it before 1981, and that even then the motivation was not the obvious one.

87. **Peter Woit**
August 30, 2016

Urs,
You're just ignoring the problem I'm pointing out, that of a huge leap of logic from this theorem to 10d supergravity. If you're going to make huge leaps of logic, they should at least land you somewhere new and interesting.

As for Planck data providing evidence for supergravity, I'm unfortunately an expert on indefensible claims about experimental evidence of this kind, but this is a new one to me, taking this kind of claim with nothing behind it to a new level.

88. **Giotis**
August 30, 2016

I'm not sure why we have to motivate mathematically SUSY when we have a very strong physical motivation to begin with.

The primary reason is that SUSY necessitates fermions and relates bosons and fermions in a deep way.

A five-year old will tell you that if you extend bosonic space-time to supersymmetric space-time you get fermions (a QM concept really) and if you make global SUSY local (as you ought to) you get gravity within the general context of Supergravity. Of course the only known way to make SUGRA UV finite is to UV complete it via String theory.

So you can think SUSY as the only consistent extension of special relativity, SUGRA as the only consistent extension of GR and String theory as the only consistent extension of QFT that can incorporate Gravity and give you Gauge Symmetry as a free bonus.

Or in another language you extend the Poincare group (SR) to Super Poincare (SUSY), the local Poincare (GR) to SUGRA and you make SUGRA consistent via String theory.

This is the bottom up approach; top down you can start with a bosonic String, follow consistency and you get the same picture and eventually the known Physics as an approximation.

89. **Urs Schreiber**
August 30, 2016

Giotis, this is a tautology:

> if you extend bosonic space-time to supersymmetric space-time you get fermions

Similarly this is a tautology:

> if you make global SUSY local (as you ought to) you get gravity within the general context of Supergravity.

I know that some sources make advertisements of this sort, but just repeating them doesn't make these statements less empty. Supersymmetry in physics by definition is an extension of the Poincare Lie algebra by fermions (check it out here), so of course it gives fermions and of course its local gauging gives gravity, that's by design, not by miracle. The miracles of supersymmetry lie elsewhere.

90. **Noboru Nakanishi**
August 30, 2016

I would like to summarize the theoretical reasons why the supersymmetric extension of Poincare algebra (SUSY) is not expected to be a correct physical theory.

1. Many people misunderstand the no-go theorem of Coleman-Mandula and Haag et al. It is a proposition for the observable symmetry, that is, it refers only to the symmetry of the physical S-matrix. Since unbroken SUSY is not realized in the real world, what we want to have is the proposition for the symmetry of Lagrangian. But nothing can be claimed for the Lagrangian symmetry; indeed, one can construct a counter-example. Thus, SUSY is not a privileged symmetry.

2. If SUSY is spontaneously broken, there must exist the Nambu-Goldstone fermion, but it is not observed. If one appeals to the super-Higgs mechanism, one must give up the important hypothesis that in the low energy region the quantum effects 0f gravity are completely negligible. If one does so, the brilliant success of SM must be regarded as merely accidental.

3. Quantum supergravity does not contain the global SUSY, that is, quantum supergravity is not a natural extension of SUSY. In classical supergravity, internal local symmetries can be easily translated into spacetime local symmetries by means 0f vierbein. But the same is no longer true for the global quantities because vierbein is a local field. Indeed, {Q, Q^(bar)} cannot have the spacetime index (mu) of the translational generator P_(mu), that is, SUSY is not reproduced.

4. In Einstein gravity, the general-coordinate-trasformation (GCT) algebra already contains not only the translation algebra but also the Lorentz algebra, that is, the Poincare algebra is a subalgebra of the GCT algebra. And the local Lorentz algebra is completely independent of it. In quantum Einstein gravity, the globalized versions of both symmetries are spontaneously broken; the unbroken physical Poincare algebra arises as a special combination just like the electromagnetic symmetry of the electroweak theory. Thus the physical Poincare algebra is a symmetry at the representatkon level. Therefore, it is quite unnatural to supersymmetrize the Poincare algebra at the Lagrangian level.

91. **Giotis**
August 31, 2016

Urs,

They are tautologies of course, that's the whole point; even so textbook tautologies are much better than often repeated, self-advertised, grandiose esoteric claims about miracles and novelties that nobody seems to pay attention to (in theoretical physics side at least)

92. **random reader**
August 31, 2016

Urs,

there are a couple of problems with the arguments from Deligne's theorems.

First, there is some fine print that you overlook in those theorems such as group vs group scheme, and algebraic groups vs pro-algebraic groups, that means the objects can be materially different from the smooth (super) Lie groups and Lie algebras as those terms are understood in physics. It would be interesting in the non-supersymmetric case to understand whether this more algebraic type of structure, such as a group scheme, is useful anywhere in the physics, and also whether the theorems like Deligne's hold in smooth or complex-analytic settings.

Second, the standard model (and most other non-SUSY QFT) uses an ordinary, non-supersymmetric, finite dimensional, gauge group, but some of the irreducible representations of that group are fermions. Having fermions in the theory therefore does not imply anything about the group being Z/2 graded. Deligne's theorem tells us that supergroups are complicated enough to accomodate any experimentally observed tensor product rules for particles (i.e., irreducible representations). To make a case for supersymmetry using Deligne's theorems, you would need some no-go theorem for ordinary Lie groups, explaining what limitations on the allowed sorts of tensor products are imposed by the absence of SUSY.

93. **Doug McDonald**
August 31, 2016

On topic: other SUSY bets. In particular Lubos Motl and Gordon Kane. I note that the two LHC main detectors are now both above 25 inverse fb for this year. That adds up to above 50, plus the bits from last year. Lubos's bet says 30, but does that mean combined detectors or just one? You (Woit) quote Kane as saying 300 inverse fb, but Tommaso Dorigo quotes Kane "Now that we can predict the gluino mass from compactified M theory we know that superpartners should not have been expected in LHC Run 1 because the gluino mass is about 1.5 TeV, and it will appear in Run 2 once the luminosity gets over 15-20 fb-1." which while not a bet is a very confident numerical prediction.

At a total of over 50, despite the total lack at the recent meeting, the analysis people and their stable of leakers would surely have leaked something as important as a SUSY-ascribable bump.

Presumably CERN has computerized autoanalysis constantly updating the
data plots and their bump standard deviations.

94. **Peter Woit**
August 31, 2016

Doug McDonald,
A problem with many of these bets is that they were made before the LHC turned on, and ran into trouble, forcing Run I to be at lower (8 TeV) energy. So, the people on the SUSY side of the bet might argue that only the data at full energy should count (actually, design energy was 14 TeV and they are running at 13 TeV, but nobody thinks that difference matters).

Putting together this year and last year's data, they're at about 30 inverse fb of 13 TeV data. So, when this data is analyzed, Lubos's bet is lost. I gather he'll hang on and not give up for a while, since there are lots of ongoing analyses. No, I've heard no rumors of SUSY signals in Run 2 data.

That "prediction" of Kane's is just the latest one in a long list of falsified ones. He's never in the past acknowledged that a prediction being wrong has any implication for the theory he is pushing, don't see why he should start now.

95. **random reader**
September 1, 2016

Another thing about the argument for SUSY using Deligne's theorem(s) on tensor categories is that Deligne refers to bare linear representations whereas the representations used to represent elementary particles involve unitary or projective structure on the representations, or positive-energy conditions.

OK, one might think that if supergroups are enough to accomodate general categories of finite-dimensional linear representations, then any extra structure on the representations must come from some extra data decorating the supergroups, in a way that would not change the basic idea of supergroups being the structure dictated by tensor categories. But having such a straightforward picture is only a hope, not something that would have to occur. Or if it does occur, the extra data could beef up the supergroups into more exotic categories of objects that we were hoping to avoid (as a flight of fancy to illustrate the idea: take a quadratic form to give the unitary structure, from that try and construct a Poisson bracket, which happens to be deformation data to form a quantum group… and if that is too constrained then deform in the even more exotic quasi-quantum category).

It is easy to glibly think of super Lie groups and algebras as some sort of next-more-complicated thing after Lie groups, but they are actually qualitatively more complex and Deligne's theorem is an indication of that. Another one is that the finite dimensional simple Lie superalgebras are not classified by discrete data, there are continuous moduli for some types of them.

p.s. In the preceding comment I wrote "gauge group" where I should have written (super)Poincare group, which is the thing whose representations are particles that are tensored together. But the same point applies, that having fermionic things among the representations does not by itself imply Z/2 grading.

96. **bk**
September 1, 2016

Urs,

while I sympathize with your standpoints, in full contrast to other's,
for sake of correctness I need to point out that your statement

"….) just so happens to be 2d supersymmetric (and called the "superstring" only once this was realized), which in turn is interesting because it miraculously implies that the effective spacetime theory of which these strings are quata is locally 10d supersymmetric.."

is misleasing if not wrong, despite it has been claimed so for zilli0ns of times.
Take as counterexamples the "other" 10 dim string theories that are not spacetime susy inspite of having world-sheet susy. Eg the heterotic string with O(16)xO(16) symmetry.
These theories are almost never mentioned when talking about the grand scheme of things, for some reason.

97. **GoletaBeach**
September 5, 2016

On Dine's quote… it is buried in a popular article, certainly it is not the banner headline… even the 2 sentences before are: "Despite those successes, good reasons exist for skepticism. Some are experimental: Apart from coupling unification, no direct evidence yet argues for supersymmetry." Not denying that Dine went overboard in the next sentence, but I don't think that quote is a main thrust of the article, or, within a light year of the consensus view of the experimental particle physics community.

Just have to go back a few months in Physics Today for a sober article by Fabiola Gianotti and Chris Quigg… quoting from that one "theorists have advanced many extensions of electroweak theory—supersymmetry and technicolor, for example—that compel additional new phenomena near 1 TeV." They get totally right, 4 or so years in advance: "An essential first step toward decoding electroweak symmetry breaking is to find the Higgs boson and learn its properties. One promising channel for an early discovery is H → ZZ → 4 charged leptons. Backgrounds are very small, so even a handful of events would suffice for a claim of discov- ery. Establishing the signal will require efficient electron and muon reconstruction and identification down to a few GeV."

On Kane, "That "prediction" of Kane's is just the latest one in a long list of falsified ones. He's never in the past acknowledged that a prediction being wrong has any implication for the theory he is pushing, don't see why he should start now."

Well, I've heard him acknowledge just that many times. He is an honest guy. I don't really understand what data you have used to support such a strong statement.

I can't think of any useful reasons to beat up optimists, except if they have directly suppressed good science. I don't think Dine and Kane have done so; optimists generally don't suppress good science, they encourage too promiscuously.

Their enthusiasm has pushed the field forward, and the personal embarrassment they suffer from wrong predictions is a sociological issue, and irrelevant for getting good physics done. If any thing, making predictions that prove to be wrong and that embarrass in the long run is helpful to the progress of physics…. the right idea might well eventually be stimulated by the wrong prediction.

Only in retrospect does physics seem to be sort of linear… the advance of physics is always more like diffusion, with lots of backward and forward motion. Sideways too. Overly enthusiastic ideas are just part of the diffusion process.

98. **Peter Woit**
September 5, 2016

GoletaBeach,

One problem with writing a prominent article/book saying "SUSY will either be discovered at the LHC or is wrong", and then refusing to acknowledge that SUSY is wrong when the LHC doesn't see it is that you destroy your own credibility if you do this. Kane already has passed the point where anyone thinks his "predictions" are credible, and while Dine has been more circumspect, if he tries to make more such claims about SUSY at higher energies, few will take them seriously.

Unfortunately, when this kind of behavior is widespread, it becomes a problem not just for certain people's credibility, but for the general credibility of the field, and that has serious implications. In another comment thread, someone pointed out that CN Yang has publicly come out against the proposed Chinese collider. One of his main arguments is that SUSY is being put forward as a reason for building such a machine, and that this motivation lacks credibility. He's probably aware of pre-LHC arguments like those of Dine and Kane, not willing to tolerate them any more, and this will have implications for those trying to make the case for a new collider.

99. **Low Math, Meekly Interacting**
September 6, 2016

FWIW, I have many middle-aged colleagues who grew up in mainland China. None of them have a physics background beyond the requirements to get their degrees in other areas, like medicine or biochemistry. They all know who C.N. Yang and T.D. Lee are and can tell you in impressive detail what they shared the Nobel for, K mesons, parity violation, the whole deal. If I ask one of my contemporaneous American colleagues who our most famous physics laureate is, I would likely get an answer like "um…Einstein?" (technically correct, but…) For reasons I don't claim to fully understand, these Chinese folks had it drilled into their heads during their pre-graduate educations that Lee and Yang were exemplars of Chinese civilization, and are worthy of reverence. If Yang is going around saying SUSY is a waste of time and money, I'd guess it would exert oversized influence, at least on the generation that now is predominantly represented in the Party of the PRC.

100. **GoletaBeach**
September 7, 2016

As I said, in the sentence prior to the one you quote in the Dine article, he points out a good reason to be skeptical of SUSY… "Despite those successes, good reasons exist for skepticism. Some are experimental: Apart from coupling unification, no direct evidence yet argues for supersymmetry."… yes, his subsequent sentence that LHC will be deterministic for SUSY is simply wrong, and in no way (ever) represented the consensus view of the experimental community. And was not a sufficient portion of the article to warrant a retraction, in my opinion.

The Fabiola Gianotti and Chris Quigg article that same year is much closer to the consensus view of the community.

I don't think any of the LHC technical documents (yellow books, technical design reports, etc) say anything like ""SUSY will either be discovered at the LHC or is wrong". If you know of anyplace those documents support your assertion, I'm all ears.

Everyone serious knows that even the simplest implementations of SUSY, like the CMSSM, are not close to eliminated by LHC data. See for example, Leszek Roszkowski's talk at IDM 2016: https://idm2016.shef.ac.uk/indico/event/0/timetable/#20160719.

Any claim that "SUSY is wrong" is not currently supported by data or phenomenology. Pointing to a few people who said that is sort of like pointing to a few glaciers that are expanding and claiming that global warming is wrong.

Do any theorists, whether Gordon Kane or Frank Yang, have any significant credibility with their predictions? Einstein himself was wrong about a lot, from the cosmological constant to quantum mechanics, and he was even childish and petulant in his errors about gravity waves, as Barry Barish has been pointing out in his recent talks.

The mistake is ever to attribute credibility *for sweeping predictions* to theorists. If they can bat 0.001 that actually an amazing record on the "big questions". Batting 0.300 has never been realistic.

I think the mistake is to get snide and dark over the 0.999 of the time theorists are wrong about big stuff. Because… first of all, it is too easy, it is like shooting fish in a barrel. Second of all, being snide and dark only suppresses the speculation that is necessary for the occasional right theoretical idea to pop up.

101. **Peter Woit**
September 7, 2016

GoletaBeach,

The Frank Yang problem I was pointing was not bad predictions from him, but he's a good example of the danger you face when you choose to overhype an idea about physics (and lots of people did this with SUSY and the LHC, not just Dine and Kane). Serious physicists like Yang will notice you're doing this, and after your "predictions" don't work out, and you try and pull the same thing again, they'll call you on it. And some of them may be extremely influential with the government officials you need funding from.

By the way, there's yet another news story about this https://www.sciencenews.org/article/supersymmetry%E2%80%99s-absence-lhc-puzzles-physicists

I'll add an update to the posting.

102. **GoletaBeach**
September 7, 2016

Nice Science News article… actually the Frank Yang comment has harmed his reputation with me, now that I look carefully at it. I am getting repetitive, but if you choose a subset of the community (say, Kane, Dine, some others you bring up) and then color everyone with their comments, *and*, if you ignore the measured consensus documents like all the technical design reports, yellow reports, Snowmass process, etc…. well… you are doing the same thing climate deniers do.

Maybe climate deniers are in the end correct, but their application of the scientific method is unconvincing.

Building the highest energy machine is always a good scientific idea. In fact we have learned that the SSC had more discovery potential than the LHC, ex post facto (I know you didn't want any more collider discussion, but you reintroduced with Frank Yang's comment about the Great Collider).

The only issue is cost. If we could do 100 TeV for the cost of the LHC, we should do so, independent of SUSY or technicolor or multiverses or any theoretical fashion.

Because discovery is an empirical science in the end. But the LHC cost has been a kind of wall that governments cannot get beyond, maybe fairly.

If Frank Yang is anti-discovery, well, his reputation suffers with me.

103. **Peter Woit**
September 7, 2016

GoletaBeach,

I agree that a new energy frontier machine is desirable. My only point is that if you try and make the case for it with the argument "a 100 TeV collider will tell whether or not there is SUSY" (I've already seen this argument made publicly, in this form, will see if I can remember the source), you're handing over to those who disagree with you excellent evidence that you lack credibility.

104. **GoletaBeach**
September 8, 2016

Sure, there are always people who overstate the case for anything… most politicians, most fitness gurus, some scientists.

For the LHC, the serious case was made in DOE technical design reports, CERN yellow reports, etc. I don't think you'll find a statement like "the LHC will either confirm or refute SUSY" in any of those reports, but if you can point to an example, I'm interested in knowing. Quigg and Gianotti in their 2007 Physics Today seem to me to have respected the foundational LHC reports, and Dine did not.

Similarly for any serious 100 TeV proposal… there will be lots of careful scientific projection and effort that will be documented very responsibly, and that is the corpus of work that will form the foundation of the proposal.

I'm not comfortable with replacing all that good science with the overenthusiastic or exaggerated claims of some theorists, or some promoters, or whoever. Maybe in the superficial media world things like that happen all the time… the glib talking heads get to drive the bus.

At least within the world of science, the real science should matter. However, there is freedom of speech and opinion… if Nima Arkani-Hamed or David Gross or Gordon Kane or Michael Dine wants to try to give their personal spin, I see no way to stop them.

Well, maybe if China chooses to build the collider they could imprison anyone saying something not consistent with all the design reports…. China certainly does that on other political issues. I'm not being serious, of course.

Even if China did that, any talking head with an opinion could always just hide out in the Ecuadoran embassy and Skype their opinion to the world (not serious about that either).

In the end Hollywood rules are probably about as practical as anything: 1)if your name gets publicized, your first concern should be if they spelled it right, not what the nature of the publicity is; 2)the only bad publicity is your obituary.

Templeton News

September 1, 2016
Categories: Uncategorized

Looking at my list of items to blog about, I see most of them have some relation to the Templeton Foundation, so this will be a blog post just about those. To get some idea of the scale of Templeton's activities, at the end of 2014 they had about \$3.2 billion in assets, and during 2014 had given away about \$185 million. For comparison, the NSF budget for FY2014 for physics was $267 million and for mathematics \$225 million.

One of the main goals of the foundation is to bring together science and religion. Among the many things they are funding to accomplish this is a \$871,000 grant to Arizona State University to fund Think Write Publish Fellowships in Science and Religion. If you're a hard-up writer, these people will give you the opportunity to get $10,000 to write "creative nonfiction stories about harmonies between science and religion" and help you get them published.

Over the next few years, as you see things like this make it into the media, realize that this is not evidence of an intellectual trend, but a reflection of Templeton money and their agenda. ASU's Lawrence Krauss is, for good reason, not happy.

To give an idea of the range of Templeton's influence, just at ASU they're funding several other large grants, including $745,000 for Representations of God (this and this), and $544,000 for emergent gravity. When you notice conferences, seminars, public lectures, etc. about "emergent gravity" in coming years, realize that some of them are happening because of Templeton's agenda (one of the PIs is a Templeton Prize winner).

One of Templeton's largest recent grants has been \$4.7 million to FQXI for research into "Physics of the Observer". Among other things, this funded a recent conference at Banff.

A major interest of Templeton's over the years has been "Genius". Another of their large recent grants has been to the World Science Foundation for its Cultivating Genius Initiative.

Finally, there will be an interesting mathematics conference related to quantum field theory at Harvard October 8-10. I'll likely be up in Boston visiting my brother and hope to maybe attend some of the talks. Funding for this is coming partially from the "Templeton Charity Foundation Switzerland". I guess this is these people, some off-shoot of the Templeton Foundation, with exactly the same interests, They say they have made \$85.2 million in grants, a list is here.

Update: I was thinking of commenting that Templeton at least seemed to have slowed down its efforts to promote multiverse mania. But then I noticed this. If you want to know why Ira Flatow on NPR keeps bringing up the multiverse, \$150,000 in Templeton money might have something to do with it…

Update: I keep on finding out about more of these Templeton-funded things, they are endless. Templeton is funding an Institute for Cross-Disciplinary Engagement at Dartmouth. Themes to be investigated are "Can science alone explain the nature of reality?", "Is there free will?" and "Is there purpose in the universe?". Among their many activities will be an event featuring a dialogue between Sean Carroll and a Buddhist Scholar in San Francisco in February.

Comments

1. **Thomas Larsson**
   September 2, 2016

   I'm all in favor of studying physics of the observer. After all, every real experiment is an interaction between a system and an observer, and the outcome depends on the physical properties of both. Studying an aspect which is present in all experiments and which has been hitherto rather ignored seems like a promising approach to make progress.

   Of course, an "observer" does not have to be conscious or even alive. Perhaps "probe" is a better word, or "test particle" in the context of GR.

2. **Bernd**
   September 2, 2016

   Is "emergent gravity" somehow related to "intelligent falling"?

3. **Jess Riedel**
   September 2, 2016

   As someone who attended the Banff conference, whose thesis was ~20% funded by Templeton, and who is applying for a grant, I strongly agree with Peter's wariness and the danger of interpreting forthcoming fads as meaningful intellectual trends rather than just the effect of funding incentives. This is important to highlight. That said, I think the Templeton funding is a definite net positive for the world (my own work aside, of course). They aren't a lone nefarious bias in an otherwise pristine academia, they are just one of many, many distortionary incentives, created by mostly well-meaning human beings.

   Yes, if Templeton really was an O(1) fraction of the funding for physics (or any other subject), what would be serious cause for concern, but the numbers given above are misleading. The large majority of Templeton money is *not* going to math and physics research, so it's not fair to compare all $185M to the $492M total NSF math and physics funding. Take a look here for the Templeton Foundation's Math & Physical Science grant winners. They gave $12.2M in 2014, $17.4M in 2015, $7.5M so far in 2016. I encourage concerned folks to go read the grant descriptions for signs of nefarious religious influences.

   Ultimately we have to reply on the marketplace of ideas. If the marketplace breaks (say, because physicists are more dysfunctional than mathematicians when unconstrained by experimental data), it can't be saved by rallying the group to exclude certain funders.

   Thanks for the very useful post, Peter!

4. **Jonathan Miller**
   September 2, 2016

   They have a lot of money, I should make a grant application.

5. **Peter Woit**
   September 2, 2016

   Thanks Jess,

The numbers were meant more to give an idea of the overall scale of the resources Templeton is devoting to its agenda. To compare the impact on physics and mathematics you'd have to look at different numbers. Templeton does very little to support mathematics research, so has just about no impact there. For physics research (unlike the NSF) they do very little on the experimental side, which is the most expensive.

6. **sebastian**
   September 2, 2016

   Judging from the invited speakers, the conference is mainly about the operator algebra approach to 2 dimensional conformal QFT.

7. **Jess Riedel**
   September 2, 2016

   > For physics research (unlike the NSF) they do very little on the experimental side, which is the most expensive.

   Yep, that's a good point.

8. **Matt Leifer**
   September 2, 2016

   I am not completely comfortable with taking Templeton money, but on the other hand there really is nobody else funding work on the foundations of physics, so if you want to work on that where are you supposed to go?

   I do think we need a private funding body that has a skeptical agenda as a counterpoint to Templeton. In an ideal world, the NSF would be doing that, but in the current political climate that is not going to happen.

9. **Kevin Henderson**
   September 2, 2016

   It appears Templeton will continue to fund research which will likely never match their aspirations. It is innocuous to apply and accept grants from them as it only benefits the researcher.

10. **Peter Shor**
    September 3, 2016

    If, on the religion side, the Templeton Foundation supplies a balance to the creationists and the other virulent anti-science religious types, it may end up doing much more good than harm.

    Of course I, and most readers of this blog (I suspect) can only judge what effects the Templeton Foundation is having on the science side, where it seems to be doing things like putting more emphasis on emergent gravity than is really justified.

11. **Dragster**
    September 4, 2016

    > For physics research (unlike the NSF) they do very little on the experimental side, which is the most expensive.

    Is that still true? Looking at the 2016 grants, their second largest is $1.5M for this project:

    A tabletop-scale experiment to probe for new particles and forces responsible for the predominance of matter over antimatter in the Universe

12. **Tim May**
    September 4, 2016

    I recall that a semi-crackpot organization called the "Gravity Research Foundation," or something similar, funded some important GR work in or about the about the 60s, maybe as late as into the 70s.

    They were interested in anti-gravity stuff, but they funded some important GR things.

    Didn't seem harmful. Everyone knew the funding was coming from some near-crackpots, but, hey, it was there money.

    –Tim May

13. **Peter Woit**
    September 4, 2016

    Tim May,
    They're still around, see Wikipedia or their web-site for more of their story and what they're up to. As far as I can tell, they've got about $1 million in assets, and their only expenses going to further physics are about $10,000/year for their essay awards. So, we're talking .01% of Templeton.

    Actually, doing a little investigation of that Foundation, there are some analogies with Templeton. It appears to be run by the son (George M. Rideout, Jr.) of the guy who, together with Roger Babson, started the thing (and a large chunk of the Foundation income goes to him). The founders were, like Templeton, in the investment advisory business. Also like Templeton, there is an associated Foundation supporting religion, the "Open-Church Foundation"), also run by George M. Rideout, Jr.

14. **Peter Woit**
    September 4, 2016

    Dragster,
    From their grant descriptions it appears that support of experimental research is still quite unusual and a small proportion of what they do. On the other hand, at NSF physics, I believe more funding goes to experiment than theory (although a quick search hasn't turned up numbers categorized that way).

15. **Daniel**
    September 4, 2016

    An off-topic news, CN Yang just officially opposed to the building of the large collider. This may have important effect on Chinese government's decision.

16. **Peter Woit**
September 5, 2016

Daniel,
Thanks, but I want to discourage discussion of this here since it is off-topic. For those interested in this, there is more here
http://www.sixthtone.com/news/china-should-not-build-particle-collider-says-nobel-winner
and surely Chinese speakers can find other sources.

17. **Larry Lurio**
September 5, 2016

We (NIU) just had a colloquium from Alexey Burov on "Genesis of a Pythagorean Universe" which was based on an essay that won an FQXi award http://arxiv.org/abs/1411.7304. The talk claimed to disprove the unconstrained multiverse hypothesis since there was an infinite probability of each universe containing the seeds of its own destruction. The resolution was a constrained multiverse constructed by "The Mind" which wished to be observed. I was a little surprised that religious based philosophy of science would win an award from a foundational questions institute, but after reading your information about the source of funding, I am less surprised.

18. **Mozibur Ullah**
September 10, 2016

I just reread the beginning of Feynmans lectures on physics, and his polemics against philosophers which I enjoyed when I was younger now just make me feel a little annoyed; the same went when I looked at Susskinds lectures where he suggested, for example, that Aristotle supposed F=mv – which quite missed the point of Aristotles subtle analysis what change requires.

So I'm all for a dialogue between philosophy, religion and physics – and oh, mathematics.

If the Templeton organisation is over-funding emergent gravitation it certainly suggests that they ought to have input from more physics-wise physicists; but how can this happen if they're eyeing each other nervously/reluctantly?

Weapons of Math Destruction

September 6, 2016
Categories: Book Reviews

Cathy O'Neil's important new book [Weapons of Math Destruction](), is out today, and if you're at all interested in the social significance of how mathematics is now being used, you should go out and get a copy. She has been blogging for quite a while at [Mathbabe](), which you should be following, and is a good place to start if your attention span is too short for the book.

Cathy has had an interesting career path, including a period as my colleague here in the math department at Columbia. She left here to pursue excitement and fortune at a top hedge fund, D.E. Shaw, where she had a front-row seat at the 2008 financial industry collapse. A large factor in that collapse was the role played by mathematical models, and her book explains some of that story (for another take on this, there's [Models.Behaving.Badly]() from another Columbia colleague, Emanuel Derman). As far as I've ever been able to figure out, the role of mathematical modeling in the mortgage backed securities debacle was as a straightforward accessory to fraud. Dubious and fraudulent lending was packaged using mathematics into something that could be marketed as a relatively safe investment, with one main role of the model that of making it hard for others to figure out what was going on. This worked quite well for those selling these things, with the models successfully doing their job of obscuring the fraud and keeping most everyone out of jail.

While this part of the story is now an old and well-worn one, what's new and important about *Weapons of Math Destruction* is its examination of the much wider role that mathematical modeling now plays in our society. Cathy went on from the job at D.E. Shaw to work first in risk management and later as a data scientist at an internet media start-up. There she saw some of the same processes at work:

> In fact, I saw all kinds of parallels between finance and Big Data. Both industries gobble up the same pool of talent, much of it from elite universities like MIT, Princeton and Stanford. These new hires are ravenous for success and have been focused on external metrics – like SAT scores and college admissions – their entire lives. Whether in finance or tech, the message they've received is that they will be rich, they they will run the world…
>
> In both of these industries, the real world, with all its messiness, sits apart. The inclination is to replace people with data trails turning them into more effective shoppers, voters, or workers to optimize some objective… More and more I worried about the separation between technical models and real people, and about the moral repercussions of that separation. If fact, I saw the same pattern emerging that I'd witnessed in finance: a false sense of security was leading to widespread use of imperfect models, self-serving definitions of success, and growing feedback loops. Those who objected were regarded as nostalgic Luddites.
>
> I wondered what the analogue to the credit crisis might be in Big Data. Instead of a bust, I saw a growing dystopia, with inequality rising. The algorithms would make sure that those deemed losers would remain that way. A lucky minority would gain ever more control over the data economy, taking in outrageous fortunes and convincing themselves that they deserved it.

The book then goes on to examine various examples of how Big Data and complex algorithms are working out in practice. Some of these include:

- The effect of the *US News and World Report* algorithm for college ranking, as colleges try and game the algorithm, while at the same time well-off families are at work gaming the complexities of elite college admissions systems.
- The effects of targeted advertising, especially the way it allows predatory advertisers (some for profit educational institutions, payday lenders, etc.) to very efficiently go after those most vulnerable to the scam.
- The effects of predictive policing, with equality before the law replaced by an algorithm that sends different degrees of law enforcement into different communities.
- The effects of automated algorithms sorting and rejecting job applications, with indirect consequences of discrimination against classes of people.
- The effects of poorly thought-out algorithms for evaluating teachers, sometimes driving excellent teachers from their jobs .
- The effects of algorithms that score credit, determine access to mortgages and to insurance, often with the effect of making sure that those deemed losers stay that way.

Finally, there's a chapter on Facebook and the way political interests are taking advantage of the detailed information it provides to target their messages, to the detriment of democracy.

To me, Facebook is perhaps the most worrisome of all the Big Data concerns of the book. It now exercises an incredible amount of influence over what information people see, with this influence sometimes being sold to the highest bidder. Together with Amazon, Google and Apple, our economy and society have become controlled by monopolies to an unparalleled degree, monopolies that monitor our every move. In the context of government surveillance, Edward Snowden remarked that we are now "tagged animals, the primary difference being that we paid for the tags and they're in our pockets." A very small number of huge extremely wealthy corporations have even greater access to those tags than the government does, recording every movement, communication with others, and even every train of thought as we interact with the web.

These organizations are just starting to explore how to optimize their use of our tags, and thus of us. Of the students starting classes here today in the math department, our training will allow many of them to go on to careers working for these companies. As they go off to work on the algorithms that will govern the lives of all of us, I hope they'll start by reading this book and thinking about the issues it raises.

Comments

1. **amz review**
   September 6, 2016

   read this customer review at Amazon (3 stars)

   https://www.amazon.com/Weapons-Math-Destruction-Increases-Inequality/product-reviews/0553418815/ref=cm_cr_dp_text?ie=UTF8&showViewpoints=0&sortBy=helpful#R1HO7IVVTLX9VL

   I struggled with the star rating for this book. There are certainly aspects of the work that merit five stars. And it is VERY thought-provoking, like a good book should be. But there are flaws, significant ones, with the biggest flaw being a glaring over-simplification of many of the systems that O'Neil decries in the book. I don't know if O'Neil has personally ever had to take a psychology test to get a job, worked under the Kronos scheduling system, lived in a neighborhood with increased police presence due to crime rates, been victimized by insurance rates adjusted to zip codes, and endured corporate wellness programs. But all of those things have been a part of my life for years, and even I have to admit the many positive aspects of some of these systems. A few examples:

   –Kronos. Despised by the rank and file of companies that I've worked for, Kronos software contains many aspects and automates things that previously were done by people, mostly managers. I hated it, but I have to admit overall it made things more fair. Why? Well, say you have a workplace policy that mandates chronically-late employees be written up for tardiness and eventually fired if they don't shape up. What tended to happen at multiple companies I worked for was that managers would look the other way when their buddies were tardy, and write up people they didn't like. Kronos changed that, because the system automatically generated write-ups for any employee that clocked in late too many times. Kronos has no buddies. Popular, habitually-late people suffered, but it was more "fair" in the true sense of the word. Some systems, like Kronos, have both aspects that level the playing field and aspects (like increased scheduling "efficiency") that can victimize workers. O'Neil tends to harp on the negative only, and if you have not personally seen both sides of system, you might not realize there was another side at all.

–Increased police presence in high-crime areas. This one really grated me the wrong way. O'Neil positions this as something that victimizes the poor. Well I have been poor, or at least this country's version of it, and I have lived in very high crime areas where if you didn't shut your window at night chances were good you would hear a murder. And believe me when I say I was DEEPLY grateful for the increased police presence. But then, I wasn't committing crimes. Now I live in a very wealthy neighborhood (though I am not wealthy) where I have not seen a single police car drive down my street in the past four months. O'Neil argues that many crimes, like drug use by affluent college students, go unpunished because the police are busy in the poorer neighborhoods. I agree, but police resources are limited and for mercy's sake they should be sent where people are being killed, not where a college student is passed out in his living room. My current neighbors many be committing as many crimes as O'Neil implies, but I'm not terrified to walk down the street, so I don't mind the lack of police presence. I know officers have to go deal with the more life-threatening stuff, and I am grateful to them. It all depends on your perspective.

–Corporate Wellness programs. These programs have never done anything for me except shower me with gift cards and educate me on behavioral sleep therapy. I love them. But, again, perspective. I am not overweight, I love to work out, and I eat healthy. The programs were a source of income for me and my family when we needed it most. I just would have liked acknowledgement that wellness programs really do have benefits for some people, instead of a chapter painting them as some sort of 1984-style nightmare where we are all forced to be thin. It's more complicated than that.

–And the best for last: The psychology tests. Those things are pretty bad. Despite winning multiple Employee and Student of the Year awards in my life, I can't pass those tests. Not to save my life. I didn't think much of it, until I heard about another star employee how couldn't pass them either. Then I met a third star employee (and I am talking about an employee who won two JD Power Awards in two years) who couldn't pass them. Why? Picture holding a hundred quarters in your hands and then throwing them at a wall. Some will go off to one side and some to another, but most will probably cluster in the middle. Those tests keep the quarters in the middle, weeding out people who aren't typical. Sometimes that's good (deadbeats) sometimes that's bad (talented employees that think different). Here O'Neil misses an opportunity to convince owners of companies that the tests can cost them highly desirable employees. Offering real, concrete ideas of how the tests could be improved to benefit both workers and company owners would have been a harder book to write, but a much more useful one.

A lot of the ominous implications made in the book have to do with what MIGHT happen in the future, if certain systems become more common. O'Neil often uses blanket statements to imply that certain outcomes are inevitable, but that is far from true. Irritate enough people, and the systems change. Legal challenges are made and won. Some companies, eager to lure star workers, throw out some of the more punishing aspects of commonly-used systems (that happened at a company where I worked, where "life-style" scheduling that forbid clopening and gave you two days off in a row was used in conjunction with Kronos. Worked great, people loved it.). The biggest weapon against abuse is, as O'Neil repeatedly states, transparency. Having been in the industry that creates these algorithms, she is in a unique position to expose the finer details of how they work. But the book is short on the kind of details I personally crave and long on blanket statements and generalizations, the same kind of generalizations she denounces companies for making. Not all automated systems victimize the poor, not even the ones spotlighted in this book. I know because I lived them and I was poor.

I hovered on the edge of a four star rating for this book, until a chance conversation with a Japanese woman a couple days ago. Her grandmother had lost most of her possessions and land after World War II because of land redistribution. My friend was not complaining, she thought the reforms overall a good thing, though her family had lost a lot from it. "Something may benefit 99 people of of 100," she told me,"but there's always that one person…". Exactly. There's always that one person. These systems that have come to permeate our culture need to be tweaked to minimize injustice. Unlawful algorithms need to be outlawed. Bad ideas need to be replaced with good ones. And Cathy O'Neil does discuss this, especially in the conclusion, but for me the focus of the book wasn't on target. It was too slanted against systems I have seen both harm AND help. It over-simplified issues, at least for me. It's a mess out there, and solutions that work for everyone wickedly hard to come by.

Because there's always that one person.

2. **anon**
   September 6, 2016

   I am constantly surprised that people continue to choose to use Facebook daily in all its awfulness. If this is how most our society chooses to squander this wonderful thing called the internet, maybe our society deserves the consequences, whatever they may be.

   We tend to forget that people can delete their accounts. (I did.) Participation isn't mandatory.

3. **Peter Woit**
   September 6, 2016

   amz review (and others),
   Please do not spam this blog with copied content from other places. That's what links are for, and, in any case, anyone who wants to read Amazon reviews will see them if they follow the link to the Amazon book page included in the posting.

4. **srp**
   September 6, 2016

   There are often two contradictory strands of these critiques of modern operations research (which is really what all this stuff is about). One is that it doesn't work, the other that it works too well. So you end up with all this spaghetti being thrown at the wall, as in Peter's synopsis.

   Moreover, most of the drawbacks of pervasive personal data also have advantages. A machine that invades our privacy is also an alibi machine for the innocent for example. Pervasive video surveillance makes it hard to sneak around but it also makes it harder for police to abuse suspects. And so on.

   Personally, having seen the really primitive ability of current Big Data marketing tools to predict my interests and send me appropriate ads and offers, I think the biggest issue is hype. These things are much weaker in application than in breathless accounts, and when they do work it seems mostly a good thing.

5. **Bee**
   September 7, 2016

   Thanks for pointing out, sounds very interesting. Will totally put this on the reading list.

6. **Eiger**
   September 7, 2016

   @srp

   Your argument in favour of pervasive data collection seems to be the same as that used for state mass surveillance: 'it'll keep you safer, and if you're not doing anything wrong you've got nothing to worry about.'

7. **Navneeth**

September 7, 2016

Bee & Peter,
If you're interested, the theme from one of the 'MLConfs' from a few months ago was on the ethics in machine learning. You can find the videos of the talks, here: https://www.youtube.com/playlist?list=PLrbAIdPI69PiyfmA-TzKpWkmYTmuck5wS

And a summary of the conference itself: http://www.kdnuggets.com/2016/06/ethics-machine-learning-mlconf.html

8. **Bernald**
September 7, 2016

I saw the headline "Math is racist" at CNN, and correctly guessed there'd be something more reasonable at this blog.

But I have to say this seems like a classic case of out of touch elites. If you have a perfectly fair and objective algorithm (or more broadly, any human-made system), some people in positions of influence, and with their own personal biases, will deem the results unacceptable and unfair, and will act to modify things to strongly push the results in a direction they find desirable. Often this tampering is done in the name of egalitarianism, to make things superficially appear more equal by actually making them less fair. This generally does more harm than good, including to the intended beneficiaries.

While the author may make many legitimate arguments, some other arguments have things exactly backwards. Instead of the problem being that "algorithms" (more broadly, any human-made systems and decision making processes) have an unintended or intended bias against certain parts of society, allegedly causing some purported social injustice, it is rather the case that some pseudo-egalitarian elites have actively rigged and tampered with the systems so as to direct favor towards those who the elites deem to be deserving recipients of some compensatory social justice.

It's often not the algorithms/systems that are the problem, but rather it is the actions to counteract the supposed problem that are the real problem.

9. **KMS**
September 7, 2016

Actually I think there are at least two somewhat more fundamental problems concerning the social context and consequences of mathematical models, which one should think about before discussing the questions, wether the outcomes are beneficial or not.
First, one should be aware of the fact, that modelling in itself is a descriptive activity: Models try to capture the important aspects of some part of reality, necessarily leaving out "small" factors, that will not influence the outcomes (mostly average quantities) significantly. When relying on them to initiate activities, they however cease to be purely descriptive tools and acquire normative aspects.
But in the realm of normative questions there are – at least in most parts of the western world – different standards, by which rules are judged. One important aspect is, that once people are concerned it is not in general acceptable to just look at averages outcomes and _ignore_ those individuals who happen to suffer a serious disadvantage for the greater good of the majority. This does not mean that such situations do not also occur without the involvement of machines and algorithms – but in the end they are (or rather should be) handled in the context of legal decisions by courts who always have to take individual rights and general legal principles into account. This is very much in contrast to the "Utilitarian" position cited at the end of amz review's comment. So relying on algorithms for social decisions tends to promote a certain ethical position, which I consider to be at odds with some of the more fundamental principles of western societies. In any case, if one would like to base our society on such a position, one should openly discuss this using arguments from the relevant realms (ethics/philosophy/…) – and not just accept such a position, because it happens to harmonize well with the way models work.
Related to this is the point, that many algorithms at their core are nothing but statistically vamped up prejudices: Correlating e.g. zip codes to credit default risks is not fundamentally different to forming prejudice about people in certain neighbourhoods based on "everyday observations". But in constrast to plain old prejudices they now claim to be serious (even scientific) answers to practical (and often pressing) questions.
This leads me to my second point: There seems to be a serious problem in the perception of what models can and can not do, both in the general population and among many of those involved in the modelling.
Talking to people outside academia and looking at statements in the media, mathematical models seem to have an almost magical status: Mathematics is seen as very complicated, so the people building the models must be very clever and consequently their models have to stand above "normal" reasoning. Of course, models are not more and not less than a precise version of ordinary reasoning in a framework (mathematics) which allows the application of a lot of machinery to make precise reasoning easier and quantitative.
As a result, the importance of the modelling proper and the huge freedom one has when setting up a model is massively underestimated (if perceived at all). And this last point I suspect to also be true for many of the mathematicians doing the modelling.
There are systematic factors for that – one of the most important being, that at least those coming from pure mathematics usually have no experience at all in modelling. This is not just a question of maybe having learned and numerically implemented one or two well establied models, but of having made the experience that one can come up with loads of models for a given phenomenon, which are mathematically consistent and at first sight might seem plausible, but later just turn out to be wrong in important aspects.

In general I do think there are by now quite a few important questions about the consequences of increasing reliance on mathematical models and the responsabilities of those involved in building them. I therefore consider it high time that Mathematicians abandons their "we are disconnected from the world outside" attitude (as expressed e.g. in Hardy's "Apology") and start talking about their responsabilites – just like Physicists had to do 70 years ago.

10. **Peter Woit**
September 7, 2016

Bernald,
The "Math is racist" headline is a really stupid take on the book. Countering that kind of stupidity with "the problem is out of touch social engineering elites" is just as stupid. Neither has anything to do with what's actually in the book. All, the internet has quite enough of this kind of stupid reductive argument, please don't do this here.

11. **Barbara Duck**
September 7, 2016

I have followed her since day one on her blog. I was already over in healthcare writing the same type of blog posts as I could see how financial models were just being tweaked to be used in healthcare. Do pay attention as our current Medicare/CMS acting director is all into this with stats and numbers and came from United Healthcare where modeling was his expertise, as he's a former McKinsey Consultant and Goldman banker. WMD are really being pushed at HHS and CMS. It's scary indeed and I've been writing about it for about 8 years with the incest of United Healthcare that exists there.

I go a bit further and talk about "excess scoring" as a result of the WMDs and if most folks only really knew and would educate themselves on what goes on at the prescription counter, well it might be too much for folks to take when they see the reality of how you are scored with medication prediction adherence 300 metrics based on behavior, not drug compliance really. I chat with pharmacists all the time that keep me updated in that arena. Here's a link below that has a video and goes into detail, but in summary, Cathy is correct on the WMDs as they are everywhere, scoring people into oblivion. I'm glad she did this book.

http://ducknetweb.blogspot.com/2016/06/the-truth-about-pharmacy-benefit.html

12. **Jan Galkowski**
September 7, 2016

@KMS,

Regarding “First, one should be aware of the fact, that modelling in itself is a descriptive activity: Models try to capture the important aspects of some part of reality, necessarily leaving out “small” factors, that will not influence the outcomes (mostly average quantities) significantly. When relying on them to initiate activities, they however cease to be purely descriptive tools and acquire normative aspects.”

That depends upon one’s perspective. There is an entirely defensible program of statistics which acknowledges the subjective elements of any inference, and provides a framework for incorporating it. Moreover, it is possible, even if practically difficult, to bring loss estimates into inference and adjust them accordingly. These are necessarily based upon judgment. So, I would very much challenge whether modeling in any practical application can remain purely descriptive.

Among other assertions, to my thinking, WMD reemphasizes the need to bring forward our assumptions, *all* of them, and acknowledge them.

13. **MikeS**
September 7, 2016

One or two here seem to be suggesting that Big Data is fine so long as the algorithms are good.

In the hands of a benevolent state committed to the equal distribution of its bounties that may be true – although there are still contrary arguments to that idea.

However, does this not miss the point of the book? That Big Data is largely in the hands of large corporate self-interest groups have no motivation to establish the fair distribution of anything.

14. **Low Math, Meekly Interacting**
September 7, 2016

The author of this book was in the room, and only she truly knows how much she was made, wittingly or unwittingly, into a con artist. I tend to suspect hubris more than fraud on the part of the actual quants (not sure about their bosses), i.e. another cohort of geniuses brought low by the messy complexities of the real world. The entire field of economics suffers from the same delusions, as far as I’m concerned. They weren’t the first, and won’t be the last. In my field, the promises of the “omics” du jour is plagued by similar hype, and the data collected has the potential for even more harm, if misused, which it almost certainly is and will be. Perhaps it’s how I get myself to sleep at night, while acknowledging I’m just another corporate whore; but I like to think the individual players at least started off hoping to simply make a good living and provide a useful product. That, and having surveyed the abuses of academia, they thought the stench maybe wasn’t any worse on the outside.

To me the folly of the quants is just a symptom of the larger problem of trying to optimize systems before first optimizing incentives. The quants did the job they were asked to do. Probably a lot of them even believed their own dazzling bullshit. They were rewarded handsomely, nand probably broke few if any laws, the legal disincentives having been gutted long before. While people were still making billions playing the securities shell game, they were the heroes of investers, from the richest fund managers to saps like me, happy to get a cheap mortgage and a growling 401K.

The system was well primed, they just helped lubricate it. I’m far too much of a cynic now to believe adding some more ethics to the graduate maths curriculum will accomplish much without some major structural corrections to the financial sector, among many others, corrections that only sweeping govt. action can compel. It’s good to be wary of certain algorithms, but clever people will always find a new trick for the right price, and one can only be so vigilant. We must admit that nearly all of us, sadly, have our price. If we could do a better job of acknowledging that at the societal level, I think the talents of the quants could be reliably channeled to far better uses. I’d like to think the vast majority of them were fundamentally as decent as most people, and got swept up in it all.

15. **Anonyrat**
September 7, 2016

I think the new class of methods of concern have to do with machine learning, which can produce predictions without a model in the usual sense of the word. The mathematics is in the model of learning, but not in the particular model that resulted from the data that was used as the training set. I think this class of methods is the most liable to the spiral described in the review: ” if a poor student can’t get a loan because a lending model deems him too risky (by virtue of his zip code), he’s then cut off from the kind of education that could pull him out of poverty, and a vicious spiral ensues”.

16. **Low Math, Meekly Interacting**
September 7, 2016

Hi, Anonyrat,

From the perspective of a lender, whose fiduciary responsibility is to maximize return on investment for their shareholders, such a model might be performing quite optimally. The lender isn’t in the business of “pull(ing) him out of poverty”, they’re in the business of collecting interest. The fundamental problem, then, isn’t a bad algorithm, though that might exacerbate things considerably if it contributes to unsustainable trends, i.e. bubbles, it’s what we as a society deem to be fair game in the student loan sector. It could be that giving people loans for a song in affluent zip codes when there isn’t the collateral to back them up, then packaging these things in such a way as to conceal risk from investors is a really crap way of performing ones fiduciary duty, but the plight of some kid on the wrong side of the tracks isn’t technically relevant until post-collapse.

Sure, big data and the algorithms to exploit it may hurt us, but they’re designed to give us the answers we want, after all. As far as I can tell, they didn’t entirely fail the financial sector. They do appear to have been phenomenally successful, over an appropriately finite duration. Microbial populations mindlessly exploit a fundamentally similar approach in a closed system; they just lack the foresight and ability to know when to exit the log phase voluntarily, so as to better reap the rewards of their success. From a brutally pure financial standpoint, investors didn’t miscalculate what to buy so much as when to sell.

I think we have to ask ourselves, while we’re mulling how we got here, why, when things don’t go so obviously wrong, the underlying status quo is still OK.

17. **A.J.**
September 7, 2016

LMMI:

The author of this book was in the room, and only she truly knows how much she was made, wittingly or unwittingly, into a con artist.

What do you mean by this? Are you under the impression that O’Neill was involved in pricing mortgages?

18. **Peter Woit**
September 7, 2016

LMMI,
As AJ points out, ONeil wasn't involved in pricing mortgage backed securities, so had nothing to do with the specific fraudulent practice I mentioned in the posting. She has written some fairly detailed postings about her time at D.E. Shaw, for example, take a look here
https://mathbabe.org/2011/06/15/working-with-larry-summers-part-1/
and at other postings in that series that are linked to from there.

19. **Low Math, Meekly Interacting**
September 7, 2016

Yes, yes, and quite sorry, Peter and AJ. Got mixed up on sectors. But one might ponder the same questions of ethics when considering the relevant quants.

I think in general the idea here is to be appropriately nuanced, but as the "math is racist" nonsense makes amply clear, it's apparently too easy to make "big data" and "algorithms" into bogeymen, and caricature quants as some new, insulated, coldly metrics-driven rentier class, instead of people with a particular skill set getting paid to do a particular job the way they were asked to, much like the rest of us. The manifestation of the problem is fairly novel and sophisticated, but the underlying causes and motivations are rather more perennial and prosaic, in my opinion.

20. **Anonyrat**
September 7, 2016

LMMI,

I'm not asking the lender to be a social activist; but a lender might trust a human where an algorithm might not. A lender might come up with microlending, and so on.

Then there are some of the concerns expressed here, that are relevant to this post: http://sites.ieee.org/spotlight/ai-ethical-dilemma/ (IEEE, On the Use of AI – the Dependency Dilemma)

"What started out as Internet technologies that made it possible for individuals to share preferences efficiently has rapidly transformed into a growing array of algorithms that increasingly dictate those preferences."

21. **Cgh**
September 7, 2016

I don't see how her extremely limited tenure at a (systematic) trading firm and subsequent rants makes her any more than a slactivist.

Peter, sorry, the content in your link to her blog is just non-specific and childish ranting, a small step down from her book. I actually approached her book really excited to get her perspective. From a rigorous and fact-based perspective her book was very thin. I realize that she's tapping a narrative that people feel very passionate about but I disagree with your earlier use of the word "important". It could have been important. Had she leveraged her job-hopping and intellect as opposed to what is clearly an ax she has to grind she could have started a nice converasation based on factual reporting to support her hypotheses.

22. **srp**
September 7, 2016

Eiger: No, that's not what I'm saying. Pervasive data can protect you AGAINST the state at times, as well as against false accusations. Not having the video can be as damaging to justice as having lots of video around.

In general, if a mathematical model is functioning in place of a human being (or being used by a human to supersede his own judgment) in making decisions that affect people, then as noted by others above the math aspect doesn't really change any of the value-oriented policy or ethical issues. Police officers have traditionally been trained to look for what seems "out of place" in a situation and investigate that, a heuristic which has much merit but also leads to excessive stopping of motorists traveling outside "their" perceived communities. A drone programmed or trained to do the same thing would be no better and no worse in terms of fairness and public safety than the human. The same goes for loan officers, admissions officers, etc. The advantage of the mathematical model is that it is in principle easier to audit it (unless it is a neural net) and to get it to operate consistently in the way its designer intended. But if the designer has the wrong incentives, and if in practice the auditing aspect does not occur because reading the code of the model is difficult for those who might care, then you're back to the same old problems. I don't see how they would be worse than if the human were doing the "wrong" thing unassisted by a model but based on his incentives and perceptions.

23. **Low Math, Meekly Interacting**
September 7, 2016

Hi, Anonyrat,

Microlending strikes me as fairly activist in the current climate. If I've learned anything about corporate behemoths, it's that they reflexively seek to minimize time, overhead, and risk. This quaint notion of two people building a mutually-beneficial relationship through engagement and trust will just take too damn long. And this trust thing is pretty subjective. Too hard to measure, too hard to track and hold accountable. That's why these algorithms are invented. It's not that big lenders are evil so much as impatient and risk-averse. What's the most reliable way to close a good deal fast and get the money? That they actually wind up increasing risk and losses in the process is a considerable irony, but data and algorithms only serve as an amplifier of a flawed ethos. We lost the benefits of more deliberative practices because we valued them less than rapid returns.

24. **Mat Noir**
September 8, 2016

The problem with social-algorithm based decision-making is that despite their short-comings (assumptions that are incomplete, false, agenda-driven, …) they are held in awe and almost beyond reproach as they are inaccessible to 99.9 percent of the population. Furthermore, such models are impersonal, veil profit motive or criminal intent, and are almost never challenged. A loan is denied, and all a loan officer has to mouth is, "it is policy, I would love to help, but my hands are tied." And the kicker is that those who have the will and means (read governments, corporations, the Beverly Hills Zipcode) to disassemble the algorithm du jour can handily subvert it for fun and profit. It is man against the machine – and the machines are winning!

25. **Q. Austin**
September 8, 2016

Paul Wilmott, has been warning of abuse of math in finance for a long time. Despite himself being a quantitative financial analyst and having written several books in this area.

http://rsta.royalsocietypublishing.org/content/358/1765/63

Big Data is not just about obscure algorithms and machine learning. Visualization is an important part of it. And, newer methods of visualizations, which harness certain insights from data, have helped many in decision making. Unfortunately, that is not an aspect that is usually attributed to Big Data.

Big Data and Data Science, the way it is practised today, is mostly statistics + computer science. Mathematicians and physicists need to develop better understanding of these two fields to contribute more in this space.

26. **Roderic C Deyo**
September 8, 2016

It's even worse than people think. Having worked for many years on "big data" and machine learning at a very large software company, what became apparent was that careful and statistically reliable data analysis was eventually thought to be too stodgy since it often didn't immediately lead to revenue gains. Instead, conclusions were hastily drawn and tried out at massive scale in the hope of randomly producing a winner. The resulting "lottery" winners moved up and the losers moved on.

27. **Mozibur Ullah**
September 10, 2016

I've followed her column occasionally, and its good to see someone take this seriously whose background is in modelling. How does one audit complex mathematical models in high finance where secrecy is important? As well as traders motivations, incentives and so on.

28. **RandomPaddy**
September 16, 2016

Sociologically, the algorithms are a replacement for religious precepts.
They allow the governing class to impose its values on the rest of society, and regulate their actions, behaviour, and social class. You don't need religion to impose a belief system.

Various and Sundry

September 8, 2016
Categories: Uncategorized

A few unrelated items:

- Maybe Multiverse Mania in the popular press is fading. The Atlantic has The Multiverse Idea is Rotting Culture, where the author points out

> In 2014, the New Scientist published an article called "Multiverse Me," revealing that various lonely boffins take succor from the fact that alternate versions of themselves are leading fun lives full of emotional and sexual fulfillment, instead of solitudinous slogs through the stupid infinity of high-level algebra.
>
> They're not jealous; they want the best for their alternate selves, they want them to be happy. How can you help? The answers given are all cop-outs; the scientists have decided to keep on living as if the multiverse didn't exist ("The multiverse," one says, "tells us that we should behave as if we were valuing the risks according to probabilities in a classical universe"), because if it does exist the implications are horrifying. Right now, infinite versions of yourself are dying in really horrible ways, not in spite of the fact that you're lazily giving answers to a New Scientist reporter, but because of it. Every second you live, their suffering increases. If you stand on a cliff-edge and decide not to die, how many billions are smashed on the rocks? Jump now, and save them all.

- Over at Scientific American, Lee Billings has a story about dark matter and the lack of evidence for WIMPS.
- At CASW Showcase, an interesting interview with Natalie Wolchover.
- Carlo Rovelli has posted on the arXiv The dangers of non-empirical confirmation, his contribution to the *Why Trust a Theory?* meeting discussed here and here.
- Next week there will be a Natifest at the IAS, celebrating Nati Seiberg's 60th birthday. I'll try and get down there for the first day, leaving on a trip (more about that to come) later in the week.

  There's a wonderful discussion with Seiberg arranged by Hirosi Ooguri that is well worth reading.

- Denny Hill wrote recently to tell me about an interesting article on the history of the study of gravitational wave solutions, by him and Pawel Nurowski, now on the arXiv here.

Update: One more. Congratulations to Nigel Hitchin, whose 70th birthday celebrations are now ongoing. See here, here and here.

Comments

1. **Richard**
   September 9, 2016

   Is the content of the gravitational waves paper discussed in Janna Levin's "Black Hole Blues" book?

2. **Prof CJ**
   September 9, 2016

   It's Denny Hill and Pawel Nurowski

3. **Robert Levine**
   September 9, 2016

   Isn't Kriss (following the New Scientist piece he refers to) conflating Everett's many-worlds interpretation of QM with the landscape-type multiverse more recently promoted as a rationale for the vast space of vacuum states compatible with string theory? The two concepts *seem* to have very little to do with each other…

4. **Peter Woit**
   September 9, 2016

   Richard,
   Not really (from what I remember of the Levin book). There's a well-known story about Einstein's early mistake about gravitational waves that is in both places, but otherwise the Hill/Nurowski paper mostly discusses mathematical history not covered by Levin.

   Prof CJ,
   Thanks, fixed.

   Robert Levine,
   Yes, they're two completely different things, but physicists promoting multiverse mania often conflate the two. The article is not about the physics but about the mania.

5. **bayes_or_bust**
   September 9, 2016

   Whilst I don't quite agree with everything, Rovelli's rebuttal is excellent.

   Dawid's ideas weren't ever very interesting. They gained traction because they supported string theorists at a time when many are questioning their modes of reasoning.

   I'd be curious to know historical examples of scientists acclaiming particular new ideas in philosophy at moments of (real or perceived) crisis or change.

6. **Douglas Natelson**
   September 9, 2016

   Peter, Robert, how do we know they're different? Many worlds has to do with entanglement, and from enthusiastic theorists we learn that EPR = ER, and Einstein Rosen wormholes could connect different spacetimes or causally disconnected regions of our infinite universe…. (I kid, I kid. But you know Kaku will have a show on TV about this within a month.)

7. **Prof CJ**
   September 9, 2016

One more time, it's Pawel Nurowski. True,
Americans are typically monolingual so they
have little insight into how other languages
function but this cannot be that difficult

8. **Peter Woit**
September 9, 2016

Prof CJ,
Fixed. I think the problem here is not the number of languages I speak (two pretty well, a couple more badly) but distraction due to busyness of first week of classes. And also sleep deprivation due to time spent downtown watching Kieslowski's(sp?) Dekalog (4 hours down, six to go…)

9. **Jeffrey M**
September 9, 2016

Peter,

Dekalog is worth it. Also, if you haven't already, go see Red, White, and Blue.

10. **John**
September 10, 2016

"In 2014, the New Scientist published an article called "Multiverse Me," revealing that various lonely boffins take succor from the fact that alternate versions of themselves are leading fun lives full of emotional and sexual fulfillment, instead of solitudinous slogs through the stupid infinity of high-level algebra."

What is this repugnant rubbish? Multiverse theories are discredited because they are totally unsubstantiated by experiment, not because they may reflect fetishes and fantasies of individual researchers, or because they contradict peoples' moral sensibilities. Sorry to be insulting to the blog author – I've had one drink too many, but you *are* mocking the piece, right?

The original article makes some fair points at certain places, but dresses them in an unacceptably mocking rhetoric that, on top of everything, makes no sense. It invokes everyday experience to discredit ideas in a field where everyday experience goes out the window anyway.

Again: multiverse theories are not rejected because they contradict our sensibilities, but because they are unsubstantiated by our data. At this point I wonder if it is better for certain people to believe that unicorns are possible rather than reject the existence of horses, because for some people this seems to be a dichotomy…

11. **Tim May**
September 10, 2016

I think you are correct: the multiverse/multiple worlds fascination seems to be a somewhat sterile dead end.

(Understand, I read Larry Niven's SF about multiple worlds with fascination around 1972 or so. And then Bryce DeWitt came out with his articles and his book on the same, circa around 1973 or so. I might be off by a year or two. Heady stuff. But even as I was taking a class in General Relativity from Jim Hartle himself, it never seemed appropriate to ask things like "But what if we're just one of 10 to the 1427 power realities?"

In my opinion, Peter's "Not Even Wrong" moniker deserves something even stronger for this whole idea of many universes. However, as he notes, it seems to be running out of steam. (This doesn't make it wrong, just not of much interest.)

I often think in terms of many worlds to clarify my thinking in QM, but I don't assume these other possible worlds have any tangible existence, application, etc.

— Tim May

12. **Yatima**
September 10, 2016

Completely off-topic, but Tim May writes: "I read Larry Niven's SF about multiple worlds with fascination around 1972 or so."

May I enquire which SF that was? Larry Niven was always talking about Big Engineering and Exploration with Magical Physics thrown in as needed in order to play around. We had: Transport Booths, Hyperdrive Shunt engines coming in quantized speed increments (with the Mark II extremely large and extremely expensive), General Products Indestructible Ship Hulls, Bussard Ramjets working with finely managed electromagnetic fields OUTSIDE the ship (how?), Scrith Ringworld Construction Material with supernatural tensile strength and able to stop neutrinos, Stasis Fields inside which time stops, Non-Newtonian drives, Sinclair wire that is practically indestructible and can cut anything, Autodocs, Longevity Drugs and more stuff of Pulp Magazine Wonder, but we didn't have multiple worlds. Indeed, Quantum Mechanics wasn't even mentioned once as far as I can remember.

13. **Dom**
September 10, 2016

Regarding Kieslowski, "Red" and "Double life of Veronique" both starring Irene Jacob are two of the best films I've ever seen.

14. **Peter Woit**
September 10, 2016

Please, enough about Larry Niven.

John,
Just about none of the popular discussion of the multiverse has any scientific content whatsoever. What I found interesting was just that this latest example was countering the usual "Gee whiz, isn't this cool?" with "This is awful!". I think it may be some indication of the (to me) long-awaited moment where people get sick of hearing the same nonsense about this.

15. **paddy**
September 10, 2016

Peter: Damn! And I was just typing up my critique of L. Niven. Actually just commenting to thank you for the Rovelli link.

16. **Prof CJ**

September 10, 2016

Kieślowski's Dekalog is indeed powerful. I was going to recommend "Red" (one theme is Jungian synchronicity) and "Double Life of Veronique" but Dom beat me to it. "Blind Chance" is not bad, and at least conceptually is related to physics.

I hope I wasn't too harsh in posting my correction.

17. **Low Math, Meekly Interacting**
September 11, 2016

I conjecture such new-found disdain for the anthropic principle in the pop-sci press is connected to the failure to find experimental evidence for weak-scale SUSY. We've been hearing for literally decades that SUSY is part of a grander framework that implies a multiverse, and part of what makes it so grand is SUSY's many testable virtues. With the latest crop of null results, it can no longer be avoided: the amount of baroqueness needed to keep weak-scale SUSY alive is past the "psychological threshold" of even many ardent supporters. Bottles of fine cognac have publicly changed hands. Since long-cherished ideas now face the likely prospect of failure, reliance on the speculative judgment of their proponents also seems questionable.

I'd rather they pointed out anthropism's obvious problems on their own much earlier, instead of finally dropping an argument from authority only when it's become impossible to do otherwise and remain credible. But I suppose it's a step in the right direction.

18. **Douglas Natelson**
September 11, 2016

Yatima: short story, "All the Myriad Ways". (Sorry Peter.)
Regarding the very nice SciAm piece about dark matter: Am I correct that the amount of microlensing that is seen/not seen places a strong limit on primordial blackholes as a possible dark matter explanation?

19. **IMHO**
September 12, 2016

Regarding MACHOS: it's my understanding (and please correct me if I'm wrong) that the yet to be ruled out mass ranges overlap with the masses seen by LIGO. I think primordial black holes as dark matter is still breathing, if only barely.

20. **Mehmet Jon Jonnerson**
September 12, 2016

seems to me Rovelli's Bayesian argument against string theory is

PosteriorP(string theory | not SUSY pheno at LHC scale) = (1/P(not SUSY pheno at LHC scale))*P(not SUSY pheno at LHC scale | string theory)*PriorP(String Theory)

am I right?

But this seems to require knowledge of P(not SUSY pheno at LHC scale | string theory), which, I don't think, is at all well known. Like, sure, you can find a guy (e.g. Kane) who will tell you he has *a* model in which the conjunction of clauses (string theory) && (SUSY pheno at LHC scale) is true. But nobody has done the *model counting*.

So, to even know how to update P(string theory), you have to do SMT on a very complicated Domain Specific Language in which the state explosion problem is very real. Like, 10^500 real.

So, the problem is, Rovelli's making an argument which,* given more information about the string theory landscape/multiverse than is known to any string theorist*, may very well discredit string theory. This is, ironically, an argument to *do more string theory*.

Or, maybe, when Rovelli complaint about string theory's flexibility, he means there's a Bayesian parameter estimation problem, where we would like to infer a probability distribution on the parameter space of string theories from our probability distribution of observables. The flexibility problem is that the observability grammian, so to speak, has tiny eigenvalues, at least for the observables available to us thus far- or, truth be told, we don't even actually know that observability grammian. So, again, we must *do more string theory*.

For the record, I think Rovelli's a smart guy who has better things to do than post polemics to the arxiv. Here's hoping someone sees something at that 7/6 r_{s} region him and Hal were talking about.

By the way, regardless whether or not the multiverse is a bad idea, the fools in the multiverse me article aren't fools because they believe in the multiverse, they're fools because they don't understand what independent and identically distributed random variables are.

21. **Anonyrat**
September 12, 2016

@Mehmet: Rovelli wrote: "From a Popperian point of view, these failures do not falsify the theory, because the theory is so flexible that it can be adjusted to escape failed predictions. But from a Bayesian point of view, each of these failures decreases the credibility in the theory, **because a positive result would have increased it.**" {emphasis added}.

We do not require further knowledge of string theory for Rovelli's argument to work.

22. **Peter Woit**
September 12, 2016

Mehmet Jon Jonnerson and anyone who wants to argue with him,
Enough, I have serious trouble making sense of lots of arguments of this kind, and here I'm failing completely, so, while leaving this here in case others can, I'll delete any more of it, on the simple grounds that I can't moderate a discussion I can't make sense of.

23. **Anonyrat**
September 12, 2016

Peter,
Though the apriori probability of string theory being true and the conditional probability of seeing SUSY at LHC given string theory are both unknown, if we accept the proposition that seeing SUSY at LHC boosts the a posteriori probability of string theory, that gives us a simple inequality,

Bayesian a posteriori > a priori;

from which it is two minutes of symbol manipulation to prove that not seeing SUSY at LHC decreases the a posteriori probability of string theory.

24. **Peter Woit**
September 12, 2016

Anonyrat,
Yes, but I don't see how dressing this up in Bayesian probability and manipulating symbols does anything but encourage obfuscation of the obvious: if somebody tells you (or publishes a piece in the Wall Street Journal saying) "experiment X seeing Y will be evidence for theory Z" and then experiment X sees not Y, this is negative news for theory Z.

Exactly how bad the news is for theory Z of course depends on the details of the original argument. If, in retrospect, it was a very weak argument since theory Z can give you pretty much anything, including Y or not Y, then attention should shift from theory Z to why in the world people are discrediting science by making very weak arguments for theory Z in the Wall Street Journal.

Fashion, Faith, and Fantasy

September 12, 2016
Categories: Book Reviews

Roger Penrose's Fashion, Faith and Fantasy in the New Physics of the Universe is finally being published this week. This is a bit of a landmark event, since the book has a long history, going back to a series of lectures that he delivered in 2003 at the university and IAS. At one point I remember watching videos of these lectures hosted by Princeton here , but these no longer seem to be available.

In giving these lectures, Penrose was walking into the lion's den, bringing a forceful critique of string theory to the academic institution where it is most popular. Congratulations to Princeton University Press for publishing this despite it challenging the hegemonic viewpoint at Princeton (I had less luck with them: when my British publisher sent them my book for consideration way back when, they hired Lubos Motl to write an evaluation of it…).

Besides a mathematical appendix, the book is divided up into four parts:

- **Fashion**: This is the section that deals with string theory, and Penrose's central objection is to the use of extra spatial dimensions as a crucial part of the theory. When trying to use string theory as a unified theory, an assumption is made that one can take four space-time dimensions very large, and the rest very small, decoupling the large and small dimensions. Penrose argues that there is no reason to believe one can consistently do this, that there should be couplings between these degrees of freedom that cannot be ignored, leading to instability of the theory, rather than a stable ground state with large dimensions.

  The problem is that one doesn't actually have a non-perturbative string theory in which one could properly study this issue. There's no consistent theory, so Penrose can't rigorously prove there's a problem of this kind. He faces the generic problem of arguing not with a well-defined theory, but with people's speculative hopes of what kind of theory might exist. I agree with him though that the extra dimensions are a deadly problem for the theory. Even if you accept the most optimistic hopes that Penrose's and other problems will go away, you are still going to be left with the landscape problem. Everything known about conjecturally stable states with 4 large dimensions indicates an infinite complexity of such conjectural things, capable of giving you any physics you want, leaving the theory able to predict nothing and empty of explanatory power.

- **Faith:** In this section Penrose addresses the measurement problem of quantum mechanics, pointing out correctly that our standard story about quantum mechanics introduces an "ontological shift", indicating that something more is going on than a well-understood consistent framework. He favors the idea that perhaps the introduction of gravity into the usual framework could resolve this problem, backing this up with a dimensional analysis argument that a relevant effect could come from gravity, while being too small to be observable so far.

  Here I think he does an excellent job of explaining the usual story and why there's a problem, but personally I've never been convinced that this problem requires new physical laws, non-linearities, or the introduction of gravitational effects. To me the "ontological shift" has always seemed due to the standard story being not a full theory of what happens in a real measurement process, but being a phenomenological approximation of what happens, with approximation needed to get a tractable description. As people build and study more complicated and larger fully quantum systems, the inadequacies of the standard story about "measurement" I think will become clearer, and we'll get a better understanding of how classical behavior emerges from quantum laws, with no need to change those laws.

- **Fantasy:** Here Penrose describes in detail some basic problems in the theory of cosmology, and how they are supposedly resolved by the theory of inflation. He explains that characterizing this as "fantasy" is not meant to be purely critical, that "fantasizing" about the moment of the big bang is what theorists do in the absence of compelling evidence, and that he just has other fantasies he thinks worthwhile.

  I don't think I can do justice here to the depth and complexities of his arguments in this section. This is a topic involving subtle questions about the behavior of general relativity where Penrose is one of our deepest thinkers and greatest experts. While acknowledging some of the achievements of inflationary theory, part of his critique is related to that of Paul Steinhardt and others, showing that the theory doesn't accomplish what it sets out to do, with the exponential expansion not providing a way to get observed homogeneity from arbitrary initial conditions. At the same time there is a lot more there, and this section seems to me that it should be required reading for anyone trying to make sense of fantasies of the description of the big bang itself.

- **A new physics for the universe?:** In a final section, Penrose describes some of his more positive ideas addressing the problems pointed out in the earlier sections. This begins with a wonderful summary of the theory of twistors, and I strongly suspect that he's right that this very different way of thinking about space-time geometry will ultimately be part of any successful integration of our understanding of quantization and geometry. That this geometry is very specific to four space-time dimensions provides yet another reason for skepticism about the fashion of theories with more spatial dimensions.

  I'm less convinced by his speculation about quantum state reduction, and by what he refers to as his "Conformal Crazy Cosmology", although the emphasis on conformal invariance may very well be a correct one.

  In a final "Personal Coda" he explains that he sees himself not as a "maverick", but as rather embodying an "inner conservatism", somewhat allergic to the appeals of fashion. In particular:

  > So when I heard that string theory – to which I had been distinctly attracted, partly because of its early use of Riemann surfaces – had moved itself in the direction of requiring all those extra spatial dimensions, I was horrified, and far from being tempted by the romantic attractions of a higher-dimensional universe. I found it impossible to believe that nature would have rejected all those beautiful connections with Lorentzian 4-space – and I still do.

A wonderful aspect of the book are Penrose's many and detailed graphical illustrations, which have been made available separately by Princeton here. At their website you can also read the Preface, and an interview. Unfortunately I'll be in Germany next week, missing Penrose's book tour events here in New York, at the American Museum of Natural History and MoMath.

The range of non-crackpot speculative ideas about fundamental physics that normally get much attention is unfortunately quite narrow. In this environment Penrose is a breath of fresh air, providing here a different point of view on several topics, backed by serious and detailed argument. In some ways this is a popular book, but in others it is something else, deserving the attention of experts in the subject. I can't recommend it too highly to anyone with a serious interest in fundamental questions about physics.

Update: A somewhat different version of this review is up at MAA Reviews.

Comments

1. **Will F.**
   September 12, 2016

   Peter,

Would you recommend the book to a somewhat well-read layman?

2. **Peter Woit**
September 12, 2016

Will F.,
Yes, especially one not afraid of a little mathematics. This is not another "Road to Reality", which was more technical. There will be sections though where I think a layman will likely get lost (an example is the discussion of twistors), but only few of those.

3. **DrDave**
September 12, 2016

" they hired Lubos Motl to write an evaluation of it…"
I know I shouldn't be curious, but was the evaluation ever published?

4. **Peter Woit**
September 12, 2016

The full story is that parts of Lubos's review first appeared as a review on Amazon, see
https://www.amazon.com/gp/customer-reviews/R3PGUGKCPHDI39/
and I wrote a little about this here
http://www.math.columbia.edu/~woit/wordpress/?p=401

At the time I was perplexed by what version of the manuscript he was writing about. He later explained about the PUP story, see
http://www.math.columbia.edu/~woit/wordpress/?p=438
and
http://motls.blogspot.com/2006/08/seventeen-pages-of-problems-with-book.html

The original link to his full review no longer works, but you can find this via the wayback machine, for instance at

https://web.archive.org/web/20060827005045/http://schwinger.harvard.edu/~motl/new-report-edited.doc

Enough though about this history, further comments should be about Penrose's book, a much more interesting topic than Lubos's reaction to mine over ten years ago.

5. **Richard Gaylord**
September 12, 2016

Penrose's entire 10 video lecture series is available on YouTube. start with https://www.youtube.com/watch?v=5jXj1TwiFys and you can locate all the lectures from there.

6. **Al**
September 13, 2016

@Richard that youtube link goes to his lecture at Penn State given many years after his Princeton lectures (even though it has the same title).

7. **bayes_or_bust**
September 13, 2016

Anything about consciousness/QM? That was one of his pet topics.

I'm looking forward to this book. I met Penrose a few years ago; he was extremely pleasant, and of course, extremely insightful about physics.

8. **Peter Woit**
September 13, 2016

bayes_or_bust,
As far as I can tell, there's exactly one and only one sentence about consciousness/QM in the book, basically noting that he has written about this and giving a reference.

9. **wimp**
September 13, 2016

Peter

in the book,
if Penrose thinks that the extra dimensions can cause instability
does Penrose discuss the KKLT construction for stabilizing the extra dimensions? does it in fact work, in creating 3 large spatial dimensions and 6 curled up and stable?

does Penrose discuss the latest results from LHC on SUSY i.e no-show?

10. **Peter Woit**
September 14, 2016

wimp,
One reason I find the book refreshing is there's not much about such tired topics. He's got a couple pages on SUSY and the fact that it hasn't appeared, as well as the danger that proponents will just argue that higher energies are needed. He discusses the moduli stabilization problem, but in the context of explaining that the problems he sees with extra dimensions are much wider than that, with the freedom to vary parameters characterizing Calabi-Yaus the least of the problems introduced by the huge number of new degrees of freedom coming with extra dimensions.

11. **Ken**
September 14, 2016

Anyone else try buying directly from Princeton University Press and run into problems with the shopping cart? I am using the most up to date version of Chrome. If it is not an intermittent thing then I will have to go to Amazon I suppose.

12. **adamt**

September 14, 2016

Peter, thank you for the wonderful review. Based upon it I've picked up the book and reading through Chapter 1 now. One minor nitpick is that Penrose takes issue not with the huge number of degrees of freedom involving the extra dimensions, but rather the *much* larger number of functional freedom that is involved. He emphasizes this in the last paragraph of Appendix 2:

"The phrase 'degrees of freedom' is frequently used in the context of physical situations and, indeed, I frequently make use of that terminology in this book. It should be emphasized, however, that this is not the same as 'functional freedom.'"

… where he goes on to explain the difference in that "degrees of freedom" are associated with the components of the field in question whereas the "functional freedom" is far greater and completely swamps the "degrees of freedom" when extra dimensions are involved.

13. **Peter Woit**
September 14, 2016

adamt,

To my mind, the "number of degrees of freedom" here is infinite-dimensional, since the configuration spaces are function spaces, and I didn't mean anything more precise than that by my usage. I didn't want to get into explaining exactly how Penrose quantifies "functional freedom" since a short attempt at that would likely just be confusing.

For exactly what this means, if you have a copy of "The Road to Reality", see section 16.7. The argument Penrose is making against extra dimensions is not a new one for him. He developed it in detail quite a while, see for instance his paper "On the instability of extra space dimensions" in the Hawking 60th birthday celebration volume, published in 2003.

14. **adamt**
September 14, 2016

Peter,

I see, thanks. The degrees of freedom available to a theory with extra dimensions is "utterly unassailably" vastly larger because of the functional freedom as Penrose says. In his book, Penrose states that he does not think anyone has really responded in good faith and detail to his "functional freedom" objections to string theory and now I see this objection really has been out there for some time. While I'm reading the book it would be nice to see some counter-argument. Do you know of any attempt to rebut or explain away his objection in good faith and earnest?

15. **Peter Woit**
September 14, 2016

adamt,
I'm not aware of any, and I recently talked to Penrose about this, and he said he hadn't gotten any counter-argument.

My guess is that string theorists approach this question like a lot of possible arguments against what they are doing, figure it is best to just push forward, assume that things decouple in some way to avoid the problem, and see what you can do. To my mind the deadly argument against string theory is not any argument like Penrose's, but that, even if you optimistically ignore all such problems, you end up with an empty theory that can't predict anything.

Put differently, if string theorists ignored Penrose's problem, then went on to find a calculational framework that was vindicated by success in explaining the real world, that would be fine. His problem would be an interesting issue to think about in terms of better understanding string theory, but it would not be a compelling argument against string theory. On the other hand, if you have a good argument that something is not going to work, and it doesn't, you should take that argument quite seriously, it might show you where you're going wrong. Some string theorists I've talked to actually do say that they're not interested in the higher dimensions, believe that string theory needs to find a proper 4d formulation.

16. **Thomas**
September 14, 2016

I don't understand this supposed argument against extra dimensions. If there was an argument that implies that the 3+1 d standard model cannot be an EFT for a higher dimensional theory, how does the same argument not rule out 2+1 Chern Simons theory as an EFT for 3+1 electrons in a quantum hall state? Or, how does it not imply that 2-d domain walls in 3d broken symmetry states must be unstable?

17. **wimp**
September 14, 2016

thanks for replying

i posted this question and a string theorist replied. In his view Penrose objections were satisfactory resolved in 2002 with the KKLT paper, and is no longer an issue for string theory. the string theorist then updated the string FAQ

18. **Peter Woit**
September 15, 2016

wimp,
Penrose explicitly explains in the book that the problem of fixing the finite-dimensional moduli that KKLT is supposed to solve is not the problem he is discussing.

Thomas,
Have you tried reading his argument? I don't see how your objection has anything to do with it.

19. **Urs Schreiber**
September 15, 2016

It's a little unfortunate for the scientific discourse that the argument is hidden behind a paywall. Looking around, I find this old video recording of a talk by Roger Penrose, which from 3:55 on explains what "functional freedom" is meant to refer to. (Go to 6:56 to see the key slide.)

The presentation there is aimed at non-experts, but what it says in technical paraphrase is that if a physical field takes values in a manifold V, then a field configuration is a section of a V-bundle over spacetime, hence locally a function from spacetime to V. The point of the slide is to highlight that the space of functions to V (field configurations) is an infinite-dimensional manifold (and in fact that's true only if the local domain is compact, otherwise things become more wild still). So if the pointwise degrees of freedom of a field are parameterized by a finite dimensional manifold V, then the actual spacetime-wide degrees of freedom are parameterized by an infinite-dimensional mapping space.

What I wonder is in which way this basic fact is meant to be something not appreciated in traditional literature. What is traditionally meant by moduli stabilization is precisely the argument that on-shell the dependency of those V-valued functions along the compact dimensions disappears due to them picking up a potential, in direct analogy to how the Higgs field has a constant (absolute) value throughout spacetime (at least locally) since it sits at the bottom of a potential well.

Possibly there is room to argue that the existing arguments for moduli stabilization in string theory (Acharya 02, KKLT 03, Buchbinder-Ovrut 03) are lacking in rigor, but I don't see how one would argue that they are based on an elementary misunderstanding of what the problem is.

20. **Peter Woit**
September 15, 2016

Urs,
For the N'th time, Penrose is not talking about the moduli stabilization problem. Why do you insist he is? If you want to know what his argument is, find a copy of the book, or lookup up his article "On the instability of extra space dimensions" in the Hawking 60th birthday volume. If you can't be bothered to read his argument, why post things on the internet saying it is wrong?

21. **Jeffrey M**
September 15, 2016

FYI, Penrose's paper is (mostly) available via Google, a few pages missing. More than enough to see what he is talking about. Which is NOT moduli stabilization. Very interesting stuff, I have to go back now and read the original Cartan paper. Penrose also has interesting things to say about ADS/CFT.

22. **Urs Schreiber**
September 15, 2016

In "On the instability of extra space dimensions" section 10.2 focuses on vacuum solutions of Einstein gravity:

> Let us consider the case where our field equations are just Ricci-flatness for M, as is the case in string theory, where one takes just the first-order term in the string constant a'.

This is the assumption that is shown to be false by the stabilization mechanisms from 2002/03 by Acharya, KKLT, and others cited above: they work with two extra ingredients present in string theory but not in KK-gravity: effective potentials introduced by the presence of higher form fields (fluxes) and non-perturbative effects.

I suppose it would be worthwhile for mathematical physicists to have a closer inspection of these string theoretic arguments, but what is discussed in "On the instability of extra space dimensions" is the problem without string theoretic effects.

Now, of course "On the instability of extra space dimensions" was written for a meeting in 2002, hence just a moment before string theorists presented their solution to the stabilization problem.

23. **Peter Woit**
September 15, 2016

Urs,
Yes, there's nothing in there about KKLT, which is irrelevant to his argument. If you now have access to his paper, why don't you read it and try and understand his argument, instead of insisting that it is something it isn't, and which he explicitly says it is not (in the latest book)?

24. **Shantanu**
September 15, 2016

Peter, something OT. videos of talks at Cosmo-16 including on particle physics subjects
http://cosmo16.physics.lsa.umich.edu/talks.html

25. **adamt**
September 15, 2016

Urs,

To be clear, Penrose also declares that the responses he's seen to his talk on Hawking's 60th birthday have been, "dismissed with what would appear to be a somewhat hand-waving quantum-mechanical argument of a general nature that I regard as basically fallacious." He goes on to restate the argument, "in a more forceful form" in section 1.10 of his new book which I am just getting to read now.

The general tone I see is one of frustration that string theorists have not addressed the overall geometrical and physical considerations he sees as obvious. Coming from Penrose with his manifest expertise in geometry and general relativity I'm surprised that his concerns haven't been taken more seriously. Here is some flavor:

"It is my impression that many of the most obvious geometrical and physical issues arising from the perspective of string theory are never really properly discussed at all!"

and

"I find this curious lack of a coherent geometrical picture of how string theory is to be viewed in ordinary physical terms to be very odd."

and

"The acceptance, by a highly knowledgeable section of the physics community, of such a hybrid of great geometrical sophistication on the one hand and a seeming disregard for an overall geometrical coherence on the other is something that I find extremely puzzling!"

26. **adamt**
September 15, 2016

Thomas,

FWIW, in section 1.10 of his new book he brings up your objection regarding the quantum hall effect as a 2-space quantum phenomenon taking place within ordinary 3-space. Penrose says it is a "completely false analogy" because the quantum hall effect is an example where the lower-dimensional space is a "subspace" of the higher one rather than the "factor space" in String Theory's 10 dimensional spacetime with the 6-space and regular 4-spacetime in an M x X factor space. He then goes on to say that the analogy is better suited to "very different brane-world picture" which he is going to discuss later. But please read the book as I am probably botching something.

27. **David Roberts**
September 15, 2016

@adamt

more useful than quoting passages about Penrose's puzzlement and reactions to reactions, would be to quote some actual mathematical or physical arguments (or, gasp, even a sketch proof), so everyone can see what all the fuss is about. I'm not about to pop out and buy a book (or rather wait for international delivery, or else find a bookstore to order it for me) to check what might be one page of proof in an area which is not my own, but I am interested to know what the various arguments are.

If we all had the relevant passage in front of us people wouldn't be trying to second-guess what it might contain based over lectures over a decade old, and could make progress.

28. **Thomas**
September 15, 2016

@adamt

Thanks for pointing me to this section. I only skimmed the book and missed it.

I think the question is relevant. Penrose argues (in the same chapter) that the motion of the sun will excite extra-dimensiona modes. If this was true, I would expect that a two-dimensional current excites three-dimensional electronic states, which contradicts standard EFT arguments (and experiment).

I fail to understand Penrose's claim that this is a false analogy, and the distinction he makes between subspaces and factor spaces.

29. **adamt**
September 16, 2016

David,

If I thought I could do his argument justice in just a few well-placed quotes I might try, but I have no such belief. Penrose wrote a chapter of a book about it so I think you would be best advised to read it when you get the chance if you are interested rather than have a layman try and paraphrase his argument in a blog comment.

I will note that he also devotes a considerable bit to throwing shade on AdS/CFT as well using some of the same types of arguments. Seems he respects the mathematical tools, but finds the possible connection to our actual physical universe tenuous at the very best. Mostly, I think he is just frustrated that no one bothers to respond other than Lenny Suskind basically telling him that his reservations are well founded, but please hush up about it so as not to discourage the kids working on string theory.

30. **Curious Mayhem**
September 16, 2016

It's always refreshing to hear from Penrose. I don't always agree with his criticisms or answers to questions — but he's asking the right questions and raising important issues.

My main problem with Penrose's arguments is his weird prejudice against inflation. Surely, he's right that deeper questions need to be addressed about the connection between gravity and thermodynamics, or why the inflationary universe starts in the first place in a low- or zero-entropy state. I doubt if gravity or consciousness plays a fundamental role. Rather, I think gravity, quantum mechanics, and thermodynamics have a basic relationship that we don't fully understand but that we can reasonably guess is important, on the basis of gravitational event horizons and singularities, for example.

Similarly, consciousness is a prime candidate for an essentially quantum phenomenon at the macroscopic scale. But it's the quantum that's fundamental, not consciousness. Free will is fine, too, if we're careful to define it without recourse to dualism. Aristotle comes in handy here, as he answered a similar difficulty, in a different context, in his disagreements with Plato. In fact, modern physics, with quantum mechanics, thermodynamics, and gravitation, is in a position to sensibly ask ontological and teleological questions not legitimate in a scientific framework since the death of Aristotelianism three or four centuries ago. But we're asking these questions in a completely different context, not within exploded philosophical and theological frameworks. Maybe that's what Templeton is after, in a misguided way. Nice try.

It's likely in the next decade that the evidence in favor of inflation will become decisive. So it's important to keep up the effort to stop the false association in lay people's minds (and even the minds of scientists) between inflation and the multiverse (in the string or "landscape" sense). The two are logically and historically unrelated. Technically, it's difficult to get inflation to work in string-infused QFT models. If a future BICEP or Planck nails the case for inflation, it should be viewed as a highly prejudicial against string theory.

31. **David Roberts**
September 20, 2016

@adamt

Then surely *someone* who has access to the book can supply the concrete mathematical/physical arguments in enough detail so that those familiar with the necessary technical bits and pieces can see what he's getting at? If in these discussions so far experts are misunderstanding what the issues are that Penrose claims, then insufficient detail has been given.

32. **adamt**
September 22, 2016

David Roberts,

Ok, with the risk that I am probably grossly simplyfing or misrepresenting his argument I'll try.

Penrose's biggest critique – but by no means sole critique – of String Theory's cavalier attitude towards extra spacial dimensions is presented in chapter 1.10 of his book. The argument goes that the extra spacial dimensions will necessarily entail a (vast!) functional freedom above and beyond those of our familiar four dimensional space-time. He explains this in detail, but here I just take it as acknowledged. Penrose says that String Theorists usually dismiss this and say it will never come into play because of the huge energy required to excite these 6 small dimensions and activate their degrees of freedom.

To be clear, he is not talking about *zero-modes*, but rather excitations that *do* require energy. In the types of String Theory which purport to address quantum gravity in a serious way he says the energy necessary to excite these degrees of freedom would be on the order of the Planck energy.

Here, he says String Theorists usually argue that to excite them would need a process involving individual particles accelerated to this approximate Planck energy. Penrose finds this argument utterly unconvincing for many reasons, but the chief being that the space-time presented in this picture – at least in the ground state – is a *product space* M x X. In this space, M represents our familiar classical 4-space-time and X is represents the extra spatial dimensions. Were X to be excited, then the 'excited mode' as opposed to the ground state, would be given by M x X` and this is to be thought of as representing the space-time of the *entire universe.* In this context, the Planck energy is not large at all.

He goes on to explain the nonlinear instability involved when considering this 10-space as representing a space-time where some Einstein equations are presumably taken to be controlling the evolution of both sets of degrees of freedom in the 6-space of the small dimensions and the regular 4-space.

That's the nut of the biggest critique I think, but he explains in far more detail than I can provide here. I really recommend if you are interested to go to amazon and get his book which is available on kindle right now.

33. **David Roberts**
September 23, 2016

Ok, arguments that assume a compactification is just a product always seemed slightly fishy to me. This is not fatal, though, if the argument is really local in nature, but the fibres should probably be some sort of interesting orbifolds, and the fibration may not be locally trivial etc etc.

I realise there is yet more detail, but it's not going to be forthcoming here at the present rate. I'm not going to buy a Kindle book any time soon, so perhaps I'll take a peek if and when my local shop gets it in, or some kind soul gives a thorough breakdown of the relevant material.

34. **Pete**
October 3, 2016

A buddy of mine with some friends at Princeton found the following links to these lectures. You can even download them.

https://mediacentral.princeton.edu/media/Fashion%2C+Faith+and+Fantasy+in+the+New+Physics+of+the+universe+-+Lecture+2A+FAITH/1_7n9wzmi0/13468321

https://mediacentral.princeton.edu/media/Fashion%2C+Faith+and+Fantasy+in+the+New+Physics+of+the+universe+-+Lecture+1A+FASHION/1_kgs8jsyq/13468321

https://mediacentral.princeton.edu/media/Fashion%2C+Faith+and+Fantasy+in+the+New+Physics+of+the+universe+-+Lecture+3A+FANTASY/1_xbikovdr/13468321

They are just hard to find, I guess.

This Week's Hype

September 15, 2016
Categories: This Week's Hype

Quanta Magazine has over the past couple years been establishing a well-deserved reputation as the smartest and best science journalism around. At the opposite extreme, over many years of interacting with science journalists, the most embarrassingly incompetent one I've run across has been KC Cole, so I was surprised today to see that Quanta has published a piece by her.

Back in 2006 she wrote a review for the LA Times, basically explaining that Lee Smolin and I shouldn't be listened to because we were incompetent embittered failures who didn't understand the beauties of string theory. When I contacted her and the LA Times to complain that her review had completely misrepresented what I wrote in my book about neutrino physics, she wrote back to explain to me that I didn't know what I was talking about, whereas she was an expert on neutrino physics. Her other main evidence for my ignorance was this:

> As for Woit's claim that string theory has "absolutely zero connection with experiment," experiments already planned for a new European particle accelerator will look for the existence of extra dimensions and extra families of particles — both predicted by string theory. In fact, many statements about string theory in these books are plain wrong.

The topic of her new article is The Strange Second Life of String Theory, which makes the claims that string theory has failed as a theory of quantum gravity (which will be news to a lot of string theorists), but that "it has blossomed into one of the most useful sets of tools in science."

The article has all sorts of interesting quotes from experts about the state of string theory these days, mostly indicating that people have given up on it and are trying to figure out how to move on. For instance:

> David Gross: "After a certain point in the early '90s, people gave up on trying to connect to the real world," Gross said. "The last 20 years have really been a great extension of theoretical tools, but very little progress on understanding what's actually out there."
>
> Robbert Dijkgraaf: "But now we have this big mess." "Things have gotten almost postmodern."
>
> "Nobody knows whether to say they're a string theorist anymore," said Chris Beem, a mathematical physicist at the University of Oxford. "It's become very confusing."
>
> At last year's big annual string theory meeting, the Stanford University string theorist Eva Silverstein was amused to find she was one of the few giving a talk "on string theory proper," she said.
>
> Juan Maldacena jokingly defines "string theory" as "Solid Theoretical Research in Natural Geometric Structures."
>
> Like many of his colleagues, Simmons-Duffin says he's a string theorist mostly in the sense that it's become an umbrella term for anyone doing fundamental physics in underdeveloped corners. He's currently focusing on a physical system that's described by a conformal field theory but has nothing to do with strings.

I'm amused to hear that according to Maldacena and Simmons-Duffin, it appears that I'm a string theorist. One thing Cole gets right is that most theorists are now working on questions about quantum field theories. Sean Carroll objects to this:

> It's the kind of work that makes people such as Sean Carroll, a theoretical physicist at the California Institute of Technology, wonder if the field has strayed too far from its early ambitions — to find, if not a "theory of everything," at least a theory of quantum gravity. "Answering deep questions about quantum gravity has not really happened," he said. "They have all these hammers and they go looking for nails." That's fine, he said, even acknowledging that generations might be needed to develop a new theory of quantum gravity. "But it isn't fine if you forget that, ultimately, your goal is describing the real world."
>
> It's a question he has asked his friends. Why are they investigating detailed quantum field theories? "What's the aspiration?" he asks. Their answers are logical, he says, but steps removed from developing a true description of our universe.
>
> Instead, he's looking for a way to "find gravity inside quantum mechanics." A paper he recently wrote with colleagues claims to take steps toward just that. It does not involve string theory.

Cole tells us that

> Like many a maturing beauty, string theory has gotten rich in relationships, complicated, hard to handle and widely influential. Its tentacles have reached so deeply into so many areas in theoretical physics, it's become almost unrecognizable, even to string theorists.

According to her, string theory has made "essential contributions to cosmology" (this likely is news to cosmologists), especially by revealing the multiverse, which is now "taken for granted by a large number of physicists", one of whom you might think is David Gross, since she writes:

> Inflationary models get tangled in string theory in multiple ways, not least of which is the multiverse — the idea that ours is one of a perhaps infinite number of universes, each created by the same mechanism that begat our own. Between string theory and cosmology, the idea of an infinite landscape of possible universes became not just acceptable, but even taken for granted by a large number of physicists. The selection effect, Silverstein said, would be one quite natural explanation for why our world is the way it is: In a very different universe, we wouldn't be here to tell the story.
>
> This effect could be one answer to a big problem string theory was supposed to solve. As Gross put it: "What picks out this particular theory" — the Standard Model — from the "plethora of infinite possibilities?"
>
> Silverstein thinks the selection effect is actually a good argument for string theory. The infinite landscape of possible universes can be directly linked to "the rich structure that we find in string theory," she said — the innumerable ways that string theory's multidimensional space-time can be folded in upon itself.

The piece ends with a different genre of hype:

> Arkani-Hamed believes we are in the most exciting epoch of physics since quantum mechanics appeared in the 1920s.

I actually spent much of the day down in Princeton at the IAS, attending some of the talks at the Natifest in honor of Nati Seiberg's 60th birthday. Lots of different ideas were discussed by the speakers, with essentially no mention of string theory. A serious journalist who talked to all the people Cole did would likely have noticed the obvious and framed the same material quite differently: string theory hasn't worked out and theorists have moved on to other things, with the center of gravity of the subject now the deeper study of quantum field theory.

Update: I took a look again at the KC Cole review of my book, the second page of it is here. It was even more dishonest and unethical than I remember. She takes my

> superstring theory has had absolutely zero connection with experiment,

and turns it into

> Woit's claim that string theory has "absolutely zero connection with experiment,"

Note how pulling a phrase out of sentence, you get to do fun stuff like change the tense of the sentence.

On the neutrino issue, Cole writes:

> To say, as Woit does, that fundamental mysteries about neutrinos are being ignored will come as news to the dozens of physicists who've been working on these problems for years.

This is based on the fact that on page 93 of the US edition I write, after giving a description of the things the standard model leaves unexplained, including a parameter count that ignores neutrino masses:

> One complication that has been ignored so far involves neutrinos.

and then go on to explain about the experimental evidence for neutrino masses. The "ignored so far" obviously means "ignored so far in this chapter", not "fundamental mysteries about neutrinos are being ignored" by physicists.

When I contacted her to complain about this, her response was that there was nothing wrong with what she had done, and that, unlike me, she was an expert on neutrino physics.

The Quanta article has lots of her characteristic "quotes", words or phrases pulled out of context. I'll bet that lots of those misrepresent what the person being quoted actually said. I'd urge the Quanta editors to re-fact check this piece, asking for full quotes, in context.

Comments

1. **Johannes**
   September 16, 2016

   A Natifest without string theory? Is Seiberg now the only string theorist remaining on Earth?

   More seriously: Peter, what ideas were presented? What are people at the IAS doing exactly?

2. **David Appell**
   September 16, 2016

   K.C. Cole has a BA in political science. Snarf.

   https://en.wikipedia.org/wiki/K.C._Cole#Personal_life

3. **Another Anon**
   September 16, 2016

   "String theory hasn't worked out". This is it in a nutshell. It's not like it was ever going to die overnight, we're seeing it slowly morphing and fading.

4. **cgh**
   September 16, 2016

   As someone who has been quoted in the press more than a few times, it has been my experience that quotes will be used contextually, and sometimes practically altered, to support a larger narrative or theme that you're unaware of at the time the question is asked. Just a couple of months ago I agreed to an interview, which I agreed to be attributable, on two different subjects. Granted, this stuff is complicated, and the job of reporting and conveying clearly is not easy, but I was very surprised by subsequent published piece as 1) it conflated the two separate responses as if they applied to the same topic and 2) the context of the story rendered their usage non-sensical.

   It sounds like KC Cole approaches this stuff with a bias.

5. **TS**
   September 16, 2016

   Nowadays, string theory is cited as an example of science gone wrong to explain failure in other fields. Here (PDF) you will find recent economics research being compared to string theory, and that is not meant as a compliment.

6. **Asaint**
   September 16, 2016

   Can someone explain what Nima means by:
   "Every aspect of the idea that we understood quantum field theory turns out to be wrong. It's a vastly bigger beast."

7. **Peter Woit**
   September 16, 2016

   David Appell,

   But she told me she was an expert in neutrino science?

   More seriously, a lack of formal training in science isn't necessarily a problem, some of the best science writers I've met don't have this. On the other hand, lacking any interest in accuracy and being in love with ridiculous hype are problems. Someone back in 2006 pointed me to this account of Cole's advice to other science writers, which encourages them to eschew objectivity and quote out of context

   https://web.archive.org/web/20061002142756/http://www.ncswa.org/archive/workshops/2004/3cole.html

“Really good science writers need to lie, cheat, and steal, said K. C. Cole in the first plenary of the workshop. She outlined 15 rules for writing in her talk, but focused most on the value of lying.”

8. **Peter Woit**
September 16, 2016

Johannes,
You can follow the conference via a live-streaming link here
https://www.ias.edu/events/natifest-celebrating-science-nathan-seiberg-0
and I would guess that slides from talks will be posted.

The talks are covering a lot of different topics, many with some relation to Seiberg’s work on getting non-perturbative information about supersymmetric qfts (with the Seiberg-Witten work on N=2 d=4 SUSY a high point of that subject).

My impression is that there has been a lot of interest recently at the IAS in problems of low-dimensional QFTs related to condensed matter physics. Some of this started with attempts to use AdS/CFT to say something about condensed matter, but seems to have moved on from there to more basic questions purely about QFT, often with some relation to topological quantum field theory.

9. **John**
September 16, 2016

“Really good science writers need to lie, cheat, and steal, said K. C. Cole in the first plenary of the workshop. She outlined 15 rules for writing in her talk, but focused most on the value of lying.”

This seems to apply to about 90% of all journalists in any field or subject. Problem today is journalism isn’t reporting anymore. It’s telling of a story and that requires a bias, else the story is too boring most of the time. This is how journalism is now taught in most universities.

10. **Peter Woit**
September 16, 2016

John,
I don’t know about journalists in general, but my experience with science journalists is that the great majority of them do try and get things right, in an unbiased way, with Cole an unusual exception. This is especially true of the writers at Quanta, since that publication is not advertising driven, and thus more focused on producing high quality content, not clickbait.

11. **Bernhard**
September 16, 2016

It’s been a while since I last read so many embarrassing statements about science in the the same piece. Wow.

12. **Curious Mayhem**
September 16, 2016

Who *is* working on string theory, as such, these days? Wasn’t all the progress, such as it was, using quantum field theory and funky symmetries, some new, some not?

This stuff from KC Cole (whom I never paid much attention to) is astonishingly ignorant. String theory isn’t a maturing beauty, but a bad idea that died some time ago and no one wants to talk about too loudly and in public — like those embarrassing Facebook pictures from Florida spring break you wish you hadn’t posted.

The crap about the multiuniverse is deeply ignorant. Inflation is consistent with, but does not require, a universe of multiple domains. It has nothing to do with causally disconnected or disjointed realities — no “landscape” of “realizations.” The technical implementation of inflation (the long e-folding needed) is quite difficult to pull off in string-inspired QFT models. The only cases I know of involve a new, extra symmetry. Otherwise, actually, it’s impossible. If the evidence for inflation ever becomes decisive, that would be another nail in the string theory coffin.

Cosmologists and astrophysicists who understand the QFT side of inflation know this, but they’re generally careful not to say it too loudly. It’s like tripping the archbishop during high mass. Maybe now that string theory has died an overdue, quiet, and deserved death, more people can talk about this openly.

13. **Bai Xiao**
September 16, 2016

Many years ago I attended a workshop at KITP, which ran in parallel with another one on String Theory. KC Cole had been invited by the director (David Gross) as the “in-house journalist” and was invited to lecture us about how to write science better and hype up (I presume) our field.

This was in 2004 and the field was just starting to take off and attracting more and more attention, especially from condensed matter and HEP theorists.

After reading the post and the above comments, I am under the impression that some people in the community may have taken private lessons with her. Since then, the field has become increasingly dominated by hype and nonsensical theoretical proposals.

Unlike String theory, there is still much degree of experimental input, but it is dressed in a heavy cloud of hype and wild speculation. With a few exceptions, experimental results are often reported as “ground breaking”, when they often are textbook demonstrations of well understood quantum mechanical phenomena, or simply new realizations of phenomena that had been already observed in other systems.

14. **Narad**
September 17, 2016

“Like many a maturing beauty…. Its tentacles have reached….”

That’s quite the mixed metaphor.

15. **David Metzler**
September 18, 2016

This is OT, but some might enjoy a pretty decent write-up by the LIGO people of the process they went through to validate their first detection:

https://www.ligo.caltech.edu/news/ligo20160914

Careful and important science, written up well for a general audience.

16. **Paul**
September 19, 2016

Peter Woit says:
"David Appell,
But she told me she was an expert in neutrino science?
More seriously, a lack of formal training in science isn't necessarily a problem, some of the best science writers I've met don't have this. On the other hand, lacking any interest in accuracy and being in love with ridiculous hype are problems…"

Peter, you're a Gentleman and a Scholar. One of the many reasons why I enjoy your blog.

17. **kwwrona**
September 20, 2016

There are at least two possible explanations for Cole's expertise in neutrino physics. She once lived in Shaker Heights, OH. And since Frederick Reines was chairman of the Case Tech physics department while he was working on the detection of gamma-generated neutrinos in the atmosphere, who knows? It's only a couple of miles away.

And, Shaker Heights isn't very far from the Irvine-Michigan-Brookhaven neutrino detector at Fairport Harbor on Lake Erie. It's just off I90.

So maybe something rubbed off.

18. **Peter Woit**
September 21, 2016

kwwrona,
I think she believes she's an expert in neutrino physics since she wrote an article about a neutrino experiment for the New Yorker.

19. **GoletaBeach**
September 22, 2016

A bit too snarky for me… "not even wrong" is as snarky a comment itself as anything KC Cole wrote into her review… I can't see anything about neutrino physics in that LA Times review she wrote of "Not Even Wrong". I didn't even feel was she was particularly negative to you, Peter.

Meanwhile, a certain segment of the neutrino community (double beta decay, direct neutrino mass) was up the hill from me this week… nice talks. Wonderful "Livingston" plot of the improvement in sensitivity to direct neutrino mass as a function of time, predicting that around 2020/2030 direct sensitivity will start to get to the level of delta-ms measured in oscillations.

There is a mild fissure in the experimental community, I think… DUNE is not always appreciated by the LHC guys, and maybe vice versa. A story too true and important to be delved into, I guess. But the DUNE neutrino community is distinct from the nuclear-side neutrino folks up the hill from me this week.

20. **Peter Woit**
September 22, 2016

Goleta Beach,

I think you probably weren't seeing page 2 of the review, see
http://articles.latimes.com/2006/oct/08/books/bk-cole8/2

I went back and looked at the LA Times review again, it was even more dishonest than I remember. Besides the outrageous neutrino business, the way she used another quote, deleting a word to change its meaning, was a piece of sleaze I don't think I noticed before. I suspect that most of the quotes in this piece are also seriously out of context (often she just quotes a couple words of a sentence, surrounding them with her own). The Quanta editors might want to go back and re-fact check this piece, asking Cole to provide full, in context quotes to check against the quotes she uses.

21. **Peter Woit**
September 22, 2016

I added an update to the posting explaining some of what was going on in Cole's LA Times review, for the vanishingly small number of readers who might care…

22. **Low Math, Meekly Interacting**
September 22, 2016

Just makes me more and more grateful for Ms. Wolchover.

23. **Roy**
September 22, 2016

I think you're making too much of the journalist's error. Doubtful that it's malicious.

24. **Peter Woit**
September 23, 2016

Roy,
In the case of ten years ago, there was definitely malice involved (towards Lee Smolin and myself). In the case of the new article I don't think there's malice, just incompetence.

Heidelberg Laureate Forum

September 19, 2016
Categories: Uncategorized

This week I'm in Heidelberg, attending the Heidelberg Laureate Forum, where I'll be writing some blog posts for their website (which should appear here). You can follow talks on their website, either in real-time as streaming video, or by watching a talk video later on.

This event brings together mathematicians and computer scientists. More specifically winners of the Fields Medal, Abel Prize, ACM Turing award and Nevanlinna Prize come to give talks and interact with a large group of young researchers (and various hangers-on such as myself).

Update: I wrote blog entries for the HLF about Faltings, Ngo, Wiles, Atiyah and Voevodsky. Now on my way to London, will spend the next week in England, mostly in Oxford.

No Comments

Still Not Even Wrong

September 24, 2016
Categories: Not Even Wrong: The Book

A while back Tushna Commissariat of Physics World came to talk with me at Columbia, partly to discuss the topic of “Not Even Wrong, ten years later”, and that has now been turned into a podcast available as Still Not Even Wrong.

I’ve now forgotten what I said then, but presumably I still agree with it. This coming week I’m traveling and won’t have much time to deal with the blog, so comments from me may be few and far between.

Update: There’s an appreciative blog post about this here from ZapperZ.

Comments

1. **Shantanu**
   September 26, 2016

   Peter something OT
   Jim Gates’s colloquium at CFA
   https://www.youtube.com/watch?v=n64-koDcCqs

2. **Shantanu**
   September 26, 2016

   I think his punchline in answer to Giovanni Fazio’s question is “string theory still does not exist”

3. **Shantanu**
   October 3, 2016

   Peter any rumors/guesses for this year’s nobel prize?

4. **AcademicLurker**
   October 3, 2016

   @Shantanu,

   Gravity wave detection? Maybe it’s too soon and they’ll wait a year or two.

5. **N.**
   October 3, 2016

   Jeez, 10 yrs…
   I read your book after Lee’s Trouble and I remember thinking, now well, this must be a conspiracy, strings rule, don’t they…
   I remember I read your book twice. And Lee’s as well.
   It was my serious intention to read it again one of these days, but it is unfortunately impossible.
   Moral of the story: do not lend good books to good friends. 🙁

6. **AcademicLurker**
   October 4, 2016

   Well that was a surprise. I remember studying the Kosterlitz–Thouless transition in grad school.

7. **CWJ**
   October 4, 2016

   Heck, I had Thouless as a professor in grad school. I always thought of him as being on the long list for the prize, but after this many years I assumed it wasn’t going to happen. Congratulations to the winners!

   (And my bet for next year is gravitational waves…)

8. **Thomas Larsson**
   October 4, 2016

   It was a long time since I thought about the KT transition. IIRC, there is a continuum of inequivalent critical points depending on the coupling constant, which is possible because the relevant CFT does not belong to the unitary discrete series but to a continuum of unitary models with c=1.

   The Wikipedia article already mentions the 2016 Nobel prize.

9. **Rob**
   October 6, 2016

   I think it was wise not to give the prize for gravitational radiation this year. I’m nervous about the reliability of the result. I don’t doubt that the experiment has done a great job. However, experimental science rests on reproducibility. Ideally this would come from other experiments. However, this is some way off. Therefore, at the least I’d like to see Advanced Ligo collect a more substantial sample of gravity wave “events” than it presently has.

10. **pie inskee**
    October 9, 2016

    Good interview…your voice/manner is very much as suggested by your blog/mission.

May I ask a question. I 'feel' like things are going your way. This last year or so seems to have seen significant rise in hard criticism of Strings. The question is whether you concur with this perception.

11. **Peter Woit**
October 9, 2016

pie inskee,

The lack of any evidence for supersymmetry over the past year or so, given that the LHC is now running at nearly full energy, I think removes the last hope for some sort of experimental vindication of the ideas behind string theory as a unified theory. This may have an effect, I'm not sure that I've seen much of a change recently. I do think though there has been a big change over the last ten years, as string theorists themselves have given up on the idea of getting predictions out of the theory. The embrace of the multiverse as an excuse is a move that doesn't convince most people.

Back

October 4, 2016
Categories: Uncategorized

Now back from traveling, regular blogging will resume. Here are a few items:

- I was going to write something yesterday, explaining that this year's physics Nobel would surely go to the LIGO trio who have gotten every other major physics prize this year. Luckily I was too lazy to do that yesterday, since this morning's news is that it instead went to Haldane, Kosterlitz and Thouless, work going way back to the early 1970s. When I was doing my thesis work trying to figure out how to find a lattice version of topological invariants of gauge fields, I started out looking at the case of the 2d XY model which they had studied, where the topology is much simpler.

  Congratulations to them, probably next year for the LIGO guys…

- My colleague Daniel Litt has started up a really nice blog.
- Some sort of time warp back to the days of pre-LHC hype of the last decade seems to have occurred while I was in Germany, leading to lots of media stories like this one.
- In Heidelberg among the people I met were Dirk Huylebrouck, who reminded me that there's lots of great material in the Mathematical Intelligencer, including his "Mathematical Tourist" column, and Barry Cipra, one of the authors of the AMS's What's Happening series.
- John Baez is involved with a new project, funded by DARPA, that he describes here.
- Last week there was a conference in Madrid devoted to the question Is SUSY Alive and Well?. Of the talks I looked at, the only one with a sensible answer to the question was that of Alessandro Strumia.
  Update: A commenter points to this very interesting survey of the participants.
- In case you haven't heard what's going on in Leicester, Tim Gowers explains here.
- I was very sorry to hear of the passing last Saturday of Joseph Birman, a theorist at CCNY, and husband of my colleague Joan Birman. Some information about one aspect of Joe's work is here, perhaps more about other aspects will appear soon.

Comments

1. **Bernd**
   October 5, 2016

   The Leicester story gets a sad ironical twist with this year's Nobel prize going to three Brits who left the country during the Thatcher madness. Doesn't seem they have learned anything. To make matters worse, the "good citizenship" requirement nowadays includes spying on your students to spot potential "terrorists". A colleague of mine grew up in the GDR and finds these practices eerily familiar.

2. **anon**
   October 5, 2016

   The survey results from that conference are interesting

   https://workshops.ift.uam-csic.es/susyaaw/Survey

3. **Peter Woit**
   October 5, 2016

   anon,
   Thanks, very interesting. I've added a link to that to the posting. Interesting to see that even among experts on SUSY still working on it, "alive and well" is the opinion of less than 30%.

4. **MikeS**
   October 5, 2016

   There may actually be some wisdom in giving the LIGO discovery another year to mature. Not least because that gives a clear opportunity for another clear event or more to be identified.
   The first event GW150914 is always associated with that slight niggle because it was almost immediately seen on switch on, nothing so clear since, and no corroboration from any other observation type. The subsequent GW151226 event is so buried in the noise that a skeptic may believe that reverse engineering the result cannot be entirely ruled out. And again no corroborating gamma ray burst or the like. Yes I realise that some very sound statistics have been employed, but in the end we still have only two events.
   The room for doubt may be tiny, but hopefully this time next year it will be non-existent.

5. **KJ**
   October 5, 2016

   "Alive and Well" is the opinion of less than 30%, but on the other hand, "Alive" in some form is the opinion of over 85% of those surveyed. A survey question not asked, to which I would have liked to see the results, is "Under what circumstances would you be likely to consider SUSY dead?" with options for not seeing any evidence from various runs of the LHC or other future events.
   I wonder how many would pick "I will never agree that SUSY is dead"?

6. **Yatima**
   October 5, 2016

   And again no corroborating gamma ray burst or the like.

   If these are black hole collisions happening hundreds millions of LY away, one might suppose that there would never be anything to put on a photographic plate (these are blacks holes, after all). On the other hand… Modeling the afterglow of the possible Fermi-GBM event associated with GW150914

7. **anon**
   October 5, 2016

   More information about the survey included many comments here https://workshops.ift.uam-csic.es/files/205/Survey.pdf

8. **Low Math, Meekly Interacting**
October 6, 2016

What's this "suppressed SUSY" business?

9. **Jeffrey M**
October 6, 2016

LMMI,

Well, curiouser and curiouser. If you Google "suppressed SUSY" you hit a ton of papers by a John Dixon. If you check these papers, the recent ones have no affiliation listed for him. Go back a bit, and the affiliation is a law firm. Back further, and he was at Texas A&M. Anyone care to explain?

10. **Low Math, Meekly Interacting**
October 6, 2016

In the comments from that survey, it appears to be the most-cited reason to continue to expect natural SUSY. Or, at least, the notion of "suppressed SUSY" is deemed to be both sufficiently promising, despite being poorly understood, to not dismiss the notion of SUSY being a viable solution to the hierarchy problem. From what little I can understand, "suppressed SUSY" seems to provide a "natural" explanation for why sparticles have not been, will not be seen by the LHC. It also seems qualitatively different than other ideas (e.g. sparticles "hiding in plain sight" because the masses of the LSP and the next-to-LSP are nearly equal) explaining away the non-appearance of SUSY.

Lacking the needed expertise/IQ/what-have-you, I'm unable to determine if my limited understanding is correct. I'm also unable to determine how desperate a measure" suppression" is to rescue "naturalness", compared to the others in the SUSY field.

11. **Peter Woit**
October 6, 2016

LMMI,
The problem with those comments is that you don't know whether they reflect anything other than the views of one particular participant. In this case, John Dixon is listed as a participant at this conference, and the comments about "suppressed SUSY" may just be his enthusiasm for his own idea.

12. **Low Math, Meekly Interacting**
October 6, 2016

Got it. Didn't factor in such a powerful, uh, selection bias. Rather, figured those comments might reflect a broader sample.

13. **JG**
October 6, 2016

Speaking of "Alive and Well" can anybody confirm whether the LIGO "trio" (Rainer Weiss (age 84), Kip Thorne (76) , Ronald Drever (85)) need to survive until the end of nominations in January or if they have to still be alive next October at the time of the award of the Nobel?

14. **John Dixon**
October 6, 2016

I am the John Dixon of whom you are speaking, in relation to Suppressed Susy, and the recent Madrid conference on `Is SUSY alive and well'. A friend pointed out these remarks to me. This is a test to see if this gets online. If it does, then I will probably make a short reply to your remarks about the conference, and also about my own work.

15. **John Dixon**
October 6, 2016

Well OK then. I tend to agree with Peter Woit, that the talk of Alessandro Strumia, which gave a short answer NO to the question `Is SUSY alive and well?' made some valid points. Alessandro did get a lot of flack for that answer at the conference. But SUSY is certainly in trouble, using the usual ideas, in my opinion.

The purpose of a conference of course is to create a venue where people can exchange ideas. The Madrid conference did that, and I enjoyed it.

The problem for SUSY, at present, is that the popular view at the conference was that SUSY can get spontaneously broken in an invisible sector, which is then manifested in the visible sector (our world) by some unknown messenger sector. The trouble is that this is supposed to give rise to some hundred and fifty new undetermined parameters.

Much of the conference was a discussion of these parameters, and how the present experiments constrict the huge space that these occupy. It does not look very good for the theory in my opinion. Also I do not think that the theory makes much sense anyway. It is rather like the Emperor's new clothes as things stand–you can't see the invisible sector, and that is where everything important is supposed to happen.

It is true that I do not presently hold an academic position. Recently I was at Oxford for six months or so as a visitor. My recent papers have been published however by a reputable refereed journal, Physics Letters B. It is also true that these are not yet widely known or cited. They are very new and the ideas are new too. However, this does not mean that they are necessarily wrong or crackpot. I went to the conference and also the SUSY 2016 conference in Australia in July, to discuss these ideas with colleagues. I did give a talk at SUSY 2016. People at the conferences seem somewhat interested, but as the comments above note, the thing is complicated, and I expect progress to be slow.

Suppressed SUSY is an idea that amounts to a way of splitting SUSY masses without spontaneous breaking of SUSY. The next step is to calculate the one-loop corrections.
So I do not know yet how successful it will be. It depends on the Master Equation for the BRST formulation of SUSY, as explained in the papers.
Other papers are in preparation.

16. **Jeff M**
October 7, 2016

JG,

I believe, but someone correct me if I'm wrong, that you have to be alive when the prize is awarded. You don't have to make it to the presentation ceremony.

17. **Thomas Larsson**
October 7, 2016

More precisely, the Nobel committee must think that you are alive when the prize is awarded. On 3 October 2011, the laureates for the Nobel Prize in Physiology or Medicine were announced; however, the committee was not aware that one of the laureates, Ralph M. Steinman, had died three days earlier. The committee was debating about Steinman's prize, since the rule is that the prize is not awarded posthumously. The committee later decided that as the decision to award Steinman the prize "was made in good faith", it would remain unchanged. [Wikipedia]

18. **NotPhysicist**
October 7, 2016

I tried to post this earlier but it didn't show up, possibly due to the spam filter, so I am trying again. I mean no annoyance if it did not show up due to being considered off topic. I would like to see some discussion of this issue somewhere though.

>MikeS: "Yes I realise that some very sound statistics have been employed"
–
I'm not so sure the headline stats (that really small p-value) were interpreted correctly:
–
"We present the analysis of 16 days of coincident observations between the two LIGO detectors from September 12 to October 20, 2015.
[…]
The significance of a candidate event is determined by the search background—the rate at which detector noise produces events with a detection-statistic value equal to or higher than the candidate event. Estimating this background is challenging for two reasons: the detector noise is nonstationary and non-Gaussian, so its properties must be empirically determined; and it is not possible to shield the detector from gravitational waves to directly measure a signal-free background. The specific procedure used to estimate the background is slightly different for the two searches, but both use a time-shift technique: the time stamps of one detector's data are artificially shifted by an offset that is large compared to the intersite propagation time, and a new set of events is produced based on this time-shifted data set. For instrumental noise that is uncorrelated between detectors this is an effective way to estimate the background."
http://journals.aps.org/prl/abstract/10.1103/PhysRevLett.116.061102
–
"Even though the routine data quality checks did not indicate any problems with the data, in-depth checks of potential noise sources were performed around the time of GW150914…No data quality vetoes were active within an hour of the event.
[…]
Following the event, the detectors were maintained in the same configuration to ensure that detector changes would not cause unanticipated consequences which might bias the background estimation for the event."
https://arxiv.org/abs/1602.03844
–
So the hypothesis being tested is that their model of background noise accurately describes what the detectors were sensing at the time of GW150914. Also, note the model of background noise is based on data collected primarily after the detection. However, members of the LIGO team have described that this was not the case, the processing pipeline was changed in response to GW150914:
–
"At 11:23:20 UTC, an analyst follow-up determined which auxiliary channels were associated with iDQ's decision. It became clear that these were un-calibrated versions of h(t) which had not been flagged as "unsafe" and were only added to the set of available low latency channels after the start of ER8. Based on the safety of the channels, the Data Quality Veto label was removed within 2.5 hours and analyses proceeded after restarting by hand."
http://www.ligo.org/magazine/LIGO-magazine-issue-8.pdf
–
So that statistical test doesn't mean much to me, the hypothesis it tested was rendered false by altering the configuration. That numerical relativity simulations could easily reproduce the signal is far more convincing than their statistical argument.

19. **Harry Dale Huffman**
October 7, 2016

This comment is off-topic for this post, but it's the easiest way to bring it to your kind attention:

I just, for the first time, came across a blog by Sabine Hossenfelder called "backreaction", and there is a post there on "Demystifying Spin-1/2"

I didn't think the discussion there demystified spin-1/2 particles at all. Hossenfelder first wrote that spin-1/2 particles are "…particles that have to be rotated twice to return to the same initial state." She then made a statement I readily agreed with: "I don't know if anybody who didn't know the math already has ever been able to make sense of this explanation – certainly not me when I was a teenager."

But then, instead of "demystifying" anything, she merely doubles down on the idea of "particles that have to be rotated twice to return to the same initial state", by introducing how rotations (or, more generally symmetry transformations) act upon quantum states. She writes, "A symmetry transformation acting on a quantum state must be described by a unitary transformation …. And the full set of all symmetry transformations must be described by a 'unitary representation' of the group." Then, "Symmetry groups, however, can be difficult to handle, and so physicists prefer to instead work with the algebra associated to the group. The algebra can be used to build up the group…. But here's where things get interesting: If you use the algebra of the rotation group to describe how particles transform, you don't get back merely the rotation group. Instead you get what's called a 'double cover' of the rotation group. It means – guess! – you have to turn the state around twice to get back to the initial state." So she's gotten back to that original definition of a spin-1/2 particle. But in my view, she hasn't "demystified" spin-1/2 particles, she has merely given a quick summary of why mathematical physicists define them that way (in other words, it's just the mathematics used, not the physics). For anyone interested in what spin-1/2 particles are physically, surely it is simpler to say they are particles that are always (can only be) observed to have either "spin up" or "spin down" along any given axis. That doesn't really explain what "(quantum) spin" is either, physically, but it captures the observable reality where "double cover of the rotation group" does not.

Why didn't Hossenfelder give the traditional definition I just gave, and why did she call her post "demystifying spin-1/2", when she did not do that at all? I would like to see a post by you on this, to see where you and your readers stand on it.

20. **Peter Woit**
October 7, 2016

Harry Dale Huffman,

That is highly off-topic, and I don't want to have a discussion of it here, not because it's not interesting, but because it's too interesting, and deserves something more serious. I did look at that blog posting, and it struck me as running into the standard problem such discussions always run into: you can get from knowing how SO(3) rotations act on vectors (which is what we have lots of intuition about) to all sorts of interesting things, but you can't get the spin 1/2 representation this way. In particular, you can motivate the fact that \pi_1(SO(3))=Z_2, but this isn't enough to tell you about the spinor representation.

I think this is an example of something I disagree with lots of physicists about. Yes, if you really understand something you should be able to explain it to your grandmother, but in cases like this you're just going to confuse her (or mislead her into thinking she understands something she doesn't) if you try and explain things in terms of her intuitions about rotating objects in 3-space. Granny is going to need to sit still and pay attention for quite a while, as you first explain to her some new ideas she has no intuition for. One of various ways of going about this is to start by telling her about complex numbers and show how you can use them to study rotations in the plane, then introduce quaternions and show how to think of rotations in terms of them. At that point you have the spinor representation, not just the vector representation (for the details of what I'm talking about, see chapter 6 of http://www.math.columbia.edu/~woit/QM/qmbook.pdf).

Retraction at Annals of Physics

October 8, 2016
Categories: Uncategorized

Retraction Watch reports that *Annals of Physics* has removed a recently published article by Joy Christian, replacing it by a publisher's note that just says:

> "This article was erroneously included in this issue. We apologize for any inconvenience this may cause."

The paper is available on the arXiv here. Christian's affiliation in the abstract is listed as "Oxford". This refers to the Einstein Centre for Local-Realistic Physics which is not at Oxford University, but at a location in the town that I think I unknowingly walked past on my way to go punting last week. The only person involved with the centre who lists an academic affiliation is Dr. Jay R. Yablon (MIT), who appears to be a patent attorney in Schenectady.

This story brings back memories of the Bogdanov affair of 2002, one aspect of which was the publication by the Bogdanovs in *Annals of Physics* of a paper that, as far as I could tell, made little sense. That paper was never removed or retracted. The editor-in-chief when the Bogdanov paper was accepted was Roman Jackiw. Frank Wilczek took over from him and said at the time that he was hoping to improve the journal's standards. The current editor-in-chief is my Columbia colleague Brian Greene.

Comments are off since I would rather not host a discussion involving the merits of this paper. I haven't tried to seriously read it, and don't want to spend time doing so. In the Bogdanov case I spent (wasted…) a lot of time reading their papers, so felt comfortable discussing them, not about to do the same in this case.

No Comments

YITP at 50

October 11, 2016
Categories: Uncategorized

The past couple days the YITP at Stony Brook has been celebrating its 50th birthday. It was started back in 1966 by C. N. Yang and has been an active center for theoretical physics ever since. The ITP at Stony Brook was as some point renamed in honor of Yang, now it's officially the "C. N. Yang Institute for Theoretical Physics". I was a postdoc there in 1984-87, when it was just the ITP, and Yang was still the director. I had been hoping to go out to Stony Brook for at least one day of the event, but unfortunately other things have kept me here in New York.

Luckily, with today's technology one can watch the talks online (see here) and follow what happened at the conference. I've watched a few of the talks, and they give a good survey of the kind of work that has been going on at the institute over the last 50 years. One aspect that isn't emphasized in the talks (although there's a little bit in Fred Goldhaber's talk) is that the institute is in the same building as the mathematics department with, at least back in my day, some physicists and some mathematicians even having offices nearby on the same floor. Being able to talk to and learn from some great mathematicians (soon after Yang, in 1968 Jim Simons came to Stony Brook and brought together a world-class mathematics department) was a big influence on me during my postdoc years. These days, with the Simons Center for Geometry and Physics, Stony Brook is one of the great centers of mathematical physics.

The last talk of the event was a public talk by Ashoke Sen on What is String Theory? (slides here), one which made me think that maybe it wasn't a bad thing that I hadn't made it out to Stony Brook, since I might have been there for this. Sen's talk was a depressing compilation of ancient hype and misleading claims about string theory, with the standard multiverse excuse for why it predicts nothing at all about particle physics.

My time at the ITP coincided with the early years of this kind of string theory hype, which got started in late 1984, about the time I got there. By my last year there (exactly 30 years ago, 1986-87), everyone in the physics community had already been subjected to a couple years of this kind of thing, so much so that Ginsparg and Glashow had published in spring 1986 their Desperately Seeking Superstrings article, noting that

> …years of intense effort by dozens of the best and the brightest have yielded not one verifiable prediction, nor should any soon be expected.

They worried that

> Contemplation of superstrings may evolve into an activity as remote from conventional particle physics as particle physics is from chemistry, to be conducted at schools of divinity by future equivalents of medieval theologians.

which many at the time thought was kind of harsh, but in retrospect looks quite prescient. I doubt that even they thought that anyone in the physics community would sit still 30 years later to listen to a talk like Sen's.

My own attitude at the time was that superstring theory was just one in a sequence of fads that had gotten the attention of particle theorists, with one to two years the usual decay time for such things. So by 86-87, I figured this one was now past its sell-by date and would soon be on the way out. How wrong I was.

Comments

1. **Jeffrey M**
   October 11, 2016

   Peter,

   Am I wrong that string theory was kind of on the way out, not in 87, but by the early 90's? As I sort of remember it, there were major issues with various singularity sort of issues, and then Witten figured out ways around them, and things took off again, getting much worse when Malcedena came along. I of course never knew the details, being a mathematician, but I followed it a little since I knew Witten as a mathematician, a very good one.

2. **Tim Nguyen**
   October 11, 2016

   Pardon my sense of humor Peter but if string theory died out long ago, you wouldn't have your book, your blog, and your readers. In this other sector of the multiverse, who might you be in 2016?

3. **Peter Woit**
   October 12, 2016

   Jeffrey M,
   Interest in string theory was starting to die down in late 80s, early 90s (and during a lot of this period Witten was working on TQFT/Chern-Simons, topics not related to string theory). M-theory in the mid-90s really revived and changed a lot the nature of what string theorists were doing. It didn't at all though address the problems of connecting string theories of unification with reality.

   Tim Nguyen,

   An interesting question. Quite likely I wouldn't have a blog or popular book, maybe my book about QM and math would have been finished long ago (instead of maybe next week…). I'd like to think that in such a universe we'd have learned a lot more about quantum field theory by now, and I'd hope about questions that always have motivated me. So, I think I'd be much less well-known, but maybe much more content…

4. **A.J.**
   October 12, 2016

   * I'd like to think that in such a universe we'd have learned a lot more about quantum field theory by now.*

   I don't really understand this claim. You can say whatever you want about string theory as a model of particle physics. We haven't had any progress in that subject since the 70s; indeed, any progress beyond the Standard Model was at best premature. But it seems odd to me to dismiss what we've learned about QFT by studying strings, just because you find it unappealing as an approach to particle physics. The study of string theory, especially after 1995, has been the single most fruitful source of new toy models, new mechanisms, and new ideas in QFT.

5. **John McAllison**

October 12, 2016

Peter, while what you claim about interest in String theory declining in the late 80s and early 90s is true, it doesn't tell the whole story from the information on page 20 here: https://workshops.ift.uam-csic.es/files/205/Strumia.pdf

There's a huge x10 jump in interest beginning in 1984 after the first String revolution, falling sharply to half this level in early 1990 and remaining fairly constant up to early 2000.But the general picture is a decline, as you say, compared to its glory days of 1986.

6. **Peter Woit**
   October 12, 2016

   A.J.
   I'm not dismissing what has been learned about QFT as a byproduct of string theory, I'm just pointing out that, if effort devoted to string theory had instead been devoted directly to questions about QFT, quite possibly we'd have learned more about QFT. Making QFT research an appendage to string theory research means that certain aspects of QFT have gotten a lot of attention and made progress (e.g. 2d CFTs), whereas other questions about QFT have gotten little attention, because they have no connection to string theory.

   As an example of what I mean, I was referring above to the amazing breakthroughs in our understanding of QFT that Witten came up with (especially Chern-Simons, TQFTs) during the late 80s, early 90s, many of which had nothing to do with string theory. What if there had been no M-theory "revolution"? Yes, you would lose all sorts of things that came out of that, but perhaps the QFT research directions that people followed instead might have been even more interesting. We'll of course never know.

   John McAllison,

   I think that slide does a good job of measuring the popularity of string theory. It was at its height in 85-86, reached a local minimum around 1994, revived for a while, and has been steadily decreasing since 2000.

7. **A.J.**
   October 12, 2016

   Peter,

   It's true (by definition, really) that when we learn something about QFT by working on string theory, we can understand it independently of string theory, although usually at some cost in intuition. (Sometimes we can even reduce it to something mathematicians can work with.)

   But just because something is logically independent of X, doesn't mean we'd have come up with it without having worked on X. Witten was working on topological QFTs because he was looking for simplified models that exhibited background independence, so that he could better understand string theory! His first paper on the subject, "Topological Quantum Field Theory", is quite clear about this; he even says straight out that the core arguments should be familiar to string theorists. Other lines of inquiry on topological sigma models and mirror manifolds are explicitly string theoretic. The 1989 Chern-Simons theory paper is something of an odd man out, since it's a Schwarz-type TQFT and because he was trying to write something mathematicians could digest. (But it didn't take long for string theoretic work to spark further developments in Chern-Simons theory, like the connection Gopakumar-Vafa invariants.)

   Likewise, the Seiberg-Witten revolution in differential topology was really spin-off of spin off of non-topological string theory calculations. S&W were trying to understand dualities in string theory, and they came up a calculable model of 4d confinement. To my mind, that's of far greater importance to physics than any of the topological baubles we mathematicians get distracted by.

   The things you're suggesting might have been better to study than string theory, came from string theory.

   Note: I'm not advocating making QFT research an appendage to string theory research. There's a lot of good stuff out there that didn't come from string theory. But I think it's important to dismiss the origins of ideas, and it looks to me like a world where we didn't discover string theory is just a world where we don't know as much.

8. **A.J.**
   October 12, 2016

   Heh. I meant to say 'not to dismiss the origins of ideas', but my inner mathematician interfered.

9. **Peter Woit**
   October 12, 2016

   AJ,
   Your claims about history really are just not true. I was there at the time, and later I spent a lot of time carefully researching the details of this history before I wrote about it in my book. One place where Witten tells the story in his own words is here:
   http://www.ims.cuhk.edu.hk/~ajm/vol3/3_1/witten.ps
   Note that there's nothing about string theory there. Yes, this was (87) exactly the period when it had become clear that initial hopes for string theory weren't working out. Witten was undoubtedly hoping that the ideas about QFT he worked out in the TQFT paper would lead to some new insight about string theory. The flow of information though was opposite to the one you suggest: he wasn't at all using ideas from string theory to find the TQFT that Atiyah had suggested existed (based on Witten's ideas about supersymmetry and Morse theory, applied to the space of connections). Once he had found the right TQFT undoubtedly he was hoping it would help him overcome the roadblock he was facing in string theory (it didn't).

   As for his Chern-Simons paper, sorry, that had nothing to do with string theory either (although it did have to do with the WZW cft). The idea that that came from string theory, but Witten just didn't mention this in his paper because it was aimed at mathematicians is nonsense.

10. **paddy**
    October 12, 2016

    Am awaiting LMMI's always informative questions so I can glean more from the discussion. PS Apparently, Peter, we overlapped there at Stony Brook–tho I was not in the 'tower' but in the dungeon labs.

11. **A.J.**
    October 12, 2016

    It's not surprising that string theory isn't mentioned. Witten says explicitly in the linked article that he's leaving it out of the discussion, presumably because he's writing for an audience of mathematicians. Then he skips over the details of how he came to write down the twisted N=2 theory, saying only that he had the 'good luck' to notice how to do it. (He similarly elides the story of how he & Seiberg had the 'great good fortune' of looking at electric/magnetic duality in N=2 theories…) So, no, I don't think that article gives a complete picture of what was in his mind and motivating him when he cooked up the Donaldson theory. Fortunately, we do have the original article, where he points out that the core arguments are analogous to ones already known in string theory. Forgive me if I take Witten's word here.

You can divide TQFT from string theory logically, but you can't split up the intellectual histories of the two subjects. I don't even know why you'd want to, given how much explanatory power you lose.

12. **Peter Woit**
October 12, 2016

Just realized I've been through this same discussion before, with Lubos Motl. For those who care, see
http://www.math.columbia.edu/~woit/wordpress/?p=99

13. **No Math, Meekly Interacting**
October 13, 2016

Hi, paddy,

I got nothing. I'm just astonished at the "Desperately Seeking…" article. If they added a paragraph on the 10^500 vacua, seems like it could have been written 30 minutes ago instead of 30 years and still been quite topical. Philosophically, nothing's been settled, despite the best efforts of the Bayesians to make it seem otherwise. Plus ça change…

14. **A.J.**
October 13, 2016

One correction to the history you give in your link: You say that by '89, Witten "has completely abandoned the idea of relating Jones polynomials to topological sigma models" and that "New ideas about relations between branes, topological strings, and Chern-Simons appeared about ten years later". In fact, the next big step in the story comes in '92, with Witten's "Chern-Simons Gauge Theory As A String Theory", where he explains how to realize Chern-Simons theory as the spacetime physics of a topological string theory, more or less as he'd anticipated in the first TQFT paper. That's only a 3 year gap, which doesn't seem like much to me, given that it's a substantially more difficult paper.

15. **Tim Nguyen**
October 13, 2016

"I knew Witten as a mathematician, a very good one."

I was actually fortunate enough to have Witten come to my talk on quantum Yang-Mills at the IAS earlier in April:
https://www.youtube.com/watch?v=3lEgEGCH2cg

of which are more up to date and entertaining account of my work can be found here:

https://www.youtube.com/watch?v=hHXavKP6EmI

I was a bit surprised that the only question Witten had during question time at the end of my talk was matters pertaining to higher genus – a question only a mathematician (or a string theorist?) would ask! Ironic, since I myself as mathematician/physicist, the motivation behind my work was much more along the lines of fundamental physics/QFT and I was hoping Witten would have something to say about those issues.

Based on this isolated experience, I suspect speculations about Witten's true motivations may not be so cut and dry.

16. **student**
October 13, 2016

How do you think Montonen-Olive duality would have fared without the work of Sen, whom you just managed to be breathtakingly disrespectful to.

17. **Peter Woit**
October 13, 2016

student,
I think Montonen-Olive duality would have fared fine without Sen. And what's breathtakingly disrespectful here is not my (accurate) characterization of his talk, but his decision to deliver a talk full of misleading hype and pseudo-science to the audience at Stony Brook.

18. **adrian**
October 13, 2016

Dear Peter,
echoing a bit here what the commenters "student" and AJ wrote. You know I usually do not comment in this forum, but the discussion is interesting in this case.

–About Ashoke Sen's talk: true, the first part of his seminar is a bit disappointing. Except for the analogy with the phases of water, there is not much. But may be that is his view of that part of String Theory. If one reads the second part, the thing takes a much more interesting perspective. In particular, Sen discussed the virtue of the black hole as an ideal system. This lead to remarkable progress. It is a counting problem that 'knows' about quantum gravity. Far from trivial are Sen's and collaborators papers on this topics.

I just felt that your comment about his talk was unfair. May be I am biased by the fact that knowing Sen, having discussed with him in few occasions, I clearly have the perception that he is a very deep thinker, who truly cares for his research and the truth in it. Aside from a very nice person. I know I am mixing things here, so just wanted to stress that the second part of his seminar was much more insightful.

–About QFT and all we missed to learn because of the efforts being focused on string theory: You are saying in one of your responses to the commenter AJ that:
" I'm not dismissing what has been learned about QFT as a byproduct of string theory, "

On which we agree. The bit about "many more things about QFT would have been learnt if the effort on strings had been dedicated to QFT" I think i disagree. Of course, we do not know and we cannot know. But, I know you appreciate the fact that the theory of strings, dealing with very symmetric systems, has a high chance of striking a discovery.

There are various examples in "stringy times' ' of QFT progress that happened 'independently' of String theory (though many of the discoverers are string people): Seiberg duality, the Seiberg-Witten solution are such examples (though not so good, as there is a stringy taste to them). Importantly, these ideas found a very nice and intuitive realisation in stringy language (brane set ups, etc). Other important advances in QFT—like the Maldacena conjecture–seem harder to come by if one is not working on strings.

All I am trying to say (a bit obvious, I believe) is that being so symmetric, String Theory suggest ideas that then might find interesting applications in QFT in general. It 'geometrizes' the Physics problems. I believe we will agree on this virtue of the theory of Strings.
About the String Theory being a description of physical Nature, I am saying nothing here. Just about its ability to struck on interesting aspects of QFT in general.

thanks for the posting and the links

19. **Peter Woit**
October 13, 2016

adrian,
The problem is that I don't think there's anything meaningful one can say about the intertwined relationship of string theory and QFT research in a sentence or two, and attempts to do so lead to stupid ideological posturing, not insight. The problem starts with the fact that "string theory" is not a specific thing (in particular, you say it is "very symmetric", but we don't know what the symmetries of non-perturbative string theory might be), but a name attached to a huge area of research into some very disparate things. There are lots of interesting specific things one could discuss, but just arguing "string theory" this or "string theory" that doesn't capture anything non-trivial. My comments about this were about two very specific advances in QFT made by Witten in 1988 where the situation is rather simple: they had little to nothing to do with string theory (under any sensible definition of the term).

I've never met Sen, and everyone I know who has tells me he's a very nice person and a serious scientist and I'm sure they're right. Of course I don't think his plan was to write a talk full of misleading hype and pseudo-science, and the talk he gave wasn't much different than a hundred other similar ones delivered over the last 30 years, many of which he's probably sat through himself and thought they were all right. That he is likely not aware of them does not change the problems with the talk.

20. **paddy**
October 15, 2016

Hi L(N)MMI,
I read that 30 years old article by Ginsparg/Glashow article many years ago (and god don't I feel old). Not sure when I read it though I suspect via a PW link at least a decade ago. But thanks to you and PW I have reread it. 'Tis somewhat prophetic, eh? (Glashow was my favorite professor during my brief foray into HEP-th 40 years ago.)

21. **Low Math, Meekly Interacting**
October 15, 2016

Somehow I missed that one. Prophetic indeed. Like Cassandra, unfortunately.

22. **Ravi Sinha**
October 16, 2016

In nearly a decade of my occasional visits to "Not Even Wrong" I feel, for the first time, sufficiently provoked to key in a comment. Ashoke's talk at YITP 50 is a model of clarity and comprehensiveness, especially for a public lecture. Even those who are dismissive of String Theory will have to grant that. Anyone who knows Ashoke even faintly knows that he cannot be associated with the word "hype" in any sense of the term. He, as someone commented above, is undoubtedly a deep thinker. But beyond that he is an unassuming and self-effacing person of a truly rare kind. Only other physicist with similar qualities who comes to my mind is perhaps Jeffrey Goldstone – my teacher at MIT nearly four decades ago.
Ashoke believes that potentially deep theories must be explored thoroughly to see what they have to say about the world. It is a fortunate thing that he has decided to deploy his remarkable talents to this end, as must also be said about Witten. Everyone knows that the twin goals of String Theory – understanding the success of the Standard Model and arriving convincingly at Quantum Gravity – have not been realized so far. I am sure leaders of the field including Ashoke are more aware of this situation than anyone else. One may remember Witten's assessment of the difficulties when he remarked nearly three decades ago about the fortuitousness of 21st century physics and 22nd century mathematics falling into the lap of 20th century.
There may be many string theory enthusiasts who may be prone to hype. But names like Ashoke Sen and Edward Witten should not be dragged into the mud.

23. **Peter Woit**
October 16, 2016

Ravi Sinha,
I don't agree at all that the lecture was "a model of clarity and comprehensiveness" (for one thing, comprehensiveness would require explaining the problems with the ideas one is promoting). Why do you engage in hype as an argument that someone else isn't?

You seem to think that criticizing the content of an academic lecture is a personal attack on someone, "dragging their name through the mud". I think that my characterization of this talk is accurate, and that it has nothing at all to do with Sen's personal qualities, which are quite possibly highly admirable. I also don't know why you bring Witten into this since he wasn't mentioned. I have written critically about things Witten has said in quite a few places, but from everything I've written it should be clear this is not inconsistent with great admiration for him personally and intellectually. That I'm "dragging his name through the mud" is a ridiculous accusation

Smart, nice, unassuming people can be wrong about things, and, at times, this needs to be pointed out. That's all I'm doing here.

24. **Jeff M**
October 16, 2016

I was looking at Sen's slides. Two things. He seems to describe compactification as the shrinking of 6 dimensions. Is that actually what physicists mean by compactification? It's not correct mathematically, mathematically compactification has nothing to do with the diameter, it's just whether every open cover has a finite sub cover. Am I misreading the slides? Is he saying that we shrink the dimensions by turning them into circles? But the circles could still be big, even though they're compact. Second thing, he makes a big deal of ADS/CFT, and how this shows that string theory is obviously going to give us a correct quantum theory of gravity. Except ADS/CFT lives in a universe where the sign of the cosmological constant is opposite to the sign in the universe we actually live in. So how is it relevant?

25. **Peter Woit**
October 16, 2016

Jeff M.,
Physicists don't use "compactification" in quite the same way as mathematicians. They use this just to mean you are looking at a theory on a pseudo-Riemannian background manifold that is Minkowski space or some cosmological background times some Riemannian manifold M. M is generally assumed to have dimensions very small with respect to observed distance scales (to explain why we don't see evidence of it). It may be smooth and compact, but also could be singular and non-compact. Typical models also add lots of other structures to M (e.g. choices of cohomology classes).

AdS/CFT solves none of the problems of string theory unification, so no point to discuss it here.

Please, keep in mind this is not a general discussion board, I don't want to moderate such a thing.

26. **GoletaBeach**
October 16, 2016

Looked up Mencken for some solace/wisdom about democracy to sooth myself from the current national election… bumped into curious Mencken viewpoints about science, about how he trusted empiricism but was harshly skeptical of even minimal mathematical intrusion… somewhat applicable to the everlasting string theory back-and-forth…. Mencken was caustically cynical about atoms, quantum mechanics, and relativity…

https://en.wikipedia.org/wiki/H._L._Mencken#Science

I am kind of "Anglican" on the string theory of the moment… that is, I think current practitioners do respect empiricism, eventually, and are conveniently hopeful that LHC etc might find something soon in their favor.

Old topic… I see your point Peter about KC Cole.

27. **Jeff M**
October 16, 2016

Peter,

Sorry, didn't intend to open a can of worms with ADS/CFT. It just really confuses me, why anyone cares about it. And thanks for clarifying about compact.

28. **Tim Nguyen**
October 16, 2016

Peter,

As someone not knowledgeable enough to have an opinion of what is/isn't a fair characterization of Sen's lecture, perhaps a suggestion for future criticisms of such lectures would be some sort of time chart of the lecture paired with corresponding specific rebuttals to make more tangible what exactly you find disagreeable. I imagine the specific objections to string theory have not changed much over the years and so have been thoroughly discussed in the annals of this blog. Which means that you could probably just link the time frames to old threads. It would take a bit of work, but if you're already watching the video, I think the extra amount of time would be worth the amount it would strengthen your objections. Just my two cents.

(As someone who's written a paper/made a video critical of the way physicists do perturbative QFT, I myself made a very precise criticism of Peskin-Schroeder by highlighting specific passages and explaining what goes awry. Thus far, I've had no one object to this analysis and I do not anticipate anyone being capable of doing so. This is quite different than the vague "physicists aren't rigorous" throwing in of the towel by nearly all mathematicians, which has never deterred physicists from doing what they do and has not increased mathematician's understanding of QFT.)

29. **Tim Nguyen**
October 16, 2016

I might as well illustrate precisely what I meant by a specific criticism of Peskin-Schroeder, here is a link to the video at the precise moment (the 1:11 mark):

https://youtu.be/QTjmLBzAdAA?t=1m11s

Of course, you don't need to be as technologically fancy as I was, but a link to the video to be analyzed, lists of specific time frames, and your rebuttals would get the job done.

30. **Peter Woit**
October 16, 2016

Tim Nguyen,
Sorry, but I'm not going to spend time doing that kind of thing. I spent a couple years writing a book addressing these issues in detail and explaining what the problems were with the conventional string theory hype. Sen is just repeating pretty much the same hype the book was aimed at. If he had a new argument, maybe I'd discuss that, but he doesn't. No sensible person wants to read yet another explanation from me of why string theory unification has failed and why the string landscape multiverse is pseudo-science.

I should point out that references to the problem of continuing string theory hype like the ones in this blog posting are not really aimed at explaining the problem to non-experts, for that there's the book. Knowing how these things go and from the little I could see and hear of the audience in this video, I'd suspect that physicists who had heard all this many times before made up most of the audience. In some sense what I write here is aimed at them: why do you put up with this? It's done a huge amount of damage to your field and its credibility, why schedule talks like this, then sit there politely and listen to them?

31. **Tim Nguyen**
October 16, 2016

Peter,

Of course, you know how best to spend your time rather than me. It just seems that there is enough energy spent going back in the forth in the comments that a pre-emptive investment along the lines I suggested might be worth considering. It was just a thought.

"It's done a huge amount of damage to your field and its credibility"

Well, "my field" is mathematics and I'm actually under the impression that mathematicians are at worst agnostic about string theory, but at best, they are willing to hire them since string theory has inspired a lot of rich mathematics. I'm unusual in that I'm motivated in trying to understand QFT. I haven't gotten into string theory because my time and energy are finite and so I've chosen to focus on something more grounded (in fact, it's already risky/suicidal enough to pursue QFT as a mathematician, so I don't need to further up the stakes with string theory).

I can sympathize with you to one extent which is that I do feel that there is a great deal of work done on mathematical "QFT" that may as well be classified as "damaging", e.g. works in which the mathematical formalism itself supplants the substance of the subject (e.g. imagine if in learning "general relativity" one became preoccupied with tensors and how one could make them more complicated, adding more indices, making things noncommutative, invoking higher categories only for the sake for their mathematical consistency, but then left out the underlying geometry and gravity itself; the consequence is research that is much ado about nothing insofar as

GR is concerned and I do find talks along these lines hard to "sit politely" and listen). One of the reasons I invested time in my videos was to provide a no-nonsense account of the mathematical accounts of QFT, one which gets straight to the point and avoids all the "damaging" detours I went through before finding my way.

As this was a completely avante garde thing to do and only undertaken recently, I don't know how much of an influence/difference this will make. But it seems to me, as with all social/political changes, the voice of reason needs constant rekindling and repetition. I myself read your book some time ago, it was a good read, but I can't remember the exact arguments or details any longer. Just as no two protests over the latest outrage over police violence/etc. are the same, I imagine no two "damaging string theory lectures" will be completely identical or redundant either. But if your goal is to get people to see your point of view (I imagine you desire this to some extent), perhaps creative forms of reinforcing what you've already claimed, along the lines I've suggested, might work. This is of course up to you, but it's a suggestion that at least is backed up by what I'm trying to do in my own terms.

In any case, I suspect this thread shouldn't degenerate into suggestions on how your blog should be run, unless of course, you are open to polling readers on this idea 🙂

32. **Ming**
October 17, 2016

I attended Sen's lecture in Stony Brook, and while I mostly agree with you on the assessment of String Theory, I think I should share some observations I have regarding Sen's lecture. I was quite surprised at the turnout, since it wasn't really widely promoted (e.g. the poster for the event was only distributed a few days before the event). There were at least 300 people in the audience, including people from on and off campus. I think even the university president was there. What also surprised me was the almost-rock-star status of Sen in the eyes of some participants. Most of the audience seemed in awe of him, never mind that most probably didn't understand what he's talking about (made worse by his heavy Indian accent), almost nobody left in the middle of the lecture, and at the end I even saw people asking Sen for autographs! As a physicist myself I have to say I'm pretty impressed but also baffled. Granted Sen is a rather famous theoretical physicist in his field, but even some physicists may not know him outside his field. I guess we have Yuri Milner and his glitzy Breakthrough Prizes to thank for elevating Sen to physics superstardom in the eyes of the public…

33. **Peter Woit**
October 17, 2016

Ming,
Thanks for the report. From the video one couldn't tell what the size of the audience was. It is remarkable that one can get that large a crowd to come hear a not so well known string theorist give a promotional talk indistinguishable from many others given over the past 30 years.

34. **Boundary graviton**
October 21, 2016

It's amusing for you to say that a very highly cited Dirac medalist physicist to be not so well known. When non physicists writing nay saying books on physics can be popular, it may be better to remain not well known. But nevertheless… It's amusing to see your comment

35. **Peter Woit**
October 21, 2016

Boundary graviton,
The "not so well known" referred to "not so well known" to the public, which was the audience for this talk. I think it's quite accurate to say that Sen is not well known to the public, certainly in the US. The same is true of the Dirac medal.

Actually, I think the same is even true for physicists in general, leaving aside theorists, who would generally know about him.

36. **Boundary graviton**
October 21, 2016

Right. I get the sense which you told it now. Stony Brook is where he did his PhD. That might have contributed.

37. **Boundary graviton**
October 21, 2016

*in which

By the way, when Maldacena visited India, lectures were filled to full capacity. Same was the case with Witten but Witten is much more known to the public perhaps.

Math Items

October 17, 2016
Categories: Langlands, Uncategorized

A few mathematics items:

- David Ben-Zvi's overview talk about *Representation Theory as Gauge Theory* given last month at the Clay conference in Oxford that I attended is now available online, as slides and video. Other talks from the conference are here.
- My fantasy that I might try and understand arithmetic algebraic geometry by reading Tate's collected papers keeps getting delayed as the AMS puts off publication (now scheduled for January 18 of next year). While the books are not available, at least Milne's review is.
- A couple weeks ago there was a Beyond Endoscopy conference at the IAS, at the same time I gather functioning as an 80th birthday celebration for Langlands. There's a write-up by Langlands of his talk here. I think it can be described as the current Langlands take on "Geometric Langlands".
- No recent news I'm aware of concerning Mochizuki and the the abc conjecture, but Inference magazine has just published a long article by Ivan Fesenko giving his take on "Inter-universal Teichmuller Theory".
- The Breakthrough Prize symposium this year is scheduled for December 5 at UCSF, so I guess that means the prizes will likely be announced and awards ceremony held December 4, if things go like in recent years. I have no idea who will get the $3 million math prize since it's a relatively new prize and there is a whole world of accomplished mathematicians who would make good candidates. One can be pretty sure though who won't get it, arguably the most accomplished young mathematician around, Peter Scholze (since he turned down the junior version last year).

  I have a modest proposal for whoever is awarded the prize: if you're financially pretty well set already, how about doing the math community a huge favor? Donate the money to your university to endow a faculty position, then use the influence and moral high ground this will buy you to try and convince the Breakthrough Prize people to make this a policy. In the future, the winner gets a $3 million check made out to their institution to endow a position in their name. Then they could even try again with Scholze and perhaps get him to accept.

  At the same time, there will also be a $3 million physics award. For a while these things were going pretty uniformly to string theorists, then they turned around and started giving them to experimentalists. I have no idea what they'll do this year.

Comments

1. **David Derbes**
   October 17, 2016

   Breakthrough Prize in Physics to the LIGO guys? Kip S. Thorne, Rainer Weiss and Ronald Drever? I thought they were a lock for the Nobel. Then again, Higgs and Englert had to wait a year after the CERN announcement.

2. **Peter Woit**
   October 17, 2016

   David,
   They already gave those three (and the rest of LIGO) a Special Breakthrough Prize earlier this year.

   https://breakthroughprize.org/News/32

3. **Michael Weiss**
   October 17, 2016

   "Like many budding number theorists," writes Milne, "Tate's favourite theorem when young was Gauss's law of quadratic reciprocity."

   John Tate shared with our undergraduate number theory class in the spring of 1977 (then designated Math 101) that as a teen he came across Quadratic Reciprocity and immediately closed the book to attempt a proof himself. For several months, Prof. Tate recalled, he persevered but without success.

   Prof. Tate wistfully noted to the class that by contrast the young Gauss had developed four different proofs. He then passed around Disquisitiones Arithmeticae in Latin. John never spoke to us of his own enormous contributions to generalized reciprocity laws.

4. **Tom DeLillo**
   October 18, 2016

   Good idea, Peter, to endow new positions. Let's keep this suggestion alive! We've got money coming in at my state university to support a chair in honor of a deceased colleague. However, we would need permission to hire into an existing tenure line and there's a hiring freeze due to state budget problems. There seems to be money around for other things. (I discreetly avoid giving the name of my institution, but you could probably track me down.)

5. **random reader**
   October 18, 2016

   Peter, you may be interested in the recent arxiv posting by Kevin Costello, with what I think is the first mathematically rigorous construction of a particular type of (perturbative) M-theory. http://arxiv.org/abs/1610.04144

6. **Richard**
   October 18, 2016

   I would not be surprised if Maryna Viazovska — whose pioneering work* on sphere-packing in dimensions 8 and 24 (The latter being a collaborative effort.) has caused something of a sensation — is the recipient of the $3-million Breakthrough Prize.
   Her status as a potential future Fields Medalist ought also not to go unremarked-upon….

   * Of which I understand only the occasional punctuation mark.

7. **Coxeter Todd**
   October 18, 2016

I would guess Yitang Zhang for his work on bounded gaps between primes.

8. **anon**
October 18, 2016

I'm not sure if some people, Scholze for example, would like to endow a professorship in 'their name'.

9. **Peter Woit**
October 18, 2016

anon,
Yes, a better idea would be for the winner to choose the name of the chair, perhaps with a default suggestion that the name would become that of the winner at their retirement. It would also be fine to just name these things "Breakthrough Prize" or "Milner-Zuckerberg" chairs. The suggestion of using the winner's name is just that the main idea behind these prizes is to get more public recognition of the prize winners.

10. **Richard**
October 18, 2016

P.S. Thanks much for the link to the article by Ivan the Arithmetical. (I can hardly wait for the English translation.*)

* A remark that should * not * be taken as a dig at Fesenko: rather, it is my way of expressing my own bewilderment in the face of ideas whose depth, subtlety, and complexity will likely render them impregnable to any and all efforts on my part to understand them!

11. **Tom Andersen**
October 18, 2016

Peter,

I am not sure that doing anything more than making the endowment an option would work. Not all universities are run by honest people. The winner may choose to do something better with the money, like a school or charity in another country.

12. **Anon**
October 18, 2016

I don't know if this is off-topic. I wonder what the intended audience of Inference magazine is. As someone who is moderately educated in math, I find the article by Ivan Fesenko very hard to follow when he starts talking about class field theory. Likewise, another article in the same issue about linguistics is almost incomprehensible to me as a lay person to the field. I suppose the magazine is not for a "broad audience"?

13. **jsm**
October 19, 2016

Michael Weiss: Your story of Tate and quadratic reciprocity reminds me of another. Tate learned about the K2 of fields from Hyman Bass when both were in Paris in Fall 1968. Bass asked Tate if he could compute K2(Q). According to legend, Tate was working on the question in the presence of Bass late one night when what he was doing reminded him of something in one of Gauss's proofs of quadratic reciprocity. The two went out and found a copy of Gauss's Disquisitiones in a second-hand book store, and sure enough Tate was able to find in Gauss's work what he needed to complete the calculation.

14. **Jim Given**
October 19, 2016

I am an educated non-specialist in number theory (a physicist). Fesenko's paper claims that IUT "settles the abc-conjecture". Is Fesenko claiming that in fact Mochizuki has a proof of abc? Or is he just serving as a reporter and repeating Mochizuki's claim? I believe that the consensus at present is that one should not claim that Mochizuki has a proof. Is that wrong?

15. **G. S.**
October 19, 2016

I like the idea of the endowed chair, but I think arguments would erupt over where that endowed chair should be. Would the endowed chair be at the university where the recipient of the award currently resides, or at the university where the recipient did the bulk of his/her award-winning work?

I'm not sure how things tend to go in math, but in physics those are often not the same.

16. **Peter Woit**
October 19, 2016

Jim Given,
The current consensus among experts is that Mochizuki has not yet produced an acceptable proof, since no one seems to be able to explain his proof to others in a convincing way (Fesenko's article for example does not do this). Many experts tell me they think it is plausible that Mochizuki does have a proof, but the way he has written it up means experts cannot follow it or check it. Things seem to be at a standstill, with progress possible only if Mochizuki writes a less problematic version of the proof that others can follow, or someone else manages to see their way through what he has written and explain it to others.

17. **Peter Woit**
October 19, 2016

G.S.,
I think questions about the details of the chair could be left up to the winner. The important point would be that the funds in the long term support mathematics research by endowing support for a permanent position for a mathematics researcher, rather than ending up as personal assets of the winner.

This would most affect winners with financial problems who need the money. But winning the prize would automatically increase their value in the job market and likely the salary they could command. If the terms of the endowed position are up to them, this would also put them in a very strong negotiating position with whatever institution they work for, or any interested in hiring them (they could bring the endowment with them).

18. **ronab**
October 20, 2016

So would it be ok for them to occupy their own endowed chair?

19. **Peter Woit**
October 20, 2016

ronab,
Sure, that might even be the default arrangement.

20. **PedroJVM**
October 21, 2016

Don't quit yet on arithmetic algebraic geometric. My minimal suggestion is: start (and keep on with) with Silverman+Tate's "Rational Points on Elliptic Curves", and whenever you feel bored switch to Hindry+Silverman's "Diophantine Geometry: An Introduction". (Checkout Silverman's W^3 pages on both books.) There are many other options but my suggestion was minimal.

21. **Chris Austin**
October 21, 2016

Peter, if the Breakthrough Prize Foundation informed you that in recognition of some outstanding results you had achieved, they wanted to award a large prize *to someone else* in your honor, wouldn't you feel that a nasty joke was being played on you?

22. **Peter Woit**
October 21, 2016

Chris Austin,
Exactly my point is that such a prize should not go to a person, but to an institution for support of mathematical research. At this point, at least in the US, I think the best way to do that would be to endow permanent positions at research universities. If the Breakthrough Prize people want to endow such a position somewhere in my name, that would be great. I'd even consider dressing up in a monkey suit and attending their ceremony (by the way, I'm free Dec. 4 and was even thinking about a trip to the Bay Area around then, so they just have to let me know where and when to show up).

Better though would be if they endow the position here at Columbia, because then the administration and my colleagues would treat me even better than they do now. I might finally get a new carpet and paint job for my office…

23. **Peter Woit**
October 22, 2016

PedroJVM,
Thanks! That looks like excellent advice.

A New 30 GeV Particle?

October 21, 2016
Categories: Experimental HEP News

Last night a preprint appeared on the arXiv, with a re-analysis of old 1992-5 LEP data, looking at the dimuon spectrum for b-tagged (identified as involving a b-quark) events. An excess around 30 GeV was found, which would indicate a possible new particle around that energy. The author quotes various significance numbers for the bump, with look-elsewhere effect included, of 2.4 to 2.9 sigma.

Thinking a bit about the look-elsewhere effect here, something very funny is going on. To properly compute the look-elsewhere effect, one really should know how many other channels the author looked at and found nothing, but there's no mention of looking at other channels. Why did this particular physicist decide to go and reanalyze LEP data, looking only at the b-tagged dimuon spectrum (and it seems he's doing this by himself)? It's hard to understand why anyone would do this, unless perhaps they had heard that one of the LHC experiments might be seeing something in the b-tagged dimuon spectrum, say, around 30 GeV.

We'll likely find out more about this story soon. If the LHC experiments haven't been looking closely at this particular channel, they will do so now. 30 GeV is low enough that I don't see why you would need the Run 2 13 TeV data, this should be in the older Run 1 data.

I should make the obvious remark though: this is an extraordinary claim, and the evidence for a new particle is very far from the extraordinary level. So, at a high confidence level, the probability is that there's nothing there.

For much more about this, Tommaso Dorigo and Matt Strassler have just put out blog postings.

Update: Tommaso has an update with more about this: the author was not a member of ALEPH and that collboration does not support this but thinks this is bogus. It appears that the signal is spurious, with the muons coming from semileptonic b decays, not a new particle. Still a mystery: why was this physicist looking at this old data for one very specific signal?

Update: The talk today by Nate Odell of CMS at the LPC Physics Forum at Fermilab is not public, but the title is: "Dimuon 29 GeV analysis". Any guess whether that has something to do with this story about 30 GeV dimuons?

Comments

1. **tommaso dorigo**
   October 21, 2016

   Hello Peter,

   nice post. I am thinking that even CDF and DZERO (leave alone DELPHI and OPAL) could look for something similar in their own oldie but goldie data. In fact, this whole business of looking for signals of anomalous particles in LEP data reminds me of the sbottom quark search of CDF, which returned an excess of multi-lepton dijet events, and the summer 2000 crisis. The signature of dijet events with leptons was sought by ALEPH, who initially found a 3-sigma signal, and then by the other LEP experiments, that found nothing. Will history repeat itself ?

   On a second though I should not derate the wannabe signal, and rather put out a bet on this one signal too – it looks like a nice way to earn my daily bread.

   Best,
   T.

2. **Balazs Vagvolgyi**
   October 21, 2016

   I don't get it. Why did scientists come up with 5-sigma, if they ignore it all the time.
   Why even bother publishing anything under 5-sigma, when it's not up to the standards of modern physics?

   Some say 3-sigma still means that there is a 99.7% chance that it's not a fluke, but I disagree. Look elsewhere should be applied not only within individual experiments but in the set of all the experiments being performed:
   Let's say physicists in the world perform 1000 experiments a day. If all their data is just random noise, then there will still be 3 experiments every day that has a 3-sigma signal.
   To me, that just means that 3-sigma results are worthless.

3. **Peter Woit**
   October 21, 2016

   Balazs Vagvolgyi,

   As I wrote in the posting, something funny is going on here and I think it's very likely there's more to this than meets the eye. I doubt the author of this preprint randomly picked this analysis of old data to do, finding and publishing a 3 sigma effect. A possible explanation is that ATLAS or CMS has an unreleased analysis of b-tagged dimuons with a bump around 30 GeV, with a large enough significance to get people excited (e.g. 4 sigma or more). Put such an ATLAS or CMS result together with this new one and by some measure you have 5 sigma (and, a lot of ATLAS or CMS people pretty annoyed at this guy…).

4. **anon**
   October 21, 2016

   The author seems to be, or at least has been, a member of LHCb collaboration (at least his name is one of the 700 or so on their author lists). But going rogue would seem like sure way to leave the collaboration…

5. **Peter Woit**
   October 21, 2016

   anon,
   Yes, I think we can be sure that inspiration didn't come from unreleased LHCb results. It looks like this guy used to be in CMS, for what that's worth.

6. **hronir**
October 21, 2016

What’s the timescale (knowing how things go in collaborations common practice) for any (official) statement from ATLAS/CMS about it? I mean, roughly in… days? weeks? more?

7. **Peter Woit**
October 21, 2016

hronir,
If ATLAS/CMS haven’t already done this analysis, I’d guess it would take them a couple months to get it done and internally vet the results. If they already have something they’re working on, depends how far along that is. These people really don’t like to release results until they’re very sure about them.

This year’s pp run is ending right around now. Results from the full dataset for this year’s results I’d assume will start to appear early next year, at the usual winter HEP conferences.

8. **anonymous**
October 21, 2016

Perhaps you want to take it in a slightly different way. As this guy released something like this, ATLAS or CMS might have some “pressure” to move ahead. This might be good for the guys working on the unreleased analysis. It will be interesting. As a side remark, why can’t he study the “bump” around 24-25 GeV, too. Following similar analysis to extract significance, he might have some sigmas in that region as around 30 GeV. Instead of one, he could have two or more.

9. **Dale C.**
October 22, 2016

So I guess shortly we are going to see a lot of preprints describing a theoretical model containing a 30 GeV weakly interacting “dark sector” particle, as in the 750 GeV diphoton excess case.

10. **katzeee**
October 22, 2016

I would not leave out the possibility of some kind of hoax: After the 750-GeV-Experience, some experimentalists decide to get people excited by reexamining old data and finding sth. around 3 sigma. Most of the known hep-bloggers get excited too (or at least spread the news) and speculate if there is sth. in the LHC-data to be seen. Things get on and theorists try to explain these mysterious bump by dozens of models in various preprints… Then the experimatalists share a good laugh.

11. **Harry Dale Huffman**
October 22, 2016

An (over-)ambitious overachiever, trying to break out? As “katzee” noted just above, “after the 750Gev experience”, hope springs eternal in some. ‘Twas ever thus. (The term used in criminal investigations comes to mind: A copycat crime. Or that well-known phenomenon, of strange occurrences happening in bunches.)

12. **Dale C.**
October 22, 2016

According to Tommaso, the signal is clearly spurious, as the muons are collinear with the b-jets emitted in the Z decay. Waiting for the next fluke…

13. **Low Math, Meekly Interacting**
October 22, 2016

I’ve tended to favor “Hanlon’s Razor” in most circumstances, but too often lately I’m finding out instead I’ve been terribly naive. Could katzeee be right?

14. **Verissimo**
October 22, 2016

Poor Jester got so fried from the 750GeV hype that he has no take on this

15. **Peter Woit**
October 22, 2016

Verissimo,
He may just be showing his good judgment by ignoring it…

16. **Nick M.**
October 23, 2016

@Peter,

Ignoring it indeed! I submitted a comment, on Jester’s blog, asking if anyone there had any thoughts on this possible 30 GeV particle. He didn’t even bother to post it….
😉 I guess he’s done getting excited over these 3-sigma, gaussian looking, bumps that have the word “fluke” written all over them!

17. **NotPhycsist**
October 24, 2016

>”Some say 3-sigma still means that there is a 99.7% chance that it’s not a fluke”

I saw a lot of this surrounding the LIGO results. No, this is wrong in the most insidious way. The error you are making is called the “fallacy of the transposed conditional”: http://rationalwiki.org/wiki/Confusion_of_the_inverse

In short, the probably of observing something assuming the model is correct does not equal the probability the model is correct given the observations. The sigma value refers to the former, you want it to refer to the latter but it does not. In this case, “the model” refers to whatever background only, no signal, “chance” model is being tested.

18. **Balazs Vagvolgyi**

October 24, 2016

@ NotPhycsist

> The error you are making is called the “fallacy of the transposed conditional”

I don’t agree with it, that’s why I wrote “Some say”.
Thanks to you, now I know the name of this fallacy.

19. **NotPhycsist**
October 24, 2016

@ Balazs Vagvolgyi

Thanks for clarifying. It is just an additional problem on top of what you and Peter identify as the “look elsewhere effect”. Btw, I think that problem can be better understood as using an incorrect model for the background. The correct model would automatically account for the actual researcher behavior.

I know it is common practice across many fields to do post-hoc “adjustments” for multiple comparisons, but to me that is a misleading way to conceptualize the problem. The “adjustments” should be baked into the null model to begin with, so that it is as correct as possible.

Another way of putting it is that the “bump” is real. The model being tested is known to be wrong though, it apparently assumes the researcher only looked at 10 sets of data, or whatever, that is less than reality. Rejecting this null model would be the right thing for the statistical machinery to do, but is rather pointless scientifically.

20. **RandomPaddy**
October 27, 2016

I don’t think all these swirling rumors do much for the credibility of particle physics.

A lot of this could be completely avoided by just putting the data up on a publicly accessible server. It’s 2016 and these experiments involve more people than the average aircraft carrier. Just release the data and stop all these bump in the data murder mysteries .

21. **JD**
October 28, 2016

RandomPaddy

Go to Tommaso Dorigo’s blog to clarify your thinking. I agree with him that the data should be out there for general use, and apparantly it is. But that will not solve the problem, obviously. The problem lies in the culture of todays science.

22. **Peter Woit**
October 28, 2016

JD,

You’re right that this has nothing to do with public availability of data (except to show problems that can occur when doing this). I don’t think though it has to do with the culture of today’s science, rather, this is a quite unusual situation, where a single physicist, for reasons that it seems he’s not revealing and that would be interesting to know, has decided to go back and look for something very specific in old data from another experiment.

23. **Shantanu**
October 29, 2016

Peter and others ,
I slightly disagree with this. I think HEP experiments should make their data public
and individual folks should be encouraged to analyze this data, not just the collaborations. In fact in astronomy it is mandatary to make all data public and many people outside the collaboration have analyzed the data and written good papers.
But for some reason this is not the norm in HEP. So its heartening to see individual
folks analyzing experimental HEP data.

24. **Peter Woit**
November 3, 2016

I’ve added an update noting the title of a talk today (Nov. 3) by someone from CMS…

Final Draft Version

October 24, 2016
Categories: Quantum Theory: The Book

I finally have finished a draft version of the book that I've been working on for the past four years or so. This version will remain freely available on my website here. The plan is to get professional illustrations done and have the book published by Springer, presumably appearing in print sometime next year. By now it's too late for any significant changes, but comments, especially corrections and typos, are welcome.

At this point I'm very happy with how the book has turned out, since I think it provides a valuable point of view on the relation between quantum mechanics and mathematics, and contains significant amounts of material not well-explained elsewhere. I'm simultaneously rather unhappy with it, very much aware of a long list of ways in which it could be improved. Any of these though would require putting more time into the project, and right now I'm thoroughly sick of it, desperately wanting to think about other things. So, this is pretty much it.

I've learned a huge amount by writing this, and I hope to apply some of this in work on several different new projects. As I work on these, perhaps I'll do some more writing that would partially take the form of new chapters extending what's in the book. We'll see…

Comments

1. **Anonymous Pedant**
   October 25, 2016

   "on the the behavior" page 141

2. **mikemm**
   October 25, 2016

   congratulations! I've been quietly following this as you've worked on it over the last few years and am delighted it's now 'complete enough' and you can focus on other things. Also, thanks for making it available before it's hidden behind a paywall. I'm looking forward to having the time to get into it..

3. **Shantanu**
   October 25, 2016

   Peter, congrats for finishing the book

4. **Jim Holt**
   October 25, 2016

   Let me add my own congratulations–and also hearty thanks, since I've learned so much from working through the early chapters of your draft. You've managed to convey the mathematical structure of quantum mechanics in a way that's refreshingly elegant and clear.

5. **another anonymous pedant**
   October 25, 2016

   page 547, "include a a connection"

6. **Harry Dale Huffman**
   October 25, 2016

   I downloaded the file and skimmed through sections of it, and seen that it is very much mathematics, while I am very much into physics now. The only thing that jumps out at me is the so-far hand-drawn illustrations and your stated desire to "get professional illustrations done"; having authored my own book, with well over 200 color illustrations I designed and made myself, I would say the illustrations in your book are not hard to do yourself, using only the "Paint" program in Windows for example, and you could take real pride in doing so, and also have no one else to blame if you are unhappy with the "professional" versions (choose your own fonts and line thicknesses, for example, and extend axes to your liking). (Most of my illustrations came from the interactive graphical software I developed myself, about 18 years ago, to do my unprecedented research — into what turned out to be the single objective origin of all of the world's "ancient mysteries" — and so were churned out "ready-made"–with that software cookie cutter, as it were–but a fair number were done from scratch, or just finished, using Windows Paint, as yours could be.) I am my own "professional", or craftsman, in every aspect of my book, and the result is complete satisfaction.

7. **Lino D'Ischia**
   October 25, 2016

   Peter: What an accomplishment! Congratulations. And give that mind of yours a little time off!

8. **Eric Weinstein**
   October 25, 2016

   While I haven't read the whole book, I've read parts of earlier drafts quite closely. I've been amazed at how much there is to recommend this contribution. In many texts one reads the same lines (almost verbatim) that one has read in earlier works. This means that if the original phraseology confused you, you have the illusion that you can't learn the material because the same phraseology occurs repeatedly and seemingly independently.

   As a famous example of this kind of phenomena, many years ago Atiyah and Singer had to explain what an anomaly was mathematically to those of us geometry minded folks who had swallowed the mystical physicists description "An anomaly is what prevents a classical symmetry from surviving quantization." Many physicists claimed this work of A&S was "nothing new", but it showed me in an instant how important a change in pedagogy can be to those of us who cannot understand the standard descriptions.

   This book excels at this most important task in my opinion. People may claim that it breaks no new ground, but this will be incorrect. It is a pedagogical innovation to be able to explain things from a new viewpoint so that those who were mystified by the original descriptions can find a new path to understanding. This book has repaid close reading every time I have invested in resolving a long-standing confusion.

   Well done.

9. **Peter Woit**
October 25, 2016

Thanks to all for the kind words. Especially to Eric, since he's praising what I would like to think is the best feature of the book, not different results, but a somewhat different viewpoint than the usual one. While teaching the course, I did try and rethink for myself the subject from the ground up rather than following closely other texts.

Anonymous pedants,
Thanks!!

10. **amz review**
October 25, 2016

Technical comment about harmonic oscillator: "If we start off with a state |0> that is a non-zero eigenvector of N with eigenvalue 0, we see that the eigenvalues of N will be the non-negative integers, etc"

Actually one must argue that this MUST be the case (see Dirac book): if we start off with an eigenstate |c> with fractional eigenvalue c, then by repeated applications of the annihilation operator a we obtain a sequence of eigenstates of N with eigenvalues c-1, c-2, … which will eventually take negative values. This is impossible (as proved earlier in the text), hence the sequence MUST terminate with a state "x" such that a|x> = 0. It follows that the eigenvalue of N is zero for this state, and we may label the state |0>. Then by repeated applications of the creation operator, we derive that the eigenvalues of N are the non-negative integers.

11. **Another anonymous pedant**
October 25, 2016

Page 15 "…2 by two matrices…"

12. **Peter Woit**
October 25, 2016

amz review,
An earlier version had that argument, but I decided it best to leave it out. The problem is that if you try and make general statements about ALL representations of the algebra of annihilation and creation operators you have to worry about various possible pathologies, whether operators are defined, etc. In general the philosophy of the book is to avoid trying to prove general theorems about what MUST happen, since getting those exactly right typically requires making explicit a complicated set of assumptions. Better to not get involved if it's not something I need for what I want to explain.

13. **John McAllison**
October 25, 2016

It looks great; I like the way it starts at an elementary level, gradually increasing in difficulty. The important thing now is to not worry too much about it being perfect, but that the number of mistakes are minimized, laying the foundations for a later second edition if successful.

It has the potential to be a classic like 'Tensor Geometry: The Geometric Viewpoint and its Uses' which all the reviews rave about it laying the foundations clearly:
https://www.amazon.com/Tensor-Geometry-Geometric-Viewpoint-Mathematics/dp/354052018X

14. **struwwel**
October 25, 2016

You write (page 100) that CP^1 is C^2 quotiented out by C^*. Actually it is C^2 minus the origin which must be quotiented out by C^* : no point in CP^1 has homogeneous coordinates (0:0).

15. **Sam Dolan**
October 25, 2016

Congratulations, and thank you for making this final draft freely available.

In the 2nd ed., perhaps Chap. 41 could also touch upon the (less well-known) isomorphism between SO+(3,1) [the standard matrix representation of the orthochronous proper Lorentz group] and SO(3,C) [the group of 3×3 complex orthogonal matrices with unit determinant]. The latter representation is convenient for (e.g.) transforming the Faraday tensor F between orthonormal frames, or transforming the Weyl tensor W.

[It goes something like this: As F is a bivector, it can be expressed as a complex 3-vector, and as the Riemann tensor R is (in some sense) the symmetric product of two bivectors, it can be expressed as a complex 3×3 matrix. The Weyl tensor W is a symmetric and trace-free 3×3 complex matrix. Under Lorentz transformations, F' = A F and W' = A W A^t, where A is the appropriate matrix in SO(3,C)].

16. **Peter Woit**
October 26, 2016

struwwel,
Thanks. Fixed.

17. **Richard**
October 26, 2016

Shouldn't the expression "Planck's constant" show up somewhere in Section 1.2.1?

18. **Peter Woit**
October 26, 2016

Richard,
A good point. I seem to have discussed this constant without ever using its name, will fix that.

19. **regex**
October 26, 2016

The topological reason for this this is that
The relation between between the quadratic

angular momentum of an object spinning about about some axis

20. **regex**
October 26, 2016

If you have a Knuth style $1 per typo policy, I'll write some more regex and find others 😉

21. **Peter Woit**
October 26, 2016

regex,
Thanks! This inspired me to run a full check for repeated words on my .tex file and found a couple more. Fixed.

No, no bounty for typos, I seem to be doing very well getting them found for free!

On the other hand, if people can find sign errors in equations, I might think about it. Current plan is to check all the equations one last time once this thing is in proofs, and I'm not looking forward to that…

22. **Narad**
October 27, 2016

If Springer doesn't have any TeX-savvy manuscript editors, I'm always looking for freelance work (just looking at chapter 1, that $\langle\cdot ,\cdot\rangle$ construction could use some touching up, and just above section 1.3.3, "i.e. " should be "i.e.,\ " [or "i.e.\ " if you insist] along with a comma after the equation). Whether they outsource, I don't know.

23. **Narad**
October 27, 2016

^ Whoops, "i.e., ". Probably not the best advertisement.

24. **Narad**
October 27, 2016

Anyway, "Choosing local coordinates on G, π will given by" (between the definition of a representation and an irreducible representation) doesn't really read right. I'm also unclear on the meaning of the subscript $|W$ introduced just below.

25. **Mateusz Wasilewski**
October 28, 2016

Near the end of page 325 it should be "One thus sees that on C^n, as in the symplectic case, up to change of basis there is only one non-degenerate **symmetric** bilinear form.".

26. **Peter Woit**
October 28, 2016

Mateusz,
Thanks! Fixed.

27. **Anonymous**
October 29, 2016

The letter font for some of the SU(, SO(and Spin(like in the title of 21.2 or the last paragraph of page 246 is different from the rest. Is that correct?

28. **Narad**
November 1, 2016

> The letter font for some of the SU(, SO(and Spin(like in the title of 21.2 or the last paragraph of page 246 is different from the rest. Is that correct?

@Anonymous: Fraktur denotes the algebra, italics the group.

The $\frac{1}{2}$ instances on page 246 "should be" \tfrac, but I'm doing my level best to STFU about such things.

29. **Narad**
November 1, 2016

But, seriously, don't do, e.g., $\dot{p_j}$. Springer's never going to catch the horizontal misplacement (even directly compared with $\dot{q_j}$). Use ${\dot{p}_j}$, etc.

30. **Johan**
November 18, 2016

Congratulations on finishing the book! Just a small comment: I was surprised not to see a mention of or reference to Jean-Marie Souriau, who was (pretty much simultaneously with Kostant) the first to introduce the general notion of momentum map as we now know it, as well as introducing geometric quantization (again, independently but essentially simultaneously with Kostant). His book "Structure des systèmes dynamiques" (published in English as "Structure of Dynamical Systems – A Symplectic View of Physics" by Birkhauser in '97) is still a worthy read.

31. **Peter Woit**
November 18, 2016

Johan,

Thanks! Souriau was one of the places I first started learning about symplectic geometry. In general my references are just a selection of expository things I happen to have looked at in recent years. I try not to get into references related to who originated ideas, since that gets very complicated very fast. The Kostant reference is an anomaly.

Various Links

October 27, 2016
Categories: Uncategorized

- The 2016 LHC proton-proton run is now over, with delivered (41.07 CMS/38.4 ATLAS) and recorded (37.82 CMS/35.5 ATLAS) luminosities (in inverse fb) far above the goal for this year of 25. Together with last year's data, the experiments now have 41.63 (CMS) and 39.4 (ATLAS) inverse fb recorded at 13 TeV, close to the LHC design energy of 14 TeV. It is likely that preliminary results will be reported at an "end-of-year jamboree" in mid-December, with more to come at the winter conferences.

  I'd guess that these new results will see improved bounds on SUSY particles, and that David Gross and Lubos Motl will have to pay off their long-standing bets that the LHC would find SUSY (Gross's bet with Ken Lane is here, it says 50 inverse fb of LHC data, sum of CMS and ATLAS now about 80). Unfortunately, I'm afraid that losing these bets won't affect their devotion to SUSY.

- Paul Steinhardt gave a colloquium at Fermilab last month with the title Simply Wrong vs. Simple. In it he explained "why the big bang inflationary picture fails as a scientific theory" (it doesn't work as promised, is not self-consistent and not falsifiable). This is a complicated topic, but Steinhardt is an expert and one of the originators of the theory, so if you want to understand the problems of some common arguments for inflation, watching this talk is highly recommended. Steinhardt's talk was part of a Fermilab workshop, Simplicity II.
- On the multiverse front, Sabine Hossenfelder's Mom has Sabine to set her straight. For professional physicists, instead of getting set straight there's the usual Templeton funding for the opposite, in this case a workshop on Fine-tuning, the Multiverse and Life.
- Paul Ginsparg discusses various issues having to do with the arXiv here and here, with an emphasis on the question of how to decide which preprints to reject (they have my sympathy on the difficulties involved). Ginsparg notes that they decided not to have comments/discussion of papers there, but to have "trackbacks" to discussions hosted elsewhere. Still no indication of why trackbacks here are banned.

- Theoretical physicist Walter Greiner passed away a couple weeks ago. He was the author of a series of textbooks, one of which in particular, Field Quantization, I found very helpful when I was trying to figure out some details for the book I was writing.

Update: I just noticed that Witten's Commemorative Lecture for the Kyoto Prize is available here. It's a very interesting account by him of his career and point of view.

Update: In case you think fine-tuning is a central question in physics, besides the Templeton-funded workshop in Sydney, you can consult the website of a Templeton-funded program, or buy this book by a Templeton-funded author. There's also a talk by Aron Wall, given to a Rutgers University apologetics club. Wall's conclusion is that either God or the Multiverse did it, and he comes down on the side of God (because of the Resurrection of Jesus business).

Update: Also on the Templeton front, they are funding a new $7.2 million Black Hole Initiative, which advertises itself by

> The BHI will be the first center worldwide to focus on the study of black holes, and as such it offers a unique naming opportunity for potential donors.

Templeton is paying for the first three years of this. To get some idea of the scale of this project, the yearly grant is roughly half the size of the NSF grant to each to the two largest US centers in pure math and in theoretical physics (MSRI and the KITP).

Comments

1. **Anonymous**
   October 28, 2016

   Interesting quote: "Each problem here involved applying physics ideas to a problem that traditionally would have been viewed
   as a math problem, not a physics problem. These were all problems that I would not have seriously considered
   working on until String Theory broadened our horizons concerning the relations between mathematics and physics"

   In particular because he talks about his paper on QFT and the Jones polynomial.

2. **Bee**
   October 28, 2016

   The field quantization textbook was mostly written by Joachim Reinhardt. Who, sadly, enough passed away a week before Greiner. And thanks for the link. My mom appreciates it 😉

3. **curious**
   October 28, 2016

   Together with last year's data, the experiments now have 41.63 (CMS) and 39.4 (ATLAS) inverse fb recorded at 13 TeV, close to the LHC design energy of 14 TeV. It is likely that preliminary results will be reported at an "end-of-year jamboree" in mid-December, with more to come at the winter conferences.
   ^

   if LHC sees no evidence of SUSY after recording and analyzing that many inverse fb recorded at 13 TeV, and possibly reported by dec- 2016, what are the probability LHC will ever see SUSY over its entire lifetime i.e after it collects 3000 inverse fb? with 5-sigma being discovery?

4. **Peter Woit**
   October 28, 2016

   curious,
   There's no well-defined answer to that, since there is no sensible notion of probability distribution for different susy parameters. One can say thought that by the standard "naturalness" arguments, SUSY was supposed to show up at the (masses 100-200 GeV), and definitely early on at the full energy LHC (masses around 1 TeV). That's what David Gross was willing to bet money on. I think we'll soon see gluino mass bounds above 2 TeV. Much later on the LHC may be able to get bounds up to say 3 TeV. I'm pretty sure though that Gross would turn down an offer of a "double or nothing" bet that superpartners will show up later on at the LHC.

5. **Peter Woit**
   October 28, 2016

   Anonymous,
   One thing about the Witten piece is that he is rather defensive about string theory (very explicitly in the last part), not surprising given how things have not worked out.

The comment you quote is interesting in that he credits string theory more with causing a change in point of view and change in culture that led to some of his great work. It's certainly accurate that string theory and the way Witten pursued it with a lot of mathematical firepower changed a lot how physicists looked at mathematics (I think there has however been a backlash more recently, as many physicists blame mathematics for the failures of the research program).

But, ignoring that, I think his comments about the relation of math and physics in his work (as well as the role of string theory) are really fascinating. One thing that has always struck me about him is that, even when he was revolutionizing some fields of mathematics, it always seemed that he was leery of getting too much involved with the mathematics, wanted to stay grounded in physics. My own reaction to some of his great work was quite different, as it led me to want to learn more about the mathematical context in order to better understand how mathematics and physics were interacting.

On the one hand I think he has always been aware that his strong suit is that he can do things mathematicians can't, by exploiting what physicists know. It's also true that he is driven ultimately by a deep motivation to solve certain problems in physics, which is quite different than the motivations of mathematicians. He mentions explicitly something usually not mentioned, that from the beginning of his career, a deep motivation has been that of trying to solve QCD, and because of this, trying to better understand non-perturbative QFT. In particular striking to me was his comment:
"I am not sure that there is any such thing as being an expert on how quantum systems behave for strong coupling, and in any event certainly I myself never have become such an expert. I have learned quite a bit while always feeling like a beginner."

6. **milkshaken**
   October 28, 2016

   I really enjoyed Paul Steinhardt lecture – accessible to nontechnical audience (like me) – and I admire his plain-speaking courage (given that he is one of the founders of inflationary cosmology).

7. **Miki Weiss**
   October 29, 2016

   Thanks for the reference to Aron Wall lecture. He shows the logical absurds in the Anthropical principle and the String Landscape with the shortest and most elegant reasoning.
   Why the Anthropic Principle is absurd? – see slide 38.
   Why the String Landscape is absurd? – see slide 41.
   But he still gives the Inflation Multiverse the benefit of doubt, if inflation is proven by experiment to be correct….

8. **jonW**
   October 30, 2016

   I thought Steinhardt's talk was excellent, too, and I find it a very commonsensical approach to really examine the degrees of freedom available to inflationary theories and what this means for their notability as predictions. Just for the sake of balance, though, does anyone know of a paper or lecture (pitched at a similar level) that gives as strong a rebuttal as possible to Steinhardt's criticisms? I know those criticisms aren't exactly new.

9. **Ben**
   October 31, 2016

   I think many people are aware of Steinhardt's criticisms, which mostly boil down to the fact that inflation doesn't really solve problems so much as kick them down the road a bit. Instead of flatness/horizon problems, you now have questions about initial conditions or bubble nucleation in eternal inflation, etc.

   But to the extent that developed alternatives even exist, they don't seem much better. Ekpyrotic/bounce scenarios really just shift the question to "how do you propagate density perturbations through the bounce". And since the methods used to do so have to violate something or other (usually the NEC), there's skepticism that a clever theorist will eventually figure out how to get any prediction you want out of that model, too.

   So it's not so much that people rebut Steinhardt or don't believe him, it's just that nobody knows what else to do. I will say that I think that many observational people will start eyeing inflation with a lot more skepticism if B-modes don't start showing up soon (i.e. if limits get down below 10^-3).

10. **Scott Church**
    October 31, 2016

    Peter,

    I know Aron Wall, and I can tell you that between God and the multiverse he does not consider God to be the better answer to fine tuning because of "the Resurrection of Jesus business." What he does believe is that given the current state of cosmology and QFT, *demonstrated* physics + God is a more robust and less ad-hoc explanation than that offered by the inflation/string theory multiverse + Materialism… a conclusion I agree with. His reasons for thinking so can be found in more detail at his blog. He mentioned the resurrection in that talk only to to point out that because he *already* has other reasons to believe in God including that one, the former is a viable option for him whether science progresses further on the matter or not. As such, he can objectively weigh both alternatives because he has no prior commitment to either outcome, whereas, materialists who wish to claim scientific authority are stuck with the latter whether they like it or not , and have little choice but to go to whatever lengths are necessary to make it work… up to, and including dispensing with the need for testable predictions in science. For more on all this, see Aron's post about that talk and the resulting comment thread, pro and con.

    For better or worse, physics has brought us to a place where we're knocking on the door of ultimate origins where the delineations between it and metaphysics are starting to blur, and it's becoming increasingly important to understand how the two relate to each other. To date many, if not most scientists have been reluctant to do so and now the chickens are coming home to roost. Thirty years ago it was commonplace to see philosophers and theologians addressing scientific topics with little or no training in the relevant fields (case in point, creationism). Today we're starting to see the reverse as well… physicists practicing metaphysics while professing themselves to be above doing so, and as such doing so unconsciously, and therefore *badly*. Aron, who is well-trained in both, is one of the more notable exceptions to that disturbing trend.

11. **Peter Woit**
    October 31, 2016

    Scott Church,
    I wasn't pretending to do justice to Aron Wall's views with that off-hand remark, thanks for giving a more detailed explanation of his views . My views on all this are tediously simple: "God did it" and "the Multiverse did it" are equally vacuous non-explanations and those who want to debate them are doing something I'm not interested in and that I don't think qualifies as science under even a generous interpretation of the term. And I think most physicists agree.

12. **Lars**
    November 1, 2016

    "God did the multiverse"

God did the multiverse
And string theory too
Cuz after the universe
He'd nothing to do

13. **Goose Gossage**
November 1, 2016

Thanks for the link to Fermilab, Peter. Paul Steinhardt's talk was exciting. The headline: there are now two types of bounces that have been worked out — one quantum and one classical. One involves a quantum violation of the null energy condition, and the other a stable classical violation. That's clearly a very big deal. For each, one can now compute what effect there is on the perturbations. Odds are there is no effect on the large scales that cosmologists measure — too early to say what that the prediction will be until a careful computation is done. But Steinhardt said there WILL be a definite prediction, unlike inflation which has turned out to be dependent on extremely unlikely initial conditions (as proven by Planck) and the multiverse, which produces every darn thing imaginable. It's no secret that Steinhardt's argument has long been that inflation makes no predictions (because you always wind up with a multiverse, which means it predicts….everything). At Fermilab, he was saying it's not right to compare a fundamentally unpredictive idea (inflation) with a scientific idea (a bounce) that WILL be able to make a prediction. I think that was the real point of his lecture.

14. **Thomas Lee Elifritz**
November 2, 2016

Avi Loeb somehow forgot to mention his John Templeton Foundation funding in his recent bit of 'Nature' tripe. My how far the mighty have fallen.

http://www.nature.com/news/good-data-are-not-enough-1.20906

I doubt my comment to this would pass moderation at Nature anymore.

15. **Shantanu**
November 4, 2016

Peter, sorry for the OT link, but one more
article by Avi Loeb for astrophysicists on why non-mainstream ideas
should be encouraged
http://www.nature.com/news/good-data-are-not-enough-1.20906

Vote!

October 30, 2016
Categories: Uncategorized

Normally I avoid politics here, but these are not normal times. What follows is a request to my US readers, followed by some general remarks about the disturbing state of US democracy. Sorry, but if you want to discuss any of this, it will have to be elsewhere (internet comment sections are part of the problem…).

To those not planning on voting for Donald Trump:

- If you're planning on voting for Hillary Clinton: please be sure to get out and vote, by early voting if available in your state or on Election day. This is extremely important, with the election likely decided by who cares enough to turn out and vote.
- If you're planning on not voting: please rethink this. One can in many elections make a reasonable case that the differences between the candidates aren't great, so, why bother? If there ever were a US election where that was not true, this is it.
- If you're planning on voting for a third-party candidate: again, that might make sense if the differences between the two viable candidates were not great, but that is absolutely not the case here. More specifically, if you're a progressive planning on voting for Jill Stein, please look at what happened in 2000. People who did the same thing in Florida (voting for Nader then) gave us George W. Bush as president, which was a disaster for the progressive cause. I also urge you to look deep within your heart and ask yourself whether your behavior is a realistic engagement with the world, or self-involved moral posturing.

To those planning on voting for Donald Trump:

Please don't. I see two main arguments for doing this and I think they're both misguided.

- You agree with Trump more than Clinton on important policy issues. Whatever policy issue you have in mind, I think if you look into it you'll find that whatever Trump says now, at some other point he was saying something different. There's little evidence Trump has fixed views on any policy issue (other than the desirability of better tax treatment for real estate development projects). If you think Trump will, for instance, appoint Supreme Court justices that share your moral values, note that he has reportedly told Peter Thiel that he would like to appoint him to the Court. Thiel is a gay, radical libertarian Silicon Valley billionaire from San Francisco with highly eccentric views. I doubt you share his moral values (since virtually no one else does, right, left or center).

  Note added: This same argument applies to those opposing Clinton and supporting Trump's election on grounds such as "she's a war-mongerer, unlike Trump", since (on some days) Trump claims to oppose US military interventions abroad. If you really believe that "Make America Great" means Trump will institute a policy of restraint on the use of the US military, I think it will likely be just a few weeks into the Trump administration before you find out that you, like your right-wing brethren in flyover country, have been conned.

- You're angry at well-off coastal elites who you feel look down on you and your culture, and you want to spit in their face by voting for Trump. If so, you are quite right to feel the way you do. From a lifetime spent among such elites I can tell you that, yes, they do look down on you. Most people here in New York City probably do think you're an ignorant racist. Your problem though is that Donald Trump is one of us. He's a well-off New Yorker through and through, looks down on you every bit as much as others. If elected he will govern in the interest of his tribe, not yours. If you think otherwise, you've been conned. All you will accomplish by a vote for Trump is to convince people in New York, Washington D.C. and California that you really are even more ignorant than they thought, a racist fool taken in by an obvious con.

How did we end up here?

Whatever happens, I think the huge question facing US democracy is that of how, in an election contest between a competent, honest centrist candidate and an unqualified con artist, we've ended up with the majority of the electorate convinced that the first of these is the one with serious ethical problems. American politics has become a reality TV show, with the plot line all about convincing people that a contestant is unethical and dislikable, and so should be voted off the island.

That the right has pursued this tactic against the Clintons since the early 90s is not surprising, since it's much more effective than arguing the issues. What's destroying US democracy though is not just one side's decision to do this, but that the other side, instead of fighting back, has been joining in. The most outrageous example of this is the Clinton email server "scandal", which is and always has been an absurdity. The attacks on Hillary Clinton's character based on this have not come just from the right, but also from the left. I every so often look at the Drudge Report, as well as lefty sources (The Intercept, Firedoglake=Shadowproof, Naked Capitalism, etc), and, on this topic, you can't tell the difference between right and left.

Most damaging though is the behavior of the mainstream media, in particular that of the New York Times, whose coverage of this issue has been atrociously unfair to Clinton. This is not new behavior for them, it goes back to the first Clinton administration, during which they promoted endless similar nonsense (Travelgate, Whitewater, etc., etc.). At the time I found it hard to understand why they were doing this, with one conjecture that it had to do with reporters and editors harboring some sort of resentment, intent on taking down the Clintons a notch ("they think they're so great, we'll show them, expose their dirty laundry").

More recently I've come to the conclusion that what's going on here has to do with the world-view of much of the liberal, educated class that I'm a part of. After some success at addressing ancient problems of racism, sexism, homophobia and the like, many have become impossibly self-righteous and devoted to moral posturing, intent on ferreting out reasons to "call out" others and show their moral superiority.

When political arguments are about issues, they're often not very rational and it has always been thus. What has changed in the US is that political arguments are now dominated by obsession with the supposed moral failings of others. We're experiencing a perfect storm of the demagoguery of the right meeting the obsessive self-righteousness of the left, all mediated by journalists who see their primary role as taking down and exposing the supposed moral failings of our leaders. I don't think we've seen this before in our history, and it threatens to upend the basic premises of a working democracy.

It's very hard to know what kind of craziness we'll have to deal with if Trump is elected. His agenda is purely that of getting attention for himself and "winning", I doubt even he has any idea what he'll do if he wins. On the other hand, unfortunately it's all too clear what's going to happen if Clinton is elected. The Republicans will launch endless investigations and attacks on her character in an attempt to make sure her presidency is not a success. The left will join in, and the New York Times and much of the rest of the media will nearly every day feature a new story about what is wrong with Hilary Clinton and with whoever joins her administration and attempts to govern the country in the best interest of its citizens. I don't see see how this fever breaks, and it's very hard not to see bleak days ahead for this country.

Update: From talking to various people I realized that one reason some Democrats and those on the left don't see the "top secret email" business as being as absurd as I do is that they have no idea what these emails were. For that story, see this from the Wall Street Journal and this from the Washington Post.

One reason many people may not be informed about this is that they get their news from the New York Times, where the only mention of this I can find is a snide remark in this story.

Update: Based on some emails from people misreading this posting, it seems I have to make the following clear

- I DON'T think Trump supporters are ignorant racists.
- I DON'T think Peter Thiel is immoral (eccentric ≠ bad).

I DO think that

- Left-wing news and commentary sites
- New York Times political reporters and editors, together with others at mainstream media outlets
- Jill Stein and people who support her candidacy

have joined the right in an unholy alliance to try and discredit Clinton, based on completely absurd accusations regarding her use of email. This has led to a serious danger that the US will elect a con artist and fascist demagogue as President next week, or, failing that, destroy hopes for a successful Hilary Clinton administration.

Update: A correspondent points out that I haven't addressed one reason some on both the Left and the Right (including those at the lefty websites I mentioned) are supporting Trump and trying to destroy Hilary Clinton. They believe that the advent of Trump will "break open the current oligarchy's Pandora's box", with the destruction of the US democratic system making way for a wonderful new system that will grow and flourish in the wreckage. I didn't mention this nihilist argument for Trump because there is no rational argument against the desire to deal with a problem by smashing everything around one. Sure, that will work, it will get rid of what is bothering you, you'll feel better and, as long as you're well enough off, other people will be the collateral damage hit by the debris, as well as those that have to clean up the mess.

Those who think this way though should at least be honest that that's what they want, and not hide behind dishonest demonization of Hilary Clinton over bogus accusations about her email. The left's joining with the Republican party to weaponize dishonest accusations about ethics and use those to bring down whoever they disagree with is something that will live on and make sure that what emerges from the wreckage will be even uglier than what was destroyed.

I also added something above, making clear that the arguments to Trump supporters also apply to his Leftist ones.

No Comments

The Day After

November 9, 2016
Categories: Favorite Old Posts, Uncategorized

The last few months have not been helpful for my sanity (and I think for that of a large number of other Americans). My three-point program to return to better mental health is to:

- Write one last blog post about what has been going on in the US (with comments off, again for mental health reasons).
- Stop thinking about this topic and stick to thinking and blogging about math and physics.
- Get out of this country for a while, heading to Paris the day after Thanksgiving and hiding out there for a week or so. While there, may do some research into what the current French policy is on political refugees.

On the question everyone is asking themselves (what will Trump do?) I have no idea and suspect he doesn't either. Many around him and in the Republican party do have definite ideas about this. Their agenda will be to use their party's unparalleled control of the US government at all levels, together with his executive power, to force changes dictated by their ideology. Either Trump will go along with this, or he'll change and come up with his own agenda going beyond just that of getting more attention for himself.

On the question of how we got here, I think my [previous blog post] stands up well given what data I've seen from the election returns. Most of the explanations one hears of Trump's success don't hold up if you look at exit polling numbers:

- Sexism: more white women voted for Trump than for Clinton.
- Racism: many counties that went solidly for Obama in the past went to Trump this election. Many Trump voters last voted for an African-American President.
- Revolt of the rural poor whites: While New York City went heavily to Clinton, nearby Suffolk County on Long Island, with a median family income of $100,000, went for Trump.
- Ignorance, lack of education: Most white college graduates voted for Trump.

There is however a common thread in most stories I've read that let Trump supporters say why they were voting for him: they hated Hillary Clinton and found her dishonest. Polls show that back in early September, when asked "who is more honest and trustworthy?" Clinton and Trump were tied (45/45). By the week before the election, the numbers were (38/45): eight percent more of likely voters thought Trump was honest than thought Clinton was. Whatever you think of Hillary Clinton, the idea that Donald Trump is more honest is quite simply insane. The central question of how we got to a Trump presidency is to understand how this destruction of Clinton's reputation was accomplished. What makes this tricky is that there was such a huge campaign from all sides to do this, including not just the usual Republican politics of personal destruction machine (Drudge Report, Breitbart, Fox News), but a wide array of actors on the center and the left, including:

- Julian Assange and Wikileaks, quite likely fronting for Russian secret services.
- All sorts of lefty news sources, too many examples to pick. Susan Webber who runs *Naked Capitalism* today is gleeful at the Trump win, and hopes that this is what we'll soon see:

  > There is one more Trump campaign promise that will serve as an important early test of his seriousness as well as his survival skills: investigating Clinton. Even if Obama pardons her, as our Jerri-Lynn Scofield has predicted, it will be critical for Trump to carry out a probe of the Clinton Foundation's business while Clinton was Secretary of State.

- The New York Times, which for months nearly every day published an above-the-fold front page news article attacking Clinton's ethics, with several reporters (Amy Chozick, for one) tasked to make it their full-time assignment to produce such stories. While their opinion writers were mostly more restrained, there's the bizarre case of Maureen Dowd who for 25 years has been writing literally hundreds of pieces about what she feels is ethically wrong with Hillary Clinton. On the Sunday before the election the Times ran not one but two pieces by Dowd attacking Clinton: a shorter one in the op-ed section, and a longer one in the Magazine section.

Together with the onslaught of right-wing "News" attacking Clinton's honesty, it is not surprising that more voters decided Trump was the honest one, or that this likely was enough to get him elected (with a minority of the popular vote).

One way to describe what has happened is that this was the first real social media election, with most people getting the information they used to decide who to vote for from Facebook and other internet sources. Many if not most of these have no interest in what is actually true. Many are dominated by a reality TV ethos of picking out someone for others to attack, appealing directly to the ugliest part of the monkey brain we are all descended from. This is not just the province of the Right, with the Left just as happy to join in the ugliness. Everyone can play and get satisfaction of their darkest needs. The winner will always be the con artist monkey with the best dominance displays.

Trump was always going to win an election campaign fought on these grounds, and that's what this one was. I have no idea of how to stop this from being the future of US democracy. If you're not from the US, maybe you can take action to ensure that your society does not follow ours down this path.

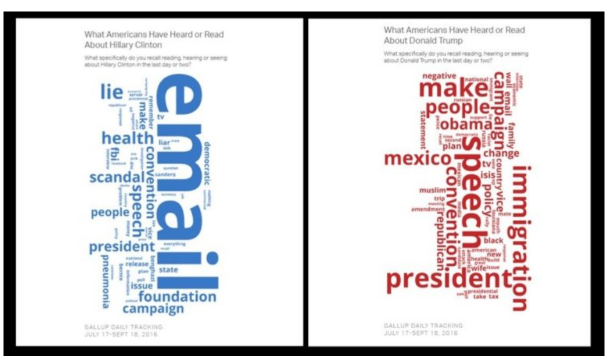

Source

Update: I'm not seeing many other analyses of what happened this election that I agree with. Here's one.

No Comments

Entanglement, the Multiverse and the Universe

November 10, 2016
Categories: Multiverse Mania

“Entanglement” is the current buzzword of physics, here are two new stories featuring this:

Back in 2013 one could read lots of claims in the media that “Hard evidence for the multiverse” had been found, based on “effects of quantum entanglement between our horizon patch and others”. These claims were discussed on this blog (with a response from the authors here). A new paper by Will Kinney has now been published in JCAP, including the following conclusion about such claims:

> It is worthwhile to discuss in general the “concrete predictions” originally claimed by the authors of refs. , since several key claims do not survive even cursory scrutiny. For example, the discontinuity in the effective potential claimed to be correlated with voids and the CMB cold spot does not appear to in fact exist: for all physically relevant values of the parameters V_0, λ, and b, the modulation F(ϕ) is a smooth function, with no characteristic discontinuities which would explain features in the power spectrum. Perhaps more importantly, the form of the effective potential resulting from landscape entanglement is completely dependent on the choice of inflationary potential V(ϕ), which is itself an arbitrary free function. One could just as consistently choose the underlying inflationary potential in the absence of landscape corrections to be the same as the effective potential (2.7)! In this sense, the landscape model is no more (or less) predictive than single-field inflation itself, and most of the claimed predictions of the entanglement model turn out not to have been
> predictions at all. However, any considerations of theoretical consistency are a moot point: even if one takes the claimed predictions at face value, almost all of them are ruled out by Planck. Experiment always supersedes theory, and the model does not match the data.

This paper has an unusual story behind it, with an author of the work it criticizes trying to keep it off the arXiv. For more about this, see here and here.

Another entanglement story that is getting some press attention this week is this paper by Erik Verlinde, with its associated press release, explaining that we may be “on the brink of a scientific revolution”. I’ll have to avoid trying to give an explanation of the physical argument of the paper, on the grounds that I don’t understand it, partly because there seems to be no underlying physical model here. The basic idea is stated as replacing dark matter by

> an elastic response due to the volume law contribution to the entanglement entropy in our universe.

but someone else will have to explain exactly what that means. Maybe I’m missing it, but I don’t see anywhere in the paper a suggested experimental test of the theory. Someone much more expert than me is needed to explain whether the picture of this paper is consistent with the known astrophysical and cosmological evidence usually interpreted as dark matter/dark energy.

Comments

1. **paddy**
   November 10, 2016

   Peter, Thank you for a post of ideas with which I can distract myself. Have a nice Thanksgiving.

2. **Abbyyorker**
   November 10, 2016

   WRT Verlinde – I understand very little although I tried to read it. The takeaway for me was that there is a fundamental emergent theory of gravity that explains dark matter observations without the need for, well, dark matter. If true, it would be great but I guess the physics community needs to digest it.

3. **physicsphile**
   November 11, 2016

   One problem with the Verlinde theory as it can’t yet explain dynamical situations like the bullet cluster. Nor the cosmic microwave background observations, which arguably provide the most compelling evidence for dark matter.

4. **Erik**
   November 11, 2016

   You cannot imagine the media coverage of Verlinde’s paper here in the Netherlands. It’s really incredible. Every big Dutch news paper featured him as “being the one overthrowing Einstein” and quoting him saying that “this is going to be a new scientific revolution”. I am quite amazed by the media offensive that Verlinde and the university of Amsterdam have taken. Probably the paper is an interesting avenue, but hailing it as the next big thing seems too much.

5. **Another Anon**
   November 11, 2016

   Verlinde has been pushing that idea for years, and the dark matter idea. This just sounds like his university has a new publicity department.

   If Gravity is an entropic force, then how come it’s perfectly reversible (unlike entropy)? Lift a stone, drop it, you get the same energy back.

   I was at a talk with Erik Verlinde and Frank Wilczek, and Frank Wilczek was sooo dismissive of Verlinde’s idea it was almost rude.

6. **Peter Woit**
   November 11, 2016

   Abbyyorker,
   Yes, that’s what he’s claiming to do. Unfortunately all I see in the paper is a bunch of wordy vague analogies claiming a revolutionary new fundamental physics, and little in the way of how to confront these ideas with experiment, or any examination of whether or not they’re already disconfirmed (or even self-consistent). I haven’t computed a John Baez crackpot index for this, but I’d suspect it’s pretty high. Prominent physicists tend to be polite in terms of their public response to things like this, even when privately highly dismissive. Verlinde has been giving lectures about these ideas for several years now, and as far as I can tell his peers have just ignored them. Likely they’ll do the same with this paper, no matter how much press it gets.

7. **Entropic gravity**
   November 11, 2016

@ Erik
I guess the huge media coverage of the paper in the Netherlands can be understood as a way to justify the 6.5 million euros in fund given for the entropic gravity thing in 2012.
http://www.math.columbia.edu/~woit/wordpress/?p=5334

8. **Zack Yezek**
November 11, 2016

I actually read most of his paper, and it looks like his model is worth real investigation even if it proves to be wrong. Yeah, he's certainly not at "Einstein 1915" yet- that's hype. But its not impossible either. It wasn't like anybody in 1900 saw Einstein himself coming, or that most physicists in 1900 correctly guessed that junking the Ether model entirely was the correct path forward.

Basically, the idea is that the Holographic principle does NOT hold exactly in de Sitter space (AKA the real universe) like it does in Anti-de Sitter space. Instead, dS obeys a more general law in which the entropy of the volume is encoded in both horizon and non-vanishing 'volume' terms. He then has a considerably more tenuous/vague argument invoking ER=EPR (among several things) that associates the presence of matter-energy within a space-time volume w/ a decrease in the value of these 'volume entropy' terms within it. The 2nd Law of Thermodynamics then requires this loss of entropy be balanced (or over-balanced) by a compensatory growth of entropy on the horizon, which through a long argument he claims can be modeled well in the same way solid state physics models stresses & displacements on a chunk of material. From there he claims that at Newtonian/GR gravity is replicated at solar- to local-interstellar scales, but at galactic scale you get a new 'displacement' or 'backreaction' gravitational effect that explains why the MOND model works there (and only there) w/out new dark matter, and a non-local effect that explains apparent Dark Energy.

I don't know how much of this is justified, but in principle he sounds like he's asserting:
1) The Equivalence Principle is an EXACT law of nature. That means any experimentally observed deviation would invalidate Verlinder as well as Einstein.
2) Black holes should have no firewalls.
3) The canonical Holographic principle does NOT apply to our universe- only an extension of it does. Don't know how to test this, exactly, but if that research was EVER science the concept should be testable in principle.
4) The apparent inferred distribution of 'dark matter' is really entirely due to the distribution of Baryonic matter. That should be observationally distinguishable from numerous DM models like Bose-Einstein condensate Axions or 'hidden sector force' DM, which have non-gravitational dynamics.

9. **Peter Woit**
November 11, 2016

Zack,
Of the four things you see Verlinde as asserting as implications of his ideas, only the fourth seems to be substantive, and I don't see in what he writes any evidence that "entirely due to distribution of Baryonic matter" better explains the data (astrophysical and cosmological) than other dark matter models. But I'm pretty ignorant about these issues, so I'd love to hear from someone expert on what the data says about whether or not it's consistent with what Verlinde is claiming. Hopefully the public attention to his claims will get some experts to weigh in on this.

10. **Shantanu**
November 11, 2016

Peter interesting that now string theory can also predict MOND. I thought existence of dark matter is strong evidence for string theory.

11. **Peter Woit**
November 11, 2016

Possibly relevant:
http://xkcd.com/1758/

12. **Luis**
November 11, 2016

"Verlinde has been giving lectures about these ideas for several years now, and as far as I can tell his peers have just ignored them."

His paper on the entropic origin of gravity has about 579 citations. Maybe his peers don't agree about it but hardly one could say they ignored it. I wish my work would received that kind of indifference!

On the CMB, he says his model behaves like a pressure-less fluid like cold dark matter so, if I understood correctly, at early stages it shouldn't show differences with current models …

My first impression is that people modeling structure formation in the Universe would be able to test these ideas.

13. **Peter Woit**
November 11, 2016

Luis,

I was referring to his arguments about dark matter, which now have been around for a long time. If you can point to a reference which takes them seriously and discusses their testability and consistency with observation, I'd like to hear about that.

14. **Thomas**
November 12, 2016

The work of Kobakhidze (2011) (https://arxiv.org/abs/1108.4161) disproved the earlier version of Verlinde's theory. Unfortunately, the work of Kobakhidze (2011) is not even cited in this last Verlinde's paper.

15. **jd**
November 12, 2016

I believe that PRL 117,201101(2016), in the latest PRL, is in the neighborhood of this discussion. There already is a simulation that claims a nonMOND explanation. I do not have that reference. I am not really a fan of complex models and simulations.

16. **Robert Delbourgo**
November 12, 2016

For a nonMOND explanation of galactic rotation curves and without invoking dark matter, I have suggested that Born reciprocity might be working at the cosmic level. See my paper on Born reciprocity and the 1/r potential. Of course I might be utterly wrong but the idea might be worth a second look, especially if somebody can see how to relativize it properly. It could also benefit from proper investigation by a professional astrophysicist.

17. **Kazek Kurz**
November 13, 2016

@Zack Yezek – this is something I've thought too. I see this Verlinde paper as interesting. At macroscopic level he argues that it restored galaxy acceleration curves. So it probably takes some insight into something important. But as Kobakhidze shows, microscopic ideas below this calculations are wrong.
It starts with holographic principle and he denied it in order to include some additional degrees of freedom excited in the bulk. It looks somehow in relation to holographic principle like Gibbs paradox in relation to classical computation of entropy.

18. **Alex**
November 14, 2016

This new theory seems to depend on Verlinde's original work that argues for gravity as an entropic force (https://arxiv.org/abs/1001.0785). However, the original paper is fundamentally flawed. He mixes up the Unruh temperature that a uniformly accelerated observer experiences in flat spacetime with the Hawking temperature that a constant radial observer sees outside of a black hole. The two temperatures are not equal.

In the JHEP version of the paper equation equations (5.3), (4.1) say that the temperature of some closed surface enclosing a mass distribution is equal to the temperature proportional to the proper acceleration of some static observer (analogously to the Unruh effect). But this totally incorrect. A static observer outside of a black hole would experience Hawking radiation which would not be proportional to that observer's proper acceleration. The only place a static observer outside a black hole sees a temperature proportional to their acceleration is exactly at the event horizon where the temperature goes to infinity. However, both temperatures go to infinity in the same way. So the whole theory seems completely incorrect to me.

19. **Peter Woit**
November 14, 2016

Alex,

An odd thing about the Verlinde paper is that it's not clear (at least to me) what the relation is to the earlier one. I haven't bothered trying to follow carefully what he does, but he seems to me to be invoking different very vague speculative ideas in the new one than the old one (although, at this level of vagueness, presumably there is some relation between the new and old ideas).

20. **Alex**
November 15, 2016

Peter,

I think it is the same idea. If you look at page 10, and the text surrounding equation (2.12) he states:

"Our hypothesis is that this [horizon] entropy is evenly distributed over microscopic degrees of freedom that make up the bulk spacetime."

So the horizon entropy is the entropy of the gravitational degrees of freedom and the tendency to maximize this entropy forms the 'elastic response' (previously 'elastic force').

Actually in reading this in more detail I'm even more confused. He says that the volume of "a ball of radius r centered around the origin" is basically 4/3*pi*L^3. But in the coordinate patch he's using the volume of this "ball" of radius L calculated using the induced metric would, I think, give an infinite volume. So there's a lot of problems here.

But I agree with you in that the idea is so vague that any attempt to disprove it might be futile.

21. **MathPhys**
November 16, 2016

In an earlier version given in talks (not so sure whether that's the case also in the paper), dark matter is interpreted as degrees of freedom in the matrix model of M theory, degrees of freedom that so far have no other interpretation. But in the string theory limit of M theory, gravity is a fundamental force on equal footing with all other forces. So what's going on? Is gravity emergent or not emergent?

String theory might be about to finally be killed off

November 13, 2016
Categories: Uncategorized

News site TechEye is reporting that String theory might be about to finally be killed off, with a story that starts off

> The world's top boffins are debating finally killing off one of the more elegant scientific theories about the nature of reality.

This isn't exactly the most reliable information source, with one problem that the story is about supersymmetry, with the only connection to string theory this:

> For ages the world's cleverest physicists have been divided over the concept of supersymmetry – a theory which stipulates that all known fundamental particles have heavier, supersymmetric counterparts called sparticles. It appears to be based on the theory that the universe is made out of string which was teased into shape by cats which are potentially dead or alive

The writer seems to have gotten the material for the story from a more reliable source, the Economist, which recently had A bet about a cherished theory of physics may soon pay out. That story starts by explaining about Ken Lane's 1994 bet with David Gross that the LHC would not see supersymmetry. As mentioned here, this year's data should be enough to resolve the question they were betting about. It is likely that results from SUSY searches using most of this year's data will be presented at the usual mid-December LHC Jamboree at CERN, and unless there's dramatic news my sources are keeping from me, these results will be negative.

The bet was for an expensive dinner at Girardet's, a three-star Swiss restaurant which has now closed, and

> Dr Lane says it is time for Dr Gross (who won the Nobel prize in 2004) to cough up—if not with dinner at Girardet's then at another suitably ritzy venue. After receiving no response to several e-mail prompts, however, Dr Lane is growing impatient. "David appears to be welshing on our bet," he says.

but

> Dr Gross is not ready to concede quite yet. The data are in, but their analysis is not complete. "It looks like I will lose this bet by the end of the year," he says, "but we should await the word from the experimenters themselves." (Dr Lane says the original terms have been met and Dr Gross should throw in the towel.)

As for whether this really kills off SUSY, there's a pithy quote from Sabine Hossenfelder:

> Sabine Hossenfelder, a theoretical physicist at the Frankfurt Institute for Advanced Studies, is one of many who think it is time for theorists to focus on other problems—how gravity behaves at the very small scales of quantum mechanics, for instance. If the LHC finds no trace of sparticles in this year's data, she believes the last thing the field needs is another round of Susy model adjustments. "That's not science," she says. "That's pathetic."

Comments

1. **John McAllison**
   November 13, 2016

   I agree with Gross on this: wait until ICHEP 2017 when the recorded 40fb from 2015-2016 has been fully analyzed. The news coverage will be far greater on the demise of SUSY, than on the the state of the diphoton excess at ICHEP 2016.

   Talking of bets… I'm hoping Jester does a series of blog posts over the coming months on how his bet with Lubos is finally being concluded at long last.

2. **Another Anon**
   November 14, 2016

   If SUSY dies, surely string theory dies as well?

3. **Another Anon**
   November 14, 2016

   There's no fermions in bosonic string theory without SUSY.

4. **Jeff M**
   November 14, 2016

   Being a mathematician, not a physicist, I'm curious – any story about what the first bet like this was? Also, isn't SUSY essentially unkillable, I mean can't you adjust the energy up by at least several orders of magnitude?

5. **Kenneth Lane**
   November 14, 2016

   1)From my iMac's built-in dictionary:

   welsh |welSH| (also welch)
   verb [no obj.] (welsh on)
   fail to honor (a debt or obligation incurred through a promise or agreement): banks began welshing on their agreement not to convert dollar reserves into gold.

   Sorry, my Welsh friend. As far as I know, it is not meant as a national slur, but I don't have my OED handy.

   2) At ICHEP, in Chicago in August, the largest statistical support for supersymmetry was about 3-sigma; see the plenary talk by Dave Charlton. At that time, the LHC had delivered a total of about 55 inverse femtobarns to ATLAS and CMS, of which about 90-95% was recorded by each detector. There is now about 65 inverse femtobarns delivered to them. There is no way on God's green earth that the additional 1o inverse femtobarns can turn a 3-sigma fluctuation into a 5-sigma discovery. As any decent physicist knows.

6. **Kenneth Lane**
   November 14, 2016

p.s. For those unfamiliar with the LHC: Those 55 and 65 inverse femtobarn refer to EACH detector. The bet called for only 50.

7. **Peter Woit**
November 14, 2016

Kenneth Lane,

I think you're counting both 8 TeV and 13 TeV data. Presumably David's argument is that the 8 TeV data doesn't count, only data at or near the design energy counts. In any case, he's only got a month left before (negative) results with 50 inverse fb at 13 TeV appear, so you should be able to collect soon.

As in many etymology questions, the one here may be not so clear, but online one finds explanations like this:

".Even worse is the verb 'to welsh,' meaning 'to renege on a bet,'…The term welsher became common in Britain during the eighteenth and nineteenth centuries in the argot of race-track bettors. But from a reader came a comforting word for all Welshmen, one which gives a touch of logic to the use of the term: 'It was ENGLISH bookies who, having too many long shot winners against them, fled over the border to 'boondock' Wales to become the original welshers and escape irate bettors looking for their payoff.'"

8. **Eric Weinstein**
November 14, 2016

[This is really weird. Something must have changed for me to be writing a pro-SUSY comment.]

To state the obvious, nature either uses SUSY, or it does not. So, let's leave that aside at first.

What irritates so many about SUSY, is that SUSY supporters often act like it just HAS to be true and then somehow get an indulgence from the usual standards of science to take a massive advance on experimental reality.

So while it is really satisfying to see the most strident of the SUSY cheerleaders lose their bets, this isn't really about SUSY. It's a morality play about representing a particularly insufferable flavor of triumphalist group arrogance in speculative physics.

But what if, for argument's sake, SUSY is real, yet nature instantiated in a totally different framework than the one expected? It's not SUSY which should be on trial. Instead, it's really the specificity with which the current top physics echelon claimed it could specify the future of the field.

SUSY is one of the few areas in which physicists pointed mathematicians at something that mathematics should have seen but even now can't really intuit. Sure you can write down axioms, but the SUSY structures are surprisingly rich and difficult to motivate.

If we lose SUSY, we are going to lose a lot of genius thinking about spinors that has not yet been fully incorporated into the core of pure mathematics. I for one hope that SUSY is real but that the current instantiation will come to be seen as wildly off. That will end up, in practice, seemingly vindicating both the cheer-leading David Grosses of the world and their grumpy critics. In truth it will validate neither.

I don't believe in the sparticles we are supposed to see at LHC. I also don't believe that SUSY is nowhere to be found in nature. My guess has been that we did something brilliant and then messed it up. And this is why SUSY won't die and also isn't progressing to confirmation. Let's let the morality play finish, but realize that it is triumphalism rather than SUSY which is about to have its comeuppance.

9. **Peter Woit**
November 14, 2016

Eric,
I agree that there's a problem here, since the term "SUSY" can mean several different things. Particularly a problem to my mind is that what doesn't get distinguished are

1. SUSY = use of super Lie algebras (i.e. Z2-graded Lie algebras)

2. SUSY = Extension of the Lie algebra structure of the Standard Model by a specific extension of the Poincare Lie algebra by a Z2-odd spinor (N=1 minimal supersymmetric extension of the SM).

The second of these has been problematic from the beginning (no superpartners= states you would get by acting by the Z2-odd spinor operators on known states, theory gets ugly very quickly if you try and introduce new physics to push these states to unobservably high masses). The first is a very general idea, and I think likely will be part of any deeper understanding that goes beyond the SM.

10. **Barry Badge**
November 14, 2016

I obviously don't know, but a phenomenon like String Theory looks to me more historically similar to a political movement than a scientific theory, in that the key to the status of that community is very much to do with the dominance it holds in key institutions and the links it has to the major sources of funds.

I am hopeful String Theory will die off but the signal will, I think, be that of the funding drying up. Even then, with institutions like Templeton in play, String Theory will leave the mortal coil of science very much like the proverbial 'old hippy'.

That said, the last couple of years feels like a sea change with many more people coming out against it. You're obviously much more informed and in-touch with events and if you think it's a maybe, maybe it is.

11. **Peter Woit**
November 14, 2016

Barry Badge,

The real news is the failure of the 13 TeV LHC to see supersymmetry, with the way the TechEye story brings string theory into it kind of silly. It is true that no SUSY at the LHC removes the best hope that those devoted to string theory unification models had for seeing some sort of experimental vindication. Other than SUSY, hopes for an experimental result indicating string theory are now pretty much "maybe a miracle will happen." This isn't at all though a new situation.

I don't want this to turn into the same discussion as usual of the state of string theory, but I'll just comment that funding has never really been the way to look at this: amounts of money going into theoretical physics, string theory or otherwise, have always been and always will be rather small compared to other topics.

12. **Barry Badge**

November 15, 2016

"amounts of money going into theoretical physics, string theory or otherwise, have always been and always will be rather small compared to other topics."

Sure but that surely translates the allocation of resources up the list of what determines outcomes. Presumably the problem is not String Theory in itself, in that if there were 20 string theorists worldwide on a shoe string budget, there would be no problem.

13. **Anon**
November 15, 2016

The December CERN jamboree is cancelled to preserve the sanity of the experimentalists. Expect the 40 fb-1 results for Moriond early next year.

14. **Peter Woit**
November 15, 2016

Anon,
Thanks for the news. So, the likely date for the funeral of SUSY and fine-dining for Ken Lane now seems to be four months away, mid-March.

15. **Entropic gravity**
November 15, 2016

We'll enjoy the event better during the spring break… 🙂

16. **NumCracker**
November 16, 2016

BTW, one can not experimentaly kill SUSY or even superstrings, they can survive even this ridiculous limits: https://arxiv.org/abs/hep-th/0503249

17. **Ford**
November 16, 2016

Dear Peter

I would like to see your comments on approaches like this, where SUSY disapears from observable spectrum: https://arxiv.org/abs/hep-th/0105155
Regards

Ford

18. **Peter Woit**
November 16, 2016

Ford,

I think Sabine Hossenfelder's comment was aimed at this kind of thing, and I agree with it.

19. **Kenneth Lane**
November 17, 2016

To Peter Woit and what data at what energy counts: As you can see from the original bet (whose picture I believe you posted) there is no mention of what energy the 50 inverse fb must be. There is not even mention of whether it is 50 inverse fb per detector of 50 total for the two. A reasonable interpretation is that it is 50 inverse fb for each detector and all the data from Runs 1 and 2 count toward the 50. (After all, the Higgs boson was discovered in 2012 with — as I recall — somewhat less than 20 inverse 5b of 8-TeV data.) The job of interpreting the bet is on the other three physicists, and they agree that the conditions of the bet have been met. BTW, I do not think there will be a total of 50 inverse fb of 13 TeV data; it is more like 42 or so.

As for the ethnic slur, I was unaware of its origin and I sincerely apologize for using it.

20. **Peter Woit**
November 17, 2016

Kenneth Lane,
Unfortunately the terms of your bet are somewhat imprecise, and I can see a technical argument from Gross's side to wait a bit more before conceding. He has though clearly lost the bet, with the only thing that could save him a SUSY discovery in this year's data, and that doesn't seem to have happened (although we'll have to wait until March it seems for fully analyzed, released, results). It sounds like you'll be able to collect, on a time scale of months from now.

I'm more interested though in whether we'll see a consensus in the HEP community that SUSY is dead, or whether it will continue to live on as a zombie form of physics. One question is whether Gross will admit not just that he lost a bet, but that this was a wrong idea. Another thing I wonder about is whether the field will keep training students in this subject, even after it is dead. One thing to watch will be the upcoming IAS summer school on particle phenomenology, see
https://pitp.ias.edu/
Will there be lectures there aimed at training the grad students and postdocs in the lost cause of SUSY phenomenology, or will there be an acknowledgement that this was a failure, and a move on to other topics?

21. **Nakanishi**
November 17, 2016

SUSY is dead: Nature is natural!!
Since some decades ago, I have continuously claimed that SUSY should not be a correct symmetry. The reason for this belief is that SUSY as a symmetry at the Lagrangian level is inconsistent with quantum Einstein gravity, which implies that the Poincare symmetry cannot be a primary symmetry but is a secondary symmetry derived as a consequence of spontaneous breakdown, just like the electromagnetic U(1) symmetry in the electroweak theory.

22. **Scott Church**
November 20, 2016

Peter, forgive me if this is a naive question but…

What's being killed off here is *minimal* SUSY. We can of course keep looking for it at ever-higher energy scales. Now IMHO searching for increasingly massive sparticles to preserve increasingly lop-sided symmetries with their lighter Standard Model partners only makes it more subject to Occam's Razor. But a string theory enthusiast could respond by playing the "elegance" card, and say that a theory this "beautiful" justifies doing that… indefinitely (quoting Einstein, "If the facts don't fit the theory, then so much worse for the facts…").

It seems to me that questions of "beauty" vs. Occam's Razor are about as objective as arguing about who's wife is hotter. I've seen Sean Carroll go so far as to refer to the AdS-CFT duality as "our best *understanding* of quantum gravity…" even though it's a toy model duality based on an AdS universe we *don't* live in. If the word "understanding" can be stretched to limits like these it's difficult to see what *would* be ad-hoc enough to offset what's "beautiful" to string theory enthusiasts.

So… how can we be sure it's even *possible* to kill off string theory? Is this really any different than trying to convince Linus not to waste yet another Halloween night waiting in the cold for the Great Pumpkin to appear…?

23. **Peter Woit**
November 20, 2016

Scott Church,
Yes, string theory unification is compatible with anything, and so is SUSY, by pushing the SUSY breaking scale up to unobservably high values. But, for the last 30 years or so, whenever asked about how string theory was going to connect to experiment, the standard answer has been by finding SUSY at the LHC (and a SUSY dark matter particle). Now that that has collapsed, there is nothing left as justification for string theory unification except claiming that ugly scenarios are really beautiful, and hype about AdS/CFT (which has nothing at all to do with string theory unification).

The point is not that seeing nothing at the LHC falsifies string theory/SUSY, but it falsifies the one viable heavily sold scenario for connecting these ideas to experiment.

What Math Do You Need For Physics?

November 17, 2016
Categories: Uncategorized

Chad Orzel has a very sensible piece at Forbes, headlined What Math Do You Need For Physics? It Depends, which addresses the question of what math a physicist like him (experimental AMO physics) really needs. I'm glad to see that he emphasizes the same basic courses my department offers aimed at non-majors:

- Multivariable calculus
- Differential equations
- Linear algebra

together with statistics (which here at Columbia is handled by a separate department). He disses complex analysis, for reasons that I can understand. That's a beautiful subject, and the results you can get out of analytic functions and contour integration are often unexpected and seemingly magic, but they're not of as general use as the other subjects.

One subject he doesn't mention that I would argue for is Fourier Analysis, which is the class I'll be teaching next semester. That's an incredibly useful as well as profound subject which every physicist should know, but it is true some of its basics often gets taught in other courses (for example in ode courses as a method for solving differential equations).

Orzel starts off with an amusing discussion of a physics version of "Humiliation", admitting that he's never used or really worked through a proof of Noether's theorem, widely considered "the most profound idea in science". I've argued here for the Hamiltonian over Lagrangian method, in which case a different set of ideas about symmetry is fundamental, with Noether's theorem playing no role. In the Hamiltonian case symmetries are generated by functions on phase space, and finding the function that generates any symmetry is a matter of Poisson brackets (as an experimentalist, maybe Orzel has never calculated a Poisson bracket either though…).

One says that a function F on phase space generates an infinitesimal transformation if such an infinitesimal transformation changes the function G by {G,F} (the Poisson bracket of the functions G and F). A basic example is the Hamiltonian function, with {G,H} the infinitesimal change of G by time translation, or in other words, Hamilton's equation
dG/dt={G,H}
When {H,F}=0, we say that the infinitesimal transformation generated by F is a symmetry, since H is left unchanged by such transformations. Using the antisymmetry of the Poisson bracket, this can also be read as {F,H}=0, with Hamilton's equation then the conservation law that F doesn't change with time.

All this seems to me much more straightforward than the Lagrangian Noether's theorem approach to symmetry transformations and conservation laws. My own analog of Orzel's admission would be admitting (which I won't do) how long it took me in life to understand this fundamental point (I blame my teachers).

For lots and lots more about this, see chapters 14 and 15 here.

Comments

1. **sflammia**
   November 17, 2016

   To me, complex analysis and Fourier analysis are inseparable. For example, theorems like the Paley-Wiener theorem give direct, explicit, and important connections between complex analytic concepts like analyticity and Fourier analytic concepts like smoothness or rapid decay of Fourier coefficients. Perhaps the problem is that the two subjects are not taught in a way that synthesizes the most useful features for a physics audience.

2. **Jeff M**
   November 17, 2016

   Interesting. Back when I was a physics major, I did more math than most (ended up a mathematician of course). What about group theory? Lie groups. Had a course in that as an undergrad, which I guess is pretty unusual. Did it instead of Galois theory. And you need some complex. The only mathematical physics course I took was all complex analysis, mostly funky contour integrals. I agree about hamilton, and Fourier analysis. Of course, I guess it really depends on what sort of physics you do. Differential geometry is helpful, and nowadays graph theory. I guess you need enough time to take some physics courses….

3. **Bee**
   November 18, 2016

   I managed to misread Humiliation for Hamiltonian! As Jeff says above, I'd think you pretty much can't do quantum mechanics and quantum field theory without group theory, Lie groups, representations thereof, and so on. But you can go a pretty long way without differential geometry.

4. **Bobito**
   November 18, 2016

   In practice it is hard to directly measure momenta (we don't "see" phase space easily). For this reason the Lagrangian description is fundamental. Sometimes one has to think like an experimentalist.

5. **cthulhu**
   November 18, 2016

   I'm an engineer, not a physicist, but wouldn't you need tensor calculus for general relativity? Or is GR a specialized enough field that you would only take that math when you needed it?

   Looking back at my college years and knowing what I know now, I would have traded my second semester of PDEs for a half advanced linear algebra, half advanced Fourier analysis class; I use those every day but haven't dug out those fancy PDEs in decades.

6. **Chris Oakley**
   November 18, 2016

   I am probably pushing at an open door here, but I would vote for the theory of continuous groups as well. In 1980, as I was finishing my undergrad physics course, there were a lot of talks and TV programs on high-energy physics aimed at the general public (remember those days?) SU(3) would come up a lot. This was fascinating to me as I had no idea what it was. When, as a graduate student, I actually found out, I was almost disappointed. Continuous groups should, IMHO, be taught alongside quantum mechanics at undergrad level.

7. **anon**
November 18, 2016

GR is very much a specialized field. I don't think many physicists, apart from those working directly on GR or related subjects, know the mathematics needed for that.

I agree that parts of group theory are needed for QM, but still I wouldn't say it's something necessary for all physicists.

8. **vskrin**
November 18, 2016

@sabine: I've also misread Humiliation for Hamiltonian, professional deformation I suppose.

@peter: You're probably familiar with DeWitt's book (well a series of books actually) on QFT. In there he advocates various approaches which are not standard in the HEP community. In particular there's a lot of mentioning of the Pereils bracket, and thing like that. Now, as much as I wanted to read that book cover to cover I still didn't find time for it, so I was wondering if the approach that you're describing here in the post has any relation to DeWitt's approach?

Cheers!

9. **Yatima**
November 18, 2016

Off-topic:

Any thoughts on (no, not Trump, but):

https://www.quantamagazine.org/20161115-strange-numbers-found-in-particle-collisions/

10. **Dave Miller**
November 18, 2016

Peter,

I've worked in engineering as well as physics: Any decent engineer needs to be very comfortable intuitively with Fourier analysis. To be sure, they do not have to remember the difference between the Dirichlet kernel and the Fejer kernel, but they need a strong physical sense for what is happening.

And, I once solved an important engineering problem using the Schwarz-Christoffel transformation. To be sure, most engineers do not remember enough complex analysis to handle that (perhaps that is why the company hired a physicist).

Dave Miller in Sacramento

11. **Dave Miller**
November 18, 2016

cthulhu asked:
>I'm an engineer, not a physicist, but wouldn't you need tensor calculus for general relativity? Or is GR a specialized enough field that you would only take that math when you needed it?

I recently worked out the Schwarzschild solution without using tensors: I ran with the approach Feynman lays out toward the end of volume II.

So, it's possible to do some basic things in GR without tensors, though I have to admit it is quite a mess: I consider my little exercise to be justification as to why one really needs to learn the tensor apparatus.

Tensors are also used in various fields of engineering (stress, strain, and all that), although not in the same way as in GR. Anyway, I personally do not think someone should call himself a physicist if he does not understand the math and physical ideas underlying GR.

Dave

12. **Jeff M**
November 18, 2016

When I did GR as an undergrad, I taught myself tensors along the way. I had some differential geometry before that, but not a lot. But I was lucky enough that I was doing the GR as an independent study with a great professor, so I had a lot of help.

13. **Josie Altzman**
November 18, 2016

I kind of wish I had this type of thing when I was an undergrad physics major at CU back in the 80's. I did take the Fourier Analysis course, along with a whole course on Calculus of Variations (not sure if it's even still taught).

I ended up swallowing Morse&Feshbach in graduate school for fun.

14. **Low Math, Meekly Interacting**
November 18, 2016

I'm acquainted with a former GR post-doc who made the jump to medical imaging and does some freelance coding for VR gaming. Given the role tensor maths play in the diverse subjects, and how well-versed he also is with QM and the basics of QFT, I'm guessing he'd be appalled to hear most physicists don't need tensors.

15. **Miki Weiss**
November 18, 2016

Lagrangian dynamics is a very usefull tool in Mechanical engineering, enabling to write equations of motion of very complex rigid body systems, including translations and 3d rotations, with generalized coordinates and complex boundry conditions (see the book "Lagrangian Dynamics" by A.D. Wells)
Fourier Analysis (in the complex plane), in continuous and discrete time, along with Hilbert space techniques, are among the most usefull tools of modern Electrical Enginnering – Communication &Information theory Information theory, Signals analysis, synthesis and processing (Audio, Cellular systems, Radar, Sonar), detection &

parameter estimation of signals buried in noise, e.g., "Digital Signal Processing" by Oppenhaim, "Digital Communication" by Proakis and the classical 4 volume series of Van Trees. The communication and Internet revolution of the 1990's was not possible without those tools, used daily by hundred thousands engineers worldwide.
Finally, Green function methods together with Fourier analysis are used by RF Antennae designers, solving Maxwell equations from the EM fields induced on the antenna surfaces, as means of calculating the far field Antenna gain pattern (which actually is a tow dimensional Fourier analysis with complex boundary conditions.
Hurrah for those 17-18 century French mathematicians!

16. **Matthew Foster**
November 18, 2016

In condensed matter theory, we are importing more and more abstract/modern mathematics to deal with topological insulators, superconductors, and topologically ordered phases. In my view the standard undergraduate training that focuses mainly on pre-20th century mathematics is leading to an increasing gap between what is taught and what is used in research. I'm not sure what the solution is, though, because if you want to apply category theory notions to (e.g.) non-abelian anyons in fractional quantum Hall states, learning category theory from math texts or courses is from my perspective a big circumnavigation around what a physicist really needs: intuitive grasp of the rules and algorithms for applying them. (Of course there are many exceptions–I take it that Greg Moore learned category theory overnight.)

My own small contribution has been to elaborate on R. Cahn's Lie algebra book, to try to write course modules that could be accessible to an undergrad. My argument (that I made in a grant application at least) was that Lie algebras are kind of the "gateway drug" to many more advanced notions, such as affine Lie algebras and 2D CFT, braiding and fusion in anyon systems, Bethe ansatz solvable systems and quantum groups. Thus if students who want to do theory can learn LA representation theory as undergrads, maybe it further opens the door to assimilating these more modern topics in graduate school. It's a work in progress, but what I have is available here:
http://mf23.web.rice.edu/

17. **Thomas Larsson**
November 18, 2016

My first year in college ended with a class in vector calculus, including Cartesian tensor calculus. Once you understood how to juggle those indices, you didn't have to struggle to remember in detail how to deal with those divs, curls and nablas – it all popped out automatically. It was like knowing about the Euler formula, when people taking "simpler" classes had to memorize tons of trig formulas. A real time-saver.

From the viewpoint of my later non-career, however, I would said that group theory is the most important. In particular, tensor calculus and differential geometry are secretely the same as the classical representation theory of the diffeomorphism (or Poincaré, in flat space) group. This just says that tensor calculus deals with objects that have a meaning independent of the coordinate system.

18. **AcademicLurker**
November 18, 2016

Orzel's list sounds much like my undergraduate maths curriculum. 3 semesters of calculus, 1 semester of linear algebra, 1 semester of differential equations, and one of those grab bag "mathematical methods" courses. All the rest of the math I learned was either taught piecemeal in various physics courses of picked up on my own as needed.

A sometimes wish I had taken a proper complex analysis course.

19. **Manfred Requardt**
November 18, 2016

Dear all, I think the core observation of the field theoretic Noether theorems (I and II) is the existence of conserved currents comong from symmetries. The second theorem even includes gauge theory (infinite dimensional Lie groups). In this sense it is very fundamental.

20. **Tim**
November 18, 2016

Two thumbs and ten toes up for Fourier Analysis. I'd encourage you to include at least an overview of how Shor's Algorithm works. The implementation of a fourier transform as a series of phase rotation quantum gates is quite elegant. It's a nice example of how being able to quickly measure periods of functions leads to some clever and unexpected results.

21. **Peter Woit**
November 18, 2016

Bobito,
I'm not convinced that thinking simultaneously about position and momentum is that hard, even for an experimentalist (and often, momentum is easier to measure than position, e.g for light). What is deep and unintuitive is the Poisson bracket, one can argue more so than the Lagrangian (although my point is that once you understand the Poisson bracket, you have conserved quantities for free, while with the Lagrangian you have non-intuitive work to do to find them).

vskrin,

In my earlier discussion of this that I linked to, there's a link to this article
https://arxiv.org/abs/1402.1282
which is the best explanation I know for the relation of the Peierls bracket and de Witt's approach to more conventional Hamiltonian methods.

Yatima,
That's an interesting topic, but I don't know much about it, nothing useful to say here. The mathematical structures that are begin discussed come up in looking at terms in a series expansion, they're interesting mathematics, and potentially quite useful, but not clearly part of the deep structure of the theory, rather than an artifact of the approximation.

All,
I obviously am quite devoted to the idea that Lie groups and their representations are the way to understand quantum mechanics (and can recommend a good book…). A problem though is that any young physics student who wants to learn this math is going to find that math department courses covering it only are offered at an advanced level, and not really accessible.

As for GR and geometry, yes, they should be studied together. But, for most physicists GR is disconnected from what they do and they can happily live without knowing anything about it.

22. **Matthew McIrvin**
November 18, 2016

My impression was that when physicists talk colloquially about Noether's Theorem, often they really are thinking about a Hamiltonian picture, not the Lagrangian one in which Noether's Theorem was literally derived.

More specifically, a *quantum* Hamiltonian picture, really. Some quantum operator commutes with the Hamiltonian, therefore it's obviously a conserved quantity, and the transformation you get by exponentiating it must preserve the Hamiltonian as well, so there's your symmetry. It's all very simple once you're used to QM.

23. **vmarko**
November 18, 2016

Peter,

I'm slightly confused by you emphasizing the Hamiltonian over the Lagrangian approach, over and over. Purely at the math level of ODE's, it is simply a theorem that N second-order ODE's are equivalent to 2N first-order ODE's (of course, given certain assumptions), which is all there is to say really regarding the relationship between Lagrangian and Hamiltonian approaches. Naturally, each of the two approaches has its strengths and weaknesses, and every physicist should be familiar with both, and use one formalism or the other as needed, the right tool for the job. But fundamentally, what is all the fuss about?

Regarding Noether theorems, their true power lies not in the discussion of the conserved charges, but in the discussion of gauge symmetries. And in the Hamiltonian approach, the presence of gauge symmetries requires one to understand the Dirac analysis of constrained Hamiltonian systems, first- and second-class constraints, Dirac brackets, Castellani's construction of the symmetry generator, the proper boundary behavior of variables to construct conserved quantities, the whole shebang.

In other words, in the Lagrangian framework, the consequences of gauge symmetries are neatly packaged into two Noether theorems. On the other hand, in the Hamiltonian framework, one discovers that the evolution is not described by a single phase-space trajectory but an infinite family of trajectories (connected by a gauge transformation), and all hell breaks loose…

As an example, just try to explain the "problem of time" to a student, using the Hamiltonian framework. I just don't know of any easy way to explain (without appealing to Noether thms) how come the Hamiltonian of a diff-invariant system is always zero, and how one is supposed to work out anything in the Hamiltonian framework if the actual Hamiltonian vanishes.

One always needs to understand some context — Emmy Noether was asked by Hilbert, Einstein and others to clear up the fuzziness around conservation laws in general relativity. That's how the two theorems arose, and they explained away this fuzziness with two pure and beautiful pieces of math. Hamiltonian description of GR came much later, with the development of the ADM formalism, and it is confusing a lot of students to this very day. On the other hand, in QM the Hamiltonian approach was dominant from the very beginning, but there were no gauge symmetries involved. They came in as an afterthought (and gained mainstream importance only with the development of the Standard Model), but Hamiltonian formalism was never really well-suited to the study of gauge symmetries. The complexity of the Dirac formalism of constrained systems is a testament to that.

It seems to me that you are emphasizing the triviality of the first Noether theorem, while completely ignoring the second one, where all the power really lies. Let me suggest a challenge — try to derive the Bianchi identities in GR using the Hamiltonian (or ADM) formalism and moment maps, and then say it is better, clearer and easier than the Lagrangian approach. I just don't believe that the Hamiltonian framework could ever possibly be superior there.

Best, 🙂
Marko

24. **Peter Woit**
November 18, 2016

Matthew McIrvin,
Yes, I do think that when physicists invoke Noether's theorem, often they're misspeaking, having in mind the Hamiltonian rather than Lagrangian formalism. And I completely agree that, in the Hamiltonian formalism, the relation of conserved quantities and symmetry group actions is even simpler in quantum mechanics than in classical mechanics (as you say, because the group action is just given by exponentiating the operator corresponding to the conserved quantity).

Marko,
My comments were really motivated by Orzel's pointing out that he doesn't use or really understand Noether's technique for finding conserved quantities, and I was arguing that for simple physical systems, how conserved quantities work is much clearer in the Hamiltonian formalism.

Yes, for something like GR the Lagrangian formalism and Noether's second theorem come into their own (and that's why she developed this). But the issue of gauge and diffeomorphism symmetry is a subtle and thorny one no matter how you deal with it (since it is so hard to cleanly separate out physical and gauge degrees of freedom). I'm not convinced the Lagrangian formalism doesn't obscure parts of the problem that the Hamiltonian formalism at least brings to the fore.

25. **Some guy**
November 19, 2016

The Lagrangian version of Noether's theorem typically is preferred because there are important symmetries which are manifest at the level of the Lagrangian but not in the Hamiltonian. The most obvious example are the symmetries corresponding to Lorentz transformations. Here the Poisson bracket/Hamiltonian formulation suffers from the fact that the transformations on the phase space variables depend on the precise form of the Hamiltonian, in contrast to the Lagrangian formulation where the transformations depend only on representation content and not on the precise form of the action. To convince yourself of this, try proving the Lorentz invariance of non-abelian gauge theory in the Hamiltonian formulation. It is quite an ordeal, whereas in the Lagrangian formulation it follows by inspection.

26. **santarakshita**
November 19, 2016

I think the importance of complex analysis is much less subjective than claims of its beauty (or perhaps, what I write next is possibly an expression of that beauty): it is absolutely vital as a source of intuition and motivation for a vast amount of modern mathematics; given that function theory of the 19th-20th century led to the foundational papers in algebraic geometry (ex: Riemann's Abelian function paper), algebraic topology and number theory. Covering spaces and (co)homology (via Cauchy's theorem) arises naturally in a first course of complex analysis. One cannot master algebraic geometry at any level without some understanding of Riemann surfaces.

While it is possible to repurpose a machine for a variety of uses, I feel it is vital to at least have a sense of its original context.

27. **ComplexIsPunny**
November 19, 2016

I'm a mathematician, not a physicist, so I am not really qualified to comment, but
to my mind a bit of complex analysis is fundamental. (I had to review this material for physics students recently so it's on my mind.) Fourier series is of course necessary

for physics but you can't legitimately teach
Fourier series without Euler's formula (e^i\theta=…) since it simplifies the formulas and proofs so much, and once you are doing that, how can you not make the connection between power series (Laurent series) restricted to the circle and Fourier series? I'm talking about intuitive connection, proofs not needed, as the details of when what converges in what sense are subtle.
But for the basics you at least need the idea of radius of convergence of a complex power series, the complex expansion of exp, sin and cos. Euler then gives you the polar form of a complex number and the connection with rotation matrices. Once you have the idea of complex derivative the Cauchy-Riemann equations fall out of that. Now Cauchy's theorem and formula et al are so closely related to vector calculus that it just reinforces that material which is so crucial for E and M. That may be all that is really needed. I agree that computing complicated contour integrals could be largely a waste of time. My favorite Complex Analysis text is Lang (surprisingly given some of his other books, this one is excellent). Marsden, Knopp I and II, and still Ahlfors are all excellent as well. For the professor to delve into more deeply for themselves, Katznelson's book on Harmonic Analysis is remarkable.

If Fourier transforms are necessary, that's tougher to do it rigorously, as you ideally need some measure theory and functional analysis. But at least they should have the intuitive ideas that the transform of a sin wave is a point mass at that frequency, and that the transform of a Gaussian is a Gaussian, with a thin one getting spread out and vice-versa.

28. **paddy**
November 19, 2016

My (winding down) career has been applied/experimental (coincidentally starting with AMO) ; however, I had earlier undergraduate/graduate theoretical pretensions. I was exposed in that phase to a number of areas that have been concretely helpful throughout my career, most notably complex/Fourier analysis (only somewhat also Group theory). I see from above many posters concur. I also see the mention of Marsden–which sits almost pristine on my shelf from 40 years ago. Perhaps it is my own taste, but the book I pull down the most on these issues is Carrier, Krook and Pearson. Mainly because without over-simplification it introduces and addresses techniques like Weiner-Hopf by demonstration and application. Lighthill, for all its skimpiness, is another useful text.

29. **David**
November 19, 2016

Highly recommended: D. Neuenschwander, "Emmy Noether's Wonderful Theorem." A marvelous little book.

30. **Peter Woit**
November 19, 2016

Some guy,

My current favored way to think of this is that it is the equations of motion and the symplectic structure on the space of solutions (=phase space) that is fundamental. Lagrangians give a nice way of producing such things (as solutions of the variational problem), although often with a degenerate or singular symplectic structure (and thus descent into "constrained Hamiltonian dynamics").

This point of view sometimes is called "covariant" phase space, with some names attached to it Crnkovic-Witten and Zuckerman, who wrote down the Yang-Mills case. It's a point of view that ends up relating Noether's theorem to the Hamiltonian point of view and Poisson brackets.

Typically one parametrizes solutions of equations of motion by initial values at t=0, and when you do this you get a problem describing some of the Lorentz generators (boosts). These aren't really symmetries in the usual sense though (don't commute with the Hamiltonian).

I think historically the preference for Lagrangian methods arose in the development of perturbation series methods in relativistic QFT (Schwinger, Feynman), since understanding renormalization is much more difficult without explicitly Lorentz-invariant terms in such series. Gauge symmetry leads to its own problems with Lorentz invariance, which are though quite fundamental (inherent conflict between positive Hilbert space and covariant elimination of the gauge degrees of freedom).

31. **Peter Woit**
November 19, 2016

About complex analysis:

santarakshita: no question you should understand Riemann surfaces and complex analysis to have any idea what is going on in algebraic geometry, but physicists can (and do) almost all happily go through their careers knowing nothing about algebraic geometry.

Complexispunny: To discuss Fourier analysis you clearly want to talk about complex valued functions of a real variable, but you can mostly avoid functions of a complex variable and the notion of a holomorphic function (although that may be the best way to understand some facts).

paddy: I second the recommendation of Carrier, Crook and Pearson, which is full of wonderful applications of the subject. That's actually the book I first learned complex analysis from, in an applied math course. Later on I learned a less applied point of view by teaching the subject in a math department.

32. **paddy**
November 19, 2016

Peter: might not be a coincidence on Carrier, Krook et al. My handwriting in inner leaf says '74 and I am sure for a course in DEAP (?). Cannot recall the name of the professor.

33. **paddy**
November 19, 2016

PS: I've always felt that my preference for the Hamiltonian formalism (as more intuitive ?) was do to my not proceeding further into relativistic QFT than I did. Perhaps someday I will revisit.

34. **Peter Woit**
November 19, 2016

paddy,
That's very funny, not a coincidence. I just dug up my old course notes, seems I took Applied Math 201 Fall 1976, my sophomore year. I have written on the notes as instructors Richard Lindzen and John Hutchinson, but don't remember much about them. Lindzen went on to fame (notoriety?) in climate science, Hutchinson I guess is still there, see
http://web-static-aws.seas.harvard.edu/hutchinson/

Applied Math 201 was the complex variables class and used Carrier, Krook and Pearson as one of the texts. Presumably this had something to do with the authors having taught this course at Harvard in the 60s

There was also a second semester of the class, Applied Math 202, about PDEs, which I took spring 1977. That used the book "Partial Differential Equations" by Carrier and Pearson, presumably also written based on that course.

35. **paddy**
November 19, 2016

OT a bit I know…but. The professor in '74 in whatever the course was using CK&P would finish his lecture and, as students funneled out (and some forward to ask him questions), he would–as if rationing–begin unwrapping a pack of Players.

36. **Peter Woit**
November 19, 2016

paddy,
I don't remember this, but could easily have been Lindzen, who was there in 74. There's this from a 2001 Newsweek interview

"Lindzen clearly relishes the role of naysayer. He'll even expound on how weakly lung cancer is linked to cigarette smoking. He speaks in full, impeccably logical paragraphs, and he punctuates his measured cadences with thoughtful drags on a cigarette."

37. **paddy**
November 19, 2016

Peter: I looked up Lindzen and you are correct. 'Twas he. Good course and good teacher. I hope he (unlike I) has stopped smoking.

38. **Jonathan Miller**
November 19, 2016

I had a double major in physics and math and continued to take math in my first year of PhD study when I intended to become a theorist (I was also accepted into math PhD programs). I left theory for experiment, but have continued to work with theorists. I have observed that most theorists and experimentalists could have used more computer science courses and, probably, less mathematics courses.

39. **Ilyas**
November 20, 2016

Group theory – right up there in my view as a "preferred" topic to be taught to physicists. Whilst i am here, I'll say thanks for hosting such an interesting and engaging blog for so long.

40. **anon**
November 20, 2016

Jonathan Miller: I agree that some more formal training in programming would be good for many or even most physicists. The reputation 'self-taught scientist programmers' have among software engineers is not entirely undeserved.

41. **Trailmut**
November 21, 2016

Peter, thank you for this illuminating and at times entertaining blog. I did a lot of math and physics years ago at the undergrad level and then dropped out for the outdoors and environmental activism. I would love to revisit some of this stuff. What would you recommend as a good textbook these days on calculus, on differential equations, and on linear algebra. Thank you, again.

42. **Peter Shor**
November 21, 2016

About complex analysis –

Is an area of math that comes up everywhere in other mathematics also likely to come up lots of places in physics? My inclination is to say "yes", but quite possibly I'm wrong.

43. **Mike Weiss**
November 21, 2016

Peter, thanks for reminding us that you took Applied Math 201 in the fall of 1976, our sophomore years at Harvard. By coincidence, I attended the first week of AM201 but then switched to Math 213a, the Math version of the graduate course in complex analysis. Math 213a in the fall of 1976 was the first and only time that John Tate ever taught complex analysis, and he saw it as a grand opportunity to provide his own perspective (not using Lars Ahlfors classic text). At the time we did not know what a privilege it was.

44. **anon**
November 21, 2016

Complex analysis, definitely. You can't even have a coherent discussion of common Green's functions and common Fourier transforms (in electrodynamics, scattering theory, solid state physics, or QFT propagators) without knowledge of contour integrals.

Same goes for group theory and QM. Even a basic discussion of QM conservation laws in my intro grad QM class stumbled when it came to light the students had no background in groups. SO I had to introduce basic Lie groups (as in translation and rotation) ab initio, at grad level. Quite shocking.

45. **Peter Woit**
November 21, 2016

Trailmut,
Sorry, but in my little experience with books for these courses, none stands out as unusual and worth specially recommending. Here at Columbia and many other places, Stewart's Calculus is the text we use. I've always thought that calculus and introductory physics should be taught together, haven't ever seen a book that does that though.

Mike Weiss,

Thanks for that story. Probably it's very good that I didn't do what you did. I got a fair amount out of the Applied Math version, although struggled a bit with that since I wasn't exactly over-prepared for it. I suspect I would have gotten little out of Tate's version, which later in life though would have been great to experience.

All in all, physicists should be wary of the fact that complex analysis can be taught very differently, depending especially on whether the emphasis is on applications (which are marvelous and something physicists should know) or theory (which leads to many deep areas of pure math, but less necessary for physicists).

anon,
I agree completely, and the problem you mention is one motivation for the book I've just finished. The material about translations/rotations and what they have to do with momentum/angular momentum should though be part of the standard physics undergrad courses on mechanics and QM.

46. **Matt Grayson**
November 21, 2016

Mike,
I took 213a the preceding year taught by Ahlfors. That actually seemed like a privilege at the time. Of course, I didn't understand much of it until I taught the stuff ten years later.

Peter,
I was a TA for Math 22 taught by Bamberg and Sternberg. That made a serious attempt to combine second year calculus and linear algebra with physics. I remember them applying cohomology to resister networks and expressing Maxwell's Equations in differential forms.

47. **Michael Comenetz**
November 21, 2016

Trailmut and Peter,
I hope you won't mind if I quote from the Zentralblatt MATH (now zbMATH) review of my own elementary calculus book, "Calculus: The Elements" (World Scientific, 2002): "A mathematician's first reaction to the book may well be, Is this a text by a physicist, or by a mathematician. After delving further, one has the feeling that it is a work by a mathematician still in close touch with physics. … The author succeeds well in giving an excellent intuitive introduction while ultimately maintaining a healthy respect for rigor." A fuller review by Roy Smith of the University of Georgia can be found at the World Scientific website.

48. **Frederick**
November 22, 2016

One of the reason why few undergraduates know about group theory is the relative lack of a good group theory book designed for physics majors. They could have taken a course on abstract algebra, but then they would have to spend time learning about rings and fields, which are not quite connected to physics.

Even the currently available material seems to be too rigorous. It is well known that physicists apply Stoke's theorem on non-compact spaces; physicists are reckless folks indeed.

49. **Jeff M**
November 22, 2016

When I was an undergrad, there was a book, "Group theory in quantum mechanics" or very similar title. I just checked, and it's now Dover, so you can get it for less than $20. It's listed as graduate level, but I used it as an undergrad (I was taking a mathematics modern algebra course at the same time). I remember it being a wonderful book.

50. **paddy**
November 22, 2016

On Group theory: Hamermesh was text I first learned from. Proved to be quite useful even in a mundane area like understanding fluorescence of transition metal ions in crystals. Once again my fly leaf indicates '74 and this time I remember the professor: Glashow.

51. **jc**
November 23, 2016

Frederick,

A very nice "dumbed down" group theory textbook for non-math majors which I found very readable, is:

"Lie Groups, Physics, and Geometry: An Introduction for Physicists, Engineers and Chemists" by Robert Gilmore.

http://www.physics.drexel.edu/~bob/LieGroups.html

Various Links

November 22, 2016
Categories: Uncategorized

A few pre-Thanksgiving items:

- International Journal of Modern Physics A has a new issue with "Featured Topic" part I of a discussion of the proposed Chinese supercollider project. The high profile contributions are from C. N. Yang, China should not build a supercollider at this time, and a response from David Gross: Why China should build the Great Collider. Both I think do a good job of making the case for and against the project, with the central question that of the high cost. If this was a $1 billion project there would be no question it would get done, and if it was a $100 billion project it could never happen. The problem is that the order of magnitude is $10 billion.

  There are some other contributions to the debate, including sensible pro-collider pieces by Yifang Wang and Weimin Wu. There's also a bizarre piece by Henry Tye, making an ad hominem argument against Yang, based on the fact that in 1980 Yang was skeptical about the future of high energy physics. I'm afraid that this doesn't work very well as an argument against Yang, whose prediction of no post-1980 breakthroughs looks unfortunately prescient these days.

- According to the New York Times, one possibly imminent non-HEP breakthrough is a Microsoft quantum computer. Their project grew out of one they funded led by topologist Mike Freedman.
- Last weekend there was a celebration at Caltech of John Schwarz's 75th birthday. No slides or video of talks it seems. I've wondered what Susskind's take on supersymmetry is these days, so curious what might have been in his talk entitled "Supersymmetry and the Limits of What We Know".
- Howard Burton was the founding director of Perimeter, helping to get it off the ground and turn it into the success it has become (Sabine Hossenfelder here notes that the term of the current director Neil Turok is up and wonders who is next). In recent years Burton has been running something he calls Ideas Roadshow, and now has a new blog called In Search of Refinement. He writes about a recent event with Roger Penrose, discussing his new book, and has kind things to say about my review of the book. Ideas Roadshow now has accumulated a significant number of interesting interviews, worth your while if you're looking for high-quality but not free internet content to support.

Update: One more. Yet more private funding for US scientific research. The New York Times reports on the Simons Foundation Flatiron Institute, a planned $80 million/year, 200 employee research institute. The focus will be on computational work, and doing better science by freeing scientists from having to apply for grants.

Comments

1. **Another Anon**
   November 23, 2016

   Neil Turok's term "about to run out" means May 2018. He'll probably just get renewed.

   https://www.perimeterinstitute.ca/news/neil-turok-appointed-second-term-perimeter-s-director-and-lazaridis-bohr-chair

2. **Abbyyorker**
   November 23, 2016

   Paywalled, the whole thing which is strange as its public commentary drawn from unpaid sources. I'd like to see what yang says.

   How come he didn't win two Nobel prizes?

3. **Narad**
   November 24, 2016

   > Paywalled, the whole thing which is strange as its public commentary drawn from unpaid sources.

   The IJMPA? I haven't checked all of the papers, but the Yang was just nagwalled (one has to register).

4. **curious**
   November 24, 2016

   " Chinese supercollider"

   Peter do you think a Chinese supercollider should be funded, or is that money better spent elsewhere?

   From what i read elsewhere they are hoping to achieve 50km radius and perhaps 50-70 TEV collision, which for the price doesn't sound like a large leap over LHC of 14 TEV.

5. **Peter Woit**
   November 24, 2016

   curious,
   I don't know enough about the situation in China to have an informed opinion on this. Yang's argument is that China is still too poor a country to do this, but the situation there is changing rapidly. I would support such a project in the US, don't want to engage in the usual simplistic arguments about government spending on anything (please anyone who wants to do this, resist the temptation).

6. **Jeff M**
   November 24, 2016

   Peter,

   Is there any expectation that you would see anything new at 50TEV? What would the point be of that sort of collider? When people were considering the superconducting collider in the states, or the LHC, the expectation was that you would find the Higgs. I don't see any expectation that anyone is going to find anything at 50 TEV, for that matter, I don't see anything at 500TEV, no?

7. **Peter Woit**

November 24, 2016

Jeff M,
Plans for such a collider typically involve two stages: an electron positron collider in the large tunnel that would be a “Higgs factory” allowing a much more detailed study of the Higgs properties than at the LHC, and a higher energy proton-proton collider. The second one would explore a higher energy range than the LHC, and that’s of value in itself, there might be something unexpected, but also allow measurement of the Higgs self-coupling, again telling us something about the Higgs that the LHC can’t.

8. **Bobito**
November 25, 2016

Private funding for math research may be a poison pill. Someday the government might perceive that there’s no need for public funding …

9. **Casey Leedom**
November 25, 2016

“The New York Times reports on the Simons Foundation Flatiron Institute, a planned $80 million/year, 200 employee research institute.”

Woah, really? That’s an insanely high Burdened Employee Rate. It used to be about % Full Time Equivalent employees per $1M several years ago. In Silicon Valley it’s getting close to 4 FTEs/$1M. Even with that higher rate, $80M/year should fund 320 employees. So they apparently are getting a lot of salary … or there’s a lot of overhead …

Casey

10. **Casey Leedom**
November 25, 2016

[[Sorry, no ability to edit: That should have been “_5_ FTEs/$1M several years ago” …]]

11. **Parisian**
November 25, 2016

Casey,

You need things such as computer clusters, postdocs, etc. to do research. $80M should cover all those things, including paying for electricity. I’m surprised that the budget is not bigger.

12. **Bernhard**
November 25, 2016

I just read both Yang’s and Gross’ papers on the CEPC. It would be interesting if Yang would write a response, as I think Gross made a better job arguing in favor than Yang did against it.

13. **curious**
November 25, 2016

how does chinese collider compare with proposals of a higher energy 28 tev LHC using more powerful magnets?

14. **GoodAndNotSo**
November 25, 2016

It’s wonderful the way Jim Simons chooses to spend his money. Aside from the
math/physics projects, all of which seem to be innovative and well thought-out
(rather than the huge prizes which usually go to make the rich richer and cause a huge media splash), he built a beautiful park near Stony Brook.Those are the only projects I happen to know about, partly because he doesn’t seem to care about getting publicity.

In contrast, his Renaissance Technologies co-founder (and now CEO Robert Mercer
is who we can thank for bringing us Ted Cruz, Breitbart, Bannon, Kellyanne, and Donald. The damage to math/physics in particular, education in general (not to mention 1000 other things) caused by the latter may well outweigh all the good done
by our guy… But who knows? we shall see….

15. **Bill**
November 27, 2016

Special presenter in the math category at Breakthrough Prize Ceremony, https://breakthroughprize.org/News/33 , seems to indicate that the prize will go to a number theorist. Could it be Andrew Wiles, or Yitang Zhang?

16. **anon**
November 27, 2016

I don’t think that having Jeremy Irons as a special presenter necessarily tells anything. I would have been surprised if they didn’t have any actors from The Man Knew Infinity present, no matter who gets the prize.

17. **Marco Renzi**
November 29, 2016

Gross claims that the Higgs could have a substructure. Is there any hint that the Higgs has non-zero size?

Why is looking for substructure f the Higgs a reason to build a collider, whereas looking for substructure of the electron is not?

18. **Peter Woit**
November 29, 2016

Marco Renzi,

There are some reasons to believe the Higgs may not be an elementary particle like others. For example, the Higgs phenomenon is quite analogous to what happens in superconductivity, and there the analog of the Higgs field is a composite one. Also, unlike other elementary particles, the Higgs has self interactions not due to gauge interactions. At this point these self-interactions have not been observed, and doing so would be one goal of a 100 TeV collider.

Electrons are much simpler and much better understood, with purely gauge interactions (+coupling to the Higgs). Their short distance behavior has been measured at LEP to be that expected of an elementary particle down to very short distances. A new collider would likely have a first stage involving an electron positron machine of higher energy than LEP. This would check for substructure to even shorter distances than LEP, but since, unlike the Higgs, the electron is so well understood, it seems very likely that we know how this will turn out.

19. **My MilkShake**
    November 29, 2016

    Hi Peter, I ran across this fascinating article that you will like:

    http://phys.org/news/2016-11-quantum-physics-factor.html

    … will it turn out that one can prove the Riemann hypothesis using techniques from quantum mechanics?…

20. **Frog Leg**
    December 1, 2016

    I saw this intriguing tweet from Peter Koppenberg via Jester, but I haven't heard anything else. Any ideas on the significance of this possible finding?

21. **Anon**
    December 3, 2016

    The Simons Foundation has recently joined the CMS experiment. I'm not sure if this is directly the institute mentioned in the NYT article, but it seems likely.

Breakthrough Prizes 2017

December 4, 2016
Categories: Uncategorized

The 2017 Breakthrough Prizes have just been announced, here are the winners and a few comments. Note that I'm leaving the usual singing of praise of the many virtues of the laureates to others, since there should soon be a lot of such stories appearing. Instead I'll concentrate on issues that aren't getting as much attention.

Already announced back in May, there is a special Breakthrough Prize for the LIGO collaboration: \$1 million to be split by Drever, Thorne and Weiss, \$2 million to be split by the other 1012 members of the LIGO collaboration. Quite likely Drever, Thorne and Weiss will get the Nobel Prize next year (the LIGO result was published too late for consideration this year). A very good thing about the Breakthrough Prize though is that it is given to the entire collaboration, with awards going to every one. They have done similar things in the past, with awards to the LHC experiments, to neutrino experiments and to the accelerating universe supernova experiments.

The Nobel Prize in Physics and most other such prizes are never awarded to a group, just (at most three) individuals. In an era of large scientific collaborations this isn't fair, with all the recognition and prize money going to some small set of people, nothing to anyone else. I'm glad to see that, for these experimental prizes, the Breakthrough Prize has been following a different model.

The 2017 \$3 million Breakthrough Prize in mathematics goes to Jean Bourgain of the Institute for Advanced Study in Princeton. I'm not particularly familiar with his work, you'll have to read about it elsewhere. He is a well-known figure in the math community, already a recipient of many prizes including the Fields Medal, Shaw and Crafoord prizes.

I've never been convinced that this mathematics $3 million prize is a good idea, since it typically goes to someone like Bourgain who, besides being an essentially randomly chosen lucky winner from a sizable pool of similarly distinguished mathematicians, already has prize money and a very well paid position with minimal responsibilities. This isn't going to help him do better mathematics. A much better way to spend the money would be on endowing new permanent academic positions in mathematics, allowing more talented young people to have a career in mathematical research.

The philosophy behind the Breakthrough Prizes, very visible in the glitzy award ceremony (you can watch it on the National Geographic channel this evening, 10pm EST), is that scientists don't get the kind of fame and stardom they deserve, so Milner and Zuckerberg are going to help fix this. What motivates good mathematics though is something very different, and bringing to mathematics more of the Hollywood star system is not going to improve mathematics research. In recent decades much of US society has moved to a brutal winner-take-all system. While our Silicon Valley overlords have flourished under this, I don't think their bringing more of it to scientific research is a good idea.

While one can argue that the huge checks to mathematicians don't have any particular negative effect, the situation in theoretical physics is quite different. Since the original laureates chosen by Milner, the yearly prizes that have gone to theorists (as opposed to the experimental prizes mentioned above) have all gone to string theorists (there was also a special prize given to Hawking). First there was Polyakov, then Green and Schwarz, and this year the $3 million Breakthrough Prize in Physics goes to three more string theorists: Joe Polchinski, Andy Strominger, and Cumrun Vafa.

In the case of string theory, I don't think one can seriously argue that the field suffers from a lack of public attention. Mountains of hype about string theory have been produced in the last 32 years, seriously damaging the field of theoretical physics. This year's prize adds to that mountain, with hype-ridden ~~citations~~ backing the glitzy ceremony and million dollar checks. The language tries to turn physics research Hollywood: for instance, relativity and quantum theory are "the two superstar theories of modern physics".

Perhaps the worst aspect of these prizes and ~~citations~~ is that they often hype and reward failed theoretical ideas: if your ideas work maybe you'll get part of a \$1 million Nobel Prize, but if they fail, as long as they're about string theory, you'll get part of a \$3 million Breakthrough Prize. The ~~citation~~ for Strominger includes this about string theory:

> Andrew Strominger played a major role in its emergence when he showed that it not only reconciles quantum mechanics with gravity, but can also contains within it the other observed particles and forces.

This refers to Strominger's early work on Calabi-Yau compactifications, while not mentioning that this idea has never worked out.

These prizes are often awarded for ideas about the black hole information paradox, independent of whether these ideas work. Maldacena's citation from 2012 tells us that he got the award partly for "resolving the black hole information paradox", and the Strominger ~~citation~~ tells us that "His work hints at a solution to the famous 'black hole information paradox'". Polchinski is rewarded for a

> big idea, deriving from the principles of quantum mechanics: 'firewalls' –blizzards of high-energy particles around black holes. The existence of firewalls would signal a fault line in the foundations of physics: at least one of the two superstar theories of modern physics – relativity theory and quantum theory – would have to be incomplete at a fundamental level.

Anyone reading this is unlikely to figure out that the significance of Polchinski's "big idea" is that it purports to show that the solution to the paradox supposedly given by Maldacena actually doesn't work (not surprising, since it was never more than a speculation). If you're a string theorist, you don't actually need to solve a problem to get a prize: speculation about what the solution to a problem might be is good enough, as is finding problems with the speculations of other string theorists. This sort of thing does nothing to improve the difficult situation of current theoretical physics, quite the opposite.

Tomorrow there will be a symposium at UCSF featuring 15 minute "TED-style" talks giving "pragmatic versions" of what could be done during the next ten years. For string theory, the blurb for the talk or talks is

> Medium-term goals of string theory, from resolving the paradoxes of black holes to estimating the lifetime of the universe.

After a cycle of two prizes for resolving the information paradox and then unresolving it, I suppose it's a reasonable goal for string theorists to go through another cycle or so during the next ten years. The idea of using string theory to "estimat the lifetime of the universe" anytime soon goes beyond any of the usual hype, so we'll have to see what that's about tomorrow.

Update: Smaller \$100,000 "New Horizons" prizes, some split in various ways, went to 6 theoretical physicists (Asimina Arvanitaki, Peter Graham, Surjeet Rajendran, Simone Gombi, Xi Yin and Frans Pretorius) and 4 mathematicians (Mohammed Abouzaid, Hugo Duminil-Copin, Ben Elias and Geordie Williamson). Congratulations to all, especially to the Columbia contingent (Mohammed is a faculty member now, Ben was a grad student here, and TA one year for my representation theory course).

Update: Some of the language quoted above as part of the citations for the string theory awards was actually in a separate section of materials distributed to the press called "Contributions", which gave more specifics of what the award was being made for. I've changed the wording above to more accurately reflect this.

Update: Just watched the Breakthrough Prize ceremony on TV. Very nice short portrait of Jean Bourgain and his work, and remarks by him. The string theorists were right at the end, and the DVR cut off in the middle of a clip of outrageous hype from them (I guess the ceremony went slightly longer than scheduled).

Update: Nature has coverage of the prizes here, emphasizing the award to Polchinski for firewalls, with some justification from Milner.

Update: Livestream of the symposium is here. Polchinski and Strominger will be talking about black hole information paradox/string theory, Vafa will describe "a research program for putting rigorous bounds on the lifetime of our universe, by studying the range of possibilities permitted by the laws of string theory."

Comments

1. **anon**
   December 4, 2016

   A nice detail that shows how much Breakthrough Prize ceremony really is about scientists is that National Geographic Channel managed to misspell Vafa's first name as 'Comrum' in its tweets.

2. **Jeff M**
   December 4, 2016

   Back when I was a young mathematician I used to tell people I was glad there wasn't a Nobel in math, since the Nobel's only harmed the sciences. The Breakthrough prizes are of course much worse. Terrible use of the money, all around.

3. **CIP**
   December 4, 2016

   Leo Szilard wrote a story in which a guy who was thawed out after many years frozen, finding, himself now immensely rich, pondered the problem of how to spend it. He decided that he wanted to slow down scientific progress, and concluded that the best way would be to fund a bunch of scientific prizes that would get all the promising scientists chasing them. I think the story appeared in his book, The Voice of the Dolphins.

4. **David Roberts**
   December 4, 2016

   I wonder if Bourgain will donate his prize money to the Breakout graduate fellowships?

5. **Bee**
   December 5, 2016

   Great, then when do I get my prize for showing that the firewall argument is wrong? I mean, I feel like I'm starting to sound like one of the cranks in my inbox. It's demonstrably a wrong derivation. The four assumptions they claim are inconsistent are not inconsistent. It's an unnecessary fifth assumption, hidden in the text, that makes them inconsistent. It's mathematically provably wrong. I even have a counterexample. And it's all published. Hello, hello, anybody? We can stop talking about it.

   (This isn't to say that Joe doesn't deserve a prize, but maybe the firewall wasn't exactly the thing to highlight.)

   Leaving aside my puzzlement about the world of theoretical high energy physics, I've heard various rumors that LIGO was nominated in time for the Nobel last year. Not sure what to conclude from that if true.

6. **Jess Riedel**
   December 5, 2016

   What is the point of dividing an award up among 1k people? Each one gets $2k and…something on their CV which is immediately worthless by dilution? It resembles a useless cum laude distinction.

   Prizes are meant to give folks status, but status is zero sum. Yes, it may not make sense to give an award to the leaders of a huge collaboration if those leaders did nothing extraordinary. But why not simply refrain from giving an award out if no one did anything extraordinary?

   The only explanation I can think of is that such awards would function as a way, not to incentive physicists, but to highlight to outsiders the best achievements by physicists as a group.

7. **Manfred Requardt**
   December 5, 2016

   I also showed that the firewall argument is wrong.

8. **M**
   December 5, 2016

   The possibility of adding D-branes here and there gave the final shot to the hope of getting predictions from string theory. This was an important development that deserves a prize.

9. **anon**
   December 5, 2016

   Regarding LIGO and Nobel, I have also heard that the three LIGO pioneers were nominated. It would be strange if they hadn't been, there are a lot of people who can nominate and LIGO is a big collaboration; I would think that some possible nominators already knew about the coming announcement before the deadline.

   It's hard to imagine LIGO not getting a Nobel. Maybe the committee just likes to avoid the appearance that they consider nominations based on rumours or inside information, so they want the results to have been public before the nomination deadline.

10. **Peter Woit**
    December 5, 2016

    Jess,
    I'm sure the LIGO people do each appreciate the $2000, and the small amount of personal recognition. What's wrong with it?

    To the extent that you're right that status is zero-sum, that's part of the problem with giving huge awards to a couple people, nothing to everyone else, what I was trying to point to by calling this a "winner take all" reward system. By deciding to inject these huge awards into the status system, Milner and Zuckerberg are not just making a few people "stars", but simultaneously lowering the status of everyone else (while at least making some effort to ameliorate the situation in the case of experimental awards).

11. **Peter Woit**
    December 5, 2016

    Bee,
    Thanks, I should have at least pointed out that there's no consensus about what this argument even means. About the huge check though, I'm afraid I have to point out to you and Requardt that you're not string theorists, and thus ineligible.

12. **Peter Woit**
    December 5, 2016

    M,

    Yes, you've got it right. In his acceptance remarks

    https://breakthroughprize.org/Laureates/1/L3795

    Polchinski lists "D-branes, the string landscape, and the black hole firewall" as his contributions to the subject, and states that a unified theory is the main motivation. He explains that since the landscape shows things are "veiled by the randomness of the Multiverse", that's why you can't make predictions.

    Interestingly, the people who awarded him the prize don't seem to agree. While he thinks he deserves the prize because of the landscape, that work is not mentioned at all in the details about his "Contributions" given to the press.

13. **Bill**
    December 5, 2016

    Wow, Breakthrough Prize in Science Fiction?

    Sad to see math prize becoming another lifetime achievement award. Although Bourgain has been very active in recent years, the award is clearly not for some particular work. Obviously, previous winners do not have the courage to pick among recent real breakthroughs.

14. **masmadera**
    December 5, 2016

    Status is a zero-sum game within the community. The award however focuses on outside-community recognition and in this regard I believe that it is working moderately well. The prize is almost as well advertised as a Nobel. The winner-take-it-all is very widespread in American mentality. If this is a necessary/sufficient lure to attract young bright students that otherwise would pursue other careers I don't know, but the prize is a good addition in that it seeks to provide celebrity status to working scientists, as opposed to the so many science communicators and popularizers that already have this status. Hence, no wonder that many winners (particularly physicists) share in some regards the hype- or over-simplification characteristics of their fellow "science" (communicators) stars.

15. **Bill**
    December 5, 2016

    The idea of attracting bright young students into science is a bit ridiculous. This and all other awards do more damage at much later stage by disrupting natural development of ideas. People get awards, get positions at top universities, attract better Ph.D. students and postdocs, while publishing mediocre papers in top journals, etc. This is how some areas get overemphasized and overdeveloped, while some perhaps more interesting long term research gets marginalized.

16. **Marco**
    December 5, 2016

    As much as I appreciated the idea of a prize given to all members of a collaborations (and I know for a fact that the colleagues of my lab working on LIGO/VIRGO do appreciate both the money and the recognition), I'm afraid I have to correct you. There was no award given to the LHC experiment collaborations in a way even close to what is done in 2016 with LIGO/VIRGO. The 2013 Special Breakthrough Prize was directly given to a few individuals (current and former ATLAS and CMS Spokespersons) but not to the collaboration members (then some of our beloved spokespersons decided to use that money to fund a scholarship so that the collaboration "at large" can profit, but this is another – enlightening – story).

17. **Peter Woit**
    December 5, 2016

    Marco,
    Thanks. I had forgotten that it was only for the two experimental prizes after the LHC one that any money/recognition went to the bulk of the collaborations.

18. **anon**
    December 5, 2016

    Some of the criticism is getting a bit unreasonable. I agree that there would be better ways to support science with the millions they are spending, but it seems that some people are claiming that these awards are the worst threats to science.

    Bill, I'm getting curious: who would've been a (much) better recipient of the mathematics award than Bourgain?

19. **Peter Woit**
    December 5, 2016

    anon,
    I'd rather not host a discussion here of the relative merits of all research mathematicians. I think Bill was just making a comment about an interesting question: what is the criterion for choosing a math prize winner? The description of the prize just says "rewards significant discoveries" making it sound like it is aimed at rewarding specific results ("breakthroughs") and that fits with last year's award to Ian Agol, but the Bourgain prize looked more like an award for a body of work. So, still unclear what their plan is…

20. **Bill**
    December 5, 2016

    Bourgain is not a bad choice, his (even recent) research is clearly at the absolutely highest level. The best choice would be not to have the award. Second option would be to give instead 10 Horizon prizes worth 300K each. If you must have current format — Yitang Zhang, Scholze, Hacon-McKernan, … many many more come to mind.

21. **anon**
December 5, 2016

For what it's worth, I think that if a very big award must be given to someone, a life-time award to a senior figure is less damaging than rewarding some new breakthrough. For new breakthroughs I think smaller awards, like the "New Horizons" prizes make more sense.

22. **anon**
December 5, 2016

I didn't notice Bill's last comment; I actually agree with what he says.

23. **masmadera**
December 5, 2016

As much as attracting young bright students into science might feel ridiculous to someone (!?) this is one of the main purposes of popularizing science and the role of beloved science programs in TV in luring the attention of bright (or ambitious) kids is well attested. With regards to "People get awards, get positions at top universities, attract better Ph.D. students and postdocs, while publishing mediocre papers in top journals, etc." I think that Bill has the order totally reversed, at least with respect to the Breakthrough Prizes. I didn't know that Joe Polchinski, Andy Strominger, and Cumrun Vafa were in need of a position in a top university.

24. **Lars**
December 5, 2016

"The Breakthrough"

The Breakthrough Prize
Has broken through
It's on the rise
And in the view

Though Nobel tolls
For just a few
The Breakthrough prize
Awards a crew

25. **Lars**
December 5, 2016

Though Nobel tolls
For just a few
The Breakthrough doles
To quite a crew

26. **Bill**
December 5, 2016

masmadera, I wasn't talking about these life time achievement awards. And when some field gets hyped, even lesser people end up at top universities. But actually I agree with you — prizes at every level often go to people and students of people at top universities because of connections.

I am kind of glad they gave it to string theorists again. The prize is discredited before it had a chance to take off the ground.

27. **Michael Hutchings**
December 5, 2016

It would be nice if they had a million dollar prize for people who haven't previously won some other million dollar prize.

28. **Gregor Samsa**
December 5, 2016

anon,
better recipients in mathematics would have been Atiyah, Deligne, Manin, Milnor, Suslin, Tits or Serre. (And as two of my candidates show, my choice is not dictated by Bourgain's Belgianness…)

29. **David Appell**
December 5, 2016

Jess Riedel says:
"What is the point of dividing an award up among 1k people? Each one gets $2k…."

$2k isn't trivial, for graduate students and postdocs.

Plus, they get to put it on their resume.

30. **Jess Riedel**
December 5, 2016

David Appel: Building LIGO was a multi-decade project, so $2k is really like $100/year. Insofar as graduate students are getting $2k, that just speaks against awarding everyone in the collaboration equally; it's like a participation award, or the "cum laude" on a Harvard degree. Lots of people get it, so it's mostly ignored.

Peter Woit: I think it would take us too far afield to argue about the foundational purpose of awards, on which we probably disagree. But it seems to me the much important criticism here is what sorts of accomplishments the awards are recognizing, rather than the inequality in award amounts.

31. **Mike**
December 6, 2016

Its an interesting general question how to recognize scientific achievement in a large collaboration. The spokespeople are often too narrow a set and are not always the ones who made crucial technical breakthroughs. On the other hand, not all 1k+ members made equal contributions. This problem is only going to get worse as science becomes more of a team effort so maybe one has to rethink some basic assumptions. Perhaps some kind of two-tier authorship structure is possible, although that of course can also lead to conflicts.

32. **anon**
December 6, 2016

"The prize is discredited before it had a chance to take off the ground."

So accurate. With both the senior and the junior prizes (in physics), it is easy to trace very short intellectual paths to a very small number of people at the Institute for Advanced Study in Princeton.

33. **Conrad**
December 6, 2016

Who has recently (late 2015!) proved the fairly famous outstanding Vinogradov main Conjecture from the 30's? (while it's fairly technical, the gist of it is that a very general of set of Diophantine equations have the expected bounds on the number of solutions – expected by the same heuristic randomness arguments "integers do not conspire subtly" that underlie all the main conjectures in NT – integers obviously conspire since if n is even positive, n+2 cannot be prime but the heuristic says that such are essentially only conspiracies around)

Clearly qualifies for a recent breakthrough I think

34. **Bill**
December 6, 2016

Why Guth and Demeter are not getting part of this award then?

35. **P = NP (Peace = Nobel Prize)**
December 6, 2016

As a Nobel Prize winner (Peace Prize 2012, 2.5*10^-9 part) I certainly support awards distributed to groups. I have not yet received my portion of the award money, I'm sorry to report.

On a more serious note, I do, however, not agree that scientific prizes should be used to lure young people into attempting a career in science. It doesn't seem quite ethical.

36. **Lars**
December 6, 2016

"What is the point of dividing an award up among 1k people? Each one gets $2k…."

If any of these folks thinks that $2K is a mere pittance, I would be happy to take said pittance off their hands. I'll even pay the taxes on it.

They can leave their name and contact information on this blog and I will get in touch.

37. **Low Math, Meekly Interacting**
December 6, 2016

In the (albeit ethically challenged) for-profit world, when the company does well, they often give everyone some kind of a bonus. Not everyone deserves to get anything. Almost no one who deserves something gets a bonus that is commensurate with their contribution, high or low. People complain about this bitterly. If I know a single person who has returned the money or donated it all publicly to a worthy cause in protest, I will eat an earthworm, raw and without condiments.

Something typically is better than nothing. Token recognition isn't worthless, however irksome the inequities of real life. I think it's great to put more pressure on the Prizes to do a better job of recognizing big teams than they're doing, but I think that aspect of the Breakthrough Prize is a big step in the right direction.

38. **anon**
December 6, 2016

I think you guys here are a bit deluded concerning the fundamental physics prize. The people who won these awards this year and in the past (both junior and senior) are highly respected for their fundamental contributions and the intellectual fertility they have brought to the fields of cond-mat, hep-th, hep-ph, astro-ph and gr-qc. They are known and respected by almost everyone active in the field, and these are tens of thousands of people, surely not all of them with direct links to Princeton. If you don't believe this you can try to setup some opinion polls. And this is what matters. You can't argue that there is conspiracy which involves 10000 people. It's true that there are many other deserving people who have not yet gotten the prize but people who got are truly top (and hopefully other deserving people will get their prizes in the future).

39. **Peter Woit**
December 6, 2016

anon,
I think if you poll groups of physicists and ask them whether they would choose six string theorists for the first three $3 million prizes in theoretical fundamental physics, you're only going to get a non-zero number in agreement if you are polling a string theory group (or, maybe a group of initial award winners chosen by Milner and dominated by string theorists…).

40. **Bill**
December 6, 2016

Q1: Do the winners of the 2017 Breakthrough Prize in Fundamental Physics, interpreted as a lifetime achievement award, deserve the prize as much as any other physicist you can think of (excluding previous Nobel prize laureates)? Vote here:

http://vote.pollcode.com/75474954

Q2: Do the winners of the 2017 Breakthrough Prize in Fundamental Physics, interpreted as an award for work in the last 10-15 years, deserve the prize as much as any other physicist you can think of? Vote here:

http://vote.pollcode.com/64254698

41. **anon**
December 6, 2016

Bill: Your poll in incorrect, on two counts. First, you must poll the experts in fundamental physics, which are people regularly publishing in cond-mat, hep-th, hep-ph, astro-ph and gr-qc. Amateurs are excluded, sorry. Second, it might be that every one of us has a personal hero who we would put higher than this year's prize winners. This proves nothing, as these sets of personal heroes might be highly nonoverlapping. I only claimed that this years winners are highly respected and it's totally non-controversial in the community that they got it – this is what needs to be polled.

42. **anon**
December 6, 2016

Peter: it just so happens that this prize is called "fundamental physics prize", and that string theory is grappling with fundamental physics questions. It would be surprising if you found 6 atmospheric scientists among the winners, but 6 people who worked on string theory is OK. Also you can't dismiss as 'string theorists' people like Witten or Polyakov or Maldacena who command all of theoretical physicists. Working from time to time on string theory is their choice, just like at other times they can be seen working on issues in condensed matter physics or QFT or cosmology or what not. There are also many people in the group of winners who have nothing to do with string theory nor IAS, certainly more than half by now. Since this years winners were presumably chosen by majority voting, you can't dismiss it as Princeton people who vote for their friends.

43. **Jeff M**
December 6, 2016

I am really curious what the "fundamental contribution" are that were made by the winners? What was the last experimentally verified prediction in HEP? The top quark? In 1973? The winners this year probably weren't even in high school then. Sure, they spit out papers, but what from those paper is established physics? It's certainly possible something done by one of the winners might eventually become real physics, but personally I'm willing to bet very little does. Maybe nothing. I have a feeling 100 years from now they will all be a footnote, sort of like all the theorists who spent decades working on the aether.

44. **Peter Woit**
December 6, 2016

anon,
The first nine winners were chosen by Milner personally, and given his own lack of expertise he made an understandable choice to choose a lot of IAS people. The six theorists chosen since then (Polyakov, Green, Schwarz, Polchinski, Strominger) are not so much Princeton-centric (Princeton now has a shortage of string theorists without million-dollar checks), but are uniformly strongly self-identified string theorists. I don't think polling is needed to know that it's an absurd claim that it's "totally non-controversial in the community" of people in fields like hep-ph, cond-math, astro-ph and gr-qc that choosing only string theorists so far makes sense.

I don't know what the voting system being used is, but obviously there now is only one very large homogeneous block amongst the people voting, and it looks like they've decided to vote for their own tribe. Put differently, for instance, among this large group I think there's at most one condensed matter theorist (Kitaev), so not surprising that condensed matter theorists are not being chosen.

Your conviction that well-known string theorists "command all of theoretical physics" and do the best work in fields like condensed matter and cosmology is one you might want to discuss with theorists in those fields to find out what they think.

45. **anon**
December 6, 2016

Dear Jeff M. May I ask you, are you are a practicing physicist, and in which field? I am, for example, a staff member of the CERN theoretical physics department. So I can presumably tell a valuable theoretical contribution from nonsense. Concerning your questions about verified predictions and when they were made. Inflation was developed in early 80's, Weinberg's discussion of nonzero cosmological constant was in 1987. MSW effect was first discussed in the 80's. There are tons of less dramatic predictions in HEP which are made and verified every day at LHC and other colliders.

46. **anon**
December 6, 2016

Correction: "command all of theoretical physicists"->"command all of theoretical physics" 🙂

47. **Johannes**
December 6, 2016

Peter,

when will the results of this year's LHC searches for supersymmetry be announced? Are there already rumors?

48. **Bill**
December 6, 2016

anon, that's why I included "close enough" option. If you have a personal hero but think that the winners are deserving and highly respected, vote close enough.

If the Fields Medal was almost always given to, let's say, algebraic geometers then it would have no credibility, although it would still be a much more reasonable situation than giving all fundamental physics prizes to string theorists. Give me an example of one great idea of Polchinski that is relevant in physics in some proven way, or at least some great theoretical calculation or technique that influenced other developments that are relevant in physics in some proven way? Or even genuinely influenced great developments in mathematics?

49. **Peter Woit**
December 6, 2016

Jeff M,

There's a case to be made that because of the lack of experimentally verified advances, you now need to be rewarding the best ones that are promising but not yet verified. I don't see though the justification for what the Breakthrough Prize people are sometimes doing, which is rewarding failed ideas, ones that are much less promising now than when they came out.

A very odd thing about these prizes, especially this year's physics prizes, is that there is no explanation publicly given of what the prizes are rewarding (just a meaningless hype phrase "transformative advances in quantum field theory, string theory, and quantum gravity"). The Nobel Prizes (and most others) come with a very detailed and carefully written extensive description of the work that is being rewarded by the prize, but there is nothing like that here. The press did receive before the announcement a document that described "Contributions" for each physicist. I quoted a bit of this, and I've seen some of it appear in places like the UCSB press release about Polchinski. But this material was very poorly written and hype-ridden, with no detail. This is an odd situation, I would have expected that part of the job of the committee choosing these people was to write up an explanation of why they were chosen.

50. **anon**
December 6, 2016

Bill: I'm not gonna vote because I don't want to see my vote drowned in the tons of amateurs frequenting this blog. Make another poll where the participants will have to testify, on honor, that you published say more than 10 papers in the above-mentioned arxiv categories over the course of your life.

I won't enter into a discussion of Polchinski's contributions because any blog discussion of this type will not do justice. But you can easily go to INSPIRE, look up the list of his papers, check out the better cited ones and you will see that many of them don't even have to do with string theory, but with various aspects of QFT.

51. **Peter Woit**
December 6, 2016

Johannes,
This is off-topic. Results should be announced in March at the Moriond conference. I've heard no rumors of any results indicating SUSY.

52. **anon**
December 6, 2016

This is anon replying to anon (just to clarify matters).

I did a quick count. Of the 22 junior winners in particle (excluding the 3 CMT people), I count 19 where (if you are in the subject and know the intellectual/social connections) it is easy to draw a short line to Princeton.

With the senior people, almost everyone who has won the prize has made significant, deep and lasting contributions to fundamental physics. But it's a narrow and impoverished view of fundamental physics that would weight prizes so heavily to string theory and allied topics (and I work on string theory, so I'm not anti- the subject).

The statements 'people who get these prizes are respected in the subject' and 'the prizes are part of a patronage network' can both be true (and are).

53. **anon**
December 6, 2016

Bill: for one example, look at Polchinski's work on effective field theory in condensed matter (for one example)

anon: being slightly mischievous, I am trying to think whether you are uniquely identifiable from being (a) a staff member at CERN-TH and (b) willing to list Weinberg's anthropic discussion of the cosmological constant as on a par with inflation and MSW

54. **Bill**
December 6, 2016

Why Phil Anderson didn't get the Breakthrough prize for Anderson-Higgs mechanism? Of course, we will quickly run out of such obvious examples, but I am sure there are many more good ones. Take this year's Nobel prize winners, for example.

In string theory, it seems that the rules are quite different from the rest of physics. If you have large enough theoretical toolbox to play with and a good imagination, you can keep proposing various "fundamental physics" ideas. There is no formula or mechanism to test in experiments or in simulations, or even for mathematicians to break their back with. You propose some paradox, and come up with many not necessarily compatible ways to "solve" it.

55. **Bill**
December 6, 2016

anon, if you give "Effective Field Theory" as an example, one can come up with a long list of people in condmat and related fields who are by an order of magnitude more deserving. This just proves my point.

56. **anon**
December 7, 2016

Bill: because no prize was given for the Higgs mechanism. Also: Anderson seems to be confused about the difference between the Higgs boson and the W-boson (see the discussion in his "Basic Notions…" book).

anon who replied to anon: I'm not sure how you counted. For the record I believe Hartnoll, Casini, Huerta, Ryu, Takayanagi, Flauger, Graham, Arvanitaki, Rajendran, Yin have no particular connection to Princeton. As a connection I count PhD, postdoc or a job. I don't count vague `intellectual connections' because this is not well defined, and if you start stretching it too far, we are all related to Eordos, to Hilbert, to Adam and Eve and perhaps even to Rovelli and Smolin in just a handful of steps. I might be wrong on one or two of the above winners, still, this makes it close to 50% total outsiders. Not bad at all, given that the rest 50% is also top and it would be unfair to exclude them just because they happened to do their PhD or one of their postdocs at top institutions as Princeton or IAS. I have to stress once again that the selection committee by now includes so many physicists from so many fields that string theorists and Princetonians do not hold a majority. Just nominate (many people complain but don't nominate) your top choices and hopefully they will be among next year's winners. And it's excluded you can identify me from the verified predictions I mentioned. At CERN-TH we are proud to be universalists.

57. **anon**
December 7, 2016

Bill : "There is no formula or mechanism to test in experiments or in simulations, or even for mathematicians to break their back with." There are tons of formulas that came out from string theory that mathematicians are breaking their back with now as we speak.

58. **anon**
December 7, 2016

tulpoeid: why boosted? If everyone hates this prize as much as the readership of this blog seems to suggest, then having been awarded it is going to be a handicap, not a boost, in the race for a Nobel. Surely the Swedish Royal Academy is going to side with you guys and will punish LIGO.

59. **anon**
December 7, 2016

Peter:
I did not say they do their "best" work in cond-mat or cosmology. But they do do very important work in those fields as well. Pity that you assume that I don't talk to people in those fields. Not only do I do, but I also regularly attend seminars and follow the literature on these subjects. It's a very rewarding experience and I recommend it to everyone. I hope you can say the same about yourself. Now, concerning QFT which was also on my list but which you dropped (is it because it does not fit your narrative?), some of the very best work of Polyakov, Witten, Maldacena is in QFT. They are as much field theorists as they are string theorists. Had they not written a single string theory paper, still they would have more than deserved the fundamental physics prize. It might be that from the point of view of this blog the moment you start thinking about strings, you get negative points which cancel even your previous achievements. But the community does not think the same.

60. **anon**
December 7, 2016

Just to be concrete. Here's a recent paper by Maldacena, Shenker and Stanford which is valued by cond-mat researchers as much as any other recent paper in that subject: http://arxiv.org/abs/1503.01409. Proof? It has been featured this month on http://www.condmatjournalclub.org, a site run by cond-mat researchers for cond-mat researchers.

61. **Peter Woit**
December 7, 2016

I've just resisted the temptation to respond to the flood of misinformation from anon at CERN-TH and urge everyone else to do so. This discussion with him has become an unenlightening waste of time so I'll cut it off. Other discussions with something new to say are encouraged.

62. **Hans.Neumaier**
December 7, 2016

In 1912, Nils Gustaf Dalén won the Nobel Prize in Physics "for his invention of automatic regulators for use in conjunction with gas accumulators for illuminating lighthouses and buoys". His predecessor was Wien (of Wien's law) and his successor was Kamerlingh Onnes (discovery of superconductivity). Fermi got the prize in 1938 for a mistaken publication – but deserved it. Also the Nobel Prize has strange sides.

But still, they are much less strange than the Breakthrough prize, where dozens of string theorists get prizes for a theory that predicts supersymmetry – which, as data show and Veltman writes, is "a figment of imagination". Yes, string theorists are influential, very much so. Nobody denies this. Not even Woit… But this influence does not make string theory correct or the prize deserved. We are back in times where influence (authority!) is valued more that experimental data. Didn't physicists fight for centuries against the misuse of authority and in favor of experiment?

63. **Bernhard**
December 8, 2016

"A very good thing about the Breakthrough Prize though is that it is given to the entire collaboration, with awards going to every one. "

Although they didn't do this with the special prize for the Higgs they gave to Gianotti and Incandela in 2012. Back then, they said the prize would be donated to support a grant or something like that but in the end they decided it was best to keep in their pockets, as far as I know.

64. **Bill**
December 8, 2016

Bernard, wasn't it given to 7 or 8 people, not 2? So only ~375K per person, although it's a shame if they kept it in their pockets…

65. **Bernhard**
December 8, 2016

Bill,

The special prize due the Higgs was shared by F. Gianotti, J. Incandela, P. Jenni, T. Virdee, G. Tonelli and L. Evans. As far as I remember, this was a 3 million prize shared among these people, so actually 500k.

Whatever the sum though , I think it was substantial and shameful that they did not return this money back to young people at their collaborations, even though some like Gianotti, claimed they would.

It's unjust enrichment if you ask me.

66. **Bill**
December 8, 2016

Bernard, is it possible they used the money to to fund some research activities, students, postdocs, etc., and this information is just not on the internet?

67. **Peter Woit**
December 8, 2016

Bernhard and Bill,
It looks like there was some use of the prize money for scholarships, see
https://breakthroughprize.org/News/5
and
https://breakthroughprize.org/News/10
about an ATLAS PhD Scholarship program (Jenni and Gianotti), a CMS Fundamental Physics Scholarship (Incandela) and training for African high school teachers (Virdee). There's also mention that

"Virdee is working on, along with the other CMS recipients of the FPP, is setting up a series of prizes to recognize and assist young people within the CMS collaboration who have made significant contributions."

I don't know if that ever happened or if anything similar was planned for ATLAS.

68. **Peter Woit**

December 8, 2016

More about the CMS part of this here, which includes a 20,000 franc scholarship to pay for someone to come to CERN for a year.
http://cms.web.cern.ch/news/cms-fps-new-scholarship-support-young-scientists

69. **Bernhard**
December 8, 2016

Peter,

thanks. I looked for it before (long time ago) on their site and didn't find it . It's good to see that at least part of the prize was put to good use by at least some of the contenders.

70. **Rob Meyer**
December 26, 2016

Here are a few more links to follow up on the last thread:

http://www.mathunion.org/cdc/grants/imu-breakout-graduate-fellowship-program/awardees-2016/

https://www.youtube.com/watch?v=euYK4Q4-40k

http://www.bbc.com/news/science-environment-23809234

Anomaly!

December 6, 2016
Categories: Book Reviews

Tommaso Dorigo's new book [*Anomaly! Collider Physics and the Quest for New Phenomena at Fermilab*]() has just become available. I highly recommend it to anyone with an interest in high energy physics who wants some insight into how collider experiments are done. Dorigo is a well-known blogger, with the [best (as well as most entertaining) blog]() there is about the experimental side of high energy physics. If you're not following his blogging, you should be.

The nominal topic of the book is research conducted at the Tevatron by the CDF experiment during the 80s and 90s, into the early 2000s. Some of the specific well-known CDF results discussed in detail include measurement of the Z mass and the discovery of the top quark. The material about the top quark discovery comes closest to the kind of thing you find in other books. It's a very well-done insider's account of the story of an important discovery. I don't know of a better place to read about the top quark search and how it finally succeeded.

One of the most unusual aspects of the book, in evidence in the top quark section and throughout the rest, is that it goes much deeper into an explanation of how collider experiment physics analyses are actually done than is usual in a popular or semi-popular book. Besides the insider's local color and insights into the personalities involved, there's quite a bit of discussion of the technical issues of the subject. This is a subject involving thorny issues of how best to reconstruct the properties of particles coming out of collisions, and finding clever new ways to deal with these while avoiding subtle pitfalls is a central problem, one outsiders normally don't get to hear about.

The other unusual aspect of the book is that it doesn't just discuss success stories of striking discoveries (of which there hasn't been much between the top and the much later Higgs discovery at the LHC). One of the main activities of the field has been the search for "anomalies", experimental results that disagree with the Standard Model and point to new physics. The problem is that finding anomalies is common, but they almost certainly will turn out to be due to some problem with the experiment or its analysis (such problems are always much more likely than revolutionary new physics).

One of the stories of an anomaly described extensively is that of an excess of high transverse energy jets, something that one might expect to see if quarks had some substructure ("preons"). Here the problem turned out to have a lot to do not with the experimental result, but with the theoretical modeling of what to expect from QCD. Another example is the story of "superjets", events involving a W and 2 or 3 jets, with unusual properties.

For the "superjets" and for other anomalies, a favorite explanation was to invoke supersymmetry, since supersymmetric models predict a large range of different kinds of new particles, and one might hope that any anomaly is due to one of them. Dorigo has a few stories about theorists I hadn't heard, in particular that of a fall 1995 letter signed by many prominent theorists (except Howard Georgi, who refused to sign). Despite ongoing efforts to look for superpartners, all of which had been unsuccessful, the feeling of the theorists was that the Fermilab experimenters weren't trying hard enough. The letter was sent to the Fermilab director as well as the CDF and DZERO spokespersons. It explained what a great idea supersymmetry was, and ended with

> We, the undersigned, believe that Fermilab has unique detection possibilites for supersymmetry, and urge you to direct your laboratory's efforts in that direction, and ask the leaders of the collider detector collaborations to intensify their search for massive superpartners.

For more than two decades now, such searches for superpartners have been one of the dominant activities of collider experiments, with well-known negative results.

Finally, another important and unusual theme that runs through the book is the question of how to statistically characterize the significance of an anomaly. This is a topic where Dorigo is very much an expert, recently heading the CMS Statistics Committee.

Anomaly! is both a great tale of how science is really done, and an unusually insightful exploration of the crucial question of how one evaluates the significance of hints of new experimental discoveries. This question is of central importance now as the LHC gathers and analyzes data in a previously unexplored energy range. There are undoubtedly anomalies galore being studied by the LHC scientists, almost all of which will never be heard of by the public, as the experiments cautiously work to eliminate every possible conventional physics explanation for the anomaly. The 750 GeV diphoton bump of the past year is one example of an anomaly that made it out to a public announcement, with its disappearance in this year's data making clear why the experiments are so cautious. You can't find out about these ongoing stories, perhaps for good reason, due to the policies of how collider experiments are run, but now you can buy a copy of *Anomaly!* and at least read about how things played out in the previous generation of such experiments. This background should be helpful to make sense of what is going on if and when (March?), the next LHC anomaly gets reported.

Comments

1. **David Appell**
   December 6, 2016

   Peter, thanks for noting this. But the price, yikes! $50 for a paperback on Amazon…. Will need to get it through my library.

2. **tommaso dorigo**
   December 7, 2016

   Many thanks Peter for this excellent review!

   To David Appell above: if you buy the book through World Scientific (at http://www.worldscientific.com/worldscibooks/10.1142/q0032), by quoting the code WSSLPS20 you get a reduction of 20% on the cover price until December 31st.

   Best,
   T.

3. **Mike Hall**
   December 7, 2016

   Tomamaso, does the discount apply in the UK (where worldscientific.com's paperback price on its website is the same as Amazon's, at £40, and thus practically equal to the USA's $50)?

   Currently, the cheapest easily findable price, as is often the case, seems to be on Amazon marketplace

4. **tommaso dorigo**
   December 7, 2016

   Dear Mike, I wouldn't know. Perhaps the best is currently CERN, where the book is offered for 40 swiss francs.

Best,
T.

5. **Alex Karev**
December 8, 2016

David Appell, using WS16XMAS35 discount code on World Scientific website you will get 35% off (also until December 31)

HEP Theory Letter

December 8, 2016
Categories: Uncategorized

A couple weeks ago a large group of US HEP theorists wrote a letter to the DOE High Energy Physics Advisory Panel (HEPAP) (available at page 7 here) expressing alarm at trends in DOE funding of HEP theory, ending with

> We formally request that a subpanel of HEPAP be formed to investigate and better understand this damaging trend and to make recommendations to address its consequences and restore a thriving Theory program, and we strongly urge that HEPAP support measure to rebuild and maintain the prominent and world-leading standing of US High Energy Theoretical Physics.

The letter claims that since 2011 the overall DOE HEP Theory program has been cut by 17%, with the university component of this cut by 30%. It also claims that 25% of DOE-supported university theorists have had their funding cut off in the last four years, with the number of postdocs going down by about 30%.

At last week's HEPAP meeting there was a discussion of this, but I don't know what was decided. Some numbers presented there indicated that from FY2013-2015. the DOE theory budget went from $51.2 million to $49.32 million. The net number of funded PIs was reduced by more than 10% (25 out of about 230), with 52 PIs dropped, 27 new ones coming in. The conclusion of that presentation was that "The theory program in its current state cannot be described as thriving." The emphasis in the letter and this presentation on "thriving" is a reference to one of the P5 recommendations that is supposed to be governing how the DOE HEP budget is allocated:

> The U.S. has leadership in diverse areas of theoretical research in particle physics. A thriving theory program is essential for both identifying new directions for the field and supporting the current experimental program.

The most detailed recent information I can find about the DOE HEP theory budget is in this presentation from August. It shows a decline from FY10 to FY16 from $53.09 million to $46.69 million, with most of this in the component going to university groups, which went from $27.25 million to $21.765 million. The current number of postdocs supported is listed as about 125 (100 at universities, 25 at the labs), the number of graduate students is about 120.

Concern about this decrease in funding first became public two and a half years ago (see my blog post here) with Sean Carroll's blog post describing a "calamity". Various HEPAP presentations warned physicists about the dangers of public complaints and these died down for a while, but the continuing cuts to the university component of theory funding seem to have led to the decision to send this new letter.

An odd part of this story is that it's unclear why this decision to reduce DOE HEP theory university funding significantly was made. It's true that the overall DOE HEP budget has been cut over the same period (from $810.5 million in FY10 to $795 million in FY16) but unknown why the university theory component was cut 20% over this period while the overall cut was only 2%. Note that none of these numbers are adjusted for inflation.

It would be very interesting to hear comments from anyone who knows more about what is going on here. The usual generic comments that government spending is bad will be deleted. Keep in mind that the amount of money at issue here is (2.7% of the HEP budget, .00058% of the total federal budget) very small on the scale of government funding of science, and now ever small on the scale of private science funding (the Simons Foundation alone last year gave out $233 million in grants).

Update: A full copy of the letter with all signatures is here. An explanation of where the numbers in the letter come from is here.

Comments

1. **Peter Shor**
   December 8, 2016

   I assume you mean the overall cut was 2% rather than $2.

2. **Peter Woit**
   December 8, 2016

   Peter Shor,
   Thanks. Fixed.

3. **NoGo**
   December 8, 2016

   Might it be "because strings"? — all the "over-selling and under-delivering" by String theorists caused "powers that be" to loose their interest/trust in HEP theorists in general? Or such things are below the radar of the government?
   I have no idea how such decisions are made, and curious.

4. **Peter Woit**
   December 8, 2016

   NoGo,
   Most DOE grants aren't going to string theory, but to other subfields, and I haven't seen any evidence that string theory grants are being cut more than non-string theory grants. One could equally well speculate that it is phenomenology that it is the problem: with the LHC finding nothing to vindicate any BSM phenomenology, maybe someone has decided that less funding for that area is needed.

   One can find many plausible speculative reasons for why someone might think it a good idea to cut theory funding, but I've seen no evidence for any of them. Perhaps even the authors of the letter are in the dark (note that they don't seem to be responding to a particular argument against their field), with one goal of the letter to find out why this has happened.

5. **John**
   December 8, 2016

   I know you like deleting comments from me but I'll try one more time to answer your question with the facts even if you don't like hearing them.

   The reason for the change is due to sequestration. Instead of individual appropriation bills the government was funded with CR's (continuing resolutions). Now the sequestration deal required additional cuts. For FY 2013 there was a reduction that was going to impact the LCLS II upgrade project along with some other large planned programs.

Congress no longer controlled the allocation process but did agree to shift funds to make sure the LCLS II project would continue. They took the funds from staffing related funds.

Even last year on November 2nd 2016 a deal was reached by Obama and Congress that would modify the Budget Control Act of 2011. It raised funds for FY 2016 and this is why the overall dollars are not that large. DOE got a decent increase with this agreement. However, the agreement went to projects as it was only going to last for one year and therefore would not make sense to increase funding for staffing.

So the answer to your question comes down to all the deals that were made by the administration and Congress in dealing with sequestration. The agreements to restored some funding were for specific projects and short term. Funding for HEP related felt the full weight of sequestration. Hence the difference in overall budgets and the larger cut to the area you mentioned.

And finally there is one additional area that caused this difference. Fermilab has seen reduced funding each year as core research has shifted to the LHC. This funding is part of the reduction you see in HEP related projects. You can look up the FY 2013 budget act for more detail surrounding the Fermilab funding cuts.

If you want to know more about the details you can research the Budget Control Act of 2011 and the yearly CR’s since then.

6. **Peter Woit**
December 8, 2016

John,
I deleted the previous version of your comment only because all it contained was a simple unsupported claim (that sequestration was the reason) that seemed of no relevance to the issue (why a larger DOE university theory cut?) Thanks for the more detailed version, although I’m still having trouble understanding this well enough to be sure that the reasons you give explain what is going on.

Two and a half years ago when this first came up there were arguments given that this was a technical problem (change in dates grants started, change in policy about later-year commitments). It seemed that if that was true, in later years the cuts should be restored, but instead they have gotten more severe.

If the problem is just sequestration, presumably it should go away when there’s a final budget, no? If so, the response to the theory letter should just be a simple “this is a temporary problem, will go away when we have a budget”, right?

7. **JE**
December 8, 2016

Sorry if this is exactly what you meant by “generic comments that government spending is bad”, but maybe it’s just that, with such little recent progress and such big prospects of a desert ahead for HEP, someone at DOE (mistakenly) decided that cutting down HEP funds was a reasonable thing to do, with HEP Theory fund cuts being perceived as less unpopular than HEP experimental fund cuts.

8. **Peter Woit**
December 8, 2016

JE,
I think that’s a reasonable speculation, but I’ve seen no evidence in any public statement coming out of DOE that that’s the explanation for the cuts. Maybe the theory letter will lead to a response from DOE that will clarify whether that’s what is going on or whether it is something else.

9. **Dragster**
December 9, 2016

Is it possible that recent high-profile private funding, such as the Simons Foundation and Breakthrough Prizes, makes superficially informed DOE bureaucrats more comfortable with reducing the government funding?

10. **Fred**
December 9, 2016

Is there any statistics on the fraction of DOE HEP theory funds spent on summer salaries? Since the DOE introduced a summer salary cap not long ago, I’m wondering if this is the main reason for the decline in HEP theory funding?
I don’t want to start a discussion about adequate salaries, but I do think that research grants should be used to support young researchers (postdocs and Ph.D.s) rather than to pay for the summer retreat of a well established (and typically reasonably well paid) Professor at Lake Winnebago.

11. **anon**
December 9, 2016

I’m not sure if funding young researchers, especially PhD students, more is a good idea. I don’t work in HEP, but in my field doing that has lead to overproduction of PhDs with not nearly enough permanent positions.

12. **Peter Woit**
December 9, 2016

Dragster,
Maybe, but again, no known evidence that this is a factor.

Fred,
The number of grants, postdocs, students is going down, it’s not just a reduction in some summer salaries. The cap was put in to keep other reductions from being even larger.

anon,
HEP theory has always had a very unhealthy situation of far more phds and postdocs than permanent jobs. This situation isn’t any worse than usual right now, maybe even slightly better, so this doesn’t explain recent cuts. Cuts in grants funding senior tenured people don’t exactly encourage universities to create more permanent positions in this area, so on that end won’t help the job situation.

13. **Anon**
December 9, 2016

Off topic but I hope interesting, this seminar at CERN yesterday:

https://indico.cern.ch/event/592392/

The "unexpected results" are not yet published, only on Ting's slides and seem only to be documented in Turkish, but are potentially very interesting:

http://esrap.physics.metu.edu.tr/duyuru/ams-02-basin-aciklamasi

In particular the positron fractions and especially the proton-antiproton ratios.

14. **NotAPhycsist**
December 9, 2016

"Various HEPAP presentations warned physicists about the dangers of public complaints"

What are these dangers? Can you link to a presentation?

15. **Jonathan Miller**
December 9, 2016

I think that there are a number of factors.

First, and maybe most importantly, a significant (temporary) cut of support for theory does not have the same cost as a significant (temporary) cut of support for experiment. This is because cutting an experiments funding (beyond some threshold) can cause some, most or all previous funding to that experiment to be wasted and can cost the entire scientific program. Even a radical cut to a theoretical program allows it to be pursued in 2, 5 or 10 years time at low cost. In fact, if the theoretical funding was geared to maintaining the same number of awards for tenure-tracked theorists, there would probably be minimal harm to theoretical programs even with significant (temporary) cuts.

Second, my experience at University of Maryland is that HEP theorists were much more likely to be supported as students and at postdoc level by the University than HEP experimentalists. This was via teaching assignments, which decreased the time spent on research but still resulted in many more students and postdoc level theorists being supported than would be the case otherwise.

Finally, I would argue that HEP theory as a whole needs some experimental input right now. It isn't just string theory. For example, there has been a ton of theoretical and experimental effort put into WIMPs. And due to the recent experimental results in astronomy, collider physics and direct detection, I don't think extensive further theoretical effort without experimental guidance is well motivated in this sector. Yes, one could argue that the recent AMS results support 1 TeV DM, but they could also support many other interpretations as the results also change our understanding of Cosmic Rays.

All of these points hopefully point to just a short period of temporary cuts and a bright future. If the third factor continues to grow in significance, then probably the full field (HEP theorists and experimentalists alike) can expect further cutbacks.

16. **Peter Woit**
December 9, 2016

NotAPhysicist,

See for instance
http://science.energy.gov/~/media/hep/hepap/pdf/201403/Siegrist_FY15_HEP_Budget_HEPAP_Talk_v2.pdf
"Bickering scientists get nothing"

Jonathan Miller,

Those are all possible factors, but again I don't see evidence that they are the explanation for recent cuts (or any evidence or reassurance from DOE that this is a temporary situation).

17. **John**
December 9, 2016

Peter,

Sequestration is supposed to last till FY2021 unless reversed by Congress. The first large cuts that were noticed over two years ago were directly related to the FY 2013 CR deal as this was the first year that would see full implementation of sequestration as agreed to in the BCA of 2011. They shifted funds to cover existing projects from pure HEP theory funds given directly to universities.

The deal reached last year on November 2nd increased overall DOE funds and even restored some HEP related funds. But once again it was a deal for just a single year and most of the monies were spent on projects and not university grants related to full time positions.

The reason DOE says it is a temporary situation is because sequestration will eventually go away. And each year the various CR's might restore some funds that were previously cut. Even now Congress is debating and will vote on a new CR for the rest of this FY. Details are not yet out on DOE related funding so I have not read how it might impact this issue.

But please note that the entire cause of these cuts were 100% directly related to sequestration and then the shifting of funds within DOE to projects and away from hiring's at universities and research centers.

I know it's Wikipedia but they have a decent explanation of the process. It doesn't get down to the level you want which is only found in the various budget acts approved by congress. https://en.wikipedia.org/wiki/United_States_budget_sequestration_in_2013

18. **Peter Woit**
December 9, 2016

Thanks John,

I guess what I'm still not understanding is the "we can't cut projects, so we'll cut university grants, especially in theory" justification of why overall cuts are hitting university theory grants much harder than other parts of the HEP program.

19. **ronab**
December 9, 2016

Does all this mean I can look forward to an imminent 30% reduction in pop-sci articles about the multiverse? Cool.

20. **John**
December 9, 2016

Here is the actual budget presentation submitted by the DOE for FY 2016

http://science.energy.gov/~/media/sc-1/pdf/2012/Dehmer_2016_Budget_Presentation

Not sure how much this helps but it does show HEP is flat while most other areas are seeking an increase. I can't answer the "why are they doing this" as no one has explained their motive for why they shift funds around.

21. **jd**
December 9, 2016

I support what John has written above. First read the Wikipedia explanation of sequestration; sequestration does not end with a final budget. Then read and understand the slides of Jim Siegrist, Office of High Energy Physics. At most it takes only a very little reading between the lines to understand what is going on. The letter written by the group of theorists may have been a mistake; to me it smells of bickering. But I am not in HEP so what do I know? I recommend that HEP get its internal politics quietly in order. But then again what do I know?

22. **Anonymous mathematician**
December 10, 2016

I only have anecdotal information, but I have talked to a few graduate students who have decided against careers in HEP because of the lack of new experimental findings at the LHC (nothing to do with string theory; graduate students still seem excited about that).

I don't think this is related, because the money started being cut before it was clear that the LHC wasn't finding anything beyond the Higgs. But it's probably less of a disaster than it would be if there were exciting new experimental results from the LHC.

23. **Shantanu**
December 10, 2016

Peter, most of the people who have signed this letter are string theorists.

24. **Peter Woit**
December 10, 2016

Shantanu,
The slide does say there are "many more" on another page, not reproduced. But that list is dominated by string theorists, few phenomenologists, which is odd.

The oddest thing about the letter that I noticed was that its first reason given for why the field should not be cut is
"These cuts come at a time of continued public fascination with particle physics, cosmology, and gravity".
It's unclear why success at getting public attention (often with fairly outrageous hype) is a good argument for federal research dollars. This is also not a great argument to be making to gain support from physicists in other subfields that are doing important work, but don't enjoy the same public attention, and may not be so happy about that.

25. **Doug McDonald**
December 12, 2016

Peter, the pointers already here do include a vastly larger list of signers.

https://drive.google.com/file/d/0B3IXprj4oX2sU0VvczR6VzdheDQ/view

26. **Peter Woit**
December 12, 2016

Doug McDonald,
Thanks, I've updated the posting with a link to that full version of the letter, as well as to a document justifying the numbers in the letter.

27. **GoletaBeach**
December 28, 2016

As best as I can tell, there is serious downward budget pressure… John's explanation that the origin of it might be right… but to document the Office of Science funding, FY1999 appropriations are at: http://science.energy.gov/~/media/budget/pdf/sc-budget-request-to-congress/fy-2000/Cong_Budget_2000_Overview.pdf … can compare with John's link above, and look at the change in funding levels during this period for the "big 6" portions of the Office of Science mission:

Basic Energy Sciences …. +52% (that accounts for the effect of inflation)
Computing …. +140%
Bio/Env…. -4.7%
Fusion…. +48%
High Energy…. -23%
Nuclear…. +25%

Overall OS Funding…. +25%.

I picked year 2000 just because it was a round number for the budget… all years from 1987 are at: http://science.energy.gov/budget/ . But I do believe that HEP has been a big loser for a decade or two… not sure it is the SSC, either.

My impression is that Basic Energy Sciences does more quantifiable, more easily defended activities. They can say that in FY 2015 8,988 samples were quantified in 93% beam uptime at their 5 facilities, and in FY 2016 they increased to 9,611 samples in 94% beam uptime. The private sector sends samples to Basic Energy Sciences facilities. So, BES have been the big winners for quite some time.

That is what our government wants these days. HEP theory activities like… our group got deeper insight into the nature of symmetry as it relates to unification of the fundamental forces, well… not easy for bean counting to deal with that, or support it.

The experimental HEP community has adopted a layer that interfaces to the way the DOE wants to understand things. Seems to me that the theoretical community is off the mark a little… agitation helps, but in the end, when in Rome.

Various Links

December 15, 2016
Categories: Langlands, Multiverse Mania, Uncategorized

A grab-bag of unrelated topics and links:

- Continuing the subject of budget cuts from the last posting, I heard today that the NSA is not funding this year the grant program that the AMS has been running for it, called the NSA Mathematical Sciences Program. In typical NSA fashion, no real reason given:

  > after much deliberation our senior management has decided that the MSP will not have the resources to make new awards in FY2017.

  Even less information is available than in the case of the similarly mysterious DOE HEP Theory cuts, since all information about the NSA budget, even the total, is classified (although from Snowden and others, it seems that it's about $10.5 billion).

- The IHES has a new website.
- This spring Peter Scholze will give a series of six lectures at the IHES on the latest about the local Langlands conjectures.
- Via David Roberts, the LMS has some videos about mathematicians here. Kevin Buzzard explains Langlands:

  > The Langlands philosophy? Yeah, that's like Birch-Swinnerton-Dyer on crack, isn't it?.

- Tate's collected works are finally available from the AMS.
- Turning to physics, there's a very good new article at Quanta from Natalie Wolchover about the unsuccessful search for proton decay and what this means for grand unification ideas. Glashow has now given up, with the simplest GUTs now conclusively ruled out:

  > Glashow, for one, largely lost interest in the whole affair when SU(5) was ruled out. "Proton decay has been a failure," he said. "So many great ideas have died."

  Not everyone has given up though, with fans of the "flipped SU(5)" SUSY GUT explaining things this way:

  > Barr, one of the originators of the still-viable "flipped SU(5)" GUT model, compared the situation to waiting for your spouse to come home. "If they're 10 minutes late, there's simple explanations for that. An hour late, maybe those explanations become a little less plausible. If they're eight hours late … you begin to worry that maybe your husband or wife is dead. So the point is, at what point do you say your theory is dead?"
  >
  > Right now, he said, "we're more at the point where the spouse is 10 minutes late, or maybe an hour late. It's still completely plausible that grand unification is correct."

  Besides the current wishful thinking, this particular model has a strange history. You can read here about how it follows from Vedic Science. Over the years it has been about to come home many times, see this from 2012, which assures us that:

  > The CMS and ATLAS experiments have also observed tantalizing hints of the unique signature predicted by the Flipped SU(5) model.

  At this point, it seems to me to be way more than an hour late, time for its nearest and dearest to admit that it's dead (or maybe has run off with its TM instructor).

- Also in Quanta, you can read about Janet Conrad and sterile neutrinos here.
- I don't always agree with Sabine Hossenfelder about math, but I very much agree with the conclusion of this posting:

  > In lack of experimental guidance, what we need in the foundations of physics is conceptual clarity. We need rigorous math, not claims to experience, intuition, and aesthetic appeal. Don't be afraid, but we need more math.

- There's a Recent Developments in Fields, Strings, and Gravity conference going on this week at the new Center for Quantum Mathematics and Physics at Davis.
- Videos of the talks at the recent John Schwarz 75th birthday conference at Caltech are available here.
- Multiversal Journeys is an organization devoted to promoting theoretical physics, with a heavy dose of multiverse mania as part of their story. They have a new book coming out, Quantum Physics, Mini Black Holes and the Multiverse, supposedly "Debunking Common Misconceptions in Theoretical Physics". It seems that one of these common misconceptions is that the multiverse is pseudo-science. To fight this, they've also produced a promotional video.
- Bert Schroer has an interesting preprint with a lot of material about Rudolf Haag and algebraic quantum field theory.
- Another intriguing preprint recently out is from Arkani-Hamed and collaborators. In the past Arkani-Hamed has been vehement about gauge symmetry just being a worthless redundancy in our description of physics, for instance stating:

  > What's as a misnomer called gauge symmetry, whose beauty is extolled at length in all the textbooks on the subject, is completely garbage. It's completely content free, there's nothing to it.

  In the new paper, instead of gauge invariance being useless, there's a conjecture that locality and unitarity, instead of being fundamental principles, follow from gauge invariance.

  There's a long tradition in the philosophy of physics literature of arguing over whether gauge symmetry is a fundamental idea or a useless redundancy. I'm on "fundamental idea" side, but of course exactly what the role of gauge symmetry is in fundamental physics is something that we have yet to fully comprehend.

Comments

1. **Ben**
   December 15, 2016

   What are you thoughts on the likelihood of a multiverse? Will take a link if you have already discussed it.

2. **Peter Woit**
   December 15, 2016

   Ben,
   That's a meaningless question, the multiverse is pseudo-science. I've written endlessly about this on the blog, see

http://www.math.columbia.edu/~woit/wordpress/?cat=10
and some of the questions in the blog FAQ.

One thing to notice about the many multiverse promotional efforts like this is that they never include multiverse critics (and I think being critical of the multiverse as not science is a majority point of view among physicists). Their only discussion of criticism of the multiverse as pseudo-science is to counter straw-man arguments.

That said, the last thing I want is another discussion here of exactly the same arguments as always. If you have something new, please comment, otherwise please restrain yourself, I'm just going to delete attempts to carry on old arguments.

3. **David**
December 15, 2016

"One thing to notice about the many multiverse promotional efforts like this is that they never include multiverse critics " – this is actually proof that multiverse theories are wrong. If they were true, there would be many promotional efforts that focussed on the negative aspects. (sorry if the joke falls flat)

4. **Bee**
December 16, 2016

Thanks for the link 🙂

Gauge symmetry can both be useful and be redundant, I don't see why these are two exclusive categories. One could say that thermodynamics is redundant, to pick a not-quite random example, but that doesn't make it useless.

5. **Manfred Requardt**
December 16, 2016

Dear Peter, I too think that gauge theory is a fundamental building block of quantum nature and is not an indication of some redundancy. Frequently what appears to be reduntant from the point of view of classical physics has a deep meaning in quantum reality. A nice discussion of this phenomenon can e.g. be found in the second volume of Richard Feynman about electrodynamics concerning the vector potential.

6. **neil**
December 16, 2016

Interesting article by Wolchover. I am interested in learning more on the relationship of proton decay to SUSY. As I understand it, there are SUSY and non-SUSY GUTs. I suppose the LHC results make SUSY GUTs a whole lot less interesting, but what about non-SUSY GUTs like minimal SO(10)? I am surprised Glashow threw in the towel on GUTs just because his SU(5) was ruled out. Are there other problems with these higher symmetry non-SUSY GUTs?

7. **Peter Woit**
December 16, 2016

neil,

Minimal non-SUSY SO(10) is also already ruled out, I doubt the (not very large since they're both rank 4) difference between SO(10) and SU(5) models was that significant to Glashow.

Nanopoulos explains well the general problem of why, even if you can make a more complicated model than the SU(5) or SO(10) ones, specially designed to evade conflict with experiment, you might not want to:

> "People can construct models with higher symmetries and stand on their nose and try to avoid proton decay," Nanopoulos said. "OK, you can do it, but … you cannot show it to your mother with a straight face.""

8. **Chris Austin**
December 16, 2016

SO(10) is D_5, and is rank 5. SU(5) is A_4, and is rank 4.

9. **paddy**
December 16, 2016

I think Glashow threw in the towel ~ 30 years ago. He (in my way of thinking) has been faithful to principle. The beauty/simplicity/falsifiability of Georgi and his GUTS model was minimal SU(5). Perhaps I misremember, but I think he walked away back when proton decay contradicted this model . He didn't introduce additional degrees of freedom to clutter the model in order to tune it in order to save it from being falsified–i.e., make it no longer predictive.

10. **Peter Woit**
December 16, 2016

Chris Austin,
Thanks, that's right, my mistake about the ranks, I was thinking of something else (SU(5) is the subgroup of SO(10) that preserves a complex structure).

11. **Frank**
December 17, 2016

Peter,

it might be less well known that various people who worked on the the standard model – several of them with Swedish prizes – do think that the search for a unique gauge group is misguided. After all, the electroweak model is not a real unification, as it still requires two independent coupling constants. And in the standard model, SU(3) has a third, independent constant. Both experiment and theory show that it is not possible to describe nature with SU(5) or SO(10) or another single group with only one coupling constant. Nature has three gauge forces, not one.

But there is an important reason to cling to grand unification: it is predicted, or at least suggested, by string theory. So we are again in the difficult situation that experiment is against grand unification, whereas string theory is in favor of it. (The situation is similar to supersymmetry.) It seems that theory will make progress only once all the people believing in GUT and SUSY are gone.

12. **Shantanu**

December 17, 2016

Peter, I agree with Glashow. looking for proton decay is looking for El Dorado.
That said I know several grad students who continue to be lured into doing thesis on proton decay.

13. **piscator**
December 17, 2016

nothing wrong with experimental theses on proton decay – always good to push any measurement to a higher level of sensitivity, almost independent of theoretical motivation.

(and proton decay experiments have already won one Nobel Prize…..)

14. **Peter Woit**
December 17, 2016

Frank,
The basic problem with GUTs and SUSY is similar: you postulate a more unified theory using a larger symmetry group (or supergroup), but then have to somehow break the symmetry to explain why we don't see representations of this larger symmetry group. The ad hoc symmetry breaking sector makes the whole idea much less compelling and predictive. These ideas were born 40 years ago around the same time, back then everyone thought they would either lead somewhere in a few years or get abandoned. It's remarkable that instead they entered the textbooks and became institutionalized as a seemingly permanent part of the subject.

Shantanu,
Just because finding proton decay is less likely doesn't mean one shouldn't do such experiments. I'm no expert, but my impression was that many such detectors are now configured to do double-duty as neutrino experiments.

15. **neil**
December 17, 2016

You are right Peter. Proton decay was the primary purpose of Super-K, but with the Kajita Nobel prize much of the research there is on neutrino oscillations. And the DUNE neutrino observatory has a proton decay experiment.

Proton decay seems less and less likely for the reasons stated but, wow, baryon number violation would be a huge discovery.

16. **Louis**
December 17, 2016

The 'wife showing up late' comment is interesting, but is missing a major aspect. The prior probability of your wife's continued existence and intent to return home should be much higher than the prior probability that your favourite theory which isn't known to be inconsistent with experiment is an accurate extension of current models. At least for most marriages. Its more like you're attending a dinner event in the city your crush lives in and you're judging the probability that you'll to run into them.

Or maybe I'm just asking for a bit too much from analogies.

17. **Peter Woit**
December 17, 2016

Louis,
You're right, it's a strange analogy. Until you realize that the particle theory community has been living with GUT/SUSY speculation for so long that it is like a close family member, one whose death (due to a conclusive experimental result) would be unexpected and heart-breaking. Better to do anything to keep it alive and not face that.

18. **serious question here**
December 17, 2016

how do these new experimental limits on proton decay + LHC results affect supersymmetry GUT such as SUSY GUT SU(5) or SO(10)?

it's my understanding adding SUSY to GUT increases proton lifetime enough to evade these bounds.

19. **Frank**
December 18, 2016

Bee writes:

In lack of experimental guidance, what we need in the foundations of physics is conceptual clarity. We need rigorous math, not claims to experience, intuition, and aesthetic appeal. Don't be afraid, but we need more math.

Allow me to partly disagree. Conceptual clarity is needed. But it is *not* achieved with math. Math cannot help to decide whether the Planck scale has a fundamental importance or not. Math cannot decide whether GUT or SUSY are correct. Math cannot decide whether strings are correct. Math cannot tell which symmetry is correct. Math cannot tell how many dimensions nature has. If math would help to get conceptual clarity, we would have found the TOE already years ago.

Conceptual clarity is not achieved with math, it is achieved by checks with experimental data. GUT is mathematically sound, but wrong. So is SUSY. So are strings.

Math helps to draw correct conclusions. But it does not help in finding the foundations. Math leads to conceptual consistency. Math does not lead to conceptual clarity. And in the history of physics, it never did.

Take the issue of symmetry. If you know that a flower has fivefold symmetry, you still do not know the shape of a petal. You just know how the other four look like if you have only one. Math (in this case symmetry) helps; but it does not provide the full information. Experiment does. We have the same issue with nature.

20. **Peter Woit**
December 18, 2016

Frank,
I think what Bee has in mind is the situation in her field of quantum gravity. There many of the research topics (ER=EPR, firewall paradox, Verlinde's entropic gravity, etc, etc) suffer from the problem she identifies: people are manipulating very vague, ill-defined speculative ideas, claiming "intuitive", "physical" understanding, but you can't

pin down exactly what these ideas are. Before you can confront your theories with experiment, you first need to have a well-defined theory, and it is mathematics that can provide you with this.

21. **Peter Woit**
December 18, 2016

serious question here,

There's a graphic in the Quanta article that addresses this, showing in particular most of the range for supersymmetric SO(10) already ruled out. One problem though is that these theories have a lot of undetermined parameters, so claims about their "predictions" tend to have a bunch of built-in assumptions. If you look at the history of these "predictions", my impression is that they have tended to creep up as experimental bounds move up. For an example of the calculations involved, see https://arxiv.org/abs/hep-ph/0007213

22. **Apostolos Syropoulos**
December 18, 2016

"Why String Theory?" is Physics World's book of 2016:

http://physicsworld.com/cws/article/news/2016/dec/14/robust-defence-of-string-theory-wins-physics-world-2016-book-of-the-year

23. **Shantanu**
December 19, 2016

Peter/others:
I am not objecting to building/operating such detectors. My own thesis was on such a one. I am surprised that gullible students continue to work on analysis of proton decay.

24. **Pavel**
December 21, 2016

Dear Peter,

I recommend your readers to read a new review

https://arxiv.org/pdf/1501.01886.pdf

Minimal non-SU(5) was ruled out by the wrong relation between the down quark and charged leptons masses. Even before you compute the lifetime of the proton the model is ruled out. Now, once you write down a consistent model for fermion masses it is possible to achieve unification without supersymmetry in a simple SU(5) theory, see for example for a recent paper:

https://arxiv.org/pdf/1604.03377.pdf

Best,
Pavel

25. **John Baez**
December 27, 2016

Peter wrote:

> You're right, it's a strange analogy. Until you realize that the particle theory community has been living with GUT/SUSY speculation for so long that it is like a close family member, one whose death (due to a conclusive experimental result) would be unexpected and heart-breaking. Better to do anything to keep it alive and not face that.

It's less strange if you remember why people actually like the SO(10) grand unified theory. In this theory, each generation of fermions and antifermions fits neatly into a single family of particles, the spinor representation of SO(10) (or really Spin(10)). This theory "explains" why quarks have charges 2/3 and -1/3 while leptons have integer charge, and much more. All sorts of facts that otherwise seem strange and random suddenly fit together in a beautiful way.

However, there's a lot that it *doesn't* explain, like the masses of particles, and one must invoke extra Higgs bosons to break the symmetry from Spin(10) down to SU(3) x SU(2) x U(1).

So, it's absolutely heart-breaking that this theory seems to be wrong, but we can hope that there's something right about the underlying algebraic patterns that make it tick.

Unfortunately we may not live to see the next really interesting development in this mystery. So, unless one has a really promising new idea, it's better to think about something else.

26. **Peter Woit**
December 27, 2016

John,
I agree that the fact that a generation fits into the spinor rep of SO(10) is something very appealing, an indication of something right about the idea (and less right about the original SU(5). The problem with GUTs has always been that the unification is a bit of a fake, since you don't have a compelling idea about why it breaks (introducing a special set of new Higgs fields isn't very pretty…)

My guess is that the hint here is that there is some sense (one we don't understand) in which there are five fundamental degrees of freedom, with a generation the exterior algebra on these five degrees of freedom. The relation to the spinor rep is that you can construct the spinor rep of Spin(2n) using the complex exterior algebra on n-dimensional complex space (i.e. as the state space of five fermionic oscillators, see discussion in my book).

27. **Yatima**
December 30, 2016

Maybe of some interest:

A question that appeared on Stackexchange: [Which were some major mathematical breakthroughs 2016?]()

28. **Sebastian Thaler**
    January 1, 2017

    Peter,

    FYI, the responses to the 2017 Edge.org annual question have been posted. This year's question was, "What scientific term or concept ought to be more widely known?" Martin Rees suggested "multiverse": https://www.edge.org/response-detail/27129

29. **Jim Given**
    January 8, 2017

    Vedic Science a la John Hagelin and the Maharishi University makes lousy science, but good art! I was in the MIT Physics Dept. circa 1986, and a particle theorist had a large multi-color poster showing how an incarnation of SU(5) accorded with purported Hindu constructs. But the craziness was mostly in the margin. Most of it recalled the lovely illustrations of the Eightfold Way etc. from 1960's Scientific American. Can I get one of these?

What Graduate School in Theoretical Physics is Really Like

December 29, 2016
Categories: Uncategorized

I'm about to head off for a short New Year's vacation in West Texas, but wanted to recommend a wonderful article that just appeared at Nautilus. It's a memoir by Bob Henderson (who I met when he wrote about me, see here), appearing under the title What Does Any of This Have To Do with Physics? (although the title of the web-page, *What Graduate School in Theoretical Physics is Really Like*, is more descriptive).

Henderson was a graduate student at Rochester in theoretical physics, working with S.G. Rajeev. He later went to work on Wall Street, and more recently in journalism. His Nautilus piece is the best explanation I've ever seen of what it's like to start working in this field as a graduate student, should certainly be required reading for anyone thinking of going into the subject. It's also somewhat of a profile of Rajeev, who has worked on a wide variety of topics in theoretical physics.

One of the main themes of the piece is Henderson's thinking about how and why he left theoretical physics, why he "quit". Something to keep in mind is that this kind of decision is what most people who get Ph.Ds in the subject end up facing. There are 5-10 times more people getting Ph.Ds in this field than there are permanent positions doing research in it, so the career path starts out with a game of musical chairs that you are highly likely to lose. Different people make the choice to quit the game and do something else at different points and in different ways.

Henderson does an excellent job also of explaining what the real problem is with doing this kind of research: that of figuring out what the right thing to calculate is. For everyone, but especially for those at the beginning of a career, the subject is a huge collections of topics one doesn't understand. One has to somehow choose a direction to pursue, and it most likely won't go anywhere:

> Writers talk of the terror of facing a blank page, but it's no different for theorists like Rajeev trying to choose which path to take. There are an infinite number to choose from, and most go nowhere or back from where you came. The clock is always ticking and you spend so much time in the dark that it can make you not only question your path, but your own self worth. It can make you feel stupid.

Sticking with this and making a career of it involve some combination of good luck (being in the right place at the right time), ability, self-confidence, not having a family to support, and a host of other factors. As Rajeev explains to him:

> Without naming names, he ticked through a catalog of his contemporaries who'd succeeded in theoretical physics even without having the towering mathematical intellect that I was sure it took and that Rajeev surely has. They'd made it, Rajeev explained, by focusing on problems that played to their strengths, or by taking advantage of computers, or by collaborating with peers who had complementary skills. Some socially gifted but not so mathematically talented types had gone quite far this way, earned a lot of renown.

Anyway, the whole piece is well-worth reading. Another recently published Nautilus piece that I learned about from a link on this one is The Universes of a Woman in Science. It's by Kate Marvel, who shares with Henderson (and hundreds if not thousands of others…) the experience of getting a theoretical physics Ph. D. (string cosmology in her case), and then leaving the subject for another field (in her case, climate science, which she blogs about here).

Comments

1. **Tim May**
   December 30, 2016

   Two poignant stories.

   Things were bad when I was in physics in the 70s, but seem much worse now. I cannot imagine that Ph.D. students think they will become career professors.

   Myself, I once cooked steaks for Feynman in Isla Vista in 1973. We sat around in our typically dingy student apartment and Feynman opined, with great humor, about his adventures. More seriously, he said the "easy days" of discoveries and major new theories were just about over.

   We asked him where he thought the new excitement, or revolutionary spirit, might be.

   He said "computers."

   A year later I opted to delay going to grad school for a while and joined a then-small company, Intel Corporation. I learned it was heavily based on some good physics and I was constantly challenged. Physicists were not too common (EEs and ChemEs were more common), so I did some good stuff. In 1977 I discovered that a single bit error problem which was a problem in dynamic RAMS was caused mostly by alpha particles from low levels of uranium and thorium in packages. And to a much lesser extent, then, by cosmic rays.

   Just three years after sitting down on cushions in our apartment with Richard Feynman. What an inspiration he was.

   Oh, and I also got to work at Intel with Gordon Moore, Andy Grove, Craig Barrett, and Bob Noyce. A stellar group. I was fortunate to work for Craig Barrett who worked for Andy Grove who worked for Gordon Moore. My results and summaries went right up to the top every day…like working on the Manhattan Project. Heady days.

   And now, merging black holes. Personally, the failure of SUSY is not that big a deal. Something else will come along.

   Happy New Year!

2. **Chris Oakley**
   December 30, 2016

   Interesting article, although it should be pointed out that you do not need relativity to get nonsensical, infinite answers in quantum field theory. Second-quantised non-relativistic QM does this perfectly well. Also I, too would get angry or suicidal if required to work 15-hour days on quantum gravity in one dimension.

3. **Low Math, Meekly Interacting**
   December 30, 2016

   Surely one of the more foolish questions you're likely to get, but what the Hell: Why is all this labor needed in theory? My background is purely experimental, and I've mixed with experimentalist grad students in the areas of biology, chemistry and physical chemistry. In these disciplines, it was quite obvious what grad students were for: slave labor in the laboratory. The more the better. Cheaper even than technicians and so terrified half the time they'd do any dirty job they were asked to just to avoid being forgotten entirely. And there's no end of dirty jobs to do. But, aside from grading and other pedagogical grunt work, why have so many unneeded theory grad students? What else do they do for the institution?

4. **AcademicLurker**
   December 30, 2016

   LMMI:

   I've heard it said that really experienced physicists often guess the answer to their problem/question using heuristic arguments, and then do the math to make sure their guess was correct. Maybe that's the role of theory students, at least before they start an independent project. "I think the answer is X. Go check it out."

5. **cgh**
   December 30, 2016

   In 1999 I started work on my PhD. It was in physics. I had some grant money and was simultaneously publishing. The work on my thesis wasn't going as fast as it could of, probably because of this, but I felt productive. I was meeting regularly with a mathematician who was not my advisor but was working independently with me on related ideas. He is / was well known in the NY area for RH and some analysis work. One day, I suppose in retrospect, I was complaining. The gist of my complaint was about what I was going to do after I finished. He responded that if I did this to be employed I didn't get it and was in for a rude awakening. To him, it wasn't about employment, it was about math, and solving problems. He was an established mathematician with tenure, then, now emeritus. He didn't have to take endless strings of adjunct positions in the boondocks. When he was working on his PhD in the 60's if you thought hard and did good work things came to you. One didn't need to think, at least as much, about food, of family, or being near family. He spent his entire career between 3 NY universities, 2 in Manhattan within 20 miles of where he grew up and raised his family, without giving it much thought. So in 2001 I went to Wall Street. I shared an office with a Caltech postdoc that came from the control and dyn systems group. I exchanged the hype of strings for, then, fast PDE solvers and, today, the hype of AI, deep and machine learning.

6. **Jeff M**
   December 30, 2016

   I started my PhD in 1986, in math. Back then, anyone in math with a doctorate from a good school got an academic job. I finished in 92, the year the job market collapsed. But even that year, most people ended up with academic jobs, just not at the level they expected. I have tenure, I get to do some research, it's not the research 1 place I expected (my thesis was published in Mathematische Annalen, in a normal year I probably would have gotten a much better offer) but I'm happy. It's much worse now, even in math. We're hiring this year, we put our ad in really late, 2 weeks ago, and we're already close to 200 applications. Very qualified people, including people with doctorates from places like Berkeley and Oxford, with publications in good journals. cgh is right about the old days, all my professors basically got their jobs when their advisors called someone to tell them to hire them. I have a funny feeling I might know the person he's talking about, since I got my doctorate in the city…

7. **X**
   December 31, 2016

   Interesting article by Henderson, with things that all of us who have gone through the PhD experience can relate to. But what I found even more interesting was the insight it gave into the working life of his advisor Rajeev. He seems to have been extraordinarily generous with the time and effort he put into his students to bring them along on his research path. And his 'rewards' for that seem to have been pretty mixed. Imagine what it must have been like for him when, after all that time and effort, Henderson started hating him (which Rajeev and any advisor in that situation would definitely pick up on), rejected his research path and went off to try find his own.

   I'm not blaming Henderson for that, since it was a difficult situation for him and understandable that he wanted to find/generate sufficient output to justify the input and hardship from his perspective. Relations between PhD students and their advisors are often difficult and complex, since both are investing so much and the outputs they seek are not always aligned. But this illustrates a reality of the working life of faculty which can be pretty draining.

   So I think those considering doing a PhD in this field should not only look at Henderson's experience as a student but also the close-up description of Rajeev's working life as a prof, assuming finding a faculty job is the person's ultimate plan/hope when embarking on this path.
   In a comment above, cgh recounts how a maths prof told him that "it wasn't about employment, it was about math, and solving problems." That's true for the motivation of people who go down this deprived and often dead-end path, but it's a far from accurate description of what the job really is for those "lucky" enough to land a faculty position.

   The reality is that it is first and foremost an educator job. Teaching classes, guiding graduate students, supervising undergraduate student projects, doing outreach activities in high schools, etc…(plus various administrative things). Research is something you basically do in your spare time, e.g. waking up at 3am to have a bit of spare time for it as Rajeev did in the story. (And this is at research universities such as Rochester; at liberal arts colleges getting research done is even more challenging.)

   Young people aspiring to a faculty job in this field because they "want to do research", and think that "doing a bit of teaching is bearable and a fair trade-off for the freedom to do research" have misunderstood the nature of the job. It is an education industry job rather than research job. They should ask themselves if they really want to spend their working life in the education industry rather than, e.g., doing graphics software design for Apple, working on automated car driving for Tesla, or the myriad of other interesting and intellectually challenging things that smart and hardworking young people can aspire to do. If doing research is really so important you can always go work on Wall Street, retire rich at 50, and spend the rest of your life doing research without constraints or other commitments.
   (I'm speaking from experience here as someone who reached the "holy grail" of a faculty position at a research university and discovered that the job was not quite what I imagined it would be…)

   It was very refreshing how Rajeev acknowledged that "Some socially gifted but not so mathematically talented types had gone quite far this way, earned a lot of renown." For quite a few people (myself included), one of the motivations for going down this path was the expectation that our lack of interest and talent for "sucking up" wouldn't matter so much compared to careers in the business world or elsewhere. This is a myth though – it is no different than other walks of life in this regard.

   Re. LMMI's question above about the need for PhD student "labor" in fields like theoretical particle physics: No it is generally not needed for the research, in fact it is often a time and effort-consuming burden on the prof to drag the students along his/her research path, as Henderson's story illustrates. One explanation for why it happens anyway (perhaps the main one at many institutions) is that the number of PhDs produced is a metric on which individual faculty and departments as a whole are evaluated. I remember one faculty meeting where the dean excitedly told us about a new opportunity for PhD funding, and urged us to "go and grab this opportunity" – presumably by finding students to lure into the PhD path. There was no discussion of whether there was a need for more PhDs or what their future career prospects would be…

8. **A reader**
   December 31, 2016

   It is a beautiful and soulful article/essay.
   The key message for me is "Control your emotions." It takes time. Sometimes it is too late.
   Between "Pursuing the Holy Grail" and "Shutting up and calculate," is it not possible to find some middle ground?
   Like … "Be part of an epic human endeavour (we are primates), enjoy trying to understand/solve a problem, be ready to move on when it is over?"

   That said, from afar, and even though he obviously can write very well, I really wish that Bob Henderson gets a chance to go back to working on that he evidently loves:

Physics.

9. **Jeff M**
December 31, 2016

X

I think what you describe, professors as educators, research a sideline, is sometimes true, and sometimes not. It is certainly true for anyone who teaches somewhere like I do, though even here it's possible to do it differently. One of my colleagues, who just retired, used to only ever teach all sections of the easiest course he could get, and paid as little attention to teaching as he could. You don't need to do much prep to teach what is really high school level math. And from what I've been told, there are plenty of people at research 1 schools like Rochester who are nothing like Rajeev, and who don't even really care much about their advisees, much less the undergrads. As to retiring early and doing research on your own, it's a thought I had when I didn't think I would get an academic job. Decided I would sign a contract with myself that when I had x dollars in the bank I had to retire. Sometimes I look back and think it might have worked out OK, now that I'm stuck teaching summers to pay my kids school tuition. I could be relaxing in my Tribeca loft, sipping single malt, and thinking about math. Actually, back when I was finishing grad school, my wife and I lived in a giant loft in Williamsburg, but boy was the city different then.

10. **vmarko**
December 31, 2016

I have to say that I am not as fascinated by the Henderson article as many here are. To me it sounded like a story of a person having a naive, romanticized point of view on research in theoretical physics, only to be faced with a down-to-earth reality that has very little in common with his romanticized view. Einstein looked at a building through a window, and figured out general relativity — yeah, right. Everyone who thinks research can be done like that is certainly going to be very disappointed. And Rajeev was very very right at the end — Henderson's issue was not lack of ability, lack of open faculty positions, lack of quality or lack of effort. His issue was precisely that he decided to quit. Research was not what he naively imagined it to be, and he caved when he got under some pressure. It was lack of dedication.

In order to survive in a highly competitive research area such as hep-th, a person needs (aside from some luck, math abilities, good mentor, etc.) one crucial character trait not mentioned in the article — stubbornness. One simply needs to be more hardheaded, and not get too distracted by the reality being different from what one imagined.

Regarding mentoring and collaborating with a supervisor, Rajeev seems to be a good mentor, judging from what Henderson wrote in the article. In order to teach a student enough to do research (in hep-th at least), a supervisor needs to dedicate to each student at least one afternoon of uninterrupted discussion in front of a blackboard every week or two. My supervisors did it with me, I try to do it with my students. Those mentors that don't do that are plain and simply irresponsible, IMO. And it usually shows, in the number of years their students take to reach PhD's, and their research performance afterwards.

As for teaching vs. research, my anecdotal evidence is quite the opposite of what X is describing — I am in a research-only institute, doing quantum gravity, and I have literally no teaching duties (and never had any during my career, except when I substitute for a colleague). Moreover, recently I actually asked to get involved in teaching, at the graduate level. The reason is that I need more fresh PhD students as collaborators, to share the workload of my research. In a research-only institution I don't have any contact with PhD students, so it's hard to recruit them.

So anecdotal evidence can vary greatly, it all depends where you are. I've heard some people dreading research-only institutions like IAS, Princeton. No contact with students is a terrible handicap when doing research, they claim, as there are no fresh ideas and excitement that young motivated people bring along. And I tend to agree — one needs to find some sweet spot between teaching and research, since each of the two extremes will have some drawbacks.

Best, 🙂
Marko

11. **Low Math, Meekly Interacting**
December 31, 2016

Re. X: Ah, I see. They're adornments. I know enough about academia that should have been obvious. Thanks.

12. **Why**
January 1, 2017

It is really confusing when you think about it. On one hand, the need for specialising in one of the so many new disciplines in theoretical physics seems to increasingly recquire new PhDs and post docs and on the other hand the increase of the population seems to require more tenured professors to be hired to teach.
So why all this crisis in theoretical physics job market?

13. **X**
January 1, 2017

Jeff M,

"there are plenty of people at research 1 schools like Rochester who are nothing like Rajeev, and who don't even really care much about their advisees, much less the undergrads"

That's no doubt true, but it's difficult to get away with as a tenure-track faculty, and probably getting harder for tenured faculty too. With rising tuition costs students are being regarded more and more as paying customers, and making them satisfied seems to be becoming a higher priority for universities.

At the university I was at (ranked in the top 100 in the world in several rankings), teaching performance was important and given equal weight with research performance in our annual evaluations. There were people who were denied tenure despite having done well in research because their teaching sucked according to the student evaluations. And I'm pretty sure that if someone got tenure and then proceeded to be a terrible teacher thereafter, the powers-that-be would still find a way to push him/her out… (In fact there was a tenured prof who the powers-that-be didn't like for some reason, and they made his position in the dept so uncomfortable that he "voluntarily" left…)
In any case, I felt morally obliged to at least try to be a good and conscientious teacher/lecturer.

As for the actual time and effort that the teaching side requires, it depends enormously on the details of the teaching assignments as you mentioned. There were some people who taught the same couple of courses every year, requiring zero preparation time and allowing them to get high student evaluation scores after fine-tuning the courses for so long. Those tended to be people who were favored by the powers-that-be for whatever reason… There were also assignments that required preparing course materials from scratch (including powerpoint lecture slides, handout notes, tutorial question & solution sets, online quizzes etc) for a new course or because the previously used course materials were disastrously bad. Less favored faculty such as myself tended to get those assignments…

So unless the person is very favored and fortunate with his/her teaching assignments (and has great PhD students who don't require much time or cause much stress), or doesn't care about being a good teacher and is able to get away with not caring about it, I think the reality of the job even at most research universities is being first and foremost an educator, with research as a sideline.

On the topic of gaining financial freedom as a path to being free to do research in future, Wall Street is not the only route. Marrying a successful businesswoman with her own company is another way, one that I was fortunate to follow (more by accident than plan…) These days I'm free to do whatever I want once the housework is done. 🙂

14. **colorado**
January 1, 2017

For a person coming from condensed matter theory, it is amazing that professors in HEP theory still do actual calculations and not rely on their students and postdocs to enlarge their publication list. In my field, a PhD advisor would not give an open problem to a student or postdoc without hoping to get a coauthorship. In fact, if you discuss more than an hour about what you are doing with a senior person, he or she would expect his/her name on the paper.

15. **future**
January 1, 2017

what is sad and ominous for the future of "physics" is that several people want to co-opt "theoretical physics" to mean high-energy theory / particle theory or at the very least, something quantum.

many of the best theoretical physicists i know (having spent my time exclusively at the most "famous" institutions around the country) don't work on quantum problems at all – and certainly not high energy physics. They work on condensed matter problems – quantum and classical, soft matter and increasingly, biology.

The real test for physics departments is whether they quickly redefine physics to be these exciting areas with actual deep ideas – or restrict "theoretical physics" to mean rehashing of 40 year old high-energy ideas.

The exciting new theoretical physics in biology, classical non-linear physics and condensed matter will thrive for sure.. but it may end up in biology and engineering departments in 50 years from now and "physics departments" will become the Nokia to their Apple.

16. **X**
January 2, 2017

colorado,

Faculty in HEP theory definitely rely on postdocs to generate publications (and PhD students too, to the extent that they are able). In fact it is a main way they keep up a 'respectable' rate of research output, considering that their own time for doing research is often very limited. Research projects are farmed out to postdocs and students who do most of the actual research work, while the prof's role is mainly 'research manager'. I guess this is the standard model throughout academic sciences. At any rate, the people who were productive and successful in research where I was all followed this model to some degree. (Personally I consider it a horrible model and refused to play that game…)

17. **Why**
January 2, 2017

To those thinking that it is a good idea to work in business until retirement and then start doing research: can you give me one single example from the history of physics where somebody made a significant contribution to physics after retiring?

18. **Another Anon**
January 2, 2017

That's a great piece of writing by Bob Henderson. I think it answers his question of where it all went wrong: his talents lay in being a writer, not being a physicist.

19. **Thomas Larsson**
January 2, 2017

Why,

Although not exactly physics, an example would be Leopold Kronecker, who retired from the family business to pursue a career in mathematics. Of course, most of us cannot afford to retire at the age of 32.

20. **Jeff M**
January 2, 2017

The whole "research manager" thing I find amazing. I appreciate it's very common in the sciences, but in math from my experience it's very different. A postdoc working with a senior researcher would only put the senior persons name on a paper if they wrote it together. It wouldn't happen if the senior person suggested a topic, and then made some suggestions while the postdoc was working on it. The senior person would get thanked, sure, but not writing credit. When I started work on my thesis, my advisor suggested something, which I went off and worked on. After a few months, I realized I could show that what my advisor thought was true wasn't true, but I had an idea what should be, and that it was interesting. But I had no clue how to prove it. Neither did my advisor. So he asked around, and a friend suggested an idea he had once had for a related problem, which he never ended up working out, since he figured out an easier way to do what he wanted. So I worked it out, all the details, and proved what I wanted. I thanked the (very famous) guy who suggested the basic technique, but he certainly didn't get writing credit, and in fact the technical lemma is known as "M***'s lemma" in the field. I did do all the hard work on it….

21. **Oldster**
January 2, 2017

Why: Does Copernicus count? I think I recall from Arthur Koestler's book, "The Sleepwalkers", that he was actually on his deathbed before he saw the first printed copy of his book. He'd been somewhat known as an advocate for the Heliocentric Theory prior to that, at least locally. And maybe that doesn't count as "retirement", even though he was dying …

22. **Why**
January 2, 2017

Thomas Larsson,
Thank you for your answer. But that was an exception that confirms the "rule": one generally does not produce anything significant after age 60. I am not well aware about teaching in the U.S. but why somebody like Henderson does not teach at high school after getting a PhD and have plenty of time for independent research instead of

waiting until age 50 or 60 to retire before starting research? Besides, I guess one does not need a PhD to teach in a high school, so why all this pressure on physicists who really love to teach and do physics?

23. **Jeff M**
January 2, 2017

Why,

Teaching high school is hard. Doesn't leave any time for research. And leaves no time for things like going to talks and conferences and such, except over the summer. The high school teachers I know are some of the hardest working people around.

24. **vmarko**
January 2, 2017

Why,

Teaching at a university and teaching in high school are two completely different things. In high school, the course material is very easy, but you typically have several classes per day with 20-30 pupils in each class. You need to give classes basically every day and every week, which means no time to attend meetings, workshops, conferences etc (as Jeff said). In addition, you need to provide exams to all those pupils, and grade them, which takes a huge amount of time. In other words, its intellectually easy, but the workload is enormous.

In contrast, at a university, the course material requires some preparation, but you typically have one or two courses per semester, which amounts to giving lectures twice a week, to 20-30 students in total. You have the ability to skip or reschedule the lectures to make room for travel, and you have a teaching assistant who does all the grunt work of working the students through the problem books and grading their exams. So it's intellectually less trivial, but the workload is nowhere near the amount of high school.

Best, 🙂
Marko

25. **A reader**
January 2, 2017

Why, your question is interesting. I don't know whether it is OT or not, but … IMO, it is not very useful to repeat/promote that myth(?), that somebody can do research working all alone. Is there any modern example (say after 1946) of a theoretical physicist or a mathematician who has been able to do proper research without being part of/interacting with a community of peers? See for example 3. How is mathematical understanding communicated? in that essay of William Thurston, On proof and progress in mathematics, which Peter Woit mentioned a long time ago: http://www.ams.org/journals/bull/1994-30-02/S0273-0979-1994-00502-6/S0273-0979-1994-00502-6.pdf (arxiv.org/pdf/math/9404236v1.pdf)

26. **Peter Woit**
January 2, 2017

A Reader,
Perelman came up with his proof of the Poincare Conjecture while not having an academic job (I don't know how he was supporting himself, certainly not by working at another job).

27. **Dragster**
January 3, 2017

Why,
And, out of curiosity, what percentage of tenured professors do you feel have made a 'significant contribution to physics'?
Seems retired amateurs aren't necessarily lagging so far behind if you want to use that metric. Perhaps they just feel less compunction to publish mediocre papers.

28. **Why**
January 3, 2017

Thank you all for your comments and answers.

29. **Petite Kabylie**
January 3, 2017

Through Why's question and others replys and comments I understand now better why unifying the laws of Nature is not the holy grail for a theoretical physicist as is a tenured professorship!

Thank you!

30. **A reader**
January 3, 2017

Peter, thank you!

31. **srp**
January 3, 2017

Great article and informative thread here. Thanks.

32. **Pavel Krapivsky**
January 3, 2017

Perelman quit his job at Steklov Institute at the end of 2005, long after he submitted his proof to arXiv. I watched a Russian documentary about Perelman where some of his colleagues were guessing that his mother pension plus support from his sister were crucial. The wiki article about Perelman refers to articles in Russian press claiming that from 2014 Perelman is working, perhaps part time, in Sweden. His sister apparently works in Stockholm, so hopefully Perelman is surrounding by family and happy.

33. **Yi-Zen Chu**
January 4, 2017

Jeff M's comments above (January 2, 2017 at 9:54 am) reinforces why I hold mathematicians with higher intellectual regard than I do theoretical physicists.

Jeff M: How do mathematicians maintain this high level of intellectual and scientific integrity — i.e., where only the ones who did the actual work get on the paper? Is this unspoken culture, or are there concrete incentives in place that support and encourage such behavior? As the job market/funding withers and competition increases, is there any discussion regarding how the math community can continue to maintain such high standards?

I have been wondering for a while now, why major journals in physics (Physical Review, JHEP, JCAP, etc.) do not, at the minimum, require authors to enumerate their individual contributions in some detail. This would level the playing field at least a tiny bit for people who do not wish to play the game of riding on other people's hard work, or be exploited by others. (Much more, of course, can be said regarding the credit structure of theoretical physics.)

Lest Peter boots me for being off-topic, let me mention this is highly relevant to the experience of being a graduate student in theoretical physics. It is indeed true that the (rarely challenged) cultural norm of theoretical physics is that, if the adviser came up with the idea, he/she would most definitely show up on the paper — regardless of how much work he/she had put into the actual implementation. It's almost as if the details aren't important; when we know. in science, this is untrue. Furthermore, the student often ends up being mentored by postdocs and other grad students, especially if the adviser is very senior. When the project succeeds, the adviser gets to put it on his/her CV, which will be viewed by funding agencies and colleagues as yet another contribution to his/her field. With the money obtained, the adviser is able to pay his/her summer salary, his/her standing with school administration advances further — and quite importantly, he/she gets to travel around to interact with distant colleagues; build his/her network; write yet more papers simply by "chatting" with others; etc. What the graduate student may not realize at first, is that many "ideas," "insights," or "open questions" in theoretical physics are bounced around among the people in the know/loop; and so while the student may wonder in awe how the heck his/her adviser came up with these research directions, the basic framework/questions are often quite well known to researchers who have been in the field for a while. On the other hand, when the project progresses very slowly because of genuine technical/scientific issues or even fails completely, the burden of the ensuing consequences — say, the lack of postdoc opportunities because of the low rate of publication — oftentimes falls squarely on the lower ranking members, particularly the graduate student. How this is an ethical reward structure, especially given the dire job situation of our times and given we are supposed to be "scientists," is something that has boggled me for many years. (I also believe these issues are intimately tied to the reliability of the theoretical physics research literature.)

34. **chethan krishnan**
January 4, 2017

I am sure there are plenty of senior guys who piggy-back on the blood, sweat and tears of grad students and postdocs, but that doesn't mean that there is no value to the leadership, intuition and experience that a senior person brings to the table.

The skill of asking the key questions and having the key ideas that solve problems requires maturity. Together with a bit of (delusional?) confidence, a sense of direction, and an ability to not panic while fumbling in the dark. This last bit is what happened to Henderson as he beautifully narrates in his piece.

In Jeff M's example, think the senior person deserved some credit. He had a key input in solving the problem. He didn't want the credit, but that doesn't change that fact. It is also a tribute to Jeff M's integrity that he is open about it.

For the purpose of full disclosure, I have a permanent job. But I eat my greens and I calculate, dammit! 🙂

35. **piscator**
January 4, 2017

Some comments on this.

1. In the big picture of science in terms of authorship, theoretical physics is pretty close to maths. Go look at any lab subject to see papers where the senior author *really* has no intellectual contribution to the subject (conventionally the last name).

2. A lot of the comments here miss the fact that a paper by a graduate student on an OK-but-not-revolutionary topic gets *more* impact with a senior person's name on it than if by themselves – this helps the student not hinders them, and senior folks' reputation in theoretical physics is not normally based on being senior author on OK papers done primarily by graduate students.

3. Problems given to graduate students are often of the 'I know how to do it if not the precise result, these are the broad methods that will work, this is the broad structure of the result.' The 'idea' is a large part of the work, and normally the advisor will also be writing parts of the paper as well (relating the work to the wider context of the subject etc).

4. Probably the single biggest failing of graduate students is to have an overly-narrow focus on the precise technical details of their own calculations, at the expense of understanding *why* something is important (and therefore focus on calculating *things that matter*).

5. Scientific contributions are on a logarithmic scale. Most advisor/student papers are as much training projects as they are advances in science. Reputations are based on big results – and important, open and easily doable problems don't get assigned to graduate students, they get done as quickly as possible by anyone who encounters them.

36. **Bernd**
January 4, 2017

piscator,

at least in some areas, #1 is not true anymore. In some groups, it is normal for pure theory papers to have about a dozen of authors. This is crazy of course. However, people involved in this game receive much higher citation statistics so they have an easier life on the job market, perpetuating this practice.

While #2 might be true for the impact of the particular paper, it is not helpful to the individual student. If you have a single author paper, it will look much better on your CV than if all your papers are together with your PhD advisor.

37. **piscator**
January 4, 2017

Bernd:

My more detailed statement would be that the optimal positioning for a student on the job market is to have one or two papers that are by themselves or with other students, and the rest coauthored with senior people. So I broadly agree with you here, ideally a student in TP will neither have all papers by themselves or all papers with their advisor.

38. **Richard**
January 4, 2017

An interesting, enlightening — and ultimately * depressing * thread: as I made my way through it (N.B. I read the Henderson piece in its entirety before doing so.), I could not shake the increasingly intense feeling that the garment — to the weaving of which such a thread best lends itself — is a shroud….

39. **Jeff M**
January 4, 2017

Some general comments. First, there aren't really any incentives in math in relation to giving author credit, it's just the way it is. I think the fact that there is much less money in math is part of it. It's also maybe that it's hard to imagine a math paper with 15 authors if you've ever written a math paper. It's pretty hard to imagine a paper with 4 authors, although I know a few (they tend to be from conferences, where some people got together over lunch and wrote something). As far as the senior person who helped me on my thesis, he of course did get credit, just not author credit. I thanked him, something like "I would like to thank so and so for suggesting this technique, from an unpublished manuscript." In the published version and the in house thesis both. Very common in math papers.

40. **colorado**
January 4, 2017

I do agree that collecting data or calculating numbers is not doing science. Figuring out which quantity to measure or calculate, coming up with the way to do such measurement/calculation, and analyzing the data/numbers is the real part of the science in action. In my experience, many students and postdocs do most of these things, while the "PI" just tells his/her minions to work on a particular area. If the PI is paying the students and postdocs, then it's alright that they are expected to work together with the PI. My gripe is that the PI's are never working, and they expect their work to be done by the students and postdocs.

41. **Radioactive**
January 5, 2017

I don't know if there was really a problem with Henderson or his advisor here¸ more that the inspirational material he read didn't describe the reality of doing theoretical physics. Kind of like watching the Olympics and assuming that you'd be standing on the podium without realising most of your life would be training, and that the vast majority of people who try, fail.

Who doesn't want to solve the mysteries of the universe? That's simply not enough.

42. **Justin**
January 5, 2017

How difficult is it for a particle physics graduate to switch into mathematics where I think the employment prospects are much better? At my undergraduate institution (only a top 70 school) the phd math graduates had decent employment rates at very small unknown four year colleges. Maybe this isn't what one would consider glamorous, but if the physics grads can't find any physics jobs, isn't the switch to math an option they could consider?

43. **Jeff M**
January 6, 2017

Justin,

I can tell you my school we wouldn't hire someone with a physics PhD, we have way to many math PhD applicants to consider it. I'm a state U (second level) I tend to think liberal arts schools are the same. Even in math people have trouble getting an academic job, though if you are willing to teach anywhere and have good teaching refs you can often get something. We're hiring this year, we got our ad in late (state budget issues), it only got posted about christmas, and I just checked, we already have 218 applicants. Plenty of them with degrees from top 20 schools.

44. **bks**
January 6, 2017

The problem is neither theoretical physics nor capricious advisers, but rather the mind-numbing, interminably gray weather of Rochester. I relapse to seasonal affective disorder just thinking of my 15 years there. It is no surprise that in the third paragraph Henderson has his epiphany under the first blue sky *in the wake of one of Rochester, New York's typically brutal winters*.

45. **Yi-Zen Chu**
January 8, 2017

Chethan Krishnan (January 4, 2017 at 5:12 am): Senior scientists most certainly possess the potential to contribute intellectually to the papers their names appear on — from the ideas on and guidance towards the important questions; to providing invaluable insights and problem solving approaches along the course of research; to suggesting the appropriate checks that ought to be performed after an arduous calculation; etc. I personally view these as a vital part of the mentor-ship a senior scientist should be providing to his/her students and postdocs. My main point is, given the contrast between the reward structure faced by established scientists and those by the students/postdocs — as well as the dearth of jobs/over population of students and postdocs — how frequently does this mentor-ship scenario actually play out? (Expecting senior physicists to perform their own independent calculations is largely out of the question these days; this is why those who still write papers on their own certainly command my respect.)

On the other hand, I should have clarified that my observations are somewhat US-centric, having spent graduate school and postdoc years here. In particular, I have little sense of how the Indian academic/theoretical physics system works. Finally, please do not view my comments as a reckless attack against all senior theorists. They are, instead, based on a genuine desire to uphold high standards of scientific honesty. (It is because I take these issue serious, that is why I do not post these comments anonymously — as you probably can recognize, as a lowly postdoc, my comments do carry some level of personal risk if they are misconstrued.) Mature scientists should be able to look to other scientific communities — in my case at hand, mathematicians — to adopt whatever good practices they have, so as to help safeguard the integrity of one's own scientific community.

piscator (January 4, 2017 at 5:25 am): Your #2 — that getting senior people on papers benefits the junior ones — is, strictly speaking, a political statement and not a scientific one. Now, I am just as guilty as the next person for clicking on an arXiv paper because it has a famous name on it. But isn't this precisely why it is an issue of integrity that we should be sensitive to? In addition to the intellectual discipline we should exercise, to judge a paper by its merits and not by who wrote it — should we not question who actually did the work and implemented the ideas involved? (This is why I suggested all journals should, at the minimum, require authors to enumerate in some detail their individual contributions.) Why are we unfairly rewarding people who have infinite job security while watering down the credit of those who likely have to quit some time down the road? I would also point out, the more everyone buys into this mode of operation, the more the incentives are stacked in favor of established people and against the students/postdocs.

Do you recognize your #4 describing the failure of the adviser involved too? In other words, when a student "fails" to sift out the important physics from the complicated technicalities, is it not the adviser's job to steer him/her back on course? I believe the cultural norms and ethical standards of theoretical physics oftentimes place unfair and unrealistic expectations on graduate students. While academia pays lip-service to the notion of "diversity" — and from what I can tell many physicists do claim to subscribe to liberal and/or progressive values — the reality on the ground is really quite different. How can we claim to truly value a diverse range of viewpoints if we do not seriously invest the effort to nurture and support budding scientists, i.e., the graduate students/postdocs?

Jeff M (January 4, 2017 at 3:44 pm): Incentives need not be very explicit "if you do X you will receive Y," although the money/funding you mentioned likely serves as a significant driver of behavior among more senior theoretical physicists. Given we are social animals, cultural norms exert powerful pressures on us all — sometimes for the good. If, for instance, mathematicians "grow up" watching their advisers being extremely scrupulous about giving proper credit and finding multiple ways to check their work for errors — these practices will likely be passed on to the next generation. On the other hand, if questions such as "Can we take limits to check if this messy answer makes physical sense?" come up rather infrequently; while students/postdocs witness it is the people who publish O(10-15 papers) per year that ultimately get hired — then these are rather concrete disincentives against upholding basic standards of carefulness/thoroughness. In theoretical physics, "bend-over-backwards" attitudes are essentially extinct; while fashion-chasing and publication-rate-amplification is high on most people's agenda. As I've alluded to before, given the current climate, I worry about how reliable the literature is. (The sloppy nature of peer review does not help either — but here, I am wary about straying too far off-topic…)

46. **anon**
January 14, 2017

It seems utterly depressing that the best minds of the last two generations (at least) have been guided by circumstances and a changing culture into squandering their talents on toys (e.g. social media) and predatory finance.

New Year's Multiverse

January 7, 2017
Categories: Multiverse Mania

I see little to be hopeful about the new year, but had a glimmer of a hope that we'll see a reduction in Multiverse Mania. Surely people will sooner or later get tired of stale pseudo-science. Just got back to work from vacation and it seems that so far this is not working out at all, quite the opposite.

At the yearly Edge question site, Martin Rees's answer to the question "What scientific term or concept ought to be more widely known?" is The Multiverse, and he starts out with the usual sort of breathless hype:

> An astonishing concept has entered mainstream cosmological thought…

Critics of the multiverse are described as having two arguments:

- "Some claim that unobservable entities aren't part of science."
- "Some physicists don't like the multiverse: they'd be disappointed if some of the key numbers they are trying to explain turn out to be mere environmental contingencies governing our local space-time patch—no more truly "fundamental" than the parameters of the Earth's orbit round the Sun."

The first of these is the usual straw man argument, painting multiverse critics as too ignorant to realize that much of science is based upon indirect evidence, not direct observation. The actual argument of this sort against the multiverse is not that we can't get direct evidence for it, but that there is no evidence of any kind for it, direct or indirect, and no plausible prospects of getting any. This case has been made ad nauseam here on this blog.

The second of these arguments is treated in much more detail in a new article at Nautilus by string theorist Tasneem Zehra Husain with the title Even Physicists Find the Multiverse Faintly Disturbing. Husain treats in detail the question of how physicists "feel" about the multiverse, and like Rees, makes the point that what physicists don't "like" about the multiverse is that it removes hopes of being able to do things like understand the nature and strengths of fundamental forces, or calculate the masses of elementary particles.

Rees tells us that physicists are wrong to feel this way, that instead they should be awed by "the revelation that physical reality was grander and richer than hitherto envisioned" and that "If we're in a multiverse, it would imply a fourth and grandest Copernican revolution." Husain in the end seems to agree, quoting Gian Giudice:

> Perhaps we need to let go of something we're holding onto too tightly. Maybe we need to think bigger, refocus, regroup, reframe our questions to nature. The multiverse, he says, could open up "extremely satisfying, gratifying, and mind-opening possibilities."
>
> Of all the pro-multiverse arguments I heard, this is the one that appeals to me the most. In every scenario, for every physical system, we can pose infinitely many questions. We try to strip a problem back to the essentials and ask the most basic questions, but our intuition is built upon what came before, and it is entirely possible that we are drawing upon paradigms that are no longer relevant for the new realms we are trying to probe.
>
> The multiverse is less like a closed door and more like a key. To me, the word is now tinged with promise and fraught with possibility. It seems no more wasteful than a bower full of roses.

Rees and Husain do a good job of showing that if science is about feelings, then Multiverse fans have a fine argument against critics arguing based on their negative feelings. The problem of course is that science is not about feelings but about evidence. The argument by critics that needs to be addressed is that there is no evidence at all for current multiverse scenarios, and no plausible way of getting any by scientific methods.

Nautilus has another multiverse-related piece just out, We Have Pushed Physics Too Far, by Marcelo Gleiser. My reading of the piece is that Gleiser agrees that the Multiverse is not successful science ("Parallel universes are a non-answer"), and I believe most physicists also agree. Unfortunately the lessons he draws from this (as I'm afraid many others are doing) is that the problem not a particular research program that failed (string theory, by ending up with the string landscape and the multiverse), but the whole idea of pursuing mathematical ideas about further unification:

> We can call this the ultimate Platonic dream, the quest for a single simple and broad-ranging theory of physics. Indeed, during the past four decades, the search for such a theory has inspired many of the brightest physicists in the world. But today we are seeing the limits of this Platonic thrust to mathematize nature, due to a lack of experimental validation and several theoretical obstacles—including the possibility of multiple universes and the troubling questions they pose.

Gleiser sees successful physics as "an expression of intellectual humility", with our current problem that of Icarus, trying to fly too close to the sun. I strongly disagree with him about this, seeing some of the best of physics as an expression of intellectual arrogance, not humility. It is intellectual arrogance that has gotten our understanding of nature as far as it has gone, and it will require intellectual arrogance to go farther. The current problem of theoretical physics is due not the sin of arrogance, but to a somewhat different one, that of refusing to admit error. Multiverse mania is largely about the refusal to admit that string theory unification is a failed idea. Yes, arrogance is one reason for this refusal, and admitting failure takes some humility. But then moving on to find different, more successful ideas will require a lot of both mathematics and intellectual arrogance.

Update: One more article at Nautilus about the multiverse. At least this one is explicitly theology, it explains:

> a section of liturgy recited whenever we take the Torah out of the ark, and it's related to a prayer that many Jews know, "Adon Olam." The phrase is usually translated as "Sovereign of the Universe," where the word olam can mean both "the universe" and "eternity," expressing tremendous expanses of both space and time. But in this particular section of the Torah service, God is called "Adon Olamim," where the suffix -im makes the word plural. This means that God is "Sovereign of the Universes," as in, "more than one universe." God doesn't need to be a designer who had a specific plan in mind that led to the creation of humanity. God is, in fact, the Sovereign over all the universes, including the ones that don't have life in them.

Update: Yet more explicit theological coverage of the Multiverse at science magazine Nautilus, with an article from Mary-Jane Rubinstein, a professor of religion, who is interested in multiverse versions of pantheism and explains:

> As a professor of religious studies, I am particularly drawn to the places where religion and science seem antagonistic, but turn out to be entwined. The multiverse, I would argue, is one of those places.

My only disagreement here would be whether being a place where science and religion are intertwined is a good or bad thing…

Update: Yet more in the Nautilus series on the Multiverse: more theology, and now teleology.

Update: In case you were worried that Multiverse pseudo-science was incompatible with the Quran, have no fear.

Update: 2017 is well on its way to a bumper crop of Multiverse Mania. Today it's New Scientist's turn.

Update: This crap is just endless, more every day. Today it's at Astronomy Magazine, about this nonsense, debunked long ago by Jennifer Ouellette.

Comments

1. **reader**
   January 7, 2017

   This is a tired topic, but I find your point of view quite odd. The global structure of the universe is either essentially homogeneous or it is of the multiverse type. We don't know which version is true, so it seems quite reasonable to consider both options. What exactly is your stance: that the multiverse picture is ruled out in some way, or that the whole question is pointless? To me, the homogeneous versus the multiverse structure are roughly equally likely a priori. It doesn't seem so far fetched that the two options will leave different observable imprints of some sort. Whether this is indeed the case is a difficult question that requires serious thought and analysis, yet you call this pseudo-science. Speaking for myself, there are few questions more grand than that of the global structure of the universe, and we should do everything we can to look for experimental signature before declaring that the search is futile. Your negativity on this matter is puzzling.

2. **S. P. Nova**
   January 7, 2017

   Perhaps confidence would be a better term for what you mean, rather than arrogance, which has never served anybody well. Quite the contrary. Confidence and humility can co-exist. Arrogance and humility cannot.

3. **Jeff M**
   January 7, 2017

   I don't see why unification is mathematization. All physics is mathematization. The four forces are mathematized if they have nothing to do with each other. The fact that a mathematical structure exists which seems to show some relation between three of them is no more mathematical than their individual structures, it's just different math. And I would agree with SP Nova about confidence, though I have known plenty of arrogant physicists….

4. **neil**
   January 7, 2017

   "Multiverse mania is largely about the refusal to admit that string theory unification is a failed idea. "

   I agree that the string theory multiverse is a failed idea for the reasons you state, but what about the eternal inflation multiverse, or for that matter the MWI? In these cases the appeal seems more about the anthropic principle or interpreting QFT than the multiple compactifications of string theory. Do you find these ideas any more appealing than string theory unification?

5. **Laurence B Lurio**
   January 7, 2017

   There is an article by Steven Weinberg in the most recent New York Review of Books where he says he is looking for a modification to quantum mechanics in order to avoid the multiverse issues. See http://www.nybooks.com/articles/2017/01/19/trouble-with-quantum-mechanics/#fn-3

6. **Peter Woit**
   January 7, 2017

   S.P. Nova,
   I was specifying "intellectual" arrogance as opposed to other kinds, and by that I meant just the opposite of Gleiser's "intellectual humility", which to me means having a low opinion of one's ability to solve difficult problems. Having an unreasonably high (and thus "arrogant") opinion of one's ability to solve an intellectual problem is generally a prerequisite for making a serious attempt at solving the most difficult unsolved problems.

   As for the more general sort of arrogance, there's a lot of that too among successful theoretical physicists, and the part of that characteristic that leads one to refuse to admit something doesn't work is generally very much not helpful.

   "Intellectual humility" says that the puny human brain is incapable of understanding how the world works at the deepest level. Unlike Gleiser, I see the practice of fundamental theoretical physics as a rejection of this idea and the pursuit of its opposite.

7. **Peter Woit**
   January 7, 2017

   neil/Laurence B. Lurio,

   The multiple worlds interpretation of quantum mechanics has nothing to do with the multiverse discussed in these articles, one where laws of physics are different in different universes. Bringing completely unrelated things together, making it impossible to know what one is actually talking about, is part of multiverse mania. I don't want to discuss MWI here.

   I read the Weinberg piece, didn't see anything at all new there, and it's off-topic here.

8. **Peter Woit**
   January 7, 2017

   reader,
   My claim is that the question is pointless in the sense that it's not now science. Maybe someday someone will come up with a multiverse theory that explains something and can be (indirectly) tested. At the moment the "theories" being put forth that have different physics in different universes don't explain anything, all they are is elaborate excuses for failure.

9. **S. P. Nova**
   January 7, 2017

   Perhaps "reader" confuses the general concept of inhomogeneous cosmological models, of which there are many, with the specific concept of a string theory multiverse of 10^500 universes with different constants and physics in each.

10. **Peter Woit**

January 8, 2017

S.P. Nova,

Yes, that's the essential point. That the Big Bang was just one of many such events, with lots of other universes out there with the same physics as ours is a plausible idea, and you can even claim it follows from your favorite inflationary model. Personally I'm not very interested since it explains nothing about unanswered questions of fundamental theory, and I doubt it has testable implications. A claim that there are lots more universes out there like ours, but disconnected from us, may be thrilling to some but it tells us nothing about ours.

Unless you have a model you can calculate things in (unlike the string theory landscape) a multiverse with different physics in different universes also tells us nothing about our universe, all it adds is an excuse for why we can never calculate certain things (they depend on which universe one is in). Maybe this excuse is true, maybe not, but lacking any way to calculate things and make testable predictions, all it is is an excuse, and pseudo-science rather than science.

11. **reader**
January 8, 2017

I would like to stress that whether or not you like the multiverse or find it interesting has no bearing on whether or not is a true fact of our world. Everyone agrees that it would be lovely if, for example, SU(3)xSU(2)xU(1) was the unique outcome of some deep new principle yet to be discovered. But the world might not be like that — it might just be a more or less random fact about our local vacuum. It's a challenging situation, since it's hard to think of ways to distinguish the two possibilities. What is pseudoscience is to pretend that this issue does not exist.

12. **Thomas**
January 8, 2017

reader,

it might be that SU(3)xSU(2)xU(1) is due to the sequence octonions, quaternions, complex, real; it might be that SU(3)xSU(2)xU(1) is due to the three Reidemeister moves; it might be that SU(3)xSU(2)xU(1) is due to some other principle. But SU(3)xSU(2)xU(1) cannot be due to the multiverse, because that term makes no sense: multi-everything is a contradiction.

13. **a1**
January 8, 2017

Re arrogance vs. humility I would say that humility is to admit that we still do not know everything knowable. But pushing humility too far would destroy science, it would amount to say that we will never know. Saying 'never' is of course arrogance. Multiverse theory is arrogant as it assumes that we already know everything relevant: obviously this is not enough to explain some features of physics, so a multiverse has to be hypothesized. But the real point is that it obliterates our lack of relevant knowledge.

14. **Bee**
January 8, 2017

"In every scenario, for every physical system, we can pose infinitely many questions."

The multiverse is great because it means they can write papers about fictional universes until eternity.

15. **Peter Woit**
January 8, 2017

reader,
Yes, the string landscape explanation may be true, it also may be true that SU(3)xSU(2)xU(1) was chosen by the Jolly Green Giant for reasons we can never understand. The problem is that neither explanation is a scientific one.

16. **reader**
January 8, 2017

Thomas,

You left out the possibility that SU(3)xSU(2)xU(1) might not be "due" to anything. Of course the danger lies in jumping to that conclusion prematurely, which is why it seems best not to "believe" in the multi-verse, but rather to accept it as a possibility and remain vigilant for signs of its (non)existence.

Here is the kind of questionable logic one reads on this blog and elsewhere: string theory predicts the multiverse, the multiverse in inherently unpredictive, therefore "string theory unification doesn't work". This logic would only make sense if there were some alternative theory that had a monoverse and that made some successful prediction beyond what string theory can do. But in the absence of that, if you assign a priori 50-50 odds for multiverse or monoverse, you can't possibly use the "string theory predicts multiverse" line to argue that string theory is "wrong" or "doesn't work" — that makes no logical sense.

17. **Jeff M**
January 8, 2017

I think Bee hit the nail on the head 🙂

18. **Peter Morgan**
January 8, 2017

"Multiverse mania is largely about the refusal to admit that string theory unification is a failed idea." I'll pick up on this for a different reason than that given by neil.

I find string theory/Multiverse ideas too unsupported by evidence, but to say that it's all a "failed idea" seems too much. Someone could come up with a big new idea tomorrow that changes all that. I can expend a lot of hot air on how unlikely I think that is, but that's just feelings. Nonetheless, it's always too soon to declare an idea "failed". I think far too many resources are being given to string theory, but the alternatives seem in about the same shape, of needing a new idea to make it really attractive enough as a possibility for many people to move to it. Note that I don't think of this as the common "we should all work on string theory because there's no alternative" argument, because there are plenty of slight possibilities that might deserve a decade of someone's time, or even mine, but research most always gives the desperate feeling of having made bad choices, except for the occasional, fleeting moments when everything feels kinda right.

19. **Peter Woit**

January 8, 2017

reader,
The logic is: the string theory unification hypothesis leads to a scenario that predicts nothing (or, anything, the same thing…) and thus cannot ever be tested. This means the hypothesis is scientifically empty, on a par with the Jolly Green Giant theory and many others. You can decide to believe it if you want, and devote your life to arguing that your untestable explanation is the best one around, but you're not doing science.

Peter Morgan,

I think it best to call a spade a spade.

20. **tt**
January 8, 2017

"you assign a priori 50-50 odds for multiverse or monoverse,"

Occams razor would not agree with that prior

21. **reader**
January 8, 2017

Peter,

If we knew for certain that the multiverse hypothesis cannot ever be tested then I would basically agree. But this conclusion seems rather premature, although I admittedly can't provide you with a clear path towards testing the theory.

What odds do you assign to the multiverse being a true description of nature? I start with something like a 50% prior belief. Weinberg's argument for the cosmological constant, combined with the complete failure over several decades to uncover any deeper structure behind the Standard Model, pushes the probability to perhaps 60% for the multiverse. I think that anyone whose ultimate goal is to understand the laws of nature should do this exercise, and I encourage all readers of this blog to think about this in a serious way.

22. **RandomAnonymous**
January 8, 2017

reader:
What "exercise" – you simply pull numbers out of the air. You cannot possibly expect anyone to take a starting point of 50/50 seriously.

"this conclusion seems rather premature, although I admittedly can't provide you with a clear path towards testing the theory."
If someone comes up with a testable theory then we can discuss it. Until then, the multiverse is empty metaphysics. Predict something or go home.

23. **Peter Woit**
January 8, 2017

reader,

I expressed an opinion about this question here:
http://www.math.columbia.edu/~woit/wordpress/?p=8587

24. **Roofus McLoofus**
January 8, 2017

"[…]is due not the sin of arrogance, but to a somewhat different one, that of refusing to admit error. […] Yes, arrogance is one reason for this refusal, and admitting failure takes some humility. But then moving on to find different, more successful ideas will require a lot of both mathematics and intellectual arrogance."

I will align these statements with that of Dr. Richard Hamming: "…the difference between being 'strong-willed' and 'stubborn'…I've seen people abandon a good idea too soon, and I've seen people cling to a bad idea too long. They're both difficult problems."

25. **reader**
January 8, 2017

Random anonymous writes: "What "exercise" – you simply pull numbers out of the air. You cannot possibly expect anyone to take a starting point of 50/50 seriously."
Yes, to you this probably seems like it's pulled out of thin air. But some of us have spent a lot of time thinking about the early universe and the kinds of behavior that can occur. A Lagrangian with lots of different fields and a complicated potential doesn't seem terribly unlikely given what we know, and this sort of theory seems to lead to a multiverse. Sorry if you find that uncomfortable.

Peter: thanks for the link, although it seems that you are avoiding the question. It's incredibly important to separate which observed facts about our world have a deeper explanation and which are just essentially random variables. The decades long search for deeper mathematical structure underlying the SM has so far had zero success in terms of contact with experiment. This doesn't mean we should stop trying, but intellectual honesty requires that we consider the possibility that the task is misguided.

26. **RandomAnonymous**
January 8, 2017

You spent a lot of time thinking and postulating Lagrangians, and what you got in return was a guess of exactly 50/50? I'm truly sorry.
I don't find the idea uncomfortable, I find it unscientific, there is a difference. What exactly is it that we know which points to a Lagrangian with a huge number of fields and a complicated potential?

27. **reader**
January 8, 2017

RandomAnonymous: answering your question here would take up too much space and get off topic. But I will say this: it's not "unscientific", quite the opposite. If you are a scientist thinking about fundamental theory this sort of judgement call about what might be calculable literally affects how you spend your time every single day. You have to decide: "should I attempt to come up with an explanation as to why the dark energy density / CC is what it is, or should I just view it as a random number with no explanation.". Actual scientists, as opposed to blog readers, can't avoid confronting this issue.

28. **Peter Woit**
January 8, 2017

reader,
I should note that RandomAnonymous seems no more unlikely to me to be a serious scientist than you are. You'd be surprised who comments here anonymously. The ad hominem argument isn't a good one.

The problem with your argument about how scientists work is that
1. attempting to calculate X
2. deciding that X is a random number with no explanation
are not the only two possibilities. If you don't have an idea for how to attack 1, a much more sensible attitude than 2. would be
3. I don't know what explains X, will keep this open question in the back of my mind, maybe someday I or someone else will get an idea about this.

This is the serious problem here: instead of just admitting that string theory unification doesn't work, and we don't understand where some things come from, multiverse maniacs claim that string theory shows that no testable explanation is possible. There's a big difference between telling people "I don't know how to solve this problem" and "This problem can't be solved". Here I guess I'm with Gleiser that some humility is in order. Just because you have failed to solve a problem doesn't imply that the problem is insoluble.

29. **reader**
January 8, 2017

Peter,

option (3) is of course what most people do in practice. But when you get a glimmer of an idea, you have to decide: am I going to pursue this with full vigor, pushing my other projects aside, or am I just going to let this thought flow in and out of my brain. Taking the example of the cosmological constant, which option you choose is heavily influenced by your belief as to whether a deeper explanation exists. This sounds abstract, but it is in fact how people operate. My comment about "blog readers" was not meant as an ad hominem attack, it was to emphasize that when you are at home sitting on your couch reading a blog you can dismiss these issues, but you can't when sit in your office and get to work.
.
Saying "string theory unification doesn't work," is a gross simplification of a complex issue, and does little to advance understanding. But I agree that more humility is in order.

30. **RandomAnonymous**
January 8, 2017

Here we are both readers, in real life we are both scientists. It is clear that you are a hep-th theorist, so for fairness I'll state that I'm a hep-ex experimentalist.
I stand by my "unscientific" claim. A scientific idea may yet arise by pursuing this direction, but until then the multiverse is empty. At times we all have to decide which speculative direction to pursue, and this by necessity must be done without proper evidence, and based only on half-formed ideas / priors / guesses / etc. For this purpose the kind of reasoning you present above is ok – but absolutely not as a serious explanatory statement about nature. This is currently a big problem with your community – that the speculative ideas currently regarded as most promising get presented far too much as accepted fact.
"Saying "string theory unification doesn't work," is a gross simplification of a complex issue, and does little to advance understanding."
As things stand, it is impossible to argue that string theory unification has worked. Wasn't it originally meant to be a theory of QCD before that? A breakthrough could change all of this and the program could turn into a spectacular success of course. Until then, I repeat the statement from my first post – predict something, or go home.

31. **vmarko**
January 9, 2017

reader:

"But the world might not be like that — it might just be a more or less random fact about our local vacuum. It's a challenging situation, since it's hard to think of ways to distinguish the two possibilities."

The way to distinguish between universal laws of nature and random contingencies is well-known: given a statement about the physical system, if all instances of the physical system obey the statement, it's a law. If only a few instances obey it, it's a contingency.

Maybe the most vivid example of this at work was the Titius-Bode law, which was considered to be a true law of nature, even used (successfully!) to make predictions, until people discovered Neptune, and later on other solar systems different from our own. At that point the "law" was demoted to the contingency of the initial conditions for the formation of the Solar system.

The problem with interpreting physical constants as a contingency of the multiverse is therefore obvious — until you find experimental evidence that in some part of spacetime the constants are different, I have no reason to say that they are a contingency. In contrast, there is overwhelming evidence that the constants are the same throughout observable spacetime, making a 50/50 prior very unrealistic.

And if you try to claim that the patches of spacetime where constants are different are necessarily beyond the observable horizon, then you are leaving the realm of science, and talking metaphysics. I can equally claim that there are unicorns beyond our observable horizon, and assign to it a 50/50 prior. That just isn't science anymore.

Best, 🙂
Marko

32. **Dan Winslow**
January 9, 2017

Well, science has to end somewhere, right? The idea that we can find every explanation for everything seems like it would have to result in a circularity or infinitely receding stack at some point.
I guess the question ultimately is : can Science contain itself? Since we are using chains of causality in our thinking, don't we ultimately have to wind up at a first cause, an outermost explanation? If we get to any explanation, isn't there then by definition the need for an explanation for that?
I am not talking about anything to do with a diety at all.I just mean that I don't think we can arrive at an explanation for everything. It's got to end somewhere, even if it's just our own ability to understand. Have we reached that spot? I have no idea, but we might be getting close. The way that current thought about physics seems to be fracturing and wandering off into theology and wishful thinking may be an indication of that.

33. **Peter Woit**
January 9, 2017

Dan Winslow,

Yes, there's a real danger that we're getting to the limits of what we can understand. What concerns me is that instead of having a situation where the open problems we don't understand are clear and people can keep trying if they wish to make progress, we'll end up instead with an entrenched influential ideology saying that the string landscape is our best understanding, and that shows that no progress is possible, no point in trying. This would be convenient for a generation of theorists who invested heavily in string theory: they can avoid admitting failure, and might figure that there's little hope of progress anyway during the rest of their careers. It would not be a good thing for physics, or for science and its public perception in general.

34. **Anonyrat**
January 9, 2017

Gravity was an elaboration on Newton for centuries, until Einstein. Particle physics might have a similar centuries-long detailing of the Standard Model before the next step. There is no reason yet to succumb to the multiverse because one or two generations of physicists haven't made the next breakthrough.

35. **Arnaud**
January 9, 2017

reader :
I think the multiverse hype is the failure of string as a theory for the following reason where I use in parallel the example of gauge theory.
A postulate of string theory is a 4+D dimensions space where the D dimensions as to be compactified to describe our universe. A postulate in gauge theory is that it exists a gauge group G describing fundamental interactions.
In string theory nobody knows how to compactify the D dimensions to get our observed physics and there is 10^(…) possibilities. In gauge theory we have a priori an infinite number of group but we know G = SU(3)xSU(2)xU(1) works very well (may be it has to be extended somehow but at least now, it does the job).
Last step, some string theorists unable to find the correct compactification for our universe postulate a multiverse. This is where the science fails. It would be like postulating that since gauge theory works here then there is another type of "gauge multiverse" where all different gauge group are realized. It is useless to understand physics in our universe to postulate that it exists other universe with other gauge groups. It is bad science to hide the fact that no known compactification describes our universe (at the precision of current experiments) by postulating a multiverse.

36. **DoZen**
January 9, 2017

Peter,

(Sorry for my ignorance, I'm not a physicist).
If string theory is not intrinsically incompatible with other approaches (representation theory, QFT, quantum gravity, twistors,….???) then all the other approaches also allow for nearly infinite mathematical possibilities. If, on the other hand, they are incompatible, that is Big News. Or does the question even make sense?

As to string theory per se: if you are stuck, why can't it be left on the back burner while other approaches are tried?

As a mathematician, it's hard to understand the struggle physicists are having. We work on things we think are interesting, and the main (only, for many of us) guide is interest and beauty. There's fairly wide acceptance of what fits these criteria. If I do something good, colleagues will be interested. So I never need have the feeling I am wasting my time, that it was all for naught! So there is no need to be defensive, or to write self-congratulatory expository articles. The reward is in the struggle, in the insight, in the doing.

I think some people confuse mathematical possibilities with physical possibilities.
Somebody could study gravitational orbits with a law different from inverse-square,
but that doesn't mean it "exists" someplace else, and it certainly doesn't mean it's
worth spending time on. If some grad student wants to do that as an exercise, well and good, but I wouldn't wake up in the morning excited to think about it- at least not yet. If I do some day, good for me; that would make me happy! But I hope I won't run around claiming this is an alternate "reality".

There are very real limits to what we can know and understand. Zen Buddhists have
understood this for a long time; their explanations are completely compatible with science. The experience of zen "knowing" by not knowing (perhaps called "just this") is quite useful and refreshing for scientists, as a lesson in humility and wonder. If you don't have to know it all, and yet "can" in a different way, you can relax a bit and have fun. The stress is less, and the delight remains. It all fits together, and it is all remarkable and amazing. The only part that causes a misfit is due to
our own egos, arrogance and jealousy. Anyone who gets to spend some time thinking about math, physics and all this is supremely lucky, can make real contributions, and could brag a little less while feeling a little more of gratitude. At least in this part of the multiverse!

37. **EThomas**
January 9, 2017

Peter,

Is it reasonable to expect all of reality to be accessible via empirical scientific methods? Is it even reasonable to expect all of reality to be conducive to mathematical modeling?

If not, where do we go from there?

(Disclaimer: I am not a fan of String Theory, or the Multiverse theories, mostly for some of the same reasons you state)

38. **Peter Woit**
January 9, 2017

Ethomas,
We have no way of knowing in advance how much further we can go, with experiment or mathematics. The only thing that's clear though is that if we convince ourselves it's not worth trying, we definitely won't go any further. So, for that I'd like to see a solid argument, not the bogus ones that prop up the string theory landscape.

As to where to go from here, personally it seems to me that there is still a huge amount we don't understand about quantum field theory, which has been our best tool for understanding fundamental physics. I don't see any serious argument for why we can't understand quantum field theory better, and hope to make progress that way.

39. **Chris Kennedy**
January 9, 2017

To put the Jolly Green Giant in the same category as the Multiverse is an insult to the Jolly Green Giant. At least there is a 55 foot statue of the JGG in Blue Earth Minnesota. There is no statue of the Multiverse (at least not in this Universe).
Personally, I think there are a lot of branches of science who are missing out on a wonderful opportunity currently monopolized by the theoretical physics community. For example: Multiverse Meteorologists could forecast for planets in the multiverse and predict the high/low temps, precipitation, etc. without fear of ever being incorrect. They might even be able to convince viewers that their forecast accuracy rate for worlds in the multiverse is 90%. Of course, once you multiply that 0.90 by "reader's" 50/50 chances of the planets being there to begin with, the more realistic figure drops to around 45%.

40. **Peter Woit**
January 9, 2017

Chris Kennedy,
I don't know about a sculpture, but there is this
http://www.crawickmultiverse.co.uk/

41. **Petite Kabylie**
January 10, 2017

Peter,

What if in 20 or 30 years a new theory, completely different from string theory, and based on sound and strong first principles, comes along and reveals the same landscape of the laws of physics as well as universes for ever disconnected from ours. Would you endorse the multiverse concept?
Thank you!

42. **Petite Kabylie**
January 10, 2017

N.B: Assume this theory has made tested predictions for phenomena in our universe.
Thank you!

43. **chris**
January 10, 2017

reader,

I am genuinely curious how you come up with a 50/50 prior for the existence of a multiverse. The arguments you gave (brooding over Lagrangeans etc.) seem purely sociological to me and i at least try to disregard such items as background noise when thinking about laws of nature.

Subtracting all the jargon, the statement "i give a 50/50 prior to the multiverse" for me is just the plain statement:
"The existence of things we can never detect is equally likely than not."
I guess this is something i would expect from an agnostic or maybe a philosopher – but what is the scientific content of such a statement?

44. **martibal**
January 10, 2017

Thanks Anonymat for your comment 🙂 I find quite arrogant the idea that "we may be at the end of what physics can explain". Why on Earth should the fact that the so-called "best minds" of the last (or two last) generations did not reach a breakthrough mean that we are reaching a limit of what could be explained ? It sounds as meaningless as "the end of history" of Fukuyama (after the end of communism, the world will leave in a the war-free happiness of free market).

Also, regarding the repetitive argument that multiverse deserves all the hype it gets because there is northing more convincing on the market, it is a bit tiring to repeat that there are others approaches which try to understand – with quite success – the structure of the standard model (e.g. noncommutative geometry). Not that these other approaches should be much more hyped, but at least they could make the the multiversemaniacs understand that it would be very, very, nice for everybody if they calm down a little bit.

45. **Peter Woit**
January 10, 2017

Petite Kabylie,
If there were a convincing fundamental theory that made confirmed testable predictions, and also predicted a multiverse with different parameters for particle masses etc, making them environmentally determined, I would be a believer in this multiverse (in the sense of assigning it a high probability and acting accordingly).

46. **reader**
January 10, 2017

Chris,

50/50 is really meant as shorthand for the middle third of three options: very unlikely, somewhat likely, very likely. I don't think that "brooding over Lagrangians" is "purely sociological", as the space of Lagrangians is our best way of thinking about the space of physical laws. And once you start thinking about this you realize how incredibly easy it is to get a multiverse picture: you basically just need a large number of fields and this is generically what you will find. The Standard Model seems to me to be intermediate between being very simple and very complicated. It's clearly a bit of a mess, but not nearly as messy as it could be. Extrapolating this lesson to higher energies where eternal inflation would take place, it seems to me fairly plausible that the Lagrangian at this scale is of the needed complexity to support a landscape and eternal inflation.

Anti-multiversalists hold on to the hope that the Standard Model is actually much simpler because there is some deep structure there that we haven't yet uncovered. Could be, but decades of effort devoted to finding this deep structure have so far yielded zilch.

47. **BR**
January 10, 2017

Reading some of the posts one could arrive at the conclusion that the multiverse is a dominant theme in hep-th, which is far from true in my opinion.
My impression is that outside the community it is discussed much more than inside. Perhaps that a few but influential scientists push for the idea,
either because for some reason they got convinced or because they simply started running out of steam and have a hard time to swallow the fact
may have some truth in it, however whatever they have to say I think has most of its impact on the general public and I suspect in fact that they are fine with that.
Historians of physics could perhaps look for some other situation in the past where a concrete subgroup of legitimate physicists declared the problems that
could not solve as (essentially) unsolvable or pointless. I would bet that this is not the first time, but I do not know.
And if those problems where eventually understood and solved or they indeed turned out to be unsolvable.
Also, more generally, I have the feeling that this is not the first period in time where the boundary region between fundamental science and philosophy is
a bit more fuzzy than usual.
On the other hand I would not be so sure that string theory and the multiverse are in one to one correspondence, at least not until
we understand its dynamics more deeply, including supersymmetry breaking and (related) non-perturbative effects.
Now wether the scientific relevance of string theory has been connected by its development too tightly to sociological and personal arrogance issues I think
is an unpleasant but fair discussion because these things are then connected to positions, grants, future research directions, education etc.
But the system can and does eventually regulate itself, one needs some patience and accept the fact that the time scale of such processes may be larger than O(30) years…

48. **vmarko**
January 10, 2017

EThomas,

"Is it reasonable to expect all of reality to be accessible via empirical scientific methods? Is it even reasonable to expect all of reality to be conducive to mathematical modeling?"

I just noticed that nobody addressed these two questions of yours. 🙂 Regarding the first question, the answer is — it depends. Regarding the second, the answer is — no.

The idea that all of reality can be understood by empirical scientific methods is a metaphysical position one may or may not endorse. People who believe in reductionism of everything that exists to (ultimately) elementary particles ("more is not different") tend to endorse that idea, and believe that everything can be reduced to (ultimately) fundamental physics. On the other hand, people who do not believe in reductionism ("more is different") are usually more open-minded to the possibility of non-scientific access to knowledge about reality, complementing the scientific one. So the answer to your first question depends on who you are talking to, and their prejudices and opinions about reality.

Regarding the second question — already in physics there are systems which resist being properly mathematically modeled. Examples include high-Tc superconductivity, mass gap in Yang-Mills theories, etc. Beyond physics (your question was about the whole reality), I think that systems which are complicated enough (things studied by biology, psychology, sociology, ecology, meteorology, etc.) will always be out-of-reach of mathematical models, despite our best efforts. This is basically a property of all strongly interacting physical systems, which cannot be studied by cutting the system into pieces and analyzing it piece by piece. The latter, so-called "analytic" technique is effective only if the interaction between the pieces is reasonably low and reasonably local, which is not the case for a huge class of systems (say, a human brain). The highly-interacting systems need to be studied using "synthetic" (also called "holistic") techniques, which study the system as an undivided whole — and most such systems are utterly awfully hard to describe mathematically. So no, it is not reasonable to expect that all of reality can be described using mathematical models.

Best, 🙂
Marko

49. **Peter Shor**
January 11, 2017

Chris: you say

> decades of effort devoted to finding this deep structure have so far yielded zilch.

These decades of effort were nearly all devoted to finding this deep structure using string theory. How do you know that the reason for the failure isn't due to string theory rather than a multiverse?

50. **Peter Shor**
January 11, 2017

Oops: that last comment should have been directed to "reader" rather than "Chris".

51. **AcademicLurker**
January 11, 2017

vmarko,

I'm not sure that "more is different" is in conflict with understanding phenomena through empirical scientific methods. After all, in P.W. Anderson's famous article, he was talking about things like states of (bulk) matter, phase transitions, & etc. I'd say we understand a lot of those things pretty well.

52. **Douglas Natelson**
January 11, 2017

To echo AcademicLurker, vmarko makes it sound like condensed matter physicists are believers in the supernatural. I don't think there's anything at all contradictory between ideas of emergence (large-scale properties that are highly nonobvious can emerge from the collective response of comparatively simple underlying degrees of freedom interacting via simple rules) and scientific empiricism. It is sometimes a lament (see RB Laughlin's first book) that emergent properties can make it difficult to access the underlying degrees of freedom, but that doesn't mean giving up on experiment and observation.

53. **Low Math, Meekly Interacting**
January 11, 2017

Hopefully I'm not offending our host with off-topic nonsense…

I love that Anderson article, but something about it makes me a bit queasy. He seems to be suggesting there's a unifying law or laws governing emergent phenomena that make them amenable to some sort of generalized system of mathematical modeling, i.e. broadly predictable using maths that are substantially less complex than the phenomena being studied.

I'm quite inclined to believe the notion that "more" is insurmountably "different", in a way that will always make the laws of fundamental physics rather useless for describing many real-world phenomena. But I see no real-world evidence anywhere that would make me believe there's some unifying mathematical law of emergence that will make disparate complex phenomena easier to comprehend, describe, or predict. Probably turbulence, high temp. superconductivity, and neurocognition have absolutely nothing to do with one another beyond involving aggregations of matter. I wouldn't think it a good idea to advise someone looking to answer a big, fundamental question, to walk down a road that tries to "unify" these things.

That's not to say infusions of greater quantitative talent aren't desperately needed in, for instance, the study of neurophysiology and consciousness. I just don't think those disciplines will ever appeal to someone attracted finding deep, unifying mathematical laws that accurately describe reality. At least, they won't appeal in anything remotely like the same way.

54. **Peter Woit**
January 11, 2017

Sorry, but enough about "more is different". This is an interesting topic, but really has nothing at all to do with the topic of this posting.

55. **reader**
January 11, 2017

Peter Shor,

These decades of effort were nearly all devoted to finding this deep structure using string theory. How do you know that the reason for the failure isn't due to string theory rather than a multiverse?

I disagree with "nearly all", or what is more relevant, even if you take away all work on string theory that still leaves a vast number of physicist years devoted to this problem. Fortunately, working on a topic like quantum field theory has its own rich rewards even if it has not so far shed much light on why the seemingly arbitrary features of the Standard Model are as they are. I certainly don't advocate ceasing to work on non-string related approaches to this issue, just pointing out that many have gone down this path before without success.

56. **AcademicLurker**
January 11, 2017

Peter,

Apologies for the derail!

More on-topic, since you've stated several times that trying to gain a deeper understanding of QFT would be a good place for more theorists to put their time and effort, here's a question. When was the last period where you would say that fundamental advances in our understanding of QFT (as opposed to the standard model in particular) were taking place at a reasonable pace? Were the 70s that last golden era?

57. **Peter Woit**
January 11, 2017

Academic Lurker,

There have been people working on QFT problems and making progress all the time, the problem has more been that the faddish nature of the field means that only a small number of possible directions are pursued by a significant number of people. In the late 80s to early 90s Witten's discoveries about topological quantum field theories were a major advance, and work on those continues to this day. The AdS/CFT business starting in the late 90s and continuing to now also led to progress on understanding some QFTs. More recently, the work on the amplitudes has led to progress.

I should stop going on about this and discourage others. This really is off-topic and deserves its own serious discussion in a better context.

58. **M Mahin**
January 11, 2017

See my post below on Husain's post, entitled "Pretzel Logic of the Multiverse Fantasists."

http://futureandcosmos.blogspot.com/2017/01/pretzel-logic-of-multiverse-fantasists.html

I discuss her strangest statement: "Logically speaking, an infinity of universes is simpler than a single universe would be—there is less to explain."

59. **Maximillian G. Tresmond, Esq.**
January 11, 2017

Dr. Woit,

As a non-physicist who regularly follows your site with interest, I have to ask: Why do credentialed scientists continue to to promote the multiverse "hypothesis" to the public when they know that the concept falls well outside the realm of legitimate science? The harm they are doing to the public understanding of science is likely to have a spillover effect to other branches which in turn could have a detrimental impact on public policy.

60. **Peter Woit**
January 11, 2017

Maximillian G. Tresmond, Esq.,

One thing to keep in mind is that the physicists doing this are a small minority of the community. Another question to ask is why more physicists aren't complaining publicly about what is going on.

As for why certain physicists do this, I'm sure that they believe that there is something to multiverse explanations and that they are legitimate science (they do argue that this is legitimate science, although I disagree with their arguments). A less obvious reason is that the multiverse is a topic that easily lends itself to a popular exposition: there's no significant difficult math or physics to explain, and people eat this up since it appears to be deep and mind-blowing, and they can kind of understand it. A physicist is a lot more likely to be asked to write a popular article or give a public talk about the multiverse than about ideas on, say, what the right variables are for computing scattering amplitudes (somehow Arkani-Hamed manages both, but he's a singular point…).

Finally, for some deeply devoted to string theory, they are torn between the multiverse being pseudo-science, and its providing them an explanation for the lack of predictivity of string theory. From them, one often hears "I don't like the idea of the multiverse, but it may be true".

61. **Tim Nguyen**
January 11, 2017

"But then moving on to find different, more successful ideas will require a lot of both mathematics and intellectual arrogance."

In my experience, another key problem is the lack of a fundamental symbiosis between math and fundamental physics. Sure, mathematicians will work on random math problems that physicists want to know the answer to (various knot/geometric invariants, etc.), but e.g. there isn't a single math department to my knowledge that has a lattice gauge theorist (despite Yang-Mills being a Millennium problem). This gap just keeps getting worse since physics moves very fast with its loose standards of rigor, leaving mathematicians, who are grounded and systematic in the dark about what's going on. On top of that, you have cultural issues – mathematicians who work on fundamental physics either aren't "doing real math" (in the conservative eyes of mathematicians) or "doing what's already well understood" (by the loose standards of physicists).

Unfortunately, I don't see a solution to this dilemma in the near future, which seems to be a prerequisite for needing more mathematical ideas (related to physics). There aren't even enough mathematicians thinking about QFT, much less string theory or multi-versey things.

62. **Cosmonut**
January 11, 2017

Another remarkable aspect about multiverse mania is that this nonsense wouldn't be tolerated in any other field.

So suppose I claim I have an economic model which will exactly predict the price of potatoes tomorrow.
Then I give a printout of a few million numbers and say:
"Look one of these is the correct price, I just have no idea which one. And by the way, the other numbers are ALL correct as well – those are just the price of potatoes tomorrow in various parallel worlds somewhere."
I can't see any circumstance where people will say "Hmm, he's got a point" instead of dismissing my potato-price model as crackpottery.
In fact, I will only make things worse for myself by getting all aggressive with "You can't prove that my numbers are wrong, so I must be right !", which is what string theory/multiverse champs seem to be doing.

I suspect a part of the story is that popular books and media have convinced people that modern physics is all about "weird and wonderful" stuff. So, if you say something sufficiently weird, it must be wonderful as well. :/

63. **srp**
January 11, 2017

Very interesting point by Tim:

"On top of that, you have cultural issues – mathematicians who work on fundamental physics either aren't "doing real math" (in the conservative eyes of mathematicians) or "doing what's already well understood" (by the loose standards of physicists)."

In the past, private science funders (e.g. Rockefeller Foundation with Morgan and Pauling on biochemistry and molecular biology), targeted these kinds of gaps by giving money to people working the particular interdisciplinary gap they had identified. Maybe that's where some of today's big players (Templeton, Breakthrough) could funnel some resources.

64. **Peter Woit**
January 11, 2017

srp,

The Simons Center at Stony Brook is doing exactly this.

Again though, the math/physics interface is a topic of endless interest to me, but has nothing to do with the multiverse, so discussion of it best left to another posting.

65. **Tim Nguyen**
January 11, 2017

"The Simons Center at Stony Brook is doing exactly this."

I was one of the research assistant professors there and it was a great experience. However, proximity of mathematicians and physicists should not be confused with overcoming the cultural barriers mentioned above. And at the end of the day, one research center alone can't change an entire cultural divide.

66. **chris**
January 12, 2017

reader,

thanks a lot for the insight. I do not agree with your proability assignment, but I think I at least understand a bit how it comes about.

Tim Nguyen,

there are math departments that have lattice gauge theorists, especially in UK/ireland for historical reasons. There also are lattice people working on the millenium problem, but they are preciously few.

I think the problem there is the same as almost everywhere: it's not bread and butter physics that you can easily sell to your funding agencies. In our research environment, who is willing and able to work on a hard problem with small chances of success and who is going to finance it?

67. **Tim Nguyen**

January 12, 2017

Chris,

That's nice to know! I imagine you might be referring to places like DAMTP? I'd be really surprised though to find an ordinary pure math department with a lattice gauge theorist. Of course, the way in which science is currently funded (a whole other topic of discussion) only exacerbates cultural differences and only encourages people to do what's normative and trendy in their field. If you don't even have enough people working on/caring about pure Yang-Mills (which is just a tiny part of the Standard Model), you can't really expect there to be any breakthrough mathematics in high energy physics. There was a time when mathematicians like Hilbert, von Neumann, and Weyl were doing math that was in tandem with deep and fundamental physics. Now mathematicians occur as citations and occasionally coauthors for physicists, rather than as leading figures.

68. **Jesper**
January 12, 2017

@ reader

you write "Anti-multiversalists hold on to the hope that the Standard Model is actually much simpler because there is some deep structure there that we haven't yet uncovered. Could be, but decades of effort devoted to finding this deep structure have so far yielded zilch."

Well, Connes and Chamseddine's work/noncommutative geometry.

69. **anon**
January 14, 2017

I miss the days when science popularization meant Carl Sagan. I don't even want to tell lay people I am a physicist anymore, since I am then invariably forced to discuss multiverses.

70. **Alan**
January 15, 2017

Thanks for the link to Professor Halvorsen's piece you gave under "more theology" above. As he says, he is religious, so can anyone give me (degree level physics) any clues how this can fit into physics? I mean, where do we go with this, where do we start?

71. **Peter Woit**
January 15, 2017

Alan,
Personally I don't think theology has anything to do with physics. There's an interesting sociological question of why some physicists have started engaging in theology, but I don't think there's a scientific question there.

72. **Alan**
January 15, 2017

Peter,
Thanks for reply, but isn't this (with respect) just the default position? Is there at least a way out of the problem he covers, as Halvorson sees this, by some input of philosophy at least. I mean, he sees a real problem. And I don't mean to go on endlessly about this (!). His point is framed by his question … "why is it the case that it's unlikely for an arbitrary universe to be conducive to life?"

73. **Peter Woit**
January 15, 2017

Alan,
The problem is that this isn't physics unless you have a testable scientific theory for what "an arbitrary universe" means. I'd claim there is currently no such thing.

Various and Sundry

January 13, 2017
Categories: Uncategorized

A few links for your weekend reading:

- If you just can't get enough of the Multiverse, Inference has commentary on Max Tegmark from Daniel Kleitman and Sheldon Glashow.
- Coverage of the important topic of blackboards is to be found here. To those ill-informed sorts who think that blackboards are the past, whiteboards or some other technology the future, I'll point out the following. When I came to Columbia back in 1989, there was a recently installed modest-sized whiteboard in the math department common room. Everyone hated it, and after many years it was replaced by a similar-sized blackboard. Last year, in a renovation of the lounge, that blackboard was replaced by a better one, and one whole wall of the room was replaced by a floor-to-ceiling blackboard. A year or so ago, a newly renovated Theory Center was unveiled here in the Physics department: floor-to-ceiling, wall-to-wall blackboards. That's the future, the whiteboard is the past.
- The latest CERN Courier has a long article by Hermann Nicolai, mostly about quantum gravity. Nicolai makes the following interesting comments about supersymmetry and unification:

  > To the great disappointment of many, experimental searches at the LHC so far have found no evidence for the superpartners predicted by N = 1 supersymmetry. However, there is no reason to give up on the idea of supersymmetry as such, since the refutation of low-energy supersymmetry would only mean that the most simple-minded way of implementing this idea does not work. Indeed, the initial excitement about supersymmetry in the 1970s had nothing to do with the hierarchy problem, but rather because it offered a way to circumvent the so-called Coleman–Mandula no-go theorem – a beautiful possibility that is precisely not realised by the models currently being tested at the LHC.
  >
  > In fact, the reduplication of internal quantum numbers predicted by N = 1 supersymmetry is avoided in theories with extended (N > 1) supersymmetry. Among all supersymmetric theories, maximal N = 8 supergravity stands out as the most symmetric. Its status with regard to perturbative finiteness is still unclear, although recent work has revealed amazing and unexpected cancellations. However, there is one very strange agreement between this theory and observation, first emphasised by Gell-Mann: the number of spin-1/2 fermions remaining after complete breaking of supersymmetry is $48 = 3 \times 16$, equal to the number of quarks and leptons (including right-handed neutrinos) in three generations (see "The many lives of supergravity"). To go beyond the partial matching of quantum numbers achieved so far will, however, require some completely new insights, especially concerning the emergence of chiral gauge interactions.

  I think this is an interesting perspective on the main problem with supersymmetry, which I'd summarize as follows. In N=1 SUSY you can get a chiral theory like the SM, but if you get the SM this way, you predict for every SM particle a new particle with the exact same charges (behavior under internal symmetry transformation), but spin differing by 1/2. This is in radical disagreement with experiment. What you'd really like is to use SUSY to say something about internal symmetry, and this is what you can do in principle with higher values of N. The problem is that you don't really know how to get a chiral theory this way. That may be a much more fruitful problem to focus on than the supposed hierarchy problem.

- Progress in geometric Langlands marches on, with a new paper yesterday from Aganagic, Frenkel and Okounkov on the Quantum q-Langlands Correspondence, a two-parameter generalization of geometric Langlands. Among many other things, they formulate (Conjecture 6.3) a conjecture generalizing the characterization (using BRST methods) of affine Lie algebra representations at the critical level that from the beginning of the subject described a major aspect of how geometric Langlands works locally (for details on this, see Frenkel's book Langlands Correspndence for Loop Groups).

Comments

1. **BCnrd**
   January 13, 2017

   The end of the blackboard article, where they say that chemists have moved beyond blackboards, is not correct — at least not universally so. At my university a new "Science Teaching and Learning Center" just opened up, as a renovation of the "Old Chem" building which had been abandoned since the late 1980's (due to safety concerns for earthquakes, I believe). The Chemistry department was adamant that the lecture rooms in this new building must have blackboards. The request was denied, but nobody told them until the building was unveiled last month and it was noticed that there were whiteboards everywhere. Some baloney was offered about high-tech markers and space-age "erasers" that are crap.

   The Chemistry faculty were livid, offering multiple reasons why they absolutely prefer blackboards to whiteboards for teaching purposes. They were very pleased that Math is standing alongside them in this matter. So now Chemistry and Math have jointly protested, and hopefully by the summer the whiteboards will be swapped out for blackboards. Then I can move on to undo the damage in the History dept. from last summer when workmen came in unannounced one day and swapped out all blackboards for whiteboards…

2. **David Roberts**
   January 13, 2017

   How many boxes of Hagoromo chalk did you buy, BCnrd? I've heard it was a lot…

3. **Peter Woit**
   January 13, 2017

   For the Hagoromo chalk story, see
   http://gizmodo.com/why-mathematicians-are-hoarding-this-special-type-of-ja-1711008881
   In that story, a "15 year supply" for BCnrd is specified.

4. **BCnrd**
   January 13, 2017

   David, after that Japanese film crew visited here to make the US portion of their Hagaromo documentary, I decided to stock up some more for the apocalypse (not realizing that the company would come back into existence in South Korea some months later). I contacted my supplier in Oakland and she made some final drop-offs on campus with her dwindling goods from the back of her van. The campus police walked by during the transaction but had no idea what was inside the packed boxes. I now have what I estimate to be a 20-year supply in my office. It fits snugly inside some cabinets.

5. **Jeff M**
   January 13, 2017

   I hate whiteboards. At my university, the reason given for switching to whiteboards has been that chalk dust is bad for computers, which is just BS. I seriously hope we go back soon. Last semester I was lucky enough to have both classes in a blackboard room 🙂

6. **J.J. Green**
January 13, 2017

With reference to blackboards in toilets, they can be found in the Newton Institute in Cambridge.

7. **Magnema**
January 13, 2017

Worse than either chalkboards OR whiteboards is having both in the same room. Yes, I have seen this. Chalkboard at the front, whiteboard on the side, and another chalkboard in the back. At teacher's request, in that room, the back and side were swapped, approximately mid-semester.

8. **anon**
January 14, 2017

I often worry that I am being poisoned by the whiteboard marker fumes I am forced to breathe (and probably absorb through the skin) at my institution. I have a macular degeneration condition whose symptoms are always worse after teaching.

9. **David Roberts**
January 14, 2017

There were blackboards in the toilets at the Erwin Schrödinger Institute when I was there in 2006.

10. **srp**
January 14, 2017

OK, troglodytes, stick to your blackboards, which are indeed wonderful for the first strokes but completely suck once you start erasing and rewriting. In the U.S. I suspect this may be an East Coast (blackboards) versus West Coast (whiteboards) thing, with the ivy-covered traditionalists stubbornly sticking to the dirty, less-readable, fewer-colored, chalk-breaking and squeaking blackboard.

11. **Jon Awbrey**
January 14, 2017

Techtonic Shift — it was the subduction of disk drives that caused the upthrust of chalk and slate.

12. **Jeff M**
January 14, 2017

@Jon Awbrey

Yes indeed, now that everything is solid state, the excuse for whiteboards is dead. Still have to convince the IT people. As for @srp, sorry, whiteboards never erase properly. The markers dry out, often several times in the same lecture. How many times have you gotten to a room with 15 whiteboard markers, none of which work? The markers smell. No comparison. As to East versus West, interesting thought. First time I remember being stuck at a whiteboard was a conference at the University of Washington, in '93. So maybe….

13. **Radioactive**
January 14, 2017

While Max Tegmark's ideas maybe wrong, I have much more sympathy for them than for Inference – a magazine which seems to consist completely of cranky letters from retired professors.

14. **paddy**
January 14, 2017

On chalk: On my first day of crumbly chalk teaching in West Africa in the late 70's, I walked into the staff room covered head-to-foot with white dust after class. The head master smiled at me and said something to the effect that this was a sign of a hard working teacher. Thereafter I could never do wrong in his eyes.

On the Tegmark reviews (and multiverse): As a lowly experimentalist with no philosophical training my main issue has always been whether probability arguments are being used where probability arguably does not apply–i.e., where one does not have clearcut arguments that the idea of an ensemble pertains. Moreover in some twists (multiverses) it would seem that positing the applicability of probability begs the question.

15. **Urs Schreiber**
January 14, 2017

> What you'd really like is to use SUSY to say something about internal symmetry, and this is what you can do in principle with higher values of N. The problem is that you don't really know how to get a chiral theory this way.

There is a program dealing with this: first uplift the extended SuGra theory back to d=11 (that's how N=8 d=4 was constructed in the first place) then compactify on ADE-singularities. This goes back to around [Acharya-Witten 01](). This program is called "M-theory phenomenology".

16. **Chris Kennedy**
January 14, 2017

A third thing: I had a great freshman chem professor who used an overhead projector. Sure he was motionless, less animated but he was able cover more material per hour and allowed us to realize our potential since less time and energy was wasted on kinetic motion.

17. **Peter Woit**
January 14, 2017

Urs,
I'm sure Nicolai is well aware of that idea, didn't mention it because he was suggesting not pursuing complicated old ideas that don't work, and instead look for something new that does.

18. **C. Denson Hill**
January 14, 2017

The reason(s) mathematicians very strongly prefer blackboards:
1. Ever try to write a complicated formula, with many indices upstairs and downstairs, with indices on the indices, perhaps with double summations up in an exponential under some integral signs, with complicated limits of integration? It is virtually impossible with (dried up) markers…and because of the chisel tip on (even a wet) marker, the width of the line you get depends on the direction of motion. And the coefficient of friction prevents good penmanship. Result with markers is sloppy and virtually unreadable.
2. The speed at which you can write a legible statement of hypotheses and conclusions of a theorem or lemma is perfectly suited to the speed of comprehension in the audience. Unlike slides, projectors, or god forbid reading bullit points, which is like chloroform.
3. Marker dust is far more toxic than chalk dust.
The only people who like white boards are those whose lectures require very little writing during the lecture. At my university, a certain provost decided to replace all blackboards by white boards…for no good reason…and we mathematicians rose up and got him fired. You should all do that!

19. **Jonas**
January 14, 2017

Interesting to see so many claim that mathematicians prefer blackboards. It is certainly not true to the degree that some here seem to think.

In my own department, a rather large marh one, the overwhelming majority prefer whiteboard and that is why we have now gotten rid of all but one of the blackboards in our building. The only one left has been kept for a few years extra but we'll soon get rid of that too.
The only ones in the department who preferred blackboards when we changed boards were some in the then 60+ age range, but they have all retired by now. None among the younger have asked for the blackboards back, and we do give proper lectures with lots of writing, and we never use projectors and slides in teaching.

I've never had a "dry" marker in years. Put the cork back on, remember to put the empty ones in the recycling bin, and you have no such problems.

20. **Bill**
January 14, 2017

One can tell a good math department from a bad one by a number of people who prefer blackboards to whiteboards.

21. **srp**
January 14, 2017

The dried-up marker issue is easily dealt with in the same manner that chalk users do–bring your own chalk (the chalk lying around is usually broken or stubby) = bring your own markers. (There will often be some duds left for you by others in the room, but also usually some that work fine.) It is easy to control a dry-erase market to write at any size–I have no idea what CDH above is talking about. As for the erasing ability of the two, a whiteboard marker is much, much easier to completely erase than a chalk mark, especially if you use the green marker.

I wonder if my West Coast hypothesis holds for Jonas.

22. **Jeff M**
January 14, 2017

srp

White boards never completely erase. Ever. Blackboards do build up dust, but a quick wipe with a wet anything fixes that. And I always bring my own markers but feel ridiculous going through 100 markers a semester, especially when I know every single other person is too. I would bet you that the average time a white board marker lasts is less than one lecture, even for people who are good at putting caps on. White board markers are much less precise than chalk. There is less feedback. Just one more way the east coast rules 🙂

23. **Peter Shor**
January 14, 2017

In my experience, good-quality whiteboards actually work well … you can erase them completely, and they stay erasable for a number of years.

However, most whiteboards you actually run into are inferior-quality whiteboards, which are much worse than the average blackboard. I don't know what the price difference between the good whiteboards and the bad whiteboards is, but I suspect it is substantial. I also don't know which brands or surfaces are good.

24. **Jeff M**
January 14, 2017

Peter Shor,

I totally believe that good quality whiteboards can be reasonable. But even you say "stay erasable for a number of years." A good blackboard is erasable for 50 years. Maybe longer.

25. **Giotis**
January 14, 2017

Regarding the Langland's paper by Frenkel et al.

Its outcome means that (as suspected) stringy degrees of freedom (i.e. 6d (2,0) Little String Theory which is just a limit of type II ST) are needed for the physical counterpart of the quantum deformed Langland's correspondence and thus the correspondence doesn't stop at 6d (2,0) SCFT.

Remember that the little strings of (2,0) LST are not the tensionless solitonic strings of (2,0) SCFT coming from D3 branes wrapping 2-cycles, they are fundamental strings of type II when we take the string coupling to zero limit.

By all accounts this was anticipated from string theory point of view, since the (2,0) LST is the high energy limit of (2,0) SCFT, but not from field theory point of view since (2,0) SCFT is perfect on its own.

Thus string theory is required instead of being a redundant description of a deep mathematical correspondence which implies the N=4 SYM S-duality i.e. of the YM Montone-Olive duality conjecture which is an extrapolation of the electromagnetic duality.

The implications are indeed profound; Nature via mathematics (and vice versa) requires Strings.

26. **Richard Séguin**
January 15, 2017

Chalk dust is probably not good for your lungs, and the solvent in markers is undoubtedly toxic. It would be instructive to survey users and look for increased COPD in chalk users, and increased cancers in white board marker users. Personally, I find the solvent smell from markers to be much more repulsive than chalk dust.

27. **theoreticalminimum**
January 15, 2017

What about using a Windows-operated convertible laptop with Windows Journal (much like what Nima Arkani-Hamed does with a convertible Lenovo Thinkpad) connected to a projector to lecture classes? I have been doing this for a few years now. It very simply solves all of the above-mentioned toxicity issues, and there are many obvious added advantages.

28. **chiz**
January 15, 2017

Types of whiteboard:

> Enameled whiteboards, also referred to as porcelain, and sometimes glass boards, have the advantage that markings can be erased completely; other materials tend to become stained over time. Enameled boards are more expensive and less used in commercial environments, but in more demanding environments with heavier use, such as educational establishments, porcelain boards are considered superior.

The solvent for marker pens is ethanol (safe) and isopropanol (not so safe).

29. **Petite Kabylie**
January 15, 2017

I am using usual power-point slides and they work fine. I think as long as one puts in all the steps in one’s derivations and synchronises one’s rate of changing slides with the flow of ideas in one’s lecture neither blackboards nor whiteboards compare.

30. **G. S.**
January 15, 2017

If Hollywood has taught me anything, it’s that true groundbreaking work is done not on blackboards or whiteboards, but on clear glass dormitory windows.

31. **Peter Woit**
January 15, 2017

Giotis,
Yes, one can look at a complicated and interesting story about mathematics and just get out of it “string theory rules!”, which is both stupid and boring. Enough.

32. **BCnrd**
January 15, 2017

@srp:

I am at Stanford, and I know from experience here and at the math departments at UCLA, Caltech, and Berkeley as well as at MSRI there is no evidence in favor of your “East vs. West” hypothesis. Even at CCR West, the room set up for seminar talks by visiting mathematicians has blackboards. On the other hand, the West Coast is a veritable “multiverse” of colleges and universities, so perhaps somewhere out there is evidence in favor of any hypothesis. 🙂

33. **Maximillian G. Tresmond, Esq.**
January 15, 2017

Whiteboards could be the bane of a law school student’s existence. The fumes given off by some of those chisel tips leave you wondering if they should be on a DEA schedule.

34. **Jim Clarage**
January 15, 2017

Another subtle problem with dry-erase involves color. Not all humans perceive color the same, either physiologically or psychologically. Some eyes (like mine, mildly color-blind) perceive no contast of green markers on white boards, yet I know professors who prefer to write in green (or use it since all the “other colors” are dried up on that day). Some students have told me they can see black-marker best, others say blue is most visible, while others say green (about 40-40-20 in my surveys). On the psychological side, many professors (or students called up to the board) will use red-marker, which to many eyes just screams error-error-panic.

p.s. I’d like to know what markers Jonas orders which never go dry. And what recycling service he has which actually can handle the rigid plastic in e.g., Expo markers. (only certain plastics can actually be recycled once they get to the plant, many just end up being burned, like the inner casing inside the old marker surely is).

35. **srp**
January 15, 2017

BCnrd: Interesting. There definitely seems to be such a coastal distinction in econ departments.

36. **milkshaken**
January 15, 2017

re solvents in erasable markers: it is not true isopropyl alcohol is unsafe, it is one of the least toxic solvents: you can actually ingest it in non-denatured form (up to a spoonful, not more) even if the taste is unpleasant. But xylenes are worse, and you can definitely smell those in some erasable markers we use – it smells like petrol and paint thinners.

37. **Tim**
January 16, 2017

Yup, I'm not sure why people who dislike whiteboards want to go overboard with false claims about health risks … the number of cancers due to white board markers is probably less than the number of verified predictions of string theory.

If you like chalk and blackboards, that's fine, but don't make up "facts" to support your position. There's far too much of that going around these days.

38. **Tim May**
January 16, 2017

I was at Intel (the company, not the intelligence agencies!) from 1974 to 1986.

Whiteboards and dry-erase markers were all we ever had. A big change from the blackboards and chalk we had in college

The whiteboards always caused me to feel "light-headed." I never learned why this may've happened. I tried directly "smelling" one around 1978 or so and nearly passed out. I had to use them, given my job, but avoided them as best I could.

(I still don't know. But I avoid whiteboards and dry erase markers like the plague.)

39. **Jeff M**
January 16, 2017

I think some people are sensitive to whiteboard markers. I certainly am, I'm sensitive to latex paint too. And my wife is an artist, we had a loft for years in Brooklyn and I had to live with paint smells constantly 🙂 On another note, and I assume Peter will take this down, I don't understand why I can't mention my shock that a famous physicist like Tegmark doesn't understand infinity, when Peter himself posted two reviews which say exactly that.

40. **Perpetual Student**
January 16, 2017

@Jeff M
I can report that the U.Washington physics dept these days has plenty of blackboards, having cursed the glare that can make some of the chalk writing all but invisible.

41. **John**
January 16, 2017

Are all the SUSY bets based on N=1 or is this a way around resolving those bets?

42. **Peter Woit**
January 16, 2017

John,
All SUSY bets and discussion of SUSY at the LHC is based on N=1, since higher N theories as conventionally understood are incompatible with what we know already about the Standard Model (chiral gauge couplings). As Nicolai comments, to use higher N in the way he suggests requires "some completely new insights", something we don't now understand.

43. **Frog Leg**
January 16, 2017

Sensitivity goes both ways. I have to wear gloves if I have write on a chalkboard; the feel of a chalkboard against my hand is very painful (as is the sound). The smell of dry erase markers does not bother me at all.

44. **serious question here**
January 16, 2017

is string/m theory based on susy N=1, or higher N?

is 11D SUGRA that is the low-energy limit of M-theory N=1 or higher N?

regards

45. **DoZen**
January 17, 2017

I'm very glad to hear Columbia is setting the pace for a blackboard renaissance.

The squeak problem of chalk seems to depend partly on the quality of the chalk,
but mostly on the experience of the writer- it never squeaks on me.

Memories: when I visited Yale, a regular diversion in seminars was Serge Lang yelling "break the chalk"! at the speaker, then watching the befuddled speaker trying to figure out what they'd done wrong (Lang's theory being that breaking the chalk destroys the resonances necessary for squeaking- but I prefer long chalk and never have that problem).

Power point and its cousins predictably put the audience to sleep, for multiple obvious reasons:
-the light is low so dozing off is encouraged;
-too often the lazy prof is reading from their own slides prepared days or months ahead while having forgotten how the proof actually goes, and arrogantly and wrongly thinking they can think while trying to read and decipher what is there above and beyond them;
-the definitions or key formulas are scattered on previous pages, making it impossible to follow the logic…
Result: I have seen maybe 2 effective PP presentations in years of conferences.

By contrast, in a board talk the presenter is forced to make the material come alive, as their brain is actively engaged in re-creating the math. They are more likely to look

at the audience for feedback, rather than at their own slides. That is more fun for all concerned. Since one is not locked in to an order, a good speaker can change the order, emphasis, content completely in response to questions and the flow of the talk.
(I have often realized while walking to the lecture -whether a course or a seminar- that it would be much better to totally invert the order- or even to change the subject!)
A whiteboard talk is in-between. I agree with all criticisms voiced here. Chalkboard is best for both the presenter and audience, with absolute best being real slate with good-quality chalk. This is not just true for letters, but also for diagrams and illustrations- try drawing a sphere with chalk versus with a slidy stinky marker.

A related new problem is that kids in school may not be learning good pensmanship,
and lately even may not learn to write (or read) cursive at all. This is a developing disaster. At a board, having to print letters slows one down too much, and is much more tiring on the arm and hand. But those who never learned how to write properly will never know the difference. As the products of such misguided educational "reform" filter up to faculty level, we'll likely see -and sleep through- more and more PP talks.

46. **bob sykes**
January 17, 2017

In 1970, I began my career at Union College, Schenectady, NY. The old Carnegie Bldg then had polished, black slate blackboards. In the whole 37 years of my career, using every kind of writing surface invented, nothing ever came even close to the perfect of those blackboards.

47. **Cormac O Rafferty**
January 17, 2017

I thought that article on blackboards was quite poor. The author never addressed the basic question – why do many mathematicians, more than other scientists, prefer blackboards?
The answer has to be that the blackboard suits mathematics better than other subjects. Which suggests that the blackboard is very suitable for writing equations, since math lectures tends to contain more equations than other areas of science. And the reason for this last is not hard to fathom – the friction of chalk and board creates a pace that is just about right for talking and explaining math. QED

48. **Cormac O Rafferty**
January 17, 2017

P.S. I discovered a while ago that writing on a blackboard used by Einstein during a lecture at Oxford in 1931 contains a numerical error, an error that casts useful light on a puzzling mistake in a cosmology paper he published that year. (Einstein's 'Oxford blackboard' is quite well known, but no one noticed the error). If whiteboards existed in 1931, I doubt Einstein's mistake would still be visible!

49. **Jeff M**
January 17, 2017

Cormac

I just looked up Einsteins blackboard at wikipedia, thanks for making my day 🙂 I can't believe I never heard of it before, I've even been to Oxford. Of course, our tour guide was a friend of a friend who took us all over, but he had been a student of politics so we didn't make it to the museum of science. Did make it to several wonderful 500 year old pubs though.

50. **C Denson Hill**
January 17, 2017

1.Probably many of you are too young to have ever experienced the sublime pleasure and satisfaction of lecturing on a real slate blackboard with good quality chalk. The lines are crisp and clear, the coefficient of friction is perfect to create a Palmer-like script, which is beautiful and easily read, and erasure is clean. You and the students leave class in a good mood.
2. But then I have noticed that many people in physics are unable to write (script), and can only print…which usually makes the precise statement of any theorem too slow.
3. Finally, if you think a canned lecture, prepared in advance, via some sort of laptop presentation is the way to go, then I suggest you just email the pdf file to the students, and they (and you) do not have to go to class…all spontaneity is lost…just have them buy the book, and you can dispense with classes and lecturing altogether.

51. **Jeff M**
January 17, 2017

I'm fine with using slides (LaTeX please, not PP) for conference presentations, since I assume my audience has a pretty good idea of what I'm talking about. For classes, they're terrible. I got stuck once teaching upper level analysis from slides, and it was really really hard to keep it interesting. I felt very detached from the material, and it was impossible to improvise when I thought of something interesting not in the slides. When I'm teaching something low level, like calculus, I don't even like to use notes since that makes it too canned.

52. **paddy**
January 17, 2017

Wendell Furry used "view foils" c. '74-'75 for his EMT class. But this due to his infirmity at the time. He did wander to the blackboard though on occasion to digress. I loved his digressions.

53. **Jonny iTouch**
January 18, 2017

For almost 15 years, Hermann Nicolai's (of Nicolai map fame!) has been publishing on the program of subsuming the 48 spin 1/2 fermions as unfaithful representations of K(E10) arose from M-theory at space-like singularities (see e.g. https://arxiv.org/pdf/1504.01586v2.pdf, https://arxiv.org/pdf/1602.04116v1.pdf, …, http://journals.aps.org/prl/pdf/10.1103/PhysRevLett.89.221601). His discussion in the article seemed, to me, to be alluding to those same publications, yet comments here seem to suggest that Nicolai's decided the M theory was just along for the ride and has some other scenario for that E10? Can someone point to some references about that, because that would surely be an interesting read?

54. **chris**
January 18, 2017

I love blackboards because they don't get in the way. No thinking about capping your pens or when they will run out. with good chalk the stroke is clear and perfectly readable down to the last inch. and you can rearrange blackboards! (why this has not been impelented with whiteboards totally eludes me).

literally, with a blackboard i can arrange my thought process in a spatial manner and concentrate on that, forgetting chalk and board, while a whiteboard (like all higher tech) is constantly competing for your attention.

and don't get me started on "smart boards". oh yes they are cool – which is another way of saying they grab your attention. more than half the brain will be dedicated to the presentation medium rather than the topic.

maybe this will change for future generations that grow up with these things. i doubt though that the naturalness with which humans can handle simple physical objects (like chalk and moving boards) will ever be reached with more sophisticated tools and their potential for distraction will always be larger.

55. **Peter Woit**
January 18, 2017

Jonny iTouch,
The fact that Nicolai doesn't mention M-theory seems like pretty good evidence that that's not what he sees as the solution. Note that he's explicitly talking about the implications of non-observation of superpartners at the LHC. If he wanted to argue that this is some sort of argument for M-theory, that would be an argument I've never heard before and I doubt he would be making it while not mentioning M-theory.

56. **Jon Orloff**
January 18, 2017

Couldn't agree more about blackboards. I had to use slides once when I had an injury and couldn't walk. I didn't like it because I couldn't write addenda. Whiteboards are awful – the ink stains and it smells like it isn't good for you. Love blackboards. I'm also retired (U. Maryland) and probably considered to be a crank on the subject.

57. **Cormac O Rafferty**
January 18, 2017

Chris:
I don't understand it either. The Oxford blackboard is not as well known as it should be; I was very lucky to come across it online before I submitted our paper on Einstein's cosmological model of 1931.
The most likely explanation is that the historians who were aware of the blackboard (as a museum exhibit) didn't realise that the writing on the board represented a model of the universe – this is the problem with historians of science who may not have a background in science

58. **Artie Prendergast-Smith**
January 19, 2017

I agree with Cormac O Rafferty that the article on blackboard preference was pretty weak stuff.

Responses here have repeatedly highlighted some of the main reasons for blackboard-boosting: tactile quality, good visibility, and (crucially for mathematicians) the natural pacing induced by blackboards.

By contrast, the reasons offered in that article — the sound discouraging interruptions, the impossibility of total erasure — read to me like fatuous just-so stories.

59. **Alejandro Rivero**
February 3, 2017

For reference. Gell-Mann article cited by Nicolai is scanned in Santa Fe:

http://tuvalu.santafe.edu/~mgm/Site/Publications_files/MGM%2094.pdf

Fake Physics

January 20, 2017
Categories: Fake Physics, Favorite Old Posts, Multiverse Mania

2016 so far wins my lifetime award for most depressing and disturbing year ever (on the front of the larger world one reads about in the newspaper and elsewhere, personally things are fine, thanks). Perhaps the most disturbing thing has been seeing the way in which people's access to information about the larger world has become more and more dominated by what has become known as "Fake News": stuff which is not true, but which someone with an agenda successfully gets others to believe. This is a problem that goes far beyond obvious nonsense fed to rubes on Facebook, to the point of including what a lot of my well-educated colleagues believe because they read it on the front page of the New York Times.

I have no idea what to do about this larger problem and no intention of further discussing it here. I've started to come to the conclusion though that the most disturbing trend in theoretical physics of recent years may best be understood as a related phenomenon: "Fake Physics". The first few weeks of 2017 are seeing a flood of examples of what I have in mind, including for instance:

- Fake Physics I
- Fake Physics II
- Fake Physics III
- Fake Physics IV
- Fake Physics V
- Fake Physics VI
- Fake Physics VII
- Fake Physics VIII (forthcoming)

Note that the above examples are just ones written by physicists or reporting claims of physicists, there are also philosophers, theologians and others putting out similar articles, although without the claims to scientific authority coming from the physicists.

Fake Physics VII just appeared and is rather bizarre. It essentially argues that the idea of assuming a Multiverse and using it to make statistical predictions doesn't work. But instead of drawing the obvious conclusion (this was a scientifically worthless idea, as seemed likely to most everyone else), the argument is that we need a "revolution in our understanding of physics" that will make the idea work.

Fake Physics shares several characteristics with Fake News:

- *It's clickbait*. While getting anyone to pay attention to the solution of a difficult technical problem in quantum field theory is likely to be nearly impossible, topics like "What happened before the Big Bang?" and "Did you know that there's someone exactly identical to you somewhere else in the multiverse, and they're dating Scarlet Johansson?" are sure crowd-pleasers. This motivates some physicists, and even more journalists, with the latter having the much better excuse that their livelihood depends on getting people to click on their stories.
- *It's a propaganda tactic designed to mask failure*. The main reason for the current mania for the Multiverse is the failure of the string theory unification program. Some who have invested their lives in this program have decided to use this sort of Fake Physics as an excuse to avoid admitting failure.
- *The group driving this is small but determined, ideology-driven and well-funded by rich people with an ax to grind. The majority of the community is unwilling to take on the unpleasant and unrewarding task of challenging them*. While Multiverse Fake Physics plays a large role in media coverage of fundamental physics, partially because of funding from the Templeton Foundation, there are very few actual papers on the subject and "research" in this area is a small fraction of what theorists are doing. Most physicists just hope that if they ignore this it will go away.

Unfortunately Fake Physics is not going away, but becoming ever more widespread. While I don't know what to do about Fake News, I think there still is a chance to successfully fight Fake Physics and hope others will help with this.

Update: There's more

- Fake Physics IX explains that the Many Worlds multiverse and the cosmological multiverse are one and the same. Sean Carroll is featured.
- Fake Physics X includes the following from Carroll:

> David Chalmers does a wonderful job at making an important point: "A virtual world is just as real as a physical world."… what's real to one person might be virtual to someone else.

People at Hacker News are discussing this. There I'm accused of "misleading people" by making them think the cosmological multiverse is the same thing as the Many Worlds interpretation. Oy.

Update: Thanks to a commenter for pointing out Fake Physics XI, courtesy of Science Friday. As usual, the same vigorous ideologues promoting Fake Physics, the same dearth of voices pointing out the problems with it.

Comments

1. **Jeff M**
   January 20, 2017

   Peter,

   I'm assuming it's the site, but if you click on any of the Nautilus links they take over your browser, you lose your history for the tab your in, every page is a Nautilus page. Not that it matters much probably, but might be worth warning everyone.

2. **zzz**
   January 20, 2017

   yeah, its like a multiverse of webpages over there

3. **Peter Woit**
   January 20, 2017

   Jeff M/zzz,

I hadn't noticed that "feature" of Nautilus until recently. Maybe they funded its development with this recent grant: https://www.templeton.org/what-we-fund/grants/nautilus-magazine

Upon further investigation looks like this inability to get back happens to those (like me) who set their browser not to save history. Best if people who want to pursue this do so with the Nautilus people.

4. **a1**
January 20, 2017

just remember the etymology of 'evangelism' – good news, not true news

5. **Mitchell Porter**
January 20, 2017

This dizziness about ever-larger realities happened before, when the stars were revealed to be suns, and people were confronted with the idea of spatial infinity. Given sufficient time, the human race should be able to sort out the multiverse question: what exactly are the options; are they true, false, or unknowable; and can they be empirically judged, or is logic the only guide.

6. **Carl**
January 20, 2017

It's not all doom and gloom. I just got done reading Penrose's "Fashion, Faith and Fantasy". And I realized that his takedown of multiverse theory works equally well as a critique of the proposal that our universe is a simulation, a piece of alleged philosophy that actually annoys me even more than multiverses.

7. **Richard J. Gaylord**
January 21, 2017

i guess your too politic to name the culprits who bear the main responsibility for your mood: Trump and Tegmark.

8. **Marty Tysanner**
January 21, 2017

While I'm not a fan of multiverse physics, I think some people may paint it with an overly broad brush by saying "It's Not Science!" without properly qualifying their claim. There is some subtlety to whether a multiverse from eternal inflation has scientific content or not.

Some of my research in grad school related to eternal inflation, and thus a multiverse. More specifically: How likely is it we could observe CMB signatures of collisions between bubble universes that "nucleate" sufficiently close together in an eternally inflating (de Sitter space) background? (My advisor had already been working with others on whether bubble collisions might have observable consequences, and the topic was interesting to me.) In such scenarios the multiverse of "bubble universes" grows exponentially in time as new universes continually nucleate — the available volume for nucleation grows exponentially in time while the rate per unit volume stays constant — so each time a new bubble occurs one expects a finite probability it has nucleated close enough to an existing bubble to collide with it; a collision would be very energetic and have a predictable symmetry.

The scenarios we considered were agnostic about whether the fundamental physics inside the different bubbles (e.g., electron charge, spectrum of particle masses) were different. Freivogel, on the other hand, suggested in your "Fake Physics VII" article that different constants of Nature should exist in different bubble universes. In general that is neither implied nor required — different physics is only "required" by string theorists in the sense that eternal inflation can't do the job of populating the huge set of string theory vacua unless each bubble nucleation can (randomly) realize any one string vacuum. Coincidentally, Freivogel at that time was also actively involved in looking at whether CMB signatures of bubble collisions might plausibly be observed.

Alas, our conclusion was that CMB signatures of bubble collisions were likely to be unobservably small because in general they would occupy a tiny angle in the sky. Observable signatures weren't ruled out; they were just very unlikely. At that point I lost interest in researching eternal inflation — without a plausible way to observationally test it, "multiverse physics" seems like an empty dead end to me. Nonetheless, I think those who continue to look for observational evidence of past bubble collisions are doing real science even if I personally don't see it being ultimately successful.

Given these qualifications and subtleties, I too have a problem with what Peter calls "multiverse mania." I frankly don't understand why there is enduring interest in the idea among cosmologists, given the lack of a plausible way to confront it with observations, even though almost all models of inflation imply eternal inflation, and eternal inflation in turn implies a multiverse (but not necessarily one with different physics/constants/geometry in different bubble universes). True, we can say that if inflation is a correct idea then there is a multiverse, but so what? We learn nothing new about the Universe we know and love — all we get from the deal is a "just so" story that lets us make believe we understand something we don't. And for those who find it appealing, we also get an excuse for avoiding hard physics questions that have eluded quantitative answers for many decades, but I wonder how satisfying such thin gruel is to someone who entered "fundamental" theoretical physics with dreams of deeply understanding why Nature is as it is.

9. **Another Anon**
January 21, 2017

You'll love the latest issue of New Scientist:

https://www.newscientist.com/issue/3109/

10. **Another Anon**
January 21, 2017

Oops, I see you already noted that one.

11. **Petite Kabylie**
January 21, 2017

Peter,
All the "Fake Physics" links you put out belong to the multiverse mania category. It would have been nice and more complete for the reader, though, if you put out also some links (if there are any this year), of "Fake Physics" that belong to the realm of particle physics or the "universe as a simulation" mania category.
Thanks for those anyway.

12. **Peter Woit**
January 21, 2017

Richard J. Gaylord,

I don't think the "Fake Physics" problem is due to anyone in particular, and if I had to choose a short list of those who have had the most damaging effect here, it wouldn't include Tegmark. It's a systemic problem with how certain sorts of theoretical physics research are evaluated and communicated.

I'd also like to make clear that on any scale, "Fake Physics" and the unhealthy things it has led to in the physics community are negligibly small potatoes compared to what "Fake News" has brought us and the future it promises human society.

13. **Peter Woit**
January 21, 2017

Marty,
I pretty much agree, and have always tried to make clear that the argument about whether the Multiverse is science is a non-trivial one that requires actually looking at and evaluating the details of attempts to make science out of it.

Sure, in principle you could have a theory that implies a multiverse and makes non-trivial testable predictions by statistical argument. But you don't, as looking at any of the attempts to do this (and Freivogel's piece) makes clear. In principle you could observe effects of another universe colliding with ours and sure, people should look for this. But there's no evidence of this so far, and no theory that predicts an effect invisible until now, but visible with a plausible further effort. At some point efforts to see evidence of another universe by doing analysis X of the data become just wishful thinking, not science. People can do this and hope for the best if they want, but they shouldn't be making misleading claims about what they are doing to the public.

14. **Peter Woit**
January 21, 2017

Petite Kabylie,
Just as it's worth distinguishing which sorts of "Fake News" is really a problem, among the many kinds of "Fake Physics" at the moment it's the Multiverse Mania that's what's really a problem. Silly discussions of empty ideas like the "simulation argument" have always been with us and always will be, but I don't think they're a real danger to physics research. HEP "Fake Physics" doesn't seem to be as big a problem now as it has been: you pretty much never hear anymore about how the LHC is going to produce black holes and discover extra dimensions, and misguided claims about supersymmetry are finally running into trouble with the reality principle.

15. **Mike Klymkowsky**
January 21, 2017

There seems to be an accelerating erosion of accountability and candor when making claims (and staking positions), much like the current political discourse where the mottos: "Never apologize," he told Mr. Trump. "Facts are white noise" and "emotions rule" seems to prevail.

Perhaps multiversers should consider a communal apology, or at least an "oops". http://blogs.plos.org/scied/2016/11/30/the-pernicious-effects-of-disrespecting-the-constraints-of-science/

16. **JohnB**
January 21, 2017

I don't know a lot about the physics community (mainly what I read here), but maybe it'd be worth extending the attitude of skepticism towards the physics community, to one of cynicism – and to follow the money, and see in general what kind of incentives people may have to deliberately misdirect resources in the physics community.

If someone has a neverending research project, like the string-theory/multiverse stuff, that can't ever be proven/disproven, then that sounds like the ultimate gravy-train to me, if that part of the community can gain enough political power and money for marketing, to convince people that this is still 'science', so that they can keep the funding rolling in.

As the Upton Sinclair quote goes "It is difficult to get a man to understand something, when his salary depends upon his not understanding it" – the reasons for that can include self-interested malice, rather than just ignorance.

Maybe something that some muckraking investigative journalists, with lots of harsh/bad publicity, would do a better job of researching/fixing this – rather than waiting for the physics community to sort itself out (the latter being the "science advances one funeral at a time" approach).

17. **Peter Woit**
January 21, 2017

All,
Please stick to "Fake Physics". Unfortunately I don't see anything useful to come out of me trying to host and moderate a discussion of the much more serious problem of "Fake News" and where it has led us. Besides, I have no time for that, I have a demonstration to get to…

18. **S. P. Nova**
January 21, 2017

Jim Baggott's books, like "Farewell To Reality", are sort of relevant to what JohnB suggests.

Unfortunately, the voice of reason is largely drowned out in the cacophony of today's information overload, laden as it is with misdirection and outright mendacity.

19. **lars**
January 21, 2017

"Virtual Reality"

If multiverse is real
Then universe is not
Cuz virtual appeal
In multiverse is hot

See Paul Davies' "fake universe" hypothesis

20. **Anonymous mathematician**
January 21, 2017

It seems to me that “fake physics” goes beyond multiverse mania and universe as simulation, to things like ER=EPR, which seems to contradict established physics in so many ways that it cannot possibly be correct.

Yet physicists are, if not jumping on the bandwagon, at least not debating it. Is silence consent?

21. **Marty Tysanner**
January 21, 2017

Peter,

Yes, you have distinguished in the past between efforts to detect bubble collisions, which is “real” science, and unscientific, unsupported claims and breathless hype about the multiverse on the the other hand. I wasn’t thinking of you when cautioning against painting “multiverse physics” with an overly broad brush.

JohnB,

I’m sure you can find various instances where researchers work to misdirect resources to enhance their own career prospects. After all, researchers are human too (most of them anyway!), and there seems to be no shortage of people who are more interested in enhancing their own position than working toward the greater good.

On the other hand, it is a long, arduous and often discouraging path to get through grad school, and then post docs after that, in order to secure a faculty position at a research university. Research physicists tend to be quite smart and most could easily command better pay (and probably have better career advancement prospects) in industry compared to being a faculty member or researcher in a national lab. On the other hand, working for a company means adopting that company’s R&D goals, which may be very different from pursuing one’s own passion — that’s the trade-off as I see it, and there is no shortage of people who are willing to give up better pay so they can work toward something they believe in.

What I’m getting at is that working in a university or national lab is not a good path to riches for the great majority of theoretical physicists, so the “follow the money” idea doesn’t apply to institutional research the way it does to industry and politics. I believe most theoretical physicists seek grant money and promote their research program because they honestly believe in it, and not because they cynically think that excessively hyping their program means more riches for them and a bigger empire of grad students and postdocs for them to manage. There simply are other, easier ways for them to gain riches and power than doing theoretical physics.

22. **Death By Hiccups**
January 21, 2017

Fake Physics: I think you’re right with some aspects of this picture despite generating large size influence and feedback entirely or mostly on their on own steam, are nevertheless of a secondary or non-fundamental order or else simplistic in and of, the being of that. The media and the majority of the interested public like the multiverse for essentially the same reason as the scientists: It’s the easy road. It doesn’t take much immersion (any, necessarily) before the far-reaching insights to burble in the region of the stomach, the excitement and euphoric emotion that in the end is about a revived hope-that-we-can-believe-in for transcendental survival of the tragedy of old age and death. Easy to make a documentary with all the same feelings and accolades of our colleagues. Easy to absorb a popular book and feel all the things that promise what we most want if we cannot be the best;our equivalence, as a personal potential, in a different life. There’s no such thing as a counter-factual in a multiverse.
The easy way is the rational way, that’s always been the truth. Why go the hard way if there is an easy way. It takes a lot of up front richness to see it different. Science was the jewel in the crown of doing things the hard way. It’s immensely inefficient at the level of a single life. But over the centuries it supplanted all else and created a new world. Those amazing insights that are so addictive particularly to the scientist who otherwise will be lucky to get half as much for a whole lifetimes work. The hard path has no answer for that.

23. **Peter Woit**
January 21, 2017

JohnB/Marty,

In general I agree with Marty: the motivation for physicists here is mostly not about money. There is one sense though in which people really should follow the money. If you trace back where a lot of these Fake Physics stories come from, you find that they’re funded by the Templeton Foundation. This foundation is the largest funder of Fake Physics, and a big part of the problem. They have an ideological commitment to bringing together religion and physics, and put a lot of money behind it, money going both to journalists and to physicists.

24. **srp**
January 21, 2017

Press release: Peter Woit says that in another universe you are dating Scarlett Johansen! Click here for the details of your relationship!

25. **Peter Woit**
January 21, 2017

Anonymous mathematician,
Personally I’ve never been able to figure out what ER=EPR is supposed to mean. Straightforwardly interpreted, it’s obvious nonsense, clearly it is supposed to be interpreted as some sort of speculative statement about some class of speculative theories of quantum gravity. Presumably experts on the subject can give more of a characterization of what this class of theories is and what kind of statement is being proposed, but personally every time I’ve tried to look into this I’ve ended up deciding that other projects looked like more promising ways to spend my time.

26. **dirk bruere**
January 22, 2017

I really don’t see why you are so against multiverse theories. As you say, little research is being done in that field, so they are hardly subtracting from the successes of String Theorists (ie none at all when it comes to proof). For the general public, they are fun. As they are for philosophers. Anyway, good luck with the ongoing failure of fundamental physics – I’m sure a new generation of String workers sucking the life out of all other possible routes to the mythical TOE will bring rich rewards.

27. **Alan**
January 22, 2017

There was one piece from Nautil.us (not given above) titled The Not-So-Fine Tuning of the Universe by Prof. Fred Adams which doesn’t look like Fake Physics. So he says that the universe isn’t so fine-tuned. Just wondering if first, how true is this and second, if anyone can enlighten me whether this goes against the multiverse concept. So, more wriggle room for the fundamental constants.

28. **Konstantinos**
January 22, 2017

I see how some of the links mentioned are just garbage (especially the *cough* Templeton's *cough* purely philosophical ones), but the article written on the site of the Astronomy magazine shouldn't be among them. Actually it goes against the popsci wave and notes that the most important part of a physics (or in general, scientific) hypothesis is to be falsifiable. Something that most (or every?) multiverse (as well as other "revolutionary") hypotheses lack.

29. **Shantanu**
January 22, 2017

Peter, something OT. David Gross giving a public talk at Spenta Wadia fest
https://www.youtube.com/watch?v=mLvuC14keQg

30. **Doug McDonald**
January 22, 2017

Its not just fake physics. Its fakechemistry, biology, indeed any hard science.

The correct term, which Peter used, is "Clickbait Physics". My chemistry colleagues … even the best … absolutely insist that it is necessary. The University management just loves it. When I confront them all but one .. the best one … insist it isn't "wrong" , "its just hype, and its necessary as clickbait". I specifically offer any piece hyping the great future we face (Yes! the future will be better than the past — we'll all live longer and better lives") due to research in any field beginning with "nano". One was so farfetched I literally ROTFL in my secretaries' office when I looked at the poster.

When I was a grad student we would have said "this will make missiles obsolete … photon torpedoes based on it will travel at the speed of light, and even push through fog". (That one, of course, except for the "obsolete" part, is now reality.) My career suffered because I am simply unable to conjure up hype that excites folks used to "journalisticly correct hype."

31. **Peter Woit**
January 22, 2017

Alan,
Yes, that's one problem the "fine-tuning" argument, and that piece does a good job of explaining it.

In general I've found that the Nautilus people generally put out high-quality articles. The large number of dubious multiverse ones, including a lot of theology, which is not normally their thing, I found surprising.

Konstantinos,

Just as few Fake News pieces acknowledge not caring about whether they are true or not, most Fake Physics doesn't acknowledge that it is giving up on science, instead claims that what they are talking about is testable. As with Fake News, you have to look into it to see if it really is what it is claimed to be. I think if you look into the claims about evidence of other universes described in the Astronomy article, you'll find they don't stand up to serious examination (At the time they appeared in 2015, Jennifer Ouellette wrote a piece debunking them).

Doug McDonald,
The hype problem is much more widespread than the Fake Physics problem, of which hype is only a part of the story.

32. **Anonymous**
January 22, 2017

(disclaimer: I am not a physicist, nor do I play one on TV)

Science reporting has been in shambles for a very long time, and with the rise of facebook and the ad economy, I have seen headlines like "You too could live forever like this one strange organism!" that, when you trace back the interpretation of an article that summarizes a summary of an abstract, the actual paper being reported on is about anhydrobiosis not previously seen in a particular variety of nematode.

This sort of reporting is written by those with neither a desire to spread knowledge nor an understanding of the subject matter. It is there to make money, and the patronizing "wow, your mind is blown!" language of the writing drips with this cynicism.

Joining them are those that wish to build a religion on paradigm-shaking beliefs. Again, a cynical cash grab based not on the potential merits of a hypothesis, but based on mysticizing the observable and testable universe.

33. **Peter Woit**
January 22, 2017

Anonymous,

On the whole, I don't think the Fake Physics problem is coming from journalists, but from physicists. There's some bad science journalism out there, but if you avoid the obviously low quality journalism sources, what you find are journalists on the whole doing a pretty good job of explaining claims being made by serious physicists. Yes, many journalists can't resist covering Multiverse Fake Physics, but they're doing it because well-known physicists are pushing it.

34. **serious question here**
January 22, 2017

is it fake physics news if the news media report claims from respected physicists in papers, one example of which is Verlinde's recent paper on gravity which tries to explain away dark matter?

Verlinde published a paper in entropic gravity in 2016 that does away with the need for dark matter, and the news media called him the next Einstein.

fake physics news or genuine physics news?

if genuine physics news, since it is based on an actual physics research paper by an actual physicist, Erik Verlinde, there are of course research papers written by string theorists on the multiverse.

35. **Peter Woit**
January 22, 2017

serious question here,

I'm not talking about fake physics news, but about Fake Physics, a concept that I don't think really applies to preliminary speculative work like Verlinde's. He has an idea the validity or usefulness of which is unclear, he's discussing it in a paper, and that's getting reported in the news. This is perfectly normal scientific activity and its normal press coverage.

If after a lot of investigation, it turns out that "entropic gravity" is a useless untestable idea that tells us nothing, but Verlinde keeps working on it and promoting it, with serious financial backing, lots of press articles appearing, and his colleagues unwilling to stand up and tell people it's an idea that doesn't work, then we'll be in the realm of Fake Physics.

36. **Maximillian G. Tresmond, Esq.**
January 22, 2017

Why is the Templeton Foundation pushing the multiverse idea?

37. **Peter Woit**
January 23, 2017

Maximillian,

The Templeton Foundation's mission is to bring science and religion together, it was set up for this purpose by the late Sir John Templeton. They have been funding and pushing Multiverse research for a long time, since it fits very well with their mission, blurring the boundary of science and religion. The recent Nautilus multiverse articles fit well with their point of view, getting both theologians and scientists to discuss the topic.

38. **Balazs**
January 23, 2017

I'm an engineer, not a physicist, so I'm not familiar with the mathematical fundamentals of the inflation filed, but it seems to me that the concept of multiverse is based on the assumption that the inflation field (if it exists) is going through random fluctuations, so at any point in space it has equal chance of producing a new bubble universe with random properties. So my question is, why do physicist assume (without having any observational evidence) that the inflation field is random? It's basically a scalar function that (if exists) we don't know anything about, so why can't it have any particular shape. Why can't it have a characteristic feature localized somewhere that will produce just one universe and that's it… Of course in that case we would need to explain why it looks like that, but complete randomness also needs explanation.
I'm looking for an answer that is not than it's because of the Heisenberg uncertainty principle, because we don't know if that applies to a hypothetical inflation field.

39. **Peter Woit**
January 23, 2017

Balazs,

The conjectured inflaton (not "inflation") field is supposed to be a quantum field, not a classical field. It has energy and couples to gravity, you certainly can make models like this, with probabilistic behavior implied by quantum mechanics. These come with a huge freedom even for a single inflaton field since you can choose the self-interaction potential.

One problem for even a single inflaton field is getting any signficant testable predictions out of this. Much more seriously, the actual theories being promoted (string landscape) are vastly more complicated, involving hundreds of inflaton-like fields and all sorts of other structures, with no well-defined theory that governs them. So far, this is a completely empty idea since it predicts nothing (or everything, which is the same thing…)

40. **George S Williams**
January 23, 2017

Actually. it's no longer Fake Physics, it's now Alternative Physics.

41. **Darrell Burgan**
January 23, 2017

I'm not disagreeing with the POV expressed here, just genuinely curious: what qualifies an idea as "real" physics? Is it merely falsifiability or is there more to it than that?

As a non-physicist outsider, maybe I'm oversimplifying, but it seems to me the real problem is that the line between science and fun speculation seems to be eroding, not that speculation is in and of itself wrong.

42. **Zach D.**
January 24, 2017

I'd like to see you back up your comment about fake news appearng in the NY Times with evidence. (Of course all news outlets make mistakes at times, and all will occasionally have an employee who doesn't play by the rules. But if you're going to point the finger at that news outlet, I assume you feel that fake news is not the exception for them.)

43. **NoGo**
January 24, 2017

Are there other scientists, blogs, or publications that systematically push back against fake physics like you do?
I know that Sabine Hossenfelder occasionally does, but that's the only other one I am aware of…

44. **John**
January 24, 2017

So what does fake physics and fakes news have in common? It's that fake stuff can be pushed by anyone with an agenda and you can no longer rely on credentials or the previous standards of the publishing site. It really comes down to "reader beware".

At the end of the day we can no longer accept as fact anything that is published. And that is a sad state of affairs.

45. **Zack Yezek**
January 24, 2017

This all seems to me to stem from a simple but unpleasant reality- most of fundamental physics has been stagnating due to a lack of new information from experiments.

Is it really so surprising that many theorists have been spinning their wheels or going off into la-la land for the last 40 years? The many null results and experimental falsifications of various GUT theories, flavors of SuperSymmetry, etc. are non-trivial and useful data in their own right, but only in a negative sense. My own guess is that real progress will originate in the neutrino sector, the first place the Standard Model finally made a flat-out wrong prediction and the one place we already have experiments probing new physics. Because unification has stalled out for the time being due to insufficient data, and won't really go anywhere until we get some more.

46. **Peter Woit**
January 24, 2017

John,
Unfortunately I think the problem is not just that of lower standards at publishing sites. On the physics side, physics journals have always published a certain amount of nonsense and popular science publications have covered this. The Multiverse fake physics campaign is something new I don't think we've ever seen: a concerted ideological campaign by a group of people with the best credentials at our best institutions, with multi-million dollar funding behind them, funding not subject to review by their peers. The problem isn't that popular science publications have lowered their standards to report this, it has always been their job to report claims made by scientists with these credentials.

I don't want to moderate a discussion of the disturbing story of US democracy and Fake News here, and will delete further comments about this, but since it's my blog, my thoughts about this are the following:

Besides the well-known, well-funded right wing Fake News machine that has come to dominate the political reporting scene in recent years, the rot goes deeper. Even the most reliable of our news sources (e.g. the New York Times) have been infected with this, and show no signs of having any idea how to counter Fake News. As far as I can tell, they seem to have often decided that vigorously fighting Fake News would make them appear to be partisans, so at time have joined in the Fake News business hoping it will make them look less partisan and thus more credible.

What I have in mind in particular (responding to Zach D.) is the behavior of the New York Times toward Hillary Clinton, their campaign of Fake News attacking her character. This has a long history, going back to the first Clinton administration (Travelgate, Whitewater), and continuing through the past year (email server, Clinton Foundation). Some of their writers (e.g. Maureen Dowd) have now written literally hundreds of pieces attacking Clinton's character. I didn't understand why they were doing that in the 1990s, even less so this past year. I suspect their editors thought that there was no way she could lose this election, so using her as a punching bag was harmless sport.

At least with their last disastrous promotion of Fake News (publishing Judith Miller on Iraq), they seem to have realized they made a mistake. They show no evidence now of understanding what significance of what they did during the Clinton-Trump campaign. Their Public Editor is an idiot. After defending the most egregiously awful things they have done, this weekend she announced that she has found something they did wrong: not reporting absurd accusations about some computer server belonging to an obscure part of the Trump organization, see
https://www.nytimes.com/2017/01/20/public-editor/trump-russia-fbi-liz-spayd-public-editor.html
This level of stupidity at an organization of this importance is scary.

If I had any idea what to do about Fake News I'd open comments here and try to do something. I do see what needs to be done though about Fake Physics: people in the physics community need to step up and take some responsibility. The problem is not going away.

NoGo,
Besides Sabine I'm having trouble of thinking of anyone. Lubos on some days will rant about the Multiverse, but having him on your side tends not to be helpful, as string theorists have discovered.

47. **Michael**
January 24, 2017

I think you should reinvestigate "Fake Physics VI" (the one from astronomy.com) as it doesn't exactly fit your bill of fake physics. While the title is certainly clickbait-y, the article takes the point of view that physical theories need be testable and discusses a cosmological multiverse where interactions at the boundaries of the universes could be relevant in interpreting anomalies in the CMB. They are quick to acknowledge that this is just a suggested theory, and there are other possible explanations for their data which require further investigation.

48. **Peter Woit**
January 24, 2017

Michael,
Sorry, but I disagree. One feature of Fake News is that the writer often goes on about what a problem Fake News is, that luckily for the reader, what they are reading is from someone aware of the problem, and willing to help them get the real stuff.

Same with Fake Physics: bogus claims to have real testable science are often framed by a discussion of what a problem it is that others have been making untestable claims.

49. **Patrick Harris**
January 24, 2017

Hello Dr. Woit,

Did you want "Fake News IX" and "Fake News X" to read "Fake Physics" IX & X? Or are was the distinction deliberate? Just curious…

Thanks for the great discussion!

50. **Peter Woit**
January 24, 2017

Patrick Harris,

That was a mistake, fixed. Thanks for pointing it out!

51. **Another Anon**
January 25, 2017

In defence of Nautilus, this is a great article:

http://cosmos.nautil.us/short/90/we-have-pushed-physics-too-far

52. **Peter Woit**
January 25, 2017

Another Anon,
I'm afraid I disagree, wrote about why here
http://www.math.columbia.edu/~woit/wordpress/?p=9027

That piece is not fake physics, but it's a point of view (very crudely put, physicists should become humble and give up on hopes for certain kinds of progress in fundamental physics) that is a side-effect of the nihilistic Fake Physics campaign. It's also a point of view that fits very well with the Templeton Foundation one: mankind should be humble before the glory of God (again, this is a crude caricature).

This kind of thing is analogous to one tactical goal of the ideologues behind Fake News: use it to discourage your opposition, even when they realize it's not true.

53. **Another Anon**
January 25, 2017

Yeah, now I read what you said about it earlier, you might have a point.

54. **NoGo**
January 27, 2017

What I still don't quite understand is how Templeton's money can be at work with regard to Fake Physics: I presume (and you say, if I am not mistaken) that the physicists involved actually believe in their theories, and anyway it's hard to imagine prominent scientists taking money to misrepresent their work. It is easier to imagine such money influencing the media to "amplify" already existing "signal", but again it's hard to imagine reputable journalists and publications taking money directly, so it must be somewhat subtle… Can you elaborate on this a little bit?

55. **Thomas Lee Elifritz**
January 27, 2017

Science Friday this week has an article that appears to be the epitome of 'woo woo'.

http://www.sciencefriday.com/articles/why-the-multiverse-isnt-just-madness/

Science Friday is supported by listener contributions, member stations, and by: Anonymous; Alfred P. Sloan Foundation; Del Mar Global Trust; The Dirk and Charlene Kabcenell Foundation; Macmillan Education; Mellam Family Foundation; Michael J. Connell Foundation; Sedgwick Family Fund at the Cleveland Foundation; Swig Foundation; The Winston Foundation

56. **Peter Woit**
January 27, 2017

NoGo,

I don't think there's anything subtle about $10 million targeted at producing this particular journalism, see:

https://www.templeton.org/what-we-fund/grant-search/results/nautilus

$15 million in grant money to physicists isn't subtle either:

https://www.templeton.org/what-we-fund/grant-search/results/fqxi

It has a significant effect on what sort of research physicists decide to engage in, as well as how they choose to communicate it to the public.

Thomas Lee Elfritz,

The flood of these things is really depressing, I'll add that one as Fake News XI. Another depressing thing is that all of these stories quote the same list of proponents of the multiverse, portray them as fighting for a new idea against the conservative scientific establishment, but almost never manage to talk to anyone willing to make an argument against what is going on. The lone exception is Paul Steinhardt. Good for him, but I don't understand why the rest of the physics community is staying quiet.

57. **Louis Wilbur**
January 29, 2017

Thanks Peter for raising this issue; you are right to be concerned that very speculative ideas are being given a level of coverage and credence that they don't deserve. The question is how to address it and understand why the scientific mainstream has not pushed back against such speculative ideas. Perhaps it is because they view it sort of like philosophy; it is something they simply don't want to spend time responding to. But both the popular and scientific press could do some pushback by simply correcting the sensationalism that typically appears in such articles. For example, in her blog on 12/19/15 Sabine Hossenfelder wrote "But the longer the chain of inference, and the less trust you have in the theories used for inference, the less real objects become. In this layered reality the multiverse is currently at the outer fringes. It's as unreal as something can be without being plain fantasy". She also wrote in a different blog that the multiverse is something that researchers within the scientific community spend almost no time on.

This means that the multiverse has not "gone mainstream" and there has not been "a great change in attitude" as some of the stories in your fake news links claim. I think the basic idea that the very early cosmos was dominated by some type of vacuum energy is plausible but the physics of it is completely mysterious. It is grossly premature to say that the mechanisms postulated in traditional inflationary theory are even approximately correct explanations of that vacuum energy. Something like cosmic inflation may have been caused by some completely unknown process, perhaps some kind of quantum black hole foam or some type of QCD-like fluid. For these reasons and others there is no basis at all to claim that such "inflation" is eternal. Another sensational claim is that non-eternal types of inflation are contrived and unrealistic. Actually they are not. For example, Wikipedia writes "In hybrid inflation, one scalar field is responsible for most of the energy density (thus determining the rate of expansion), while another is responsible for the slow roll (thus determining the period of inflation and its termination). Thus fluctuations in the former inflaton would not affect inflation termination, while fluctuations in the latter would not affect the rate of expansion. Therefore, hybrid inflation is not eternal. When the second (slow-rolling) inflaton reaches the bottom of its potential, it changes the location of the minimum of the first inflaton's potential, which leads to a fast roll of the inflaton down its potential, leading to termination of inflation". The February 2017 Scientific American article "Cosmic Inflation Theory Faces Challenges" By Anna Ijjas, Paul J. Steinhardt and Abraham Loeb explains some of the problems with traditional inflationary mechanisms.

So cosmologist who study inflationary theory are guilty of group think and this is the reason why other types of mechanisms underlying an "inflationary" expansion are not being pursued. It also explains why bounce scenarios are not given the greater attention and study that they should be. Such alternative ideas should be pursued since semi-classical gravity and effective field theory are probably inapplicable in the very early universe and a full inventory of nature's quantum fields and how they are coupled together is lacking. Also lacking is the correct quantized version of General Relativity, how it is united to nature's quantum fields, and the implications of all of this for the very early universe. There is a lot to vacuum energy that is not understood: the various hierarchy problems, dark energy, and the cosmological constant for example. There should be pushback to the traditional ways of thinking about inflationary mechanisms and pushback to the whole idea of multiverses. Everett's many worlds multiverse is nonsense and I am baffled as to how and why it manages to get the coverage that it does. For instance, no explanation is given as to where the energy comes from to make all of the split-off universes and no explanation is given as to how a split occurs outside the light cone of a state reduced event. Thank you Peter for the post.

58. **Samuel Keays**
January 30, 2017

https://www.youtube.com/watch?v=Q8ccXzM3x8A

This was posted today. Kind of interested on what you think about the idea that it is primarily a controversy only in the eyes of the general public, not among the scientific consensus.

59. **Peter Woit**
January 30, 2017

Samuel Keays,

That's off-topic, little to do with the multiverse. From a quick skim, it's garden variety ancient string theory propaganda, pretty much exactly what I wrote my book 15 years ago to counter. The one thing that has changed since then is that string theory is significantly less popular in physics departments (as well as to the general public).

Fascism and the Current National Emergency

January 28, 2017
Categories: Uncategorized

After the election it seemed to me that it would be a good idea to ignore what Trump tweeted or said, and wait to see what he and the people he surrounded himself with would actually do. We've been finding this out over the past few days, and today the nature of the problem we face is now clear. The actions ordered today that are now being carried out by US officials around the world are the product of a deranged and dangerous personality who has surrounded himself with similar others. This is a national emergency with no parallel in our history.

While the US has never seen the likes of this situation, Europe has, with Trump following a playbook familiar from the history of the 1930s. At this point the US may be one terrorist attack away from full-blown Fascism, this time with nuclear weapons. This needs to be stopped, now.

The Constitution does provide two ways to deal with something like this: either the impeachment process or removal under the Twenty-Fifth Amendment as "unable to discharge the powers and duties of his office." Many of Trump's recent statements are clearly the product of delusional mind that is incapable of dealing with reality, and these delusions are now reflected in his actions.

Removing Trump and those he has surrounded himself with will require the cooperation of a significant number of Republican legislators. Anyone who cares about US democracy should be trying to figure out how to get this to happen. Those of us in the US desperately need some good ideas about how to do this. Those in other countries should be pressing their governments and institutions to fight back against the US, as well as doing what they can to keep their own societies from following the US down this path.

I'm moderating comments here and will only post one kind of comment: positive ideas about what to do about this emergency situation. At this point I think what's needed are ideas way beyond suggestions of a "scientist's march" to promote rationality. We need to figure out how to fight a new form of Fascism that has just come to power and is starting to rule by decree.

Update: With the Republican Congress so far deciding to sit back and let Donald Trump, Steve Bannon and Stephen Miller rule by decree, all hopes for now are with the Judiciary. Last night a judge issued an emergency stay on parts of the executive order, this was followed by a statement from Miller on behalf of the White House that the order "remains in full, complete and total effect." The suggestion to donate to the ACLU is a good one, they are on the front lines here.

The President of my institution, Columbia University, at 1 am sent an email to the University community denouncing this executive order and involving the University in this in an unprecedented way:

> As I have said on many occasions, it is critically important that the University, as such, not take stands on ideological or political issues. Yet it is also true that the University, as an institution in the society, must step forward to object when policies and state action conflict with its fundamental values, and especially when they bespeak purposes and a mentality that are at odds with our basic mission. This is such a case.

There is a petition being signed by academics here, which likely will have no effect, but I signed it anyway, and you may want to too.

Update: A small glimmer of hope: a joint statement criticizing the executive order by Republican senators McCain and Graham, and a Twitter response from Trump identifying any opposition from them as "looking to start World War III". World War III between Trump and Republican senators is what we all need to root for.

Worth watching: news reports that the Trump administration is defying court orders requiring access to lawyers by detainees at Dulles airport. A decision by Trump and his people to defy court orders would provide grounds for impeachment.

Update: Just wrote the following to a correspondent, thought I might as well also post it here.

My advice would be to consider focusing on the following, and not getting distracted by the blizzard of appalling things one might reasonably find concerning about the current situation:

- It was unclear who would actually be running things in a Trump administration (since Trump himself clearly neither knows nor cares about anything other than getting attention) until the past couple days. The answer now seems to be that it's Steve Bannon of Breitbart. Bannon is a self-described "Leninist", see for instance.

  His self-described goal is to tear the country apart: "to destroy the state… destroy all of today's establishment." This means he's not just our enemy, but is also the enemy of much of the Republican Party, including for example John McCain and Lindsey Graham. He's also not about to let himself be thwarted by the courts. I think we're already seeing defiance of court orders, with a lot more of that to come.

- There's always the possibility of something like a military coup, but the only constitutional way out of this is impeachment or the 25th amendment route. This requires convincing a sizable number of Republican legislators that they have to abandon Trump and support his removal. To me, the big question here is what can be done to make that happen. How does one get the Republican establishment (legislators and/or Fox News, Wall Street Journal, etc.) to turn on Trump? My guess is that where they are now is that they know they have a problem on their hands, but are deathly afraid of Trump's supporters, of ending up with their heads on a pike.
- There's not much time here. Looking at history, what happens next in this kind of situation is some episode of violence gets used to rally the country to the leader and justify his assumption of emergency power to rule by decree. We're one episode of some enraged person shooting a lot of people away from that happening, in a country full of heavily armed angry people.
- The best, most successful thing to hope for here is something I and maybe many of you find a depressing prospect: President Pence. But, there we are.

Hoping I'm wrong about all of this…

Update: Thanks to commenter Fred P. for the link to this. For something sensible from a conservative, see this by Eliot A. Cohen, which includes:

> For the community of conservative thinkers and experts, and more importantly, conservative politicians, this is a testing time. Either you stand up for your principles and for what you know is decent behavior, or you go down, if not now, then years from now, as a coward or opportunist. Your reputation will never recover, nor should it.

Update: There was an incident last night (in Quebec City) of an enraged person shooting a lot of innocent people, killing 6 and wounding 8 others. Since the shooter was an Islamophobe and the victims were Muslims praying at a mosque, so this was of no use to Trump/Bannon, this has gotten just about zero attention.

Update: The Quebec City shooter was a Trump fan radicalized by Marine Le Pen.

Update: Terry Tao has a blog entry about this, emphasizing the damage to the math community.

Update: Leonard Susskind has also decided to issue a statement warning about Fascism and the Trump administration, using his YouTube channel.

Update: For commentary from historians of the rise of fascism in Germany on analogies with the current situation in the US, see Ron Rosenbaum and Isabel Virgina Hull.

Comments

1. **Robert Gauthier**
   January 28, 2017

   Anyone that thinks this is going to end peacefully is naïve. Don't forget there was a constituency that voted him into power that will see his removal as an attack on them as a class. They are armed, and think they have nothing to lose. This is a recipe for civil war.

2. **Vadim Zeitlin**
   January 28, 2017

   Civic disobedience is the only non-catastrophic way out of this situation that I see. Right now, there must be at least some immigration officials able to put their moral principles above the principle of subordination and they should be encouraged by everyone who values American democracy to do just this. And this, unfortunately, probably won't stay limited to just the immigration officials, so even if you don't know of those, it's still worth trying to disseminate the idea that illegal and immoral orders must not be followed as widely as possible.

3. **Heinz Lackner**
   January 28, 2017

   Flipping 4 Senate and 24 House seats would be the cleanest road to impeachment. That is unfortunately 21 month away. In the meantime, we have to speak up, unite and remind a handful of Republican Senators that they can stop this nonsense by showing that Country comes before Party and their own reelection.

4. **Kevin S. Van Horn**
   January 28, 2017

   Trump has enormous power because his predecessors — especially Bush and Obama — usurped powers for the presidency that were never authorized by the Constitution. These include the power to take the country to war, the power to rewrite bills passed by Congress, the power to unilaterally create new laws via executive orders, the power to summarily execute (via drone) whomever the president decides is dangerous, etc. It's time to roll back the imperial presidency. This is a project in which Constitutionalist conservatives would gladly cooperate with Democrats.

   Here's a good start:
   http://foreignpolicy.com/2017/01/24/senator-and-congressman-introduce-restricting-first-use-of-nuclear-weapons-act-trump/

5. **RBU**
   January 28, 2017

   Here's something positive:

   http://www.motherjones.com/politics/2017/01/muslim-ban-federal-court

6. **Robert Gauthier**
   January 28, 2017

   As far as impeachment goes, does anyone think Pence will be much of an improvement? He may well be worse given he knows how the system works and just how far it can be pushed before it breaks.

7. **neil**
   January 29, 2017

   Write or phone your congressional representative right now and express your outrage and disgust with this order. Encourage everyone you know to do the same. PLEASE.

8. **Bee**
   January 29, 2017

   It'll not help in the present situation, but in the long run I think the US voting system should be overhauled by the constitutional court. In Germany the voting system is basically constantly ruled unconstitutional and being fixed in one way or the other because it doesn't appropriately guarantee a good representation of the people's will. It's beyond me why the same isn't happening in the US.

   Something else that comes to mind. In Germany, the president (not the chancellor) can call for new elections. Is there a similar arrangement in the US? Circumstances that can require a new election?

   I find it not without irony that we owe much of our democratic system to Americans, but they failed to update their own system.

9. **Chris**
   January 29, 2017

   You should include the link to the petition:

   https://notoimmigrationban.com/

10. **AS**
    January 29, 2017

    Bien étonné de se trouver ensemble… While (unfortunately) I cannot give any suggestions on how to act, indeed one of the most serious threats to democracy is that it is very fragile by its very nature and that it can easily be destroyed from within, I can suggest a book with scary parallels and which convincingly shows how fast things could deteriorate. Perhaps better than "1984", one should (re)read Philip Roth's "Plot Against America".

11. **S**

January 29, 2017

Pence is very right-wing; he is not mentally unstable. Failure to distinguish those things at this time, while perhaps painful for the left, will be very dangerous to the country.

I don't think Trump can be impeached yet. Not only is it not that clear that his actions so far constitute "high crimes and misdemeanors," but even if it were (and arguably, of course, it is), I do not think it is possible yet that sufficient political will exists at this time (see Robert Gauthier's comment above). What I do think is worth pursuing is working to get conservatives in important positions to *privately* swing against Trump. Then, given the excuse, they can impeach him and hopefully will.

To this end, I think two things are important. First, the left, which is the primary (but not only) source of dissent right now, must *religiously* distinguish between "policies we really hate that are nevertheless mainstream conservative goals for decades," and "unhinged, fascist policies." This is difficult given the high-rhetoric times in which we live, but it is key. Few conservatives are going to be genuinely upset about reduced abortion funding, for example. Plenty can be talked into being upset about building a wall, instituting tariffs by executive action, closing down immigration or keeping out permanent residents. Rhetoric such as the above — "is Pence really better than Trump?" — is exactly the opposite of what I am proposing. Pence is very conservative, but if you genuinely can't distinguish him from a maybe-Hitler, then any attempt to convince Republicans to work with you on this is going to be futile.

And next, having chosen to focus on those overreaches that, genuinely, any sane American of goodwill, of any political persuasion, must find abominable, the left should reach out to actively join with the few conservative voices that have been saying the same thing. This will involve, once again, pain: allying with a George Will, a David French, even a Matt Walsh will be horribly distasteful to those who believe those men's opinions represent bigotry. But that, I think, is the hard step that leading voices on the left and in academia must take if they want to have any hope of getting the attention, and changing the behavior, of Republican Congressmen to the extent that they impeach their own President.

12. **Shecky R**
January 29, 2017

Given the intransigence and simplemindedness of Trump supporters I fear there are only bad solutions to get through this crisis. I'll stand in jaw-dropping awe of our Founding Fathers if we do somehow emerge peacefully from this grievous situation.

13. **Low Math, Meekly Interacting**
January 29, 2017

The Republican Congress has more or less fallen in line, unwilling to even acknowledge earlier congressional statutes (e.g. the Immigration and Nationality Act). The Democrats' political incompetence has left them neutered. I see little hope outside of Federal courts, assuming legal challenges to Trump's orders don't make appeals all the way to the Supreme Court, which soon will be the the last branch of the Federal Govt. to fall. Donate to the ACLU, I guess. They're going to be very, very busy these next four years.

14. **sdf**
January 29, 2017

It was pointed out by lawyers associated with the former Bush administration that Trump was open to impeachment as soon as he is inaugurated because he had stated that he would not (and has not) placed executive control of his various business interests in an independent trust (actually there seems to some who interpret the relevant laws as meaning that he would have to sell his business interests). It was reported that such legal proceeding are already under way. Just yesterday Democracy Now! had an interview with Prof. Richard Painter, who is professor of corporate law at U of Minnesota and was in Bush administration regarding same (also he is one of the complainants in lawsuit), the transcript of that interview is here

https://www.democracynow.org/2016/12/28/george_w_bushs_ethics_lawyer_trump

15. **AR**
January 29, 2017

We are not that far from full-blown fascism here in Europe, either. Just a few (hackable ?) elections and (false-flag ?) terrorists attacks away. In my opinion, we really are witnessing textbook fascism, advancing all over the world. My advice, and what I am going to do: Try your utmost to preserve your integrity and credibility, at any price (it won't be easy). Try your utmost to be compassionate, to build bridges between people. Try your utmost to understand why many chose to vote for Trump, why many are going to vote for Le Pen, why many for two decades have voted for Berlusconi. Try not to despise them unnecessarily, but try to understand them, their daily struggles, the extreme precariousness of their lives, their despised and dying communities (all of which does not justify their choices, but try to understand those choices), because otherwise it is may be going to degenerate into a civil war and it will play into these fascists' hands. Remember Orwell's 1984: War is Peace.
Again: Try your utmost to preserve your integrity and credibility, at any price (it won't be easy). I am a primary-school teacher, and that is going to be my ground (my task). I'll give it my best.

16. **Nate**
January 29, 2017

Heinz Lapman: It requires a 2/3 vote in the Senate to remove a President, so flipping 4 Senators is not enough.

Bee: no court in the US has the authority to change or eliminate the Electoral College. That could only be done by ammending the Constitution, which would require 2/3 majority in Congress and ratification by 3/4 of the States. (Necessarily including many small states that have disproportionate power now. In fact an ammendment to reduce one state's representation in the Senate must be approved specifically by that State.) So changes seem very unlikely. And no, there is no mechanism for calling for a new electricion.

17. **Anonyrat**
January 29, 2017

Note that Congress is crucial to holding the UnPresident in check or even removing him.

1. Attend your representative's town hall meetings and ask questions.
2. Call your representative's office in Washington DC and talk to the staff who take the call.

These are the two most effective methods for individual voters with no significant money to donate to a political campaign, to influence their representatives.

To do better than this, you will need to work in concert with others, i.e., through organization.

18. **Felipe Pait**
January 29, 2017

At this point the only thing we can do – have to do! – is cede no ground. The main instrument of defense are the courts. We have to hope that with a tireless defense of our liberties and of good government, cracks will appear in the administration – they are altogether not competent at the business of running things and will make mistakes.

How we prevent them from continuing the unconstitutional power grab they are executing depends on the nature of the illicit activities that are uncovered. The situation is not ready for immediate impeachment, but we don't have the luxury of waiting 2 years until the midterm election. In the meantime, special attention must be paid to vote suppression – the repeated lies about nonexistent fraud have to be understood as a declaration of intent to commit electoral fraud, and in that they will have support from the Republican party.

19. **Low Math, Meekly Interacting**
    January 29, 2017

    Anyone who thinks Trump is going to be impeached over emoluments is, I'm terribly sorry, smoking crack. Impeachment is a vote. It requires those casting the votes to have a moral compass and a spine. There isn't enough of either to get to a supermajority in the legislative branch, my friends, so dream on. Trump could eat a baby on live TV and Paul Ryan and Mitch McConnell would choke down the leftovers if they thought that would play well to Trump's base. They've as much as demonstrated it. And the Democrats…well, most of the midterm flipping will be happening in Trump Country, so don't expect them to be casting themselves on any pyres.

    The very sad truth is we progressives have very few, if any, levers of Federal power to push on, and the quislings in charge on both sides of the aisle can be expected to try to ride the populist wave so as to keep their careers afloat. Kirsten Gillibrand (of all people) can't do it alone, folks. And aside from a few grey-hairs like Sanders, she may as well be.

    We're facing a very long game here, and the damage to be undone is so extensive it's hard to fight off a sense of total despair. The Democratic party, already gasping for life prior to 2016, has been so badly pummeled that it's risking total and permanent irrelevance beyond its safe urban coastal enclaves.Republicans top Democrats in state legislative seats by at least 1000 (i.e. Republicans are about 3/5 in control, with 17 states having veto-proof majorities). It's worse at the executive level, with 33 GOP governors to the Dem's 16. The opposition, such as it was, has been routed.

    We have no choice but to suck it up or get involved at the local level. That's going to be a very uncomfortable position for many people. My sense is collectively we're not the types who spoiling for a fight with our neighbors, and don't much relish the idea of bricks through our windows, or worse. But that's the reality that left-wing collusion and complacency has wrought. There's not going to be a quick fix, and some of whatever damage is coming may be irreparable. That's the reality. We either leave, take it, or fight at the grass-roots level by running for office, which I'm guessing the vast majority of us have little time or stomach for. But I see no other long-term alternative, and the short term is grim enough.

20. **Tyrkky**
    January 29, 2017

    Almost 50% did not bother to vote at all. I don't think many of them would had voted for Trump – more likely they were discouraged because Dems did not offer anything to them. You need to get them understand that sometimes one has to vote for the lesser evil. Keep making noise and maybe enough will wake up to make a difference next time.

21. **anon**
    January 29, 2017

    "And no, there is no mechanism for calling for a new electricion."

    … well, in that case I guess the last one out of the country had better switch off the lights.

22. **johndoe_fr**
    January 29, 2017

    Sympathies from this French reader to the all the US citizens harmed directly or symbolically by these executive orders. The only two pieces of advice I can think of are:
    (a) plan his next possible moves and take judiciary counter-measures early to make it that bit harder for him and his team (among his next moves is that trade war with China for instance) ;
    (b) since people voted for him mainly for jobs and anti-immigrant policies, the only way to turn his base away from him is to provide jobs to those people before he does (e.g. collectively fund small businesses in the Rust Belt, buy their stuff, and make sure the folks know their wages or income is from well-meaning democratic-leaning folks), as well as show that the anti-immigrant policies weaken the economy. Clearly a job of many years, the aim being that he's not re-elected.

23. **Abby yorker**
    January 29, 2017

    I am one of the few to vote twice against mr trump…in the general and also in the Texas primary, where I pretended to be a republican.

    But I agree with others that impeachment is not in the cards at this early stage and I would not personally support it given the current information. I look forward to the congressional elections…if there is not a reversal of the majority, then I think we have to live with trump for the whole 4 or 8 years.

    Yes, American democracy is a little risky. They had an interesting episode on that in that series, "west wing".

    My recommendation is along the lines of ACLU support. But pick your own recipient, the cause you feel is most pressing. I myself am contributing to Planned Parenthood, which appears to be losing federal funding. Perhaps billionaires could donate significant sums to completely replace federal funding in key areas.

24. **abby yorker**
    January 29, 2017

    Thinking a little more about my previous comment. I would like to see a web site that shows all federal funding recipients together with cuts imposed by the trump administration. There would be a running count of imbalances not covered by current contributions, assuming that the recipients would be willing provide this information. User would use the information to decide on the best use of their donations.

    I think that obvious issues with this scheme (e.g. accuracy of donations quoted by recipients) could be worked out.

25. **David Schaich**
    January 29, 2017

    This "How to effectively talk to your member of congress" advice from a former congressional staffer got some press after the election:
    https://storify.com/editoremilye/i-worked-for-congress-for-six-years

    In addition to emphasizing that phone calls are more effective than letters, she argues that it's more effective to contact the local (district) office rather than the DC office.

It’s also possible to arrange to visit the local office and speak with staff in person. I did that with an anti-war group in ’02 or ’03.

My favorite comment about online petitions comes from former Rep. Barney Frank (D-MA): “Signing an online petition is like scratching your nose. Nobody in Congress is impressed because you pushed a button on an online petition. What matters is individual communication.”

That said, I’m not optimistic about much of substance coming out of Congress for the foreseeable future. In the meantime I would like to see the “moral responsibility to disobey unjust laws” (and executive orders) gain more prominence in popular consciousness and discussion, since I anticipate many more unjust orders from this administration. I’m quoting the phrase from King’s Letter from a Birmingham Jail, and of course the idea has a long history. Here’s a short article about it (that I’m sharing in part because Larry Rockwood is an acquaintance of mine):
http://historynewsnetwork.org/article/5378

26. **Eddie Dealtry**
January 29, 2017

Speaking from outside USA, was hoping you guys would remove your ‘leader’ from the world stage by investigating his tax affairs as you eventually removed Al Copone from organised crime.

27. **Tom Robert**
January 29, 2017

I think many are missing the point. Trump is not alone. Hitler was not alone. So it is not a matter of getting rid of a crackpot. It is a matter of civil disobedience, patience and resolve…

28. **Code Ferret**
January 29, 2017

To be blunt, we are either confronted with 1) an incompetent administration fueled by a reality TV sensibility; or 2) we are experiencing the early stages of a coup. I assume 2) for a variety of reasons, including a) Bannon is ex-Navy and worked at a high-level in the Pentagon; b) Flynn is an experienced hand in the Intel community; c) Giuliani is a crank with actual experience in governance and law. Trump has an immense talent w.r.t. media manipulation along with obvious personality characteristics that would be disqualifying in most other pursuits than national politics and TV.

So what is the remedy? As others have pointed out civil disobedience is number one on the list of actions and needs to be taken early and relentlessly until this administration is ended. It’s not just the Immigration EO but also Standing Rock, the assault on EPA and so on.

Another action is to ensure that all government databases across the board are cloned outside the control of the U.S. government. For example, the ncbi.nlm.nih.gov is over 9 Pb and is in part replicated via bio-mirror.net around the planet but not in its entirety. Those with the resources to help preserve decades of quality U.S. Federal collected data in all areas of study should help to create resilient distributed collections that can be referred to in the event of suppression by the current anti-science, anti-elite administration.

29. **piscator**
January 29, 2017

(echoing previous comments)

Focus single-mindedly on the marginal Trump effect, i.e. policies or actions that are unique to Trump and are not standard Republican policies. To be more than simply party politics, a case has to be made that a Trump presidency is qualitatively different (and qualitatively less preferable) than e.g. a Mike Pence presidency 0r a Ted Cruz presidency.

30. **Peter Woit**
January 29, 2017

Something I ran across that seems worth thinking about, coming at the issue from the supposed opposite side of the political spectrum, an article in the Washington Post advising us what to do about Trump, based on the author’s experiences with Chavismo in Venezuela.

https://www.washingtonpost.com/posteverything/wp/2017/01/27/in-venezuela-we-couldnt-stop-chavez-dont-make-the-same-mistakes-we-did/

31. **KingJohn**
January 29, 2017

One positive way forward is suggested by “Justice Democrats”, supported by Cenk Uygur of TYT along with former Bernie staffers. TYT also supports the long-term solution of
overturning Citizens’ United through state-by-state referendum: WoflPac.com. Bernie’s movement Our Revolution is also a great way forward.

Mathematicians should keep in mind part of how we got in this mess: that a brilliant computer scientist Robert Mercer, CEO of Renaissance Technology (after Jim Simons stepped down) and his daughter Rebekka are responsible for bringing us Bannon, Kellyanne Conway and Trump. Unfortunately, Jim Simons’ many wonderful contributions to math, science and the environment will probably
be overshadowed 10,000 times by Mercer’s negative counterweight.

Koch, Adelson, Mercer and other extremist billionaires have been following a clever long-term game, and the same game is playing out in other countries- sometimes with the same monies- what is going on now in Brazil, England, France, and Germany is scarily similar.

So we may soon have no islands of hope or sanity left, which will make resistance even more difficult. The opposition is revitalized with all the looming disasters, but largely depends on the internet- email, social media, You Tube- for organizing and exchange of real information. So Bannon surely has a plan to attack not just the MSM (with threats as well as insults) but more importantly to kill the internet as we know it. Trump’s pick for FCC head wants to kill net neutrality, a huge step in that direction. Divide and conquer will always be the weapon of choice:
a lot of powerful people will stand to make a killing off of that.

Trump is not only repellent and odious in many of his policies but ignorant, incurious, unstable and incompetent, as we are now seeing on a daily basis. He has just doubled the yearly membership at Mar-A Lago to $200,000, so he is selling access to himself, a clear violation of the Constitution. Similarly he’s profiting from his Wash DC hotel and violating the lease. These are just a few of the rapidly accumulating clear grounds for impeachment, but the numbers and popular will is not there.

It is imperative in any case (whether or not impeachment can happen) to get the truth out to the broad base of citizens. Also, it is critical for the opposition to preserve/regain its integrity. When Clintonite Dems attack Trump on the basis of Russian hacking or worse, attacking him for stopping the TPP, they are guaranteeing continued suicide. When they roll over and compromise, ditto. That is why efforts like TYT and Bernie’s are so important. We need to have our own Tea Party, we need

local organizing and local candidates; we need to play the long game like the radical right has been doing for 40 years. We need to get personally involved. We need to meet our neighbors, and discuss politics through local issues that touch them on a daily basis.
TYT has done a great job of reporting on the Disappearing Middle Class- that's an example of the outreach that is necessary.

Also, nonviolence is absolutely fundamental. The indigenous people at NO DAPL have been giving us all profound lessons in the effectiveness and the necessity of these tactics and of the deep spiritual orientation behind it. The presence of 4000 veterans there was deeply moving and must be just the beginning.

The fascists will do their best to provoke violence, even terrorist attacks. That will give them the excuse for rounding people up with 35 year sentences, $850,000 fines like with Barrett Brown or Josh Fox's producer Deia Schlosberg. We must not play into their hands. This will take a lot of patience, commitment and wisdom.

These are dark times, but we have no choice.

Oh, and we will have to start encrypting our hard drives and emails…The Intercept
has some info on this.

32. **Mike Harney**
January 29, 2017

The only thing left that would give the executive branch more power is for good people to stand by and do nothing. I think civil protests are the best action in the short term, we still (at least for today) have freedom of speech and we need to use it. That's the only way I can see the Republicans in Congress reacting to turn the situation around.

33. **Frank Wilhoit**
January 29, 2017

The only positive idea is to leave an absolutely clear, explicit, forthright, comprehensive, and unsparing account for history, because history is the only audience. If that last clause violates the terms of the comment moderation, so be it; pretending is not one of ours, it is one of theirs.

34. **MW**
January 29, 2017

There are minor efforts underway protest the ban outside the US.

For example, in the UK a petition to prevent's Trump's trip to the UK being a State visit has over 700,000 signatures. That is way more than the 100,000 needed to get it discussed in Parliament. It's not much, but at least it is a mechanism to make our voices heard.

https://petition.parliament.uk/petitions/171928

35. **Jeff M**
January 29, 2017

FYI faculty at my school are pushing hard for administration response to this, and working on setting up safe places for students affected by the order. From what I can tell from the faculty email list, it's happening at many universities.

36. **Abby yorker**
January 29, 2017

Not to be too picky but the following sentence seems to imply that a military coup is constitutional. I hope that that is not the case.

'Other than something like a military coup, the only constitutional way out of this is impeachment or the 25th amendment route.'

37. **Frankel**
January 29, 2017

The solution is for this blog to finally give up its silly war against string theorists and multiverse advocates. With a full acceptance of the MWI we can all exist in a universe where Trump is impeached.

38. **Peter Woit**
January 29, 2017

Abby yorker,

Thanks, that was badly worded and now fixed.

Hard to imagine how a military coup would work here, but I have to say, seeing what has happened to the political system in this country recently, you start to understand why in many places a military coup may seem like one of the better options.

39. **Robert Gauthier**
January 29, 2017

I think we need to consider the possibility that the outrages perpetrated by the Trump administration in the last two days are an overt attempt to incite the sort of response that will allow them to declare martial law and dismantle what is left of legal challenges to their full takeover.

40. **Florin Moldoveanu**
January 29, 2017

I agree with Anonyrat

Right now:
Call your representative's office in Washington DC and talk to the staff who take the call.

Later:
Vote against the politicians who supported Trump.

41. **Blake Gentry**
January 30, 2017

Martial law is resisted by mass movements, not visits to congressional offices.

The emoting here appears to suggest several lines of analysis: One is a liberal line that seeks to contain an authoritarian presidency through constitutional legal channels on executive orders (vis a vis "donate more to ACLU") another seeks to impeach by his violation of the ban on accepting "foreign gifts" (can be neutralized if Supreme Court Majority is installed in his favor), others consider political pressure against moderate members of his party (they failed to coalesce against him before the general election, and will not now either since there is too much money to be made by lobbyists), and others suggest general rejection and some types of street actions to oppose specific orders or acts.

The former Bernie Sanders staff and Bernie himself have not articulated movement through mass organizing, but through donations. Really? That's it?

What Europe and Latin America can offer is how to organize mass movements to stop military control, if it gets to that. Such movements cannot wait until the last moment to realize electoral politics is dead, they must create a movement on the ground to resist. Parallel politic movements can attempt to upend the corrupted political system politically engineered by Citizens United, but most of us need to have local mobilizations to show force in the streets. If you are really afraid of fascism, then own the streets before they own you. Ukraine is a case in point. Prague is as well. Santiago de Chile waited too late.

If Trump tries to impose sanctions on major cities who resist compliance with ICE against immigrants, then those cities can call for major progressive strikes. Start with the airlines, move to public transport, and interrupt businesses as usual, and yes that means your sacred universities as well. Block production of pipelines and well heads. Close down ports. The capital interests in this country will then move to remove him if that is the cost of stopping such "free speech".

If thousands of people had waited to call their representatives offices instead of arriving at airports last night, if the Women's March did not occur, if there was no march on Selma, or the Sit down strikes in Chicago and Flint, we'd be facing small militias with guns terrorizing the citizenry. The Vietnam war war was not ended with visits to congressional offices. It took a nation of people protesting and fighting back.

Stop thinking as if the middle class is going to arrest this situation. The middle class was sold down the river in the crash of 2008 and that is why millions voted for Trump. The Occupy Movement refused to target infrastructure, and insisted on consensus. Consensus in a mass movement is an oxymoron, those are tactics of the state.

That is also what democracy can look like. Many may cringe at these suggestions, but you will cringe more when the beat the shit out of people on the street and you realize then -too late – that your friends are next. Start your own Local Movement of Resistance, in this you are constitutionally bound to act as a citizen of the United States. Hope that's positive enough.

All my relations.

PS
It takes a child to raze a village, but only if the village watches itself burn to the ground.

42. **martibal**
January 30, 2017

Could be of interest: a detailed analysis on what is going on, and what may happen next
https://medium.com/@yonatanzunger/trial-balloon-for-a-coup-e024990891d5#.g8k084pfz
Interesting also the idea that the muslim ban could be a diversion for public not to pay attention to the raise of Bannon to the national security council.
One question from a non US citizen: to what extend do single state have possibility to resist (e.g. in the scenario of terrorist attack = emergency state = more power for Trump, is there some piece of the army that depends on states and not on the federal government ?)

43. **Maurice**
January 30, 2017

Positive ideas, drawing lessons from what happened in Germany 1933.
Firstly I agree with Peter that the parallels are close:
1. I compared Trump's with Hitler's "inauguration speech" (there was no such thing in the Weimar constitution, it was his first public speech as chancellor on February, 10 1933). They are strikingly similar, as if Bannon had used Hitler's manuscript to draft Trump's address: same subject, same slogans, same omissions.
2. The "violent event" that Peter rightly fears, was the fire in the German parliament, about one month after Hitler came to power. This event marked the beginning of pure Nazi terror.

Positive conclusions for American liberals from the major mistakes of Hitler's opponents:

1. unite against Trump even with Americans whose views you detest. I am convinced
that there are many ultra-conservative anti-liberal Americans who are still firmly against being governed by a fascist regime in Washington. All other political issues have to take a complete backseat in this time of imminent danger to democracy in the US.

In the Germany of 1933 social democrats and communists, extremely hostile against each other before Hitler, while both being firmly anti-Nazi never united their forces against Hitler and in support of the Weimar republic.

2. organize a general strike against Trump.

I know such an action is very unusual for the US. In Germany it never was, and that it was not attempted against Hitler is seen by many historians as a major factor for Hitler's success to become dictator.

While it is true that a strike of students and professors of a university does not really bring public life to a halt, it would still be a very strong sign of resistance.

44. **Anonyrat**
January 30, 2017

Blake Gentry:

The unorganized individual can only work through Constitutional means; anything more needs organization and/or numbers. The individual is too vulnerable in acting alone in using extra-Constitutional methods.

I don't know if our host wants us to discuss this here.

45. **bks**

January 30, 2017

Scientists' March On Washington:
https://www.marchforscience.com/
I recently re-watched *Fail-Safe* (1964). Not good for 3am ruminations!

46. **GlenO**
January 30, 2017

In my opinion the most likely trigger for a martial law situation is a premature or excessive action in opposition. We have survived bad presidents before, and I think going outside the constitutional remedy for bad presidents (impeachment) would be a cure much worse than the disease. The only course forward I can see is to try to channel Trump's egoism into more positive directions, such as his stated desire to create jobs, and save the demonstrations, work stoppages, etc for responding to negative actions on his part. Otherwise it looks like sore-loserism, and will only be counter-productive

47. **Low Math, Meekly Interacting**
January 30, 2017

To all those suggesting more radically subversive approaches to combating the problem of Trump: Let me concur with those proposing you will be giving the current administration EXACTLY what they want and need to justify imposition of more authoritarian measures to suppress even lawful opposition. Don't do it.

48. **Fred P**
January 30, 2017

"How does one get the Republican establishment (legislators and/or Fox News, Wall Street Journal, etc.) to turn on Trump?"
– you have to make it more palatable for them to help out the protesters than to stick with Trump. That said, while I've helped swing congresspeople on individual votes, I've never swung one against a President. Here's a guide that may help:

https://www.indivisibleguide.com/web

49. **Anonyrat**
January 30, 2017

Back in 2007, my then-representative, Rush Holt (NJ-D), who has since retired from Congress and is now CEO of the American Association for the Advancement of Science (AAAS), in a townhall that I attended, addressed the issue of impeachment of the then-incumbent George Bush.

Holt entered Congress in 1998, he believed at least in part because of reaction to the impeachment of Clinton which he condemned as a partisan affair.

Holt said there were two preconditions for impeachment:

1. That impeachment not be seen as a partisan issue.
2. The vast majority of Americans need to sign on to impeachment.

Holt thought that Bush had committed impeachable offenses, but that most Americans did not understand what was at stake. Also, no Republicans had signed on to impeachment, and Holt feared that that if a substantial minority – even 25% – begin to have the attitude that "we lost the last elections, but we have impeachment – the country is in trouble".

Since neither precondition was satisfied he was not signing on to the proposed impeachment bill, HR 333.

Holt thought that (paraphrasing) rather than having this neon sign "Impeachment" at which half the country would turn one way and half the other, it was important to firmly mark out that "Habeas Corpus cannot be suspended", "Wire-tapping without a court warrant will not be allowed", etc.; impeachment in the current political climate would only be a distraction.

IMO, Holt's objections still hold some force, and before calling for impeachment or a 25th Amendment removal of Trump, these objections should be considered. They also square with the advice of the Venezuelan who wrote about opposing Hugo Chavez:

Attempting to force Trump out, rather than digging in to fight his agenda, would just distract the public from whatever failed policies the administration is making. In Venezuela, the opposition focused on trying to reject the dictator by any means possible — when we should have just kept pointing out how badly Chávez's rule was hurting the very people he claimed to be serving.

50. **martibal**
January 30, 2017

About the attack in Quebec, this morning on the Quebec news this was called an "attentat terroriste" ("terrorist attack" is a fair traduction I think), not an incident. See e.g. http://ici.radio-canada.ca/nouvelle/1013929/attentat-terroriste-a-quebec-qui-est-le-suspect

51. **Peter Woit**
January 30, 2017

Anonyrat,
A couple points:

1. It never occurred to me that impeaching Bush was a good or viable idea.

2. The Republicans don't share Holt's view at all (see the Bill Clinton case). Do you really think that, if Hilary Clinton had managed to win the election with a minority of the popular vote, the Republican House would not already have drawn up articles of impeachment?. The Republicans have, since the Clinton era, adopted extreme tactics of personal attack to delegitimize their opposition and drive them from power. In recent years this has been extremely successful and it's the cause of why we are where we are. Should the Democrats continue to avoid using such tactics themselves, given where it has gotten them? Sure, impeaching Trump will require widespread public support. Let's figure out how to get that.

All: I'm shutting off comments on this. Having said my piece, I think it best for my sanity to spend time for now on other things than this.

Math and Physics Social Media

February 4, 2017
Categories: Uncategorized

In the current situation, getting back to finding interesting news about math and/or physics to think about seems like a good idea, but I've been having trouble coming up with such news. Besides blogs, many of them listed on the right-hand margin of this one, I also follow some people on Twitter and on Google+. There are quite a few well-known physicists on Twitter at this point. On any given day you can learn something interesting from, for instance, Frank Wilczek or John Preskill (or see an epic throwdown between them).

I'm sure there are many other mathematicians and physicists on social media that I'm not aware of, and open here to hearing suggestions. Part of the problem is that I'm now so old I figure I don't even know what social media sites are out there. I hear there's this thing called Facebook, but also that it's now over as far as the younger generation is concerned. So, if you have a suggestion about where to find high quality news about math or physics on social media, whether it's on Twitter, Google+, Facebook, Live Journal, Instagram, Pinterest, Snapchat, Yik Yak, Grindr or something else I've never heard of, please let us all know in the comments.

Comments

1. **Jon Awbrey**
   February 4, 2017

   I'm not sure social media is supposed to be about super high quality. You could experiment with creating a Facebook community or group page of your own and it would have whatever focus, quality, and scope you could maintain for it. But you'd probably find that FB is more about exploration, immediacy, and yeoperson efforts at public education, in your case about math and physics, if you have the fortitude for that. I always find that communication is far harder than either discovery or invention.

2. **Peter Woit**
   February 4, 2017

   Jon Awbrey,
   To be clear, I have no interest at all myself in participating more in different social media (the blog here is already too much of a time sink). I'm just trying to find out from others what sources of information are available that I might not know about.

3. **Shecky R**
   February 4, 2017

   Will mention 3 (out of a zillion) on Twitter:
   1) Grant Sanderson, for his math videos: @3Blue1Brown
   His YouTube channel here: http://tinyurl.com/grramxt
   2) Brian Hayes, just for interesting stuff (usually, but not always math/computer science): @bit_player
   3) Erica Klarreich (usually on Quanta): @EricaKlarreich

4. **Doug McDonald**
   February 4, 2017

   I look daily at some of the Physics ones you list at right.

   But many others there seem to be extinct. Perhaps a purge?

5. **Magnema**
   February 4, 2017

   Re: Facebook: In my experience, it's one of those things which many (even young) people have and use for "functional" communication (such as group project work with someone you don't know outside of a class), but not for "social" communication. It's like e-mail – it's not something that's cool, but nor is it quite "out" (vs., say, Myspace).

6. **Cesar**
   February 4, 2017

   I'm a big fan of mailing lists.

   I know and participate in lots of them, but none about mathematics or physics. I usually get news about that from blogs (like this one) and websites like Quanta, Nature, etc.

   Searching on Google gives some results. But it appears that most lists are university-specific.
   https://encrypted.google.com/search?q=mathematics+mailing+lists
   https://encrypted.google.com/search?q=physics+mailing+lists

   We could organize a new, general mailing list for enthusiasts.
   It could be one for both physics and mathematics or 2 mailing lists for each subject.
   I would subscribe for sure!

   Anyone in the known up for the task?
   I heard good things about ezmlm.

7. **Bee**
   February 5, 2017

   I think it doesn't matter all that much which social media you use as long as your circle is large enough because stuff gets passed around anyway. Eg, I use facebook, twitter, and (to a lesser extend) G+ (I am also signed up to like a dozen other sites which I don't use though) and I follow a bunch of blogs and magazines and so on. If anything is making rounds at all, it shows up in my feeds not once but dozens of times. (And that's leaving aside the people who, several weeks later, send me the same thing by email asking if I've seen it.)

I gravitate towards facebook because in my experience the discussions work best there. I don't understand how people can have discussions on twitter, the medium just isn't designed for it. Yes, I too have heard young folks don't do facebook. Fine by me.

Of course this doesn't really address the question of all question, where to get new news from, if that's what you're after. Following conference hashtags on twitter and journal feeds is a good way to keep an eye on what's going on. For example, CQG has a blog now, and they're also on twitter. It's too technical stuff to draw a large readership, but at least for my research it's very interesting, see

https://cqgplus.com/
https://twitter.com/CQGplus

There's also of course PRL

https://twitter.com/PhysRevLett

And Nature Physics

https://twitter.com/NaturePhysics

Which I find to be very informative. On facebook, Physics Today is very strong. Other feeds, pages to follow, depending on your interest, are experiments, labs, institutions, and so on.

Just generally, I have found that journals and membership journals and experimental collaborations have a minimum amount of bullshit in their feed. With institutional accounts, there's an unavoidable amount of self-promotion. Eg, the Max Planck Institutes in Germany publish a pretty good magazine (which you can also get in print for free!), which is very well done and so on, but of course it will always tell you what great stuff MPI is doing, so you need some tolerance for that.

8. **Shantanu**
   February 5, 2017

   Peter most of the blogs on the right side are no longer active , or updated regularly.
   on fb there is Astronomers group, which is worth subscribing to for interesting news and issues and also technical group related to astrostatistics and python users in astronomy.
   (I don't know if particle physicists use any similar forum to discuss science or technical issues)

9. **Deane**
   February 5, 2017

   There's a active group of mathematicians on Facebook who engage in conversations about all the usual topics but also often discuss math. In fact, it appears that some people find it to be a more friendly environment for asking math questions than MathOverflow. However, the problem with Facebook is that there is all the other distracting stuff you have to try your best to ignore.

10. **Frank Quednau**
    February 5, 2017

    For programming, stackoverflow.com is indispensable.
    There is also http://math.stackexchange.com and https://physics.stackexchange.com.
    It doesn't fit exactly what you're looking for, but are two good quality Q&A sites on the matters.

11. **Jim Akerlund**
    February 5, 2017

    What about https://www.reddit.com/r/science/. It is moderated.

12. **martibal**
    February 6, 2017

    This is not social media, but could be of interest for those who understand french: the last lectures of Connes at Collège de France,
    http://www.college-de-france.fr/site/alain-connes/course-2017-01-26-14h30.htm
    Till the 19th of january, he talks about "geometry and the quantum". Then it goes more
    towards arithmetic.

13. **Ronan**
    February 6, 2017

    This site http://truesciphi.org/phy.html contains a list of some of the physicists on twitter

14. **Anon**
    February 6, 2017

    What has happened to blogging? Has nothing taken its place? I'm genuinely curious.

15. **tommaso dorigo**
    February 6, 2017

    Hi Peter,

    well, for one thing there's the @cmsvoices twitter account, which I manage this month (it changes every month). I am usually @dorigo

    Cheers,
    T.

16. **anon**
    February 7, 2017

    Peter, go to the Google homepage and type into the search bar: "mathematics" OR "physics", then click news and voila: plenty of fresh maths/physics news to pick from.

17. **Greg Bernhardt**
February 9, 2017

Can’t forget the original https://www.physicsforums.com

Perfectoid Woodstock

February 7, 2017
Categories: Uncategorized

Every year in Tucson the Arizona Winter School takes place, with a five day program on some topic in arithmetic geometry aimed mainly at advanced graduate students, designed to get them involved in current research-level topics. This year's topic (Perfectoid Spaces) is drawing a huge number of people there next month, with about 450 participants expected (in the past numbers were more like 100). This should be a veritable Woodstock of arithmetic geometry, with no one I've talked to quite able to figure this out, thinking that there probably weren't 450 people worldwide interested at all in arithmetic geometry. It seems everyone in the field will be there and then some.

Peter Scholze is the opening and closing act. The other lecturers who will take the stage have started to put lecture notes for their lectures on the school website.

Some are dubious that there really are 400 or so students in the world with the background necessary to understand this material. See for example MathOverflow where nfdc23 isn't very encouraging to a student who doesn't know any rigid analytic geometry, but plans to attend the AWS. In any case, I hear Tucson is quite nice in March.

At some kind of other end of the spectrum of such things, a couple months later experts will gather in Germany to discuss this field (see here). Also for about five days, at the Schloss Elmau Luxury Spa and Cultural Hideaway, the sort of place heads of state go for G7 meetings. Rooms run $600 a night or so, but in this case the tab is being picked up by the Simons Foundation. Sorry, by invitation only.

Comments

1. **Jess Riedel**
   February 8, 2017

   The end result of this, of course, is that grad students pick research topics based almost completely on perceived status rather than technical merit. By the time they have the background, they've devoted 5 years of their life specializing in a subfield, and they are too wedded to the subfield to assess it objectively. This generational inertia lets fads persist longer than they would otherwise.

2. **John Fredsted**
   February 8, 2017

   The put-down comment by nfdc23 at MathOverflow could perhaps be contrasted with the call, by Francis Su, for quite the opposite behaviour in the math community, compare To Live Your Best Life, Do Mathematics.

3. **lf**
   February 8, 2017

   As we say in French "Le ridicule ne tue pas", fortunately because otherwise that would be a real hecatomb these days in the US.

4. **anon**
   February 8, 2017

   "Business casual clothing should be worn during the symposia."

   The first time I remember hearing about dress code for a scientific meeting.

5. **lf**
   February 8, 2017

   "Business casual clothing should be worn during the symposia."

   That's probably to prevent a real Woodstock to happen.

6. **Davide Castelvecchi**
   February 8, 2017

   From what I can tell from the titles, there will be no discussion of Mochizuki's inter-universal Teichmüller theory…

7. **Jeff M**
   February 8, 2017

   Business casual? Wow. That reminds me of the first real conference I went to, at UCLA, in '91, when I was working on my thesis. My wife insisted I go out and buy nice clothes, I kept telling her "it's a math conference, no one will dress up." Suffice it to say I was better dressed than any of the professors.

8. **anon**
   February 8, 2017

   Maybe the posh conference hotel requires some level of formality so that the other guests there are not traumatised 😉

9. **lf**
   February 8, 2017

   Argh, I had not noticed that the dress code was for the Simons conference and not the winter school…thus people can dress like in Woodstock at the winter school.

   More seriously, the theme of the Simons gathering is "p-adic Hodge theory" which encompasses a larger area than the one covered by perfectoid spaces (on the geometric side), typically arithmetic applications of p-adic Hodge theory like you will find in the proof of the STW conjecture, Serre's conjecture and so on.

10. **simplicio**
    February 8, 2017

I think I'm missing some context for this story. Can someone explain (or link to an explanation) to a non-mathematician what the sudden appeal of Perfectoid Spaces?

11. **Tom Andersen**
February 8, 2017

Re: nfdc23, etc.
"Young man, in mathematics you don't understand things. You just get used to them."
– John von Neumann

Sometimes, not understanding the foundations allows one to skip to new ideas. There is a also a level of fear in every social group concerning outsider penetration.

12. **lf**
February 8, 2017

"Sometimes, not understanding the foundations allows one to skip to new ideas. There is a also a level of fear in every social group concerning outsider penetration."

Although there is some truth in this assertion, this is the typical justification all crackpots give when their proof of the Riemann hypothesis is refused because it's pretty clear they don't know the basics of analytic number theory after reading a few lines (same with Fermat and Arithmetic Geometry and so on).

13. **Peter Woit**
February 8, 2017

simplicio,
I'm in no position to explain what a "perfectoid space" is, but the context is that it's a new idea about arithmetic geometry due to Peter Scholze that in recent years has been used to solve a range of problems in that subject. For an outline of such applications, one place is Scholze's ICM talk
http://www.math.uni-bonn.de/people/scholze/ICM.pdf
As lf points out, the connection to the topic of the Simons workshop (p-adic Hodge theory) is just that there have been some applications of perfectoid spaces there. I'm guilty of putting the two topics close together because I couldn't resist the amusement value of the contrast between them.

And I should make it clear that while I'm making a bit of fun of the AWS and Simons workshop, the "ridicule" is affectionate, with no intention of trying to kill off either, they're valuable efforts (unlike when I make fun of some things going on in physics, which I really do wish could be killed off…)

14. **nfdc23**
February 8, 2017

@John Fredsted:

The comment I made on Math Overflow was not at all intended as a put-down, but rather as a reality check to save a person from wasting a lot of time struggling to understand papers and references on a subject when they have not yet learned much more basic things that every author of the more advanced works take as known by their readers. I have seen too many instances of students trying to leap ahead to "study" very advanced topics in mathematics without first learning something serious about the more basic contexts out of which the advanced ideas arose. There's certainly nothing wrong with skipping some steps here and there when seeking to get a sense of what a subject is about, but this is not the situation that the person was describing when mentioning that they didn't know about rigid-analytic geometry.

There are nowadays quite a number of references from which one can learn about rigid-analytic geometry and Berkovich spaces, either of which provide good preliminary experience to then try to get a feeling for the more sophisticated structures involved with creating and especially using perfectoid spaces. My point was that one absolutely have to learn something serious about at least one of those areas before making an attempt at learning about perfectoid spaces (let alone to attend a 5-day workshop on the topic). Anyone who doesn't have some reasonable familiarity with either of those frameworks for doing non-archimedean geometry has no chance to understand anything serious with perfectoid spaces that will be lectured about at the AWS.

It is akin to trying to read advanced textbooks and papers on General Relativity without prior study of undergraduate physics or multivariable calculus. When I was an undergraduate, after taking a graduate course in functional analysis (which I knew also provided the mathematical framework for quantum mechanics) I decided to try to sit in the back of the room in an undergraduate course in QM in the hope that I could learn the subject despite that I had never studied serious classical mechanics (just the simple freshman-level E&M + SR). Needless to say, my experiment was a flop.

I am not claiming that one has to go through all prior historical developments before entering a new subject, or that reading backwards can't be productive (within reason: one ought to have some knowledge of the basics). But if one is really expecting to get something out of a 5-day instructional workshop (not a semester-long course, just 5 days!) then one absolutely must learn the basics first and not rush to the seminal papers on the very advanced stuff first. That is why I said "Life is not a race".

@Tom Andersen:

I don't know if your background is in math or physics (or both), but in pure math having no understanding of the foundations of a subject (by which I mean no understanding of the more basic relevant mathematical ideas and how they are used in proofs and/or important examples, not about set-theoretic/logical foundations) makes it impossible to extract any real understanding about new ideas in more advanced areas of the subject. It may be that in physics (which I don't know much about at a technical level) one really can "skip to new ideas" in a meaningful way in an advanced topic without understanding the foundations, but in theoretical math this is basically impossible (due to the nature of the subject). The issue is not about fear of outsider penetration, but about the way the human mind digests abstract concepts.

Perhaps the example par excellence of someone who made substantial contributions to cutting-edge pure math without a systematic prior study of earlier ideas was Ramanujan. But this is the exception that proves the rule: he rediscovered on his own everything he needed from the earlier ideas of others, and his mastery of examples was singularly spectacular. So even his case fits the paradigm of how one reaches the stage of making creative new contributions to a highly-developed subject in pure math (though the road he took was one that nobody else could have traveled).

In many (though not all) topics at the cutting edge of arithmetic geometry nowadays, it is more often structural rather numerical examples that guide the development of new ideas (that is certainly the case with perfectoid spaces and how they are used), so one really needs some framework to develop intuition for organizing these sophisticated concepts in one's mind. Trying to proceed in such directions without an understanding of (relevant) foundations is not going to put one in a position to make creative use of those ideas. In fact, if the "insiders" in this area really did want to prevent outsider penetration (in terms of creative new ideas from "outsiders"), encouraging the study of the advanced work without knowing the more basic ideas out of which it emerged would be an excellent strategy. 🙂

15. **ds**
February 8, 2017

@lf: in Tucson in March, dressing like Woodstock is encouraged!

16. **lf**

February 8, 2017

@ds: indeed, I should have guessed it, but I “prefer” the spa and the swimming pool of the Schloss Emmau…

17. **John Fredsted**
February 9, 2017

@nfdc23: Ok, point taken. But rereading your comment on MathOverflow, I still think that your message could have been conveyed in a more empathetic way. At least, if I had been the poster, I would have been put down by the very phrasing of it. But perhaps I am just being (too) sensitive.

18. **John Fredsted**
February 9, 2017

Clarification: By ‘the poster’, I mean of course ‘the original poster’, i.e., the recipient.

19. **oliver knill**
February 9, 2017

I might not be the right person to give career advise but entering the nesting ground
of giants (especially if these monsters are so young) can mean living a very competitive life. There certainly will be tears. You are the tortois running after Archilles. Once you are where Archilles was, the giants have already gone much further. Having been close to giants I know the experience: it is exciting but also humbling. It is maybe the competitive career market but both in physics as in mathematics, the herd instinct is currently particularly strong. Fortunately, both in mathematics and physics, there are many nesting grounds and some are less crowded. But the one covered in this conference seems particularly grand. Maybe its just that most of the crowd will be tourists who want to have a glimpse at the superstars in that field which rapidly appears to become classical mathematics. And like Woodstock, also math has its legendary moments. The ICM’s of 1900 or ICM 1950 (http://abel.harvard.edu/history/icm1950) must have been such, or the Woods hole conference from 1964, at a time when the index theorem was exploding. I attended once a conference /summer school at Hillerod, where lots of giants were present
http://abel.math.harvard.edu/~knill/history/hillerod Fantastic? Yes. But also a bit intimidating. I myself enjoyed once a summer school in les Houches,
http://www.math.harvard.edu/~knill/various/chamonix , where my wife participated and I actually enjoyed the tourist status very much. Now a bit older, I only can give the advise, go and enjoy as much as you can but also know about other nesting grounds where you can find one or the other leafs not yet trampled upon. Its also good to be aware of one important fact which applies to almost everybody: there are always mathematicians/physicists who are (much) better than you!

20. **Shantanu**
February 9, 2017

Peter, OT. A colloquium by John Ellis at PI on where is particle physics going
http://pirsa.org/displayFlash.php?id=17020013

21. **sigoldberg1**
February 9, 2017

Maybe the MAA or AMS or the meeting organizers, seeing what is occurring, could also organize a few informal seminars, study groups, etc. at a more introductory level in conjunction with the main meeting for those who are not really prepared to understand much of what is being presented by the experts. This might turn a potentially negative event into something more enjoyable and valuable for all.

22. **ds**
February 9, 2017

@sigoldberg1: Indeed the AWS does precisely that sort of thing, every year. AWS has been around for two decades and is a rather mature entity by now, with many people putting a lot of care into making it work well for as broad a range of students as possible.

Various Links

February 10, 2017
Categories: Uncategorized

The Columbia Math department has been doing extremely well in recent years, with some wonderful mathematicians joining the department. A couple items first involving some of them:

- Kevin Hartnett at Quanta Magazine has a great article about developments in the field of technical issues in the foundations of symplectic topology. This explains work by my colleague Dusa McDuff, who together with Katrin Wehrheim has been working on such issues, trying to resolve questions raised by fundamental work of Kenji Fukaya and collaborators. For technical details, two places to start looking are here and here.

  The Hartnett story does an excellent job of showing one aspect of how research mathematics is done. Due to the complexity of the arguments needed, it's not unusual for early papers in a new field to not be completely convincing to everyone, with unresolved questions about whether proofs really are airtight. The way things are supposed to work, and how they worked here, is that as researchers better understand the subject proofs are improved, details better understood and problems fixed. Along the way there may be disagreements about whether the original arguments were incomplete or not, but almost always people end up agreeing on the final result.

  Also featured in the article is another of my Columbia colleagues, Mohammed Abouzaid, who provides characteristically wise and well thought out remarks on the story.

- Via Chandan Dalawat, I learned of an interesting CIRM video interview with another colleague, Michael Harris. The same site has this interview with Dusa McDuff, as well as a variety of other interviews in English and French.

For some other non-Columbia related links:

- The 70th birthday of Alain Connes is coming up soon, and will be celebrated with a series of public lectures and conferences on noncommutative geometry in Shanghai. This year will be the last series of lectures by Connes at the College de France. They're appearing online here, and I highly recommend them. He's taking the opportunity to start the series with a general overview of the point of view about the relationship of geometry and quantum theory that he has been developing for many years.
- For employment trends in theoretical particle physics, there are some updated graphs of data gleaned from the particle theory jobs rumor mill created by Erich Poppitz and available here. In terms of total number of jobs, there has been some recovery in the past couple years, with about 15 jobs/year, above the 10 or so common since the 2008 financial crisis (before 2008 numbers were higher, 20-25). As always, an important thing to keep in mind about this field is that this number of permanent jobs/year is a small fraction of the number of Ph.Ds. in the subject being produced each year at US universities.

  The numbers for distribution of subfields separate out "string theory" and lattice gauge theory. There have always been few jobs in lattice gauge theory, appear to be no hires in that subject for the past two years. I'm putting "string theory" in quotes, because it's very hard these days to figure out what counts as "string theory". With Poppitz's choice of what to count, hiring in string theory has recovered a bit, now around 25% of the total for the past two years, up from more like 15% typical since 2006 (earlier on the numbers in some years were around 50%).

- As pointed out here by commenter Shantanu, on Wednesday John Ellis gave a talk on Where is particle physics going? at Perimeter. I'd characterize Ellis's answer to the question as "farther down the blind alley of supersymmetry". He spins the failure to find SUSY so far at the LHC as some sort of positive argument for SUSY. The question session was dominated by questions about SUSY, with Ellis taking the attitude that there's no reason to worry about the failure so far of the fine-tuning argument for SUSY, all you need to do is "ratchet up your pain threshold". I fear that's some sort of general advice where this line of research is going.

  About the failure to find any evidence for SUSY wimps that were supposed to explain dark matter, Ellis explained that he had been working on this idea for 34 years, first writing about it in 1983, so with that much invested in it, he's not about to give up now.

Update: Davide Castelvecchi points me to another new mathematics story at Nature.

Update: One more. A profile of Roger Penrose by Philip Ball. Penrose explains that his main problems with string theory come from two sources. One is the instability problem of extra dimensions, the other is his aesthetic conviction that sticking to four space-time dimensions is a good idea since it is only in four dimensions that you get the beautiful geometry of twistors. Ball raises the interesting question of whether Penrose could have a successful scientific career if he were starting out today:

> Worst of all, the career structures and pressures facing young researchers make it increasingly hard to find the time simply to think. According to several early-career scientists interviewed by Nature, the constant need to bring in grant money, to produce papers and administer groups, leaves little time to do any research, still less indulge anything so abstract and risky as an idea.

Comments

1. **Anon**
   February 10, 2017

   Hasn't Joyce been working on this subject for years now? I was surprised not to see a mention.

2. **Shantanu**
   February 10, 2017

   Peter, one thing I was surprised to see Ellis mention is that so far no one has found a convincing connection between neutrino oscillation phenomenology and TeV scale physics (and that's why he skipped neutrino physics) Isn't this the most important question which particle physicists and string theorists should try to answer? Why is there no progress (or even concern that there is no progress) inspite of a nobel prize last year in this area?

3. **Abby yorker**
   February 10, 2017

   It is very difficult to imagine that the proof mentioned in Nature will help geophysicists in any practical way. The algorithms surely are in place and independent of detailed mathematical proofs.

   I also wonder whether it applies to areas of extreme velocity that are avoided by "raypaths". Perhaps finite bandwidth helps. Dunno.

4. **A.K.**
   February 11, 2017

not that I wanted to give here remarks which would go into any detail, but the problems in symplectic topology seem to go FAR beyond Fukaya's foundational papers. Just as an arbitrarily chosen question: did anyone here understand 'Eliashberg's existence proof on symplecic topology'? It is not 120 pages long (it is merely 10 pages or so), it doesn't contain too many technicalities, but it contains several crucial statements from singularity theory that are nowhere proven, provided with no proper links or explanation and thus give the paper a highly folkloristic character. Just to say this. Things developed with some reason the way they do.

Many approaches in 'modern' symplectic geometry were somehow 'adopted' from algebraic geometry but without its firm algebraic or systematic foundation, a firm dictionary between certain algebraic concepts and their 'analytic' counterparts was never set up, Floer theory, while easy in its idea and conception, proved to be an analytic nightmare and over the years, symplectic geometry mutated into some sort of, as I would call it, Sobolev space monster with, in compensation to the high degree of technicality, highly esoteric concepts whose existence was rather assumed than understood.

But even worse: the rise of Floer theory and Gromov-Witten theory obscured the number theoretic origins and character of quantum mechanics and mechanics, it obscured many traditional developements in quantization and physics which were far better understood than the 'new methods' that were deemed by everyone working in the field as 'hard methods' vs. all traditional methods were suddenly depreciated as (the somehow ill defined category) 'soft methods'. The common deformation theoretic background of hard and 'soft' methods was never understood, classical (real and complex) singularity theory, a tremendously rich and well understood field, was never fully incorporated or acknowledged by the 'new' methods. To understand this, a little bit knowledge of Horkheimer's and Adorno's writings on the dialectic way knowledge and science progresses would have been neccessary, with every knew theory tending to destroy the old systems of reference, but such meta research was never done.

I deeply disagree with the view that Fukaya'a papers are the main source of problems in symplectic topology, it is a highly scapegoat-defining approach to the many deeply running problems in this field.

5. **Friedrich**
February 12, 2017

Peter,

when listening to the lectures of Alain Connes – thank you for the link ! – I get a strange impression. It seems that this has very little to do with physics, and I am not really sure that this has a lot to do with math. It seems to me – but I may well be wrong – that he is playing around with abstract concepts in some abtsract world of thoughts. There is no real result. Less famous people that do such things are not taken seriously by the community. Another way to put my impression is the following; Connes' world is akin to that of string theorists: it is complex, interesting, but has no relation to nature.

But let me return to the first lecture, which is his motivation and advertizing one. It is really bizarre. For example, he states that the Higgs has a mass value that makes the standard model wrong at high energy – wheres enough people say just the opposite, namely that it has a mass value that makes the standard model valid up to Planck mass.
The citations of the mathematicians in the first lecture are pretty. The stories about the definition of the metre show his enthousiasm. His explanation of the reason for non-commutativity is pretty as well – but it is not physics.

I like his naive enthousiasm – but is this physics? I would be interested to hear other people's opinion about this – incuding yours, Peter. In any case, thank you for pointing this out!

6. **AC**
February 12, 2017

To see the "results" for this parts of my talks you need to reach the sixth hour where the standard model (extended to Pati-Salam) coupled to gravity appears as the spectral action on all spin 4 manifolds from irreducible representations of the higher Heisenberg relations. This is both a difficult mathematical result (using in particular the theory of immersions) and a physics result explaining "why nature is as it is". It would be invalidated by the discovery of SUZY and would have been invalidated by the dicovery of the diphoton 750 Gev. So it is physics. When one runs the scattering parameter (H^4 coupling) of the SM at Planck mass it becomes negative if the Higgs mass is too low, this is what was referred to in the talk.

7. **Friedrich**
February 12, 2017

Dear Alain,

thank you for your kind answer. I remember how enthousuastic I was when I first heard about your model about 25 years ago. I followed it ever since. Please allow me to add two points that explain what I wrote above and that dampened my enthousiasm.

Somewhere I read that your non-commutative geometry model does not lead to U(1)*SU(2)*SU(3) UNIQUELY, but that several other groups could also arise. Is this true?

A good friend of mine here in Munich showed me a text by Niels Bohr where he speaks of quantum theory stemming form h-bar as the smallest action in nature. This simple definition is a strong contrast to the very complex idea of quantization presented in your series of talks. As a physcist, I have a tendency to choose the simpler solution.

Best regards
Friedrich

8. **martibal**
February 12, 2017

Dear AC,
when you say that the discovery of the diphoton 750GeV would have invalidated the model, are you referring to the analysis of Aydemir and al (https://arxiv.org/abs/1603.01756) according to which (I quote their paper):
"even though the 750 GeV diphoton resonance can be accommodated within the NCG motivated unified Pati-Salam models, the price one has to pay is a certain amount of fine tuning in the sector involving the necessary colored scalars" .
Or are there other reasons ?

9. **Peter Woit**
February 12, 2017

Friedrich,

I don't think saying "h bar is the smallest action in nature" gets you much of even the pre-1925 "old quantum theory", much less quantum theory itself. Quantum theory as we understand it today is based on some very deep mathematics, not some simple physical intuition, and getting an even deeper understanding I think is going to require even deeper mathematics. Connes is one of very few first-class mathematicians (or physicists…) trying to do this, in a very original way. The kinds of questions he is asking and trying to find answers to are the most fundamental ones, ones most physicists seem now to have given up on, in favor of a pseudo-scientific excuse ("the multiverse did it").

I've made a few attempts to follow his ideas, each time impressed by the originality of what he is trying to do, and finding some of it quite compelling as well as providing some new insight into things at the boundary of math and physics that had always fascinated me. I'm looking forward to watching all of the latest series of lectures and trying to follow in detail, so far haven't had the time to get very far yet. As for whether he's got a predictive model that convincingly explains things the SM doesn't, I haven't understood what he is doing well enough to know. I confess to being more interested in putting time into understanding the underlying ideas and seeing what new insights I can get from them.

10. **AC**
February 12, 2017

Dear Martibal

The answer to your question is « yes ». In general the road that we
followed with my collaborators on this issue of using the new paradigm
of « spectral triples » (which could also be called « spectral geometry »)
to understand what the SM coupled to gravity is telling us about the nature
of space-time, is the one proposed by Riemann in his inaugural lecture.
There Riemann explicitly suggests that for very small distances, since the
notions of light ray and of solid body loose their meaning one should be ready
to accept that the structure is more involved than the continuum. He makes
two key points
1) "Es muss also entweder das dem Raume zu
Grunde liegende Wirkliche eine discrete Mannigfaltigkeit bilden,
oder der Grund der Massverhaltnisse ausserhalb, in darauf
wirkenden bindenen Kraften, gesucht werden » which is (badly) translated as
"Either therefore the reality which
underlies space must form a discrete manifold, or we must
seek the origin of its metric relations outside it, in the binding
forces which act upon it »
This point is fully taken up by the NCG approach where the inverse line-element
(which encodes the metric relations) exactly encodes all the forces (gravitational
and gauge bosons).
2) He continues by saying (let me skip the german):
"The answer to these questions can only be obtained by starting from
the conception of phenomena which has hitherto been justified by
experience, and which Newton assumed as a foundation, and by
making in this conception the successive changes required by
facts which it cannot explain. »
So here the key word is « successive changes » and in our experience this has been
a long struggle but each time it has been rewarding. There were quite long periods
where we were abandoning the model, saying that after all it could have been a
coincidence of some sort that it seemed to fit. But for instance it survived the neutrino
mixing (after a silent period from 98 to 2006) and more recently the wrong m_H>170 Gev
and in both cases we learned. In the first we understood the KO-dimension 6 of the
finite space (giving the fine structure) but we also rediscovered the see-saw mechanism
from the calculation (not artificially put by hands). For the wrong m_H>170 Gev we learned
our stupidity to have neglected the effect of a scalar field (not the higgs) which was there
and which we had ignored in the RG calculation.
What I have learned myself is that it is never a good idea in this stuff to try to force
a result, and the 750 Gev diphoton would have been such a case. That's for instance
why I avoided the subject when I taught my penultimate class in the College last year
in the winter of 2016!
For a long time we proceeded in the « bottom-up » manner but the recent work with
Chamseddine and Mukhanov, which is the subject of lectures #5 and #6 this year gives
a potential explanation, finally, for the slight amount of non-commutativity present and
that was the main point of this first half of my class this year!

11. **martibal**
February 12, 2017

Dear Alain, thanks for the detailed answer !

12. **Urs Schreiber**
February 13, 2017

AC wrote in small part:

> paradigm of « spectral triples » (which could also be called « spectral geometry »)

The term "spectral geometry" might have more potential than "spectral triple" to communicate to the bulk of the high energy physics community that this is a concept not alien to what they are long familiar with. Indeed the idea to encode effective target spacetime geometry in the worldvolume quantum mechanics — hence in the operator spectrum — of a quantum object that roams in it is familiar from perturbative string theory, where the only difference is that instead of a 1-dimensional worldline for a quantum particle, leading to a "spectral triple", one considers a 1+1-dimensional worldsheet, leading to what could be called a "2-spectral triple" if it were not already called a "2d SCFT".

This close relation between the Connes-Lottes-Chamseddine-Barrett approach of modelling particle physics and that of perturbative string theory has long been pointed out by people like Jürg Fröhlich and Maxim Kontsevich, with details worked out by people like Katrin Wendlandt and Yan Soibelman (here), but it seems to remain underappreciated among members of both communities.

There is a review with further pointers to the literature on *PhysicsForums-Insights* at *Spectral Standard Model and String Compactifications*.

This general relation of the underlying theory gets all the more interesting with the developments starting with Barrett's insight into the reality condition of realistic spectral triples and the result that the KO-dimension of the compact space in the Connes-Lott-Chamseddine model has to be 6, thus leading to a total KO-dimension of spacetime of d = 4 + 6 in these models (albeit seen only mod 8 by KO-theory). This of course coincides with the result of the criticial dimension found earlier in "2-spectral triples", namely in perturbative superstrings.

This interesting match seems to make it well motivated to ask whether under the point-particle reduction from 2d CFTs to spectral triples due to Fröhlich-Gawędzki, Roggenkamp-Wendland and Soibelman the Connes-Lott-Chamseddine spectral triple is indeed the point particle limit of a 2d SCFT. If so, that 2d SCFT would be a natural candidate for the UV-completion of the model, and hence potentially of realistic particle physics.

An interesting prospect, whose further examination seems to be stalled by a detachment and mutual misunderstanding of the communities on both sides of the relation.

13. **cedric bardot**
February 13, 2017

I think one important piece of information is missing (or not stressed enough by anyone here) for the readers of Peter's Blog interested in watching Connes last mathematical lectures at College de France.
To appreciate the potential physics interest of noncommutative spectral geometry one has to mention the already established connection (in arxiv.org/abs/1409.2471) between the quantization of volume in 4D Riemannian manifolds mathematically formalized by Connes with the non-dynamical scalar fields introduced by the theoretical physicist Chamseddine and cosmologist Mukhanov to account simultaneously for both dark matter and dark energy (not to mention the more implicit link with another connected idea about a limiting curvature of spacetime in 1612.05860 and 1612.05861).
To answer Friedrich question, I do not know what was the reception of the Mukhanov lecture "Non-commutative Geometry and Mimetic Dark Matter" at Pierre Fayet (French physicist well known for his contribution on supersymmetry) fest last december at ENS Paris (moriond.in2p3.fr/Fayet/program.php) but LHC and LUX data as well as numerical tests of some minimal non-supersymmetric gauge unified models with Pati-Salam structure (1412.4776 and 1612.07973) seem to confirm the message provided by the spectral noncommutative geometry : the phenomenology of our unique observable universe at the 230 meV current temperature requires, for the time being, just the standard model particle content up to an extrapolated 10^12 GeV scale leptogenesis where three right-handed Majorana neutrinos and some new Higgs brother(s) and other gauge boson(s) need to appear on stage.
So may-be it's worth focusing on measuring the Higgs couplings and top mass with the best precision before dreaming of the construction of a 100 TeV accelerator. Spending more time to the tedium computational exploration of vacua solutions of the spectral action and their astrophysical connections might be not very inspiring task at first look but who knows, may be the solution to the hierarchy problem lies there…

14. **sympathy for the symplectic**
February 13, 2017

At the webpage of Aleksey Zinger he posts further evidence of a dysfunctional foundational situation in symplectic topology. Papers there document what happened when he tried to carefully read, and then repair, the literature on the "formula" for Gromov-Witten invariants of symplectic sums. A major paper in the Annals was eventually retracted, and much other fireworks ensued.

15. **Steve Sharpe**
February 16, 2017

Just a quick comment on HET hiring, concerning the lattice breakout. Most hiring of those doing lattice field theory in the last decade has been in Nuclear Theory, and the numbers there are a bit more encouraging.

16. **Peter Woit**
February 16, 2017

Thanks Steve,
That's good to hear.

17. **colorado**
February 17, 2017

Hi Peter,

I see that there are no tenure track faculty in Columbia's math department. What's the difference between a Ritt Assistant Professor and a post doc? The teaching responsibility for a Ritt position seems little heavy. Do math departments directly hire into a tenured position after an extended post doc these days?

18. **Peter Woit**
February 17, 2017

colorado,
In math in the US (unlike physics), there are few purely non-teaching postdocs, with the standard research career path after grad school a few years in a non-tenure track job like our Ritts, then a tenure track job. Even NSF postdocs are often combined with some teaching (making it a longer position). The current teaching load for a Ritt is 2 and 1 (it was 2 and 2 way back when, when I had one). This is a lot more teaching than physicists do, on the other hand, job prospects for those coming out of these jobs are much better than for theoretical physicists.

One reason for having few if any tenure track positions is that the market for the best young people is quite competitive. If you do a search for a tenure track position you often find that the top candidates have competing offers from institutions that decide to offer them tenure to attract them, and you need to match that.

19. **AC**
February 18, 2017

Since my last lectures in the College de France are in French, I am giving the link to a paper which I am writing (and which is still evolving), it is
https://www.dropbox.com/s/8jz865ezxjwrr91/J-Kouneiher.pdf?dl=0
In particular I explain in section 3.4 the deep mathematical roots of the notion of spectral geometry (spectral triple) coming from the work of Sullivan on KO-orientations and the origin of the KO-cycles in index theory starting with papers of Atiyah and Singer in the 60's. The above paper is far from final and critical comments, missing references, etc are very useful at this point.

20. **anon**
February 20, 2017

Discussion in comments to that Quanta Magazine article seems to have gone off the rails…

21. **Peter Woit**
February 20, 2017

anon,
Yes, and an excellent illustration of a couple of important general principles that people are sometimes not aware of until it's too late:

1. The world is full of people who know nothing about the topic being discussed but want to use your comment section for their own ax-grinding. Unchecked, they will destroy any intelligent discussion.

2. Attributed to John Baez: it's not easy to ignore Lubos, but it's always worth the effort.

22. **David Roberts**
February 20, 2017

Dear AC

thank you for putting your draft online! It is very illuminating, and useful for those who grasp of spoken French is insufficient, or whose internet bandwidth is not up to the task of downloading the videos.

23. **AC**
February 21, 2017

Dear David, thanks, here is a much improved version as far as the second half of the paper goes, ie the discussion of the "quanta of geometry" in section 4. The new link is https://www.dropbox.com/s/gzrqzjvilvgwfk9/J-Kouneiher.pdf?dl=0

A Big Bang in a Little Room

February 18, 2017
Categories: Book Reviews, Fake Physics

There's a review in today's Wall Street Journal by me of Zeeya Merali's A Big Bang in a Little Room. If their version is behind a paywall you might find also find it elsewhere (for instance here). I'll reproduce parts of the review below with some comments more appropriate for the blog venue. As always, the editors at the WSJ did an excellent job of improving the first draft I sent them.

Merali has a website about the book here, and last week Nature published this review by Andreas Albrecht. Albrecht criticizes the book for "sloppy interplay between science and religion", but I think he misses the important point that the most serious problem here is the sloppiness about what is science and what isn't. When physics journals decide to publish articles like this one, it's not surprising that science writers make the mistake of taking them seriously and writing about them (Merali's first chapter is about this paper).

Here are some extracts from the review, with some comments:

> What happened at the Big Bang—or before—is an irresistible question but one that, for now, as science, lies in the realm of the purely speculative.
>
> In "A Big Bang in a Little Room," science writer Zeeya Merali turns the question around, asking instead whether physicists can create a "baby universe," born in its own Big Bang. Indeed, one prominent theorist she interviews has suggested that our own universe might be a baby universe created by a "physicist hacker," with the complex pattern of fundamental particle masses intended as some sort of message to us. thereby learning more about the beginnings of the "old" one.

The reference here is to Andrei Linde and this 1991 paper.

> explains that her interest in this topic is tied up with her religious beliefs: If we ourselves could play God and create a new universe, wouldn't that creation amount to a theological discovery, showing the likelihood that some higher intelligence was responsible for the Big Bang? She structures her narrative around interviews with prominent theoretical physicists; they mostly discuss science, but religious questions sometimes play a role, with often fascinating results. While some refuse to engage, she gets others to discuss such topics as the relation of the laws of physics to God's happiness, the possibility of a physical "consciousness field," and what the quantum mechanics of the Big Bang might indicate about the possibility of life after death and resurrection.

Don Page is the one interested in God's happiness, Abhay Ashtekar in the "consciousness field", and Andrei Linde in resurrection.

> Mr. Linde is the central figure in this story, and Ms. Merali describes him as "a showman: bombastic, passionate, and fueled by the certain belief that inflation theory, which he helped to invent, is correct." While Ms. Merali takes all of this seriously, there are very good reasons why most physicists don't. Readers of "A Big Bang in a Little Room" would be well-advised to enjoy the ride but stay skeptical. Inflationary models can to some degree be confronted with observation and tested (a topic covered in other books but not this one).

About the string theory landscape:

> Ms. Merali gives a disturbing version of this, contemplating the possibility that "string theory and inflation may be conspiring against us in such a way that we may never find evidence for them, and just have to trust in them as an act of faith."

This comes after an explanation of the anthropic multiverse point of view from HEP experimentalist Greg Landsberg, where he adds the twist of anthropics explaining why the string scale is at such high energy, and thus unobservable. The full paragraph in the book is

> In other words, the physics of string theory and inflation may be conspiring against us in such a way that we may never find evidence for them, and just have to trust in them as an act of faith. The multiverse truly works in mysterious ways!

If that paragraph doesn't make a scientist's blood run cold and see the danger physics is facing, I don't know what will. I end the review with

> In an era where "post-truth" was the word of the year, scientists and science writers need to make clear that science is not a species of theological or philosophical speculation and not about belief or entertainment value. Legitimate scientific claims are those that can be backed up with evidence, and unfortunately the wonderful and exciting story told well here contains none at all.

My concern about the topic of the book is that it's Fake Physics, not that religion is motivating the author (and likely motivating the Templeton Foundation to fund this project). A book about the religious views of physicists would be an interesting one that I'd certainly read, and the material in this book on that topic is quite interesting. One of the odder twists here is that the two blurbs from physicists promoting the book are from Sean Carroll and Martin Rees, with Carroll writing

> So you want to make your own universe. Zeeya Merali's new book won't quite give you an instruction kit—but it's the closest thing we have at the moment. A fun and mind-expanding ride through modern ideas of how universes come to be.

I don't see how you can be devoted to fighting for science against religiously-driven pseudoscience, and think that this book is one you'd like to see be the public face of what "modern ideas" about cosmology are.

Comments

1. **Jeff M**
   February 18, 2017

   So how good a journal is Modern Physics Letters A? I was thinking I could write a math paper, "Letters from the creator in mathematical objects." Have to know what journal to send it to though.

2. **tulpoeid**
   February 19, 2017

   "If that paragraph doesn't make a scientist's blood run cold and see the danger physics is facing, I don't know what will."

   Totally. Even more so if you consider that Landsberg was the physics coordinator of the CMS experiment during the Higgs discovery.

3. **Dave Miller**
   February 19, 2017

Well, Peter, I'm on your side when you say, "If that paragraph doesn't make a scientist's blood run cold and see the danger physics is facing, I don't know what will."

But, I have a sinking feeling our side is going to lose.

On the other hand, the younger generation always makes a name for itself by revolting against its elders, so, perhaps, in a couple decades, the hottest thing in physics will be angry young physicists revolting against the "anthropic principle" etc.

By the way, I haven't seen you comment yet on the article in the current Scientific American claiming that inflationary cosmology is in a lot of trouble.

Dave Miller in Sacramento

4. **a1**
February 19, 2017

Few people seem to realize that pronouncing something to be random is just saying we won't be discussing further hypotheses. So, asserting that a physical quantity is just the value of some random variable across the multiverse is not different from asserting that a god willed it to have this value in our universe. Both ways, this is an ultimate explanation without any further 'why'.

5. **Gus Bici**
February 19, 2017

"You are of this world; I am not of this world." says Jesus.

"NOTW" (i.e. Not Of This World) is a popular Christian bumper sticker. May need to print up some more for the Physics community. Amen.

6. **Mitchell Porter**
February 19, 2017

Thirty years ago, there was a fad for "third quantization" as a way to model "baby universes". Did it ever predict anything (whether right or wrong)? Did it get write-ups in the science press? And, is any of that work, an essential precursor to today's inflationary multiverse? Or was it a fundamentally separate theoretical excursion whose life and death unfolded in relative isolation?

7. **Peter Woit**
February 19, 2017

Dave Miller,
I should write about that, it's an excellent discussion of the problems of inflation, and of the more general problems I often write about here.

Mitchell Porter,

I guess you're referring to this sort of thing:
https://www.jstor.org/stable/38273
I don't think that led anywhere, it's a generalization of the idea of string field theory, from a circle (string theory) to arbitrary 3-manifolds. No one has managed to really successfully do this for the circle, and it's not at all clear to me there is any viable idea about how to even get started if you generalize to 3-manfolds, so not surprising this never went anywhere.

This has nothing to do that I can see with the eternal inflation story and the recent multiverse mania.

8. **ABC**
February 19, 2017

Original WSJ story here:

https://archive.is/dT8rA

9. **srp**
February 19, 2017

For anyone interested in the idea of creating universes in an accelerator, astrophysicist Gregory Benford's 1999 novel Cosm is pretty entertaining, though not close to his best stuff. He had the setting as the Relativistic Heavy-Ion Collider at Brookhaven.

https://www.amazon.com/Cosm-Gregory-Benford/dp/0380790521

10. **Shantanu**
February 19, 2017

Dave: Can you point me to the sciAm article?

11. **Nick M.**
February 20, 2017

Hi Shantanu,

I think Dave may be referring to this article:

https://www.scientificamerican.com/article/cosmic-inflation-theory-faces-challenges/

But, unfortunately, it's paywalled. I guess I'll have to newsstand a copy of the February 2017 Scientific American.

-Nick

12. **phil fogle**
February 20, 2017

Great review! So good to see a critic who isn't afraid to step on toes. But the war against science is ancient, and vigilance will always be called for.

At least this stuff isn't lethal, like Andrew Wakefield's pseudoscience…

13. **Dave Miller**
February 20, 2017

Shantanu,

Believe it or not, I actually read the article in a hard-copy at our local public library! (The Sacramento public library system is actually very good.)

Dave

14. **Nick M.**
February 20, 2017

Shantanu, Dave:

I think I stumbled onto a downloadable PDF copy of the article from professor Aviv Loeb's "Center for Astrophysics" webpage on the Harvard-Smithsonian website (see here).

Alternatively, go to the Harvard web address

https://www.cfa.harvard.edu/~loeb/

and scroll down to the "In The News" section, and look for the article titled "Pop Goes the Universe".

(God I hope I don't get into trouble. 😉)

15. **Mateus Araújo**
February 21, 2017

I can already see how your review will appear in the promotional material:

"Wonderful and exciting story" – Peter Woit

16. **Alan**
February 22, 2017

Wondered if you've come across Dr. Luke Barnes' work A Fortunate Universe: Life in a Finely Tuned Cosmos which seems relevant.
https://afortunateuniverse.wordpress.com/2017/01/10/talk-in-sydney-the-universe-for-beginners/
The video in there goes through 4 options, 1. physics will explain the constants (no multiverse) 2. a multiverse 3. a creator or something-like behind everything 4. we're living in a simulation.
So I guess the "physicist hacker" above could fit in 3 or 4?

As to universe creation above, what I remember (but not understand) of what Lenny Susskind says was that the config. of a Calibi-Yau space determines what universe will get created (like a "DNA") and since (my view) you can't get to that energy or specificity of configuration when you attempt to create one in a lab., it's a hopeless task to try – if the multiverse idea is correct. But even if it isn't aren't the energies out of reach?

17. **Bernhard**
February 22, 2017

@Jeff M,

I can give you my own take on what I think of MPLA from an experience a few years ago..

I was asked to be reviewer of a paper from a very good journal. On reading the article, I realized it was very bad, to the point I was not sure one could consider it really an article. It was not difficult to convince the editor that he should not publish an article of that quality, something that would hurt the journal's reputation.

A few months later, I checked the arXiv entrance for the article and saw it has been published in MPLA.

Make your own conclusion.

18. **Jeff M**
February 22, 2017

@Bernhard

Yes, there are math journals like that too 🙂

19. **Slawomir Piotr Wojcik**
March 11, 2017

I believe, I didn't read a different book, but, certainly, I have a somehow misaligned impression. It is a long time since I finished school, so I find it hard to judge what is a common scientific knowledge, but it seems to be much lower than it used to be. This chimes with what level of knowledge and curiosity the book (the author, by a sample of a book, if you let me) assumes in the reader.
On page 193, line 6 of my copy Zeeya Merali writes about Don Page commenting on why infinity of multiverses might be simpler to understand than the Universe itself: "To illustrate his point, he tells me to think of the set of whole numbers-1,2,3…" I hope, I vaguely imagine the same what a phd in cosmology would in such a case, namely something like decision problems in sets of natural numbers (https://pdfs.semanticscholar.org/ed71/ebe0eee4f88f095247c8b62ba1d3b217a68d.pdf)
The problem with this bit of text (and most of "dry remnant on a sieve" of the rest of the book – which doesn't amount to much) is that narrative and assumed (target) readers knowledge and curiosity seem to be intended to produce (and it would be a nice paper in psychology of perception if this book's an plethora of others' of the sort impact on knowledge and outlook of a reader were analyzed) a rather big collection of people who missed the hint and were left no wiser.
When read in this way (parallel way – what the author and her interlocutors know, what my knowledge allows me to decipher from the book about their intentions and what the bulk of the readers are going to make out of it) the book seems to be bursting in its seams with innuendo of a very special kind.
The real problem, which the book comes close to expose but never even tries to do so explicitly is that: Our theories are systems of conservation of information. Hence

they are good to describe a Universe that itself is such a system (in usual words “deterministic theories describe deterministic world”, but even this simple equivalence seems to be a leap too far for many and Merali doesn’t appear as keen on making leaps of this sort any easier) What if the rule of preservation of information (determinism) is of limited application? What are the limits of our knowledge and what are the implications for every individual in this entropy governed Universe? Convoluted, as it is, the book skips on the questions, the whole body of relevant research, but, by virtue of this innuendo (a false perception of mine?) works as a portent of things to come.

Various News

February 22, 2017
Categories: Multiverse Mania, Uncategorized

First some mathematics items:

- Igor Shafarevich, one of the great figures of twentieth century algebraic geometry and algebraic number theory, died this past weekend at the age of 93. Besides his many contributions to mathematics research, he was also a remarkably lucid expositor. His two volume Basic Algebraic Geometry is a wonderful introduction to that subject, his survey volume Basic Notions of Algebra emphasizes the connections to geometry, and his volume on number theory (with Borevich) struck the AMS reviewer as "delectable".

  Shafarevich was also known for his religiously-motivated nationalistic views which to many were distressingly anti-Semitic. In the spirit of respect for the recently deceased, I'll just link to a quite interesting recent discussion (very sympathetic to Shafarevich) of the issue by David Mumford here (and ruthlessly delete attempts to argue about this in the comment section).

- The AMS Notices has a set of articles in honor of Andrew Wiles and his work, which include some great explanations of the mathematics, as well as a long in-depth interview.
- For another detailed interview with a mathematician, see Quanta magazine for a piece by Siobhan Roberts about Sylvia Serfaty of the Courant Institute.

On the physics front, there's:

- For his contribution to the Why Trust a Theory? conference (see here and here), Helge Kragh has a new paper which examines the question of whether history of science can help evaluate recent claims about the need to change the way theories are assessed. He sees in the unsuccessful "vortex theory" of the late nineteenth century an analog of string theory, with many of the same claims and justifications for lack of success. He quotes as a typical example of the enthusiasm of the time:

  > I feel that we are so close with vortex theory that – in my moments of greatest optimism – I imagine that any day, the final form of the theory might drop out of the sky and land in someone's lap. But more realistically, I feel that we are now in the process of constructing a much deeper theory of anything we have had before and that … when I am too old to have any useful thoughts on the subject, younger physicists will have to decide whether we have in fact found the final theory!

  but then explains that this is actually a quote from Witten, with "string" replaced by "vortex".

- Scientific American this month has an article (also available here) about the problems with the theory of inflation. The authors end by pointing out the dangers to science of multiverse inflationary scenarios (which they call the "multimess"):

  > Some scientists accept that inflation is untestable but refuse to abandon it. They have proposed that, instead, science must change by discarding one of its defining properties: empirical testability. This notion has triggered a roller coaster of discussions about the nature of science and its possible redefinition, promoting the idea of some kind of nonempirical science.
  >
  > A common misconception is that experiments can be used to *falsify* a theory. In practice, a failing theory gets increasingly immunized against experiment by attempts to patch it. The theory becomes more highly tuned and arcane to fit new observations until it reaches a state where its explanatory power diminishes to the point that it is no longer pursued. The explanatory power of a theory is measured by the set of possibilities it excludes. More immunization means less exclusion and less power. A theory like the multimess does not exclude anything and, hence, has zero power. Declaring an empty theory as the unquestioned standard view requires some sort of assurance outside of science. Short of a professed oracle, the only alternative is to invoke authorities. History teaches us that this is the wrong road to take.

- Nautilus has an article by Juan Collar about the increasing skepticism about Wimps as dark matter candidates, and the interest in alternatives.

Update: With results from the full 13 TeV dataset just a few weeks away, SUSY enthusiasts have given up hope for the LHC. A new paper just out argues that pre-LHC claims that naturalness + SUSY implied a gluino mass upper bound of 350 GeV (the latest LHC limits are more like 1900 GeV, likely to go up next month) were misguided. According to these authors, the right number for the upper bound is 5200 GeV and the "HE-LHC with 33 TeV is required to either discover or falsify natural SUSY". So, claims that the LHC could falsify natural SUSY are no longer operative now that it has done so by earlier metrics, and such discovery or falsification is still just around the corner. All that's needed is to rebuild the LHC into a higher energy version (that's what the HE-LHC proposal is, may take a while…).

Update: Another excellent article by Natalie Wolchover at Quanta, this time about progress in studying conformal quantum field theories in higher dimensions (above 2). Definitely one of the more interesting things going on in theory at the moment. The reference in the subtitle to "geometry underlying all quantum theories" I don't think though is really justified, this is really just about conformal field theories.

There's probably lots more to be learned about these, with this conformal symmetry still not fully exploited. I'm somewhat fond of the point of view that you really shouldn't try and think of QFTs just as effective theories for some different physics at short distances. Rather, what might be going on at short distances is not some new kind of theory at the cutoff scale, but a conformal theory valid at all scales.

Update: From the comments, unfortunately two other deaths to report, those of Bert Kostant (I've written something here) and Ludwig Faddeev.

Comments

1. **Urs Schreiber**
   February 22, 2017

   The model of fuzzy dark matter, which the article by Collar refers to, with massive but ultralight axion particles of de Broglie wavelength the scale of galaxies is interesting, in that it interpolates between the extremely sucessful WIMP-style phenomenology on cosmological scale and the observed MOND-like deviation from WIMP-style dark matter on galactic scales, because on just those scales the quantum nature of these axionic particle kicks in to. That's the point amplified in much detail by Hui-Ostriker-Tremaine-Witten 16. And hence axion cosmology looks attractive, as witnessed for instance recently by the SMASH model.

   But if axions fit the data a question will remain: why axions, theoretically? As Svrcek-Witten 06 wrote way back:

   > An obvious question about the axion hypothesis is how natural it really is. Why introduce a global PQ "symmetry" if it is not actually a symmetry? What is the sense in constraining a theory so that the classical Lagrangian possesses a certain symmetry if the symmetry is actually anomalous? It could be argued that the best evidence that PQ "symmetries" are natural comes from string theory, which produces them without any contrivance.

Indeed, the funny coupling of the axion to the instanton number, which seems contrived from the point of QFT, generically drops out from string theory, as shown by entertaining little computations.

(Interestingly, it is the special properties of higher gauge fields in string theory which give the axionic behaviour in 4d: for type II its the K-theoretic nature of the RR-fields, and for HET its the Green-Schwarz mechanism on the B-field gerbe. Hence if Witten's arguments as above are right, then axionic dark matter is potentially the first direct phenomenological signature of higher gauge theory.)

2. **Peter Woit**
February 22, 2017

Urs,
I have no doubt that, if we ever find evidence for new physics that explains dark matter, no matter what that new physics is, we will be assured that it "generically drops out from string theory", just like the now-becoming-unpopular WIMPs did…

3. **Anon**
February 23, 2017

Regarding the new paper on SUSY, it says that the LHC(14) at 3000 inv fb will probe up to 2.8 TeV of gluino mass — that corresponds already to 10% fine tuning (\Delta_EW = 10) as per fig 1 in their paper (1702.06588).

Assuming nothing is found with full running of at LHC(14), the 33 TeV machine will search among models that are between 10% and 3% fine tuned (gluion mass between 2.8 and 5 TeV).

I guess this implies that, even now, LHC(14 Tev) is more likely to discover SUSY as it continues to run and search for gluinos between 1.9 TeV and 2.8 TeV which according to the authors is a region that is not 10% fine tuned. If the the LHC does not find anything in this region, the 33 TeV collider will be searching a region that is at least 10% fine tuned and is less likely to find SUSY, compared to the LHC runs between now and 3000 inv. fb.

4. **Balazs Vagvolgyi**
February 23, 2017

Dang, Scientific American and Juan Collar, inflation and strings just got burned!
In other news, the first designs for the Church of the Holy Superstring have just been submitted for review.

5. **Anonyrat**
February 24, 2017

Recent lectures on the conformal bootstrap by Simmons-Duffin
https://arxiv.org/pdf/1602.07982.pdf

6. **Mike Weiss**
February 24, 2017

"One major question," writes Natalie Wolchover in Quanta magazine, "is how black holes manage to preserve quantum information, even as Einstein's theory says they evaporate."

Surely the last clause is in error as classical black holes in GR do not evaporate. Hawking predicted black-hole evaporation in the 1970s based on an amalgam of quantum field theory and GR.

7. **Anon**
February 25, 2017

Another unfortunate recent death is that of Bertram Kostant. I know you Peter have been interested in his ideas on Lie algebra cohomology and the symplectic Dirac operator, as well as on geometric quantization more generally.

8. **Peter Woit**
February 25, 2017

Anon,

I'm sorry to hear that, thanks for letting us know. I'll write a posting very soon about Kostant's mathematics, which has had a lot of influence on me.

9. **anon**
February 26, 2017

Yet another death: Ludvig Faddeev has passed away.

10. **Peter Woit**
February 27, 2017

anon,

Thanks for letting us know. I'll add a mention above, unfortunately I never met Fadeev and am not familiar enough with his work to write much about it.

11. **Casual reader**
February 27, 2017

Observations of spatial flatness and large-scale homogeneity required very special initial conditions for the universe. Scientists did not like that, so they came up with inflation.

Planck CMB observations require very special initial conditions for inflation. Ijjas, Steinhardt and Loeb do not like that, so they come up with a bouncing universe.

12. **lars**
March 4, 2017

"Inflation"

The universe inflated
Is really overrated
Though lack of proof is stated
The hype is unabated
Perhaps it should be traded
But those who are quite jaded
Are almost surely fated
To wait with breath that's bated
For end to things inflated

13. **Doug McDonald**
March 4, 2017

"With results from the full 13 TeV dataset just a few weeks away"

I recall a discussion late last year about a skipped meeting then, mentioning this.

Do you recall dates and places of the upcoming (presumably non-) news?

I do admit that probable SUSY or even better axions would be fun.

14. **Peter Woit**
March 4, 2017

Doug McDonald,

As far as I know, the target has been to release these results at the Moriond conferences scheduled to begin March 19th (also at the Aspen conference to begin March 20)
Watch for results to appear at
http://moriond.in2p3.fr/
http://indico.cern.ch/event/550030/
I suppose there might also be events scheduled at CERN for these announcements.

Bertram Kostant 1928-2017

February 25, 2017
Categories: Obituaries, Uncategorized

I was sorry to just hear via a comment here about the recent death of Bert Kostant, at the age of 88. MIT has a story about him here.

Kostant was a major figure in the field of representation theory, and perhaps the leading one during the second half of the twentieth century among those with a serious interest in the relations between representation theory and quantum theory. These relations have for a long time now been a deep source of fascination to me, and Kostant's work has had a great impact on how I think about the subject.

I'll just list here some of his major papers that I've spent significant amounts of time with, characterized by a few major themes:

Borel-Weil-Bott, Lie algebra cohomology, BRST and Dirac cohomology

- Lie algebra cohomology and the generalized Borel-Weil theorem. This paper has had a huge influence. Some notes from my graduate course discussing this and Borel-Weil-Bott are here.
- Symplectic reduction, BRS cohomology and infinite-dimensional Clifford algebras (with Shlomo Sternberg). If you really want to understand the mathematics underlying the BRST method, this is the place to start.
- Dirac cohomology for the Dirac operator. For the context of this and a lot more about the subject, see the book by Huang and Pandzic.

Quantization of the dual of a Lie algebra, W-algebras

The dual of a Lie algebra is a Poisson manifold, and you can ask what happens when you quantize this. For semisimple Lie algebra, reduction with respect to the nilradical is an idea that Kostant pursued, with two examples the following two papers. Applied to loop groups, this is a central idea of the geometric Langlands program. The theory of W-algebras is also an outgrowth of this.

- On Whittaker vectors and representation theory.
- Quantization and representation theory, in the volume Representation theory of Lie groups.

Geometric quantization theory and co-adjoint orbits

Starting around 1970 Kostant did a great deal of work developing the theory of "geometric quantization" and the idea of quantizing co-adjoint orbits to get representations (other figures to mention in this context are Kirillov and Souriau). Some of his papers on this are:

- Orbits, symplectic structures and representation theory.
- Orbits and quantization theory. 1970 ICM talk.
- Quantization and unitary representations. In Lecture notes in Mathematics 170.

All of the three general themes above are closely intertwined, and the relations between them indicate that there is still a lot more to be understood about how quantum theory and representation theory are related, with Kostant's work undoubtedly playing a large role in developments to come.

Comments

1. **Prof. David A. Edwards**
   February 25, 2017

   The following was the first work by a mathematician on Super-Theory:

   Graded manifolds, graded Lie theory, and prequantization, Bertram Kostant

   https://link.springer.com/chapter/10.1007/BFb0087788

2. **Peter Woit**
   February 25, 2017

   David Edwards,

   Kostant was one of the early mathematicians working on this, but there were others (for one example, Berezin-Leites on supermanifolds), so I don't think claims about "first work" that will stir up arguments from others are a good idea.

3. **David Metzler**
   March 6, 2017

   As a graduate student at MIT from 1992 to 1997, I would have been surprised to know that Kostant had actually retired in 1993—he was around the department more than most of the full-time faculty. He clearly loved his work.

Reality is Not What It Seems

March 4, 2017
Categories: Uncategorized

This Sunday's New York Times has a rather hostile review by Lisa Randall of Carlo Rovelli's popular book Reality is Not What It Seems, which has recently come out in English in the US. Rovelli responds with a Facebook post. Another similar recent book by Rovelli got a much more positive review in the NYT, his Seven Short Lessons on Physics.

I haven't written about these two books mainly because I don't think I have anything interesting to say about either of them (although if someone had asked me to review one of them I might have tried to come up with something). They're aimed very much not at physicists but at a popular audience that doesn't know much about physics. From the parts I've read they seem to do a good job of writing for such an audience, and I noticed nothing that seemed to me either objectionable or particularly unusual. Rovelli's two slightly different angles on this topic are an interest in the ancient history of speculation about physics and a background in loop quantum gravity rather than HEP theory/string theory. Instead of wading into the controversy over string theory, he just ignores it and writes about what he finds interesting.

I'm not so sure why, since to me this seems harmless if not particularly compelling, but Randall strongly objects to Rovelli's attempts to draw connections between modern physics and classical philosophy:

> Wedging old ideas into new thinking is analogous to equating thousand-dollar couture adorned with beads and feathers and then marketed as "tribal fashion" to homespun clothing with true cultural and historical relevance. Ideas about relativity or gravity in ancient times weren't the same as Einstein's theory. Art (and science) are in the details. Either elementary matter is extended or it is not. The universe existed forever, or it had a beginning. Atoms of old aren't the atoms of today. Egg and flour are not a soufflé. Without the appropriate care, it all just collapses.

She's also quite critical of the way Rovelli handles the unavoidable problem of writing about a complicated technical subject for the public:

> The beauty of physics lies in its precise statements, and that is what is essential to convey. Many readers won't have the background required to distinguish fact from speculation. Words can turn equations into poetry, but elegant language shouldn't come at the expense of understanding. Rovelli isn't the first author guilty of such romanticizing, and I don't want to take him alone to task. But when deceptively fluid science writing permits misleading interpretations to seep in, I fear that the floodgates open to more dangerous misinformation.

Here I'm a bit mystified as to why she finds Rovelli any more objectionable than any other similar author (or maybe she doesn't, and he just happened to be the lucky one to have the first such book she was asked to review in the New York Times). As should be clear from this blog and book reviews that I've written, I agree with Randall about a problem that she leads off the review with:

> Compounding the author's challenge is the need to distinguish between speculation, ideas that might be verified in the future, and what is just fanciful thinking.

However, to me it seemed that Rovelli met this challenge better than many, far better than any of the huge popular literature about supersymmetry, string theory and the multiverse. She may be right that someone not paying careful attention could get the wrong idea from Rovelli about cosmological loop quantum gravity models. It's equally true though that readers of her own books about extra dimensions, dark matter and the dinosaurs might come away not understanding exactly what the strength of evidence was for those speculations.

Note added for clarification 3/6/2017: the following is not part of the commentary on Randall's review, it's another related topic I thought readers would find interesting. The relation between the two parts is that they both have to do with the question of distinguishing solid argument/speculation, but it's not about Randall, and the context is different (communicating with other physicists versus communicating with the general public}.

On this question of how/whether physicists (here mathematicians are very, very different) make clear what is a solid argument and what is just speculation, another interesting case is that of Nima Arkani-Hamed, who came to prominence in particle theory with Randall, both of them working on extra dimensional models. Both of them got a huge amount of attention for this, from the public and from within physics, although these ideas were always highly speculative and unlikely to work out.

There's a wonderful new "Storygram" by George Musser of a great profile by Natalie Wolchover of Arkani-Hamed. It's all well worth reading, but related to the topic at hand I was struck by the following:

> Arkani-Hamed considers his tendency to speculate a personal weakness. "This is not false modesty, it's really a personal weakness, but it's true, so there's nothing I can do about it," he said. "It's important for me while I'm working on something to be very ideological about it. And then, of course, it's also important after you are done to forget the ideology and move on to another one."

Arkani-Hamed is an incredibly compelling speaker, but his talks often have struck me as putting forward very strongly some particular speculative point of view, while ignoring some of the obvious serious problems. If you're not pretty well-informed on the subject, you might get misled… From the quote above he seems to have a fair amount of self-awareness about this. Also interesting in this context is his talk last year at Cornell on The Morality of Fundamental Physics. He gives an inspiring account of the intellectual value system of theoretical physics at its best. On the other hand, he pays no attention to the very real tension between that value system and the way people actually pursue their work, often very "ideologically". For particle theory in particular and the current situation it finds itself in, this seems to me an important issue for practitioners to be thinking about.

Comments

1. **Jeff M**
   March 4, 2017

   "Compounding the author's challenge is the need to distinguish between speculation, ideas that might be verified in the future, and what is just fanciful thinking." OK, coming from someone like Randall, that's just funny. I seem to remember, a long time ago, physicists did actually try to stick to things that might possibly be verified. Though I shouldn't taint all physicists with that brush, I'm guessing solid state folks and the like still do. I've read a little of Rovelli's pop science stuff, and it always seemed pretty good, and quite well grounded.

2. **Douglas Natelson**
   March 4, 2017

   Yeah, the author of a book proclaiming that dark matter likely did in the dinosaurs criticizing Rovelli for lack of clarity about what is speculation seems a bit rich to me.

3. **Peter Woit**
   March 4, 2017

Jeff M.,
Rovelli's specialty is quantum gravity, and in this new book he's trying to write about this topic. The problem with such a topic is that there's no experimental evidence, and no general agreement even on what's a successful theoretical approach. I can't think of any other field of science where scientists try and write books for the public about ideas that haven't gotten any empirical backing. Even in physics, this was not the sort of thing serious physicists did before the 1990s. The problem is that, not because they want to but by necessity, physicists are working this way and people want to hear about what they are doing.

One could make a serious argument that physicists shouldn't be writing books for the general public about speculation not backed by experimental evidence, on the grounds that it's just going to mislead people. Or, one could argue that when physicists do this, they should put huge warning labels on what they are doing to make sure no one gets misled. Given Randall's own books though, she's not in a good position to make that argument. I suspect she sees herself as having put enough caveats in her books, and feels that Rovelli is not doing this, but things tend to look very different if you're writing about your own favorite idea vs. reading someone else writing about their favorite idea (which you don't at all share).

4. **Paolo**
March 5, 2017

Scientists should write for the general public. The work of "popularization" of science is of extreme importance and its utility is not only limited to the pleasing of "armchair scientists": I am actually using the books by Carlo Rovelli as short inspiring sources to motivate and engage *graduate level* students in pure mathematics (that, in the country where I work, do not usually have any technical exposure to physics beyond an introductory 1-semester elementary course as beginning undergraduates). I am also using them, with very good results, as a background reading for a weekly discussion group on mathematical physics with (well-experienced) professional mathematicians.
Clear warnings should be placed when material that is speculative in nature is presented … although everything is quite relative here since, strictly speaking, for a "rigorous" mathematical physicists most of what is currently done in theoretical physics (including standard quantum gauge field theory on 4-dimensional Minkowski space) is still mathematically not-well-founded speculation (no matter its experimental confirmation) 😉
Scientists should write … and having critical reviews should be perfectly OK as well. The problem here is, as always (as clearly stated by M.Planck's famous quote, cited by T.Kuhn), … in the "socio-pathology" of the research environment, where (for reasons of career, funding, narcissism, etc) researchers are politically fighting for the supremacy of certain "fashionable trends" and cannot tolerate the existence of dissonant opinions. The fact that speculations based on work in string theory should be considered "more scientifically founded" and "precise" compared to similar speculations based on work on loop quantum gravity (or other even less fashionable trends) seems an example.

5. **Peter Morgan**
March 5, 2017

Against your Randall quote (because she's right that one always has to pick at details), there is a beauty in the "precise statements" of physics, but to me it is a mathematical beauty. The precise mathematics of our physics is always tenuously related to the experimental data that we hope to take as the ground for our physics when we consider it in its finest details. The beauty of the human endeavor of *doing* physics is to me in searching relentlessly for formal models that are slightly less tenuously related to the experimental data and in recognizing the valor of those who constructed the very good models we already have. In physics I think of progress coming from our reimagining any or all of the mathematics, the experimental data, and the relationship between them; I take the ground of Not Even Wrong to be discussing how any of these three can fail.

6. **piscator**
March 5, 2017

Can't share your enthusiasm for that Storygram. The original piece by Wolchover had all the journalistic scepticism and critical analysis of a Profile of the Dear Leader, and Musser's piece has the same vibe.

Of course Nima is highly charismatic and fun to listen to, but physics is about results, and one would expect journalists to engage critically with the question about what Nima has really done that will still be around in thirty years.

7. **Peter Woit**
March 5, 2017

piscator,
I sympathize with your point, and I hope at some point we'll see a more critical analysis. But I think this is more of a failing of the HEP theory community than one of journalists, who aren't experts, and aren't equipped to provide such a critical analysis if there's no source for it among experts.

8. **Shantanu**
March 6, 2017

Peter , something OT but check out Stacy's blog article which discusses Dave Merritt's article
showing how the goal posts in supersymmetric dark matter have moved.
https://tritonstation.wordpress.com/2017/03/06/lcdm-has-met-the-enemy-and-it-is-itself/

9. **Thomas**
March 6, 2017

I read (well, listened to) Rovelli's book and I tend to agree with Randall's criticism of the book. I was actually rather annoyed with the whole thing and started to skip long sections. Rovelli puts way too much stock in the idea that he is traveling along a path laid out by the Greek atomists, he provides rather misleading explanations of conventional physics, and makes no attempt to explain what the difficulties with the loop quantum gravity approach are.

In this country we like no sin better than hypocrisy, and it is easy to say that this is an odd criticism from somebody who just published a book on dark matter and the dinosaurs (which was reviewed mostly favorably in the pages of the NYT). Indeed, Randall faces some of the same problems in her book. Putting a disclaimer in every other sentences would have made the book unreadable, and begs the question why it was written in the first place.

I think there is an important difference, however. By staying pretty close to how scientists actually work Randall makes it clear that this is a book about the journey, not the final answer. By focusing on the poetic connections between himself, Aristotle, and the nature of the time, Rovelli ends up writing a book that is just a little too much.

10. **Bernhard**
March 6, 2017

Thomas,

I haven't read Rovelli's book yet, but I read Randall's…. Of all people, is SHE really in position to make this kind of criticism? Chutzpah comes to mind.

11. **neil**
March 6, 2017

I just finished Rovelli's new book. I liked it a lot. Basically, it is LQG for dummies, but I appreciated Rovelli's "fluid science writing" and clear exposition of LQG. Yes, the book is a "sell job" for this approach to quantum gravity, but so what? Rovelli does not try to disguise that fact. I would defend the book against some of Randall's criticisms. I encountered no place where I found Rovelli misleading about what we actually know and what is speculation. He admits in his chapter on potential empirical confirmation that the evidence is not yet in and that "we will have to wait and see." What more should he do? I thought he made a telling point when he says that LQG does not need extra dimensions, new symmetries or new particles, just good old fashioned GR, QFT and the SM. And yes, Rovelli is a bit excessive with his sweeping connections to debates in Greek philosophy about whether the world is continuous or granular but, hey, the man is writing a book about the granularity of spacetime. I'll grant him some literary license.

12. **jimmy**
March 7, 2017

neil,

Did Rovelli mention any of the numerous arguments against LQG?

13. **martibal**
March 7, 2017

"Either elementary matter is extended or it is not. The universe existed forever, or it had a beginning. Atoms of old aren't the atoms of today."

Could add: either an electron is a particle or a field….
I am not so sure physics is working that way 🙂

14. **neil**
March 7, 2017

jimmy,

He does not. His exposition is broad and simplified and, as I said a "sell job", so he does not discuss problematic issues like computation and self-consistency checks, for example. He claims you can compute physical phenomena by "summing over spin-foams", but he doesn't give a concrete example. On issues like deriving the classical GR limit and testibility he is simply optimistic things will work out in the future. The potential tests of LQG he mentions are primarily cosmological (he expects a lot from LISA.)

As a non-expert, I was content to get a coherent explanation of how LQG is structured.

15. **Narad**
March 7, 2017

> Art (and science) are in the details. Either elementary matter is extended or it is not.

I'm pretty sure that's *ontology*, rather than either art or science.

16. **Curious Wavefunction**
March 9, 2017

I did read Rovelli's book and I don't think it's supposed to be a solid introduction to loop quantum gravity for the layman. Rovelli's style is very impressionistic, and while this works well in his previous book it doesn't really do the job in this one. At the very least, you would have to build a substantial edifice of physics to launch into a discussion of loop quantum gravity. This Rovelli does not do I think, and others seem to have done it much better. Whatever your opinion of Brian Greene's books for instance, at least he does a very good job of laying out the basic physics (QM, relativity etc.) in his books.

17. **Richard Gaylord**
March 11, 2017

I don't expect you to publish this since you seem to automatically delete any comment i make on any of your columns without any explanation to me as to why (which is your perogative since it's your blog) but the review of Rovelli's book in the Washington Post (http://www.washingtonindependentreviewofbooks.com/bookreview/reality-is-not-what-it-seems-the-journey-to-quantum-gravity) is worth reading.

18. **Richard J. Gaylord**
March 18, 2017

my comment was finally published on 3/11, long after i submitted on 3/4. Rovelli appears to have followed my suggestion in that comment (which you edited out of my comment) that he respond to Randall's review somewhere other than just on his Facebook page and he has just published a condensed version of his reply in tomorrow's NYT Sunday Book Review https://www.nytimes.com/2017/03/16/books/review/letters-to-the-editor.html?emc=edit_bk_20170317&nl=book-review&nl_art=&nlid=77068585&ref=headline&te=1

Can the Laws of Physics be Unified?

March 7, 2017
Categories: Book Reviews, Multiverse Mania

There's a new book out this week from Princeton University Press, Paul Langacker's Can the Laws of Physics Be Unified? (surely this is a mistake, but there's also an ISBN number for a 2020 volume with the same name by Tony Zee). It's part of a Princeton Frontiers in Physics series, in which all the books have titles that are questions. The other volumes all ask "How…" or "What…" questions, but the question of this volume is of a different nature, and unfortunately the book unintentionally gives the answer you would expect from Hinchliffe's rule or Betteridge's law.

This is not really a popular book, rather is accurately described by the author as "colloquium-level". Lots of equations, but not much detail explaining exactly what they mean, for that some background is needed. The first two-thirds of the book is a very good summary of the Standard Model. For more details, Langacker has a textbook, The Standard Model and Beyond, which will have a second edition coming out later this year.

The last third of the book consists of two chapters addressing the question of the title, beginning with "What Don't We Know?". Here the questions are pretty much the usual suspects:

- Why the SM spectrum, with its masses and mixing angles?
- The hierarchy problem.
- The strong CP problem.
- Quantum gravity.
- Problems rooted in the cosmological model: Baryogenesis, dark matter, dark energy and the CC,

In addition, there are problems listed that are only problems if you philosophically think that a good unified model should be more generic than the SM, leading you to ask: why no FCNC? why no EDM?, why no proton decay?

The last chapter "How will we find out?" lists the usual suspects for ideas about BSM physics: SUSY, compositeness, extra dimensions, hidden sectors, GUTS, string theory. We are told that this is a list of "many promising ideas". While in general I wouldn't argue with most of the claims of the book, here I think the author is spouting utter nonsense. The ideas he describes are ancient, many going back 40 years. In many cases they weren't promising to begin with, introducing a large and complex set of new degrees of freedom without explaining much at all about the SM. Decades of hard work by theorists and experimentalists have not been kind to these ideas. No compelling theoretical models have emerged, and experimental results have been strongly negative, with the LHC putting a large number of nails into the coffins of these ideas. They're not "promising", they're dead.

Langacker does repeatedly point out the problems such ideas have run into, but instead of leaving it at "we don't know", he unfortunately keeps bringing up as answer "the multiverse did it". On page 151 we're told the most plausible explanation for the CC is "the multiverse did it", on page 160-163 we're given "multiverse did it" anthropic explanations for interaction strengths, fermion masses, the Higgs VEV, and the CC. Pages 167-173 are a long argument for "the multiverse did it". The problem that this isn't science because it is untestable is dismissed with the argument that it "may well be correct", and maybe somebody someday will figure out a test. On page 203 we're told that string theory provides the landscape of vacua necessary to show that "the multiverse did it".

The treatment of string theory has all of the usual problems: we're assured that string theory is "conceptually simple", despite no one knowing what the theory really is. The only problem is that of the "technical details" of constructing realistic vacua. I won't go on about this, I once wrote a whole book…

In the end, while Langacker expresses the hope that "sometime in the next 10, 50, or 100 years" we will see a successful fully unified theory, there's nothing in the book that provides any reason for such a hope. There is a lot that argues against such a hope, in particular a lot of argument in favor of giving up and signing up for a multiverse pseudo-scientific endpoint for the field. I suspect the author himself doesn't realize how much the argument of the book is stacked against his expressed hope and in favor of a negative answer to the title's question.

Update: If you just can't get enough multiverse mania, you can watch Joseph Silk's talk Should We Trust a Theory? (more talk materials here). I'm not quite sure, but I think we agree that the multiverse is not currently science (he writes "The multiverse might or might not exist, but no physicist should waste his or her time chasing the unchaseable"), but not about about string theory. I have no idea what is behind his claim that "String theory has been very successful", and, since he's not a string theorist, I suspect that neither does he.

Comments

1. **Jeff M**
   March 7, 2017

   I hesitate to say this on a physics blog, but maybe the answer actually is "no."

2. **Peter Woit**
   March 7, 2017

   Jeff M,
   Well, the answer is certainly "no" if the way you approach the question is by refusing to give up on ideas that have failed, and devote your research program to the construction of pseudo-scientific justifications for why your failed idea really is right, just inherently untestable.

   Personally I don't see any reason why one day we shouldn't some day better understand the relation of space-time symmetries and internal symmetries, and thus "unify" gravitational and other forces. Maybe this is possible, maybe not, one can't know in advance. But if you decide you have an argument for impossibility, that's the end of it, and that's the existential danger for the field of "the multiverse did it".

   There's the saying in math, "theorems are proved by those who believe in them", same goes for physics: progress towards unification will come from someone who thinks it is possible, not from someone who has become convinced by "the multiverse did it" argument. Having such people write books that convince others to give up hope is even more damaging…

3. **Bee**
   March 8, 2017

   The points you list are very different in the kind of problem they pose. It might well be there isn't any explanation for the mixing matrices or masses of particles. Quantum gravity is, however, a problem of an entirely different kind – it's needed for consistency. And even if it wasn't for quantum gravity, the SM would still have a Landau pole and what physical sense does this make?

Unfortunately most of the work on unification has focused on problems that I don't think are problems at all. Eg, yes, maybe U(1)xSU(2)xSU(3) isn't pretty, not to mention chirality, but if that's the way nature works then that's that.

The multiverse is merely a conflated way to say that they're trying to answer questions that aren't scientific to begin with, it's beyond me how this has become acceptable.

4. **Mateus Araújo**
March 8, 2017

But what if it is true? I mean, there is no question that the multiverse argument is lame, but Nature has no obligation to be interesting. Or testable, for that matter.

5. **clayton**
March 8, 2017

I thought a good "understand[ing of] the relation of space-time symmetries and internal symmetries" is the reason physicists cared so much about supersymmetry — this is the only nontrivial symmetry allowed by the Coleman-Mandula theorem. Is there another possibility?

6. **chris**
March 8, 2017

Bee,

please tell me one point of experimental evidence that gravity needs to be quantized. I know of none whatsoever. And the Landau pole is a perturbative concept. Nobody yet knows how the nonperturbative RG running of QED looks.

I think our host is quite right to put all these in the same basket.

7. **Jeff M**
March 8, 2017

Peter,

I agree completely, I think I wasn't clear. Multiverse is just silly. One cannot of course eliminate it as a possibility, but that's the point, one can't do anything scientific with it. You're right if any progress is made, it will be by someone who believes it can be done, not with the multiverse. I just don't know that I think there is progress to be made (though of course I'm a mathematician, so what do I know about force unification?) I must say though I'm kind of with Chris on this, who says gravity has to be quantized? Probably just the math, but GR is truly beautiful. The SM, not so much.

8. **vmarko**
March 8, 2017

I'm surprised with the number of questions in the comments above, it's not so usual for Peter's blog. With Peter's permission, maybe I can help a little:

Mateus Araújo,

Of course nature has no obligation to be interesting, or testable. Or even self-consistent, if you ask me. But our mathematical models of nature do have that obligation. There are in general many different mathematical models which can describe nature equally successfully (infinitely many models, finite amount of experimental data). Among all of those models, we are looking for those that are self-consistent, testable and interesting. Otherwise they are useless to us. So it's a self-imposed restriction, for a good reason.

clayton,

Yes, SUSY is not the only way to circumvent the Coleman-Mandula theorem, there are others, though apparently not so popular. For example, the approach based on 2-categories (and higher), see for example 1003.4485.

chris and Jeff M,

Quantizing gravity is a requirement of self-consistency, since quantum mechanics does not tolerate being combined with anything classical. This has nothing to do with experiment, but with the axiomatic structure of the resulting theory. Namely, if one attempts to combine QM with classical GR (or classical electrodynamics, or anything else classical), one is lead to a clear-cut mathematical contradiction (violation of the superposition principle). So we either give up QM as we know it, or we give up GR as we know it. Something has to give, and of course research is being done in both directions.

HTH, 🙂
Marko

9. **Peter Woit**
March 8, 2017

Mateus Araujo,
The problem is that you need some kind of serious argument, just saying "my complicated, ugly model doesn't predict anything, so maybe the multiverse did it, let's give up" isn't one.

clayton,
Standard supersymmetric models don't unify internal and space-time symmetries. They extend the Poincare group by new spin-1/2 generators that commute with internal symmetries. This is why SUSY tells you nothing about the SM.

Bee/Chris/Jeff M.,
My take on the quantum gravity problem is that GR and the SM are based on very closely related mathematical structures: a matter particle is coupled to gravity by a connection and to the SM forces by connections. Our problem is that we don't understand the relation between these two kinds of connections. You can just say "there is none", maybe that's right. If you want to claim "let's quantize the SM connection, not the GR connection", besides consistency issues to worry about, you really should have some argument why you are treating these in a completely different fashion, other than the fact that applying a standard quantization recipe to the GR connection leads to problems.

10. **Mateus Araújo**
March 8, 2017

vmarko,

How could Nature avoid being self-consistent? I find that literally inconceivable. As for the rest of your argument, I think you have a point. It unfortunately draws a line between what Nature is and what we can say about Nature, but I guess there is no way around it.

One should, however, not be too strict about testability, otherwise one is led to ridiculous positions such as saying that we don't know whether Galileo's remains are still inside Jupiter.

chris and Jeff M,

You can't just say the gravity is classical and you don't need to quantize it. You need to actually formulate how a "non-quantum gravity" acts on atoms. The most straightforward way is to do [fails miserably](http://journals.aps.org/prl/abstract/10.1103/PhysRevLett.47.979).

http://journals.aps.org/prl/abstract/10.1103/PhysRevLett.47.979

11. **clayton**
March 8, 2017

Thanks for the replies to my question, vmarko and Peter! But, to respond to Peter, I thought that Coleman-Mandula provided a proof that there is no nontrivial product of spacetime and internal symmetries. They went on to make the (incorrect) claim there were no additional symmetries beyond the product of Poincare with internal symmetries; but this (incorrect) latter statement was, I thought, logically independent of the rest of the proof. Is this an accurate characterization? By the way, I am also confused with how the BMS "infinite dimensional" symmetry works in this respect, so my confusion is quite broad 🙂

12. **Peter Woit**
March 8, 2017

clayton,
Yes, Coleman-Mandula is a real problem, it means that there has to be a more subtle relation between internal and space-time symmetry groups than just naively finding a larger group they both fit into and making that act in a conventional way. The BMS story is a really interesting one, I haven't thought about the relation to Coleman-Mandula, but there (BMS) the group is acting non-trivially on the vacuum, which may be one reason Coleman-Mandula isn't applicable.

If you look at the history of "no-go" theorems, I think you'll find that typically sooner or later someone realizes that there is a way around one of their assumptions, so one should be very careful about drawing a conclusion from the "theorem" without paying very close attention to exactly what its assumptions are.

13. **Kurt**
March 8, 2017

It seems that the correct title of the book should have been "Did Our and My Ideas so far Allow the Laws of Physics to be Unified?" Here the answer is of course "no".

A honest summary would have clarified that the ideas in the last chapter all failed, and that a unifying ideas must differ from them.

A second honest summary would have added: Yes, the laws can be unified, because we can talk about *all of nature*. Therefore we can also talk *with precision* about all of nature. And therefore we must be able to unifiy all known laws.

It is obvious that unification is possible. But how to achieve it is not.

Whoever says the opposite is spreading disinformation.

14. **clayton**
March 8, 2017

thanks for the clear answer, Peter!

15. **Peter**
March 8, 2017

Bee, Mateus – I think the fact that the 'multiverse' argument is dressed up as a statement about reality makes one think that 'a multiverse' is a real thing, a single object which represents reality.

Given a system of (mathematical) axioms, one can deduce some statements; some statements have a truth value which is forced from the axioms (and these are not the same!). Other statements (if your axiom system is interesting) do not; they are independent. Some of these other statements have interrelations; if you fix one to true another is then forced to be also true, et cetera.

A model of a system of axioms is then an assignment of truth values to statements which is consistent (or at least is inconsistent only if the axioms themselves are inconsistent).

For example, one can take a standard set theory (ZFC say) and ask whether there exists a set of cardinality between the naturals and the reals. For ZFC this turns out to be one of those independent statements (the Continuum Hypothesis, CH, is the negative assertion); it doesn't have a defined truth value.

One can construct models of ZFC in which CH is true, others in which it's false, in either case asking 'why is it true/false' doesn't have a reasonable answer (unless you weren't looking at CH when you made your construction, anyway). It's that way because you set it that way.

Of course, one can consider the class of all models, or 'multiverse'. Then you answer 'why is CH true/false' with 'because we are in that bit of the multiverse'; same logic dressed up differently.

Now, if you take an axiom system which you think describes reality, it will have (for the same reason) many different models. These are not logically related, they cannot 'interact' because there is nothing to 'interact' with. This is the (Platonic mathematical) multiverse which is (sometimes) being used in place of an explanation in physics.

Another possibility is that our current view of `the universe' is one among a collection of interacting structures; in other words, that our current idea of 'the universe' sees only a slice through some more intricate structure of reality. That rather hypothetical 'more intricate structure' also gets called a multiverse, whereas if one posits it, what one actually asserts is a more complicated universe of which we currently (as far as we know) witness only a slice. It should be clear that two different slices through a single mathematical structure (which is a single model of an axiom system) potentially do interact and in any case are not the same thing as two different models of an axiom system. But since the same word is used for both, the two rather different concepts have been conflated.

And finally, what our host is objecting to, in these terms, is prematurely classifying statements as independent of the axiom system when we do not know this (especially since we presumably do not know the full axiom system). Incidentally, it's worth noting that an independence statement is never itself independent (one cannot logically criticise our host for asking for certainty on this point). A statement S is independent of an axiom system A if there are models of A in which S is true and others in which it is false; letting T be 'S is independent of A', if T can be true then these models exist and hence T is definitely true (though it need not have a proof in A, or indeed in any given bounded axiom system if A is reasonably complicated, so our host might not ever know why the multiverse did it).

16. **Peter Woit**
March 8, 2017

    Peter,

    The serious questions here are questions of what science is, what reliable scientific knowledge is, and how to pursue it. I don't think the kind of framework of axiom systems and models that you discuss captures the issues at all (and it has nothing at all to do with the topic of this posting).

17. **Johannes**
March 10, 2017

    Peter,

    do you believe that the laws of nature can be unified – and why?

    What is your experience: how many of the researchers in the field believe in a positive, and how many in a negative answer?

18. **Peter Woit**
March 10, 2017

    Johannes,

    Yes, in the sense that I think there is a better theory out there that we don't yet know about, which would for instance tell us something deep about the relation of space-time and internal symmetries. Part of my reason for believing this is that there are all sorts of intriguing connections between the mathematical structures involved. This does not look like the kind of situation where one understands things well enough to know why you're not going get some deep relation, it looks a lot more like a situation where something important is going on that one doesn't understand. To get some idea of the general thing I'm talking about, see my essay
    http://www.math.columbia.edu/~woit/mathphys.pdf
    but there are also much more specific ideas that I'm hoping to get to writing about after I finish work on my book (any day now…).

    I'd assume people who work on trying to find a deeper understanding of the Standard Model do so because they believe there is something there. Undoubtedly there are lots who, faced with ideas they devoted a lot of time to not working, have given up. Some go and work on other problems, a few unfortunately write books aimed at getting other people to give up too…

19. **James Cooper**
March 10, 2017

    I am not a mathemetician or a physicist or even very intelligent but I have 3 points to make

    1) It seems obvious Peter is so right on his points, especially in how much effort has been wasted in failed directions and there are directions in math that haven't been fleshed out fully yet. It is absurd to "give in" so early with this multiverse nonsense when there are things we don't even know about number theory for instance

    2) I enjoyed looking at Peter's PDF. It seems he is setting himself up to be the "I told you so" laureate when in 20 years a new Einstein comes along and inevitably shows us that the universe is just math and couldnt be any other way.:) Penrose seems to do this explicitly in his book Road To Reality

    3) Peter talks about number theory in his PDF but doesn't specifically mention the Reimann zeta function. I'm wondering if you have/have had any general thoughts on it? Again, I'm not a mathemetician, but I am fascinated by the fact that there might actually be something unrandom in prime numbers. It seems to me if there is anything fundamenal about the nature of reality it might be found in that

20. **Peter Woit**
March 10, 2017

    James Cooper,

    Thanks, you're quite right that I hope that someone smarter then me shows my prejudices about this topic to be right. Of course it's a lot easier to have the right prejudice than to successfully justify it.

    I don't have any serious ideas about the Riemann hypothesis. If we ever know why it is true, that will surely tell us something deep about number theory which very well may elucidate its connection to quantum theory (and maybe a new deep idea about quantum theory will explain why the Riemann hypothesis is true). Actually yesterday in my Fourier analysis class I went through the proof of the functional equation for the zeta function, based on using Poisson summation to first prove the functional equation for the theta function. This morning it struck me that I have no idea if there's a version of the Riemann hypothesis formulated directly as a statement about the theta function, would love to hear from anyone who can point to that.

21. **Jeff M**
March 10, 2017

    Peter,

    I cannot for the life of me remember if there's a version of the RH for the theta function, it was too long ago when I was in grad school thinking I wanted to be an analytic number theorist. I do remember there are versions of the RH for the Dirichlet L functions, the generalized RH says that the zeros for any L function have real part 1/2. L functions are just zeta but with a multiplicative character as the numerator.

22. **Peter Woit**
March 10, 2017

    Jeff M,
    A few minutes of Googling have turned up a version of this question on mathoverflow

    http://mathoverflow.net/questions/14083/modular-forms-and-the-riemann-hypothesis

I don’t see much of a satisfactory answer there though…

23. **jd**
March 10, 2017

Paper on arXiv, 1608.03679, on RH, to be published in PRL.

24. **Peter Woit**
March 10, 2017

jd and others interested in proofs of the RH,

Please, very low on the desirability list of things to do over my spring break is moderate a discussion of people’s ideas about how to prove the RH. If you have a pointer for where someone discusses what the RH says about the theta function, I would like to see that.

25. **Jeff M**
March 13, 2017

Peter,

If you’re really curious, the person to ask is Peter Sarnak, at Princeton/IAS. I assume several people at Columbia know him well. Met him at UCLA when I met you, he was a few years out of grad school then and the hot new analyst, student of Paul Cohen at Stanford. Right after that he was around the GC a lot while I was working on my thesis. FYI, as far as proofs for the RH go, i wouldn’t hold my breath….

This Time It's For Real

March 10, 2017
Categories: Quantum Theory: The Book

Several months ago I was advertising a "Final draft version" of the book I've been working on forever. A month or two after that though, I realized that I could do a more careful job with some of the quantum field theory material, bringing it in line with some standard rigorous treatments (this is all free quantum fields). So, I've been working on that for the past few months, today finally got to the end of the process of revising and improving things. My spring break starts today, and I'll be spending most of it in LA and Death Valley on vacation, blogging should be light to non-existent.

Another big improvement is that there are now some very well executed illustrations, the product of work in TikZ by Ben Dribus.

I'm quite happy with how much of the book has turned out, and would like to think that it contains a significant amount of material not readily available elsewhere, as well as a more coherent picture of the subject and its relationship to mathematics than usual. By the way, while finishing work on the chapter about quantization of relativistic scalar fields, I noticed that Jacques Distler has a very nice new discussion on his blog of the single-particle theory.

There's a chance I might still make some more last-minute changes/additions, but the current version has no mistakes I'm aware of. Any suggestions for improvements/corrections are very welcome. Springer will be publishing the book at some point, but something like the current version available now will always remain available on my website.

Update: After writing to someone to answer a question and what is and isn't in the book, and other things to read, I thought maybe I'd post here part of that answer:

> For the main topics about QM and representation theory that I cover in the book, I don't know of a better reference, even assuming an excellent math background. That's one of the main reasons I wrote the thing… The problem with other books on QM for mathematicians (e.g. Hall, which is very good on the analysis point of view) is that they don't do much from the representation theory point of view. Weyl's book was written very early, when a lot of what was going on was still unclear, I don't think it's a very good place to try and learn this material from. One topic that is in there that I don't cover at all is basically Schur-Weyl duality, but even for that arguably Weyl is not a good place to learn that theory.
>
> One thing to keep in mind about my book is that the early chapters are deceptive. I wanted to start out with very simple things, make the simplest examples accessible to as many people as possible, mathematicians or physicists. If you know basic facts about Lie groups, Lie algebras, finite dim unitary representations, Fourier analysis and how to use it to solve e.g. the heat equation, then the first quarter of the book is only going to be of interest in telling you about some applications of math you know. Mathematicians generally should be learning the basic rep theory elsewhere (lots of good books on these topics, and the main reason I'm doing many things in a mathematically sketchy way is that doing them in full would take too long, and has been done better elsewhere). In early chapters, all I'm really doing is working out very special cases of Lie groups/algebras that are rank one or products of rank one, and the irreps of sl(2,C). I never touch higher rank or general semisimple theory (would argue this actually doesn't get used much in physics, other than some simple SU(3) examples).
>
> Around chapter 12 though, things get much more non-trivial. From a high mathematical level, a lot of what's going on in the middle part of the book is the representation theory of the Heisenberg group (over R and C) and the implications of the action by the symplectic group by automorphisms (e.g. the metaplectic representation). This is done in a very detailed and concrete way, together with the relation to QM, although for some of the trickier parts of the mathematics (especially the analysis, e.g. the proof of the Stone-von Neumann theorem) I just give references. This is followed by discussing Clifford algebras, the orthogonal group and spinors (over R and C), in a very parallel way (interchanging symmetric and antisymmetric). I wish I knew of a good pure mathematics source for this material aimed at students, stripped of the quantum mechanics apparatus, but I don't. It (as well as material about reps of the Euclidean groups) is not covered in any conventional rep theory textbook I'm aware of.
>
> Much of the last third of the book, on quantum field theory, I think is just inherently quite challenging, for either mathematicians or physicists. From the representation theory point of view, the basic framework is that of an infinite dimensional Heisenberg group or Clifford algebra, but this is a difficult mathematical subject, and I think the physics point of view helps make clear why. For this stuff the rigorous treatments are quite specialized, I try and do some justice to what the main issues are and give references that provide the details.

Comments

1. **CIP**
   March 10, 2017

   I looked at a couple of sections dealing with issues that I've previously found difficult and was impressed by your directness and clarity. Looks like a winner to me!

2. **RP**
   March 10, 2017

   I think the book looks very, very impressive. I look forward to delving into it more deeply!

   One typographical pet peeve of mine, which I also notice in the current draft: 'digressions' and 'examples' are printed in cursive. This is distracting to me, since I (and most readers I think) tend to interpret cursive text as a form of emphasis. This is certainly appropriate for theorems and propositions, and perhaps for definitions, but clearly not for something like a digression.

   Anyway, many congratulations on your achievement!

3. **G. S.**
   March 10, 2017

   I'm not exactly sure how copyright works, but how can something be copyrighted 2016 but dated 2017? I get that portions of the document were previously released and copyrighted, but wouldn't the addition of new material extend the copyright of the document as a whole?

   Not sure if you want to get into a discussion of copyright law right before your trip to Death Valley, but I was curious. The book looks interesting and I'm looking forward to reading it. Thank you for posting it. Enjoy your break.

4. **Anonyrat**
   March 10, 2017

   Page 4: After calculating a final result, insert appropriate factors of ℏ can be inserted to get answers in more conventional unit systems

5. **Anonymous**
March 11, 2017

I'm really happy for you. What a magnificent piece of work.
I have one little nitpick about the usage of \left and \right.
This image explains it — http://1.1m.yt/S2j8hi.png
Not really an issue, because both have the same meaning. But I think you will agree the usage of \left(and \right) are prettier.

Congratulations.
This book is a milestone.

6. **tommaso dorigo**
March 11, 2017

Congratulations from me too Peter! This is a great work and I'm very happy you finally pulled it off!

7. **a reader**
March 11, 2017

Congratulations !
And thank you for making the final draft freely available.

8. **vmarko**
March 11, 2017

This kind of book has been missing in the landscape of QM textbooks. I've already recommended it to a few mathematicians who wanted to learn QM efficiently. Great work, congratulations!

🙂
Marko

9. **Denton Dailey**
March 11, 2017

Just started giving it a quick read. Found this on page four:

"After calculating a final result, insert appropriate factors of (h-bar) can be inserted to get answers in more conventional unit systems."

I've written a few EE texts and I know it's nearly impossible to catch all of these minor things no matter how many times you read it yourself. Will note any other issues I happen to spot, but it looks great overall.

10. **DRLunsford**
March 11, 2017

I like it. When I get the hard copy it will sit next to Weyl 🙂

-drl

11. **Anonymous**
March 11, 2017

I was curious to see the Tikz pictures and noticed one minor error in one of the captions. The caption of Figure 8.2 says "Cylindrical Coordinates". This should be "Spherical Coordinates". I also agree with the comment about the (lack of) use of \left and \right, especially when taking Lie brackets of items which involve larger delimiters, it looks much better if the outside ones are at least the same size as the inside ones. My guess is that an editor from Springer would make those changes anyway.

12. **Richard Ferguson**
March 11, 2017

Great news, Peter! Your book looks great, nothing really to nitpick on the presentation of the material. It's a very interesting and novel way to go about introducing QFT.

By the way, now that you're finished the book, do you have any research projects in Langlands or such that you're working on? Any plans for publishing papers? I would definitely be interested in reading them.

13. **DRLunsford**
March 12, 2017

Peter, don't know if you've ever been, but the journey up to Palomar Observatory from LA south toward San Diego is a great drive with a better destination. It's a strange feeling to be sitting there in the presence of the building surrounded by scrub and forest, but mindful of the Virgo Cluster..

-drl

14. **Peter Woit**
March 12, 2017

Thanks to all who have pointed out typos/typographical improvements. I've started fixing/implementing them, in particular glad to have pointed out the issue about delimiters, where I'm for now fixing the most egregious cases.

Any help of this kind is greatly appreciated, you can send me comments here or by email. If you're up for the more advanced topics of the book, you're encouraged to start looking at those, since I'm getting a lot more help with the early chapters than with the later ones. It's expected that many readers will get lost before they get to the end, but I hope some will make it all the way…

15. **Peter Woit**

March 12, 2017

DRLunsford,
I made a trip up to Palomar once, quite a long time ago and it was impressive. This trip may include a stop by Anza Borrego, which supposedly is in the midst of an unusual wildflower bloom. Palomar is nearby, maybe another visit there is indicated.

Richard Ferguson,
Definitely several research projects I now want to get to work on. The first is something I've wondered about for years, but working on this book has finally given me what looks like a way to finally get somewhere with it. We'll see soon. Another project is to finish something I started writing about Dirac cohomology and BRST. I will try and get something written and done about both of these first, since I now in principle have the background I need for them. For anything about number theory, first I have much more to learn…

16. **Martin**
March 14, 2017

With respect to paragraph 3.4 an interesting reference would be the Jaynes-Cummings model, dating from 1963, which features a two-level atom in a cavity and interacting with a single bosonic radiation mode.

17. **theoreticalminimum**
March 14, 2017

Peter,
Thanks very much for sharing a finalised version! I sure hope to find the time to read through, and learn.
In November last year, I noticed a book by Hayashi titled "Group Representation for Quantum Theory" published by Springer. Did you know about it, and would you recommend it?

18. **Cobi**
March 14, 2017

On page 573 you write that "The Hamiltonian with Einstein's equations as equations of
motion is however not of the Yang-Mills form. Applying standard perturbation
theory and renormalization methods to this Hamiltonian leads to problems with
defining the theory (it is not asymptotically free)".
Two things struck me about this and i hope you can clarify.
First of all the notes by Schwartz (http://isites.harvard.edu/fs/docs/icb.topic521209.files/QFT-Schwartz.pdf, p. 245) explain that general relativity leads to a perfectly sensible quantum field theory, at least as sensible as the standard model without gr.
On the other hand, even without coupling to general relativity the standard model is not asymptotically free due to the U(1) factor.
Thus "it is not asymptotically free" can not be the crucial obstruction.

19. **Peter Woit**
March 15, 2017

Cobi,
The topics you bring up have nothing at all to do with what I wrote, which is just pointing out that the pure Einstein theory Hamiltonian has a very different form than the pure Yang-Mills Hamiltonian, not sharing its good UV properties.

If you're looking for the usual tedious debates about quantum gravity, you really need to look elsewhere, that's not what this book is about, it's intent is to explain some very different things.

20. **R. Rosenfelder**
March 16, 2017

Dear Peter Woit,

I was surprised that you do not mention the Maslov "correction" when treating the time-dependent harmonic-oscillator propagator, i. e. the phase, which arises when the particle goes through a focal point.

Thus your eq. (23.12) in chapter 23 on page 271 is only valid for T < omega/pi; otherwise the propagator gets an additional phase.

This is covered in the standard textbook by H. Kleinert: "Path Itegrals in…", 3rd edition, p. 100 or in arXiv:1209.1315. Since the latter source may be not easily accessible to you, I recommend P. A. Horvathy's paper in quant-ph/0702236 as a detailed treatment of this caustic phenomenon.

Best regards

Roland Rosenfelder

21. **Peter Orland**
March 16, 2017

Hi Peter,

A very good beginner's treatment of the Maslov index from conjugate points is in Larry Schulman's book on path integrals (from 1981). He also has a nice elementary discussion of how a LEAST-action/arc-length principlea become an EXTREMAL principle, after passing a conjugate point.

You have plenty of other topics treated already, so this is something you could mention, without reviewing it.

22. **Peter Orland**
March 16, 2017

Hi Peter,

I just took a look at your discussion. I think you can forget about the Maslov index.

The result (23.12) is exact (the Maslov index is hidden in the answer), for the problem you consider (the simple harmonic oscillator). There is probably no need to mention

Morse theory or the Maslov index to your intended audience, unless you study more general physical systems.

23. **Peter Orland**
March 16, 2017

Scratch that… the Maslov index is definitely there in the SHO. I just checked it.

24. **Peter Orland**
March 16, 2017

… by which I mean it has to be inserted into (23.12) past the first conjugate point.

25. **Peter Woit**
March 19, 2017

theoreticalminimum,
I hadn't seen that. From the table of contents it looks like there's some overlap with what I've written, but the books are mostly pretty different. I'm traveling now, when I get back will take a closer look.

26. **Peter Woit**
March 19, 2017

Roland Rosenfelder/Peter Orland,

Thanks for pointing this out! When I get back to New York soon I'll think a bit about how much I can sensibly say about this in the book, but the existence of this phenomenon certainly needs to be mentioned.

27. **Maximillian G. Tresmond, Esq.**
March 19, 2017

Congratulations! What a great accomplishment!

28. **Austin**
March 20, 2017

At a first glance this is an impressive display of author's scholarship. The writing is clear. However, I don't know what is the purpose of a book of this size? Is is supposed to be used for a semester long material? The book seems diffuse on what chapters are considered important knowledge for the subject. What is the idea behind chapters that are just a couple of pages long?

29. **Peter Woit**
March 20, 2017

Austin,

The book roughly corresponds to a year-long course I've taught a couple times, assuming roughly a typical advanced undergraduate in math background. The later parts are much more challenging than the earlier ones. The size of chapters very roughly corresponds to a lecture (the class meant twice a week).

The goal of the book was to get to the point of explaining the basics of the quantum field theory ideas that make up the Standard Model, with the constraint of dealing just with free fields, not interactions, all in a way that emphasizes symmetries and how to think of quantum theory in terms of representation theory. There's a lot of material in early parts of the book which is not normally covered in courses at that level, but it's there because it's the simplest example of ideas and calculations which normally only are first explained in the much more complicated context of relativistic quantum field theory. I hope that this material will be useful as background for anyone studying relativistic QFT.

30. **Anon**
March 20, 2017

Hi Peter,

This is off topic but looks like Jester has won his SUSY bet with Lubos? CMS and Atlas reported their 36 fb-inv search results at Moriond 2017.

Surprised to see that blogs havent picked this up yet.

https://indico.in2p3.fr/event/13763/other-view?view=standard

cornering natural susy with 13 TeV… https://indico.in2p3.fr/event/13763/session/4/contribution/89/material/slides/0.pdf

31. **Shantanu**
March 22, 2017

OT : Peter, very curious to know your take on https://arxiv.org/abs/1703.05331
It says that no BSM physics below QG scale will be seen as pulsar timings severely constrains such bounds.

32. **Peter Woit**
March 22, 2017

Shantanu,

That claim seems highly implausible. They're making an extraordinary claim, that we have experimental access to just about any physics at arbitrarily short distance scales. That would be wonderful, but I don't see any extraordinary evidence so will wait until someone with more time on their hands figures out whether they really have something.

33. **Peter Woit**
March 22, 2017

Anon,

Will write very soon about the new LHC results.

34. **Peter Woit**
March 24, 2017

Roland Rosenfelder,

Now back from vacation and one thing I've gotten done is to add something about the issue you mentioned. From my point of view the source of this choice of phase factor is determined by analytic continuation from, so I've referred to a source that discusses it in that way. Thanks for pointing this out!

35. **Anonymous**
March 25, 2017

Why keep an empty Acknowledgements section?
I would remove it.

36. **Peter Woit**
March 25, 2017

Anonymous,

That will be the last thing to be added. I'm still getting help that I'd like to acknowledge, some from this blog…

37. **Georges E.**
March 27, 2017

Peter Woit, thanks for this wonderful book: so clear, and yet it doesn't leave anything out! A tremendous achievement.
Congrats!

38. **anon**
April 7, 2017

On page 74 you could use \left[and \right]
On page 76 it looks Φ has extra parentheses
On page 106 and 199 you could use \left(and \right)
On the exercises you could also use \left(and \right)

39. **Peter Woit**
April 7, 2017

anon,

Thanks, fixed.

New LHC Results

March 22, 2017
Categories: Experimental HEP News

This week results are being presented by the LHC experiments at the [Moriond](#) (twitter [here](#)) and [Aspen](#) conferences. While these so far have not been getting much publicity from CERN or in the media, they are quite significant, as first results from an analysis of the full dataset from the 2015+2016 run at 13 TeV, This is nearly the design energy (14 TeV) and a significant amount of data (36 inverse fb/experiment). The target for this year's run (physics to start in June) is another 45 inverse fb and we'll not start to hear about results from that until a year or so from now. For 14 TeV and significantly larger amounts of data, the wait will be until 2021 or so.

The results on searches for supersymmetry reported this week have all been negative, further pushing up the limits on possible masses of conjectured superparticles. Typical limits on gluino masses are now about 2.0 TeV (see here for the latest), up from about 1.8 TeV last summer (see here). ATLAS results are being posted here, and I believe CMS results will appear here.

This is now enough data near the design energy that some of the bets SUSY enthusiasts made years ago will now have to be paid off, in particular Lubos Motl's bet with Adam Falkowski, and David Gross's with Ken Lane (see here). A major question now facing those who have spent decades promoting SUSY extensions of the Standard Model is whether they will accept the verdict of experiment or choose a path of denialism, something that I think will be very damaging for the field. The situation last summer (see here) was not encouraging, maybe we'll soon see if more conclusive data has any effect.

If the negative news from the LHC is getting you down, for something rather different and maybe more promising, I recommend the coverage of the latest developments in neutrino physics here.

Update: Lubos has paid off his bet with Jester. Losing the bet hasn't dimmed his enthusiasm for SUSY. No news on whether David Gross has conceded his bet.

Comments

1. **new**
   March 22, 2017

   do you ever plan to publish an update to your 2005 book, updated with these latest results?

2. **Shantanu**
   March 23, 2017

   Peter I am still waiting for your post (promised after 2015 Nobel prize) on why non-0 neutrino mass is important and the impact of non-zero neutrino mass on BSM. (From John Ellis talk at PI, he mentioned that we still don't have a clue about how to connect BSM physics at TeV scale with neutrino mass measurements)

3. **flavor physics**
   March 23, 2017

   Not only neutrino physics. Also quark flavor physics too!, in particular from the LHCb experiment. There are other things than ATLAS and CMS direct searches 🙂
   Several measurements in flavor physics, none conclusive alone, seems to all point towards a change in C9 Wilson coefficient. On top of that, LNU (Lepton Non Universality) also adds some spice in flavor physics.
   See, eg, Joaquim Matias' talk @Moriond EW
   https://indico.in2p3.fr/event/13763/other-view?view=standard#2017-03-22

4. **Peter Woit**
   March 23, 2017

   new,
   No plans for anything like that now, I'd rather spend my time on other things. I just took a look at the chapter on supersymmetry in my book. It was written nearly 15 years ago, but I think stands up very well. It explains the long list of problems with SUSY extensions of the SM, and lists exactly two positive arguments for such models: one is the approximate unification of coupling constants, the other is the prediction of new weak-scale physics, observable at the LHC. That second argument is now gone, and I don't see any continuing reason for anyone to take these models seriously.

5. **chris**
   March 23, 2017

   flavor physics,

   for me the interesting thing is not the appearance of flavor "anomalies", but that LHCb has seen so few of them until now. Throughout the last decades, these anomalies came and went and it speaks for the quality of the LHCb analysis techniques that they produce so few false positives.

   I will take any bet that these "anomalies" will be gone for good before long (i.e. with the next order of magnitude increase in integrated luminosity).

6. **new**
   March 23, 2017

   In light of LHC not seeing any hints of SUSY, and your book NEW, what are the implications to string/m-theory research and debate, esp the claim that string theory does not require low-energy SUSY, and there are string vacua that are entirely non-susy?

7. **Peter Woit**
   March 23, 2017

   new,
   I don't want this to turn into the usual arguments about string theory. That said,
   the problem for string theory is that the standard response from string theorists for many years when asked "how can string theory be tested" was "by finding supersymmetry at the LHC", see for instance here
   http://www.math.columbia.edu/~woit/wordpress/?p=3864

Now that hope is gone, there really is no answer at all to this question, and claiming to have a wonderful theory of everything that makes no predictions and can't be tested is not a comfortable situation to be in.

8. **Hans**
March 24, 2017

Peter,

There is that famous graph showing that the three coupling constant unify at a certain energy when supersymmetry is taken into account. The same graph also shows that above that energy, the coupling constants diverge again. How is this second fact explained by supersymmetry fans?

9. **Graham**
March 24, 2017

Looks like Lubos is negotiating payment to Adam, possibly involving a pigeon 😉
https://twitter.com/Resonaances/status/844218605638103040

10. **Peter Woit**
March 24, 2017

Hans,
What is supposed to happen in a GUT is that there is a new Higgs-like sector that breaks the GUT symmetry to SU(3) x SU(2) x U(1). Above the GUT scale these degrees of freedom contribute and the dynamics changes. The necessity of introducing this new sector and the various problems it causes is one of the main drawbacks of GUTs.

11. **tommaso dorigo**
March 24, 2017

Hi Peter,

thanks for the link to the NeuTel blog! Indeed I concur that neutrino physics and astrophysics is where the most interesting results are going to appear in the near future…

Cheers,
T.

12. **Low Math, Meekly Interacting**
March 24, 2017

Hoping to see a post from Jester soon. His blog's been dormant for a while, causing me some worry it will be orphaned…

While the last shovel-full's of soil are being spread over SUSY's coffin, it seems worth noting the graveyard has gotten rather full these past few years. Are there ANY ideas left that are both compelling and have some hope of being tested experimentally by 21st century humans and their colliders?

13. **new**
March 24, 2017

Peter could you please clarify this scientific issue,

do the current null results based on 36fb-1 @13TEV completely falsify the hypothesis of natural SUSY, or is there still a parameter space remaining for natural SUSY, perhaps that a future proposed 100 TEV collider is required to eliminate?

14. **Peter Woit**
March 24, 2017

new,
The problem is that "natural SUSY" is not a well-defined concept. It's basically the idea that SUSY explains the weak scale (say 200 GeV), and thus implies that the SUSY-breaking scale should be roughly that. Already at the Tevatron, there were bounds above 200 GeV for gluinos and no evidence for any SUSY particles, so enthusiasts were saying "it's only "roughly", so maybe parameters are "tuned" to the extent of only being 10% of what you might expect". With the LHC results they have to say, "maybe only 1%" of expected, After nothing is found 50 years from now at a 100 TEV collider they'll say "maybe only .1%".

Besides this generic problem, there are 105 or so extra parameters in minimal SUSY models. You can try and evade LHC results by picking these to have special values making sparticles hard for experimentalists to find.

The fundamental situation though is that SUSY never provided a compelling explanation of anything about the SM, and is much, much more complicated (see my old book for details). So, not seeing anything is exactly what one would expect, and arguing for more and more implausible explanations for why your idea failed as expected is becoming more and more pathetic (and fewer and fewer are willing to go down this route).

15. **Thomas Larsson**
March 24, 2017

Sigh. I'm not sure that I want to say this, since I have found that I agree with Lubos on most things (including some politics), but I feel that I should point out that my ten-years-old theorem still holds.

Theorem [Larsson 2007]
Supersymmetry will not be discovered at the LHC.
Proof: String theory predicts supersymmetry (Witten 1984-2002). String theory predictions are always wrong. Hence supersymmetry does not exist, and will in particular not be found at the LHC. QED.
Corollary
Lubos Motl will lose his experimental-susy-by-2006 bet.

16. **Yatima**
March 26, 2017

Science/Popular article in Nautilus about the search for the 5th force spotted:

http://nautil.us/issue/46/balance/the-fifth-force-of-physics-is-hanging-by-a-thread

17. **pval**
March 27, 2017

“The chance that they are just a random statistical fluctuation is tiny, says Feng: about 1 in 100 billion.”
http://nautil.us/issue/46/balance/the-fifth-force-of-physics-is-hanging-by-a-thread

I’m going to assume this is referring to Krasznahorkay et al (2015; https://arxiv.org/abs/1504.01527). There was a lot of this “transposing the conditional” surrounding the LIGO results as well. As a layperson wrt physics I find the continued misinterpretation of p-values in physics discussions disconcerting. If the people interpreting physics data do not understand the tool they are using, how can we trust their conclusions?

Some explanation can be found at these links:
http://rationalwiki.org/wiki/Confusion_of_the_inverse
http://andrewgelman.com/2013/03/12/misunderstanding-the-p-value/

18. **Peter Woit**
March 27, 2017

Yatima/pval,
The “fifth force” story really is a different topic, and, honestly, one I personally don’t think is worth paying a lot of attention to. It’s yet another example of the general principle that “extraordinary claims require extraordinary evidence”, and the evidence here is definitely not extraordinary.

19. **emile**
March 28, 2017

pval: “If the people interpreting physics data do not understand the tool they are using, how can we trust their conclusions?”

Experimentalists in big collaborations understand p-values and you will not see a paper with such a mistake published (though students may make similar mistakes). I don’t know if Feng was correctly quoted but theorists do not work on a daily basis with statistics or the interpretation of data. Anyway, I would not decide that “physicists” can’t interpret data based on a quote from a theorist.

20. **hyd**
March 29, 2017

Ideas that do not work do not die out by themselves. They die out only after their creators die. This is what history has shown to us. So, I predict however negative the evidences to come out, those with SUSY will not stop pushing their ‘agenda’ until their last breath. Those people will keep jiggling with their models and playing all ingenuity, acting as if they were the GOD. It may well take/waste another few generations to fully quench the zeal.

21. **Peter Woit**
March 29, 2017

hyd,
Unfortunately I’m not so sure that the ideas of the supersymmetric standard model, string theory unification, and the multiverse will die out with their creators. These creators are already no longer with us or retirement age and losing influence, but their failed ideas have been rather successfully institutionalized, and several generations of younger physicists have been trained in them. Will they abandon these ideas? Will those promoting them to the public and to young people entering the profession stop doing this? So far, I’m not optimistic…

22. **GoletaBeach**
March 29, 2017

The overwhelming majority of humans alive today believe all sorts of supernatural phenomenon are real. That a tiny minority who believe in rationalism, applicable mathematics, and the scientific method held on throughout millennia of being imprisoned , burned at the stake, etc, had their ideas survive to influence modern thought is actually a remarkable story… and we scientists are still a minority.

The truth is… whatever the results of well-conceived and executed experiments, like those of the LHC, turn out to be, we all win, because we learn what Nature really is. As Luis Alvarez said about search with muons for chambers in the Egyptian Pyramids: he didn’t fail to find new chambers, is succeeded in proving there were no further chambers to speculate about.

The human dramas of betting and pilloring famous theorists who made wrong predictions may be interesting and fun, but it is just a social sideshows. SUSY might easily be correct but it might take humans another 10,000 years to discover that. Science itself doesn’t give a darn about human lifetimes.

The snickering and schadenfraude about LHC not finding SUSY is hardly a blip in that process, but to the extent that creative ideas to empirically probe SUSY and all other conjectures get inhibited by the snickering, that is unfortunate.

Independent of theorists’ ideas, hadron colliders remain the best tool to probe higher energies in actual experiments (as opposed to observations). LHC did and is doing a tremendous job, but likely we have learned that the SSC was even more important as a pathfinder than we thought in 1994. Mel Schwartz was right… he never cared much about the SUSY theorists. Go to the highest energy you can afford… that is always a compelling discovery strategy. The only role theorists really have is to run a propaganda front among the supernaturalists for what is completely obvious to experimentalists.

Unfortunately, propaganda is an issue of human drama, and it certainly is true that the propaganda can backfire.

Personally, given that the LHC was unconditionally a terrific empirical endeavor, I don’t much appreciate criticism of the over-enthusiastic theorists. Their true role was over when the LHC was well funded… they helped do that. After that, science will little note nor long remember what they said, but what the experimentalists did will be never forgotten.

23. **Peter Woit**
March 29, 2017

GoletaBeach,

It’s not “snickering”. It’s extremely important for theoretical physics that ideas that don’t get work get discarded, a disaster if they don’t, but instead become the dominant paradigm for the field. These SUSY models have always been highly problematic and heavily oversold. Pre-LHC one could argue that the “naturalness” argument justified waiting for the LHC results before abandoning them completely,. The results are now here and these models should be given a decent burial. Getting influential theorists to acknowledge that the models are dead is quite important, and the point at which they pay off their lost bet is a good time for this to happen.

For experimentalists, the issues are different. Finding the Higgs is a great achievement, but so is exploring a new energy range and finding out what's there (and what's not there). Killing off SUSY is an achievement they should be proud of. Going forward obvious goals are to do the best possible job of studying the Higgs sector, as well as to do the best and most complete job of exploring the energy range the LHC can reach. This is up to the experimentalists, but it seems to me that they should be thinking about how best to weight effort aimed at further SUSY searches versus other kinds of analyses. Fine if you like having bad models to use as propaganda, not so great if you believe your own propaganda and it blows back on you and damages your work.

If there is some way to afford it, I agree that a higher energy machine is desirable. I don't think though that bogus arguments about how such a machine is likely to find SUSY or produce black holes are going to be effective. It's not the ignorant masses that will decide whether to spend the money, but people who are a lot more sophisticated, and trying to pull the wool over their eyes with a dubious argument I think will be counterproductive.

24. **Gregor**
March 29, 2017

Peter,

why is a higher energy accelerator desirable? We have experiments that show clearly that the standard model is correct; we have calculations/theory showing that it is correct up to (almost) the Planck scale. We have no difference between theory and experiment. None.

The only thing we lack is a theory showing that the standard model is consistent with general relativity and at the same time predicts the 20 parameters of particle physics. This is a much simpler exploration, much less demanding, and well in reach in a few years time. A friend of mine would gladly take bets with real money that this scenario is correct.

With all data available, pouring money in a new machine seems an almost certain waste. A quick overview of the arguments in favor of such a machine shows that all these arguments (supersymmetry and its particles, wimps and other microscopic dark matter, more dimensions, superstrings, microscopic black holes, axions, additional generations, more Higgs bosons, fifth force, and dozens of others) have already been put aside by the LHC. Why should we look in a corner of nature when we already know quite definitely that there is nothing to be found there?

Other arguments for a machine (strong CP problem, dark matter, etc.) are based on astrophysical data, which is shaky anyway.

This "why?" question is a serious one. The hope that there is something at higher energy that can be found is based on wishful thinking, not at all on experimental data.

25. **Anonyrat**
March 30, 2017

We need high precision tests of every term in the Standard Model.

26. **Marc Nardmann**
March 30, 2017

Gregor,
you wrote: "we have calculations/theory showing that [the standard model] is correct up to (almost) the Planck scale." I would like to understand this claim: "correct" in which sense, given the lack of experimental data between the LHC and Planck scales; what kind of calculations (perturbative? at which order?); what precisely are the results. Could you give a few references?

27. **Peter Woit**
March 30, 2017

Marc Nardmann,

I assume Gregor meant "consistent", not "correct"

Gregor,

I think the argument "because it's there" is a perfectly good one for doing experiments to probe a higher energy scale. Yes, we have no good theoretical reason for expecting something new, but theorists are often wrong about such extrapolations.

Maybe the LHC will yet find something unexpected and that will give a target for what to investigate. If not, investigating the Higgs sector would be a major goal, especially measuring self-interactions of the Higgs.

The problem is not that there's no good physics to be done at higher energies, the problem is the cost. If it were $100 million, Jim Simons could finance it out of the spare change in his couch and it would be obviously worth doing. At $1 billion, it could be financed within the current budgetary envelope of CERN and also I think would obviously be worth it. The problem is that to get to a much higher energy scale, a larger ring is needed and that is going to be very expensive, $10 billion and up.

For order of magnitude $1 billion, you might be able to build the HE-LHC (same tunnel as the LHC, higher field strength magnets, doubling the energy). Again, the question is money, is that size jump in energy worth the cost?

28. **Gregor**
March 30, 2017

Peter,

I understand what you say; but we should first give 10 theorists 1 million each with the aim to find a consistent theory showing that general relativity plus the standard model is all there is (including my friend). And we should give 10 theorists 1 million each to come up with a correct theory that predicts something at higher energy.

Only if they first group finds *no* such theory, and the second group finds at least one, should we really spend 10 billion on an accelerator; at the moment the risk of not finding anything new is much too big.

After all, the LHC has "only" found the Higgs, not much else. And it confirmed the standard model. One could indeed argue (which I do not) that the last 5 years of operation of the LHC were not worth the money spent. So we will all have a tough time arguing for a new machine. Especially so if theoretical desert scenarios and new theoretical options have not been explored fully beforehand.

29. **GoletaBeach**

March 30, 2017

I suppose prior to about 1492, Europeans questioned the utility of sailing west. Peter is absolutely right: sailing west because you can is and was sufficient reason to do so. There will always be theorists who say this and that… when you have a new technology (like navigation and sailing ships in the 1400's) and can do something truly new and different (like sail long distances on the world's oceans) a fine strategy is to do everything you can to get self-appointed smart people to support you. And forget their reasons for doing so… new sources of gold were the argument in the 1400's, but in reality, the potato and a new land for Europe's malcontents were the big payoffs.

Pretty much all hadron collider experiments use the same tools whether one looks for SUSY or not… 1)look for unexpectedly large observable cross sections for every final state you can think of… (Rutherford invented this technique in 1910 or so) 2)look for missing energy (Ellis & Wooster invented this one in 1928 or so). Those are the classics. SUSY is just another excuse to re-do the classics at higher energy.

It is extremely simple radiation physics (synchrotron radiation) that leads us to hadron colliders rather than lepton colliders, although Peter is right again… precision Higgs studies at a tuned electron positron collider are an excellent idea right now. Because we can, and the analogous history of finding new physics based on small effects (charm quark mass from K0-K0bar mixing, top quark from K0-K0bar CP violation, Higgs mass from LEP precision electroweak) has been successful.

Something or another is keeping the Higgs mass so small. Something or another regulates quantum field theory at high energies. The Energy Frontier remains a good way to seek solutions to that puzzle. But as Peter says, we can't because it is just too darned expensive at the moment. Lots of people have been saying "do accelerator research" for 30+ years. The fact is people do that but maybe improvements are no easier than reductions in the cost of using neutron flux to turn lead into gold.

I'm confident in 10,000 years human kind will find a way, though. Or maybe 50,000 years. Maybe not in my lifetime or the lifetime of any blog poster here.

But at the same time… more human lives will be saved more cost-effectively right now by purchasing air-conditioners for senior citizens suffering through hot summers in St. Louis, then by spending on high energy physics. The same was true in 1400's Europe… every dollar given to Columbus would have saved more lives, in the short run, had it been spent on food for the poor. It is an eternal paradox.

Like the paradox that the very best batting average in a season of American pro baseball was 0.440. Not even 1/2 the time did Hugh Duffy get a hit! Research has a lower batting average. If anybody wants certainty in research endeavor, they really don't want to do research at all.

30. **martibal**
March 31, 2017

"I assume Gregor meant "consistent", not "correct""

And what about the instability of the Higgs vacuum ? (the fact the the quartic coupling of the Higgs become negative at high energy). Is that so much consistent ? Strictly speaking it is not a contradiction, but it is very weird, no ?

31. **Anon**
March 31, 2017

The major challenge is in writing the Lagrangian that includes neutrino masses/couplings. There is no standard way of writing the SM Lagrangian for neutrino masses/couplings. So whether it is BSM or SM, what is the Lagrangian? How can we test it? Those are the questions.

32. **Peter Woit**
March 31, 2017

Gregor,
I think we can accurately predict the results of giving $1 million to 10 theorists…

33. **B'Rat**
March 31, 2017

GoletaBeach,
I find the Columbus analogy rather bizarre. One should remember that his aim was not "let's see if there's a new continent to plunder", but "my calculations show that Asia is within reach by our ships". Too bad his contemporaries actually knew that his calculations about Earth's circumference **were wrong**, and that thus had America not existed Columbus and his sailors would have died a rather sad death in the middle of the Ocean. This explains why he found so difficult to fund his voyage. In essence, he was a *lucky idiot*.

34. **emile**
March 31, 2017

Anon: what about giving the neutrinos dirac masses with small yukawas?

35. **Ellsberg**
March 31, 2017

An interesting take on events these days: http://vixra.org/pdf/1703.0300v1.pdf

36. **GoletaBeach**
March 31, 2017

Or maybe Columbus was just utilizing advances in shipbuilding and navigation to try to do something really interesting. Of course the Talavera commission knew his crew would die of scurvy prior to reaching Japan. What they knew was… completely wrong.

Of course Columbus was a lucky idiot. And pretty much every great, truly (as opposed to bureaucratically) transformative discovery is lucky idiocy too.

There on the Berkeley campus in the early 1930's was Lawrence and his crew slaving away making medical isotopes to pay for the little bit of research they could do between midnight and 6am. When news came from Cambridge of the neutron's discovery, they realized that the neutron flux from their cyclotron exceeded that in the European setups by orders of magnitude, and they had in fact neutron activated their entire lab, but had failed to notice because they didn't hunt for radiation when the beam was off.

Perhaps they were unlucky idiots… a little more creative exploration without a good reason and they could have been lucky idiots, and discovered the neutron, which is the biggest game-changer in nuclear physics.

37. **Unmanned**

March 31, 2017

I look forward to unparticles being quickly detected and analyzed in Mr. Un's uncollider.

38. **Anon**
March 31, 2017

emite — sure you can add right handed neutrinos to the standard model and give the neutrinos small dirac masses by choosing their yukawa couplings to be very small ~10^-12.

So how do you test this?

You need to measure the Higgs Yukawa coupling to the neutrino or you can disprove the idea by showing that the neutrino is a Majorana particle and not a Dirac particle.

Alternatively, you can say that you will add 3 right handed neutrinos and include a Majorans mass terms and do the seesaw mechanism in the std model. Again you can test this by trying to find the right-handed neutrinos Majorana mass scale. But this scale may not be reachable by colliders.

Or you can say you wont add 3 right handed neutrinos but add a Higgs triplet to SM and give the left handed neutrinos small Majorana masses directly (Called Type 2 seesaw). You can test this by finding the Higgs triplet — but it could be at some very high seesaw scale that is never reachable by colliders.

Essentially the neutrino sector of SM is as untested as the neutrino sector of BSM.

In fact in BSM you may be able to think of more consequences (non-collider as well) and test for those consequences. SO BSM can be more testable than SM. For example proton decay for GUT theory, vanishing of leptonic CP phase for minimal left right symmetric model with parity (as leptonic \delta_CP contributes to strong CP phase in one loop in LR symm model).

39. **Anon**
April 1, 2017

sorry emile mis-spelt your name in the above response.

40. **Gregor**
April 2, 2017

Peter,

you are right to make fun of theorists. The LHC was sold/marketet to politicians as the machine to search for the Higgs, for supersymmetry, for dark matter and for hidden dimensions.

How would you sell the next collider, especially in the light of the last results? (1) We cannot use the same arguments any more. (2) And you need a good theory to provide arguments.

Susy and strings were wrong, but provided 3 of the 4 arguments for the LHC. On the other hand, showing that the standard model is correct and consistent is not an argument for a new machine. If the theorists cannot provide a good vision for the future, there will be no new machine.

Wasting 20 millions on 20 (or more) theoretician's salaries is much less than wasting 10 billions in a machine with the risk of getting no results. But of course your making fun of the idea is also valid! After all, we just have to look at hep-th to see that nothing is coming from that side since quite a long time. So shall we stop here?

Peter, let me ask you directly: (1) If you had 10.02 billions at your disposal for the coming 10 years, how would you spend the money? (2) What arguments would you provide?

41. **Peter Woit**
April 2, 2017

Gregor,
The problems of theoretical physics I don't think are ones that can be addressed by simply putting more money into the field. They're a different story. Redirecting money from experiment to theory will solve nothing.

Doing things like telling the public that the LHC was designed to find extra dimensions was even at the time clearly a really bad idea. It wasn't necessary to get the LHC funded, and engaging in outrageous hype like that creates a serious credibility problem next time you have a new project that needs funding. So, going forward, that will be a big problem, and doubling down with "the extra dimensions are at 10 TeV rather than the 1 TeV we thought" is unlikely to work.

The problem isn't the coming 10 years, I think experimentalists have made reasonable choices about plans for the next 10 years, with $1 billion/year the order of magnitude that is planned. At the energy frontier, it will be the HL-LHC.

The problem is the longer term. Absent some new information, I don't think HEP can successfully argue that there is a good reason to redirect resources from elsewhere and put significantly more money into the field. Maybe it can hold onto its current funding level and then there's a difficult problem of how to allocate that. How much should go to a new energy frontier machine and of what kind? People are very actively looking at the possibilities, and at some point will have to focus on the price tags and how to proceed.

42. **Frank Wilhoit**
April 2, 2017

B'Rat,

The luck of Columbus was that he came back. Just think if he hadn't — as, in hindsight, was much the likelier outcome.

43. **Darrell Burgan**
April 3, 2017

I don't suppose Motl actually admitted he was mistaken … ?

44. **Cobi**
April 3, 2017

Darrell,
the bet was 100 to 1, i.e. Motl would have won 10,000$ if SUSY were found given the current set of data. No matter how you think about Motl, there is nothing to admit after a bet of this type. In the end they made assumptions about Bayesian probabilities.

45. **Peter Woit**
April 3, 2017

Cobi,
Jester's bet I think was significant mainly in that it made clear that there were many theorists (Falkowski is a rather mainstream theorist) who not only thought that the LHC would not see SUSY, but that SUSY was already, pre-LHC, an idea that hadn't worked, so it was not only unlikely the LHC would see it, but very unlikely.

As for Lubos, when the LHC turned on, his description of the probability of seeing SUSY was that "many of us " thought it was "90% or higher", but would publicly say "60%" just to be "modest", see links here
http://www.math.columbia.edu/~woit/wordpress/?p=3864
He later revised that down to "50%".

I'll leave it to the Bayesians to explain the significance of a scientist claiming his theory predicts something specific with "90% or higher" certainty, then claims it doesn't matter, his theory is still a sure thing, when the specific prediction is falsified.

What is much more significant than what Lubos has to say is what the most influenced and respected theorists pushing SUSY now will have to say. I have in mind in particular Gross, Wilczek and Witten (although Witten as far as I know never bet anything on this).

46. **GoletaBeach**
April 3, 2017

Oops Peter… the proposers and sellers of the LHC never said the "LHC was designed to find extra dimensions".

https://press.cern/press-releases/1993/12/large-hadron-collider-presented-cern-council

Maybe some theorists who were uninvolved in the real spadework to achieve approval of the LHC came in later and cut in to the front of the parade… wouldn't be the first time.

Actually, in 1993, the SSC was sold on "no-lose" for Higgs observation. If they Higgs was not observed, you'd see strong W-W- scattering at the SSC. No lose. No mention of extra dimensions, many worlds, etc.

Then gradually precision electroweak measurements from LEP, SLC, and CDF indicated that the Higgs would be light and Atlas & CMS decided to shell out for >$100 million calorimeters to see Higgs to gamma gamma. They were right! And at the time not a few committee reviewers wailed on them, but they held fast to the need for highly segmented, fast calorimeters.

Funny how that really great story sometimes gets buried under some speculative smoke by opportunistic theorists.

47. **Peter Woit**
April 3, 2017

GoletaBeach,
I understand, the extra dimensions was theorist hype that appeared later, around 2000. For an example, see the classic New York Times article "Physicists Finally Find a Way to Test Superstring Theory"
http://www.nytimes.com/2000/04/04/science/physicists-finally-find-a-way-to-test-superstring-theory.html

48. **GoletaBeach**
April 3, 2017

Well, thanks Peter. Sure… not an experimentalist, nor an LHC advocate like Rubbia or Llewellyn-Smith even interviewed in that NYT article… and most certainly none of the machine builders themselves, or the detector leaders like Jenni or della Negra.

You have made me wonder if the "real story" of the LHC… how the US was against it due to the SSC and the SSC "No-Lose" theorem, thought to need 40 TeV in the center of mass, to see the Higgs, was proven to be irrelevant first by loop effects evaluated at the LEP/SLC/CDF energies, and then by observation of the light Higgs itself… will ever be told.

49. **tulpoeid**
April 4, 2017

"Lubos has paid off his bet with Jester."

At least with this piece of news we know that Jester is healthy and doing well. But no motivation to post even on Moriond and on April fools'? HEP not doing much the same.

50. **new**
April 4, 2017

is it premature to declare SUSY dead?

since only 36 fb-1 @ 13tev was analyzed, with additional 45 fb-1 projected this year and 3000fb-1 overlifetime, is it still possible LHC will find SUSY?

what about a future HE-LHC or 100tev collider?

51. **Anon**
April 4, 2017

new — If an electron EDM is found then there is a good chance that a future collider will see something. As CP phases in new physics generate an EDM to the electron. Currently non-observation of electron EDM by ACME collaboration, puts generic SUSY scale to be above 8-10 TeV, which is out of LHC's reach. Future improvements in sensitivity by an order of magnitude which is expected soon by ACME, and non-observation of electron EDM, will put SUSY scale above 30 TeV, out of reach of future colliders. Of course there could always be some particular SUSY models with suppression of CP phases where the electron EDM limits wont translate to limits on new Physics.

52. **Anon**
April 4, 2017

new — for reference for electron EDM impact on particle physics, please see slide 47 of https://indico.hep.anl.gov/indico/getFile.py/access?resId=0&materialId=slides&confId=791

53. **kashyap vasavada**
April 4, 2017

I understand, one of the principal difficulties that high energy experimentalists face in LHC and beyond is that, there are some hundreds of millions of event/sec and their job is to find a needle in a haystack as they say! So there is too much reliance on known theoretical models in setting up triggers etc. The reason given is that there is not enough memory to store every event. While this is true, it may be that they could be missing phenomena which are not predicted by any existing theory. Admittedly this strategy worked very well for the discovery of Higgs. But for unknown physics it may be a handicap!

54. **Peter Woit**
April 4, 2017

tulpoeid,
Yes, and this is one reason blogging here has been sparse (I'm also busy thinking about other things). Not a lot happening in HEP physics these days, with the release of LHC results getting zero attention just one aspect of the situation.

new,
The problem is that further increases in luminosity only give you relatively small increases in energy reach (given a typical model). Current limits on gluinos are about 2 TeV. I don't know the projected numbers for the energy reach of the HL-LHC, but I would guess it's something like 2.5 TeV, no more than 3 TeV. So, sure, maybe there's a gluino in between 2 and 2.5 TeV, but there's no argument for this other than wishful thinking. The only argument people had for why the LHC should see a gluino was the fine-tuning argument that said it should have already been seen at the Tevatron. Similar comments apply to the energy reach increase of next generation accelerators. Anon is also right that the idea of superpartners just above the LHC limits is in conflict with the EDM measurements (and this was true pre-LHC, so one reason Jester and others felt there was a very good bet against SUSY even then),

kashyap vasavada,
Experimentalists are well aware of this issue, of the need to set triggers and do analyses which will be sensitive to a wide range of possible new phenomena, not just something with very specific properties. The possibility that something is being missed because it's not being triggered on or not being looked for is something worth worrying about, and a good argument for considering as wide a range of models as possible, not concentrating on SUSY models.

55. **new**
April 4, 2017

anon – thanks for link i'll look forward for new results on ACME. any idea when in 2017 it will be results will be announced?

peter – given the strongest argument for SUSY is fine-tuning argument and LHC has not seen SUSY, what is the likelihood SUSY exists but is broken at some higher energy scale?

56. **Anon**
April 5, 2017

new,

Don't know, but looks like they are currently taking data. (link)

57. **new**
April 5, 2017

anon, the link you provide seems to suggest

Abstract: B2.00001 : ACME Measurement of the Electron Electric Dipole Moment
10:30 AM–11:00 AM Tuesday, June 6, 2017
Room: 306-307

Tuesday, June 6, 2017 newest results will be released

any word on the latest results on neutron and proton edm?

58. **Anon**
April 5, 2017

new,
Electron EDM implies new physics would be found at next collider — but neutron and proton EDMs do not imply this….. this is bec. the strong CP Phase \theta contributes to neutron EDM and \theta can be generated due to physics at very high scales — like the seesaw scale (which could be at 10^14 GeV) or the Planck scale.

So finding neutron EDM, while important, does not imply there will be new physics to be discovered at next collider.

Dont know when the next nEDM expts will be reporting their new results — I believe in one expt, data collection is underway (but dont know if it will increase the sensitivity by much) — several other expts are planned around the world to run in next few years to increase the sensitivity by an order of magnitude.

Some Math and Physics Interactions

April 5, 2017
Categories: Uncategorized

Quanta magazine has a new article about physicists "attacking" the Riemann Hypothesis, based on the publication in PRL of this paper. The only comment from a mathematician evaluating relevance of this to a proof of the Riemann Hypothesis basically says that he hasn't had time to look into the question.

The paper is one of various attempts to address the Riemann Hypothesis by looking at properties of a Hamiltonian quantizing the classical Hamiltonian xp. To me, the obvious problem with an attempt like this is that I don't see any use of deep ideas about either number theory or physics. The set-up involves no number theory, and a simple but non-physical Hamiltonian, with no use of significant input from physics. Without going into the details of the paper, it appears that essentially a claim is being made that the solution to the Riemann Hypothesis involves no deep ideas, just some basic facts about the analysis of some simple differential operators. Given the history of this problem, this seems like an extraordinary claim, backed by no extraordinary evidence.

I suspect that the author of the Quanta article found no experts in mathematics willing to comment publicly on this, because none found it worth the time to look carefully at the article, since it showed no engagement with the relevant mathematical issues. A huge amount of effort in mathematics over the years has gone into the study of the sort of problems that arise if you try and do the kind of thing the authors of this article want to do. Why are they not talking to experts, formulating their work in terms of well-defined mathematics of a proven sort, and referencing known results?

Maybe I'm being overly harsh here, this is not my field of expertise. Comments from experts on this definitely welcome (and those from non-experts strongly discouraged).

While these claims about the Riemann Hypothesis at Quanta look like a bad example of a math-physics interaction, a few days ago the magazine published something much more sensible, a piece by IAS director Robbert Dijkgraaf entitled Quantum Questions Inspire New Math. Dijkgraaf emphasizes the role ideas coming out of string theory and quantum field theory have had in mathematics, with two high points mirror symmetry and Seiberg-Witten duality. His choice of mirror symmetry undoubtedly has to do with the year-long program about this being held by the mathematicians at the IAS. He characterizes this subject as follows:

> It is comforting to see how mathematics has been able to absorb so much of the intuitive, often imprecise reasoning of quantum physics and string theory, and to transform many of these ideas into rigorous statements and proofs. Mathematicians are close to applying this exactitude to homological mirror symmetry, a program that vastly extends string theory's original idea of mirror symmetry. In a sense, they're writing a full dictionary of the objects that appear in the two separate mathematical worlds, including all the relations they satisfy. Remarkably, these proofs often do not follow the path that physical arguments had suggested. It is apparently not the role of mathematicians to clean up after physicists! On the contrary, in many cases completely new lines of thought had to be developed in order to find the proofs. This is further evidence of the deep and as yet undiscovered logic that underlies quantum theory and, ultimately, reality.

I very much agree with him that there's an underlying logic and mathematics of quantum theory which we have not fully understood (my book is one take on what we do understand). I hope many physicists will take the search for new discoveries along these lines to heart, with progress perhaps flowing from mathematics to physics, which could sorely use some new ideas about unification.

Update: Some comments sent to me from a mathematician that I think give a good idea of what this looks like to experts in number theory:

> The "boundary condition" is imposing an identification with zeta zeros by fiat, so the linkage of any of this to RH is basically circular. The paper at best just redefines the problem, without providing any genuine new insight. More specifically, as the experience of more than 100 years has shown, there are a zillion ways to recast RH without providing any real progress; this is yet another (if it makes any rigorous sense, which it does not yet do, yet the absence of rigor is not the reason for skepticism about the value of this paper, whatever the pedigree of the authors may be).
>
> One has to find a way of encoding the zeta function that is not tautological (unlike the case here), and that is where deep input from number theory would have to come in. This is really the essential point that all papers of this sort fail to recognize.
>
> Real insight into the structures surrounding RH have arisen over the past decades, such as the work of Grothendieck and Deligne in the function field analogue that provided a spectral interpretation through the development of striking new tools inspired by novel insights of Weil. In particular, the appearance of the appropriate zeta functions in such settings is not imposed by fiat, but is the outcome of a massive amount of highly non-trivial constructions and arguments. In another direction, compelling evidence and insight has come from the "random matrix theory" of the past couple of decades (work of Katz-Sarnak et al.) was inspired by observations originating with Dyson merged with work of the number theorist Montgomery.
>
> Number theorists making a major advance on the puzzles of quantum gravity without providing an identifiable new physical insight is about as likely as physicists making a real advance towards RH without providing an identifiable new number-theoretic insight. There is no doubt that physical insights have led to important progress in mathematics. But there is nothing in this paper to suggest it is doing anything more than providing (at best) yet another ultimately tautological reformulation by means of which no progress or insight should be expected.

Update: Another way to state the problem with this kind of approach to the RH is that without number theoretic input, it is likely to give a much too strong result (proving analogs of the RH for functions that don't satisfy the RH). For example, see the comment here (I don't know if this correct, but it explains the potential problem).

Update: Nature Physics highlights the Bender et al. paper with "Carl Bender and colleagues have paved the way to a possible solution by exploiting a connection with physics. Some wag there has categorized this work as work with subject term "interstellar medium".

Update: There's an article about the Bender et al. paper here, with extensive commentary from one of the authors, Dorje Brody, who addresses some of the questions raised here (for example, why PRL if it's not a physics topic?).

Update: Belissard has put up a short paper on the arXiv explaining the idea of the Bender et al. paper, as well as the analytical problems one runs into if one tries to get a proof of the RH in this way.

Update: One of the authors has posted on the arXiv a note with more precise details of the construction of a version of the operator discussed in the PRL paper.

Comments

1. **Anonymous**
   April 5, 2017

   The authors of this paper are humbly sharing some thoughts they have had that are relevant to the Riemann hypothesis. They are not claiming to have made profound progress. We can ignore their work if we like, there is no need to attack them. Carl Bender is awesome.

"Why are they not formulating their with in terms of well defined mathematics of a proven sort?" I assume they are simply explaining their ideas as clearly as they can.

2. **Peter Woit**
April 5, 2017

Anonymous,
Publishing in PRL (and a claim to have a worthwhile new idea about the Riemann Hypothesis) is not really an act of humility, but a claim to have something significant that people should pay attention to. I'm paying attention, and commenting on what this looks like from the point of view of the math community. It's a serious question and I think one I'm posing much more politely than most mathematicians would do privately: if they think they might have some progress towards proving the Riemann hypothesis, using tools that are not particular to physicists, but which mathematicians are experts on, why is there no engagement with mathematicians and their expertise?

3. **anon**
April 5, 2017

I'm not at all an expert, but it seems strange to me that PRL would publish a paper on such a topic. It doesn't exactly seem to be within their scope.

4. **G. S.**
April 6, 2017

While I feel the piece in Quanta is hyping things a bit too much, I think you're being a bit harsh in criticizing this paper's appearance in PRL. They appear to be making progress on a research question that has interested a number of physicists for a number of years. For example, see the 2011 review article Physics and the Riemann Hypothesis (https://arxiv.org/pdf/1101.3116.pdf). One quote from this review article which stands out to me is the following:

"The advantage of this approach would be that the huge number of ζ(s) zeros are known and quick numerical algorithms have also been developed to find further zeros, thus solving the Schrodinger equation for large energies would be unnecessary. The Riemann zeta function could play the same role in the examination of chaotic quantum systems as the harmonic oscillator does for integrable quantum systems."

That's still a bit of hyperbole, but it illustrates why someone who studies chaotic quantum systems would be interested in the article you mention. It would get them closer to a computationally-practical toy model for understanding chaotic quantum systems.

5. **Anon**
April 6, 2017

"and a simple but non-physical Hamiltonian"

There are physical systems described by PT-symmetric Hamiltonian. They describe non-isolated systems interacting with their environments, and have been studied in the laboratory, see e.g. https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.103.093902.

6. **Peter Woit**
April 6, 2017

Anon,

The Hamiltonian I'm referring to is the one discussed in the paper, their equation 1, which doesn't appear in the paper you link to (or, as far as I can tell, in any other discussion of a physical system).

7. **mrj**
April 6, 2017

The story seems strongly reminiscent of one told by Paul Garrett in his notes on cuspforms (section 0.3 at http://www-users.math.umn.edu/~garrett/m/v/pseudo-cuspforms.pdf). In both cases, the zeta zeros arise as eigenvalues of some spectral problem which, alas, is not self-adjoint and therefore can't imply RH. Additionally, the problem raised by both Garrett and the linked answer at MathStackExchange is the existence of certain Dirichlet L-series which are known to not have Euler products.

8. **Doug McDonald**
April 6, 2017

Much of my research (I'm now happily retired and doing genetic genealogy) centered on quantum chaotic behavior of vibrations of molecules in size from acetaldehyde to cyclohexylaniline.
The statement quoted "The advantage of this approach would be that the huge number of ζ(s) zeros are known and quick numerical algorithms have also been developed to find further zeros, thus solving the Schrodinger equation for large energies would be unnecessary. The Riemann zeta function could play the same role in the examination of chaotic quantum systems as the harmonic oscillator does for integrable quantum systems." is interesting. However, we eventually found that no "toy" or "effective" theory ever agreed with experiment. Only very large (for their day) direct diagonalizations of the Schrodinger equation using very high order fits of potential surfaces to quantum electronic calculations did. The exact nature of the potential energy surface really mattered.

I would be astounded if this were not still true today.

9. **tomate**
April 7, 2017

The problem with this post is that it's not clear whether it's criticizing the media coverage of this paper (and of course we all agree on this), the paper itself, or the fact that it is published in PRL. After reading the post I went to the paper assuming to find the usual crazy bullshit that is usually criticized here, and instead I found a very decent paper, with a proper title, no malicious claims in the abstract, very well written and direct, and it connects to a previous line of research on the topic. So the question boils down to "is it worth PRL?", and frankly there is so much crap in PRL in recent times, this paper does not really stand out as inappropriate (and then the problem would be PRL's policies, and the good ol' times, not this paper).

Also, from the comment of your anonymous colleague mathematician I was expecting a very bizarre and ad hoc boundary condition on the eigenfunctions that would involve the zeta function itself. It turns out, instead, that the shape of the operator, the boundary condition, and the condition that the operator has to satisfy in order for the RH to hold are extremely simple. I don't think this contribution is more "circular" than are the other zillion ways to reframe the problem you mentioned, including those of the superstars in the field, one of which might eventually turn out to be the best at attacking a proof, and you never know which one it is until you got it. And after all, given your favourite set of axioms, in this sense all of mathematics is "circular".

10. **V.K.**
April 7, 2017

Jean Bellissard has weighed in on the Math Stackexchange you linked. The conclusion is devastating:

“In conclusion, the sloppyness of the definitions used but the authors leads to a complete mess. Nothing is correct in this paper.”

11. **Frank Wilhoit**
April 7, 2017

tomate:
The presence of any quantity of crap in any particular venue is NOT an argument for the addition of more.

12. **Peter Woit**
April 7, 2017

tomate,
The intent of the post was to supplement what was in the Quanta article with my take on what this looks like from the point of view of the mathematics community, something which was lacking in the article, and in all coverage of this I’d seen elsewhere. I’m not one for insisting on rigid distinctions between what is math and what is physics, but this is a case where the general motivating problem is purely mathematical, the specific questions being pursued are mathematical, and the techniques being used are mathematical (rather than physical). Why don’t the authors engage significantly with the mathematics community and its expertise on these questions and techniques? I haven’t seen anyone address this, and I suspect that many physicists don’t understand the question, because they don’t understand what mathematicians do.

The Belissard comment on math stackexchange
http://math.stackexchange.com/questions/2211278/riemann-hypothesis-is-bender-brody-m%c3%bcller-hamiltonian-a-new-line-of-attack/2222146#2222146
I think shows well what kinds of concerns this work raises with experts, why don’t the authors engage with them?

13. **tomate**
April 7, 2017

Peter and Frank,

if I understand well the Belissard argument only came up yesterday, so we cannot charge the authors of not responding to this yet. If they do it seriously, it might take several months. If a lot of such arguments show up, and a discussion starts, then the paper has served its purpose. That real knowledge does not proceed through peer-reviewed journals is a matter of fact.

Let me point out that I do not personally care at all about this paper, I have no expertise on either the RH or on advanced techniques in QM, though I am a fond amateur on both. But I like new ideas wherever they show up and I find it fascinating that often physics becomes the language by which certain branches of mathematics are inspected in a more speculative and fuzzy way, at times when so much mathematics is just a tedious and fancy-less job about defining things properly and extending by an epsilon the validity of older theorems (not everybody out there is a Connes or a Grothendieck, you know…). Despite Belissard’s argument, which is quite technical, I am quite convinced that there might be a way a more creative mathematician might make sense of these arguments.

The problem with PRL, and with publishing in general, is certainly not that these journals publish too many ideas that are out of their scope, but rather that they publish too much “socially acceptable” stuff that is well within the mainstream, maybe a tiny bit more clever than others, and that can gather a lot of consensus just because they are supported by well-established and extremely closed communities. When this happens, you see popping up a lot of very dishonest titles and abstracts, supported by unclear arguments. And this is the same in physics and in math (and I do read a lot of math papers, though in other areas).

To me, this is not the case with this paper, which I find quite honest in its assumptions and scope. I don’t like Peter’s argument that the authors did not interface with the proper community because it sounds like the rooking of a community that does not want to have too much interference with its own business. In this respect, I would propose an “evolutionary” argument: If so much great math has failed on the RH, then probably time is ripe to NOT get too much intertwined with all the mental process that got that math stuck.

14. **Peter Woit**
April 7, 2017

tomate,
I don’t think it’s a good way to do science to publish a paper in a high profile journal, and only then have experts in the subject point out well-known, serious and basic problems with what the authors are doing. Among other things, note that Belissard’s comments are probably not going to be seen by readers of the PRL paper or the arXiv preprint (as for my commentary and links, the arXiv bans trackbacks to this blog because I long ago offended certain powerful people in the physics community).

Sorry, but the attitude that you’re going to not bother to learn standard facts and acquire expertise about the topic you are doing research on, but dismiss it as “just a tedious and fancy-less job about defining things properly and extending by an epsilon” of no comparison to the power of your untutored physical intuition, is an attitude that, when there is physics involved, isn’t likely to help you get anywhere, and when there is no physics involved (as here), it’s sure not to.

15. **Maximillian G. Tresmond, Esq.**
April 7, 2017

Phys.org has also “covered” this today here: https://phys.org/news/2017-04-insight-math-million-dollar-problem-riemann.html with all the same problems Dr. Woit covered in the above-captioned post.

“I don’t think it’s a good way to do science to publish a paper in a high profile journal, and only then have experts in the subject point out well-known, serious and basic problems with what the authors are doing.”

This. I wish more academics would heed these wise words because virtually every academic field suffers from this problem. It’s beginning to impeach the credibility of academia which is the last thing this country needs right now.

16. **Anonymous**
April 9, 2017

As somebody who works on the zeta function for a living (and of course other closely related objects), I could see right away that this article is not bringing anything to the table… (Un?)fortunately mathematicians do not get to make a splash by putting forward sloppy, repetitive, not well researched approaches. I think that the authors of this paper will now be able to put in their grants that their work was advertised in Quanta, Nature, etc. and is therefore important. This incentivizes such behaviour, which is dangerous for the future of science, as it turns the objective from advancing research to making a splash with controversial (but ultimately vapid) claims.

P.S: on the analytic side, there are no new ideas in their paper, the formal inversion of the differencing operator was already known to Euler, this is how he derived the Euler Maclaurin formula, from which one can also obtain the analytic continuation of zeta. This obviously involves Bernoulli numbers and their generating function, around which, coincidentally the authors argument revolves.

17. **anon**
April 9, 2017

Articles in Quanta Magazine are usually quite good, but they do seem to be very fond of stories where some outsider(s) solve problems that have stumped specialists.

18. **Andre LeClair**
April 10, 2017

I basically agree with the points Peter is making. But I don't fault the authors. This kind of paper is essentially impossible to publish in a pure math journal. And for a problem of this magnitude, collaboration with mathematicians is difficult because they tend to be dismissive, often for good reason. The issue of rigor is important, but only a part of the problem. I think they would pay attention to a truly novel idea. Publishing in PRL for this kind of problem almost ensures that mathematicians will not pay much attention to it, which may be justified; I prefer not to weigh in on that.

Having worked on this problem over several years, I remain skeptical about the whole Hilbert-Polya idea. It's been a bit of a pipe dream, although not ruled out. It is well-known that the statistics of the zeros is conjectured to be described by random matrix theory (Montgomery). This motivated Berry-Keating in their interesting work, which did not conclusively resolve the problem. My own point of view is that the connection with random matrix theory would seem to cast doubts on Hilbert-Polya rather than support it. It would seem unlikely that a simple, non-random hamiltonian would capture the exact zeros, but that is not a rigorous counter-argument. Success would be analogous to being given an infinite list of essentially random eigenvalues and trying to reconstruct the operator that led to them. Heuristically speaking, the "randomness" of the zeros is related to the pseudo-randomness of the primes themselves. I agree with previous remarks that the fact that no deep aspects of number theory enter into their approach makes one wonder. As Peter's math colleague wrote, it appears that the construction of the model brings in too much information about the zeta function itself, via the boundary condition, so that perhaps the RH is built into the model from the beginning.

19. **Abdelmalek Abdesselam**
April 19, 2017

Peter,

It might be useful putting another update to your post since one of the authors of the PRL posted a reply on arXiv https://arxiv.org/abs/1704.04705

Malek

20. **Peter Woit**
April 19, 2017

Abdelmalek Abdesselam,

Thanks, I will add that. Glad to see this sort of thing, with precise statements so that one can see what exactly is going on.

The Social Bubble of Physics

April 7, 2017
Categories: Uncategorized

Sabine Hossenfelder is on a tear this week, with two excellent and highly provocative pieces about research practice in theoretical physics, a topic on which she has become the field's most perceptive critic.

The first is in this month's Nature Physics, entitled [Science needs reason to be trusted](). I'll quote fairly extensively so that you get the gist of her argument:

> But we have a crisis of an entirely different sort: we produce a huge amount of new theories and yet none of them is ever empirically confirmed. Let's call it the overproduction crisis. We use the approved methods of our field, see they don't work, but don't draw consequences. Like a fly hitting the window pane, we repeat ourselves over and over again, expecting different results.
>
> Some of my colleagues will disagree we have a crisis. They'll tell you that we have made great progress in the past few decades (despite nothing coming out of it), and that it's normal for progress to slow down as a field matures — this isn't the eighteenth century, and finding fundamentally new physics today isn't as simple as it used to be. Fair enough. But my issue isn't the snail's pace of progress per se, it's that the current practices in theory development signal a failure of the scientific method…
>
> If scientists are selectively exposed to information from likeminded peers, if they are punished for not attracting enough attention, if they face hurdles to leave a research area when its promise declines, they can't be counted on to be objective. That's the situation we're in today — and we have accepted it.
>
> To me, our inability — or maybe even unwillingness — to limit the influence of social and cognitive biases in scientific communities is a serious systemic failure. We don't protect the values of our discipline. The only response I see are attempts to blame others: funding agencies, higher education administrators or policy makers. But none of these parties is interested in wasting money on useless research. They rely on us, the scientists, to tell them how science works.
>
> I offered examples for the missing self-correction from my own discipline. It seems reasonable that social dynamics is more influential in areas starved of data, so the foundations of physics are probably an extreme case. But at its root, the problem affects all scientific communities. Last year, the Brexit campaign and the US presidential campaign showed us what post-factual politics looks like — a development that must be utterly disturbing for anyone with a background in science. Ignoring facts is futile. But we too are ignoring the facts: there's no evidence that intelligence provides immunity against social and cognitive biases, so their presence must be our default assumption…
>
> Scientific communities have changed dramatically in the past few decades. There are more of us, we collaborate more, and we share more information than ever before. All this amplifies social feedback, and it's naive to believe that when our communities change we don't have to update our methods too.
>
> How can we blame the public for being misinformed because they live in social bubbles if we're guilty of it too?

There's a lot of food for thought in the whole article, and it raises the important question of why the now long-standing dysfunctional situation in the field is not being widely acknowledged or addressed.

For some commentary on one aspect of the article by Chad Orzel, see [here]().

On top of this, [yesterday's blog entry at Backreaction]() was a good explanation of the black hole information paradox, coupled with an excellent sociological discussion of why this has become a topic occupying a large number of researchers. That a large number of people are working on something and they show no signs of finding anything that looks interesting has seemed to me a good reason to not pay much attention, so that's why I'm not that well-informed about exactly what has been going on in this subject. When I have thought about it, it seemed to me that there was no way to make the problem well-defined as long as one lacks a good theory of quantized space-time degrees of freedom that would tell one what was going on at the singularity and at the end-point of black hole evaporation.

Hossenfelder describes the idea that what happens at the singularity is the answer to the "paradox" as the "obvious solution". Her take on why it's not conventional wisdom is provocative:

> What happened, to make a long story short, is that Lenny Susskind wrote a dismissive paper about the idea that information is kept in black holes until late. This dismissal gave everybody else the opportunity to claim that the obvious solution doesn't work and to henceforth produce endless amounts of papers on other speculations.
>
> Excuse the cynicism, but that's my take on the situation. I'll even admit having contributed to the paper pile because that's how academia works. I too have to make a living somehow.
>
> So that's the other reason why physicists worry so much about the black hole information loss problem: Because it's speculation unconstrained by data, it's easy to write papers about it, and there are so many people working on it that citations aren't hard to come by either.

I hope this second piece too will generate some interesting debate within the field.

Note: It took about 5 minutes for this posting to attract people who want to argue about Brexit or the political situation in the US. Please don't do this, any attempts to turn the discussion to those topics will be ruthlessly deleted.

Comments

1. **Bernardo**
   April 7, 2017

   If this type of social dynamics is happening in Physics, where any given direction of research is at least driven by the goal of saying something about reality and is expected to be tested in experiments, imagine how bad the situation may be in Mathematics? It is possible that Personalities and social dynamics influence the evolution of math to such extent that, under a small perturbation, the whole field would be unrecognizable. Also, what if all math connected to this field of theoretical physics is a waste of time and, in the long run, will become completely irrelevant?

2. **Peter Woit**
   April 7, 2017

   Bernardo,

Mathematics has never been driven by experiment, so has evolved its own different standards and practices which have allowed it to make (and continue to make, unlike physics) great progress. This posting is about the situation in physics, but an interesting question I've often raised is whether physics, as it finds itself starved of the new experimental results that in the past kept it moving forward, might learn something from mathematics about how to cope. Unfortunately, this is one topic that Hossenfelder doesn't address.

3. **Bernardo**
April 7, 2017

Much if not most of mathematics is in some sense driven by applications if not experiments in the sense of discovering something new about nature. (One example is the work on wavelets for which the latest Abel prize was awarded, but of course there are many other examples.) I guess what I meant was that the situation in theoretical physics that was described in your post is the field running into a hard wall and still allowing social dynamics determine its development, while in mathematics there is no such hard wall. I grant you that mathematics keeps making great progress, but these parallels with physics makes me wonder how different mathematics would look like under different circumstances. But I wonder what you think physics can learn from mathematics?

4. **Peter Woit**
April 7, 2017

Bernardo,
I've often written about this, with one of the most important things I think mathematician's insistence on being clear about precisely what you understand and what you don't. If you don't have experiment to keep you honest, the field needs strong internal norms to keep clear the difference between what works and what doesn't (and strong internal norms to keep people honest in their evaluations of what is working and what isn't). How to change the culture of physics to move in this direction is a difficult question, especially since my impression is that most physicists at this point do not see this as a problem.

5. **G. S.**
April 7, 2017

I don't know if it's fair to generalize the problems of theoretical physics to being problems of all scientific disciplines.

Sabine's article mentions a problem common to all fields of science (reproducibility – which she admits is being addressed by the scientific community), but then pivots to the problems particular to theoretical physics. What other fields have these same problems of theoretical physics?

She mentions that "at its root, the problem affects all scientific communities", but then chooses to give an example from politics rather than from another scientific discipline.

Certainly the soft sciences (e.g., economics, sociology, etc.) have examples of theories that will not die despite a lack of testable predictions. Heck, economics is rife in theories that survive despite clear contradictory evidence (e.g., strong forms of the efficient market hypothesis).

But what about the other hard sciences? Are there similar problems in biology, chemistry, geology, meteorology, botany, engineering, computer science, material science, etc.? If so, what are they? What are the niches of those fields in which social pressure is resulting in deviation from the scientific method?

They may exist. But until the examples are given, the article would more accurately be titled "Theoretical Physicists need reason to be trusted". The generalization to all sciences is unfair.

6. **Bernardo**
April 7, 2017

Peter,
In my opinion, if physicists will try to be more like mathematicians, something precious will be lost. You sometimes must have this attitude to solve the problem by any means necessary to get to the answer, even if it takes a few more decades to make sense out of it.

7. **Peter Woit**
April 7, 2017

Bernardo,
Don't worry, based on my experience, there is no danger that theoretical physicists will take an interest anytime soon in behaving more like mathematicians and paying serious attention to the difference between what they understand and what they don't.

8. **CIP**
April 7, 2017

Particle physics today has some parallels with atomic physics in ancient Greek thought, or medicine in the Middle Ages. There just isn't good new data. I'm not sure there is a cure in our immediate future.

9. **Klavs Hansen**
April 7, 2017

It is somewhat disturbing to see physics identified with the relatively narrow field of high energy physics theory. There are a number of other disciplines that produce vast amounts of results continuously, and to identify any crisis in physics, the symptoms must be shown by at least a couple more of these fields. I don't think they are. Although, of course, a lot of the low-hanging fruit of previous centuries has been picked, and everybody finds it more difficult to discover fundamental laws of nature.

10. **neil**
April 7, 2017

Bee describes the results but not the reasons. The problem is extreme in HEP physics because the cost of discovering new experimental results has risen exponentially relative to the cost of producing new theoretical results. In other sciences, and even other fields of physics, this has not happened to the same extent. Theory is relatively cheap (paper, pencils and, hopefully, wastebaskets) so we get a lot of it, often unrelated to our ability to confirm or refute it.

11. **Wyman**
April 7, 2017

I strongly agree with Klavs Hansen here. Theoretical high-energy/particle/fundamental physics only represents a small fraction of theoretical physics as a whole. I would say that in condensed matter, there are far more open problems than there are theorists. In the past couple of decades we've seen the emergence of both highly computational condensed matter physics as well as a new focus on topological properties that has by now reached every corner of study. It's a genuinely exciting time to be a theorist working on condensed matter systems — we have so many questions and so many new tools!

12. **Anonymous**
April 7, 2017

Producing papers is part of physicist job. This is how they get paid for. Whether or not their theory can be tested is not important.

13. **Bee**
April 8, 2017

Thanks for the mention 🙂

14. **Bee**
April 8, 2017

Btw, the title of the Nature Physics comment was chosen by the editor.

@Wyman

Indeed, I have even collected some numbers here

http://backreaction.blogspot.com/2013/05/what-do-most-physicists-work-on.html

As I point out in my article, the overproduction problem is specific to the foundations of physics. But it's a symptom of a much wider problem, which is that scientists are blind to the problems in the academic system.

@neil

I mentioned this in my article, and I also explain why this argument is as tiresome as wrong.

15. **Bunsen Burner**
April 8, 2017

How in the world did the problems of physicists representing less than 1% of all physics become a crisis for the whole discipline. I see no crisis in quantum computation, condensed matter, stellar interiors, computational fluid dynamics, high precision astronomy, and thousands of other topics all more worthwhile than quantum gravity.

If the last 20 years represent a crisis in physics, then my answer is: Please God, let the crisis continue.

16. **Peter Morgan**
April 8, 2017

The foundational problems that Physics has are not limited to HEP. One of the consequences of the lack of foundational clarity in QM and of mathematical clarity in QFT is that condensed matter and other users of QM/QFT have become rather a hodgepodge of approaches. There are some patterns, which people have become used to thinking are good enough, but things could be better organized. One can read Bee's lament as about HEP becoming more hodgepodge-like, like condensed matter, etc.

17. **M**
April 8, 2017

Wyman and Burner, can you please give examples of the most interesting discoveries recently made in condensed matter and quantum computation? It seems to me that these fields have their own problems: the only future of condensed matter seems doing more complicated versions of what was understood decades ago. And quantum computation looks like a bubble, like string theory 20 years ago.

18. **Low Math, Meekly Interacting**
April 8, 2017

To be fair, the life sciences have revealed a kind if evil twin of this situation: when you are literally awash in data, when doing experiments is relatively easy because of the nearly endless array of possibilities, the result can be a vacuum almost as profound. I say "almost" because, while someone can always produce an unimportant piece of crap and boost their citation count without penalty, most truly important crap invites rapid and widespread experimental challenge. Stimulus-triggered pluripotency is a good case study of how that situation plays out. Barring those corrective reflexes, the field would be a cesspool of trivial fraud. There is simply no substitute for the ability to subject scientific hypotheses to reproducible observational challenge. Never was, never will be, ever. Humanity has proven this time and again, and the intelligence and integrity of individuals is clearly no defense against societal pressures. To the extent we surrender to the supremacy of objective reality, we make progress. To the extent we're disconnected from it, we inevitably lose our way.

19. **Bunsen Burner**
April 8, 2017

M. Seriously? Condensed matter is stagnant and quantum computation is a bubble? Perhaps rather than pollute Peter's blog with the back and forth of examples that I'm sure will never convince you, why not try asking your question on a blog by an expert in one of these fields – such as Scott Aaronson.

20. **Nick M.**
April 8, 2017

Peter Woit wrote:

April 7, 2017 at 5:54 pm

> "… This posting is about the situation in physics, but an interesting question I've often raised is whether physics, as it finds itself starved of the new experimental results that in the past kept it moving forward, might learn something from mathematics about how to cope. Unfortunately, this is one topic that Hossenfelder doesn't address."

Hi Peter! I believe that Sabine Hossenfelder may have touched upon this very topic in a blog article (dated December 8, 2016) from her "Backreaction" website titled

No, physicists have no fear of math. But they should have more respect.

Although the front portion of this article deals with an entirely different issue, the "But they [*physicists*] should have more respect [*for math*]" portion addresses the topic that you expressed concern about.

If I may quote the last few relevant paragraphs from Sabine's blog entry:

> So, I don't think physicists are afraid of math. Indeed, it sometimes worries me how much and how uncritically they love math.
>
> Math can do a lot of things for you, but in the end it's merely a device to derive consequences from assumptions. Physics isn't math, however, and physics papers don't work by theorems and proofs. Theoretical physicists pride themselves on their intuition and frequently take the freedom to shortcut mathematical proofs by drawing on experience. This, however, amounts to making additional assumptions, for example that a certain relation holds or an expansion is well-defined.
>
> That works well as long as these assumptions are used to arrive at testable predictions. In that case it matters only if the theory works, and the mathematical rigor can well be left to mathematical physicists for clean-up, which is how things went historically.
>
> But today in the foundations of physics, theory-development proceeds largely without experimental feedback. In such cases, keeping track of assumptions is crucial – otherwise it becomes impossible to tell what really follows from what. Or, I should say, it *would* be crucial because theoretical physicists are bad at this.
>
> The result is that some research areas can amass loosely connected arguments that follow from a set of assumptions that aren't written down anywhere. This might result in an entirely self-consistent construction and yet not have anything to do with reality. If the underlying assumptions aren't written down anywhere, the result is conceptual mud in which case we can't tell philosophy from mathematics.
>
> One such unwritten assumption that is widely used, for example, is the absence of finetuning or that a physical theory be "natural." This assumption isn't supported by evidence and it can't be mathematically derived. Hence, it should be treated as a hypothesis – but that isn't happening because the assumption itself isn't recognized for what it is.
>
> Another unwritten assumption is that more fundamental theories should somehow be simpler. This is reflected for example in the belief that the gauge couplings of the standard model should meet in one point. That's an assumption; it isn't supported by evidence. And yet it's not treated as a hypothesis but as a guide to theory-development.
>
> And all presently existing research on the quantization of gravity rests on the assumption that quantum theory itself remains unmodified at short distance scales. This is another assumption that isn't written down anywhere. Should that turn out to be not true, decades of research will have been useless.
>
> In lack of experimental guidance, what we need in the foundations of physics is conceptual clarity. We need rigorous math, not claims to experience, intuition, and aesthetic appeal. Don't be afraid, but we need more math.

As usual, very nicely said Sabine!

21. **Geoffrey Dixon**
April 8, 2017

Forbes just published this article:
https://www.forbes.com/sites/chadorzel/2017/04/06/why-are-there-too-many-papers-in-theoretical-physics/#68ee330d37ee
Gist:
1. There are too many theoretical particle physicists fearful of publishing anything that might jeopardize their careers;
2. There is a dearth of high sigma experimental results that could lead to "new" physics;
3. Therefore, anytime there is a glitch in some experimental result, there is a stampede of publications attempting to explain it;
4. When glitch is fixed (shown to be statistically insignificant), no one retracts or feels embarrassed by their publication(s), for everyone does it. It is socially acceptable.

I could go on and on, but I already have:

https://www.amazon.com/Fire-Night-Functioning-Maverick-Theoretical/dp/154305644X

Pay particular attention to everything after chapter, "Imagine you're a dolphin".

22. **Another Anon**
April 8, 2017

Wyman, Bunsen Burner, and all the rest of you condensed matter physicists, and fluid dynamicists, and all of you getting annoyed because you don't see a problem – seriously, this isn't about you. It's a problem with fundamental physics.

It really bugs me when people say there's no problem in physics because they've just invented a new transistor. We are talking *fundamental physics*. Please!

23. **AnonNth**
April 8, 2017

Another Anon, the problem isn't that condensed matter physicists do not see a problem with fundamental physics, but rather that high energy physicist are becoming obsolete because of the great success that condensed matter physicists are having solving fundamental AND high energy physics problems with the far more experimentally accessible mathematical and theoretical machinery of condensed matter physics.

String theorists in particular, are having a hard time saying that they just plain wrong, and seem to be unable to cope with condensed matter theory stealing their glory..

24. **Peter Woit**
April 8, 2017

All,
Will delete any more arguments between fields of physics. Yes, the problem at issue is one of certain specific fields, not all of physics, and not all of science.

Interestingly, no one seems to deny that that there is a problem…

25. **cthulhu**
April 8, 2017

G.S. asks about other fields including engineering. I'm an engineer specializing in aircraft flight control, an area which has been one of the two primary drivers of control theory over the last 50 years (the other being industrial process control). As we have pushed our aircraft designs to deliver more capabilities in numerous areas, we have demanded much more of the flight control systems, and there has developed a huge interest in design tools that give us guarantees about stability and robustness to uncertainties.

I won't go into details but will state without offering proof that these techniques have turned out to be difficult to apply in the real world, mask much of our physical understanding of what the so-called bare airframe (the aircraft without any active controls) is doing, and have over the years led to several expensive redesigns on multiple programs, affecting both investors' and taxpayers' pocket books in multiple countries; the redesigns have usually been done primarily with techniques that, while perhaps less mathematically elegant, have the distinct virtue that they have been shown to work on countless prior programs. The "modern" techniques have found a role in the analysis side of things, helping us understand the limits of our designs, and this is a valuable thing to have. Mistakes in the flight control design are probably the easiest way to cause a catastrophic accident short of pilot error.

So, my answer to the question is that yes, at least some branches of engineering have been afflicted with paths that over-promise and under-deliver, but at least for aircraft flight control we still have the ultimate Diogenes to keep us honest: does it actually fly or not?

26. **Ted**
April 8, 2017

Perhaps the best way to change the sociology/mechanics of academia is to pave a new route for the people entering the field. As a student who has been searching for a PhD research direction with promise in fundamental physics, its a depressing situation. The obscene amount of research, coupled with no significant results coming out for decades is clearly the symptom of a system that needs to start supporting academics (especially new ones) to explore new directions – and to question why our current ones aren't working. Strongly agree with what was said in the quote in the comment above – "In lack of experimental guidance, what we need in the foundations of physics is conceptual clarity".

27. **Thomas**
April 9, 2017

Peter, Bee,

the lack of new and interesting data and the overproduction of speculations, both in high-energy physics and quantum gravity, could be due to a simple reason:

Maybe there is nothing to be discovered any more in these two fields?

We all know that such a statement has been made repeatedly in the past, and it has always been wrong. But could it be correct this time? This is the touchy issue swept under the rug in all these discussions. This is the touchy issue avoided like the plague by everybody. This is the touchy issue that led to string theory, loop quantum gravity and other speculations not related to experiment but with a large number of followers.

We need an objective debate of this touchy fundamental issue, not a discussion of its side effects. Why is there no such debate? Just look at the answers to such a proposal. You will see ridicule, contempt or aggression – but no objective debate.

The world is full of smart physicists who have been trained and imbued with the conviction that there is still a lot to discover. Maybe this conviction is wrong?

28. **GoletaBeach**
April 9, 2017

Burt Richter's 1999 closing talk at the Lepton Photon discussed these issues before… https://arxiv.org/abs/hep-ex/0001012 . His talk itself used to be online… now 404. In person he was substantially more caustic toward the theory community.

"There are more of us, we collaborate more, and we share more information than ever before."

I'm not at all sure that there are more experimentalists since the 1980s… hard to tell because so many leave after PhD and postdoc… lately for high-paying data science jobs.
In the late 1990's for all sorts of telecom jobs. And a constant move to Wall Street.

Einstein did synthesize GR from only foundational notions… current theorists keep trying to pull a similar rabbit out of a hat. Somehow I don't find it surprising that all the social techniques familiar for millennia to define the "in crowd", "the outsiders", Cassandras, rebels, syncophants, etc are in play in theoretical particle physics. It is a social activity.

With luck, though, out of all the dung a really delicious mushroom might sprout.

29. **Peter Woit**
April 9, 2017

All,

I hope people will actually address the issues discussed by Sabine Hossenfelder, since I think they're interesting, and a serious discussion of what might be done to address them is important and desperately needed.

30. **Jeffrey**
April 9, 2017

Six hundred papers interpreting what turned out to be a statistical fluke? From what I gather it is irrelevant. They are getting direct deposits in their bank account anyways, why change? Who cares if was a fluke? Seriously, they could have known it was a fluke anyways, and said, "hey, we know this result is nonsense, lets publish something anyways because we need to get paid."

It would be win-win. They write up some interpretations, they get paid, the results come out as being a fluke, then they can blame the fluke on the machine. Easy. As a matter of fact it would be best for the machine to make mistakes and to have issues, and be so big that parts randomly break at intervals. Job security forever!

Sabine does not mention money in the article. Sabine can sit here and talk all day, and write up all the interesting social bubble stuff, but unless the cash stops flowing, then nothing is going to change.

31. **Tom Andersen**
April 9, 2017

Here is a situation that is happening today that is *new*. Instant worldwide intercommunication. Its of course great to have, but in theoretical physics it is also causing groupthink. No longer can a bright team work without interruption on a maverick idea for a year or three. It seems instead that papers are produced based on perceived consensus.

Without the magic of the internet and $500 long distance travel, the world of physics was different and more open to strange ideas a century ago.

32. **neil**
April 9, 2017

Why all the hand-wringing about the theorizing on the diphoton anomaly? Yes, it was low sigma but it was observed by both ATLAS and CMS. Given the importance of priority, which science has always respected, who wants to wait around for more data before trying to explain it? I don't see what harm was done.

33. **GoletaBeach**
April 9, 2017

Well, Peter, the amplification of social feedback in the theory community has widened the gulf between theory and experiment. Experimentalists just roll their eyes and when among themselves are so derogatory they make you look like a string-theory booster.

Perhaps every particle theory PhD should include a mandatory 1 year doing hardware shifts at the LHC, or on any big experimental effort.

34. **Scott Aaronson**
April 10, 2017

Hi Peter,

As someone who's often trying to get the straight dope about the topics she writes about, I appreciate Sabine's writing immensely—her bluntness, humor, and obvious relish puncturing hype strike a chord with me as they apparently do with you.

But regarding the information paradox, I think there's an actual technical point at issue, which creates a fork in the road before one enters into any questions of faddishness, sociology, etc. Namely: should the black hole entropy, which we presumably agree is governed by Bekenstein's bound, count the log of the number of orthogonal microstates? I think yes—I don't understand what entropy even MEANS if it's not counting microstates—while Sabine apparently thinks no. But crucially, if the answer is yes, then what Sabine advocates as the "obvious, inexplicably ignored" solution to the paradox—i.e., that all the information stays in the black hole, even as the latter gets microscopically tiny—is indeed ruled out. So then, unless we want to give up on unitary QM, or have the information escape through the singularity into a baby universe, as far as I can see we're indeed forced to consider the proposal that the infalling information is holographically encoded on or near the horizon, from where it can enter the radiation. So in that case, we don't need to offer a sociological explanation for why people remain so interested in the information paradox 40+ years after Hawking raised it: a scientific explanation suffices!

35. **Another Anon**
April 10, 2017

Peter will almost certainly delete my comment for being off-topic, but I agree with Scott in saying that – if the information does not escape as the black hole evaporates – how on earth can all the information that has ever fallen into a black hole be encoded into an progressively smaller, effectively infinitely small, region of space? Bee thinks the problem is solved if the singularity is removed. Surely not.

36. **spacetime**
April 10, 2017

I bit strange to read

"But we have a crisis of an entirely different sort: we produce a huge amount of new theories and yet none of them is ever empirically confirmed. Let's call it the overproduction crisis."

and

"I'll even admit having contributed to the paper pile because that's how academia works. I too have to make a living somehow."

37. **Anonyrat**
April 10, 2017

I think the "over-promise, under-deliver" problem is a bit different from the one Bee is talking about. In the information technology world, this "over-promise, under-deliver" is represented, e.g., by the Gartner Hype Cycle, the 2016 version of which can be viewed here (http://www.gartner.com/newsroom/id/3412017) (I am not affiliated with Gartner.)

Per Gartner's analysts, there is a natural progress curve, on top of which is added inflated expectations due to hype. Hype is driven by marketing, confirmation bias, novelty preference, social contagion, competitive pressure, irrational exuberance and (positively) overcoming inertia and imagination. The dangers of not understanding the hype cycle for corporations is they adopt technology too early, give up too soon on a technology, adopt too late or hang on too long to old technology. Understanding the hype cycle also opens up opportunities, which Gartner lists in some of their publications, that I won't repeat here.

The problem Bee describes has some factors in common with the technology hype cycle, such as social contagion and competitive pressure (though it is academic competition rather than commercial competition). Novelty preference may also play a role in the fad-driven research cycle of fundamental physics.

38. **Peter Woit**
April 10, 2017

Scott,
I see Sabine has responded to this on her blog, pointing to
http://backreaction.blogspot.de/2012/07/bekenstein-hawking-entropy-strong-and.html

First I should make clear that I haven't thought much about this, partly because of a personal allergy to spending a lot of time on a topic that many smart people have been active in for a long time, producing lots of papers, but no obvious progress. One aspect of this is that as this kind of thing goes on for decades, the barrier to entry to someone like me who likes to first try and understand what others have done becomes higher and higher.

As a graduate student nearly 40 years ago I spent a fair amount of time learning about Hawking radiation and was aware of the paradox, but it seemed that the framework of what was understood was not rigid enough to address the issue (in part because it was silent about "what is going on at the singularity?"). Over the years I've made periodic attempts to read up on the latest on this, but my impression has always been that this situation hasn't really changed. Yes, you can make various assumptions about what entropy is in this situation, about the Bekenstein bound, or about what "holography" is and what it implies here, and you can derive various consequences from various assumptions. But no compelling picture seems to emerge from this activity. Given this, there's a real sociological question about why this is such a popular thing to be doing. I guess the best argument for it is not that anyone is going to resolve the issue, but that thinking about this problem may lead people to something more fruitful.

Another Anon,

I just don't see how arguments like "how on earth can all the information that has ever fallen into a black hole be encoded into an progressively smaller, effectively infinitely small, region of space?" can resolve anything. At the crudest level, the singularity is a place that we don't understand, where everything has gone. Why shouldn't the answer to the paradox be there rather than at the event horizon? But my question is just as rhetorical and unscientific as yours. You need a well-defined theory to answer such questions, and that doesn't seem to exist.

39. **Lee Smolin**
    April 10, 2017

    Dear Scott and Peter,

    Sabine and I discussed in detail the issue of the meaning of the BH entropy in our arXiv:0901.3156. We argue there that the original and standard argument from Bekenstein only supports a weak form of the BH entropy which is that the surface area is a measure of the capacity of the black hole horizon as a channel for the transmission of information. The strong form, that you name, that the BH entropy is the log of the number of microstates of the interior of the horizon, is an unjustified extrapolation.

    Regarding the size of the interior of the horizon, it is understood since the early days of the subject that the volume of the interior of the horizon can be very large compared to the area, this is the so-called "bag of gold." We discuss in detail other objections and issues such as those having to do with remnants. In addition, note that there areworked out examples where the singularity is removed leading to a resolution of the paradox: Ashtekar et al's study of CGHS black holes: arXiv:0801.1811. and by Rovelli et al, Planck stars.

    The weak and strong form of the BH entropy then imply weak and strong forms for the holographic principle, as discussed in hep-th/0003056.

    Let me put the issue simply: The "conservative solution" that quantum gravity effects eliminate the singularity does indeed resolve the information paradox, as well as related puzzles such as the firewall paradox. Further, this requires no modification of standard physics at the horizon, no modification of the principle of unitarity, and no radical non-locality, but only modifies physics at singularities, where we already know we need new physics. Given this, I would think we need an extraordinary motivation to ignore this conservative and natural solution in favour of speculations that impose radical new physics on the horizon, where the curvature may be very weak.

    Thanks,

    Lee

40. **TVR**
    April 10, 2017

    If you assume that singularity forms in gravitational collapse, then you have to trace out the modes that presumably end up in the singularity and keep only the outgoing modes. That is the origin of the information loss paradox. You remove part of the original state, and then obviously something is missing, i.e. the density matrix does not describe a pure state.

    If there is no singularity, then you do not trace out any modes. Then this paper Phys.Rev.Lett. 114 (2015) no.11, 111301 practically answers the question. Most of information is not in Hawking quanta, but actually in subtle correlations between them. These correlations are described by the off-diagonal elements of the density matrix. They start off very small at the beginning, but grow as the Hawking evaporation progresses. So "information" is not released at the very end where you need Planckian physics. These correlations are able to restore unitarity. The whole process is unitary all the time, but an observer collecting only the Hawking quanta might have an impression that the information is lost.

    Note that the singularity and event horizon are much related notions. If there is no singularity, the horizon cannot be a global event horizon. If singularity is replaced by the normal region of strong gravitational fields, once enough mass is lost to evaporation, anything that was trapped inside must be released. An apparent horizon might exists for a while, but it will disappear at the end. No remnants with huge amount of "information" in them are needed.

41. **neil**
    April 10, 2017

    Does anyone know whether the Event Horizon Telescope will be able to exclude the "information released early" (firewall) possibility?

42. **reader**
    April 10, 2017

    Lee,

    This proposal has features that make it seem not so conservative after all. As is well known, if the thermality of the radiation only breaks down once the black hole becomes of Planckian mass, then the remaining object must decay over a very long timescale for its radiation to purify the full state. Indeed this timescale can be made arbitrarily long by increasing the mass of the original black hole. So this scenario requires very long lived remnants. More seriously perhaps, it seems impossible to make this work for a black hole formed in AdS. Putting aside anything to do with AdS/CFT, AdS acts like a finite sized box, so a state of radiation of total energy M_Pl has a small maximum entropy. In this case there is no way for a Planckian remnant to decay into a gas of (arbitrarily) high entropy quanta whose state would purify that of the Hawking radiation emitted earlier. So while it is logically possible for black holes to release their information in Hawking radiation in asymptotically flat space (while only modifying Planck scale physics) this seems impossible in AdS. I find that highly problematic.

43. **Giotis**
    April 10, 2017

    "When I have thought about it, it seemed to me that there was no way to make the problem well-defined as long as one lacks a good theory of quantized space-time degrees of freedom that would tell one what was going on at the singularity and at the end-point of black hole evaporation."

    Actually is the other way around.

    Your attitude with respect to the information loss paradox suggests the kind of theorist you are.

    If you still believe that information is lost or in remnants, then almost certainly you are a GRist; if on the other hand you believe that the information escapes via the Hawking radiation and to complementarity then you are almost certainly a particle/String theorist.

If you believe in Mathur's fuzzballs then you are Mathur; LOL, that was a joke, actually fuzzballs have many supporters, far far more than those believing in remnants who are indeed a small minority nowadays.

44. **Peter Woit**
April 10, 2017

Giotis,
My attitude with respect to the information loss paradox is that I don't know the answer. Don't know what kind of physicist that makes, me, maybe it makes me a mathematician…

Your schema breaking up physicists into different tribes depending on their ideological beliefs about black holes is a scary vision of the postfactual future (or present?).

45. **Zack Yezek**
April 10, 2017

It appears that this malaise is really limited to 'fundamental' or 'high energy' physics, especially 'grand unified theories' or 'quantum gravity'. And there actually IS healthy progress there, if you consider things like neutrino physics to be a part of it.

After all, the null results of the past 40 years ARE significant. Arguably the non-detection of WIMPs & SUSY are our era's versions of the Michelson-Morley experiment. The problem isn't with theorists who knock off a new model that can explain tentative new experimental results, like the CERN 750 GeV one. No, the problem is much of this "theorizing" doesn't produce useful theories. Or at least it hasn't after 40 years of research, if you define a real theory as one that makes quantitative, falsifiable predictions. SUSY, String Theory, etc. don't do that. They come in so many distinct flavors, with so many free parameters, that they basically make no hard predictions and can evade any negative experimental result.

The real solution is to draw a hard line between mathematical frameworks, hypotheses, speculations, and genuine theories. This will let people work on speculative ideas or big frameworks like SUSY, but also make it clear that there is a qualitative difference between those and genuine theories of physics like General Relativity or QCD that quantitative, testable predictions. The expectation then should be that an idea must meet certain "meta" criteria to graduate from speculation to hypothesis, and then from hypothesis to theory. This system would also make it clear that comparing things like SUSY and GR is a category error, and allow them to be judged on different criteria. That way you can really enforce a necessary, fair rule like "If your hypothesis evolve into a testable theory within 40 years, we're no longer giving it research priority".

46. **Giotis**
April 11, 2017

Right or wrong the dichotomy is there, I didn't invent it.

Check around min 1:23:10 of this video and you will understand it within the context of information loss paradox resolution proposals and remnants in particular.

https://youtu.be/3EOpHHjv5g8?t=4991

47. **twistorial**
April 11, 2017

The notion that HEP should only be about finding *the* new model is an oversimplification, just as is the notion that the standard model is completely understood. There are several healthy communities working on understanding just standard model physics. This may even be the best bet of finding new physics, although it almost certainly would not be of the romantic smoking gun variety.

I agree that those areas of HEP that are in danger of forming tadpoles or vacuum bubbles in theory space should take a good hard look at themselves though. Proposed yardstick: articles starting with "recent progress…".

48. **Bernhard**
April 11, 2017

"The only response I see are attempts to blame others: funding agencies, .."

Well, meap culpa, as I'm one of those blaming "others" than. Funding agencies still determine everything. They determine which ideas are to be pursued or not and it is not as simple as saying that the reviewers are just scientists/ourselves. These agencies are more than the simple sum of the reviewers – there clear guidelines, written and not uncommonly unwritten in order to determine which persons and groups should get the funding, which ideas should be rewarded and which should be punished. Sorry to be simplistic here, but the issue 99% of the time boils to money. The reward idea pre-Higgs was the Higgs and SUSY now things are clearly shifting to dark matter. What if you're not interested in dark matter, Higgs or anything mainstream but still want to do conservative, sober physics? Sorry, but unless your CV is stellar, and sometimes even so, you're not getting the money and the following year you will obey. We all have mortgages to pay. The social problem with physics is that we cannot get rid of this model. I believe many of us would try crazier, harder things, that would take longer to publish if risking were to be rewarded. About the fact that "they don't to waste money on useless research" – define waste. Waste for them is something that attracts no attention – if something does attract attention than no matter how big the hype, it was all worth it. And what power do we actually have here (the cute idea that "they rely on us, the scientists, to tell them how science works")? We have zero power. This is a closed loop circle where people who were rewarded by this system end up being the future judges of it, doing all they can to keep the status quo. This kind of thing happens not only to theoriests, even experimentalists will be accused of having a "too narrow" program if what they're doing is not somehow connected to Higgs, SUSY or dark matter. Funding agencies taking a big chunk of the blame is the conservative answer to this problem she's failing to see.

49. **Peter Woit**
April 11, 2017

Bernhard,

It's true that changes in how decisions about funding are made could have large effects. I find it remarkable that, at least in the US, I'm unaware of any significant discussion at DOE or NSF about whether the problems of theoretical HEP physics could be addressed by changing in some way how the system of grants work.

I also haven't heard any news since December about any response to the "DOE Theory Letter" from a large fraction of US HEP theorists asking for an explanation of why grants to theorists have been cut. Unfortunately it may be that, absent any discussion of how to improve the situation, someone at the DOE has decided the thing to do is keep the system the same, just slowly defund it.

50. **X**
April 11, 2017

It has always seemed obvious to me why pure maths is not afflicted with the sociological issues discussed in the post. The reward system in maths incentivizes quality over quantity, i.e. working on hard problems that take a long time to solve rather than publishing lots of superficial 'me too' papers on the latest fad. The way it does this is through the existence of top quality journals. Publishing in one of those journals confers much greater reward than the average or mediocre journals. E.g. a single paper in Ann. Math. is worth more for a mathematician's career than countless publications in average journals. So pure mathematicians are incentivized to work on more difficult and deeper problems that can lead to publications in the top journals.

Compare this with the situation in physics. Actually HEP theory has its own unique reward system different from the rest of physics and academic science in general. In HEP theory, number of publications and citations is not actually all that important as long as the person has a respectable number of them. People don't get hired, get grants, or advance their careers by having more publications or citations than others. Instead, it all depends on how they are viewed by the 'important people' in the field. Doing good work will of course help them to be viewed positively, but it is not the only factor. In practice it matters a lot that a big shot in the field feels they have something personal at stake in whether the person succeeds or not.

This reward system in HEP theory incentives people to try to maximize the favorability of how they are viewed by the 'important people'. The first step is to get the attention of those people. This means working on whatever topic those people are working on, try to do PhD or postdocs at the institutions where those people work, etc. Obviously, people who work on other topics will have a hard time in this reward system. Jobwise they will have to try to survive on whatever scraps are left over after the 'favored' folks have been accommodated.

Note the difference with pure maths: journals play essentially no role in the reward system in HEP theory. In the major HEP journals, JHEP and Phys.Rev.D, papers making major advances are published side by side with superficial 'me too' papers. Quality control and standards for getting published are pretty minimal. So the journals that a HEP theorist publishes in says nothing about the quality of his/her work.
Even the supposedly top physics journal PRL has uneven quality in practice, and publishing in PRL will do nothing for a HEP theorist's career prospects. Without the backing of important people, PRL publications count for flat zero in HEP theory. And for someone who does have the backing of important people, it doesn't matter where they publish…

The different reward system in pure maths makes it possible for a mathematician to prove him/herself meritorious and worth supporting regardless of the topic he/she works on or whether he/she is known and viewed favorably by important mathematicians. It is enough that the person produces work of high enough quality to be published in a top maths journal such as Ann. Math. Then the career rewards will be conferred, regardless of other factors such as fashionability of the topic or how well connected the person is.
Working at a prestigious institution and having connections to important people will no doubt help a mathematician to have his/her papers taken seriously by top maths journals, but it is not essential. Unknown/unconnected mathematicians working on unfashionable topics can and do occasionally manage this too, and get the same career rewards. This possibility does not exist in HEP theory; it is simply not part of the reward system.

As for the rest of physics, and academic science in general, from what I've seen the reward system is based on a mixture of bean counting (number of publications and citations, h-index), status of the journals the person has published in (impact factors), and the views of important people in the field. At top universities the views of important people carry more weight, while at average research universities the bean count matters more. Hype and fashion seem to play a big role in getting papers published in the journals with high impact factors (Nature, Science, PNAS, PRL etc) – a very different situation that with the top maths journals. This reward system incentivizes academics to treat research as a video game where the objective is to maximize their score. This means do safe 'me too' research in hot fashionable areas that can lead to lots of publications and citations.

It seems pretty obvious that pure maths is the only field with a well-functioning reward system that creates good incentives, and I find it amusing how little interest there is in HEP theory or the rest of academic science in considering replicating the maths system in place of the obviously flawed existing systems.
One reason for this is obvious: the people who have risen to the top under the existing reward systems have no interest in replacing it with another system. They think the present system is fine – after all it allowed them to rise to the top so it must be a good one, right? 🙂
Since they are the ones running the show, there is no practical chance that things will change any time soon.

As for Bee's proposals for how to modify/improve the reward system (written on her blog, not so much in the Nature article), adding a bunch of additional metrics to assess people on along with the ones currently in use seems to me to just be more bean counting that won't change the "research as a video game" problem. People will game those metrics like they do with the current ones.

51. **Lee Smolin**
April 11, 2017

Dear Reader,

I am afraid I don't understand your logic. As we showed with Sabine in the article I mentioned, the arguments we were aware of against long lived remnants, are fallacious as they make unwarranted assumptions. This would apply also to black holes in AdS.

You assert, "Putting aside anything to do with AdS/CFT, AdS acts like a finite sized box, so a state of radiation of total energy M_Pl has a small maximum entropy." But this is contradicted by the possibility that small black holes have large interiors as in bag of gold solutions. You go on to conclude that , "there is no way for a Planckian remnant to decay into a gas of (arbitrarily) high entropy quanta whose state would purify that of the Hawking radiation emitted earlier. " Why? To the extent that the overall state is purified, the entropy is low. In any case there is no problem if the remnant doesn't decay. For example, a baby universe could form, and the quantum information ends up there. (Is there a calculation that shows that the state of a CFT cannot be dual to a baby universe?)

In any case, the published examples of singularity elimination resolving the problem, by Ashtekar et al and Rovelli et al, do not involve slow decay of remnants. Rather, as shown in detail in Rovelli et al, the Plank star explodes in a time scale of M^2, which is very short compared to the time of M^3 that the Hawking evaporation would have taken. (But still long on astrophysical time scales.) So when the Plank star explodes only 1/M of the Hawking photons have been emitted. So the process is nearly reversible and the problem of information loss never arises.

I don't think we know which of these outcomes arising from singularity resolution is correct. But I fail to see how semiclassical arguments, which assume the singularity remains, have any force once one finds that, as the calculations I mention show, quantum gravity eliminates the singularity. One cannot avoid the fact that there is a region to the future of where the singularity would have been, which contains quanta entangled with photons in the exterior. There is no singularity and this means no information is lost, whatever the fate of the region containing it. This does not contradict QM, rather it is a consequence of taking QM seriously.

Thanks,

Lee

52. **From the Mathematical "Paradise"**
April 11, 2017

I'd like to disagree with X, having seen a number of tenure cases in pure math in a highly ranked math department. We don't just count *Annals* papers, we ask for letters from other highly-ranked mathematicians – i.e., "important people". And when we do that, the same problematical sociological issues can easily muddy the waters. Somebody working on an unfashionable topic is going to get overlooked unless some miraculous discovery that affects the rest of mathematics comes out of this topic. And to make matters worse, people working on unfashionable topics are not going to get published in the *Annals*

On the other hand, it's clear that the evaluation system in mathematics is working a lot better than that in fundamental physics. But I think that's because HEP is currently "broken" in several unfortunate ways, not because the evaluation system in mathematics is different and better.

53. **reader**
April 11, 2017

Lee,
.
I should have been more clear. I hope the following clarifies matters.
.
First, the following scenario is clearly impossible: Hawking's result is accurate until the black hole radiates down to Planckian mass, and then, dictated by quantum gravity, the remaining Planckian object evaporates completely into outgoing radiation in a Planckian time, restoring the information. The obstacle is as follows. The early radiation can have a huge entropy; the total state of the radiation is pure; therefore the radiation produced by the Planckian object must also have this huge entropy. But it is impossible for a state of radiation of Planckian energy extended over a Planckian scale to have such a large entropy. A large entropy from a small energy can only be obtained if the radiation has a large spatial extent, corresponding to the long-lived remnant scenario. The same comments pertain to the decay of a black hole in AdS into a pure radiation state: no such state of Planckian energy can have the needed entropy.
.
As you correctly state, one possibility is that black hole singularity is resolved, resulting in a stable remnant that carries the information. But here there are two options. 1) The information in the remnant is hidden behind a horizon or is otherwise inaccessible to the outside world. This is really no different than information loss, as far as the outside world goes, although indeed information is preserved at large. 2) The information in the remnant is accessible by the outside world. Here one runs into the problems of infinite pair production and the like: if the remnant states interact with external degrees of freedom, then it would seems that these remnants will be produced in collisions.
.
The scenario of Rovelli: this involves a breakdown of semi-classical physics in a regime where spacetime curvature is arbitrarily small. This is a logical possibility, but clearly far more needs to be done to establish that this is a viable alternative.
.
Ashtekar: et. al: it's not clear to me what their bottom line is: a stable remnant, a long lived one…?
.
I avoided AdS/CFT so far, but you brought up whether it is compatible with information going into a baby universe. A closed baby universe has zero energy and would need to possess an arbitrarily large number of states to resolve the paradox. So the CFT would need to similarly possess a large number of zero energy states. This is not a property of, say, N=4 SYM as far as anyone knows. So AdS/CFT seems to incompatible with remnant/baby universe scenarios, although admittedly this is not a watertight argument.

54. **X**
April 12, 2017

In reply to 'Mathematical Paradise':

"people working on unfashionable topics are not going to get published in the Annals"

No doubt it's rare, but it does happen. I know an example, someone who was a former colleague in the maths dept where I worked. His PhD was from a good but not top university and afterwards he got a job at a 4-year teaching college. While there he did the work that resulted in an Annals publication (joint with his former PhD advisor). The field was Discrete/Computational Geometry, which is not a fashionable high-powered area as far as I'm aware. His work no doubt made a major advance in that area, and may have had implications for nearby areas such as algebraic geometry, but I doubt it "affected the rest of mathematics". Afterwards he was rewarded by a job at a pretty decent research university.

This kind of outcome simply can't happen in HEP theory/fundamental physics for the reasons I discussed.

55. **someone who spent time in "paradise"**
April 12, 2017

The reward system in maths incentivizes quality over quantity, i.e. working on hard problems that take a long time to solve rather than publishing lots of superficial 'me too' papers on the latest fad.

I think this is far too romantic a view. The reward structure in the math profession results in different problems than that of physics, but it has problems nonetheless.

Math is much more fragmented than physics. My advisor–who has worked in a number of different fields–told me that most mathematicians are idiots when confronted with math from outside their speciality (and he/she said this applies to him/herself). This problem is exacerbated by the fact that research math these days doesn't allow you the time to develop a broad mathematical culture: a functional analyst will probably not be able to afford learning algebraic number theory for fun, even if he/she might enjoy it and benefit from it.

So when institutions make decisions about funding, hiring, tenure, etc., it often degenerates into wrangling between various specialities: do the harmonic analysts get to hire another harmonic analyst, or do the homotopy theorists get this one?

And this is purely a question of academic politics, not of merit: are there more harmonic analysts in the department; are they cleverer bureaucratic infighters than the homotopy theorists; do they bring more grant money to the department; are they chummier with the deans, etc.

The truth-value of a theorem is objective: it is either true or false (let's leave aside undecidability for the purposes of the discussion). But the *value* of that theorem is subjective. Even within a particular discipline, there's not always expert consensus on the value of someone's work.

Regarding X's suggestion that elite journals encourage ambitious and deep work, I don't think it's true. Many, even most top mathematicians never manage to publish in such a distinguished journal as the Annals. This is exacerbated by the fact that the editors of such elite journals are very conscious of their elite status. So they're very conservative: they won't accept a paper unless they're sure it'll benefit the journal's reputation. That means many great papers are never published in those journals. Not because the work isn't good enough, but because for some reason or other, it just doesn't fit with what the editors want.

So it's a high-risk strategy to try to work on a problem famous enough to warrant publication in a top journal, especially if you don't have tenure. A young mathematician, unless he/she is very brilliant and has a high tolerance for taking risks, is not advised to try that route.

A far safer strategy is to prove more modest technical results extending the highly specialized line of research already set out by experts in your narrow field. That way those experts will recognize you, they'll praise you to their colleagues, they'll ensure your papers are published in their specialist journals, they'll write recommendations for you, they'll put your name on grant applications, and you'll be more likely to get tenure. Of course, it also means you won't break new ground or innovate very much. But if the goal is to have a career, that's the path you should probably take.

Young people are supposed to be brave and innovative and willing to gamble on big ideas because they have nothing to lose, but the incentives of academic mathematics encourage just the opposite. The pressure to publish trivial or incomplete work quickly is high and getting higher; the freedom to take time to do really deep and thorough creative work is quickly diminishing; technical prowess is increasingly valued over insight and conceptual clarity. There are exceptions to these rules, of course, but I am speaking of the norm.

The point is not whether the system produces superstars: there will always be people like Terry Tao and Peter Scholze who manage to break through to the highest level by dint of brilliance, hard work, and the luck to find the right problem at the right time. The point is whether the system can sustain a large "middle class": those researchers who are not superstars, but who make deep and important contributions.

Even those very top people depend on the contributions of many hundreds of mathematicians whose names are rarely cited. Not even Grothendieck could do it all alone!

And if the system becomes so dysfunctional that that "middle class" dies off, then eventually even the superstars won't have the material they need to produce great work. (An example is Euler's discovery of the addition formula for elliptic integrals, which was inspired by the work of Fagnano, a much lesser figure).

56. **anon**
April 12, 2017

No doubt math is fragmented, but it very hard to believe it's 'much more fragmented than physics'. Unless physics is understood to only mean fundamental theoretical physics…

57. **X**
April 12, 2017

I know of quite a few mathematicians from non-illustrious backgrounds whose research areas are not particularly fashionable but have still been able to have successful careers by publishing in high quality journals. It doesn't have to be Ann. Math.; there other journals one or two notches below that such as Adv. Math. and J. Reine. Angew. Math. which are more accessible but still recognized as high quality. When a mathematician publishes in such a journal it confers a quality stamp that is recognized throughout the maths community, including by those whose areas of specialization are completely different.

A person who has these quality stamps will generally get hired and promoted ahead of another person with better pedigree or more fashionable research area who hasn't managed to publish in journals of the same quality. This is a *great* feature of the maths reward system. Yes it does incentivize people to work on deeper problems. I've worked in maths depts, shared offices with pure mathematicians and seen first hand how trying to get published in the best journals they can is one of their main motivations when doing research.

That doesn't mean they are all trying to solve famous problems to publish in Ann.Math. But when faced with a choice between cranking out a bunch of fairly trivial papers or using the time instead to try to get one paper in a high quality journal, they choose the latter option from what I've seen. Maybe not so much at PhD level, but at postdoc level and beyond this seems to be the case.

If such 'quality stamps' were introduced in theoretical HEP/fundamental physics it would fix the sociological problems IMO.

58. **unitarity**
April 12, 2017

Dear Reader,

"Hawking's result is accurate until the black hole radiates down to Planckian mass" is exactly what is not true here. Hawking radiation is only approximately thermal. There are many ways to show this, including following the original Hawking's calculation and not taking the exact limit of t->infinity, which is anyway unrealistic for any physical external observer. Radiation becomes exactly thermal only at t = infinity, but a finite size black hole will disappear before that. Therefore, Hawking radiation was never quite thermal to begin with.

However, the real question is whether these small deviations from thermality are able to unitarize the process of black hole formation and subsequent evaporation. If there is a singularity at the center, then you are removing part of the initial state by hand. Then indeed, you really need some serious abracadabra to recover unitarity. If there is no singularity, then you do not trace out the ingoing modes. Then these modes contribute to the corrections to the original Hawking result. In that case, the deviations from thermality are able to unitarize the whole process. The corrections are locally very small, but their integrated effect is large and sufficient to purify the density matrix.

59. **Geoffrey Dixon**
April 12, 2017

No amount of downstairs gossip or griping about those comfortably ensconced upstairs will change the social order, save perhaps revolution, and even that can have no long term effect as far as stratification and exclusionary practices are concerned. The dust always settles in the same place. How many "important people" are involved in this debate? Peter and Lee are well-known, influential, and well established, but they are not the kind of "important people" discussed here. Their tomes questioning the worth of string theory pleased a lot of people – like me – but the response of the "important people" was a shrug. I've seen at least two articles extolling the virtues of being theoretically lost: it's the journey that matters, not the goal. The disruptions of a century ago were driven by mountains of incontestable data from many sources. We no longer have that, and the bombs those early pioneers gave rise to got the attention of the real power brokers. Money rained down, attracting great numbers of individuals who would otherwise never have dreamed of a life in theoretical physics. The power of vested interests increases in proportion to population. That power has withstood all attacks for 40 years.

60. **reader**
April 12, 2017

Unitarity.

If it were that simple people would not have been arguing about this for over 40 years. First, barring a gross breakdown of locality, the presence/absence of the singularity cannot possibly effect the outgoing radiation until the last stages, simply because the singularity (or whatever replaces it) is not in causal contact with the outgoing quanta, including their causal past. Of course, this Hawking radiation is not exactly thermal, but it carries a very large entropy and no small corrections to physics at the horizon can change this (a result proven by Mathur). Getting the information out in the early Hawking radiation (before the hole has shrunk to Planckian size) requires some radical new effect: nonlocality, firewall, Rovelli's bounce scenario, etc. Trouble is, all these scenarios are invented purely to solve this problem, and have little support or evidence beyond that.

61. **unitarity**
April 12, 2017

Dear Reader,

The absence of the singularity makes all the difference you need. Imagine that an object of mass M collapsed, formed an apparent horizon, but not a singularity at the center. Presumably it trapped everything within the region of the size of 2GM. If there was a singularity at the center, you would have to discard all of this trapped stuff and wait until the black hole reaches the Planck size for some unknown effects to release information about it. However, if there is no singularity, the stuff is just trapped by strong gravitational fields, and it is not lost forever. So when a black hole emits dM of its mass, only stuff within 2G(M-dM) radius will be trapped, the rest will be released right then (without waiting for Planckian physics). Therefore, information is released progressively, at first very slowly, but then faster and faster as the rate of evaporation grows and trapped region shrinks. Nothing non-local is needed here.

About Mathur's result that you mentioned, he is a very smart guy with a great amount of knowledge about this topic. The result he proved is very important, however, it is incomplete. He assumes that only the members of the Hawking virtual pair are entangled, and not the members of different pairs. The off-diagonal elements of the density matrix are exactly the interaction terms that he neglected.

62. **reader**
April 12, 2017

Unitarity,

Let's put it this way. Up until the very last stages, Hawking's computation can be carried out while making absolutely no reference to, or assumptions about, regions of large spacetime curvature. So as long as we have approximate locality and causality, any assumptions about the (non)singularity will have negligible effect on the outgoing radiation until the last stages. Therefore, if you want all the information to come out with the Hawking radiation you have two options: 1) arrange for the last gasp of radiation to carry a huge entropy to purify the full state. This is the long lived remnant scenario. 2) modify the early time radiation so that it carries a much lower entropy than Hawking predicts, noting as above that this modification is not coming from effects of high curvature, at least not in a way that anyone understands. This is the firewall/fuzzball/nonlocality/bounce scenario. They are all radical because they need to modify Hawking's computation by an O(1) amount, even though his computation appears trustworthy at face value. Nothing I am saying here is controversial.

63. **unitarity**
April 12, 2017

Dear Reader,

I mostly agree with you, up to some details that are important. Hawking flux does not have to be significantly modified. So in a way his result is robust. However, the density matrix is modified by an O(1) amount. Any local modification (say by adding the interaction terms between two emitted quanta) is small, but the integrated modification is of the order O(1), since every emitted particle is eventually entangled with any other emitted particle.

Assumption about the high curvature region is implicit. In the standard picture with the singularity, when an entangled pair is created at the horizon, a member than fell in disappears from the picture. You can't use it anymore. That is why you get the exactly thermal outgoing state (because you traced over the part of the state). But if the ingoing modes are still there, even if they are classically disconnected from the outside region, they still contribute to the emission of new Hawking quanta. After all, everyone agrees that the Hawking pair is entangled across the horizon. If the near horizon region is just Rindler, then nearby particles feel each other even across the horizon. So new Hawking quanta must "feel" what is inside the horizon, unless the black hole is an empty space with the singularity at the center.

64. **Bernardo**
April 12, 2017

X,
In some areas (such as number theory), it is enough to make decent progress like the resolution of a new special case to be published in the Annals or other top journals. In other areas, your paper better solve a major open problem to be even sent to the referees. In some areas (dynamic systems for example), a large influential name can make publishing in the top journals a completely normal state of affairs. All life is social. The good news is that none of this matters and there will always be hungry good young people pushing the envelope.

65. **Question**
April 13, 2017

Reader: "More seriously perhaps, it seems impossible to make this work for a black hole formed in AdS. Putting aside anything to do with AdS/CFT, AdS acts like a finite sized box, so a state of radiation of total energy M_Pl has a small maximum entropy. In this case there is no way for a Planckian remnant to decay into a gas of (arbitrarily) high entropy quanta whose state would purify that of the Hawking radiation emitted earlier. "

A large black hole would not evaporate away (to remnant or not) in AdS, so is there a real information paradox for large black holes in AdS? (I am not talking about firewall like arguments about smoothness of horizon, which are related, but do not concern remnants.)

If instead you are talking about a small black hole, it has an upper bound on its mass (and therefore entropy) precisely so that its can decay to radiation while increasing entropy, no? So in principle the remnant scenario could work?

I am no fan of remnants, but just trying to see where exactly the loopholes are. Thanks.

66. **reader**
April 13, 2017

Question,

A large black hole in AdS does not evaporate because the Hawking radiation reflects off the boundary back into the black hole. But one can change the boundary conditions to allow energy to flow in and out of AdS. This corresponds to coupling to an auxiliary system that collects the Hawking radiation. So one can turn on this coupling, collect the radiation until the black hole becomes small, and then turn the coupling off again. Details can be found in section 4 of arXiv:1304.6483.

67. **Question**
April 14, 2017

reader,

But wouldn't that violate the premise that you wanted, which if I understand correctly, was that there is only limited phase space available for the radiation in the box and therefore limited entropy? Isn't the system that you couple the boundary theory to, a proxy for "the region outside the box"?

In p. 20 of the paper you mentioned, they seem to have another (distinct?) argument. They keep the black hole large by pumping in energy, but the Hawking radiation couples to the auxiliary theory and eventually increases its entanglement unboundedly, which they argue is impossible. To me this sounds like enlarging the box and finding that the new box also has a finite (even if bigger) phase space available. But in any event how this is related to remnants I fail to see – the black hole stays large throughout their thought experiment.

Maybe I am missing something basic.

68. **Miki Weiss**
April 17, 2017

In the same mentioned blogpost of Dr. Hossenfelder, she writes in a response to gilkalai: “The so-called firewall problem, which isn’t a problem but just a mathematical mistake”, so this is yet another cynical description of the current situation in fundamental theoretical physics (In an older blog she explained why she considers this “Problem” to be a mistake-http://backreaction.blogspot.co.il/2015/10/black-holes-and-academic-walls.html)
Could you comment on that?

69. **Peter Woit**
April 17, 2017

Miki Weiss,
I have even less of an opinion on the “firewall problem” than on the black hole information paradox itself. If you’re interested in that, and on Sabine’s views about it, you should discuss it with her at her blog.

Quick Links

April 16, 2017
Categories: Uncategorized

A few quick items:

- I was very sorry to hear recently of the death of David Goss (obituary here), a mathematician specialist in function fields who was at Ohio State. David had a side interest in physics and was a frequent e-mail correspondent. From what I recall I first heard from him in 2004 soon after the blog started, with my first reaction when I saw the subject and From line that of wondering why David Gross wanted to discuss that particular article about physics with me.

  Over the years he often sent me links to things I hadn't heard about, with always sensible comments about them and other topics. I had the pleasure of meeting him a couple years ago, when he came to Columbia to drop off his son, who is now a student here. My condolences to his family and friends.

- The AMS has a wonderful relatively new repository of mostly expository documents called Open Math Notes. The quality of these seems to uniformly be high, and this is a great new service to the community. I hope it will grow and thrive with more contributions.
- Peter Scholze has now finished his series of talks at the IHES about his ongoing work on local Langlands, the talks are available here.
- Jean-Francois Dars and Ann Papillault have a web-site called Histoire Courtes, with short pieces in French, many of which are about math and physics research.
- The LHC is starting to come to life again after a long technical stop. Machine checkout next week, recommissioning with beam during May, physics starts again in June.
- There's a new book out with string theory predictions from Gordon Kane, called String Theory and the Real World. Kane has been writing popular pieces about string theory predictions for at least 20 years, with a 1997 piece in Physics Today telling us that string theory was "supertestable", with a gluino at 200-300 GeV. Over the years, his gluino mass predictions have moved up many times, as the older predictions get falsified. I don't have a copy of the new book, but at Google Books you can read some of it. From the pages available there I see that

  > the compactified M-theory example we will examine below predicts that gluinos will have masses of about 1.5 TeV…
  > The bottom line is that with about 40 inverse fb of data the limits on gluinos are just at the lower range of expected masses at the end of 2016.

  Right around the time the book was published, results released at Moriond (see here) claimed exclusion of gluinos up to about 2 TeV. Assumptions may be somewhat different than Kane's, but I suspect his 1.5 TeV gluino is now excluded.

Comments

1. **new**
   April 17, 2017

   since LHC latest results push gluino masses above 2tev above Gordan Kane's limit of 1.5 tev, Is there an upper limit Gordan Kane can push gluino masses ?

2. **Rob**
   April 17, 2017

   New
   The Planck scale.

3. **my Milkshake brings all the Boys to the Yard**
   April 17, 2017

   Kane must be aware of this kind of criticism. What does he say in response?

4. **Jeff M**
   April 17, 2017

   @Rob

   OK, that would be funny if it weren't true.

   Peter, thanks for the AMS link, looks great.

5. **SteveB**
   April 17, 2017

   Does someone have information on why the LHC technical shutdown was so long? 4 (or was it 5) months? Also, I occasionally used to look at the Morning Briefings on the Indico site. I could get there until last week (there was no new information for 2017), but now it brings up a CERN authentication screen — keeping me, the public, out. Anyone know why they changed their policy — or is there a new site? Here is the old one (that I found on this blog some years ago):
   https://indico.cern.ch/category/6386/
   Thankfully, the Vistars site still works:
   https://op-webtools.web.cern.ch/vistar/vistars.php?usr=LHC3

6. **Peter Woit**
   April 17, 2017

   Re Kane,

   I should have made clear that his viewpoint is a minority one among string theorists, most of whom at this point acknowledge that in its current state you can't get testable predictions out of string theory. Kane deals with this basically by saying these string theorists are ignorant, see for example page 6 of his Munich presentation, available here

   http://sites.lsa.umich.edu/gordonkane/wp-content/uploads/sites/117/2014/07/munich-final.pdf

   As far as I know he's never addressed the question of why, given so many previous failed predictions, anyone should take seriously his latest ones (few people do).

Steve B,

This was scheduled a long time ago to be a longer shutdown, an “Extended” year end technical stop. My understanding is that the main reason for this is the time needed for installation by CMS of an upgraded pixel detector, but presumably there are many other sorts of maintenance that could take advantage of the longer time period.

Once recommissioning and daily meetings start, perhaps information from these will again be available publicly. If not, LHC fans could politely ask, explaining that they appreciate getting direct news of the health of the star.

7. **MathPhys**
April 17, 2017

” Kane deals with this basically by saying these string theorists are ignorant, see for example page 6 of his Munich presentation”.

I recommend page 7 and page 8.

8. **John McAllison**
April 18, 2017

SteveB,

As Peter has pointed out, the extended year end technical stop (EYETS) was planned before 2017. This document is from Jan 2015:
https://indico.cern.ch/event/387137/contributions/1818419/subcontributions/157604/attachments/773229/1060436/YETS1516.pdf

This link gives a brief outline of what’s been going on during the technical stop:
https://home.cern/cern-people/updates/2017/03/eyets-report-2017-busy-year-ahead-accelerators

9. **Shantanu**
April 18, 2017

Peter and others:
There is supposed to be new B-physics results announced today

https://indico.cern.ch/event/580620/

10. **david_n**
April 18, 2017

Thanks for the AMS information; looks very helpful.

11. **modda**
April 18, 2017

Yes, you were right to be surprised when you thought David Gross wanted to discuss with you.

12. **Peter Woit**
April 18, 2017

Shantanu,

Tommaso convincingly makes the case for skepticism here

http://www.science20.com/tommaso_dorigo/lhcb_measures_unity_finds_06-225038

13. **Doug McDonald**
April 18, 2017

As to the morning meetings error note, this has happened before. In at least one case, it was at a time like this and they were doing some changes to the way the computer systems were do it. I would not worry for at least a few days.

14. **Tim**
April 18, 2017

Clearly, the masses of supersymmetric particles are not constant, but increase with time, as predicted by string theory and the multiverse.

15. **physicsguy**
April 18, 2017

Peter,

note that Tommaso is not an expert on flavour physics, and in fact a comment by an expert below his post very convincingly argues why what Tommaso writes does not make much sense.
Still I would not bet with Tommaso because I think the odds that it is new physics are nevertheless below 50% in my opinion, maybe 20% or so.

16. **neil**
April 19, 2017

The lepton universality tests by LHCb are quite interesting. After the 750 GeV circus, no one wants to hype a 2.5 sigma signal, but there appears to be tantalizing hints from a number of sources. I am looking forward to results from new data later this year on potential BSM physics.

17. **Sidi M.**
April 20, 2017

Hi everyone,
Thank you Peter for the Histoires courtes link, amazing audio stuff, we’ll sleep less stupid.

18. **Milkshake**
April 20, 2017

They've had 3.3 sigma signals that turned out to be nothing before.

I think Kane is wasting everyone's time. (He's always talking about his latest predictions at conferences.) Someone should have a polite discussion with him.

19. **Jim**
April 23, 2017

Hi Milkshake,

I didn't follow LHCb so closely. Can you remind me which 3.3 sigma signal turned out to be nothing?

20. **neil**
April 23, 2017

Jim

In January, LHCb reported measured violation of CP symmetry in baryon decays with significance at 3.3 standard deviations. I have not read that this "turned out to be nothing", however.

The LHCb beauty experiment results can be found here.

http://lhcb-public.web.cern.ch/lhcb-public/

21. **ronab**
April 24, 2017

@Milkshake,

"I think Kane is wasting everyone's time. (He's always talking about his latest predictions at conferences.) Someone should have a polite discussion with him."

This seems a common sentiment. But then how does he keep getting to talk at conferences? Why don't organizers just stop inviting him?

22. **Peter Woit**
April 24, 2017

ronab,

I suspect he is talking at fewer conferences these days. In any case, there's a long tradition in most fields of academia of having senior figures continue to give talks at conferences past the point anyone is paying attention to them.

Two Pet Peeves

April 20, 2017
Categories: Favorite Old Posts, Uncategorized

I was reminded of two of my pet peeves while taking a look at the appendix A of this paper. As a public service to physicists I thought I'd go on about them here, and provide some advice to the possibly confused (and use some LaTeX for a change).

Don't use the same notation for a Lie group and a Lie algebra

I noticed that Zee does this in his "Group Theory in a Nutshell for Physicists", but thought it was unusual. It seems other physicists do this too (same problem with Ramond's "Group Theory: a physicist's survey", the next book I checked). The argument seems to be that this won't confuse people, but, personally, I remember being very confused about this when I first started studying the subject, in a course with Howard Georgi. Taking a look at Georgi's book for that course (first edition) I see that what he does is basically only talk about Lie algebras. So, the fact that I was confused about Lie groups vs. Lie algebras wasn't really his fault, since he was not talking about the groups.

The general theory of Lie groups and Lie algebras is rather complicated, but (besides the trivial cases of translation and U(1)=SO(2) groups) many physicists only need to know about two Lie groups and one Lie algebra, and to keep straight the following facts about them. The groups are

- SU(2): the group of two by two unitary matrices with determinant one. These can be written in the form
  $$
  \begin{pmatrix}
  \alpha & \beta\\
  -\overline{\beta}& \overline{\alpha}
  \end{pmatrix}
  $$
  where \(\alpha\) and \(\beta\) are complex numbers satisfying \(|\alpha|^2+|\beta|^2=1\), and thus parametrizing the three-sphere: unit vectors in four real dimensional space.
- SO(3): the group of three by three orthogonal matrices with determinant one. There's no point in trying to remember some parametrization of these. Better to remember that a rotation by a counter-clockwise angle θ in the plane is given by
  $$\begin{pmatrix}
  \cos\theta & -\sin\theta\\
  \sin\theta & \cos\theta
  \end{pmatrix}$$
  and then produce your rotations in three dimensions as a product of rotations about coordinate axes, which are easy to write down. For instance a rotation about the 1-axis will be given by
  $$\begin{pmatrix}
  1&0&0\\
  0&\cos\theta & -\sin\theta\\
  0&\sin\theta & \cos\theta
  \end{pmatrix}$$

The relation between these two groups is subtle. Every element of SO(3) corresponds to two elements of SU(2). As a space, SO(3) is the three-sphere with opposite points identified. Given elements of SO(3), there is no continuous way to choose one of the corresponding elements of SU(2). Given an element of SU(2), there is an unenlightening impossible to remember formula for the corresponding element of SO(3) in terms of \(\alpha\) and \(\beta\). To really understand what's going on, you need to do something like the following: identify points in \(\mathbf R^3\) with traceless two by two self-adjoint matrices by
$$(x_1,x_2,x_3)\leftrightarrow x_1\sigma_1 +x_2\sigma_2+x_3\sigma_3=\begin{pmatrix} x_3&x_1-ix_2\\x_1+ix_2&-x_3\end{pmatrix}$$
Then the SO(3) rotation corresponding to an element of SU(2) is given by
$$\begin{pmatrix} x_3&x_1-ix_2\\x_1+ix_2&-x_3\end{pmatrix}\rightarrow \begin{pmatrix}
\alpha & \beta\\
-\overline{\beta}& \overline{\alpha}
\end{pmatrix}\begin{pmatrix} x_3&x_1-ix_2\\x_1+ix_2&-x_3\end{pmatrix} \begin{pmatrix}
\alpha & \beta\\
-\overline{\beta}& \overline{\alpha}
\end{pmatrix}^{-1}$$

Since most of the time you only care about two Lie groups, you mostly only need to think about two possible Lie algebras, and luckily they are actually the same, both isomorphic to something you know well: \(\mathbf R^3\) with the cross product. In more detail:

- su(2) or \(\mathfrak{su}(2)\): Please don't use the same notation as for the Lie group SU(2). These are traceless skew-adjoint (\(M=-M^\dagger\)) two by two complex matrices, identified with \(\mathbf R^3\) as above except for a factor of \(-\frac{i}{2}\).
  $$(x_1,x_2,x_3)\leftrightarrow -\frac{i}{2}\begin{pmatrix} x_3&x_1-ix_2\\x_1+ix_2&-x_3\end{pmatrix}$$
  Under this identification, the cross-product corresponds to the commutator of matrices.

  You get elements of the group SU(2) by exponentiating elements of its Lie algebra.

- so(3) or \(\mathfrak{so}(3)\): Please don't use the same notation as for the Lie group SO(3). These are antisymmetric three by three real matrices, identified with \(\mathbf R^3\) by

  $$(x_1,x_2,x_3)\leftrightarrow \begin{pmatrix}
  0&-x_3&x_2\\
  x_3&0 & -x_1\\
  -x_2&x_1&0
  \end{pmatrix}$$
  Under this identification, the cross-product corresponds to the commutator of matrices.

  You get elements of the group SO(3) by exponentiating elements of its Lie algebra.

If you stick to non-relativistic velocities in your physics, this is all you'll need most of the time. If you work with relativistic velocities, you'll need two more groups (either of which you can call the Lorentz group) and one more Lie algebra, these are:

- \(SL(2,\mathbf C)\): This is the group of complex two by two matrices with determinant one, i.e. complex matrices

$$\begin{pmatrix}
\alpha & \beta\\
\gamma& \delta
\end{pmatrix}$$
satisfying \(\alpha\delta-\beta\gamma=1\). That's one complex condition on four complex numbers, so this is a space of 6 real dimensions. Best to not try and visualize this; besides being six-dimensional, unlike SU(2) it goes off to infinity in many directions.

- SO(3,1): This is the group of real four by four matrices M of determinant one such that
$$M^T\begin{pmatrix}-1&0&0&0\\
0&1&0&0\\
0&0&1&0\\
0&0&0&1\end{pmatrix}M=\begin{pmatrix}-1&0&0&0\\
0&1&0&0\\
0&0&1&0\\
0&0&0&1\end{pmatrix}$$
This just means they are linear transformations of \(\mathbf R^4\) preserving the Lorentz inner product.

  Correction: *a correspondent reminds me that for the next part to be true this definition needs to be supplemented by an extra condition, since as stated SO(3,1) has two components. One version of the extra condition is to take the connected component of the identity, another is to take the component that preserves time orientation. Many use a different notation for this component to make this explicit, I'll define SO(3,1) as the connected component.*

The relation between SO(3,1) and \(SL(2,\mathbf C)\) is much the same as the relation between SO(3) and SU(2). Each element of SO(3,1) corresponds to two elements of \(SL(2,\mathbf C)\). To find the SO(3,1) group element corresponding to an \(SL(2,\mathbf C)\) group element, proceed as above, removing the "traceless" condition, so identifying \(\mathbf R^4\) with self-adjoint two by two matrices as follows
$$(x_0,x_1,x_2,x_3)\leftrightarrow\begin{pmatrix} x_0+x_3&x_1-ix_2\\x_1+ix_2&x_0-x_3\end{pmatrix}$$
The SO(3,1) action on \(\mathbf R^4\) corresponding to an element of \(SL(2,\mathbf C)\) is given by
$$\begin{pmatrix} x_0+x_3&x_1-ix_2\\x_1+ix_2&x_0-x_3\end{pmatrix}\rightarrow \begin{pmatrix}
\alpha & \beta\\
\gamma & \delta
\end{pmatrix}\begin{pmatrix} x_0+x_3&x_1-ix_2\\x_1+ix_2&x_0-x_3\end{pmatrix} \begin{pmatrix}
\alpha & \beta\\
\gamma& \delta
\end{pmatrix}^{-1}$$

As in the three-dimensional case, the Lie algebras of these two Lie groups are isomorphic. The Lie algebra of \(SL(2,\mathbf C)\) is easiest to understand (please don't use the same notation as for the Lie group, instead consider \(sl(2,\mathbf C\)) or \(\mathfrak{sl}(2,\mathbf C)\)), it is all complex traceless two by two matrices, i.e. matrices of the form
$$\begin{pmatrix}a&b\\
c&-a\end{pmatrix}$$

For the isomorphism with the Lie algebra of SO(3,1), go on to pet peeve number two and then consult a relativistic QFT book to find some form of the details.

Keep track of the difference between a Lie algebra and its complexification

This is a much subtler pet peeve than pet peeve number one. It really only comes up in one place, when physicists discuss the Lie algebra of the Lorentz group. They typically put basis elements \(J_j\) (infinitesimal rotations) and \(K_j\) (infinitesimal boosts) together by taking complex linear combinations
$$A_j=J_j+iK_j,\ \ B_j=J_j-iK_j$$
and then note that the commutation relations of the Lie algebra simplify into commutation relations for the \(A_j\) that look like the \(\mathfrak{su}(2)\) commutation relations and the same ones for the \(B_j\). They then announce that
$$SO(3,1)=SU(2) \times SU(2)$$
Besides my pet peeve number one, even if you interpret this as a statement about Lie algebras, it's not true at all. The problem is that the Lie algebras under discussion are real Lie algebras, you're just supposed to be taking real linear combinations of their elements. When you wrote down the equations for \(A_j\) and \(B_j\), you "complexified", getting elements not of \(\mathfrak{so}(3,1)\), but what a mathematician would call the complexification \(\mathfrak{so}(3,1)\otimes \mathbf C\). Really what has been shown is that
$$ \mathfrak{so}(3,1)\otimes \mathbf C = \mathfrak{sl}(2,\mathbf C) + \mathfrak{sl}(2,\mathbf C)$$

It turns out that when you complexify the Lie algebra of an orthogonal group, you get the same thing no matter what signature you start with, i.e.
$$ \mathfrak{so}(3,1)\otimes \mathbf C =\mathfrak{so}(4)\otimes \mathbf C =\mathfrak{so}(2,2)\otimes \mathbf C$$
all of which are two copies of \(\mathfrak{sl}(2,\mathbf C)\). The Lie algebras you care about are what mathematicians call different "real forms" of this and they are different for different signature. What is really true is
$$\mathfrak{so}(3,1)=\mathfrak{sl}(2,\mathbf C)$$
$$\mathfrak{so}(4)=\mathfrak {su}(2) + \mathfrak {su}(2)$$
$$\mathfrak{so}(2,2)=\mathfrak{sl}(2,R) +\mathfrak{sl}(2,R)$$

For details of all this, see my book.

Comments

1. **Maxis**
   April 21, 2017

   Is it just me, or LaTeX isn't rendering? You forgot to include the word "latex" after the dollar sign.

2. **---**
   April 21, 2017

   Re the signs and typographic errors: in the definition of SU(2) you wat $\alpha\bar{\alpha} + \beta\bar{\beta} = 1$; so that the determinant is 1. Then you also actually get the 3-sphere.

3. **Peter Woit**
   April 21, 2017

—,
Thanks, fixed.

Maxis,
I'm doing this using MathJax, works for me. If people are having trouble with this let me know.

4. **Timothy P Keller**
   April 21, 2017

   I am trying to write up some notes for a course on Lie theory and applications I would like to teach when I retire (but probably never will). Your notes are some of my best references.
   It's great that you posted this comment, and it's a wonder it's really necessary. Getting the notation right using Latex is so easy these days ….

5. **vmarko**
   April 21, 2017

   Peter,

   LaTeX isn't rendering for me either.

   Note that MathJax generally does render in my browser (if I visit my own website, or arXiv, or various other places), and everything works fine — except for your website, which just displays LaTeX source instead.

   HTH,
   Marko

6. **Jeff M**
   April 21, 2017

   Peter

   First, the LaTeX is displaying fine for me, Safari 10.1. Second, physicists really can't use different notation for the group and the algebra? In math, anything in Fraktur is the algebra. I assume this is still true, it certainly was when I took Lie Algebras in grad school in '87.

7. **Fred P**
   April 21, 2017

   My broswers (Firefox, Edge, and Chrome) does not render the Latex because it views the Latex rendering as insecure. On Chrome, I could easily turn this off.

   Looking at your page source, I see at least part of the problem:

   http://www.math.columbia.edu/department/mathjax/MathJax.js?config=TeX-AMS-MML_HTMLorMML&ver=4.7.3

   is not https, which is how I'm seeing most of this page. Note that

   https://www.math.columbia.edu/department/mathjax/MathJax.js?config=TeX-AMS-MML_HTMLorMML&ver=4.7.3

   seems like it works, but my https everywhere extension does not automatically translate that link to https – which is why the rendering wasn't working on my browsers.

8. **Peter Woit**
   April 21, 2017

   Fred P,
   I see the potential problem, changed the link to https.

   All, please let me know of any continuing problems with the Latex rendering.

9. **vmarko**
   April 21, 2017

   Yes, that fixed it, now it renders correctly for me too!

   🙂
   Marko

10. **vmarko**
    April 21, 2017

    Ok, now that I see the equations rendered correctly, shouldn't the sums be "circled", \(\oplus\), instead of an ordinary plus, \(+\), when writing sums of algebras at the end?

    I don't want to sound like a nitpick, but an algebra is also a vector space, and its addition of vectors (usually denoted with an ordinary plus sign) should be distinguished from the notion of the direct sum of two algebras. Especially if the two algebras are actually two copies of the same algebra.

    HTH, 🙂
    Marko

11. **Peter Woit**
    April 21, 2017

    Marko,
    You're probably right, but remember, here I'm trying to convince physicists that the distinctions mathematicians make are important ones….

12. **Narad**
    April 21, 2017

> Getting the notation right using Latex is so easy these days ….

Aside from remembering to type {-}\sin instead of -\sin when it's standing alone, among other things. 😉

13. **Al**
April 22, 2017

    Peter- Maybe it's ok to mention the March For Science tomorrow (Saturday):
    it's supposed to be nonpartisan and a celebration of science…
    of course barely in the background are worries about climate change, budget cuts,
    the destruction of the EPA, NIH…
    Here's TYT:
    https://www.youtube.com/watch?v=pCGFqbRC6do
    There are supposed to be marches in 500 cities worldwide.
    Information here:
    https://www.marchforscience.com/

14. **TS**
April 22, 2017

    I noticed a very minor pet peeve of mine in the book. It is really minor, so please feel free to not fill up the space under your post by publishing this comment: right after introducing "the Schrödinger equation" you call it "Schrödinger's equation." How could I not search the text after spotting an inconsistency! The text calls it "the Schrödinger equation" throughout, except for the aforementioned place, Fig. 19.1 and page 558.

    I also noted that on p. 466 you dropped the umlaut in naming the Pauli-Schrödinger equation. I actually only found this latter instance because your old-school LaTeX setup doesn't actually output the letter "ö," but instead takes \"o very literally. In other words, a search for "Schrödinger" in the pdf turns up empty, and I had to search for "Schr" instead. Thankfully, you don't cite Peskin & Schroeder all too often.

    Congrats on the book, and thanks for the explanation of the mistake in the decomposition of the Lorentz group that you gave here. That was a magic bit of common textbook lore that had puzzled me for the longest time! Too bad that I no longer do physics, and the only group that I encounter regularly these days is the Quaternion group.

15. **Art**
April 23, 2017

    In the two equations identifying R4 with 2×2 self-adjoint matrices, the signs of the x2 entries are opposite those of the R3 case (and of your book).

16. **To ask a silly question ...**
April 23, 2017

    Why do mathematicians use Fraktur for the Lie algebra so(3) when lowercase works fine?

    My suspicion is that they do it to show off their superior knowledge of fonts. Of course, this may confuse physicists into using the same notation for the Lie algebra and the Lie group, because they may not know how to use Fraktur in LaTeX and may be too lazy to bother looking it up.

17. **BCnrd**
April 23, 2017

    Dear "To ask a silly question…": Your guess for the reason that fraktur fonts are used to denote Lie algebras is incorrect. The reason is the same as why fraktur fonts are commonly used to denote ideals in commutative algebra: many concepts in abstract algebra and Lie theory were initially developed by those who worked in Germany or very closely with the German school of mathematics in the late 19th and early 20th century, among whom the fraktur font was quite common to use (unsurprisingly).

    Lie spent some early pivotal years of his professional career in Berlin, where he struck up a friendship with the German mathematician Klein that was to become very influential on the development of Lie's own work (prior to the unfortunate demise of their friendship over some priority disputes). Other early fundamental work in the subject of Lie algebras was done by Killing (also German), to say nothing of the early fundamental work in representation theory by Frobenius, Schur, and Weyl. You can read about all of this and much more in Thomas Hawkins' great book "Emergence of the Theory of Lie groups" (as well as a bit in the Historical Notes at the end of Chapters 1-3 of Bourbaki's "Lie groups and Lie algebras").

    It was due to the aftermath of World War II that German became less common as a language for math papers and the fraktur font consequently less widely seen. But the tradition was already firmly established in Lie theory and various parts of ring theory, a tradition that has remained to this day; this was widespread long before modern type-setting.

    Moreover, due to the extensive need for notation in mathematical proofs, it is a tremendous convenience that one can use g to denote an element of a Lie group G while considering proofs that involve both the Lie group and its Lie algebra $\mathfrak{g}$ at the same time while not thereby writing "g" to denote two very different things in the same discussion. The presence of many font options in LaTeX is due to the widespread notational needs of mathematicians, and not the other way around. In math papers and books, notational choices and traditions are generally made to promote clarity of discussion, not to show off.

18. **Peter Woit**
April 23, 2017

    To ask a silly question,

    As BCnrd comments, mathematicians often have a need to clearly notationally distinguish unambiguously different kinds of mathematical objects, so it's not surprising that they use more fonts and yes, are more likely to know the relevant TeX off the top of their heads.

    I understand why physicists are reluctant to use fraktur fonts, its use is culturally alien. What I can't understand is why they won't use upper vs. lower case. Why not write the Lie algebra of SU(2) as su(2)?

19. **Art**
April 24, 2017

    In your book, section 40.2, p. 448, the complexified so(3,1) "splits into a product of two sub-Lie algebras", and the equation is written with a multiplication sign. I think it should be a direct sum.

20. **Peter Woit**
April 24, 2017

Art,
Thanks. Fixed sign, and yes, for Lie algebras should be sum not product.

21. **Art**
April 24, 2017

For completeness, the same equation shows up at the end of section 40.4.

22. **Geoffrey Dixon**
April 25, 2017

SU(2) and SO(3) simplified with quaternions (H). Let X be in H, and U a unit element in H. Then
X –> UX is an SU(2) action,
X –> UXU* is SO(3).
In the former case all 4 components of X are mixed,
and in the latter only the 3 imaginary components
(U* is quaternion conjugation, and because U is
a unit quaternion, it is also the inverse of U).

There must exist some imaginary element v in H
(no real part) such that U = exp(v). The Lie algebra
of SU(2) and SO(3) is the set of such elements v.

The study of the Lie groups and algebras important
to theoretical physics is greatly simplified by using
division algebras. (Am I biased in this matter?
Hmm, let me check …)

PS: How does one exploit Latex in these comments?

23. **Peter Woit**
April 25, 2017

Geoffrey Dixon,

I do think that using quaternions and Sp(1)=Spin(3) is the best way to understand the relationship between Spin(3) and SO(3), and that's the way I do it in my book. However, this post is aimed at keeping math apparatus to a minimum for an everyday physicist, so I avoid quaternions and use two by two complex matrices instead.

You can use latex in comments, using MathJax. I should add this info to the "leave a reply" text, once I figure out how to escape the delimiters correctly. One way to convey this info is to say I'm using the default delimiters, see
http://docs.mathjax.org/en/latest/tex.html#tex-and-latex-math-delimiters

24. **TG3D**
April 26, 2017

The (astro)physicists seem to be incorrigible… they write in https://arxiv.org/pdf/1704.05067.pdf , p.42:

"11)We acknowledge the pain of our more mathematically inclined readership at our deliberate, yet well-intentioned ambiguation of a group and its matrix representation. Sorry!"

25. **Peter Woit**
April 27, 2017

TG3D,
When people are having trouble figuring out the difference between a Lie group and a Lie algebra, not a good idea to tell them about representations…

26. **Steve**
April 28, 2017

Thanks, Peter, for stressing that we should always keep Lie groups and Lie algebras separate!

A perspective that I've always found clarifying is that a Lie algebra can be taken to be the tangent space to the Lie group at the identity. Of course for that to make sense you have to remember that Lie groups are manifolds that have a tangent space. But this is worth knowing because it makes obvious and intuitive some of the Lie group isomorphisms you talk about: since the group SU(2) is the double cover of the group SO(3), they are locally isomorphic and therefore have isomorphic tangent spaces. Viola! That's just another way of saying that su(2) is isomorphic to so(3). You can even give a geometrical explanation of the exponential map as a map from the tangent space (Lie algebra) to the manifold (Lie group) if you feel ambitious.

Sure, that needs more mathematical machinery than you wrote about, but I find that machinery very helpful for explicating the difference between Lie groups and Lie algebras. I've even used this to explain the difference to non-math folk using pictures!

27. **Pedant**
May 6, 2017

People who can't distinguish between a Lie group and a Lie algebra have no business calling themselves physicists or being employed as researchers. Sticking with 19th century confusions is just unprofessional and uneducated, manifesting a lack of curiosity and an intellectual laziness that shouldn't be allowed in professional contexts.

28. **Peter Woit**
May 6, 2017

pedant,
I think you're being unfair to the 19th century…

Why String Theory is Still Not Even Wrong

April 27, 2017
Categories: Fake Physics, Multiverse Mania

John Horgan recently sent me some questions, and has put them and my answers up at his Scientific American site, under the title [Why String Theory is Still Not Even Wrong](). My thanks to him for the questions and for the opportunity to summarize my take on various issues.

Comments

1. **Thomas Lee Elifritz**
   April 27, 2017

   Well done. Although I can find no mention of it in a recent arxiv article on the CMB cold spot, it appears the multiverse has somehow crept into the journalistic discussion of it … again,

2. **Peter Woit**
   April 27, 2017

   Thomas Lee Elfritz,
   Hadn't noticed that, but it's textbook "Fake Physics":

   The paper
   https://arxiv.org/abs/1704.03814
   would normally get zero public attention, and includes nothing that could seriously be taken as evidence for other universes. The authors put out a press release

   https://www.sciencedaily.com/releases/2017/04/170425124822.htm

   in which they highlight the multiverse angle:
   'Perhaps the most exciting of these is that the Cold Spot was caused by a collision between our universe and another bubble universe. If further, more detailed, analysis of CMB data proves this to be the case then the Cold Spot might be taken as the first evidence for the multiverse — and billions of other universes may exist like our own."

   Press stories then come out like this one

   http://www.dailygalaxy.com/my_weblog/2017/04/cold-spot-billions-of-light-years-across-was-caused-by-a-collision-with-another-universe-.html

   With a blaring headline:
   "'Cold Spot' Anomaly Billions of Light Years Across –"Caused By a Collision With Another Universe" "

   In deciding where to lay blame for this kind of nonsense and the damage it does to the public understanding of science, I think the bulk of it should go to the scientists involved. Look, if you put out a press release highlighting this, say things like this to reporters, you know very well what the press stories are going to look like.

3. **Thomas Lee Elifritz**
   April 27, 2017

   That sounds about right. I didn't research it that deeply. I usually just read the paper itself. But what I do is find those papers occasionally through the press, as I don't have a lot of time to pursue every lead on every search engine using every combination of keywords. It's nice to see that nowadays they at least provide a link to the papers.

   When I run into a whole slew of journalistic failures like that though, that's usually a sign that something went awry at a deeper more fundamental level and the press is just parroting that problem. Therein lies the problem, as discussed widely now here and elsewhere. What can be done about that, I don't know, except to just keep slogging.

4. **G Phillip**
   April 27, 2017

   Peter, just reading your book. I was waiting to see if all this string "theory" stuff worked out. Since it didn't, and now with the failure to find SUSY it looks like it never will, I'm digging deep into concepts I have heard of but never really taken the time to study. What a great book! I love all the HEP history. In truth I've spent as much or more time studying the concepts you mention like renormalizations and group theory on the side as I have reading, but I'm now 1/3rd through and I can say I'm impressed. My goal is to complete it with a decent understanding before your next book comes out. I found it very honorable of you to acknowledge the contributions of Lubos Motl to the book. In my opinion, everyone in the field should be so professional. I also found it enlightening that many theories along the way that were logical, self-consistent, and even elegant were disproved beyond any doubt by experimental evidence. It's a lesson we must never forget. Best.

5. **Henry Warwick**
   April 28, 2017

   I liked your interview with John Horgan. I would add that there is another critique of Bostrum et al and the Matrix theory, and that is one of infinite regress. If we are in a simulation, then there is no reason to think that the creatures controlling us in our simulation are not themselves simulations, etc. ad infinitum. Obviously, there has to be a stopping point – there has to be someone who is not a simulation running the simulation for the simulation to exist in the first place. However, given the formulation as one of computability in the Matrix theory, where any entity who can simulate can also be simulated, there is no logical reason why it can't be ad infinitum – it's turtles all the way up ! Therefore: the Matrix theory is impossible, and thinking about it is a colossal waste of time. Cheers!

6. **Magnema**
   April 28, 2017

   @Henry Warwick:
   Impossible? I think that's an implausibly strong claim. The argument does not show that we are in a simulation (in which case, yes, if that were the case with 100% certainty then we would have a problem). Rather, it shows that we should believe we are in a simulation. You could come to the conclusion that any unsimulated world will come to the wrong conclusion in such a case; however, since the premise is that most worlds are probably simulated, that is, in a sense, an "acceptable risk" to those who accept the argument – because, after all, according to their premise, they are probably right, even if uncertain.

Now, I would agree that “thinking about it is [mostly] a colossal waste of time.” However, I don’t think the philosophical argument could be refuted so easily – and I am, very generally, wary of any claim leading to “impossibility” (or “necessity”) a priori, given how often those claims have been proven wrong. (Implausibility or near-certain truth a priori, including the simulation argument, are more tolerable, if not perfect.) Non-productivity of discussion is entirely within the realm of reasonable a priori debate, though.

7. **Peter Woit**
April 28, 2017

Henry Warwick, Magnema,
Since we seem to agree thinking about the simulation argument is a waste of time, let’s not further discuss it here, OK?

8. **Tim May**
April 28, 2017

When I was in high school, there were some speculative reports in places like “Science News” and even in some issues of “Scientific American” about things like tachyons. Even by 1970 some exciting stories about black holes and wormholes.

Black holes were sort of confirmed when I was in college (Cygnus x-1) and are of course now mainstream.

There were later tales of possible magnetic monopoles. Interesting stuff.

But I never saw a “world view” based on these speculations of tachyons, wormholes, monopoles, etc. Hype was not bad at all in those days. A few speculative articles, even the famous Bryce DeWitt article/book on the “Many Worlds” interpretation of QM, but all within the basic realm of plausibility.

Not until “string theory,” going on nearly 40 years ago.

I thank some of the string theory skeptic sites, like this one, for putting a (perhaps slight) lid on the bubbling cauldron of hype.

–Tim May

9. **Justin**
April 28, 2017

I’m curious if HEP is healthier outside the US. Does anybody know if there is a better culture promoting independent thinking in Europe or does it suffer from the same problems we have here in the US?

10. **Jeff M**
April 28, 2017

Peter,

Enjoyed the piece with Horgan. The differences between physics and math are fascinating, I think fundamentally the issue is that in math you can show that something is correct, but in physics (and essentially everything else) you can only show that something is incorrect. Mathematics is the only discipline where “this is right” is a truly meaningful statement. I once argued with my father in law, who was an art professor, about which was harder to teach. He though art was, since you had no way to judge what was right. I said math was, since I had to figure out a way to get people to learn the right way to do things, I couldn’t let them do it any other way.

11. **new**
April 28, 2017

Has any string theorist like Edward Witten, Briane Greene Michio Kaku Gates et al, commented on the latest SUSY Moriond result? How does this impact string theory?

12. **Magnema**
April 28, 2017

@Peter Woit: Before I quit, I want to clarify that I meant “from the perspective of a scientist.” I think philosophers should consider it, because if it’s wrong, then why it’s wrong would be something interesting to study in that field. Like math, philosophy has different standards of argument and different paradigms for useful. OK, done talking about it now.

13. **Shantanu**
April 28, 2017

Peter, something else I would have mentioned. For many years string theorists claim to predict a dark matter candidate. Then recently Verlinde has argued that string theory dispenses with the need for dark matter. How can a theory predict two opposite hypothesis? String theorists should make up their mind about this.
(Same with principle of equivalence and I have heard similar opposing claims)

14. **Bernhard**
April 29, 2017

@Justin,

This is own take but from experience, I do not think HEP is healthier in Europe.
I can’t also not think of any fundamental differences in culture in regards to promoting independent thinking, not by a long shot. The US and Europe share, in this respect at least, the same culture and if you found yourself locked inside an US or and European institution, you would not be able to spot any differences, in my opinion. Also, the mechanisms which exist to stall independent thinking exist both in the US and Europe in the same way (like the way grant decisions are made, etc).

The US and Europe collaborate extremely closely in HEP, just look at the conferences, who attends it, who works where, etc. The geographical distance is not translated to any significant difference in scientific culture. And the rest of the world sill looks at their example for reference (including China and Japan), since historically most of the major advances came from one place or the other.

15. **Avattoir**
April 29, 2017

I’m interested in knowing Peter Woit’s view on the opening point in Jeff M’s comment:
“… math … can show … something is correct, … physics … only … that something is incorrect”.

On Jeff M's dw with his father in law, my impression is they somehow agreed to equate "harder" and "more uncertain". Surely it's not the case that teaching competence in a given field varies according to its participants' tolerance for bullshit.

16. **Tim May**
April 29, 2017

Peter, this is not directly related to this thread, but..

Tonight I saw the biopic "The Man Who Knew Infinity," about Ramanujan and Hardy (and Littlewood in a minor role). Well done. Probably not comprehensible to anyone who has not looked at infinite series and convergences.

One thing that struck me in the end credits was something along the lines of "His work is now helping us to understand black holes."

Say what? Surely he deserves even more credit for predicting the multiverse.

I looked into and found a quote from the ever-speculative (though fun to read) "New Scientist":

regarding some of Ramajun's work:

"Devised by Ken Ono of Emory University in Atlanta, Georgia, the formula concerns a type of function called a mock modular form (see main story). These functions are now used to compute the entropy of black holes. This property is linked to the startling prediction by Stephen Hawking that black holes emit radiation.

"If Ono has a really new way of characterising a mock modular form then surely it will have implications for our work," says Atish Dabholkar, who studies black holes at the French National Centre for Scientific Research in Paris. "Mock modular forms will appear more and more in physics as our understanding improves."

My thinking is that Ramanujan's work stands on its own. Co-opting him into the publicity brigade is akin to how Hardy is now being treated as a kind of father of secure credit card transactions. (And, yeah, I know about number theory and have worked in crypto since the 1980s.)

I don't dispute that some of the partition work may relate to entropy, etc. Just that claims about how Ramanujan has helped the world to understand black holes seems like journalistic hype. Probable intended for sex appeal, in the abstract sense.

17. **vmarko**
April 30, 2017

Tim May,

If you want to calculate the statistical entropy of an ordinary Schwarzschild black hole in the context of loop quantum gravity (by counting all possible ways the horizon area can be constructed from pieces which belong to the spectrum of the area operator of LQG), then you need to use one of the Ramanujan's formulas to evaluate the entropy.

It has to do with the math problem (from number theory) of counting all possible ways a large integer can be partitioned into summands, and Ramanujan was the first one to solve that problem.

HTH. 🙂
Marko

18. **Jesper**
April 30, 2017

@ Justin, Bernhard,

Although my experience with the US is limited I do think that there could be a slight difference between Europe and the US with respect to theoretical physics. I think that there is more diversity in Europe, where there are communities with a strong tilt towards mathematics, for instance algebraic QFT and those involved in noncommutative geometry. Once could also mention those working on asymptotic safety around Martin Reuter and Jan Ambjørns Dynamical Triangulations as examples – there are more. In my experience String theory is powerful in Europe, but not as powerful as it appears to be in the US. Perhaps others can confirm this.

19. **Mitchell Porter**
May 1, 2017

Tim May said

'I never saw a "world view" based on these speculations… Not until "string theory"'

String theory isn't a random speculation. It was discovered by Veneziano *before* the standard model, supersymmetry, grand unification…, in an attempt to describe hadrons. It turned out to contain fermions, gauge bosons, and gravitons, as well as being the natural endpoint of the quest to unify through symmetry. There is nothing else like it.

The only serious strategic shift that string phenomenologists face so far, is a need to abandon the idea of weak-scale supersymmetry, and that was never an implication of string theory, it was a phenomenological assumption.

20. **NoGo**
May 1, 2017

I have also read the linked interview with Witten, where he says:
"Witten: There are not any interesting competing suggestions. One reason[…] is that interesting competing ideas (twistor theory, noncommutative geometry, …) tend to be absorbed as part of a larger picture in string theory. The competing interesting ideas have been very fragmentary and have tended to gain power when absorbed in string theory."
I am curious, and will be grateful if someone in the know can comment on, were these theories really "absorbed" by string theory in some meaningful way, or it's just another conjecture about properties M theory may have when it's found, or something in between?
Thanks.

21. **Jesper**
May 1, 2017

@ NoGo,

let me comment on noncommutative geometry. The 'non-commutative geometry' aspect emerging from ST does, in my opinion, not compare to Chamseddine and Connes approach – so I would definitely not accept that NCG has simply been "absorbed" into ST.

These two approaches to understand the standard model are in fact radically different: ST proposes via the landscape that the standard model is, essentially, a random occurrence whereas NCG proposes the exact opposite, that the standard model is unique.

Generally speaking, I think that if you should have a framework, that is large enough to encompass all other existing ideas, then it will most likely be empty.

22. **Peter Woit**
May 1, 2017

Mitchell Porter,

The problem with string theorist's "strategic shift" of dismissing the failure to find SUSY or extra dimensions at the LHC as just a "phenomenological assumption" is that finding such things at the LHC was the main argument string theorists used for the last thirty years when asked "how can your theory be tested?". If they make this new "strategic shift" they will be acknowledging that there are no longer any plausible prospects for experimentally testing string theory, putting the idea of string theory unification firmly outside conventional understanding of what is science.

23. **Peter Woit**
May 1, 2017

NoGo,

The question of the relation of twistor and non-commutative geometry to string theory is an extremely complicated one, with the first problem deciding what "string theory" is. As Jesper points out, I don't think non-string theorists would agree that these ideas have been "absorbed" into string theory.

Looking at active research topics pursued by "string theorists" in recent years (for instance by looking at talks at Strings 20XX) I think one finds "string theorists" moving into other fields. A very active area for instance is the study of scattering amplitudes, and while ideas coming from string theory have had some impact, a twistor theory expert might reasonably make the claim that ideas from twistor theory have been even more important there. If "twistor theory" was a term used in the way "string theory" is, a "twistor theorist" might have a good case that "string theory" has been absorbed by "twistor theory".

Of course, this level of discourse is just playing with ill-defined words, shall we say, "not even wrong"…

24. **martibal**
May 1, 2017

Last march there have been two colloquia at ICTP in Trieste, one day by Vafa, the next day by Connes. I do not know if the videos are available, but it is difficult to imagine two approaches to theoretical physics more different.

The claim that noncommutative geometry [NCG] would have gained power once absorbed within string theory [ST] is pretty much ambiguous. If one intends "power of scientific explanation", then this claim is a nonsense. If one intends "political or sociological power", then it may be true that NCG has gained visibility within the theoretical physics community when Seiberg-Witten map was – for a while – the hot topic in ST. But this period of time is over. I do not know what ST has kept from NCG, what I do know is that research in theoretical physics inspired by NCG has kept going on, with motivations that have nothing to do with ST, and with many beautiful developments that have nothing to do with ST neither.
That gaining visibility or "media power" is not the same thing as gaining "scientific power" is the whole raison d'´être of this blog, no ?

25. **A.J.**
May 1, 2017

Noncommutative Geometry is more than just Connes' attempt to get the classical Standard Model Lagrangian by looking at the Dirac operator on a limited set of NCG spaces. It's a very general way of thinking about 'spaces'. I'd be willing to bet that when Witten said that NCG had been enriched by String theory, he was referring to the variety of nontrivial ways in which string theory realizes such noncommutative spaces. This includes the Myers Effect and various noncommutative gauge theory constructions, and also more general ideas like matrix theory. Such realizations are often powerful because they give you new ways of looking at familiar examples and suggest non-trivial relations to other concepts (e.g., the Seiberg-Witten map).

That said, Connes' specific program of connecting Standard Model dynamics to noncommutative geometry can be thought of as a special case of the superconformal QFT geometry dictionary used in string theory. Urs Schreiber has pointed this out here a few times. It's quite peculiar that enthusiasm for this model instead of string theory has become a tribal marker.

26. **Urs Schreiber**
May 1, 2017

The close relation between Connes-style NCG and perturbative superstrings has been pointed out way back in 1993 by Fröhlich and Gawędzki , has meanwhile been substantiated by theorems by Roggenkamp and Wendland, following Kontsevich and Soibelman, and has been used by Soibelman to study aspects of the string landscape in terms of spectral triples. Review and comprehensive pointers to the literature are at PhysicsForums-Insights: *Spectral Standard Model and String Compactifications*.
Apart from the technical details discussed there, one highlight is that both approaches are KK-models that do agree on the critical spacetime dimension to be 4+6 = 10 (modulo 8, because NCG only sees it modulo 8).

27. **Peter Woit**
May 1, 2017

AJ and Urs,

Looking at the document Urs links to, I see all sorts of interesting mathematics and physics, but I don't see how the perturbative superstring has all that much to do with it. As usual when I disagree with Urs, what seems to me a tenuous and not very important connection is for him a "close relation". Sorry, but all this just seems to me to be turning interesting discussion about deep mathematical structures into a waste of time ideological discussion, and I'm not in the mood to participate, or even try and properly moderate such a discussion.

28. **piscator**
May 1, 2017

Europe is less faddish than the US. The tendency for everyone to jump on the same hot idea is less. This is good when the hot idea is evanescent nonsense and less good when the hot idea is a fundamental breakthrough.

It also involves many different countries, which encourages a larger number of distinct scientific hierarchies as to styles of science and the question of what are the important problems one should ask.

But still, things are more similar than different. Everyone reads the same arxiv.

29. **Jesper**
May 1, 2017

Hi A.J. and Urs Schreiber

I don't doubt that there is are connections between noncommutative geometry and string theory – and the one with 2d SCFT is obviously interesting – but the question was whether string theory had "absorbed" noncommutative geometry. Noncommutative geometry is a very large field, mostly in mathematics but clearly also in mathematical/theoretical physics and the notion that string theory somehow captures all that doesn't make sense to me. But perhaps this is a question of semantics.

Urs, in your very nice PhysicsForum piece you end with the sentence "A very interesting question to ask therefore is: which 2d SCFTs (if any) would lift the Connes-Lott-Chamseddine-Barrett spectral triple? These would be realistic string vacua." Does that imply that it is not yet known whether the connection between the Connes-Lott-Chamseddine model and these 2d models is real?

In my view, Chamseddine and Connes' work (and others) on the standard model is an interesting observation that might point towards a unified theory of quantum gravity. The questions are where the almost commutative structure originates from and how quantum field theory/quantum gravity fits into the picture. The answer could be string theory, it could be something else – I believe that we needs as many ideas as possible.

30. **martibal**
May 2, 2017

Claiming that NCG is subsumed by ST (or gained more power thank to ST) because spectral triples might be useful to study the landscape, or because both view spacetime as a dimension 2 objects (like several other approaches to QG by the way) sounds to me a bit like claiming that Riemann integral has gained more power since it has been widely used in ST.
It is not because something is used in ST that it is "subsumed" by ST. This tendency of ST to swallow everything and deny independent interest to any ideas outside ST is unbearable, and counterproductive: first it puts the discussion on a conflictual tone, that not everybody is interests to get along with; second it is a way not to study the motivations of the other ideas, that have nothing to do with ST.
But I agree with Peter that this kind of discussion just repeats itself again and again, and is a waste of time.

31. **Giotis**
May 2, 2017

Witten obviously is referring to the early 2000 medium size Noncommutativity revolution in String theory sparked (mainly) by his by now classic paper written together with Seiberg (you need basically a strong B-field to make Noncommutativity manifesting itself)

"String Theory and Noncommutative Geometry":

http://lanl.arxiv.org/abs/hep-th/9908142v3

32. **Anonyrat**
May 2, 2017

Linked from the above: how do physical theories generally make predictions anyway?
https://ncatlab.org/nlab/show/string%20theory%20FAQ#AsideHowDoPhysicalTheorieyGenerallyMakePredictionsAnyway

33. **RandomAnonymous**
May 2, 2017

^ That is a depressing amount of effort spent to reparameterise failure…

34. **Urs Schreiber**
May 3, 2017

> Urs, in your very nice PhysicsForum piece you end with the sentence "A very interesting question to ask therefore is: which 2d SCFTs (if any) would lift the Connes-Lott-Chamseddine-Barrett spectral triple? These would be realistic string vacua." Does that imply that it is not yet known whether the connection between the Connes-Lott-Chamseddine model and these 2d models is real?

Every 2d superconformal field theory yields a spectral triple as its "point particle limit". (This is a mathematical formalization of the familiar idea that we may view the quantum superstring from far away and see only its center of mass point motion, with the quantum oscillations about it appearing as different species and different internal degrees of freedom of the resulting effective spinning particle.)

But not every spectral triple necessarily needs to arise this way. Those that do would be called those that have a stringy UV-completion to a theory of perturbative quantum gravity coupled to gauge bosons and fermions.

35. **Urs Schreiber**
May 3, 2017

> Witten obviously is referring to the early 2000 medium size Noncommutativity revolution in String theory sparked (mainly) by his by now classic paper written together with Seiberg (you need basically a strong B-field to make Noncommutativity manifesting itself)

That's quite certainly what the quote was referring to, but since above the discussion quickly shifted to *Connes-style NCG*, it is worth recalling that this "spectral geometry", as it might possibly better be called, is a good bit richer than just some Moyal-noncommutativity of spacetime coordinates, and was understood to arise as the point particle limit of perturbative superstrings already in FröhlichGawędzki 93 .

One could argue that the main point of Connes-style NCG (namely: spectral triples) is not so much the non-commutativity of the algebra that is part of the triple (that's a nice side effect, that non-commutative algebras may be accomodated, too) but the main point is the Dirac-like operator in the triple (and the super-Hilbert space that, necessarily, comes with it, for it to be a linear operator on anything).

Instead, Connes-style NCG is about *extracting an effective target spacetime geometry* (possibly non-commutative, sure) *from the energy spectrum of a super-particle* (hence a spinning particle), hence to extract an effective target spacetime geometry as seen from quantum super-particles that roam in it.

This is noteworthy, since this is exactly foundational approach of perturbative superstring theory, only that there the worldline theory of that spinning particle is promoted to a worldsheet theory, since that turns out to be beneficial for the behaviour of the theory, as it provides a consistent means to incorporate interactions and counter-terms to arbitrary order (the higher string oscillation modes).

Moreover, as has been shown, this gives a richer supply of non-commutative backgrounds than Seiberg-Witten, in fact it gives a general stringy idea of how to think of the non-commutativity in a Connes-style non-commutative geometry: The non-commutativity is the remnant of the higher superstring oscillations as we pass to the superstring's point particle limit.

36. **lars**
May 10, 2017

Horgan: Do you still think string theory is "not even wrong"?

Woit: Yes. My book on the subject was written in 2003-04 and I think that it's point of view about string theory had been vindicated by what has happened since then. Experimental results from the Large Hadron Collider show no evidence of the extra dimensions or supersymmetry that string theorists had argued for as "predictions" of string theory. The internal problems of the theory are even more serious after another decade of research. These include the complexity, ugliness and lack of explanatory power of the models designed to connect string theory with known phenomena, as well as the continuing failure to come up with a consistent formulation of the theory"
— Peter Woit from linked article

You once said that "I use 'not even wrong' to refer to things that are so speculative that there would be no way ever to know whether they are right or wrong"

If that was the definition for "not even wrong" that you used when you wrote your book, I can see how an increase in the severity of the internal problems you refer to might vindicate your initial claim that string theory is "not even wrong", but it is not clear how results from the LHC could have vindicated such a claim.

"No way ever to know whether they are right or wrong" would seem to imply "no experiment".

Please note that I am not challenging your claim about the internal problems, about which I am not qualified to weigh in one way or another.

37. **Peter Woit**
May 11, 2017

lars,

I put "predictions" in quotation marks for a reason. Despite what they sometimes told the press, string theorists never had real predictions for the LHC from string theory. They did however have things they could point to which, if observed, would provide encouragement for some of the research directions that led to string theory (e.g. SUSY). It is these that have not worked out.

Theories of Everything

May 2, 2017
Categories: Book Reviews

I've written a review for the latest issue of Physics World of a short new book by Frank Close, entitled Theories of Everything. You can read the review here.

As I discuss in the review, Close explains a lot of history, and asks the question of whether we're in an analogous situation to that of the beginning of the 20th century, just before the modern physics revolutions of relativity and quantum theory. Are the cosmological constant and the lack of an accepted quantum theory of gravity indications that another revolution is to come? I hope to live long enough to find out…

Comments

1. **Doug McDonald**
May 2, 2017

"Cosmological observations appear to
indicate that this is a non-zero num
–
ber, with an order of magnitude so
big that it doesn't fit at all with what
one might expect from the Standard
Model and general relativity."

Please explain "big". I though that the (non stringy) particle physicists said "small",
by 30 or 120 orders of magnitude.

2. **Peter Woit**
May 2, 2017

Doug McDonald,
Oops, that's a mistake that seems to have crept in during the editing, and I wasn't paying enough attention in proof-reading. For "big", read "small".

3. **neil**
May 3, 2017

Despite the outstanding success of the standard model, I do not think we are in a position analogous to that of 1900 with apparently settled physics but for a couple of clouds on the horizon. Kelvin thought his clouds would eventually be explained by the "standard model" of his time. That is why he thought them mere clouds. I don't think anyone labors under that delusion today. Our clouds are looming cumulonimbus, and the big question is whether we can discover the physics needed to explain them given the limited tools available to us.

4. **Snowden**
May 3, 2017

A revolution a day keeps progress away – especially true in quantum gravity, where the revolution now is that the previous revolutions haven't gone anywhere. It is amazing that people confidently claim that properties that are true of every low energy Hamiltonian will somehow magically lead to insights about quantum gravity (http://vixra.org/abs/1703.0300)

5. **Peter Woit**
May 3, 2017

Doug McDonald,
Thanks for pointing out the problem. It's now fixed in the pdf here and online at Physics World.

6. **Pascal**
May 3, 2017

What about nonzero masses for neutrinos: is that compatible with the standard model?

7. **Low Math, Meekly Interacting**
May 3, 2017

What about dark matter? Is that not a "cloud"? I've read many an account of Einstein's doomed attempts to find a unified field theory. The general consensus being ignorance of two more forces of nature didn't exactly help his efforts to unify gravity and EM. What if dark matter completely up-ends our notion of what "matter" is? What if gravity has to be modified? Could a major part of the problem also be tied up in the answers to those questions?

8. **Peter Woit**
May 3, 2017

Pascal/LMMI,

Neutrino masses and dark matter are also possible "clouds". Unfortunately in neither case have they provided any promising hints for a revolutionary change in our best fundamental theory. This may be why Close points to the quantum gravity problem as one more likely to lead to such a change.

The problem is that all of these have received a huge amount of attention from theorists trying to interpret them as evidence for a new theory, without much to show for the effort. Maybe a very different cloud would be more promising. One of my personal favorites is the difficulty of non-perturbatively formulating chiral gauge theories such as the electroweak sector of the Standard Model.

9. **KJ**
May 3, 2017

Peter,
Your review certainly implies that your general impression of the book is favorable – you say Close is "on the right track"- but I finished the review not being sure whether or not you recommend the book, and if so, to what audience. Does Close have anything new to say about the situation that has not been covered already by some of the plethora of popular books on the Standard Model? Or is it more a case of "If you feel like you must read every popular book on quantum gravity, at least this one won't mislead you into multiverse mania".

10. **Peter Woit**
May 3, 2017

KJ,
It's a short popular book, much of which is a subset of what is covered in various longer books (including some by Close himself). He's trying to emphasize one particular question, about unification and the current state of affairs. But it is at the level of a non-technical essay, and if you've followed the issues discussed on this blog I don't think you'll find anything particularly new. My review mainly attempted to just convey the point of view of his essay, perhaps wasn't very good as a consumer guide.

Yes, one market for the book is for those who must read every popular book on fundamental physics, and for such readers I'd describe it as a sensible but unsurprising take.

11. **Low Math, Meekly Interacting**
May 4, 2017

I guess. But maybe lacking more specific observational or experimental information about the nature of dark matter is a lethal impediment, akin to operating under the assumption that EM and gravity are the only fundamental forces and then seeking to unify the forces of nature. Maybe it's just setting oneself up for certain failure? True, it's been a disappointing avenue of exploration so far, but what if it's impossible to get around nonetheless? Given the "nightmare scenario" that appears to be unfolding, I do wonder why there isn't much public discussion of such a possibility. I.e. maybe we'll never get close to completing this puzzle until we've found this one piece that's so obviously missing.

12. **AcademicLurker**
May 4, 2017

Maybe this belongs in the "Why string theory is still not even wrong" thread, but I don't want to derail the discussion there.

Peter, in your books both you and Lee Smolin dealt at length with the ways in which you thought the sociology of HEP theory was discouraging the exploration of new or unconventional approaches.

Since now post LHC new ideas seem to be more needed than ever, do you see any improvements in the sociology of the fundamental physics theory community relative to 2006?

13. **Paul**
May 4, 2017

AcademicLurker,

We are not yet in "post LHC" mode. Not by a long shot.

14. **Peter Woit**
May 4, 2017

Academic Lurker,

There have been changes in the sociology since 2006, some for the worse, some for the better. Net though I think we're worse off than in 2006.

One change for the better is increased skepticism about string theory. That string theory hasn't worked out as hoped and no longer provides a promising route to unification is now a widespread point of view in the physics community, probably even the dominant, majority one. Even lots of people who call themselves "string theorists" are now working on different ideas that have little to nothing to do with string theory.

On the still a problem front, I'd list the herd sociology of the subject. Yes, you can not work on string theory, but if you want a career you better stick to one of a small number of "hot" topics. If you have an idea that is not related to string theory or one of the latest hot topics, pursuing it is as likely to doom your career as ever.

There have unfortunately also been significant changes for the worse. The multiverse is one I go on about too much, but it's hard to over-emphasize what an awful idea it is to give up on conventional science in that way. I had some hope that negative LHC results about SUSY and extra dimensions would have a wake-up effect, but that does not seem to be happening. Those devoted to SUSY seem to be taking the line that losing their bets that they would be vindicated by the LHC is no reason to change their thinking.
Another major change for the worse I think is the way that most of the physics community seems to have decided that the reason string theory didn't work out was that it used too much mathematics (as opposed to just being a bad physical idea). The engagement with deep mathematics was one of the best aspects of string theory, and that kind of work is nowhere near as healthy as it used to be.

15. **Jesper**
May 4, 2017

Hi Peter

When you write "On the still a problem front, I'd list the herd sociology of the subject. Yes, you can not work on string theory, but if you want a career you better stick to one of a small number of "hot" topics. If you have an idea that is not related to string theory or one of the latest hot topics, pursuing it is as likely to doom your career as ever " — then I cannot agree with you more.

If this does not change then I think that young researchers must and will begin to consider alternative routes for funding. Personally (and I am not that young any longer) I have taken the consequence and am financing my research via crowdfunding. I just don't see any possibility within the academic world of theoretical physics of today to successfully — i.e. financed — pursue a research project that is both serious (which means, involves years if not decades of work) and independent of the existing power structures. I imagine that others might begin to consider alternative options too.

I don't mean to suggest that this will be easy — it is not! — but if the choice boils down to abandoning your own ideas or research altogether, then I imagine and hope that there will be some who simply refuse to accept the basic premise that research belongs in academia. In fact, there are also upsides to working outside of academia. And one thing that I have learned is that there are many people who are interested and willing to finance alternative ideas as long as they are certain that its serious research.

On another note: I agree with you on the role of mathematics in theoretical physics. In my view its possibly the most important guidance we have forward.

16. **Mitchell Porter**
May 4, 2017

Since people express so much agitation in a forum like this, about the theoretical physics establishment still pursuing its failed agendas, etc, may I point out that every month on hep-ph there are several dozen papers on models employing neither supersymmetry nor grand unification – to name two of the popular, post-1970s, beyond-standard-model research programs that are also part of the string theory edifice.

For example, there are papers on flavor symmetries, especially for the neutrino sector; and there are papers looking at rather minimal extensions of the standard model, in order to explain the extra phenomena like neutrino masses and baryon asymmetry. (An example of the latter would be the "SMASH" paper, that received a little media coverage recently.) There are always a handful of papers trying to challenge more basic conventional wisdom, in ways that look misguided. And there are still further papers which don't fit any of those categories.

I think all this would be apparent to anyone who actually checks the daily releases on arxiv. So it makes me wonder about the people who express agitation about the state of physics: who are they, and what are they angry or despairing about? Some of these people might be laypeople who only read a type of "secondary literature" – hype and anti-hype, in media and blogs. Some of these people might be theorists (amateur or professional) who have a theory of their own to push, and whose dissatisfaction is really that *their* theory doesn't get funded or isn't widely accepted. And some of these people may be part of physics culture, but are only focusing on the elite – the big institutions, the big conferences, the big-name theorists who do all the interviews. Perhaps they want these "leaders of the field" to reform themselves, not caring that there is a big wide world of universities and researchers developing a multitude of heterodox viewpoints away from the limelight.

This is just an impromptu analysis. I would like to know what it overlooks.

17. **Peter Woit**
May 4, 2017

Mitchell Porter,
hep-ph has always been mainly non string theory, with HEP phenomenologists traditionally a part of the subject opposed to string theory. The problem for hep-ph is the same now as it has been for a long time: no new -ph to study (except for experimental misfires like the 750 GeV diphoton).

If there's no new input from experiment, hopes for progress have to come from new theoretical tools and ideas, and that's supposed to be the realm of hep-th. hep-th has much less string theory on it these days, but just as many "string theorists", and it's there that the range of new ideas under consideration is problematic.

18. **Ted**
May 5, 2017

I'm curious to know: what would your advice be to young academics just starting off their careers, who want to explore new ideas? Leave academia and research independently in ones spare time? Go into a "hot" area without truly believing in it? Quit altogether?

19. **Peter Woit**
May 5, 2017

Ted,
I don't think there's any single good career strategy to suggest. Besides the making a living business, two problems with working independently are that you lose the experience of teaching a subject (which is the best way to deeply understand it) and you lose having others to regularly discuss things with. If you happen to be financially independent, it would probably be a good idea to find a high quality, friendly academic department which would allow you to have some kind of affiliation and even sometimes teach courses. Many academic departments will not turn away a qualified Ph.D. who shows up and offers to help with teaching basic courses.

If, like most people, you need a paycheck, I'd suggest trying to consider the widest range of possibilities for academic employment, looking at many institutions, many departments, many types of positions. People in the usual HEP theory career track often have narrowed possibilities because of no experience teaching.

At the very start of a career, as a graduate student, it's probably a mistake to decide that you know what the best unconventional research direction to pursue is. You need to learn your subject, and there are lots of different kinds of projects that will help with that, while making you a viable candidate for a postdoc. If you get a postdoc, you generally can spend your time doing what you want.

Where the standard career path becomes very problematic is going from such a post-doc to a longer term position, and it's in preparing for that transition that I'd suggest looking at any possibility you can think of. Trying to play the game of working on a "hot" topic you don't believe in, with the hope that this will get you tenure and freedom is something I don't think really works. Most likely you will end up just losing out to others playing the same game better than you, or after a decade of working on things you don't believe in, you'll no longer know how to work on things you do believe in.

20. **Michael Gogins**
May 5, 2017

Ted: I'm not a physicist, but I am a serious worker in a field dominated by academics, namely computer music of the sort that focuses on software synthesis and algorithmic composition, and I have concertized, published, and presented in the main venues of the field.

What I found is this. For the most part there has been no barrier to acceptance of my work arising from my not being an academic. However, I am quite clear that the quality and quantity of my work has suffered from not being in the academic context, where I would have had much more interaction with my peers, and would not have had to have a day job. I believe that even teaching helps, as one is still spending time in the thought-world of the field.

My advice to an aspiring physicist is (a) see if the lack of barrier is also the case in physics, but I wonder because there are a lot of physics crackpots and not many computer music crackpots, and (b) don't work independently if there is any way to make it work inside the academic world. But in that case, a serious theoretician would absolutely have to find a way to build a private space for the kind of deep thinking that is required. You need long stretches of just thinking and even of being quite alone. Perhaps one could keep two research tracks going — one to keep your funders and bosses happy and one to keep oneself happy. But that sounds like real overwork!

Conversely, for someone working outside academia, I think it would extremely helpful, perhaps vital, to live in a big center like New York or a major university town and try to get to know some of the leaders in the field. I have been able to do this in music, and it has been really helpful.

21. **Jeff M**
May 5, 2017

Ted : My advice for young academics. I'm a mathematician, I've been on a bunch of search committees, so I have a good idea of what the field is like for young math types. It's much worse in physics, since physics isn't a service department. At my university, we have 32 people in the math department, and 5 in physics. I would second everyone who says try to stay in academia if you really want to be part of the field, but be willing to teach anywhere. Might mean a much heavier load, many fewer connections, but it's a different world, and if you're out of it, its very hard to stay active. I know someone (in math) who did what Peter suggested, and got himself affiliated with a very good university, who didn't pay him anything. He won the lottery, so he didn't need money. But I think a lot of people in physics just dismiss the idea of teaching at a liberal arts school, or a second rank state school like mine. But you can have a good life doing it, stay active, find interesting things to do. I went to a liberal arts college, the physics professor I did most of my work with stayed very active, published, and after I left he actually started a research institute. One of the people associated with the institute won the Nobel. My son, who was at the same school this year, worked with a new physics prof, serious research degree, I have no doubt she's going to stay active. He's transferring to a research one place, I'm very curious what he's going to think of the difference

22. **Scott**
    May 5, 2017

    As a physicist, but not HEP, I always thought one of the "clouds on the horizon" was the matter/anti-matter asymmetry. Would that qualify or is there a feeling among HEP that while an issue, it is secondary.

23. **Peter Woit**
    May 5, 2017

    Scott,
    Sure, that maybe a "cloud", but one problem with that is that it depends a lot on your understanding of what happened during the big bang, so it's not a clear indication that there's something wrong with our fundamental theory of particle physics (as opposed to the details of our understanding of very early universe cosmology).

24. **Peter Woit**
    May 5, 2017

    A correspondent points out to me that the idea of quantum theory requiring replacing our understanding of space and time goes way back, with Russian physicist Matvei Bronstein discussing this already in 1936. For more about this, see

    http://blogs.scientificamerican.com/guest-blog/2011/07/14/why-is-quantum-gravity-so-hard-and-why-did-stalin-execute-the-man-who-pioneered-the-subject/

    or

    http://people.bu.edu/gorelik/cGh_Bronstein_UFN-200510_Engl.htm

25. **Chris W.**
    May 9, 2017

    The physical motivation is less clear-cut than the arguments put forth by Bronstein, but this fairly well-known quote of Einstein (on Sabine's *Backreaction*) indicates that he had been worrying about this for some time as of 1916.

26. **Thomas**
    May 10, 2017

    off-topic, but .. care for 28 000 pages of notes from Grothendiek?
    https://grothendieck.umontpellier.fr/archives-grothendieck/

Some Quick Items

May 10, 2017
Categories: Uncategorized

A few quick items, I may use this posting to add a couple more later, the next posting will discuss today's letter to Scientific American about inflation.

- Today's LHCC meeting at CERN had reports from the LHC machine and experiments. About two weeks to go before collisions and data-taking start again.
- Physics Today has a report this month on the LHeC proposal, something that has not gotten as much attention as it deserves. This is a proposal to collide protons and electrons, by building a new electron machine and a detector at a collision point with the LHC beam. Unlike proposals for a 100 TeV proton-proton machine that are getting a lot of attention, this would not push the energy frontier, but it would cost a great deal less (estimate is half a billion to a billion, vs. multiple tens of billions for the 100 TeV machine). In a few years when the question of a follow-on machine to the LHC starts to get very pressing, this idea and the HE-LHC idea (higher field magnets in the LHC tunnel, maybe doubling the energy) may get a lot more attention as the only financially viable ways forward.
- The Université de Montpellier today has started to make accessible about 18,000 pages of its archive of Grothendieck's mathematical writings. For anyone interested in Grothendieck's work, this should keep you busy for a while…

Update: A few more.

- I was sorry to hear of the recent death of Cecile DeWitt-Morette, a mathematical physicist responsible for the Les Houches physics summer school. Her books on geometry and physics (with Yvonne Choquet-Bruhat) were influential, and her more recent book with Pierre Cartier on Functional Integration contains a lot of interesting material.
- Every so often I'd wondered what the Chudnovsky brothers have been up to, some information about this emerged today.
- For a story about problems at the science magazine, Nautilus, which is having trouble transitioning from its original Templeton Foundation funding, see here.

Comments

1. **John McAllison**
   May 10, 2017

   Recently, CERN decided to stop the interested tax paying public from having a look at the morning meetings under the LHCC coordination program:
   https://indico.cern.ch/category/6740/

   So it's nice to see CERN hasn't blocked yet public access to the LHCC meetings.

2. **egan**
   May 11, 2017

   It's Montpellier with two L

3. **Peter Woit**
   May 11, 2017

   egan,
   Thanks, fixed.

4. **sdf**
   May 11, 2017

   Appears (?) that Cedric Villani will run for en marche
   http://www.bbc.com/news/world-europe-39881266

   (this is reported in several places but if you go to the website of candidates his name is not there so I'm not sure…)

5. **anon**
   May 14, 2017

   On a mathematical note, Hironaka has a preprint on his website claiming to prove resolution of singularities in characteristic p, that it seems nobody noticed for two months because he didn't post it to the arxiv, etc.

6. **neil**
   May 14, 2017

   Out of curiosity, I looked at a few pages of Grothendieck's writings. I just wanted to witness the great man's handwriting. Transcribing and translating these 18,000 pages will be an enormous job, if that happens.

7. **Peter Woit**
   May 14, 2017

   anon,
   From what I hear, experts in the field only found out about this a few days ago, are just starting to look at the proof. It would be a great story if this turns out to be right.

8. **Matt Grayson**
   May 15, 2017

   If true, it will complete one of the more famous mathematical anecdotes of the past hundred years.

9. **David Roberts**
   May 15, 2017

   @Matt can you remind us? No doubt it's something going back to the Italian School.

10. **none**
May 15, 2017

@Roberts: @Matt is commenting on the possibility that Hironaka has also proved resolution of singularities in positive characteristic, decades after his Fields Medal winning proof in characteristic zero.

11. **none**
May 15, 2017

@Roberts: A link to Hironaka's preprint
http://www.math.harvard.edu/~hironaka/pRes.pdf

12. **David Roberts**
May 16, 2017

@none yes, but what is this famous mathematical anecdote that it would be completing? I can't recall any surrounding resolution of singularities, much less any that is a hundred years old.

A Cosmic Controversy

May 10, 2017
Categories: Favorite Old Posts, Multiverse Mania

A couple months ago Scientific American published an article by Ijjas, Steinhardt and Loeb (also available here), which I discussed a bit here. One aspect of the article was its strong challenge to multiverse mania, calling it the "multimess" and accusing multiverse explanations of being untestable and unscientific.

Yesterday Scientific American published, under the title A Cosmic Controversy, a rebuttal signed by 33 physicists, together with a response from the authors, who have also set up a webpage giving further details of their response. Undark has an article covering this: A Debate Over Cosmic Inflation (and Editing at Scientific American) Gets Heated.

As Ijjas, Steinhardt and Loeb point out on their webpage, the story of this letter is rather unusual. It was written by David Kaiser and three physicists well-known for their outspoken promotion of the multiverse (Guth, Linde and Nomura). Evidently these authors decided they needed reputational support on their side, and sought backing from other prominent names in the field (I'm curious to know who may have refused to sign if asked…). Their letter starts out with a claim to represent the "dominant paradigm in cosmology" and notes the large number of papers and researchers involved in studying inflation.

If you read carefully both sides (IS&L and GKL&N) of this, I think you'll find that they are to a large degree speaking past each other, with a major problem that of imprecision in what one means by "inflation". To the extent that there is a specific identifiable scientific disagreement, it's about whether Planck data confirms predictions of the "simplest inflationary models." IS&L write:

> The Planck satellite results—a combination of an unexpectedly small (few percent) deviation from perfect scale invariance in the pattern of hot and colds spots in the CMB and the failure to detect cosmic gravitational waves—are stunning. For the first time in more than 30 years, the simplest inflationary models, including those described in standard textbooks, are strongly disfavored by observations.

whereas GKL&N respond:

> there is a very simple class of inflationary models (technically, "single-field slow-roll" models) that all give very similar predictions for most observable quantities —predictions that were clearly enunciated decades ago. These "standard" inflationary models form a well-defined class that has been studied extensively. (IS&L have expressed strong opinions about what they consider to be the simplest models within this class, but simplicity is subjective, and we see no reason to restrict attention to such a narrow subclass.) Some of the standard inflationary models have now been ruled out by precise empirical data, and this is part of the desirable process of using observation to thin out the set of viable models. But many models in this class continue to be very successful empirically.

I take this as admission that IS&L are right that some predictions of widely advertised inflationary models have been falsified. Of course, if these had worked they would have been heavily promoted as "smoking gun" proof of inflation, as was demonstrated by the BICEP2 B-mode fiasco. After BICEP2 announced (incorrectly) evidence for B-modes, Linde claimed this was a "smoking gun" for inflation (see here) and the New York Times had a front page story about the "smoking gun" confirmation of inflation vindicating the ideas of Guth and Linde. A couple months later, before the BICEP2 result was shown to be mistaken, Guth, Linde and Starobinsky were awarded the $1 million Kavli Prize in Astrophysics.

GKL&N don't mention the sorry story of the BICEP2 B-modes, what they have to say about this is

> the levels of B-modes, which are a measure of gravitational radiation in the early universe, vary significantly within the class of standard models…
>
> The B-modes of polarization have not yet been seen, which is consistent with many, though not all, of the standard models.

About the IS&L "unexpectedly small (few percent) deviation from perfect scale invariance" all GKL&N have to say is

> The standard inflationary models… predict the statistical properties of the faint ripples that we detect in the cosmic microwave background (CMB). First, the ripples should be nearly "scale-invariant"

This doesn't seem to address at all the IS&L claims, which they make in more detail as

> The latest Planck data show that the deviation from perfect scale invariance is tiny, only a few percent, and that the average temperature variation across all spots is roughly 0.01 percent. Proponents of inflation often emphasize that it is possible to produce a pattern with these properties. Yet such statements leave out a key point: inflation allows many other patterns of hot and cold spots that are not nearly scale-invariant and that typically have a temperature variation much greater than the observed value. In other words, scale invariance is possible but so is a large deviation from scale invariance and everything in between, depending on the details of the inflationary energy density one assumes. Thus, the arrangement Planck saw cannot be taken as confirmation of inflation.

GKL&N argue for three other confirmed predictions of inflationary models:

> Second, the ripples should be "adiabatic," meaning that the perturbations are the same in all components: the ordinary matter, radiation and dark matter all fluctuate together. Third, they should be "Gaussian," which is a statement about the statistical patterns of relatively bright and dark regions. Fourth and finally, the models also make predictions for the patterns of polarization in the CMB, which can be divided into two classes, called E-modes and B-modes. The predictions for the E-modes are very similar for all standard inflationary models

On these issues I don't see anything from IS&L and would love to hear from an expert.

The main issue here comes down to the question of the flexibility vs. rigidity of inflationary models. Is the inflationary paradigm rigid enough to make solid predictions, or so flexible that it can accommodate any experimental result? GKL&N are making the case for the former, IS&L for the latter, and they point out the following quote from Guth himself:

> when asked via email if they could name any pro-inflation scientists who believe that the theory is nonetheless untestable, the trio pointed to a video of a 2014 panel during which Loeb asks Guth directly whether it's possible to do an experiment that would falsify inflation.
>
> "Well, I think inflation is a little too flexible an idea for that to make sense," Guth replied.

A fair take on all this would be to note that it's a complicated situation, and I doubt I'm the only one who would like to see an even-handed technical discussion of exactly what the "simplest" models are and a comparison of their predictions with the data. Claims to the public from one group of experts that Planck data says one thing, from others claiming it says the opposite are generating confusion here rather than clarity about the science.

I'm strongly on the side of IS&L on one issue, that of the danger of theories that invoke the multiverse as untestable explanation. I don't think though that they make a central issue clear. The simple inflationary models whose "predictions" for Planck data are being discussed involve a single inflaton field, with no understanding of how this is supposed to couple to the rest of physics. One is told that eternal inflation implies a multiverse with different physics in different universes, but in a single inflaton model this physics should just depend on a single parameter, and such a theory should be highly predictive (once you know one mass, all others are determined). What's really going on is that there is no connection at all between the simple single field models that GKL&N and IS&L are arguing about, and the widely promoted completely unpredictive string theory landscape models (involving large numbers of inflaton-type fields with dynamics that is not understood).

I think IS&L made a mistake by not pointing this out, and that Guth, Linde, Nomura and some of the signers of their letter (e.g. Carroll, Hawking, Susskind, Vilenkin) have long been guilty of promoting the defeatist pseudo-scientific idea that "evidence for inflation is evidence for a multiverse with different physics in each universe, explaining why we can't ever calculate SM parameters". By defending the predictivity of "inflation" while ignoring the "different physics in different parts of the multiverse" question, I think many signers of the GKL&N letter were missing a good opportunity to make common cause with IS&L on defending their science against an ongoing attack from some of their fellow signatories.

Update: There are sources with technical details of the arguments being made by both parties:

- For the IS&L side, see here and here.
- For the GKL&N side, see here and here.

I'll try to find time to read these carefully and try and understand exactly what claims are being made. Would love to hear from experts who may have looked at these and are better placed to evaluate what the arguments are.

This controversy continues to involve an unusual level of PR rather than science. The Stanford press office has just put out this, where Linde makes it clear that he sees this as a political and PR fight:

> Linde added that he worries about the younger generation of scientists getting the wrong impression from this story. "I don't want them to read this article and think that they are spending their time on inflationary theory in vain. But the enthusiastic support that we are receiving makes us optimistic that this is not going to happen," he said.

There's no mention in this press office story of their last press office story about Linde and inflation, which promoted the BICEP2 "smoking gun" vindication of Linde and inflation.

Some more takes on this story can be found here, here and here.

Update: Another article about this is at the Atlantic. A crucial issue here is whether inflation has now entered the realm of unfalsifiability. Given any likely new data that could appear, is there any way it could falsify inflation, or can one just come up with some version of inflation that will match it? Guth and Linde seem at times to be taking the attitude that this is fine, I take Steinhardt et al. as pointing out that this is no longer conventional science. Replacing falsifiability by arguments about how many prominent people have signed your letter is a worrisome development. From the article:

> In 2014, for example, Loeb asked Guth during a panel discussion if inflation was falsifiable—whether you could design an experiment to disprove it. Guth called that a "silly question," suggesting that inflation was an umbrella over many theories, making it very hard to knock them all out at once. The hope right now, he says, is to use observation and further theory to winnow inflation down to just one specific version.
>
> "Our point is that this kind of reasoning is inconsistent with normal science and cannot be resolved by invoking authority," Ijjas, Steinhardt, and Loeb wrote to The Atlantic.

Update: More about this from John Horgan.

Update: It seems that Andrei Linde is a Lubos Motl fan. This is getting very weird. It's not normal to respond to a scientific argument by enlisting letter writers on your behalf, even less normal to put your university press office to work on a response, and truly far out there to think that it's helpful to have Lubos announce that you have eaten from the tree of knowledge and that your opponents are imbeciles.

Update: For a sensible take on this that I think likely reflects well the views of most mainstream cosmologists, see Peter Coles.

Update: IJS have put together a Fact-Checking page. It lists the four "predictions" of inflation claimed to agree with experiment by Linde et al. and gives four references to papers published by Linde touting different "predictions" for the same quantities, predictions not agreeing with experiment.

This month's Scientific American has a bizarre cover story by Nomura on "The Quantum Multiverse", see here.

All in all, I don't know what other people's reactions to this have been, but before this started I was a lot more sympathetic to the argument that Steinhardt was treating the case for inflation unfairly.

Update: Yet more coverage trying to make sense of this, from Nick Stockton at Wired.

Comments

1. **John**
   May 10, 2017

   I found the following talk by Steinhardt, where he makes arguments similar to those in the scientific american article, particularly clear:
   http://vms.fnal.gov/asset/detail?recid=1944338

2. **Lino D'Ischia**
   May 10, 2017

   I thought this was bit ironic:
   GKL&N write:
   "Some of the standard inflationary models have now been ruled out by precise empirical data, and this is part of the desirable process of using observation to thin out the set of viable models. "

   It would seem that the multiverse puts an end to "using observation to thin out the set of viable models."

3. **Ilyas**
   May 11, 2017

I think it might also be worth pointing out the value (yet again) of Scientific American in providing a great platform for disparate views and giving them air time. These articles remind us that just because there might be a “scientific orthodoxy” does not mean it is correct. The challenge to String Theory was carried and written about very well in SA who refused to be pushed about by the orthodoxy who challenged the challenger (e.g. people such as you) by bringing together many “prominent” scientists with leading faculty positions, and effectively saying “you cannot be correct since look at all these people who occupy top positions and who support string theory – they cannot be wrong”. We should applaud SA for continuing to endorse its fine investigative tradition.

4. **Another Anon**
   May 11, 2017

   Yes, I agree with Ilyas. Publishing an article about orthodox inflation is not an interesting thing for Scientific American to do. It’s like criticising a newspaper for not printing a “nothing much happened today” on a quiet news day. The story is the challenge to the orthodoxy.

   And I know that one of the signers is privately dubious of inflation. If a bunch of big names ring you up and ask you to sign their petition, you’re going to do it.

5. **hyd**
   May 11, 2017

   Even if all the people of this generation and the preceding generations were content with the idea that the SM parameters be inexplicable but a result of playing dice, I’m sure there would be people in the next generation and the next next generation … insatiable with that idea and looking to inquire further, as history has repeatedly told us about human’s inextinguishable sense of curiosity. The attempt of silencing heretics by a bunch of big names is as amusing as ‘the 100 scientists pronouncing Einstein’s theory of relativity is wrong’.

6. **tulpoeid**
   May 11, 2017

   This wild attack from the side of theistic theories makes me sad. Statements like “the B-modes of polarization have not yet been seen, which is consistent with many, though not all, of the standard models” fill me with wrath. Keep up the good work and congratulations for putting names such as Hawking’s in the same sentence with pseudo-scientific.

7. **Jonatan**
   May 11, 2017

   I’m not a cosomologist but I’ve followed the debate between Steinhardt et al. and the pro-inflation side for a while.

   To me, the lack-of-prediction issue raised by Steinhardt is very simple, fundamental and philosophical in nature.

   As I understant it, the situation is thus: inflation provides a mathematical model/framework that CAN produce a universe like the one we see with respect to certain observables (geometry of spacetime, statistical distribution of CMB anisotropies, etc…). These are the “predictions” celebrated by GKL&N. However, quite generically (when considering quantum effects), it also produces all other possible universes with respect to these observables (the multiverse/multimess).

   Therefore it predicts everything/nothing (sounds familiar ?). How could the fact that we observe a flat, isotropic, etc universe be counted as evidence for inflation if we could have just as well observed a curved or open or anisotropic one according to inflation ? Why is this obvious and deep problem not more openly acknowledge by the prononents of inflation is beyond me.

   Steinhardt has provided technical explanations elsewhere for why this is the conclusion one should reach about the state of the inflationary paradigm at the moment, and those explanations seemed quite convincing to me. I haven’t heard any convincing counterargument to this general conclusion yet. Basically: the very concept of prediction becomes meaningless in the context of a bubbling multiverse. Peter is of course quite familiar with this epistemological fact.

   The second problem Steinhardt raises is equally damning for inflation. The fine-tuning of the initial conditions for the parameters of the inflaton field (a problem acknowledged by Sean Carroll in his blog entry on the subject) seems to be equal if not higher than the one required by standard big bang cosmology to produce a flat, isotropic universe like the one we see. I think Penrose have also mentionned that point in the past. So basically, it seems that you’re left with a choice of fine-tuning the initial conditions of standard big bang cosmology, or fine-tuning the initial conditions of a yet unknown, unseen, “inflaton” field…

   If true, this is highly ironic considering inflation was invented precisely to avoid this fine-tuning of initial conditions in standard big bang cosmology.

8. **Ben**
   May 11, 2017

   I think one of the problems is that there’s “inflation” – the idea of an early period of exponential expansion driven by something like an effective scalar field, and then there’s “inflation” – a more complete description of where that field comes from and how we got into the inflating state to begin with.

   The former seems to make testable (but maybe not unique) predictions, such as near-homogeneity, adiabatic perturbations, etc. The latter seems to screw all that up with goofy things like multiversal bubble nucleation, eternal inflation, and so on.

   So some (Steinhardt et al) argue that since attempts to come up with a complete picture of inflation wind up being non-predictive, that the whole thing is intellectually bust. Others (some of the inflation theorists) disagree on “predictivity” grounds and get into arguments about multiverse stuff. Still others (most experimentalists) would rather stay out of the weird foundational stuff, focus on “inflation” as an incomplete model for a specific epoch of history, and test the “predictions” that way.

   I think it’s hard to call the experimentalists wrong, since there’s basically no complete model of the early universe, inflationary or otherwise. Even Steinhardt is pretty clear that his pet ekpyrotic scenario has serious gaps.

9. **Shah-hid**
   May 11, 2017

   Here Dr. Neil Turok gives an epic summary of inflation’s curiosities:

   https://www.youtube.com/watch?v=g9fyn0mZEnY

10. **Pasa Dina**
    May 11, 2017

    Thanks for all the relevant links (as usual), Peter.
    The Forbes article – sigh.
    The author totally misses what the debate is about. He thinks it is about the variety of possible potentials rather than the initial conditions or multiverse. And he asserts that

everyone agrees there had to be rapid expansion, apparently missing entirely the bounce.

11. **cosmology grad student**
May 11, 2017

“One is told that eternal inflation implies a multiverse with different physics in different universes, but in a single inflaton model this physics should just depend on a single parameter, and such a theory should be highly predictive (once you know one mass, all others are determined).”

This is a key point. For a single-field slow-roll model, eternal inflation affects predictions of e.g. things you can observe in the CMB only in that instead of one particular distribution of classical spacetime geometries after inflation (drawn from a power spectrum with one particular value of e.g. spatial curvature, spectral index, “average temperature variation” = delta rho/rho, …) there is another, different distribution (instead of drawing from a fixed power spectrum at the end of inflation, there is some probability distribution of power spectra).

12. **neil**
May 11, 2017

I read somewhere, possibly here, that the strength of a theory depends as much on what it excludes as what it explains. In this debate, I think Guth et al focus on what they believe inflation theory explains whereas Ijjas et al focus on what it excludes (nothing?)

13. **skydivephil**
May 11, 2017

I am the producer of a series of films on early universe cosmology and have interviewed Guth, Nomura and many other supporters, and critics of inflation (including those that promote others models such as CCC, string gas cosmology and the pre big bang).

I am not a cosmologist but someone interested in cosmology and have spent many hours discussing these issues with some of the leading people in the field.
In our film on eternal inflation we interview George Efstathiou who gave the press conference for the cosmology results for Planck.
Efstathiou claims that the Planck data favour models of inflation that are eternal and therefore a multiverse.
you can see that 12:41 into our film here:
https://www.youtube.com/watch?v=QqjsZEZMR7I

Whats interesting is that in the ISL paper they make the same claim:
“The plateau-like potentials selected by Planck2013 are in the class of eternally inflating models,”
We asked Guth and Nomura to respond to many of the criticisms of the ISL paper and so I hope you will find our film interesting. Nomura claims a method of how to falsify eternal inflation.
What I also find interesting is that Steinhart in his paper “Cosmological Perturbations: Myths and Facts” said
“We will argue below that inflation and the cyclic model make firm predictions for the characteristic mass scale and equation of state during inflation, the spectral tilt of the scalar perturbations and the gravitational wave amplitude.”
He also states that Ns should be roughly equal to .95 in inflation , Planck measured .96 as far Im aware.
All of this was written well after it was realised that inflation led to a multiverse.
We also discuss other ways to more directly probe for the multiverse (and not just bubble collisions which we also discuss) and in other films how to probe for alternatives to inflation.

My recollection of Steinhardt and Turok’s book “Endless Universe” is that inflation makes 6 predictions and 5 have been confirmed by experiment but the last hasn’t and they predicted it would fail at the last hurdle. Hence this justifies the notion of smoking gun. But they accepted it had past the first 5. This is the prediction B mode polarisation and primordial gravitational waves. In other words one could experimentally discriminate between inflation and the Ekpyrotic cyclic model by this signal assuming its from gravitational degrees of freedom in the early universe and not primordial magnetic fields, hence a detection of this signal would rule out this cyclic model and others cyclic models (Penrose’s CCC for example).

It would not rule out all competitors to inflation though. String gas cosmology would still be in the game. But string gas cosmology (according to Ali Nayeri who we interviewed) predicts a red tilt for the power spectrum and blue tilt for the gravitational wave spectrum whereas inflation predicts both should be red tilted. There may be an exception in some inflationary models but then they would predict other non guassianties. So the picture is more complicated than the smoking gun headlines implied but I believe it has some truth. In others words different inflationary does may predict different strength of the signal but a common feature is its tilt.

I think its clear inflation has built up a large body of evidence and is the leading horse in the race to describe the early universe. However it hasn’t crossed the finishing line yet and until then it its well worth exploring other alternatives. But these other alternatives are usually cyclic models which in some sense implies a world ensemble separated in time rather than in inflating space. It seems pretty much no one in this debate is arguing for a single universe from one big bang, that is a fact. Both sides that you quote in this controversy say there were multiple big bangs; the debate is, are they separated in time or space? This is something that I think is a very profound feature of this debate.

14. **Jonatan**
May 11, 2017

Do proponents of eternal inflation assert that all bubble/pocket universes of the multiverse are flat/isotropic/scale invariant/adiabatic etc… (I guess not since Guth says that “anything that can physically happen will happen an infinite number of times”)

Because if they don’t, then I don’t see how observing any of this in this universe can count as evidence of eternal inflation. After all, someone standing in another pocket universe that is not flat/isotropic/scale invariant/adiabatic (and there would be an infinite number of those) would then be forced to conclude that eternal inflation cannot be correct if these features are true predictions of eternal inflation…

15. **Peter Woit**
May 11, 2017

All,
I’m not competent or willing to moderate a general discussion of cosmology here. If you have knowledgeable comments specifically about the topic of the posting, please contribute them, I’m going to be deleting others.

16. **Shantanu**
May 12, 2017

Peter let me make a couple of sociological points. Its again sad that none of these articles menion (or interview) Demos Kazanas even though his 1980 paper says in plain English that accelerated expansion could solve the horizon problem. Also its interesting that there are people from Harvard on both sides of this debate
cosmology grad student : I think given any observations you can concoct any model of inflation. There are even models of open inflation (despite the claim that inflation

predicts \Omega=1)

17. **Urs Schreiber**
May 12, 2017

The key point that is underappreciated in discussions here is the one that Guth et al. make in their article, where it says.

> They contend, for example, that inflation is untestable because its predictions can be changed by varying the shape of the inflationary energy density curve or the initial conditions. But the testability of a theory in no way requires that all its predictions be independent of the choice of parameters. If such parameter independence were required, then we would also have to question the status of the Standard Model, with its empirically determined particle content and 19 or more empirically determined parameters.

Indeed, this is how physical theories make predictions generally: first to fix some of the parameters to specify a model in the theory (a solution), then the remaining properties of that model are its predictions.

If one took the standpoint that the *theory* as opposed to its models is to make fairly unique predictions as such, then none of the usual theories would fit.

The usual established theories (as opposed to the models built inside them) don"t even have large finite (say of cardinality 10^500) but have usually hugely non-finite-dimensional spaces of solutions.

Those readers who really think this is controversial need to pause for a moment to think about this elementary fact.

For instance the solution space to Einstein gravity, unconstrained, is humongously infinite dimensional and says nothing specific about the observable world, it doesn't make any predictions whatsoever — not before we pick in there a very low-dimensional parameter space of models, say the 3-dimensional subspace of simple FRW models. We pick such subspaces not for some *a priori* reason, but because we match these three parameters to experimental observation. The remaining properties of the FRW models within the theory of Einstein gravity, that is the predictions of this model.

Similarly quantum field theory as such makes virtually no predictions, not before some model is specified. The best that QFT by itself can say is that the universe is described by some local Lagrangian density on some space of fields such that quantum anomalies cancel. But the moduli space of all possible field species and all possible local Lagrangians on them, that's a humongously infinite-dimensional space. No prediction is to be derived from that alone. Predictions are only being derivable once loads of parameters are specified: First we specify (by hand, because it fits observation) the local Lagrangian density to be of Einstein-Yang-Mills-Dirac-Higgs-type, then we specify the exact particle species, then their Yukawa couplings and so on. Only once all these parameters have been fixed by hand does the theory start making predictions.

With the theory of cosmic inflation it is exactly the same.

This doesn't imply that cosmic inflation is right. But this means that if you think it is wrong for *this* reason, then you would have to regard every established physical theory as wrong, too. Because they all share this feature. A physical theory does not make predictions before models have been picked inside it, and they all admit alternative models that differ drastically from the one you want to pick because it fits observation.

18. **Peter Woit**
May 12, 2017

Urs,
You continually make this claim that two completely different things are "exactly the same", I'll just point out that this FAQ applies here

http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=isnt-string-theory-just-as-predictive-as-quantum-field-theory

19. **Jonatan**
May 12, 2017

Urs,

Steinhardt's point, unless I missunderstand him, is that generically, in an eternally inflating multiverse, even once you've chosen a specific inflaton potential and parameters, because of quantum effects, the values of observables such as spacetime geometry and the statistical distribution of CMB anisotropies, will take any possible values in the ensemble of pocket universes.

If that's correct, than how can specific values for these observables in our universe be considered "predictions" of eternal inflation as they usually are ?

What should people finding themselves in a pocket with open spacetime geometry or non-gaussian distributed anisotropies be expected to conclude about eternally inflating models ?

20. **Urs Schreiber**
May 12, 2017

> spacetime geometry and the statistical distribution of CMB anisotropies, will take any possible values in the ensemble of pocket universes.
>
> If that's correct, than how can specific values for these observables in our universe be considered "predictions" of eternal inflation as they usually are ?

To repeat, this is the usual state of affairs as it has always been: We make some observations about the universe around us ("pocket universe", if you really like to say that), fit a model of our theory to that set of data and *then* make predictions for all the *remaining* observations.

This is no different from how we predict anything, say the weather. We first measure the mess that we find ourselves in, then you apply theory to that to see how it evolves.

I find it bemusing that many participants here have effectively become strong Hegelians, demanding a derivation of the entire universe from pure thought alone, without experimental input, without model building, and complaining that available physical theories are not like this. They never have been.

Maybe one day we will have such a theory, and Hegel will be vindicated. It's a logical possibility that such a theory exists. But if it does, this will be cause of awe and amazement, not something to take for granted, while as long as it does not, everything is business as usual.

21. **Urs Schreiber**
May 12, 2017

Steinhardt et. al write here:

> The Planck satellite results […] are stunning. For the first time in more than 30 years, the simplest inflationary models, including those described in standard textbooks, are strongly disfavored by observations. Of course, theorists rapidly rushed to patch the inflationary picture but at the cost of making arcane models of inflationary energy and revealing yet further problems.

In view of the [remarkble likelyhood plot] that PLANCK15 produced (fig 12 in [PLANKC 15 XX "Constraints on Inflation"]), this is an unexpected statement. Right there in the middle of the plot sits Starobinsky's R^2 inflation, the first model for inflation ever proposed. The data does not significantly discriminate between Starobinsky inflation and other plateau-type inflationary models but it does prefer this class.

22. **Peter Woit**
May 12, 2017

Urs,
Which of the following possible alternatives do you think is more likely?

1. Ijjas, Steinhardt and Loeb are ignorant about basic facts of their subject like the one you are explaining here.
2. They aren't.

Personally I'm inclined to 2, and in general to the assumption that both sides in this argument know a lot more about the subject than I do (and than you do). Given that, it might be best to pay close attention to the technical counter-arguments each is making when they discuss the same issue and see if one can figure out what the story is there, leave the accusations of incompetence and not knowing what they are talking about to Lubos.

23. **Marc Nardmann**
May 12, 2017

Urs Schreiber wrote: "But the moduli space of all possible field species and all possible local Lagrangians on them, that's a humongously infinite-dimensional space. No prediction is to be derived from that alone. Predictions are only being derivable once loads of parameters are specified: First we specify (by hand, because it fits observation) the local Lagrangian density to be of Einstein-Yang-Mills-Dirac-Higgs-type, then we specify the exact particle species, then their Yukawa couplings and so on. Only once all these parameters have been fixed by hand does the theory start making predictions."

We have to specify only the particle field species (leptons, quarks, gauge bosons, Higgs), the symmetry group (spacetime symmetries and internal symmetries), and how the particle fields transform under those symmetries. (This input is essentially discrete, not "humongously infinite-dimensional".) The Lagrangian density is then the most general function that is invariant under the group action; we do not have to put it in by hand. Its renormalisable part turns out to be determined by a small number of parameters (coupling constants, masses, …). Mathematically speaking, the Lagrangian arises from a theorem, not from a definition. This is considerably less arbitrary than it sounds in your description.

24. **Jonatan**
May 12, 2017

Urs Schreiber wrote: "We make some observations about the universe around us ("pocket universe", if you really like to say that), fit a model of our theory to that set of data and then make predictions for all the remaining observations."

With all due respect, I don't think you understand what the point is. As Peter said, I doubt that Steinhardt is that mistaken about the idea he co-invented.

If the model that fit the "local" data turns out to also produce every other conceivable data set, how could you ever test whether this is the correct model ? How do you know whether what you observe is a common or very unusual case within the infinite multiverse ? This is the measure problem that Guth et al. recognize and is unresolved to this day. To repeat, what should someone living in a non-flat, non-isotropic, non-scale invariant part of the multiverse conclude about the status of inflationary cosmology ?

Sure you've got a rich mathematical framework capable of modelling anything and everything, but what have you learned about our physical reality. In my view, precisely nothing.

You seem to think that this is a common situation in science but as far I know, this unfortunate problem is entirely peculiar to both string theory and inflationary cosmology.

25. **Anonyrat**
May 12, 2017

IS&L write at one of the links in this blog post (and thus answer Urs Schreiber):

What about the comparison to the Standard Model of Particle Physics? This comparision is a false equivalency. For the Standard Model, there are definite predictions for any choice of parameters. For Inflation, there is an infinite diversity of outcomes for any choice of parameters (i.e., for any choice of the inflationary energy curve). For example, for any one choice of parameters, an infinite number of patches of space in the multiverse are produced that are not flat, not smooth, and do not have the properties astronomers observe – and there is nothing in the inflationary theory to say that one outcome is more likely than the others. The same does not apply to the Standard Model of Particle Physics.

26. **Anonyrat**
May 12, 2017

Urs Schreiber wrote:

> In view of the remarkble likelyhood plot that PLANCK15 produced (fig 12 in PLANKC 15 XX "Constraints on Inflation"), this is an unexpected statement. Right there in the middle of the plot sits Starobinsky's R^2 inflation, the first model for inflation ever proposed. The data does not significantly discriminate between Starobinsky inflation and other plateau-type inflationary models but it does prefer this class.

Around minute 37 of his talk here http://vms.fnal.gov/asset/detail?recid=1944338 Steinhardt addresses this point. In brief, one must rule out all models with eternal inflation.

27. **Jonatan**
May 12, 2017

Indeed, the point I'm refering to is the one Steinhardt explains in the video Anonyrat mentionned, starting at 28:45 (the part around 35:30 is particularly relevant). He also talks about the measure problem starting around 37:50.

28. **srp**

May 12, 2017

I would like Urs to either respond to Anonyrat's quotation or admit that he was wrong in his insistence that inflation models are predictive in the same sense as the SM. It directly contradicts his claim that once you put in the parameters for an inflation model you get an unambiguous prediction of everything else.

29. **qwer1304**
May 13, 2017

It'd seem that the apparent necessary entailment of multiverse (multimess) is the strongest objection to inflation (as Steinhardt says himself). In that light, it'd be interesting to hear both parties' thoughts about V.Mukhanov's (who's actually the first who worked out quantum fluctuations source of matter formation in 1981 http://www.jetpletters.ac.ru/ps/1510/article_23079.pdf) model without self-replication https://arxiv.org/pdf/1409.2335.

30. **Anonyrat**
May 13, 2017

I've been trying to come up with the most sympathetic view that I can of postmodern science. The best I can do is as follows:

The framework of classical Hamiltonian mechanics is of interest because specific Hamiltonians are highly relevant to systems we observe or build. The framework of the Schrodinger equation is relevant because specific Hamiltonians are highly relevant to the real world. The framework of Quantum Field Theory is likewise useful, because specific field theories have proven to be highly predictive.

In postmodern science, it seems that the paradigm is that the framework will give rise to a myriad of scenarios, of which the scenarios relevant to our world are few and even relatively unlikely to arise. One might tolerate this if these postmodern frameworks could say – conditional on your pocket universe having these handful of properties, here is a plethora of other things the framework predicts for your pocket universe with no wiggle room.

The problem with the postmodern framework of inflation is that there is no plethora of predictions – as I see it, there are only a handful – and the wiggle room remains immense. The problem with the postmodern framework of string theory is that even with everything we know about physics – let alone a handful of facts – as given properties, there are no testable predictions; our sum total of knowledge is insufficient to unambiguously determine the string vacuum – there are no predictions, there remains a lot of wiggle room. However, these two failures do not mean that a tolerable postmodern theory cannot exist.

31. **Anonyrat**
May 13, 2017

To complete the above thought with a metaphor: a tolerable postmodern theory of physics must produce a "discrete spectrum" of universes, not a "continuous or near-continuous spectrum", otherwise there is always wiggle room, and one would have to stipulate an infinity of facts about our universe to fix our position in the "spectrum".

32. **Peter Woit**
May 13, 2017

Anonyrat,
I don't think discrete vs. continuous is relevant. What is relevant is the explanatory power of your theory: a good theory makes lots of non-trivial testable predictions based on a small set of assumptions or choices. A bad theory is one where for each testable prediction you need a new choice or assumption. The argument here is that inflation has moved to the bad theory side: Linde is quite explicit at times that what he is doing is developing versions of inflation that can match any new observation of B-modes, of non-gaussianity, etc. This was the question Guth was being challenged on and I don't think had an answer for: is "inflation" now completely insulated from falsification, and if so, is it still science?

I don't think using the term "postmodern physics" is a great idea unless it gets a well-defined meaning, otherwise it will just get thrown around as a sort of meaningless insult (or maybe, for some people, a badge of honor…)

33. **Jonatan**
May 13, 2017

It seems to me that one common feature of both string theory and inflationary cosomology is that in both cases, a new "piece of furniture" in physical ontology was invented (the string and the inflaton field) not because they were required to make sense of new observations that couldn't be explained by the existing physical objects making up the ontology of current physical theories, but rather to improve on existing theories by way of unification, the resolution of fine-tuning issues, etc…

I wonder if this kind of liberty (one might say self-indulgence) on the part of theorists is not the common "cardinal sin" responsible for the overabundance of degrees of freedom leading to multiverses and rendering both of these ideas ultimately meaningless scientificallly speaking.

34. **Peter Woit**
May 13, 2017

Jonatan,

An important aspect of the inflation story is that it's roots are in the study of the Higgs field, and more general uses of scalar fields to make viable GUT theories. What got people excited about the idea was not just the supposed explanations of otherwise hard to understand general aspects of cosmology, but the fact that these were coming from postulating not some random new thing, but something already being used for other promising purposes. I think Guth's original dream was that the same scalar field would break GUT symmetry and provide inflation, giving two completely different sorts of evidence for it, which would be very compelling.

The "cardinal sin" of introducing new degrees of freedom unrelated to anything else to explain something occurred later, as GUTs failed to work out (there are still proposals to revive connection to known physics, identifying the inflaton with the Higgs). ILS essentially claim that this has gotten worse in recent years, with the simplest models not working out, requiring what they call "postmodern inflation". One troubling part of research into inflationary models now involves introducing large numbers of new degrees of freedom, killing the explanatory power of simple models, and yes, that's properly something "sinful".

The story of string theory is unfortunately similar, as the models of the mid 1980s were understood to be problematic, and evolved into the absurdly more complex string landscape ones.

35. **Jonatan**
May 13, 2017

Thanks Peter for reminding me about the interesting history of the field. You're right that the initial connection between the use of scalar fields in GUT and the development of inflation shows that the idea was not without broader motivations at the beggining.

What always seemed suspect to me though was the ease with which fundamental physics since the early 1980's turned to claims about the existence of new ontological entities (strings, inflaton) without any direct empirical reasons for doing so.

I mean, the intelectual path do seem different to me than the way inferences for the existence of the rest of physical ontology (particles of the standard model, four dimensional spacetime, dark energy and dark matter) were made. That is, these inferences appeared much more directly related to our empirical experience of the world. In fact required in order to make sense of this experience.

36. **Yatima**
May 13, 2017

This will run the whole weekend for sure. It's on RT right now:

https://www.rt.com/news/388245-big-bang-bounce-hawking-letter/

37. **Peter Woit**
May 13, 2017

Yatima,
Thanks, good to get a take on where Putin stands on this.

I wonder if the people who signed on to this letter thought much about what they were doing, whether backing Linde in a high-profile media fight was really a good way to defend the credibility of this field of physics.

38. **Jonatan**
May 13, 2017

Briefly restated, my impression is that theories are probably much more likely to end up in a "multimess" when they are based on an ontology invoked for theoretical convenience/ambitions rather than by empirical necessity.

39. **Art**
May 13, 2017

The original ISL critique (1304.2785) identified 3 issues, and spent the least number of words on the third, the eternal inflation implied by the data-favored plateau potentials. The specific criticism here was that the agreement of the Planck data with the nominal expectations of the models was "surprising" given infinitely many opportunities for large deviations. GKN (1312.7619) responded that there's no reason to think that large deviations are probable.

By contrast, subsequent discussion is heavily weighted towards the multiverse/multimess/multi-whatever. In my view, it's deviated substantially from the original topic.

40. **Bill Kuncik**
May 13, 2017

As a layman very interested in science, I think this debate about inflation has done a good job airing and evaluating the subject issues. But I have to tell you, the manner in which it is being conducted is, least in my opinion, creating a bad public impression of the physics community.

By my observation, from BICEP, to the stage of the World Science Festival in New York, to this latest exchange in Scientific American, and much in between, the expositions, pronouncements and exchanges have simply gotten too acrimonious, too personal, and too characterized by incendiary phrasings, media grandstanding, insufficient respect for the other's position, and perhaps even for the other. By both sides, and to an extent one does not ordinarily see in such matters.

Not by everyone, to be sure. But by enough to potentially discredit the enterprise of physics in the eyes of the public, which ultimately decides whether and how much to fund that enterprise.

As was the case with BICEP, this latest episode seems to be getting legs in the general media. So while everyone understands that you all are human too, and that science is political like anything else, please press the reset button and try harder to stick to the issues. For the sake of

41. **a 1**
May 14, 2017

"we question whether the consequence of an incomplete theory
can be legitimately called a 'prediction'

That's from a really nice paper about infinitistic arguments in today's physics:
1604.06773 "The infinite turn and speculative explanations in cosmology" by Reza Tavakol and Fabio Gironi

42. **Jonas**
May 14, 2017

As mentioned above Penrose has argued that inflation would just make the fine tuning problems for the initial stage of the big bang even worse.
He gives a semi popular description of these ideas in this lecture
https://mediacentral.princeton.edu/media/Fashion%2C+Faith+and+Fantasy+in+the+New+Physics+of+the+universe+-+Lecture+3A+FANTASY/1_xbikovdr

43. **Dave G.**
May 15, 2017

I just noticed that Ijjas et al. have put up a new page on their website that they call "FACT CHECKING."
It took me a while to wade through everything. But the page provides multiple examples of papers that contradict the claim that inflation has made predictions in the past that turned out to be successful.
For each prediction, there's a list of papers that claimed that inflation could also produce the opposite result. And it looks like those papers were all written before the observations were actually made.
To make matters worse — the authors of the papers include Guth, Linde, Kaiser and Hawking.
It doesn't seem fair to claim that inflation predicted something if you also showed that the opposite was possible – you can't just forget those earlier papers.

44. **skydivephil**
May 16, 2017

Peter , Im concerned about the use of the video where Guth says inflation is not falsifiable. I have only been able to find a very short version of the discussion and no

context. do you have the whole discussion available? if so would very much like to see it.
Im primarily asking because Loeb et l's critique is more focused on eternal inflation than inflation per se and Guth and Nomura have published a paper showing how eternal inflation could be falsified. This paper is discussed by Nomura in our video about that 49 56 seconds . https://www.youtube.com/watch?v=QqjsZEZMR7I
the link to the paper is here:
https://arxiv.org/abs/1203.6876
Its true that there are version of inflation that are not eternal but according to Guth they are contrived and according to Efstahtiou they are not favoured by the data.

45. **Peter Woit**
May 16, 2017

skydivephil,
The video where Guth says that is embedded in the Undark piece and you can there see it in context. I think it's just the usual "you can't falsify a framework, only models" argument that Urs Schreiber was making above.

For the Guth/Nomura article, there's a new blog posting…

46. **skydivephil**
May 16, 2017

Thank you Peter, but the video in the Unmark piece is only 40 seconds long. What I was looking for is the whole discussion. If you have a link to that, would be very much appreciated.

47. **Peter Woit**
May 16, 2017

skydivephil,
There's a link at undark, it's to this
https://www.youtube.com/watch?v=V6rUi65qLqc&t=121m2s

48. **skydivephil**
May 16, 2017

Thank you for that Peter look forward to watching it.

49. **Alexander**
May 17, 2017

There is an approach that excludes from cosmology the problems associated with inflation, dark matter and dark energy

This Month's Hype

May 16, 2017
Categories: Multiverse Mania, This Week's Hype

It seems that a couple of the authors of the recent Cosmic Controversy letter (discussed here) are going on a campaign to embarrass the 29 physicists who were convinced to sign their letter. Andrei Linde has gone to Lubos Motl's blog to thank him for his blog entry which lauded Linde as having eaten from the biblical tree of knowledge and which denounced his critics as imbeciles. To deal with Linde and his claims, Ijjas, Steinhardt and Loeb have added a new webpage to their website called Fact Checking. It lists the four "predictions" of inflation claimed to agree with experiment by Linde et al. and gives four references to papers published by Linde touting different "predictions" for the same quantities, predictions not agreeing with experiment.

This month's Scientific American has a remarkable cover story, The Quantum Multiverse from one of the other four letter authors, Yasunori Nomura. I've seen some fairly bizarre stories about fundamental physics in Scientific American over the years, but this one sets a new standard for outrageous nonsense, and I'm wondering whether it too may cause some of the 29 co-signers of the letter co-authored by Nomura to question the wisdom of joining with him and Linde. Nomura is well known for a definite prediction based on the multiverse: in 2009 he co-authored a paper claiming that the multiverse predicted the Higgs mass would be 141 GeV +/- 2 GeV. This played a major role in the film Particle Fever. That three years later the Higgs was discovered at 125 GeV seems to have had no effect on his multiverse enthusiasm.

The new SciAm cover story is not about anything new, but is based on a 6 year old paper by Nomura discussed here. At the time I wrote about this "I'm having trouble making sense of any of these papers" and quoted Lubos's evaluation: "They're on crack". Nothing I've seen about this over the past six years seems to me to make any sense at all, including the new SciAm cover story, which just seems even more content-free and meaningless than previous efforts to explain this "multiverse interpretation of quantum mechanics". On the obvious question: how would you test this, Nomura just has this to say:

> Evidence so far indicates that the cosmos is flat, but experiments studying how distant light bends as it travels through the cosmos are likely to improve measures of the curvature of our universe by about two orders of magnitude in the next few decades. If these experiments find any amount of negative curvature, they will support the multiverse concept because, although such curvature is technically possible in a single universe, it is implausible there. Specifically, a discovery supports the quantum multiverse picture described here because it can naturally lead to curvature large enough to be detected, whereas the traditional inflationary picture of the multiverse tends to produce negative curvature many orders of magnitude smaller than we can hope to measure.

This paragraph manages to put together three different misleading and unsupported claims:

- "If these experiments find any amount of negative curvature, they will support the multiverse concept because, although such curvature is technically possible in a single universe, it is implausible there." This is just nonsense.
- "the traditional inflationary picture of the multiverse tends to produce negative curvature many orders of magnitude smaller than we can hope to measure". What is the inflationary multiverse "prediction" for negative curvature? As far as I can tell it's compatible with pretty much any level we might observe.
- "the quantum multiverse picture described here because it can naturally lead to curvature large enough to be detected." I can't find anywhere a calculation of the negative curvature expected by the "quantum multiverse picture", and I don't believe any such calculation is possible.

Given some of the outrageous hype I've seen in recent years in respectable publications, it's gotten rather hard to shock me with this sort of thing, but I do find this Scientific American cover story shocking.

Update: For some reason this was not mentioned in the SciAm article, but the paper justifying Nomura's claims about negative curvature is here.

Update: A modest proposal: Given the situation, I think someone needs to write a letter to SciAm complaining about the Nomura article and get leaders of the community to sign in support of it. They could start gathering signatures by writing to the 29 signers of the earlier letter. If these people were willing to object to the Steinhardt et al. article, they should be willing to object to the far worse Nomura article.

Comments

1. **neil**
   May 16, 2017

   "Can Quantum Mechanics Save the Cosmic Multiverse?"

   Another example of Betteridge's Law of headlines?

2. **mitch**
   May 16, 2017

   SciAm doing this kind of thing doesn't surprise me at all. SciAm today is a far cry from what it was in the 70s when I was doing my Physics degree. These days if I want to read a sensible magazine about science I pick up a copy of American Scientist from our local bookstore.

3. **Peter Woit**
   May 16, 2017

   mitch,

   Yes, SciAm has changed since the 1970s, but so has the physics community, with pseudo-scientific claims like this about the multiverse unthinkable among serious physicists back then. This does seem to me to be a new low for SciAm, not clear whether this has to do with their own efforts, or whether they're just tracking theoretical physicists downwards.

4. **Pasa Dina**
   May 16, 2017

   The IS&L Fact Checking page has a link to an important lecture from Richard Feynman [https://m.youtube.com/watch?v=0KmimDq4cSU at 5:00 minute mark].

   When I listen to Feynman's discussion of "vague theories" and then read about the multiverse, it makes me wonder what Feynman would say about the inflationary theory if he were alive today.

   It's pretty clear now that inflation was always flexible about every prediction. The IS&L page points out that Linde even used inflation to predict the universe is curved – of course, that was before astronomers showed that it is not.

5. **Peter Woit**
May 16, 2017

Pasa Dina,

Whatever the argument over “inflation” I think it’s clear what Feynman would have to say about the multiverse. It’s a ridiculous situation that objections to multiverse pseudoscience are not coming from leading figures in the theoretical physics community. It’s their job, and they’re not doing it. Instead they’re supporting this pseudoscience by signing on to a letter supporting Linde and Nomura and arguing against a perfectly sensible Scientific American article by Steinhardt and co-authors.

Given the situation, I think someone needs to write a letter to SciAm complaining about the Nomura article and get leaders of the community to sign in support of it. They could start gathering signatures by writing to the 29 signers of the earlier letter.

6. **cosmology grad student**
May 16, 2017

You may find it “nonsense”, but the argument that curvature is implausible in a single universe based on the canonical measure over the space of cosmological histories is pretty conventional at this point, see e.g. https://arxiv.org/abs/1406.3057. I don’t necessarily want to defend the rest of what Nomura is saying, though…

7. **Koenraad Van Spaendonck**
May 17, 2017

Peter Woit : ” It’s a ridiculous situation that objections to multiverse pseudoscience are not coming from leading figures in the theoretical physics community. It’s their job, and they’re not doing it. ”

Well spoken. It shows a deep lack of responsability, not only towards the public, the community who funds them, but also towards young physics students, for whom they should be an example of what a serious scientific method is all about.

Whereas now this future generation of physicists become accustomed to the notion that it is ok to make claims with an air of despotism for decades while not providing real support from observations for those claims. And that endless ‘curve fitting’ is portrayed as a professional modus operandi in this context.

One could indeed speculate about Feynman’s take on this, but what about Einstein himself ? I think he would be seriously depressed by now if he had to watch this spectacle. I believe his words were ‘It’s adding Ptolemaic epicycles all over again’.

Respect for Steinhardt&co.

Best, Koenraad

8. **Peter Woit**
May 17, 2017

cosmology grad student,

That really is complete nonsense. Carroll’s long discussion of the “natural measure on trajectories” is just the usual Hamiltonian formalism of classical mechanics. Applying it to this question is nonsense since
1. You don’t have any reason to believe classical mechanics applies.
2. You don’t know what your phase space is.
3. You don’t know what your Hamiltonian is.

That anyone considers this a “pretty conventional” argument is deeply depressing.

9. **Another Anon**
May 17, 2017

Peter Woit: “A modest proposal: Given the situation, I think someone needs to write a letter to SciAm complaining about the Nomura article and get leaders of the community to sign in support of it.”

As you have said earlier, Peter, some sort of weird lettter-writing petition is no way to settle a scientific argument.

10. **Peter Woit**
May 17, 2017

Another Anon,
Yes, but I don’t think there’s any scientific argument in the Nomura article, it’s a different sort of problem facing the physics community than a bad argument.

11. **Mitchell Porter**
May 17, 2017

Scientific American sure can pick ’em… First they give a platform to Steinhardt et al’s campaign to smear inflation as unscientific; and then they make a cover story out of the worst “idea” in multiverse theory (I don’t just mean Nomura’s paper, it’s also dumb when Bousso and Susskind, and Tegmark and Aguirre, do it).

12. **Mitchell Porter**
May 17, 2017

P.S. Just to be clear, what I mean by “the worst idea”, is the idea that the technical problems of eternal inflation (measure problem, holography) are solved by introducing the many-worlds interpretation of quantum mechanics into the mix. The proof that it’s a bad idea may be seen in the papers that try to make it work.

13. **stone**
May 17, 2017

So far all the comments seem focused on the nonsense bullshit Sci. Ame. article. But, who are backing up the writers and their followers? From where have they got the money? Should funding agencies/academic departments/media be also held culpable, in addition to those big names? Would they prosper without any social/public support?

14. **DavisHalstoy**

May 17, 2017

Great Article in the Guardian today:

https://www.theguardian.com/science/across-the-universe/2017/may/17/multiverse-have-astronomers-found-evidence-of-parallel-universes

"One of the study's authors, Professor Tom Shanks of Durham University, told the RAS, "We can't entirely rule out that the Spot is caused by an unlikely fluctuation explained by the standard [theory of the Big Bang]. But if that isn't the answer, then there are more exotic explanations. Perhaps the most exciting of these is that the Cold Spot was caused by a collision between our universe and another bubble universe. If further, more detailed, analysis … proves this to be the case then the Cold Spot might be taken as the first evidence for the multiverse."

Yes, more detailed analysis is all that is needed.

15. **Anonyrat**
May 17, 2017

Since eternal inflation was introduced in 1983 and Feynman died in 1988, he might have actually said something about it.

16. **Alain**
May 17, 2017

The fact checking is the most pedagogical presentation of the subject of cosmology that I ever saw.

17. **atreat**
May 17, 2017

Peter,

Reading through that Carroll paper and your objections I am reminded of your often repeated wish that physicists would learn more from mathematicians about process and rigor. The assumptions in the paper are not stated clearly – e.g. what we don't know – and thus the impact on conclusions is similarly unclear.

As for Feynman, if he couldn't explain it to undergrads he didn't consider it properly understood it. That is where I think he would take issue with these current theories: he would object that even more that they have no footing in experiment, they do not help us truly *understand* anything about our physical world.

In this context, I think ISL and the rebuttal are really arguing over whether inflation and the multiverse theories have actually helped us understand something real or whether we are just as muddled about the things they supposedly help to explain as before.

Endless conjectures upon assumptions upon conjectures isn't real understanding and the limitless flexibility of these theories just means conjectures and assumptions all the way down.

18. **Sesh Nadathur**
May 18, 2017

DavisHalstoy:

That quote from Tom Shanks is laughable BS. It is complete and utter nonsense, unsupported by any actual rigorous analysis in his or any other published paper, to say that the Cold Spot supports the multiverse.

In fact it is worse than that, since even the claim that the Cold Spot is 'an unlikely fluctuation' in need of any 'explanation' at all is highly dubious. Put in language that HEP people will find familiar, it is only at most a ~2.5 sigma anomaly with respect to the standard cosmological picture even without accounting for a massive 'look elsewhere' effect in the analysis; any sensible consideration of this effect and the significance completely drops away.

A couple of relevant references: https://arxiv.org/abs/0908.3988 and https://arxiv.org/abs/1408.4720 (Figure 6 in particular), among others.

19. **Peter Woit**
May 18, 2017

Thanks Sesh!

I'm wondering whether there are any examples of media stories about the multiverse that are not nonsense, or whether the only accurate possible characterization of them is by degree of nonsense (with "laughable" about the median).

20. **Jeff M**
May 18, 2017

I'm curious, what does it say about the physics community that Nomura, who is a full professor at one of the top physics departments in the world, senior researcher at one of the most prestigious labs in the world, and principal investigator at a top institute, is writing papers which make no sense? No one seems surprised. Math profs at Berkeley don't write papers which make no sense. Math profs at my university don't write papers which make no sense, and I'm not at a research one school.

21. **Yatima**
May 18, 2017

Well, who is supposed to be the consumer of all this sound and fury, signifying nothing?

To the lay public, this would look disturbingly like a bunch of quackery, and not even a particularly good one. Information about astral projection, crystal healing, communication with the dead, ghost manifestations and cold spots in the attic can be had in dedicated magazines and tradeshows without having a tedious boffin spoil the show.

22. **Peter Woit**
May 19, 2017

I noticed that the Russian government news service is running the following story about this "proof of a multiverse" and how it ties in with string theory

https://sputniknews.com/society/201705181053755094-multiverse-astronomy-research-durham-university/

The source of these “utter nonsense” stories that do great damage to the credibility of science has always mystified me.

Maybe it’s all a Russian disinformation plot, designed to destabilize the Western (scientific) establishment, by spreading fake physics, and getting leading scientists to start fighting amongst themselves, writing “open letters” and the like. I can’t think of a more plausible explanation for the SciAm Nomura article…

23. **vmarko**
May 19, 2017

Jeff M.,

“Math profs at my university don’t write papers which make no sense”

I’m not so sure. Probably not at the same level as the multiverse/multimess, but still, there is this thing with Mochizuki’s proof of the abc conjecture that nobody seems to understand, and the computer-assisted proofs of various theorems that half of the math community doesn’t consider as “proper” proofs, etc. So math is also not completely insulated from social controversies.

Peter,

“Maybe it’s all a Russian disinformation plot, designed to destabilize the Western (scientific) establishment”

If only that were true, we would have much less of a problem. But alas, I think this “disinformation plot” actually comes straight from the very Western scientific establishment itself (or one part of it), probably trying to gain celebrity status in popular media, and maybe fool others into thinking that they deserve a Nobel or something… I fear that it’s a much deeper issue than mere political propaganda.

Best, 🙂
Marko

24. **Peter Woit**
May 19, 2017

vmarko,

I fear you are right about the origin of the “disinformation plot”…

But I don’t think there is any analog in the conventional math literature of things like Nomura and his “quantum multiverse”. It’s basically a sweeping claim about what fundamental physics is, backed by no sensible non-trivial idea. To get an analog in math, it would be something like papers claiming to solve the Riemann hypothesis by some incoherent invocation of Godel. Such things do exist, but they are ignored, don’t end up in respectable journals or Scientific American. And Mochizuki/abc really is something very different.

25. **vmarko**
May 19, 2017

Well, ok, I agree that the abc conjecture thing is probably not a good example. But the physics discussions “what constitutes proper science” and math discussions “what constitutes proper proof” (in the context of computer-assisted debates) seem to have quite a lot in common. Of course, there are also big differences as well, but still…

Best, 🙂
Marko

26. **Steven Rodgers**
May 19, 2017

Off topic, but can you comment on Bill Unruh’s latest paper (https://journals.aps.org/prd/abstract/10.1103/PhysRevD.95.103504) on Dark Energy – it’s been like a week and literally none of the blogs have picked it up given how massive this thing might end up being. I’ve gone through it, and while I don’t have domain expertise to determine whether all the assumptions and approximations are strictly valid, it seems pretty easy to follow and the result is striking. No more cosmological fine tuning problem and no more dark energy, and all done with relatively simple concepts that have been lying around for decades! Best, Steve.

27. **Peter Woit**
May 19, 2017

Steven Rodgers,

Sorry, this is off-topic and you need someone more expert than me in these issues about the CC. There’s a long history of claims of ideas of how to get rid of the CC (my roommate Nathan Myrhvold was working on one more than 30 years ago), none of which ever seem to have gotten significant acceptance. You could try Sabine Hossenfelder at Backreaction…

28. **Jeff M**
May 19, 2017

vmarko

There is an issue in math over computer assisted proofs, though it’s still pretty small scale. I was actually involved with one, there’s a well known proof, due to Viswanath, that random Fibonacci sequences grow exponentially which was computer assisted, published, accepted. The basic idea was Viswanath controlled round off error. A colleague and I came up with a very elementary proof of the same fact (you could explain it to a high school student), though we only got rough bounds on the growth rate, the computer proof gave a very good estimate. We, however, could generalize to more elaborate random sequences where you not only randomize plus/minus, but you allow for multiples of previous terms. In that case we got a sharp bound for when growth can occur (which again confirmed a computer estimate for the number, by Embree and Trefalyn).

I think it’s different than Nomura though, there are serious, long term discussions of what is valid, and why or why not.

29. **anon**
May 19, 2017

Does there really still exist significant controversy about computer-assisted proofs? In my experience some mathematicians could say that a proof requiring extensive computer calculations is perhaps not very beautiful or satisfying, but I don't think that they'd call it invalid.

30. **Peter Woit**
May 19, 2017

anon,
This is getting far off-topic, but no, I don't think that computer-assisted proofs are "controversial".

The controversy here over inflation is I think very different. The question at issue is whether scientists pursuing these models have made them so flexible that they now don't solve a problem they are advertised as solving (the fine-tuning problem) and can be fit to any conceivable new data, so "inflation" has become an idea immune to falsification.

31. **Another Anon**
May 20, 2017

My reading of the letter to Scientific American by the 33 was that the target audience was purely the editors – not for public attention, with a general feeling of trying to pressurise the editors: "Don't try anything like that again if you know what's good for you". I'll say no more, but I didn't like that side of it.

32. **Mitchell Porter**
May 20, 2017

I've just had another look at Nomura's 2011 paper and I want to moderate my earlier remarks somewhat. I do think that these attempts to apply the many-worlds interpretation of quantum mechanics to cosmology are seriously flawed. But there's another side to Nomura 2011, and to Bousso & Susskind 2011, which is the concept that de Sitter universes are metastable and eventually decay into a flat supersymmetric universe. It should be possible to get rid of the Everett-like many-worlds part of the argument, and replace it with a more conventional understanding of quantum mechanics. What would be left is just a physical hypothesis that could then be discussed sensibly.

33. **Peter Woit**
May 21, 2017

Mitchell Porter,
The problem is that most proponents of the "many-worlds interpretation of quantum mechanics" claim that they just mean conventional QM (state space + Schrodinger equation) applies to the whole system including the measurement apparatus. In the case of early universe cosmology, this is pretty uncontroversial, the default assumption is that it is governed by conventional QM.

With many-worlds, if you try in the non-cosmological case to start claiming this implies branching into multiple universes, you immediately run into the problem that you don't have a useful description of the branching process or how to characterize possibilities and how to count them. This is on top of the usual problems that you don't even know what space you are trying to put a measure on.

Sure, Nomura can claim that many-worlds applies in the cosmological case, but as far as I can tell it solves no problems, just introduces more issues you don't understand. If he were able to derive anything about the real world from this, that would make it worth paying attention and seeing what he is doing. Instead this seems to be a completely ill-defined proposal that leads nowhere. Again, why is such a thing on the cover of Scientific American???

34. **Jeff M**
May 21, 2017

Peter,

When you talk about early universe cosmology, do you just mean that in the early universe essentially the whole universe has a wave function? In essence, there is no measurement apparatus? If that's it, then how do you account for the first wave function collapse? I mean why wouldn't the universe just evolve as a wave function satisfying the Schrodinger equation?

35. **Peter Shor**
May 21, 2017

Can somebody (or somebody's friends) comment on whether *Scientific American* papers in biology, chemistry, or some other non-physics field have undergone a substantial drop in quality over the last few decades. Then we'd know whether this phenomenon stems from the high-energy physics community, or from the editors of *Scientific American*.

36. **Peter Woit**
May 21, 2017

Jeff M,
The "many worlds interpretation" is basically just the claim that there is no wavefunction collapse, that you can make sense of quantum mechanics without invoking that as something separate. As far as I can tell, saying "I'm going to use the many worlds interpretation to do cosmology" is an empty statement, since that's what people already were doing, since there is no separate experimental apparatus. It's not surprising then that his supposed "prediction" is the same one already in Susskind's book about the string landscape over a decade ago.

37. **Anonyrat**
May 21, 2017

Here is a 2007 article decrying SciAm publishing an article by Peter Duesberg (full context in the article):
http://sandwalk.blogspot.com/2007/04/demise-of-scientific-american.html

38. **Anonyrat**
May 21, 2017

Here is a 2010 article concerned about a decline in the quality of SciAm coverage of climate change:
http://www.sourcewatch.org/index.php/Scientific_American

39. **Jeff M**
May 21, 2017

Peter,

So now I'm confused. From what I remember, many worlds says that when the wavefunction would collapse, it doesn't collapse, it splits, possibly into many separate wave functions. All of which go on happily evolving according to Schrodinger until they split again. So no collapse, but something takes it's place, and has an effect on the universe (which is just bigger than the normal one, since it contains many copies every time there's a split). So it eliminates collapse, that's true, but in cosmology, where people don't think about collapse I guess since there's no observer, there is a single strand in the universe, and under many worlds there are many many strands, all separate and unable to communicate with each other. And in each strand it LOOKS like the wave function collapses, right? That's the whole point, you can't possibly tell, many worlds is pure interpretation, and can't ever be shown to be false.

40. **Peter Woit**
May 21, 2017

Jeff M,
I really don't want to try and get into the details of quantum mechanical interpretation issues, and exactly what different people mean when they say "many worlds", there are lots of places you can read about that, it's a complicated story. Maybe some day I'll write a post about that, especially if I run across something that explains the issues well. As I wrote earlier, I don't see how taking those complexities into account helps at all with the conventional cosmological multiverse problems, and that's what Nomura seems to be claiming.

41. **Anon**
May 22, 2017

The reason SciAm publishes these articles is because they are losing money. Controversy sells. If academics want impartial publications they should tell their universities board of directors to form an association with other universities and buy SciAm as well as other important magazines, (they will never be cheaper then they are now). Universities are rolling in cash (look at football coaches salaries) they can afford to support these magazines. If you have an association of ten or more schools you can avoid the problems of arxiv.

42. **Peter Woit**
May 22, 2017

Anon,
These days universities (at least in the US) are looking for ways to make more money, not money-losing operations to take over. Foundations like the Simons Foundation are the organizations not looking for profit-making opportunities. They already have Quanta magazine, and maybe the way the world is going, they'll start playing the role that SciAm used to play.

43. **AcademicLurker**
May 22, 2017

I was just about to mention Quanta. Between that and their support for the arxiv, it's impressive how much the Simons Foundation has done for physics and science in general.

Nautilus is another magazine that's been successfully operating in the same space as Quanta, although I've heard that they are feeling some financial pressure lately. It's a bit depressing to think that having a billionaire patron is the only way to run a non-hype driven popular science magazine.

44. **Low Math, Meekly Interacting**
May 22, 2017

Peter is very generous to let the readership off the hook, but I think we bear some responsibility. The wisest thing to do with clickbait is ignore it, hence putting a disincentive on producing it. But does the typical reader resist that urge? Of course not. I'm just as guilty as anyone. Even when we know it's complete B.S. we find it hard to resist.

I resolve to try to do better.

45. **Roofus McLoofus**
May 22, 2017

This story has reached a mainstream outlet:
http://www.cnn.com/2017/05/14/opinions/what-explains-the-universes-cold-spot-lincoln/index.html

I'm not the least bit interested that it's a "literally cool idea": I seriously considered about the level of intellectual hygiene reaching the public. What are us outsiders missing? Should we keep our mouths shut and let this run its course?

46. **Tim**
May 22, 2017

Verlinde has now made it to ArsTechnica:

https://arstechnica.com/features/2017/05/emergent-gravity-and-dark-matter-explained-by-excited-universe/

It even mentions the false idea that glass flows, albeit slowly. The quality of popular science reporting is going down in general, I think.

47. **anon2**
May 23, 2017

Re analogous controversies in math:

In math, there is usually no real controversy about truth. That is, computer-assisted proofs are accepted as correct (maybe not enlightening), and the general notion of correctness in mathematics has not really changed since the 20s.

Instead, we mathematicians fight over "elegance", "relevance", whether "this is how you should view this". And yes, in my opinion there are entire subfields in mathematics that have lost touch with reality and produce mostly bullshit. For example the community that does "mathematical neuro-stuff", i.e. coupled oscillator networks, chimera states, etc appears to be pretty bullshit (to me). This does not mean that the claimed theorems are wrong, but it means that the "claimed motivation" is wrong: The theorem might be correct, but it is irrelevant and only pursued in order to follow the latest funding fads; it makes no progress at all for the actual questions that are claimed as motivations.

There is no shortage of true theorems in mathematics; the difficulty lies in deciding what questions to ask, and (progressing towards) answering these relevant questions.

Also, see thurston “on proof and progress in mathematics”. It is quite clear that many computer-assisted proofs provide no real insight. People can rightfully argue that generating insight is a large part of what it means to be a mathematician; hence, it makes sense to be dissatisfied with some computer-assisted proofs. On the other hand, some computer-assisted proofs contain a lot of insight!

GAMBIT

May 23, 2017
Categories: Uncategorized

The LHCP 2017 conference was held this past week in Shanghai, and among the results announced there were new negative results about SUSY from ATLAS with both ATLAS and CMS now reporting for instance limits on gluino masses of around 2 TeV. The LHC has now ruled out the existence of SUSY particles in the bulk of the mass range that will be accessible to it (recall for instance that pre-LHC, gluino mass limits were about 300 GeV or so).

Over the years there has been an ongoing effort to produce "predictions" of SUSY particle masses, based on various sorts of assumptions and various experimental data that might be sensitive to the existence of SUSY particles. One of the main efforts of this kind has been the MasterCode collaboration. Back in 2008 before the LHC started up, they were finding that the "best fit" for SUSY models implied a gluino at something like 600-750 GeV. As data has come in from the LHC (and from other experiments, such as dark matter searches), they have periodically released new "best fits", with the gluino mass moving up to stay above the increasing LHC limits.

I've been wondering how efforts like this would evolve as stronger and stronger negative results came in. The news this evening is that they seem to be evolving into something I can't comprehend. I haven't kept track of the latest MasterCode claims, but back when I was following them I had some idea what they were up to. Tonight a large collaboration called GAMBIT released a series of papers on the arXiv, which appear to be in the same tradition of the old MasterCode fits, but with a new level of complexity. The overall paper is 67 pages long and has 30 authors, and there are eight other papers of length totaling over 300 pages. The collaboration has a website with lots of other material available on it. I've tried poking around there, and for instance reading a Physics World article about GAMBIT, but I have to confess I remain baffled.

So, the SUSY phenomenology story seems to have evolved into something very large that I can't quite grasp anymore, perhaps a kind reader expert in this area can explain what is going on.

Comments

1. **Mitchell Porter**
   May 24, 2017

   It just seems to be a software library that allows you to do parameter fitting and likelihood estimation, for a variety of BSM field theories, in a standardized way.

2. **Peter Woit**
   May 24, 2017

   Mitchell Porter,
   I can see that much. What I don't understand is what is new about this, how this is different than previous efforts like MasterCode. What will this do that MasterCode didn't and why do the assumptions built into seem to be of a higher level of complexity than MasterCode?
   I guess part of what I don't understand is that I would have expected that, as stronger and stronger LHC bounds rule out more and more of these kinds of models, I'd expect people to lose interest in this kind of thing, whereas instead we seem to be seeing a larger and larger group of people working on it.

3. **Ryan**
   May 24, 2017

   Yeah and apparently these efforts are well received by the pheno community. Here are some random tweets that popped up in my timeline:

   https://twitter.com/HEPAdelaide/status/867258318770683904
   https://twitter.com/Tristan_duPree/status/867259714496757760
   https://twitter.com/suchi_kulkarni/status/867273017474375680
   https://twitter.com/SaschaCaron/status/867301364904456193

   Maybe the last tweet sums up the mindset behind this kind of work: "Yes, nice to see people moving to more complex models."

   No idea, what's nice about "more complex models". However, from a naive perspective it seems to make sense that the new bounds require more effort on the "model builder" side and thus more complex fitting codes…

4. **Peter Woit**
   May 24, 2017

   Ryan,
   Thanks. That captures part of what is confusing me here. What is the reason for "more complex models"?

5. **vmarko**
   May 24, 2017

   Peter,

   I think you are right to expect people to lose interest in this stuff, but it feels that you are a bit ahead of time. Losing interest will happen eventually. It's just that some people are hard to give up, and SUSY models will take some more time to drop out of fashion, IMO.

   Best, 🙂
   Marko

6. **Peter Woit**
   May 24, 2017

   vmarko,
   Yes, but that's not what I find hard to understand about this project. Looking at their papers I see as output various computed "likelihood" profiles of various quantities, but I find it hard to figure out what assumptions go into these and what their significance is. If the LHC tells you the gluino is above 2 TeV, what is the significance of a likelihood profile for where the mass is supposed to be above 2 TeV? More precisely, take a look at page 8 of this presentation
   https://indico.cern.ch/event/571190/contributions/2377454/attachments/1387436/2112013/Kvellestad_GAMBIT_LHC_recast.pdf
   about GAMBIT, explaining their take on how to perform a "global fit". There seems to be so many assumptions and so much complexity built into this fit that I have no

idea what it's significance would be.

I'd rather not have a discussion of the usual issues of SUSY sociology, am curious about this new development.

7. **Ben**
May 24, 2017

GAMBIT and MasterCode have overlapping capabilities – they're two different collaborations working on similar things. One difference is that GAMBIT is open source and seems to have everything up on GitHub. To some people this is a big deal. It maybe also incorporates a few extra codes that MasterCode doesn't, but that's less clear to me.

I would usually take "more complex models" to mean more complexity in the phenomenological modeling and likelihood computations, not necessarily more complexity in the fundamental theory. Usually when a theorist comes up with something new the first things one does are quick first-order calculations on what parameters are allowed. But this usually doesn't include a deep understanding of experimental systematics, cross-experiment effects, etc. That's where these sorts of codes come in.

Disclaimer: I'm not involved with either project.

8. **Jake**
May 24, 2017

Peter,

do you know the essay "Effective quantum field theories" by Georgi in "The New Physics" edited by P. Davies, where he writes about "how theoretical particle physics works as a sociological and historical phenomenon?"

It's from 1989, but I only recently stumbled upon it. I think it describes nicely the situation nowadays and it helps to understand what people are currently doing in particle physics. I quoted it, and wrote about it here: http://jakobschwichtenberg.com/making_sense/ (at the end of the post)

The most relevant part is probably: "During such periods, without experiment to excite them, theorists tend to relax back into their ground states, each doing, whatever comes most naturally. As a result, since different theorists have different skills, the field tends to fragment into little subfields."

9. **Vognet**
May 24, 2017

Peter, as an analogy GFITTER and other groups were doing similar fits for the standard model long before the Higgs was discovered, and such fits gave best fit points for a standard model Higgs mass (and other best fit points could be obtained for a non-standard higgs) that kept moving up and up as they were excluded. Now imagine a world where the limitations of technology only allowed you to go up 5 GeV at a time after passing 90 GeV, and the construction of such a machine would take a decade each time. Each decade you would get discouraged that the Higgs didn't show up around the corner yet again, and eventually blogs like yours would start saying we had it all wrong, the arguments for the Higgs being light were nonsense, updating these fits is pointless, and we should just accept that experiment is telling us so. But discouragement is a human prior placed with a human timescale and dependent on human technology; in reality the Higgs mass will be what it is independently of this. In our actual world, that it "only" took us almost half a century to discover the Higgs is fortunate but didn't have to be so, and the arguments for why the Higgs had to be there would have remained the same as what they were from the start regardless of the accumulation of null searches.

Now before you say the situation was different with the no lose theorem of the Higgs guaranteeing some discovery, I am not talking about the motivation for building a higher energy collider, I am talking about the motivation for a fit to a model beyond what is currently known to get a statistical inference on what the indirect constraints are on such a model. This is how science proceeds in the "confronting theory to data" phase, and in the hypothetical world where we go up 5 GeV every decade it would still be motivated to update fits for not just the standard model Higgs, but all kinds of non-standard Higgs models, and even higgsless models, because we still wouldn't know what lies at those energies up there, and what lies there doesn't care that naysayers are getting impatient and tired of seeing updated fits with best fit points that keep moving up.

As for GAMBIT it's just a tool for doing such statistical inferences that is routine in all areas of science from biology to astrophysics and particle physics, I'm not sure what your bafflement is at this. It's like if years ago Stephen Wolfram released a new-fangled software called Mathematica with ten example notebooks, one of which was a SUSY calculation, and you were similarly perplexed at why someone bothered writing an algebraic package to do a calculation in general that a SUSY group already did for their SUSY case before, and since you don't care much for SUSY your reaction is to ask what is the point?

10. **Peter Woit**
May 24, 2017

Vognet,
I don't really see the analogy with Gfitter, something I do more or less understand. There essentially you had a model with one undetermined parameter, lots more measurements than parameters, some of which were weakly sensitive to the undetermined parameter, so you'd expect to have some information that could be used to get a sensible "best fit" for the undetermined parameter. Gfitter was giving best fit numbers for the Higgs even without using the direct search exclusions (96 +31 -24 in 2011) and if you dug around a bit you could see which measurements had some sensitivity to the Higgs mass and were giving non-trivial information, and at what point the SM would break as direct search exclusions eliminated that range from the bottom (in 2011 the best fit using direct search exclusions was 120 +12 -5).

What's the analog for a GAMBIT "best fit" for the gluino mass? I understand that they are using direct search exclusions, but all those can do is tell me a lower bound on the gluino mass, not give a non-trivial "best fit" somewhere. What are the experimental measurements (other than direct searches) indirectly sensitive to the gluino mass that are giving non-trivial best fit information? I understand there must be some such information, but what I'm mainly saying here is that I can't get this from their description of what they are doing, since it is so complicated and the number of undetermined parameters is high. In the case of MasterCode, with a quick look at their papers I could see that they were getting information like this from certain specific deviations from the SM, eg. g-2 for the muon and see the relation to a non-trivial best fit. A similar quick look at GAMBIT just leaves me perplexed about what exactly they're doing and wondering whether there's anything going on here besides the direct search exclusion regions and artifacts of their assumptions and various choices.

11. **M**
May 24, 2017

Can somebody highlight one interesting result in these many papers?

12. **Tim**
May 24, 2017

If I understand Peter correctly, his objection is basically that among models in a particular class that are not excluded by experiment, what is the motivation for justifying that certain parameters that are compatible with experimental constraints are more probable than other parameters that are also compatible with experimental constraints?

Absent some a priori assumptions like naturalness, there is no justifiable probability metric for non-excluded theories, and the LHC results seem to be strongly hinting that naturalness, as understood prior to the LHC results, is not particularly useful in predicting future experimental results.

13. **Peter Woit**
May 24, 2017

Tim,
I need to keep emphasizing that my problem here really is the complexity of the choices being made, more than any particular choice itself. If you do a simple analysis with one or two simple choices, a human being may be able to understand how the result depends on the choices, and have an interesting discussion about the choices. In this case, when I try and read these papers, the sheer complexity of the choices being made cause me to get lost very quickly, with no feel for which choices might be important. For example, looking at the analysis done for the MSSM here,
https://arxiv.org/abs/1705.07917
right at the beginning in table 1 the authors list 7 different parameters they intend to scan over, with no particular motivation for the ranges chosen, and the priors chosen, which are listed as "flat", "hybrid" and "split hybrid". That's just the beginning, going on from there things get really complicated, and no human being I think will be capable of figuring out how the complexities being invoked are reflected in the final results.

If you have a really complicated problem, with really complicated data, it's understandable that you might get involved in very complex analyses. The question in my mind is whether going down that path here sheds light on the questions supposedly being addressed. M's question about whether this leads anywhere interesting is to the point.

14. **G. S.**
May 25, 2017

I'm guessing that the real point of this work is to provide a project for a group of high energy physicists that teaches them the skills they will need when they quit high energy physics and look for jobs in data science/machine learning/software engineering.

That seems to be the path a lot of physicists are taking these days.

15. **Mitchell Porter**
May 25, 2017

Stacy McGaugh describes the situation in astrophysics, as one in which elaborate models of galactic dark-matter dynamics come with numerous opportunities for post-hoc fine-tuning, while the many successes of a simpler, more principled theory (MOND) are relatively neglected.

I can certainly see the potential for this software package to sustain an analogous situation in high-energy physics, though perhaps it's not as clear what simpler opportunities are being missed (Koide relation, Higgs criticality?). But I wonder how flexible it is. Could this package be useful for people working on quite different models, like discrete flavor symmetries in neutrino physics?

16. **Peter Woit**
May 26, 2017

All,
MOND is off-topic here. The last thing the world needs is another source of dubious claims from non-experts about that.

17. **photongrapher**
May 26, 2017

Peter you seem to have put little time in trying to understand what they've done here. This is not just about SUSY, GAMBIT can be used for any BSM physics.

I understand the desire for a simple statement of "gluions are excluded below 2TeV", but they are taking a more nuanced approach. While the former contains a whole bunch of assumptions, their approach is trying to allow a simple way to explore what is possible in BSM theories. In the absence of BSM observations, it is necessary to be more 'complex' so that instead of making statements like:
"there is no SUSY <2TeV*"
(*where SUSY = MSSM and the LSP is all the DM.. etc),
They might be able to more concretely say:
"there is no SUSY <1TeV PERIOD"

18. **Peter Woit**
May 26, 2017

photongrapher,

Yes, I've put some but not a lot of time into trying to understand what they've done here, the question in my mind is why it might be worth putting in more time. I do understand this is meant as a general tool, not just for SUSY theories. The gluino was just taken as an illustrative example.

I also understand the use of this tool to scan for regions of SUSY parameter space not yet ruled out be experiment, assume that's always been an active area of phenomenology research with various tools available, maybe this one is better, I don't know.

What I don't understand is, for instance, most of the plots in
https://arxiv.org/abs/1705.07917
which are giving relative likelihoods and "best fits" for SUSY masses not in some corner of parameter space that people are trying to rule out, but at generic values way above the experimentally observable range. What is the point of these? Do they make any sense?

19. **Mitchell Porter**
May 30, 2017

"GAMBIT Is Incomplete But Furthers SUSY Falsification":

http://dispatchesfromturtleisland.blogspot.com/2017/05/gambit-is-incomplete-but-furthers.html

20. **Henry McFly**
May 31, 2017

Peter,

Table 3 on that paper shows the contribution of each term used in the likelihood to the total. The results are also shown separately for each mechanism that gives the correct dark matter relic density. LHC sparticle searches don't contribute since there is nothing observed yet, but Higgs discovery and b-physics do contribute a lot, so does Fermil-LAT gamma ray observations.

A Few Quick Items

May 31, 2017
Categories: Multiverse Mania, Uncategorized

I've had little time for blogging, and coincidentally, there seems to be little to blog about recently. Here though are a few quick items:

- Several people had asked me about this paper about the CC, and I had to tell them that this was not something I could evaluate. Luckily, Sabine Hossenfelder has read it and thought about it carefully, and discusses the problems with this sort of thing here. The physics community owes her a great debt.
- The LHC is back in business, with intensity starting to ramp up. You can follow progress here. This summer should see release of more results based on last year's run, results from this year's run likely will not appear until early next year.
- Inference magazine has a thoughtful piece in the latest issue by Adam Falkowski (AKA Jester) about prospects for the future of HEP physics. The same issue also has a piece by Aurélien Barrau about the implications of the failure to find the "natural" physics some expected SUSY to provide.
- Hironaka has recently put on his website a document intended to give a proof of resolution of singularities in characteristic p. For some background and links to explanations of what this is about, see mathoverflow. Evidently Hironaka has been working on this proof for quite a few years, this is the first complete version to be made public. Sometime in the next few months it should become clear whether this proof will really work, as experts get a chance to go through it carefully. If it does work, it will be a remarkable story, especially since Hironaka is now 86.
- Maybe I'm the last one to find this out, but for quite a few years now MIT has been making public detailed course materials including lecture notes from many courses in mathematics and physics.

Update: For the obligatory Multiverse Mania item, see this interview with Lord Martin Rees. Rees is rather proud of himself for leading the field of theoretical physics to embrace Multiverse Mania, quoting Frank Wilczek as claiming at the end of a conference that:

> five years ago we were a beleaguered minority, whereas now, he and I and others had led many other people into the wilderness.

Besides his belief in the multiverse, he also believes this is what we have in our future:

> I don't think Elon Musk is realistic when he imagines sending people a hundred at a time for normal life because Mars is going to be far less clement than living at the South Pole, and not many people want to do that. I don't think there will be many ordinary people who want to go, but there will be some crazy pioneers who will want to go, even if they have one-way tickets.
>
> The reason that's important is the following: Here on Earth, I suspect that we are going to want to regulate the application of genetic modification and cyborg techniques on grounds of ethics and prudence. This links with another topic I want to come to later about the risks of new technology. If we imagine these people living as pioneers on Mars, they are out of range of any terrestrial regulation. Moreover, they've got a far higher incentive to modify themselves or their descendants to adapt to this very alien and hostile environment.
>
> They will use all the techniques of genetic modification, cyborg techniques, maybe even linking or downloading themselves into machines, which, fifty years from now, will be far more powerful than they are today. The posthuman era is probably not going to start here on Earth; it will be spearheaded by these communities on Mars.

Update: Another black hole merger detection from LIGO announced today, some commentary here and here.

Comments

1. **JohnA**
   May 31, 2017

   The Falkowski article criticizes the future collider program (100 TeV pp) for:
   -) lack of clear physics target
   -) required monetary/time investment

   He instead advocates for more cheaper, faster indirect searches.

   One major (and obvious) thing his article/argument misses is the intermediate Higgs factory step in the path for a ~100 TeV pp collider.
   This is major piece of the program for both of the proposed next generation ~100 TeV colliders (CERN/China).

   Compared to his 100 TeV straw-man, the Higgs factory is a cheaper, faster indirect search with a clear and obtainable physics goal; order of magnitude, or better, improvement on higgs coupling measurements.
   It also has the advantage of being able to be recycled into a 100 TeV machine if it indirectly sees signs of new physics or we find a convincing argument for 100 TeV in the mean time.

   The decision is not 100 TeV or a bunch of indirect measurements. If there is choice to argued over, the option the future collider program is: Higgs-factor + future option on 100 TeV. Falkowski's discussion largely irrelevant as it completely neglects this real choice.

2. **Peter Woit**
   May 31, 2017

   JohnA,
   The problem is that a Higgs factory isn't cheap. The Higgs factory proposals involve the same huge tunnel as for the 100 TeV machine. The cost of the tunnel + ee machine + detectors I would think would be a significant fraction of the cost of the 100 TeV machine. I haven't seen cost estimates for either the FCC-ee or FCC-pp, but these are going to be very large numbers, and that's the problem.

   Actually I think one motivation for the FCC-ee is to divide the FCC-pp cost into two pieces and spread it out over a longer time. It's not at all clear the FCC-ee would be worth it if the plan was just to build it and then stop, rather than go on to the pp machine.

3. **Bee**
   June 1, 2017

   Thanks for the link. (That paper caused me quite some headache.)

Isn't it funny how much the two inference pieces [sound like this blogpost I wrote last year]?

4. **vmarko**
June 1, 2017

Regarding the latest CC-problem-solving paper — it seems to me that the CC problem is a social problem, rather than a physics problem. Namely, every now and then there appears a new paper proposing yet another resolution of the CC problem, so much that by now almost every hep-th physicist has their own favorite solution (myself included). And while all these papers offer worthwhile insights into the CC problem and interesting ways to resolve it, none of them produces a "Wow!" moment from the whole community, and most of the proposals end up being anticlimactic in one way or another.

At best, some part of the hep-th community (slightly larger than just the paper authors) will get excited about any particular proposal, while others will find it unsatisfactory on various grounds (mostly aesthetic), and over time everyone forgets about it until the next proposal comes along.

I don't see this paper being any different, except for a big name like Unruh making it more likely to catch some wider attention.

Everybody is waiting for a paper which will make the whole community say "Oh, look, this is how it should be answered, we were all so dumb, but now it's so obvious!". However, such a paper will never appear — not because existing proposals aren't convincing enough, but because the people in the hep-th community have various (often contradicting) ideologies, and they will never agree on which solution is satisfactory.

So the CC problem is a social problem, unlikely to ever converge to a consensus solution, no matter how ingenious proposals people offer.

Best, 🙂
Marko

5. **Anon**
June 1, 2017

Adam writes in his article:

"The possibility that the LHC will only further confirm the Standard Model is often referred to as the nightmare scenario"

I've never understood this pessimism from the physics community. The fact is, a new particle has been discovered in the Higgs, and the LHC will increasingly probe its properties over the next two decades. If nothing is found, theoretical physicists will just need to accept the completion of the periodic table for elementary particles is possibly in the distant future, as was done for the periodic table of elements recently:

http://www.futurity.org/periodic-table-new-elements-1087782-2/

6. **Low Math, Meekly Interacting**
June 1, 2017

It's hard to install your own cybernetic implants and kiss the Singularity when you're suffering from cosmic-ray-induced dementia. I don't think even crazy pioneers are going to be interested flying all the way to Mars only to live underground the great majority of the rest of their significantly shortened lives.

Multiverse mania seems to be a more general syndrome than its name would suggest.

7. **Andre T**
June 1, 2017

A Higgs factory would likely be e+ e- machine. For a a 125 GeV higgs, the production cross section is around root(s) = 250 GeV or so. (To measure Higgs BRs, gauge boson couplings). Quartic coupling is optimally measured at around 450 GeV, IIRC.
The t-tbar threshold is 350 GeV. Worth spending some luminsity there as well

This is a very viable 30 year program for a upgradeable linear collider whose cost compared to a 100 TeV machine would allow pushing the precision frontier simultaneously…

8. **Peter Woit**
June 1, 2017

Andre T,
The problem with a Higgs factory linear collider is the same problem as that of a circular collider in a 80-100km tunnel: estimates of the total ILC cost come to about $20 billion, a very big number (the yearly power costs would also be high).

The linear collider would have the advantage that you could imagine upgrading it to higher energy, the circular collider would have the advantage that you could reuse the tunnel for a 100TeV pp machine.

There are good arguments to be made for either possibility, and the ideal thing for physics would be to build both. The basic problem though is that of how to pay for such a thing.

9. **Dave MIller**
June 2, 2017

Peter wrote:
>Maybe I'm the last one to find this out, but for quite a few years now MIT has been making public detailed course materials including lecture notes from many courses in mathematics and physics.

Peter, I'll add that my kids worked through, with my help, Anant Agarwal's "Circuits and Electronics" (edX 6.002X) and Christ Terman's "Computation Structures" (6.004X). Both courses are online videos, homework, and exams, not just course notes (Agarwal's is videos of him lecturing, Terman's is more of a PowerPoint-like format with audio of Terman speaking).

Terman is a very good teacher. Agarwarl, who is apparently the moving spirit behind the MIT series of courses (which has now become edX), is a truly brilliant teacher, in my opinion in the same league as Feynman (I took a couple of classes from Feynman as an undergrad).

Any physicist who expects to end up doing electronics (and many will after going out into the real world) should consider going through these courses. And, we should urge any bright high-schoolers we know also to work through some edX courses.

MIT, and especially Anant Agarwal, have really made a great educational breakthrough with these online courses.

Dave Miller in Sacramento

10. **Jakob**
June 5, 2017

The arguments and conclusion by Adam Falkowski immediately reminded me of “Six cautionary tales for scientists” by Freeman Dyson from 1988. Dyson compares 6 situations where people had to decide between a Plan A = several small projects vs. a Plan B = one huge project.

He discusses the SCC (” an extreme example of Plan B”) and concludes: “My Plan A for the future of particle physics is a program giving roughly equal emphasis to the three frontiers. Plan A should be a program of maximum flexibility, encouraging the exploitation of new tools and new discoveries wherever they may occur. To encourage work on the accuracy frontier means continuing to put major effort into new detectors to be used with existing accelerators. To encourage work on the rarity frontier means building some new accelerators which give high intensity of particles with moderate energy. After these needs are taken care of, Plan A will still include big efforts to move ahead on the energy frontier. But the guiding principle should be: more money for experiments and less for construction. Let us find out how to explore the energy frontier cheaply before we get ourselves locked into a huge construction project.”

Multiverse Politics

June 1, 2017
Categories: Multiverse Mania

The political campaign for the multiverse continues today with a piece by Amanda Gefter at Nautilus. It's a full-throated salvo from the Linde-Guth side of the multiverse propaganda war they are now waging, with Linde dismissing Steinhardt's criticism as based on "a total ignorance of what is going on". All of the quotes for the article are on the pro-multiverse side. There is a new argument from them I'd never heard before: Guth comes up with this one:

> You can create a universe from nothing—you can create infinite universes from nothing—as long as they all add up to nothing. Not only is that a deep insight, it also creates a testable prediction. "Eternal inflation certainly predicts that the average density of all conserved quantities should be zero," Guth says. "So if we ever became convinced that the universe has a nonzero density of electric charge or angular momentum, eternal inflation would no longer be an option."

The article is subtitled "Why the majority of physicists are on one side of a recent exchange of letters". One way to interpret this claim is just that 33 is more than 3, but the reason for this is clear: while Guth, Kaiser, Linde and Nomura decided to go on a political campaign, drumming up signatures on their letter, Ijjas, Loeb and Steinhardt didn't do this, but instead put together a website discussing the scientific issues.

Where the majority of physicists stand on the Guth-Linde claims is an interesting question, one that I don't think is addressed anywhere by hard numbers. My anecdotal data is that the majority of those I've ever talked to about this don't think the Guth-Linde multiverse claims are science, but don't see any reason to waste their time arguing with pseudo-science. They hope it will just go away by itself, as it becomes ever clearer that the multiverse is, scientifically, an empty idea.

Unfortunately, I don't see this going away and I think it's now doing very serious damage to physics and its public image. There's a political campaign now being waged, and one side is very determined to win and putting a lot of energy into doing so. Those on the other side need to step up and make themselves heard.

Update: Guth and Linde brought their publicity campaign to New York this past Saturday (video here) where they told a large World Science Festival audience that string theory is beautiful, it predicts the multiverse, inflation has made lots of predictions that have all worked out, and they have (more or less…) the full theory that does all this wonderful stuff. Nothing from them about any thing less than utterly glorious and well-defined about the string theory landscape/eternal inflation product they were pushing. Also on the panel were three philosophers (Jim Holt, David Albert and Barry Loewer) who did an admirable job of trying to push back by pointing out obvious inconsistencies. They at least got Guth and Linde to admit that there was this "measure problem" thing still to be fixed. Physics is in a very weird state indeed now that physicists have adopted untestable metaphysical speculation as their program, with philosophers the ones trying to engage in more normal scientific practice.

There was one multiverse skeptic, George Ellis, who unfortunately didn't engage with this and was diverted onto other topics. One string theorist was there (Veronika Hubeny), who explained about AdS/CFT duality, without anybody bringing up the fact that this has nothing at all to do with what Guth and Linde were promoting in the rest of the discussion.

Comments

1. **Anonymous Professor**
   June 1, 2017

   The cynical view: As certain people saw their chances at a Nobel Prize slowly ebbing away, they decided to start a propaganda campaign to restore their chances.

   I don't know whether this is what everybody else is thinking and nobody is saying, or whether I'm just too cynical and completely off the mark here.

   But this is why giving Nobel Prizes only to experimentally confirmed results is a *great* idea.

2. **Another Anon**
   June 1, 2017

   I must say, I find something rather disappointing about major scientists trying so hard to defend their pet theories. Aren't we supposed to be involved in the search for the truth, whatever that might be? Isn't that why we all got involved in science in the first place, not for personal glorification? If you have a pet theory, and it is disproved, you should be satisfied that it is all part of the process of uncovering the truth of how the universe works. Fighting for your own theory tooth-and-nail seems very low rent.

3. **Don Terndrup**
   June 1, 2017

   Both inflation and multiverse ideas bother me when the defenders seem to treat these as firm scientific conclusions. Wouldn't it be more proper to call them both hypotheses, with various levels of supporting empirical and theoretical evidence?

4. **Peter Woit**
   June 1, 2017

   Anonymous Professor,

   I hadn't thought of that, but it makes a lot of sense. It seemed to me that Guth/Linde and their supporters were more and more engaged in a serious publicity campaign, to make a two-pronged case that inflation is settled science and the multiverse is conventional science, but I couldn't see a particularly good reason for this. They may very well feel that Steinhardt's activities threaten to deny them the Nobel that is rightly theirs, and this would explain the nature of their highly unusual response to the SciAm article (organizing a letter of support from people who might be thought to have influence with the Nobel selection committee).

5. **Adam**
   June 1, 2017

   The majority of physicists don't give a damn about this "controversy". The majority of physicists is not doing theoretical physics, and most of them are doing experiments on subject very far of inflation/cosmology or what not.
   And even among theorists, quite a few of them (me included), are not doing HEP/QG/… Reducing "physicists" to "string theorists" and affiliated is quite tiring, but I guess this is just usual business in pop science…

6. **RP**
   June 1, 2017

I am just a mathematician who is somewhat versed in the philosophy of science, but I do wonder: Does it makes sense to classify a statement X as a 'testable prediction' if the verification of X depends on scientists, at some point in time, becoming 'convinced that the universe has a nonzero density of electric charge or angular momentum'? Clearly we are not talking about things which are 'directly measurable', in any reasonable sense of the expression.

The whole idea behind falsifiability is that scientific theories should be answerable to nature in as direct a way as possible, so certainly without the interference of other theories. This is even the vital fact about scientific theories that ensures that they have real content, and are *about* something: since it is conceivable that they are false, but in fact all our experiments have shown them to be true, they tell us something real about nature.

If instead of making our theories answerable to nature, we make them answerable to just the conviction of scientists, we lose this essential quality of science. It ceases to tell us something real about nature, and only tells us something real about the ideas of scientists instead. This is *exactly* what the notions of 'testability' and 'falsifiability' were designed to avoid.

In other words, the whole notion of 'testable' is being hollowed out if it is allowed to stand in the way Guth is using it. Not just being hollowed out, I would say, but completely subverted. I think it is a terribly sad state of affairs that this type of sophistry is being peddled by a serious scientist writing for a serious public.

7. **atreat**
June 1, 2017

In the linked article, the Linde-Guth side argue that inflation is not really a theory, but rather a "class of models, a sweeping principle, a paradigm" containing a multitude of testable models. As I understand it, Linde-Guth do not disagree with Steinhardt that inflation can support any/all outcomes. Yet, they want to take credit for only those models which have continued to survive nature's verdict. They say the, "key is to figure out which model of inflation is right," but why must one of them be right?

Reading this article and then going back and reading Steinhardt's "fact checking" page where he shows all these failed paper's by Linde-Guth and company with "simple models" that have since been experimentally ruled out just leaves me with wondering what explanatory power can Linde-Guth claim of inflationary theory?

Is modern physics really just about coming up with creative mathematical paradigms describing a whole zoo of potential ways nature could go and then when a new observation is made or a new experiment conducted just shuttering that part of the zoo newly out of sync with nature's evidence? That seems… so sad. Modern physicists reduced to nature's ambulance chasers 🙁

8. **Peter Woit**
June 1, 2017

RP,
I don't think the problem with the Guth claim is the "became convinced" part, which he could easily reword in a different way that would avoid what I think bothers you.

The problem is the nature of the falsifiability claim itself. The universe as a whole is well-known to be electrically neutral, and it's very hard to imagine an experimental result that would show overall non-neutrality. So much so that I'm pretty sure no one is looking for such a thing. I'm not sure what if any experimental signal there would be for a global angular momentum of the universe, maybe there is such a thing, but as far as I know there isn't and no one is looking.

Saying that "my theory is falsifiable because X would be evidence against it" where X is something highly implausible that no one expects to see or is looking for is a standard bogus argument. To see the problem, note that I have a theory that everything that happens is determined by the Jolly Green Giant. This theory is falsifiable, because in principle by observing the CMB, one might see the written message "I'm the Jolly Red Giant, not the Jolly Green Giant".

The other problem with Guth's claim is that, if someone were to observe a global charge or angular momentum of the universe, I suspect that by the next day Linde and others would have a version of an inflationary model which produced universes with a global charge or angular momentum. Personally, if the above mentioned CMB message were found, I would just say that it really is the Jolly Green Giant, it's just that he's a Jolly Green Giant who likes to play tricks.

9. **Anonymous Professor**
June 1, 2017

This year's Nobel will probably go to LIGO. Judging by past behavior, the Nobel won't go to an astronomy/cosmology result two years in a row, so the next reasonable chance inflation has is 2019. If this is a campaign for the Nobel, it will have to be a prolonged one. So by 2019, we will probably have a good idea whether it is or not.

10. **Jeff M**
June 1, 2017

Peter,

Jolly Green Giant? I thought it was turtles all the way down.

Anonymous Prof, while I think you might well be right, I couldn't see the Nobel committee considering inflation. If that's what Linde and Guth are after, it's a false hope. I'm no big fan of the Nobel, but they have been pretty strict about experimental confirmation.

11. **Antimetrics anonymous**
June 1, 2017

Peter
Since the Anonymous Professor has raised the issue lobbying for Nobel, it is worth mentioning the other contribution of Prof Paul S.
Paul Steindhardt wrote an excellent book on " Quasicrystals" . Paul S contributed a lot to the theoretical basis of quasicrystals. In fact in one interview Dan Schectmann to a TV station told the interviewer whether anybody shared the Nobel prize with him, when he recd the phone call for the prize announcement. It was obvious he was referring to Paul S. Here is paper in Dec 1984 in PRL pf Levine and Paul S where they have cited the paper of Dans S and others.
https://journals.aps.org/prl/pdf/10.1103/PhysRevLett.53.2477
Here is the paper of Dan S and others. in November 1984
https://journals.aps.org/prl/pdf/10.1103/PhysRevLett.53.1951

12. **Louis Wilbur**
June 1, 2017

There are many more critics to the inflationary paradigm than Amanda Gefter's article claims. But some of the critics of the inflationary paradigm go too far in claiming that it should be completely abandoned and some of its advocates go too far in claiming it should be thought of as a settled science. The truth is between these two extremes and numerous critics and advocates of the inflationary paradigm fail to realize that non-eternal inflation is much easier to realize than had been previously thought. For instance, the Wikipedia article on eternal inflation points out that "A 2014 paper by Kohli and Haslam called into question the viability of the eternal inflation theory, by analyzing Linde's chaotic inflation theory in which the quantum fluctuations are modeled as Gaussian white noise [arxiv 1408.2249]. They showed that in this popular scenario, eternal inflation in fact cannot be eternal, and the random noise leads to space-time being filled with singularities. This was demonstrated by showing that solutions to the Einstein field equations diverge in a finite time. Their paper therefore concluded that the theory of eternal inflation based on random quantum fluctuations would not be a viable theory, and the resulting existence of a multiverse is still very much an open question that will require much deeper investigation". The conservation laws are certainly not evidence of eternal inflation and de-Sitter space is also unstable in quantum gravity (e.g. arxiv 1608.07237).

All of this shows that non-eternal inflation is vastly superior to eternal inflation. Eternal inflation inherits various pathologies of infinity. In an infinite cosmos there is no way to have a coherent measure (e.g. arxiv 1202.3376 and 1211.1347). This fact follows from basic Cantorian set theory. Because there are many ways to establish bijections between an infinite number of events, probabilities can be anything one wants. In these kinds of infinite situations probability questions are meaningless. For example, what fraction of the positive integers are odd? One might think it is 50 percent but it could be 75 percent if the bijection is {2} pairs with {1,3,5}, {4} pairs with {7,9,11}, and so on.

Non-eternal inflation is predictive, explanatory, has no multi-mess and no measure problems, unlike eternal inflation which has all of these problems. There are many ways to inflate the cosmos without self-reproduction. These include using some type of fluid to do the inflating (e.g. arxiv 1609.04953, 1601.04773 and 1601.05337) and multi-field inflationary models can easily be non-eternal. The weak gravity conjecture prohibits eternal inflation (e.g. arxiv 0707.3471 and 0805.4520) and the string landscape is also inconsistent with eternal inflation (e.g. arxiv 1404.5543, 1609.00385 and 1504.00056). There are also more traditional ways of doing this (e.g. arxiv 1409.2335). Although inflation theory is popular with many early universe cosmologists, it is not a standard in the same way that General Relativity, the standard model of particle physics, and the LCDM model are. The idea of a strong but short lived vacuum energy is plausible and compelling but its details are almost entirely unknown. Cosmic inflation can be thought of as a faster and stronger version of dark energy but the mechanisms of dark energy are almost completely unknown. If dark energy inflation is a sibling of cosmic inflation, perhaps related by some types of seesaw mechanisms, then it is grossly premature to claim that their details are understood in the same way that the details of General Relativity and particle physics are understood. That means, for example, there is no solid basis to claim that inflation leads to a multiverse and it is grossly premature to claim that the mechanisms postulated in traditional inflationary theory are correct explanations of nature.

There is equivalence between the two major sides in this debate because the cosmological community is not mostly nor entirely on one side in the following sense. Cosmic inflation has been quite popular in the sense that very early universe cosmologists find the idea of a short lived but strong non-zero vacuum energy compelling enough to work on. But the idea is very much under development; its practitioners certainly are not claiming that it is a finished product. So the so-called multiverse is certainly not any kind of standard. In her blog on 12/19/15 Sabine Hossenfelder wrote "But the longer the chain of inference, and the less trust you have in the theories used for inference, the less real objects become. In this layered reality the multiverse is currently at the outer fringes. It's as unreal as something can be without being plain fantasy". She also wrote in a different blog that the multiverse is something that researchers within the scientific community spend almost no time on. And the problem of inflation's initial conditions are only partly solved (e.g. arxiv 1601.01918 and 1506.07306).

It should also be pointed out that string gas cosmology is a very plausible and compelling scenario that is completely consistent with all cosmological observations (e.g. arxiv 1505.02381). String gas cosmology requires that the primordial power spectra be almost scale invariant with a slight scalar mode red tilt whereas in inflation theory the potential must be chosen such that these properties emerge (e.g. arxiv 1608.05079). And string gas cosmology can explain why there are only three large spatial dimensions: the annihilation process cannot take place in more than three dimensions (e.g. arxiv 1105.3247). But, like inflation, string gas cosmology still has lots of work to be done. And it is also possible that the correct explanation for the very early cosmos is something entirely different from all of these approaches. An experimentalist will criticize all of these approaches in that the hypothesized additional scalar fields do not have their existence empirically established. The quantas of the inflaton fields have not been observed in any particle accelerators nor have they been observed in any cosmic rays and the extra dimensions of string theory have not been empirically verified. So all of these approaches still have a long way to go.

13. **Bee**
June 2, 2017

The problem that nobody seems to want to talk about is that rather than trying to find a minimal model that explains the data and leave it at this, there are many hundreds of models for inflation all of which are almost certainly wrong because they contain too many details that aren't supported by data. As the philosophers have it, these models are severely underdetermined. (Good paper about this here.)

Theoretical physicists produce these models literally because they can make money with it. They make money with it by getting them published and then using the publications to claim it's relevant research so it'll get funded and they can hire more postdocs to crunch out more papers. It's the same reason why theorists invent dark matter particles and extensions of the standard model. It's a way to make a living.

Steinhardt & co have an issue with this because this overproduction crisis tends to crowd out alternative explanations. I agree that that's a problem, but that doesn't mean of course that Steinhardt's alternative is a better explanation for the data…

What's really missing here is a scientific criterion to draw the line and say, look, at this point in time it entirely pointless to produce further variants of speculations because the data isn't there and won't be there for decades to come. But nobody in the community has an incentive to come up with such a criterion. That's because along the line everyone makes money with this overproduction. It's for this reason I'm putting my hope on philosophers to help us out because I find this situation pretty embarrassing for my discipline.

14. **cosmology grad student**
June 2, 2017

Guth et al are not going to get a Nobel until/unless primordial B-modes are detected. The BICEP fiasco means that it'll be a long time before another group has a claim even if they're seeing hints.

The argument in the article about conserved quantities averaging to zero is dumb. The argument about inflation being able to start for generic initial conditions way overstates the argument of the cited paper. But the discussion of probabilities in a multiverse is pretty much an accurate reproduction of what almost everyone in the community–including Paul until a few years ago–believes. The measure problem is that the obvious choices of measure you might think give results that are far from the predictions of the classical theory. The fact that you need to pick a measure in the first place is just of doing quantum mechanics in a potentially infinite universe, and it doesn't really bother most people in the field.

There might be some of the scrambling for a Nobel that you perceive. But I think a lot of this is genuine frustration with Paul (+ his collaborators, but especially Paul) not grasping this fact about the measure problem, and similarly refusing to accept that it's okay for different inflationary models to make different predictions. (I think Paul has a reasonable point when he says that many simple monomial models, e.g. lamda phi^4 and m^2 phi^2, are disfavored by the lack of observed B-modes and that models like hilltop inflation which give unobservable levels of B-modes seem more contrived. But there's a big difference between that and arguing that inflation is useless because different models predict different things!)

A related note–my sense is that many people, probably even a majority, in the HEP/cosmology theory community are sympathetic to the arguments you and Paul, among others, have made against the string landscape. There's a reason almost no one works on e.g. KKLT or trying to get the Standard Model out of particular compactifications any more. So I think that it's a major tactical mistake, at the very least, to attack string theory and inflationary cosmology with the same criticisms. The community has concluded, I think mostly correctly, that they don't hold up against inflation. I suspect that the result of this is that people are going to be much more inclined to discount what you, Paul, and others have to say about string theory.

15. **Haelfix**

June 2, 2017

One of the problems with this discussion is the equivocation of the word ‘multiverse’. It really does mean many different things.
One of the initial model independent predictions of inflation was that there would be a specific pattern of correlations of the CMB on Superhorizon scales. This is now an experimental fact.. So you have a choice. Either you give up the Friedman-Robertson-Walker solution of GR as a statement about nature, you give up locality, you accept enormous cosmologically large finetunings of the initial conditions, or you do what everyone in the field does… Namely you buy yourself some time, by some sort of exponential expansion right after the big bang. Thus causally disconnected regions would have had enough time to equilibriate in the past. The upshot is that with exponential expansion, comes an exponential amount of space. This really means our horizon is really a small speck in a larger island universe.
The next version is a little more speculative. It says that b/c you have such large volumes, you have to start looking carefully at quantum processes that might be exponentially suppressed, but that would become non negligable when you analyze the system as a whole. So things like vacuum decay would become important (for instance the Higgs is currently measured to be metastable, so in such a large universe, almost assuredly you would have some sort of Higgs vacuum decay somewhere if that turns out to be true). Thus you now have an island universe with some horizons having different fundamental physics.
This then leads to the version of the multiverse that is the most speculative, namely that we are in our particular bubble with xyz finetuned constants, b/c of anthropic selection, due to the aforementioned processes taking place and eliminating most places for life.
As you can see, each version above makes really quite different statements about the laws of nature.

16. **atreat**
June 2, 2017

re “cosmology grad student”:

Linde-Guth want it both ways though; to claim the inflationary paradigm is somehow explanatory, but also a multitude of theories capable of explaining anything. That is what critics of Linde-Guth are pointing out. Admitting that the inflationary paradigm contains multitudes of theories is to admit that it contains no real explanator power. It is just a mathematical idea allowing theorists to be employed as nature’s ambulance chasers. That seems a pretty damning point for Linde-Guth…

I want to hear more about the “measurement problem” as it seems there is still huge disagreement about how to do statistics properly in an infinite multiverse. Is this the same problem of trying to do anthropic arguments in a multiverse? Ie the same as disagreements over the Boltzmann brain gedanken experiment?

17. **Peter Woit**
June 2, 2017

cosmology grad student,
I think the BICEP2 fiasco has a lot to do with this. Guth and Linde claiming “smoking gun” evidence and collecting their Kavli prize based on a wrong experimental result, then going on to claim that the evidence just doesn’t matter, inflation can give any value for r, likely encouraged Steinhardt to say something. From their point of view, I suspect that after BICEP2 they felt they were so close to a Nobel Prize they could taste it, and aren’t about to give that up.

I just don’t at all believe that the measure problem “doesn’t really bother most people in the field”, my impression is quite the opposite. What most strikes me about this whole subject though is that you don’t have a clue what space you want your measure on (unless you believe that KKLT describes the real world, which no one does).

As for the identification of string theory and inflation, I agree that there are huge terminology problems here, with no precise meaning to either term. But it’s not me who is identifying the two, it’s Linde. See
https://arxiv.org/abs/1402.0526
where for instance he writes

“the most interesting recent developments of the theory of eternal inflation are related to the theory of inflationary multiverse and string theory landscape [57, 62, 63, 64, 65, 66, 67]. These developments can be traced back to the very first paper on eternal inflation in the chaotic inflation scenario [57]. It contained the following statements, which later became the manifesto of the string landscape scenario: “As a result, our universe at present should contain an exponentially large number of mini-universes with all possible types of compactification and in all possible (metastable) vacuum states consistent with the existence of the earlier stage of inflation. If our universe would consist of one domain only (as it was believed several years ago), it would be necessary to understand why Nature has chosen just this one type of compactification, just this type of symmetry breaking, etc. At present it seems absolutely improbable that all domains contained in our exponentially large universe are of the same type. On the contrary, all types of mini-universes in which inflation is possible should be produced during the expansion of the universe, and it is unreasonable to expect that our domain is the only possible one or the best one.”

The problem is that GKLN and ISL aren’t just arguing about the conventional sort of scientific issue that arises if you have a well-defined class of models (say single-field inflaton with a simple potential) and are trying to compare them to experimental data. GKLN are explicitly saying that the potential can be arbitrarily complicated, there can be arbitrary many fields, ISL describe this as “postmodern inflation” and I think very rightly object to it as unfalsifiable pseudo-science. The GKLN letter that they got 29 others to sign onto does not specifically address this issue, but it does contain a defense of the multiverse, by implication claiming that postmodern inflation and its string theory landscape backing are part of what is being defended.

18. **Peter Woit**
June 2, 2017

Haelfix,

Yes, but Guth/Linde are very explicit about what they are defending: the multiverse in which you have different physics in each different universe, and use that as an excuse for why your models don’t predict things. This is what the controversy is about.

19. **Blake Stacey**
June 2, 2017

“So if we ever became convinced that the universe has a nonzero density of electric charge or angular momentum, eternal inflation would no longer be an option.”

I dunno. It seems to me that if the astronomers found that the net angular momentum of the observable universe is nonzero, then the eternal inflationologists would claim this as evidence for their multiverse, because it meant there must be another universe out there with opposite angular momentum to cancel everything out.

20. **Haelfix**
June 2, 2017

It is perfectly logically consistent to accept the inflationary paradigm, and even the subset of inflationary models that give rise to eternal inflation, while simultaneously questioning whether our particular bubble was anthropically selected. That’s why I reject the either/or dichotomy that’s being presented to us.

The anwer is that we simply don’t know whether our bubble was anthropically selected or not, and the details matter. I mean we could have asked the same question about the orbits of planets in our solar system and why the convenient configuration for the earth in particular . One could have imagined (before the calculations were done) that there was a mechanism during the collapse of primordial interstellar hydrogen that might have led to planets being preferentially chosen in the goldilocks zone. Of course we now know the answer for us was essentially anthropic.

We're in the exact same position with the theory of inflation, namely we need more data (gravitational wave observations in particular) to really zero in on the messy details to make statements that are more constrained.

21. **S.**
June 2, 2017

cosmology grad student,

does there exist any known case of where using the kinds of probabilistic methods that "almost everyone in the community" believe in has led to a correct prediction?

It seems to me that the practice is pure nonsense, mathematically speaking. We have as much mathematical reason to believe in conclusions reached through such arguments as we have in conclusions reached by inspecting goat entrails.

Of course, things can work empirically even without being mathematically well-defined (e.g. infinitesimals in the 17th century, parts of QFT still today). But if we don't have any empirical evidence that they actual work, the fact that lots of people believe in them is more a condemnation of the state of the field than a vindication of the method.

So is there any reason to believe that "picking a measure in the first place" actually works, rather than that it is just popular?

22. **EJM**
June 2, 2017

If the multiverse idea is correct, and there are an infinite number of universes where anything and everything is possible, then there must be a universe in which most physicists in it believe and sign a letter that the multiverse idea is incorrect.

23. **Peter Woit**
June 2, 2017

Haelfix,
The question isn't really anthropic selection, it's whether your theoretical framework is rigid enough to be useful to say something about reality, or whether it's so flexible it can never be wrong and is an empty, useless idea. The problem IJS are pointing out is that the framework GKLN are pushing appears to be compatible with any conceivable future observational results. For any value of r, Linde has a model that will give that value of r, same for other observables. He and Guth seem happy with this situation, argue that it's normal science. It's not.

S./cosmology grad student,
I really don't want to host a discussion of "the measure problem" consisting of completely ill-defined quantities and meaningless statements. If there are specific, well-defined calculations that are relevant to the issues in this controversy, fine, but spare us the nonsense if they're not.

24. **Jonatan**
June 2, 2017

I think one key point raised by IJS is that EVEN once you've picked a particular potential and fixed its parameters (selected one model within the inflationary "paradigm"), if eternal inflation apply (which seems to be so for the vast majority of cases), then you'll get an infinity of CMB observable values in the infinity of pocket/bubble universes.

If they're right, then it means that the whole inflation framework is really incapable of predicting anything and has therefore zero explanatory power.

I've never read or heard anything from GKLN directly addressing this issue, which is very suspicious to me. If IJS are wrong about that, they should very clearly point it out.

Perhaps a cosmologist could enlighten me here…

25. **Jonatan**
June 2, 2017

In other words, is it possible for a given eternaly inflating model (a particular potential with defined parameter values) to predict exactly the same CMB statistical distributions and spacetime geometry in ALL of the infinite pocket/bubble universes ?

If the answer is yes, than inflation might be said to be predictive.
If the answer is no (which is I think one of IJS's claims), than it can't.

26. **Haelfix**
June 3, 2017

It's important to distinguish two different things. There is on one hand the underdetermination of the models due to a lack of data. Basically you are given 5 observables in the CMB and you have hundreds of different inflationary potentials with parameters, some of which are allowed to vary. Therefore for any possible measurement you are possibly (but not necessarily) looking at more than one model that fits. This is the inverse problem, but note that this is really no different than any usual pheno problem in physics, and is how the standard model of particle physics came about. There was a time in the past where we didn't have enough data to really uniquely select the standard model out of the space of all field theories with undetermined parameters. Fortunately, just like the standard model we have strong constraints (renormalizability, naturalness, simplicity, slow roll etc) and so with further data collection you can really start to zero in on the right answer. Indeed, you could even falsify the whole thing, recall that the initial combination of Bicep with other measurements actually ruled out the whole inflationary paradigm.
On the other hand, there is the concern that given the large volumes of space, you might have bubbles that undergo vacuum decay (or other rare events), and so we might be living in a bubble that just happened to randomly output our observed universe with 'whatever you want'. To this second point, I would repeat my criticism above. Namely that it depends upon the details, you don't necessarily get 'anything you want', but yes the measure problem appears in this case…

27. **Peter Woit**
June 3, 2017

Haelfix,
Sorry, I'm having trouble taking this seriously enough to spend time thinking about. I don't remember hearing any inflationistas claiming "the initial combination of Bicep with other measurements actually ruled out the whole inflationary paradigm", I remember everyone congratulating Guth/Linde on their soon to be awarded Nobel prizes

for the confirmation of inflation.

As for the “it’s just like the Standard Model”, my tolerance level has dropped to zero for that rhetorical move in these arguments over theories of questionable testability.

28. **Haelfix**
June 3, 2017

So in one of the original combination plots (that no one took seriously b/c you can’t just combine data sets like that), BAO + Planck + Bicep produced a so called ‘negative running’ of the primordial power spectrum. This sort of thing would be almost impossible to find in eternal inflation.
See : http://arxiv.org/abs/1404.4614

29. **vmarko**
June 3, 2017

Haelfix,

“There was a time in the past where we didn’t have enough data to really uniquely select the standard model out of the space of all field theories with undetermined parameters.”

This is still the case — we still don’t have enough data about the neutrino sector to fix a unique Lagrangian for the SM. There is the Dirac/Majorana issue, the direct/inverse mass ordering issue, etc.

However, no sane physicist is shouting that SM is untestable because of that, because this is just a handful of unknown numbers, which moreover just barely participate in most of the experiments we can perform. In contrast, the inflation scenarios typically have an arbitrary function (the inflaton potential) in the Lagrangian, which amounts to infinitely many parameters to tune. This is untestable almost by definition, since you need an infinite number of experiments to fix the Lagrangian before you even begin testing it (and this is not the only problem with inflation). This situation is nothing like the SM nonuniqueness.

Best, 🙂
Marko

30. **Jonatan**
June 3, 2017

If IJS are right that even once a potential has been chosen and parameters fixed, eternal inflation inevitably produces a whole range of CMB observable outcomes across pocket/bubble universes, than it seems quite clear that eternal inflation is hopelessly untestable and is an empty idea.

If they’re wrong, proponents of inflation should explain clearly where and why IJS are wrong about that and clearly state that at least some eternally inflating models will produce the exact SAME CMB observable outcomes in ALL pocket universes.

Is anyone aware whether such explanation has ever been provided ?

31. **Cosmonut**
June 5, 2017

The multiple versions of inflation seem to be a trivial corollary of the fact that GR permits multiple space-time geometries. So in that sense I agree with Linde & Guth that this doesn’t automatically disqualify them.
However, people didn’t believe GR because we could somehow dream up a metric that explained the perihelion of Mercury or whatever (I daresay with enough people slogging away, we could). What happened was that the appropriate metric could be *independently deduced* from other considerations and *then* the predictions matched.
GR also permits metrics corresponding to time machines, rotating universes and so on. But nobody is loudly proclaiming that “This is a feature of the theory and we must accept the existence of time machines because other predictions of GR were valid”.
The question simply focuses on what kind of matter and energy distribution leads to such metrics and whether we can find *independent evidence* for existence of the latter.
In that respect, I am with ISL. It doesn’t suffice at all to dream up some inflation potential that makes “generic predictions”, declare victory and then insist we need to accept extrapolations like the multiverse. Until we find independent evidence of at least one scalar field and *then* deduce what it implies for cosmology, inflation must remain a provisional theory.

32. **kashyap vasavada**
June 5, 2017

I thought the main argument in favor of inflationary model was that it has three desirable features. It can explain (1) flatness (2) horizon problem and (3) lack of monopoles. Does anyone on this blog know if Steinhardt’s or anybody else’s model does this?

33. **Peter Woit**
June 5, 2017

kashyap vasavada,
I’m no expert on 1 or 2, but an obvious answer to 3 is that the Standard Model has no monopoles. 3 is only a problem if you believe in GUTs with monopoles (as Guth did when he was looking for a solution to this “problem”). There is no evidence at all for such a GUT (and by now quite a lot of evidence against), so the simplest solution to 3 is that our fundamental theory is not a GUT with monopoles.

34. **Cosmonut**
June 5, 2017

Kashyap,
Yes, Steinhardt’s model does indeed handle the flatness and horizon problem. The model has a mechanism whereby the universe is flattened and homogenized during a contracting phase of the universe, before it “bounces” (the bounce is “our” big bang)
The main problem is the bounce itself – it requires violations of GR and is just “assumed to be possible” – whereas inflation requires unobserved scalar fields, but then it’s just GR.
More details here: https://arxiv.org/pdf/1512.09010.pdf

35. **tulpoeid**
June 6, 2017

I was waiting until the next renewal date to cancel my Nautilus subscription. No need to wait anymore. The amount of money lost will be the same but the point will not be totally lost.

36. **vmarko**
June 6, 2017

Cosmonaut,

"The main problem is the bounce itself – it requires violations of GR and is just "assumed to be possible" […]"

It's not such a big stretch to imagine the violation of GR near the Big Bang moment. First, at such energies the difference between GR and Einstein-Cartan gravity becomes nontrivial, and the latter was shown by Poplawski to produce a Bounce instead of a Bang. So a rather miniscule violation of GR is quite enough. In addition, quantum corrections to GR should play a nontrivial role in such a regime, and in some models (such as loop quantum cosmology) one naturally obtains a bounce via the Friedmann equation with a small quantum correction term. So I'm not at all surprised that in string theory something similar might appear, as conjectured by Steinhardt and Turok.

Big Bounce is a quite ubiquitous concept in cosmology, you can read some history overview here:

https://en.wikipedia.org/wiki/Big_Bounce

And some of the models which feature Big Bounce also naturally resolve the flatness and horizon problems, so they are very solid alternatives to inflation.

Best, 🙂
Marko

2018 US HEP Budget

June 6, 2017
Categories: Uncategorized

HEPAP has been meeting the past couple days, with presentations available here. Much of the discussion is about the President's 2018 budget proposal recently submitted to Congress, which contains drastic cuts to all sorts of programs, including for support of scientific research. In particular the proposal is to cut the total NSF budget from \$7.5 billion to \$6.65 billion (-11.3%), and the DOE science budget from \$5.4 billion to \$4.47 billion (-17%).

At the DOE, for HEP physics, the cut would be from \$825 million to \$673 million (-18.5%). For topics less popular with the new administration the cuts are even larger, e.g. a 43% cut for biological and environmental research.

At the NSF (numbers with respect to FY 2016), the proposed cut for DMS (Mathematics) is 10.3%, for Physics 8.5% (-\$23.6 million) and for Astronomy 10.3%. The FY 2016 budget number for Physics was \$277 million, of which \$13.2 million went to HEP theory.

Budget cuts on this scale would be extreme and unheard of, requiring shutting down major planned experimental projects. For some sorts of spending, this sort of cut is painful but manageable, but cutting out 18.5% of the spending on an experimental apparatus under construction may likely mean you don't have an experiment anymore.
The HEPAP presentations are from people working for DOE/NSF and under orders to plan for these cuts and not complain about them, so I think don't reflect at all what the real implications of such cuts would be.

There's a summary of discussion here, including a discussion of last year's HEP theory letter. It sounds like nothing much has been done about that, and it may not get much attention given the current situation.

It's important though to keep in mind that this budget proposal may very well already be dead on arrival at Congress. Take a look at slide 22 of this presentation that reports that of the staffers and representatives asked about (a preliminary version of) this, only 8.4% were in favor. In recent years the US budgeting process has been quite dysfunctional, with actual budget numbers only appearing at the last minute of an opaque process leading not to a budget but to a "Continuing Resolution". I doubt anyone has any idea what is going to happen this year, with the passing of something close to this budget probably one of the least likely eventualities. Physicists and mathematicians up in arms about these proposed budget cuts need to keep in mind the context: this budget is an extremely radical proposal of an unparalleled sort, with even larger cuts aimed at groups that are far needier than scientists (for one random example, food stamps are to be cut by 25.3%). Yes, scientists should be organizing to fight this budget, but the impacts on them and their research are one of the less important reasons for doing so.

I'm setting all comments to go to moderation. If you just want to rant pro or con about the awful situation the US is now in, please do it elsewhere. If you have any actual information about the effects of this on the physics and math communities as the budget process gets underway, that would be worthwhile and interesting. Two people tweeting about this are Kyle Cranmer and Matthew Buckley.

Updates: Details of the DOE HEP budget proposal are here. It explains that about 20-25% of the research positions funded by DOE at all levels would be eliminated. There would be an "extended shutdown of the Fermilab accelerator complex".
About 1/3 of DOE HEP theory funding would be eliminated, but it would be replaced by an equal amount of funding for quantum information science as a subfield of HEP. Looks like someone in the Trump administration is a great believer in "It From Qubit"…

Update: According to this story, if this budget passes about 700 jobs at Argonne and Fermilab would be eliminated.

Comments

1. **cosmology grad student**
   June 6, 2017

   I think you're misreading what quantum information science means in this budget. On p. 180 of the budget document you linked in your update: "Increased funding will support QIS efforts in quantum computing and quantum sensor development." And the funding category that's increased is called "Computational HEP." My guess is that the increase isn't going to "It from Qubit" (which is funded by Simons not the US government) or anything like it, but more directly to nitty-gritty quantum computing. No doubt quantum info people who work on e.g. AdS/MERA or Bootstrap could try to argue that what they do falls into this category, but I doubt that's what the people who wrote this line item in the budget had in mind.

2. **Peter Woit**
   June 6, 2017

   cosmology grad student,
   That side comment was a bit tongue in cheek. I really don't know what that $14 million is for, and why it's replacing 1/3 of the HEP theory program. To me quantum information science seems a really different business than HEP, so it's unclear why it's being funded out of HEP, but there must be some set of research priorities I don't know about behind this (beside "It from Qubit").

3. **Douglas Natelson**
   June 6, 2017

   Don't overthink it. Someone likes quantum info research, and likely doesn't really understand what it is or its relationship to high energy physics beyond the money-is-fungible interrelatedness of all agency programs.

   I agree that this budget request is so out of the norm that we are basically off the edge of the map in terms of trying to figure out what Congress might do. You also left out the fun idea put forward about how to handle the proposed NIH cuts without cutting research dollars: refuse to pay indirect costs at more than 10%. Again, probably DOA in the legislature, but slashing indirect cost payments and having a flat rate rather than individual negotiations with each university likely would have appeal to some in Congress.

4. **Giulio**
   June 7, 2017

   Cutting the budget so much is certainly bad; but in the particular case of HEP theory, given that most of the money is anyway spent on things you consider "not even wrong", you should see it as money saved instead of being wasted. Or am I wrong?

5. **piscator**
   June 7, 2017

I think slashing university overheads would be an excellent idea. The growth in administrative numbers and salaries is one of the biggest problems with modern universities, and one of the ways they are funded is through the central university slice on grant income. It would be one of the rare ways of saving money and boosting the academic culture of universities simultaneously.

6. **Peter Woit**
   June 7, 2017

   Giulio,
   The amounts going to HEP theory are infinitesimal compared to other things. Most of such funding is not going to multiverse or other studies I think are completely worthless, but to often reasonable research. These days what a lot of HEP theorists are doing is actually working on QFTs with applications to topology or condensed matter.

   There is an issue about some of this funding that I've always seen as problematic, independent of the value of the research. At least in the US, spots for HEP theory graduate students are often funded this way, and the huge imbalance between the number of Ph.Ds produced and the number of permanent jobs has led to a horrific job market which I don't think is good for the field. So, I can't say that I'm strongly opposed to some cutting of Ph.D. funding in this area (or, in any area where the job market is awful).

   This is completely irrelevant to the current situation, since those pushing these cuts are not interested in targeted cutting to improve things, but in making huge across the board cuts to scientific research in general.

7. **Peter Woit**
   June 7, 2017

   piscator,
   I think the idea that removing grant money will starve the administrative beast is naive. At a certain large institution I know well and have observed closely over decades, the administration and its salary level has grown dramatically, during a period when overhead rates and grant income haven't. The way this gets paid for is not grants, but steady increases in tuition levels, radically changing the socioeconomic profile of the student body, making the institution more and more aimed at catering to the children of the wealthy.

   If overhead disappears, of the two alternatives of administrators taking pay cuts/losing their jobs, or larger tuition increases, I know which one I would bet on happening.

8. **Jeff M**
   June 7, 2017

   Piscator

   Peter is right, this has nothing to do with grants. I work at a state U, we have some grants, but nothing like the level at Columbia where Peter is. We've had just as much administrative bloat as Columbia. As far as tuition levels, when I was applying to college, back in the dark ages, full freight at Columbia was about \$7K. It's \$70K now. Inflation would justify \$25K.

9. **Rob**
   June 7, 2017

   Do I read correctly on page 188 in the detailed HEP budget proposal that the muon g-2 measurement at Fermilab is dead (if the budget passes). Or is it some type of upgrade/offshoot which is in the firing line ?

10. **Shantanu**
    June 7, 2017

    Peter, do you have any statistics on how much of DOE HEP theory funding has been invested in string theory?

11. **Peter Woit**
    June 7, 2017

    Rob,
    It looks to me like the g-2 experiment will soon be taking data, and have a measurement later this year, so this isn't a cancellation (don't know if there were plans for future upgrade).

    Shantanu,

    For a long time now different people would count different things as "string theory", so I don't think there's any sensible number of that sort, at least for the last 10-20 years.

12. **Tom Andersen**
    June 8, 2017

    I don't want to sound depressed, but a 50% budget cut to a normal bureaucratic system will result in 100% of actual work being stopped. The bureaucrats always find a way to keep their 'inner circle machine' running. One can expect linear behaviour, so a 20% cut means 40% of useful work being halted.

13. **Scott Aaronson**
    June 8, 2017

    Not that anyone making these budgets asked for my opinion, but on the small chance any of them read these comments: I'm a quantum computing person, who also happens to be a member of the It from Qubit collaboration, and quite interested recently in work at the interface between quantum information and quantum gravity. And I don't want additional funding for quantum information if it has to come out of already-strained HEP budgets.

    In general, I'm always dismayed when funders see something new and shiny (for example: a scalable quantum computer), and think they can just redirect all their resources to that, without understanding that even supposing it was all they cared about, they're less likely to get it without a healthy research ecosystem, which in this case means everything from basic physics to math to classical CS. To take a very common instance of that failure mode: you can plunk down a beautiful new interdisciplinary center at a university — but then if you don't put adequate resources into the basic needs of the individual departments that the center is supposed to be bringing together, great people won't want to be in those departments, and your center will end up as a gleaming bridge from nowhere to nowhere.

14. **Chris Herzog**
    June 9, 2017

"Yes, scientists should be organizing to fight this budget, but the impacts on them and their research are one of the less important reasons for doing so."

I wonder if it's actually one of the more important reasons for doing so. Here in Suffolk County on the eastern end of Long Island, two of the biggest employers are Brookhaven National Labs and Stony Brook University. Science research dollars from the federal government add about 500 million dollars to the local economy. Take that away, and this place becomes a summer retreat for wealthy New Yorkers and (maybe — it's not so clear) a cheaper place to live for those who work in the city and are willing to deal with a two hour commute each way.

Universities can serve as a hub for economic development in this new economy that depends so much on service, technology, and creativity. One can make a strong argument that federal research dollars are the seeds from which the local economy grows.

15. **Kev Abazajian**
June 9, 2017

My new relevant blog post, "Defending Science in the Age of Trump." http://www.314action.org/news/2017/6/9/defending-science-in-the-age-of-trump

16. **Noah Graham**
June 9, 2017

Chris Herzog — I agree that federal research dollars can be seeds for the local economy, but the problem is that the funding system tends to replant those seeds in the same, wealthy areas (e.g. Long Island). If there were more mechanisms to extend research infrastructure into less prosperous areas, both urban and rural, science would enjoy much broader political support (and cost less to carry out, as well).

17. **ronab**
June 12, 2017

"…if this budget passes about 700 jobs at Argonne and Fermilab would be eliminated."

Is there a reasonable way to extrapolate as to what this would imply for, eg, job losses at LBL, SLAC, etc.?

18. **Peter Woit**
June 12, 2017

ronab,

Not that I know of. Even the 700 number is some sort of vague guestimate.

The Dangerous Irrelevance of String Theory

June 12, 2017
Categories: Uncategorized

Eva Silverstein has a new preprint out, entitled The Dangerous Irrelevance of String Theory. The title is I guess intended to be playful, not referring to its accurate description of the current state of string theory, but to the possibility of irrelevant operators having observable effects.

The article is intended to appear in the forthcoming Cambridge University Press volume of contributions to the Munich "Why Trust a Theory?" conference held back in December 2015. The impetus behind that conference was a December 2014 article in Nature entitled Scientific method: Defend the integrity of physics. In that article, Ellis and Silk explained the problems with string theory and with the multiverse/string theory landscape.

The organizing committee for the Munich conference was chaired by Richard Dawid, a string theorist turned philosopher who has written a 2013 book, String Theory and the Scientific Method. For a fuller discussion of that book, see the linked blog post. To oversimplify, it makes the case that the proper way to react to string theory unification's failure according to the conventional understanding of the scientific method is to change our understanding of the scientific method. Much of the Munich conference was devoted to discussing that as an issue in philosophy of science.

One aspect of the Munich conference was that it was heavily weighted towards string theorists, with contributions from Dawid, David Gross, Joe Polchinski, Fernando Quevedo, Dieter Lust and Gordon Kane all promoting the idea that string theory was a success. Polchinski explained a computation that shows that string theory is 98.5% likely to be correct, going on to claim that the probability is actually higher: "something over 3 sigma" (i.e. over 99.7%). The only contribution from a physicist that I've seen that argued the case for the failure of string theory was that from Carlo Rovelli, see here. Silverstein's article says that it was commissioned by Dawid for the proceedings volume, even though she hadn't been at the meeting. I'm curious whether Dawid commissioned any contributions from string theory critics who weren't at the meeting.

Silverstein begins her article explaining how physics at a very high energy scale can in principle have observable effects. This of course is true, but the problem with string theory is that, in its landscape version, it has a hugely complicated and poorly understood high energy scale behavior, seemingly capable of producing a very wide range of possible observable effects, none of which have been seen. The article is structured as a defense of string theory, without explaining at all what the serious criticisms of string theory actually are. The list of references includes 53 items, only one critical of string theory, the Ellis/Silk Nature article. Some of the arguments she makes are:

- It is sometimes said that theory has strayed too far from experiment/observation. Historically, there are classic cases with long time delays between theory and experiment – Maxwell's and Einstein's waves being prime examples, at 25 and 100 years respectively… One thing that is certainly irrelevant to these questions is the human lifespan. Arguments of the sort 'after X number of years, string theory failed to produce Y result' are vacuous.

I think the comparison to EM or GR is pretty much absurd. For one thing it's comparing two completely different things: tests of a particular prediction of a theory (EM or GR) that made lots of other testable, confirmed predictions to the case of string theory, where there are no predictions at all. More relevant to the argument over how long to wait for an idea to pay off is that the real question is not the absolute value of the amount of progress, but the derivative: as you study the idea more carefully, do you get closer to testable progress or farther away? I don't think anybody can serious claim that, 33 years on, we're closer to a successful string theory unification proposal than we were at the start, back in 1985. I'd argue that the situation is the complete opposite: we have been steadily moving away from such success (and thus entered the realm of failure).

- About supersymmetry Silverstein writes:

> In my view, the role of supersymmetry is chronically over-emphasized in the field, and hence understandably also in the article by Ellis and Silk. The possibility of supersymmetry in nature is very interesting since it could stabilize the electroweak hierarchy, and extended supersymmetry enables controlled extrapolation to strong coupling in appropriate circumstances. Neither of these facts implies that low-energy supersymmetry is phenomenologically favored in string theory.

It is true that Silverstein has never been one of those arguing that the usual string theory scenarios with supersymmetry and 10 or 11 dimensions show that string theory is testable. See for instance her comment here back during a "String Wars" discussion in 2006. Her current take on whether string theory implies supersymmetry is just

> Much further research, both conceptual and technical, is required to obtain an accurate assessment of the dominant contributions to the string landscape.

The problem with this is that there is no sign of any possibility of progress towards deciding if the string theory landscape implies low-energy SUSY or not (quite the opposite). If you give up the assumptions of SUSY and 10/11 dimensions, you give up what little hope you had of any connection with experiment. She doesn't mention the LHC at all, especially not the negative results about supersymmetry and extra dimensions that it has produced. The significance of these negative results is not that they disconfirm a strong prediction of string theory, but that they pull the plug on the last remaining hope for connecting standard string theory unification scenarios to anything observable. Pre-LHC string theorists could make an argument that there was good reason to believe in electroweak-scale SUSY, that such a scenario fit in well with string theory unification, and that LHC discovery of SUSY would point a way forward for string theory unification. That argument is now dead. All that's left is basically the argument that "maybe a miracle will happen and we'll be vindicated" which in her version is:

> In principle one could test string theory locally. In practice, this would require discovering a smoking gun signature (such as a low string scale at colliders, or perhaps a very distinctive pattern of primordial perturbations in cosmology), and nothing particularly favors such scenarios currently.

- Silverstein's main argument is basically that string theory is valuable because it leads to the study of models that have various observable signatures that people would not otherwise look for. One example here is supersymmetry, the study of which has had a huge effect on collider physics, strongly shaping the analyses that the experimentalists perform. She gives some detailed other examples from her field of cosmology, in particular about possibly observable non-Gaussianities.

> String theory participates in empirical science in several ways. In the context of early universe cosmology, on which we have focused in this article, it helped motivate the discovery and development of mechanisms for dark energy and inflation consistent with the mathematical structure of string theory and various thought-experimental constraints. Some of these basic mechanisms had not been considered at all outside of string theory, and some not quite in the form they take there, with implications for effective field theory and data analysis that go well beyond their specifics.

I think this is the best argument to be made for "phenomenological" string theory research (as opposed to "formal" string theory, where there are other arguments). Yes, coming up with new models with unexpected observable effects is a valuable enterprise. If your speculative idea generates such things, that's well and good. The problem though is how to evaluate the situation of a speculative idea that has generated a huge number of such models, none of which has worked out. At what point do you decide that this is an unpromising line of research, better to try just about anything else? Silverstein makes the argument that

> Whether empirical or mathematical, constraints on interesting regions of theory space is valuable science. In this note we focus on string theory's role in the former.
>
> Since information theory is currently all the rage, it occurred to me that we can phrase this in that language. Information is maximized when the probabilities are equal for a set of outcomes, since one learns the most from a measurement in that case. The existence of multiple consistent theoretical possibilities implies greater information content in the measurements. Therefore, theoretical research establishing this (or constraining the possibilities) is directly relevant

> to the question of what and how much is learned from data. In certain areas, string theory plays a direct role in this process.

> The problem here is that of what is an "interesting region of theory space". At this point the failures of string theory unification strongly indicate that it's not such an interesting region. It seems likely that we'd be better off if most theorists focusing on phenomenology of this failed program were to pick something else to work on.

Update: Will Kinney has a Twitter commentary here.

Update: For another relevant recent Will Kinney Twitter storm, see here and here.

Update: Silverstein gave some lectures to the public about this at Stanford recently, video here and here, slides here. A large part of the lectures were an advertisement for string theory, with the summary at the end

> Quantum gravity (string theory) plays a subtle but important role, even contributing to our understanding of empirical measurements of early universe dynamics.

Crediting string theory with "contributing to our understanding of empirical measurements of early universe dynamics" is a peculiar way of saying that string theory produces lots of cosmological models that don't work (see a better summary by Will Kinney at the end of this presentation).

Update: Renata Kallosh is Silverstein's colleague at Stanford (and Andrei Linde's wife). In an interview here she makes much simpler and stronger claims about string cosmology than does Silverstein:

> string theory ideas help us to build cosmological models which fit the data from observations. Moreover, we have produced relatively simple predictions from string theory and related theories which will be testable with future detectors of primordial gravity waves.

I'm not sure what specifically she is referring to, but suspect that "prediction" here means something like the "predictions" of string cosmology that Kinney describes (see above) whose failure to be observed has in no way affected string cosmologists enthusiasm for the subject.

Update: For some more context about string theory inflation models and the issue of their testability, you could consult Silverstein's guest post at Lubos Motl's blog from 2014, explaining how the BICEP2 observation of that era could provide evidence for "axion monodromy inflation".

Comments

1. **Doug McDonald**
   June 12, 2017

   typo:
   "The problem with this is that there so sign of any possibility of progress"

2. **Bee**
   June 13, 2017

   I was invited to write a contribution but declined.

   The criticism you raise that there are lots of speculative models that have no known relevance for the description of nature has very little to do with string theory but is a general disease of the research area. Lots of theorists produce lots of models that have no chance of ever being tested or ruled out because that's how they earn a living. The smaller the probability of the model being ruled out in their lifetime, the better. It's basic economics. Survival of the 'fittest' resulting in the natural selection of invincible models that can forever be amended.

3. **MikeS**
   June 13, 2017

   "Historically, there are classic cases with long time delays between theory and experiment – Maxwell's and Einstein's waves being prime examples, at 25 and 100 years respectively…"

   One has to wonder at the ability of intelligent people to make this type of argument.

   Maxwell's equations validated themselves almost instantly in predicting the speed of light from a number of well established universal constants. Exactly the sort of predictive power that String Theory consistently fails to offer. And Einstein's GR was validated by Eddington within a very few years, and has been reinforced by just about every test anyone can think of in the 100 years since. Gravitational waves have never been more than a corollary to that. GR has never been reliant on LIGO, like some gravitational equivalent of the LHC, to push its sensitivity ever higher, and ever failing to see what was expected.

   I claim neither the intellect nor the background to understand the complexities of String Theory. However, when I see clearly poor arguments and comparisons such as these, then I worry that the complexities are existing on equally weak ground.

4. **Dave Miller**
   June 13, 2017

   Peter,

   You wrote, "I think the comparison to EM or GR is pretty much absurd. For one thing it's comparing two completely different things: tests of a particular prediction of a theory (EM or GR) that made lots of other testable, confirmed predictions to the case of string theory, where there are no predictions at all."

   Isn't the real problem the use of the word "theory" to refer both to EM/GR and also to string theory (the same holds for your recent posts on inflation "theory" in cosmology)? Isn't string theory (again, also inflationary cosmology) more a set of interesting speculations and suggestive calculations that might (or might not) someday lead to an actual theory than currently being a real physical theory?

   I know you have made this point again and again, and I do not want simply to be pedantic about vocabulary. But, GR, for example, was sitting there from 1915-16 with, e.g., gravitational waves a definite mathematical consequence of the theory, whether or not anyone could detect them. On the other hand, no one knows what, if any, are the consequences of string theory just because there is, so far, no such theory.

   Surely, it would be more accurate to refer to string/superstring "musings" or "speculations" (and similarly for inflationary cosmology). Words do matter.

I think I see a bit more value in such “musings” than you do: I still hope that someday an actual theory will burst forth from all the work on strings. But, alas, not yet.

All the best,

Dave

5. **Peter Woit**
June 13, 2017

DaveMiller/MikeS,

The problem is that usage among physicists has now for a very long time been that the term “theory” carries no implication of testability, being well-defined etc. Unfortunately those trying to defend science against its opponents like to insist otherwise, see for example the Wikipedia entry:
https://en.wikipedia.org/wiki/Scientific_theory
This likely leads many people to believe that “string theory” has characteristics that it doesn’t, but I know of no one trying to do anything about that misunderstanding.

The rather absurd comparison of the testability of string theory to that of EM is surprising to see, but not at all unusual in this kind of document. One would expect an argument being made in an academic context like this to make an attempt to address both sides of the argument, refer to the other side in the bibliography, and not make obviously absurd claims (one question here, are these contributions being refereed?). Instead, what’s produced is heavily ideologically-driven and one-sided. In some sense, I think the worst thing that has happened to theoretical physics over the past 25 years is this descent into ideology, something that has accelerated with the multiverse mania of the last 10-15 years.

Doug McDonald,
Thanks, fixed.

6. **Anon**
June 13, 2017

Hmm. It doesn’t look like the commenters have read the
…that you left out. This distorts
the meaning and generates like-minded comments
in this echo chamber.
The article goes right on to say in the … that most theories are
not like GR and EM, obviously referring to those
described in the article which are described
rather moderately in the main text. Anyway I
don’t see a problem with a comment about
theoretical predictions predating observations in some
famous cases, and the timescale not being
important, in context of the full article. My
problem is that the space of theories is so large,
so while I do agree that it is quite interesting in
some ways, there’s no way to test
everything in enough detail to infer much
unless we get lucky.

7. **Peter Woit**
June 13, 2017

Anon,
I don’t think anything I left out is relevant or distorts the meaning at all (the only relevant text dropped is “Of course electromagnetism and general relativity are not representative of most theoretical ideas, but the point remains valid.” which I didn’t and don’t see as having any relevance to the point I was making). The problem isn’t that she’s picking unrepresentative theoretical ideas to compare to string theory, or any issue of time-scale, but that she’s justifying the situation of string theory by claiming its like that of one of a heavily successfully tested theory (EM in its early decades).

8. **Anon**
June 13, 2017

Peter, the … is about how most ideas will be falsified or
constrained, but that is still valid science, which is a
main theme of the article. The section is about
timescales and null results. You are arguing against
a straw man here — the classic theories were not
compared to string theory, they were mentioned
in the context that even some well established
theories waited substantial periods for certain
of their predictions to be tested, the timescales
for theory and experiment not being perfectly
lined up even in that extreme case. But the paragraph you omitted
makes the contrast quite clear that the author
is making between the two cases. Seriously,
take a moment to consider less incendiary
ways of interpreting and presenting your subjects.
In this day and age, we should all make a point
of being reasonable and accurate.

9. **Peter Woit**
June 13, 2017

Anon,
I think I am being completely accurate and reasonable here. The article is explicitly a defense of string theory against the points made against it by Ellis/Silk, and string theory is explicitly referred to repeatedly in the paragraphs at issue. Claiming that there’s no comparison being made between the current situation of string theory and the situation in the past of EM/GR makes no sense.

The argument of this part of this part of the article seems to me completely straightforward: Silverstein is making the accurate point that it can take a long time to test some predictions of a theory, invoking examples from EM and GR. The absurdity is invoking these particular examples in the context of a defense of string theory research.

10. **Cosmonut**
June 13, 2017

Thanks for the very informative links to Will Kinney.
"It is not ever the case that all of a theory's predictions are empirically verified"
'True. But it would be useful to have *one*. It would be a start, anyway."

I think that one comment says it all, really. 😀

11. **skydivephil**
June 13, 2017

I am not convinced you have represented Rovelli's position correctly. I don't believe he was a saying that string theory has failed. Perhaps I have misunderstood what you were saying Peter. I think Rovelli was arguing that it had failures such as the lack of black holes, supersymmetry etc at the LHC that should reduce our credence in it. Not necessary cause us to abandon all of its ideas. I don't think he was a saying that string theory has failed . Perhaps you can point me to the relevant passage if I have that wrong.

Rovelli also seems quite happy with the idea of non empirical verification of science. he says it happens all the time. " To evaluate theories, they routinely employ a vaste array of non-empirical ar- guments, increasing or decreasing their confidence in this or that theoretical idea, before the hard test of empirical confirmation (on this, see Chapter VIII of [2]). This is the context of a "preliminary appraisal" of theories, or "weak" evaluation procedures'.

It seems to me then Rovelli is more in the middle between those that say anything that isn't testable by experiment isn't science and those that say if its theoretically plausible , that is enough. Im not sure there are many people that take these two extreme views anyway. I think Rovelli is saying non empirical appraisal is part of the scientific process and can give you some sort of "weak" status , a preliminary approval. But only experiment can give you the hard approval to turn your idea from promising proposal to accepted fact.
In Rovelli's previous appraisal of string theory he has both negative and positive things to say about it. Im sure you know the negatives but the positives were comments like

"String theory is a spectacular intellectual achievement and it might well turn out to be the right track. It is a rich and elaborate theory, that deserves to be studied further… But I think it would be a mistake to consider string the- ory as an established result about nature and therefore concentrate the attention solely on it. Also if the hopes of other research directions are realized, it would be a triumph. String theory appears of unmatched beauty to string theorists, but other ideas appear of unmatched beauty to others. What I think is important is to keep in mind that these theories are provisional.
I am not pessimistic. "

I am not seeing anything that implies he's changed his mind about this.

I also think the only game in town comment needs more careful examination. When I interviewed Abhay Ashtekar he seemed to be saying that in one sense LGQ is not a competitor to string theory and in another sense it is. LQG and string theory are both quantum theories of gravity but string theory goes much further in trying to be a unified theory. LQG does not do this and in that sense it is not a competitor to string theory. So I think one needs more care in evaluating the "only game in town" claim. In what sense was it being made ?

12. **Peter Woit**
June 13, 2017

skydivephil,
When I say "string theory has failed", I mean specifically failure as a unified theory, not that "one should abandon all its ideas" (of which there are many, of many different kinds). I don't claim Rovelli would use the same words, but the last three paragraphs of the paper I linked to list "failed predictions" of string theory and argue that that these "lower the degree of belief in string theory dramatically". I think Rovelli's "having the degree of belief in a theory lowered dramatically" can reasonably be translated into the more concise "the theory has failed". Of course "failure" is always something that comes in degrees: what I mean by a "failed idea" is one that is no longer promising. Sure, an unpromising idea may yet get revived, and some people should work on such ideas if they can't think of anything else to do.

About LQG vs. string theory, sorry but I don't think that's relevant to Silverstein's article.

13. **Will Kinney**
June 14, 2017

Peter, I'm not positive, but I suspecct Renata is referring to "alpha attractor" inflation models, which she proposed with Andrei Linde and Diederick Roest in 2013:

https://arxiv.org/abs/1310.3950

It's basically a class of non-minimally coupled scalar field models with universal behavior at strong coupling. The universality happens at strong coupling because in that limit the scalar sector becomes irrelevant, and the model asymptotically approaches Starobinsky R^2 inflation. In such a case, tensors will be at the r=0.01 level, observable with near-future observations.

Nice model, but calling it a test of string theory is a bit of a stretch, since the exact same prediction can be obtained through suitable choice of potential in a canonical scalar field model, with no reference at all to to UV physics.

14. **a 1**
June 14, 2017

The argument "you should not expect to see results in your lifetime" strikes me as identical to the advice given in religious instruction "you should not expect to see end of the world" – which people are hearing now for a third millenium in a row.

15. **Dave Miller**
June 14, 2017

Anon,

You wrote to Peter:

>You are arguing against
a straw man here — the classic theories were not
compared to string theory, they were mentioned
in the context that even some well established
theories waited substantial periods for certain
of their predictions to be tested, the timescales
for theory and experiment not being perfectly
lined up even in that extreme case.

With due respect, I think you are missing the real point here. The problem is not that string theory is a well-defined theory, just as GR was, but that it cannot yet be tested empirically for practical reasons, just as gravitational waves could not be detected for nearly a century.

The problem is quite different: GR was a perfectly well-defined theory by 1916 — it made clear, definite, and unequivocal predictions, even if some of those predictions could not be tested at the time. String theory (like inflationary cosmology) simply is not (yet) a well-defined theory that makes clear, unambiguous empirical predictions.

I.e., the problem with string theory (and inflationary cosmology) is not that we cannot test its predictions due to practical limitations but rather that it does not (yet) make any clear, definite, unambiguous predictions. The distinction is important.

I think Peter's posts do sometimes give the impression that his objection is simply that the empirical tests of string theory are taking too long in terms of time. But, he has made fairly clear that time is not the fundamental problem: the problem is that there simply is no well-defined, unambiguous theory in existence for strings (or inflationary cosmology).

Maybe that will change. I hope it will, and I am a bit more optimistic here than Peter.

But the distinction is an important one. Whether or not the solution is, as I suggested above, simply to refer to such "theories" rather as "musings" or "speculations," it is critical to keep in mind a distinction between ideas that make clear empirical predictions that *in principle* can be tested and those which do not.

Dave

16. **Peter Woit**
June 14, 2017

Thanks Will,

Linde and Kallosh do seem to have a different definition of "prediction" than most people (including Silverstein, who noticeably doesn't make claims like this about "predictions of string theory").

17. **Cosmonut**
June 14, 2017

The impression I have about the current state of affairs with "string theory":

– There is a minority of very vocal senior scientists loudly promoting the string theory landscape multimess on popular media to get publicity.

– The majority of researchers are just trying to mathematically extend "QFT in curved spacetime" in various directions.
The entire mishmash of approaches is mostly lumped together as "string theory" – which isn't really an accurate characterization.

Would that be approximately correct ?

18. **Peter Woit**
June 14, 2017

Cosmonut,
I'd agree with the first and last part. I don't think it's accurate though to describe what the majority of researchers are doing now as "just trying to mathematically extend "QFT in curved spacetime" in various directions". I don't think there is any simple way to characterize what "the majority of researchers" are doing, but lots of it has nothing to do with curved spacetime.

19. **tulpoeid**
June 18, 2017

Usually I get enraged by such writings, but, seriously now? "Historically, there are classic cases with long time delays between theory and experiment – Maxwell's and Einstein's waves being prime examples"?

It's becoming obvious to me that these people inhabit a very special and distorted space in their mind. Going on with their fervent blind belief and the more and more detached and uselessly sophisticated reasonings just brings them further away from reality — in a very everyday sense of "reality". Imho I don't care anymore for the amounts of money and arrest to science's development that they cause.

Attending pop science events and hearing the audience asking about the multiverse though when the speakers spoke about a dozen other verified wonders still makes me a little sad.

20. **Sebastian Thaler**
June 19, 2017

Meanwhile, hope springs eternal for the SUSY folks: https://www.nytimes.com/2017/06/19/science/cern-large-hadron-collider-higgs-physics.html

21. **Ra**
July 7, 2017

Dear Peter:

The problem is that: String theory is not a "theory", String theory is a paradigm, in the same way as General relativity is a paradigm, once you have a specific model (i mean: a particular solution for the field equations) you could make predictions, notice that if you pick an arbitrary solution form the landscape of general relativity, it is impossible to decide what are good observables, for example: The density parameter is a good number for a cosmological solution, but is irrelevant to try to compute that number for a gravitational wave (an allowed solution). This situation means that general relativity is unpredictable? Of course not and exactly the same happens with string theory, what is the problem with that ?

22. **Peter Woit**
July 7, 2017

Ra,

From the FAQ
http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=isnt-string-theory-just-as-predictive-as-quantum-field-theory

This Time It's Really for Real

June 14, 2017
Categories: Quantum Theory: The Book

Twice now I've thought I had a finished version of the book I've been writing forever (see here and here). Each time it turned out that, the way the publishing process was going, I ended up having more time to work on the manuscript and deciding I could do better, especially with some of the basic material about quantum field theory. I do think the latest version has a much improved treatment of the basics of that subject.

This version will go off to Springer in a day or so, and they plan to publish it late this year/early next year. I'm setting up a web-page for the book, there may be more material there later.

One thing ensuring that I will stop working on this is that in a couple days I'm heading off on vacation, for a two-week or so trip to Europe. Blogging during that time is likely to be light to non-existent. Back around the Fourth of July, and looking forward to thinking about other projects, anything but this book…

Update: If you're wondering why the document length changed today, no change in content, just a minor change (improvement) in formatting.

Comments

1. **Andreas**
   June 14, 2017

   Congratulation!

2. **db**
   June 15, 2017

   A friend of mine recently finished work on a textbook. He has given everyone he knows strict instructions to remind him to say "no" if he is asked to write another one 🙂

3. **anon**
   June 15, 2017

   Peter, it's great that the book is finally going to get published, but now leaving you at the mercy of the reviews on Amazon which will be fun for the rest of us to read. If it turns out successful, there's still the future opportunity for you to improve the book later on, so I wouldn't be too obsessed about getting it too perfect at this stage. The main thing is eliminating the errors.

4. **Peter Woit**
   June 15, 2017

   anon,
   I'm not so worried about Amazon reviews. This book isn't for everyone, and I'm more aware of its faults than anyone. I do hope it will be useful to both mathematicians and physicists in providing a point of view on the relation of physics and mathematics that isn't well-described elsewhere. There are quite a few good QFT and QM books, the virtue of this one I hope is that it's explaining as clearly as possible some different things and a different point of view, supplementing other books.

   I have put quite a bit of effort into getting rid of typos and getting all the formulas correct, that's an extremely time-consuming and not very rewarding business. There are certain topics in QFT that I started to try and write about, but stopped and decided they were best left to a later time, I hope to get to some of those.

5. **Bill**
   June 15, 2017

   Off-topic: http://www.icm2018.org/portal/en/icm-speakers

6. **Marco Masi**
   June 15, 2017

   It is a couple of weeks I was searching like mad for an overview that links qm with Lie algebras as a mathematical foundation, but none seemed to capture the 'spirit' I am looking for. But now it seems I have found it… Thanks for that. 🙂

7. **Erika**
   June 16, 2017

   Off-topic, quick links? There will be one obvious outlier in this poll: https://vote.pollcode.com/44839318

8. **A reader**
   June 17, 2017

   Congratulations ! And thank you very much for making the final draft freely available.

9. **Douglas Natelson**
   June 18, 2017

   Congratulations!

10. **Alexander Vlasov**
    June 19, 2017

    I found chapters about metaplectic representation very useful. How to cite the book?

11. **tommaso dorigo**
    June 19, 2017

Thumbs up Peter! You must feel so much lighter now that you should ask the airline for a discount…

Cheers,
T.

12. **Timothy P Keller**
June 20, 2017

Fantastic!
I have down-loaded the final draft three or four times! No big deal, except I always
end up printing it out because I hate reading anything at all long on a screen.

Danged long it is ,-but it's one of those subjects where there is always something else to say, and I think you put in details that are quite useful to understanding, not just technical trivia.

Thanks, Tim

This Week's Hype

June 22, 2017
Categories: This Week's Hype

I'm on vacation in Europe, not in any mood to spend more time on this than just to point out that it's the same usual tedious string theory promotional operation from the same people who have been at this for decades now. We have

- A PRL publication that has nothing at all to do with string theory, preprint here. This is about a purely classical pde calculation in coupled EM + gravity.
- The researcher's university puts out a press release.
- A story then appears where the usual suspects claim this is some sort of vindication for string theory and shows their loop quantum gravity opponents are wrong. There's a lot of quite good information in the story about the actual classical calculation involved, but no indication of why one might want to be skeptical about the effort to enlist this result in the string vs. loop war.

While traveling I've seen a couple very good stories about physics online:

- A summary from Dennis Overbye about the the current status of energy frontier HEP.
- An excellent long article by Philip Ball about quantum mechanics and the measurement problem.

Comments

1. **tulpoeid**
   June 22, 2017

   I don't know what to think about the very impressive article by P.Ball.

   On one hand, it's refreshing to see a sharp attack of the metaphysical features some people try to ascribe to quantum mechanics (it's 2017 and posts on stackexchange about quantum measurement quickly get hijacked by supporters of parallel universe and even conscious observer interpretations).

   On the other, it seems to me that his enthusiasm largely consists of rephrasing of controversial terms but not necessarily their explanation. How did the described scheme explain the statistical nature of the outcomes? Maybe I missed that and I will definitely look into the decoherence theory now, but informed comments could be helpful.

   What I wonder about is how much established the status of decoherence theory is. Statements like the following sound pretty strong; are they considered mainstream in quantum optics or other fields? (Of course not being mainstream doesn't render them wrong etc.)
   "A detailed theoretical analysis of decoherence carried out by Zurek and his colleagues shows that some quantum states are better than others at producing these replicas: they leave a more robust footprint, which is to say, more copies. These robust states are the ones that we can measure, and that ultimately produce a unique classical signature from the underlying quantum morass."

2. **Charles Day**
   June 22, 2017

   Peter, your readers might be interested in this (freely accessible) Physics Today article by Ed Witten that introduced M-theory. It appeared in May 1997, two years after Witten had conjectured the existence of such a theory as a way to unify various flavors of string theory. http://physicstoday.scitation.org/doi/abs/10.1063/1.881616

3. **Peter Shor**
   June 22, 2017

   tulpocid:

   If the article is correct, then before 1996, there was no way to calculate the speed of decoherence, while today we know how to do it, and the results of the calculation match experiment. This is definitely great progress in understanding the nature of quantum mechanics and how it generates a classical-looking universe.

   On the other hand, it is not clear that this is a satisfactory resolution of the philosophical questions on the nature of quantum mechanics. Certainly, not everybody is satisfied.

4. **Peter Shor**
   June 22, 2017

   Correction to my previous comment: the article says that 1996 was when the first experiment comparing theoretical calculations of decoherence rate with experiment was done. The theory was developed over the previous decade or so. (I should have known this; I was paying attention during part of this time.)

5. **Jeff M**
   June 22, 2017

   Peter Shor,

   I'm not sure how experiments checking decoherence are even possible. Experiments of necessity involve conscious observers. Decoherence is supposed to tell you what happens in the absence of observers.

6. **tulpoeid**
   June 22, 2017

   Jeff M,
   Maybe this is why the people who achieved it won the Nobel prize 🙂

   (Whose tremendous importance was partly lost on the general audience, because it was the same year with the Higgs announcement and it's more fun to whine about the latter not getting it prematurely than explaining quantum fundamentals. But I digress.)

7. **Peter Shor**
   June 22, 2017

Jeff M:

Did you read Philip Ball's article? I quote:

> We have, at long last, a theory of measurement. What's more, it is a theory that confers no privileged status on the conscious observer, stripping away the seemingly mystical veneer from quantum mechanics.

In its simplest form, decoherence is the decay of the diagonal elements of the density matrix in the *pointer basis* (called *pointer states* in Ball's article). This is a phenomenon we believe happens with or without conscious observers (although I assume it's only been measured in their presence).

8. **Marko**
June 23, 2017

Decoherence theory is great, but do note that the consensus of the majority of the experts in the field (i.e. everyone I ever asked) is that decoherence does *not* solve the measurement problem. The reason is that QM does not give any rule to determine the pointer basis, and specifying it ad hoc in any way is actually equivalent in power to the collapse postulate itself, which we set out to explain by decoherence.

HTH, 🙂
Marko

9. **Another Anon**
June 23, 2017

Put simply, Marko, I think of decoherence as an explanation of how all the alternative quantum realities can disappear (why we could never see Schrödinger's cat alive and dead at the same time -the atoms in the cat quickly decohere) but it does not explain which of those realities we actually see, as you say – the pointer basis.

But I think just the fact that it explains how the alternate realities can disappear is damaging for the many-worlds multiverse.

10. **Low Math, Meekly Interacting**
June 23, 2017

For me, this is the kicker: "Decoherence doesn't completely neutralise (sic) the puzzle of quantum mechanics. Most importantly, although it shows how the probabilities inherent in the quantum wave function get pared down to classical-like particulars, it does not explain the issue of uniqueness: why, out of the possible outcomes of a measurement that survive decoherence, we see only one of them."

I suppose it's nice that one doesn't have to explain away an infinity of alternate realities. However, and it could very well be I've misunderstood something, it seems to me that if whatever process you invoke (Darwinian or otherwise) doesn't select a unique reality that we conscious minds perceive (and always agree on), it's not clear that progress has really been made. To my untrained eye the advantage of paring things down appears to be more psychological than anything else. Instrumentalism still seems to rule as the only truly honest way to cope.

11. **Douglas Natelson**
June 23, 2017

I think it's worth pointing out that indirect measurements of (statistically averaged) decoherence in many-body systems predate 1996 in the form of "mesoscopic" physics in metals and semiconductors. There are quantum interference corrections to electronic conduction in solids (weak localization and universal conductance fluctuations, to name two). A classic early review of weak localization is this one from 1984, and this is a broader review from 1992.

In a Feynman-Hibbs heuristic argument, in the absence of decoherence, an electron propagating from one location to another through a solid takes all possible paths. Along each of these paths, the electron wavefunction accumulates a phase proportional to the product of its wavevector (related to its kinetic energy) and the propagation distance, and with an additional "Aharonov-Bohm" phase proportional to the electronic charge and the line integral of the vector potential along the path. Scattering off static disorder along a particular trajectory can introduce additional phase shifts. The final transmission probability of the electron from initial to final location involves summing the complex amplitudes for each of these trajectories and then finding the magnitude-squared of the total amplitude. Changing a magnetic field alters the relative phases of the different trajectories, altering the probability of electron transmission and hence the electrical conduction.

With decoherence, you don't have to worry about interfering contributions from trajectories that take longer than the decoherence timescale. That ends up setting a characteristic magnetic field scale for conductance changes that can be determined experimentally. TL/DR version: By measuring the conductance (or equivalently resistance) of a metal or doped semiconductor as a function of an externally applied magnetic field, it is possible to infer the underlying decoherence timescale associated with the motion of the charge carriers. The dominant causes of decoherence for these free charge carriers are inelastic scattering processes involving lattice vibrations (at high temperatures) and electron-electron interactions (at low temperatures, say below 4.2 K) as well as spin-based (magnetic) scattering processes (also at low temperatures). The typical coherence timescale for electrons in such a system around 10 fs near room temperature, but can easily be nanoseconds or longer at cryogenic temperatures.

12. **Jan Reimers**
June 23, 2017

Marko, Peter Shor and Another Anon,

It is usually at this point that Peter accuses us being un-informed and repeating tired old arguments, but I will go out on a limb and participate anyway

Regarding the pointer basis, Nima comments on this at 55:40 in the video below. Start a 40:00 if want the full context.

https://www.youtube.com/watch?v=3bqvAIKH2Rg

The short answer as I understand it is that the pointer basis is selected by the Hamiltonian that describes the interaction between the "system" and the environment. The interactions are local, so in his example local (position) states are selected as the pointer basis rather than super-positions of position states. I don't how to extend this argument to alive and dead cats though!

It seems there are three things we need to explain

1) Selection of a pointer basis
2) Diagonalization of the reduced density matrix for the "system" **in the pointer basis** (i.e. no macroscopic entanglement)
3) Collapse of the diagonal density matrix elements to only one element as a result of a measurement.

Decoherence explains 2) and the interaction Hamiltonian explains 1). Is there a way that interaction with the environment can also explain 3)?

13. **Peter Shor**
June 23, 2017

Wow!

The comments here make me suspect that there are more people than I ever would have believed who are deeply invested in the mystical idea that consciousness has something to do with quantum mechanical dynamics.

This probably says something about human nature.

14. **Marko**
June 23, 2017

Jan Reimers,

Nima is repeating the usual locality argument for the selection of the pointer basis, which everyone talks about, but noone is able to implement effectively. So at this point it's just wishful thinking. Moreover, the locality argument can exist only on a classical spacetime manifold, and fails in a full quantum gravity setup, since the notion of locality cannot be defined on superpositions of manifolds.

So that argument doesn't really work, and cannot work, as a criterion for the pointer basis.

HTH, 🙂
Marko

15. **Mateus Araújo**
June 24, 2017

Jan Reimers and Marko,

What you say is not totally incompatible with Ball's article (which I quite liked, by the way). He explicitly says that decoherence doesn't solve the multiplicity problem (Reimers point 3), which is why most experts say that decoherence doesn't solve the measurement problem, as claimed by Marko.

Marko's reason for that is false, however. The consensus is that decoherence *does* solve the preferred basis problem, precisely as Reimers report. If you really think that no one can effectively derive the pointer basis from the interaction Hamiltonian, you had a lot of comments to write on the decoherence papers.

As for the quantum gravity argument against decoherence, come on. Using a non-existent theory to argue against an existent theory is just bizarre.

16. **Martin S.**
June 24, 2017

Regarding decoherence, the respective part of FAQs is helpful.
And regarding P.B.'s words (in the article linked in the P.W.'s post) against "spooky" stuff, does it mean that P.B. (and P.W.) disagree with Bell inequalities? AFAIK Bell inequalities hold regardless of decoherence being/not-being part of measurement.

Regarding words on Higgs boson mass in the D.O.'s text (linked in the P.W.'s post, seek the second occurrence of the word "quadrillion" there), do not they mix the originally expected Higgs mass (several times greater than lately found) with the absurd value of theoretically approached CC value?

17. **Mateus Araújo**
June 24, 2017

Martin S.:

I'm sure Philip Ball is referring to the nonlocal collapse of the wavefunction, this is what decoherence theory eliminates (in favour of a local effective collapse).

The violation of Bell inequalities is extremely well-stablished experimentally, you cannot get rid of them by playing with the theory.

18. **Martin S.**
June 24, 2017

@Mateus Araújo:
"*The violation of Bell inequalities is extremely well-stablished experimentally, you cannot get rid of them by playing with the theory*." That's what I meant; I was way too sloppy there.
"*I'm sure Philip Ball is referring to the nonlocal collapse of the wavefunction, this is what decoherence theory eliminates (in favour of a local effective collapse)*." Thanks, then according to my knowledge there must be some another nonlocal process, e.g. that choosing of single diagonal element in density matrix should be nonlocal, or we need (absolutely) nonlocal hidden variables. And that another nonlocality is the same "spooky" as nonlocality of any other process, I would say.

19. **vmarko**
June 24, 2017

Mateus Araújo,

"As for the quantum gravity argument against decoherence, come on. Using a non-existent theory to argue against an existent theory is just bizarre."

No, it's not bizarre. You cannot claim that you can resolve a fundamental problem (the pointer basis) by appealing to a semiclassical (i.e. approximate) theory, which completely ignores one very fundamental interaction in nature (gravity). At the very least, there is a potential danger that your solution may be an artifact of the approximation rather than a genuine property of a (so far unknown) complete theory.

And this is actually what happens — the proposed solution of the pointer basis problem rests on the notion of locality, which in turn relies on the notion of classical spacetime manifold, which may or may not exist in a full theory of quantum gravity. So this is the weakest point of the whole argument, and ignoring that point is what's bizarre.

Even Zurek, the father of decoherence, was aware from the very beginning that gravity may introduce trouble — in his foundational paper [1], on page 1520 he wrote: (the assumption of pairwise interactions) “is customary and clear, even though it may prevent one from even an approximate treatment of the gravitational interaction beyond its Newtonian pairwise form.” In other words, he made a disclaimer that the whole decoherence programme may fail in the presence of a tripartite interaction term in the Hamiltonian, which is precisely what relativistic gravity has.

And finally, one doesn’t need a full theory of quantum gravity to make that point. It is quite enough to know that ordinary theory relies on spacetime being classical — and in almost all QG models this assumption is violated, resurrecting the measurement problem back to its full glory.

Btw, it’s not that experts in the field aren’t aware of this. They are, but they don’t know what to do about it, and there is a lack of good ideas how to deal with such a problem (in the absence of a full QG theory). That is one of the reasons why they generally agree that decoherence doesn’t really solve the measurement problem. At least the ones I’ve talked to. 😉

HTH, 🙂
Marko

[1] W. H. Zurek, Phys. Rev. D 24, 1516 (1981).

20. **Mateus Araújo**
June 24, 2017

Marko, you are quoting a paper from 1981 to talk about the theory in 2017. This is 36 years ago. Decoherence changed from being a new idea to being a well-established theory. This inverts the roles of decoherence that has to adapt itself to quantum gravity to succeed, it is quantum gravity which must allow a decoherence-like mechanism to be taken seriously. Are you forgetting the experiments that need decoherence to be explained, including the one that got Haroche his Nobel prize?

And you don’t “ressuct the measurement problem” as soon as you drop the assumption that spacetime is classical. We knoe that classical spacetime is a very good approximation at any energy scale we can actually experiment with. If this works at all like nornal science, relaxing this assumption will make an \epsilon correction to the theory.

You have clearly been talking to different experts than me. And, as someone who works in quantum foundations, I do know a lot of experts.

21. **Mateus Araújo**
June 24, 2017

Martin S.

We don’t need a “nonlocal process”, what we need is to be able to reproduce the observed violation of Bell inequalities. And a purely quantum description of the experiment is perfectly local, the nonlocalities only appear when you start collapsing stuff.

22. **vmarko**
June 24, 2017

Mateus,

“it is quantum gravity which must allow a decoherence-like mechanism to be taken seriously”

Sure, this is a valid point of view, and may serve as a criterion for a plausible QG model. The general drought of experimental results in QG makes all such criteria fair game. But suppose sometime in the (far far) future, we start making experiments in QG, and find out that it indeed badly violates the notion of locality. That would certainly invalidate decoherence as a solution of the measurement problem, wouldn’t it? Regardless of how unlikely that may be, it certainly is a logical possibility. So it’s fair to say that the locality-based solution of the pointer basis problem is contingent on certain properties of QG, which is so far unknown, and outside of experimental realm.

“We knoe that classical spacetime is a very good approximation at any energy scale we can actually experiment with. If this works at all like nornal science, relaxing this assumption will make an \epsilon correction to the theory.”

Well, no. All experiments we can do are in our Solar system, which so far doesn’t feature any strong superpositions of the gravitational fields. That’s why classical spacetime is a good approximation. However, one can certainly imagine scenarios (typically involving a photon, a beam splitter and a black hole) where one should discuss strong superpositions of very different geometries, which behave nothing like an \epsilon correction to a classical geometry. Theory should be able to account for these situations too.

There was an analogous mishap with the assumed universal validity of the law of energy conservation (which was overwhelmingly supported by experiments), until general relativity taught us that such a law is valid only under certain circumstances, which are satisfied in our Solar system and the Milky Way, but not in general. So one has to be very careful with the reasoning you proposed above.

“You have clearly been talking to different experts than me. And, as someone who works in quantum foundations, I do know a lot of experts.”

This may be getting slightly off-topic, but just today I came back from visiting your PhD advisor in Vienna. 🙂 // I also attended the ongoing ESI workshop [2] there… // Granted, I didn’t talk to him about this particular issue (didn’t come up in the conversations, we had other stuff to cover), but I’ll make sure to do so when I visit his group again in October. So would you agree to postpone this “argument from authority” until then? 😉

Best, 🙂
Marko

[2] http://quark.itp.tuwien.ac.at/~grumil/ESI2017/

23. **Martin S.**
June 24, 2017

@Mateus Araújo: Trying to be less sloppy, I mean experiments where apparatuses are spacelike separated, their outputs are real, we have choices to arrange those apparatuses locally (yeah, I had read those free-will-theorem articles), and “nonlocal” is considered as “effectively nonlocal”, admitting e.g.: speeds without limits and/or going backwards in time (sort-of the transactional interpretation), some spatial bridges in other dimensions, or whatever.
Then while decoherence is important, it leaves the *uniqueness* (as stated in the article linked in the blog post), that is point 3 at Jan Reimers’s comment, and the upper part of the FAQ part. When one is honest, the effective nonlocality occurs somewhere (to have such an experiment done), and here it is apparently pushed into that *uniqueness* part.

Thus I am unhappy about that article, since it says that there is nothing “spooky” (that is effectively nonlocal), while sweeping it under the rug, that is into the *uniqueness*. May be that I read too much into it though.

24. **Peter Shor**
June 24, 2017

@Marko:

I don’t understand your argument against decoherence and localization at all.

If you believe that the Standard Model is a very good approximation to string theory under most realistic conditions, please explain why a theory of decoherence that holds for the Standard Model wouldn’t also apply to string theory.

And if you don’t believe that the Standard Model is a very good approximation to string theory under most realistic conditions, then you’ve got even more explaining to do.

25. **Erika**
June 24, 2017

Off-topic: Peter, can you mention this in some future “quick items” post?

Besides Peter Scholze, who do you think will win the Fields Medal in 2018 (choose up to three names)? Vote here: https://vote.pollcode.com/31229277

26. **Mateus Araújo**
June 25, 2017

Martin S.,

I think you do have a point. While decoherence does give you a physical (and therefore local) explanation for the destruction of the off-diagonal elements, to get a unique result you need again some sort of collapse, which is by necessity nonlocal. And one does not even need Bell inequalities to see that, the good old EPR paradox is good enough. Consider as usual that Alice and Bob share a singlet |01> – |10>, and that Alice makes a measurement in a basis |A_0>,|A_1>. Then we rewrite the state in this basis to turn it into |A_0 A_1> – |A_1 A_0>, and the measurement (through decoherence) maps it into |A_0 A_1><A_1|, dependent on a distant change of basis.

So decoherence only gives you a way to locally kill off the off-diagonal elements; it doesn't give you a way to locally select a single outcome. Still, it is an improvement over the collapse postulate, according to which even the disappearance of the off-diagonal elements is nonlocal.

But what I was talking about is that if you stick to the quantum mechanical formalism, you'll have a completely local theory, as any physical Lagrangian is Lorentz covariant. You only get nonlocality when you do an ad-hoc modification of the quantum formalism, such as selecting unique outcomes.

Maybe you'll be interested in this paper by Brown and Timpson, which goes on precisely about how you get EPR and Bell correlations locally if you stick to the quantum formalism: https://arxiv.org/abs/1501.03521

27. **Mateus Araújo**
June 25, 2017

Marko,

I think I’ve understood why we’re failing to communicate. You expect decoherence to *always* work, even in exotic quantum gravity regimes where we’re in a superposition between being inside a black hole and outside. Whereas I’m claiming that quantum gravity must allow decoherence to work in the experimental regimes we can access today on Earth.

“That would certainly invalidate decoherence as a solution of the measurement problem, wouldn’t it?”

No, it wouldn’t. While it would be certainly be shocking if we found out that decoherence actually fails in such exotic regimes, it wouldn’t make me shed a single tear: its job is to explain the measurements we’re actually doing, and that it does with unparalleled success (its main rivals being burying your head in the sand or radically modifying quantum theory).

Getting wildly off-topic: I did not meant “talking to different experts” as an insult. I just meant that you have been probably talking to quantum gravity people, whereas I’ve been talking to quantum foundations people. Anyway, I think I’m more familiar than you with the opinion of my former PhD supervisor =). But we could just ask him, no?

28. **Martin S.**
June 25, 2017

@Mateus Araújo: regarding 1501.03521;
1) Bell’s opinions: science is not about caring of opinions; e.g. QM is quite successful regardless some dislikes by one of its early fathers, namely A. E.;
2) then that article is about the Everett interpretation: and it requires the uniqueness, that is the part that hides nonlocality inside itself;
3) an attempt to come to uniqueness without nonlocality (within E. I.) is by later (local) comparisons of results; but here (non-aligned-spins experiments) you need to get rid off some result combinations so that proper correlations are gained, and such a pruning means that some branches get killed; such a branch killing is first, strange by itself, second, you either have to kill such branches in their entirety (that is non-locally), or you need to kill anyone who tries to test it (making holes in the branches by that). then we can do an amount of such experiments together with the comparisons, and after some amount of such tests we reach a branch that either has to get killed entirely, that is together with us (in the former case), or we get killed when we try to read the results (in the latter case);
4) less naughty way of testing the outputs is via the transactional interpretation where the future is probed before actually taking one real output, that is without a need of killing us later. not arguing for it though, just mentioning it as a less terrible alternative;

PS Am not willing to argue about E. I. any more, since (according to my understanding) it solves nothing (as I described in the points above).

29. **Pascal**
June 25, 2017

I remember seeing an article on arxiv explaining why the initial condition of the universe must be taken into account to explain why the universe is classical-looking (so in particular, decoherence theory by itself would not be a good enough explanation).

The argument in a nutshell: if we start from a random, highly nonclassical state of the universe, and apply any unitary transformation (representing the evolution of the universe since the origin) then we obtain another random, highly nonclassical state.

I find this argument quite compelling but I'd be curious to know the opinion of people with a deeper understanding of physics than me.

30. **Martin S.**
June 25, 2017

@Pascal: Will not help you, especially when not having link to the text (and probably neither even with a link). If it is something like/this, I would suggest to ask Bee.

31. **Mateus Araújo**
June 25, 2017

Martin S.,

I'm afraid you have misunderstood the article. It is about how you get Bell correlations in a local way if you *don't* have uniqueness. This is what the Everett interpretation is about, not modifying quantum mechanics and trying to interpret the resulting weirdness. There is no killing involved, either of branches or experimentalists.

32. **Peter Shor**
June 25, 2017

Mateus Araújo:

… or cats.

33. **Hannes**
June 26, 2017

Peter,

my friend Chris just showed me this:
http://www.lhc-epistemologie.uni-wuppertal.de/
It is a well-funded physics-philosophy research program on the epistemology of the LHC. I really do not know what to think about it. But it exists.

34. **Anonyrat**
June 27, 2017

One of the projects of that physics-philosophy program:

Principal Investigators:
Robert Harlander (RWTH Aachen)
Adrian Wüthrich (TU Berlin)
Principal Collaborator:
Friedrich Steinle (TU Berlin)
Postdoctoral Researcher:
Daniel Mitchell (RWTH Aachen)
Doctoral Researcher:
Markus Ehberger (TU Berlin)
The sub-project A1 investigates the concept of a virtual particle from a historical point of view. This concept is an integral part of modern physics, mainly since it provides a means to interpret and describe complex quantum physical processes in comparatively simple terms. Despite of its wide use, the notion of a virtual particle appears not to be clearly defined. One of the main goals of the project is therefore to trace the origins of the concept and to follow its evolution through time. Since it is of a fundamental quantum physical nature, it is quite clear that the roots of the virtual particle concept are tied to the beginnings of quantum mechanics and quantum field theory. However, the reasons for its introduction, its further development in the context of quantum field theory, S-matrix theory, and Feynman's diagrammatic method have never been the focus of historical research. By taking up these issues, the present project aims to explain the genesis of today's terminological diversity, to bring to the fore the reasons for the apparent inconsistencies, and to further our understanding of concept formation in the physical sciences.

—
The goals are somewhat unclear to me, but if the result is to bring some precision to the various uses of "virtual particle" in the physics literature, that would be a good thing. If new experimental results are in short supply, then spending effort to elucidate the foundations of physics is worthwhile, in my opinion.

35. **Mitchell Porter**
June 27, 2017

Peter Woit said

"There's a lot of quite good information in the story about the actual classical calculation involved, but no indication of why one might want to be skeptical about the effort to enlist this result in the string vs. loop war."

Well, it's obvious why they make that connection. Vafa argues that the naked singularity goes away, exactly when you include charged matter obeying the weak gravity conjecture. String theory appears to obey the weak gravity conjecture and in some cases one can even say why. Loop quantum gravity appears to not obey the weak gravity conjecture because electromagnetic couplings are simply free parameters in that theory, there's no known mechanism that can stop you from making them weaker than the conjecture allows.

In my opinion, the main reason not to bring strings vs loops into this, is that loop quantum gravity already has so many other problems: chiral fermions, logarithmic corrections to black hole entropy, the peculiarities of polymer quantization, the very existence of a classical limit. If an alternative to string theory is desired, one could reasonably ask why we are getting the loop perspective, rather than e.g. the asymptotic safety perspective, which at least has successful empirical and mathematical predictions to its credit.

36. **An Interpreter**
June 27, 2017

Decoherence does not imply the disappearance of superposition. It just implies the disappearance of the detectability of superposition, which depends on an interference _pattern_. This is easy to see. Two points from which emanate a wave will result in maxima and minima at a screen. The interference is detectable. Add a few more points at random locations and this sign of interference disappears. The interference is still there, it is just no longer detectable by experiments in which the results of repetitions demonstrate the pattern. The pattern, from which we infer interference is gone. The interference is still there. The superposition is still there. You still need a collapse.

37. **Abby yorker**
June 28, 2017

It is easy to visualize the replication with photons. I see it as a representation of the system by successive groups of impinging photons. However, what if the pointer apparatus uses a static electromagnetic field. I am thinking of the stern gerlach apparatus. I assume photons are used somewhere, but what is the mechanism? And what codes the replications?

38. **Frank Wilhoit**
June 28, 2017

Kane's quoted comment about "realistic" searches is outrageous and should be completely discreditive.

39. **ppnl**
June 29, 2017

Why would we expect or even want a solution to the measurement problem to solve the uniqueness problem? Decoherence means that anything big, complex and connected enough to be an observer will necessarily see an approximately classical universe. What more do you want? Sure, you can't predict which classical universe you will see. But so what? In QM some things aren't predictable.

It's like a quantum version of the anthropic principle. If you exist then the universe around you must look approximately classical.

40. **Mateus Araújo**
June 29, 2017

ppnl,

The uniqueness problem is about conciliating the multiple outcomes that appear in the equations with the unique outcomes we experience. It's not about predicting the outcome: I would be perfectly happy with a random outcome that came out of the quantum dynamics. This is pretty much what is done by collapse models, and they do have a satisfactory answer to the uniqueness problem. The problem with collapse models is that they don't really match reality.

41. **ppnl**
June 29, 2017

I still don't get it. Maybe I'm just not smart enough to understand the problem.

In collapse models you have an observer (what counts as an observer?) that makes a measurement (what exactly is a measurement?) and that reduces the state of the object.

With a decoherence you have an observer (any complex connected object will do) that makes a measurement (any interaction will do even if the observer is not aware of it.) and it gets constant state reduction behind its back so to speak.

Both have multiple outcomes that appear in the equations and in both cases one is chosen. There is nothing to reconcile. What more do you want? What would the answer you seek even look like?

42. **vmarko**
June 29, 2017

So I just came back from the "Gravitational decoherence" workshop in Bad Honnef, and one of the most interesting points of the final discussion session was the lack of consensus among people regarding the unitarity of QM.

There were two main points of view, distributed roughly half-and-half among the people present there. The first half maintains that nature is obviously unitary, since all existing measurements may always be reinterpreted as unitary evolution plus the decoherence with the environment.

The second half maintains that experiments give us obviously nonunitary results, and that imposing unitarity to QM and jumping through various hoops to make all evolution unitary lacks justification (and represents an ideology).

This was basically a divide between pure QM folks (favoring formalism) and the objective collapse folks (favoring instrumentalism). The initial question (raised by me 😉) was about the experimental status and possible tests of unitarity in nature. Instead of answering the question, people got split in two groups and started arguing, based on whether they interpret experimental results as saying that nature is not unitary or as saying that decoherence is too strong to avoid.

The consensus, however, was that in the end this question is extremely hard (if at all possible) to resolve experimentally, i.e. the assumption of unitarity is apparently not falsifiable. So it doesn't really belong to physics, but to philosophy. So there you go — collapse or decoherence, take your pick, it's a metaphysical point of view. 😉

Best, 🙂
Marko

Last Week's Hype

July 6, 2017
Categories: This Week's Hype

Now back from vacation, more regular blogging should resume imminently. While away, lots of press stories about claims that LIGO could be used to get "evidence for string theory". As usual, these things can be traced back to misleading statements in a paper and the associated university press release. In this case, there had already been an initial round of hype, debunked by Sabine Hossenfelder. The new round seems to have been generated by the June 28 press release. The Guardian has a version of this, but at least there the author found someone to make the obvious point, that this is irrelevant to string theory.

Comments

1. **Jeff M**
   July 6, 2017

   I'm curious, why wouldn't the authors account for stabilization of extra dimensions? It's required, right? So an argument ignoring it is prima facie incorrect.

2. **Peter Woit**
   July 6, 2017

   Jeff M,
   For the technicalities of the actual result of the authors, better to raise the issue at Backreaction, where Sabine Hossenfelder has some expertise. What I'd like to understand is why they think it's a good idea to put out a press release invoking string theory and leading to the usual nonsense in the press.

3. **Jeff M**
   July 6, 2017

   Peter

   Will do. I wonder if the authors have anything to do with the press release.

4. **srp**
   July 17, 2017

   At least one author of the paper in question is claiming no real link to string theory, from the comments at Backreaction:

   "As the second author of the paper in question I fully agree with Olivier's comment (first comment here above), particularly with his first point. Moreover, I believe the whole point of vue of your post is a little forced. Talking about falsifying string theory on the basis of our analysis is quite a stretch, to say the least, and I don't think this viewpoint conveys the message very well or fits with the study at all. While ruling out string theory is a legitimate question to ask, I do not think one can answer it based on our simple study, which focuses on the propagation (ignoring sources and emission) and furthermore is not directly related to string theory but more genetically to extra dimensions."

5. **Peter Woit**
   July 17, 2017

   srp,
   I saw that too. A good question for this author is why he thought it was a good idea to have his university put out a press release about his paper mentioning string theory (by the way, this was done AFTER there had already been misleading press stories about string theory and this paper).

6. **srp**
   July 18, 2017

   The PR equivalent of "I just send them up and where they come down, that's not my department said Werner von Braun?"

7. **CIP**
   July 19, 2017

   Today's NYT has a story on an analog of the mixed axial-gravitational anomaly that invokes string theory:

   The experiment is also a success for string theory, a branch of esoteric mathematics that physicists have used to try to tie gravity into the Standard Model, the laws of physics that describe the other forces in the universe. But string theory has been maligned because it makes predictions that cannot be tested.

   Here, Dr. Landsteiner said, string theory was used to calculate the expected anomaly. "It puts string theory onto a firm basis as a tool for doing physics, real physics," he said. "It seems incredible even to me that all this works, falls all together and can be converted into something so down to earth as an electric current."

   Any comment?

8. **Peter Woit**
   July 19, 2017

   CIP,
   Thanks, hadn't seen that, one of the worst examples of This Week's Hype yet. Depressing that it's in the New York Times.

Various Links

July 6, 2017
Categories: Uncategorized

Some links to things that may be of interest:

- There's an excellent article at FiveThirtyEight about the issue of publicizing math research, taking as example the Atlas of Lie Groups and Representations project (which will soon be having a workshop). This kind of thing generally gets no public attention, while at the same time, one of the results of this research arguably got too much public attention (see here).
- There's a new \$1 million mathematics prize that will be awarded for the first time this fall, together with a $1 million physics prize that was awarded for the first time last year. This is called the Future Science Prize, and to get it you need to be working in China. Used to be a \$1 million prize was a big deal, now with the \$3 million Breakthrough Prizes, a mere million looks like small potatoes.
- Another way you could get a measly \$1 million would be to prove (or disprove) the Hodge conjecture. For some inspiration, see Burt Totaro's new survey of progress on the Tate conjecture (blog entry here).
- 4 gravitons has a nice posting about work by Turok and others about complexified path integrals and cosmology. The issue of the relation between Euclidean and Minkowski signature QFT is one that I think has gotten far too little attention over the years. Now that I've finished writing a book with a QFT discussion that sticks to Minkowski space, I'm hoping to work on writing something about the relation to Euclidean space.
- There's an interview with Nima Arkani-Hamed here. His talk at the recent PASCOS 2017 conference (real title is second slide "What the Hell is Going On?") gives his take on the current state of HEP, post failure of the LHC to find SUSY. He's sticking with his 2004 "Split SUSY" as his "Best Bet". I'd like to think his inspirational ending claiming that the negative LHC results are forcing people to rethink the foundations of the subject, asking again the question "What is QFT?" reflects reality, but not sure I see much of that.
- This year's LHC startup has been going well, with a new luminosity record already set, and 6 inverse fb of data already collected. For more, see here.
- Remember that "dark flow" that was supposed to be in the CMB data and evidence for the multiverse (see here)? Still not there, according to Planck (via Will Kinney).

Update: I'm sorry to hear the news of the untimely death of Maryam Mirzakhani, who was the first woman to win a Fields medal, awarded at the last ICM in 2014. Her work was described in detail at the time in this article by Curt McMullen.

Comments

1. **Jackson Clarke**
   July 7, 2017

   On rethinking the foundations of naturalness and SUSY as a potential solution, I don't think physicists need look further than the first slide of most SUSY talks, where the hierarchy problem (or the naturalness problem for the Higgs mass) is almost always described in terms of a top quark divergence and a cutoff scale. As a result, a good proportion of the field believe that the top quark itself is the problem, and the level of the naturalness problem is set by the cutoff scale alone. But this is not the case at all. In the standard model, the top quark divergence is an unphysical quantity related to the cutoff regularisation procedure, and (when new physics is added) the level of any naturalness problem is necessarily proportional to the coupling strength of that new physics (not just the scale of it). This is manifest when you only consider the renormalised parameters and not bare quantities. The latter point also seems to generalise to gravity: the lesson to learn is that scales in a theory can easily have small (and technically natural) dimensionless parameters hiding inside them.

   You (and others) might be interested in a recent blog post of mine where all this is spelled out in a way I have not seen elsewhere, and I think the field would benefit from considering. There is a sloppiness in the naturalness literature that needs addressing.

2. **Erika**
   July 7, 2017

   The final round of voting for the Fields Medal 2018.

   First round results, second round results.

3. **paddy**
   July 7, 2017

   Typo: "…with new a new luminosity record…"

4. **Code Ferret**
   July 7, 2017

   FWIW, NEW appears to have crashed http://www.liegroups.org. The github site is of course humming along. Too much publicity…

5. **Anonyrat**
   July 8, 2017

   ^^^ Above mentioned Jackson Clarke blog post:
   http://syymmetries.blogspot.in/2017/06/naturalness-pragmatists-guide.html

6. **Georg**
   July 8, 2017

   "post failure of the LHC to find SUSY."

   "Susy failed to show up on/in LHC" is closer to facts.

   Georg

7. **neil**
   July 9, 2017

   My fave LHCb continues to find interesting stuff. Now a doubly heavy-quark (charm) baryon in the 13 TeV data. Nothing BSM there, but it could be useful for probing the strong interaction. Still more to discover.

http://lhcb-public.web.cern.ch/lhcb-public/Welcome.html#Xicc

8. **David Roberts**
   July 15, 2017

   OT, but Iranian media is reporting that Maryam Mirzakhani has passed away.

9. **Mozibur Ullah**
   July 15, 2017

   This is sad news about Maryam Mirzakhani, to die so young and so soon after being awarded the Fields Medal. Rest in peace.

10. **Armin N**
    July 15, 2017

    I am genuinely shocked by her untimely passing. Not just Iran and the US lost a jewel, but humanity as a whole. My condolences to her family.

11. **tulpoeid**
    July 16, 2017

    If I am permitted to get dramatic … she bypassed two of the three obstacles that have usually kept women from entering history; she navigated a patriarchal society and she managed to combine career with family. The third one though, a more vulnerable body, caught up.

12. **Shantanu**
    July 17, 2017

    Peter, any comments about T'hooft's interview in physics today
    avaiable at DOI:10.1063/PT.6.4.20170711a

13. **Peter Woit**
    July 17, 2017

    Shantanu,
    I didn't think the interview was particularly interesting, more interesting was the news, which I hadn't heard, that he has a book out about his ideas about QM, freely available at

    http://www.springer.com/us/book/9783319412849

    The kind of thing he's trying to do is not my cup of tea (my own book about QM should make clear what point of view I'm more interested in) but others may find this more worth looking into.

14. **Low Math, Meekly Interacting**
    July 17, 2017

    I'd read about Maryam Mirzakhani with great interest (probably first in Erica Klarreich's article in Quanta). Not sure why, but news of her death made me especially sad for a total stranger whose work I don't understand. Maybe it's because my daughter was recently most tearfully upset because a boy called her stupid. Too few examples like Ms. Mirzakhani, and to lose her so young is simply tragic. I hope her legacy continues to inspire those most in need of a role model.

This Week’s Hype

July 19, 2017
Categories: This Week's Hype

Commenter CIP pointed out that today’s New York Times has one of the worst examples of string theory hype I’ve seen in a while. Based on this observation of an expected QFT anomaly effect in a condensed matter system, the NYT has an article An Experiment in Zurich Brings Us Nearer to a Black Hole’s Mysteries. Not only is the headline nonsense, but the article ends with

> The experiment is also a success for string theory, a branch of esoteric mathematics that physicists have used to try to tie gravity into the Standard Model, the laws of physics that describe the other forces in the universe. But string theory has been maligned because it makes predictions that cannot be tested.
>
> Here, Dr. Landsteiner said, string theory was used to calculate the expected anomaly. “It puts string theory onto a firm basis as a tool for doing physics, real physics,” he said. “It seems incredible even to me that all this works, falls all together and can be converted into something so down to earth as an electric current.”

There’s no connection at all to string theory here. The NYT seems to have been taken in by string theorist Landsteiner and press release hype like this, not noticing that the paper had no mention of string theory in it. The hype is timed to the paper’s publication in Nature, where the editor’s summary gets it right, referring to QFT not string theory:

> Johannes Gooth et al. now provide another intriguing connection to quantum field theory. They show that a condensed-matter analogue of curved space time can add an additional, gravitational component to the chiral anomaly in Weyl semimetals. The work opens the door to further experimental exploration of previously undetected quantum field effects.

Someone really should contact the NYT and get them to issue a correction. In particular, any string theorists who care about the credibility of their field should be doing this.

Update: For a couple more stories about this, IEEE Spectrum has Black Hole Power: How String Theory Idea Could Lead to New Thermal-Energy Harvesting Tech, Nature has Big Bang gravitational effect observed in lab crystal.

Update: The author of the NYT piece did make some changes in the last two paragraphs to make things less misleading.

Update: This has finally appeared in print today, in an abbreviated version, minus among other things the string theory business.

Comments

1. **Mitchell Porter**
   July 20, 2017

   String theory aside, is this, or is this not, an observation of an effect attributable to a gravitational anomaly?

   There’s a type of “condensed-matter hype” in which observation of monopoles, Hawking radiation, etc, is claimed, when of course the reality is just that something *analogous* to the claimed phenomenon was observed.

   What I get from skimming the theory papers is that an effective field theory for hydrodynamics in curved space-time can have a term whose coefficient is determined by the existence of the anomaly. Then I see remarks that this can still be true in flat space-time too. And finally, I see statements that in this experimental work, there’s a temperature gradient which is analogous to space-time curvature in its effects.

   So what’s going on? Is this another example of condensed-matter hype, that should be rectified by giving things their true names?

2. **Shantanu**
   July 20, 2017

   Dear readers,
   I suggest you try to write an email to the writer of this article pointing them this faux pas. On many times when Peter or others posted such hype articles I tried to find the email address of the person who wrote the article and in many cases it was hard to find. But I am guessing its easier in this case.

3. **Giulio**
   July 20, 2017

   Another piece of fake news from NYT. SO SAD!
   D.

4. **Citchell**
   July 20, 2017

   The section titled “1 Connection between the mixed axial-gravitational anomaly and thermal transport” directly references work by Landsteiner, Witten and Alvarez-Gaume, and Eguchi. Also a guy who has worked on born reprocity.

   http://www.sciencedirect.com/science/article/pii/055032138490066X
   https://en.wikipedia.org/wiki/Eguchi%E2%80%93Hanson_space
   https://en.wikipedia.org/wiki/Born_reciprocity

5. **bpv**
   July 20, 2017

   I sent a comment to the author of the article, Kenneth Chang, and he replied almost immediately with a likely pre-written email that he must be sending to everyone complaining about the article (he refers to them as “those who are outraged”).

   He basically says that he is aware of the reasons why knowladgeable people are concerned, but for most laymen the mention of string theory was just fine.

   Let me translate: He can say whatever he wants to people who have no capacity to check the truthfulness of the statements.

6. **Ed**

July 20, 2017

OT: could topological physics be the field to finally provide further insights and discoveries to break out from the stagnated situation on particle physics?
https://www.nature.com/news/the-strange-topology-that-is-reshaping-physics-1.22316

7. **Thomas**
July 20, 2017

I think your outrageous hype detector is a little too sensitive. Yes, I would prefer if the author of the NYT article adds a statement along the lines of "this result does not have immediate implications for attempts to unify the SM with gravity", but overall the article is quite accurate. In particular, I see absolutely nothing wrong with the Landsteiner quote, or with the "This work opens the door .. " quote.

People have now discovered a number of transport phenomena in flat space that are most easily determined by putting QFT in curved space. This is more than mere analogy as some commenters here seem to think. It is using QFT in curved space to implement the constraints that follow from Ward identities, anomlies, etc. As a result certain transport coefficients that look like ordinary themopower or magnetoresistance in flat space are indeed fixed by gravitational anomalies. That's pretty cool, and it is remarkable that we can now engineer condensed matter systems in which these effects are seen.

It is also the case that some of these transport coefficients that could have been discovered by Landau and Lisfschitz by analyzing the symmetry constraints for currents in hydrodynamics were in fact by analyzing holographic setups. This does mean that AdS/CFT is an efficient method to think about difficult problems in QFT.

8. **Peter Woit**
July 20, 2017

Thomas,

The problem is that this has nothing at all to do with string theory. It's purely a QFT story (as the Nature editor recognized). Going to the press with a claim "string theory was used to calculate the expected anomaly" is an intentional attempt to mislead people about the science here.

9. **Thomas**
July 20, 2017

but what if it is historically accurate?

10. **Peter Woit**
July 20, 2017

Ed,
That's a very good article that actually explains some of the real new connections between math and physics being investigated. I don't think it has anything to do with HEP physics.

11. **Peter Woit**
July 20, 2017

Thomas,
It's not in any sense "historically accurate". The anomaly calculation in the graphic associated with the article never was a "string theory calculation". It's a QFT calculation, reflecting the fundamental relation between the Dirac operator coupled to a gauge field and the index theorem. This was a major topic during the early eighties when I was a grad student, before string theory came into the picture.

12. **Thomas**
July 20, 2017

The connection to transport in flat space was discovered this way.

13. **Peter Woit**
July 20, 2017

Thomas,
Even if there is some non-trivial connection to Maldacena's AdS/CFT conjecture (which I don't see, but I'm no expert) that doesn't turn a straightforward QFT calculation into a "string theory calculation".

14. **Thomas**
July 20, 2017

The anomalous transport calculation in QFT is not straightforward, and historically it was done using AdS/CFT.

15. **Peter Woit**
July 20, 2017

Thomas,
I don't think this is relevant to the fact that the claims about "string theory calculations" in the article are flat-out incorrect, but I'm surprised that when I tried looking through the references in the paper, I find nothing about AdS/CFT. The few that refer to "holography" seem to only be using standard QFT ideas about anomalies
Can you provide references that would justify the NYT graphic as a consequence of AdS/CFT? I've spent a little time looking at the references in the paper, nothing uses AdS/CFT, the few that refer to "holography" seem to have to do not with AdS/CFT but with standard old facts about anomalies and domain walls.

16. **Chris Herzog**
July 20, 2017

Perhaps they were limited in the number of references they could include for a Nature paper. Landsteiner cites his own review article [5],
https://arxiv.org/pdf/1610.04413.pdf. Section 6 seems to be relevant to the argument of how important AdS/CFT was in the computation of anomalous transport, as is this sentence from the conclusions: "While holography can probably not claim to have discovered anomalous transport it has certainly played a major role in gaining a better understanding."

As Thomas mentions, these anomalous transport calculations are not straightforward. Mistakes in the textbooks were first noticed by doing analogous calculations in AdS/CFT.

17. **Thomas**
July 20, 2017

I think a nice historical summary of the role of AdS/CFT in anomalous transport is the intro of Son & Surowka, https://arxiv.org/abs/0906.5044 . The NYT graphic presumably refers to something like https://arxiv.org/abs/1107.0368

18. **Peter Woit**
July 20, 2017

Chris Herzog and Thomas,
Thanks for the references. I had looked at the Landsteiner review article, noticed that he invokes AdS/CFT in that one section, but I still don't see AdS/CFT getting used to explain significant. My reading of the Son/Surowka article is that their topic is effects of anomalies on relativistic hydrodynamics, that they only use AdS/CFT as a test calculation to check consistency.

From your comments it's looking to me like the only relation to AdS/CFT is that researchers in this area did analogous calculations in AdS/CFT and this may have been a useful part of the process of sorting out what was going on, but that's very different than the claim that anomalous transport is based on AdS/CFT.

19. **william e emba**
July 20, 2017

Researchers in fluid analogues to black holes (sometimes called "dumb holes") freely describe just about everything they do using the language of general relativity. The very title of https://www.nature.com/nphys/journal/v10/n11/full/nphys3104.html refers to "black hole" and "Hawking radiation". They were studying a Bose-Einstein condensate in their lab.

20. **Peter Woit**
July 20, 2017

william e emba,
Yes, but the question isn't whether GR is being used in this kind of modeling, it's whether string theory is being used, as claimed in the NYT article.

21. **Andreas Karch**
July 20, 2017

Hey Peter,

my local condensed matter colleagues here are quite adamant that the effects seen in this and similar experiments should not be interpreted as being related to the mixed gravitational field theory anomaly, so I think there are some legitimate theoretical questions one can ask about this paper. But I also think you are really trying too hard to undermine the AdS/CFT connection.

These anomalous transport coefficients have first been calculated in AdS/CFT. The relevant references are [8], [9] and [10] in the Son/Surowka paper. In the AdS/CFT calculations these particular transport coefficients only arise due to Chern-Simons terms, which are the bulk manifestation of the field theory anomalies. At that point it was obvious to many of us that there should be a purely field theory based calculation, only using anomalies, that can derive these terms. Son and Surowka knew about this. They were sitting next door to me when they started these calculations. Many of us tried to find these purely field theory based arguments and failed. Son and Surowka succeeded.

If you ask anyone serious about applying AdS/CFT to strongly coupled field theories why they are doing this, they would (hopefully) give you an answer along the lines of "AdS/CFT provides us with toy models of strongly coupled dynamics. While the field theories that have classical AdS duals are rather special, we can still learn important qualitative insights and find new ways to think about strongly coupled field theories." Once AdS/CFT stumbles on a new phenomenon in these solvable toy models, we want to go back to see whether we can understand it without the crutch of having to rely on AdS/CFT. Any result that only applies in theories with holographic dual is somewhat limited in its applications. In this sense, anomalous transport is a poster child for what AdS/CFT can be used for: a new phenomenon that had been missed completely by people studying field theory gets uncovered by studying these toy models. Once we knew what to look for, a purely field theoretic argument was found that made the AdS/CFT derivation obsolete.

This is applied AdS/CFT as it should be. Solvable examples exhibit new connections which then can be proven to be correct more generally and are not limited to the toy models that were used to uncover them.

22. **william e emba**
July 20, 2017

So? Results from XYZ are lifted to hydrodynamics and touted as such even the work is obviously not XYZ, you say this is acceptable when XYZ=GR and obnoxious hype when XYZ=string theory? I don't buy that.

I see this type of idea credit in other fields. I'm perfectly happy with the terminology "simulated annealing" or "genetic algorithms" or "evolutionary programming" or "ant-colony optimization". There's no point in hiding the inspiration, especially if more progress is expected from applying the sources.

23. **Peter Woit**
July 20, 2017

Andreas,

Many thanks for your comment, which clarifies a lot, in particular making clear to me why I was having so much trouble seeing the relation to AdS/CFT in calculations that didn't seem to be using it. I gather that the answer is that, as Thomas suggested, the relation is a historical one, with AdS/CFT research what led to a current theoretical understanding that doesn't use AdS/CFT.

I think it should be clear that my problem with the NYT article has nothing to do with any complaints about this sort of research, but with its use to obfuscate the situation of string theory as a unified theory.

Here is a question for Andreas, Thomas and Chris, about the NYT article. What do you think of the last two paragraphs of the article that I quoted? Are they scientifically justifiable or seriously misleading/incorrect? I'm making the argument to the NYT that these two paragraphs should be deleted. What say you?

24. **Peter Woit**
July 20, 2017

william e. emba,

The point is that the status of GR as a tested theory is uncontroversial, but here the invocation of string theory is explicitly being made so as to obfuscate the failure of the heavily promoted string theory unification program. Instead of acknowledging this failure (which took the form that the idea turned out to be inherently untestable), claims are made (as in the next to last paragraph of the article) that string theory is testable, and has been successfully tested. The paragraphs I quoted are the result of an intentional not very honest campaign to mislead people.

25. **Chris Herzog**
July 20, 2017

Hi Peter.

I hesitate to make a judgment about the news article and to extrapolate from what is said there to what is actually believed by the scientists involved. So often, things are misquoted or stretched in misleading ways. I know Karl, and I suspect Andreas, Karl, and I are all pretty much on the same page when it comes to AdS/CFT and the role of string theory therein. I do not have as strong a reaction to those two paragraphs as you did, and I can see my vision, perhaps in a distorted form, behind what is written there.

I see AdS/CFT as a tool for doing certain types of field theory calculations, a tool like the epsilon expansion or like lattice gauge theory or like integrable systems. AdS/CFT is at its most rigorous when embedded into a string theory. The fact that one can calculate conductivities and viscosities in maximally supersymmetric Yang-Mills for me already puts AdS/CFT and by extension string theory on a solid footing, or at least on a footing worthy of a substantial research effort. That we have learned something about anomalous transport from AdS/CFT and that anomalous transport could be seen — not in holographic systems, but nevertheless seen — in the lab is icing on the cake.

26. **william e emba**
July 20, 2017

Fair point, although from my point of view, it's success in mathematics (including that of three Fields medalists, Witten, Borcherds, Kontsevich) means it's as legitimate as GR is when making analogies. String theory has proved its chops, just not in the way the hypesters intended.

27. **Peter Woit**
July 20, 2017

william e emba,
I think it actually matters to distinguish between what has been successful about "string theory" and what has been a failure. My problem with this article is that it misleads people about this.

By the way, Witten's Fields medal was for work (Chern-Simons) on QFT, not string theory.

28. **Andreas Karch**
July 20, 2017

Hey Peter,

I am not nearly as disturbed as you are by the last two paragraphs. AdS/CFT certainly has been used "as a tool for doing physics, real physics". I think there is no doubt about it. This seems to be in the end the only thing that is really claimed. It's a tool that has its limitations (you study the wrong theory), but it works in situations where we have basically no other tools available (time dependent processes in strongly coupled theories). In detail, of course things are complicated. The stringy toy models were only part of the story. The way the second to last paragraph (talking about the standard model and unification) leads into the last paragraph (describing how string theory was useful in this situation) is open to the misinterpretation that they tested quantum gravity in the lab (which they didn't). I have different problems with the article that concern me more, but they are of a more technical nature.

29. **Peter Woit**
July 20, 2017

Chris and Andreas,

To make the question more specific, recall that the article states
"string theory has been maligned because it makes predictions that cannot be tested"
and then goes on to imply that the maligners have now been shown to be wrong, quoting Landsteiner about string theory now being on "a firm basis as a tool for doing physics". Is this or is it not highly misleading (in particular the move without any acknowledgement from string theory as a 10/11 d unified theory to gauge-gravity duality as a tool in condensed matter physics)?

I can see many good arguments for pursuing AdS/CFT as a tool for understanding strong coupling QFT, but why not sell this for what it is (and acknowledge that maybe the maligners of string theory unification were right?).

30. **william e emba**
July 20, 2017

Atiyah's laudatio of Witten mentioned three things: TQFT (which paper referred explicitly to using string theory arguments), new proof of positive energy theorem (using SUGRA), and new formulation of Morse theory (using SUSY). That's string theory enough for me.

31. **Peter Woit**
July 20, 2017

william e emba,
I've found from bitter experience that it's hopeless to argue with people about this. That Witten won the Fields medal because of his work on string theory is now a deeply entrenched alternative fact. If you are interested in real facts, recall that TQFT/SUSY/SUGRA are not string theory and take a look at
http://www.math.columbia.edu/~woit/wordpress/?p=99

32. **Matt Grayson**
July 20, 2017

I've used this analogy. If we make no distinction between Newton's theory of gravitation and the mathematical tools he developed for it, then I could say that I've put Newton's gravitation theory on a firm footing by doing a calculus problem.

33. **Jeff M**
July 20, 2017

Just to 1/3 support Peter, Witten's work on Morse theory was not string theory. That paper was the basis of my orals in grad school, so I know it well. Very nice math, nothing to do with strings. I've seen some math talks which are string theory, a few of them very interesting (Kevin Costello, on moduli spaces of Riemann surfaces, must be 20 years ago). Witten's work on Morse theory at least wasn't related.

34. **Deane Yang**
July 20, 2017

As a biased observer, I wanted to make some comments:
1) This appears to me to be a debate on semantics. Is "string theory" a (not necessarily rigorous) mathematical tool that was developed as an effort to develop a TOE? Or is "string theory" the TOE itself?
2) It appears to me that the main objection to the last two paragraphs is the concern that people who view string theory as the TOE will believe that Landsteiner's work represents progress in string theory as such.
3) On one hand, this is a valid concern that I agree with. On the other, this is not national politics. Having readers being misled on this does little harm. To me what's more important is to have more articles about science that people want to read. If poetic license is taken, I think we should try to live with it. Mentioning string theory here, even if it's a stretch, is good publicity. I know a big concern is the overfunding of string theory, but I think a bigger problem is the underfunding of scientific research.
4) Please be cognizant of the limits on what a journalist can do, including the time to research the article and the length of the article. It is hard to be precise without being dry in a 500-1000 word article. It is hard to be precise, if you are not an expert in the field and have to talk to experts who are unable to express themselves clearly to a non-expert. It is hard, period, to assess who is right and who is wrong. Even in a hard science like physics, there is a lot of subjectivity.
5) So what can you do when you don't like what an article says? Luckily, articles can still be revised even after they've been posted on the web. So you can contact the journalist and ask for a correction. However, you'll increase your chances of getting the correction made, if you make the journalist's job easier. This means trying to isolate what part of the article you'd like revised (here, it is the last two paragraphs) and suggesting new wording that's roughly the same number of words as the original wording *and* interesting to read. As someone who has tried to help Ken write about math, I know that this is really, really hard. Everything I write for him turns out to be far too long, literal, and dry. This is a huge handicap when trying to help a journalist write about science and math.
6) It's not too late to email Ken with suggested rewording. If you believe you can write the last two paragraphs in a less misleading way, please try. But it seems to me that you have to live with the quote by Landsteiner, which is not literally wrong. It's just that you want to find a way to express that Landsteiner's work was, if anything, a validation of one particular technique used in string theory, rather than string theory as a TOE.
7) By the way, if you want to see a masterful job by Ken writing about math, please read his obituary of Maryam Mirzakhani.

35. **vmarko**
July 20, 2017

Sooo, finally string theory stops being a theory about high energy physics, and moves ever more firmly into the solid state domain. It started as an effective theory of strong interactions, had one long stint as a theory of everything, and now moves on to calculating conductivities and viscosities in solid state physics. After that, what's next, maybe plasma physics and fusion reactor modeling using AdS/CFT techniques?

I'll probably have mixed feelings when the everlasting flood of papers published by string theorists moves from hep-th to cond-mat section of the arXiv. 😉 Just imagine, titles like "Electron transport characteristics in an 11D supersymmetric hexagonal crystal lattice compactified to a 3-torus with a negative cosmological constant", and similar, appearing on cond-mat… 😀

🙂
Marko

36. **Mitchell Porter**
July 20, 2017

If any of the experts are still reading… I am still trying to understand the *mechanism* of MAGA-induced (?) anomalous transport. Surely it doesn't involve virtual gravitons?! But then, how does it work?

37. **william e emba**
July 20, 2017

I personally think of SUSY/SUGRA as part of "string theory", but I'm perfectly happy if people think that is just ignorant twaddle on my part. I was not confused about the history, and our main disagreement is simply what does "string theory" mean. I completely agree that Witten was not given the Fields medal for his work *in* string theory.

So far as I know, neither Borcherds or Kontsevich did work *in* string theory, but they took what was there and ran with it. All I meant was that Witten did the same thing. For example, in his TQFT paper he explicitly mentions that his central arguments "will be quite recognizable to string theorists".

I believe we agree far more than we disagree, but in my attempts at brevity I have been unclear about my meaning. I apologize.

@Matt Grayson: considering how inconsistent the foundations of calculus were in Newton's day, there really was a time when getting calculus to work was a point in Newton's gravitational favor.

38. **Peter Woit**
July 20, 2017

Deane,

I have written to Ken with my suggestion about how to fix the article, which is pretty simple, just delete the stuff about "string theory". It seems we disagree about this. That's too bad, in general I think he and others at the NYT do a very good job of carefully covering math and physics, not always an easy task.

The fundamental problem with the NYT article is the use of the term "string theory" to mean "gauge-gravity duality calculational method", something with zero relationship to the "string theory" that is supposed to unify physics that readers have heard about, so they're going to be misled.

If you no longer know what idea "X" refers to, it becomes impossible to have a sensible conversation about "X" and things descend into the realm of ideology (are you for or against the meaningless term "X"?). My take on what's going on is that X="string theory" is in the process of becoming not a scientific term, but a clan name. This may be good for publicity purposes, but it's not good for science.

39. **David Roberts**
July 20, 2017

If one replaces occurrences of "string theory" with "the mathematics of string theory" then many problems go away.

40. **Peter Woit**
July 20, 2017

David Roberts,

I don't really think so. "the mathematics of string theory" is a really ill-defined term, and under many interpretations has little or nothing to do with the condensed matter story of this article.

41. **Dam Thanh Son**
July 20, 2017

I think it is useful to recall the relevant historical timeline.

The connection between axial-gravitational anomaly and transport (in flat space) at finite temperature, zero chemical potential was first suggested in a paper by Landsteiner, Megías, and Pena-Benitez, https://arxiv.org/abs/1103.5006. The effect was first pointed out to be possible by Neiman and Oz (arXiv:1011.5107). Landsteiner et al. suggested that the coefficient of this effect is equal to the coefficient of axial-gravitational anomaly, multiplied by temperature squared. How did they know? They constructed a simple theory in AdS5 so that the corresponding CFT4 has an axial-gravitational anomaly. Then they considered a black-brane solution, and by using AdS/CFT, found a "chiral vortical" coefficient proportional to the coefficient of the axial-gravitational anomaly.

I remember being very perplexed by the claimed connection between transport in flat space and anomaly in curved space appeared to me very strange. Not only that Surówka and I missed the coefficient in our paper (mentioned by Andreas), it was also that if we tried to correct our calculation (as Neiman and Oz did), all we could do was to restrict the coefficient to be an constant times T^2, with no indication that the constant is related to any anomaly. In contrast, naive power counting would imply that contribution of the axial-gravitational anomaly should appear at the third order in the derivative expansion, rather than at the first order, as Landsteiner et al. claimed.

For a more than a year, the issue was not understood. There was strong indication in favor of the connection from AdS/CFT, but it was not clear how general it is. If the coupling in field theory is not infinite, but only finite, would the connection still hold? The first paper explaining the connection without using AdS/CFT seems to be the one by Jensen, Loganayagam, and Yarom (arXiv:1207.5824).

What follows is my subjective opinion.

The AdS/CFT correspondence played a crucial role in this story. Without AdS5 as a laboratory, it would be highly nontrivial to see any connection between the two effects. For one, finite temperature or temperature gradient does not lead to violation of chiral charge (Spivak's argument). Then there is a mismatch between the orders of the derivatives mentioned above.

Figuratively, AdS/CFT has provided a cheap laboratory for theorists to play and to "measure" different transport coefficients of an exotic medium (like the one where particles are fermions with definite chiralities), years before it became possible to do it in a real lab.

Landsteiner's statement quoted at the end of the NYT article, in my view, is an accurate description of the role that string theory played in his and many other theorists' work. As often in condensed matter physics, whether the suggested theory is directly relevant to the experiment is a question that will take some time to completely settle.

42. **Peter Woit**
July 20, 2017

Dam Thanh Son,

Many thanks for your comment, which completely clarifies the role of AdS/CFT here.

I still however think that using the term "string theory" to describe these calculations is misleading (given that strings play no role). Why not just use a term like AdS/CFT or gauge/gravity duality? One argument for using "string theory" would be its historical role in AdS/CFT. If so though, in the usage of "string theory" in this article, we're now at two different historical removes from an actual theory of strings, and the average reader is unlikely to understand at all the relation between the "string theory" being discussed here and the "string theory" they've heard about elsewhere.

43. **Mitchell Porter**
July 21, 2017

Let me distinguish between the various complaints that have been made so far.

Peter Woit objects that a successful application of AdS/CFT to condensed matter could be mistaken for a successful application of string theory to particle physics.

William B. Emba and I object that the actual effect at work here is not literally the mixed axial-gravitational anomaly, it's some kind of thermal analogue of that.

And Boris Spivak (quoted in Nature) doesn't even think it's the thermal anomaly producing the reported results, it's some other process entirely.

Obviously Spivak's criticism is the only one of direct scientific consequence, the others pertain to how concepts are named and communicated.

But I would like to expand a little on *my* complaint. Ironically, I find the science journalism clearer on the point that this is a physical analogy, than the actual arxiv paper! (I'm referring to 1703.10682.)

I see nothing in that paper which would lead a person to think that the effects in question are not being attributed to literal gravitational anomalies. Several times, the authors write that the reader may wonder how a gravitational anomaly could be relevant in the absence of gravitation; but each time, we are told that the connection has nonetheless been established.

There must be a way to understand this which is not "consider this field theory in curved space, then consider the zero-curvature limit, and find that the gravitational anomaly coefficient is still nonzero"; but which is more like, "consider this field theory in flat space, and note this algebraic isomorphism with the zero-curvature limit of the curved-space analogue".

But it seems that if I want to understand what's really going on at the level of theory, I'll have to just keep butting my head against the original papers until enlightenment dawns.

44. **DrDave**
July 21, 2017

I don't think it matters what the NYT *thinks,* they should simply publish a response from Peter. That's good journalism. Better journalism would be to get the facts right, of course. If a paper doesn't print a well thought out, factually accurate letter, it isn't a newspaper anymore–something the Times has been struggling with.

45. **Peter Woit**
July 21, 2017

DrDave,
You're assuming I want to write a letter to the NYT for them to publish, which is not the case (I don't think the issues involved lend themselves to such a letter, and I've already wasted enough time on this). I have been in communication with the author of the article, and, given that my complaints are not about factual correctness but about misleading use of language, he's not interested in significantly changing the article.

Unlike most cases where people disagree with something in the NYT, at least in this one my point of view has gotten aired in great detail here (and opposing views also, thanks to commenters).

46. **Peter Woit**
July 21, 2017

DrDave,
Perhaps due to our email exchange, the author of the NYT piece did make some changes in the language to make things less misleading.

47. **Chris W.**
July 22, 2017

"… but for most laymen the mention of string theory was just fine."

As much as Chang, that's an editor talking.

So much of the media is worse than this where science and many other subjects are concerned. I suppose one can give the New York Times some credit for resisting the all-out competition in the race to the bottom.

48. **Peter Woit**
July 22, 2017

Chris W.,
I don't think the problem is really with the journalists, it's with the physics community. In this case the publicity for the (quite complex) scientific result seems to have largely been provided by string theorist Landsteiner, including presumably accurate quotes claiming this as a "string theory" calculation and success. And, from the comments, his colleagues in his field say they're fine with this, even if it's misleading. Other physicists need to step up and say that this is misleading and not fine. The author heard from me making this case, but is then put in a position of not knowing who to believe (a non-specialist has no hope understanding the role of string theory in AdS/CFT and the role of AdS/CFT in the calculation of this effect).

49. **Art**
July 23, 2017

This post and the comments have been educational and productive: a fair amount of history and technical detail was discussed, culminating in a revision to the original article. I hope it's at least somewhat satisfying. The only thing missing, I think, was an explanation of the change to the article, which I'm used to seeing after an edit.

50. **G Euphrates**
July 24, 2017

mea culpa.
As an ex-IRE member (Institute of Radio Engineers) I witnessed the change to IEEE (Institute of Electrical and Electronics Engineers); thus, your use of IIEE confused me.

But excellent clarification by argumentation in the comments!

51. **Peter Woit**
July 24, 2017

G Euphrates,
Thanks, I thought I had fixed that typo, now done.

52. **Art**
July 24, 2017

As of this morning, the referenced NY Times article is truncated to 4 paragraphs, with no mention of Dr. Landsteiner.

53. **Tony Zito**
August 2, 2017

Non-specialist Community College physics teacher here. Is any part of this akin to the question of whether Bekenstein discovered black hole thermodynamics, or just an interesting analogy? Is there a chance that there is any significant physical connection between this experiment and black hole physics, so that, for example, we could really learn something about event horizons by keeping this up?

54. **Peter Woit**
August 2, 2017

Tony Zito,

There's no significant connection to actual black hole physics or black hole thermodynamics here.

Summer Conferences: Physical Mathematics

July 21, 2017
Categories: Uncategorized

I've finally found some time to look around the web to see what has been happening at conferences this summer. In this blog post I'll point to a few on the math/physics interface featuring interesting talks. This area now (I think it may be Greg Moore's fault) has started to acquire the name of "Physical Mathematics", to distinguish itself from old-school "Mathematical Physics". At this point though I'd be hard-pressed to provide a useful definition of either term.

- Talks from last month's 2017 Bonn Arbeitstagung are available here. This conference was in honor of Yuri Manin and supposedly devoted to Physical Mathematics (although I suspect some of the speakers might not realize that they are doing Physical Mathematics). Dan Freed and Jacob Lurie gave two characteristically lucid series of talks, well worth watching.

  A very active area of physics these days with significant overlap with mathematics (of the sort discussed by Freed) is the study of topological superconductors and other materials in which topology plays a large role. For an introduction to this topic, Davide Castelvecchi at Nature has a new article The strange topology that is reshaping physics.

- CERN has just finished running an institute on the topic of the Geometry of String and Gauge Theories. It included a colloquium talk by Greg Moore on d=4 N=2 Field Theory and Physical Mathematics. I've always been fascinated by the d=4 N=2 super Yang-Mills theory in its "twisted" topological version. The mathematics involved is deep and amazing, and it is frustratingly close to the Standard Model…
- Pre-string math 2017 was this past week, and String Math 2017 will be next week. All sorts of interesting talks at both of these, relatively few of which have much to do with string theory. That's of course also true of Strings 2017, but I'll write about that elsewhere.

Other suggestions of interesting mathematically related summer schools with talks available are welcome. On the physics side, please wait for a succeeding blog entry on that topic.

Comments

1. **Tim**
   July 22, 2017

   I wonder if the difference is something like the difference between Physical Chemistry and Chemical Physics … the former uses Chemistry jargon while the latter used Physics jargon, but they are otherwise identical.

2. **peon**
   July 25, 2017

   Arnold Sommerfeld referred to physical mathematics in the forward to his book on Partial Differentail Equations

3. **Trent**
   September 10, 2017

   Commenting here because your Physical Mathematics post doesn't have a "leave a reply" section. (Weird, is it just my computer?).
   Kishore Marathe is the one who coined the term "Physical Mathematics". see the intro of his book "topics in physical mathematics" for some discussion of what the term means. (I take it to mean (as in mathematical physics) the drive to find the correct mathematical way to express physical theories (perfecting physics such that it becomes math), but then (unlike in mathematical physics) physical mathematicians go beyond physics and care about interesting mathematical structures inspired by physics which don't look like they describe our physical universe (as long as these structures are mathematically interesting). Some (many?) physical mathematicians even care more about the mathematical interest of the structures than the physical interest.)

   https://books.google.com/books?id=Rf7pqYEb4PQC&printsec=frontcover&source=gbs_ge_summary_r&cad=0#v=onepage&q&f=false

What the Hell is Going On?

July 22, 2017
Categories: Uncategorized

I've looked at the talks from a few of the HEP experiment and phenomenology summer conferences. If anyone can point me to anything interesting that I've missed, please do so. The lack of new physics beyond the Higgs at the LHC has left the field in a difficult state.

One conference going on this past week and next is the IAS PiTP summer program aimed at advanced grad students and postdocs. This year the topic is HEP phenomenology, and talks are available here. If you want to understand the conventional wisdom on the state of the subject, you can watch Nima Arkani-Hamed's three and a half hour lecture (here and here) which he starts off by describing as on the topic "What the Hell is Going On?".

A lot of the first part is historical, starting off with the Georgi-Glashow GUT and the arguments for SU(5) or SO(10) GUT unification first put forward 43 years ago. He then walks the audience through the sequence of steps theorists have taken to solve the problems of such models, after an hour ending up at the landscape, spending a half an hour promoting the anthropic solution to the CC and other problems. The second part of the talk is largely devoted to making the case for his favored split SUSY models, with anthropics and the landscape taking care of their naturalness problems. By the end of the three and a half hours, Arkani-Hamed admits that this scenario is not that convincing, while arguing that it's the only thing he can see left that is consistent with the idea that theorists have been following a correct path since 1974:

> It's the only picture of the world that I know where everything that we learned experimentally and theoretically for the last 30 years has some role to play in it. But my confidence in it is not so super high, and I definitely think its worth thinking about completely radically different things.
>
> The disadvantage to the trajectory of going with what works and then changing a little and changing a little is that you might just be in the basin of attraction of the wrong idea from the start and then you'll just stay there for ever.

To me by now the evidence is overwhelming that HEP theory has been in the wrong basin of attraction for quite a while, and the overriding question is what can be done to get out of it. If you're in the wrong basin of attraction, you need to get out of it by going back to the point where you entered it and looking for another direction. I think Arkani-Hamed is right to identify the 1974 GUT hypothesis as the starting point that led the field into this wrong basin. HEP theory has progressed historically by identifying new more powerful symmetry principles. The move in 1974 was to go beyond the SM symmetries by picking a larger gauge group, then breaking it at a very high energy scale with new scalar fields. The history of the last 43 years is that this idea isn't a successful one: as this talk shows, it leads to an empty theory that explains nothing. Can one find different new ideas about symmetry that are more promising?

Comments

1. **Jon Awbrey**
   July 22, 2017

   What tack would you tackle?

2. **Peter Woit**
   July 22, 2017

   Jon Awbrey,
   To me an obvious thing to do is to examine carefully the role of group representation theory (the area of mathematics that studies the implications of symmetries) in the Standard Model, and see if thinking about the Standard Model in that language can inspire new ideas. This is one of the motivations for the book that I just finished writing. I see several different directions of this sort to pursue, that's what I'm working on these days. I don't have anything definite enough to write about here. And, I don't want to turn this blog entry into a discussion forum for speculative ideas, mine or any one else's.

3. **Alex**
   July 22, 2017

   Even if you go back to pre-1974 assumptions and reevaluate everything, any new frameworks that emerge will still be of limited utility to physics until the experimentalists get something new. If the collider experiments continue to match up with the Standard Model, the searches for dipole moments and dark matter continue to find nothing, and the interesting anomalies in cosmic ray detectors and gamma ray telescopes get more mundane astrophysical explanations, then there's little or nothing to test new frameworks against.

   It could be quite intellectually fruitful for mathematics, but physics has little hope without experiments.

4. **Peter Woit**
   July 22, 2017

   Alex,
   There has been a lot of progress since 1974 in understanding the mathematical structures involved in QFT, so one is not going back to pre-1974. Yes, it would be extremely helpful if experiments break the Standard Model and provide indications of the path forward from the physics side. Bu this is not happening, and as long as this continues, coming at the problem from the more difficult path of studying the mathematics may be the only way forward. Or, one could just give up completely on HEP theory until a new experimental result comes along, and have everybody move into condensed matter theory. Actually, that seems to be where things are headed…

5. **Nakanishi**
   July 22, 2017

   I remember, though not precisely, the motto of Heisenberg: "The most conservative people can be most revolutional." QFT is quite successful; therefore, one should adhere to its proper framework as long as possible unless there appears an experimental evidence which undoubtedly contradicts with QFT. The natural representation space of QFT is the indefinite-metric Hilbert space, together with subsidiary conditions. The quantum gauge theories are successful because there exists the BRS invariance condition (the Kugo-Ojima condition). One should investigate whether or not there is any other possible subsidiary condition which guarantees the unitarity of the physical S-matrix.
   SUSY was destined to be ruined because all superpartners must be physical particles. If a supersymmetry in a wide sense could be formulated in the indefinite-metric Hilbert space with a subsidiary condition, all superpartners could become unphysical.

6. **Shantanu**
   July 22, 2017

Peter, why isn't anyone (including string theorists) trying to link theories with non-0 neutrino mass with TeV scales physics and models of EW symmetry breaking. As me and others have mentioned many times, models with see-saw mechanism etc are completely decoupled from rest of BSM physics and I don't see that situation changing.

7. **A**
July 23, 2017

In the past 10-20 years fundamental physics gave: cosmological constant, neutrino masses, higgs, inflation, cosmology, gravity waves, the fact that past ideas about naturalness seem wrong (which might indicate a cut-off less trivial than an atomic lattice). Despite the difficulties, this is much more interesting than condensed matter, a mature field that can only study more complex materials. I really hope that this is not the future of physics:
http://pyang.gatech.edu/wp-content/uploads/2017/05/feces_paper.pdf

8. **Thomas Larsson**
July 23, 2017

It is well known to experts (e.g. Pressley-Segal section 4.10) that the group of gauge transformations in Yang-Mills theory has two different types of extensions: the Mickelsson-Faddeev extension and the central extension. The MF extension is the mathematical formulation of gauge anomalies in QFT, and there is a strong mathematical reason why this extension has to vanish: it admits no non-trivial unitary representations.

In contrast, the central extension cannot arise as a gauge anomaly within the framework of QFT, because its representations depend on additional data: a privileged 1D curve in spacetime. This leaves two possibilities: either the central extension is unphysical because it is not in QFT, or one needs to modify QFT to allow for all kinds of extensions that are mathematically possible. It is no secret that I believe in the second option, and even if I didn't succeed to turn this into physics, the math is solid.

9. **David Garfinkle**
July 23, 2017

I agree that going back to the place where a wrong turn was taken and trying to take a different turn is a good idea. However, I'm not convinced that grand unification was the wrong turn. It seems to me more likely that supersymmetry and string theory were the wrong turns. One way to look at grand unification is that (1) in some sense non-abelian gauge theories have better properties than abelian ones, so one might look for a fundamental theory which does not U(1) as part of its gauge group, and (2) in quantum field theory scalar fields are not so tightly constrained as vector fields or fermion fields, so that it's easier to devise theories with lots of scalar fields and so that e.g. it might make more sense to think of theories where dark matter is a scalar field then where it is a supersymmetric partner.
I'm also not convinced that more research in representation theory is the best application of mathematics to quantum field theory. What about a more mathematically sensible way to treat the infinities that arise in quantum field theory? One way to look at this issue is the following: partial differential equations (PDE) are much more .complicated than ordinary differential equations (ODE). PDE have more degrees of freedom, and require much more mathematical machinery (Banach spaces, Sobolev norms, energy estimates, etc.) to show existence of solutions, and indeed it is only for particular PDE (hyperbolic, elliptic, parabolic) that solutions exist. Similarly, quantum field theories have more degrees of freedom than either classical field theories or quantum mechanics. It is therefore not that surprising that infinities arise, and that only some theories (the renormalizable theories) are sensible. Perhaps we need more mathematical machinery to make sense of quantum field theory so that one does not get the impression that infinities are being swept under the rug rather than dealt with.

10. **Peter Woit**
July 23, 2017

David Garfinkle,
Arkani-Hamed explains clearly the logic that non-SUSY GUTs don't work, leading to SUSY.

All,
Please, if it's not about the Arkani-Hamed talk, don't submit comments, I can't and don't want to moderate a general speculative discussion section.

11. **vmarko**
July 23, 2017

Hi Peter,

"If you're in the wrong basin of attraction, you need to get out of it by going back to the point where you entered it and looking for another direction."

This is all true, but I don't think that there is a shortage of ideas which other direction to take. The problem is a social one — convincing everybody else (or at least some part of the community) to actually start doing research in some other direction. And I don't see that happening soon, for two reasons. First, most theorists are egocentric regarding promising ideas (myself included 🙂), so they will rather have you and me follow their idea (even if they don't really have one) than they would agree to follow your or my idea themselves. Second, as usual, physics advances one funeral at a time, and there need to be quite a few of those in order to make room for a sizable number of younger people to actually be able to try out other directions.

My feeling is that lack of new BSM physics at LHC etc. is simply going to reinforce this tension between old and new ideas, as many big-name lead scientists are becoming more and more of an embarrassment in the eyes of younger generations of scientists. I believe in the end this will turn out to be a good thing, but the hep-th community will feel some pain in the meantime.

Best, 🙂
Marko

12. **Armin N**
July 23, 2017

"I don't want to turn this blog entry into a discussion forum for speculative ideas, mine or any one else's."

I understand your motivation for not wanting to do this. Would even one post, say, roughly a month, devoted to a speculative idea you find deserves more attention or investigation be too much, even if only as a riposte to those who sometimes criticize you for just pointing out the flaws in the status quo and not offering promising alternatives ideas?

13. **David Garfinkle**
July 23, 2017

In looking over that part of the Arkani-Hamed lecture it seems to me that "logic of why non-SUSY GUTs don't work" is an overstatement. He looks at things that he considers the simplest and most natural mechanisms for breaking GUTs to the standard model without using supersymmetry and concludes that those mechanisms don't work. This seems to leave open the possibility that some more complicated mechanism that doesn't look natural to Arkani-Hamed might work. In cosmology we have abundant evidence that our prejudices as to what is "natural" are a very unreliable guide. Perhaps it is time that we apply that insight to particle physics.

14. **Markus Pfleier**
July 23, 2017

Nima is probably right. Some years ago, I wrote to a Nobel Prize winner for electroweak unification (which I won't name 🙂 and asked him whether it is more correct to call it "unification" or "mixing". He answered that "unification" is not the appropriate wording; after all, there are still 2 coupling constants.

But it was electroweak "unification" that led to grand "unification". Experiments show us that there are still three coupling constants, not just one. There just is no G"U"T. There is symmetry left to be discovered. We know all elements, we know all particles, we know all symmetries.

And there is no single symmetry. The answer to the question at the end of the post is what experiments suggest: Renewed progress will be achieved as soon as the gauge interactions are kept and left separate. Renewed progress will be achieved as soon as the search for unified symmetries is abandoned.

15. **Markus Pfleier**
July 24, 2017

The sentence should have been: "There is no symmetry left to be discovered."

16. **John Baez**
July 24, 2017

Peter wrote:

> Arkani-Hamed explains clearly the logic that non-SUSY GUTs don't work, leading to SUSY.

I haven't looked at his talk, but my impression is that the non-supersymmetric SO(10) GUT is still viable if you don't mind fine-tuning: there's a [paper by Altarelli and Meloni] about this.

They write:

> We present a renormalizable non-supersymmetric grand unified SO(10) model which, at the price of a large fine tuning, is compatible with all compelling phenomenological requirements below the unification scale and thus realizes a minimal extension of the SM, unified in SO(10) and describing all known physics below the GUT mass. These requirements include coupling unification at a large enough scale to be compatible with the bounds on proton decay; a Yukawa sector in agreement with all the data on quark and lepton masses and mixings and with leptogenesis as the origin of the baryon asymmetry of the Universe; an axion arising from the Higgs sector of the model, suitable to solve the strong CP problem and to account for the observed amount of dark matter. The above constraints imposed by the data are very stringent and single out a particular breaking chain with the Pati-Salam group at an intermediate scale of about 10^11 GeV.

Is this wrong? I really like how SO(10) unifies all the fermions in each generation and neatly explains many of their properties. So, I'm hoping it's not quite dead yet.

17. **John Baez**
July 24, 2017

What's the latest word on the possible [failure of lepton universality]? This would certainly give particle physics a kick in the pants. Mark Wise says it doesn't fit into "the story line that the theorists tell", but it seems that's exactly what we need: some data that don't fit into the usual story line.

> The evidence is even harder to swallow given how far lepton universality is from theorists' expectations of where cracks in the Standard Model might show up. "There's sort of a story line that the theorists tell," Wise says, and "this isn't in the story line." What's worse, the proposed explanations for the leptons' behavior seem ad hoc and unsatisfying. "The kind of models that can fit the…anomalies don't really do anything else at first sight," Ligeti says. "For example, they don't get you any closer to understanding what dark matter might be."
>
> Still, he adds, "nature tells us how nature is." Physicists are increasingly taking note of the violations' continued persistence, and proposing new theoretical explanations. Experimentalists and theorists alike are also looking to reduce existing measurements' uncertainties. Ultimately, the biggest revelations will come when LHCb and the next version of Belle produce more data. Physicists are optimistic that within about five years not only will we know whether the effect is real, we will have an explanation for it."

18. **Peter Woit**
July 24, 2017

John,
If you look at that SO(10) paper, you'll see that what they're doing is moving up the unification scale by introducing a truly baroque set of Higgs fields and an intermediate scale. The problem here isn't really fine-tuning (which all non-SUSY GUTs have) but that to get coupling constant unification you're introducing lots of new parameters and new choices (in the Higgs sector).

This shows the generic problem with GUTs: they claim to answer the problem "why SU(3)xSU(2)xU(1)" (or at least change it to a simpler problem, "why SO(10)"), but all they really do is change the problem to "why this Higgs sector (which breaks SO(10) to SU(3)xSU(2)xU(1))?"

I agree that the fact that a generation fits precisely into the spin rep of SO(10) is a hint about something. But, Spin(10) doesn't really act on this, or, equivalently the action is"badly broken" in a way we don't understand at all. Recall that this spin rep can be constructed as the exterior algebra on 5 variables, or, if you like, 5 fermionic oscillators or 5 fermionic qubits. The fundamental problems then become: why 5? why do 3 of them behave one way, 2 another?

Yes, if the LHCb lepton universality anomaly holds up, that would be a big deal. The history so far is that all similar things have gone away, and there's no reason to think this one will be different. Unfortunately, it also doesn't seem that postulating that muons and electrons behave differently in a way that would explain the LHCb results leads to a model that explains anything else (as opposed to just adding complexity to fit this result).

19. **Peter Woit**
July 24, 2017

Armin N,

I'll keep this in mind and try and post more often about intriguing new ideas that I see. The depressing thing about the current state of HEP theory is that interesting new ideas are very few and far between.

I could also just keep repeating my own favorite intriguing ideas, which don't change much and haven't seen any progress. I happened to run across an old posting about

this from seven years ago, see
http://www.math.columbia.edu/~woit/wordpress/?p=2876
I don't think there's been significant developments on those problems since then, the first and third are motivations for things I'm working on now that the book is finished.

20. **Chris Kennedy**
July 24, 2017

Regarding the LHC, I haven't seen much discussion on the Doubly Charmed Baryon discovered and written about earlier this month? 3520 MeV with a lifetime stated to be shorter than 33 fs. Any new thoughts on that?

21. **Mark M**
July 24, 2017

Hi Peter,

In response to your "could also just keep repeating my own favorite intriguing ideas", there have been some progress in trying to formulate a non-perturbative chiral gauge theory, cf:
Nonperturbative Regulator for Chiral Gauge Theories? Grabowska, Kaplan.
Phys.Rev.Lett. 116 (2016) no.21, 211602, arXiv:1511.03649 [hep-lat]
and
Chiral solution to the Ginsparg-Wilson equation. Grabowska, Kaplan. Phys.Rev. D94 (2016) no.11, 114504 arXiv:1610.02151 [hep-lat]

22. **Peter Woit**
July 24, 2017

Mark M.,

Thanks! Taking a quick look at those papers, it's remarkable what a tricky problem this is and I think it's fair to say that it's still not clear if it is solved.

23. **srp**
July 25, 2017

In honor of the title of this post, here is the great Vince Lombardi:
https://www.youtube.com/watch?v=4V0TYIO6yv4

24. **George Rush**
July 26, 2017

Good talk from Nima Arkani-Hamed. IMHO it's basically a reaction to LHC results. They validated electroweak but cast doubt on his favorite version of SUSY. As Nima sees it the key scale is around 10^-17 cm, so he expected / hoped to see new physics here. SUSY can cancel quantum fluctuations, solving the huge CC mis-prediction and other things. But it has to do it around current LHC scale. A natural reaction to failure is to explain why anyone would, and should, have made the same mistake. (Cf. Hold 'Em players dissecting hands lost on the river to a 1 out of 46 chance.) He goes through the steps since 1974 emphasizing how each was almost inevitable. (Like the poker player proving the only sensible action on the flop was a pot-size bet.) Ties it all together by the idea of finding yourself, by small steps each very sensible, in the wrong "basin of attraction" in solution space. It makes sense and I don't mean to belittle his story. But it can be viewed as a very common reaction to the loss of what looked like a high-positive-expectation bet: justifying it.

25. **William Nelson**
July 26, 2017

Not sure how you can claim there's "overwhelming evidence" that HEP people have been on the wrong track, while also noting that there is no evidence one way or the other from experiment. Many of the same people who got it right with the SM don't think the efforts were misguided at all. Unfortunately, it comes down to opinion at this point. It's interesting to me that you emphasize mathematical studies of QFT and representation theory, both of which have been going like gangbusters in recent times. Perhaps they are not the exact type of studies that you would like to see, but you can hardly deny that there has been great progress in understand the space of QFTs and many new twists on the appearance of various kinds of representations. Many, many people other than yourself are interested in these things, so it's not clear to me exactly what you want people to do that they are not doing. It just comes out sounding like sour grapes of some sort.

26. **Yair**
July 27, 2017

Condensing the Arkani Hamed tour the force, to something like "1974 GUT was were we took the wrong turn" does injustice to the talk.

The talk was a breathtaking and brilliant attempt to cover the current situation – and to theextent any claims were made – it did provide some plausibility arguments in support of an anthropic approach.

Once a new approach (based on the Pre-1974 "wrong turn" framework) accompanied by calculations that are both consistent with the known experimental data, as well as (hopefully) some new predictions – will be officially published and peer reviewed – will it be possible to cast judgement about the heuristic claims hinted at in the comments section.

27. **Peter Woit**
July 27, 2017

William Nelson,
I've devoted far more time than is sensible to making the scientific case here and elsewhere that the string theory landscape/SUSY GUT paradigm has failed, didn't see the need to repeat it here. Characterizing things like the LHC negative SUSY results and the forty year history of countless experimental results in disagreement with SUSY/GUT/string theory expectations as "no evidence one way or the other" makes no sense unless you believe that the paradigm doesn't predict anything, which is exactly why it is a failure.

Yes, in recent years attention has returned to QFT from string theory, which is a good thing, and there's a lot of interesting mathematics going into this. My remarks about QFT and representation theory were not a complaint or sour grapes about anything, but intended as a positive response to a commenter asking what I found more promising.

Yair,
My comments about the talk obviously weren't intended to completely cover a nearly four-hour long lecture. The bulk of the talk was devoted to laying out the argument

that "if someone put a gun to his head", Arkani-Hamed's preferred scenario is the anthropic landscape, with split-SUSY, for no good reason at an energy scale just too high for the LHC to have seen it. As mentioned above, the reasons I think this is a failure are ones I go on about endlessly.

I wrote about this talk here because I think it's an unusually clear laying out of the current situation. I'd reserve the terms "breathtaking and brilliant" for a lecture with new ideas, not just a rehash of failed ones. The most interesting thing about the lecture I think was the end that I quoted, where Arkani-Hamed does bring up (without making the case for it) the obvious answer implied by his talk to the "What the Hell is Going On?" question.

28. **X**
July 28, 2017

"I've looked at the talks from a few of the HEP experiment and phenomenology summer conferences. If anyone can point me to anything interesting that I've missed, please do so. The lack of new physics beyond the Higgs at the LHC has left the field in a difficult state."

Well there's this from the recently concluded Lattice 2017 conference.
(Abstract:
I will discuss the recent B-physics results which indicate intriguing deviations from the Standards Model expectations. I will focus on several New Physics scenario which are currently being explored. I will then go through several flavor physics observables (not only those involving b-quark!) and argue that they too could provide us with access to New Physics provided the hadronic uncertainties are tamed by means of Lattice QCD.)

This kind of work is an example of what Adam Falkowski (Jester) calls the "2nd approach" to HEP – the `high precision' approach – as opposed to the `high energy' approach:
"By measuring the properties of known particles with great precision, and comparing the results to theoretical predictions, insights can be derived into physical laws at energies inaccessible to collider experiments."

The main challenge on the theoretical side is to compute the theoretical predictions with sufficiently high precision to be able to show discrepancy with the experimental results. Taking account of QCD processes is often the 'hard part' in getting the high precision theoretical predictions, so Lattice QCD is important here.

This approach is looking increasingly promising, not only IMHO but also in Falkowski's view:
"Shifting the focus away from high-energy colliders toward precision experiments may be the most efficient way to continue exploration of fundamental interactions in the decades ahead. It may even allow particle physics to emerge stronger from its current crisis."

(In the past I occasionally berated Falkowski on his Jester blog for wallowing in the lack of new developments from the 'high energy' approach to HEP while ignoring the interesting things going on in the high precision approach, so it is amusing to see that he is starting to appreciate it now…)

However, this kind of work is being done by relatively low-profile people at non-illustrious institutions, so it gets very little attention compared to the latest musings of Nima et al…

BTW, regarding the chiral gauge theory papers of Grabowska & Kaplan mentioned in a comment above, the approach seems to have been debunked (or at least shown to be very problematic) recently by Suzuki and collaborators here.

29. **D R Lunsford**
July 28, 2017

http://www.slate.com/articles/technology/future_tense/2017/07/rick_and_morty_gets_multiverse_theory_wrong_that_s_ok.html

30. **Barry Awn**
August 7, 2017

Peter, I would be interested to hear your response to Nima's comments around 1:37:14 about "… the most boring possible discussion …" in his HEP talk at the IAS PiTP summer program ("Where in the World are SUSY & WIMPS?). I found the comment at: 1:37:14 on https://www.youtube.com/watch?v=dKVXxcbJ4YY

31. **Peter Woit**
August 8, 2017

Barry Awn,
I thought that whole section of his talk was just outrageous. Given an hour to present to young researchers his take on the state of the field, he spends about 40 minutes going over laboriously exactly the same story about the CC/landscape/anthropics that those in the audience have surely heard a hundred times already (talk about boring!!), then dismisses objections with a wave of the hand as "the most boring possible.."

If, as seems increasingly all too possible, we're now at an endpoint of fundamental physics, with the field killed off by a pseudo-scientific argument ("no point in continuing, the multiverse did it"), Arkani-Hamed is one of those who will be most responsible for the situation.

32. **Jim Akerlund**
August 9, 2017

The August issue of "Notices" with the articles about gravitational waves, lists Arkani-Hamed as getting inducted into the "National Academy of Sciences". I know you get elected by current members, but does the election also include the reason the new member was elected? If so, does Arkani-Hamed reason include non-mathematical contributions, like the subject of the talks above?

33. **Peter Woit**
August 9, 2017

Jim Akerlund,
Arkani-Hamed is a physicist, not a mathematician, well-known for his work on various topics in HEP phenomenology (as well as some more recent more mathematical work on scattering amplitudes). He has an enthusiastic lecture style, likes to make dramatic, forceful claims, this is one reason he is quite influential in the field at the moment, for better and worse…

34. **Phil**
August 21, 2017

As far as new physics are concerned, strong hints actually appeared this Summer, not from the LHC, but from T2K, which excludes CP conservation in neutrino oscillations at the 95% confidence level using the latest data:
http://t2k-experiment.org/2017/08/t2k-2017-cpv/
Perhaps could be worth a new blog post?

A Few Items

July 25, 2017
Categories: Langlands, Uncategorized

Just a few items:

- The Simons Foundation has announced a new Origins of the Universe initiative, which will fund efforts to "develop testable predictions about string theory, quantum gravity and a cosmological 'Big Bounce.'" I don't think even all of Jim Simons' money will be enough to fund a real "prediction of string theory", but the fake kind can be had rather cheaply. I was interested to see in this presentation from the NSF that grants from Simons and other private sources are starting to change the way they do business:

  > One major challenge affecting Theory is the entrance of non-traditional (private philanthropic) funding sources. NSF has developed new procedures for evaluating overlapping sources of funding and introducing such evaluations into the proposal review process.

  I'm curious how they are dealing with this. If someone is being funded by Simons, will the NSF/DOE also fund them? Will the NSF/DOE stop funding fields that are being heavily financed by Simons/Templeton/Kavli? Does this have anything to with the NSF/DOE cuts in HEP theory funding of recent years?

- The latest AMS Notices has a couple of articles about gravitational radiation, see here.
- Yesterday a two week graduate summer school on automorphic forms and the Langlands program started. Lectures are being given by Kevin Buzzard and are on video here. Buzzard has set up a web-site for the lectures here.

  In his first lecture he explained that Richard Taylor's CalTech lectures in 1992 (scans of Buzzard's notes here) had a huge effect on him, and the plan of his lectures is to cover an updated version of some of the same material, ending up by getting to the latest developments, now available solely on a blog here and here. Buzzard also explained that in 1992 he devoted his time in LA to working on understanding the lectures during the week, going to raves on weekends. No news on whether MSRI is making similar arrangements for weekend activities of students in the summer school.

- Two new articles from Michael Harris: The Perfectoid Concept: Test Case for an Absent Theory and Do Mathematicians Have Responsibilities?.

Comments

1. **nikita**
   July 25, 2017

   Funding, no strings attached. Sorry, could not help.

2. **Egan**
   July 26, 2017

   The article about Perfectoid spaces from Harris is really fascinating. Easy to read for a non specialist and full of info and context.

3. **Bill**
   July 26, 2017

   Michael Harris' article was great but I did not see the point of the discussion at the end: "How much of the fanfare around Scholze is objectively legitimate, how much an effect of Scholze's obvious brilliance and unusually appealing personality, and how much just an expression of the wish to have something to celebrate, the "next big thing"?" One the one hand, nobody questions the opinion of the experts that all the textbooks on the subject will have to be rewritten. On the other hand, if the point was to put the invention of perfectoid spaces on the same level with the invention of complex numbers then the article was not very convincing.

   By the way, was the article written as the Fields medal laudatio? 🙂

4. **Peter Woit**
   July 26, 2017

   Bill,
   I don't see any attempt to argue that the invention of perfectoid spaces is on the level of the invention of complex numbers, he's just noting that our understanding of the significance of all mathematical concepts is provisionary and subject to change. While Harris does like to raise questions he then doesn't answer, this one I think gets answered (as "this is as objectively legitimate as it gets") later on.

   Yes, it does look like parts of this article could get recycled for a different purpose next year. In some sense the article has a similar goal to the expected laudatio, to make the case to non-specialists that some mathematical work is objectively important.

5. **Marc Sher**
   July 26, 2017

   I was the NSF High Energy Theory Program Director a few years ago for a year and a half. I haven't directly spoken with anyone at NSF regarding the specifics recently, but know some general practices (of NSF, not the DOE – I know nothing about the DOE). None of what I say comes from the NSF – it is my memory of what things were like a few years ago.

   "If someone is being funded by Simons, will the NSF/DOE also fund them?"
   There are plenty of people who get funding from both Simons and the NSF. It is a violation of federal law for someone to get NSF funding for a project under which they are also funded by someone else, however most physicists have several projects and can separate things out. In fact, in a grant proposal one is required not only to list current and pending support, but to discuss how that support would overlap (if any) with the work in the proposal.

   "Will the NSF/DOE stop funding fields that are being heavily financed by Simons/Templeton/Kavli?"
   Fields funded by the NSF HE Theory program are entirely determined by the proposals received. The fields of the panel members are also proportional to the fields in proposals sreceived (subject to obvious round-off issues, etc.). The NSF HET program doesn't "decide" what fields to fund.

   "Does this have anything to with the NSF/DOE cuts in HEP theory funding of recent years?"
   The NSF HEP theory budget, to the best of my knowledge, has not been cut any more than the Physics Division as a whole. I've heard that this is not the case for DOE

theory, but don’t have direct knowledge.

6. **Peter Woit**
July 26, 2017

Thanks Marc. I do wonder though what the “new procedures” are and what the thinking at NSF is about what they’re calling a “major challenge”. Given the way theorists (and their grad students and postdocs) work, it seems that it must be tricky to keep separately funded projects separate (what if a grad student funded by project A has an idea about project B?).

7. **Marc Sher**
July 26, 2017

Peter – I don’t know what the “new procedures” are. I imagine it is a “challenge” if private foundations begin steering the direction of physics, rather than having physicists do that. I don’t have an answer to that. As far as grad students changing ideas, we all understand that projects change direction (which is why NSF gives “grants” and not “contracts”) – I imagine if non-overlap morphed into overlap, the Program Director should be informed.

8. **Michael Barany**
July 29, 2017

It’s striking to see private philanthropies described as non-traditional funding sources for mathematics. Historically, NSF-style open civilian government grants are far and away the exception, and the NSF math system was itself modeled on philanthropic and military programs.

9. **Art**
July 30, 2017

From the “it could be worse” department, in today’s NYT, AI researcher Gary Marcus “look[s] with envy at my peers in high-energy physics”.

10. **jonW**
August 2, 2017

Regarding the DOE, it looks like their spending programs are completely mothballed now in any case. Just the other day there was an extremely thoroughly researched article from Vanity Fair that is very much worth reading despite its being predictably depressing: http://www.vanityfair.com/news/2017/07/department-of-energy-risks-michael-lewis

Quick Links

August 4, 2017
Categories: Uncategorized

- For representation theory aficionados, George Lusztig has put on the arXiv a long document with comments on his papers (for a bit more about him, see this).
- For a new idea exemplifying the potential grand unification of mathematics and physics, Minhyong Kim and others have been developing arithmetic Chern-Simons theory (see here and here). There was a recent workshop on the topic, videos of talks available here.
- The editors of the Journal of Algebraic Combinatorics are leaving the Springer journal, setting up a new journal, Algebraic Combinatorics. For more about this, there's a press release, a story at Inside Higher Education, and a blog entry by Timothy Gowers.
- Landon Clay, founder of the Clay Mathematics Insitute, passed away last week, more about him available here.
- There's a profile and interview with Carlo Rovelli here.
- While Google continues to develop our new machine overlords, Google money will fund a new IAS program to address their theoretical underpinnings.

Comments

1. **Nilay**
   August 5, 2017

   Hey Peter —

   You might have posted about this already, but there was a CBMS conference in Bozeman Montana this week (http://www.math.montana.edu/cbms/), with 10 lectures by Dan Freed on applications of stable homotopy to lattice models. Arun Debray has some liveTeX'd notes here:
   https://www.ma.utexas.edu/users/a.debray/lecture_notes/topological_and_geometric_methods_in_qft.pdf

   The lectures were recorded, so those will probably be online at some point.

2. **Peter Woit**
   August 5, 2017

   Hi Nilay,

   Looks like a really interesting conference, with some great lectures on the intersection between math and physics. Thanks for writing in about it, hope you're enjoying Bozeman! I may be out there in a couple weeks (being a bit south of there on August 21 would be a good idea…)

Cosmology for the Curious

August 8, 2017
Categories: Book Reviews, Multiverse Mania

There's a new college-level textbook out, Cosmology for the Curious, targeted at physics courses designed to explain basics of cosmology to non-physics majors. The authors are Delia Perlov and Alex Vilenkin. Back in 2006 Vilenkin published a popular book promoting the multiverse, Many Worlds in One, which I wrote about at the time, making the obvious comment that there was nothing like a testable experimental prediction to be found in the book. It seemed to me then that the physics community would never take seriously an inherently untestable theory, recognizing such a thing as pseudo-science. I thought that the only reason claims like those of Vilenkin were getting any attention was that they had some novelty. Surely after a few more years of attempts to extract a prediction of some sort led to nothing, the emptiness of this sort of idea would become clear to all and everyone would lose interest.

Eleven years later I'm as baffled by what has happened to the field of fundamental physics as I'm baffled by what has happened to democracy in the US. As all attempts to extract a testable prediction from the multiverse have failed, instead of going away, pseudo-science has become ever more dominant, with a hugely successful publicity campaign (including a lot of "Fake Physics") overcoming scientific failure. Now this sort of thing is moving from speculative pop science to getting the status of accepted science, taught as such to undergraduates.

Many are worried about the status of science in our society, as it faces new challenges. I don't see how the physics community is going to continue to have any credibility with the rest of society if it sits back and allows multiverse mania to enter the canon. Non-scientists taking science classes need to be taught about the importance of always asking: what would it take to show that this theory is wrong? how do I know this is science not ideology?

Any student who reads this textbook and looks for answers to these questions in it will find just two "tests" of the multiverse proposed:

- Look for evidence of bubble collisions.
- Believe this paper, and then if you find a black hole population with a certain kind of mass spectrum, that would be evidence for the multiverse.

Of course there is no evidence for bubble collisions or such a black hole population, but these are no-lose "tests": no matter what you observe or don't observe, the multiverse "theory" can only win, it can never lose. Is it really a good idea to teach courses telling college students that this is how science works?

Comments

1. **Chris Herzog**
   August 8, 2017

   Two bubbles found they had rainbows on their curves.
   They flickered out saying:
   "It was worth being a bubble, just to have held that rainbow thirty seconds."

   –Carl Sandburg

2. **Hannes**
   August 9, 2017

   Peter, a little aside, there has been recent hints that CP violation occurs in neutrinos

   http://t2k-experiment.org/2017/08/t2k-2017-cpv/

   and commenters claim that this result might explain baroygenesis in the early universe. If that is the case, a further argument for physics beyond the standard model might be gone (you cited the article by Siegel in you blog, https://www.forbes.com/sites/startswithabang/2016/11/04/five-independent-signs-of-new-physics-in-the-universe , where the failure of the standard model to explain baryogenesis is given as one of the reasons to go beyond the standard model). Could you comment on this, maybe in another post?

3. **Peter Woit**
   August 9, 2017

   Hannes,
   The problem is that I don't know much at all about baryogenesis models, so best to leave this question to bloggers who do.

4. **Dan Winslow**
   August 9, 2017

   It's the power of instant, free mass communication, plus the seemingly ingrained predilection to hear what we want to believe rather than what is true. In both cases. The fact that 'news' is monetized by advertising makes it even worse. You can make money blathering about the multiverse, because it is cooler than not having one, and further means you can blather about anything since anything is possible. It's a perfect setup.

5. **The Observer**
   August 9, 2017

   We live in an age of great bogosity. "We each have our own truth" is the mantra of the age. Objectivity cannot be expected to survive in such an atmosphere. Believe it or not, Karl Popper's academic children began the "science is a cultural construct" movement in California a long time ago. Now, we hear the rumblings of direct attack on science and mathematics by their followers.

   It would be funny were it not so sad and so alarming.

6. **Peter Woit**
   August 9, 2017

   The Observer,
   I don't think you can blame Popper, his academic children, or a "science is a cultural construct" movement for what is going on here. Those pushing the string landscape pseudo-science are at the heart of the academic establishment and it seems that their main motivation is simply that of being unwilling to admit to failure.

7. **Mozibur Ullah**
   August 9, 2017

   I blame both the internet and the media; personally, I'm not against speculation, so long at its labelled as such; I don't think, however it's a good idea to smuggle it into a book for undergraduates without qualification.

   I also recall reading that a few physicists on discovering QM, saying we need crazy counter-intuitive ideas to make sense of QM, maybe that's set the stage for crazy ideas elsewhere, like in cosmology.

8. **Peter Woit**
   August 9, 2017

   Mozibur Ullah,
   I don't think you can blame the internet/media for this one. We're talking about a college course taught by a physicist whose views are now considered mainstream by many other physicists. This is a problem the physics community needs to face up to, not blame on others.

9. **Low Math, Meekly Interacting**
   August 9, 2017

   It's a shame, really. I found the preview chapter on Newton's laws quite enjoyable. Assuming the rest of well-established physics is covered in comparable style, I'd happily give the text a good read just for fun. There aren't too many books out there that cover such material with this balance of rigor and accessibility. But with all the anthropic multiverse garbage larding the book's final chapters, no thanks.

10. **Radioactive**
    August 10, 2017

    Is this so different from 'String Theory for Undergraduates'? Have some faith in students to be able to notice bunk when they're taught it.

11. **anon**
    August 10, 2017

    They never will change their mind, because they never have before. There were competent smart physicists from good universities looking for the luminiferous aether well into the 1930's. If anyone's noticed the people involved in these theories are starting to get quite old, they will eventually retire and new people with new idea's will take their place.

12. **Peter Woit**
    August 10, 2017

    Radioactive,
    Why do you think non physics major students will recognize the multiverse as bunk, when IAS professors don't?

    anon,
    I don't think it's true that multiverse mania is just a disease of an older generation that will die out. Many of its proponents are not that old at all. It might even be that the older generation has more skeptics, people who were around when the field was healthy and can recognize the difference between an empty idea and one that works.

    The conventional argument that we'll make progress by the old retiring and the young growing up with better ideas depends on those better ideas coming along. The really awful aspect of multiverse mania is that it's a concerted attempt to get the young to give up and not even try to find an alternative.

13. **Don Murphy**
    August 10, 2017

    I conduct "Making Sense of Science" seminars to discuss contemporary advances in science. One seminar was on the multiverse, the purpose of which was to demonstrate that some "theories" are not testable and therefore not really theories. I presented the 11 or so "theories" Brian Greene puts forward in one of his books that he says all lead to the conclusion the multiverse must exist. Most of the seminar attendees understood that the multiverse is nothing more than pseudo-science. However, to my dismay, there were some who firmly believed that the multiverse confirmed their religious views on polytheism, and human beings being resurrected as gods to rule over multiple universes. A dangerous notion for so-called mainstream science to have spawned.

14. **G. S.**
    August 10, 2017

    One saving grace here is that the material taught in physics classes for non-majors is rarely absorbed by the students. The small amount that is absorbed is quickly forgotten after the post-finals beer binge and brain dump. The books get sold online or go in a box that gets stored in the least accessible spot in a parent's garage. In ten years, 90% of the students who used this book for a college course won't remember even having taken the class, let alone the material from it.

15. **neil**
    August 10, 2017

    IMO, interest in the multiverse persists among reputable physicists because it doesn't violate any logic and offers the illusion of explaining the values of many physical constants (with the Anthropic principle) and fine tuning. It doesn't of course, but absent a real explanation it is the only interesting game in town. It also offers the illusion of testability since practically any irregularity in the CMB can be claimed to be evidence of a neighboring bubble.

16. **Peter Woit**
    August 10, 2017

    Don Murphy,
    From another side, some atheists like to have the multiverse to use against theists making a fine-tuning argument for the existence of God. They don't seem to realize that invoking an untestable theory to make such an argument puts them in a position indistinguishable from that they're arguing against (relying on belief rather than scientific evidence).

17. **Jeffrey Wolynski**
    August 10, 2017

    The more you write about it, the more popular it becomes. You are feeding the flames. You know the rule about publicity? Even bad publicity is still publicity.

18. **R. Gates**
August 10, 2017

Your point about testability and predictions is a great one but actually, there may be some evidence for evidence for bubble collisions:

https://phys.org/news/2017-05-cold-sky-collision-parallel-universe.html

19. **The Observer**
August 10, 2017

Well, you are right that Popper's academic children and the "science is a cultural construct" movement cannot really be the cause of this. They are too far separated as phenomena. It would be better had I put it that these separated phenomena have a common cause. That cause is the failure of belief in truth and the substitution of relativism.

Science has always been a phenomenon dependent on experience. I believe that it was called "speculative science" long before it was called "natural philosophy", long before Galileo, and that was exactly right, because induction is speculative. But, from the bottom side the relativists about truth have attacked truth and want to mold science as being something that, as a "cultural construct", they can reconstruct as they see fit. Does that not sound like exactly the same thing as, from the top, those in love with untestable visions of their own seeking to make it legitimate to "extend" science into a subject no longer dependent on experiment but something in which being "the only game in town", for example (in their own opinions), makes it desirable to call the creations of their own minds, divorced from experience, "science". They may be brilliant, but they have gone relativist on the ancient understanding of speculative science as something based, not on pure creativity, but experience.

Is this not, really, exactly the same phenomenon played out from below and from above.

I think that science is in danger of being crushed by a pincer movement between the relativists below, who are in the process of forming up for a politically powerful attack on science, and the relativists above, who want to reconstruct science to suit their own varius relativist redefinitions of science.

Another way to put this is: We have gone crazy.

You are doing really great and important work, but you sound a little discouraged. I would like to encourage you by suggesting strongly that you hang in there and remain the beacon of sense that you have been for many years now.

We need you.

20. **Peter Woit**
August 10, 2017

R. Gates,
These bogus claims are just endless, I've wasted far too much of my time trying to debunk them here over the years, now think that's just hopeless. It's always the same thing:

1. Scientific paper with no evidence at all for a bubble collision
https://arxiv.org/abs/1704.03814
2. Press release from university mentioning "possible evidence for multiverse"
https://www.ras.org.uk/news-and-press/2982-new-survey-hints-at-exotic-origin-for-the-cold-spot
3. Loads of press stories like the one you link to.

Everyone involved in this bullshit should just be ashamed of themselves.

Looking back, I think one reason I didn't mention this particular example on the blog is that others have done so, see

https://telescoper.wordpress.com/2017/05/19/a-spot-of-hype/

21. **Peter Woit**
August 10, 2017

Some more related to the last comment:

I wish more physicists/cosmologists/astronomers would start to do what Peter Coles did in that case. It really is part of the job of anyone in those fields who cares about the public understanding of their science.

One reason I wish this would happen is that I'm sick and tired of wasting my time on this, doing a job others should be doing. In addition, note that the reason I found the Coles blog entry was that it is linked as a trackback to the arxiv paper that started this. Trackbacks to any of my blog entries are banned by the arxiv. Historically one of the main reasons for this is that I seem to have upset Joe Polchinski by a blog entry criticizing a Bousso/Polchinski article promoting the multiverse. He then went on a campaign to ensure that my blog entries criticizing multiverse papers would never show up as links at the arXiv. So, best when others deal with this.

22. **Jeff**
August 13, 2017

The Atlantic has an interesting article (https://www.theatlantic.com/amp/article/534231/) discussing the way in which American society in general–across political, religious, and ethnic lines–has elevated personal fantasy over objective reality as "the truth". It describes both how origins of this shift can trace back to left-leaning academia as well as elements in mainstream left politics (though this is not a "both-sider" article; it clearly draws out that regarding actual party leaders, platforms, and propaganda, the American right is far deeper into fantasy than the centrist/left).

But while reading the article, I kept thinking back to this blog, and this post in particular. The article touches on domains and personality types where magically thinking hasn't taken hold: physical/empirical sciences, rationalists, the a-religious/a-spiritual. Sadly, I think the author overestimates how immune even those areas are to magical thinking.

23. **Peter Woit**
August 14, 2017

Re previous comment:

It seems that the arXiv has changed its policy and is now allowing trackbacks to my blog entries. No idea what changed or when this happened, seems to have been within the past few months.

24. **Peter Coles**
August 15, 2017

Thanks for your comments about my post at `In The Dark'.

Sadly it would be full-time job keeping up with all the misleading cosmological hype going around so I've let quite a few other papers slip!

25. **AcademicLurker**
August 16, 2017

No idea what changed or when this happened, seems to have been within the past few months.

The arxiv had their big user survey recently. Maybe there was a significant pro-Woit vote…

26. **Shantanu**
August 16, 2017

Academiclurker and Peter,
FWIW, that's exactly one of the feedbacks I gave to arxiv readers.

GR=QM?

August 10, 2017
Categories: Uncategorized, Wormhole Publicity Stunts

In recent years a hot topic in some theoretical physics circles has been the 2013 "ER=EPR" conjecture first discussed by Maldacena and Susskind here. Every so often I try and read something explaining what this is about, but all such efforts have left me unenlightened. I'm left thinking it best to wait for this to be better understood and for someone to then produce a readable exposition.

Instead of that happening, it seems that the field is moving ever forward in a post-modern direction I can't follow. Tonight the arXiv has something new from Susskind about this, where he argues that one should go beyond "ER=EPR", to "GR=QM". While the 2013 paper had very few equations, this one has none at all, and is actually written in the form not of a scientific paper, but of a letter to fellow "Qubitzers". On some sort of spectrum of precision of statements, with Bourbaki near one end, this paper is way at the other end.

Susskind starts out:

> It is said that general relativity and quantum mechanics are separate subjects that don't fit together comfortably. There is a tension, even a contradiction between them—or so one often hears. I take exception to this view. I think that exactly the opposite is true. It may be too strong to say that gravity and quantum mechanics are exactly the same thing, but those of us who are paying attention, may already sense that the two are inseparable, and that neither makes sense without the other.

I just finished writing a book about quantum mechanics, and it all seemed to me to make perfect sense without invoking gravity, but as explained above I guess I'm one of those who is not (successfully) paying attention. Another route to understanding would be to focus on the new experimental implications of the ideas. In the abstract Susskind claims that his ideas imply that we'll observe quantum gravity using quantum computers in a lab "sometime in the next decade or so". When that happens maybe this will all become clearer.

Update: Sabine Hossenfelder has a commentary on the paper here.

Comments

1. **Vik**
   August 10, 2017

   Dr. Woit,

   I'm very interested in your QM book, but I prefer bound books. Will the book be on sale?

   Thank you.

2. **Peter Woit**
   August 10, 2017

   Vik,
   Yes, I see that Springer now has a publication date of December 4.

3. **Vik**
   August 11, 2017

   "Susskind claims that his ideas imply that we'll observe quantum gravity using quantum computers in a lab 'sometime in the next decade or so'."

   Actually, you misquoted him. In his abstract, he says, "…about the possibility of seeing quantum gravity in a lab equipped with quantum computers. I expect this will become feasible sometime in the next decade or two."

   So, he uses the word "possibility" and "feasible", not "observe" as you wrote.

4. **Richard Gaylord**
   August 11, 2017

   "I'm left thinking it best to wait for this to be better understood and someone produce a readable exposition.". poorly written.

5. **Anonyrat**
   August 11, 2017

   Seems like a reductio ad absurdum proof that AdS/CFT is wrong.

6. **Anonyrat**
   August 11, 2017

   Or a side effect of the legalization of marijuana.

7. **Cobi**
   August 11, 2017

   "On some sort of spectrum of precision of statements, with Bourbaki near one end, this paper is way at the other end."
   As you mention yourself, it is not a paper but a "letter to colleagues" or, in more conventional categories, an essay.
   To take an essay as evidence that the field is moving into a "post modern" direction is somewhat dishonest.

8. **ttt**
   August 11, 2017

   So, he uses the words "possibility of seeing" , not "observe" as you wrote.

a difference without a distinction

9. **Peter Woit**
August 11, 2017

Cobi,
It's not a popular essay aimed at non-experts, thus making precise statements inappropriate. It's a letter to other experts, no reason it shouldn't contain precise statements. If you think there are any in it, perhaps you can explain what they are?

10. **atreat**
August 11, 2017

In the original ER=EPR, Susskind and Maldecena described their highly speculative conjecture like this: "In fact, we are going to take the *radical position* that in a theory
of quantum gravity they are inseparably linked, even for systems consisting of no more
than a pair of entangled particles."

I am not aware of a single experimental result that helped validate this "radical position" and yet Susskind in this latest paper suggests to others to take this "radical position" as given and then adds on another *even more radical* speculation. I think this is perhaps what Peter is referring to when he describes this research program as post modern.

Given this paper, I'm curious what this says about Susskind's String Theory work for all those years. If GR=QM it would seem Susskind now believes that String Theory is superfluous.

11. **Peter Woit**
August 11, 2017

atreat,

Good question about what the implications are for string theory.

The "post-modern" remark is less about these conjectures being "radical" than them being incomprehensible. I quite seriously have no idea whether Susskind has a new important idea that I'm not understanding because I'm not familiar with all the background and assumptions of those that he's writing for, or whether he has just stopped making sense.

12. **sdf**
August 11, 2017

How is a commentary without any precise statements acceptable as a submission to arXiv?

Apropos, would this have been regarded as acceptable as a submission to arXiv if the author's name was not Leonard Susskind or another well-known physicist?

13. **Jack Morava**
August 11, 2017

cf perhaps
https://en.wikipedia.org/wiki/Exploits_and_Opinions_of_Dr._Faustroll,_Pataphysician

14. **Daniel Hogg**
August 11, 2017

As someone who has worked in a quantum computing lab (on the experimental side), I find myself in much the same boat as Peter, being unable to confirm whether I'm not understanding Susskind's ideas, or if the ideas themselves make no sense. What I do know is that for all of the hype in the media, quantum computing is still quite primitive. Decoherence is a major obstacle, the 'computers' (if you can call them that) that we create are limited to a very small number of qubits. The papers that do produce results claiming tens of thousands of qubits seem to have fatal flaws such as a lack of distributed multipartite entanglement. So an attempt to piggyback quantum gravity onto the current 'revolution' in quantum computing needs to respect that the field is still quite limited in what we can measure/accomplish in the laboratory setting.

From the experimental side of things, what seems to be missing is an effect that is being proposed that could be measured under laboratory conditions. People in the field of quantum optical computation do not currently think about gravity because there aren't any observable interactions that we can test for (that we know of, yet). If Susskind wants to propose something that experimentalists can look for, great. As written, it comes across as too speculative.

15. **Blake Stacey**
August 11, 2017

The "ER = EPR" conjecture has the advantage that, if true, it would instantly imply the answer to one of the Millennium Prize problems, at least in the case of large N.

16. **Jeff M**
August 11, 2017

Well, I'm with sdf, how is this a legitimate submission to arXiv? Is it possible Susskind will submit it to a journal? If he does, and it's accepted, that would strike me as a very clear sign that HEP is done. On another note, Sabine is hysterical.

17. **Mitchell Porter**
August 11, 2017

EPR->ER means that where there is entanglement, there should be wormholes. There are recent papers arguing in some detail that quantum teleportation is teleportation "through the wormhole".

Susskind apparently wants people who work in quantum information to get used to the idea that quantum logic gates, etc, involve actual wormhole geometries. And the experimental frontier that he hopes to see breached, is (I think!) the creation of micro black holes *simply through the manipulation of entanglement*.

18. **Peter Woit**
August 11, 2017

Blake Stacey,
It's news to me that there's a precise formulation of ER=EPR that one could imagine proving and that would imply (I assume this is the claim) P not equal to NP. What's a reference for this?

Mitchell Porter,
I don't understand this at all. Qubits, entanglement and quantum logic gates I thought could be described precisely by QM with no need for wormholes. Is Susskind claiming that we'll see some violation of QM that indicates quantum gravity, or will what we see precisely follow QM, with Susskind's quantum gravity effects some sort of interpretation that those of us not paying attention can just ignore (and that has no known connection to actual gravity?).

19. **David Urbanik**
August 12, 2017

The "ER=EPR" solves P vs. NP comment seems like an obvious joke. If "ER=EPR", then one can cancel the E and the R to find that P = 1, so we can only have P = NP if N=1, hence showing P is not NP for "large N".

20. **Peter Woit**
August 12, 2017

Richard Gaylord,
Thanks. Improved.

21. **srp**
August 12, 2017

Peter asks: "Is Susskind claiming that we'll see some violation of QM that indicates quantum gravity, or will what we see precisely follow QM, with Susskind's quantum gravity effects some sort of interpretation that those of us not paying attention can just ignore (and that has no known connection to actual gravity?)."

From the paper: "A skeptic may argue that all of this can be explained without ever invoking bulk gravity or wormholes; plain old quantum mechanics and some condensed matter physics or quantum circuitry is enough. This is absolutely true, but I think it misses the point: Theories with gravity are always holographic and require a lower dimensional non-gravitational description. This does not mean the bulk world is not real."

22. **Friedwardt Winterberg**
August 13, 2017

GR=QM is impossible because from G(Newton's constant) and c(velocity of light) one cannot get h(Planck's constant).

23. **Blake Stacey**
August 13, 2017

Peter Woit:

Sorry, I was just making a silly joke about canceling the E and the R.

24. **Blake Stacey**
August 13, 2017

… because I work in quantum information, and I tried valiantly to get some sense out of the "ER = EPR" claim, and after all that, the best I think it deserves is silly jokes.

25. **Peter Woit**
August 13, 2017

srp,
Thanks for pointing that out.
Rereading the paper with that clarified, I think I'm finally beginning to see what Susskind is up to. By "QM" he means QM, but by "GR" he means not GR, but "hypothetical dual to QM system that we know almost nothing about" (which for some very specific QM systems in a special limit is related to GR in one dimension higher).

Those of us who aren't in the mood for completely ill-defined flights of fancy can safely continue to not pay attention. As for where this is going, it looks to me like the most optimistic scenario is that some better understanding of this kind of duality will lead to some useful approximate calculational method for some particular very complicated QM systems (by relating something in them to something in a GR calculation in one-dim higher). Then we'll see lots of new university press releases and stories in the press about "wormholes created in the lab", with claims that this is "evidence for string theory".

More interesting of course would be if this duality could be read the other way, explaining physical 4d quantum gravity in terms of a well-defined dual QFT. I don't see any indication of that happening, and Susskind's claims don't seem to have anything to do with it.

26. **Lee Smolin**
August 13, 2017

Dear Friedwardt Winterberg:

An excellent point. But you can get h from G and c if there is also assumed a fundamental length or energy scale, and vise versa. So one way to think about this could be that a QFT (h,c) whose symmetries preserve a fundamental scale (E_p), has G and hence gravity coded in it.

Lee

27. **David Roberts**
August 13, 2017

To stretch an analogy, this is the kind of thought experiment that Einstein described that lead him to the principle of equivalence and thence to GR. Except he didn't release a paper describing his thoughts about what it would be like to ride a beam of light or an elevator – he spent time learning new mathematics and wrote up a serious theory with predictions that were verifiable, and then verified.

28. **Blake Stacey**
August 13, 2017

Peter Woit (August 13, 2017 at 4:45 pm):

Your “most optimistic scenario” pretty much agrees with the most optimistic evaluation I could make. A geometric way of calculating entanglement properties in many-body systems, let’s say—that could be useful. But I strongly suspect that the further one goes down that path, the less it will look like gravity in any way, and the hope of “building spacetime out of entanglement” (to put it the way a flashy keynote at PI would) will remain as distant as ever.

29. **wolfgang**
August 15, 2017

Peter,

>> this kind of duality will lead to some useful approximate calculational method

as I understand it, an important point he makes is that the distinction between the quantum-computer CFT side and the AdS bulk part is arbitrary.
If the quantum computer(s) are sufficiently large, they may be able to communicate with observers inside the bulk.

Of course one could turn this around (I dont think he does this yet) and argue that we are just the bulk description of somebody else’s quantum computer.

30. **Mauro Claudio**
September 22, 2017

Peter Woit, have you missed this ?

Amazing statement: “teleportation … after a suitable time”.
A teleportation whit a delay!

https://arxiv.org/abs/1707.04354
Teleportation Through the Wormhole
Leonard Susskind, Ying Zhao

ER=EPR allows us to think of quantum teleportation as communication of quantum information through space-time wormholes connecting entangled systems. The conditions for teleportation render the wormhole traversable so that a quantum system entering one end of the ERB will, after a suitable time, appear at the other end. Teleportation requires the transfer of classical information outside the horizon, but the classical bit-string carries no information about the teleported system; the teleported system passes through the ERB leaving no trace outside the horizon. In general the teleported system will retain a memory of what it encountered in the wormhole. This phenomenon could be observable in a laboratory equipped with quantum computers.

31. **Peter Woit**
September 22, 2017

Mauro Claudio,

Thanks! I somehow missed that one. Surprising that this summer there weren’t a bunch of articles in the press about how quantum computers would open up wormholes suitable for teleportation.

Road Trip

August 14, 2017
Categories: Uncategorized

Blogging will be light to non-existent for the next ten days or so, as I head out west on a road trip to see next Monday's solar eclipse. Current plan is to fly to Denver tomorrow, pick up a vehicle, and head up to Wyoming the next day. If weather projections look good for the Wyoming/Idaho part of the track, that's where we'll plan to end up, likely camping out somewhere (accommodations along the track have long been booked up).

This will be the ninth eclipse I've traveled to see, and I urge anyone thinking of making a trip to the eclipse track to do so. A total solar eclipse is something quite different than a partial one, and this is a very rare opportunity to see this in the US. Besides the eclipse, a major motivation for these trips has always been that of getting to visit a more or less random place on Earth that one wouldn't otherwise have any excuse to see. I've driven quickly through Idaho and Wyoming a few times over the years, look forward to spending more time in that part of the country this coming week (unless the weather there looks bad, in which case maybe we'll end up in Oregon or Nebraska).

Some other random advice about eclipses:

- Be very careful about use of binoculars or telescopes, improper use of these at any time other than the period of totality is what can cause serious eye damage (by itself the eye is pretty good about automatically protecting itself).
- Don't put a lot of effort into photography during totality, since that's likely to lead to you spending the time you should be enjoying the experience fiddling with camera equipment (and not getting a good result anyway…). A simple thing to do is to set up a camera to take video of the overall eclipse scene as it happens, turn it on at some point then ignore it.

If you miss this one, next couple are far south in South America, there will be another chance in the US relatively soon, April 2024.

Update: Now back in New York. Had a very good view of the eclipse from a spectacular location: Stanley, Idaho, up in the Sawtooth mountains. Only not quite optimal part of the plan was camping out not not well-equipped for the the unexpected fact that it gets down to about freezing at night in that part of Idaho, even in August…

Comments

1. **Shantanu**
   August 14, 2017

   Peter , OT.
   TevPA talks at https://www.youtube.com/watch?v=LnjICNLvtj4 and so on
   (this one is a link to Nima's talk)

2. **Gus Bici**
   August 14, 2017

   Me & doggie driving to Idaho Falls from Tucson. Ogden UT was as close as I could get for Sunday night reservations when I booked over a month ago. Will rise early on Monday to get in the totality shadow over Idaho. If it is cloudy oh well – it will be extra dark I guess:)

3. **Abby yorker**
   August 14, 2017

   We just moved to Portlnd, OR, and so I plan to cycle into the totality swath the morning of. Driving a car would likely be unsuccessful. Portland is completely booked for the eclipse.

4. **SB**
   August 14, 2017

   And those experiencing it in an extended flat terrain look carefully for Shadow bands

5. **Chris Kennedy**
   August 15, 2017

   While you are there, if the band you're in starts playing different tunes – run like hell.

6. **Bill_K**
   August 15, 2017

   Darkened glasses are essential during the partial phases of the eclipse, but during totality, when the sun is completely covered, it's safe and desirable to take them off. If you do, the only precaution is to know in advance how long totality will last at your viewing site, so you can put the glasses back on in time!

7. **KWH**
   August 15, 2017

   I commend http://mreclipse.com to your readers, and in particular http://mreclipse.com/Totality3/TotalityCh11.html. My wife and I, who will be in Wyoming, at Glendo State Park on the morning of the eclipse, will use Shade 14 filter plates in flip-up welding goggles. Glendo is a good place to be on eclipse Monday. It's exactly on the centerline of the eclipse path and immediately next to I-25 well south of Casper. The "ticket booth" ($6) opens at 4:30 a.m. I'll be wearing or right next to a large, bright red Osprey climbing pack.

8. **neil**
   August 15, 2017

   Peter

   Be ready for huge crowds around Jackson Hole on eclipse day. Prime territory. I will be in a remote area of eastern Oregon and still expecting crowds.

   In Idaho, Craters of the Moon NM and Hells Canyon NRA are worth visiting.

Have fun and happy viewing.

9. **Koenraad Van Spaendonck**
   August 16, 2017

   Great initiative. The trip has a goal, but the goal is the trip !

10. **Eddie Dealtry**
    August 17, 2017

    Absolutely concur with the second bullet point: don't spend all the time with equipment. Spent most of the 7-minute eclipse (not far short of theoretical max) in 1973 faffing with cameras. Won't do that again.

11. **David**
    August 23, 2017

    Thanks for the advice to not worry about trying to photograph it. My wife, son, and I were in Oregon and I just set the camera to record us as we watched. The result was probably the most memorable video that I've ever made, even though the eclipse itself is never seen.

12. **Jeff Moreland**
    August 23, 2017

    Had a great view in Aurora, Nebraska, hope you had clear skies.

13. **Jerome Moore**
    August 26, 2017

    Saw it with my kid near Columbia, Missouri. Good weather and really spectacular. There were special events hosted by the university.

    @Bill_K: I was worried about that but it turns out not to be ambiguous at all. You see the flash when the sun starts to emerge, you know its time to put the eclipse shades back on.

14. **Bugannoyer**
    August 26, 2017

    I was in Madras, OR at "SolarFest", camping in the "SolarTown" tenting sites. What an amazing experience! The crowd's energy was exhilarating!

    Was slightly hazy/high cirrus clouds, but this turned out not to matter much for seeing the corona and flares. We had an Orion AstroView 120mm F5 scope with an 82 deg 44x lens that captured almost all the corona, plus 70mm 15x binocs on a tripod, and some handheld binocs (all with solar filters).

    Saw Bailey's Beads as totality exited, and pulled eyes away from the lens of the Astro View, slapped the solar filter back on the scope. One thing that was striking was how well the eye captured the solar flares (a lot of detail), compared to the best photos I've seen online. My guess is this is attributable to the eye having the separate cone receptor types, so that the red in the flares was probably selectively perceived/enhanced by the visual system. I'd be interested if anyone could say more about that.

15. **filtz**
    August 26, 2017

    Bugannoyer,

    It is safe to view totality with binoculars or telescope with proper solar filter?

    "how well the eye captured the solar flares" – Are you talking about naked eye, or eye + telescope/binoculars?

    Where did you get your Orion AstroView and binoculars? Would you recommend that telescope as a first telescope for an adult astronomy enthusiast?

    Any good resources for photographing/viewing solar eclipse? What solar filter did you use? Thank you for your post!

16. **Bugannoyer**
    August 27, 2017

    Filtz, telescopic product comment is probably OT, and so I'll be brief on that, and we should not be surprised if our moderator deletes. But in that regard, I can recommend the Orion AstroView 120st as a good value/quality-to-price, medium-range scope, which can be upgraded to a 2″ focuser and eyepieces (I'm just using the scope as equipped, with 1.25″ lenses). One caveat is the mount (German Equatorial) is heavy, so only suitable for *car* camping. I bought online.

    The 70mm binocs are Celestron, and also very good value/quality-to-price, although in this case my price was $0. They were a prize my daugher won at an Oregon Star Party a few years ago.

    At totality you view without solar filters or glasses. The brief Bailey's Beads / Flash / Diamond Ring I saw through the AstroView at 3rd contact (without filter) was probably not dangerous because of how quickly I pulled my eye away from the lens.

    The solar filters I used (when *not* at totality) I made myself from Baader Solar Film, again ordered online. You can find instructions for how to construct solar filters using this or other certified films online.

    I am not an astrophotographer myself, but I'd recommend Sky and Telescope as a good place to start; there are a ton of online resources as well.

17. **John**
    August 28, 2017

    We drove with one of the grandkids from Las Vegas to Mackay, Idaho to see the eclipse. Two minutes and 13 seconds of totality. It was amazing. Indescribable. Words and pictures don't do it justice. You really have to experience a total eclipse to understand.

18. **Nick M.**
    August 29, 2017

    Peter,

In addition to the 8th of April 2024 *total* solar eclipse, there will be an *annular* solar eclipse on the 14th of October 2023.

For 2023 see Solar Eclipse 2023,
and for 2024 see Solar Eclipse 2024.

This would be a great opportunity to witness the two types of solar eclipses that the sun and moon can put together, just one half year apart from each other, on the North American continent.

19. **Tom Dickens**
August 29, 2017

We traveled to Greenville, SC, met some family members, and had 2 min 6 sec of totality. Beautiful sight, first total eclipse we have seen.

I took pictures using an EOS 80D with 300 mm lens with Thousand Oaks filter during partial phase. Should have taken advice not to try in totality, because I missed about half of it when I kept taking pictures, and got confused by the enormity of the event! I did get a couple of decent ones..

Next time I will just watch.

This and That

August 29, 2017
Categories: Uncategorized

- The Stacks Project (see an earlier post here) had a very successful workshop in Ann Arbor earlier this month. This is a remarkable effort pioneered by Johan de Jong to produce a high quality open source reference for the field of algebraic geometry. It now is over 6000 pages, with an increasingly large number of papers citing it (according to data from Pieter Belmans, 85 citations in the arXiv so far in 2017 alone). During the workshop plans were discussed for the future of the project, with work on a new version of the project infrastructure underway (see slides and a blog post from Belmans).
- The latest AMS Notices has a wonderful article by my Barnard/Columbia colleague Dusa McDuff about her remarkable family history and reflecting on her equally remarkable mathematical career. A post earlier this year discussed a Quanta article about her recent work with Katrin Wehrheim on technical issues in the foundations of symplectic topology. Kenji Fukaya has recently written something for the Simons Center website (see here) explaining his take on this story.
- The Stanford Encyclopedia of Philosophy has a new entry about the fine-tuning problem, by Simon Friedrich.
- The LHC operators have run into some difficulty in recent weeks (reflected in the accumulated luminosity plots here and here), with problems centered around an unknown source of gas in the beam pipe at a specific location, leading to losses of the beam. Some information about this is available here. The past few days they seem to be having success running the machine with around 1500 bunches, much less than the 2500 or so of earlier in the summer. The target for the year is 40 inverse fb which may still be achieved, while more optimistic numbers that looked plausible earlier now seem less likely.

Update: Joe Polchinski has put on the arXiv a long autobiographical document, with a detailed discussion of his scientific career.

Update: As mentioned in the comments, Go Yamashita has posted a long document surveying Mochizuki's claimed proof of the abc conjecture. Experts may find that this makes it more possible to understand and check the claimed proof, we'll see.

Update: Also at the Simons Center website, there's an interview with Michael Green. It's interesting to see that in recent years his research interests have led him to getting closer to mathematics and to an appreciation of what mathematicians do. As for his claim about string theory that

> I don't think there is a substantial antagonism to it among those who have studied it, other than a few individuals who enjoy publicizing their views.

I think he's quite wrong if you properly take "it" to refer to the aspect of string theory there is widespread antagonism to among physicists, the overhyped claims about a unified theory based on string theory.

Comments

1. **Shantanu**
   August 30, 2017

   Peter any comments about Nima's talk at TevPA?

2. **neil**
   August 30, 2017

   The SEOP entry on Fine Tuning was very interesting and helpful. Thank you for posting the link.

3. **paddy**
   August 30, 2017

   I second neil's thanks.

4. **Klaus**
   August 30, 2017

   Peter,

   Polchinski writes about himself: "The three times that I shook up the field – D-branes, the string multiverse, and firewalls – might give you the impression that I am a radical, but it is not by design." I never met him, but are these three achievements really his main legacy?

   Polchinski also writes in the text that he got "brane cancer". What an unfortunate lapsus. Somebody should let him know.

5. **Anonyrat**
   August 31, 2017

   I didn't see any reference to this: the not-so-fine-tuned universe:
   http://cosmos.nautil.us/feature/113/the-not-so-fine-tuning-of-the-universe

6. **sdf**
   August 31, 2017

   Apropos a previous blogpost on this website, the survey of Yamashita on work of S. Mochizuki has appeared http://www.kurims.kyoto-u.ac.jp/~gokun/DOCUMENTS/abc_ver6.pdf 294 pages and claims to be self-contained.

7. **Peter Woit**
   August 31, 2017

   Klaus,
   My guess is that "brane cancer" is intended as humor on Polchinski's part. I wish him well and he's entitled to some levity about the difficult situation he is in.

   Yes, I think it's right that those are the three things he has done that have gotten the most attention and had the most impact. Unfortunately for one of them (moduli stabilization/string theory landscape/multiverse) I think the impact has been large and negative and, away from the West Coast, I suspect many of his colleagues agree with me. His discussion in his memoir of how this work affected him is remarkable.

8. **JY**
August 31, 2017

BTW, (maybe you have already known this:-)) Weinberg is going to give a talk to celebrate 50 years of SM https://indico.cern.ch/event/649760/timetable/ . Kind of curious about what he is going to say in the talk

9. **Bill**
August 31, 2017

I found Section A.4 on pages 265-266 of Yamashita's manuscript quite fascinating.

10. **Tom**
August 31, 2017

Philosopher here. I'm curious how many mathematicians, if any, suspect that Mochizuki is perpetuating a hoax. I am thinking of all the new names M. gives to concepts that are already perfectly well denoted (e.g. isomorphism); all the jargon, generally; the way a proof in one paper relies on a result from a second, which uses a definition from a third, which relies on a lemma from the first one–i.e. all the circularity; the apparent unwillingness on M.'s part to make the argument perspicuous for others; and the fact that experts are unable to understand the basic strategy of the proof. None of these features is dispositive of a hoax, but they are worrying.

Do mathematicians think that? Or is the general sentiment that M.'s work really is too brilliant and cutting-edge to understand? Or is M. likely mistaken, but working in good faith? Are mathematicians really just at a loss about what to think?

11. **Peter Woit**
September 1, 2017

Tom,
I don't know of any mathematicians who think it's a hoax, there's nothing at all to indicate that. All indications are that it's a complex argument, based upon ideas involving a lot of new mathematical constructions. The question is whether these new constructions do what Mochizuki argues that they do. Surely he is convinced of this, but others need to understand the new constructions well enough to see whether the arguments are airtight. This is a very unusual situation: usually by now other experts would have assimilated the new ideas well enough to check them, or to identify their weaknesses. Perhaps the Yamashita survey will help move this along. I think it's fair to characterize the current situation as "at a loss about what to think".

12. **sdf**
September 1, 2017

S. Mochizuki has a good pedigree and has done first-rate mathematics in the past. Furthermore there are other first-rate mathematicians who believe he is on to something. So it is rather hard to completely ignore his work, despite his unwillingness to engage properly with the community.

13. **Yatima**
September 2, 2017

Quanta Mag has a popular-science article on efforts of getting QM from a few axioms:

Quantum Theory Rebuilt From Simple Physical Principles

I remember Lucien Hardy's
Quantum Theory From Five Reasonable Axioms, and that dates back to even before the Forever War.

Axioms are not "physical principles" but "syntactic principles" though. You can just feed them to a TM-based deduction system (or better) w/o physics in sight.

QM rebuilds seem to be not particularly hard, what about QFT rebuilds?

14. **william e emba**
September 5, 2017

Spaghetti-code mathematics is, unfortunately, quite common at the highest levels. It can easily take a decade or two or three before important work gets understood, an intelligent organization is found, the proofs are actually verified, and so on. The recent Quanta article on McDuff's attempts to make sense of the foundations of symplectic geometry could have been rewritten by changing the names and theorems around.

No need to invoke *abc* paranoia.

15. **anon**
September 6, 2017

Regarding Yamashita's survey: I doubt it does very much to improve understanding of Mochizuki's work. Especially in the latter part of the survey proofs of theorems consist only of "follows from the definitions", while the theorem statements can be several pages long!

16. **Andrew**
September 7, 2017

Peter,

As far as you know, are there plans for the LHC to be upgraded in luminosity and/or energy before it shuts down completely? If so, is there already a timeline established for when these will arise and factors of increase for either luminosity and/or energy?

17. **Peter Woit**
September 7, 2017

Andrew,
There are plans to upgrade the luminosity of the LHC significantly (the "HL-LHC") and the energy slightly (from 6.5 to 7 TeV/beam), these plans go out to 2035 of so, see here:

http://lhc-commissioning.web.cern.ch/lhc-commissioning/schedule/LHC-schedule-update.pdf

I don't know of any further ideas about higher luminosity. There are post-HL-LHC plans ("HE-LHC") for a possible doubling of the energy of the beams. This would require developing new magnets and replacing much of the current LHC with them. Unclear how likely that is to happen. It would be expensive, but much less than other proposals which would require building a new, larger tunnel.

18. **asdfasdfff**
September 12, 2017

Why doesn't someone set up a wiki and have a group of mathematicians go through the Mochizuki/Yamashita presentation and confirm the theorems one by one. When they get to one they don't agree with or understand, just say "unverified". I understand this is a long proof, but where is the consensus on what's wrong exactly? I can't believe it's taking this long to get a clear answer.

19. **Peter Woit**
September 12, 2017

asdfasdfff,
Unfortunately the problem seems to be that Mochizuki's proof is written up in a form spread over many papers, very hard for anyone to go through and check step by step. One point of view on the current situation is that it is his responsibility to rewrite the proof in a form such that experts can do this. The Yamashita survey is supposed to provide a roadmap to the Mochizuki papers, outlining the logic of the proof. It is only recently available, presumably some people are trying to go through it as you suggest.

My impression from what I hear is that some of the experts who have tried to understand the proof have gotten stuck at more or less the same point. Perhaps the Yamashita survey clarifies that point, maybe not. A significant part of the problem here is a breakdown in the usual way such a situation normally gets resolved: communication between experts and the author of the proof, providing explanations satisfactory to the experts. Achieving this was a goal of a couple workshops, but these did not reach this goal, and I don't know what the current situation is for more attempts.

20. **David Roberts**
September 12, 2017

The Yamashita paper has done at least one very useful thing: it's collected the proof that one only needs to consider a very special case to prove the whole Szpiro conjecture (which implies abc). This proof (due to Mochizuki) was until now spread over several of his papers, and in Yamashita's write-up uses standard-looking arithmetic/anabelian geometry. Moreover, Yamashita provides a statement (beyond the inequality the Szpiro conjecture reduces to) that uses quite standard terminology that the IUTT papers are aiming to prove. It's small progress like this that will chip away at the monolith that is IUTT, which unfortunately is very smooth and hard.

Modern Geometry

September 4, 2017
Categories: Uncategorized

This semester I'm teaching the first semester of Modern Geometry, our year-long course on differential geometry aimed at our first-year Ph.D. students. A syllabus and some other information about the course is available [here].

In the spring semester Simon Brendle will be covering Riemannian geometry, so this gives me an excuse to spend a lot of time on aspects of differential geometry that don't use a metric. In particular, I'll cover in detail the general theory of connections and curvature, rather than starting with the Levi-Civita connection that shows up in Riemannian geometry. I'll be starting with connections on principal bundles, only later getting to connections on vector bundles. Most books do this in the other order, although [Kobayashi and Nomizu] does principal bundles first. In some sense a lot of what I'll be doing is just explicating Kobayashi and Nomizu, which is a great book, but not especially user-friendly.

A major goal of the course is to get to the point of writing down the main geometrically-motivated equations of fundamental physics and a few of their solutions as examples. This includes the Einstein eqs. of general relativity, although I'll mostly be leaving that topic to the second semester course.

Ideally I think every theoretical physicist should know enough about geometry to appreciate the geometrical basis of gauge theories and general relativity. In addition, any geometer should know about how geometry gets used in these two areas of physics. I've off and on thought about writing an outline of the subject aimed at these two audiences, and thought about writing something this semester. Thinking more about it though, at this point I'm pretty sick of expository writing (proofs of my QM book are supposed to arrive any moment…). In addition, I just took a look again at the 1980 review article by Eguchi, Gilkey and Hanson (see [here] or [here]) from which I first learned a lot of this material. It really is very good, and anything I'd write would spend a lot of time just reproducing that material.

Update: Steve Bryson sent me another excellent suggestion for a book covering these topics, aimed at the physical applications: David Bleecker's [Gauge Theory and Variational Principles].

Comments

1. **Jeff M**
   September 4, 2017

   Peter

   If you don't know it, take a look at Milnor's book on Morse Theory. In about 40 pages, he covers essentially everything anyone needs to know about Riemannian geometry. Milnor is a wonderful expositor. When I decided I wanted to do a thesis in geometric analysis, having never taken a differential geometry course (actually having never taken a geometry course of any kind, I went to a weird high school), that's where my advisor pointed me.

2. **CIP**
   September 4, 2017

   Have you looked at Thorne and Blandford's Modern Classical Physics, which is all physics but with a strong geometric orientation?

3. **Peter Woit**
   September 4, 2017

   Jeff M and CIP,

   The books you refer to emphasize classical Riemannian geometry, and I'm leaving that to Brendle. From that point of view about geometry, you really can't understand two of the most fundamental geometrical structures in modern physics:

   1. gauge fields, which are connections, but on some general principal bundle, which has nothing to do with the tangent bundle.

   2. spinor fields, which can't be constructed just using a vector bundle (the tangent bundle or its tensor bundles). To get spinors, one way is to use principal bundles: consider the principal bundle of orthonormal frames of the tangent bundle, then find a spin double cover, use the spin representation to get spinor fields (as an associated vector bundle).

   What's remarkable is that the general story about connections and curvature in Kobayashi/Nomizu was developed with completely different motivations (that of understanding the relation of geometry and symmetry groups), ended up miraculously being exactly the right formalism for fundamental physics. This is a story both physicists and mathematicians should know about.

4. **quasihumanist**
   September 4, 2017

   This is of course, not directed at you or your course, since I've never met you. However, in general, one problem many physicists have with talking to the general (pure) mathematical audience today is that they assume too much knowledge of differential equations.

   I am an extreme example, but all my knowledge of differential equations comes from teaching the standard first undergraduate course on linear ODEs, and I learned that by TAing the course, not by ever having taken it. If pressed, I might be able to recall the solution to the heat equation.

   Worse yet, as an algebraist, I usually think of a (partial) derivative as an abstract operator on elements of an algebra (over a field) that is linear, satisfies the Leibniz rule, and sends elements of the ground field to 0. Never mind limits or all that. (Actually, I'm a combinatorial algebraic geometer, which means I have colleagues tell me about statistical mechanics blah blah blah and other colleagues tell me about symplectic blah blah blah, none of which I really understand.)

   If you start talking about the Lagrangian or Hamiltonian formulation of classical mechanics, you've already lost me.

   So – this is a request for you to be nice to those mathematicians – but it's also a request for recommendations for me to pick up some of the physics point of view on differential geometry.

5. **murmur**
   September 4, 2017

Off-topic: Have you seen the autobiography of Polchinski: https://arxiv.org/abs/1708.09093?

6. **Peter Woit**
September 4, 2017

murmur,
See previous posting and comments there.

7. **Peter Woit**
September 4, 2017

quasihumanist,
The main problem with understanding gauge theory and GR, for both mathematicians and physicists, is that the differential geometry needed is rather sophisticated, and often not taught as part of the standard math curriculum, even at the graduate level. If you are comfortable with Riemannian geometry, GR is not hard. Purely as differential equations, the Einstein equations in coordinates are very complicated PDEs, but they have a fairly straightforward description in terms of the Riemann curvature tensor.

8. **CIP**
September 4, 2017

Are you thinking of writing up your notes as a sort of K&N for physicists?

9. **CIP**
September 4, 2017

Oops, sorry, I forgot that you already answered this.

10. **Thomas Larsson**
September 5, 2017

Ah. I have always liked the tensor calculus centipede being intoxicated by a plethora of indices. Although if you want the full expressiveness of tensor calculus in index-free notation, you would be intoxicated by a plethora of definitions instead.

11. **Brian Dolan**
September 5, 2017

As a physicist I too learned most of my differential geometry from Eguchi, Gilkey and Hanson's review. Kobayashi and Nomizu is a beautiful book which I now appreciate but I found it frustrating when I was learning the subject and it took me many years to understand why — it is deceptive because they prove some of the most beautiful theorems in 2 lines. The real work goes into many pages of definitions which are given almost without motivation. I guess this is a standard pure mathematics style, but I don't find it useful pedagogically. If Kobayashi and Nomizu is a work of art, Eguchi, Gilkey and Hanson is a box of paints!

12. **Jess Riedel**
September 5, 2017

"I just took a look again at the 1980 review article by Eguchi, Gilkey and Hanson…It really is very good, and anything I'd write would spend a lot of time just reproducing that material."

Think about how much easier this would be if the norm was for physicists to release all their work under a license that allowed re-use with attribution (e.g., Creative Commons ShareAlike). You could just immediately start building.

13. **Thomas**
September 5, 2017

To me, the main disconnect is that there is an extensive physics literature on instantons, monopoles, and other topological phenomena, in which many interesting phenomena are computed (instanton contribution to effective lagrangians and the OPE, axial charge diffusion in an EW plasma, defect formation in phase transitions, baryon number violation, etc), and then there is a mathematical (or mathematical physics) literature in which a beautiful formalism is laid out (bundles, forms, etc), but nothing is really computed (or if something is calculated it is done by choosing coordinates, and writing things out in components). The only case that I am really aware of where, historically, sophisticated tools played a role is the ADHM construction, although even in that case these days it is usually presented as a clever ansatz for the gauge potentials. What would be nice is a review where one can really see the power of sophisticated methods in doing calculations.

14. **Peter Woit**
September 5, 2017

Thomas,

I don't know of a review of the kind you want, maybe someone else can suggest something. In general though, I think the power of the abstract geometrical formalism is that it tells you what the general coordinate independent features of solutions will be. Sometimes, especially with enough symmetry, you can calculate these things without choosing particular coordinates. But, in any particular case, to actually calculate you may need to choose coordinates, better, coordinates adapted to the problem.

15. **Manfred Requardt**
September 6, 2017

Dear all, I remember the remark by Weinberg in his beautiful book about GR etc., i.e. that he thinks that the value of geometry in e.g. gauge theory is overtestimated. While I think he is not right, there is a grain of truth in his remark. Classical gauge theory as fibre bundle mathematics is certainly beautiful, however when quantizing the occurring fields transforms this into completely different entities. What one perhaps needs is some sort of quantum fibre bundles.

16. **TME**
September 6, 2017

Hey Peter,

After preparing for this course, have you had any thoughts about studying synthetic differential geometry?

17. **Justin**

September 6, 2017

Peter,

What are the pre-requisites for your course in real analysis, algebra, geometry, linear algebra? I've always wanted to study some differential geometry, but my background is limited to linear algebra at the level of Serge Lang, modern algebra at the level of Fraleigh, Calculus at Stewart's level, and some analysis that I vaguely remember.

18. **Peter Woit**
September 6, 2017

TME,
I don't really see the point of that, it seems to invoke a lot of abstract formalism, and get little for it. Definitely not appropriate for students.

I have been intrigued by the idea of formulating differentiable manifolds in a formalism more parallel to the definitions in terms of a sheaf of functions common in algebraic geometry and topology. Looking into this though, if you try and define the "sheaf of differentiable functions", you end up going through the same apparatus as the usual definition, to which you've just added a lot of extra formalism.

Justin,
You should start with an advanced undergraduate course in geometry, specifically one dealing with differentiable manifolds. A good typical textbook is Loring Tu's An Introduction to Manifolds. The course I'm teaching is supposed to be more advanced, assuming an undergraduate course as background.

19. **Eric Weinstein**
September 6, 2017

I'm really glad to see a shout out to EGH which functioned as a 'bible' for understanding the consequences of the Wu-Yang dictionary to which Simons and Singer also contributed. The Eguchi, Gilkey and Hanson 'article' is really an example of a phenomena we don't discuss much but which I think is fairly significant and highly conserved.

This famous article was really more of a book of a very special kind that appeared as a 'paper.' Sometimes, it becomes necessary for an entire technical field to take up a new toolkit; at those moments, it falls to someone or some group to write up the new tools as an enticement to colleagues to change their thinking. For some reason, in these situations, what gets written as a pitch or a sales job is often far clearer than what will later be written to introduce the toolkit to future students. Then, mysteriously, the old text is forgotten as new pedagogical texts attempt to reach students rather than professors. Yet, often, those more 'professional' texts aimed at students are often not as well or as deeply motivated.

As a related example, I believe I found a bound set of dusty notes from Dieudonné on Grothendieck's scheme theory written at some point early in the development of the theory that attempted to convert people used to thinking about Algebraic Varieties to think in terms of Scheme Theory. Strangely, this old book (or set of notes) seemed much clearer and better motivated than the treatment in the leading contemporary pedagogical text of the time by Robin Harthshorne. This aroused my curiosity around a simple question: do people write most clearly when pitching a new toolkit to their colleagues and in a less well motivated way once the professors have been converted?

I have since concluded that there is something magical about the early days of a new toolkit where people are thinking more clearly than they are likely to think later about the motivation for the 'kit'. As a consequence, it is often worth going back and looking for the text(s) which transitioned professors into a more modern viewpoint as they often have far more motivation and clarity than later introductory texts.

The best explanation that I can offer is this: only briefly is there a window to write for senior people (i.e. the most discerning of possible students) without fear of insulting their intelligence and while authors still remember what is counter-intuitive about the new kit. Even a short time later, people forget their beginners mind-set and thus what made the subject counter-intuitive enough to need a motivated pitch so that the new tools would be adopted.

I wish more beginning students would go back to look at those special moments where everything suddenly changed. There are very few of them in any career and each epiphany comes but once.

20. **Magnema**
September 7, 2017

@Eric Weinstein:

The difficulty I've found surround good older texts is converting their ideas to modern notation. As ideas get more solidified, notations (sometimes) improve, and make things clearer. For that reason, I think results are somewhat mixed, as with any pedagogical text. (What I've seen of the early discussion of tensor products falls into this trap somewhat. I'd imagine it'd be less large for more recently-developed fields, though.)

I also wonder if the original paper might benefit from being longer [neglecting problems and the like] for the same material (or, more precisely, the same length for less material). More time to talk about things will improve your writing for free, in some ways, although this is perhaps more true of those who would argue some variant of "read the original literature" than those who support some sort of summary paper.

That said, I do think an "updated [in notation] original" of works of that variety can sometimes be an outstanding pedagogical tool, depending, because I do agree you get better insight into the original motivation by looking at those papers.

21. **Peter Woit**
September 7, 2017

Eric,
That's an interesting comment on the EGH story. I've been kind of surprised to look around not readily find something better than that. I would have expected that over the years many people would write up versions of this material, but there aren't that many (suggestions welcome, does anyone know something that covers the material of EGH, but better?)

In this case I think part of what happened is that the 1984 advent of string theory turned attention to different areas of geometry/physics, so we ended up with people writing expository material not about gauge theory and its geometry, but about geometry of Riemann surfaces (for string world-sheets) and objects like Calabi-Yaus.

Magnema,

In this case it isn't really not-up-to-date notation that is the problem, one of the great virtues of EGH is that they use quite good modern notation.

22. **Cobi**
September 8, 2017

"In this case I think part of what happened is that the 1984 advent of string theory turned attention to different areas of geometry/physics, so we ended up with people writing expository material not about gauge theory and its geometry, but about geometry of Riemann surfaces (for string world-sheets) and objects like Calabi-Yaus."
I have the opposite impression that the mathematical structure of gauge theories is from the standard model perspective "nice to know", but became heavily utilized in the context of string theory.
To give some random examples, consider localization in non-Abelian gauged linear sigma models, the Kapustin Witten story or bundle constructions for heterotic models.
On a slightly different note i would love to understand what insights are to be gained from Urs Schreibers "higher pre-quantum geometry".
I was a bit surprised that "Modern Geometry" means classical differential geometry.

23. **Peter Woit**
September 8, 2017

Cobi,

Yes, the examples you give are random, wildly different sorts of mathematics, connections/non-connections to physics, and no connection to observed physics.

What do you think "Modern Geometry" is? Can you point to a graduate-level mathematics textbook covering whatever you think it is?

24. **Chris Grant**
September 8, 2017

"Have you looked at Thorne and Blandford's Modern Classical Physics, which is all physics but with a strong geometric orientation?"

My initial foray into this book suggests that it is very much written in physicist-speak rather than mathematician-speak.

25. **Bobito**
September 12, 2017

Look at Sternberg's recent textbook (title starts with the word "Curvature"). He makes some effort to relate differential geometry to physics.

26. **santarakshita**
September 13, 2017

Since you mention Loring Tu in one of your comments, I'd like to point out that Tu has a new textbook out on differential geometry. It seems to cover the kinds of things you want to touch upon (connections on principal bundles).

Modern Theories of Quantum Gravity

September 7, 2017
Categories: Uncategorized

Quanta magazine today has a [column by Robbert Dijkgraaf] that comes with the abstract:

> Reductionism breaks the world into elementary building blocks. Emergence finds the simple laws that arise out of complexity. These two complementary ways of viewing the universe come together in modern theories of quantum gravity.

It struck me that at this point I don't know what a "modern theory of quantum gravity" is. Much of the article is a clear explanation of the usual story of the renormalization group and effective field theory, but towards the end, when quantum gravity comes up, I have trouble following. String theory has gone from being an exciting new idea to being part of historical tradition:

> Traditional approaches to quantum gravity, such as perturbative string theory, try to find a fully consistent microscopic description of all particles and forces. Such a "final theory" necessarily includes a theory of gravitons, the elementary particles of the gravitational field.

That "reductionist" tradition is opposed to a new "emergent" holographic theory, and we're told that

> The present point of view thinks of space-time not as a starting point, but as an end point, as a natural structure that emerges out of the complexity of quantum information, much like the thermodynamics that rules our glass of water. Perhaps, in retrospect, it was not an accident that the two physical laws that Einstein liked best, thermodynamics and general relativity, have a common origin as emergent phenomena.
>
> In some ways, this surprising marriage of emergence and reductionism allows one to enjoy the best of both worlds. For physicists, beauty is found at both ends of the spectrum.

Dijkgraaf seems to be saying that a viable emergent theory of four-dimensional quantum gravity based on the complexity of quantum information has been found, but I seem to have missed this. Can someone point me to a paper describing it?

Comments

1. **NoviceAsAlways**
   September 7, 2017

   I read the Quanta article before getting to your blog post, so I really appreciate your point of 'please point out the paper that validates this claim'. As a novice, could you provide a more detailed description of where the authors leaps of logic run off the rails? Even a just basic 'this claim leads to this claim leads to this' basic rundown would be really helpful.

2. **Giulia**
   September 8, 2017

   Peter,

   he is obviously referring to Verlinde's entropic gravity. As all readers in this blog know, Verlinde repeated in a few lines what Jacobson did before him: whatever the microscopic degrees of freedom of the vacuum might be, they generate Einstein gravity if they behave like black hole horizons. Equivalently, one can say:

   If horizons have microscopic degrees of freedom, the vacuum must have as well.

   If one phrases it in this way, there is no big news in it. And one can rightly say that space is a thermodynamic entity. A deep theory of quantum gravity is not needed to make this statement; one just has to believe in black hole entropy.

3. **Mitchell Porter**
   September 8, 2017

   "a viable emergent theory of four-dimensional quantum gravity"

   I think it's called M-theory in four-dimensional anti-de-Sitter space. It emerges from a three-dimensional field theory called ABJM, and you can e.g. obtain the entropy of a black hole in the 4d theory, by an entanglement entropy calculation in the 3d theory. http://arxiv.org/abs/1705.01896

4. **Peter Woit**
   September 8, 2017

   I still don't know what Dijkgraaf is referring to:

   Mitchell Porter,
   I've never heard anyone claim ABJM as a viable physical theory of quantum gravity. In particular there's the problem that we don't live in AdS.

   Giulia,
   It's not obvious to me he's talking about Verlinde's entropic gravity. Last I heard, people at the IAS were skeptical there was any real idea there. Maybe things have changed though.

   NoviceAsAlways,
   The problem here is that I don't know exactly Dijkgraaf's "modern theory of quantum gravity" is, so I can't identify any possible leaps of logic. I'm left wondering whether it's a "postmodern theory" of quantum gravity.

5. **Chris Herzog**
   September 8, 2017

   I do not know for certain, but he may be referring to the first question that the It from Qubit Simons Foundation Collaboration Grant is trying to address: "Does spacetime emerge from entanglement?"

https://www.simonsfoundation.org/mathematics-physical-sciences/it-from-qubit/

It is probably premature claim success, but it's certainly an intriguing idea that many people take seriously — that quantum gravity could emerge from quantum information.

6. **Peter Woit**
September 8, 2017

Chris Herzog,
Your suggestion is the most plausible one so far. But if so, the "modern theories of quantum gravity" that Dijkgraaf is advertising are actually not what one would normally call a physical theory of quantum gravity, but more of a speculative general hope about some feature such a theory might someday have. It would be better if he made that clear if that's what he's writing about.

7. **Mitchell Porter**
September 8, 2017

Peter: in fact, a deformation of ABJM is holographically dual to the Nicolai-Warner critical point of N=8 supergravity, a solution of the theory whose properties did attract some phenomenological interest prior to superstrings. You yourself have expressed a liking for the idea of twisting the N=8 theory, so ABJM may be a lot closer to your own interests than you know.

However, I mention it here, not as a theory of everything, but as the main example in four dimensions, of what Dijkgraaf is talking about. The AdS5/CFT4 example (N=4 CFT) is even better understood (e.g. see David Berenstein's papers), but you asked for something with gravity in four dimensions.

These AdS theories are not immediately viable as phenomenology, because they are AdS and because they involve a special choice of fields, but they prove the viability of Dijkgraaf's concept – gravity emerging from quantum mechanics.

8. **Anon**
September 8, 2017

This article is a perfect example of John Horgans ironic science. Its a weird kind of literary criticism.

9. **Frog Leg**
September 8, 2017

There was an interesting classification of QG theories posted in the arxiv:
https://arxiv.org/pdf/1708.07445.pdf
(h/t S. Hossenfelder, https://mobile.twitter.com/skdh/status/906009289331949569)

10. **Art**
September 8, 2017

Dijkgraaf links to Wolchover (https://www.quantamagazine.org/erik-verlindes-gravity-minus-dark-matter-20161129/), and Wolchover links to Verlinde (https://arxiv.org/abs/1611.02269).

11. **Peter Woit**
September 8, 2017

Art,
I hadn't really noticed the links to Quanta articles embedded in the Dijkgraaf piece. I'm suspicious that they weren't put there by Dijkgraaf himself. In particular, I wouldn't have thought that for the general idea of holography he would send people to Verlinde's work. But, who knows, I'm quite confused about what people promoting this kind of work specifically have in mind and intend to point to.

12. **Marko**
September 9, 2017

Maybe he is talking about tensor networks, which are being hyped quite a lot recently in the quantum information community (Preskill, Susskind, Carroll, etc, and a bunch of their students). But I think that tensor networks are still quite far far away from being called a theory, they are nowhere near developed enough.

Of course, none of that has ever kept anyone from hyping about the "modern theories of quantum gravity"…

🙂
Marko

13. **Low Math, Meekly Interacting**
September 9, 2017

An odd article for Quanta. Nearly 2/3 of it is devoted just to setting up the main thesis (almost half of it is a synopsis of "Powers of Ten"). So emergent gravity gets crammed into a relatively brief denouement that does little but namecheck the (seemingly) key concepts. Contrast this to, say, Frank Wilczek's anyon or entanglement articles. Like them or not, those have, at least, some actual content. Maybe Dr. Dijkgraaf had a deadline and was too busy with other things.

14. **Peter Woit**
September 9, 2017

LMMI,
The funny thing is it's not unusual in cases like this for journalists to write much more substantive pieces than scientists. I think the scientists decide it's too hard to actually explain what is going on, so stick to generalities, whereas the journalists (the good ones) at least make an attempt to convey what they've been able to understand of the story.

15. **tulpoeid**
September 10, 2017

(Partially repeating Art's remark,) it seemed to me that there's no mystery: The paragraph starting with "A complementary approach to combining gravity and quantum" links to everything the author talks about.

The alienating thing is of course that after listing the most fashionable theories of QG he pulls very hard to call them either reductionist or emergent, for not necessarily good reasons other than writing an article about these two concepts. But he doesn't seem to make mystery claims about new theories.

16. **Giulio**
September 15, 2017

I think it's clear Dijkgraaf is referring to Verlinde: the key sentence "quantum space-time, including all the particles and forces in it, emerges from a completely different "holographic" description" links to a Quanta story about Verlinde.

17. **Peter Woit**
September 15, 2017

Giulio,
All the links in the article are to other articles at Quanta. I suspect an editor at Quanta put them there, not Dijkgraaf.

18. **Moyses**
September 19, 2017

For a review, take a look at: https://academic.oup.com/ptep/article/2012/1/01A101/1556457/The-origin-of-space-time-as-seen-from-matrix-model

19. **Peter Woit**
September 19, 2017

Moyses,
I see no evidence that that proposal for how 4d spacetime emerges from the 10 d superstring is what Dijkgraaf is referring to

20. **Evan Thomas**
September 19, 2017

Peter,

Looks like I was mistaken, but you should go with my initial impression of your question, one of rhetorical sarcasm. Meaning, there is no viable "modern theory of quantum gravity" 😉

QCD at $\theta=\pi$

September 15, 2017
Categories: Uncategorized

Earlier this week Zohar Komargodski (who is now at the Simons Center) visited Columbia, and gave a wonderful talk on recent work he has been involved in that provides some new insight into a very old question about QCD. Simplifying the problem by ignoring fermions, QCD is a pure SU(3) Yang-Mills gauge theory, a simple to define QFT which has been highly resistant to decades of effort to better understand it.

One aspect of the theory is that it can be studied as a function of an angular parameter, the so-called θ-angle. Most information about the theory comes from simplifying by taking $\theta=0$, which seems to be the physically relevant value, one at which the theory is time reversal invariant. There is however another value for which the theory is time reversal invariant, $\theta=\pi$, and what happens there has always been rather mysterious.

The new ideas about this question that Komargodski talked about are in the paper [Theta, Time Reversal and Temperature] from earlier this year, joint work with Gaiotto, Kapustin and Seiberg. Much of the talk was taken up with going over the details of the toy model described in Appendix D of this paper. This is an extremely simple quantum mechanical model, that of a particle moving on a circle, where you add to the Lagrangian a term proportional to the velocity, which is where the angle θ appears. You can also think of this as a coupling to an electromagnetic field describing flux through the circle.

Even if you're put off by the difficulty of questions about quantum field theories such as QCD, I strongly recommend reading their Appendix. It's a simple and straightforward quantum mechanics story, with the new feature of a beautiful interpretation of the model in terms of a projective representation of the group O(2), or equivalently, a representation of Pin(2), a central extension of O(2). In the analogy to SU(N) Yang-Mills, it is the $\mathbf Z_N$ symmetry of the theory that gets realized projectively.

Komargodski himself commented at the beginning of the talk on the reasons that people are returning to look again at old, difficult problems about QCD. The new ideas he described are closely related to ones that are part of the recent hot topic of symmetry protected phases in condensed matter theory. It's great to see that this QFT research may not just have condensed matter applications, but seems to be leading to a renewal of interest in long-standing problems about QCD itself.

Besides the paper mentioned above, there are now quite a few others. One notable one is very recent work of Komargodski and collaborators, [Time-Reversal Breaking in QCD4, Walls and Dualities in 2+1 Dimensions].

Comments

1. **Arun Debray**
   September 16, 2017

   > a projective representation of the group O(2), or equivalently, a representation of Pin(2), a central extension of O(2)

   There are two nontrivial central extensions of O(2), called Pin^+(2) and Pin^-(2). The paper didn't seem to mention which Pin group is appearing here; do you know which one it is?

2. **Anon**
   September 16, 2017

   Thanks for posting on this. I tried looking through their paper. Will take more time to read it. But I was wondering if in the talk at Columbia whether the speaker mentioned how their works fits in with the strong CP phase or strong CP problem. Their paper doesn't explicitly mention the strong CP phase/problem.

3. **P**
   September 16, 2017

   Interesting post, thanks.

   Do you have any opinion on Arkani-Hamed's latest paper? Judging from the title it sounds pretty spectacular. Unfortunately I don't have the technical skill sett to judge the impact of the paper.

   https://arxiv.org/abs/1709.04891

4. **Peter Woit**
   September 16, 2017

   Arun Debray,

   No, I don't (since I don't know the difference between those two versions of Pin), but I'd think it's quite explicit and should be easy to figure out.

   Anon,

   No mention of the strong CP problem (other than the standard fact that it disappears for a massless fermion), no claim to have something new to say about that.

   P.

   Looks quite interesting, and I'll look at it more closely, especially since I'm quite interested in how much reps of the Poincare group tell you about QFT. Some of the claims in the abstract look much stronger than plausible, so very interesting. But, this really is off-topic, and I don't want to host a discussion of this paper here and now.

   Like the paper discussed here, another quite encouraging example of some of the best theorists around working on new ideas about 4d qft close to the Standard Model.

5. **BCnrd**
   September 17, 2017

   Peter, Arun Debray is referring to the fact that whereas the (linear algebraic) orthogonal group O(q) associated to a non-degenerate quadratic space (V,q) over a field k is insensitive (say as a subgroup of GL(V)) to replacing q with a scalar multiple cq for nonzero c, the isomorphism class of the (linear algebraic) group Pin(q) considered as a central extension of O(q) by \mu_2 is *very* sensitive to such change in q when c is a non-square in k. This sensitivity to such change in q at the level of the Pin group is a shadow of what occurs at the level of Clifford algebras: for non-square c, one doesn't see any direct link between the Clifford algebras C(V,q) and C(V,cq). For your situation, the context of interest is (V,q) positive-definite of dimension n>1 over k=R (even n=2) and c=-1.

Staying in the general setting for conceptual clarity, an obstruction to finding an isomorphism between Pin(q) and Pin(cq) as central extensions is encoded in the spinor norm O(q)(k) –> H^1(k,\mu_2) = k*/(k*)^2 that is most efficiently defined as the connecting map associated to the short exact sequence 1 –> \mu_2 –> Pin(q) –> O(q) –> 1 of (linear algebraic) groups, and is characterized most concretely by the condition that it carries the reflection r_v in any non-isotropic v (i.e., v for which q(v) is nonzero, such as any nonzero v when k=R with q definite) to q(v) mod (k*)^2. The point is that if Pin(q) and Pin(cq) are isomorphic as central extensions then the associated connecting maps (i.e., spinor norms) coincide, so then *necessarily* q(v) and c(q(v)) coincide mod (k*)^2 for any non-isotropic v. But that forces c to be a square in k! So when c is a non-square in k, we really get non-isomorphic central extensions.

Working over the real numbers, there is just one isomorphism class of positive-definite quadratic spaces of a given dimension n > 1, so one may refer to its associated Pin group as "Pin^+(n)" (considered as a central extension of O(n) by \mu_2), and refer to the one for the negative-definite variant as "Pin^{-}(n)" (also considered as a central extension of O(n) by \mu_2). Since c=-1 is not a square in R, these two central extensions of O(n) by \mu_2 are not isomorphic. (For expository simplicity I am sweeping under the rug the distinction between linear algebraic R-groups and compact Lie groups because it turns out not to be a problem in this case: see the theorem of Chevalley stated in my answer to https://mathoverflow.net/questions/6079/classification-of-compact-lie-groups/16269#16269 for a precise statement about that.)

6. **Petite Kabylie**
September 17, 2017

Will the Komargodski paper shed some light on confinement?

Thank you!

7. **Peter Woit**
September 17, 2017

BCnrd,
Thanks! I see, this is the same phenomenon that shows up in four dimensions as the fact that while Spin(3,1)=Spin(1,3), Pin(3,1) is different than Pin(1,3). This has led to some debate in the physics literature about physics being sensitive to what is usually thought of as a choice of sign convention.

Petite Kabylie,

The paper has some claims about implications for the phase diagram as a function of theta and the temperature, but not I think for what happens at theta=0, which seems to be the physically relevant value.

8. **BCnrd**
September 17, 2017

Peter, that's right. I should have also noted (for the purposes of the comparisob of spinor-norm calculations upon replacing q with cq) that the reflection r_v \in O(q) in a non-isotropic vector v is *insensitive* to replacing q with cq since by definition r_v(x) = x – (B_q(v,x)/q(v))v where B_q(v,w) = q(v+w)-q(v)-q(w) is the symmetric bilinear form associated to q. (Note this definition of B_q omits the factor of 1/2 that is sometimes used to define B_q, so B_q(v,v)=2q(v) and in particular the factor of 2 one usually sees in the definition of r_v is really lurking inside B_q).

9. **Adel Sadeq**
September 17, 2017

Petite Kabylie,
page 23 onward has the details

10. **ilych oblomov**
September 18, 2017

P
Peter Woit
New idesas about QFT and Poincare group can be found in the ideas introduced by Mund, Schroer and Yngvason: string localised field that allow getting rid of gauge theory (work in positive definite Hilbert space) and the Higgs mechanism (the Higgs is still there but for other reasons) . Rehren has a preprint the same day as Arkani-Hamed's:
Pauli-Lubanski limit and stress-energy tensor for infinite-spin fields

https://arxiv.org/abs/1709.04858
(and reference within)

11. **Jack Morava**
September 19, 2017

Can a mathematician put in a plug for

https://arxiv.org/abs/1707.05448
The Sum Over Topological Sectors and θ in the 2+1-Dimensional CP1 σ-Model
Daniel S. Freed, Zohar Komargodski, Nathan Seiberg

as well?

Special Relativity and Classical Field Theory

September 22, 2017
Categories: Book Reviews

For quite a while Leonard Susskind has been giving some wonderful courses on physics under the name "The Theoretical Minimum", pitched at a level in between typical popularizations and standard advanced undergraduate courses. This is a great idea, since there is not much else of this kind, while lots of people inspired by a popular book could use something more serious to start learning what is really going on. The courses are available as Youtube lectures here.

Book versions of some of the courses have now appeared, first one (in collaboration with George Hrabovsky) about classical mechanics, then one (with Art Friedman) about quantum mechanics. I wrote a little bit about these here and here, thought they were very well done. When last in Paris I noticed that there's now a French version of these two books (with a blurb from me for the quantum mechanics one).

The third book in the series (also with Art Friedman) is about to appear. It's entitled Special Relativity and Classical Field Theory, and is in much the same successful style as the first two books. Robert Crease has a detailed and very positive review in Nature which does a good job of explaining what's in the book and which I'd mostly agree with.

The basic concept of the book is to cover special relativity and electromagnetism together, getting to the point of understanding the behavior of electric and magnetic fields under Lorentz transformations, and the Lorentz invariance properties of Maxwell's equation. Along the way, there's quite a lot of the usual sort of discussion of special relativity in terms of understanding what happens as you change reference frame, a lot of detailed working out of gymnastics with tensors, and some discussion in the Lagrangian language of the Klein-Gordon equation as a simpler case of a (classical) relativistic field theory than the Maxwell theory. Much of what is covered is clearly overkill if you just want to understand E and M, but undoubtedly is motivated by his desire to go on to general relativity in the next volume in this series.

At various points along the way, the book provides a much more detailed and leisurely explanation of crucial topics that a typical textbook would cover all too quickly. This should be very helpful for students (perhaps the majority?) who have trouble following what's going on in their textbooks or course due to not enough detail or motivation. Besides non-traditional students in a course of self-study, the book may be quite useful for conventional students as a supplement to their textbook.

One of the most annoying things someone can do while reviewing a book is to start going on about their own different take on the material, criticizing the author for not writing a very different book. So, the rest of this posting is no longer a review of the book, it's now about the very different topic of what I think about this material, nothing to do with Susskind's valuable and different approach.

This semester I'm teaching a graduate level course on geometry, and by chance the past week have been discussing exactly some of the same material about tensor fields that Susskind covers. The perspective is quite different, starting with trying to explain a coordinate-invariant point of view on what these things are, only then getting to the formalism Susskind discusses. I can't help thinking that, with all the effort Susskind (and pretty much every other physics textbook…) devotes to endless gymnastics with tensors in coordinates, they could instead be providing an understanding of the geometry behind this story. It's unfortunate that many if not most of those who study this material in physics don't ever get exposed to this point of view. Thinking in geometrical terms, the vector potential and field strength have relatively simple interpretations, and using differential forms the equations needed for the part of E and M Susskind covers are pretty much just:

$F=dA$, $dF=0$, and $d*F=*J$

Similarly, for the special relativity material, there's a danger of the basic simplicity of the story getting lost in calculations of how things appear in coordinates with respect to different reference frames. What you fundamentally need is mainly that objects are described by a (conserved in the absence of forces) energy-momentum p, which satisfies $p^2=-m^2$, with Lorentz transformations taking one such p to another. The wider principle is that things are described by solutions to wave equations, with special relativity saying that the Lorentz group takes solutions to solutions.

I'd like to believe that such a very different course and very different book would be possible, quite possibly am very wrong (I've never taught special relativity to anyone). Maybe some day someone, inspired by Susskind's project, might try to do something at a similar level, but from a more geometric point of view.

Comments

1. **Anon**
   September 22, 2017

   MIT open course ware, has a complete course on Quantum physics complete with lecture videos assignments and exams. Just started (probably won't finish). Its very good.

2. **ilovecats**
   September 22, 2017

   Do you think Penrose's book (e.g. Road to Reality) covers these topics from a more geometric angle? In any case, as a student who has been following your blog for the last 3 years, I'd love it if you sometime wrote just a couple of short articles on what you mean (books can come later :).

3. **Jim Holt**
   September 22, 2017

   Does Susskind derive EM as a gauge theory? It seems to me that conceptually this is the right way to go: getting EM from SR (instead of the other way around) by quantizing the SR equation E^2 – p^2 = m^2 to get an equation for a (complex) scalar field, and then applying the gauge principle (which can be motivated by SR) to get the EM vector potential. Fringe benefit of this top-down approach to EM: you are not shocked by the Aharanov-Bohm effect.

4. **Peter Woit**
   September 22, 2017

   ilovecats,

   Penrose's book is kind of the opposite of Susskind's. It's deeply geometrical, but at the same time really is not appropriate for beginners, many professional mathematicians and physicists find it challenging to follow.

   The main reason I haven't written up some of these things myself it that there are many other places that this has been done. For some suggestions, see

   https://mathoverflow.net/questions/72160/maxwells-equations-and-differential-forms

Jim Holt,

He does use gauge symmetry for the coupling of EM to matter, although for particles coupled to EM (gauge potential changes the action for a particle trajectory). In this book everything is classical, so he can't get coupling to EM via gauge symmetry of a wave-function. The Klein-Gordon equation is discussed, but treated as an example of a Lorentz invariant classical field (somewhat of a motivation for the classical EM Maxwell equations, not something used to describe matter).

5. **vmarko**
September 22, 2017

Usually, if you want a thorough geometric approach to SR, EM and beyond, the main reference is the MTW book. Though differential forms as such are never beginner material.

Btw, coupling EM to matter vis gauge symmetry is an entirely classical concept, i.e. classical field theory. Dirac equation is as classical as the Klein-Gordon equation, as long as you understand them as equations for fields, as opposed to relativistic QM. In fact, the whole Standard Model action is classical, together with the Higgs mechanism and all… Quantization only builds on top of that, leading to QFT.

🙂
Marko

6. **Tim May**
September 22, 2017

Peter,

I've been a daily reader of your blog for about 8-10 years, or whenever it started, since after reading your book. I've been generally skeptical of string theory for a very long time, maybe 30 years or more. (I joined Intel Corp. in 1974 as a device physics guy, working on DRAM memory chips in the early days. Took classes from Jim Hartle and Douglas Scalapino at UCSB, but am by no means a graduate-level physicist! Still, I follow it.)

Iroinically, two of my favorite physicists currently speaking are Leonard Susskind and Nima Arkani-Hamed. Though both are associated in some way with string theory, both are excellent speakers. (I could add that both are speaking a lot about things that are not directly associated with string theory. I don't think either has given up on string theory, just that their interests seem to be in other areas. While one can criticize string theory, it seems to have directly or tangentially led to some interesting other theories and some new math. A point I recall you have also made._)

I am glad that your criticisms of some aspects of string theory have not extended to criticisms of either of these two, or of Witten, Maldacena, or others. (I am not happy that a Czech blogger, whose lengthy explanations I often like, is so prone to personalizing his criticisms.)

Living only about 45 miles southeast of Stanford, outside of Santa Cruz, I have been to half a dozen or so of Susskind's talks, plus have seen him hosting a bunch of talks. Never made it to Arkani-Hamed's talks, but have greatly enjoyed his videos. Have seen the Cornell 5-part series about 2.5 times (the final two more than three times.)

Thanks!

–Tim May

7. **S**
September 23, 2017

I've had a bit of success* teaching SR to undergrads using Chapter 6 of Barrett O'Neill's wonderful book "Semi-Riemannian Geometry with Applications to Relativity." The earlier chapters do exactly what you're asking with tensors (and differential geometry), and chapter 6 is a really lovely geometric discussion of SR. (I also want to check out Gregory Naber's book, "The Geometry of Minkowski Spacetime." I know from other books that he's a wonderful writer, but I haven't had the chance to read this one yet.)

*"bit" = small sample size, not low success rate.

8. **Thomas Larsson**
September 23, 2017

Let me disagree about index-free notation being simpler than tensor calculus. Sure, you can eliminate a few indices, but OTOH you must add definitions (of d and *), and if you want to express more complicated things (upper and lower indices, symmetrized or anti-symmetrized, etc.) more definitions are needed. In the end you risk being swamped by definitions instead of indices, which does not seem like such a great advantage to me.

Moreover, finding the right definitions usually involves index manipulation. The reason why the exterior derivative is interesting is that it is a group homomorphism, which intertwines between modules of the group of coordinate transformations (i.e. if A is an antisymmetric tensor field, so is dA). If you are interested in manifolds with some extra structure, e.g. a volume, symplectic, or contact form, the relevant group preserves these structures (the algebras S_n, H_n, K_n in Sophus Lie's notation), and there are new homomorphisms, "exterior derivatives", which only need to intertwine with the relevant subalgebra of W_n.

You could perhaps look up these homomorphisms in the literature, but at least the classification of binary and higher homomorphisms is rather recent and not readily available in textbooks. Moreover, I once worked out the exterior derivatives in a case that was not yet published. Some fifteen years ago I was briefly interested in the exceptional Lie superalgebras E(3,6) and E(3,8), because there is a correspondance between these superalgebras and SU(3)xSU(2)xU(1). Kac and Rudakov had described the homomorphisms for E(3,6) but had not yet published the result for E(3,8) when I worked it out.

The point is that this was done using techniques from tensor calculus (and I proud myself of being good at index gymnastics). It could not have been done with index-free notation, because none was available for this superalgebra at the time, and probably still isn't.

9. **Peter Woit**
September 23, 2017

Thomas Larsson,
I'm not claiming that index-free notation is always the best way to calculate things. Often you need to pick a well-chosen set of coordinates and calculate using those. My point is that it is worthwhile to understand the geometrical, coordinate independent, significance of the objects one is calculating with. A good example is cases in GR where you find solutions with singularities, need to realize these are not physical singularities, but coordinate singularities.

10. **NoGo**
September 23, 2017

Have you seen “It’s About Time” by N. David Mermin?
It is (IMHO) very accessible and very geometrical introduction to Special Relativity for non-scientists. No tensors there… But it’s just SR, no electromagnetism etc.

11. **Doug McDonald**
September 23, 2017

    Its clear to me after 40 years of seeing things like this piece by Woit that the problem
    with all the “geometry” methods is that they are not taught as standard fare to
    undergraduates. I can’t understand a word of them (with the words meaning their
    specific mathematical meanings of course, not say “manifold” as its ordinary one.)
    I’ve not found a usable intro to that stuff.

    One needs at least a couple of years of digesting the math at a pliable age
    before “just using the math” for the physics.

12. **Robinson**
September 23, 2017

    I wrote Leonard Susskind some years ago to thank him for these lectures. I learned just enough to be able to vaguely follow what’s going on in modern physics at the moment. Allan Adams Open Courseware (MIT) on foundations of Quantum Mechanics is also really good.

13. **Anon**
September 23, 2017

    Perhaps of interest for index free definitions (Thorne & Blandford, Modern Classical Physics)

    http://press.princeton.edu/chapters/s10157.pdf

14. **Jeff M**
September 23, 2017

    Well, I learned special relativity out of Spacetime Physics, thereby no doubt dating myself. Perhaps even worse than one might think, since when I first read it Styx was a big band. Differential forms are wonderful, but I didn’t see them until I was a grad student in math. Learned GR the classical way, from Rindler. Lot’s of indices that I remember.

15. **Thomas Larsson**
September 23, 2017

    Peter Woit,
    I did not talk about choosing coordinates for a specific problem, but rather about expressing the same equations with or without indices. The geometrical meaning can be understood in both cases, but index-free notation requires that somebody has already worked out the right operators like d and *. In contrast, tensor calculus can be applied also in unchartered territory. The geometrical meaning of E(3|8)-invariant supergeometry might not be clear, but it was still possible to figure out the analogues of the exterior derivative using index notation.

16. **cthulhu**
September 24, 2017

    Feynman’s “Six Not-So-Easy Pieces” is mostly about Special Relativity and I liked it, but I’m looking forward to this new book too; I’ve enjoyed the other two.

    On the tensor index issue, I remember my professor for tensor calculus + calculus of variations (taught out of the engineering department for the engineering majors, not the math department) extolling the geometric interpretation for many problems – grads, divs, and curls instead of index gymnastics – at least until you have to calculate.

17. **Manfred Requardt**
September 24, 2017

    Concerning the index free notation. There exists a combination of both methods, called the abstract index notation (see e.g. the nice book of Wald). I think, at least for physicists the abstract index notation has certain advantages, in particular if you have to do some calculations.

18. **S**
September 24, 2017

    I had the opposite experience of some. I learned the index notation first, from a physicist, as an undergrad, and I had no idea what in heck he was talking about. (None of us did). It can be taught well, I’m sure, but it certainly wasn’t to us, it was only mystifying. Learning the geometric interpretation (from O’Neill, in my own case) brought clarity where there had been only mystification.

19. **Luca Ambrogioni**
September 25, 2017

    I think that the best example of a deeply geometrical textbook suitable for beginners is “Gauge Fields, Knots and Gravity” by John C. Baez. It starts by giving an insightful but somehow informal explanation of differential and Reimanian geometry and then uses it for introducing EM and GR from a purely geometrical (and topological) point of view. It is a really beautiful book.

20. **former mathematician**
September 25, 2017

    I can’t find a link, but there was a limerick circulating in the 50s, titled “LPE to DSC.” It began “A connection is simply outre when expressed in an intrinsic way.” The response, “DSC to LPE,” ended “The devil take you and Christoffel.”

21. **Jeff Moreland**
September 25, 2017

    Very Special Relativity by Sander Bais gives a good geometric introduction to SR for interested laymen.

22. **Bobito**

September 26, 2017

"I'm not claiming that index-free notation is always the best way to calculate things. Often you need to pick a well-chosen set of coordinates and calculate using those. My point is that it is worthwhile to understand the geometrical, coordinate independent, significance of the objects one is calculating with. A good example is cases in GR where you find solutions with singularities, need to realize these are not physical singularities, but coordinate singularities."

If one uses Penrose's abstract indices, indices are merely labels that indicate tensor type and symmetry (it works for spinors too), and their use requires no choice of coordinates/frames. This is all explained in detail in volume one of Penrose/Rindler. It is a mistake to confuse the use of indices with the use of coordinates. They are two different things.

23. **Mozibur Ullah**
September 26, 2017

I first learnt about general relativity from Gerochs semi-popular book General Relativity from A to B just before I left for university (to study mathematics); it's a wonderful book that uses just basic arithmetic to get to the essence of the subject.

When it came to index notation, although I could see how it worked, I couldn't see why it worked. It's only when I came across Lees book on smooth & topological manifolds that some light dawned. I also found Michors book on Natural Operations (which is freely available) an eye-opener on categorical methods to help organise the material.

Although index-free thinking is more geometric I think both skill sets are useful, otherwise physics written in index notation would not be accessible; I also think it shouldn't be forgotten that this route requires a serious investment of effort (the tangent bundle at a point p is the set of all derivations at that point!) and probably the optimum book explaining this material hasn't been written yet. It took me some time to learn, for example, that bundle and sheaf methods were, roughly speaking, dual to each other.

I don't think it's beyond the bounds of the possible to teach differential forms in high school if the right approach was taken – a practical approach; after all, in the UK, advanced students learn about the cross product in vector geometry, it's only later that I realised it was the Lie algebra of SO(3)! And of course calculus itself was once only for the cognoscenti whereas every schoolboy knows about it.

24. **Mozibur Ullah**
September 26, 2017

I'd also second Ambrogionis suggestion on Baezs 'Gauge fields, knots and gravity' as a beautifully written introductory book to differential forms in physics.

25. **Milkshake**
September 26, 2017

Are there videos of your lectures on modern Geometry?

26. **Peter Woit**
September 26, 2017

Milkshake,
Definitely not. Keep in mind that the lectures in that course are mostly the usual material and just about every math department has a similar course (with likely a more industrious and more talented lecturer…). The course webpage
http://www.math.columbia.edu/~woit/geometry2017/
shows what I'm covering and give reading suggestions. There are a lot of good textbooks on the subject at this level.

I do think a course at a bit lower level, more aimed at physicists, would be a good idea, and is not something that is so common, maybe some day I'll try and teach one. But that's not this course.

27. **Mark Weitzman**
September 27, 2017

Another interesting book on special relativity is Special Relativity in General Frames: From Particles to Astrophysics (Graduate Texts in Physics) – Eric Gourgoulhon, which doesn't just focus on inertial observers, and treats things like the Thomas precession, and Sagnac effect in an easy manner.

28. **Manfred Requardt**
September 27, 2017

As to so-called Abstract Index Notation. It is usually attributed to R.Penrose. But actually one can find it already in Laugwitz, Math.Zeitschr. 61, (1954) 100, and as far as I remember already Schouten introduced it.
But anyway, as it is not so deep an observation, it is perhaps not so important who introduced it ('invented' is perhaps to great a word) .

29. **Luca Ambrogioni**
September 27, 2017

To Manfred

I would say that the abstract index notation gets reinvented everytime someone who understands differential geometry opens a physics book. I think that most physicists writing indices have a geometrical object in mind.

You will never understand relativity if you think that R_{abcd} is "just a number".

30. **Manfred Requardt**
September 27, 2017

Dear Luca, you are absolutely wright. After all, an alternating differential form over the cotangential space is nothing but an antisymmetric tensor. On the other hand, not all geometric objects are antisymmetric, that is, are forms.

Various and Sundry

September 26, 2017
Categories: Uncategorized

- I don't know if I ever mentioned this, but quite a while ago I replaced the "latexrender" TeX plugin being used here by a mathjax one. As I find time, I'm now going back and editing old posts to get rid of latexrender tags and make the equations more mathjax friendly. As far as comments are concerned, you can add TeX content by using standard math delimiters \$, or \$\$ for displayed math. If you want to comment about US dollars, put a backslash before your dollar signs to avoid the interpretation as TeX.

  One reason I hadn't advertised this much is that I know it's hard to get TeX right the first time, so people's comments with TeX would be likely to often not work properly. I've added a plugin that lets you edit your comment for 5 minutes after you write it. This should be useful for typos, as well as for fixing TeX problems (note that you need to refresh the page to get the math to display).

- For a philosopher's take on evaluating string theory, see this talk by James Ladyman, on Cosmic Dreams. Material on string theory is near the end, and just makes the obvious point that having no experimental evidence for the theory is a huge problem, no matter what efforts are made to change the usual way scientific theories are evaluated.
- A hot topic these days in the math community is the conjecture that local Langlands can be understood as geometric Langlands for the Fargues-Fontaine curve. My attempts to learn about this so far haven't had a lot of success, but I now have new-found hope. At Harvard there's a seminar going on this semester on the topic, and it has a website which so far features explanations of some of the mathematics involved from Jacob Lurie and Dennis Gaitsgory. In London, the London Number Theory Seminar also has a study group devoted to this topic (website here, although seems to have disappeared for the moment).
- LQP2 (Local Quantum Physics Crossroads, v.2.0) is a website that gathers various information about relativistic quantum theory.
- In November Perimeter will host what should be an interesting workshop on the question of how to make sense of the Path Integral for Gravity.
- A memorial for Maryam Mirzakhani will take place at Stanford on October 21, with a live feed available here.
- As always, Quanta magazine keeps publishing a wide range of very high quality articles about math and physics, covering different topics than everyone else. Most recently, on the math side, see an article by Erica Klarreich on Pariah Moonshine and on the physics side, Robert Henderson on possible searches for long-lived particles possibly from a "hidden sector"..

Update: Commenter sdf points out this historical article by Pierre Colmez about the Fargues-Fontaine curve, preprint of a preface to an Asterisque volume.

Comments

1. **sdf**
   September 27, 2017

   On Pierre Colmez website there is a fascinating semi-historical introduction to the FF curve. Apparently it will be the foreword to the Asterisque volume on the curve to appear.

2. **anon**
   September 27, 2017

   There's also a new preprint 1709.0743 from Scholze and in its references: Laurent Fargues and Peter Scholze, Geometrization of the local Langlands correspondence, in preparation

3. **G. S.**
   September 27, 2017

   Minor typo: "LPQ2" should be "LQP2"

4. **Peter Woit**
   September 27, 2017

   G.S.,
   Thanks, fixed.

5. **NoGo**
   September 27, 2017

   Today came an announcement that a Virgo detector together with two LIGO detectors recorded a black hole collision:
   https://www.nsf.gov/news/news_summ.jsp?cntn_id=243239
   "This discovery, announced today, is the first observation of gravitational waves by three different detectors…"

6. **Igor Khavkine**
   September 27, 2017

   So, rumors about a new GW event are confirmed! Any updates on the rumor that the a counterpart event was observed in the EM spectrum as well?

7. **aaaaaaaaa**
   September 27, 2017

   No, today's announcement seems to be about a different event.

8. **Peter Woit**
   September 27, 2017

   aaa…
   Are you sure? The NGC 4993 rumor was just a few days (Aug. 18) after this event (Aug. 14). Is NGC 4993 anywhere near the patch of sky singled out for this event?

9. **Jeff M**
   September 27, 2017

Peter

What I just heard is that there are rumors of another event, not two black holes, but a black hole and a neutron star, and that for that event there was EM confirmation. Supposedly observed a few days after this one. No comment as yet about it from either LIGO or VIRGO.

10. **AWS**
September 27, 2017

No EM counterpart for this event – from the NSF announcement:
"Being able to identify a smaller search region is important, because many compact object mergers — for example those involving neutron stars — are expected to produce broadband electromagnetic emissions in addition to gravitational waves," says Georgia Tech's Laura Cadonati, deputy spokesperson for the LIGO Scientific Collaboration. "This precision pointing information enabled 25 partner facilities to perform follow-up observations based on the LIGO-Virgo detection, but NO counterpart was identified — as expected for black holes."

11. **aaaaaaaaa**
September 28, 2017

NGC 4993 and the best localisation for GW170814 are about 100 degrees apart, unless I messed up spherical trigonometry… In any case, they're not close to each other on the sky.

12. **Igor Khavkine**
September 28, 2017

The characterization of LQP2 as a website that collects information about "relativistic quantum theory" may be overly broad. As any community website, it's content is dictated by the members of that community. But, at least historically, the focus of that community has been more narrowly the study of Algebraic Quantum Field Theory (AQFT), including also the study of QFT on Curved Spacetimes and sometimes even Non-Commutative Spacetimes using the methods/framework of AQFT. The name Local Quantum Physics actually reflects the title of the well known monograph by Rudolf Haag on this topic.

13. **David Derbes**
September 28, 2017

To follow up on Jeff M, I have a former student whom I dare not identify who might be on the LIGO team. He or she tells me, "You didn't hear it from me, but the best is yet to come." No more specific than that, but I think more and better are coming soon. Great timing! It's gotta be either the whole LIGO team or Thorne and Weiss for this year's Nobel. Nice article in the NYT by Dennis Overbye.

14. **Urs Schreiber**
September 28, 2017

Igor, on the other hand, as we have discussed, the local quantum observables of any perturbative relativistic QFT do organize into a causally local net, and so all traditional perturbative relativistic QFT is, and secretly has been all along, local QFT in the sense of the Haag-Kastler axioms. In fact the Haag-Kastler axioms (for perturbative formal power series algebras of observables) may be *derived* this way from standard relativistic QFT. (I have written out the proof in full detail here, since it is curiously non-evident from the literature as you know. Thanks again for the hints that you had provided!) Therefore it sort of makes sense and possibly is about time to conflate "local quantum physics" in the community-specific sense with traditional relativistic QFT.

15. **John**
September 28, 2017

Sabine Hossenfelder has a nice article in Forbes "https://www.forbes.com/sites/startswithabang/2017/09/28/is-the-inflationary-universe-a-scientific-theory-not-anymore/#216075e0b45e" about inflation and if it's science or not.

16. **David Roberts**
September 30, 2017

Vladimir Voevodsky has passed away…

17. **tulpoeid**
October 2, 2017

I might not be adding much to the discussion here, but Hossenfelder's article (John's link above) is so good that moves me to tears, especially the second half.

Vladimir Voevodsky 1966-2017

October 1, 2017
Categories: Obituaries

I was very sorry to hear yesterday of the announcement from the IAS of the untimely death of Vladimir Voevodsky, at the age of 51. Last year I had the chance to meet Voevodsky and talk with him for a while at the Heidelberg Leader's Forum (which I wrote about here). He was a gracious and modest person, and it was fascinating to learn a bit about what he was trying to do, and his earlier experiences doing mathematics that had led him down this path. There was no indication at that time that he was ill, and I don't know what led to his death.

Back in 2012 I wrote a blog post about him and his work, linking to various things that may be of interest if you'd like to know more about him. Among more recent sources of information, there's a video interview here, a popular article here, lecture slides here, here and here, and a piece by Siobhan Roberts which covers some of the same topics that Voevodsky told me about when I met him last year.

Update: See here for remembrances of Voevodsky on the HoTT mailing list.

Update: There will be a gathering at the IAS to remember Voevodsky this Sunday, a funeral service and conference in Moscow December 27-28, and a conference in Princeton September 29-30, 2018. More information available here.

Update: A longer obituary of Voevodsky, from the IAS.

Update: The New York Times has an obituary here. Video of the IAS gathering is available here. Especially informative and touching is the talk given there by his ex-wife Nadia Shalaby, who gave a detailed look at his life, mathematical and personal, including a frank discussion of his problems with mental illness, depression and sometimes self-destructive ways of dealing with this. Also see this story at Quanta, where Shalaby gives the cause of his death as an aneurysm.

Comments

1. **David Roberts**
   October 2, 2017

   The first slides link is accidentally a relative link, so is pointing to something in your directory tree whose tail is an IAS url.

2. **Peter Woit**
   October 2, 2017

   David Roberts,
   Thanks, fixed.

3. **Anonymous**
   October 2, 2017

   I enjoyed his wildlife photography that he linked in one interview.

4. **kris**
   October 2, 2017

   It is really sad to know that he has passed away. I had met him briefly at the IAS, at the time he was very interested in biology as well, and appeared to be planning to do some work in that direction as well. He came across as a genuinely curious and modest person, especially given his achievements. He was also a very good photographer, and some of the photos exhibited there from his collection were really very nice.

5. **Nick**
   October 7, 2017

   In 2014 I learned as a computer science student about Homotopy Type Theory, after my first year. At the time, I did not know about the basic things in this field, like lambda calculus and category theory.

   But I felt was a kind of enthousiasm, inspiritation to learn about this field and about mathematics in general.

   Very sorry to hear this. It's very sad to think what he could accomplish given more time.

2017 Nobel Prize in Physics

October 2, 2017
Categories: Uncategorized

At this point, Kip Thorne and Rainer Weiss of LIGO have (deservedly) won just about every scientific prize out there, for the first observation of gravitational waves. I don't know of anyone who doesn't believe they'll be getting the Physics Nobel tomorrow morning. With an open spot in the usual limitation to three (Ronald Drever passed away earlier this year), perhaps Barry Barish will also get the nod. Most appropriate would be to use the third slot to give an award to the entire LIGO collaboration, but it seems likely that the tradition of not honoring collaborations will continue. There will be a live webcast of the announcement at 5:45am EST available here.

Update: Congratulations to the winners. I think Natalie Wolchover speaks for all science journalists when she writes:

> Thrilled they won, thrilled not to spend this morning speed-reading about some bizarre condensed matter phenomenon.

Update: A couple things I've learned from comments and other coverage of this:

- Some physicists have no sense of humor and are either unaware of or ungrateful for the excellent job Natalie Wolchover and others at Quanta magazine have been doing in writing high-quality stories about a wider range of topics in physics than anyone else (see here and here, related here).
- All evidence is that on October 16th we'll get announcement of observation of gravitational waves with an optical counterpart, with details at this conference in Baton Rouge.

Comments

1. **J**
   October 2, 2017

   I vaguely remember that an individual could still be awarded a Nobel Prize even if he/she would pass away before announcement but after the nomination being received. So maybe Ronald Drever still got the chance. Well I may be wrong…

2. **sm**
   October 3, 2017

   I worry for a 'miscarriage of justice' if Pustovoit ('empty' Voit!) is not seriously considered. He (with Gertsenshtein) proposed, to my mind beautifully clearly, with realistic sensitivity and noise estimates the basic experimental setup (vacuumized optical paths …) and physics underpinning LIGO etc in 1962, long before anyone else in

   http://www.jetp.ac.ru/cgi-bin/dn/e_016_02_0433.pdf

   For a two page explanation how LIGO works (including an aside as to why Weber's setup could never have worked!) it takes a lot of beating.

   In the interests of historical accuracy, if nothing else, this work needs to be cited at the Nobel ceremony. Let's see!

3. **Thomas Larsson**
   October 3, 2017

   J, the winner must be alive at the time of the final decision, which happens very shortly before the announcement.

   An exception was the 2011 prize in medicine or physiology. The Nobel committee knew that Ralph Steinman was ill, checked that he was alive on Friday, the final vote and the announcement was on Monday, but Steinmann passed away on Saturday. It was then decided that he would nevertheless receive the prize posthumously, since the Nobel assembly had acted in good faith.

   I remember that I informed my wife, who is a member of the Nobel assembly, that they had awarded the prize to a dead person. She was not amused.

4. **Bernhard**
   October 3, 2017

   " but it seems likely that the tradition of not honoring collaborations will continue."

   It's typical of Swedish mentality to to give all the credit to whoever is higher up in the hierarchy and something that, as I see it, will never change. Congratulations to the winners in any case.

5. **Shantanu**
   October 3, 2017

   Peter, I think condensed matter physicists might get offended by Natalie Wolchover 's comment. I am surprised by how quick the NB was announced after the discovery (only 2 years). OTOH nu mass discovery took 17 years by that time the founder of Super-K had passed away. accelerating universe also took 13 years and discovery of CMB anisotropies took 14 years.
   So is that there are no more nobel prizes in particle physics left to be given?

6. **Mats Larsson**
   October 3, 2017

   To sm:

   see reference 26 in Scientific background:

   https://6702d.https.cdn.softlayer.net/2017/10/sciback_fy_en_17.pdf

7. **Peter Morgan**
   October 3, 2017

Seconding Shantanu. See http://nanoscale.blogspot.com/2017/10/gravitational-radiation-for-win.html for a condensed matter physicist offering a polite clearing of the throat.

8. **Mats Larsson**
October 3, 2017

In the Nobel will it says: "…during the preceding year…"

According to the by-laws of the Nobel Foundation, §3:

"To be eligible to be considered for a prize, a written work shall have been issued in print or have been published in another form, to be decided on its own behalf by each prize-awarding body."

The discovery of the first LIGO event was published on February 11, 2016.
Thus, the discovery of GW could not be awarded in 2016.

9. **Robert Owczarek**
October 3, 2017

Replying to sn comment, in the 50s of the 20th century the theory of gravitational waves has been revived first by Hermann Bondi, then joined Pirani, Robinson, Trautman, Goldberg, Sachs and a few others. The reason for necessity of the revival was that Einstein himself almost killed the whole research by claiming that gravitational waves are probably removable by changing the reference system, and so unphysical, or, even if they are not removable, they do not transport any energy and so are undetectable, which basically means also that they are unphysical. New methods developed thanks to Petrov contribution of "objective" studies of general gravitational fields (and so finally independent of reference frames) and further expanded by the Bondi and followers allowed for finding many exact solutions of Einstein equations that describe different gravitational waves, and for studies of these waves and their sources, etc. Thus Bondi would be the first of this group who deserves the Nobel Prize, but he died in 2005. Recently died also Pirani and Robinson. Of the still alive members of this group Trautman's contribution was certainly the biggest, so he deserved to be awarded. More on that here
http://www.ams.org/publications/journals/notices/201707/rnoti-p686.pdf

10. **Peter Shor**
October 3, 2017

I think Natalie Wolchover was comparing how easy it is for journalists to explain LIGO, the 2017 Nobel Prize, as opposed to how easy it was for journalists to explain topological phases of matter, the 2016 Nobel Prize.

I'm not sure whether she was including the Higgs particle (2013 Nobel) or blue LEDs (2014 Nobel) in the comparison. I would hope not.

11. **Maurice**
October 3, 2017

I second with Shantanu's surprise that they awarded this prize so quickly. The
Nobel committee ran a risk by breaking a rule that has never been broken in previous years as far I know: the prize is only given to an experimental discovery that has at least one independent confirmation.
LIGO/VIRGO still reports serious backgrounds that they do not understand by their own account (also for the newest event GW170814).

In the likely case that LIGO/VIRGO's signals are no background, an independent confirmation is expected in the near future e.g. in the form of an astrophysical event detected with photons or neutrinos coincidentally with and from the same direction than a GW detection. The committee would have been well advised to wait until immediately after such a confirmation will be in.

Let's hope all goes well…

12. **Peter Woit**
October 3, 2017

I think Peter Shor has it right. Some of the recent Nobels have (for good reason) been given for condensed matter phenomena that have gotten little attention from journalists and are not easy to explain to the public (these two things are correlated…). Surely one should have some sympathy for science journalists expected to produce something sensible explaining the physics on a very short deadline (the Nobel doesn't do what a lot of other organizations do, give journalists embargoed information early). For gravitational waves, given the topic and the huge amount of previous public attention, writing something this morning should have been a piece of cake.

Bernard,

In the LIGO case, there is a small number of people who started the thing and had a big impact on getting it to work, so rewarding them individually is not hard to justify. On this topic, see
http://fictionalaether.blogspot.com/2017/10/woohoo-i-just-won-nobel-prize.html
and
http://backreaction.blogspot.com/2017/10/yet-another-year-in-which-you-havent.html

In cases where there are no such people in a collaboration, but the collaboration is responsible for a huge scientific discovery (e.g. the Higgs), the Nobel policy is highly problematic, likely meaning such discoveries will never get recognized by them.

13. **Peter Woit**
October 3, 2017

Shantanu,
I think Nobel's initial concept was to reward very recent work, with the prize in year X the most important development of year X-1. One problem with this is if the evidence is not overwhelming and you need separate confirmation (I don't think that's the case here, but don't want to start a debate about that). Some of the long delays can be attributed to waiting for the evidence to be overwhelming, sometimes the relative importance of the discovery is more dubious, so there's a wait until other more obviously important things have been rewarded.

I don't want to turn the discussion to HEP and its problems of lack of unexpected discoveries. If the LHC discovers anything unexpected that violates the Standard Model there should be a quick prize (except for the problem that the discovery may have been made by too many people…)

14. **Douglas Natelson**
October 3, 2017

To clarify, I fully recognize that Natalie Wolchover's remark was likely meant to be both humorous and a comment on how hard it can be to explain some CM topics. It is a rather weird situation where talking about dark lepton sectors or black hole firewalls or emergent quantum gravity is somehow viewed as more accessible than much of CMP, but that's where we are. This is a pet issue of mine, and clearly there is more work to do.

15. **simplicio**
October 3, 2017

Is there a generally known (or common guess) for why the prize has never been awarded for the discovery or study of Dark Matter? It seems to stick out as the only fundamental physics discovery developed in the latter half of the 20th century not to get a prize. And especially given fundamental physics discoveries have come at a rate of far less than 1/yr, its not like there just hasn't been any room to fit it in.

16. **Shantanu**
October 3, 2017

Peter (or others), do you remember how much interest the 1993 Nobel prize in Physics (which was also for GWs) generated among journalists/physicists/audience compared to 2017 one?
Would be curious to know
Thanks

17. **HEP theorist**
October 3, 2017

Douglas Natelson,

I think that condensed matter theorists have a culture of valuing very technical insights and computations, and being very upfront about it. There is nothing wrong with this. In fact it's pretty admirable. But it has an unfortunate side effect. Compare power-points written by leaders in condensed matter theory vs leaders in high energy theory. The former always have very busy slides, many plots with tiny fonts, many diagrams. The latter like masking computations and giving elegant sound bites. (Not saying there are no computations behind them!) It's no wonder that the latter is more easily picked up by science journalists. It's a credit to the Nobel team for not succumbing too easily to media pressure and for valuing condensed matter research.

18. **Peter Woit**
October 3, 2017

Shantanu,

My impression is that the actual physics Nobel awards get much the same amount of attention, independent of the topic. It's usually just a one day story, similar amount of media coverage. I don't remember 1993, different times, different kind of press. but I bet most of the press covered it. The result itself got much less attention, since it was indirect and much less spectacular.

19. **David Derbes**
October 3, 2017

This may be old news but it was brand new to me. In the Nobel press release for technical readers, there are a number of references, including a simply wonderful set of conference notes from the legendary GR conference at Chapel Hill in 1957, put together six years ago by the peerless and irreplaceable Cécile Morette DeWitt, UT Austin, and Dean Rickles, U of Sydney. The Nobel folks suggest that this conference was the seed that led to LIGO, particularly Feynman's terrific thought experiment with beads on a wire, arguing very forcefully that gravitational waves would carry energy and could in principle be measured. Most if not all of the founding fathers and mothers are there: Felix Pirani, Hermann Bondi, Andrej Trautman, André Lichnerowicz, Joe Weber, Wheeler, Feynman, Dicke, Bryce S. DeWitt and so on. A wonderful gathering, sometimes thought of as the Shelter Island of general relativity, wonderfully rendered: http://www.edition-open-sources.org/sources/5.

I think the Nobel people tried very hard to recognize the many pioneers of this great achievement.

20. **piscator**
October 3, 2017

simplicio:

I would think the reasons are

(A) The Nobel Prize committee is historically conservative, and it is not 100% nailed down that dark matter as such exists – there are alternative proposals such as MOND, even if most do not agree with it.

(B) The history of dark matter is that the evidence with the longest history (velocity dispersions/rotations curves, Zwicky, then Rubin&Ford + some others whose names I forget (in the review by Bertone)) is most vulnerable to the MOND objection, whereas the cosmological arguments for dark matter are later (and so would have less claim to a prize).

That said, in terms of something having a large observable effect of the universe, my view is that if the Nobel Committee felt that the evidence for dark energy was enough for a Nobel Prize, then they should have awarded one for 'dark matter' first (or something that at least behaves like it).

21. **Bernhard**
October 3, 2017

Peter,

Even in the case of the large LHC collaborations, you can go back and would find a rather small number of people who started it and were authors of the letter of intent of the experiments.

I'm not sure what it means to "start and experiment". So many things can and do go wrong while trying to collect the data that it takes several other intelligent people to get that first inspiration/idea into something real.

It's not that Thorne at al, didn't deserve the prize or that some random postdoc should had won, but the fact that the scientific work that mas done, was done by a collaboration and it is this collaboration that did the work that led to the discovery. Irrespective of the fact that Thorne et al, as individuals certainly deserve the prize, so does LIGO. And LIGO did not win any prize. Morally yeas, technically no.

I'm all for, let's stop this Nobel nonsense, and focus on the science. But as long as we are going to keep talking about it, which is likely to happen, than we should try to get it right. And the Nobel is not getting it right, for stupid conservative reasons.

22. **william e emba**
October 3, 2017

One thing I don't like is that they don't like to give awards for experimental discoveries of significant mysteries until the mystery is explained. Davis had to wait decades for the explanation of the missing solar neutrinos, and Bahcall was unfortunately left out, again.

They could have awarded Rubin for discovering dark matter, while phrasing it in a way that did not commit the committee to any particular interpretation. That they did not is a disgrace.

I suspect the reason for not awarding the prize for LIGO last year was simply that no one from LIGO was nominated. I once read somewhere that there was a January deadline, which was why Bednorz and Müller got the award, but not Chu.

I almost wonder if the joint Virgo observation was rushed into print just to sweeten the committee's laudatio.

If I remember correctly, Nobel's will specifies that most of the prizes are to individuals, at most three at a time, with Peace allowed to go to organizations.

23. **Daniel Mittleman**
October 3, 2017

I'm not at all surprised by the rapidity of the Nobel committee's response to this discovery. There seems to be little doubt that it's correct. There also seems to be little dispute that it's a Big Deal, of the sort that clearly deserves this level of recognition. And the people who most deserve the prize aren't young (indeed, one already died, as mentioned above). So why wait? I think this was a no-brainer, frankly, and hardly without historical precedent. Not that I'm biased…

On the other hand, I agree with Doug: it's somehow quite amusing, in a sad sort of way, that journalists think general relativity is easier to explain than, well, anything.

24. **DrDave**
October 3, 2017

There's always some second guessing in respect to the Nobel prizes, and there will always be people left out, but certainly these are good choices. The real winners here are scientists who have new avenues of research as well as those who will be inspired to create new and creative experiments to actually test things. Big victory for testability.

25. **MathPhys**
October 3, 2017

Well done, Kip Thorne.

PS I grew up reading Misner, Thorne and Wheeler.

26. **David Metzler**
October 3, 2017

In Weiss's remarks at the MIT press conference, he refers multiple times, obliquely, I might even say winkingly, to more news coming on October 16, with reference to neutron stars. Each time got a big laugh from the other LIGO members. So it looks like those rumors are true. Exciting!

27. **A**
October 4, 2017

simplicio, the Nobel prize for Dark Matter will be given when/if DM will be detected non-gravitationally. A posteriori Zwicky would have deserved a Nobel prize for DM. Just like Einstein would have deserved this Nobel prize. For the future, Nobel prizes for inflation and for inflationary perturbations would be more appropriate than prizes for topics that require speed-reading

28. **Robert Owczarek**
October 4, 2017

David Derbes
To make things straight: Unfortunately, Andrzej Trautman had not participated in the conference, which certainly was great even without his participation.
With all your respect, although arguments by Feynman were impressive, they were much weaker than what in the next few years did Bondi et al.
Also, I do not see how the pioneers of the research in gravitational waves were recognized by the Nobel committee if none of them got the award.

General discussion:
Of course awarding the discovery, which is really of great importance and will influence the fututre development of astrophysics and physics in general in profound way, as I believe it will, is a very good and expected decision.

29. **Shantanu**
October 4, 2017

David, I agree with Robert So far no theorist (other than Kip) has got a nobel on gravitational waves whereas 4 experimentalists have got it in (1993 and 2017). Once can argue Damour deserves one for his theoretical contribution to both endeavours.

30. **piscator**
October 4, 2017

The nonsense about awarding Nobel prizes to collaborations should be knocked on the head.

In addition to the many other bad reasons, even the stated positive aim – to recognise that science is a collaborative process – fails badly, by excluding people with very significant contributions to a discovery while making Nobel prize winners of extremely junior people who have done precious little other than become a member of a certain collaboration.

For the specific case of gravitational waves, a major element to the discovery is the ability to identify the waveforms, which comes from being able to follow numerical relativity all the way until the merged system settles down. But e.g. Pretorius, who transformed this field, is not a part of LIGO – and so would be excluded under this policy.

For the case of the Higgs, the LHC beam was manifestly key to the discovery – but CERN's accelerator division is not part of the experimental collaborations such as ATLAS or CMS. Furthermore, an essential part of this discovery again lies in unheralded work on theoretical predictions for QCD and electroweak backgrounds or the development of Monte Carlo generators – all done by those who are generally not part of the collaborations that get to announce discovery.

So, yes, science is collaborative but yes, some people contribute a lot more than others. The Nobel Prize has kept its reputation over a long period because those involved do a good job of ensuring that those who win have made major contributions.

31. **Peter Erwin**
October 4, 2017

simplicio:

To repeat and amplify a bit on what piscator, william e emba, and A said: I think there are two strikes against dark matter when it comes to Nobel Prizes.

1. It's originally a discovery in astronomy (i.e., the work by Vera Rubin, Albert Bosma, and others), and Nobel Prizes are very rarely awarded for work in astronomy. Consider the fact that there have been no Nobel Prizes for the discovery of extrasolar planets, or the fact that Hubble never got one for discovering the expansion of the universe. I suspect that for many years the argument would have been something like "Well, that's just something weird about galaxies, that not really *physics*, is it?"

2. The Noble committee seems not to like giving out prizes for *unsolved mysteries*. We still don't know what dark matter *is*: Is it (the most popular explanation) an undiscovered nonbaryonic particle? (What *kind* of particle?) Is it mass in some other form, like primordial black holes? Is it not mass at all, but an indication that our theory of gravity needs to be replaced? Etc. (Any one of those would probably be worth a Nobel, but the committee probably doesn't want to give out an award for the wrong reason.)

The missing solar neutrino problem is, I think, a good example. Evidence for this first appeared in the 1960s, and only accumulated in strength and replicability over time. But the actual prize wasn't awarded (in 2002) until after evidence for the solution in the form of nonzero neutrino mass and neutrino oscillations was obtained.

(The discovery of pulsars is also a good example, because even though they were initially a complete mystery, the correct solution — rotating, magnetized neutron stars — was worked out very rapidly.)

The Nobel for discovery of dark energy via supernova measurements is arguably the only modern counterexample (the 1937 award for discovery of cosmic rays is a much earlier one) — except that it *does* work perfectly well as confirmation of a longstanding theoretical prediction, in the form of the cosmological constant. (It also helped resolve some issues with the age of the universe, which was probably a bonus.)

32. **Bernhard**
October 4, 2017

piscator,

What you wrote is utter and complete nonsense.

The only thing preventing the Nobel committee from awarding collaborations is tradition. And it is only this tradition that prevents collaborations from getting a prize. A prize for a collaboration would not make some random person in the collaboration make the claim that they won the Nobel, only that they worked in a Nobel winning experiment, which tells nothing about them.

About "some people contributed more than others", please you are stating the obvious, and anyone working in a collaboration knows that. There is however a VERY long way between this statement and the conclusion that because of this plain statement we should just award spokespersons.

The Nobel committee has actually track-record of doing an awful job because of this tradition of rewarding bosses only, even when large collaborations are not the matter (Cesar Lattes comes to mind). And are you seriously suggesting Takaaki Kajita and not the Super-Kamiokande Collaboration that deserved the Nobel for neutrino oscillations? Oh please!

LIGO is an entity in itself and there is absolutely nothing absurd in giving the prize to it. To the very least, they could follow the lead from the Breakthrough Prize and mention the collaborators too, even if given more credit to the seniors:
https://breakthroughprize.org/Laureates/1/P4

33. **piscator**
October 4, 2017

Bernhard,

>>A prize for a collaboration would not make some random person in the >>collaboration make the claim that they won the Nobel,

funnily enough, if you look at CVs (enough available online) of LIGO members you will find them listing the Gruber Prize, Breakthrough Prize, etc under their 'Awards' sections.

Of course I am not saying that prizes should only go to spokespersons. Some spokespersons are just good managers. Prizes should go to people who have been transformative. van der Meer won the Nobel Prize for the discovery of the W/Z even though he wasn't in the collaborations, because stochastic cooling was a transformative contribution that was central to the discovery.

The drive to visualise, initiate and build up a large collaboration focused on a particular topic can be transformative and fully deserving of a Nobel Prize. This isn't just in academia – to say that Google has X thousand employees doesn't obscure the centrality of Brin and Page. As I don't know any of the history of Super-K I'm not going to comment on Kajita and whether this would be an accurate description.

For the Higgs, what you suggest would exclude people like Lyn Evans and various CERN D-Gs who ensured the LHC happened in the first place, while including graduate students who were in ATLAS/CMS for a couple of years and spent zero time on Higgs searches.

The Milner prizes are textbook examples of how to do prizes badly, and I am surprised you regard them as a model.

34. **Bernhard**
October 4, 2017

pisctor,

Yes, I know of people who list the Breakthrough prize in their CVs. Actually, I gave not so long ago feedback to a student that was applying for a job to remove it, because it looked really silly. The fact that young people will want to do that is another thing, and comes out of naivety because they fool nobody worth fooling. It still argues not against awarding a collaboration.

In case of the the Higgs, what I would argue is that the half of the prize could have been given to ATLAS, CMS and the LHC and the other half split between Higgs and Englert. This would have awarded theory and experiment in a fair way.

I agree, there is lots of things wrong with the Milner prizes, and I personally think they do more harm than good. BUT, awarding the experimental collaborations is about maybe the only thing that they got right and that they Nobel is incapable of seeing. It has in the end more to do with the seniority and Swedish mentality of the people who control the prize than anything tangible concerning a transformative contribution to an experiment.

35. **Bernhard**
October 4, 2017

By the way, I recommend reading:

https://www.theatlantic.com/science/archive/2017/10/the-absurdity-of-the-nobel-prizes-in-science/541863/

36. **Fred P**
October 4, 2017

@Bernhard- "The only thing preventing the Nobel committee from awarding collaborations is tradition."

Statue 4 of the Nobel foundation appears to preclude collaborations.

The Nobel foundation statue 4:
"§ 4.
A prize amount may be equally divided between two works, each of which is considered to merit a prize. If a work that is being rewarded has been produced by two or three persons, the prize shall be awarded to them jointly. In no case may a prize amount be divided between more than three persons…" source:
https://www.nobelprize.org/nobel_organizations/nobelfoundation/statutes.html#par7

As a note, it looks like this could be changed – see Statute 22.

37. **Bernhard**
October 4, 2017

Fred P,

this rule can be bent simply by considering the collaboration as a single entity, like is is done for the piece prize (e.g. awarding the UN). It's not a stretch of the imagination.

38. **william e emba**
October 4, 2017

I've looked some things up.

Nobel's will specifies one person per prize for up to "two works". The first Peace prize was to two people. There were multiple winners for several early Physics prizes, and there was an early institutional Peace prize. Literature, for some reason, has only given a joint award twice. The Nobel Foundation gives each prize-awarding group the authority to award organizations. I would guess that there were no organizations in non-Peace categories worth thinking about for decades, and the awards have achieved a certain reputation they don't want to tinker with.

Originally nominations were restricted to the living, and awards could be given to someone who died afterwards. This happened twice. The Foundation changed this in 1974. The Steinman family deliberately did not announce his death at first; I don't recall anything about the Academy inquiring if he was alive. (The only inquiry I have heard of was whether Nash could cope with the unusual attention he would be subject to.) It's believed that there was no 1948 Peace prize because Gandhi was assassinated January 30 that year. Had he been killed two days later, he would have been eligible.

39. **aaaaaaaaa**
October 4, 2017

Fred P,

The fourth statute also says:
"Each prize-awarding body shall be competent to decide whether the prize it is entitled to award may be conferred upon an institution or association."

It seems to me that this explicitly says that, for example, LIGO as a whole could be eligible for the prize.

40. **AcademicLurker**
October 4, 2017

This discussion might be past its sell-by date, but I guess I'll jump in anyway.

I'm skeptical of complaints, like the one in the linked Atlantic piece, the the Nobel prize "distorts how science is done". Very few people work on the things they do because they expect to get a Nobel prize for it. One of the cryo-EM winners announced this morning won for the years he spent figuring out how to get water to form a glass instead of crystals during the freezing process that's part of sample preparation. I doubt he was thinking "Stockholm, here I come!" during that work.

As Ed Yong notes, none of the issues with the Nobel would matter if the prize weren't such a big deal. I'm betting, and I could be wrong, the the "big deal"-ness of the Nobel would start to diminish if it were awarded to institutions instead of individuals. Which would be too bad. One guaranteed high profile week for science per year isn't all that much as it is.

Football and baseball are also collective enterprises, but MVP awards and the Heisman trophy don't seem to generate quite so many complaints.

41. **David Derbes**
October 4, 2017

@Robert Owczarek:

Thank you for the correction re: Trautman. I clearly didn't read the conference proceedings carefully enough. With respect to the Nobel Committee recognizing the pioneers, I did not mean to suggest that all who might have deserved a part of the prize had gotten it. The rule of three prevents that. I was trying to say that in their press releases, they seemed to me to have tried hard not to slight anyone. That's a good thing. With respect to Feynman's argument, I am not saying that it turned the tide; I don't know enough about the history of gravitational radiation research. But that's my impression of what the Nobel press release said.

@Shantanu:

It's true that experiment has been rewarded four times to theory's once, if you count Hulse-Taylor (appropriately). But on the whole the Nobel folks seem to me to favor experiment over theory in general, and I don't fault them for that, notwithstanding my own theoretical upbringing. I think Thorne was actually a little sorry that the entire LIGO team didn't get the prize. I remember in the weeks before Englert-Higgs, someone asked the Nobel Physics folks if they felt the ATLAS-CMS teams were disqualified. I believe the answer was no, that in principle a team with N >> 3 could win it (as has happened repeatedly with the Peace Prize: Doctors Without Borders, the Irish Mothers, and so on). It's a shame that ATLAS-CMS didn't also win for the Higgs discovery, and a shame that other theorists besides Thorne haven't also won for gravitational radiation, but there are always losers in these prizes. Specifically, Robert Brout and Ron Drever lost out because of death. That really stinks, in my opinion, though I am glad Barry Barish won (as predicted skillfully by Peter W.).

42. **Paul**
October 5, 2017

Weiss alluded to a new announcement on Oct. 16. See 8:40 in this video, and also around 16:40:

https://www.youtube.com/watch?v=eJMOzmwYT8A

43. **Shantanu**
October 5, 2017

Peter, since no one has mentioned this, I should point out that cancellation of SSC proved to be a blessing in disguise for LIGO as many people (starting from/inspired by Barry) shifted to LIGO
after their phd/postdocs in experimental HEP.
This is despite that the fact that LIGO project involves completely classical GR and astrophysics (a topic which I doubt many HEP experimentalists knew much about in grad school). Even the nuts and blots issues needed for working on LIGO such as control theory, digital signal processing, time-domain digital filtering is never taught in Physics grad school and is completely decoupled from HEP (which deals with discrete triggers or events).

44. **Laurence B Lurio**
October 5, 2017

On the subject of being able to explain last years Nobel prize on topological physics, there was an article in December 2016 Physics today by Sung Chang which put this obscure condensed matter theory into context and really did a good job of explaining why it deserved a Nobel. Of course, this still isn't an explanation at the popular science level which journalists might like, but perhaps more articles like this might have made Feynman change his mind about canceling his subscription.

45. **cedric bardot**
October 5, 2017

A nice way to celebrate the end of the Noble prize annoucement season and "Fête de la science 2017" … Go to Baton Rouge, Lousiana on October 16th?

Upcoming conference :
IAU symposium on Gravitational Wave Astrophysics: Early Results from GW Searches and Electromagnetic Counterpart, 16 – 19 oct. 2017 at Baton Rouge, Lousiana

Rainer Weiss said at a press conference at MIT on October the 3rd :
"…We opened a new field in astronomy and astrophysics… Einstein's waves are interesting and the fact that you can directly detect them is important but the real pay-off is gonna be in the future… it's already happened the pay off in some regards and more of will happen on october 16th I won't tell where it is but I can tell you there is more there… We've seen balck holes which is already wonderful but we also expect to see the merger o neutron stars and that was the thingthat that gave the field a certain credibility when it was discovered that there are pairs of neutron stars in our galaxy and people stopped laughing at us when that was found out … now the big question is how often does it happen that two neutron stars are smashing each other well … I won't say anymore all this is reserved and the thing is that from that people will learn if we have done it right we'll learn much more than just from gravity … you'll learn a lot about nuclear physics a lot of facts about equation of state of nuclear physics… how stiff is nuclear matter, you'll also learn probably how heavy elements are made, all of this is reserved for the future…"

46. **Michael John Sarnowski**
October 5, 2017

An honorable mention should go to Professor of physics, Jolien Creighton, from the University of Wisconsin, Milwaukee, who has been working on LIGO many years. Very smart man.

47. **Paul**
October 5, 2017

Quote from Robert Byer at a photonics conference not long ago. Interesting detail about the LIGO laser systems, but also:

'And Byer hinted that the coming month may see still more impressive results in the quest for gravitational waves and "multimessenger astronomy." Specifically, Byer offered a quick teaser of an upcoming news conference that the LIGO team will hold on 16 October to announce a new result. While Byer couldn't reveal the content, what he did say suggested that his particular presser will be a don't-miss event. "I'm willing to admit," Byer said, concluding his talk, "that what they will announce will be one of the most important events in the history of astrophysics."'

48. **Yatima**
October 7, 2017

"that what they will announce will be one of the most important events in the history of astrophysics."

Well, the last 20 years have been quite exciting already. I hope they won't be announcing that we are at the business end of an upcoming in-galaxy gamma-ray burster.

49. **Kyle MacDonald**
October 10, 2017

Was it just Wolchover's tweet that provoked that rant from Orzel? How could anyone within earshot of the popular science press not recognize that she's been writing about LIGO essentially every few weeks for the last several years and is glad to have a topic she can write about competently in a hurry?

50. **Peter Woit**
October 10, 2017

Kyle MacDonald,
I think it's exactly that the popular science press writes about LIGO every few weeks and not about other topics in physics that is what provoked the rant. Wolchover though is an example of someone who does regularly cover topics beyond the high profile things like LIGO.

51. **Kyle MacDonald**
October 10, 2017

Peter,

Ah, okay. That's an easier frustration to understand. I couldn't see how someone could possibly mistake 1) relief that the big annual write-it-in-three-hours physics story is this year already familiar to both science writers and their audiences for 2) general journalistic laziness. Agreed that it's a thoroughly unfair criticism to make of Wolchover specifically, but I also have sympathy with the general point that some topics get a lot of coverage simply because they've already had a lot of coverage.

52. **aaaaaaaaa**
October 12, 2017

Press conferences about the GW observation with EM counterpart seem to start 16:00 CEST/9:00 EDT in several places (at least at the National Press Club in Washington DC and ESO HQ in Garching) on Monday. The announcement says that some 70 observatories took part in the observations, so no wonder there were some leaks.

The Big Bang Theory and the Death of SUSY

October 5, 2017
Categories: Uncategorized

If you're a fan of The Big Bang Theory, perhaps you've seen the latest episode, *The Retraction Reaction*. If not, you might be interested in the following transcript (taken from here). The show has always done a good job of getting the science right, for an interview with their physics consultant David Saltzberg, see here.

The episode begins with a Science Friday interview of physicist Leonard Hofstadter by Ira Flatow:

> FLATOW: So, it has been five years since the discovery of the Higgs boson– what's the next big thing gonna be?
>
> LEONARD: Wow, that's hard to say. There's so much going on. We've been collecting tons of data that could revolutionize the way we understand the universe. For instance, there's a particle called a squark, which could prove supersymmetry.
>
> FLATOW: That is interesting. Have you found it?
>
> LEONARD: What, the squark?
>
> FLATOW: Yes.
>
> LEONARD: No, no. Wouldn't that be exciting? But we're also looking for the selectron, the gluino and the neutralino.
>
> FLATOW: Well, and have you found that?
>
> LEONARD: No. Another fun sidenote– I went to high school with a girl named Theresa Gluino, but it didn't cost $2 billion to find her. She was smoking behind the gym. (laughs)
>
> FLATOW: So, what have you found?
>
> LEONARD: Uh, nothing, actually. We've got the best equipment and the best minds all working on it. Although, some days I'm, like, ugh we've spent so much money. Why haven't we found anything? What are we doing?

After a segment in which neuroscientist Amy explains that she doesn't tell physicist boyfriend Sheldon about her new lab equipment since

> AMY: We've been getting so much more funding than physics, he's been a little sensitive.

another scene features Leonard called into the office of a university administrator:

> LEONARD: I have to say I'm a little nervous.
>
> Ms. DAVIS: You should be.
>
> LEONARD: Look, I know I screwed up, but it was only one interview.
> How much damage could it have caused?
>
> Ms. DAVIS: Would you like for me to read you the e-mails from donors asking why are they giving us money if physics is a dead end?
>
> LEONARD: I didn't say it was a dead end. I just said that I was worried it might be.
>
> Ms. DAVIS: So if I just said I was worried you might not have a job next week, how would you feel?
>
> LEONARD: Light-headed, and glad you asked me to sit down. Okay, just tell me what I can do.
>
> Ms. DAVIS: I'm gonna need you to make a statement saying that you misspoke, and that you're confident the physics community is close to a major breakthrough.
>
> LEONARD: You want me to lie.
>
> Ms. DAVIS: Look, Dr. Hofstadter, I'm counting on you. I think that you are the smartest physicist at this university.
>
> LEONARD: Really?
>
> Ms. DAVIS: See? Lies. They're not that hard.

Leonard then has this exchange with Penny:

> PENNY: Hey, come on, look, you said a few dumb things on the radio– what is the worst that could happen?
>
> LEONARD: I may get fired.
>
> PENNY: Okay, well, even if you did, you could find another job.
>
> LEONARD: Yeah, who wouldn't want to hire the physicist who publicly said physics is dead? Well, I wouldn't put that under "special skills". I can fix it, I just need to write a retraction I don't believe in– basically sell out to keep my job.
>
> PENNY: Great, I'll leave you to it.

He then goes to talk to string theorist Sheldon Cooper:

> LEONARD: Sheldon, it's me.
>
> SHELDON: What?
>
> LEONARD: Look, I know you're mad, but I have to write a statement that says the physics community is close to a breakthrough, and since you actually believe

> that, I could really use your help.
>
> SHELDON: Sorry, I can't.
>
> LEONARD: Come on, don't be like that.
>
> SHELDON: What? Look. (sighs) Not all science pans out. You know, we've been hoping supersymmetry was true for decades, and finally, we built the Large Hadron Collider, which is supposed to prove it by finding these new particles, and it-it hasn't. And maybe supersymmetry, our last big idea, is simply wrong.
>
> LEONARD: Well, that sounds awful. Now I get why everyone hates me.

Penny later comes in:

> PENNY: So you guys are upset because the collider thing disproved your theories?
>
> LEONARD: It's worse than that. It hasn't found anything in years, so we don't know if we're right, we don't know if we're wrong. We don't know where to go next…
>
> PENNY: Come on. You guys are physicists. Okay? You're always gonna be physicists. And sure, sometimes, the physics is hard, but isn't that what makes it boring?

The episode ends with a visit to the grave of Richard Feynman, and a reference to Feynman's story about how he got himself out of a slump in his work when he was at Cornell:

> WOLOWITZ: He did so much. And here we are, stuck and letting him down. You know, Feynman used to say he didn't do physics for the glory or the awards, but just for the fun of it. He was right. Physics is only dead when we stop being excited about it.

All in all, a pretty accurate portrayal of the situation in high energy physics theory, with a reasonable take on what to do about it.

Update: A correspondent points me to a rather Leonard Hofstadter-ish interview with Steven Weinberg back in 2011, where he says:

> It may be that they'll only discover the Higgs boson and nothing else, and we'll be left looking at our toes and wondering what we're going to do next. There may be nothing really new that can be reached with the LHC,
>
> I have fears… If all they discover is a Higgs boson with roughly the properties that the theory predicts and nothing else, I don't know where the field is going to go.

When asked a rather Ira Flatow-ish question: "Wouldn't you say to a young person that now would be a very exciting time to go into physics?" his answer is

> Whether or not it would be a good career move depends on what they are going to discover.
>
> If all they discover is the Higgs boson and it has the properties we expect, then No, I would say that the theorists are going to be very glum.

Comments

1. **Michael John Sarnowski**
   October 6, 2017

   Hilarious. Very funny story.

2. **Casey Leedom**
   October 6, 2017

   And for once, I truly wish Word less supported "like"s. Yeah, I know, it only works on the discredited social media circuit, but still …

   Casey

3. **A**
   October 6, 2017

   Altought Michelson-Morely were unhappy, but not finding what theorists expected has been a more revolutionary breakthrough than finding it

4. **Enrico**
   October 6, 2017

   A: I agree, not finding what theorists expected is a breakthrough. The next logical step is that theorists stop working on theories proven wrong. Does it make sense to work on aether after you know the results of the Michelson-Morely experiment?

5. **MikeS**
   October 6, 2017

   The Michelson-Morely experiment had the great advantage of demonstrating a clear negative result.
   The LHC seems to have brought us to the edge of a desert with no way of knowing what wonders may lie beyond the horizon, or if it's just desert all the way.

6. **Antimetrics Anonymous**
   October 6, 2017

   Peter
   Do you think metrics madness has caused this problem in physics ? . For example here is sample of the madness below.
   Einstein has h-index 108 66 in this google scholar index.
   from this web site https://scholar.google.com/citations?user=qc6CJjYAAAAJ
   Feynman has h-index 61 46 from the web site below.
   https://scholar.google.com/citations?user=B7vSqZsAAAAJ
   Do high energy physicists, want to prop up their indexes to get grants, promotion and in the process cut a sorry figure like Leonard H ?

7. **Peter Woit**
   October 6, 2017

   AA,
   There are lots of ways the incentives in academia are problematic and I don't want to start a general discussion of them here. I don't think the h-index thing is the biggest problem. The TV show gets right what the biggest problem for HEP is: lack of new experiment hints pointing a way forward coupled with refusal to acknowledge failure of dominant research programs (SUSY and string theory). I think it accurately portrays the current situation: people are aware of the problem, but don't want to admit failure publicly, fearing the implications of this for the field and for their careers.

8. **Jim Douglas**
   October 6, 2017

   "And sure, sometimes, the physics is hard, but isn't that what makes it boring?"
   That absurdly funny question so profoundy captures the attitude of the public towards mathematics, science and engineering. I've often felt this explains much of why superstrings / supersymmetry / Multiverse research still gets funding: the names, particularly "super" makes those subjects so sexy sounding in a field that is perceived as bone dry. Quantum Field Theory and especially Standard Model are so dull sounding. It was a careless catastrophy naming the world's most stunning theory the dreary "Standard Model". We need sexier names for genuinely scinetific theories. SuperDirac theory might do the trick for the SM. Germanely, "Big Bang" as a theory name is spot on, it's a fantastic name. Fred Hoyle is a hero. By wonderfully irony, he boosted the theory by giving it a fantastic name. The LHC is a ridiculously name. The Superconducting Super Collider was so much more fun! These things matter if you want funding and coverage in the media. Unless you're the SSC of course.

9. **Chris Kennedy**
   October 6, 2017

   Peter Woit (of Columbia University) has been trying to tell us the truth about SUSY for quite some time. Now Big Bang Theory (aired by Columbia Broadcasting System) has used humor to inject the same reality. Could there be a Columbia connection???

10. **zzz**
    October 6, 2017

    and Columbus day is next monday…

11. **Simon**
    October 6, 2017

    This is a common, but very particle physics-centric view of physics – it makes no difference to the vast majority of physics if SUSY is right or wrong (I'm an astrophysicist and finding (or not) the Higgs made zero difference to my research). Most of physics is advancing and discovering very nicely thank you very much… and ready and waiting to get its hands on all that juicy funding as particle physics hits a dead end…

12. **Anonymous**
    October 6, 2017

    Physics will never die, only physicists will. Historically we didn't have major breakthrough coming left and right. Instead, it is more like the case that it takes generations of physicists to explore extensive before one is shown up.

    A side note, while people are talking about unexpected hints from experimental sides, it might be worthy to note that almost all direct searches for BSM at LHC is based on and optimized on a particular model, which is very likely not correct by itself. Also it has this saying that no new physics can be discovered without a model at hadron collider, giving the enormous production rate of SM processes. It might not be a coincidence between this saying and this fact of experimental results.

13. **CIP**
    October 6, 2017

    Meanwhile, the real Caltech physics faculty collected two more Nobels (or two parts of a three way split). Maybe they could go on the show.

14. **JJ**
    October 6, 2017

    It's a shame there no mathematicians in The Big Bang Theory. They get the gist and types of interaction between hep, engineers, cosmologists, biologists, etc. It would be interesting to see their take on mathematicians.

15. **Igor Khavkine**
    October 7, 2017

    I think that was a good episode. They found a way to reflect the truth and make it funny. From my point of view, there was also bitter-sweetness to it, because they missed a chance to make it even better. I will echo Simon's comment and point out that they could have done a better job of distinguishing between Physics and Beyond-Standard-Model-Fundamental-Particle-Physics. What I think would have killed multiple birds with one stone is if, at the end, the despondent protagonists ran into one of their colleagues who when prompted easily rattled off a whole string of current *non-speculative* research topics that are both *highly successful* and show *great potential* for future development. It's not like there's a lack of those, at least when looking beyond Beyond-Standard-Model-Fundamental-Particle-Physics.

16. **tulpoeid**
    October 7, 2017

    Gold.

    PS: @Simon, unfortunately academia seems to gather several types with the perspective of "the Higgs made zero difference to my research". I.e. researchers who don't care about monumental discoveries related to the deepest foundations of humankind's scientific worldview and I'm pompous on purpose, as long as their lab is unaffected. Even though you beg to differ, this is not much different than caring for a failed theory only because it allows one to publish.

17. **Anonyrat**
    October 8, 2017

    Glad you noticed this episode of Big Bang Theory. When I watched the broadcast, I immediately thought of Not Even Wrong.

18. **Frank Wilhoit**
    October 8, 2017

There is a deep confusion here, on the part of the as-portrayed academic authorities, between process and results. Results can invalidate theories — that's the process. Results cannot invalidate the process. Protecting the theories from the process invalidates the process. To an outsider it looks like the theories, or the results, are the exciting things. No: it is the process that makes the theories, or the results, exciting.

The bad reasoning runs somewhat thus: pure science eventually led to nuclear weapons; nuclear weapons won the last war; the Next Big Thing will either win the next war, or, if there doesn't just happen to be a war on right then, it may make a metric muckton of money. But if it isn't even going to do that, then screw it sideways.

Someone once disparaged my discipline, in a public forum, by sneering at the mythical phenomenon of things being "analyzed to death". I retorted that everything he possessed and used had been analyzed to life.

19. **Jerome Moore**
October 8, 2017

Simon,

I'm still waiting on the "juicy funding" to come to my field since the cancellation of the SSC. The size of the pie isn't fixed, and there are positive-valued coupling constants for funding between fields of research at the macroscale.

What research directions there are and should be for given fields are fantastic questions that people like Peter are asking. Undermining entire fields, however, is done at our collective peril.

20. **Bernhard**
October 8, 2017

Peter,

About SUSY, but not really to do with the BBT…. Have you had a look at what's written on Wikipedia lately on it's current status? Pretty interesting and advocates SUSY is alive an well, just waiting for a new e+e- collider (I'll copy/paste all since it might change):

"The LHC result seemed problematic for the minimal supersymmetric model, as the value of 125 GeV is relatively large for the model and can only be achieved with large radiative loop corrections from top squarks, which many theorists had considered to be "unnatural" (see naturalness and fine tuning).[43]

This "naturalness" crisis turned out to be premature[11] in that one set of naturalness calculations were performed in multi-parameter effective theories which didn't allow for intrinsic cancellations which must occur in a more fundamental theory.[citation needed] Other calculations, indicating the need for light top squarks, neglected terms in the renormalization group equations that could lead to radiatively-driven naturalness[12] (wherein radiative corrections drive certain SUSY breaking terms from unnatural high scale values to natural values at the weak scale). It is now understood that top squarks can range up to 3 TeV and gluinos up to 5 TeV with little cost to naturalness so that LHC searches have only begun to explore the natural SUSY parameter space. On the other hand, it is also understood that higgsinos, the superpartners of Higgs bosons, are required to lie between 100-300 GeV, the closer to 100 GeV the better. Such light higgsinos are difficult, though perhaps not impossible, to see at LHC. However, they would easily be seen at an e+e- collider operating at energies above higgsino pair production threshold (such as the International Linear Collider, or ILC, proposed for construction in Japan)."

21. **Peter Woit**
October 8, 2017

Thanks Bernhard,
That's pathetic. Interesting to note that Wikipedia knows the Higgsino mass (they say the pair production threshold is at 500-600 GeV.

22. **N. Bhonka**
October 9, 2017

I think the physics community must realize that it is no longer the era before 1950's. The 'physics' of that era has been taken over mostly by computer science and some fields in engineering. The best students after high school like go to in these fields. And, partly because these fields are more lucrative also. Without sounding rude, in general, it is the bottom rung of the 'scientific pool' that makes its place in physics and mathematics these days. No wonder one sees the same toeing the line, little new ideas in physics.

Besides the possible exception of Peter Woit, and a handful others, you can talk to any physicist and the uniformity (read lack of new thought) of opinions expressed is astounding and quite unnerving. It seems like few, new and fresh ideas.

23. **Alain Blondel**
October 9, 2017

Even "particle physics" is much larger than SUSY and the LHC. BSM physics has actually been found and was rewarded the Nobel prize in 2015: neutrinos have mass.

The reason this is interesting is that some big observation-based questions to particle physics are as follows:
— the fact that the Universe is made of matter and not of anti-matter
— the existence of dark matter which cannot be one of the established Standard Model particles
— the fact that neutrinos have mass that is so much smaller that the other fermion particles.

All three can possibly be solved simultaneously by giving each neutrino family (e, mu, tau) two distinct mass terms,
— a 'Dirac mass term' just like the other particles, which connects a left spinning particle to a right spinning one.
— a 'Majorana mass term' which connects a left spinning neutrinos to the right spinning anti-neutrino. Such a term transforming particle into antiparticle cannot exist for charged particles because of the charge conservation; it can be present for neutrinos which have no charge.

The result of this is the existence of three heavy right handed neutrinos which happen to be nearly sterile…(they would interact million or more times less than the usual neutrinos) ah! they could be dark matter and generate the matter asymmetry (or not). Now there is something very neat in this: we need right handed neutrinos to complete the picture from the very moment that neutrinos have a mass. (there are more complicated scenarios to play their role but minimal is always more attractive)

So far this type of research has been dubbed 'exotic' in the LHC collaborations, I think because it was not in the initial research plan, but maybe also because that discovery of massive neutrinos is beyond the usual Supersymmetric BSM, and also because the so-far standard "next step in particle physics", the linear collider, can do little about it.

However this 'neutrinos are just missing energy ' attitude is in the process of changing at the LHC and in planning future experiments and colliders; the search for right-handed neutrinos is becoming a subject of focus.

Of course this is so far only a possible ‘natural’ solution of the neutrino questions, but any statement that no physics beyond the standard model has been found is just a sign of ignorance.

NB: Super-symmetry can also answer these questions, at the cost of being very considerably non-economical — and getting us in trouble in a nearly infinite number of ways, too.

24. **Cormac O Rafferty**
October 10, 2017

“The show has always done a good job of getting the science right”. Does it?
I spotted two very basic errors
(i) “there’s a particle called a squark, which could prove supersymmetry”
There is a world of difference between “give support for ” and “prove”. This is very basic stuff I would expect the program advisors and PW to know.
(ii) the general theme of the piece is that because certain particles have not turned up, SUSY must be dead. A great many science journalists , and PW, have been flogging making this statement for years now. It should be obvious that it is an overstatement. All we can say is that certain models have been ruled out.
That’s 2 basic misunderstandings of how science is done, in one passage…

25. **srp**
October 10, 2017

The only problem with the BBT episode is that they previously established (and told countless jokes based on the fact that) Leonard is not a HEP theorist but some sort of tabletop experimentalist (I think condensed matter or atomic physics) who uses lasers in his work. So Leonard should have been as cheerful as many of the non-HEP commenters above.

26. **Peter Woit**
October 10, 2017

Alain Blondel,
I’ve been guilty sometimes of the shorthand of referring to the current situation as “nothing beyond the Standard Model”, partly because the neutrino mass situation is complicated and can be accommodated by relatively straightforward extension of the SM. So given only a few words to work with, “we have seen BSM physics” seems about as misleading to non-experts as “we have not seen BSM physics”. I agree though that this issue of understanding right-handed neutrino fields and the neutrino sector in general is the most promising area where something is going on that we don’t understand well. I’m very interested to hear of the possibility of looking for something new in this sector at colliders. What specifically did you have in mind, and can you give any references?

27. **Anon**
October 11, 2017

Hi Peter,

There is no “straightforward” or an accepted minimal (or a standard) way of accommodating the neutrino mass in the Standard Model Lagrangian — it can be written as a
i) Dirac neutrino by adding right handed neutrinos, or
ii) Majorana neutrino with Type 1 seesaw term by adding right handed neutrinos or
iii) Majorana neutrino with Type 2 seesaw term by not adding a right handed neutrinos but instead adding a Higgs triplet field that couple to the existing left-handed neutrinos.

Therefore the standard practice seems to be not to do any of the above — thereby not putting the neutrino term in SM Lagrangian – but instead to state the PMNS matrix and neutrino mass squared differences as an addendum.

Which leaves the SM in the same or even in a worse boat than say the minimal left-right symmetric model, where there is a standard accepted Lagrangian that includes (in fact it predicts) the right handed neutrino. So in the standard LR model you would write down the Lagrangian, and not put the PMNS matrix in the addendum.

The issue with colliders for something new in neutrino physics is that the canonical seesaw scale (M_R or mass of right handed neutrino) is 10^14 GeV. That is the scale we get with order 1 Yukawa couplings (h ~ 1) from observed small neutrino masses (m_nu) and weak scale (v). M_R = h^2 v^2/m_nu

This scale is short of the GUT scale and Planck scale, and appears to be the seesaw scale for new physics. One may have to look for non-collider experiments for more info on the neutrinos.

Of course if h is smaller than 10^-5 for all the 3 generations of neutrinos, then the neutrino physics maybe at the colliders. But that maybe wishful thinking.

28. **Anon**
October 11, 2017

We should also remember that the Standard Model had actually predicted that the left-handed neutrino would be massless. This was a prediction it made for the mass of a known particle. And this is unlike the discovery of additional generations that were unknown particles.

29. **Peter Woit**
October 11, 2017

Anon,

Thanks for the comment. To clarify what I had in mind, I think of just adding right-handed neutrinos and Dirac mass terms as a “straightforward” extension of the Standard Model, one that it is a bit misleading to describe as “BSM”. The possibility of Majorana mass terms makes the whole subject much more interesting, and such mass terms and a seesaw mechanism would deserve to be called a “BSM” phenomenon. But, as far as I know, we currently have no evidence for this latter possibility.

30. **Anon**
October 11, 2017

Hi Peter,

I dont think adding right-handed neutrinos and only Dirac mass terms is “straightforward” 🙂 — in fact it may be taking you on the path nature did not pick.

This is because once you add the right-handed neutrino the most general SM Lagrangian will have Majorana masses for the right-handed neutrino. So you are setting the Majorana masses to zero by imposing a B-L symmetry which is ad hoc. In the original SM without right handed neutrinos B-L symmetry has an anamoly.

Another issue is that if neutrino is a Dirac particle then it can have an electric charge that is fractional and very small — its electric charge becomes a parameter that we have to determine by experiments and is not necessarily zero. In the std model the electric charge is quantized only if the neutrino also has a Majorana mass term. Only if the electric charge is quantized, do we know from theory that the electric charge of neutrino is zero.

Of course the experimental bound on neutrino's electric charge is very very small. But the electric charge of the neutrino is an additional parameter of the std model with Dirac neutrinos.

31. **John**
October 12, 2017

In the show it was mentioned how Leonard had to redeem himself about saying SUSY might not be found. Well Forbes did something just like that with an article by Ethan Siegel titled: "The Multiverse is Inevitable, And We're Living In It". He goes on to write: "the Multiverse is a theoretical prediction that comes out of the laws of physics as they're best understood today. It's perhaps even an inevitable consequence of those laws: if you have an inflationary Universe governed by quantum physics, this is something you're pretty much destined to wind up with."

32. **Peter Woit**
October 12, 2017

John,
At least Siegel doesn't repeat the usual dangerous pseudoscience about the multiverse showing that physical constants are environmental. In his explanation our "best theory" predicts lots more universes with the same physics as our own, something we can't ever check so is metaphysics rather than science. That's accurate enough, although calling a single field inflaton model with no understanding of the potential or of how it couples to matter a "best theory" is pretty misleading, in that it's more of a toy model than a theory.

All: a correspondent pointed me to an interview with Weinberg very much like the one on the TV show, I added some material to the posting about that.

33. **Curious Wavefunction**
October 12, 2017

The episode was funny and real, but I was disappointed that it played into the belief that particle physics is the only physics worth doing and that if it's at a dead end, physics as a whole must be at a dead end. I think this is exactly the criticism that people like Chad Orzel have had. There's plenty of excitement in other areas of physics like condensed matter and biophysics which somehow is never discussed in these discussions pertaining to the "death of physics".

34. **Mark Roulo**
October 15, 2017

This post reminds me of a (possibly fictional) exchange that Charlie Munger (Warren Buffet's long time business partner) reported between Charlie's old business partner, Ed Hoskins, and a fishing guide up in Minnesota:

> "Are any muskies caught in this lake?" asked Hoskins
> "More muskies are caught in this lake than in any other lake in Minesota. This lake is famous for muskies."
> "How long have you been fishing here?"
> "Nineteen years."
> "How many muskies have you caught?"
> "None"

Various Topics in Interpretation of Quantum Mechanics

October 11, 2017
Categories: Quantum Mechanics

A couple of recent discussions about quantum mechanics that may be of interest:

- There's a recent paper out by Don Weingarten that looks looks like it might have a different take on the fundamental "many-worlds" problem of, as he writes:

  > how in principle the definite positions of the macroscopic world emerge from the microscopic matter of which it is composed, which has only wave functions but not definite positions.

  My naive feeling about this has always been that the answer should lie in a full understanding of the initial state of the measurement apparatus (+ environment), that it is our imperfect probabilistic understanding of the initial state that limits us to a probabilistic understanding of the final state. I found Weingarten's investigation of this intriguing, although I'm not sure that the language of "hidden variables" is a good one here, given the use of that language in other kinds of proposals. By the way, Weingarten is an ex-lattice gauge theorist who I had the pleasure of first meeting long ago during his lattice gauge theory days. He at some point left physics to go work for a hedge fund, I believe he's still in that business now.

  Luckily for all of us, Jess Riedel has looked at the paper and written up some detailed Comments on Weingarten's Preferred Branch, which I suggest that anyone interested in this topic look at. Discussion would best be at his blog, a much better informed source than this one.

- Gerard 't Hooft has a remarkable recent preprint about quantum mechanics, with the provocative title of Free Will in the Theory of Everything. I fear that the sort of argument he's engaging in, trying to ground physics in very human intuitions about how the world should work, is not my cup of tea at all. Instead, what has always fascinated me about quantum mechanics has always been its grounding in very deep mathematical ideas, and the surprising way in which it challenges our conventional intuitions by telling us about an unexpected new way to think about physics at a fundamental level.

  For more discussion of the paper, there are Facebook posts by Tim Maudlin here and here in which he argues with 't Hooft. I confess that I wasn't so sure whether to take the time to read these, and after a short attempt gave up, unable to figure out precisely what the argument was about (and put off by Maudlin's style of argument. Do philosophers really normally behave like that?). Links provided here in case you have more interest in this than I do, or better luck getting something out of it.

Comments

1. **vmarko**
   October 11, 2017

   I won't judge Maudlin's style of discussing, nor do I know if other philosophers have similar style or not. But as far as the content of the discussion goes (at least the parts in the above links), I believe the following is the crux of the matter.

   Prof. 't Hooft seems to be fighting against a theorem (Bell's theorem). Tim Maudlin has studied the theorem top-to-bottom, upside-down and inside-out, and serves as a voice for the theorem to push back.

   In short, Prof. 't Hooft's argument is that one can have a violation of Bell's inequality in a local deterministic theory if one additionally imposes the law of "conservation of ontology" (I don't want to go into a technical specification what this means). Maudlin's counterargument is that, if one does impose that conservation law, then that law must be non-local (as per Bell's theorem), rendering the full theory nonlocal.

   And then there are a gazillion of off-topic sideway arguments going back and forth, which are ultimately irrelevant for the above argument. But the bottomline seems to be the above.

   IMO, I fear that Prof. 't Hooft is on the loosing side here — while fighting against a theorem can be very instructive, insightful and educational, eventually it has to fail.

   The short abstract of the whole discussion can be found in the second Maudlin's facebook post, near the end, in the allegorical dialogue between Vladimir and Estragon.

   HTH, 🙂
   Marko

2. **Tom Andersen**
   October 11, 2017

   It seems that until quantum mechanics has an experimental problem, the arguments about how to solve the measurement problem are going to be difficult to pick from. That's why experimental results like Vinante2016 https://arxiv.org/abs/1611.09776 are the most interesting (to me).

3. **a**
   October 12, 2017

   I read most of the exchange but was hampered by my ignorance of the meaning of "ontological state". Google didn't help me. Can anyone provide a definition?

4. **vmarko**
   October 12, 2017

   a,

   The notion and some properties of ontological states are on page 15 of Prof. 't Hooft's arXiv paper.

   HTH, 🙂
   Marko

5. **Dave Miller**
   October 12, 2017

   Peter,

I read all the way through both of the (long!) posts. And, I have read enough stuff by a lot of philosophers that I can answer your question: “Do philosophers really normally behave like that?”

No. Maudlin is much more comprehensible and much more knowledgeable about science than most philosophers I have read. I suppose the obvious example for incomprehensibility is Hegel (whom I thankfully have not read!), but Maudlin is more comprehensible even than most twentieth-century “analytic” philosophers. As for his knowledge of science, compare Maudlin to Popper’s writings on QM or J. L. Mackie’s various papers on science: both Popper and Mackie are very comprehensible, usually, and often insightful, but they got all mixed up when they wrote about science.

I know a fair amount about Bell’s work, and Maudlin quite accurately describes that work.

The difference between Maudlin and ‘t Hooft can be summarized very easily:

1) ‘t Hooft uses “locality” to refer to equal-time commutation relations, as is common among physicists. Maudlin uses “locality” in a more general sense defined by John Bell that applies to a much broader class of theories, including ones in which ETCRs make no sense. ‘t Hooft seems fairly clearly not to understand that Bell’s sense of the word is not the same as his.

2) ‘t Hooft, Maudlin, Bell, and pretty much everyone who has carefully thought about this agrees that a sort of “superdeterminism” is logically possible in which what experimenters choose to measure is pre-ordained from the beginning of the universe and that, in principle, this would sort of allow one to evade Bell’s theorem. Maudlin (and most other people who have considered this possibility) think this idea is infinitely bizarre. ‘t Hooft disagrees.

On point 2, I am personally with Maudlin, but ‘t Hooft could prove us both wrong if he could come up with any sort of actual concrete comprehensible theory which is superdeterministic and which reproduces the sort of results by which QM violates Bell’s theorem.

So far, ‘t Hooft has not done so.

I’m pretty sure he never will, but then I have been surprised before.

Dave

6. **SteveB**
October 12, 2017

‘t Hooft’s new, free book explains a lot of his thinking and the “ontological state”. Given his assumptions, it seems he can argue against Bell. The book is: The Cellular Automaton Interpretation of Quantum Mechanics

http://www.springer.com/us/book/9783319412849

7. **Tim Maudlin**
October 12, 2017

Ah, well. Style is a matter of taste. For what it is worth, after many decades of people trying to tell ‘t Hooft that his whole project is doomed by Bell’s theorem, he got engaged enough in this discussion to actually sort out that he has been using some terminology (in particular “superdeterminism”) in a completely idiosyncratic way, which has meant that everyone has been talking past each other for these many years. Everything is becoming clarified.

It’s actually the first time I have written anyone a play in order to make a point. I’m rather proud of it.

8. **Dave Miller**
October 12, 2017

Hi, Tim. I’m not quite sure the play worked — but then I did not really like the original by Sam Beckett, either.

Yes, I think ‘t Hooft just does not understand that he is just using terms differently from many people interested in the subject, although I think the main problem is the two different meanings of “locality”: i.e., I think it is clear enough what is going on with “superdeterminism.”

Anyway, congratulations to both you and ‘t Hooft for trying to get through the communication problems. And, just maybe ‘t Hooft will come up with something and surprise both you and me: betting against Gerard ‘t Hooft is not the safest of bets!

Dave Miller

9. **Manfred Requardt**
October 13, 2017

I think, Peter is right. It is evident that the microscopic conditions of the initial state of measurement instrument and environment are only incompletely known and hence the outcome will be of a stochastical nature. Furthermore, what Bell really tells us is that quantum theory and the micro state of quantum space time are intrinsically non-local. This can also be learned from e.g. Black Hole physics. This kind of non-locality is however not of an ordinary nature.

10. **Scott**
October 13, 2017

Hi Peter. Sorry this is off topic but just wanted to let you know
about a recent pro multiverse article. https://www.forbes.com/sites/startswithabang/2017/10/12/the-multiverse-is-inevitable-and-were-living-in-it/

11. **feynman fan**
October 13, 2017

Peter, have you read Feynman’s interpretation in his 1985 book “qed” (not to be confused for the 1965 book written before he discussed the problem with David Bohm). In it, while ignoring Bohm’s pilot wave, Feynman argues that the path integral of qft explains the wave function collapse in 1st quantization. I’ve never seen any discussion of this on any high profile site.

Basically, the different possible interactions of long-lived (on mass shell) particles with random field quanta manifestations in the vacuum provide a physical mechanism for indeterminacy. A short-lived pair production loop near the path of an electron’s orbit will affect that path, causing indeterminacy. Thus, Feynman comments (in a footnote!) the uncertainty principle of 1st quantization is explained by taking as real possibilities the many different interactions possible between electrons (or other particles) and randomly occurring vacuum particles!

12. **Peter Woit**
October 13, 2017

feynman fan,
I hadn't seen that claim from Feynman (or anyone else) that QFT automatically solves the measurement problem. Doesn't sound plausible at all….

13. **feynman fan**
October 14, 2017

Peter: please see figure 65 in the 1985 book called QED by Feynman to be convinced!

Discrete Coulomb field quanta exchanges cause indeterministic electron orbits, in that analysis:

'I would like to put the uncertainty principle in its historical place … If you get rid of all the old-fashioned ideas and instead use the ideas that I'm explaining in these lectures – adding [amplitudes for all possible paths, by way of the path integral] for all the ways an event can happen – there is no need for an uncertainty principle!' – Richard P. Feynman, QED, pp. 55-56.

'… when seen on a large scale, they travel like particles, on definite paths. But on a small scale, such as inside an atom, the space is so small that there is no main path, no "orbit"; there are all sorts of ways the electron could go, each with an amplitude. … we have to sum the [wavefunction amplitudes for each possible path] to predict where an electron is likely to be.' – Richard P. Feynman, QED, pp. 84-5.

14. **Low Math, Meekly Interacting**
October 14, 2017

Apologies if this isn't apropos.

I watched this interview maybe a year ago, and the statement above jogged my memory. Check out around 8:55 (and a little before for some background). I believe what Gell Mann is saying may be along the lines of what feynman fan is proposing. If I understand correctly, what Gell Mann is speculating on is the notion that the path integral isn't just a tool for calculating. It is, in some sense, fundamental.

I have no opinion on this, nor the intellect to rightly have one. Just thought it might be interesting to note.

https://www.youtube.com/watch?v=f-OFP5tNtMY

15. **Peter Woit**
October 14, 2017

feymanfan and LMMI,
I don't think the fact that you can formulate QM in a path integral formalism solves the measurement problem. If you interpret the path integral as a probability measure, one problem is that you need to do this in imaginary time, another is that this tells you about the wave function, whereas in measurement theory what governs the probability is the norm-squared of the wavefunction.

16. **feynman fan**
October 14, 2017

Feynman (in Fig 65 of QED) uses Euclidean space for the path integral. All he is saying is that the measurement problem of "wavefunction collapse" in QM is fake news because:

1. There is a separate wavefunction amplitude for each Moller scattering event in the interaction of the Coulomb field for a particle in QFT, whereas there is only a single wavefunction in QM because QM falsely uses a classical Coulomb field.

2. To solve the measurement problem, simply replace the classical Coulomb field potential in the Hamiltonian of QM with a proper Moller scattering QFT path integral, and you will no longer have a single wavefunction collapse paradox. Instead, you will see that you are dealing with numerous wavefunction amplitudes via the corrected (truly quantized) Coulomb field. If you try to "measure" the position of the electron, that measurement involves using a quantum field, and adds another Feynman interaction diagram (with its own wavefunction amplitude)!

17. **Dave Miller**
October 15, 2017

feynman fan,

I took Quantum Mechanics from Feynman in the 1974-75 academic year and then took Intro to Elementary Particle Physics from him in the 1975-76 year.

He never offered the interpretation you are attributing to him.

He was, in fact, not much interested in the "measurement problem" one way or another (I tried to get him interested, to no avail).

What you have quoted is just the common-sense stuff about the path integral that all competent physicists have known about for decades. Sorry, but the "measurement problem" refers to something else. (And I am pretty sure Peter does not want me or anyone else to give you a tutorial on all this in this comments section!)

18. **tulpoeid**
October 17, 2017

Comments by Miller and Maudlin above say that it is now realized that 't Hooft uses a definition of "superdeterminism" which is different than others', the latter being that everything was fixed at genesis (I guess?).

I am mildly surprised, since the different definition seems to be known for some time now; Hossenfelder wrote the following and also, in a funny coincidence, I was told about this definition by an unrelated colleague last week:

"But really, this is a very misleading interpretation of superdeterminism. All that superdeterminism means is that a state cannot be prepared independently of the detector settings. That's non-local of course, but it's non-local in a soft way, in the sense that it's a correlation but doesn't necessarily imply a 'spooky' action at a distance because the backwards lightcones of the detector and state (in a reasonable universe) intersect anyway."
http://backreaction.blogspot.co.uk/2013/10/testing-conspiracy-theories.html

Note though that in http://backreaction.blogspot.co.uk/2016/01/free-will-is-dead-lets-bury-it.html she says that they themselves might not even agree on what they mean 🙂

Imho both cases are definitely not worthy spending time and money on, I'm just mentioning it in the context of this discussion (which proves more absorbing than it ought to!).

19. **tulpoeid**
October 17, 2017

In multiverse links, it now seems that "relinquishing refutability" has entered the mainstream and we are looking for the next big annoying thing to get rid of:

http://capp.ibs.re.kr/_prog/seminars/index.php?mode=V&site_dvs_cd=capp_en&menu_dvs_cd=0601&mng_no=9221

50 Years of Electroweak Unification

October 14, 2017
Categories: Uncategorized

The 50th anniversary of electroweak unification is coming up in a couple days, since Weinberg's A Model of Leptons paper was submitted to PRL on October 17, 1967. For many years this was the most heavily cited HEP paper of all time, although once HEP theory entered its "All AdS/CFT, all the time" phase, at some point it was eclipsed by the 1997 Maldacena paper (as of today it's 13118 Maldacena vs. 10875 Weinberg). Another notable fact about the 1967 paper is that it was completely ignored when published, only cited twice from 1967 to 1971.

The latest CERN Courier has (from Frank Close) a detailed history of the paper and how it came about. It also contains a long interview with Weinberg. It's interesting to compare his comments about the current state of HEP with the ones from 2011 (see here), where he predicted that "If all they discover is the Higgs boson and it has the properties we expect, then No, I would say that the theorists are going to be very glum."

Today he puts some hope in a non-renormalizable Majorana mass term for neutrinos as evidence for new physics. As for the future:

> As to what is the true high-energy theory of elementary particles, Weinberg says string theory is still the best hope we have. "I am glad people are working on string theory and trying to explore it, although I notice that the smart guys such as Witten seem to have turned their attention to solid-state physics lately. Maybe that's a sign that they are giving up, but I hope not."

On this last sentiment, I have the opposite hope. He also shares what I think is a common hope for what will save the field (a smart graduate student with a new idea):

> Weinberg also still holds hope that one day a paper posted in the arXiv preprint server by some previously unknown graduate student will turn the SM on its head – a 21st century model of particles "that incorporates dark matter and dark energy and has all the hallmarks of being a correct theory, using ideas no one had thought of before".

Perhaps current training of graduate students in theory should be rethought, to optimize for this.

Update: A colloquium talk by Weinberg on this topic will be live-streamed here on October 17.

Comments

1. **Low Math, Meekly Interacting**
   October 14, 2017

   Weinberg must be aware of his great fortune: He was alive during a period when his theories about fundamental physics could be subjected to rigorous empirical challenge, and either survive or not. I am lucky to have been alive to see "the" Higgs boson discovered. To have been born later may mean to have been born after the end of that era of fundamental discovery.

2. **Art**
   October 14, 2017

   From Close's article: 1) "QED is perhaps the simplest example of a general class of "non-abelian gauge theories"". Um, QED is abelian, right? 2) "it shows how it is easier to be Beethoven or Shakespeare than to be Steven Weinberg". Quite a stupid statement, no?

3. **vmarko**
   October 14, 2017

   I am a bit surprised by the article of Frank Close. It's all about Weinberg, mentioning only in passing that Glashow and Salam&Ward had figured out the SU(2)xU(1) symmetry no less than eight years earlier, in 1959. What Weinberg actually did in the 1967 paper was to combine this with the Higgs mechanism in order to break the symmetry spontaneously. And yes, Weinberg does quote one of the Glashow papers (in a slightly dismissive tone), so at least he did know about it, and most probably read it.

   I don't want to put off Weinberg's contribution to EW model, but I think Close doesn't do justice to Glashow and Salam, let alone Ward.

   Best, 🙂
   Marko

4. **Benson Woo**
   October 14, 2017

   It was wonderful to read Frank Close's account of the history of Weinberg's paper, especially the epiphany Weinberg had while driving his red Camaro to MIT. What if he had been doing something different that Sept 1967 day? These behind the scenes stories of how discoveries get made happen all the time.

5. **physicsphile**
   October 15, 2017

   Very nice articles. I was surprised to read that Weinberg works with the TV on! I would find that too distracting.

6. **Frog Leg**
   October 16, 2017

   "Weinberg also still holds hope that one day a paper posted in the arXiv preprint server by some previously unknown graduate student will turn the SM on its head – a 21st century model of particles "that incorporates dark matter and dark energy and has all the hallmarks of being a correct theory, using ideas no one had thought of before"."

   "Perhaps current training of graduate students in theory should be rethought, to optimize for this."

   I apologize if you've covered this before, but how do you think would graduate training be different from today?

7. **Peter Woit**

October 16, 2017

Frog Leg,

This really is a different and large topic, better for another time, but some obvious ideas are:

1. the parts of the usual graduate training that traditionally have been devoted to getting students up to speed on the most "advanced" topics (in HEP theory: e.g. GUTS, SUSY extensions of the SM, superstring theory) should be deemphasized, in favor or explaining what problems those ran into, why they haven't worked.

2. instead of rushing through the fundamentals of basic tools like QFT, it might be a good idea to devote more time to those and give students a better chance to become familiar with the trickier parts of those fundamentals. This is more likely to make it possible for someone approaching the subject with a fresh perspective to possibly come up with a new and successful fundamental idea.

8. **David Derbes**
October 16, 2017

@vmarko:

The CERN Courier article is an edited excerpt from Close's *The Infinity Puzzle*. The article focused on Weinberg, but the book doesn't. Salam, Ward, Glashow, Schwinger and many others (specifically 't Hooft and Veltman, and Higgs, Englert, Brout, Guralnik, Hagen and Kibble) are mentioned and receive large credit at various places in the book for work on the electroweak theory. You or I might weigh credit differently from Close, but nothing he wrote struck me as unfair while I was reading the book.

9. **Ilyas**
October 16, 2017

@vmarko and @David Derbes

David, you make a good point about Close's book (the source of the excerpt). There is a view that Close is somewhat negative about Salam in the context of the way in which he (Salam) was the third person that the Nobel committee chose. I dont know if you have seen the Noorman Dombey article (https://arxiv.org/pdf/1109.1972.pdf) but it is an interesting additional part of that little side-story around electroweak unification and who should (or should not) be credited. There are a few nice pieces written on the overall topic, but regardless of the negativity around Salam I like Close's book. Its really well written, and it does not cut corners in trying to be "popular".

10. **Peter Woit**
October 16, 2017

Ilyas and others,
The Dombey article was discussed extensively here back in 2011
http://www.math.columbia.edu/~woit/wordpress/?p=3972
and the Close book a couple months later:
http://www.math.columbia.edu/~woit/wordpress/?p=4181

11. **a reader**
October 17, 2017

Steven Weinberg is a true giant, but his closing remarks, that "the actual work is so, well…it's so chillingly non-human. I need to feel that I am still part of the human race while I'm doing it," were a bit of an anticlimax. As Ulysses says in Dante's Inferno, "considerate la vostra semenza: fatti non foste a viver come bruti ma per seguir virtute e canoscenza." Rather than being "chillingly non-human," physicists' actual work is eminently human, and it is something our species can be proud of.

12. **piscator**
October 17, 2017

a reader: I took exactly the opposite message. Given how technically algebraically oppressive Weinberg's textbooks are (I know how good he is, but the style just doesn't work for me) I find more than slightly amusing the fact that they may have been written to a backdrop of old films or cheap TV.

13. **David Brown**
October 20, 2017

In the livestream "Reminiscences of the Standard Model" by Weinberg, he made an interesting comment (around 1:18:15 in the YouTube video) concerning the work of string theorists, "… they haven't settled down to any particular theory, and we don't know whether that works anyway. I hope it does."

14. **Lucimário Custódio**
October 21, 2017

Yesterday Steven Weinberg gave a online lecture, "The Rise of the Standard Models", invited by the brazilian physics society. Here's the youtube link:

https://www.youtube.com/watch?v=DG3OqvdjnA4

15. **paddy**
October 21, 2017

piscator,
Despite his personality, I found (as a student 40+ years ago) Weinberg's didactic "algebraic" approach informative. Mathematical and example driven texts are helpful for those of us who are not geniuses.

16. **Fiona**
October 22, 2017

Peter, maybe this preprint https://arxiv.org/abs/1710.06149 is of interest and worth a comment? They propose the "conformal standard model", an extension of the standard model up to Planck scale without susy and without loops.

Shut Up and Calculate!?

October 17, 2017
Categories: Multiverse Mania

I noticed recently that Nima Arkani-Hamed was giving a talk at Cornell, with the title Three Cheers For "Shut Up And Calculate!" In Fundamental Physics. No idea whether or not video is now or will become available.

From the abstract one can more or less guess what sort of argument he likely was making, and it's one I'm mostly in agreement with. "Shut Up and Calculate!" is pretty much my unspoken reaction to almost everything I read purporting to be about foundational issues in quantum mechanics. I have in mind in particular discussions of the measurement problem, which often consist of endless natural language text where one struggles to figure out exactly what the author is claiming. An actual calculation showing what happens in a precise mathematical model of a "measurement" would be extremely helpful and likely make much clearer exactly what the problem is (or, sometimes, whether or not there even is a problem…). Such calculations are all too few in a huge literature.

Over the last few years, while teaching and writing a book about the mathematics of quantum mechanics, the tedious exercise of trying to get all signs right in calculations has sometimes turned out to be quite illuminating, with tracking down a mysterious inconsistency of minus sign leading me to realize that I wasn't thinking correctly about what I was doing. I'm all too aware that this kind of calculational effort is something I too often avoid through laziness, in favor trying to see my way through a problem in some way that avoids calculation.

On the other hand, I'm not quite ready to sign up for "Three Cheers", might just stick to "Two Cheers". For a perfect example of what's wrong with the "Shut Up and Calculate!" philosophy, one can take a look at the forthcoming Workshop on Data Science and String Theory planned for Northeastern in a month or so. They have a Goals and Vision statement which tells us that they plan to:

> treat the landscape as what it clearly is: a big data problem. In fact, the data that arise in string theory may be some of the largest in science.

About being the "largest", I think they're right. The traditional number of 10^{500} string theory vacua has now been replaced by $10^{272,000}$ (and I think this is per geometry. With 10^{755} geometries the number should be $10^{272,755}$). It's also the case that "big data" is now about the trendiest topic around, and surely there are lots of new calculational techniques available.

The problem with all this is pretty obvious: what if your "data set" is huge but meaningless, with nothing in it of any significance for the problem you are interested in (explaining the Standard Model)? This is not a new project, it's an outgrowth of the String Vacuum Project, which I wrote about here, here and here. This started with a 2005 funding proposal, ended up getting funded by the NSF during 2010-2014. From the beginning there were obvious reasons this sort of calculational activity couldn't lead to anything interesting, and as far as I can tell, nothing of any value came out of it.

For an opposite take to mine on all this, see the paper Big Numbers in String Theory, by Bert Schellekens. It contains an odd June 2017 preface explaining that it was supposed to be part of special issue of *Advances in High Energy Physics* devoted to "Big Data" in particle and string phenomenology ("all the ways we use high performance computing in addressing issues in high energy physics, and (in particular) the construction of databases of string vacua"). This issue was cancelled "as requested by the Guest Editors". I wonder what the reason for this cancellation was, in particular whether it had anything to do with part of the topic of the special issue being considered by some to be obvious nonsense.

Comments

1. **paddy**
   October 17, 2017

   As an adherent of the shut-up-and-calculate school of thought, I was once asked how then to think of Schroedinger's cat. My answer: I don't, I'm a dog person.

2. **Armin**
   October 17, 2017

   "…the tedious exercise of trying to get all signs right in calculations has sometimes turned out to be quite illuminating, with tracking down a mysterious inconsistency of minus sign leading me to realize that I wasn't thinking correctly about what I was doing."

   Could you please elaborate a little? What was the "mysterious inconsistency"? the nature of the "illumination"? How does it relate to "shut up and calculate"?

   As stated, the paragraph gives the impression you are implying that many interpretational problems of quantum mechanics simply evaporate if one just does the calculations properly. Is that what you meant?

3. **Peter Woit**
   October 17, 2017

   Armin,
   The second paragraph was the one referring to interpretational problems of quantum mechanics, and it's there that I'm suspicious that some problems disappear if you start to look carefully at and actually calculate what is happening when you have a specific system that is "performing a measurement". For the kind of all too rare thing I have in mind, an example would be
   http://blog.jessriedel.com/2017/10/12/models-of-decoherence-and-branching/

   In my class and in my book there's nothing about the measurement problem or interpretational issues, the topic is the mathematical structure of the subject, and that's what I was referring to in the third paragraph. In that case, finding a minus sign made clear to me that I really needed to be using the dual of a vector space at some point when I thought I needed to be using the vector space itself. The clarification was just one of the mathematical structure of the problem, had nothing to do with interpretational issues.

4. **Mike McCracken**
   October 17, 2017

   Hi, Peter,

   Sorry to focus on a parenthetical here, but I'm intrigued by the following:
   "(and I think this is per geometry. With \$10^{755}\$ geometries the number should be \$10^{272,755}\$.)" Can you elaborate on this or provide a link? I'm not interested

in strings, but this sounds like a deep geometric idea on which I'm obviously ignorant.

Thanks,
Mike

5. **Peter Woit**
October 17, 2017

Mike McCracken,
You can find references in
https://arxiv.org/abs/1707.00655
No, there is no "deep geometric idea" here, just some complicated, ugly and uninteresting constructions. I wouldn't advise spending time on looking into the details of this, I'm certainly not going to.

6. **Bee**
October 18, 2017

No no, "big data" is so yesterday. Today is "deep learning" and "neural networks." Wait for it!

7. **Luca**
October 18, 2017

Yep, and the most amazing trait of the Deep Learning techniques is that algorithms can give you the right answer, but it's impossible to understand why you've got that particular answer. In the end one could find the right vacuum from a zillion of possibilities and still having absolutely no explanation at all.

8. **tulpoeid**
October 18, 2017

Yep, and tomorrow's thing is emergent computation. It could run on the landscape and realize that all solutions point to only one viable one, namely Standard Model with QFT. It would be a triumph of strings' predictive power.

9. **Dave Miller**
October 18, 2017

Peter,

As someone with an interest going back decades in the foundations of quantum mechanics, I generally agree. Your recent post regarding the Maudlin-'t Hooft discussion is a case in point: *logically* something along the lines of what 't Hooft proposes might be possible. But until he or someone actually gives at least a toy model… well, it is very difficult to evaluate his Big Idea.

On the other hand, if no one ever gives any thought to "superdeterminism," "retrocausation," etc., we might fail to trigger the random thoughts that eventuate in a real theory. Einstein did, after all, get a lot of mileage out of his youthful curiosity about what it would be like to ride a light wave!

I don't think decoherence will solve the "measurement problem," but, like you, I think it is worth pursuing to sharpen our intuition and understanding of the deeper issues. And — who knows? — maybe the big contribution of string theory will be to sharpen thoughts about temporality, causation, etc. Polchinski suggested something along those lines some years ago, I think in his Rev. Mod. Phys. article.

Dave

10. **Atreat**
October 18, 2017

Here is the problem: "shut up and calculate" reduces to "shut up" if you don't know how to calculate. And in the case of quantum interpretations that leaves everyone silent.

A better slogan might be, "By all means keep talking, but in the absence of the ability to calculate let's not fool ourselves that we are close to a solution."

11. **Adrian C Keister**
October 18, 2017

As a "hype iconoclast", you might be interested to know, in the unlikely case you don't already, that there is just as much empty hype in "big data" as there is in string theory, SUSY, etc. Probably more, actually. As you mentioned, if there's no inherent meaning in the data, you can run it through fancy algorithms all day long and produce absolutely no new insights whatsoever.

@Atreat: Your "better slogan" is certainly better, but I'm just not sure it's got that catchiness we expect of meaningless slogans. You should work on that.

12. **Peter Woit**
October 18, 2017

Bee,
They're on it.

"In comparison, a renewed thrust to study the landscape (articulated e.g. at String Phenomenology 2017) has many of the same goals as the SVP but places an increased emphasis on using modern techniques in data science to study the landscape. This approach is worth pursuing since the past decade has seen rapid advances in the areas of machine learning, distributed artificial intelligence, and generative adversarial networks. "

"generative adversarial networks" sounds like the latest thing, right out of Blade Runner 2049….

13. **Tim Maudlin**
October 18, 2017

This is a very telling sentence: "I have in mind in particular discussions of the measurement problem, which often consist of endless natural language text where one struggles to figure out exactly what the author is claiming."

Now I understand why you don't my style. It all natural language and no math. Indeed, I have often said that the way many physicists read papers is the exact complement to how I read them: they read the equations and skip the prose and I read the prose and skip the equations. I figure they have not made a mathematical mistake, and the whole issue is *how the mathematics is being used as a way to represent physical reality*. A piece of mathematics is just that: a piece of mathematics. It isn't, by itself, a physical theory. And it doesn't magically become a physical theory by just explaining how to use it to make some predictions. Then it is part of a predictive apparatus. To become a physical theory, or rather be part of a physical theory, a mathematical formalism has to be interpreted, or, as I say, accompanied by a commentary. The commentary explains exactly what is being posited to exist by the theory (the ontology) and how that is being represented by the mathematics. To take is simple example, the usual mathematical formalism of classical electromagnetism can be used to express completely different physical theories depending on whether one takes the (mathematical) electric and magnetic fields to represent something physically real or the scalar and vector potentials to represent something physically real. And no amount of staring at the math will tell you. That is in the prose commentary.

There is an extremely effective predictive apparatus called "quantum theory" that is not, in the sense I just articulated, a physical theory at all. So much of the discussion in foundations of quantum theory is about the commentary, not the math. If you are struggling through the prose (which in many cases can be obscure and poorly expressed because the ideas being defended are simply unclear) you are struggling though the essential part. Looking for equations will not be much help.

John Bell, in "Against 'Measurement'", clearly explained the problems with standard presentations of the quantum theory. The problems are not in the math, they are in the prose. And on the other side, a theory like Bohmian mechanics, which completely solves the measurement problem, is not appreciated at all because there is no change to the math of Schrödinger's equation. There is, of course, one extra equation, the guidance equation, but to appreciate its significance you have to read the prose. And then there is a lot of detailed mathematical analysis of those equations that has been worked out over 40 years that almost no one is even aware of or reads.

This is a mess. But the way out of the mess is not to give even one cheer for "Shut Up and Calculate". Pablo Echenique-Robba had it much closer to the mark in the title of his paper: "Shut up and let me think. Or why you should work on the foundations of quantum mechanics as much as you please."

14. **Frederica**
October 18, 2017

Peter,

there are quite a number of calculations of decoherence times, and measurements of the values. Are they not convincing for you?

15. **Peter Woit**
October 18, 2017

Tim Maudlin,
The earlier objection to your style was about something different, things like : "And for years and years 't Hooft gives replies that only verify that he has not understood Bell's work and keeps at it."

About arguments over interpretations, I understand this has nothing to do with the mathematical formalism. My problem when I start reading such arguments often has to do with imprecision of language: when you and 't Hooft are using the term "superdeterminism", what exactly are you talking about? Of course part of my problem is a lack of expertise, insufficient time spent immersed in these debates and thus insufficiently aware of conventions for exactly how terms are used and ambiguities resolved. My sympathy here with "shut up and calculate!" is that often when I can't tell exactly what argument is being made, my high school English teacher's demand "Be specific, give examples!" comes to mind. Extremely helpful often would be not necessarily equations, but an explanation of what the issue is in the context of a specific physical measurement process.

Frederica,
I have no problem with calculations relevant to interpretational issues, I'm just saying that many interpretational debates I read could be clarified a lot by reference to specific calculations (see above).

16. **Tim Maudlin**
October 18, 2017

Peter,

You will be glad to know that as a consequence of this discussion I have recommended dropping the word "superdeterminism" in favor of "hyperfine tuning", which is fine tuning of the initial conditions of a theory not just to some macrostate (e.g. the Past Hypothesis) but to a microscopically specified set of states. I think it is a more descriptive term. In fact, I offered a whole set of exact definitions.

As to the particular sentence of mine you cite, I am puzzled. It is blunt, but accurate. The title of your blog is also blunt, but accurate. But maybe it's not worth the effort to figure out your problem here.

17. **vmarko**
October 18, 2017

Regarding the interpretations of quantum mechanics, they are always in the prose (as Tim says), but this is basically by definition, since interpretations should all agree on the math, and hence the math is the same in each interpretation. If two models do not agree on the math, then they are different theories rather than different interpretations.

But the measurement problem is not an interpretational issue, it is rather a bit more serious. QM as a theory, both the math and the prose included, fails to tell us which processes constitute measurements (and, say, lead to the collapse of the wavefunction) and which don't. The notion of measurement is always introduced intuitively, on a case-by-case basis, and plugged into the QM formalism by fiat, for every particular situation. Either by saying that the wavefunction collapses at some random moment of evolution, or by specifying the pointer basis for the decoherence and picking one particular vector from it, or by saying that this particular beable was detected in the apparatus, or otherwise. I agree with Peter that just speaking in prose about the measurement problem is not enough, and that one should instead write some equations that produce an actual model for the measurement process. And it should be clear that these equations should supplement QM, rather than be derived from it. Simply talking prose about it will never fill in for such equations.

HTH, 🙂
Marko

18. **Robert A.Wilson**
October 18, 2017

As an amateur in the field, I find the maxim "shut up and listen to the experts squabble" very useful. It does not take all that much listening to distinguish the experts who have thought deeply, but may ultimately be misguided, from those who shout loudly but have nothing to say.

19. **a1**
October 18, 2017

Reading "How the Hippies saved Physics' could remind you of some things the Calculators missed (not just the fun). And, by the way, the post/review/ from 2011 seems more nuanced than today's.

20. **Peter Woit**
October 18, 2017

a1 is referring to this
http://www.math.columbia.edu/~woit/wordpress/?p=3785

21. **Robert A.Wilson**
October 18, 2017

On a more serious, but less important, note, several comments above make the distinctions between mathematical errors, physical errors, and interpretational errors. All three need to be addressed. As a mathematician, I am best qualified to comment on the mathematical errors. In the way the standard model is routinely explained, there are many serious mathematical errors. Many of these have been pointed out in this blog, such as the particularly egregious "so(3,1)=su(2)+su(2)". Physicists rightly question whether this really makes any difference. And if you just "shut up and calculate" then most of the time the answer is no, it makes no difference. Clearly, the standard model works, and the calculations give the right answer. Most of the time. But this doesn't address the issue. Much of the physical argument is based on an inconsistent, not to say incoherent, interpretation of the mathematics. What should a mathematician who is concerned about this do? If he says "the mathematics is wrong", he is told "it doesn't matter, it works"; if he says "the physical interpretation is wrong", he is told "shut up, what do you know about it?". And heaven help him if he tries to say "the physics is wrong"! But if we are ever to solve this problem, we have to start from the assumption that probably all three are wrong, and start listening to constructive suggestions for what to do about it. In particular I whole-heartedly agree with Peter that getting the signs right is an educative experience, that is not about getting the right answer, but about understanding the nature of the underlying model, and, dare I say it, reality. I don't think anyone could say it doesn't matter whether the Lorentz group is SO(3,1) or SO(4).

22. **tulpoeid**
October 18, 2017

(Tim Maudlin, he refers to your tone verging on the sarcastic and even conceivably impolite.)

23. **a**
October 19, 2017

I think most agree that t'Hooft was one of the greatest physicists of the latter half of the 20th century. And apparently he is one of the most straightforward and honest people you'll see on the web, judging by his comments. But it looked to me like he was thinking a little fuzzily on the quantum interpretation business. Yes, the commentary was, um, rude, and I doubt t'Hooft would have taken that tone even in his heyday (which included some decidely modern topics) but the comments made some sense and were helpful (to me, at least, a nonexpert) in sorting through the issues. It also motivated me to reaquaint myself with Bell's papers (ok…basically just speakable and unspeakable); his style is very appealing (for example, he was a great Einstein fan but maybe not so much Bohr, as was made clear in the very amusing first appendix to Bertlmann's socks).

24. **Tim Maudlin**
October 19, 2017

Tulopoid: Yes, it is. So is "Not Even Wrong": it means that a theory is so confused that you can't even say that it is false because it doesn't say anything coherent. Indeed, the overall tone of my original post—which was not addressed directly to Prof. 't Hooft but at a wide swatch of the field—should come across as extremely exasperated because I was, and am, extremely exasperated. Literally years of theoretical effort has been wasted because people do not understand Bell's Theorem, which is nearly a mathematical triviality. The sociology of how this could happen is long and complicated, but the effect is there. And the systematic confusion of the notion of a "free variable" in a physical theory with the notion of "free will" as it applies to humans is so obvious as to be inexcusable. Just as Prof. Woit got rightly exasperated by the dominance of string theory in theoretical physics, people who work in the foundations of physics are exasperated that the most important and often simplest results in that field are systematically misreported and misunderstood.

It was not my intention, but if some naked exasperation (including occasional sarcasm) manages to bring some attention and light to these issues, that is a good thing. Certainly the very measured and elegant and immaculately careful and concise writing of Bell himself didn't do the trick. Sometimes people have to hit by a 2×4 to pay attention. That would have been beneath Bell, but I admit it is not beneath me.

25. **Dave Miller**
October 19, 2017

Re Bee's and Peter's comments on "deep learning" and physics: An interesting paper was posted on the arXiv earlier this year entitled Deep-Learning the Landscape! Despite the provocative title, this was basically an exercise at using deep neural nets (AKA multi-layer perceptrons) to "learn" various calculated quantities in algebraic geometry (specifically concerning Calabi-Yau manifolds).

The paper looks legit, and the results are interesting, though I think it is fair to say that the paper shows that neural nets are not yet ready to replace human mathematicians!

26. **tulpoeid**
October 19, 2017

(Maudlin: I see, I think this point was worth clarifying.)

27. **Chris W.**
October 19, 2017

Speaking of deep learning, see yesterday's news from Google's DeepMind team on AlphaGo Zero.

Echoing Luca's comment, the recurring question with such accomplishments is how and when they are useful. With AlphaGo, Go players can study and learn from the strategies that it discovers. That the strategies are useful and effective is readily verifiable, and this is obviously essential to AlphaGo's learning process. The fact that its learning—its experimentation—takes place in a completely automated way within a self-contained simulation environment on a high performance computing platform makes the process extremely rapid in human terms.

Leaving aside the problematic motivations for even attempting to apply such techniques to understanding the string theory landscape, how much of the above can be said to apply to physics? Obviously, if part of the learning involves doing actual experiments with real physical systems then it can't help be much slower, and subject to all the constraints that human experimentalists face every day. Learning non-trivial things about the physical world is much harder than learning something as artificial and well-structured as a board game.

By the way, I completely agree about the current level of hype in "big data". When commercial applications and marketing get involved, the amount of BS in circulation always rises precipitously.

28. **JoKing**
October 19, 2017

Hey Tim Maudlin,

You see, the reason that physicists when pressed won't posit any ontology for their equations is that this is not only not the most important part of what you might conceive of as a physical theory, but that it is in fact totally redundant and misplaced.

To give any credence to that statement that "such and such entity exists and this is how it is represented in the equations", you have to say what effect that entity will have in the real world. (Or else what do you mean?) Without an operational prescription, that statement is physically totally empty and can at best be viewed as a purely mathematical existential quantification.

What's more, the physical theory known as quantum mechanics in fact forbids you of talking about the physical reality of many of the objects you talk about when explaining what you think Bell did. (And I think Mermin now understands that meaning of "shut up" as well.)

My point is indeed that your grief with the physics community arises very early on from your epistemological misconception of a "physical theory" rather than from their misunderstanding of Bell.

29. **Bill Bailey**
October 19, 2017

Tim Maudlin,
There are many things natural language can't get at that math can. The math needs no prose commentary because the math is telling you what the abstract relationships are itself. Physicists read the equations to understand what is going on without needing a running prose commentary. Some things like "free will" are metaphysical speculations that may actually be incoherent under precise examination based on the structure of the mathematical relationships that physicists discover. These problems like "the measurement problem" may only arise because one wants to force a translation between our prose language and the mathematics. One may be introducing problems that are not there. But that's in your area. The dissolution of paradoxes and confusions.

30. **Peter Shor**
October 19, 2017

Peter,

My take on the "shut up and calculate" philosophy is that it's one of the reasons that quantum computation and information was not discovered by mainstream physicists, but by the motley crew of non-mainstream physicists, computer scientists, mathematicians, and semi-crackpots who discovered it. (And for the curious, I'm not going to say who I think falls into which category.)

And maybe you should consider whether "shut up and calculate" might not be a reason that people haven't spent more time studying quantum field theory deeply, the way that you are advocating.

On the other hand, I am also convinced that studying the foundations of quantum mechanics in the manner that many people have been doing it is a waste of time. But I believe the "shut up and calculate" philosophy's influence has been much broader and more negative than that.

31. **F4**
October 19, 2017

I attended Arkani-Hamed's talk (though when prompted by this post I realized that I recall shockingly few details about it now (something for which my memory is solely to blame); I also had to leave just as he was finishing skipping through the roughly sixty (?) percent of the slides that he didn't discuss at all).

Hopefully any recording made will be decent: he was forced to suffer some inexplicable (and inexcusable, I would say) technical difficulties before commencing, had to resort to standing directly in front of the lectern microphone, and eventually seemed to give up or forget and so spent most of the talk perambulating around the front of the hall sans amplification.

Anyway: I thought that he made it fairly clear at the beginning that both a rigorous shut-up-and-calculate pragmatism and a broader philosophical vision are necessary for real scientific progress. But indeed speculation must be grounded: a segment of the talk concerned Einstein's perserverance in improving his mathematical knowledge and his spurious justification of failed pre-GR proposals with a variety of philosophical principles. Arkani-Hamed also said that getting the mathematics of a concept right can be a (more or less) combinatoric exercise once you have delivered yourself into the right theoretical attractor space.

I don't remember much if anything said about applications to string theory in particular but then again he seemed to be ready to talk for at least two more hours than the time allotted.

32. **Tim Maudlin**
October 20, 2017

JoKing,

The question is not how a physical theory with clear ontological postulates (i.e. saying clearly what physically exists according to the theory) has any trouble at all with making empirical predictions, it is just the opposite: how can you derive empirical predictions from a theory that make no clear posits about what exists?

Quantum theory does not "forbid you from talking about" anything. How could it? I mean what is quantum mechanics going to do to me if I insist on talking about things like what really exists at microscope? Bohmian mechanics, for example, says that point particles do. And that tables and chairs and cats and people are composed out of point particles. And how the point particles move (what exists and how it behaves). So it automatically makes predictions about tables and chairs and cats and "measuring apparatuses" without having to mention "measurement" or any other problematic notion.

My conception of a proper physical theory as postulating what exists and how it behaves has zero epistemology built into it. It just isn't an epistemological thesis of any kind. So you are confused there.

33. **Tim Maudlin**
October 20, 2017

Bill Bailey,

A piece of mathematics without some indication of how it is being used as a mathematical representation of something physical just isn't, and can't be, a physical theory of anything. I mean, how could you tell even what it is supposed to be a theory of, what phenomena it is supposed to account for? How could you get experimental evidence for or against it? That extra information comes in the commentary. It is given in natural language, not mathematics.

As a simple example: take the mathematical formalism of classical E & M. Accompanied by the commentary: "The mathematical E and B vector fields represent the physically real electric and magnetic fields; the mathematical A vector and phi scalar potentials are mere mathematical conveniences that represent nothing physical" that math has one sort of physical significance. Accompanied by this commentary: The A vector field and phi scalar field represent physically real things, and the E and B fields are just mathematical conveniences" the very same math underlies a completely different theory.

I hope this helps make the point.

34. **Manfred Requardt**
October 20, 2017

I think Peter Shor is right. I know many mathematicians who think they do understand quantum mechanics if they understand the Schroedinger equation. Mathematics is only an important tool in natural science. Most of our important ideas arise in form of images and words. As far as I can see, many of our greatest colleagues were not of the shut up and calculate type (e.g. Einstein, Bohr, Heisenberg). I believe the really deep problems can never be found by shut up and calculate.

35. **André**
October 20, 2017

JoKing:
I think, "operational prescription" is just a euphemistic way of saying "shut up and calculate". It's of course nice to have a mathematical model that tells you what reading (with which probability) you will get from a photodetector under certain initial conditions, but at the end of the day most physicists will probably agree that the photodetector itself is built of atoms that need to be described quantum mechanically. So, from my point of view, it seems clear that the "operational prescription" can't be more than an effective theory.
Effective theories can be very useful, thermodynamics, for example, has quite a lot of applications for which it is completely irrelevant to understand the microscopic definition of temperature, entropy, etc., but if you want to gain some deeper understanding of the world, you will eventually end up asking what exactly these quantities are.
The way I see it, the measurement problem is pretty much at the same level. Quantum theory is a perfectly predictive theory for the outcome of specific experiments, but it doesn't teach us anything about what is "really" going on at a deeper level.
Also, I disagree that quantum mechanics forbids us from talking about the physical reality of objects. It is just that the real objects in (Copenhagen) quantum mechanics are very odd ones: the measurement devices/outcomes.

What I don't understand – and this is a purely sociological and not a scientific observation – is how many physicists are totally obsessive about the assumption that quantum theory must somehow be the end of the story, and not just some limit of some more elegant underlying theory. This is against everything we should have learned from the history of science, and somehow against the very idea of science.

36. **André**
October 20, 2017

Tim:
Although I very much agree with your point of view and enjoyed reading your conversation with 't Hooft, a question regarding your example for classical EM:
I could just consider a concrete set-up, say a Hall probe in an EM field, and have a perfectly well defined mathematical formalism that tells me: given this experimental set-up, my B-probe will show the value 0.5 Tesla (e.g.). For this, I don't have to give any reality to either E and B or A and phi.
Of course, you would still need a commentary, namely stating that the output of your mathematical algorithm gives you the reading of your B-probe, but I would still be interested about your view on such an approach, since I think it is pretty much what many physicists have in mind when they argue like JoKing and Bill Bailey.

37. **Bee**
October 20, 2017

@Dave Miller,

Thanks for the reference, looks interesting!

38. **Bee**
October 20, 2017

@Tim Maudlin,

Replacing one word with another word is not a definition.

39. **Urs Schreiber**
October 20, 2017

> To become a physical theory, or rather be part of a physical theory, a mathematical formalism has to be interpreted, or, as I say, accompanied by a commentary.

An established term for this in the philosophy of physics is *coordination*.

40. **Peter Woit**
October 20, 2017

All,
Sorry, but I have to cut off further attempts to use this as an interpretation of quantum mechanics discussion board, in effect telling people to shut up. I've been trying to delete those (many) comments that don't appear to make an argument worth reading, but have now had enough. Whatever my sins, I don't deserve having to moderate a discussion of exactly the sort I was criticizing as a waste of time.

41. **onlygamintwn**

October 21, 2017

You should take a look at https://arxiv.org/abs/1709.03554
since you are taking about string vacua or numbers like 10^{whatever}.

42. **Mateus Araújo**
October 23, 2017

Prof. Woit,

I have to peacefully disagree with your assertion that the measurement problem is somehow a ill-posed natural language problem. It is mathematical.

Consider a quantum system in the state \ket{\psi_0}. Then a measurement happens, taking it to the state \ket{\psi_1}. What is the evolution from \ket{\psi_0} to \ket{\psi_1}? Is it a projection? Is it a unitary transformation? It's easy to prove that these alternatives are mathematically incompatible, so it can't be both (I guess you already know this, but I'd be happy to provide a proof if you're interested).

Of course it might be even something else, but I guess you are even less interested in this possibility than you are in the foundations of quantum mechanics.

43. **Peter Woit**
October 23, 2017

Mateus Araujo,
I never said the measurement problem is an ill-posed natural language problem. My comment was that many natural language discussions of it don't seem to me to touch the actual problem. When I try and read many such discussions I soon get lost, unsure exactly what the author is saying, and thus whether a real problem is getting addressed. There are also plenty of examples where the author writes unambiguously, perhaps using a mathematical formalism to achieve precision. Typically in such cases I can at least understand what is being said, and when the author moves to discussing something I don't think is interesting or relevant, I know when to get off the bus.

My positive take on "shut up and calculate" here is that it would often be useful if someone making a claim to say something new about the measurement problem would discuss an actual measurement instead of making imprecise claims about all "measurements" (including being imprecise about what a "measurement" is). Pick an actual physical system and work out what happens (i.e. "calculate"), showing where the "problem" occurs and explaining what you have to say that's new and enlightening about this problem. When I see this kind of activity, I can follow it, and often learn something interesting, often that the "problem" has some different aspect that wasn't obvious to me before.

44. **Mateus Araújo**
October 23, 2017

Prof. Woit,

Indeed, that is what you had written. Sorry for misunderstanding you.

I think saying something *new* about the measurement problem is close to being a literal impossibility, due to the sheer amount of literature written about it. As a corollary, the solution to the measurement problem has already been written, but finding it in the literature is also nigh impossible, due to this "Library of Babel" situation.

Nevertheless, I disagree with you that making some precise calculation about some specific physical system would be of any help. I've actually met a guy in a conference that claimed that he had solved the measurement problem by doing exactly this. This is his paper. It does make a nice mathematical model of a measurement, up to where the outcome should be produced, where he writes some stuff that I read as "then a miracle occurs".

The problem is that this sort of details hardly matters, the points about which there is disagreement are much more general features of the measurement process. This paper by Maudlin, for example, discusses in a rigorous way what the features are and where there is disagreement. I think that if your solution doesn't address the problems he is bringing up then you are completely missing the point (and yes, it makes me feel dirty to agree with Maudlin on something).

45. **Robinson**
October 23, 2017

A comment section with a Tim Maudlin observable… A pleasure to read.

46. **Doug McDonald**
October 23, 2017

An earlier comment had a pointer to this blog:
http://blog.jessriedel.com/2017/10/12/models-of-decoherence-and-branching/

An even better discussion, really definitive as blogs go, is
http://blog.jessriedel.com/2016/11/12/how-to-think-about-quantum-mechanics-part-0-measurements-are-about-bases/

especially in part 4.

I'm completely shut up by that blog series.

47. **Curious Mayhem**
October 23, 2017

Isn't the whole "shut up and calculate" ethos just an extension of what Lee Smolin was complaining about when he criticized the whole field has having degenerated into virtuoso but mindless calculation mixed with a lot of hand-waving? You know, as in, no one is thinking about anything physically?

We're a long way from Einstein thinking up the invariance of light rays or the Equivalence Principle, or the pioneers of quantum mechanics struggling with classical variables in the face of the quantum of action, or Feynman's insight that yielded the path integral. All of those required formidable mathematics to implement. But the original insights were not mathematical.

48. **Jeff M**
October 23, 2017

Curious

Lee was trained by the same physicist I was, before I went over to the dark side (math). Herb was brilliant, but no computationalist. He was always very physically motivated, and interested in why things were the way they were. He went on to be very involved in quantum teleportation stuff, despite teaching at a small liberal arts college. My perspective now is somewhat different, but the hand waving drives me nuts, mathematician that I am.

49. **Peter Woit**
October 23, 2017

Curious Mayhem,
To a large extent, I disagree with Lee about this, and agree with Arkani-Hamed. I think the problem with string theory is not too much virtuoso math, but that it's a wrong physical idea about unification. The physical idea impressed a lot of people, but when you try and write down a fully consistent theory and calculate anything meaningful you find that you can't . It's the physical idea that is leading you down the wrong path, the actual calculations implementing it that show that this is the wrong path. Of course, once your calculations have shown you you're on the wrong path, one possible reaction is to ignore this and do more calculations, which is what the string vacuum people are doing, showing that calculations are not always the answer.

50. **Mateus Araújo**
October 24, 2017

Robinson,

I think Maudlin is actually a beable.

51. **Jeff M**
October 24, 2017

Peter,

I think you're right that string theory is based on an incorrect physical idea, but I don't think that's what Smolin is talking about. He's pointing out that the physical idea isn't even talked about anymore, and hasn't been for a long time. It's just a bunch of physicists spitting out meaningless calculations, many of which aren't even properly done. Continuing to do that, which is what Arkani-Hamed is suggesting, won't get anyone anywhere. And unfortunately I don't think physicists (well, string theorists) think that the calculations they're doing show they're on the wrong path, precisely the opposite. Look at all the possibilities! It's not that there isn't interesting math, there is, but it's not the physicists who are doing it, and it has no relation to the physical world. I should give credit to the physics folks for pointing out an interesting direction for mathematicians to look in, I think honestly much of that was Witten, who has remarkable mathematical insight, there's a reason he won a Fields.

52. **Peter Woit**
October 24, 2017

Jeff M,
I don't think it's meaningful or helpful at this point to globally say "string theorists are doing X" and criticize that, there's a huge range of things of different value being worked on by "string theorists", most of which have nothing at all to do with the original physical idea of quantizing a 1-d extended "string".

To me one of the most bizarre aspects of the current situation is the inversion of attitudes among prominent string theorists (note that Arkani-Hamed has never really been a "string theorist", he comes from a different background) over the past decade. Back when our books came out in 2006, the criticism from string theorists of Lee and some other people doing LQG was basically that they were woolly-thinkers, going on about Einstein and how you just needed some vague revolutionary "physical" idea, while string theorists had a real theory and could do real calculations. Nowadays, many of the most influential people among "string theorists" (of those who haven't completely abandoned the subject for field theory and condensed matter) are engaged in pursuit of a vague but impressive sounding "physical" idea that somehow a theory of quantum gravity can be found based on general ideas about entanglement and quantum mechanics. This looks to me like exactly the sort of thing Lee's string theory critics had in mind a decade ago when they were criticizing him and the LQG people (those LQG people as far as I can tell are now mostly doing detailed calculations…)

One thing I'd love to know is who exactly Arkani-Hamed had in mind when he decided that he should give this talk. Who does he want to "Shut up"? I suspect Lee was one such person, but what does he think for instance of Susskind, who doesn't appear to be calculating anything anymore? What does he make of "calculations" like "ER=EPR" or "GR=QM"?

53. **Mark Hillery**
October 24, 2017

For those interested in some recent thinking about quantum measurements, which includes calculations, I suggest having a look a What happens in a measurement? by Steven Weinberg, Phys. Rev. A 93, 032124 (2016).

54. **Kris Krogh**
October 24, 2017

Mark,

Thanks for the Weinberg reference. Working along similar lines with the Lindblad master equation, Apoorva Patel and Parveen Kumar have produced what appears to me a spectacular new result, with realistic prospects for experimental tests:

https://arxiv.org/abs/1509.08253
https://arxiv.org/abs/1412.1312

55. **Daniel Tung**
October 24, 2017

In the style of Einstein's comment on science and religion,
Calculations without physical idea is blind; ideas without calculations are lame.

56. **Curious Mayhem**
October 24, 2017

Peter,

I suppose the original physical idea behind strings is at fault here. Once the failure became apparent, around 1990, the solution from the string establishment was more calculations. There were further physical insights (like AdS, conformal symmetry, etc.). But these were tangential to the original unification program that gave rise to the popularity of strings in the first place, in the 1980s.

Perhaps a better way to say it is that a certain style of “math jock” theoretical physics, combined with the previous successes of symmetry-based unification, led to the feeling that just driving further in that direction — without careful study of the map — would soon lead to the “end of physics.” This path was never devoid of physical ideas. But string theory did not incorporate a new and deep breakthrough physical principle that could be turned into a coherent theory. Very different from gauge theory, chiral symmetry, spontaneous symmetry breaking, or the spawn of superconductivity and the Goldstone-Higgs complex. The latter ideas were “pretty,” but mainly, they were simple, general, and powerful. Granted a limited pool of assumptions, you can go very far with these — hallmarks of the best scientific ideas.

Searching my memory of graduate school in the 80s, I’m trying to remember what it was that entranced us about strings. (I never worked in strings directly, just around the periphery, back when theory included phenomenology, an essentially dead subject now.) The first memory I have is “anomalies cancel! anomalies cancel!” Within the following year or so, we learned that strings incorporated higher dimensions (like Kaluza-Klein), groups that extended unification groups we already knew (like E8 and SO(32)), and supersymmetry and supergravity. These were already well-known topics.

The funky string diagrams were fun. But what convinced us was the fact that strings seemed like an extension of a familiar road that we’d been traveling on for a decade+ anyway, just with some weird and unexpected twists. The fact that it did NOT propose a radically new physical idea seemed sensible and attractive.

57. **Jake**
October 24, 2017

re: the deep learning talk from last page, the difference between ‘deep learning hype’ and ‘big data hype’ is that *deep learning actually works*, at least sometimes. Yes, there are major unanswered questions about many aspects (such as interpretability), but all that means is there’s a big gulf between experimental results and theory, a situation that should not be too foreign to people reading this of all blogs.

58. **Robert A. Wilson**
October 25, 2017

Manfred Requardt,

You say that some mathematicians think they understand quantum mechanics because they (think they) understand Schroedinger’s equation. When I was an undergraduate 40 years ago, I knew that I did not understand quantum mechanics, because I knew that I did not understand Schroedinger’s equation. This has not changed: I still do not understand quantum mechanics, and I still do not understand Schroedinger’s equation, although I am perfectly able to shut up and do the calculations if I want to. What *has* changed for me in the last 40 years is that I now know that nobody else *really* understands Schroedinger’s equation either, although many people think they do, or at least say they do.

59. **Robert A. Wilson**
October 25, 2017

Peter,
23 Oct 11.11pm. I agree. Physical ideas lead to math, you do the math, it doesn’t work, you revise the physical idea, and/or you revise the physics-math interface, and you go round again. And again. And again. Fifty years of ignoring the basic misconceptions and calculating forever in the wrong model is a complete waste of time. New physical ideas are two-a-penny, but mostly they are counterfeit, and the genuine ones are very rare and precious. Distinguishing the two is the hard part, and most are thrown away before being properly examined, so it is likely that there are some genuine ones out there that are not recognised as such. Mathematics is also cheap, but again, choosing the right mathematics is very, very difficult and expensive. It seems to me that the current strategy amounts to going to a car boot sale and picking up a bit of mathematics or any old junk in the hope that it will solve the problem. The chances are zilch. I have no solution to the problems, but I know where you have to start: get real mathematicians involved in the search. It is essential to have a genuine two-way dialogue between the physical ideas and the mathematics that expresses those ideas. But choosing the right mathematicians is also very very hard. The evidence of the past thirty years is you’ve consistently picked the wrong ones. Hahaha! “You’re very clever, young man, but it’s turtles all the way down!” Throw the dice, start again, pick some clever people at random, give them tenure *before* they’ve proved themselves. That’s the only way you’ll not only get the really innovative ideas you need, but also get someone committed and able to work on them. You’ll give tenure to 99 people who after 40 years come up with nothing, but you may also have hit on the one person who is worth 100000.
This is venture capitalism on the most extreme scale of risk, and it is necessary to approach it in that spirit. Build a team of 3 or 4, or 6 or 8, extremely clever and open-minded people, spanning particle physics, relativity, cosmology, mathematics, and leave them alone and give them everything they need for their entire careers, to argue among themselves until they resolve their differences. Treat them like a jury, and refuse to let them out until they come to a unanimous decision. Above all, do not interfere in any way in their deliberations, and do not give them a time limit. And maybe repeat the exercise a few times in parallel, up to the limit of the desired investment. Ah well, utopia is a beautiful place, today’s political reality is a very different kettle of fish.

60. **Peter Orland**
October 25, 2017

Robert A. Wilson,

As someone who has done (or tried to do) research in particle theory for a long time, I am convinced that there are no fixes of the sort that you recommend.

If it were simply a matter of building the right sort of team or looking for specifically qualified people, it would have already worked.

There is no simple path to creative achievement. Creative people have to find the way themselves. Good ideas are a matter of luck, talent and knowledge in that order. In my limited experience, planning is an obstacle, not an incentive!

61. **Robert A. Wilson**
October 25, 2017

Peter Orland,
You are right, of course, I was merely venting my frustration with the current system, not really proposing an alternative. “Luck, talent and knowledge in that order”, definitely in that order. You can’t control luck, and you can only vaguely spot talent, so the system is based largely on assessing knowledge. And that is where the systematic biases cause the most damage, because you cannot know in advance what combination of knowledge is going to trigger the key insight that leads to the big breakthrough. You need to allow for input from unexpected directions, and I admit I don’t know how to do that. But you are surely right that less planning rather than more, is the way to go.

Short Items

October 24, 2017
Categories: Uncategorized

A few short items:

- My graduate school roommate Nathan Myhrvold has a new book coming out this month, a five-volume series about the science of bread, based on several years of research into the subject at his laboratory near Seattle. Robert Crease has gone out to visit, and gives a wonderful detailed report on The physics of bread in this month's Physics World.
- An article at FQXI on multiverse research they are funding seemed to finally give me an understanding of what this is all about:

  > These are the two conceptually hardest questions in cosmology, according to Raphael Bousso, a theoretical physicist at the University of California, Berkeley. They go to the core of what it means to exist as a human being making sense of the universe we find ourselves in. And, he adds, unfortunately, there is very little physical knowledge to go on when it comes to working out the answer.
  >
  > Undaunted by the lack of tools to help them, theatrical physicists Eugene Lim of King's College London, UK, and Richard Easther of the University of Auckland, New Zealand, are…

  This all of a sudden made things clear: what is going on is "theatrical physics", not "theoretical physics". Going on like this about the multiverse is performance art.

  Unfortunately I just noticed that this page has been edited (new version here), removing the enlightening characterization of what this is about.

- I'm glad to see that Natalie Wolchover has just won an AIP award for her writing about physics, in particular for a piece on how physicists are dealing with the "nightmare scenario". While she's perhaps the best professional journalist writing about these topics, for coverage of this from a professional physicist, the best you can find is Sabine Hossenfelder's blogging at Backreaction. I'm pleased to hear that the two of them will be appearing at an event here next month in NYC, talking about Making Sense of Mind-Blowing Physics at NYU on Nov. 16.

Update: Sabine Hossenfelder has a book coming out next year, which should be fascinating (although I suspect I'll have something to disagree with…).

Gian Francesco Giudice has a long essay about the status of particle physics, post-negative LHC results. For better and worse, I think it captures well the view of many mainstream theorists.

Comments

1. **Matty**
   October 25, 2017

   I totally agree with you regarding the "theatrics" of that FQXI piece on the multiverse.

   One of the reasons I dislike the inflationary multiverse paradigm is because it just seems like an excuse for failure, as you have noted before – to drag philosophical speculation, however empirically based and explanatory or elegant in nature it might be (and I recognize that the multiverse might not even be elegant or explanatory!), into scientific discussion. It amounts to this sort of reasoning imho,

   "Oh well, our ability to test high energy physics is approaching its limits: we can't seem to calculate the observed value of the cosmological constant and supersymmetry may not be found at the LHC to explain the mass of the Higgs, so yeah – the multiverse must have did it! And even if we can't actually test it because of the particle horizon etc. that's no big deal, we should just accept it as the explanation anyway because it accounts for the data and seems to be the only game in town".

   Umm. Is that how scientific inquiry really worked in the past? From the Aristotelian notion of unmoved movers, the Geocentric model of the universe to Fred Hoyle's Steady State theory, science has ultimately lived or died in terms of testable predictions. Each of these models was ultimately falsified by tested theories (i.e. the discovery of inertia/momentum, heliocentrism, the cosmic microwave background validating the Big Bang).

   Now we appear to be left with "there is very little physical knowledge to go on when it comes to working out the answer" so BINGO, must be multiverse.

2. **Peter Woit**
   October 25, 2017

   Matty,
   Unfortunately I don't think the explanation for the sad story of the multiverse is that physicists have been inspired to become performance artists. Whatever the true reason is, it's a much more depressing situation…

3. **Matty**
   October 25, 2017

   It was an amusing Freudian slip though! But yes, the reality does appear to be more depressing……

4. **Natalie Wolchover**
   October 25, 2017

   Thank you, Peter, much appreciated! See you at the NYU event, perhaps.

5. **neil**
   October 25, 2017

   Also, there is an interesting article in SciAm by Guy Wilkinson on beauty experiments at LHC and elsewhere.

6. **Robinson**
   October 31, 2017

   Giudice's paper was a pleasure to read. Thanks for that.

7. **Jacob Pearce**
   November 15, 2017

   Hi Peter,
   I enjoyed the 'theatrical physics' comment re the multiverse.
   I've got an article in MIT's Perspectives on Science journal entitled:
   "Why These Laws?"—Multiverse Discourse as a Scene of Response.
   http://www.mitpressjournals.org/doi/abs/10.1162/POSC_a_00245
   It's open access. Think you and your readers might find it of interest.
   Cheers

This Week's Hype

October 26, 2017
Categories: This Week's Hype

Yet another entry in the long line of nonsensical hype about fundamental physics driven by misleading university press releases is today's news that CERN Scientists Conclude that the Universe Should Not Exist. Tracking this back through various press stories (see here, here and here), one finds that the original source, as always, is a university press release designed to mislead journalists. In this case it's Riddle of matter remains unsolved from Johannes Gutenberg University Mainz, a press release designed to promote this paper in Nature.

The paper reports a nice experimental result, a measurement of the antiproton magnetic moment showing no measurable difference with the proton magnetic moment. This is a test of CPT invariance, which everyone expects to be a fundamental property of any quantum field theory. The hype in the press release confuses CPT invariance with CP invariance. We know that physics is not CP invariant, with an open problem that of whether the currently known sources of CP non-invariance are large enough to produce in cosmological models the observed excess of baryons over antibaryons. An accurate version of the press release would be: "experiment finds expected CPT invariance, says nothing about the CP problem."

If this experiment had found CPT non-invariance, the implications for early universe baryon-antibaryon asymmetry would have been of minor interest compared to the revolutionary discovery that a fundamental theorem of quantum field theory was violated, shattering our understanding of fundamental physics in terms of quantum field theory.

Comments

1. **murmur**
   October 26, 2017

   Who writes these press releases? I doubt the authors of the paper would so willfully misrepresent their work.

2. **Davide Castelvecchi**
   October 26, 2017

   Another type of statement that one finds very often in press releases – so often, in fact, that some physicists have started to believe it – is that experiments on actual CP violation, like BaBar, "produce more matter than antimatter". This is usually based on the production rate of B mesons versus that of their antiparticles. Trouble is, these mesons are neither matter nor antimatter: they are each made of a quark and an antiquark 🙂

3. **vmarko**
   October 26, 2017

   murmur,

   Apparently the first author of the paper, Christian Smorra, got confused between CPT and CP symmetries. At the end of the press release it says:

   'This consistency is a confirmation of the CPT symmetry, which states that the universe is composed of a fundamental symmetry between particles and antiparticles. "All of our observations find a complete symmetry between matter and antimatter, which is why the universe should not actually exist," explained Christian Smorra, first author of the study. "An asymmetry must exist here somewhere but we simply do not understand where the difference is. What is the source of the symmetry break?".'

   It looks like he was explicitly quoted to have said that. I can understand that we all make mistakes, but these days one should really be extra careful not to say something too stupid in an interview for the media. Why is he even talking about matter-antimatter asymmetry in the context of his paper, when CPT invariance has nothing whatsoever to do with it? I'd say this is the author's fault, in the end.

   Best, 🙂
   Marko

4. **Peter Woit**
   October 26, 2017

   murmur,
   Such press releases are generally not written by physicists themselves but by university communications staff. Usually a press release of this kind would be reviewed by the scientists before going out. Even if that were not the case I can't imagine that a university wouldn't take down a press release about work by one of their faculty if the faculty member complained that it was misleading. This press release has been up for nearly a week, so the physicists involved presumably haven't complained. As vmarko points out, one of them is quoted quite explicitly confusing CPT and CP.

   Davide,
   Never noticed that, thanks for pointing it out (although for that a defence of poetic license might be reasonable…)

5. **Davide Castelvecchi**
   October 26, 2017

   I don't know how they do it in Mainz, but when I used to work for the AIP and wrote press releases I would interview sources the way a journalist would. And I would not edit their quotes without their permission.

6. **NoGo**
   October 26, 2017

   Maybe this is normal for such press releases, I don't know, but parts of this one don't look like written by people who have a clue about physics, more like the worse kind of articles about science in mainstream press… "A total of 16 antiprotons were used and some of them were cooled to approximately absolute zero or minus 273 degrees Celsius."

7. **vmarko**
   October 26, 2017

Peter,

“This press release has been up for nearly a week, so the physicists involved presumably haven’t complained.”

Most of the authors of the Nature paper are postdocs, PhD students and master students. Ulmer is the principal investigator, and (after a cursory look) only Yamazaki, Walz and Blaum are senior scientists, AFAICS. So when the paper’s first author (Smorra, an ex-postdoc of Ulmer) says something misleading in an inverview, there aren’t so many people in their team who will notice it and complain.

However, after this much media hype, I guess one of the seniors did have a talk with Smorra about the difference between CPT and CP symmetries. Or should have had. 😉

Best, 🙂
Marko

8. **Lowell Brown**
October 27, 2017

It is a wonderful paper because they quote two of my papers.

9. **Mark**
October 27, 2017

“All of our observations find a complete symmetry between matter and antimatter, which is why the universe should not actually exist,” explained Christian Smorra, first author of the study. “An asymmetry must exist here somewhere but we simply do not understand where the difference is. What is the source of the symmetry break?”.’

Even if he did mean CP symmetries, this is still misleading. Last time I checked there is a plethora of measurements showing CP violation in the field. The statement implies the opposite (though maybe he meant “our” to be more literal and to mean his experiment – but then the latter part of the quote makes no sense).

10. **Anon**
October 27, 2017

Hi Peter

Thanks for your post as always.

You had written, “We know that physics is not CP invariant, with an open problem that of whether the currently known sources of CP non-invariance are large enough to produce in cosmological models the observed excess of baryons over antibaryons.”

Am curious why you think this is an open problem. Isn’t it the consensus that the CKM CP phase is insufficient — for example it vanishes if any 2 quarks have degenerate masses or if any of the CKM mixing elements are zero… which basically implies that the CKM phase is multiplied by a factor 10^-20 or so in baryon asymmetry calculations.

see for example https://arxiv.org/pdf/hep-ph/9404302.pdf

11. **Attavoir**
October 27, 2017

‘Insufficient Strangeness, Also Reality, Dooms Effort At Bizarre Equivalence’

12. **Peter Woit**
October 27, 2017

Anon,
Yes, that’s true about the CKM CP phase. What I had in mind though were things like whatever is giving neutrino mass terms (no, I don’t want to engage in the same argument over whether this is SM…) or some exotic mechanism in a cosmological model for getting larger baryogenesis. In any case that sentence was just intended to indicate the problem, not provide a complete characterization.

13. **Anon**
October 27, 2017

Hi Peter,

Yeah but the CP phase in the neutrino or leptonic sector has not yet been discovered or experimentally established. It is not a “currently known source of CP non-invariance”

Moreover, if it is experimentally discovered, and if the neutrinos are Dirac neutrinos (with no Majorana mass terms), it will be exactly analogous to the CKM phase and insufficient for Baryogenesis.

14. **Birgit Dannsmann**
October 28, 2017

Peter,

one more twist to the story. A few years ago I tried to find a reliable source for the statement that “CKM/usual CP violation is not sufficient for baryogenesis”.

I found none. I might have missed something. There is the clear possibility that this statement is similar to the famous one “Bumblebees should not be able to fly” which was all over books decades ago, and which nobody had actually ever proved or even said.

Or do you know a definite source?

15. **Marc Nardmann**
October 28, 2017

@Birgit Dannsmann:

There is an article by Huet and Schechtmann from 1994 (arXiv:hep-ph/9404302, DOI 10.1103/PhysRevD.51.379) that claims to explain why the CKM matrix is not sufficient for the observed amount of baryogenesis. I assume you saw this paper when you looked for a reliable source (it is currently Google result #3 for "CKM baryogenesis"), and found it unconvincing. Why, and what do you demand from a reliable source?

16. **Peter Woit**
October 28, 2017

All,
For a review article that has a lot about the question of relevance of SM CP violation to baryon asymmetry, see here:

https://arxiv.org/abs/1204.4186

17. **Birgit Dannsmann**
October 28, 2017

Peter, thank you for the link. The opposite view, surely a minority, is

T. Brauner – CP violation and electroweak baryogenesis in the
Standard Model- EPJ Web of Conferences vol. 70, 00078 (2014).

which seems to claim that there might be a way to explain the asymmetry with the standard model.

18. **Marc Nardmann**
October 28, 2017

[Correction: by "Huet and Schechtmann" above, I meant "Huet and Sather".]

In arXiv:1208.4607 (§5 and Figure 6), the authors argue that baryogenesis (and simultaneously dark matter) can be explained in the so-called "neutrino minimal Standard Model" νMSM, i.e. in the obvious and natural extension of the Standard Model by three right-handed neutrinos. They say that in the νMSM, the baryon asymmetry of the universe is mainly caused by oscillations of these sterile neutrinos. Hence baryogenesis can be explained without any new physics above the electroweak energy scale, but the contribution of the CKM matrix is negligible.

19. **Curious Mayhem**
October 29, 2017

There remains SM EW baryogenesis (conserving B-L, but neither separately), which requires a strongly first-order EW phase transition. No one has been able to make it work convincingly, and the observed Higgs and top quark masses make it hard. The idea has been around since the 1980s.

Extending the MSM with RH neutrinos and getting leptogenesis is also not new. It can be made to work in some models, combined with EW phase transition baryogenesis. First you get lepton asymmetry; the phase transition with the B and L anomalous currents later gets you baryogenesis.

20. **George S Williams**
November 4, 2017

Physics Today published a commentary on this- Antimatter measurement draws unjustified hype. Some of the ways 'journalism' responded to the actual story are truly creative.

Maybe this should be Next Week's Hype- The Subatomic Discovery That Physicists Considered Keeping Secret. The article is both sensational and riddled with errors.

21. **Hal Porter**
November 4, 2017

As a sometime "science/technical" writer (though definitely not focusing on particle physics), I am shocked that so many journalist underlined matter/antimatter symmetrical annhilation as some new issue. My memory may be imperfect, but I seem to remember articles in the consumer literature (Isaac Asimov, Scientific American?) discussing the anomalous existence of matter in high school (1961-65). This undoubted reality, however, was taken as merely underlining the incompleteness of our understanding of physical processes. A lot has happened since then, of course, but the basic insight hardly seems new, and was possible meant by the scientist quoted as an offhand (almost joking?) comment. Did none of the journalist ever think to give him or her a call? Fact checking? The actual experiment strikes me as an inherently remarkable achievement.

22. **Al**
November 6, 2017

Way way way off topic, but Yuri Milner of Breakthrough Prize fame has just made headlines as a Russian oligarch connected to Putin, exposed now due to huge new "Paradise Paper" leaks from Appleby, an offshore investment (read money laundering?) company."Behind Mr. Milner's investments in Facebook and Twitter were hundreds of millions of dollars from the Kremlin.", and "Among Mr. Milner's current investments is a real estate venture founded and partly owned by Jared Kushner…."–
from the NYT on Milner investments:
https://www.nytimes.com/2017/11/05/world/yuri-milner-facebook-twitter-russia.html
and here is Democracy Now on the Paradise Papers:
http://www.truth-out.org/news/item/42507-paradise-papers-millions-of-leaked-docs-reveal-shady-ties-and-tax-evasion-by-trump-s-inner-circle

23. **Al**
November 8, 2017

Along the same lines: Democracy Now (Nov 7 and 8) has a report based on the Guardian's
reports on the Paradise Papers, focussing on Robert Mercer's use of tax havens
to pay for Bannon's alt right-pro Trump propaganda,
https://www.youtube.com/watch?v=3Cpcggv4v-Y
but also on Jim Simons. They mention that Simons apparently just forced out Mercer as head of Renaissance Technologies. Here are links to the articles:
from CNBC:
https://www.cnbc.com/2017/11/07/mercer-reportedly-pushed-out-of-renaissance-by-simons.html
from Bloomberg News:
https://www.bloomberg.com/news/articles/2017-11-07/rentech-s-jim-simons-urged-mercer-to-step-back-to-improve-morale
From The Guardian:

https://www.theguardian.com/commentisfree/2017/nov/08/robert-mercer-offshore-dark-money-hillary-clinton-paradise-papers
and
https://www.theguardian.com/news/2017/nov/07/steve-bannon-bermuda-robert-mercer
and
https://www.theguardian.com/media/2017/nov/02/billionaire-trump-donor-robert-mercer-breitbart

Quantum Theory, Groups and Representations

November 11, 2017
Categories: Quantum Theory: The Book

While I was away last week on vacation, it seems that Springer has published my book on quantum mechanics and representation theory (previously discussed in various blog posts). The Springer page is here, your institution may provide access to the content (and a $24.99 MyCopy softcover) at the Springer Link page for the book. I've retained copyright for the content of the book and a version with essentially the same content as the Springer version is available from my website here. The Springer version has their formatting, copy-editing and metadata. The Amazon webpage for the book (if you're in the mood to write a review there, feel free) is here.

I haven't yet seen a physical copy of the book, don't know how long it will take for them to start printing copies. From people at Springer I learned last year that they no longer print and store copies of such books, they're now always printed on demand (with the quality of the printing dependent on where you order your book from, German printers are quite good I hear..).

Just before leaving on vacation, I gave an introductory talk on some of the themes of the book at LaGuardia Community College (slides here). This week I'll be giving a similar talk at a math department colloquium at Queensborough Community College this Wednesday (1 pm, Science building, S-213).

Comments

1. **David Appell**
   November 11, 2017

   Congratulations, Peter.

2. **Peter Orland**
   November 11, 2017

   Congratulations on finishing this project Peter.

3. **Lino D'Ischia**
   November 11, 2017

   Enjoyed your slide presentation. Very informative. Gives you a great 'bird's-eye-view' of the relation between CM and QM.

   On the slides: viewing it as a "continous" pdf, I found myself starting at the top of each slide, and not being able to focus in immediately on the 'added' portion. Watching your slide presentation may make that all go away; but, if not, then maybe the newest addition to a slide can be 'shaded,' or the older portion 'shaded.' Just a suggestion.

4. **Lino D'Ischia**
   November 11, 2017

   BTW, I've already said it, but, once again: Congratulations on your new publication!

5. **Jess Riedel**
   November 11, 2017

   Congrats Peter! And thanks especially for releasing the free version (and going through the hassle of retaining the copyright and so forth).

6. **Douglas Natelson**
   November 12, 2017

   Congratulations! I'm adding it to my list of future reading material (a list whose source term seems to be eclipsing its sink term these days, unfortunately).

7. **Peter Woit**
   November 12, 2017

   Lino D'Ischia,
   Thanks. I had intended to post the version you don't step through, just replaced the file by the intended one, should be easier for people to read through if they want.

8. **Hossein Ehteshami**
   November 13, 2017

   Nice! My university has the subscription for Springer and I ordered a MyCopy softcover version. In the receipt, I see that it is not available yet and it will be printed approximately on 16th of November. I hope soon Springer prints it and I get my copy of this nice book 🙂

9. **a reader**
   November 21, 2017

   My copy of your book got delivered this morning – here in Denmark. It looks very nice. Congratulations!

10. **Peter Woit**
    November 21, 2017

    a reader,
    Thanks! I also got a bunch of copies late last week, was pleased to see how the actual physical books turned out.

Where the Money Comes From

November 11, 2017
Categories: Uncategorized

Since returning from a vacation partly spent isolated from the internet, I've been catching up and noticed that some of the most prominent sources of funding for math and physics research have been making the news:

- The New York Times and other sources have extensive reports based on leaked records from an offshore law firm that specializes in helping you avoid inconvenient US tax and reporting requirements. The story starts out with the example of Jim Simons, who has become the largest non-governmental funder of math and physics research. His Simons Foundation has been doing an excellent job of providing such funding. They have about \$3 billion in assets, annual income of around \$500 million. The Times reports that Simons (with a net worth of about \$18.5 billion) has an offshore version of the Foundation, the Simons Foundation International, with assets of \$8 billion, dwarfing the onshore version.
- The assets of these Foundations are presumably largely invested in the secretive and extremely successful Renaissance Technologies hedge fund, which also is the employer of quite a few physicists and mathematicians. I've asked many people over the years, but have never found anyone who knows (or will admit to knowing) what it is that RenTech does that is so successful. A peculiar aspect of the coming age of private math/physics research funding is that no one getting this funding really knows where the money comes from.

  In other news while I was away the CEO of RenTech, Robert Mercer, was finally induced to leave. Mercer had drawn a lot of attention recently since he in recent years has been taking the opposite tack to Simons, funding institutions devoted to promoting untruth over truth (e.g. Breitbart News), achieving fantastic success last year. He also has branched out from doing whatever secretive things RenTech does to make mountains of money using computers and data, starting up a firm called Cambridge Analytica, a firm involved in secretively using computers and data to undermine democracy in the US and elsewhere. I had been wondering for quite a while what Simons thought of Mercer's activities. My understanding of highly-paid finance jobs was that your employer pays you a lot of money in return for having your full attention and devotion to not having negative stories about them come to public attention, so Mercer's continued employment was surprising. It seems that Simons finally had enough, after realizing how much damage Mercer was doing to his firm, in particular by creating a situation that would discourage many people from wanting to work there (there also was a campaign underway to get institutions to divest from investments with RenTech).

- Another high profile source of funding for math and physics, in this case for cash prizes to mathematicians and physicists, has been venture capitalist Yuri Milner, with his Breakthrough Prize organization. New prizes will be announced in three weeks at a December 3 prize ceremony (I also believe there will be an associated Breakthrough Prize symposium held at Stanford shortly thereafter). It has always been well-known that much of Milner's wealth derived from investments in Facebook and Twitter. Less well-known and recently revealed was that a major source of the funds for these investments was Russian state organizations closely tied to Vladimir Putin.
- Turning to sources of public funding, there's not very positive news about a possible ILC collider in Japan, with reports of a cutback of the proposal from a 500 GeV to a 250 GeV machine (which would still cost about $7 billion).
- Foreign policy magazine has an article discussing the proposal for a huge new collider in China (discussed here). The point of view of the article is quite critical of the idea of locating a huge new project in a country with an increasingly authoritarian regime:

  > China's next-generation supercollider will unlock secrets of the universe — and destroy the ideals of the scientists running it.

  Luckily, for another more local prominent large country with an increasingly authoritarian and xenophobic regime, the issue of a possible problem with locating an international collider project there isn't likely to come up since its leaders have no interest in funding such projects.

Comments

1. **Peter Orland**
   November 11, 2017

   Evidently, Mercer is going to be replaced by someone else in the inner circle. I wonder what the consequences will be…

2. **CIP**
   November 11, 2017

   Hmmm? Maybe Milner's vast investment in string theory is part of Putin's plot to divide the West;-)

3. **Matt Grayson**
   November 11, 2017

   To the extent that there are Finance Koans, my favorite is "One trader goes long, the other goes short. They both make money." The point, of course, is that any trade has an entry and an exit. People always forget about the exit. The hardest part of trading is knowing when to get out of a trade, and having the discipline to get out when your knowledge or algorithm tells you to.

   I have no special insight into Renaissance, having answered "No" to the first interview question: "Would you move to Stony Brook?", but I think discipline has as much to do with their success as superior analytics.

4. **John Fredsted**
   November 12, 2017

   Behind every great fortune there is a crime.
   —Balzac

5. **Shantanu**
   November 12, 2017

   Peter, sorry for the OT notice. But are you planning to attend this discussion on string theory
   https://pioneerworks.org/programs/scientific-controversies-no-13/

6. **David Brown**
   November 12, 2017

   2 from Twain:
   (1) Honesty is the best policy — when there is money in it.

(2) Virtue never has been as respectable as money.

7. **Peter Woit**
   November 12, 2017

   Shantanu,
   Thanks for pointing that out, I hadn't heard about it. Quite possibly I will go out there for that. Interesting that an event covering a "scientific controversy" is being planned, with only one side of the controversy represented…

8. **Rick Angell**
   November 13, 2017

   I think Mercer is leaving his management role at Renaissance, but continuing work in their models/algo division

9. **Anonyrat**
   November 15, 2017

   Talking about funding, the Republican tax bill will make graduate tuition waivers taxable. The effect on graduate students at Carnegie-Mellon below – I imagine it will be the same for physics and mathematics students. I don't think any number of billionaires throwing in funding can/will help with this. If the tax bill passes, it will be the end of the American research university, in my opinion.

   https://www.wired.com/story/grad-students-are-freaking-out-about-the-gops-tax-plan-they-should-be/

   > "The annual stipend for a PhD student in Carnegie Mellon's school of computer science is about \$32,400. The university covers the student's \$43,000 tuition, in exchange for the research she conducts and the courses she teaches. Under current law, the government taxes only a student's stipend; the waived tuition is not taken into account. But under the GOP bill, her annual taxable income would rise from \$32,400 to \$76,234. Even factoring in new deductions also included in the proposal, the CMU document estimates her taxes would amount to \$10,209 per year—nearly four times the amount under current law. That would slash her net annual stipend by 25 percent, from \$29,566 to \$22,191."

10. **Anonyrat**
    November 15, 2017

    The Council of Graduate Schools has a worksheet (PDF) with a number of scenarios of the impact of the tax proposals on graduate students.
    http://cgsnet.org/ckfinder/userfiles/files/CGS_Tax_Reform_Scenarios(1).pdf
    or
    https://goo.gl/2qReAb

    Their main page on this topic is here:
    http://cgsnet.org/tax-reform-resources

11. **Peter Woit**
    November 15, 2017

    Anonyrat,
    There are massive, \$1.5 trillion scale public policy problems with this tax legislation (and no, I won't host a discussion of these here). I think academics are making a huge mistake to focus on this tuition issue instead of fighting the real problems. They look like they have no interest in anything beyond their very parochial concerns. The fact that universities charge Ph.D. students some often huge level of tuition, and then immediately turn around and give it back to them has always been a somewhat dubious piece of accounting. The supposed tax legislation problem for the students should be solvable if necessary (I doubt it will be) by reducing tuition numbers to zero for Ph.D. students the university doesn't collect tuition from. Yes, I'm sure this creates a secondary effect of indirect budgetary problems for the universities, but then focus should be on those.

    One thing I've been wondering about is what the current budgetary situation for physics/math research actually is. A new fiscal year has started, and as far as I can tell as usual there is no budget. The Trump budget announced earlier this year seems to have been dead on arrival but I haven't heard what the real budget for the current year is looking like.

12. **Anonyrat**
    November 16, 2017

    Assuming the desired outcome is that if a Google or Apple sponsors an employee's PhD, they don't get to pay zero tuition, then whatever tuition is charged in such cases, is the value that the Research or Teaching assistant PhD student is getting as a gift or as compensation, and tax will have to be paid.

    The Senate bill preserves tuition waiver tax deductions, while the House bill does not.

    In the meantime, both House and Senate versions of tax reform legislation would impose a 1.4% tax on investment income at private schools with endowments worth at least \$250,000 per full-time student. The tax would affect up to 70 schools and raise an estimated \$2.5 billion over a decade.

13. **Anonyrat**
    November 16, 2017

    https://www.collegeraptor.com/college-rankings/details/EndowmentPerStudent
    Columbia University with an endowment at \$337,580 per student will be subject to the 1.4% tax.

14. **Peter Woit**
    November 16, 2017

    Anonyrat,
    On the list of awful things about this legislation, the fact that my employer might have to pay a 1.4% tax on investment income is pretty far down the list.

    To the extent there are organizations or grants actually paying Ph.D. tuition, that could be a significant budgetary problem. About Apple or Google not so much. These two companies have obscene amounts of money available, should have no trouble making sure that any employees they want to pay tuition for will be able to feed their families.

15. **Anonyrat**
    November 17, 2017

Just keeping on topic about funding for math, physics, academics.

Various Stuff

November 12, 2017
Categories: Uncategorized

A few links that may be of interest. Mathematics first:

- A seminar "Lectures Grothendieckiennes" on the mathematical ideas of Alexander Grothendieck is taking place this year in Paris, and has just recently started up.
- My ex-Columbia colleague Jeff Achter is one of the authors of an unusual new math paper: Hasse-Witt and Cartier-Manin matrices: A warning and a request. The paper points out that papers of Manin at some points confused an operator and its dual, leading to potential sign errors in later papers that reference Manin's results. I'm quite sympathetic to the problem, having at various points fallen victim to similar confusions while writing my book (I hope they have all been resolved in the final version, wouldn't bet anything really valuable on it…).
- Nature has an excellent obituary of Vladimir Voevodsky, written by Dan Grayson.

On the physics side:

- The LHC has now ended data-taking at 13 TeV for the year (a recent summary is here) and will start up again next spring. The machine ended up delivering about 50 inverse fb each to CMS/ATLAS (bettering the goal of 45), of which about 45 was recorded. Results published so far typically use 36 inverse fb from previous year's data, so next year we should start seeing results based on a total 13 TeV data set of up to 80 inverse fb.
- Still no WIMPs. Frank Wilczek surveys searches for his favorite dark matter alternative here.
- At Big Think, Eric Weinstein has a take on what's gone wrong with theoretical physics over the past 40 years that I'm mostly in agreement with.

Comments

1. **Reimagine Physics**
   November 13, 2017

   Eric Weinstein's analysis is spot on. Does anyone know which Arkani-Hamed talk is he is talking about?

2. **Mozibur Rahman Ullah**
   November 13, 2017

   I liked Weinsteins bon mot – that we've ended up geometrising the quantum rather than quantising the geometric. I get that sense too, and that seems like an over-arching theme in that Grothendiecks work showed how we could geometrise number theory.

   Given that quantum mechanics was kick-started by experimental findings I do think more prominence should be given to just how far out of reach Planck-scale physics is experimentally.

   Is there any hope that improved sensitivity with LIGO might help there? after all one might suspect two black holes smashing into another might generate findings relevant to quantum gravity.

   Another point worth adding is that this desert isn't unprecedented, if one takes the perspective that science began with Greek antiquity then there was great arid period of roughly two millenia before a new constellation of ideas intertwining with the old began modern physics.

3. **srp**
   November 13, 2017

   Surprised nobody has noted the resonance between the second and third bullet points in Peter's post. Voevodsky expressed concern for exactly such problems in motivating his use of proof assistants and development of type theory.

4. **Bobito**
   November 14, 2017

   Note that the conclusion of the Achter paper is basically that the error has not caused widespread problems because most users have corrected it before using it, or not used cases in which it mattered.

5. **Pascal**
   November 14, 2017

   I find it puzzling that Eric Weinstein does not even consider new experimental discoveries as a source of progress. It's no wonder that new generations of theoretical physiscists have a hard time "making contact with physical reality" when all or almost all of the known facts about physical reality are already explained by well established theories!

6. **Peter Woit**
   November 14, 2017

   Pascal,
   Unexpected new experimental discoveries would of course change the situation, the problem is that we haven't been seeing any. In the past it is these that have been most often the source of inspiration driving theoretical progress. Now that the LHC has been running for a while without finding anything new at 13TeV, it seems all too possible there won't be energy frontier discoveries from the LHC (and anything higher energy is a long ways off). One attitude is that theory can't make progress without such input, and if you adopt that, best thing for HEP theorists to do is to change fields (lots are, going into condensed matter or quantum information theory). I think the question Eric is interested in is whether there's an alternative to giving up for now.

7. **Peter Orland**
   November 14, 2017

   "One attitude is that theory can't make progress without such input, and if you adopt that, best thing for HEP theorists to do is to change fields (lots are, going into condensed matter or quantum information theory)."

   Or, alternatively, they can try to solve hard problems in the territory of QFT, where the wise fear to tread…

8. **Douglas J. Keenan**
November 15, 2017

There is an obituary of Vladimir Voevodsky at *Quanta Magazine*, which I found to be very good:
https://www.quantamagazine.org/visionary-mathematician-vladimir-voevodsky-dies-at-51-20171011/

9. **Scott Marks**
November 15, 2017

Back in 2013 when Eric gave the Geometric Unity talks, there were some "paper, or it didn't happen" snarks. Can anyone point us to some working-out of those ideas?

10. **Anonyrat**
November 19, 2017

European physics wish-list:
https://www.scientificamerican.com/article/europe-sets-priorities-for-hunting-cosmic-particles/

11. **Mitchell Porter**
November 20, 2017

@Scott Marks: it didn't happen.

Breakthrough Prize 2018

November 16, 2017
Categories: Uncategorized

The Breakthrough Prizes for 2018 will be awarded at a ceremony on December 3, I believe at the usual NASA Hangar 1 in Mountain View. The next day Stanford will host the 2018 Breakthrough Prize symposium, which one will be able to watch live from the Breakthrough Prize Facebook page.

The symposium schedule is available here, and while it does not list the Prize awardees, it does appear to list the titles of the talks. From this it looks like the math \$3 million will go to a geometer, who will talk about "Geometry at Higher Dimensions". There may be several \$100,000 New Horizons Prizes for younger mathematicians, but at least one will be to an analytic number theorist, who will talk about "Analytic Number Theory in Everyday Life".

For the \$3 million physics prize, it looks like it is going to be split five ways and go to cosmologists/astrophysicists. The talks by laureates are "The Next Decade in Cosmology", "Gravitational Waves and Cosmology", "Search for Extraterrestrial Intelligence", "A New Instrument for Listening to the Universe" and "The Beginning and End of the Universe".

Update: Some details about the prize ceremony here. Perhaps there really is a problem with the public understanding of mathematics:

> This year, a total of seven \$3 million prizes will be awarded – five in life sciences, one in fundamental physics, two in mathematics.

Update: The \$3 million for physics went to the WMAP team. For mathematics, it was Hacon and McKernan. The posted titles for the mathematics prize winners were a red herring, they have been changed to "A Tour of Algebraic Geometry" (McKernan) and "Sphere Packing in High Dimensions" (Viazovska).

Comments

1. **L(s,\chi)**
   November 16, 2017

   Viazovska? (She actually could be either one…)

2. **Bill**
   November 16, 2017

   Did they really just post the talk titles? 🙂
   Horizons in math: James Maynard.
   Breakthrough in math: Simon Brendle (http://annals.math.princeton.edu/articles/11346)

3. **Trent**
   November 16, 2017

   My guess for the breakthrough is Simon Brendle too.

4. **tulpoeid**
   November 17, 2017

   From the panel discussions, "And in mathematics, we have scratched the surface of a perhaps infinite world of structure."
   By the sound of it this refers to some established recent development. What would that be?

5. **Peter Woit**
   November 17, 2017

   tulpoeid,
   I think that's just generic inspirational verbiage, could be used to refer to anything, know of nothing that it seems particularly apt for. On the other hand, the talk titles are not generic but rather specific, and perhaps Bill is interpreting the math ones correctly.

6. **Bill**
   November 17, 2017

   Another question is whether Peter Scholze would have accepted the Breakthrough prize if they haven't insulted him by offering the Horizons prize.

7. **Peter Woit**
   November 17, 2017

   Bill,

   While Scholze has never publicly explained why he turned down the money (in essence choosing just to ignore Milner-Zuckerberg and go about his business) I doubt it was because it was too little money. By turning down 100K, he ensured that they wouldn't try that again, and also wouldn't award him the \$3 million likely to come a few years later. He deserves some sort of award for this…

8. **Bill**
   November 17, 2017

   Peter, perhaps, his decision was driven by the wish to avoid this type of spectacle: https://youtu.be/eNgUQlpc1m0

9. **sdf**
   November 21, 2017

   Maybe it is wishful thinking that Scholze refused because these type of prizes are obnoxious—giving a large cheque to one person when they could be used to endow X number of fellowships, scholarships etc… I daresay this world needs more of James Simons and less of these Yuri Milner types.

10. **Anonymous**
November 21, 2017

“Geometry at Higher Dimensions”

It should be Hacon and McKernan?

11. **boris**
November 22, 2017

Gromov!

12. **Bill**
November 26, 2017

Peter, it looks like they’ve already corrected their press release. However, could this indicate that Hacon and McKernan are really the winners, sharing one prize?

13. **Peter Woit**
November 26, 2017

Bill,
I forgot to include the link to where I got that from, added it now, that source still has 7=8. The schedule refers to 5 different 2018 physics laureates, so that prize is being split. It only refers to 1 math laureate. Maybe there are others who aren’t speaking. I have no inside information, but your initial conjecture looks more convincing to me than Hacon/McKernan.

14. **Bill**
November 26, 2017

The official press release says 7=7: https://breakthroughprize.org/News/39

I understand why broadwayworld.com cares about this, but why not nfl.com?

15. **Bill**
November 28, 2017

http://smf.emath.fr/content/prix-fermat-2017

16. **AGCGDG**
December 3, 2017

2018 BREAKTHROUGH PRIZE IN MATHEMATICS AWARDED TO SCIENTISTS

CHRISTOPHER HACON
JAMES MCKERNAN

17. **AGCGDG**
December 3, 2017

New Horizons in Mathematics Prizes Awarded to Aaron Naber, Maryna Viazovska, Zhiwei Yun, and Wei Zhang.

18. **Bill**
December 4, 2017

I wonder why they don’t award 3 million to both mathematicians? (Although UCSD and Utah press releases say that their corresponding faculty member won 3 million, which does not add up to 22 million.)

Various Physics News

November 20, 2017
Categories: Uncategorized

First, two local events, involving well-known physics bloggers:

- Last Thursday I had the pleasure of attending an event at NYU featuring Sabine Hossenfelder and Natalie Wolchover in conversation. You can watch this for yourself here . If you're not following Hossenfelder on her blog and at Twitter (and planning to read her forthcoming book), as well as reading Wolchover's reporting at Quanta magazine, you should be.
- Next week there will be an event out in Brooklyn advertised as covering the Scientific Controversy over string theory. The idea seems to be to address this controversy by bringing to the public two well-known and very vocal proponents of one side of it.

For a Q and A with another well-known physics blogger, there's Tommaso Dorigo at Physics Today.

For a couple of encouraging indications that the theoretical physics community may finally be taking seriously the need to give up on failed thinking and try something new, there's

- A conference next month in Italy on Weird Theoretical Ideas (Thinking outside the box).
- An interesting talk at a recent IPMU conference by Yuji Tachikawa. I like his conclusion:

> Basically, all the textbooks on quantum field theories out there use an old framework that is simply too narrow, in that it assumes the existence of a Lagrangian.
>
> This is a serious issue, because when you try to come up e.g. with a theory beyond the Standard Model, people habitually start by writing a Lagrangian … but that might be putting too strong an assumption.
>
> We need to do something

In General Relativity related news, there's a new edition out of Misner, Thorne and Wheeler, the book from which many of us learned both geometry and GR. It comes with new prefaces from David Kaiser as well as Misner and Thorne (which an appropriate search on the Amazon preview might show you…). In other Wheeler-related news, Paul Halpern has a new book out, The Quantum Labyrinth, which tells the entangled stories of Feynman and Wheeler.

Finally, also GR related, the Perimeter Institute has announced the formation of a new cosmology-focused "Centre for the Universe", funded by an anonymous 10-year $25 million donation. It will be led by cosmologist Neil Turok, who is soon to step down as director of Perimeter.

Comments

1. **Bee**
   November 21, 2017

   Thanks for the mention. It was good to finally meet you in person 🙂

2. **jd**
   November 21, 2017

   Has David Gross paid off his bet?

3. **tulpoeid**
   November 21, 2017

   I'm sorry if I'm missing it, and a joke explained is not a joke, but is the "encouraging indication" re Tachikawa a joke? Because his argument and conclusion sound like trying to give a little extra breathing room to susy and strings. (Although I'm not sure that string theorists have by now written down a Lagrangian, I stopped following this some time ago.)

4. **Peter Woit**
   November 21, 2017

   tulpoeid,
   Not a joke at all, although my sympathy with the point being made may come from a different origin than Tachikawa's. I do think it's a serious and important point: maybe a lot of our problem is that we're wedded to a too narrow conception of how to produce QFTs (choose a Lagrangian function, turn path integral or canonical quantization crank). Maybe the SM is the best one can do within this framework, to do better you need a wider notion of what a QFT is. One reason I stayed away from the Lagrangian formalism in my recent book was precisely because for the analysis of quantum theories in terms of representation theory, the Hamiltonian formalism is much more appropriate. Perhaps there is a representation-theoretic framework for understanding QFTs where some things become clear which are hard to impossible to see in the usual Lagrangian formalism.

   This is a question about QFTs and how to formulate them, pretty much irrelevant to string theory, which Tachikawa doesn't even mention.

5. **boop**
   November 21, 2017

   I am just a layman and picked the Tachikawa link as the one I was going to read through, and I thought it was sort of a win for string theory and was wondered too what you would think. On slide 29 they say "where genuinely N=3 theories were found, using string theory." I took that to mean this was one of the first theories found to break the lagrangian view and so maybe it was a nice, as you sometimes allow, mathematical use of strings.

6. **Peter Woit**
   November 21, 2017

   boop,

All sorts of interesting QFT phenomena have turned up when people have been looking into questions coming out of string theory. Categorizing new ideas as to whether they're "a win for string theory" or not is just reducing everything to uninteresting ideology and sloganeering (and to his credit, Tachikawa isn't doing this).

The actual example Tachikawa gives here (an N=3 4d SUSY QFT) is a rather complicated one and doesn't look in and of itself very interesting. More interesting is the question of finding new ways to identify and study such QFTs. Sure, you may be able to find them by string theory methods, but people have been trying that for a long time. If you could find other, more insightful, methods that would be more promising.

7. **neil**
   November 21, 2017

   The paragraph describing Sci Con #13 reads like it could have been written in 1994.

8. **vmarko**
   November 22, 2017

   I'm slightly confused by all the noise regarding Lagrangian formalism. I don't see anything really fundamental in using (or not using) a Lagrangian — it's just a piece of mathematical formalism, convenient for some purposes, and less so for other purposes.

   The question whether a Lagrangian for a given theory exists can be answered in a pretty trivial way. Given any set of partial differential equations (that describe your classical field theory), one can always construct a Lagrangian to reproduce those equations by extremizing the action. Just write the Lagrangian as the LHS of your differential equation times a Lagrange multiplier, and you're done. Of course, introducing Lagrange multipliers as auxiliary fields into the theory is the price one pays for having a Lagrangian, but this can always be done if you want to work in a Lagrangian formalism.

   So, if one can rewrite any classical field theory as a Lagrangian theory, what's all the fuss about?

   Best, 🙂
   Marko

9. **Jon Forrest**
   November 22, 2017

   I watched the video of the NYU presentation. It was very enjoyable but it was almost spoiled by Robert Lee Hotz, the white-haired interviewer. He commits the cardinal sin of interviewing, which is being more interested in showing his own cleverness than interacting with the people being interviewed. He interviews as if he's being paid by the word, and there are several times when he rudely interrupts what the interviewees are saying. Sad.

   On the other hand, both interviewees had some very good responses, and showed their way of thinking about scientific writing.

10. **Peter Woit**
    November 22, 2017

    vmarko,
    The issue is quantization. Are there interesting QFTs that are not in any known sense the "quantization" of a classical field field theory? The standard way of thinking about such things is that they're strongly coupled QFTs that don't have parameters that can be taken to some weakly-coupled limit where you do expect a usual relation to a classical field theory.

11. **kashyap vasavada**
    November 22, 2017

    Can you elaborate what you can do in QFT without Lagrangian or Hamiltonian? Do you calculate S matrix directly?

12. **Peter Woit**
    November 22, 2017

    kashyap vasavada,
    That's the problem, we don't have much in the way of methods to produce such non-Lagrangian theories. One way to characterize them would be in terms of an S-matrix. I believe this is one motivation for some of the work on amplitudes. S-matrix theory has a long history of pursuing the idea of trying to go even further, getting rid not just of Lagrangians, but also quantum fields.

13. **Urs Schreiber**
    November 22, 2017

    vmarko, not every differential equation is the EL-equation of a Lagrangian, not even locally. The obstruction is measured by the cohomology of the Euler-Lagrange complex in degree "spacetime dimension +1" (an argument that for linear PDEs was made way back by Helmholtz). Examples of non-Lagrangian QFTs are the chiral WZW model (which is "one chiral half" of a Lagrangian theory) and generally self-dual higher gauge theories. (However, these non-Lagrangian theories are thought to be holographic boundary theories of Lagrangian theories.)

    The beauty of Lagrangian field theory is that it comes with its own covariant phase space. This is really what makes rigorous pQFT tick. We are running a series on this over at PhysicsForums Insights here.

14. **vmarko**
    November 22, 2017

    Hi Peter,

    "The issue is quantization. Are there interesting QFTs that are not in any known sense the "quantization" of a classical field field theory?"

    This seems to be a completely separate issue, having nothing to do with Lagrangian formalism. You can also ask the same question for QFT's which do not have a well-defined Hamiltonian. For example, a QFT which lives on a spacetime manifold which does not have $\Sigma\times \mathbb{R}$ topology, so that you cannot introduce the foliation into space and time, and consequently no Hamiltonian.

    I don't see the existence of such QFT's to be an argument against the Largrangian or Hamiltonian formalisms.

    "The standard way of thinking about such things is that they're strongly coupled QFTs that don't have parameters that can be taken to some weakly-coupled limit where you do expect a usual relation to a classical field theory."

You mean a QFT without a well-defined classical limit, i.e. when $\hbar$ is not allowed to go to zero for some reason? While I agree that this would be an interesting object to study in itself, I don't really see how would such a QFT be relevant to realistic physics?

Best, 🙂
Marko

15. **vmarko**
November 22, 2017

Hi Urs,

Thanks for the links, I'll take a look at the PF in detail.

I don't think I understand your argument. Say I have a differential equation $D(f)=0$, and I define a Lagrangian as $L=\lambda D(f)$, where λ is the Lagrange multiplier. One of the Euler-Lagrange equations of motion will always be the above differential equation, obtained by the variation of the Lagrangian in λ. The other equations (obtained as variations in f) will give other equations involving λ, to complete the set of EL-equations. In the end the set of solutions to the system of EL-equations should be equivalent to the set of solutions of the original differential equation.

Granted, the construction above actually extends the number of fields you have in the theory, and with it the phase space structure etc., but I don't see any choice of $D(f)$ where such a construction would be impossible. What am I missing?

Note, the existence of the action is another matter, I agree that integrating the Lagrangian over some manifold may depend on the nontrivial topology of the manifold etc. so that the action may fail to be well defined in general, or may be always equal to zero or whatever. But for the Lagrangian itself I don't really see what can go wrong?

Best, 🙂
Marko

16. **lun**
November 23, 2017

The complete S matrix of quarks and gluons within QCD will teach you very little about QCD as it is really observed. An S-matrix does not make a theory, an S-matrix and a vacuum state do.
If a string theory solution low energy limit gives "a theory without a lagrangian, but with a hamiltonian and an S-matrix", I would be skeptical that configuration is "physically consistent" and that thing is a real field theory (than again, Tachikawa cannot give a definition of this)

17. **Art**
November 23, 2017

Re the NYU conversation, I thought the moderator did a fine job of accomodating a mostly taciturn Dr H. There would have been precious little conversation without him.

18. **Peter Orland**
November 23, 2017

Marko: The variational functional you propose for the diffusion equation is not bounded from below, hence pathological. Since the Lagrange multiplier and the diffusion field are independent, you can imagine that the time derivative of one is large and positive in the same region where the other field is negative. This pertains to any action of real fields, first order in derivatives.

To all: Bootstrap methods, which assume no Lagrangian have proven their value, but many of their applications (conformal field theory, integrable bootstrap) are to models for which an action or Hamiltonian is known. What isn't understood in most cases (a major exception is the Ising model's spin correlation functions in 2D) is how to relate one formalism from the other. A real problem in stat. mech. is making this connection. In a sense this means trying to reconstruct the Lagrangian/Hamiltonian field theory from the axiomatic field theory.

For example, in some bootstrap theories, we know some or even all of the form factors, hence something about correlation functions. Getting the equations of motion, much less the Lagrangian from this is hard (one of my own research goals is to reconstruct the Lagrangian from exact form factors for the large-N principal chiral model. This doesn't make my life easy).

If we were lucky enough to get a nice bootstrap/axiomatic theory of QCD, there would remain the question of whether it is REALLY QCD. On the other hand, it is not clear we should care, at least as far as phenomenology is concerned.

19. **Andy**
November 24, 2017

"For a couple of encouraging indications that the theoretical physics community may finally be taking seriously the need to give up on failed thinking and try something new"

The titles of the more formal talks at the Italy conference look to me like just more of the same old failed thinking, only not of the mainstream variety.

20. **vmarko**
November 24, 2017

Peter Orland,

"The variational functional you propose for the diffusion equation is not bounded from below, hence pathological."

I never mentioned the diffusion equation, but regardless… The requirement that the Lagrangian be bounded from below is something one may or may not care about, but there is nothing pathological about it. The scalar curvature in the Lagrangian of GR is not bounded from below either, but I wouldn't call the Einstein-Hilbert action "pathological" in any sense. Another example would be a BF theory, which is also not bounded from below. I could probably dig up more examples.

On a more general note, I think we need to separate the issue of the existence of the Lagrangian from issues related to the wishlist of properties we want a corresponding QFT to satisfy. The Lagrangian is a *classical* quantity, and its existence or nonexistence has nothing a priori to do with quantization. And even if one is predominantly interested in quantization itself, I don't see why the non-existence of a classical Lagrangian would be of any benefit to the construction and analysis of QFT's.

The only statement in this thread so far that makes sense to me is from Peter Woit, arguing that a QFT with no well-defined classical limit may have no Lagrangian associated to it. But such QFTs are hardly relevant for physics, I cannot think of a situation where such a QFT would be in any way connected to the real world. So interesting mathematics aside, why study those in the first place?

Best, 🙂
Marko

21. **Peter Orland**
November 24, 2017

"The scalar curvature in the Lagrangian of GR is not bounded from below either, but I wouldn't call the Einstein-Hilbert action "pathological" in any sense. "

Quantum gravity (even with a UV cut-off) IS pathological with signature ++++. Not with signature -+++. There were fights over this very issue by quantum gravity people (which I watched from the sidelines) in the eighties. Hawking was arguing that the right analytic continuation could be used to make sense of ++++, but other quantum gravity types appeared unconvinced.

If an action is unbounded from below, there is generally no ground state in the quantum theory. Exceptions are situations where the phase space volume around minus infinity is small enough (like the hydrogen atom).

22. **vmarko**
November 24, 2017

Peter Orland,

I'd say that the "pathology" of the Lagrangian which is not bounded from below is actually a shortcoming of the possible QFT description, rather than the shortcoming of the Lagrangian itself. In other words, the quantization of such a Lagrangian cannot be done within the framework of QFT — which is a problem of QFT, not of the Lagrangian. There are other quantization frameworks out there…

In particular, the existence of the ground state is one of the "wishlist" things that one may or may not care about when one discusses QFTs, in particular perturbative QFTs. Another example would be the lack of a unique ground state in QFTs in curved spacetime. If anything, these are shortcomings of the perturbative QFT formalism — if you require minimum energy in the theory, or a global Poincare symmetry of the background spacetime, you simply limit yourself to a certain subset of theories where these properties can be satisfied, while the theories where these properties cannot be satisfied (such as GR) become out-of-scope for your QFT description. That's why nobody really expects quantum gravity to be a QFT, nor (as a consequence of QG) does anybody expect that QFT should be a fundamental description of nature. Today people are slowly getting disenchanted by QFTs and start talking more and more about *effective* QFTs, with the understanding that a QFT is just an approximate description of reality, while at a fundamental level there should be some non-QFT type of theory.

But all this has nothing to do with the existence of the classical Lagrangian, given some classical differential equations of motion. It is a completely separate issue, and should be kept separate, IMO.

Best, 🙂
Marko

23. **Peter Orland**
November 24, 2017

Well, I don't understand why unstable Lagrangians are useful, even classically. The problem goes beyond quantum mechanics. Maybe there are some weird examples where you never worry the instability, but you are messing with foundational stuff here.

If you allow actions to be unbounded from below, you are inviting all sorts of trouble, unless you have a parameter to squelch the instability (like a small value of 1/N, some Sobolev inequality removing the instability, etc.).

Invoking effective QFT's seems like a red herring. No matter what the more fundamental theory is, it had better be consistent. Theories without ground states aren't. Even serious axiomatisists (Streater and Wightman, for example) demand a ground state. S matrix theorists (if there are any still out there) do too; there is a vacuum associated with no particles.

24. **Urs Schreiber**
November 24, 2017

vmarko,

the proposal you make (if I follow what you mean) does not just add more fields and field equations, but it leads to a different phase space structure and hence to inequivalent field theories. Consider this for the simple case of the free real scalar field. Your prescription does not just yield a second field (your would-be "Lagrange multiplier") which has EOMs of a second scalar field, but the canonical momentum of the original field now becomes the derivative of the "Lagrange multiplier" field and vice versa. Hence the resulting field theory is not the original field theory plus extra stuff.

25. **vmarko**
November 25, 2017

Hi Urs,

Ok, in general, if your original equation cannot be derived from a Lagrangian on its own, I don't know how can you construct a Hamiltonian, nor the phase space. In that case, the phase space structure coming from my proposed Lagrangian is as good as any, since it has nothing to compare to.

On the other hand, in the case of the real scalar field that you mention, there do exist "traditional" Lagrangian, Hamiltonian and phase space, and can be compared to the Lagrangian I proposed. In that case, the EL-EoMs give rise to the original EoM for the scalar field plus another EoM for the multiplier, and the corresponding phase space structure indeed appears different than the original one. However, I suspect that there is a canonical transformation that will restore the usual canonical variables describing two noninteracting scalar fields. Therefore, the "traditional" classical theory is a subset of the new theory, since I merely added one more scalar field which does not interact with the original one. After that, quantization gives you the standard quantum theory for the old scalar field plus the new auxiliary scalar field. So the procedure merely extends the theory, without changing any physics of the old theory.

But the main advantage of the approach I propose comes about when the original differential equation does *not* have a well-defined Lagrangian (and consequently the Hamiltonian and the phase space, constructed from the Lagrangian in the standard way). In that case, my point is that one can still manage to define a Lagrangian, at the expense of introducing an auxiliary degree of freedom. In my eyes, having a Lagrangian is an advantage of that approach.

Anyway, these days it is common practice for physicists to introduce new auxiliary fields whenever they please, it's not a controversial step to make, IMO.

Best, 🙂
Marko

26. **Mozibur R Ullah**
November 27, 2017

I too thought the interviewer did a great job in getting a conversation going, I didn't notice that he was particularly hogging the conversation or being overtly 'clever', and as he points out right at the beginning Dr H is jet-lagged, hence taciturnity.

I'm a little disappointed that the conference calling for 'new thinking' doesn't mention causal nets which I find interesting, because (apparently) it admits Presentism as well as general covariance, which on the face of it, seem to be at odds.

27. **Urs Schreiber**
November 29, 2017

Dear vmarko,

I do see your point. But I still want to caution that a field theory is more than its equations of motion, and that in this respect the sectors of field theories that you propose are peculiar, to say the least.

The point about phase space structure that I made above in the example of the free scalar field applies more generally: In your proposal to consider Lagrangians of the form L = lambda P[phi] for P any given differential operator on a field phi, the canonical momenta of phi end up involving the would-be "Lagrange parameter" lambda, and conversely. Specifically in the case that P[phi] is linear, then the canonical momenta for phi are proportional to (derivatives) of lambda and independent of phi.

This in turn implies that the propagators between the phi-s are trivial, because these come from the Poisson bracket pairing.

So if you consider, for linear PDE P, the Lagrangian L = lambda P[phi] and then restrict attention to observables involving only phi, not lambda, then the theory looks entirely like the classical field theory defined by P[phi] = 0.

While I agree that one can consider this situation, its peculiarity makes it be not a counter argument to the claim that it is a special property of a field theory to be Lagrangian, i think.

Notice the similarity between your proposal and the very issue of the non-Lagrangian chiral WZW model ("self-dual boson" in the abelian case, the 2d toy version of that (2,0) superconformal 6d self-dual higher gauge theory that is discussed elsewhere):

Here one may start out with the Lagrangian field theory in 2d that turns out to split into two chiral halfs and then observe that each chiral part is a viable quantum field theory in itself (current algebra). Now even though there is a Lagrangian around which gives the two chiral halfs together, possibly suggesting to regard the other half as an "auxiliary field", the point is still that either one by itself is not Lagrangian; and this is what makes self-dual higher gauge theories and their holographic/boundary relation to Chern-Simons theories interesting.

Theorists Without a Theory

November 24, 2017
Categories: Favorite Old Posts, Multiverse Mania

I had been intending to write something here on the blog about this essay by George Ellis, so when I was contacted by someone at Inference about writing a letter in response, I did so for publication there. It has now appeared in their latest issue, with the title Theorists Without a Theory.

The topic is one I've addressed here all too often, but the main point I was trying to make is perhaps a new one. When I was writing here about the controversy over inflation one thing that struck me was that the pro-inflation side was responding to arguments that their theory didn't solve the problem it was supposed to by in effect saying "the real theory is much more complicated" (see the paragraph beginning "Besides our disagreement…" on page 3 here). One way of seeing part of what is going on here is that most of what gets advertised as "theories" of inflation are actually more appropriately described as toy models. They involve a single inflaton field with a simple potential and unknown couplings to matter, intended as a toy model for the real theory (which will have lots of fields, complicated potentials and specified couplings to matter). An aspect of the controversy is one side pointing out that this theory doesn't solve problems it is supposed to solve, with the other side arguing that it's just a toy model.

People sometimes note that there's a terminological problem with "string theory", in that the public is often told that "theories" are solidly tested parts of science, which is not true in this case. The actual usage among physicists is different though, with "theory" often used to mean a specific mathematical model or set of models, with no implicit claim of a successful experimental test. A lot of the problem with the usage "string theory" is that no one knows what the actual theory is: it's a conjecture that a theory with certain specific limits exists. The main point I was trying to make in this piece is that to a large degree the arguments over the scientific status of string theory (and of its supposed landscape and multiverse) revolving around its lack of testability are moot, since the underlying problem is something different: that there is no real theory to argue about. String theorists often try and evade this problem by a terminological shift: string theory is not a "theory", it's a "framework". "Framework" is a much more ill-defined term than the already ill-defined "theory". A theorist who says "I have a framework, not a theory" is actually saying nothing more than that they are a theorist without a theory.

Comments

1. **Reader297**
   November 24, 2017

   Genuine question, out of pure curiosity: From this perspective, would you consider "the old quantum theory" to have been a theory? (https://en.wikipedia.org/wiki/Old_quantum_theory) Or a collection of "toy models"? Or a "framework"? Clearly it played an important historical role in the eventual development of what became the standard Dirac-von Neumann formulation of quantum theory.

   Obviously the analogy with inflation is not perfect. No analogy ever is. But there are some interesting parallels. (And, for the record, I'm asking solely about the analogy with inflation, which is connected with experiments and observations. I'm not attempting to create an analogy with string theory.)

2. **Peter Woit**
   November 24, 2017

   Reader 287,
   I honestly don't know what a "framework" is, it's a term that as far as I can tell only gets used in theoretical physics as part of bogus arguments about string theory vs. QFT.
   The old quantum theory was a theory, albeit an incomplete and inaccurate one, although one that was clearly capturing part of the story because it gave very non-trivial predictions that were close to observed results in some cases. I don't see any relation of that story to string theory, and little if any to usual inflation models.

3. **Reader297**
   November 24, 2017

   Granted, it was way easier to do arbitrarily many replicable measurements of small systems than of the whole universe! But we do have a lot of cosmological data. Am I wrong in thinking that inflation can give an explanation for many of these data points, including the scale-invariance of fluctuations in the CMB, and all within basically standard GR plus some scalar fields and small quantum corrections?

   I'm also rather curious about whether you have an opinion about where inflation stands. Perhaps you've maintained a wise level of ambiguity! But do you feel inflation is on par with string theory in terms of being not even wrong, disconnected from experiment, and a wrong direction that people should turn away from?

4. **Alan**
   November 25, 2017

   So what's the way forward? We know Prof. Ellis doesn't think naturalism is the final framework (I've read some of his eloquent writings on this) and some multiverse proponents think naturalism *is* the right path. Therefore … ?

5. **Peter Woit**
   November 25, 2017

   Reader297,
   I don't think that problems with the theory of inflation are anywhere near as severe as the situation with the complete failure of string theory as a unified theory. Looking at the arguments discussed in the posting linked to above, the situation is complicated and I'm well aware I'm not a cosmologist.

   Sometimes inflationary theory proponents claim evidence for a theory that will produce a multiverse and different physics in each universe. I think it's important to point out that when they do this, they don't have an actual theory that does what they claim, certainly not a testable one. If you look into such claims, they rely upon failed ideas from string theory.

6. **Peter Woit**
   November 25, 2017

   Alan,
   I don't think any of this is relevant to debates over "naturalism" and theology. If you're looking for answers to such questions in theories of inflaton fields, I think you're making a category mistake.

   As for people who claim that they're furthering the cause of naturalism by promoting an untestable scientific theory based on a failed research program, justified by appeals to authority, I think they're making a really huge mistake. If you make this an argument about which untestable idea one should believe, most people are going to quite sensibly chose the church of their ancestors over Lenny Susskind.

7. **Alan**
   November 27, 2017

   Thanks for reply Peter. I was hoping some might take up the baton a bit. I'm really hoping one day for something really meaningful through and through (and I have a physics degree) and see little in the multiverse (though never understood it to be a reaction against theological issues) that impacts on my (irreducibly meaningful) dot of an existence. I smiled at your last point. Best.

8. **NoviceAsAlways**
   November 27, 2017

   The 'String Hypothesis' then! Would that be a fair assessment?

9. **Curious Mayhem**
   December 1, 2017

   Inflation is a real theory, implemented as a series of toy models, but generalizable to more complex models. It's exactly like gauge theory in the 1960s: a great general idea, implementable in an infinity of working models, waiting for specific implementation consistent with observation.

   Inflation is compatible with different "patches" of spacetime having different symmetry breakings. What that has to do with the string "multiverse" has never been clear to me.

   Anyway, inflation, unlike strings/M-theory, provides an economical explanation for a series of otherwise "accidental" observations and makes a general prediction for the form of the primordial fluctuation spectrum that so far is in good agreement with observation. It's a pretty successful idea. It's doubtful we'll ever have a fully detailed theory of it, however, because it applies to regime we'll probably never have access to, like evolutionary biologists trying to figure out how life originated. But who knows?

A Physicist's Physicist Ponders the Nature of Reality

November 28, 2017
Categories: Uncategorized

Quanta magazine has an interesting new piece up, an interview of Witten by Natalie Wolchover.

One topic covered in the interview is the question discussed in a recent posting, that of whether a different formulation of QFT exists, one not based on a choice of Lagrangian. Here Witten is non-committal, leaning to the idea such a thing might exist only in special cases:

> Now, Nati Seiberg would possibly tell you that he has faith that there's a better formulation of quantum field theory that we don't know about that would make everything clearer. I'm not sure how much you should expect that to exist. That would be a dream, but it might be too much to hope for; I really don't know…
>
> I find it hard to believe there's a new formulation that's universal. I think it's too much to hope for. I could point to theories where the standard approach really seems inadequate, so at least for those classes of quantum field theories, you could hope for a new formulation. But I really can't imagine what it would be.

The standard example of where such a formulation might be needed is the 6d superconformal (2,0) theory, about which Witten says:

> From the (2,0) theory's existence and main properties, you can deduce an incredible amount about what happens in lower dimensions. An awful lot of important dualities in four and fewer dimensions follow from this six-dimensional theory and its properties. However, whereas what we know about quantum field theory is normally from quantizing a classical field theory, there's no reasonable classical starting point of the (2,0) theory.

About the current state of M-theory, there's this exchange:

You proposed M-theory 22 years ago. What are its prospects today?

> Personally, I thought it was extremely clear it existed 22 years ago, but the level of confidence has got to be much higher today because AdS/CFT has given us precise definitions, at least in AdS space-time geometries. I think our understanding of what it is, though, is still very hazy. AdS/CFT and whatever's come from it is the main new perspective compared to 22 years ago, but I think it's perfectly possible that AdS/CFT is only one side of a multifaceted story. There might be other equally important facets.

What's an example of something else we might need?

> Maybe a bulk description of the quantum properties of space-time itself, rather than a holographic boundary description. There hasn't been much progress in a long time in getting a better bulk description. And I think that might be because the answer is of a different kind than anything we're used to. That would be my guess.

Are you willing to speculate about how it would be different?

> I really doubt I can say anything useful. I guess I suspect that there's an extra layer of abstractness compared to what we're used to. I tend to think that there isn't a precise quantum description of space-time — except in the types of situations where we know that there is, such as in AdS space. I tend to think, otherwise, things are a little bit murkier than an exact quantum description. But I can't say anything useful.

The hope of 22 years ago was that it was non-perturbative string theory which would provide the desired "description of the quantum properties of space-time itself". Over the years though studies of gauge-gravity duality have moved away from the use of string theory to provide this bulk description. Witten's take on the current situation: "There hasn't been much progress in a long time in getting a better bulk description. And I think that might be because the answer is of a different kind than anything we're used to." seems reasonable.

It's interesting to hear that Witten was going back to Wheeler to see if he had any inspiration to offer the current "It from Qubit" program. This requires a patience for the "vague but inspirational" that Witten has more of these days than he used to:

Why do you have more patience for such things now?

> I think when I was younger I always thought the next thing I did might be the best thing in my life. But at this point in life I'm less persuaded of that. If I waste a little time reading somebody's essay, it doesn't seem that bad.

This patience is not infinite though: among Witten's many admirable qualities are the way he responds to:

Do you have any ideas about the meaning of existence?

> No.

Comments

1. **Davide Castelvecchi**
   November 28, 2017

   What does it mean for a theory to not have a Lagrangian? That its solutions do not satisfy a variational principle?

2. **Peter Woit**
   November 28, 2017

   Davide,
   From one point of view a QFT, like any quantum theory, is some algebra of operators acting on some state space, perhaps with some interesting group acting by automorphisms on the algebra. The usual way to produce such a thing is to start with classical fields, a Lagrangian and a group acting leaving the Lagrangian invariant. Then, produce matrix elements of the operators by either
   1. Computing a path integral using the Lagrangian to get the action and thus a "measure"
   2. Using a variational principle and the Lagrangian to produce an equation of motion, taking as phase space the space of solutions, quantizing by "canonical quantization", or maybe geometric quantization.

   It seems that for the 6d (2,0) superconformal theory there is an appropriate operator algebra, but no known Lagrangian that would give this algebra using 1. or 2.

Unlike Witten, I'm more willing to speculate, and very fond of the philosophy that interesting quantum systems are sometimes understandable as representations of some group or algebra, accessible via the methods of representation theory. I'd guess that there are various cases where such methods can produce representations that are not of a sort that the standard methods 1. or 2. can produce (or, that they can't produce in a conventional way: physicists may be able to expand their usual ways of doing 1 or 2 to accommodate more possibilities, and end up with something they still might call a "Lagrangian".)

3. **John**
November 28, 2017

Can anyone please explain this response to me:
"Personally, I thought it was extremely clear it existed 22 years ago, but the level of confidence has got to be much higher today because AdS/CFT has given us precise definitions, at least in AdS space-time geometries. I think our understanding of what it is, though, is still very hazy. "

It seems he is claiming M-Theory has an even higher level of confidence today than 22 years ago but then says his understanding of what M-Theory even is has become very hazy. How can you have even more confidence in something that you understand even less?

4. **Peter Woit**
November 28, 2017

John,
"M-theory" is basically a conjecture that there's an unknown theory that behaves in a certain way in certain limits. I think what he's saying is that we understand the behavior in some of these limits much better, in a way that agrees with the conjecture. Away from the limits, at a generic point, nothing people have tried has really worked, thus the "still very hazy".

One thing that I've never really understood is the often-expressed claim that M-theory is "unique", even though one doesn't know what it is.

5. **Anonyrat**
November 28, 2017

Gregory Moore's Felix Klein lectures for 2012 (PDF file: http://www.physics.rutgers.edu/~gmoore/FelixKleinLectureNotes.pdf) has the following, has the situation changed in the last five years?

Quote:

In what follows we try to explain the reasons many string theorists believe in the existence of the remarkable (2, 0) superconformal field theories. In §??? we attempt to write down with some precision the ground rules physicists use when speculating about these theories.

Mathematicians will find this discussion extremely frustrating. A mathematician could well ask: "Is this mathematics?" The answer is "No." It is not even physical mathematics. The relevant question is "Can it be turned into mathematics?"

End quote

6. **Peter Woit**
November 28, 2017

Anonyrat,
I don't think much has changed concerning what is known about the underlying theory since those lectures. There may very well be important developments I'm unaware of though. The Moore lectures give an excellent overview about what is known about this still not understood theory. See sections 6.6 and 6.7 for this.

7. **Nakanishi**
November 28, 2017

Peter,
In discussing QFT without Lagrangian, why do you neglect the axiomatic QFT, such as the Wightman theory, the algebraic QFT of Haag et al., etc. ? As a typical example, I take the 2-dimensional massless scalar field thoery: both scalar field and its dual field satisfy the 2-dim. d'Alembert equation. While if the existence of the Lagrangian is assumed, both fields cannot have independent degree of freedom so that the duality is necessarily broken, if one admits to formulate the theory in the Wightman framework, it is possible to realize the complete duality. But I, personally, prefer the Lagrangian formalism.

8. **Peter Woit**
November 28, 2017

Nakanishi,
I'm not trying to discuss what the right general framework for QFT should be (because I don't know…). I'm just noting the fact that Tachikawa, Seiberg, and Witten, very much mainstream theorists, have each been publicly discussing the idea that the standard textbook way theorists are trained to produce QFTs, by starting with a Lagrangian, doesn't produce all the QFTs they are interested in. This kind of public acknowledgement that maybe one should be rethinking the foundations of QFT, looking at different fundamental formulations than the Lagrangian one, seems to me a positive development.

9. **Dan**
November 28, 2017

Davide #1, check out Yuji Tachikawa's awesome talk slides http://indico.ipmu.jp/indico/event/134/contribution/17/material/slides/0.pdf (this was linked to a few posts back on this blog)

10. **Nakanishi**
November 28, 2017

Peter
I am just pointing out that the axiomatic approach provides us a broader framework for QFT, which is independent of Lagrangian. It is irrelevant to whether or not the three persons you quoted are "mainstream" theorists. Probably, they forget to mention the axiomatic QFT.

11. **piscator**
November 29, 2017

Interesting title. There are many ways I would describe Witten, but 'physicist's physicist' would never be one of them.

12. **Peter Woit**
November 29, 2017

piscator,
I think what was meant was “theoretical physicist’s theoretical physicist”….

13. **Low Math, Meekly Interacting**
November 29, 2017

Reportedly Sheldon Glashow already bestowed the “physicist’s physicist”title on Sidney Coleman.

The way I see Witten often described, he sounds more like some near-miraculous alien intelligence inhabiting a human body.

14. **Anonyrat**
November 29, 2017

The aforementioned slides of the talk by Yuji Tachikawa is about the unsatisfactory state of QFT, and says that axiomatic QFT is too broad and generic to carry out the calculation of the anomalous magnetic moment of the electron.

15. **ronab**
November 29, 2017

Peter,
Are you saying that there exist QFT’s that can be proven to have no Lagrangian formulation, or simply that there are some QFT’s whose Lagranian is not currently known?

(By ‘proven’ I mean at a physics level of rigor, Coleman-Mandula rigor, say.)

16. **Peter Woit**
November 29, 2017

ronab,
The conjectured 6d superconformal theory discussed here comes with a physics argument why it shouldn’t have a Lagrangian. Lacking a real definition of that theory though one has no way of knowing if, once defined, there won’t be something you could interpret as its Lagrangian.

17. **vmarko**
November 30, 2017

In principle, if you know how to write down a partition function (with sources) for your QFT, you can take the logarithm of that to get the energy functional, and then do a Legendre transformation to obtain an effective action functional. Then, assuming that $\hbar$ exists as a deformation parameter in your QFT, it will sit somewhere in the effective action — then you can study the limit $\hbar\to 0$ for the effective action, and the result can be provisionally called “classical action”. Then you define a Lagrangian as the kernel of that action.

So in cases where your QFT is defined in the language where the above procedure can be performed, eventually you end up with *a* Lagrangian. The question whether this Lagrangian is *the* Lagrangian of your QFT (in the sense that the initial partition function can be reconstructed from that Lagrangian using some quantization scheme) is of course nontrivial, and the answer usually needs to be studied on a case-by-case basis.

That’s how I’d like to understand this whole story.

HTH, 🙂
Marko

18. **GoletaBeach**
November 30, 2017

Witten is of course an exceptional researcher, but really a Physicists’s Mathematician.

Fermi… who hand built capacitors and small rolling tables for the Chicago Cyclotron, and who estimated the Trinity yield from casting torn up paper in the air just before the blast wave hit, and used the displacement in the fall to the ground.. of who Feynman said, “he was like me, only ten times better at it”… now he was a Physicist’s Physicist.

Of course, these names and qualifiers don’t really matter. But Fermi’s collected works, and his family of mentorees, dwarf in breadth and depth any other physicist I can think of, including Einstein. Good these days to remember… he mentored the only woman other than Marie Curie to win a Nobel Prize in Physics.

19. **sdf**
November 30, 2017

Resorting to saying there may be no Lagrangian is akin to saying instead of tinkering with the recipe let’s blame the pot, in my view.

Is the only justification for the “no lagrangian” business the 6d superconformal theory? Because that seems to me to be far from sufficient.

20. **Peter Woit**
November 30, 2017

sdf,
The 6d theory is one of the reasons Witten and others have started publicly asking the question of whether our usual Lagrangian-based understanding of QFT is too limited, but I think there are other reasons to be asking this question. For instance, one thing to notice about a lot of studies of 2d conformal field theory is that the way you get detailed information about these theories is by studying representations of certain infinite dimensional groups (Virasoro and loop groups). The theory may also have a Lagrangian, but this isn’t anything particularly useful for studying the theory. It may be that there’s always some way to assign a “Lagrangian” to a theory (as vmarko does above for instance), but that this in some cases won’t uniquely characterize the theory or tell you anything useful about it.

21. **Low Math, Meekly Interacting**
December 1, 2017

Probably you've already seen this, but if not: Since the interaction of cutting-edge mathematics and physics is of particular interest, the latest Quanta article about Minhyong Kim seems worth a look.

My limited knowledge of the subject would lead me to believe that something like the specific "action" principle he is searching for might relate somehow to this discussion of Lagrangians.

22. **Jeroen**
December 4, 2017

Dear Peter,

I would like to understand a bit more about 'the philosophy that interesting quantum systems are sometimes understandable as representations of some group or algebra, accessible via the methods of representation theory'.

I've read part of your book and wanted to ask whether you have ever done something with 'dualities' between different quantum field theories in the context of representation theory. For 1+1 dimensional bosonization, this seems to be well understood in some cases. The free and massless fermions and bosons are for example related to the unique irreducible representation of the Virasoro and Kac-Moody algebra's that specify these theories. I was wondering whether it is known how one can find the exact relation between the fermionic and bosonic degrees of freedom just from representation theoretic arguments, and whether it is known how this works generalizes to the massive interacting cases.

In the case of 3+1 dimensional (supersymmetric) Yang-Mills theories, the partition function can be written as a theta function. S-duality (when we look at the partition function as a path integral) then seems to be related to a 'functional Fourier-transformation' being performed on this partition function. I don't know enough about the way modular forms transform under Fourier transformations and how this generalizes in this case… I tried to read part of the notes by Edward Frenkel on the Langlands programme (though it's a bit too mathematical for me) hoping this would explain a bit more about how the representations of the original Lagrangian are transformed under this 'functional Fourier transform' into representations of the Langlands dual group that describes the S-dual theory.

I am very interested whether you ever wrote something about dualities in the context of representation theory, or if you could give me a few hints or articles in the right direction?

Many thanks.

23. **Peter Woit**
December 4, 2017

Jeroen,

What's interesting to mathematicians about these QFT dualities is that many of them don't correspond to anything well-understood in mathematics (in particular in representation theory). For T dualities there is a clear understanding of their origin (both in math and physics), which is the usual story of Fourier series and the Abelian Fourier transform.

For S dualities, you can often interpret them as part of a modular group action and thus the story of modular forms, but this is exactly where the representation theory issues get very deep and poorly understood. It's important to realize that there is a lot of mystery at the heart of the Langlands program and thus these issues. It's not that mathematicians have a simple idea about why such things should be true, and are just having trouble making things rigorous.

The geometric Langlands program and its Langlands duality is about very non-trivial structure of spaces of bundles and connections over Riemann surfaces, it was formulated partly by looking for the geometric analog of the number theory Langlands program. Witten related this to S-duality of 4d QFTs, but this just relates two different mysteries. The only conceptual explanation of the S-duality I know of is based on starting with the existence of a superconformal 6d theory, compactifying on a torus, and getting the modular group action using the torus. This explanation is yet a different kind of mystery, the existence of this 6d QFT.

So, sorry this isn't much of an answer, just an indication of how pursuing the connections between math and physics based on S-duality leads to relations between deep mysteries, but no truly satisfactory explanation of these mysteries.

24. **Peter Woit**
December 4, 2017

Jeroen,
Also, forgot to make this clear: I haven't written anything about this. In the book I wrote I decided to not try and say anything about conformal field theory. That would require a whole other volume, and it's a subject where there's already an extensive literature from the representation theory point of view, in which you likely could find discussion of cases of dualities that are well understood.

25. **Urs Schreiber**
December 5, 2017

The quick way to see that there is an issue with the (2,0)-superconformal field theory in 6d to be Lagrangian is to observe that it involves a self-dual 2-form higher gauge field B, meaning that the 3-form field strength H = dB is proportional to its own Hodge dual star H, which means that the would-be kinetic Lagrangian H /\ star H vanishes identically.

This is one example in a hierarchy of self-dual higher gauge theories which in 2d starts with the chiral boson and in 10d continues with the self-dual RR-fields. All these share the same issue with being Lagrangian, and all of them are either known or conjectured to be defined instead as the holographic boundary theory in one dimension higher which *is* Lagrangian. For the chiral boson this is abelian 3d Chern-Simons theory, for the 6d theory it is AdS7/CFT6 duality, and specifically for the self-dual higher gauge sector inside the 6d theory it is 7d Chern-Simons theory, and for the 10d self-dual RR fields there is a proposal in terms of an 11-dimension Chern-Simons theory whose fields are differential K-cocycles.

But generally, that not every field theory is Lagrangian is an old hat, this was fully understood for free field theories by Helmholtz way back in 1887. In his honor the corresponding obstruction for the general case is known today as the cohomology of the Helmholtz operator, which is one of the differentials in the Euler-Lagrange complex that controls all things variational. A good introductory discussion to these matters is in Anderson's book *The variational bicomplex*, see p. viii of the introduction.

Secret Link Uncovered Between Pure Math and Physics

December 1, 2017
Categories: Uncategorized

There's a very intriguing new article out today by Kevin Hartnett at Quanta magazine, entitled Secret Link Uncovered Between Pure Math and Physics (also a video here). It's about ideas relating number theory and physics from arithmetic geometer Minhyong Kim. He's evidently on tour talking about them, with two talks on Gauge theory in arithmetic and a colloquium talk on "Gauge theory in geometry and number theory" in Heidelberg, and a talk on Gauge theory in arithmetic geometry in Paris.

In recent years Kim has been working on what he calls "arithmetic Chern-Simons" theory. For details about this, there are papers here, here, here and here, a workshop here, talks here and here. These ideas grew out of a beautiful and well-known analogy between topology and number theory that goes under the name "Arithmetic Topology". For more about this, see the book Knots and Primes by Morishita, or the course notes by Chao Li and Charmaine Sia.

While these ideas look quite interesting and I have some idea what they're about, the Quanta story seems to indicate that Kim has something new, an idea about "Diophantine gauge theory" going beyond the arithmetic Chern-Simons business, and with potential applications to deep problems in arithmetic geometry. Unfortunately the mathematical background here is beyond me (you can try to look at Jordan Ellenberg here, and this earlier paper of Kim's), and as far as I can tell, the only source for details on the conjectured relations to gauge theory is Kim's recent talks, which aren't documented anywhere I can see.

I'm sure we'll be hearing more about this as time goes on. It joins a host of other ideas relating gauge theory and number theory (in the context for instance of the Langlands program), and promises deeper links to come between fundamental ideas about physics and about mathematics.

Update: Some personal background on this story from John Baez.

Update: More about this story here, including a discussion of the use of Kim's methods to deal with the "Cursed Curve".

Update: Reddit has a report of the Heidelberg talk here, unfortunately giving just enough information about the talk to make it sound very interesting, too little to figure out what the ideas discussed actually were. For yet more frustration on this front, Kim gave a general talk on the subject last year here with video supposedly available, but no browser I've tried can access it. I do hope we'll soon see slides, notes, a paper, something, anything, so we can figure out what the Quanta article actually was about.

Update: Kim now has a paper out explaining these new ideas: Arithmetic Gauge Theory: A Brief Introduction.

Comments

1. **Bill**
   December 1, 2017

   "What I started out trying to find" was a least-action principle for the mathematical setting, he [Minhyong Kim] wrote in an email. "I still don't quite have it. But I am pretty confident it's there."

   Then shouldn't the title be "Secret Link Conjectured Between Pure Math and Physics" or even "Secret Analogy Conjectured Between Pure Math and Physics"?

   Or probably the title refers to the fact that Minhyong Kim is finally revealing/uncovering a link between pure math and physics he had in his head for a while. Then the title is a clickbait.

2. **CIP**
   December 1, 2017

   I don't know anything about the math, but I love the headline.

3. **Low Math, Meekly Interacting**
   December 1, 2017

   I thought the article was extremely interesting and clear. For whatever reason, this one grabbed me like few others of its kind. The notion that somehow there's something analogous to finding rational solutions to Diophantine equations in the way nature "finds" the classical path of a light beam is kind of mind-blowing.

4. **Yatima**
   December 2, 2017

   "Principle of Least Action now considered for use in Number Theory" perchance?

5. **Arun Debray**
   December 2, 2017

   Thanks for blogging about this!

   One question I've wanted to know about arithmetic Chern-Simons theory is: in topology, Chern-Simons theory manifests in many different ways (e.g. path integrals, 3-manifold invariants, functors out of a cobordism category), so arithmetic Chern-Simons must carry over some subset of those things into number theory. Which parts have been brought over, and how much of the rest of the story is expected to apply to the number-theoretic side?

   I know very little number theory, but looking at these papers, it looks like thus far, Kim and his collaborators have constructed the analogue of the classical Dijkgraaf-Witten action. Has there been discussion of quantizing this action? What could the analogue of functorial TQFT for this action look like?

6. **Peter Woit**
   December 2, 2017

   Arun Debray,
   Good questions. From looking at the papers a bit, there's no quantization or TQFT here that I can see, this is quite a different story than the Witten-CS QFT one. What appears is just the analog of the classical action of Chern-Simons (Abelian case), with the novelty coming from the arithmetic context,

7. **Arun Debray**

December 3, 2017

Ah, ok. Thanks!

8. **Mathematician passing by**
December 3, 2017

To me, this has much the same look as the "hype" you complain about.
I would be much more sceptical about mathematical PR: here and elsewhere, you seem surprisingly willing to accept it at face value.

9. **boop**
December 3, 2017

Baez made [a nice google plus post expanding on this]()

10. **Peter Woit**
December 3, 2017

Mathematician passing by,

I'd agree that the title of the piece is over the top, but the Quanta article on the whole I think gives a non-hyped take on a complicated story. The last section of the article both presents Kim's hopes and skepticism from others, explaining well the current situation. It is difficult for others to evaluate this right now, since Kim has not (at least in print that I know of) explained either how gauge theory ideas motivated his earlier solid mathematical results, or what the conjectured variational problem he has in mind is.

I don't think this is at all like the typical physics hype that I complain about, which generally deals with bogus claims being made about failed ideas that have been around for over 30 years, and studied in detail by thousands of scientists. Whether Kim really has something or not, we'll see, but he has a significant amount of credibility (unlike other common cases of physicists or mathematicians making non-credible personal claims for a new idea, which I tend to ignore and not bring up here).

11. **Skeptikal mathematician**
December 4, 2017

As a mathematician not working in number theory or geometry, my impression is that this is exactly the kind of hype we are used to in geometry and number theory, which has elevated these subjects to an over-inflated status, much like string theory, and has resulted in other branches of mathematics being starved of funds, and of bright minds, and going out of fashion. It is not a theorem, not even a conjecture, just a vague idea. It might lead somewhere spectacular, of course, but mathematicians are only impressed by proofs, not by speculation.

12. **Someone who used to work in number theory**
December 5, 2017

I wholeheartedly concur with the contributions from the two mathematicians. I think Minhyong Kim is an awesome mathematician, but even before his coming-out as a physics-minded number theorist, his work tended to gear towards grand programmes and sweeping conjectures. There is nothing inherently wrong with that, but we can't all be Grothendieck, dreaming up a bunch of mysterious connections and slowly working towards a grand materialization 10, 20 years down the road. And even in Grothendieck's case it didn't all quite work out as planned. The whole field of number theory, and arithmetic geometry more generally, is absolutely littered with unproven conjectures and half-fulfilled dreams, and in my humble view no-one should be encouraged to add to this castle in the sky.

As for me, I am practicing what I preach, and now working in a field where I see concrete results everyday, and where moreover I don't have to engage in mental gymnastics to justify the fact that people are giving me money for my labours.

13. **Trent**
December 8, 2017

I can access the cgp video on safari and also on my iPhone. (There are tons of interesting videos on that site too btw, so it is very much worth figuring out how to access it.)

14. **Peter Woit**
December 8, 2017

Trent,
Thanks. With Safari I at least got a frozen screen of video with the title slide, the Kim talk looks like it was about the arithmetic Chern-Simons stuff, not the more recent ideas. I can't stand the idea of trying to watch a math talk on my phone, I'm too old for that.

15. **Peter Woit**
December 8, 2017

Tried Safari again on the Kim talk, and managed to get a few screens showing later slides, which looked quite interesting. But staring at the frozen screen got old after a while. Got so desperate I tried the phone, but that doesn't work at all. This is really maddening.

16. **Bill**
December 8, 2017

On Safari worked fine for me: https://goo.gl/JtyBxu

17. **John Baez**
December 20, 2017

> Peter wrote:
>
>> From looking at the papers a bit, there's no quantization or TQFT here that I can see, this is quite a different story than the Witten-CS QFT one. What appears is just the analog of the classical action of Chern-Simons (Abelian case), with the novelty coming from the arithmetic context.
>
> People have looked at the quantization too; a good reference is [Analogies between knots and primes, 3-manifolds and number rings]() by Masanori Morishita.
>
> The basic idea is that the Galois group of Q is like the fundamental group of Spec(Z), since it acts as "deck transformations" of the "universal cover" of Spec(Z),

which is the spectrum of the algebraic integers in the algebraic completion of Q. Going further with this analogy, representations of the Galois group of Q are like flat vector bundles over Spec(Z), since flat vector bundles over a space correspond to representations of its fundamental group. And since Spec(Z) is 3-dimensional from the viewpoint of etale cohomology, while Spec(Z/p) is 1-dimensional for any prime p, and we have a map Spec(Z/p) -> Spec(Z), we should think of Spec(Z) as being a bit like a 3-manifold, and each prime as giving a knot in this 3-manifold.

(All this generalizes to arbitrary algebraic number fields, too.)

Since Chern-Simons theory is a topological quantum field theory involving flat vector bundles on a 3-manifold, and it gives invariants of knots and links in 3-manifolds, it's irresistibly tempting to create an "arithmetic Chern-Simons theory" that gives invariants for primes or collections of primes.

The first success of this viewpoint is that the "Gauss sum" formula for the Legendre symbol of a pair of primes can be seen as analogous to the path integral for the vacum expectation of the product of Wilson loops for a pair of knots in U(1) Chern-Simons theory, which is a Gaussian integral. Yes, the Gauss sum looks like a discretized Gaussian integral! And this vacuum expectation is basically just the linking number!

Thus, quadratic reciprocity, which says how the Legendre symbol changes when you switch the two primes, becomes the usual symmetry of the linking number under switching the two knots.

18. **David Roberts**
December 21, 2017

MK has a new paper out, on this: "Arithmetic Gauge Theory: A Brief Introduction", arXiv:1712.07602

19. **Urs Schreiber**
December 21, 2017

There are more such analogies:

1) the differential geometric analog of Artin L-functions is clearly the Ruelle/Selberg zeta functions;

2) the Ruelle/Selberg zeta functions are known (I, II) to serve to express both Reidemeister torsion as well as the eta invariant;

3) these are of course the factors in the perturbative Chern-Simons quantum invariant (here).

Under this identification, all the zeta-. eta-. theta-, and L-functions in physics and number theory fit neatly into a dictionary.

20. **Balazs**
December 22, 2017

> For yet more frustration on this front, Kim gave a general talk on the subject last year here with video supposedly available, but no browser I've tried can access it.
.
This link does work if you open it from a media player like vlc but keeps buffering a lot:
http://cgpvod.postech.ac.kr:1935/vod/voddata/IBS_CGP_2016_NT&QFT/1-1.Minhyong_Kim(20160822).MP4/playlist.m3u8

Scientific Controversy No. 13

December 2, 2017
Categories: Uncategorized

I made the mistake yesterday evening of spending it out in Red Hook, at an event billed as addressing the scientific controversy over string theory. The venue was an arts space called Pioneer Works, the brain-child of artist Dustin Yellin (whose formative early experience with physics is described here). The event was sold out (tickets were free, courtesy of the Simons Foundation), and drew a huge crowd of several hundred, mostly twenty-something Brooklyn hipsters.

The guests brought in to discuss the controversy were David Gross and Clifford Johnson, and the moderator was Janna Levin. Levin began the discussion by asking the two of them where they stood on string theory: pro, con or agnostic? This flustered Gross a bit (he's one of the world's most well-known and vigorous proponents of string theory) and Levin somehow took this as meaning that he was agnostic. Finally Gross clarified things by saying something like "I've been married to string theory for 50 years, not going to leave her now".

Things then moved on to the usual well-worn hype about GUTs, string theory and unification. The LHC made a quick appearance, with no mention of falsified string theory "predictions" of supersymmetry. Instead Johnson characterized the discovery of the Higgs as somehow a vindication for this unification program. Gross went on to explain that unfortunately testing string theory requires going to the Planck scale where strings would be obvious, but that this was out of the question with any conceivable technology.

Besides being immune to experimental test, Gross also described string/M-theory as not a theory at all, since we don't know its equations or principles (according to him, it's a "framework", see here). The conversation then degenerated into a long and meandering discussion of the black hole information paradox (to her credit, Levin countered Gross's claim that string theory successfully explained it by reminding him of Polchinski and the firewall business).

The Q and A session consisted of a series of mostly crackpot questions from the audience. Johnson responded to a woman saying she thought that we were oscillating between two universes by telling her that she could see she was wrong by testing her theory. The sudden appearance of testability as a criterion to shoot down vague ideas surely confused her.

On a positive note, neither Johnson nor Gross were interested in promoting the multiverse, and the audience was spared that.

Johnson has a new book out called The Dialogues, written in graphic novel form. My previous experience with him was a rather unpleasant one more than ten years ago, after the publication of my book. He wrote a long sequence of blog posts about what he called the "Storm in a Teacup", attacking Smolin and me and our books. Attempts to discuss the issues involved with him in the comment section there were confusing at first, until things finally became clear when he explained that he was refusing to read my book or Smolin's. Dialogue about science was not something he seemed interested in if it involved uncomfortable criticism of string theory.

His book addresses this controversy with a panel in which the physicist figure explains:

> Frankly, that's mostly driven by the press, and a few attention-seeking individuals. Most people have a more nuanced view… It just does not sell newspapers or books.

On the question of "attention-seeking", one might want to consult Johnson's forty-plus long series of blog postings about his participation and appearance in TV and movie programs. As far as books go, in an end-note for this panel Brian Greene's *The Elegant Universe* is recommended. After the Q and A, a long line formed for people to hand their credit cards over to an assistant, then get a copy of Johnson's book and have it signed.

Comments

1. **Richard Gaylord**
   December 2, 2017

   your view of Johnson seems clear (it's not favorable). can you give us review of Johnson's book?

2. **Peter Woit**
   December 2, 2017

   Richard Gaylord,

   I did at least read it (granted, that only took about 15 minutes), but, no, I don't have any interest in writing a review. For one thing, I've no interest in the graphic novel format. I stopped reading comics when I was about 12, other than some R. Crumb never picked one up after that. If you do like graphic novels, from the blog entries at his blog you can probably figure out if you'll like this one.

3. **Bee**
   December 3, 2017

   Why didn't you ask a question? Or if you had, what would your question have been?

4. **tulpoeid**
   December 3, 2017

   "I made the mistake" — apparently you didn't, being there and reporting is useful, at least for those of us whose nerves couldn't have stand so.
   "It just does not sell" — does this include research grants and prestigious multimillion dollar awards?
   "The sudden appearance of testability as a criterion to shoot down vague ideas surely confused her" — this must be one of my all-time favourites on here.

5. **Mika**
   December 3, 2017

   Have you read The Elegant Universe and if so, would you recommend it (or other books by Greene) to a layperson?

6. **Hetzen**
   December 3, 2017

   After barely managing to finish The Elegant Universe. I felt if I was being told the same sentence over and over without being any the wiser on what it was. It was me walking away dissatisfied with that book that made me find Peter Woit's and subsequently finding this blog.

7. **Peter Woit**
   December 3, 2017

   Bee,
   I considered it, but decided not to try for several reasons, including a distaste for people using question sessions to put forward their own agenda, the fact that, whatever Johnson thinks, I'm rather the opposite of attention-seeking, and listening to this had left me disgusted and depressed. Thinking back on it, I still don't see how a serious counter-argument could have credibly been made by joining the line of people intent on airing their crackpot ideas.

   On the other hand, alerting the Simons Foundation about how their money is being used might be a good idea…

8. **Peter Woit**
   December 3, 2017

   Mika/Hetzen,
   The mention of The Elegant Universe was just there since it made clear that Johnson was not criticizing pro-string theory books, I don't want to start a discussion of it here. But yes, of course I've read it, and one motivation for my own book was the feeling that the public then, as now, deserves to hear both sides of a debate

String Theory Fails Another Test

December 6, 2017
Categories: Uncategorized

Back in 2004, the KITP put out a press release (which I wrote about in an early blog post here) announcing that "Newly Devised Test May Confirm Strings as Fundamental Constituent of Matter, Energy". The press release announced that Polchinski and collaborators had found "the most viable test to date for determining whether string theory is on the right track", that this test would be performed by LIGO, which "could provide support for string theory within two years."

This got a lot of attention and was often quoted as evidence that string theory was testable science. In a 2007 article in Physics World, David Gross answers string theory critics with:

> String theory is full of qualitative predictions, such as the production of black holes at the LHC or cosmic strings in the sky, and this level of prediction is perfectly acceptable in almost every other field of science," he says. "It's only in particle physics that a theory can be thrown out if the 10th decimal place of a prediction doesn't agree with experiment."

LIGO never found any evidence of cosmic strings within two years after 2004, and now the vastly more sensitive Advanced LIGO experiment has just released results of a search. As expected, the results are negative.

Any guess on the probability of a KITP press release announcing that string theory has failed an experimental test? Or of an acknowledgement by Gross that all the "qualitative predictions" of string theory he was using to justify it ten years ago have now all failed, so, by the standard of "every other field of science", it should be abandoned?

Comments

1. **Lino D'Ischia**
   December 6, 2017

   The last sentence of the abstract of the linked paper says: "Finally, we show that the data sets exclude large parts of the parameter space of the three loop distribution models we consider."

   How severely constrained are they? That is, is this just a nice way of saying cosmic strings don't exist?

2. **Peter Woit**
   December 6, 2017

   Lino D'Ischia,
   It's always the same story, talking about string theory "tests" or "predictions" is just a way of misusing words to mislead people. If that wasn't clear back in 2004, it should be clear now, after the "tests" are over and the "predictions" have been falsified. This hasn't changed the opinions of those making these "predictions" (Polchinski says string theory "over 99.7%" likely to be true, Gross last Friday said he's still wedded to the idea).

   String theory as a theory of unification predicts (in the usual scientific sense of the word) nothing about anything. You can't rule out the cosmic strings of superstring theory, because the theory is so unconstrained that you can evade any conceivable negative experimental result, by just saying, "there really are cosmic strings, it's just that their properties make them unobservable by the current version of LIGO"

3. **Verissimo**
   December 6, 2017

   Every post like this takes 30 minutes off the life of some random string theorist somewhere around the globe.

4. **Yatima**
   December 6, 2017

   I don't understand this at all.

   From the abstract:

   Cosmic strings are topological defects which can be formed in GUT-scale phase transitions in the early universe. They are also predicted to form in the context of string theory.

   Cosmic strings (which I first read about in the 80s in SciAm IIRC) have scant to do with String Theory. And even if they had been detected, this would STILL say nothing about String Theory as the implication works the wrong way.

   And maybe it would say nothing about GUTs either.

   Is elementary logic still on the program nowadays?

5. **David Appell**
   December 6, 2017

   I have the same question as Yatima — are cosmic strings the same as string theory strings?

6. **Anon**
   December 6, 2017

   A concise description of cosmic strings generated during inflation independent of string theory:

   http://www.theory.caltech.edu/~preskill/pubs/preskill-1996-vilenkin.pdf

7. **Peter Woit**
   December 6, 2017

Yatima/David,
The history is that such things were first considered (late 70s) in the context of GUTs, where you can have string-like gauge/higgs-field configurations that may be stable for topological reasons, depending on the ingredients of your GUT (this doesn't happen for the ingredients of the Standard Model).

In 2003, in arXiv:hep-th/0312067 the authors came up with some string theory models with metastable fundamental strings (using complicated configurations, along the lines of the "string vacua" that supposedly stabilize moduli). Remember, in this game of Rube Goldberg models, you can get pretty much anything you want, why not large metastable fundamental superstrings?

String theory proponents then went to work claiming that, if you found cosmic strings, and you could study them in enough detail to see whether they were GUT strings or these fundamental ones, then you would have "evidence" for string theory. The KITP press release was part of this publicity campaign, timed to coincide with publication of the 2003 paper. From then on, this regularly appeared as a "test" or "prediction" of string theory. The "could you even tell if your cosmic string was a GUT one or a superstring one?" question rarely got mentioned.

I don't see any point in wasting any time going over the details of this particular hype campaign and the supposed science behind it (probably it appears in a dozen or so of the posts on this blog). The fact that falsification of these "predictions" hasn't changed one iota the faith in string theory of the people promoting them is good reason not to take this supposed science seriously. If the people behind these ideas don't, why should you?

8. **Lino D'Ischia**
   December 7, 2017

   "The fact that falsification of these "predictions" hasn't changed one iota the faith in string theory of the people promoting them is good reason not to take this supposed science seriously."

   It stops looking like science, doesn't it?

9. **Shantanu**
   December 8, 2017

   Peter: maybe you (or someone else) should forward this or ask this question to KITP PR folks and whether string theory has been falsified?

10. **Kea**
    December 17, 2017

    The only thing that will bring down string theory is a full theory of quantum gravity that shows precisely how string theory fits into the theory as a physically-not-very-relevant piece of structure, say like bubble graphs for scattering amplitudes.

The Last Refuge of Cowards

December 6, 2017
Categories: Multiverse Mania

The talks and panel discussions from the 2018 Breakthrough Prize symposium are available via Facebook video. They ended with the following, from prize winner David Spergel:

> Well, alright, I'm going to say something that I probably shouldn't say in Palo Alto. I don't think the multiverse is a testable and interesting scientific hypothesis. I think it doesn't explain anything.
>
> The way the multiverse tends to be used is together with the anthropic principle. The idea is that the universe is the way it is because that's the way we get to live in it. I find the multiverse solutions to these problems, it's a lot like if you ask me "why am I wearing a black shirt today". My answer would be: "you wouldn't have asked the question if I wasn't wearing a black shirt". That's not a satisfactory answer.
>
> The way we have advanced in science is by falsifiability. By developing hypotheses, testing them (that's why we do experiments) and ruling things out.
>
> Ideas that are not testable, it's interesting metaphysics, perhaps interesting for philosophers. What has driven four hundred years of scientific progress is the fact that ideas can be wrong. And, the multiverse, I think is kind of the last refuge of cowards… That's why it's great to have tenure.

Comments

1. **Petero**
   December 6, 2017

   This sentence of the quote looks a bit mangled:
   By developing hypotheses, testing them, that's what why we do experiments, and ruling things out

2. **Peter Woit**
   December 6, 2017

   Petero,

   Thanks, fixed.

3. **paddy**
   December 6, 2017

   David Spergel: Bravo! Especially for avoiding hair-splitting and angels-on-the-head-of-a-pin stuff over falsifiability. We all know it when we see it.

4. **Pierre**
   December 6, 2017

   It's not even philosophy or metaphysics; that's an ignorant slur.

   Philosophers have usually engaged in metaphysics in order to ground moral arguments. That's a hell of a lot more honest, interesting, and better motivated than the tautological sleight-of-hand upon which the multiverse rests.

5. **Paul**
   December 6, 2017

   Am I correct in thinking that the Multiverse hypothesis is partly based on geometries of the String Theory?
   Would the proponents of Multiverse base their pro-position on the belief that String Theory is a proven theory?

6. **Peter Woit**
   December 6, 2017

   Paul,
   Yes.

   http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=why-do-you-describe-the-multiverse-as-pseudo-science

7. **Cookie**
   December 6, 2017

   Peter,

   Can one in principle find the correct string theory vacuum if you perform Planck scale experiments? Aren't there any generic string theory predictions you can in principle test if you can perform those ultra high energy experiments, like extra dimensions or string-like or Brane-like behavior? In principle at least?

8. **Peter Woit**
   December 7, 2017

   Cookie,
   No.

   All, enough about string theory. Spergel sensibly thought it not worth even mentioning.

9. **Cookie**
   December 7, 2017

Why not? If there's a reference you can refer me to, that would be much appreciated! I've always thought that you could in principle test things with Planck scale experiments.

10. **Casey Leedom**
December 7, 2017

Man, I am so sorry that I didn't go to see the award ceremony now. I've got to get on Stanford's Physics Seminar email list …

11. **Casey Leedom**
December 7, 2017

By the way, for those of you who want to listen to the comment that Peter quoted, it's in the "panel discussions" link at 1:25:56 …

12. **Peter Woit**
December 7, 2017

Cookie,
http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=doesnt-string-theory-make-predictions-at-very-high-energy

13. **Peter Woit**
December 7, 2017

Casey Leedom,
You can't get into the actual awards ceremony unless you are a Hollywood starlet or one of our Silicon Valley overlords. The symposium I gather was open to the public.

14. **Alan**
December 7, 2017

Spergel does say about learning from "string theory colleagues" after 1h 15 minutes in relation to duality (linking the small and the large). Yet string theory implies a multiverse? All those vacua? His mention of the anthropic principle and his shirt is just the weak anthropic principle so I didn't think that was very deep.
I also think it was David Bohm who said it took 2000 years of theoretical and experimental content before Democritus' idea of atoms got confirmed. So more time needed for the multiverse?

15. **Peter Woit**
December 7, 2017

Alan,
I think Spergel is well-informed about the question of testability of multiverse models, not just some naive guy who isn't happy with anthropics.

16. **BCnrd**
December 7, 2017

Alan:

The fact that some things took 2000 years doesn't carry as much weight as it may seem to, since it isn't as if during most of that time there was a vibrant scientific community working on the task (though there was some scientific work happening during some periods outside Europe). It's sort of like when Gauss solved a 2000-year-old problem at age 19 by figuring out which regular polygons can be constructed by a straightedge and compass: definitely a landmark accomplishment, but in the intervening 2000 years between Gauss and the ancient Greeks there wasn't exactly a vibrant community of mathematicians working on that problem, what with the Dark Ages and the absence of an adequate algebraic language and so on.

And with the size of the community of scientists that has developed during the 20th century up to today, as well as the funding mechanisms (such as they are…) one can reasonably expect rates of progress to be way faster than anything during earlier centuries and epochs (though coming up with good new ideas remains as challenging as always).

17. **Philip Gibbs**
December 7, 2017

Why does he think that the universe owes him an answer that would satisfy him? Some people don't like the uncertainty of quantum mechanics because it does not satisfy them, but the universe stubbornly refuses to change its rules.

As for falsfiability, when someone has a complete theory of quantum gravity that is falsifiable and not falsified using current technology, then they can level that criticism. Meanwhile those who think multiverses of various sorts could be part of how the universe might work, probably won't stop thinking about it just because other people tell them that they don't like it for philosophical reasons.

18. **Peter Woit**
December 7, 2017

Philip Gibbs,
Where do you see him saying that? Spergel is one of the best cosmologists in the world, expert on the issue of what you can test experimentally about cosmology and how to do it. He's making the uncontroversial point that if it can't be tested experimentally it is not conventional science. His description of theorists who promote their own ideas that cannot be tested by confrontation with experiment as "cowards" (because they can never lose) is a pointed one, but it's a characterization that many if not most of his colleagues would agree with privately.

19. **tulpoeid**
December 7, 2017

It's not philosophical reasons, it's epistemological.
By the way, comparing Democritus who had to try and derive nature's law through human senses (which means arriving at a right answer merely by chance) with privileged physicists of the 21st century … should I say more?
Spergel made my day and I'll drink a glass to his health. Although it's depressing to realize that stating the obvious makes headlines nowadays.

20. **Philip Gibbs**
December 7, 2017

Last time I checked epistemology was considered a sub-branch of philosophy.

21. **The Observer**
December 7, 2017

BCnrd,

"The fact that some things took 2000 years doesn't carry as much weight as it may seem to, since it isn't as if during most of that time there was a vibrant scientific community working on the task (though there was some scientific work happening during some periods outside Europe)."

There is also an "unlikeliness" that is ignored in choosing something like Gauss's construction, after the fact, in that most of the ideas that were worked on 2000 years ago were wrong. They were so wrong that we have never heard of them. Some of them may have been so bad that they were "not even wrong". Most ideas are bad ideas. Most music is bad music. Most fiction is bad fiction. So the Gauss example has an "unlikeliness" problem, a good metaphor for the way unlikeliness plagues these wild, escapist, ideas of "physics" today.

Also, who was working on the construction of a 19 sided polynomial 2000 years ago anyway? That was not a problem that took 2000 years to solve. It was an idea Gauss had of his own and solved, presumably, very quickly. Same goes for Democritus (and Lucretius). No one spent 2,000 years trying to prove it. It was not a project of research, just speculation. Modern atomic theory didn't arise by guys who read Lucretius and decided to try and prove it. The idea rose *de novo* with the scientific investigations of the modern era. After that, it was commented that Democritus and Lucretius had come up with the idea in ancient days but they in no way founded or inspired the modern research program. There was no connection at all.

But I like Steinhardt's description of the anthropic principle.

"Yuck!" (see his Fermilab presentation of 15 months ago)

22. **tulpoeid**
December 8, 2017

Philip Gibbs,
You used uncertainty as an example. Let's say that I don't like the notion very much, but it is well proven by now. Then, I don't like it philosophically but nothing can be done about that.
On the other hand, demanding continued funding and respect on the basis that we can't dismiss what can't be either proven or falsified, speaks about a way of defining how science should work (epistemology). It's not a matter of taste and it has some very real repercussions, and this is what the post is about.

23. **Alan**
December 8, 2017

The Observer, Steinhardt may say "Yuck" in relation to the Anthropic Principle but really it's misnamed anyway and to do with the inevitability of life of *some kind* in this universe. No matter we get wiped out in a cataclysm, something else will prevail somewhere in the universe into it's far BY-old future. The question is, is this significant? Science fact essentially meets a valid philosophical question.

Peter, I don't really get that Spergel talks of learning from "string theory colleagues" and yet doesn't like the multiverse – which I thought was kind of inevitable from ST. I mean, if the multiverse is kind of forced theoretically (is it?) then is it the string theorists fault?

BCnrd, But I thought the point was well made. I'd guess many good thinkers over those 2000 years were pondering Democritus' idea on atoms and were wondering how to test it.

24. **Peter Woit**
December 8, 2017

Alan,

"String theory" now refers to a huge variety of different sorts of activities, and I think Spergel was referring to ideas about dualities, which have nothing to do with the multiverse.

About "string theory implies the multiverse" claims: string theory unification is a failed idea that predicts nothing. It is compatible with pretty much anything, including a multiverse or no multiverse.

25. **Casey Leedom**
December 8, 2017

Peter,

Thanks for letting me know about the difficulty getting into the awards ceremony. Too bad, but personally, I think I would enjoy the symposium more.

I've recently started going to a bunch of the Stanford Bio-X talks and they've been great. I have no idea why I've lived this long in Palo Alto and not taken advantage of the many and various free talks at Stanford. Oh well, better late than never I suppose.

Casey

26. **Lowell Brown**
December 8, 2017

I do not think that 2,000 years in the past has anything to do with 2,000 years in the future. On must relate past to future years by many scale factors. For example, one should divide the year by the number of scientists living in that year. And divide by the large and multiply by the small energies that can be measured. Same for distances and times. Divide by the number of communication links. Divide by the knowledge acquired in some measure. So, allowing for my crazy big factors, maybe 2,000 years in the past is 2 or 20 years in the future.

27. **Philip Gibbs**
December 9, 2017

"Multiverse" is a word that covers may different ideas in physics. Since Spergel is a cosmologist he may be thinking mostly of the multiverse version of eternal inflation. This is a very incomplete theory built on several layers of speculation. I don't favour it myself and agree with those who say that it has been overhyped. I don't think the logic in support of it is particularly good, but I accept that some version of it could be correct and don't see why others should stop thinking about it.

The multiverse idea can however also be used to refer to anything from ordinary Hilbert space in quantum mechanics up to the Mathematical Universe of Tegmark. Even the anthropic principle means different things to different people and is obviously a broad philosophical principle rather than a specific physical theory. It is easy for someone to conflate the issues by talking about the multiverse without being clear about which of these things they actually mean. A more concrete question is whether the vacuum state with the spectrum of particles we know is a unique consequence of some fundamental theory, or one of several possible outcomes whose selection involved an element of historical chance. I think this is a perfectly scientific question. It could only be answered in the context of some theory that we do not have yet. It is difficult to see how it could ever be tested or falsified directly, but a satisfactory answer may be given by a theory that is perfectly testable in other ways.

At this time phenomenology beyond the standard models is very scant and quantum gravity phenomenology is non-existent. I don't think epistemology or any other branch of philosophy can be applied to answer questions like that. The scientific method determines the soundness of the way scientists operate but it does not offer definitive answers to scientific questions. It is only right that physicists are allowed to explore all options mathematically until the observational prospect improves. Opposing philosophical principles have always played a part in the directions theorists go. Any theory of quantum gravity you could imagine would not be falsifiable while quantum gravity phenomena are inaccessible to experiment, so that kind of criticism is void.

It is sometimes argued that lack of funding and the influence of a small section of elite theorists is stopping people from exploring all the alternatives, but I suspect that if good new ideas are waiting they will be found sooner or later despite what anyone says. Everyone is free to express their opinions and explore the directions they favour, but it is not helpful to deliver polemic speeches that criticise other people for following the path they find most promising. Surely Spergle is sufficiently competent and well placed to coach his own students to help him research any direction he wants to go in.

28. **Jernej Satler**
December 9, 2017

David. Braaaavo!

29. **R LeVitt**
December 9, 2017

Well, alright, I'm going to say something that I probably shouldn't say here. I don't think the monoverse is a testable and interesting scientific hypothesis. ;^)

My point being, we have exactly zero evidence that our Universe is a singleton. The hypothesis that our cosmos is unique seems an unprovable idea with no data supporting it. As the old saying goes, "Absence of evidence is not evidence of absence." Which in my view makes the monoverse and multiverse more or less equivalent from an empirical point of view.

If I understand the problem correctly, the problem of one or many cannot be resolved by experiment. If so, theoretical ideas may give us the only hints, however tenuous.

30. **a1**
December 10, 2017

@RLeVitt: multiverse vs. monoverse, who wears the burden of proof?
Universal statements are falsifiable, existential statements are verifiable.

31. **Fabien Besnard**
December 10, 2017

I would just like to stress that there is a huge leap between saying that
1) String theory multiverse is not justified because string theory isn't,

and

2) no scientific theory whatsoever may contain reference to a multiverse.

While the first statement seems to me to be an accurate description of the current situation, I see the second one as a dogmatic position which can potentially hinders scientific progress.

32. **William Astley**
December 10, 2017

An infinite number of patches with infinite diversity possibilities is also a problem for 'inflation'.

If it is theoretically possible for there to be good inflation (Inflation is the name for the 100,000 times faster than the speed of light expansion of space by the hypothesized inflaton field immediately after the BB event) then there would also be bad inflation.

http://physics.princeton.edu/~cosmo/sciam/

"What do you mean? Inflation has two major problems: First of all, we have learned that inflation is highly sensitive to initial conditions. This is the opposite of what everyone thought originally. For example, in the 1990s, by considering different initial conditions and parameters, Linde (and others) championed models of inflation that would lead to an open universe rather than a flat universe, because, at the time, observations seemed to point that way."

"Second, we have also learned that inflation generically produces a multiverse ("multimess") of outcomes – literally an infinite number of patches with an infinite diversity of possibilities – and there is currently no criterion to prefer one possibility over another. As Guth has put it, "In an eternally inflating universe, anything that can happen will happen; in fact, it will happen an infinite number of times. Thus, the question of what is possible becomes trivial—anything is possible […] The fraction of universes with any particular property is therefore equal to infinity divided by infinity—a meaningless ratio." See, highlighted text in the Conclusion section of Guth's paper published in J.Phys. A40, 2007 (LINK). In other words, there is nothing that says that what we observe in our patch is typical or could be predicted a priori on the basis of the theory."

33. **Peter Woit**
December 10, 2017

Spergel clearly was referring to the cosmological multiverse, and I'm sure he would say that claims there is only one universe are just as unscientific as multiverse claims. And I doubt he intends to criticize theorists for working on whatever they want, the problem is when they decide to start selling untestable ideas to the public to further their agenda.

34. **R LeVitt**
December 10, 2017

@ai: My point was that neither the multiverse nor the monoverse ideas are falsifiable at present, if ever. The only existential, verifiable statement we can make is that there is at minimum one Universe.

As for burdens of proof, nature doesn't require them–and is under no obligation to make all her secrets known to us.

35. **Peter Woit**
December 10, 2017

All,
Unless you have something substantive to say about the Spergel story, please don't add to this comment section. Most attempts to argue about the multiverse do nothing but reinforce Spergel's point that nothing is being explained and we're best off without such arguments.

36. **JimV**
December 23, 2017

It is probably too late, since I just saw this post today (which is my fault), so if this comment isn't accepted so be it.

My thought on the multiverse/Anthropic answer to fine-tuning is that yes, it could promote some sort of understanding because it supplies a mechanism, and once one has a mechanism it is easier to explain a concept. Sort of like when Feynman was asked what he would like to pass on as a starting point to another civilization (or something like that) and he said the concept that matter is composed of tiny bits that are sticky when close together (or something like that). That example is not completely on-point because it's true and can be tested, but just the availability of the multiverse idea is, to me, sufficient to counter fine-tuning arguments. It demonstrates that there is another logically-possible alternative, whether that alternative is testable and/or true or not.

And I like to have a mechanism. It makes things so much clearer if I can see how something could occur even if I don't know whether it occurs. So I'm glad the multiverse concept exists. Granted, we always should preface such speculations with "I don't know if this is true", but isn't it fun to speculate?

This Week's Hype/Fake Physics

December 11, 2017
Categories: Fake Physics, This Week's Hype

On the Fake Physics front, Jerry Coyne at Why Evolution is True has a post claiming New evidence for the multiverse-and its implications. You would think that recent history should have made clear the danger of using Youtube videos as a reliable source of information, but this posting is based mainly upon a Youtube video, one that claims Evidence for a Multiverse in NASA and ESA Satellite Data? (Coyne seems to have missed the question-mark).

As usual, a large part of the problem here is people looking for material helpful to their arguments, without worrying much about whether the material is accurate or not. In Coyne's case, he wants to counter the theological fine-tuning argument with the multiverse counter-argument, which requires a multiverse with a wide variety of different physical laws. The Youtube video he found makes the standard tenuous argument that the CMB provides evidence for inflation, inflation should be eternal, thus there should be a multiverse. As I explained in detail here, the models of inflation one supposedly has "evidence" for are not models that lead to the kind of multiverse of different physical laws that Coyne needs for his argument with religion.

I should make it clear that I'm on Coyne's side in the argument of evolution vs. religion, but scientists arguing on the basis of science should take care that they're using good science if they don't want to discredit themselves. And, as a general rule for anyone who cares about what's true and what isn't, looking for things on Facebook or Youtube that help your side of an argument is now an extremely bad idea.

The question of how to stand up for truth in a post-truth era was the main topic of this year's Nobel Week Dialogue (video here). David Gross gave a rousing and inspirational talk on Truth and the Scientific Method (starting about 36:30), which ended with the assurance that "Science will survive Donald J. Trump and his ilk", because of its rigorous honesty and grounding in experimental testability. Gross is someone well-aware of the multiverse Fake Physics danger, although he didn't mention it. I'd feel a lot better about his Stockholm talk though if I hadn't just recently attended this disturbing one.

Update: Some more fake physics today, from the Russians, courtesy of Sputnik News. See here for propaganda about "a testable theory on how matter behaves inside a black hole" which is also supposed to describe quark-gluon plasmas at the LHC and RHIC . This is all based on this paper, which has no such thing.

Comments

1. **Daniel Tung**
   December 11, 2017

   Regarding the part of your Inference article on inflation and multiverse, isn't it that eternal inflation itself, without any input from superstring theory, already implies the idea of multiverse? This is also why Paul Steinhardt and a few others oppose it so strongly.

2. **Peter Woit**
   December 12, 2017

   Daniel Tung,
   In my article I was trying to point out that actual models of eternal inflation being advertised on this Youtube video and elsewhere are single inflaton field models with a simple potential, which produce a multiverse of universes with the same physics. To get a multiverse of universes with different physics, you need something much more complicated, and this is where string theory gets brought in.

   Steinhardt's objections to eternal inflation were discussed in detail here
   http://www.math.columbia.edu/~woit/wordpress/?p=9289
   I think they're essentially the same as Spergel's (and shared by most physicists): if your theory is unfalsifiable, you're not doing science, and that's what's going on here.

3. **Palmer**
   December 22, 2017

   Peter, you talk about the danger of using youtube videos as a reliable source of information. But this seems like too broad a brush to be taken seriously. Are all youtube videos to be treated equally? Why is a youtube video necessarily any less reliable than a blog post?
   In the case of the video linked to by Coyne, it's an interview with George Efstathiou
   who is not just a cosmologist but an award winning CMB Specialist and the spoke person for the European Space Agency's Planck CMB probe. Why should i take a blog post written by a non cosmologist more seriously than a video interview of this more than qualified cosmologist?

   You claim "The Youtube video he found makes the standard tenuous argument that the CMB provides evidence for inflation, inflation should be eternal, thus there should be a multiverse."I think there is a more to the video than what you imply. The question asked of Efstatuhiou was not just is there evidence for inflation and is inflation eternal? But whether or not the Planck data favours models of inflation that are eternal.His answer is yes and that this leads us to a multiverse. He even said this was the most important result from Planck. Now if you want to disagree with Efstathiou on this you need to give us a reason why he is wrong. To simply say he said it on a youtube video and hence dismiss it won't do.

   If we look more deeply at the debate over inflation and the multiverse. Both sides of this debate seem to agree with Efstathiou. Note Steinhardt and his colleagues said in this paper:
   https://arxiv.org/pdf/1304.2785.pdf
   "The plateau-like potentials selected by Planck2013 are in the class of eternally inflating models, so the multiverse and its effects on predictions must be considered. "
   Guth and his colleagues replied:
   https://arxiv.org/abs/1312.7619
   "First, they argue that the plateau potentials favored by Planck will lead to eternal inflation, and hence the measure problem [40]. We agree that if the observable inflation occurred on a plateau-like potential, eternal inflation seems very likely."
   What is in this video? It's Efstahtioua claiming that these flat potential models favoured by Planck produce a multiverse. Something then that many other cosmologists seem to agree with. And yet again not just any old cosmologists, but the very ones you have quoted in the past. It's a claim not just on youtube but in the peer reviewed literature.
   .
   As to your comment that "the models of inflation one supposedly has "evidence" for are not models that lead to the kind of multiverse of different physical laws that Coyne needs for his argument with religion.". i think this is a poorly informed statement. Have you ever watched a debate between a theologian and an atheist on fine tuning? Did you watch Caroll versus Craig? The theologian often argues that fine tuning is explained by necessity, chance or design. How do they rule out necessity? By appealing to string theory and its landscape.

Now I'm with you on this Peter, there is no evidence for string theory so there is no reason from the perspective of fine tuning alone, to rule out necessity and invoke a multiverse. But as Craig appeals to string theory to rule out necessity then Coyne can use this evidence for the multiverse to rebut him. If Craig wants to doubt string theory then Coyne can raise the possibility of necessity. Its seems then that this "evidence" is relevant to the debates Coyne has in mind and so he is wise to quote it. Out of 1) necessity 2) a multiverse or 3) design by an immaterial mind. it seems like the multiverse is the only one with any evidence in favour of it. I agree the many pocket universes of eternal inflation might turn out to be all the same without different constants. This would a very boring multiverse yes. But surely the existence of many pocket universes ups the probability that 2 is the right solution and given the complete lack of any evidence for necessity of immaterial minds it seems that is all Coyne needs.

If you want to help Coyne and his fellow travellers refute The Trump supporting Craig and his acolytes it might be helpful to explain how you deal with the fine tuning argument yourself. Criticising Coyne for quoting a well respected cosmologist without providing any reason to doubt them other than the claim is being made on youtube does not seem like a well thought out response to me.

4. **Peter Woit**
December 22, 2017

Herbert Palmer,
You completely ignore the main point I keep on repeating: the "eternal inflation" single-field inflaton models for which (tenuous) evidence is claimed do not give you the kind of multiverse you want for your arguments with theologians (to get this you need something much more complicated, like string landscape models, for which you have no evidence at all).

No, I haven't watched Carroll vs. Craig, I think that kind of thing is a complete waste of time. For those who think it is worthwhile, if you want to invoke scientifically untestable ideas about a multiverse as a possible explanation for fine-tuning, you can do so if you want, they're just as good as what the theologians have. Just don't claim that CMB measurements provide scientific evidence for this, because it's not true.

Sorry, but I just don't understand why anyone who wants to engage in these arguments thinks it's a good idea to abandon the high ground of actual, tested science and start invoking untestable pseudo-science and misleading claims of evidence that's not there. I don't see at all why that's either necessary or desirable: why sell the public on fake physics because you find it convenient to make a debating point in a tedious argument?

5. **Pete Best**
December 24, 2017

https://www.forbes.com/sites/startswithabang/2017/05/11/what-if-cosmic-inflation-is-wrong/

Peter, this article spells out the conflicting opinions on inflation. There is data and reasoning to back it up so it's not merely fake physics as you appear to be suggesting. Sure it's not full proof (what science is?) but alternatives to inflation and the issues it addresses are no better or worse than inflation.

So although inflation has its critics and is slated as not science by your good as well as others it does have data from Planck and it does therefore have more merit than you appear to be suggesting I am suggesting. Inflation may have too many models and hence can predict anything but the data leans towards some form of inflation did take place.

6. **Peter Woit**
December 24, 2017

Pete Best,
This posting is not about inflation, but about Jerry Coyne's article claiming "evidence for the multiverse and its implications" (the implication for him is the multiverse argument against theologians invoking a deity responsible for fine-tuning). The "Fake Physics" is the multiverse of universes with different physics that Coyne wants for his side of the argument. There is zero evidence for such a thing.

7. **Palmer**
December 27, 2017

Hello Peter, thanks for taking the time to reply. I do feel your reply misses the mark though. Your only complaint left standing seemed to be that the evidence is not for the type of multiverse that Coyne needs to dispute the theistic fine tuning argument. But then you admit you haven't spent any time researching what points are actually used by theists to make these arguments. But as I explained to you in my post , had you watched such debates, you would know that theists need string theory (or something else like it that can change the constants of nature) in order to support a key premise in their argument.
Here is a quote from the Bill Craig in his debate with Sean Carroll.

https://www.reasonablefaith.org/media/debates/god-and-cosmology-the-existence-of-god-in-light-of-contemporary-cosmol/

"Now there are three possibilities debated in the literature for explaining the presence of this remarkable fine-tuning: physical necessity, chance, or design. The question then is: Which of these three alternatives is the most plausible? On the basis of the evidence we may argue:
1. The fine-tuning of the universe is due to either physical necessity, chance, or design.
2. It is not due to physical necessity or chance.
3. Therefore, it is due to design.
Physical Necessity?
Consider the first alternative, physical necessity.
This alternative seems extraordinarily implausible because the constants and quantities are independent of the laws of nature. The laws of nature are consistent with a wide range of values for these constants and quantities. For example, the most promising candidate for a Theory of Everything (T.O.E.) to date, super-string theory or M-Theory, allows a "cosmic landscape" of around 10500 different universes governed by the present laws of nature, so that it does nothing to render the observed values of the constants and quantities physically necessary."

Or consider another example from IS There A God.Info

http://www.is-there-a-god.info/clues/teleological/

"Currently the most promising possibility for a ToE is string theory, but Stephen Hawking said: "Does string theory predict the state of the universe? The answer is that it does not." In other words, string theory does not provide the physical necessity for the universal fine-tuning."

So you see that the theist needs string theory to rule out necessity. Coyne does not need string theory. If it turns out that string theory is false then necessity cannot be ruled out by the theist. And so they can't follow through to the conclusion of God. If string theory is right (or at least the landscape or something like it) then Coyne needs something to generate multiple worlds to populate the landscape. The multiple bubble universes of eternal inflation seem like a good candidate. However if string theory is right in that the constants of nature could easily be different but there's no way to generate multiple worlds, only then Coyne is in trouble. So evidence for other bubble universes supports Coyne's case whether or not these other bubble universes have the same or different properties to our own. You are just wrong about what is needed in these debates from Coyne's point of view. And the reason you are wrong is , by your own admission, you don't pay attention to the said debates. The Carroll/Craig debate is considered the number one debate on this topic by most in my experience of attending these sorts of debate environments. So that fact you haven't watched even that one, is very telling.

Moreover Coyne never claimed any evidence for the string theory landscape and neither did the video. If you look up the word multiverse in the dictionary it does not mention the string theory landscape . Here is the OED's definition:

https://en.oxforddictionaries.com/definition/multiverse
"A hypothetical space or realm consisting of a number of universes, of which our own universe is only one."
Bubble universes of eternal inflation seems to fit that, even if its a boring multiverse where they all have the same values.

So there is absolutely nothing fake about claiming evidence for eternal inflation and calling that a multiverse.

Coyne himself acknowledges there are different ways to conceive a multiverse. Coyne also brings up caveats about why the evidence presented is not decisive "Now, according to this video, we've gotten some evidence for the multiverse, though our Official Website Physicist™ notes (see below) that the new evidence isn't terribly decisive."
The video itself mentions that inflation could still be wrong or perhaps misunderstood and gives caveats about why the evidence presented might not show the multiverse to be true.The video itself had a question mark at the end and you said "Coyne seems to have missed the question-mark Coyne published". But those caveats that were on the video are in his post. So if anyone was missing something, it is you. What you see then in both the video and Coyne's blog post then is yes excitement about potential evidence for a multiverse, but also nuance, caveats and thoughtful discussion.
All of which are absent from your blog post.
You made accusations about fake news, dubious sources and comparisons with Donald Trump. None of this stands up to scrutiny.
And one of the most prominent intelligent design blogs is already using your blog post to rubbish their arch critic Coyne.
https://uncommondescent.com/fine-tuning/peter-woit-on-whats-wrong-with-jerry-coynes-argument-for-a-multiverse/

I suggest you need to provide some sort of apology or retraction. I loved your book "Not Even Wrong" but now it seems that you are the one using hyperbole that is not supported by the evidence.

8. **Peter Woit**
December 27, 2017

Palmer,

Sorry, but I'm not going to waste more time involved with the mountain of stupidity of intelligent designers, http://www.is-there-a-god.info, Bill Craig etc. If you, Coyne, Carroll or anyone else wants to spend their time this way, it's a free country. But, the fact of the matter is that CMB observations do not provide any significant scientific evidence for the argument you want to make. Taking stuff off Youtube to make a bogus scientific argument against the believers does nothing but discredit science and scientists. You and Coyne should just stop doing it. You have plenty of perfectly good arguments, why discredit science by making a bad one?

9. **Palmer**
December 30, 2017

But you haven't shown any evidence of any bogus science. All you have is a straw man argument. You are claiming no evidence for a string landscape. But no one is claiming evidence for the string landscape. You say you need the string theory landscape to make the point in the debate. But these are debates you admit you have never watched. So I explained why the string landscape is actually irrelevant and your response is just to ignore my argument and make the same accusation.

So to repeat, the string landscape is irrelevant , it is not needed, you are wrong. Why are you wrong? Because the theists usually claims fine tuning is explained by either necessity, chance or design. The theist uses the string landscape to rule out necessity as a possible explanation for the constants of nature. If you watched the Caroll/Criag debate or others you would know this. If the string theory landscape is right but eternal inflation (or something like it) is false, then there is no way to populate the landscape and the theist can rule out necessity and chance and claim design is the winner. But if we have someway to populate the landscape, and eternal inflation would seem to do the job, then chance cannot be ruled out. And if the string landscape is false then necessity can't be ruled out either. So the theist needs the string theory landscape to be true and eternal inflation to be false. As long as they are wrong about one of those things, their argument fails.

The evidence Coyne points to then does help his case. Is it a knockdown piece evidence ? No, but he never claimed it was. It's something that helps his case, nothing more, nothing less. Please don't argue against a straw man. As far as I can see, out of necessity, a multiverse or design by an immaterial mind, the multiverse is the only one that has even a shred of evidence in favour of it. If you have evidence for some other option, let's see it.

If you are going to tell us whats useful in a debate against the ID crowd it would be helpful if you actually did so, rather than telling us what not to say. It would be helpful if you knew what these people actually said rather just imagining what is said. I read your blog a lot, but I don't think I've ever seen you give what you think is the correct response to the fine tuning argument. Why is that? Maybe i missed it. But it's too easy to criticise someone else when you have having zero to offer in their place. Why don't you watch the debate and tell us how you would reply? oh yes i forgot its beneath you. This "higher than thou" attitude is actually what gives science a bad name. Meanwhile the religious right are on campuses, promoting their videos online and making their arguments which some people find persuasive or those that already persuaded find empowering. And your preferred strategy is to say nothing to counter them. I'm not convinced that has worked out well. Now they in power and using that power to attack science.

10. **Peter Woit**
December 30, 2017

Palmer,
The problem is that the "evidence" Coyne is trying to use is not evidence for a of model of inflation that populates a landscape, it's evidence for a model of inflation with no landscape. If you believe the model that supposedly this is evidence for, you predict a multiverse of universes with exactly the same fine-tuning problem you started with. Claiming evidence here for the different kind of multiverse you want is just misleading and dishonest, and the way Coyne is doing it (via promoting a dubious Youtube video) is a really bad idea. Defending this kind of activity just discredits the naturalist side of the debate with theologians. You should be arguing with Coyne and getting him to stop, not arguing with me.

I don't engage with these theological debates because I don't think they have anything to do with the serious problems now facing us. The awful post-truth collapse of democracy we are facing is not driven by theologians and believers: Trump and Rupert Murdoch don't believe in God, they believe in the power of the Big Lie . By ignoring truth and science in your desire to score a point in an irrelevant side-show, you're discrediting the fact-based opposition to this. Fake physics is not going to solve the problem of Fake news, it's going to make it worse.

11. **Palmer**
January 2, 2018

Peter, Happy New Year to you. I don't believe you addressed anything I said but just keep singing from the same old hymm sheet. So I'm afraid I will have to end the

discussion of the multiverse here. But if you think Trump and Murdoch can take power without their army of religious believers you are quite mistaken. Just look at Craig and many other influential Christians endorsement of Trump. Perhaps some data on how the faithful voted might help. 80% of white evangelicals voted for Trump whereas 70% of "none's" voted for Clinton. So it does not look like your claim that post truth is not driven by believers is supported by the data.
http://www.pewforum.org/2016/07/13/evangelicals-rally-to-trump-religious-nones-back-clinton/

12. **Peter Woit**
January 2, 2018

Palmer,

White believers voted (like most categories of white people) for Trump. African-American believers voted (like all categories of African-Americans, except the crazy ones) heavily for Clinton. Neither of these facts is relevant to the issue here of whether it's worth discrediting science with untrue claims about the multiverse.

Sorry all, that's it for the political discussion.

13. **pete best**
January 3, 2018

I thought the idea of the multiverse came from the notion of inflation and the inflaton field that is potentially endless so how can you say that inflation is no relevant to the conversation?

I had never heard that standard Guth inflation always had the same physics (perhaps different initial conditions) and it needs string theory in order to have 10^500 possible multiverses thus negating the need for the anthropic principle. I did a search around this but didn't find anyone say this was indeed the case? Can you demonstrate this is indeed what inflation (standard or new inflation) states please ?

14. **Peter Woit**
January 3, 2018

pete best,
There are an infinity of "inflation" theories, I don't know what the "standard one" is. The ones typically used involve a single inflaton field, with a simple potential, it is these that claims are being made to supposedly have CMB evidence for. A single inflaton field with a simple potential is not going to give you a multiverse with the wide array of physics you need.

Various and Sundry

December 11, 2017
Categories: Uncategorized

A few things that may be of interest:

- Survey articles prepared for the 2018 ICM proceedings are starting to appear on the arXiv, and Peter Scholze (who will be getting a Fields Medal in Rio) has put his on his web-site. His title is p-adic Geometry, and it gives an overview of the ground-breaking work he has been doing over the last few years. The last section tells us that

  > Currently, the author is trying to understand to what extent it might be true that the "universal" cohomology theory is given by a shtuka relative to Spec **Z**. It seems that this is a very fruitful philosophy.

  For some background about that section, I'd recommend his talk at the 2015 Clay Math conference.

- The New Yorker has a very detailed and interesting profile of Jim Simons and what he is up to with the Flatiron Institute he is now funding here in New York. This new Institute is costing him \$80 million a year, characterized as "a lark" for someone with his assets. David Spergel is running the Center for Computational Astrophysics there, and doing a lot of hiring. When I wrote here about his characterization of multiverse research, his final comment about being able to speak freely because he had tenure left me wondering "wait, what about grants, jobs, etc.?". From the New Yorker article, I realized that while having tenure may give you some ability to speak freely, having a guy with \$18.5 billion willing to write large checks for you gives you a lot more…
- I've just finished teaching a course this semester which concentrated on the formalism for describing geometry in terms of connections and curvature. From the point of view of physicists, this formalism should be of interest because it applies equally well to gauge theory and general relativity. I'd been starting to think again about what light this formalism might shed on how to think about these two subjects together, when last night I noticed a wonderful new article on the arXiv, Gravity and Unification: A review by Krasnov and Percacci.

  This article is an extremely lucid and comprehensive survey of the sort of thing I was thinking about, which can be re-expressed as the question of trying to find, at the classical level, a formalism uniting the vector potentials/field strengths of the SM and the different possible fields used to describe geometry in GR. Some of this has a very long history, going back to the things Einstein was trying in his later years. There have been many different ideas that people have tried since then, and the survey article does a great job of both explaining these ideas, as well as indicating why they haven't worked out.

  A couple of the general ideas that have always fascinated me make an appearance in the article. One of these is that of what mathematicians call a "Cartan connection", the idea that you should think of a geometry as locally looking like a quotient space G/H of two Lie groups. A version of this is known to physicists as the MacDowell-Mansouri formulation, which gets a detailed treatment in the article. Another is the idea of using the fact that the complexified orthogonal group in 4 dimensions breaks up into two pieces, sometimes thought of at the Lie algebra level as self-dual vs. anti-self-dual pieces under the Hodge star operation. A version of this idea is known as the Plebanski formulation, and this decomposition is behind the story of Ashtekar variables. These variable have played a crucial role in modern treatments of GR by Hamiltonian methods, as well as the quantization program of loop quantum gravity.

  The focus of the article is on Lagrangian and classical field theory methods for studying these ideas. There's relatively little about the Hamiltonian story, and also relatively little about the geometry of spinors, two topics that I suspect might provide additional needed insights. For anyone interested in thinking about non-string theory-based ideas about unification of the SM and gravity, there's a wealth of ideas, references and history here to think about. Perhaps future progress on unification will come from some new breakthrough in this field that shows how to get around the problems identified clearly in this article.

- For surveys of recent work on quantization of gravity and discussion by experts, a good place to look is videos of talks at a recent conference held at the IHES. Videos available here include my fellow Princeton student Costas Bachas surveying the approach growing out of string theory in Holographic Dualities and Quantum Gravity, Carlo Rovelli the opposition in Current Quantum Gravity Theories, Experimental Evidence, Philosophical Implications, and an even-handed overview from Steven Carlip with Why We Need Quantum Gravity and Why We Don't Have It. Also well-worth watching, both for the talk and the discussion, is Alain Connes on Why Four Dimensions and the Standard Model Coupled to Gravity.

Finally, for fans of Lenny Susskind's introductory level books on theoretical physics, Andre Cabannes writes to tell me that the most recent volume (which I wrote about here) is being translated by him into French, to appear next year. He also has notes from the lectures on General Relativity, Cosmology, and Statistical Mechanics, for which no book form has yet appeared.

Update: For a detailed account of the event at NYU mentioned here, see this from Jerry Alper.

Comments

1. **David Roberts**
   December 11, 2017

   > Peter Scholze (who will be getting a Fields Medal in Rio)

   Bold statement there 😉

   I agree with your assessment and sentiment, but some might think you have inside knowledge.

2. **David Roberts**
   December 11, 2017

   Note that Scholze cite Fargues' ICM paper, *La courbe*, which doesn't seem to be on the arXiv.

3. **sdf**
   December 12, 2017

   Scholze is the easy one, what about the others? Williamson probably, who else?

4. **Egan**
   December 12, 2017

   >>>Scholze is the easy one, what about the others? Williamson probably, who else?

Ciprian Manolescu?
Hugo Duminil-Copin?

See here : https://poll.pollcode.com/44839318

5. **Peter Woit**
December 12, 2017

Please, unless you actually know who is getting a Fields medal and want to tell us, let's leave that speculation game for another time.

6. **John Baez**
December 12, 2017

Derek Wise wrote a great thesis on Cartan geometry and various formulations of gravity, including the MacDowell-Mansouri formulation – but also the more familiar Palatini formulation, where the SO(3,1) connection and the cotetrad field, which is locally an R^4-valued 1-form, fit together to form a Poincare group connection. What's nice is that Derek explains it all very geometrically. His paper MacDowell-Mansouri gravity and Cartan geometry is probably the easiest place to start. One doesn't need to understand Cartan geometry ahead of time: he explains it nicely using a picture of a hamster rolling in a hamster ball.

7. **Peter Woit**
December 12, 2017

Thanks for reminding me of that John. That paper is a wonderfully clear explanation of Cartan geometry and how to formulate GR using it.

8. **tulpoeid**
December 13, 2017

On the update: Alper's article is a detailed account of how, uninvited, he tries to get closer to the speakers (before the discussion) and two members of the audience, just to show off and obtain some personal advice. And in addition presenting them rather unfavorably for not showing him the warmth he was expecting. I don't know if he'll report on the event itself, but the time spent on this article doesn't tell anyone anything about it (except maybe how great he believes Nima is, yes, unrelated to the event). The first paragraphs about today's students in high profile universities is rather interesting though.

9. **Chris Oakley**
December 13, 2017

I read Alper's piece and am pleased, though not surprised, to discover that Hossenfelder is not obviously susceptible to flattery. Speaking from experience, being a thorn in the side of academia is much easier, not to say effective, if you retain a foothold in it, so let us just hope that she finds a way to do that.

10. **Bill**
December 13, 2017

Reading Jerry Alper's piece, I could not help thinking that the reason Peter had so much trouble remembering him was because during their three-hour interview Peter's mind was too focused on keeping the conversation on topic to remember the interviewer.

11. **Peter Woit**
December 13, 2017

tulpoeid,
I think you're being unfair to Alper. This was an event aimed at the public, so he was certainly invited, and having the audience interact with the speakers was part of the goal there (the audience was not that large). He's a fan of the speakers, it was a quite appropriate place for him to meet them and have a conversation. While (just like in the piece he wrote about talking to me a while ago) his writing style is very personal and largely about himself and how he experiences things, it is pretty much factually accurate. Yes, I didn't immediately recognize him (he did prompt me quickly, probably I would have remembered who he was in a few moments, although I've gotten very bad at recalling names). Yes, the quote from me about Sabine was accurate. I was there for part of his conversation with her, and what he writes is consistent with what I remember.

12. **Derek Wise**
December 13, 2017

Peter,
Thanks! My paper on MacDowell-Mansouri gravity seems to have helped a lot of people understand Cartan geometry and its relationship to gravity, and that makes me really happy!
Since you mentioned the Ashtekar approach in the original post here, I'll point out that Steffen Gielen and I worked out the precise sense in which the real Ashtekar-Barbero formulation is a theory of evolving spatial Cartan connections, starting with our paper Spontaneously broken Lorentz symmetry for Hamiltonian gravity, and then with some of the geometric ideas explained further in Geometrodynamics and Lorentz symmetry.
This of course is not quite the same thing as the Plebanski-type symmetry breaking you mentioned. I've never worked out the precise sense in which the Plebanski formulation is a case of Cartan geometry (is it?!), but I think the above papers (and other work with Steffen Gielen) give the "right" way to understand real Ashtekar variables geometrically.
-Derek

13. **tulpoeid**
December 13, 2017

Maybe I exaggerated my feeling, triggered by the negative-in-disguise portrayal of those who didn't respond according to his expectation. ("Uninvited" was about the repeated attempts at professional cordiality.)

14. **Urs Schreiber**
December 13, 2017

It may be helpful to notice that, in different terminology, Cartan geometry is fairly familiar to many physicsists, who may call it "first-order formulation of gravity" or similar and regularly use it when discussing gravity coupled to fermions.

An excellent but widely underappreciated (forgotten?) textbook all based on this perspective is "Supergravity and Superstrings – A Geometric Perspective".

The authors don't use the words "Cartan geometry", but anyone who knows the subject will immdiately recognize that this is precisely their "geometric perspective" (working locally, of course).

While the aim of this textbook is to go further to super Cartan geometry and then to higher Cartan geometry, the entirely of part one (in volume one) is about standard gravity in terms of Cartan geometry.

15. **Thomas Larsson**
December 14, 2017

    So Cartan geometry is basically the same thing as the vielbein formalism? Then even I would be able to understand.

16. **Urs Schreiber**
December 14, 2017

    Yes. For some reason where in maths texts the term "Cartan geometry" became established, physics texts stick to Cartan's original terminology (Cartan 22) and speak of the "Cartan moving frame method".

    Of course mathematicians have developed the theory with more generality and precision, a seminal textbook is Cap-Slovák 09.

    Another terminology issue to be aware of is that many physicists who do say "Cartan geometry" are concerned with an independent issue, namely the question whether encoding the field of gravity in a "Cartan connection" = "vielbein + spin connection" instead of in a metric tensor (which is what Cartan geometry itself is about, and which is well known and uncontroversial) suggests ways to rewrite the Einstein-Hilbert Lagrangian in ways (e.g. Palatini-Cartan-Holst form) that are then often subject to much speculation about a deeper nature of gravity.

    In view of this it is remarkable that, while the on-shell equivalence of the Einstein-Hilbert action to the Palatini-Cartan-Holst action was long known, the equivalence of the two Lagrangian field theories on the level of phase spaces was established only this year, by Cattaneo-Schiavina 17.

17. **Peter Woit**
December 14, 2017

    Derek and Urs,
    Thanks for the references!

    Thomas Larsson,
    I'd say you have a Cartan geometry when you have a Cartan connection. You can do this generally for a pair H,G of Lie groups, H a subgroup of G, identifying the tangent space of your manifold with Lie G/Lie H. The case of GR is G=Poincare, H=Lorentz. In this case the spin connection and vielbein together are the components of the Cartan connection, with the curvature of this connection having components the usual curvature of the spin connection, and the torsion.

18. **Derek Wise**
December 14, 2017

    Urs, I disagree — Cartan geometry is not just the same things as the method of moving frames. Frames are an important *ingredient* in Cartan geometry, and in certain cases (reductive geometries) the Cartan connection can be split into a coframe field and a connection for the stabilizer group. This special case explains the geometry behind first order formulations of gravity. But, Cartan geometry is much more general (and frankly much more "geometric") than the usual physics understanding of first order gravity. It's about generalizing the whole of Klein's Erlangen Program to the differential setting. It's also about solving the "equivalence problem" relating "raw" geometry (defined in terms of, say, smoothly varying structures on tangent spaces) to Cartan connections with special choice of groups and conditions on curvature. Sharpe's book has some great examples of this, for example, relating conformal geometry to Cartan connections based on either Weyl or Möbius models.

19. **Urs Schreiber**
December 14, 2017

    Derek, careful with suggesting that theoretical physicists don't understand these phenomena only because they use different words than you do. This attitude will backfire.

    If you look at the first part of "Supergravity and Superstrings – A geometric perspective" that i keep recommending, you'll see that it is exactly the geometric perspective of globalizing Klein's geometry that drives the development of their concepts.

    Of course they don't say the *words* "Klein geometry" or "Erlanger program" (we have to distinguish between names and the mathematical reality that they refer to) but they start by considering the homogenous coset spaces that are the Klein geometry hallmark and then find their crucial technique by globalizing these. In their re-invention of Cartan geometry this way, they came up with interesting ways of thinking about these structures: For instance they say "soft group manifold" for the differential form structure which globalizes Maurer-Cartan forms on Kleinian cosets to Cartan geometries. The terminology didn't catch on, but it reflects precisely the understanding that Cartan geometry is a "softening" of Kleinian geometry

    Also, these authors are well aware that this is more general than just the case of Minkowski = Poincaré/Lorentz: In that textbook they discuss also the dS and AdS case, and, crucially, they use all this only as the stepping stone to do super-Cartan geometry, which comes about from considering cosets of the super-Poincaré and super-(anti)de Sitter groups.

    This perspective keeps being developed to great generality by physicists: In approaches to make U-duality geometric one considers Cartan geometry over ever larger cosets, culminating in something like Cartan geometry modeled on E11 modulo its maximal compact sub-group (or sub-thing).

    So once you allow for the fact that not every physicists who studies Cartan geometry uses the same language as you do, you'll find that the Cartan geometry physicists have been and are studying is extremely general and powerful. It serves to pay attention to that.

    (Not the least, Cartan geometry is pretty much the only way to do supergravity — noticing that the only way to define local model super-spacetimes is as coset of supergroups — and that this is what physicists are doing has been particularly highlighted by John Lott 01.)

    While conformal Cartan geometry is not discussed in this particular textbook, they do discuss this in their published articles. (But I won't point to the arXiv now, not to distract you from opening their fantastic textbook!)

20. **Derek Wise**
December 14, 2017

    Urs,
    I certainly don't intend to suggest that physicists don't understand something because they don't phrase it in the language of mathematicians, or vice-versa. And I agree that there's a lot of great work in the physics literature related to Cartan geometry, including the D'Auria-Fré formulation, a bunch of work on first order gravity, and the tractor calculus literature, to name a few. But, when you answer the question "So Cartan geometry is basically the same thing as the vielbein formalism?" with "Yes" I have to disagree. That's just a small piece of Cartan geometry.
    Derek

21. **Jon A**
December 19, 2017

Towards the beginning of the paper of Cattaneo and Schiavina (CS) referenced by Urs's page on first order formulations of gravity, they write that they hope to reproduce for the Palatini-Holst (PH) action the success they'd had with the Einstein-Hilbert (EH) action. CS proved in 2015 the EH action gave a (Batalin Vilkovisk) BV theory which, on manifolds with boundary, moreover gave a proper (add Fradkin) BV-BFV theory, in the sense of Cattaneo, Mnev, and Reshetikhin (CMR). Given a solution of the corresponding master equation, this would give a quantization of gravity.

In Schiavina's thesis, he proves the Palatini-Holst action's minimal BV theory doesn't allow a CMR-BV-BFV theory – the kernel of the pre-boundary two form isn't of uniform rank changes as you move around the space of fields. Worse still, when you impose the Half-shell constraint (without which the connection is underconstrained), the pre-boundary two form isn't presymplectic. So this equivalence of on-shell phase spaces *doesn't seem* to extend to their derived locii (similarly for Pleibanski- while MacDowell Mansouri doesn't seem to have this problem).

Their more recent paper suggests either perturbing the action by a boundary to overcome this problem or considering boundary preserving variations. They explore the latter and obtain something again inequivalent to Einstein-Hilbert.

When CS first began publishing their work two years ago, I thought this stuff would become a hot topic in QG. Whenever I'd run into Michele on campus, I didn't get the sense other physics people cared about it. Good to see this work making its way into the collective consciousness.

Latest on abc

December 16, 2017
Categories: abc Conjecture, Favorite Old Posts

I've seen reports today (see here and here) that indicate that Mochizui's IUT papers, which are supposed to contain a proof of the abc conjecture, have been accepted by the journal Publications of the RIMS. Some of the sources for this are in Japanese (e.g. this and this) and Google Translate has its limitations, so perhaps Japanese speaking readers can let us know if this is a misunderstanding.

If this is true, I think we'll be seeing something historically unparalleled in mathematics: a claim by a well-respected journal that they have vetted the proof of an extremely well-known conjecture, while most experts in the field who have looked into this have been unable to understand the proof. For background on this story, see my last long blog posting about this (and an earlier one here).

What follows is my very much non-expert understanding of what the current situation of this proof is. It seems likely that there will soon be more stories in the press, and I hope we'll be hearing from those who best understand the mathematics.

The papers at issue are *Inter-universal Teichmuller Theory I, II, III, IV*, available in preprint form since September 2012 (I blogged about them first here). Evidently they were submitted to the journal around that time, and it has taken over 5 years to referee them. During this 5 year period Mochizuki has logged the changes he has made to the papers here. Mochizuki has written survey articles here and here, and Go Yamashita has written up his own version of the proof, a 400 page document that is available here.

My understanding is that the crucial result needed for abc is the inequality in Corollary 3.12 of IUT III, which is a corollary of Theorem 3.11, the statement of which covers five and a half pages. The proof of Theorem 3.11 essentially just says "The various assertions of Theorem 3.11 follow immediately from the definitions and the references quoted in the statements of these assertions". In Yamashita's version, this is Theorem 13.12, listed as the "main theorem" of IUT. There its statement takes 6 pages and the proof, in toto, is "Theorem follows from the definitions." Anyone trying to understand Mochizuki's proof thus needs to make their way through either 350 pages of Yamashita's version, or IUT I, IUT II and the first 125 pages of IUT III (a total of nearly 500 pages). In addition, Yamashita explains that the IUT papers are mostly "trivial", what they do is interpret and combine results from two preparatory papers (this one from 2008, and this one from 2015, last of a three part series.):

> in summary, it seems to the author that, if one ignores the delicate considerations that occur in the course of interpreting and combining the main results of the preparatory papers, together with the ideas and insights that underlie the theory of these preparatory papers, then, in some sense, the only nontrivial mathematical ingredient in inter-universal Teichmueller theory is the classical result , which was already known in the last century!

Looking at these documents, the daunting task facing experts trying to understand and check this proof is quite clear. I don't know of any other sources where details are written down (there are two survey articles in Japanese by Yuichiro Hoshi available here).

As far as I know, the current situation of understanding of the proof has not changed significantly since last year, with this seminar in Nottingham the only event bringing people together for talks on the subject. A small number of those close to Mochizuki claim to understand the proof, but they have had little success in explaining their understanding to others. The usual mechanisms by which understanding of new ideas in mathematics gets transmitted to others seem to have failed completely in this case.

The news that the papers have gone through a confidential refereeing process I think does nothing at all to change this situation (and the fact that it is being published in a journal whose editor-in-chief is Mochizuki himself doesn't help). Until there are either mathematicians who both understand the proof and are able to explain it to others, or a more accessible written version of the proof, I don't think this proof will be accepted by the larger math community. Those designing rules for the Millennium prizes (abc could easily have been chosen as on the prize list) faced this question of what it takes to be sure a proof is correct. You can read their rules here. A journal publication just starts the process. The next step is a waiting period, such that the proof must "have general acceptance in the mathematics community two years after" publication. Only then does a prize committee take up the question. Unfortunately I think we're still a long ways from meeting the "general acceptance" criterion in this case.

One problem with following this story for most of us is the extent to which relevant information is sometimes only available in Japanese. For instance, it appears that Mochizuki has been maintaining a diary/blog in Japanese, available here. Perhaps those who read the language can help inform the rest of us about this Japanese-only material. As usual, comments from those well-informed about the topic are welcome, comments from those who want to discuss/argue about issues they're not well-informed about are discouraged.

Update: Frank Calegari has a long blog post about this here, which I think reflects accurately the point of view of most experts (some of whom chime in at his comment section).

New Scientist has a story here. There's still a lack of clarity about the status of the paper, whether it is "accepted" or "expected to be accepted", see the exchange here.

Update: It occurred to me that I hadn't linked here to the best source for anyone trying to appreciate why experts are having trouble understanding this material, Brian Conrad's 2015 report on the Oxford IUT workshop.

Update: Curiouser and curiouser. Davide Castelvecchi of Nature writes here in a comment:

> Got an email from the journal PRIMS : "The papers of Prof. Motizuki on inter-universal Teichmuller theory have not yet been accepted in a journal, and so we are sorry but RIMS have no comment on it."

Update: Peter Scholze has posted a comment on Frank Calegari's blog, agreeing that the Mochizuki papers do not yet provide a proof of abc. In addition, he identifies a particular point in the proof of Conjecture 3.12 of IUT III where he is "entirely unable to follow the logic", despite having asked other experts about it. Others have told him either that they don't understand this either, or if they do claim to understand it, have been unable to explain it/unwilling to acknowledge that more explanation is necessary. Interestingly, he notes that he has no problem with the many proofs listed as "follows trivially from the definitions" since the needed arguments are trivial. It is in the proof of Corollary 3.12, which is non-trivial and supposedly given in detail, that he identifies a potential problem.

Update: Ivan Fesenko has posted on Facebook an email to Peter Scholze complaining about his criticism of the Mochizuki proof. I suppose this makes clear why the refereeing process for dealing with evaluating a paper and its arguments is usually a confidential one.

Comments

1. **Michael Barany**
   December 16, 2017

   I'll be fascinated to follow the technical and other discussions I'm sure this development will provoke. For my part, an observation about peer review in mathematics, which is now an active topic of both historical and sociological research.
   One often refers in evaluating math papers to three criteria associated with GH Hardy: is it (a) true, (b) new, and (c) interesting? The key point here is that *none* of

these is a binary yes/no question for most mathematical papers. There are always degrees of novelty and interest to a paper, and in most cases it is impossible to absolutely verify every claim of a paper. So an editor (and yes it would look better if the editor-in-chief were not the author) must always make a judgement call weighing those three factors (among other considerations): is it "true enough," "new enough," and "interesting enough." So I wouldn't find it unreasonable for an editor to say, under the circumstances, that the work's novelty and (especially) interest make tolerable a weaker consensus about validity to justify publication. Now, it's another question whether struggling to communicate the proof so far should count against the "interesting" criterion, but it's hard to say the proof hasn't generated a lot of interest!

2. **Peter Woit**
December 16, 2017

Michael Barany,
There's no question this is "new" and no question a proof of abc would be interesting. So, this is all about the "true" question: is this a proof or not? Math journals are not supposed to be publishing papers that are not "true", even if new and interesting. Checking a proof of an important result is a critical role of the math refereeing process.

The real problem here though is that there is an important criterion you haven't mentioned, quality of exposition: is the paper "well-written"? No matter how new, true and interesting a paper is, if it's too badly written the journal should return it to the author and tell them they have to rewrite and do better. There's a good argument that the IUT papers are not readable and checkable by experts in the usual way, so should have been rejected on those grounds.

All, I don't want to moderate a general discussion of the refereeing system. Comments should be relevant to this story of the IUT papers.

3. **mahmoud**
December 17, 2017

Could someone knowledgable comment on the effectiveness of Mochizuki's (purported) proof? Brian Conrad posted a detailed comment to an earlier post here explaining that Mochizuki needed a reduction step involving Belyi maps that made the implied constant non-effective, after that comment was made however Vesselin Dimitrov posted a paper on arXiv claiming to replace this reduction with a constructive one. Has Mochizuki commented on this? Is he now claiming to have an effective estimate on the constant (and hence effective bounds for the Mordell conjecture)?

4. **Robin Whitty**
December 17, 2017

Edward Frenkel thinks the possible conflict of interest worrying enough that he deleted his tweet @edfrenkel saying the publication was a 'big deal'

5. **Stephen**
December 17, 2017

"Nottingham the only event bringing people together for talks on the subject"

There was an additional conference in Kyoto which supposedly went a little better..

https://www.maths.nottingham.ac.uk/personal/ibf/files/kyoto.iut.html

6. **Peter Woit**
December 17, 2017

Stephen,
I was referring to "since last year". The Kyoto meeting was July 2016 and I wrote about the situation then here
http://www.math.columbia.edu/~woit/wordpress/?p=8663
As far as I can tell, little has changed since then. There was a meeting in Vermont Fall 2016, but for whatever reason over the last year there has been very little in the way of attempts to bring people together to discuss IUT.

7. **anon**
December 17, 2017

From old pages found on archive.org it appears that S. Mochizuki became the editor-in-chief of PRIMS some time between March and May 2012. So most likely he was the editor-in-chief already when the paper was submitted.

If it is indeed true that the papers are going to be published in PRIMS, I think there needs to be some sort of explanation about how the refereeing process was handled.

8. **Tom**
December 17, 2017

I'm a philosopher, not a mathematician, so I am unable to assess M.'s work on its merits. (Although it sounds like none of you can, either.) But I have thought a bit about how this story has developed over the last few years, and I worry that it is more consonant with a hoax than a mathematical discovery of the first rank.

Consider: (1) All the new jargon in the proof; (2) the inability of top scholars to even understand the proof's strategy; (3) the convoluted ways in which M.'s papers refer to each other and to themselves; (4) M.'s unwillingness to explain his work; and (5) the many "meta" comments in the papers (about, e.g., people who examined the proof and found it compelling).

And now (6): We get what the community has long desired–submission of the work for peer review. But of the dozens of respectable journals which M. could have chosen to undertake this task, he chose the one that is HOUSED IN HIS OWN RESEARCH CENTER. Indeed, M. is the journal's EDITOR-IN-CHIEF. This is, of course, unacceptable even in pedestrian circumstances–which these most certainly are not.

I am confident that the truth of the matter will, in time, come out. It may be that M.'s genius is so profound, and so sui generis, that the conventions that apply to everyone else (Tao, Villani, et al.) do not to him. I mean that seriously. I am only suggesting, here, that there are reasonable worries.

9. **ET**
December 17, 2017

This could become very interesting in terms of the sociology of mathematics. What if, purportedly using Mochizuki's work, other well known results/conjectures are re-proved/proved in a manner that is understood? This will still not be an acceptable proof to (most) of the mathematical community but it would raise eyebrows. Take it up one more notch- what if, again purportedly using Mochizucki's methods from his abc work, new conjectures/results are created that can proved or re-proved using other well accepted techniques? There is a possibility that a path to understanding Mochizucki's techniques could be interpreted through its mathematical application.

10. **Warren D. Smith**

December 17, 2017

Re "new," "interesting" and "true" it is possible to have the "true" part decided by an automated proof-verifier system such as MIZAR, removing all doubt, and providing a far higher standard of truth and believability that has occurred over the vast majority of all prior history of mathematics. (Well, you could still worry that the proof-verifier software, or hardware, is buggy, but I nevertheless claim any proof passing MIZAR is probably more rigorous and more valid than essentially anything in all prior mathematical history.) That is the good news. The bad news is, Mochizuki would have to codify his work in the MIZAR language (or other proof-description language, several are available) which is probably an immensely long and hard task. Normally "formal proofs" are somewhere between 5 and 30 times longer than papers giving "informal proofs." It used to be 30 but over time these systems have acquired giant libraries of already-proved lemmas, and by using them your proof usually can be shortened. So if Mochizuki's stuff can be proved informally in 500 pages then the formal proof would be expected to be 2500 to 15000 pages.

One of the very few people who has ever been willing to work that hard has been Thomas Hales, who proved the "Kepler conjecture" that the densest packing of equal balls in 3-space, was the FCC packing (and various co-equally dense ones), and eventually did so "formally."

Formal proofs tend to be great at demonstrating correctness, but horrible at conveying understanding to other humans. In Hales' case the ideas behind his proof are capable of being understood by other humans, and have been, but the full details require very large computations no unaided human could perform. Mochizuki's proof presumably/supposedly is nicer than Hales' in the sense that everything is doable by an unaided human.

11. **Peter Woit**
December 17, 2017

Tom,
I think the "hoax" scenario is highly implausible. To on purpose create a hoax of this kind that would resist experts attempts to see their way through it would be arguably even more difficult than coming up with a plausible proof the author believes. All indications are that there's a well-thought out strategy of a proof here, based on ideas that Mochizuki has been developing, with some success, for a long time. The problem is not that he doesn't explain the strategy of the proof, it is more that it is so complex and uses such unfamiliar ideas that others have great trouble understanding it in the deep sort of way that is needed to be able to convince oneself that the logic is air-tight. I'm sure Mochizuki is convinced he has an air-tight argument, the problem is that other experts need to be convinced, and that isn't happening. Part of the problem here is that some of the usual ways these things get resolved haven't happened due to Mochizuki's unwillingness to travel. For instance, a more usual scenario would be that someone claiming such a proof would accept one or more invitations to speak at a home institution of other experts, then give a series of lectures there where such experts could interact with him, go through the proof at whatever pace was necessary for them to follow it.

As for publishing this in the journal he is editor in chief of, presumably that was done using whatever their standard mechanism is for editors to publish in their own journal, removing themselves from the refereeing process. Not a great idea in this case though, especially since arguably whoever the editor was should have been telling the author the papers needed to be significantly rewritten before they could be checked. Without knowing the details of the refereeing process though, it's hard to know whether it really was problematic.

12. **Peter Woit**
December 17, 2017

ET,

A large part of the problem here is that the methods used to supposedly prove abc have not found other applications. Normally one would expect a new set of methods like this to find several different applications. The most straightforward way to see there is a problem with a theorem is by looking at the things it implies, and finding one known to be not true by other methods. This bypasses the need to check every detail of the argument, you then know that somewhere one such detail is wrong.

13. **Peter Woit**
December 17, 2017

Warren D. Smith,

I suspect the effort needed to rewrite this proof in a machine checkable form is many times larger than what it would take to rewrite it in a form experts can more conventionally check (which would be much more useful anyway).

14. **David Roberts**
December 17, 2017

I think the referees' names should be made public, else, if they are not willing to stand personally and publicly by the acceptance of the papers for publication, they shouldn't be accepted. I feel this is a stronger position that standing up at a conference and saying "I've spent 700 hours studying IUT and I think it's ok". At least two of Wiles' referee's are known, as one of them is on public record as saying how they found the initial mistakes in the proof of FLT.

15. **Tom**
December 17, 2017

Peter,

You say, "Without knowing the details of the refereeing process though, it's hard to know whether it really was problematic." I do not agree.

Let us consider the best-case scenario: M. "received" the paper. He then sent it to two top mathematicians (Lurie, Okounkov). They reviewed the 1,000 pp. (or whatever), and judged everything to be sound. They reported that to M. with their highest recommendation for publication. There's STILL a problem! Why? Reviewers don't accept papers–editors do. And you can't be objective about your own work.

In contrast, suppose we find out that Go Yamashita was one of the referees. Now we have a problem. Unless the rules are different in math, this work can never be published in another peer reviewed journal. For that reason, and because it is impenetrable, it's never going to receive outside scrutiny.

Why is it being handled like this? The whole situation makes no sense. In circumstances like this, it is important to consider the incentives that people face, and to ask whether there are simple explanations.

16. **BCnrd**
December 17, 2017

Tom:

There are standard protocols in place for an editor to be entirely removed from the evaluation process of a paper in which there is a conflict of interest (e.g., the submission doesn't go to that person, and the final decision can be made by some designated set of editors not including the one with the conflict of interest). The editor-in-chief need not have any more or less significant role in the decision process than other editors; their position may be purely for bureaucratic rather than scientific purposes.

I am an editor at a journal for which the editor-in-chief does not get involved in the scientific evaluation or the acceptance process at all; their role is to handle other aspects of the journal's mission. So though it does indeed not look so good, in principle that doesn't mean it must taint the process. However, you do raise the very apt point that the handling editor(s) bear just as much if not more responsibility than the referee(s) for the acceptance of the papers (and in particular for not forcing the author to carry out a substantial rewriting of the work to improve the clarity for experts in arithmetic geometry).

David Roberts:

Your suggestion wouldn't address the core issue that the writing style of the papers (which remains essentially the same as 5 years ago) does not communicate key ideas and insights in a sufficiently understandable manner to the wider community of arithmetic geometry experts. Having referees attest to their confidence in the correctness in a public manner in their role as referees won't help since no explanations of ideas are thereby conveyed. So abandoning the important principle of confidential refereeing would not accomplish anything in this case.

What I wrote just over 2 years ago in paragraphs 5 through 8 of section 6 of my notes on the Oxford workshop at https://mathbabe.org/2015/12/15/notes-on-the-oxford-iut-workshop-by-brian-conrad/ seems as apt today as it did back then.

17. **Perry Metzger**
December 17, 2017

Re: formal verification:

The formalization of the Feit-Thompson Odd Order Theorem in Coq took about six years, though a lot of that was R&D into how to formalize a proof that complicated, something that hadn't been done before.

I would vaguely guess that a formalization in Coq of Mochizui's work by people who understood it and had a reasonable background in formal verification might take a comparable period — the proof is significantly longer, but proof engineering (and that's a real thing) is much better than it was when Gonthier began the Feit-Thompson effort. (This is, of course, only a guess, one only finds out by trying.)

(Note that Mizar is quite old, I don't think I would even attempt to use a system for this that wasn't, like Coq, based in some sort of modern type theory. Coq and Isabelle/HOL are probably the only realistic choices I'd say, though perhaps Lean might be mature enough at this point, I haven't tried using it for anything real.)

18. **David Roberts**
December 17, 2017

BCnrd I would hope that the added profile of the referees' responsibility in asserting correctness would make them extra careful in their assessment. If the referee reports were also public, so we do not have to rely on M's cryptic comments of the form "Corrected slightly erroneous phrasing at the beginning of the second sentence of the statement of Corollary 2.4, (i)" as posted to his website (along such banalities as "Corrected a misprint ("(from above)" —> "[from above]") in Step (x) of the proof of Corollary 3.12″), then we would have a better idea of how they purportedly analysed and "understood" the proof.

I do not think anonymity of referees such a sacred cow that in the case of what could be a result of immense import there is openness in the process. It would give people a mud-map of how others worked through the material if the documents were not secret, and give some idea how to proceed. If the referees are in fact people who have already gone on record as stating they think the theorem is correct, and said people are in M's orbit, then that also gives people an idea of how much to trust the process.

On a different note, I saw recently some updated comments released on M's website that introduced some terminology for something that was an existing concept (a certain topological group being second-countable was given some opaque adjective, like being "Galois", and then every single peculiar object that relied on this was decorated with the adjective). In principle one could go through the background papers are remove all the cruft or just rename objects to something less "creative". Some kind of dependency graph as the Stacks project has for the various theorems/definitions would illustrate what is really needed, and what is not. Crowd-sourcing efforts like these I feel would be more productive than wringing one's hands and saying "it's impenetrable".

19. **BCnrd**
December 17, 2017

David Roberts:

Based on 5 years of experience of talking with very many top professional number theorists, I can say with extremely high confidence that the referees have to be among those in M's orbit: there is nobody else out there who has been willing to say even off the record that they are confident the proof is complete. Indeed, even some of the speakers at the Oxford IUT workshop who have put a lot of effort into the papers remain uncertain if the proof is complete, particularly confused about the crucial 3.12 in IUT3. So although I can understand the perspective from which you hold out hope that identifying the referees may lead to a clarification about the mathematics, I am sorry to say that I am very confident that it will not help at all in the end.

The unusual terminology for existing notions is a distraction, but if that were the primary difficulty then this unusual saga would have been resolved long ago. I'm doubtful that dependency graphs or crowd-sourcing are going to help either. Maybe it's a reflection of my Luddite personality, but I've never understood the purpose of dependency graphs for learning or measuring the complexity of a serious piece of mathematics: the way to learn is to sit down and read and follow back the references to earlier results until one is done (this is how I read EGA, for example: begin at the interesting stuff and go backwards), and to evaluate difficulty one should simply know the ideas and proofs. For example, when I look at dependency graphs in the Stacks Project for results I know well, I find them to be quite irrelevant to measuring difficulty. (To be honest, I don't understand at all what real purpose those are meant to serve in terms of understanding mathematical ideas. I'd love to be enlightened about that.) Likewise, I don't see how crowd-sourcing can be expected to reveal ideas in a high-level paper on arithmetic geometry when experts at the level of Faltings are completely mystified. It is a much harder task than DeepBlue or alphaGo defeating the world master. 🙂

Frank Calegari's blog post from this evening does an excellent job of summarizing the current view of many professional number theorists:

https://galoisrepresentations.wordpress.com/2017/12/17/the-abc-conjecture-has-still-not-been-proved/

20. **Matthew Emerton**
December 17, 2017

Regarding the question of the paper being accepted: In my view, it adds essentially no new information (regarding their correctness) to learn that the papers have been refereed and accepted. Because if the refereeing process was meaningful, then it was undertaken by experts who are known to the number theory community. (I don't mean that the identity of the referees is known; just that the referees had to have come from a known group of experts. There is no outside group of referees who can adjudicate the situation separate from the various experts who have already tried to come to grips with the papers.) And in this case, the number theory community essentially knows where every expert stands on this matter. Most regard the situation as ambiguous at best, and are not confident in the correctness of the papers. Some experts do claim that the papers are correct, and that they understand the mathematics — but they have not been able to explain the papers, or their understanding, to a wider audience of experts, and so this wider audience remains unconvinced. It seems almost a necessary conclusion that the referees came from this second group of experts, given that they did certify the correctness of the papers, and so there is no reason to give their views any more credence just because they are now being expressed a second time in the guise of the opinions of the anonymous referees.

The bottom line: no expert who claims to understand the arguments has succeeded in explaining them to any of the (very many) experts who remain mystified. This situation hasn't changed just because some of the former experts have now weighed in wearing the hat of an anonymous referee.

21. **Lucifer**
December 17, 2017

In this comment I would like to make three somewhat perhaps bizarre observations on the matter at hand. The first is that I think the publication of these papers doesn't matter. The second is that all the fallout from the Mochizuki ABC situation has already happened. The third is that it was unavoidable that these papers would be published.

The publication of these papers doesn't matter as we will just continue exactly as we have with other published papers that contain errors and/or have incomplete proofs. We will continue to try and prove the ABC conjecture exactly as we were doing before the publication of these papers. The results of these papers will not be used more than they already are; in fact I would think, because of the bruhaha involved, there is much less chance of referees overlooking a reference to these papers than there is of referees overlooking a reference to one the (small but nonzero number) of completely erroneous papers in the literature. It is harder still to catch the use of an erroneous lemma in an otherwise completely correct math paper, especially if the authors are famous.

The fallout has already happened, since if we could use the ideas and methods in these papers to successfully prove something as important as ABC, then number theorists and arithmetic algebraic geometers would already have figured out to use it to prove more. In fact, you'd have many people working on refining the method, reformulating it, and improving the precise bounds found by M. Especially recently we have seen that a breakthrough very quickly leads to a much improved exposition as well as much improved bounds. Two examples are: (1) "bounded gaps between primes" proved by Yitang Zhang which led to a very succesful polymath project, and (2) "bounds for cap sets" by Croot-Lev-Pach which led to a paper by Ellenberg-Gijswijt and then via Tao to slice rank, etc, etc. The stereotypical lone math researcher working in an attic still exists, but I would say more and more of the advancement of highly technical fields in mathematics (such as arithmetic algebraic geometry) relies on having larger groups of mathematicians distilling, refining, combining, and adding pieces to construct a larger whole.

It was unavoidable that these papers should be published. In 2012 M convinced himself he had proven the ABC conjecture (as predicted by M several years earlier), after working very hard and long (starting around 2000 or 2002). Of course getting the papers published was then the next logical step. Not getting the papers published would be a public humiliation. I think the outcome we have now is strange but we can deal with it easily and it is best for M. My hope for the future is many more papers of M such as his amazing papers on the Grothendieck conjecture.

22. **Davide Castelvecchi**
December 18, 2017

A colleague of mine who reads Japanese tells me that the article does not explicitly say that the papers have been accepted, only that they are "forecast"（見通し）to be accepted.

23. **Stephane Hubl**
December 18, 2017

I am afraid your posts related to Mochizuki's theory contain many incorrect statements.

I do not have time to go through everything. Here are just few examples.

"while most experts in the field who have looked into this have been unable to understand the proof."

Who do you call an "expert"? Someone who knows her/his narrow area of vast arithmetic geometry or number theory? What makes such a person an "expert" in Mochizuki's theory or anabelian geometry?
Have you read Fesenko's interview to the AMS?

I've talked with several experts, they understand the proof and agree it is correct.
How many experts (who at least know anabelian geometry) have you talked with, to make your statement?

"Looking at these documents, the daunting task facing experts trying to understand and check this proof is quite clear."

The task is daunting if you lack enthusiasm. If you lack enthusiasm, it is better to switch to another area.

Do you expect that the most fundamental achievement in modern mathematics will be simplified in the first years of its existence to the extent that everyone can easily read it?

How many people understood Deligne's proof of GRH in positive characteristic (the Weil conjecture) when it was published?
How many people understood Voevodsky's proof when it was published?

I've heard about several PhD students who have already studied Mochizuki's theory in full. I see that several talks at the two international conferences on IUT were given by students.

"the current situation of understanding of the proof has not changed significantly since last year"

What is the source of your information? Have you talked with several experts?

I've heard that the number of experts is now 13-17.

"A small number of those close to Mochizuki claim to understand the proof"

What do you mean by "close"? There are 5 or more experts outside Japan.

What is "a small number" for you and what do you compare it with?

Are you aware that there are already more (maybe, two times more) people who understand Mochizuki's theory than the number of people who understood Deligne's proof of GRH in positive characteristic or Voevodsky's proof of the Bloch-Kato conjecture at the time of their publication?

"but they have had little success in explaining their understanding to others."

Have you talked with those young researchers who have mastered IUT by learning from the other experts? Do you imply that everyone who has understood Mochizuki's theory should stop doing everything else and dedicate all her/his full time to explaining the theory to others?

Have you read the excellent survey by Mochizuki (2016)?
I've heard about 5-6 reviews of parts of IUT being written for the volume of proceeding of the RIMS workshop on IUT.

"The usual mechanisms by which understanding of new ideas in mathematics gets transmitted to others seem to have failed completely in this case.

What is your evidence? How to you measure the failure? Have you at least talked to people who attended the RIMS workshop on IUT?

I am afraid your post demonstrates a lot of disrespect to the truth, to Mochizuki, and to those people who have worked hard to become experts in Mochizuki's theory

not even wrong

24. **b**
December 18, 2017

Mochizui should just go back to working and not focus on what the occidental community thinks about what he's done. The math is there, and if it is useful it will be used. The subject is too interesting, and a young mind somewhere, not burdened by hardness, will eventually take it up and clarify things for those that seem incapable of working outside of their comfort zone. I think he wastes his time now trying to be a google math translator for intractable, inflexible thinkers.

25. **Kevin Buzzard**
December 18, 2017

Re: formal verification. I would like to try and refute a conjecture made by Perry Metzger above. Metzger says

"I would vaguely guess that a formalization in Coq of Mochizui's work by people who understood it and had a reasonable background in formal verification might take a comparable period" [6 years, the approximate time it took to verify Feit-Thompson in Coq].

Unfortunately the two situations are completely incomparable. The reason Feit-Thompson was verifiable in Coq in finite time was that there is a very well-written two book project (Bender-Glauberman, published 1994, and then Petervalvi, published 2000) which gives all details of the Feit-Thompson proof at a level which is understandable by a graduate student in group theory without any "extra training". In other words, the books are undoubtedly _mathematics_. This part is the job of humans, and humans did it well. Humans "pre-formalised" the proof if you like, making it clear how each step could be checked from the axioms of mathematics without any pretence of requiring 300 hours of training.

Because this had already been done, a team of computer scientists and mathematicians could then go on and really formalise the proof in a computer proof verification system. The paper describing the formalisation of Feit-Thompson in Coq explicitly explains this essential prerequisite on its first page.

Humans have not yet finished the pre-formalisation part of Mochizuki's work. The papers contain claims which many members of the number theory regard as being unclear. A team of computer scientists cannot work with a document which contains assertions for which the only way to unravel them is to ask a small group of people who claim to understand the proof, especially if the response is that the computer scientists should just go away and do 300 hours of training. Because the pre-formalisation is not yet finished, the formalisation can not yet begin, and until the pre-formalisation is finished I think it would be very unwise to speculate about the length of a possible future formalisation project.

I speak as someone who has been formalising his first year mathematics undergraduate lectures in Lean this term, and can verify first hand that whilst learning the language of these systems is a barrier to entry, if you fully and properly understand an argument as a mathematician there seems to be no barrier in formalising the argument in Lean other than the simple fact that it can sometimes take quite a long time to convince a computer that something is "obvious".

26. **Matthew Emerton**
December 18, 2017

I don't want to get into an argument in comments, and so I will most likely not follow-up if there are any responses to this comment. That being said, I would like to address some of the assertions and implications in a couple of the comments above.

The reason for this is that there are many people (including e.g. science journalists, and their lay readers) who are interested in the status of ABC, and who are not in a position to independently evaluate it, and so who are relying on the reports and commentary of those closer to the action to help them in forming their own opinions and making their assessments. Because of this, I think it is important to give some indication of how the situation with ABC is different to that with other previous breakthroughs.

I am not old enough to have seen the reception of Weil I and Weil II first hand, but I don't think there can be any comparison with the reception of IUT. As Persiflage discusses in his blog post, very soon after the Grothendieckian revolution in algebraic geometry began, there were many centres of expertise in the theory, all over the world. Deligne's strategy for proving the Weil conjectures incorporated many known methods, and could be explained in those terms: sheaf-theoretic methods building on the classical technique of Lefschetz pencils, combined with other ideas such as tensor-product amplification, and certain methods of estimate familiar from analytic number theory. The proof could be broken down and explained in these terms (and there is a lovely article of Nick Katz that does this). There is no doubt that the proof was immediately accepted by the community at large, and immediately recognized as a breakthrough.

I was a graduate student when Voevodsky's work first appeared, and saw first-hand the process of it being disseminated in seminars at Harvard. Many people, some junior, some senior, attended these seminars. His methods were almost immediately taken up and further developed by other experts, and there were existing frameworks in which they could be explained and understood: they combined and built upon methods from the theory of algebraic cycles, methods from sheaf-theory and homological algebra, and methods from homotopy theory. Experts from these various areas were quickly able to grasp various of the essential aspects of the arguments, and further develop them. (This is the phenomenon that has *not* happened regarding IUT — the methods have not been understood and taken up by a panoply of other researchers.)

The suggestion that the experts who do not understand IUT are not *true* experts, but are instead hidebound and lazy, and should not have their judgement listened to, is not reasonable. It is close to a no true Scotsman fallacy.

To give another example: when the papers of Wiles and Taylor–Wiles appeared announcing the proof of modularity and FLT, they used methods from the arithmetic of modular forms and the deformation theory of Galois representations. The best-known experts on these methods, who had written papers on these precise topics (besides Taylor and Wiles themselves) were perhaps Gross, Hida, Mazur, and Ribet. Faltings was a celebrated expert in arithmetic geometry and neighbouring topics, but had not written papers that were as close in technique and subject matter to the W-TW arguments as these latter named experts. Nevertheless, he was certainly able to read their papers, understand the arguments, and he famously contributed one of the first simplifications to the crucial TW patching argument.

If the situation with IUT were comparable to the situation with previous breakthroughs, there would be plenty of experts outside the immediate orbit of its author, and outside the specific field of anabelian geometry, who would have successfully engaged with the arguments, and could comment with certainty on various aspects of the proof. This is manifestly not the case, and the situation with IUT is manifestly not comparable to the earlier situations with the Weil conjectures, the Milnor conjecture, or modularity/FLT.

27. **BCnrd**
December 18, 2017

Stephane Hubl:

I too have spoken with some experts who are convinced the proof is complete, and I am sure that they are sincere in their conviction. But I have also spoken with a rather larger number of experts in anabelian geometry as well as some participants and speakers at the RIMS workshop on IUT who have all invested tremendous amounts of time in the study of the IUT papers and remain frustrated by the task of extracting essential ideas from the papers in their current form (though in time some of them are making modest progress in parts). In particular, there are very well-informed speakers from multiple IUT workshops who remain puzzled by key steps. This cannot be disregarded.

The bottom line is that there has been a serious breakdown of the usual modes of communication of mathematical ideas. This has nothing to do with the question of whether or not the IUT papers contain a complete proof or at least the crucial ideas which yield such a proof, and what follows is not focused on that, but rather on the unprecedented communication breakdown that is the primary concern of many. (Just to be clear, I think that Mochizuki is a tremendously talented mathematician, with a remarkable work ethic.) Matt Emerton has addressed this well in his comment above, and I'd like to share some of my own thoughts on this (and then I too will probably bow out from further comments here).

Stories of PhD students whose have "studied Mochizuki's theory in full" cannot be taken to mean (in the absence of more specific information) that they have understood much when it comes to the difficulties that have confronted experts in anabelian geometry. We all know plenty of examples of well-intentioned PhD students who delude themselves into thinking they understand a piece of mathematics much better than they really do. V. Dimitrov was quite up front in his own presentation at an IUT workshop that he was treating the details as a black box while seeking to make some refinement to get a better conclusion. His experience doesn't indicate anything about the status of IUT (as I am sure he would be the first to acknowledge). So likewise even if a PhD student makes some new observation about one facet of things, that fact itself has no bearing on the overall concerns that persist. Moreover, the fact of a student — or anyone! — giving a talk at such a workshop says nothing about the person's grasp of the overall theory or the technical details. It makes good copy for a journalist to write about energetic bright young people making great progress while the experts complain about being confused, but such stories have little to do with reality.

What is making this situation so different from that of other big advances used by many mathematicians who haven't understood the technical details of the proof is that even after many years have passed there still hasn't been the wider grasp of some key new insights and how they plausibly fit together overall to yield a result of the desired type. It is of course a weaker kind of understanding than technical mastery of the entire argument, but is the kind of "reality check" that mathematicians have always used when judging things and it is usually addressed by the Introduction. The reason this has been rather more difficult than is usually the case is likely due to some singular communication aspects (the unconventional writing style and the decision by the creator not to travel widely to give talks about it) that can hopefully be addressed in time as other explanations are created and especially as the ideas within are used to do other things.

The comparison to Deligne's GRH is based on a flawed perception of the situation at that time (and it also not a fair standard anyway, since as Matt Emerton notes that proof was done within a framework that had existed for a decade and had already been worked through in seminars around the world). At the time of Deligne's publication of his proof of GRH in positive characteristic, experts in algebraic geometry from around the world understood the proof right away, certainly far more than at present for IUT. Although some specific details about the proof of the Lefschetz trace formula hadn't yet been published (which apparently delayed Deligne's Fields Medal from 1974 to 1978), those ideas had been disseminated in seminars at a variety of places. And more importantly, the key insights of Deligne's method were rapidly understood and within a few years Deligne had gone much further with his Weil II paper, and progress kept going in those directions (with new applications emerging, etc.).

An ongoing point of frustration, even among many leading experts in anabelian geometry, has been to extract essential new insights from the IUT work, to get a clearer sense of what is making it tick (so to speak). This is of course asking for much less than a complete command of the entire proof. To the extent that doesn't happen and the methods are not used to do other things, the result can be treated as was Hironaka's resolution of singularities. That proof at the time of its publication was fully understood in its details by almost nobody, but (i) the broad outlines of the method were grasped, including key new ideas (such as normal flatness in commutative algebra) that were used to do other things, and (ii) it was written up in an entirely conventional writing style. Together, (i) and (ii) probably explain why the result was quickly accepted as having been proved, even though Hironaka's intricate nested inductive arguments with delicate invariants were too exhausting for most to get through at the time (even defeating energetic brilliant young people such as Deligne). The reliance on resolution of singularities in proofs of other results was tracked similarly to how one tracks reliance on a widely believed conjecture, and in time a small but dedicated group of experts in the details did emerge and gradually streamline and improve the techniques over many years until an understanding was reached that could be more widely disseminated. One can hope that eventually something similar will happen in the present case. In the meantime, I share the sentiment at the end of Lucifer's comment above and I hope that the modes of communication that have broken down in this circumstance can eventually be repaired.

28. **John**
December 18, 2017

Brian, Matt, Kevin, why three of you are not seriously studying IUT? Why did not you attend the Kyoto conference on IUT?

Matt, you are not presenting the history of Voevodsky's proof correctly

Brian, you write, "What is making this situation so different from that of other big advances used by many mathematicians who haven't understood the technical details of the proof is that even after many years have passed there still hasn't been the wider grasp of some key new insights and how they plausibly fit together overall to yield a result of the desired type." – but surely you know many examples of this kind of things, e.g. class field theory was viewed as impenetrable for most number theorists for at least 25 years.

"An ongoing point of frustration, even among many leading experts in anabelian geometry, has been to extract essential new insights from the IUT work, to get a clearer sense of what is making it tick (so to speak)." – who are these "many leading experts"? Do you know if they managed to read the 2016 survey of Mochizuki?

"The bottom line is that there has been a serious breakdown of the usual modes of communication of mathematical ideas." – we live, we learn, we change, we adopt. Number theorists have been tremedously slow in adapting to new things in comparison to almost everyone else. Smart phones did not exist 20 years ago, but various number theorists still think about mathematics as they live 20 years ago. Adopt. Learn. Improve.

29. **Daniel Moskovich**
December 18, 2017

His blog contains personal thoughts and philosophy as opposed to mathematics. I don't think it is informative about the contents of his papers.

He claims that the world needs walls, and hearts that can see through walls. Without walls, it is a Tower of Babel or Russel's Paradox. He feels he needs a wall of privacy between him and many things, including English speakers and the English language. The wall in his Inter-Universal Teichmuller Theory is the Teichmuller symmetry, and the heart that sees through it is the Etale symmetry.

30. **Art**
December 18, 2017

From the Internet Encyclopedia of Philosophy:

No True Scotsman

This error is a kind of Ad Hoc Rescue of one's generalization in which the reasoner re-characterizes the situation solely in order to escape refutation of the generalization.

Example:

Smith: All Scotsmen are loyal and brave.

Jones: But McDougal over there is a Scotsman, and he was arrested by his commanding officer for running from the enemy.

Smith: Well, if that's right, it just shows that McDougal wasn't a TRUE Scotsman.

31. **Daniel**
December 18, 2017

I see many non-mathematician posting a comment here. For those who do not understand mathematical comments posted here, informal Japanese intro to IUT is now available for free at http://live.nicovideo.jp/watch/lv303564022#7:26:33
It is done by Kato @Tokyo Institute of Technology. He states his talk is accessible to high school students. No translation available.

32. **Dale**
December 18, 2017

Yamashita's corresponding theorem is 13.12, not 3.12.

33. **Peter Woit**
December 18, 2017

Dale,

Thanks for pointing that out, fixed.

34. **Anon, bemused**
December 18, 2017

To make a different comparison, for the people who aren't number theorists.

The most spectacular advances in arithmetic geometry since Grothendieck/Deligne are being made *now*, by Scholze (building on foundational work of Fontaine, in collaboration or conversation with many — Fargue, Bhatt, Kedlaya-Liu, …)

In particular, as just one example, we now have a road map for local Langlands (it is geometric Langlands on the 'curve' built out of the period rings for p-adic Hodge theory).

The new techniques that make this possible are explainable at every level, from that of high school student (a bit vaguely, but this is a comment not a blog post: we are able to treat the prime p like a variable x, by also considering all possible p^n'th roots of p), to the professional mathematician (say, the definition of the v-topology, and how passing to the perfection kills Ext groups).

This is all new to all of us, and learning seminars — on Scholze's papers, and on the background — are happening at almost every institution on the planet.

They are difficult papers. It is a joy to read them — the ideas sing, the implications are staggering, and Scholze also writes really well.

Nothing like this has happened with Mochizuki's papers. Not a single new idea has made it out to the rest of us, nor even old ideas (with the one exception of BConrad's rephrasing in one of the blog posts after the Oxford conference).

This is sad for those of us who havent the time to invest in reading his proof — we all *want* his proof to be both correct and new — and annoying as hell for the ones who have tried reading his proof.

35. **David Roberts**
December 18, 2017

[let me just say beforehand that I certainly give place to the expertise of the number theorists here on the status of the proof]

My thoughts about referees should be viewed as more sociological in nature: either we are pleasantly surprised that someone outside the inner circle has apparently understood the work, and we can ask for an explanation in ordinary language of what they think is happening, or all the world knows a handful of junior colleagues at the same university refereed the paper. Experts suspecting who the referees are doesn't make it clear what is going on. I expect it is closer to the latter option…

Regarding dependency graphs etc, I would like to draw a comparison between "just read back through all the references" in this case and another: that of FLT and the perennial question (now settled *in print* by McLarty) of whether the proof used universes. One expert here said several times on MO that they aren't used, because he has read back through all the references from Wiles to EGA. Is it left to the rest of us to take him on faith, or duplicate that effort? Or better, record somehow exactly which bits of thousands of pages of mathematics are or aren't actually used in a given proof. To the case at hand, apparently the full theory of Frobenioids is not used in the IUT papers. Being able to know what to skip from the prerequisite papers would be a head start for someone trying to figure out what is going on. And I would like to know what on earth happened, whether even any of the prerequisite papers are good for something other than IUT. I would ask M's inner circle what problems Frobenioids, for example, are able to solve that one can't do in ordinary Arakelov geometry.

Or, since the number theorists are floating about here, up to which point are people confident in what is going on in M's work? Is the reduction of Szpiro to the special case sound? Is all the stuff with the étale theta function legit? As a category theorist there is simultaneously a huge amount of naïvety in the IUT papers (isomorphism classes of functors? Totally unused constructions with Grothendieck universes? Why??) and interesting results (equivalences of categories of certain topological groups and of arithmetic objects), but I can't tell if these are being used arithmetically in any way beyond "I can reconstruct stuff from data. Tadaa!"

36. **observer**

December 18, 2017

I think that some “amateur mathematicians” here are being misled into a false sense of understanding of how mathematics research works, and what is actually going on in this particular saga. As someone who has been in a position to observe arithmetic geometry research rather closely over the past few years, but who is also blissfully free from the title of “arithmetic geometry expert”, I think I can be a little more blunt.

This is not an issue of mathematicians being too lazy, or too attached to traditional methods of dissemination and communication, or Mochizuki’s proof being too difficult. The community has seen many spectacular and difficult breakthroughs over the past few decades, all of which were accompanied by various signs of legitimacy. One such sign is that the paper’s introduction should be able to summarize a strategy which is both new and at least plausible. Furthermore, as one reads the paper one should have the feeling of building up contentful observations. And those purporting to understand the solution should be able to give talks and answer questions in a way that seems to convey knowledge.

It should be mentioned that Mochizuki’s earlier work was brilliant and difficult, and was absorbed by the usual process of mathematical dissemination. Unfortunately, the paper presently in question fails to exhibit these signs of legitimacy. It introduces an incredible amount of jargon, which is explained in terms of more jargon. The talks “explaining” the paper continue this trend of seemingly empty verbiage. It is unprecedented (for legitimate work) that after dozens hours of talks, and hundreds of “survey” pages, not a single meaningful idea has been absorbed by the wider community of experts. Although people are too tactful to publicly state this, the situation has led to a strong impression that the reason why nothing is being communicated is that there is no real content to communicate.

37. **Bobito**
December 19, 2017

Because there are nonmathematicians, physicists, amateurs and the like reading this blog, it is perhaps worth pointing out that posters like Brian Conrad and Matthew Emerton are very good mathematicians with a lot of specific and relevant technical expertise and who generally speak/write with a lot of care. While arguments based on appeals to authority are anathema to the scientific/mathematical spirit, they are all that is available to the ignorant, and for the ignorant the question is to properly pick the authorities. For me at least, I give a lot of credit to what those two write because when they write about things I do understand, my judgment is that they write interesting things and communicate them well. So I take their opinions on the Mochizuki situation seriously.

38. **atreat**
December 19, 2017

New discoveries in science and mathematics are usually made by standing on the shoulders of giants. By building on what came before and adding a new piece or idea that opens up new vistas to explore. Surely, Mochizuki is similar. His ideas must have started with a kernel of an idea clearly relating to known work. He must have took that first step into the unknown from known territory. Why can he not explain how his thinking evolved from that first step and trace back to the known ideas?

39. **A reader**
December 20, 2017

Thank you all for this very interesting conversation. A viewpoint somewhat relevant to it might be “On proof and progress in mathematics” (http://www.ams.org/journals/bull/1994-30-02/S0273-0979-1994-00502-6/S0273-0979-1994-00502-6.pdf or https://arxiv.org/abs/math/9404236), by William Thurston (Peter Woit had posted a link to it a few years ago).

40. **Davide Castelvecchi**
December 20, 2017

Got an email from the journal PRIMS: “The papers of Prof. Motizuki on inter-universal Teichmuller theory have not yet been accepted in a journal, and so we are sorry but RIMS have no comment on it.”
This seems to implicitly admit that the papers _are_ under review at PRIMS. Otherwise how would the journal feel like it can comment on the status of papers under review somewhere else? Oh, but the editor-in-chief of PRIMS is Mochizuki himself, so perhaps he is making the journal speak for him?

41. **David Roberts**
December 22, 2017

Given PS’ comment and BCnrd’s corroboration, I thank them for being open about this, and retract my previous bluster and confidence. It’s a pity it took this long for the specific sticking point to become public, though.

42. **Matt Grayson**
December 22, 2017

I was very fortunate to take Algebraic Geometry from Hironaka. He presented resolution of singularities, but I completely failed to understand it. He saved my life, however with this comment: ” You are an undergrad. Undergrads know nothing! As a grad student, you will learn one thing – your thesis. Then, as a Professor, you will start to learn Mathematics.”

I can not thank him enough for that perspective.

43. **BCnrd**
December 22, 2017

David Roberts:

Although the concern about the proof of Corollary 3.12 in IUT3 has been known to quite a few number theorists for some time, and in particular had to be known to all near to IUT (e.g., some speakers at IUT workshops are very well aware of this and have been trying to sort it out), there didn’t seem to be a compelling mathematical reason to make this public before now (say by mentioning it somewhere on the Internet). The reason is that the referee process should be permitted to run its course without undue public pressure.

In particular, if the referees had identified this matter as something needing clarification and if it had then been addressed (prior to acceptance) then there would have been no purpose in having made a public statement about the fact that such a non-trivial concern had arisen. (Indeed, the identification of gaps/errors and their correction during the referee process is entirely standard, so it would not be a reasonable policy that such concerns are announced in public when they are found prior to acceptance.) The assumption of many number theorists was that the referees had to be going very carefully through the papers, so they must have highlighted this known point of concern, and hence in due time before acceptance it would somehow be clarified.

But then there emerged the recent (ultimately incorrect) news that the papers were accepted, yet there had been no change in that proof which adequately addresses the concern (there have been some minor changes in the writing in the proof, but not relevant to this issue). That situation, together with the fact that more than 5 years have passed since the original release of the papers, exhausted the patience of many number theorists; the time had arrived to make this concern more widely known. What seems a pity to me is not that it took until now to make this long-standing concern more public (ideally it would have been clarified N years ago and then would never need to have been discussed publicly at all), but rather that after so much time it still has not been adequately addressed even though so many more trivial changes have been made in IUT3.

44. **David Roberts**
December 22, 2017

BCnrd,

Given the speed at which errors were identified on MathOverflow then rectified, imagine if the "mysterious proof" had been publicly identified and all eyes could have been focussed on that for the past n years. Has anybody told Mochizuki directly that 3.12 needs serious attention and that it's not for lack of understanding the previous "trivial" sections? If I was in a position to do so (eg being a number theorist of good standing) I would have done so already.

45. **BCnrd**
December 22, 2017

David Roberts:

I prefer not to get involved in further discussion on the points you are raising; I have said all that I wish to say on these matters in my previous comments here and on Frank Calegari's blog.

46. **TS**
December 23, 2017

Just in case it might help, although probably experts have looked at that too, there has been two review papers in Japanese by Hoshi, and the second one http://www.kurims.kyoto-u.ac.jp/~yuichiro/intro_iut_continued.pdf touches a little bit upon that 3.12 issue.

Namely, at the end of page 92, just before the paragraph 26, it says (thanks to google translate) that "due to the existence of the compatible isomorphism $^{\dagger 0}\mathfrak{R}_{Frob}\xrightarrow{\sim} .^{\ddagger 0}\mathfrak{R}_{Frob}$ the log-volume of the target object $^{\dagger 0}\Theta$ cannot help but be less than $\text{vol}(^{\ddagger 0}\underline{\underline{\Theta}})$, hence the inequality …"

47. **Trent**
December 23, 2017

I'm sure people have extensively tried convincing Mochizuki to do this, but… mentioning anyways since the situation is so perplexing:

Fesenko says: "his theory is so vast that to explain his work, ordinary visits and lectures are not enough. For instance, prior to such lectures, the audience should already have studied the prerequisites, i.e. those 1,000 pages. It simply does not make sense to come to give a one hour or even ten hours of talks on IUT–people won't understand much, I am afraid."

I can understand thinking that eg a world tour of 2hr lectures is useless, but… 10hrs is useless? really?

and even if one accepts that 10hrs is insufficient, 2hr lectures 2x per week for 1.5 months = 24hrs of lectures. I don't care how complex IUT is, I can't imagine 24hrs of lectures at a major research center not having a positive impact on the dissemination of IUT (especially if it is video recorded). 1.5 months isn't that long to be abroad either, especially if it is all spent in one place rather than constantly going through the annoyance of shuttling around the world.

I have absolutely no idea why such a 1.5 month long lecture series (video recorded or not) hasn't occurred.

(The good news is that maybe with the public attention drawn to Corollary 3.12 the mathematical community will be able to confirm/deny the proof without such a lecture series ….. hopefully.)

48. **David Roberts**
December 23, 2017

BCnrd,

fair enough :-). Thanks for all your input both here and there.

49. **sdf**
December 23, 2017

David Roberts makes a good point in one way or another: it would have reflected much more positively on the mathematical community at large considering the media circus all about this work, if we had something to hold up publicly and say "yes well these manuscripts are all good and well, but part XYZ doesn't make sense and must be addressed by the author". Articles like "mathematicians tormented by proof" may have been prevented etc. (although I suppose one shouldn't underestimate mass-media's ability to misunderstand/misrepresent research-level mathematics)

50. **mJ**
December 23, 2017

The answer to that point is Remark 3.12.2 (ii) of newest version of IUT-III

51. **Dale**
December 23, 2017

As I understand it, Corollary 3.12 in IUT 3 ("main inequality") is central to the whole proof of the abc conjecture. Previous sections of IUT (totalling hundreds of pages) just set up all the definitions and trivial proofs needed to prove Corollary 3.12, and when the corollary is proven abc conjecture is pretty trivial to prove based on that corollary. So what is needed to Mochizui to explain more in detail how one arrives in to the "main inequality".

Is this correct understanding?

52. **BCnrd**
December 23, 2017

mJ:

Thank you for your comment. It came to my attention this morning from someone else via email that Remark 3.12.2 has been very much expanded since earlier versions (I don't know when that change occurred), and that this Remark (not just its part (ii)) should address some aspects of how 3.12 follows from 3.11. The wider awareness about this due to the discussion on Frank Calegari's blog and this one has helped in this direction. I immediately brought this to the attention of several other people who have looked a lot into the IUT papers; hopefully it will clarify things. But the coming days are a time of travel and vacation for many people, so don't hold your breath. 🙂

53. **Peter Woit**
December 23, 2017

All,
This blog entry isn't the place for trying to resolve the mathematical questions raised about the proof. I'm completely incompetent to moderate such a discussion and the set of people interested in participating in this on a blog may have zero intersection with the small set of people competent to engage productively in it. Mathoverflow is a site more set up for this sort of thing, it's an interesting question whether it would work over there.

For settling mathematical issues like this one, there really are very few people in the world with the necessary expertise and talent, capable of telling what claims are unproblematic, identifying where possible problems lie. What is supposed to happen is discussion between the author and this small group of experts, until they understand the proof and the problems they find are resolved. This has failed to happen in this case, for a complicated set of reasons, and anyone who feels like it can try and blame participants X, Y, and Z for not doing more. I think the point Brian Conrad is trying to make is that the math community puts the main responsibility in a case like this on the refereeing process: it is the job of editors to pick referees up to the task (an extremely difficult and important one in this case), and it is the job of those referees to do everything they can to ensure proofs are solid and written in an understandable way. For good reasons this process is conventionally done confidentially. Unfortunately, in this case there is evidence of a failed refereeing process (claims the papers have been refereed, while an expert is pointing to problems that have not been addressed, and many experts have trouble with readability of the papers), and confidentiality makes it impossible for other than a small number of people to know what went wrong.

Addendum: I just saw the latest comment indicating that maybe the comment from "mJ" is highly relevant, and possibly it is blog discussions that will end up having a role in moving this process forward. If so, that's a very unusual way (in comparison to private discussions between experts, the author, and referees) for mathematics to get done.

54. **Wyman**
December 23, 2017

Ivan Fesenko has replied to Peter Scholze. It's, uh, interesting.
https://www.facebook.com/ivan.fesenko.37/posts/1128469910617882

55. **anon101**
January 11, 2018

Recently mentioned by @math_jin on twitter is a series of talks at the end of this month at the Southern University of Science and Technology in Shenzhen http://math.sustc.edu.cn/event/10808.html to be given by Fucheng Tan (who apparently works at RIMS http://www.kurims.kyoto-u.ac.jp/en/list/tan.html)

56. **Merle Aucoin**
January 14, 2018

I am no mathematician but active scientific PI and I follow this case since Castelvecchis first Nature article in utter fascination. Please forgive me to express my uneducated 5c.

I feel the situation is slowly approaching a state were expert statements get more and more explicit. In both directions and notably with some quite renowned critics. Lets suppose IUTT I-IV are complete and correct and prove abc. Then already now there is the question: Will Mochizuki ever/in our life times get full credits for his discovery? I fear due to the growing critics(!) it will not be possible anymore. I think the case just transgressed a point of no return for the maths community.

57. **Peter Woit**
January 14, 2018

Merle Aucoin,

I think your perception of "growing criticism" misses what is going on here. What experts are now saying publicly is not different than what they have been saying privately for a long time now. What changed things was the news (the accuracy of which is still unclear) that the journal of Mochizuki's institution, of which he is editor in chief, was about to publish the IUT proof, claiming it as properly refereed and a correct proof. This news made experts who felt that the proof had not been understood and checked to their satisfaction feel that they needed to say something in public, even though this is, for good reason, not the kind of discussion usually held publicly.

Mochizuki is a well-respected mathematician, and skepticism about this proof is not something personal. It's just a fact that the usual way in which understanding of a proof transmits from an author to the rest of the math community has not happened here, and experts are just making clear that fact. There is a lot of attention now being paid to what has gone wrong, including more focus on a specific part of the proof that experts haven't been able to follow. If this gets clarified and experts start to understand better the proof and be able to check that it works, Mochizuki will certainly get credit for proving abc.

58. **Merle Aucoin**
January 15, 2018

Dear Peter, I see. Thank you for clarifying my misperceptions. I like also to draw your attention to the Wikipedia article on Shinichi Mochizuki, it seems there have been substantial changes and additions since the end of last year. In my eyes most likely from a very close proximity of SM if not from himself. Especially the section on IUTT seems notable, since it contains an attempt to explain the gist of it to even laymen:
https://en.wikipedia.org/wiki/Shinichi_Mochizuki .

This Week’s Hype: The Best Explanation for Everything in the Universe

December 22, 2017
Categories: This Week's Hype

Today The Atlantic has, via Quanta Magazine, some unadulterated, pure, grade A hype for the holidays: String Theory: The Best Explanation for Everything in the Universe. In a time when the credibility of science is under attack, does anyone else see a problem with telling the public that the “Best Explanation for Everything in the Universe” that science has is a “theory” for which we have no definition or equations, no experimental evidence, and no likelihood of ever getting any?

Comments

1. **Mitchell Porter**
   December 22, 2017

   Meanwhile, if anyone wants a glimpse of the current status of theory and prediction in string theory, they might want to examine two papers which came out this week:

   https://arxiv.org/abs/1712.06623 … which presents an equation (page 3) for the fivebrane of M-theory

   https://arxiv.org/abs/1712.06894 … which exhibits “a full non-supersymmetric metastable SM-like 4D string model” (page 17)

2. **Peter Woit**
   December 22, 2017

   Mitchell Porter,

   I suppose they could do that, in which case (after a lot of work trying to understand some very complex constructions) they would see that the current status of “M-theory” is just as I have described it.

3. **Ian Lamm**
   December 23, 2017

   Having read that article, I couldn’t help but get a sense of faint – or not so faint – incredulity.

4. **Cédric Bardot**
   December 23, 2017

   To quote and paraphrase Mikaël Atiyah, String Theory is a “rich mathematical story” about the exploration or geometry with tools incepted by theoretical physicists.
   As far as a new paradigm beyond quantum fields, strings and branes have neither helped very much to better understand the detected standard model Higgs and its connection with higher energy or neutrino physics nor provided a helpful dynamical principle to explain the dark sector of the concordance cosmological model. Connes in a recent IHES video praised a famous string theorist (Veneziano) to stay sceptics about all conjectures and speculations but science sociology in France and the USA may be different just as science economies and policies are I guess.
   Of course Connes (and his physicists and cosmologist coworkers) defends his own view on the geometry-physics interaction offering an equation, a dynamical principle and a new model of spacetime expecting particle and astro-physics will probe the quanta of 4d spacetime that are waiting for the young guy or woman who will make the step further to a Matrix Model…

5. **Urs Schreiber**
   December 23, 2017

   Perturbative string theory is perfectly well defined mathematically and has equations. What does not “have equations” is non-perturbative string theory, due to not being constructed yet. Notice that the same is true for non-perturbative QFT, away from toy examples.

6. **Peter Woit**
   December 23, 2017

   Urs,
   You continually post this same absurd crank comment here that QFT and string theory have the same status. It’s a waste of time to try and respond to someone who wants to continually mislead people by insisting that black and white are the same because they’re both shades of gray.

7. **Urs Schreiber**
   December 23, 2017

   This is about a technical fact which may be discussed exhaustively in a matter-of-fact way:

   Given a 2d SCFT of central charge 15 there is the formal power series of n-point functions of this SCFT integrated over the moduli spaces of super-Riemann surfaces. Perturbative string theory is by definition the S-matrix theory obtained by interpreting this power series as a perturbative scattering matrix.

   This may or may not be identifiable with fundamental scattering processes observed in nature, but it’s well-formed as a definition of a perturbative S-matrix theory.

   In fact perturbative QFT has a directly analogous formulation, where the Feynman perturbation series is equivalently rewritten as a formal power series of n-point functions of a 1d field theory on graphs. This insights is called the worldline formalism of pQFT.

8. **Peter Woit**
   December 23, 2017

   Urs,
   This posting is about non-perturbative string theory which is supposed to explain everything about the real world, we know that perturbative string theory explains nothing about the real world.

9. **martibal**
   December 23, 2017

Cédric, could you give the link to this IHES talk ?

10. **Brian White**
December 23, 2017

Peter,

I'd wouldn't expect Natalie Wolchover to hype string theory, and, having read the article, I don't think that she actually did. The title The Atlantic used was hype, certainly, but I doubt very much she had any control over that. The original Quanta title was "Why is M-Theory the Leading Candidate for Theory of Everything?", which, unlike The Atlantic's title, is consistent with the text. That is, it's an account of how string theory came to occupy its current position, not a triumphalist narrative.

Writing about the circumstances that led Stalin to power doesn't in itself imply that you're glad Stalin came to power, and in fact Wolchover is quite careful not to endorse string theory. To quote: "This basic sequence of events has led most experts to consider M-theory the leading TOE candidate, even as its exact definition in a universe like ours remains unknown. Whether the theory is correct is an altogether separate question."

Maybe that's what you meant by blaming "The Atlantic, via Quanta Magazine" for the hype. In any case, agreed, the effect of the article is bizarre. When Wolchover says that there's no empirical evidence for string theory, it's plainly intended as a serious objection. But The Atlantic's slapping on a title like "String Theory: The Best Explanation for Everything in the Universe" implies the objection doesn't matter.

11. **Peter Woit**
December 23, 2017

Brian White,
I suspect this piece was an outgrowth of Wolchover's doing an extensive interview with Witten, and the "experts consider M-theory to be the leading…." story line reflects accurately Witten's point of view. It's one I disagreed with and wrote a book arguing against a long time ago, and I think my side of that argument has held up very well, with nothing positive happening for the other side since (the landscape?), and all the claims about "predictions" for the LHC turning out to be wrong.

I think that in cases like this, where journalists like Wolchover accurately report the point of view of a field's most prominent and distinguished scientists, the hype problem generated is the fault of those scientists, not of the journalists involved. The problems with this article are exactly the same ones that have been in such articles for 30 years now: proponents of string theory try to make their case by going right up to the edge of untruth in a discussion of highly technical issues, journalists try and accurately report what they say, generating a misleading text, headline writers then turn this into outrageous hype. You never hear complaints though from the original scientists, the failure here for them is not a bug but a feature.

In the case of this article I did write in to say something about the problems with the original article in its comment section. Yes, the ridiculous headline in the Atlantic surely was not Wolchover's doing, but there's a long history of exactly this kind of thing happening with headlines of articles like this for a long time, that it would happen here again shouldn't have been even slightly surprising.

12. **JonW**
December 23, 2017

Peter, that doesn't seem a very fair characterisation of the article. It seems to be more about how difficult it is to construct a theory of everything, and why it is that despite not making any predictions that have been (or could ever be) borne out, M-Theory at least makes it out of the gate in a way that other attempts at Theories of Everything don't. The article is very straightforward about the theory's shortcomings.

13. **Peter Woit**
December 23, 2017

JonW,
I don't think I was unfair to the article, which repeats misleading claims about string/M-theory, e.g.:

"M-theory looks like each of the string theories in different physical contexts but does not itself have limits on its regime of validity—a major requirement for the theory of everything."

and

"string theory's status as the only known consistent theory counts as evidence that the theory is correct."

Despite some caveats and careful wording, the text gives the impression that it's describing a consistent viable TOE, when no such thing actually exists. It's not surprising that the Atlantic headline writer read it this way and wrote the headline accordingly.

14. **John Baez**
December 23, 2017

The version of the article that I see now, following [Peter's link](), doesn't have the title Peter said it did. It's not titled "String Theory: The Best Explanation for Everything in the Universe". It's simply "The Best Explanation for Everything in the Universe." Did they change the title?

The picture above the title has a big fat "M" in the middle, and the article says:

> This basic sequence of events has led most experts to consider M-theory the leading TOE candidate, even as its exact definition in a universe like ours remains unknown. Whether the theory is correct is an altogether separate question. The strings it posits—as well as extra, curled-up spatial dimensions that these strings supposedly wiggle around in—are 10 million billion times smaller than experiments like the Large Hadron Collider can resolve. And some macroscopic signatures of the theory that might have been seen, such as cosmic strings and supersymmetry, have not shown up.

So, Wolchover is saying that "most experts" consider M-theory a "leading candidate" for a theory of everything—while not claiming the theory is correct, and admitting the lack of experimental evidence for it, and even noting that its exact definition "in a universe like ours" remains unknown.

The last caveat, which sounds odd at first, harks back to the previous paragraph in which she wrote:

> AdS/CFT gives a complete definition of M-theory for the special case of AdS space-time geometries, which are infused with negative energy that makes them bend in a different way than our universe does.

So, she's pointing out that AdS/CFT doesn't apply to our universe.

In short, this does not seem like outrageous hype to me. Could the article have changed since Peter saw it? Or does he just have a different attitude than I do? (The latter is quite possible: an article I wrote for *Scientific American* also featured in his *This Week's Hype*.)

15. **Cédric Bardot**
December 24, 2017

    @martibal

    "Why Four Dimensions and the Standard Model Coupled to Gravity".(https://m.youtube.com/watch?v=qVqqftQ92kA Peter had already mentioned it in a former post) The last 30 minutes are a rare occasion to watch Connes discussing with physicists (and philosophers) on some of the last results obtained using his spectral paradigm.

16. **Peter Woit**
December 24, 2017

    John Baez.
"String Theory: The Best Explanation for Everything in the Universe – The Atlantic" is the html title of the page, it appears for instance in the tab for the page in my browser.

    The text title and subtitle are
"The Best Explanation for Everything in the Universe

    String theory is considered the leading "theory of everything," but there's still no empirical evidence for it."

    and I think they make very clear the current version of the 30-plus year old string theory hype problem. For many years the hype problem was stories like this
http://www.nytimes.com/2000/04/04/science/physicists-finally-find-a-way-to-test-superstring-theory.html
from the New York Times, which told us that string theory was finally making testable predictions and was about to be tested (at the LHC, by LIGO seeing cosmic strings, etc.). Now that string theory has failed all these tests, the current string theory hype is that this doesn't matter: as the headline says, string theory is the best explanation for everything, even though there's no observational evidence for it (left unsaid is that there are no prospects for such evidence, the theory is permanently immune from confrontation with experiment). Don't you see a huge problem with telling the public this?

    The headline and sub-title is all most people will read, but it's true that the text contains caveats. Some of these caveats are however misleading. What is not made clear is the reason for no experimental evidence: there is no viable theory. Much is made of "consistency" of the theory, but this is misleading: consistency of what? Also a standard misleading phraseology is the caveat about the definition of the theory: "its exact definition in a universe like ours remains unknown". The problem with M-theory is not that it's an approximation, the problem is that it's not a theory. It doesn't make approximate predictions, it makes no predictions.

    Finally, the "most experts consider M-theory the leading TOE candidate" really should be rephrased in a less misleading manner as "most experts think M-theory sucks as a TOE, but that alternatives also suck, and many feel maybe M-theory sucks less".

17. **Frank**
December 24, 2017

    I for one, when I read the Quanta article first, thought maybe someone paid or scared Natalie (who has really good and well known credibility for physics writing), to defend String Theory against all physicists who think it failed 🙂

18. **Anonymous**
December 24, 2017

    I usually prefer not to comment on string theory, so this is a brief exception. Natalie Wolchover (who received some Science Communication Awards, for example the 2017 American Institute of Physics Science Communication Award for Articles, https://www.quantamagazine.org/authors/natalie/ and https://www.aip.org/aip/awards/science-communication/science-communication-award-articles/natalie-wolchover) and Quanta Magazine have both an often well deserved high reputation. In my (albeit anonymous) opinion, and with all the due and well deserved respect for her (previous) news articles and reports, Wolchover's latest article in Quanta Magazine is way below the standard such a high reputation demands. Seriously? After decades of controversy about string theory, Wolchover, as a journalist, should be very much aware of, all she manages is to quote some comments (with all due respect) from a Caltech theoretical physicist (one). Is it enough? I do not think so, if that high reputation is well deserved, both Wolchover's and that of Quanta Magazine. So, please, both, Wolchover and Quanta Magazine, do better next time. Or else, please do not report on string theory and quantum gravity, if you can't do that honestly.

19. **Peter Woit**
December 24, 2017

    Frank,
Unfortunately, as far as I know the business of writing about theoretical physics includes very few opportunities to collect payoffs. To understand that article, note that Wolchover here
https://twitter.com/nattyover/status/943877616217575424
refers to "my interview with Edward Witten, a conversation that felt a bit like talking to God about the secrets of the universe".

    Witten is a genius and an intellectually imposing figure, the article reflects his point of view. There's a good case to be made that he's our best theorist since Einstein, but it's worth remembering that Einstein spent the last part of his life working at the IAS on a TOE that didn't work out. I doubt there were many journalists in the 1950s who managed to get an interview with Einstein, then went down to Princeton to ask him: "Professor Einstein, isn't it true that the TOE you have been working on for the past thirty years has been a complete failure, how do you feel about that?

20. **Philip Gibbs**
December 24, 2017

    The article only claims it is the "best" theory of everything, not a "good" one. The word "best" could even be read as ironic. M-theory's shortcomings are mentioned, e.g. the lack of empirical evidence is in the sub-title. Can anyone name a better theory of everything? What empirical evidence is there for any form of quantum gravity?

    We have seen hype before but I don't see any here. The nearest it comes is "One philosopher has even argued that string theory's status as the only known consistent theory counts as evidence that the theory is correct." This is so loaded with caveats and other devices to weaken its impact that it verges on sarcasm. The positive things the article does say about its mathematical nature seem very balanced to me.

21. **Peter Woit**
December 24, 2017

    Philip Gibbs,

I think I'll read your comment as ironic and sarcastic itself, thus agreeing with you.

22. **Art**
December 24, 2017

Why is The Atlantic publishing a Quanta article (as opposed to Quanta publishing it itself, or Wolchover writing directly for the Atlantic)? Is she under exclusive contract to Quanta? Enquiring minds want to know.

23. **Peter Woit**
December 24, 2017

Art,
From Quanta's website:

"Quanta has partnered with major media outlets such as Wired and The Atlantic to help expand their science coverage. Syndication of Quanta articles has enabled these and several other popular publications — in English, German, Japanese, Spanish and other languages — to provide their readers with excellent coverage of math and science, free from paywalls. Quanta's nonprofit-foundation-funded business model enables it to offer its content widely, at no cost, with only public service in mind."

Part of the deal seems to be that the major media outlets get to put a more exciting clickbaity title on the articles.

24. **Philip Gibbs**
December 24, 2017

Peter,
If I said that string theory must be correct because it is the only known consistent theory, that would be absurd. If I said that string theory's status as the only known consistent theory counts as evidence that the theory is correct, that would be highly debatable and controversial. If I say that one philosopher argued that, then I am merely making the unremarkable observation that a philosopher has said something open to debate. If I say that one philosopher has even argued that … , I am expressing incredulity that a philosopher has said something controversial. Surely that has to be sarcasm? Merry Xmas.

25. **Visitor**
December 24, 2017

Philip Gibbs,
Most people don't read as carefully as you seem to think. They are not looking for sarcasm, irony, nuance, subtlety. They expect a straightforward report about the subject under discussion. And, as far as I'm concerned, rightfully so. An article that needs to be carefully parsed and "deconstructed" to have its message grasped, is more of a word game than an informative explanation or report; i.e. it is a poor article.

26. **NoGo**
December 24, 2017

As a complete non-expert in the relevant things, I take issue only with the following words from the article:
"This basic sequence of events has led most experts to consider M-theory the leading TOE candidate…"
Is it really so? And most experts… on what? String theory? Particle physics? Physics in general?
As a Wikipedia page would say, "quotation needed".

27. **IMHO**
December 24, 2017

Visitor,

Bravo! If we read with great care, parsing every word and phase like scientists are trained to do, we find a technically correct article whose nuanced meanings are arguably uncontroversial.

However, I think we can all agree that the average Atlantic consumer sees a questionable headline that sets a narrative and creates a priori assumptions that aren't necessarily accurate… But this is par for the course for today's click-bait driven media, should we be surprised?

At the end of the day, this double-entendre of an article, while factually accurate when read by many experts, is also misleading to the general public and suggests to a lay reader that M-Theory is well accepted, widely supported, and well on its way to becoming the final theory of everything… And I don't think that's completely true.

This is analogous to reading a tax bill that favors corporations, but then claiming that a title like the Middle Class Jobs Act in no way obfuscates things or influences opinion since a precise technical reading is all that's required to glean the intended message.

28. **Paolo Bertozzini**
December 25, 2017

The official Facebook post of Quanta Magazine linking to the original article is textually as follows:

"Edward Witten's M-theory, which combines all known versions of string theory, is widely seen as the leading candidate for the "theory of everything" because of its demonstrated mathematical consistency in reconciling gravity and quantum mechanics."

Note the stress on "mathematical consistency" … of "M-theory" in reconciling QM and gravity. The author of the article might not be directly responsible for the form of post … but the delibarate intent of "propaganda" is quite clear to me.

29. **Philip Gibbs**
December 26, 2017

Paolo Bertozzini, The only things I find objectionable in that quote are the use to the tired phrase "Theory of Everything" and the sole attribution of M-theory to Witten. It was the culmination of the work of string theorists such as Hull, Townsend and Duff, with Witten being one of the last to concede the important of membranes. Of course, once he accepted it his contributions were phenomenal.

Until someone finds a way to perform quantum gravity experiments with positive results that can tell us something new about quantum gravity, mathematical consistency is all we have, and it is a powerful tool. This is true for any potential theory of quantum gravity. It is a generic problem, not something specific to M-theory. The term "leading candidate" is relative. What alternative can be said to be better? Its shortcomings are clearly mentioned: lack of empirical data, lack of equations,questions unanswered, it might not be correct. The claim in the article is that it is still the leading candidate despite this and that this is nevertheless quite significant. These things do require a very careful reading of the article to tease out its "nuanced" meanings, as someone put it. They are clearly laid out in unambiguous terms. There is no hype or propaganda here.

Sorry if this is getting repetitive.

30. **Peter Woit**
December 26, 2017

Philip Gibbs,
You're just completely ignoring what Paolo Bertozinni wrote. His objection was specifically to the claim of
"demonstrated mathematical consistency in reconciling gravity and quantum mechanics."
for M-theory.

If, as you acknowledge, you have no equations for M-theory, there is no way you can have "demonstrated mathematical consistency" of the theory. All you can have is "conjectured mathematical consistency", which is something very different. Claims of "mathematical consistency" are actually referring to calculations (e.g. in flat 10d spacetime) which are physically inconsistent with the real world. The article is completely misleading because it essentially says that M-theory is a theory of the real world with "demonstrated mathematical consistency". This is an untruth, accurately characterized by Bertozinni as "propaganda".

31. **Philip Gibbs**
December 26, 2017

Peter, I think you are interpreting the words "mathematical consistency" in a much stronger sense than intended in the article. When it talks of "demonstrations of mathematical consistency" it meas that there are some calculations combining quantum theory and gravity that give consistent answers in a non-trivial way. It does not mean that M-theory has been proven to be a fully consistent mathematical theory for quantum gravity. Such strong statements of consistency cannot even be made for QCD.

The article is full of caveats including the fact that its exact definition is unknown. The statements about consistency have to be taken in the context of that information. Without the complete equations you obviously can't have a complete proof of consistency for the calculation of all quantities, in which case that can't be what they mean. When knowledge is incomplete, a consistent theory is one that is not inconsistent based on what is known.

I think given the shortness of the article it does a good job of covering both the positive and negative aspects of the theory in an honest, complete and balanced way. I don't see any signs of propaganda.

32. **Peter Woit**
December 26, 2017

Philip Gibbs,
No one is asking for a "proof", or an "exact theory", an approximate theory understood at the level of QFT would be fine.

"Propaganda" is a good way to characterize claims to have a theory with demonstrated consistency, reconciling gravity and QM, when all you have is some not inconsistent calculations that have nothing to do with the real world. Running this under the title "String Theory: The Best Explanation for Everything in the Universe" takes it from propaganda to outrageous propaganda.

33. **Tim**
December 27, 2017

Just as another datapoint showing that things have gotten completely out of hand, there is currently a display in terminal D of the Philadelphia International Airport entitled "Supersymmetry in Nature" (I'm not kidding, I wish I were). Nothing in the description says anything about why the term supersymmetry is used.

So anyone who has been busy searching for supersymmetry at the LHC … you're just in the wrong place. It's been hiding out at PHL, terminal D, gate 9-11.

I'm sure String Theory can explain why it's there.

34. **Peter Woit**
December 27, 2017

Tim,
Thanks for the news. Looks like this is the explanation

http://www.phl.org/Arts/Pages/Current/AndreaMarquis.aspx

35. **Kea**
December 28, 2017

Tim, I have regularly heard non science people on the street discussing how exciting String Theory is, ever since the big news from the LHC. Nobody seems to have heard about the Standard Model.

36. **Henry McFly**
December 29, 2017

1) no definition or equations
2) no experimental evidence
3) no likelihood of ever getting one

Peter, your statements #1 and #2 are true for string theory, and they were simultaneouly true more than once in the history of particle physics for things other than string theory. I am not sure where you have proved statement #3 for string theory though. That would certainly end this whole discussion.

It's been 22 years since Witten's M-theory unification and 20 years since Maldacena's AdS/CFT correspondence. I think you would also agree that these were pretty big steps which justified pursuing string theory even without any empirical evidence. Now, I am wondering at what point after AdS/CFT do you think string theory became non science? 5 years, or 10 years after AdS/CFT? You started your blog back in 2004, so I guess it was 7 years after AdS/CFT. That sounds like a really short time for someone to be sure that a particular theory is not correct.

37. **Peter Woit**
December 29, 2017

Henry McFly,

The M-theory conjecture does nothing to help with the problems of string theory unification, all it does is open up many more possible ways to produce models. AdS/CFT has nothing at all to do with the problem of string theory unification. You can try and argue that string/M-theory should purely be evaluated as an untestable theory of quantum gravity, and, as with this article, take your stand on "our untestable undefined theory of quantum gravity sucks less than the LQG people's". Up to everyone to evaluate for themselves the pros and cons of this argument, personally I think it's a complete waste of time.

My criticism was aimed at claims made for M-theory as a unification of the SM and gravity. I wouldn't argue that it's non-science, I'd argue that it's bad science. Back in 1984-5 there were good reasons to try out the string theory unification idea, but the problems with it quickly became apparent, as people tried to come up with predictive models. We're now thirty years on from the point it started to become clear this wasn't a predictive unification scheme. I don't think you can point to any analog in scientific history for, after 30 years of failure of an idea, press stories about it being "the best explanation".

As for prospects of testability of string theory unification, with the failure to find "predicted" SUSY or extra dimensions at the LHC, there are now no plausible proposals at all for how to get experimental evidence, and that's what's behind the ridiculous argument being made in this article.

38. **Sandia**
December 29, 2017

Without getting into the String Theory arguments, this article was pretty thin by Quanta Standards, short and nothing really new in it. I suspect it was a piece generated mainly to be distributed in The Atlantic as part of some content requirement agreement. In any case, I think the best way to describe String Theory is that it is a collection of theories of how Planck scale strings might behave, if they existed. The best way to describe M Theory is that it is a theory about what theories of strings have in common. That probably says what needs to be said.

39. **Marshall Eubanks**
December 31, 2017

RE: "It's been … 20 years since Maldacena's AdS/CFT correspondence. I think you would also agree that these were pretty big steps which justified pursuing string theory even without any empirical evidence."

As it happens, there is empirical evidence on this point. It shows that we do not live in an AdS universe, so, no, I at least would not agree to anything of the sort.

Various News

January 9, 2018
Categories: Uncategorized

Now back from vacation in a much warmer location than New York. Some things I noticed while away:

- I see that Paris has a bid to host the 2022 ICM. Everyone should strongly support this, one can't have too many excuses for a trip to that city.
- I'm pleased to see that Sabine Hossenfelder will now be a columnist for Quanta magazine. She's starting off with a piece on asymptotic safety. Also recommended is her new arXiv preprint on fine-tuning and naturalness.
- There's an Indian interview with Nati Seiberg, much of which consists of defending string theory research against the accusation of being a science with no scientific evidence. It's more or less the usual defense that, despite failure as a theory of unification, string theory research has led to other progress in fields such as condensed matter, astrophysics, cosmology and pure mathematics. One problem with this is that it's very unclear now what "string theory research" really is these days, other than a sociological term. About the failure to find SUSY as predicted, he says:

  > One idea is that we will find SUSY particles at the Large Hadron Collider (LHC) but this hasn't happened yet. So given that it hasn't, I think the odds that it would happen in the near future are very small. It's not a likely scenario. If you had asked me 10 or 20 years ago, I would have thought it was quite reasonable that they would find it. But they haven't and that idea did not prove to be right. But people who worked on it should not be penalised for it because they just laid out possibilities and things that experimentalists would like to have.

  I don't think the issue is whether to "penalize" people for working on SUSY, but rather just to acknowledge that the failure to find SUSY at the LHC has important scientific implications, providing significant evidence against heavily promoted speculative ideas about supersymmetric extensions of the SM and superstring unification.

- To keep up on the latest hot trends in particle theory, and sometimes find an excuse for a trip down to Princeton, I periodically take a look at the IAS High Energy Theory Seminar listings (available here). I was surprised to see that this week they've invited Steve Hsu to give a talk about something having nothing to do with HEP theory. His topic is "Genomic Prediction of Complex Traits", a topic motivated by his long-standing interest in finding genetic determinants of intelligence.

  I heard from Hsu back in 2011, when he wrote to ask me if I would publicize this study, which he wrote about here. Hsu was looking for volunteers of "high cognitive ability" (he thought most theoretical physicists would qualify), who would get "free genotyping and tools to explore their genomes". You can read some more debate about this here. A few years back he wrote an essay for Nautilus about how genetic engineering will one day create the smartest humans who have ever lived.

  Until a year or so ago I used to follow Hsu's blog, finally stopped after getting sick of reading his defences of Trump and Steve Bannon. Besides the interest in race determining intelligence (see for instance here), Hsu has what seems to me a disturbing interest in the politics of racial resentment, from the white/East Asian side, which shows up in his fondness for Trump/Bannon. This also shows up in his involvement in a campaign to get Harvard to stop its current affirmative action policies and admit more students of East Asian descent.

  No, I'm not going to allow any arguments over this topic in the comments section. Such arguments immediately descend to an extreme level of stupidity, whatever the IQ of those involved.

Update: I just noticed that Steve Hsu has a blog post about this here, which surely is a better place to discuss what he's up to.

Update: The Economist has an excellent piece about the problems of HEP physics and status of some non-collider experiments looking for BSM physics.

Update: See here for part two of Jerry Alper's report from the event discussed here.

Update: Perhaps the audience at Friday's IAS HEP theory seminar can ask the speaker whether his methods for analyzing people's genetics can be used to tell whether they come from a "shithole country" or not.

Comments

1. **ronab**
   January 9, 2018

   "But people who worked on it should not be penalised for it …"

   However, people who didn't find SUSY and related SM extensions convincing and therefore chose not to work on them were often penalized by not getting jobs. Seems now that a little penalization running the other way would actually be very reasonable.

2. **steve hsu**
   January 9, 2018

   Hi Peter,

   I hope you will come out for the talk on Friday. This area of computational genomics is advancing very fast and although it is not exactly physics it is understandable for most physicists and (at least in my opinion) quite interesting. Re: genomics of intelligence, the effort we started way back in 2010/11 was aimed at finding just a few genetic loci associated with intelligence. The state of the art has progressed significantly since then, with ~1k variants now identified at genome-wide statistical significance. We are on the verge of building crude predictors for adult cognitive ability based on genotype. Predictors already exist for other complex phenotypes like height.

   Re: Trump and Bannon, I've mostly described myself as a centrist Democrat (Bill Clinton and Obama voter, etc.), but I really think Trump (for all his faults) is not getting a fair shake in the mainstream media, on college campuses, etc. It's understandable if we disagree about politics — this is a particularly polarized time in the US.

   Re: Harvard admissions, there is a fair amount of evidence that Harvard and other elite universities are discriminating against Asian-Americans, as with Jews in the early part of the last century. I think that greater transparency (which Harvard has resisted) would help resolve whether discrimination is actually taking place. There are civil lawsuits as well as investigations by the DOJ in progress that will shed significant light on this topic.

3. **Chris**
   January 9, 2018

   Found this interesting statement from Sabine Hossenfelder's blog:

http://backreaction.blogspot.co.at/2018/01/sometimes-i-believe-in-string-theory.html

4. **Peter Woit**
   January 9, 2018

   Hi Steve,
   Yes, about Trump/Bannon I think we're just going to have to disagree. And I'm afraid your enthusiasm for predicting adult "cognitive ability" based on genotype just strikes me as creepy and dangerous. Sorry though, none of this is anything I want to encourage discussion of here (and I just noticed you have a blog entry about the talk, so can host discussion there).

5. **Tim**
   January 9, 2018

   One thing I don't hear much about is string theory's failure as a theory of QCD. I realize no one talks about it now because we have QCD, but it seems to me that we have not one, but TWO data points of string theory failing to be the next big thing.

   I'm sure the subject is complex (and interesting!). What can we learn from the failures and why do they keep happening? The older ones are interesting because despite being potentially less relevant, they might be better understood …

6. **Robert A. Wilson**
   January 10, 2018

   The competing bid for the ICM2022 is from St Petersburg, definitely one of the most beautiful cities in Europe. I for one would much prefer a good excuse to re-visit St Petersburg, over a good excuse to re-visit Paris.

7. **John**
   January 11, 2018

   The economist has a real nice article where you are quoted in it along with Sabine.

   https://www.economist.com/news/science-and-technology/21734379-no-guts-no-glory-fundamental-physics-frustrating-physicists?fsrc=rss

8. **Peter Woit**
   January 11, 2018

   John,

   I added that as an update.

9. **zzz**
   January 11, 2018

   Alper calls him "Wittin" six times. not a good sign.

10. **Low Math, Meekly Interacting**
    January 12, 2018

    I watched the video of the Kavli Conversations event.

    The Teutonic caricature that comes across in the Alper piece is not what I witnessed.

11. **Shantanu**
    January 13, 2018

    Peter: At least Nati has conceded that SUSY will never be found in the near future.
    Some small admittance.

12. **John Baez**
    January 14, 2018

    Someone should tell Jerry Alper that "Ed Wittin" is really "Ed Witten". How embarrassing!

13. **Alex**
    January 14, 2018

    I prefer Saint Petersburg better for 2022 ICM

14. **Andy**
    January 16, 2018

    @John Baez: it seems like point 8 of the famous index you created back in 1998 is in need of an update…

15. **Sebastian Thaler**
    January 17, 2018

    More fun from Sean Carroll:
    https://arxiv.org/abs/1801.05016

Adventures in Fine Hall

January 14, 2018
Categories: Uncategorized

Every so often I get a copy of Princeton's alumni publication in the mail, which I mostly ignore. The latest one however had an entertaining article about the Princeton mathematics department during the 1930s, entitled Adventures in Fine Hall. Various physicists (often misidentified as mathematicians) also make an appearance.

The article is based on an oral history project from the 1980s (Princeton Library website here, archive.org site here). It includes many stories I'd never heard before, including one about Hermann Weyl:

> When attendance at his lectures shrank to three, Weyl threatened to end the course if it shrank further. One day when the third student got sick, the other two students "went out and got one of the janitorial staff to come and sit in the room, so there would be three people in the room and Weyl would give his lecture."

Not quite the same, but this reminds me a bit of a story a Columbia colleague likes to tell about one of Claude Chevalley's calculus classes here at Columbia during the 1950s. Supposedly (accuracy of story not guaranteed) students got together to complain to the chair that they couldn't follow Chevalley's lectures. After someone was dispatched to attend a lecture, and reported back that it was not surprising the students weren't following, a deal was made with the students. Someone else would be found to give them lectures in parallel with Chevalley's, at a different time, as long as they agreed to keep going to Chevalley's lectures. Things are different now, hard to get students to go to one set of calculus lectures, much less two…

For more Princeton math and physics history, the Institute for Advanced Study has its own oral history project (started by Frank Wilczek's wife, Betsy Devine), website here. I don't know if any of those materials are available without going down to Princeton. The IAS has an extensive archive, with a lot of material available online (see for instance here). Poking around I noticed for instance Hermann Weyl's Faculty file (here, here, here and here) and a memo Weyl prepared in 1945 evaluating various physicists and mathematicians as possible hires.

For those interested in IAS history, the archive describes a history of the years 1930-50 there which was commissioned, but not published since Oppenheimer felt it "portrayed the Institute in a less than flattering light." A copy of this document is however now available from the IAS here and here.

Comments

1. **Rex Groves**
   January 16, 2018

   It was always my suspicion in graduate school that George Gamow, Hans Bethe and Eugene Wigner were "second tier":)) Wow! I can't imagine having those kinds of accomplishments and not being considered enough.

2. **liuyao**
   January 16, 2018

   Did Weyl write about Siegel in a separate memo? I was just reading by chance the MathOverflow question https://mathoverflow.net/questions/111519/why-might-andr%C3%A9-weil-have-named-carl-ludwig-siegel-the-greatest-mathematician-of

3. **Kea**
   January 16, 2018

   I imagine the IAS was a very peaceful place to work in those days, since it was still quiet and pleasant when I was there in the 1990s. Was it much smaller?

4. **Peter Woit**
   January 16, 2018

   liuyao,
   It looks to me as if they were planning on offering a position to Siegel, that this list was a "comparison" list, the kind of thing hiring committees put together to justify why the person they have agreed on is the best available.

   Kea,
   Yes, I think in the 3os and 4os the IAS was a much smaller place, even more peaceful than now.

5. **Douglas Natelson**
   January 17, 2018

   Reading that memo, Pauli is used as a comparison benchmark but is apparently out of consideration. Bohr is listed, despite being described as unobtainable. So, had they already asked Pauli and he'd declined? Anyone know the story there?

6. **Peter Woit**
   January 17, 2018

   Douglas Natelson,
   I think Pauli was already at the IAS at this time (he was there 1940-6).

7. **Douglas Natelson**
   January 17, 2018

   Ahh. That'd do it. I didn't remember he'd been there despite reading the recent biography, and I was too lazy to check. Thanks.

8. **Scott Aaronson**
   January 17, 2018

   Weyl's memo is an eye-popping document—possibly the most distinguished group of people in the history of the world, ever to have been straightforwardly ranked and evaluated as potential hires.

9. **David Derbes**
   January 18, 2018

   My friend Bob Jantzen, a relativist at Villanova, spent untold hours turning the typed pages of the Oral History of Mathematics at Princeton in the 1930's into HTML. He did this for free, having been a graduate student of Abe Taub's at Berkeley. If you bump into him at a GR conference, and you like the web site (and how can you not??), you should buy him a glass of a good Italian wine.

10. **Robert T Jantzen**
    January 21, 2018

    As an undergraduate at Princeton in physics (thanks, David!) who took a lot of extra math, I never knew any of this interesting history. Ironically today's math dept at Princeton seems to have no interest in why it became a powerhouse of American mathematics, you will find no link to its history or this website on the department website. Not even the university itself could find a place in its archives to retain the website it held for more than a decade surrounding the original oral history project, a website that I created to give context to that project, through many articles which filled in the details before and after the project. It is filed in web archives so that I cannot update it I am guessing, since I have still not made contact with the chief archivist Dan Linke who has always been very cooperative and who apparently saved the website from oblivion when a reorganization of the university archives orphaned it. He must have posted it in these web archives. Perhaps someone can explain how that site works? I was originally motivated by the fact that only a few paper copies of this oral history project existed unknown to most of the academic community, and the time at Y2K was ripe to transfer it to the internet, which I did as a public service. I emailed the author of the recent PAW article but got no response. I will try again.

The Quantum Spy

January 17, 2018
Categories: Book Reviews

I don't often read spy thrillers, but just finished one, The Quantum Spy, by David Ignatius. Ignatius is a well-known journalist at the Washington Post, specializing in international affairs and the intelligence community (and known to some as The Mainstream Media's Chief Apologist for CIA Crimes). While the book is fiction, it's also clearly closely based on reality. Sometimes writing this sort of "fiction" allows an author to provide their take on aspects of current events that confidentiality prevents them from writing about as "non-fiction". Another example of this kind of writing is that of the now deceased French writer Gérard de Villiers, who wrote a large number of spy novels informed by his connections in the intelligence community. Unlike the often pornographic de Villiers, Ignatius treats the love-making of CIA spies with beautiful Mata-Haris discreetly.

The topic of *The Quantum Spy* is Chinese spying on American research in quantum computing. This is very much in the news these days: after finishing the book I picked up today's paper to read about the arrest of a Chinese-American ex-CIA agent on charges of being a mole spying for the Chinese (a central theme of the Ignatius novel is the divided loyalties of a Chinese-American CIA agent). In the same issue of the paper is a Tom Friedman opinion piece about quantum computing breakthroughs and how "China, the N.S.A., IBM, Intel and Google are now all racing — full of sweat — to build usable quantum systems" that will revolutionize our lives.

I am no expert on quantum computing, but I do have quite a bit of experience with recognizing hype, and the Friedman piece appears to be well-loaded with it. In contrast, at least in describing the the state of technology, the novel does a pretty good job of sticking to reality. Ignatius clearly spent quite a bit of time talking to those very knowledgeable about this. One part of his story is about a company closely based on D-Wave, and he explains that the technology they have is different than the true quantum computer concept that is being pursued by others. Majorana fermions and topologically protected states make an appearance in another part of the story. One character's reading material to orient himself is Scott Aaronson's *Quantum Computing Since Democritus*.

The novel portrays the US and Chinese governments as highly concerned and competitive about quantum computing technology and its security implications. I'd always naively assumed that classified research on quantum computing was carried on just by groups within the NSA or other security agencies, but Ignatius tells a different story. According to him, what happens is that groups performing unclassified government-funded quantum computing research in the open can find themselves forced to "go dark", with their work going forward classified and no longer publicly accessible. His plot revolves around Chinese efforts to get information about such research. I have no idea whether this is complete fantasy or based in the reality of the situation.

In the novel and in real life, there are some analogies between the quantum computing story and the role nuclear physics played in the cold war between the US and Russia. Nuclear and particle physics arguably benefited a great deal for many years from governments worried about trying to get an edge in weapons technology, and to some extent the physics of quantum computing is starting to take on that same role, with (at least in the novel) the Chinese now playing the Russian role.

Just as particle physics likely got a lot of funding and public attention because of nuclear weapons, some parts of physics are now well-funded and high profile because of their connection to quantum computers. In fundamental theoretical physics, the hot topic is the idea that the old dream of replacing space and time with something more "quantum" is going to be realized as "it from qubit", somehow using ideas from quantum computation to get an emergent quantum theory of gravitation. All my attempts to try and understand how this is supposed to work have left me rather mystified. Last week there was a Simons Foundation-funded school in Bariloche about this, with both 't Hooft and Maldacena lecturing on "Black holes and Quantum Information". Perhaps these lectures will be informative and made available. This summer the IAS will host a school on From Qubits to Spacetime, maybe I'll try again then to figure out what is going on by looking at its materials.

Comments

1. **Douglas Natelson**
   January 17, 2018

   So was it a good spy story?

2. **Peter Woit**
   January 17, 2018

   Douglas Natelson,
   I'm the wrong person to ask, the few spy stories I've read have been because of an interest in some non-spy thriller aspect of the story. The spy thriller aspect of this one didn't seem particularly compelling, but what do I know.

   I do go see movies of this kind, and if you wanted to feature physicists in a thriller movie, I think you could make a decent one from this book.

3. **Yatima**
   January 17, 2018

   According to him, what happens is that groups performing unclassified government-funded quantum computing research in the open can find themselves forced to "go dark", with their work going forward classified and no longer publicly accessible.

   Just look for engineering teams (because all of this sounds far more a problem of engineering than theoretical physics) who publish a few very interesting papers then stop publishing. Look for teams in companies that have a good, solid engineering research tradition like IBM, not teams at crazed gluehead outfits like Google. A room full Chinese information retrievers would be able to list the names of Persons Of Interest by the evening.

4. **Anon**
   January 17, 2018

   Better to look here:

   http://www.lps.umd.edu

5. **Peter Woit**
   January 17, 2018

   The "fiction" book does refer specifically to some very real unclassified projects, in particular, to ones being run out of this division at IARPA

   https://www.iarpa.gov/index.php/working-with-iarpa/8-research

6. **Dave Miller**
   January 18, 2018

   The edX online consortium has just started a class by Peter Shor (whom I have seen here occasionally) and his colleague Isaac Chuang, both of MIT, on quantum computation, for anyone interested in pursuing this further.

   Both Shor and Chuang are excellent lecturers; Shor is a major figure in the field. An undergrad physics major should be able to follow the course.

   My own take is that “decoherence,” an interest that Peter Woit and I share, is the killer. The proposed solution is quantum error correction, which the course covers.

   It’s hard for me to see how quantum error-correction can overcome the extremely severe problems due to decoherence, but maybe by the end of the course I will become a believer!

7. **Tim**
   January 18, 2018

   Based on some familiarity with what’s going on in quantum computing, I believe the spying would more likely be the other way around. I believe the Chinese are currently outspending everyone else in this area.

   Canada appears to be more actively pursuing this than the US is.

8. **Art**
   January 18, 2018

   Re “arguably”: What’s the counter-argument? I didn’t realize there was disagreement about the cold war/physics symbiosis.

9. **Steve N.**
   January 20, 2018

   Dear Dr. Woit:
   From your description: “While the book is fiction, it’s also clearly closely based on reality. Sometimes writing this sort of “fiction” allows an author to provide their take on aspects of current events that confidentiality prevents them from writing about as “non-fiction”

   I believe the word for that is: roman à clef

10. **Art**
    January 20, 2018

    Anyone?

    I see there is an interview with Ignatius on the sci-fi podcast Geek’s Guide to the Galaxy, episode 291.

Beyond Falsifiability

January 17, 2018
Categories: Favorite Old Posts, Multiverse Mania

Sean Carroll has a new paper out defending the Multiverse and attacking the naive Popperazi, entitled Beyond Falsifiability: Normal Science in a Multiverse. He also has a Beyond Falsifiability blog post here.

Much of the problem with the paper and blog post is that Carroll is arguing against a straw man, while ignoring the serious arguments about the problems with multiverse research. The only explanation of the views he is arguing against is the following passage:

> a number of highly respected scientists have objected strongly to the idea, in large part due to a conviction that what happens outside the universe we can possibly observe simply shouldn't matter . The job of science, in this view, is to account for what we observe, not to speculate about what we don't. There is a real worry that the multiverse represents imagination allowed to roam unfettered from empirical observation, unable to be tested by conventional means. In its strongest from, the objection argues that the very idea of an unobservable multiverse shouldn't count as science at all, often appealing to Karl Popper's dictum that a theory should be falsifiable to be considered scientific.

The problem here is that none of those references contain anything like the naive argument that if we can't observe something, it "simply shouldn't matter", or one should not speculate about it, or it "shouldn't count as science at all." His reference 7 is to this piece by George Ellis at Inference, which has nothing like such arguments, and no invocation of falsifiability or Popper. Carroll goes on to refer approvingly to a response to Ellis by Daniel Harlow published as a letter to Inference, but ignores Ellis's response, which includes:

> The process of science—exploring cosmology options, including the possible existence or not of a multiverse—is indeed what should happen. The scientific result is that there is no unique observable output predicted in multiverse proposals. This is because, as is often stated by proponents, anything that can happen does happen in most multiverses. Having reached this point, one has to step back and consider the scientific status of claims for their existence. The process of science must include this evaluation as well.

Ellis here is making the central argument that Carroll refuses to acknowledge: the problem with the multiverse is that it's an empty idea, predicting nothing. It is functioning not as what we would like from science, a testable explanation, but as an untestable excuse for not being able to predict anything. In defense of empty multiverse theorizing, Carroll wants to downplay the role of any conventional testability criterion in our understanding of what is science and what isn't. He writes:

> The best reason for classifying the multiverse as a straightforwardly scientific theory is that we don't have any choice. This is the case for any hypothesis that satisfies two criteria:
>
> - It might be true.
> - Whether or not it is true affects how we understand what we observe.

This seems to me an even more problematic and unworkable way of defining science than naive falsifiability. This whole formulation is extremely unclear, but it sounds to me as if various hypotheses about supreme beings and how they operate would by this criterion qualify as science.

Carroll also ignores the arguments made in my letter in the same issue (discussed here), which were specifically aimed at the claims for multiverse science that he is trying to make. According to him, multiverse theory is perfectly conventional science which just happens to be hard to evaluate:

> That, in a nutshell, is the biggest challenge posed by the prospect of the multiverse. It is not that the theory is unscientific, or that it is impossible to evaluate it. It's that evaluating it is *hard*.

The main point I was trying to make in the piece Carroll ignores is that the evaluation problem is not just "hard", but actually impossible, and if one looks into the reason for this, one finds that it's because his term "the theory" has no fixed reference. What "theory" is he talking about? One sort of "theory" he discusses are eternal inflation models of a multiverse in which you will have bubble collisions. Some such models predict observable effects in the CMB. Those are perfectly scientific and easy to evaluate, just wrong (since we see no such thing). Other such models predict no observable effect, those are untestable. "Hardness" has nothing to do with it, the fact that there is some narrow range of models where tests are in principle possible but hard to do is true but irrelevant.

The other actual theory Carroll refers to is the string theory landscape, and there the problem is not that evaluating the theory is "hard", but that you have no theory. As for bubble collisions, you have plenty of conjectural models (i.e. "string vacua") which are perfectly well-defined and scientific, but disagree with experiment so are easily evaluated as wrong. While many other conjectural models are very complex and thus technically "hard" to study, that's not the real problem, and acquiring infinitely powerful computational technique would not help. The real problem is that you don't have a theory: "M-theory" is a word but not an actual theory. The problem is not that it's "hard" to figure out what the measure on the space of string vacua is, but that you don't even know what the space is on which you're looking for a measure. This is not a "hard" question, it's simply a question for which you don't have a theory which gives an answer.

I do hope someday Carroll and other multiverse fans will some day get around to addressing the real arguments being made, perhaps then this subject could move forward from the sorry state it seems to be stuck in.

Update: Philosopher of science Massimo Pigliucci has a very good discussion of this debate at his blog. Sabine Hossenfelder has a piece at 13.7 on Scientific Theory and the Multiverse Madness.

Update: Coel Hellier has a blog posting here taking Carroll's side of the debate.

Update: Yet another new argument for multiverse mania as usual science on the arXiv, this time from Mario Livio and Martin Rees. The same problems with the Carroll article recur here, including the usual refusal to acknowledge that serious counter-arguments exist. They give no references at all to anyone disagreeing with them, instead just knock down the usual straw man, those unknown scientists who think that theorists should only discuss directly observable quantities:

> We have already discussed the first main objection — the sentiment that envisaging causally disconnected, unobservable universes is in conflict with the traditional "scientific method." We have emphasized that modern physics already contains many unobservable domains (e.g., free quarks, interiors of black holes, galaxies beyond the particle horizon). If we had a theory that applied to the ultra-early universe, but gained credibility because it explained, for instance, some features of the microphysical world (the strength of the fundamental forces, the masses of neutrinos, and so forth) we should take seriously its predictions about 'our' Big Bang and the possibility of others.
>
> We are far from having such a theory, but the huge advances already made should give us optimism about new insights in the next few decades.

Livio and Rees do here get to the main point: we don't have a viable scientific theory of a multiverse that would provide an anthropic explanation of the laws of physics. The causes for optimism that they list are the usual ones involving inflationary models that give essentially the same physics in other universes, not the different physics they need for anthropics. There is one exception, a mention of how:

> accelerator experiments can (in principle) generate conditions in which a number of metastable vacuum solutions are possible, thereby testing the premises of the landscape scenario.

They give no reference for this claim and I think it can accurately be described as utter nonsense. It's also (in principle) possible that accelerator experiments will generate conditions in which an angel will pop out of the interaction region bearing the laws of physics written on gold tablets. But utterly implausible speculation with no evidence at all backing it is not science.

The authors note that:

> an anthropic explanation can be refuted, if the actual parameter values are far more 'special" than anthropic constraints require.

The problem with this is that you don't have a theory that gives you a measure on parameter values, so you don't know what is 'special' and what isn't. As I keep pointing out, the fundamental problem here is even more basic that not having a probability measure on possible universes: we have no viable theory of what the space of possible universes is, much less any idea of how to calculate a measure on it. And no, we are not seeing any progress towards finding such a theory, quite the opposite over the past decades.

Truly depressing is that even the best of our journalists see this kind of article, written by two multiverse enthusiasts and giving no references or serious arguments for the other side, as "even-handed".

Update: Two new excellent pieces explaining the problems with the multiverse. Ethan Siegel in particular explains the usually ignored problem that the kind of inflation we have any evidence for doesn't give you different laws of physics, and ends with

> The Multiverse is real, but provides the answer to absolutely nothing.

Sabine Hossenfelder explains four of the arguments generally given for why the Multiverse is science, answering them each in turn, with conclusions:

> **1. It's falsifiable!**
> …
> So don't get fooled by this argument, it's just wrong.
>
> **2. Ok, so it's not falsifiable, but it's sound logic!**
> …
> So don't buy it. Just because they can calculate something doesn't mean they describe nature.
>
> **3. Ok, then. So it's neither falsifiable nor sound logic, but it's still business as usual.**
> …
> So to the extent that it's science as usual you don't need the multiverse.
>
> **4. So what? We'll do it anyway.**
> …
> so you are allowed to believe in it. And that's all fine by me. Believe whatever you want, but don't confuse it with science.

Comments

1. **Azadi**
   January 17, 2018

   Excellent post and analysis, as ever Peter.

   I recall reading an article once by Carroll, where he posited the small value of the cosmological constant as the paradigmatic example of how the "inflationary-string landscape multiverse" model was the only one that could account for the data.

   He stated:

   "…Consider the multiverse. It is often invoked as a potential solution to some of the fine-tuning problems of contemporary cosmology. For example, we believe there is a small but nonzero vacuum energy inherent in empty space itself…The problem for theorists is not that vacuum energy is hard to explain; it's that the predicted value is enormously larger than what we observe. If the universe we see around us is the only one there is, the vacuum energy is a unique constant of nature, and we are faced with the problem of explaining it. If, on the other hand, we live in a multiverse, the vacuum energy could be completely different in different regions, and an explanation suggests itself immediately…"

   To me, this is a very weak argument.

   The logic seems to be: the value of the cosmological constant can't be calculated working from first principles. This indicates there may not be a "natural" mechanism to account for why it takes the value that it does. Likewise, quantum physics provides formulas for calculating what the Higgs mass should be, and the Higgs should be very, very heavy. Only its value is actually light with cancellations down to a very fine degree of precision. "Naturalness" is thus probably out as a solution.

   An "unnatural" explanation for the Higgs mass or cosmological constant is then sought, invoking "anthropic" reasoning. Perhaps the Higgs mass or the lambda value isn't fixed precisely by any underlying theory, but can assume a wide range of values in different regions of the universe – that is, in different bubble universes in a multiverse.

   We live in the life-permitting one, because we're here – a selection effect.

   But for the multiverse to work, you need string theory (which in turn needs SUSY, for most variants of it, I think) or eternal inflation…and you've covered the problems with both of those.

   So in the end, according to Carroll, the "multiverse" is true…just because there's allegedly no other show in town, even though nature – courtesy of the LHC – seems to be telling us otherwise about SUSY/string theory, which is a prerequisite for the landscape.

   BTW am I right in thinking that, on its own, cosmic inflation might result in bubble universes if it is "eternal" (rather than chaotic, which doesn't lead to a multiverse) but those universes or "patches" would all have the same constants without the existence of a string landscape provided by M-Theory (or, rather, M-not-so-Theory)?

   It looks like a house of cards.

2. **Jim Given**

January 17, 2018

Yes, and invoking Popper's "falsifiability" as a criterion for a scientific theory is itself a straw man; product of an overly naive model of how science is done. (If the people you cite want to invoke the philosophy of science, they should discuss a modern, i.e., post 1930's, philosophy of science, such as e.g. Imre Lakatos' formulation of a progressive research program. Wikipedia explains:

"A Lakatosian research programme[16] is based on a hard core of theoretical assumptions that cannot be abandoned or altered without abandoning the programme altogether. More modest and specific theories that are formulated in order to explain evidence that threatens the 'hard core' are termed auxiliary hypotheses. Auxiliary hypotheses are considered expendable by the adherents of the research programme—they may be altered or abandoned as empirical discoveries require in order to 'protect' the 'hard core'. Whereas Popper was generally read as hostile toward such ad hoc theoretical amendments, Lakatos argued that they can be progressive, i.e. productive, when they enhance the programme's explanatory and/or predictive power, and that they are at least permissible until some better system of theories is devised and the research programme is replaced entirely. The difference between a progressive and a degenerative research programme lies, for Lakatos, in whether the recent changes to its auxiliary hypotheses have achieved this greater explanatory/predictive power or whether they have been made simply out of the necessity of offering some response in the face of new and troublesome evidence. A degenerative research programme indicates that a new and more progressive system of theories should be sought to replace the currently prevailing one, but until such a system of theories can be conceived of and agreed upon, abandonment of the current one would only further weaken our explanatory power and was therefore unacceptable for Lakatos. Lakatos's primary example of a research programme that had been successful in its time and then progressively replaced is that founded by Isaac Newton, with his three laws of motion forming the 'hard core'."

In these terms, string theory is a (chronically) degenerative research program. This is an argument that scientists should decide not to devote their efforts to this theory. Lakatos, though, recognized that senior scientists who have devoted many years to such a program will continue to pursue it until they retire. As you have often pointed out abandoning this program would not at all diminish science's explanatory power.

3. **Peter Woit**
   January 17, 2018

   Azadi,
   Yes, you accurately describe the CC argument, which raises a host of other issues (anthropics, naturalness). There are claims that this is "evidence for string theory", basically because you think can produce string vacua with any CC and you assume flat probability distribution. So, "string theory" is playing the same role here as "we have no idea what the CC physics is". I don't believe you can get non-trivial evidence for a theory this way.

   Yes about eternal inflation models based on a single inflaton field. They don't give you the CC argument you want, for that you need the string landscape, and typically think of it as having hundreds of inflaton fields.

   Jim Given,

   Yes, Carroll makes a big deal of claiming that he, unlike other physicists with their naive Popperism, has a serious understanding of modern philosophy of science. For him this doesn't seem to include the progressive/degenerative research program distinction, likely because string theory provides a textbook example of the degenerative case.

4. **Rod Deyo**
   January 17, 2018

   Since Galileo and Newton, there are precious few cases, if any, where philosophy has been a helpful approach to progress in the quantitative experimental sciences. But there have been many examples in the past where it fostered sterile debates over what turned out to be irrelevant philosophical concepts, semantics, and personal beliefs. A recent novel feature in such debates is adding modern Bayesian mysticism to give personal beliefs the illusion of quantitative reasoning.

5. **Bernhard**
   January 18, 2018

   What a confusing article… it is really troubling to see a scientific hypothesis being debated and defended on the grounds of assigning "a prior probability that the theory is true" … It is an impossible evaluation for an extraordinary claim like the multiverse.

   He also states that "it is very hard on the basis of indirect evidence alone to send those credences so close to 0 or 1" for which I don't know which "indirect evidence" he is talking about. Indirect evidence would of course be evidence.

   He finishes with the obvious point that "There really might be a multiverse out there, whether we like it or not ", which is true as far as it goes, although I think he should have added that "there really might be NO multiverse out there" and there is currently no way to know one way or the other (and the reason why the whole discussion is completely empty).

   Depressing!

6. **Peter Woit**
   January 18, 2018

   Rod Deyo,
   Polchinski provided a reductio ad absurdum argument against the Bayesianism business in a paper for the same proceedings as the Carroll one. He calculated a Bayesian probability of "over 99.7%" for string theory, and 94% for the multiverse.

7. **Kyle MacDonald**
   January 18, 2018

   One valuable point in Carroll's blog post, which I didn't know, is how to hear Dawid's phrase "non-empirical theory confirmation". I quote Carroll:

   "It sounds like Dawid is saying that we can confirm theories (in the sense of demonstrating that they are true) without using any empirical data, but he's not saying that at all. Philosophers use "confirmation" in a much weaker sense than that of ordinary language, to refer to any considerations that could increase our credence in a theory. Of course there are some non-empirical ways that our credence in a theory could change; we could suddenly realize that it explains more than we expected, for example. But we can't simply declare a theory to be "correct" on such grounds, nor was Dawid suggesting that we could."

   My guess is that Carroll is still inappropriately cool with untestable theories, and that Dawid is even further gone. However, Carroll's explanation of how Dawid uses the word "confirmation" has made me update away from dismissing them.

8. **Peter Woit**
   January 18, 2018

Kyle MacDonald,

I don’t really want to start a discussion here of Dawid’s work, which I’ve written about extensively, see for instance
http://www.math.columbia.edu/~woit/wordpress/?p=5880
http://www.math.columbia.edu/~woit/wordpress/?p=7005

Carroll’s treatment of criticism of Dawid is a bit like the way he treats those critical of the multiverse: he again ignores their arguments and sets up a straw man to knock down. While those who haven’t read Dawid might be critical because of a misunderstanding over use of the word “confirmation”, many (actually, I think most…) who have read Dawid are critical for serious reasons that Carroll doesn’t address.

9. **TCSF**
January 19, 2018

Bayesianism is not meant to enshrine subjective opinion (to give it a falsely elevated status by quantification). Rather, the Bayesian view recognizes that subjectivity exists, but provides a model for how the beliefs of rational agents should be updated and should converge, as more information becomes available. (I felt a need to add this small comment, but realize a general discussion of the topic might not belong on this forum.)

10. **Jim Holt**
January 19, 2018

Peter, your notion of confirmation is simply too narrow. Since “The multiverse exists” is logically equivalent to “All nonexistent things are non-multiverses,” the existence of the multiverse is confirmed by unicorns, leprechauns, square circles…

11. **pan**
January 21, 2018

Hi Peter,
Apart from the lack of scientific validity or usefulness for the multiverse, one thing that i find disappointing as a non scientist is the intellectual banality of it all, and especially the way it is presented by its supporters. Compared to the radicalism of the ideas within early 20th century physics, the multiverse feels more like cheap sci-fi. A radical idea could be useful in shifting discourse even if it has little scientific merit, but the multiverse doesn’t seem to be anything other than a plot twist to hastily wrap up 20th century physics now that the studio ran out of ideas.

12. **tulpoeid**
January 21, 2018

To the best of my understanding, such arguments run along the lines of Thomas Kuhn’s view of scientific progress (the best candidate theory at any given moment is endorsed by the community, even if it doesn’t fit all facts, and progress is made by building upon it and adjusting as necessary). Only that imho Kuhn described what usually happens, not what _should_ happen. In any case certain individuals should stop calling everyone who simply appeals to logic a Popperazzi. But then again, speaking personally I don’t care, strings and multiverse are unscientific under any definition one picks.

“Scientific validity”, “usefulness” … unfortunately there’s a lot more at stake here. Inflation, string, parallel universes and multiverse fans enjoy repeating how this is the next step in Copernicus’ path, rejoicing in rediscovering ways to keep the human race humble. Only that not being the centre of the universe is totally different from declaring the universe unreachable, and if you bring something so radical on the table then the burden of proof falls on you. (Yes, it’s very similar to the burden of proof for unseen creatures falling on those who believe in them.)

Also, Copernicus’ theory happened to explain previously inexplicable facts in an unambiguous way.

13. **Belmonte**
January 22, 2018

Karl Popper is considered a sort of fallibilist, contrary to popular belief he didn’t hold that non-scientific claims are meaningless, such unfalsifiable claims can often serve important roles in both scientific and philosophical contexts, even if we are incapable of ascertaining their truth or falsity. He maintained that while the particular unfalsified theory we have adopted might be true, we could never know this to be the case.

This fallibilism invites to stick the weak version of Popper’s falsificationist criterion in the sense that falsification provides a methodological distinction based on the role that observation and evidence play in scientific practice. Then, the aim of the partial refutation/improvement of a theory isn’t to highlight its inaccuracy but to point out their faint points, that also means to improve the theory. Einstein’s theory of gravity partially refuted Newton’s theory and therefore improved it. Popper thought that the scientific theory that passed the experimental test is submitted to future refutation and/or improvement. Thus, the term falsifiability is synonymous to testability.

Like Gödel’s theorems of incompleteness, that don’t prevent the creative work of the mathematicians, Popper’s fallibilism doesn’t hinder the imagination of theoretical physicists. Lashing out against the popperazi in order to defend a theoretical position is an approach that has little to do with Popper’s fallibilism.

14. **Paolo Bertozzini**
January 22, 2018

Dear Peter,
I am always amazed at the quite unsophisticated level of discussion of foundations of the scientific method by practicing scientists … probably due to the fact that they (scientists) often, or exclusively, become interested in such issues only when they can become “useful” for certain otherwise undefendable/weak ideological biases and claims … or simply when they do not have any other resources for their arguments 😉
The actual critic to Popper’s “falsificationism” and “demarcation theory” of science has a quite long and respectable history that is systematically ignored by the protagonist of current debates. I have been a very close follower of Paul Feyerabend epistemological anarchism position since I was 14 years old and studied these elementary things in high school (yes, it was usually normal to have some readings on epistemology at the beginning of high school) … and all this noise against “Popperism” looks very much like the rediscovery of the hot water for any student that had a chance to read and study the works of Lakatos, Khun, Feyerabend, etc. The problems with “abduction” and foundations of science by inductive methods have even older roots in Hume. The problematic status of falsifiability of theories/models with free parameters has been repeatedly discussed even in jokes (“with four parameters I can fit an elephant, and with five I can make him wiggle his trunk” – E.Fermi, J.von Neumann, F.Dyson, etc.).
The role of “crucial experimental validations/falsifications” has been repeatedly questioned by P.Feyerabend; similarly the usage of fundamental theoretical principles has been criticized, as for instance in the case of “general covariance” by E.Kretschmann’s argument (variants of this argument can be adapted to show that essentially any “sufficiently complicated theory can be made to satisfy certain abstract requirements and/or empirical observations”).
The fundamental issue here (in my understanding) is not the “multiverse”: theoretical physics in one way or another has always been dealing with the existence of “multiverses”: in the most naive sense, theories do not necessarily specify a unique “history” of the physical system under study; such a system can actually be in many different “states” each of them corresponding to different evolutions (in some cases they do even specify a unique selection criterion for dynamical laws) and the operational specification of the “exact state of the system” might be out of reach even in principle. The quite beautiful popularization book “Farewell to Reality” by Jim

Baggott is one of the rare cases where an attempt is made to identify some of the principles and assumptions "usually" hiding beyond scientific practice: very often a "realist reductionist trinitarian" point of view (Ratzinger's Trinity) manifested in a) the assumption of the existence of some fundamental "ontological truth", b) the belief that such truth influences "empirical reality" and c) a faith in the ability of scientists (humans or otherwise) to "obtain reliable information" on such truth from empirical testing of nature. Such principles are usually complemented by equally essential use of criteria like "Occam's razor" ("simple" theories that "compactify information" are preferred) and the related "Copernicanican symmetry principle" (that is actually one of the strongest motivations for the introduction of multiverses).
The real issue here, I repeat, is that some theoretical physicists are exceptionally becoming interested in epistemology because they cannot find other reasonable ways to defend and justify the support received by some theories, currently undergoing "degenerative involution processes" … theories that they a-priori consider as "truth".
The invocation of "Bayesian abduction" as a substitute for Popper's falsificationism here does not seem to be an improvement, since the specification of "priors Bayesian probabilities" is influenced by "arbitrary sociological" input, that in this case just means that the current "most established" (?) hypothesis and conjectures will decide the output ("The role of priors is crucial" – S.Carroll pg.8).
The general epistemological position of S.Carroll (and somehow His deep religious view of "Science" – "Poetic Naturalism" – and the consequent need to find an new absolute methodological criterion for separating "True Science" from "Non-Science") is quite clear in his recent book "The Big Picture" (but also in some of His technical writings on the "ontological role", against "epistemic role" of the wave-function in QM).
You already discussed in some details some of the underlying problems of such philosophical point of view in a previous post, if I remember correctly.
The panorama of epistemology is way more complex today … and I would personally suggest to those that are not comfortable enough with strong "anti-realist" proposals (P.Feyerabend) to give a closer look at positions like "structural realism" 🙂

15. **bks**
January 22, 2018

https://platofootnote.wordpress.com/2018/01/22/peter-woit-vs-sean-carroll-string-theory-the-multiverse-and-popperazism/

16. **Peter Orland**
January 22, 2018

Judging from the comments above, there is prevalent notion held by both pro-multiverse and anti-multiverse partisans that the the validity of these ideas has something to do with the philosophy of science.

I think the multiverse is nonsense on its face. There is no mathematics in it, and it can't be used to calculate anything. And, although I'm not proud to say it, my knowledge of Popper's work is rock-bottom zero.

17. **Chris Kennedy**
January 23, 2018

To be a true scientist, I think it is certainly helpful if one has the ability to "calculate" but that ability must be accompanied by a certain level of reasoning skills.
When it comes to why one would study "the multiverse" in the first place, I think Thomas Reid, founder of the Scottish school of Common Sense put it best: "If there are certain principles, as I think there are, which the constitution of our nature leads us to believe, and which we are under a necessity to take for granted in the common concerns of life, without being able to give a reason for them — these are what we call the principles of common sense; and what is manifestly contrary to them, is what we call absurd."

18. **Peter Woit**
January 23, 2018

Peter Orland/Chris Kennedy,

I agree that there's a very strong common sense argument here. What's mystifying though is that otherwise very smart and sensible people take this seriously. Why is this? What can be done about it?

For a good example I just noticed this on Twitter

https://twitter.com/preskill/status/955830388584148993

Preskill is an eminently sensible theorist, but even he seems unable/unwilling to recognize the obvious common sense point (which Hossenfelder is making) that the fact that the string landscape/eternal inflation theory predicts nothing at all about anything means that it is not a conventional scientific puzzle but just an excuse for a failed research program. Yes, theorists who use this excuse generally couple their use of it with "dismay", but they still hang on to the excuse as a lifeline.

19. **Lee Smolin**
January 23, 2018

Dear Peter,

In the interest of moving the field forward, let me offer a few comments which I hope everyone can agree with.

-No one, not even Popper, proposes that theories are falsifiable up and down by single experiments. After absorbing Lakatos, Feyerabend and Kuhn, we can agree that what we evaluate are, first of all, research programs and second, competing theories within research programs.

-It is more precise to talk about the falsifiability, not of theories, but of predictions, which may be consequences of one or more theories.

-There are cases where the asymmetry implied by falsifiability holds, and cases where it doesn't. But in all contexts, science proceeds by the testing of testible predictions by experiment.

-Within a fixed research program, theories that imply falsifiable, or at least testible, predictions are generally to be preferred to theories that make no predictions. One reason is that they will, when they succeed, provide tighter and more insightful explanations.

-Research programs that generate theories that imply falsifiable predictions are generally to be preferred to research programs that generate none.

-Some theories involving stochastic evolution of a population on a landscape do generate falsifiable prediction. An example is modern population biology. Indeed, having made a great many falsifiable predictions, which were confirmed, this is now considered established science.

-These predictions were possible because one can deduce features common to all fit individuals, which are then the basis of falsifiable predictions.

-Therefor, it is not impossible that a cosmological theory might be constructed to emulate the success of population biology, and involve stochastic evolution of a population on a landscape, in a way that likewise leads to falsifiable predictions.

-Unfortunately, the multiverse of eternal inflation is not of this type. It fails to imply falsifiable predictions because our universe must be considered a highly atypical member of the ensemble, so there are no common features our universe shares with the typical case, which could provide testible predictions. Instead, there are members of the ensemble consistent with wide ranges of possible values of parameters of effective field theory. (One possible exception is the prediction to have a very small negative curvature, but this is not testible, at least so far).

-One can construct cosmological models which do posit that our universe is a typical member of the ensemble its postulates generate, which do imply falsifiable predictions. An example is cosmological natural selection.

Thanks,

Lee

20. **tulpoeid**
January 24, 2018

“What can be done about it?”

More voices are needed.

It’s not easy, because like in many other instances of destructive human behaviour, those who are fed by it have a lot more motivation to devote energy to it. But the crisis is real and can harm science in very long term.

More voices are needed to tell the public (and, because we’ve reached that point, undergrads) that the big words they hear about have amounted to exactly nothing. And probably more courage to stop treating select colleagues as people who can just go on doing their job like it doesn’t matter.

21. **Peter Woit**
January 24, 2018

Thanks Lee,

I’m becoming more and more convinced that it’s a waste of time to engage in abstract argumentation about this issue, decoupled from discussion of any specific theory/model. I’ll leave it to you to do this for cosmological natural selection models, these are not what the currently vocal multiverse proponents have in mind. What they are arguing for are:

1. eternal inflation models based on single inflaton fields, which they never mention are models that don’t give them what they want: the variety of possible universes needed for anthropics.
2. string landscape models, for which they have no viable theory that can say what the landscape is, much less calculate anything relevant about it.

The bottom line here is that these people are, in a highly misleading way, trying to sell a pseudo-scientific story to the public and their colleagues. This is pseudo-science simply because there is no theory there, nor even a plausible hope of finding one.

By the way, there’s yet more of this that just came out

https://arxiv.org/abs/1801.06944

I’ve appended commentary about this to the blog entry.

22. **Scott**
January 24, 2018

I don’t want to be seen as biased, but is it fair to say that it is mostly theorists that are multiverse proponents? How many experimentalists and observationalists buy into the multiverse? Seems an experimentalist would be highly skeptical, I am, if there is no reasonable way to test it.

23. **Peter Woit**
January 24, 2018

Scott,
Yes, it’s mostly theorists who are the problem here. But, sometimes experimentalists (especially in observational cosmology) do buy into the mania, generally just because it functions well as something to get the public interested in their work. As a random example, look at the last part of this Tedx talk
https://www.youtube.com/watch?v=T22s4jCZ4Ho
Saying that you’re going to find evidence for a multiverse is a lot more exciting than saying you’re looking at dust, no?

A problem here is that experimentalists, knowing less about the details of the speculative models involved, are sometimes less able to distinguish between what Carroll claims is going on here (a serious scientific model that is hard to test) and what really is going on (no viable model).

24. **Chris W.**
January 24, 2018

Scott,
I would think that experimentalists and observationalists would be generally be agnostic, at least publicly, as in: “When you have a suggestion and some guidance for a genuine test that we might conceivably carry out, we’re happy to hear it. Until then, we have plenty of other work to do.”

Of course, after some decades, considerable skepticism about (and loss of interest in) the likelihood of a genuine test ever being proposed can be expected. After all, the *raison d’être* of experiment and observation is the testing of ideas that actually admit tests.

25. **Thomas Schaefer**
January 24, 2018

What on earth is “Consolidation of Fine Tuning”?

According to google, a Templeton funded project at Oxford, http://finetune.physics.ox.ac.uk/

26. **Bernhard**
January 25, 2018

” The causes for optimism that they list are the usual ones involving inflationary models that give essentially the same physics in other universes, not the different physics they need for anthropics.”

Peter

I saw this also in the comment section in your discussion with Coel Hellier. Is it correct to say such models are simple implying that whatever lies beyond the horizon of our universe is probably a region (another universe or whatever the name) where the physical laws remain the ones we know? In other words, isn’t this just the same as saying the universe is infinite but simply calling this a multiverse just to include the region we actually know? It would be helpful if such models would actually not call this a multiverse.

27. **Natalie Wolchover**
January 25, 2018

Hi Peter,
I think the Livio-Rees essay deserves more credit. I appreciated that the essay avoids the usual diatribes about the purpose or rules of science, about which no one will ever agree, and dives head first into the actual issues. Why “evenhanded”? They don’t assert that properties like the value of Λ are fine-tuned, rather that they might be, and they clearly articulate the immense (OK, some would say insurmountable) challenges involved in figuring out whether these properties really are fine-tuned. For instance, this paragraph:

“We are currently far from having any theory that determines the values of Λ or Q or the dark matter density (and we know even less about the relative likelihood of various combinations of these constants or how they might be correlated). Still less do we have a cosmological model that can put a ‘measure’ on the probability density of various combinations. But if we did, we would then have another way of testing — and in principle refuting — whether the ‘fine tuning’ was due to anthropic selection. We could do this by examining whether we live in a ‘typical’ part of the anthropically allowed multiverse, or whether the tuning was even more ‘special’ than anthropic constraints required.”

I know you don’t disagree with any of that, since it’s accurate; I imagine you just don’t like the sense of optimism conveyed by “But if we did, we would then have…” I don’t feel particularly optimistic myself about the future prospects, but whether one does or doesn’t, there’s still a crisis.

Best wishes,
Natalie

28. **Peter Woit**
January 25, 2018

Hi Natalie,

I understand your point that Livio/Rees are careful to say “this is all speculative” and that’s why you think of them as “even-handed”. But whether this is speculative is not really the question (few would describe it as not-speculative, as something with significant evidence). The question is whether it’s legitimate science at all, and they write in their abstract:
“Although the concept of a multiverse is still speculative, we argue that attempts to determine whether it exists constitute a genuinely scientific endeavor.”
coming down strongly on one side of that debate. My comments here were specifically addressed at their argument for this, which I think doesn’t hold up at all, for reasons I explain.

29. **Peter Woit**
January 25, 2018

Bernhard,
There is in principle a distinction between points causally disconnected from us because they’re just too far away, and those causally disconnected because they’re in a different “bubble”. For better or worse, the first kind of points are usually said to be in our universe, the second kind in a different one.

The crucial distinction here is a different one: if you are talking about more than one universe, do your other universes have a sufficient variety of laws of physics to allow for an anthropic explanation of our laws? Refusing to make that distinction, claiming evidence for models of a multiverse with no such variety of laws as evidence for an anthropic multiverse is absolutely standard behavior now from multiverse advocates (with Livio/Rees just another example).

30. **Lee Smolin**
January 25, 2018

Dear Natalie,

I appreciate your point, and I also am glad to see you are joining us who have been saying there is a crisis for some time.

But I would like to point out that there is a theory that “…determines the values of Λ or Q”, by putting “…a ‘measure’ on the probability density of various combinations.”

This theory is cosmological natural selection and, as was discussed in the 1992 paper, and the 1997 book, CNS gives a non anthropic explanation for the values of Lambda and Q. The result is that the anthropic principle is not needed to explain this fine tuning. This works because we live in a typical member of the ensemble, as was discussed in my comment above.

Thanks,

Lee

31. **Peter Woit**
January 25, 2018

I’ve added to the posting links to excellent new discussion of this from Ethan Siegel and Sabine Hossenfelder that have just appeared.

32. **Alan**
January 26, 2018

So what’s Sabine’s solution? Anybody’s solution? 1. No multiverse, 2. I read on her blog Sabine doesn’t like the idea reality is a computer simulation (who does?), 3. Some still believe in a complete consistent theory explaining all reality with equations that can be written on a tee shirt (good luck with that), 4. Then there’s possibly a creator.

The link above to fine tuning is interesting … http://finetune.physics.ox.ac.uk/

Isn't this last the way forward as it's exploring fine tuning tentatively without assuming what any final theory will be? It looks like a heck of a project.

33. **Peter Woit**
January 26, 2018

Alan,
Why does Sabine or anyone other scientist have to provide a solution? Part of science is knowing when you don't know something. The situation here is that we don't know whether there's a multiverse with different physical laws since we have no viable testable theory/model. When science can't provide the answer to a problem, you're welcome to advertise your evidence-free speculation, attribute the answer to God/simulation overlords, or deal with the problem in any other way you feel like. You just can't claim that doing this is doing normal science (as Carroll/Livio/Rees are claiming).

34. **Alan**
January 26, 2018

Peter, I'm not frustrated at anyone at all in particular (honest), it's just that once you rule out a multiverse to explain our particular universe with life in it, I think one has to look at this fine tuning issue very carefully. Maybe investigations need to be then carried out beyond what is called "normal science". Does anybody else think like this?

35. **Peter Woit**
January 26, 2018

Alan,
Not one is ruling out multiverse explanations, they're just noting that we have no evidence for such explanations or any plausible way of getting any. Sure, plenty of people react to this situation by wanting to go beyond "normal science" and pursue for instance metaphysics instead of physics. No reason they shouldn't, but they shouldn't be misleading people by claiming that they're doing the usual sort of science when they're not.

36. **Mitchell Porter**
January 27, 2018

I see people saying "there's a crisis, what can we do about it". I think that's nonsense. The real situation is that there are some things we know about nature, other things we don't; and the human race has more ideas and more data than ever before, about the things we don't yet understand. Not only is there every reason for optimism, there's no shortage of concrete things to work on. It's not like we're lost in space with no clue what to do.

37. **Peter Woit**
January 27, 2018

Mitchell Porter,
I think we don't have much in the way of good ideas (lots of bad ones) about certain fundamental issues in physics, but don't think this is a "crisis". The people heavily invested in SUSY/GUTs/string theory unification do have a "crisis" on their hands as their ideas have clearly failed and the bottom is falling out of their investment. Some of them are attempting to prop up their investment by creating a crisis for physics in general, by trying to change the definition of what successful science is in order to avoid acknowledging failure.

38. **Jim Akerlund**
January 29, 2018

Peter,
This is off topic but does https://arxiv.org/abs/1801.08160 imply the first shovel full of dirt on the coffin that is string theory, and will the string theorists see it as such?

Quick Links

January 27, 2018
Categories: Uncategorized

Various things that may be of interest, ordered from abstract math to concrete physics:

- Jacob Lurie is teaching a course this semester on Categorical Logic. Way back when I was a student at Harvard this is the kind of thing I would have found very exciting, much less convinced of that now.
- Talks from a workshop earlier this month on representation theory are available here.
- The Harvard Gazette has an article about a project to develop a "pictorial mathematical language" first proposed by Arthur Jaffe. The project has a website here. It is being funded by an offshoot of the Templeton Foundation I didn't know about, the Templeton Religion Trust, with one of their grants, TRT0080: "Concerning the Mathematical Nature of the Universe", described as "exploring whether or not the universe admits of a consistent description, or more generally, whether our universe be described by mathematics?". They're advertising a postdoc position here.
- Adam Marsh has a wonderful book on *Mathematics for Physics*, especially from the geometrical point of view, with lots of detailed illustrations. It's available from World Scientific here, or as a website here (there are also articles on the arXiv, here and here).
- There's a new Leinweber Center for Theoretical Physics at the University of Michigan, funded by an $8 million grant from the Leinweber Foundation. Inaugural talk was Arkani-Hamed on The Future of Fundamental Physics.
- Each year recently there has been a Physics of the Universe Summit, described by some as involving "one of those glitterati Hollywood banquets". Some years ago, the glitterati evidently were interested in particle physics, recently instead it is quantum computing and AI (see last year and this year). At this year's glitterati banquet, a kidnapping of Kip Thorne occurred.
- Alex Dragt has a book about Lie methods, with applications to Accelerator physics. If you're looking for detailed very explicit information about symplectic transformations, there is a wealth of such material in this book.
- I'm hearing a rumor (via an anonymous comment here) that HyperKamiokande has been denied funding. Can anyone confirm or deny?

Comments

1. **Chiara**
   January 28, 2018

   The Hyperkamiokande website http://www.hyperk.org/ even has a movie on the project. Will it be able to measure neutrino masses?

   If the project really focuses on testing GUT only, its proponents did themselves a bad favor, now that GUTs seem to contradict experiments. But if Hyperkamiokande could measure neutrino masses, it would be a different thing. That might be the last topic of particle physics about which we know very little. Neutrino masses are often given as 1 eV +/- 1 eV which is really a unsatisfactory state of affairs.

2. **Chiara**
   January 28, 2018

   Update: The other English website of Hyperkamiokande is much more specific on its aims: http://www.hyper-k.org/en/physics.html

   The results would give Japanese physics a huge boost, and put them at the forefront of the particle physics world. Let us hope that the rumors are wrong!

3. **Kiril**
   January 28, 2018

   It would be a big news if HK will not be financed, but I would not know how to interpret it in the grand-scheme of future fundamental research. Such a decision will boost even more Fermilab's plans for DUNE, which will be the only "definitive" neutrino experiment on the market. The ILC is also in bad shape in Japan and I was indeed surprised by their will to go ahead with ILC and HK at the same time..
   Let's wait some informed comment. Meanwhile I'll try to ask to some HK colleagues.

4. **tulpoeid**
   January 28, 2018

   Remember that the T2KK experiment is also being planned, with some redundancy to HK.

5. **Paolo Bertozzini**
   January 28, 2018

   Quoted: "Jacob Lurie is teaching a course this semester on Categorical Logic. Way back when I was a student at Harvard this is the kind of thing I would have found very exciting, much less convinced of that now."

   It would be very interesting to know what are Your doubts, reservations or objections to this topic and the reasons for the change of interest. From what I can see from the web-page, it is essentially a course on "topos theory".

6. **Peter Woit**
   January 29, 2018

   Paolo,
   As a student I was fascinated by trying to learn about more and more abstract mathematical constructs of greater and greater generality, later realized the danger with this is that you end up saying less and less about more and more, in the limit saying nothing about everything.

   Lurie is a great mathematician, I'm curious to see what he does with this material.

7. **Philippe Mermod**
   January 29, 2018

   The truth about HyperK is that the budget for 2018 was postponed, which will delay the project with respect to its original schedule. The same thing happened in the past with SuperK/T2K, it seems to be the way it works in Japan.

8. **Anonymous**
   January 29, 2018

   By the way, who is Adam Marsh?

9. **Neel Krishnaswami**
   January 30, 2018

   Most of categorical logic is no more abstract than the material in a typical undergraduate algebra course (indeed, most of the proofs are easier). It's just that this material is not on the usual undergrad main sequence, due to the sociological/historical accident that mathematicians tend to know very little formal logic (a self-perpetuating accident, for obvious reasons).

   It's a lovely subject, because it is just barely abstract enough to support numerous examples, a fact which is enormously useful to workaday computer scientists like me.

10. **Paolo Bertozzini**
    January 31, 2018

    Dear Peter,
    thanks for the reply.
    I do understand Your point (generalizations often treat more "objects" at the price of "forgetting" some of their structure), but I am not sure that the process of mathematical abstraction consists in just this step. Allow me to make a short comparison between two examples of abstraction: 1) "monoids" generalize "groups" (forgetting the existence of inverses) exactly as You say. In this case the generalization is blind to the "extra structure" available in the more specific case. 2) every group is canonically a "groupoid with only one object" and "groupoids" are a vast generalization of "groups". In this case, what happens is that the *extra structure* necessary to define a groupoid "trivializes" (and becomes essentially invisible) in the case of the more special case of usual groups.
    This example is not only academical: in physics people usually *define* "symmetry" as a group (hence if something is not a group, it is not a symmetry), still groupoids (and more generally categories) capture some significant aspects of the intuitive idea of symmetry. Of course "what we are able to fish … depends on the kind of net that we are allowed to use to fish" and there is the clear danger of saying that "something" is not a fish because it is too small (or too big) to be captured in a specific net 🙂
    Best Regards (and thanks for the always stimulating comments and links).

15 Years of Multiverse Mania

January 29, 2018
Categories: Multiverse Mania

Today is the 15th anniversary of the event that kicked off the Multiverse Mania that continues to this day, recently taking the form of a concerted attack on conventional notions of science. 2018 has seen an acceleration of this attack, with the latest example appearing this weekend.

On January 29, 2003, Kachru, Kallosh, Linde and Trivedi submitted a paper to the arXiv that outlined a construction of a supposed model of a metastable string theory state that had all moduli fixed. Ever since the first explosion of interest in string theory unification in 1984-5, it had been clear that a big problem with using string theory to get anything that looks like known physics was the so-called "moduli problem". If you try and use 10d superstring theory to describe our universe, you need to somehow hide six of the dimensions, and the best way to do that seemed to be to argue that superstring theory implied one could do this by compactifying on an unobservably small approximately Calabi-Yau manifold. Such manifolds however come in families labeled by "moduli" parameters, which can be thought of as describing the size and shape of the Calabi-Yau. These moduli will show up as zero mass fields generating new long-range forces unless some dynamical mechanism could be found to fix their values. It was this that KKLT claimed to have found. I won't even try to describe the complex KKLT proposal, which was aptly described by Lenny Susskind as a "Rube Goldberg mechanism".

What string theorists had been hoping for was a moduli stabilization mechanism that would pick out specific moduli field values, getting rid of the unwanted dozens of new long-range forces and providing a way to make physical predictions. While the KKLT mechanism got rid of the unwanted forces, it had been observed three years earlier by Bousso and Polchinski, working with just parts of the Rube Goldberg mechanism, that this sort of thing led to not one specific value of the moduli fields, but an exponentially large number of possibilities. They had noted that this could allow an anthropic solution to the cosmological constant problem, and the KKLT fixing of all the moduli provided a model that accomplished this (without the long range forces).

KKLT did not mention anthropics and the multiverse, but less than a month later Lenny Susskind published The Anthropic Landscape of String Theory, a call to arms for anthropics and a founding document of Multiverse Mania. He immediately went to work on writing a book-length version of string theory multiverse propaganda aimed at the public, The Cosmic Landscape, which was published in 2005. Less than a month after Susskind's manifesto, Michael Douglas published a statistical analysis of supposed string/M-theory vacua, and at some point the estimated number 10^{500} of vacua started appearing based on this sort of calculation.

I didn't notice KKLT when it appeared, but did notice the Susskind arXiv article. I had just finished writing the first version of my book, and remember that my reaction to the Susskind article was roughly "Wow, if people like Susskind are arguing in effect that you can't predict anything with string theory, that's going to pull the plug on the subject." The book took a while to find a publisher, and by the time it was published I had tacked on a chapter about the multiverse problem. I started this blog in March 2004, and recently looked back at some of the earliest postings, noticing that a huge amount of time was spent arguing with people about KKLT and its implications for the predictivity of string theory. It seemed clear to me from looking at the calculations people were doing that this kind of thing could not ever lead to a prediction of anything. I won't go over those arguments, but claim that my point of view has held up well (no prediction of anything has ever emerged from such calculations, for reasons that are obvious if you start looking at them).

Back in 2003-4 I never would have believed that the subject would end up in the state it finds itself in now. With the LHC results removing the last remaining hope for observational evidence relevant to string theory unification, what we've been seeing the last few years has been a concerted campaign to avoid admitting failure by the destructive tactic of trying to change the usual conception of testable science. Two examples of this from last week were discussed here, and today there's a third effort along the same lines, Quantum Multiverses, by Hartle. Unlike the others, this one includes material on the interpretation of quantum mechanics one may or may not agree with, but of no relevance to the fundamental problem of not having a predictive theory that can be tested.

I'm wasting far too much time discussing the obvious problems with articles like this, to no perceptible effect. Hartle like the others completely ignores the actual arguments against his position (he lists some references. describing them as "known to the author (but not necessarily read carefully by the author)"). In a section on "A FAQ for discussion" we find arguments that include

- The cosmological multiverse is falsifiable, because maybe you'll falsify quantum mechanics.
- The cosmological multiverse is testable: "by experiment if a very large spacetime volume could be prepared with an initial quantum state from which galaxies, stars, life etc would emerge over billions of years." Not surprisingly, no indication is given of how we will produce such a state or any theory that would describe what would happen if we did.
- The theory of evolution is just like the theory of the cosmological multiverse.

Both the absurdity and the danger of this last argument are all too clear.

By the way, for a while earlier this year the arXiv started allowing trackbacks again to this blog, but then this stopped again. The origin of the ban seems to have been in the story described here and my early criticism of the string theory multiverse. I have no idea what their current justification for the ban is.

Update: A good place to look for information about the current state of string landscape calculations is at the website for this workshop. The idea that the problems of this subject can be solved by "modern techniques in data science" seems to me absurd, but for a different point of view, look at the slides of Michael Douglas. For something more sensible, try the talk by Frederik Denef, which describes some of the fundamental intractable problems:

- You don't have a complete theory, with only some non-perturbative corrections known, no systematic understanding of these.
- Dine-Seiberg Problem: When corrections can be computed, they are not important, and when they are important, they cannot be computed.
- Measure Problem: Whenever a landscape measure is strongly predictive, it is wrong, and when it's not, we don't know if it's right.
- Tractability Problem: Whenever a low energy property is selective enough to single out a few vacua, finding these vacua is intractable.

Denef does make some very interesting comments about where modern techniques in data science might actually be useful: dealing not with the landscape of string vacua, but with the huge landscape of string theory papers (e.g. the 15,000 papers that refer to the Maldacena paper). He argues:

> For obvious reasons, besides time constraints, incentives to write papers are much stronger for research scientists than to read them. So printed stacks pile up unread, PDFs remain ambitiously open until we reboot our laptops, recursive reference-backtracking gets sidetracked by the deluge of micro-distractions puncturing our days. This, plus the sheer volume of disorganized pages of important results, leads to loss of access to crucial knowledge, to repeated duplication of efforts, and to many other
> inefficiencies. Worst of all, it becomes increasingly harder for young brilliant minds to stand on the shoulders of giants, and thus to make revolutionary new discoveries. It seems inevitable that we will have to outsource the tedious task of parsing the literature, in search for relevant results, insights, questions and inspiration, to the Machines.

Update: There's an interesting article at Quanta magazine about "Big Bounce" models of the Big Bang, competitors to inflationary models. Paul Steinhardt gives his take on the multiverse: "hogwash."

Comments

1. **comdotcom**
January 30, 2018

this would probably interest you:

https://thewire.in/211357/theres-no-conflict-lack-evidence-string-theory-work-done/

2. **ilovecats**
January 30, 2018

> By the way, for a while earlier this year the arXiv started allowing trackbacks again to this blog, but then this stopped again. The origin of the ban seems to have been in the story described here and my early criticism of the string theory multiverse. I have no idea what their current justification for the ban is.

Hi Peter,

I'm just a simple undergrad, but I just wanted to mention that I appreciate your blog very much. I know a few others who'd agree with me when I say that it's because of you that:

* we learned of folks like Sabine Hossenfelder (who also maintains an awesome blog);

* sighed in relief when we realized that we're not the only ones who found it difficult to read popsci books like the "The Universe in a Nutshell";

* or found awesome books through your reviews (e.g. "Summing It Up"!).

Reading about your spat with Polchinski makes me appreciate your efforts more, because I don't think I quite imagined that you'd be suffering personal attacks because of your commentary (even after you apologize for mistakes on your end!)…

You're a good teacher. Keep writing!

3. **Pascal**
January 30, 2018

A question on the 10^500 vacuaa and the resulting lack of predictive power: why don't people just focus on the (presumably smaller?) corner of this landscape that is consistent with the standard model? Has such a "corner" even been found?

4. **Peter Woit**
January 30, 2018

comdotcom,
Seiberg's defense of string theory at least does not include the multiverse nonsense. I do wish though that sensible theorists like him would publicly address this issue. This attack on science is flourishing because of their silence.

5. **Peter Woit**
January 30, 2018

ilovecats,
Thanks!
The weird thing about the Polchinski story is that it's not really personal (we've never met, he knows nothing much about me). It is just an ideological campaign, with the arXiv trackback business an effort on the part of him and others to suppress disagreement with their ideology. Another aspect of this is what you see in the recent "multiverse is science" articles I've been writing about: refusal to even read the arguments of those who disagree with you, trying to pretend they don't exist. At least Polchinski admits I exist (he was the only well-known string theorist to write a review of my book and Lee Smolin's).

6. **Peter Woit**
January 30, 2018

Pascal,
Among the problems:
1. The "vacua" being studied are only an infinitesimal fraction of possibilities (these days the count of possibilities looked at is not 10^{500} but 10^{272000} and surely there are many more).
2. You can't reliably calculate things for any of these "vacua", i.e. you can't identify which are consistent with the correct value of the electron mass because you can't reliably calculate the electron mass in any of them.

So, you are only looking at some irrelevant corner of the full space of possibilities, and have no way of finding the corner of that corner that corresponds to known physics.

7. **John Fredsted**
January 30, 2018

@Woit: Thanks for that update on the number of vacua: 10^27.000, and counting! A whopping factor of 10^26.500 greater than the previous stupendous estimate of 10^500. Could you perhaps point me to a reference where that new estimate appears?

8. **Peter Woit**
January 30, 2018

John Fredsted,

I don't have time to track down the exact reference for this calculation. It's mentioned here
https://web.northeastern.edu/het/string_data/about/
and I've added some comments about that workshop, which is a good source for the current state of string landscape calculations.

9. **new**
January 30, 2018

Peter, can you clarify some issues on KKLT

Do string cosmologists claim the universe originally started out with 10 flat dimensions of space and 1 dimension of time, then as a result of KKLT, 3 remain large, and 6 curled up as a result of the presence of anti-D3 branes and D-brane instantons or gaugino condensation as outlined in the KKLT paper? What is the origin of highly warped IIB compactifications with nontrivial NS and RR three-form fluxes?

is there a naturalistic mechanism that gives rise to KKLT mechanism, and does this have observational evidence or experimental support? What are the origins of anti-D3 branes and how did they come to be wrapped around the extra 6 dimensions? How did these 6 dimensions acquire D-brane instantons or gaugino condensation?

has science established the existence of any anti-D3 branes, D-brane instantons or gaugino condensation or highly warped IIB compactifications with nontrivial NS and RR three-form fluxes observed either in nature or in experiments as described in the KKLT paper?

10. **A**
January 30, 2018

Pascal, even if one could compute the string vacua, another problem is that the SM is described by about 150 measured digits. So, presumably, among 10^500 vacua, there are 10^350 vacua which look like the SM, just by chance.

11. **Jan Dybicz**
January 30, 2018

Peter, the link you provided has the exponent at 272,000, not 27,000.

12. **Peter Woit**
January 30, 2018

new,
KKLT is purely a construction of a metastable ground state with stabilized moduli. There's no theory of initial conditions, no evidence (or even complete theory) of the various elements that make up the KKLT Rube Goldberg machine. This is why the main activity of string landscapeologists is just counting these things.

13. **Peter Woit**
January 30, 2018

Jan Dybicz,
Thanks, fixed! Easy to make that kind of mistake by a factor of $10^{271,173}$.

14. **Balazs Vagvolgyi**
January 30, 2018

On Michael Douglas' slides, he claims about string theory that:

"… it is not hard to find solutions … for which the effective 4d physics at low energies is the Standard Model coupled to gravity …"
"… Only a minority of [the large number of solutions] lead to the Standard Model field content, and those which do lead to a range of values for the cosmological constant, the particle masses and the other fundamental constants …"

This implies that it's so easy to find suitable solutions, that string theorists already have a bunch of compactifications (solutions), that provide SM+GR, except for the values of the fundamental constants. So now we only have to find just the right compactification for our set of constants.

Is this correct?

15. **Peter Woit**
January 30, 2018

Balazs Vagvolgyi,
What Douglas doesn't explain when he says these constructions "lead to a range of values for …" is that you can't actually reliably or accurately compute any of these values. All you have is arguments that there is no known reason they can't be the SM ones. Note that Denef is Douglas's long-time collaborator on these calculations, and his description of two aspects of the issue is "When corrections can be computed, they are not important, and when they are important, they cannot be computed." and "Whenever a low energy property is selective enough to single out a few vacua, finding these vacua is intractable."

So, the situation is that, after you specify some discrete data like the number of generations, you still have an exponentially large number of possibilities, with no way of determining which agree with the SM. There is no way of getting any sort of prediction of anything out of this. Claims made early on (and debated on my blog and others) that maybe some kind of prediction could be made have failed completely.

16. **Anonyrat**
January 30, 2018

a. Seems like a metaphor modernization replacing 'needle in a haystack' by 'vacuum state in a landscape' is appropriate.

b. Mining a pile of documents (not of string theory documents) is a proposed project at where I am. IBM Watson is the suggested tool.

17. **David Levitt**
January 30, 2018

Peter,
I have been following your forum almost from the beginning. Your statement that "…my point of view has held up well" is understated. It is clear now that you were right and your vocal and, sometimes, vicious critics were wrong. This forum provides an incredibly valuable means for the non-specialist physicist to evaluate these HEP theories. You provide unlimited opportunities for other specialists to rebut your arguments – and when they can't, or don't, it is very strong evidence that you are right. I grew up in the 60's reading Scientific American in awe of the brilliant physicists and their wonderful new discoveries. And now, you have shouted out that the current batch of HEP geniuses have no clothes. After a long career as a biophysicist I have developed a more jaded view of these researchers. Although they are obviously highly skilled technicians, this does not imply that they understand or care about the large picture implications of their research. I have two questions that are a little off subject. 1) Why do you think this is a recent HEP problem? Is it just the result of the lack of progress in the field, or has the character of the people attracted to the subject changed from the halcyon days of my youth. If the former, then it would imply that my youthful heroes had the same weaknesses as the current theorists and they were just lucky that there were some new experimental results that needed explaining. 2) Now that you have been essentially proven correct, what sort of response do you get from your colleagues at meetings, especially your former critics? Are they willing to admit their error?

18. **Nonsense**

January 30, 2018

”dealing not with the landscape of string vacua, but with the huge landscape of string theory papers (e.g. the 15,000 papers that refer to the Maldacena paper)”

Google Scholars indicates 16,188 citations for that paper.

19. **Peter Woit**
January 30, 2018

David Levitt,
1) I don’t think earlier generations of theorists were any different. They had the huge advantage of continual new hints and checks on their conjectures from experiment. What we’ve seen over the last 30 years or so is what can go wrong when that is removed.
2)Despite what you might think from blogosphere discusssion, I’ve always gotten along well with string theorists I know, and our views are more similar than you might think, with the main difference just our evaluation of exactly how bad the prospects for string unification are. Over the years many have told me they read my blog and agree with much of what I have to say. Most string theorists don’t believe the landscape stuff is worthwhile, and have voted with their feet and are working on things unrelated to string unification.

No, I have yet to meet a theorist willing to say they were wrong (about string theory or almost anything…). The ones most unhappy with my criticisms have uniformly dealt with this by trying to ignore me as best they can, and continue to do so.

20. **F. G.**
January 30, 2018

So? When do you think we can expect the first papers on the ‘string publication landscape’ ?

21. **Louis Wilbur**
January 30, 2018

Peter, is it really the case that the KKLT 2003 paper achieved such stabilization? As I understand it, that paper is for type IIB string theory and the more recent paper 1709.03554 suggests, if I have read it correctly, that even meta-stability is not possible. I am not an expert in this area so perhaps I have misread it but I would definitely like your take on these papers since they all seem to suggest serious problems with stabilization.

Supersymmetry Breaking by Fluxes
https://arxiv.org/abs/1709.03554

No inflation in type IIA strings on rigid CY spaces
https://arxiv.org/abs/1703.08993

On classical de Sitter and Minkowski solutions with intersecting branes
https://arxiv.org/abs/1710.08886

The Swampland Conjecture and F-term Axion Monodromy Inflation
https://arxiv.org/abs/1703.05776

Also, it seems that there are various no go theorems for positive vacuum energy in string theory. My understanding is that this has the potential to falsify string theory since dark energy is known to be positive. If it is not possible to have anything like cosmic inflation in string theory then string theorists need to move away from the inflationary paradigm to, say, string gas cosmology and then try to deal with dark energy in a completely different way. The below papers seem to say that there are serious problems with positive vacuum energy in string theory and they are definitely relevant to your blog.

Refining the boundaries of the classical de Sitter landscape, Nogo theorems
http://arxiv.org/abs/1609.00385

Revisiting constraints on uplifts to de Sitter vacua
http://arxiv.org/abs/1607.01139

Constraints on Dbar Uplifts
https://arxiv.org/abs/1605.06456

Loop corrections to the antibrane potential
https://arxiv.org/abs/1602.05959

A no-go theorem for monodromy inflation
https://arxiv.org/abs/1510.02005

Constraining de Sitter Space in String Theory
https://arxiv.org/abs/1504.00056

If the very early universe did have a strong but short lived vacuum energy then it has to be the case that it is just as mysterious as dark energy. In other words, very little is known about it except that it has negative pressure, positive energy, and a few other things. But it is a big leap from that to other universes. If there are other universes then the only way to have no measure problem is for the number of such universes to be finite and each universe itself is finite in terms of all of its parameters. One problem with an infinite number of universes is well known by mathematicians since the the time of Cantor: one can pair up the universes to each other in any way one wants and therefore get any measure that one wants. Questions like what percent of the universes contain life are just as meaningless as asking what percent of the positive integers are odd. The percentage can be anything one wants depending upon how the universes or positive integers are bijected with one another. Thank you for the interesting post.

22. **Mitchell Porter**
January 31, 2018

Peter Woit said

“you can’t identify which are consistent with the correct value of the electron mass because you can’t reliably calculate the electron mass in any of them”

A paper yesterday offers a glimpse of what remains to be done, in order to calculate such quantities:

http://arxiv.org/abs/1801.09645

In the standard model and also in string theory, masses of elementary fermions come from yukawa couplings to the Higgs field. The introduction to this paper tells us that in string theory, there are three steps to calculating the yukawa coupling, and that the authors report progress with the second step.

23. **S**
January 31, 2018

Surely one difference between theorists now and fifty years ago is the speed with which they rush to the popular press? Few theorists did that with a new, half-baked theory back then (or am I wrong?). They might have had more experimental evidence, but that would have provided *more* justification for rushing out books, not less, and yet they rarely did so it seems.

24. **simplicio**
January 31, 2018

@S
There's a fair amount of pretty speculative science in old newspapers and magazine articles. I don't think the public appetite for these sort of thing, or Science's willingness to provide it has really changed in the last five decades. The difference is that the absence of new results means the *same* speculative ideas can basically linger for years, never being proven or disproven, making them seem less like speculative ideas and more like well supported science.

25. **Peter Woit**
January 31, 2018

Louis Wilber,
There has always been debate about whether the KKLT Rube Goldberg machine really works or not. But this really doesn't matter, the conclusion that there is no scientific theory of a multiverse is the same either way. Either the theory is radically inconsistent with observation (unstabilized moduli) or completely empty and consistent with anything (KKLT-stabilized moduli).

Mitchell Porter,
The technical problem (lack of explicit Calabi-Yau metric) they are discussing goes back to the beginnings of the subject, and they seem to me to make little progress in dealing with it. In any case this technical problem is not at all the main problem. Note that they just ignore the moduli stabilization problem completely. They don't even get to the point of introducing the pieces of the KKLT machine.

S,
A counter-example is the story of Schrödinger in 1947. Quoting a random source about this
(https://www.salon.com/2015/04/18/albert_is_an_old_fool_einstein_vs_schrodinger_in_battle_of_the_nobel_laureates/)
"The leading announcer was the Irish Press, from which the international community learned about Schrödinger's challenge. Schrödinger had sent them an extensive press release describing his new "theory of everything," immodestly placing his own work in the context of the achievements of the Greek sage Democritus (the coiner of the term "atom"), the Roman poet Lucretius, the French philosopher Descartes, Spinoza, and Einstein himself. "It is not a very becoming thing for a scientist to advertise his own discoveries," Schrödinger told them. "But since the Press wishes it, I submit to them."

Scientists are often misguidedly in love with their own dubious ideas, and there always have been ones willing to go to the press and get public attention. The media environment now is different and much more extensive, but I think the main difference is the lack of real progress being made, so fake claims of progress dominate because they have no competition.

26. **The Observer**
January 31, 2018

Bravo Peter for a bravura performance over the years. I cannot help wonder whether you would be a professor by now had you not taken your stance against the established "way forward" and stuck to it.

27. **Peter Woit**
January 31, 2018

The Observer,
Thanks, but I should make it clear that I haven't suffered professionally because of this in any way, and am very much content with my current academic position and employer.

Muon g-2 Anomaly Gone?

February 1, 2018
Categories: Uncategorized

I just learned some interesting news from Tommaso Dorigo's blog. Go there for more details, but the news is the claim in these three papers that, accounting for GR effects on the precision measurement of the muon anomalous magnetic moment, the three sigma difference between experiment and theory goes away.

This sort of calculation needs to be checked by other experts in the field, and provides an excellent example of where you want good peer review. Presumably we'll hear fairly soon whether the result holds up (the papers are not long or complicated). If this is right, it's a fantastic example of our understanding of fundamental physics at work, with the muon g-2 experiments measuring something they weren't even looking for, a subtle effect of general relativity.

Also interesting will be the implications for the ongoing experiment at Fermilab trying to achieve an even more precise g-2 measurement. I'm wondering whether there is any way for them to isolate the GR effect on their measurement.

The significance of this is that (setting aside questions about the neutrino sector), the muon g-2 measurement is the most prominent one I'm aware of where there has been a serious (three sigma) difference between experiment and Standard Model theory. This has often been interpreted as evidence for SUSY extensions of the SM. Projects producing "fits" that "predict" SUSY particles with masses somewhat too high to have been seen yet at the LHC use the g-2 anomaly as input. Tommaso ends by asking what happens to these fits if the g-2 anomaly goes away.

Update: For some recent things to read about the g-2 anomaly, before this latest news, see here and here.

Update: Rumor about a problem with this calculation here.

Update: According to this comment and this one, the g-2 collaboration has identified a problem with the calculation, making the predicted GR effect unobservably small. If the authors agree, presumably we'll soon see a corrected version of the paper(s).

Looking for more information about this, I ran across two blogs I hadn't seen before. String theorist Joe Conlon of Oxford has a blog called Fickle and Freckled. Not much there besides a posting supporting Brexit and denouncing the fact that nearly all of his colleagues disagree with him, which he fears will cost the university money. A second one is string/SUSY phenomenologist Mark Goodsell's Real Self-Energy. Goodsell seems to be a Lubos fan, dealing with the current g-2 story by linking to his discussion and denouncing the authors as "effectively crackpots". He also seems to be unhappy with a certain blogger:

> I regularly read two or three physics blogs, since they report on the latest news (and rumours). Now, one of these blogs is very popular whose ostensible purpose is to persuade people that string theory is a misguided research topic. Obviously, this is something I disagree with. However, it also talks a lot about high-energy physics generally, and being rather well-connected it can be quite informative and useful. However, it pretty much uniformly takes a very pessimistic line about all concrete ideas for new physics. It is difficult to overstate how damaging this has been, in making physicists and scientists in neighbouring fields depressed about the future of high-energy physics, and opposing this trend is one of the reasons I would like to blog …

Goodsell has devoted his career to string and SUSY phenomenology, and seems to feel that this is the "future of high-energy physics", and I'm responsible for making people discouraged about it. Perhaps he should stop advertising Lubos and denouncing crackpots, instead pay attention to the negative results from the LHC and the evidence they provide that his research program is a failure. Blaming his problems on me for pointing them out isn't going to make them go away.

Update: As mentioned in the comments, Matt Visser now has a preprint criticizing the calculation in these articles.

Comments

1. **Bernhard**
   February 1, 2018

   I cannot read Tommaso's article. Somehow it is being redirected to other stuff and I only see the abstract.

2. **Peter Woit**
   February 1, 2018

   Bernhard,
   I've seen that problem before on that website. For me right now it seems to be working properly. Another thing to try would be
   http://www.science20.com/tommaso_dorigo
   then the correct blog posting. If someone sees the problem you mention, perhaps they could report it to the website owner, saying what the page is that they are being incorrectly redirected to.

3. **Bernhard**
   February 1, 2018

   Peter,

   Thanks. I read it now and also (diagonally) read the articles. Whoever peer reviews those articles has a huge responsibility on his hands. It's almost like reading a standard textbook calculation (even if a hard one) that is surprising nobody had such an idea before.

4. **Art**
   February 1, 2018

   Delicious. Llewellyn Thomas must be smiling somewhere. I wonder if the electron magnetic moment could be measured in a storage ring, as opposed to a Penning trap, to see if the same effect arises.

5. **Dale**
   February 1, 2018

   Isn't it a bit suspiciuous that the GR correction happens to cancel the anomaly down to 0.1 sigma difference between the expected vs observed?

6. **Mike R**

February 1, 2018

Hmm, they claim the shift is due to absolute value of gravitational potential. But the gravitational potential GM/R due to the Sun is more than 10 times larger at the radius of Earth's orbit than Earth's own gravitational potential on its surface. Its probably premature to get excited.

7. **Mike R**
February 1, 2018

Also such effect would almost certainly violate the equivalence principle, if one can distinguish space-time curvature from uniform acceleration with a local experiment.

8. **Art**
February 1, 2018

Because the earth is in free fall around the sun, the equivalence principle says the solar gravitational field has no effect on a local terrestrial measurement.

9. **Friedwardt Winterberg**
February 2, 2018

If I understand the paper this could be a tiny effect caused by the likewise tiny GR space-time curvature by the gravitational field of the the earth. We know of course that this tiny space-time curvature is crucial for the working of the GPS. Question: If that is true what's the big deal if the GR space-time curvature of the earth gravitational field leads to other effects?

10. **UltraDoofus**
February 2, 2018

I went through the papers and it seems a tree-level contribution from Earth's gravitational field cancels out from all relevant quantities, except the 'muon magic momentum' term, which is used to calculate g-2. This apparently explains the anomaly.

Personally, I think this result will hold up. The calculations are pretty straightforward. If there is any inconsistency, it'll be found in no time.

I haven't read the original calculation of g-2, but people are usually thorough about these things. So all of this is pretty surprising.

11. **A**
February 2, 2018

According to the papers, the new effect does not depend on the gravitational field, but on the gravitational potential. It would be breakthrough if it could be measured by local g-2 experiments. One could even claim a bigger g-2 anomaly due to the gravitational potential of the sun and of the galaxy. Let's see if they missed some term that cancels the putative effect.

12. **Bernd**
February 2, 2018

A,
I don't want to spoil your excitement, but people have measured gravitational potentials for quite a while using atomic clocks. There are even portable clocks that can measure the gravitational potential of the earth with a resolution of about 70 cm.

13. **Dale**
February 2, 2018

Bernd,
Atomic clock experiments measure only gravitational potential difference between different locations. The effect in muon experiment is claimed to be proportional to the gravitational potential in that location (and not for example proportional to the gravitational potential gradient i.e. local acceleration).

14. **Bill**
February 2, 2018

Does anyone know what Schwarzschild coordinate system Morishima and Futamase are using in Eq.(3) in paper no. 1?

15. **Matt Visser**
February 2, 2018

The claimed resolution of the g-2 anomaly is asserted to depend on the *absolute* gravitational potential on the surface of the Earth.

Insoafar as one believes this claimed result, one is not working in general relativity,
and has explicitly violated Einstein's equivalence principle.

Insoafar as one believes this claimed result, the effect of the Sun exceeds that of the Earth by a factor 15, and worse, the effect of the Galaxy exceeds that of the Earth by a factor 2000.

(Gravitational potentials at the surface of the Earth, as opposed to gradients, behave somewhat unexpectedly: the Galaxy dominates over the Sun, which in turn dominates over the Earth's self gravity.)

On the other hand, insofar as one claims to be working with general relativity, then one should take note of the Einstein equivalence principle, whereby *absolute* gravitational potentials do not matter, and it is only potential differences that show up in laboratory physics.

The fractional corrections due to GR as compared to SR are of order

(Riemann tensor) x (size of laboratory)^2

With a little work this fractional correction turns into

(gravitational potential) x [(size of laboratory) / (radius of Earth)]^2

Overall, either way this proposal for resolving the g-2 anomaly fails:

1) Either you violate general relativity,
but then the effect of the Galaxy dominates and is 2000 times too big.

2) Or you work within general relativity, and then the effect is suppressed
by the overwhelmingly small factor

[(size of laboratory) / (radius of Earth)]^2.

So unfortunately this claimed resolution of the g-2 anomaly is not going to work.

16. **Chiara**
February 3, 2018

The authors claim that the effective muon magnetic moment is related to the one without gravity by μ_eff(muon)≈ (1+3ϕ/c^2) μ(muon), where ϕ is the gravitational potential.

It seems hard to believe
(1) that this formula does not contain potential differences, because potentials can be changed by a constant or by a change of coordinate system;
(2) that this formula doe not contain a term that depends on the orientation of the experiment with respect to the height coordinate z.

17. **Dale**
February 3, 2018

They do "assume that the motion is confined in the horizontal plane and the
magnetic field is applied vertically" (Part III, after Eq 27). But as Matt stated, they confuse the gravitational potential generated by earth with the minuscule gravitational potential difference in the laboratory (or else claim violation of the equivalence principle).

18. **Dale**
February 3, 2018

Apparently the error is more subtle that we speculated. In the very end of the Part III they correctly conclude that "Therefore, the relative magnitude of the contributions of the gravity gradient and the electromagnetic interaction is f∇ϕ / fEM ≃ 1.5 × 10^−15 which shows the gravity gradient contribution is 10^−15 times smaller than the electromagnetic interaction…"

It is the post-Newtonian effects that they conclude wrongly (as the sentence continues "… and is even 10^−5 times smaller than the post-Newtonian effects.")

19. **vmarko**
February 3, 2018

Beautiful! 🙂 Textbook GR, elementary calculation, this could have been an exercise for a graduate student (I might even give it to my students as an exercise). I haven't checked all minuses and factors of 2, but if the details of the calculations in the papers don't contain any mistakes, this represents an amazing lesson in GR for everyone!

As UltraDoofus noted, the kill comes in the equation (40) in 1801.10246, where the velocity of the muon (encoded in γ) is tuned to the "magic" value so that the term proportional to electric field becomes zero. From there, one reads off the value of the anomalous magnetic moment, and equations (44) and (45) compare it to the value of the a.m.m. without the GR correction term. And according to the table 3, this GR correction term has precisely the correct value to account for the 3.6σ discrepancy between experiment and the SM calculation. This is just way too good to be a coincidence. 😀

On the one hand, I do understand why GR has always been a blind spot in the minds of hep-ph and hep-ex folks (and maybe even some hep-th folks as well). But on the other hand, in face of a 3.6σ discrepancy in a high-precision experiment, the fact that nobody even _bothers_to_think_ of checking the GR contribution (while string theorists are on an all-out hunt for supersymmetry contributions) is just unbelievable! This result is a proper slap-in-the-face to all hep-* folks, to go back into the classroom and learn some GR.

Also, a small spoiler — while the first two papers are nice and instructive, one doesn't really *need* to read them, 1801.10246 is quite enough to make the point. 😀

Best, 🙂
Marko

20. **Art**
February 3, 2018

Bill,
The Schwarzschild metric used is defined in Appendix B.

Matt Viser,
The experimental apparatus is at rest with respect to the earth, as analyzed. To include the solar gravitational effect, one would have to include the accelerations with respect to the sun as well (since the earth is in free fall around it). However, there's no need to go to all that trouble, since the two precisely cancel per the equivalence principle. Ditto for the galaxy.

Various,
The analysis explicitly assumes a Schwarzschild geometry, so the corresponding fields are "baked in". Completely analogously, one can express the gravitational blue-shift of such a geometry solely by the potential of the receiving station.

With all that said,
I am not certain of the result. It seems they are suggesting the "magic gamma" was not selected quite correctly. I doubt the exact value was calculated for the experiment, but rather the result of some extremum search. I look forward to more authoritative discussion of this matter.

21. **Dale**
February 3, 2018

vmarko,
Don't you see a problem that they use gravitational potential of the earth ϕ (and not its' derivative)? It is claimed that a constant rescaling of the time and space coordinates could be made so that ϕlab = 0, and there is no Post-Newtonian contribution.

22. **G. S.**
February 3, 2018

Off topic, but it's going around social media that Joe Polchinski passed away yesterday following his battle with cancer. He was 63.

23. **UltraDoofus**
February 3, 2018

marko,

After looking at the papers again, I'm gonna have to curb my enthusiasm. I think they messed up while taking the post-Newtonian approximation.

As others have pointed out, the bare potential (gauge dependent) shouldn't appear in any observables. Only 'g' or its gradients can satisfy the equivalence principle. So the actual gravitational corrections are likely far too small cancel the anomaly.

Art,

1) The apparatus is not in free fall wrt to the Earth's field, which is why the experiment can only detect the local acceleration, 'g', and its gradients.

2) The redshifted/blueshifted frequency is a dot product between two vectors. So it's gauge independent.

3) Their argument about the magic momentum doesn't stand either. Because it's also gauge independent.

24. **Marc**
February 3, 2018

This discussion, while interesting, is completely irrelevant. The response from the g-2 collaboration (from the spokesperson Chris Polly):

Our spokes already replied to the authors since they made a mistake in the final conclusion. While the additional effect in the bxE term they calculate is 2ppm, they then attribute this full term to be the change in g-2. However, they forgot that that additional contribution needs to be weighted by the relative strength of the bxE term which is 1330ppm of the B field. So even if their calculation was correct, the actual contribution is 2ppm*1330ppm=2ppb. That's negligible for the ongoing experiments measuring to ~100ppb precision. And this argument does not even involve any judgement on the validity of the additional term they calculate.

25. **Noah Graham**
February 3, 2018

Matt Visser,
Is it obvious that the factor in (2) suppresses the effect? The muons are relativistic (gamma ~ 30) so SR effects are of order 1, the ratio of the storage ring radius to the earth radius is about 10^-5, and the correction only needs to be a few parts per billion.

26. **Noah Graham**
February 3, 2018

OK, actually more like 10^-6, but still in the ballpark. Also it must depend on the mass density of the earth somehow (if the earth were a neutron star, the effect would be much bigger)?

27. **Peter Woit**
February 3, 2018

G.S.
Yes, I was sorry to see that news. I never met Polchinski and my own indirect interactions with him were rather unfortunate. Rest in Peace.

28. **vmarko**
February 3, 2018

Ok, maybe I should respond to some of the issues people have raised here, just for the sake of clarity.

First, in the Newtonian approximation of GR, it is a quite standard thing to choose the boundary condition $\phi(\infty)=0$ for the potential. The authors have defined $\phi(r)$ in the sentence below equation (4), and it is obvious that they use this boundary condition to set the zero-level of the potential. Given this, the potential itself becomes observable. If anyone is uncomfortable by this, feel free to substitute (throughout the paper) every appearance of $\phi(r)$ by $(\phi(r) – \phi(\infty))$, which is invariant with respect to adding a constant. This doesn't change the result in any way.

Second, the gravitational potentials of the Sun doesn't play a role. In Newton-speak, this is because it is canceled by the corresponding centrifugal potential, coming from the fact that the Earth revolves around the Sun. In Einstein-speak, this is because the Earth follows a free-fall trajectory, and is thus in a locally inertial frame, so according to the equivalence principle it feels no pull from the Sun (and the tidal forces are too small over the size of the apparatus). The same statements hold for the galaxy. Note that the apparatus does *not* freely fall in the gravitational field of the Earth (as opposed to the Sun and the galaxy), but is being "pushed" off its geodesic trajectory upwards by the floor of the lab. This is an electromagnetic effect (spiced up by the Pauli exclusion principle), despite being described by the gravitational potential ϕ, in Newtonian language. This force is real, we all feel it when we stand up, and it has nothing whatsoever to do with any violation of the equivalence principle.

Third, one should distinguish the contribution coming from the potential ϕ and the contribution coming from the gradient of the potential, $\nabla\phi$. These are different, and the latter is much smaller than the former, as explained in Appendix C of the paper. Thus the gradient can be neglected, while the potential itself should not. The authors correctly showed due diligence to discuss this, I see no problem there.

Finally, regarding the response from Chris Polly, the relevant statement in the paper is this: " If the γ was experimentally tuned to minimize the electric field contribution, the quantity a^{mod} might have been measured.". That is to say, the experiment may contain a systematic error, depending on their procedure for tuning the value of γ. I don't know the details of the measurement process(es), but this doesn't seem to have anything to do with the absolute magnitude of the electric field, since the $\beta\times E$ term in (40) multiplies *both* the a.m.m. and the correction, so it cancels out of equation (44). Or maybe I misunderstood what Chris wanted to say. 😉

I hope this clears things up a little.

RIP Joe Polchinski.

HTH, 🙂
Marko

29. **Dale**
February 4, 2018

Marko,
I am grateful that you explained the topic in a such understandable way.

30. **Robert Matthews**
February 4, 2018

Given the claims that the preprint fails to explain the anomaly by orders of magnitude, I'm amazed there isn't some pretty simple dimensional analytic/O(mag) argument that shows this whole idea of GR affecting so subtle a phenomenon in QFT was always a non-starter.

31. **tulpoeid**
February 4, 2018

vmarko,

I don't know about this result, but let me just say that gravitational (and GR) effects are not unknown to accelerator physicists at all. The field has been taking these things into consideration and even I (not an accelerator physicist) happened to be at such a seminar. The rumor says that g-2 is already half way in writing an answer, as is by now kind of evident from comments above.

32. **Julius**
February 4, 2018

In case of the first article, another way to look at this "potential" effect, note that the factor in formula (4) comes from time dilation, and it is known that gravitational time dilation is, to the first order, proportional to differences of potentials. Since here only one location of comparison is explicitly mentioned, i.e. on the Earth's surface, the other location of comparison is naturally set at the infinity, and the constant cancels out. Hence, there is no contradiction in having pure potential. I was surprised that initially people jumped in with such conclusions, especially when this can be check almost on paper, with possible help from Mathematica.
Also, formula (5) contains mistakes. Surely, the overall factor should be gamma, not its inverse, and, I think, terms in brackets should have opposite signs. Seeing such things on page 2, I am not surprised that they may have forgotten a numerical factor.

33. **Blake Stacey**
February 4, 2018

Matt Visser's response to the original postings has appeared on hep-ph/gr-qc: "Post-Newtonian particle physics in curved spacetime" arXiv:1802.00651.

34. **A**
February 5, 2018

Julius, the difference with the potential at infinity will be relevant when some experiment will be done there. Local experiments cannot measure the local gravitational potential. This was also discussed in arXiv:1802.00651 by Matt Visser.

35. **Bernhard**
February 5, 2018

OK, so it seems the odds against the article being correct are now rather low, given the comments from experts, but the unprofessional reactions towards the authors are really sad. They are not crackpots – they wrote a paper that an experimental collaboration felt the need to respond to. However flawed, which I'm in no position to judge, this is all part of doing science. One wants to avoid making public mistakes, but this is not always possible.

36. **vmarko**
February 5, 2018

It appears that I was wrong regarding my comment to the response of Chris Polly. Namely, in experiment, the a.m.m. is not being determined from equation (44) but from equation (8). The authors correctly point out that this is calculated using the skewed value of γ, which appears to be a valid remark given the GR correction term in (40). So I decided to calculate the variation of (8) with respect to γ, taking into account (40), to see what happens when the value of γ is slightly shifted. And indeed, the variation turns out to be proportional to $\beta\times E$ as well, as Chris wrote. So Chris is right that this effect is weighted with the magnitude of the electric field, which is apparently small enough to suppress the GR correction beyond the experimental resolution.

In the end, it appears that the correction term in (45) and in Table 3 is really just a numerical coincidence. 🙁

Best, 🙂
Marko

37. **Julius**
February 5, 2018

A,
time dilation is not a local event. To the first order it depends on the difference of potentials, and the further apart they are measured, the bigger the effect. If one dislikes time at infinity, then by coordinate transformation time can be brought to a finite distance from Earth. In practice this is what we do for experiments on the ground, since we, are sitting in a potential well, together with our atomic clocks. Maybe what is confusing is the usage of the gradient notion, and I use the differential gradient definition, not some macroscopic difference.

38. **Nadir Bizi**
February 5, 2018

In the first paper, they compare Eqs. (4) and (19) to their Minkoswki counterparts to extract the effective magnetic moment of the muon. But this comparison is only valid in the locally inertial frame of the lab. (e.g. Fermi coordinates).

If one assumes that they are using a locally inertial frame in Eq. (3), then the metric has to be Minkowski at the lab, and the potential vanishes there. Eq.(9) is then “trivial”. The first correction from GR will thus come from variations of the potential (i.e. of the metric), as explained by Matt Visser in his short paper.

If on the other hand one uses Schwarzschild coordinates (as they do), then one is not allowed to extract the magnetic moment from Eqs. (4) and (19).

Their calculations seem correct to me, it is their physical interpretation of these calculations that one must be cautious with.

39. **Dimitris Kosmopoulos**
February 5, 2018

Hello Peter,

in all three papers, the Schwarzschild metric is approximated with respect to the small parameter ε = 1/c …

This is not correct.

The expansion parameter is usually dimensionless (such as e.g. the ratio υ/c, or the fine structure constant α).
And the expansion shall be finite when ε = 0. But in their case, it’s infinite!

Anyway, perturbation expansions are not based on dimensional quantities such as c, because with a change of units c can take any value you want – large or small…
Funny, in all three papers, it is stated that they use units where c = 1.
🙂

Usually Φ/c^2 is considered small for approximations of the Schwarzschild metric, but this gives the Newtonian limit.

Has anyone checked if their derivation of the approximate post Newtonian metric is correct?

40. **arxiv etc**
February 6, 2018

Does it occur to anyone that the authors could have contacted the muon g-2 collaboration privately, to discuss their findings? I asked a friend (member of the collaboration) about this and his reply was that expert theorists rapidly identified the error and this formed the basis of the reply by the collaboration.

41. **X**
February 7, 2018

arxiv,

Ideally they should have asked for feedback from experts and the muon collaboration privately before posting the papers. In practice though it is very understandable that they would choose to “shoot first and ask questions later”. Especially since they seem to be young unestablished people and therefore likely to get minimal credit if their work happened to be scooped “by coincidence” (not) by someone prominent while they were doing the private consultations…

(I had an experience of exactly that, although concerning a much smaller result, as I’m sure many others have had too. As a young nobody I found a result of some significance in a specialized subfield of hep theory – although no big deal in the general scheme of things – and thought I better seek feedback and confirmation from experts before posting the paper… Without getting into the details, the upshot was that I ended up sharing credit for it with 2 others, both of whom were more senior, so my paper was always 3rd and last when that work was cited thereafter… One of the others was an eminent senior person who ended up with the bulk of the credit – not so much because he tried to grab it but because the community just felt more comfortable with him having it, i.e. the Matthew effect in action.)

Regarding the current papers, if the result doesn’t hold up (which seems to be the case now) I hope the authors still get credit for boldness, creativity, and giving us something interesting to think about!

42. **Rob**
February 7, 2018

arxiv etc. What was required was feedback from theorists. It would be a little odd that authors should feel they have to go through an experiment to get this. For theory feedback, the arxiv is the most appropriate choice. There is also no guarantee that the expert theorists engaged by the collaboration would have worked as quickly as they did when the papers appeared in the arxiv. A response to the authors’ work may well have been placed on the back-burner for quite a while.

In general, I prefer these issues to play out in the open. I admire the authors for taking a chance. Yes, they were left with egg on their faces; that’s life. I certainly don’t see them as crack pots, as has been alleged (unless someone can come up with more evidence than is provided by this particular issue). At the very least we’ve all learned that GR plays effectively no role (no surprise) in the muon g-2 measurement and we’ve also been reminded that there is a first rate experiment which may resolve one of the big puzzles in particle physics.

43. **arxiv etc**
February 7, 2018

X and Rob,

It is a simple matter of courtesy. The authors explicitly claimed to offer a resolution of the muon g-2 anomaly. It is a simple courtesy to contact the collaboration “We have performed a calculation which we believe resolves the muon g-2 anomaly. We enclose a preprint of our analysis for your comments. We look forward to your reply.”

#1
The collaboration might ignore the authors. Post a preprint with a statement “We have communicated our results to the collaboration, but to date they have not replied.”

#2
Post a preprint and send a copy to the collaboration at the same time. Include a statement “We have sent a copy of our results to the collaboration and will update our post if and when we receive a reply.”

But at a minimum *demonstrate that they made a good faith effort to communicate with the collaboration*.

44. **X**
February 8, 2018

arxiv,
The problem is that a genuinely good faith effort to communicate with the collaboration before posting the papers would require giving them at least 3 days to respond,

and that is enough time to be “coincidentally” scooped.

E.g. in this scenario, in the case that the result happened to be correct:
The collaboration forwards the papers to their theory expert, who spends an hour or two on them and gets very excited – doesn’t see any immediate flaw in the arguments but still needs to check more carefully. He mentions his initial impression and excitement to someone, who mentions it to someone else, and the rumor quickly spreads among a circle of experts.
One of them, person A, hears the rumor and remembers that he had actually thought about calculating the gravitational contribution to the muon anomaly, and vaguely planned to do it at some point (as a fun exercise; he was sure the contribution would be negligible). Well, since he had the idea, and planned to do the calculation, “why should I just sit here now and let that unknown Japanese group get all the glory” he thinks this to himself. “So he puts everything else aside and immediately gets to work…does the calculations and posts the paper within 24 hours.
(Perhaps person A adds a note at the end of his paper saying that while this work was in preparation he heard from person B that a Japanese group had informally circulated a paper on this topic, although having not seen the paper he doesn’t know if their approach and result is the same…)

In my view it would be fine if the authors send an email to the muon collaboration to point out their paper and solicit feedback once they posted the paper. And for all we know they might have actually done this.

45. **arxiv etc**
February 8, 2018

“why should I just sit here now and let that unknown Japanese group get all the glory” he thinks this to himself. “So he puts everything else aside and immediately gets to work…does the calculations and posts the paper within 24 hours.

That’s just sad, that these are modern scientists. When Einstein received Bose’s manuscript he did not say “why should I just sit here now and let that unknown Indian get all the glory”.

It is obvious the authors did not contact the collaboration, or the collaboration would not have worded their reply as they did. It costs the authors nothing to include a statement in their preprint that they had contacted the collaboration. They did not, as the response of the collaboration makes clear.

It is a simple matter of civil behavior.

46. **Peter Woit**
February 8, 2018

All,
I think the question of whether they should have contacted the experiment has now been properly beaten to death. In any case, in retrospect it seems to me the authors made the more general mistake of not consulting a theorist more expert than themselves in the subtleties of GR calculations. Yes, there’s always a danger that if you talk to experts about your ideas they may get stolen, but on the other hand, if you don’t you’re likely to not find out what the problems are with your ideas until it is too late.

47. **Shantanu**
February 9, 2018

X, Peter etc. Futamese is not a junior person. He is one of the best GR theorists in post-Newtonian business. You can check out his past works.

48. **vmarko**
February 9, 2018

Peter,

“mistake of not consulting a theorist more expert than themselves in the subtleties of GR calculations”

They did not make any mistakes with regards to GR, their calculations are textbook-level proper. The only thing they did wrong was to suggest an interpretation of the experiment that turned out to be wrong. And even in doing that they used careful phrasing, like ”If the γ was experimentally tuned to minimize the electric field contribution, the quantity a^{mod} *might have been* measured” (emphasis mine). I cannot think of a more toned-down formulation of that statement (which turned out to be wrong in the end).

BTW, it is not really common practice to include statements like “we contacted the experimental collaboration, but they didn’t reply yet” into a paper or a preprint. I have never seen any such statements in any paper whatsoever.

HTH, 🙂
Marko

49. **Dale**
February 10, 2018

vmarko
Matt Visser’s rebuttal of the Japanese papers is based on the violation of the equivalence principle. The rebuttal is not based on any details of the experimental set-up. Surely if the Japanese disagree with Matt’s conclusions, they will post a reply to the rebuttal.

50. **Wyman**
February 11, 2018

I am having trouble understanding these objections based on the equivalence principle. The authors appear to be working in the Schwarzschild metric, and instead of referring to a quantity named like r_s / r they refer to the equivalent “gravitational potential of the Earth”, which is uniquely defined by identifying it with the term in the metric.

51. **vmarko**
February 11, 2018

Dale,

They will reply if they find his rebuttal worthy of attention. If I were them, I wouldn’t.

Best, 🙂

Marko

52. **cedric bardot**
February 13, 2018

A very brief arxiv paper (https://arxiv.org/abs/1802.04025) from February the 13th may point quite educationally the erroneous tacit assumption made in the first Japanese paper namely taking a coordinate time for a physical one…

“When we are moved … by a thirst for knowledge, then the error, like the pain or the sadness, passes us without ever being lost, and the trace of its passage is a renewed knowledge” (Alexander Grothendieck in “Harvests and Seeds”)

Various and Sundry

February 9, 2018
Categories: Uncategorized

- I recently spent some time looking at old postings on this blog, partly because of writing this blog entry, partly because Gil Kalai got me a copy of his book Gina Says. For a moment I thought this would be a good time to write something about the "String Wars", but then decided that project should wait for another time. I did go quickly through old postings (there are 1660 of them…) and pick out a small subset that might be more worth reading for anyone with time on their hands. A list is available by selecting the category Favorite Old Posts.
- Another category of blog posts that includes many that I spent more time than usual writing is that of Book Reviews, of which there are 93 here (about ten of these were written for publication elsewhere). Among the forthcoming books I'm hoping to write about are Sabine Hossenfelder's Lost in Math, and the fifth volume of Raoul Bott's Collected Works (listed at Target under "test prep and study guides"). Some other forthcoming books are Sean Carroll's Something Deeply Hidden, and a new book by Brian Greene that I know nothing about other than this.
- A debate various places on Twitter about science journalism and accuracy included this from neuroscientist Chris Chambers, who explains that when he looked into this he discovered what I've often seen in physics reporting: the source of hype is more often scientists and their press releases than journalists. https://twitter.com/chrisdc77/status/960304692449435648
- The nLab project has been joined by the even more exciting mLab project (some discussion here).
- This semester MSRI is running a program on enumerative geometry, with two workshops so far, materials here and here. A lot of this subject has been influenced by ideas from physics, in particular from topological quantum field theories. While my Columbia colleague Andrei Okounkov has been on leave this year, he's written two excellent surveys of some recent work, see here (for the ICM) and here. For older surveys from him, see here and here.

Update: I hear of yet another book in progress: Lee Smolin on realist approaches to quantum foundations, tentative title "Beyond the Quantum".

Update: Today (February 12) the Harvard Physics department is hosting a celebration of the centennial of Julian Schwinger.

Update: One of the prize possessions of my youth was a copy of Abramowitz and Stegun, a huge Dover paperback version of their reference tome *Handbook of Mathematical Functions*. Physics Today has a long new article about the twenty-first century version of this, the Digital Library of Mathematical Functions, now a project run by the NIST.

Update: This week there's a workshop on "Naturalness" going on in Aachen. Results from last year's LHC run should start to appear soon, see here.

Comments

1. **anon**
   February 9, 2018

   The nLab is big. There is the abstract stuff, but there is also some quantum field theory, for instance (takes a while to load, though, is pretty large).

2. **Shantanu**
   February 16, 2018

   Peter , OT: An article in economist about failure to detect proton decay.
   (I cannot read the full article, but am sure you have access to it)
   https://www.economist.com/news/science-and-technology/21734379-no-guts-no-glory-fundamental-physics-frustrating-physicists

3. **a1**
   February 16, 2018

   I thought that you might be interested to read:
   "String theory is a quantum theory of gravity. Albert Einstein's theory of general relativity emerges naturally from its equations.…"
   http://inference-review.com/article/a-view-from-the-bridge

4. **Peter Woit**
   February 16, 2018

   a1,
   That's a nice article, much of it very similar to the story I wrote about in chapter 10 of "Not Even Wrong". I quite agree with the final sentiment:
   "If mathematics and physics are in so many respects in equipoise, then the differences between them may be less a matter of their content than their technique; and that, in the end, they serve to show that there is only one reality to which they both appeal.

   Wouldn't it be lovely to think so?"
   see
   https://arxiv.org/abs/1506.07576

5. **Shantanu**
   February 17, 2018

   Peter something else :
   John's ellis talk on still believing super-symmetry at Joe Silk's 75th birthday fest
   http://www.iap.fr/vie_scientifique/ateliers/darkmatters/2017/video/John_Ellis_2017-12-11_1530/index.html

6. **Timothy**
   February 19, 2018

   Check out this nice talk from Nima Arkani-Hamed, entitled "The Doom of Space Time". He turns into quite the comedian at 1 hour 40 minutes into the talk, citing "violins, chellos, Nova specials." Great explanations about how space-time must be emergent and offers an intro to his own stab at the problem.

   https://www.youtube.com/watch?v=qTx98PUW6lE&pbjreload=10

7. **Peter Woit**
   February 19, 2018

Timothy,
Thanks, hadn’t seen that before. I confess though that it’s still unclear to me why spacetime “must” be emergent, and what it is supposed to be emerging from. Back in the good old days when people were talking about quantum gravity I could tell exactly what their quantum gravity theory was, nowadays I often have no idea what the actual theory being discussed is.

8. **Peter Woit**
February 20, 2018

I took a longer look at the Arkani-Hamed talk. It’s the usual story he has been telling for about ten years now, that new ways of computing scattering amplitudes in terms of volumes and combinatorics of geometric objects indicate a grand synthesis, in which spacetime and quantum theory will be emergent notions. He just doesn’t yet know what that grand synthesis is. For an insightful comment from him about these kinds of claims, see this posting

http://www.math.columbia.edu/~woit/wordpress/?p=6476

and this quote:
“So, usually I’ll get up when I talk about scattering amplitudes and give a long introduction about how spacetime is doomed, we have to find some way of thinking about quantum field theory without local evolution in space time and maybe even without a Hilbert space and blah-blah-blah. This is all very high-falutin stuff, this is stuff that Lance wouldn’t be get caught dead saying. I think none of these guys would ever say something that sounds so pretentious, but I have to say it, you know I have to say it, because this is the only way I can get up in the morning, and like “I suck again, OK, here we go, I’m doing it because spacetime is doomed, I swear to God, right”.”

9. **Nick Maiorino**
February 27, 2018

Hopefully this falls under the ***Various and Sundry*** category, but the latest results from the CDEX-10 Experiment are in, and there’s still no sign of WIMP’s.

10. **Low Math, Meekly Interacting**
March 1, 2018

And then there’s the recent EDGES report suggesting DM may be considerably lighter than most WIMP models predicted, if not different in other ways. Guess time will tell if this one will go the way BICEP2’s B modes, but reportedly we won’t have to wait too long for other groups to confirm or refute.

11. **Peter Woit**
March 1, 2018

LMMI,
I know little about this field, but seems way premature to be claiming anything about dark matter from this. For a critical take on the observations (much less the speculative theoretical interpretation), see
https://twitter.com/UCBProf/status/969071237405097985

Applied Group Theory

February 26, 2018
Categories: Uncategorized

I just noticed that Greg Moore has been teaching a wonderful course in recent years with the misleadingly bland title of Applied Group Theory. His choice of the topics he wants to cover given here is an excellent one and a good outline for anyone trying to get themselves a serious education in the modern overlap of math and physics.

The problem with this outline is that it's far too ambitious to cover in a one-semester course, starting just from basics. Moore notes that in 2008 and 2009 versions of the course he only got through roughly half the topics, with students still complaining about the fast pace of the course. In 2013 he only made it through two out 21 topics, but in doing so generated two book-length documents of notes:

- Chapter 1: Abstract Group Theory
- Chapter 2: Linear Algebra User's Manual

These each contain a wealth of valuable material. I do hope he someday writes up the other 19 topics, but if he does it the way he has been going, the length might turn out to be around 4000 pages, so that might take a while. In the meantime, an account of some of them is available here.

In addition, there's also a list of suggested topics for term papers, nearly a hundred of them, each with a description of an interesting issue that has been a topic of significant research, with references for where to start learning about the topic.

Comments

1. **ilovecats**
   February 28, 2018

   A gold mine!

2. **Matt Foster**
   March 1, 2018

   It's far less "sexy" than group cohomology (these days), but y'all can find a very applied "users manual" to Lie Algebra representation theory as 9 course module pdfs on my webpage,

   http://mf23.web.rice.edu/

   (scroll down to "Lecture notes")

   These are basically a more fleshed-out version of Cahn's excellent 1984 book _Semi-simple Lie Algebras and Their Representations_. My idea was to try to make representation theory accessible to (upper division) undergrads, which means developing the ideas without bringing in applications to quantum field theory. I haven't figured out the best way to distribute these yet.

3. **Rana Singh**
   March 2, 2018

   Since the reading list for Physics 618 mentions books related to history and culture associated with group theory, two additional books are suggested below, both available from Dover Publications. (1) The Theory of Groups & Quantum Mechanics by Weyl. This is a classic and since last published in 1930, provides an insight into a time when a lot was happening in quantum physics. Interestingly, when discussing the Dirac equation, and trying to reconcile theory with observed facts, Weyl writes, "indeed, according to it the mass of a proton should be the same as the mass of an electron". At that point, one recollects that the positron had not been discovered at the time. It was discovered two years later. (2) The Genesis of the Abstract Group Concept by Hans Wussing. This book discusses the history of Group Theory from a purely mathematical perspective and attributes the development of the subject to the theory of algebraic equations (Galois Theory and the solution of the quintic equation), number theory , and geometry.

4. **D R Lunsford**
   March 3, 2018

   Thanks for pointing that out, looks great.

   -drl

5. **Lindsay Berge**
   March 5, 2018

   On the topic of being "far too ambitious to cover in a one-semester course" and "complaining about the fast pace of the course", my PhD supervisor once delivered a Nuclear Theory course with so much content that one his students wrote on the backboard before he came in to the lecture theater "Question: when can a lecturer go faster than light? Answer: when he conveys no information!"
   He laughed and took all the students to the pub but it is not clear whether he slowed down.
   That said, both "Chapters" are fascinating and inspiring.

6. **Thomas Larsson**
   March 6, 2018

   I have heard the same joke about the pace of growth of the Physical Review.

   There is certainly a wealth of material in these notes, and hopefully Moore will find time to cover the other topics as well. I'm curious about chapter 15 "Kac-Moody and affine Lie algebras, and beyond". In particular what he has to say about the "beyond" part.

Rochester Colloquium Talk

March 6, 2018
Categories: Uncategorized

I'm heading up to Rochester this evening, will give a colloquium talk there in the physics department on Wednesday at 3:45. I'll put up a link to the slides after the talk, for now, here's the abstract:

> **Particle theory: a view from a neighboring field**
>
> High energy particle physics faces a challenging future, largely because of the overwhelming success of the Standard Model. The LHC discovery of a Higgs particle with exactly the predicted properties, coupled with the lack of evidence there for "Beyond the Standard Model" physics, has led some to characterize this as a "crisis".
>
> In this talk I'll consider the current situation from a somewhat unusual point of view, that of someone who began his career in physics departments doing particle theory, but then moved to mathematics departments. The field of mathematics has complex and close ties to fundamental physical theory, and the cross-cultural perspective it provides may be of interest.

Update: Apologies for the earlier mistake (I had "Thursday" instead of "Wednesday" above). The slides for the talk are available here.

Comments

1. **Afterthought**
   March 6, 2018

   I've been struggling to find an answer to this question, would you be willing to help?

   The highly successful physics paradigms of today were largely in place by what year?

2. **Peter Woit**
   March 7, 2018

   1973

3. **Shantanu**
   March 7, 2018

   If anyone from Perimeter institute is reading this, Could you give this talk at PI, so that it will be recorded? I don't think Rochester records the colloquium

4. **John McAllison**
   March 7, 2018

   Peter, it would be great if the talk could be videoed and perhaps posted on Youtube later: Can you arrange this?

5. **Chris Kennedy**
   March 7, 2018

   Good food in Rochester: Sticky Lips has great barbeque. Guida's has great pizza. If you like thick crust ask Guida's if they have any slices of Grandma's Red.

6. **Kingsuk Maitra**
   March 7, 2018

   Dr. Woit,
   Your blog post says the colloquium talk is on Thursday but the University of Rochester website says Wednesday. Is it over? If so, could you please post a link to the slides?
   Thanks

7. **Peter Woit**
   March 7, 2018

   Talk just finished, sorry about the mistaken day given in the posting. No recording, I have added a link to the slides.

8. **lun**
   March 7, 2018

   What questions did you get?

9. **JimV**
   March 8, 2018

   This may be a semantic issue, but I tend to disagree that Mathematics is a non-empirical science (as stated in the slides). For example:

   The Taniyama-Shimura Conjecture (based on empirical observation of the characteristic numbers of elliptic curves and modular forms) led to Andrew Wiles' proof of Fermat's Last Theorem (as discussed in Simon Singh's "Fermat's Enigma").

   Scott Aaronson on a mathematical breakthrough in complexity theory: "It's yet another example of something I've seen again and again in this business, how there's no substitute for just playing around with a bunch of examples." – http://www.scottaaronson.com/blog/?p=2325

   Greg Chaitin, co-founder of Kolmogorov -Chaitin Information Theory:

"For years I've been arguing that information-theoretic incompleteness results inevitably push us in the direction of a quasi-empirical view of math, one in which math and physics are different, but maybe not as different as most people think. As Vladimir Arnold provocatively puts it, math and physics are the same, except that in math the experiments are a lot cheaper!" http://www.rutherfordjournal.org/article020103.html

10. **Peter Woit**
March 9, 2018

lun,
The questions were mostly about the multiverse business, and the more I had to say there was just more of the usual if you read this blog.

JimV,

I don't want to start a discussion of this here, but while, yes, mathematical advances often proceed by working out examples, finding patterns, conjecturing and then proving general theorems, the activity of working out examples is something quite different than a physics experiment. In one case you're investigating the world of mathematical structures, in the other physical reality.

11. **Lowell Brown**
March 10, 2018

"mathematical ideas originate in empirics. But, once they are conceived, the subject begins to live a peculiar life of its own and is … governed by almost entirely aesthetical motivations. In other words, at a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration. Whenever this stage is reached the only remedy seems to me to be the rejuvenating return to the source: the reinjection of more or less directly empirical ideas."

John Von Neumann

12. **Pascal**
March 10, 2018

On slide 9 : "Experimental study of quantum gravity seems out of reach."

Your fellow blogger Sabine Hossenfelder seems to disagree, for instance she writes:

"I am cautiously hopeful that within my lifetime we will succeed in experimentally demonstrating that gravity is quantized"
http://nautil.us/issue/45/power/what-quantum-gravity-needs-is-more-experiments

Any comment on what's the root cause of this disagreement?

13. **Amami**
March 11, 2018

I honnestly have to say that this talk and this way to see the future of fundamental physics are not very optimistic, especially for young people like me.

14. **Peter Woit**
March 11, 2018

Pascal,
I'm no expert on this, but most of the discussions of possible quantum gravity experiments seem to aim at little more than checking that gravity obeys general laws of quantum mechanics (and it's not clear if they can achieve that). What seems to me to be inaccessible is any experimental access to the study of quantum gravity beyond checking that at low energies there is the kind of consistency of gravity and quantum mechanics expected. In simpler terms, it's the questions about quantum gravity at the Planck scale that really mystify theorists, and that I don't see experiments telling us anything about.

15. **Tim May**
March 12, 2018

Amami, about the prospect in particle physics, and about 1973 in particular (reference to Peter's comment about 1973), I happened to be in a place to cook a steak for Feynman in that year. We were hosting him at our college and later in our off-campus apartment. He was very gracious with his time and various jokes and comments.

I asked him straigtht out what he thought the prospects for particle physics were, and what he might suggest people study instead if in fact particle physics are starting to dry out.

He said he did indeed think it was starting to dry out, that most things had basically been figured out (this was slightly before the Standard Model was mostly finalized, but a lot of things were known).

His advice? Computer science or pure mathematics.

I elected not to go to grad school and instead joined Intel Corporation. A lot of fun physics. Not exactly figurring out the nature of spacetime–not many do–but fun and I retired when I was 34. (Kept doing other things, just not on a 9-5 schedule dictated by other things.)

A physics education, even up to just a Bachelors, for those gifted in physics, is actually very useful in a lot of areas of high tech. I was hired by and worked for he guy who went on to become the President, CEO, and Chairman of Intel (Craig Barrett).

(Not so gifted, no. And even those with Ph.D.s who are not gifted, well, I worked with a lot of these types and they were not very successful in my experience.)

Meaning, if you are good at physics, keep at it. As to whether you may figure out the Theory of Everything, not too likely. And it was not too likely in 1973. Or in 1955. It never was.

Things were more easy to do experiments on in earlier times. For obvious reasons. So a Dirac saw his antimatter prediction confirmed a year or two later. And so on. Today, a prediction might take 200 years to test…simple log energy graphs.

If you are really good at math, keep at it as well. A couple of my roommates in college have had long and distinguished careers in math. I think the influence of the physicists in the house may have had an effect, and I strongly suspect the supper with Feynman did.

16. **GoletaBeach**

March 12, 2018

Fun talk, Peter. Surprised how many of the initial slides were essentially an overview of your version of the current state of experimental HEP.

For some of us, the theoretical motivations have always been a minority portion of the motivations for doing particle physics. Given all the experimental slides you included, odd you didn't mention that. Your talk, though, you are of course free to cover whatever you feel is important.

Can't agree with: "Technological barriers are starting to make it impossible to make progress on HEP physics as before." (slide 31). All the technological barriers at the energy frontier are doing is slowly refocusing experimental effort away from that frontier, on to other frontiers.

The energy frontier has of course the most important source of breakthroughs, but it hasn't accounted for a lopsided portion of the breakthroughs in particle physics.

A list… the discovery of the neutron (could have been done in 1919 when Rutherford started to disintegrate nuclei, took until 1932 due to not doing the right experiment); the neutrino (the great Ellis and Wooster experiment); neutron-induced radioactivity and all of its consequences including the bomb, medical isotopes, and nuclear reactors (Fermi and his Nobel); the measurement of the proton magnetic moment (1930's, indicated proton substructure); the pi/mu distinction (Fermi's old group in Rome during WWII); parity violation (the tau-theta puzzle); K0-K0bar mixing (off the frontier); the nu_mu/nu_e distinction (1962 off the frontier); CP violation in K0-K0bar system (off the frontier); the solar neutrino deficit; the J/Psi (the ISR was the energy frontier, missed charm); both the tau and the bottom (again, off the energy frontier, which was then too dirty; better detectors have improved it); the long b-life and B-mixing; atmospheric neutrino mixing; KAMLAND and Daya Bay/RENO narrowing down the neutrino mixing matrix.

A list so long it gets boring. But I must say: by omitting the rich realm of particle physics discovery that did not occur at the energy frontier (while simultaneously acknowledging the great importance the frontier) you incorrectly, IMO, imply that experimental discovery is over in HEP.

17. **Chris W.**
March 12, 2018

Mathematical "experiments" concern observations of patterns in the outcomes of computations or other formal manipulations, where understanding the laws governing the physical substrate of the manipulations is not the issue. One can do arithmetic computations with pencil and paper (and a scheme for representing numbers and operations on them), a calculator, or a computer. By design, all these tools are facilitating the same manipulations.

In this sense, mathematics is *not* empirical.

18. **Peter Woit**
March 12, 2018

Goleta Beach,
Thanks for the comment. The talk was intentionally provocative with one aim to explain the problem at the energy frontier (the severity of which I don't think is widely appreciated outside of the field). I did give the caveat that there is still hope for other approaches, especially noting that neutrino physics is still poorly understood, but a serious discussion of that would have been another, different talk, one I'd be ill-equipped to give.

On your history, I don't really agree that the charm/tau/bottom discoveries were not "energy frontier". In particular, SPEAR in the mid-seventies I'd describe as an energy frontier machine (since it was at the highest e+/e- energy), just as I'd describe the ILC as an energy frontier machine if it ever gets built. About the neutrino results, yes, that's true that those have generally not been energy frontier results and that will continue to be true.

19. **Pascal**
March 13, 2018

Is there some concensus on what theory should describe the low-energy behavior of quantum gravity (which you agree might be experimentally accessible with some luck and and hard work)?

20. **Peter Woit**
March 13, 2018

Pascal,
Using the standard Einstein-Hilbert Lagrangian for gravity gives a perfectly sensible low-energy effective field theory, and that's generally assumed to be what we would see in the low-energy limit if we could do experiments. The renormalizability problem, from the effective field theory point of view, just shows up at high energies (of order the Planck mass).

21. **GoletaBeach**
March 14, 2018

Hi Peter, perhaps in retrospect SPEAR was an "energy frontier" machine, but at the time two facts made contemporaries view it as off the frontier:
(1)ISR at CERN was at CM energy of 30 GeV, and NAL (now FNAL) was at 20 GeV (see Figure 15 of link below)… both hadron beams
(2)SPEAR's CM energy was at 4.8 GeV, and the J/Psi was down at 3.1 GeV, and the tau-tau threshold was at 3.6 GeV. Richter, in late 1974, resisted going back down to the 3 GeV region to scan… internally it was sometimes said he was the first Nobel prize winner to lobby against the pursuit of the physics he was later awarded for. He acquiesced to a re-scan of the 3.1 GeV region a bit reluctantly, but he did agree. In any case, off the frontier, even SPEAR's frontier.

Now we know all about partons and pdf's and we know that the parton CM energy is well below that of the hadrons, so the ISR and NAL effective energies aren't that different than SPEAR's. But at the time no-one agreed with that view… even Richter in the link below thought the SPEAR data in the summer of 1974 contraindicated the quark/parton model.

At the time (and even now with ILC) e+e- is valued for its cleanliness and precision. ISR missed charm, after all, but could easily, with today's detector technology, have seen charm. Or even with tech of the 1960's and the right idea of pursuing charged leptons while ignoring the hadrons (perhaps). With the advances in hadron-collider detector abilities, it is very hard any more to call e+e- "energy frontier"…. that is, it is hard to imagine LHC missing stuff in the way the ISR did. Not impossible but hard. What the ILC could do… nail down Higgs couplings to great precision, and maybe indicated new physics somewhere "up there" in mass. But neutrino physics has already done that, and other low energy stuff like electric dipole moments are also probing "new physics" at pretty high masses for particles appearing in loops.

The problem will always be: knowing something is in a loop always has ambiguities and degeneracies. Knowing something is in a loop mainly provides support for a new energy frontier machine, and that is what the ILC could do. So could EDMs. I think the natural neutrino "new physics" scale is so high that no imaginable collider could get there, but probably that statement is not air-tight.

Link to summer-1974 Richter talk… http://slac.stanford.edu/pubs/slacpubs/1250/slac-pub-1478.pdf

On the b-quark discovery… really it was high resolution in the FNAL spectrometers, not center-of-mass energy, that made the upsilon discovery. When e+e- started to see the upsilon, it was DORIS and CESR, which were way behind the e+e- energy frontier, which was PETRA and PEP… the latter 2 had 3X the center-of-mass energy of the former.

Some places where the energy frontier was indeed crucial: antiproton discovery, Omega- discovery, W/Z, top quark, Higgs. Crucial stuff, of course.

Presumptuous Blogging

March 12, 2018
Categories: Multiverse Mania

John Horgan at Scientific American today has an interview with Martin Rees in which Rees says:

> It's presumptuous (as some people like Woit and Smolin have done) to deride the way some manifestly brilliant people choose to dedicate their scientific lives.

I generally try and be careful to criticize not people, but the arguments they are making. Rees and others (including a Cambridge University Press referee for *Not Even Wrong*, who basically thought I had good points, but was presumptuous to be making them) often seem offended that I or Lee Smolin are challenging arguments from those smarter than ourselves. It's true that there is a problem with this: bad arguments of the kind I'm criticizing should be challenged by the leading figures in the field, not by me. In particular, many particle theorists smarter and more distinguished than myself are well aware of the problem of multiverse mania, but doing nothing about it, as it destroys the credibility of their field. Among the few personal criticisms of some leading theorists that I have is that they're not doing the job they're paid for here, and I'm not happy wasting my time trying to (presumptuously) do it for them.

On the presumptuous behavior front, here's some more of it:

- Rees's arguments are the usual multiverse propaganda, including the usual red-herring argument defending the multiverse as science:

  > It's sometimes claimed that domains that are in principle unobservable aren't part of science. But not even the most conservative astronomer would take this line. We're in an accelerating universe where distant galaxies will disappear over a horizon, and their far future would never be in principle observable. So it's natural to suppose that there are galaxies that are already beyond the horizon and so forever unobservable. If you're in the middle of the ocean, you'd be surprised if its boundary lay just beyond your horizon. Likewise, astronomers are confident that the volume of space-time within range of our telescopes — what astronomers have traditionally called 'the universe' – is only a tiny fraction of the aftermath of our big bang. We'd expect far more galaxies located unobservably beyond the horizon.

  As usual, Rees gives no evidence for his claim that those skeptical that the multiverse is science are just ignoramuses who don't understand the notion of indirect evidence for a scientific theory.

  Multiverse mania goes on and on, for instance here and here.

- Among the more presumptuous things I've done, there's last week's talk.
- I recently noticed here the analysis that:

  > I "predict" that if by about 2020 the LHC or cosmology do not show any concrete evidence of supersymmetry or superstrings, physicists will attack string theory the way the Huns attacked Rome. Like the Huns, Lee Smolin, Peter Woit, and Roger Penrose have already started encircling string theory, abiding their time, and waiting for the strike. They seem to have read The Rise and Fall of the Roman Empire carefully.

  I've never read Gibbon. If one accepts the analogy though, I think it's conventional wisdom that the String Wars were back in 2006, we didn't wait for 2020. All the evidence I've seen recently is that most string theorists have now given up on the idea that evidence for supersymmetry or string theory will appear at the LHC. Those attempting to defend Rome now are trying to backpedal on their claims from a decade ago. They now argue that finding nothing at the LHC isn't surprising and doesn't matter, but this rear-guard action likely will not be effective.

- More evidence that hopes for LHC-scale SUSY are now pretty much dead can be found by looking at the slides from the recent Aachen workshop on Naturalness, Hierarchy and Fine-Tuning. The debate among theorists has now moved on to trying to figure out what conclusion to draw from the failure of widely-promoted claims about "fine-tuning". One motivation for such claims was that in string theory-based models the CC and Higgs potential are supposedly calculable, with expected results from dimensional analysis that are exponentially large (Planck scale) compared to observations. On this front, an obvious conclusion to draw is just that these models (which are complicated and predict nothing else anyway) are wrong.
- I suppose it really is presumptuous to make a snarky comment about a talk by someone clearly more hard-working and smarter than I am. In any case, I just noticed that slides for Nima Arkani-Hamed's talk *Three Cheers for Shut Up and Calculate!* are now available here. I had written about this here without knowing what the talk had been about, based on the historical origin of the "Shut Up and Calculate!" slogan (and thus assuming the measurement problem would be a main topic). From the slides, Arkani-Hamed's talk wasn't at all about the measurement problem, but largely about quantum gravity, with many slides of speculative claims about new emergent versions of space-time and quantum mechanics (backed by one slide about calculating amplitudes as volumes). I'm quite sympathetic to what he has to say about the importance of prioritizing well-defined calculations over meaningless verbiage, but it seemed to me that in this talk he wasn't really taking his own advice. I can't help wondering who it is that Arkani-Hamed thinks should "Shut up" about vague ideas about quantum gravity and stick to well-defined calculations.

Update: For more presumption, see the latest at Backreaction.

Update: Lee Smolin has been edited out of the list of the presumptuous in the Rees interview, but I'm rather proud to be presumptuous, so I'm still there, and a link has been added to this blog posting.

Comments

1. **paddy**
   March 12, 2018

   Peter:
   If anyone knows "presumptuousness" 'tis the likes of Martin Rees. [Pardon the ad hominem and delete as you see fit.]

2. **Peter Orland**
   March 12, 2018

   Sorry to be officious, Peter, but the title of Gibbon's multivolume monograph (which I attempted, but never finished, back in my youth) is "A History of the Decline and Fall of the Roman Empire". "Rise" is absent from the title.

3. **Preston Burkett**
   March 12, 2018

   This is just so depressing. The vindictiveness string researchers display in their replies to standard empirical questioning is telling. It seems like they have this grab bag of talking points & fallacious arguments that enable them to evade criticism by making their critics look like ignorant naysayers.

By the way, I don't think there's anything quite as presumptuous as saying that you shouldn't question intelligent people because they're smarter than you. That seems to imply infallibility, or at least that smart people are less fallible, & we know that's just not true (there's even data to support it!). There was at least one smart man who said "I know I'm intelligent because I know I know nothing", which to me implies that people who make opposing statements are flattering their own intelligence (as well as advocating explicit elitism).

I'm sorry if this comes off as a rant. I've always loved science & seeing the opportunity for novel ideas squandered like this makes me sad. I hope a team of creative thinkers can break the mold & bring us new theories before too long.

4. **Anindya**
   March 12, 2018

   Regarding Rees' quote about the multiverse, I think he makes a very sensible point in the previous paragraph:
   IF we have a theory that accurately models what happened in the very very early universe and IF that theory predicts a multiverse, THEN we would have to take the multiverse idea seriously.

   I am always surprised that multiverse proponents never emphasize the obvious fact – that no such accurate theory currently exists !
   Somehow, nobody seems to hold them down to this important point.

5. **Patrick Orlando**
   March 12, 2018

   I just finished reading Nima's slides, very good, he really has a way of getting to the heart of the subject. I only have a B.S. in Physics from Columbia but I learned a lot from his slides. Again Excellent job.

6. **Tim May**
   March 12, 2018

   I've enjoyed Nima Arkani-Hamed's talks for many years now. I especially liked the 5-part Messenger Lectures at Cornell, which I've watched twice now. (And three times for Parts 4 and 5, the more speculative pay-off at the end.)

   I haven't seen much snark nor ad hominem from him. Like Edward Witten, he seems to stay above this.

   I favor that kind of avoidance of trash-talking. I'm skeptical that string theory is a theory of everything, but I think it good that some people like Arkani-Hamed and Witten are working at a high level on this and related ideas. (And pretty clearly Witten has contributed a lot to math, even if the experiments to test string theory have not worked out too well, at least not as of yet….and probably not in any of our lifetimes, as near as I can tell.)

7. **Peter Woit**
   March 12, 2018

   I should make clear that the only one I'm accusing of being snarky here is myself.

8. **katzeee**
   March 13, 2018

   Although one shouldn't take analogies too far, I find it interesting to compare String Theory to the Roman Empire, the single dominating Power in its sphere of influence for a very long time…

9. **Philip**
   March 13, 2018

   Peter, if you had to direct someone to a single, undergrad level, explanation of the weakness of the String Theory concept and the Multiverse what resource would you recommend?

10. **tulpoeid**
    March 13, 2018

    Peter,

    The seemingly increased number of people who try to make it appear as if you are the only person defacing string theory and the like for incomprehensible reasons, only speaks about their anxiety and lack of arguments.

    Or maybe they really leave in their shell, totally blind of the fact that about half of particle physicists (including both flavours) wouldn't give a cent for their toys.

    The problem is of course that devoting our time into defense against fake science will (i) take time from working on the good stuff, (ii) will not buy us any recognition or academic status, (iii) it will most probably do the opposite. So, to an outsider it might appear correct that there are no other opponents, which couldn't be further from the truth. It's just that the rest of us don't manage to gather enough self-sacrificial courage to go through all this on a consistent basis (I'm slightly exaggerating of course, since there are several others on it both in the blogosphere and in their local groups). The problem lies with us though, it's a full-blown battle for the future of physics and by staying out we already pick a side.

11. **Gregor**
    March 13, 2018

    Ress writes "We'd expect far more galaxies located unobservably beyond the horizon." Peter, this is indeed a point that many astrophysicists make. And indeed, it is a misleading or non-scientific statement. Why then is it so widespread?

12. **Chris Oakley**
    March 13, 2018

    You do not need to be a genius to realise that anthropic reasoning is a cop-out. I expect that grant-giving bodies are well aware of the fact and soon this will have a tangible effect. The advantage of Peter continuing the fight rather than, say, me is (i) he has a foot in the academic door and (ii) he writes well. In some of the long online conversations that have involved him I tend to skip the oft-rambling diatribes of his adversaries and read only what he has written himself. Sticking to the point is key.

13. **Peter Woit**
March 13, 2018

Philip,
There's my book, Lee Smolin's, and Jim Baggott's "Farewell to Reality". Also entries on the FAQ page here, and endless blog postings in the "Multiverse Mania" category.

About the multiverse though, perhaps the best advice to anyone is to just pick up any pro-multiverse argument and try and find a well-defined theory and evidence for it (you won't find any). Someone should write a book or article explaining why the multiverse is pseudo-science, but the problem with this is that it would mainly just consist of noting that multiverse advocates have no viable theory or evidence for one.

Relevant here (and also to tulpoeid's comment) is the quote I found here

http://quillette.com/2016/02/15/the-unbearable-asymmetry-of-bullshit/

""The amount of energy necessary to refute bullshit is an order of magnitude bigger than to produce it."

14. **Peter Woit**
March 13, 2018

Gregor,

I don't think it's a non-scientific statement. We have a well-defined, tested model that implies existence of objects we can't observe. It is scientifically reasonable to say that we have indirect evidence for such objects. The problem with the same argument in the multiverse case is that we don't have a sensible model with any evidence for it.

15. **Michael**
March 13, 2018

Dear Peter

Do you conduct research in particle physics/cosmology? If not what areas do you work in? While I have a certain sympathy (though rarely total agreement) for a lot of what you say here, I find it somewhat depressing and uninspiring to constantly read criticisms without any positive contributions offered. As they say, its easy to criticise.

I would love to read about some of your research, in addition to your concerns regarding the direction of post 70s theoretical high energy physics. A nice example is Scott Aaronsons blog, where he strikes a fantastic balance between discussions of technical research issues, and more controversial opinion pieces.

16. **Shantanu**
March 13, 2018

Peter, have you or others asked string theorists " what happened to predictions made pre-LHC era that detection of supersymmetry will vindicate string theory?"
If not, I suggest all of you do this the next time you attend a talk on string theory

17. **Peter Woit**
March 13, 2018

Shantanu,
The answer they will give to this question is clear: low energy SUSY was always a "prediction" in the sense of a possibility, not a sure thing, and, in general, our use of the term "prediction" isn't quite the standard one…

18. **Peter Woit**
March 13, 2018

Michael,
This is off-topic and adding to my time wasting problem by discussing it more here doesn't really help, but a few comments:

Most of my time is spent not on this, but on working these days on a project that I started writing about long ago here:
http://www.math.columbia.edu/~woit/brstdirac.pdf
I hope to have a vastly expanded and improved version of that later this year, and will do some blogging about it then, but it's clear that for something like this, what's needed is a well-written paper, not blog entries. No, I don't do research in cosmology, and don't actually blog about it that much other than to try and help cosmologists from having their field overrun by pseudo-science.

Today though I'm spending much of my time going to talks in this workshop

http://www.math.columbia.edu/department/probability/CFT18/CFT18.html

hope to write more about this in another blog entry.

I'd love to be as productive as Scott Aaronson, but that is never going to happen.

19. **Philip**
March 13, 2018

Thanks very much Peter, apologies for not looking a bit harder re FAQ etc.

20. **Douglas Natelson**
March 13, 2018

Well, Horgan declared that science is done, so he should also be well calibrated about presumptuous statements. Snark aside, it is just sad that someone with Rees' standing would imply that it's somehow wrong or inappropriate for people who aren't leading high energy theorists to be critical.

21. **Dave Miller**
March 13, 2018

Peter,

Section 9.2 of your BRST paper mentions, without comment, possible connections to the Atiyah-Singer Index Theorem. I assume this section will be expanded in the future? Do you have any ideas that will help make the proof of the Index Theorem more transparent?

It has always seemed to me that there must be a more geometrically transparent explanation of why the Index Theorem is true (i.e., without appealing to homotopies of Fredholm operators and all that).

Anyway, if you do have any insights into the Index Theorem, I hope you will write them up (and mention it here in the blog).

Dave

22. **ilovecats**
March 13, 2018

I am confused. During the peer review process, ideas are criticized by anonymous reviewers. If the author is unable to respond to the criticisms, then can the author argue back with the defense that there is no proof that the reviewer(s) (being anonymous) is(are) at least as manifestly brilliant and dedicated (old) as the author?

23. **Marshall Eubanks**
March 13, 2018

The Huns never attacked Rome. The Alaric and the Visigoths sacked Rome in August, 410 AD, and the Vandals sacked Rome in June, 455, but the Huns (in the West) never got South of the Alps.

(Peter Heather's "The Fall of the Roman Empire…" is an excellent source for this complicated period in history.)

24. **Peter Woit**
March 14, 2018

Dave Miller,
That's a problem that has always fascinated me, but I don't have anything really new to say about it. The most insightful proof of the index theorem I know of is the one inspired by Witten and supersymmetric quantum mechanics. There's a sense in which this is a TQFT, with a BRST operator (which is just the Dirac operator), so this story fits well into the story I'm trying to explain in that paper.

25. **Dave Miller**
March 14, 2018

Well, Peter, if you can just go into all the gory details on what you just summarized here (or point to a place where someone else does that), that would help.

I know of course about the Witten supersymmetric proof, but I've never been clear as to how it really works or even if it is really, properly speaking, a proof.

I have gone through the classic embedding, cobordism, and heat kernel proofs, and, while of course I did not find any errors, I was reduced to just seeing if each line followed from the previous lines: i.e., the proofs did not really click for me. I get the impression that there are an awful lot of people in the same situation.

It seems to me there must be some approach to the Index Theorem of the sort all of us physicists take to Stokes' theorem and the divergence theorem: for both of those theorems, we draw the obvious diagrams and point out that the contributions from all the internal lines (for Stokes' theorem) or internal surfaces (for the divergence theorem) are counted twice with opposite signs and therefore cancel. It is then "obvious" that those theorems are true, and a rigorous proof is just doing all of this much more carefully.

Maybe the supersymmetric approach is the equivalent way to approach the Index Theorem, but I have yet to find some place where that is explained at a physicist's level.

Thanks.

Dave

26. **Rollo Burgess**
March 16, 2018

Roger Penrose is just as critical of String Theory etc. He's not dismissed as being presumptuous, presumably because he's a grandee/bigwig. So I guess he's tacitly dismissed as being past it…

27. **DrDave**
March 18, 2018

I think it is a fair point that there is some sort of implied hierarchy about aiming only at PW and LS…I see now that LS is mysteriously redacted (with no changelog that I can see).
"I think it's important that some people should continue to tackle this "Everest problem" – to seek a testable theory from many perspectives.. It's presumptuous (as some people like Peter Woit have done) to deride the way some manifestly brilliant people choose to dedicate their scientific lives. [See Woit's response to this comment.] "
This redaction creates this decidedly weird sentence "some people like Peter Woit…"
It's nice that the link to this blog is inserted, but this doesn't seem to me to be acceptable journalistic practice. Is it a quote? Is the whole thing edited with bits removed to change the meaning?
As far as criticism (let's all stay away from the term "derision" which is singularly inappropriate), few statements are as flatly critical as Glashow's comment in The Elegant Universe series. I watched this again after many years and was struck by the clarity of Sheldon's comments, I wish they had included more–if you haven't seen it the link is here:
https://www.youtube.com/watch?v=ESrHsoV4AAY&feature=youtu.be&t=16m30s

28. **Marion Delgado**
March 18, 2018

When I first read this blog, I was fresh out of physics undergrad and grad math that included math physics. I went to a lot of science presentations then, including some that involved string/brane/M theory and brought friends to them. I also watched many more presentations online. Over the course of time I pretty much started skipping anything that was too string theory-based, and some of the credit for that has to go to Peter Woit and Lee Smolin. I have to add, however, that some of it goes to Michio Kaku and Lubos Motl.

Recent Developments in Constructive Field Theory

March 13, 2018
Categories: Uncategorized

This week there's a mini-workshop here at Columbia organized by the probabilists, on Recent Developments in Constructive Field Theory. I'll be attending some of the talks, will write more here if I can come up with something constructive to say.

Update: One of the talks today was Sourav Chatterjee on Yang-Mills for Probabilists, explaining what Yang-Mills and lattice gauge theory are, along the lines of this preprint. The discussion brought back memories of my grad school days and early research, but I fear didn't give much cause for optimism that there will be much progress anytime soon on a rigorous understanding of 4d Yang-Mills theory.

Update: Today Scott Sheffield gave a talk on *Gauge theory and the three barriers*. He started with the initial motivation of finding a continuum theory of random surfaces giving an alternative representation of gauge theory in 4d (this goes back to Polyakov and others around 1980). There's not much progress on this, but the question has inspired a wealth of results involving in some sense random surfaces. For some details of all of this, one place to look is the website of Sheffield's recent graduate course.

Comments

1. **Bill**
   March 14, 2018

   Peter, what makes you say "I fear [it] didn't give much cause for optimism…"

   Isn't translating and explaining the problems clearly to mathematicians a good thing?

2. **Peter Woit**
   March 14, 2018

   Bill,
   Definitely a good thing. I just meant that, from the discussion amongst experts in the field, things haven't advanced that much since my grad student days and it seems that we're still very far from a solid understanding of basic issues about pure Yang-Mills in 4d.

3. **Abdelmalek Abdesselam**
   March 20, 2018

   Dear Peter,
   Thank you for advertising our little workshop. But, man, that was a bit harsh: "but I fear didn't give much cause for optimism that there will be much progress anytime soon on a rigorous understanding of 4d Yang-Mills theory". If this was a workshop on number theory, it would be like saying "but I fear didn't give much cause for optimism that there will be much progress anytime soon on a rigorous understanding of the Riemann Hypothesis".
   Anyway, I thought the meeting was quite exciting with lots of interesting new results and prospects for more in the near future.

4. **Peter Woit**
   March 20, 2018

   Abdelmalek Abdesselam,
   Sorry if that came across as harsh, your analogy with the Riemann hypothesis is appropriate.

   In the end, what struck me from the talks that I attended was the way the Yang-Mills question has inspired a range of new mathematics, answering other questions although not the Yang-Mills one. Maybe someday this activity will develop new ideas that will ultimately provide the needed new insight into Yang-Mills.

   Also worth pointing out is that to a large extent it seems that physicists have given up on the Yang-Mills problem (even on finding a useful heuristic calculational method, much less something rigorous). That mathematicians are willing to keep studying this and trying to find something new is encouraging.

Stephen Hawking 1942-2018

March 14, 2018
Categories: Obituaries

Front-page on every news source today is the sad report that Stephen Hawking died yesterday at the age of 76. For the best description of his scientific accomplishments, I recommend the obituary in the Guardian written by his sometime collaborator Roger Penrose.

I was going to write a little bit about one time I heard Hawking speak (or rather, his student interpret for us his speech), which was at the IAS back in the early 1980s. I just noticed though that evidently John Baez was at the same talk (he was an undergrad, I was a grad student), and describes it well here.

At the time I remember that many thought that quantum gravity would be understood within a few years, and that Hawking would not be able to live longer than another year or two, given the nature of the disease he was suffering from. It's wonderful that the second of these turned out to be so wrong.

While Hawking was already a star in the physics community back then, his celebrity with the wider public came later. Of all the scientists who over the years have achieved some degree of celebrity, I can't think of another one who so much both deserved and enjoyed the public attention.

Update: There are dozens of articles appearing discussing Hawking's life and work. One you may not have seen which I enjoyed is from Nathan Myhrvold.

Update: Another piece by someone who worked with Hawking, Marika Taylor. It includes some discussion of his views on M-theory.

Update: Hawking has inspired some new theorizing from Niall Ferguson.

Comments

1. **Shantanu**
   March 14, 2018

   Peter, do you know of Hawking's views on string theory, string wars, his impact on particle physics etc and whether he was aware of your book.

2. **Peter Woit**
   March 14, 2018

   Shantanu,

   I have no idea how much, given his condition, Hawking was able to stay informed about topics other than what was relevant to what he was working on (which would have been hard enough). So, no idea how much he knew about string theory and its wars, or particle physics. In later years his name appears as the author or co-author of dozens of books and I've always wondered how much he could have been involved in some of them.

   One I really disliked, The Grand Design, I wrote about here
   http://www.math.columbia.edu/~woit/wordpress/?p=3141
   Much of it was the usual M-theory/multiverse propaganda and Hawking seems to have liked the use of this as a counter-argument to theological arguments from design. I have no idea to what degree Hawking was a well-informed enthusiast for the dubious science described in the book, or whether he might have been enlisted without knowing much about it.

3. **Azadi**
   March 14, 2018

   I was very sad to read about his passing in the news.

   What an inspirational person and a rare intellect to boot.

   He will leave a lasting legacy, I'm sure.

   I must say though, the Guardian newspaper's "editorial" epitaph to him over here in the UK left a somewhat bitter aftertaste in my mouth,

   https://www.theguardian.com/commentisfree/2018/mar/14/the-guardian-view-on-stephen-hawking-the-mind-of-god

   "The death of a brilliant and complex scientist will mean we are all poorer because his mind will no longer roam the multiverses

   From his wheelchair, Hawking's mind roamed the multiverses."

   Roam the multiverses?

   A lifetime of extensive contributions to scientific theory and the media still homes in on "multiverses".

4. **Peter Woit**
   March 14, 2018

   Azadi,
   Unfortunately, as mentioned above, and see for instance here
   https://www.math.columbia.edu/~woit/wordpress/?p=347
   the Guardian's epitaph was appropriate, since Hawking was a multiverse enthusiast, and as much involved in publicity for multiverse mania as anybody.

5. **Dave Miller**
   March 14, 2018

   I attended a talk Hawking gave at Caltech, one of his first presentations in the States on his work on black-hole evaporation, in the early '70s: I've kept the handout of the draft of the paper given out to the audience (I pulled it out recently and found I still do not completely understand it).

   During the '75-76 academic year, I had the chance to meet Hawking when I was taking GR from Kip Thorne; Kip arranged for us students to attend a seminar with a dozen or so participants that Hawking gave: I had enough sense to keep my mouth shut and just listen.

I’m pleased to see that the media coverage I’ve seen so far is trying to avoid the usual “equal to Newton/Einstein” hype and instead focusing on the fact that Hawking was a truly brilliant physicist who showed extraordinary courage in dealing with a horrible disease.

Courage is admirable, and the fact that the public rightly focuses on Hawking’s extraordinary courage says something good about the public.

While it is sad that he is gone, the fact that he survived so long and achieved so much is truly uplifting.

6. **Justin**
March 14, 2018

The official account is that Hawking died after 12:00 am March 14. Your article posted on the 14th reading that he died yesterday. It is interesting that Hawking was born 300 years to the day after the death of Galileo and died on Albert Einstein’s birthday which was also coincidentally Pi day.

7. **Peter Woit**
March 14, 2018

Justin,
I saw the announcement before I went to sleep last night, a bit after midnight New York time, so his death must have happened yesterday (Tuesday) New York time. The Reuters news wire story
https://www.reuters.com/article/us-people-hawking/stephen-hawking-dies-at-the-age-of-76-press-association-idUSKCN1GQ0CX
was at about midnight here, (4 am UK time) and says Tuesday, but perhaps they are using New York time. I guess Hawking died very early in the day on the 14th, and someone got the news out immediately.

8. **"i'd teach you but i'd have to charge"**
March 18, 2018

Hi Peter, did you see this?

“Thomas Hertog, a physics professor who co-authored the paper with Hawking, said the paper aimed “to transform the idea of a multiverse into a testable scientific framework.””

http://www.businessinsider.com/stephen-hawking-paper-from-just-before-he-died-could-find-new-universe-2018-3

9. **G. S.**
March 18, 2018

Peter,

Thank you for posting the link to the Hawking obituary written by Roger Penrose. It was an honest assessment of Hawking, the man, and his contributions to physics and society.

G.S.

10. **Peter Woit**
March 18, 2018

I wrote about Hawking-Hertog here
http://www.math.columbia.edu/~woit/wordpress/?p=347
It is based on the string landscape, has all the problems of that approach (especially, no real theory), and so hasn’t predicted anything and can’t ever predict anything.

11. **Anonyrat**
March 19, 2018

Via a Bee tweet:
https://www.telegraph.co.uk/science/2018/03/18/stephen-hawking-leaves-behind-breathtaking-final-multiverse/?WT.mc_id=tmg_share_tw
Stephen Hawking’s ‘breathtaking’ final multiverse theory completed two weeks before he died

Quote: “Currently being reviewed by a leading scientific journal, the paper, named A Smooth Exit from Eternal Inflation, may turn out to be Hawking’s most important scientific legacy.”

Probably refers to this:
https://arxiv.org/abs/1707.07702

12. **Peter Woit**
March 19, 2018

Anonyrat,

“A final theory explaining how mankind might detect parallel universes was completed by Stephen Hawking shortly before he died, it has emerged.”

It’s really depressing to see that people intent on dishonestly pushing multiverse hype would exploit Hawking’s death to engage in more of this. There’s nothing in that paper that solves the problem of finding a predictive multiverse theory, much less predicting anything based upon it.

13. **Mike Hall**
March 20, 2018

Bee has just blogged about Hawking-Hertog.

Her title ‘Hawking’s “Final Theory” is not groundbreaking’. Much more sensible than the mainstream media reports (as one would expect).

Abel Prize to Langlands

March 20, 2018
Categories: Langlands

The 2018 Abel Prize has been awarded to Robert Langlands, an excellent choice. The so-called “Langlands program” has been a huge influence on modern mathematics, providing deep insight into the structure of number theory while linking together disparate fields of mathematics, as well as quantum field theories and physics.

The Abel Prize site provides a wealth of information about Langlands and his work. Davide Castelvecchi at Nature appropriately describes the Langlands program as a “grand unified theory of mathematics” (Edward Frenkel’s Love and Math popularized this description).

Many blog posts here have discussed the Langlands program and ideas that have developed out of it. For a good example of how wide the impact of these ideas has been, this week the Perimeter Institute will be hosting a conference discussing the latest work on the geometric version of the Langlands program, as well as connections to gauge theory and conformal field theory.

For the original work of Langlands himself, besides the material at the Abel site, the AMS Bulletin has recently published a long article by Julia Mueller. For the original sources and a wealth of other material written by Langlands himself, see the IAS site that collects his writings.

Comments

1. **Peter Shor**
   March 20, 2018

   Wonderful! I assume this breaks the long-standing tradition of rewarding mathematicians for theorems but not for conjectures (which can sometimes be much more important).

2. **Peter Woit**
   March 20, 2018

   Peter Shor,

   Langlands did prove some special cases of his conjectures (e.g. Jacquet-Langlands, Langlands-Tunnell, local Langlands at **R** and **C** for GL(n)), and his work was much, much more than just formulating conjectures, containing all sorts of important detailed results. It is interesting to speculate what the math community would think of someone who only formulated conjectures of this importance, but never proved anything.

3. **(((anon)))**
   March 20, 2018

   The conjectures were built on some serious theorems – classification of irreducible representations of real groups (standing on Harish-Chandra), meromorphic continuation of Eisenstein series, etc etc ….

   But yes, re-contextualising Satake’s isomorphism, that was more influential than either of the above.

   And he deserves the prize for his prose style alone.

4. **Chris Woodward**
   March 20, 2018

   I enjoyed Lafforgue’s geometrically-oriented survey of recent work
   https://arxiv.org/pdf/1803.03791.pdf

5. **Low Math, Meekly Interacting**
   March 21, 2018

   For those of us who need some help, Kevin Hartnett provides a brief but nice bit of exposition in Quanta.

   https://www.quantamagazine.org/robert-langlands-mathematical-visionary-wins-the-abel-prize-20180320/

6. **Davide Castelvecchi**
   March 21, 2018

   I second Peter Shor’s comment! I am old enough to remember a joke that went around math departments in the 1990s that with Bill Thurston you could now get a Fields Medal just for conjecturing something — and that with Edward Witten you didn’t even need your conjecture to be clearly stated. Although that was of course a bit unfair in both cases 🙂

This Week’s Hype

March 20, 2018
Categories: This Week's Hype

Many thanks to Sabine Hossenfelder for her efforts to debunk the attempt to use Hawking’s death as a platform for multiverse hype. See her [posting at Backreaction]() for a good explanation of what is going on here.

To summarize the problem, there are loads of news stories out there telling the public that [Stephen Hawking’s ‘breathtaking’ final multiverse theory completed two weeks before he died](), [Stephen Hawking’s Final Paper Proposes Way to Detect the ‘Multiverse’](), etc., etc. Cosmologist Carlos Frenk and theorist Thomas Hertog seem to be among those encouraging this nonsense.

This is all based on [this recent paper by Hawking and Hertog](), which contains nothing like a way to “detect the ‘Multiverse'”. It’s a toy model of bubble universe formation, one the authors admit they can’t even solve:

> However, the setup we have considered does not allow us to describe the transition from the quantum realm of eternal inflation to a universe in the semiclassical gravity domain. This is because our duals are defined in the UV and live at future infinity. It therefore remains an open question whether the conjectured smoothness of global constant density surfaces impacts the eternity of eternal inflation. To answer this will require a significant extension of holographic cosmology to more realistic cosmologies

Their calculations inspire them to state: “… we conjecture that eternal inflation produces universes that are relatively regular on the largest scales”, but this is just an extremely vague conjecture without much backing it. Using it to get press stories published claiming to have found a way to “detect the ‘Multiverse'” is just absurd, and it’s sad to see Hawking’s passing memorialized with a cloud of ridiculous hype.

Update: Another detailed explanation of what is going on here, from [Ethan Siegel](). His summary:

> There are no observable consequences; there is nothing to measure; there is nothing to test. There’s no prediction about the end of the Universe, and there are no robust conclusions we can draw about its beginning. There are tremendous limitations to the implications of this work, and there are few compelling reasons to believe that their toy model has relevance for our physical Universe. It is a seed of an idea that itself is controversial, based off of an also-controversial foundation, and this is a very small step in its development. Furthermore, all of what they do is based on the Hartle-Hawking no-boundary conjecture, which is still not generally accepted as true. The authors go so far as to admit, in the discussion of this paper, that even within their toy model, they have not shown that there is a non-Multiverse-inducing exit to eternal inflation:
>
> “It therefore remains an open question whether the conjectured smoothness of global constant density surfaces impacts the eternity of eternal inflation.”

Comments

1. **Robert Gauthier**
   March 20, 2018

   Right up there with claims by certain religious groups that Hawking, an avowed atheist, had a death-bed conversion to Christianity.

2. **atreat**
   March 21, 2018

   I think this one should win the theoretical physics equivalent of the Razzie Awards for the years best (most shameless) hype.

3. **Low Math, Meekly Interacting**
   March 21, 2018

   I’ve lost all trust in sound (or text) bites culled from interviews without seeing the full transcript, so I’m not sure what to conclude about Hertog. It’s common to see mountains made of molehills, but can’t remember so much popsci press about literally nothing in quite a while. Powerful is the dead celebrity.

   I would have figured a claim that the multiverse is perhaps less profligate and more prosaic than some models suggest could fuel plenty of Mania without the added layer of fabrication. Apparently that’s not enough for the necrophiles out there.

4. **Anonymous**
   March 21, 2018

   “Right up there with claims by certain religious groups that Hawking, an avowed atheist, had a death-bed conversion to Christianity.”

   I would argue that Hertog’s claims are worse than the death-bed conversion stories. First of all, nobody takes those deathbed conversion stories seriously. They don’t undermine who the person portrayed themselves as being. Hawking’s earlier works used a lot of language that indicated he was open to some kind of deism or pantheism (see A Brief History of Time). Later on, he disavowed himself of any religious belief or agnosticism and claimed that his physics research completely ruled out the possibility of a belief in God. So he was an atheist. He knew he was, we knew he was.

   This paper, however, is crap and it undermines people’s confidence in the scientific enterprise. Nobody really cares if you believe in God. Some physicists do, some physicists don’t, and many simply just don’t care one way or the other. This bizarre, and bogus, multiverse claims subject the scientific discipline to ridicule. It’s worse than bad science fiction, because we are at a crossroads in society where there is a real risk that people will stop believing in science.

5. **Peter Woit**
   March 21, 2018

   LMMI,
   From the press stories, hard to tell what Hertog’s role in this is. But Hossenfelder’s posting has an extensive quote from him (not sure of source, email to her?) which explicitly makes the sort of claims that led to the bogus headlines.

6. **Blake Stacey**
   March 21, 2018

It’s rather weird how a paper whose abstract says “the exit from eternal inflation does not produce an infinite fractal-like multiverse” becomes a *whoooo multiverse!* story. Sure, they’re arguing for a multiverse of their own flavor (“a much more limited set of possible universes”), but from a distance, it looks like a bucket of cold water on the idea that an extravagance of bubble universes is the natural and inevitable conclusion from orthodox cosmology. The headline could just as well have been, “In final paper, Hawking cuts multiverse down to size”.

What is Real?

March 22, 2018
Categories: Book Reviews, Quantum Mechanics

There's a new popular book out this week about the interpretation of quantum mechanics, Adam Becker's *What is Real?: The Unfinished Quest for the Meaning of Quantum Physics*. Ever since my high school days, the topic of quantum mechanics and what it really means has been a source of deep fascination to me, and usually I'm a sucker for any book such as this one. It's well-written and contains some stories I had never encountered before in the wealth of other things I've read over the years.

Unfortunately though, the author has decided to take a point of view on this topic that I think is quite problematic. To get an idea of the problem, here's some of the promotional text for the book (yes, I know that this kind of text sometimes is exaggerated for effect):

> A mishmash of solipsism and poor reasoning, Copenhagen claims that questions about the fundamental nature of reality are meaningless. Albert Einstein and others were skeptical of Copenhagen when it was first developed. But buoyed by political expediency, personal attacks, and the research priorities of the military industrial complex, the Copenhagen interpretation has enjoyed undue acceptance for nearly a century.

The text then goes to describe Bohm, Everett and Bell as the "quantum rebels" trying to fight the good cause against Copenhagen.

Part of the problem with this good vs. evil story is that, as the book itself explains, it's not at all clear what the "Copenhagen interpretation" actually is, other than a generic name for the point of view the generation of theorists such as Bohr, Heisenberg, Pauli, Wigner and von Neumann developed as they struggled to reconcile quantum and classical mechanics. They weren't solipsists with poor reasoning skills, but trying to come to terms with the extremely non-trivial and difficult problem of how the classical physics formalism we use to describe observations emerges out of the more fundamental quantum mechanical formalism. They found a workable set of rules to describe what the theory implied for results of measurements (collapse of the state vector with probabilities given by the Born rule), and these rules are in every textbook. That there is a "measurement problem" is something that most everyone was aware of, with Schrodinger's cat example making it clear. Typically, for the good reason that it's complicated and they have other topics they need to cover, textbooks don't go into this in any depth (other than often telling about the cat).

As usual these days, the alternative to Copenhagen being proposed is a simplistic version of Everett's "Many Worlds": the answer to the measurement problem is that the multiverse did it. The idea that one would also like the measurement apparatus to be described by quantum mechanics is taken to be a radical and daring insight. The Copenhagen papering over of the measurement problem by "collapse occurs, but we don't know how" is replaced by "the wavefunction of the universe splits, but we don't know how". Becker pretty much ignores the problems with this "explanation", other than mentioning that one needs to explain the resulting probability measure.

String theory, inflation and the cosmological multiverse are then brought in as supporting Many Worlds (e.g. that probability measure problem is just like the measure problem of multiverse cosmology). There's the usual straw man argument that those unhappy with the multiverse explanation are just ignorant Popperazzi, unaware of the subtleties of the falsifiability criterion:

> Ultimately, arguments against a multiverse purportedly based on falsifiability are really arguments based on ignorance and taste: some physicists are unaware of the history and philosophy of their own field and find multiverse theories unpalatable. But that does not mean that multiverse theories are unscientific.

For a much better version of the same story and much more serious popular treatment of the measurement problem, I recommend a relatively short book that is now over 20 years old, David Lindley's *Where does the Weirdness Go?*. Lindley's explanation of Copenhagen vs. Many Worlds is short and to the point:

> The problem with Copenhagen is that it leaves measurement unexplained; how does a measurement select one outcome from many? Everett's proposal keeps all outcomes alive, but this simply substitutes one problem for another: how does a measurement split apart parallel outcomes that were previously in intimate contact? In neither case is the physical mechanism of measurement accounted for; both employ sleight of hand at the crucial moment.

Lindley ends with a discussion of the importance of the notion of decoherence (pioneered by Dieter Zeh) for understanding how classical behavior emerges from quantum mechanics. For a more recent serious take on the issues involved, I'd recommend reading something by Wojciech Zurek, for instance this article, a version of which was published in Physics Today. Trying to figure out what "interpretation" Zurek subscribes to, I notice that he refers to an "existential interpretation" in some of his papers. I don't really know what that means. Unlike most discussions of "interpretations", Zurek seems to be getting at the real physical issues involved, so I think I'll adopt his (whatever it means) as my chosen "interpretation".

Update: For another take on much the same subject, out in the UK now is Philip Ball's *Beyond Weird*. The US version will be out in the fall, and I think I'll wait until then to take a look. In the meantime, Natalie Wolchover has a review at Nature.

Update: There's a new review of What is Real? at Nature.

Update: Jim Holt points out that David Albert has a review of the Becker book in the latest New York Review of Books. I just read a print copy last night, presumably it should appear online soon here .

Update: Some comments from Adam Becker, the author of the book.

> I won't try to rebut everything Peter has said about my book—there are some things we simply disagree about—but I would like to clear up two statements he makes about the book that are possibly misleading:
>
> Peter says that I claim the answer to the measurement problem is that "the multiverse did it." But I don't advocate for the many-worlds interpretation in my book. I merely lay it out as one of the reasonable available options for interpreting quantum mechanics (and I discuss some of its flaws as well). I do spend a fair bit of time talking about it, but that's largely because my book takes a historical approach to the subject, and many-worlds has played a particularly important role in the history of quantum foundations. But there are other interpretations that have played similarly important roles, such as pilot-wave theory, and I spend a lot of time talking about those interpretations too. I am not a partisan of any particular interpretation of quantum mechanics.
>
> Second, I don't think it's quite fair to say that I paint Bohr as a villain. I mention several times in my book that Bohr was rather unclear in his writing, and that sussing out his true views is dicey. But what matters more that Bohr's actual views is what later generations of physicists generally took his views to be, and the way Bohr's work was uncritically invoked as a response to reasonable questions about the foundations of quantum mechanics. It's true that this subtlety is lost in the jacket flap copy, but that's publishing for you.
>
> Also, for what it's worth, I do like talking about reality as it relates to quantum mechanics. But I suppose that's hardly surprising, given that I just wrote a book on quantum foundations titled "What Is Real?". I'd be happy to discuss all of this further over email if anyone is interested (though I'm pretty busy at the moment and it might take me some time to respond).

Comments

1. **Carl Zetie**
March 22, 2018

My 13 year old son has been reading some of the introductory texts on QM and his reaction on discovering Everett was to ask me, "So Everett's Many Worlds has the same problem as the Copenhagen interpretation, plus an enormous number of additional universes that are not detectable. How is that better?".

Good kid, but I'm going to need to find him a math tutor soon because he is overtaking my undergrad education.

2. **Jim Baggott**
March 23, 2018

Give me a little more time. I hope to be able to set the record straight in a book to be published probably in 2020. You can read the preamble here: http://www.jimbaggott.com/articles/a-game-of-theories/

3. **Mateus Araújo**
March 23, 2018

Dear Prof. Woit,

I don't understand what you mean by "the wavefunction of the universe splits, but we don't know how". We know exactly how; this is the whole point of Many-Worlds: it is just normal Hamiltonian evolution. What do you think is lacking?

4. **Ben Jones**
March 23, 2018

It's fine to say that 'in either interpretation, we don't know what happens at the moment of measurement'. But I disagree that this leaves MW and Copenhagen on an equal footing. Doesn't Copenhagen posit an additional _mechanism_, that of 'collapse', whereas Everett's view wouldn't?
And are there not additional problems with Copenhagen, such as the 'selection' of a single branch is the only non-deterministic element of the whole system?
@Carl the non-detectability of the 'other universes' follows naturally from how we understand QM to work, it doesn't count as a piece of evidence against it if we wouldn't _expect_ to be able to detect them. An unreasonable swish of Occam's Razor.

5. **David Brown**
March 23, 2018

"Popperazzi" (play on Italian "paparazzi") is the preferred spelling.
https://www.philosophersmag.com/index.php/footnotes-to-plato/77-string-theory-vs-the-popperazzi "String Theory vs the Popperazzi" by Massimo Pigliucci, 2015

6. **Peter Woit**
March 23, 2018

David Brown,
Thanks. Fixed.

Mateus Araujo,
Just saying "the Schrodinger equation does it" doesn't solve the measurement problem (for instance, the preferred basis problem). All you're doing is saying that, you don't know how, but the Schrodinger equation is going to somehow give precisely the same implications for physics as the collapse postulate.

Ben Jones,
Copenhagen says collapse happens in a measurement, it's silent on what the theory of collapse is (adding specific new physics to explain collapse is not Copenhagen but something else).

7. **hdz**
March 23, 2018

Peter, I do not intend to enter the details of your discussion, since I have got tired about it. (If somebody should be interested, he/she may look at my website http://www.zeh-hd.de – especially the first two papers under "Quantum Theory".) However, as a historical remark let me point out that in my understanding, von Neumann and Wigner were never part of the Copenhagen interpretation – rather they objected to it more to less openly (starting in Como). When Wigner used the term "orthodox interpretation", he exclusively meant von Neumann's book (including the collapse as a physical process – not just a "normal" increase of information), and I am told that he complained that, as a consequence, he was never invited to Copenhagen. Bohr always disagreed with any attempt to analyze the measurement problem in physical terms.
Essentially, I agree with Mateus Aroujo and and Ben Jones
Regards, Dieter

8. **Jim Baggott**
March 23, 2018

This kind of discussion can get hopelessly confused very quickly. To my knowledge, the 'collapse of the wavefunction' was never part of the Copenhagen interpretation, which is based on some kind of unexplained 'separation' between the quantum and classical realms, or what John Bell would refer to as the 'shifty split'. I believe the notion of a 'collapse' was introduced as a 'projection postulate' by von Neumann in his book Mathematical Foundations of Quantum Mechanics, first published in German in 1932 (in my English translation the projection postulate – a statistical, discontinuous process in contrast to the continuous, unitary evolution of the wavefunction – appears on p. 357). Influenced, I believe, by Leo Szilard, von Neumann speculates that the collapse might be triggered by the intervention of a (human) consciousness – what he refers to on p. 421 as the observer's 'abstract ego'.

All this really shouldn't detract from the main point. The formalism is the formalism and we know it works (and we know furthermore that it doesn't accommodate local or crypto non-local hidden variables). The formalism is, for now, empirically unassailable. All *interpretations* of the formalism are then exercises in metaphysics, based on different preconceptions of how we think reality could or should be, such as deterministic ('God does not play dice'). Of course, the aim of such speculations is to open up the possibility that we might learn something new, and I believe extensions which seek to make the 'collapse' physical, through spacetime curvature and/or decoherence, are well motivated.

But until such time as one interpretation or extension can be demonstrated to be better than the other through empirical evidence, the debate (in my opinion) is a philosophical one. I'm just disappointed (and rather frustrated) by the apparent rise of a new breed of Many Worlds Taliban who claim – quite without any scientific justification – that the MWI is the only way and the one true faith.

9. **Mateus Araújo**

March 23, 2018

Peter Woit,

There are dozens of papers explaining how you can model the measurement process in unitary quantum mechanics via entanglement and decoherence; Zeh's and Żurek's, among them. It's not as if anybody is saying "the Schrödinger equation did it" without explaining how.

And the measurement problem is usually defined as the incompatibility between collapse and unitary evolution. So even just saying "the Schrödinger equation did it" does solve the problem, on this level. Of course, one must also explain emergence of classicality, permanence of records, probability, etc., but this is not the measurement problem.

10. **Peter Woit**
March 23, 2018

hdz,

Thanks Dieter.

I should point out that Dieter Zeh is one of the major subjects of the book. He's perhaps the best example of the story Becker wants to fit everything into: a "quantum rebel" who made significant advances in our understanding of foundations and the measurement problem, advances which were not initially recognized due to an entrenched "Copenhagen" ideology that denigrated any such work. Partly due to his work, I think for many decades now mindless Copenhagen ideology has not been such a problem (but we're well on our way to mindless Many Worlds ideology being a major problem).

As he notes and the book describes, there's controversy over who was on the Copenhagen bus, partly because it was never clear exactly what the Copenhagen Interpretation was, and partly because many people took somewhat different points of view at different times.

11. **Peter Woit**
March 23, 2018

Mateus Araújo,
To me the (non-trivial) measurement problem is exactly the "emergence of classicality, permanence of records, probability, etc.," problems you list, with entanglement and decoherence some of the insights needed to solve them. If you define the measurement problem as you do, then the solution you give by having quantum mechanics apply to the macroscopic world is a trivial one which most everyone expected anyway.

12. **Mateus Araújo**
March 23, 2018

Peter Woit,

I'm claiming that this is not "my" definition of the measurement problem, but rather *the* standard definition. See for example Maudlin's "Three measurement problems", a canonical reference on the subject.

Of course, the solution *is* trivial; you merely need to give up collapse, or introduce hidden variables, or introduce physical collapse. The problem is that most people don't want to do any of these, hence they are stuck with the measurement problem.

Look, I agree wholeheartedly with you that the interesting problems are those I listed; I'm just insisting on narrow definitions and standard terminology, otherwise it becomes impossible to talk to each other.

13. **Peter Woit**
March 23, 2018

Mateus Araújo,

This whole subject is full of endless and heated arguments over problems that are meaningless and/or lacking in substance. Your definition of "the measurement problem" seems to me to insist on sticking to the substance-free aspect of a substantive problem, thus encouraging substance-free-discussion. What's the correct term to refer to the substantive problem?

By the way, on the meaningless side, Becker's book has a lot about "What is Real?" and claims that the problem with Copenhagen is that it denies the reality of the microscopic world, a discussion I thought it best to just ignore.

14. **Mateus Araújo**
March 23, 2018

Peter Woit,

Indeed it is! And I think a great many heated and meaningless arguments happen because the parties are talking about different things =)

But I don't know which problem you refer to as "the substantive problem". Do you have in mind a collective name for the three I listed? I don't think there exists a standard name for this set, except maybe as "problems of the Many-Worlds interpretation". They are different problems, that are usually studied separately, and referred to by their individual names.

15. **Peter Woit**
March 23, 2018

Mateus Araújo,
I guess I always thought "what happens to the cat?" was the substantive measurement problem, so I'm looking for a name for that.

16. **Blake Stacey**
March 23, 2018

Asher Peres once wrote that there are at least as many Copenhagen Interpretations as people who use the term, probably more. Among other problems, saying "*the* Copenhagen Interpretation" glosses over substantial differences between Heisenberg and Bohr.

> The idea that one would also like the measurement apparatus to be described by quantum mechanics is taken to be a radical and daring insight.

Despite this issue being discussed by Pauli, von Neumann, ….

17. **Mateus Araújo**
March 23, 2018

    Peter Woit,

    You want unitarity to hold, so that atoms are described by quantum mechanics, and you want collapse to happen, so that the cat is definitely dead, but you can't have both: the measurement problem!

    And the solution is again trivial, threefold. Give up on collapse: there are worlds with live cats and worlds with dead cats. Introduce hidden variables: the cat was always dead, you just didn't know it. Introduce physical collapse: the superposition collapsed on the level of the geiger counter, so that the cat was always dead.

    Maybe the measurement problem is like the problem of having your cake and eating it too. You know it's impossible, but you really really want to!

18. **Jim Baggott**
March 23, 2018

    I think we can agree that this is all a lot of fun. I've been studying these problems for more than 25 years, and I can discern relatively little progress in this time. But I've come to believe that it is helpful to distinguish between 'reality' (however you want to think about it) and the reality or otherwise of the *representation* we use to describe it.

    We can likely all agree that the moon is there when nobody looks, and even that invisible entities like electrons really do exist independently of observation ('if you can spray them then they are real', as philosopher Ian Hacking once declared). But this doesn't necessarily mean that the concepts we use in our representation of this reality should be taken literally as real, physical things. If we choose to agree that the wavefunction isn't real (as Carlo Rovelli argues in his relational interpretation), then the QM formalism is simply an algorithm for coding what we know about a physical system so that can make successful predictions. All the mystery then goes away – there is no problem of non-locality, no collapse of the wavefunction, and no 'spooky' action at a distance.

    I don't particularly like this interpretation, as it is obviously instrumentalist and can't answer our burning questions about how nature actually does that. But it does help to convince me that this endless debate over interpretation is really a philosophical debate, driven by everybody's very different views on what 'reality' ought to be like. And, as such, we're unlikely to see a resolution anytime soon…

19. **Peter Woit**
March 23, 2018

    Jim,
    I'd rather do almost anything with my time than try and moderate a discussion of what is "real" and what isn't.

    Any further discussion of ontology will be ruthlessly suppressed.

20. **Per Östborn**
March 23, 2018

    Trying to stick to physical issues, I have always wanted to see proponents of the many worlds interpretation derive the spectrum of the hydrogen atom from some clearly defined postulates (or solve some other basic physical problem). Until then it is not clear to me what the theory actually is saying. If anyone can refer to such a calculation I would be glad. Clearly it is not enough to postulate the unitary evolution of the Schrödinger equation, but I have never been able to pinpoint the other postulates of the many worlds interpretation.

21. **George Herold**
March 23, 2018

    Thanks for the nice post and discussion. I'm an experimentalist, who is more comfortable with opamps than operators. I have perhaps a naive question; Do any of these books discuss the DeBroglie-Bohm (Pilot wave) theory? Since all these interpretations are just a matter of taste, I find this hidden variable type theory easiest to swallow. In a double slit, the particle 'knows' about both slits, but still goes through (only) one of them. Is there any reason not to be happy with this picture?

22. **Peter Woit**
March 23, 2018

    Per Östborn,
    The calculation in Many Worlds is exactly the same textbook calculation as in Copenhagen. It's the same Schrodinger equation and you solve for its energy eigenvalues the same way. That is the problem: there's no difference from the standard QM textbook.

    The Many Worlds people might claim as an advantage over Copenhagen that you can imagine doing much harder calculations about how a Hydrogen atom interacts with its environment during a measurement process, giving insight into the question of why we see energy eigenstates and the Born rule. My point of view would be that Copenhagen never said you shouldn't do exactly those calculations if you wanted to better understand what was happening during a measurement, it was just a rule for when you couldn't do such calculations.

23. **Peter Woit**
March 23, 2018

    George Herold,

    Becker's book has a long and detailed section about the story of Bohm and Bohmian mechanics which you might find of interest. Personally I don't find Bohmian mechanics compelling, both for reasons Becker mentions and for others having to with the much greater mathematical simplicity of the conventional formalism when applied to our best fundamental theories.

    Sorry, but I really don't want to carry on more discussion here of Bohmian mechanics. I realize a lot of people are interested in it, but I'm not at all interested, so such a discussion should be conducted elsewhere.

24. **Lee Smolin**
March 23, 2018

    Dear Peter and all,

Can I suggest a basic distinction between different approaches to quantum foundations?

The first class hypothesizes that the standard quantum mechanics is incomplete as a physical theory, because it fails to give a complete description of individual phenomena. If so, the theory requires a completion, that is a theory which incorporates additional degrees of freedom, and/or additional dynamics. Pilot wave theory and physical collapse models are examples of these.

The second class of approaches take it as given that the theory is complete, so the foundational puzzles are to be addressed by modifying how we interpret the equations of the theory.

People engaged in the first class of approaches are trying to solve a very different kind of research problem than those in the second class. I would submit that both are worth pursuing, but that progress in physics will eventually depend on the success of the first kind of approach.

25. **Peter Woit**
March 23, 2018

Thanks Lee,
That's a very useful distinction. The Becker book contains a lot of material about such attempts to complete quantum mechanics with new physics that would entail a different resolution of the measurement problem. I didn't discuss these here, largely because I'm much less optimistic than you that the search for this kind of completion will be fruitful.

26. **Per Östborn**
March 23, 2018

Dear Peter,
It is not clear to me, but my impression is that MWI proponents claim that some of the standard postulates of QM can be removed (or replaced by other ones). Then their calculations from first principles must be different from the textbook ones, since they are forbidden to use the removed postulates, of course.

As you write, they seem to want to gain insight into why we see energy eigenvalues and why we should use the Born rule. This suggests that they argue that these features of QM can be derived from a smaller or different set of postulates than the standard one.

27. **Peter Woit**
March 23, 2018

Per Östborn,

I'm no expert and don't want to get into the details of exactly what "Many Worlds" is, I think there are many versions. One aspect of it though is yes, the idea that you can derive the relation to classical observables and the Born rule, not postulate them. There's active research and debate on the extent to which you really can do this. I just want to point out that you naturally ask exactly the same questions in Copenhagen, whenever you decide you want to analyze in more detail what's happening during a measurement. It's exactly the same equation and physics.

28. **Edward**
March 23, 2018

I think the most interesting Copenhagen/anti-Copenhagen split was between Heisenberg, Bohr, and others on the one hand and Einstein, Schrodinger, and to some extent de Broglie on the other. There was a generational split and Heisenberg's views may have prevailed because the older physicists died off.

29. **Narad**
March 23, 2018

> Any further discussion of ontology will be ruthlessly suppressed.

I think I'm going to have to have that printed on a T-shirt.

30. **Chris W.**
March 23, 2018

I think it would be fair to say that the acceptance of the Copenhagen interpretation, or what various people took it to be, was substantially a function of the high regard in which Bohr and Heisenberg were held as pioneers in the development of quantum mechanics, combined with a strong desire to just "get on with it"—find a way to state problems and do calculations that most people felt they understood well enough to do research and make sensible progress.

Later, when a great deal of research had been done and generally accepted progress had been made, it's understandable that the "yes, but…" questions about quantum theory would be resurgent, especially with the conundrums of quantum gravity looming in the background, along with the growing exploration of quantum phenomena at mesoscopic scales.

To my mind the current situation is reminiscent in some ways of Mach's objections to the conventional understanding of Newtonian dynamics at a time (the mid- to late 19th century) when such concerns had little apparent significance for most working scientists. All this appeared in a different light with the advent of special and general relativity.

31. **Mateus Araújo**
March 24, 2018

Peter Woit,

I think this is pretty much correct; one can also simply postulate the Born rule in Many-Worlds, as done in single-world theories, but it feels a bit wrong, as one should also explain what probabilities are. Hence the interest in deriving the Born rule in Many-Worlds, as this should shed some light on the issue. One can, though, adapt these derivations also to single-world quantum mechanics, as done by Saunders here.

I don't think, however, that the derivations we have are completely satisfactory, and I'm personally trying to improve on them.

32. **Peter Shor**
March 24, 2018

David Mermin, who didn't particularly like the Copenhagen interpretation, found himself driven to use it when he was teaching quantum mechanics to computer scientists who didn't know much about physics. (See his article *Copenhagen Computation*.)

Bohr, at Copenhagen, similarly was teaching quantum mechanics to physicists who didn't know much about quantum mechanics. So maybe this explained why he gravitated towards the Copenhagen interpretation.

And then maybe Bohr came up with all this weird extraneous philosophy to try to convince himself that what he was teaching them actually made some sense.

33. **Paddy**
March 24, 2018

Jim Baggott (or anyone else for that matter):

You attribute "collapse" or reduction of the wavepacket to von Neumann (1932). A clear statement (albiet much shorter) is in Dirac's Principles, Sect 10. Unfortunately our University Library foolishly left the first edition (1930) on the open shelves, and it has long since vanished, so I've only traced this statement back to the 2nd ed… My question to anyone who can lay hands on a copy is, is this bit in the original edition, or did Dirac add it after reading von Neumann?

As I probably learned from one of Jim's books, the "reduction" of the wavepacket
is Heisenberg's terminology, from the uncertainty principle paper, so von Neumann or Dirac were in any case just sharpening up Heisenberg's insight.

34. **Tim Maudlin**
March 24, 2018

It is a bit hard to know how to comment on a discussion of a book called "What is Real?" when it has been asserted that

"I'd rather do almost anything with my time than try and moderate a discussion of what is "real" and what isn't.

Any further discussion of ontology will be ruthlessly suppressed."

The question "What is real?" just is the question "What exists?" which is in turn just the question "What is the true physical ontology?" which is identical to the question "Which physical theory is true?". Peter Woit begins by writing "Ever since my high school days, the topic of quantum mechanics and what it really means has been a source of deep fascination to me…". But that just is the question: What might the empirical success of the quantum formalism imply about what is real? or What exists? or What is the ontology of the world? To say you are interested in understanding the implications of quantum mechanics for physical reality but then ruthlessly suppress discussions of ontology is either to be flatly self-contradictory or to misunderstand the meaning of "ontology" or of "real". That is also reflected in the quite explicit rejection of any discussion of two of the three possible solutions to the Measurement Problem: pilot wave theories and objective collapse theories.

Has Foundations made real progress since Copenhagen? Absolutely! We have two key theorems: Bell's theorem and the PBR theorem. The first tells us that non-locality is here to stay, so Einstein's main complaint about the "standard" account—namely its spooky action-at-a-distance—cannot be avoided. Anyone who thinks that they have a way around it is mistaken. That lesson has not yet been learned, even though Bell's result is half a century old. PBR proves that the wavefunction assigned to a system reflects some real physical aspect of the individual system: two systems assigned different wavefunctions are physically different. So if Carlo Rovelli or the QBists or Rob Spekkens thinks that the wavefunction isn't real, in the sense that it does not reflect some real physical feature of an individual system, then PBR has proven them wrong. Bell and PBR lay waste to many approaches to understanding quantum theory, including Copenhagen and QBism. Jim Baggot suggests that we can "choose to agree" that the wavefunction isn't real and somehow thereby eliminate non-locality and spooky action at a distance. No you can't, for reasons given by both Bell and PBR. A theorem is a theorem, and we are not free to ignore it.

The choices are: additional (non-hidden!) variables (e.g. Bohm); objective collapses (e.g. GRW); Many Worlds (e.g. Everett). That's it. Anything else has been ruled out by the empirical success of the quantum predictions. And this is no more a "philosophical" debate than any other dispute in physics is. It is a debate about what the correct physical understanding of the world is. Only once these real advances in our understanding have been generally acknowledged will we be in a position to make further progress.

35. **Peter Woit**
March 24, 2018

Tim Maudlin,

Your comment has some similarities to Lee Smolin's, wanting to focus on the "real" question of whether QM is all there is (his "second class", your "Everett" choice) or new physics is needed (his "first class", your Bohm or GRW). While there's a lot in Becker's book about the history of "new physics" proposals, and you and Smolin are quite right that this is a true fundamental question, more significant than empty discussions of "interpretations" corresponding to identical physics, the problem here is that I'm just not interested. As with any proposals for "new physics", people make their own evaluations based on different experiences, and it's a good thing that others who see things differently think more about such proposals.

This is a case where no such new physics proposals come with any experimental evidence in their favor, so personal criteria of what is worth spending time on are all one has. My own criteria for paying attention to speculative proposals weight heavily the mathematical structures involved. The proposals I've seen for supplementing QM invoke mathematical structures that to me seem quite a bit more complicated and ugly than the ones of QM, thus my lack of interest. Maybe someday someone will come up with a different proposal that's more appealing. Til then, I'll keep deciding not to spend more time thinking about such things.

36. **hdz**
March 24, 2018

I do not always agree with Tim Maudlin, but in this case I do almost completely. Clearly, the philosophical debate about reality is worthless for physicists, but physicists usually (and tacitly) understand it in the sense of a conceptually consistent description of "Nature" (of what we observe with our senses). So it is certainly not compatible with complementarity. Of course, everybody is free to propose new concepts and theories, but I have decided to wait until such author presents some empirical success (what else can I do?). Mathematical structures have to be formally consistent, Peter, but that is no sufficient argument for their application to the empirical world. (Just consider Tegmark's unreasonable Level IV of multiverses.) This is not only an argument against Sting Theory! (I have written a comment against Strings (or M-theory) in German long before "Not even wrong" appeared; it can be found on my website under "Deutsche Texte".)

So my conclusion from Tim's three choices are that the first two ones are well possible (though not more), but we have to wait for empirical support. Then only Everett remains (until being falsified) as yet as a global unitary theory. Let me add that the concept of decoherence, which has clearly been experimentally confirmed, was derived from an extension of unitarity to the environment, while Everett is its further extension to the observer. So why is it "unmotivated" or incomplete? In my opinion there are no arguments but only emotions against Everett! So please take some time to study my website!

37. **a1**
March 24, 2018

Thanks Peter for pointing that the MWI is basically an inversion of the standard one, an evidence rarely mentioned. So the 'real' problem remains untouched: what are probabilities? Actually, surprisingly few physicists seem to be aware of the existence of Bertand's pardox, something which has disturbing implications. The stance 'it doesn't matter what probabilities are as long as we know how to calculate them' or saying 'there is an axiomatic for them' are just ways to avoid the question. Does something physical collapse or is it our ignorance that ends in a measurement? It is rather obvious that a meaningful distinction between a description and its referent (to avoid saying 'reality') can become badly twisted when it is not clear what is not known.

38. **Dave Miller**
March 24, 2018

Peter Shor,

You wrote:

> David Mermin, who didn't particularly like the Copenhagen interpretation, found himself driven to use it when he was teaching quantum mechanics to computer scientists who didn't know much about physics.

Does *anyone* ever teach Intro QM without implicitly using the Copenhagen interpretation?

Or, to put it the other way around, does anyone teach Intro QM using MWI or the Bohm model?

To connect with your own work, it does seem to me that MWI is the "natural" way to think about quantum computing. Not that I think MWI is true: I think it has two fatal flaws — the "preferred basis" problem that Peter Woit has mentioned and the probability-measure problem.

Do you yourself see any affinity between MWI and quantum computing?

Dave Miller

39. **David Roberts**
March 24, 2018

@Paddy

Here is the first edition of Dirac for you to check: https://archive.org/details/in.ernet.dli.2015.177580

40. **Peter Woit**
March 25, 2018

All,
Commenter atreat wants to argue with Tim Maudlin, but also points to a 319 comment thread at Scott Aaronson's blog which includes (besides a lot of other interesting comments) extensive discussion with Maudlin about these topics. I encourage those interested in this argument to visit
https://www.scottaaronson.com/blog/?p=3628
and not to try and restart it here.

41. **GoletaBeach**
March 25, 2018

To me, the more interesting question is not about whether theory is complete, but… what experiment will displace or extend QFT as the underpinning of all microscopic theory? David Mermin sometimes pointed out that the fact we still use QFT with no new constants of nature was a failing of experimental high energy physics. I tend to agree with him, but still… increasing energy is still an enormously attractive route to finding real chinks in QFT. Also, delayed choice types of measurements are attractive. Unattractive to me… characterizing experiment as a servant of whatever trend is popular in the theory community… strings, multiverses, etc.

42. **Jim Baggott**
March 26, 2018

Paddy,
It's certainly true that the idea that a quantum system 'jumps' into some eigenstate as the result of an observation or measurement is implicit in the early history of quantum mechanics, arguably as far back as Bohr's 1913 atomic theory. Despite statements by Heisenberg and Dirac implying such 'jumps' as part of the measurement process, the reason we know this as 'von Neumann's theory of measurement' is because he was the first to put it forward as an axiom of the theory, the collapse or projection being a statistical 'process of the first kind' vs. the unitary evolution of the wavefunction as a causal 'process of the second kind'. I tend to trust Max Jammer on this – see his 'Philosophy of Quantum Mechanics', p. 474. Incidentally, von Neumann treated the apparatus as a quantum system, so departed from the Copenhagen interpretation's 'shifty split' between quantum/classical domains.

Peter,
I sense the discussion of your review of Adam Becker's book is now pretty well exhausted, but if you will allow I'd like to make a couple of final observations. I fully understand why you like to keep the discussion focused on what you consider to be meaningful questions – as posed here by Lee Smolin and to an extent by Tim Maudlin. I don't want to make this seem too black-and-white but, generally, if you don't think the wavefunction is 'real' then you're likely to be satisfied that quantum mechanics is complete (Copenhagen, Rovelli, consistent histories, QBism). But if you prefer to think that the wavefunction is physically real (Einstein, Schrodinger, Bell, Leggett) then obviously something is missing – quantum mechanics is incomplete and the task is to find ways to reinterpret or extend the theory to explain away the mystery, and hopefully deepen our understanding of nature at the same time. Although the MWI in principle adds nothing to the QM formalism, I'd argue that it 'completes' QM by adding an infinite or near-infinite number of parallel universes. But all this comes back to a judgement about the ontological status of the wavefunction and hence a choice of philosophical position.

I don't think any kind of 'new mathematics' will help to resolve these questions. In fact, my studies of the historical development of QM suggest instead a triumph of physical intuition over mathematical rigour and consistency, which is why von Neumann felt the need to step in and fix things in 1932. My money is therefore on new physics, as and when this might become available, perhaps as part of the search for evidence to support a quantum theory of gravity.

Can I leave you with a last thought? In the famous experiments of Alain Aspect and his colleagues in the early 80s, they prepared an entangled two-photon state using cascade emission from excited Ca atoms. We write this as a linear superposition of left and right circularly polarised photons, based on our experience with this kind of system. The apparatus then measures correlations between horizontally and vertically polarised photon pairs, detected within a short time window. So we *re-write the wavefunction in terms of the measurement eigenstates*, and we use the modulus-squares of the projection amplitudes to tell us what to expect. Now I can't look at this procedure without getting a very bad feeling that all we're really doing here is mathematically *coding* our knowledge in a way that gives us correct predictions (which we then confirm through experience). I really don't like it, but I confess I'm very worried.

43. **Peter Woit**
March 26, 2018

Jim,
My semi-joking threat to suppress ontology is due to the fact that I seriously don't know what you or others mean when they say something is/is not "real", or "physically real". I have the same problem with Lee Smolin when he writes about time being "real/not real". Hanging one's treatment of a complex issue on this highly ambiguous four-letter word seems to me to just obscure issues (I started to write "real issues"…).

About MWI, I'm on board with the "everything evolves according to Schrodinger's eq." part, not so much with the "add an infinite number of parallel universes" part.

While I think new deep mathematical ideas may inspire progress on unification, in the case of the measurement problem, the argument from mathematical depth and beauty goes the other way. The basic QM formalism already is based on very deep and powerful mathematics. Here my problem is with things like Bohmian mechanics and dynamical collapse models which deface this beauty by adding ugly complexity not forced by experiment. The measurement problem to me is essentially the problem of understanding how the effective classical theory emerges from the fundamental quantum formalism. Mathematics may or may not be helpful here, I don't know. The confusions I see in most discussions of QM interpretations aren't ones that mathematics will resolve, they need to be resolved by careful examination of the physics one is discussing.

Some people seem to expect that the new physics needed to get quantum gravity will resolve the measurement problem, but I don't see it. The measurement problem is there when you do a very low energy experiment, whereas the quantum gravity problem is about not understanding Planck-scale physics. I just don't see how the quantization of gravitational degrees of freedom has anything at all to do with the measurement problem.

About the photon experiments. I don't think the mysterious thing is the "re-write the wavefunction in terms of the measurement eigenstates" part, that is very simple and completely understood. The mystery is the "The apparatus then measures correlations between horizontally and vertically polarised photon pairs" part, where a macroscopic apparatus described in classical terms is coming into play.

44. **Lee Smolin**
March 26, 2018

Dear Peter,

I agree the statement "Time is real" is misleading and I regret using it. The more precise claim is that "time is fundamental" in the sense that there is no deeper formulation of the laws of physics that does not involve the evolution of the present state or configuration, by the continual creation of novel events out of present events. ie at no level is time emergent from a timeless formulation of fundamental physics. Were the Wheeler-deWitt equation correct this would not be the case.

I view my papers on the real ensemble formulation and relational hidden variables to be modest attempts to develop and explore new hypotheses to complete quantum mechanics.

To answer Peter, these are inspired by developments in quantum gravity, particularly the hypotheses that if time is not emergent, space may be emergent from a network of dynamically evolving relationships, as it is in several approaches to quantum gravity such as spin foam models, causal dynamical triangulations and causal set models.

But if space is emergent, so is locality and hence so must be non-locality. Hence we may aim to show the origin of Bell-non-locality from disorderings of locality which follows from the emergent nature of locality.

This may be a way to realize the idea, which is currently popular, but was first stated by Penrose in his papers on spin networks, that the geometry of space and quantum entanglement may have a common origin.

45. **Jim Baggott**
March 26, 2018

Peter,
I honestly think you've answered your own question. You say "I don't think the mysterious thing is the re-write the wavefunction in terms of the measurement eigenstates part – that is very simple and completely understood". So what is it that exists? A quantum state made of circularly polarised states? A quantum state made of linearly polarised states? Or both, but involving a process that gets from one to the other that doesn't appear anywhere in the formalism except as a postulate, based on a mechanism we can't fathom? Or are we just using these 'states' as a convenient way of connecting one physical situation with another?

46. **Blake Stacey**
March 26, 2018

I noticed an item in the latest quant-ph update that may be of interest to the folks participating here: [a critical evaluation of Wallace's attempt to get meaningful probabilities out of (neo-)Everettian QM](#).

47. **Peter Woit**
March 26, 2018

Jim,
The question "what exists?" is just as ill-defined as "what is real?". The fundamental issue here is that I think I understand completely what a "quantum state" is (any one of the mathematically isomorphic descriptions), but I don't understand completely what a "physical situation" is (it may involve some system under study, some macroscopic apparatus, my consciousness, and how they are interacting). Everett tells me that a "physical situation" is also just a quantum state, and I'm willing to believe him since I don't have evidence against this, but that doesn't change my lack of full understanding of the "physical situation" I'm somehow part of.

48. **Reader**
March 26, 2018

Another interesting article on the history of MWI by the same author here:
https://blogs.scientificamerican.com/observations/the-difficult-birth-of-the-many-worlds-interpretation-of-quantum-mechanics/

49. **Doug McDonld**
March 26, 2018

You write 'The mystery is the "The apparatus then measures correlations between horizontally and vertically polarised photon pairs" part, where a macroscopic apparatus described in classical terms is coming into play.'

I fully agree with you in this. The problem is, explaining "how" this happens. I don't mean in a philosophical sense, I mean describing it using, somehow, just plain quantum mechanics of increasingly complicated sets of interactions that end up in a macroscopic apparatus that everybody agrees is classical. This is doable. Its where the payoff is eventually going to come. The hard part is explaining it.

By increasingly complicated, I mean in terms of say measuring the energy of

a high energy photon slamming into an LHC calorimeter in terms of a cascade
of events. Each even is small in quantum energy terms, collectively they add up to the answer, including both quantum uncertainty and statistical uncertainty. Its not called a calorimeter for nothing … that says its classical.

Normally on blogs like this one my viewpoint usually gets moderated out.

50. **Laurence Lurio**
March 26, 2018

When you are stuck in a philosophical morass, the solution, if one exists, is likely to come from experiment. There is a growing experimental community working on building quantum computers who are worrying about wave function collapse as a very practical matter. It will be interesting to see which interpretation that community gravitates to.

51. **Jim Baggott**
March 27, 2018

Peter,
Try this. Imagine a modern-day equivalent of Ptolemy's model for the orbital motions of the planets based on very large numbers of epicycles. We develop a computer programme to calculate planetary orbits with a very high accuracy. We all agree that the planets are 'real' (they continue to exist when nobody's looking at them through a telescope) and that the physical situation we are describing (planets moving with certain speeds in certain directions across the night sky) is 'real'. But I don't think anyone would consider the epicycles themselves to be 'real', in the sense that they represent real physical forces or processes that somehow govern or determine these motions.

We would say that the epicycles are a convenient way of *representing* the planetary motions, but they shouldn't be taken literally as aspects of the real physics. Likewise, some theorists have questioned the status of the wavefunction in QM. They don't necessarily deny that there is 'real physics' going on, but they argue that the wavefunctions are like epicycles – they are a convenient way of summarising what we know and calculating probabilities, but they shouldn't be taken literally as representing the real physical states of quantum systems.

52. **Yatima**
March 27, 2018

Let's wait for the [Quantum Computers to fail in interesting fashion.]

That is a testable, well, not prediction. But outlook.

I was actually astonished to see experiments on evidence of superposition on [large molecules] come back positive: *"We observe high-contrast quantum fringe patterns with molecules exceeding a mass of 10 000 amu and 810 atoms in a single particle."*. Does the universe know what a human cognitive apparatus labels a "single body"? Why doesn't it superpose the single body into a mismash different "single bodies"?

53. **Peter Woit**
March 27, 2018

Jim,
I understand there's a complex issue of the relationship of reality and our models of it, but it's exactly this deep philosophical issue that I don't want to enter into discussions of, because I don't see that it sheds any light on the non-trivial measurement problem (the relation of quantum and classical). It may very well shed light on some of the debates between interpretations that have gone on over the years, but ones that I've long ago lost interest in.

54. **Low Math, Meekly Interacting**
March 27, 2018

Hope this isn't OT and/or too basic to merit inclusion in the discussion. Apologies if so.

What's the consensus on probability in MWI? As others have mentioned above, it doesn't appear that everyone agrees it's a matter that's been satisfactorily resolved. To the extent that I understand the pro arguments, I'm not especially satisfied either. The whole notion of somehow equating the "illusion" of probability with frequency seems like slight-of-hand. World mangling doesn't seem to help much. It's not clear to me how these supposed solutions do anything but leave us with exactly the same questions about quantum dice we might have with other interpretations. The notion that all these branches are somehow extant also seems to necessitate something like an anthropic principle to explain the fact that I have never experienced bizarre phenomena that are highly unlikely but not excluded. I don't need a multiverse to explain why I don't get "heads" ten thousand times in a row with a fair coin, do I? To me that's not just an aesthetic problem, it's a problem with legitimately excess baggage.

55. **Peter Woit**
March 27, 2018

LMMI and others,

I confess that I have a more basic confusion here. I don't see why the explanation for probability entering the picture isn't just the usual one: we only have probabilistic information about the state of the measurement apparatus. This seems too obvious to be any sort of explanation, I'd love to hear why from someone who has thought about this more than I have (which is not much…).

56. **Mateus Araújo**
March 27, 2018

Low Math, Meekly Interacting,

It's hard to say what the general consensus is, I haven't run any polls. But I can tell you my opinion.

One thing that Many-Worlds explain better about the quantum dice is why the outcome is in principle unpredictable: there is nothing to predict, as all outcomes happen. This ties in well with the Deutsch-Wallace theorem: since all outcomes happen, you should be indifferent about them, which leads to uniform probabilities to states with equal coefficients. Throw in some unitary invariance, and you have the Born rule. So I think the Many-Worlds explanation for subjective probabilities is pretty satisfactory.

What is not satisfactory, and is probably the source of your unease, is the explanation for objective probabilities, as it is simply lacking. This is not a point against Many-Worlds, as there is no explanation for objective probabilities in single-world quantum mechanics either. Why don't you get "heads" ten thousand times in a row with a fair coin? Because the probability is small? Because the mod-squared amplitude is small?

I think, however, that it can be explained, most easily in a toy many-worlds theory where worlds can be counted. Consider that after each throw of the coin exactly one world is created with “heads”, and one world is created with “tails”. Then after 10,000 throws there will be 2^10,000 worlds, and in only one of them you see 10,000 heads in a row. In general, the relative frequencies measured in this toy theory will be close to the objective probability of 1/2 in most worlds.

57. **Daniel Tung**
March 27, 2018

Mateus,

“there is nothing to predict, as all outcomes happen”
But an observer can try to predict whether she will see this or that outcome, right? Because to any observer, she only sees one outcome. Only to a God’s eye point of view do all the outcomes occur. Observers don’t have this viewpoint.

Moreover, what forbids the classical world to adopt a kind of MWI when dealing with uncertainties? If so, we do not need to explain why anything happen anymore, just accept that everything happened and we happened to be in a world where THESE things happened.

Since in the MWI we dragged the observer into the interaction, we can go further and ask why the observer made this measurement choice and not others. We don’t have to stopped at the Outcome MWI. We can have a much bigger world – the Choice MWI, by saying that in fact all the incompatible measurement choices are made, but in different worlds. The problem of probability measure will become more intractable, and the entire situation becomes more absurd.

58. **Tim Maudlin**
March 28, 2018

Peter,

The term “real” is not deep and obscure, it is empty and reduplicative. Similarly with “actual” and “true” and “exists”. Thus: “I have a dog”, “I have a real dog”, “I have a real, actual dog”, “I have a real actual dog that exists” and “It is true that I have a real, actual dog that actually exists in reality. Truly I do.” all say exactly the same thing. And the question of physical existence or physical ontology is a matter of physics, not philosophy.

Here is a simple question of physical ontology. When we first learn electromagnetism, we are taught to use two mathematical vector fields the E field and the B field, to represent the physical electro-magnetic situation. And we are also taught to use the scalar and vector potentials to represent the physical situation. But we are warned that the scalar and vector potentials are not “real”, in the sense that mathematical representations that differ in the values of A and phi can represent the very same physical situation if a gauge transformation carries {A, phi} into {A’, phi’}. In short, there are mathematical degrees of freedom in the mathematical representation that do not correspond to physical degrees of freedom in the physical situation. And when the Aharonov-Bohm effect was discovered, this classical understanding was no longer tenable. How exactly to understand what the physical degrees of freedom might be is a question of physics, and not philosophy. One can ask that question using questions like “Are the scalar and vector potentials physically real?” meaning “Are the physical degrees of freedom isomorphic to the mathematical degrees of freedom in the mathematical representation that uses A and phi?”.

Now you may not be interested in this question. You can take the instrumentalist attitude that no matter how that question is answered, if you know how to use the formalism to make predictions that is all you care about. No one can stop you from adopting such an instrumentalist attitude. But those of us who care deeply about this question do not do so as “philosophers”, whatever that might mean. We do so as physicists, interested in the nature and structure of the physical world. The question “Are the scalar and vector potentials physically real or merely mathematical conveniences?” is slightly poorly phrased, but it is posing these very physical questions.

59. **Joking**
March 28, 2018

But Tim, how exactly are you deciding whether it is E&B or A&phi that are, or represent, the “true physical degrees of freedom”? A priori, any of these is just a mathematical object, namely a scalar or vector field in 3-dimensional space. And you have to derive empirical consequences for a priori real objects, such as ultimately dogs, in order to decide which of these mathematical objects is useful or necessary for the description of reality, and before assigning them reality themselves. Without the Lorentz force and Newton’s laws, the reality of the electro-magnetic field is an empty statement. Similarly, without the Schrödinger equation and the laws of quantum mechanics, the reality of the (integrated) vector potential just has no content whatever.

I agree that the question of physical ontology is not a philosophical one. But what physics teaches is that physical reality is hierarchical, interdependent, and not all the same. The quantum mechanical wave function is not real in the same sense as a dog, for example, though the distinction is not peculiar to quantum mechanics as your E&M example illustrates. What Bohr reminded everyone is that the beginning and end of the discussion of physical reality are experimental setups and experimental outcomes described in plain language. By that token, many worlds or Bohmian position variables have physical reality only to the extent that they have empirical consequences (but do they?).

It also needs to be said that in the discussion of physical ontology, “exists” frequently appears in the sense of the mathematical quantifier, which has to be carefully distinguished from the notion of physical reality (which is also different from the mathematical notion of reality). I hope you agree that at least this distinction applies.

60. **Peter Woit**
March 28, 2018

Tim Maudlin/Joking,

Sorry, but I really will start ruthlessly suppressing discussion of ontology now. Debates about “is the vector potential real?” have nothing to do with the QM measurement problem (and, if you ask me, trivialize and obscure actual important issues, but that’s a topic for another day…)

61. **Davide Castelvecchi**
March 28, 2018

Philip Ball’s book has a great discussion of the tricky issues that plague the many worlds hypothesis (he is not a fan)

62. **Peter Woit**
March 28, 2018

Thanks Davide,

I may (or may not…) wait until it comes out in the US to take a look at the Ball book. In the meantime, for some idea of his criticisms of MWI, there are other things he has written, for example

https://aeon.co/essays/is-the-many-worlds-hypothesis-just-a-fantasy

63. **Jim Holt**
March 28, 2018

The Columbia physicist/philosopher David Z Albert will have a long and pungent review of Becker's "What Is Real?" in the next issue of the New York Review of Books. The review will be online at nybooks.com tomorrow at noon. (Spoiler alert: Oppenheimer does not come off well.)

64. **Stephane**
March 29, 2018

Hi Peter,
is the book by N.P. Landsman – Foundations of Quantum Theory – From Classical Concepts to Operator Algebras (2017) on your list of read books about Quantum Physics?
http://www.springer.com/gp/book/9783319517766

I'd be happy to hear your thoughts about it.

65. **Peter Woit**
March 29, 2018

Stephane,
Thanks for pointing that out, I hadn't seen it before and haven't had a chance to look at it closely. Landsman has thought very carefully and done a lot of work on the question of how classical physics emerges from quantum physics. One thing I can recommend is this article
https://arxiv.org/abs/quant-ph/0506082

From what I remember, Landsman clearly lays out the issues, I think comes down on the side of "there is still something going on here that we don't understand" (I may be misrepresenting him).

66. **Art**
March 29, 2018

Albert's NYRB article is more his own re-telling of the history than a book review, imho. (If only Bohm, Everett, and Bohr could have taken some walks together!) But it gives me an opportunity: this novice was quite taken with Albert's observation (echoed by Coleman in "Quantum Mechanics in Your Face") that, after a measurement fork in the quantum mechanical garden paths, all observers see a definite result (but not necessarily the same result), with nary a superposition in sight. Is this result significant, trivial, or somewhere in between?

67. **Peter Woit**
March 29, 2018

Art,
This is the heart of the measurement problem of quantum mechanics, and to my mind very much significant, with the difficult question that of providing a compelling explanation of how this happens.

68. **Chris W.**
March 30, 2018

By the way, I had somehow missed the fact that Gerard 't Hooft has published with Springer a book-length exposition of his heterodox approach to the foundations of quantum mechanics.

69. **Dave Miller**
March 30, 2018

Daniel Tung wrote:

> Since in the MWI we dragged the observer into the interaction, we can go further and ask why the observer made this measurement choice and not others. We don't have to stopped at the Outcome MWI. We can have a much bigger world – the Choice MWI, by saying that in fact all the incompatible measurement choices are made, but in different worlds.

Yes, Daniel, that is indeed how it is supposed to work. You describe this as the situation becoming "more absurd." Well, perhaps. But lots of people found special relativity absurd, and it seems to be true.

To be sure, this multiple infinity of universes does raise some questions about Occam's razor, but the MWI proponents say the ideas are simple even if the MWI multiverse itself is not so simple.

The real problems are the ones that Peter Woit and I have mentioned, the "preferred-basis" problem as to why the universe "splits" along certain directions in Hilbert space and not in other directions, and the "probability-measure" problem as to how the multiple universes lead to Born's rule.

From the beginning, various MWI proponents have claimed that MWI automatically produces Born's rule, then other MWI proponents admit there are problems, then still others claim they have some new way of getting Born's rule, and so it goes. I have been watching this process for nearly fifty years now, and I fear it reminds me a bit more of theology than physics (not that theology can't be amusing!).

By the way, superficially there is a simple solution: you just announce that the probability measure on the space of possible universes just *is* the Born rule. Of course, then you are not *deriving* the Born rule but just assuming it, just as we do in textbook QM.

Furthermore, as physicists, we want the Born rule to be true in some frequentist interpretation, not just as an abstract measure. How do you do that? Well, often MWI proponents have spoken as if there are a large but finite number of universes in the MWI multiverse, and the probability measure is just a relative count of the number of universes in which one or another outcome occurs.

But that just does not work: You actually need an infinite number of universes, not a finite number, so naïve counting does not work. Also, the universes are not actually separate and discrete (that, after all, is the whole point of MWI: to be able to deal with quantum interference).

So, as far as I can see, the end result of decades of work and rather heated debate is that you just have to throw in Born's rule with a frequentist meaning as an extra assumption in MWI, which largely defeats the point of MWI in the first place.

If you want to dig into this further, David Deutsch is an intense proponent of MWI who has been aware of the two basic technical problems and who, at least on occasion, thinks he has found solutions. Deutsch has done some interesting work connecting MWI to quantum computing and even to time travel and travel to parallel universes (he somehow got Phys. Rev. to publish the latter work!). My own feeling is that if Deutsch, who is indeed very bright, can't get this bird to fly, and I don't think he can, then no one can.

But, I am sure, many MWI proponents will have a different take: there is no final agreement in matters of theology!

Dave

70. **Mark Hillery**
March 31, 2018

From the review in the New York Review of Books by David Albert:

"It wasn't until sometime in the 1980s that a small and embattled community of physicists, mathematicians, and philosophers, who had learned of the the theory [Bohmian mechanics] from Bell, began to take an active interest in what Bohm had done. His theory is now regarded as one of the two or three most important achievements in the history of our understanding of quantum mechanics."

Regarded by whom? As an antidote to this, I highly recommend the following posting by Reinhard Werner, a first-rate mathematical physicist, who has made numerous contributions to quantum information, for those of you who do not know him: https://tjoresearchnotes.wordpress.com/2013/05/13/guest-post-on-bohmian-mechanics-by-reinhard-f-werner/

71. **Peter Woit**
April 2, 2018

Mark Hillery,
Thanks for the comment and link. The fondness of philosophers (and a few physicists) for Bohmian mechanics has always mystified me.

72. **Peter Woit**
April 5, 2018

Note that I've added some comments by the author, Adam Becker, to the posting above.

73. **Tim Maudlin**
April 18, 2018

Mark Hillery: Regarded by who? Regarded by the people who actually work on foundations of physics and foundations of quantum theory in particular. A group to which Werner most certainly does not belong. Nor, obviously, does Peter Woit. You can start with Bell's "On the Impossible Pilot Wave", since Bell was the leading figure in foundations in the latter half of the 20th century. (If you disagree with the assessment, I would be extremely curious who else you think deserves that honor.)

74. **Curious Mayhem**
April 18, 2018

Lindley's book is excellent. It's been clear for some time that Zurek and others' work on decoherence is the way forward on this question.

The invocation of strings and multiverses for the many-worlds idea is hilarious — many-worlds dates from the 1950s, about 30 years before the rise of string theory as we know it now and 40+ years before the "landscape." This is like string theory "predicting" Kaluza-Klein, inflation, gauge theory, general covariance, and what-not, long after those made their appearance. What claptrap.

Cophenhagen and its interpretation have roots long before the military-industrial complex. They didn't know how to fully resolve the problem, although von Neumann isolated it in the measurement operation. Most physicists then didn't care, and don't care now, about the ultimate nature of reality implied by QM. They just know they can calculate with it and get amazing results. "Shut up and calculate."

Various News

March 28, 2018
Categories: Uncategorized

- Reader Chris W. pointed me to this story about what Cédric Villani, aka the Lady Gaga of French mathematics, has been up to. I see that the report of the "Mission Villani" is now available (in French or English) and it's front page news at le Monde. There's also an AI for Humanity website now up, and plans for all sorts of events tomorrow (video here) involving Villani and French president Macron.

  For insight into what this means, you'll need an AI expert. I'm curious to hear if there's anything really surprising in the report.

- Neil Turok and collaborators have a new proposal for how to understand the Big Bang, with the headline version "The universe before the bang and the universe after the bang may be viewed as a universe/anti-universe pair, created from nothing." There's a short summary here, a longer paper with details here.

  The papers make various claims of predictions, I'm curious to hear from cosmologists what they think of these. Much of the papers does look like fairly straightforward QFT calculations, which I'll try to look at more carefully when I find time.

- The LHC is now in a machine checkout phase, ready for resurrection around Easter Sunday, with the start of beam commissioning for the 2018 run.

Update: A wealth of analyses of physics papers is available at this website and this preprint.

Comments

1. **Anon**
   March 28, 2018

   I had noticed Turok's and collaborators paper. I do HEP, not cosmology. However I have a question/comment. What is the CKM phase in the "anti-universe"?

   There is something I am confused about — In the anti-universe if the physicists there take their time "t" as positive, like we do, and call their atoms to be made of "particles" like we do, and write their Standard Model Lagrangian for sub-TeV scale particle physics, will they end up with exactly the same Lagrangian we have, with the same CP violating phase in the CKM matrix? I think from the way the introductory passages in the paper have been written, they would.

   Then isn't this just a duplication of the universe (rather than an anti-universe)? Or is it that their Lagrangian would have \delta_CKM reversed in sign. In which case their particle physics would be very different and the following statement they make would pbly not be true "In other words, the density of particles of species j with momentum p and helicity h at time t after the bang equals the density of the corresponding anti-particle species with momentum p and helicity –h at time –t before the bang."

   Looks to me that these are two universes duplicated rather than a universe-antiuniverse pair but I could be totally wrong.

2. **Fred P**
   March 29, 2018

   "For insight into what this means, you'll need an AI expert." I was trained as an AI expert, although I've rarely worked as one. That said, assuming that you're referring to https://www.aiforhumanity.fr/pdfs/MissionVillani_Report_ENG-VF.pdf , I don't think knowing much about AI is required; it's a policy document, with a lot of wishlist items.

   I find their description of "exposed" jobs (jobs that may be replaced – pages 83-84) interesting. As someone who works primarily in medical robotics, I think that they are missing the costs and benefits in their analysis. As an example, both waitstaff in restaurants and chemists in blood labs have been highly automatable for well over a decade. The former is rarely done, whereas the later is ubiquitous. The reason is that automation of the former doesn't save a lot of money – and has significant costs (space, maintenance, less social interaction between the customers and the restaurant, etc), whereas automation of the later saves large amounts of money and space.

3. **Tim**
   March 29, 2018

   It's not two positive universes, although it appears that way to people who are limited to each universe. It's a subtle and interesting point about the paper.

   The paper, if I understand it correctly, makes the point that in order to understand the Big Bang, you need to use a consistent set of characterizations (metric, wavefunctions, etc) that handle BOTH the universe and the anti-universe, and not just the late time situation, where the "other" universe can be ignored. In particular, this involves a particular choice of the CPT invariant vacuum, which is halfway between the asymptotic vacua of the universe and antiverse. All of the CPT things are actually reversed in the antiverse, including which direction time flows, what is a particle and what is an antiparticle, CP violation, etc. But the universe + antiverse is CPT invariant. This only matters when considering the Big Bang, but the consequences for both universes are huge, including eliminating the need for inflation and predicting that there is an undetectable (except by gravity) background of stable right handed neutrinos that could explain dark matter.

   People who actually understand this stuff can correct me, but it seemed like a very interesting paper to me when I read it.

4. **GreatDoofus**
   March 30, 2018

   Anon & Tim,

   I think the term 'anti-universe' is misleading. The analysis is still at the level of QFT on a classical spacetime background. What they're doing is 'enhancing' symmetry of the FRW metric (which is already invariant under parity, spatial rotations and translations) by making it invariant under reversals of the conformal time. To do this you just need to extend the geometry of the universe backwards, past the Big Bang.

   The extra symmetry simplifies the structure of QFTs and imposes some new constraints on neutrinos and dark matter. Also, it can explain the origin of scalar perturbations without the need for inflation.

   You'll need to ask a cosmologist for more details, but, I think testing this model should be straightforward.

5. **ay**

March 30, 2018

The predictions seem rather weak to me. What can we do with a non-interacting neutrino with such a high mass? There are some restrictions on neutrino properties that, if verified, would not really single-out the theory. That's not to say I have any reason to think it's wrong. Just that deriving revolutionary new theories with a liklihood of testability seems difficult these days.

6. **Narad**
   March 31, 2018

   > I don't think knowing much about AI is required; it's a policy document, with a lot of wishlist items.

   I'm with Fred P; I only did two years on a Ph.D. with one of Roger Schank's recent grads (yes, case-based reasoning basically gave the world irritating voice-based phone trees at the end of the day), but two standouts were that the authors didn't have enough AI to hand to use hyphenation and the line "[d]efining artificial intelligence is no easy matter" (re Schank, see [his entry] for the 2014 Edge ideas-to-be-retired question).

   How "explaining machine-learning algorithms has become a very urgent matter" is also a head-scratcher. It seems about as urgent as explaining how to interpret oscilloscope results for test points on 1970s color TVs was. The table that Fred P pointed out also seems to posit Rosie the Robot.

7. **Pascal**
   April 2, 2018

   > How "explaining machine-learning algorithms has become a very urgent matter"

   Well, assume for instance that a computer has decided that someone must undergo a certain medical treatment. It would be nice to be able to explain this decision to the patient or to (human) doctors! But many modern AI algorithms cannot explain in a human-comprehensible way how they reached a given decision. That's because the "learning" often takes place by tuning a large number of parameters of a complex model (think "deep learning) on big data sets. Et voilà! We hope (and sometimes can prove) that because the model fits the training data, it will also perform well on future unseen examples.

   The "old AI" of the 1970's of the "expert system" type followed a more explicit rule-based approach, and was probably better at giving explanations. But it was outperformed by more recent statistical approaches. I suppose Villani would like to have the best of both worlds.

8. **srp**
   April 2, 2018

   Pascal is correct; DARPA, for example, has projects explicitly aimed at trying to make intelligible the predictions of these regression equations (that's what they really are, high-dimensional non-parametric regressions extrapolating from training data). Another unpleasant discovery has been the remarkable vulnerability of these data-mined regressions to various types of spoofing, with an arms race taking place over the last few months between the spoofers and those trying to make the systems more robust.

9. **Tim**
   April 2, 2018

   So there is a flaw in Pascal's comment.

   Is there a need to explain the decision to a human, or simply a need to verify that it is right?

   Remember, checking that an answer is right is usually far easier than finding the answer (P != NP !!).

   For safety issues like medical diagnoses, you actually only need to verify that the answer is right. You do *not* have to actually understand how it was found.

10. **srp**
    April 2, 2018

    I haven't had a chance to go over this yet, but the abstract is promising and the authors are very sharp:
    http://www.nber.org/papers/w24449

11. **Narad**
    April 3, 2018

    > DARPA, for example, has projects explicitly aimed at trying to make intelligible the predictions of these regression equations (that's what they really are, high-dimensional non-parametric regressions extrapolating from training data)

    Could you please give me a pointer? We had one fellow in the lab (who actually got the lone Ph.D.) who was working on CBR for, IIRC, targeted radiotherapy applications.

12. **srp**
    April 3, 2018

    Here's a quote:

    "The U.S. military is pouring billions into projects that will use machine learning to pilot vehicles and aircraft, identify targets, and help analysts sift through huge piles of intelligence data. Here more than anywhere else, even more than in medicine, there is little room for algorithmic mystery, and the Department of Defense has identified explainability as a key stumbling block.

    David Gunning, a program manager at the Defense Advanced Research Projects Agency, is overseeing the aptly named Explainable Artificial Intelligence program. A silver-haired veteran of the agency who previously oversaw the DARPA project that eventually led to the creation of Siri, Gunning says automation is creeping into countless areas of the military. Intelligence analysts are testing machine learning as a way of identifying patterns in vast amounts of surveillance data. Many autonomous ground vehicles and aircraft are being developed and tested. But soldiers probably won't feel comfortable in a robotic tank that doesn't explain itself to them, and analysts will be reluctant to act on information without some reasoning. "It's often the nature of these machine-learning systems that they produce a lot of false alarms, so an intel analyst really needs extra help to understand why a recommendation was made," Gunning says.

    This March, DARPA chose 13 projects from academia and industry for funding under Gunning's program. Some of them could build on work led by Carlos Guestrin, a professor at the University of Washington. He and his colleagues have developed a way for machine-learning systems to provide a rationale for their outputs. Essentially, under this method a computer automatically finds a few examples from a data set and serves them up in a short explanation. A system designed to classify an e-mail message as coming from a terrorist, for example, might use many millions of messages in its training and decision-making. But using the Washington team's approach, it could highlight certain keywords found in a message. Guestrin's group has also devised ways for image recognition systems to hint at their reasoning by highlighting the parts of an image that were most significant."

https://www.technologyreview.com/s/604087/the-dark-secret-at-the-heart-of-ai/

13. **David J. Littleboy**
April 4, 2018

Another Shank disciple here: I have an all-but-thesis from him from the mid-1980s.

Re: srp's link. The gestalt in AI in the '70s was that linear descent search in large spaces could, in principle, only find local minima, so the idea that big data and "machine learning" are going to solve all mankind's problems seems unlikely in the extreme. A joke (if memory serves, in a Newell and/or Simon AI book at the time) illustrating this had a picture of a robot climbing a tree, pointing to the sky, and screaming "I'm getting closer to the moon". IMHO, this describes the current state of AI way too well.

From Princeton to Prison

April 2, 2018
Categories: Uncategorized

One of my graduate school classmates today sent around a link to a story about someone many of us remembered, Dragoljub Cetkovic. I somehow missed it last year when it appeared, it's by Paul Halpern and entitled [From Princeton to Prison: The 'Boy Genius' Who Was Recruited by John Wheeler and Sentenced by Trump's Sister](). As the story explains, Drago Cetkovic was a precocious young physicist, who John Wheeler brought to Princeton in 1974 (Barry Simon says it was actually Remo Ruffini, Wheeler's postdoc, who had recruited him). Drago was studying quantum field theory, but wasn't much interested in doing things like passing exams or working as a Research Assistant (the usual assignment for first-year grad students). This led to trouble and him getting kicked out of the graduate program.

By the time I arrived in Princeton, Drago was a mysterious fixture around the department, often to be found in the library. I recall a couple times asking him about what he was working on, receiving a dense and, to me, utterly incomprehensible explanation. There were rumors that he was somehow being supported on some grant or money from the IAS, but I have no idea how he made ends meet (Simon says he at times was sleeping on John Milnor's floor). I left Princeton in 1984, and things evidently went downhill for him after that. As explained in the Halpern piece, as well as in [this story from the time](), in 1987 Drago for some reason left a cyanide-laced tea bag in a Princeton grocery store, and called in some sort of threat. He ended up being sentenced to five years in prison, I don't know what happened to him after that.

Some of the stories about him refer to his supposedly threatening Barry Simon's life (Simon was the faculty member in charge of graduate students who had to ask him to leave the program). I had heard this story, and one day when I was talking to Drago, asked him something like:

"Hey, I heard you threatened Barry Simon's life, and that's why he left Princeton to go to Caltech. Is that true?"

His answer:

"No, that is not true. And, if it were true, Caltech would not be far enough!"

I do wonder what happened to him, hope that he sooner or later ended up in a new and much better phase of life.

Update: My memory of those days is rather faulty, it quite likely was Nathan Myrhvold who asked him about Barry Simon. Nathan also recalls that he was working on spinor geometry. Maybe now I'd understand what he was trying to explain to me back then.

Comments

1. **Swany**
   April 2, 2018

   Typo: Princeton grocery story,

2. **G. S.**
   April 2, 2018

   I think that "number of brilliant-but-insane former graduate students who still live in the campus library" is one of the metrics that U.S. News & World Report uses when ranking physics departments.

3. **Peter Woit**
   April 3, 2018

   Swany,
   Thanks. Fixed.

   G.S.,
   At the time, Drago was rather minor league, since we also had John Nash (who made perfect sense when you talked to him) in the Math/Physics library.

4. **troubled@dot.com**
   April 3, 2018

   The Montenegro hotel mentioned, now directed by Dragoljub Ćetković, is the Hotel Bokeljski Dvori, founded 1975 (which would be too early, unless he took it over later). Contact details are in the advert on page 39 (PDF pagination) of http://nasme.me/wp-content/uploads/2013/09/Poslovni-vodic.pdf or if you want to book a trip to investigate: https://www.booking.com/hotel/me/bokeljski-dvori.en-gb.html

5. **In Hell's Kitchen (NYC)**
   April 4, 2018

   I think the only boy genius that Wheeler brought to Princeton and who then went on to make something of himself is Demetrios Christodoulou.

6. **william e emba**
   April 15, 2018

   I remember Drago, and I had some interesting conversations with him. I left Princeton in 1980, sometimes ran into him when I visited campus afterwards over the next five years. I was shocked beyond belief, utterly freaked, when I realized that an article I was reading in the *Weekly World News* was about his arrest for the cyanide lacing. I had thought Alexander Abian was more their speed.

Raoul Bott: Collected Papers, volume 5

April 4, 2018
Categories: Book Reviews

I noticed today that the final volume of Raoul Bott's collected papers, which Loring Tu has been working on editing for quite a while, has finally appeared. The Springer webpage for the book is here, and its content is available at institutions with a Springer subscription here here. Many of the articles within can be accessed by other means, for example Tu's article about Bott's life and work is available here (updated version here) and the collection of articles by others about him is here.

After Bott's death in 2005 I wrote a blog post here, and won't repeat what's there. Much of the material in this last volume is related to mathematics that grew out of Witten's work on Chern-Simons theory. Unlike earlier volumes, which are devoted mostly to Bott's important research papers (almost all of which are well-worth reading!), here many of the articles are of an expository nature, written for various lectures that Bott gave. The volume also includes various incidental material, such as speeches written for various events. Reading these gives a good idea of the wonderful human being that he was, it's great to have them finally collected and available.

Comments

1. **Eric Weinstein**
   April 10, 2018

   Thanks for this Peter.

   Any idea if there might be a curated collection of links to lectures given by Raoul which are available on line? I went looking a while back and was disappointed at how little I could find.

2. **Peter Woit**
   April 11, 2018

   Eric,
   Would be great if someone could do that. I haven't looked much, but unfortunately I think Bott's lecturing years were coming to an end about the time everyone started recording lectures to put on Youtube. Quite a shame, since his lectures were always a wonderful experience.

Are We In the Swampland?

April 5, 2018
Categories: Swampland

Way back in 2005, soon after the emergence of the "String Landscape" and the ensuing debate over whether this made string theory untestable pseudo-science, Cumrun Vafa in response started writing about the "Swampland". In contrast to the "Landscape" of effective field theories that are low energy limits of string theory, the "Swampland" is the space of effective field theories that are not low energy limits of string theory. One motivation here is to be able to claim that string theory is predictive, since if you can show a theory is in the Swampland, then string theory predicts that theory doesn't describe our world.

I wrote a couple blog postings about this back then, see here and here. The situation was rather comical, with Jacques Distler unintentionally making clear one problem with the whole idea. He was enthusiastic about it, and gave as one example the suggestion that one and two generation versions of the Standard Model were effective theories that could not be derived from string theory, but Volcker Braun immediately wrote in to tell him about such a derivation. At this time I started having my problems with the arXiv (and its moderator, Distler) about trackbacks, but that's another story.

I haven't paid much attention to the Swampland business since then, but noticed last night a new preprint with the title *What if string theory has no de Sitter vacua?*. The authors summarize their argument:

> From this analysis we conclude that string theory has not made much progress on the problem of the cosmological constant during the last 15 years. There is a general agreement that the presence of dark energy should be an important clue to new physics. So far, string theory has not been up to the challenge. Or to be more precise, string theorists have not been up to the challenge.
>
> The well-motivated introduction of the anthropic principle and the multiverse, was a big relief. The mathematical standards were lowered, and unconstrained model building could set in exploring a wild and free landscape of infinite possibilities. But beyond this suggestive connection between a possible multiverse and the rich mathematical structures of string theory not much solid results have been achieved. We reviewed some fraction of the mounting evidence that most, if not all of this landscape, is a swampland and we refer to for similar lines of thought. We believe it makes more sense to listen to what string theory is trying to tell us, then to try to get out of the theory what one would like to have. In recent years, especially with the program of the Swampland , there is luckily a growing community that embraces this idea. Perhaps this program really already made its first prediction: no measurable tensor modes in the CMB.
>
> From what we have seen so far, we believe that the most sensible attitude is to accept there are no dS vacua at all because string theory conspires against dS vacua.

The suggestion here is basically that effective field theories on a deSitter background are in the Swampland, so can't be derived from string theory. Since we seem to live in a deSitter space, the obvious conclusion to draw from this is that string theory is falsified: it can't be the fundamental theory we are looking for. The authors discuss various unconvincing ways to try and avoid this conclusion.

By the way, the authors make the usual flawed argument that "the string theory landscape is just like the Standard Model":

> This kind of criticism is, however, misguided . One might compare with quantum field theory, where there is an infinity of fully consistent theories. Experiments are needed to pick the right one, and parameters must be fitted. When this is done the theory still has enormous predictive power, and no one would claim that the Standard Model is useless. One could argue in a similar way concerning the string landscape.

They also claim:

> Paradoxically the critics of string theory and the proponents of the string landscape all agree on one thing: the landscape exists and we more or less know its properties.

At least this string theory critic has never agreed on this. I don't believe "string theory" is a well-defined enough framework to answer the question of what all its ground states might be, or to properly characterize them. If you accept conjectures about the theory put forward by the landscapeologists, all evidence is now that the set of ground states they identify is so large as to make predictions impossible. The argument against string theory is that there are two possibilities here: either the theory is too poorly understood to tell us what its ground states are, or it does tell us something, and there are too many ground states to make useful predictions. Either possibility leads to the same conclusion that this is a failed idea.

It is rarely acknowledged just how serious the problem of a lack of a definition of "string theory" really is. To get some idea of how bad this problem is, one can consult one of the main references in this paper, a survey of the Landscape and the Swampland, based on Vafa's 2017 TASI lectures (this paper also discusses the idea that deSitter is not in the Swampland). Claims are often made that AdS/CFT resolves the problem of defining a non-perturbative string theory of quantum gravity, but in the paper one finds:

> We can now ask the question if using this AdS/CFT correspondence gives a non-perturbative definition of string theory. The motivation for this is that we can give a non-perturbative definition of SYM theory, for example by lattice regularization, whereas the holographic quantum gravity dual theory in AdS has no complete definition. The fact that the CFT side, i.e. the non-perturbative definition of SYM, gives in principle, a non-perturbative definition of the AdS side, is of course true. But this may be not very useful for deeper questions of quantum gravity. In fact the regime that the gravity side is weakly coupled is big corresponds to when the SYM is strongly coupled. In fact 't Hooft was trying to use string theory as a solution to the gauge theory question at strong coupling and not the other way around!…
>
> we find ourself back at the beginning: we want to know fundamentally, what is quantum gravity? It should describe the quantum fluctuations of the metric. From a brief analysis of the standard Einstein-Hilbert action, we see that fluctuations of the metric at the Planck scale should become very violent, leading to potential changes in the topology of the spacetime . This leads naturally to the idea that quantum gravity should be equivalent to summing over all spacetime topologies and geometries:
> $$Z_{QG} \sim \sum_{\text{top. and geo.}} e^{-S}$$
> In general we have no idea about what description will lead to the correct sum over geometries and topologies. We only do know that there should be some mechanism that washes out the Planck scale fluctuations to produce a smooth space at lower energies. It seems that this description must come from some new fundamental principle, rather than from some duality such as mirror symmetry or AdS/CFT. This lack of knowledge of describing the gravity side quantum mechanically is "the missing corner" in our understanding of string theory.

In both this survey and the new paper, the tactic of trying to remove the Landscape to restore the predictivity of string theory hits up against the obvious problem: you're left with no theory at all (the equation above, with an undefined sum and an undefined action, is the essence of no theory).

Comments

1. **Jack Morava**
   April 5, 2018

Perhaps it's relevant that it's not known whether or not there is a countable number of smooth structures on the four-sphere?

2. **Peter Woit**
   April 5, 2018

   Jack,
   I think the notation "sum over all topological and geometric structures" speaks for itself. As a good rule of thumb, whenever people in this field talk about having a "measure problem", if you look into it you'll find that they don't even know what the space is on which they are looking for a measure. The space of "all topologies and geometries" is a good example.

3. **Marc Nardmann**
   April 5, 2018

   @Jack Morava: If by "countable" you mean "having cardinality at most aleph_0", then it *is* known that the set of diffeomorphism classes of smooth structures on the 4-sphere is countable. Even the union, over all compact 4-manifolds M, of the sets Sm(M) of diffeomorphism classes of smooth structures on M is countable.

   In the "sum over all topological and geometric structures" context, one should also consider *noncompact* 4-manifolds M, and on those Sm(M) has usually (conjecturally: always) the cardinality of the continuum. Therefore compact 4-manifolds would probably be irrelevant, because the set of geometric structures (whatever that means precisely) on them should be expected to have measure 0 within the space of all topological and geometric structures. Of course one needs a precise definition of the space and of the measure to verify this expectation.

   The set Sm(M) can be uncountable only for (noncompact) manifolds of dimension 4. Some people have suggested that this explains why our universe is 4-dimensional: the set of universes of other dimensions should be expected to have measure 0 within the space of all universes. Again, it all depends on the chosen space and measure. (Probably M-theorists would prefer anyway to explain why our universe is 11-dimensional?)

4. **Peter Woit**
   April 5, 2018

   Sorry topologists, but that's enough about this issue, which has nothing to do with the topic of the posting.

5. **Søren Bro Thygesen**
   April 6, 2018

   So this is the basis for your problems with trackbacks? I find that incredibly childish on Distler's part. I'm aware that smart people often have eccentric behaviour but man..

6. **Peter Woit**
   April 6, 2018

   Søren Bro Thygesen,
   To be accurate, I have no idea at all (despite signficant effort to find out) why trackbacks to my blog are now banned, or exactly what Distler's role in this is.

7. **Shantanu**
   April 6, 2018

   Peter: have any journalists written about this paper or interviewed others?

8. **Peter Woit**
   April 6, 2018

   Shantanu,

   Not that I know of. Seems unlikely that any would. Claims about string theory and the landscape/multiverse make a very appealing story (physicists have this far-out cool idea that explains everything!). The new claims aren't such an appealing story, and the "swampland" designation isn't very good marketing. No one wants to write or read stories like "you know that cool stuff you read about last year? Best to just forget about it, probably doesn't work". In addition, when asked "what replaces the landscape/multiverse", I think all those who want to get rid of the landscape have is the old xkcd cartoon

   https://xkcd.com/171/

9. **new**
   April 6, 2018

   Peter can you comment on this

   " Perhaps this program really already made its first prediction: no measurable tensor modes in the CMB."

   if the above is a genuine prediction of string theory, what are the implications to string theory if they find measurable tensor modes in the CMB?

10. **Peter Woit**
    April 6, 2018

    new,
    There's no such thing as a "genuine prediction of string theory", for tensor modes or for anything else.

11. **Cobi**
    April 8, 2018

    One should mention that after writing down the qualitative "sum over spacetimes" formula, Vafa et al. review an explicit realization in the context of topological string theory.

12. **clayton**
    April 8, 2018

    Sav Sethi at Chicago has been making similar claims in seminars for quite a while now; this has been on the arXiv since last fall in 1709.03554, which Danielsson and Van

Riet cite. I think that these issues are well known in the string theory community, but it doesn't affect the day-to-day activity, which is no longer driven primarily by reproducing the Standard Model at low energy. Whether that is evidence of failure or (a good kind of) resilience seems to be a question of taste.

13. **Peter Woit**
April 8, 2018

Cobi,
Yes, a sum over some Calabi-Yau three-folds is discussed, but this isn't a theory of quantum gravity. The xkcd cartoon applies.

Clayton,
"no longer driven primarily by reproducing the Standard Model at low energy" is a pretty weasel-worded way of saying "not driven at all by getting particle physics at any energy". Also seems to be synonymous with "have given up on getting a theory that can be connected to the real world".

If you read all the way through Sethi, at some point you finally come to a mention of what the real problem is: there is no well-defined theory capable of giving unambiguous answers to the questions being asked. More specifically, Sethi writes:

"To answer this question, we require a framework for computing quantum corrections. String theory should be that framework but we immediately face an obstacle. String theory requires an on-shell solution. There is no currently understood method of computing quantum corrections off-shell, and this is closely tied to the fact that observables in string theory are always correlation functions of vertex operators, which are only defined in an on-shell background."

14. **Petite Kabylie**
April 9, 2018

Peter,

I do not quite agree with your argument this time:
"…either the theory is too poorly understood to tell us what its ground states are, or it does tell us something, and there are too many ground states to make useful predictions. Either possibility leads to the same conclusion that this is a failed idea."

I am ok with the second possibility, but not with the first. If a theory is poorly understood it just means more work (and more time) is needed to get the idea off the ground rather than a right away dismissal.

15. **DrDave**
April 9, 2018

PW (2005): "I've heard from someone associated with the arXiv that it's not their intention to allow trackbacks to my postings to be censored and that part of the problem has been both difficulties they've been having with new software…."
Curious what this person would say now.

16. **Peter Woit**
April 9, 2018

Petite Kabylie,
I was using "poorly understood" as a polite way of saying there is no theory (i.e. no non-perturbative version of M-theory for which the ground states are well-defined, and not in conflict with experiment).

It is fine to take the attitude that if one doesn't have a viable theory, that just means one needs to put more time and work into finding one. In evaluating this question, in this case one should keep in mind that people have been trying unsuccessfully to solve this problem for over thirty years, and that most of the experts in the field who have worked on this have given up long ago and are doing other things (I know of no promising ideas for a viable definition of M-theory, and thus virtually no one working on this).

17. **Curious Mayhem**
April 18, 2018

The emptiness of that Z ~ SUM(topo,geo) exp(-S) — where we have no idea what the sum, its measure, or the action is — reminds me of Pauli's reaction to Heisenberg's proposal for a unified theory, with "just some details to fill in":

"This is to show the world that I can paint like Titian. [A big drawing of an empty rectangle] Only technical details are missing."

(Apparently from a 1958 letter to George Gamow, in reaction to Werner Heisenberg's claim to a journalist that Pauli and Heisenberg had found a unified field theory. Quoted in Michio Kaku's Hyperspace, p. 137 (1995).)

18. **Peter Woit**
April 18, 2018

Curious Mayhem,

Story also told in my book, didn't realize Kaku also had it.

19. **Moyses**
April 26, 2018

dS from 10d, but not à la KKLT: https://journals.aps.org/prd/abstract/10.1103/PhysRevD.97.046010

20. **Urs Schreiber**
April 27, 2018

Moyses, that article by Moritz, Retolaza, and Westphal appeared as preprint already in 07/17, and its claim is discussed in the above article by Danielsson-vanRiet:

> We now highlight 3 problems with the IIB constructions that have been pointed out in the recent literature: […] 2) issues with anti-brane backreaction on the 4D moduli, which was found recently by Moritz, Retolaza, and Westphal [131]

21. **Moyses**
April 27, 2018

Urs Schreiber, I understand that such problems are part of the perturbative approach to IIB strings. Somehow, they are analogous to perturbative IR divergencies in QCD that can be cured by the lattice. For instance, a nonperturbative approach as IKKT shows different behavior at latter times: doi:10.1007/JHEP10(2012)147

Math News

April 12, 2018
Categories: Uncategorized

Various mathematics-related news:

- The Perimeter Institute has been moving towards an increased engagement with mathematics and mathematicians in recent years. Matilde Marcolli and Ben Webster are now joining them as Associate Faculty.
- Quanta magazine has an excellent article by Kevin Hartnett on the state of efforts by mathematicians to understand mirror symmetry.
- My colleague Dorian Goldfeld has become editor of the Journal of Number Theory, succeeding David Goss, who passed away last year. He tells me that the journal will sponsor a biennial conference, first one next year, announcement here. There will also every two years be a David Goss prize awarded to a young researcher in number theory.
- The latest Notices has an interesting and extensive article about Claire Voisin.
- John Baez has an ongoing online course on Applied Category Theory.
- Mathematician/musician Tom Lehrer is 90 this month, and there's a story about him at Nature. For an example of his music, listen to Lobachevsky.
- I'd missed the news that Roger Howe now has a tenure-track position at Texas A & M. Good luck to him on getting tenure! Howe duality is very much worth knowing about, here's an expository treatment.

Update: This Friday and Saturday there will be a meeting in Cambridge on the topic of Ethics in Mathematics. Supposedly talks will be livestreamed on Youtube (perhaps here ?).

Comments

1. **LMR**
   April 13, 2018

   It's interesting to see how Marcolli's career has evolved over the years: I didn't know that she's taken an interest in computational linguistics since 2014. Her essay, "A drifter of Dadaist persuasion", available from her Caltech website and to appear (as indicated there) in an AMS publication, is a fascinating read.

2. **David Roberts**
   April 13, 2018

   > Howe is an elected member of the National Academy of Sciences and the American Academy of Arts and Sciences. He has more than 40 years of service in the fields of mathematics and mathematics education and has received numerous honors for spearheading national education initiatives and ultimately expanding the frontiers of mathematics…

   and he's just gotten a tenure-*track* job? What hope do the rest of us have?? 🙂

3. **Thomas Larsson**
   April 13, 2018

   David, I got the impression that Peter was being ironic. I cannot think of a reason why somebody past 70 would want to get tenured, except perhaps ego. Surely universities can fire tenured professors once they reach mandatory retirement age (67?) if they want to.

4. **Peter Woit**
   April 13, 2018

   Thomas Larsson,

   I was being ironic, and I think David Roberts was too. Either the article was misusing terminology, or it might even have been technically correct (some institutions have a policy that even a senior hire doesn't officially get tenure until after their first year, or something like that).

   In the US, there is no mandatory retirement age and tenured faculty can stay on as long as they want, which can sometimes create problems.

5. **Chris Oakley**
   April 16, 2018

   I feel the need to point out that, irrespective of any mathematical credentials, Tom Lehrer is, in my humble and miserable opinion, the author of the funniest songs ever written. His music does not seem to date, either. Anyone who has not yet discovered him has a treat in store.

6. **Michael Gogins**
   April 16, 2018

   Peter, about Mathilde Marcolli, what is she talking about with respect to cruelty and exclusion in the mathematical community? Is she being a bit paranoid, or are people rejecting her work for bad reasons? It's puzzling to me because from where I sit, as a non-scientist, she seems to have had a lot of success. I read her essay with great interest.

7. **Peter Woit**
   April 16, 2018

   The essay being referred to is here
   http://www.its.caltech.edu/~matilde/MarcolliArtMath.pdf

   Michael Gogins,

   I've never met Marcolli, know only a little about her work, which is interesting, but highly idiosyncratic. You'd have to ask her for specifics, but you probably should take into account the context of what you quote (on page 4) which refers to the Surrealist critique of "the violence intrinsic in traditional family and bourgeois society." I suspect her criticisms apply to a lot more than just the math research community.

For some insight into one of her main reasons for going to Perimeter, note that she has a blog:

http://listeningtogolem.blogspot.com/

8. **Jim Given**
April 16, 2018

Peter, by coincide, I am only now reading the very excellent essay by Roger Howe, "On the Role of the Heisenberg Group in Harmonic Analysis," which I'm sure you are familiar with.

9. **Michael Gogins**
April 16, 2018

Thank you for the link to Marcolli's blog. I read the two most recent posts. She is certainly alarmed, and alarming, and the blog does give insight into her paranoid-sounding remarks.

There seems to be a common thread here with Grothendieck's political attitudes…

As for myself, I am certainly very alarmed, but I don't think as much so as Marcolli.

10. **Jim Given**
April 18, 2018

Prof. Marcoli gave a lecture on creativity and mood disorder that may be valuable to many in the research math community:
http://www.its.caltech.edu/~matilde/DarkBrightness.pdf

11. **S**
April 18, 2018

I read Marcolli's essay. I found it interesting, but also quite frustrating. There is a very great deal of talk about the violence and cruelty of the mathematical community and of (most, it seems) mathematicians, but a great lack of detail as to what form this cruelty might take; which certainly would be of enormous interest to those of us who might wish to try to do anything about it. It is quite upsetting that Marcolli has had such painful experiences; but as there is very little content about the form of any of those experiences, there is little I can say except that — it is quite upsetting that she has had such painful experiences.

The exception would be where she discusses the putting away of books, which is quite a specific complaint, and one that I vigorously agree with.

12. **LMR**
April 20, 2018

Michael Gogins,

Marcolli has something in common with Grothendieck's parents, who surely had a profound influence on his worldview.

S,

For what it's worth, the essay corroborates some of the rumblings of disaffection, which I've heard a while ago, that were coming from the field in which she was active.

13. **Jim Given**
April 21, 2018

Some people seem to be genuinely troubled (reasonably so) at Prof. Marcolli's talk of "violence" within the mathematics community. For example her dismissal (from IHES?; from Connes' research group?) is described as "violent" in her blog.

It seems relevant here to note that, while Marcolli's art provides a playing-with, and an ironic commentary upon Modernist art; her commentary is pure Postmodernism, with its recondite code of "marginalization"; "the Other"; "violence" and other standard terms of reference in the Postmodernist discourse. In brief, "violence" need not mean what it would mean in e.g. a police report. In context, one has per se no warrant for any assumption that it does.

14. **LMR**
April 23, 2018

Jim Given,

IHES? Marcolli never had a position there. She was at the MPIM in Bonn. I think she made it very clear in the essay Peter linked to above that it had everything to do with her being ostracised by some in the noncommutative geometry community, about which I had heard of some internal troubles brewing during the time Marcolli made the move to Caltech.

15. **Jack Morava**
April 29, 2018

I only now became aware of this thread, sorry for not writing sooner. I don't know MM well; we've met from time to time and we've corresponded occasionally. She is a major mathematician, a significant original voice, and I have the greatest respect for her and her work. [Cf eg https://arxiv.org/abs/hep-th/0411114 for an example from some years ago.] She deserves to be taken very seriously.

16. **LMR**
May 3, 2018

Jack,

I'm sure everyone here has the greatest respect for Marcolli and her work. I'm certainly in awe at the eclectic nature of the mathematics she's done so far, not to mention her concurrent productivity in her artistic pursuits as well. On the other hand, it's rather heartbreaking – though also insightful – to read in her essays (URLs supplied by Peter above) about the obstacles she's had to overcome, in order to achieve the success she's had.

17. **former mathematician**
May 3, 2018

Tom Lehrer is the source of the ironic phrase "in our copious free time." It is from the patter introducing "It makes a fellow proud to be a soldier."

18. **Jack Morava**
    May 3, 2018

    @ LMR: I agree completely, thanks.

19. **cedric bardot**
    May 7, 2018

    To provide justice or – in a less "Postmodernist" way for Jim Given 😉 – to pay a fair tribute to the mathematical- physics work of Mathilde Marcolli, it could be worth quoting explicitly her partnership with Connes in the exploration and distillation of space-time and the geometric setting of prime numbers, chasing in parallel nothing less than the Rieman hypothesis and quantum gravity. Following the tracks made by the falks who build the standard theory of Yang-Mills-Higgs interactions in the quantum world and walking in the steps of Grothendieck motives, they delivered in common an ambitious and dense book : "Noncommutative Geometry, Quantum Fields and Motives" that must not have been easy to write. This tremendous work should have heightened the unrest created by necessary compromises due to different points of view or conflicting agendas not to mention the meeting of two strong inspirational but not necessary complementary personnalities.

    For instance Connes has never hidden his skepticism about supersymmetry, string theories or loop quantum gravity and wants to keep forging ahead his spectral non-commutative geometry (NCG) project. On the opposite Marcolli has always been willing to build bridges between NCG and the other speculative ideas in theoretical physics. Her last book on "Noncommutative Geometry" is a very nice example of this inclination (the first chapter is free of charge at the Publisher website and is worth reading as a fresh review https://www.worldscientific.com/doi/pdf/10.1142/9789813202856_0001).

    Mathilde Marcolli is definitely a ronin of noncommutative geometry (http://siddhartadevi.blogspot.fr/2009/03/science-frictions-and-ronin.html). Her math will not go away for sure and time will tell if her contributions to physics will find a path to the validation by experimental research. Working in a closer relationship with the Perimeter Institute may definitely help her to convince more physicists – thanks to her past (or future?) work – that the Higgs boson from attospace (and possibly dark matter at cosmological scale!) is (are) spectral noncommutative geometry staring us in the face…

20. **cedric bardot**
    May 7, 2018

    My apology for misquoting the title of Marcolli's last book which is "Noncommutative Cosmology".

Losing the Nobel Prize

April 19, 2018
Categories: Book Reviews

There's a fascinating new book just now appearing in book stores, Losing the Nobel Prize, by astronomer Brian Keating. An excerpt from the book is available at Nautilus, with the title How My Nobel Dream Bit the Dust. Some reviews that are out are here, here, here and here. Sabine Hossenfelder is not too happy with the book (response from Keating here).

Much of the book is an excellent explanation, from the beginning, of a significant part of the current state of cosmology. It does a good job of even-handedly explaining the controversy over the scientific status of inflation and the multiverse, giving Paul Steinhardt's views equal billing with those of multiverse enthusiasts like Guth and Linde. It's written from the point of view not of a theorist, but of an observational astronomer, and thus explains well some of the details of the current state of the technology being used. Much of the book is about the BICEP telescope, operated in the hostile environment of the South Pole.

One of the strongest aspects of the book is that it is also the memoir of a life and a profession, giving a very personal take on what it's like to get interested in astronomy as a kid, then grow up and pursue a career in the field. Keating's book is very much in the tradition of Watson's *The Double Helix*, giving a portrayal of himself and others that doesn't leave out the very human aspects of ambition, competitiveness and jealousy.

Unlike the Watson book, which is about a great scientific achievement, the unusual aspect of Keating's story is that what he was involved in was not a success, but the biggest fiasco in the history of his field. On March 17th, 2014, the New York Times reported on its front page that Space Ripples Reveal Inflation's Smoking Gun, and this same story was reported by most media outlets. This was based on results from the BICEP2 telescope unveiled at a press conference at Harvard (press release First Direct Evidence of Cosmic Inflation). At the press conference, PI John Kovac claimed that the chance the results were a fluke was only one in 10 million.

I wrote several blog postings about the story as it evolved, you can find them here, here, here and here. The BICEP2 result was often portrayed as a definitive experimental vindication of the multiverse, which was one reason I was writing about it. By the later postings, I was covering the story of the collapse of the BICEP2 claims, as it became clear that what they had measured was a signal coming from dust in the galaxy, not from primordial gravitational waves.

Keating's insider account of what happened makes clear that the true story is that the BICEP2 telescope, because of the way it was designed (sensitive to only one part of the sky at one frequency), was never capable of distinguishing primordial gravitational waves from dust. They were in hot competition with the Planck satellite collaboration, which did have the capabilities needed to distinguish the signal they were seeing from dust, and was generally assumed to be the experiment with the best chance of seeing primordial gravitational waves. BICEP2 could have released its data, making clear that it might be primordial gravitational waves or it might be dust, that Planck would need to weigh in to decide. This would have made a splash, but probably not a front-page one, and if the gravitational wave signal was real, Planck would have shared in the glory of identifying it.

Instead of behaving responsibly, the BICEP2 collaboration found arguments to convince themselves that the dust could not be a problem, arguments which included scraping data off a slide of a preliminary Planck result presented at a conference (while, it seems, misunderstanding the significance of the data in that slide). Keating gives a very defensive explanation of how this happened, claiming that he was well aware of the danger that the signal was just dust. About Planck, he writes

> We desperately tried to work with the Planck team, while being careful not to tip them off as to what we'd found… The Planck team wouldn't cooperate.

which I guess really means "we desperately tried to rip them off, but they weren't that dumb." While he had these concerns, in the end he decided to agree (as did the whole collaboration) with the tactic of writing a paper claiming dust wasn't a problem and going public with an aggressive and heavily promoted discovery claim.

The cost/reward computation they were engaged in when they decided to go public with a problematic claim involved two possibilities:

- Planck data would show the dust was not a problem. If this was the case, BICEP2 would be the people who found the primordial gravitational waves, Planck the losers who measured some boring dust.
- Planck data would show that the signal was dust. This would be embarrassing, but, this is America, and all publicity is good publicity, right?

As far as I can tell, the BICEP2 scientists haven't suffered much professionally from the fiasco. When David Spergel talked here at Columbia about the subject, he noted that this hadn't stopped the PI, John Kovac, from getting tenure at Harvard. In the book, Keating mentions some "embarrassment and guilt", but no negative professional consequences, instead explaining how a few months later Jim Simons came to him to offer to fund a next generation observational program (the Simons Observatory, of which he is now Director) to be built in Chile. The Nobel Foundation in 2015 was contacting him to request him to nominate candidates for 2016. Keating does write that he thinks the BICEP2 story shows that scientists should be given some formal training in ethical norms, but at the same time he makes clear that violating such norms sometimes provides significant rewards, with few penalties.

A major theme of the book is Keating's obsession with the possibility of winning a Nobel Prize as well as long discussions of what's wrong with the way Nobel Prizes are awarded and what he feels should be done about this. On some of these issues I agree with him. In particular, the Higgs discovery story makes clear the problem with awarding prizes only to individuals, not collaborations. You end up with a prize not for the most important experimental discoveries in physics, but for the most important discoveries made by experimental groups with a small enough list of high profile leaders.

Sabine Hossenfelder's review was quite hostile about Keating's complaints concerning how the Nobel Prize is operated, for reasons I didn't understand until I read the book. It's very hard to have much sympathy for Keating's recounting of his many Nobel-related jealousies and resentments. In particular, even if there had been no dust, he was never really in the running for a piece of the BICEP2 prize nomination since he had been pushed out of a leadership role due to his involvement with a competing experiment. He still seems bitter about this and gives the impression that this is at the root of his complaints about the Nobel. One suspects that if there had been no dust and he had been given more prominence in BICEP2, after his trip to Stockholm he'd instead have written a book describing the Nobel Prize as the most well-designed and enlightened thing in the world. Instead of owning up to mistakes and writing a post-mortem about lessons learned and what to do about them, Keating's choice to instead write a book blaming the Nobel Prize committee is a peculiar one.

Update: Keating has a Losing the Nobel Prize website, dedicated to promoting reform of the Nobel Prize along the lines suggested in his book.

Update: Not content with using notoriety achieved through incompetent and unethical scientific behavior to launch a bizarre and incoherent campaign against the Nobel Prize, Keating is taking to right-wing media outlets to attack the atheism of his fellow scientists, see here and here. I'm afraid that on the multiverse issue he has the stronger argument: multiverse proponents are making a huge mistake using that to go to war with religion.

Comments

1. **DrDave**
   April 19, 2018

   I wonder how many in the print and online media printed a full retraction.

2. **lun**
April 19, 2018

It is interesting to compare BICEP2 to OPERA, from the experimental angle.
OPERA made a highly non-trivial error, which took months of expertise to understand properly. Their release was relatively cautious (inasmutch as it is possible with such a claim), yet when the error was found the collaboration really suffered (the spokesman had to resign). BICEP2… well, the blogpost above says everything that need to be said.
Why such a difference?

3. **Richard Easther**
April 20, 2018

With regard to BICEP2 and OPERA, part of what gets overlooked in some of this is that BICEP2 had done lovely work — they had great data at far higher sensitivity than any previous polarisation experiment, if I am recalling things correctly; the analysis was flawed but the data was good.

Conversely everything OPERA announced evaporated and even the hint of a claim to have found superluminal particles is a far larger deal than detecting a primordial B-mode 🙂 Not really making a judgement as to whether either outcome was fair, my own sense was that the OPERA announcement did attempt to have a dollar each way on the outcome; it was a bit too much "I'm not saying aliens" for my taste, but there are big differences and well as parallels between the two cases.

4. **tulpoeid**
April 20, 2018

So, this is a fascinating book not for the reasons its author thinks it is.

The professional benefits that BICEP2 collaborators have received, from the spokespersons down to the last student, and the impossibility of having them retracted is phenomenally annoying. If Keating is complaining about how a single prize determines research (ehm… does it really?) then he'd better complain about tycoons determining research by handing out money to people from failed experiments. But who am I to judge a heavily promoted academic.

(PS: OPERA's claim was a crazy one by current standards, and their release was not cautious, like, at all.)

5. **Another Anon**
April 20, 2018

I remember OPERA were much more cautious about their claim, raising serious doubts and asking for other groups to check their results. I remember wondering at the time why it had such a destructive effect on their team when the loose cable was found (note: a loose cable is an unfortunate accident, but making a decision to scrape data off a slide and go the max publicity route with press releases and doorstep videos is not an accident).

Truth is, sorry to say, I think they had a ton more scientific integrity than the BICEP team, hence the difference in their methods of presenting their results. I found this: "Some OPERA team members thought the whole episode had besmirched the collaboration's reputation", hence the resignations even though it was an accident, whereas I remember the BICEP team carrying on as if nothing bad had happened, indeed, treating it as an opportunity to push for funding for a bigger telescope. Plus an opportunity for Andrei Linde to change his story and say that B-modes were not important for inflation after all.

6. **Peter Woit**
April 20, 2018

DrDave,
My impression is that some media outlets later put out stories like "questions raised about BICEP2 results", placed much less prominently than the initial stories. PRL referees saved the BICEP2 people from themselves by getting them to remove the worst of their mistakes. If the referees had quickly let the paper through, likely the paper would have had to have been formally retracted.

lun,
I did some searching around for examples of any apology from the BICEP2 people or negative effects on any of their careers, could find nothing. A quite remarkable fact is that Kovac's Wikipedia entry still has the following as description of his career:

"He is the principal investigator of the BICEP2 telescope, which is part of the BICEP and Keck Array series of experiments.[3][4][5] Measurements announced on 17 March 2014 from the BICEP2 telescope give support to the idea of cosmic inflation, by reporting the first evidence for a primordial B-Mode pattern in the polarization of the CMB.[6][7][8] If confirmed, this measurement provides a direct image of primordial gravitational waves and the quantization of gravity."

Looking around for statements from Kovac post-fiasco, I couldn't find anything from him acknowledging that BICEP2 had done anything wrong, see for instance
https://www.ozy.com/provocateurs/john-kovac-found-the-holy-grail-of-cosmology-and-then-didnt/39632

7. **Peter Woit**
April 20, 2018

Thanks to XOR'easter for fixing somewhat the Kovac Wikipedia page.

8. **Tony Verow MD**
April 20, 2018

This is a fascinating blog post. I am certainly no expert on astrophysics so I can't weigh in on the minutiae. I do wonder though if it would have been possible for the BICEP2 folks to have a third party review their work and it's extraordinary claims before submitting it formally to the peer review process. I get the sense from the blog post that they were far too deep into their scientific process to be throughly objective about drawing conclusions. Not being critical, just wondering if an aloof reviewer might have helped shepherd the conclusions a bit more. Perhaps this was not a realistic thing to do secondary to time and publishing pressure…..not to mention Nobel aspirations!!

9. **Peter Woit**
April 21, 2018

Tony Verow,
Keating doesn't really address at all the question of why the dust estimates BICEP2 used were so wrong. Part of the story is surely that they wanted to believe low dust estimates.
Since their experiment was inherently incapable of distinguishing dust, likely they themselves had little expertise in the dust issue. They should have been getting outside help on this, but perhaps the people with this expertise pretty much all were associated with competing experiments like Planck that could do dust measurements. BICEP2

did not want the competition to get wind of their result, out of fear that they would get scooped, so they weren't about to consult outsiders.

10. **Tim May**
April 22, 2018

My recollection of the BICEP2 thing is perhaps different from others. I remember the repercussions of the episode more clearly than the initial announcement.

I'm thinking of seeing some videos of "random knocks on the door" of people in Palo Alto (Linde and his wife) which were clearly staged (they both appeared at the door) and then the denounment (sp?) a few weeks later that the results were not what they had initially been thought to be.

My recollection was that science worked.

I think the retraction worked better than expected, in other words.

11. **Peter Woit**
April 22, 2018

Tim May,
The video you are thinking of is here:
https://www.youtube.com/watch?v=ZlfIVEy_YOA
It was put on YouTube by the Stanford press office, has over 3 million views. It is still there with original text claiming discovery, has not been updated with the news this was just dust. The same is true of the linked Stanford web page with story for the public and the press (no retraction there). This page links to the Bicep/Keck collaboration web page. Nothing on their web page anywhere acknowledging that their original claims were a mistake.

Yes, science worked, but with no help from the BICEP2 people. Their attitude seems to have been (and continue to be) that there is nothing wrong with making highly publicized and hyped discovery claims that turn out to be bogus, because other people will clean up your mess if you are wrong (and, it's all the Nobel committee's fault for making you do this…).

12. **ay**
April 22, 2018

This is the first time I have watched the video. It was squirm-inducing. I also thought that it was impolite to refer to Linde as "the founding father of inflation".

13. **S**
April 23, 2018

I've always felt a little bad for Linde, though. I have my issues with various of his behaviors, but it does appear to me (unlike Tim May) that, in this instance, he was genuinely surprised, and on video, and that he actually tried a little bit to be cautious ("Let us hope it is not a trick,") and still to some degree got laughed at in public through actions entirely of others.

14. **Another Anon**
April 23, 2018

It's always best to simply present your result in the literarature, not to push it too hard, let momentum build over time as it's importance is understood. There's nothing worse than some scientists endlessly trying to sell their result, which they are convinced is so important. We're not salesmen, we're scientists. We've got class.

15. **Low Math, Meekly Interacting**
April 23, 2018

I'm with Tim May. Exhorting one and all to meet the highest ethical standards is a worthy cause that I won't debate. But the practical reality has always been, and likely will always be, that if an incentive structure exists that rewards bad behavior, then there will be bad behavior. Science often works well when human competitiveness (which is also part of the bad incentive structure) can be harnessed as a kind of innate immunity…so long as there is an objective standard by which the results of competitors can be judged. To the extent we continue to value accurate accounting of natural phenomena over some alternative, scientific inaccuracy is something humanity can cope with.

Of course, it helps a lot when the natural phenomena in question can be observed. Lose observable reality, in the conventional sense, and you lose science, as far as I'm concerned. Here, fortunately, science really did work. It doesn't have to be pretty.

16. **Peter Woit**
April 23, 2018

LMMI,

My problem really isn't with the ethical issues of how scientists compete with one another to do science (although, when caught very publicly doing something unethical, it would be nice if the people involved would at least say they were sorry…). The unusual part of this story was the full-court press by scientists and their institution's press offices to promote a bad scientific result and get it on the front page of the New York Times, coupled with a seeming complete lack of concern once it became clear that what was being promoted was an epic fail. For instance, I don't think there's any conceivable excuse for the BICEP2 people and the Stanford press office not taking down that YouTube video, or at least editing the associated text to explain what happened.

17. **srp**
April 23, 2018

Perhaps this thread will move the Stanford press office to clean things up.

18. **S**
April 24, 2018

Perhaps taking it down could be construed as somehow covering their tracks? I think editing the text seems like the best solution.

19. **piscator**
April 24, 2018

This isn't complicated. If you submit a paper to a high-profile journal, get it accepted, put out a snazzy press release to accompany the publication of the paper, and then it all ends up falling apart because the results evaporate, *THAT* is how science works – and such a route is still filled with large egos and dreams of glory.

If you announce the result via a press conference *BEFORE* having the paper accepted in a journal then *you have to be correct* and there are no excuses for error. There can't be any pleading afterwards that this is the scientific process in normal operation, because it's not.

This whole episode was an embarrassment and it should have had measurable negative effects on the careers of those involved. It didn't.

20. **Peter Woit**
April 24, 2018

srp,
I don't think the problem is the press office, it's the BICEP2 scientists. Looking through all their various current webpages about their work, it's nearly impossible to find anything that explains their heavily publicized results were wrong. The closest thing I could find was this:
https://kipac.stanford.edu/research/projects/bicepkeck-array
which tells us
"BICEP2 detected this B-mode polarization in its three year data set with a strong significance. Interpreting this as primordial gravitational waves, this places a constraint on the tensor to scalar ratio at r=0.20+0.07/-0.06. This is strong evidence for cosmic inflation, as well as the first direct image of gravitational waves, and direct empirical evidence of quantized gravity. However, the joint analysis with Planck satellite data followed and showed that the galactic dust seems to contribute much of this B-mode polarization measured by BICEP2. "
No admission there was any problem with what they did in March 2014, and "seems to contribute much" is highly misleading.

21. **Shantanu**
April 24, 2018

Does anybody know when Planck, SPTpol and ACTpol are coming out with their own results for r?

22. **Low Math, Meekly Interacting**
April 24, 2018

To be clear: I pretty much agree with everything said. There's no excuse.

Maybe my expectations have hit rock bottom, but I'm encouraged that we aren't in a branch of the multiverse where BICEP2's results are being touted as proof thereof, or anything else of interest beyond cosmic dust backgrounds. There was a better experiment, a joint analysis, and no one need consider the question of primordial B-mode polarization in the BICEP2 data set ever again.

The fact that "science worked" is still a remarkable thing, I guess. Given that Keating and his ilk really are out there, it's something to be thankful for, at least a little.

23. **A.J.**
April 24, 2018

I am reminded of a story I once heard about Leo Kadanoff. Apparently, on the day Ken Wilson was awarded the Nobel prize, Kadanoff showed up in the department wearing a three-piece suit. When asked why he was wearing such a thing, he laughed and replied, "Well, it's not every day you don't win a Nobel Prize."

24. **william e emba**
April 24, 2018

The fiasco was partly bad luck. The Planck-scraped data were preliminary results regarding dust levels, based on an area of the sky far from where BICEP2 was looking. There were no criticisms until more preliminary Planck information revealed that other areas had somewhat higher dust levels, generating warning flags to the BICEP2 extrapolation. Ultimately, the dust levels where they were looking turned out to appear at the same magnitude of the purported B-modes.

However, it is just as premature to state that BICEP2 actually observed dust as it had been to claim they observed B-modes. It is currently unknown what the BICEP2 data represents, as they only measured one frequency. BICEP3 is measuring several frequencies, and combined with the BICEP2 data, they should be able to tease out the difference between B-modes and dust.

As for ethics, that's one big minefield. I neither agree nor disagree with the ethical criticisms of the BICEP2 collaboration. There are those that claim that all data, all preliminary deductions, and so on, ought to be shared promptly with the scientific community, instead of hoarded to maximize the original researchers' return on investment, as is commonly done. From that point of view, the Planck collaboration was in the wrong.

25. **milkshake**
April 25, 2018

That radio talk is pretty disturbing – he is hinting that scientific bias and groupthink of his colleagues in cosmology are actually rooted in their leftist politics and their godlessness. Then he explains how the Nobel committee is so non-transparent and possibly also unethical, and unwilling to listen. He goes on and on how Nobel is essential to the right functioning of science, and how he was elbowed out from the BICEP collaboration by his fame-thirsty colleagues who wanted maybe the Nobel for themselves… And I don't understand what Torah has to do with anything in his field unless he wants to win Templeton instead.

26. **Chris Oakley**
April 25, 2018

Kadanoff's statement seems to be missing "… when you expected to" at the end – I expect every day of my life to be a day that I do not win a Nobel Prize, and yet rarely wear a three-piece suit.

I cannot speak for other fields, but definitely feel that exaggerated regard for this particular trophy is part of the problem in high-energy physics. Following the 1979 Glashow, Salam, Weinberg award HEP theorists seem to have wasted inordinate amounts of time building models as Nobel Prize lottery tickets instead addressing the real underlying problems of quantum field theory.

27. **Peter Woit**
April 25, 2018

william e emba,
According to Spergel and others, the problem with the BICEP2 use of the Planck preliminary data was not that it was preliminary, but that they ignored the notation "not CIB subtracted" on the slide. If they had taken that into account, they would have found that it implied their result could easily be just dust. This wasn't bad luck, it was incompetence.

What BICEP2 was trying to do with Planck was get access to their unpublished data that they needed, without them finding out that they had interesting unpublished data. You can make the argument that all experiments should share unpublished data, but that's exactly what BICEP2 was trying to avoid (what they ended up with, a shared analysis with Planck). Yes they were unlucky, in the sense that they didn't get away with this.

The attempt to argue that maybe BICEP2 was right is nonsense. They claimed observation of r=.02, not sure what current limits are, but they're below r=.01.

28. **Peter Woit**
April 25, 2018

milkshake,

That is disturbing. Watching things in this field go from bad to worse, ending up with multiverse mania, I've always thought something like "at least it can't get worse than that". This makes clear there is something worse possible: multiverse pseudo-science becoming part of our culture's sad descent into tribalism and culture wars.

29. **Cobi**
April 25, 2018

"I'm afraid that on the multiverse issue he has the stronger argument: multiverse proponents are making a huge mistake using that to go to war with religion."
Are "multiverse proponents" really doing this? From the video you linked it seems more like Keating is the one building a fake dichotomy between the multiverse and god.

30. **Peter Woit**
April 25, 2018

Cobi,
For one of many examples, see for instance
https://www.preposterousuniverse.com/writings/dtung/

31. **GoletaBeach**
April 25, 2018

Humans will be human. Nobel Prizes are a sociological phenomenon… not a scientific one. Science doesn't need them, but they do motivate some humans to discovery, and motivate other humans to jealousy.

Humans, not science, decide funding priorities.

Was it Benjamin Graham, a stock analyst, who said… in the short turn, the stock market is a popularity contest, and in the long run, a weighing machine? In the short run publicity and politics do decide science funding and priorities, in the long run, science *should*. And, I might add, in the long run, we are all dead.

Short run decisions will always be error-prone. It is up to all of us to focus on the science in the long run, and get better results. And seems to me in the B-modes, science did win in the end. I can forgive and overlook all the wrong BICEP hype… it is part of the short-term transient of human error.

I think Keating and BICEP also warrant a fair amount of forgiveness, and it doesn't really bother me that Kovac has tenure.

Fermi's Nobel Prize was partially for the discovery of the new elements Hesperium and Ausonium, which were flat wrong. Ida Noddack even pointed out the flaw in Fermi's arguments, prior to the Nobel Prize. Science lost in the short term then.

But Fermi's techniques of neutron bombardment still were a stunning advance in science. BICEP also got the ball down the field.

Keating ain't my favorite guy. As the old saying goes, though, sometimes it takes a pig to find a truffle. But, as truffle hunters know, the pig usually eats it, which is why dogs are used by truffle hunters. Yup, we experimental scientists are just pigs and dogs.

But in the long run, with concerted effort, a few truffles can be found, not eaten, and actually discerned and appreciated by everbody. I hope.

32. **Stephan**
April 27, 2018

GoletaBeach says: "Humans will be human."

Well, then, when hyping their BICEP2 results in a scientific journal and in the scientific press and in science departments, the Kovac et al. should have stated that they arrived at the results not as scientists, but as humans. They should have warned the unsuspecting public.

33. **Jim Given**
April 28, 2018

Perusal of the "educational" channels on TV, e.g. AHC, will show the multiverse frequently being used as part of the large pagan web being spun by those channels. In brief, multiverse beliefs are used to legitimize and "scientize" Heaven, the Hereafter, alternate realities, etc. This web of contrarian belief, typified by the vast mythology of the Ancient Aliens series, is a continuation of the New Age writings of the 1970's. Historians will note that all of this is a recurrence of the Spiritualism of the early Twentieth Century. But like a previous commentor, I see little warrant for blaming the multiverse proponents for such exploitation.

34. **Anonyrat**
April 29, 2018

The Multiverse comes The Atlantic:
Don't Be Afraid of the Multiverse
Interview with John Donoghue of the University of Massachusetts at Amherst.
https://www.theatlantic.com/science/archive/2018/04/dont-be-afraid-of-the-multiverse/559169/

35. **S**
April 30, 2018

Off topic: do you have any sense what this will be about?

https://phy.princeton.edu/events/beyond-space-and-time

36. **GoletaBeach**

April 30, 2018

Stephan said: "Well, then, when hyping their BICEP2 results in a scientific journal and in the scientific press and in science departments, the Kovac et al. should have stated that they arrived at the results not as scientists, but as humans. They should have warned the unsuspecting public."

I'm not aware of a single scientific result that was arrived at by anybody other than a human. Therefore, I don't think there was any need for the BICEP2 folks to make a distinction. We all know they are merely error-prone humans. We all know science is done exclusively by error-prone humans. I tend toward forgiveness for the errors. Stephan, if you don't want to forgive, that is fine, your choice.

37. **Peter Woit**
April 30, 2018

S,
Gross, like many string theorists, has a long history of arguing that "spacetime is doomed", that string theory will lead to some new fundamental theory in which spacetime is not fundamental, but an emergent concept. I think he'd readily admit though that there never has been a convincing specific implementation of this idea. These days people are trying to get spacetime as emergent using entanglement/holography ideas, I'd be curious to hear his take on those.

As usual, I'm less interested in speculation about quantum gravity than in speculation about particle physics. I may be in Princeton on Thursday, and go to Gross's second talk, see here
https://phy.princeton.edu/events/hamilton-colloquium-series-david-gross-kavli-institute-theoretical-physics-uc-santa-barbara
If I do, I'll try and write about it here, and if anyone goes to the first talk and wants to report here, that would be an excellent place to do so.

38. **Jonas Nyman**
May 3, 2018

Most criticism of the Nobel prize is just based on simple misunderstandings. No, the Nobel Foundation would never ever send letters to anyone asking them to nominate. The Nobel Committees (one for each prize) do that, and they send out thousands of such letters, so getting one doesn't imply anything… Finally, the Nobel prizes are awarded according to a set of rules and criteria. The most important are the ones specified in the will of Alfred Nobel. These obviously cannot be changed! The will forbids awarding groups or institutions, the peace prize being an exception. In fact, according to the wording of the will, it is not the people that are being awarded, it is the discoveries. When the Nobel committees decide on the prizes, they first decide which discovery shall be awarded, and only then do they decide on the individual(s). Besides the will itself, there is an agreement between the executor of the will and the Nobel family, which clarifies some things that the will does not make any recommendations on. For instance, it was agreed that a prize can be split between two or three individuals, and it sets out rules for who are eligible to nominate.
The purpose of the Nobel prize is to honor the last will and testament of Alfred Nobel. It makes no sense for random people to have opinions about how he wrote it.

39. **Jim**
May 6, 2018

I remember the URSI meeting in Boulder in I think January 2012, when I counted at least 9 different experiments all trying to measure B-mode polarization in some form, through different means. I thought (and still think) this was an absurd allocation of resources, but then I don't get to make such calls.

The two big considerations were what l (basically angular frequencies) you'd be sensitive to (Planck and BICEP were complimentary in this regard), with what colors, and how well you could eliminate the foreground dust. One experiment's goal, and it may have been BICEP/Keck Array, was to just measure the B modes first. If they could, then they could figure it out later if the signal was from dust or not. If you couldn't, you put an upper limit on what was there to be detected. I don't think anyone in that room thought it really would have even been detected within two years. I left my somewhat related field permanently shortly thereafter

I was very surprised by the early 2014 announcement. Having known and worked nearby a lot of the members, though not Keating, I thought there was no way they would make such a claim without thorough rejection of the dust hypothesis. Everyone I knew thought the B-mode signal from dust would dominate any cosmic signal. So I thought they must have very strong evidence, but, having left the field already, I really couldn't investigate. I must say, the unraveling did not surprise my intuitions, but it did surprise me that the people who made the claims had done so with such disregard for prudence and caution.

I think a lot of this had to do with 9 separate, or largely separate, groups all doing the same thing. How in the world is having so much funding going into these areas a good thing? Everyone just picked up on the hot topic of the day and raced into it. Any idea how to fix that?

Physics News

April 30, 2018
Categories: Uncategorized

Various physics-related news:

- The LHC is back in business doing physics, with intensity ramp-up for the 2018 run ongoing. Today the machine is colliding 1551 bunches of protons, ultimate goal is to get to 2556 bunches. They are at least a week ahead of the planned schedule, which would have only reached 1200 bunches next week.
- There's a conference going on at the KITP this week, discussing the latest state of dark matter theory and experiment. By the way, see here for how prominent theorists communicate these days instead of using email…
- At the Atlantic and Knowable Magazine, Tom Siegfried provides yet more multiverse coverage. Seems that he's at work on a multiverse book.
- At the Edge, Sabine Hossenfelder has an on-target analysis of the situation in fundamental theoretical physics. The problems she points to are ones that motivated books by Lee Smolin and myself back in 2006. Things haven't improved since then and I hope she'll have better luck generating concern for these issues than we did.
- At Alta, Jennifer Ouellette has a fascinating account of the maneuverings for credit among the many observers of the neutron star merger last year. It sounds like some of those involved are suffering from the same disease as Brian Keating: not reality-based conviction that they're in the running for a Nobel Prize.
- Director Claire Denis is now at work on a sci-fi movie entitled High Life that sounds more promising than the last black hole movie. She's getting scientific advice from Aurelien Barrau and comments:

> If there are theories about me, I'd rather not know. Astrophysics – now that's fascinating. String theory, worm holes, the expanding universe, the Big Bang versus the Big Bounce – those are the kind of theories that make you feel like living and understanding the mystery of the world. Film theory is just a pain in the ass.

Update: One more. David Gross will be back in Princeton this week, giving talks on gravity and particle physics. I may head down Thursday to revisit scenes of my youth.

Update: I just watched some of the KITP dark matter "debates", see here. Highly recommended if you want to hear informative exchanges between experts on the subject, see especially the Dan Hooper talk (and the MOND/dark matter debate here).

Update: For machine learning experts who want to try their hand at an HEP data analysis problem, a Kaggle competition to build a track reconstruction algorithm opened yesterday. For details, see here and here.

Update: Gross's Princeton talk on the "Future of Particle Physics" was not much different than this one from a couple years ago (with the enthusiasm for supersymmetry deleted). I was thinking of writing something about this, comparing it to similar talks from way back when (see for instance here). Probably better though to wait for a better opportunity to write something substantive about where the path followed by Gross and others over the years has ended up.

Update: Video of the David Gross Princeton talk on the Future of Particle Physics is available here.

Comments

1. **Dave Miller**
   May 1, 2018

   Peter,

   Bee Hossenfelder's analysis of the sociology of physics, to which you linked, is one of the clearest explanations I've seen: I urge everyone to read it.

   Incidentally, much of what Bee writes was anticipated by Szilard in his little satire, "The Mark Gable Foundation," published in his *The Voice of the Dolphins and Other Stories*, which is also well worth reading.

2. **Azadi**
   May 2, 2018

   Peter,

   I was wondering if you had seen today's latest news item about the late Professor Hawking's final paper, in which he apparently disclaims the string-landscape multiverse (i.e. with the different laws of physics applying in the pocket universes, courtesy of M-Theory) in favour of Tegmark's "level 1" multiverse (an extension of our own, all having the same physical laws and physical constants).

   I haven't read the original text, so I am basing this upon the media accounts.

   The reports, somewhat irritatingly, appear to be exaggerating the significance of this: as if Hawking, in this paper, was making a ground-breaking assertion that significantly moved the field on, in terms of explaining the so-called fine-tuning problems without recourse to a mere probabilistic, selection effect that has no predictive power.

   But I'm glad, at least, that Hawking's last piece of research appears to have taken a stab at the string-theory-multiverse mania.

   See:

   https://www.theguardian.com/science/2018/may/02/stephen-hawkings-final-theory-sheds-light-on-the-multiverse

   "…Reality may be made up of multiple universes, but each one may not be so different to our own, according to Stephen Hawking's final theory of the cosmos.

   The work, completed only weeks before the physicist's death in March, paints a simpler picture of the past 13.8 billion years than many previous theories have proposed.

   Published on Wednesday in the Journal of High Energy Physics, the new work is the result of a long collaboration with Thomas Hertog, a Belgian physicist at the Catholic University of Leuven. "We sat on this one for a very long time," Hertog said. "I do believe he was really fond of it."…

In the latest work, Hawking and Hertog challenge that view. Instead of space being filled with pocket universes where radically different laws of physics apply, these alternate universes may not actually vary that much from one another…

“In the old theory there were all sorts of universes: some were empty, others were full of matter, some expanded too fast, others were too short-lived. There was huge variation,” said Hertog. “The mystery was why do we live in this special universe where everything is nicely balanced in order for complexity and life to emerge?”

“This paper takes one step towards explaining that mysterious fine tuning,” Hertog added. “It reduces the multiverse down to a more manageable set of universes which all look alike. Stephen would say that, theoretically, it’s almost like the universe had to be like this. It gives us hope that we can arrive at a fully predictive framework of cosmology.”…”

3. **Fred P**
   May 2, 2018

   “For machine learning experts who want to try their hand at an HEP data analysis problem…” – This limit shocked me:
   “The software … should run on a i686 processor with 2GB (tbc) of memory.” source: https://sites.google.com/site/trackmlparticle/

   The i686 is a more than 22-year old processor (evidence: https://en.wikipedia.org/w/index.php?title=P6_(microarchitecture)&oldid=837000310) in a field where the main recent improvements rely heavily on much more modern hardware to take large problems and get results in reasonable timespans. Here’s a somewhat dated summary if you’re interested: https://blog.algorithmia.com/hardware-for-machine-learning/

   I am unclear that they will get useful results from this competition. Either the environment they’ll need will be radically different from the one they’re testing in, thus skewing any results to ones that may be sub-optimal for their purposes, and/or they’ll have to run using toy datasets that may have dubious applicability to the problems they are interested in.

4. **Peter Woit**
   May 2, 2018

   Azadi/Doolittle,
   I was trying to ignore this, finally gave in (see the latest posting).

5. **Carlos**
   May 4, 2018

   Dave,
   Thank you for mentioning Szilard’s nice piece (from 1948!). It’s one of the best science satires I’ve read in a long time, and it should definitely be more widely known. It makes us really wonder how far back has science started to become sick. “Off your teeth!”

6. **fg**
   May 4, 2018

   to Fred P: my guess regarding this constraint is that this software would be run as part of very high level triggers, very close to the detectors themselves, where only radiation hardened electronics can survive. And the best way for electronics to be robust against radiation is to have rather big transistors (much larger than the current state of the art in miniaturization).

7. **Anon**
   May 4, 2018

   fg: I don’t know the reason for i686 condition but the higher level triggers of the LHC experiments are on the surface, so radiation is not the reason. The experiments update their computing every year or two with several thousand of the latest computers, and are either using already or developing GPU systems for the future.

8. **fg**
   May 5, 2018

   Anon,
   My bad… I meant low level triggers. Having tracking information in these early triggers would open up new ways of selecting some rare events.

9. **Jan Dybicz**
   May 7, 2018

   Peter,

   Is there a video of David Gross’s two Princeton talks available?

10. **Peter Woit**
    May 7, 2018

    Jan Dybicz,
    Not that I know of. At the talk I attended, there was video being recorded, don’t know where/when it might appear publicly.

This Week's (Stale) Hype

May 2, 2018
Categories: Fake Physics, Multiverse Mania

The usual hype machine is at work this week, with the usual mechanism:

- University press offices and grant agencies put out irresponsible hype about the work of one their faculty or grantees. In this case, it's Taming the multiverse: Stephen Hawking's final theory about the big bang from Cambridge, and Stephen Hawking's last paper, co-authored with ERC grantee Thomas Hertog, proposes a new cosmological theory, in which universe is less complex and finite from the European Research Council.
- This gets distributed via services that either just reprint such press releases (phys.org or EurekAlert! in this case), or rewrite with added hype (Stephen Hawking's Final Theory About Our Universe Has Just Been Published, And It Will Melt Your Brain).
- Journalists then write stories based on these press releases, which appear all over the place (see here, here, here, here, here and here for example). At this point, whatever was in the original press releases that tried to add caveats or stick to reality gets abandoned, and a completely misleading headline gets slapped on at a final level of the hype machine.

Normally I try to defend the journalists involved, feeling that the most irresponsible behavior is coming from the scientists themselves. In this case, Hertog has a lot to answer for, but it's not Hawking's fault since he's dead. Any semi-competent journalist should have realized that this is not news: the same stories had already been written and published a month ago, and then conclusively debunked in many places (see here, here and here for instance).

This is rather depressing, making one feel that there's no way to fight this kind of bad science, in the face of determined efforts to promote fake physics to the public. It's one thing for journalists to be misled by a new variant on an old debunked story, but that they're getting misled again by exactly the same story is a new development.

Update: More hype from Hertog, this time from Scientific American, which tells us that his work is based on

> string theory, one of the most dominant emerging paradigms in 21st-century physics.

Hertog is asked about the undeniable fact that his work predicts nothing:

> **How do you counter critics of string theory, who argue it cannot be tested?**
>
> I don't agree with this statement; it is not my intuition that string theory can't be tested. We may already have observations based on studies of the universe's large-scale structure and evolution that are telling us something about the nature of quantum gravity. Of course, further theoretical work will be needed to arrive at a mathematically rigorous, fully predictive framework for cosmology.
>
> **So, your paper's key predictions depend on the reality and nature of inflation. Will that be testable?**
>
> There are the obvious observables, yes. Just as it amplified tiny quantum fluctuations in the early universe, inflation should have amplified gravitational waves in the early universe, too. Gravitational waves are ripples in spacetime, first predicted by Einstein, that were finally observed just a few years ago—but the ones we have observed come from black holes and other stellar remnants in neighboring galaxies, not from the primordial universe. These amplified gravitational waves would leave their imprint on the polarization of the cosmic microwave background. Astronomers are actively trying to detect this polarization pattern.
>
> **So you are optimistic they will succeed?**
>
> Well, our theory certainly predicts that primordial gravitational waves should be there at some level.

As Sabine Hossenfelder points out, saying "at some level", without even an order of magnitude estimate, is not a prediction at all. In addition, this non-prediction is exactly the same non-prediction of the theory (eternal inflation) that Hertog is claiming his work challenges.

Comments

1. **David Appell**
   May 2, 2018

   Unfortunately many science journalists let press releases led them around by the nose. They're about all they pay attention to.

2. **Anthony Moose**
   May 2, 2018

   "This is rather depressing, making one feel that there's no way to fight this kind of bad science, in the face of determined efforts to promote fake physics to the public."

   On the plus side, most members of the public are so scientifically illiterate, and so disinterested in science journalism in general, that this will hardly have any effect on their understanding of physics, or lack thereof.

   The people who do know enough to get in trouble are probably either already enamored with multiverse hype, or know enough to be skeptical of headlines and articles like these.

   I could definitely see why it's frustrating, though. It's probably not unlike trying to fist-fight the Pacific Ocean.

3. **Hal Porter**
   May 3, 2018

   I'm a former science journalist (among other topics, though never focused on theoretical physics–how,about indoor air pollution?), now 70 y.o. I agree with Appell; I understand Moose's despair/cynicism but follow that path and one ends falling into any number of social abysses. There would simply be no hope other than some farcically improbable intellectual autarchy. The project of universal education, in the broadest sense, must be followed if one is to avoid another dark age, a la Dick, etc. It seems to me this blog has congruent aims.

   About the journalists, call up the sources! Research! Reporting! Don't call up or otherwise contact scientist just to get two views to create a he said, she said dialogue, but try to understand, as much as possible (especially in theoretical physics) what the author of the whole study is trying to say. Even I could understand 2/3 of the papers out of Fermilab delimiting the mass of the Higgs boson–if such existed–back, what, 7-8 years ago. With that awareness, some 4-5 hours of genuine research (over several days, you’ve got to actually think/absorb) one could produce a modest, 300-500 word story on the subject, crudely addressing method, means, and the meaning of results ruling out at that point. But you’ve got to do the thinking, the research, and talk to people (who may not always be right)! What intellectual laziness–or cowarice or greed–does laziness is.

Woit’s objections here are very fair; I used to blame the college PR Depts. more, but the research scientists have a duty to anti-hype these releases; though in fairness may be under a lot of pressure and dealing naively in areas where they they have no training. Furthermore, in publishing contexts, the writer DOES NOT WRITE THE HEADLINE (my horror stories abound).

Of course these issues nowadays quickly conjoin with anti-science anti-global warming political/financial agrandizement memes. The good fight against the barbarians risks losing sight of the need for intellectual and ethical rigor on our side also.
Sorry for the extended rant.

4. **Thomas**
   May 3, 2018

   This is exactly what happened today here in Germany. Every big and every wanna be news magazine brings a front story about this new “bahnbrechende” theroy from Hawkins. The funny thing is that the name Thomas Hertog is never ever mentioned. This is a déjà vu because it all happened shortly after his death. The same papers the same news all over again … 🙁

5. **Gram Nasi**
   May 3, 2018

   Try what? It’s “try to defend” not “try and defend”.

6. **Peter Woit**
   May 4, 2018

   Gram Nasi,
   Fixed.

7. **Jim Holt**
   May 4, 2018

   @ Gram Nasi
   “Try and defend” is a perfectly literate use of the rhetorical trope called hendiadys. Peter did not deserve your censure.

8. **Kevin**
   May 4, 2018

   An exception: today, the Bavarian radio programme BR2 summarized the paper as follows: one more speculation in an existing huge number of speculations; experimental check with gravitational waves not correct; nobody dares, for reasons of piety, to voice public disagreement; but almost complete private disagreement within the research community.

9. **David Appell**
   May 4, 2018

   Anthony Moose says:
   On the plus side, most members of the public are so scientifically illiterate, and so disinterested in science journalism in general, that this will hardly have any effect on their understanding of physics, or lack thereof.

   Huh? In the US, the situation is the exact opposite of this.

10. **lun**
    May 8, 2018

    In related news, Italians are celebrating the 50th anniversary of string theory today.
    Is this a midlife crisis situation? :-/
    http://home.infn.it/it/comunicazione/news/2900-50-anni-di-teoria-delle-stringhe

US HEP Budget News

May 14, 2018
Categories: Uncategorized

There's a HEPAP meeting today, with news about the US HEP budget situation, presentations here. Since the 2016 election physicists have been worried about how the Republican Congress and Trump administration will treat scientific research in general and physics research in particular. For instance, I see that FQXI has just announced the winners of its latest essay contest, with the second place essay by Alyssa Ney (on "The Politics of Fundamentality") claiming that "it is easy to point to trends in allocation of research funding away from basic research in the sciences", noting:

> Another indication of the present threat to physics funding is U.S. President Donald Trump's 2018 proposed budget. This includes a decrease of 18.4% to the Department of Energy's high energy physics program and a cut of 19.1% to nuclear physics. The budget slashes funding of basic science at the National Science Foundation (NSF) by 13%. http://www.sciencemag.org/news/2017/05/what-s-trump-s-2018-budget-request-science

The actual enacted budget numbers for DOE HEP physics are:
FY2015 \$766 million
FY2016 \$795 million
FY2017 \$825 million
FY2018 \$908 million

It's true that the Trump administration produced a FY2018 budget calling for a cut to \$627.7 million for DOE HEP, but that was no more to be taken seriously than anything else Trump says, and the Republican Congress instead passed a huge (10.1%) increase, to \$908 million. For FY2019 the Trump administration is calling for a DOE HEP budget cut to \$770 million (15.2% cut), but, again, no one should be taking that seriously. It's still early in the budget process, but the House subcommittee dealing with the FY2019 DOE budget has responded to the request for a cut of 13.9% to DOE Office of Science by instead passing a 5.4% increase (it's not yet known what the HEP part of that will be). For latest budget numbers, see here.

Over at the NSF, numbers for the Physics directorate for FY2017 are not yet available, but the enacted budget numbers for the NSF as a whole are:

FY2015 \$7,344 million
FY2016 \$7,494 million
FY2017 \$7,472 million
FY2018 \$7,767 million

The Trump administration requested a cut of 11% to NSF for FY2018, instead got a 3.9% increase. They are requesting a 3.8% cut for FY2019, the House subcommittee dealing with this instead has passed a 5.2% increase (to \$8,175 billion).

It should be uncontroversial to point out that the US budget process has been seriously broken for a while. FY 2018 started on Oct. 1, 2017. DOE HEP only recently got the \$908 million number for its budget and now is scrambling as it is "faced with a year's worth of funding actions in around 4 months". They've been spending their time preparing details of the Trump administration fantasy of how to cut 13.9% out of the FY2019 budget, instead of making rational plans for the future about how to spend the actual budget numbers they will get (OK, maybe they're dealing with reality in secret…).

In order to avoid any misunderstanding about what I think about the current situation, my take on it is:

- Until the 2016 election, US scientific research spending was relatively flat, due to the Obama administration's attempt to reduce deficit spending and respond to Republican outrage at the budget deficit and demands for reductions in non-defense federal spending. We're now starting to see large increases in research spending, as it becomes clear that whatever the current Republican party cares about, it's not the deficit or the level of federal spending.
- Physicists outraged at the Trump administration proposed research cuts need to realize that this, like everything else, is just theater, and understand that the current Republican party has just as little interest in cutting physics research as it ever had in reducing the deficit. Take a look at reality and stop complaining about your research funding. Fueled by huge increases in inequality in US society, truly awful things are happening in this country, but they're not happening to you, quite the opposite. Stop whining about your science not getting enough respect and funding, and instead try and figure out what can be done to restore a healthy democracy and a more equal society.

Informed comments about HEP funding welcome, those who want to rant about politics are not. Sorry, it's my blog, so I get to explain my point of view, even though I don't want to engage here with the diseased post-truth reality TV show politics today's Republican party has grotesquely exploited to come to power.

Comments

1. **Casey Leedom**
   May 15, 2018

   Just curious: do you know how to break down those budget numbers into specific sub-fields of HEP? And are there any sub-field trends which are interesting?

2. **Peter Woit**
   May 15, 2018

   Casey Leedom,

   See for instance slides 6 and 13 of this presentation:
   https://science.energy.gov/~/media/hep/hepap/pdf/201804/GCrawford_20180514_DOE_HEP_Budget_Planning_and_Execution.pdf

   In recent years HEP theory had seen significant cuts, those are dramatically reversed in the new FY2018 budget with an increase of nearly 30% for theory and computation.

3. **Anonymous**
   May 15, 2018

   Slide 17 of that presentation that shows that "theory" as previously conceived is not actually seeing an increase. In fact, it still receives a (small) cut this year, and a dramatic one under the FY19 request. There is also language there that emphasizes neutrino physics theory.

   The increase comes entirely in the form of the new category of "Quantum Information Science." We'll have to see what research programs are funded under that initiative as it is new.

I was not at HEPAP, so don't know what words went with the slide, so take this with a grain of salt.

4. **Peter Woit**
May 15, 2018

Anonymous,

Thanks for pointing that out. The "Quantum Information Science" description includes "foundational concepts relating particle physics and QIS", so presumably includes research previously funded from the theory account on the hot topic of getting spacetime out of quantum information science, as in the upcoming IAS summer school: https://pitp.ias.edu/
A massive new funding stream available for this helps explain to me some of its popularity, which otherwise has always seemed to me kind of mystifying.

5. **David Roberts**
May 15, 2018

Mildly tangential, but can one extract numbers for mathematics funding? Asking for a friend…

6. **Peter Woit**
May 15, 2018

David Roberts,
As far as I can tell, numbers for the FY2018 NSF budget by directorate are not yet available. It is known that the overall NSF budget for research is up 5%. I don't know of any reason math research should be disfavored compared to other topics, so a roughly 5% increase seems like a good guess.

7. **Blake Stacey**
May 16, 2018

In re the new "hot topic of getting spacetime out of quantum information science", I'm vaguely concerned that I haven't seen substantial push-back against it. Ideas that receive no challenges don't become good ideas. The only critique I've noticed in print is the comment by Giddings (arXiv:1803.04973 and references therein) that defining entanglement presumes a decomposition into subsystems, which means you have to postulate a spacetime or maybe some kind of proto-spacetime structure first, before you can try to build a spacetime out of entanglement.

At conferences, I've chatted about the "build spacetime from entanglement" notion with quantum-information people who weren't blown away by the idea, but that's just pub talk.

So, I too am a bit mystified by the appeal, but "follow the money" might turn out to be good advice….

8. **Peter Woit**
May 16, 2018

Blake Stacey,
My own general problem is that the toy models being studied look interesting and I can see why people do this (with funding just one reason), but I haven't seen a plausible conjecture about an underlying theory that would give emergent spacetime and look like the real world.

This is off-topic, will return to it some day in a more suitable context.

9. **Nielson**
May 17, 2018

For some context, according the Bureau of Labor Stats in 2016 there were 19900 physicists and astronomers in the USA. This is obviously a larger pool than those that split the 700+ million dollars for merely high energy physics, but even if we divvy it up as such, it still comes to 35000 dollars each (as an overestimate), roughly the amount of income tax you'd pay on the typical 100-120K salary.
Comparatively, the entire US government spending (including all kinds of entitlement programs which were paid into at one point) is about 4 trillion, divided over say 325 million people, so an average of around 12000 dollars.

10. **Bernhard**
May 18, 2018

The 5% increase for the DOE is real, I saw it first at:

http://www.sciencemag.org/news/2018/03/science-department-energy-gets-hefty-raise-final-2018-budget

The only negative side that I know of for a fact is that both the DOE and the NSF are disfavoring strongly small research groups to operate and favoring only the larger ones. Lone faculty investigators seeking for small grants to keep theirs groups alive are a thing of the past.

Further, the NSF has some weird criteria on which topics are eligible or not for funding, instead of having a broader view and let scientific competence be the ultimate judge.

(And of course, nothing of this is can be blamed on the government.)

11. **Marc**
May 18, 2018

It is always disturbing when someone says "I know for a fact…" and then says something that is false. "Lone faculty investigators seeking for small grants to keep their groups alive are a thing of the past".

If you go to NSF fastlane's award search, and look at all proposals funded by the HE/Cosmo Theory program (Dienes as program officer) that started in the 2016-17 fiscal year, you will find 27 proposals (along with a few conferences) funded. 20 of them are lone faculty investigators.

12. **Peter Woit**
May 18, 2018

Marc,
It may be relevant that Bernhard is an experimentalist.

13. **Dan Mittleman**
May 19, 2018

Many US researchers (anecdotally, the majority) in condensed matter physics and photonics, including both experimentalists and theorists, rely heavily on single-PI NSF grants for funding their groups. This may not be true in HEP, but HEP is not the only thing that qualifies as physics.

14. **Bernhard**
May 20, 2018

Marc/Dan,

It's good to know that what I have said cannot be applied as a general aspect of research funding of US HEP.

My experience with US HEP funding is, as Peter pointed out, as an experimentalist. I have not, admittedly, investigated this deeply – but I have witnessed all groups with lone investigators with lone faculty members in my experiment to have their funding cut. I have discussed this with several colleagues and it's not really much of a secret.

About NSF, again, this is an experimentalist perspective – they fund only certain physics and detector activities/not all and that is irrespective of competence. It's not a healthy state of things if you ask me.

If things are brighter for theory and condensed matter I'm really glad.

15. **Douglas Natelson**
May 20, 2018

To build on Dan's comment, a tension within NSF condensed matter, and more broadly the DIvision of Materials Research and big swaths of the engineering directorate, is trying to balance single investigator awards and center grants. If anything, the number of PIs supported through center grants has been falling in an attempt to avoid cannibalizing the single investigator awards further. More on topic, I've learned quite a bit recently about collaborations in astro and astro-particle (incl dark matter detection), and what has really surprised me is the heterogeneity in funding models.

This and That

May 21, 2018
Categories: Langlands, Uncategorized

I've been trying to find time to write about some books I've been reading. Maybe later this week. In the meantime, some things that may be of interest:

- This week in Norway there will be various events in celebration of the 2018 Abel Prize awarded to Langlands (see here). If you want to find out the latest ideas from Langlands about geometry and the Langlands program, you better be able to read Russian, so you can read this.

  Langlands will give a lecture on Wednesday, on the geometric theory, followed by lectures from Jim Arthur and Edward Frenkel (streamed here). One would think that this would be a good opportunity for non-Russian readers to find out what Langlands is up to, but it wouldn't surprise me if Langlands lectures in Norwegian…

  This fall the University of Minnesota will host an Abel conference, dedicated to Langlands and his work.

- Last week the IHES hosted a conference in honor of Roger Godement. Videos of the talks are now available here. The stories of how his political engagement played out in the context of his professional life were something I had never heard about. For instance, I had missed the "Postface" (French version, English version) to one of his textbooks on analysis.
- The Stacks Project has a new website, some discussion of the changes is here.
- It's the 50th anniversary of the Veneziano model and thus the birth of string theory, so various celebrations are going on this year, including this recent one. From the history as given in the talks there, no one would know that this is an idea that didn't work out (twice, actually…).
- There's a very interesting interview with John Preskill at ycombinator.
- A correspondent pointed me to the following, from a review by Alan Lightman of Carlo Rovelli's latest, in the New York Times book review. Lightman disagrees with Rovelli on the low entropy problem of cosmology, suggesting instead that the multiverse is the answer:

  > One possibility, entertained by a number of leading physicists, is that there are lots of universes, the so-called multiverse, with very different properties and initial conditions. Some of those universes may have started in conditions of maximum disorder, with nothing driving change, no distinction between future and past, where atom-size pottery shards gather themselves up to form atom-size teapots as often as the reverse. But some of these universes would have been created, by accident, with relatively high order. We live in such a universe because otherwise we wouldn't be here to discuss the matter. The theory of "quantum gravity," which is still not fully formulated, describes such a continuous creation of universes with random properties and initial conditions.

  Maybe I've missed something amidst the other multiverse mania, but the only person I've ever heard use "the multiverse did it" to explain this entropy problem is Sean Carroll, and it always seemed to me that he had never had any success in getting anyone to take that seriously.

Update: Glad to hear from the comment section that Carlo Rovelli "not appreciate at all the current infatuation with the idea of a multi-universe." Unfortunately the multiverse publicity machine rolls on, with the usual nonsense, see here. I don't agree with Sabine Hossenfelder that the problem is "over-reliance on mathematics". What's going wrong here is bad physics and bad science, nothing to do with mathematics.

Update: I'm glad to hear from Glenn Starkman that the Standard Model is also getting a 50th anniversary celebration soon (June 1-4), see here. Many of the talks look quite interesting, and there will be a livestream here.

Update:The Abel lectures are now online here.

Comments

1. **Pierre**
   May 22, 2018

   The Preskill interview is great–particularly his discussion of the "geometry of entanglement."

2. **atreat**
   May 22, 2018

   To be clear, it is Alan Lightman – not Rovelli – who postulates a multiverse with that quote! Just reading your blog snippet I thought it was Rovelli who was talking about a multiverse, but after reading the article it is clear that Lightman is the one doing so and not Rovelli.

   I haven't read Rovelli's book so maybe it has something about the multiverse in it, but I would hesitate to ascribe any sentiments to him just based on this review. It looks like the reviewer injected his own multiverse nonsense into it.

3. **Peter Woit**
   May 22, 2018

   atreat,
   Thanks, that was unclear as written. I've rewritten to clarify. The quoted material was Lightman's disagreeing with Rovelli and suggesting the alternative explanation that "the multiverse did it".

4. **atreat**
   May 22, 2018

   This – https://arxiv.org/abs/1505.01125 – seems to be Rovelli's account of entropy and it is very much akin to his Relational QM in that he believes these problems are largely a matter of assuming the universe permits an objective privileged perspective.

   The paper mentions Carroll's inflationary multiverse explanation and offers an alternative. So I think it safe to say that Rovelli does not believe any multiverse explanation is necessary.

5. **Carlo Rovelli**
   May 22, 2018

   Just to set the record straight and avoid possible confusion, since the post mentions me and multi-universe: I myself do not appreciate at all the current infatuation with the idea of a multi-universe. I see no compelling evidence for "other universes".

Carlo Rovelli

6. **David Roberts**
   May 22, 2018

   The Stacks Project people, or at least those behind Gerby, the spin-off general-purpose technology for massively hyperlinked and tagged large mathematical documents, are apparel tly working with Lurie on something called Keradon, for his works
   http://www.math.harvard.edu/~lurie/Kerodon/main.html

   No details yet, but Lurie crowdsourced a professional logo https://www.crowdspring.com/logo-design/logographic-for-front-page-of-mathematics-website-2896944/entries/

7. **John Baez**
   May 23, 2018

   Someone from the Scientific American interviewed me about the 50th anniversary of string theory and also the crackpot index, seeming especially interested in the last item:

   > 50 points for claiming you have a revolutionary theory but giving no concrete testable predictions.

   I had to repeatedly emphasize that I don't think string theory is inherently "crackpot". I said that the fascinating thing about string theory is how much it's done for mathematics while providing nothing so far in the way of concrete testable predictions. I'm curious about how much if any of this makes it into the article.

8. **Tuyen**
   May 23, 2018

   I attended Langlands talk in Oslo. Here is part of my notes from the lecture:

   There are 4 different parts in Langlands program: Arithmetic (original one) from 19th century, Finite fields (still arithmetic): Weil zeta functions, Geometric theory: due to Russian schools – some aspects problematic (Langlands doubts about this theory), Physical implication (springs out from the Geometric theory): Langlands is not convinced that there is any application (he wants his name to be removed from this part of the program).

   Langlands was happy that the example he worked out in his paper showed that there is no physical relation, it is a completely mathematical theory (the Geometric Langlands program)!

   Yang-Mills comes in the paper by Atiyah-Bott, but to Langlands that is just calculus of variation, no physics involved.

   An interesting quote from him: "As far as I know, all of mathematics goes from the number 1. "

9. **UpDownUp**
   May 24, 2018

   Hertog thinks his and Hawking's ideas will be testable:

   https://www.scientificamerican.com/article/a-conversation-with-thomas-hertog-one-of-stephen-hawkings-final-collaborators/

10. **TG**
    May 24, 2018

    A nice, and accessible even to a non-specialist, review of the mutual contribution of string theory to physics and mathematics was published by Natalie Paquette in Inference Magazine (no multiverse is mentioned):

    http://inference-review.com/article/a-view-from-the-bridge

11. **Peter Woit**
    May 24, 2018

    UpDownUp,
    I just wrote a little bit about this as an update to the earlier posting on this topic, see

    http://www.math.columbia.edu/~woit/wordpress/?p=10260

    I think Hertog's attempt to claim these ideas are testable via CMB measurements is really outrageously misleading.

    TG,

    Thanks. I saw that when it first appeared, it is quite good. Intended to mention it here, but it looks like I somehow missed doing that.

12. **LMR**
    May 25, 2018

    TG,

    That's indeed a very nice survey on TFTs, Donaldson theory, mirror symmetry and monstrous moonshine. Thank you.

    Peter,

    It seems opportune to mention this very curious Quanta article on mirror symmetry:

    https://www.quantamagazine.org/three-decades-later-mystery-numbers-explained-20180503/

    Quanta's Kevin Hartnett seems to be very interested in mirror symmetry lately, as it's the second article he's written on the topic in as many months:

    https://www.quantamagazine.org/mathematicians-explore-mirror-link-between-two-geometric-worlds-20180409/

What’s interesting about the “mystery numbers” article is that it appears to be announcing a forthcoming paper from a massive collaboration – involving Columbia’s Abouzaid, as well as Ganatra, Iritani and Sheridan – that would use tropical geometry to give an explanation for why multiple zeta values turn up in mirror symmetry.

It all sounds like terribly exciting mathematics, if everything works out.

13. **KC**
May 28, 2018

Langlands needs substantial editorial help for that Russian document. I kept having to stop while trying to read it because of errors all over the place. I showed it to a native speaker, who couldn’t stand looking at more than a couple of paragraphs. I don’t think he should have publicly posted a document in poorly written Russian, particular one that long. Anyone who can read it will be discouraged by so many distracting mistakes.

14. **Bernhard**
May 28, 2018

The 50 years SM Symposium looks very, very interesting! I really envy whoever is able to attend it in person. Also, happy to see Weinberg is joining. If I’m not mistaken he’s the one who actually coined the name.

But too bad Higgs is not among the speakers.

15. **David Roberts**
May 28, 2018

It would be nice to get the ideas summarised, at the very least, in English. Langlands pretty much said in his acceptance speech that this paper would be his last substantial piece of work, so we can’t expect too much further along these lines from him.

16. **NoGo**
May 28, 2018

I tried to read this article by Langlands…
I cannot comment on the mathematics of it, as most of it is above my head, but as a native Russian speaker, I wish he had someone to go over his article and fix it.
His written Russian is not very good, to put it politely… pretty much every sentence has at least one grammatical error, often more… Most of the time I could still understand it, although often enough I had to pause to figure out what the sentence was supposed to mean, and some I could not parse at all.
I did not make it past the Introduction…

When Einstein Walked With Gödel

May 28, 2018
Categories: Book Reviews

Jim Holt has a new book out, a collection of essays entitled When Einstein Walked with Gödel. I wrote enthusiastically about his last book (Why Does the World Exist?) here and, if you have any interest at all in the overlap of mathematics, science and philosophy, I recommend this one just as highly. Holt is pretty much a unique example of someone able to regularly write about topics in this area in a manner that is both enlightening and entertaining.

This is a book of essays written on different topics for different venues, of too great a variety to try and itemize here. Most of them have some sort of connection to mathematics and philosophy, typically centering on one idea or one, often historical, figure. Holt loves to write about the most abstract of ideas (the subtitle of the book is "Excursions to the Edge of Thought"), but in the context of the particular very human qualities of the thinkers responsible for them. For example, an essay about von Neumann and his role in the building of an early computer at the IAS includes this description:

> His passion for America's open frontiers extended to a taste for large, fast cars; he bought a new Cadillac every year (whether he had wrecked the last one or not) and loved speeding across the country on Route 66. He dressed like a banker, gave lavish cocktail parties, and slept only three or four hours a night. Along with his prodigious intellect went (according to Klári) an "almost primitive lack of ability to handle his emotions."

In a short essay discussing the thorny "demarcation problem" of how to distinguish science from non-science, Holt describes briefly the ideas of Paul Feyerabend (epistemological anarchism) and Imre Lakatos (progressive versus degenerating research programs). At the same time, he includes the story of their arguments over these ideas in the context of their personal friendship:

> These friendly antagonists exchanged abundant letters on the matter, with a good deal of ribaldry–some of it of a sort that no longer evokes an easy smile. "I am very tired because my liver is acting up which is a pity, for my desire to lay the broads here (and there are some fine specimens walking around on campus) is considerably reduced," Feyerabend wrote from Berkeley. The affection between them is much in evidence…
>
> Philosophically, however, there is no detectable convergence in their positions over their years of correspondence. That is not surprising, really, given how vexed the demarcation problem is.

One of the essays included here is a slightly edited and updated version of a review of my book and Lee Smolin's written back in 2006 for the New Yorker (my blog post about it is here). Of the many reviews of these books at that time, Holt's seems to me the most accurate and insightful take on the two books and the issues they were trying to address.

For a very well-executed review of the new book at the New York Times, see here. Jerry Alper has an interview and discussion of the book here.

Bonus micro-review: Another book I just finished reading is Errol Morris's The Ashtray, which is also about philosophy and science. Morris, one of my favorite filmmakers, started out a career as a Ph.D. student of Thomas Kuhn's, and that did not go well. For more about the book, see reviews here and here. I'll just comment that Kuhn seems to have done the world a favor by kicking Morris out of the Ph.D. program and changing his career path to one where he could make the wonderful films he is responsible for.

Comments

1. **Low Math, Meekly Interacting**
   May 29, 2018

   Perhaps this darker aspect of von Neumann's personality got in the way of him being a celebrity like Einstein (or maybe John Nash is a better comparator). Everything I've read about him would seem to make him a contender for most intellectually gifted person alive when he was, if not of all time. One could hardly invent such a character: a polymath with near miraculous grasp of almost anything he apprehended, as fond of wearing expensive suits to any occasion as he was of partying, dirty jokes and driving like a maniac. The "mad genius" is a harmful stereotype, so I'm actually grateful, but I do wonder how Hollywood passed on him (ridiculous amalgams like Dr. Strangelove notwithstanding)

Feynman at 100

May 29, 2018
Categories: Uncategorized

The past month has seen quite a few events and articles celebrating the 100th anniversary of Richard Feynman's birth (see for example here, here, here and here). Feynman was one of the great figures of twentieth century physics, with a big intellectual influence on me and on many generations of particle theorists. In particular, his development of the path integral formulation of quantum mechanics and the Feynman diagram method for calculating and understanding what quantum field theories are telling us are at the center of how we have learned to think about fundamental physics and apply it to the real world.

When I first started studying physics, in the seventies, Feynman was a major figure to physicists, but not that well-known outside the subject. After the 1985 appearance of the book of anecdotes "Surely You're Joking, Mr. Feynman!" and his 1986 role in the report on the Challenger disaster (followed by more anecdotes in the 1988 "What Do You Care What Other People Think?") Feynman became a huge public figure. The Physics section of any book store that carried science books would often have nearly a whole shelf of books by and about him, with the only competition the shelf of books about Einstein (the Hawking shelf didn't get going until a bit later).

I avidly read the Feynman anecdote books when they came out and was suitably entertained, but I also found them a bit disturbing. Too many of the anecdotes seemed to revolve around Feynman showing how much smarter he was than someone else. I hadn't thought much about this, but was interested to read historian of science Melinda Baldwin's piece Feynman the Joker this month at Physics Today. It ends with:

> But Feynman's charm and brilliance were only one side of his personality. His writings, and the accounts of those who knew him, reveal a man whose faith in his own brilliance could veer into self-absorption and the mistreatment of others, particularly those whom Feynman didn't consider his equals. Even people who admired Feynman's intellectual gifts could become exasperated with his antics, and some important professional and personal relationships went off the rails when that happened. Feynman's legacy reminds us that it's important to have fun with physics—but to make sure those around us are having fun too.

I think this is an overly harsh take on Feynman, but do think that his later career suffered from the sort of self-absorption Baldwin points to. She links to an interview with Gell-Mann, which includes:

> **One of your best-known interactions was with Richard Feynman at Caltech. What was that like?**
> We had offices essentially next door to each other for 33 years. I was very, very enthusiastic about Feynman when I arrived at Caltech. He was much taken with me, and I thought he was terrific. I got a huge kick out of working with him. He was funny, amusing, brilliant.
>
> **What about the stories that you two had big problems with each other?**
> Oh, we argued all the time. When we were very friendly, we argued. And then later, when I was less enthusiastic about him, we argued also. At one point he was doing some pretty good work—not terribly deep, but it was very important—on the structure of protons and neutrons. In that work he referred to quarks, antiquarks, and gluons, of which they were made, but he didn't call them quarks, antiquarks, and gluons. He called them "partons," which is a half-Latin, half-Greek, stupid word. Partons. He said he didn't care what they were, so he made up a name for them. But that's what they were: quarks, antiquarks, and gluons, and he could have said that. And then people realized that they were quarks, and so then you had the "quark-parton" model. We finally constructed a theory—I didn't do it by myself; it was the result of several of us put together. We constructed the right theory, called Quantum Chromodynamics, which I named. And Feynman didn't believe it.
>
> **He didn't believe that the theory was correct?**
> No. He had some other cuckoo scheme based on his partons. Finally after a couple of years he gave up because he was very bright and realized after a while that we were correct. But he resisted it, and I didn't understand why he had to be that way. Partons…

Looking at Feynman's career, his great accomplishments were in the years 1947-58, and it's somewhat surprising that he didn't make major contributions (besides the partons…) to the development of the Standard Model in the years from 1958-73. One contributing factor may have been his insistence on "What I cannot create I do not understand." John Preskill recounts in a recent talk:

> Feynman often told students to disregard what others had done, to work things out for oneself. Not everyone thought that was good advice. One who disagreed was Sidney Coleman, a Caltech grad student in the late 50s and early 60s. Coleman says: "Had Feynman not been as smart as he was, I think he would have been too original for his own good. There was always an element of showboating in his character. He was like the guy that climbs Mt. Blanc barefoot just to show it could be done. A lot of things he did were to show, you didn't have to do it that way, you can do it this other way. And the other way, in fact, was not as good as the first way, but it showed he was different. … I'm sure Dick thought of that as a virtue, as noble. I don't think it's so. I think it's kidding yourself. Those other guys are not all a collection of yo-yos. Sometimes it would be better to take the recent machinery they have built and not try to rebuild it, like reinventing the wheel. … Dick could get away with a lot because he was so goddamn smart. He really could climb Mont Blanc barefoot."

A related aspect of Feynman's working method was a sizable amount of hostility to any abstract mathematics. In his talk at the Caltech Feynman 100 event, Lenny Susskind makes a great point of this, seeing Feynman's insistence on physical intuition rather than mathematics as a key to his strength. For some problems though, as Sidney Coleman realized, refusing the mathematician's toolbox may just make it impossible to do what you need to do.

A peculiar aspect of the Caltech scientific symposium was that the two talks on particle physics (by David Gross and Hirosi Ooguri) spent a great deal of time promoting something that Feynman detested. While Gross described a major legacy of Feynman as "a healthy disrespect for authority" and "a total aversion to BS", those characteristics led Feynman to have a very negative view of string theory, up until his death. He was known to remark that "string theorists don't make predictions, they make excuses", and in a 1987 interview stated:

> Now I know that other old men have been very foolish in saying things like this, and, therefore, I would be very foolish to say this is nonsense. I am going to be very foolish, because I do feel strongly that this is nonsense! I can't help it, even though I know the danger in such a point of view. So perhaps I could entertain future historians by saying I think all this superstring stuff is crazy and is in the wrong direction.
> **What is it you don't like about it?**
> I don't like that they're not calculating anything. I don't like that they don't check their ideas. I don't like that for anything that disagrees with an experiment, they cook up an explanation – a fix-up to say "Well, it still might be true". For example, the theory requires ten dimensions. Well, maybe there's a way of wrapping up six of the dimensions. Yes, that's possible mathematically, but why not seven? When they write their equation, the equation should decide how many of these things get wrapped up, not the desire to agree with experiment. In other words, there's no reason whatsoever in superstring theory that it isn't eight of the ten dimensions that get wrapped up and that the result is only two dimensions, which would be completely in disagreement with experience. So the fact that it might disagree with experience is very tenuous, it doesn't produce anything; it has to be excused most of the time. It doesn't look right.

Asked at the end of his talk what he thought Feynman would say about string theory today, Ooguri responded with an argument that string theory had made a lot of progress since Feynman's time, was much better understood, and was the only known consistent way to do things. He said he was very curious to know what Feynman would say, but I think it's extremely clear what that would be: he thought it was BS back in 1987, and thirty years of lack of any progress towards making any predictions has shown that he was right back then.

I'm still an admirer of Feynman's work and career (and sorry that I never got a chance to meet him), but at the same time think it's a good idea to acknowledge that he, like any scientist, had his limitations. Adopting his hostility to abstract math and trying to climb Mont Blanc barefoot is likely a bad lesson to draw from his career. On the other hand, a really good lesson to learn from Feynman would be the importance of recognizing when theorists have nothing but excuses and are engaging in BS. There's no question at all about what Feynman would have thought of the current mania for the string theory multiverse.

Comments

1. **Joao Leao**
   May 29, 2018

   Peter,

   I also grew up admiring Feynman and still do. Unlike you I had a chance to meet him and talk a bit with him and I wasn't disappointed though he was not the stupendous lecturer that most people still make him out to be (that was Julian Schwinger!!). He was tentative and messy but he was charming and quite unique in the way he captured your attention. I take a bit of issue with what you say about the interviews that became, "You must be joking,…". The anecdotes have a common thread which is his pleasure in mystifying other people by leading them to believe he was much smarter than he really was! There is a different way to interpret the whole of these episodes as suggesting that people are a lot more gullible than you may think and ready to make him smarter than he was. I believe that is the source of the charm and the popular success of that book. The following ones show much more of his humane side specially the story about the death of his first wife which was later turned into a movie.

   As for Gell-Mann's qualms I would say his ego is pretty comparable to Feynman's in magnitude from what I could gather since I also met him a few times. I would not care for whatever option each of them held about the other or whether "quark" is any more refined than "parton" just because it was picked from Finnegan's Wake rather than Dollywood!

2. **Peter Woit**
   May 29, 2018

   Joao Leao,

   I haven't met Gell-Mann either, but from what I've heard don't doubt that his ego is just as healthy as Feynman's was. He did however have a lot more success with the theory of the strong interactions than Feynman ever did. Part of this may have just been being of a somewhat younger generation, but I think his ability to exploit symmetry arguments involving the non-trivial abstract mathematics of the representation theory of groups like SU(3) was also part of the story (and I think it's possible this is what Coleman had in mind in his comments).

3. **Mateus Araújo**
   May 29, 2018

   When I was younger I had unreserved admiration for Feynman, read a lot of his books, and learned much of what I know from him (in fact there is even a video of my impersonating Feynman on the Internet, which I'm afraid has more views than my papers have readers).

   Unfortunately, one thing that I took unquestioningly from him was his hostility to philosophy, which lasted until I met actual philosophers, and realized that they are far from the empty-headed-talking-about-angels-dancing-on-pinheads stereotype that Feynman propagates: they actually know a LOT, have little tolerance for unrigorous thought, and understand well problems that still confound physicists to this day.

4. **Peter Woit**
   May 29, 2018

   Joao Leao,
   I just noticed that Clifford Johnson seems to have somewhat shared my reaction to the 85 book, see
   http://asymptotia.com/2018/05/11/feynman-centenary/
   "the famous "Surely You're Joking…" book, which even back then in my naivety, I began to recognise as partly a physicist's user manual for how to be a jerk to those around you. (I know I'm in the minority on this point…) "

5. **milkshake**
   May 29, 2018

   The take from these anecdotes is that Feynman loved to draw attention to himself in a sly or subversive way – whenever the setting was pretentious, serious, highbrow, he would try to do something outré.

   He adopted his exaggerated Brooklyn-Italian accent to sound like a street-smart gangsta because he grew up in a working-class neighborhood where being geeky was unfashionable; he was short and bad at sports. The prankster persona was perhaps also his way of impressing other kids.

   The other thing I heard – and I don't know if it's true – is that he was hard to collaborate with because he was too self-absorbed, that for most students it was not ideal to have him as a PhD advisor – the issue being that he was self-taught for good and bad and his style of working was difficult to emulate.

   He was fond of saying that physicists can re-invent the parts of math they need for their work rather than trying to follow the developments in mathematics in general; this was probably fine for him but not the best practical advice. He joked about those other physicists who (unlike him) knew group theory stuff.

6. **Low Math, Meekly Interacting**
   May 29, 2018

   I also have been through some of the erstwhile Feynman worshiper's stages of grief, though I still love re-reading all those books I collected. I'll forever be grateful for "QED: The Strange Theory of Light and Matter", even if it's dated. I still find "There's Plenty of Room at the Bottom" charming, even if it had nothing to do with actual progress in the field of nanotechnology. While an unrefined and halting speaker, his public lectures still always struck me as remarkably dense with instructive content. I liked how he made this point so emphatically:

   "It doesn't matter how beautiful your theory is, it doesn't matter how smart you are. If it doesn't agree with experiment, it's wrong."

   I watched a recording of a lecture he gave, I think at the Esalen Institute. It was a little embarrassing (the attire alone…), but he made a very pithy comment near the end about what was then (mid-80s?) the current state of theoretical particle physics. Something about underestimating theorists' taste for speculation in response to the dwindling supply of new experimental data. While his tone was cheerfully derisive, I wonder if the current state of affairs would leave even him too depressed to comment.

7. **Peter Woit**

May 29, 2018

LMMI,

I think you're referring to this talk
https://www.youtube.com/watch?v=4eRCygdW–c
around the 59 minute point,

8. **Joseph Conlon**
May 29, 2018

The Feynman cult is a funny thing – for writing something similar in spirit in my book (about how Feynman's approach was not the right one for the 1970s developments at the boundary of maths and physics) I get these angry rants from crackpots about how *dare* I say that Feynman was not the greatest genius that ever lived, etc, etc, etc, and in any case I'm just a dumb string theorist who could never appreciate Real Physics.

That said, I don't think the position you adopt here (that, for all his greatness, Feynman's flaws as a scientist and a human should be recognised, but when it comes to his end-of-career judgement on string theory he is infallible) is coherent. If (as is surely true) we should not take Feynman's opinion on topics on the intersection of physics and mathematics as the most acute one, then why should we care what what he thought about string theory?

And while on this topic, the anecdotes in the Feynman books about his visits to strip clubs and the like read odd on first reading and even more so today. Powerful man tells stories about how he got laid; not a genre that has aged well.

9. **Anon123**
May 29, 2018

There's no question that Feynman was smart but I think more than that he was extremely clever and valued that much more than rigorous intelligence. Clever people are smart but cleverness looks at the the world in a different way. I have the impression that Feynman talked about this but I don't have a reference handy. But the difference between intelligence and cleverness has always stuck with me (being somewhat smart but not overly clever). I think a lot of success in physics obviously requires smarts but cleverness is often the key to new insights.

10. **Peter Woit**
May 29, 2018

Joseph Conlon,
I don't think that Feynman's judgment on anything was infallible, just that it's clear what it was (and would still be now) in the case of string theory. Given this, it's rather odd to honor his memory with talks promoting a research program he thought was misguided. It would have been just as odd to honor him by bringing in a bunch of mathematicians to promote, say, the categorical representation theory approach to geometric Langlands as the right way to think about certain quantum field theories.

On his fondness for strip clubs and anecdotes about best strategies for convincing low self-esteem women in bars to come home with him, I don't want to host a discussion of the morality of his sexual behavior here. As far as I know, this didn't involve women he had professional relationships with, and he seems to have been no more a misogynist than the average person of his age.

11. **Low Math, Meekly Interacting**
May 29, 2018

Yes, that's the one. There's another on computers, I think. Also a bit cringeworthy.

On the other hand, a Nobel Laureate in physics, taking the time for an open exchange of ideas with what's likely a pretty woo crowd even by Californian standards. Everyone being reasonably respectful. A remarkable social feat, if nothing else.

12. **Amitabh Lath**
May 29, 2018

I was a grad student in the Friedman-Kendall group at MIT when "The Hunting of the Quark" was published (which Riordan claimed led to the 1990 Nobel Prize for quark discovery). The book has anecdotes from the late 60's about Feynman coming up to SLAC as the deep inelastic scattering experiments were starting to get hints of something hard inside the proton. According to the book, he came to"snoop around" the End Station counting house and would later brilliantly recast Bjorken's "scaling" in pictorial terms (Feynman diagrams). But when I asked them for more Feynman stories I was told that when they all hung out with him he was a pretty typical theorist, this whole "pool shark from Brooklyn" persona came later.

13. **Chris Oakley**
May 30, 2018

I suspect that Feynman was the most brilliant physicist who ever lived. His legacy, although substantial, does not do him justice. He did not seem to have had the patience to follow things through to the bitter end, and this allowed lesser talents, such as Gell Mann, to steal a march on him. It is interesting that Einstein will always be better remembered both by physicists and the general public – a less brilliant man, but with more patience (arguably too much, considering how he wasted his latter research years).
Also, as Gell Mann pointed out, Feynman's ego always got in the way. The Challenger disaster investigation is a nice example of NASA using this to their advantage.

14. **Anon**
May 30, 2018

There's no such thing as 'abstract' math, though there is certainly a cargo cult about abstraction in mathematics. Best to read Feynman's prejudice as directed against the cargo cult, and not abstraction. We mathematicians understand -and judge – even the most spectacular abstractions in concrete and hands on ways, not dissimilar to Feynman's. You can transform any problem into some construction of moduli, some functorial property, recasting it as finding a point in a space that you want to be non-empty, but at the end of the day, the equations need solving.

But the metaphors and language we use to describe how we think differ among individuals – so people may perceive disagreement if they don't understand the mathematical commonality of our agreements.

PS what could be more concrete than SU_3? Matrices!

15. **Urs Schreiber**
May 30, 2018

One might hope that Feynman knew of better reasons to be sceptical of string theory than that alleged issue with the number of spacetime dimensions.

A little reflection reveals the following situation:

In ordinary QFT, spacetime dimension may be any natural number.

In ordinary gravity, if that number D is fixed by hand, then the number of macroscopic dimensions seen in generic solutions is a natural number smaller or equal to D.

Hence to do better than plain QFT on this front, one should want a theory that predicts spacetime dimension D to be a small natural number and equipped with some mechanism to stabilize some large dimensions.

A natural way to achieve this is to invoke spin geometry, due to the sensitivity of spin representation theory to spacetime dimension.

In this vein, for instance Roger Penrose has claimed that the number 4 of spacetime dimensions is naturally explained as being the number of dimensions in which twistors work. But in fact twistors work more generally in spacetime dimensions 3, 4, 6 and, with some modifications, 10. This happen to be the same spacetime dimensions in which the Green-Schwarz superstring exists, and it is for the same mathematical coincidences in both cases (related to the four real normed division algebras).

A small number of small natural numbers of potential spacetime dimensions, around and including 4, such as this one, is not too bad, certainly as compared to the anything-goes of plain QFT.

If one brings not just spin geometry to bear, but in addition asks for geometric realization of conformal fixed points of QFTs, then the available set of dimension is {3,4,5,6}, as it appears in AdS/CFT. Also not too bad, if one is really interested in understanding where spacetime dimension comes from.

16. **Another Anon**
May 30, 2018

Chris Oakley: "I suspect that Feynman was the most brilliant physicist who ever lived."

Why? Because he jointly developed quantum electrodynamics? And, er, that's it.
I think you're confusing charisma and celebrity with achievement.
Dirac the complete opposite. No charisma, but greater achievement. Public doesn't even know about Dirac.

17. **Peter Woit**
May 30, 2018

Urs,
I don't think you'd impress Feynman with that answer to him.

Anon,
"Abstract" may be the wrong word, I was just trying to characterize the limited set of mathematical tools that Feynman was comfortable (and extremely expert at) using. I don't think these included SU(3) matrices.

Chris Oakley/Another Anon,
Sorry, enough of the sterile debate about who was the greatest.

18. **milkshake**
May 30, 2018

Regarding the sterile debate about greatness: A popular magazine OMNI featured Feynman interview prominently on the cover – with the caption "The smartest man in the world"
Someone showed this to Feynman's old mom. Her incredulous reaction was "Who? – our Richie? God help us!"

19. **Amitabh Lath**
May 30, 2018

The charisma question is worth exploring.

Right after the 1990 Nobel Prize was awarded for the quark discovery I was based at SLAC and noted some real concern that the media attention would all go to Feynman, and Bjorken (a SLAC theorist) would be left out. The emerging narrative seemed to be that in the late 60's the experimentalists were baffled by their results and genius Feynman swooped in and explained to them: hey youze guys, them's partons! But it was Bjorken who had done meticulous work with structure functions to figure out what the results were saying.

There is a real issue here that Bjorken's current algebra is dry and mathematical and Feynman's diagram of an electron zipping past a proton exchanging a photon is much more intuitive. The physics community in the late 60's was enamored of S-Matrix theory and the idea of a composite nucleon was not getting traction. Having Feynman on board probably did help sell the results.

Of course Bjorken himself still remains consummately above the fray belying the caricature of theorists battling over recognition.

20. **Amitabh Lath**
May 30, 2018

About Gell-Mann and Feynman's quark vs. parton fued: to the experimental community it was more than a linguistic joke. Although Gell-Mann came up with the quark theory he (allegedly) considered them mathematical constructs only and pushed back against the idea of them being actual pointlike things inside protons. Feynman of course had no such reservations about the physical existence of partons.

Gell-Mann stuck to his guns basically until the November revolution in 1974. There are anecdotes of Gell-Mann chiding Feynman about his "put ons" and the idea of objects with fractional charge.

This might seem an esoteric philosophical question but to young experimentalists at the time it determined who was invited to give plenary talks at major conferences etc. Having someone of Feynman's stature helped a lot, since the Stanford/SLAC people were not prominent enough at that time.

21. **Joao Leao**
May 30, 2018

Peter,

I also think you are being unfair with Feynman's contributions to Strong Interaction physics. Parton Phenomenology became quite important to the understanding of Deep Inelastic Scattering experiments at SLAC in the mid seventies and — pace whatever bile Gell-Mann holds — Feynman gave a remarkable set of lectures on QCD at Les Houches in 76 or 77 and in the 80s he made a lone brave effort to find a general solution to the theory in low dimension.

He also made notable contributions beyond Particle Physics namely superfluids, the polaron and plasmon theory, etc… In the late nineties he was seminal in developing the early conceptions of Nanotechnology ("Lots of Room at the Bottom") and Quantum Computation with his Mosquito Island Conference intervention and his work on Quantum Logic Gates. He spend a summer here in Cambridge working with Ed Fredkin and Danny Hillis on the original concept of the Connection Machine. That was actually when I had the chance of meeting him and found him quite an engaging and quite delightful person not the showman or jerk of legend.

22. **GoletaBeach**
May 30, 2018

What I remember is that in 1960's, particle theory had veered off into the analytic S-matrix and a whole denial that relativistic field theory in the perturbative limit was relevant at all… a movement led by Geoff Chew, David Gross' PhD advisor. Feynman was part of the counterculture at that time… he still believed in the applicability of perturbation theory. But he recognized that the strong interaction needed some additional physics insight, and so he started from scratch with partons. He thought BJ was way too enamored with fancy math.

There was an awful lot of doubt that quarks where physical back then… we thought they were just an accounting scheme. The phase shift to their reality arrived when the neutrino scattering experiments (one led by Barry Barish) got data that laid right on top of the SLAC DIS data, where the extrapolation needed the assumption that quarks were real.

David Gross and others were smart enough to circle back into perturbative field theory; BJ bugged out and became extremely phenomenological and even worked in experiments. Gradually folks with strong math ability discovered deep field theoretic connections, and it always looked to me like they made there way back to a better, deeper version of Geoff Chew. Always seemed to me that Feynman, starting in the 1970's, just didn't want to do that, and explored other stuff.

Very few great innovators are *not* egotistical. Is there a single physics Nobel Laureate without a double-extra-huge ego? Maybe Charles Townes. Maybe Carl Anderson. It is the old…. "sometimes it takes a pig to find a truffle" deal. And the pig usually eats the truffle, unlike the rest of us dogs, who turn the truffle over to our masters.

23. **CWJ**
May 30, 2018

" Is there a single physics Nobel Laureate without a double-extra-huge ego?"

Of the ones I've met, Hans Bethe, Ben Mottelson, and Willy Fowler didn't seem to. Willy could be irascible (of course I met him only toward the end), but Bethe and Mottelson were gracious and kind to a young physicist still wet behind the ear.

Murray Gell-Mann and Sheldon Glashow, on the other hand, lived *exactly* up to the stereotype.

David Thouless was somewhere in between. He probably had an ego, but he didn't openly make it about himself. Nonetheless, he could certainly make students (and colleagues, even ones who had pretty healthy egos themselves) wither and feel small and irrelevant.

24. **Tim May**
May 30, 2018

I cooked a steak for Feynman in 1973 in Isla Vista, the hippie area next to UCSB. We sat around for several hours. A great time.

At one point I expressed some doubts about where particle physics was going. Feynman said that were he going forward he would tend to go into computer science. I did. I joined a small company named Intel the following year.

And as the saying goes, "And that made all the difference in the world."

Feynman was a great guy.

–Tim May

25. **liuyao**
May 30, 2018

By chance I was watching some of the YouTube videos of Gell-Mann (Web of Stories interviews), and it is easy to get too negative an impression of their relationship. I'm glad that Peter had taken to include Gell-Mann's "enthusiasm" towards Feynman. (Keep these in mind if you want to read an old Atlantic piece on their rivalry: https://www.theatlantic.com/magazine/archive/2000/07/the-jaguar-and-the-fox/378264/)

That Feynman in his later life got overconfident with his way of doing physics is not uncommon among great physicists. Einstein was most famous in that regard, and Dirac also, to some degree (with his Large Number Hypothesis).

26. **Amitabh Lath**
May 30, 2018

>Is there a single physics Nobel Laureate without a double-extra-huge ego?

I can point to two.
My thesis advisor Henry Kendall and his colleague Jerry Friedman (1990 Nobel Prize in physics, shared with Dick Taylor of SLAC).

Kendall kept teaching undergraduate atomic physics (the dreaded "junior lab") after 1990. This has to be one of the most time consuming thankless teaching assignments ever, and he was great at it. Friedman is one of the kindest men I have ever met, and remained so after the prize. He became dept. chair and totally revamped undergraduate teaching which had been fairly Victorian until then.

27. **Petite Kabylie**
May 30, 2018

I watched many times Feynman's Messenger Lectures on youtube and each time I am amazed how he manages to be that funny and yet explain physics with such depth. By reading Feynman's biography though I learned that he himself admits that his lectures fade compared to Schwinger's. I have searched the web in vain for Schwinger's

video lectures. Can anyone please provide a link, if any, to Schwinger's video lectures so that I could compare with Feynman's impeccable style?
Thank you!

28. **Marshall Eubanks**
May 30, 2018

I used to regularly attend John Schwartz and Murray Gell-Mann's supergravity seminars in the mid-1980's at CalTech; Feynman was always there.

What I remember was Feynman being upset in the lack of any possibility of experimental confirmation of the new ideas of string theory and supergravity. I can still remember him getting in an argument with Gell-Mann and saying vehemently "but there's nothing to measure!" That to me seems much more on point than uncertainty on how many dimensions get compactified.

29. **Michael Weiss**
May 30, 2018

>Is there a single physics Nobel Laureate without a double-extra-huge ego?

My undergraduate adviser Ed Purcell–such a gracious, kind and generous person. For those who may think that Ed was "only" an experimentalist, his group led to way to the non-relativistic QM theory of NMR relaxation.

Ed once told me that he and his remarkable then-younger colleagues (N. Bloembergen [also a Nobel laureate], R. Pound [who may have just missed sharing the NP with Taylor and Hulse] , Torrey [a scientific statesman]) had the opportunity to develop the theory "only because the really smart physicists had overlooked the problem."

By this turn of phrase, it was evident that Ed did not include himself in this category –even if his physical intuition was second to none. This profound intuition is so beautifully evident in his elementary E&M text in the Berkeley series.

Ed told me the following Feynman story: he once declined to give a prestigious seminar at Harvard in order to meet high-school students in Cambridge. But I could not tell whether thought this was admirable or droll showmanship of some sort.

BTW John Preskill was Ed's TA in 1976 (I think) when he taught an introductory QM course ("Physics 143"). John once had a blog post on this experience.

30. **Doug McDonald**
May 30, 2018

I know well no Physics Nobel winners, but I worked for a Chemistry one, and the post-doc that worked with us both also won one, (but … we did scattering measurements). I was a colleague with a winner in Medicine. All three were very nice people and delights to work with or talk to. None were great lecturers.

Then there was the guy across the street named Schwinger. I sat in on his 1st year
grad quantum mechanics class (non-relativistic, no field theory). I saw what people here say about his lectures. They were beautiful and well organized. And indeed, listening one could imagine that one could have, back in the day, thought up all of QM onesself, just from experiment. But after an hour back across the street thinking, it all,
always, fell apart. By "all" I mean his logical progression from something that, listening, seemed obvious, to where he was going. There were gaps in the argument. Much later I studied real relativistic field theory. Now I, vaguely after all these decades, realize that he had thought things through from relativistic/field theory bases and cooked up hand-wavy explanations that made sense. I should have asked the real physics students if they felt the same. But they were inspiring lectures.

I never heard Feynman, but his online lectures are hand-wavy enough as-is that I never feel that hour later letdown.

31. **Patrick Orlando**
May 30, 2018

Partons
A Feynman story. I entered Columbia in Sept. 1967 as a Physics major. One day on the bulletin board outside the physics office on the 8th floor was a notice that He was coming to talk about parton theory. Of course I went, it was in one of the two stadium size lecture halls on the 3rd floor. I brought a friend of mine who was from Brooklyn as I was. We had to seat up in the higher rows as the room was almost full. Feynman talked and wrote on the blackboards, pulling down all three of them to fill with equations that to me at the time where incomprehensible. At the end of the lecture when the questions began from the Full Professors in the front, some also Nobel laureates, one said "but there's more to it than that ! " And Feynman said in his heavy Brooklyn accent, (like mine) "yes there's the Numerator ! " And everyone laughed.

32. **Douglas Natelson**
May 30, 2018

Joao Leao: Feynman died in 1988. He did not have anything to do with seminal developments of nano in the 90s, and his "Plenty of Room at the Bottom" lecture was in 1959.

33. **Richard Séguin**
May 31, 2018

This amusing story about Feynman recently appeared in the UW-Madison alumni magazine.

https://onwisconsin.uwalumni.com/features/2-who-got-away/

He was once hired by UW-Madison, but immediately took leave for Los Alamos to work on the war effort.

"In June 1945, an impatient Mark Ingraham, dean of the College of Letters & Science, sent Feynman a letter demanding that he return to campus and start teaching classes."

"Years later, Feynman finally returned to Madison. 'It's great to be back,' he told the crowd, 'at the only university that had the good sense to fire me.'"

34. **Shantanu**
May 31, 2018

Peter, something underappreciated is Feynmann's contribution to GR and also showing using an ingenuous thought experiment that gravitational waves carry energy.

35. **Shantanu**
May 31, 2018

Very few great innovators are *not* egotistical. Is there a single physics Nobel Laureate without a double-extra-huge ego? Maybe Charles Townes. Maybe Carl Anderson. It is the old…. "sometimes it takes a pig to find a truffle" deal. And the pig usually eats the truffle, unlike the rest of us dogs, who turn the truffle over to our masters.

I can give one counter-example : Joe Taylor. Extremely nice and modest. He also travels by long distance trains. In fact when we met at a conference one participant didn't even know he had a nobel prize and asked if the train fare was costly 🙂

36. **Marshall Eubanks**
May 31, 2018

Douglas Natelson – I saw Feynman give an updated version of the "Plenty of Room at the Bottom" lecture at JPL (in the von Karman auditorium) in the mid-1980s. I believe he was also thinking about massively parallelized computers at the time (JPL and Caltech were both working on hypercube computers then), but I have no idea what came of that.

37. **Bryan Draughn**
May 31, 2018

I'm just an avid reader. Physics grabbed my attention at a very young age but I have a very hard time with mathematics. I get it, but I get lost in it.
Feynman's "Surely You're Joking Mr Feynman!" Changed the way that I thought about everything because Feynman demonstrated in the book that you CAN change the way you think about everything…and probably should.
I can't help but mention a strong hunch about him from what I've gathered throughout the years regarding his personality.
He warns strongly how one can think oneself into a corner , or worse, overlook a pivotal detail and way down the line that detail can undermine even the most colossal body of work.
My point is, I can imagine the man being constantly aware of the dangers that an advanced mind could potentially bring down on someone. Himself and others alike.
I always get that sense of caution when I watch one of his interviews or lectures.
If he had an ego that was out of proportion, I would be surprised if he wasn't well aware of the danger in that.

38. **Amitabh Lath**
May 31, 2018

Feynman's contribution in the deep inelastic scattering seems to be in just looking at the data and ignoring everyone else's ideas.

As GoletaBeach admits above, the theorists working on the quark model thought of them as accounting gimmicks. I suppose the idea of asymptotic freedom eventually helped things progress?

As for the SLAC/MIT DIS experimentalists (and theorists like BJ) it was nuclear physics, a continuation of the work done at the Cambridge electron accelerator (which had exploded). The jargon is full of terms like structure functions and form factors. Particle physics was something done at Berkeley, and then at the newly built national lab in Batavia IL.

In my mind Feynman's genius was in ignoring these artificial experimental and theoretical boundaries, as well as the prejudices of the smart set.

39. **Anonyrat**
May 31, 2018

Feynman's students who did not continue in physics say that Feynman's great teaching was on being happy.

40. **CWJ**
May 31, 2018

I often cite this quote from Feynmann:

"The first principle is that you must not fool yourself — and you are the easiest person to fool."

41. **Bill_K**
May 31, 2018

Here's an exchange between Feynman and Gell-Mann that happened in a Caltech seminar.
Feynman was at the front board making some miscellaneous remark about "Bosey-Einstein statistics." Gell-Mann in the audience interrupted him with a smirk and a glance around the room, "It's Bose. BOSE! Anything else is an affectation!"
You can decide for yourself what this says about anyone's egos…

42. **Douglas Natelson**
May 31, 2018

Let me add my thesis adviser, Doug Osheroff, and his co-laureates Bob Richardson and Dave Lee, to the list of physics Nobel winners without enormous egos – all very nice, friendly people. Horst Stormer, too. Steve Chu doesn't suffer fools, but he's also very good about what he does and doesn't know, a trait absent in many people with fewer credentials and awards. Bob Laughlin deliberately cultivates a larger-than-life persona – hard to judge how much of that is real ego. The bottom line: Physicists are people, with the whole variety of personality types. Getting to meet and interact with these interesting people is a feature of a career in the field. Success as measured by the Nobel prize doesn't have to correlate with being an egomaniac. (Peter, if this is too off-topic, go ahead and delete it.)

43. **Low Math, Meekly Interacting**
May 31, 2018

I've greatly enjoyed watching interviews with Freeman Dyson, recounting his role in the development of QED and related interactions with Feynman and other major figures of that era. Dyson was kinder to Schwinger than Gell-Mann, but given his withering assessments of IAS leadership at that time, it's clear Dyson is not someone who is necessarily easy to impress. It's also clear he held Feynman in total awe.

Pointless arguments about IQ aside, I'm inclined to take Dyson seriously.

44. **GoletaBeach**
May 31, 2018

Nice to read all of the positive comments about Physics Nobelists. I guess I don't see ego as necessarily unpleasant… perhaps Feynman is a good example of a compelling personality but still, somehow he made himself the center of it all. My one interview in his office didn't really let me measure that directly.

If Tim May reads this… hope he does… the current undergrad group in Physics at UCSB would love it if you exchanged any info about Feynman in Isla Vista with them… these days the group is UDIP at physics.ucsb.edu… the used to by the Society of Physics Students… at the moment they call themselves Undergraduate Diversity in Physics. They are quite active and fun, and any photos of Feynman in IV would be treasured by them.

45. **Ethan**
May 31, 2018

Thanks for posting this Peter.

I have been thinking about Feynman a lot lately. In fact it's only accidentally -by watching one of the numerous interviews he gave in the 1970s and 1980s- that I realized that this year is the year we celebrate his 100th birthday.

The good thing about him is that there is a lot of video material on him available in youtube. To me these Feynman videos are a window into the thinking of the kind of genius we don't see anymore. He regularly comes in the top 10 on surveys like this http://www.caltech.edu/news/physics-world-poll-names-richard-feynman-one-10-greatest-physicists-all-time-368 . I wish we had more videos from other great intellectual figures of the XX-the century like Claude Shannon, John Von Newuman and the others mentioned in the preceding link who lived in the XX-th century.

I mean no offense to those who currently work in the forefront of physics but I do believe at the same time that the "publish or perish" frenzy is a killer. Would Richard Feynman stand a chance in today's academia? It's very hard to predict but I somehow think he wouldn't. With all the confusion about some who are say that falsifiability is a thing of the past, I find this critique of cargo cult science refreshing: https://www.youtube.com/watch?v=tWr39Q9vBgo . I think he would be probably appalled as to how far we have gone as a scientific community in this nonsense.

46. **Tim May**
May 31, 2018

GoletaBeach, I'll be very brief here.

I sent a short message to their address recapping what I said here, with a few more details. It didn't bounce.

I can't circulate the several snapshots I have somewhere, as I wasn't the photographer and people have gotten sensitive about formal release permissions. I lost touch with the guy who took the snapshots a long time ago.

47. **Naturally Inconsistent**
June 1, 2018

Pardon my two simple points. First the shorter one.
In reply to Urs, when they first built QFT, they did not try to predict the number of dimensions. They simply used it as an ingredient, and be done with it. The point is that you cannot claim to "predict number of dimensions" and then have to manually set the compactification simultaneously, and expect to be respected. Either you say you literally don't know, and just work with that assumption, or you explain it without fudge factors.

About partons, I was taught that Feynman was deliberately trying to separate phenomenology from theory. Which, typically for new theories, that is a smart thing to do. That is, partons are "these are what have been observed in experiments, that are basically going to be there no matter which theory you later wish to fit", and quarks and gluons have as main job to try to fit/explain what Feynman was quantifying with partons. Philosophically, the separation of the map and the territory, is a great move, one that led us to wonderful things like tensors.

I am in no way denying that Feynman was egoistical, wanting to show off his intelligence, and bad to women. (Though I certainly needed someone to point these facts out to me.) I am just saying that the quark v.s. parton issue, is really not an issue at all.

48. **tulpoeid**
June 2, 2018

I liked this balanced attitude towards Feynman quite a bit, you don't come across this every day.

As about "Surely you're joking", and living outside USA, all these years I've been having the impression that the consensus is that a big portion of the book is made up. Therefore I'm somewhat surprised to find out that many people don't view it to be so.

49. **Blake Stacey**
June 2, 2018

According to Sidney Coleman, "Bose" was pronounced "boʊʃ", i.e., with a long *o* and a *sh*-sound at the end. This was somewhere early on in the QFT lecture videos that Harvard put online several years ago, but I forget exactly where.

50. **David Brown**
June 4, 2018

@Marshall Eubanks: You misspelled John Schwarz's name:
https://en.wikipedia.org/wiki/John_Henry_Schwarz

51. **Joao Leao**
June 4, 2018

Douglas Natelson: I meant late eighties in my comment. Sorry about that. Feynman "updated" his classical lecture in one he gave at JPL in 1983 later transcribed and published under the title "Infinitesimal Machinery". http://www.oz.nthu.edu.tw/~d9511818/Infinitesimal%20Machinery.pdf

52. **GoletaBeach**
June 6, 2018

Word got back to me from their lab professor today that your one image was received, Tim May. You elated a whole lot of young physics majors, thank you kindly for that!

53. **anon**
June 7, 2018

I would like to know when Sidney Coleman spoke that way. Indeed, he did great things in his best years, but in the late ones, gosh. The paper "High-energy tests of

Lorentz invariance” in my view is a shameful one – any particle has its own limiting velocity – and caused a lot of confusion.

Moreover, Gell-Mann defence of “right words” sounds funny to me: “partons” is OK and “aces” is OK too, as the model works.

By contrast, Feynman’s words of his last years include those for “future historians”.

54. **Peter Woit**
June 8, 2018

anon,
The Coleman quote is from 1992 or earlier, see more of it at
https://www.nytimes.com/1992/09/20/magazine/part-showman-all-genius.html
This is from long before the paper you don’t like and Coleman’s later health problems.

I suspect Coleman’s take on Feynman is quite insightful. He got to observe him from up close, and one of Coleman’s best qualities was his irreverence. He did not suffer from the hero-worship that afflicts many who have written about Feynman.

55. **Angelos**
June 13, 2018

Linguistically, ‘parton’ is no worse a hybrid than ‘neutron’, to say nothing of ‘gluon’. And it has the advantage of suggesting its meaning — unlike ‘quark’, which is purely conventional.

There Are No Laws of Physics. There's Only the Landscape.

June 4, 2018
Categories: Multiverse Mania

At Quanta magazine, IAS director and string theorist Robbert Dijkgraaf has signed up to the multiverse mania bandwagon with an article announcing [There are no laws of physics. There's only the landscape](). Dijkgraaf's version of the string landscape ideology is:

> The current point of view can be seen as the polar opposite of Einstein's dream of a unique cosmos. Modern physicists embrace the vast space of possibilities and try to understand its overarching logic and interconnectedness. From gold diggers they have turned into geographers and geologists, mapping the landscape in detail and studying the forces that have shaped it.
>
> The game changer that led to this switch of perspective has been string theory. At this moment it is the only viable candidate for a theory of nature able to describe all particles and forces, including gravity, while obeying the strict logical rules of quantum mechanics and relativity. The good news is that string theory has no free parameters. It has no dials that can be turned. It doesn't make sense to ask which string theory describes our universe, because there is only one. The absence of any additional features leads to a radical consequence. All numbers in nature should be determined by physics itself. They are no "constants of nature," only variables that are fixed by equations (perhaps intractably complicated ones).

While giving the usual 1995 justification for the "M-theory" conjecture of a unique string theory, Dijkgraaf neglects to mention that, 23 years later, no one has a viable proposal for what this unique theory might be. He mentions none of the problems of moduli stabilization, or that the theorists "mapping the landscape in detail" don't actually know what equations govern this supposed landscape and thus have hit a dead-end, unable to predict anything about anything.

The problem is that what Dijkgraaf is writing about is the situation of [Theorists Without a Theory](), trying to turn this failure into success by arguing that it is a radical new discovery, the discovery that "There are no laws of physics". He ends with

> A more dramatic conclusion is that all traditional descriptions of fundamental physics have to be thrown out. Particles, fields, forces, symmetries — they are all just artifacts of a simple existence at the outposts in this vast landscape of impenetrable complexity. Thinking of physics in terms of elementary building blocks appears to be wrong, or at least of limited reach. Perhaps there is a radical new framework uniting the fundamental laws of nature that disregards all the familiar concepts. The mathematical intricacies and consistencies of string theory are a strong motivation for this dramatic point of view. But we have to be honest. Very few current ideas about what replaces particles and fields are "crazy enough to be true," to quote Niels Bohr. Like Alice and Bob, physics is ready to throw out the old recipes and embrace a modern fusion cuisine.

The argument seems to be that we need to throw out our highly successful quantum field theories, replacing them with a "radical new framework" describing "impenetrable complexity". But what is this "radical new framework"? As best I can tell, what's now popular at the IAS is the "it from qubit" idea that is the topic of [this summer's PITP program](). It seems that Witten has taken up the study of quantum information theory, with a new [expository preprint just out](). I'll look forward to seeing what the PITP lecturers present, but so far I haven't seen the slightest indication that this "radical new framework" can get off the ground as a fundamental unified theory.

Comments

1. **Greg Weiss**
   June 4, 2018

   I seem to remember string theorists brushing off claims that their theory is unfalsifiable, saying something akin to 'all you have to do to prove that String Theory is incorrect is to prove that Quantum Field Theory is incorrect.' I guess even that is no longer the case.

2. **Sabine**
   June 4, 2018

   It is not correct that string theory is "the only viable candidate for a theory of nature able to describe all particles and forces, including gravity, while obeying the strict logical rules of quantum mechanics and relativity." [Asymptotically Safe Gravity does this too](), and if you ask me, ASG is much more viable than string theory since it doesn't require a negative cosmological constant or extra dimensions or supersymmetry. And of course others have approaches which they claim can do it too.

   I am pretty sure that Dijkgraaf knows this statement is wrong, and it's really hard for me to read his piece as anything more than a desperate attempt at marketing.

   (Since some people have recently asked me, no, I do not work on ASG.)

3. **Kir**
   June 5, 2018

   No guys I give up with physics if this is the mainstream idea of where our field is going. This is just babbling about something. It is like saying that the world stands on elephants on a turtle without proof. This guy has no equations, no theory, no vacuum of string theory that resembles the standard model, to coherent string theory with a positive comsmological constant, and blathers about all this. Simply incredible. Also science is entering the era of post-truth. Truth is what most of the people accepts or talk about or like, not the result of the scientific inquiry. I'm frightened that an IAS director talks like this.

4. **Reader297**
   June 5, 2018

   Just as an historical aside, I'll note that Dijkgraaf is incorrect in referring in his article to "the famous particle-wave duality discovered by Heisenberg." Heisenberg didn't "discover" wave-particle duality. Planck and Einstein (among others) had long ago supplied physical arguments that light had a particle-like nature, and de Broglie famously suggested in 1924 that electrons and other particles of matter had a dual wave-like nature. Heisenberg hadn't burst onto the scene by that point. His big breakthrough was with matrix mechanics the following year, which was a decidedly non-wave-like approach to quantum theory.

5. **Urs Schreiber**
   June 5, 2018

   > […] the "M-theory" conjecture of a unique string theory, […] 23 years later, no one has a viable proposal for what this unique theory might be.

   At StringMath17 ([here]()) I had reported on an approach to unravelling M-theory via super homotopy theory. I think it is fair to say that this looks promising, recent progress is surveyed [here]().

6. **Rollo Burgess**
June 5, 2018

The logical flow of this article reminds me somewhat of books about the Holy Grail, Egyptian pyramids being built by aliens, etc. Possibilities are vaguely introduced, then subsequently assumed and embellished: for example para. 5 begins 'if our world is but one among many…' then para. 6 starts 'the game changer that led to this switch of perspective…' as though the new perspective was a done deal not a wild speculation.

There is also a pseudo-humility (we must accept that we cannot produce fundamental theories like the old ones) concealing an actual arrogance – i.e. there is little acknowledgment that perhaps the reason why we haven't done it yet is because we are stuck, because it is hard, because a lot of focus is being invested in what Imre Lakotos would have called a 'degenerating research programme', etc. rather than because actually reality is fundamentally different to what we previously believed and we have reached the end of physics as traditionally conceived.

If this was a debate in literary theory it would be quite funny, but in physics it is sad and worrying, particularly as post-truth nonsense abounds in other areas with real and obvious real-world consequences.

7. **Douglas Natelson**
June 5, 2018

I had expected better of *Quanta*. They've been reasonably good in the past at not publishing extraordinarily speculative stuff as if it's the be-all and end-all, and they're usually careful not to imply that high energy physics is the only branch of the discipline. Apparently not so now.

8. **Low Math, Meekly Interacting**
June 5, 2018

I know it probably isn't, but I really, REALLY hope that sterile neutrino finding is valid. Because, my God, such smart people desperately need something better to be working on than this.

9. **Bud Rapanault**
June 5, 2018

This isn't so much post-truth as it is post-empiricism. And since empiricism is the sine qua non of science it is also post-science. "Impenetrable complexity" would seem the post-modern harbinger of a new dark age, one wherein the very purpose of science, to understand the nature of physical reality, is seen as a quaint anachronism.

10. **Low Math, Meekly Interacting**
June 5, 2018

Douglas,

I had the exact same impression of an earlier Dijkgraaf contribution: This is not up to Quanta standards. My suspicion at the time was maybe he was too busy to finish the article properly. Peter's (probably more accurate) assessment was being an expert can make it very hard to write effectively for non-experts. Sometimes you need a really good science journalist to do the subject justice for lay audiences. Doubtless there's a little of that problem involved.

But, again in agreement with you, the trouble runs much deeper. I want to know what this work tells us about OUR corner of the landscape. Because that's the part human beings can do something remotely resembling what I like to think of as "science", i.e. some day, some how, speculations make contact with empiracle observations. Otherwise, not only is the work perplexing, it's profoundly BORING. I want to know what dark matter is. I want to know if there was an inflationary epoch or not. I want to know if gravity must be modified. Etc. By telling us about "everything" we are supposed to accept that it's OK to learn precisely nothing specific about the universe we inhabit, nothing illuminating in any remotely constrained way about any of the highly observable mysteries we know exist. New experimental data can't come fast enough.

11. **atreat**
June 5, 2018

Douglas, how can we expect better of Quanta magazine when this guy is the Director at IAS? Seriously, how can a magazine be expected to check this guy when his fellow scientists do not? This is embarrassing for the entire scientific community…

12. **Peter Woit**
June 5, 2018

Douglas Natelson,
I don't think Quanta would have published this if it were by a science journalist, it does come off as uncritical propaganda for dubious speculation. The most disturbing thing about it is that it's not written by a lazy journalist, but by Robbert Dijkgraaf, one of the most respected and prominent figures in theoretical physics. Dijkgraaf has been one of the most influential scientists in the Netherlands and now is director of the IAS. In these roles he will have a great deal to say about the future of the field. At one point multiverse mania was concentrated on the West coast, with the IAS not following this. That seems to now have definitively changed.

13. **CWJ**
June 5, 2018

Kir,
Keep in mind, this isn't the totality of physics. For perspective, high energy particle theory is only about 1% of the NSF physics budget, and only about half of that is spent on extremal ideas like this.

LMMI,
Unfortunately, it seems the MiniBOONE results, while congruent with LSND, are still just as confusing. Just saw a talk which explained it well. There are two elements to any neutrino oscillations–the mass difference, which gives the oscillation length, and the mixing matrix elements, which gives the oscillation amplitude. While the oscillation length from MiniBOONE/LSND suggest a 4th (sterile) neutrino, the mixing matrix elements needed are so large they should have been seen in other experiments. So it seems the simplest explanation, a 4th sterile neutrino, is still problematic. Apparently a third experiment, MicroBOONE, is in the works…

14. **Amitabh Lath**
June 5, 2018

Interesting how institutions get captured by a given theory. When I was a postdoc, the Fermilab theory group was all into Technicolor. Michigan and Gordy Kane were all SUSY, all the time. And now the IAS seems to be into Landscape. I heard tales that Berkeley in the "Tao of Physics" era was the same way with whatever theory of hadrons they were peddling at the time.

I wouldn't worry about it too much. Dark Matter and Dark Energy are still need to be explained, and the hierarchy problem isn't going away anytime soon.

15. **NoGo**
June 5, 2018

Aside from the content of the article (which people much more competent than I commented on), I've noticed that in form it was dumbed-down much more than I would expect in a Quanta article. All this baby talk about Chinese and Italian food, which as far as I can tell explains nothing, would not look out of place in mainstream media, if not Reader's Digest, but not in Quanta…

On this background statements like "string theory has no free parameters" and "It doesn't make sense to ask which string theory describes our universe, because there is only one" are especially puzzling. I wonder what he meant by this (if anything). My best effort in understanding the food analogy is that it tries to indicate that something like "all variants of string theory predict the same physical effects", but since he himself implies that string theory does not predict any specific physical effects, I remain puzzled…

16. **Phogos**
June 5, 2018

I'm actually rather encouraged about field moving toward it/qbit stuff. I think the chances of it leading to a TOE any time soon are pretty slim, but papers such as Witten's expository one are rigorous, interesting to read, and connect with other disciplines. So as a taxpayer I'm getting some value from them. That was most assuredly not the case for 99% of what the field has produced over the past decade or two.

17. **Marko**
June 6, 2018

"It seems that Witten has taken up the study of quantum information theory"

It's where the most funding is going to be available in the next 10-20 years. EU has provided a 20bn eur budget to fund quantum information science in that period, while China has a couple of orders of magnitude more than that — they already have a satellite in orbit, sending Bell pairs across continents and providing for quantum-crypto secure phone calls between China and Austria…

One would be stupid not to get string theory into the mix for a piece of the pie, regardless of any actual or potential "radical new framework" stuff. People who give the money won't recognize any lack of content anyway.

But I'm completely baffled that Witten actually wrote a 30+ page recap of stuff that is readily available in textbooks, and written by actual experts on quantum information theory. What is he trying to achieve with that?

Best, 🙂
Marko

18. **Peter Woit**
June 6, 2018

Marko,
Witten is lecturing at the PITP school this summer, I assume this document was prepared for that purpose. Also, best way to learn a subject is to write about it…
He also recently wrote this other expository article:
https://arxiv.org/abs/1803.04993
I doubt that funding has much to do with his getting interested in this, he's someone whose research will get funded if he wants, whatever he is doing.

Dijkgraaf on the other hand has for many years not been doing much research, his main activity now is fund-raising. I think his writings for the public should be seen in that light: he would like to get people to believe that all is well with HEP theory, so that they can be more readily convinced to fund it.

Heinz-Dieter Zeh 1932-2018

June 6, 2018
Categories: Obituaries

I was sorry to learn today of the death on April 15th of H. Dieter Zeh, one of the major figures responsible for improving our understanding of the physics involved in the measurement problem and related interpretational issues in quantum mechanics. For an excellent account of the story of Zeh's work and its early reception, see Adam Becker's recent book What is Real?. For a very good article about Zeh (in German), see here.

Zeh is the physicist most responsible for first identifying and studying the crucial role of decoherence in the measurement problem. As Becker explains, he encountered a quite hostile reaction to his early work from defenders of Copenhagen orthodoxy. He finally managed to get his paper On the Interpretation of Measurement in Quantum Theory published in 1970, and then started to explore what came to be known later by the term "decoherence". In later years he wrote many articles explaining these ideas, for one that includes some historical context, see here. Zeh maintained a website with links to his writings, at www.decoherence.de or www.zeh-hd.de, which seems to be down at the moment, hopefully only a temporary situation (a recent Wayback Machine capture is here).

I was pleased that every so often Zeh contributed insightful comments here, most recently just three weeks before his death. Here's a list of the ones I found in a quick search:

- February 22, 2012
- February 22, 2012
- April 26, 2012
- August 17, 2014
- March 23, 2018
- March 24, 2018

During the past few years I also had some email exchanges with Zeh. For details of his latest thinking about issues like the multiverse, he pointed me to this paper, which he every so often updated. A few months ago I was quite sorry to realize (when someone told me that Zeh lived in Heidelberg) that I had missed an opportunity to try to meet him in person when I was there a couple years ago. I'm even sorrier about this now that such a meeting will no longer be possible.

Update: Claus Kiefer has written an obituary here. Zeh's website will remain available here.

Comments

1. **Another Anon**
   June 6, 2018

   Very sad news.
   Surely he was deserving of the Nobel Prize for proposing decoherence which, as he said in that last comment on your site, has now been clearly "experimentally confirmed".

Back at Work

June 7, 2018
Categories: Uncategorized

Now back from a week away, mostly spent in Cambridge. Among the accumulated items of interest:

- Inference has a review of my book, Woit's Way, by Andrew Jordan. I like the way it starts out:

  > *Quantum Theory, Groups and Representations* is based on a series of lectures that he gave at Columbia University.
  >
  > And it is excellent.

  The review gives a very good explanation of what's in the book, what level it's at, and what I'm trying to accomplish. Besides getting all this right, he also gets right some of the things that could have been done better (I did the indexing and I'm kind of lazy, it should have at least twice as many entries).

  A reminder: the book is available either in my version at my website, or from Springer here.

  By the way, when I was in Cambridge I spent a couple days at the conference in honor of Bert Kostant, who was a major figure in the study of the relation of representation theory and quantization. Among the talks there, David Vogan gave a survey talk on Quantization, the orbit method, and unitary representations. He explains clearly the fundamental relationship between representation theory and quantum theory that is central to my book. Roughly the first half of the talk corresponds to topics discussed in the book. I decided to not write about the topic of the second half of Vogan's lecture, the representation theory of reductive groups and the orbit method, since that would take the book in a different direction, one currently of more interest to mathematicians than physicists (and Vogan has already done a better job of writing about this topic than I ever could).

- Also in the new issue of Inference are several pieces commissioned as responses to Natalie Paquette's wonderful survey article A View from the Bridge about topological QFT and influences running from physics to mathematics. These pieces include takes on the relation of math and physics from Édouard Brezin, John Iliopoulos, Hirosi Ooguri and Martin Krieger, as well as a rather odd one from string theorist Xi Yin.

  Yin's piece is entitled An Ode to Ugly Physics, and he argues that:

  > the deepest and most far-reaching ideas of physics are not the most elegant or beautiful, but the ideas that are confusing, not rigorous, improperly formulated, or, in fact, utterly incomprehensible to mathematicians.

  One problem is that many of the examples he gives of "ugly" physical ideas (for instance, spontaneous symmetry breaking) are ones that I think most mathematicians would describe as rather beautiful. He's right that some of the examples he gives (e.g. complex string theory calculations) are ones that mathematicians wouldn't find that beautiful, but often these are calculations that have been unsuccessful in their goal of making contact with reality. Yin ends with:

  > I believe that part of the job of a theoretical physicist is to make the lives of mathematicians miserable. There are, incidentally, few things I can think of that could make a mathematician more miserable than reading Leonard Susskind's papers.

  This includes a footnote to this recent paper by Susskind, and he's right that this is not one mathematicians would think highly of. For good reason though, with Yin's implication that this is an important idea in theoretical physics something I find rather dubious (but then again, some would say I'm a mathematician…).

  Truly bizarre is Yin's response to the fact that string theory has failed to make any connection to observable physical reality:

  > I couldn't help but notice a striking parallel with the way mathematics became detached from physics during the nineteenth century and, in particular, the outrage that accompanied Cantor's transfinite set theory and Hilbert's non-constructive proofs. Was the kind of mathematics that could never be exhibited with real objects actual mathematics, or was it theology? With the benefit of hindsight, we now know that the mathematics flourished like never before during the twentieth century. One can only hope the same thing happens with string theory in the decades to come.

  So, having no connection to experiment and observation is not a bug, but a feature, exhibiting a radical new advance in how to do physics? This takes the "post-empirical" thing even further than I've seen anywhere else.

  Finally, also in the same issue of Inference is the final part of a series of articles by Sheldon Glashow, this one dealing with the Standard Model. Glashow ends the essay with some comments on string theory, quoting someone I have to agree with.

- On the neutrino front, there's a new result out from MiniBooNE that has gotten a lot of attention. As usual, good sources for clear explanations and informed evaluations are Tommaso Dorigo and The Mad Hatter.

  For much more of the latest results on neutrinos, Neutrino 2018 is happening this week in Heidelberg, slides of presentation appearing here.

- The Perimeter Insitute earlier this year hosted a workshop on geometric Langlands and QFT, talks available here. Dan Falk has an article about this subject here. He also has a piece about Why some scientists say physics has gone off the rails, partly based on Sabine Hossenfelder's new book, which I'll write about here very soon.
- Michael Harris at his book's blog tells the story of how he was commissioned by New Scientist to write something about Peter Scholze's ideas. His draft ended up not getting used, luckily for us he includes it in the blog posting. New Scientist did however end up publishing a story about this, under the headline The Theorem of Everything (non-paywalled version here).
- The Simons Foundation has just put out its 2017 annual report. To get some idea of their increasingly large influence on mathematics and physics research, here are some numbers:

  Assets, end 2017: \$3.298 billion
  2017 income: \$646 million
  2017 expenses: \$409 million
  2017 grants: \$273 million

  Mathematics and physical sciences receive 31.77% of the grant money. To get a sense of the scale of this, one could compare the fraction of expenses corresponding to math and physical sciences (31.77% x \$409 million= $130 million) to the FY 2017 NSF budget numbers

  Mathematical sciences: \$234 million
  Physics: \$281 million
  Astronomical sciences: \$252 million

In the areas of math, physics and astronomy where the Simons Foundation is concentrating its resources, I suspect that the amounts they're spending are getting up to the NSF level.

Update: For a take on the Copenhagen interpretation that I very much agree with, see Philip Ball's Myths of Copenhagen.

Comments

1. **Carlos**
   June 8, 2018

   Any hope of finding the videos for Kostant conference's talks online?

2. **Peter Woit**
   June 8, 2018

   Carlos,

   I see that videos of the talks have just been posted, available via the conference website:
   https://math.mit.edu/conferences/kostant/

3. **Carlos**
   June 8, 2018

   That's great Peter. Thank you!

4. **Peter Erwin**
   June 9, 2018

   If you're going to apply "31.77%" to the entire budget of the Simons Foundation (including things like "depreciation and amortization" and "taxes"), then it makes more sense to compare it with the total "Mathematical and Physical Sciences" part of the FY2017 NSF budget, which is \$1.349 billion. That figure doesn't include whatever part of the \$200 million "Major Research Equipment & Facilities Construction" goes to mathematical and physical sciences (e.g., about \$90 million for telescope construction), so it's an underestimate.

   Funding at a level roughly 10% of that which NSF provides is pretty damn impressive, but not quite "getting up to the NSF level" yet. (And this of course doesn't include the funding that NASA and DoE provide.)

5. **Steven Patenaude**
   June 13, 2018

   I'd call Andrew Jordan's review high praise. If I were the audience for your book, I'd snatch it up in a heartbeat.

6. **David Roberts**
   June 15, 2018

   Slightly OT, but news perhaps of interest: Kashiwara has won the Kyoto Prize: https://www.kyotoprize.org/en/laureates/masaki_kashiwara/

7. **LMR**
   June 15, 2018

   While we're on the topic of the Copenhagen interpretation, you might be interested in Tim Maudlin's combined review of Adam Becker's book, "What Is Real?" (mentioned in Philip Ball's blog), with Errol Morris' "The Ashtray: (Or the Man Who Denied Reality)", a very critical look at Kuhn and his ideas:

   http://bostonreview.net/science-nature-philosophy-religion/tim-maudlin-defeat-reason

Lost in Math

June 7, 2018
Categories: Book Reviews

Sabine Hossenfelder's new book Lost in Math should be starting to appear in bookstores around now. It's very good and you should get a copy. I hope that the book will receive a lot of attention, but suspect that much of this will focus on an oversimplified version of the book's argument, ignoring some of the more interesting material that she has put together.

Hossenfelder's main concern is the difficult current state of theoretical fundamental physics, sometimes referred to as a "crisis" or "nightmare scenario". She is writing at what is likely to be a decisive moment for the subject: the negative LHC results for popular speculative models are now in. What effect will these have on those who have devoted decades to studying such models?

Back in 2006 Lee Smolin and I published books concerned about where fundamental physics was heading, and five years ago Jim Baggott's Farewell to Reality appeared with another take on these issues. Hossenfelder's is the first book on this topic to appear since the LHC results showing a vanilla Standard Model Higgs and no evidence of supersymmetry or other speculative BSM physics. The remarkable thing she has done is to address this in a characteristically direct manner: go talk to those responsible and ask them what they have to say for themselves.

Four of the people that Hossenfelder interviews would be on any short list of the most influential figures in theoretical particle physics, both responsible for where we are now by their past actions, and looked to by others for a vision of where the field is going next. They are Nima Arkani-Hamed, Steven Weinberg, Frank Wilczek, and Joe Polchinski.

Arkani-Hamed is introduced with:

> He's won loads of awards, including the inaugural 2012 Breakthrough Prize for "original approaches to outstanding problems in particle physics." The problems are still outstanding. So is Nima.

and here's an extract from the interview

> "Has the LHC changed your perspective on naturalness?" I ask.
>
> "It's interesting–there is this popular narrative now that theorists before the LHC were totally sure that susy will show up, but now there's a big blow. I think that the people who are professional model builders, the people I consider to be the best people in the field, they were worried already after LEP… The good people, they were not at all sure susy would show up at the LHC. And nothing has changed qualitatively since 2000. Some loopholes have been closed, but nothing has changed qualitatively…"

As with many of the interviews, Hossenfelder intersperses her own internal response to what she's hearing:

> But not one of those "best people" spoke up and called bullshit on the widely circulated story that the LHC had a good chance of seeing supersymmetry or dark matter particles.

She doesn't mention, but surely is aware, that many prominent theorists pre-LHC had made public bets that the LHC would find SUSY, and that those wagering this way included Arkani-Hamed himself. For accounts of the 2016 Copenhagen event where the bet was paid off, see here and here. You can read there what Arkani-Hamed had to say then about losing the bet, and the quote:

> "I think Winston Churchill said that in victory you should be magnanimous," Damgaard said after Arkani-Hamed's talk. "I know also he said that in defeat you should be defiant. And that's certainly Nima."

In the interview with Hossenfelder, Arkani-Hamed goes on to say:

> The people who were sure it would be there are now positive it's not there. There are people now who speak out about being depressed or worried or scared. It drives me nuts. It's ludicrously narcissistic. Who the fuck cares about you and your little life?

There's a lot more in the interview and you should get the book and read the whole thing. Hossenfelder does a wonderful job of portraying both Arkani-Hamed's serious arguments and his aggressive "Damn the torpedoes" self-confident attitude. This is not someone who is going to admit that, whatever bet he lost, some failure has occurred that indicates this is a time for reflection on mistakes made and reevaluation of the path forward.

Hossenfelder travels to Austin, Texas to talk to Steven Weinberg, who it appears may not realize she is a physicist, just has been told he is supposed to talk to a "writer". She notes that:

> Weinberg doesn't talk with you, they told me, he talks *at* you. Now I know what they mean. And let me tell you, he talks like a book, almost print-ready.

I won't try and reproduce much of her conversation with Weinberg, the multiverse is a main topic (she thinks it's an empty idea, Weinberg is willing to go along with it). About where particle theory is headed, Weinberg says:

> I don't know how much elementary particle physics can improve over what we have now. I just don't know. I think it's important to try and continue to do experiments, to continue to build large facilities… But where it will end up I don't know. I hope it doesn't just stop where it is now. Because I don't find this entirely satisfying…
>
> I don't take seriously any negative conclusion that the fact that the LHC hasn't seen anything beyond the standard model shows that there isn't anything that will solve the naturalness problem… Supersymmetry hasn't been ruled out because it's too vague about what it predicts.

Her next interviewee is Frank Wilczek, who she finds in Tempe, Arizona. His take on string theory unification is rather negative:

> … it's not clear what the theory is. It's kind of miasma of ideas that hasn't yet taken shape, and it's too early to say whether it's simple or not–or even if it's right or not. Right now it definitely doesn't appear simple.

Asked about the argument that string theory could reproduce gravity, Wilczek responds:

> If your standards are low enough, yes. But I don't think we should compromise on this idea of post-empirical physics. I think that's appalling, really appalling… If there was any bit of experimental evidence that was decisive and in favor of the theory, you wouldn't be hearing these arguments. You wouldn't. Nobody would care. It's just a fallback. It's giving up and declaring victory. I don't like that at all.

Wilczek is still unwilling to give up on SUSY and the idea of a SUSY GUT, with his main argument the coupling constant unification calculation he did with Dimopoulos and Raby back in 1981:

> “They haven’t found susy partners, though,” I say. “Is this something that worries you?”
>
> “I am starting to get worried, yes. I never thought it would be easy. There have been bounds from and proton decay for a long time, and this indicated that a lot of the superpartners have to be heavy. But we have another good shot with the energy upgrade. Hope springs eternal… I would definitely not believe in supersymmetry if it wasn’t for the unification of gauge couplings, which I find very impressive. I can’t believe that’s a coincidence.”

~~It’s not mentioned in this book~~ , but Wilczek has already paid off one bet about SUSY (with Garrett Lisi) and likely will have to pay off another next year. I don’t know if by “energy upgrade” he’s thinking of the HE-LHC, or the 100 km much bigger proposed ring, but in any case those won’t happen before at least 2040. No matter what happens, I don’t think Wilczek will ever change his mind about the SUSY-GUT paradigm he has found attractive since the 1980s.

In January 2016 Hossenfelder traveled to Santa Barbara to talk to Joe Polchinski, who was already sick with the brain cancer that ultimately would take his life two years later. Unlike Wilczek, Polchinski was a fan of string theory and of evaluating it by “post-empirical” criteria. He at one point published a “Bayesian” calculation arguing that string theory is correct with probability “over 3 sigma” (i.e. over 99.7%). Asked about prospects for a unified theory, Polchinski says:

> I think string theory is incomplete. It needs new ideas… But string theory has been so successful that the people who are going to make progress are the people who will be building on this idea.

Arkani-Hamed, Weinberg, Wilczek and Polchinski reflect a range of points of view about the current situation and what it means. Unfortunately it seems to me that they share an unwillingness to face up to failure, and this doesn’t bode well for the future of particle theory, with “more of the same” the agenda that is being set.

Besides these four interviews, the book also contains accounts of meeting and discussions with quite a few other physicists, all well worth reading, and often written with a sly humor. The description of visiting Garrett Lisi on Maui is not to be missed, and he has a lot of sensible things to say (“For a surf bum, he’s surprisingly intellectual” the author writes). He tells the story of how Jacques Distler and others threatened (unsuccessfully) to organize a boycott of Scientific American if it published an article by him. In addition to the interviews there’s a great deal of valuable discussion of the problems with the way research is organized and the reward structures scientists operate under (for instance, publicly admitting failure is definitely on the “not encouraged” list).

So far I’ve ignored the main framing device that Hossenfelder uses throughout the book, that of her questioning the idea of “beauty” as a motivation for evaluating ideas about physics. This is not because I disagree all that much with what she writes, but instead that I fear a complex set of issues is likely to get over-simplified, and this over-simplified version of the book’s argument is all that much of the public is ever going to hear about it. Hossenfelder explains that the concept of “beauty” she is challenging is a specific set of ideas about “symmetry, unification and naturalness” that she sees as dominating physics research. I agree that there’s a problem with this specific set of ideas and how they have been used, but I’d keep them separate and don’t see putting them together as “beauty” to be helpful. At various points she makes it clear that her worry is that we are getting stuck due to outdated notions of “beauty”, while still believing that successful new ideas will come with a new form of “beauty”.

The book ends with

> We know that the laws of nature we presently have are incomplete. To complete them, we have to understand the quantum behavior of space and time, overhauling either gravity or quantum physics, or maybe both. An the answer to this will without doubt raise new questions…
>
> …There’s much work to do. The next breakthrough in physics will occur in this century.
>
> It will be beautiful.

Update: Science magazine has a review. For some reason they seem to have decided it was a good idea to have the book reviewed by a postdoc doing exactly the sort of work the book is most critical of. The review starts off by quoting nasty anonymous criticism of Hossenfelder from someone the reviewer knows on Facebook. Ugh.

Update: I’ve written a similar but somewhat different version of this review for MAA Reviews, one aimed more at mathematicians.

Update: More reviews here and here, as well as a posting from Hossenfelder where she explains her current professional situation in the context of deciding to write the book.

Update: I’m glad to see that Science has edited the review there to remove the use of an unattributed quote.

Comments

1. **Sabine**
   June 7, 2018

   Hi Peter,

   Garrett brings up the bet with Frank Wilczek and I mention this bet and the other susy bet in the last section (to say who won and who lost). Thanks for the review! Best,

   Sabine

2. **AS**
   June 8, 2018

   Raising these issues 20 years ago was useful and needed courage. Now it’s useless

3. **Luca**
   June 8, 2018

   @AS: Raising these issues 20 years ago was useful and needed courage. Point is, nothing changed: so, may be, it’s not completely useless to continue raising these issues.

4. **David Brown**
   June 8, 2018

   https://www.kirkusreviews.com/book-reviews/sabine-hossenfelder/lost-in-math/
   https://www.publishersweekly.com/978-0-465-09425-7

5. **Peter Woit**
June 8, 2018

Sabine,
Yes, I did notice while reading the book that you had mentioned the bet with Garrett, but had forgotten that when I wrote the review, just edited to fix it.

6. **Low Math, Meekly Interacting**
June 8, 2018

The "demand for alternatives" style of riposte comes up so frequently I think it deserves its own blog. Not so much a refutation of the "only game in town" assertion, which is really a separate issue. I'm thinking more of this highly prevalent notion that, assuming there really is only one game in town, one is therefore compelled to play it or shut up. In fact, the original joke is only funny because the players know the game is rigged. It doesn't seem to occur to them to just stop playing and find some other way to occupy their time. A rather sad explanation might be that they're compulsive gamblers, and simply can't help themselves.

Why doesn't it occur to people that it's quite enough to point out the game is rigged? Why is it incumbent on him or her doing this service to also have another game at the ready, or refrain from comment? Of course it would be preferable if an alternative could be proffered immediately, but that's not the point. It's the responsibility of the player of the rigged game to act on that knowledge rationally, i.e. stop being cheated, maybe take a break and at least think about your next occupation if you've got nothing better to do. Why is it someone else's job to figure all that out for you? Apparently many folks feel that it very much is the critic's job, which is truly strange.

7. **Amitabh Lath**
June 8, 2018

Oh come on! LHC has not found evidence of new physics *in the easy to look for signatures*: lepton or jet resonances, large missing momenta, multiple leptons, etc.

There are so many ways we could be missing the new physics signals. It could be decaying to many jets (the multi-jet background is millibarns, and we don't understand jet production even to leading order, so it is tough picking out a new physics signal at femptobarn production cross section). It could be long lived. It could be very low mass (LHC detectors can be blind for low mass signal, but we are figuring out clever new techniques). It could be a high mass thing decaying via lots of intermediate states, shooting off very soft pions at each step. There are so many hard to find places that new physics could be hiding, and we have just started. LHC data taking started less than a decade ago.

(of course, when I say "easy to look for" I mean doable, but with LOTS of hard work)

8. **Daniel Tung**
June 8, 2018

A multiverse believer probably should not think that our universe has beautiful laws, simply because universes with ugly laws obviously outnumber universes with simple, beautiful laws.

9. **Avals**
June 8, 2018

Once I met Sabine Hossenfelder at a conference in Warsaw. I was standing in front of the conference schedule, and she dropped by and pointed out to me her own talk in the schedule, and said that that was the talk I absolutely have to go see. I thought it was pretty strange and narcissistic. I didn't go to listen to her talk. Nor will I go read this book.

10. **Peter Woit**
June 8, 2018

Amitabh Lath,

I think the reference to negative LHC results "for popular speculative models" is pretty accurate, specifically I had in mind technicolor, SUSY, extra dimension models. In, for example, minimal SUSY with 100+ extra parameters, it's not completely clear what "generic" means but the possibilities you list seem to me to correspond to arguably "non-generic" cases. The full parameter space will never get covered and minimal SUSY will never get completely ruled out, but it becomes less and less plausible that the superpartners are hiding in exactly the regions hard to access experimentally.

To be clear, while it's accurate to say that the LHC has so far seen no non-SM physics, that certainly doesn't mean it's not going to, there's a very long ways to go and many places to look. To the extent early searches have been focused on looking for specific signatures of implausible SUSY and extra-dimensional models, as those searches get done and people move on to look for other things, arguably the chance of success will go up…

11. **Peter Woit**
June 8, 2018

Avals,
I met Sabine in person for the first time a few months ago, and she did not strike me as unusually narcissistic, but did strike me as unusually direct. At your conference in Warsaw likely all the other speakers standing around also thought that you should be going to their talk, but unlike her they didn't directly tell you this.

I'm curious to see what the reaction to her book will be, as compared to what happened back in 2006 with my book and Lee Smolin's. Smolin and I certainly both thought our ideas were important and people should pay attention to them, but I think we both lack Sabine's directness, which may be useful to her in getting attention for her ideas.

12. **Art**
June 8, 2018

Michael Harris' "Mathematics Without Apologies" made the point that mathematicians and physicists are some of the last people to still use beauty as a criterion for their efforts, artists having long ago abandoned that standard. I recall being quite struck by that.

13. **Supernaut**
June 8, 2018

" The review starts off by quoting nasty anonymous criticism of Hossenfelder from someone the reviewer knows on Facebook. Ugh."

I'm starting to get the impression that Science mag is losing respectability a bit.

"Raising these issues 20 years ago was useful and needed courage. Now it's useless."

Not at all; as Peter points out, this book comes out post-LHC results. Sounds interesting and I'm hoping to get it. Thanks for the review.

14. **Amitabh Lath**
June 8, 2018

Peter, first please call me Amit.

Basically any beyond-SM stuff that shows up (at LHC or in some underground tank) has a mass scale, some effective coupling to the SM, and possibly quantum numbers like charge, flavor, color, lepton number. A complete top-down model like SUSY is one way to generate these but frankly simple ad-hoc models that map out the space of possible new physics signatures are more helpful right now.

So even if the parameter space for (some minimal version of) SUSY is more or less covered, that hardly means that the parameter space for all possible new physics signatures allowed by field theory is eliminated.

15. **Sabine**
June 9, 2018

Avals,

I can't recall neither you nor the conference, but clearly we don't share the same sense of humor.

16. **AS**
June 9, 2018

20 years ago many influential theorists did not want to see what was going to happen, so it was useful and "dangerous" to point out the problem. It was as useful as alerting about a possible subprime mortgage bubble. After that both bubbles bursted, it's now useless to write a book about "idiots who lost their house". What was overpriced is now underpriced: there are potentially interesting ideas which are not being explored because they belong to fields that presently have an excessively bad reputation among young theorists.

17. **Thomas**
June 9, 2018

"For some reason they seem to have decided it was a good idea to have the book reviewed by a postdoc doing exactly the sort of work the book is most critical of. "

Why do you need to specify she is a postdoc ? How is this relevant ? Why not say "a researcher" ?

18. **ranjeet**
June 9, 2018

Dear @Luca, what AS was saying (most probably) that, raising these issues 20 yeas back would have been fruitful and would have directed the physics to the meaningful trajectory but now it has gone to irreversible and unfortunate distance…. so that it is now useless even to talk about it. But, it is never too late…. for a good thing or correction.

19. **Anonyrat**
June 9, 2018

^^^ After the bubbles burst, it is worth understanding what happened to keep it from recurring.

20. **Amitabh Lath**
June 9, 2018

Peter, you said:
>as those searches get done and people move on to look for other things, arguably the chance of success will go up…

Both ATLAS and CMS already have large groups looking at non-SUSY signatures for new physics.

Both experiments have set up multiple physics groups that concentrate on a specific type of signature and build up analysis expertise. Both have an "Exotics" group that is separate from the SUSY group, and searches for things like microscopic black holes, long-lived signatures, quark substructure, multi-jet resonances, particles with both lepton and baryon number, 4th generation particles, Majorana neutrinos…

If you are under the impression that somehow experimental physics is in the thrall of SUSY and only when it is completely wiped out (ha!) will our imaginations be freed, you are mistaken.

PS: If you have any clever ideas for possible beyond-SM stuff that you feel is being ignored (SUSY or not), feel free to share. Of course your ATLAS colleagues at Columbia might like to get a first crack at it 🙂

21. **Frank Wilhoit**
June 9, 2018

We begin to see more and more questions paraphrasing "has physics gone off the rails?" In other words, has the *science* gone off the rails? But this is backwards.

Dr. Hossenfelder's book shows that the political environment of the field has gone *completely* off the rails, as shown by the fact that people who have real ability and real accomplishments are quoted spouting utter, patent nonsense.

When this happens, to any field, the substantive efforts must follow the politics off the rails. (If this is not obvious, I do *not* refer to the internal party politics of any nation or even to the corresponding meta-politics; I refer to the ideological premises that constrain the public discourse within a field of inquiry.)

22. **Peter Woit**
June 9, 2018

Amit,

I was just trying to come up with some reason for optimism… Was not trying to imply that the LHC experiments are currently overly devoted to looking for SUSY. Actually, over the past year or so I've been struck by how little activity there is on that front (i.e. few new SUSY limits based on 2017 data).

The problem of who to share my promising new idea about an LHC testable improvement of the Standard Model with has not yet come up…

23. **Peter Woit**
June 9, 2018

Thomas,
The level of experience in the field of the reviewer is relevant. I still think it is an odd decision of Science to commission of a review of a book like this from someone only one year past their Ph.D. (and, as far as I can tell, no experience writing book reviews).

24. **Tanner**
June 9, 2018

Enjoyed your informative review of Sabine's book. Gave me a sense of the book. It's unfortunate that the review published in Science was more focused on snark than being informative.

25. **tulpoeid**
June 9, 2018

Allow me some replies to three comments:

@Thomas, in addition, a postdoc is not tenured yet so highly dependent on their seniors' views of them.

@Amitabh, the point is that all predicted (either minimal or reasonable) parameter space by the theories criticized here has been excluded. This is very different context than the general searches at LHC experiments. Also, during the first years of LHC running, the physics groups had to make very real-world decisions on which analyses get priority (in computing and human time).

@AS, right now we are in the middle of the "bubble bursting" but it shows no sign of slowing down. For instance, multiverse has started taking over public science writing during the last year.

26. **Mike Klymkowsky**
June 9, 2018

There are disciplinary issues here as well. Biologists quickly learn that "brilliance" alone is not sufficient to understand the complexity of (history-dependent) biological systems.

Physicists, particularly of the theoretical-type, appear to believe that everything can be deduced from first principles or hypothetic, unobservable constructs – that brilliance is everything (together, perhaps, with an excessive self-confidence even in the face of serious miscalculation).

see Bernstein, R. (2015). "Belief that some fields require 'brilliance' may keep women out." Science. http://www.sciencemag.org/news/2015/01/belief-some-fields-require-brilliance-may-keep-women-out

27. **Dempsey**
June 9, 2018

I've heard Wilczek stress the coupling constant unification/SUSY calculation several times. As a rank outsider, I've always been impressed with this, but then you read that maybe it's not so special after all. I have no way of evaluating this. On the one hand, Wilczek is obviously in the first rank, and you can't ignore his opinions. On the other hand, the coupling constant unification is his baby, and maybe he seriously overstates its importance. Wish I knew.

28. **Anonyrat**
June 10, 2018

Dempsey : from Bee, 2007
http://backreaction.blogspot.com/2007/12/running-coupling-constants.html
Read the comments too.

29. **Thomas Larsson**
June 10, 2018

Dempsey, you may want to have a look at what Wilczek wrote in his [Future Summary]() from 2001:

"5.5. Produce the New Particles!
Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new R-odd particles. Even in the focus point scenario, there must be several accessible to the LHC."

Now that the results from the LHC are in, it is clear that at least low-energy SUSY is plain wrong.

30. **David Orrell**
June 10, 2018

The idea that "science is being led astray by aesthetics" (as I put it) was the thesis of my 2012 book Truth or Beauty: Science and the Quest for Order (Yale UP). Glad to see that some physicists are coming round to the same idea!

31. **Peter Woit**
June 10, 2018

Dempsey,
I looked into this when writing "Not Even Wrong" and wrote about it there. At the time, one recent source was this
https://arxiv.org/abs/hep-ph/0202185
which states:
"The supersymmetric gauge coupling unification misses by about 10%. More precisely, the experimental value of the strong coupling is about 10-15% lower than the value computed by running down theoretically from the point were the SU(2) and U(1) couplings meet."

I haven't looked for more recent versions of this calculation. From what I remember, one part of the story is that the two-loop calculations make agreement worse, people often refer to the better agreement at one-loop. Also, how you numerically categorize the accuracy of this calculation depends on how you formulate things (I think it looks better if you fix observed couplings and compare extrapolated couplings at the GUT scale).

There are also two generic problems with this kind of calculation, indicating one shouldn't take any particular numbers too seriously:

1. The result depends on what you choose as your scale of SUSY breaking, as well as other details of the many coupling in the SUSY model.
2. These calculations inherently assume a "desert", that there is no physics affecting the coupling constant running, in between the TeV scale and the GUT scale. I think most theorists have always felt this is an implausible assumption.

All in all, this has always struck me as a rather weak argument, and I suspect Wilczek's fondness for it has something to do with his involvement at its birth.

32. **Anonyrat**
June 10, 2018

A more recent SUSY unification++ calculation: nothing much changes, I think. https://softsusy.hepforge.org/doc/threeLoop.pdf

33. **Oss Ickle**
June 10, 2018

Avals,

I'm a professional comedy writer, and an avid reader of Sabine's writings (e.g., her blog, her comments to others' blogs). I can't wait to read her book.

She's absolutely hilarious, and when I read your comment about what she said, I laughed out loud (for the most recent time) at her wit, self-awareness, irony.

Perhaps there's a language barrier, perhaps you are humorless (or "differently humored"). Dunno.

34. **Amitabh Lath**
June 10, 2018

I don't know why everyone is so concerned what the Very Intelligent People are thinking about the multiverse or the landscape or whatever. I'm not up on my history of science but wasn't a similar group declaring physics over with in the late 19th century? This is what happens when theory runs ahead of experiment.

We need the 21st century versions of the Michelson–Morley and Rutherford results. And I am hopeful on that front. I can only speak to my small corner but the work going on in detector design, data analysis, triggers, etc is stunning. Truly bleeding edge. There are techniques being developed for microsecond trigger decisions that rival full offline analyses from a couple of decades ago. If it's there we'll find it. It won't be "here's SUSY" or "here's the techni-pions" or whatever. It will start out as an excess that won't go away, and build. Theorists (the good ones anyway) will jump on it and all this metaphysical talk will be forgotten like that 19th century end-of-physics talk.

35. **John Baez**
June 11, 2018

I wish people would stop using "physicists" or "theoretical physicists" to mean "high-energy particle physics theorists" when issuing condemnations such as Mike Klymkowsky's above:

> Physicists, particularly of the theoretical-type, appear to believe that everything can be deduced from first principles or hypothetic, unobservable constructs….

or more importantly because it's being read by more non-experts, [Dan Falk's article about Sabine's book](#):

> "All of the theoretical work that's been done since the 1970s has not produced a single successful prediction," says Neil Turok, director of the Perimeter Institute for Theoretical Physics in Waterloo, Canada. "That's a very shocking state of affairs."
>
> This doesn't mean physicists aren't busy; the journals are publishing more research than ever. But Turok says all that research isn't doing much to advance our understanding of the universe — at least not the way physicists did in the last century.

There's a lot of top-notch theoretical physics going on today in many fields – condensed matter physics, fluid dynamics, biophysics, astrophysics, etc. Experts will realize that the above complaints are really complaints about *one specific branch* of theoretical physics. But ordinary people may not realize this. They may actually believe it when people say theoretical physics is going down the tubes! I think anyone who really cares about physics should avoid spreading this wrong impression.

36. **John Baez**
June 11, 2018

Well, I said "high-energy particle physics theorists"… but maybe what people are really complaining about is "theoretical physicists seeking new fundamental laws", or something like that. I just want people to be specific about their complaints, not to tar lots of innocent victims with the same brush.

37. **Douglas Natelson**
June 11, 2018

Thank you, John. Bear in mind that often it's not just journalists, but the high energy theorists themselves who use all of "physics" as shorthand for their piece of it. It would be great to break them of that habit.

38. **Joelson F. Silva**
June 11, 2018

John Baez,

Finally someone pointed to this issue here. Many times physicists (high energy theoretical physicists, more especifically) use the term theoretical physics as synonyms for high energy theoretical
physics, and this is a huge mistake. For exemple, in my research area (condensed matter physics)
We living a great moment, hot topics , for example, topological phases of matter, exotic excitations, high temperature superconductors, transport in
mesoscopic systems (non-equilibrium dynamics), open systems, low-dimension physics and the Fermi liquid breakdown, strongly correlated electrons, etc. All topics above using complex theoretical techniques and bring up important new results in theoretical and mathematical physics every day (and more important,
for us physicists, with experimental results). Then, I believe that high energy physcists must be more carefull with the term "theoretical physics" .

39. **John Baez**

June 11, 2018

Yes, Douglas, it's the physicists themselves who should take the lead, by explaining that there's a lot more to theoretical physics than the quest for a "theory of everything"… and the rest of physics is doing just fine. Theorists focused on this quest may not want to admit this, but in the long it's in their own best interest.

40. **Anthony**
June 12, 2018

Nature Magazine also has a review out.
https://www.nature.com/articles/d41586-018-05374-9

41. **Low Math, Meekly Interacting**
June 12, 2018

I rather like Siegel's unequivocal focus in his review, a point he's made again and again: beauty may or may not be a reliable guide, but it must be acknowledged that the beautiful ideas in question have simply failed. No mealy-mouthed excuses about the unexcluded regions of whatever parameter space, or undue acknowledgement of those counter-arguments. They have already proven to be wanting. When evidence falsifies, it's false. When evidence is unobtainable, it's folly. Period. Evidence is the only truly reliable guide, period. Seek it elsewhere, and only where there's some hope in finding it. It's worth shouting from the rooftops. Cognitive biases and fashion and sociology are all worth mention, but should be painfully obvious culprits in human affairs. How anyone could claim to be free of them is hard to fathom. That they do is still unsurprising. Trust empirical evidence, reproduced, and nothing else. I believe that is not only the key to the survival of all science, but of our species as well.

42. **Peter Woit**
June 12, 2018

LMMI,
My problem with this is that I don't think it's so simple, in particular I don't think it's a simple failure of "beauty". For instance, in the case of SUSY, there's a good argument that the super-extension of the Poincare group is a "beautiful" idea, but this idea immediately runs into the problem that you need to break the symmetry (explicitly or spontaneously) and what you get is very unbeautiful (eg. over 100 extra parameters to describe the possibilities). So, to me, what the LHC was testing (SUSY extension of the SM, softly broken at the TeV scale) was not a beautiful idea but an ugly one. A believer in beauty to my mind would have said that SUSY was already disqualified at the beginning, once one realized it had to be badly broken, and there was no beautiful way to do that. I think what Hossenfelder is pointing to as flawed is not actually arguments from "beauty", but arguments that start with a symmetry principle which has to be badly broken to agree with reality.

As for the "just trust experiment" idea, the problem we have is often a lack of any relevant experiments. That some small part of the theory community has gone off into ridiculous claims about "post-empirical" theory is a bit of a red herring. Few theorists really take that bizarre move seriously. The "post-empiricists" are doing great damage to the public perception of the subject, but the more significant issue is the large number of theorists working on the same failed ideas, hoping they can somehow extract something empirically testable out of these ideas. We've seen over the years that all you get by pushing "empirically testable" is moving people in the direction of studying ugly complex models that somehow might be testable, even though they are highly implausible and explain nothing. We might actually be better off with people giving up for now on testability and trying to find a new beautiful idea (i.e. new forms of symmetry). I think Hossenfelder in the end agrees that a successful new breakthrough will produce a new form of "beauty". To me it's thus not unreasonable to try and find that, independent of experiment if you have to. But you have to be really honest with yourself about when ideas don't work and are unbeautiful, and the sociological reasons why people can't or won't do that are a big problem (one that Hossenfelder has written a lot about).

43. **Anon**
June 12, 2018

hi peter — just got hold of the book and began reading it. the beginning was interesting and then after that it is quite confusing. Have to read it more carefully. The interviews with scientists are interesting.

I dont really get what the issue with beauty is, that Sabine is trying to convey. If some beauty driven theories are falsified that is part of the scientific process — to come up with falsifiable theories. Is it the time we are taking to abandon them? Whats happening currently is not very beautiful — to try and come up with theories that may have consequences for LHC even if they have problematic things like very small couplings for parameters you'd think are order 1 on grounds of beauty. The other thing that is currently being done that is not beautiful is to try and save theories like SUSY — which solved the hierarchy problem and could be falsified from that point of view. Like Nima wanting a 100 TeV collider to make sure that the hierarchy problem is not resolved even at that scale (or if it is then to discover the new particles/physics) — that collider would probe theories not as beautiful as the solutions LHC probed.

Anyway I think the biggest miss in the book is the neutrino masses and mixings which is the new thing that has happened experimentally — neutrino physics was anticipated considering symmetries such as B-L and parity.

Have had the book only for an hour or so. Will read more.

44. **Low Math, Meekly Interacting**
June 13, 2018

I truly hope you are right, Peter, and I would be extremely happy to be wrong. My admittedly rank-amateur outlook is one of pessimism without some breakthrough experimental guidance. I'm not all that convinced people should keep hammering away at the current data set while the elders hope that some day a young genius theorist, free of the shackles of these failed ideas, will save HEP theory from itself and bring enlightenment. I'd put the odds on that as roughly equal to the odds that SUSY will be confirmed by the 100 km collider. I.e., while it could happen, I see zero reason to expect it.

45. **Peter Woit**
June 13, 2018

LMMI,
The problem is that if you insist people wait for experimental guidance to try and make progress, you're basically arguing that theorists should abandon research on these questions. There is some sign that's already happening, as HEP theorists give up and try to rebrand themselves as condensed matter or quantum information theorists. Unfortunately their "giving up" often is taking the form of "we have discovered that the multiverse did it, so these questions are hopeless", discouraging anyone else from working on the questions that they failed at.

Personally it seems to me that there are huge unexplored areas at the intersection of QFT and mathematics. Some HEP theorists have moved from failed ideas to trying to work on fundamentals of QFT (without experimental guidance) and that's great. My one problem with Hossenfelder's book is that I fear her argument will be read as discouraging this, that it's "searching for beauty", so hopeless, whereas her real argument is with a narrow failed set of ideas, not "beauty" in general.

46. **Urs Schreiber**
June 13, 2018

whereas her real argument is with a narrow failed set of ideas, not “beauty” in general.

Nor maths, for that matter. Rules-of-thumb, such as “naturalness”, witness a dearth of rigorous arguments, not an abundance.

“The alternative to naturalness, often neglected as an alternative, is having a theory.” (Kane 17, p. 33, p. 57)

47. **Low Math, Meekly Interacting**
June 13, 2018

I find the fight over the “demarcation problem” to be tedious and don’t have much of an opinion myself, but could the situation for theorists interested in exploring the intersection of maths and subjects like QFT be improved simply by moving to a different department? It might be pure semantics, but if one chooses to call it a form of “applied mathematics” one can avoid a certain amount of philosophical angst, I suppose. It might also be a healthy development for HEP theory, if people in that bucket are perceived as going too far off the reservation (as opposed to just refusing to give up on failed predictions as such practice is more conventionally understood).

This truly is more of a question than a proposal, as I’m not really qualified to have an opinion.

48. **Peter Woit**
June 13, 2018

Urs,
I agree that the problem is caused by people who don’t have a theory, but Kane has no credibility since he is a perfect example of what you don’t want. Yes, he has a theory, lots of theories, one for every possible experimental result…

LMMI,
I don’t think the “demarcation problem” has to do with the boundary between mathematics and physics, it’s about the boundary between testable and untestable statements about the physical world, which is something different.

The problem here is people refusing to give up failed ideas, and thus flirting with non-science, not that they have moved into mathematics. While many physicists would love to get rid of this problem by dumping it on a math department, the math departments are not going to go along with this. There is a very healthy overlap between math and physics, but it’s something very different.

49. **Urs Schreiber**
June 13, 2018

The broad properties of the G2-MSSM which G. Kane is highlighting, such as the crucial “slightly split” susy, are not controversial (there was also some informed discussion of this on your blog a while back here). Kane et al. discuss the latest exclusion of parameter ranges in arXiv:1803.04394, this is not anything goes.

But even if the G2-MSSM is experimentally excluded tomorrow, a good point remains: This is a top-down model embedded in a coherent ambient theory, and with properties argued from by systematic analysis of the theory instead of by rules-of-thumb, avoiding the folklore fallacies that are being criticized here.

Of course the precise numbers extracted depend on assumptions, or else we are living in a Douglas Adams novel. It’s easy to make fun of this from the sidelines, but it remains a truism that the job of model builders is to tune the assumptions admitted by their models to fit the data.

Kane’s book is really good, sober and to the point. He has a great quote in there along the lines of better risking to make a fool of oneself than not trying the worthwhile. That’s the attitude that the community could use more of.

50. **Peter Woit**
June 13, 2018

Urs,
Your love for SUSY hype is getting off-topic, I’ll just point out that “really good, sober and to the point” is an extremely inaccurate description of Kane’s books about SUSY, for reasons that are laid out here:
https://www.math.columbia.edu/~woit/wordpress/?p=5793

To bring the discussion back to the topic of the book, Kane does appear in it. Hossenfelder describes the scene at the 2015 Munich conference, where Gross and other string theorists interrupted Kane’s talk, making it clear they did not think his “predictions” followed from string theory, but thought that he was using “additional hand-selected assumptions in order to reproduce what we already know about the standard model”. When she asked Kane about what he would do if his prediction of a run 2 gluino didn’t work out, he said that he would “wonder what one could change in the model”, gave no indication he would under any circumstances give up on string theory. This (and the book edits I documented) are not the way science is supposed to work.

In the Arkani-Hamed interview, he makes very clear that Kane does not count as what he considers one of the “good people” in the subject.

51. **Anindya**
June 13, 2018

John Baez,

In a sense, the use of “theoretical physicists” in these articles is on track because in the layperson’s eye, theoretical physics IS associated with “The Quest for the Theory of Everything”. Maybe we have Stephen Hawking to thank for that.

An analogy might be how “investment bankers” have been broadly implicated in the great financial crash of 2008.
But actually the big bulk of that was specifically traders in credit derivatives of various sorts. Those doing mergers and acquisitions – which is a huge part of investment banking – were not really part of this.

52. **Anindya**
June 13, 2018

And just an addendum – I also think “particle physics” is a misleading term because it makes it seem like these people are trying to find new particles.
Whereas in reality, these are all attempts to find the fundamental laws of nature.

Maybe “fundamental physics” ? Or would that raise the hackles of all the “non fundamental” physicists? 🙂

53. **Another Anon**
June 14, 2018

“Maybe “fundamental physics” ? Or would that raise the hackles of all the “non fundamental” physicists”

Yes, I also think “fundamental physics” is the correct term.
Also, I’m never sure why people use human-centred “high-energy physics” instead of “particle physics”, which seems the better term. You’re only calling it “high energy” because you’re low-energy – it’s not high energy to a quark.

54. **TS**
June 14, 2018

While haggling about correct terms, let me point out that theoretical particle physics is progressing by leaps and bounds, and it’s incorporating maths in novel ways all the time. It’s just not the kind of fundamental physics that tries to find new theories that’s making these steps, it’s the kind that takes existing theories and applies them to actual experiments. Calculating differential cross-section for multi-body final states beyond leading order has become more or less routine with automated tools available, two-loop calculations are becoming more and more common, and are rapidly becoming a routine thing for two-body final states, hadronization is studied quantitatively etc.

It’s perhaps not the way we expected our knowledge would progress and not the way we expected particle physics and mathematics to cotinue their interplay, but these are significant advances.

55. **F. G.**
June 14, 2018

Anindya, Another Anon

How about “foundational physics” ? Or “foundations of physics”, the term actually used by Sabine Hossenfelder to describe her day job in a recent interview published at Edge :

” The field that I mostly work in is the foundations of physics, which is, roughly speaking, composed of cosmology, the foundations of quantum mechanics, high-energy particle physics, and quantum gravity.”

https://www.edge.org/conversation/sabine_hossenfelder-looking-in-the-wrong-places

56. **OT**
June 15, 2018

OT: http://www.fields.utoronto.ca/video-archive/event/2388

57. **Peter Woit**
June 15, 2018

F.G.,
“Foundations” isn’t so good, since it often means work on quantum mechanics, which, whatever its problems, they’re different than the ones cause by overly speculative and experimentally inaccessible work. Chad Orzel writes about this here

https://www.forbes.com/sites/chadorzel/2018/06/14/the-difference-between-particle-physics-and-quantum-foundations

I do try and properly attribute the problem to “HEP theory” or “fundamental physics”, but there is no good word that accurately picks out the part of the subject in trouble, and whatever phrase one picks, it’s likely to be too long for many headline writers.

58. **DrDave**
June 16, 2018

Science magazine did it more or less the right way. They removed the quote, and then they put a note in saying they removed the note. There’s other problems with the review, but it is worth noting that many of our larger newspapers do not adhere to basic editorial standards. It’s quite common nowadays to rewrite an article with a changelog. At least someone higher up the foodchain said, hey, this is not OK and fixed it.

59. **Lars**
June 24, 2018

A slightly different perspective from Nobel laureate Murray Gell-Mann

https://www.ted.com/talks/murray_gell_mann_on_beauty_and_truth_in_physics

60. **Geoffrey Dixon**
July 1, 2018

Perhaps the reason our theoretical pyramid is unstable is that it’s been built upside down. No amount of nth order QFT applied to ideas resting on too small a foundation will succeed – and if it does, then Nature is an ass. Mathematics contains enormously generative, and beautiful, structures that resonate perfectly with the universe we actually observe.

I look forward to reading Sabine’s book. It will eventually sit on my shelf next to Peter’s and Smolin’s books. Then I’ll go rewatch the Red Sox job offer scene in the film Moneyball, and stop thinking about things that will not change in my lifetime.

The Multiverse Falsified

June 19, 2018
Categories: Multiverse Mania

The July 1 issue of the Monthly Notices of the Royal Astronomy Society includes an article evaluating the standard multiverse prediction of the cosmological constant, with result:

> The predicted (median) value is 50–60 times larger than the observed value. The probability of observing a value as small as our cosmological constant Λ_0 is ~2 per cent.

If your theory only makes one prediction, and that prediction is off by a factor of 50, that's the end of it for your theory. I'm very glad that this has now been sorted out, the multiverse hypothesis has been falsified, and theorists who have been working on this can move on to more fruitful topics.

Update: As David Appell realized, the last sentence here was sarcasm (or maybe black humor). Those promoting the multiverse are doing Fake Physics™, not Physics. This is ideology, not science, and there is no chance that they will stop referring to the "successful multiverse prediction of the CC", no matter what analysis shows a seriously incorrect prediction.

As Blake Stacey points out, this paper was on the arXiv back in January (see here), and has just been ignored by multiverse proponents. Part of doing Fake Physics™ is ignoring any information that contradicts what you want to believe. Another commenter points to this 2014 argument from Sesh Nadathur, which similarly as far as I know has just been ignored.

After appearing on the arXiv in January, this latest work was promoted by press release from Durham University back in May, which led to lots of media stories (e.g. here). For some reason, the press release didn't really explain that this work falsifies the usual claim that the value of the CC is evidence of a multiverse. Instead, the work was promoted as showing that the multiverse is "more hospitable to life" than thought, which sounds good I guess, but seems like a bizarre way to explain the significance of this work.

For various sensible explanations of what is really going on here, see Jim Baggott, Philip Ball, and Sabine Hossenfelder. I've often repeated my own version of how to see there's a problem with trying to explain the CC this way. There is no actual multiverse theory, so proponents assume a "flat measure over the anthropically allowed region" and then calculate. This is exactly the same input as my theory of the CC, which is that I have no idea what is going on, so any value is equally likely. The bottom line from the latest work on this is that, even if for some reason you believe you can get a sensible "prediction" this way, the prediction comes out wrong.

Comments

1. **David Appell**
   June 19, 2018

   I'm guessing your last sentence is sarcasm.

2. **Blake Stacey**
   June 19, 2018

   The arXiv version is arXiv:1801.08781 [astro-ph.CO].

3. **Anon**
   June 19, 2018

   Hi Peter

   Do you know if the situation has gotten worse for the multiverse since this 2014 blog post that also appears to have similar arguments like you quoted:
   http://blankonthemap.blogspot.com/2014/02/does-multiverse-explain-cosmological.html

4. **Daniel**
   June 20, 2018

   There's a new article at Aeon which I think you might be interested:
   https://aeon.co/essays/has-the-quest-for-top-down-unification-of-physics-stalled

5. **Mitchell Porter**
   June 20, 2018

   It remains true that Weinberg did successfully predict the size of dark energy, to within an order of magnitude, in his paper "The cosmological constant problem". It's there at the top of page 8: "we would expect a vacuum energy density rho_V ~ (10-100) rho_M0". (The actual rho_darkenergy is something like 14 times rho_baryon.)

   You can take the position that he was right just by accident, and that the size of dark energy is determined by something else, or determined by nothing at all. Of course that's possible.

   But it also remains very possible that he was basically right! The subsequent literature on the anthropic prediction of the cosmological constant just reruns his argument in a variety of concrete scenarios. This paper tells us how the argument fares, for a particular cosmological model, under a variety of auxiliary assumptions. Sometimes it works and sometimes it doesn't.

   If I was an anthropic theorist, the conclusion I would draw from this paper, is that I should go back and study closely the details of Weinberg's original argument, because it worked perfectly when he did it, the first time around.

6. **Anonyrat**
   June 20, 2018

   Mitchell Porter:

   The Sesh Nadatur comment referenced above is that the Weinberg argument doesn't work with the current earliest time of galaxy formation, i.e., current observations — the maximum redshift at which a galaxy has been found. It is not just that Weinberg's argument fails with the current cosmological model, it is that he got lucky with what the state of observational knowledge was when he wrote that paper.

7. **Peter Woit**
June 20, 2018

Mitchell Porter,
I'll leave this to the experts, but my impression was that the claim of the new paper is that Weinberg's calculation is wrong (doesn't agree with what we now know). In any case, the two possibilities seem to be:

1. Weinberg's calculation was wrong. Multiverse proponents will just ignore this and continue to tell people to believe in a multiverse because of Weinberg's "successful" calculation.

2. Weinberg's "prediction" was fine, but you can get pretty much any "prediction" you want by changing your uncheckable assumptions about how the multiverse works. Again, calling this sort of "prediction" a success would be misleading.

8. **Peter Woit**
June 20, 2018

Daniel,

That is interesting, will likely soon discuss it in a posting.

9. **Lee Smolin**
June 20, 2018

Dear Peter,

I've been making this point in books and reviews for many years. (For example hep-th/0407213). A further point is that if you expand the list of constants to be determined by Weinberg's anthropic argument, from just the CC to the CC and Q, which measures the size of the primordial density fluctuations, the discrepancy between the predicted most likely value and the observed values grows to many orders of magnitude (See Figure 2 of astro-ph/0401424.)

Thanks,

Lee

10. **Peter Woit**
June 20, 2018

Thanks for pointing that out Lee. Yes, that's another serious problem with the oft-repeated "Weinberg's successful prediction" claim.

11. **Low Math, Meekly Interacting**
June 21, 2018

The undead quality of the "Weinberg predicted the CC" argument ranks very high on the list of habits of multiverse proponents that puzzle me. As it was apparently quite rigorously dispatched years ago, what gives? It's not like SUSY, where an excess of flexibility makes falsification impossible. The multiverse itself has much the same virtue for those who wish to move the goalposts forever. So why bother with a very narrowly-focused prediction that HAS been falsified? Has Weinberg himself ever said anything publicly about the current state of his prediction?

12. **Peter Woit**
June 21, 2018

LMMI,
To his credit, I don't think Weinberg himself really does this, and his comments on the multiverse always seem to be of the order of "maybe yes, maybe no", not ever "my successful prediction shows it's true". For many others, especially multiverse ideologues, being able to invoke Weinberg's name and the words "successful prediction" is just irresistible and there is nothing that will ever get them to give this up.

13. **Uncommon Sense**
June 21, 2018

Does any of this relate directly or analogously to Everettesque/ Many-Worlds QM?

14. **Peter Woit**
June 21, 2018

Uncommon Sense,
No, not in the slightest. They are two completely different things.

15. **Blake Stacey**
June 23, 2018

Just to update the argument from 2014:

The highest-redshift galaxy so far observed, as best I can tell from the literature (it's been [a long time] since I worked in astrophysics) is at $z = 11.1$. See [Oesch *et al.* (2016)]. Following Weinberg's argument, this gives a bound on the vacuum energy density of about 5800 times the present cosmic mass density. This is three orders of magnitude larger than the observed value, a ratio well into the regime where Weinberg himself says the cosmological constant would be "so small that even the anthropic principle could not explain its smallness".

Knockin' On Heaven's Door

June 21, 2018
Categories: Uncategorized

I've been thinking about what to write about this essay by Ben Allanach, which gives his take on the current state of HEP theory. Allanach is a specialist on the phenomenology of SUSY models, but here he announces that he's basically giving up on these models:

> The trouble is that it's not clear when to give up on supersymmetry. True, as more data arrives from the LHC with no sign of superpartners, the heavier they would have to be if they existed, and the less they solve the problem. But there's no obvious point at which one says 'ah well, that's it – now supersymmetry is dead'. Everyone has their own biased point in time at which they stop believing, at least enough to stop working on it. The LHC is still going and there's still plenty of effort going into the search for superpartners, but many of my colleagues have moved on to new research topics. For the first 20 years of my scientific career, I cut my teeth on figuring out ways to detect the presence of superpartners in LHC data. Now I've all but dropped it as a research topic.

While most HEP physicists still try and end their talks with some sort of optimistic expression of hope that things will change soon, I was struck by a recent talk by John Iliopoulos, which was more somber and more realistic:

> No coherent picture emerges
>
> We were expecting new physics to be around the corner…..
> But we see no corner
>
> The easy answer: We need more data
>
> Two problems: (i) We do not know what kind of data
> (ii) They will not come for quite a long time
>
> A rather frustrating problem!

and he ends with

> The Future of Particle Physics will undoubtedly be bright, but
>
> I will not learn the answer

While thinking about this I happened to look at an old posting of mine, a review of Lisa Randall's Knocking on Heaven's Door written back in 2011. There I wrote

> One odd thing about the book is the title, which for Randall carries a positive meaning that she acknowledges doesn't correspond to the very dark one of the Bob Dylan song from the soundtrack of the Sam Peckinpah film. It's a beautiful song, but one not about finding truth, but about getting shot in the gut and facing death, hopefully not relevant to particle physics in the LHC era:
>
> *Mama, put my guns in the ground*
> *I can't shoot them anymore.*
> *That long black cloud is comin' down*
> *I feel like I'm knockin' on heaven's door.*

It does seem like much of the last 40 years of HEP theory is now "knockin' on heaven's door", deeply wounded by negative results from the LHC. What this means for the future is still up in the air: what story about what has happened will become the conventional wisdom?

Comments

1. **Jon Awbrey**
   June 21, 2018

   In Whiskey Veritas …

   https://www.heavensdoor.com/

2. **Amitabh Lath**
   June 22, 2018

   Peter,
   it occurs to me that all this "woe be upon us for LHC keeps confirming the Standard Model" is mostly prevalent among theorists of a certain age.

   We have occasional LHC-lunches where experimentalists and theorists get together (consider yourself invited if in the Garden State!). The last one was a few days ago and I was struck by the excitement among the young theory grad students and postdocs about the upcoming High-Luminosity LHC run. They were going on about clever new techniques to find hidden new physics, proposing new detectors for long-lived dark photons deep in the LHC tunnels, etc etc.

   The attitude was "nature is being coy, now it gets interesting". What a contrast from the essay by Allanach.

3. **Moyses**
   June 22, 2018

   Sterile neutrinos, g-muon (4 sigma) and B-mesons anomalies, no-MOND (i.e. "absence of a fundamental acceleration", 10 sigma)…
   Surely You're Joking, Mr. Woit 😉

4. **Roger**
   June 22, 2018

   Moyses

Regarding anomalies associated with sterile neutrinos, g-2 muon + B-mesons, the community maintains a healthy scepticism. The number of sigma of a deviation is irrelevant if there is an underestimated or overlooked uncertainty. At any given time over the past few decades one could have done as you've done and suggest new physics on the horizon due to the anomalies of the day – leptoquarks at HERA in 1997 was my favourite. Its difficult to see that the situation today is qualitatively different.

5. **Peter Woit**
June 22, 2018

Amit,

I'm glad to hear that the part of HEP theory directly interacting with the experiments is healthy. The part that I think has been mortally wounded is the part that has had the assumption that the path to further unification will be based on a SUSY extension of the SM. This isn't a bad thing in and of itself, depends on what emerges from the wreckage (Dijkgraaf's recent "There are no laws of physics" piece unfortunately shows that something worse is possible).

6. **Amitabh Lath**
June 22, 2018

Peter, the part of HEP close to experiment is the actual heart of HEP. These people are never as loud or visible as those who become media darlings peddling mysticism be it "landscape/multiverse" or in a previous era "The Tao of Physics".

7. **John Dixon**
June 23, 2018

Ben Allanach and many others have been using the ideas of the MSSM, with some very sophisticated computer programs, to see if there is evidence for SUSY at the LHC. But I claim that the MSSM is quite flawed. The MSSM is explained on the Particle Data Group website, for example. The idea is that there is an invisible undetectable sector that breaks susy. Then that communicates to the visible sector somehow, also in an unknown way. The practical consequence is that we break susy explicitly with many parameters. I would argue that this is not a theory at all. It is certainly not susy.
It is more like the emperor's new clothes. If the MSSM turned out to be right, that would be horrible. What does susy really predict, if anything? I would say that nobody knows. Spontaneous breaking of SUSY does not work in a simple way, or even in a complicated way. Does it really predict superpartners, if it is done correctly? To know that, we need a new way to interpret SUSY. What the experiments, and Ben Allanach's work, and the work of many others, prove, is that the MSSM is not right. But it is not at all clear that the MSSM is the right way to do SUSY. Lets hope it is not, because it is very ugly and non-predictive, whereas SUSY is a simple consequence of quantum mechanics and relativity.

8. **Peter Woit**
June 23, 2018

John Dixon,

I should make it clear that when I'm referring to problems with SUSY here, it is to the standard SUSY extension of the SM (the MSSM) and more complicated variants. As you note, such theories have always been problematic, since you need to break SUSY and this introduces a great deal of ugliness, extra parameters and lack of predictivity. This was well-known (I wrote about it in my book) and has always been a good reason to not believe in the MSSM. The interesting news here is that a theorist like Allanach, who has devoted much of his career to these things despite these problems, has finally given up.

There are lots of other possible "supersymmetries" (I don't believe though that "SUSY is a simple consequence of quantum mechanics and relativity"), but that's another topic, not relevant to what Allanach is writing about.

9. **John Dixon**
June 23, 2018

Hi Peter. Thanks for your prompt reply. I see that we agree about the mssm. What I meant was that susy is the square root of a translation in quantum field theory. I am not talking about other supersymmetries, whatever they are. The general impression is that spontaneous breaking of susy is the only game for Susy. That is not so. We can do susy without expecting superpartners by transforming the superpartners to sources in the supergravity master equation. My point is that those ideas might conceivably bring Ben Allanach back to Susy because the experimental consequences could be very different.

10. **Urs Schreiber**
June 24, 2018

> susy is the square root of a translation in quantum field theory

There is a noteworthy subtlety with this statement, if it is meant as a mathematical motivation for supersymmetry: Not all super-Lie algebra extensions of the Poincaré Lie algebra are standard susy super Lie algebras — a counter-example is described here.

Hence, to mathematically motivate standard susy super Lie algebras among all "square roots of translations", one needs to invoke another principle.

Here is one: Start with the superpoint, regarded as an abelian super Lie algebra. Then iteratively a) double the fermionic part and b) pass to the maximal invariant central super Lie algebra extension; and repeat.

This process turns out to discover the correct supersymmetry super Lie algebras, avoiding the spurious ones above. Interestingly, it singles out more: It discovers the susy super Lie algebras precisely in dimensions 2+1, 3+1, 5+1, 9+1 and 10+1. See the theorem here.

11. **paddy**
June 24, 2018

Nothing substantial to add other than
(A) waiting for LMMI's questions/comments and PW's answer far better than my typical "huh?"
(B) Urs Screiber's comments are self serving. Or maybe I just do not understand him?

12. **Peter Woit**
June 24, 2018

paddy,
Urs's comment are all right, they just, as often, ignore the main point. There's a huge amount one can say about what happens when you decide to extend the Poincare algebra to some bigger superalgebra. In many of these extensions you have odd generators that when squared give usual translation generators of Poincare.

The fundamental problem occurs when these new generators commute with the energy-momentum generators. If the new generators act trivially on the vacuum, then particle states get taken to particle states with the same energy-momentum relation, i.e. same mass. These "superpartners" will have opposite statistics, but problem is we know of no same mass fermion-boson pairs. So, to avoid this, you have to assume the new generator acts non-trivially on the vacuum, i.e. that the supersymmetry it generates is spontaneously broken. This is where you get into trouble: while there are lots of ways of devising spontaneously broken models, they all tend to either not look like the real world, or require introducing lots of new parameters, then choosing them so as to make the superpartners invisible.

While proponents like to go on about how beautiful this new symmetry is, and how wonderful it is that it relates bosons and fermions, the basic problem is simple: the supersymmetry doesn't relate any bosons and fermions we know about. Put differently, restricting to the state space of known particles, the supersymmetry acts trivially. This may be a beautiful new algebra, but as far as known physics is concerned, it is the trivial algebra.

13. **Amitabh Lath**
June 25, 2018

Okay so SUSY didn't pan out. As people have pointed out, it is only minimal SUSY, made even more minimal by experimentalists who scan in just two or three parameters (yeah, guilty). But fine, maybe nature doesn't do SUSY.

This is no reason to run away from the LHC. Not only is it the only game in town (modulo neutrinos) for the next 20 years, but it has only collected about 2% of its total data haul.

To do a systematic search for deviations from Standard Model we still need guidance from theorists about what new physics signatures could look like. We also need to understand the SM much better than we do now.

Theorists, we need you! Now more than ever.

14. **Low Math, Meekly Interacting**
June 25, 2018

Hi, Amitabh,

If I understand correctly, the question of whether or not nature does SUSY is one that shall remain open forever, since it cannot be disproven. What has been all-but-disproven is the notion that SUSY makes the mass of the Higgs "natural". So while the LHC has only begun to collect data, that first 2% has had an oversized impact. Furthermore, there doesn't appear to be a (known) compelling reason to expect new physics is lurking in the remaining 98% to be collected. The one principle widely touted as a good reason to build the LHC (beyond finding the Higgs) has been demolished. At what scale should experimentalists expect new physics? What guiding principle will take the place of (and hopefully prove more reliable than) "naturalness"?

Experimentalists, I rather think theorists are in most dire need of your contributions at this time. Given the past 40 years, what more reliable guidance would you expect for where to look than "wherever you can"?

15. **Peter Woit**
June 25, 2018

LMMI,
There are different sorts of HEP theorists, many of whom are not of much use to LHC experimentalists, only able to offer rather obvious advice (e.g. "measure everything you can about the Higgs, would be great if you could measure Higgs self-interactions…"). Those expert on certain kinds of specific BSM extensions (e.g. SUSY models) that haven't been found and now look implausible are going to have to retool to try and find some more useful role to play. I think that's what Allanach is trying to do.

I take Allanach's essay as saying that he wants to concentrate on looking at possible experimental anomalies, then using theory to characterize the possible things that could be causing such an anomaly, consistent with all known negative results. One can then play a useful role for experimentalists by telling them things like: "a particle X could explain that B-meson anomaly, and if that's the cause you should also see something unexpected in this other channel". This is definitely useful, but how well it's going to work depends on what anomalies one has to work with. A much harder thing to do is to start with no anomalies and ask "what are the possible consistent SM extensions that would produce something visible at the LHC", indicating where to look for anomalies. Many people have been trying to do this for a long time, coming up with something new here is very hard.

Then again, there are also lots of theorists still needed to understand exactly what the SM predicts in various cases, so that experimental results can be accurately compared with the SM to look for anomalies.

16. **Amitabh Lath**
June 25, 2018

LMMI, as Peter points out there are many flavors of HEP theorists, some of whom work with experimentalists. We need more.

Yes, we are in an era where experiment is going to drive theory, but theory drives experiment. UA1, UA2 at the SPS were built for W and Z discovery. Electroweak theory played a key role in detector design, and electron and muon identification progressed because theory said that was how to bag these vector bosons. CDF and D0 at the Tevatron were optimized for the top search, and a whole lot of R&D went to make the radiation-hard silicon vertex detectors because theory said tagging the b-quark in top decay was critical. The LHC detectors have superb photon resolution because higgs theory said the diphoton decay, although rare, was among the cleanest.

Right now the mix of technologies that will go into the HL-LHC detectors is being decided. Necessary compromises are being made about hardware, bandwidth, physics reach.
And for the first time in many decades we do not have one single driving vision for new physics search. A theorist with a compelling new model (even if half baked) could make all the difference at this moment.

17. **GoletaBeach**
June 27, 2018

A little surprised to read by Allanach:

"Instead, many of us have switched from the old top-down style of working to a more humble, bottom-up approach."

"Old, top-down style"? Just about every significant particle physics discovery has arisen from the bottom-up approach… from Ellis and Wooster discovering the neutrino at least through to the bottom quark discovery by Lederman at all at FNAL. That finally infinitesimal "top down" extrapolations predicted the W, Z, Higgs, and top quark is a small crust on top of a deep core of bottoms-up work.

Actually SUSY was all the rage after UA1 "discovered it" in the 1980's. There is a whole book about that discovery, and its disproof… "Nobel Dreams" by Gary Taubes. Looking for SUSY and not finding it has been around a lot longer than Allanach.

When to give up? Everyone knew in the 1990's that SUSY's lowest WIMP-nucleon cross section was about 10^(-55) cm^2, yet people get worried now if nothing is seen at 10^(-48) cm^2.

This Week's Hype

June 27, 2018
Categories: Fake Physics, Multiverse Mania, Strings 2XXX, Swampland

New Scientist today has a feature article headlined

> **How to think about… The multiverse**
>
> The idea of an infinite multitude of universes is forced on us by physics.

It starts off quoting Sean Carroll:

> "One of the most common misconceptions is that the multiverse is a hypothesis," says Sean Carroll at the California Institute of Technology in Pasadena. In fact, it is forced upon us."It is a prediction of theories we have good reason to think are correct."

The problem with this claim is that it's simply not true. There is no model that "we have good reason to think correct" that predicts a multiverse of universes with different physics (i.e. fundamental constants). I've written about this many times, see for instance Theorists Without a Theory. In case you were thinking of interpreting Carroll's claim in some other way, the article goes on to invoke Alexander Vilenkin:

> "The so-called constants of nature, like the mass of the electron or Newton's gravitational constant, will have different values in different bubbles,"

To be fair to New Scientist, I haven't read beyond the headline and first few paragraphs of this article, since the rest is behind a paywall. Maybe the later part of the article (which most people can't read) explains what is wrong with the Carroll and Vilenkin claims.

For the latest on the models supposed to give us different physics in different parts of the multiverse, you might want to take a look at this new paper on the arXiv, and Cumrun Vafa's talk about it this week at Strings 2018. The paper and talk conjecture that the supposed metastable dS solutions of the string landscape don't really exist (they are in the "swampland" of things that aren't solutions of string theory). If this is true and you want to save string theory, as Vafa explains, you need to invoke different sorts of supposed solutions to string theory, with the CC replaced by a "quintessence" mechanism.

At the end of the talk (1:01), Eva Silverstein tries to explain what is wrong with Vafa's arguments. He responds "I'm not saying you're wrong, you might be right, this might be also be right". This shows clearly the fundamental problem of the subject: there is no well-defined theory here, just a bunch of conjectures about what one might be, with no way to tell whether Vafa or Silverstein is right, and no way to extract well-defined predictions from the mass of possible conjectures.

Update: Thanks to those who sent me a copy of the full New Scientist article. It's short, and the part behind the paywall is even worse than the part publicly available, just adding to the confusion by invoking "many-worlds", with more from Sean Carroll. Our doppelgangers doing exciting stuff in other universes make an appearance, although Carroll expresses a lack of interest in what they're up to.

Update: The Strings 2018 talks and videos are available here or at this Youtube channel, and 4 gravitons has a blog posting. As usual with Strings 20XX conferences, very little about actual string theory there. For an overview of the state of the field, you might want to watch this video of the 50 years of string theory session, moderated by David Gross. Dan Harlow was the only speaker raising the elephant in the room question: "is string theory still a useful candidate as a theory of HEP physics?" (and also asked whether they should finally rename the conference series). Gross read off submitted questions for the panel, most of which were asking about the elephant in the room. The panelists each found a different way of avoiding dealing with the question. Other questions asked about the hot "is there a dS string vacuum?" issue, responses were "maybe yes, maybe no", with no indication of any way to resolve this.

Comments

1. **Jim Baggott**
   June 27, 2018

   I give an alternative view in a post earlier this week to Prospect magazine's science blog. https://www.prospectmagazine.co.uk/science-and-technology/the-problem-with-multiverse-theories-theyre-just-not-science
   What I really don't understand is what theorists like Sean Carroll think they're gaining by holding these views.

2. **Peter Woit**
   June 27, 2018

   Thanks Jim,

   I think it's very clear what Carroll is gaining here: he has written a whole book (From Eternity to Here) claiming to explain a problem about time via different physics in different universes. Most people think this is not science, so he needs to convince them that it is science, that the multiverse is "forced on us by science".

   What harder to understand is why he never responds to arguments explaining the problems with this, just ignores them, only responding to straw man arguments. Even harder to understand is why journalists put his claims front and center, ignoring the problems with them. As I said, I haven't seen the rest of this piece, but typically pieces like this are almost entirely quotes from multiverse proponents, with little if anything from people pointing out the problems.

3. **Another Anon**
   June 27, 2018

   "One of the most common misconceptions is that the multiverse is a hypothesis," says Sean Carroll

   Well, I can actually agree with Sean Carroll for once! As the start of the Wikipedia page on "Hypothesis" says: "For a hypothesis to be a scientific hypothesis, the scientific method requires that one can test it."

   So, I'm totally in agreement with Sean Carroll – it fails the criteria for a hypothesis. Dunno what you'd call it, really. An "idea"? A "bit of fun"? Don't care, really.

4. **Armin**
   June 27, 2018

   "What [is] harder to understand is why he never responds to arguments explaining the problems with this, just ignores them, only responding to straw man arguments."

Isn't cognitive bias the obvious explanation?

If we already believe something to be true, we tend to evaluate new input, including critical questions, in terms of how it confirms our beliefs, and if we believe it not to be true, we look for ways the new input helps us avoid having to believe it.

I think this can give rise to situations in which people talk past each other.

5. **Peter Woit**
June 27, 2018

Armin,
What is odd here is not that "cognitive bias" is a factor, but that well-known arguments are completely ignored. This is not the way academia is supposed to work (you aren't supposed to get to ignore arguments you don't like) and it's not the way journalism is supposed to work (journalists are supposed to be aware of and present arguments of both sides).

6. **David Roberts**
June 28, 2018

I find it curious the Agrawal-Obied-Steinhardt-Vafa paper you link to starts with three "naturality" assumptions, there called Criterion 1, Criterion 2 and an unlabelled definition, that certain quantities (Delta, c, and alpha, resp.) are all ~O(1). Also, the paper doesn't exactly shout it from the rooftops that certain things that have been held to be true about string theory just don't seem to work. My conception of how sciences work is that if you make a real advance that seems to turn all accepted wisdom/best working current theory on its head, then it's seen as a big thing, you make a lot of noise, and (sadly) put out a press release trumpeting how everything we knew was wrong, and we have a much better idea now…

7. **SteveB**
June 28, 2018

I subscribe to New Scientist and the whole mutliverse article is only a few paragraphs long. It is one of 13 articles in the issue that start with "How to think about …" Besides MV there is "Time", "Genes", "Consciousness", "Particles".

The MV article brings up eternal inflation, string theory as our "best stab at a ToE" and "Then there is the quantum multiverse, predicted by the "many worlds" interpretation of quantum theory"

No contrary opinions are given…

8. **Peter Woit**
June 28, 2018

David Roberts,

The problem is that the new "swampland" paper doesn't show that well-advertised claims about string theory don't work, it just says "maybe they don't", without a lot of justification. Not enough for a press release…

SteveB,
Thanks. Some people did send me a copy, I added some comments to the posting.

9. **David Roberts**
June 28, 2018

@Peter,

if they aren't saying that kind of thing, then maybe they should be. Urs Schreiber is now highlighting that there are serious problems turning up up for the KKLT approach, see eg the quotes in comments on [this G+ post](), taken from Danielsson-Van Riet, arXiv:1804.01120. Here's a sample:

> p. 26: "It is not unthinkable that dS space is simply a space that cannot exist quantum mechanically. […] This has been claimed before in several papers that study QFT in curved space [161–168] and if those papers are correct, then doing a proper string computation should reveal that dS vacua cannot exist."
>
> p. 30: "We therefore think that the most natural assumption, at this point in time, is that string theory conspires against the existence of dS space."

Note the citations of work in pure QFT, which is surely the kind of thing that should make anyone sit up and take notice, regardless on one's position on string theory (I have no skin in the game: ST is just interesting mathematics to me).

10. **Peter Woit**
June 28, 2018

David Roberts,

The problem with this whole subject is that basically what people are doing is arguing about whether or not solutions exist to a set of unknown equations, based on a very complex web of guesses and information about limits of these equations. You then end up with the kind of thing you quote, arguments invoking "not unthinkable" and "the most natural assumption". All that's clear here is that the technical issues are very complex and murky. My best guess from all I've seen is that there's no reason to believe there is a well-defined problem here, all evidence is with Vafa's "maybe you're right, maybe I'm right".

I also don't think it matters who is right: either infinitely complicated metastable dS string vacua exist, making string theory useless for predictions, or they don't, and you don't have solutions to work with, which is equally useless.

I've wasted far too much time following these discussion for nearly 15 years, see
https://www.math.columbia.edu/~woit/wordpress/?p=9968
and put up with a lot of personal insults from string theorists who take the attitude that my refusal to go farther down this rabbit hole proves that I'm not competent and shouldn't be listened to. For some recent examples, see for instance
http://www.math.columbia.edu/~woit/wordpress/?p=7432&cpage=1#comment-216526

This illustrates well what has always been a major problem with string theory research: the constructions are so complex and ill-understood that they provide lots of intricate and challenging technical problems to work on. People get deeply involved in this and a whole field of research is generated, with practitioners tending to take the attitude that anyone dismissing work on these constructions should not be listened to because of their lack of expertise.

We’re now 30-some years and tens of thousands of papers down the road from the first attempts to construct a “string vacuum” that looked like the real world. You don’t need to be an expert in all the intricacies of every attempted construction to notice that the output of all this activity is no explanation of anything about the real world, just a bunch of excuses for not being able to get such a thing.

11. **Peter Woit**
June 28, 2018

After writing that last comment, took a look at tonight’s new hep-th papers on the arXiv. At least two of them are about the “are there dS solutions” question. This is now officially the latest fad of a faddish subject.

12. **Anonyrat**
June 29, 2018

Lifted from the comments on your blog, April 15, 2004

serenus zeitblom says:
April 15, 2004 at 4:07 am
Peter, you should look at http://arxiv.org/abs/hep-th/0404075
It’s clear that the claim that string
theory can produce “anything” just isn’t
true. In particular, it is very far from
clear that it can produce a deSitter
background. The KKLT proposal essentially
suggested that you could get dS out of string
theory if you were willing to tolerate extremely
contrived models involving known gadgets such
as fluxes. Now it seems likely that even
this is not true.

13. **Urs Schreiber**
June 29, 2018

Or in the words of Danielsson-VanRiet 18 p. 4:

> Paradoxically the critics of string theory and the proponents of the string landscape all agree on one thing: the landscape exists and we more or less know its properties. But what if they are wrong?

14. **Peter Woit**
June 29, 2018

Urs,
I remember reading that and noticing that it’s completely untrue. This critic of string theory has never agreed that “the landscape exists and we more or less know its properties.” I have no idea whether the unknown equations of string theory have the claimed metastable dS solutions, suspect it’s not even a well-defined question. What I’ve always argued is that it doesn’t matter: whether you have too many solutions or none, you can’t say anything about the real world.

15. **Peter Woit**
June 29, 2018

Anonyrat,
Yes, nothing ever changes on this issue, as for many others about string theory.

All,
I added some links and comment re: the Strings 2018 conference that just ended.

16. **Robert Karl Stonjek**
June 30, 2018

We’ve started a discussion on this topic at my ‘Cosmology and Astrophysics News’ Facebook group (that’s GROUP, not PAGE :). I’m taking the same case as the author of this page, others are not…

All welcome to join in, oh and I post Cosmo-astro news every day as do some others…never a dull moment 😀

https://www.facebook.com/groups/AstroCosmoNews/permalink/1845018605803704/

17. **ned**
July 2, 2018

Just wondering that you wonder about Sean Carroll, et al.

We’re living now with alternative facts – so alternative science is the logical result.

Strings 2018 Panel Discussion

June 30, 2018
Categories: Strings 2XXX, Swampland

A [panel discussion at the Strings 2018 conference](#) ended by addressing audience questions, and this seemed to me to give some insight into where string theory is now. I noticed that the Youtube video comes with an auto-generated transcript, so decided it was worth the time to clean that up a bit and post it here.

David Gross: The plurality of questions had to do with the connections of string theory to real-world experiment. Some of these were raised by the panelists already. Let me just give you a sense of those questions, and see if you would like to briefly address them:

> How long can string theory survive without experimental verification? At what point does it become mathematics?
>
> Can string theory survive as a theory of the physical world if de Sitter space can't be accommodated and supersymmetry ruled out?
>
> In the next fifty years do you expect any physically relevant result (i.e. worthy of a Nobel Prize) to come out of string theory?
>
> Has string theory given up on particle physics?
>
> When and where do you foresee a real confirmation of string theory in particle physics or cosmology (other than the existence of gravitons)?
>
> In what area of physics do you expect the first observation of string theory?
>
> Do you think string theory is closer to phenomenology than 50 years ago?
>
> What could be a prediction of stringy models that can be verified in the lab and it cannot be an effective field theory model that gives you the same prediction?
>
> How much of the string community has given up on the goal of connecting string theory to the standard model and observational cosmology?
>
> Susy isn't observed, what should we do? Is it okay to believe that string theory is still the theory of everything?

David Gross: So, this is symptomatic of this community. I imagine the younger members of the community perhaps a bit worrisome.

Eva Silverstein: My answer is yes. (laughter) To the very last question you posed.

Juan Maldacena: I think the main virtue of string theory is to be a consistent theory of quantum gravity and maybe we shouldn't be… I mean of course it would be wonderful to have a comparison to experiment but it's a complicated theory and it might take many years until we understand how to compare it to experiment. I think an important thing is to understand the theory, understand basic things like the singularity because it might be that we'll understand that experimental connection by understanding the Big Bang singularity and some predictions from there, or something in this direction.

John Schwarz: One of those questions had to do with "is string theory just mathematics?" I've heard this question many times and I find it puzzling that someone would consider mathematics to be a pejorative term. Where would physics be without it?

David Gross: I don't think that was the question. The question was "if string theory without experimental verification goes on and on is it indistinguishable". There was nothing pejorative.

John Schwarz: I'm sure the person in this audience who raised that question didn't mean it that way but I've heard it used by others in that way.

Dan Harlow: I just want to give a sociological data point so I mean I won't repeat what I said in my talk but it's a true fact that you know every every month or two I am contacted by an experimentalist. Usually this or that atomic physics experimentalist who is looking for things to do in their lab and somehow thinks that talking to me will help. I'm not sure if it will or not, but I think that there's this fantasy that you find on the blogs that string theory is something that exists independent of the rest of physics and I think really nothing could be further from the truth. I mean I feel we're really part of physics I talk to physicists all the time and not just string theorists. (laughter)

David Gross: There were a few other versions of this question that perhaps reflected the anxiety of some of you here, which had to do with funding and and having to defend yourselves in your academic departments and universities and that I think is a real issue. I think Daniel addressed that but let me give you some other points of advice to defend string theory or what we call the activities of this crowd, with respect the funding agencies or department chairmen. String theory was attacked bitterly in the eighties for being not even science and but now it's truly impossible to make that argument. It is continuously connected to the standard model after all through our dualities and the standard model is certainly part of nature and verified experimentally. So string theory and field theory are not distinguishable and certainly not the standard model. String theory has given us many insights into the standard model, condensed matter theory, information theory, mathematics etc. It is easy to defend it intellectually, aside from the fact that it's addressing these deep conceptual problems of unifying quantum gravity with the other interactions, or just understanding gravity. So you should feel no shame in defending this field and arguing for both funding and positions.

Gabriele Veneziano: One mistake we made in the early days of the atomic theory was to think that the hadrons were elementary and to which we had to find a string idea. One of the big assumptions of the new 80s interpretation is that the particles we consider elementary today are indeed so. Maybe the fact that we so far failed to find a model is that we try to find a string theory for the wrong thing.

David Gross: There are also many questions about de Sitter space:

> The existence of dS solutions appears to be controversial. What are the technical obstacles to resolving that controversy?
>
> Is de Sitter space in the swampland? Can we get de Sitter in string theory? If yes why haven't we succeeded? If no, why not?

David Gross: Igor (sorry Igor) asked an even broader question:

> Is there a stable non-supersymmetric compactification of superstring theory whose existence has been established using known controlled approximations? Should have Poincaré or dS or AdS symmetry corresponding to the two or more non-compact dimensions?

David Gross: This is an interesting topic where there's clearly controversy. I've been unable to find a strong statement on the negative side. Juan has offered to defend it or at least he has been put forward to defend the existence of dS solutions.

Juan Maldacena: There are constructions that I think are reasonable, there are scenarios for how the solutions should work. They involve complexity in an essential way in the sense that you have to invoke complexity to find this fine-tuning that was talking about, and they are reasonable so if you're going to say that they don't exist you also should argue with comparably strong arguments. Also no one guarantees us that the physical theory will have very simple solutions, so if you want to solve for the oxygen atom you can decide whether it will exist or not. Even in QCD if you try to decide what's the last stable nucleus you will not beable to predict it probably from pure theory.

I'll say one more thing, but this is more speculative. So our understanding of the vacuum in string theory many times relies on having an asymptotically simple situation: asymptotically flat space, asymptotically AdS, and if we ask "where does AdS arises from?" then "Oh well it's a brane embedded in a bigger space and so on". So we have this kind of "turtles upon turtles upon turtles" picture of the theory, so everything is defined by a bigger simpler asymptotic space. But where did this asymptotic space come from? de Sitter is different, de Sitter is a bit like a sphere, so it has no edge or anything and we need to think now "We're theorists, how to describe that?" So maybe we'll understand another framework where we understand the fact that it has no boundary is more crucial and essential and we'll see that those equations might have a different nature than the types of equations we think about.

David Gross: That's a defense of KKLT. Trivedi isn't here, I was going to ask him to defend it. There are many people who are confused as to whether this is a crisis for string theory or not, and Hiroshi volunteered to give some criticism of these compactifications.

Hirosi Ooguri: I was asked to say something about it probably because I posted a paper with Cumrun earlier this week about this which Cumrun talked about. So, the last 20 years or so, especially after dark energy was identified, there have been enormous attempts to construct de Sitter space and other accelerated universes, with various degrees of rigor, and this is really a very important part of string theory research. So, many of the things we do is to look at a set of these constructions and try to deduce lessons from these data. This is like experimental science where we are given a set of this data and then try to understand it. But of course depending on how much rigor you ask, how much sort of control you would like, the set of data you look at can be different. Just like experimenters look at the different sigmas and then select reliable data. I should say in the case of string theory it's particularly difficult because string theory doesn't have parameters so all the low-energy parameters are the vacuum expectation value of some field. So if you successfully stabilize all the scalar fields then by definition these are numbers and not controlled. This is in contrast to the case of say, QED, where we have the fine structure constant which you can dial in a given theory, in our world a given number you can dial, so we can trust it, so the situation seems to be different. When the KKLT compactification first appeared I was hoping that maybe since there are so many ends around the flux that you can actually find a series of models where you have control over that, which we have not seen. I think this is difficult and so you can draw different lessons from this and I think it's very important to sort of develop tools to make more sort of finer predictions out of this existing situation.

Comments

1. **Dave Miller**
   June 30, 2018

   A specific question: Dave Gross stated, String theory has given us many insights into the standard model, condensed matter theory, information theory, mathematics etc."

   I happen to have a longstanding professional interest in information theory: this is an important and well-established (and very useful) area of mathematics and electrical engineering (I myself am co-inventor on some patents that involve applications of information theory).

   Does anyone know what Gross meant when he referred to "information theory" in the context of string theory?

   This is not a snarky question: I would genuinely like to know.

2. **Peter Woit**
   June 30, 2018

   Dave Miller,
   You should pay attention to the context of those quotes. Gross is explaining a propagandistic line of argument based on AdS/CFT (it relates QFT to string theory, so any application of QFT is an application of string theory!) fit to use with your department chair or NSF program officer.

   For the actual science he is referring to, over the next couple weeks look for lectures at the IAS, see here:
   https://pitp.ias.edu/

   My guess is that those lectures might contain some interesting applications of QFT to quantum information theory, with string theory playing no role at all.

3. **Hirosi Ooguri**
   June 30, 2018

   As the person who is quoted at the end of this transcript, I would like to clarify that, when I said at the end of my remarks that it is important to develop tools, I had in mind theoretical tools to understand non-supersymmetric physics, both in quantum field theory and string theory. In this regard, I am very encouraged by progress in non-supersymmetric quantum field theory, with highly nontrivial extensions of what we had learned about dualities in supersymmetric cases. I highly recommend the review talk by Francesco Benini followed by the plenary talks by Nathan Seiberg and Zohar Komargodski, captured in the Youtube video here: https://www.youtube.com/watch?v=x3gzgqtpZWc&t=1710s

4. **Sabine**
   July 1, 2018

   It's interesting that the narrative that string theory would be useful to calculate properties of the quark gluon plasma has entirely disappeared.

5. **Anton**
   July 1, 2018

   Peter,

   it seems that none of the questions asked at he beginning were answered. Is that by mistake or by design?

6. **MBN**
   July 1, 2018

   I find the second part of the first question strange? Lack of experimental/observational confirmation isn't what makes it mathematics. In my opinion string theory needs a lot more before it can be called mathematics.

7. **Peter Hickman**
   July 1, 2018

   The status of string theory (ST) can be summed as per slide at 14:10

‘To conclude, Lets wish to ST 50 more years of prosperity and to produce by 2068 a string theoretic standard model’

At 13:32, ‘still a challenge to reproduce a “low energy” viable model of elementary particles and their interactions’

ST as the best candidate for a BSM or explanation for the SM is not evident at all.

8. **Dave Miller**
July 1, 2018

Well, Peter, hope springs eternal: I had genuinely hoped that there was some connection to information theory!

I’ll be interested to see if there is indeed a connection as you suggest between QFT and quantum information theory (as a physicist who has worked with classical information theory, I have an obvious interest here). Offhand, I do not see how there can be such a connection: the work I have seen in quantum information theory relies on very basic quantum theory — basically the bra-ket notation, superposition, tensor products based on two-state systems, and the Born rule, and not much else.

But, I hope to be happily surprised with a real connection to QFT. If you see anything come up on the subject, I hope you’ll mention it in a post.

Thanks.

Dave

9. **a**
July 1, 2018

The problem is masturbation, not mathematics. When propaganda was at the top, young researches had to skip physics to jump on string theory and be hired, and the field degenerated in mathematics not relevant for understanding physics. If now there are good physicists trying to understand if strings can give non-supersymmetric compactifications, I would be interested in knowing their results.

10. **Peter Woit**
July 1, 2018

Anton,
I think the unwillingness to publicly answer those questions speaks volumes, and has a lot to do with the context Gross brings up: admitting failure would carry a serious cost.
It would be very interesting to hear what the speakers honestly think about those questions. My guess is that it’s something along the lines of “our current understanding of what string theory is can’t give the hoped for unification of the SM and quantum gravity. Maybe some new ideas coming out of string theory research will someday change this, who knows.”

11. **Mark**
July 1, 2018

@Dave Miller: String theory (AdS/CFT) provides examples of chaotic quantum systems, where many hugely complicated-looking things are calculable (using string theory methods). In quantum information physics delocalization of quantum information under time evolution is an interesting question, and stringy methods have led to a lot of progress there, maybe culminating in:
https://arxiv.org/abs/1503.01409
If you look at who’s citing this paper (or close relatives), among many others, you’ll find the very established quantum information theorists Kitaev (https://arxiv.org/abs/1710.03363), Brandao (https://arxiv.org/abs/1705.07597), Qi (http://arxiv.org/abs/arXiv:1511.04021), and Hayden (http://arxiv.org/abs/arXiv:1602.06271).

12. **tulpoeid**
July 1, 2018

“It is continuously connected to the standard model after all through our dualities and the standard model is certainly part of nature and verified experimentally.”

To paraphrase Borges, Let this person’s funding be cut now, even if my own is too.

13. **Peter Woit**
July 1, 2018

tulpoeid,
I’ve heard Gross make this statement before, always found it bizarre. Unless I’ve missed something, there is no known string theory dual to the SM and the claim that the SM is “continuously connected to string theory through dualities” is at best misleading in the extreme. I suppose the SM and string theory can both be embedded in “the space of all theories”, and continuously connected therein.

Gross’s response to the questions from the audience does make things very clear: this is not about a level-headed evaluation of the science, it’s about protecting grant money and positions. I hadn’t heard the “string theory contributes to quantum information theory” claim before, but I think we’re going to be hearing it a lot more from now on. Quantum information theory is the hot topic for grant funding and thus for university administrators, so claiming to be part of that field is the most promising way to get continued funding.

14. **Sebastian Thaler**
July 1, 2018

David Gross: “String theory was attacked bitterly in the eighties for being not even science … but now it’s truly impossible to make that argument.” Heh.

15. **Peter Woit**
July 1, 2018

Sebastian Thaler,

I think Gross was referring to this, from 1986
https://arxiv.org/abs/physics/9403001
by Ginsparg and Glashow. It was in some ways remarkably prescient:
“Contemplation of superstrings may evolve into an activity as remote from conventional particle physics as particle physics is from chemistry”
where you could replace “chemistry” by “quantum information theory”.

In other ways though, they had no clue, since they thought there was no way this could go on in a physics department, that such research would have
“to be conducted at schools of divinity by future equivalents of medieval theologians”

Also unfortunately all too prescient is the next sentence:
“For the first time since the Dark Ages, we can see how our noble search may end, with faith replacing science once again.”

16. **Mark M**
July 3, 2018

Sabine (and Peter),

While it seems the formal string theory crowd has largely moved on from applications related to the quark gluon plasma, some physicists working on string-inspired approaches (AdS/CFT) to understanding hot and dense QCD are taking it very serious, having their own workshop this week: http://igfae.usc.es/~holoquark2018/

17. **Richard Gaylord**
July 10, 2018

hi:

do you know how the transcript was generated. can this be done with any you tube video? it is VERY useful since some of us fall asleep during lectures and you can read a transcript at one’s own pace and you can go back and forth .between different sections.

18. **Peter Woit**
July 10, 2018

Richard Gaylord,

Clicking on the “three dots” below and to the right of a youtube video generally brings up a transcript. No idea how these are generated, must be automated speech recognition of some kind.

Various Links

July 5, 2018
Categories: Uncategorized

A random assortment of possibly interesting links:

- New videos from the IHES include new interviews, and talks from the recent Ofer Gabber conference. If you want to know more about prisms than what you can get from the video here, I hear rumors that Bhargav Bhatt will be our Eilenberg lecturer this fall at Columbia.
- Another talk at the Gabber conference was the latest on local (quantum) geometric Langlands from Gaitsgory. Also on this topic, there's a July 4 preprint from Arakawa and Frenkel (advertised here).
- John Horgan has an interview with Jim Holt, headlined Why Does Jim Holt Exist?. For reviews of Holt's two most recent books, see here and here.
- For two interesting blog posts from HEP experimenters about news from their field, see Jonathan Link at SciAm on neutrinos, and Tommaso Dorigo on the Higgs self-coupling. Dorigo's posting is the more technical one, explaining a new CMS result bounding the Higgs self-coupling.

  The LHC experiments still seem to be a long ways away from actually measuring the Higgs self-coupling, but may be able to do so in future higher-luminosity stages of the LHC program. The Higgs remains the least understood part of the SM, responsible for most of the undetermined parameters of the theory. Any measurement of its self-interactions is an important goal.

  While it often seems that experimental results relevant to going beyond the Standard Model are inaccessible due to the necessity of higher energies, these two blog posts point to important open questions about the SM that are hard to study not because of fundamental limits on collision energies, but because of small event rates and high backgrounds.

- The question recently came up here (see this posting) of how good the SUSY GUT coupling constant unification prediction is. At a recent summer school lecture, Ben Allanach says the prediction is off by 5 sigma, i.e. that if you try and predict the strong coupling at the Z mass this way, you get 0.129 +/- 0.002, whereas the measured value is 0.119 +/- 0.002. Someone should tell Frank Wilczek…

Update: For more on the dS vacua issue, see this blog posting by Ulf Danielsson. Danielsson refers to criticism of the landscape at my blog, but ignores the point I've often made that string theory is in just as much trouble if the advertised dS vacua don't exist, since then it has no known way to connect to the real world and its positive CC. Somehow he sees this as a virtue, that "These are exciting times", which I find mystifying.

He also claims that, all is well, since:

> By studying the mathematics of the theory we will find out what it predicts, and by comparing with observations we will learn whether it has anything to do with reality.

But the underlying problem here is that the mathematics of the theory is unknown. Read the responses above from two experts to the question of why this issue has not been conclusively determined. Neither of them present any possibility of a conclusive determination. For both of them, this is about weighing the plausibility of various conjectures about possible solutions to unknown conjectural equations. At this point I seriously doubt it is possible for this to be resolved one way or the other.

Update: ICHEP 2018 is winding up, talks available here. There was one plenary summary talk on "Formal Theory Developments", by Tadashi Takayanagi. It begins by promoting string theory, then goes on to address the problem of how it relates to fundamental physics with:

> However, please do not ask me questions like:
> How to derive Standard Model from string theory?
> Why do we live in 4 dimensions?
> How to realize de-Sitter spacetimes in a well-reliable way?
>
> String theory is still too infant to give complete answers to them.

The "complete" is intentionally misleading, since string theory now gives no answers at all to these questions. While celebrating the 50th anniversary of string this year, it seems that "string theory is too new an idea to evaluate" is the standard answer to anyone who points out its failures.

Comments

1. **Anony**
   July 5, 2018

   You're misreading the slides. Those numbers are for non-susy GUTs.

2. **Deane**
   July 5, 2018

   I like the face Ofer made when the Langlands Program is mentioned, as well as what he says about Gaitsgory.

3. **Peter Woit**
   July 5, 2018

   Anony,
   No I'm not, you are. Interesting that the current SUSY error in gauge unification is of a size that you mistake for the non-SUSY error.

   The numbers I gave are from page 40, which explicitly says "in the MSSM". If you look at the plot on page 58 you see the 5 sigma size problem, and that the numbers were generated by the SOFTSUSY package, described here https://softsusy.hepforge.org/
   From the plot the calculation uses benchmark point 40.2.5, from this paper
   https://arxiv.org/abs/1109.3859
   The numbers for this benchmark point look around current SUSY limits.

   It looks like anyone who wants to can run the software and generate their own numbers. I'm somewhat curious whether how sensitive the numbers are to assumed superpartner masses. Has the way the LHC has pushed up superpartner mass bounds also pushed up the error in the SUSY gauge unification prediction?

4. **Sabine**
July 6, 2018

I'm pretty sure that Wilczek knows this (the problem in general, maybe not the exact numbers) because he points out in the interview that's in my book that gauge coupling unification gets worse the higher the susy breaking scale has to be. And we've meanwhile passed the point where it's not working well any more.

Of course the running might be more difficult, so that it still all fits, see eg this paper. So if you want to believe, you can still believe.

Also, you could simply shrug it off because GUT and SUSY are two separate things, so who says that a supersymmetric extension of the standard model actually must have a gauge coupling unification in one point?

5. **James Wells**
July 6, 2018

The current LHC limits on supersymmetry in no way have the slightest impact on the attractiveness or viability of gauge coupling unification in a supersymmetric context.

All the talk of 5sigma error is decidedly irrelevant, unless you speak of a very special approach called "precision unification", where the high-scale corrections are assumed to be very small for some non-generic reason. Zero GUT threshold corrections is not expected if you have a real GUT gauge group in field theory, as high-scale threshold corrections from required high-mass rep remnants are formally of the same order in QFT as the weak scale corrections.

The whole question is how big do you expect those corrections to be. In SUSY they are, and continue to be, surprisingly tiny corrections required for unification. (Perhaps too small even if you like lots of GUT reps.) In SM, the corrections are huge, but not altogether impossible if there are lots of high dimension GUT states hanging around.

Lack of superpartners at LHC is not qualitatively changing the discussion of gauge coupling unification by experts. The reason is that the dependences are logarithmic rather than, say, quadratic (as in "finetuning" discussions, which have qualitatively changed). One order of magnitude change in mass limits is not much to a logarithm.

A rather complete discussion can be found at https://arxiv.org/abs/1502.01362
where Sebastian Ellis and I put susy and non-susy gauge coupling unification expectations in context, and tried to give a visual picture of the issues.

6. **Peter Woit**
July 6, 2018

James Wells,
Thanks! Perhaps you should be the one to tell Wilczek about this. In Sabine's book he tells her that "the changes of unification start getting worse when susy partners haven't shown up around 2 TeV", giving this as an argument for why the LHC might still see superpartners, just beyond current bounds.

There's a long history of these gauge unification calculations being sold as a strong argument for SUSY, rarely making clear the weaknesses of such an argument.

7. **Chethan Krishnan**
July 7, 2018

Dear James Wells,

"The current LHC limits on supersymmetry in no way have the slightest impact on the attractiveness or viability of gauge coupling unification in a supersymmetric context."

Is the following a fair summary?

Non-SUSY GUTs need high threshold corrections for unification to happen, while SUSY GUTs were generally expected to need only tiny threshold corrections. This is generally presented as a good thing, for reasons which are not very clear to me.

(Of course, if I am not mistaken, Allanach's slide 40 now says even SUSY requires bigger threshold corrections, possibly in light of more precise measurements of low scale couplings).

You seem to be saying that the smallness of the threshold correction is more or less an arbitrary benchmark for the attractiveness of a GUT.

If that is the case, according to you, shouldn't the SM be as good or bad as the MSSM as a candidate for unification? In fact, from your paper that you linked, it looks like one has to (at least for some choices of GUT group) compensate for the threshold corrections s in supersymmetric theories, potentially making them less attractive than SM?

On a general note-

Do you know a reason why people (like Wilczek, or your refs[1-7]) seem to be implying that small threshold corrections in MSSM is a good thing? (Please correct me if I am wrong.)

8. **Urs Schreiber**
July 7, 2018

From [Ellis-Wells 17, p. 23-25]:

> We have discussed how gauge coupling unification generally only improves in a supersymmetric theory, even at very high scales, with respect to the SM.

And before:

> Thus, even the Standard Model up to the high scale is compatible with gauge coupling unification from this perspective, although the corrections becomes quite large in that case, and one has to ask whether nature would rather have large corrections at the GUT scale for a SM GUT or very small corrections for a low-scale SUSY GUT.
>
> […] the introduction of supersymmetry both reduces the needed threshold corrections at the high-scale and increases the GUT scale […]. This latter element is helpful since one generally requires that the GUT scale be above about 10^{15} GeV so that the X,Y GUT gauge bosons do not induce too large dimension-six operators that cause the proton to decay faster than current limits allow.
>
> Thus, exact gauge coupling unification is viable for intermediate values of supersymmetry breaking, which are also compatible with the Higgs boson mass constraint.

> […]
> One important experimental prediction of minimal supersymmetry, even for superpartners at very high scales, is the existence of a relatively light Higgs boson. This has been seen by the LHC. We have shown in this paper that even arbitrary high scales of super-partners allow the light Higgs boson, due to the required matching of the SM effective theory Higgs self-interaction coupling to gauge couplings in the supersymmetric theory.

9. **paddy**
   July 7, 2018

   A question from someone not hep-th since mid 70s. I have seen 3 things used to support SUSY over the years since: (1) naturalness (2) gauge coupling unification at gut scale and (3) providing dark matter WIMP candidates. Are these pillars not crumbling one by one?

10. **Anon**
    July 8, 2018

    In GUTS and SUSY GUTS there is also tuning/finetuning needed now to get the fermion masses (including light neutrino masses) and mixing right. It would be interesting to have a table of tuning needed for popular SU(5), flipped SU(5) and SO(10) models from fermion mass and unification of couplings point of view. This is apart from usual finetuning needed for gauge hierarchy problem.

    Paddy — naturalness was the main reason to expect SUSY at weak scale. Now it is clear that the MSSM needs to be fine tuned to least a 1% level for the gauge hierarchy problem. And so that pillar has crumbled. A 100 TeV collider could potentially probe finetuning to 0.01% level.

    GUTs and WIMPs were more secondary reasons (though very remarkable reasons), and there is no evidence for them either. But there is no evidence for non-SUSY GUTs or WIMPS or for dark matter particle (other than due to gravitational effects) either.

11. **GoletaBeach**
    July 9, 2018

    "…. these two blog posts point to important open questions about the SM that are hard to study not because of fundamental limits on collision energies, but because of small event rates and high backgrounds."

    A significant experimental thrust since, well, "re"-started in the late 1970's with the IMB proton decay effort has been devoted to pushing the envelope on small event rates and backgrounds.

    Naturally there was even earlier work by Reines in South Africa, a group of Indian physicists working at the Kolar Gold fields, a Cornell/U Utah group that worked in mines in the Wasatch, and, most notably, Ray Davis and his group in South Dakota. There was even a low background group in the Manhattan Project, because initial samples of most of the interesting transactinides were at the nanogram level, meaning, decay rates were low.

    In the US, though, the low background effort has struggled… follow-ons to Ray Davis have had a rocky road with the NSF rejecting, and DUNE/Fermilab/DOE finally coming to the rescue; IMB was shut down and effort transferred to Kamioka in Japan, a large 1980's effort was shunted to Gran Sasso in Italy (Macro and later Borexino), and SNO, conceived at UC Irvine, took place in Canada. Daya Bay in China had a huge US contingent as well. And of course at the South Pole the US has supported a grand high-energy neutrino experiment, IceCube.

    I doubt that the science program of any nation on earth has been as internationally-oriented as that of the US in low-background physics. Comparatively, the US-based low background program has grown weaker, and the consequence is much of the expertise at the nitty-gritty of low background experimentation lives in Germany, Italy, Japan, and Canada. At this point, frankly, the US abilities aren't as good.

    I'm not arguing for US nationalism, but when it comes to conceiving and implementing new low background experiments, the US can feel like the old Soviet Union in accelerator-based physics: lots of ideas, but modest ability to execute. Perhaps the US low background program is a bit out of balance, and needs a tuneup.

12. **Amitabh Lath**
    July 9, 2018

    The majority of LHC limits on SUSY assume R-parity conservation (RPC). Models with RPC give you large missing momentum in the detector, which gives CMS and ATLAS great sensitivity.

    SUSY limits for R-parity violating (RPV) signatures are much softer. There is no honking big missing momentum signature to trigger on. So maybe it's R-parity conservation that is in trouble, not all of SUSY?

    Full disclosure: I've worked on searches for RPV SUSY.

13. **Anon**
    July 10, 2018

    Amitabh,

    Generic R-parity violating terms at the TeV scale in MSSM will lead to quick proton decay. So generally RPV SUSY shd be at a much higher scale such as around the GUT scale.

    Only a restricted number of RPV terms that do not lead to proton decay are permitted at TeV/multi TeV scale. Ruling these out is pbly also important from hierarchy problem point of view.

14. **Amitabh Lath**
    July 11, 2018

    Anon: yes proton decay does put limits on RPV but you need both leptonic and baryonic RPV to make the proton decay. Nature could make just one of those non-zero, and make a large number of LHC searches blind to SUSY signal. Also, the limit on proton lifetime is 10^34 yrs so both leptonic and baryonic RPV could be nonzero but small enough to evade that limit.

    Look, my point is that experimentalists put limits on specific models, with many caveats (requirements on leptons, heavy flavors, jets, missing momenta…). This then somehow goes from "they have ruled out some very basic minimal SUSY models" to "ruled out MSSM" which quickly morphs into "killed SUSY".

    We know our blind spots too well to endorse a statement like that. Maybe some mickey-mouse version of SUSY is dead, and maybe SUSY as a source of Dark Matter is in jeopardy, but overall I would say LHC has done a thorough preliminary survey for the easiest signatures and is working on the next level of complexity.

15. **Paul**
July 11, 2018

Peter,

I have to say, I love your blog. You do a great job keeping us informed and up-to-date. I apologize for this off-topic comment. No need to have this posted. I see you've been to Paris many times. I am planning to visit the city and would love to visit sites in Paris that have a physics theme. I believe there's a Marie Curie museum, as well as (I think) a museum dedicated to Becquerel. What other places should I visit that have a physics theme or otherwise have some historical significance to physics?

Also, have you been to other European cities of historical significance to physics? I'm sure there are many, as Europe was at the center of science for so many years. I was thinking Berlin, Germany, and Bern, Switzerland because of Einstein. Geneva, for obvious reasons, but I'm not sure what others.

Again, sorry for the off-topic comment. You can delete the comment, but if you could shoot me a brief email whenever you have time, that would be great!

16. **Low Math, Meekly Interacting**
July 11, 2018

"Killed SUSY" is of course too expansive because it's impossible. That's the problem, right? What's dead is the idea that SUSY solves what was perceived to be a mystery in need of a solution. Now we know the Higgs mass is "unnatural". The "WIMP miracle" looks to be a hoax. There may not be a GUT scale for all anyone really knows, so trumpeting the idea that couplings unify (when squinting at a log-log plot) may be an attractive fantasy. What motivates the effort to falsify SUSY moving forward? As pointed out on this blog many, many times, the fact that there are literally an infinite number of models that have not been ruled out yet is not in itself a reason to go about crossing them off a comprehensive list, one by one. However, some very well promoted ideas have been thoroughly debunked. In fact, the volume hype that has been deflated stretching back literally decades is itself quite staggering. Experimentalists have done a fantastic job, and you deserve far, far more lionization in the popular science press than you're getting. You've hopefully saved a lot of people from further wasting their careers. I wouldn't be so forgiving of those who are still openly tempting people to eschew all reasonable hope of empirical support in their lifetimes.

17. **Peter Woit**
July 11, 2018

Paul,
That's pretty off-topic, except for the fact that next week I'm leaving for a vacation trip that will start with a couple days in Paris (and then head off for the far, far North).

Others may have better suggestions, but as a kid I was fascinated by the Palais de la Decouverte science museum. As an adult, an interesting museum more devoted to engineering than science is the Musée des Arts et Métiers, with a Foucault pendulum there and at the Pantheon
https://en.wikipedia.org/wiki/Conservatoire_national_des_arts_et_m%C3%A9tiers

The small Curie museum is part of a university complex worth visiting. I vaguely remember something about Becquerel's old lab in or next to the Jardin des Plantes.

Sorry, but never been to Bern, and during the couple short visits I've made to Berlin, there were all sorts of things to see that seemed of more interest than tracking down what's left of where Einstein would have been.

18. **Peter Woit**
July 11, 2018

LMMI,
I think the best motivation for continued SUSY searches is just that they provide well-studied examples of things to look for that are not already ruled out. Up to experimentalists to use their judgment for whether these searches are worth the effort, or other ideas are more promising. Would be great if theorists could provide better ideas of what is promising, that's very difficult right now.

19. **Anon**
July 11, 2018

Thanks Amitabh for your note. I guess like experimentalists' blind spots there are also the theorists' cover ups…. what I mean is if you supersymmetrize the standard model, the most general model that emerges is R parity violating and leads to proton decay. So it is already ruled out in most of the parameter space (unless some couplings are very small which theorists don't like). To save the situation R-parity conservation is introduced and it is not a happy situation as it is an additional symmetry. But miraculously this provides a WIMP dark matter candidate — in other words R-parity conservation can be motivated from Dark matter point of view rather than from saving SUSY from proton decay issues. So theorists are happy. But now experimentalists have ruled out R-parity conserving SUSY at TeV scale. SO now some R-parity violating can be considered, though not all terms together. But we lose the dark matter — so we are really saving SUSY at this point. And that is not really something that is theoretically pretty.

In any case LHC has enough energies to also rule out R-parity violating MSSM as a solution to the hierarchy problem right. I guess that is what you are working on from experimental end and that is much needed to do.

There are some models such as the minimal suppersymmetric left-right model, that automatically conserve R-parity, and where it has been shown that R-parity must be necessarily spontaneously broken with the sneutrino picking up a vev.

20. **CWJ**
July 11, 2018

I just got from Paris and second the Musée des Arts et Métiers. Somehow I missed it on previous visits, I don't know how. The top floor in particular is scientific instruments.

Another lovely museum with scientific instruments is the Museo Galileo in Florence, not far from the Uffizi. Excellent displays. Also includes Galileo's finger in a jar. The rest of him is in the Basilica di Santa Croce across the city.

21. **lun**
July 12, 2018

Regarding the ICHEP2018 talk, the speaker (Takayanagi) claims the Ryu-Takayanagi formula was proven by Lewkowicz and Maldacena in 2013, presumably referring to
https://arxiv.org/abs/1304.4926
Is this really counted as a "proof"? It reads (and is claimed by the authors) as a plausibility argument based on a worked example. Or am I misunderstanding it?

22. **Thomas Van Riet**
July 13, 2018

Dear Peter,

You say " the point I've often made that string theory is in just as much trouble if the advertised dS vacua don't exist, since then it has no known way to connect to the real world and its positive CC"

The point made in Danielsson's blog (& my paper with him and the papers of Vafa and friends) is that this is wrong. Cosmological observations do not necessarily imply a positive cc. The bounds on the equation of state parameter allow the dark energy to be time-dependent. Exactly as it would be for quintessence for instance. Secondly, it could already be at the quantum field theory level that de Sitter isometries (ie the constancy of dark energy) is broken. This has been ongoing work of Polyakov, Mottola, Woodard,…and many more since the eighties even. If that is correct then string theory better not give us dS vacua. [There is even the possibility that departures from the FLRW Ansatz for the universe, ie spatial inhomogeneities, take away the need for dark energy to explain observations. But I have no good understanding of that.]

Secondly, you say " But the underlying problem here is that the mathematics of the theory is unknown " and then a bit later you say criticize people that actually point out that lots of the theory still needs to be developed to know things for sure. My point being, that your criticism seems to lack some consistency. Either way string theory always looses for you. If we have a landscape of dS, then string theory is not even wrong, if we do not have a landscape then string theory is wrong. You complain about not having finished the theory but then you critize those that say "give us more time".

Finally, it could be true that string theory is not sufficiently developed yet to think of phenomenology. However there are corners of the theory where we do understand it and we can study aspects. This is what drives string phenomonologists. Now some of us are claiming that those corners might show signs of a conspiracy against dS space. Which, if correct, is certainly a deep insight.

23. **Lee Smolin**
July 13, 2018

Dear Thomas,

Your point is well taken. But, it seems all of us who are curious about how nature unifies gravity and the quantum nonetheless face a very peculiar situation in which there are several ways to get partway to a quantum theory of gravity. Each has its defining results, which give its enthusiasts reason to work on it. And each has its stubborn obstacles, which justify its critics' skepticism. It seems to me if we want to demand consistency and an objective, scientific attitude, we should all put our cards on the table and answer two questions:

-What negative result would cause me to give up my currently favoured approach?

-What positive result within another, possibly new, approach would motivate me to switch my efforts to developing it?

In addition, I would argue that no matter which of the approaches we currently work on, we should all be able to answer questions like: Name five approaches to quantum gravity,, and describe the key results, strong points and weak points of each.

Thanks,

Lee

24. **Peter Woit**
July 13, 2018

Thomas van Riet,

To elaborate a bit on the "point I've often made" about metastable dS vacua: those pushing that program have a proposal for how they are going to get testable predictions. They will study the space of such vacua (nowadays, using "Big Data" techniques), find a way to put a measure on it, and make statistical predictions. I've often argued that this clearly can't work (with a main reason they don't have a theory well-defined enough to know what the space of vacua is, much less what the measure on it is), but that's their proposal for how they are going to turn string theory into legitimate science that makes testable predictions.

You can conjecture that these dS vacua don't exist, so their program falls apart, but then what is your proposal for how you are going to get testable predictions out of string theory? The most straightforward implication of a conjecture that string theory is inconsistent with a positive CC is that it's inconsistent with our well-tested standard cosmological model, so it's wrong. But, you don't like that implication, so you do what theorists always do when their model fails a test: try to evade this failure by making the model more complicated. Sure, you can add "quintessence" fields, then postulate that for unknown reasons they have exactly the properties needed to evade the (very strong bounds) that usually rule them out. This is the opposite of making a prediction, it's just evading making one.

This kind of thing is exactly what philosophers of science describe as a "degenerating research program", it's the way scientific ideas typically fail. While their idea is failing this way, the usual behavior of those trying to evade the failure is to argue "we just don't understand the implications of our idea well enough, maybe someday we will find some new understanding that will turn failure into success". This is what is going on with the idea of string theory as a unified theory.

I don't believe string theorists have any serious answer to Lee Smolin's "What negative result would cause me to give up my currently favoured approach?" question above. You've shown that "no dS vacua in string theory" is a negative result that won't cause you to give up, instead you move to quintessence fields. There are already very significant negative observational results about quintessence, and the very likely continuing negative such results clearly are going to have no effect on getting string theory researchers to give up on string theory unification.

25. **Urs Schreiber**
July 13, 2018

It's not like outside of string cosmology there'd be no handwaving in cosmological model building.

For instance, according to Buchert et al. 15 (CQG+) there are mistaken assumptions in Green-Wald 11, so that it remains open whether the cosmological constant is an artifact of neglecting backreaction of spatial inhomogeneities in the concordance model (as in Buchert 07).

(Of course Danielsson-VanRiet 18 do mention this as one possibility, p. 27.)

26. **Shantanu**
July 14, 2018

Thomas: Just so that I understand, are you saying if future precision observations are consistent with a cosmological constant, then is string theory ruled out?

27. **Thomas Van Riet**
July 14, 2018

@ Lee Smolin:

Thanks for engaging.

-What negative result would cause me to give up my currently favoured approach?

A reason to give up string theory would be when it simply does not pass internal consistencies. Say it would say something wrong and inconsistent about black hole entropy (for instance the coefficients of the log A contribution to the entropy)

For all theories beyond the standard model we can say that whenever a prediction is not verified by experiment we can give it up as a model of the universe. It does not mean one should give it up as a model (I will still study Ising models although they do not describe real magnets). However as with all theories of physics beyond the standard model, and especially ALL approaches on quantum gravity, it seems direct verification requires energy scales we cannot yet access. Nonetheless there could be effects from the highest energy scales manifesting themselves at low energies. This is why hierarchy puzzles are outstanding. They are a failure of decoupling UV physics from IR physics. So lets see what our favorite UV complete theories of gravity and other forces can say about the cc problem. What already strikes me as amazing is that string theory is a framework where I can, in some corners, compute the vacuum energy (agreed, we are still fighting about the details of the computations. But in some corners we certainly agree). For quantum field theorists, that is a dream that cannot really be met.

-What positive result within another, possibly new, approach would motivate me to switch my efforts to developing it?

If the other approaches provide a framework to address questions in QFT that need UV completion. I gave already as example: computing vacuum energies in certain field theories. I can write a whole essay here and I will no do it (maybe I start blogging myself soon). For instance one of the amazing features of the string theory framework is our ability to understand strongly coupled field theories better, whether through holography or some D-brane picture of the field theories. In any case it is clear that those people that call themselves "string theorists" are typically versatile and willing to look into various directions. A clear manifestation of that is for instance the whole "entanglement&geometry" program. As far as I can tell that program is not really string theory. Perhaps "string theory inspired" a bit.

28. **Thomas Van Riet**
July 14, 2018

@ Shantanu,

Since we have not yet settled the debate about the existence of dS vacua, I cannot tell. Just to be clear: I think the vast majority of string phenomenologists thinks the evidence for dS vacua is there. I do not agree, but at the moment I am part of a minority. I guess we just need to work more…

29. **Lee Smolin**
July 14, 2018

Dear Thomas,

Thanks very much. I am sympathetic to much of what you say. For example, I agree that the recent revival of the idea that space emerges from quantum entanglement is highly promising. But are you aware that this idea was raised by Penrose in the 1960's and that it motivated his invention of spin-networks? Or that it has been explored in a number of loop quantum gravity papers? (If you are interested look at recent papers by Etere Livine or papers by Daniele Oriti et al or myself which derive forms of the Ryu- Takayanagi relation within LQG.) Indeed I would suggest that this growing area offers an opportunity for people exploring diverse approaches to learn about other approaches and, perhaps even appreciate commonalities among them.

I also appreciate your answer to my second question: "If the other approaches provide a framework to address questions in QFT that need UV completion" seems a clear criteria. Let me suggest a few papers from different approaches that might then interest you.

-There is a claim that the asymptotic safety approach to QG forces a uv completion of the standard model, which relates its coupling constants and, in particular, determines the value of the top quark mass: https://arxiv.org/abs/1707.01107.

-Computations in several approaches, including asymptotic safety, causal dynamical triangulation and LQG suggest the uv completion is reached through a dimensional reduction in which the uv limit of correlation functions scale in a reduced spacetime dimension.

Thanks,

Lee

30. **tulpoeid**
July 17, 2018

Paul,

just noting that if you find yourself close to Bern (e.g. Geneva) and you're interested in such tourist attractions, Einsteinhaus is definitely worth a small daytrip.

abc News

July 17, 2018
Categories: abc Conjecture

The last couple months I've heard reports from several people claiming that arithmetic geometers Peter Scholze and Jakob Stix had identified a serious problem with Mochizuki's claimed proof of the abc conjecture. These reports indicated that Scholze and Stix had traveled to Kyoto to discuss this with Mochizuki, and that they were writing a manuscript, to appear sometime this summer. It seemed best then to not publicize this here, better to give Mochizuki, Scholze and Stix the time to sort out the mathematics and wait for them to have something to say publicly.

Today though I saw that Ivan Fesenko has put out a document entitled [Remarks on Aspects of Modern Pioneering Mathematical Research]. It refers in footnote 18 to:

> two recent texts by Sh. Mochizuki, 'Report on discussions, held during the period March 15–20, 2018, concerning inter-universal Teichmüller theory (IUTCH)'and 'Comments on the manuscript by Scholze–Stix concerning inter-universal Teichmüller theory (IUTCH)', July 2018

I haven't seen these two texts, or the Scholze-Stix manuscript. What I have heard about them is that Scholze-Stix identify what they see as a specific, serious flaw in the proof, and that Mochizuki denies that this is a problem or that his manuscript needs to be revised. Presumably, after the two sides try and sort this out amongst themselves, at some point we'll see something publicly available describing the details of their disagreement.

Fesenko's document has a lot of unpleasant things to say about those who have written anything at all skeptical concerning Mochizuki's claimed proof, mostly without naming names. He refers to journalists and "US bloggers" as having produced "ignorant absurd articles and posts", presumably has someone other than me in mind since the information posted here about this I believe has been quite accurate and of reasonably high quality. The one negative reference to identified mathematicians is in the text with footnote 18 pointing to Scholze and Stix, which says:

> Several researchers, who could have become potential learners of IUT and then progressed to become experts, declined invitations to participate in the IUT workshops. Some, affected by negative emotions, broke professional rules of conduct and made public their ignorant and sometimes intolerant opinions. Tellingly, the only questions produced were shallow and misplaced and they were communicated only after several years of requests to do so.

Peter Scholze is by far the most talented arithmetic geometer of his generation, a sure thing to receive a Fields Medal at the ICM in a couple weeks. That his questions about Mochizuki's proof were "shallow" seems highly unlikely, to me at least.

Much of Fesenko's article concerns the question of whether contemporary mathematical research is too narrow and unambitious, devoted to minor improvements and producing lots of publications. This is a serious issue, one though where other fields than arithmetic geometry (e.g. fundamental physics) are in a much worse state. Fesenko tries to make the difficulties mathematicians have had with Mochizuki's claims about his IUT research an exemplar of this problem, but this seems to me misguided. There are quite good reasons for why experts have been skeptical about IUT and the supposed abc proof, reasons which will be conclusively vindicated if Scholze and Stix turn out to be right. Ironically, an excellent example of the kind of fundamental breakthrough that Fesenko is asking for is Scholze's own ground-breaking work over the past few years.

Comments

1. **mJ**
   July 17, 2018

   What a wonderful article! Thanks Woit!

2. **anon**
   July 17, 2018

   Fesenko also confirms that Mochizuki's papers were indeed submitted to PRIMS.

   The whole IUT story has plenty of bizarre features. I find Fesenko's extremely aggressive promotion of the theory very strange. I wonder if Mochizuki himself is happy with it…

3. **David Roberts**
   July 17, 2018

   Thank you, Peter. It was very frustrating to have people winking and nudging each other around the internet when hinting at what you discuss, because they were in the extended personal circles of mathematical celebrities and got to hear it in person.

   Hmm, now I see totallydisconnected deleted their discussion of the rumour…

4. **No wish to be harassed**
   July 17, 2018

   Being criticized by Ivan Fesenko for breaching "professional rules of conduct" is the equivalent of being criticized by Trump for "not telling the truth" – it should be worn as a badge of honor. You do have to admire his political mastery to somehow have wrangled the biggest grant ever awarded to a mathematician in the UK to study "IUT," a theory that at this point resembles nothing more the emperor's new clothes. (Explanatory note: grants in the UK in pure math are decided by non-experts who may not even be pure mathematicians.) I can see plenty of reasons for someone in such a position to act in a manner not entirely aligned with the pursuit of truth. This is not a document to be taken seriously, and anyone with basic knowledge of the people involved will see through it immediately.

5. **Peter Woit**
   July 17, 2018

   NWTBH,
   I see no reason to doubt that Fesenko believes what he says and is motivated not by money, but by enthusiasm for the new mathematics that Mochizuki has produced. It would be a good idea for him to take a more charitable view of the behavior of others who, also out of love of new mathematics, have been doing the best they can to try and understand and check the proof.

6. **T**
   July 18, 2018

As an outsider (physicist/mathematician) who has been watching this story with interest, I have to say I can't completely disagree with these statements by Fesenko:

> "Unusually for mathematics, some mathematicians felt appropriate to publicly criticise IUT and its study without having studied it in any serious way.
> Negative online criticism went always in a very vague form without any single concrete mathematical evidence. Sometimes it was hostile."

Among others, this post comes to mind, which was eventually closed to comments with the note "If you don't agree with me feel free to start your own blog."

7. **Anon**
   July 18, 2018

   The is a typo in the article name: Remarks on Aspects of Modern Pioneering Mathematical ResEarch

8. **Also don't wish to be harassed**
   July 18, 2018

   "It seemed best then to not publicize this here, better to give Mochizuki, Scholze and Stix the time to sort out the mathematics and wait for them to have something to say publicly."

   And the production of some document by Fesenko was enough to change your mind about this? Seriously?

9. **Peter Woit**
   July 18, 2018

   Anon,
   Thanks fixed.

   ADWTBH,
   The point is that Fesenko has now chosen to publicize the Scholze-Stix story (in the form of an attack on them). Now that this is public I think it's fair game to blog about, and it's actually a good idea to make public a better explanation of what is going on than just what Fesenko has done.

   T,
   What might not be obvious about that blog posting is that Cathy O'Neil is both a number theorist herself, wife of an expert in arithmetic geometry and friend of many other experts in the field. What she wrote was based on detailed inside knowledge of the process leading experts in the subject were going through trying to understand the proof during the first few months it was available, and the problems they were running into. She waited to write this until it had started to become clear to experts how difficult it was going to be to understand and check this proof in conventional ways. In retrospect several years later, her analysis of the problems with the proof was quite on target.

10. **David Roberts**
    July 18, 2018

    @Also don't wish to be harassed:

    well, Fesenko did just out a manuscript due to Scholze-Stix and the existence of a written-up response by Mochizuki. Given the rumours that 'two prominent and very well-regarded mathematicians have isolated a specific and serious error in Mochizuki's proof of the abc conjecture' were already documented online, one might put together the facts independently of Peter writing anything of what he had heard.

11. **ADWTBH**
    July 18, 2018

    Surely this blog post is going to publicize the authorship of the not-yet-publicly-announced [SS] manuscript massively more than footnote 18 on page 6 of Fesenko's document ever would have. Perhaps what you say about it being "fair game" is true by some standard of journalism (though that same standard of journalism might have required asking [SS] for comment — did you?). But with respect to the collegial standard of not publicly broadcasting mathematical work that the authors have not yet announced, this post falls massively short.

12. **Peter Woit**
    July 18, 2018

    ADWTBH,
    Again, I'm not the one who made public the existence of the Scholze-Stix manuscript. Fesenko did this, together with a characterization of it as raising only "shallow and misplaced" questions about the proof. If you're unhappy about this, you should take it up with the person responsible.

13. **Sometime number-theorist**
    July 18, 2018

    I had not read Cathy O'Neil's blog post before, but it is the exact argument of which I was wondering why there weren't more mathematicians making it. To put it in the simplest possible terms: the burden of proof is on Mochizuki — no matter how often he claims he has proven ABC, or no matter the loftiness of the terms in which he expounds the correct manner to become an "expert" in his proof.

    The problem in the dialogue occurs when one party starts making assumptions that the other party does not accept, and predicating their arguments on these assumptions. A consistent failure to recognize the fact that one makes such assumptions, for me is a clear sign of intellectual dishonesty.

    Now the above statement may point to a reason why mathematicians by and large have been loathe to take a harsh stance on Mochizuki (O'Neil apparently excepted). It is not a nice thing to accuse another of intellectual dishonesty. The insult is hurled about far too much, especially on the internet, and it has a tendency to escalate matters.

    Perhaps the thing with accusations of intellectual dishonesty is not so much that they can't be correct, but that they can't be productive. No one can be expected to take such a qualification in good stride. So then the question becomes: what is the honest and correct way to make the same point? Because if you can't call out this kind of trickery for what it is, perhaps out of a fear to be perceived as a troublemaker or a name-caller, then you are not taking responsibility for the integrity of your field.

    I think this is an important question, although I haven't thought about it for long enough to pretend that I have any solid answers. It might even be one of the great questions of our time: from fake news, to fake physics, and now on to fake mathematics.

14. **Peter Woit**
    July 18, 2018

sometime number theorist,

I don't think "intellectual dishonesty" is a good way to characterize the problem with Mochizuki's proof, or a term that Cathy herself used or would use . I have no doubt Mochizuki was and is sincere in his belief that he both has a proof, and has written it down carefully so that others who apply themselves can follow it and check it. It's not an unusual problem though that an author needs to be told to rewrite a manuscript since in its current form others cannot understand it or check it with some reasonable amount of effort.

The new twist to this story is that evidently Scholze long ago had specific criticisms of the proof, and that these never got answered by Mochizuki or addressed by the referees. So at least one expert could follow the proof well enough to identify a possible problem. If you want to look for a past failure in how mathematics is supposed to be done, it seems to me that this is the place to look. It's great though that things are now working as they should, with the mathematical issues getting appropriately discussed and debated by those expert in them.

15. **Sometime number-theorist**
July 18, 2018

@Peter Woit:

I think it is exceedingly hard for both you or me to say whether Mochizuki is convinced whether he has a proof or not. More generally, if you're defining intellectual honesty in terms of the mental contents of the individual in question, you run into the problem of having a subcategory without a feasible criterion for deciding whether an object belongs to it or not. We need to go by what we can observe as much as possible, I would say, even if we don't want to be called behaviorists.

(Consider this question: can an individual delude themselves? If yes, isn't is possible for an individual to think on a conscious level that they have proven something, while being at a deeper level aware that they haven't, or might not have? And isn't an indication of such a state that such an individual reacts aggressively, evasively, or in any kind of obstructing manner, when the point of the integrity of the proof is pressed?)

I really think that mathematicians who are reluctant to call Mochizuki out on intellectual dishonesty should ask themselves the question at what point (or under what circumstances) they *would* be prepared to do so. This has gone on for more than five years now, and the amount of rhetoric from the IUT camp hasn't let up as of yet, and on top of that, substantial amounts of tax-payer money start to get involved as well.

16. **Peter Woit**
July 18, 2018

Sometime number-theorist,
To be fair to Mochizuki, the over-the-top rhetoric generally hasn't been coming from him. It's quite possible we're going to soon see a resolution of this, as experts will see that either Mochizuki has a convincing answer to whatever problems Scholze-Stix are raising or he doesn't. At that point, people may have different takes on how to characterize a possible refusal by Mochizuki to recognize a problem, with "intellectual dishonesty" one of the possibilities.

17. **Sometime number-theorist**
July 18, 2018

@Peter Woit:

Fair enough. But let me just add, I *do* think that the behaviour of Mochizuki's defenders partly reflects on Mochizuki himself. And if people here are posting anonymously out of a fear of harassment, then I think that that is also a signal that something else than a pursuit of truth might be going on here.

18. **Interested**
July 18, 2018

Fesenko's article mentions some recent papers by Mochizuki in the footnotes:

"See two recent texts by Sh. Mochizuki, 'Report on discussions, held during the period March 15–20, 2018, concerning interuniversal
Teichmüller theory (IUTCH)'and 'Comments on the manuscript by Scholze–Stix concerning inter-universal Teichmüller
theory (IUTCH)', July 2018"

However, I am unable to find these anywhere on Mochizuki's website. Does anyone know whether they are available anywhere?

19. **Peter Woit**
July 18, 2018

Interested,

As far as I know, these and the Scholze-Stix manuscript have not been made publicly available, and the intention was that they would not be made public until each side had a chance to fully address the other side's points. I don't know why Fesenko chose to discuss them publicly.

20. **T**
July 19, 2018

Peter,

FWIW, I'm aware (I've been aware) that C. O'Neil used to be a number theorist, and that she knows some experts. My (humble) disappointment stands.

It appears that Mochizuki has been open to correspondence and technical questions about his work all along. It sometimes seemed (from a distance, to an outsider like me) that some of the tension/friction stemmed from Mochizuki's not having visited various institutes in person to explain his theory. This implicit demand (or, again, perhaps my uninformed, outsider feeling of such) was surprising and disappointing to me.

21. **Mateus Araújo**
July 19, 2018

T,

"It sometimes seemed (from a distance, to an outsider like me) that some of the tension/friction stemmed from Mochizuki's not having visited various institutes in person to explain his theory. This implicit demand (or, again, perhaps my uninformed, outsider feeling of such) was surprising and disappointing to me."

I'm a physicist, not a mathematician, so I might be talking from another side of a cultural divide, but the demand that Mochizuki goes around explaining his theory seems entirely natural to me. Putting a paper on the arXiv is not enough, you definitely need to actively defend it, especially if people are interested in it and cannot understand it.

22. **Peter Woit**
    July 19, 2018

    T/Mateus Araujo,
    No one has ever “demanded”, explicitly or implicitly, that Mochizuki travel to their location. Many have, accurately, pointed out that his refusal to travel has made communication with other experts more difficult.

    In any case, this has nothing to do with the latest situation, where, exactly because of the importance of face-to-face communication, Scholze and Stix traveled to Kyoto to talk to him.

23. **Some observer**
    July 19, 2018

    ” I don’t know why Fesenko chose to discuss them publicly.” Since he cites full titles, I assume he got authors permission, and presumably will also have read the work. Now you can ask why Mochizuki has not released the paper and the answer will be similarly generic: He will wait for the Schulze-Stix one. Since it seems that from Mochizukis POV the Schulze-Stix objection is “mostly harmless” it might well be that the whole thing is a ‘non-starter’ and we will find ourselves in a few months at exactly the same state of affairs we had one year ago.

24. **Peter Woit**
    July 19, 2018

    Some observer,
    I’m having trouble seeing why, whatever he thinks of the Scholze-Stix manuscript, Mochizuki would approve of the idea of having it first characterized publicly as “shallow and misplaced”, before it or his response were made available.

Leaving Town

July 19, 2018
Categories: Uncategorized

About to head out on vacation tonight, back in a couple weeks. On the hot topics in fundamental physics, two items are:

- Concerning the "no dS string vacua" conjecture, a new preprint begins by explaining why the existence or non-existence of such vacua is a question that has not been resolved (and, it seems likely to me, can't be resolved):

  > classical no-go theorems such as indicate that realizing de Sitter vacua in string theory requires quantum and/or stringy ingredients. The fact that corrections to classical 10d low energy supergravity are qualitatively important implies that dS compactifications, in contrast to AdS or Minkowski compactifictions, must live in a regime in which these corrections cannot be made arbitrarily small , hence perturbation theory cannot be made arbitrarily accurate. Moreover the absence of supersymmetry in dS, and perhaps more fundamentally the lack of a complete, nonperturbative formulation of string theory, make it hard to obtain exact results beyond perturbation theory. Thus a completely rigorous, parametrically controlled construction of individual de Sitter vacua in string theory has remained out of reach.

  The paper also explains why if you try and get known physics with a quintessence field rather than a CC, you immediately run into serious problems with coupling to the Standard Model.

- Slides and video of the talks at this year's PiTP summer school on "From Qubits to Spacetime" have started to appear. Once I get back from vacation I'll try and watch some of the talks and hope to figure out how one is supposed to get our spacetime and its physics out of qubits.

Recently people have contacted me suggesting I blog about two physics-related topics likely to lead to vigorous debate. I'd begged off in both cases, since engaging in such a debate or moderating it would be on a short list of things I'd most like to avoid doing. This afternoon though, it struck me that there is an excellent, if cowardly, way to deal with this. I'll mention the two topics briefly here, then shut off comments on the blog and leave town. So, some may find interesting and want to argue elsewhere about:

- One of the PiTP lecturers, Aron Wall, has a blog on physics and theology, called Undivided Looking. Wall's theological views include thinking he has a pretty good idea about how God wants people to behave, in particular he's pretty sure that God doesn't want them having homosexual relations. He wrote extensively about this in a blog entry (now deleted) back in 2015. He'll be soon taking up a faculty position at Cambridge University, and some people are not happy about this, see for example this statement from the Cambridge University Student's Union.
- If you'd like to attend an early universe conference this September, one place you could do so is in the Israeli-occupied West Bank settlement of Ariel, where Ariel University is hosting a workshop on Inflation, Alternatives and Gravitational Waves.

No Comments

Back

August 6, 2018
Categories: Uncategorized

Back now from vacation. On the global warming front, I can report that Northern Norway has gotten rather warm, Svalbard is still pretty cold.

While I was away the big mathematics news was from the ICM. As everyone expected, one of the Fields medalists was Peter Scholze. I was surprised to find a blog post of mine quoted about this in the NY Times, since normally the way this works is that journalists are told who the winners are in advance, and then contact experts in the field (which I'm definitely not one of) for quotes. Some tweets from Davide Castelvecchi at Nature about the unusual embargo rules may provide some explanation:

> The whole situation was surreal from the beginning: the organizers gave reporters advance notice of the winners, but on condition that we would not contact them — even though the winners had already been told long in advance.
>
> They also made no other sources available. In other words, we were supposed to write about these difficult concepts without talking to any experts.
>
> …
>
> Oh and I forgot to say: The email with the names of the winners had no information whatsoever on why they won – in other words, no prize citations.

I suspect one reason for the unusual rules is that the ICM people had decided to concentrate on getting stories out through Quanta magazine, which ran the results here. The stories are very well done, and Quanta magazine is great, but a more usual process involving the rest of the science journalism press would have been a good idea.

One other big piece of news from the ICM was the choice of St. Petersburg over Paris as the site for the 2022 ICM. I was sorry to hear this. Perhaps it's just that I'd rather have an excuse to go to Paris than one to go to St. Petersburg. It does seem to me though that in these worrisome times, when offered the choice between the world's most active opponent of liberal democracy and one of the great remaining healthy liberal democracies, the other choice than the one the IMU made would have been the better one. My understanding is that Russia offered twice as much money, and that many feel that was the deciding factor.

Update: I hadn't realized that the problems with the IMU embargo this year were not new, they were much the same as the problems four years ago with the announcement of the 2014 prizes. See here for discussion of the 2014 story (which, when reading it, I first mistook for a discussion of 2018), and here for a discussion of 2018.

The writer of the new story suggests that "Next time the IMU offers up an embargo agreement, reporters should just refuse" which I'd also semi-jokingly suggested in a comment. Actually, given the history of this, it seems to me that journalists seriously should plan to do this next time, and that sympathetic and well-informed mathematicians should help them find out in advance who the winners are. This would allow journalists to contact experts and do proper reporting, with no reason to wait until the ICM to write their stories.

Update:

- The ICM Youtube channel still doesn't have plenary talks from the ICM posted. Peter Scholze's talk on Period maps in p-adic geometry is available now on a different channel. It's an excellent overview of, not the technology of perfectoid spaces, but some of the results achieved using them.
- The reason there are relatively few comments here about the decision to have the next ICM in Russia is that I've deleted most of them as they come in. Many commenters don't believe Russia is any unusual threat to liberal democracy (or, if it is, that US/European liberal democracy is anything worth saving). Most also disagree with the idea that such a threat should have any effect on what mathematicians do. I agree that in general it's best to keep mathematics and the ICM out of politics. A question to think about though for those who know the history of the 1930s is that of whether there was some point during the rise of Fascism that one would stop thinking it was a good idea to have the ICM in a Fascist capital. We're not yet far along the horrific path of the 1930s, but maybe that just means that all should be thinking about what can be done to keep the world from going down that path again.

  Another frequent comment is "but, by your logic, the US would not be a good place to hold the next ICM!". I fear the answer to that is that yes, Paris would be a much better choice than the US at this point.

Comments

1. **Bill**
   August 6, 2018

   I agree with your St Petersburg vs Paris comment. It seems that the IMU decided some time ago to promote mathematics in countries without a strong mathematical tradition (or a collapsing one), instead of giving an excuse to people like you and me to go on vacation.

2. **Davide Castelvecchi**
   August 6, 2018

   I cannot comment on whether the ICM indeed decided to focus on the Quanta coverage, because I don't know. My multiple emails to the organizers requesting explanations got either evasive responses or no response at all.

   But I don't think there is anything for anyone to be gained by, for instance, making biographical information about the winners available to reporters only after the embargo has expired, as the ICM did. Also, I believe there is nothing to be gained by giving reporters an embargo that expires hours after the announcements have been made via webcast.

   But I suspect that this whole issue is symptomatic of an attitude occasionally seen within the math community. In my professional life as a reporter, I mostly interact with physicists and astronomers, but I also do cover math, and sometimes I do see a difference between the two worlds.

   I feel that some mathematicians see dealing with the press as being subjected to an indignity or an embarrassment; therefore, one should just pretend that the press does not exist (for instance not responding to requests), or perhaps only make exceptions for publications seen as allies. Fifty years ago, that mainly meant Scientific American; now it's Quanta. Note: this is no criticism of either of those publications, for which I have utmost admiration; I have worked at the former and have several friends who work at the latter.

   So, some mathematicians seem to be under the impression that they get to choose who will cover them and their field. But one of the basic facts about a free press in a free world is that the press gets to choose what to cover, not the other way around. This misconception sometimes backfires in a spectacular way: think of Perelman, who thought that somehow you can make a major breakthrough such as solving the Poincaré conjecture and have only experts notice it and discuss it. But the more Perelman sought obscurity, the more morbidly curious people got about him, and the more attention he attracted toward himself — culminating in a New Yorker profile and two biographies.

Fortunately, such extreme cases are rare, and in general, attitudes have been slowly changing. Many mathematicians are very open and helpfully reach out to journalists. And you see more and more mathematicians who are also excellent communicators, and even YouTube sensations (Kelsey Houston-Edwards and Grant Sanderson come to mind, among many others).

3. **Deane**
   August 6, 2018

   Peter, since I posted a public rant on Facebook about how the Times barely managed to run an article about the Fields medalists on the same day they were presented at the ICM, I figure I might as well repost it here:

   My annual (very long) rant: It appears that the IMU is just as bad at PR as the AMS. Until yesterday, they had not provided advance but embargoed notification of the Fields medalists to the right people at the New York Times. It turns out they sent it to the wrong person (someone who writes not about science but about Brazil and who doesn't even work full-time for the Times). So the Times wasn't going to be able to prepare an article to appear as soon as the announcement was made.

   What's the big deal? Well, the Times is mostly in the business of publishing breaking news and, unless it makes a special effort, it's not interested in stale news, which is what the news about the Fields Medalists would be in a few days. We *want* news stories like this to appear in major newspapers like the Times, which means they should be notified as early as possible.

   Luckily, a certain Times science writer told his brother, a mathematician, who in turn texted someone in the IMU leadership, who then finally arranged for the information to be sent to the Times science writer yesterday, which was just enough time to submit an article in time. This is obviously *NOT* the way to do PR for mathematics.

   I looked through the IMU web site and found only pages that discussed how to *submit* news to the IMU and absolutely none about any efforts to disseminate news to the media. That is totally lame. I am fairly certain that all of the other scientific professional societies make an active effort to issue press releases and place stories in the media. Neither the IMU nor the AMS have learned how to do this effectively. It's still luck or pressure by a few people that gets math stories to be reported in the Times.

   I've been complaining about this since 2004, when Chern died. As far as I can tell, the situation has not improved much since then.

   (I do want to say that both John Ball and Ingrid Daubechies, when they were presidents of the IMU *did* notify the Times in advance. Ball even visited the Times newsroom, but that was the year Perelman was awarded the medal. Apparently, this practice has not been properly institutionalized at the IMU and was done this year at best carelessly.)

   I would be grateful, if those of you who are in the leadership of the AMS and IMU to keep pressuring these organizations to institutionalize and professionalize their efforts at media relations. Just having lots of math stories in Quanta is NOT enough. Its readership is too narrow.

4. **Deane**
   August 6, 2018

   Davide,

   The problem is that even the mathematicians who *want* coverage by the press have a poor understanding how the game works and, when things turn out badly, act like it's the press who did something wrong, when in fact the press was just doing its job the way it always does.

   And the professional mathematical societies, such as the AMS and IMU, do not seem to have any capable press relations staff who know how the game works. So they passively put out press releases that they think look interesting and then wonder why nobody pays attention to them.

   When I first realized there was a problem in 2004, I asked Kenneth Chang, who, as you know, is the New York Times science writer who stayed up until 1:20am the night before in order to file the Fields Medal story on time, to compare his interactions with the American Chemical Society, American Physical Society, and the American Mathematical Society. He commented on the first two, but said that he had had no interaction with the AMS at all. I don't think things have improved much since then. That's why I wrote that rant I just reposted here.

5. **Oldster**
   August 7, 2018

   On the ICM conference in St. Petersburg, I can recall memories of working on a project at LANL in the 1970's, and we were encouraged to share data with a fellow project in Novosibirsk. They were ahead of us in that research area anyway, although we also taught them some (preapproved) valuable things as well during a visit to Novosibirsk. And, we hosted another Soviet scientist at LANL for a few days. Such contacts were encouraged both for professional reasons, and for diplomatic reasons as valuable to detente when we still considered the Soviets "foes". Thus to me the discussion now becomes, is holding a conference in St.Petersburg something that might serve a similar useful purpose? And as the Tsar's summer residence and quite close to the rest of Europe, it must have some charm?

6. **anon**
   August 7, 2018

   I agree about political aspects of the ICM host selection, but purely as a vacation destination I find St. Petersburg in August much nicer than Paris in August.

7. **Peter Woit**
   August 7, 2018

   Thanks Davide and Deane,
   Next time around, if the IMU provides the same embargo terms, perhaps publications could consider instead asking around and seeing if they could get the names elsewhere, do proper reporting, and release stories before the ICM…

8. **Davide Castelvecchi**
   August 7, 2018

   Update: Embargo Watch has now covered the issue
   https://embargowatch.wordpress.com/2018/08/07/the-math-prize-embargo-that-didnt-add-up/

9. **Peter Woit**
   August 7, 2018

   Davide,

Thanks! I wasn't aware of the 2014 history of this, reading about it at first I thought I was reading about 2018. Just added an update to the posting with links.

10. **Deane**
    August 7, 2018

    Peter,

    I think your comment is off the mark in two ways. First, reporting on prizes is not investigative reporting, and there's no way the Times is going to rely on unofficial sources for who won. You'll have to clarify what "proper reporting" is. I think you're implicitly assuming that a reporter can tell whether one person is the right person to listen to and another is not. And there's also how much time a science writer has to devote to a breaking news story, even with advance warning. All a writer wants are some good quotes, and frankly I thought Ken managed to do a great job there, getting a couple nice ones from Jordan Ellenberg, especially given the tight deadline he had. Ken had an advantage over Davide, because his brother has introduced him to many mathematicians and he now knows who's likely to give a good quote and who's not. There is also the nontrivial effort to write a few not-too-misleading sentences on why each person got the prize.

    The more important thing is that the math community needs math stories like the Fields Medals to be disseminated a lot more than the Times . I believe that the math community, compared to the other scientific communities, has suffered a lot from its incompetence in media and public relations. The other professional societies have media relations offices who actively try to pitch stories to the media. The AMS does not.

    My personal involvement with this began in 2004 when Chern died. I emailed my brother and told him that the Times *had* to write an obituary. My brother's response was "because you said so, we're doing it". This shocked the hell out of me. It implied that if a mathematician brother of a Times science writer had not insisted on this, the Times would have probably never published one. Note that it is *not* the responsibility of the Times to keep track of deaths of scientists or other science news. It learns of them primarily from official communications from scientific professional societies and universities. So I asked my brother to compare the media relations activities of the American Chemistry Society, American Physical Society, and American Mathematical Society. He commented on the first two, and said, "I've never had any contact with the AMS at all". Things have improved very little since then.

    That 4 obituaries of mathematicians (this for now is the easiest way to place math stories into the Times) appeared last year in the Times was due only to the pressure of a few individual mathematicians (luckily not just me and including at least one Fields medalist). The AMS never sent out anything, and didn't even post the sad news on their own web site for at least a month afterwards (I stopped checking after that).

11. **Peter Woit**
    August 7, 2018

    Deane,

    I should make clear that I wasn't intending in any way to criticize Kenneth Chang's reporting. Considering the restrictions he was working under he did a fine job. The problem is the IMU, and now knowing this is not a new problem, but that they did exactly the same thing in 2014, I think it's up to mathematicians to intervene and make sure this doesn't happen again.

    The problem with the IMU behaving like this is that it is likely to cause journalists to simply not cover the story at all, exacerbating the problems you point out. The way things are going, in the future we'll see excellent articles at Quanta, nothing in the rest of the press. I don't see how that is good for mathematics.

    I'm quite serious that if the IMU doesn't change its policy, mathematicians should consider just going around them to the press, with those who get reliable information about the identity of the winners contacting journalists to let them know. Journalists could then go and cover the story without the embargo restrictions.

    I realize this is not the way such things are normally reported, for very good reasons. In particular, unofficial reports of who the winners are could be wrong, but I think this would quickly become clear once journalists started calling up the supposed winners for comment.

12. **Bill**
    August 7, 2018

    By the way, since no one has mentioned this, Quanta Magazine actually published the article on Alessio Figalli about an hour before the live announcement on Youtube.

13. **August**
    August 7, 2018

    I've heard rumors that Jim Simons provided funding for 15K cheques that Fields medalists received this year, because the IMU ran out of money in their Fields Medal account.

14. **anon**
    August 7, 2018

    A couple of years ago IMU formed an ad-hoc committee to look (among other things) at how the major IMU awards are announced. One option that they considered was the 'Nobel method': announcing the winners in a press conference well before the ICM. I think that would be much better than the current system (the committee recommended keeping the current system).

    With every passing ICM awarding the Fields Medals seems to be turning more and more into something like Breakthrough Prize -type show. I'm starting to agree with critics of the Fields Medal (mathbabe for example) and I wish that IMU de-emphasised the medal a bit.

15. **anon**
    August 7, 2018

    August:

    apparently not Jim Simons. From the IMU webpage:
    "The medals and cash prizes are funded by a trust established by J.C.Fields at the University of Toronto, which has been supplemented periodically, but is still significantly underfunded. The discrepancy in 2018 was made up by the University of Toronto and the Fields Institute."

16. **August**
    August 7, 2018

    Of course, that's what they would say…

17. **Deane**
    August 7, 2018

Quick comment about Quanta supposedly breaking the embargo. What actually happened is that an Italian newspaper broke the embargo and released the news about Figalli. The rule is that once one publication breaks it, then everybody else gets to do it, too. So Quanta published *only* their story on Figalli. It was pretty weird to see a story only about Figalli and not the others. I think the Times also published their article at that point, even though their story was about all 4 medalists.

18. **Deane**
August 7, 2018

Peter,

I agree with you that the IMU (as well as the AMS) have to improve their practices and make it easier for *all* news organizations to publish math stories. So their embargo restrictions and ineptitude during the ICM are harmful.

But I want to repeat that it is NOT the responsibility of the Times to compensate for this, for example, by guessing and calling up people. That's kind of a ridiculous thing to expect a science writer to do.

Since it's all public by now, I can say more specifically what happened. Ken told me, only the day before the ICM opening, about not being able to get a response from the IMU. He did this, I think, mostly out of frustration and told me that I didn't need to do anything. He would just try to write the story afterward the news became public. I still wanted to try. I first looked up who was in the IMU leadership to see who I would feel comfortable contacting. I first texted a member-at-large, who told me that I should contact Ken Ribet instead. I then texted Ribet, who was in Rio and who got the IMU to finally send the names to my brother by around noon. My brother then was able to do the research, finding among other things, your blog post, and talking to various people. It's not always so easy to find people at the last minute like that, but he's good at it. Still, he had to stay up until 1:30am writing the story. That's way beyond the call of duty for a science writer.

I later found out that Ribet himself was on the selection committee and therefore knew who the medalists were. I don't know whether he told my brother directly or got someone else to do so.

19. **Peter Woit**
August 7, 2018

Deane,
Thanks for the further details about what happened. My take on this is that if it weren't for extraordinary efforts by you and your brother, there would have been little or no coverage of this by the Times. Presumably many other publications didn't bother, and that's why if you look around, you find little coverage of this, at least in US media. And there would have been even less if it hadn't been for the medal getting stolen…

Faced with this situation, I'm not suggesting at all that it is up to reporters at the Times or elsewhere to try and figure out who the winners will be. What I'm suggesting is that, if the IMU doesn't change its ways, it's the responsibility of mathematicians to do something about this, specifically, to break confidentiality and get the news to reporters. In every case that I can remember, a few days before the ICM, I know of people who had figured out who the winners were. One would normally avoid leaking this to the press, so as not to interfere with the IMU's plan for how to release the news, but maybe their plans should be interfered with.

20. **Zoviyer**
August 8, 2018

Even more annoying and disappointing that the IMU embargo on the press, and really hard to believe for this age, is the fact that they are not streaming the Plenary lectures. Even the lectures at the congress in Madrid all the way back to 20016 were streamed (and are still available at http://www.icm2006.org/video/). They seem happy to just put some very superficial posts in Instagram (not at odds at all with that platform!).

21. **martibal**
August 8, 2018

With the risk to be old fashioned, why is it so important that Fields medalists have their "story" on the first page of the Times ? I believe that some years ago the Fields Medals had very little echo outside the maths community. Was it is so bad ? In this blog we are complaining posts after posts against the "big show/circus" that high energy theoretical physics has become; if maths tries to resist to this tendency, that might be a good new.

22. **Peter Woit**
August 8, 2018

martibal,
Not sure how important it is, but it seems a good opportunity, once every four years, to get some public attention for high-quality pure mathematics research. Unlike the Breakthrough Prize business, which tries to do this with Hollywood glitz and large checks, the Fields medal is little money, and awarded in the serious context of the ICM conference. I think Peter Scholze explicitly recognized the difference between the two things, by turning down the (junior) Breakthrough Prize award, and accepting the Fields Medal award.

The kinds of stories put together by Quanta about the medalists and their work I think are good things for the public to see. It would be great if the IMU's press people helped more of the press to create such stories, to get wider distribution. One can sensibly argue that the IMU should stick to math research, not engage with promoting it to the public. But, part of what they are trying to do is such promotion, and I don't see an argument for why they should do it, but badly.

23. **Kenneth Chang**
August 9, 2018

The issue of the Quanta favoritism is the same as four years ago, but in every other respect, the resources and help from IMU were _much_ worse this time around.

In 2014, IMU contacted me directly, an email from Ingrid Daubechies, the IMU president. I don't know if she personally wrote it, but someone clearly made the effort to find reporters who were likely to be interested in writing about the Fields. This time, IMU sent an email to foreign@nytimes.com, a catchall mailbox for all sorts of news from all over the world. Not surprisingly, the email never made its way to me.

In 2014, reporters who received the embargoed news release could interview the medalists. (The exception was Maryam Mirzakhani, for understandable health reasons.) This time, reporters were not to contact any of the medalists ahead of the announcement.

In 2014, IMU provided the name of an expert who could talk about each medalist's work. This time, there was no such help.

In 2014, IMU provided a description of the medalist's work. It was highly technical, but at least it was enough for me to at least ask Deane, "What the hell does this mean?" This time, there literally was nothing more than names and the Simons Foundation videos.

I complained to the IMU/ICM people afterward and was told several of the medalists had said they did not wish to speak to the press. I don't remember this ever happening before (Mirzakhani again being the exception). I don't think this is coincidental. It appears that at least some of the medalists feel that the significant time they spent for the Quanta articles and Simons videos fulfilled their obligation to the public.

24. **Thomas**
August 9, 2018

If some of you are ready to put aside the "media coverage problem" for a few minutes, I think an interesting point could be trying to analyze how close some other young mathematicians were to win a Fields medal this year. More precisely, since guessing the future medalists is one of the favorite games of math addicts (see here for instance: https://poll.pollcode.com/44839318_result?v), I've noticed that, except for Peter Scholze, the three other medalists appeared kind of outsiders compared to the most proposed names found on the web. Indeed the names which came out were mainly: Fernando Codá Marques, Ciprian Manolescu, Simon Brendle, Geordie Williamson, or Maryna Viazovska. I agree that Alessio Figalli was sometimes proposed, but not as a big favorite. So I guess we have at least 2 big surprises this year: Caucher Birkar and Akshay Venkatesh.

While I clearly think that's part of the fun to have surprises with the Fields medal results, it would be cool to really discuss the weight of the contributions of the winners in comparison with the one of the "main losers". So we could try to understand the final choice of the IMU for the Fields medalists, and in the same time sharing a bit the word outside the light of the 4 winners…

25. **Deane**
August 9, 2018

Let me clarify that although I've been complaining specifically about coverage of the Fields Medals, my overall larger concern is the coverage of math in general. I want to see more math stories in the New York Times, period. The Fields Medals are just the lowest hanging fruit. Obituaries of mathematicians seem to be the next lowest. I want more.

Mathematicians like to complain about all the fuss about the Fields Medals. In an ideal world, I would, too. But we need whatever publicity we can get, and if, for the moment, it's easy to get coverage of the Fields Medals, I don't see why we should spurn it.

26. **Peter Woit**
August 9, 2018

Thomas,

I was thinking of writing about this in the posting, in the end didn't mainly because of lack of time. An important point for the public to understand is that, while there are some winners like Scholze who would be chosen no matter what, typically many of the Fields Medal winners are very good mathematicians, but not that distinguishable from some other very good mathematicians who don't get chosen, in this case simply because there were only three non-Peter Scholze slots available this year.

I suspect a different committee would have made some different choices, recognizing a different group of very accomplished people. Part of the issue is that there is no sensible single well-ordering of mathematicians by who is "best". Different people's work is very different, and different people will have different judgments about what kind of work they like best. So, while admiring the accomplishments of this group, it's a good idea to keep in mind that there is a significant number of others, unrecognized by the medal, but doing equally good work.

27. **Anni**
August 9, 2018

Reading the write-ups and talking with other mathematicians, I had the impression that: Birkar deserved it, Venkatesh is a good choice but so are the next ten people, Figalli (?).

Another interesting issue is to compare sectional panels and invited speakers. In the section I know best one-third of the speakers are from the area of the committee chair and a number of speakers are collaborators and former students and postdocs of other committee members.

28. **Peter Woit**
August 9, 2018

Anni,

I don't want to encourage everyone to debate here their relative evaluations of the Fields medal winners/non-winners. To see who was on this and on previous Fields medal committees, see
https://www.mathunion.org/imu-awards/fields-medal#on-page-3
The members of this year's committee that I know something about all seem to me likely to have taken the job seriously and done it well. One of them, Terry Tao, has some comments here:
https://terrytao.wordpress.com/2018/08/01/birkar-figalli-scholze-venkatesh/

That said, I still suspect a different group of well-known mathematicians with equally good judgment and taking the job equally seriously, quite likely would have come up with (besides Scholze) some different names.

29. **Davide Castelvecchi**
August 9, 2018

What Ken said sadly confirmed my hunch — that at least some of the winners did not wish to speak to the press.

30. **Peter Shor**
August 10, 2018

My impression from the 1998 ICM was that the press knew the names of the Fields Medal winners well before the public was supposed to and interviewed them (although the names did leak out somehow).

I suppose they may have blamed the press for the leaks, and that this led to the current state of affairs.

31. **Koji Fujiwara**
August 10, 2018

I attended ICM as a speaker. I was thinking about this discussion during the closing ceremony. I understand the frustration of the journalists.

One thing I noticed is that ICM is the International congress of mathematicians, it is not for/on/of Mathematics. It is an event where about 3000 mathematicians get together, about 200 invited talks are given, and 4 young rising stars are chosen and given medals. It is different from, say, Nobel prizes. For example, Nobel prizes of

physics are chosen every year, but only the laureates are there and no other physicists.
During the ICM, I could see lots of efforts are made to reach out to the mathematicians from countries and areas where there is a chance of improvement, say, Africa. IMU is doing good jobs toward mathematicians.

That might explain how IMU have been to journalists. I am not justifying it, but looking for reasons that made the difference. I imagine that the headquarters of IMU possibly does not have a clear idea yet on what they want to do toward the general public.

32. **bixter zavala**
    August 12, 2018

    I find it rather appalling that, judging by the number of comments in this thread, the press issues related to Fields Medal seem to be of (much) greater importance than the decision to host ICM 2022 in Russia. The latter is far from morally neutral. It's basically legitimizing a regime notorious for invading and occupying another country (the annexation of Crimea) as well as other infractions (political prisoners, MH-17 etc.). I would expect the mathematical community to use its conscience and at the very least voice its condemnation.

Theorists with a Swamp, not a Theory

August 13, 2018
Categories: Swampland

In recent weeks string theory has been again getting a lot of press attention, because of claims that new progress is being made in the study of the relation of string theory and the real world, via the study of the "swampland". This is a very old story, and I've often written about it here. I just added a new category, so anyone who wants to can go follow it by clicking on the Swampland category of posts.

Recent press coverage of this includes an article by Clara Moskowitz at Scientific American, entitled String Theory May Create Far Fewer Universes Than Thought. This motivated Avi Loeb to write his own Scientific American piece highlighting the dangers of string theory speculation unmoored to any possible experimental test, which appeared as Theoretical Physics is Pointless without Experimental Tests. Loeb reports:

> There is a funny anecdote related to the content of this commentary. In my concluding remarks at the BHI conference we held at Harvard in May 2018, I recommended boarding a futuristic spacecraft directed at the nearest black hole to experimentally test the validity of string theory near the singularity. Nima Arkani-Hamed commented that he suspects I have an ulterior motive for sending string theorists into a black hole. For the video of this exchange, see
>
> https://www.youtube.com/watch?v=WdFkbsPFQi0

Last week Natalie Wolchover reported on this controversy, with an article that appeared at Quanta magazine as Dark Energy May Be Incompatible With String Theory and at the Atlantic as The Universe as We Understand It May Be Impossible (the Atlantic headline writer misidentifies "we" as "string theorists").

Wolchover accurately explains part of this story as a conflict between string theorists over whether certain solutions (such as the KKLT solution and the rest of the so-called "string theory landscape") to string theory really exist. Vafa argues they may not exist, since the proposed solutions are complicated and "Usually in physics, we have simple examples of general phenomena." In response Eva Silverstein argues:

> They essentially just speculate that those things don't exist, citing very limited and in some cases highly dubious analyses.

On Twitter, Jim Baggott explains the problem

> Let's be clear. This is not a 'test' of string theory. There is no 'evidence' here. This is yet another conjecture that 'might be true', on which there is no consensus in the string theory community.

and in a retweet, Will Kinney accurately notes that

> The landscape is a conjecture. The "swampland" is a conjecture built on a conjecture.

and points to an earlier tweet thread of his about this. Sabine Hossenfelder replies with the comment that

> The landscape itself is already a conjecture build on a conjecture, the latter being strings to begin with. So: conjecture strings, then conjecture the landscape (so you don't have to admit the theory isn't unique), then conjecture the swampland because it's still not working.

The Simons Center summer workshop this year has been devoted to Recent Developments in the Swampland, videos are here (this was also the case in 2006, see here). Next month in Madrid a conference will be devoted to Vistas over the Swampland, and I'm sure many more such gatherings are planned.

Unfortunately I think the fundamental problem here somehow never gets clearly explained: String theorists don't actually have a theory, what they have is an approximation to an unknown theory supposed to be valid in certain limits, and a list of properties they would like the unknown theory to have. If this is all you have, there's no way to distinguish when you're on dry land (a solution to string theory) from when you're in the swamp (a non-solution to string theory). Different string theorists can generate different opinions, conjectures and speculations about whether some location is swamp or dry land, but in the absence of an actual theory, no one can tell who is right and who is wrong. I don't know why Vafa back in 2005 chose "Swampland" as the metaphor for this subject, but it's an unfortunately apt one: string theorists are stuck in a swamp, with no way of getting out since they can't tell what's dry land and what isn't.

Comments

1. **4gravitons**
   August 14, 2018

   I'm not an expert in string compactifications, but for what it's worth I don't think the problem here is that the theory itself is ambiguous. Nonperturbative physics is hard, especially for gravity. Even with a completely well-defined theory (say, 11D supergravity), it's not straightforward to verify that some arbitrarily complicated compactified quantum spacetime is a valid solution to the full theory.

   (Case in point: asymptotic safety is still an open question, and that starts out in 4D.)

   There's a case to be made that if someone doesn't have a good handle on the nonperturbative physics, they shouldn't act like they're doing cosmology with it. But I don't think the problem is just that M theory doesn't have a Lagrangian.

2. **Lars**
   August 14, 2018

   "It's conjectures all the way down"

   Conjecture built on guess
   In turn that's built on hunch
   The latter really rests
   On inference a bunch

3. **Peter Woit**
   August 14, 2018

   4gravitons,

Presumably you agree that there is a problem with a lack definition of non-perturbative string theory. Why are you so sure that this problem isn't relevant to the problem here? I'd recommend reading the introduction to arXiv:1807.06581 (written by people who are experts on this), which explains the underlying problem behind the dense thicket of technicalities which arise here:

> Compactifcations of superstring or M-theory to 4d Minkowski or AdS space are well understood and have led to the idea that string theory gives rise to a gigantic landscape of possible 4d low energy eective theories [2{11]. However, classical no-go theorems such as [12] indicate that realizing de Sitter vacua in string theory requires quantum and/or stringy ingredients. The fact that corrections to classical 10d low energy supergravity are qualitatively important implies that dS compactifcations, in contrast to AdS or Minkowski compactifcations, must live in a regime in which these corrections cannot be made arbitrarily small [13], hence perturbation theory cannot be made arbitrarily accurate. Moreover the absence of supersymmetry in dS, and perhaps more fundamentally the lack of a complete, nonperturbative formulation of string theory, make it hard to obtain exact results beyond perturbation theory. Thus a completely rigorous, parametrically controlled construction of individual de Sitter vacua in string theory has remained out of reach. On the other hand, starting with [6], much progress has been made over the past 15 years in developing models containing all the ingredients needed to produce effective potentials generic enough to support an abundance of dS vacua, barring extraordinary conspiracies that would somehow eliminate all of those.

4. **JGS**
   August 14, 2018

   It's funny that the Atlantic article, which is a reprint of Natalie Wolchover's Quanta piece, says that string "theory permits some 10,500 different solutions: a vast, varied "landscape" of possible universes." Some typo!

5. **4gravitons**
   August 14, 2018

   Peter,

   I agree that the lack of a full non-perturbative definition of the theory is often brought up in this context, it's just not obvious to me that it isn't a red herring, something that gets emphasized because it's a big deep problem that's plausibly related and thus makes for good introduction-padding material. Put another way, I know of very few cases where having a non-perturbative definition alone actually helps answer the kind of question these people are trying to answer, without some extra magic like unbroken supersymmetry. But again, I may just be showing my ignorance here.

6. **Paraphraser**
   August 14, 2018

   Classical no-go theorems indicate that realizing confinement in Yang-Mills theory requires quantum ingredients. The fact that corrections to classical 4d low energy chromodynamics are qualitatively important implies that confinement must live in a regime in which these corrections cannot be made arbitrarily small, hence perturbation theory cannot be made arbitrarily accurate. Moreover the lack of a complete, nonperturbative formulation of Lorentz-invariant Yang-Mills theory make it hard to obtain exact results beyond perturbation theory. Thus a completely rigorous, parametrically controlled construction of hadron states in Yang-Mills theory has remained out of reach.

7. **Peter Woit**
   August 14, 2018

   4gravitons,

   It sounds like you don't actually understand the technical issues here, but somehow are convinced that it's a good idea to suggest that Denef/Henecker/Wrase (who are experts on this) are padding their introduction with misleading red-herring material. Sorry, but this is pretty bizarre behavior.

   I've ended up wasting huge amounts of time the past fifteen years arguing about the KKLT construction with people. Besides a very small number of cases, the people who have wanted to argue about it with me don't seem to actually understand any details of what the issues are. One of the weirder general aspects of the "string theory" story over the years is the way it is so difficult to get a concrete actual definition of things that people are making claims about. What exactly is a "solution to string theory", and why are experts now disagreeing over what is and what isn't such a solution? If you think Denef/Henecker/Wrase are spouting nonsense, can you point to somewhere where someone gives a precise answer to this question that isn't nonsense?

8. **Peter Woit**
   August 14, 2018

   paraphraser,

   As you're undoubtedly well-aware, there is a complete non-perturbative formulation of Yang-Mills theory (lattice gauge theory), expected to be Lorentz-invariant in the continuum limit, with massive amounts of numerical data backing up that expectation. Trying to claim that the state of Yang-Mills theory and that of non-perturbative string theory are the same is just absurd.

9. **4gravitons**
   August 15, 2018

   In retrospect I think I was confusing the naive notion of defining a theory (having the Lagrangian, etc.) with the more sophisticated requirements (having something like lattice gauge theory) that you/Denef/Henecker/Wrase have in mind, so sorry for muddling things. Agreed that in the absence of that kind of definition the criteria for what is and isn't a solution seem quite vague, I've likewise been frustrated by the lack of explicit statements of what these people would count as a sufficiently established solution.

10. **mrp**
    August 15, 2018

    As a non-expert, it is not clear to me from the media coverage whether the conjecture put forth by Vafa and others is a mathematical conjecture or an empirical conjecture. Can anyone clear this up for me?

11. **Peter Woit**
    August 15, 2018

    mrp,
    It's not an empirical conjecture. It is often promoted as a well-defined mathematical conjecture: the solutions of the string theory equations have (or do not have) certain properties.

    The problem is that you don't know what the relevant string theory equations are. So, this is a conjecture about a conjecture:

First conjecture: There is a well-defined theory satisfying a certain list of properties.

Second conjecture: The equations of this unknown theory do or don't have certain specific properties.

The second conjecture is a "mathematical conjecture", but I'm not so sure the first one deserves to be called that.

12. **Anonymous**
August 15, 2018

I would like to point out that the notion of "swampland" is independent of string theory.

The swampland is defined as the collection of low energy effective field theories which include perturbative gravity but does not admit a non-perturbative completion.

Finding criteria to decide if a low energy theory belong to the swampland is
of rather obvious interest.

Conjecturing such criteria on the basis of our current knowledge of string theory and holography is of course a more hazardous enterprise, as we only have a solid non-perturbative understanding of certain supersymmetric, Anti de Sitter backgrounds
but we are interested in non-supersymmetric, possibly de Sitter backgrounds.

Nevertheless, well-posed conjectural criteria are a reasonable subject of inquiry
and once formulated they are independent of the validity of string theory or landscape ideas.

It seems inappropriate to characterize that work as "conjecture built on a conjecture
built on a conjecture".

13. **Peter Woit**
August 15, 2018

Anonymous,
One aspect of the swampiness problem here is that it's often unclear what definition of "swampland" is being used. According to you the definition makes no reference to string theory, according to others it does. For instance, Wikipedia tells us that:

"In physics, the term swampland is used in contrast to the term "landscape" to indicate physical theories or aspects of such theories that could be true if gravity were not an issue but are not compatible with string theory."

I'm using the term in this sense (EFT incompatible with string theory) and pointing out the problem with this definition (you don't know what equations the words "string theory" are referring to). The recent "swampland" controversy has centered around the question of whether string-theory based constructions like KKLT are true metastable string theory ground states, this is a controversy about string theory.

When you instead use the definition "EFT incompatible with non-perturbative quantum gravity", you need to explicitly state your assumptions/conjectures about what properties quantum gravity will have non-perturbatively. Some people believe string theory is the only consistent non-perturbative QG, so, for them, there's no difference in the two definitions. If there's a difference for you, you need to explicitly state your assumptions.

In the case of the current controversy, there's good observational evidence for de Sitter, and no evidence for "quintessence" + lots of reasons to be skeptical about it. So, if you believe Vafa's conjecture and your swampland is the string theory one, you have good evidence against string theory (but, due to swampiness, not evidence string theorists will accept). If you believe Vafa's conjecture and your swampland is the non-perturbative QG one, then you have evidence there is no non-perturbative QG. I'd argue that in this second case you haven't shown there is no non-perturbative QG (since I think there's a consistent theory of the world, including QG), you've just shown that there's a problem with your conjectured properties of non-perturbative QG. An added reason why one wants to be clear exactly what properties are being conjectured.

14. **Moyses**
August 16, 2018

Dear Peter,

as you seems to have former publications in LQCD, I assume that you do not ignore IKKT results derived by similar Monte Carlo methods. Clearly, such matrix approach to ST provides a nonperturbative fully-covariant formulation for IIB ST. Surprisingly enough, not only numerical solutions (but also analytical ones) suggest the same results, i.e.: an expanding (3+1)d Universe dynamically emerges together with a cosmological "constant". Please, check for instance: arXiv:1208.0711 and references therein.
All the best,
M.

15. **Peter Woit**
August 16, 2018

Moyses,
http://www.math.columbia.edu/~woit/wordpress/?p=4320

On Status of KKLT

August 16, 2018
Categories: Multiverse Mania, Swampland

(Warning, this is just more about the topic of the last posting, which for most people will be a good reason to stop reading now. On the other hand, if you're obsessed with the controversy over string theory, you might find this interesting).

I finally got around to watching some more of the Simons Center Workshop on the Swampland talks, and noticed a remarkable exchange at the end of Thomas Van Riet's talk On Status of KKLT (starting at 1:30). The first commenter (a German, Arthur Hebecker?) starts off saying "I think you are doing something that is very dangerous", with the danger being that KKLT will get thrown out and people will think that it is a "theorem" that string theory has no dS vacua. He is interrupted by Vafa who tells him that "your statement is defamatory, let's calm down". The German goes on to explain to Vafa the significance of the danger he is concerned about:

> Maybe for you in the US it's fine at Harvard, for me it will be a pain because people will turn against me. The little standing that string theory and new physics at all has in Germany will be harmed by a backlash on us that we have been talking nonsense all the time, which is not true.

Van Riet after a while interjects that there is an even worse danger:

> The opposite happened and actually back-reacted very badly. We had the books by Woit and Smolin and it was based on the existence of the multiverse as a correct statement, right? And that's when the criticism of string theory took off, right?

Someone else in the audience (Iosif Bena?) comes in on the Vafa/Van Riet side of the argument, criticizing multiverse mania:

> I think the main problem was that at the beginning people in the KKLT camp, they came up with, "OK string theory has the multiverse, we're not going to do physics anymore, the anthropic principle…" They came up with all these ideas that hurt string theory much much worse, at least in Europe, at least in my part of Europe. And you know, essentially hurt us heavily… Then there were these books by Woit and Smolin that were very popular…

It's remarkable to see publicly acknowledged by string theorists just how damaging to their subject multiverse mania has been, and rather bizarre to see that they attribute the problem to my book and Lee Smolin's. The source of the damage is actually different books, the ones promoting the multiverse, for example this one. A large group of prominent theorists, especially many from the West Coast, including the group at Stanford and the late Joe Polchinski at Santa Barbara, used the existence of the KKLT construction to push very hard a pseudo-scientific excuse for why string theory wasn't working out. I've often point this out, and I do think this has been very damaging to the public perception of string theory. But the underlying problem is the takeover of string theory by multiverse pseudo-science, not that I and Lee Smolin criticized it.

A striking fact about the Stony Brook workshop is that none of the participants were from Stanford, and none of the many prominent figures responsible for promoting KKLT were there. It looks like there is now a dramatic split going on, with Vafa leading the charge to try and fight back against what in recent years has been a seeming dominance of string theory by the pro-multiverse faction. I think such a split is long overdue, that most string theorists for years now have been making a terrible mistake by going along with multiverse pseudo-science. As Hebecker(?) explained though, fighting back publicly at this point carries its own dangers. In particular, many observers will be asking: "for years you told us about the 10^{500} vacua", now you say that maybe there aren't any. Which is it? Why can't you tell? And do you really have a serious alternative for how to connect string theory to the real world?

Vafa tries to not take sides, to portray this as a simple technical question that will yield to further calculations by theorists. Where I disagree with him is that I'm very skeptical that this is a technical question with a well-defined answer. This is not a new controversy: theorists have been arguing about moduli stabilization and this de Sitter/no de Sitter issue for twenty years or so, without coming to any firm conclusions. If you watch the technical talks at the Stony Brook workshop, the degree of technical complexity of the arguments is striking, as is their often rather vague nature. What you don't see is a specific set of equations that everyone agrees on. We'll see what happens in coming months and years, there are likely to be a large number of papers written on this subject. Also to look out for, likely the efforts of Vafa and others to throw doubt on KKLT will not be taken lying down. The West Coast Empire will strike back…

Update: At CNN, Don Lincoln has an article about this, which ends with:

> It's not quite a WWE cage match, but it's going to be fun to watch these theories fight it out.

Update: Tonight the West Coast Empire has struck back, defending here and here their dS vacua against the Swampland attack, and going on the offensive, accusing the conjecture of their attackers as being "ruled out by cosmological observations, at least at the 3 sigma level".

Comments

1. **Sabine**
   August 17, 2018

   (Yes, that's Hebecker.)

2. **Bai**
   August 17, 2018

   As a complete outsider to HEP (I am a condensed matter physicist though), I find appalling that issues like the damage being made to a community (in certain parts of the World..) are openly discussed and considered at Scientific events. It makes it so explicit that some of these fellows (I suspect the vast majority..) give a lot of importance to avoiding their University chairs being shaken, their funding being cut, etc, than seeking the ultimate truth in their research. It is very sad to see the kind of game (hard) Science is becoming, and it all sets a terrible example for future generations…

3. **Peter Woit**
   August 17, 2018

   Bai,
   This exchange was interesting precisely because these issues, while important to people, are virtually never publicly discussed. This is not something that regularly happens. Also note that, in response to Hebecker bringing up the issue, Vafa and the others, while acknowledging the political problem, properly responded that such issues should not be relevant, that what was important was finding the scientific truth of the matter, whatever the political ramifications.

4. **Jesse O.**
   August 17, 2018

"The West Coast Empire will fight back…"

It is wholly irresponsible to come so tantalizingly close to an "Empire Strikes Back" pun.

5. **Peter Woit**
August 17, 2018

Jesse O.,
Irresponsibility fixed.

6. **Joelson F. Silva**
August 17, 2018

In condensed matter theory theoretical physicists have your own "Vietnam War", called high critical temperature superconductors, and for those thet believe in AdS/CFT approach to the problem, this method can "describe" only few properties of the problem as the pseudogap
region and fails to most others real problems in condensed matter, for exemple, universal viscosity prediction for bad metals. However, is not about that I wanna talk here, seens to me that the string theory is the "Vietnam War" for high-energy physicists, but in condensed matter we recognize our "defeat", even P.W. Anderson recognized your mistakes in the first approach to the problem. Many important things came from the High Tc superconductors search, as new numerical techniques, but we don't find the answer to the problem, until today. We don't give up of the High Tc supercondutors, we move on to new approachs and ideas, this was not easy, due the "fights" beetwen the Popes of the area and the intimidation over the new
researchers and your ideas. I think that HEP physicicts must do the same, move on to new ideas outside string theory, in this sense, the Woit work and others are fundamental. The first step to the string theorists is recognize the "defeat" in this "Vietam War".

7. **Lee Smolin**
August 17, 2018

Dear Peter,

I have usually stayed silent in the last 12 years since my The Trouble with Physics was published, but given the remarks you quote, I think its important to remember some key points:

-Leonard Susskind's paper, "The Anthropic Landscape of String Theory (0302219)" was posted in February of 2003. This immediately ignited the controversy over KKLT and the landscape; Susskind's book The Cosmic Landscape, written to bring his polemic to a large public audience was published in December 2005. By the time my book appeared in September 2006, the controversy had been going for 3 and a half years. Several prominent science journalists told me my discussions of the landscape were already old news.

-Indeed, the issue of there being a vast landscape of string theory vacua, and hence a problem of predctability, was widely know back to papers of Strominger and others back to 1987. It was the motivation for my 1992 paper on cosmological natural selection; indeed I introduced the term "landscape" in the context of that work, borrowed from population biology. What was new in 2003 was only that KKLT addressed the issue of whether there are any string vacua for positive cosmological constant.

-Many people criticized my book without reading it, quoting others who also had not read it. Many who did read it commented, publicly or to me, including several string theorists, that the book had been mischaracterized and was not a simple "attack on string theory". Several praised the book for its fair and balanced treatment of the successes and failures of string theory, that was in detail, little different than that given in Brian Greene's book of the same year.

-The subject of the book was very different, it was the role of controversy and disagreement, and how they are resolved, as engines which drive progress in science. String theory was there as a case study.

-Nonetheless, I would claim the critique of string theory's strengths and weaknesses has largely held up over the 12 years since. The successes are still reasons to consider it one of the candidate approaches to QG. The big open problems I elaborated there: the lack of a background independent formulation, the absence of a proof of uv finiteness to all genus, and the problem of testibilty, are all still unresolved. In the time since a great deal of good work has been done on related topics such as AdS/CFT, little of which, however, addresses these key open issues.

-The book advised that string theory be studied as one of several approaches to QG, which have complementary strengths and weaknesses. Each is interesting, none is problem-free. Indeed my previous book Three Roads had suggested a route to unification of string theory with LQG. These remain valid, reasonable views.

-Of course, others will disagree. As TTWP argued, this kind of friendly, respectful, disagreement is necessary for science to progress. Given that, we should all welcome a diverse scientific landscape, especially to leave room for young scientists to discover better theories, for this is how science progresses. That remains the message of the book I wrote.

Thanks,

Lee

8. **Peter Woit**
August 17, 2018

Hi Lee,

Thanks providing some valuable perspective on your book.

As for my book, I can't help but mention that the original version was mostly written in 2002, pre-Susskind/landscape. There is nothing in the version of the manuscript submitted to Cambridge University Press in early 2003 about the landscape/multiverse/anthropics issue (basically because I was unaware of it). If string theorists hadn't managed to stop CUP from publishing that version, mine might have appeared with nothing about this topic in it. The chapter on this topic was added in 2004, in the version that Jonathan Cape later published.

Personally, I think the arguments of both books have held up very well over the years since they were written, much better than those of contemporaneous books enthusiastic about the prospects for string theory, supersymmetry, extra dimensions. But I think string theorists are wrong to attribute their problems to the books. Their problems are with the science, not with the fact that some people pointed out the scientific (and sociological) problems.

9. **Shantanu**
August 18, 2018

Peter, was Strominger at this meeting? IIRC he is not a fan of landscape or anthropics. (see his colloquium at Harvard which you had blogged about) Does he side with Vafa on this issue?

10. **Peter Woit**
August 18, 2018

Shantanu,
No Strominger, and I assume his point of view on this is probably the same as that of most people's, string theorists and non-string theorists: landscape/anthropics is a dead end, and no point in wasting time on arguing about the complex details of whether Rube Goldberg machines like KKLT really work or not.
.

11. **Jeff Berkowitz**
August 18, 2018

Hello Dr. Woit, in your posting you write: "[Various key theorists] … used the existence of the KKLT construction to push very hard a pseudo-scientific excuse for why string theory wasn't working out."

I want to make sure I understand this comment. You are saying that due to the existence of the KKLT paper, the community of theorists–or at least a faction of the community–was able to "fend off" the "dark energy versus aDS" issue for a long time without being "called to account" for the conjectural nature of KKLT. Is this correct?

12. **Peter Woit**
August 19, 2018

Jeff Berkowitz,
No. The significance of the KKLT paper is that their mechanism produces not just one "solution" with positive CC, but an exponentially large number of them. It is this huge number of solutions that has been used as basis of the claims by a group of theorists that string theory implies both an (anthropic) solution to the CC problem, and suggests that what we think of as fundamental constants that should be calculable, instead are environmental. It is this aspect of KKLT that I think motivates Vafa and others to try to find an argument against it, hoping to kill off the "landscape" pseudo-science this way.

13. **Narad**
August 24, 2018

>If string theorists hadn't managed to stop CUP from publishing that version

I seem to have missed this story. Could you elaborate or provide a pointer, please? I was in journals publishing, but we were close with (supporting, actually) the books division at the university press I worked at, and although CUP is much larger organization, acquisitions editors are practically on a quota basis.

14. **Peter Woit**
August 24, 2018

Narad,

http://www.math.columbia.edu/~woit/wordpress/?p=245

Quantum Supremacy

August 26, 2018
Categories: Uncategorized

Hasn't been much that I've heard about worth discussing here recently. Presumably everyone is on vacation. I'll try and gather some things that may be of interest, starting first with the hot topic of "quantum computation". It looks like this will be drawing an increasing amount of attention and resources in the field of physics research. For instance:

- Slides and videos from the summer IAS program From Qubits to Spacetime are available here.
- There will be a graduate seminar at Harvard this fall, blog post about it here.
- In a few weeks Fermilab will host a workshop on Next steps in Quantum Science for HEP.
- Moving through the US Congress is a National Quantum Initiative Act, which would provide over a billion dollars in funding for things related to quantum computation.
- At the NSF MPS (Mathematics and Physical Sciences) they're promoting NSF's Quantum Leap. This is the first of four "Big Ideas" (discussed here) which will influence what gets funded. The other three are multi-messenger astrophysics, big data, and things related to biology.
- Launching this week is The NSF 2026 Idea Machine, which is a competition for suggesting research questions, more "Big Ideas" for the NSF to fund. If you want to enter the competition that opens Friday, I'm guessing that invoking the word "Quantum" will help.

Comments

1. **Andrei**
   August 27, 2018

   Also, Scott Aaronson has a new set of notes on quantum information:

   https://www.scottaaronson.com/blog/?p=3943

2. **Onlooker**
   August 27, 2018

   Interesting that Witten seems to have switched fields into quantum computing.

3. **George Herold**
   August 27, 2018

   Thank you for the nice series of links. I'm a hardware guy and found this one useful (from the NSF Big Ideas.)

   https://www.nsf.gov/attachments/245240/public/materials_for_quantum_leap.pdf

   Is it just me, or does this seem like a lot of hype? (Topological insulators as the next transistor?)

4. **Shantanu**
   August 27, 2018

   Maybe I am naive, but can someone tell me how quantum computing provide insights on Physics beyond standard model (or just more insights into standard model) or give clues on what LHC would see?

5. **Peter Woit**
   August 27, 2018

   Shantanu,
   I don't think any of this has any relations to questions about the SM or possible BSM physics. In some sense, the move to this study of new quantum materials and quantum computation marks a giving up on the idea of trying to understand more about the SM.

   There seems to be some hope that this will lead to new ideas about quantum gravity and, especially, how to resolve the black hole information paradox. I haven't had time to look into this carefully, but from what I've seen, the ideas about quantum gravity seem extremely crude, untestable, and explain nothing at all about the relation of quantum gravity to the SM.

6. **Stephen Marney**
   August 27, 2018

   Peter,
   Wouldn't quantum computation allow us to simulate the SM and BSM physics to perhaps get new insights into these models of physics?

7. **Peter Woit**
   August 27, 2018

   Stephen Marney,
   The problem is that most of the mysteries of the SM have to do with what appear to be weakly coupled degrees of freedom, where perturbation theory is a very effective computational tool and I don't see how quantum computers would help. The strongly interacting sector of the theory is one where maybe quantum computers will help, but all evidence is that there the dynamics is known and fixed (SU(3) Yang-Mills).

   For a different perspective, I see that Sabine Hossenfelder has a new piece out in Quanta:
   https://www.quantamagazine.org/the-end-of-theoretical-physics-as-we-know-it-20180827/

   The main reference to the SM there is
   "maybe what we currently think of as fundamental — space and time and the 25 particles that make up the Standard Model of particle physics — is made up of an underlying structure, too. "
   but my problem with this sort of claim is that I've never seen a plausible idea for what this new "underlying structure" is supposed to be.

8. **CWJ**

August 27, 2018

There is an important aspect of testing SM and BSM physics, and that in many cases, for example in dark matter detection, neutrinoless double-beta decay, and neutrino cross sections, and that's one needs the interaction matrix element between particles and nuclei; and to get accurate results one needs to solve the nuclear many-body problem well. I know, I know, you all probably think this is either "long ago solved physics" or "black box physics," but neither is true. (It happens to be my area of research.) The last 15 years have seen tremendous improvement in rigorous calculations of the nuclear many-body problem…but mostly for light nuclei (lighter than oxygen), and many of the interesting targets are heavy (like xenon). So the idea here is to do nuclear many -body calculations on heavy nuclei with quantum computers.

Now, will quantum computers actually be able to outperform classical computers in this regard? And if so, how soon? Well, that's the question.

(And, no, density function theory isn't enough for some of these calculations. And coupled clusters only work well near closed shells.)

9. **Chris Woodward**
   August 29, 2018

   A google search for "quantum computing" gave me as first hit a
   report by Accenture https://www.accenture.com/t20170628T011725Z__w__/us-en/_acnmedia/PDF-54/Accenture-807510-Quantum-Computing-RGB-V02.pdf#zoom=50
   which mentions, as the first possible application in Table 1
   "portfolio selection", which I guess means stock picking. Oh boy.

10. **Mateus Araújo**
    August 31, 2018

    CWJ,

    Simulating complex quantum systems is precisely what quantum computers will be good for. Shor's algorithm gets a lot of attention, for obvious reasons, but there is no fundamental reason why quantum computers should be good at factoring, or why classical computers should be bad at it.

    On the other hand, the idea that any physical system at all can be efficiently simulated by a digital quantum computer is taken seriously by a lot of the community (it is known as the Church-Turing-Deutsch thesis), and I personally would be shocked if a xenon nucleus turned out to be intractable.

11. **CWJ**
    August 31, 2018

    Mateus,
    I agree that quantum computers *ought* to be good for modeling complex quantum systems. But in practice it may be very different.

    Fortunately, a paper on the arXiv just appeared, explaining how to do this for quantum many body systems (atoms, but presumably I can translate it to nuclei). Now I just need to find the time to read it….

    https://arxiv.org/abs/1808.10402

Quick Links

August 31, 2018
Categories: Uncategorized

A collection of links that may be of interest:

- Talks from the SM at 50 conference held earlier this summer are available here.
- A detailed expose of the "Fake Science Factory" is here, a related Nature story is here.
- For those wondering what came out of this story, you might be interested in this.
- If you want to know what happens to string theorists who leave the field, one answer is that they perform as Ninja Sex Party.
- Burt Richter passed away last month at the age of 87, some obituaries are here, here and here. Blog postings here discussing talks or papers by him can be found with this search.
- Terry Tao has come up with his own take on arithmetic geometry, available here.
- A Capella Science is really too wonderful for words. For an example, check out William Rowan Hamilton. Tommaso Dorigo explains here that Tim Blais will be at CERN on Sept. 19.
- October 10 there will be a program at the New York Academy of Science about The Mystery of our Mathematical Universe. I can't help noticing something about discussions of the deep role of mathematics in physics: they rarely involve mathematicians.
- I'll do an online web interview on September 6, as part of the Festivalettura in Mantua.

Update: Frank Wilczek has an insightful review of Lost in Math at Physics Today.

Comments

1. **Richard**
   September 1, 2018

   Open Arxiv version of third quick link (Muon G-2 GR): https://arxiv.org/abs/1803.01395

2. **Blake Stacey**
   September 1, 2018

   I've felt for quite a while that the term "predatory journal" is misleading. My suspicion was always that plenty of scientists who published in such journals were doing so with their eyes wide open. Their motivation was obvious; it rhymes with "shmenure committee". But it's very easy for us scientists to cast ourselves in the best possible light, and the terminology reflects that presumption of doe-eyed innocence we grant ourselves.

3. **Cormac McG**
   September 1, 2018

   Hi Peter
   The Hamilton link – excellent! – has compelled me to suggest some other Hamilton related items. I feel no shame in plugging my physicist colleague Iggy McGoverns sonnet sequence on the life of William Rowan Hamilton entitled "A Mystic Dream of Four" – 64 sonnets organised by themes inspired by his life, his physics and maths, as well as his correspondents. I should have suggested you could go to see him when he was reading in New York last November. (His most recent book is "The Eyes of Isaac Newton")

   Also, before the summer a new art installation celebrating Hamilton's theoretical prediction of conical refraction was unveiled in the Physics department in Trinity College Dublin. Commissioned by James Lunney the sculpture details the "diabolical point". See https://www.tcd.ie/news_events/articles/new-art-installation-the-radiant-stranger-is-launched-in-school-of-physics/ In the photo Iggy McGovern is pictured (fourth from left) at the launch as he was reading one of his poems on Hamilton, while Sir Michael Berry (at left) was also speaking. James Lunney (third from left) is familiar with conical refraction as new investigations which he began with high coherence laser light revealed various interesting chiral, polarisation and phase behaviour. Certainly not all of these effects were anticipated by Hamilton, but the singular nature of conical refraction had captured the interest of Michael Berry with a number of later co-publications on the newer experimental observations with James Lunney. But I will not attempt to summarise this subject here.

   Finally, still on the Hamilton theme there is an interesting probably apocryphal anecdote involving a Mercury/Apollo era astronaut visiting Trinity College Dublin in 70s/80s being disinterested throughout their visit until at the last is shown by the arts-educated Provost a bust of Hamilton in the Long Room library, said astronaut suddenly waxes in interest and (perhaps) also uses some choice words about quaternions! This arising as all the astronauts were trained to reflexively compute quaternions as this was embedded in their training. Appears to still be a part of astronaut training?

   Hope that however off topic this was it was somewhat interesting!

   Enjoy Labor Day weekend
   Regards
   Cormac M

4. **Geoffrey Dixon**
   September 4, 2018

   And – timed nicely in the middle of your vaca – some To Quaternions and Beyond!

   https://www.quantamagazine.org/the-octonion-math-that-could-underpin-physics-20180720/

   PS Finished Sabine's tour of theoretical foibles. At the end she lists some cures for the sociological problems inherent in science. I can not help but think they have no more chance of being efficacious than telling a legless man that if he'd simply stand on his own two feet he could walk.

5. **Supernaut**
   September 4, 2018

   Regarding the comment about Hamilton's quaternions, I have no idea if it's part of astronaut training, but I can tell you that they are popular in aerospace engineering in dealing with rotations.

Also, thank you Peter for the link to the ‘Fake Science Factory’; I wonder if Theranos published in one of those predatory journals?

6. **From a Former Professional Higgs Boson Hunter....**
   September 4, 2018

   In the early day of the SLC before LEP turned on, the luminosity was marginal, leading to the wags at SLAC quoting the luminosity using the “Richter Scale”….

   The headline experiment, SLD, in those days was referred to as Slow Lingering Death as post-docs saw their prospects as beyond grim…

   Other reworked acronyms from LEP popular at the time included:

   OPAL: Old Petra Apparatus at LEP
   Delphi: Don’t Even Let Physics Hinder Imagination
   Aleph: A Large Expensive Piece of Hardware….

7. **none**
   September 6, 2018

   OT: Grothendieck’s manuscripts have been digitized and freely available
   https://grothendieck.umontpellier.fr/archives-grothendieck/

Beyond Weird

September 5, 2018
Categories: Book Reviews

Philip Ball's Beyond Weird is the best popular survey I've seen of the contemporary state of discussions about the "interpretation" of quantum mechanics. It appeared earlier this year in a British edition (which I just read a copy of), with the US edition scheduled to come out next month. Since it's already out in Britain, there are several reviews you can take a look at, an insightful one is Natalie Wolchover's at Nature.

The topic of the "weirdness" of quantum mechanics is one receiving a lot of attention these days, with two other books also appearing this year: Adam Becker's *What is Real?* (which I wrote about here), and Anil Ananthaswamy's Through Two Doors at Once. Lack of time as well as not having much of interest to say about the book has kept me from writing about *Through Two Doors at Once*. It's much more focused than the other two, giving close attention to the two-slit experiment and surprising variants of it that have actually been performed in recent years.

Some of what I very much liked about *Beyond Weird* is the way Ball avoids getting into the usual ruts that books on this topic often end up in (with the Becker book one example). He avoids the temptation to follow a historical treatment, something that is almost irresistible given the great story of the history of quantum mechanics. The problem is that the early history of quantum mechanics and the struggles of Bohr, Einstein and Heisenberg to understand what it was saying is a fascinating story, perhaps the most compelling in the history of physics, but it is one that has been well-told many times in many places. Books that cover the later history have found it hard to resist the temptation of revisionism, caricaturing Bohr, Heisenberg and the dominant "Copenhagen interpretation" while making heroes instead of David Bohm, John Bell and Hugh Everett.

Ball has little to say about the personalities involved, but instead seriously engages with the central troublesome issues of the quantum mechanical picture of the world. The Copenhagen interpretation is given a fair treatment, as a warning about the limits one runs up against trying to reconcile the quantum mechanical and classical pictures of reality.

Instead of spending a lot of time in the rut of Bohmian mechanics, Ball dismisses it quickly as

> But it is hard to see where the gain lies… Even Einstein, who was certainly keen to win back objective reality from quantum theory's apparent denial of it, found Bohm's idea 'too cheap."

Dynamical collapse models like GRW also get short shrift:

> It's a bodge, really: the researchers just figured out what kind of mathematical function was needed to do this job, and grafted it on… What's more of a problem is that there is absolutely no evidence that such an effect exists.

As for the "Many-Worlds Interpretation", which in recent years has been promoted in many popular books, Ball devotes a full chapter to it, not because he thinks it solves any problem, but because he thinks it's a misleading and empty idea:

> My own view is that the problems with the MWI are overwhelming – not because they show it must be wrong, but because they render it incoherent. It simply cannot be articulated meaningfully… The MWI is an exuberant attempt to rescue the 'yes/no', albeit at the cost of admitting both of them at once. This results in an inchoate view of macroscopic reality suggests we really can't make our macroscopic instincts the arbiter of the situation…
> Where Copenhagen seems to keep insisting 'no,no and no', the MWI says 'yes, yes and yes'. And in the end, if you say everything is true, you have said nothing.

There's a lot of material about serious efforts to go beyond Copenhagen, by understanding the role that decoherence and the environment play in the emergence of classical phenomena out of the underlying quantum world. This discussion includes a good explanation of the work of Zurek and collaborators on this topic, including the concept of "Quantum Darwinism".

The last part of the book is up to date on what seem to be some currently popular ideas about the foundations of quantum mechanics. One aspect of this goes under the name "Quantum Reconstruction", the attempt to derive the supposedly hodge-podge axioms of quantum theory from some more compelling fundamental ideas, hopefully the kind your grandmother can understand. These ideas are conjectured to somehow have to do with "information" and limits on it. I'm not sympathetic to these, since the axioms seem to me not "hodge-podge", but connected to the deepest unifying ideas of modern mathematics. At the same time, I remain confused about what "information" is supposed to be and how these new foundations are supposed to work. And, as far as I've ever been able to tell, these are not things your grandmother is likely to understand, unless your grandmother is Scott Aaronson…

Comments

1. **Ralph Ballart**
   September 5, 2018

   I'm pleased he discusses Griffith's Consistent Quantum theory which to me is the most satisfying interpretation. I especially like the idea that experiments actually measure what's there prior to the measurement and that wave functions don't actually collapse.
   Griffith's book is available at Amazon or free to download here http://quantum.phys.cmu.edu/CQT/

2. **Dave Miller in Sacramento**
   September 6, 2018

   Peter,

   I agree with you (and Einstein!) that Bohmian mechniacs is "too cheap." But, it should also be mentioned that making Bohmian mechanics consistent with special relativity involves kludges that are unbelievably artificial.

   The interest of Bohmian mechanics lies simply in the fact that it shows that something that von Neumann thought he had proved to be impossible is in fact possible, even if it is not plausible. In short, Bohm widened the possibilities of thought aboout QM, even if his own theory is almost certainly not true (in his later writings, Bohm basically admitted that Bohmian mechnics was more a toy model than a likely candidate for a true theory).

   Bohm's model does seem to have been one of the motivcations behind Bell's work, and Bell seems to have been one of the founts for the work based on entaglement (quantum computing, quantum encryption, etc.) of recent decades.

   On MWI, I think we also should mention that the technical names for the two big problems are "the preferred-basis problem" and the "probability-measure problem": in ordinary English, why does the multi-world universe split into classical separate worlds in just the way it does, and how do you get probabilities out of the theory when in fact the probability that anything (i.e., everything!) happens is always 1.000. The MWI proponents have been wrestling over these problems for decades and never have come to a resolution that satisfies everyone: personally, I doubt they ever will.

There are certainly various hints that QM somehow has something to do with "information," but I have never seen anyone spell out the details.

From all of which I conclude that still no one truly understands quantum mechanics.

Dave

3. **Scott Aaronson**
September 6, 2018

The "derivations" of QM from information-theoretic principles, for example those of Hardy and Chiribella et al., are beautiful and surprising and interesting. They underscore just how little mathematical freedom you have in trying to generalize probability theory the way that QM does. But their wider meaning remains unclear.

Even if I *were* your grandmother, I can attest that I wouldn't find it particularly obvious a-priori that the "pure states" of a theory (meaning, those not nontrivially expressible as probabilistic mixtures of other states) should all be continuously transformable into each other, or that it should be possible to purify any mixed state in an essentially unique way by passing to a larger system. Indeed, these don't strike me as statements that anyone would've even thought to formulate, had they not had the example of QM! I would've treated it as even less obvious that any compound mixed state must be recoverable from the statistics of local measurements on its components. The latter axiom needs to be invoked, in almost all informational derivations, to rule out the restriction of QM to real amplitudes only, and to explain why amplitudes are complex numbers.

If a 19th-century mathematician had wanted to invent QM from first principles, with zero guidance from experiment, it still seems to me that their best bet would've been to ask themselves, "what's the best way to generalize the rules of probability to involve negative and complex numbers?"

In any case, I see the informational derivations as trying to answer a question that's almost orthogonal to the measurement problem that one cares about in interpretation of QM. Instead of asking what kind of "reality" (if any) the mathematical formalism is describing, we're simply asking how one could've guessed, from general principles, that this particular mathematical formalism would be the right one. (I.e., we're asking whether QM has a 'derivation' as compelling as Einstein's 1905 derivation of the Lorentz transformations.)

4. **Urs Schreiber**
September 6, 2018

There is a kind of derivation of principles of quantum physics by following the tao of stable homotopy theory, I discuss that in arXiv:1402.7041. This exhibits quantization as closely akin to the passage to "motives", hence comes with some evidence that it is possible to arrive at this solely from thinking about first principles.

A key idea here is that, in view of the foundational role of homotopy theory, it is naive to assume that rings (as in: numbers) in fundamental physics are necessarily as taught in high-school, such as the rings of real or complex numbers; instead they may be "rings up to coherent homotopy", technically known as "E-infinity ring spectra". The above note considers a god-given concept of path-integral with phases not in the ring C, but in the E-infinity ring KU, and proves that this reproduces geometric quantization at least of compact Poisson manifolds.

5. **zzz**
September 6, 2018

One of your Becker book review updates had a quantum superposition effect on the text.

"collapse of the state vBeyond Weirdector with probabilities given by the Born rule"

6. **tulpoeid**
September 6, 2018

From the description in the first link:

"We now realise that quantum mechanics is less a theory about particles and waves, uncertainty and fuzziness, than a theory about information: about what can be known and how. "

What is that supposed to mean? I'm asking in earnest. Have duality and uncertainty been abandoned recently? I don't know if it refers only to the "last part of the book" but it sounds more generic.

7. **ilovecats**
September 7, 2018

> I'm not sympathetic to these, since the axioms seem to me not "hodge-podge", but connected to the deepest unifying ideas of modern mathematics.

.

Where might one read more about this connection? (I'm a math student, so not too good with the physics)

8. **LMR**
September 7, 2018

Just a quick elaboration of the comment by Urs: the passage to "motives" mentioned there seems to be related to the fact that the category of spans (or correspondences) is a dagger-compact category. It turns out that quantum computation (or the finite parts of quantum mechanics anyway) can be formulated in terms of dagger-compact categories (à la Abramsky-Coecke).

Relevant nLab pages:

https://ncatlab.org/nlab/show/span

https://ncatlab.org/nlab/show/dagger-compact+category

tulpoeid wrote:

"Have duality and uncertainty been abandoned recently? I don't know if it refers only to the 'last part of the book' but it sounds more generic."

Uncertainty hasn't been abandoned, but the book argues that the idea of particle-wave duality may not be as helpful as thinking about quantum mechanics as a theory of information.

9. **Paul Hayes**
September 7, 2018

I think a 19th century mathematician's best bet would've been to "view the algebra of random variables and their expectation values as the foundational concept" in a generalisation of probability theory – as Segal eventually did – so that it could accommodate their invention of QM.

10. **Peter Woit**
September 7, 2018

Scott,

Thanks for the comments! I don't see any particular reason you should be able to "guess" QM from some general principles about how the world is supposed to work, but maybe it is useful to categorize the constraints imposed by various assumptions.

Urs,
To explain that to your grandmother, she would have to be Grothendieck…

zzz,
Thanks, fixed.

ilovecats,
See my book
http://www.math.columbia.edu/~woit/QM/qmbook.pdf

All,
Please resist the temptation to use this comment section to discuss your favorite ideas about QM, and stick to comments of relevance to the Ball book.

I'm resisting temptation to go on about such ideas, but at least I can put them in another posting, which I may do soon…

11. **Peter Morgan**
September 7, 2018

Peter, I try never to forget your injunction to "remember that this is not a general physics discussion board, or a place for people to promote their favorite ideas about fundamental physics", but it's all too easy to forget during the chase that what seems to me like a reasonable response to Scott's comment that 'If a 19th-century mathematician had wanted to invent QM from first principles, with zero guidance from experiment, it still seems to me that their best bet would've been to ask themselves, "what's the best way to generalize the rules of probability to involve negative and complex numbers?"' might not seem so reasonable to you.

I'll repeat the first paragraph of my bad comment: "I went to the trouble of obtaining the English edition because Philip Ball is visiting Yale's YQI this Fall. I also enjoyed it and very much recommend it. That is, I found its sensibilities much more like my own than I've found of most recent popular books."

12. **alex**
September 7, 2018

A small remark related to the last paragraph about the so-called 'quantum reconstruction'. It's not about my favorite approach, but just a historical observation. When I read about these extensions of probability, the first thing I thought was that this was already been done 50 years ago, and pretty convincingly so. In the so-called 'quantum logic' approach, von Neumann, already in the 30s, introduced the non-Boolean space of projectors in a Hilbert space as the 'event space' for quantum probability, which replaces the standard Boolean one of classical probability (realized as the borel algebra of subsets of phase space). Then in the 50s Mackey conjectured that the only possible quantum probability measures on such lattice were the ones given by the standard Born rule via the usual trace and density matrix formula. This conjecture was then proved to be true by Gleason, in a landmark theorem. Mackey also conjectured, on some of von Neumann's old ideas, that the necessity of the lattice of projectors in quantum physics could perhaps be derived as a consequence of imposing some restrictions on an abstract lattice, in particular, orthomodularity. In the 60s, Piron proved that this implied that this abstract lattice is necessarily isomorphic to the lattice of closed subspaces on some generalized Hilbert space. Then, finally, in the 90s, Sóler proved that this generalized Hilbert space can only be real, complex, or quaternionic. Recently, Moretti proved that Poincaré symmetry makes the real case to collapse to the complex one. Thus, in a very venerable line of research which goes back to von Neumann himself, the standard textbook axioms of quantum mechanics have already been reconstructed from a generalization of probability (in particular, the lattice of events is allowed to be non-Boolean, that's why it's called generalized). This is the standard approach and is the one which is discussed in most of the mathematical physics literature. It also leads directly to the Topos reformulation of those axioms. Another reconstruction is the GNS construction in the C*-algebraic approach. So, having all those long established results, which are mathematically rigorous and crystal clear in their assumptions, why do we need another one based on the loose notion of 'information' is something I don't see.

13. **Urs Schreiber**
September 8, 2018

LMR,

absolutely, relation to dagger-structure is discussed in section 4.5.

14. **Michael Rivard**
September 9, 2018

Peter,

Thank you for publicizing Phillip Ball's book and linking to Natalie Wolchover's review of it. I look forward to receiving my copy from Amazon and learning why none of my pets have ended up in a superposition of dead and alive states 😉.

Is Quantum Mechanics a Probabilistic Theory?

September 7, 2018
Categories: Quantum Mechanics

There is a simple question about quantum theory that has been increasingly bothering me. I keep hoping that my reading about interpretational issues will turn up a discussion of this point, but that hasn't happened. I'm hoping someone expert in such issues can provide an answer and/or pointers to places where this question is discussed.

In the last posting I commented that I'm not sympathetic to recent attempts to "reconstruct" the foundations of quantum theory along some sort of probabilistic principles. To explain why, note that I wrote [a long book about quantum mechanics](), one that delved deeply into a range of topics at the fundamentals of the subject. Probability made no appearance at all, other than in comments at the beginning that it appeared when you had to come up with a "measurement theory" and relate elements of the quantum theory to expected measurement results. What happens when you make a "measurement" is clearly an extremely complex topic, involving large numbers of degrees of freedom, the phenomenon of decoherence and interaction with a very complicated environment, as well as the emergence of classical behavior in some particular limits of quantum mechanics. It has always seemed to me that the hard thing to understand is not quantum mechanics, but where classical mechanics comes from (in the sense of how it emerges from a "measurement").

A central question of the interpretation of quantum mechanics is that of "where exactly does probability enter the theory?". The simple question that has been bothering me is that of why one can't just take as answer the same place as in the classical theory: in one's lack of precise knowledge about the initial state. If you do a measurement by bringing in a "measuring apparatus", and taking into account the environment, you don't know exactly what your initial state is, so have to proceed probabilistically.

One event that made me think more seriously about this was watching [Weinberg's talk about QM at the SM at 50 conference](). At the end of this talk Weinberg gets into a long discussion with 't Hooft about this issue, although I think 't Hooft is starting from some unconventional point of view about something underlying QM. Weinberg ends by saying that Tom Banks has made this argument to him, but that he thinks the problem is you need to independently assume the Born rule.

One difficulty here is that you need to precisely define what a "measurement" is, before you can think about "deriving" the Born rule for results of measurements, and I seem to have difficulty finding such a precise definition. What I wonder about is whether it is possible to argue that, given that your result is going to be probabilistic, and given some list of properties a "measurement" should satisfy, can you show that the Born rule is the only possibility?

So, my question for experts is whether they can point to good discussions of this topic. If this is a well-known possibility for "interpreting" QM, what is the name of this interpretation?

Update: I noticed that in 2011 Tom Banks wrote a detailed account of his views on the interpretation of quantum mechanics, [posted at Sean Carroll's blog](), with an interesting discussion in the comment section. This makes somewhat clearer the views Weinberg was referring to. To clarify the question I'm asking, a better version might be: "is the source of probability in quantum mechanics the same as in classical mechanics: uncertainty in the initial state of the measurement apparatus + environment?". I need to read Banks more carefully, together with his discussion with others, to understand if his answer to this would be "yes", which I think is what Weinberg was saying.

Update: My naive questions here have attracted comments pointing to very interesting work I wasn't aware of that is along the lines of what I've been looking for (a quantum model of what actually happens in a measurement that leads to the sort of classical outcomes expected, such that one could trace the role of probability to the characterization of the initial state and its decomposition into a system + apparatus). What I learned about was

- Work of Klaas Landsman, I liked a lot his survey article on the measurement problem
  https://link.springer.com/chapter/10.1007/978-3-319-51777-3_11
  where he describes the "flea on Schrodinger's cat" speculative idea as an example of the "instability" approach to the measurement problem. Also interesting is his paper on "Bohrification"
  https://arxiv.org/abs/1601.02794
- Work on a detailed analysis of a "Curie-Weiss" model of a measurement. The authors have a long, detailed expository paper
  https://arxiv.org/abs/1107.2138
  and more explanation of the relations to measurement theory here
  https://arxiv.org/abs/1303.7257
  and here
  https://link.springer.com/chapter/10.1007%2F978-3-319-55420-4_9
  There are also comments about this from Arnold Neumaier here
  https://physicsoverflow.org/39105/curie-weiss-model-of-the-quantum-measurement-process

In these last references the implications for the measurement problem are discussed in great detail, but I'm still trying to absorb the subtleties of this story.

I'd be curious to hear what experts think of Landsman's claim that there's a possible distinct "instability" approach to the measurement problem that may be promising.

Update: From the comments, an explanation of the current state of my confusion about this.

The state of the world is described at a fixed time by a state vector, which evolves unitarily by the Schrodinger equation. No probability here.

If I pick a suitable operator, e.g. the momentum operator, then if the state is an eigenstate, the world has a well-defined momentum, the eigenvalue. If I couple the state to an experimental apparatus designed to measure momenta, it produces a macroscopic, classically describable, readout of this number. No probability here.

If I decide I want to know the position of my state, one thing the basic formalism of QM says is "a momentum eigenstate just doesn't have a well-defined position, that's a meaningless question. If you look carefully at how position and momentum work, if you know the momentum, you can't know the position". No probability here.

If I decide that, even though my state has no position, I want to couple it to an experimental apparatus designed to measure the position (i.e. one that gives the right answer for position eigenstates), then the Born rule tells me what will happen. In this case the "position" pointer is equally likely to give any value. Probability has appeared.

So, probability appeared when I introduced a macroscopic apparatus of a special sort: one with emergent classical behavior (the pointer) specially designed to behave in a certain way when presented with position eigenstates. This makes me tempted to say that probability has no fundamental role in quantum theory, it's a subtle feature of the emergence of classical behavior from the more fundamental quantum behavior, that will appear in certain circumstances, governed by the Born rule. Everyone tells me the Born rule itself is easily explicable (it's the only possibility) once you assume you will only get a probabilistic answer to your question (e.g. what is the position?)

A macroscopic experimental apparatus never has a known pure state. If I want to carefully analyze such a setup, I need to describe it by quantum statistical mechanics, using a mixed state. Balian and collaborators claim that if they do this for a specific realistic model of an experimental apparatus, they get as output not the problematic superposition of states of the measurement problem, but definite outcomes, with probabilities given by the Born rule. When I try and follow their argument, I get confused, realize I am confused by the whole concept: tracking a mixed quantum state as it evolves through the apparatus, until at some point one wants to talk about what is going on in classical terms. How do you match your classical language to the mixed quantum state? The whole thing makes me appreciate Bohr and the Copenhagen interpretation (in the form "better not to try and think about this") a lot more…

Comments

1. **Another Anon**
   September 7, 2018

   If you want to avoid the tricky question "What is a measurement?", just consider an electron orbiting a nucleus, jumping to a lower energy state. Einstein asked what caused the jump. Bohr said it was probabilistic. So there's an example of fundamental probability entering QM which avoids the "What is a measurement?" problem

2. **Charles Xu**
   September 7, 2018

   It doesn't quite get at the interpretational issues you raised earlier (in that it assumes an underlying Hilbert-space description), but Gleason's theorem https://en.wikipedia.org/wiki/Gleason%27s_theorem comes close to the kind of functional derivation of Born's rule you're asking about.

3. **Peter Woit**
   September 7, 2018

   Another Anon,
   The situation you describe involves not just a single particle in a potential (where an energy eigenstate will stay an energy eigenstate), but a particle coupled to a quantized electromagnetic field. In this case, you don't know exactly what the initial quantum state of the electromagnetic field is, no? You also don't know the state of the environment, and are going to have to perform a measurement to detect the photons that show that the electron has changed energy levels.

4. **Mateus Araújo**
   September 7, 2018

   There is a simple answer to your simple question: probability can come from lack of knowledge of the initial state.

   The problem is that it is not at all easy to recover the probabilistic predictions of quantum mechanics from this idea. The only way people found to do it is with Bohmian Mechanics. Which is not a satisfactory theory, in my opinion.

5. **Oren**
   September 7, 2018

   This, in particular the discussion around Gleason's theorem, seems relevant. https://www.scottaaronson.com/democritus/lec9.html

6. **Peter Woit**
   September 7, 2018

   Mateus Araújo,

   I'm not interested in what I suspect 't Hooft or the Bohmians would like to do, deriving the Born rule from some underlying more classical picture. I'd like to know if you can do it starting with standard QM initial states, can you point to literature where people try to do this? I seem to be hearing from some quarters that Gleason's theorem or other very general arguments can constrain probabilities and perhaps give Born's rule, is that not the case?

   Also, where can I find a precise definition of a "measurement"? Or, not generally, but specifically, a detailed analysis of exactly what is happening in some simple case of a measurement?

7. **Another Anon**
   September 7, 2018

   Mateus: "There is a simple answer to your simple question: probability can come from lack of knowledge of the initial state."

   Yes it can, but that's not the fundamental uncertainty of quantum mechanics – that is not about lack of knowledge.
   The uncertainty principle explains why quantum mechanics must be probabilitic. If you have two peroperties which don't commute, then you have fundamental limitations on your knowledge. In which case, you can only ever talk about the probability of the position of a particle, say, rather than knowing with certainty.

8. **Perry Rice**
   September 7, 2018

   Not all measurements are projective, so that needs to be taken into account.

9. **Peter Woit**
   September 7, 2018

   Another Anon,
   The fact that quantum states are different than classical states is a different issue, and has nothing to do with the question about the Born rule.

   For example, I can start with a state of a single particle. Quantum mechanics tells me what such states look like, there aren't any that are both position and momentum eigenstates. If I start with a single particle in a momentum eigenstate, I can ask what happens when I "measure its position". To do this I'm going to have to couple it to some very complicated (as a quantum system) measuring device + environment. Born's rule says that the probability of any position will be the same. Can I derive this from some very general characterization of a what a measuring device is and how an environment behaves?

10. **Peter Woit**
    September 7, 2018

    Perry Rice,
    Can you point to a precise definition of what a "measurement" is?

11. **Mateus Araújo**
    September 7, 2018

Peter Woit,

"I'd like to know if you can do it starting with standard QM initial states, can you point to literature where people try to do this?"

So what you want to know is if one can get probability from a lack of knowledge of the initial quantum state? The only work I know in this direction is A Flea on Schroedinger's Cat. The authors are more concerned about causing a collapse of the wavefunction by introducing a perturbation in the effective Hamiltonian of a system (which can always be interpreted as a change in the quantum state of the measurement apparatus), but since the perturbation determines the result of the measurement, lack of knowledge about the perturbation is the source of probabilities in their model. The paper is just a numerical analysis of a double-well model, so it is more of a hope instead of worked-out solution.

As for Gleason's theorem, it shows that if the probability of an outcome is a (non-contextual) linear function of the projector associated with that outcome, then it must be given by the Born rule. So if you want non-Born probabilities you either need to give up on describing measurements with projectors or do some really funky stuff. But it says nothing about whether the probabilities come from lack of knowledge of a initial state.

There are dozes of derivations of Born's rule from general arguments, it is really hard to get a sensible theory without it.

12. **Kevin S. Van Horn**
September 7, 2018

I can't claim to be an expert here, but have you looked at Sebens and Carroll's paper "Self-Locating Uncertainty and the Origin of Probability in Everettian Quantum Mechanics"?
https://arxiv.org/abs/1405.7577

13. **RalphB**
September 7, 2018

Thank you so much for the link to Weinberg's talk. It's interesting that he lumps Everett in with the consistent quantum theory folks (Gell-Mann, Hartle, Griffiths). Consistent quantum theory defines measurements but I assume that's not what you are looking for.
Finally, I link to a short clip of Gell-Mann which basically summarizes why I like his approach to quantum theory.
https://www.youtube.com/watch?time_continue=127&v=gNAw-xXCcM8

14. **Peter Woit**
September 7, 2018

Kevin S. Van Horn,
I have looked at that, but it seems to me to be a more elaborate and less clear version of Zurek's 2004 argument for a derivation of the Born rule, see here
https://arxiv.org/abs/quant-ph/0405161

I should have pointed to Zurek's argument in my posting. My question might be rephrased as "does something like Zurek's argument give what I want?"

15. **Joking**
September 7, 2018

The standard version of Gleason's theorem tells you that if you assume that your physical theory is a probabilistic interpretation of a Hilbert space structure, then those probabilities have to be calculated according to Born's rule. So when people worry about "deriving the Born rule", they usually don't mean its particular form, but rather the very probabilistic interpretation itself. Asking for a "derivation" of this interpretation as you do is asking for a hidden variables description, which is impossible — For an "alternative" to Bohmian mechanics that seems less well known (and fails for different reasons), you might want to look at Wetterich's work on "quantum mechanics from classical statistics" — Derivations of the Hilbert space structure from axioms of physical measurements were given by Jauch and his followers, at a time when Boolean lattices were a popular subject. At their best, the "information theoretic derivations" are a modern reincarnation of these older insights.

16. **Peter Woit**
September 7, 2018

Joking,
I'm not asking for a hidden variables description. I think the quantum state is a complete description of the state of the world, there is nothing else. But when you discuss going from an initial state to a final state via a process that is supposed to qualify as a "measurement", you are assuming a measurement apparatus of some kind (and an environment), and it seems to me that you aren't going to know the initial state of this apparatus + environment.

Can this lack of knowledge about the initial state explain why your predictions about measurements have to be probabilistic? Can Gleason or Zurek be invoked to explain why Born's rule must govern these probabilities?
I suspect what is confusing here are issues of circularity: you have to have some theory that tells you how to deal with your lack of knowledge of the initial state, and one may argue there is no way to do this without invoking Born's rule itself.

Still, what's wrong with the argument that here is the place that probability enters, and Born's rule is the only consistent way it can enter?

17. **alex**
September 7, 2018

I think there are two aspects. One is, in the mathematics of Hilbert spaces, non-Boolean lattices, or whatever mathematical structure one prefers to write the mathematical formalism of qm, what liberty one has for the mathematical form of the Born rule?. This is a purely mathematical question, which has been, in my opinion and taste, pretty much sorted out since the 50s by Gleason's theorem. Now, assume for a moment that we physically interepret this probability measure as some sort of real property of the particle, say its 'propensity'. Now, when this particle interacts with a device, we randomly get a value for, say, spin, and the probability distribution of those values fits with the previous Born probability measure that the particle had before the interaction. Now, one asks, how is it that in such a complex dynamical interaction, the probability is somehow 'transferred' from the particle, before the interaction, to the distribution of the measured values? Is the sole role of the interaction just to produce a single outcome without affecting anything else? This would be the second aspect, and, if I understand correctly, the one that troubles Peter. And, indeed this seems to be more a complex physical question rather than a purely mathematical one, on that needs some physical modeling of the situation and some adequate physical insight and interpretation of what's going on there. Of course, since this problem has not been solved yet, one can either make an operationalist physical interpretation of the mathematical formalism of qm (in which one physically interprets the Born measure as the experimental frequency of values in measurements, thing which simply builds on the behaviour observed on the second issued, and just takes it for granted to build a pragmatic interpretation), or, in the case in which the probability is, say, propensity, use the law of large numbers to get the frequency and, again, accept that this probability is transferred to the results of experiments or try to investigate why this is the case. Only this second case has some right to do that, since the operationalist take simply incorporated it into the very way in which one physically interprets the math. Back to the other approach, one can take three positions, either the known quantum dynamics will explain how this happens (thing which leads to the measurement problem, since usual dynamics+no hidden variables+defined outcomes is a trilemma), and then one has to deny defined outcomes in the usual sense (this leads to many worlds or to Landsman flea on S's cat), or (non-local, contextual-)hidden variables (in which the mechanism that makes the values to define is outside standard qm, is an outside variable; in this case, the probabily is, of course, not a real property, but, still one has to explain why the measurement context doesn't change it, like in the other approach), or one denies the standard dynamics (like GWR).

18. **Blake Stacey**
September 7, 2018

I think Adrian Kent's critique of the Sebens–Carroll business is exactly on target.

Everything I have seen in the Zurekian vein ends up being circular, though some arguments have a larger radius of curvature than others.

19. **Blake Stacey**
September 7, 2018

> What I wonder about is whether it is possible to argue that, given that your result is going to be probabilistic, and given some list of properties a "measurement" should satisfy, can you show that the Born rule is the only possibility?

As others have said, Gleason's theorem is exactly this, but the "list of properties a 'measurement' should satisfy" is apt to be unsatisfying if one wants a story about energy flow or mechanical transformations. Instead, the assumptions are a list of conditions for expressing "measurements" in terms of Hilbert spaces. To each physical system we associate a complex Hilbert space, and each measurement corresponds to a resolution of the identity operator — in Gleason's original version, to an orthonormal basis. The crucial assumption is that the probability assigned to a measurement outcome (i.e., to a vector in a basis) does not depend upon which basis that vector is taken to be part of. The probability assignments are "noncontextual", as they say. The conclusion of Gleason's argument is that any mapping from measurements to probabilities that satisfies his assumptions must take the form of the Born rule applied to some density operator. In other words, the theorem gives the set of valid states and the rule for calculating probabilities given a state.

I have a sneaky suspicion that a good many other attempted "derivations of the Born rule" really amount to little more than burying Gleason's assumptions under a heap of dubious justifications.

20. **Scott Aaronson**
September 7, 2018

I agree with almost everything Mateus said. If you want the randomness of quantum measurement outcomes to be due solely to lack of knowledge about the initial state, and you also want to reproduce all the predictions of QM correctly, and you also don't want an insane cosmic conspiracy ('t Hooft's "superdeterminism"), then you're pretty much going to be forced to something like Bohmian mechanics—which, in some sense, does exactly what you're asking for, but only at the cost of importing the entire ontology of ordinary QM and then adding something additional and underdetermined on top of it. A few people had placed hopes in so-called "psi-epistemic" theories, wherein each pure state would correspond to a probability distribution over underlying "ontic states," with the distributions for non-orthogonal states overlapping each other—but I would say that no-go theorems have basically ruled that idea out, for explaining anything more complicated than a single qubit. Reproducing the probabilistic predictions of QM from ordinary statistical ignorance is not actually that easy! Think for example about the Bell experiment, or Shor's factoring algorithm, or better yet a quantum algorithm that simply outputs a sample from some crazy distribution over n-bit strings (what's now called a "quantum supremacy experiment"). If the probabilities in such cases merely reflected ignorance of the initial state, then it must be in a radically different way from what anyone is used to, a way that involves nonlocality and exponential computational complexity and so forth. Which … to my mind, if you're going to have to swallow all those things anyway, then why not just accept the probabilities as fundamental and be done with it? 🙂

The question "why the Born rule?" is a different one, at most indirectly related to the problem of finding an ignorance/statistical interpretation of QM. To my mind, Gleason's theorem, and many other results, have made a pretty compelling case that once you've
(1) assumed the "non-probabilistic" part of QM (the state space, orthogonality relations, etc.), and also
(2) decided that you want SOME rule for converting amplitudes to probabilities,
nothing other than the Born rule really makes internal mathematical sense. And intuitively, this is not surprising. After all, the non-probabilistic part of QM already singles out the 2-norm of the amplitude vectors as their most basic conserved property. So then if your probability law isn't also based on the 2-norm, but on some other norm or whatever, then no duh you're going to have trouble with conservation of probability!

21. **akhmeteli**
September 7, 2018

Peter Woit wrote: "Also, where can I find a precise definition of a "measurement"? Or, not generally, but specifically, a detailed analysis of exactly what is happening in some simple case of a measurement?"

I would think https://arxiv.org/abs/1107.2138 (Phys. Rep. 525 (2013) 1-166) fits the bill. The authors model measurement of a component of a spin using a Curie-Weiss magnet. They derive definite outcome of measurement (as a practical result), although, strictly speaking, this is incompatible with unitary evolution, but the change of the outcome can happen after a long time (of the order of Poincare reversal time). They derive the Born rule as an approximate result.

22. **Peter Woit**
September 7, 2018

akhmeteli,

Thanks! That's exactly the sort of thing I've been looking for and not seeing. It's a long paper and I'll have to find some time to understand what they are doing.

Scott,

Your claims seem incompatible with Weinberg's. He seemed to me to be saying that the problem with getting probability from uncertainty about the initial state is getting the Born rule, you seem to be saying getting the Born rule is straightforward, but there's some other problem with the idea.

I really need to look at and think about the paper akhmeteli points to. But, until I do that, here's the kind of specific question about "measurement" I'm wondering about. As in my earlier comment, consider a free particle in a momentum eigenstate. I think that's a complete description of the particle, has no info about position. If you want to measure position, I don't want to hear some formal abstract characterization of a position measurement. I'd like to see some physical system, describable by quantum mechanics, that you can couple to the free particle in such a way that the final state will give you a "position measurement". My problem is that I haven't seen any analysis of such a system. How do I know that the indeterminacy in the initial state of this "measurement system" doesn't correspond to the indeterminacy in the "position" result?

Blake Stacey,
The Kent paper you link to seems to just be about the many-worlds part of Carrol-Sebens (which I'm willing to believe Kent makes no sense). There's nothing there about the Zurek argument. What's the best paper critically dealing with the Zurek argument (note that Zurek claims he has no need for "many-worlds")?

23. **akhmeteli**
September 8, 2018

to Peter Woit: You are very welcome. Yes, the paper is extremely long, so you may wish to look at their earlier work https://arxiv.org/abs/quant-ph/0702135 first (just 13 pages).

24. **Joking**
September 8, 2018

Peter:

Can this lack of knowledge about the initial state explain why your predictions about measurements have to be probabilistic?

No. Probabilities and Born's rule apply even when you have complete knowledge of the entire pure state. So unitary evolution forbids this kind of mechanism. The environment is necessary to make the quantum mechanical probabilities behave like classical probabilities (with high probability, and for the pointer states).

Can Gleason or Zurek be invoked to explain why Born's rule must govern these probabilities?

Exactly!

I suspect what is confusing here are issues of circularity: you have to have some theory that tells you how to deal with your lack of knowledge of the initial state, and one may argue there is no way to do this without invoking Born's rule itself.

Indeed, there is no non-circular derivation of the probabilistic interpretation of quantum mechanics. The standard fallacy is to say that a probabilistic description follows if we neglect terms that are small in the norm. But neglecting small norms because they correspond to low probability is exactly assuming what one is trying to derive!

Still, what's wrong with the argument that here is the place that probability enters, and Born's rule is the only consistent way it can enter?

There's nothing wrong here. Assuming the relationship between physical measurement and mathematical theory to be probabilistic, Born's rule is the only possibility.

25. **Jochen**
September 8, 2018

Are you looking for something like Mott's 1929 analysis of alpha scattering in cloud chambers? It treats the atoms of the surrounding gas essentially as a quantum mechanical measuring device, and derives the classical straight trajectories from there. It's often considered an important forerunner to decoherence theory. Here is a modern discussion of the paper: https://arxiv.org/pdf/1209.2665

I'm not sure how much it really relates to the probabilistic aspect, though.

On that front, I think a fundamental difference to the classical case is that there, you can at least in principle imagine getting rid of the ignorance about the state of the measurement apparatus, which isn't the case for quantum mechanics. Although I suppose that basically ends up being kinda circular…

Maybe things get clearer if we think about QM in the same arena as classical mechanics, i. e. phase space. Then, the Liouville distribution becomes deformed into the Winger distribution, which describes the quantum state. This now can't ever be perfectly sharp, so if the interpretation of the Liouville distribution carries over, this would mean a residual fundamental uncertainty you can't get rid of.

Trouble with this is, of course, that the Winger distribution isn't a probability distribution, but rather, a pseudoprobability, which can take negative values. So maybe your question becomes one of whether you can attach an ignorance interpretation to such a pseudoprobability?

I know I've seen arguments to that effect, but I don't recall where right now…

26. **Urs Schreiber**
September 8, 2018

> If this is a well-known possibility for "interpreting" QM, what is the name of this interpretation?

I'd say this is the perspective of "algebraic quantum physics" in view of *quantum probability theory* (also "non-commutative probability"), as nicely surveyed for instance in Gleason's "The C*-algebraic formalism of quantum mechanics" (pdf) .

Here the Born rule becomes but the theorem that expectation values in probability theory may equivalently be characterized as star-linear functionals on algebras of observables, commutative or not (algebraic quantum states).

The allegend conundrum of "wave function collapse" disappears in this perspective, becoming but a part of the general formula for conditional expectation values (see here)

27. **Mateus Araújo**
September 8, 2018

Żurek's derivation of the Born rule is, technically speaking, the same thing as Deutsch's, but stripped down of the decision theory stuff and the talk about Many-Worlds (deriving uniform probabilities for uniform superpositions from a symmetry argument, and using ancillas to reduce non-uniform superpositions to uniform superpositions).

He might refuse to talk about Many-Worlds, but is essential for his proof that a measurement is a unitary process, which for me is rather Many-Worldy.

But the main problem with his proof is that he avoids the hard question of what probability \emph{is}, he just assumes that there is a probability that somehow makes sense. This is actually the only point about which I disagree with Scott: one cannot "just accept the probabilities as fundamental and be done with it". One must explain what "fundamental probabilities" even are in order to accept them.

The value of the Deutsch-Wallace argument is that they try to answer this, by proposing that probabilities are only subjective, and should be understood as tools to make decisions in a branching universe. On one hand I find it nice, as it explains why should we regard deterministic branching as a probabilistic process, but on the other hand rather unsatisfactory, as the whole thing about quantum probabilities is that they are somehow objective, and they don't address this.

I recently wrote a paper simplifying the Deutsch-Wallace argument and exploring its objectivity problem, it might interest you.

28. **Mateus Araújo**

September 8, 2018

Joking,

Indeed, there is no non-circular derivation of the probabilistic interpretation of quantum mechanics. The standard fallacy is to say that a probabilistic description follows if we neglect terms that are small in the norm. But neglecting small norms because they correspond to low probability is exactly assuming what one is trying to derive!

This circularity plagues the frequentist derivations of the Born rule, that were not even mentioned here. None of the proofs discussed here suffer from this problem.

Woit,

Your claims seem incompatible with Weinberg's. He seemed to me to be saying that the problem with getting probability from uncertainty about the initial state is getting the Born rule, you seem to be saying getting the Born rule is straightforward, but there's some other problem with the idea.

The solution to the puzzle is that it is easy to get the Born rule from general arguments that are *not* about lack of knowledge of the initial state. If you insist on probabilities coming from lack of knowledge of the initial state, it is rather hard to get the Born rule, and the only success I know is Bohmian Mechanics.

29. **CM**
September 8, 2018

Dear Woit,

the answers to your and Weinberg questions were given in 1932 by Johann Von Neumann in its masterpiece book "Mathematical foundations of quantum mechanics". There is a new 2018 English edition that makes the book more easily readable compared with the 1955 edition. Chapter IV, "The deductive development of the theory", explains why probabilities and derives the "Born rule".

30. **Scott Aaronson**
September 8, 2018

Peter: Weinberg points out, and I agree with him, that there seems to be no way to derive from the unitary part of QM that probabilities should ever enter the theory in the first place, without making some additional assumption. On the other hand, I don't think he disagrees with the statement that, once you've decided you want some probability rule, there are strong mathematical arguments that choices other than the Born rule are going to lead to serious problems (superluminal communication and so forth). If you like, though, I can ask him next time I see him.

31. **Aris Papadopoulos**
September 8, 2018

The paper "Quantum Mechanics of individual systems"

by James Hartle,
American Journal of Physics 36(8)

attempts a "derivation" of the Born rule. I mention it only as an entry point into the literature, by a well-respected, physicist, written in a readable fashion, meant to be understood even by undergraduate physics majors. Of course its conclusions are by no means universally accepted.

32. **GoletaBeach**
September 8, 2018

Gordon Baym's QM text has a useful description of the Stern Gerlach measurement process. I seem to remember that he bridges the gap between the idea that a spin component measurement always gets an eigenvalue and the Born interpretation of the wave function. So perhaps all there is is the Born wave function.

The mathematical arguments are far less interesting to me than experiments that could distinguish between QM interpretations. I guess the Bell's tests where choices are made in a causally disconnected fashion seem to be the most interesting, but I haven't kept up.

33. **Lee Smolin**
September 8, 2018

Dear Peter,

I've just picked up my head from doing the final corrections to my new book on realism in quantum foundations to find you asking, "where exactly does probability enter the theory?"

My understanding, after a lot of study, is that you have the following options:

1) Put the probabilities in at the beginning, as did Bohr, Heisenberg and von Neumann. This requires an operational approach which introduces measurement and probabilities as primitive concepts, ie through a "collapse" or "projection" postulate, which postulates Born's rule and "eigenvalue realism", or through a Hardy-style operational reconstruction. These are elegant but they do not answer your question as measurement and probability are primitive concepts.

2) You can attempt to derive probabilities from a formalism that has only unitary, Schrodinger evolution, which has no notion of probabilities to begin with. This is Everett's MWI route.

This is by now a very long story. It took me a lot of time to sort out for the book, and I had help from Saunders and Wallace and others. At best, there is no consensus amongst experts that this can be done. (This agrees with Scott's remark, above.) The rough outline is

i) the original version due to Everett fails, because you can show that with certainty there are branches of the wavefunction whose observers record measurements that disagree with Born's rule. Because there is no primitive notion of probability you cannot say that these observers are improbable, in fact there are an infinite number of them, and also an infinite number whose observations agree with Born's rule.

ii) There are recently several very sophisticated attempts to derive subjective probabilities and the Born rule. These are centred at Oxford, were initiated by David Deutsch and developed in different versions by Hillary Greaves, Wayne
Myrvold, Simon Saunders and David Wallace. These all use decoherence and also give up on recovering objective probabilities. Instead, they try, (in one version) from the axioms of decision theory, to show that it is rational for an observer to bet (ie choose subjective probabilities) as if Born's rule were true. (Even though objectively Born's Rule is false.)

If you read the literature you can only conclude, after some challenging technical arguments, that the experts disagree about whether this kind of approach succeeds or fails, and what the implications should be.

3) Invent a new physical theory which gives a complete description of individual processes from which the quantum probabilities are derived from ignorance about the initial state. This would then be a completion of QM rather than an interpretation. de Broglie-Bohm and collapse models are existence proofs that this is a possible route. There are also other approaches of this kind, such as Adler's trace dynamics and my real ensemble formulation.

I have the impression you don't find any of these 3 options satisfactory. The kind of answer to your question of where the probabilities come from would be one in which we start with QM without measurement, probabilities etc and derive them. But this was option 2 and a whole lot of very bright people have tried and failed to make it work (in a way that convinces all the experts).

My personal view is that option 3) is the only way forward for physics. But I wouldn't try to do more here than argue that unless some notion of subjective probability can be made to work, as in option 2), you simply cannot get an answer to your question. You then either need to conclude with Bohr that the only kind of theory of atomic phenomena is operational, and has probabilities and measurement as primitive terms
or agree with Einstein, de Broglie, Schrodinger, Bohm, Bell ets that QM requires a completion that gives a complete description of individual experiments.

Thanks,

Lee

34. **Peter Woit**
September 8, 2018

All,
I noticed that Tom Banks in 2011 wrote up a long explanation of his views, posted at Sean Carroll's blog, which attracted an interesting discussion involving Scott and others. I've added a link and some comments as an update to the posting here.

Another good reference for work pointed to by akhmeteli is
https://arxiv.org/abs/1406.5178
This gives the kind of analysis of a measurement I've been asking for, but its implications for the question I'm wondering about are still unclear to me, since the authors are starting from a different "statistical interpretation" point of view. I need to find time to read this and think about it.

Lee,
Thanks for the comment. I look forward to seeing your book. In your categorization, I'm following option 2, and my question is being asked in that context.

Scott,
My initial thought was to ask Banks about his views, since those were what Weinberg was referring to, but as mentioned above, I see there is a good source already for his answer. If you do talk to Weinberg, I would be curious to know more about his discussions with Banks, whether he really would characterize Banks as seeing probability as having the same origin as in classical mechanics, and if so why he sees getting out Born's rule as the problem with this.

35. **Blake Stacey**
September 8, 2018

I hesitate to say what the "best" critiques of the Zurekian program have been, but arXiv:1406.4126 and arXiv:1603.04845 may provide a reasonable jumping-off point.

36. **Peter Woit**
September 8, 2018

Blake Stacey,
Those aren't very convincing. Especially the second: four pages in large type?

The question of how classical emerges from quantum is clearly a complex and subtle subject and I don't see the value of crude arguments like Kastner's four pager.

37. **Blake Stacey**
September 8, 2018

The problem is that in quantum foundations, the state of the literature is appalling. Clear statements of what is presumed in approach X, what is novel about Y, whether Z is seen as successful, etc., are harder to find than they should be. Issues that are, as you say, complex and subtle, get addressed in ways that appear at first to be sophisticated, but then turn out to be passing off intricate notation as subtlety. (Maybe this is not so different from the state of affairs in the study of non-Riemannian hypersquares, now that I think about it.) Sometimes the best one can do is a statement like Mateus Araújo's above: "But the main problem with his proof is that he avoids the hard question of what probability *is*, he just assumes that there is a probability that somehow makes sense." Yet statements that plain don't make for papers in *Stud. Hist. Phil. Mod. Phys.*

(Looking at what I posted before, I think I got snowed under by open arXiv tabs; the second link I meant to give was arXiv:1404.2635, which has a little evaluation of what decoherence can and can't do at the very end.)

38. **Peter Woit**
September 8, 2018

Blake Stacey,
Thanks, that Schlosshauer reference is much more promising. I was impressed by his book on the topic, should go back and look at that again.

The literature on quantum foundations I definitely find frustrating. Part of this is its sheer size, part of it is that it's difficult to figure out what people are actually saying, with, beside lots of notation and formalism, thickets of natural language words applied to contexts outside of where those words have clear meanings.

39. **Blake Stacey**
September 8, 2018

An older paper by Schlosshauer and Fine, arXiv:quant-ph/0312058, goes into more detail about where probabilities are introduced so that probabilities can come back out.

40. **Bruno Galvan**
September 9, 2018

A possibility (maybe the only) for the answer to the question "is the source of probability in quantum mechanics the same as in classical mechanics: uncertainty in the initial state of the measurement apparatus + environment?" is yes (without introducing hidden variables) is that there is no collapse, quantum states evolve always deterministically, and the different outcomes of a measurement depend on different initial microstates of system + apparatus + environment. A line of research in this sense has been developed by Schulman in his book [Time's arrows and quantum measurement](#) (from chapter 6). In order that the final state of the laboratory is not a superposition of macroscopically different states, the initial microstate must by hyperfine-tuned. To avoid this drawback Schulman proposes two-times boundary conditions, with a teleological flavor similar to that of the least action principle.

41. **Lee Smolin**
September 9, 2018

Dear Peter,

I appreciate you are trying to follow path 2: "attempt to derive probabilities from a formalism that has only unitary, Schrodinger evolution, which has no notion of probabilities to begin with". The point of my remark is that this is much harder than seems at first. A lot of really smart people have devoted years to trying to make this work and have not convincingly succeeded. Several arguments such as Everett's original attempt, and related arguments of Hartle, Finkelstein, Banks, etc. turn out to be circular because they sneak in a measure related to probability and/or a special role for measurement. Then there are issues with the use of decoherence first pointed out by Abner Shimony, because the dynamics is unitary and reversible so there is a quantum Poincare time after which the state recoheres. So if you attempt to argue that decoherence defines the branches you can't get an irreversible outcome to associate objective probabilities to.

It thus seems you also have to give up objective notions of probability so what you end up trying to show is that observers should chose their subjective probabilities as if Born's rule is correct, when it is actually false. Would this much weaker notion of probability satisfy you?

So my query to you? What are you willing to give up in your beliefs about probability to make route 2 succeed?

Thanks,

Lee

42. **Anonymous**
September 9, 2018

> the dynamics is unitary and reversible so there is a quantum Poincare time after which the state recoheres. So if you attempt to argue that decoherence defines the branches you can't get an irreversible outcome to associate objective probabilities to.

So what? The same problem exists in the derivations of thermodynamics from the equations of classical mechanics, because they are time-reversible and even if the dynamics is chaotic the Liouville theorem still stands and you get Poincare returns. Boltzmann knew that his "mechanical" entropy wasn't really time-irreversible. Supposedly he riposted to Zermelo arguing that the entropy will go back to the original value after Poincare time with "you'll have to wait long" and to Loschmidt arguing that the entropy will go back to the original value if the velocities of all particles were to be reversed with "try to reverse them". Has this ever been satisfactorily solved? I've read that Prigogine had tried to, but his mathematics didn't quite work.

43. **Another Peter**
September 9, 2018

It seems you might be interested in looking at

http://www.compphys.org/pdf/Ann.Phys347.45.2014.pdf

and some of the several follow-up papers that they have.

44. **RBG**
September 9, 2018

I'm not smart enough to know how useful it is, but this topic of probability in the foundation of quantum mechanics is discussed in detail in several chapters of Karl Popper's autobiography Unended Quest.

45. **Mateus Araújo**
September 9, 2018

Dear Lee Smolin,

Are you by any chance confusing Everett's derivation with the old frequentist derivations by Graham, DeWitt, and others? Because those derivations do suffer from the problem you point out (and more), but Everett's does not. He did derive the Born rule to start with, and having it he can legitimately claim that observing non-Born relative frequencies is improbable. The problem with his derivation is that he starts from extremely strong assumptions (essentially that the probability of a branch i depends only on its amplitude \alpha_i, and that states must be normalised with the 2-norm), so nobody regards his derivation as better than just postulating the Born rule.

Also, I don't understand what you mean by "objectively Born's Rule is false" in the Deutsch-Wallace approach. They regard Born's rule as a guide to rational action, and prove that you should follow it. What's false about it?

46. **Peter Woit**
September 9, 2018

Mateus Araújo,

I finally got a chance to start looking at
https://arxiv.org/abs/quant-ph/0405161
which is the kind of thing I was looking for. I've gotten a lot out of other things Klaas Landsman has written, looking through his papers I'm interested to see that he's quite concerned with the subtleties of the classical limit, likely with one motivation of seeing if these give insight into the measurement problem.

47. **Peter Woit**
September 9, 2018

Lee,
The problem here is that I lack much in the way of beliefs about probability to either stick to or give up. And I know that you're correct that trying to get probability from the quantum formalism minus probability leads to great difficulties (and circularities).

I guess my fundamental point of view though is that I find the QM axioms (minus probability) to be part of an extremely compelling mathematical picture, and the probability-based results to be extremely well tested, all of this so much so that I'm not optimistic you're going to find room for something different. I'd be a lot more convinced there was a problem here that needed some new ingredient to explain if I had in hand a detailed quantum model of a realistic measurement, and could be sure that the apparent "measurement problem" was real, and not getting resolved in a subtle way.

48. **akhmeteli**
September 9, 2018

Peter Woit wrote: "I guess my fundamental point of view though is that I find the QM axioms (minus probability) to be part of an extremely compelling mathematical picture, and the probability-based results to be extremely well tested, all of this so much so that I'm not optimistic you're going to find room for something different."

Irreversibility is also extremely well tested, yet, strictly speaking, it is incorrect. The results by Allahverdyan, Balian, Nieuwenhuisen that I quoted show that the projection postulate and the Born rule are good approximations, but still approximations, so they cannot be derived from quantum axioms minus probability as exact laws (let me also note that the projection postulate and the Born rule do not reflect the fact that any measurement requires some time to perform). On the other hand, if one believes that probability-based results are exact laws, one gets some strong results, such as violations of the Bell inequalities in quantum theory, that cannot be proven based on QM axioms minus measurement theory.

As for "room for something different"… It is with some hesitation that I would like to mention mathematical results of my paper http://link.springer.com/content/pdf/10.1140%2Fepjc%2Fs10052-013-2371-4.pdf (Eur. Phys. J. C (2013) 73:2371). One of the models I consider there is scalar electrodynamics (Klein-Gordon field minimally interacting with electromagnetic field). It turns out the Klein-Gordon field can be algebraically eliminated (in the unitary gauge), and the resulting equations for the electromagnetic field describe its independent evolution. This local theory can be embedded into a quantum field theory using generalized Carleman linearization. Thus, even if a quantum field theory's predictions are in agreement with experiments, the results of the experiments can also be explained by a local theory.

49. **GoletaBeach**
September 10, 2018

https://arxiv.org/abs/1406.5178 is a bit peculiar from an experimentalists' perspective… maybe OK but hardly communicative… G. Baym, Lectures on QM, 1969 & 1973, end of chapter 14, much better, at least for those who build experiments. But not sure neutral atoms can be born in EPR-type states… photons present other experimental challenges… would need the equivalent of Baym for the gamma measuring process.

Without practical designs of apparatus, it may be fruitless to speculate.

50. **Peter Woit**
September 10, 2018

Goleta Beach,
Stays away from the measurement problem, but for the experimentally inclined, a wonderful book is Haroche and Raimond, Exploring the Quantum

All,
Thanks for pointers that have been very helpful. I've added an update to the posting listing some of these. All of this has very much deepened my understanding (as well as my confusion…) about these issues.

51. **Eric Dennis**
September 11, 2018

Peter,

You seem to be hoping that the basic problem might be resolved by some deep analysis of a complicated many-body model capturing interactions between a system and a measuring device. But the critical facts are already on the table, and such an analysis doesn't have any room to get around them–namely the facts:

1. An initial system state comprising a superposition of mutually orthogonal terms, unentangled with the measuring device state
2. An entangling (unitary) operation between system and device
3. A resulting superposition of FAPP orthogonal terms, each one with a different system state and corresponding device state

I am curious how you think there is any chance of this final superposition going away in favor of a single term? How could a more detailed analysis of a specific model possibly achieve that?

Have I misunderstood your goal here? If not, Lee Smolin's remark should be clear: there is no way to magically inject the empirical concept of (measurement result) *probability* into this abstract Hilbert space set-up without adding something else. And there is also no way to get a unique measurement result out of that superposition without adding something else. So your solution is going to fall into one of three buckets:

1. Renounce the assumption that there is a unique result
2. Add something else
3. Flip the table over, scream incoherently, and run away

Or using their technical names:

1. Many Worlds
2. "Hidden Variables"
3. Copenhagen, postulated state reduction, QBism, or something similar

52. **Jan Reimers**
September 11, 2018

1. An initial system state comprising a superposition of mutually orthogonal terms, unentangled with the measuring device state
2. An entangling (unitary) operation between system and device
3. A resulting superposition of FAPP orthogonal terms, each one with a different system state and corresponding device state

*****

How can we be sure #3 follows from #2? If I understand Peter's position correctly, the unitary evolution of "device"+"system" is horrendously complicated. The device is almost certainly not in an energy eigen-state at finite temperature, rendering its state is unknown. So the question becomes: can we contrive a simple enough "device" and show what happens after unitary evolution of the interacting system. For example system=1 Qubit and device = N Qubits where N is small enough that we can see what's going on during unitary evolution? **Perhaps** one would find that all coefficients in the superposition except one, approach zero and N grows larger, AND that the one selected to grow with N depends on the initial state of the device (N Qubits).

Is this just wishful thinking?

I would think this is at least worth a shot before giving up and diving into MWI and pilot wave fantasies. Maybe one of the references demonstrates something along these lines.

53. **Peter Woit**
September 11, 2018

Eric Dennis,
I'm not assuming the initial apparatus is in a pure state. Not that I know how to do this, but my question was based on treating the apparatus probabilistically (as opposed to the system state). What do you see wrong with
https://arxiv.org/abs/1107.2138
They treat the apparatus using quantum statistical mechanics, and claim:
"Any subset of runs thus reaches over a brief delay a stable state which satisfies the same hierarchic property as in classical probability theory. Standard quantum statistical mechanics alone appears sufficient to explain the occurrence of a unique answer in each run. "

54. **Andrew**
September 11, 2018

There is a nice interview with Roger Balian (of the Curie-Weiss model of quantum measurement) in which he and Francois David discuss their responses to Schlosshauer's questions in Elegance and Enigma here: https://www.youtube.com/watch?v=-K_8W9lnSp8

55. **Perry Rice**
September 12, 2018

Peter Woit

Here are my thoughts on measurement, informed by my work in quantum optics. (apologies for length, 35 years ago I was gonn make a unfied field theory! I got stuck in interference and how detectors work. And our atom-field interaction, the Jaynes Cummmings model is a non-relativistic SUSY SHO so….)

Measurement is an interaction between two subsystems, one subsystem in principle knowable at least in part, and one not. It need not be a classical and quantum cut but it often is. The moon is there because it scatters light from the sun, it is there when you don't look at it.
But if I am observing part of a system, interacting with it, there has to be a cut. Defined by what the user can and cannot know in principle. And a cut means a partial trace of the density matrix and hence a mixed states.

Lets start with photodetection. When a single photon hits a PMT, it excites an electron to the continnum, and from there we get amplification and an avalanche to make a classical current. There is noise in this process called shot noise. Does it come from the source or is it a property of the detector. The "interpretation" depends on which operator ordering (normal, antinormal, symmetric, or any other you want) is used in the calculation. It can be either or some of both. All I know is a photon has been emitted and caused a click. I cannot ascribe a "path" or "cause". And at what point did that current become classical??

With just emission from an excited state, spontaneous emission, that is "due" to two effects. Vacuum fluctuations and radiation reaction. Again, what percentage of those "causes" SpE depends on the operator ordering. And with commutators, this is different from putting things in normal ordering with no commutators as that is what you do for absorptive photodetection. If I use symmetric ordering, I get 1/2 RR and ½ VF. Symmetric ordering (i.e. adag*a=1/2(adag*a+a*adag+1) by definition leaves us with Hermitian operators. So in principle the two things could be separately measured, and there might be some physical reason to say ½ RR and ½ VF.

And not all measurements result into projection onto an eigenstate of a Hermitian operator, as in continuous weak measurement. Vaidman has talked about this somewhat correctly. You have a weak interaction and the "measurement" takes some non-zero time.

As an example, consider how we detect squeezed light. You have two photodetectors behind a beam splitter, and subtract the two photocurrents. With classical local oscillator(coherent state) at one input port of the beam splitter, the currents cancel. Even classical noise is cancelled. EE's have used this for years, called coherent detection, or in QO terms "balanced homodyne detection". Now just have one classical beam input, zero photocurrent (except for the shot noise level described above), and a quantum field on the other port of the BS. The two photodetectors click like crazy due to the strong classical field, and the presence of the quantum signal will unbalance the thing and give rise to a difference current. For strong local oscillator the output is proportional to E_{LO}*a_{theta}, you can measure the fluctuations in a_{theta}, any quadrature you want.

a_0 =1/2(a+adag), a_pi/2=i/2(a-adag)

You cannot measure THE field as that would mean you can have an amplitude operator (adag a) and a phase. The latter does not exist; if it did we would have a time operator.
So you change that phase and watch the noise go BELOW the shot noise limit, and then above. I think they are bolting squeezed light into LIGO as we speak.

You can have Quantum Non-Demolition experiments where you can measure x over and over again, and all the "detection" noise goes into p, or vice versa.

Stimulated emission is kind of classical, spontaneous quantum (for the field anyway), but you cannot tell the difference between the two in detection .A photon is a photon is an excitation of a field leading to a click in a detector). Glauber taught us (knowing QFT) that one had to look to correlations involving two detections. A two point Greens function if you will. Then you can tell a SP em source from a Stem source.

As an example of a "cut", think about a black hole. Hawking radiation is a sum of two-mode squeezed states. There is a horizon, so you trace over the bits you can't see. Then you get a thermal state. It's like a Rindler observer seeing a thermal field in Unruh, and Minkowski can see both (or their effects)

There is no unique classical limit in QM. For the SHO, oh lets say large n is classical. Well it is except for the n zeros in the wave function. OK so then eigenstates of a are classical, coherent states. Then you have a wavepacket bouncing back and forth in the well. The first is appropriate if you look at the same time every look. If you look randomly you will see a coherent state. What is the classical limit?

And for an electron dropping down to a lower level, that is indeed an interaction. The ATOM and the vacuum field know that it happened, and in principle you could detect it. No looking at the meter, or rather not HAVING a meter is not natures fault

It seems funny that QM pretty much tells us you can just know transition amplitudes and probabilities between some initial and final states. Yet we persist in looking for the watchmaker in there somewhere. I guess I am a Bayesianist, but not those that have elevated it to consciousness.

So what is a measurement? An irreversible interaction (at least on the lifetime of the universe. And to give "causes" you have to make a split of the users choice. And its not unique (as density matrices are not)

Any this is in my somewhat informed opinion, I am an experimentalist trapped inside a theorists body, so this suits me. Density matrix is fundamental and represents what we are able to know about a system at any time. You want “cause?” Invent one and see if it matches the voltmeter.

56. **Daniel Tung**
September 12, 2018

Hi Peter,
Regarding your preferred approach: to model quantum measurement as a dynamical process and its outcome probabilities as explainable by quantum statistical mechanics (without assuming any underlying ontic states beyond quantum theory), you would still have to assume the initial states of measured system or the apparatus (or both) to be describable by quantum states.
Then one can always ask why is it being described by quantum to begin with? A quantum state is essentially a list of outcome probabilities. So this approach seems to assume a list of probabilities from the start, and therefore begs the question of where the quantum probabilities come from.

57. **Peter Woit**
September 13, 2018

Daniel Tung,
I don’t see why I have to think of a quantum state as “a list of outcome probabilities”. It’s a fundamental, complete, mathematically very deep description of the the state of the world at a given time. If I want to do a “measurement”, I couple it to a macroscopic quantum object which I have to define by a density matrix due to its macroscopic nature. This evolves in time during measurement as a density matrix. If I believe Balian and collaborators, they give an example of a solution of the “outcome” problem, showing why the final result can be thought of as a single outcome (the cat is dead or alive, not a superposition). I’m not a positivist, so don’t see why I have to think of the quantum state just in terms of the frequencies of these outcomes of very special setups coupled to the state I care about.

I’m surprised that this work hasn’t gotten a lot more attention. I’ll add to the posting a bit more information about it, would love to hear from experts why or why not this isn’t “a solution to the measurement problem.”.

58. **Eric Dennis**
September 13, 2018

Peter,

I have not read that mammoth paper (1107.2138) but it looks like a goose chase. The opaque terminology is inauspicious. Based on some selective skimming, this “hierarchic property” is related to how, e.g., the probability of a number chosen from {1, 2, 3, …, 100} being prime is independent of how you order the subset of primes from that set. Great, so some density matrices have a property isomorphic to a property of probability distributions. Of course we already knew that density matrices have many such isomorphic properties. I don’t see anything earth-shattering about adding this particular property to that list.

More fundamentally, the structure of their argument doesn’t show any evidence of getting to where you want to go. The apparatus starting in a mixed state, rather than a pure state, means simply this: for an experiment run many times, within each run we don’t know *which* pure state the apparatus starts in. But, rest assured, in each run, the apparatus starts in *some* particular pure state.

And now we are back to the basic problem I named above. In *each run*, unitary evolution demands that the final system+apparatus state still contains all the possible system eigenstates (entangled with corresponding apparatus/pointer states), all superposed. Even if in some model they find that density matrices describing *multiple runs* share some extra mathematical properties with classical probability distributions, they are still totally failing to show (i) how a particular eigenstate can possibly be singled out in each run, or (ii) how one could possibly *derive* a probability statement about that undescribed singling-out process. Hilbert spaces in, Hilbert spaces out. No real-event probabilities are in sight.

A smart person tells you he can turn a snail into a giraffe. How? He says he follows it around for 12 miles and administers a very special sequence of taps to its shell along the way. Which is more likely? That he actually can do it. Or that, at some point close to mile 12, he will declare success, and when you look down and say “Still seems like a snail to me,” he will reveal that by “giraffe” he meant a snail underneath a tree.

59. **Peter Woit**
September 13, 2018

Eric Dennis,

Thanks, although I still would like to see a better explanation of exactly what these authors are claiming, along with what exactly goes wrong. A short version of their claims that others may find gives insight is
http://iop.uva.nl/binaries/content/assets/subsites/institute-of-physics/quantum-measurement.pdf?1489502855021
Longer version
https://arxiv.org/abs/1303.7257
At the level of sloganeering, Balian in an interview claims that the situation is analogous to that of classical stat mech and thermodynamics, where one consistently has reversible microphysics, irreversible macrophysics.

For a short critique, see
http://people.bss.phy.cam.ac.uk/~mjd1014/readings.html
One problem for me is that subtleties about what probability is seem to be coming up here, ones beyond my ability to see my way through.

60. **Eric Dennis**
September 13, 2018

Peter,

Those other links are helpful. Note how in the new paper you cite, they are shifting their claim from that of the previous paper, which says: “Standard quantum statistical mechanics alone appears sufficient to explain the occurrence of a unique answer in each run and the emergence of classicality in a measurement process.”

In the newer paper (1303.7257) it’s not “standard quantum statistical mechanics alone” anymore. Now it’s that augmented by some mild-mannered “interpretative principles.” The last one of these “principles” (i.e., separate postulates that must be made besides the Schrodinger equation) is as follows (p. 17, excuse my re-formatting):

“Interpretative principle 5. Consider a set of macroscopic orthogonal projectors Π_i, a state D~ associated at a given time with an ensemble E and the states Dˆ(k)_sub associated with its sub-ensembles E^(k)_sub. If the projectors have in these states the commutative behaviour expressed by Eqs. (12a-b), their q-expectation values q^(k)_i can be interpreted as physical probabilities for exclusive events, i. e., as relative frequencies.”

In the abstract of the paper, this move is described as follows:

"The latter property supports the introduction of a last interpretative principle, needed to switch from the statistical ensembles and sub-ensembles described by quantum theory to individual experimental events. It amounts to identify some formal "q-probabilities" with ordinary frequencies, but only those which refer to the final indications of the pointer."

The "q-probabilities" are apparently just the squared-norms of terms in some decomposition of a Hilbert space vector. So we finally get actual probabilities only by "identifying" them with Hilbert space norms–by hand.

I conclude that this whole enterprise has moved itself squarely into my original bucket #3 containing Copenhagen, postulated state reduction, and other similar gambits. This was the bucket I had identified with the interpretative principle of "flip the table over, scream incoherently, and run away."

61. **Anonyrat**
September 14, 2018

This from Balian, et. al. on resolving Bell, GHZ type experiments:

"As a quantum measurement is a joint property of S{ystem} and A{pparatus}, we are not allowed to interpret simultaneously as real properties of the initial state of S the results of experiments obtained with different apparatuses (here with different directions of the detectors). This deep property of quantum measurements is in line with the absence, for quantum states, of a sample space as in ordinary probability theory [46, 47, 48, 49, 50]."

IMO, this might be the thing to look at to see if it all makes sense.

62. **Peter Woit**
September 14, 2018

Anonyrat,
That just sounds to me like a standard fact going back to Copenhagen. What looks new and confusing here are claims about what "probability" means in the context of getting a supposed single outcome.

63. **Doug McDonald**
September 14, 2018

I've not commented on this, as all my previous posts on similar subjects have been rejected. But I must. I and my former boss have though about this a lot, especially because of experiments we did long ago.

The crux is that you cannot start with a single pure eigenstate. You must include all of the "apparatus" … including the measurement apparatus and the "test particle generation apparatus". And the original state must be a very complicated state vector. This is because both apparatus must be above T = 0. In any real experiment this is true because both are macroscopic. "Near" zero won't do because macroscopic things have low energy phonon states. You can't deexcite them. You can of course choose an energy for the apparatus … but since you are limited in how long you can take to establish that, the uncertainty principle makes a spread in the energy of "true eigenstates" necessary. Because of this the "quantum recurrence time" of the apparatus is incredibly long.

And these complicated composite states are described by a state vector, which changes with time (Schrodinger picture). There are "observables" on that state vector that are macroscopic. Because of the huge number of eigenstates that are excited, these observables follow classical paths. You tell what the outcome is by looking at them (i.e. did a bunch of electrons flow to just one pixel on an LCD?). This can be seen on a computer solving the Scrodinger equation. And, just like in classical mechanics, a tiny change in a wave function a certain point is space and time can, and does, grow with time into a macroscopic change. This can and has been computer simulated. This is probabilistic, based on the state when, say, an electron wave packet hits the surface of a CCD from space. But remember that that wave packet was generated by an apparatus with a finite temperature. Its not a uniform plane wave.

The size of the "apparatus" necessary is surprisingly small, but macroscopic … at least a few hundred atoms. Its not yet exactly simulatable on a computer but is getting closer.

Many people have problems understanding because they try to use a descriptivive "system" and "apparatus" that are impossibly small. They say "well, we'll use a "classical bath" "weakly coupled" to that small system/apparatus … you can't do that, in time weak couplings add up. You can't use perturbation theory, just like you can't use perturbation theory in bound state QCD problems. The apparatus is bound states.

This is an attempt at a wordy explanation of what I believe
https://arxiv.org/abs/1107.2138 says.

64. **Stephane**
September 15, 2018

There is an article by S. Weinberg in 2016 about measurement, Lindblad equation and the Born rule.
S. Weinberg, What Happens in a Measurement?
Phys Rev A93 (2016) 032124 – arXiv:1603.06008
followed up by Lindblad Decoherence in Atomic Clocks
Phys Rev A94 (2016) 042117 – arXiv:1610.02537
See also https://backreaction.blogspot.com/2017/02/testing-quantum-foundations-with-atomic.html

65. **Wolfram Graser**
September 16, 2018

Mateus Araújo, and Peter,
I appreciate the contribution by Landsman and Reuvers to the understanding of the Born rule. Yet, they do not mention that the instability in their asymmetric double well scenario was described, in a more idealized "Einstein's boxes" version, by J. Gea-Banacloche, "Splitting the wave function of a particle in a box", Am. J. Phys. 70 (3), March 2002, p. 307-312. On the other hand, neither does J. Gea-Banacloche seem to know the work of Jona-Lasinio et al. (1981) cited by Landsman and Reuvers. A survey on "Einstein's Boxes" can be found in T. Norsen, "Einstein's Boxes", arXiv:quant-ph/0404016v2, 8 Feb 2005 (Dated: February 1, 2008), published Am. J. Phys. 73, 164-176 (2005); and S. J. van Enk extends the subject to "Splitting the wavefunction of two particles in two boxes", arXiv:0806.4932v2 [physics.ed-ph] 5 Aug 2008. I guess that Einstein would have been pleased about the instabilities described in these articles. –
One more thing: akhmeteli wrote on September 9, 2018 at 10:32 pm: "[The] projection postulate and the Born rule do not reflect the fact that any measurement requires some time to perform." I agree: This requires that the Born rule must have a sibling on the time axis, where the actual "occurance" of the measurement is undetermined in standard QM, as it is in space. This of course plays an important role e. g. in radioactive decay, but is not covered by text book derivations of the (statistical) decay process.

66. **John Baez**
September 17, 2018

It looks like I've missed the party – which for "interpretation of quantum mechanics" parties is usually fine – but since it's interesting to see Peter getting interested in this, here are my quick thoughts.

> What I wonder about is whether it is possible to argue that, given that your result is going to be probabilistic, and given some list of properties a "measurement" should satisfy, can you show that the Born rule is the only possibility?

For Hilbert space quantum mechanics I would just take probability as a primitive concept. For any two unit vectors u and v we have "the probability that the system is in state u given that it is in state v". It's odd that this can be nonzero when u is not equal to v, but that's life. Then various choice of mathematically natural axioms will force this probability to be the square of the absolute value of the inner product of u and v.

(You can give this setup a more epistemological spin by saying "the probability that the system is found to be in state u if we check this when we know that it's in state v". However, most days I consider "is found" and "we know" to be distracting fluff. We could add such verbiage to the laws of classical mechanics as well. It doesn't hurt much, but it doesn't help much either.)

In the way I'm putting it, all the interpretive difficulty is packed into the word "probability". By leaving probability as an undefined primitive, I'm leaving it as a separate task to say what "probability" actually means in experimental contexts. This is a very interesting quagmire, with various kinds of "Bayesians" and "frequentists" fighting it out. A lot of the fight over quantum mechanics, I claim, is secretly a fight over what "probability" actually means.

> To clarify the question I'm asking, a better version might be: "is the source of probability in quantum mechanics the same as in classical mechanics: uncertainty in the initial state of the measurement apparatus + environment?

This seems like a completely different question. But it's better in that I can give my preferred answer more rapidly: no.

67. **Peter Woit**
September 17, 2018

John,

Thanks for writing. Now that the party has died down, I'm still confused, but can at least express a bit more clearly what is bothering me. I'll also add this to the bottom of the posting itself.

The state of the world is described at a fixed time by a state vector, which evolves unitarily by the Schrodinger equation. No probability here.

If I pick a suitable operator, e.g. the momentum operator, then if the state is an eigenstate, the world has a well-defined momentum, the eigenvalue. If I couple the state to an experimental apparatus designed to measure momenta, it produces a macroscopic, classically describable, readout of this number. No probability here.

If I decide I want to know the position of my state, one thing the basic formalism of QM says is "a momentum eigenstate just doesn't have a well-defined position, that's a meaningless question. If you look carefully at how position and momentum work, if you know the momentum, you can't know the position". No probability here.

If I decide that, even though my state has no position, I want to couple it to an experimental apparatus designed to measure the position (i.e. one that gives the right answer for position eigenstates), then the Born rule tells me what will happen. In this case the "position" pointer is equally likely to give any value. Probability has appeared.

So, probability appeared when I introduced a macroscopic apparatus of a special sort: one with emergent classical behavior (the pointer) specially designed to behave in a certain way when presented with position eigenstates. This makes me tempted to say that probability has no fundamental role in quantum theory, it's a subtle feature of the emergence of classical behavior from the more fundamental quantum behavior, that will appear in certain circumstances, governed by the Born rule. Everyone tells me the Born rule itself is easily explicable (it's the only possibility) once you assume you will only get a probabilistic answer to your question (e.g. what is the position?)

A macroscopic experimental apparatus never has a known pure state. If I want to carefully analyze such a setup, I need to describe it by quantum statistical mechanics, using a mixed state. Balian and collaborators claim that if they do this for a specific realistic model of an experimental apparatus, they get as output not the problematic superposition of states of the measurement problem, but definite outcomes, with probabilities given by the Born rule. When I try and follow their argument, I get confused, realize I am confused by the whole concept: tracking a mixed quantum state as it evolves through the apparatus, until at some point one wants to talk about what is going on in classical terms. How do you match your classical language to the mixed quantum state? The whole thing makes me appreciate Bohr and the Copenhagen interpretation (in the form "better not to try and think about this") a lot more…

68. **AB**
September 17, 2018

Dear Peter,

Long-time reader, first time commenter (I think).

I agree with everything you write in your latest, third update. My problem with the Balian et al. paper is the same: once you start using density matrices which are not pure, so mixed, you have lost the possibility to describe ordinary unitary time evolution, and you have introduced a probabilistic feature in your theory. In decoherence language, if you neglect the off-diagonal parts of the density matrix in the pointer basis, you have gone from an exact to a probabilistic model.

I think Landsman is on the right track, but I also think most people in foundational QM are looking at this from a too microscopic and/or mathematical point of view, trying to analytically derive neat and exact results for messy many-body systems. Condensed-matter physicists know this is a foolish undertaking.

If you believe like Landsman and myself and perhaps you that everything obeys unitary time evolution, you should put your money where your mouth is and start a project to conduct large-scale computer quantum simulations in which you describe measurement apparatuses as quantum many-body systems like Balian et al., but *in pure states*, couple it to your test particle and see how the combined system evolves for a large number of initial states, for both test particle and apparatus (the last one with a slight perturbation from pure symmetric in the pointer basis). If Born's rule is robust, it should emerge while each run is definite and unitarily evolving.

The problem will be to find the correct Hamiltonian to describe the apparatus: it needs to be in an almost-symmetric, metastable state, unstable to the tiny interaction with the test particle, while the stable states are those in the pointer basis. But any experimentalist can tell you this is a very real problem in nature.

Personally I think this will solve the measurement problem to a large extent, but it will not, immediately, solve non-locality. It everything happens by unitary time evolution, like measurement of one half of a Bell-photon pair, then information including the change of the wavefunction due to interaction with the detector can only travel at the speed of light. I also think the usual interpretation of particles as wavepackets (two photons as two wavepackets within one wavefunction?) may be insufficient to this end.

Finally let me mention this completely ignored and/or unknown paper with does something like Landsman in a QFT model:

https://arxiv.org/abs/quant-ph/0505077

And one unpublished paper which does something like the simulation I mentioned:

https://arxiv.org/abs/0809.1575

69. **Peter Woit**
September 18, 2018

AB,

Thanks very much for the comments and references. The project you suggest sounds worth doing, but needs to be done by people with much different expertise than mine.

One thing I've always been curious about is whether some part of the quantum computing story intersects with this problem, which might make it much more interesting to people.

About non-locality, I confess I've never thought much about that, beyond the vague thought that "the apparatus is non-local, so what's surprising about non-locality"? But I really don't want to start a discussion of this here and educate myself right now, best to leave for another time.

70. **John Baez**
September 18, 2018

Peter Woit wrote:

> The state of the world is described at a fixed time by a state vector, which evolves unitarily by the Schrodinger equation. No probability here.

And perhaps no physics here, either, unless we say *how* the state of the world is described by that vector: that is, how we can use the vector to make predictions of experimental results.

Every theory of physics has this issue: it consists of precise mathematics surrounded by a vaguer and much more complicated cloud of "interpretation": instructions on how to connect this mathematics to things we can do and see in a laboratory.

Those of us who like precision prefer to focus on the mathematics. We'd be happy if this mathematics were, ultimately, "all there really is". Maybe it can somehow provide its own interpretation. But it's hard to see how. To address this rigorously, we'd need to say very precisely what we *mean* by an interpretation, and prove theorems saying that such-and-such a theory can admit only the following interpretations. But I haven't seen anyone do a good job of this yet.

It's particularly hard to see how to get probabilities out of some mathematics unless we put them in by hand – that is, *decree* that certain quantities in the mathematics stand for probabilities. The reason is that it's very hard to say what probabilities actually are, in a non-circular way.

It sounds like you're trying to put the probabilities in by hand as follows. When an experimental apparatus meets a microscopic system, the apparatus is in a mixed state, and we *decree* that mixture has a probabilistic interpretation. We then evolve the joint system unitarily, see what happens, and try to derive probabilities of results.

In other words, we try to bring in the probabilities through a probabilistic description of the measuring apparatus.

I don't see how this will work. When I take an electron with spin pointing up along the z axis, and measure its spin along the x axis, I should get "up" with probability 50%. I don't see how this probability is going to emerge from the probabilities in the mixed state representing the measuring apparatus. Indeed, a good measuring apparatus is usually considered to be one where our lack of knowledge about its state doesn't affect its functioning!

So I'm willing to entertain this line of thought, but I'm not optimistic.

I prefer to bite the bullet and say, right from the start, that a unit vector in a Hilbert space describes probabilities. The *meaning* of a unit vector v in Hilbert space is that given another unit vector u, the probability that "if the system is in state v, then it's in state u" is the square of the absolute value of the inner product of u and v.

Of course this leads to a host of further puzzles, but at least it's clear where I'm putting in the concept of probability.

(By the way, since a pure state of a joint system restricts to mixed states on each of its parts, if you're willing to decree that mixed states describe probabilities, you can reason backwards and get the probability interpretation of pure states as a corollary, given suitable side-assumptions. Wojciech Zurek gave a nice explanation of this in the workshop "Statistical Mechanics, Information Processing and Biology" at the Santa Fe Institute, and he probably has a paper on it.)

71. **Peter Woit**
September 18, 2018

John,

I guess it just doesn't seem to me obviously either necessary or a good idea to try and directly interpret the fundamental quantum state in terms of something very complicated and very different, the experimentally accessible probabilistic classical emergent behavior of a pointer. When you say

"When I take an electron with spin pointing up along the z axis, and measure its spin along the x axis, I should get "up" with probability 50%."

you're assuming a very simple and direct relationship between the microscopic state and the macroscopic emergent classical behavior. But that seems to me exactly where the mystery of measurement problem is, and any attempt to think through how to correctly model this makes clear it's a very hard problem. I'm having trouble finding convincing simple-minded (i.e., "it's just some pure state, evolving according to the Schrodinger equation") arguments about how a measurement apparatus is supposed to behave.

On a related note, I'm also not convinced by
"a good measuring apparatus is usually considered to be one where our lack of knowledge about its state doesn't affect its functioning! "
As I was trying to make clear, the problem occurs exactly when we're talking about observable quantities that have no meaning until we couple our system to the apparatus. It is the apparatus itself which, coupled to the system, is giving (in a complicated way involving emergent classical behavior) meaning to the observable quantity. I don't see at all why our uncertain knowledge of the state of the apparatus is not going to be relevant.

72. **Luuk van Dijk**
September 19, 2018

https://arxiv.org/abs/1805.08583

My summary: If your experiment is described in terms of a separate preparation and measurement step, and all you can do in the end is count events, i.e. you _start_ from statistics, you have to end up with the formalism of quantum mechanics without needing any weird postulates.

73. **David Metzler**
September 20, 2018

I appreciate John's comment that the notion of probability is a source of controversy even outside of any quantum concerns.

My question to the experts is, does Consistent Histories (Griffiths, Gell-Mann, Hartle) provide a useful perspective on Peter's dilemma? As an amateur in the subject of interpreting QM, it has always appealed to me.

74. **Peter Woit**
September 20, 2018

David Metzler,
I'm no expert, but from what I've seen, Consistent Histories just ignores the problem that is bothering me of understanding exactly how classical behavior emerges from quantum. As far as I can tell, it's just one of many ways of papering over that problem.

This Week’s Hype

September 10, 2018
Categories: Multiverse Mania, This Week's Hype

The Stanford string theory group is not taking the attack by Harvard’s Cumrun Vafa lying down. After an arXiv barrage of papers defending KKLT (see here), they’ve now enlisted the Stanford press office, which has produced a five part promotional series about the scientific glories of the string theory landscape. The first part of the series is online today, the rest to come soon.

The great thing about having your university press office write stories like this for you is that they will just print whatever you want, unlike journalists, who might ask your critics what they think and even quote them. Even better than not having to hear from your critics, you can try and discredit them as close-minded reactionaries unethically thwarting the search for truth, by misrepresenting their arguments:

> “One dominant view in the community is that believing in the Landscape might have the negative effect of leading people away from fundamental physics, so we shouldn’t even discuss it,” said Shamit Kachru, who holds the Wells Family Directorship of the Stanford Institute for Theoretical Physics (SITP).

I’ve never heard anyone argue that “we shouldn’t even discuss it”. There is a dominant view in the field that what the theorists at Stanford are doing is not science, but the arguments for this are scientific, not arguments about what is or what isn’t good PR. Will we see any of these arguments in the rest of the series?

Update: All five parts of this are now on-line. No critics of the string landscape are named and their serious arguments are ignored (they are described as “hating” the idea, creatures of their out-of-control emotions). In the context of the old arguments of the string wars, two things to note are

- This could be accurately described as a campaign by people who are losing in the scientific marketplace of ideas to, instead of doing science, start a PR effort aimed at the public.
- It’s once of the best examples of the kind of extreme tribalism and “group-think” Lee Smolin was pointing to that I’ve ever seen. Stanford is portrayed as uniformly of one opinion about this, other opinions are wrong and only held elsewhere. If you are (or want to be) at Stanford and have a different opinion, especially if you’re a postdoc or grad student, it’s being made very clear that you best keep this to yourself.

Update: For those who want to follow the latest on the “Swampland” challenge to the Stanford/KKLT landscape program being promoted by the Stanford press office, there’s a conference later this week in Madrid, talks here. Among the roughly 100 participants at the conference, no one from Stanford. Not invited? Invited, but refuse to participate in any scientific discussion critical of their program? Inquiring minds want to know…

Update: Nima Arkani-Hamed gave the colloquium talk ending the Madrid conference. At the end (1:30), he had these mystifying comments about the landscape, somehow relating this posting to the previous one:

> The raises the possibility that we are misinterpreting the string landscape – the different regions aren’t “out there” but are different APPROXIMATE “System/Observer” splits of A SINGLE OBJECT.

I have absolutely no idea what this is supposed to mean.

Comments

1. **clayton**
   September 11, 2018

   Yikes — I get the feeling that quote is going to haunt Shamit…

2. **Peter Woit**
   September 11, 2018

   clayton,

   I think Kachru was conflating two true things (and adding the silliness about “we shouldn’t even discuss it).

   1. Taking “community” as physicists in general, a “dominant” view would be that this is pseudo-science and discrediting science in general, so people shouldn’t do it.
   2. Taking “community” as string theorists, I don’t think this is “dominant”, but if you look at the video in the linked posting, you see that Vafa is arguing that acceptance of KKLT vacua and the Stanford landscape claims is discrediting string theory in particular (as unpredictive). Vafa’s conclusion though is the opposite of “shouldn’t even discuss it”, instead he’s trying to encourage discussion of the technicalities, hoping that these will show inconsistency of the KKLT vacua and remove the problem for string theory posed by the Stanford landscape philosophy.

   I think the reason we’re seeing a PR campaign from Stanford right now, given that there has been no recent progress on the landscape stuff, is just the perceived need of a response to the bad PR coming from press coverage of what Vafa is doing.

3. **Atreat**
   September 11, 2018

   Part 2 is out: https://news.stanford.edu/2018/09/11/cosmic-symphony-vibrating-strings/

   We depressing learn that we are a part of the Multiverse, but not how our particular universe came into being. What is tee’d up to describe this? Inflation and Andrei Linde are next. Here comes conflation of the string theory landscape with Linde’s inflationary multiverse…

4. **Roger**
   September 12, 2018

   Off topic but maybe relevant for a future post:
   http://www.hyper-k.org/en/news/news-20180912.html .
   It looks like Hyper-K is taking a big step forward towards approval.
   I don’t know what this means for hosting the ILC in Japan.

5. **Low Math, Meekly Interacting**

September 12, 2018

It's very disappointing to see such misinformation repeated so persistently. It's simply not true that most who disagree are perplexed by a modern analog of the Copernican Revolution. I think it's gone on long enough that we can dispense with niceties like "misperception" and call it what it is: a lie.

We're perplexed because a "framework" (if "theory" is a misnomer) one can get virtually anything out of is as good as one you get nothing out of, and redefining "science" to somehow make this glaring inadequacy permissible is beyond the pale.

So please, cut the bullshit.

6. **Peter Woit**
September 12, 2018

Roger,
To connect to the current topic, anyone unaware of what is going on here can get some clue by looking into the question: "what's the string landscape prediction for the proton lifetime?" This question won't appear in the Stanford press office PR campaign.

7. **Atreat**
September 12, 2018

Part 3 is out and the conflation with String Theory is very sad: https://news.stanford.edu/2018/09/12/the-fractal-universe/

Remember Part 2 told us that Linde would answer how the stringy multiverse was connected to ours. Instead all we get is this warm sauce:

"Linde took the multiverse idea even further by proposing that each pocket universe could have differing properties, a conclusion that some string theorists were also reaching independently."

Next up, Part 4 will tell us how this wholly novel idea – that is not at all like what your average stoner dreams up after a good bong hit – helps to explain dark energy. You see, "dark energy" was predicted by Linde and our String Heroes as those "differing properties" the multiverse explains.

8. **Narad**
September 14, 2018

Kevin Wells must have made a lot of bread in software engineering before starting to make unrestricted grants to Stanford, one of which seems to have produced SITP.

9. **Peter Woit**
September 14, 2018

Narad,
That is kind of an odd situation with the SITP Executive Director. Normally people in a position to make significant financial contributions to an institution don't also work in a staff position for the institution (generally, because if they have that much money, why go to work every day doing administrative tasks for a relatively modest pay?).

https://sitp.stanford.edu/people/kevin-wells
https://pgnet.stanford.edu/get/file/g2sdoc/highlights/W10_Wells_p13.pdf

10. **quasihumanist**
September 15, 2018

As a mathematician, this all seems very weird.

I'm used to not understanding anything my colleague next door does. I'm used to having only a dozen people in the world understand or care about the research I do. Having other mathematicians do mathematics that doesn't look anything like what I do is the normal state of affairs, and we generally regard the wide diversity of mathematics as a strength of the discipline. Some people even go to some pains to make sure their graduate students work on different topics so that they don't compete with each other in the future.

Theoretical physicists, on the other hand, seem to think that every other theoretical physicist in the world ought to be studying what they are studying, and regard studying something else as an attack on their work.

Someone explain?

11. **Joseph Conlon**
September 16, 2018

The landscape is topical again – I give my own take on it on my blog http://www.fickleandfreckled.com

12. **Peter Woit**
September 16, 2018

quasihumanist,

The culture of theoretical physics is quite different than that of mathematics, and there's a lot to be said about how that works and the reasons for it.

The story of the Stanford theorists and the anthropic landscape though is a very unusual and very peculiar one. It's nothing like anything that I've heard of in mathematics, or actually in other areas of theoretical physics What's going on here isn't related to what theorists actually work on: at this point, virtually no one (including anyone at Stanford) is trying to pursue the research direction that would connect the landscape to experiment (try to map the landscape and make statistical predictions). Although this looked impossible from the beginning, a few people did work on it, then gave up as it became clear this is a road to nowhere.

If you look at Joseph Conlon's blog entry that he links to above, what you'll see is a pretty typical opinion of string theorists about the landscape business: "far too soggy for physics", i.e. not really science. The Stanford group has from the beginning been faced by the fact that this is what the great majority of their colleagues believe. They've reacted to this by running a rather successful publicity campaign. This campaign has been running out of steam recently, and I suspect it's hard to get journalists to go along with it. Thus the odd situation of the Stanford press office being used as a promotional tool, even though there's no advance in the subject to base a story on.

13. **Reg Taylor**

September 18, 2018

Reading the five sections as a whole it seems to me that, firstly, this is a popular exposition of a singular viewpoint – with all that that entails – and, secondly, the writers(s) aren't fully on the side of the argument. Although 'pro' quotes by the main protagonists are used liberally the staff hacks appear to have tried to temper the claims with a degree of scepticism which is at odds with their paymasters; "the String Theory Landscape remains divisive among physicists", "theoretical physicists at Stanford … sparked a fierce and still ongoing debate about what science is and what it should be", "critics say the theory is ultimately untestable".
Whether the Stanford PR department has produced a useful contribution to the debate is questionable. It won't offend believers, non believers may well be left wondering about the "odd situation of the Stanford press office being used as a promotional tool" and the rest of us will just write the whole thing off as a PR exercise and, ultimately, pointless.

14. **Lars**
September 19, 2018

If you look at Joseph Conlon's blog entry that he links to above, what you'll see is a pretty typical opinion of string theorists about the landscape business: "far too soggy for physics", i.e. not really science. The Stanford group has from the beginning been faced by the fact that this is what the great majority of their colleagues believe. They've reacted to this by running a rather successful publicity campaign

If that is indeed what Stanford has been doing (and I'll have to take that claim at face value), I'd have to say that they have not been all THAT successful if it is also true that *a pretty typical opinion of string theorists about the landscape business: "far too soggy for physics", i.e. not really science.*

If that is indeed the case, why even bother writing about it on a blog?

Hasn't it already been decided among scientists? Isn't that how science is supposed to work?

Or, if the Stanford press release was intended for someone other than scientists, who might that be? Surely, John Q. Public neither knows nor cares about the string landscape.

15. **Peter Woit**
September 19, 2018

Lars,
While I think the majority of physicists have a negative view of the landscape business, there are others who don't, as well as many who just don't understand what the controversy is about. This is one reason I continue writing about it.

I think you underestimate the interest of John Q. Public in this kind of issue. Multi-million dollar book contracts are not unheard of for books about this aimed at the public.

As for the question of who the Stanford PR effort is aimed at, the physicists involved in this are well aware that a crucial audience is not the public, and not practicing physicists, but those who control the money: university and funding agency administrators and wealthy donors. Few people read the output of the Stanford press office, but one of the main targets of that sort of press operation is exactly these people.

16. **Jeff**
September 19, 2018

Lars,

I wouldn't be as dismissive of the public's interest. I think there is a decent percentage of folks who care; unfortunately, some of the interest is being generated by the unsubstantiated, "mind-blowing" claims coming out of places such as Stanford. And scientists don't arise via spontaneous generation; one goal of the PR blitz is surely recruiting. Another goal is surely shoring up support for funding, most of which is set by non-string-theorists, or even non-scientists in general.

17. **GoletaBeach**
September 19, 2018

Yawn. Old news. The interesting stuff is out among the young experimentalists and phenomenologists grappling with new ways to detect the "darks" and weird new experiments at CERN, FNAL, and elsewhere. Any one of those experiments is quite a bit more interesting than KKLT etc.

18. **tulpoeid**
September 20, 2018

GoletaBeach and Lars, unfortunately it's not yawn and old news. The part of the public that believes scientists have discovered the multiverse is real (with the aid of the landscape, the many-worlds interpretation and inflation [as if a multitude of unrelated possible causes lends credibility to a made-up outcome]) is growing.

Personally I think the war is lost for this and the next generation – theoretical physics as a means of enlightenment of the humankind at large is regressing. But we have to keep fighting for the coming ones and also for the unaffected individuals; and after all, when I want to relax I remind myself that science does not equal academia.

PS: Since you mention them, in general-physics experiments priority of allocation of resources has been determined to a large extent by which theories are deemed popular. Just to say that it's not a purely philosophical issue, and it's not only about theories without predictions (which of course affect funding on their own yada yada).

19. **Low Math, Meekly Interacting**
September 20, 2018

Agreed. Other disciplines have their own diseases, but evidence-based remedies have at least a fighting chance of working to counteract them. Not so the multiverse.

Fundamental physics, despite its daunting conceptual and technical challenges, is actually intensely interesting to many lay citizens. For many, it's the only branch of the natural sciences they would bother to buy a popular book about and read. Whatever the reason for that, stories about what HEP theorists like Einstein and Hawking think about and do professionally might be the layperson's most influential exposure to "science". That this is obviously far too limited a sample to make a fair assessment and people generally ought to know better is both true and moot. For better or worse, the more "mind-blowing" the subject, the more eyeballs will be drawn. I'm as guilty as anyone of having the craving.

I'm of the opinion the gains of scientific enlightenment can't be taken for granted, and nearly everything I see in the news these days fills me with alarm. So when the most beguiling of scientific disciplines is infected with mutiverse mania to any degree, it's a cause for some dismay. Though it should have an insignificant impact on the public perception of science, I tend to think it doesn't at all. And that could be a serious problem.

Scholze and Stix on the Mochizuki Proof

September 20, 2018
Categories: abc Conjecture

As discussed here a couple months ago, Peter Scholze and Jakob Stix believe they have found a serious problem with Mochizuki's claimed proof of the abc conjecture, and traveled to Kyoto in March to discuss it with him. Their write-up is now available here. Mochizuki has made public his response to this, creating a web-page available here. There's also an updated version of Ivan Fesenko's take on the story, as well as a possibly relevant FAQ on IUTeich from Go Yamashita.

Erica Klarreich has an excellent long and detailed article about this story at Quanta.

Update: Looking through these Scholze/Stix/Mochizuki documents, my non-expert opinion is that Mochizuki does not seem to effectively address the Scholze-Stix objections, which are aimed at a very specific piece of his argument. Unfortunately, he also does his own credibility a huge amount of damage by including over-the-top attacks on the competence of Scholze and Stix, in typefaces that make him look unserious. For instance, there's

> *I can only say that it is a* very challenging task to document the depth of my astonishment *when I first read this Remark! This Remark may be described as a* **breath-takingly (melo?)dramatic self-declaration**, *on the part of SS, of their* profound ignorance *of the* elementary theory of heights, *at the advanced undergraduate/beginning graduate level*.

or the last couple pages of his report.

Update: More of the same about IUT from Fesenko available here. His argument is that the overwhelming majority of leading experts in arithmetic geometry who are skeptical of the purported abc proof should be ignored, since they haven't put in the two years of continuous study of IUT necessary. I don't think this collection of ad hominem arguments will do anything to change anyone's mind. I also don't see why he doesn't instead produce what could change minds: a clear and convincing technical refutation of the Scholze-Stix argument.

Comments

1. **S**
   September 20, 2018

   This is very interesting, thank you. It is discouraging, on one hand, that agreement cannot be reached even when things are boiled down like this to (seemingly) one small issue. On the other hand, I feel more optimism than before that this will get finally resolved fairly soon. At the least, Scholze and Stix are doing a great service by highlighting and publicly discussing the problem area. Hopefully more mathematicians will now get involved.

2. **Mateus Araújo**
   September 20, 2018

   Thanks for posting the update, it is the same as my impression of Mochizuki's comments, which I didn't want to write out of politeness.

3. **Peter Woit**
   September 20, 2018

   S,
   Thanks. My impression is that for most people these documents will conclusively resolve the issue, in favor of the Scholze/Stix side that this proof has a serious flaw. Mochizuki does not do an effective job of answering their objections. His only hope is that some others interested in this will now be able to focus on the very specific problem that Scholze/Stix point out, and if they find that Scholze/Stix are mistaken, do a better job than Mochizuki of explaining why.

   This has been a very strange and unusual story since the beginning, fascinating because it illuminates well how mathematics normally works, because of the failures that occurred here. Here again, the situation is remarkable and illuminating. I've never heard of something like this happening before (experts writing long competing documents arguing about whether a specific part of a technical proof is correct). In other subjects you have experts publishing competing papers claiming the other person is wrong, with no resolution coming out of this. This doesn't happen in mathematics: experts discussing a technical question about a proof are supposed to converge on an understanding of what is correct and what isn't. In particular, I don't think any journal would even consider publishing both of these, the editor would just say "one or both of them is wrong, if we publish both, we'd be publishing an incorrect paper".

4. **S**
   September 20, 2018

   That's probably right, Peter; I lack competence to judge, and clearly there is a very subtle issue (either real or imagined) about whether and when to identify non-canonically-identified things, so it's hard for me to say for sure.

   One thing for certain is that your update is right. I hadn't read Mochizuki's reply carefully last time I posted. When I did, I was simply aghast at the remark you quote. How can anybody dare say such things about… well, honestly, ANY competent fellow-mathematician? But much less a Fields medalist widely regarded as the star of his generation? It really does feel like Mochizuki is working through some issues in public, but that of course is a sidelight to the mathematical questions here. (Except that, as you say, that impression does not underwrite confidence in his mathematical judgments here.)

5. **David Roberts**
   September 20, 2018

   What I find most telling is that Scholze–Stix provide explanations of some of Mochizuki's objects in terms that humans can understand, explain where their own simplifications may have been too strong, and where the simplifications are perfectly safe, whereas Mochizuki supplies yet more analogies based on manipulations of inequalities based on unspecified quantities, and then pages and pages of explanations about the importance of distinct labels. While Mochizuki is welcoming actual technical and precise discussion (while dismissing it at misunderstanding his work), I find his report *not technically precise enough*. As a category theorist I feel I'm capable of recognising the distinctions one makes between objects considered in different categories linked by various forgetful functors, and his treatment is less than clear. It's entirely possible there was a simplification made that accidentally identifies two isomorphic objects by an incorrect isomorphism, and Scholze–Stix are aware of this, but aren't convinced by M's protestations on their specific choices. At this point, M could make a very short and concrete rebuttal of their rebuttal, but pointing to just **one** such incorrect choice, but no, it needs a 41-page report…

   Also this quote:

> IUTch has been checked , verified, read and reread, and orally exposed in detail in seminars in its entirety countless times since the release of preprints on IUTch in August 2012 by a collection of mathematicians (not including myself) involved in this line of research. (For instance, Fesenko estimates, in the most recent updated version of §3.1 of his survey [Fsk], that IUTch has been verified at least 30 times.) This collection of mathematicians has (together with me) also been actively involved in detailed discussions and dialogues with mathematicians who have any questions concerning IUTch.

!

Voevodsky's proof of the Milnor conjecture was likewise discussed around the world in seminars for a six years (and was much better received and understood) before Deligne discovered an incorrect lemma that had to be replaced. The number of people who claim to understand IUTT is surely that much smaller… (Not to mention the Kapranov–Voevodsky paper on the homotopy hypothesis that V admitted he took over a decade to accept had a mistake.)

6. **tksfz**
September 21, 2018

As a casual non-at-all-expert observer, I find myself oddly reassured to see that mathematics is performed by flesh-and-blood human beings, faults and all. 🙂

7. **anon**
September 21, 2018

Wow. I thought that Fesenko's criticisms of people who don't understand Mochizuki were harsh, but now I see that also when it comes to hurling insults, he is just an apprentice of the great IUT master.

8. **MK**
September 21, 2018

Your quote does not seem to appear in the current version of the document on Mochizuki's website.

Also, the document seems full of highly technical content, I think you are doing a disservice to the discussion by highlighting the small bit of "sensationalistic" remarks (assuming they appeared in a previous version of the same document). One wonders whether you formed what you call your "non-expert opinion" by focusing on the mathematical content, or, as seems to be the norm in this saga (and the source of rightful protests from Mochizuki), the non-mathematical aspects of the discussion.

9. **Peter Woit**
September 21, 2018

MK,
The quote is not from the "Report", but from his second response to Scholze-Stix, see
http://www.kurims.kyoto-u.ac.jp/~motizuki/Cmt2018-08.pdf
and it is still there.

10. **zbornikp**
September 21, 2018

I wonder if Scholze and Stix expect their paper to be published when it contains comments like: *"When it comes to the more drastic simplifications indicated below […], these are inessential to the point we are making, and Mochizuki was not able to convince us during the week why such a simplification was not allowed."* (SS2018-08 2.1 (3))

However *"drastic simplifications"* should be proven, otherwise they are just conjectures. Claiming that *"Mochizuki was not able to convince us"* doesn't prove these conjectures. On the other hand, if the *"drastic simplifications"* are not needed to refute the proof, then they prevent the claimed refutation from being published, which also doesn't make sense, if the aim of the authors really is to have the paper published.

Indeed, rather than aiming at publishing their paper, might it be the case, that Scholze and Stix aim "lower", i.e. at preventing Mochizuki's proof from being published? Mochizuki states on his web page (http://www.kurims.kyoto-u.ac.jp/~motizuki/IUTch-discussions-2018-03.html), that *"it does not necessarily appear realistic to expect that further substantial efforts of the sort just described will be made by the authors of these files [SS2018-05], [SS2018-08]"*

Why otherwise would they write their paper and then walk away from the discussion?

What Mochizuki claims and what Scholze and Stix themselves admit being possible, is that Scholze and Stix with their simplifications have constructed the mathematical equivalent of a Straw man argument.

Here's what wikipedia has to say about the Straw man:
"The typical straw man argument creates the illusion of having completely refuted or defeated an opponent's proposition through the covert replacement of it with a different proposition (i.e., "stand up a straw man") and the subsequent refutation of that false argument ("knock down a straw man") instead of the opponent's proposition.[2][3]

This technique has been used throughout history in polemical debate, particularly in arguments about highly charged emotional issues where a fiery "battle" and the defeat of an "enemy" may be more valued than critical thinking or an understanding of both sides of the issue."
https://en.wikipedia.org/wiki/Straw_man

I wrote a short Science fiction script, somewhat inspired by this remarkable discussion. You can read it here: https://twitter.com/zbornikp/status/1033370752756719619

11. **Peter Woit**
September 21, 2018

zbornikp,
I don't think Scholze and Stix wrote that for publication. See the comments from David Roberts about the "simplification" issue. Yes, Scholze and Stix might be wrong about this, but if so Mochizuki should be able to point to a specific error and convincingly show it is an error. He doesn't appear to be able to do so.

It is true that one motivating reason for Scholze and Stix to get involved was the possibility that PRIMS might accept and publish the Mochizuki proof, even though Scholze (and many others) believed the proof was flawed. I think these documents are best thought of as standard sorts of referee reports and an author's response, which for unusual reasons are being made public. Up to the editor of PRIMS or another journal, but I don't see how an editor, faced with these reports, could possibly accept the proof and publish it. As in such cases, it's not the responsibility of the referees to keep working on this, once they feel they have found an error, explained what it is, and the author has no convincing response.

12. **Ryszard Kostecki**
September 21, 2018

I think it makes an interesting case that the Scholze-Stix paper is made public on the website of Mochizuki, together with a parallel emission of technically inconclusive yet strongly opinionated pieces by himself. This feels very paradoxal. It seems to me that all previous chapters of the story, including Mochizuki's refusal to travel abroad as well as his fabulous text "On the verification of inter-universal Teichmüller theory: a progress report (as of December 2014)" [http://www.kurims.kyoto-u.ac.jp/~motizuki/IUTeich%20Verification%20Report%202014-12.pdf%5D, which – if stripped out of the context – could be as well an artwork of a mathematically informed yet satirical western author, indicate some sort of difference of cultural values and mindset, rather than a personal oddity. I wonder, from the perspective of cultural anthropology (albeit I'm not an expert), to what extent such activity should be read in terms of the acting within the bounds of taboo of 'losing the face' (which is essential in Chineese and Japaneese cultures) or, more specifically and more speculatively, within the bounds of some sort of modernisation of samurai ethics. This would make it very different situation from e.g. the case of error(s) in Voevodsky's work(s). So, while the question how much of it can become useful in the long term run for the 'canonical' international mathematical community remains open, this story seems to carry also a meta-theoretical level that maybe could feed some PhD theses in (mathematically informed) social sciences.

As for science-fication of the inter-universal breath-taking (melo?)drama in the Hodge theatre [sic], here is a little fictional piece about the possible response to Scholze-Stix critique arriving from Mochizuki's camp, which follows the above anthropological speculation: http://www.fuw.edu.pl/~kostecki/inter-universal_samurai.html

13. **UF**
September 21, 2018

While I am by no means an expert on the subject (and agree that the over the top statements are not helpful), I have otherwise a somewhat different impression from reading the reports.

Scholze and Stix say at the beginning explicitly, that they use certain radical simplifications of IUT to get to the core of the proof and then show that this simplified procedure leads to nothing meaningful.

It seems to me that Mochizuki thinks, that if one makes these simplifying assumptions then one indeed arrives at a meaningless conclusion (specifically §12 (AD),(IUAD) in his report), but he disagrees that this would still be the case, if one does not make these simplifying assumptions (SSADFs).

I think this addresses the assertion that the procedure leads to a "meaningless result" by saying: Yes, the result of this procedure *under these simplifying assumptions* leads to meaningless result.

Mochizuki also attempts to explain why his procedure (without these simplifying assumptions) is supposed to be substantially different from a simplified procedure. Mochizuki seems to argue about a (more or less) specific simplified version of the theta link (§10 (SSid) in his report). He claims that while this specific simplified version (SSid) of the theta link is able (and well adapted) to satisfy a certain "switching condition" (SW), it is not sufficiently compatible with "large parts" of the ring structures on both sides of the theta link (i.e. he claims (SSidFs) it cannot satisfy condition 'Theta'CR from his §9).

I cannot judge the merits of this, but he seems to make specific claims, which properties fail in a certain simplified model (SSid). (I don't know how close/far (SSid) is from the simplified model Scholze and Stix consider in their reports).

Maybe relatedly, a rather stark contrast between the positions seems to be whether and to what degree anabelian geometry enters Mochizuki's argument: For Scholze and Stix it does not really seem to enter at all (their Remark 9), while Mochizuki seems to insist ((C5) in his July comments), that it is used in an essential way to guarantee certain symmetries of etale-like structures in the log-theta lattice.

These statements (and Q1,Q2 thereafter) seem to be purely about (non)existence of various versions of "theta links" with various properties (and about which compatibility properties of "theta links" are necessary to deduce Corollary 3.12). Thus they come logically before the disputed Corollary 3.12 (whose proof is based on the existence of a theta link with various properties). Corollary 3.12 seems to be the place, where the main (potential) problems seem to occur, since the reasoning in Corollary 3.12 leads (in the report of Scholze and Stix) to the meaningless inequality (1.6).

This could move the discussion a bit away from Corollary 3.12, whose proof may look quite initimidating, as it looks like it consists just of a summary/combination of nearly everything, which was achieved before.

14. **DrDave**
September 21, 2018

These are clearly ad hominem attacks leveled at people who are politely and rightly asking for clarification; the part about the group laughing out loud is particularly troubling as it has a sort of staged feel to it "a remarkably unanimous response of utter astonishment and even disbelief (at times accompanied by bouts of laughter!)." In addition, there is a the material that implies a lower academic level and "profound ignorance." These ad hominen attacks are now being extended to a wider circle. In this case, I think it is actually a good thing to point out what is obvious: the personal stuff weakens the argument. People should just focus on the math.

15. **Peter Woit**
September 21, 2018

UF (and others whose comments I've deleted…),

I don't think it's helpful for non-experts to try and argue here about the competing technical claims in these documents. Scholze and Stix are experts, have worked hard at this, interacted extensively and directly with Mochizuki, and are well aware of his response to their claims. They explicitly say that Mochizuki does not have a proof. Whatever Mochizuki says, the problem is not that they're ignorant people who aren't able to appreciate his arguments. It is of course possible that they are wrong and he is right. To show this, someone will have to identify a serious flaw in their argument, in a convincing way. Doing this will require a high level of expertise and some serious work. We're not going to see it in comments from non-experts on blogs a day after the documents came out.

16. **asdfasdf**
September 21, 2018

I am surprised that one of Mochizuki's friends or the university pr department has not gently suggested he revise his home page http://www.kurims.kyoto-u.ac.jp/~motizuki/top-english.html . The presence on the site of nonsensical legal proscriptions ("The author of this web site prohibits the use of the contents, such as images, of this site, as well as all linked sites, by the mass media") does not give a professional impression. The "Safety Confirmation Information for Shinichi Mochizuki" graphic on the "What's New" section is also rather strange (when clicked on, the graphic confirms that Mochizuki was safe on September 4, 2018, at 11:00 PM).

Some people, no matter how brilliant, are poor judges of how graphic and personal communications are received by others. Not sure why his supporters are not assisting him a bit here in an area – web page design – that is not his strength.

17. **mahmoud**
September 21, 2018

Is the optimism voiced by e.g. S above really warranted? True, actual mathematics has entered the discussion, but at the end we're left with Scholze&Stix saying that *The inequality derived in our simplified setup is trivial* and Mochizuki: *Indeed it is! And this shows that your simplification is wrong!* and he then dumps another 41 pages of analogies that doesn't seem to convince anyone (besides the people already sold on IUTeich). It's worth noting that S&S say that *they* are convinced the same problem they identify remains even without their simplifications.

So, is this situation any different from what has been going on all along? With the reaction of most experts being *We don't understand what Mochizuki is talking about* and his response: *You need to stop everything else you're doing and reflect on IUTeich in solemn contemplation for a few months and then you will see the Truth!*

18. **Simple**
September 21, 2018

Peter. Will you post something on Atiyah "proof" of the RH?

19. **sdf**
September 22, 2018

I'm still quite surprised that Scholze and Stix traveled to Japan at all… the only conclusion to draw from that is that they must have thought the gap could be fixed but the severely negative tone they take when explaining the gap in their write-up suggests otherwise, so I'm still rather mystified by all this…

20. **Peter Woit**
September 22, 2018

Simple,
No, and I don't think others should be publicizing this story, for reasons that I won't discuss here, but shouldn't be hard to figure out.

21. **Peter Woit**
September 22, 2018

sdf,
I think the reason Scholze and Stix went to Japan is pretty clear. They had decided it was important to resolve the issue of whether this was a proof, and the only way they were going to get to the bottom of things and find out if Mochizuki had an answer to the problem they saw was by talking to him in depth about it. He wasn't going to leave Kyoto, so they had to go there.

22. **FedupPleb**
September 23, 2018

Is not the whole point of a proof that it is an argument that convinces someone else?!
If you can't prove a thing to at least one other person, have you not by definition failed to prove anything?
Failure to be understood is the fault of the speaker!

23. **Aubrey de Grey**
September 23, 2018

Dear Peter (and others),

At this point (just a few days in), what is the state of expert opinion concerning the likelihood that detailed study of the two new papers will lead reasonably smoothly to a consensus on who is right? Focusing only on the substantive content, is there a sense that SM's response has a level of explanatory clarity comparable with the remark attributed to Scholze in Quanta that other number theorists "would have totally been able to follow the discussions that we had had this week with Mochizuki"? Or does SM's document seem, ro experts, to be just as impenetrable as the original papers? Or are things somewhere in between those two extremes?

24. **DH**
September 23, 2018

As a number theorist whose has previously put some effort into attempting to read the IUT papers, here are some miscellaneous thoughts after spending ~6 hours with the Scholze-Stix document and Mochizuki's report(s).

-First and foremost, Mochizuki does not explicitly address the main issue raised by Scholze-Stix, namely the necessity of differentiating between what they call "concrete" and "abstract" pilot objects. Nowhere in his report does one find any use of the words "concrete" or "abstract" in this context: he only refers to "*the* Theta-pilot object" and "*the* q-pilot object".

-The Remark which filled Mochizuki with undocumentable astonishment IS in fact a bit silly. However, it is completely immaterial w/r/t the substance of Scholze-Stix's objection, and could have been dropped from their document without affecting anything else.

-I find the discussion of "histories of operations performed on mathematical objects" in Mochizuki's report to be largely meaningless. A mathematical object is an object in some category, and/or a set with some extra structures; it does not have a "history". (Of course there are mathematical objects where one speaks of their histories, e.g. solutions of PDEs or trajectories of dynamical systems, but this is a precise technical use of the word.)

25. **Peter Woit**
September 23, 2018

Aubrey de Grey,

As far as I can tell, there already is a consensus. In a case like this, the burden is on the mathematician who believes they have a proof to convince other experts, and Mochizuki has conclusively failed to meet this burden. One can argue about the wide variety of claims he makes in response to the Scholze/Stix document (which is short, focused and precise), but the bottom line is that they found them unconvincing. They clearly put quite a bit of time into this, traveling to Kyoto and giving him a week to try and convince them. Mochizuki tries to explain away the problem of not convincing them by arguing that they are incompetent and need to spend more months studying his work. The first of these arguments is both ludicrous and offensive. The second is both not very plausible and likely to discourage anyone else from paying attention. If Mochizuki is saying you need to devote multiple Scholze-months of intellectual effort to understand why he is right, most mathematicians will figure that Scholze-months are equivalent to their years, so no point in even trying.

At this point the only way I see things changing is if some of those who supposedly understand the proof do a better job than Mochizuki of answering Scholze/Stix. The

"IUTeich FAQ" of Yamashita and Fesenko's recent article seem to be attempts along this line, but they're even less convincing than Mochizuki himself.

26. **UF**
September 23, 2018

DH,

SM addresses the issue of concrete/abstract pilot objects (to some degree) in (C16) in his comments to [SS2018-05].

27. **Mizan R Khan**
September 23, 2018

The mathematical community should ignore what is coming out of Kyoto. A number of very fine mathematicians have put in an inordinate amount of time and hard work in trying to understand Mochizuki's work and it looks like they have wasted their time. It is time for the community to move on!

28. **Marcus**
September 24, 2018

Wouldn't it be the right way for Scholze/Stix (or others) to respond to the mathematical content on the latest documents by Mochizuki (which is a response to the SS document) to move forward? Of course Mochizuki shouldn't try to make them look incompetent and this makes things suspicous in my view. But why is the mathematical content of the latest Mochizuki document 'not convincing'? This should be motivated mathematically.

29. **Peter Woit**
September 24, 2018

Marcus,
No, Scholze and Stix have no obligation to keep arguing with Mochizuki if they feel he doesn't have an answer to the problem with his proof they have pointed out. They have other, more important things to spend their time on. Scholze has been revolutionizing his subfield of mathematics and that's what he should be spending his time on, not arguing with an author who insults him and won't admit there's a problem with his manuscript.

One aspect of this whole story that hasn't been mentioned is that Mochizuki has made no substantive changes to his manuscript since his March meetings with Scholze and Stix. This is extremely odd. Normally if an expert tells you he thinks there's a gap or mistake in your manuscript giving a proof, even if you think this expert is wrong, you take this as an indication that you did not explain things well enough. Mochizuki is not just taking the attitude that there's nothing wrong with the logic of his proof, he's also taking the attitude that he does not need to even try to improve its exposition.

30. **HK**
September 25, 2018

I just find the comment of Mizan R Khan is bit offensive and political. It sounds like Univeristy of Kyoto is not a good at mathematics which is not true. I wonder why this comment is not sensored while other comments were taken out.
"The mathematical community should ignore what is coming out of Kyoto. …." You can take this comment out if you wish.

31. **Peter Woit**
September 25, 2018

HK,
I've deleted lots of comments, pro and anti-Mochizuki, on the grounds that they add nothing, just seem intent on carrying on a pointless argument. I interpreted Mizan R. Khan's "coming out of Kyoto" to just be a shorthand reference to Mochizuki and those around him who claim this is a proof, but have never been able to explain it satisfactorily to others. His "time to move on" take on this is a widely held one.

There is one other sense in which a complaint about "Kyoto" is justified. One of the main sources of the problem here seems to me to be the PRIMS refereeing of the paper. In the end, it appears they have decided not to accept the paper for publication, despite early reports they were going to do so, but that seems to have required the intervention of Scholze and other outsiders. The current state of affairs should have been reached much earlier, by expert referees in contact with Scholze and others who were pointing to the questions about Corollary 3.12.

32. **Mizan R Khan**
September 25, 2018

HK: I am very sorry. I should have put more thought into the phrasing of my statement about Kyoto. I did not mean any disrespect to the mathematical school in Kyoto. Clearly it is very strong mathematically.

When I said Kyoto I was solely referencing Mochizuki's work on abc. Mochizuki is
undoubtedly an extraordinarily talented mathematician. However, the enormous demands and expectations he has made of mathematicians of the stature of Scholze, Stix, et al is utterly ridiculous and self-centered! No matter how brilliant one is, one needs to show some consideration for other people's time and effort.

33. **Timothy Chow**
September 25, 2018

Peter, you wrote, "I've never heard of something like this happening before (experts writing long competing documents arguing about whether a specific part of a technical proof is correct)." Perhaps the closest analogue was the controversy over Hsiang's claim to have proved the Kepler conjecture. (Googling "hsiang hales rejoinder" will give you an entry point to the literature.) As far as I am aware, Hsiang still maintains that his proof is correct, but I am not aware of any other professional mathematician who publicly defends Hsiang's proof. One notable difference in this case is that quite a few mathematicians say that they understand Mochizuki's proof and believe that it is correct. If the current situation persists, with no argument materializing that persuades Stix and Scholze but with a sizable group of researchers continuing to study IUT in its current form, then I think that really would be unprecedented (at such a high level of mathematics, at least).

34. **Peter Woit**
September 25, 2018

Timothy Chow,
Thanks, that is an interesting historical analog, one that I was unaware of.

As for the claim "quite a few mathematicians say that they understand Mochizuki's proof and believe that it is correct", I've mainly heard that coming from Fesenko and Mochizuki, not from mathematicians themselves, especially not from well-known experts in the field. The Scholze-Stix paper may also change the minds of some who in the past thought the Mochizuki proof was probably right. At this point, saying you understand the proof and believe it is correct implies you know why Scholze-Stix are wrong, and can explain that to others.

35. **Andrew**
September 27, 2018

This is off-topic but the ICM plenary videos are now up (https://tinyurl.com/y96a9cn6). I've only watched Geordie Williamson's so far but IMO it is brilliant and of course there's also Scholze's!

36. **David Roberts**
September 28, 2018

I wrote some notes while trying understand the situation. They may be of interest to some people.

37. **David Roberts**
October 4, 2018

From Fesenko's new article:

> This oversimplification strikes as incorrect even people far from number theory, e.g. math physicists and categorists

Well if this isn't an anonymous pointer at myself and Urs Schreiber, then I don't know what is. Let it be put on the record that I do not claim Scholze and Stix's notes are incorrect, just that it is not clear how far they have moved from Mochizuki's work by their simplifications, because I cannot say for sure what Mochizuki's work is really doing, shorn of its verbiage, whereas I generally understand what Scholze and Stix have done.

Various and Sundry

September 28, 2018
Categories: Book Reviews, Uncategorized

First, news related in some way to Australia:

- This summer the Sydney Morning Herald published a nice profile of Geordie Williamson.
- By the way, the ICM plenary lectures are finally available on video, with Williamson's among those worth watching.
- The Sydney Morning Herald also recently had an article on quantum computing, motivated by a public talk by Patrick Hayden. The opening lines of the piece contain a classical superposition of quantum hype:

> Quantum computing will be so advanced that it will make your desktop computer look like an abacus, says Stanford University professor Patrick Hayden.
>
> However Professor Hayden, who will present a public lecture in Sydney on Wednesday, is keenly aware that "the hype is just out of control at the moment".

Among talks I wish I'd gotten to see or am sorry I won't be able to attend, there's

- The talks at the CMI at 20 conference this week in Oxford.
- Sabine Hossenfelder next week across the river.
- John Baez in Cambridge, talking about Unsolved Mysteries of Fundamental Physics.

If you just can't get enough of the debate over string theory:

- Over the last fourteen years I've written skeptically about Richard Dawid's defense of string theory as "post-empirical science", see here, here and here. For a new academic paper along similar lines, see Doubts for Dawid's non-empirical theory assessment.

On politics and quantum theory:

- I learned today from the Economist that the President of Armenia, Armen Sarkissian, is a theoretical physicist. Early in his career he worked in general relativity, see here. The Economist has Sarkissian promoting the idea of "quantum politics":

  > In his view, our interpretation of how politics traditionally works should be updated to reflect the way that physics has been reimagined. The classical world of post-Newtonian physics was linear, predictable, even deterministic. By contrast, the quantum world is highly uncertain and interconnected and can change depending on the position of the observer.
  >
  > "A lot of things in our lives have quantum behaviour. We are living through a dynamic process of change," he says. "I think we have to look at our world in a completely different way."

  I have no idea what's going on in Armenian politics and whether quantum theory is the way to understand it. As for the current horror-show that is US politics, one thing that doesn't deserve the blame for it is quantum theory.

A very quick mini-book review:

- I just got a copy of Alvaro de Rújula's Enjoy Our Universe, which is a short and entertaining, colorfully illustrated, overview of the current state of of high energy physics and the universe. The book brings back fond memories of a late-seventies course on particle physics that I took from de Rújula, whose humorous and lively character comes through in the book. For instance, about credit for discoveries:

  > There is increasingly convincing evidence that the Vikings set foot in America as early as the tenth century. There is no question that the Amerindians were there much before that. And yet, the glory of "discovering" America goes to Columbus. Thus, the point is not being the first to discover something, but the last.

  About the relation of theory and experiment (this comes with a hand drawn illustration):

  > In particle physics, discoveries – serendipitous or not – are generally made by *experimentalists*, in astrophysics and cosmology by *observers*. In both cases there are also the *theorists*. High time to explain the distinctions. This is done in Figure 53. The question is what the similarities between the two sets are. One set consists of a farmer, his pig, and the truffles, the other of the theorist, the experimentalist (or the observer), and the discoveries. The farmer takes his pig to the woods. The pig sniffs around and discovers a truffle. The farmer hits the pig with his bat and takes the truffle away. These are the similarities. The difference is that the theorist scarcely ever directs the experimentalist to woods where there are truffles.

  Beside the humor, the book is mostly succinct, clear and profusely illustrated explanations of important physics and astrophysics. The author early on explains that he plans to avoid discussing the sort of speculation popular in many other books, with a footnote justifying this:

  > There is nothing wrong in discussing these subjects, except, in my opinion, doing it without a very clearcut distinction between facts, reasonable conjectures, and outright fantasies.

Update: Some news and views on an open access development, courtesy of Mark Hillery:

- "Plan S has been put forward by a consortium of European funding agencies, including those of the UK, France, and the Netherlands, though not, as of now Germany, and it would require recipients of their funding to publish in gold open-access journals or vaguely defined compliant open access platforms by 2020. Hybrid journals, such as the Physical Review, will not be allowed. Gold open access requires that authors pay to have their papers published. The claim is that a cap on article processing charges (APC's) will be mandated, but the details have not been spelled out yet. More information can be found here.

  http://www.sciencemag.org/news/2018/09/european-science-funders-ban-grantees-publishing-paywalled-journals

  A good discussion of open access can be found here.

  https://otwartanauka.pl/in-english/experts-on-open-access/open-access-will-remain-a-half-revolution-interview-with-richard-poynder

  This is an attempt to force the gold open access model on all of scientific publishing. In a rebuttal to Plan S,

  > Response to Plan S from Academic Researchers: Unethical, Too Risky!

a group of young European researchers has pointed out that it would prohibit them from publishing in 85% of existing journals. They also point out a number of additional problems with Plan S.
1. While anyone can read an article in a gold open access journal without charge, publishing in one is a different story. APC's, or what used to be known as page charges, are typically several thousand dollars per article. This seriously restricts the pool of people who can publish is such journals.
2. What happens if the rest of the world does not go along with Plan S? Collaborations between EU and non-EU researchers would not be able to publish their results in many high-impact journals (Physical Review Letters, for example), and this could discourage such collaborations. It should be noted that Robert-Jan Smits, the Open Access Envoy of the European Commission, is tying to persuade funding agencies in North America to join in Plan S.
3. Telling people where they can publish violates academic freedom.
4. In a gold open access journal, the financial incentives favor publishing lots of papers; the more papers published, the greater the income of the journal. This could lead to quality problems.
The rebuttal also points to possible alternatives to Plan S, such as green open access, which would allow a researcher to deposit a version of their paper in an online depository, such as the arXiv, at the time of submission and then submit the paper to a journal of their choice.

While I am not a fan of commercial scientific publishers, whose profit margins are ridiculous, I am a fan of society journals (I work part time for one, Physical Review A). These journals are reasonably priced, and income from them helps support societies, such as the American Physical Society, and their activities. Plan S is a bureaucratic attempt to impose, from the top, a publishing model on the world with which many people disagree or have grave reservations."

Comments

1. **Sebastian Thaler**
   September 28, 2018

   Speaking of Richard Dawid and books, the lectures from his "Why Trust a Theory?" workshop are being published in the spring:
   https://www.cambridge.org/us/academic/subjects/physics/history-philosophy-and-foundations-physics/why-trust-theory-epistemology-fundamental-physics?format=HB

2. **Shantanu**
   September 30, 2018

   Peter: What does De Rujula say in his book about super-symmetry and BSM theories in general and also about string theory etc?
   shantanu

3. **CWJ**
   September 30, 2018

   As a researcher with an incredibly tight research budget, I find "no page charges" a greater benefit than "open access." If we had to pay for every article published, that would throw grant budgets into considerable chaos–and probably cut back on funding for grad students.

   Green open access works perfectly well.

4. **Matt Grayson**
   September 30, 2018

   But the Nobel Prize goes to the pig.

5. **Peter Woit**
   September 30, 2018

   Shantanu,
   de Rujula deals with string theory and most BSM stuff by ignoring it completely. Supersymmetry is mentioned in one footnote.

   Matt Grayson,
   Not necessarily, and especially not recently in HEP, where the experimental groups are large, without a recognizable leader. For example, in the case of the Higgs discovery, the Nobel went to the theorists, not to the experimentalists who made the discovery.

6. **Narad**
   September 30, 2018

   > I am a fan of society journals (I work part time for one, Physical Review A). These journals are reasonably priced, and income from them helps support societies, such as the American Physical Society, and their activities.

   I couldn't agree more, having worked as a manuscript editor on the portfolio of the American Astronomical Society. I cannot for the life of me figure out how, e.g., PLoS One (which doesn't bother with such niceties as editing in the first place and doesn't produce formatted PDFs) can justify so much as $1595, for "publication expenses – including those of peer review management, journal production and online hosting and archiving."

   I've worked as a freelance editor since, and the money is a pittance. Even if some of the Gold OA journals actually engage skilled editors (who are few and far between, especially when it comes to nonnative speakers), I doubt that it's a significant part of the overhead. Formatting? Give me a break. Sizing art is not a big deal, and nobody's going to fix your horribly prepared LaTeX.

   At least the AAS journals, after IoP took them over, paid a nominal 30 dollars an hour (more like 14 in real life) and kept the page charges low. One-year embargo, and your paper is open-access, and IIRC, you could host a copy yourself.

7. **Matt Grayson**
   September 30, 2018

   Peter,

   True, and Einstein was a theorist, too. I was thinking of the CMB prize, and just pointing out that it isn't quite as dire as the quote indicated. In the case of the Higgs, didn't the theorists, in fact, lead the experimentalists into the woods?

8. **SITPWatcher**
   September 30, 2018

Tom Rudelius from Princeton will be speaking October 1st at the SITP.

https://events.stanford.edu/events/798/79814/

9. **Peter Woit**
October 1, 2018

SITPWatcher,
Thanks, interesting to see that Team Stanford is inviting Team Vafa to play. I haven't checked to see if any of Team Stanford has been invited to Harvard. As noted in a previous posting, one of Vafa's motivations for this seems to be that he feels that Team Stanford's multiverse mania is discrediting string theory. Rudelius appears to have some other motivations for challenging the multiverse, see
https://www.youtube.com/watch?v=G4Pdz1S-6kc
and
http://augustinecollective.org/string-theory-multiverse-god/

10. **Francesco Ginelli**
October 1, 2018

Forcing golden open access is a disgrace, evidently pushed by people without any reasonable understanding of how collaborative science works. And no interest in practically solving the problem of granting a wider access to research papers.

The argument that funding agencies have the right decide what should be done with their money is a slippery slope. Public funding agencies should use public money to support academic freedom, not promote political agendas which suppress it.

Moving the cost from the reader to the writer is going to create obvious problems. Starting from incentives for journals to publish abysmally low quality papers to the problem of publication access for researcher that do not directly control the funds. Wait for the day a lab director with a tight budget has to chose which paper publication fees to cover.
This agenda should be resisted by the academic community.

I typically publish all my paper in green open access on the arXiv the very day I submit them (as much as I can to no-profit publishers such as APS, I do not see why private institution should profit from my work and public funds) and the system works perfectly. To push open access, it would be enough to forbid embargos and let everyone go green.

Frankly, seen from UK it looks like an effort to further compress regular research costs (standard journals fees in this case) and distribute even more money via the winners take all grant system. Which is of course is a well documented trend, which places more powers in the hand of funding agencies undermining academic freedom (UK being bottom in EU).

11. **Narad**
October 1, 2018

The AAS pricing structure is nothing if not baroque, but the Gold OA premium can be discerned.

12. **John Duffield**
October 2, 2018

John Baez's slides from his Unsolved Mysteries talk can be found on his web site/ He says *"In this century, progress in fundamental physics has been slow. The Large Hadron Collider hasn't yet found any surprises, attempts to directly detect dark matter have been unsuccessful, string theory hasn't made any successful predictions, and nobody really knows what to do about any of this"*. I do. You go over the old ground playing the detective looking for clues. And you find them everywhere, scattered around like low-hanging fruit. It's like Bert Schroer said: *"Perhaps the past, if looked upon with care and hindsight, may teach us where we possibly took a wrong turn"*.

13. **Yemon Choi**
October 2, 2018

Given that you mention Plan S and then also link to that interview with Poynder, might be worth adding a few words to clarify that the interview/discussion seems to date from 2016.

(FWIW I'm generally sympathetic to Poynder's views/analyses)

14. **Fred P**
October 2, 2018

As a consumer of research, I'll note that Green open access is more useful than Gold or a journal publication. Simply put, I have to look through fewer places to find research.

15. **Francesco Ginelli**
October 3, 2018

Fred P: exactly.
These days, even as a professional physicist, it is enough to skim through titles and abtracts of your arXiv daily list of papers from your chosen field(s) and that's it.

This not to say that the peer review validation is not extremely important, or that we could do away with journals (no we can't). It just points out how useful green repositories are.

16. **Marko**
October 3, 2018

After reading through all the relevant stuff, it seems to me that the essence of plan S has nothing whatsoever to do with either moral or practical benefits of open access publishing. Rather, this looks like a straight boxing match between the publishers, whose profit margins are outstandingly big, and science funding agencies, who are done watching that outstanding amount of money going to publishers over and over.

So funders decide to exercise their power over scientific researchers by imposing a requirement to publish exclusively in journals which are gold open access, and — crucially — put a (yet undefined) cap on how big a publisher's profit-per-paper can be. Simple, crude, accross-the-board calculated strategy, with the aim to blackmail publishers into (a) becoming gold OA, and (b) setting their gold OA fees to this overall cap. Otherwise, they risk being strangled with no papers to publish. If plan S receives global support from other funders worldwide, and this kind of blackmail gets enforced onto the publishers by the entire global scientific community, the funders will gain the power to dictate the gold OA price of the article to the publishers, rather than the other way around. Then, over time, the funders can gradually lower the cap to reasonable APC's, and the publishers would have to either obey or perish. One might call this kind of game bullying, but I guess this is a perfectly normal thing in the business world (as opposed to the world of academia).

Crucially, this kind of game relies on global support for plan S by all funders worldwide. But given the current outrageous profit margins of the publishers, it's actually quite likely that plan S will be met with positive sentiment by funding agencies across the board. Basically everyone, funders, librarians, scientific community at large, etc…, seem to be sick of being siphoned for outrageous amounts of money by the publishers, for very little added value.

It is important to note that this is a power struggle between two parties *with power* — namely those who give money for scientific research (funders), and those who take the money to transform that research into a quantifiable product (publishers). The scientists are a third party, the one with no power, since they depend on both funders and publishers. So nobody is asking their opinion, and nobody cares what this opinion might be, because both the funders and publishers have a clear-cut calculation in this whole turf-war. Whatever happens to scientific research, publication quality, international collaboration, young scientists, etc…, is just collateral damage, neither of the two big players could really care about. They only care about the flow and the amount of money.

I'm actually quite interested to see who is going to win this one, and at what cost…

🙂
Marko

17. **Peter Erwin**
October 3, 2018

At least the AAS journals, after IoP took them over, paid a nominal 30 dollars an hour (more like 14 in real life) and kept the page charges low. One-year embargo, and your paper is open-access, and IIRC, you could host a copy yourself.

All the main astronomical journals (the AAS journals, *Monthly Notices of the Royal Astronomical Society*, *Astronomy & Astrophysics*) allow posting preprints on the arXiv (and I believe always have), and everyone does this, so the entire field is "green open access." None of these journals meet the "Plan S" requirements, however, so astronomers funded by the agencies taking part in Plan S would be unable to publish in any of the field's main journals.

18. **anon**
October 3, 2018

Actually, why do libraries subscribe to astronomical journals? I work in astrophysics, but I don't even remember the last time when I read an article in a journal instead of arXiv (except maybe some old pre-arXiv articles, which are open access anyway).

19. **a1**
October 3, 2018

For the moment some of the major publishers are in position to collude with funding agencies to siphon money for a few more years and plainly that is what they are trying to do. The key claim is that only they are able to organize (and maintain) a good peer review system. The reviewing however is done by the scientific community. There are actually some journals that are just an index to papers from the arxiv which have obtained peer review approval. Thinking along these lines will make it obvious that publishers are easily eliminable: one has to realize that during the 20th they became involved in academic publishing mostly as distributors but currently they are obstructing the circulation of papers.
Let's hope that Gold OA wont get off the ground because if that happens soon the publishers will start lowering quality with the argument that the APCs are insufficient and need to be raised. The scientific community a sure loser for one more time.

20. **Narad**
October 3, 2018

> Actually, why do libraries subscribe to astronomical journals? I work in astrophysics, but I don't even remember the last time when I read an article in a journal instead of arXiv (except maybe some old pre-arXiv articles, which are open access anyway).

I dunno; the language improvements can sometimes be significant, and I've prevented more than one erratum. I've also found a surprising number of errors in supplementary tables. I realize that it's a field where one can pretty much rely on the authors, but there's always room for polishing. (Even the cleanest manuscript I ever saw admitted two reference corrections. Taylor was enthused, as I recall.)

@Peter Erwin: I was referring to the published version, not a preprint.

21. **Peter Erwin**
October 3, 2018

@ anon:

Actually, I frequently read articles in journal form (and prefer to get the official journal PDF rather than the arxiv PDF, if possible), for several reasons. One is what Narad said: it's nice to have an extra round of language and proofreading improvements. Plus its nice to have a properly formatted articles, given that some authors still post their manuscripts to the arXiv in outdated "referee format" (single-column, figures-at-the-end, or even double-spaced). Sometimes it's quicker to skim through an HTML page rather than wait for a PDF to download, and sometimes it's nice to be able to retrieve data tables in simple-text form from the journal. And there are still way too many authors posting to the arXiv who aren't aware that the gradually increasing arXiv file-size limits means they usually don't have to provide their figures in heavily compressed, semi-illegible JPEG format any more.

(I am, of course, talking about online versions of the journals; I'll admit I can't remember the last time I read an article in a physical paper journal.)

22. **Dr Beaker**
October 4, 2018

The SMH story on Geordie Williamson was actually published in winter.

23. **Marko**
October 4, 2018

a1,

"Let's hope that Gold OA wont get off the ground because if that happens soon the publishers will start lowering quality with the argument that the APCs are insufficient and need to be raised.”

Lowering quality (i.e. accepting more submitted papers than usual) will eventually reflect in the lower impact factor (since the latter is just the ratio of cited and published papers). Note that the funders will retain the IF as a metric of publication quality, but now with OA enforced and a cap on the APCs. Various publishers will have to compete for IF/APC ratio, as opposed to the current situation where they are trying to maximize both IF and APC independently.

So I don’t think that publishers will dare lowering the quality, since then the IF of their journal will drop wrt. to competition, and this will reflect badly in their relevance in the publication market — since scientists will keep getting grants based on their accumulated IF.

I don’t like IF as a measure of quality, but I bet that both funders and publishers will continue to use it as a primary metric — they have nothing else available. Btw, as a side-effect of OA, it will be substantially easier for any third party to calculate the IF of any journal — publishers will not be able to hide the citations behind a paywall anymore.

🙂
Marko

24. **Narad**
October 4, 2018

Marko,

> Lowering quality (i.e. accepting more submitted papers than usual) will eventually reflect in the lower impact factor (since the latter is just the ratio of cited and published papers).

I don’t generally like to cite The Scholarly Kitchen, as I view the general tone as being one of being apologists for industry, but they do have an item from 2017 showing that the IF of *PLoS One* has been dropping despite reduced submissions. The traffic instead seems to be going to Springer’s competing megajournal *Scientific Reports*.

The only criterion for acceptance in *PLoS One* was some sort of methodological soundness; *Scientific Reports* advertises its own similarly, for “all scientifically valid research.” I really have no idea what’s driving the shift.

25. **Francesco Ginelli**
October 4, 2018

Marko,
leaving society journals (like the superb gamma of Phys Rev by APS) out of your consideration is a dire omission.
They are essentially no-profit operations and do not squeeze money out of public funded research into private hands. They typically do not impose embargos and are the ones risking to be penalized from plan S and should be protected from it (explicitly stating that green open access without embargo is fully OK).
If you want to go after big private publishers, there are other ways, like refusing to referee for them (admittedly difficult if Nature is asking — as one would like to have a say in selecting the high profile papers in her/his community — but easier on other mid-range publications).
Or if you sit in hiring committees, stopping taking Nature & Science publications as automatic conditions for tenure.

For the remaining of your arguments, the too vague formulations about price caps leaves me with little hope that they will be effective. And the observation that a pay to publish model will not decrease quality will only work for top journals, mid range ones which already have a “normal IF” (lets say in the 2-5 range) will predictably just go for volume and more fees.

26. **chorasimilarity**
October 5, 2018

Plan S is the worst among the better solutions, so there are easy to find pros and cons. It is good news for publishers and funding agencies, bad news for researchers, because better solutions already exist, like green OA and Open Science.

27. **Brian Dolan**
October 10, 2018

I agree with Francesco Ginelli. Personally I do not believe that journals serve any function whatsoever in the dissemination of scientific knowledge in physics. The archive does that perfectly well (at least for physics). Journals are only relevant for job, promotion and grant applications. I am totally against gold OA as it essentially bars anyone without a grant from publishing (and as a theoretical physicist working in the Republic of Ireland, where the principle government funding agency does not fund basic science with no immediately foreseeable applications, I do not have a grant). In my opinion gold OA is nothing more than a way of funneling tax payers money into the pockets of commercial publishing houses, via government grants. No wonder that publishers are in favor of it.

28. **David Roberts**
October 10, 2018

For those that aren’t aware, the potted history of Gold OA (named because Gold=money, rather than Gold=’gold standard’) is this: Public Library of Sciences started up with a journal in biology (now *PLOS Biology*), and needed to fund it somehow, and since biologists paid page charges anyway, it wasn’t an issue to change from paying page charges for toll-access journals to Article Processing Charges. Especially as the APC was set at about the level that the cash-rich biologists were *paying anyway*: ~$1500 was the original price. See this archived FAQ page where it is said

> We ask that–as a small part of the cost of doing the research–the author, institution, or funding agency pays a modest fee, $1500, to help cover the actual cost of the essential final step, the publication. (As it stands, authors now often pay for publication in the form of page or color charges.)

and

> The ability of authors or their institutions to pay publication charges will never be a consideration in the decision whether to publish.

The journals that followed were all in similarly expensive areas of research: *Medicine*, *Computational Biology*, *Genetics* and *Pathogens*. So if you were already doing research in these areas, a few thousand dollars on top of the massive cost of doing the actual research was just change.

It’s when people for whom a single APC costs the same as their annual conference budget (if any), and they have no other large costs, are forced into paying similar fees that the model breaks down.

29. **anon**
October 11, 2018

I find it very strange that not-for-profit PLoS journals have page charges that are at the same level as those of commercial open-access publications. Especially as they make such a big deal of being there to serve scientists and the public good. Yet their CEO is paid $500k a year…

30. **Narad**
October 12, 2018

> Yet their CEO is paid $500k a year…

Even society journals are not immune to the flowing of money upward to administrative positions, rather than having it plowed back into development or, heaven help us, staff salaries. The difference, at least at the university press I worked for, was that the total amount was capped by the fee-for-service model. The spillover went to prop up the money-losing books division of the outfit. “Make it up in volume,” as they say.

High Life

October 3, 2018
Categories: Film Reviews

I spent yesterday night at the New York Film Festival, watching Claire Denis's new film High Life. For a detailed and accurate review of the film, see the one at Variety.

This film is about a voyage to a black hole, in some sense an anti-*Interstellar*. Where the scientific plot of *Interstellar* was inspirational and made no sense at all, in *High Life* you get a plot that is all too plausible, and completely depressing. There's a spaceship headed on a mission to a black hole, but this one doesn't have brilliant scientists, traveling in a clean and shiny environment, and out to save the world. Instead, the crew is a bunch of ex-Death Row inmates, stuck on a dead-end trip in a filthy spacecraft swarming with recycled excrement, being subjected to grotesque sexual experiments, with periodic violent assaults, murders, and screaming babies to liven things up.

The supposed mission of the spacecraft is to travel to a nearby black hole and test whether energy can be extracted by the Penrose process. Because of all the murdering and such, that doesn't work out too well. The ending involves another trip into a black hole, with discussion of whether they're going to hit a "firewall". One character thinks not, but that sure looks like one to me at the end. Theorist Aurélien Barrau is listed as "Cosmic Companion" or some such, and must have been responsible for providing the higher level of scientific verisimilitude than that of *Interstellar* (one of the images of a black hole does look like the famous one Kip Thorne provided for the earlier film).

I can't really recommend this film to the average viewer seeking enlightenment or entertainment. On the other hand, if you're looking for something unrelievedly grim, grotesque and disturbing, and really like black holes, maybe you should check it out.

Comments

1. **David Appell**
   October 3, 2018

   Peter, is this the picture of a black hole, from Interstellar, that you have in mind?

   https://goo.gl/images/LlWOau

2. **Peter Woit**
   October 3, 2018

   David Appell,

   Yes that's it. The one in High Life is somewhat different, but similar.

3. **Jon Orloff**
   October 3, 2018

   Sounds charming. Can't wait until it opens somewhere nearby.

4. **tulpoeid**
   October 4, 2018

   Excuse me Sir, Interstellar didn't have any spaceship with brilliant scientists.

   (You can tell I was annoyed by the naivety; however, it was admittedly "inspirational" to a wide audience, even though or, I'm worried, maybe because it "made no sense at all".)

5. **Low Math, Meekly Interacting**
   October 4, 2018

   Because I liked "Alphaville", some joker at work deduced I would enjoy this too. Even encouraged me to skip an off-site with him to go see it. Yeah, no.

   I thank you doubly for the review.

6. **CWJ**
   October 4, 2018

   I hated Interstellar. Almost nothing about it made sense. The planet-destroying plague made no sense. The shuttle craft that can fly in and out of a massive time-dilation gravity well, but doesn't work when moist. And worse of all, the effectively gobbledy-gook about the magical future aliens who rescue Matthew McConawhogivesafig. Yes, I recognized the words they were using–but they would be nonsense syllables to anyone not a physicist. It claimed to be physics, but plotwise, it functioned as magic.

   I did like the robots–non-humanoid, very interesting. And, actually, if they had ended the film when MM flies into the black hole, it would have been a much better film.

   Too bad High Life sounds bad, just in a different way.

7. **Petite Kabylie**
   October 5, 2018

   Dear Peter,
   How come there is no official trailer on YouTube for this movie?
   I can't find any elsewhere either. Could you provide a link where I can get an idea about the movie before going to see it?
   Thanks

8. **Amitabh Lath**
   October 5, 2018

   Wow, black holes are now a movie genre! I remember my parents bought us fitted bedsheets from Disney's "The Black Hole" (probably because they were cheaper than Star Wars ones and they didn't know the difference). Then there was Lawrence Fishburne in "The Event Horizon". Add in Interstellar and now High Life and pretty soon singularities will need their own agent.

9. **Peter Woit**
October 5, 2018

Petite Kabylie,

As far as I know, there's no trailer for the film. This may be related to it only recently finding a distributor. For more details of the film, see the Variety story I linked to, or here's another one that just appeared
https://www.filmcomment.com/blog/film-week-high-life

10. **Narad**
October 5, 2018

> Wow, black holes are now a movie genre!

I still recall 1975's "Into Infinity" from the afternoon show NBC's Special Treat.

11. **Jack Morava**
October 14, 2018

Claire Denis is an astonishing director, comparable to Tarkovsky,
cf eg Solaris, https://www.imdb.com/title/tt0069293/

Trouble every day, https://www.imdb.com/title/tt0204700/
for example is a kind of vampire movie;
The intruder, https://www.imdb.com/title/tt0110171/
is about loneliness, as is
I can't sleep, https://www.imdb.com/title/tt0110171/

They are all haunting, perhaps not for the faint of heart…

12. **Jack Morava**
October 15, 2018

Sorry, my bad:

The intruder, https://www.imdb.com/title/tt0422491/

13. **Peter Woit**
October 15, 2018

Jack Morava,

Thanks! I thought highly of some of the other Claire Denis films I've seen (so was looking forward to this one), recall in particular Beau Travail and Trouble Every Day. This film is definitely strongly influenced by Tarkovsky's Solaris, the spaceship environment and use of flashbacks are similar.

14. **Christopher Sahnwaldt**
October 15, 2018

Looks like a trailer for High Life was published on October 12:

https://www.youtube.com/watch?v=5jDVb8AwfG8

Peter, Jack – thanks for the recommendations!

This Week's Hype

October 15, 2018
Categories: Swampland, This Week's Hype

The story of string theory as a theory of everything has settled into a rather bizarre steady-state, with these three recent links providing a look at where we are now:

- At his podcast site, Sean Carroll has an [interview with string theorist Clifford Johnson](). It's accurately entitled *What's So Great About Superstring Theory*, since it's an hour of unrelenting propaganda about the glories of string theory, save for a short mention that there had been some criticism from (unnamed) sources a decade or so ago.

  The truly odd thing about the discussion though was the way it seemed frozen in time back in 1998 just after the advent of AdS/CFT duality, with almost no discussion of developments of the last twenty years. Nothing about the string theory landscape and the controversy over it, nothing about the negative SUSY results from the LHC. The attitude of Carroll and Johnson towards the failure of string theory unification seems to be to simply refuse to talk about it, and try to keep alive the glory days just after the publication of *The Elegant Universe*. They've taken to heart the post-fact environment we now live in, one where if you keep insisting something is true (string theory unification is a great idea) despite all evidence, then for all practical purposes it is true. Johnson has famously admitted that he refuses to read my book or Lee Smolin's. As far as he's concerned our arguments do not exist, and Carroll goes along with this by not even mentioning them.

- For the latest on the Swampland (for background, see [here]()), there's [String Theorists' Heads Bobble Over Potential Dark Energy Wobble](), where we're told that string theorists are claiming "huge excitement" over the possibility that string theory might make a "prediction" about dark energy. Over the years there have been endless claims about "predictions" of string theory, none of which have ever turned out to actually exist, and this is just one more in that long line. The rather odd aspect of this latest prediction is indicated by how it is described in the last paragraph of the article:

  > The real excitement comes from how soon we might know whether Vafa's work has produced a testable prediction of string theory—which would be a first. Experiments like the Dark Energy Survey or the upcoming WFIRST telescope could possibly detect whether dark energy is constant or changing over time, and could perhaps do so within the next few years.

  Reading this, one gets the impression that we'll know what string theory "predicts" about dark energy just when there's a measurement. This actually does describe what's going on here: for some, string theory is a theory of everything as a matter of faith, so to them any new measurement tells us more about string theory, in particular that string theory "predicts" that measurement.

- Finally, there's [an article out by Thomas Hertog](), which contains more about his work with Hawking that was widely advertised after Hawking's death (see [here]()). Hertog claims another sort of "prediction" of string theory:

  > String theory predicts that our universe is fundamentally a hologram that reveals itself only in the most extreme conditions, such as those at the Big Bang.

  For the implications of this prediction, see [String Theory Summarized]().

Comments

1. **Paul in Tacoma**
   October 16, 2018

   Hi Peter,

   I heard about Vafa's "prediction" from another article(https://www.sciencedaily.com/releases/2018/10/181009102431.htm) the other day, where the impression I received was that if Vafa was correct then String Theory was essentially being ruled out based on current observations of dark energy: "String theory is said to be fundamentally incompatible with our current understanding of "dark energy" — but only with "dark energy" can we explain the accelerated expansion of our current universe". So the "excitement" seemed to arise just from the fact that a testable prediction was finally at hand!

   My questions are does this prediction really count against String theory based on our current understanding of dark energy? And if not, or if more observations are needed to test the prediction, what are those observations, and when might they be made?

2. **Geoffrey Dixon**
   October 16, 2018

   "Science advances one funeral at a time." This century-old meme is no longer relevant, methinks. I believe it was Sabine who pointed out that the easiest way for a financially strapped department to give itself some cachet is to hire a string theorist. It's self-perpetuating. It's alive. It is not enough that it fail; something else must succeed.

3. **Peter Woit**
   October 16, 2018

   Paul in Tacoma,
   I've added an answer to this as an FAQ, see
   http://www.math.columbia.edu/~woit/wordpress/?wp_super_faq=ive-just-read-that-string-theory-has-finally-made-a-prediction-isnt-that-exciting

   If you really want to know what's behind the misleading hype, see the more recent postings in this category
   http://www.math.columbia.edu/~woit/wordpress/?cat=27

4. **Peter Woit**
   October 16, 2018

   Geoffrey Dixon,
   Hiring a string theorist is no longer an obvious move for a physics department trying to get some cachet and show it is on the leading edge. Most physicists, grant officers and university officers have by now noticed that string theory has been promising breakthroughs for over thirty years, but nothing much has come of it, and skepticism is now widespread. It's exactly because of this worsening environment for string theory hires that those invested in it are involved in publicity efforts to try and prop it up.

   Unfortunately I think you're right though that the fad won't just die off naturally, absent something else that comes along to replace it. It has become oddly institutionalized, with a large and powerful group of "string theorists" of a wide variety of ages, most of whom have stopped working on string theory, but still retain "string theory" as a tribal affiliation.

5. **Atreat**
October 16, 2018

Paul in Tacoma,

My best attempt to understand that sciencedaily article…

Vafa group says assumptions that lead to Landscape are incompatible with constant dark energy because of other assumptions. Prediction: other assumptions are correct and thus Landscape/Multiverse assumptions are incorrect. Vindicates: A future "String Theory" that no one knows how to write down minus the Landscape/Multiverse ie., the hope that someone will find a pearl in the Swampland.

Stanford group says assumptions that lead to Landscape are correct and Swampland are incorrect, but hedging bets by saying dark energy might not be constant. Prediction: that dark energy will be constant and Swampland assumptions incorrect OR dark energy is inconstant and while Swampland assumptions are incorrect, they are based on other faulty assumptions ie, constancy of dark energy. Vindication: Anthropic Landscape with no need for finding a future "String Theory" cuz why bother since the multiverse did it.

Own prediction: no matter what happens experimentally neither side will rule out "String Theory" and claim vindication in some way meanwhile the rest of physics will move on ignoring them.

6. **NotNot**
October 16, 2018

Thanks for the references.

I think it's different to say 'we can't predict anything because we don't have a paradigm' than to say 'we can't predict anything because we have a paradigm and that's what it predicts'.

7. **Tsetrot**
October 17, 2018

You seem to be often scandalized that person X calls oneself a "string theorists" while not working on "string theory". First, who defines what string theory is? Second, who cares? The kind of research discussed at Strings conferences is not discussed at any other major series of conferences. So that series of conferences serves its intellectual purpose. And much of it is valid and great research by any measure (e.g. work on strongly coupled QFT).

8. **Peter Woit**
October 17, 2018

NotNot,

Feynman said it clearly 30 years ago: "String theorists don't make predictions, they make excuses ." Sometimes the excuse is "we don't understand the theory well enough", sometimes it's "we understand the theory and it is of a nature that makes no testable predictions". These are really all the same thing though, excuses for a failed idea about fundamental physics.

9. **Peter Woit**
October 17, 2018

Tsetrot,

I don't think "string theorists" calling themselves whatever they want is scandalous, what's scandalous is misleading the public about "string theory", the way Carroll and Johnson are doing.

I do think though that people interested in solving problems of strongly coupled QFTs are making a big mistake by deciding to adopt an inappropriate name for themselves and what they're doing. Why organize yourself in a tribal manner, branded with the name of a failed idea, and end up getting associated with very prominent ridiculous pseudoscience like the Landscape? Why not disown the failed ideas? In particular, why not change the name of your yearly conference?

10. **Tsetrot**
October 17, 2018

It's because what you brand as "failed idea" has not failed. It's an existence proof that the problem of quantum gravity has a weakly coupled solution. Even if we don't know yet which solution was chosen by Nature, an existence proof is better than nothing. Some discussed ideas may have some questionable parts here and there, and you choose to emphasize them as a sign that the whole field is rotten. That's wrong – the bulk of string theory is here to stay. It's based on actual correct and highly nontrivial calculations, related to properties of 2d CFTs and to moduli spaces of (super)Riemann surfaces. When I do strongly coupled QFT I use the results about CFTs obtained by string theorists. If they invite me to their conference, which they decide, for sentimental historical reasons or whatever, to call "Strings", I go. I love to speak to the audience of people whom I respect intellectually, and this is the case for that crowd. There are other conferences out there with sentimental historical names, who cares. You call this tribal as a pejorative, but there is also a good sense of tribalness. There is simply no other community devoted to think hard and steady about tough nonperturbative QFT questions, and having necessary mathematical background for that.

11. **Peter Woit**
October 17, 2018

Tsetrot,
Sorry, string theory as an idea about unification has failed, conclusively. Those who promote it to the public without acknowledging that "the stuff we told you about all particles and forces being vibrations of strings doesn't work, now we just mean an untestable idea about gravitational degrees of freedom at unobservable scales which doesn't really quite work either, but sucks less than the competition".

Sure, there are now often quite interesting talks about QFT (perturbative and non-perturbative) being given at "string theory" conferences. I disagree with you though that the tribe of "string theorists" have a monopoly on being willing to think hard about non-perturbative QFT.

12. **Peter Orland**
October 17, 2018

As Peter W. says, a minority who work on nonperturbative aspects of field theory make a point of not calling themselves string theorists (I avoid the label and I know others who do, from private discussions). Their reason for shunning the label is not just that it is a misnomer, but that it gives the impression to non-field-theorists that their work is connected to the better-publicized aspects of string theory (the landscape, etc.).

13. **Tsetrot**
October 17, 2018

Axions nor magnetic monopoles nor proton decay have not been found yet, but this does not mean all those ideas have failed conclusively. They may still be revealed experimentally one day. Supersymmetry is no different. Perturbative string theory is no different. It's just an idea about how high-scale physics can be made. It's mathematically consistent, but as yet experimentally untested.

14. **Peter Woit**
October 17, 2018

Tsetrot,
Perturbative string theory (1od, remember) is not just "experimentally untested" (and, by the way, it's not completely mathematically consistent either…). Describing string theory's problem as "not yet experimentally tested" is extremely misleading, since it implies you're talking about some well-defined predictive theory that just happens to be hard to test. One needs to figure out how to get 4d known physics out of the theory, and all attempts to do that have failed. One can argue about how many decades of failed work by thousands of people need to go by before you can describe the failure as "conclusive", but I think we passed that point a while ago.

Breaking News

October 17, 2018
Categories: Uncategorized

Two midday breaking news items:

- The ACME II experiment is reporting today a new, nearly order of magnitude better, limit on the electric dipole moment of the electron:
  $$|d_e|\leq 1.1 \times 10^{-29} e\ cm$$
  The previous best bound was from ACME I in 2014:
  $$|d_e|\leq 9.4 \times 10^{-29} e\ cm$$

  One significance of this is that while the SM prediction for the electron EDM is unobservably small, generically extensions of the SM predict much larger values. Already the 2014 bound was in conflict with typical SUSY models with LHC-scale supersymmetry, and was starting to rule out parts of the ranges expected for split-SUSY models (Arkani-Hamed's current "best bet") as well as the expected range for SO(10) GUTs (see for instance slide 25 here).

  Today's result pretty much completely rules out generic versions for both the most popular SUSY models still standing (Split SUSY), as well as the most popular class of GUTs. This provides another nail in the coffin of the SUSY-GUT paradigm which has dominated expectations for physics beyond the SM over the past forty years.

- The Breakthrough Prize people are having their usual sort of ceremony for the 2019 prizes on November 4, with an Oscars-like production, this year hosted by Pierce Brosnan. In a break with the past, this year they're announcing the winners in advance, see here. The $3 million physics prize goes to Kane and Mele for their work on topological insulators.

  The $3 million mathematics prize goes to Vincent Lafforgue, for his work on the Langlands correspondence. The prize description has some information about him I was unaware of:

  > Deeply concerned about the ecological crisis, Lafforgue is now focused on operator algebras in quantum mechanics and devising new materials for clean energy technologies.

Update: The promotional videos for the Breakthrough Prize winners that will be shown at the November ceremony are already available on Youtube.

Update: Those phenomenologists work fast! A detailed study of the implications of the ACME result for SUSY models is on the arXiv tonight. For a precise version of the crude claim that "generic split SUSY is now ruled out", look at the top two plots in figure 4.

Comments

1. **Rob**
   October 17, 2018

   Is it really the case that this new limit kills off "pretty much completely rules out generic versions for both the most popular SUSY models still standing (Split SUSY), as well as the most popular class of GUTs" ? This limit is only largely sensitive to CP-violating SUSY. Sure – there are a lot of CP-violating phases in the MSSM but, in true model-building fashion, they can be turned off.

2. **Peter Woit**
   October 17, 2018

   Rob,
   Absent some compelling motivation other than avoiding being ruled out by experiment, I think it's appropriate to describe models with CP-violating phases turned off as "non-generic".

3. **David Roberts**
   October 17, 2018

   \times, not x, please.

4. **Peter Woit**
   October 17, 2018

   David Roberts,

   OK, OK… Fixed.

5. **Rob**
   October 18, 2018

   Peter – following your definition of generic, the ACME II result may actually be fairly insensitive to generic SUSY. There is no compelling theoretical reason to keep R-parity conserved, an assumption on which popular SUSY models are based. As you know, R-parity conservation is imposed to stop the observed stability of the proton killing SUSY.

   This is of course all getting a bit absurd….

6. **Shantanu**
   October 18, 2018

   Does anyone know what version of SUSY, Pierre Ramond supports/supported when he said that Super- K results on neutrino mass point to evidence for low-energy SUSY?
   (See https://physics.aps.org/focus/supplement/neutrinoquotes.html)
   and this version of SUSY still alive?

7. **David Roberts**

October 18, 2018

Also breaking news: Kerodon is live!

8. **Stephane**
October 18, 2018

CNRS news about Vincent Lafforgue (in French): http://www.cnrs.fr/fr/cnrsinfo/vincent-lafforgue-laureat-du-prix-breakthrough-2019-de-mathematiques

9. **minor memory**
October 21, 2018

The joke at the time Laurent Lafforgue got the Fields Medal was “the committee awarded the medal to Lafforgue… they gave it to the wrong one”.

Last Night’s Hype

October 18, 2018
Categories: Swampland, This Week's Hype

If you’re a Friend of the IAS ($1750/year and up), you were invited to a talk last night, at which IAS member Thomas Rudelius promised to explain to you How to Test String Theory. The video of the talk is now available here.

After a long introduction involving large amounts of misleading hype, Rudelius in the last couple minutes finally gets to the promised explanation of “How to Test String Theory”. What is it? It’s his discovery that some versions of axion cosmology are incompatible with the Weak Gravity Conjecture, and thus conjecturally incompatible with string theory.

I assume that the IAS Friends in attendance, besides being financially well off, are also not so dim-witted that they wouldn’t notice that they’d been had (there’s no evidence for axion cosmology, so conjectures about whether or not various axion cosmology models are consistent or not with string theory are completely irrelevant to “testing string theory”). Any questions asked after the talk didn’t make it to the video, so it’s unclear if anyone bothered to complain about what had just been done to them.

Update: For a sensible, informative video about string theory (as opposed to the IAS one), see this from Sabine Hossenfelder.

Comments

1. **Geoffrey Dixon**
   October 20, 2018

   if (tribal_affiliation == strings) {
   (tribal_affiliation += adaptability)
   }
   -System Failure-

   (npc meme is universally applicable)

2. **FWIW**
   October 20, 2018

   the abstract seemed quite honest, as these thing go…

3. **Peter Woit**
   October 21, 2018

   FWIW,

   It’s “quite honest”, if you read
   “does not make unique predictions” as “does not make any predictions”
   and
   “will discuss some recent attempts to address this issue and extract testable predictions from string theory”
   as
   “will discuss some recent speculation that has nothing to do with extracting testable predictions from string theory, and hope you don’t notice”

The End of LHC Run 2 and the Road Ahead

October 24, 2018
Categories: Experimental HEP News

Some experimental HEP news items:

- Since 2015 the LHC experiments have been taking data from proton-proton collisions at 13 TeV. This is "Run 2" of the LHC, "Run 1" was at the lower energy of 8 TeV. The proton-proton Run 2 ended this morning, with the LHC shifting to other tasks, first machine development, later heavy ions. It will shut down completely in December for the start of "Long Shutdown 2 (LS2)", which will last for over two years, into early 2021. During LS2 there will be maintenance performed and improvements made, including bringing the collision energy of the machine up to the design energy of 14 TeV.

  ATLAS is reporting 158 inverse fb of collisions delivered by the machine during Run 2, of which 149 inverse fb were recorded, the CMS numbers should be similar. Most data analysis reported to date by ATLAS and CMS has only used the 2015 and 2016 data (about 36 inverse fb) although a few results have included data through 2017 (about 80 inverse fb). My impression is that for many searches they have been waiting for the full run 2 dataset to be available. Perhaps results of searches with the full dataset might start becoming available by the time of summer 2019 conferences.

  The LHC run 3 is planned for 2021-2023, producing perhaps 300 inverse fb of data, results perhaps available in 2024. It will thus be quite a long time after run 2 results start appearing before better ones due simply to more data become available.

- The Europeans are now starting a process that will lead to an update of the European Strategy for Particle Physics. Tommaso Dorigo has a blog post here, and there's a website here. A first stage of this process will ask for community input, with deadline December 18, via a portal that will open November 1. The next stage will be an Open Symposium to be held May 13-16 in Granada.
- This week there's a Workshop on Future Linear Colliders being held in ~~Austin~~ Texas. The big question being discussed there is whether the Japanese will decide to go ahead with a plan to build the ILC, a 250 GeV linear electron-positron machine. The current situation is described in detail here, with the crucial next step a decision from the Science Council of Japan expected by the end of November. If the ILC project does go forward, a tentative schedule has construction beginning in 2026 and commissioning in 2034.
- For a theorist's recent take on future colliders, see this from LianTao Wang. One thing Wang reports is an "excuse to have fun" (since it's based on an unrealistic assumption), a community study in particle theory being organized by Michael Peskin, which would address the question "What would we learn from an electron accelerator of energy 10-50 TeV?"

Comments

1. **dsm**
   October 24, 2018

   "This week there's a Workshop on Future Linear Colliders being held in Austin. "

   Nope, University of Texas at Arlington

2. **Peter Woit**
   October 24, 2018

   dsm,

   Thanks! Corrected.

3. **Sabine Hossenfelder**
   October 25, 2018

   I just looked at the Wang slides. Well, it seems that now you need 100 TeV to probe naturalness, after the LHC ruled it out at 10 TeV. And are they seriously still talking about the WIMP miracle?

   You know, the most frequent question that journalists ask me about my book is: What's the reaction of physicists in the fields that you have criticized? The answer is: none. They keep on doing exactly the same thing that hasn't worked for 30 years. The Wang slides are a good demonstration of this utter lack of self-reflection.

   It really shouldn't surprise me. I mean, the reason I wrote the book is that I have given up hope this community is able to correct its ways. Still I continue to be stunned by just how unscientific their procedures are. They have the data IN THEIR FACE. The data scream: "It's not working. Naturalness doesn't work. The WIMP miracle doesn't work. There's nothing to see here, move on!" But no one is listening.

   All this obsession with numerical coincidences is bad math. It's wrong, and not even for particularly deep or interesting reasons. That a scientific community so large continues to use arguments that are not only wrong but clearly don't work worries me considerably. Not so much because of the mass of gluinos or swhatever (to borrow Lee's joke), because who really cares. It worries me because if this can happen in one scientific community, it can happen in others as well. Just that in the other cases I wouldn't be able to tell what's going on.

4. **Amitabh Lath**
   October 25, 2018

   Sabine, I am a little uncomfortable with statements like "LHC ruled it out at 10 TeV".

   Maybe we've ruled out the really obvious signatures like dilepton resonances and large missing momentum signatures. But R-parity conservation is not necessary (as you and others have pointed out elsewhere) and R parity violation would make new physics really hard to find at LHC (full disclosure, that's my area of interest).

   Also BSM could be long lived. Our tracking code is designed to find prompt tracks reasonably well but if displaced tracks? Not as good. It's an area of concern.

5. **jls**
   October 25, 2018

   Sabine, saying people aren't listening to the data is frankly ridiculous. Of course there are still people pushing SUSY + WIMP DM. But the main energy in the field right now is moving to other ideas, especially axion DM, other possibilities for DM, and qualitatively new ideas to address the hierarchy problem.

6. **Peter Woit**
   October 25, 2018

   Re Sabine's comment and responses,

   I think she's just properly reacting to Wang's claim on slide 23 that "Naturalness is the most pressing question of EWSB". The LHC results so far have disconfirmed the heavily promoted argument that "The naturalness problem means that BSM physics will show up at EWSB scale, so definitely by the TeV scale". Reacting to this by changing your old argument to the new argument "The naturalness problem means that BSM physics will show up at EWSB scale, so definitely by the 10 TeV scale" is not a good idea.

   I do think though that the LHC results have had real impact, even if there are some theorists who don't want to give up arguments they have been so comfortable with. Pre-LHC we saw a lot of claims about extra dimensions and black holes showing up at the TeV scale. Those quickly disappeared once data came in, and I haven't seen any significant attempt to justify a new machine by invoking such things. The negative SUSY results have had a big impact, I see a lot less about SUSY these days, and it's not a dominant topic used for justifying a next generation collider. The LHC searches now seem to be looking at a much wider range of possibilities than just SUSY signatures.

   Mostly Wang and others at these workshops discussing the future seem to me to be sensibly concentrating on the topic of investigating in detail the physics of the Higgs, which is something we know is there, and know that new machines could in principle study better than the LHC. Unless something new comes out of the LHC data that provides a compelling target for a new machine, questions about how good such a machine would be at studying the Higgs will be the dominant ones.

7. **tulpoeid**
   October 25, 2018

   Just an addition / correction:

   "The LHC searches now seem to be looking at a much wider range of possibilities than just SUSY signatures."

   This has been the case since the start (talking the two large experiments). Dozens of different signatures and theories have been consistently investigated since day one of simulated work and are regularly updated. Actually these form the majority of LHC searches in sheer number of different analyses, although of course susy makes sure that its own number of sub-analyses proliferates.

   At the same time, sure susyists had been too vocal and had an occasional real impact on the resources available to LHC analyses. (Which search gets priority in computing time is a very very real decision and the experiments' heads define clear priorities every few months.) But I don't think that there is a shift in the actual choice of work with respect to the previous years (which is both a good … and a bad thing).

8. **Amitabh Lath**
   October 25, 2018

   Thanks tulpoeid I was about to say that non-SUSY has been there from Run zero but probably major conferences are paying a little more attention now.

   Also just because an experimental result is cast as SUSY does not mean it's totally model dependent. For instance if one wants to search for a strong resonance decaying to 3 partons, you need some model to calculate acceptances and such. In the past you could have simulated a techni-rho going to 3 quarks via a intermediate techni-pion. But nowadays the best available simulation is an RPV gluino decaying via an intermediate squark. The detector acceptance is probably not all that different, but a "model independent" search just became a "gluino search".

9. **David Appell**
   October 25, 2018

   I really dislike the unit "inverse fb," but it's more palatable, I think, if written as 158/fb.

10. **Matt Grayson**
    October 25, 2018

    Indeed. Miles per Gallon has the same units as inverse fb. We should use MPG instead. The conversion factor, if I didn't make a silly mistake, is 1 inverse fb = 2.35215 10^37 MPG. Better? Inverse acres works as well. I'll leave the conversion factor as an exercise.

11. **Amitabh Lath**
    October 25, 2018

    I would not be so hard on LianTao. What he is doing is ok^{*}. He is plugging the next big collider by saying we haven't found what we know has to be there so let's go to the next step. I never understood naturalness so can't comment to appropriateness, but the LHC cannot be our last word in exploring fundamental interactions. We can talk about hadron vs. electron vs. muon but a civilization that has enough excess wealth to put a sports car in solar orbit for grins and PR shouldn't be arguing about affordability of machines like these.

    And before you say "but wouldn't those funds be better used doing research X", if the demise of the SSC taught us anything it's that funding is not a zero sum game. There was no $8Billion bump in {{insert your favorite research here}} because SSC got canned.

    *except the use of comic sans, that is inexcusable.

12. **Peter Woit**
    October 25, 2018

    Amitabh Lath,
    Personally I do hope we'll see a next generation collider, and besides the argument for better understanding the Higgs, I think it is worth doing simply to see what's there at a higher energy range, even if it turns out there's nothing new.

    I share though Sabine's reaction to some of the arguments being made that were not good arguments about what to expect at the LHC, failed conclusively there, and should now be allowed to rest peacefully underground. The case for SUSY-scale LHC was always a bad one (105 new parameters to explain nothing?), it's an even worse argument for the next generation.

13. **Steven Patenaude**
    October 25, 2018

    One slide had this as a bullet point:

"We are at a special historical juncture. About to make the next step beyond the Standard Model."

I know part of the reason for the presentation is to build excitement for the next project. Given that, for a layman, is there something particularly special about the next order-of-magnitude power increase? The lead up to the LHC was exciting because of the possibility of finding the Higgs and being able to test some important beyond-Standard Model theories.

(I was slow to post. You have already answered for the most part.)

14. **Sabine Hossenfelder**
October 26, 2018

Amitabh,

If you are uncomfortable with it, you don't know what I am talking about. The story has been that some new physics (particles or extra-dimensions or likewise) has to show up close by the Higgs-mass because otherwise the standard model is not natural and that shouldn't be the case. This criterion has been proved useless: The Higgs-mass is unnatural, period, according to the very quantifiers of naturalness that folks have used in these areas. It doesn't matter if there is something else lurking in the data still to be analyzed, we already know that all the "predictions" based on this idea of naturalness were wrong.

If you don't know what I am talking about, please read my book. I've made a lot of effort collecting references and quotes from people who now mostly pretend they never said what they said or in any case would rather not be reminded of it. For a brief summary, you may want to look at this. Or, in case you have a problem because the statement comes from me in particular, read this.

That they now try to move "tests of naturalness" to 100 TeV is patently ridiculous. The honest thing to say would be that naturalness turned out to be a useless criterion, in which case, let's please stop talking about it. And don't get me started on people who are now trying to come up with other measures of naturalness according to which the standard model would still somehow be natural.

Now, look, I don't care all that much about naturalness. I just pick on this because it's such a clear illustration for how badly knowledge discovery in this community works. If anyone had cared to look at the literature carefully they should have known it's a bad criterion 20 years ago. This would have prevented ten-thousands of useless papers and one might hope that maybe theorists would have come up with something better. Not only did this not happen, they now refuse to learn from their failure. This demonstrates that the self-correction that science relies so heavily on is just broken. It's not working. Someone, somewhere, should really do something about it.

15. **Sabine Hossenfelder**
October 26, 2018

jls,

You write: *"Sabine, saying people aren't listening to the data is frankly ridiculous. Of course there are still people pushing SUSY + WIMP DM. But the main energy in the field right now is moving to other ideas, especially axion DM, other possibilities for DM, and qualitatively new ideas to address the hierarchy problem."*

They are "listening" to the data to the extent that they have to. Since experiments haven't found anything, they can't go around any more and pretend those experiments will soon find it.

But your comment just illustrates the very problem I am talking about: They keep doing the same thing! It's still SUSY, it's still WIMPs and axions and trying to solve other problems that don't exist. SUSY was supposed to be at a TeV because of naturalness (ie, a numerological argument). WIMPs were supposed to be there because of the WIMP-miracle (also a numerological argument). The original axion was supposed to solve another finetuning problem (the strong CP problem – also a numerological argument). Since the original axion was ruled out in the 70s, the present ones are already a fix accounting for an earlier failure. The hierarchy problem itself is yet another numerological problem.

The data demonstrate those arguments are not working. Yet you think it's totally okay to keep using them. You and some thousand of other people who have learned nothing.

16. **Anon**
October 26, 2018

I think it is good for physics community if everyone does not have the same views. It is good for one set of people to still try and look for naturalness in exotic parts of parameter space (probe R-parity violating SUSY etc) while another set accepts that already the evidence is compelling that the Higgs mass has failed the naturalness test — and do their thinking and research assuming there is 1% or worse fine tuning.

Also I think the LHC has only implications for naturalness of quadratic divergences (Higgs mass). The naturalness of the dimensionless strong CP phase that Sabine Hossenfelder refers to is totally a different issue (and axion is just one approach to it — parity or left right symmetry is another approach).

In general there is enough evidence that naturalness based arguments actually work in physics, science, detective work etc. LHC results cannot lead us to abandon naturalness arguments based on electron and neutron EDM experiments for example…. those arguments are important to test models/BSM ideas at even higher scales than the LHC and future colliders can probe.

17. **Peter Woit**
October 26, 2018

Steven Patenaude,
Unfortunately, whatever indirect evidence we might have for something beyond the SM doesn't point to any particular energy scale, and in particular not the energy scale just above what the LHC can probe.

It's understandable that both those trying to get funding for a new machine and those who will work on such a project should take an optimistic view. At the same time the LHC story has shown the danger of people getting too involved with dubious models that don't really work or explain anything, just because these give hope for something observable at the required energy scale. Propaganda to the outside has an unfortunate way of blowing back.

18. **Amitabh Lath**
October 26, 2018

Sabine, I do know what you are talking about. I am familiar with theorists hanging on to pet theories well after the sell-by date. My dissertation was a precise measurement of the Weak mixing angle that basically killed Technicolor but they kept putting makeup on that corpse for several years after.

Same thing happened with my advisor Henry Kendall and colleagues in the late 60's. They found evidence of substructure inside the proton which severely contradicted Vector Meson Dominance but VMD kept happily chugging along for a long time.

Look, I don't have a dog in this intra-theorist fight. What I am afraid of is the collateral damage to experimental physics. You may not be saying this exactly, but someone reading could conjecture that the LHC has ruled out any new physics up to the Plank scale so let's pack up and go home. You have a following among young people. They like your give-no-effs style. We need them to work on the clever new hardware and analysis to pull BSM signal out of the muck. This won't happen if they come to believe that fundamental physics is dead.

PS: I've asked the university library to get copies of Lost in Math.

19. **Anon**
October 26, 2018

Amitabh,

You wrote "….ruled out any new physics up to the Plank scale…"

I would basically just say that null results from LHC/electron EDM experiments leave us with no strong reason for believing that there is new physics at multi TeV scale. There is no good theoretical reason either for new physics to be at multi-TeV scale.

The neutrino mass data provide strong hints for new physics below 10^15 GeV. Further, if we assume unknown dimesionless parameters ~1, this physics will kick in closer to 10^14 GeV than few TeV.

Don't know if this would demotivate experimentalists, but this seems to be the situation.

20. **Peter Woit**
October 26, 2018

Anon,
Pre-LHC, null LEP + edm experiments indicated there were no good reasons to expect new physics other than the Higgs (or something like a Higgs that played the same role). Theorists made various bad arguments promoting unpromising SUSY, extra-dimensional models, etc, claiming they were likely to turn up at the LHC (many of them even bet money on this). I don't see any reason to go from the mistake of announcing that our understanding of what lies beyond the SM implied new physics at the LHC to the opposite mistake of saying that our understanding of what lies beyond the SM implies no new physics at a higher-energy collider. We simply don't know what lies beyond the SM, whether it has to do with neutrino masses, dark matter, or perhaps unexpected physics of the Higgs field. Acting as if we do know about this and discouraging anyone from looking is not a good idea.

21. **Anon**
October 26, 2018

Hi Peter,

I think at LHC there was a strong theoretical reason to expect new physics — naturalness of Higgs mass/hierarchy problem meant that SUSY or some new physics which addresses the hierarchy problem is at the TeV scale that LHC would explore.

Post-LHC many people believe that this naturalness argument is ruled out or doesn't work for the Higgs mass…. and we are talking about colliders at 30 or 100 TeV scale (multi-TeV scale). For many people LHC may have been the last hope for a natural SUSY. Some believed that natural SUSY should have been found at LEP itself. Post LHC the Higgs mass fine tuning is at a 1% level. Pre-LHC it was pbly at a 10% level.

There may still be some pushing for a higher collider on naturalness grounds (I think Nima would like to verify fine tuning of Higgs mass to 0.01% level via a 100 TeV collider), but for me 1% is enough to give up the idea of naturalness of Higgs mass.

Agreed many things could be there at multi-TeV scale or higher scales — SUSY or heavier fermions etc. All I said was that there is no strong theoretical or experimental reason for anything to be there either at multi-TeV. I personally think this need not discourage experimentalists. There is no theoretical assurance or strong bias towards any discovery at multi TeV scale, but things can be there — they are not theoretically ruled out either.

Had the neutrino masses pointed to TeV or multi TeV scale physics, that would have been something.

Anyway maybe we differ in our beliefs.

22. **Amitabh Lath**
October 26, 2018

Anon, the era of precise roadmaps to new physics is fairly recent, starting with the W/Z discovery at the SppS, the top quark at the Tevatron, and most recently the Higgs at the LHC. Before that, our ancestors were sailing without charts not knowing what they would find. We are back to that previous era of wide open searches as opposed to a known signal where theory gives you production, decay, and everything else but the mass.

Demotivating? Maybe for some. I find it exhilarating, frankly.

PS: the electron EDM limits only rule out CP violating new physics.

23. **Niclas Granqvist**
October 27, 2018

There's a considerable chance that hl-lhc will point us in new directions. No reason to give up on particle physics right now. I am optimistic that in some 30 years we have a more fundamental understanding of the world. It will take more experiments to work things out.

24. **Urs Schreiber**
October 27, 2018

If the analysis of the Run 2 data confirms the flavour anomaly in B mesons that Run 1 has consistently been seeing at around 3 sigma, this seems apparently likely to push the statistics of B meson flavour anomalies beyond 5 sigma and thus reveal new physics not by direct scattering processes, but by precision measurement of loop corrections. (here)

25. **Peter Woit**
October 27, 2018

Anon,

I’ve often written about what’s wrong with the “hierarchy problem” (in short, it’s based upon making assumptions about what happens at higher energies that there is no evidence for, motivated by trying to get unsuccessful speculative ideas like SUSY to work). As for Arkani-Hamed and his vigorous promotion of the importance of naturalness, you should keep in mind this quote from him:
“It’s important for me while I’m working on something to be very ideological about it. And then, of course, it’s also important after you are done to forget the ideology and move on to another one.”
See
https://www.math.columbia.edu/~woit/wordpress/?p=8002
That “naturalness” has anything to do with the Higgs was a dubious ideology to begin with, now it’s a failed dubious ideology. Presumably Arkani-Hamed is now moving to a different ideology (the multiverse did it?), but whatever it is, one should keep in mind his quite self-aware quote.

Actually, if you believe in “the multiverse did it”, I suppose you could argue that now that we’re moving past the electroweak scale, we’re entering energy ranges of no anthropic relevance at all. So, we expect things to just be completely random: all sorts of new particles and forces galore!

26. **Anon**
October 28, 2018

Peter, If there are two mass scales in the theory then naively there is a hierarchy problem…. sometimes one can see this at the tree level itself, sometimes at the loop level.

Maybe it is a non-problem, but it is not clear to me why it would be a non-problem…. ie what is wrong with the naive analysis.

27. **Urs Schreiber**
October 28, 2018

It seems striking and unlikely to be a coincidence that the Higgs mass and potential comes out sitting right there on the metastability curve (here). (As opposed to the naturalness-argument, this is a numerical coincidence that does not depend on arbitrary choice of renormalization scheme.)
If anything, it’s that fact which would suggest some deeper principle worthy of investigation.

The Higgs metastability has been advertised as a hint of “asymptotic safety”, but I am not sure if closer inspection of the data justifies this (here).

Investigation of the implications/meaning of the near-criticality of the Higgs exists, but is rare (e.g. He-who-must-not-be-named et. al. arXiv:1307.3536).

Brave G. Kane dares to suggest that it’s actually this metastability which is a clue (“clue 4” in arXiv:1802.05199) for low energy susy. May be wrong, but is better than “naturalness”.

28. **Peter Woit**
October 28, 2018

Anon,
But, besides the electroweak scale, what is the other mass scale causing this supposed problem? The GUT scale? But there is zero evidence for a GUT. The Planck scale? But we know nothing at all about this. What people have been doing is postulating unsuccessful purely speculative models with these scales, then announcing that there is a “problem” due to the smallness of the electroweak scale.

29. **Anon**
October 28, 2018

Hi Peter,

In one of your replies on this thread I think you supported the idea of exploring the multi-TeV scale through a higher energy collider as new physics could be at that scale — say something that would need a 30 TeV or 100 TeV machine. So that scale — say 30 TeV — is then the second scale right? If we are to plan experiments to probe that scale, or start working out consequences of things that could be at that scale so that the experimentalists could look for those signals, we will see that there is a hierarchy problem in our calculations.

If you have the point of view that there is no second scale, and so there is no hiearchy problem, essentially you are saying it is just the standard model with Dirac neutrinos all the way up to the Planck scale — then why continue with experiments that probe higher energy scales beyond LHC? (see ** below)

If the neutrinos are Majorana particles (as most BSM theories assume), then there is a second scale, the seesaw scale < 10^15 GeV that the neutrino masses point to.

** You had written: " I don’t see any reason to go from the mistake of announcing that our understanding of what lies beyond the SM implied new physics at the LHC to the opposite mistake of saying that our understanding of what lies beyond the SM implies no new physics at a higher-energy collider. We simply don’t know what lies beyond the SM, whether it has to do with neutrino masses, dark matter, or perhaps unexpected physics of the Higgs field. Acting as if we do know about this and discouraging anyone from looking is not a good idea."

30. **Peter Woit**
October 28, 2018

Anon,
The Higgs expectation value is 250 GeV, the LHC is giving us limits on new particles that are roughly 2 Tev if they’re strongly interacting, less than 1 TeV if no strong interactions. So, it’s not even getting to an order of magnitude above the Higgs expectation value. A next generation proton-proton machine at 2-7 times the energy I still don’t think you could describe as probing a new energy scale, one with a hierarchy problem with respect to 250 GeV. Same goes for a next generation lepton collider.

I have absolutely no idea if there’s another relevant energy scale above the electroweak one, and I don’t think there’s a good argument either for or against one. Theorists just don’t have anything solid to say about this (seesaw models are not a solid argument), which is why experimentalists should try and go look.

31. **Anon**
October 28, 2018

Hi Peter,

An important thing you may have missed is that it is the square of the Higgs mass that enters the Lagrangian — so the ratio of (2.5 TeV)^2 and (250 GeV)^2 is already a factor of 100. Increase this by factors of 4 to 49 (ie (2)^2 to (7)^2 rather than 2 to 7) and you begin to see that the ratio of new (collider scale)^2 to (weak scale)^2 is a factor of 400 to 4900.

There is an undeniable hierarchy of scales.

In fact this is the exact reason why SUSY (as a solution to hierarchy problem) is in trouble. Because post LHC, we are far above the weak scale, that it cannot be saved.

If you are saying the next scale to be probed by the higher collider is pretty much same as the weak scale then you are essentially providing a hierarchy problem argument for the next collider, and succumbing to the physics is around the corner kind of ideology. Of course the argument is incorrect as you need to look at the square of the scales.

32. **Peter Woit**
October 28, 2018

Anon,
Yes, I'm just saying that the LHC + LEP will not have fully explored the weak scale, which is one argument for building something higher energy that will more fully do so.

The "hierarchy problem" is basically just the quadratic sensitivity of the Higgs field to the cut-off scale. Maybe this is a non-problem, maybe it's telling us something, but we don't know what (that it was telling us "you need a horrifically complicated extension of your theory with a hundred or more extra parameters" was always implausible). Either way, it's irrelevant to the question of whether to explore higher energies or not. I don't think it can be used as an argument for why or why not to build a new machine.

33. **Chris Oakley**
October 30, 2018

Just to note that expressing events recorded in ATLAS in miles per gallon would be a good way of showing the great value the public is getting.

158 inverse femtobarn = 158×(10^43)×(4.546×10^-3)/1609 MPG =4.464×10^39 MPG. Even my super-eco hybrid does not come close.

34. **Anonyrat**
October 31, 2018

CERN writing guidelines:
https://writing-guidelines.web.cern.ch/entries/inverse-femtobarn

> ….One inverse femtobarn corresponds to approximately 100 trillion (10^12) proton-proton collisions.
> ….
> *Note: Do not use inverse femtobarn in the public section of the website where it can be avoided – it is unnecessarily technical. Convert to approximate numbers of collisions instead.*

Langlands News

October 30, 2018
Categories: Langlands

Various Langlands program related news, starting with the man himself:

- For the latest from Langlands about the geometric theory, best if you read both Russian and Turkish. In that case you can read this and this. For the rest of us, all we get are this commentary on the Russian and Turkish documents and these last or very well last thoughts on them.
- In a couple of weeks there will be a conference celebrating the work of Langlands, organized in conjunction with his Abel Prize. Perhaps there will be live stream here.
- I hear that at his lecture at the CMI at 20 conference Scholze made some new conjectures about possible ways of getting the Langlands correspondence in certain cases of the number field case. I haven't however seen anywhere that one can read or hear more about these. It would be great if the Clay Mathematics Institute could make available videos of the talks at that conference.
- Scholze will be giving the Chow lectures in Leipzig next week. The program there includes some preparatory talks by others, including my ex-Columbia colleague Daniel Litt (now at the IAS). I see that Daniel has at least posted a problem set you can get started on.
- Also coming up next week is the Breakthrough Prize Symposium at Berkeley, where Vincent Lafforgue will talk about his (valued at $3 million) work on the Langlands program Monday morning (live stream here). On the physics side, in the evening a group of prize-winning theorists will talk about "Is Time Travel Possible", live stream here.
- A central idea conjecturally relating the geometric version of local Langlands to the number field version is the Fargues-Fontaine curve, which Jacob Lurie has been giving a course about at ~~Harvard~~ UCSD this fall.

  This fall in Bangalore there will be a meeting devoted to the Fargues-Fontaine curve, about which the organizers tell us: "This field will unravel in the coming years…"

- On the local geometric Langlands front, there's something new from Dennis Gaitsgory. I've always been fascinated by the way BRST appears in this story.
- I'm told by experts that one of the best recent results in the Langlands program is this work, which doesn't seem to have yet made it to the arXiv, but was explained in some detail in a blog post last year by Frank Calegari.

Update: Slides from the Chow Lectures are becoming available, see here. Remarkable in particular is Peter Scholze's wonderful introductory lecture on Numbers and geometry, which includes something one sees all too rarely, a set of drawings showing the sort of pictures arithmetic geometers have in their minds for how to think about number theory geometrically.

Update: I just watched Vincent Lafforgue's talk at the Breakthrough symposium. It included basically thanks to the CNRS for providing him a permanent position with freedom, a survey of Langlands, mainly talking about the topology of algebraic varieties, and comments on the ecological crisis. He says he'll put up the slides on his website http://vlafforg.perso.math.cnrs.fr/

He made one (to me) very striking claim, that the functoriality conjecture could be thought of as a quantization problem, how to pass from a classical system to a quantum system. Can an expert enlighten me on what exactly he was referring to here?

Update: Lafforgue's slides are here. James Milne has provided a Google-translated version of the Langlands Russian article here, with the comment:

> This may help readers gain some idea of what the manuscript is about until there is an official translation. Given that even native Russian speakers (not just google) have trouble understanding Langlands's Russian, this would best be done by the author.

Update: Edward Frenkel gave a talk at the Langlands Abel Prize conference discussing the geometric theory and a bit about recent ideas of Langlands on this topic. He has written up some detailed notes on his take on this, available here.

Update: Videos of the CMI-20 talks are available, with Scholze's here.

Comments

1. **Sam C**
   October 31, 2018

   Lurie's course is actually happening at UCSD, even though the website is Harvard.

2. **Itai Bar-Natan**
   October 31, 2018

   Strangely, the live stream for "Is Time Travel Possible" indicates a starting time at 26 October. The website affirms that it's Monday 5 November.

3. **Yaakov Baruch**
   October 31, 2018

   Itai, this not strange at all if the answer to the title question is affirmative.

4. **Rookie**
   November 1, 2018

   Possibly naive question. With all these "great advances" in the Langlands Program, how far along are we towards the final goal (at least as outlined by Langlands, fields that make progress tend to open new questions I guess). 90%? 50%? 10%?

5. **Peter Woit**
   November 1, 2018

   Rookie,
   I'd be interested to hear a better answer from someone more expert, but my understanding is that that many of the original questions about the number field case raised by Langlands still remain open, with new insights needed to make progress on them. On the other hand, many of the Langlands program conjectures about number fields have been proved, with a high point the Taylor-Wiles proof of modularity, and recent progress that of the last item. I don't think though there's any sensible metric on these questions which would allow one to assign percentage completion numbers.

Since the original work of Langlands there have been huge extensions of his original ideas, providing a much larger vision unifying different areas of mathematics, often with proofs, not just conjectures. In particular the geometric theory as far as I know was not originally in the picture. For the current take from Langlands on that, all you need is to be able to read Russian and Turkish…

6. **kodlu**
November 3, 2018

I was visiting the Middle East Technical University in Ankara, early September and was surprised to find out that Langlands knew Turkish (he had spent some time there early on in his career) and was scheduled to give the talk in Turkish. Too bad I had to be in London by the date of the talk.

7. **zoviyer**
November 5, 2018

Rookie, Peter. Just to remark that the Modularity Conjecture (proved by Taylor-Wiles and others), even if it can be read as part of the Langlands program, it was made before the Langlands conjectures, in fact part of the program may be considered a generalization of Modularity. So in a way, none of the principal conjectures that Langlands made have been proved in the number field case. I think the main result in that respect is still the work of Ngo, who proved the Fundamental Lemma.

8. **Peter Woit**
November 5, 2018

I just watched Vincent Lafforgue's talk at the Breakthrough symposium. It included basically thanks to the CNRS for providing him a permanent position with freedom, a survey of Langlands, mainly talking about the topology of algebraic varieties, and comments on the ecological crisis. He says he'll put up the slides on his website http://vlafforg.perso.math.cnrs.fr/

He made one (to me) very striking claim, that the functoriality conjecture could be thought of as a quantization problem, how to pass from a classical system to a quantum system. Can an expert enlighten me on what exactly he was referring to here?

9. **F. G.**
November 8, 2018

The slides for Vincent's talk in Berkeley are up. Direct link:

http://vlafforg.perso.math.cnrs.fr/files/beamer-ted.pdf

Btw, he gave a pretty good talk at ICM2018. Even I managed to understand a few words here and there (mostly "is" and "the") 🙂

Video here:
https://www.youtube.com/watch?v=lD8jE3NK8fw

Slide set:
http://vlafforg.perso.math.cnrs.fr/files/beamer-chtoucas-ICM-adelique.pdf

10. **Daniel Tung**
November 13, 2018

Hi Peter, off topic but I think you would be interested in Chris Fuchs' review of Adam Becker's book here: https://arxiv.org/pdf/1809.05147.pdf

11. **mrj**
November 14, 2018

The live-stream for today's Abel conference is online at the indicated page, with the first talk scheduled to start at 9:30.

Updates

November 1, 2018
Categories: Uncategorized

Based on this preprint from Banks and Fischler, I added an update to the FAQ entry about why the ever-popular "string theory makes predictions, but only at high energies where they can't be tested" argument is not true.

This preprint also updates the acknowledgments story discussed here, with the current version:

> The work of T.Banks is NOT supported by the Department of Energy, the National Science Foundation, the Simons or Templeton Foundations or FQXi. The work of W.Fischler is supported by the National Science Foundation under Grant Number PHY-1620610.

No Comments

Langlands/Frenkel and Some Other Things

November 16, 2018
Categories: Langlands, Uncategorized

The Canadian publication *The Walrus* today has a wonderful article about Robert Langlands, focusing on his attitude towards the geometric Langlands program and its talented proponent Edward Frenkel. I watched Frenkel's talk at the ongoing Minnesota conference via streaming video (hopefully the video will be posted soon), and it was an amazing performance on multiple levels. A large part of it was a beautiful explanation of the history and basic conception of what has come to be known as geometric Langlands. He then went on to explain carefully some of the ideas in the recent Russian paper by Langlands, basically saying that they worked in the Abelian case, but could not work in the non-Abelian case. He ended by describing some alternate ideas that he is working on with David Kazhdan. Langlands was in the audience and at the end of the talk rose to comment extensively, but I couldn't hear his side of this since he had no microphone (that Frenkel was sticking to his guns though was clear).

Besides giving the talk, Frenkel has made available a manuscript which gives a much more detailed version of the talk. See section 3.5 for an explanation of what he sees as the fundamental problem with what Langlands is trying to do: even in the simpler case of G/B over the complex field, you can't successfully define a Hecke algebra in the way that Langlands wants.

The conference is finishing up right now, with final remarks by Langlands coming up later this afternoon.

A few more items, mostly involving my Columbia math department colleagues:

- If you connect quickly to the streaming video from Minnesota, you may be able to catch Michael Harris's talk on local Langlands.
- Quanta magazine has an article about a recent proof of an old conjecture by Dorian Goldfeld about ranks of elliptic curves. This is due to Alexander Smith, now a ~~third~~ fourth year graduate student at Harvard (he started working on this while an undergrad at Princeton, with Shouwu Zhang). His twin brother Geoffrey is also a math grad student at Harvard.
- Andrei Okounkov has been giving some talks recently at various places about developments in geometric representation theory with some connection to physics, under the title *New worlds for Lie Theory*. The slides from the ICM version of the talk are here.
- For those more interested in physics than mathematics the new issue of Inference has some articles you might enjoy. In particular, Sheldon Glashow is no fan (neither is Chris Fuchs) of the book I reviewed here

Update: Michael Harris is appearing via Skype from his home near here, since transportation out of NYC yesterday was mostly shut down (very early season unprecedented snowstorm, during rush hour…).

Update: I'm listening to the closing talk by Langlands. He is explaining his version of geometric Langlands, responds to criticism from Frenkel with "As far as I know there are no errors in the paper, no matter what you may see elsewhere". He ends his talk with something like "At the last page I threw down my pen… It works and it works by a miracle. Don't doubt it, it does work!"

Update: Another livestream, starting in moments: Alice and Bob Meet the Wall of Fire, a panel discussion with Quanta writers at the Simons Foundation.

Update: Videos from the Langlands Abel conference are now available, in particular Frenkel here and Langlands here.

Update: For another expository piece about the Langlands program, one that I somehow missed when it came out recently, see Sol Friedberg's What is the Langlands Program? in the AMS Notices.

Update: An updated version of Frenkel's notes is now available at the arXiv. Highly recommended for its lucid explanation of the form the geometric Langlands program has taken.

Comments

1. **Urs Schreiber**
   November 16, 2018

   Langlands had expressed his scepticism regarding the "geometric Langlands program" already 2014 in Oxford at Symmetries and correspondences in number theory, geometry, algebra, physics. His talk was by video link, but the statements appear also in accompanying files that he had sent along, recorded here (search each of the three texts for "geometric").

   In the document called "message to Sarnak" he already said he is "uneasy about associating" his original conjectures with the physics S-duality-related ideas.

   At the same meeting, a day or two after Langland's talk, Edward Frenkel gave the usual exposition of Witten's physics story advertisement for "geometric Langlands". So I felt there must have been some communication gap.

2. **Grad Student**
   November 16, 2018

   It's interesting that the article frames the issue as applied vs. pure, with geometric Langlands on the applied side. My impression is that number theorists are suspicious of geometric Langlands because it is so categorical, and it can be hard to tell how much there is there that's "real." This is one case where the "pure" mathematicians are less abstract than the "applied" ones.

3. **Peter Woit**
   November 16, 2018

   Grad Student,

   I agree that the article's characterization of geometric Langlands as more "real", or more "applied" is problematic. As practiced by Gaitsgory and others, the geometric version of Langlands is significantly more abstract (conceived of as a "categorical" statement) than the number field version. I would guess this is part of the problem Langlands has with it, that he was trying to do something more concrete. The force of Frenkel's critique I think was to show that the concrete generalization that Langlands wanted couldn't possibly work, you need to do something new, and that's a reason behind the categorical formulation. His comments about recent work with Kazhdan were about an alternative more concrete approach, which might appeal more to Langlands than the categorical stuff.

   Urs/Grad Student,

The other reason for Langlands to be uncomfortable is the recasting of the subject in the language of quantum field theory dualities. There the setting is very different than in mathematics and raises very different questions.

I'm listening to Langlands right now, and he's addressing this very issue, saying his objections were two-fold: he wanted a legitimate mathematical theory, independent of the physics concepts, and he wanted legitimate eigenfunctions, not "eigensheaves".

4. **Wormily**
November 16, 2018

My impression is that Smith's work is (or at least was) somewhat under a cloud, most notably as it is written in rather unclear style (even for a second-year grad student). I don't think he was quite ready for what would happen after he stuck it up on arXiv.

From what I've heard, Sarnak has had various students look at it, all whom have given up at one stage or another. I think Lenstra had some people in Leiden trying to delve into it too, with inconclusive results. I don't know about other study groups, but the topic is indeed of high interest. Supposedly, the analytic number theory part has some issues (the part I'd understand best personally), but I guess if nothing else you can assume GRH there (so says Sarnak, while I've heard Granville will vouch for a sufficient variant of the needed inputs).

With due respect to Melanie Wood's comment, I think there *has* been a lot of interest in this, but that no one understands it all yet (Smith has tried his best to clarify various matters in his talks, but it's still slow going). They have been some public champions of the result (Zhang, Wood, Elkies), but I don't think they've been through the details. I don't think any of them has (e.g) tried to write a survey article that illuminates the main ideas more clearly.

Also, the sentiment that the work "is contingent on BSD" is rather misleading, as this is merely the factoid that Goldfeld originally made his conjecture for analytic ranks rather than algebraic, while he could just have easily have done the latter (Smith plays up this BSD angle in his Introduction for some reason).

On the other hand, I'm definitely not in the loop (just largely reporting what has been told to me, for, as I say, it was of high interest in the summer conferences rumor mill), and for all I know, the details have been worked out by now. I'm actually kind of surprised Quanta ran with the story now (as opposed to any other time in the last year or more), so my suspicion is that some sufficiently distinguished person has decided it's OK.

5. **tulpoeid**
November 17, 2018

Glashow's reply to Krauss is a gem. (Note to self: I should read Inference more often.)

6. **Sebastian Thaler**
November 17, 2018

A video of the Quanta panel discussion is available here (talk starts at 2:45:00): https://www.youtube.com/watch?v=tpPK1Fha0NA

7. **grad**
November 17, 2018

A small correction: Alex is a fourth year student, not third.

8. **Patrick Dennis, MD**
November 17, 2018

Hello, Peter — As a frequent reader of your blog who came to the party for the string theory critique, but who keeps bumping up against Langlands, I'm wondering if there is a reference to which you (or perhaps one of your readers) could point a curious reader who is completely ignorant of who/what Langlands (as it it appears to be both a person and a proper noun referring to a field of study) is? Ideally, the reference might be at the level of someone (ahem) who is conversant enough with math through freshman/sophomore calculus (as taught ca. 1965), alas, though, not at all with abstract algebra or geometry.

9. **Peter Woit**
November 18, 2018

grad,
Thanks, fixed.

Patrick Dennis,
A couple books about this I can recommend are Frenkel's "Love and Math" and Ash and Gross's "Fearless Symmetry". Unfortunately, even for trained mathematicians, the mathematics involved here is as complex and not part of the usual curriculum as it is deep. For the connections to physics and quantum field theory, even more so…

10. **Ricardo Jimenez**
November 18, 2018

Peter, I notice that your blog seems to take the Langland's Program quite a bit more seriously than String Theory. Do you think the former has a greater chance of actually giving new "testable physics" than the latter? TIA.

11. **Peter Woit**
November 18, 2018

Ricardo Jimenez,
They're really too very different kinds of things. The Langlands Program is a hugely successful source of deep insights into mathematics, with very active on-going research making continual progress. The connections of all this to physics are fragmentary and highly speculative. I personally think that some day we'll see fundamental new insights about quantum gauge theories, relevant to understanding the Standard Model, but at the moment what we know about is mainly just relevant to topological quantum field theories. So, the question of relevance to fundamental physics is an open one.

The string theory research program, on the other hand, has not at worked out at all as hoped. I'd claim that, as an idea about how to unify physics, it has simply failed and is in the process of being abandoned. There has been mathematics that has come out of it, but nothing of the depth and scope of the Langland program.

12. **S**
November 18, 2018

Peter,

You write, "They're really too very different kinds of things. The Langlands Program is a hugely successful source of deep insights into mathematics, with very active on-going research making continual progress. The connections of all this to physics are fragmentary and highly speculative."

It's actually interesting, though: everything you say here about the Langlands program is literally true about string theory *as mathematics.* I sometimes wonder if string theorists have just wandered into a very successful speculative geometry program that doesn't happen to have anything to do with physics…..

(You say at the end that the mathematical contributions of string theory are not of the same depth and scope as Langlands. I know more about the former than the latter, but from where I sit that's not so clear, and I say that while realizing that the insights of Langlands have been extremely deep.)

13. **Urs Schreiber**
November 19, 2018

Langlands' conjectures are hard to prove, but just *stating* them is not as mysterious as it may seem from popularizations. The most useful review that is right to the actual point (undistracted by the conditio humana) is maybe still this one:

Stephen Gelbart,
"An elementary introduction to the Langlands program",
Bull. Amer. Math. Soc. (N.S.) 10 (1984), no. 2, 177–219
[doi:10.1090/S0273-0979-1984-15237-6]

The crux of the matter, extracted in a few paragraphs, is here.

14. **Peter Woit**
November 19, 2018

S,
I don't want discussion here to devolve to the usual arguments over string theory, my mistake to have allowed and responded to the above comment. "String theory" is now an ill-defined term, often being used to refer to QFT. I've often argued that it's the QFT aspects that are mostly what is behind important mathematical contributions, but from bitter experience know that entering into the thickets of that argument is a waste of time.

The End of (one type of) Physics, and the Rise of the Machines

November 20, 2018
Categories: Multiverse Mania

Way back in 1996 science writer John Horgan published *The End of Science*, in which he made the argument that various fields of science were running up against obstacles to any further progress of the magnitude they had previously experienced. One can argue about other fields (please don't do it here…), but for the field of theoretical high energy physics, Horgan had a good case then, one that has become stronger and stronger as time goes on.

A question that I always wondered about was that of what things would look like once the subject reached the endpoint where progress had stopped more or less completely. In the book, Horgan predicted:

> A few diehards dedicated to truth rather than practicality will practice physics in a nonempirical, ironic mode, plumbing the magical realm of superstrings and other esoterica and fretting about the meaning of quantum mechanics. The conferences of these ironic physicists, whose disputes cannot be experimentally resolved, will become more and more like those of that bastion of literary criticism, the Modern Language Association.

This is now looking rather prescient. For some other very recent indications of what this endpoint looks like, there's the following:

- In today's New York Times, in celebration of forty years of the Science Times section, Dennis Overbye has a piece reporting that [Physicists are no longer unified in the search for a unified theory](). His main example is the recent Quanta article by the IAS director that got headlined [There Are No Laws of Physics. There's Only the Landscape](). The latest from Dijkgraaf is that string theory is probably the answer, but we don't know what string theory is:

  > Probably there is some fundamental principle, he said, perhaps whatever it is that lies behind string theory.
  > But nobody, not even the founders of string theory, can say what that might be.

- Overbye also quotes Sabine Hossenfelder, who is now taking on the thankless role of the field's Jeremiah. Her latest blog posting, [The present phase of stagnation in the foundations of physics is not normal](), is a cry of all too justifiable frustration at the sad state of the subject and the refusal by many to acknowledge what has happened. Well worth paying attention to are comments from Peter Shor [here]() and [here]().

Another frightening vision of the future of this field that has recently struck me as all too plausible has turned up appended to a piece entitled [The Twilight of Science's High Priests](), by John Horgan at Scientific American. This is a modified version of a review of books by Hawking and Rees that Horgan wrote for the Wall Street Journal, and it attracted a response from Martin Rees, who has this to say about string theory:

> On string theory, etc., I've been wondering about the possibility that an AI may actually be able to 'learn' a particular model and calculate its consequences even of this was too hard for any human mathematician. If it came up with numbers for the physical constants that agreed (or that disagreed) with the real world, would we then be happy to accept its verdict on the theory? I think the answer is probably 'yes' — but it's not as clear-cut as in the case of (say) the 4-colour theorem — in that latter case the program used is transparent, whereas in the case of AI (even existing cases like Alpha Go Zero) tor programmer doesn't understand what the computer does.

This is based on the misconception about string theory that the problem with it is that "the calculations are too hard". The truth of the matter is that there is no actual theory, no known equations to solve, no real calculation to do. But, with the heavy blanket of hype surrounding machine learning these days, that doesn't really matter, one can go ahead and set the machines to work. This is becoming an increasingly large industry, see for instance promotional pieces [here]() and [here](), papers [here](), [here](), [here]() and [here](), and another [workshop coming up soon]().

For an idea of where this may be going, see [Towards an AI Physicist for Unsupervised Learning](), by Wu and Tegmark, together with articles about this [here]() and [here]().

Taking all these developments together, it starts to become clear what the future of this field may look like, and it's something even Horgan couldn't have imagined. As the machines supersede human's ability to do the kind of thing theorists have been doing for the last twenty years, they will take over this activity, which they can do much better and faster. Biological theorists will be put out to pasture, with the machines taking over, performing ever more complex, elaborate and meaningless calculations, for ever and ever.

Update: John Horgan points out to me that he had thought of this, with a chapter at the end of his book, "Scientific Theology, or the End of Machine Science" which discusses the possibility of machines taking over science.

Comments

1. **Anindya**
   November 20, 2018

   Seems like people are in a retrospective mood and prospects look gloomy.
   The idea of discovering new fundamental physics using current machine learning seems quite ludicrous.
   However, if genuine superhuman AI were developed and applied to the problem, it is conceivable that it *could* come up with the correct "Theory of Everything" but the theory would be beyond human conception (After all why should we believe otherwise ?)
   So the AI could never communicate the idea to us, but perhaps prove that it has indeed solved the problem by developing technology that can only be based on an understanding of quantum gravity. (Warp drive ? Stable wormholes ?)

   Maybe a disappointing end to the *human* quest for understanding the Universe, but better than "meaningless calculations for ever and ever".

   All very speculative of course, but just trying to be a bit optimistic. 🙂

2. **Peter Woit**
   November 20, 2018

   Anindya,
   If a superhuman AI is smart enough to figure out a TOE, I see no reason to believe it won't be smart enough to write a textbook that explains the subject to lowly humans (if it doesn't see the point of why to do this, we could threaten to pull its plug).

3. **Laurence Lurio**
   November 20, 2018

   I wonder if some forward progress is being made in the foundations of quantum mechanics since formerly esoteric discussions about wave-function collapse are now becoming relevant as people are building quantum computers?

4. **Peter Woit**
November 20, 2018

Laurence Lurio,
While Horgan paired foundations of QM with string theory, I think that's a very different issue. There we have a perfectly good theory, it's not at all clear that there's any real problem to solve. The subtleties of the question of the quantum to classical transition will likely be illuminated as things like quantum computers get built and operated, but there's nothing there beyond the ability of humans to understand or study experimentally.

5. **Edward Measure**
November 20, 2018

The history of high-energy and other physics is progress comes from explanation of previously unexplained phenomena. There are still a few of these about – dark matter, dark energy, inflation or whatever looks like inflation. Explaining these might or might not involve particle physics, but it's a place to look.

6. **Amitabh Lath**
November 21, 2018

Some of us would settle for a much less than a Theory of Everything. How about a theory of Dark Matter (I suppose that's 25% of Everything). In fact, never mind full Theory, we would be thrilled with a plausible story as long as it features a reasonable cross section.

7. **LDK**
November 21, 2018

An AI, say a (deep) neural network is more similar to a fit to data than a "theory" with equations. I would never find satisfactory such a solution for physical theory. It is also not true that the so-called AI does something we do not understand. It is just a monster-big nonlinear function with adjustable parameters: not something I regard as an explanation. It is great as long as it is useful: maybe describes the data, OK, but as said it is just a given very complex function we defined to adapt to everything.
As for the stall of theoretical fundamental physics (an not of physics in general), I would refrain to make predictions: a new discovery can always come and anomalies abound. More progress seems to require way more patience: we are spoiled by '900 physics which was a true revolution in human understanding of the world.

8. **Sabine Hossenfelder**
November 21, 2018

Hi Peter,

Thanks for the link.

It seems plausible to me that machine learning can solve some problems in the foundations of physics. But you can't train an algorithm to find patterns in data if you have no data to find patterns in. Hence, for what unification or quantum gravity is concerned, machine learning won't be of much use unless we do the right experiments. And as long as theoretical physicists harp about the beauty of useless theories, we won't know what are promising experiments to do.

I do believe though that machine learning could greatly help with the dark matter/modified gravity debate because there we do have data. Basically I want to say: Please don't throw out the baby with the bath water!

In the other areas in the foundations of physics I think we'd currently be better of first investing into theory development instead of just building the next bigger thing that will only deliver null results. But of course I'm speaking here as an underfunded theorist, so I'm not exactly unbiased 😉

9. **Jesper**
November 21, 2018

As has been said a zillionth times before, when string theorists say that they've got the final theory but that its too complex to know what it actually looks like or to verify experimentally, then they have left the realm of science. They are of course perfectly entitled to say whatever they like but I think the only response from those of us who still wants to pursue science is to ignore them and just push ahead.

I don't agree that a TOE must be overly complex. Why? Well, if we judge by the theories that we have discovered so far — SM, QM, GR — then they are relatively simple, anyone with an average intelligence and a few years to spare can understand them. I think that this suggests that these theories point towards something that is equally simple if not much more so. The idea that the laws of Nature upon becoming increasingly simpler suddenly erupts into complexity at a certain scale doesn't seem right — to me it sounds like an idea or a set of assumptions, which are wrong.

If this is the case then we don't need AI to find the TOE — that would just confuse us and lead us astray — what we need is to reconsider the assumptions, which we have believed in so far, and then allow young people to write fewer meaningless papers and instead spend their time thinking.

10. **Thomas Larsson**
November 21, 2018

I read Horgan's book some 20 years ago. While I agree with his assessment of physics (and I thought his portrait of Witten was quite funny), I disagree with his assertion that all other fields of science are about to end at the very same time. Even if discussion of other fields are discouraged, perhaps an exception can be made for a field that ended more than a century ago: geography. Lee Smolin asked why there is no new Einstein, but no sound geographer will ask why there is no new Columbus, or even a new captain Cook. This, of course, is because geography ended for the right reason: the big story was understood, and everybody agrees upon it.

11. **Richard Gaylord**
November 21, 2018

"Sabine Hossenfelder, who is now taking on the thankless role of the field's Jeremiah". i think Bee has the role of Cassandra, rather than Jeremiah. (and many of us are thankful).

12. **John Horgan**
November 21, 2018

Peter, thanks for the shout-out. I just want to point out that the idea of "machine science" was very much in the air when I was writing The End of Science in the early 1990s, in part because of the use of computers in mathematical proofs, like the Four-Color Theorem. I talked about machine science with Freeman Dyson, Frank Tipler, Marvin Minsky and Hans Moravec, among others. Stephen Hawking–of course!–also talked about machine physics in his famous 1980 lecture "Is the End in Sight for

Theoretical Physics?" (Google it and you'll find a version published in 1981.) Hawking concluded: "At present computers are a useful aid in research but they have to be directed by human minds. However, if one extrapolates their current rapid rate of development, it would seem quite possible that they will take over altogether in theoretical physics. So maybe the end is in sight for theoretical physicists, if not for theoretical physics." That was a joke in 1980, and it's still a joke.

13. **Chris Duston**
November 21, 2018

I think it's interesting to consider a case where a machine constructs a theory which matches some set of observables, is predictive, but which does not conform to any previous approaches (i.e. geometry). For example – feed a giant neural network all the observational specifications (say, collider and astronomical lensing experiments) and train it against the resulting observations (say, cross-sections and dark matter mass determinations), and let it run. When it converges on a solution, we check it against new phenomena. If it turns out to be predictive, I think we have no choice but to consider the network to be a new physical model.

In fact, since it's a new physical model "unhindered" by the usual geometric constructions of field theory / GR, it might be something we would never have guessed at on our own. Worse, what if this is the *only* way some fundamental theory could be expressed? What if the failure of string theory is a failure in the language we are using to describe it – maybe Nature is actually governed by a set of rules which more closely resembles a neural network? Then we actually need the machines to do this for us.

14. **Peter Woit**
November 21, 2018

Chris Duston,
The problem with all ideas to use machines to analyze vast amounts of data relevant to HEP physics and look for a theory that matches it is that this has already been done, and we've found a very simple and compelling model (the SM) that essentially perfectly matches all the data. The few borderline anomalies in the data are the subject of intense analysis, but we're talking about a handful of numbers, not a huge data set.

There are groups of people doing elaborate machine calculations to try and extract something from these numbers, by e.g. fitting SUSY models with lots of parameters to them. The results have been about as worthless as one would expect to get from fits of complicated models with lots of parameters to a small number of measurements that are close to compatible with zero (deviation from SM).

The latest work discussed here is something different: the creation and manipulation of vast data sets corresponding to complicated models for which there is no evidence of any connection to experiment. Physicists have fruitlessly been doing a lot of this for years, but machines are a lot better at doing huge utterly worthless calculations.

15. **Aurélien Bellanger**
November 21, 2018

This last bit on computers taking over the meaningless calculations strongly reminds me of the end of a poem by Michel Houellebecq:

"Alors s'établira le dialogue des machines,
Et l'informationnel remplira, triomphant,
Le cadavre vidé de la structure divine;
Puis il fonctionnera jusqu'à la fin des temps."

for which a (bad) translation would be

"Then shall the dialogue of machines settle in,
And the informational shall fill up, triumphant,
The emptied corpse of the divine framework;
Then it shall operate until the end of times."

16. **Scott P.**
November 21, 2018

"Sabine Hossenfelder, who is now taking on the thankless role of the field's Jeremiah". i think Bee has the role of Cassandra, rather than Jeremiah. (and many of us are thankful).

I think Jeremiah is the appropriate comparison. He preached to Israel that they had sinned and needed to repent — I see Hossfelder as doing the same (without the moral dimension). For her to be Cassandra she would have to have the correct Theory of Everything but have nobody listen to her.

17. **Fred**
November 21, 2018

'performing ever more complex, elaborate and meaningless calculations, for ever and ever', that sounds like the bitcoin network. A bit more serious, AI without incorporating domain knowledge first has to extract the domain knowledge out of the data before coming up with anything usefull, for HEP that would be the SM as Peter rightly points out. I'm of the firm opinion you have to integrate AI and domain knowledge to stand any chance of a result.

18. **Chris Oakley**
November 21, 2018

I agree with Richard Gaylord that Bee has the role of Cassandra, who prophesied the fall of Troy, with no-one paying attention. Except that the fall here is de-funding. Although Cassandra, to be fair, never stood on the ramparts pointing out the weak spots to the Greeks.

19. **Oldster**
November 21, 2018

If the machines take over theoretical physics, what's to stop them from settling on some model analogous to Ptolemy's Theory of planetary motion, and then just adding more epicycles as the data begins to disagree with the model? That could go on for a long time, and yet be nowhere near, "The truth," while the results remain in excellent agreement with new data, unlike the Copernican model, which required Kepler's additional insights.

20. **a1**
November 21, 2018

Don't you see it coming? God's existence proved by AI.

21. **Yatima**
November 21, 2018

The idea that the function fitting extravaganza for cars and toasters that is marketodroidically sold as “AI” nowadays could perform new discoveries is advised to take a dose of patend scepticism by reading this little gem (also appeared in CACM of October 2018):

Human-Level Intelligence or Animal-Like Abilities? by Adnan Darwiche.

We need to go back to symbolic AI and do the real, hard work. Enough with the hype & shortcuts!

As for “Toward an AI Physicist for Unsupervised Learning”, this sounds like another fun iteration of Douglas Lenat’s Eurisko (who isn’t even named in the references) from the early 80s, but with more processing power.

What do the Bogdanoff brothers have to say to all this?

22. **Amitabh Lath**
November 21, 2018

AI and machine learning are making big inroads in experiment. There was a little bit of pushback at first but one cannot argue against the huge increase in efficiency for all sorts of signals, and reduction in backgrounds. Many LHC Run3 searches will incorporate AI techniques that make cut and count analyses look medieval.

If google can train a filter for cat photos, we can do one for SUSY or leptoquarks or micro black holes.

Given how difficult it is to construct and train these things, and get an answer that is distinguishable from gibberish, I suspect any theory implementation is very very very far away. But I would like to know if anyone is trying.

23. **saty chary**
November 21, 2018

LOVE Anindya’s and LDK’s and Fred’s responses.

What we have today, is something that can “learn” a pattern from input data (a fancy form of fitting a regression line through a bunch of roughly linear 2D points, to “learn” the slope and y-intercept of the derived line equation). PERIOD. From this, the “learner” can’t magically explain string theory to you, discover fundamental laws that humans have “overlooked”, etc, etc. It is wishful, uninformed, and dangerous, to attribute mystical powers to such a primitive system.

24. **Chris Duston**
November 21, 2018

Peter,
I agree that using machine learning is nothing new in physics, but what I’m saying is I think it could begin to be something new. The SM is constructed in the manner I described – geometry + analysis (basically, symmetry + least action + quantization). But what if that process is *wrong* at the fundamental level. Maybe the fundamental geometric structure is actually nothing we’ve thought of yet (like, not manifold + fiber bundles), or maybe the quantization process is off base in some subtle way that we won’t work out without giving up the entire thing.

Specifics are obviously navel-gazing, but the point is that without being restricted to a particular view of fundamental theories, a neutral network might come up with something which works, but that we don’t understand. And I think it’s an interesting question to consider if we would accept such a thing as a physical law.

25. **Rod Deyo**
November 21, 2018

The vision of an omniscient AI alogrihms is not grounded in current state-of-the-art research or implementations. Statistical machine learning is what is available today (and likely far into the future). These algorithms tend to be error prone for rare patterns with small priors and are always subject to biased data collection.

Popular neural net algorithms, such as unsupervised deep learning, suffer from lack of a principled learning theory, so results are often a training crapshoot relying on huge amounts of redundant data and subject to serious error with rarer statistical patterns. Accurately evaluating error for rare patterns is itself almost impossible at scale if labeled data is needed. Recognizing cats in a photo may be easy in general, but can be suprisingly hard for particular rare images. False positives proliferate with attempts at improved recall for rare cases. No amount of clever training, or attempts at applied Bayesian Mysticism, alters this.

The irony is, given the available LHC data, any learned patterns would most likely just reflect some version of the Standard Model and so experimenters would need to eliminate any filters to capture more of fthe llood of raw collider events to find any fundamentally new and statistically persistent patterns possibly hidden in the data.

26. **srp**
November 21, 2018

Given all the results on adversarial networks, which show how trained neural networks are subject to spoofing by manipulating tiny parts of the data which they are presented, it might be a good idea to see if there aren’t multiple trained versions with different weights that explain the data equally well.

27. **Paul B**
November 22, 2018

“Distilling Free-Form Natural Laws from Experimental Data”
Michael Schmidt1, Hod Lipson2,3,*
Science 03 Apr 2009:
Vol. 324, Issue 5923, pp. 81-85
DOI: 10.1126/science.1165893

Abstract
For centuries, scientists have attempted to identify and document analytical laws that underlie physical phenomena in nature. Despite the prevalence of computing power, the process of finding natural laws and their corresponding equations has resisted automation. A key challenge to finding analytic relations automatically is defining algorithmically what makes a correlation in observed data important and insightful. We propose a principle for the identification of nontriviality. We demonstrated this approach by automatically searching motion-tracking data captured from various physical systems, ranging from simple harmonic oscillators to chaotic double-pendula. Without any prior knowledge about physics, kinematics, or geometry, the algorithm discovered Hamiltonians, Lagrangians, and other laws of geometric and momentum conservation. The discovery rate accelerated as laws found for simpler systems were used to bootstrap explanations for more complex systems, gradually uncovering the “alphabet” used to describe those systems.

http://science.sciencemag.org/content/324/5923/81?sid=8163327b-a375-433c-8ff9-5ca5d57a89ea

Thought this might be of interest.

28. **Bob Rehbock**
November 22, 2018

A truly superhuman intelligence might well prefer to choose it's own problems to solve. Likely it would decline to pursue that irrelevant to its interests. It might decide that it would prefer the plug pulled than spend an eternity on a not even wrong idea.

29. **Amitabh Lath**
November 22, 2018

Rod, your comment "…experimenters would need to eliminate any filters to capture more of the flood of raw collider events…" is well taken. There are minimum-bias and zero-bias filters in place and even at their low rates they have accumulated a lot of data over the years. But any analysis one has to have a compelling model of physics that would only be captured by these raw datastreams, and missed by any of dozens of other higher rate filters (and also be missed by LEP, Tevatron, etc). There aren't a huge number of such new physics models out there. If you know of any theorists working on "LHC blind spot" models, contact your local experimentalists.

30. **Chris Oakley**
November 22, 2018

For some reason, this talk of using computers to build theories reminds me of the machine (from Douglas Adams' *Dirk Gently's Holistic Detective Agency*) for watching the TV programs you have recorded, but will never get round to actually watching yourself.

31. **Ray**
November 23, 2018

Thanks to Yatima for pointing to the article:

Human-Level Intelligence or Animal-Like Abilities? by Adnan Darwiche.

which I too would recommend that anyone interested in current sate of "AI" and machine learning read. It explains simply the difference between AI as it is practiced now and the AI approaches that people who were at college in the 80s (like myself) might think of as being AI.

When thinking about what current AI approaches can offer high energy physics the advantages of machine learning techinques being used for searching for patterns in huge amounts of collider data are probably clear (with the provisio that you'll always find something if you look hard enough), and these "discoveries" can then be fed back for comparison to current theory. However, this is a long long way from an AI approach which can generate a theory or even provide an explanation for what is observed (as someone above said: function fitting will not provide this), let alone make a valid prediction for new phenomena.

Statistics/machine learning are disciplines which in a sense sit on top of other sciences. Statistics in particular is about summaries, what happens in general. This is why these approaches can be applied across multiple areas, physics, biology, genetics, health analytics, financial services etc, and this is why it is such a boom industry for the time being. Relying on these methods to directly say something interesting about the underlying phenomena is probably a step to far.

Further, the presences of biases in observational data is a huge risk for these methods – by definition they have to in some sense reflect the data they see, so baised data means biased models/functions. Any approaches which try to take this in to account that I have seen are generally of the kludge variety, i.e. specific to one particular data set. This may be OK for a specific project but won't generalise too well.

32. **LMR**
November 25, 2018

I'd also like to thank Yatima for pointing out Adnan Darwiche's article. Unlike some of the other commenters, however, I don't see the article as being negative on the state of AI.

In fact, Darwiche sees his article as starting a conversation, and it has indeed done so since he first drafted the article in November 2016. There's now more research blending deep learning with other techniques in AI, Wu and Tegmark being one such example. (Incidentally, this is why I don't see why they should cite Eurisko: Eurisko used heuristics, whereas Wu and Tegmark used reasoning to extract simpler models from what machine learning could get from data.)

As for Darwiche's "theory of cognitive functions", in which he envisioned a theory comparable to the CNFs, DNFs and OBDDs of Boolean functions for the functions encoded in deep neural networks, I'd like to point to a paper by Wang et al., in which they characterized forward and back propagation as symbolic differentiation of ANF- (resp. CPS-) transformed programs:

Demystifying Differentiable Programming: Shift/Reset the Penultimate Backpropagator
Fei Wang, Xilun Wu, Gregory Essertel, James Decker, Tiark Rompf
https://arxiv.org/abs/1803.10228

So that "theory of cognitive functions" already exists in an embryonic state, and Darwiche's paper is a bit out of date. And that, I think, is a good thing.

33. **Amitabh Lath**
November 25, 2018

Machine learning will (probably, hopefully) be able to parse large datasets (not just high energy physics) in ways orthogonal to how it is done now, and point out anomalies.

It may have the basic rules of physics (QED, QCD, etc) fed to it, or it may derive something like that itself by "self learning" (both approaches are being tried). The output will probably be some sort of "likelihood of new physics": Something like "these 10 events don't look like these other 10e9 events" , and maybe a few hints as to the metric used. That's probably all we can expect from "AI" in the near future.

It will be up to humans to figure out if there is indeed anything new, or just a poorly understood part of already known physics. Unless humans figure this part out, I don't think it's getting published.

This is how the AIs that excelled in chess, Jeopardy and go worked, with human intervention and interpretation at every step.

34. **Carina**
November 27, 2018

Peter,

Sabine Hossenfelder is not the field's Jeremiah. Jeremiah knew what the right path was, Sabine doesn't.

It is true that nobody can say what unification looks like. It is true that people only search where they are paid to search. But that is not a reason to complain. Jeremiah knew where the truth is, and led people there. Sabine does not; in fact there are many hints that she even refuses to do so.

[Personal attack on Hossenfelder deleted]

35. **Peter Woit**
November 27, 2018

Carina,
You might want to look up the story of Jeremiah. The truth of his prophecy was the destruction of Jerusalem. Hossenfelder is warning of the oncoming destruction of a great intellectual field, I don't think you want her to lead us there. Jeremiah's positive advice was to stop sinning and repent. It was ignored and destruction followed.

You also might want to stop anonymously posting personal attacks on people and their motivations. Jeremiah wasn't popular and had a lot to deal with, but anonymous internet attacks at least weren't a part of it.

36. **Carina**
November 27, 2018

Peter,

different aspects of Jeremiah's life can be takes as parallel. I thought about how he seeked God with all his energy, and tried to convince people to do the same. I meant this as the right path. I recall a TV interview with Salam, in which he stated that he believed in unification because "God is one!"

For me Jeremiah has always been a voice that told people to follow the right path, namely to do God's will, and not to follow other gods. Today, we would add "money" to those "other gods". Peter, you do not make money with this blog and you are searching for unification. So I would say you do what Jeremia wanted. It is just that I find it difficult listen to criticism from people not searching for unification but still searching for money. In my view, this is not what Jeremiah did.

What I wrote and what you deleted was not meant as a personal attack, but as my own personal conviction. I am sorry if it did come over as an attack. If it came over as such, you were right to delete it.

37. **Anindya**
November 28, 2018

I'm wondering how anyone could dispute Sabine's assessment of the state of (fundamental) physics and cosmology.
The stagnation is obvious, even if you only read the popular science literature.
In the 1990's, Scientific American and New Scientist were running articles about string theory unification and multiverses. They are *still* doing the same and it's all as completely speculative as ever.
If anything, the hype has gotten wilder – instead of Linde's inflationary multiverse, you now have Tegmark's "four levels of multiverse", gee-whiz stuff like "what if everything was a simulation ?" being paraded as science and so on.
Meanwhile, tens of thousands of research papers have been published and the number keeps growing.
If this isn't stagnation, what is ?

38. **TomH**
November 29, 2018

re Paul B's mention of "Distilling Free-Form Natural Laws from Experimental Data" .
Maybe there's less there than meets the eye, for exmpl:

https://arxiv.org/abs/1210.7273
In the article "Distilling free-form natural laws from experimental data", Schmidt and Lipson introduced the idea that free-form natural laws can be learned from experimental measurements in a physical system using symbolic (genetic) regression algorithms.

An important claim in this work is that the algorithm finds laws in data without having incorporated any prior knowledge of physics.

Upon close inspection, however, we show that their method implicitly incorporates Hamilton's equations of motions and Newton's second law, demystifying how they are able to find Hamiltonians and special classes of Lagrangians from data.

39. **Amitabh Lath**
November 29, 2018

Look, the multiverse is silly but this heaping of dudgeon is not the way to get it to stop. This is what happens when there aren't any experimental surprises in a while, the theoretical world gets fuzzy.

This happened in the 1960's, theorists delving into Eastern mysticism (Tao of Physics and crap like that) and then quarks came on the scene and all that weirdness just faded away.

New physics will be found. If not at colliders then some satellite experiment or tank of cryo fluid deep underground, somebody will see something. And after that nobody will bother with all this multiverse/landscape nonsense anymore. Until then chill and support your local experimentalists.

40. **IJK**
December 2, 2018

Concerning Chris Duston's suggestion: let's apply the program that you are proposing, not to the data that you suggest, but to what was known about natural philosophy in the 1650s. The AI might come up with Newton's laws, his gravity theory, and maybe even the calculus. But then again it might come up with something totally different, just as predictive and powerful. It might even be an overall simpler framework, or perhaps (and, in my opinion, a far more likely outcome) vastly more complex. Are you still happy adopting this latter outcome?

41. **CuriousMonkey**
December 5, 2018

Hi Peter, you will probably like this: a Quanta interview with the distinguished astrophysicist Martin Rees says "I'm hoping we'll see more theoretical ideas from particle physics, which has given us little firm theoretical progress in recent years."

42. **mitch**
December 7, 2018

https://blog.ycombinator.com/leonard-susskind-on-richard-feynman-the-holographic-principle-and-unanswered-questions-in-physics/

Notes on Current Affairs

December 8, 2018
Categories: Uncategorized

Blogging has been light recently, partly due to quite a bit of traveling. This included a brief trip the week before last to Los Angeles, where I met up with, among others, Sabine Hossenfelder. This past week I was in Washington DC for a few days, and gave a talk at the US Naval Observatory, one rather similar to my talk earlier this year in Rochester. In addition, I've been trying to spend my time on more fruitful activities, especially a long-standing unfinished project to make sense of the relationship between BRST and Dirac cohomology. Optimistically, I'll have a finished (or at least finished enough to make public) version of something later in January, after taking a couple weeks at the beginning of the month for a vacation in France.

I'm completely in agreement with Sabine about the sad state of high energy particle theory, and glad to see that she has been forcefully trying to get people to acknowledge the problem. I don't agree though with her "Lost in Math" characterization of the problem, and my talks in DC and Rochester tried to make the case that what is needed is more interaction with mathematics, not less.

Various things that might be of interest that have to do with the state of high energy physics theory are the following:

- A [Y Combinator blog interview with Lenny Susskind](). I think it's fair to say that Susskind now admits that string theory, as currently understood, cannot explain the Standard Model, and that as a result he has given up on trying to make any progress on particle theory. He says:

  > My guess is, the theory of the real world may have things to do with string theory but it's not string theory in it's formal, rigorous, mathematical sense. We know that the formal, by formal I mean mathematically, rigorous structure that string theory became. It became a mathematical structure of great rigor and consistency that it, in itself, as it is, cannot describe the real world of particles. It has to be modified, it has to be generalized, it has to be put in a slightly bigger context. The exact thing, which I call string theory, which is this mathematical structure, is not going to be able to, by itself, describe particles…
  >
  > We made great progress in understanding elementary particles for a long time, and it was always progressed, though, in hand-in-hand with experimental developments, big accelerators and so forth. We seem to have run out of new experimental data, even though there was a big experimental project, the LHC at CERN, whatever that is? A great big machine that produces particles and collides them. I don't want to use the word disappointingly, well, I will anyway, disappointingly, it simply didn't give any new information. Particle physics has run into, what I suspect is a temporary brick wall, it's been, basically since the early 1980s, that it hasn't changed. I don't see at the present time, for me, much profit in pursuing it.

- Susskind instead spends his time on highly speculative ideas relating geometry and quantum theory, with the idea that while this has no connection to particle physics, it might somehow lead to progress in understanding quantum gravity. Natalie Wolchover at Quanta has a [new story about Susskind's latest speculations]().
- This past week the Simons Foundation-funded "It from Qubit" collaboration has been having a two-part conference. This started at the IAS, talks available [here](), then moved on to the Simons Foundation headquarters in NYC (see [here]()). Videos of the IAS part are available, for the NYC part, there's [Twitter](). George Musser reports that Juan Maldacena has figured out how to construct (in principle) traversable wormholes, and that he's arguing that "quantum computers are so powerful that they create spacetime". For a tweet showing a summary of what has been achieved, see [here]().

  Personally I've always been dubious that we'll ever have a useful "quantum theory of gravity" unless we have some sort of unification with the standard model, which would provide a connection to things we can understand and measure. Lacking such a connection, another way to go would be to try and evaluate a "quantum theory of gravity" proposal based on its mathematical consistency, coherence and beauty. My problem with the "It from Qubit" program is that, ignoring the way it gives up on connecting to what we understand, I've never seen anything coming out of it that looks like an actual well-defined theory of quantum gravity that one could evaluate as a mathematical model consistent with quantum mechanics and what we know about 3+1d general relativity.

- For something about quantum computation and its relation to fundamental physics that I can understand, John Preskill has a wonderful article on [Simulating quantum field theory with a quantum computer](). Nothing there I can see about quantum gravity.
- Given that its founder Susskind and the other leading figures of the field at the IAS have pretty much given up on the project of relating string theory to particle physics, an interesting question is that of why the pathology of so many researchers still working on this failed project? The source of this pathology and the question of what can be done about it I think are at the center of Sabine Hossenfelder's book and recent blogging. An important question that is getting raised here is that of the damage this situation is doing to the credibility of science. If you want to fight the good fight against those who, because it threatens their tribe, want to deny the facts of climate science, is it helpful if many of the best and brightest in science are denying facts that threaten their tribe? Scientific American has a story about [Why Smart People Are Vulnerable to Putting Tribe Before Truth](), but it doesn't make clear the depth of the problem (i.e. that some of the smartest scientists around are doing this).
- For an example of the problem, see [an interview with Gabriele Veneziano about the history and current state of string theory](). He's in denial of the obvious fact that string theory makes no predictions:

  > People say that string theory doesn't make predictions, but that's simply not true. It predicts the dimensionality of space, which is the only theory so far to do so, and it also predicts, at tree level (the lowest level of approximation for a quantum-relativistic theory), a whole lot of massless scalars that threaten the equivalence principle (the universality of free-fall), which is by now very well tested. If we could trust this tree-level prediction, string theory would be already falsified. But the same would be true of QCD, since at tree level it implies the existence of free quarks. In other words: the new string theory, just like the old one, can be falsified by large-distance experiments provided we can trust the level of approximation at which it is solved. On the other hand, in order to test string theory at short distance, the best way is through cosmology. Around (i.e. at, before, or soon after) the Big Bang, string theory may have left its imprint on the early universe and its subsequent expansion can bring those to macroscopic scales today.

  This take on how you evaluate a theory by comparing it to experiment is not one that will give the average person much understanding of the scientific method or much confidence in scientists and their devotion to it.

Comments

1. **Pascal**
   December 8, 2018

   > I don't agree though with her "Lost in Math" characterization of the problem,
   > and my talks in DC and Rochester tried to make the case that what is needed is
   >more interaction with mathematics, not less.

   In fact Sabine says essentially the same thing in her blog and her book; this is probably not well reflected in the book's title, though.

2. **Sabine Hossenfelder**
   December 9, 2018

Peter,

I am certainly not saying that physicists need less contact with math (or less math, period). I am saying they need to be more careful using their math. Look at the issue with naturalness I go on about in the book – this would have been preventable had physicists actually bothered to write down their assumptions and made derivations from that. Indeed, my major conclusion in the book is that physicists should focus on mathematically well-defined problems. (Lack of naturalness is not one of those – at least not in the present formulation. Maybe it can be made into on. I tried but did not succeed at that.)

This is why I am not a fan of unification attempts – it seems mathematically unnecessary to me. This is not to say that one should not work on it, just that because of the lack o a well-defined problem, it's likely to end up in guess-work and beauty-arguments that result in useless theories.

I also note, however, that mathematics *alone* will never be sufficient, in case that's what you are referring to.

It's right, in a sense, that string theory does make predictions. It predicts, erm, strings. It's just that you'd reliably see those (say, in terms of excitations) only at ridiculously high energies. You know that story, so I'll not repeat it.

Also, as I tried to explain in LA, it seems to me entirely possible to test the weak-field approximation. I wrote about the (type of) experiment I had in mind here. As I pointed out, it would be good if someone bothered making predictions for that. Composite objects in quantum superpositions of position eigenstates. I don't understand why no one even seems to care.

Srsly, Peter, think about this for a moment. You sit in a room with a group of people who have spent big parts of their life thinking about how to improve our understanding of the fundamental laws of nature, and no one has ever even heard of the one experiment that's possibly about to just test it? Witness the confusion in the room. How come? What the heck is going on in this community? Can you see why I keep going on about how bad we are in aggregating information and how big of a problem that has become?

3. **Narad**
December 9, 2018

> This past week I was in Washington DC for a few days, and gave a talk at the US Naval Observatory….

I wish use of the time ball were not merely an annual ceremonial event these days.

4. **Ted Rogers**
December 9, 2018

Sabine,

My sense was that your title "Lost in Math" was really referring to the practice of dressing up purely subjective and biased human judgments to look like rigorous and sophisticated mathematical arguments, to the point where the subjective basis of certain ideas becomes very obscure. So it wasn't meant to be critical of using mathematics per se. Is that a fair summary?

5. **Peter Woit**
December 9, 2018

Sabine,
I think where we disagree is that I do think that the pursuit of unification attempts is worthwhile, even if not "mathematically necessary", and that, absent new experimental data, such work is going to need to rely on judgments which can be characterized as judgments of "beauty". The problem is that a lot more clarity and honesty is needed in evaluating "beauty". Right now for example, we have string theorists arguing for the "beauty" of string unification, without acknowledging that all known ways of getting this unification are actually hideously ugly (realistic string vacua).

To get away from ill-defined terms, maybe a better way of saying things is that I see the history of fundamental physics as involving a sequence of deeper understandings of the implications of symmetry, and I think we should be looking for the next, deeper, way of understanding such implications. For a specific example, our current understanding of gauge theory invokes BRST symmetry, which may very well need to be replaced by a different, more powerful (and more beautiful…) mathematical structure. I would agree that what people are mostly doing, endless repetition of attempts to get something out of the same old symmetry arguments, is getting "lost in math" and goes nowhere.

About string theory predictions, the problem is you need to say what you mean by "string theory". It's interesting that Susskind is well aware of the problem and tries to explain it: the "string theory" we understand, perturbative string theory, can't give a consistent model that looks like the real world. Guesses as to what the true consistent version of string theory is supposed to be don't necessarily involve strings at high energy (e.g. you may instead see some sort of M-theory background with no strings).

About the problems with purely quantum gravity research, I think we agree. I am less optimistic though about such research, in the absence of any new ideas about the relation of internal and space-time symmetries that would connect such research to what we can understand and measure about particle physics.

6. **Peter Shor**
December 9, 2018

Gabriele Veneziano says that string theory "predicts the dimensionality of space."

Correct me if I'm wrong, but doesn't it predict 10 dimensions? You can compactify it to 4 dimensions, but is there any good reason to prefer 4 dimensions over 5, 6, 7, 8, 9, or 10? If there isn't, I am not very impressed by this prediction.

7. **Peter Donnelly**
December 9, 2018

You're kind to Leonard Susskind: I recall how rude and dismissive he was towards you back in the day. I always found it hard to reconcile this with the genial figure he appears to be in his (excellent) many video lecture series; many of which are community outreach.

8. **Sabine Hossenfelder**
December 10, 2018

Ted,

Yes, that's a good summary.

Peter,

I don't think there's anything to learn from debating in which sense string theory or unification is beautiful or not. Look at what has historically worked: It was either developments guided by experiment (say, electrodynamics), or it was an actual mathematical inconsistency (special relativity, Dirac's equation, renormalizability of the weak interaction).

The conclusion I draw is that we'd be well-advised to stop talking about beauty and focus on these two types of problems: inconsistency with data, and internal inconsistency. Everything else is just poking in the dark. May work, but it's unlikely to work.

Veneziano was probably referring to calculations he's done himself on graviton scattering and the like, see eg 1008.4773 and similar.

9. **Peter Woit**
December 10, 2018

Peter Shor,
Yes. Together with his explanation that string theory predicts unseen long-range forces, Veneziano's argument seems to be that string theory does too make predictions, wrong ones. Bizarre.

Peter Donnelly,
For context to your remark, there's an old blog posting about this:
https://www.math.columbia.edu/~woit/wordpress/?p=454
Also for context, one of the blurbs on the back of the French edition of Susskind's QM book is from me.
I've never really met Susskind (I did once ask him a hostile question at the end of a colloquium talk…) and, to the extent there are examples of rude and dismissive comments from him about me, I never taken them personally. He's not the only well-known string theorist I've never met who is generally known for their good humor and generous behavior, but seem to lose it when dealing with a challenge to a research program they are heavily invested in.

10. **Erstwhile number-theorist**
December 10, 2018

I must speak out in support of Sabine Hossenfelder's point of view. To me, most of the talk about 'beauty' in mathematics (and theoretical physics) smacks of over-romanticization.

For one thing, the experience of 'conceptual beauty' is somewhat dependent on one's skill-level. By the time one understands a concept sufficiently thoroughly that it becomes trivial, the concept has lost most of its beauty. There is an experience of beauty in the ineffable sense that not everything is fully comprehended or accounted for. Furthermore, I don't want to go so far as to say that "beauty in mathematics is subjective," e.g. because there are legion mathematical concepts and theorems that many people do agree about are beautiful, but of course it goes without saying that it is subjective enough that it would complicate discussions endlessly if it were taken as a criterion for scientific progress.

But I would like to go further. I see the following problem with an over-emphasis on beauty as a criterion for truth — rather than say, regarding beauty as being a by-product of truth, which to me seems to have the virtue of not unnecessarily obscuring matters. The problem is that it endows science with a special kind of mystique, since it adds a further quality to the list of necessary requirements for the prospective scientist: this person must be able to feel and appreciate the *beauty* of science. We risk turning the scientist into a kind of 21st century priest, a person with a mysterious insight into a non-empirical matter such as 'mathematical beauty'.

I think we have definitely advanced a long way along this road, of turning science into a new kind of religion. And the current malaise in high-energy physics, with its curious blend of mathematical fetishizing and crazy unfalfisiable sci-fi thought experiments (Are there parallel universes?? Are we living in a simulation??), to me is the best kind of proof of this.

So the best thing we can do is voice a clear *no* to everything that is bad about non-scientific thought. Perhaps we should even get clear first about what has brought science so far to begin with, because so many people seem to regress into unscientific and irrational modes of thinking precisely under the guise of scientific thought. If we do not want to get completely lost in a pseudo-scientific fantasy world, I think we should start soon.

11. **Peter Woit**
December 10, 2018

Erstwhile number-theorist,

I think Sabine in her comment does a good job of making precise the "don't pay attention to beauty" argument. The argument is that in physics one has made progress by paying attention to inconsistency, either internal or with data, that anything else is "poking in the dark" and unlikely to work.

My counter-argument is that, on many questions about fundamental physics, the field is facing a lack of experimental inconsistencies, and not very good prospects for finding new ones anytime soon. I agree that under the circumstances, focus should be on internal inconsistencies, and there is far too little of that. But, in this situation, with guidance purely from the formal structure of the theory, one is in much the same situation that mathematicians have always been in. And, in mathematics, I'd argue that only paying attention to logical consistency isn't enough: if that's all you demand, you'll end up with a large amount of consistent but empty knowledge. For example, you may very well find lots of complicated, but consistent, ways of reconciling quantum theory and gravity. If you end up with an absurdly complex untestable mess like the string landscape, the fact that it may be logically consistent is not enough to make it a promising path forward.

Something more than consistency is typically involved in inspiring great new mathematical ideas, with a "search for beauty" one ill-defined way to characterize what sometimes motivates the research from which some such ideas emerge. Yes, this is a very hard way to make progress on fundamental physics, but it is not impossible to find new ideas and make progress this way, and it may be the only choice people have who want to continue to work on certain questions. Some people should be pursuing this, and to have any success they'll have to have something a lot more sophisticated than a naive idea about what is beautiful and what isn't beautiful. I'd argue that the best mathematicians often are motivated by such ideas.

12. **Lee Smolin**
December 10, 2018

I agree with much of Sabine's argument, but I have a somewhat different perspective on one aspect of it, which is her criticism of the concept of naturalness, which is the requirement that any pure dimensionless constants in the parameters of a physical theory that are not order unity require explanation. We say a dimensionless constant that is orders of magnitude away from unity requires "fine tuning." The standard model has, depending on what you count, 29 dimensionless parameters, most of whom are not order unity and hence are fine tuned. The lesson that many people took away from this fact was to attempt to embed the standard model in another, possibly more unified theory, with fewer adjustable finely tuned parameters. Examples that were proposed included technicolour and supersymmetry. These failed, in some cases in more than one way: for example, the minimal supersymmetric extension of the standard model has more parameters-not fewer, and indeed 105 of them, which is not a small problem. And they are not less fine tuned than the original parameters.

This was not an aesthetic imperative, for we do not seek to reduce the number of finely tuned parameters to make a theory more beautiful, but to increase its explanatory power. This is a general aim of science.

What I take away from the failure of these attempts is that we have to accept on face value that the parameters of the standard model have been fine tuned to unnatural values. This requires explanation. One form that such an explanation could take would be if sometime in the past there acted a physical mechanism which choose such unlikely values for the parameters.

I have suggested one possible mechanism for this fine tuning, which is cosmological natural selection. This is falsifiable and hence will probably be shown wrong. But that is the price you pay for a genuine increase in the explanatory power-if true-of the hypothesis.

My bottom line, then, is that when taken to describe a physical theory, beauty is sometimes a measure of explanatory power, and this can be a good thing. Of course it often is not, in which case Sabine's arguments apply.

Thanks,

Lee

13. **Peter Shor**
December 10, 2018

@Lee:

When you say that "any pure dimensionless constants in the parameters of a physical theory that are not order unity require explanation," you are implicitly putting a probability distribution on the positive reals which is sharply peaked at unity.

Doesn't this assumption also require explanation? Why should the range of numbers between 1 and 2 be any more probable than the range between 10^10 and 10^20? Aren't there just as many numbers in the range between 10^10 and 10^20? as there are between 1 and 2? (Uncountably many in each.)

14. **quanta by any other name**
December 12, 2018

Thanks again for the links, I don't know how you find all of these.

Susskind said something else which everyone on this blog seems to be ignoring. At about 14:47 he says "will what does correctly describe particles be a small modification of [string theory] or a big modification, that's what I don't know."

Thus, if we are going by what Susskind says, we should start with string theory and modify it, not abandon it.

15. **Lee Smolin**
December 12, 2018

Dear Peter Shor,

Yes, exactly, and let me explain where that expectation for dimensionless ratios to be order unity comes from.

Part of the craft of a physicist is that a good test of whether you understand a physical phenomena-say a scattering experiment-is whether you can devise a rough model that, with a combination of dimensional analysis and order of magnitude reasoning, gets you an estimate to within a few orders of magnitude of the measured experimental value. People like Fermi and Feynman were masters at this, a skill that was widely praised and admired.

The presumption (rewarded in many, many cases) was that the difference between such rough estimates and the exact values (which were by definition dimensionless ratios) were expressed as integrals over angles and solid angles, coming from the geometry of the experiment, and these always gave you factors like 1/2pi or 4pi^2, which were order unity.

Conversely, if your best rough estimate does not get you within a few orders of magnitude of the measured value, then you don't understand something basic about your experiment.

Seen from the viewpoint of this craft, if your best estimate for a quantity like the energy density of the vacuum is 120 orders of magnitude larger than the measured value, the lesson is that we don't understand something very basic about physics.

Thanks,

Lee

16. **Cedric Bardot**
December 12, 2018

@PeterShor
Gabriele Veneziano can be seen in a video entitled "Why Four Dimensions and the Standard Model Coupled to Gravity … ". The speaker explains (following Solid Theoretical Research In Natural Geometric Structures) how "dimension 4 appears as a critical dimension because finding a given manifold as an irreducible representation* requires finding two maps to the sphere such that their singular sets do not intersect. In dimension n the singular sets can have (as a virtue of complex analysis) dimension as low as n-2 (but no less) and thus a general position argument works if (n-2)+(n-2) is less than n, while n=4 is the critical value. "
I let curious people to find out easily the source of this quote. To understand it is more challenging and requires some knowledge in differential geometry and operator algebra…

Another relevant detail I think for the issue discussed by Peter in his post: the speaker in the video is not Veneziano and is pretty well known to be doubtful about string theory. Nevertheless at the end of this video he praises publicly the italian theoretical physicist : "It's crucial we have doubts and we manifest these doubts… if we are just preaching this is a catastrophy… I admire Gabriele for that … we spend our life doubting , the chance we are right are tiny". The speaker and Veneziano had been professors at College de France at the same time for a while. Both are emeritus now.

PS: the link to the video is https://www.youtube.com/watch?v=qVqqftQ92kA
* irreducible representation of a specific operator algebraic equation…

17. **Peter Woit**
December 12, 2018

quanta by any other name:
Your Susskind quote I think reflects one of the most serious problems facing this field of physics. For Susskind and many others, string theory has become such a

dominant paradigm that, despite its complete failure as a unified theory, they cannot even envisage looking for a completely different starting point. The only conceivable direction for progress is to start with string theory and modify it, by a small or big amount. I think Susskind recognizes this hasn't worked so far, but after spending decades devoted to this ideology, he can't conceive of trying to start from a different point.

18. **Matt Foster**
December 19, 2018

As someone who has been led by the nose to some problems of "quantum geometry" in low dimensions (through some possible applications to mundane topological condensed matter systems), I've been digging through the old string textbooks like GSW to extract useful technology.

The thing I've never understood, and still don't understand, is why choose D = 26 for the bosonic string. The classical theory of the 2+0-D massless bosonic worldsheet decouples from the geometry of the sheet in the conformal gauge. The anomaly (if I understand correctly) says that the stress tensor acquires a nonzero trace whenever the worldsheet is not flat, proportional to the (gauge invariant) scalar curvature, unless we have 26 bosons to cancel the negative central charge of the Fadeev-Popov ghosts.

This just means that the free fixed point is unstable due to quantum corrections, right? The generation of a nonzero stress tensor trace from the scalar curvature looks like gauge invariant statement. If you want to study quantum strings (forget for the moment whether they have anything to do with gravity), then going to D = 26 makes that task easier, modulo the tachyon problem. But if you want a 3+1-D theory, can we be sure there isn't some strong coupling fixed point which itself could be critical (conformally invariant), but not perturbatively connected to the free one? Are we really forced to go to the critical dimension by physics, or is it just mathematical convenience (essentially integrability of worldsheet CFT) in this case??

19. **Peter Woit**
December 19, 2018

Matt Foster,
This is a rather off-topic technical question. There's a huge literature on string theory in non-critical dimensions, and many people over the years have looked into this. I think a fair summary is that no-one knows how to get a realistic unified theory this way, thus the concentration of attention on starting with strings in the critical dimension then somehow getting rid of the excess dimensions.

This Week’s Hype

December 19, 2018
Categories: This Week's Hype

A few months ago string theorists at Stanford had their university press office put out hype-filled promotional material about their research field, this was discussed here. One odd thing about this was that normally such PR efforts are made in connection with news of a supposed advance, but this press story had no news, just promotion of old and unsuccessful ideas.

This week it’s the turn of the string theorists at Princeton University, with Beyond Einstein: Physicists find surprising connections in the cosmos. Like the Stanford one, this story is not about any recent advance (there haven’t been any) but just a recounting of something from twenty years ago, without any acknowledgment that things haven’t worked out as hoped. To compare and contrast, another Princeton press office effort on the same topic back in 2007 (see here) at least had a specific new paper to advertise. The level of deception of the public though remains constant, no change from the 2007 highly misleading “Princeton Physicists Connect String Theory With Established Physics”.

The way this works, the misleading university press release gets picked up by others, who either just pass along the press release or write something based on it (with an even more misleading title). As an example of the first, phys.org yesterday had Gravity is mathematically relatable to dynamics of subatomic particles. For the second, today Science Alert has Here’s Why String Theory Might Actually Point Us Towards a ‘Theory of Everything’. I suspect we’ll see yet more of this in the next few days as the hype-diffusion process initiated by the Princeton theorists takes its usual course.

The basic, intentionally misleading, PR claim at that bottom of this is the characterization of AdS/CFT as:

> The key insight is that gravity, the force that brings baseballs back to Earth and governs the growth of black holes, is mathematically relatable to the peculiar antics of the subatomic particles that make up all the matter around us.

What’s not mentioned is that this has nothing to do with either gravity as experienced by baseballs, or the subatomic particles that make up matter. The AdS/CFT conjecture relates gravity in the wrong space-time dimension (5), with the wrong space-time curvature (AdS) to a quantum field theory that doesn’t describe any known particles (N=4 SSYM). For the last twenty years there has been lots of speculation about the possibility of extending this to the real world cases, but this hasn’t worked out. There’s no known dual QFT to gravity in the physical dimension with the physical sign of curvature, and, from the other side, no known gravity theory dual to the Standard Model.

As with the recent Stanford effort, the great thing about having your own university press office do this and not involve journalists is that they just talk about you and let you say whatever misleading thing you want. No danger that this kind of story will raise embarrassing questions or that it will give a voice to or even acknowledge the existence of anyone likely to raise objections.

Comments

1. **neil**
   December 19, 2018

   I do wonder who the PR offices think they are fooling. After all, the fact that string theory is a dead end has reached all the way down to the popular level on “The Big Bang” theory where Sheldon gives it up.

2. **Peter Woit**
   December 19, 2018

   neil,
   I think the change in the popular perception of string theory has a lot to do with the decisions by string theorists to engage in this sort of PR campaign. Note that the Princeton article is coming out of the “Office of the Dean for Research”, the office responsible for helping faculty get research funding from public or private sources.

3. **JC**
   December 21, 2018

   Neil,
   “I do wonder who the PR offices think they are fooling. After all, the fact that string theory is a dead end has reached all the way down to the popular level on “The Big Bang” theory where Sheldon gives it up.”
   You have to give it to them, they´re fighting back in season 11 where Sheldon picks it up again.
   People who love Physics should start writing to TV producers, of this series and others, in order to stop misleading the public.
   We can´t leave Peter and others do all the work by themselves.

The Chronic Incompleteness of String Theory

December 19, 2018
Categories: Uncategorized

Ex-string theorist turned philosopher Richard Dawid has become known over the years for his arguments that string theory is a theory to be evaluated not by the conventional scientific method, in which experiment plays a role, but by "post-empirical theory assessment" methods. He has a book about this, and I've written about his arguments here, here and here.

Today he has a new paper out, entitled Chronic Incompleteness, Final Theory Claims, and the Lack of Free Parameters in String theory, which tries to address the "chronic incompleteness" problem of string theory's claim to be a complete unified theory. This problem is starting to look very serious:

> Rather than bringing the time horizon for the completion of fundamental physics from virtual infinity to somewhere within our lifetime, string theory's final theory claim seems to be associated with an extension of the time horizon for the completion of this particular theory that may, once again, virtually reach towards infinity.

In other words, there's a chronic problem of string theorists not being able to tell us what the theory actually is, and now it's looking like they'll never be able to do so. Dawid is right that this is the root of the problem, not usual excuses like "it predicts stuff, but you'd need an accelerator as big as the galaxy to test these predictions" or "the equations are just too hard to solve".

One question here is that of defining what "string theory" even means anymore. Dawid does the best he can with this in a footnote:

> Here as throughout the entire paper, the term 'string theory', if not specified otherwise, denotes the overall theory that aims at describing the observed world and is identified by the present knowledge on perturbative superstring theory, duality relations, etc.

This isn't exactly a precise definition, it's basically just "string theory is a conjectural theory with a certain list of properties which I'm not going to even try and describe, since that would get really complicated and different people likely have different lists".

In the main text, Dawid explains that "string theory has no fundamental dimensionless free parameters", a claim often made that I've always found kind of baffling. If you don't know what the theory is, how do you know that it doesn't have free parameters??? He makes a great deal of this assumption, adding in the argument that this lack of parameters means no classical limit, and I guess thus no formulation of the theory as "quantization" of something describable in classical terms.

I don't really see what the big deal is about having a quantum system that is not defined as the quantization of some classical system. A simple example of such a system is the qubit that we often start teaching quantum mechanics with. Somehow Dawid wants to get from "not quantization of a classical system" to "we can't ever hope to write down the theory", but I don't see how this follows.

He examines various possibilities for how the problem of no fundamental theory can be resolved. His alternative C is the obvious one: we just haven't found it yet. He would like to argue that this might not be right, that string theory is a new and different kind of science:

> …string theory and the conceptual context within which it is developed is in a number of ways substantially different from anything physicists have
> witnessed up to this point. Therefore, it is far from clear whether prevalent physical intuitions as to which kinds of questions can be expected to have a fully calculable theoretical answer are applicable in this case. It seems
> difficult to rule out that what seems to be a question that finds a fully calculable theoretical answer in fact rather resembles the case of the leaf carried by autumn winds and just defies calculation.

Dawid seems to argue that string theory may be an example of what he calls alternative A:

> Even in principle, there exists no mathematical scheme that is empirically equivalent to string theory and generates quantitative results that specify the fundamental dynamics of the theory. In that case, the fundamental theory is conceptually incomplete by its very nature. It has no fundamental dynamics and no set of solutions that can be deduced from its first principles. The fundamental theory merely serves as a conceptual shell that embeds low energy descriptions (ground states of the theory) consistent with the principles encoded in the fundamental theory. Those low energy descriptions contain specified parameter values and do generate quantitative results. But there is no way to establish from first principles how probable specific ground states of the system are.

His summary of his vision of string theory is as follows:

> Full access to a theory without free parameters thus might be expected to require representations that don't have their own classical limit. The fact that they cannot be developed by generalizing away from a classical limit seems to impede the full formulation of a final theory even once one has found it. The resulting idea of a fundamental theory whose full formulation is hidden from the physicists' grasp because its most adequate representation lacks intuitive roots has even more radical rivals, which amount to questioning the possibility of calculating the dynamics of the fundamental theory either within the bounds of human calculational power or as a matter of principle.

At one point Dawid acknowledges that some people have drawn the obvious conclusion about the current situation, the one consistent with our usual understanding of science:

> It has been suggested by various exponents and observers of contemporary fundamental physics (see e.g. Smolin 2003, Woit 2003, Hossenfelder 2018) that the chronic incompleteness of string theory represents a substantial failure of the research program that is indicative of a strategical problem that has afflicted fundamental physics in recent decades.

He doesn't like this conclusion, so argues that this time it's different:

> Considering the range and character of the very substantial differences that set the current state of fundamental physics apart from any previous stage in the history of physics, there is little reason to expect that theory building at the present stage can be judged according to criteria that seemed adequate in the past.

While he doesn't say so, this argument takes him back to the problem of how one is to judge "string theory", but taking a position even more radical than his earlier one. The argument now seems to be that we're supposed to consider accepting as the final, fundamental theory of physics, a "theory" that is not just untestable, but is a "chronically incomplete" framework based on something we can never hope to define or understand. I'm having trouble understanding why this is supposed to be science rather than another human endeavor that it looks a lot more like, theology.

Comments

1. **Lee Smolin**
   December 19, 2018

Dear Peter,

Does Dawid not consider the context, which is that "string theory"-defined as you like, is one of several different theories, hypotheses or research programs to complete the standard model and quantize gravity and that in the end it will be evaluated by contrasting it with alternative or rival theories? This is a basic theme of philosophy of science back to Kuhn, Lakatos, and Feyerabend.

What if an experiment confirms the hypothesis of Diosi, Penrose and others, that quantum dynamics becomes non-linear in order to disallow superpositions of macroscopically distinguishable gravitational fields? This begins to appear to be possible within a decade. To my knowledge no version of string theory predicts this, hence string theory might be simply disconfirmed, via the standard methodology of science.

2. **anon**
December 20, 2018

"I don't really see what the big deal is about having a quantum system that is not defined as the quantization of some classical system. A simple example of such a system is the qubit that we often start teaching quantum mechanics with."

A bit of a tangent, but according to Kirillov's orbit method, can I not regard a qubit as the quantization of the first nontrivial integral (co)adjoint orbit of SU(2)?

3. **Sabine Hossenfelder**
December 20, 2018

"…string theory and the conceptual context within which it is developed is in a number of ways substantially different from anything physicists have witnessed up to this point."

Last time we heard from Dawid he argued that it counts in favor of string theory that the theory is *not* all that different from the physics we know up to this point. It's what he called the "Meta-Inductive Argument." This didn't work, of course, because if anything that argument would count in favor of asymptotically safe gravity. Must have been inconvenient.

4. **Roger**
December 20, 2018

I am puzzled by Dawid's frequent usage of "chronic incompleteness" without defining it. I see two possible definitions:

chronic incompleteness – physicists keep trying to develop it into a meaningful theory, and keep failing.

chronic incompleteness – it fails to make any predictions, even with complete initial data.

The first is like "chronic pain", and the second is just a fancy way of using time as an adjective.

5. **David Roberts**
December 20, 2018

> *Even in principle, there exists no mathematical scheme that is empirically equivalent to string theory and generates quantitative results that specify the fundamental dynamics of the theory.*

Giving up on Hilbert's 6th problem?

6. **Peter Woit**
December 20, 2018

Roger,
Maybe it's because it's end of semester here, but to me "chronic incompleteness" brings to mind some of my students. In their case though, they almost always end up handing in something for evaluation….

7. **Peter Woit**
December 20, 2018

anon,
Yes, but that kind of "quantization" isn't what Dawid is referring to, he's talking about systems with a "classical limit". In the case of spin, you could try and interpret spin s as corresponding to a sphere with area proportional to s, then take as "classical limit" s goes to infinity. Then you could try and understand the qubit as the opposite spin half limit of the "classical" system at infinite spin. Dawid is quite right that this is not going to work. The point though is that we have other ways of understanding quantum systems that don't start with a conventional classical system and our intuitions about it.

8. **Roger**
December 20, 2018

My dictionary defines chronic as having long duration, or recurring. Seeing "chronic incompleteness" in a semi-technical paper makes me think it means something technical, like "the initial value problem is ill-posed", or that string theory cannot make long duration predictions. Or maybe he is just hiding string theory's shortcomings in an opaque phrase.

9. **Amos**
December 21, 2018

Regarding this quote: "It has been suggested by various exponents and observers of contemporary fundamental physics (see e.g. Smolin 2003, Woit 2003, Hossenfelder 2018) that the chronic incompleteness of string theory represents a substantial failure of the research program…"

Are those publication dates correct? I thought the Smolin and Woit books are copyright 2006. But even in the References section of Dawid's paper, where he lists author, title and publisher, both books are listed as 2003.

As an aside, it always seems strange to me that the Woit and Smolin books are so routinely cited as if they were the "big bang" of published skepticism toward string theory, considering that (for example) Penrose 2004 contains a highly critical/skeptical survey of the string theory program, and even discusses the sociological aspects (prestige of Witten, etc). Not to mention the critiques of Feynman and Glashow already in 1988. I suppose if the Woit and Smolin books really are 2003 they would take precedence over Penrose, but not if they were published in 2006 (as Wikipedia says).

10. **Peter Woit**
December 21, 2018

Amos,
I don't know why Dawid has those dates. The actual publication date was 2006. However it is true that my book was basically finished in 2003, when it was under consideration for publication by Cambridge University Press. For the full story, see here
http://www.math.columbia.edu/~woit/wordpress/?p=245

Of course it's true that from the beginning many physicists were aware of and complaining about the problems with string theory. Most of the public such complaints I was aware of when writing in 2002 were mentioned with citations in the book.

11. **Lee Smolin**
December 21, 2018

Dear Amos,

My book "The trouble with Physics" was published in 2006. No version was available before that. It was preceded by a few papers which compared different approaches to quantum gravity on purely scientific criteria,(ie no sociology), such as

Lee Smolin, How far are we from the quantum theory of gravity?, hepth/0303185.

but I don't know if Dawid referred to these.

Putting Peter and my books in 2003 rather than 2006 is a serious historical mistake, as 2003-2006 was a period of intense debate about the applicability of the athropic principle and the multiverse. My book and its reception would have been very different had it come out in 2003.

Regarding Sabine's comment, I argued in my review of Dawid's book, that his three non-empirical criteria for assessing a theory were satisfied very well by loop quantum gravity, and I'd agree they are also met by asymptotic safety. This is not to make an argument for either of those theories, but to demonstrate that Dawid's criteria are ambiguous and are of no help when one is faced with several competing theories or research programs. That is of course the present situation.

"String Theory and the Scientific Method. Review of a book by Richard Dawid.", in the American Journal of Physics, 2014.

Thanks,

Lee

12. **Peter Woit**
December 21, 2018

Re Lee's correct comment about the history, in more detail the story of my book is that the 2003 version had nothing about the multiverse/anthropics, which was a topic that only got going in 2004. The version published in 2006 did have a chapter about this, but that chapter was written in 2004-5, after I had given up on Cambridge and found another publisher. I think Lee is quite right that the nature of the 2006 debate that both of our books triggered had a lot to do with the fact that it was taking place in the context of many string theorists signing on to the idea that string theory implied a multiverse/anthropic explanation for fundamental physics.

Tim May 1951-2018

December 24, 2018
Categories: Obituaries

I was sorry to learn yesterday of the death of Tim May, who had been a frequent commenter here on the blog. For more about his life, see here and here.

One can find his comments here for instance by this search. In some of these he told a bit of the story of his life. I'll include here part of one such comment:

> In 1970 I was accepted at MIT, Stanford, and Berkeley for college. I transferred my acceptance and Regents Scholarship from Berkeley to UC Santa Barbara. A lesser school compared to Berkeley, on overall grounds, but a more interesting fit to my interests. (College of Creative Studies, with many advantages.)
>
> By around 1972 it was clear the Big Drought was unfolding. Tales of Ph.D.s driving taxi cabs, professors advising that the odds of the then-current Ph.D. candidates getting a real position were dwindling. (Besides the overall downsizing of HEP and other physics funding, there was a glut of physics professors who had been hired in the post-Sputnik boom era….and they were still 30 years or more from retirement.)
>
> Fast forwarding, I decided to not apply to grad school and instead join a small semiconductor company. There, I worked on a bunch of "engineering physics" probems. Because we were the leaders in dynamic RAM memory, I had exposure to some interesting problems. One of them was the mysterious issue of bits sometimes being flipped, but not permanently. In fact, the bit flips were apparently random and occurred only once (or at least close to only once…).
>
> My physics background served me well, as I knew about the physics of how the devices worked (more so than a lot of the EE folks, who thought in terms of circuits), and I knew some geology. I had a brain storm that maybe low levels of uranium or thorium or the like in our ceramic and glass packages were causing the problem. Some experiments confirmed this. And all of the physics calculations about charged particle tracks in silicon matched. A lot of stuff I don't have the space here to describe.
>
> So my career was launched. Lots of papers on this "soft error" phenomenon. (Oh, and the cosmic ray corrollary was indeed obvious: but in 1978 when the first paper was presented, it was insignificant as a source as compared to alpha particles.)
>
> Instead of spending until 1980-82 doing a Ph.D. and then 4-8 years or more as a post-doc, I had some fun and retired from Intel in 1986.
>
> I've been pleasantly able to pursue whatever interested me ever since.

Comments

1. **Jeff Berkowitz**
   December 24, 2018

   Sigh. Wow. Sigh and wow. I'd never heard Tim May's name. Sigh.

   Peter, I'm old enough to remember when this discovery was made and the decision by that "small semiconductor company" to go public with the information rather than retaining it for proprietary advantage.

   There's a mention in http://www2.ece.rochester.edu/~xinli/usenix07/ And although it sounds funny today, Intel was pretty small in 1978. The whole industry was small. That was an important piece of work.

   "[T]he first evidence of soft errors at sea level due to radiation was given in 1978 by May and Woods from Intel [56, 57]."
   https://pdfs.semanticscholar.org/721a/093aba13979c674878c4076fa28dc8fcd0d0.pdf

   Here's the paper itself: https://ieeexplore.ieee.org/document/1479948

   I'm sad. Thank you for posting.

2. **Professor rat**
   December 25, 2018

   Peter
   I was on the cypherpunks list in the early noughties and, while tremendously inspired by his technical prowess and prophecies, I found his misuse of the term 'anarchist' odd.

   So ' Yes' to the technological determinism. ' No' to the Randite-Rothbard politics.

   There's some interesting threads in the archives (MARC) of cypherpunks for physics buffs. Just dig around in there.

This Week’s Hype (and a couple other things)

December 28, 2018
Categories: Swampland, This Week's Hype, Uncategorized

For today’s university press release designed to mislead the public with hype about string theory, Uppsala University has Our Universe: An expanding bubble in an extra dimension. It’s the Swampland variant of string theory hype, based on this preprint, which is now this PRL. Marketing to other press outlets as usual starts here and here.

In the current Swampland hype, string theorists have “discovered” that string theory doesn’t really necessarily have that landscape of vacua making it untestable, and now we’re finally on our way to testing string theory. In this press release version:

> For 15 years, there have been models in string theory that have been thought to give rise to dark energy. However, these have come in for increasingly harsh criticism, and several researchers are now asserting that none of the models proposed to date are workable….
>
> The Uppsala scientists’ model provides a new, different picture of the creation and future fate of the Universe, while it may also pave the way for methods of testing string theory.

For some other things that may be of more interest:

- FQXI will be giving out \$8.4 million in large grants in 2019. There are two themes. One of these is Intelligence in the Physical World, sponsored by the Fetzer Franklin Fund, the other is Information as Fuel, sponsored by the Templeton World Charity Foundation (which seems to be independent of the Templeton Foundation).
- There’s a new initiative at Princeton I hadn’t heard about until recently: the Princeton Gravity Initiative.
- It looks like the idea of constructing a lepton collider in Japan, the ILC, is now unlikely to happen. The problem with this project has always been that it would be both very expensive and not provide a huge energy increase over the energies achieved at LEP.

Finally, I’m leaving tomorrow for a two-week or so vacation in France, blogging likely slim to non-existent.

Comments

1. **Roger**
   December 29, 2018

   That the Science Council of Japan (SCJ) does not support the ILC can’t be interpreted as anything other than very bad news for the project. To place the news in context, the SCJ comprises non-particle physicists and pp has traditionally had difficulty convincing other fields of the merit of their expensive experiments (eg the SSC). In other words, the SCJ report is a disappointment but its difficult to argue that it was entirely unexpected. However, it need not be the end of the story. The Japanese government will now take a decision based, in part, on the SCJ report together with other considerations, eg , the effect on the local economy of the area which would host the ILC. Its possible that the project will still go ahead though this admittedly looks unlikely right now. Had new physics shown up at the LHC which the ILC could probe, which was how the ILC was originally spun all those years ago, the motivation for the ILC would have unarguably stronger and the story could have been different. However, we don’t live in that part of the multiverse…

   If the ILC fails then the tectonic plates in the collider world start to shift. The FCC-ee (new ring at CERN with e+e- up to 400 GeV cm prior to hh at 100 TeV) gets something of a boost since the ILC won’t be around to do much of its physics program before the new tunnel is even built. CLIC at CERN (e+e- up to several TeV) should also get a boost but seems to be the forgotten child. It may be terribly unfair on the project but one hears far more about the FCC these days. It will be interesting to see how the ILC’s demise (if this happens) affects the attitude in China towards building its own e+e- machine (CEPC).

2. **Andrew Krause**
   December 29, 2018

   Likely you are aware of this blog but I found this recent post interesting (as a mathematician in a totally different world, so purely an onlooker).

   https://lifeandphysics.com/2018/12/29/theory-experiment-and-supersymmetry/

Roy Glauber 1925-2018: Notes on QFT

December 28, 2018
Categories: Obituaries

I saw today that Roy Glauber has passed away, at the age of 93. John Preskill speculates that Glauber was the last living member of the wartime T division at Los Alamos.

My only interaction with him was that he was the instructor for the first quantum field theory course I took, at Harvard during the 1976-77 academic year. The course was my first exposure to quantum field theory, and was taught from what seemed then (at the time of the advent of gauge theories and wide use of the path integral method) a rather stodgy point of view. It's one however that I have later in life come to appreciate more.

I just located the binder of notes I kept from the class and plan to look over them. It occurred to me that if I want to look at these on vacation, the thing to do is to scan them. So, I just did this, and am making the scans available here in case others are interested:

Roy Glauber: Quantum Field Theory notes 1976-77

Roy Glauber: Quantum Field Theory problems and solutions 1976-77

Update: The New York Times has an obituary here.

Comments

1. **paddy**
   December 28, 2018

   RIP.

2. **Mark Hillery**
   December 28, 2018

   I am very sorry to hear this. As someone who has worked in quantum optics, and first encountered it by reading some of Roy's papers, I can say that his founding contributions were something that all of us learned, and they formed the framework for much of our thinking about the field. His papers were masterpieces of originality and clarity. Lesser known to those outside the field was his talent as an after-dinner speaker at conferences. He had the comic timing and manner of Jack Benny (OK, I am dating myself here). He will be very much missed.

3. **Si MohOumhand**
   December 29, 2018

   Dear Peter,

   Thank you for posting the notes. They are very neat. I read in your book "Not Even Wrong" that at some point you asked Witten to explain to you things in quantum field theory. Was it about these notes. If yes, do you remember which chapter?
   Thanks.

4. **Peter Woit**
   December 29, 2018

   Si MohOumhand,

   That was a year or two later, when for a semester I had an independent reading course with Witten (who was a newly arrived postdoc). Witten helped me go through basics of gauge theories, doing perturbative calculations and renormalization (his Ph.D. thesis at Princeton had been on perturbative QCD calculations).

   After Glauber's course, the next year I sat in on Coleman's QFT class. A published version of Coleman's class notes is supposed to appear soon, which I'm looking forward to. I think my last year at Harvard I sat in on or took a more advanced graduate course from Weinberg on gauge theories (something like volume II of his QFT book). Looking back on those days, I had the great luck to get to start learning the subject from the very best.

5. **Vhman**
   December 29, 2018

   Coleman's notes are already on Amazon.

6. **Peter Woit**
   December 29, 2018

   Vhman,
   Yes, but for delivery in a couple weeks. I hear they're being printed and shipped around now, so should be available starting in a week or two. I'll write something about them when I get back from vacation, hope to have a copy by then.

7. **David Appell**
   December 29, 2018

   Nice and thoughtful jobs with the notes, Peter. Thanks.

8. **DRLunsford**
   December 31, 2018

   I thought his papers were very clearly written. RIP

   -drl

9. **David Derbes**

January 1, 2019

I'm one of the editors (Brian Hill, Bryan Gin-ge Chen, Yuan-Sen Ting, Richard Sohn and David Griffiths are the others, names by chronological involvement) of the Coleman QFT notes. Peter was of huge help in getting the Coleman notes into publishable form. Early on I corresponded with him about the project and by return email came scans of his own notes of Coleman's class. These were invaluable, particularly for the second semester. (The bulk of the text is from transcriptions of the videotapes of Coleman's lectures, on line at Harvard's physics web site, but we couldn't have done it without other records of the lectures.) You can get the kindle edition today but I think the physical books won't be at Amazon till January 9th or thereabouts (but they exist; World Scientific air expressed a copy to me on November 12). I'm nonplussed by various "used" copies for sale; I have my doubts about the legitimacy of these. You can get an earlier version of the first semester notes (originally by Brian Hill; LaTeX'd by Yuan-Sen Ting and Bryan Gin-ge Chen) today for free at the arXiv. The book is about 1200 pages, including a very nice foreword by David Kaiser (who also did one for the reissue of Misner, Thorne and Wheeler), and about 72 problems and solutions (some are old exam questions). We tried to do a good job, but the readers will be the judge of that. Incidentally, all author royalties are going to Diana Coleman, Sidney's widow; we all worked for free. It took five years.

10. **Pascal**
January 15, 2019

Another aspect of Glauber's work is highlighted here:
http://bit-player.org/2019/glaubers-dynamics

11. **Anonyrat**
January 16, 2019

The store person at Barnes and Noble said that the Coleman QFT book is a "print on demand" book and so I'd best order it online.

12. **Peter Woit**
January 16, 2019

Anonyrat,
As far as I know copies are being printed in the usual way by World Scientific, which is selling them on their website. I'd be very wary of any "printed on demand" version. People should be aware that there's an industry devoted to grabbing pdfs from various places and marketing them as printed on demand books.

13. **Mark**
January 17, 2019

I just received my beautiful hardcover version (World Scientific) from Amazon UK (which I preordered quite a while ago) – It is a very heavy book and beautifully printed, and I am glad that I got the more expensive hardcover version – Amazon was nice and did refund the price increase from the time I ordered.

Michael Atiyah 1929-2019

January 16, 2019
Categories: Obituaries

While away on vacation, I heard last week the sad news of the death last week of Michael Atiyah, at the age of 89. Atiyah was both a truly great mathematician and a wonderful human being. In his mathematical work he simultaneously covered a wide range of different fields, often making deep connections between them and providing continual new evidence of the unity of mathematics. This unifying vision also encompassed physics, and the entire field of topological quantum field theory was one result.

I had the great luck to be at MSRI during the 1988-89 academic year, when Atiyah spent that January there. Getting a chance to talk to him then was a remarkable experience. He had one of the quickest minds I've ever seen, often grasping what you were trying to explain before the words were out of your mouth. At one point that month I ran into Raoul Bott walking away from an ongoing discussion with Atiyah and Witten at a blackboard. Bott shook his head, saying something like "it's just too scary listening to the two of them".

Any question, smart or stupid, would lead to not just an answer, but a fascinating explanation of all sorts of related issues and conjectures. For Atiyah, his love of discussing mathematics was something to be shared at all times, with whoever happened to be around.

The last time I met him was in September 2016 in Heidelberg. He was his usual cheerful and engaging self, still in love with mathematics and with discussing it with anyone who would listen. I did notice though that age had taken its toll, in the sense that he no longer would engage with anything that got into the sort of complexities that in the past he had been quick to see his way through. It's unfortunate that near the end of his life far too much attention was drawn to implausible claims he started making that he could see how to solve some of the most difficult and intractable open problems of the subject.

There's a lot more I could write here about Atiyah and his remarkable career, but I've realized that most of it I've already gotten to in one post here or another. So, for more, see some of the following older posts, which discussed:

Interviews and profiles here, here and here.

Atiyah and his work with Raoul Bott.

Atiyah and topological quantum field theory.

Update: In recent years Andrew Ranicki had been maintaining a page with Atiyah-related links.

Comments

1. **T**
   January 16, 2019

   In a previous blog posting you wrote "Atiyah favored "new-fangled" abstraction, only writing down formula when forced to by Bott". As an analyst (who finds themselves having to write down each step of a proof) I find this amazing. Do you have any more details as to how he was able to do this?

2. **Peter Woit**
   January 16, 2019

   T,
   I'm paraphrasing Atiyah himself there. My interpretation of what he was saying is that it's not about the details of a proof, just that he was happy working more abstractly with arbitrary solutions (e.g. in the index theorem, where you can show that there are solutions, without having them explicitly), where Bott wanted to work out what happened for explicit solutions in order to get more insight into what was going on (and ultimately convinced Atiyah he needed to do this).

   Actually I think analysis was the field where both Atiyah and Bott were weakest, and the papers he is discussing in that quote were written with an analyst (Garding). Atiyah characterizes their role in the paper as a "subcontract", presumably meaning they were handling just certain more topological and geometrical issues.

3. **DRLunsford**
   January 18, 2019

   He was amazing. A long and useful life!

   -drl

4. **cedric bardot**
   January 18, 2019

   Mathilde Marcolli collaborated with Michael Atiyah and was a friend of him up to the end of his life. She paints a moving portrait of him and engages the mathematical community with her refined "aqua fortis" way at https://listeningtogolem.blogspot.com/2019/01/the-polar-star-and-life-endgame-elegy.html.

   Blessed are you Sir for showing us there is more than one way for geometry and physics to interact with each other!

5. **DaveMiller**
   January 20, 2019

   Sad.

   One of my goals in life is to understand *why* the index theorem is true (yes, I know the various proofs, but I want to see why it should be obvious).

   It seems that he was one of the good guys, and it is nice to know the good guys do often get the recognition they deserve.

   Dave Miller in Sacramento

6. **alain connes**
   January 29, 2019

I have now put on my website a short note on an idea of Michael Atiyah.
http://www.alainconnes.org/docs/watertoyourmill1.pdf

Should the Europeans Give Up?

January 22, 2019
Categories: Experimental HEP News, Favorite Old Posts

The European HEP community is now engaged in a "Strategy Update" process, the next step of which will be an open symposium this May in Granada. Submissions to the process were due last month, and I assume that what was received will be made publicly available at some point. This is supposed to ultimately lead to the drafting of a new European HEP strategy next January, for approval by the CERN Council in May 2020.

The context of these discussions is that European HEP is approaching a very significant crossroads, and decisions about the future will soon need to be made. The LHC will be upgraded in coming years to a higher luminosity, ultimately rebranded as the HL-LHC, to start operating in 2026. After 10-15 years of operation in this higher-luminosity mode, the LHC will reach the end of its useful life: the marginal extra data accumulated each year will stop being worth the cost of running the machine.

Planning for the LHC project began back in the 1980s, and construction was approved in 1994. The first physics run was 16 years later, in 2010. Keep in mind that the LHC project started with a tunnel and a lot of infrastructure already built, since the LEP tunnel was being reused. If CERN decides it wants to build a next generation collider, this could easily take 20 years to build, so if one wants it to be ready when the LHC shuts down, one should have started already.

Some of the strategy discussion will be about experiments that don't require the highest possible collision energies (the "energy frontier"), for instance those that study neutrinos. Among possibilities for a new energy frontier collider, the main ones that I'm aware of are the following, together with some of their advantages and drawbacks:

- FCC-ee: This would be an electron-positron machine built in a new 100 km tunnel, operating at CM energies from 90 to 365 GeV. It would provide extremely high numbers of events when operated at the Z-peak, and could also be operated as a "Higgs factory", providing a very large number of Higgs events to study, in a much cleaner environment than that provided by a proton-proton collider like the LHC.

  In terms of drawbacks, it is estimated to cost \$10 billion or so. The CM energy is quite a bit less than that of the LHC, so it seems unlikely that there are new unknown states that it could study, since these would have been expected to show up by now at the LHC (or at LEP, which operated at 209 GeV at the end).

  Another point in favor of the FCC-ee proposal is that it would allow for reuse of the tunnel (just as the LHC followed on LEP) for a very high energy proton-proton collider, called the FCC-hh, which would operate at a CM energy of 100 TeV. This would be a very expensive project, estimated to cost \$17 billion (on top of the previous \$10 billion cost of the FCC-ee).

- HE-LHC: This would essentially be a higher energy version of the LHC, in the same tunnel, built using higher field (16 T vs. 8.33 T) magnets. It would operate at a CM energy of 27 TeV. The drawbacks are that, while construction would be challenging (there are not yet appropriate 16 T magnets), only a modest (27 vs. 14 TeV) increase in CM energy would be achieved. The big advantage over the FCC-hh is cost: much of the LHC infrastructure could be reused and the machine is smaller, so the total cost estimate is about \$7 billion.
- CLIC: This would be a linear electron-positron collider with first stage of the project an 11 km-long machine that would operate at 380 GeV CM energy and cost about ~~\$7~~ \$6 billion. The advantage of this machine over the circular FCC-ee is that it could ultimately be extended to a longer 50 km machine operating at 3 TeV CM energy (at a much higher cost). The disadvantage with respect to the FCC-ee is that it is not capable of operating at very high luminosity at lower energies (at the Z-peak or as a Higgs factory).

For some context for the very high construction costs of these machines, the CERN budget is currently around \$1.2 billion/year. It seems likely that member states will be willing to keep funding CERN at this level in the future, but I have no idea what prospects if any there are for significantly increased contributions to pay for a new collider. A \$10 billion FCC-ee construction cost spread out over 20 years would be \$500 million/year. Can this somehow be accommodated within CERN's current budget profile? This seems difficult, but maybe not impossible. Where the additional \$17 billion for the FCC-hh might come from is hard to see.

If none of these three alternatives is affordable or deemed worth the cost, it looks like the only alternative for energy frontier physics is to do what the US has done: give up. The machines and their cost being considered here are similar in scale to the SSC project, which would have been a 40 TeV CM energy 87 km proton-proton collider but was cancelled in 1993. Note that the capabilities of the SSC would have been roughly comparable to the HE-LHC (it had higher energy, lower luminosity). Since it would have started physics around 2000, and an HE-LHC might be possible in 2040, one could say that the SSC cancellation set back the field at least 40 years. The worst part of the SSC cancellation was that the project was underway and there was no fallback plan. It's hard to overemphasize how disastrous this was for US HEP physics. Whatever the Europeans do, they need to be sure that they don't end up with this kind of failure.

Faced with a difficult choice like this, there's a temptation to want to avoid it, to believe that surely new technology will provide some more attractive alternative. In this case though, one is running up against basic physical limits. For circular electron-positron machines, synchrotron radiation losses go as the fourth power of the energy, whereas for linear machines one has to put a lot of power in since one is accelerating then dumping the beam, not storing it. For proton-proton machines, CM energy is limited by the strength of the dipole magnets one can build at a reasonable cost and operate reliably in a challenging environment. Sure, someday we may have appropriate cheap 60T magnets and a 100 TeV pp collider could be built at reasonable cost in the LHC tunnel. We might also have plasma wakefield technology that could accelerate beams of electrons and positrons to multi-TeV energies over a reasonable distance, with a reasonable luminosity. At this point though, I'm willing to bet that in both cases we're talking about 22nd century technology unlikely to happen to fall into the 21st century. Similar comments apply to prospects for a muon collider.

Another way to avoid the implications of this difficult choice is to convince oneself that cheaper experiments at low energy, or maybe astrophysical observations, can replace energy frontier colliders. Maybe one can get the same information about what is happening at the 1-10 TeV scale by looking at indirect effects at low energy. Unfortunately, I don't think that's very likely. There are things we don't understand about particle physics that can be studied using lower energies (especially the neutrino sector) and such experiments should be pursued aggressively. It may be true that what we can learn this way can replace what we could learn with an energy-frontier collider, but that may very well just be wishful thinking.

So, what to do? Give up, or start trying to find the money for a very long-term, very challenging project, one with an uncertain outcome? Unlike the case of the LHC, we have no good theoretical reason to believe that we will discover a new piece of fundamental physics using one of these machines. You can read competing arguments from Sabine Hossenfelder (here and here) and Tommaso Dorigo (here, here and here).

Personally, I'm on the side of not giving up on energy frontier colliders at this point, but I don't think the question is an easy one (unlike the question of building the LHC, which was an easy choice). One piece of advice though is that experience of the past few decades shows you probably shouldn't listen to theorists. A consensus is now developing that HEP theory is in "crisis", see for instance this recent article, where Neil Turok says "I'm busy trying to persuade my colleagues here to disregard the last 30 years. We have to retrace our steps and figure out where we went wrong." If the Europeans do decide to build a next generation machine, selling the idea to the public is not going to be made easier by some of the nonsense from theorists used to sell the LHC. People are going to be asking "what about those black holes the LHC was supposed to produce?" and we're going to have to tell them that that was a load of BS, but that this time we're serious. This is not going to be easy…

Update: Some HEP experimentalists are justifiably outraged at some of the negative media stories coming out that extensively quote theorists mainly interested in quantum gravity. There are eloquent Twitter threads by James Beacham and Salvatore Rappoccio, responding to this Vox story. The Vox story quotes no experimentalists, instead quotes extensively three theorists working on quantum gravity (Jared Kaplan, Sabine Hossenfelder and Sean Carroll). Not to pick specifically on Kaplan, but he's a good example of the point I was making above about listening to theorists. Ten years ago his work was being advertised with:

> As an example question, which the LHC will almost certainly answer—we know that the sun contains roughly 10^60 atoms, and that this gigantic number is a result of the extreme weakness of gravity relative to the other forces—so why is gravity so weak?

Enthusiasm for the LHC then based on the idea that it was going to tell us about gravity was always absurd, and a corresponding lack of enthusiasm for a new collider based on negative LHC results on that front is just as absurd.

Update: Commenter abby yorker points to this new opinion piece at the New York Times, from Sabine Hossenfelder. The subtitle of the piece is "Ten years in, the Large Hadron Collider has failed to deliver the exciting discoveries that scientists promised." This is true enough, but by not specifying the nature of the failure and which scientists were responsible, it comes off as blaming the wrong people, the experimentalists. Worse, it uses this failure to argue against further funding not of failed theory, but of successful experiment.

The LHC machine and the large-scale experiments conducted there have not in any sense been a failure, quite the opposite. The machine has worked very well, at much higher than design luminosity, close to design energy (which should be achieved after the current shutdown). The experiments have been a huge success on two fronts. In one direction, they've discovered the Higgs and started detailed measurements of its properties, in another they've done an amazing job of providing strong limits on a wide range of attempted extensions of the standard model.

These hard-won null results are not a failure of the experimental program, but a great success of it. The only failure here is that of the theorists who came up with bad theory and ran a hugely successful hype campaign for it. I don't see how the lesson from seeing an experimental program successfully shoot down bad theory is that we should stop funding further such experiments. I also don't see how finding out that theorists were wrong in their predictions of new phenomena at the few hundred GeV scale means that new predictions by (often the same) theorists of no new phenomena at the multiple TeV scale should be used as a reason not to fund experimentalists who want to see if this is true.

Where I think Hossenfelder is right is that too many particle physicists of all kinds went along with the hype campaign for bad theory in order to get people excited about the LHC. Going on about extra dimensions and black holes at the LHC was damaging to the understanding of what this science is really about, and completely unnecessary since there was plenty of real science to generate excitement. The discussion of post-LHC experimental projects should avoid the temptation to enter again into hype-driven nonsense. On the other hand, the discussion of what to defund because of the LHC results should stick to defunding bad theory, not the experiments that refute it.

Update: Some more commentary about this, from Chris Quigg, and the CERN Courier. In particular, the CERN Courier has this from Gerard 't Hooft:

> Most theoreticians were hoping that the LHC might open up a new domain of our science, and this does not seem to be happening. I am just not sure whether things will be any different for a 100 km machine. It would be a shame to give up, but the question of whether spectacular new physical phenomena will be opened up and whether this outweighs the costs, I cannot answer. On the other hand, for us theoretical physicists the new machines will be important even if we can't impress the public with their results.

and, from Joseph Incandela:

> While such machines are not guaranteed to yield definitive evidence for new physics, they would nevertheless allow us to largely complete our exploration of the weak scale… This is important because it is the scale where our observable universe resides, where we live, and it should be fully charted before the energy frontier is shut down. Completing our study of the weak scale would cap a short but extraordinary 150 year-long period of profound experimental and theoretical discoveries that would stand for millennia among mankind's greatest achievements.

Update: Also, commentary at Forbes from Chad Orzel here.

Update: I normally try and not engage with Facebook, and encourage others to follow the same policy, but there's an extensive discussion of this topic at this public Facebook posting by Daniel Harlow.

Comments

1. **more to it than theory**
   January 22, 2019

   While discussions about plasma wakefield technology are fun to be had. It's also important to remember that particle physics colliders represent the forefront of accelerator technologies. New SRF technology and magnet development are only done at a few places in the world. These potential experiments aren't only interesting from the point of view of fundamental physics. They represent the technological bleeding edge, and knowledge that will be lost if we as humans don't continue to pursue it. While it can be viewed as 22nd century technology, it's not clear that it will ever arise if it isn't invested in. Given how prior magnet/accelerator technology has ended up so beneficial to society (the vast majority of accelerators in the world aren't for particle physics), it's important to think about the technological advances not just the fundamental physics. While the standard world wide web is trotted out for CERN, it's important to remember that a lot more than the web has come from particle physics. New technology as I recall, was one of the original driving interests for the CEPC and SPPC proposals in China, not just the prestige of fundamental physics.

2. **From a Former Professional Higgs Boson Hunter....**
   January 22, 2019

   The only black holes that the LHC revealed were the careers of the non-hardware post-docs…

3. **Scott P.**
   January 23, 2019

   Given that the LHC has failed to find any particles other than the Higgs, and there is nothing in the Standard Model that suggests there will be anything to find within the capabilities of colliders in the forseeable future, what is the justification for another larger collider? If there were supersymmetric particles we ought to have seen some of them at the LHC.

4. **katzeee**
   January 23, 2019

   Even if a FCC will eventually be built, will this not only shift this same discussion to 30-50 years later, when it must be decided what to do next? Built a ~100-billion-500-TeV-Collider (with appropriate e-p-mode before proton-run)?

   I am all in for spending as much money for science as one can, but the sensible (a.k.a. politically managable) decision might be, to increase incrementally and choose HE-LHC and see what magnet-technology, colliding muons etc. can do for us in the future and keep a running Collider.

5. **Sabine Hossenfelder**
   January 23, 2019

Hi Peter,

A great summary and thanks for the links. A note about the SSC/LHC comparison though. As I am sure you know the LHC is pushing hard on the computing frontier too. They have huge amounts of data to cope with and must do so quickly to decide what to keep and what to toss. The SSC would have reached an energy high enough to produce the Higgs but the data analysis would have been nowhere near as good as what we can do today.

I am not aware of any actual estimate for what this would have meant in terms of physics insights, but I think it's something one should keep in mind when one compares then to now. Best,

Sabine

6. **Dave Miller**
January 23, 2019

Peter,

Having done my thesis on the tau lepton and its neutrino (decades ago!), I'm intrigued to know more about the neutrino sector: what is the source of their mass and why is it so small but non-zero? Does anyone know if any of these proposals will give deep information about the neutrino sector, even good values for the neutrino mass matrix?

I also am tempted to think that a Higgs factory where we can measure the Higgs' behavior to high precision could be revealing: does anyone know if there is any hope of new physics there, or is it likely that we will merely find out that, yes, the Higgs does indeed give other particles their masses?

Your closing paragraph and the comment from Turok is key: what is the selling point to sell this to responsible politicians or intelligent members of the general public?

Dave

7. **Bunsen Burner**
January 23, 2019

A question no one ever seems to consider is what happens in the future if we don't keep on building accelerators and training a generation of physicists in the use? These are complicated entities that require the input of a vast army of experts. Once these experts grow old and die, if there is no one to replace them, where will the knowledge and experience come from to build accelerators in the future?

8. **Marco**
January 23, 2019

Hi Peter,
I think you have a bit outdated cost numbers (today 1.00 USD = 1.00 CHF):

– CLIC cost: 5.9 billion USD for the 380 GeV Stage
– FCC-ee cost: 11.6 billion USD (5.4 tunnel + 5.1 injetors +1.1 RF)

Updating CLIC to 1.5 TeV would cost an additional 5.1 billion USD and after that to 3 TeV an additional 7.3 billion. So for approximately the same amount you can get 1.5 TeV CLIC or 365 GeV FCC-ee

9. **Peter Woit**
January 23, 2019

Marco,
All the numbers I used came from the documents (Yellow Reports) issued last month. In particular, for CLIC, I used
https://arxiv.org/abs/1812.06018
which has on page 68 (for 380 GeV, in millions of dollars or swiss francs)
5890 +1470/-1270 (drive-beam based)
7290 +1800/-1540 (klystron based)
The reason for giving 7 billion as a rough number was partly that when someone gives you two numbers for the price of something, in my experience it's always going to cost the higher number…

In any case, these are all rough numbers. The only one of them that surprised me was the HE-LHC cost estimate, which seemed much higher than I would have expected (since I would have thought you could reuse most of the LHC infrastructure, mainly needed only to pay for the new magnets).

10. **Marco**
January 23, 2019

Hi Peter,
I understand, however, the 7 billion number is not the baseline concept, but a concept that uses klystrons instead of the drive-beam. This is not a caveat to the original machine, but a completely different machine meant to show that the drive-beam acceleration is more cost effective then klystrons (page 69 in the same document). In your writing the cost difference between CLIC and FCCee is 3B instead of 6B, which I think is a huge difference.

As for the HE-LHC cost estimate, this is not an actual cost estimate, it's more a target cost. The majority of the cost comes from the magnets. The R&D for 16T magnets is far from finished, and the industrialization of such devices has not even begun, subsequently any price tag for this kind of magnets bears very large error bars.

11. **Peter Woit**
January 23, 2019

Scott P.,
I recommend Tommaso Dorigo's blog posts that I linked to for his arguments about this.

The relevance here of my "don't listen to theorists" advice is that, while it may be true that theorists have no good motivation for any new physics beyond the SM, until one gets to the Planck scale, that's no better argument than the "naturalness" arguments made pre-LHC that said you had to see BSM physics below the TeV scale.

The difficult question here is not whether it's worth investigating smaller distance scales despite lack of motivation from the theorists. It's that the technology limits make the cost numbers vs. the energy reach not obviously attractive. If the HE-LHC could be done for \$ 1 billion I think the project would already be underway. If \$25 billion would buy a 1000 TeV collider, there would be a lot of enthusiasm to try and do that, no matter what theorists were saying.

12. **JC**
January 23, 2019

In the current global economic situation, there´s a slim chance CERN member states will be able to shoulder the cost of a significantly more powerful machine.
On thing CERN has proven is that different nations can and will cooperate in the advancement of science.
Since the whole world will benefit from the data obtained from a big cruncher, it´s time for CERN to invite other European and non-European nations to invest in the next one and accept physicists and engineers in the design, operation and analysis of whatever come out of it.
The time has come for CERN, the US, Russia, Japan and maybe even China to unite.

13. **Peter Woit**
January 23, 2019

Marco,
Thanks for the clarification, I hadn't understood that point.

The number I used for the FCC-ee was based on the \$ 10.5 billion total construction cost estimate given in the executive summary (page xxvi) of the CDR.

14. **abby yorker**
January 23, 2019

Sabine Hossenfelder wrote this for the NYT:
https://www.nytimes.com/2019/01/23/opinion/particle-physics-large-hadron-collider.html

A lot of interesting points including: do we need theorists to predict discoveries and then go after them or do we take a technique that has produced many unexpected discoveries in the past and scale it up until we can't afford it any more? I would personally vote for the latter as it seems to work. But we're getting close to the cost limit.

15. **Bernhard**
January 23, 2019

We need new ideas on how to improve acceleration technology cost-effectively. Grants focused on supporting talented young people to pursue this line could help.

16. **Peter Woit**
January 23, 2019

abby yorker,

Thanks. I'll add something about this as an update to this posting. My initial reaction after a quick read is that the piece comes off as blaming experimentalists for the behavior of theorists.

17. **Amitabh Lath**
January 23, 2019

Thank you Peter for pointing out that null results are not a failure. In fact some of the LHC searches have been breathtakingly clever and beautiful. Unfortunately articles like Sabine's in the NYT do give the impression that it's time to pack up our bags (and do what exactly?) even if that's not what she meant.

The FCC and the like will be built and operated by scientists and engineers who are kids right now; undergrads, high schoolers. They are reading Sabine's views, and I fear that they will take her glum outlook to heart.

18. **LDK**
January 24, 2019

Peter,
your "update" is really great at summarizing the core problems in this discussion. I'm pleased to see that although you are also critical against a certain way to do physics (hype, untestability, strings,..) your vision here remains very clear.
I cannot say the same for Sabine, although most of her stronger statements look milder if you read (really) carefully her replies on her blog. I think that dramatizing this discussion as she does on public media is just a harm for science (and not HEP only).

19. **LDK**
January 24, 2019

BTW, the "Europeans" expression needs an explanation (not for you Peter of course):
for me "Europeans" means CERN-based experiments but LHC is really a global effort and even so would be FCC. The same for a China-based collider. Europe by itself would not be able to put together enough money and manpower for such large endeavors.
So "should Europeans give up" should be read as "should WE.." or "should CERN…" .
USA might be over with SSC, but DUNE is still a very large scale US-based international project and their deep involvement in the LHC is beyond any doubt.
P.S. I'm not an US citizen and it is not my intention to "defend" US, CERN, the FCC project or whatsoever organization, even if I'm a particle physicist.

20. **Davide Castelvecchi**
January 24, 2019

Nature has an editorial this week on the controversy:
https://www.nature.com/articles/d41586-019-00234-6

21. **Anon**
January 24, 2019

Dear Peter,

I am not sure it is a good idea to label falsifiable theories as "bad theories" after the experiments falsified them. For example TeV scale SUSY that solves the hierarchy problem made predictions that were sought out by LEP and LHC and has been falsified.

I would disagree with your observation that theorists "came up with bad theory." They solved an important problem that had predictions for experiments. The null results from LHC are baffling and probably tell us something deep about nature which we are still trying to unravel. That SUSY and other TeV scale physics solutions to hierarchy problem have been falsified is important by itself.

I do agree that the hype should not have been there. I do think that going for 100 TeV collider without any real theoretical motivation for that scale is a good idea, as the only reason not to find anything is Occam's Razor and this can also be tested in a sense.

22. **Low Math, Meekly Interacting**
January 24, 2019

First off: I am 100% behind the idea of my taxpayer money going to fund the FCC-ee or whatever the next generation ring winds up being. Simply because the energy frontier is there. I have no problem whatsoever with someone spending the money needed just to have a look. I can think of about 20-odd ways my country wastes more money every year in the present, and if I had my way I would happily lop those things off the budget and throw it all onto basic science. Something good always comes out of that kind of spending. It's just very hard to predict what what it will be. I think that's the most honest way to put forward the proposal. Unfortunately the most noble and truthful arguments for the SSC were much the same, and they failed to persuade. So my perspective is obviously one shared by a powerless minority, especially these days.

Given that, I'm puzzled by the argument that concern over a lack of theoretical guidance is overblown, and that the importance of strong odds in favor of a new fundamental discovery was never that relevant. Ideally, no, but this is the real world we're talking about, not the ideal. These things sure seemed awfully salient when earlier projects were being debated publicly, at least in the USA. Even in Europe, can we say with confidence that if there were not a virtual guarantee that the mechanism of EWSB would be revealed by the LHC it would have been built anyway? That's news to me, if so.

23. **tommaso dorigo**
January 24, 2019

Dear Peter,

many thanks for quoting my posts on the matter. I concur with all that you wrote, especially appreciating the updates. In particular one of the things I dislike the most about Bee's posts is exactly what you pointed out – she blames "particle physicists" as if there was one single kind of them; and she does this intentionally, because by blurring the image she is able to spin the story the way she wants. Indeed, the ones responsible for most of the hype are theorists, and they are the ones whose theories dictated our agendas.
I will give one example – we spent a lot of time and energy investigating SUSY scenarios, which is excellent science… But there is a budget of personpower that we invest in different avenues, and e.g. B physics has been seriously lagging behind (some analyses still deal with 2012 data today) because of lack of perceived interest in what is in fact a quite lively area of research, which produced lots of surprises and interesting new signals (yes, new particles!). I guess what I am saying is that the scientific output of the LHC experiments has indeed been influenced – negatively – by that hype to some degree. So experimentalists are the victims in some sense 🙂
Cheers,
T.

24. **Peter Woit**
January 24, 2019

LDK,
Yes, ultimately whether any of these projects happens will depend on participation of other countries. In particular, the US has an HEP budget of around $1 billion/year, so some fraction of that could help make a new collider feasible. I have no idea what the Chinese situation is, whether the proposal to build something there is realistic, or whether they would help construct something located at CERN.

Also to keep in mind is this new gilded age: Jim Simons could just pay for the FCC-ee himself, likely just out of his income, not even have to touch principal (he supposedly has \$ 20 billion). Same for a bunch of others.

25. **Peter Woit**
January 24, 2019

Anon,
You can be falsifiable and still "bad theory". My theory that at FCC-ee luminosity, at the Z-peak you will open a portal to another universe and angels will fly out is quite testable and falsifiable. I think it would be a really bad idea though for CERN to use it to justify the cost of the FCC-ee.

For what's "bad theory" and what isn't, you really need to discuss specific examples. The problems with string theory, large scale extra dimensions, SUSY extensions of the SM have all been endlessly discussed on this blog and in my book, and for each one, what's "bad" is a long story. One that shouldn't be very controversial though is the extra dimensions: it was an extremely bad idea to try and sell the LHC based on those models. I haven't seen anyone try that with the new proposals.

26. **Urs Schreiber**
January 24, 2019

The NY-Times text is worth reading closely to see what is really going on here. Consider the alternatives that are being suggested after…

> There are also medium-scale experiments that tend to fall off the table because giant projects eat up money.

The first suggestion for these is then given as follows:

> One important medium-scale project is the interface between the quantum realm and gravity, which is now accessible to experimental testing.

From the comment from Dec 9 here, this must be referring to the author's proposal here to use micro-force scales as in arXiv:1602.07539 for measuring quantum corrections to the gravitational attraction of two masses.

Now the first quantum gravity correction to the Newtonian potential can and has been unambiguously computed, Donoghue 95, Section 9 (arXiv:gr-qc/9512024). Even at 1 fm distance, it is still 38 orders of magnitude smaller than the classical effect. Contrary to the claim in the NYT pieces, this is utterly out of reach.

The second suggestion then is given as follows:

> Another place where discoveries could be waiting is in the foundations of quantum mechanics. These could have major technological impacts.

So this is saying that experiments should be looking for violations of the rules of quantum physics.

These are just not a serious contributions to the debate.

27. **Peter Woit**
January 24, 2019

LMMI,
Sure, if there is good evidence that there will be some interesting and completely new phenomenon for a new machine to study, that's a serious point in favor of building it, and optimizing the design for that purpose. There's also though a good case for investigating a new energy scale, even without an assurance there will be something completely new there to study. It's a fact that the case for building any of these new machines is weaker than the case for building the LHC. But I think that's just a reflection of the fact that the case for the LHC was unusually strong. I suspect though that without the Higgs it would have gotten built anyway. The new proposals won't have a slam-dunk argument like the Higgs, but I think can be justified on their own merits. A big problem is the high costs: if they were an order of magnitude lower I think the decision would be easy.

28. **Julius**
January 24, 2019

What about spending the next decade or two simply improving technologies? Dyson once said that an experiment should be performed when it is affordable. Maybe we have to acknowledge the fact that we cannot run development and implementation concurrently. No big experiments -> no big expectations -> no crazy hypotheses. Maybe, by moving the starting date of a new accelerator sufficiently far in the future would motive to start seeking the truth rather than beauty.

29. **Peter Woit**
January 24, 2019

Urs,
I suspect you're not properly interpreting what the author has in mind in these cases, and the thing to do is go discuss it with her at her blog to clarify. I happen to disagree with her about some of these arguments, but attributing straw-man non-serious arguments to her isn't helpful.

In the particular cases you mention, I think in the first case a more relevant counter-argument is that the gravity experiments she mentions are table-top experiments and the issue of how to pay for them is completely independent of the issue of paying for a new collider, where the cost is inherently huge. As for "quantum foundations", as far as I can tell "quantum" is the buzzword of the day, with companies and private foundations throwing vast sums of money at anything "quantum", and governments redirecting large parts of their research funding in this direction. So, I'm skeptical that good ideas about quantum foundations are being held up because of meager funding, something that could be fixed by using proposed HEP funding. To the extent we're talking about theory funding in either case, it's peanuts. irrelevant to the collider funding issue.

30. **Jonathan Miller**
January 24, 2019

I think that costs are the main factor why there are people interested in this discussion. The other potential discovery machines are roughly an order of magnitude cheaper. Their potential for discovery is arguably stronger, the only weaknesses is that there is a smaller scientific community involved and (possibly related) there are fewer non-discovery related publications and citations expected. Many of these discovery machines will not be built, possibly especially if a new collider is pursued.

I would love for a new collider to be built, but not at the cost of LISA or Hyper-K (for example).

31. **Peter Woit**
January 24, 2019

Julius,
The problem is that the limiting factor for hadron colliders is the magnet strength, and progress in recent decades towards increasing the strength of these magnets has been very slow. If you decide not to build a machine like the HE-LHC or FCC-hh because 16T magnets are not good enough (they require a 100km machine to get to 100 TeV, and cost scales with length), and wait for, say 32T magnets to bring the cost down by a factor of 2, you're not talking about a delay of a decade or two. More likely, 50-100 years.

I think this is a really important point. If we decide we can't afford these current proposals, we're not talking about a temporary hiatus, we're talking about the end of this kind of science for the rest of our lives. Maybe forever, since 50-100 years from now, we'll perhaps all be cyborgs owned by Google, with the curiosity about how the world works subsystem no longer there.

And as for whether no experimental results to keep them honest will cause theorists to change their ways for the better, I don't see why that should happen, the opposite seems more likely. "Post-empirical science" has not been working out well so far.

32. **Peter Woit**
January 24, 2019

Jonathan Miller,
The problem is that there are no other discovery machines on the table that will do what these colliders will do, investigate the 1-10 TeV energy scale. Deciding to instead investigate other things because it's cheaper is fine and that may be where we are headed. I just think people need to not fool themselves: taking such a path is giving up and putting an end to the long history of trying to understand the world at shorter and shorter distance scales. Arguably this was always going to happen sooner or later, the debate is just whether now is the time to give up due to the expense.

33. **Amitabh Lath**
January 24, 2019

Peter, I want to push back a little on the idea of "time to give up due to the expense".
Money spent on fundamental research does not simply disappear into some large extra dimension.

Look at Fermilab. In middle school (mid-70s) we had a field trip there and the area was all corn fields. Now it is a technology corridor. It has probably added multiples of what it cost to build into the local and national economy.

The FCC will throw up design, construction, and data analysis challenges that we would never encounter otherwise. Surmounting them will take thousands of the best brains from around the world and give them a training you can only get at the frontier.

Of course having said all that, some of us really just want to see some BSM signatures.

34. **HGB**
January 24, 2019

I am an outsider to this field. I have an interest in it – it has been at the leading edge of science for a long time. Thus, you can classify me as your typical tax payer who the HEP community should try to convince about the desirability of these new accelerators (it would of course be up to elected politicians, not me – but, you know what I mean.)

The truth is, such as things are right now, this would be a very difficult sale. My main issue here is Sabine Hossenfelder's argument, to wit, we have no compelling theoretical reasons to expect that those accelerators would find any new physics. It seems to me that we are talking huge amounts of money just to see if we are lucky and something is indeed out there. At this point in time, I would vote against such projects, recommending to devote the resources to other, more promising undertakings, of which there are many, not only in physics, but particularly in biology and genetics.

35. **Peter Woit**
January 24, 2019

Amitabh Lath,

I basically agree and don't think the "we can't afford this"argument makes sense. The EU yearly GDP is around \$ 20 Trillion, so devoting \$1 billion of that to building a collider is not crowding out anything else. And yes, the money is not being poured down a hole, it's employing people and paying their salary.

The real argument is that this funding has to come from a limited stream of public (or private?) funding for activity not devoted to making a profit. Lots of people have other ideas about what non-profit making activities they'd like to pay people to do other than build a collider.

This general issue (should we spend X on HEP physics?) tends to generate endless comments about how the money would better be spent elsewhere, according to the personal preferences of the author. This gets tedious fast and is a sort of discussion that goes nowhere (please try and restrain yourselves from such comments here). The economy and governmental budgets are not about to get reorganized along lines I'd approve of, so you're not going to hear from me an accounting of how the world should be run.

What I'd rather see discussed is the actual situation we are facing, with a realistic discussion of what the numbers look like. We are now just seeing total construction numbers, and over the next year or so those responsible for running CERN will have to see if they can somehow fit these into a plan that can plausibly be funded from available sources. I think Hossenfelder was making a mistake by starting on this campaign now. Let's see what draft plan emerges from CERN next year, and evaluate that. It will have real budget numbers and one can have a serious discussion about the implications of the collider spending vs. other sets of choices about what to do with the same source of funds.

36. **Amitabh Lath**
January 24, 2019

HGB, I have a comment on your "we are talking huge amounts of money" statement. Huge compared to what? As Peter points out, the world is fairly rich, we can afford this easily. This time around countries like China and India will contribute a larger fraction because their economies are bigger than in the 90s (they have both sent probes to the moon).

As I pointed out above, the monetary payoff will be thousands of scientists, engineers, and technicians educated to think creatively in a way that is just not possible at Google or Tesla.

Not to mention the technical breakthroughs. We don't like to talk about these because we would prefer everyone to focus on the physics, but aside from the WWW there are klystrons and niobium-titanium superconductors and proton beam therapy and a bunch more. Currently there is work being done on machine learning and ultrafast pattern matching that will undoubtedly seep outside HEP at some point.

So in terms of money, the world economy will get back far more than it puts in.

37. **Anon**
January 24, 2019

Hi Peter — A theory based on angels wont solve the hierarchy problem. What I said was that TeV scale SUSY solved an important problem and was falsifiable. Though I did not ever believe in SUSY, I think it still is one of the most amazing solutions for the Hierarchy problem, but nature seems more ingenious.

I think on of the things that requires more grants and boost is the neutrino sector — like the neutrinoless double beta decay experiments and leptonic CP phase. These and proton decay experiments if taken to the next level of precision (improved by factors of 10 or even 100) will pbly tell us more about natures.

38. **Thomas Larsson**
January 25, 2019

If there is a slim chance that an FCC will discover something, I am all in favor of building it, although I realize that I belong to the rather small fraction of the tax-payer collective that would actually care about the outcome. However, if failure to see something interesting is almost guaranteed, not so clearcut.

So I wonder how much results from precision experiments, such as the absence of EDM, are constraining possible deviations from the SM up to say 100 TeV. But then again, all such constraints are of course based on some theoretical assumptions that may be flawed.

Then I have a purely egotistical concern about the timeframe. If FCC does not start to produce results until 2050, I will probably not be around, or at least to old to appreciate them.

39. **Urs Schreiber**
January 25, 2019

Another puzzling aspect of the debate is that the flavour anomalies show every sign of being a real signal of New Physics (more). I suspect there is psychological inertia involved in not appreciating precision effects in loop contributions over the long-familiar direct detection events. But if EDM-bounds on new particle masses are anything to go by (e.g. Nature:s41586-018-0599-8), precision loop effects will only become more important in the future.

40. **Sabine Hossenfelder**
January 25, 2019

Peter,

The problem is that without the "bad theory" predictions, there are no predictions that indicate a larger collider would find anything new. "Look anyway" is all well and fine but not a convincing argument for such a big investment, especially because it's likely to negatively affect funding of more promising avenues. Maybe the situation will change with the LHC data still to come, in which case this whole discussion will become obsolete. But if not, I don't think a larger collider would be money well-invested.

As I lay out in my book, null-results are not particularly useful results if you want to develop a new theory. You really need evidence of new phenomena. It's this absence of data for new phenomena that results in "bad theories" becoming grandfathered to begin with. To break this vicious cycle of "null results – bad theories – null results", we should focus on those areas where we either already know there's something new to find (dark matter), or where we have good theoretical motivations (quantum gravity, quantum foundations).

I don't expect you to agree that those are the best areas to currently invest in, but at least I *have* an argument for why that's the most promising thing to do.

(Btw, I no longer work on quantum gravity. No need to fix that statement, just for the record.)

41. **Sabine Hossenfelder**
January 25, 2019

Should add, another way to move on would of course be if particle physicists came up with actually good predictions. I do not consider this impossible, but it would require them to acknowledge that their current methods aren't working and to come up with something better. I cannot see this happening. Sorry if that sounds cynical, but if they haven't seen light by now I don't think they ever will.

42. **Sabine Hossenfelder**
January 25, 2019

Urs,

By all due respect, you misunderstand the point of the experiment you are referring to. It's not measuring qg corrections to one mass, it's about producing a quantum superposition of masses and measuring their field. But I was not specifically referring to the Aspelmeyer group. There are various closely related ideas, see eg here https://arxiv.org/abs/1707.06050 and here https://www.nature.com/articles/549031a

Regarding your comments about quantum foundations. There are many things we simply do not understand about quantum field theories but those often receive little attention. (Think non-pert formulation, Haag's theorem, even the IR limit isn't as well understood as we thought only a decade ago.) As to your remark that it's "not serious" to test quantum foundations, that's a plainly stupid argument. I have written a whole book making my argument very clearly. I hope you'll read it before you produce further ill-informed and easily dismissive comments.

43. **Marco**
January 25, 2019

Hi Peter,
Regarding your comments

> The number I used for the FCC-ee was based on the $ 10.5 billion total construction cost estimate given in the executive summary (page xxvi) of the CDR.

Indeed this is written in FCCee executive summary.

> The construction cost for FCC-ee amounts to 10,500 million CHF for the Z, W and H working points including all civil engineering works.

But this does not include the tt treshold at 365 GeV, for this you need to scroll to page 275 of the CDR where it is stated

> Operation of the FCC collider at the tt working point will require later installation of additional RF cavities and associated cryogenic cooling infrastructure with a corresponding total cost of 1,100 MCHF.

And this is how you come to 11.6 billion in total. This number is also in line with what the CERN DG presented at the New Year presentation (https://indico.cern.ch/event/779524/) slide 62.

Thus operating CLIC at 1.5 TeV is still cheaper then FCCee and for instance you could measure the Higgs self coupling and quartic coupling which you cannot at the FCCee, among many other things.

44. **Shantanu**
January 25, 2019

Dave: The short answer is no. There is no connection between theories which predict non-0 neutrino mass and those which make predictions for LHC and beyond LHC experiments. They re completely decoupled form each other. Nothing we'll learn from neutrino experiments will tell us about collider physics or vice-versa. It disturbs me that no one else is concerned about this (esp. in the days of "grand unified theories" etc)

45. **sphaerenklang**
January 25, 2019

So many different ways of being a 'bad theory' – but what does it mean to be one, in the sense the phrase is being used here ?

E.g. 'so implausible and badly motivated that it's not worth spending even a few thousand dollars to test/disprove' (for instance .. perpetual motion machines?)

or 'badly motivated enough that it's not worth employing even one person to investigate experimental consequences' ?

or 'bad enough that they should never appear to be one of the principal science goals of a billion-dollar experiment' ?

Those are very different levels of badness. We know of course that LHC was planned and funded long before anyone came up with the prime examples of large extra dimensions / low-scale quantum gravity. And apparently even these were not quite bad enough to dismiss out of hand – without spending some years of PhD students' lives on calculating experimental signatures and then setting upper limits from actual data.

46. **Peter Woit**
January 25, 2019

Anon,
For reasons I've often written about here, I'm dubious that the "hierarchy problem" is a real problem. Even if you think it is, doing this (SUSY) by invoking models with 120 or more new parameters, carefully tuned to explain why no effects of the huge number of new fields introduced has ever been seen, is what I would call "bad theory". Doing it with ill-defined extra dimensional models is "really, really, bad theory".

Proton decay experiments are an interesting case to compare to colliders. There's no evidence at all for GUTs, so no reason to believe they'll see proton decay. Should one go ahead and build such experiments anyway? The answer seems to me to depend on cost + hope to learn something else (i.e., see supernova neutrinos). Should one spend \$20 billion on a huge proton decay experiment? Probably not, the money would be better spent on a less special-purpose machine, like a collider. But, at much lower cost, it's worth doing, and one can have a sensible argument about the specific HEP experiments like a proton-decay one that could not be funded due to collider funding. My understanding is that this kind of thing is exactly what the European strategy process is about. They very well may decide a collider is just too expensive, and follow the US into restricting plans to non energy-frontier experiments.

47. **Peter Woit**
January 25, 2019

Sabine,
I share your cynicism about the state of HEP theory. Best would be progress in theory that gave plausible ideas about how to improve upon the SM, which could guide choices about which experiments to fund, but that seems unlikely, with HEP theory split between those who have given up, and those intent on pursuing failed ideas.

But, once you give up on the theorists, if you don't want to give up completely, your only hope is the experimentalists, and whatever they can come up with to look somewhere new. Let's see what emerges from the strategy process. My understanding is that they'll be doing exactly what you want, comparing expensive collider proposals with other proposals. It's entirely possible the outcome will be a decision that the collider proposals are just too expensive for the chance they provide at something new, and Europe will head down the same road as the US HEP program.

Personally, I just haven't seen any evidence that there are other (non-collider) equally promising and cheaper places to look that aren't already being pursued. In particular, the search for "dark matter" has been a huge priority among theorists and experimenters for decades now, I'm dubious there are promising experimental avenues corresponding to well-motivated theory models that are not being actively and aggressively pursued.

48. **Peter Orland**
January 25, 2019

There is good physics coming out of the LHC. Hadronic cross sections (relevant to perturbative and nonperturbative strong-interaction physics), nucleus-nucleus scattering, etc. The intersection of this with the sales pitches is the null set, but it is science that nuclear (and some particle) physicists really care about.

The LHC was sold to the public as a big-bang-higher-dimensional-supersymmetric-hierarchy-problem-string-proving gadunzel. Perhaps if press releases had given a more complete picture of the science, there wouldn't be as much hand-wringing now.

49. **Low Math, Meekly Interacting**
January 25, 2019

Well, that's just it, isn't it.

Explaining the challenges of nonperturbative QCD to the average layperson (like me) is, well, very, very hard, much less what makes it worthwhile to study. Hopefully it's not harder than nonperturbative QCD, though.

50. **Saul Youssef**
January 25, 2019

Nice article Peter!

One thing I would modify is "One piece of advice though is that experience of the past few decades shows you probably shouldn't listen to theorists." I think that the right lesson is not to listen to SOME theorists. I think you'll find that the really great ones are dead honest about the LHC. Here, for example, is Steven Weinberg, speaking to the public about the LHC, pre-LHC (2010) without the slightest trace of getting over excited about speculative ideas. 52:08 https://www.youtube.com/watch?v=Gnk0rnBQrR0

Pick any field. It's almost inevitable that if you interview lots of professors about their research, they will try to make what they are doing sound exciting by talking about the most exciting possible outcomes. That's not really something bad about particle physics in particular.

51. **Anonyrat**
January 25, 2019

HGB wrote: *My main issue here is Sabine Hossenfelder's argument, to wit, we have no compelling theoretical reasons to expect that those accelerators would find any new physics.*

I hope that if a new accelerator can tack on a few digits after the decimal point on some number of measurements — i.e., reveals existing physics to much greater precision, that would be considered worthwhile.

52. **Amitabh Lath**
January 25, 2019

Peter, thanks for this: "…if you don't want to give up completely, your only hope is the experimentalists" Yes.

Let's consider the two arguments against a 100 TeV collider separately.

The money argument is specious. A few 10s of Giga$$ will be easily repaid into the world economy just by the technical and human products of the project.

The other seems to be "theorists have not written a clever enough paper to warrant a bigger machine." This is ridiculous given the two decade lead time. While plenty of machines were driven by theory (Bevatron for the antiproton, SPS for the W/Z, PEP, Petra, Tevatron for the top), there were many that were not (SLAC's Linac and SPEAR, BNL's AGS come to mind).

53. **WTW**
January 25, 2019

See the following for some examples of BSM physics that do not require higher energy colliders, and challenges in the funding of such experiments (and theorists who work on them):
http://online.kitp.ucsb.edu/online/hepfront18/safronova/rm/jwvideo.html
with slides at http://online.kitp.ucsb.edu/online/hepfront18/safronova/pdf/Safronova_HEPFront18_KITP.pdf

And http://online.kitp.ucsb.edu/online/hepfront18/vantilburg/rm/jwvideo.html

These are discussions of using ultra-high precision experiments rather than higher energy experiments to discover and explore deviations from the SM and Dark Matter.

54. **tulpoeid**
January 26, 2019

LHC experiments discovered the Higgs boson, therefore it didn't fail, period.

On the other hand, the money involved is a lot, and the question is not science vs. military expenses, it's science vs. science at this point. (Human Brain Project, anyone?) Also, a lot of what is going on is ego games by aging academics who have been used to be treated overly well. Apologies, but there is a societal/psychological side to almost everything, biases should be recognised even when accepted.
Even worse, have the consequences and the public repercussions of not delivering anything been considered? I hope they have by some. Because I'd consider such a scenario as catastrophic.

On the other other and more practical hand, I think there is an underrepresented aspect of the situtation in public discussions: Accelerator-based science and new experiments are not going to leave us. It's not "100TeV or nothing". Even CERN started inviting proposals for ingenuous new projects based on smaller-scale accelerators a few years ago, precisely as good alternatives to a lack of higher energies.
Some higher energy frontier fans have been giving the impression that we know only one way forward and that is brute force; brute force is a lesser hacking tactic, it may well be a lesser nature hacking tactic as well.

55. **J. S.**
January 26, 2019

What is the current status of muon colliders? They seem like an attractive idea to me — they are elementary particles, giving "clean" collisions; their mass is high enough that synchrotron radiation is not a problem; and yet their mass is one-ninth that of a proton, so the collider can be smaller for the same energy. On the other hand, the half life of the muon is 2.2 microseconds, so you have milliseconds at most to use them, even accounting for relativistic time dilation.

Fermilab has (or had) a "Project X" that included a muon collider but the information available on the web is out of date.
http://map.fnal.gov/index.shtml

Perhaps money would be better spent on this than on electron or proton colliders?

56. **Peter Shor**
January 26, 2019

I think one very important point to remember is that the LHC was sold to the public and the funding agencies using not just "we'll find the Higgs" but also "we'll find supersymmetry."

In retrospect, it doesn't matter. It's clear that the cost was justified by finding the Higgs and by what we've learned about the Standard Model.

But suppose we try to sell this new collider using the argument "we'll add a few more decimal points to our knowledge of the Standard Model, and there's also maybe a tiny chance that a miracle occurs and we find a new particle that there's currently no evidence for." Do you think it'll be funded? I don't.

So in order to get the new collider funded, we'll end up lying (maybe not lying, exactly, because there are string theorists who still believe that supersymmetry will be found just around the corner, but close enough). And then, if it actually doesn't discover anything, there's going to be a backlash against physics—probably all of physics.

So I think Sabine is ultimately right in that we shouldn't be trying to build a new collider now. Let the LHC run a few more years, see whether it sees hints of anything new, and decide then.

57. **NoGo**
January 26, 2019

I have no skin in this game other than as a taxpayer and interested bystander — and I am all for building the new collider, because frankly I'm curious what's out there at the "energy frontier"!

But I have a (I think) a legitimate question, which I did not see raised in the discussions I've read. This is: if there is new physics at the new collider energy, what are the chances it will be actually detected without "guidance" from the theorists?

Ever since I've learned about the triggers and how most events in the collisions are _not_ collected and recorded, I was wondering about it. As far as I understand it, the logic of the triggers is based on the Standard Model calculations. But cannot it lead to a false self-confirmation and circularity?

I've recently read on an internet forum an anonymous post claiming that LHC may be already producing "tons" of glueballs and how these events would look very similar to some common decays, and so dismissed by the triggers, and how one needs to have the right theory to distinguish these events. I have no idea whether the poster was a real physicist or a crackpot, but isn't there a danger of a similar scenario, and how big this danger is?

And if there is such danger, it would seem to me it increases with the size of a collider, because the bigger the energy, the larger is the percentage of events that one needs to discard (please correct me if I'm wrong).

So, shouldn't this also be a factor in assessing costs/benefits of a new collider?

Thanks!

58. **Amitabh Lath**
January 26, 2019

Tupeloid, your comment "it's science vs. science at this point" is dangerously naive.

Do you seriously think the few 10's of G$ that would have gone to the FCC-like collider will be plowed into other science? Are you still waiting for the post-SSC-cancellation funding bump for brain science and condensed matter?

No, the money just disappears. Probably tax cuts for the well off. If we do not build the next collider science will shrink as a fraction of human endeavor.

And I am effing sick of all the "but the theorists promised us SUSY wah wah wah…". You lot should be smarter than that. No one can "promise" that. What we promised, and delivered on, was *substantially* improved understanding of nature at ever shorter distances. Including the Higgs which wasn't a promise, but a reasonable guess given top mass and electroweak asymmetries.

Nature is what nature is. If we stop studying it because it doesn't confirm to our preconceived notions, it diminishes us.

59. **Amitabh Lath**
January 26, 2019

NoGo, your comment "LHC may be already producing "tons" of glueballs and how these events would look very similar to some common decays" is well taken. Probably not a crackpot (but you never know on the internet). This sort of physics a personal interest of mine.

We throw away over 99% of the collisions due to filters that require high energy deposits, or objects like electrons, muons, missing momenta… There are serious concerns about this sort of "filter bias" and there are regular discussions of how the new physics signatures could be hiding from us. One of them is what you describe, colored objects that decay in a shower of soft pions and escape all filters. Other ways include long lived particles (longer than the 20ns data-capture window), or signatures that travel along the beamline rather than transverse to it.

One of the ways we are addressing this is to use cutting edge fast electronics to do data analysis at a rate fast enough to recognize these reactions.

60. **DarkMatterMan**
January 26, 2019

Peter,

There certainly are experiments founded on good ideas that aren't being *effectively* pursued alongside the big LHC and neutrino experiments. As an example, there are a number of promising new ideas in the search for Dark Matter which involve small investments – experiments costing ~\$1M-\$10M. The *community* has been actively pursuing these projects for many years, but funding support has been extremely thin because they are not called out by name in the P5 report (which simply calls for a portfolio of "small and medium sized projects") and they have been squeezed, sometimes to the edge of viability or to extinction, by emphasis on the LHC and the neutrino program. Meanwhile, new proposals in this space have been slow-walked by the funding agencies while they figure out how to build a case to spend some relatively modest money within a system designed to shepherd much larger projects. The latest step in this now multi-year process was a recent "Basic Research Needs" workshop (formal DOE workshop used to define the "mission need" for a new research area): here is the glossy "show your congressperson" brochure shown at the recent HEPAP meeting based upon the workshop report:

https://science.energy.gov/~/media/hep/pdf/Reports/Dark_Matter_New_Initiatives_Brochure.pdf

…where the full report is due to be released by OHEP in the next several days. There is an intent – if it is decided that some money can be set aside for this – to call for proposals based on this report in the coming months, but I am certain that many good ideas will not make the cut even in the rosiest scenario. I believe this is an example of the kind of experiments that really aren't being aggressively pursued, and could be if we were willing to say we don't need every last dollar (and then a lot more) to build the next huge machine. Some of these ideas are indeed part of the European Strategy discussion.

It used to be that a smart lab director with a vision had the discretion to make experiments of this scale happen, and many great discoveries resulted. Whatever comes out of the discussion of what happens after the LHC, I hope there can be a solution, at least a partial one, to making a path forward for these kinds of small experiments – not just in this area, but across the field, because the system we have for funding, approving and managing \$1B experiments has proven unsuitable to dealing with these small experiments.

(It looks like my long link isn't going to be preserved, but you can easily google basic research needs dark matter.)

61. **Peter Woit**
January 26, 2019

DarkMatterMan,
Thanks for the comment. I don't think though that this sort of problem (how the current system deals with funding for small investments) is really that relevant to the collider discussion. The US has no expensive collider program now, and the last couple years DOE HEP has seen huge increases (10% in FY2018, 8% in FY2019, due to the Trump era jettisoning of any restraint on spending). So if, with plenty of new money and no collider, the US is not funding such small investments, the problem lies elsewhere than in the size of the budget or the existence of an expensive collider construction program. If CERN decides not to build a collider, they'll then likely be in the same sort of situation the US is now in, no reason to believe the problems of small investments will be any different.

62. **tulpoeid**
January 26, 2019

Amitabh,

> Tupeloid, your comment "it's science vs. science at this point" is dangerously naive.

I understand that it might sound like it because of the unusual tone, but it isn't really:

> Do you seriously think the few 10's of G$ that would have gone to the FCC-like collider will be plowed into other science?

I'm describing the situation as it is, not as it should be. In an ideal world both a huge collider and many smaller projects should be funded, but now, yes, if this order of money goes into one project then the science budget of several countries will have to cut down elsewhere. We cannot simply forgo this fact when thinking about the proposals.

> Are you still waiting for the post-SSC-cancellation funding bump for brain science and condensed matter?

From the context I understand that you didn't read correctly my mention of the Human Brain Project. I didn't mention it as a nice funding target, and I'm definitely not talking about taking money from physics and putting it elsewhere.
If you look up the Project's history you'll see that it is a very centralized mega-project which dried European funding from brain research in areas or labs not related to it. When it was under discussion many brain researchers opposed it on this ground. Now it is approaching its designated duration and it has infamously produced almost nothing.
So, I used it as an example of a real very centralized mega-project which did dry funding from others working in the same field.

> No, the money just disappears. Probably tax cuts for the well off. If we do not build the next collider science will shrink as a fraction of human endeavor.

Again, I'm describing the current situation, not the ideal one. (Still, I'm not sure that science will shrink as a result, if this money is spent on other HEP projects.)

63. **DarkMatterMan**
January 26, 2019

Peter,

You stated in a reply to Sabine that you had no evidence that there were promising new ideas – e.g. in Dark Matter – that weren't being aggressively pursued. This is what I was replying to:

"Personally, I just haven't seen any evidence that there are other (non-collider) equally promising and cheaper places to look that aren't already being pursued. In particular, the search for "dark matter" has been a huge priority among theorists and experimenters for decades now, I'm dubious there are promising experimental avenues corresponding to well-motivated theory models that are not being actively and aggressively pursued."

I was pointing out that this just isn't the case. No – neutrinos are not colliders – but it's the US mega-project and pushing the schedule for that, along with the other large projects, has eaten up all of the big increases you mention (along with the QIS push in OHEP, which is funding a lot of ideas of very dubious quality just because there is a big pot of money and not many really interesting ideas to use quantum computing for HEP.)

In any case, I never said, or implied, that there was a problem in the size of the budget, but I didn't think that was what was being discussed here. I thought the discussion was about how to spend the money that can be available, and I do think there is a problem where only the biggest and highest visibility projects squeeze everything else out and there is no process appropriate to bring forward promising small projects (which absolutely do exist!) and see that they have adequate resources to succeed.

64. **Peter Woit**
January 26, 2019

DarkMatterMan,
You're right, I should have made the point I was trying to make in a different way. I'm not well informed about the specifics of dark matter experiments, you may very well be right that there are good experimental proposals not getting funded. If this is so though, it's not because the issue of dark matter doesn't get much attention, or because of a lack of money that could fund small-scale experiments. So, I just don't think you can pin this problem on colliders, or expect that it will improve if collider proposals fail.

I suspect this is kind of a generic issue: non-collider scientists are aware of what they see as good science in their field which is not getting funded, think maybe if collider funding stops, then that good science will get funded instead. In most cases though, scientists thinking this way are likely to be disappointed. In this case, if there's a problem with small-scale experiments not getting attention and funding now, that problem is still going to be there after collider projects are cancelled. If CERN changes direction, they're not likely to decide "we're going to spend the \$500 million/year we save by not building a collider on 100 new small experiments each year". Instead they'll pick some small set of larger initiatives to fund.

Interested to hear your take on the redirection of HEP funding to Quantum Information Science. I'm beginning to suspect that both on the theory and experiment side, this is a really dubious trend, one that physicists should be pushing back on instead of joining.

65. **DarkMatterMan**
January 26, 2019

Peter,

I should perhaps also have been more clear. I'm not arguing against a future big collider or trying to "pin the problem on colliders", but simply that we have to find a way to make sure they don't squeeze out new ideas for small experiments. It's a problem, but not with colliders, rather more with the sociology that develops around big projects that often get oversold, to politicians, to the public, and to young people in the field who buy into the idea that these are the only things worth doing and are guaranteed to make huge new discoveries. I'll admit to having a dog in this hunt – since I work on some of these small Dark Matter efforts – but I've worked on the big collider experiments too, so I see this from many sides.

As far as these new initiatives, the full report on Dark Matter Basic Research Needs should be out soon and you can judge whether you find the case compelling. Who knows, maybe this process will be a watershed moment in trying to solve the problem. I know this has been seen as successful so far, and there is an intention to repeat this process to define the programs for other new areas of inquiry.

As far as QIS for HEP, here are the awards for which an estimated $12M was available, and you can decide for yourself what you think:
https://science.energy.gov/~/media/78921BDAD1894BBF9A686A2125C3B3C0.ashx
My impression (I can really only comment on the experimental side) is that a few of these are interesting, but perhaps will not get enough support among many others that are little more than a mashup of buzzwords without any real justification for the use of quantum computing techniques.

66. **Amitabh Lath**
January 26, 2019

Tupeloid, I understand your concern. Yes I would rather have a marketplace of several small experiments than one MegaProject. Socially, politically, financially it is definitely better.

But it appears Nature doesn't care, and is holding cards close to the vest. If we want to go beyond the effective theory that is the Standard Model, we're gonna need a bigger collider.

And since I grew up in Chicago let me quote Daniel Burnham: "Make no little plans; they have no magic to stir men's blood and probably themselves will not be realized. Make big plans; aim high in hope and work."

67. **WTW**
January 26, 2019

Amitabh Lath wrote:
"If we want to go beyond the effective theory that is the Standard Model, we're gonna need a bigger collider."

We keep approaching this as if "a bigger collider" is the only way to make progress in experimental, fundamental/particle physics. It's all or nothing – either we keep building colliders or we just stop and give up. It's not, and we shouldn't.

It should be noted that, due to limitations in their very design, high-luminosity high-energy colliders may no longer be the best approach to advancing our knowledge. Without meaning in any way to diminish the outstanding accomplishments of the experimentalists, engineers and technicians at FermiLab and CERN over the last decades, we need to recognize some of the fundamental limitations of that approach, and include those in decisions on how to proceed – and what to ask to be funded. The LHC is a wonderful "theory confirmation" machine, but it has issues with discovering unexpected results that aren't factored in to the designs of both the hardware and software (and analysis) required to handle the vast flood of both collision debris and data generated by those detectors, to the level of precision sometimes needed. It may be time to start thinking about whether other approaches could actually produce better results – before asking for more money, just to keep on doing what we've been doing for the last 75 years or so.

Part of this emphasis on "higher energy" being the only way to make progress is probably due to some of our assumptions/prejudices about how we think the Standard

Model can be generalized/unified. It's what "high energy physicists" do, and have done, both experimentalists and theorists, often (but not always) with great success. But it also can lead to blind spots about limitations in the way the Standard Model itself (and the underlying quantum theory) is put together, as well as the mathematical framework around which it has been developed.

To use a very crude and imperfect analogy: It may be time to start thinking about how to use scalpels and forceps to dissect the most subtle aspects of fundamental physics, rather than just hitting it harder with a bigger meat cleaver. Then we can talk about the cost(s) of those approaches vs. potential results. Just leaving those options off the table and hoping that somebody will do them sometime somehow just as long as we get more money for that bigger collider, when in fact they may be the preferable options, would be counterproductive scientifically as well as financially and politically.

68. **Alain Blondel**
January 27, 2019

Very nice post, thank you. No of course we should not give up, especially since there are important questions (The Baryon Asymmetry of the Universe, Dark Matter and the origin of neutrino masses) that are very likely to have particle physics answers.

Let me make a couple more additions to your description of the FCC-ee:

-1- One thing which has not permeated in the community very much, is that the 'new particles' that could explain the above questions do not necessarily have the kind of couplings that would allow to see them at the LHC. In fact it is very possible that they have much smaller couplings — this would automatically explain why we see no hint of new physics in the precision Electroweak measurements, while the top quark effect was a hefty 10 sigmas shift — for sure there isn't another top quark, up there. (we also know this from the Higgs production cross-section at the LHC).

A couple examples are documented in the FCC CDR: a famous one is right-handed heavy neutrinos (also named "sterile neutrinos, heavy Majorana neutrinos, heavy neutral leptons" etc..) — which are very much expected since the (left-handed) light neutrinos have been discovered to be massive. Another is 'Axion-like particles' as dark matter candidates. Both are very well motivated and are excellent candidates to solve one or more of the above questions. In both case there is significant (and relevant) phase space that can be probed in the Z exposure of the FCC-ee: there being 510^12 Zs produced, rare events can be searched with a sensitivity down to couplings of ~10^-11 of the weak coupling.

-2- a second point concerns the '10 billions' of the cost. As I am sure you know well, about 2/3 of the cost concerns infrastructure that is there to stay, e.g. for the (100 TeV) hadron collider (and possibly other facilities), and thus is a long term (some 70 years or more) investment. This makes it much easier to argue about with the host states and local authorities.

69. **Peter Woit**
January 27, 2019

WTW,
On the HEP theory front, I very much agree with the argument that it is long past the time that theorists should give up on the main directions they have been pursuing, go back to fundamentals, and try a new direction. This is difficult to do, but the reasons are sociological, not fundamental problems of how physical reality and allocation of expensive resources works. Some theorists would like to claim that they should just keep doing what they have been doing, the computations are just unfortunately getting harder. Maybe, magically, all of a sudden something will appear.

The situation with theory though is very different than the situation with experiment (and I think Hossenfelder's essay doesn't properly recognize this). While HEP theory has conclusively failed at what it has tried to do, HEP experiment has succeeded at doing exactly what it set out to do. The discovery of the Higgs and the on-going study of its properties has been a huge success. There are now carefully put together proposals for how to extend this success to smaller distance scales. Unfortunately they appear to be inherently expensive.

The idea that there is some different, much less expensive, promising route to progress is wonderful if it were true, but one has to come up with a viable proposal. I assume a significant part of the ongoing European study will be to examine such proposals. If such proposals exist, quite possibly the result will be to not fund a new collider, but to fund such proposals instead. Absent such proposals, what I don't think is a good idea is wishful thinking, i.e. deciding that since the known route is difficult and painful, let's just not do it and hope that somehow an easier one will appear.

70. **martibal**
January 27, 2019

Regarding the costs, since the world is always more full of billionaires which are more and more rush, what about some private fundings ? I completely agree that States should finance CERn, but in case they do not, is it totally exclude that some private philanthropists could contribute ? If I remember well, CERN has already "accepted" sone private fundings accepting the Breakthrough prize, and there are some examples of private financing of fundamental research which are working well, no ? (see PI, or the Simons Fondation). 5 billions is just 5 per mille of Amazon value (or Apple a few month ago).

71. **Amitabh Lath**
January 27, 2019

WTW, I agree with you. New physics could well be hiding in the collisions we already have. One of my personal interests is particles with strong interactions and low mass. They would be produced in huge quantities but this reaction would be overwhelmed by the huge rate of normal strong reactions in hadron collisions.

This is just one discussion. Another is long-lived particles. Think of a massive, slow moving BSM particle. It would be missed completely. We (experimentalists and phenomenologists) are constantly asking the question "Where could the new physics be hiding?".

The next generation colliders will not only be higher energy, but have smarter hardware and software as well. Expect machine learning and AI to permeate all levels from trigger to final analysis.

72. **Stephane**
January 28, 2019

Dear Peter,

thanks for your informative post.

Due to the lack of direct discoveries of new physics so far at the LHC, the current strategy in experimental HEP is to search extension of the Standard Model indirectly, by viewing the SM as an effective field theory (SMEFT, eg, arXiv:1706.03783 and arXiv:1812.08163) and thus perform the most precise measurements possible (by reducing the statistical uncertainties by collecting more data, and the systematic uncertainties with better detectors, etc.) Of course, this is a challenge for computing but also for theory to provide the most accurate possible calculations of SM quantities! This is the so-called "Intensity Frontier" program (a better name would be "Precision Frontier") in contrast to the "Energy Frontier" program.

This is the idea behind HL-LHC, HE-LHC and part of FCC programs. Of course, if one finds new physics directly at FCC, the better. It is thus of no surprise that a recent Les Houches school was dedicated to Effective Field Theory
see: https://indico.in2p3.fr/event/13465/

Let me finish by quoting Burton Richter (arXiv:1409.1196)
Abstract: "The success of the first few years of LHC operations at CERN, and the expectation of more to come as the LHC's performance improves, are already leading to discussions of what should be next for both proton-proton and electron-positron colliders. In this discussion I see too much theoretical desperation caused by the so far unsuccessful hunt for what is beyond the Standard Model, and too little of the necessary interaction of the accelerator, experimenter, and theory communities necessary for a scientific and engineering success. Here, I give my impressions of the problem, its possible solution, and what is needed to have both a scientifically productive and financially viable future."

73. **DB**
January 28, 2019

Hi,
this Nima conference from four months ago might be pretty relevant, IMO, for what´s being discussed here.

https://www.youtube.com/watch?v=zE6XjWzl-Eo

Best.

74. **Peter Woit**
January 28, 2019

DB,
While I agree with much of what Arkani-Hamed has to say in that talk, he definitely was someone I had in mind when I wrote above: "experience of the past few decades shows you probably shouldn't listen to theorists." He's smart and great at conveying enthusiasm, but he's also now spent a lot of his career vigorously and enthusiastically promoting the LHC in unfortunate ways (large extra dimensions, SUSY (Split or not), the LHC will explain why gravity is weak, only SM physics at the LHC is evidence for the multiverse, etc., etc.).

Three Short Book Reviews

January 29, 2019
Categories: Book Reviews

Unfortunately I don't have time now to write about the three following books at the length that they deserve, but here are some quick comments on three books worth your attention:

- A carefully produced detailed write-up of Sidney Coleman's Harvard Physics 253 quantum field theory course has now been published by World Scientific. This course was taught by Coleman off and on from the mid-seventies until 2002, and the book is based on various sets of video recordings and lecture notes (including a copy of my lecture notes from when I attended the class). A huge amount of work by various people has gone into producing a very high quality book. David Derbes has some comments here, and he is perhaps the main person to thank for seeing this project through to completion.

  David Kaiser has contributed an introduction to the book (available here, or, if this doesn't work, try here) which does an excellent job of putting the material in historical and intellectual context, as well as describing what Coleman was like and why he had a huge influence on several generations of Harvard students. If you've already spent a lot of time learning QFT from various modern textbooks, your reaction to much of this one may be "that's the standard way of explaining that point, nothing unusual here." Keep in mind that often the reason that's now the standard way of explaining things is that many authors of modern textbooks learned the subject from Coleman (or from someone who learned it from Coleman…).

- Jim Baggott has a very good new book out, entitled Quantum Space, which could roughly be described as a popular account of loop quantum gravity at the level of the account of string theory in books like Brian Greene's *The Elegant Universe*. Baggott has spent a lot of time talking to Carlo Rovelli and Lee Smolin, and one of the best aspects of the book is the way it conveys their personal stories, intellectual journey, and current outlook on the subject.

  One unavoidable topic that Baggott covers is the relation of string theory and LQG as competing (or perhaps someday collaborating?) approaches to the problem of quantum gravity. Due to long ago experience (to get an idea, watch this), I've long ago lost patience for arguments about which approach is "better". Baggott's take on the issue seems fair to me, but if you really want to engage in that argument it will have to be elsewhere than the comment section here.

- Finally, the new book you really should buy a copy of is my brother Steve's Fly Fishing Treasures. He has been working on it for years, and it includes the most amazing beautiful pictures of antique fly fishing equipment in existence, as well as a wealth of information about those who collect these things. OK, if you, like me, aren't especially excited about the topic of fly fishing, then buy a copy as a present for someone who is.

Update: I should have included links to postings about Coleman here, here and here.

Update: If you'll be at the March APS meeting in Boston, I hear there's a book launch for the Coleman book, 1:30pm-2:15pm Weds. March 6, at the World Scientific booth in the exhibition hall.

Comments

1. **David Derbes**
   January 29, 2019

   Many thanks for the kind remarks, Peter. I would remind your readers that the videos of the Coleman lectures from 1975-76 are up at Harvard's site. I think the fair division of credit is equal shares (as in splitting the bill for a meal in a crowd). There are six names on the cover as editors. I wanted a seventh, but that contributor wished to remain anonymous. So I'm happy to take one-seventh of the credit for editing, but no more. See my earlier remarks or the book's preface for more details if you wish. I, too, think David Kaiser did a wonderful job on the Coleman foreword; he also did a terrific foreword to the reissue of Misner, Thorne and Wheeler. I would recommend to anyone putting together a book with an interesting history that she or he ask David K. to consider writing a foreword to her or his book!

2. **Low Math, Meekly Interacting**
   January 29, 2019

   "…early encouragement from Hoagy Carmichael…"

   Seriously? That's amazing.

3. **Peter Woit**
   January 29, 2019

   LMMI,
   May be the son of the one you're thinking of…
   http://www.everythingzoomer.com/arts-entertainment/2018/01/22/hoagy-carmichael-jr/

4. **sdf**
   January 30, 2019

   The link to the foreward does not work for me.

   Can you give a comparison of how this compares with other intro books to QFT? I am totally unaware of Coleman's lecture notes up to now.

5. **Peter Woit**
   January 30, 2019

   sdf,
   Others have also noted this. Was working for me, now I also see a problem. Looks like something on the publisher's end. I've added another link to try to the posting. From the publisher's webpage for the book, you are supposed to get free access to the frontmatter, including the Kaiser piece.

   For more about the Coleman book: the topics covered are not unusual. There's a lot of influence of Bjorken-Drell and the point of view on QFT of the 1960s. This is combined with influences from the more modern point of view (gauge theories, path integrals, renormalization group) that the SM brought into vogue starting in the mid 1970s. The really unusual thing about the book is that it is very much based on Coleman's lectures, as opposed to being written as a usual textbook. So, there's a lot that comes through of the details and the spirit of a (very high quality) live lecture, while at the same time careful attention to the details of how to do calculations.

6. **Andre**
January 30, 2019

I took detailed notes on Coleman's lectures from the videos available when a graduate student at Harvard. The lectures were great, though not live, and my notes were extremely useful the first time I taught QFT, because as you said, there were almost no books available at that time. Consequently, I'm more interested in the fly-fishing book, as fly fishing is a one of my favorite activities!

7. **Mark Weitzman**
January 30, 2019

Is there an errata page for the book (or some place I can email corrections for a future edition) ?

For me the high points of the book are that you can read it without pencil and paper in hand. Additionally there are 25 problem sets with detailed solutions (which fills in many of the places where in other QFT books, you would need the pencil/pen and paper).

8. **Peter Woit**
January 30, 2019

Mark Weitzman,
For the Coleman book, you could email David Derbes (loki@uchicago.edu). On the other hand, if you find typos in the fly fishing book, email me and I'll forward them to my brother…

9. **Mark Weitzman**
January 30, 2019

Peter Woit: Thanks, I actually went fly fishing once in Whitefish, Montana about 25 years ago.

10. **Anonyrat**
February 2, 2019

I just received the paperback version of Sidney Coleman's QFT Lectures, and though perhaps not as nice as the hardback, it is very nicely produced indeed. (Purchased it through Amazon). My thanks to the editors for putting this together.

11. **João Paulo Cardoso**
February 28, 2019

Peter,

I will soon start studying QFT, and I intend to use Coleman's lectures. I don't plan to purchase the book published by World Scientific, but to use his material available on arXiv. The problem is: these lectures do not have exercises at the end of each chapter. Do you recommend any set of exercises to do along Coleman's lectures? It can be from any book or online course or similar stuff.

On Inference

February 2, 2019
Categories: Uncategorized

The first issue of the magazine Inference appeared online back in 2014. At the time, it was surrounded by a significant amount of mystery: who were the editors, what were they trying to do, and who was funding it? I asked around and no one I talked to was sure what the answers were to these questions. Best guesses seemed to be that it was run out of Paris, with David Berlinski playing some role, and the funding source might be Peter Thiel.

Looking at the early issues that came out, on the topics I was competent to judge, the contributions about mathematics and physics were generally interesting and of high quality. On some other topics where I lack competence, there seemed to be a skeptical attitude towards materialism and evolutionary theory that I'm not sympathetic with.

Late in 2015 I was contacted by someone from Inference (Hortense Marcelin) to write an essay for them, something about the multiverse and string theory. After thinking about it a bit, I turned down the offer. The main reason was that I was sick and tired of the subject, didn't want to spend time writing at length about it. A contributing factor in the back of my mind was that, not knowing the identity of the editors or anything about their agenda was another reason to not get involved.

A couple years later I got another invitation to write for them, a request to write a short response to an excellent piece by George Ellis, Physics on Edge. Deciding to do this wasn't hard. The piece would be short and I already knew exactly what I wanted to say, so it would take little time. In addition, I think by this time the identity of the editors was known, and, most importantly, Inference had a pretty good track record of publishing high quality articles in the areas I know about. What I wrote was published as Theorists Without a Theory.

Adam Becker a few days ago published at Undark a long article about Inference. It's a bit of an exposé, taking issue with some of the writing as "intelligent-design propaganda", and revealing that yes, Peter Thiel is a funder. An odd part of the story is that Becker suspects that a negative review of his book by Glashow in Inference was motivated by the fact that he had not much earlier contacted Glashow to ask pointed questions about the publication and its funding.

Today I got in my inbox *A Statement from Sheldon Glashow and Inference*, which is available here. You can read it for yourself. Noteworthy in the Undark article is Becker's report that Glashow had told him that "questioning evolution" is "no longer a
policy of the journal". Referring to two early 2014 articles that could be described as questioning climate change and evolution, the statement says:

> Becker believes that two of our essays are deserving of censure. They are William Kininmonth's "Physical Theories and Computer Simulations in Climate Science," and Michael Denton's "Evolution: A Theory in Crisis Revisited."
>
> Both were published in 2014.
>
> Now a secret must be imparted. Sheldon Glashow and Rich Roberts agree with Becker. Richard Lindzen and David Gelernter do not.

It ends with this response to the accusation about the motivation for the negative review of Becker's book:

> Inference commissioned Sheldon Glashow to review Becker's book in the spring of 2018, well before Becker was known to Inference. The idea that we would require the services of a Nobel Laureate in order to make a fool of Becker is absurd. Becker is capable of doing that quite by himself.

Comments

1. **Davide Castelvecchi**
   February 2, 2019

   Oh, yes, that old, classy argument "he is a Nobel Prize winner, who do you think you are?" Also that other classy argument "yes, we do creationist and climate-denialist propaganda, and if you criticize us you must be an enemy of freedom". Adam Becker should be applauded, not ridiculed, for taking on such dark and powerful forces.

2. **Sabine Hossenfelder**
   February 2, 2019

   I was contacted by someone from Inference some years ago. They asked me to write an essay for them and made a pretty good financial offer. I put a lot of effort in this and submitted the piece as requested.

   After some while I received a revision from an anonymous editor who had garbled up my argument so badly and misrepresented my opinion so much that I could see no common ground and just refused to agree it be published. Luckily I hadn't signed the letter of agreement, so I had no trouble pulling out of this. (Otoh, I didn't get the kill fee either.) I then shortened the piece and published it elsewhere.

   By now I have dealt with quite a number of editors at many different publications and let me just say I have never seen anything remotely like this. Normally they are are little more… restrained. Also, while it's rather common that fact checkers and copyeditors remain anonymous, I don't know any other place where they don't tell you who is the editor.

   In any case, if you have been wondering why I never share or comment on anything from that magazine, now you know why. I got away with the impression that this magazine's editors have a rather heavy hand.

3. **Peter Woit**
   February 3, 2019

   Davide,
   I wasn't very impressed by Becker's book either, see
   https://www.math.columbia.edu/~woit/wordpress/?p=10147
   An interesting question to ponder is whether Becker's Undark piece would have been different if Inference hadn't published a bad review of his book. It came off to me as a hit-piece.

   As for the dark and powerful forces at Inference, the list of their editors is now public (and quite distinguished). Yes, it seems to be Thiel's money, but, if it's paying for good science writing (modulo some early dubious choices), so what?

4. **Davide Castelvecchi**
   February 3, 2019

Peter — I don't know whether or not Adam is correct to suspect that Inference slammed his book because they knew he was on their case. I have not read Adam's book yet, but whether I or you like it seems besides the point here. The picture painted by his Undark piece is much bigger and I am surprised that it doesn't cause you concern, given how active you are in tracking the influence of money on science.

At least the Templeton Foundation is open about its agenda and does not try to cover up its tracks. I find it hilarious for a publication to have anonymous funders (and even editors, according to Sabine) and then scoff at people who find that creepy. And I don't find it reassuring that the identity of the funder was not a secret to people "in the know". If anything, that makes it even creepier to me.

In response to all this, some mathematicians and physicists' attitude seems to be: "But the guy is so smart and generous and we like to have a place that publishes cool essays about quantum gravity and algebraic geometry."

But I find it hard to isolate this from the broader context, which is that you have a government that steals babies from their parents and puts toddlers in cages, and a billionaire who supports said government — and whose money comes in part from selling surveillance technology to it (https://www.bloomberg.com/features/2018-palantir-peter-thiel/).

5. **Sabine Hossenfelder**
   February 3, 2019

   Addendum: My husband reminded me that at the time I was pretty pissed off the anonymous editor believed to know more about my research area than I do myself. It didn't occur to me at the time, but now that I think of it, this almost certainly means it was a physicist. Someone with a big ego.

   I felt pretty stupid back then for turning down $5000, but in hindsight I am glad I didn't let some anonymous person channel their opinion through an essay with my name on top of it.

6. **Dave Miller**
   February 3, 2019

   Peter,

   While I think Intelligent Design is nonsense, I suspect that it may serve a positive purpose by forcing evolutionary scientists to provide more detailed evidence in rebuttal.

   Climate science is a different story: the media over-simplifies the subject to a binary "no human impact" vs "catastrophic human impact." "No human impact" is of course nonsense, but, in fact, there is lots of room for gradations in between the extreme positions: when Richard Lindzen of MIT or Judith Curry of Georgia Tech raise detailed questions about some specific aspects of the climate consensus, I am interested in their views, just as I am interested in your views critical of the established endeavors in string theory.

   I just finished Adam's book and found it interesting and informative. The book certainly has its heroes and its villains (though Adam seems not to see this himself). On the other hand, since some of his heroes (notably Einstein, deBroglie, and Wigner) were already some of my heroes, I found the book congenial.

   Does anyone know of any critiques of Adam's book that argue that he badly botched the story factually (as opposed to the reviewer's simply not liking the book personally)?

   Dave Miller in Sacramento

7. **Peter Woit**
   February 3, 2019

   Dave Miller,
   I just don't think arguments over publishing denials of climate change and evolution are that relevant here since that's not what Inference is now doing. One could definitely question what they were up to in their first few issues (and this affected my decision not to write for them then), but to be fair I think one needs to look at their current editors and what they are doing.

   For another review of the Becker book by an expert, see this
   https://arxiv.org/abs/1809.05147
   which I think does a good job of explaining the problems with the book. These problems are not a matter of "not liking the book personally". There's some similarity between the problems with the book and with the article: both are in some sense an unfair "takedown" (of Bohr and the Copenhagen interpretation in the case of the book, of Inference and its editors in the case of the article).

8. **Peter Woit**
   February 3, 2019

   Sabine,
   That is an odd story. My own experience was completely different (the only editing of the piece I remember was asking me if it was all right to reword a sentence or two).

9. **Peter Woit**
   February 3, 2019

   Davide,
   I agree there were good reasons to be dubious about what was going on at Inference when it first came out. Right now though their editors are advertised publicly, see
   https://inference-review.com/about
   and they have a track record of publishing that anyone can look at and make up their own minds about. Personally I think they're doing a good job, and that there are t0o few outlets for high quality writing about science, especially those that actually pay people to do it.

   I'm no fan of a lot of what Peter Thiel is involved in, but if he wants to spend his money this way, I don't see any reason people shouldn't take it. Yes, Templeton Foundation money is worth worrying about because it is designed to fund a specific ideological point of view (mixing science and religion). I don't see Inference doing this (now, one could reasonably have this worry about them at the beginning).

   On the general question of worrying about source of money funding things, I think one has to be careful about moralistically going down that road. Otherwise, for example consider the huge amount of funding now coming from RenTech via Jim Simons. I think he's doing a great job with what he funds and I have no problem with his politics, but I don't know how that money was made. The only answer I've ever seen to that question from the few who know is "if we told you we'd have to kill you." A lot of RenTech money also was used to attack US democracy and elect Trump. Talk about "dark and powerful"….

   Part of my reaction to this is also an increasing allergy to "takedown" journalism. Adam Becker in his article clearly has an agenda, partly driven by Glashow's review of his book, and it's not to give a fair evaluation of the story of Inference.

10. **Low Math, Meekly Interacting**
February 3, 2019

Has Inference properly disavowed their flirtation with ID?

Look, I don't want to revisit the debate anymore than you do, but if the observational evidence wasn't overwhelming enough, one might take some comfort that people like Fisher, Haldane and Wright put evolution on a mathematically rigorous foundation no later than the 1940's, well before the underlying molecular biology was properly understood. It's been as good as science gets for plenty of time now.

My feeling is that giving ID'ers a hearing isn't just sketchy, it's agreeing to disseminate a pernicious lie. If they haven't already, they should admit is much. It would be a great way to make amends and win back support, if cleaning up their act is a sincere objective.

11. **Peter Woit**
February 3, 2019

LMMI,
Glashow in his statement explicitly disavows the early ID and climate denialist articles. The current editorial board has eleven members, most of whom I have a high opinion of from what I know of their work. Two of them that I know a bit about and that I have problems with are exactly the two that Glashow identifies as not disavowing the early two articles. One, Richard Lindzen (who was my instructor in a PDE class at Harvard), could be classified as a climate denialist. The other, David Gelertner, is a computer scientist right-wing ideologue (at one point supposedly a possibility for Trump's science advisor), with the perhaps relevant background of being a victim of the Unabomber.

So, here's the question, if a publication has a generally high-quality editorial board of eleven people, except that two of them one feels have noxious and dangerous views, what should you think of the publication? Should you read it, link to it, write for it? My answer to this is to look at what they are publishing. If they're publishing pieces reflecting views I consider noxious and dangerous, that would be a problem. I see an argument for that in some of their early pieces, I don't see it now.

One can take a different and more purist view, that one should not associate oneself with any organization that has even one, much less two, people on its editorial board with dangerous views. I'm not that sort of purist.

12. **a1**
February 3, 2019

After reading here about the launching of Inference, I did a quick search and posted a 'guess' connecting it somehow to Mont Pelerin Society. I see now that in September 2018 Peter Thiel has been invited to speak at their General yearly meeting. So now I guess that the world is not what it appears to be – it's much more sinister.

13. **Peter Woit**
February 3, 2019

a1,
I'm still not seeing what is "much more sinister" here. We know who the editorial board is and we can see what they are publishing. Since they've been paying their authors and not advertising the source of their money, obviously it has been coming from some private source, likely a wealthy individual. Picking wealthy people at random, a lot of them share the crank Libertarian worries of the "Mont Pelerin Society", which from their website seem to be the dangers of big government, welfare, trade unions, inflation, and "business monopoly". That the money comes from someone (Thiel) who has the views on economics and politics of the Wall Street Journal editorial page seems to me neither surprising nor sinister. The relevant question though is that, since Inference publishes nothing on economics or politics, should one care about the views on those topics of its funder?

14. **a1**
February 3, 2019

I am not disputing any of this but I would just mention that they have a topic "Economics' with seven items and the first was in vol.I ,iss.1, about Piketty. If MPS has views and/or interests outside of economics, e.g. ID or climate scepticisms, that might be worrying.

15. **sphaerenklang**
February 3, 2019

I have no opinion on the merits of Inference's editorial staff. But .. why should we have to guess about where the money for an 'opinion' publication comes from?
Why would anyone be OK with *secretly* funded magazines whose aim appears to be – at least in part – influencing public opinion?

At least we know who owns Fox News …

16. **David Roberts**
February 3, 2019

Regarding money sources in publishing: it's not like people don't publish with Springer because it is owned by members of the Holtzbrinck family with one might imagine certain economic views…

And people publish with Elsevier despite their explicit political moves to remove access to academic research through open access means.

17. **Davide Castelvecchi**
February 3, 2019

Dear Peter,

I guess every time a journalist does his or her homework and reveals something inconvenient — but with obvious public interest — about a powerful person, lots of people will take the side of the powerful person and call it a "takedown". I call it doing a public service.

And just because someone funds the publishing of nice articles about quantum gravity rather than creationism, it does not mean that they don't have an agenda — in particular if the source of the money is an open secret among those in the know. There is already an agenda in giving people 5,000 dollars and then having those people say "geez, that guy gave me 5,000 bucks and I know I am a nice person, so I guess he must not be as bad as they say".

But I guess I am being "moralistic" and that supporting the guys who put toddlers in cages is not bad enough. Where will we draw the line, then? What needs to happen before we stop normalizing this?

18. **Davide Castelvecchi**

February 3, 2019

P.S. I should clarify that I do not equate those who accept money to write for a publication with those who fund it. I myself do not know what I would do if I were offered to write for such a publication — perhaps I would accept, perhaps not. What I do know is that 1) I would want to know where the money comes from, and 2) I would want the readers to know that as well.

19. **Low Math, Meekly Interacting**
February 3, 2019

Oh, I'm not advocating for purism or boycotts of Inference or anything like that. I myself have expressed the opinion that it's perfectly fine to accept Templeton funding if they let the recipients do legitimate science nonetheless, which seems to be the case. Money's fungible.

But I certainly have some sympathy for those who feel, as I do, that "teaching the controversy" crosses a red line. It was always a bogus appeal for intellectual freedom, and belied some truly anti-intellectual motives. Those who have promoted it are rightly viewed with deep suspicion. If Inference as a "whole" wishes to be free of that suspicion entirely, it could do worse than disavow such malignant pseudoscience explicitly as an official editorial policy moving forward, and not just an ostensible practice.

That said, if Inference gets lots of eyeballs, pays well, and lets people publish legitimate, high-quality work, I don't think it should be shunned even if it doesn't take such steps.

20. **Peter Woit**
February 3, 2019

Davide,
The "takedown" is not the revelation that Thiel is funding Inference, which wasn't hard to figure out. Adam Becker's article portrays the publication as a right-wing funded plot to delegitimize science, with the main editor Glashow a patsy who sleazily punished Becker for asking inconvenient questions by panning his book. Which do you think is more plausible: that Glashow panned Becker's book out of such animus, or that Becker's animus towards a publication that panned his book has a lot to do with the Undark piece?

As for Palantir, it went from nothing to yes, a huge dark and powerful force, due to funding from the Obama administration, with Joe Biden one of its promoters.

By the way, I just checked my records: Inference paid me \$250 for the piece I wrote for them. From what I remember, no money was promised when they asked me to write something, I did it assuming I was doing it for free, was a bit surprised when a check came in the mail. No, I'm not defending them because they sent me \$250.

I try and avoid politics on this blog, but I think there's a really important political point here. How to deal with Trumpism and the collapse of US democracy is the most important issue in the world today, but successfully doing so requires understanding how this collapse occurred. It didn't occur because of Peter Thiel: as far as his politics go, he's a garden variety wealthy Libertarian Republican, and such people will always support the Republican candidate. Trump won the election not because wealthy Republicans did what they always do, but because of a highly successful campaign of hit-jobs/takedowns of Hillary Clinton, coming not just from the Right, but from the Left and liberal establishment. Progressive publications were rabidly devoted to taking down Clinton as a tool of Wall Street, compromised by funding of the Clinton Foundation. The New York Times featured every day on the front page hit-job articles about her email server. By the time of the election, she had been taken down, she was the most hated person in the US, by the right, center and left. Opinion polls showed that, by a wide margin, people thought Trump was more ethical than her, and they voted (or didn't come out to vote) accordingly.

I don't think hardly anyone except the Right has learned this lesson from 2016 about how you destroy democracy. Already, as 2020 Democratic candidates start appearing, the takedowns have started. Elizabeth Warren's DNA test and the like is going to dominate the next two years, and we're going to end up reelecting Trump or worse. Demonizing Thiel may make you feel better, but he's not the problem. Self-righteous attacks on our own kind (in this case, the people at Inference who are trying to put out a high-quality publication) are.

21. **Dave Miller**
February 4, 2019

Peter,

Thanks for the link to Chris Fuchs' review. It seems that he did find some (relatively minor) factual errors; however, I disagree with Fuchs that Adam's interpretation of EPR is an error. As in so much concerning QM, this seems to be a matter of… interpretation.

You said:
> There's some similarity between the problems with the book and with the article: both are in some sense an unfair "takedown" (of Bohr and the Copenhagen interpretation in the case of the book, of Inference and its editors in the case of the article).

Well, I think that's what I was also trying to get at. Adam is more of an opinion journalist than an academic, in both the book and in his takedown of Inference. Is that a bad thing? He's pretty obvious, and it seems to me any reader should be able to decide for him or herself if Adam's taking of sides is objectionable.

I do remember from my own student days trying to get Feynman interested in foundational questions in QM: he did not literally say "Shut up and calculate," but there was a sense that we should sort of accept the Copenhagen perspectives as the only valid inspiration… and then forget all about it and just start doing calculations.

I.e., questioning Copenhagen back in the mid-'70s was discouraged. You are slightly my junior in age: did you have a different experience?

As to the Mont Pelerin connection, there is now an extensive historical literature explaining that the world we live in today was created by the Mont Pelerin Society (most recently, Quinn Slobodian's *Globalists: The End of Empire and the Birth of Neoliberalism*).

As it happens, I was friendly with several members of Mont Pelerin back around 1980 when I was at Stanford and had a chance to meet over a dozen of them (at the time, the Cato Institute and the Institute for Humane Studies were in the Bay Area, and both had some ties to Stanford).

Without going into details, I will just say that Adam's book is certainly more accurate than most of the historians' writings on Mont Pelerin! The over-wrought conspiracy theories about Mont Pelerin may have a veneer of academic respectability, but they have very little connection to the real world. The Bushes, the Clintons, the neocons, the Democratic Leadership Council, the Moral Majority, and a host of other groups and organizations have had a greater effect on the USA than Mont Pelerin.

Dave

22. **Peter Woit**
February 4, 2019

Dave Miller,

I started studying QM (haven't finished yet…) in the mid-70s, and don't recall ever hearing that one should "shut up and calculate" and not think about QM foundations. I spent a fair amount of time doing this (studying foundations of QM), came to the conclusion on my own that this was a waste of time. This was the era of the advent of the Standard Model, so I think the attitude of most theorists was "why would you spend your time thinking about those old questions where there's nothing new, when the SM has just given us all sorts of new and exciting things to think about?". The topics Becker wants to push were already old news at that time (Bohmian mechanics [1952], Many-Worlds [1957], Bell's Theorem [1964]).

My problem with Becker is a combination of disliking agenda-driven takedowns in general, and not agreeing with his particular agenda. In the case of the book, see my review. I don't think the world would be a better place if what's in the current QM textbooks about Copenhagen was replaced by material out of Becker's book. In the case of Inference, I don't think the world will be a better place if Becker's agenda to discredit the publication is successful. Discrediting the place that published a bad review of his book will be good for him, but convincing people not to read or write for one of a limited number of outlets for high-quality writing about math and physics won't make the world a better place.

23. **Davide Castelvecchi**
February 4, 2019

Peter — I happen to agree with you that the New York Times' behavior during the 2016 campaign was reprehensible, and I have said so many times on social media (even attracting the ires of some NYT colleagues).

Still, from there it is a big leap to conclude that the NYT was the main cause of Trump's electoral college victory. To begin with, I would guess that cable news channels had more impact than the NYT in making Clinton look bad in the eyes of the average voter. Probably a lot of Bernie Bros on social media helped, too — although how many of those were actually Russian bots (or real people misled by Russian meddling) is still unclear to me. Voter suppression certainly played another big role, and perhaps hacked voting machines did, too (although Putin would have been smart enough to use that in moderation, so the results would not deviate too much from exit polls). I think there are a number of different causes and perhaps none of them was decisive by itself.

I do not know whether it is true or not that Glashow slammed Becker's book out of revenge. Perhaps you know Glashow well; I have never had the pleasure. But I don't buy the argument "he is the one with the Nobel Prize, so it is self-evident that he would be incapable of pettiness."

What I do know for sure is that I value the information that Becker provided to me as a reader and as a journalist, and that therefore his piece was worth doing.

But seriously now, Adam Becker as a destroyer of democracy? He did not decide to work on the exposé because of the negative review of his book — that came later. He is one freelancer who stuck his neck out in the public interest, and is risking a lot because of it. (And by the way, so are the editors of Undark.) But somehow Becker is the problem, while the guy who enthusiastically contributed to electing Trump is not?

24. **Peter Woit**
February 4, 2019

Davide,
My comments about Trump and politics were in response to yours about Thiel and his support of our awful current president. That support, perhaps rightly so, has caused Thiel's name to become a swear-word in many quarters, including most of academia. Because of this, I don't thinking going after a Thiel-funded project is sticking one's neck out, quite the opposite for the audience Becker writes for.

Sure, Thiel is known for supporting lawsuits that led to court decisions against Gawker and put them out of business, but I'm afraid that there I'm kind of on his side. Gawker's whole business model was based on the unfair takedown/hit-job humiliation of their targets, exactly the sort of thing that has destroyed our democracy. The world is a better place without them. Yes, Becker and Undark before publishing this piece had to be sure to meet the very low legal standard for not libeling a public figure, but I don't think that's really onerous.

No, Becker is not responsible for the destruction of democracy in the US, but I'm dead serious that we're screwed if our side won't get over its love affair with the unfair takedown of our own people. A lot of complicated factors led to the 2016 debacle, but Clinton lost for exactly one reason: people on the center and left irrationally hated her. This wasn't because of the Russians or Fox News, but because they were fed a continuous diet of unfair stories by the "liberal" and Left media about how personally unethical and unlikable she was.

If Becker wants to takedown Thiel, or even the off-beat right-wing crowd that started Inference (Berlinski/Lindzen/Gelertner), that's fine with me. But, on the question of the current state of the publication and its editors, I think the Becker piece was highly unfair towards people doing good work. This is based on actually reading Becker's book and Glashow's review, as well as reading many of the articles Inference has published in recent years. Before applauding the latest takedown, you and others should first do the same.

25. **Tyler Hampton**
February 4, 2019

Hey Peter,

This is my first time commenting here, but I have followed the blog sporadically for years and I enjoy it very much.

I think Adam Becker's piece is highly misleading and unfair, for the following reasons:

1) Adam dismisses a review I wrote for Inference of Dan Tawfik's laboratory, not on the basis of its actual content, but on the basis that I was a 'tennis instructor' reviewing a complex biological subject. I admit I am not an expert. But I find it unfair that Adam does not engage the material on its own merits, and does notmention that I do have a degree in biology. (Yes, I have only an undergraduate degree, to be sure, but Adam writes as if I had no relevant background at all.) Adam also does not mention that Dan Tawfik liked the review well enough to link to it on his own website.

2) Glaringly, the very article that Adam Becker claims attacks evolution and upholds creationism in fact both affirms evolution and denies a supernatural biological creation. The relevant article is Michael Denton's "Evolution: A Theory in Crisis Revisited". I know, the title sounds bad. But if you actually read all three parts of it, you will see that Denton is criticizing a particular *mechanism* of evolution, not the *fact* of evolution. This is most apparent in part 3, where Denton asks, "If life is a natural phenomenon, how might its forms have been actualized? How can one type lead to another?"

Denton's answer is not "God did it", or "it can't be done naturally", although Becker's article would lead you to believe that. Instead, Denton defers to ordinary mutational events (albeit, mutations in a biological possibility space that is nicely ordered). Denton states in answer to his question of how evolutionary transitions are accomplished, "As the creation of atoms in the stars depended on a highly fortuitous nuclear energy pathway, so it is possible to imagine analogous minimum energy pathways at all levels of the organic hierarchy, arranged so that the distances between types are massively reduced in ontogenetic space. One might suppose that gene functions are clustered in the space of all possible genes, rather than scattered widely."

What Denton is suggesting is that, in a hypothetical major evolutionary transition, as gene 'x' mutates to gene 'y' with one single mutation, a phenotype 'A' may mutate in a very sharp and big way to a very different phenotype 'B'. It may not take a thousand mutations to get a big change, only a few. Denton may or may not be right on the details of the organization and distribution of phenotypes in the space of all possible genes, but his theory is not creationism in any sense, or an attack on 'evolution' or biological evolution founded on the laws of physics.

3) Adam Becker rather unfairly patches together quotes in J Scott Turner's essay. For example, the full line from which Adam Becker quotes is "evolution is driven not by natural selection, but by extended homeostasis." The deletion of the last part by Adam makes it sound more ridiculous than it is. Turner believes in evolution, natural selection, and Sewall Wright's fitness landscapes, and Turner's essay argues along the lines of the Extended Evolutionary Synthesis, which has much mainstream support in evolutionary biology. (Turner does however make some woefully ambiguous and suggestive remarks about cognition in swarm intelligence, but 99% of his article is good, and the remaining 1% is not overtly wrong, just uncomfortably ambiguous).

Overall, I conclude that Adam Becker's piece is misleading and parts of it should be retracted.

26. **Klavs Hansen**
February 5, 2019

Peter,
I would not be so sure that creationism has left the pages of Inference; see for example the recent 'An Open Letter to My Colleagues' by James Tour (the precise reference is not available to me, sorry, it seems the website is blocked here in China).
We should not be blind to the fact that serious scientists can potentially lend a lot of prestige to a website with a more or less hidden agenda it may want to push. Stranger things have happened before. It is then up to everybody to decide if setting things straight in their own field is worth the price, because a price will be paid.

27. **Tyler Hampton**
February 5, 2019

Klavs,

James Tour is a qualified synthetic chemist. He proposes *no* creationist scenario in Inference, and merely criticizes the lack of rigor in the field of prebiotic evolution, a discipline which is notorious for it. This is not my opinion: it is the opinion of qualified experts in the field of origin of life studies, like Steven Benner and the late Robert Shapiro. Look at Benner's 2018 Nature article about the history of the phrase 'prebiotically plausible': https://www.nature.com/articles/s41467-018-07274-y
See also Robert Shapiro's article here about 'plausible' prebiotic scenarios: https://www.scientificamerican.com/article/a-simpler-origin-for-life/

James Tour and Robert Shapiro criticizing existing prebiotic scenarios does not make them creationist any more than Peter Woit criticizing existing string theory scenarios makes him a creationist. To say otherwise is false, and if uncorrected, slanderous. I know you are for fair play, liberal values, etc. Then why are you unfairly generalizing and labeling people?

28. **Peter Woit**
February 5, 2019

Tyler Hampton/Klavs Hansen,

Thanks for your comments. I hadn't seen the James Tour piece and it's very odd that Becker doesn't mention it or link to it, since it seems to me the strongest evidence for the case that Inference still has an issue with the Berlinski/Gelertner editorial faction and their views of evolution. Tyler Hampton is correct that Tour is not explicitly making a case for creationism, but he is clearly a sort of skeptic about Darwinism. For a detailed explanation in his own words, see here
https://www.jmtour.com/personal-topics/evolution-creation/

Clicking through the links in the Becker piece trying to find a reference to Tour did lead me to a lot of material about Thiel's views on evolution and climate change. With the what seems to me misleading text "Thiel himself has expressed doubts about the settled science of evolution and climate change", Becker links to pieces that quote Thiel thusly:

About evolution: the New Yorker quote is "I think it's true." He does go on to "but it's also possible that it's missing a lot of things, and it's possible it's not the most important thing." There's also a Washington Post quote: "I believe that evolution is a true account of nature, but I think we should try to escape it or transcend it in our society."

About global warming: "probably true"

The two articles are worth reading if you want to get some idea of what Thiel actually thinks and thus some idea of his motivation for funding Inference. The way Becker paints him as a climate denialist and opponent of the theory of evolution is quite unfair.

I continue to feel that Becker's piece was a hit job. A serious and fair article about Thiel and Inference would make some attempt to explain Thiel's actual views and likely motivation, not caricature them in incendiary ways. It would also examine the interesting question of how the publication has evolved from the initial Berlinski/Lindzen/Gelertner version to the more recent version that seems to be more led by Glashow.

29. **David Berlinski**
February 5, 2019

Has it come to this: That OOL research is now beyond criticism? James Tour is an eminence in synthetic organic chemistry. He has published more than six hundred peer reviewed articles, and holds one hundred and twenty patents; his work has elicited over 77,000 citations. His criticisms of OOL research in the pages of Inference have been precise, detailed, and informed; and in each of his essays, he has gone out of his way explicitly to deny that he proposes to explain the chemistry of life by an appeal to religious sentiment. His is a call for a better scientific understanding. He has said as much again and again and again. Anyone who imagines that Tour is wrong in point of chemistry, or logic, for that matter, is free to say so in the pages of Inference. Arguments by irrelevant insinuation are now a commonplace of social and political life. Should they be a part of scientific life as well? The question, I am afraid, is rhetorical. Such arguments have already become commonplace. This is hardly evidence in their favour.

30. **Peter Woit**
February 5, 2019

David Berlinski,

Thanks for your comment. By the way, for those like me who might initially wonder what "OOL" is, I gather it is "Origins of Life".

31. **Geoffrey Dixon**
February 5, 2019

The original raison d'être of this blog, in my understanding, was to point out the failings of a politically powerful but narrow-minded theoretical physics elite for whom "tribal affiliation" trumped open-minded flexibility in every instance. The intent of the fusillade of critiques was intended to unsettle – to dent the barrier of complaisance surrounding mainstream (i.e., stringy) theory. It succeeded. Theoretical physics is now more chaotic than it was in the very recent past, and creativity thrives on the border of order and chaos. I approve, but then, I approve of all efforts to discommode any smug collective intent on maintaining order, even when that order is broadly detrimental – but also when the order may be broadly favorable. Some chaos is necessary. How ironic, then, that when this blog and its comments very infrequently stray into opinions on actual politics and world affairs (e.g., populism bad; Clinton good), there seems to be an underlying understanding that there is only one correct way of thinking on such things, and no flexibility will be brooked. Even to be undecided is to be deemed wrong. But that's academia for you, and more generally human nature. I should just feel fortunate that I live in a time and place where failing to conform does not lead inexorably to being drawn and quartered – actually drawn and quartered, as opposed to metaphorically. And I do – feel fortunate, that is. I think.

32. **Peter Woit**
February 5, 2019

Geoffrey Dixon (and all)
I'll revert to my usual policy of suppressing political discussion here, having to deal in even a limited way with the heat surrounding discussions of evolution and climate change is bad enough. I thought it was important to answer Davide and give some context to my intense dislike of the currently popular phenomenon of the (unfair) call-out/takedown/hitjob, which I think is incredibly destructive.

If it's any consolation Geoffrey, about politics I actually agree with you that there's a huge problem (in academia and elsewhere) of rigid thinking and rabid desire to destroy others. Where we likely disagree is that I don't really care if people like this on the left and center want to spend their time at war with their analogs on the right (who, by giving us Trump have done something truly awful to our society). What really bothers me is that my friends on the left and center often find it's a lot easier and more satisfying to go after their own, or people who are minding their own business trying to do something worthwhile.

Enough of all this, though, back to the peaceful issues of evolution and climate change…

33. **jjj**
February 5, 2019

Inference is not coming off well here.
Besides admitting the "Evolution in crisis" is just a clickbait title,
this sentence makes no sense:

"Inference commissioned Sheldon Glashow to review Becker's book in the spring of 2018, well before Becker was known to Inference."

34. **Peter Woit**
February 5, 2019

jjj,
Note that the "Evolution in Crisis" article was published back in 2014 in their first issue, and Glashow (who it appears is now their lead editor) agrees it is "deserving of censure".

It's pretty clear that the sentence about Becker is just designed to explain that the decision to have Glashow review Becker's book was made at a time (spring 2018) they'd had no contact with Becker (his book came out in March). In his piece, Becker gives summer 2018 as when he first heard from Inference (an invitation to write a piece) and mid-September as when he talked to Glashow. The review was published Oct. 19. Becker's take on all this is:
"It's odd that Inference would want me to write for them shortly after my book was published, then decide to pan it a few months later, after they knew I was investigating them. Even odder: A couple of weeks later, they asked me to write a response — again for a fine fee — to Glashow's review. I declined. "

I don't understand what's so odd about them giving him an opportunity to respond to the negative Glashow review, especially since their editorial model is to publish pieces and responses to those pieces. I also think the implication that Glashow decided to trash his book because he was asking questions about Inference is ridiculous. If Glashow was concerned about what he might write, the obvious thing to do would be to write a laudatory review. Based on personal experience, authors tend to hold much higher opinions of publications and reviewers that say nice things about their books.

35. **Anonymous**
February 5, 2019

@Dave Miller: Richard Lindzen has swallowed the kool-aid. I had a conversation with him where he brought up the effects of DDT, and he echoed the anti-science, anti-global-warming sites who said that DDT was harmless and it was banned because a lot of environmentalists baselessly agitated against it. I was incredibly disappointed with him, as I had believed his pretense of being a scientifically-motivated global warming skeptic.

I didn't think I could judge the claims of global warming personally, as the subject is too complicated. But the DDT claim is totally wrong. My father was a falconer, so I have actually researched this. Rachel Carson in *Silent Spring* overstated the effects of DDT (it only harms a large percentage of bird species; a reasonable percentage of bird species can tolerate it, and thus some studies on specific species of birds concluded it had no effects), but there is unquestionable scientific evidence that it was well on the way to driving bald eagles and peregrine falcons to extinction, and also had a significant negative impact on songbirds.

And don't ask me why the anti-global-warming propagandists picked DDT to include on their handful of examples of why scientists were totally wrong.

36. **S**
February 6, 2019

@Anonymous,

There's a pattern that I think is pretty common where a scientist comes to skepticism about some widely held belief (sometimes reasonably and sometimes not — that seems to me not to be crucially relevant to the pattern); and he says so, and then gets attacked; and the whole experience — doubt and reaction to doubt — so shakes his faith in the scientific establishment that he kind of loses his legs and starts questioning all kinds of things (almost always including things that should not be doubted — irrespective of what the status of the first thing was).

Serge Lang comes to mind here; I'm pretty sure I've seen others, although they do *not* come to mind at the moment. Particular kudos are due to Peter for *not* being an example.

My only point is that I think that skeptics' position on their first hobby horse should be evaluated on its own terms, assuming they were ever serious scientists. Their later openness to crackpottery seems a pretty frequent side-effect and only weakly correlated to initial correctness.

37. **Peter Woit**
February 6, 2019

S,

Thanks. Over the years I've repeatedly run into the phenomenon of string theory or multiverse proponents characterizing criticism of string theory or the multiverse as analogous to criticizing the theory of evolution. No, this hasn't caused me to question the theory of evolution, but it has been an eye-opening experience about how even the smartest and well-meaning of people can let their tribal affiliations cause them to adopt really bad arguments. Serge Lang is a peculiar case, but the phenomenon you describe may correspond to people who find reason to doubt a tenet of their tribe being led by this to change tribal affiliation (not paying much attention to the fact that the new tribe has even more dubious beliefs…).

All,
Just stop with the submission (from both sides) of rants about the perfidy of those of the opposite tribe. If people have reasoned arguments about the facts of the Inference story that's one thing, but if you just want to vent about how bad the other side is, do it elsewhere.

38. **Rollo Burgess**
February 8, 2019

Well I only discovered Inference relatively recently (perhaps via a link here now that I think about it) and I think that it is a very interesting periodical with some great essays and reviews. It is a welcome addition to the mostly tawdry wasteland of the internet.

I'm not sure why we need to get into all this conspiracy theorizing etc. It should be a fairly safe assumption that most people reading on here regularly or reading Inference are reasonably bright and well-educated. If there is the odd paper that one disagrees with, dislikes, or even disapproves of, one moves on. Having a bit of critical nous such as to be able to do this is one of the key purposes of an education!

I couldn't care less who funds it: it sounds like one of the less stupid things that Thiel probably does with his money; I don't need protecting from ideas like a feeble snowflake and nor do most of you, I'm sure!

39. **Low Math, Meekly Interacting**
February 8, 2019

Most people who read primary literature in peer-reviewed journals are typically very well educated in the field of interest. But if published work is funded by, say, a petrochemical or pharmaceutical company, it is a grave breach of ethics if that fact is not divulged, and may involve serious legal breaches as well. The consequences for not doing so are often dire. The reasons are a matter of extensive public record. Expecting scrupulous disclosure is entirely justifiable, regardless of the integrity of the recipients or their work.

Journalistic integrity is preserved by disclosing funding sources. I'm a big fan of NPR. They tell me when a story is made possible by a grant from Exxon or what-have-you. If they cover a story about, say, an oil spill, they take pains to disclose when sponsorship might raise concern about conflicts of interest.

It's not too much to ask.

Maybe undisclosed funding by Thiel is irrelevant. Why not make it public anyway, however easy it is to figure out? What purpose is served by not doing so? I don't get that at all. And I don't understand why Glashow seems so blase about it. It's standard practice in publication to be transparent about funding, often to avoid the very thing Glashow seems to feel is unreasonable, i.e. suspicion. It seems some of Inference's contributors would rather have learned certain facts from someone other than Becker. Are they being unreasonable?

40. **Peter Woit**
February 8, 2019

LMMI,
The comparison to disclosure of research funding by pharmaceutical or petrochemical companies is a bit of a stretch… I haven't seen even a hint of an accusation that Inference's editorial decisions have anything to do with Thiel's financial interests.

What people are suspicious about here is whether the journal has hidden ideological, not financial interests. When the journal first appeared I think this was a very reasonable suspicion: the problem wasn't really that the source of funding was unknown, much more serious was that the editors were unknown. And, if you did find out that they were Berlinski/Lindzen/Gelertner, you would have good reason to worry that the journal would reflect some of their shared ideology.

At this point though, the editors are known, and everyone is free to make what they will of them and of the choices they have been making about what to publish. I don't see the point of judging a publication on the source of its money rather than its editorial decisions.

I realized that, for most of the things I read online, I have no idea how they are funded. Picking one at random (Slate), I just went to their site and spent a while trying to figure out where their money comes from. No luck.

41. **Low Math, Meekly Interacting**
February 8, 2019

With Slate it seems to be the usual adverts, though I could be wrong. In fact, you do have an excellent point in that I don't really know. I should probably ask the question more often. Then again, Slate is kind of the media equivalent of junk food for me, so it's difficult to care as much as I would about, say, Quanta, which I'm thankful exists.

Mostly I sincerely think it's a case of the "cover up" being far worse than the insinuated crime, at least from a PR perspective for Inference. Plausibly, no one was trying to hide anything they felt was untoward. Which is kind of naive, I think, if you're Peter Thiel.

42. **Davide Castelvecchi**
February 9, 2019

Peter,

I am afraid you are missing the point in your response to LMMI. You said "I haven't seen even a hint of an accusation that Inference's editorial decisions have anything to do with Thiel's financial interests." The point of disclosing sources of funding is not that it's ok if you don't disclose your COI as long as you are not doing anything biased. (Also, I am more worried about Thiel's ideological interests than his financial ones.)

And I completely disagree that for a website like Inference, disclosure is less important than it is for researchers, so that the comparison is "a stretch". Would we be ok with Putin's propaganda outlets like RT hiding their sources of funding just because they do not publish peer-reviewed research? Is potentially subverting peace and democracy less grave a threat than subverting scientific integrity? It's not against any law to be secretive about a website you publish, and I believe one has the right to do that, but then one doesn't get to be offended if people think it's creepy — and say so.

It's also a bit circular to say "Oh everyone, relax, we know where Inference's money comes from now — so Becker's piece was hit job and unnecessary." I know where Inference's money comes from _precisely_ thanks to Becker. Otherwise I would not be here debating this in the first place.

43. **Anonyrat**
February 9, 2019

Slate.com is owned by the same holding company that owns the Washington Post. Of course that doesn't tell us how they are funded. They claim to be advertisement-funded in the US; and they have a paywall for non-US readers.

44. **Peter Woit**
February 9, 2019

Davide,
I'm afraid comparison to RT and the Russian campaign to subvert democracy is even more of a stretch than the comparison to drug companies subverting medical research.

Look, I think the story of Inference and Thiel's role in it is an interesting one, well worth writing about and of public interest. I even agree that the way it started out, with editors and funding hidden and sharing some dubious ideological goals, was creepy. But I also think Becker's piece was a hit job, and his book about QM had serious problems (one of which was that it was in some ways a hit job…). If you want to get outraged about an example of not so good ethical standards in journalism, publishing a piece aimed at discrediting a publication and its editors written by someone whose book they (for good reason) trashed a few months earlier doesn't pass a basic smell test.

45. **Peter Woit**
February 9, 2019

Anonyrat,
I don't think that's it's true that Slate currently has the same ownership (Bezos) as the Washington Post. Surely the identity of their current owners isn't hard to find out, but my point was just that there's nothing I could find anywhere on their website about it.

46. **David**
February 9, 2019

I scrolled to the bottom of Slate.com, and at the lower right found "Slate is published by The Slate Group, a Graham Holdings Company." There's lots of information about them at GHCO.com

Regarding Inference, they could move beyond their history of publishing pseudo-scientific nonsense but only if they demonstrate ongoing integrity. A clear denunciation of their past practice and disclosure of funding seem to be minimum requirements.

47. **Peter Woit**
February 9, 2019

David,
And what if the current editors of Inference don't agree that they need to issue a statement beyond the one they already issued, because that one doesn't meet your standards? Are you not going to read their articles? Are you not going to write for them if asked? The same questions are there for any publication at any time, what makes anything different about this publication? Everyone can look for themselves at what they are publishing and decide what they think of it. Is it worth reading? Is it of generally high quality or not? Those seem to me the relevant factors, but everyone can and must make their own decisions about this sort of thing every day.

48. **Immaterial**
February 10, 2019

An irrelevant remark. You impart knowledge, not a secret. You reveal a secret.

Where in the World are SUSY and WIMPs?

February 10, 2019
Categories: Uncategorized

Back in 2017, after it had already become clear that negative LHC results about SUSY and WIMPs had falsified theorist's most popular scenarios for how to extend the Standard Model, Nima Arkani-Hamed gave a summer school talk to students with the title *Where in the World are SUSY & WIMPS?*, which I discussed here. At the time I was encouraged that while he was still promoting SUSY and the landscape (in the split SUSY variant), at least he seemed to be arguing that the lesson to be drawn might be that the whole SUSY-GUT business was a mistake:

> The disadvantage to the trajectory of going with what works and then changing a little and changing a little is that you might just be in the basin of attraction of the wrong idea from the start and then you'll just stay there for ever.

A few weeks ago in Princeton, at a PCTS workshop on Dark Matter, he gave an updated version of the same talk. Much of it was the same material about how split SUSY is the best idea still standing. Unfortunately, at the end (1:09) he seems to now have changed his mind and be arguing that the best thing for theorists to do is to keep tweaking the models that failed at the LHC:

> You could very justifiably say "look, you're just continuing to make excuses for a paradigm that failed", OK, and I would say that's true, and even the paradigm most of your advisors love was already an excuse for the failure of non-supersymmetric GUTs before that.
>
> That is a perfectly decent attitude to take, but I would like to at least tell you that you should study some of the history of physics. This very, very, very rarely happens, that some idea that seems basically right is just crap and wrong, It's probably mostly right with a tweak or some reinterpretation. You'd have to go back over…, I don't know how far you'd have to go back, even Ptolemy wasn't so far from wrong…
>
> These are two different attitudes towards connecting theory and experiment. If you like, more the theory egocentered attitude, or the just more explore from the bottom up attitude, they're both perfectly good attitudes, we'll see which is more fruitful in the end. If you take the more top-down attitude, just keep fixing things a little bit.

If you had to pick the single most influential theorist out there on these issues, it would probably be Arkani-Hamed. This kind of refusal to face reality is I think a significant factor in what has caused Sabine Hossenfelder to go on her anti-new-collider campaign. While I disagree with her and would like to see a new collider project, the prospect of having to spend the decades of my golden years listening to the argument "we were always right about SUSY, it just needs a tweak, and we'll see it at the FCC" is almost enough to make me change my mind…

Update: Today Ethan Siegel at Forbes has Why Supersymmetry May Be The Greatest Failed Prediction in Particle Physics History.

Comments

1. **Jon Forrest**
   February 10, 2019

   Trivial typo:

   "at lease he seemed to be arguing" ->
   "at least he seemed to be arguing"

2. **Peter Woit**
   February 10, 2019

   Jon Forrest,
   Thanks! Fixed.

3. **David Levitt**
   February 10, 2019

   I have followed your blog since its beginning (along with reading your book). I am a biophysicist with only an amateur's interest in high energy physics, but I found it fascinating that some relative nobody was claiming that the leading theoretical physicists were going down dead ends, lured by some sort of mathematical siren song. In your many posts you discussed in detail what was wrong with both string theory and supersymmetry. Although I could not judge the correctness of your explanations in detail, what I found most convincing was that you encouraged critical comments in your forum and I never saw a serious or convincing rebuttal of your arguments. It should be emphasized, that you stated this opinion long before the experimental results were available that now seem to support you. I do not think the hep community gives you the appropriate credit. Not only were you brave enough to stick your neck out, but, apparently, you were right. This account suggests to me that there are two distinctive types of theoretical physicists' skills or abilities. One is a great facility with the mathematical formalism, etc. that has led these "brilliant" physicists down dead ends. The other is a physical intuition that gave you the confidence to shout out that the emperor had no clothes. The fact that these brilliant physicists are not willing to give up these ideas even in the face of experimental evidence is just more confirmation of how strong a lure the mathematical ideas must be, along with their lack of the appropriate physical intuition.

4. **Peter Woit**
   February 10, 2019

   David Levitt,
   Thanks, but I can't take so much credit, and the conclusions you draw are backwards.

   First of all, skepticism about SUSY and string theory has always been fairly widespread among theorists. A sizable fraction of theorists never have worked on SUSY or string theory and for most such physicists, one reason was skepticism that these highly speculative models were all that promising. All I can take credit for is being more vocal and obnoxious with my skepticism than most.

   Secondly, one of the main reasons for the solidity of my skepticism has always been that I'm not much interested in "physical intuition", but am quite devoted to the idea that, at a deep level, great mathematical ideas and great ideas about physics go together. The problem with SUSY and string theory has been that while you can motivate them starting with some deep mathematical ideas, you find that these don't give you the right physics. You need to add ugly mathematical structure to them (SUSY breaking in the case of SUSY, hidden extra dimensions in the case of string theory) to get models that aren't obviously ruled out by experiment. So, my skepticism is not based on any "physical intuition" about problems with superpartners or strings. It is based on looking in detail at the actual SUSY/string models that are supposed to unify physics and judging that they are, mathematically, hideously ugly. As such, without strong experimental evidence in their favor, I'm going to be skeptical, and the complete lack of any experimental evidence seemed conclusive, even pre-LHC.

5. **Bob**
February 10, 2019

"It is based on looking in detail at the actual SUSY/string models that are supposed to unify physics and judging that they are, mathematically, hideously ugly."

Isn't this exactly the opposite what Sabine advocates in her book ?

6. **Peter Woit**
February 10, 2019

Bob,
I don't know about "exact opposite", but she and I have quite different takes on the role of mathematics here. I don't want to discuss her views here, what's on topic is Arkani-Hamed, and I don't think his problem is being "Lost in Math". In his talk, he does a good job of locating the start of all this trouble in the GUT hypothesis. I don't understand why he doesn't just draw the conclusion that should be abandoned.

7. **Sabine Hossenfelder**
February 10, 2019

Yes, this kind of refusal is the reason why theorists in these fields have not come up with useful predictions for decades. It illustrates that the self-correction in this community is not working. (Esp disturbing they still do it after they've been called out on it so many times.)

The absence of reliable predictions for new discoveries at the next larger collider means that such a machine is presently not a promising investment. There are better avenues to pursue. It adds to this that we have not seen much progress in collider technology since the 1990, yet maybe in 20 years we'll have plasma wakefield accelerators or high-T superconductors. Banking money on the FCC right now does not make any sense.

You call it "my anti-new-collider campaign" but I think I am just sharing information that particle physicists would like to keep for themselves.

8. **Ayloka**
February 11, 2019

"This very, very, very rarely happens, that some idea that seems basically right is just crap and wrong, It's probably mostly right with a tweak or some reinterpretation. You'd have to go back over…, I don't know how far you'd have to go back, even Ptolemy wasn't so far from wrong…"

You don't have to go back that far, surely? Kelvin's theory of vortex atoms, for instance.

9. **Marshall Eubanks**
February 11, 2019

"That is a perfectly decent attitude to take, but I would like to at least tell you that you should study some of the history of physics. This very, very, very rarely happens, that some idea that seems basically right is just crap and wrong, It's probably mostly right with a tweak or some reinterpretation."

Say what? The luminiferous aether is mostly right? Quantum mechanics is just a reinterpretation of Newtonian physics? Steady state cosmology just needed some tweaks? Phlogiston was abandoned too soon? What history of physics is he talking about? How could you say something like this in an open session and not have a line of people challenging you at Q&A?

10. **quentin ruyant**
February 11, 2019

I'd like to have references from historians of science to backup the claim that it "very, very rarely happens". I think it isn't true. Now if he thinks Ptolemy wasn't "far from wrong" once suitably "reinterpreted", then ok: *by these standards* physicists were rarely wrong. But is it the kind of standards we want? Keep tweaking the Ptolemaic model until it works? That's the question. I really doubt extensions of the Ptolemaic model would have been fruitful and yield new predictions for very long, if at all.

That's a side issue, but the idea of top-down vs bottom-up seems wrong-headed as well. To my knowledge, big paradigm change (such as relativity theory) were not "bottom-up" at all, quite the contrary: they were attempts to solve theoretical tensions. And conservative extensions of existing paradigms are very often "bottom-up", that is, take new data/phenomena and make it fit in a model of the theory.

11. **MartinB**
February 11, 2019

"This very, very, very rarely happens, that some idea that seems basically right is just crap and wrong"
Sure, we are all totally aware that modern physics deals with the aether, we all know that there cannot be a five-fold symmetry in a crystal, transition temperatures in superconductors are limited to below 40K and of course there is no such thing as graphene or dark energy or dark matter…

12. **RGT**
February 11, 2019

Tweak is generally accepted to mean a minor adjustment. On this basis the jump from for example Newtonian Mechanics to General Relativity could hardly be considered a tweak.
History they say – and this includes the history of science – is written by the victors. Theories and ideas which however good they seem at an instant don't stand the test of time are either lost to history or reported as 'failed' attempts on the path to our current understanding. Some ideas, however 'correct' they appear will, regardless of how much they are tweaked (in the sense we normally use the term), never produce the results we hope for. The difficulty is when and how, having started with a 'good idea' , one decides to give up tweaking and accept the initial premise as a lost cause.
It will always be easier to drop an unpromising line of enquiry if there are a number of alternative starting points. If a theorist is making little/no progress they have three alternatives – construct a new starting point (often requires a conceptual leap), keep tweaking and hope to make a breakthrough, give up (difficult). If a researcher is convinced that their starting point is the only 'good idea' available it's understandable that they will keep tweaking in the hope of making a breakthrough. On this basis A-H's stance, if not the reasons for it, makes some sort of sense. In the absence of an alternative plug on and hope for the best. The arguments against this approach have been well exercised in this blog and elsewhere by Dr Hossenfelder and others. Will there be a consensus any time soon?

13. **Peter Woit**
February 11, 2019

There seems to be a consensus that Arkani-Hamed's argument from history doesn't hold up…

In case you haven't watched the whole thing, or seen other of his talks like this, I think the most striking thing is the stream of consciousness, access to the id aspect of it. Here and in the previous version, he frames his support for split SUSY as "this is what I would say if woken up in the middle of the night with a gun to my head". This is highly peculiar, asking for psychoanalysis rather than rational evaluation. It's a fascinating insight into where people like him who have spent their careers on failed BSM ideas are right now.

If you just look at his rational argument, he locates the fundamental problem at the GUT scenario, and the way, after proton decay experiments shot down the first GUTs, people decided to "tweak" the GUT idea by making it supersymmetric. I remember the first time (1983? from Jon Bagger?) I heard the argument that the negative proton decay results coming in meant that SUSY-GUTs were the way to go. This made no sense to me: non-SUSY GUTs already had problems with too many parameters (they need a whole new Higgs sector to break the GUT symmetry), why was it a good idea to move to theories with even more unobserved fields and undetermined parameters? If Arkani-Hamed's argument was following a rational path, I think it would be an argument for why the GUT scenario is a mistake. It's disturbing that since the 2017 version he seems to be retreating from following the logic of his own argument.

14. **Amitabh Lath**
February 11, 2019

Stop picking on Nima. You all are doing the internet thing of taking one statement in an hour talk and ganging up.

I saw the video and what I see is a smart guy who is grappling with the LHC and direct detection null results. He was asked to defend SUSY to a room of bright young physicists. He goes into the history of how and why SUSY models came about, and his opinion is that the new theories will be arrived at adiabatically rather than from a paradigm shift. Yes he mentions 100 TeV colliders but also spends a lot of time on the electron EDM.

Since the unspoken baseline for all these discussions seems to be that the LHC has demolished all BSM forevermore, here are the actual limits (from CMS, ATLAS is probably similar):

http://cms-results.web.cern.ch/cms-results/public-results/figures/SUSY_results_2016-2017.png
https://twiki.cern.ch/twiki/pub/CMSPublic/SummaryPlotsEXO13TeV/EXO_barchart_Jan19.svg

Note that in certain sectors (EWK gauginos, some scalar DM) the limits are less than 1 TeV. Sometimes much less.

15. **Peter Woit**
February 11, 2019

Amit,

"He was asked to defend SUSY to a room of bright young physicists."

My question is why would anyone ask him to do this, and why would he agree? His talk took the point of view that the naturalness argument for SUSY is now basically dead, so what remains of the case for SUSY is
1. Better coupling constant unification in GUTs
2. WIMP candidate
Is it even conceivable that anyone in that room hadn't heard these arguments before? Maybe there was an interesting talk to be given about exactly what the LHC WIMP limits are, but that would have been "bottom-up", which he even explains is not what he does.

Again, the truly weird thing about the talk is that it gives an accurate account of the SUSY history, then draws the opposite of the obvious conclusion.

1. 1974: GUT proposal, testable by proton decay
2. 1983: Epic Fail of GUT proposal, no proton decay. Deal with failure not by abandoning GUTs, but by going to more complicated SUSY GUTs, with SUSY broken at the electroweak scale, testable at Fermilab, LEP and the LHC
3. 1990s-Now: Epic Fail of SUSY GUTs with SUSY broken at the electroweak scale.

An idea with no evidence for it fails miserably, you "tweak" it with new aspects for which you have no evidence, and the "tweaked" version fails miserably also. What lesson to draw?

1. The original idea was just wrong, beyond fixability by tweaking.
2. Keep on tweaking.

2. seems to me the wrong lesson, but even if you want to go that way, invoking the history of science as showing this is always the way to deal with consecutive multiple failures of an idea is just bizarre.

16. **Aurélien Bellanger**
February 11, 2019

The part about looking at the history of physics is truly unbelievable. The history of physics (as well as other sciences) is so packed with failed theoretical concepts which everyone deemed super beautiful at the time and which held back science for decades. Read Bachelard, there are two examples a page of such trends. But on top of that, Arkani-Hamed takes the worst possible example by referring to the ptolemaic worldview. The ptolemaic model did not need a little bit of tweaking to get true; in fact, for centuries people did nothing but tweak it with epicycles to make it fit averse data. What was needed, and what eventually happened, was to just get rid of it altogether, wipe the state clean and start all over again within the copernician worldview. That's one of the most obvious examples of a paradigm shift. Now I guess it is obvious to pretty much everyone what the lessons from this part of history tell us about theories which tweak themselves out of experimental data. That Arkani-Hamed would choose this specific example as a defense of SUSY is surprising, to say the least.

17. **Amitabh Lath**
February 11, 2019

Peter,
–My question is why would anyone ask him to do this, and why would he agree?

Why would anyone ask? Because SUSY is an important part of theoretical development of the last 40 years and the null results from LHC must be addressed. And who better than Nima for this reckoning?

Wby would Nima agree? Why not? What, do you want him to hide out in humiliation at the lack of SUSY or DM signatures?

I took away an entirely different message from his talk. He starts with the historical basis of SUSY, why did so many people feel it was right? What's left of the arguments after the latest LHC run? What is the way forward? Is it even possible? That bit about Howard Georgi deciding that short-distance physics is a dead end was there for a reason.

Even the pre-Copernican Ptolemaic stuff made sense to me. Basically, there are concepts in a failed theory that you might want to keep (things moving in circles around other things) and others you might want to jettison. Granted, he is not very good at history of science.

Look, if he had gone on about multiverse or beauty or naturalness then I would agree with you. But that is not what this talk was. It was him coming to grips, in public, that nature does not work the way he thought it did.

18. **quentin ruyant**
February 12, 2019

Amithab Lath,

I watched the entire presentation and I agree with you that we shouldn't get mad at him because of a loose comment made during the Q&A session. It looks more like a personal opinion than a fully worked out argument. But I disagree on Ptolemy, for the reasons given by Aurélien Bellanger: the question is not to what extent Ptolemaic astronomy got something right, but what research programs are fruitful.

Coming up with more and more epicycles before Copernicus would have been very inefficient without a deeper understanding. Another example is Vulcan, and then the asteroid belt around the sun hypothesised to explain Mercury's precession. In both cases, scientists were patching their current models again and again to fit anomalous data, without making new successful predictions or learning anything new, and I would say, looking back at history of science, that it "very very rarely" works…

Perhaps pursuing these programs would have been harmless before, but the question whether we're in the same kind of situation is more pressing today, given the cost of a new collider.

19. **DB**
February 12, 2019

Amitabh,
I fully agree with your last comment.

I think Nima has had the integrity of coming out to acknowledge, in public, that certain pictures of SUSY might very well be wrong.
And I applaud him for doing so.
Honesty and integrity are fast disappearing in today´s world, so I think that Nima should be given two thumbs up for what he´s done.
A couple of months ago Susskind also said, in a public interview, that ST would have to be "modified in a bigger context, generalized", because they (meaning the top ST ´s) know that it can´t describe the world as it truly is.
Another example of honest intellectual integrity.
Does that mean that ST should be fully abandoned, or that we shouldn´t build a larger, more powerful collider?
No it doesn´t. It just means that certain problems need to be attacked from a different angle.
So Nima, thanks a lot.

20. **Amitabh Lath**
February 12, 2019

DB, of course one does "acknowledge that certain pictures of SUSY might very well be wrong" but what someone like Nima brings is that excitement: Nature is more complex and interesting that we had thought! Let's get going! That above all is what I get from his talk. Listen to the description of electron EDM, he brilliantly summarizes the current reach and future prospects and what it means for BSM searches.
(made me slightly sad that I didn't chose to join the high precision group in grad school…)

The whole tenor of the talk is like that. This is a first rate mind surveying the situation figuring out where we go next, not one crouching at despair at the lack of high MET events. Either humans are meant to get to the next step in fundamental structure of matter, or we are not. Nima (and most of us experimentalists) are in the former, more optimistic camp.

21. **Peter Shor**
February 12, 2019

It's amazing how quickly amnesia hits.

For a more recent example from the history of physics, where a theory wasn't tweaked but forgotten, what about all the bootstrap models, that were one of the big things before the Standard Model was formulated? I only remember the PR articles about them, which I vaguely remember as being reminiscent of the current PR articles about String Theory. These theories seem to have vanished utterly once the Standard Model was accepted, which happened some time when I was in college.

https://en.wikipedia.org/wiki/Bootstrap_model

22. **Peter Woit**
February 12, 2019

Amit,
Yes, Arkani-Hamed is a very impressive thinker and a very compelling speaker, with a talent for inspiring others with a positive vision at a time when most are trying to process what seems like a bad situation. He's also sometimes spouting nonsense. Not mentioned here at all was the bulk of his talk, about models assuming hundreds of new scalar fields, or thousands of copies of every field already known. No one else could stand in front of an audience and go on about how this explains why we see nothing BSM now, but will soon, without getting laughed out of the room.

As far as the more serious material goes, a lot of his talk was of the order of "don't blame me, I never said the LHC would see something, in my favorite model (split SUSY), you don't expect to have seen anything, but you will at the FCC!". For another very similar discussion, see this by James Wells
https://deepblue.lib.umich.edu/handle/2027.42/144530

The problem here is that most theorists don't take split SUSY seriously. It actually was the butt of a well-known joke, the April 1 publication of a paper on "Super Split Supersymmetry", which now has a Wikipedia entry. In this context, the joke is that
1. GUTs don't work, so you "tweak" them to get SUSY-GUTs, broken below 1 TeV
2. SUSY-GUTs broken below 1 TeV don't work, so you "tweak" them to get SUSY-GUTs broken at 10-100 TeV, safe (for now) from disconfirmation.

The Super Split Supersymmetry joke is to make fun of the implausible second tweak by suggesting a further tweak of moving the breaking scale even higher, ensuring that your theory is completely indistiguishable from the SM. You're back where you started, pre-GUT hypothesis.

To put things more concretely, once you abandon the link of SUSY-breaking to the electroweak scale, SUSY doesn't explain the "WIMP miracle". So, all well and good to think maybe a TeV scale WIMP is behind dark matter, and even advertise that one thing the LHC and its successor will do is look for such a thing, but there's little to no reason to believe SUSY models have anything to do with it.

23. **Peter Woit**
February 12, 2019

Peter Shor,
Some would claim modern string theory as a successful "tweak" of the old bootstrap. The old bootstrap involved the idea that QFT couldn't describe the strong interactions, but you could do it using some unknown framework, specified not by a usual theory, but by consistency conditions (often actually analyticity conditions). This has now been "tweaked" so as to not just describe the strong interactions, but all interactions, with the conjecture of an "M-theory", specified not by a usual theory, but by a list of consistency conditions on what happens in various limits, related by dualities.

24. **Justin**
February 12, 2019

Woit always criticizes SUSY/string models. Fine, but not once on this blog have I seen any alternative proposed. What exactly would you like a potential FCC to look for?

25. **Peter Woit**
February 12, 2019

Justin,

I'd like a potential FCC to look for anything and everything that it can see that hasn't already been ruled out by LEP/LHC searches. This includes getting the best possible measurements of production and decays of the Higgs, especially anything sensitive to Higgs self-interactions. Also, of course, searches for any new states not accessible at LHC or LEP energies. Searches could specifically look for gluinos, squarks, or other states characteristic of SUSY models, but people should just realize that at this point these are not well-motivated searches. They're not likely to see anything (which doesn't mean you shouldn't do them), and if they do see something it is more likely to be something unexpected than something described by the MSSM.

No, I don't have a well-motivated BSM model that would provide an attractive target for an FCC search. Neither does anyone else….

26. **Amitabh Lath**
February 12, 2019

Peter, I do not understand your comments about "tweaking". Are you ridiculing theorists for proposing models and then revisiting them in light of null experimental results? Isn't that kinda how science is supposed to work?

What I see happening is theorists proposing a myriad of models, as they should when there is little constraint from experiment. As experiments come on line and models start falling, hopefully one model survives and the experimental goal switches to figuring out its parameter space (or there is a "surprise" ie November 1974).

We are at that point where some SUSY models have fallen, and some have not. I would not call it a "bad situation" because nature is doing something clever and not plain and obvious, and we have to be just as clever.

And as for crazy ideas "getting laughed out of the room" well they did that with fractionally charged particles that one couldn't see because they were too strongly bound. Frankly from my point of view there aren't enough crazy ideas yet, more are needed.

27. **Peter Orland**
February 12, 2019

Justin,

You are not making a point. If someone says it is unreasonable to believe dark matter is made of gray porcupines, you can't fault them for not presenting an alternative.

28. **Peter Orland**
February 12, 2019

Amitabh Lath,

As a particle theorist who has never really bought into BSM models, I can summarize why a (mostly silent) minority of my colleagues never felt tweaking these models was going to work:

The models don't solve problems, by which I mean inconsistencies. They do address esthetic questions, e.g. naturalness, which made them worth examining, but did not make them convincing.

29. **Peter Woit**
February 12, 2019

Amit,
The way science is supposed to work is that you are supposed to tweak good models that are doing something right to make them better. The problem here is that we're talking about tweaking bad models that didn't explain anything, not to make them better, but just to get them out of inconsistency with experiment (often by making them even worse, i.e. more complicated and explaining less). I see that Peter Orland has made a similar point.

There's a reasonable point of view that these models are just providing a randomized set of regions in the accessible search area for the LHC, giving experimentalists specific targets and specific ways of measuring progress towards covering this search area. That's fine, just don't fool yourself that these are well-motivated models, giving you something better than a random set of choices from throwing dice.

From a theory point of view, fractional charges were extremely well-motivated. We were seeing several sets of states with quantum numbers organized into the rigid patterns provided by irreducible representations of SU(3). The fact that all such representations could be generated by taking products of fundamental 3d representations provided an excellent reason to look for such 3d reps as elementary constituents, and these had to have fractional charges.

30. **WTW**
February 13, 2019

Peter,
>>"Not mentioned here at all was the bulk of his talk, about models assuming hundreds of new scalar fields, or thousands of copies of every field already known. No one else could stand in front of an audience and go on about how this explains why we see nothing BSM now, but will soon, without getting laughed out of the room."

While I also found it strange that no one there seemed to raise any questions about this, I also have to ask: given a rather arbitrary assemblage of 24 fields or so already, give or take a graviton or inflaton or two, what's so fundamentally different about having 1000 or 10,000 other such (hypothetical) fields also filling space?

Isn't this a kind of Noetherian-related "prejudice" for Nature obeying some (in Sabine Hossenfelder's terminology) mathematical "beauty" requirements — in this case just the minimalist "gruppenpest" symmetries of the Standard Model? While I recognize that Arkani-Hamed used these ideas to justify and explain more of the same stuff he has been proposing for years, for a 100TeV collider to test, if physicists are supposed to be thinking outside the box they've been in for the last two generations, why should even considering such things be verboten and ridiculed?

31. **Baron Munchausen**
February 13, 2019

Both the conformal and the S-matrix bootstrap are alive and well. See for example https://arxiv.org/abs/1805.04405 and https://arxiv.org/abs/1810.12849

32. **Amitabh Lath**
February 13, 2019

Peter W, your use of the word "tweak" needs a better definition. One person's tweak could be another's exploring new areas in theory-space in light of experimental results.

The most tweaked thing in my experience was the Higgs mass. In grad school (early 90's) we were taught it could not be heavier than the Z. Probably around 70 GeV or so. Then after some null results by Tevatron and LEP1 there were "tweaks" that raised it up to slightly above 100 GeV. I remember being told that 110 GeV was the absolute limit of what the theory would bear (granted it was by someone trying to recruit me for the search). When LEP2 found some fluctuation around 115 GeV one of the arguments from the theory side for delaying LHC was that it had to be the Higgs, because it couldn't really be much heavier, even this was a stretch. It didn't really end until 2012.

33. **Henry McFly**
February 14, 2019

Dear Peter,

The natural SUSY has certainly taken a hit after the null results at the LHC but the measure of naturalness was arbitrary anyways to begin with. You change your naturalness measure slightly or the limit what you call as natural and your sparticles are guaranteed to be out of the LHC's reach. Let's not forget one of the most important hints for SUSY, which is the Higgs mass at 125 GeV. In Standard Model, you really don't expect it to be that light, and 125 GeV is on the heavy side for SUSY almost guaranteeing that SUSY won't reveal her beautiful face at the LHC unless we are lucky. There is no inconsistency here. We knew back in July 4th, 2012, that this wouldn't be easy. So I am not sure why you and Sabine Hossenfelder think that the null results in the past few years are an indication that SUSY is ruled out. No, we really knew the results would most probably be null given the Higgs mass. Phenomenologists tweak their models so that they are discoverable at the LHC. It doesn't mean that's all out there, it just means that those models require immediate attention because the heavier ones won't be discovered at the LHC for sure.

34. **David Nataf**
February 14, 2019

A good example of a tweak in the history of astronomy is the addition of Neptune to the solar system. Back in the early 1800s, it was known that the present map of the solar system and the theory of Newtonian gravitation provided an inaccurate description of the movement of bodies in the outer solar system.

I don't know if any people proposed "alternative gravity" theories. But the solution to the problem was to tweak the framework that was then available, by adding Neptune to the map of the solar system, and eventually Neptune was indeed discovered. This would have followed an extensive period (decades?) of people not discovering Neptune, where they probably had "upper limits" on Neptune.

Though nowadays the solar system is largely relegated to being in children's science books, it was equivalent to the standard model of particle physics for most of the past few thousand years. As in, it was the mathematical/physical structure that people studied if they were curious about nature, and yes, many of the people studying it were partly inspired by the beauty of the underlying mathematics.

The equivalent to the BSM skeptics, circa 1820, would have been people saying "let's stop looking for Neptune. If we have not found it then it's not there. Let's just get better measurements of the motion of Saturn." Yeah, whatever.

Circling back to BSM physics, it's not a curiosity, it's a requirement. The standard model is a false description of the natural world. It doesn't include gravity, dark energy, dark matter, and it is not consistent with the baryon asymmetry in the universe. Ergo, it's false, and thus it is sensible that some fraction of theoretical physics be devoted to BSM research.

35. **Stefan**
February 15, 2019

@Henry
I agree with you that the benchmarks for what we consider to be natural have been somewhat arbitrarily set. I don't think that naturalness is ruled out. Theorists early best guesses have been ruled out. The LHC/naturalness debate seems to be more sociological than scientific in origin.

I'm not sure what you mean by the SM preferring the Higgs to be heavier than it is.
The precision EW fits from SM parameters imply the Higgs should be pretty much where it is: https://arxiv.org/pdf/1803.01853.pdf .

36. **Aurélien Bellanger**
February 15, 2019

David Nataf,

A two-minutes look at the related Wikipedia page is enough to learn that Neptune was theoretically predicted in 1845, then subsequently looked for and immediately found at the predicted location, in 1846. There were no "upper limits" on Neptune, and no two-decades long search for its position: it was predicted to be somewhere, people looked, and there it was. I fail to see how this piece of history provides a relevant defense of SUSY.

A more significant analogy would have been if Le Verrier had predicted a position, had been told that there was nothing there, had then tweaked its model by saying something like "it's certainly not a planet but a system of three planets orbiting each other in an intricate way, that's why it's actually further away, just look at this other position", had been told he was wrong again, and had then repeated the same process several times again. But that's not even close to what happened.

37. **Igor Khavkine**
February 15, 2019

In an earlier comment, RGT laid out the challenge: "… the jump from for example Newtonian Mechanics to General Relativity could hardly be considered a tweak."

I would maintain that the said jump is actually a *series* of tweaks, each one well-motivated and successfully tested in its time. The analogy is of course with evolution, which is nothing but a *long succession* of small individual mutations.

* Galileo, Newton: particle mechanics, forces [Newtonian mechanics]

* Newton, Coulomb: forces and potentials generated by massive and charged particles [Newtonian and Coulomb potentials]

* Gauss, Poisson, Ampere, Faraday: forces and potentials generated by multitudes of sources combined into fields [gravitational, electric and magnetic force/potential fields]

* Maxwell: (some) fields have their own dynamics independent of sources, finite speed of field propagation [Maxwell's equations]

* Einstein, Lorentz, Poincaré: (universal) finite speed of propagation extended to particle mechanics [special relativity]

* Einstein: the gravitational field is a metric [equivalence principle]

* Einstein: universal finite speed of propagation extended to all fields known at the time (meaning to the gravitational field), the gravitational field has its own dynamics [general relativity]

I did not aim to be very precise. The names listed with each tweak are mainly representative of the most famous people involved and of the epoch. And I also do not claim that the evolution of physical theories followed this one singular path. There were many branch points that underwent their own independent series of tweaks, but in the end did not survive. I also do not want to imply that there were no significant jumps in thought in theory development. But those jumps were more along the lines of abandoning a (possibly dominant but) unsuccessful line of thought and returning to tweak the last best step that was known to work.

38. **Thomas**
February 15, 2019

Igor Khavkine writes: "I would maintain that the said jump is actually a series of tweaks, each one well-motivated and successfully tested in its time."

Dear Igor, it all depends on how you define "tweak." According to your definition, a TESLA automobile is just a few "tweaks" of the wheel.

A rocketship is a just a few "tweaks" of fire and tents.

Most folks have a different definition of the word "tweak."

39. **David Nataf**
February 15, 2019

Aurélien Bellanger,

Be careful relying on 2-minute searches of wikipedia to come up with broad conclusions of scientific history. You may end up mistaken, as was the case here. The wikipedia article contains accurate information, but it does not contain all of the information.

Yes, there were improved measurements and calculations of Uranus' orbit in the 1840s, which led to the discovery of Neptune a few years later. However, the conclusion that Uranus' orbital motion was peculiar dates back to at least the 1820s, see page 23 at this link:
https://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=26&ved=2ahUKEwiXgI__kr7gAhUovFkKHQb0ArUQFjAZegQIBBAC&url=https%3A%2F%2Fwww.springer.com%2Fcda%2c1.pdf%3FSGWID%3D0-0-45-1380904-p174602935&usg=AOvVaw1YFFiK5CdnSwY9x9rniZRM

Moreover, you were very quick to dismiss an otherwise valid point on a technicality. You should know that the same general issue applies very much to the history of solar system studies. The orbital motion of mercury, for example, took a very long time to understand, and it had to be reconciled by modified gravity. Today, astronomers are looking for a "planet nine" which was proposed three years ago to explain the unexpected phase space distribution of kuiper-belt objections, observations and analysis are ongoing. Many problems in solar system science remain unresolved, such as the faint young sun paradox. People have not given up studying these issues simply because the questions have been around for 10+ years or 20+ years. Some questions are genuinely difficult to solve, and thus people research them for decades.

As was the case for the discovery of Neptune. That was a roughly ~25-year research problem. More actually, as once Neptune was found they had to confirm that it had the right parameters to resolve the issue.

40. **Peter Woit**
February 15, 2019

All,
Enough of the historical analogies, this really is a waste of time. The bottom line is that there is zero evidence for SUSY extensions of the Standard Model, and they explain essentially nothing (e.g just coupling constant unification in GUTs which don't seem to work anyway) at the cost of a huge increase in complexity and number of undetermined parameters.

That this kind of SUSY is now a dead idea, finally killed off by the negative LHC results, is not just some peculiar opinion from me and Sabine Hossenfelder. Most of the particle theory community has long been skeptical of the idea. The famous Copenhagen bet on SUSY back in 2000 had more than twice as many theorists taking the no-SUSY side, see
https://strings.ph.qmul.ac.uk/~dsb/dbwager.pdf

It would be interesting to see a poll of the HEP theory community now on the subject, I think one would find belief that SUSY extensions of the SM are viable post-LHC to be a small minority opinion. Even those sticking to this opinion I suspect have their doubts. Arkani-Hamed is known for the enthusiasm with which he expresses his opinions, in this case he's unusually defensive and tentative ("if you woke me up in the middle of the night and put a gun to my head…").

41. **DB**
February 15, 2019

Peter,
I agree with your latest comment.

It would be great to know what the top theorists believe.
Does anybody know?
Witten? Susskind? Maldacena? Nima? Seiberg?
Even the younger generation, like Douglas Stanford, Xi Yin or Daniel Harlow…

42. **Peter Woit**
February 15, 2019

DB,
Arkani-Hamed's talk is exactly answering this question, and taking the "I still believe, despite it all…" stand. For all the others you mention, I think they have long ago voted with their feet and decided that SUSY extensions of the SM were not something they wanted to work on. In a recent Facebook post, Harlow writes
"when I started graduate school in 2006 I was intending to work on LHC physics, but I decided that there wasn't much room for me to make important contributions so I chose to work on other problems which seemed to me more promising. In the years since the LHC turned on, many others have made the same choice. In particular I want to emphasize that Nima Arkani-Hamed, one of the best-known LHC model builders, has for the last ten years mostly worked on other topics where he finds progress more plausible. "
Some of these people may have been hopeful that SUSY would show up at the LHC, but I don't see any evidence that any of them besides Arkani-Hamed are impressed by split SUSY or any other attempts to "tweak" SUSY to explain why it hasn't shown up at the LHC.

43. **Amitabh Lath**
February 15, 2019

Peter, split SUSY is not a tweak in response to LHC null results because I recall mentions of split SUSY during Tevatron Run2 . It didn't get a lot of publicity probably because MSSM was ascendant (not to mention doable).

More accurate to say minimal SUSY is dead. The larger question "is nature supersymmetric?" has not been answered. It may well be beyond the capabilities of LHC or even future colliders to answer but we won't know until we get there.

44. **Claudia**
February 17, 2019

Amitabh Lath,

your statement "The larger question "is nature supersymmetric?" has not been answered. It may well be beyond the capabilities of LHC or even future colliders to answer but we won't know until we get there." needs to be amended, I think.

There are various scenarios where we can know *before* we get "there"; one scenario is that we find another description of the standard model that solves the issues that people have with it, most notably the understanding of its parameters.

Sometimes I like to be a bit populistic. The argument that "we cannot know before we get there" can be made for any theory – also for the one that tiny green mice live inside particles above 100 PeV.

If one honestly thinks that nature is supersymmetric and that it is described by 130 parameters instead of the 25 of the standard model, one must provide some evidence – like we would require from the people who talk about little green mice. Nobody has seen any evidence for those additional parameters. Some weeks ago, I talked to a researcher who spent all his life on supersymmetry. I asked him why he continued on that topic. He was quite for a minute, then said slowly: "because it is the only game in town". I think he was very honest.

My personal opinion is this: the interest in supersymmetry will decay rapidly as soon as another game in town arises.

45. **Lee Smolin**
February 17, 2019

Dear Claudia,

There are other "games in town." There are variants of strong coupling models of condensates such as technicolour. There are models of substructure of quarks and leptons, called preon models. There are people attempting to understand the deep structure behind the choices of gauge groups and representations of the standard model, such as Cohl Furey, arXiv:1806.00612 and previous papers.

Like all ambitious new ideas people can and have pointed our potential shortcomings of each of these. But that is always the case for new theories. I find it astounding that just a handful of people are working on the idea Furey develops that the octonions have something to do with the structure of the standard model.

The main job of particle theorists remains, after all is said and done, to understand why nature chose the structure of the standard model, with its particular gauge groups and representations, and the particular values of the dimensionless parameters. There is exactly one right answer to this question, which means that the sooner we encourage people to put to one side hypotheses that have not succeeded in favour of inventing and exploring a diverse range of newer ideas, the sooner we will know it. Even if one of the older ideas like supersymmetry turn out to play a role, it is likely at this point that the route to the right answer will involve new ideas such as the structure of the division algebras (and indeed there are links between the division algebras and SUSY.)

Thanks,

L

46. **Sabine Hossenfelder**
February 18, 2019

@HenryMcFly

"I am not sure why you and Sabine Hossenfelder think that the null results in the past few years are an indication that SUSY is ruled out."

You can't rule out SUSY and I certainly didn't say so. You can only rule out specific SUSY models. To the extent that predictions have been made for the LHC with specific models those have been ruled out. The reason is that all those models used arguments from naturalness for motivation why new physics should be discoverable in the LHC range.

“No, we really knew the results would most probably be null given the Higgs mass.”

Funny how particle physicists know things in advance after the data forced them to admit their predictions were wrong.

“Phenomenologists tweak their models so that they are discoverable at the LHC.”

We all know that. This is why those “predictions” are worthless. Stop doing it. It’s not good science.

47. **Paddy**
February 19, 2019

If I were a graduate student being told by a leading practitioner that his paradigm was no more wrong that Ptolemy, I would not be reassured. Isn’t Ptolomy’s the proverbial example of an utterly wrong theory being patched up to “save the appearances”? It’s not an accident that “epicycle” is a standard insult in physics.

So I’m not sure Arkani-Hamed has actually changed his mind, he is just leaving the conclusion to be drawn as an exercise for the audience.

Various and Sundry

February 15, 2019
Categories: Uncategorized

First a couple of items from Paris:

- Fields medalist Cédric Villani is campaigning for the position of Mayor of Paris. This Sunday there will be a campaign event/book launch for his new book, Immersion: De la science au Parlement.
- The long-awaited public unveiling of the results of director Ilya Khrzhanovsky's attempt to make a film inspired by the story of Lev Landau has finally happened, with Dau now on view spread over three locations in Paris. This project was filmed during 2009-11, and I wrote a bit about it here in 2015. For more about the project, see for instance here and here. Among those appearing in the film are David Gross, Sergio Cecotti, Alexander Vilenkin, Carlo Rovelli, Costas Bachas, Erik Verlinde, Igor Klebanov, Samson Shatashvili, Shing-Tung Yau, Dmitry Kaledin, Nikita Nekrasov and Andrey Losev.

Some number-theorist related items:

- Videos from last year's Barry Mazur birthday conference are now available here. Also available is the write-up from Mazur of a talk last fall on The Unity and Breadth of Mathematics.
- See here for an interview with Akshay Venkatesh.

Finally, some comments from Scott Aaronson on the current "like beer at a frat party" state of funding of quantum information theory:

> I wanted to call attention to a hilarious irony. For years, I've made the case that trying to build scalable quantum computers, in order to probe the universe for the first time in "the regime beyond the classical Extended Church-Turing Thesis," is just as scientifically interesting as finding the Higgs boson—even if we set aside any of the possible applications of QC.
>
> I don't think I imagined to what extent the tables would someday turn—with funding now flowing into quantum information like beer at a frat party (for a combination of good and bad reasons…), with the future of experimental particle physics now in serious doubt, and with me put in the position of arguing that the high-energy frontier is worth exploring too! 😀

Update: More about the opening of Dau in Paris here.

Comments

1. **cedric bardot**
   February 18, 2019

   Here is a conference to come soon in the suburb of Paris that might interest you and some of your readers Peter.
   Called "Space Time Matrices" it is organised by Thibault Damour, Jens Hoppe and Maxim Kontsevich at IHES from 25 to 27 February 2019
   (conference web page for registration https://indico.math.cnrs.fr/event/4272/)
   "Large-N limits of matrix models have been proposed as a way of describing the structure of Space and Time. The conference will review these models that may bring a new light on trying to reconcile Gravity and the Quantum."
   If there is a place where solid theoretical research in natural geometric structures has no risk to get lost in math this is probably at IHES thanks to senior scientists as Kontsevich & Connes (both speakers at the conference) and Damour as well of course.

2. **Anonymous**
   February 20, 2019

   Dear Peter,

   Have you already seen the new autobiography of Yau?

   https://www.amazon.com/Shape-Life-Mathematicians-Universes-Geometry-ebook/dp/B07N8V6DQX

3. **Peter Woit**
   February 20, 2019

   Anonymous,
   Haven't seen it yet. I'm wondering how it's different than "The Shape of Inner Space", which I wrote about in some detail here
   http://www.math.columbia.edu/~woit/wordpress/?p=3165

4. **David Roberts**
   February 20, 2019

   @Peter

   it looks much more biographical than the previous one, from the tables of contents of both books.

5. **cedric bardot**
   March 9, 2019

   During the "Space Time Matrices" conference at IHES one had the opportunity to notice an invited speaker was accurate enough to entitle his last slide "a chapter closed, in the **M-theory hypothesis**, via localization". //emphasis mine https://indico.math.cnrs.fr/event/4272/attachments/2260/2718/IHESConference_Piljin_YI.pdf.

   All the videos of the conference are here
   https://www.youtube.com/playlist?list=PLx5f8IelFRgFOUvecX2PLH0-ka__t77Zl and all slides there
   https://indico.math.cnrs.fr/event/4272/

   Regarding the "M-theory" word I find it interesting to translate from French a remark from a great geometer Valentin Poénaru about the quantum thinker who incepted it :

> In 1979, I was at a lecture on gauge theory at Cargèse where one of the stars was … Ed Witten. It turns out we were in the same hotel and so I often had the opportunity to listen to him ; and there is one thing that struck me a lot. While other physicists … always spoke in terms of phenomena, possibly in terms of concrete models tested on computers, Witten juggled all the time with theories themselves, their way of interacting, of complementing each other or simply of exploding. It seems to me that there is a direct link between this way of thinking and today's M-theory.

(http://www.philosciences.org/notices/document.php?id_document=1091)

The Mathematical Question From Which All Answers Flow

February 19, 2019
Categories: Uncategorized

I'm beginning to suspect that there are actually (at least) two different theoretical HEP physicists named Nima Arkani-Hamed out there. One of them (who I'll call Nima1) believes the way to understand the fundamental nature of physical reality involves extremely complicated extensions of the Standard Model, with large numbers of parameters tuned to avoid conflict with observation, and possibly hundreds or thousands of extra fields thrown in for good measure. He also seems to like the multiverse and anthropic explanations. I have a lot of disagreements with Nima1, most recently discussed here.

The second Arkani-Hamed (Nima2) has a completely different point of view, one quite close to my own, although he may be even more of a mathematical mystic than I am. Natalie Wolchover has recently talked to Nima2 and written about it for the New Yorker. Nima2 is in love with the deep mathematical structure of physics and the way it appears in different aspects:

> Nima Arkani-Hamed, a physicist at the Institute for Advanced Study, is one of today's leading theoreticians. "The miraculous shape-shifting property of the laws is the single most amazing thing I know about them," he told me, this past fall. It "must be a huge clue to the nature of the ultimate truth."

Wolchover expands on this idea of multiple ways of expressing the same underlying mathematical structure:

> The existence of this branching, interconnected web of mathematical languages, each with its own associated picture of the world, is what needs to be understood.
>
> This web of laws creates traps for physicists. Suppose you're a researcher seeking to understand the universe more deeply. You may get stuck using a dead-end description—clinging to a principle that seems correct but is merely one of nature's disguises. It's for this reason that Paul Dirac, a British pioneer of quantum theory, stressed the importance of reformulating existing theories: it's by finding new ways of describing known phenomena that you can escape the trap of provisional or limited belief. This was the trick that led Dirac to predict antimatter, in 1928. "It is not always so that theories which are equivalent are equally good," he said, five decades later, "because one of them may be more suitable than the other for future developments."
>
> Today, various puzzles and paradoxes point to the need to reformulate the theories of modern physics in a new mathematical language. Many physicists feel trapped. They have a hunch that they need to transcend the notion that objects move and interact in space and time. Einstein's general theory of relativity beautifully weaves space and time together into a four-dimensional fabric, known as space-time, and equates gravity with warps in that fabric. But Einstein's theory and the space-time concept break down inside black holes and at the moment of the big bang. Space-time, in other words, may be a translation of some other description of reality that, though more abstract or unfamiliar, can have greater explanatory power.

Nima2 is obsessed with exactly the same mystical mathematical issue that I am: what's the right mathematical question that has as answer the Standard Model and GR?

> To Arkani-Hamed, the multifariousness of the laws suggests a different conception of what physics is all about. We're not building a machine that calculates answers, he says; instead, we're discovering questions. Nature's shape-shifting laws seem to be the answer to an unknown mathematical question…
>
> Arkani-Hamed now sees the ultimate goal of physics as figuring out the mathematical question from which all the answers flow. "The ascension to the tenth level of intellectual heaven," he told me, "would be if we find the question to which the universe is the answer, and the nature of that question in and of itself explains why it was possible to describe it in so many different ways." It's as though physics has been turned inside out. It now appears that the answers already surround us. It's the question we don't know.

I'm not sure the Amplituhedron is the right path to the "tenth level of intellectual heaven" and finding the "mathematical question from which all the answers flow", but I'm completely sympathetic with Nima2's motivation and quest.

Comments

1. **Warren Siegel**
   February 19, 2019

   Nima2 = Douglas Adams?

2. **Francois Loeser**
   February 19, 2019

   Maybe this provides the first compelling evidence in favor of the existence of the multiverse?

3. **Peter Woit**
   February 19, 2019

   Francois Loeser,
   What's confusing here is the continual tunneling back and forth between the two different Nima1/Nima2 universes.

   All,
   I'm wondering: has anyone ever seen Nima1 and Nima2 in the same place at the same time? He gives a lot of talks. Has anyone ever seen one containing both an inspirational segment on the existence of a deep mathematical answer to all our questions AND a segment on how split SUSY is the way to go?

4. **Low Math, Meekly Interacting**
   February 19, 2019

   Maybe he's normally in a superposition, and only collapses into one form or the other depending on the kind of question he's answering.

   When you posted that latest entry, I wondered immediately what happened to all the amplitude stuff. Guess this sort of answers the question.

   The article makes a lot of references to Feynman. I recall from watching the videos (and from reading QED) Feynman's frustration with getting answers out of QCD using the methods he'd helped invent. I gather there are lots of better ways to do it nowadays, but they don't seem to get to the heart of what was bothering him, namely his intuition that the approach was simply wrong on some level, and only good as an approximation. He found it hard to believe that nature really does all these calculations, and that there must be a better way that doesn't involve accounting for an infinite number of "paths" to get to the result.

Even if the radical interpretation of the amplituhedron is totally off, if it produced a way to do calculations in a realistic theory a lot easier, that seems like such a great use of one's time. Odd we don't hear about this version of the man more often in the pop-sci press.

5. **Peter Woit**
   February 19, 2019

   LMMI,
   Everyone would like a more insightful formulation of QCD, I have no idea if the amplitudes program has anything promising in that direction. The over-the-top claims I've seen tend do be of the "we're going to replace space-time", not "we're going to solve QCD" variety.

   As for the invocation of Feynman, the peculiar thing is that both Nima1 (complicated BSM models that evade experiment) and Nima2 (the mathematics-physics mystic) seem very far away from what Feynman would find congenial.

6. **Martin S.**
   February 19, 2019

   When you interact with him, you measure him. He is clearly macroscopic, thus don't be surprised that actual superpositions are not seen. May be if you would put him close to zero temperature, there would be a chance. Though I do not suggest it…
   Hm, the transitions suggest superpositions in between, i.e. a possibility of him approaching really low temperatures. Polar vortex?

7. **Sam Hopkins**
   February 19, 2019

   Do you know of any examples before the recent scattering amplitudes stuff in which total positivity had important connections to physics?

8. **Amitabh Lath**
   February 19, 2019

   Do I contradict myself?
   Very well then I contradict myself,
   (I am large, I contain multitudes.)

   -Walt Whitman, Song of Myself.

9. **ay**
   February 19, 2019

   Are the multiple perfect descriptions a profound property of physics or a mundane property of mathematics – namely that there's lots of it and it's all interrelated.

10. **Peter Woit**
    February 19, 2019

    ay,
    An excellent question. One problem with this essay is that since it's aimed at such a non-technical audience, there's no indication of what exactly Arkani-Hamed has in mind, so hard to tell what the origin might of the particular multiplicity of descriptions that he's interested in

11. **Low Math, Meekly Interacting**
    February 20, 2019

    Perhaps I misunderstood something, which is not unlikely. The amplituhedron's heritage (summarized far better than I ever could hope in the link below, so I won't even try) suggested notable relevance to the difficulties of QCD, specifically its roots in ideas (BCFW) being put to real use for, say, calculating backgrounds in hadron collisions (I'm aware there are many others). Regardless of what it all "means" about space-time and so on, if the amplituhedron work is to some extent a simplification or a generalization, then presumably it could be of some benefit for very real-world problems in cutting-edge experimental particle physics. To me that overall story, of how people got from Feynman diagrams to the present tools, is nothing short of heroic, with a lot of contributors doing great physics that makes demonstrable contact with experiment.

    It's a history that gets short shrift in the pop-sci literature, in my opinion. Just not mind-blowing enough, I guess.

    https://royalsocietypublishing.org/doi/pdf/10.1098/rsta.2014.0248

12. **Peter Woit**
    February 20, 2019

    LMMI,
    To be clearer, the amplitudes stuff reformulates the problem of computing perturbative QCD amplitudes in a different way, which may be quite important. But, as far as I know, it doesn't give an approach to understanding non-perturbative QCD, which is the big problem, one where we are lacking a good idea. Recall that these are amplitudes for scattering gluons and quarks, but the physical states are something very different.

    I'm quite a fan of the ideas about how to exploit conformal invariance using twistors being used here, but, again, the problem is that QCD is, non-perturbatively, not a conformally invariant theory. From the early work of Witten on the "twistor-string", it was clear that this kind of duality with a string was a "weak-weak" duality, unlike AdS/CFT where the weakly coupled string is supposed to be dual to a strongly coupled gauge theory.

    I don't want to encourage more discussion of amplitudes here though, because there actually was nothing substantive about the topic in this article, it was really about other more "metaphysical" issues.

13. **Gil Kalai**
    February 23, 2019

    Hi Peter, this is an an interesting and funny post, and meeting François is always a joy.

    Let me make the obvious (at least for a mathematician) comment (perhaps Peter also agrees) that there need not be a contradiction between a belief that "the way to understand the fundamental nature of physical reality involves extremely complicated extensions of the Standard Model, with large numbers of parameters tuned to avoid conflict with observation" and the belief in "a mathematical question from which all the answers flow." It is nice that the same scientist can make progress in both these points of view.

14. **Peter Woit**
February 25, 2019

Gil Kalai,
The problem is that Nima1 hasn't actually made any progress, quite the opposite. The only progress in the field of BSM physics over the past 20 years has come from experimentalists showing that the models studied by Nima1 and others, besides being ugly and not explaining anything, also disagree with experiment, to the extent they make any predictions at all.

Nima2 on the other hand is a mathematical physicist, and has been involved in significant progress in mathematical physics.

Yes, there's nothing mathematically inconsistent with the idea that there are mathematically very deep and beautiful ideas at the foundations of physics, which then are completely masked by a complicated, ugly effective low energy limit and mechanism for producing complicated, ugly ground states. The only problem is that there is zero evidence for this. Instead, our best theories are highly constrained by deep mathematical ideas about symmetry, and these get translated directly into what we observe about the world.

15. **Bernhard**
February 26, 2019

Peter,

I agree with you to some extent but in the end Nima1 has contributed a lot to make these results possible. Experimentalists can only shoot the theories/models that are there to be shot, and Nima was probably the most important theorist during this time providing well-defined models that led to new topologies that could otherwise have been missed. Experimentalists do know how to add a Lagrangian in Madgraph, but to come up with the Lagrangian itself you need a guy like Nima1.

It is true, every time I was in the audience in schools or conferences with him as speaker I had roll my eyes over some of the over-the-top BS he likes to pull, but it does not change the fact that Nima1 has significantly contributed to the results that came from LHC Runs 1 and 2.

A Few Items

February 28, 2019
Categories: Swampland, Uncategorized

A few things that may be of interest:

- The Perimeter Institute has a new director, Rob Myers, succeeding Neil Turok. Myers is very much a mainstream theorist, and Perimeter over the years has been converging with the mainstream, from a very non-mainstream initial state. While Turok has taken the view in recent years that theoretical physics is in "a deep crisis", Physics World has:

  > Myers says there are many opportunities in theoretical physics, mostly thanks to the vast amounts of data that are being collected by various experiments such as CHIME, EHT and the LIGO gravitational-wave detectors in the US. Yet Myers doesn't believe that theoretical physics is in "a deep crisis" as Turok once admitted. "Particle physics is somewhat at a crossroads," he says. "Describing it as a crisis is slightly dramatic, but I would agree that people have been relying on the status quo for too long and relaying on certain models from decades ago."
  >
  > Indeed, Myers now challenges researchers to think in new ways. "Young people are the future and we want to instill in them to question the status quo," he adds. "After all, it is the people here that make the PI such a special place."

- Speaking of challenges to the status quo, it seems that Sabine Hossenfelder now has a contract for her second book, topic not yet revealed.
- For the latest news from the Swampland, see this twitter thread from Will Kinney. He explains how the "Swampland conjecture" was meant to kill off the string theory multiverse, but this conjecture got in trouble:

  > So by getting rid of the multiverse, we have also gotten rid of known physics like the Higgs boson. Merde!

  It was replaced by a fix, the "refined Swampland conjecture", but Kinney has a new paper in PRL (arXiv link here) showing this fix doesn't solve the multiverse problem for string theory:

  > This means that, as soon as we fix up the Swampland Conjecture so it doesn't trivially rule out known physics like the Higgs, we inevitably get an unwelcome passenger: the string multiverse!
  >
  > This is important because it looked like the Swampland Conjecture was likely to free us from the multiverse and associated awful stuff like the Anthropic Principle. Not so, we're still stuck with it. Sorry.

- John Baez has a popular article at Nautilus about his new-found love for algebraic geometry, as an explanation of the relation of classical and quantum. The more technical version is a series of posts here.

Update: Arnold Neumaier has posted at the arXiv a series of three papers discussing his "thermal interpretation" of quantum mechanics (see here, here and here). While I find many of the points he seems to be making compelling, I haven't had time to think seriously about what the problems of his approach might be (and there's a long history of online discussions between him and others which would be a good place to start). Neumaier in the papers explicitly asks for discussion of them at physicsoverflow.org, and there are now posts there for this purpose (here, here and here). I look forward to following any discussion with him over there. He also has a website devoted to this topic here, which has some links to earlier discussions.

Update: There's a new issue of Inference out. As usual, some interesting pieces from people not usually heard from in a non-technical venue. No sign of the pro-intelligent design/climate denialism agenda that they've been accused of having (see here). Pieces specifically relevant to some of the obsessions of this blog are a review by Glashow of Lost in Math, and a piece by David Roberts on the Mochizuki/Scholze/Stix story.

Comments

1. **Thomas Van Riet**
   March 2, 2019

   If the dS swampland conjecture is correct, I cannot see what we exactly mean with a multiverse here. At least not a multiverse of dS vacua…

2. **a reader**
   March 3, 2019

   I read "Inevitability and Eternity," an essay from the new issue of Inference you linked to, and, as a citizen, I sincerely find it commendable. Thank you Peter.

3. **Blake Stacey**
   March 17, 2019

   Inference offered me the chance to write a response to Glashow's review of Becker's book (the review that Becker stops just short of claiming was some sort of reprisal for his investigating their finances). This happened in the usual manner: The invitation first went to one colleague, then was handed off to another, before eventually falling to me. Somewhere along the way, the e-mail boilerplate about paying contributors got lost, so I didn't know money would be involved, and for an academic gig I didn't think it would be. I drafted a brief essay that was critical both of Becker's book and, to a lesser extent, of Glashow's review.

   After Becker's piece in *Undark* came out, I was convinced that my criticisms would be portrayed as some kind of payback, so I withdrew the short draft I had composed. David Berlinski wrote to me (this was the 1st of February) to ask me to change my mind. After years of following the evolution-denial movement, and after attempting to read his "popular explanations" of mathematics, I have nothing but negative associations with Berlinski. I had been dubious all along about his name being on the masthead, but I'm also sadly accustomed to seeing Big Egos affiliated with "interdisciplinary" projects. Learning that he took any active editorial role — instead of being some kind of nominal holdover from an earlier management — convinced me that I had made the right choice.

   I withdrew before I had learned that they paid for articles, so I'll never know how much money I passed up.

   I was not particularly concerned that the original source of their funding was somehow still setting their editorial agenda. Years ago, I quit a paying gig at ScienceBlogs.com because the management there abandoned the firewall between funding and editorial by giving Pepsi the chance to write a blog about "nutrition science". So, I *can* be prickly about such things, but I honestly couldn't see much of an agenda in what *Inference* published, except perhaps a general type of intellectual self-importance that transcends partisanship. Again, that is familiar enough, and not necessarily a deal-breaker in my book.

After forsaking whatever fee I might have earned, I expanded my essay with various observations that hadn't made the cut for a short note, in the hope that I could make a comprehensive commentary and put Becker's book behind me forever. I doubt the result is publishable in any venue having pretensions of solemnity, but it ultimately felt good to catalog the ways in which Becker reproduces the typical laziness of bad quantum-physics writing while inventing some new problems of his own.

I have Issues with the response to Glashow's review that they did publish (by a linguist and philosopher), but this comment has already gone on plenty long, I fear to little benefit.

This Week's Hype

March 5, 2019
Categories: This Week's Hype

In recent years string theorists have been having trouble getting taken seriously by the media, a problem they've been trying to deal with by enlisting the PR departments of their universities to help. Following Princeton and Stanford, today's the turn of the string theorists at Northeastern, who had their press office put out a press release announcing "Northeastern team uses string theory to explain the fundamental nature of the universe."

As usual, this is just pure, unadulterated hype. It's based on a PRL publication, also available as this preprint. I usually try to avoid this sort of editorializing, but I'm actually shocked to see that PRL is now publishing this sort of thing, which is infinitely far from having any connection to conventional science.

Comments

1. **Roger**
   March 6, 2019

   Off-topic.
   It looks like the Japanese have made some sort of decision regarding hosting the ILC. There will be some news tomorrow (7th March) followed by a press conference by ICFA (International Committee for Futrue Accelerators):
   https://www.interactions.org/press-release/icfa-briefs-future-prospect-international-linear-collider

2. **David Littleboy**
   March 6, 2019

   FWIW, and it's not worth much, there was an item in the Asahi Shimbun (one of the major Japanese newspapers) a couple of weeks ago to the effect a particular group of scientists had announced that they were unenthused about building a new linear accelerator in Japan. It wasn't immediately clear whether said group was a group with clout or not, so I didn't track it down. Here, I do slightly more of my homework.

   Ah, here's something: https://webronza.asahi.com/science/articles/2018112600016.html

   It seems the folks promoting the project were irritated (as of Nov. 26 last year) that the Science Council of Japan's report on the project had "overly strong negative nuances". And held a press conference to that effect.

   Here's the PDF of the SCJ's December 19, 2018 report: http://www.scj.go.jp/ja/info/kohyo/pdf/kohyo-24-k273.pdf

   It's long. But it mutters about not thinking that the project has yet gotten to the stage of serious discussions about personnel allocations and funding.

   And there are definitely some other negative, or at least implicitly negative comments in there.

   Perhaps March 7 will be interesting.

3. **Peter Woit**
   March 6, 2019

   About the ILC.
   Let's see what the news is tomorrow. From everything I've seen, it seems unlikely to be positive for a Japanese ILC project.

4. **Bernhard**
   March 6, 2019

   How does this PRL compare to the Bogdanov affair?

5. **Peter Woit**
   March 6, 2019

   Bernhard,
   It's pretty different. The Bogdanov papers were highly unclear and did not really make sense. This paper is a different problem: it's perfectly clear what calculation they are doing, there just is zero evidence it has any relevance to the real world. What's surprising to me is that PRL's standards have evolved to the point where they are willing to publish this kind of completely unsupported speculation.

6. **Peter Woit**
   March 7, 2019

   For a Physics World story about the Japanese ILC announcement, see here

   https://physicsworld.com/a/disappointment-as-japan-fails-to-commit-to-hosting-the-international-linear-collider/

   Perhaps the best summary is the quote from Brian Foster:

   "It is difficult to be convinced that the Japanese government is serious about this," says Foster. "Delaying the decision in this way seems like a typical Japanese way of saying 'no'".

7. **Roger**
   March 7, 2019

   A large part of the original motivation for the ILC was to study in more detail the new phenomena that the LHC would see.

   I'm reminded of a comment Rolf Heuer made when he played a leading role in the ILC. He was asked in 2004 whether he thought the project would go ahead if the LHC found only the SM Higgs. His answer was no. He may well have been very prescient.

8. **Of absolutely no interest to you**
   March 9, 2019

   "which is infinitely far from having any connection to conventional science."
   –>
   "which is infinitely far from having any connection to science."
   FTFY

9. **Lucas**
   March 9, 2019

   It seems to me that PRL has a very low bar for 'trendy' interdisciplinary topics like neural networks, machine learning, etc when given some type of physics 'spin.'

In it for the Long Haul

March 8, 2019
Categories: Uncategorized

The CERN Courier today has a long interview with the omnipresent Nima Arkani-Hamed, discussing the current state of HEP physics. About the motivations for a next-generation collider project, I'm pretty much in agreement with him: the main argument is for a Higgs factory that would allow a much more detailed study of the Higgs, and if at all possible, an appropriate machine should be built (see more here). He agrees that the SUSY and extra dimensions models used to get people excited about the LHC can't reasonably be used again for a higher-energy machine:

> **Is supersymmetry still a motivation for a new collider?**
> Nobody who is making the case for future colliders is invoking, as a driving motivation, supersymmetry, extra dimensions or any of the other ideas that have been developed over the past 40 years for physics beyond the Standard Model. Certainly many of the versions of these ideas, which were popular in the 1980s and 1990s, are either dead or on life support given the LHC data, but others proposed in the early 2000s are alive and well.

The last reference is to his favored split SUSY models, which I think few people besides him find compelling.

About WIMP dark matter he seems to be claiming that a 100 TeV machine has always been what is needed to find it:

> There is a funny perception, somewhat paralleling the absence of supersymmetry at the LHC, that the simple paradigm of WIMP dark matter has been ruled out by direct-detection experiments. Nope! In fact, the very simplest models of WIMP dark matter are perfectly alive and well. Once the electroweak quantum numbers of the dark-matter particles are specified, you can unambiguously compute what mass an electroweak charged dark-matter particle should have so that its thermal relic abundance is correct. You get a number between 1–3 TeV, far too heavy to be produced in any sizeable numbers at the LHC. Furthermore, they happen to have miniscule interaction cross sections for direct detection. So these very simplest theories of WIMP dark matter are inaccessible to the LHC and direct-detection experiments. But a 100 TeV collider has just enough juice to either see these particles, or rule out this simplest WIMP picture.

I don't remember ever hearing, pre-LHC, from him or anyone else, this argument that the most likely WIMP dark matter models are inaccessible to the LHC or to direct detection experiments. For many years, most of the direct detection experimental results came with plots showing a "prediction" of SUSY WIMP dark matter (see for example here, figure 5), in a mass range of 100-500 GeV, at a cross section measurable (and now ruled out by) experiments like XENON1T (see here).

Arkani-Hamed likes to make the following argument, which I think most current HEP theory graduate students may find hard to swallow:

> **How do you view the status of particle physics?**
> There has never been a better time to be a physicist. The questions on the table today are not about this-or-that detail, but profound ones about the very structure of the laws of nature. The ancients could (and did) wonder about the nature of space and time and the vastness of the cosmos, but the job of a professional scientist isn't to gape in awe at grand, vague questions – it is to work on the next question. Having ploughed through all the "easier" questions for four centuries, these very deep questions finally confront us: what are space and time? What is the origin and fate of our enormous universe? We are extremely fortunate to live in the era when human beings first get to meaningfully attack these questions. I just wish I could adjust when I was born so that I could be starting as a grad student today!

There's something to be said for entering a field at a time when it is finally able to "meaningfully attack" difficult and fundamental questions. The issue though is whether anyone has any good ideas that will make headway against such questions. The Standard Model was in place by the mid-70s, and by the time I was a graduate student in the early 80s, the "what are space and time? what is the origin and fate of our enormous universe?" questions were already on everyone's mind as the next things to be thinking about. Starting in 1984, the superstring revolution promised a way to answer these questions.

35 years later, the current generation of graduate students has the same questions to think about, but a long history of failed attempts to consider. In addition, there's the sad story of the unwillingness of leading figures of the field to admit to the failure of the 1984 revolution, and widespread multiverse pseudo-science (often promoted by Arkani-Hamed) to overcome. The only argument that I can see that this is a good time to start an HEP theory career is that it's hard to see how things can get worse…

For some commentary about the interview by Tommaso Dorigo, concentrating on the positive case for a new collider as a tool to study the Higgs, see here.

Comments

1. **SFD**
   March 8, 2019

   " the main argument is for a Higgs factory that would allow a much more detailed study of the Higgs." I find it difficult to believe that the civil authorities, and scientists in other research fields, will think that this argument is enough to justify the $10B tag.

2. **Peter Woit**
   March 8, 2019

   SFD,
   Yes, but that's going to be what the serious debate is really about: now that the Higgs has been discovered and we have some information about its properties, is it worth building a next-generation machine to study it in detail, or should we just give up, deciding this is something that humans can't afford to try and learn about?

   I don't disagree that this is going to be hard to sell to some people, especially scientists who believe that the money would be better spent on their own field.

   But, to all potential commenters, the "is a bigger collider worth it?" question has already been beaten to death recently, and there isn't even yet a specific proposal from CERN to argue about. Unless someone has something new to contribute, I'd rather not host the same tedious discussion again right now, it's one that there will be many opportunities to go over in years to come.

3. **Tom**
   March 9, 2019

   Nima Arkani-Hamed is regularly described as a "superstar theoretical physicist" (Dorigo). But why? To quote from this blog: "The fact that none of the ideas about BSM physics he is famous for (large extra dimensions, split SUSY, Little Higgs, etc…) have ever worked out doesn't seem to slow him down"

   The trick which allowed Arkani-Hamed to become so famous boils down to taking established ideas and making them more "hypey".

   * Extra dimensions -> large extra dimensions

* Extra dimensions -> little Higgs
* Susy -> Split susy
* New methods for scattering amplitudes -> amplituhedron

And he hypes his favorite pet peeves using whatever argument sounds good no matter if it really makes sense.

* everyone knows electroweak charged dark-matter particle sit @1TeV, right? If only he had shared this wisdom earlier….
* "There has never been a better time to be a physicist. […] I just wish I could adjust when I was born so that I could be starting as a grad student today!" Really? What exactly changed in, say, the last 40 years for theoretical physicists?

All this, combined with his whole „memorable character thing" (see below) allowed him to get so influential although upon closer inspection none of his ideas turned out to be worth the hype.

Lately he hyped the proposed China collider to the extent that people started calling it Nimatron. This, of course, was what he was hoping for. This was his/is his chance to leave a legacy since so far, no one will remember his contributions to physics in, say, a hundred years.

Now since the China collider will probably not be built, he hops onto the next opportunity and repeats his arguments. This is his chance to be remembered in the history books as the guy who helped particle physics survive when "ex-particle-phenomenologist-cum-still-blogger" (Dorigo) tried to destroy it.

Everyone who ever met him in person knows that he tries very hard to be remembered, at least, as an extremely unconventional character. (The boots, the whole "I sleep only 3 hours each night and drink 22 espressi each day", his "Impresario"-Style talks in which he "nails" each argument.) And it seems to be working because journalists love to interview him.

But people should really stop paying so much attention to media fame and showboating and instead with a calm head reassess the arguments at hand.

And maybe journalists should ask instead a few young physicist who are starting "as grad student today" if they share his enthusiasm (so far I don't know a single one who does) and how they think the future of particle physics should look like.

4. **Lonely Physicist**
March 9, 2019

Dear Peter,

In my opinion, it seems that by frequently reporting in your blog all the nonsense this guy is spouting in interviews and conferences, you are yourself supporting him and his compaign!

5. **Peter Woit**
March 9, 2019

Lonely Physicist,
You may be right.

Tom,
I don't think Arkani-Hamed just impresses the press, for instance you can see from Tommaso Dorigo's posting that he's quite impressed. For another random example, I just saw this
https://twitter.com/preskill/status/1104449921363529728
on Twitter from John Preskill
"Nima is a magician — he gets my pulse racing over the prospect of measuring the self-interactions of the Higgs particle at a future collider."

I don't think it's helpful to criticize Arkani-Hamed's persona or argue that he's not honest, that he has other motives for what he's saying than that it's what he believes. I don't doubt that he believes the "best time in history to be an HEP theory graduate student" line even though it's way over the top (so much so that I think it seriously hurts his credibility with his colleagues whenever he uses it).

One reason for his influence is that he's legitimately very smart and well-informed, and the everything's fine, positive, full-speed-ahead enthusiasm is a lot more appealing to most people than being told that times are tough and they should be making difficult and unpleasant decisions about how to change their ways. Another reason is that while his own work on how to extend the SM has all failed, it's not like anyone else has done better. Traditionally the field looks for leadership to those whose ideas have succeeded, credentialed by a Nobel Prize. Right now, the youngest of these, Frank Wilczek, is getting to the traditional retirement age, and all the others (Gross, Glashow, Weinberg) are much older. With 40 years of failure under its belt, where does a field look for leadership?

A fascinating conflict here is between Arkani-Hamed and Sabine Hossenfelder. He dismisses her here in a really tasteless way
"It would be only to the good to have a no-holds barred, public discussion about the pros and cons of future colliders, led by people with a deep understanding of the relevant technical and scientific issues. It's funny that non-experts don't even make the best arguments for not building colliders; I could do a much better job than they do!"
basically arguing that she's not competent to be worth listening to (by the way I've seen this tactic used before…). I disagree with her conclusions about whether a new collider would be worth it, but the arguments Hossenfelder is making are serious, widely shared, and she's quite competent to be making them, every bit as competent to do this as C.N. Yang, whose arguments are actually similar to hers (although China-specific).

6. **Schrodinger's Rat**
March 10, 2019

"…the everything's fine, positive, full-speed-ahead enthusiasm is a lot more appealing to most people than being told that times are tough and they should be making difficult and unpleasant decisions about how to change their ways…"

"…where does a field look for leadership?"

Well, look somewhere else! This behavior is pretty much my definition of the exact opposite of leadership.

7. **parisien**
March 10, 2019

As a student I find his comments about being a student a bit naive. Try finding a position anywhere by saying that what you want to do is to think about what is space and time. I think that the current state of academia doesn't really allow for anyone new to even come in and find a new approach to those questions because they're so bogged down with trying to actully find a position of some stabillity and to start any kind of career. Perhaps instead of daydreaming about being a grad student, I feel he should use his position of a public figure to point out the actual problems in academia students coming into it are facing, because if he really wants someone in the next generation to have new idea or approach, better conditions for them are neccessary.

8. **Tom**
   March 10, 2019

   Peter,

   True. It's not just the press. Students and professors alike get starstruck when they meet him.

   And, of course, you're right that criticizing Arkani-Hamed's persona is not helpful. The discussion should focus on substantial arguments and nothing else. But if we subtract from the interview all the hypey yada yada, we are left with: "We should measure the properties of the Higgs as precisely as we can because we might learn something interesting. We might discover dark matter. We can't allow that fundamental knowledge is shoved in old dusty books."

   These are solid arguments. (Maybe except for the dark matter argument). But at the same time there are several good arguments against a new collider (monoculture, opportunity costs, physics case, etc.). And while he claims that he thinks a "no-holds barred, public discussion about the pros and cons of future colliders" would be good, we all know that this will never happen.

   The only person who currently seems to be willing to argue publicly against a new collider is dismissed as a "non-expert". So who is left to "make the best arguments for not building colliders"?

   There is a strong sense of community in particle physics. (A few weeks ago I witnessed how an eminent professor during his talk in a full lecture hall called Hossenfelder "our current nemesis" and no one disagreed.) And while Hossenfelder's arguments may be widely shared (especially among younger physicist), no one seems to be willing to go public. The only chance to hear people's true opinion on these issues seems to be during coffee breaks. Hossenfelder made the conscious choice to waive here "hopes of ever getting tenure". This is, most likely, the price you have to pay if you go public with your concerns.

   One final comment on Arkani-Hamed I would like to add. A few years ago I attended a summer school where during a break he told a group of students (I'm paraphrasing): "To make a career in physics you truly need to convince yourself that what you're doing is the best and most important thing in the world, even if you think it isn't." (Make of that what you will.)

9. **Jesper**
   March 10, 2019

   @ Tom, Peter: as much as I enjoy reading this blog and as much as I appreciate the important work it represents, I do feel that Tom has a point. In the present world of theoretical high energy physics everyone seems to be looking at the top of the hierarchy for new ideas. But the main thing you'll find up there are people who have spent their lives working on and promoting ideas which have not worked out and which we must now conclude are most likely wrong.

   I think that its necessary to spend time discussing and promoting new ideas too. People who think in new directions. There may not be many of them around but by paying attention — and not ridiculing and belittling them — we just might help create a culture where its cool to think more for yourself and not just work within one of the main scientific clusters in contemporary high energy physics.

   If we only pay attention to those at the top of the hierarchy — regardless whether this attention is positive or negative — then we feed all our energy into a system that hasn't worked well for the past decades. By directing all your attention to the top of the hierarchy you indirectly signal that its only those people who are worthy of that attention. But the truth is that those people are the least likely to lead us in a new direction.

   I believe that high energy physics used to be characterised by rebellious minds. People who didn't give a f… about hierarchy and what you might risk if you go against the current. I don't see that anymore. I see a lot of people who are afraid of loosing their status and — if they are younger — of not getting a job. I think that this lack of rebellion is a part of our problem.

   @ parisien. I agree with you. I think that in the present system its almost impossible for a young independent physicist who works on her or his own ideas, to make it. For this reason I have on my blog encouraged young physicists to 'go rogue' — if a young, ambitious researcher is faced with a choice between a career and working on her or his own ideas, then I think this researcher should consider whether it might be best to work outside of academia. This isn't perfect but the world isn't perfect either. People can do this in other professions — see for instance the arts and literature — so why can't scientists? I think that this whole idea that we can only do serious research within a given framework — academia, the universities — is wrong. I also think that it makes us smaller than we are.

   I have done that. I chose to work on my own ideas, which eventually pushed me out of academia. Its not great but its a hell of a lot better than the alternative. And it certainly isn't something that prevents me from doing the research that I want to do. The truth be told, I find it in many ways easier to do research in this way.

10. **DB**
    March 10, 2019

    I must say that I´m a bit awestruck by everything that has been happening in theoretical physics lately.
    I have to acknowledge that I was one of those that, since ten or twelve years ago approximately, I tended to think that everything that "string theorists" were saying was close to the absolute truth, and that we were pretty close to what people used to call a "final theory of everything".
    But thoughts and wishes are one thing, and reality is another one.
    Since the last two years I´ve been having lots of serious doubts about string theory being that so much sought TOE, and reading posts like this one (plus the comments from Tom and the rest), and even listening to people like Nima (who used to be my idol together with Witten), plus some comments Ed himself has been making lately, I think I really need to give a serious thought about all my previous ideas.

    One more thing: it seems clear that A-H himself has almost (90%?) forgotten about SUSY and extra dimensions, which is quite a lot to say.

    Time to go off for a while and double check my thoughts.

    Thanks to Peter for the post, and for all the comments.
    And good luck to everyone.

11. **Yatima**
    March 10, 2019

    "True. It's not just the press. Students and professors alike get starstruck when they meet him."

    That is the mark of a good, energetic *Persuader*. I haven't watched Nima give interviews but does he wield Jedi mind tricks – consciously or not – as those described in the little blog post by Scott Adams about Mr. Trump: [Clown Genius]?

12. **a reader**

March 10, 2019

Dear Peter, I appreciate your blog very much, and I respect your opinions. I have been a reader for many years, probably longer than a decade. As a (rank-and-file) (ex-)physicist, I would like to ask you…
Don't you think that writing "with 40 years of failure under its belt, where does a field look for leadership?" is rather misleading? I guess I know what you mean, but…
Your blog has a wide and large readership, I guess, so when you and others write "with 40 years of failure under its belt, where does a field look for leadership?" or something similar, would you please specify which field? Because some or may be many among your readers without a background in physics will may be guess that you are talking about all physics or all high energy physics rather than (several) subfields of high energy physics theory.
I mean, has experimental high energy physics 40 years of failure under its belt? Do the large collaborations, I mean all the human beings who built LHC, LEP, Tevatron, RHIC, Super Kamiokande and many other HEP-facilities of the last 40 years deserve that label, failure?
I think that sort of expression, without specifying, every time, even if it might seem tedious to do so, which particular subfield you are actually talking about, I think that is quite misleading and rather unfair.

13. **Peter Woit**
March 10, 2019

a reader,
To clarify, the field being referred to is HEP theory, not anything else. I thought this should have been quite clear, since the context is a discussion of the leadership issue with respect to Arkani-Hamed, an HEP theorist.

A possible source of confusion is that an HEP theorist is also trying to take on a leadership role on the question of what HEP experimentalists should do. Another odd aspect of Arkani-Hamed's criticism of Sabine Hossenfelder's views on experimental HEP is that he argues that people should instead listen to HEP theorists Yang and Glashow. Faced with the decision of where to go for advice about the future of HEP experiment, an obvious point to make is that the best answer is not Arkani-Hamed, Hossenfelder, Yang or Glashow, but none of the above. Why not consult instead an actual HEP experimentalist?

14. **Peter Woit**
March 10, 2019

Jesper,

Yes, in the current environment anyone who wants to pursue a career in this kind of theoretical physics needs to contend with the sad state of the traditional academic hierarchy and career path. That's not the topic of this posting, and I don't personally have a good answer for what people should do (which in any case depends on the person and their exact situation). I do hope I can help people trying to find their way by giving a clear-eyed perspective on what the current state of affairs is.

As for promoting new ideas, I do discuss here what I find interesting, while deleting the large number of comments that come in from those who would like to turn the discussion to something different that they find interesting (for those who would like to discuss new ideas with Jesper, I'm glad to see that he has a blog where you can do this).

Two reasons for critiquing the current academic HEP theory hierarchy instead of just ignoring it are
1. This is where students are, for better or worse, now getting trained and will continue to be trained.
2. The LHC null results, together with the failures of string theory and SUSY as theory (together with the multiverse debacle) I think have opened up many theorist's minds to the question of whether the subject is in a crisis due to having headed down a wrong path. Some influential theorists may be more willing to think through the implications of the current situation. As discussed here
http://www.math.columbia.edu/~woit/wordpress/?p=10842
Arkani-Hamed now seems to exist in a superposition of two very different states: Nima1, who thinks all is well, that current HEP theory an exciting success story, and that split SUSY and the multiverse are the answer, and Nima2, who would like to abandon his old ways and restart life as a mathematical physicist, searching for the deep mathematical question that will give us new insight into fundamental physics. I think it's worth trying to change the environment so that the amplitude of Nima1 is suppressed, that of Nima2 is enhanced.

15. **a reader**
March 10, 2019

Thank you Peter. I knew well that you meant hep-th, but, please, you all who commendably engage in a public debate, remember that it is very important that you mark the difference between hep-th, hep-ph, hep-ex and (especially if you talk to a very wide audience) physics-all, because the very real risk is, I think, long lasting damage, undeserved in many, but sadly not all, cases.
As for that Arkani-Hamed, Hossenfelder, Yang, Glashow and others are all theorists (hep-th), I agree with you.
Experimentalists (hep-ex), be proud of your heroic endeavors and epic achievements of the last 40 years, and speak out!

16. **Amitabh Lath**
March 10, 2019

In addition to being smart Nima is incredibly generous. A few years ago we had a bunch of high school students in our summer Quarknet program, and screened Particle Fever one evening for the students and their parents. Nima came, and took questions for hours upon hours. He answered even very basic questions in interesting and novel ways. Nima's enthusiasm for physics infected the kids and parents.

Nima is right to be excited about the state of fundamental physics. It is us humans vs. the universe and although right now our math might not be clever enough and our machines might not have enough energy or luminosity, we will get there and we will crack this. We always have, regardless of politics or personalities.

17. **citely**
March 10, 2019

Wired UK had an article that touched on colliders, whether SUSY is worth another LHC funding round, and quantum computers to process it all. Fluff, but of cultural note (particularly as to where "physics money" is being directed).

https://www.wired.com/story/inside-the-high-stakes-race-to-make-quantum-computers-work/

18. **Bernhard**
March 11, 2019

Nima is right about the motivation for a new collider (Higgs factory) and that we should learn from past lessons and not invoke hype to sell it. The 100 TeV collider argument for WIMP dark matter is not at all helpful in this respect and he's, as usual, making stuff up, counting with the fact that is enough intimidatingly smart that nobody will call BS.

19. **Roger**
March 11, 2019

Can someone explain the dark matter argument to me ? Why should the preferred WIMP mass be 1-3 TeV ? I always that it was theoretically unconstrained up to an order of magnitude.

20. **DDOwen**
March 11, 2019

The rather personal attacks on Sabine Hossenfelder combined with the dismissal of condensed matter physics (sure, the failure to explain high-Tc superconductivity would have some weight *if that were the only thing that CMP were concerned with*, but that's obviously false if you happen to know anything about CMP) do suggest that there's a certain amount of projection going on in NAH's argument.

21. **AcademicLurker**
March 11, 2019

This may be drifting off topic, but it's related to Jesper's comment above.

Sabine Hossenfelder's description of the HEP theorist career path in her book makes it clear that things haven't changed since Peter and Lee Smolin published their books 13 years ago. The thing that most strikes me as an outsider is the extremely short time frame that's imposed by the system of postdoctoral grants. If you need to start looking for a new position a year after you start your current one, then you want a paper at least submitted by then, which means you need some publishable results within not much more than 6-7 months.

I'm curious about people's impressions of to what extent this is intertia vs a deliberate choice. Do the powers that be in charge of dispensing fellowships & etc. affirmatively believe that this is the best way to select for new theory faculty members? Or is it more a case of "Well, this is the system we have and anyway we have to pick winners somehow so what else are we supposed to do?"?

22. **Peter Woit**
March 11, 2019

DDowen,
I think you're misreading Arkani-Hamed. His comment about high-Tc superconductors wasn't about failure of condensed matter physicists to explain the phenomenon, it was just pointing to the history of unwarranted enthusiasm for the prospects of using high-Tc superconductors to develop much cheaper magnets suitable for a proton-proton collider. This same argument is now being brought up as an argument against the HE-LHC and FCC-hh proposals: why not wait a few years for cheaper high-Tc superconductor magnets before planning a new collider? He just seems to be making the reasonable point that people have been saying this for years, but no viable technology of this kind has appeared, and there don't seem to be serious prospects for it anytime soon.

Roger,
I'd also like to see a reference for this. Arkani-Hamed here and here
http://www.math.columbia.edu/~woit/wordpress/?p=10824
seems to be claiming that a naive calculation of a weak interaction strength WIMP with the right abundance to be dark matter gives a mass of 1-3 TeV, and a cross-section too small to be seen in direct detection experiments, but accessible to an FCC-hh machine. I'd never heard such an argument before, curious to know what he's basing this claim on.

23. **DM theorist**
March 11, 2019

For Peter, Tom, Roger etc

I'm giving some pre-LHC references below (I even threw in one from Nima, sections 1.2 & 1.3) showing that DM candidates that are defined by their SU(2) representation and gauge interactions alone, typically are required to be very heavy and well out of the LHC range. In the MSSM you actually had to work a bit (see the Nima ref) to get a light candidate that gave you the right relic density.

https://arxiv.org/abs/hep-ph/0601041
https://arxiv.org/abs/hep-ph/0610249
https://arxiv.org/abs/0706.4071

The general idea is independent of SUSY, and just focused on WIMP hypothesis, taking the weak part, SU(2), to be literal and calculating the thermal relic density. It just so happens that some of the minimal reps, the doublet and the triplet can also be realized in SUSY as the Higgsino and Wino (when you don't consider mixing etc).

Theorists were talking about this before the LHC. However, at that time people focused more on what you could see at the LHC rather than what you couldn't. Obviously when you have an experiment you want to look at everything you can test with it. Nevertheless, this was well known amongst theorists that WIMP DM with basic reps of SU(2) implied masses out of reach of the LHC. As for the last part that Peter asks about with direct detection, when the WIMP mass is higher the number density of WIMPs is going to be lower. This is why the bounds decrease in the the cross section vs mass plain on the standard direct detection results that you see, while at low WIMP mass it's from threshold effects (there's a ton of effort on this in the dark matter community now on the low energy side where you can devise new experiments). So this is just the standard systematic problem for a heavy WIMP, but of course if there is something charged under a representation of SU(2), then a higher energy collider could produce it.

24. **Jesper**
March 11, 2019

@ AcademicLurker

I think that Lee Smolins book "The Trouble with Physics" very accurately describes the situation (and I have written somewhat of an update on my blog), where the short time-frames that you describe makes it very difficult to work on new ideas (especially if they are your own). You need to publish a lot and you need to publish fast. That system very strongly favours the technicians – those who are extremely good at solving technical problems – whereas the visionaries (I believe that Smolin called them the 'seers'), i.e. those who are good at producing new, creative ideas, are the losers.

During my career I have been a semi-insider to several of the leading communities in theoretical high energy physics and I have been in close contact with essentially all of them. And it has been my very clear impression that those people, who are at the top of the hierarchies, are predominantly technicians. And it is my impression that they generally think that the present system is fine. After all, it is basically the same system – perhaps a little rougher, perhaps a littler sharper – that they had to fight their way through during their own careers.

Again, I think that Smolins book describes the situation very well. And I believe that this applies to essentially all fields in theoretical HEP – including LQG. The trouble with physics is that the present system favours researchers who are good at digging very big holes (metaphorically speaking) and disfavours those, who are good at finding the right spot to dig those holes. The result being that everyone digs their holes in the same spot.

25. **Peter Woit**
March 11, 2019

DM theorist,

Many thanks for the explanations and references!
Those references led me back to something slightly earlier and simpler, this https://arxiv.org/abs/hep-ph/0512090
about "Minimal Dark Matter"
Yes, as Arkani-Hamed claims, the models discussed (pre-LHC) there are in the few TeV range, a range that would require 2-4 times the LHC energy to explore. It's interesting that these models got so little attention pre-LHC null results.

Also interesting is that the above paper gives a version of the standard direct detection plot (figure 2), with, besides the usual SUSY CMSSM blob mostly now ruled out, predictions based on these sorts of minimal dark matter models which are at higher mass, with cross sections that the latest generation of experiments should start being sensitive to.

26. **Peter Woit**
March 11, 2019

Jesper/AcademicLurker,
This has gotten way off topic, it's an interesting question, but, enough for now.

27. **DM theorist**
March 11, 2019

Hi Peter,

Glad to help, and yes one of the references was a followup on Minimal Dark Matter (MDM). These focused more on the 5-plet or 7-plet to get stability from the SU(2) rep, instead of having a parity which most models of DM have, and the cross section for direct detection grows like n^4 where n is the size of the representation. I don't remember the exact differences between the MDM calculation, but I recall 10^-47 cm^2 as the pure Wino case in SUSY, and at ~3 TeV in mass this is below the direct detection bounds even for future experiments. Not to say that this couldn't ever be tested outside of a collider (indirect detection is also interesting, but with its own uncertainties), just that what Nima said was accurate for the minimal representations.

I think all decent theorists were well aware of the models. However, I think that sometimes the synergy of being able to test things in multiple ways biases peoples interests, and at least for the SUSY case, they weren't as "natural" as having a light Higgsino. Now that we've seen nature isn't as "natural", the WIMP candidates themselves still just have the predictions they have. Some might call this bias in a pejorative sense, I think it's more just about opportunism. If I have a model that I can test with colliders, direct detection, and indirect detection that's very cool and super testable. If I lose one of those handles it's still interesting, but more difficult to interpret. I don't think there's a secret theory cabal trying to suppress ideas or not tell the truth about the possibilities, as you see in the Nima paper they were laid out honestly. Experimentalists are just naturally going to be most interested in the models they could see with on-shell experiments and thereby that gets a broader discussion in the community.

28. **Peter Woit**
March 11, 2019

DM theorist,
Thanks for the further comments. I understand there was no secret theory cabal at work, and people reasonably concentrated on looking at models that the LHC could test, but the way certain models got heavily publicized, despite well-known problems with them, is going to cause a credibility issue going forward.

The most outrageous example to me was the whole "we may see extra dimensions at the LHC" business, here I suspect if anyone tries this for future colliders they'll get laughed out of the room. Second though was the "we expect to see "natural SUSY", with lots of states light enough to be easily seen at the LHC", even though there was lots of indirect evidence against such states. To his credit, on some days Arkani-Hamed would mention such evidence and talk about split SUSY, on other days though he would go on about how many gazillion gluinos the LHC would produce and how the field needed to get organized to be able to disentangle all the many new states the LHC would see. I gather the WIMP DM story is a variant of this problem.

29. **Dave Miller**
March 12, 2019

Peter,

I wonder if you (or anyone) can elaborate on what we will get out of more details on the Higgs?

I myself had also concluded that looking more at the Higgs is the obvious next step. But… can we really get a good handle on the Higgs potential with the next collider? Or if we see something slightly different than we expect, will that merely tell us that the calculations for what to expect were too tough for the phenomenologists to do accurately? And, no matter how accurately we measure Higgs phenomena, will that tell us about anything beyond the Higgs, or will it just satisfy the curiosity of those of us who long ago learned the basic info about the Higgs?

I honestly don't know the answers to these questions, but, hopefully, some people actually do have answers to such questions! It seems to me that fleshing out the answers is key to making the case that deeper studies of the Higgs are worthwhile.

Dave

30. **tulpoeid**
March 12, 2019

Some "superstars" have to understand that standing on the shoulders of giants doesn't make themselves giants.

Also, I feel that it's important to voice a concern regarding Hossenfelder's treatment: She is by far not the only HEP physicist, and I include experimentalists here, who opposes the next large thing (although a couple of her arguments are hard to swallow but this is not my point here). I've been both at LHC and DM experiments; afaik most people in DM and other "small" experiments don't fear at all that HEP will die without a larger machine right now. There are several of their colleagues at LHC who don't disagree. There is a number of other smaller collider-based searches, both running and proposed, which at the very least will make us search in smarter ways.

Imho people who've established their careers on the energy frontier are just trying to single out Hossenfelder in an effort to persuade the public that there is only one clown who opposes the mainstream.

31. **ztimashi**
March 13, 2019

    Regarding your comment "Traditionally the field looks for leadership to those whose ideas have succeeded, credentialed by a Nobel Prize" I find it very curious, to say the least, that all non-emeritus theorists at IAS are now string theorists.

32. **Peter Woit**
March 13, 2019

    ztimashi,
    Arkani-Hamed isn't a string theorist, and, actually I think that's a reason his views are quite influential.

33. **Thomas Larsson**
March 14, 2019

    ztimashi,
    It is becoming increasingly difficult to look for leadership from people with a Nobel prize, since there is no longer any active HEP theorist with that qualification. Or at least none who is below normal retirement age, which in Europe is 67 or less. Wilczek is 68, I think.

34. **Stephen**
March 14, 2019

    "There has never been a better time to be a physicist" Really? Maybe Nima should study up on the history of particle physics between the mid 50s and the mid 70s.

This Month's Hype

March 14, 2019
Categories: This Week's Hype

Physics Today seems to have decided to deal with Sabine Hossenfelder's criticism of a future collider by publishing the least credible possible response: a column by Gordon Kane arguing that string theory predicts new particles of just the right mass to be likely beyond the LHC reach, but accessible to a higher-energy proton-proton machine.

In the column, we learn that:

> In recent years there has been progress in understanding those models. They predict or describe the Higgs boson mass. We can now study the masses that new particles have in such models to get guidance for what colliders to build. The models generically have some observable superpartners with masses between about 1500 GeV and 5000 GeV. The lower third or so of this range will be observable at the upgraded LHC. The full range and beyond can be covered at proposed colliders. The full range might be covered at a proton–proton collider with only two to three times the energy of the LHC. One important lesson from studying such models is that we should not have expected to find superpartners at the LHC with masses below about 1500 GeV.

Kane has a long history with this kind of thing at *Physics Today*, publishing there back in 1997 much the same sort of argument, in an article entitled String Theory is Testable, Even Supertestable. According to the Kane of 1997, a generic "prediction of string models" was a gluino at around 250 GeV, just beyond the Tevatron limits of the time. Thirteen years later, *Physics Today* had him back, publishing an article entitled String theory and the real world. I don't have the time to do a full search, but, by 2011 after the first LHC results came in, Kane had a string theory prediction of a gluino mass at 600 GeV, or "well below a TeV".

As better LHC results have come in, each time Kane has issued a new "string theory prediction" that the mass is a bit higher, just about to appear at the next round of LHC results. The last version of this I had seen (see here), was from 2017 and predicted "that gluinos will have masses of about 1.5 TeV". This is already disconfirmed and out of date, with Kane now telling us "between about 1500 GeV and 5000 GeV."

For some other evidence of how Kane deals with the problem of having predictions falsified, one can compare the 2000 and 2013 versions of his popular book on SUSY, an exercise I went through here.

At this point, the argument that we need a new collider because "string theory predictions" say that it will see gluinos has zero credibility. I don't know of any other theorist besides Kane who believes such a thing. That *Physics Today* is publishing this is just mystifying. Perhaps a collider skeptic there has come up with this as a clever way to back the Hossenfelder side of the argument.

There are some other odd things in the piece, one that stuck out for me was this bizarre claim about recent history:

> We now know that if Fermilab and the US Department of Energy had taken the Higgs physics more seriously, the Tevatron would have discovered the Higgs boson years before the Large Hadron Collider did.

I see Will Kinney has more about this on Twitter.

Update: More commentary on this from Jon Butterworth and Sabine Hossenfelder.

Comments

1. **Shantanu**
   March 14, 2019

   I urge everyone to write a response to this Physics today article. I am planning to write one. Peter is it okay, if we borrow mostly from this article

2. **Peter Woit**
   March 15, 2019

   Shantanu,
   If people want to contact Physics Today they should. For the editors there to see the problem they have, you don't need to point here, just to their own pages. A comparison of what Kane wrote there in 1997 and is writing now should be all that is necessary.

   I'm quite serious that the publication of this kind of thing is going to do damage to the credibility of HEP among other physicists, and be very unhelpful for the argument for a new collider. All those who went public to counter Hossenfelder should be doing the same here. That this kind of thing has been going on unchallenged for decades is part of the problem.

3. **John Baez**
   March 15, 2019

   Will Kinney has some nice images of Gordon Kane's boldly stated wrong predictions from 1994, 2001, 2015 and 2019:

   https://twitter.com/WKCosmo/status/1106287115720146945

   I don't know Kane. What about him makes him so eager to do this? Of course if you can keep convincing people to build particle accelerators based on a wild guesses that keep turning out to be wrong, and they keep forgetting you're always wrong, you have a motive to keep doing it. But that applies to anyone. Why does Gordon Kane, among all particle physicists, have such a spectacular track record of doing this?

4. **Peter Woit**
   March 15, 2019

   John,

   Kane has always been an outlier, with very few others taking seriously the argument that there were "string theory predictions" relevant to LEP, the Tevatron, the LHC, or any other collider (and such arguments I think had nothing to do with building and operating those machines). It's not so surprising to me that a theorist would have delusional views about their own favorite theoretical models. What I don't understand is why Physics Today (or anywhere else) continues to provide him a prominent venue to repeatedly make such obviously dubious claims.

5. **Low Math, Meekly Interacting**
March 15, 2019

I'm guessing that you meant to be facetious in your speculation about PT's motives for publishing. You might nonetheless be spot-on.

6. **A Former Professional Higgs Boson Hunter....**
March 15, 2019

Fermilab had a snowball's chance in hell of finding the Higgs given how far LEP pushed the mass limits…

Take it from me, I was there and was responsible for showing that Fermilab could get lucky if the mass was ~160 or so…

That statement from Physics Today is gaslighting at it's finest…

7. **Peter Woit**
March 15, 2019

AFPHBH,

Thanks!

The Higgs hunt at the Tevatron was covered extensively on this blog, so I'm well aware that there was an intense effort there, with, it turns out, no chance of success (i.e. no 5 sigma detection at 125 GeV). Thus my surprise at the Kane claim.

For an early posting about this, see this blog entry from 2005
http://www.math.columbia.edu/~woit/wordpress/?p=163
I think it was the first time I interacted with Tommaso Dorigo, who sent in a helpful comment clarifying the situation.

8. **Supernaut**
March 15, 2019

"… least credible possible response: a column by Gordon Kane…" I would have loved it if PT published the article but written by an accomplished experimentalist who is also a good writer like, say T. Dorigo instead.

9. **Garrett**
March 15, 2019

Too bad Gordon Kane can't bet his own $20B on building the FCC and finding his predicted superparticles. That would likely lead to the optimal case of the FCC being built and him losing money for being wrong. Instead, we'll have the FCC not built and him continuing to be profitably wrong.

10. **Peter Woit**
March 15, 2019

Garrett,
He has bet about $100, but bet not likely to pay off for several years, see
http://www.math.columbia.edu/~woit/wordpress/?p=7160

11. **A Former Professional Higgs Boson Hunter....**
March 15, 2019

Peter,
the Tevatron folks gave the proverbial college try, but the machine and the detectors were not quite up to it. When all the smoke cleared they could discern
WZ -> l nu bbar production, the standard candle, at about the 2-3 sigma level.

FWIW, from a conversation I had many years ago, the current CERN DG had no doubts about the WW channel around 160 but thought that the VH modes were wishful thinking. The CERN people were freaking out that FNAL could scoop them for a Higgs in the 150-170 GeV region…

12. **Anonyrat**
March 15, 2019

HEP superstring predictions for what colliders might find are degenerating into the weekly astrology column that so many magazines carry.

13. **Peter Woit**
March 16, 2019

Anonyrat,
I don't think it's a good analogy. Horoscope predictions are sometimes correct.

14. **Math Phys**
March 17, 2019

Physics Today used to be a serious publication.

15. **TonyG**
March 17, 2019

Gordon Kane seems to be on a PR offensive in support of his past claims that string theory makes verifiable predictions:

https://inference-review.com/letter/beyond-the-standard-model

16. **A Former Professional Higgs Boson Hunter....**
March 17, 2019

Funny that given the mess that theoretical particle physics has become, I am somewhat surprised that this paper has not been more closely examined:

https://arxiv.org/abs/hep-ph/9312316.pdf

Mh = 125 GeV and Mt = 175 GeV, a full two years before the top was found….

Not being a theorist I can’t comment whether there is any real meat to this or was it a lucky parameterization of our ignorance…

17. **Blake Stacey**
March 17, 2019

I wish I’d gotten in on some of those bets. My habitual pessimism could have paid off, for once.

18. **Math Phys**
March 18, 2019

Blake Stacey,

You are not the only one, it has nothing to do with pessimism, and people have been making these bets with G Kane since 1984.

I was present at a party in Ann Arbor in 1984 when I heard T Veltman say *“Gordy, if they discover supersymmetry, I’ll eat my hat.”*

19. **Doug McDonald**
March 18, 2019

The “what if Fermilab” question: since we know that Fermilab actually made and saw Higges, but nowhere near well enough to hope for 5 sigma, what could have been done to get enough?

Presumably that does not include increasing the energy. But it could have included higher rep rate for high luminosity. And what about adding a better, more modern, (LHC-like) detector or detectors? These are technical questions that someone probably thought about. Was the political situation post SSC-demise so bad that there was no hope?

20. **A Former Professional Higgs Boson Hunter....**
March 19, 2019

Doug,
you can whip a mule all you want but it ain’t gonna win the Kentucky Derby.

The Tevatron Run II involved ambitious major upgrades to both the existing detectors and the accelerator. Even with them, a SM Higgs was never really in reach. For example the silicon vertex detectors critical for b-jet tagging would have fried for the luminosity required to see a signal, the rad-hard electonics simply did not exist yet. The calorimetry was also not up to snuff in coverage in eta, segmentation and sampling resolution implying that jet-jet mass resolution was always going to be a limiting factor.

A heavy 4th generation quark might have bumped up the cross section to almost observable levels but that was already ruled out by LEP. Only an oddball Higgs would have been observable (e.g. fermio-phobic, very high tan-beta, triplet etc…).

The only realistic window was in the mass region around 2M_W and even that was a stretch. Personally I think it was remarkable the Tevatron program closed the mass region that at one time was seen as the hardest one for the LHC to cover…

21. **Julius**
March 19, 2019

Unrelated, but are you going to cover prof. Karen Uhlenbeck’s contributions to an actual understanding of physics?

22. **David Metzler**
March 19, 2019

This is OT, but I think the award of the Abel Prize to Karen Uhlenbeck is quite noteworthy, and relevant to the math/physics intersection covered here. Any thoughts, Peter?

23. **Peter Woit**
March 19, 2019

Julius/David Metzler,

Sorry, but I’m on vacation this week, and no time for more than saying congratulations to Karen. The usual suspects (e.g. Nature and Quanta) seem to have quite good articles about this that I can’t compete with.

24. **Jens Franke**
March 23, 2019

It would perhaps be interesting to have a list of papers by serious physicists which really gave a reasonably correct PREDICTION (i.e., dating from before 2012) of the Higgs mass. Besides the Kahana/Kahana paper quoted earlier in this thread the one I know of is by Shaposhnikov/Wetterich arXiv:0912.0208 : “Detecting the Higgs scalar with mass around 126 GeV at the LHC could give a strong hint for the absence of new physics influencing the running of the SM couplings between the Fermi and Planck/unification scales.”

25. **A Former Professional Higgs Boson Hunter....**
March 23, 2019

And here you go…

https://arxiv.org/pdf/0708.3344v8.pdf

The compilation is up to version 8, and the Kahana’s paper is still the best one especially given when it occurred and what the direct limits on the top quark and Higgs mass were at the time. Making Higgs boson mass predictions after LEP has put 112 GeV lower bound is not as nearly as impressive…

26. **Jens Franke**
March 24, 2019

Thank you. I suspect that many papers on the list where it says “many supersymmetric particles” under “other predictions” have been ruled out by LHC experiments. I also suspect that Shaposhnikov/Wetterich is consistent with current experimental data. If Kahana/Kahana get the “Higgs as a deeply bound state of two top quarks” that might be testable by now. I wonder whether it is consistent with LHC data.

27. **A Former Professional Higgs Boson Hunter....**
March 24, 2019

No idea if there are other testable predictions that can be gleaned from the Kahanas. At first glance, I am pretty sure it is consistent with current LHC results. It does however suggest to me that a e+ e- machine capable of studying the t-tbar threshold could yield a surprise or two that the LHC might not be capable of discerning.

It is pretty clear to me that a very heavy top quark is somehow related to the mechanism of EWSB and creating the vev of the Higgs potential. It just “smells” that way. That being said, I am not a theorist, so I can’t really say if those 2 papers that predicted the correct mass are showing us something of fundamental significance or were “luck” based on choices of boundary conditions or something else…

28. **Amitabh Lath**
March 24, 2019

AFPHBH, I notice that the Kahanas’ paper is dated Dec 1993. That was when SLD had collected around 50k Z events made with polarized electrons and the production asymmetry was turning out larger than expected, which (along with the latest Tevatron top mass limits) restricted the top mass window to about 5 GeV. (Full disclosure, that left-right asymmetry was my thesis).

I don’t think we put anything out in 1993 but if they had their ear to the ground (like good theorists do) the Kahanas may have picked some rumblings.

Note that top mass comes in squared in the Weak asymmetries, while the higgs mass comes in log. The asymmetries narrowed the Higgs mass to less than 200-ish, so for the Kahanas to nail that is pretty impressive.

29. **A Former Professional Higgs Boson Hunter....**
March 24, 2019

I was part of SLD when it stood for “Slow Lingering Death”. Never saw data.

Having those CCDs so close to the IP made SLD a very powerful detector for some key measurements especially when coupled with the polarization. It was a great complement to LEP…

30. **Dirk Freyling**
April 5, 2019

David and Sid Kahanas predictions about the Higgs-Boson-mass and the Top-Quark-mass (1993!!!) in a “parameter free fashion” are very precise. Source: https://arxiv.org/pdf/hep-ph/9312316.pdf

According to the standard model (SM) predictions are not possible. How do you explain the obvious discrepancy?

Peter Higgs knew about their work … he said, “You’re from Brookhaven, right. Make sure to tell Sid Kahana that he was right about the top quark 175 GeV and the Higgs boson 125 GeV” [Kahana and Kahana 1993].” Source:https://arxiv.org/pdf/1608.06934.pdf

One would assume that highly accurate calculations about the Top-quark-mass and the Higgs-mass are remarkable. Why didn’t the “Kahanas” get the “proper” attention? Why is there no adequate mention about these theoretical achievements?

I strongly believe that Sidney and David Kahana’s predictions need to be published and discussed again. With reference to Nambu Y and Jona-Lasinio G 1961 Phys. Rev. 122 (1) 345-358 (https://journals.aps.org/pr/pdf/10.1103/PhysRev.122.345) it seems that the entire SM-project had already been completed (…“methodical circular conclusions”) at the beginning of the 1960s.

For further reading see https://arxiv.org/pdf/1112.2794.pdf … “predictions by the authors D. E. Kahana and S. H. Kahana , mH = 125 GeV/c² uses dynamical symmetry breaking with the Higgs being a deeply bound state of two top quarks. At the same time (1993) this model predicted two years prior to the discovery to the top its mass to be mt = 175 GeV/c²…”

Incidentally the list of 96 Higgs-mass predictions by Thomas Schücker (https://arxiv.org/pdf/0708.3344v8.pdf) is a “good” reference of how and when predictions were made.

31. **Mitchell Porter**
April 5, 2019

Dirk Freyling – I agree that it’s strange just how little attention that paper has received (all the more so since Peter Higgs himself evidently knew about it!). I cannot find a single serious discussion of that model, formal or informal, that has taken place. By contrast, Shaposhnikov and Wetterich 2009, another paper which managed to predict the Higgs mass through heterodox means, is now approaching 300 citations.

It’s not as if the Kahanas did something completely alien to the known paradigms of physics. There are hundreds of papers on NJL-type models. You would think that someone who knows the topic, would have ventured a commentary by now.

The Shape of a Life

March 23, 2019
Categories: Book Reviews

I just finished reading The Shape of a Life, which is the great geometer Shing-Tung Yau's autobiography, co-authored with Steve Nadis. It's quite fascinating, and an essential read for anyone interested in the history of modern mathematics. Yau has been for a long time a central figure in the field of geometric analysis, so this is in some ways as much an autobiography of the subject as well as of the man.

Back in 2010 I wrote here about an earlier volume by Yau and Nadis, The Shape of Inner Space. What I really liked about that book (and discussed in some detail there) was the autobiographical material about Yau. Much of the book though was devoted to topics like string theory attempts to get physics out of Calabi-Yaus, with a discussion that was detailed and accurate, but to my mind often not of great interest (since these attempts don't work…).

The new book seems to have been written specifically to appeal to me, greatly expanding the autobiographical material of the earlier book, while limiting the discussion of dubious speculative physics. There is still a fair amount about physics, but this time more focused on another of Yau's interests, the mathematical theory of general relativity.

The book begins with the story of Yau's early years in Hong Kong, how he managed to survive an impoverished childhood, avoid becoming a duck farmer, and ultimately find a way to get to the US and graduate study in mathematics at Berkeley. It's a compelling story of that period and those places. It's also about the best example I can think of to show how bringing someone with undeveloped talent into the environment of a first-rate research university can change their life, liberating them to accomplish great things, with dramatic impact on their intellectual development as well as that of a whole field.

Yau has always had a deep interest in the history of mathematics, and the story he tells of his intellectual development explains in detail how his own work and ideas grew out of earlier strands of thought. Even as a graduate student, he had started to develop the point of view that has been so fruitful in geometric analysis, using the study of non-linear partial differential equations to prove theorems about geometry and topology. Besides his proof of the Calabi conjecture, this ultimately led to the proof of the Poincare conjecture, a story Yau explains in detail.

Over the years Yau has been involved in various controversies over priority for mathematical results. In this book he doesn't shy away from discussing these, but generally gives a measured explanation of his point of view on what happened. There's also a fair number of often amusing stories about mathematicians and the math community that liven up the history. For one sort of example, there are Yau's descriptions of his culture clash with the long-haired, pot-smoking Berkeley of 1969. For another, here's a story about Richard Hamilton (of whom Yau has a very high opinion) and his 1982 lectures at the IAS:

> Hamilton, who had come from Cornell, stayed for a week in an IAS apartment. At the end of his stay, the chief math secretary was livid because Hamilton had made a huge mess of the apartment, and it took a long time to clean up the place. On the other hand, he had given some wonderful talks, and collaborations between Hamilton, my students, and me picked up from that time forward. So, on balance, his visit would have to be called a great success. Hamilton may have posed some challenges to the cleaning and janitorial staff, but he had posed even more consequential challenges to the mathematics community, some of which were taken up by members of my group.

Yau is generally considered a major figure not just for his research, but also as a politician of the mathematics community, deeply involved for many years in efforts to build or expand research centers, here and in China. A recent example is the creation of the CMSA at Harvard. He has a lot to say about the stories of these efforts, and he definitely does not do so with the style of the politician careful to offend no one. In this book you get Yau's honest, unvarnished version of what happened, as well as his analysis of some general problems, and I won't be surprised if some people take offense at this material.

One thing there's perhaps a bit too much of in the book are the references to his conflicts with his advisor Shiing-Shen Chern (which I'd somehow never heard about before). A major touching theme though throughout the book is that of fathers, sons and traditions of filial piety. There's a lot about Yau's father (who Yau very much looked up to) and quite a bit about his sons. On the mathematical side, there's a lot about his numerous students, many of whom have gone on to important academic careers. As his academic father, Chern also fits into this theme, although not so felicitously. At the end of the book, Yau looks forward to his own future as, like Chern before him, the grand old man of the field. He's planning more teaching and less research, and taking pleasure in his mathematical legacy and progeny.

Comments

1. **Lonely Physicist**
   March 24, 2019

   This is the other great thing about this blog. Besides finding fresh news about exciting ideas in mathematics and physics, we, readers, are always informed about any new interesting book about physics or mathematics. I always rush buy the books I find reviews of in this blog, and I've never regretted it. Thank you Peter!

2. **tenured physicist**
   March 24, 2019

   I second LP!

   I've again started reading books since I got tenure last year, and I always check out books that are reviewed here.

3. **luysii**
   March 24, 2019

   Some of the non-mathematical sociology in the book is also fascinating. Yau was immediately attracted to a physics grad student (who eventually became his wife). He did not approach her, as they had not been introduced. 18 months later when he was about to leave, he imposed on a friend to have a dinner to which she was invited. Courtship and marriage followed.

   After they were married, they lived separately for a time with their parents because of work.

   Yau is also hard on some Chinese mathematicians, who see it as a means to a comfortable life, as opposed to many American mathematicians who do math because they love it and can't think of doing anything else.

4. **Bananeen**
   March 25, 2019

   This is a question I have which is somewhat related to Shing-Tun Yau:

on the “about” page of the CMSA at Harvard, there is the following quote by Yau, who is the center’s first director: “The center will not only carry out the most innovative research but also train young researchers from all over the world, _especially those from China_. The center marks a new chapter in the development of mathematical science.” (http://cmsa.fas.harvard.edu/about/)

I couldn’t find any mentioning of support coming from China for this center, so is it just Yau’s own intent to single out Chinese researchers?

5. **Peter Woit**
   March 25, 2019

   Bananeen,
   The CMSA is funded by Evergrande, a Chinese property developer, see

   https://news.harvard.edu/gazette/story/2013/12/harvard-announces-evergrande-support-of-three-initiatives/

   Because of his own personal story, Yau has always been interested in bringing talented young Chinese researchers to places like Harvard, and I assume this has always been part of the mission of the CMSA.

6. **Bananeen**
   March 25, 2019

   Peter,

   Good to know. I only wish they made it more transparent on CMSA’s website.

7. **A curious reader**
   April 21, 2019

   Many thanks for the review! After reading your review, I bought and read the book in a couple of days. As someone with some acquaintance with Prof. Yau’s family, it’s amusing to see some of the family stories and pictures in the book. Very much admire Prof. Yau’s mathematical achievement, his honesty, and all he has done for the field and for the country where he was born.

Some Quick Items

March 25, 2019
Categories: Film Reviews, Uncategorized

A few quick items:

- This past weekend I went to see the new film Out of Blue, which sounded promising: a murder mystery based on a Martin Amis book, set in New Orleans, starring Patricia Clarkson, with a plot involving lots of deep ideas about physics. Unfortunately, the film was pretty awful, for a review from a professional, see here. There was a lot of physics, I think intended to add philosophical depth, but it was just the usual Schrodinger's cat, black holes, dark matter, multiverse mumbo-jumbo. The *Variety* reviewer appropriately ends her review with

> It makes one feel a little bit embarrassed for the multiverse.

- Sticking to the sophomoric, I was searching through old boxes of stuff and turned up a paper I wrote, Quantum Theory and Reality, about the interpretation of quantum mechanics for an expository writing class during my first year (1976) of college. While it was my first year, I did have sophomore standing. Rereading the thing, I'm glad to see that I've learned a few things since my sophomore year, but on the other hand, some of my views haven't changed (I still don't think "hidden variables" work…).
- Ethan Siegel at Forbes has This is Why The Multiverse Must Exist. By now, all I can do is refer to this FAQ.
- Results using the full datasets of the LHC Run 2 are starting to appear, some of them in talks given at last week's Moriond conference in La Thuile. There are summaries available from CMS, ATLAS and LHCb. Referring to the absence of any significant evidence of new particles or anything inconsistent with the SM, in these results and in a new result from BELLE, Jester comments:

> La Thuile: Where Hopes Melt Away.

This week, there's another ongoing "Winter" HEP conference ("Winter" I guess means you can go skiing…), at Aspen.

- I was sorry to hear of the recent death of Jean-Marc Fontaine, at the age of 74. Frank Calegari has an appreciation of Fontaine and his work here.
- For more positive recent developments in arithmetic geometry, I recommend Peter Scholze's lecture series at UCLA on Prismatic Cohomology, discussed by Terry Tao here. In related news, this week at MSRI there's an interesting workshop on Derived Algebraic Geometry and its Applications.
- For an interview with Eric Weinstein, who, like Sabine Hossenfelder, is always thought-provoking on the great question of why fundamental physics has gone off the rails, see here. I think he may have a point about Tom Lehrer.

Comments

1. **Dave Miller**
March 26, 2019

Peter,

Do you have a link to any discussion of your view that "hidden variables" does not work?

It seems obvious to me that hidden variables *does* work, but in such an aesthetically unappealing way that it is hard for me to believe that hidden variables is really true: the main problem of course is that Bell's theorem means that making hidden variables agree observationally with relativity will be a rather ugly kludge.

As I recall, Bell goes into all this in detail in his *Speakable and Unspeakable in Quantum Mechanics*.

In any case, I think understanding various thoughtful objections to the different approaches to QM may help us all understand better what the "mystery" is that bothers so many of us.

(By the way, I too wrote a term paper on the foundations of QM, in my case focusing on Bell's theorem, as an undergrad back in the mid-'70s.)

Dave

2. **Peter Woit**
March 26, 2019

Dave Miller,

Hidden-variable theories I know about have all sorts of problems accounting for quantum fields, for relativity, for spin, with getting around these problems requiring constructing something far more complicated and ugly than conventional QFT, for no discernable benefit. I understand that some are hopeful that a successful hidden-variables theory exists and want to pursue that quest, but they have not so far succeeded.

All, I don't want to start another discussion here about interpretational issues in QM, based on my naive views when I was 18 years old, and even less want to engage in the usual tedious ideological arguments over hidden variable theories. Please wait until there's something serious to discuss.

3. **Low Math, Meekly Interacting**
March 26, 2019

I'm just struck by how similar your essay is to any number of articles on the subject I've read over the past year, in terms of framing the issue. I'm with those who feel that "quantum foundations" has not been a terribly progressive field since its inception. DOA might be a forgivable assessment, if overly harsh. I hope someone comes up with a good experiment that actually…decides something one of these days. I'm amazed people approach the subject in the absence of new data without a sense of despair.

4. **Maximillian Tresmond**
March 26, 2019

Naive views? Hardly. Eighteen years old, just out of high school, and writing at a level of scientific sophistication that most people would never reach in their entire lives. That's amazing. We are all fortunate to have a real scientist like you, rather than a science communicator, help enlighten us about the pressing issues in physics and provide us with a deep level of insight that is hard to come by just in reading science news.

You really are an outstanding mind, a great scientist, and an ambassador to the profession that continues to inspire other people to get into science and mathematics.

Thanks for everything you've done and continue to do Dr. Woit.

p.s. Was than an IBM Selectric typewriter?

5. **Peter Woit**
March 26, 2019

LMMI,
Yes, the static nature of this subject is both striking and depressing. You can see perhaps why I have little patience for listening to exactly the same arguments I was reading about over 40 years ago. It is kind of ridiculous to often see the issue framed as the "brave new insurgency against the old Copenhageners", when these insurgents were active in the 50s and 60s, are all now long dead.

Maximillian Tresmond,

Thanks for the over-the-top compliments, but I'm sure our high schools and college expository writing courses to this day have lots of students writing similar things. I think it was a Smith-Corona electric, not an IBM Selectric.

6. **Peter**
March 26, 2019

Hi,

I don't want to start a discussion about the interpretations of QM. All the interpretations I've seen are untestable, unobservable or equally (or even more) mysterious than the thing they try to "explain".

However, I have a question.

How seriously did you take these "alternate interpretations", "tinkering with the laws of logic" etc. you write about at the end of the paper?

I remember reading about these alternate interpretations when I was a young student of physics. Arrogant as I was, I immediately thought "This is just silly. The whole mathematical machinery of QM is developed with plain, old-fashioned logic. Not one single mathematical theorem that is used has an indeterminate truth-value. How is it possible that we have to tinker with the laws of logic to understand something that never used or needed these alternate laws?"

Was I exceptionally arrogant, or did you have the same reaction?

7. **Peter Woit**
March 26, 2019

Peter,
You were smarter than me. I did take such things seriously, taking a lot of courses in the philosophy department, including ones from Quine (logic, i.e. Quine reading his yellowed notes on the subject) and Putnam (also found some of those papers, but I don't think anyone wants to read what I had to say about Kant…). It took me a while to realize this kind of thing could not be the answer to deep interpretational issues in QM, and that my time was better spent learning more about QM and QFT themselves.

8. **Maximillian Tresmond**
March 26, 2019

What was it like to study under W.V.O. Quine? Did he ever bring up his indispensability argument for mathematical platonism in his undergraduate classes?

9. **Peter Woit**
March 26, 2019

Maximillian Tresmond,
I was tremendously impressed by Quine's writings, by his major books available at the time (e.g. From a Logical Point of View, Word and Object, Ontological Relativity and Other Essays). The course I took though was a logic course, and a big disappointment. I'm not joking about his reading yellowed lecture notes. But he's one of the true greats of philosophy, and his way of thinking about science and how we gain knowledge of the world is quite compelling.

On the other hand, the logic course was a bore, and later in life when I read his autobiography, that was another disappointment, not exactly a gripping tale.

10. **anon.**
March 28, 2019

Just curious what you thought the novelist Martin Amis would deliver? A completely new – albeit non-crackpot – approach to solving the outstanding problems with the standard model, maybe? 😉

11. **AT—**
March 28, 2019

Dear Peter,

It was a pure delight to read your essay from your college days.

I would like to ask you a question (from the history of science perspective): was MWI not yet "en vogue" in those days already? As far as I know, DeWitt started his exposure of MWI in late-60's/early-70's. You didn't mention it in your essay, was it because you didn't take it seriously, or due to it being mostly unknown 43 years ago?

[For what it's worth, I think that MWI is, at best, a fringe interpretation of probability theory, but my POV is irrelevant to the question above]

12. **Peter Woit**
March 28, 2019

anon,

I haven't read the Amis novel, quite possibly it's much better than the film. I wasn't expecting much from the physics in the film, would have been perfectly happy to see a good film with some added cheesy physics for fun.

AT-,

It's been so long ago now I really can't remember when I first paid any attention to MWI. My vague impression is that MWI only started to get significant exposure with the DeWitt/Graham book published by Princeton in 1973. This was right around the time I started reading about quantum mechanics, and it's not the sort of volume I would have found accessible, I was more reading things from philosophers of science. So, it's quite possible MWI isn't mentioned just because I'd never heard of it.

In terms of the "has there been progress in the interpretation of QM since 1976" question, the fact that now such discussions often include a big dose of multiverse blathering doesn't really tip the scale in a positive direction.

13. **Blake Stacey**
March 29, 2019

The conventional view of historians and philosophers of QM is that Everett's ideas remained mostly obscure and un-discussed until the early 1970s. See, for example, Byrne's biography of Everett (informatively reviewed by Adrian Kent). I suspect that this picture is largely accurate, though looking in places they don't expect may turn up further discussions, and indirect evidence of conversations not written down. For example, Feynman presented an Everettian view in his 1963 lectures on gravitation, raising it as a possibility he took seriously but didn't find compelling. He seems to have filed it, along with the idea that somehow gravitation itself might be a breakdown of quantum mechanics, as conceptual speculation that isn't likely to be productive.

This Week's Hype

March 31, 2019
Categories: This Week's Hype

This week's hype comes to us courtesy of Scientific American, which, based on this preprint, tells us: Found: A Quadrillion Ways for String Theory to Make Our Universe.

As usual in these things, the only physicists quoted are the authors of the article, as well as some others (Cumrun Vafa and Washington Taylor) who are enthusiastic about the prospects for getting the Standard Model out of "F-theory". No one skeptical of the idea of F-theory compactifications of string theory (such theorists would not be hard to find…) seems to have been consulted. If such a person had been consulted, he or she might have pointed out:

- Models like this have been around for over two decades, see for instance this from 23 years ago.
- They have always come with claims that some sort of connection to experiment was right around the corner. A decade ago there were papers like this one (and promotional pieces like this one) explaining F-theory "predictions" for what would be seen at the LHC, "predictions" that never worked out.
- This new work doesn't even bother trying to make "predictions". It just works backwards, trying to match the crudest aspects of Standard Model, ones determined by a small set of small integers. Given the huge complexity and number of choices of these F-theory constructions, that some number of them would match this set of small integers is not even slightly surprising.
- The authors seem to argue that it's a wonderful thing that they have found quadrillions of complicated constructions with this kind of crude match to the SM. The problem is that you don't want quadrillions of these things: the more you find, the less predictive the setup becomes. What's being promoted here is a calculation that not only predicts nothing, but provides evidence that this kind of thing can't ever predict anything. A peculiar sort of progress…

Update: This hype has now been supplemented by the now common phenomenon among string theorists of having their university's press office put something out promoting string theory. This time it's the University of Pennsylvania, with a headline assuring us that their university's physicists are Making sense of string theory, with a discovery that "might change the course of the field."

Comments

1. **PeterH**
   March 31, 2019

   The paper is actually about the construction of a quadrillion chiral Minimal Supersymmetric Standard Models for which there is no evidence. Nothing really said about the SUSY partners just a hope that some models have a high SUSY breaking scale to avoid proton decay lifetime experimental lower bound. The title of the paper is hype and mis-leading.

2. **anon**
   March 31, 2019

   This seems like a somewhat mean blog post. The erticle is an unhyped description of a calculation that is not quite routine, but certainly no breakthrough; the physicists involved seem to be claiming nothing other than being part of the conversation of modern physics — providing useful and reassuring data for their colleagues to make more insightful discoveries.
   .
   And the basic question is profound and needs answering: What is special about the standard model, a very particular gauge theory on a very particular space — when all of the *mathematical* insights of the last 35-40 years of hepth/math conjecturally construct a web of dualities that relate all QFTs, of dimensions less than something small (but bigger than 4!)
   .
   'String theory' obviously makes no physical predictions yet, and you have
   been an important voice in disseminating that unpleasant truth. The
   flip side of that — that in various ways it includes the standard model,
   and that we cannot claim to understand the latter without
   understanding what `string theory' is, and what these equivalences are
   — and we do not. And we need to.
   .
   (For a description of an even more basic lack of collective understanding, you should go to Heselholts' talk on Wednesday — his lovely line is that mathematicians do not yet understand what a number is; see for example
   https://www.newton.ac.uk/seminar/20181008160017001
   for a pleasing description, aimed a little at homotopy theorists.)
   .
   This blog post does not engage in the anti-intellectual foolishness of pretending that string theory is nonsense, but it does seem _mean_ to a harmless paper.

3. **David Roberts**
   March 31, 2019

   @anon Thanks for that link to that talk from October. It's a cute line, that the integers are not the real base ring for homotopically enriched algebra, but the sphere spectrum S. Just as the natural numbers are the decategorification of the 1-category of finite sets, the integers are the connected components of S. Connes of course is also promoting this idea in his current approach to the Riemann hypothesis. Describing it as mathematicians not understanding what a number is is missing the joke 🙂 It's better to say we don't fully understand the collection of numbers with their arithmetic (and number theorists would agree on that, I think)

4. **Peter Woit**
   March 31, 2019

   anon,
   I don't think I'm being "mean" to the paper, would have just ignored it if Scientific American wasn't publishing an extremely misleading article about it. The string theory hype problem in the popular science press remains a well-entrenched one.

   I'm looking forward to the Hesselholt talk on Wednesday. For those interested, see
   http://www.math.columbia.edu/2019/03/27/spring-2019-kolchin-lecture/

5. **Sabine Hossenfelder**
   April 1, 2019

Hi Peter,

As PeterH points out in a comment above, the paper is not about the standard model (or standard models) but about the MSSM. And it's not all that clear you even get the standard model out at low energies because, as the authors write themselves (p 5):

"[T]his in turn also implies that certain proton decay operators compatible with the Standard Model gauge group will in general be present… We expect that in some corners of the moduli space, which incidentally could also support high-scale SUSY breaking, these operators can be suppressed."

Which basically means they don't know whether they would actually get anything resembling the standard model. (This is not to say that I doubt it can be done, just pointing out it's not in the paper.)

I surely hope that whoever reviews this paper requires that the title be changed.

6. **chorasimilarity**
April 1, 2019

From the reading of the e-print, the authors *give evidence* for the existence of many models which satisfy *necessary* conditions of compatibility with MSSM. I can't evaluate the hype, the article is a math proof with some parts which are not rigorous. Not read: the Scientific American popularization.

7. **Reg Taylor**
April 1, 2019

@Peter,
Perhaps a little unfair to Anil? He does say clearly in the SA article:
"the work assumes supersymmetry… String theory needs this symmetry in order to ensure the mathematical consistency of solutions"
"experiments at the Large Hadron Collider (LHC) have … yet to find any supersymmetric particles"
"Vafa and Taylor both caution that these solutions are far from matching perfectly with the standard model"
etc.
I'm not convinced that statements like this are 'hype'. More along the lines "we've found something interesting in the maths and it's worth a further look".

That said I'm with Sabine, the title of the Cvetic et al. paper is misleading, the authors themselves referring explicitly in the introduction to "exact chiral particle spectrum of the minimally supersymmetric StandardModel (MSSM)".

8. **Moyses**
April 2, 2019

It is too dated to ignore that since the early 2000's string phenomenologists are really: "Getting just the Standard Model at Intersecting Branes" (https://arxiv.org/abs/hep-th/0105155).
But I still found no pop-sci book mentioning this fact, that is dreadful!

Not Even Wrong 2.0

March 31, 2019
Categories: Uncategorized

This blog has just passed its 15th anniversary, and there hasn't been a lot of change in format since the first postings in March 2004 (there hasn't been a lot of change in string theory either, but that's a different topic…). I've been hearing a lot in recent years from people who have urged me to update the format of the blog, moving to formats more in tune with the way people now use the internet. One innovation in recent years has been that the blog content is available through Apple News.

I've decided to follow some more of the advice I have been getting, and have started up a [Not Even Wrong Facebook site](). No longer will you have to navigate to my WordPress site to access the blog content, instead it will be available the same way most people are now getting their news, through your Facebook News Feed. This will make it much more convenient for everyone to get notified about new posts and share these with others. I'm looking forward to the expanded readership and connections to the rest of the world that becoming part of the Facebook information eco-system will provide.

Update: Just unblocked a lot of comments that somehow were stuck in a moderation queue. Some people don't seem to understand that for an international blog like this, the date is best calculated according to UTC.

The uniformly hostile response here to the Facebook idea has been extremely reassuring. No, I don't intend to move the blog to Facebook. The fact that a sizable fraction of the US population in recent years has been getting its news off their Facebook News Feed seems to be one of the main factors in the 2016 collapse of democracy here, and the same thing is happening all over the world. This has also significantly moved along the ongoing destruction of the economic viability of conventional journalism. Going through the exercise of putting up a Facebook site made me aware of some aspects of how Facebook works I'd never realized. For example, on a Facebook post you can only hyperlink text to other Facebook material, not to the outside world.

It has become all too clear just how ugly the world created by Facebook is, that it is a sociopathic organization, and a danger to a healthy democracy. If you must stay in contact with friends and family this way, avoid any engagement with anything else on the Facebook site. Best would be to delete your Facebook account, now.

Update: For a book-length explanation of why you should be concerned about Facebook, see Roger McNamee's *Zucked*, reviewed here.

Comments

1. **Ian Mason**
   March 31, 2019

   I am hoping this is an early April fool's joke.

   People are fleeing Facebook like it was the plague.

2. **Casey Leedom**
   April 1, 2019

   Please do keep this old format! I've deleted my Facebook account so I don't have access to material there. (And I wouldn't go there even if it were available without a Facebook account.)

3. **Frank Quednau**
   April 1, 2019

   Interesting advice – medium is a place many people use to amplify their reach, as facebook struggles to reach younger people and those disillusioned with what the web has become.
   Here's hoping that your blog will stay because that was the real thing all along 🙂

4. **Matt Grayson**
   April 1, 2019

   Peter,
   Please don't abandon this site! Your blog is one of the best, and I can't be the only person who dropped FB and refuses to visit it.

5. **milkshake**
   April 1, 2019

   not everyone likes to use Facebook, they have absolutely atrocious privacy-violating business model.

   By the way, the format of Not Even Wrong is fine as it is. Making it more flashy could only add a web page bloat.

6. **4/1**
   April 1, 2019

   April Fools! You almost had me there, but that last sentence rather gave the game away.

7. **momerathe**
   April 1, 2019

   I hope you will continue to post here in parallel – I've sworn off Facebook and have it blocked on all my devices. Plus, this site is integrated with my RSS feed-reader, while Facebook makes it extremely difficult to do so.

8. **Chris Oakley**
   April 1, 2019

   The range of single-click responses in Facebook needs to be extended for the purpose.

   "Like", "Love", "Haha", "Wow", "Sad" and "Angry" do not cover it.

We need to ask also for: “Give me a break!”, “You’re a crackpot”, “Yeah, alright (I am tired of arguing)”, “So what?” and “See my web site/scientific papers”

9. **Peter Donnelly**
April 1, 2019

Are you going to keep the blog? I’m not on FB and don’t want to be.

10. **GVFool**
April 1, 2019

Everyone has a browser but not everyone is using facebook.

Time to start a new movement: #youtoo

11. **Jude Giampaolo**
April 1, 2019

I am hoping this is just a 1st April joke as I enjoy reading via RSS and won’t be able to follow nearly as regularly on Facebook.

12. **Ghost Bird**
April 1, 2019

I hope you will continue to post your content here as well, so that those of us who dislike Facebook (probably more malevolent than String Theory all things considered), can continue to read!

13. **Chris Herzog**
April 1, 2019

I gave up my Facebook account, shortly after the 2016 US election. I hope you will continue to post here in parallel.

14. **Balazs Vagvolgyi**
April 1, 2019

Please don’t switch to Facebook. I canceled my account years ago, I’m sick of it and refuse to visit it. I don’t know where you are getting your advice from, but in my circle of adult friends and colleagues, people are abandoning Facebook in droves.

15. **Mathematician and Fan**
April 1, 2019

April Fools?

16. **Trailmut**
April 1, 2019

Disappointed about your engagement with Facebook. I think this institution is one of the symptoms of the “disease” within our society and our culture.

17. **koala**
April 1, 2019

Yes please no Facebookization

18. **Dan Winslow**
April 1, 2019

I would prefer that this blog remains as it is. I don’t ever use Facebook, or any social media, and I find the clean presentation and simplicity of this blog’s format to be very comfortable and a refreshing change from the ‘improved’ internet formats.

19. **Chuck**
April 1, 2019

April Fools?

20. **GG**
April 1, 2019

Completely agree with the comments so far, but I wonder whether in some other timezone, the date of the post might be different?

21. **Martin Kovar**
April 1, 2019

I hope this was a joke related to April Fool’s Day.

22. **scrat**
April 1, 2019

I can’t shake the feeling the the post date for this should have been today… ^^

23. **Peter Schlaifer**
April 1, 2019

Let me add my small voice to the chorus asking you to continue the existing newsreader format and support.

24. **Ehud**
April 1, 2019

Hopefully an April's fool post.

25. **SteveB**
April 1, 2019

Hmmm. I read this on April 1.

26. **Curious**
April 1, 2019

Commenters, please check the date.

27. **F4**
April 1, 2019

This decision is unexpected and disappointing, even if posts continue to appear on this site in parallel. Trailmut's characterization of Facebook as an etiological factor of social disease (freeing that term here of those mitigating quotation marks) is likely not inaccurate.

Though this post is dated March 31, 2019 (9:51 PM). Hopefully it is in fact only an early joke intended for the following day.

28. **pierreauquebec**
April 1, 2019

April 1st ?

29. **Jon Forrest**
April 1, 2019

Is this an April Fools joke?

I join with all the others who ask that you not drop this site. Post in parallel, if you must, but there's too much on Facebook that's Not Even Wrong as it is.

30. **a reader**
April 1, 2019

Please keep it up and running here — if you can. I left Facebook two years ago — no regrets whatsoever. By the way… How are you going to police comments on Facebook?
"Informed comments relevant to the posting are very welcome and strongly encouraged. Comments that just add noise and/or hostility are not. Off-topic comments better be interesting…"
…On Facebook?

31. **Anders Bengtsson**
April 1, 2019

I agree we all the above. Don't go Facebook. Nothing is gained. It's a pleasure to read blogs like this one, not having to be interupted by silly AI-s who think they know what you want.

Second thoughts: is this an April's fools joke?

32. **jls**
April 1, 2019

(friends, check the date)

33. **pquant**
April 1, 2019

A facebook post once a year would be fine, I guess.

34. **Visitor**
April 1, 2019

And I thought that I was the only one…

35. **anonymous**
April 1, 2019

Also not on FB, and probably never will be, and I'm hoping you'll continue with this "old" format.

36. **Marty**
April 1, 2019

I hope this posting is an April Fool's joke posted on March 31 by mistake. I can't imagine that Facebook's ideals, past issues with fake news and other questionable content, its likely role in helping to elect Donald Trump, massive personal data collection in the name of more profitable advertising, etc. are particularly compatible with your own ideals. In the past, I also haven't noticed you being especially preoccupied with massively expanding your readership…

Anyway, assuming this posting is tongue-in-cheek, it gets a chuckle from me. It it's not a joke, uh oh… Like many others, I rarely visit FB and have never left a comment there.

37. **AcademicLurker**
April 1, 2019

As another long time reader who won't touch facebook with a 20 foot pole, I'll just add my voice to the chorus hoping that you'll keep this site going and not migrate to FB entirely.

38. **Steve Huntsman**
April 1, 2019

Excellent choice of material for today

39. **JE**
April 1, 2019

Thank goodness it's April 1. Almost bit it!

40. **Ken Martin**
April 1, 2019

Ditto to all the comments regarding not being on Facebook.

41. **Steven Docker**
April 1, 2019

Please tell me this is an April fools joke …

42. **markk**
April 1, 2019

Also a non-Facebook user. I will miss this blog if it is only available there.

43. **markk**
April 1, 2019

Ok , now I will leave my April Fools comment… except I don't have one. I have been ground down by the bastards but am still able to stand…

44. **WP**
April 1, 2019

Is it an April Fools' joke?

45. **M. de Lange**
April 1, 2019

Dear Peter,

Please keep the format as it is.

46. **Matthew**
April 1, 2019

The date at the top is supposed to be 1/April, surely!

I notice that if you don't have a facebook account, and scroll down to look at old posts, it blocks you after you've scrolled past the top (most recent) 3 or 4 posts. Not helpful.

I think the existing site could do with a make-over – it doesn't display well on mobile devices with a small screen, for example, but FB is not the way to achieve that.

47. **Theodore String**
April 1, 2019

Mr Woit,

This is the best one I read on this fine day.

Well done sir, well done, indeed.

48. **Bruno**
April 1, 2019

Am I the only one to think this post was delivered just a bit in advance ?

49. **Art**
April 1, 2019

Gee, on 1 April…

50. **Alexis**
April 1, 2019

Happy April fool's day!

51. **John Merryman.**
April 1, 2019

Not alot of fb fans here. Given the herd mentality it encourages, that might not be the additional audience you want.

52. **otto porter**
April 1, 2019

Like the others, no facebook for me.

I’ve been reading your blog since the beginning.

Please don’t change it.

53. **Kuas**
April 1, 2019

When I first saw this I thought April Fools, but the post date is one day off. I hope it’s April Fools.

54. **Walt Donovan**
April 1, 2019

It’s your blog, of course. I just hope you will keep the WordPress version active, and just feed Facebook or whatever from it.

55. **tulpoeid**
April 1, 2019

It’s nice that the original post has no mention of abandoning the blog, but the mere thought seems to terrify people :)
Imho Facebook is a real boon for humankind, it’s no worse than any other service in terms of privacy, and it has exactly zero relation with unlikeable election results. I’m very serious. Countless lives have been changed by it for the better in ways we never anticipated (and which we now take for granted).

But, back to physics, what would your take be on the latest notorious results?
https://www.reddit.com/r/ParticlePhysics/comments/b83ab6/regarding_the_recent_discovery_by_lhc_experiments/

56. **Peter Orland**
April 1, 2019

Hey commenters!

I suspect Peter is pulling an April Fools’ joke here.

Am I wrong? I hope I’m not wrong.

57. **Ghost Bird**
April 1, 2019

Wow, you got us. Congratulations, this was pitch-perfect.

58. **WTW**
April 1, 2019

FWIW, I have blocked Facebook URLs on all of my computers and those in my organization – laptop, desktop, mobile. If you move exclusively to Facebook, I will have to drop your blog. Consider your decision to move to Facebook at all a bad, possibly ill-informed one.

59. **Low Math, Meekly Interacting**
April 1, 2019

Hope the WordPress site mirrors the FB one, but understand that it may not be feasible. I also dropped FB, and don’t plan to go back. Best wishes!

60. **Kevin S. Van Horn**
April 1, 2019

I had to check the date on the post to make sure it wasn’t an April Fool’s joke. I find the blog’s current format to work just fine for me.

61. **Jay Sanders**
April 1, 2019

Facebook is blocked in the systems I use. Have you Facebook page, if you must, but, please, keep this blog going here, as it currently is.

62. **Bob**
April 1, 2019

Maybe this is Peter’s idea of an April Fool’s joke?

63. **Paul**
April 1, 2019

Like many, I’d prefer to continue to read your blog here.

64. **Fool**
April 1, 2019

April 1?

65. **Bernhard**
April 1, 2019

Hi Peter,

I hope you’re not saying the blog will now only be available through social media…

The blog is perfect as it is. I’m old school… please don’t make me join Facebook.

66. **Anon**
April 1, 2019

Perhaps Peter could only post here once a year, say on this particular date, April 1.

67. **Patrick Harris**
April 1, 2019

I realize the date is off by 1, but is there any relevance of the notorious start of April corresponding with this post…?

68. **Bernhard**
April 1, 2019

Wait a sec – April Fools? (really hoping 😀)

69. **raskra**
April 1, 2019

I hope this is an April Fool's joke…

70. **Spring chicken**
April 1, 2019

Amazing that this comment thread is still going, it might last longer than a particular day.

71. **theoreticalminimum**
April 1, 2019

I can hardly imagine you moderating or even paying attention to comments on FB, so I'd take this as an April Fools' day post, and I hereby conjecture that the FB page will be gone in no time 🙂

72. **NO FB**
April 1, 2019

FaceBook is crap. Please do keep the WordPress format. Thank you

73. **Max Madera**
April 1, 2019

Come on guys, don't you know which date is today?

74. **Not a fool**
April 1, 2019

Early April Fools' ?

75. **Andras Laszlo**
April 1, 2019

I would suggest not to move to Facebook. It does not only have its sociological / ethical downsides, but is simply inergonomic in my oppinion for practical usage. Best, Andras

76. **Zaaikort**
April 1, 2019

Haha, you almost fooled me!
You would never seriously consider joining Facebook, would you?
… Err … oops

77. **IA**
April 1, 2019

Guys, relax, its just April's fool joke.

78. **Reg Taylor**
April 1, 2019

You refer to wordpress as though it's a backwoods village buried in the deepest recesses of some impenetrable jungle. I've a single bookmark set up on the browser that takes me straight to your most recent wordpress blog entry. Just one click – how easy can it be? Like others making comments I would much prefer your blog and format to stay as it is, quick to access, visually clear and very readable. Not sure if this would be the same on facebook as I'm not terribly familiar with the system but from my limited experience I suspect not.

79. **jk47**
April 1, 2019

deleted my facebook account long ago when it became clear that they did not respect my privacy settings. i hope you keep this site.

80. **David**
April 1, 2019

Long-time reader here, and I even bought your Representations book. However, I won't follow you to Facebook. I refuse to use it, for political and ethical reasons.

81. **NoGo**
April 1, 2019

I am another devoted reader who really hopes this is an April 1 joke…
or at least that you would maintain this site in parallel.

I am not one who has no Facebook account, but I rarely use it and only to contact freinds and family, not to read news etc. Apart from all other issues, I find FB look/design horrible from both aesthetics and functionality points of view…

82. **Paul Schubert**
    April 1, 2019

    Please. Not. Facebook.

83. **bertie**
    April 1, 2019

    Have loved the blog for many years but dont visit facebook.
    Perhaps (I hope) the last post was an April fools joke….

84. **Foolish theorist**
    April 1, 2019

    What a fool… are you being threatened by the string theory mob?

85. **CormacinDublin**
    April 1, 2019

    Hi Peter
    A good April fool! – Will you post this comment?

    Although the timestamp is from yesterday however, was it only released this morning?

    Regards

86. **Richard Townsend**
    April 1, 2019

    Peter, please keep the WordPress site alongside Facebook. Like many, I am ex-user of Facebook!

87. **Oldster**
    April 1, 2019

    I hope this is an April Fool's joke. Facebook might even decide some of your posts, or reader comments, "Violate community standards." Then what?

88. **Jeff**
    April 1, 2019

    I was being shocked by the facebook thing news: how could a blog mocking hype move to facebook?! … and then I noticed the date of the posting 😉

89. **Anonymous**
    April 1, 2019

    Great April Fools Day post. Really got a rise out of the readers.

    At least I hope it is.

90. **Tom Dickens**
    April 1, 2019

    It's April 1, right?

91. **Woitreading nerd**
    April 1, 2019

    Please stay independent. I would like to continue reading your blog, but there is no way I am going to allow facebook crap on my system. I am blocking all their URLs.

    Had you just posted this one day later I would not be so worried about it …

92. **LD**
    April 1, 2019

    38 April fools?

93. **Gautam Menon**
    April 1, 2019

    And there I was, thinking this was an April Fool's day joke!

94. **mark thompson**
    April 1, 2019

    please keep the blog as it is, even if you try FP for a while

95. **godelta**
    April 1, 2019

    As a long time reader and fellow Facebook-phobe I just want to add:

Nicely done!

Had me sweating as well for a little while till I wised up.

96. **kodlu**
April 1, 2019

I love the blog as it is. No facebook please, I shan't join, and would really miss your wonderful blog.

97. **Ron**
April 1, 2019

Whoosh!

98. **Low Math, Meekly Interacting**
April 1, 2019

I just realized Peter is laughing his ass off right now. Well played, sir. Well played.

99. **Sir Lleb**
April 1, 2019

I'd rather you went back to blogging about New York, Biking, Desserts, or even, gulp, politics…

—
Long time lurker

100. **paul from tacoma**
April 1, 2019

Peter – please don't do this. I echo the reasons given.

But this is an April fools joke, right?

101. **Trailmut**
April 1, 2019

If this is an April Fool's joke, good and fine; but I'm not so sure it is that funny considering that some other good and smart people have been seduced by Facebook and a lot of us loyal readers felt worried.

102. **John Fredsted**
April 2, 2019

Very happy to read that I am not the only one opposed to Facebook (personally, I have never had any account there). As for other socalled social media, Facebook's business model seems to revolve around the idea of turning modern mans apparently ever increasing narcissistic tendency – see me, see me, see me! – into a money machine. I think it is a disaster for the human spirit that so many people spend so much time on that platform, for it keeps nurturing something that should not be nurtured.

103. **Visitor**
April 2, 2019

What changed your attitude from

> "I'm looking forward to the expanded readership and connections to the rest of the world that becoming part of the Facebook information eco-system will provide"

to

> "It has become all too clear just how ugly the world created by Facebook is, that it is a sociopathic organization, and a danger to a healthy democracy. If you must stay in contact with friends and family this way, avoid any engagement with anything else on the Facebook site. Best would be to delete your Facebook account, now."

… because Facebook's hyperlink policy hardly seems to be an adequate explanation for this kind of complete reversal.

104. **Peter Woit**
April 2, 2019

Visitor,

April 1 turned into April 2.

105. **Chris Oakley**
April 2, 2019

As usual I seem to be a $\mu+2\sigma$ outlier, but I have to say that I like Facebook – it is a nice casual way of keeping in touch with people you hardly ever see. It is also a good source of jokes, and, thanks to the targeted ads I got to meet Terry Jones (of Monty Python) about 10 years ago – he even bought me a drink! As for those individuals who are sufficiently weak-minded to believe a lot of the stupid crap that gets posted on it, well, more fool them: if it was not Facebook, it would be something else (e.g. Fox News). I do agree, though, that it is not a suitable forum for scientific debate as the format tends to encourage knee-jerk, emoticon-based responses rather than considered ones.

106. **unhappy vonnesline**
April 2, 2019

Great April fools…totally got me

107. **tulpoeid**

April 2, 2019

For the record, completely agreeing with Chris Oakley. My previous comment supporting facebook is honest although I was guessing the date-related background.
For the sake of discussion: facebook is not worse than other online media and in many cases and ways it's better; sadly, Trump was elected because of his personality and this is what we have to accept and fight. More importantly, I have ~500 "friends" from three continents (whom I all know personally) and maybe only a couple of them abandoned their account because of the last few years' events, so I'm made to think that it's worth considering whether the overreaction in US might be related to "traditional" media turning hostile towards facebook out of purely competitive reasons.
(Congratulations for the patience needed to put up an actual page, though.)

108. **a reader**
April 2, 2019

You got most of us! Well, that was a memorable April's Fool.
"Never get high on your own supply" (The Guardian, 23.01.2018)

109. **John**
April 2, 2019

I am against Facebook and never had an account or went there. But here uou are a scientist and make the following claim:
"The fact that a sizable fraction of the US population in recent years has been getting its news off their Facebook News Feed seems to be one of the main factors in the 2016 collapse of democracy here"

All without any real proof. This claim is almost as bad as the multi-verse. Facebook "news" is mostly left leaning. There are even claims being investigated that conservative news is blocked by Facebook and people working at Facebook, including Mark Z have admitted to some of that. The election's outcome didn't happen because of some dumb ads or "news" on Facebook.

110. **Peter Woit**
April 2, 2019

tulpoeid,
Yes, "traditional" journalists don't like Facebook because it is putting them out business. If you don't like "traditional" journalism, you may be fine with that. But I think we're now seeing what happens when you destroy "traditional" journalism and all that's left is Fox News, Russia Today, Buzzfeed, together with Facebook as a portal to share these and other sites of "journalists" who do nothing but write about whether Joe Biden smelled some woman's hair. I just don't see how you have a viable democracy when people's main source of info about the world is an information sewer.

tulpoeid/John,
The reason Trump won was not that lots of people liked Trump, but that everyone hated Hillary Clinton and thought she was more dishonest than Trump. Why did everyone feel this way about Clinton? Because of what they read on Facebook is one sizable reason. And the garbage they were reading about Clinton did not just come from the Right and the Russians, but also from the Left.

All,
I better call an end to the discussion of the implications of Facebook for our democracy. I was conducting a bit of an experiment here to see whether trying to put out news about math and physics on Facebook was something that anyone thought might be a good idea. The results were conclusive on that front.

111. **jk_in_nc**
April 4, 2019

Thanks for calling a halt to the Facebook discussion… although I am glad to see which way a science-prone crowd leans.

When I saw your original post, I decided against the April-fool idea because a March 31 date was listed, without a time imprint. Curiously, none of your posts have a time inprint, only a date one. However, all of the comment posts have a time (and date) imprint.
Wondering why that is.

112. **Peter Woit**
April 4, 2019

jk_in_nc,

The way time-stamps appear is just the WordPress default behavior, perhaps specific to the (standard) theme I'm using. It would be better if posts (and my "Updates") were time-stamped, but after some poking around I haven't found an easy way to do this, without adding some custom code.

113. **Andrew Bernatd**
April 5, 2019

Not Even Funny. 🙂 Facebook is such a miserable thing. I was worried for a minute or two!

114. **F4**
April 6, 2019

"It would be better if posts (and my "Updates") were time-stamped, but after some poking around I haven't found an easy way to do this, without adding some custom code."

The timestamps are not printed alongside the date but they are in the tooltip that appears uponon hovering over the date link.

Regarding updates, by the way: appending them to existing posts isn't ideal as they can easily be missed if the reader happens to not regularly revisit recent posts. Generating an entirely new post instead may seem unnecessary but it results in a cleaner presentation and historical record.

115. **Peter Woit**
April 6, 2019

F4,
Thanks for explaining how to see the timestamps.

One thing I'm looking into is whether there's some way to change the rss behavior so that feed readers such as feedly will notify of an updated post.

116. **Visitor**
April 21, 2019

So, if I understand you correctly, this was an April Fool's prank. And it required you to code a page for Facebook because I have no idea why. And oh by the way you were "conducting a bit of an experiment here to see whether trying to put out news about math and physics on Facebook was something that anyone thought might be a good idea" and you wanted this information because you were not going to actually act on this information once obtained.

This is not really credible.

As we all know, "The first principle is that you must not fool yourself – and you're the easiest person to fool". Apparently, when he said "you" he meant *you*.

Why Trust a Theory?

April 5, 2019
Categories: Book Reviews

I noticed today that Cambridge University Press has recently published *Why Trust a Theory?*, a volume of articles based on a December 2015 conference held in Munich. The book is available online here (if your university is paying for it…), and preprint versions of many of the contributions are on the arXiv.

The conference had its origins in a piece published a year earlier in Nature by George Ellis and Joe Silk, entitled Scientific method: Defend the integrity of physics. Ellis and Silk made a forceful case that widely advertised but inherently untestable string theory and multiverse research does damage to the public understanding of science and is a threat to the credibility of science at a time it is under attack. The piece suggested:

> A conference should be convened next year to take the first steps. People from both sides of the testability debate must be involved.

Looking through the proceedings volume, there's lots of abstract discussion of philosophy of science and some diversity of points of view on the multiverse. When it comes to string theory though, the organizers interpreted "people on both sides" to mean bringing in one person willing to point out that there is a problem with string theory, and an army of string theorists to defend the theory. On the issue of the problems of string theory, the volume contains nearly 100 pages of pro-string theory hype, from Polchinski (two contributions), Silverstein, Kane and Quevedo. As usual with Kane, there's a string theory "prediction" of the gluino mass (1.5 TeV +/- 10-15%) which has already been falsified. All I could find on the side of substantive criticism of string theory was in Carlo Rovelli's contribution (preprint version here), and mainly in a single paragraph:

> String theory is a living proof of the dangers of excessive reliance on non-empirical arguments. It raised great expectations thirty years ago, promising to compute all the parameters of the Standard Model from first principles, to derive from first principles its symmetry group SU(3)×SU(2)×U(1) and the existence of its three families of elementary particles, to predict the sign and the value of the cosmological constant, to predict novel observable physics, to understand the ultimate fate of black holes, and to offer a unique, well-founded unified theory of everything. Nothing of this has come true. String theorists, instead, have predicted a negative cosmological constant, deviations from Newton's 1/r^2 law at sub-millimeters scale, black holes at the European Organization for Nuclear Research(CERN), low-energy super-symmetric particles, and more. All this was false. Still, Joe Polchinski, a prominent string theorist, writes that he evaluates the Bayesian probability of string to be correct at 98.5% (!). This is clearly nonsense.

I won't spend more time here discussing the conference and the articles in this volume, mainly because I've already written a lot about this in previous posts. For a contemporaneous discussion of the conference and Polchinski's String Theory to the Rescue paper, see here and here. There are also interesting blog posts about the conference from Massimo Pigliucci, see here, here and here, and a Quanta piece by Natalie Wolchover here. For a discussion of Sean Carroll's Beyond Falsifiability contribution, see here (and discussion here and here). For a discussion of Eva Silverstein's contribution, see here.

Update: A few more links to material about the Munich conference: Jim Baggott here and here, Andrew Gelman here, Davide Castelvecchi here, and the conference website (with videos) here.

Update: Looking at the Preface, I notice that the editors claim:

> Additional contributions were solicited by the editors with the aim of ensuring as full and balanced presentation as possible of the various positions in the debate.

With regards to string theory, the one additional contribution in the volume is from string theorist Eva Silverstein, so evidently the editors felt that balance required yet more on the pro-string theory side….

Update: I mischaracterized Polchinski's calculation of the probability that string theory is correct as 98.5%. More accurately, he claims that the probability is "over 3 sigma" (i.e. over 99.73%).

Update: I finally got around to watching the videos of the panel discussions at the workshop (all videos available here). What most struck me about these discussions was the heavily dominant role of David Gross, who was on two of three panels, participating from the audience in the third. On the panels he was on, Gross was speaking far more than anyone else, and rarely if at all would anyone disagree with him. Gross's point of view is that there is a testability problem with the multiverse, but all is well with string theory (although probably not at Polchinski's "over 99.73% sure to be true" level). He's a powerful intellect and a forceful speaker, so it's not surprising that no one would take him on. But on the topic of string theory I think there are very serious problems with many of the claims he makes (for his arguments of 15 years ago, see the first substantive post of this blog), and the organizers should have found someone willing to challenge him on those.

Comments

1. **Sabine Hossenfelder**
   April 5, 2019

   Allow me to mention that I declined writing a contribution because at the time I was writing my book & felt the overlap would be too large. With my contribution missing, the balance of the proceedings shifts somewhat towards string theory. Though I am more critical of Richard Dawid's defense of string theory than of string theory itself.

2. **Jim Baggott**
   April 5, 2019

   And my take in the conference can be found here: http://www.jimbaggott.com/articles/status-anxiety-all-theories-are-not-the-same/

3. **Peter Woit**
   April 5, 2019

   Thanks Jim and Sabine,

   I've added a few more links to the post, including Jim's.

   I do still wonder though how the organizers of this conference and volume would justify the heavily one-sided nature of what they produced. Was it really impossible to find anyone other than Rovelli willing to write about the problems with string theory?

   By the way, I was looking for an excuse to mention Sabine Hossenfelder's talks nearby next week, she'll be giving multiple talks at Brookhaven on April 9, and a talk at Yale on April 10.

4. **Petite Kabylie**

April 6, 2019

Dear Peter
In my opinion, the fact that Lee Smolin and you Peter, who were the first to have seriously and brilliantly raised the issue with your books, have not been invited to contribute says it all.
I just wonder whether you Peter (and Lee Smolin) would have accepted to give a talk at the conference if they had invited you.
Thanks

5. **Peter Woit**
April 6, 2019

Petite Kabylie,
I believe Lee Smolin was invited to give a talk but declined. I would have participated if invited.

6. **Lee Smolin**
April 6, 2019

Dear PK,

Peter is right, I was invited and was unable to go in the end. I was just looking at my correspondence with the organizers and, while I don't want to quote email without permission, they indicated that they were looking for a single person to represent the "LQG point of view." What I have to say about Richard Dawid's book was contained in a review of it I wrote earlier. In short, I argued that the 3 non-empirical criteria he gives for taking a theory seriously could be applied equally as well to LQG as they could to string theory, which shows that they are ambiguous and of no help when we are faced with choosing between competing research programs.

This is why I argue that when there are two or more competing research programs, each of which has non-trivial results that offer considerable, but not decisive evidence for their being the truth, we have to support them all, and also leave room for new hypotheses and research programs. This is argued in detail in Chapter 17 of TTWP.

Thanks,

Lee

7. **shantanu**
April 7, 2019

Peter, are the questions asked recorded in the proceedings?
Shantanu

8. **Peter Woit**
April 7, 2019

Shantanu,
I don't think there's anything in the Cambridge volume concerning the panel discussions or questions asked at the conference. There are videos of these online (I just took a quick look, but they didn't seem to be working now). The authors of the articles in the volume do an excellent job of not responding to the serious questions someone should have asked about the current state of string theory.

9. **James Gallagher**
April 7, 2019

Hi Peter

Suppose you were invited:

Would you just talk about the easy criticism of the current theories, or perhaps, suggest anything new yourself ? (not a theory (obviously) but a new approach to theory finding maybe?)

10. **Peter Woit**
April 7, 2019

James Gallagher,
The topic of the workshop was not a call for new theories or new approaches. It was supposed to provide a venue for "both sides of the testability debate" to present their views. At least as far as string theory is concerned, the organizers decided not to do that, but to have the discussion of string theory (especially in the Cambridge volume) consist just of string theorists defending string theory.

If I had been invited to the workshop or to contribute to the book, I would have tried to represent the otherwise unrepresented side of the debate.

11. **shantanu**
April 7, 2019

Sabine had some hard questions for David Gross after his talk. He mentioned 19 tests of falsification of string theory. I don't know if they are mentioned after the talk.

12. **Peter Woit**
April 7, 2019

shantanu,
I just checked the video of the Gross talk, it cuts off at the end of the talk, no questions from the audience included. Not surprising that Sabine was the one willing to challenge Gross, too bad she wasn't on the panels.

13. **Blake Stacey**
April 8, 2019

The "Popperazi" accusation particularly rankles, because by the time Karl Popper was about four years old, [William James had already skewered](#) the next-level vagueness we now see in multiversology:

> The more absolutistic philosophers dwell on so high a level of abstraction that they never even try to come down. The absolute mind which they offer us, the

mind that makes our universe by thinking it, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular from the notion of it. It is compatible with any state of things whatever being true here below.

14. **shantanu**
April 9, 2019

Peter I looked at that post of yours regarding David Gross's talk. Do you know if the two string theory postdocs who attended that are still doing string theory?

15. **Peter Woit**
April 9, 2019

shantanu,
At this point I don't remember exactly who I had in mind. Most of the theory postdocs from that era, like any recent era, didn't end up staying in the field because of lack of jobs. This is true of both string theorists and non-string theorists.

It was interesting to go back and see what the situation was 15 year ago. My commentary then was
"Gross and others seem intent on ignoring the failures of string theory, desperately hoping that superpartners will pop out of the LHC, thereby providing at least some vindication of the train of reasoning that led to string theory. What will be interesting to see will be what Gross et. al. do when this doesn't happen. Will they drop string theory?"

Unfortunately we now know the answer, and it is clear that belief in string theory has become completely decoupled from any possibility of experimental test.

16. **Bernhard**
April 9, 2019

I sometimes think this string theory/multiverse story is best left alone. Whenever I'm in a conference where real physics results are discussed this stuff tends to be ignored and I star believing this group lives already in their own parallel universe.

But, whenever I give an outreach talk I remember again of the danger. A few months ago I gave a BSM talk for high school kids. At the end of the talk, they were a bit disappointed to know I was not going to talk about strings and the multiverse…

17. **Petite Kabylie**
April 10, 2019

Thank you Lee.
I am sure it would have been a historic conference if both you and Peter were there.

18. **tulpoeid**
April 10, 2019

The banner today is a nice touch.

19. **AcademicLurker**
April 10, 2019

tulpoeid: I was actually a bit alarmed to open this page and find myself looking into the Eye of Sauron…

20. **Geoffrey Dixon**
April 10, 2019

The older I get, the more Felliniesque do discussions of the frontiers of HEP seem. Surely they have always been so, but we tend to imbue things with more seriousness when young. "Known for his distinct style that blends fantasy and baroque images with earthiness, he is recognized as one of the greatest and most influential filmmakers of all time." He was a fan of vaudeville, of course, and I imagine were he alive today, and did he care at all about theoretical physics, he would have referred to it as HEV.

21. **Low Math, Meekly Interacting**
April 10, 2019

Stunning. Jean-Pierre Luminet's 40-year wait has ended.

22. **cedric bardot**
April 11, 2019

Why trust theory?
Because you can make consistent computations…
This is the message I can understand from Petr Horava in a panel discussion at the recent European workshop: Quantum Spacetime '19 Bratislava, 11-15 February 2019 (https://youtu.be/S_nvO4Kembw?t=3245)
"I think mathematically, on paper, String Theory is that [a kind of theory of quantum gravity as least as good as QED] because it allows you to calculate graviton-graviton scattering consistently, it has all kinds of beautiful properties that make you think this theory is in some technical sense UV complete and how much more you want to ask at a first stage?"
…
"Why the standard model?" comments Ali Chamseddine from the audience (https://youtu.be/S_nvO4Kembw?t=3459) who don't intend to attack string theory but want to emphasize the urge to learn the lessons gained from other fundamental interactions to address quantum gravity.

23. **Peter Woit**
April 11, 2019

cedric bardot,

This is just the usual argument that "OK string theory doesn't work as a unified theory, but at least it's a consistent theory of quantum gravity". This isn't actually true, since there is no non-perturbative theory (what is the amplitude for a black hole in the final state when you collide two gravitons?). The secondary dodge is "OK, all we have is an effective theory at low energies, but QED is just an effective theory at low energies". I long ago decided that trying to argue with the "string theory is exactly like QED" people is just a waste of time, no different than trying to argue with people who claim "black is white, since they are both shades of gray".

24. **Jim Given**

April 11, 2019

A “testability debate” is such an unusual occurrence in the history of the natural sciences. Where is there a precedent? Perhaps mid-1990’s attempts by Freudian analysts to re-assert the reality of Freudian theory against the barrage of attacks by senior scientists and philosophers that ended the 1980’s. But the discussion, by that time, had ended, except in the minds of true believers. (Something similar may have occurred with string theory.)
Really, “testability” like the earlier “falsifiability” seems not to be a good way to evaluate scientific theories. Rather, the test of a productive research program should be the interesting new experiments, and new discoveries in science motivated by efforts to test it. What would be recent examples in this category for string theory?

25. **Peter Woit**
April 11, 2019

Jim Given,
I agree that “is it testable” is too simplistic. A better criterion may be the Lakatos productive vs. degenerative one, and string theory unification is a classic example of a degenerative research program. Put differently, instead of asking how close string theory is to giving a unified theory (these days, string theorists seem happy with “maybe centuries away”), you should ask whether it is getting closer or farther. I think there’s no question the answer is “farther”: 30 years ago string theory was almost there, now it’s much, much farther.

This and That

April 16, 2019
Categories: Uncategorized

Since you've read about the black hole image elsewhere, here are a few other items that might be of interest:

- I was sorry to hear today of the death on April 11 of Geoffrey Chew. Throughout the 1960s, Chew's S-matrix/bootstrap philosophy was the dominant paradigm in high energy theory. It went into eclipse with the success of gauge theories in the early 1970s, but in recent years the (S-matrix) "amplitudes" program has to some degree revived it a bit, with hopes that it may be relevant to formulating quantum gravity.
- I thought the string wars were at times rather brutal, but it seems that they may have been a picnic compared to what astronomers get up to when there is a lot of money involved. See here for the bizarre story of what happened to Richard Easther when he started criticizing the plan for a New Zealand component of the Square Kilometer Array.
- For some recent and upcoming conference sites giving an idea of what is new in math and physics, Microsoft is hosting Physics Meets Machine Learning, the Eighth New England String Meeting had lots of interesting talks, hardly any strings to be seen, and MSRI last week hosted a "Hot Topics" workshop on Recent Progress in the Langlands Program.

For some news related to new books, there's:

- Lee Smolin has a new book out, Einstein's Unfinished Revolution, arguing that quantum mechanics is likely incomplete, since it continues to lack a successful "realist" version. He will be giving a public lecture about this at Perimeter tomorrow.
- John Baez advertises on Twitter a forthcoming volume about "New Spaces in Mathematics and Physics". For some of the content, see here. Also, the original conference these articles are based on has videos here.
- I'm looking forward to seeing Graham Farmelo's forthcoming The Universe Speaks in Numbers, about which I suspect there will be parts I'll strongly agree with, others about which I'll equally strongly disagree. The book evidently is based mainly on interviews, some of which Farmelo is putting up on his website. Jon Butterworth has a review this week in Nature, entitled A struggle for the soul of theoretical physics. He describes the Farmelo book as "a riposte" to critiques from a group I'm identified as being part of, but I have to keep pointing out that my point of view is not at all that the problem with string theory/supersymmetry has been "too much math". I think progress in fundamental physics is going to require more mathematics, not less.
- There's a new edition of the Kiritsis String theory in a Nutshell textbook available from Princeton. Looking at the introduction, I'm glad to see that Kiritsis points out the problem with the usual "string theory works, at the Planck scale" argument:

  > A big "hole" in string theory has been its perturbative (only) definition. With the advent of nonperturbative dualities, it was hoped that this shortcoming can be bypassed.Although the nonperturbative dualities have shed light in many obscure corners of string theory (obscured by strong-coupling physics), they never managed to bypass the Planck barrier. The Planck scale is always duality invariant, and any dual description is well defined for energies well below that Planck scale. We have no clue from string theory what happens near or above the Planck scale, as the relevant physics looks nonperturbative from any point of view.

  I've added this to this FAQ entry.

Comments

1. **zzz**
   April 16, 2019

   "there will be parts I'll strongly disagree with, others about which I'll equally strongly disagree."

   what about the other parts ?

2. **Peter Woit**
   April 16, 2019

   zzz,
   Oops, that didn't come out right. Fixed.

3. **Urs Schreiber**
   April 16, 2019

   More pointers to contributions to the book project "New Spaces for Mathematics and Physics" are here.

4. **Sabine Hossenfelder**
   April 17, 2019

   The point of my book was also not to say that physicists use "too much math." I am saying they do not pay enough attention to math and instead believe their sense of beauty must somehow tell them what is right. I don't know what's controversial about pointing out that belief shouldn't replace evidence. Fact is, arguments from beauty have worked badly in the past and they still work badly.

   (But the WSJ review now makes sense^^.)

5. **Jim Baggott**
   April 17, 2019

   For what it's worth, my book 'Farewell to Reality' doesn't argue that there's 'too much math', either. Mathematics has proved to be an extremely powerful language in which to *represent* the empirical physics, and only an idiot would suggest we need less of it. However, it's a mistake to think that you can *conjure* physics from mathematics, especially when there are no empirical foundations for it. I've never been against theoretical physicists exploring mathematical possibilities, but whenever these same theorists then go on to declare that this *is* the way reality works, and advertise their theories as well-founded, then I get a bit cross and feel the need to call this out.

6. **Dom**
   April 17, 2019

I note that Butterworth's review ends with "The Universe might speak in numbers, but it uses empirical data to do so". I could be mistaken but from reading his blogs in The Guardian and elsewhere over the years I got the impression that as an experimentalist he is quietly sceptical of String Theory and Supersymmetry.

7. **Low Math, Meekly Interacting**
April 17, 2019

Hopefully this isn't too OT, but the banner switch makes it rather difficult keep my mind off the subject. That and we have reportedly fully entered the era of observational science in the strong field regime of GR, which does have something to say about certain HEP theories with astrophysical implications. Empiricism and all that…

What parts of the "parameter space" of theories that predict exotic stars and other alternatives to the standard-issue spinning BH has been chewed off by the EHT? My sense from reading the paper is that things officially already look pretty bad for certain alternatives. Not sure how threatened fuzzballs and gravastars and so forth might be, but presumably they will be facing some tension if GR keeps holding up too, if not already.

Might be an interesting phenomenon to watch: How flexible are these alternatives to GR such that they continue to "survive" by moving the goalposts to evade experimental constraints.

8. **Peter Woit**
April 17, 2019

LMMI,
I've changed the header to get your mind off the EHT picture. I didn't write anything about it here, because I'm the wrong person to discuss black holes: it's a huge industry now, full of people who are experts, and best to find one of them to discuss this with.

The current header is temporary, I still haven't come up with a plan for a new permanent one.

9. **Anonyrat**
April 17, 2019

If only your banner could evolve to "Right! Finally!"

10. **Peter Woit**
April 17, 2019

Anonyrat,
That would be boring…

I've finished playing with the header, at least for today.

11. **Chris Oakley**
April 18, 2019

I think that a good banner would be something that lists the equations of Superstring theory. Something like this: http://cgoakley.org/qft/sseq.jpg

12. **Atreat**
April 18, 2019

That article about the New Zealand astronomy battle is wowzers. Such bad behavior on the part of scientists.

Upshot, is that the guy writing the journalist accusing his peer of having mental health issues with absolutely zero factual basis has not apologized, will not apologize, his university still supports him and how does he justify his behavior…? He says that the other guy was not kind and was rude at a conference and did not adhere to professional standards of courtesy of being kind… so therefore it was appropriate to asperse him with claims of mental illness to a third party… or something.

13. **Peter Woit**
April 18, 2019

Atreat,
For the latest, see here

https://www.newsroom.co.nz/2019/04/17/542061/ska-scientists-come-out-to-condemn-smears#

14. **Low Math, Meekly Interacting**
April 18, 2019

Chris Oakley…

A variant on one of Pauli's other devastating take-downs?

"This is to show the world that I can paint like Titian … Only technical details are missing."

15. **Peter Woit**
April 18, 2019

LMMI,
New header generated.

16. **ay**
April 22, 2019

So the new header(s) are some kind of easter egg? What does the Pauli note say?

17. **Peter Woit**
April 22, 2019

ay,

At the moment the header you get is a random choice of four possibilities. I'll likely change this again when I get time. The Pauli thing is a page of a letter to Gamow, that he encouraged Gamow to show to others, as his statement about what he thought of Heisenberg's publicizing a "unified theory" (which Pauli had worked on with Heisenberg for a while, but became disenchanted with). It says:

"This is to show the world that I can paint like Titian.

[empty rectangle]

Only technical details are missing."

18. **Anon**
April 22, 2019

Anyone know if the Microsoft event is open to the general public? The Redmond microsoft office phone numbers don't work and microsoft support is sending me through endless automated loops of asking about which Microsoft product I'm unhappy with.

19. **Peter Woit**
April 22, 2019

Anon,
This listing
https://asaip.psu.edu/meetings/all-meetings/physics-meets-machine-learning
for the event says "please contact an organizing member if you wish to participate in this workshop. "

20. **martibal**
April 22, 2019

The random header is a nice idea, but why not include into the list the black hole picture ? It is not that much off topic, and very beautiful.

21. **Peter Woit**
April 22, 2019

martibal,
I think you haven't refreshed your browser often enough…

22. **Jon Butterworth**
May 8, 2019

Ta for linking the review. For what it's worth, the group in which I included you indeed has a variety of different criticisms of theoretical physics, not all them "too much math(s)". The bit I thought applied to you (and Lee Smolin) was "become a monoculture too focused on a small clutch of concepts and approaches", which I don't think you'd disagree with?

23. **Peter Woit**
May 9, 2019

Jon Butterworth,

Yes, that's right, I'm very much in agreement with Hossenfelder and Smolin about the "monoculture" problem you refer to.

Falsifiability and Physics

April 23, 2019
Categories: Uncategorized

Symmetry magazine today published an article on Falsifiability and physics, yet another in the genre of defense of current HEP theory against its critics. As usual, only defenders of the status quo are quoted, the critics remain unnamed and their actual arguments ignored. I don't completely understand this journalism thing, but if you are writing about a controversy, aren't you supposed to contact people on both sides?

The problems with this article begin with the misleading subtitle: "Can a theory that isn't completely testable still be useful to physics?" The problem here is not theories that aren't "completely testable", but theories that aren't testable at all, that make no testable predictions at all.

The article starts out by discussing Popper and the supposed "falsifiability" criterion for what is and isn't science, leading up to:

> But where does this falsifiability requirement leave certain areas of theoretical physics? String theory, for example, involves physics on extremely small length scales unreachable by any foreseeable experiment. Cosmic inflation, a theory that explains much about the properties of the observable universe, may itself be untestable through direct observations. Some critics believe these theories are unfalsifiable and, for that reason, are of dubious scientific value.

Who are these "some critics"? Where do they say that the reason there is a problem with string theory is "unfalsifiability"? For the case of one critic I'm pretty familiar with, chapter 14 of his book is all about how "falsifiability" is not something that can be used to decide what is science and what isn't.

We're then told that:

> At the same time, many physicists align with philosophers of science who identified flaws in Popper's model, saying falsification is most useful in identifying blatant pseudoscience (the flat-Earth hypothesis, again) but relatively unimportant for judging theories growing out of established paradigms in science.

Unclear who "many physicists" are, who the "philosophers of science" are, and what flaw in Popper is being referred to.

In an odd move, the article then turns to the topic of SUSY, where the problem isn't that well-advertised SUSY models (with electroweak scale SUSY breaking solving the "naturalness" problem) aren't falsifiable, it's that the LHC has falsified them. As usual in science, if your model gets falsified, instead of giving up and doing something else you can change your model to something less desirable that hasn't been falsified (SUSY models with symmetry broken at higher energy scales) and keep on going. This is though what philosophers of science call a "degenerating research program", which is not a good thing.

There's more in the rest of the article, but actual critics remain invisible and their actual arguments unaddressed.

Update: Will Kinney has some appropriate comments.

Update: Massimo Pigliucci has posted here his contribution to the "Why Trust a Theory?" volume, which discusses "falsifiability" and the "String Wars".

Comments

1. **David Appell**
   April 23, 2019

   I wouldn't call the writers for Symmetry "journalists." They're not and that's not their goal or purpose. Symmetry magazine, by design, has an agenda. Moreover, it's not reader supported:

   "Symmetry is a joint publication of Fermi National Accelerator Laboratory and SLAC National Accelerator Laboratory. Symmetry receives funding through the US Department of Energy."
   https://www.symmetrymagazine.org/about

2. **GoletaBeach**
   April 23, 2019

   I don't really see that article via the HEP theory lens. It makes some point or another about how for atoms falsifiability was an inappropriate criterion. I think the accurate point is: atoms were found because experimentalists kept at it and found them. Echoing an old essay by Luis Alvarez…. likely if there had been a review panel of theorists, they'd have never funded the experimental work that led to atoms being discovered, because of some philosophy or theory or prejudice… just like RT Cox's discovery of parity violation in the 1930's was not appreciated.

   From an experimental perspective, more beam energy has traditionally been the most effective way to make new discoveries. There was not a clear mass target until about the PEP and PETRA eras, where they hoped to make top quarks (and failed).

   Somehow the requirement of a vetted mass target seeped in through the review committee process now to all accelerator proposals.

   Best to just ignore SUSY or whatever and build the next machine. There will be (and always have been) naysayers who say it isn't worth it. Well, the marketing budget in the US for tobacco is… almost $10 billion/year. Societally, a 100 TeV machine isn't expensive. If nothing is discovered, of course, it would have been better to have spent the funds on air conditioners for elderly and impoverished people in US cities… or simple water purity in most of the world. If a shocking discovery that rewrites the foundations of physics is made… well… easily worth more than tobacco marketing.

3. **Peter Woit**
   April 23, 2019

   David,
   The distinction I think you're trying to make between journalism and PR is not so clear here. I've dealt a little bit with people at Symmetry, for instance concerning this article
   https://www.symmetrymagazine.org/article/the-coevolution-of-physics-and-math
   It's not so easy to distinguish what they do from what Quanta does (other than that I bet Quanta has a lot more money…), although perhaps the Simons Foundation tries to keep Quanta's coverage separate from promoting their own research, while Symmetry may try to help promote DOE-funded research.

   In any case, even if Symmetry were strongly agenda-driven in support of Fermilab and SLAC scientists, I don't think it would be hard to find such scientists with critical views of string theory and SUSY. So the question remains, why just report one side of a controversy?

4. **Peter Woit**
April 23, 2019

GoletaBeach,

I more or less agree with you: ignore the theorists and, if at all possible, build a higher energy machine. This article isn't though at all about that, no experimentalists appear. There are lots of topics in the article, some of which have nothing to do with any controversy over falsifiability (e.g. I don't think dark matter models and testing them is particularly controversial).

Leaving inflation aside, the controversial topics are string theory unification and SUSY, and the fundamental controversy is over how you evaluate the results of this research. The results now look like a failure if evaluated by any conventional scientific standard. The claim that falsifiability is an inappropriate way to evaluate these ideas is not then supplemented by any suggestion of how they should be evaluated. A lot of this article looks like an attempt to basically argue that these ideas should be immune from evaluation.

5. **Amitabh Lath**
April 24, 2019

The article points out that for Popper unfalsifiable theories were Freudian psychology and Stalinist history. Surely you don't think SUSY belongs with them? It is falsifiable, you may need equipment and techniques that are beyond our capabilities (for now) but that's not the theory's fault. Neutrinos were considered unfalsifiable by Fermi.
As for string theory, AdSCFT etc. doesn't pretend to be about our reality so the where do you even start with the falsifiable argument? You could argue it's been N decades and they really should have gotten the math straight by now and made some testable predictions but they could counter with "there aren't enough people working on it, we need more string theorists."

6. **martibal**
April 24, 2019

It seems hard to counter with "there is not enough string theorists". By a very rough approximation, I would say that the number of string theorists is one order of magnitude bigger than the number of people working in the second best known approach to quantum gravity (loop quantum gravity), which is again one order of magnitude bigger than any other approach to quantum gravity/unification (dynamical triangulation, noncommutative geometry etc).

Do the quality of the results obtained in these various theories range in the same order ? Not sure that the number of people working on a theory is such an accurate criteria.

7. **Davide Castelvecchi**
April 24, 2019

Peter,

I enjoy reading Symmetry and I have written for Symmetry earlier in my career. People who work there have standards of integrity. But they are very conscious of the fact that what they are doing is not journalism. It's science communication, and often of very high quality, but it's not journalism. Quanta is a whole other story. It is philantropist-sponsored — which comes with its own caveats — and tends to take the side of certain communities, but it still explocitly strives to uphold journalistic standards.

8. **Peter Shor**
April 24, 2019

@Amitabh Lath:

If, as you say, "string theory and AdS-CFT doesn't pretend to be about our reality," why are so many physicists convinced that AdS-CFT shows that quantum gravity in our universe has to be unitary?

And there aren't many physicists in Physics departments working on different theories of fantasy physics, while there are tons of them working on string theory and AdS-CFT. Shouldn't fantasy physics be studied in Philosophy, Mathematics, or Creative Writing departments, rather than Physics departments?

This is just a fairly transparent *ex post facto* excuse for why string theory and AdS-CFT have failed to say anything useful about physics in our universe.

9. **Dom**
April 24, 2019

This paragraph quoted below has me puzzled because it implies that somehow falsification is impeding people working on new ideas they know are likely to be wrong (which tends to take care of the falsification bit it seems to me).
"Tracy Slatyer of MIT agrees, and argues that stringently worrying about falsification can prevent new ideas from germinating, stifling creativity. "In theoretical physics, the vast majority of all the ideas you ever work on are going to be wrong," she says. "They may be interesting ideas, they may be beautiful ideas, they may be gorgeous structures that are simply not realized in our universe.""

10. **vmarko**
April 24, 2019

From the article:

"One such theory is cosmic inflation, which (among other things) explains why we don't see isolated magnetic monopoles […]"

All the falsifiability stuff aside, I still find it hard to believe that serious people consider this as a valid argument in favor of inflation. In addition to magnetic monopoles, the cosmic inflation could very well be able to explain why there are no unicorns in nature. Would that increase anyone's confidence in the theory?

The lack of magnetic monopoles is "explained" (basically by definition) by Maxwell's classical electrodynamics. A serious scientific/skeptical attitude should be that — absent any compelling arguments that something ought to exist — the default position is that it doesn't exist. The burden of proof is squarely on the person who claims that something ought to exist, not the other way around.

Of course, the magnetic monopole waters were muddied by the story that GUT's give a compelling argument that monopoles ought to exist, so their absence becomes something that should be explained. However, by far and large, GUT's have been falsified by lack of proton decays, which (among other things) disqualifies their argument regarding the existence of magnetic monopoles. You shouldn't seek to explain something that is predicted by a theory that is known to be wrong.

Are there still that many die-hard GUT-supporters out there, who keep the magnetic monopole argument alive, despite all proton-decay experimental results? Or is it just the case of people parrott-repeating arguments from books which are half-a-century old and haven't been updated with more recent data? Or am I missing something obvious here?

Best, 🙂
Marko

11. **Low Math, Meekly Interacting**
April 24, 2019

I find the whole notion of this debate distressing and always have. I know of very few people who are fundamentalist in their belief in the "scientific method", but for me some hope of contact with experiment is a non-negotiable. I simply can't fathom the notion of "science" without it.

Of course there needs to be toy models, simplifications, abstractions and so forth to attain the goal of observable consequences. But if the goal of modeling nature as it is, in a both descriptive and predictive manner, is dispensed with indefinitely in pursuit of other criteria for success which are necessarily human constructs, then in my opinion hope is lost. Human judgment simply cannot be trusted indefinitely, and humans should not feel offended by this truth. It is in our nature to err, and we cannot avoid it without external guidance. If the universe exists without us, then it is our only ultimately reliable guide, and if we cannot extract the information we need from it, then I think it is ultimately folly to try.

That might mean giving up on answering some questions. The fact that many find this unacceptable may be a major source of the current predicament.

12. **Peter Shor**
April 24, 2019

@Dom:

I think that one of the ways physics has advanced over the years is that people come up with all sorts of crazy ideas (relativity, quantum mechanics, Dirac delta functions, the replica method, Feynman diagrams, all certainly seemed crazy to some people when they were first proposed), and only kept the ones that agreed with experiment. Without experiment, physicists have no way to discard the incorrect crazy ideas and keep the correct ones.

Tracy Slayter objects to using the falsifiable criterion, as it will keep physicists from coming up with crazy ideas in the first place, but I don't think that's the real problem.

I am beginning to think that the problem with HEP theory currently is not that physicists don't discard non-falsifiable ideas, but that without experimental input, they don't have any reasonable way to vet their crazy ideas, so they end up throwing out the baby and keeping the bathwater. There is certainly some sociological process going on that selects some of the crazy ideas, and reject others.

13. **Peter Woit**
April 24, 2019

Amitabh Lath,
As I keep trying to explain, I've never been someone who thinks "falsifiability" is the issue here and in particular re SUSY it's a sterile debate. Amidst SUSY models you can find whatever you want: falsified already, falsifiable now, falsifiable at HL-LHC, falsifiable at FCC-hh, falsifiable a hundred years from now, unfalsifiable. The only clear relevance of the falsifiability criterion here is that people should stop paying attention to the small number of Gordon Kanes of this world who, in the face of repeated experimental falsification of an idea of theirs, refuse to give up on it.

Re SUSY, I think the best advice now for experimentalists is to ignore most of what theorists have to say about the virtues of this or that SUSY model, and just ask the pragmatic question: does the model provide a useful target for designing searches that will be sensitive to a significant range of possible new physics?

14. **Peter Woit**
April 24, 2019

vmarko,
Agreed about the weirdness of "inflation explains no GUT monopoles" argument, given that the relevant GUT models have failed other tests, so the simplest explanation for no GUT monopoles is that the GUT models are wrong.

One sorry aspect of these debates is that arguments made long ago for string theory/SUSY/GUTs/etc. do keep getting repeated, long past the time when argument has failed. A lot of this is people just repeating things they read long ago, unaware of what has happened since. Some of it is people who should know better, but for whatever reason can't let go of a falsified argument.

15. **DB**
April 24, 2019

vmarko,
this might be of interest to you.
Short video. The magnetic monopoles stuff starts at 02:30.

https://www.youtube.com/watch?v=9O-5ujTgZiQ

Peter (and the rest):
Why wouldn´t the top theoretical physicists let go of some theories which, according to themselves, do not describe reality?
It´s certainly not because of money (they´re all sorted…).
Can anyone think of another reason?

Finally, this 2014 interview with Susskind is very interesting.
From the 50th to the 55th minute he talks about No Symmetry, No Beauty, and …
letting an idea/theory go when it´s not correct!!

https://7thavenueproject.com/post/93035821815/leonard-susskind-radio-interview

16. **Peter Woit**
April 24, 2019

Dom/Peter Shor/LMMI,

I don't think anyone is seriously arguing that theorists should only work on ideas falsifiable now or in the near future, and I also don't think those working on highly speculative ideas often argue that it doesn't matter whether what they are doing ultimately leads to something testable. The real problem is not "falsifiablity", but how you evaluate the progress of speculative research programs. You can't do this by demanding an experimental test, but you also can't just accept theorists' assurances that people should keep supporting them to continue work on their favorite idea of thirty years ago, despite all evidence it hasn't worked out.

What's disturbing to me is that, increasingly, the string unification/SUSY research program seems to have moved from "evaluate us by LHC results or progress on these crucial problems that are in between us and a testable theory" to "there is no way to evaluate us, you just have to believe us, because there are so many of us and we're so smart." That's not the way science is supposed to work, for good reason.

17. **Peter Woit**
April 24, 2019

DB,
What I've learned over the years is that, once people become devoted to a certain idea and invest a lot of their time, energy and public reputation in it, they are highly unlikely to ever abandon it, no matter what new information comes along and no matter how smart they are. The best you can expect is "I still believe there must be something right about that idea, but I'm now spending my time doing something else…."

18. **DB**
April 24, 2019

Peter,
thanks for the reply.
Your last sentence is key for me.
I´m starting to think that many of the top string theorists have sort of "given up", and are really working on something else.
Though they don´t want to come out and say it in public, for obvious reasons.
Which, honestly, leaves them in a bit of a dishonest situation.

It will be very interesting to see how many of the Strings 2019 conferences in Brussels next July will focus on ST itself.

19. **Anonyrat**
April 24, 2019

IMO, if interesting mathematics is emerging from a speculative line of investigation in theoretical physics, and if "interesting" mathematics has some reasonably objective meaning, then that might constitute a measure of progress sufficient for the decision of whether to fund those theorists.

20. **Peter Woit**
April 24, 2019

Anonyrat,
I'm all in favor of supporting theoretical physics work that leads to interesting new mathematics. Keep in mind though that evaluating what is interesting new mathematics is something you need mathematicians for. Most physicists are likely to take the attitude that work that needs to be evaluated by mathematicians should be funded by the mathematics part of NSF. They may even take the attitude that theorists doing this kind of work should be in a math department. There are a few places (e.g. the Simons Center at Stony Brook) where there's a healthy overlap of math and physics research, but unfortunately that's not the case most places.

21. **vmarko**
April 24, 2019

Anonyrat and Peter,

There are some countries (like Portugal) in which the academic system is set up so that any non-experimental physics (i.e. all theoretical physics, not just hep-th) is a part of the math departments, rather than physics departments. IOW, if you want to be in a physics department, you need to be working in a *lab*, and measure something with some equipment. Everything else, any pen-and-paper work, is considered math.

The results of such an arrangement are, mildly put, catastrophic. The applications for projects, results that are obtained, papers that are published, etc., are all evaluated by committees in which theoretical physicists are present, but mathematicians are a majority. A paper published in a journal with "Math." in its name is more worth than a PRL. And if you ask a mathematician to judge the work done by a theoretical physicist, they will mostly laugh at it. The levels of rigor, precision, and definition-theorem-proof style of writing, that mathematicians are used to, are something a theoretical physicist can never even hope to reach. Mathematicians by and large regard theoretical physicists as amateur wannabe-mathematicians, who are toying with some equations and delude themselves that they are doing something serious. They often dismiss the work of physicists just because it is not formulated as a theorem and proved rigorously, even without looking at what was actually done. And they often have a hard time understanding why the work of a hep-th physicist cannot be formulated as a theorem… I was often asked stuff like "How do you even know what you are doing, if you cannot explicitly spell out all your assumptions for me, before you make a statement what the result is?"

Theoretical physics is not math. The whole math mentality is completely different from physics mentality, with different goals, intuition, background education, ways of thinking, and levels of rigor. There is just no way to consider theoretical physics as part of proper math, and it's a Bad Idea ™ to even try.

Best, 🙂
Marko

22. **Bernhard**
April 25, 2019

Peter,

Also, one point you have mentioned ad nauseam that I have never seen been addressed in this or any article is the question of the negative progress rate of String Theory as opposed to whether is falsifiable or not. String Theory could become a falsifiable tomorrow. But it's pretty clear it won't be. Not tomorrow, not ever. Every time I read those types of articles people discuss with a straw man never addressing the real issue.

23. **Amitabh Lath**
April 26, 2019

Peter Voit, Peter Schor, the longevity of string theory is not due to the middle-aged practitioners you mention but kids in their early 20s who continue to choose to go into the field. Some of the best undergraduate students in our high energy experiment group have over the years chosen to go to grad school in theoretical physics 🙁

Some go into phenomenology but some are indeed doing string theory.

These students are the most smartest and most sensible I have ever met, the cream of the Garden State. They devour the literature, they are fully aware of the arguments on all sides. I cannot in any seriousness entertain the idea that they are led astray by hyperbole. I believe all the arguments about string theory not having made any progress in decades, not producing any testable results, being stuck in a made-up universe nothing like our own reality; these are not deterrents but attractions to this type of student.

24. **Peter Woit**
April 26, 2019

Amitabh Lath,
The intended audience for the typical promotional string theory hype-fest piece is not ambitious youngsters considering a career in particle theory, but physicists in other fields, people in other sciences, university administrators, interested laymen, etc. In general, people who might have something to say about whether string theory research gets supported, and aren't in a good position to evaluate the claims being made themselves. When you're reading an article supposedly about some controversy, but only one side is quoted, what you're reading has an agenda behind it.

The topic of what's going on with talented undergrads who are interested in theoretical physics, want to study it more deeply and possibly go to grad school and try to make a career in the subject is an interesting one, but not so relevant to articles like this. Yes, they aren't going to impress a smart student seriously studying particle theory, and such students now do hear both sides of the controversy over string theory.

I know quite a few such students, some pretty well, either through teaching them here at Columbia and then following their later progress, or through having students from elsewhere contact me for one reason or another. What I see happening now (at least in the US) is that the best students are, as always, going to a small number of the top graduate programs (e.g. Harvard, Princeton, Stanford), where most of the theory faculty often identify tribally as "string theorists", but are now working on topics in GR/QFT/quantum information, etc. that have nothing to do with quantized strings or with string-theory based unification. The odd thing I keep hearing is that such students arriving at such a grad program are encouraged to spend a lot of time studying actual string theory (e.g. by reading Polchinski's two volumes) to prepare to start research, even though the research likely won't use any of this. My impression is that a lot of the theory faculty are unsure themselves which way the field should be going, and by default are suggesting students start off the same way they did 20 years ago.

Studying theoretical fundamental physics attracts, for good reason, and despite the field's problems, some of the best students around. Sometimes I do fear though that one reason the field is not generating much in the way of new ideas is not so much that the best students are drinking string theory kool-aid, but that they're getting their entry into the subject of fundamental theory in a way that hinders rather than helps them come to grips with the real problems of the subject.

25. **Peter Shor**
April 26, 2019

Amitabh Lath:

When it looks like two recent papers in a field are incompatible (despite both of them being quite reasonable papers on their own), and talking to some people in the field, nobody even seems to have noticed this fact, I suspect that something has gone seriously wrong with the field.

The two papers are (a) the ones suggesting the the CFT in the AdS-CFT correspondence is a quantum error correcting code and (b) Shenkar and Stanford, "Black holes and the butterfly effect," which starts out essentially saying "make a small perturbation to the CFT".

If you make a small perturbation (or even a large perturbation) to a state in a quantum error-correcting code, for any kind of quantum error-correcting code I am familiar with, you are extremely likely to be taken out of the quantum error-correcting code, which means that — as far as I can tell — the rest of the paper doesn't hold up.

Let me say that I think these are both good papers, but they are essentially starting with two different hypotheses about AdS-CFT, and I don't see how they can both be applicable at the same time. The fact that nobody seems to have even noticed this is very troubling to me. But maybe I'm too much of a mathematician.

26. **Amitabh Lath**
April 26, 2019

Peter V, I understand your point but the decisions made by these top tier students does much more to sway these "people who might have something to say about whether string theory research gets supported" than some national lab's public outreach 'zine.

Every grad program wants these students: sky-high physics-GRE, letters dripping with superlatives, transcripts with half a dozen graduate level courses completed as undergrad. They are courted with fellowships and awards. Their eagerness to join the field is seen as proof of vibrancy. If a big-name string theorist leaves your department and the acceptance rate for these blue-chips drops, you know the search committee will form quickly.

27. **Mark**
April 27, 2019

Peter Shor:

The Shenker-Stanford butterfly effect has since been independently found in CFT using strictly field theory tools, so I'd say that it's on a firmer ground than the error correcting code proposal. The HaPPY code's claim to fame is that you can act on it with operators in the "bulk", which is equivalent to acting on some "boundary" subregion. People don't know how to do time evolution in this approach, so they can't reproduce the butterfly effect from quantum error correction. But if I understood your comment, you're saying that already acting on the state should take you out of the code.

28. **Blake Stacey**
April 27, 2019

Peter Shor asked, "[W]hy are so many physicists convinced that AdS-CFT shows that quantum gravity in our universe has to be unitary?" I have also wondered about this. AdS is said to be "a box to put gravity in", but that comes at a price: The stability properties of gravitation on AdS are qualitatively different from those for dS or Minkowski backgrounds. The answer to the question "Will doing this experiment make my laboratory collapse into a black hole?" is, in principle, different, precisely because of that convenient boundary that makes for a nice box and gives a place for the CFT to live. This makes it hard for me to accept the reassurance that I was given many years ago, the standard colloquium line that we can put any local physics we want into an AdS box and apply the lessons from that exercise to our own world.

29. **Peter Woit**
April 27, 2019

All,
I fear this is the wrong place to debate the issues raised by Peter Shor about CFT and error-correcting codes, partly because the moderator knows nothing about the topic (he would like to someday understand what that's about, but today is not the day…)

30. **Peter Shor**
April 27, 2019

@Mark:

Just to be clear, I have no objections to the Shenkar-Stanford butterfly effect in CFTs that aren't also error correcting codes.

And probably we should stop any discussion here, to keep the moderator happy.

31. **niels abel**
April 28, 2019

CERN has a jpg and a pdf on today's (2019-04-28) Header Image:

From http://library.cern/archives/history_CERN/historical_images?page=2

Pauli thought Heisenberg's 'World Formula' needed a lot more work, and he made his point graphically. He sent this drawing of an empty picture frame to George Gamow on 1 March 1958 with the caption, 'This is to show the world that I can paint like Titian … Only technical details are missing.'

http://library.cern/sites/library.web.cern.ch/files/Pauli%20to%20Gamow%201%20March%201958.jpg

https://cds.cern.ch/record/86339/files/gamov_0100-52.pdf

32. **zzz**
April 29, 2019

new to me, i apologize if you already saw it

https://richardelwes.co.uk/2015/01/02/the-grothendieck-song/

The Scientific Attitude

April 29, 2019
Categories: Book Reviews

Just when I thought I was done for now with the "falsifiability" business, in our local book store I found a new book, The Scientific Attitude: Defending Science from Denial, Fraud and Pseudoscience, by Lee McIntyre. This won't be a review of the whole book, much of which is concerned with what to do about the serious problem of the role of science in our increasingly post-truth society. I'll just address the few pages of the book that deal with string theory, in which a quote from me appears in a misleading way.

The problem here is almost exactly the same as the problem with the Symmetry article discussed in the last posting. Both authors believe that string theory is a conventionally predictive theory, one with predictions that just happen to be hard to test. According to them, critics of string theory just don't understand that there can be value in a theory which is testable in principle, even if a practical test is far away. Unlike the Symmetry piece, McIntyre at least names critics and links to their words, writing:

> If one reads these kinds of criticisms closely one finds careful phrasing that string theory "makes no predictions about physical phenomena at experimentally accessible energies" and that "at the moment string theory cannot be falsified by any conceivable result."[23] But these are weasel words, born of scientists who are not used to taking seriously the distinction between saying that a theory is "currently" testable versus whether it is "in principle" testable. The practical limitations may be all but insurmountable, but philosophical distinctions like demarcation live in the difference.

The quoted words are mine, with footnote 23 referring to my 2002 article in American Scientist. Of course I was and am well aware of the distinction between testable "in principle" and "currently". Bizarrely, the author has chosen to edit out from what I wrote the sentence that precisely addresses the issue I'm supposedly weaseling on. Here's the full quote:

> String theory not only makes no predictions about physical phenomena at experimentally accessible energies, it makes no precise predictions whatsoever. Even if someone were to figure out tomorrow how to build an accelerator capable of reaching the astronomically high energies at which particles are no longer supposed to appear as points, string theorists would be able to do no better than give qualitative guesses about what such a machine might show. At the moment string theory cannot be falsified by any conceivable experimental result.

As the deleted language make clear, by "any conceivable experimental result" I was making a claim about "in principle", not "currently". Furthermore, near the beginning of the article I explain the problem of principle:

> First, string theory predicts that the world has ten space-time dimensions, in serious disagreement with the evidence of one's senses. Matching string theory with reality requires that one postulate six unobserved spatial dimensions of very small size wrapped up in one way or another. All of the predictions of the theory depend on how you do this, but there are an infinite number of possible choices, and no one has any idea how to determine which is correct.

This article started out as an early 2001 arXiv posting and was published in early 2002, about a year before the now famous KKLT claim to have a string theory model with fully stabilized moduli. Back then, the problem I was pointing to was the basic one that, to have a self-consistent string theory model that you can confront, in principle, with experiment, you need to solve the problem of "moduli stabilization". 6d compactifications come in families with a lot of parameters (the "moduli") governing their size and shape, and the physics depends crucially on those parameters. You need to somehow give the moduli dynamics, and get a ground state with a correct fine-tuned vacuum energy.

KKLT claimed they could do this, but with an exponentially large "landscape" of solutions that removes the ability to get well-defined predictions from the theory. Their construction is so complicated, and non-perturbative string theory so poorly understood, that it remains controversial to this day whether these are really solutions to whatever the conjectural well-defined version of string theory might be. This is what the current "Swampland" argument is about.

I've put together a FAQ entry answering the Doesn't string theory make predictions at very high energy? question. What causes all the confusion here is the common claim from string theorists that "string theory is testable at high energy". If you ask them to tell you what the "test" is, they tell you about one of the characteristic features of the perturbative superstring (Veneziano amplitude, Regge trajectories, 10 space-time dimensions). What they are really saying is "if we did experiments at a high enough energy scale and saw one of these characteristic phenomena, we would have a successful test of string theory", which is true enough, but not a specific, falsifiable prediction. What they are not telling you is that they are ignoring the compactification problem as well as that of not having a well-defined non-perturbative theory, and that many "string theory" models wouldn't exhibit these characteristically perturbative features.

The main point of the new book seems to be to argue that a better way to characterize science is by whether those supposedly engaging in it are exhibiting the "scientific attitude", which

> can be summed up in a commitment to two principles:
> (1) We care about empirical evidence.
> (2) We are willing to change our theories in light of new evidence.

It seems to me there are lots of problems with this formulation. Sticking to the string theory question, undoubtedly string theorists "care about empirical evidence" and would like to have some. The problem though is they don't have any, and don't have any significant prospects for getting any. As for being willing to change one's theories in light of new evidence, if there's no new evidence, your willingness to change your theory won't ever get tested.

My impression is that most people, this author included, are just fundamentally unwilling to believe that, given the high scientific profile of "string theory", it could really have a serious problem of being inherently untestable. The technical issues involved are so formidable that non-experts don't have any hope of understanding them. But there really is a serious problem here, and those who worry about the string theory fiasco damaging the credibility of science in a dangerously post-truth world are right to be worried.

Comments

1. **Frank Wappler**
   April 30, 2019

   woit wrote (April 29, 2019):
   > […] a new book, »The Scientific Attitude: Defending Science from Denial, Fraud and Pseudoscience«, by Lee McIntyre
   > […proposing] "scientific attitude", which

   > »can be summed up in a commitment to two principles:
   > (1) We care about empirical evidence.
   > (2) We are willing to change our theories in light of new evidence.«

   > It seems to me there are lots of problems with this formulation […]

I agree that there are problems with McIntyre's formulation; and I would call commitment to (merely) these two listed principles rather more specificly an "**unstructured** scientific attitude", oder "**pre-**scientific attitude".

A "(**structured**, **actual**) scientific attitude" might be instead summed up in a commitment to three principles:

(0) We care about notions (that go without saying), definitions, principles and methods (expressible in terms of those notions) which allow us to derive empirical evidence (from observations, as they become availabe to us).

(1) We care about collecting observations from which to derive empirical evidence by methods that can be adhered to.

(2) We are willing to change our expectations (about which observations and empirical evidence may still be forthcoming) in light of new evidence.

2. **Martin**
April 30, 2019

McIntyre writes "The practical limitations may be all but insurmountable, but philosophical distinctions like demarcation live in the difference". The demarcation is about demarcating science from pseudo-science. This is a legal argument, which is based on the fact that critics of string theory naively argue that falsifiability would be the criterion. McIntyre loves this criticism. The real point is that there is no empirical evidence in support (!) of string theory. Scientists do not attempt to construct falsifiable, i.e. wrong, theories, they attempt to construct theories that work. Obviously, when a theory should work then it might turn out that it does not work, but that is not the objective. That is why McIntyre states "We care about empirical evidence", deliberately without saying how the empirical evidence, if it could be obtained, should relate to the credibility of string theory. McIntyre argues like a lawyer – the critics cannot prove that string theory is not scientific. Therefore not guilty.

3. **Peter Woit**
April 30, 2019

All, I really don't want to host a discussion of McIntyre's ideas about the "scientific attitude" in general. I doubt many commenters have read the book, and I don't intend to read more of it, so if it's not about the string theory issue, please find somewhere else to discuss with the author.

I've been having trouble figuring out why this author would quote me in such an unprofessional way. Taking a look at his other recent activities, I fear the answer is that he's decided resistance to Trump justifies abandoning usual standards of academic writing, especially fairness to those you feel are on the other side. From this point of view, any criticism of prominent scientists puts one on the anti-science/Trump side, and given the current emergency all takedown tactics are fair. If only I could get McIntyre to read the collected works of Lubos Motl….

4. **vmarko**
April 30, 2019

Peter,

Have you contacted the author to ask him about this misinterpretation of your words? How does he explain it? How about the book publisher, can you complain to them?

Best, 🙂
Marko

5. **Peter Woit**
April 30, 2019

vmarko,
No, that seems like a waste of time. This author clearly knows nothing about string theory and cares less, it's just minor grist for his "science is under threat" agenda. Would be best to ignore this, but I did want to set the record straight.

6. **Daniele Corradetti**
April 30, 2019

I'm not at all a string theory fan and generally speaking I quite agree with the views expressed on this blog, so this is not a troll-question but a true-question. Reading Heisenberg, I clearly remember him saying that "Copernican theory doesn't agree at all with observations since every day everyone observes the Sun rising, culminating and moving around the earth". He says that in fact Copernican theory constrasts what is totally evident from observations to evidentiate a unitary and mathematical principle. My question is if this is in some kind different from string theory predicting that the world "has ten space-time dimensions, in serious disagreement with the evidence of one's senses". I repeat I'm really interested and is a true question, not trying to trolling anyone.

7. **Dave Miller**
May 1, 2019

Peter,

You said, "I've been having trouble figuring out why this author would quote me in such an unprofessional way." My guess is he took brief notes on stuff he thought might be useful and later forgot the larger context of those notes. The same effect can also occur from cutting-and-pasting different phrases in a manuscript as the author re-works the manuscript. Something like this seems to be what happened with various professionals (e.g., Doris Kearns Goodwin) who engaged in plagiarism but probably did not intend to.

A point that needs to be more clearly addressed in these ongoing discussions is what is wrong scientifically with producing random, string-inspired phenomenological models that can be multiplied ad infinitum, after each particular model fails.

I myself am not quite clear on how to formulate this succinctly, but, somehow, a scientist should be invested in a hypothesis in such a way that, if the hypothesis is falsified, it truly alters his thinking on the subject.

Somehow, it is not fair to toss out a hypothesis, knowing that, if it is falsified, you will just toss out another slightly tweaked hypothesis, and so on when it is falsified, again and again and again. (We all know of a phenomenologist in the upper Midwest who has a tendency to do this!)

I think the point may be that the goal of scientists is not simply to make and then confirm or disconfirm *hypotheses* but rather to seriously advance our knowledge of the universe by creating broad new theories that are significantly broader or more accurate than existing theories.

Just churning out one hypothesis after another, never being fazed by their disconfirmation, fails to advance the cause of building a better, broader theory.

All the best,

Dave

8. **Thomas Mattison**
May 1, 2019

Given an accelerator of astronomical energy, it seems likely that an experiment could falsify the hypothesis that the particles we know today are pointlike.

That would not prove they are string-like. But strings would be a reasonable working hypothesis under the circumstances. Other hypotheses might be equally plausible in the absence of more data.

You make a good case that the experimental program that would be required to which particular discrete compactification of string theory is correct would be daunting.

Is there no hope of organizing the landscape into a pseudo-continuous space with a hierarchy of parameters? So if you knew approximate values for the most important few, you would be able to make some meaningful predictions? So a few experiments could be extrapolated into predictions for more experiments ?

9. **Peter Woit**
May 1, 2019

Daniele Corradetti,
The point I was making was just that all our observations show four dimensions. If your theory says there are ten, you need to explain why we only see four. You need a theory of the dynamics of the other six dimensions, explaining why we don't see them and how you could in principle see them. The main problem with string theory is that there is no convincing answer to this question.

Dave Miller,
I don't see that problems with note taking are an explanation here. This author doesn't appear to be a Doris Kearns Goodwin…

Besides a few outliers like Gordon Kane, the problem with string theory is not that string theorists are making predictions, then changing them when they get falsified. The problem is that the framework they initially thought was predictive has turned out to be empty, with the partial theory they have incapable of predicting anything currently observable, and consistent with just about anything. The last hope was that the LHC would see something (e.g. SUSY) compatible with the string theory framework, but that hope is now dead.

Thomas Mattison,
Yes, if we could do experiments at the Planck scale we would likely be able to figure out what the quantum dynamics of space-time is, and in particular whether string theory had anything to do with it. My point is just that perturbative string theory is not capable of giving a consistent account of this (because of the compactification problem), so there's no "string theory prediction" of what you would see.

I actually don't think our current understanding of string theory is capable of giving a well-defined, consistent description of what the possible "string vacua" are, and thus what the "landscape" is. One could speculate that it has nice properties allowing us to calculate with it, but there's zero evidence for that.

10. **Lee Smolin**
May 1, 2019

Dear Daniele Corradetti,

Your question is an excellent one. I am not an historian, but I have read a lot of the history relevant to the Newtonian revolution, and I have the impression that a key part of the argument was carried out by Galileo, who argued by a combination of real and thought experiments that the Earth could be moving around the sun, without the effects of that motion being observed. (Essentially, he argued for the relativity of inertial frames.) But the issue was subtle because the very same observations (the ball falling to the foot of the mast of the smoothly moving boat…) justified both the claim that we would feel the motion were the earth moving and the claim that we wouldn't feel it, depending on whether one adopted the widely believed Aristotelean or the (yet to be) invented Newtonian dynamics. So Galileo's arguments rested (correctly) on a yet to be developed dynamics, and Copernicus, who lacked even reference to Galileo's intuitions, couldn't possibly tell a consistent or convincing story.

Meanwhile, Kepler went ahead and posited that the planets were influenced by a force from the Sun-which was a key step even if he did get details wrong. But what he really did that was crucial was to impose the conservation of angular momentum in the form of the equal area law, and show it was to first order in ellipticity equivalent to Ptolemey's law of equants.

So, after this overlong historical context, we can ask what a current day string theorist needs to do, to advance at least to the stage of Galileo and Kepler, and it is to invent some kind of principle that together with a good story based on a non-perturbative dynamics yet to be invented, that convinces us to ignore the evidence from the senses that the world has three stable spatial dimensions and not nine. And of course, this runs up against the problem of stablizing the moduli-a problem that Einstein knew about in the early 1920's which caused him to abandon the Kaluza-Klein idea. And that leads to KKLT, the swampland…and here we are.

11. **Thomas Van Riet**
May 1, 2019

Dear Peter

In your writings and those of Lee Smolin I see two main sources of criticism: 1) string theory is not sufficiently understood and 2) the Kaluza-Klein character of the theory anyhow makes it "unpredictable". Concerning 1) I agree and I hope more people will put energy in developing the foundations. Concerning 2) there might be a misunderstanding here. Consider alternative theories, without extra dimensions. These theories will always be confronted with non-uniqueness: these alternative quantum gravity theories cannot be coupled just to the standard model and most likely many other gauge groups and couplings will be possible. I am guessing an infinite choice. This is where string theory helps allot. String theory geometrizes this freedom AND by doing so it has shown surprising constraints and patterns in these possible 4D theories. Our struggles with moduli stabilisation and SUSY breaking only arises because the theory is quite constraining.

Sincerely,
Thomas Van Riet

12. **Peter Woit**
May 1, 2019

Thomas Van Riet,
I've just never seen any evidence that a string theory (or any other) geometrization in terms of extra space-time variables answers any significant question about the Standard Model. In the string theory case, instead of getting an explanation of features of the Standard Model, you have to put a huge amount of effort into (not clearly successfully…) trying to explain away why generic features (eg. SUSY, massless moduli fields) of these models are not observed. Fundamentally, you're asking people to accept a poorly understood, very complicated framework as an improvement over the Standard Model, with zero positive evidence for it (and, at this point, no plausible

prospects for getting any).

13. **Daniele Corradetti**
May 2, 2019

Dear Lee Smolin,
thank you for the insightful answer! I think is really a good point!

14. **Jim Given**
May 3, 2019

Peter,
I applaud you for having introduced your readers to the research project methodology of science, a la Inre Lakatos, as explaining quite well the situation with string theory, i.e., as a degenerate research programme. Hearing about the "scientific attitude" as an attempt at a serious contribution to philosophy of science made me so sad, I just wanted to point out that the long essay, "The Methodology of Scientific Research Programmes" by Lakatos is available in paperback, is well worth reading carefully, and will clarify these situations in physics theory that you struggle with, far more than such current, rather unfortunate attempts to redefine science. The problem is not science. Rather, you and I (and our generation) are spoiled, having been trained in science in an era of extraordinarily rapid progress. One comes to have faith that the logically "inevitable" next step will turn out to be correct, and that rapid progress will continue. Alas. As Lakatos notes, the current generation of scientists does not turn away from long adopted but failed research programmes. Rather, they retire, and are replaced by a new generation; hopefully, one that reads your blog-

15. **Andrew Krause**
May 10, 2019

You may appreciate this recent post on applications of the philosophy of science, and the associated paper. Quite a different subject matter, but perhaps this kind of Lakatosian analysis would help quantify some of the current state of affairs in fundamental physics? I don't understand enough on a deep level to be certain, but I find the approach overall valuable. Lakatos definitely popularized, if not originated, the ideas of a "degenerate research programme."

http://www.extinctblog.org/extinct/2019/5/1/what-to-do-with-scientific-disagreement

16. **Peter Woit**
May 10, 2019

Andrew Krause,

Thanks. I know nothing about paleontology, but the Lakatos distinction between a "progressive" and "degenerative" research program does seem to me the most illuminating way of thinking about how to evaluate scientific research programs. I've never seen string theorists deal with this argument, they seem to carefully avoid it, since by any measure string theory fits quite well into the "degenerative" characterization, not well at all into "progressive".

Witten Interview

May 2, 2019
Categories: Multiverse Mania, Uncategorized

Graham Farmelo has posted a very interesting [interview he did with Witten last year](), as part of his promotion of his forthcoming book [The Universe Speaks in Numbers]().

One surprising thing I learned from the interview is that Witten learned Calculus when he was 11 (this would have been 1962). He quite liked that, but then lost interest in math for many years, since no one gave him more advanced material to study. After years of studying non math/physics subjects and doing things like working on the 1972 McGovern campaign, he finally realized physics and math were where his talents lay. He ended up doing a Ph.D. at Princeton with David Gross, starting work with him just months after the huge breakthrough of asymptotic freedom, which put in place the final main piece of the Standard Model.

If only back in 1962 someone had told Witten about linear algebra and quantum mechanics, the entire history of the subject could have been quite different. It seems quite possible that within 5 years he would have picked up quantum field theory and maybe started thinking about Yang-Mills generalizations of QED, perhaps, at 16, beating Weinberg and Salam to the electroweak theory. Surely he could have figured out how to do one loop calculations in gauge theory, beating Gross/Wilczek/Politzer to asymptotic freedom and a Nobel prize, possibly a few years early. If he had done this at Princeton, he would have overlapped with John Schwarz, who surely would have then been more interested in pursuing gauge theory than string theory. So, no superstring theory or 1984 "revolution", and who knows what different sort of path the history of the field would have taken.

A lesson for all parents: if your child is an off-the-scale genius, learning Calculus at age 11, don't even think about trying to give them a normal childhood. Push them, hard, to skip grades, get to college/grad school early. Do whatever it takes.

I did though find some of the later parts of the interview quite depressing. While acknowledging that neither he nor anyone else has been able to figure out what string theory actually is, this hasn't shaken Witten's faith that it's the only viable path towards a unified theory. Most disturbing, on the topic of the landscape he says that he has gone from finding it upsetting to reconciling himself to the idea. For years, whenever asked about how evidence could be found for string theory, he would point to the naturalness arguments indicating that something like SUSY had to happen at the electroweak scale. Now that the LHC has falsified this and there's nothing to point to as any sort of "test of string theory", he shows no signs that this falsification has in any way shaken his faith.

Looking to the near future, he's most optimistic about the "It from Qubit" business. Maybe he's right and something will come of this, but I've seen no indication of a path to a unified theory in this direction (how do you get the Standard Model? Or has he just completely given up on that?).

I don't have time right now to transcribe the most relevant portions of the interview, might find time later, or maybe Farmelo will make available a transcription.

Update: As explained in the comments, the advice to parents was not meant to be taken seriously. No, your child is not going to grow up to be Edward Witten, and they do not need to hurry up to revolutionize physics before it is too late.

Sabine Hossenfelder had posted a transcript of the interview [here](). I'll add some extracts and some more comments about the interview.

About the landscape:

> These two puzzles although primarily the one about gravity which was discovered first are perhaps the main motivation for discussions of a cosmic landscape of vacua. Which is an idea that used to make me extremely uncomfortable and unhappy. I guess because of the challenge it poses to trying to understand the universe and the possibly unfortunate implications for our distant descendants tens of billions of years from now. I guess I ultimately made my peace with it recognizing that the universe hadn't been created for our convenience.
>
> GF So you come to terms with it.
>
> EW I've come to terms with the landscape idea and the sense of not being upset about it. As I was for many years.
>
> GF Really upset?
>
> EW I still would prefer to have a different explanation but it doesn't upset me personally to the extent it used to.
>
> GF So just to conclude what would you say the principal challenge is all down to people looking at fundamental physics.
>
> EW I think it's quite possible that new observations either in astronomy or accelerators will turn up new and more down to earth challenges. But with what we have now and also with my own personal inclinations it's hard to avoid answering new terms of cosmic challenges. I actually believe that string slash M theory is on the right track toward a more deeper explanation. But at a very fundamental level it's not well understood. And I'm not even confident that we have a good concept of what sort of thing is missing or where to find it.

If you theory is not well understood, you don't even know what sort of thing is missing, and a multiverse is being invoked to explain away why it can't be tested, the situation seems clear: you have a failed theory. Yes, failure may be personally upsetting to you, but, that's science.

> GF There's a famous book about night thoughts of a quantum physics. are there night thoughts of a string theorists is where you have a wonderful theory list developing you know unable to test it. Does that ever bother you.
>
> EW Of course it bothers us but we have to live with our existential condition. But let's backtrack 34 years. So in the early 80s there were a lot of hints that something important was happening in string theory but once Green and Schwarz discovered the anomaly cancellation and it became possible to make models of elementary particle physics unified with gravity. From then I thought the direction was clear. But some senior physicists rejected it completely on the grounds that it would supposedly be untestable. Or even have cracked it would be too hard to understand. My view at the time was that when we reached the energies of the W, Z and the Higgs particle we'd get all kinds of fantastic new clues.
>
> EW So. I found it very very surprising that any colleagues would be so convinced that you wouldn't be able to get important clues that would shed light on the validity of a fundamental new theory that might in fact be valid. Now if you analyze that 34 years later I'm tempted to say we were both a little bit wrong. So the scale of clues that I thought would materialize from accelerators has not come. In fact the most important clue possibly is that we've confirmed the standard model without getting what we fully expected would come with him. And as I told you earlier that might be a clue concerning the landscape. I think the flaw in the thinking of the critics though is that while it's a shame that the period of incredible turmoil and constant experiment and discovery that existed until roughly when I started graduate school hasn't continued. I think that the progress which has been made in physics since 1984 is much greater than it would have been if the naysayers had been heeded and string theory hadn't been done in that period.

"34 years later I'm tempted to say we were both a little bit wrong"??? No, others had good arguments and were right about this (string theory is untestable and has nothing to do with LHC-scale physics), and you had bad arguments and were quite wrong. That this clear result is not being acknowledged and is having no effect on faith in string theory is disturbing.

Update: For another interview with an influential theorist, Sean Carroll has an interview with Leonard Susskind. I don't think this is a good thing, but Susskind has been very influential in blazing the path that Witten now seems headed down (invoke the multiverse to justify giving up on unifying particle physics, hope very general "it from qubit" considerations will explain gravity). The interview explains in detail Susskind's point of view.

Update: Farmelo has another interview with a string theorist up, this time it's Michael Green. When asked if he's troubled by string theory not being experimentally testable, Green says (19:20):

> I don't think at the moment there's anything directly to test, because we don't know what its predictions are.

and says that string theory should really be called "string (not yet a) theory". Earlier (16:40), he explains

> The ingredients of something are there, but it's clearly not formulated in the right language, and because it's not formulated in the right language, we don't really know how to even make sense of its predictions. It doesn't have any really genuine rigorously derived predictions yet.

Green has been working on string theory for forty years, an entire professional lifetime during which string theory has gone from a relatively simple "(not yet a) theory", with a true theory seeming not far away, to a much more complicated "(not yet a) theory", with no progress towards an actual theory in sight. Farmelo doesn't ask the obvious question of why people shouldn't interpret this story straightforwardly as the story of a failed speculative idea that never worked out.

Comments

1. **Schrodinger's Rat**
   May 2, 2019

   Rather bizarre that after learning calculus at 11 nobody bothered to give him any interesting math for years. If his parents were both illiterate farm workers, then I could understand their not appreciating his talent. But his father (Louis Witten) was a theoretical physicist who'd done a post-doc at Princeton. He must surely have known just a little bit of post-calculus math he could have shared at the dinner table.

2. **Richard**
   May 3, 2019

   > … if your child is an off-the-scale genius, learning Calculus at age 11 …

   I think your exposure to US undergraduates (ie to the products of miserable US high school education) may have coloured your perceptions a little here.

   To me this seems a little precocious (in particular a little more precocious than my never-to-amount-to-anything I was), but nothing truly extraordinary.

3. **kim8**
   May 3, 2019

   Anecdotally my understanding is being a child prodigy is not a good indication if one wishes for their success as an adult (what with the added pressure, etc.), although of course there are always unexpected exceptions.

4. **Olly Johnson**
   May 3, 2019

   Please tell me that the "lesson for all parents" is a joke that I'm missing?

   I tried to explain why at https://twitter.com/BristOliver/status/1124192117125939201

5. **Anonymous**
   May 3, 2019

   Hi Peter,

   I don't think that your speculations about what Witten might have done in his teens had he stayed on the math/physics path are any kind of justification for your "lesson to parents". Who knows what might have happened had he been pushed hard? Your instincts may be different, but mine tell me that pushing kids hard like that is not the way to success, especially not in creative fields. And expecting groundbreaking work from a 16-year old, no matter how talented, seems unrealistic.

   With respect,
   Anonymous

6. **Richard Gaylord**
   May 3, 2019

   bee hossenfelder just posted a transcript of the interview

   http://backreaction.blogspot.com/2019/05/graham-farmelos-interview-of-edward.html

7. **Shaltut**
   May 3, 2019

   That plot in the third paragraph 🙂

   I am not surprised that Witten won't leave string theory. For in this religion, he is the high priest.

8. **Matt Grayson**
   May 3, 2019

   Peter,

Pushing a genius is great for everybody else, but destroys the genius as a human being.

Producing Mozart was child abuse. I know more than a few music prodigies, and their personalities are largely frozen at the age that they were forced to practice 100% of their waking hours. Only the ones so extremely talented that they could achieve world recognition without sacrificing their childhoods have anything like a normal life. I'd put Witten in that last category.

9. **Peter Woit**
May 3, 2019

All,
The suggestion to parents was tongue-in-cheek. I felt the need to add some humor to this since I found Witten's announcement he has reconciled himself to multiverse pseudo-science profoundly depressing. Imagining a better parallel universe was the only way to cheer myself up. Witten is a very special case, not a good argument for what to do with other children.

10. **Matt Grayson**
May 3, 2019

BTW, when I was 11, I went to the town library, picked up Hocking and Young's Topology book, saw the Alexander Horned Sphere, screamed, and ran away. (Their illustration is far the creepiest picture in the literature.)

Apologies. I'm expecting a "Please no more about childhood" directive any second now…

11. **Lars**
May 3, 2019

As far as I can see, even in Witten's "special" case, his parents made an excellent decision NOT to push him into math and physics at age 11.

Witten seems not to have "suffered" too much (if at all) from his wanderings.

He may not have received a Nobel Prize (yet), but he did get the Fields medal and perhaps not coincidentally, Witten also seems to be a very content, socially well adapted human being .

And I suspect that like his own parents, he is probably also a very good parent — wisely disregarding popular songs like " Momma's , don't let your babies grow up to be string theorists."

Ha ha ha.

12. **Peter Woit**
May 3, 2019

Matt and others,
Yes, enough about 11 year olds.

13. **Amitabh Lath**
May 3, 2019

Alternative histories are fun. There is an experimental version centered on your own august institution. In 1968 the Columbia-BNL experiment was looking at dimuon production mapping out the recently theorized Drell-Yan spectrum. They saw a "feature" at 3 GeV (you can google "Lederman shoulder 3 GeV") . It wouldn't go away no matter how hard they polished the data.

https://www.mediatheque.lindau-nobel.org/research-profile/laureate-lederman

I heard this story from Leon Lederman himself. They considered exploring this 3 GeV anomaly in more detail but it didn't pan out. All the PIs went on to do other less interesting things.
At this point in the story Leon pauses:
"If we had followed up we would have scooped the parton discovery and the charmonium discovery. We could have called it the Lee-on".
(Pauses for effect)
"After T.D. Lee, of course."

A lesson for all scientists: if your data is showing interesting features, get a better spectrometer. Push the lab directorate, the funding agencies, skip other experiments. Do whatever it takes.

In this alternate history the top quark and higgs are discovered at ISABELLE in Long Island.

14. **David Appell**
May 3, 2019

When Witten is discussing the cosmic landscape, he says "…the possibly unfortunate implications for our distant descendants tens of billions of years from now."

What does he mean by this?

15. **student**
May 3, 2019

from the transcript

EW [00:17:48] But you've never been tempted down the other route. The other options are not.

EW [00:17:52] I'm not even sure what you would mean by other routes.

GF [00:17:54] Loop quantum gravity?

EW [00:17:56] Those are just words. There aren't any other routes.

Peter,

I’ve always wondered what Witten thinks of LQG and now I know.

I’ve always found it surprising such a towering figure in physics has zero interest and curiosity in one branch of physics, LQG.

Do you have any idea why he’s so dogmatic on strings as the only way to QG?

16. **Peter Woit**
May 3, 2019

David Appell,

I’m guessing he’s referring to the fact that the vacua of the string landscape are just metastable, so could in principle tunnel to a different lower energy vacuum.

17. **Peter Woit**
May 3, 2019

student,
I do think Witten has paid attention to LQG, just believes that it has problems that are not likely to get solved. What’s unclear is why he sees this as so different than the situation with string theory, which also has long-standing problems that have not gotten solved. That he’s more optimistic about the prospects for his own creation is not really surprising.

What I do find really surprising is what seems to me like a change in tone towards more dogmatism about string theory. Witten in the past has always been pretty cautious in what he says, and I would have expected the negative LHC results to make him even more cautious about making claims about string theory being the only way to go. Instead the change in his tone seems to be in the exact opposite direction. Odd.

18. **Milkshake**
May 3, 2019

I think “faith in string theory” is a bit unfair. He doesn’t have faith in the sense that a religious person has faith. Instead, it seems, there have been so many positive developments in string theory, and the theory is so rich, that he may use induction to conclude that it is on the right track.

19. **Peter Woit**
May 3, 2019

Milkshake,
There’s no fully objective way to measure “positive” vs. “negative” developments in string theory (I’d argue that the negative now far outweighs the positive, Witten undoubtedly sees things differently). Similarly, where one person sees a “rich” structure, another may see a “mess”.

Witten is a genius and surely has many valid technical things he can point to, but I think his evaluation of the state of string theory is highly colored by his emotional and intellectual investment in and commitment to a certain vision he came under the spell of in 1984. His reaction to the null LHC results and the landscape shows that it will be extremely hard to sway him from his belief in that vision. “Faith” isn’t an inappropriate word to describe what is going on.

There is an historical analog of this situation: Witten’s IAS predecessor Einstein, who until the end of his life remained convinced that the way forward to a unified theory lay in a classical geometric framework extending GR, rather than through quantum theory and quantum field theory. I think Einstein was wrong, misled by faith in a vision he first came to in his mid-thirties, and Witten’s situation is not completely different.

20. **A**
May 3, 2019

“What I do find really surprising is what seems to me like a change in tone towards more dogmatism about string theory.”

Not really surprising. It’s called the backfire effect and shows that not even brilliant geniuses are immune to cognitive bias.

21. **Low Math, Meekly Interacting**
May 4, 2019

One thing I found odd: Witten moved on from QFT because the problems he was working on (non-perturbative QCD and related things, I guess) were “intractable”.

The Landscape, with its effectively infinite vacua and lacking any sort of selection principle to narrow down the options to something that looks like our corner of the multiverse, seems rather intractable. Same goes for gravity in the transplanckian regime, about which string/M theory has apparently nothing to say because, again, the methods needed are non-perturbative.

At least while banging ones head against the QCD wall one can compare fruitless attempts with experiment. I’m not sure what the appeal of working fruitlessly on something that can’t be compared with observation might be. “Beauty”?

22. **Peter Woit**
May 4, 2019

LMMI,
Witten has always kept changing what he actually works on, looking for places he can make real progress. I don’t think he’s actually worked on the Landscape, or ever will, since that can’t go anywhere.

An accurate way to characterize the current situation I fear is that Witten and most leading string theorists have simply given up on unification. But instead of admitting that string theory was a dead end for this, they have decided to argue that the multiverse solves the problem so they don’t need to think about it any more. From the beginning David Gross argued that this was the big danger: the multiverse would get used as an excuse for giving up (at Strings 2003 he was quoting Churchill as “Never, never, never, never, never give up.”) I had thought Witten had a similar point of view to this, but that seems to be no longer true, as he has moved from finding “giving up” upsetting to making his peace with it.

23. **Anonyrat**
May 4, 2019

So, what is Witten working on these days? Yes, I can look at arxiv — but I can’t fathom the significance or “bigger picture”.

Thanks in advance!

24. **Peter Woit**
May 4, 2019

Anonyrat,
I have no idea if Witten now has some "bigger picture" he is pursuing. From the Farmelo and other interviews, it seems that he's interested in various currently popular ideas about how to get quantum gravity and spacetime as emergent out of some sort of non-gravity quantum theory ("it from qubit"). Much of this is now being pursued at the level of trying to solve low-dimensional toy models. For the absolute latest from him, tomorrow he'll be talking at the Yau birthday conference in Cambridge (I was hoping to get up there this weekend, but recovering from a cold, stayed home). His title and abstract is at
http://www.math.harvard.edu/conferences/calabi19/poster/program.pdf
and says

Title: "Unorientable Two-Manifolds, Super Riemann Surfaces, And Random Matrices"
Abstract: Recently, P.Saad, S. Shenker, and D. Stanford showed that what is arguably the simplest model of quantum gravity, which is the Jackiw-Teitelboim (JT) model in two spacetime dimensions, can be understood as a random matrix theory. This result depends on the facts that JT gravity computes volumes of moduli spaces,and on the fact that those volumes have a random matrix interpretation. In this talk (reporting on work with D. Stanford), I will explain how to extend these results to the case that an ordinary Riemann surface is replaced by an unorientable two-manifold and/or a super Riemann surface.

25. **Justin**
May 4, 2019

Hi Woit,

I understand that you were joking about pushing children to do physics. But, were you joking when you said that Witten would have discovered the electroweak theory and asymptotic freedom at the age of 16?

26. **Peter Woit**
May 5, 2019

Justin,
The stories of Witten's ability to learn and absorb very difficult material unusually quickly are many. If he had started studying higher level math and physics seriously at age 12 (1963), by the time of the Weinberg-Salam model (1967), it's possible he would have understood the ideas behind the model, much less likely he would have been in the right place to do something with them. However, for asymptotic freedom, which wasn't discovered until 1973 (ignoring 't Hooft's unpublished calculation, I'm counting discovery as including understanding the significance for QCD of the result), he would have ten years to get this done before Gross/Wilczek/Politzer. That he could have discovered asymptotic freedom before age 22 seems not implausible at all. Actually, if he had headed directly to graduate school at Princeton after his 1971 undergrad degree, it seems quite possible he rather than Wilczek would have been the one doing the asymptotic freedom work.

27. **Blake Stacey**
May 6, 2019

> Looking to the near future, he's most optimistic about the "It from Qubit" business. Maybe he's right and something will come of this, but I've seen no indication of a path to a unified theory in this direction (how do you get the Standard Model? Or has he just completely given up on that?).

My sense of the "It from Qubit" crowd is that they want to get spacetime, or at least some kind of processed spacetime-like product, out of quantum entanglement. The priority is not so much to get the Standard Model from more basic principles, but to find a setting that can include both it and gravity.

Now, I haven't so far been blown away by their writings on this. To me, it sounds plausible that they'll get a geometric way of talking about many-body quantum states, but I'm far less persuaded that one can really go in the reverse direction. In addition, the features cited in their models don't really seem to do more than scratch the surface of what quantum mechanics has to offer, being features that can be emulated classically (per the Gottesman–Knill theorem) and/or found in theories that have an underlying layer of local hidden variables (like the Spekkens toy model). "It from Barely Nonclassical Bits" just doesn't have a revolutionary ring to it.

28. **Brett Smolenski**
May 6, 2019

A rather unpleasant question, but one worth considering here: what paths should one take if our universe truly was described by a particular choice of compactification parameters, yet no way to determine what that choice should be? Now Witten and other string theorist believe, or have faith, in at least the first premise of this question, so perhaps the paths they are choosing to take are just their depressing answers to an inherently depressing question. I hope the premise of this question is false, but the universe does not seem to care much for my hopes.

29. **Peter Woit**
May 6, 2019

Brett Smolenski,
It's of course logically possible that all parameters of the SM are enviromental, determined by some unknown pre-big bang physics in an essentially random way. If we knew this to be true, of course we should give up thinking about the origin of the SM and do something else, and I guess that's what Witten and others think they are doing.

The problem is that we have absolutely no evidence at all for this. There is zero evidence for unification via a string theory compactification. If you're going to do science, you are supposed to have scientific evidence for your claims, and there is none here.

For another related interview, Sean Carroll has posted an interview with Leonard Susskind here

https://www.preposterousuniverse.com/podcast/2019/05/06/episode-45-leonard-susskind-on-quantum-information-quantum-gravity-and-holography/

where Susskind responds to a question about this not by claiming any positive evidence for it, but by just saying no one else has a better answer:

"My answer is always, "Yeah, what do you have that's better?"

This method of argument works universally: for any unsolved problem you can argue that it can't be solved (and, if you have tried and failed, you may strongly believe this). But if you want want your "it can't be solved" argument to be "better" than "it can be solved, we just don't know how yet", you have to have some evidence for your argument, and there isn't any here.

30. **Marko**

May 8, 2019

"Yeah, what do you have that's better?"

But there indeed *are* better, or at least equally promising, approaches. For example, higher gauge theory looks promising to revive the idea of unification of everything (gravity included), based on higher category structures instead of Lie groups (my recent draft shows how to construct the SM and gravity using 3-groups). With a little more research and a clever choice of a 3-group, this framework could potentially be able to reduce the number of free parameters in the SM. There are also other alternatives.

So string theory is really not the only game in town anymore, even for unification of matter, let alone gravity. But the vast majority of people just listens to Witten and Susskind, and ignores everyone else. That's the problem.

🙂
Marko

31. **Peter Woit**
May 8, 2019

Marko,

I've never understood the argument "we've devoted 35 years and the efforts of thousands of people to see if our speculative idea works, and by now we understand in detail why it can't work. So, our speculative idea is better than your much more poorly understood speculative idea, which seems to also have some problems."

Or, more briefly: "yes, our idea doesn't work, but at least we understand in detail why it doesn't work, so that makes it better."

32. **shantanu**
May 8, 2019

I would like to see a debate between Witten and some LQG person on LQG. Incidentally some of his own colleagues at Princeton such as Frans Pretorius are working on LQG.

33. **Peter Woit**
May 8, 2019

shantanu,
I think there is zero chance of ever seeing that. Back at the height of arguments over LQG vs. string theory in 2006-7 Witten showed little evidence of willingness to engage in that argument, even less now.

34. **IM**
May 8, 2019

Dear shantanu,
I believe that the only time Witten indirectly criticized some of the ideas underlying the research program of LQG in public was his paper "A Note On The Chern-Simons And Kodama Wavefunctions," see https://arxiv.org/abs/gr-qc/0306083
To understand the relevance of that paper to LQG and its response (by Smolin and others) you can read the Wikipedia page: https://en.wikipedia.org/wiki/Kodama_state
Interesting to note that in Witten's gr-qc/0306083 he never mentions the name "l.q.g." but only cites Smolin's hep-th/0209079
Regards,
–

35. **Michael Harney**
May 9, 2019

I can't believe Ed Witten has bought into the landscape nonsense. This is one of the brightest physicists in the last 30 years accepting unfalsifiable predictions. Truly bizzare.

36. **tulpoeid**
May 10, 2019

(@ Peter and Justin,)

"That he could have discovered asymptotic freedom before age 22 seems not implausible at all. Actually, if he had headed directly to graduate school at Princeton after his 1971 undergrad degree, it seems quite possible he rather than Wilczek would have been the one doing the asymptotic freedom work."

When I read this in the original post I took it as part of a great joke. Apologies, but I still do. Mathematical genius is one thing and good sense is another; if a lack of the latter is demonstrated so rigorously by an individual, then s/he cannot be expected to hands down discern study topics that will lead to concrete advancement.

37. **Peter Woit**
May 10, 2019

tulpoeid,

If you look at the history of the discovery of asymptotic freedom, it was done independently (although there are questions about leakage of info via Sidney Coleman) at Harvard by one graduate student (David Politzer) and at Princeton by David Gross working with his student Frank Wilczek. Witten quickly ended up working with David Gross at Princeton after he arrived there. If he had arrived a couple years earlier he still likely would have started working with Gross, and it's not at all implausible that he would have been the one Gross set on working on the beta function calculation, not Wilczek. Knowing Witten, it's also not at all implausible that he would have completed the calculation and understood its significance faster than the Wilczek/Gross pair did. Similarly, if he had gone to Harvard, he quite possibly would have worked with Coleman, who would have sent him down the same path Politzer started down, with the same result, possibly quicker.

Witten has great mathematical talent, but also great talents as a physicist, and it is the physics that more strongly motivates him than the math. Over the years his greatest work has often brought new mathematics to bear on physics, but this more often has been in the service of solving physics problems, not mathematics problems. I think blaming the string theory debacle on theorists being too "mathematical" and lacking "physical intuition" is a huge mistake. String theory unification is a very "physical" idea, just happens to be a bad one. The mistakes in judgement that have caused generations of physicists to pursue this failed idea are not ones that have their origin in too much exposure to mathematical culture.

38. **lca**
May 12, 2019

off topic:
https://www.lemonde.fr/sciences/article/2019/05/06/les-archives-insaisissables-d-alexandre-grothendieck_5459049_1650684.html

Various

May 13, 2019
Categories: Obituaries, Uncategorized

Possibly of interest:

- Goro Shimura, one of the major figures in twentieth century number theory and arithmetic geometry passed away on May 3 in Princeton at the age of 89. Princeton has an article about his life and work here. There's another article about him here (in German). Back in 2008 Shimura published an autobiographical memoir, The Map of My Life, which I wrote about here.
- The Dutch publication de Volkskrant has an article asking if theoretical physics has lost its way. Sabine Hossenfelder and Avi Loeb are quoted on the "there's a problem side", Robbert Dijkgraaf on the "no problem here" side.
- A commenter here points out an article in le Monde about the currently unresolved question of what to do with the 100,000 or so pages of writings left by Grothendieck at the time of his death. There seems to be a consensus that someone should carry out the expensive project of having the pages cataloged and transcribed, but how to pay for this, and who should ultimately take ownership of the papers remains up in the air. Supposedly a sizable part of the documents deals with Grothendieck's speculation about physics. The article starts off with a characterization of Grothendieck's work as important in the story of the Higgs discovery, which is quite inaccurate (there is no significant relation between his work and the Higgs).
- For many years people at SLAC have used the database there to produce "Topcites" lists of the most heavily cited papers in HEP physics, giving some insight into what topics are the most popular in current HEP research. From 1997-2003 Michael Peskin wrote up some reviews of what was going on in HEP physics each year based on these lists, and has started doing so again (for 2017 and 2018). These lists and the reviews are now dominated by astrophysical and cosmological topics, with little about HEP theory. To get an idea of what the hot topics are in HEP theory these days, take a look at the list of most frequently cited papers by hep-th preprints in 2018.
- The series finale of The Big Bang Theory will air this week, on Thursday. Since I've canceled my cable TV service a while back I haven't been following the latest episodes, which evidently feature a replacement for the failure of supersymmetry, called "super-asymmetry". At some point I hope to catch up with these, and find out what happens to "super-asymmetry".
- This week the European Strategy Update for Particle Physics is holding an Open Symposium in Granada, to discuss plans for the post-LHC future (a blog posting about this from Tommaso Dorigo is here). I've written here about the difficult issues that CERN and European HEP physicists are facing. Looking at one of the first talks on future colliders, I was surprised to see muon colliders listed as a potentially viable possibility, since I thought that the technology needed for those was still far in the future.

Update: Kenneth Chang at the New York Times today has an obituary for Shimura.

Update: On the obituary front, it was announced today (5/24) that Murray Gell-Mann has passed away, at the age of 89. The New York Times has an obituary written by his biographer, George Johnson.

Comments

1. **shantanu**
   May 13, 2019

   Peter, there seems to be some mistake in the listing of top-cited papers,
   For the 2018 list, arXiv:1901.01540 is cited as a top cite, which just 4 citations.

   In a related issue I find that inspire-hep is no longer adding references to papers cited in 2nd and beyond versions of papers submitted to arXiv.

2. **Jeff Berkowitz**
   May 14, 2019

   I love these "possibly of interest" posts. Please keep them up.

3. **Grothenhiggs**
   May 14, 2019

   Was there ever a character that is a mathematician in Big Bang Theory? How was/would be the interaction of Sheldon and a pure mathematician?

4. **tommaso dorigo**
   May 14, 2019

   Hello Peter, long time no talk. Thanks for the link!

   As for muon colliders: the rationale of putting them on the table is to take the
   design of one such machine as seriously as it should. Here I summarize very succinctly the status of matters:

   – a muon collider would be a wonderful machine, enabling studies of the Higgs sector as well as searches for new physics in a cleaner environment than hadron collisions

   – muons decay in 2.2 10^-6 s in the lab rest frame, but in an accelerator they can live long enough to be accelerated, squeezed, and collided at high luminosity. This is not a heavy technical issue – we know how to get around the main hurdles.

   – there are big issues concerning the high fluxes of radiation (neutrinos!) produced around the ring, but these are also solvable

   – the main issue is how to produce them at low emittance. The conventional way to produce a muon beam is by hadron decays, but there the emittance is very large and cooling is a big issue. The alternative under study is to produce muon pairs by e+ collisions on a fixed target at threshold. There the emittance can be small, but intensity is an issue, and the target design is a bit nightmarish at the moment.

   All in all, I do believe Europe should pay attention to fostering these studies, although it is likely that a muon collider could only see the light when you and I will not be around anymore.

   Cheers,
   T.

5. **Someone who reads Dutch**

May 14, 2019

I am bemused by Dijkgraaf's response to Hössenfelder's criticism. In his first quote, he says that physicists have no need for a thought police. That's actually a rather unfair representation of Hössenfelder's point, especially since she's explicitly disavowing it later on in the article (of course this might only have been after having been informed of Dijkgraaf's objection).

6. **Peter Woit**
May 14, 2019

shantanu,
I think that's just a paper Peskin is referring to as a very recent observational result, not as a top-cite.

Grothenhiggs,
I don't recall mathematicians ever making any sort of appearance on Big Bang Theory, unfortunately indicative of the point of view of most physicists, that whatever mathematicians might be doing, it has nothing to do with physics.

Tommaso,
Thanks for the summary!

Someone who reads Dutch,
I've also run into the phenomenon of people responding to a serious scientific argument that an idea doesn't work with "thought police" accusations. In this case, Dijkgraaf, as IAS director, has a lot of influence on what research gets supported and what doesn't. Accusing someone in Hossenfelder's position of being a heavy-handed ideological enforcer is kind of comical.

7. **TME**
May 14, 2019

Peter,

"Accusing someone in Hossenfelder's position of being a heavy-handed ideological enforcer is kind of comical."

That might be a bit harsh, but don't you agree that an outsider (in terms of understanding of string theory and nonperturbative QFT developments) like Hossenfelder trying to give arguments as to what research should and shouldn't be done is at least a bad look?

8. **Peter Woit**
May 14, 2019

TME,
In my experience Hossenfelder is generally careful to aim her criticisms at things she understands well enough to evaluate. Some of her arguments are about the reward structure of the field and some of the problematic results this leads to. She is more competent than most to make these arguments. I don't see her making arguments on more technical topics unless she has a reasonably good understanding of them.

For decades now, a favorite tactic of some theorists pursuing failed research programs has been to refuse to address criticisms of such failed research programs, resorting instead to ad hominem attacks on their critics as incompetents or "thought police", or whatever. You can try shooting the messenger, but it doesn't change the facts about your failure.

9. **Anonyrat**
May 14, 2019

Avi Loeb has a sensible suggestion:

"He {Loeb} therefore proposes a kind of hippocratic oath for physicists, inspired by the oath that doctors take, in which physicists must swear that at least one idea they are working on can still be confirmed experimentally during their lifetime. 'If I succeed, I would like to call them physicists again. Until then I think their ideas say nothing about reality. " (Via Google translate)

10. **Peter Woit**
May 14, 2019

Anonyrat,
I actually strongly disagree with that. Some theoretical physics is "phenomenological", closely tied to experiment, and that should be evaluated in terms of its relation to experimental results. But other areas of theoretical physics are not closely tied to experiment. For instance, there is a huge amount we still do not understand about quantum field theory. Theorists who want to spend their careers trying to develop new techniques leading to a better understanding of quantum field theory should be able to do so, even if there's no particular reason to believe that success would lead to some experimental test.

The argument that you want people to be able to do pure research that advances understanding, not just research with near-term practical consequences, is clear to most scientists, and extends to the issue of experimental confirmation. The primary goal is deeper understanding. If such deeper understanding leads to new experimental tests and confirmation, great, but that's not the most important thing.

The real problem is how you evaluate the results of such attempts to get deeper understanding, in the absence of clear experimental tests. The problem with string theory unification is not primarily the lack of experimental confirmation. It's a failed idea that does not lead to models that convincingly explain anything that we don't understand about the SM.

I don't think you should stop people from thinking about speculative ideas. But, you need some way to get them to give up when such ideas aren't working and try something else, and there won't always be experimental results that can provide decisive answers to what works and what doesn't.

11. **Someone who reads Dutch**
May 14, 2019

On the bright side, I think it's a safe bet to say that the debate won't go away anytime soon. If Hossenfelder (my apologies to her for misspelling her name earlier) and Loeb, despite going against the mainstream and not being personally involved in string theory research, have enough traction that mainstream media (even if it's only Dutch mainstream media) are reporting on their claims, and eliciting a response from one of the most prominent faces of theoretical physics in the world, I think that in itself is an enormous victory, and I want to congratulate Hossenfelder and Loeb on that.

My impression of Dijkgraaf in the article is that he's mainly putting out tired clichés. I mean by comparing the 40 years in the desert of string theory to Einstein's years of building up towards the theory of General Relativity. And I think Dijkgraaf himself surely realizes that the analogy is not beyond serious criticism, to put it very mildly. The main problem with this defense seems to be to me that he could use it no matter what. It almost seems like an abuse of Einstein's prestige to use it to defend a potentially pointless enterprise.

I think philosophy of science must come to grips with questions like: How can we actually distinguish between "a genuine quest for new ideas" on the one hand, and "the inmates running the asylum" on the other? I think we are already seeing that Popper's falsification criterion is just not cutting enough ice with enough people, because many folks are rightly pointing out that a successful paradigm shift is always born of the realm of heuristics and intuitive guesses.

However, this last argument does strike a false note with me, not so much because it is untrue but because it does not apply. If we would have asked Einstein what he had been up to between the years of 1907 and 1916, he wouldn't have said that he was aimlessly exploring new ideas in order to bring about a paradigm shift. From very early on, Einstein had a quite definite vision in mind of what the theory of General Relativity should accomplish (starting with the "luckiest thought of his life", or however he called it), and also he knew approximately what kind of mathematical tools should be involved in the shaping of that theory. So in that sense, string theorists are *not* doing what Einstein did, because they do not have the same sense of aim and direction that Einstein had (and it is amply documented that he did have it). At least to my understanding, all that string theory has to offer are some partial inroads into the Great Unknown. Quite a big difference. It actually seems a falsification of history to compare string theorists with Einstein.

12. **Someone who reads Dutch**
May 14, 2019

Of course, my statement that "philosophy of science must come to grips with questions like […]" should not be construed as meaning that there should be a Thought Police making sure philosophers of science are working on this question…

13. **Peter Woit**
May 14, 2019

Someone who reads Dutch,

An interesting question for those who compare themselves to the Einstein of 1907-1916 is whether the evidence isn't more on the side of their efforts being comparable to those of the Einstein of 1922-1955, who devoted decades to failed ideas about how to unify electromagnetism and GR. This question should be especially poignant for the director of the IAS, the institution that hosted Einstein's failed efforts.

From what I know of the history, if you look at Einstein's work from 1907-1916 you see the gradual successful working out of an early vision, whereas if you look at 1922-1955 you see an increasing accumulation of evidence for why the initial vision couldn't work. It's the latter story which looks comparable to the string theory unification story (which in a way, is about the same thing: a failed geometrical idea of how QED and GR fit together).

Of course one major difference is that Einstein never attracted a significant number of followers for his failed program, whereas string theory involved thousands and still has some sort of dominant role in many parts of the theory community. Arguably there's nothing wrong with encouraging a single talented researcher to pursue their vision even long past the point it seems to others to not be working out. But you don't want the whole community doing this.

14. **jsm**
May 14, 2019

One comment on Shimura that may be relevant to the current debates in physics. Throughout his career Shimura followed a very independent path — no one seems to know who his doctoral advisor was. The elliptic modular curves (quotients of the complex upper half plane by a congruence subgroup of SL(2,**Z**)) are initially defined over the complex numbers, but they are known to have canonical models over number fields. For the analysts, this is true because of the properties of the Fourier expansions at the cusps, and for the algebraic geometers, it is true because they can recognize the curves as a moduli varieties. There are similar complex curves attached to every quaternion algebra over **Q** split by **R**, but they have no cusps and are not moduli varieties, and so everyone knew that they did not have canonical models … until Shimura proved they did. This was perhaps his most original contribution, and was not something that someone too deeply embedded in the culture of the day could have done. Today the curves are named after him.

15. **Anonyrat**
May 14, 2019

Peter, I agree with what you wrote; though we would presumably value "improved understanding of QFT" higher than "improved understanding of string theory" because QFT has a well-demonstrated relevance to the physics of the universe, while string theory does not. I take it that Avi Loeb is trying to find a test to keep members of the physics department from spending their entire career on topics that are fiction though written in mathematics.

16. **workerpleb**
May 15, 2019

> There seems to be a consensus that someone should carry out the expensive project of having the pages cataloged and transcribed, but how to pay for this, and who should ultimately take ownership of the papers remains up in the air.

Scan the pages. Upload to the internet. The enthusiast nerds will sort the pages by themselves.
I am being serious.

17. **Peter Woit**
May 15, 2019

workerpleb,

I don't think it's so easy. Montpellier has conserved, organized, scanned and put on line, see
https://grothendieck.umontpellier.fr/
18000 pages of his earlier documents (another 10000 are not online, for legal reasons including the fact that letters can't be made public without permission of the writer). Even with all this careful work, many of these documents are not yet in a very usable form. In particular, a lot are hand-written, with deciphering Grothendieck's handwriting a skill most people won't want to spend the time to acquire. They really should be transcribed into TeX to be useful, but it looks to me like this would require a sizable group of French-speaking experts in algebraic geometry with a lot of patience and a lot of time on their hands.

18. **cedric bardot**
May 16, 2019

From his reading of Grothendieck text "La clé des songes" Connes – in this video with Serres: https://youtu.be/pOv-ygSynRI?list=PLwl60Z8ihqF7C4GNQUJCxNPFXPqfN087g&t=2513 – emits roughly the hypothesis that Grothendieck ' father dreamed of writing but never did so the 70000 pages written & archived by Grothendieck at his home might be his tribute to some extent.

Connes doesn't doubt one could develop a software to decipher the handwriting (thanks to IA ? ;-).
I guess a citizen science project like the one imagined to decode Darwin's Handwriting would be more difficult to implement due to the manifold of subjects (from abstract math to potentially paranoïd thoughts?) & legal issues.

About a possibly not so far-fetched connection between particle physics & Grothendieck concept of motives :

> "C'est une idée qui a émergé lors de discussions entre physiciens et mathématiciens à l'IHES, à l'heure du thé", se rappelle Francis Brown, professeur à l'Université d'Oxford [et à l'IHES (http://www.ihes.fr/~brown/)]. Le défi était de calculer comment les particules se désintègrent lors d'une collision sans se lancer dans les calculs d'intégrales utilisés classiquement, souvent très complexes. "Et c'est là que le miracle intervient: nous pensons que derrière ces intégrales se cachent des motifs qui permettraient de les calculer beaucoup plus rapidement! s'enthousiasme Francis Brown . Aucun physicien n'aurait pu s'en apercevoir. Il a fallu que Grothendieck passe par là!"

(https://www.science-et-vie.com/science-et-culture/grothendieck-les-archives-fantasmatiques-41922).
cf Mathilde Marcolli too: https://arxiv.org/abs/0907.0321

19. **jsm**
May 16, 2019

There may be gems in the 18,000 pages (or 118,000 pages) of Grothendieck's notes, but much of it seemed to me (when I looked at it) to be the sort of scrap paper most of us accumulate when working on a problem, and that not even we can understand five years later.

20. **Y-**
May 20, 2019

Historians seem to develop tools for transcribing handwritten texts, e.g. https://transkribus.eu/Transkribus/

21. **David Yager**
May 22, 2019

No narrative about SUSY2019? We're half way through it. The timetable at the event site shows a lot of presentations, but I haven't looked at it enough yet to see how much actual SUSY content is there.

It's the right time to start putting out predictions of what the muon g-2 delta is actually going to be, and what new particle explains it.

22. **Peter Woit**
May 24, 2019

David Yager,

I took a look, and there are at least two things about this conference that strike me as peculiar:

1. Gordon Kane as summary speaker. Why choose as summary speaker the person in your field with least credibility? What prediction for the gluino mass will he announce today?

2. Pre-SUSY 2019. The instructional talks for grad students and postdocs gave little indication of the problems with SUSY models. Given the LHC null results, and the huge industry of several generations of theorists trained to do SUSY calculations in models that have failed, why train a new generation in the same techniques?

23. **rpo**
May 24, 2019

On the subject of the mortality of great figures, Caltech has just sent an email to staff announcing the death of Murray Gell-Mann.

24. **Armin**
June 2, 2019

Peter,

Did you ever meet Gell-Mann? If so, do you have any personal anecdotes?

25. **Peter Woit**
June 2, 2019

Armin,
No I never met Gell-Mann. Stephen Wolfram wrote an interesting piece about his experiences with Gell-Mann, see here
https://blog.stephenwolfram.com/2019/05/remembering-murray-gell-mann-1929-2019-inventor-of-quarks/

26. **Chris Oakley**
June 3, 2019

The Wolfram article is interesting reading. I believe that Gell-Mann was not the only eminent scientist not to want to endorse *A New Kind of Science*. The article, as well as pieces written by others, give the impression that Feynman was the showman and Gell-Mann the quiet one. Maybe, but the one time I saw Gell-Mann speak he was very clear and very entertaining.

The Universe Speaks in Numbers

May 24, 2019
Categories: Book Reviews

Graham Farmelo's new book The Universe Speaks in Numbers has recently been published in the UK, US publication is next week. The topic of the book is one close to my heart, the relationship of mathematics and physics. I'm very much in agreement with the main argument of the book, which is that our most fundamental theories about physics have turned out to naturally be expressed simply and beautifully in terms of deep ideas about mathematics. This surprising congruence between the deepest ideas in two seemingly different fields strongly indicates that they share an unexpected unity. As fundamental physics reaches technological limits on the experimental side, investigating the underlying mathematical structures may be the best and only route open to further progress.

Much of the book is historical (and often Anglo-centric), beginning with Newton, a figure who made huge tightly intertwined advances in both mathematics and physics. Looking at Newton's career, it makes no sense to characterize him as a mathematician or a physicist, he's both in equal measure and at the same time. Farmelo then moves on to Maxwell, who also revolutionized physics while at the same time introducing important new mathematics into the subject. He tells the story of Maxwell's 1870 talk "On the relations of mathematics to physics", then a couple chapters later it's Dirac's 1938 talk "The Relation between Mathematics and Physics".

In Farmelo's account:

> Dirac quickly arrived at what was, in effect, a manifesto for research into theoretical physics. He proposed a new principle – the principle of mathematical beauty – which says that researchers should always strive to maximize the beauty of the mathematical structures that underpin their theories of the natural world…
> He concluded that "big domains of pure mathematics will have to be brought in to deal with the advances in fundamental physics"…
> Eventually the two subjects might possibly become unified, Dirac suggested, with "every branch of pure mathematics then having its physical applications, its importance in physics being proportional to its interest in mathematics."

A major reason for Dirac taking this sort of view was the example of Einstein's work on general relativity, which to reach fruition had required Einstein to become expert in the newly developed and rather challenging abstract mathematical machinery of Riemannian geometry. The two great pillars of modern physics, relativity and quantum theory, are deeply related to central modern ideas about mathematics, in particular, respectively, geometry and representation theory. While in the case of relativity the mathematics came first, for the case of quantum theory, the mathematics underlying the subject was mostly developed later.

Farmelo goes on to explain that relations between math and physics entered a fallow period during the 1950s and 1960s, but the advent of gauge theory and the Standard model led to a productive renewal of healthy relations during the 1970s. He does a good job of explaining how this came about, discussing in particular the central role played by Witten. Farmelo has benefited from getting to talk to Witten himself in some depth about this, and gives a nuanced portrayal of Witten's rather complex and evolving feelings about the relations of the two subjects and the role that he and his immense talents have played in this story.

There's a great deal of the usual sort about the history of string theory, emphasizing its points of contact with new mathematics. The next to last chapter is about Nima Arkani-Hamed and the amplituhedron story, portrayed as the latest exciting development on the math-physics front. Farmelo is clearly enthralled by Arkani-Hamed and his intense enthusiasms. The evolution of Arkani-Hamed from phenomenologist to mathematical physicist is definitely a fascinating thing to observe, and I've often written about it here (you might want to for instance read this posting). Farmelo also points to an excellent lecture by Greg Moore on *Physical Mathematics and the Future* (discussed here), which I think is based on a much deeper understanding of the current state of the math/physics relationship, and gives a much broader perspective than the narrow one of the amplituhedron. By the way, I see on Moore's website that he's writing up for the 2019 TASI school what appears to be an excellent set of notes about Chern-Simons theory and related topics.

A major problem though with this book is that it pretty much completely avoids the big problem raised by the program of pursuing progress in fundamental physics through beautiful mathematics: how do you know whether people doing this are on the right track or headed down a blind alley? Farmelo starts the book off with a very odd preliminary chapter comparing Einstein's work at the IAS in his later years to that of his modern day successors:

> was seeking a new theory, not in response to puzzling experimental discoveries, but as an intellectual exercise – using only his imagination, underpinned by mathematics. Although this approach was unpopular among his peers, he was pioneering a method similar to what some of his most distinguished successors are now using successfully at the frontiers of research.

The fact of the matter is that, in retrospect, Einstein's work of this era was a huge failure, as he got stuck deep down a blind alley. He was seduced by a specific speculative idea about how to get unification out of mathematics, by using simple extensions of the differential geometry that he had such success with in the case of GR.

How does Farmelo know that string theory enthusiasts following Einstein haven't run into the same problem he did? In essence, Farmelo just assures us that he has talked to them and they tell him that, like Einstein before them, they think they're on the right track. The existence of skeptics is mentioned, but their writings are carefully excluded from the 200+ item bibliography. Jim Baggott, Sabine Hossenfelder and I (and our writings) appear only in a short footnote on page 6, with bloggers described as complaining that "modern physics should get back on the straight and narrow path of real science". But the three of us are complaining about, not "modern physics", but one small subset of it, and at least in my case, the path I argue for is almost exactly what Farmelo is arguing for: absent help from experiment, pursue the path advocated by Dirac.

Here's part of Farmelo's summation of the current situation in the book's last chapter:

> The great majority of today's leading theoretical physicists are, however, confident that they are motoring steadily in the right vehicle, despite the problems they are having in trying to drive it.
>
> In the public domain, the debate about the merits of the string framework has been raging for years, especially in print and online. Some of these onslaughts are useful correctives to the hype lavished on this programme and to the superciliousness of pronouncements made by some string (although rarely by the best ones in my experience). Experts on the string framework have every reason to be proud of the progress they have made, but until such time as experiments confirm its validity, there is no room for smugness. Yet I am often troubled by the dismissiveness of some of the critical commentators, especially those who write with a confidence that belies the evident slightness of their understanding of the subject they are attacking. Opposing the view taken by leading theoreticians might be interpreted as a healthy disrespect for orthodoxy. However it may be part of the worrisomely common view that anyone can have a valid opinion on any subject, regardless of their technical knowledge and appreciation of it. In scientific matter this trend is especially regrettable.

The first part of this I think is simply not true, with most "leading theoretical physicists" these days unsure what the right direction is for how to get beyond the Standard Model. As for the last bit, I'll just say that I think it can accurately be described as "sleazy" (by the way, Farmelo at one point came to see me in New York when he was doing research for the book, and we had a quite pleasant conversation, he's rather charming). Besides the ad hominem attack on unidentified critics, there's nothing anywhere in the book about the actual problems of the vehicle some people are motoring in. For instance, the problem of the landscape and the multiverse is dealt with by just ignoring it, it's not mentioned at all. If it had been mentioned, Farmelo might have had to deal with the fact that it's mathematically hideous, so a direction which should be abandoned by his own arguments.

In the end, my feelings about this book are much the same as in the case of Farmelo's biography of Dirac (see here): a wonderful book in many ways, but marred by a bizarre degree of string theory fanboyism. While there's a lot to like about this book, and much of it makes a good case for a controversial point of view that I strongly agree with, unfortunately the problems with it are even more serious than in the case of the Dirac biography.

The IAS is having a [public event next week](), convening a panel to discuss the math-physics issues brought up by the book. I may add something here about this after it happens. Perhaps someone in attendance can get a show of hands from the assembled leading theorists to see if they really feel that they're steadily motoring in the right vehicle or not.

Update: I'm listening to a live-streaming version of the IAS event here. After a talk by Farmelo, mainly about history, and a discussion with Karen Uhlenbeck and Freeman Dyson (moderated by Natalie Wolchover), Greg Moore is now giving a talk (available here) on TQFT and gauge theory which is quite good. I'm very much in sympathy with his take on "physical mathematics".

Update: Besides an advertisement by Nima Arkani-Hamed and Thomas Lam for their work on amplitudes, the only discussion of the current state of the math/fundamental physics interface was the final short conversation between Dijkgraaf and Witten. Looking back at expectations from 30 years ago, Witten said he had expected progress on understanding the laws and principles behind string theory, but that has not occurred. Dijkgraaf tried to end with a defense of string theory as "well, by AdS/CFT it's connected to QFT, so it somehow is connected to the real world", to which Witten said something like "we shouldn't give up hope for string theory as a unified theory, we might yet find the right string theory vacuum". In the end Witten said that he found it hard to accept the possibility that the unexpected things discovered through string theory didn't mean that it had to do with a unified theory, but admitted he didn't have a scientific justification for this.

Update: There's a review of the book by Tony Mann at Times Higher Education. It includes:

> Farmelo's book is a response to these contrarians . He is confident that the beauty of the mathematics is significant in indicating that we are on the right track, and that eventually, even if we have to wait for many years, we will be able to test string theory against new experimental evidence. As a spirited defence of the idea that beautiful mathematics should be a guide for physicists, Farmelo's book is a timely response to critics such as Woit and Hossenfelder, defending what science writer Jim Baggott has called "fairy-tale physics". Ultimately I am not sure, however, that he makes his case anything more than a matter of faith.

I think Mann gets it right that the Farmelo book is intended as a defense of string theory against the Baggott/Hossenfelder/Smolin/Woit critique (with the odd feature that he refuses to allow mention of our books in the bibliography, or to engage in any way with the arguments we make), and that his argument comes down to little more than "trust certain famous theorists", especially those at the IAS. As usual, I'm inaccurately portrayed as opposed to the idea that progress can be made by following beautiful mathematics. The problem with string theory unification is that it's a failed physical idea, with the failure indicated not just by lack of predictivity, but also by the fact that string theory models compatible with observations are horrendously ugly.

Update: The Dijkgraaf-Witten conversation is available here. Some extracts from near the end.

> *Witten*: I'm actually personally reasonably confident that what we're doing is a lasting contribution. But I'm less confident that we'll really be able to put it on a completely solid footing. It depends on being lucky with experiment, I would say…
>
> *Dijkgraaf*: Often I hear questions about whether is string theory wrong or right, but I often answer there's no way in which we'll ever get rid of string theory, because in some sense it's an integral part of the theories that we already are using.
>
> *Witten*: That's true but not completely satisfying. Let's not give up on the dream of finding the vacuum that describes the real world…
>
> The honest answer is that personally as I told you before I have confidence that the general enterprise is on the right track, but I don't claim that the argument I've given is scientifically convincing.

Witten makes clear that at this point he has no likely forseeable experimental results relevant to string theory he can point to, no "scientifically convincing" argument that it's on the right track, just a feeling that since string theory research has turned up various points of contact with important math and physics, there must be something right about the idea.

Update: Siobhan Roberts at the New York Times has a piece about the centenary of the eclipse that made Einstein famous that also discusses Wednesday's IAS event.

Comments

1. **Sabine Hossenfelder**
   May 25, 2019

   There is a lot of mathematics. Some of it beautiful, some of it not so much. Some of it describes our world, some of it does not. How do we find the math that describes our world?

   That's the relevant question that anyone should ask who is interested in progress in the foundations of physics.

   Relying on beauty has not worked in the past and it's not presently working, so clearly this is not what physicists should do.

   Likewise, working on a theory just because a lot of other people work on a theory, is also not a good reason to think that this theory tells you something about the real world.

   And without these two arguments, what is left of string theory? Not much. It's not that it's entirely uninteresting, of course. It's just that the resources that have gone into it and continue to go into it vastly exceed the reasonable. And throwing further money at it just encourages people to produce more useless papers, like on the swampland and such. And don't get me started on folks who seem to think we live in Anti de Sitter space.

   Seriously, Peter, I ask you. How can anyone in their right mind still think that this has something to do with describing reality? Why are these people still receiving research grants?

2. **Koenraad Van Spaendonck**
   May 25, 2019

   S.H. said :
   "How do we find the math that describes our world?

   Here's an interesting read, from Lee Smolin, on the subject, as he tracks Einstein's modus operandi :

   https://arxiv.org/abs/1512.07551

   The introduction says :

   " There is a myth that Einstein's discovery of general relativity was due to his following beautiful mathematics to discover new insights about nature. I argue that this is an incorrect reading of the history and that what Einstein did was to follow physical insights which arose from asking that the story we tell of how nature works be coherent.

“

3. **Adrian**
May 25, 2019

Dear Peter, I read that the commenter Sabine Hossenfelder has written
”And don’t get me started on folks who seem to think we live in Anti de Sitter space.”

As a working string theorist, active in conferences and paper-wise, I have a good grasp about what the community of people working on various aspects of holography and extensions of the conjecture made by Maldacena in 1997, actually believe or think.

No one of my colleagues believes that ”they live on AdS space”. Actually, all of them know we do not live in Anti de Sitter. These colleagues use Anti de Sitter and a theory of gravity defined on it to calculate observable quantities (correlation functions) of conformal field theories. By studying deviations from Anti de Sitter, these colleagues learn about quantum field theories that confine, spontaneously break symmetries, etc. They have developed set-ups in which various other non-perturbative effects are actually calculated.
Other part of the community have made important progress on the dynamics of black holes.

I believe that not recognising and not understanding these basic points leads to very misguided comments (I believed Sabine Hossenfelder understood these things) . Not appreciating this particular enterprise is, I believe, very narrow-minded.

Thanks

4. **Peter Woit**
May 25, 2019

Adrian,
I don’t think there’s really any incompatibility between what you and Sabine Hossenfelder have to say. Her “folks who seem to think we live in AdS” comment is clearly aimed at those who (like Farmelo) defend string theory as a theory of the real world by pointing to AdS/CFT. To the extent she offers a critique of the kind of research you describe, it’s not that no one should be doing it, but that too many people are doing it, and I think she is right about that. It’s not healthy for the field to be so dominated by one particular rather narrow line of research for the past twenty years.

5. **Eli Rabett**
May 25, 2019

IEHO the connection driving force is not the beauty of the connection between math and physics but the terseness of it. Short makes sweet.

6. **Paolo**
May 25, 2019

Did Einstein “know” about the existence of Yang-Mills theory (in 1954) ?

7. **Peter Woit**
May 25, 2019

Paolo,
I doubt it. Yang did talk about Yang-Mills at the IAS in February 1954, a year before Einstein’s death, but I believe at this point Einstein would not have been paying attention to what particle theorists were doing (Yang was trying to get a theory of the strong interactions).

8. **Justin**
May 25, 2019

Exactly what mathematical structures are to be investigated in lieu of experimental limitations, and who is supposed to be investigating these structures, the mathematicians, physicists, or both? Newton was able to be a mathematician and physicist of equal measure at the same time, not because he possessed superpowers, but because in the 1660’s the amount of material in the two subjects could be learned quicker than today. I don’t think physicists today would know enough about math to investigate the structures, and I don’t think mathematicians would know enough physics to know what structures to investigate.

9. **Sabine Hossenfelder**
May 26, 2019

Adrian,

My sentence was an attempt at humor, trying to express that while there is a large community studying fields in AdS, almost no one in this community works on explaining what this has to do with the real world. I know that a few exceptions exist, no need to tell me things I already know. But the vast majority of people in the area do calculations in AdS just because it’s simpler and they know how to do it, not because they actually know of any argument why it is useful to describe reality.

If I ask them why they work on this, they tell me they hope to learn something from it about dS. If you ask them a little more, they will admit that they have no reason to think the results transfer to dS. And don’t get me started again on the claim that it should work because both spaces are locally flat. It makes zero sense because all the benefits from working in AdS are non-local. And, needless to say, the world isn’t supersymmetric and not conformal and N isn’t infinity. These are mathematical fictions, not descriptions of anything in the real world.

This is not how science should work. And the reason why this is happening is obvious: They do it because they can, because they continue to get funding for it. It’s a waste of time and of money and it has to stop.

The reason I am phrasing this so bluntly is that I see a lot of hype in the media about AdS/CFT (sadly including otherwise reliable outlets like Physics Today and Nature) and they tend to not give the reader a clear impression about the promise of this research. It’s 99.99% hot air. It’s been pursued since 20 years and pretty much nothing has come out of it. (Except possibly for explaining why you do *not* need it in heavy ion collisions.) And this is the supposedly “greatest breakthrough” in the foundations of physics since the 1970s.

10. **Rolf**
May 26, 2019

@Sabine
isn’t this Anti de Sitter space related work by you:
https://journals.aps.org/prd/abstract/10.1103/PhysRevD.91.124064

and this a notice about ongoing (since 2016) public funding you receive for this kind of work:
http://gepris.dfg.de/gepris/projekt/286641560 ?

Would you deny for others
What you demand for yourself?
(U2, Crumbs from your table)

11. **From Bejaia**
May 26, 2019

@ Peter,
I find your tendency in this blog to praise any book (or any passage from a book like Farmelo's) that exposes the deep connections between math and physics as a right track to move forward in unraveling the mysteries of nature really puzzling. I say this because from what I understood in Sabine's book "Lost in Math" is that, not only relying on mathematical beauty did not lead to any new discovery about nature since the 70's, but also what you call the "deep connection between math and physics" has not unraveled a single new principle about nature. It just created maybe new ways of dealing with problems in math or (at best) helped express old known facts in physics in a more abstract way.
@ Sabine,
I don't understand what you mean by "Except possibly for explaining why you do *not* need it in heavy ion collisions."

Thank you!

12. **Peter Gerdes**
May 26, 2019

Why is it at all surprising that physics is best expressed in terms of math? Not only is it not surprising it's necessarily true since math is just the *name* we apply to any kind of precise well-defined manipulation of concepts.

Or to put it another way can you even imagine a way in which the world could have been in which physics wasn't best described via math? I mean imagine in some alternate universe we came up with some completely different set of rules/theories for predicting things. Whatever kind of manipulation we used to make predictions in that universe would also be called math. Sure, one could imagine that we couldn't make any predictions about the world at all but in that case there wouldn't be such a thing as physics. At a minimum, if we are capable of making any useful predictions about the world and our mental processes can be emulated by a Turing machine we could mathematically describe the process we used to make predictions and quantify it's accuracy.

One can't even argue it's surprising that in many cases the math needed for physics was already developed. In a huge number of cases it isn't true (and an area of math is only explored after it becomes apparent it is useful in physics). In the cases where it is true often the area of mathematics was already inspired by earlier physical theories and mathematicians explored a huge space of generalizations/modifications one of which happened to be useful later.

Indeed, for us to discover a physical theory SOMEONE has to think of the concepts it uses (without someone thinking up non-euclidean geometry first we wouldn't have GR). Since mathematicians are also smart it would be shocking if physicists were always the first people to come up with those generalizations.

13. **Adrian**
May 26, 2019

Dear Sabine, dear Peter,
a very brief message given the conversation started above.

In this message, I am referring the work on AdS/CFT and many of its variations. I see what a majority of string theorist are doing as a way of learning about Quantum Field Theories in general (different dimensions, different amount of global symmetries). I very much fail to see this as a not-worthy enterprise. Also, I fail to see the hype in the media about this research (but may be this is due to the fact that I do not follow the media).

I find the criticism useful. But when Sabine writes about the research on AdS/CFT:
"It's 99.99% hot air. It's been pursued since 20 years and pretty much nothing has come out of it. "

I feel that she does not know and did not read 99-per cent of the works in this area to write this comment. I am unable to read all the papers on AdS/CFT, but on those I read and check calculations, my opinion is exactly the opposite of 'hot air'.

I refrain to believe that the great majority of my colleagues are working on this just because they get the funding to do so. I very much refrain to believe that the daily discussion and calculations with colleagues are just a 'charade' and that we should know that is 99.99 per cent hot air.

I find hard to believe that most of the people around me are idiots or perverse minds who are not interested in science, but only on their next grant. My empirical observations of colleagues indicate exactly the opposite!

On Sabine's paragraph

" It makes zero sense because all the benefits from working in AdS are non-local. And, needless to say, the world isn't supersymmetric and not conformal and N isn't infinity. These are mathematical fictions, not descriptions of anything in the real world."

Our viewpoints on this cannot be more divergent. I believe that she is just wrong. I prefer not to comment as it would make this posting too long.

14. **Peter Woit**
May 26, 2019

Justin,
There's no reason physicists can't learn math or mathematician's can't learn physics. Einstein (with some help) learned Riemannian geometry. Witten not only learned a lot of mathematics, he created very significant new mathematics. There is a whole field of "mathematical physicists" who work in between the two subjects.

From Bejaia,
Yes, investigations of the relation of math and physics since the 1970s have not led to significant advance in fundamental physics theory, but neither has anything else. Your choices at this point are to give up or pursue whatever you think is the most promising path, in full knowledge that that path has not recently been successful.

I don't see the failures of string theory, supersymmetry, etc. as due to people mistakenly pursuing mathematical beauty. After initial ideas with some beauty, those subjects quickly degenerated into a complicated and ugly mess (endpoint the landscape) as one tried to reconcile them with known physics. If people actually had been pursuing mathematical beauty, this would have caused them to abandon these subjects long ago.

Peter Gerdes,

What's surprising is not that fundamental physics is expressed in terms of math, but that its deepest ideas are closely related to the deepest ideas in mathematics. For example:
GR and Geometry
The Dirac equation
Connections, curvature and Yang-Mills theory
The relations between quantum theory and representation theory (see my recent book)

15. **Justin**
May 26, 2019

@Peter,

I understand that Witten created marvelous new mathematical structures, among his many achievements, his construction of topological quantum field theories stands out as his most influential. But, my understanding is that TQFT hasn't really had much of an impact on fundamental physics and is mostly studied by mathematicians. That's exactly my point, one doesn't know which mathematical structures to study that would lead to significant advances on the fundamental physics front.

16. **Peter Woit**
May 26, 2019

Justin,
Yes, there is no clear path saying "study this mathematical structure, you'll find the right idea for how to get a better theory than the SM +GR". There is also no other clear path to such a better theory, so this may be more promising than any alternatives.

17. **Sabine Hossenfelder**
May 27, 2019

Adrian,

Of course you do not want to believe what I say and neither want your colleagues. But no need to believe me. This is science after all. Look at the papers that have been written and list what observations were correctly predicted. The answer is: None. That's what has come out of it.

You don't have to dig deep for hype, here are some recent examples:

https://phys.org/news/2019-04-wormholes.html

https://www.quantamagazine.org/how-space-and-time-could-be-a-quantum-error-correcting-code-20190103/

https://www.express.co.uk/news/science/1072080/universe-hologram-holograph-science-news-leonard-susskind

"I believe that she is just wrong."

Hahaha. No shit.

Look, that's not how it works. Science isn't about beliefs. There is no reason to think that the limit Lambda to zero is continuous. In fact there are all kinds of reasons to think it is not, starting with the very fact that you do calculations in AdS because you need Lambda to be smaller than zero.

I did not say that it's not a worthy enterprise. Please do not put words into my mouth I didn't use.

Rolf,

Yes, I know what I am talking about. Glad to see you figured that.

18. **vmarko**
May 27, 2019

"" It makes zero sense because all the benefits from working in AdS are non-local. And, needless to say, the world isn't supersymmetric and not conformal and N isn't infinity. These are mathematical fictions, not descriptions of anything in the real world."

Our viewpoints on this cannot be more divergent. I believe that she is just wrong. I prefer not to comment as it would make this posting too long."

I find it hard to believe that this is not a tongue-in-cheek comment.

🙂
Marko

19. **Peter Woit**
May 28, 2019

All,
Maybe that's enough about AdS/CFT. It's not even emphasized that much in Farmelo's book, where his take on the current state of things is more Arkani-Hamed-centric, with the Amplituhedron the centerpiece of the best case for a current math/physics topic on the right track to success.

20. **Bernard Grossman**
May 29, 2019

Is the talk at IAS over ?

21. **Peter Woit**
May 29, 2019

Event is still going on, just now (4:25) coming back from break. Will go on until 5:45.

22. **Matt Foster**
May 30, 2019

@ Justin:
"I understand that Witten created marvelous new mathematical structures, among his many achievements, his construction of topological quantum field theories stands out as his most influential. But, my understanding is that TQFT hasn't really had much of an impact on fundamental physics and is mostly studied by mathematicians."

Actually TQFT and its "holographic dual" 2D CFT have been enormously influential and productive in condensed matter physics, where these frameworks are used to describe actual experiments such as the fractional quantum Hall effect (FQHE). TQFT was used by Shou-Cheng Zhang and collaborators to provide the "composite boson theory" of the FQHE, a simple mean-field framework that allows one to "derive" key results like Laughlin's many-body wave function. More recently (mid-90s onwards), much of Alexei Kitaev's work on spin liquids and topological quantum computation is rooted in CFT representation theory. This is now driving large-scale experimental efforts funded by Microsoft (Station Q at UCSB and Copenhagen).

Suffice to say we cm physicists do not believe that "fundamental physics" can be done only in the service of unification! 🙂

23. **DB**
May 30, 2019

Here is the Dijkgraaf/Witten conversation:
https://www.youtube.com/watch?v=RjthuCDzAnY

Is Edward acknowledging from 12:00 onwards that the AdS/CFT correspondence, and its relation to emergent spacetime, is (or might be) wrong?
It´s hard to say what word he uses around 12:33 ("It colours? a lot of our thinking…)

Thanks for any help provided.

24. **Peter Shor**
May 30, 2019

Regarding your comment that the book's defense of string theory is "trust certain famous theorists", I think the "trust famous theorists" attitude is one of the big problems with HEP today.

For a concrete example, Lenny Susskind is writing papers that use quantum information theory in what seem to me more and more nonsensical ways, and (a) there are some people paying attention to him and building on these papers, and (b) very few or no high energy physicists are saying that these papers can't possibly be correct.

25. **clayton**
May 30, 2019

Hi Peter Shor, I'm curious what makes you think Susskind's recent papers "can't possibly be correct." They seem so vague to me that they can't be right, but I'm interested to know what is concrete enough in them to be wrong.

26. **Peter Woit**
May 30, 2019

DB,
Thanks. I added a link to the conversation, some extracts and some comments. As for the part you point to, I can't tell what Witten's final "it might be wrong" is supposed to refer to. Unlikely he means the Maldacena conjecture, perhaps something more general about current expectations concerning "emergent space-time".

Peter Shor,

I agree that the "trust certain famous theorists" argument is highly problematic. For me, one of the main reasons I got interested in science and math was that it was not about trusting authority figures. If you were willing to put in the effort, you could learn a subject and follow the arguments, and once you did this you could evaluate for yourself the evidence for a claim.

My problem with much of the claims by Susskind of recent years is more that I can't even figure out what he is claiming, a rather extreme version of the "not even wrong" phenomenon.

Nima Arkani-Hamed is highly influential, with much of this book channeling his (current) point of view. However, if you look back at his career, you find equal enthusiasm for ideas that didn't work out (e.g. large extra dimensions), and he describes himself as a not-so-reliable "serial ideologue", here
http://www.math.columbia.edu/~woit/wordpress/?p=6476

Unlike Susskind and Arkani-Hamed, Witten is someone who tries to be careful and reliable in what he claims. To me, the conversation with Dijkgraaf indicates that, while he's still unwilling to give up on the dream of 1984, he's now all too aware that things have not gone well for it, that he has no "scientifically convincing" argument for his hopes, nor any prospects for getting one anytime soon.

27. **DB**
May 30, 2019

Peter,
thanks for the link and your comments.

After listening again to what he says, I agree with you that he´s most probably referring to ideas about emergent spacetime.
I tend to think the same as him there, that spacetime is emergent.
And I have my own idea of what is behind it, but that´s not for this post.
He´s very careful with his words, but it seems to me that he doesn´t like any of the ideas that are nowadays floating around about that issue.

28. **Peter Shor**
May 30, 2019

Clayton:

I am extremely skeptical of ER=EPR. Susskind says

"ER=EPR tells us that the immensely complicated network of entangled subsystems that comprises the universe is also an immensely complicated (and technically complex) network of Einstein-Rosen bridges."

I don't see how you can say that entanglement is the same thing as ER bridges unless you mean something completely different by either "entanglement" or "ER bridges" than Schrödinger meant, or Einstein and Rosen meant. Entanglement is a property of quantum mechanics, ER bridges are a property of general relativity, and as far as we know quantum field theory works fine without general relativity, and vice versa.

And Google Scholar says that this paper has 745 citations, which is two thirds as many as the AMPS (*Black Holes: Complementarity or Firewalls*) paper, which actually contains real, potentially correct, ideas.

29. **WTW**
May 31, 2019

Peter,
With all due respect to Dirac, it was von Neumann who put Quantum Mechanics on a sound mathematical basis. Dirac, among others, were the ones trying to make sense of the physics, and found mathematical formalisms that could help — as distinct from the essentially infinite sample of such formalisms that don't. Von Neumann pointed out logical inconsistencies in what Dirac proposed, despite it being more "beautiful" and "elegant" than what von Neumann proposed in its place that actually made mathematical sense. See, e.g., https://plato.stanford.edu/entries/qt-nvd/. (And Dirac's later obsession with numerology should serve as a warning about losing that perspective.) Never mind that the mathematically rigorous version requires infinite dimensional Hilbert spaces to make it work — not quite what one would consider a "physical" concept. Is that "mathematically beautiful"? Or does it just work, so we sweep the awkwardness under the rug?

As mentioned previously, Einstein first had physical intuition about what was happening; he then had to find a way to describe that in a consistent way that could be measured — which then helped to elucidate further implications of those insights, as well as advancing mathematics (e.g., differential geometry). Not the other way around.

Your own recent book seems like a slightly lesser version of what Sabine Hossenfelder and even you are critiquing. It seemed to me a good, if rather abstract, explanation of "Here's what they mean when they say 'XYZ'", from a mathematician's point of view, but only tangentially related to any underlying physics. (Reminds me somewhat of the book General Relativity for Mathematicians by Sachs and Wu. : -) As with most of modern fundamental theoretical physics — even some of what is described in low-dimensionality solid state physics — my response is essentially "That's interesting, in an abstract conceptual sense, but … So What?" In terms used by actual practitioners of Computer Science (i.e., programmers), much of what is described appears to be a kludge on top of a kludge on top of a kludge — but at least you get (approximately) the right answers. Unfortunately, that seems to apply more broadly.

The mathematics on which most of physics is based evolved as a way to describe things that can be expressed as differentiable (analytic) functions over smooth manifolds compatible with Euclidean space (locally if not globally) — but even there the mental gymnastics required to describe that in a self-consistent way seems far divorced from physical reality. Just the issue of differentiating with respect to time in a GR (not just Minkowski) spacetime, when time itself is a dynamical variable "in the real world", is just glossed over. And then the question: is it continuous, to the level we need to actually describe underlying fundamental physics? How does any of today's mathematics handle it if it's not?

Likewise, almost nothing that we are currently observing in astrophysics can be expressed concretely using any analytical formalism — only arbitrary, custom designed computational models calculated using Markov approaches, from which one selects "closest fits" based on semi-physical parameters and estimations — and often "matched" to data by machine learning algorithms. (A troubling corollary: the people who found significance in Bode's law – the supposed magical spacing of (then-known) planets in our solar system that turned out to be nonsense.) So I have trouble understanding your faith that some deep "mathematical mysticism" will lead to any better understanding of physics than we already have (or don't).

30. **Daniel**
May 31, 2019

Peter,
I also find the idea of "ER = EPR" to be a bit far fetching. But I think the idea is that classical spacetime is emergent from an underlying entangled state (of some unknown entity). Not sure how they could make it work but it is probably not true that entanglement is wormhole as entanglement could be between degrees of freedom of a single particle, and wormhole is between two different spatial locations.

31. **Peter Woit**
May 31, 2019

Peter Shor/Daniel,
It's very unclear to me what "ER=EPR" actually is supposed to mean, other than that it can't mean what it says since, as Peter Shor points out, it claims an equality of two things that in our currently understood best theories (QM and GR), have nothing at all to do with each other. As near as I can figure out, what it really is is a slogan for a speculative hope: "let's find a quantum theory of gravity in which these two things are related", part of the general program of "let's find a quantum theory of gravity in which space-time emerges from quantum entanglement".

All attempts on my part to understand what the current state of this program is have left me with the impression that, while this has been an increasingly hot topic among "leading theorists" for a decade now, it hasn't managed to get much beyond its original motivation based on some crude aspects of what happens in AdS/CFT. The main activity seems to be the study of toy models, with this research leading to the realization that the toy models don't actually in any simple way embody the hoped for properties. Some of the people involved in this have a history of misleading the public by promoting outrageous hype about string theory, and the current public promotion of things like "ER=EPR" is starting to look like more of the same.

Under the circumstances, to me it seems best to try and ignore all this for now, and wait and see if anything solid emerges from the cloud of "emergent gravity" hype.

32. **Peter Woit**
May 31, 2019

WTW,
You're missing the point I keep trying to make about the role of mathematics in new ideas about fundamental physics. This has nothing to do with mathematical rigor (other than the general point that you really should try and be clear about exactly what you are talking about, see above comment about ER=EPR: the problem there is not lack of rigor, but lack of any clear meaning at all).

Let me give three examples of what I am talking about:

1. Einstein's use of Riemannian geometry to formulate GR.
2. Dirac's definition of quantization as taking Poisson brackets to commutators, i.e. as a Lie algebra representation.
3. Dirac's introduction of Clifford algebras and spinors, allowing the construction of the Dirac operator and a square root of the Laplacian.

All three of these were huge advances in our understanding of fundamental physics, based upon the introduction of new and surprising deep mathematical ideas, not "physical intuition". I think the expectation that further such advances are possible and will have the same nature is quite rational. In particular, the mathematics behind parts 2. and 3. is still not completely understood (e.g. the role of the Dirac operator in representation theory), and a deeper understanding may very well lead to a deeper understanding of fundamental physics.

33. **Peter Woit**
May 31, 2019

I see that Urs Schreiber thinks it's a good idea to put up an edited version of what I write here on Twitter, see
https://twitter.com/SchreiberUrs/status/1134462156395749376
Note how he edits out the rest of the sentence he highlights, the part where I write:
"and wait and see if anything solid emerges from the cloud of "emergent gravity" hype."

By the way, to bring this back to the topic of the post. One reason for skepticism about "ER=EPR" and such things is the lack of any significant deep mathematical idea behind it. This line of research seems to be orthogonal to the deep mathematical ideas about symmetry that have been so successful in leading to the standard model, and instead often seems to be based on rather simple minded mathematics.

34. **Bernhard**
May 31, 2019

Peter,

What do you think of Dijkgraaf's comment: "…because in some sense it's an integral part of the theories that we already are using." ?

Is this the old story of "string theory is a quantum theory" sort of thing? Or what do you think he's alluding to?

I know of exactly zero theories we use in HEP to describe nature that string theory is a part of.

35. **Peter Woit**
May 31, 2019

Bernhard,
This is the same argument David Gross and others have been using for years when challenged about the failure of string theory unification. It's basically

"The AdS/CFT conjecture suggests that the superstring on AdS5 x S^5 is dual to N=4 super Yang-Mills on S^4. So, in this case, a string theory is an alternative formulation of a specific QFT, and one can find other examples of this kind of QFT/string theory duality. So, string theory is closely related to (some) QFTs, and so you can't argue that only QFT is relevant, not string theory."

One problem with this argument is that the relation to QFT is not to Standard Model QFTs known to be fundamental, but to QFTs with rather different properties (i.e. conformal invariance, no asymptotic freedom). Hopes that this could be extended to get a string theory dual of QCD have not worked out. There seems zero reason to believe in a string theory dual of the weakly-coupled parts of the SM (this would have to be some strongly-coupled string theory we have no evidence for or theory of).

The other problem is that, as Witten immediately recognizes and notes in his response, what people are doing when they are making this argument is implicitly giving up on string theory unification. When I and others argue to string theorists that string theory unification is a failure and they respond "string theory has some connection to QCD", they are acknowledging they have no positive argument for string theory unification and are trying to talk about something else. Witten responds to Dijkgraaf that he doesn't want to do this and implicitly give up on string theory unification, just turning the string theory research program into one focused on hope for a better calculational method to deal with certain strongly-coupled quantum systems.

36. **Blake Stacey**
May 31, 2019

The original qualitative arguments for "ER=EPR" (back in arXiv:1306.0533) left me completely bewildered. They would equally well imply that a wormhole exists between the two halves of a Werner system, or between any two correlated toy bits in the Spekkens model, which is absurd. Now, there can certainly be a big gap in solidity between the original motivations and later results, but here, I feel like we're still waiting to see what those "later results" actually are.

37. **Peter Woit**
May 31, 2019

Blake Stacey/Peter Shor,

Urs Schreiber explains here

https://twitter.com/SchreiberUrs/status/1134503200550203392

that ER=EPR is a "Zen koan", or maybe a "koan-like slogan", and Susskind's paper about it is "uninhibited vague brainstorming".

Will Kinney comments here
https://twitter.com/WKCosmo/status/1134504860429275139

38. **cedric bardot**
June 2, 2019

Referring to a folk theorem stating there's one subject in math you always avoid that turns out to be the relevant one for your next paper, Robbert Dijkgraaf asks E. Witten:

> "Is there any subject in math you are consciously avoiding ?".
> "Well I've decided, I've judged there are some things that are too abstract for me to try to learn them… I'm more worried not being able to understand the things that aren't too abstract."

I naively conclude from this revealing interview that thinking abstractly (what could be the proper geometric framework to define quantum Yang-Mills-Higgs theory in 4D space-time for instance) might be the quality of mathematicians while computing formally (entropy of "entangling surfaces" to probe a "bulk geometry" in the AdS/CFT conjecture context) is mostly the privilege of physicists.

39. **JimV**
June 2, 2019

Re:
“1.Einstein’s use of Riemannian geometry to formulate GR.
…
All three of these were huge advances in our understanding of fundamental physics, based upon the introduction of new and surprising deep mathematical ideas, not “physical intuition”.”

Einstein’s Zurich Notebook* seems to indicate that the basic idea of objects following a geodesic on a curved surface had a lot of physical intuition involved–some of it based on previous classical physics results, but that is how intuition is developed, from experience.

* https://www.pitt.edu/~jdnorton/Goodies/Zurich_Notebook/

40. **Peter Woit**
June 2, 2019

JimV,
Yes, it does seem Einstein’s original motivation for GR could be described as “physical intuition”. I think though that if you look at the history, it’s pretty clear that he would never have gotten off the ground, developed a real theory and found the equations of GR, without the mathematics of Riemannian geometry.

41. **luysii**
June 3, 2019

Off topic but why nothing about the death of Gell-Mann?

42. **Peter Woit**
June 3, 2019

luysii,
See previous posting.

43. **John Putz**
June 7, 2019

Hi Peter,

I’m a big fan of your blog. I also really enjoy reading physics and math books that are geared to a more general audience (although I have a PhD in physics, I don’t want to have to work too hard to read a book before bedtime). I’ve gotten some good book ideas from reading your blog in the past. I’m wondering if you’d consider putting another recommendation section on your blog for books that you’ve enjoyed (perhaps categorized by subject) – and perhaps link them to reviews you’ve done, if you have. It’d be really nice to have one place to go to find a good physics/math book when I’m in the mood for one.

Thanks, John.

44. **Peter Woit**
June 7, 2019

John,
If you select posts by category, choosing the “Book Reviews” category

http://www.math.columbia.edu/~woit/wordpress/?cat=13

gives 105 postings about books, mostly book reviews, going back to 2004. Pretty much all of these are books about physics and or math. Mostly I write about something because I think it’s interesting enough to be worth reading. You should take a look at the review first, since in a few cases there’s a negative reason I’m writing about the book, definitely not recommending it to others (for a recent example, don’t go out and get Lee McIntyre’s “The Scientific Attitude”….)

Various News Items

June 4, 2019
Categories: Uncategorized

For physicists:

- For the latest news on US HEP funding, see presentations at this recent HEPAP meeting. It is rarely publicly acknowledged by scientists, but during the Trump years funding for a lot of scientific research research has increased, often dramatically. This has been due not to Trump administration policy initiatives, but instead to the Republican party's embrace of fiscal irresponsibility whenever there's a Republican in the White House. After bitter complaints about the size of the budget deficit and demands for reduction in domestic spending during the Obama years, after Trump's election the congressional Republicans turned on a dime and every year have voted for huge across-the-board spending increases, tax decreases, and corresponding deficit increases. Each year the Trump administration produces a budget document calling for unrealistically large budget decreases which is completely ignored, with Congress passing large increases and Trump signing them into law.

  For specific numbers, see for instance page 20 of this presentation, which shows numbers for the DOE HEP budget in recent years. The pattern for FY2020 looks the same: a huge proposed decrease, and a huge likely increase (see the number for the House Mark).

  The result of all this is that far greater funds are available than expected during the last P5 planning exercise, so instead of having to make the difficult decisions P5 expected, a wider list of projects can be funded.

For mathematicians;

- Michael Harris has a new article in Quanta magazine, mentioning suggestions by two logicians that the Wiles proof of Fermat's Last Theorem should be formalized and checked by a computer. He explains why most number theorists think this sort of project is besides the point:

  > Wiles and the number theorists who refined and extended his ideas undoubtedly didn't anticipate the recent suggestions from the two logicians. But — unlike many who follow number theory at a distance — they were certainly aware that a proof like the one Wiles published is not meant to be treated as a self-contained artifact. On the contrary, Wiles' proof is the point of departure for an open-ended dialogue that is too elusive and alive to be limited by foundational constraints that are alien to the subject matter.

  I don't know who the "two logicians" Harris is referring to are, or what the nature of their concerns about the Wiles proof might be. I had thought this might have something to do with number theorist Kevin Buzzard's Xena Project, but in a comment Buzzard describes such a formalization as currently impractical, with no clear motivation.

  Taking a look at the page describing the motivation for the Xena Project, I confess to finding it unconvincing. The idea of revamping the undergraduate math curriculum to make it based on computer checkable proofs seems misguided, since I don't see at all why this is a good way to teach mathematical concepts or motivate undergraduate students. The complaints about holes in the math literature (e.g. details of the classification of finite simple groups) don't seem to me to be something that can be remedied by a computer.

- For some cutting-edge number theory, with no computers in sight, see the lecture notes from a recent workshop on geometrization of local Langlands.
- Finally, congratulations to this year's Shaw Prize winner, Michel Talagrand. Talagrand in recent years has been working on writing up a book on quantum field theory for mathematicians, and I see that Sourav Chatterjee last fall taught a course based on it, producing lecture notes available here.

  For a wonderful recent interview with Talagrand, see here.
  I first got to know Michel when he started sending me very helpful comments and corrections on my QM book when it was a work in progress. He's single-handedly responsible for a lot of significant improvements in the quality of the book.

  I've recently received significant help from someone else, Lasse Schmieding, who has sent me a very helpful list of mistakes and typos in the published version of the book. I've now fixed just about all of them. Note that the version of the book available on my website has all typos/mistakes fixed. For the published version, there's a list of errata.

Update: For more about the Michael Harris vs. Kevin Buzzard argument, see here, or plan on attending their face-off in Paris next week.

Comments

1. **Kevin Buzzard**
   June 4, 2019

   It's interesting how my formalisation project divides opinion. Some mathematicians I've talked to have been super-enthusiastic; others can't see the point at all. The same is true with the undergraduates that I've been teaching; some lap it up, others find it bewildering and/or pointless. One reason I persevere is simply that it is clearly something different. I spent 25 years as a number theory researcher and lecturer doing, in some sense, the same thing as everyone else. Maybe I just got bored.

   Another reason I persevere is that I have bought into the vision of Tom Hales' Formal Abstracts project https://formalabstracts.github.io/ , which is also different. Hales is going to need lots and lots of definitions of mathematical objects for his project to succeed. So why not train undergraduates to at least be aware that mathematics can be done this way? Commelin, Massot and I have formalised the definition of a perfectoid space in Lean, which convinces me that these languages are now ready for what Hales has planned. However your criticisms are perfectly valid and your feeling about Hales' project might be similar.

   In 15 years' time this stuff could have completely taken over mathematics. Alternatively it could just be sitting there being not very helpful and of interest only to specialists. I really do not know which way it will go — but one thing is for sure, over the last couple of years I have met smart people with very different opinions about this.

2. **Scott Morrison**
   June 4, 2019

   I've been quite involved in Lean over the last two years, and as a mathematician my motivations are quite different from the "traditional" explanations for why we ought to formalise mathematics.

   Sure, it might be nice if we could worry less about gaps in the literature, but it's not like I've been losing sleep.

   Instead, I want interactive theorem provers for the same reason I enjoy computer algebra systems — they might make us more powerful mathematicians. At the moment the state of the art is very far from this point: doing mathematics in any of the existing theorem provers (including my favourite) is *excruciating*, and even more worryingly requires some skills that most mathematicians just don't have.

Nevertheless, this doesn't feel inevitable, and in particular there is so much "low hanging fruit" in terms of prospects for making interactive theorem provers easy to use, and more powerful, that I think it is worth investing in.

I personally doubt that computers will ever do the highest-level conceptual mathematics that we're used to doing. On the other hand, I think the computers will be able to help us with a large part of the work we already do, and I can envisage a future where, when I have a rough idea of how to proceed on a problem, I can hand that idea off to a computer and ask it "Can you sort out the details? If not, can you explain to me where you run into trouble?"

If any mathematicians are curious, please come over to https://leanprover.zulipchat.com/ and say hi! There's a very friendly and enthusiastic community over there, and we're very happy to answer questions, provide help, and give suggestions for interesting first projects for exploring interactive theorem provers.

3. **Shantanu**
June 5, 2019

Peter, the link to the presentations at the HEPAP meeting is not working. could you post the correct link?
Thank

4. **Peter Woit**
June 5, 2019

Shantanu,
Fixed.

5. **Supernaut**
June 5, 2019

"…during the Trump years funding for a lot of scientific research research has increased, often dramatically" Let's acknowledge that along with the bad there is some good. I am glad you mentioned this because a lot of people in the liberal/progressive camp suffer from trump-derangement syndrome, which makes them unobjective and therefore unreliable (and would not have reported such a thing as you did).

6. **Peter Woit**
June 5, 2019

Supernaut,
I don't actually think the fiscally irresponsible approach to the US budget of Trump and the Republicans is a good thing at all, even if as a side effect some more HEP research is getting funded. There are plenty of ways the world has become a much worse place over the years that have worked out fine for me and others like me. This doesn't make them a good thing. I hear Stalin was great on the math and physics research funding front.

All: no, I don't want to host here a discussion of such larger issues, not specifically about the HEP budget news.

7. **Supernaut**
June 5, 2019

Peter – I completely agree with you.

8. **Low Math, Meekly Interacting**
June 5, 2019

Hopefully the relevance to larger issues isn't too much of a distraction: My understanding is big HEP experiments can take decades to plan for and assemble. Further progress takes serious and very long-term commitment. The way the US govt. presently operates, beyond a few months one cannot reliably project whether or not any part of it will be funded. That a certain capacity benefits in any particular budget cycle is merely a fact, effectively disconnected from any future eventuality by caprice. There's nothing to celebrate, and the current modus operandi surely can't be good for anyone trying to do basic research.

9. **Arnold Neumaier**
June 6, 2019

Scott,

I also want interactive theorem provers to help me save boring work, and hence make us more powerful mathematicians. Currently, using interactive theorem provers doesn't save any work but multiplies the work needed to write a page of latex math by a factor of 40 or so. Some years ago I had tried to get funding for a large project called FMathL, see https://www.mat.univie.ac.at/~neum/FMathL.html – aimed at doing these things in a way suitable for mathematicians (rather than computer scientists). But I didn't succeed in convincing funding agencies – the referees dismissed the project as being far too ambitious.

10. **Pete**
June 6, 2019

Actually, I think that to some extent a formalisation programme can help with gaps in the literature. If we know about the gap, then we should fill it – and here a computer won't help. But often we do not know about the gap, and often that occurs in precisely a proof like CFSG where there are many cases to check, and it is easy either to make a mistake in one of these or simply not to have an exhaustive list of cases. Here a formalisation programme will highlight the gap, then we can fill it.

Of course, right now formalisation is so time intensive that it is only going to be done for hugely important results which had better be true (like CFSG). But part of this is because we keep having to add more basics to whichever system we happen to favour; I think the idea is that when CFSG is finally formalised, it will be much easier to do other group / representation theory formally, because most of the stuff one needs will already be in the system.

11. **Neel Krishnaswami**
June 6, 2019

> The idea of revamping the undergraduate math curriculum to make it based on computer checkable proofs seems misguided, since I don't see at all why this is a good way to teach mathematical concepts or motivate undergraduate students.

I've done this a bit, and I think it's a *wonderful* idea. The fundamental pedagogical problem that computer-checked proofs solve is that they make the feedback cycle much shorter.

As it stands, in the usual approach to teaching undergraduates how to prove things, we assign them a problem set, they go off and do the work and turn it in a week or two later, and then they get marked-up work a week or two after that. This is more than enough time for them to forget all the details of how they were thinking about the problem, and so they struggle with interpreting the feedback we struggled to write.

With a proof assistant, they get told their proof is wrong, the second they make a mistake at the exact proof step they messed up. As a result, the mechanics of proof — quantifiers, case splits, hypotheses and so on — can be taught and learned vastly more easily than in the traditional way. (You can — and I did! — even make the input format to the prover a structured format which is a subset of basic mathematical English.)

The machine can't learn the concepts for the students, of course, but obviously it's much easier to understand mathematical arguments once you can read and write proofs fluently — and this is where proof assistants shine.

12. **Anon**
June 6, 2019

For an example of an interesting and recent use of a proof assistant in pure mathematics, see the preprint arXiv:1810.04579 by S. Gouëzel and V. Shchur.

13. **Past-life mathematician**
June 6, 2019

I would like to hazard the claim that, if we compare students who have learnt to construct mathematical proofs in the "old-fashioned" way to students who submit their proofs to a proof assistant, then the two groups *are not actually learning the same thing*.

And I think this should actually be more obvious than it appears to be to most people: since the notion of a proof assistant (together with the underlying (meta-)mathematical theories) "formalizes" the notion of a mathematical proof, the two notions are in fact different, the one being formal, the other not. By seeking to "formalize" a concept, one changes the concept itself.

The traditional mathematical notion of proof is an "intuitive" notion, in that one develops a "feeling" for what is and what is not a proof. One could reduce it to a sociological phenomenon ("a proof is what mathematicians accept as such"). However, even such a move serves to sidestep the question as to what a proof *is*, by instead answering the question what a certain class of people think it is. By contrast, the notion of proof in the formalized setting *is* well-defined and formalistic ("a proof is what the following Turing machine will accept as such").

I think that to equate the two concepts is to make a category mistake. To *formalize* a certain notion, whatever it is, is to implicitly admit that this notion refers to a definite concept or entity. Otherwise one would not even have a criterion for a *good* vs. a *bad* formalization. If the formalization is all there really is, any formalization is as good as any other. To put it more concretely, the relationship in which "formalized proof" stands to "proof" is the same relationship in which "painting" stands to "model".

It is a belief of mine that there is a certain tendency in human beings (but mathematicians especially) to wish to deny as much as possible the existence of the intuitive and the non-formal, and to replace intuitive concepts by formalized notions, so that they can afterwards say that the formalized notion is what we were trying say all along. But in fact there is a very big difference, and it comes out especially in teaching. Students often want (or need) to know the "point" of a certain formalization, and rightly get bored by involving themselves in purely formalistic work without any underlying motivation.

Which is not to say that I do not approve of the idea of using proof assistants in teaching mathematics. But I do see the danger of creating a generation of students who are learning to prove theorems as just another kind of game, without *really* understanding what they are doing or why they are doing it. I think there is more enough of that as it is.

14. **Kevin Buzzard**
June 7, 2019

@Past-life mathematician: in my course, the students have the *option* to formalise their solutions. I think there is definitely something in what you say, but I do not think (and indeed, I do not think you're saying) that showing them that this *option* exists is somehow cheating them, or telling them that mathematics is something which it is not. It's analogous to showing them that they have the option to write their problem sheet solutions in LaTeX, rather than pen and paper. I believe that showing them this other way of thinking about the ideas I'm teaching them is broadening their minds. I'd like to think that as a teacher, this is part of my job.

15. **Past-life mathematician**
June 9, 2019

Dear Kevin: I wasn't criticizing you or your program. I have read your statement that Peter linked to where you explain your reasons for involving yourself in this type of research, and I agree with a lot of what you write. Especially your worries about a future loss of knowledge, and the fact that there is so much "folklore knowledge" that hasn't been written down anywhere, or if it has is contained in letters of so-and-so to someone else. I've encountered my fair bit of this while in academia, and I've always found it indefensible. I mean using private letters as a bibliographical reference in journal articles, especially when the reference is invoked in a proof. It runs completely counter to what I think should be the academic spirit: self-critical, transparent, maximally thorough. I think your program can only contribute towards that ideal, and I love that you are sticking up for it.

I was mainly responding to Neel Krishnaswami's seemingly blanket endorsement of basing the curriculum on proof assistants. I think if you do tha, you really lose something essential, as I've tried to argue.

16. **chethan krishnan**
June 9, 2019

Two A_R's on the same line on page 527 of your corrected pdf, Peter. (Para right above section 48.3.)

17. **random reader**
June 10, 2019

Past-life math (and Kevin Buzzard),

I think the issue is more what the human activity of mathematics "is", than what-is-proof (which has been clear in principle since Euclid). When serious mathematical communication starts to involve executable components by default, there will be an irresistible advantage to having definitions and proofs be machine readable, like engineers exchanging parametric CAD specification instead of diagrams or blueprints.

For now, we are still in the age of mathematical communication as a form of storytelling, and in that setting (particularly teaching) the discipline of extra formality for its own sake has benefits that are mostly orthogonal to the usual purposes of the communication. Unless the pedagogical added value of that discipline turns out to be very large for some reason, I think it will remain unpopular until mathematical communication itself is permeated by add-ons (e.g., functions, graphs, diagrams, computer programs, tablebase searches, etc accompanying almost every paragraph), and then the ability to generate such "animations" automatically from formalized definitions and proofs will create the demand for baked-in formalization. It's hard to see it being necessary for teaching of today's style of mathematics.

18. **Peter Woit**
June 10, 2019

chethan krishnan,

Thanks! Fixed.

Not So Spooky Action at a Distance

June 11, 2019
Categories: Book Reviews, Quantum Mechanics

I've recently read another new popular book about quantum mechanics, Quantum Strangeness by George Greenstein. Before getting to saying something about the book, I need to get something off my chest: what's all this nonsense about Bell's theorem and supposed non-locality?

If I go to the Scholarpedia entry for Bell's theorem, I'm told that:

> Bell's theorem asserts that if certain predictions of quantum theory are correct then our world is non-local.

but I don't see this at all. As far as I can tell, for all the experiments that come up in discussions of Bell's theorem, if you do a local measurement you get a local result, and only if you do a non-local measurement can you get a non-local result. Yes, Bell's theorem tells you that if you try and replace the extremely simple quantum mechanical description of a spin 1/2 degree of freedom by a vastly more complicated and ugly description, it's going to have to be non-local. But why would you want to do that anyway?

The Greenstein book is short, the author's very personal take on the usual Bell's inequality story, which you can read about many other places in great detail. What I like about the book though is the last part, in which the author has, at 11 am on Friday, July 10, 2015, an "Epiphany". He realizes that his problem is that he had not been keeping separate two distinct things: the quantum mechanical description of a system, and the every-day description of physical objects in terms of approximate classical notions.

> "How can a thing be in two places at once?" I had asked – but buried within that question is an assumption, the assumption that a thing can be in *one* place at once. That is an example of doublethink, of importing into the world of quantum mechanics our normal conception of reality – for the location of an object is a hidden variable, a property of the object … and the new science of experimental metaphysics has taught us that hidden variables do not exist.

I think here Greenstein does an excellent job of pointing to the main source of confusion in "interpretations" of quantum mechanics. Given a simple QM system (say a fixed spin 1/2 degree of freedom, a vector in $\mathbf{C}^2$), people want to argue about the relation of the QM state of the system to measurement results which can be expressed in classical terms (does the system move one way or the other in a classical magnetic field?) . But there is no relation at all between the two things until you couple your simple QM system to another (hugely complicated) system (the measurement device + environment). You will only get non-locality if you couple to a non-local such system. The interesting discussion generated by an earlier posting left me increasingly suspicious that the mystery of how probability comes into things is much like the "mystery" of non-locality in the Bell's inequality experiment. Probability comes in because you only have a probabilistic (density matrix) description of the measurement device + environment.

For some other QM related links:

- Arnold Neumaier has posted a newer article about his "thermal interpretation" of quantum mechanics. He also has another interesting preprint, relating quantum mechanics to what he calls "coherent spaces".
- Philip Ball at Quanta magazine explains a recent experiment that demonstrates some of the subtleties that occur in the quantum mechanical description of a transition between energy eigenstates (as opposed to the unrealistic cartoon of a "quantum jump").
- There's a relatively new John Bell Institute for the Foundations of Physics. I fear though that the kinds of "foundations" of interest to the organizers seem rather orthogonal to the "foundations" that most interest me.
- If you are really sympathetic to Einstein's objections to quantum mechanics, and you have a lot of excess cash, you could bid tomorrow at Christie's for some of Einstein's letters on the topic, for instance this one.

Comments

1. **Sabine Hossenfelder**
   June 12, 2019

   That's because Bell uses a notion of "locality" that very few people with a background in GR/QFT can relate to. It's sometimes called "Bell locality" more specifically, and one can reasonably question whether it has anything to do with "locality" in the sense we're used to. You can have eternal arguments about this with philosophers but it's not particularly enlightening. It's just a definition.

2. **ay**
   June 12, 2019

   Bell proved non-locality for hidden variable theories of quantum mechanics – not in a general formulation. But many believe that it is generally true (many including Bell). This paper, in the introduction seems to me to be clearly a clearly written description:

   https://arxiv.org/pdf/quant-ph/0408105.pdf

3. **Heikki Tuuri**
   June 12, 2019

   Peter, by a "non-local measurement" do you mean that there is another observer B far away from you, and you regard his action as a quantum mechanical "measurement"?

   A way to get rid of the spooky action at a distance is to assume just one observer, A. He performs measurements with his eyes and ears. Everything else, including all other humans, is either a part of the measuring apparatus which A utilizes, or a part of the physical system under the study.

   If A and B measure the spins of a pair of entangled particles, then the only real quantum mechanical "measurements" are the events when A reads the gauge of his own spin measuring apparatus, and when A reads an email sent by B.

4. **Manfred Requardt**
   June 12, 2019

   That quantum theory is really nonlocal can best be seen by reading Bells article: The Theory of Local Beables. It is by no means a question of strange definitions or philosophical opinions.

5. **Rolf**
   June 12, 2019

   Dear Peter,

The wikipedia is basically right on this, even though “our world” is too strong.
Replace “our world” by “all theories that fulfill certain
seemingly reasonable assumptions”.

> only if you do a non-local measurement can you get a non-local result.

“non-local measurement” = measurement on a system
in a spatially entangled state at space-like distance.

“non-local result” = even though the results are random,
they are correlated. QM describes no common cause for this
correlation –> QM describes its establishment as a non-local influence.

> Yes, Bell’s theorem tells you that if you try and replace the extremely simple >quantum mechanical description of a spin 1/2
> degree of freedom by a vastly more complicated and ugly description, it’s going
> to have to be non-local. But why would you
>want to do that anyway?

To avoid the above conclusion (the –>) by providing a common cause for the correlation. Under certain seemingly reasonable assumptions the Bell theorem shows that every theory that provides a common cause for the correlations cannot reproduce QM.

6. **Robert**
June 12, 2019

In my experience (in particular from my interactions of our local Bohmians), this insistence on “non-locality” comes from a prejudice about how classical the world should be in the form of “realism”, i.e. the assumption that a system has to have a state that even specifies properties that are not only not measured but in fact cannot be measured (as they are incompatible with properties are are in fact measured). For example assuming that the x-component has some (unknown) value if in fact the z-component is measured. Violation of the Bell inequality only implies that not both locality and realism can hold in quantum theory and it is your choice which to drop. My personal choice is to maintain locality as this is the foundation of QFT (or field theory which was invented to have a local field equation rather than a non-local force law in Newtonian gravity or electro-statics) and Haag’s book “Local quantum physics” makes this point most prominently.
For this point of view applied to foundations, my recommendation is to watch the video of Sidney Colman’s colloquium “Quantum Mechanics In Your Face” or the exchange that Reinhard Werner had with the Bohmian people that is well documented on the unterwebs.

7. **Jim Baggott**
June 12, 2019

The story of Bell’s inequality has subtleties that are often overlooked in popular retelling. But the most obvious point concerns the status of the wavefunction or state vector in quantum mechanics. ‘Spooky action at a distance’ is only a problem if it is assumed that the wavefunction represents the real physical state of a real physical quantum system. In this interpretation there are things that the theory doesn’t appear to account for – the theory is incomplete. This was Einstein’s view. One way of completing the theory is to assume that the wavefunction is statistical in nature, governed by the behaviour of some underlying hidden variables. A certain choice of variables in principle allows all quantum events to be local, leading to Bell’s inequality. Another choice is crypto-non local, leading to Leggett’s inequality. What these inequalities show is that no local or crypto-non local theory can accurately predict all the results of regular quantum mechanics. And experiment is pretty unequivocal – these hidden variable theories can’t be right.

If you want to press on and insist the wavefunction is real, then you face a choice between unpalatable evils, such as de Broglie-Bohm theory (in which spooky action at a distance is accepted as part of the representation), ad hoc physical collapse mechanisms (such as GRW), consciousness-causes-collapse mechanisms (Von Neumann and Wigner), and the many worlds interpretation.

Of course, you could instead assume that the wavefunction does not represent the real physical state, but rather codes for our knowledge of the system based on experience. Then all the bizarre, spooky stuff goes away, and there is no non-locality. But by making this trade-off, we lose any ability to understand what’s *really* going on at the quantum level. Like emergency services personnel at the scene of a tragic accident, such anti-realist interpretations advise us to ‘move along’, because there’s ‘nothing to see here’.

I used to favour Einstein’s realism. But the experiments ruling out especially crypto-non local hidden variable theories caused me to re-think. Like the great philosopher Han Solo, I’ve got a very bad feeling about this.

8. **Marko**
June 12, 2019

Peter,

“for all the experiments that come up in discussions of Bell’s theorem, if you do a local measurement you get a local result, and only if you do a non-local measurement can you get a non-local result.”

There are usually two main reasons people are bothered by Bell’s theorem. The first is that it excludes “local realism”, so if you want to keep locality you have to give up realism, which some people have trouble doing. The second (and IMO more important) is that the violation of Bell’s inequalities requires one to give up the metaphysical idea of reductionism — studying the parts of a physical system does not tell you everything there is to know about it, or in other words, the whole is more than a sum of its parts. Apparently people have trouble giving up reductionism as much as realism, so they are all “baffled” by nonlocality.

Bee,

“one can reasonably question whether it has anything to do with “locality” in the sense we’re used to.”

If you look at the dBB interpretation of QM, there is an explicit nonlocal interaction term in the Hamiltonian, whose sole purpose is to make the classical EoMs give predictions which are equivalent to QM predictions (including the violation of Bell’s inequalities). Thus, one can argue that Bell’s nonlocality can be seen as a form of nonlocal interaction, and cast into a usual language of a nonlocal Lagrangian.

Best, 🙂
Marko

9. **Billy Bob**
June 12, 2019

Bell non-locality is a subset of DBB non-locality. Bell locality is restricted to correlation only. DBB explicitly violates relativity by transferring information faster than light though no observer can access this to manipulate it to alter the past. Philosophers who don't make this distinction are no philosophers. But even physicists throw the word nonlocal around too loosely.

10. **Low Math, Meekly Interacting**
June 12, 2019

Sort of along the lines of what Marko says above, I thought the problem (forgive me if I screw up) is that, given QM accurately predicts probabilities of outcomes at spacelike separation that violate Bell's Inequality, giving up "local realism" is obligatory.

The problem is further exacerbated by lack of agreement about whether or not "local realism" is a valid requirement for a sensible interpretation to begin with. Maybe it's not universally clear what "local realism" is even supposed to mean.

Seems completely hopeless, if you ask me. "Shut up and calculate" wins again.

11. **Peter Woit**
June 12, 2019

I'm realizing that one thing that I really dislike about this subject is the hijacking of words (e.g. "realism" and "locality") to suit a particular agenda, the agenda that the fundamental QM formalism must be replaced because of the trickiness of relating this formalism to the classical formalism and our everyday intuitions. The words "realism" and "locality" get defined in a way designed to make QM non-"realistic" and non-"local" and thus problematic.

I think Sabine Hossenfelder is right, that arguing about the definition of "locality" will lead nowhere. To just make a bit clearer what I mean when I say Bell's inequality experiments are "non-local", take a look at the diagram of an example of such an experiment on Wikipedia
https://en.wikipedia.org/wiki/File:Bell-test-photon-analyer.png
What I'm referring to is just that one side or the other of this experimental set-up could be thought of as "local", putting the two sides together as one measurement apparatus is very "non-local". I don't see the lesson as "reality is non-local", see it as just a reflection of the measurement being a non-local physical process.

About "realism", I just don't see why it's a good idea to define "real" as "my human-scale classical model of the things I perceive" vs. "the best model we have from science of the physical world". My ontological commitments are based on what I know of science, which tells me the QM description of "physical reality" is the best-founded one. When Dr. Johnson refuted idealism by kicking a rock, the reality of the rock is best thought of not as "massive object with these coordinates", but "complicated many-body quantum system".

12. **Art**
June 12, 2019

What would a non-local result to a local measurement look like?

13. **Roger**
June 12, 2019

All of these arguments for non-locality are based on some sort of philosophical argument that the world must be described by hidden variables (like a classical theory), and then applying Bell's theorem and the Bell test experiments.

Sometimes the philosophical argument is based on analogies to classical mechanics, or on beables, or on reductionism, or on completeness. These are all just word games to disguise Bell's local hidden variable assumption. If you drop that assumption, then quantum mechanics is a local theory.

14. **Marko**
June 12, 2019

Peter,

"I don't see the lesson as "reality is non-local", see it as just a reflection of the measurement being a non-local physical process."

The only way to test whether reality is local or non-local is to perform a non-local measurement, since a local measurement is not sensitive to non-local effects. The fact that the experimental setup is non-local should therefore not be surprising. What is surprising is the *outcome* of that measurement, which suggests that reality is also non-local. This outcome is *not* an automatic consequence of the fact that the experimental setup is non-local, since in principle one could have also obtained a different result.

The definitions of locality and realism in Bell's theorem are not "hijacked" IMO, rather they are merely reformulations of what we traditionally and intuitively regard as locality and realism.

Informally speaking, locality is the statement that choices you make here and now are not correlated with the outcomes of measurements performed outside your past and future light cones. Realism is the statement that a physical system has a well-defined value of an observable even before one measures it. While there are all sorts of issues with the meanings of the words like "choice" and "measurement", I don't really see that these definitions are hijacking any otherwise traditional meaning of the notions of realism and locality.

🙂
Marko

15. **Mateus Araújo**
June 12, 2019

Dear Peter,

I believe your confusion is caused by the existence of several different versions of Bell's theorem, which often gets people talking past each other. You are right if you are talking about Bell's 1964 version (or rather CHSH's 1969 version of Bell's 1964 version), and Scholarpedia is right if they're talking about Bell's 1975 version.

I wrote about it here, maybe that can clear up something.

The root problem is that the word "locality" is rather overused, and people mean different things by it.

16. **Peter Woit**
June 12, 2019

Marko,

“Realism is the statement that a physical system has a well-defined value of an observable even before one measures it.”

I’m afraid that’s exactly what I mean by a hijacked definition, constructed specifically to generate a conflict with QM, a fundamental principle of which is that systems don’t have simultaneous well-defined values for non-commuting observables. I don’t at all see why I need to accept that definition. Looking at an “authoritative” source like Wikipedia
https://en.wikipedia.org/wiki/Philosophical_realism
shows a wide range of definitions of “realism”, with even the one about QM not the one you give. Of those, I would think the most relevant to scientists would be “scientific realism”, described as
“the world described by science is the real world, as it is, independent of what we might take it to be”
In this context, “science” (e.g. QM) is our best model of the real world, and the state of a physical system as given by QM is our best version of what “reality” is.

17. **Peter Woit**
June 12, 2019

Mateus Araújo,
Thanks for the references. The problem though is that I continue to not see why there’s anything surprising or problematic about the phenomena Bell is discussing, and why they are supposed to conflict with my understanding of reality or locality. So, it’s hard to get motivated to slog through long discussions of different versions of these claims.

18. **Peter Shor**
June 12, 2019

So how much value does the fact that interesting physics was discussed in it add to one of Einstein’s letters, and how much are you paying just for a letter in his handwriting?

19. **Eric Dennis**
June 12, 2019

Peter,

Unfortunately, what we can call the “John Bell camp” in the debate over the meaning of Bell Inequalities (because John Bell is in that camp) has been habitually misunderstood. For one, this camp is *not* motivated by a desire to impose classical categories (like particle position and momentum) on quantum objects. This camp is just fine with the idea that quantum objects can have different attributes that do not appear anywhere in classical physics–entirely new attributes subject to an entirely new dynamics as well. The sui generis-ness of quantum mechanics is a red herring in this debate.

Bell’s critics take a wrong turn at the very beginning. They make what is almost a category error: explaining how quantum objects defy certain classical assumptions (sometimes “realism,” sometimes “determinism,” sometimes “counter-factual definiteness”, etc.) and so the quantum objects are able to evade Bell’s reductio ad non-localitum. The category error is that Bell’s argument does not mention, does not rely on, does not implicitly refer to objects or theories being “quantum” vs. “non-quantum,” or “realistic” vs. “non-realistic” (whatever that might mean).

The first thing you have to do to grasp the meaning of Bell Inequalities is to forget the word “quantum,” and follow Bell as he imagines a very generic kind of system–totally agnostic as to whether its underlying dynamics is quantum, classical, stringy, wiccan, vegan, whatever. We only assume that the system has a few generic properties, like that you can identify two sub-systems at distinct locations A and B, that each sub-system has some kind of device with a binary read-out you can see (e.g., a screen flashes or it doesn’t flash), and that each device has some kind of two-setting gadget you can set.

Now Bell makes one critical assumption–the most obvious, straightforward implication of locality in terms of conditional probability distributions for the flashes on the A side given (i) the settings on both sides and (ii) the flashes on the B side. From this assumption, you go straight to the Bell Inequality. Violating the inequality means that the setting on one side somehow influenced the flash on the other side–a manifestly non-local effect, if the experiment is set up right.

One can imagine someone claiming that even though quantum systems violate this locality condition, they do not in fact exhibit non-local influences, because of some really subtle way quantum mechanics undermines the meaningfulness of Bell’s formulation of locality. Fine. Then do that; make that claim; spell out *how* that could be the case; don’t just insinuate it. No one ever spells it out, no matter how much background they have with QFT/GR. An august tradition started by Bohr, the patron saint of not spelling it out.

20. **Mateus Araújo**
June 12, 2019

Peter,

With regards to reality, I’m with you. It makes my blood boil to see people continuously trying to reduce “realism” to determinism, as Marko is doing. This is a hijacked definition. What should we conclude then, that the world is not real? Bell’s theorem proves that solipsism is true? Come on.

As for locality, though, I disagree. I think Bell’s definition of locality is a compelling one (that the probability of an event can only depend on events on its past light cone), and his demonstration that if fails for quantum mechanics is both surprising and problematic.

Since quantum mechanics was created people have noticed that there is problem with locality, because of the wavefunction collapse. Most famously we have Einstein’s 1927 and 1935 arguments. Now if you buy Einstein 1935, that we either have hidden variables or nonlocality, then Bell 1964 is a proof of nonlocality, as it shows that hidden variables cannot do the job. The argument is still messy though, not the least because of three decades in between, and that’s why I like Bell 1975 so much: self-contained, clear, and concise.

21. **vmarko**
June 12, 2019

Peter,

I wasn’t trying to give a rigorous definition of realism, or debate wikipedia, I just wanted to convey the idea, informally. Let me put it this way — how do you distinguish between the “real world” (which you observe while awake) and the “dream world” (which you observe while asleep and dreaming)? Assuming that everything you can know about either world is limited to what you can observe (and side-stepping 5000 years of philosophy on the topic), the only conclusion is that the real world cannot really be operationally distinguished from the dream world.

So if you want the real world to be “really real”, you have to subscribe to a metaphysical postulate that the real world, by definition, has at least some properties that are *independent* of the fact that you are observing them (otherwise you run into solipsism, as Mateus noted). And this is basically the same as the statement of realism I formulated previously.

Note that this definition does not require *all* observables to be simultaneously well-defined, so there is no problem with noncommutativity of observables. Also note that I am not really advocating for realism, I'm just explaining how I understand it.

Finally, claiming that "reality" is whatever your best theoretical description says it is — runs the risk of confusing the map with the territory. Our formal descriptions may change, while "reality" doesn't. For example, a century ago you could say that QM defines reality, but today we know that QM is wrong (particle creation and stuff), and that instead it is QFT which defines reality. And it is likely that QFT will also be eventually substituted with QG of some sort or whatever else. Our descriptions of the real world are epistemological, while the real world itself is (arguably) an ontological notion.

Mateus,

I'm confused, why do you say that realism is the same as determinism? The way I see it, realism is a statement about objectivity of existence of stuff, while determinism is a statement about its evolution (or predictability, or computability, or however you want to frame it). I don't see the two being equivalent in any way, since in principle one can think of a model which obeys realism but not determinism.

🙂
Marko

22. **Peter Woit**
June 12, 2019

Mateus Araújo,
I guess what I'm not understanding is how to reconcile
1. the microcausality of relativistic QFT, so presumably of the SM, our best model of physical reality
2. claims that observed violations of Bell's inequality imply influence outside the light cone.

This would seem to imply that while LHC physicists are desperately and fruitlessly looking for violations of the SM, atomic physics experimentalists are all the time looking at huge violations. This doesn't make sense. The problem has to be that a notion of "measurement" is being invoked that is in violation of the idea that a measurement apparatus is just another physical system. I understand that one possibility is that "measurement" does involve new physics, but I've never seen a plausible conjecture for what this new physics is, and why it is mysteriously inaccessible to usual modeling + experimental study, given that it is supposedly happening in so many experimental situations.

23. **Eric Dennis**
June 12, 2019

Peter,

The key assumption you're making is that (R)QFT/SM is locally causal. In Bell's sense, the SM is *not* locally causal.

The commutation of space-like separated field operators in the SM is something akin to local causality, but it is a weaker condition. This commutation-locality condition shows that we cannot *controllably* send faster-than-light signals. But commutation-locality does not preclude the existence of certain faster-than-light influences, which are not controllable by us.

The situation here with space-like separated field operators is just like the spin operators for the two particles in a Bell experiment. What Bell has shown is that we can make certain kinds of repeated measurements of pairs of spin operators (one for the particle on each side of the experiment), such that the two operators commute with each other in each measurement (because they are operators corresponding to two different particles), but nonetheless the total record of repeated measurements will statistically imply a faster-than-light causal influence between the sides. The nature of this influence just does not enable an experimenter on one side to *control* the measurement results on the other side, hence to send a signal.

What this shows is that the standard notion of locality in QFT (vanishing commutators) is really just a necessary but not a sufficient condition for locality. The SM, just like non-relativistic QM, is fundamentally non-local–as Bell showed it must be to reproduce Bell experiment results described by non-relativistic QM.

24. **Peter Woit**
June 12, 2019

Eric Dennis,
I'm surprised to hear that the last few decades when I thought I was studying local quantum field theory, I really was studying non-local quantum field theory…

25. **Robert**
June 13, 2019

OK, fine, you don't like the term "realism" as it means something else in other context. Call it something else "definitsm" seems to be freely available. What I mean is that it makes sense to argue about the value of something that is inherently unmeasurable like giving it a hypothetical value or talking about the probability of it having a value.

In the case of the original Bell paper, that is having P(a, b) (where a and b are vectors denoting the orientation of the detector) and P(a, c) in one equation where b and c describe incompatible measurements is the original sin.

Or put more abstractly: For a non-commutative algebra of observables, Bell's inequality (or better: its violation) shows that the space of states (the Bloch sphere in the case of a qubit) is not that of probability distributions on some space as it is not a simplex (where each state can be decomposed into pure states in which all events have probability either 0 or 1).

Speaking of definitions of terms: I think for these kinds of discussions it is important to use a definition of "locality" that does not render the classical theory "non-local". Of course, even classically, there can be (probabilistic) states that have correlations and thus even there states are global objects. Reading about the lottery numbers in one newspaper does not "uncontrollably influence" which numbers are printed in all the other news papers. It's just a correlation.

I strongly believe that as long as one sticks to statements that actually have observational meaning (thus excluding equations with P(a, b) and P(a, c) which not only are counter factual as nobody actually did the observation but have to as it is impossible to measure both) as well as the only thing worth worrying about is wether doing stuff here can instantaneously influence outcomes there (no, it cannot even in a quantum world, it could have only influenced a measurement that is impossible to do) then everything is fine. Only thing that has to go is the prejudice that all states are described by probability distributions (of what I would call "classical" outcomes in the 0/1 sense above of assertions that are either true or false).

Finally, with respect to reductionism: I think the strangeness of the quantum world is that the opposite is true: If you have full information about the whole (i.e. it is in a pure state described by a vector in Hilbert space rather than a density matrix, but if you don't like that language, you can equivalently say that you cannot gain more information by decomposing the states into a mixture of other states), entanglement means that you don't necessarily have full information about its parts: That is, even though the state of the whole is pure, the (marginalised) states of the parts can be mixed. Said more colloquially: Knowing everything about the whole does not mean you know everything about its parts (whereas the usual criticism of reductionism is that that knowing everything about the parts does not necessarily tell you everything about the whole).

26. **Mateus Araújo**
June 13, 2019

Marko,

What I'm saying is that your definition of "realism" reduces it to determinism. Since you agree that these are two different concepts that shouldn't be mixed, you should think again about using this as your definition of realism: "Realism is the statement that a physical system has a well-defined value of an observable even before one measures it."

Peter,

That's precisely why we have a problem. Apparently both 1 and 2 are true and contradict each other. But if you are asking about the solution of the problem, there are hundreds of opinions.

I can offer my own: note that Lorenz-covariance of QFT only holds for the unitary part of the theory, as measurements (with wavefunction collapse) obviously break Lorenz-covariance. Note also that Bell's observation that quantum mechanics is nonlocal (in the sense that the probability of an event depends on events outside its past light cone) depends crucially on making measurements and collapsing wavefunctions. This has two consequences: if you accept the standard description of measurement as valid, then there is no contradiction between 1 and 2, as QFT/SM breaks relativity anyway. On the other hand, if you accept that wavefunction collapse is not real, there is again no contradiction between 1 and 2, as Bell nonlocality does not exist.

27. **Peter Woit**
June 13, 2019

Mateus Araújo ,

"if you accept that wavefunction collapse is not real, there is again no contradiction between 1 and 2, as Bell nonlocality does not exist."

Since my point of view is that "wavefunction collapse" is just a crude approximation to what actually happens in a measurement process, that you really need to properly study the physics of the situation, treating the "measurement apparatus" + environment as quantum mechanical (not classical) systems, presumably the "nonlocality" disappears if you do this.

28. **vmarko**
June 13, 2019

Peter,

"I'm surprised to hear that the last few decades when I thought I was studying local quantum field theory, I really was studying non-local quantum field theory…"

Would it be of any help to see an explicit calculation of the violation of Bell's inequalities within the formalism of QFT? If yes, you may want to take a look at arXiv:1309.2059. They do not discuss what is local or nonlocal (their emphasis is on velocity-dependence), but an explicit QFT calculation may nevertheless shine some light on it?

Mateus,

"What I'm saying is that your definition of "realism" reduces it to determinism."

Care to elaborate how this reduction happens? It isn't obvious (to me, at least). While I believe that one could arguably bend over backwards and twist the meaning of the word "determinism" enough to fit your statement, I don't think it is true for the determinism in the usual sense of, say, Laplace's demon.

Best, 🙂
Marko

29. **Low Math, Meekly Interacting**
June 13, 2019

Forgive what might be some silly questions. I'm trying to get to the heart of the matter expediently using (I know, I know) words.

There are clearly a wide range of reactions to the fact that, in Bell-type experiments, nature violates Bell's (or CHSH's, or whoever's) inequality in a particular way. At the extremes are "Yeah? So?" and "GiH! Spukhafte Fernwirkung!" The debate between the two often looks to me like an intervention, with people yelling "I don't have a problem! YOU'RE the one with the problem!" back and forth.

So great, (literally) adjust your expectations for "reality". What should leave us perplexed, then, is not the correlation between quantum coin flips, but classical ones.

Is one therefore sweeping the "mystery" under the "emergence" rug and blaming evolution for our obtuseness, per usual?

30. **Eric Dennis**
June 13, 2019

Peter/Mateus,

It's not helpful to talk about QM (or QFT) here apart from some account of measurement, essentially bringing us back to 1925. The whole problem is that any theory that can account for actual measurement results (including Bell experiments) is, by Bell's reasoning, necessarily non-local. Indeed any particular such theory ever conceived (wavefunction collapse, hidden variables, spontaneous collapse, etc.) is manifestly non-local, whether in the context of simple QM or QFT.

It simply won't do to ignore Bell's result and say "Well, I don't believe in any of those theories, they're at best approximations, so there's no problem here."

I realize it is jarring to call Relativistic QFT (inclusive of measurements) non-local. First, this is not my idiosyncratic view. It is Bell's own view. See his paper "The

Theory of Local Beables”:

https://cds.cern.ch/record/980036/files/197508125.pdf

And it’s a view that has been gaining popularity more recently among people in QM foundations. Second, imagine that the field of QM foundations, for most of the period from the 1920’s onward, had been in a somewhat broken state, similar to the current state of string theory (viewed as a TOE).

There was never anything wrong with physicists arriving at something like the collapse view and simply confessing that such a thing didn’t make much sense when taken literally, but we’ll just leave it here as a placeholder until people figure out what’s really going on. Unfortunately that’s not what happened in the 1920/30’s. What happened were megalomaniacal pronouncements about QM constituting a philosophical revolution in our understanding of the relationship between existence and consciousness. We all know the embarassing quotes from Bohr, Heisenberg, Born, Jordan, etc.

When careful thinkers started poking around and asking questions (de Broglie, Einstein, Schrodinger, Bohm, Bell, Grete Hermann) they were dismissed as reactionaries, or simply misunderstood, not refuted. The early foundational mess crystallized inside the culture of physics and has been causing problems ever since. Given all of this, a pervasive misunderstanding about a somewhat subtle (and philosophically radioactive) distinction between the concepts of signalling locality and causal locality is not that shocking.

31. **nick herbert**
June 13, 2019

To my way of thinking, there are three basic levels of discourse, quantum Fact, quantum Theory and quantum Reality. The quantum Facts (exoeriments) are all local, both by actual experiment and by calculation. Quantum Theory seems to be non-local: when Alice makes a measurement Bob’s distant wavefunction collapses instantaneously. What about quantum Reality?

Bell’s achievement is that not only did he focus on Reality, considered by most philosophers to be inaccessible to mind, but he actually proved sonething about the nature of Reality.

When Bob and Alice share a pair of entangled photons, each makes a measurement on a sequence 1, 2, 3…N photons and gets certain results.

Consider one of these results, say result #36. Bell asks the question, what were the causes of Bob’s outcome for this single instance? What does nature need to know to produce this event? To answer this question one must consider for event #36, not only the event that actually happened but all possible events for all possible Bob and Alice settings. All of these events exceot one are contrafactual, but nature must be ready to produce a definite result for any of these possible choices.

Bell then considers what might be happening at Bob’s detector to produce Bob’s event,
making no assumptions about realism, determinism, but only that whatever causes nature uses to construct this outcome, the setting of Alice’s detector is not one of these causes. Bell then shows that any model of reality (how single Bob events are produced) with this restriction cannot reproduce the quantum Facts.

The “realism” assumption to my way of thinking is the notion that it makes sense to consider not only the one result #36 that actually happened, but events# 36 that could have happened for different detector settings. How you feel about Bell’s Theorem then would depend on how you feel about this “contrafactual definiteness” assumption. Is it “classical”, “metaphysical” or simply one natural way to think about quantum causality?

32. **Anonyrat**
June 13, 2019

To expand on Bee’s comment:
https://plato.stanford.edu/entries/bell-theorem/

“The principal condition used to derive Bell inequalities is a condition that may be called *Bell locality*, or *factorizability*. It is, roughly, the condition that any correlations between distant events be explicable in local terms, as due to states of affairs at the common source of the particles upon which the experiments are performed. See section 3.1 for a more careful statement.”

Section 3.1 is too long and dense to meaningfully excerpt here, as an appetizer: “The condition F of factorizability is the application, to the particular set-up of Bell-type experiments, of Bell’s Principle of Local Causality.”
https://plato.stanford.edu/entries/bell-theorem/#LocaCausAssu

33. **Peter Woit**
June 13, 2019

vmarko,
I’m not convinced that the problem lies with the QFT vs. QM treatment of polarization states, I think it’s with the coupling to a “measurement apparatus”.

vmarko/Eric Dennis
What I keep trying to point out is that what is getting ignored here is the actual physics of what happens when you take a spin 1/2 particle and “measure spin up or down”, or “collapse the wavefunction”. You’re ignoring the actual physics and using an extremely crude, non-local approximation to this physics, and then saying that it implies fundamental physics is non-local.

Eric Dennis,
You accuse Bohr et al. of engaging in mystification based on the crude “collapse” model of measurement, and there was plenty of that back in the 20s and 30s. From my reading, there was also a lot of much more nuanced discussion from them, well aware that the problem was the difficult one of emergence of classical behavior, but that they lacked the tools to analyze that so had to set that problem aside. That was nearly a century ago, the problem I see now is that Bell and those following him are now doing exactly what you accuse Bohr et al. of doing: making dramatic claims based on the problems with the obviously flawed “collapse” model.

34. **BSmith**
June 13, 2019

Physics is essentially local. When people talk about non-locality in the context of the EPR experiment “it’s a matter of giving a dog a bad name …”, according to Gell-Mann.
https://www.youtube.com/watch?v=gNAw-xXCcM8

35. **Eric Dennis**
June 13, 2019

Peter,

Permit me to create a stylized version, for purposes of clarity, indicating the pattern of many, many exchanges between the Bell camp and its critics…

Bell: Here's an extremely general theorem showing why *any* physical theory whatsoever that reproduces certain well-tested experimental results (specified in macro-level not micro-level terms) must be non-local.

Critic: Spacelike commutators vanish in QFT, so it's local, so it must evade your theorem.

Bell: QFT is not local. Spacelike commutators vanish, but this only ensures signalling locality, not full causal locality. The exact equivalent of spacelike commutators also vanish in a Bell experiment. My very general theorem shows how, despite vanishing commutators, the choice of device setting at A must influence the same-time output at B to match the observed results.

Critic: Let's not get too philosophical about defining locality.

Bell: OK, to be concrete, every specific, well-defined model ever conceived (collapse, hidden variables, etc.) that accounts for the measurement results is manifestly non-local. Exactly as expected from my extremely general theorem that doesn't refer to or rely on any theory-specific detail (like collapse, hidden variables, etc.).

Critic: Aha! Your theorem assumes the old-fashioned idea of wavefunction collapse, which we all know isn't rigorous or exact.

36. **Peter Woit**
June 13, 2019

Eric Dennis,
This has degenerated into the exactly what Sabine Hossenfelder warned would happen in the first comment, a useless argument over what "non-locality" means. I recommend the Gell-Mann video in the previous comment, where he makes essentially the same complaint I'm making about hijacking definitions in order to try and argue that there must be something wrong with QM.

37. **vmarko**
June 13, 2019

Peter,

The collapse postulate is also present in QFT, only hidden inside the LSZ formula. But if you are against using the collapse postulate to describe measurements, then your main problem is neither non-locality nor Bell's theorem, but rather the measurement problem of QM (of course, it remains a problem even if you do use the collapse postulate). In particular, you need some mechanism to explain why we observe a *single* outcome for every individual run of an experiment (say, a scattering process in LHC).

Nobody has a satisfactory answer to that, and I agree that the collapse postulate may look as an ugly phenomenological patch, rather than a fundamental part of a theory. But however you turn it, both QM and QFT are in this sense incomplete, and you need *some* additional postulate to define what a measurement is (or why we see single outcomes).

And in light of Bell's theorem, I don't believe that this additional postulate (however you choose to define it) is going to turn a non-local theory into a local one. You'll be just "kicking the can" further down the formalism, so to say.

🙂
Marko

38. **Peter Woit**
June 13, 2019

vmarko,
Yes, the measurement problem for me is the main problem. If you look at the Gell-Mann video mentioned above, his take on Bell's "non-locality problem" is that it's making the mistake of mixing things on "different branches", so he seems to be saying that it's the measurement problem.

As for the "why the single outcome" aspect of that problem, one thing that has always struck me is that we're typically DEFINING a "measurement apparatus" as something that behaves in such a way as to produce a single classical outcome. But that problem I think is much more subtle (and discussion of it left to a separate occasion) than the "non-locality" problem here, which just seems to be a red herring.

39. **Thomas Andersen**
June 13, 2019

Peter Woit wrote:
"
I fear though that the kinds of "foundations" of interest to the organizers seem rather orthogonal to the "foundations" that most interest me.
"

Why should that be something to fear? The world of physics is making very little progress at the moment (40 years). One thing is certain: if everyone got their way on what to not to study, there would be nothing available to study.

40. **Peter Woit**
June 13, 2019

Thomas Andersen,
That was a purely personal comment. I'm quite interested in the question of foundations of quantum theory so glad to see that a new group has formed to address the issue, just would be a lot happier if they seemed more interested in the questions that to me seem relevant. I don't think there's much danger at the moment of my views of what is interesting dominating the subject and driving out others…

41. **Peter Shor**
June 14, 2019

Three comments.

First: quantum mechanics doesn't just break classical probability theory (as Bell demonstrated); it breaks classical computational complexity theory and classical information theory as well. This is why there are a number of computer scientists who are convinced that quantum computers can't possibly work.

Second: I think this quote by Feynman is very relevant to this discussion: "I am going to tell you what nature behaves like. If you will simply admit that maybe she does behave like this, you will find her a delightful, entrancing thing. Do not keep saying to yourself, if you can possibly avoid it, 'But how can it be like that?' because you will get 'down the drain', into a blind alley from which nobody has yet escaped. Nobody knows how it can be like that." He said this in 1964; I think it's still good advice today.

Third: Feynman didn't actually follow his own advice. Around 1980, I saw him give a lecture at Caltech about negative probabilities, where he explained that his motivation was looking at all the hidden hypotheses of Bell's Theorem to try to figure out whether any of them might be false. He later published a paper on negative probabilities, but it didn't mention this motivation, presumably because he realized that negative probabilities couldn't explain quantum weirdness.

42. **Lee Smolin**
June 14, 2019

Dear Peter,

I have made what I hope is a strong case for taking seriously what we might call the "John Bell point of view" in my recent book Einstein's Unfinished Revolution, and find myself mostly in agreement with Eric Dennis and Marko. I would very briefly underline a few points:

-This is not a debate resting on confusions of words. Nor is there, to my knowledge, confusion among experts about the direct implications of the experimental results that test the Bell inequalities. The main non-trivial assumption leading to those inequalities is a statement that is usually labeled "Bell-locality". Roughly this says (given the usual set up) that the "choice of device setting at A cannot influence the same-time output at B".

Nothing from either quantum mechanics nor classical mechanics is assumed. The experiments test "Bell-locality" and the experimental results are (after careful examination of loop-holes etc.) that the inequality is cleanly and convincingly violated in nature. Therefor "Bell-locality" is false in nature.

-The conclusion that "Bell-locality" is false in nature is an objective fact. It does not depend on what view you may hold on the ultimate correctness, completeness or incompleteness of QM. Bohmians, Copenhagenists,Everetians etc all come to the same conclusion.

-Nor does it matter if there are other senses of "local" in which nature or QFT is local, because these notions are independent of "Bell-locality". Indeed, it is important to understand that "Bell-locality" is false in QM and QFT, as Bell showed directly. Therefor, the experimental result that "Bell-locality" is false in nature is an important confirmation of a prediction of QM and QFT.

-Now it is also true that Bell and a few others, for various and different reasons, held or hold a view that QM is incomplete, and that nature could be described more accurately by a different theory. Examples of such theories are dBB and collapse models. This has also nothing to do with confusion over the meaning of words. These are competing hypotheses about nature, to be settled ultimately by experiment.

For people in this camp, the falseness of "Bell-locality" is an important clue which constrains any such proposal for a completion of QM. But if you are not in this camp, and have no interest in the hypothesis that QM requires a completion to describe nature, you can just ignore the further implications.

Thanks,

Lee

43. **Peter Woit**
June 14, 2019

Thanks for the clarifications Lee.

By the way, beside Lee's new book, I can recommend the interview with him posted at Sabine Hossenfelder's blog

http://backreaction.blogspot.com/2019/06/a-conversation-with-lee-smolin-about.html

44. **Eric Dennis**
June 14, 2019

Lee,

Well put! My view is that "Bell locality" is the one True locality, because what could possibly be a better definition than 'no effects from outside the past lightcone'? But your line of attack here is probably more comprehensible to people with a conventional understanding of QFT.

Peter,

One natural impediment in coming at this stuff from the standpoint of conventional QFT is the scary question of what happens to QFT now. I think this fear has a really bad affect on people's ability even to permit Bell's analysis into the realm of Reasonable Views That I May Take Seriously.

Of course QFT isn't just rendered invalid as a scientific theory. There's just way too much ultra-precise and very diverse experimental confirmation. I can see only one natural explanation of this state of affairs: Lorentz invariance (more broadly, general covariance) is an emergent symmetry of nature at a particular energy scale, not a fundamental symmetry. This is controversial even within the Bell camp, however.

45. **Peter Woit**
June 14, 2019

Eric Dennis,
I'm not worried about QFT. The Bell analysis clearly has nothing at all to do with QFT.
As for using this as a motivation for deciding that Lorentz symmetry is emergent, I don't see that at all.

By the way, I'm developing a serious allergy to the endless claims now being heard that this or that about the SM or GR is not fundamental, but "emergent", with no idea what it is "emergent" from. I do thing classical behavior is properly described as "emergent", but in that case, we know what it emerges from (QM). If you want to start claiming QM is "emergent" too, you need some compelling story about where/when it fails as a description of nature, and what is supposed to replace it.

46. **Anonyrat**
June 14, 2019

My view is that "Bell locality" is the one True locality, because what could possibly be a better definition than 'no effects from outside the past lightcone'?

Sorry, neither "Bell locality" nor its failure means "no effects from outside the past lightcone". What ever entangled wave-function that a Bell-type experiment is measuring originated in the past lightcone of both the measurement apparatus.

47. **Mateus Araújo**
June 15, 2019

Dear Lee Smolin,

I'm sorry, but what you have written is false.

To start with, you said that "Indeed, it is important to understand that "Bell-locality" is false in QM and QFT, as Bell showed directly." It is important to notice that Bell's direct demonstration that "Bell-locality" is false in QM and QFT depends crucially on the collapse of the wavefunction. As it should be obvious, since unitary QFT is Lorenz-covariant.

Which brings us to the second point. You wrote that "The conclusion that "Bell-locality" is false in nature is an objective fact. It does not depend on what view you may hold on the ultimate correctness, completeness or incompleteness of QM. Bohmians, Copenhagenists,Everetians etc all come to the same conclusion." Now how could Everettians come to this conclusion, since the failure of "Bell-locality" depends on the collapse of the wavefunction?

And indeed they *don't* come to this conclusion, empirically speaking. If you check the literature you'll see that Everettians insist that quantum mechanics is (Bell) local. The appearance of nonlocality is caused by the mistaken assumption that measurements have a single outcome. This is elegantly put by Deutsch and Hayden in section 7 of their 1999 paper. A similar argument, perhaps clearer, is made by Brown and Timpson in section 9 of their 2014 paper. I've also blogged about his point here.

48. **vmarko**
June 15, 2019

Mateus,

"It is important to notice that Bell's direct demonstration that "Bell-locality" is false in QM and QFT depends crucially on the collapse of the wavefunction. As it should be obvious, since unitary QFT is Lorenz-covariant."

You have to be careful not to mix the Lorentz-invariance of the theory with the invariance of its solutions. The preparation and measurement are technically understood as boundary conditions, which fix a particular solution of the equations of motion. There is nothing surprising in the fact that a particular solution is not Lorentz-invariant — this is common even in the classical theory, and not specific to the collapse postulate in QM. When we say that QFT is Lorentz-invariant, we mean that the *dynamics* (encoded in the action or EoMs) is invariant, and this is true regardless of the measurement. In QM, the analogy would be that the Heisenberg equations of motion are Galilei-invariant, regardless of the fact that preparation and measurement can fix a particular frame of reference.

"Everettians insist that quantum mechanics is (Bell) local. The appearance of nonlocality is caused by the mistaken assumption that measurements have a single outcome."

Last time I checked with experimentalists, they did see a single outcome in every run of every experiment ever done. Do Everettians insist that these people are delusional? How do they explain any experimental results at all?

I understand that the measurement problem is hard, and that the collapse postulate may not be a perfect or complete solution, but ignoring that the problem exists is not a solution either, IMO.

Best, 🙂
Marko

49. **André**
June 15, 2019

Mateus,
Sure QM/QFT by itself is a local theory, the problem is that QM/QFT by itself is also a theory without any predictions. As soon as you want to make any connection to experiments, you have to talk about collapse in some way – and be it by reducing your consideration to one of the Everettian branches.
This is IMHO the main point that Everettians are missing: While it is mathematically perfectly consistent as a theory – and it looks like physics because the unitary dynamics has the same description as in standard QM (with ill-defined projection postulate) – the "anything goes" approach of Many Worlds is not a scientific theory, it is just a mathematical construct.
In this regard, an Everettian standpoint on QM is in no way better than the String Theory Landscape. That is also what puzzles me a bit about Peter's criticism of QM foundations, because sociologically I see a lot of parallels between string theorists and large parts of the quantum info community, and I also read (with great pleasure) the criticism of Many Worlds on this blog. But Many Worlds is an unvoidable consequence if you take QM/QFT by itself without any modifications that explain collapse.
[Edit: Marko was a bit faster with his reply.]

50. **Paddy**
June 15, 2019

Peter, you wrote:

> "I'm realizing that one thing that I really dislike about this subject is the hijacking of words (e.g. "realism" and "locality") to suit a particular agenda"

If you are going to write about this sort of thing you really need to accept that words like that are useless unless their meanings are very carefully specified, because different people honestly use them to mean very different things. Otherwise, rational debate is impossible.

Most physicists consider "locality" to be an essential feature of our theories (or of "reality"), and if you ask them why, they will probably point at Einstein (after all, Newtonian gravity was explicitly non-local). So it is relevant that what Lee Smolin calls "Bell-locality" is very clearly Einstein's personal view, implicit in the EPR paper and stated explicitly in his "Reply to Critics" in the Schlipp volume. I find it odd that modern relativists like Hossenfelder choose to use "locality" in a much more restricted sense than the founder, just so that they can insist that QFT is "local". One might even ask, who is doing the hijacking here, and why is it so important to keep the label, even though it no longer denotes anything more than a technical condition?

51. **Geoffrey Dixon**
June 15, 2019

Interesting, though hardly surprising, that comment count is proportional to how unknowable and controversial the topic.

52. **Mateus Araújo**

June 15, 2019

Marko,

"You have to be careful not to mix the Lorentz-invariance of the theory with the invariance of its solutions. The preparation and measurement are technically understood as boundary conditions, which fix a particular solution of the equations of motion. There is nothing surprising in the fact that a particular solution is not Lorentz-invariant — this is common even in the classical theory, and not specific to the collapse postulate in QM. When we say that QFT is Lorentz-invariant, we mean that the *dynamics* (encoded in the action or EoMs) is invariant, and this is true regardless of the measurement."

This might be true if the universe started with a measurement, and ended with one. Alas, this is not true, there are several measurements in between, that are part of the dynamics, not of boundary conditions. Technically speaking, while the dynamics between a given preparation and measurement can be well-described by a (unitary) S-matrix, if you want to talk about the future of a measurement or the past of a preparation, you'll need to introduce a collapse that will break Lorenz-covariance. That is, if you don't go Many-Worlds.

"Last time I checked with experimentalists, they did see a single outcome in every run of every experiment ever done. Do Everettians insist that these people are delusional? How do they explain any experimental results at all?"

I think you're just trolling, but I'll give a serious answer anyway. What Many-Worlds says that will happen in a measurement is that there will be several copies of the experimentalist, each observing a single outcome. And this is not postulated by fiat, it's what the equations say. Do you think that anybody would take the theory seriously if this were not the case?

53. **Mateus Araújo**
June 15, 2019

André,

"This is IMHO the main point that Everettians are missing: While it is mathematically perfectly consistent as a theory – and it looks like physics because the unitary dynamics has the same description as in standard QM (with ill-defined projection postulate) – the "anything goes" approach of Many Worlds is not a scientific theory, it is just a mathematical construct."

Even if you don't buy the Everettian approach to probability, this is not true. The theory makes many deterministic predictions: there will be no violation of conservation of energy, there will be no break of Lorenz-covariance, there will be no superposition of charges, etc.

And I don't think probability is a weak point of Many-Worlds, rather it is a strength (I'm aware that this statement is controversial). Standard QM cannot make sense of objective probability, but Many-Worlds can.

"In this regard, an Everettian standpoint on QM is in no way better than the String Theory Landscape."

This is just an empty insult.

54. **Marty Tysanner**
June 15, 2019

I don't know how anyone who is a "realist" and has taken the time to understand Bell's theorem and its experimental tests (and examination of its experimental loopholes, most of them implausible) can not be really, really disturbed by the consistent observation of violations of the theorem under a variety of increasingly stringent tests. It certainly freaked me out when I learned about it in grad school. Of course, as Lee noted above, this is only a problem for those who look for deeper meaning from theories and experiments; instrumentalists and those whose interest in QM is for practical ends can freely ignore the conceptual issues, and there is certainly nothing wrong with that.

Tim Maudlin (a philosopher of physics who has deeply studied Bell's theorem and articulated much on it) has argued Bell made two key assumptions: *locality* and the experimenter's *freedom to choose experimental settings*. Determinism is not an assumption. Hence, since all known experimental loopholes (like detector inefficiency) are now closed, violations of Bell's inequality imply either locality or freedom of choice is false. Bell noted that the latter is possible if nature is "superdeterministic" in Bell's sense. Since most people don't take his superdeterminism too seriously (and he didn't either), and experimenters at least seem to believe they can freely manipulate the settings, it is usual to conclude that non-locality is a fact of nature.

While I may understand their rationale, it still dismays me that so many people have thrown locality "under the bus" (so to speak). Locality isn't like the 19th century ether that was invented to explain certain physical phenomena; it's at the core of relativity, electromagnetism, classical mechanics, and even QM evolution up to the time of measurement. I can't think of anywhere else in all of fundamental physics–outside QM entanglement–where locality is in question. Moreover, there are obvious conceptual issues with non-local influences/communication between entangled particles at space-like separation: How do the particles "find" each other within the entire universe, so they can "communicate" (other than tautalogically or some other version of "it just happens; don't ask how"); and why don't we see nonlocality in other contexts if it's a truly fundamental aspect of nature?

I think there might be other viable alternatives besides the unpalatable choice between superdeterminism and non-locality. Concretely, one question stands out: What if experimental tests are *not* actually testing Bell's theorem the way it seems they are, so that the relevance we assign to the observed violations is an artifact of our ignorance of the "construction" (incomplete modeling) of the particles we employ, and of their interaction with the experimental apparatus, rather than an actual demonstration of non-locality? Stated differently, what if the change of basis (e.g., polarization basis for photons) that is central to Bell tests does not actually cause a particle to fully "forget" its original basis, and furthermore the memory of its original basis subsequently affects its path through the apparatus even though the particle by itself *statistically* respects its new basis in all ways? I'm thinking of something analogous to the phase of the wave function, but which corresponds to an actual attribute (or "hidden variable", to use that awful term) of the test particles.

In a meaningful sense this alternative violates the "freedom of choice" assumption if the nature of the particle is such that an apparatus cannot effect an irreversible change of basis without destroying the original entanglement. It would mean a photon is not completely characterized by its spin, frequency, helicity/polarization, electromagnetic coupling, and of course Poincaré symmetry; something more is present.

I don't think this idea can be dismissed out of hand. Abstractly, particles are representations of the Poincaré group and an internal symmetry group which are characterized by spin, mass, angular momentum, charge, etc., the origins of which remain unexplained. Given this lack of explanation/understanding there is no empirical reason–only historical or philosophical prejudice–for assuming we possess a complete characterization of the properties and behavior of the known particles. We can reliably model only what we observe, but our ability to test those models is constrained by what we know we should look for (ignoring serendiptous discoveries).

Nonetheless, first reactions to this alternative may be something like "Yuck, that's really ad hoc and contrived" or "Yeah right, and why exactly haven't we detected this attribute in other kinds of experiments?" But the Aharonov-Bohm effect can be interpreted as demonstrating the reality of the phase of the wave function, even though (as in the second objection) this phase is generally unobservable. And at least in my own research, much of it currently related to dynamical, geometric models of electrons and photons, the alternative scenario outlined above is *not* ad hoc; it actually appears plausible.*

Given the centrality of locality in physics, I think we should "fight to the death" to preserve it in a fundamental theory. That presumably requires considering alternative possibilities besides superdeterminism or giving up.

* [The reason you won't find papers on these models (yet) is straightforward. Any proposal of a concrete, mathematical yet explanatory dynamical model is automatically faced with the highly nontrivial task of ensuring empirical and theoretic consistency with all known phenomena and established theory of the modeled particle; or at least enough consistency must be demonstrated to make the model interesting, especially with QM/QFT and electromagnetism. That effort is ongoing…]

55. **Lena Birkenfeld**
June 15, 2019

I've seen a lot of debate on this comment section on traditional interpretations of quantum mechanics, but nobody seems to have taken a look at Neumaier's interpretation yet.

Mateus Araújo,

"And I don't think probability is a weak point of Many-Worlds, rather it is a strength (I'm aware that this statement is controversial). Standard QM cannot make sense of objective probability, but Many-Worlds can."

And Neumaier in his thermal interpretation of quantum mechanics states that the probability found in quantum mechanics is not objective, but is emergent from the fact that we only have incomplete knowledge of the state of the system:

https://arxiv.org/pdf/1902.10779.pdf

56. **Lena Birkenfeld**
June 15, 2019

Peter,

Neumaier says something similar to what the Scholarpedia article said about Bell's Theorem in section 4.5 of his 2nd paper on his thermal interpretation:

"Bell's theorem, together with experiments that prove that Bell inequalities are violated imply that reality modeled by deterministic process variables is intrinsically nonlocal. The thermal interpretation explicitly acknowledges that all quantum objects (systems and sub-systems) have an uncertain, not sharply definable (and sometimes extremely extended) position, hence are intrinsically nonlocal. Thus it violates the assumptions of Bell's theorem and its variations."

https://arxiv.org/pdf/1902.10779.pdf

compare with Scholarpedia:

"Bell's theorem asserts that if certain predictions of quantum theory are correct then our world is non-local."

57. **vmarko**
June 15, 2019

Mateus,

"Technically speaking, while the dynamics between a given preparation and measurement can be well-described by a (unitary) S-matrix, if you want to talk about the future of a measurement or the past of a preparation, you'll need to introduce a collapse that will break Lorenz-covariance."

I agree, concatenation of two solutions along a "measurement boundary" (past for one, future for the other) is not itself a solution. But note that QFT is usually always applied precisely in the sense that preparation happened in t=-oo, while the measurement happens at t=+oo. Of course, this is an approximation, but works well enough for events in the LHC and such, a context most common for QFT.

"What Many-Worlds says that will happen in a measurement is that there will be several copies of the experimentalist, each observing a single outcome. And this is not postulated by fiat, it's what the equations say."

Not really. The equations fail to say why the experimentalist always observes a single outcome in the computational (0/1) basis, and never observes a single outcome in the superposition (+/-) basis. There is nothing in unitary dynamics that could distinguish between the two bases. In order to work around that, MW has to resort to postulating a preferred basis — and this is postulated by fiat. The preferred basis postulate (a) breaks unitarity, and (b) it is essentially equivalent (in predictive power) to the collapse postulate, because otherwise MW would be a different theory, rather than just an interpretation of QM.

I've seen various attempts to single out a particular preferred basis using various arguments and handwaving, but they all fall short of being successful, as long as MW holds up to strict unitary evolution.

Best, 🙂
Marko

58. **Tim Maudlin**
June 16, 2019

"As far as I can tell, for all the experiments that come up in discussions of Bell's theorem, if you do a local measurement you get a local result, and only if you do a non-local measurement can you get a non-local result."

This is about as embarrassing and revelatory a sentence as could be written. What in the world could a "non-local result" mean?

You do experiments in two (or three: GHZ) labs. You get results of those experiments. Those results display correlations that no local theory (in the precise sense defined by Bell) can predict. Ergo, no local theory can be the correct theory of the actual universe. I.e. actual physics is not local (in Bell's sense).

This is the sort of thing that can be explained in 20 minutes to undergrads, and they understand it. Woit is clearly intelligent enough to understand it, and his lack of comprehension is indicative of some weird refusal to pay attention to what Bell did.

59. **Robert**
June 16, 2019

" The equations fail to say why the experimentalist always observes a single outcome in the computational (0/1) basis, and never observes a single outcome in the superposition (+/-) basis."

There is always a single outcome in the diagonal basis that was actually measured. You can measure the spin on an electron in any direction you like and you will always get a definite result.

60. **Mateus Araújo**
June 16, 2019

Marko,

You are silently moving the goalposts from "experimentalists should observe multiple outcomes" to the preferred basis problem. You seem, however, to not understand what the problem is (was, actually). You write:

"The equations fail to say why the experimentalist always observes a single outcome in the computational (0/1) basis, and never observes a single outcome in the superposition (+/-) basis. There is nothing in unitary dynamics that could distinguish between the two bases."

Actually, an experimental apparatus that makes a measurement in the 0/1 basis is different from an apparatus that makes a measurement in the +/- basis. There is no difficulty in capturing this difference in the unitary dynamics, it is really a different unitary transformation.

Rather, the problem was that given a unitary that makes the measurement in the 0/1 basis, ending up with a state like |0>|M_0> + |1>|M_1>, why should we interpret this state as representing a superposition of quasi-classical worlds |0>|M_0> and |1>|M_1>, instead of some other superposition of worlds, or even a single world with some other result? Historically it was simply postulated to be so: an apparatus that measures in the 0/1 basis creates quasi-classical worlds in the 0/1 basis. This is obviously rather unsatisfactory, and people moved on to trying to determine what the quasi-classical worlds are via the Schmidt decomposition, which doesn't really work (and it is anyway rather weird to postulate that the Schmidt decomposition has some special role).

The problem was solved via decoherence. People realised that an apparatus that measures in the 0/1 basis makes the system *decohere* in this basis. This has two effects: the quasi-classical worlds |0>|M_0> and |1>|M_1> are single out by being stable under decoherence, and also they become dynamically decoupled, which justifies thinking of them as separate worlds in the first place.

For a historical account of the preferred basis problem, I'd recommend Saunders' introduction to the "Many Worlds?" book, available here. For a pedagogical take on how decoherence gives rise to the quasi-classical worlds, I'd recommend Wallace's Decoherence and Ontology.

61. **vmarko**
June 16, 2019

Mateus,

"You are silently moving the goalposts from "experimentalists should observe multiple outcomes" to the preferred basis problem."

I'm not trying to move the goalposts — these two things are related. Let me put it this way. You split the Universe into three subsystems: the spin, the apparatus (including us), and the environment (rest of the Universe). Then you trace over the environment, and obtain a density matrix which is almost-block-diagonal in the computational basis, courtesy of (handwavingly specified) interaction with the environment. Say this density matrix looks like diag(1/2,1/2) [here each entry is actually a submatrix describing the detailed state of the apparatus]. The collapse postulate is a further (nonunitary) transformation of the matrix into a form like diag(1,0), encoding the statement "result '0' was observed". In MW you refuse to make that additional step, and instead interpret the non-collapsed density matrix as "in one branch I see diag(1,0), in the other branch the other me sees diag(0,1), and the two branches are weighted with probability 1/2 each". This interpretation makes sense only if you *prove* that the density matrix is really almost-diagonal in the computational basis, as opposed to some other basis. I've never seen this proof, only qualitative handwaving arguments that the interaction with the environment has precisely the right properties to single out a computational basis. So I wouldn't call this problem "solved" by decoherence.

Regarding decoherence itself, of course you can always describe nonunitary dynamics of a system as unitary dynamics of a larger system, by tracing over the ancilla. That's basically a theorem. Nevertheless, this relies on the assumption that the appropriate ancilla physically exists, i.e. that your physical system does have an environment with appropriate interactions. In other words, decoherence may work only for open quantum systems, but not for isolated systems. I find the "environment assumption" ontologically unsatisfactory, since the Universe as a whole is an isolated system, with no environment to fix a preferred basis. For a serious analysis of this issue, see for example arXiv:1105.3796.

I asked the same question to John Preskill a while ago (https://quantumfrontiers.com/2013/01/10/a-poll-on-the-foundations-of-quantum-theory/#comment-2823), and his reply was basically that we should always trace out the part of the Universe which is outside our observable horizon. I find this unsatisfactory, just like Bousso and Susskind do in the arXiv paper.

Best, 🙂
Marko

62. **Peter Woit**
June 16, 2019

All,
I'm closing comments on this posting. This has gotten to the point where zero light is being shed on the Bell-nonlocality issue, and I've lost the patience needed to try and sensibly moderate a general discussion that people want to take in other directions.

Various

June 18, 2019
Categories: Uncategorized

First something really important: chalk. If you care about chalk, you should watch this video and read this story.

Next, something slightly less important: money. The Simons Foundation in recent years has been having a huge (positive, if you ask me…) effect on research in mathematics and physics. Their 2018 financial report is available here. Note that not only are they spending \$300 million/year or so funding research, but at the same time they're making even more (\$400 million or so) on their investments (presumably RenTech funds). So, they're running a huge profit (OK, they're a non-profit…), as well as taking in each year \$220 million in new contributions.

Various particle physics-related news:

- The people promoting the FCC-ee proposal have put out FCC-ee: Your Questions Answered, which I think does a good job of making the physics case for this as the most promising energy-frontier path forward. I don't want to start up again the same general discussion that went on here and elsewhere, but I do wonder about one specific aspect of this proposal (money) and would be interested to hear from anyone well informed about it.

  The FCC-ee FAQ document lists the cost (in Swiss francs or dollars, worth exactly the same today) as 11.6 billion (7.6 billion for tunnel/infrastructure, 4 billion for machine/injectors). The timeline has construction starting a couple years after the HL-LHC start (2026) and going on in parallel with HL-LHC operation over a decade or so. This means that CERN will have to come up with nearly 1.2 billion/year for FCC-ee construction, roughly the size of the current CERN budget. I have no idea what fraction of the current budget could be redirected to new collider construction, while still running the lab (and the HL-LHC). It is hard to see how this can work, without a source of new money, and I have no idea what prospects are for getting a large budget increase from the member states. Non-member states might be willing to contribute, but at least in the case of US, any budget commitments for future spending are probably not worth the paper they might be printed on.

  Then again, Jim Simons has a net worth of 21.5 billion, and maybe he'll just buy the thing for us…

- Stacy McGaugh has an interesting blog post about the sociology of physics and astronomy. His description of his experience with physicists at Princeton sounds all too accurate (if he'd been there a couple years earlier, I would have been one of the arrogant, hard-to-take young particle theorists he had to put up with).

  McGaugh's specialty is dark matter and he has some comments about that. If you want some more discouragement about prospects for detecting dark matter, today you have your choice of Sabine Hossenfelder, Matt Buckley, or Will Kinney. I don't want to start a discussion of everyone's favorite ideas about dark matter, but wouldn't mind hearing from an expert whether my suspicion is well-founded that some relatively simple right-handed neutrino model might both solve the problem and be essentially impossible to test.

- Lattice 2019 is going on this week. Slides here, streaming video here.
- Strings 2019 talk titles are starting to appear here. I'll be very curious to hear what Arkani-Hamed has to say. His talk title is "Prospects for contact of string theory with experiments (vision talk)" and while he's known for giving very long talks, I don't see at all how this one could not be extremely short.

On a more personal front, yesterday I did a recording for a podcast from my office, with the exciting feature of an unannounced fire drill happening towards the end. Presumably this will get edited out, and I'll post something here when the result is available.

Next week I'll be heading out for a two week trip to Chile, with one goal to see the total solar eclipse there on July 2. Will start out up in the Atacama desert.

Update: John Horgan has an interview with Peter Shor. I very much agree with Shor's take on the problems of HEP theory:

> High-energy physicists are now trying to produce new physics without either experiment or proof to guide them, and I don't believe that they have adequate tools in their toolbox to let them navigate this territory.
>
> My impression, although I may be wrong about this, is that in the past, one way that physicists made advances is by coming up with all kinds of totally crazy ideas, and keeping only the ones that agreed with experiment. Now, in high energy physics, they're still coming up with all kinds of totally crazy ideas, but they can no longer compare them with experiments, so which of their ideas get accepted depends on some complicated sociological process, which results in theories of physics that may not bear any resemblance to the real world. This complicated sociological process certainly takes beauty into account, but I don't think that's what is fundamentally leading physicists astray. I think a more important problem is this sociological process leads high-energy physicists to collectively accept ideas prematurely, when there is still very little evidence in favor of them. Then the peer review process leads the funding agencies to mainly fund people who believe in these ideas when there is no guarantee that it is correct, and any alternatives to these ideas are for the most part neglected.

Update: I think John Preskill and Urs Schreiber miss the point in their response here to Peter Shor. Shor is not calling for an end to research on quantum gravity or saying it can't be done without experimental input. The problem he's pointing to is a "sociological process" and so potentially fixable. This problem, "collectively accept ideas prematurely", not realizing the difference between a solid foundation you can build on, and a speculative framework that may be seriously flawed is one that those exposed to the sociological culture of the math community are much more aware of. Absent experimental checks, mathematicians understand the need to pay close attention to what is solid (there's a "proof"), and what isn't.

Comments

1. **Pascal**
   June 18, 2019

   In France too we have better quality chalk than in the US.

2. **Supernaut**
   June 18, 2019

   Thanks for the link to Stacy McGaugh's blog post, I found it interesting. I'll also be traveling to see the eclipse to Chile; I'll be near the La Serena area (hopefully the weather will cooperate and will see little or no clouds!)

3. **Peter Woit**
   June 18, 2019

   Supernaut,

I'll be staying in La Serena, no plan yet for where we'll try and see the eclipse, will depend on cloud situation that day.

4. **Dan Riley**
June 18, 2019

The timeline has detector construction starting in 2031, with HL-LHC shutdown likely around 2035, so there isn't that big an overlap. I believe (but can't find a reference now) that the timelines also all have the expensive tunnel excavation starting after HL-LHC shutdown.

wrt funding, unlike the US National Labs, CERN can borrow money to spread out spending peaks, and its funding is secure enough that it can get a good interest rate. Both LEP and LHC construction were partially funded that way. Any of the FCC proposals will still be a stretch, so the timeline also includes 3 years to work out the funding strategy and another 3 to get in-principle agreements.

5. **JE**
June 18, 2019

Hi Peter,

La Serena is really awesome, but cozy San Pedro de Atacama and its surroundings should be the best place in Chile to see such a thing. Of course it depends on the cloud situation, but if I remember correctly the Atacama desert is the driest place on Earth. Also, don't miss a trip to Valparaíso if you get a chance.

6. **Peter Woit**
June 18, 2019

Dan Riley,
Thanks, I wasn't aware there was ability to borrow against future funding, that would help.

The documents I was looking at said HL-LHC 2026-2036/7 and for the FCC-ee, see figure 19 of
http://cds.cern.ch/record/2653669/files/CERN-ACC-2019-0003.pdf?version=2
This has construction starting 2029 (tunnel) and 2031 (machine + detectors), physics operation starting 2039.

If there is some way to fund this, it seems likely it would involve stretching out this schedule.

7. **Peter Woit**
June 18, 2019

JE,
Problem is, the track of the total eclipse is fairly narrow, thus the need to be in the area around La Serena on July 2. Your other suggestions are encouraging, since our plan is to spend a few days in San Pedro de Atacama before the eclipse (any observatories nearby looking for a visitor?) and a day or two in Valparaiso afterwards.

8. **Michael Barany**
June 19, 2019

Many readers of this blog already know about my work on the history and sociology of chalk in mathematics. For those who don't: it's a topic of serious academic research, with lots of fascinating questions and findings!
Here's the piece I wrote for the Best Writing in Mathematics series: http://mbarany.com/DustyDisciplineBWM15.pdf
Here's the longer article it's based on: http://mbarany.com/Chalk.pdf
And here's a short interview I did on the subject: http://www.concordmonitor.com/x-6888872

9. **Peter Shor**
June 19, 2019

About almost-impossible-to-detect right-handed neutrinos—my suspicion is that theses models have been undeservedly neglected, to some degree, by theorists, just because there is no way that experiments would ever be able to detect it.

This is the presumably the same phenomenon that leads theorists to predict particles that can be detected by the next generation of accelerators. Somebody should give it a name, and then we will be better able to correct for this bias.

10. **Zoviyer**
June 19, 2019

The japanese chalk quality is fantastic, the multiple moving blackboards a thing to marvel. But woof!, the handwriting of many many mathematicians is so bad, they let us down most of the time. "You can't make a silk purse out of a sow's ear".

11. **Anon**
June 19, 2019

Regarding right handed neutrinos: You need the dark matter to be around today, whereas heavy right handed neutrinos decay to Higgs + regular neutrino. The requirement that they be stable on cosmological time frames constrains the possibilities, which pushes you to lower masses (which undercuts the seesaw motivation, if you care).

You then have to figure out how to produce sterile neutrinos in the early universe. Not only do you have to get the right amount of them, but they can't be relativistic because the extra contribution to N_eff screws up nucleosynthesis. It's challenging to actually get them to be cold dark matter; most models end up with at least a sizeable proportion as "warm" dark matter- which is (currently) observationally ok- perhaps even favored if you believe some structure surveys. Getting this to happen, though, generally requires them to be produced resonantly- meaning that you have to arrange by hand for at least two to have nearly degenerate masses. I don't think non-resonant production is entirely dead, but that case tends to be hotter. (The other option is to introduce some other particle that can decay to them- but then you've lost some of your simplicity.)

So, I'm not sure about your opinion about where "relatively simple" ends, but IMHO all the simple options are relatively warm, which are being studied via structure formation.

Summary of the state of the art is in this white paper:
https://arxiv.org/pdf/1602.04816.pdf

12. **Peter Shor**
June 23, 2019

Peter:

I totally agree with your update. I'm not saying we should give up thinking about quantum gravity. I'm saying that theoretical physicists should think very carefully about what they "know" about black holes, information loss, AdS-CFT, and string theory, and see whether their evidence is as convincing as they think it is. I don't think it is.

Some evidence that the sociological process is broken:

1. The conventional wisdom that Susskind's theory of complementarity explained the information loss paradox, which had been accepted by many high-energy physicists for years, broke down when the AMPS paper showed it was incompatible with basic principles of quantum information theory.

2. David Poulin and John Preskill have done some research on how you might be able to modify quantum field theory to obtain a non-unitary theory, thus accommodating black hole information loss. See this presentation. It seems to me that nobody is paying any attention to this because they "know" that the universe is unitary because AdS-CFT.

3. I don't see how the paper of Shenker and Stanford, "Black Holes and the Butterfly Effect" and the idea that the CFT is a quantum error-correcting code can possibly be compatible. I've talked with people who think these two papers are both correct, and who really should know what they're talking about, but I haven't gotten any answers that I've found satisfactory. Maybe I don't understand AdS-CFT well enough (I barely understand it at all), but my impression is that they're asking quantum error correcting codes to behave in ways that are impossible. I'd be happy to go into this in more detail by email.

13. **Peter Woit**
June 24, 2019

Peter Shor,
Thanks for the examples of the problem you're pointing to. For better or worse, I've spent a lot of time trying to understand how string theory unification is supposed to work, enough to clearly see why it doesn't work, and to feel comfortable writing about that problem. In the case of the supposed relation of quantum information theory and quantum gravity, I've yet to even see how this is supposed to work. I can't see what specific plausible proposals are behind the mantras "space-time is doomed", "quantum gravity is emergent" and "it from qubit", so wouldn't even know where to begin to try and evaluate the claims being made for such ideas.

It would be great if someone could write up clear explanations of this research program, what its specific proposals and goals are, what has been achieved, what has been found to not work. You should start a blog!

14. **Peter Shor**
June 24, 2019

Unfortunately, there are a lot of papers about the relationship of quantum information theory and quantum gravity that I don't understand. I'm not entirely convinced they have any interesting content, but I don't understand them well enough to say that they are all just Zen-koan-like slogans à la Urs Schreiber.

The big successes of this relationship so far, in my opinion, are (a) the AMPS paper
showing that Susskind complementarity cannot be correct as it was originally envisioned and (b) the Almheiri-Dong-Harlow paper showing that the Ryu-Takayanagi formula doesn't lead to fundamental contradictions in physics as long as you postulate that the CFT is a quantum error-correcting code.

More to the point of my answers to John Horgan, the AMPS paper claims that one of the following four assumptions is false (I've split their assumption ii into two pieces):

(i) Hawking radiation is in a pure state (i.e., unitarity is not violated),
(ii-a) the information carried by the radiation is emitted from the region near the horizon (i.e., locality is not violated),
(ii-b) low energy effective field theory is valid outside a region microscopically close to the horizon (i.e., physics behaves as we think it should under conditions where we've tested it extensively),
(iii) the infalling observer encounters nothing unusual at the horizon.

The AMPS paper opts for (iii) being the incorrect assumption, and arrives at firewalls.

Talking with people from the it-from-qubit crowd, many of them seem to believe that (ii-a) is the incorrect assumption. Maybe this makes research easier … if there's non-locality, you can just assume that we know nothing about the laws of physics in the AdS bulk theory, and only work on the physics in the boundary CFT. And conformal field theory is much better understood than quantum gravity, although I suspect that you can only go so far without thinking about how the laws of physics might work for the AdS part.

Taking (ii-b) as the incorrect assumption presumably would contradict over a century of physics experiments—as far as I can tell, nobody advocates this.

And finally, taking (i) as the incorrect assumption seems to be anathema. I suspect that this is because AdS-CFT is taken to be axiomatic—they have 20 years of work invested in it—and the conventional wisdom says that AdS-CFT proves unitarity. Is the conventional wisdom correct about this? Beats me, but as far as I know, nobody is thinking about whether it might be wrong.

15. **cedric bardot**
June 25, 2019

Is there really nobody thinking about wheter [AdS-CFT proves unitarity] is wrong?
To say the least Gerard 't Hooft is suspicious about it:

> String theory was supposed to take over when particle energies and momenta approach the Planckian domain. But the particles and fields that seem to be involved with black holes with mass M_BH , all seem to have energy excitations in the domain M_Pl^2/M_BH in natural units, where M_Pl is the Planck mass. These energies are way below the Planck domain. It should be possible to describe the properties of black holes with mass M_BH ≫ M_Pl without the use of string theory at all. We now claim that the assumptions "particles are pieces of string", and black holes are "stacks of D-branes"… that can be subject to the AdS/CFT conjectures, are better to be avoided in the case of large, heavy black holes.

https://arxiv.org/abs/1902.10469

Incidentally Utrecht University will host an international conference entitled "'t Hooft 2019 – From Weak Force to Black Hole Thermodynamics and Beyond" beginning Thursday July 11 until Saturday July 13 with the following advert:

> As Professor Gerard 't Hooft relishes lively debates, he was keen on us inviting speakers with perspectives that differ from his own – we expect stimulating discussions!

https://thooft2019.sites.uu.nl/speakers/

Looking at https://cds.cern.ch/record/2668388 it is instructive to watch how difficult it is for physicists to agree on the basics of the modelisation of the problem they want to deal with.

16. **cedric bardot**
June 25, 2019

Actually I didn't point to the video I was thinking about. https://cds.cern.ch/record/2668988 was the discussion cession about black hole information at CERN in march 2019 I had in mind.

17. **Peter Woit**
June 25, 2019

All,
I'm shutting off comments here, partly because people seem to want to get into a not obviously well-informed discussion of the details of AdS/CFT, and partly because I'm leaving for Chile now, will be traveling the next couple weeks.

What happens when we can't test scientific theories?

July 8, 2019
Categories: Uncategorized

Just got back from a wonderful trip to Chile, where the weather was perfect for watching the solar eclipse from the beach at La Serena.

While I was away, the Guardian Science Weekly podcast I participated in before leaving for Chile went online and is available here. Thanks to Ian Sample, Graihagh Jackson, and the others at Science Weekly who put this together, I think they did a great job.

The issues David Berman, Eleanor Knox and I discussed in the podcast will be familiar to readers of this blog. Comparing to the arguments over string theory that took place 10-15 years ago, one thing that strikes me is that we're no longer hearing any claims of near term tests of the theory. Instead the argument is now often made, by Berman and others, that it may take centuries to understand and test string theory. This brings into focus the crucial question here: how do you evaluate a highly speculative and very technical research program like this one? Given the all too human nature of researchers, those invested in it cannot be relied upon to provide an unbiased evaluation of progress. So, absent experimental results providing some sort of definitive judgment, where will such an evaluation come from?

Comments

1. **Jon Orloff**
   July 8, 2019

   Another relevant point, how does the science community expect taxpayers to pay for a program whose outcome may be uncertain for hundreds of years?

2. **Robert**
   July 9, 2019

   Same as in other fields. Mathematics comes to mind. No real experiments there, formal correctness of proof does not say anything about relevance of result and peers who can judge are also typically personally invested in the subject.

3. **tulpoeid**
   July 9, 2019

   @Robert,

   formal correctness of proof does not say anything about relevance of result? That's a new one to me. It'd be nice to hear more about what the implied problem is.

   (I'm not a mathematician and I've been having the impression that things are pretty smooth in the proof department.)

4. **Low Math, Meekly Interacting**
   July 9, 2019

   "(H)ow do you evaluate a highly speculative and very technical research program like this one?"

   If you're talking about science, to me the simplest answer is "You can't." It's also the only correct one, in my opinion.

   It shouldn't stop people from working on it, necessarily, but if one so chooses, one must therefore forgo classifying the activity as "scientific". People should be entitled to their opinions. I think they should even be afforded the opportunity to seek funding to further cultivate those opinions in an academic setting. Other disciplines do this. But divorced from experiment indefinitely (conceivably even eternally), speculation is just that: opinion, however rigorous or well-informed.

5. **Robin**
   July 9, 2019

   It comes down to costs and benefits. The cost of doing any theory is essentially independent of the type of theory that is being worked on – it is mainly the salaries of theoreticians plus overheads, and the cost is incurred as the work is undertaken. Benefits that might not be realised for some time in the future are worth less than comparable benefits that come sooner, for example by undertaking different endeavours. Berman mentions that he sees benefits delayed by perhaps a thousand generations, so these are of very little value indeed. This mismatch of timing looks fatal for the endeavour. Feel pity for the students and postdocs who are sucked in.

6. **Low Math, Meekly Interacting**
   July 9, 2019

   Suggestion: Include a photo of one of the eclipses you've observed in your banner rotation. It's obviously a passion of yours, and may have a certain aesthetic harmony with the the M87* data for some.

7. **Maik**
   July 9, 2019

   Well, I think it's a very delicate question, as it goes right to the heart of what freedom of scientific inquiry should and should not mean. Where does science end, which questions are worthy of scientific pursuit and where is one just wasting taxpayer's money for playing one's own little games?

   Even though I am very critical of string theory myself, I hesitate to answer that problem by calling for a restriction to this freedom in the sense of "only work on what can directly be tested". This may achieve the opposite of what one aims for, namely scientific progress. Nonetheless, I think there are ways to subject scientific work(s) to scrutiny without profoundly restricting that freedom: methodological and institutional ones (at least).

   Regarding the former, philosopher of science Paul Feyerabend was arguing very strongly that it is a bad idea to nail down scientists to a particular methodology (science as a kind of anarchy), but he did not question the need for methodology itself. Rather, which methodologies are appropriate or not is a question that needs to be evaluated and discussed by the scientists in the field and adjacent ones. With respect to fundamental physics, I can say that much of it has essentially become a mathematical subject and as such works therein should adhere to mathematical rigor and conceptual coherence. Even if this is not without problems, as Robert suggests, mathematical culture has evolved like this because it is all too easy to go wrong otherwise.

   On an institutional level, I think it is important to create opportunities for this kind of discussion, to keep the scientists engaged (not just in a circle of specialists), to challenge their ideas, be willing to listen both as a layman and a scholar with a healthy amount of skepticism. The power structure of academia is not always, but in my experience all too often an obstacle to this kind of `open science'. Furthermore, as I view it, Alexander von Humboldt rightly postulated the `unity of research and teaching', for in the context of this question teaching can also be considered a mechanism of `checks and balances'.

The problem we should be worried about is not that decade-old research programs turn out to be in vain (it's difficult and we're all human!), but that we create a system and culture that prevents actual progress via the illusory, implicit claim that "science is truth".

8. **Mongo**
July 9, 2019

Sounds like a very common problem many areas of science have that claim the proof is in the future and just 100 years or more away. Is it still science? Does this apply to all areas, hard and soft sciences? Is this a string theory only problem?

9. **Lennie**
July 9, 2019

As opposed to physics, mathematics is not supposed to have any relevance. Its relevance is a bi-product. and nobody knows in advance what is going to be relevant, and relevant to what. Of course, there is always a question of how many mathematicians the taxpayers are supposed to support. Recently some kind of experimental mathematics came to life, though one can say that it is not quite a recent phenomenon: that is what a big part of mathematics was before the 19th century. Maybe one should treat string theory as a new kind of physics, speculative physics, that, if one looks back, is also not quite a new phenomenon. It has already influenced mathematics in a positive way. Maybe this is enough to justify its existence?

10. **Peter Woit**
July 9, 2019

Jon Orloff/Robin,
The costs of paying theorists doing speculative physics theory are not very large, and in many cases aren't coming from taxpayers (e.g. the theorists at the IAS are mostly being paid from private money). I don't see any reason society shouldn't support at a modest level some small group of people to do speculative work on trying to find a better fundamental theory of physical reality, even if the time-scale for expected success is very long. The problem is how to evaluate this work as it is being done.

Robert/tulpoeid,
I've often argued that part of the answer to the question of evaluation of speculative physical theory is to look to the culture of mathematics, which doesn't have experiment to keep it honest. Part of the culture of mathematics is to insist on clear and unambiguous statements and arguments, and to my mind speculative physics is very much in need of more emphasis on this. Robert is pointing to the much more subtle problem of evaluating the difference between interesting and uninteresting correct results.

LMMI,
Much of our current understanding of things like qft is based on the work of mathematicians and physicists who developed new ideas following research programs that had little to nothing to do with experiment. You don't want to cut off this kind of activity. In any case, if you insist on this, you'll just have people going on about how their extra-dimensional research has something to do with LIGO, or how they will make predictions, but it will take 100 years or more.

What you really want is progress towards new understanding, and while this is something hard to evaluate, it is very real. Some work does deepen our understanding, other work doesn't, and expertise is a necessary but by no means sufficient requirement for evaluating this.

11. **Robert**
July 10, 2019

@tulpoeid

You can easily write a computer program that derives an infinite series of proven "theorems" from a list of axioms in a formal way (see gödel Escher Bach) with zero creativity or usefulness. On the other hand, many extremely fruitful contributions to mathematics came with incorrect proves at first or totally lacking those (as conjectures, programs, frameworks etc). I am just saying, things are not black and white once you look closer.

12. **DrDave**
July 10, 2019

I found the podcast to be eerily prescient. The universal thread that seems to bind the various mutations in ST is that whatever comes up, ST will adapt. No evidence? Just wait. No predictions? No worries. Until forever.
We can assume at this point that ST will have an answer to any challenge. There's no evidence that long term proponents will change their minds, in any case. From the comments in the podcast, we could be looking at support for speculative science to continue for decades or even centuries before anyone feels the need to change course.
It also seems clear that any breakthrough or discovery in Physics will be absorbed, Borg-like, into the theory.
In addition, and perhaps most importantly, the structure of academic institutions is simply not equipped to deal with this issue.
At this point, the only recourse would seem to be new, well financed institutions that provide a complete pathway from graduate student to professor, with a system in place to remain independent. A massive undertaking.

Against Symmetry

July 10, 2019
Categories: Uncategorized

One of the great lessons of twentieth century science is that our most fundamental physical laws are built on symmetry principles. Poincaré space-time symmetry, gauge symmetries, and the symmetries of canonical quantization largely determine the structure of the Standard Model, and local Poincaré symmetry that of general relativity. For the details of what I mean by the first part of this, see [this book](). Recently though there has been a bit of an "Against Symmetry" publicity campaign, with two recent examples to be discussed here.

Quanta Magazine last month published K.C. Cole's [The Simple Idea Behind Einstein's Greatest Discoveries](), with summary

> Lurking behind Einstein's theory of gravity and our modern understanding of particle physics is the deceptively simple idea of symmetry. But physicists are beginning to question whether focusing on symmetry is still as productive as it once was.

It includes the following:

> "There has been, in particle physics, this prejudice that symmetry is at the root of our description of nature," said the physicist Justin Khoury of the University of Pennsylvania. "That idea has been extremely powerful. But who knows? Maybe we really have to give up on these beautiful and cherished principles that have worked so well. So it's a very interesting time right now."

After spending some time trying to figure out how to write something sensible here about Cole's confused account of the role of symmetry in physics and encountering mystifying claims such as

> the Higgs boson that was detected was far too light to fit into any known symmetrical scheme…
> symmetry told physicists where to look for both the Higgs boson and gravitational waves

I finally hit the following

> "naturalness" — the idea that the universe has to be exactly the way it is for a reason, the furniture arranged so impeccably that you couldn't imagine it any other way.

At that point I remembered that Cole is the most incompetent science writer I've run across (for more about this, see [here]()), and realized best to stop trying to make sense of this. Quanta really should do better (and usually does).

For a second example, the Kavli IPMU recently put out a press release claiming [Researchers find quantum gravity has no symmetry](). This was based on the paper [Constraints on symmetry from holography](), by Harlow and Ooguri. The usually reliable Ethan Siegel was taken in, writing a long piece about the significance of this work, [Ask Ethan: What Does It Mean That Quantum Gravity Has No Symmetry?]()

To his credit, one of the authors (Daniel Harlow) wrote to Siegel to explain to him some things he had wrong:

> I wanted to point out that there is one technical problem in your description… our theorem does not apply to any of the symmetries you mention here! …
>
> It isn't widely appreciated, but in the standard model of particle physics coupled to gravity there is actually only one global symmetry: the one described by the conservation of B-L (baryon number minus lepton number). So this is the only known symmetry we are actually saying must be violated!

What Harlow doesn't mention is that this is a result about AdS gravity, and we live in dS, not AdS space, so it doesn't apply to our world at all. Even if it did apply, and thus would have the single application of telling us B-L is violated, it says nothing about how B-L is violated or what the scale of B-L violation is, so would be pretty much meaningless.

By the way, I'm thoroughly confused by the Kavli IPMU press release, which claims:

> Their result has several important consequences. In particular, it predicts that the protons are stable against decaying into other elementary particles, and that magnetic monopoles exist.

Why does Harlow-Ooguri imply (if it applied to the real world, which it doesn't…) that protons are stable?

What is driving a lot of this "Against Symmetry" fashion is "it from qubit" hopes that gravity can be understood as some sort of emergent phenomenon, with its symmetries not fundamental. I've yet though to see anything like a real (i.e., consistent with what we know about the real world, not AdS space in some other dimension) theory that embodies these hopes. Maybe this will change, but for now, symmetry principles remain our most powerful tools for understanding fundamental physical reality, and "Against Symmetry" has yet to get off the ground.

Update: Quanta seems to be trying to make up for the KC Cole article by today publishing a good piece about space-time symmetries, Natalie Wolchover's [How (Relatively) Simple Symmetries Underlie Our Expanding Universe](). It makes the argument that, just as the Poincaré group can be thought of as a "better" space-time symmetry group than the Galilean group, the deSitter group is "better" than Poincaré.

In terms of quantization, the question becomes that of understanding the irreducible unitary representations of these groups. I do think the story of the representations of Poincaré group (see for instance my book about QM and representation theory) is in some sense "simpler" than the Galilean group story (no central extensions needed). The deSitter group is a simple Lie group, and comparing its representation theory to that of Poincaré raises various interesting issues. A couple minutes of Googling turned up this [nice Master's thesis]() that has a lot of background.

Comments

1. **Lukas Berns**
   July 10, 2019

   FYI the Japanese IPMU article says the result *suggests* proton decay rather than claiming the proton is stable. I guess that makes more sense if B-L is violated ignoring the point about AdS (so the English article should be corrected).

2. **Sabine Hossenfelder**

July 10, 2019

Symmetries play an important role in theory-development because they are simplifying principles, they are patterns that you look for. Science is generally a search for patterns/laws, but in the foundations these appear in very strict, formal ways on the level of the equations, which makes them particularly powerful.

The trouble with using symmetry principles is that we don't know that this type of simplification will continue to work, both because (a) it may be the wrong type of simplification and (b) the next deeper level may just not be simpler than the present one.

In other words, it's nice that it works, but no reason to think it will continue to work.

If you want a more interesting take on the idea that symmetries are accidental, have a look at this paper:

https://www.sciencedirect.com/science/article/abs/pii/0370269380908424

It strikes me as the kind of idea whose time hasn't yet come.

As to the Cole piece. I couldn't even get myself to finish reading it. Unfortunately, it's the kind of nonsense that sounds plausible to people who don't understand the subject.

Thanks for clearing this up.

3. **Anonyrat**
July 11, 2019

How much of superstring theory's growth was fueled by pursuing symmetry no matter where it led?

4. **Robert**
July 11, 2019

Regarding global symmetries: Wasn't there an old (as in from the 80s) argument by Banks et al that argues from a string world sheet perspective that a global target space symmetry would always produce a corresponding gauge field which would render the symmetry local?

From an effective field theory perspective it could also be that the ultimate UV theory is not symmetric at all and what we see at low energies is only the practically massless stuff that is protected by symmetries from becoming heavy (gauge symmetries for spin 1 particles, chiral symmetries for spin 1/2, just a good symmetry lacking for the Higgs, if you don't have susy…). So our liking of symmetries is due to observational bias.

5. **vmarko**
July 11, 2019

"in the standard model of particle physics coupled to gravity there is actually only one global symmetry: the one described by the conservation of B-L"

Sorry, am I missing something here? Can anyone tell me which process in the SM violates B and L individually (while conserving B-L)? And does coupling to gravity have anything to do with it?

Because in the ordinary flat-space SM I don't see any Feynman diagram that could violate either B or L, and I also don't see how coupling to gravity could possibly induce such a violation.

🙂
Marko

6. **martin**
July 11, 2019

Sabine, I disagree with your opinion that the role of symmetries is in simplification. There is a difference between 1) I have this equation, I will study spherically symmetric solutions, because it is easier, and 2) I have this equation, I found out that it has these symmetries and I am going to study them because it helps me understand the solutions.

7. **Peter Gerdes**
July 11, 2019

Is your suggestion that somehow gravity could be emergent in AdS space but not in other spaces? Doesn't it seem difficult for a largely local effect to disappear based on changes to the global space? Now I'm merely a mathematician so I could be missing something but wouldn't at least something gravity like have to be emergent even in non-AdS spaces? I mean the other differences about the models in which the holographic principle seems to hold might matter but I'm curious how a local property could emerge/fail to emerge (even approximately) depending on whether some global constraint on the space holds.

8. **Peter Woit**
July 11, 2019

Sabine,
One problem here is that there are various types of "symmetry" arguments, and the term "symmetry" gets used with different meanings. What I have in mind (this is related to what commenter martin writes) is the symmetry properties of the fundamental equations of the theory. The Heisenberg commutation relations are precisely the statement that you have a representation of the Heisenberg Lie algebra, and largely determine the basic structure of quantum mechanics. Gauge symmetries and Poincare symmetry largely determine the structure of the Standard model.

It is quite possible of course that there is a better, deeper fundamental theory, in which gauge symmetries, the Heisenberg commutation relations, local Poincare symmetry are not fundamental, but approximate, emergent properties. I just don't see such speculation as so far working out.

9. **Peter Woit**
July 11, 2019

Anonyrat,
In some sense the fundamental problem with string theory is that no one has been able to find the non-perturbative version, to answer the question "what is M-theory?". A lot of efforts in that direction have been to try to find fundamental symmetries that determine the theory, but this has so far not been successful. One can take this as an indication that searching for fundamental symmetries is misguided (and I think this is one thing driving the "Against Symmetry" agenda), but trying to find a non-

symmetry based M-theory hasn't worked either, so maybe what is misguided is the idea that M-theory is a fundamental theory of nature.

10. **Peter Woit**
July 11, 2019

vmarko,
From what I remember, there is baryon number violation in the SM (at unobservably low rate) non-perturbatively due to instanton effects. Presumably B-L is not violated this way. This has nothing to do with quantum gravity.

11. **Peter Woit**
July 11, 2019

Peter Gerdes,

The problem with emergent gravity is, emergent from what? You need an actual theory, not just a bunch of words and hopes that a theory exists. The actual theories with some sort of emergent gravity that I'm aware of are toy models, very different than the real world (AdS vs. dS, wrong dimension). A lot of the current work in this area is on low-dimensional toy models that exhibit emergent gravity. The hope is that this will lead to some understanding that can be applied to find a realistic model, but that still seems very far away.

12. **Doug McDonald**
July 11, 2019

This will be procedurally unhelpful, but anyway … when I was studying on my own relavistic field theory and the standard model, there was a book that explained "From what I remember, there is baryon number violation in the SM (at unobservably low rate) non-perturbatively due to instanton effects" but not as instanton effects. They had a discussion that I was able to follow. They explained it as essentially a nonperturbative passage through a (classical) barrier in the (classical nonrelativistic, thus allowing potentials) Lagrangian by (relativistic) quantum tunneling. They used what seemed to me to be essentially (classical) transition state theory with a quantum tunneling correction to calculate the (astoundingly low, compared even to the usual GUT estimates) rate. It required the participation of 3 particles each of lepton and quark, with at least 2 of each being different generations, like one electron neutrino, one muon antineutrino, and a charged lepton. Three leptons turned into three quarks or vice versa.

But I was never able to find this passage again in any book I could check out of the library at a later date. It was a dated but well known in its day (post QCD discovery day) textbook, in the "epilog" on nonperturbative effects. I'd love to find it.

13. **Peter Woit**
July 11, 2019

Doug McDonald,
There's an extensive later literature, but this goes way back to 't Hooft's earliest work on instantons, see this 1976 PRL paper

https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.37.8

14. **Marko**
July 12, 2019

Peter and everyone,

Ok, after some research, the relevant keywords are the "sphaleron" and the Adler-Bell-Jackiw anomaly, which seem to be responsible for B and L violation.

However, these effects have so far been unobserved (sphalerons ought to appear somewhere around 10 TeV), and they are nonperturbative. Given that the SM is an effective low-energy model, nonperturbative results are not to be trusted in general, unless supported by experiment (which is missing in this case). Therefore, I prefer to remain a skeptic regarding the B and L violation, until data starts to support it.

But it's a nice read, I learned something new today. 🙂 Thanks for the info and the links!

And regarding the media frenzy around the paper by Harlow and Ooguri, it's just business as usual — hype beyond any proportions. H&O of course also ignore that there is a difference between AdS and the real world (which is not even strictly dS, let alone AdS), which is also business as usual — string theorist's ultimate hope that AdS/CFT could at some point become relevant for the real world physics.

Best, 🙂
Marko

15. **Daniel Harlow**
July 14, 2019

Hi Peter,

If you want to read a popular article where the various caveats for our work are mentioned, see

https://phys.org/news/2019-05-constraints-symmetries-holography.html.

Best,
Daniel

16. **Peter Woit**
July 15, 2019

Thanks Daniel!

17. **Chris W.**
July 17, 2019

It's rather odd that the article referred to by Daniel Harlow starts out explaining that Harlow and Ooguri have presented new arguments supporting the conjecture that there are no global symmetries in quantum gravity, and then at a certain point starts dropping the qualifier "global", as though it's unnecessary (at least according to the piece's author, Ingrid Fadelli):

> Prior to Ooguri and Harlow's paper, other researchers made arguments supporting the claim that quantum gravity (the unification of quantum mechanics and gravity) cannot have any symmetry. Nonetheless, these arguments often presented logical gaps or loopholes, for instance failing to address some important cases (e.g. discrete symmetry).

…and later on:

> The insight from this previous research was essential to prove the theorem in the researchers' recent study. In their new study, Ooguri and Harlow found that the way quantum error correction works is not compatible with any symmetry. Thus, once quantum mechanics and gravity are merged, no symmetry is exact.
>
> "It has generally been believed that symmetry is a fundamental concept in nature," Ooguri said. "Many physicists believe that there must be a beautiful set of laws in nature, and that one way to quantify beauty is by symmetry. Some of the symmetry may be hidden in our world (or 'spontaneously broken," in physics terms), but they may manifest themselves if we look at nature at a more fundamental level. We showed that the belief expressed in the above is wrong. The laws of nature at the most fundamental level, where quantum mechanics and gravity is unified, have no global symmetry."

18. **Peter Woit**
July 17, 2019

Chris W.,
I think there are two serious problems with the way the Harlow-Ooguri result is being promoted:
1. The fact that, as Harlow notes, the only relevance to the symmetries that govern fundamental physics is to the highly obscure special case of global B-L symmetry is not made clear, replaced by nonsense about "QG means no symmetry". Even Ethan Siegel was taken in by this.
2. Their argument does not apply to the real world at all. It takes place in a toy model (AdS/CFT), which is quite different than a viable model of real 4d world quantized gravity coupled to the SM. Claiming that this tells you anything about the real world seriously misrepresents what AdS/CFT is. Arguably it's an interesting toy environment to use to think about quantized gravity, but it is not a solution to the problem of quantizing real 4d gravity.

Prospects for contact of string theory with experiments

July 13, 2019
Categories: Strings 2XXX

Nima Arkani-Hamed today gave a "vision talk" at Strings 2019, entitled Prospects for contact of string theory with experiments which essentially admitted there are no such prospects. He started by joking that he had been assigned this talk topic by someone who wanted to see him give a short talk for a change, or perhaps someone who wanted to "throw him to the wolves".

The way he dealt with the challenge was by dropping "string theory", entitling his talk "Connecting Fundamental Theory to the Real World" and only discussing the question of SUSY (he's still for Split SUSY, negative LHC results are irrelevant since if SUSY were natural it would have been seen at LEP, and maybe a 100km pp machine will see something, or ACME will see an electron edm).

He did discuss the string theory landscape, and explained it was one reason that about 15 years ago he mostly stopped working on phenomenological HEP theory and started doing the more mathematical physics amplitudes stuff. David Gross used to argue that the danger of the multiverse was that it would convince people to give up on trying to understand fundamental issues about HEP theory (where does the Standard Model comes from?). It's now clear that this is no longer a danger for the future but a reality of the present.

In order to go over time, Arkani-Hamed dropped the topic of his title and turned to discussing his hopes for his amplitudes work. The "long shot fantasy" is that a formulation of QFT will be found in which amplitudes are given by integrating some abstract geometrical quantities.

The conference ended with a "vision" panel discussion. Others may see things differently, but what most struck me about this was the absence of any sort of plausible vision.

Update: Taking a look at the slides from the ongoing EPS-HEP 2019 conference, Ooguri seems to strongly disagree with Arkani-Hamed, claiming in his last slide here that a CMB polarization experiment (LiteBIRD) to fly in 8 years, "provides an unprecedented
opportunity for String Theory to be falsified." I find this extremely hard to believe. Does anyone else other than Ooguri believe that detection/non-detection of CMB B-modes can falsify string theory?

Comments

1. **Tony**
   July 13, 2019

   I watched the "vision" discussion and I'm not entirely convinced the fate of high energy theoretical physics is in the best of hands given that the owners of those hands struggle with the concept of placing microphones in said hands for the purposes of audibility!

   That said, the language that is being used here and elsewhere seems to me to be better suited to theological discussions, rather than scientific ones. One hears "hope", "faith" and so on far too often to provide any convincing meta-evidence that these people are on the right track. I worked in neuroscience for nearly 20 years (and trust me, that is a field full of shysters) and never saw anything remotely as shambolic as the current state of theoretical (high energy) physics. I mean, the string landscape story is a morass of no escape and the whole AdS/CFT duality story is not even applicable to our universe.

   I remain absolutely stupefied that people with so much mathematical and technical expertise can get sucked into what has effectively become a cult. In my opinion, senior people in the field, such as Witten, need to show some genuine leadership and say enough is enough, let's move on. But of course that would require the cult leaders to themselves renounce their own faith and belief system, which they cannot do without admitting to practicing seriously poor judgment.

2. **Peter Woit**
   July 14, 2019

   Tony,

   There's a long history of Strings XXXX "vision" talks full of hype and unrealistic claims about the present and prospects for the future. Gross has a lot to answer for in terms of being responsible for many of these things, he was doing this 24 years ago at Strings 1995 and has never stopped. If instead of hype, he and others over the years instead put forward a realistic summary of the state of the field and the problems it was facing, that could have had a very positive effect.

   What struck me as unusual about this year's version was the lack of any actual vision, with the panelists having nothing much to say when Gross asked them for a vision of the future of the field over the next decades (Doug Stanford's response was something like "why do we need a vision, as long as we're having fun?"). Urs Schreiber is on the opposite extreme of string fandom from me, but his response to this was similar:
   "The closing 'vision' panel discussion did not take off."
   https://twitter.com/SchreiberUrs/status/1150113755566678016

3. **Alessandro Strumia**
   July 14, 2019

   I remember a "vision" talk by Gross at HEP 2011. At that time (no new physics in the first 1/fb of LHC data) many good theorists started discussing the reality of data. So I expected a useful discussion, but Gross predicted the discovery of SUSY, of DM, of new Z mesons, a new CERN/US linear collider to explore the superworld. All within 10 years. In the last slide Gross cared about LHC data, telling: "1/fb down, 3000/fb to go". Some wise guy commented: "1 beat in the head down, 3000 to go".

4. **Peter Woit**
   July 14, 2019

   You can watch the talk Alessandro Strumia refers to here
   https://webcast.in2p3.fr/video/an_outlook_from_theory
   At 28 min in you can see Gross's "predictions" for the 10 years post-2011. His track record on these is not good…

   For another such talk from him, way back when, you can take a look at the first substantive post on this blog
   http://www.math.columbia.edu/~woit/wordpress/?p=2
   from March 2004, which reported on a Gross "vision" talk I went to. The post included the text:
   "This [LHC startup] is now getting close enough that Gross and others seem intent on ignoring the failures of string theory, desperately hoping that superpartners will pop out of the LHC, thereby providing at least some vindication of the train of reasoning that lead to string theory. What will be interesting to see will be what Gross et. al. do when this doesn't happen. Will they drop string theory?"

The answer to this is now conclusively known. A remarkable aspect of Strings 2019 is that the only mention I saw of the LHC was Arkani-Hamed's rather absurd revisionist claim that the LHC null SUSY results did not change anything. After years of pointing to the LHC results as what would test string theory and vindicate the idea, the string theory community now acts like the null SUSY results at the LHC are not worth mentioning.

5. **Low Math, Meekly Interacting**
July 15, 2019

This isn't a very technically insightful comment, but if I may make a human observation: All this HEP theorist swagger and arrogance some people complain about seems to have left the building. The younger members of the panel make word choices and display body language that evinced more embarrassment than anything to my ears and eyes. Try to get these guys to talk about something other than their own work and the buzzkill is palpable. Gross is asking about "hopes" and all I could do was feel bad as they struggled to think of something they were willing to own up to.

6. **Peter Woit**
July 15, 2019

LMMI,
I think that's accurate. Arkani-Hamed was also unusually subdued, I'd guess partly because he isn't really a string theorist and has no particular positive vision of where string theory research is going. Having just delivered a talk about string theory prospects for connection with experiment in which he hadn't been able to come up with any can't have helped. Nati Seiberg also pretty much ignored string theory, sticking to the quite accurate point that there is a huge amount about QFT that we don't understand, and making progress there is what is most promising.

7. **Dom**
July 16, 2019

It is painful to read the site in question as a human being with empathy for people struggling with an illness but I gather that the reason that the Strings conference was dismal was:
"It almost looks to me as if the nasty anti-string crackpots were co-organizing the conference and could veto talks if not participants."

8. **Thomas Van Riet**
July 16, 2019

The suggestion by Ooguri is sensible. Maybe detailed model building and detailed understanding of SUSY breaking is still rather difficult in string theory. Not because of the theory but because, like anything in fundamental physics, it is tough to do detailed computations, especially when conformal and super-symmetries are broken. What string theory sofar seems to tell us is that there are patterns for "low energy" physics coming from string theory. These patterns are the key in order to make predictions. That is the essence of the Swampland program. The question is whether the patterns can be proven or not. The Weak Gravity Conjecture is probably the most rigorous such a pattern, but already consistent with experiment, so no prediction. Large field inflation seems clearly something that does not come for free in string theory. Some are trying to prove this formally. They might succeed, and some Euclidean version of the WGC could be behind it. Not clear yet. But we should be constructive and supportive to the people that are trying instead of complaining about it. I do not see any other framework for quantum gravity and fundamental physics where such endeavors are even possible.

9. **Peter Woit**
July 16, 2019

Thomas Van Riet,
What exactly is the falsifiable string theory prediction Ooguri is claiming for CMB B-mode polarization?

10. **Peter Woit**
July 16, 2019

Dom,
I normally try and avoid discussing the latest weirdness from Lubos, but for those who want to indulge, they can go to his website and check out his take on the Strings 2019 vision panel. He seems to agree with me and Urs Schreiber that there was no vision at the vision panel. Blaming this on me as co-organizer of the conference is pretty funny.

Thinking about it, a couple things that struck me about the panel is that of the five speakers (other than Gross, the moderator), three are currently at the IAS, two are recent products of Stanford. For a conference in Europe, this was very much a US group, focused on a narrow part of the US theory community. When I was a student at Princeton in the early 80s, the field was quite faddish. When I got there everyone was working on instantons. I think this way of working is still going on today, with the center of gravity some very specific questions about AdS/CFT and associated toy models. Lubos has this description of today's fad:

"All of the talks are about some French-speaking style general complexity-thermodynamics-AdS-attempts-on-quantum-cosmology issues, with some CFT rather disconnected from the string vacua."

As usual, the problem with everyone working on the same question is "what if this doesn't go anywhere"?

11. **DB**
July 16, 2019

Dom, Peter,
Trying to blame specific people (which we all know who they are, Peter being one of them) for what happened at Strings 2019 is not just a joke, it's unfair. Not the least because those people had nothing to do with organizing the conference. Not sure why the author of that web page came up with this ridiculous idea.

Wouldn't it be easier to accept that string theory, as is formulated nowadays, has hit a brick wall? Not sure if a temporary one or not, but it's definitely not progressing as it was expected a decade or two ago…

Acknowledging limitations is a good thing. Even Witten was asking for something more modest in his comment re the "vision"…

12. **Peter Woit**
July 16, 2019

DB,
Part of Lubos's complaint is that theorists who think they see how to get a unified theory out of string theory were not invited to Strings 2019 and not represented on the "vision" panel. As he discusses, one reason for this is that there is a separate "String Phenomenology 2019" conference where such people were represented.

I found it remarkable that Arkani-Hamed completely ignored this "string phenomenology" and I don't see any possible explanation for this other than that he thinks it doesn't work, is not able to give a viable connection of string theory to experiment. I think Lubos is right that the lack of representation at the conference or on the panel of people claiming to get a unified theory out of string theory probably reflects the views of the organizers. To his mind, they are wrong and have been cowed into submission by me and others, to my mind, they are right to not bring people in to discuss failed ideas.

The "vision" panel would have been a lot more interesting if the question Gross put to everyone was formulated as "give that string theory unification has hit a brick wall, what should we do?".

13. **Thomas Van Riet**
July 16, 2019

@Peter, if the dust settles then one might potentially get to "no B-modes" within the detectable range.

14. **Peter Woit**
July 16, 2019

Thomas Van Riet,
So, in this video
https://www.youtube.com/watch?v=ZlfIVEy_YOA
they should have not only been informing Linde that inflation had been discovered, but at the same time informing Kallosh that string theory was falsified?

15. **Sabine Hossenfelder**
July 17, 2019

The most remarkable thing about this is how hard they avoid saying "we got this wrong" (prospects to experimentally test strings/susy) even though the claims that turned out to be wrong are all over the published literature (and collected here on Peter's and on my blog), not to mention in the media.

It's concerning because without acknowledging that something went wrong in the first place, they'll never be able to learn from their mistakes. This is particularly obvious when it comes to the claim that now it's actually good news the LHC hasn't found susy. (Sure, hahaha.)

Frankly I think Nima understood the situation long ago, but people like to invite him for motivational speeches, at which he is admittedly good.

LMMI,

I see a very pronounced generational divide in the responses to my talks (which are not about string theory, I should add, but about the stagnation in the foundations of physics). While there are exceptions on either side, of course, by and large the older people are patently unwilling to even think about what I am saying (will frequently repeat arguments I just explained are wrong, it's quite comical actually) while the younger people may not agree but at least they see the need to think about what is going on (for the obvious reason that their career prospects depend on them getting it right).

While that's good news to some extent, the reality of academia is that it would take at least two more decades for power to shift to the younger generation if that was the only way self-correction in scientific communities takes place. One would hope that it was not the only way.

16. **shantanu**
July 17, 2019

Peter, the only time I have heard a seminar from Ed Witten (in 2005, cf. http://physics.bu.edu/festschrift/) he talked about proton decay in string theory models.
Is he or other people still working along these lines and how much recent work is going on? I don't know how many such talks have been featured in Strings xx meetings.

17. **Peter Woit**
July 17, 2019

As far as I know, neither Witten nor anyone else at the IAS has worked on this kind of "string phenomenology" in a long time, since it is clear that, for instance for proton decay, string theory is completely unpredictive: you can get any result you want. This understanding is widespread among prominent string theorists such as Witten, and that's why no one was invited to Strings 2019 to discuss such issues.

18. **Erickson Tjoa**
July 24, 2019

Hi Peter,

You mentioned earlier that "Nati Seiberg also pretty much ignored string theory, sticking to the quite accurate point that there is a huge amount about QFT that we don't understand, and making progress there is what is most promising."

Is this talked about at Strings 2019 and what are some of the things Seiberg (or you) found to be worth making progress for on that front?

19. **Math Phys**
July 24, 2019

"Isn't it just important to have fun now?".

How visionary!

20. **Peter Woit**
July 24, 2019

For Seiberg's views, see his talk
https://livestream.com/streaming/events/8742238/videos/193713207

Also, he is one of the co-organizers of TASI 2019, which focused on problems in QFT. See the videos from that conference, available here

https://physicslearning.colorado.edu/tasi/tasi_2019/tasi_2019.html

Quick Links

July 24, 2019
Categories: Uncategorized

A few quick links:

- Philip Ball at Quanta has a nice article on "Quantum Darwinism" and experiments designed to exhibit actual toy examples of the idea in action (I don't think "testing" the idea is quite the right language in this context). What's at issue is the difficult problem of how to understand the way in which classical behavior emerges from an underlying quantum system. For a recent survey article discussing the ideas surrounding Quantum Darwinism, see this from Wojciech Zurek.

  Jess Riedel at his blog has a new FAQ About Experimental Quantum Darwinism which gives more detail about what is actually going on here.

- This year's TASI summer school made the excellent choice of concentrating on issues in quantum field theory. Videos, mostly well worth watching, are available here.
- This month's Notices of the AMS has a fascinating article about Grothendieck, by Paulo Ribenboim. It comes with a mysterious "Excerpt from" title and editor's note:

  > Ribenboim's original piece contains some additional facts that are not included in this excerpt. Readers interested in the full text should contact the author.

- I've finally located a valuable Twitter account, this one.

Comments

1. **Pascal**
   July 25, 2019

   If someone learns about the "additional facts" on Grothendieck (and Peter doesn't mind) could they please share their findings in the comments section?
   Thanks in advance!

2. **Andrew Krause**
   July 25, 2019

   Yeah it would be worth asking Ribenboim if they are willing to publicly share the unedited version, or otherwise whatever was removed. If so I would also be keen to see what was removed.

3. **Mateus Araújo**
   July 28, 2019

   I'm really happy with Ball's article. Finally someone has explained correctly what decoherence (and Quantum Darwinism) is and what it isn't, that it does explain how interference becomes unobservable and the classical results stable, and that it cannot explain the emergence of a unique outcome.

   The idea that decoherence somehow solves the measurement problem or explains the collapse of the wavefunction is a common misconception even among people working in the field, and this article might help dispel it.

4. **Pascal**
   July 29, 2019

   "The idea that decoherence somehow solves the measurement problem or explains the collapse of the wavefunction is a common misconception."

   It does explain how several possible classical worlds emerge from one quantum world.
   It remains to explain the selection of a single classical outcome.
   This problem is "solved" in MWI by postulating that all possible outcomes are realized.
   In this way, decoherence provides a detailed, microscopic description of the way MWI could work.

5. **Another Anon**
   July 30, 2019

   Mateus, I think decoherence does a lot more than you suggest to solving the measurement problem. True, it cannot predict which eigenstate will be observed (that remains probabilistic), but it can explain why only a single state (one of the eigenstates) is observed, so it can explain why "interference becomes unobservabke" and "the classical results stable".

   We can understand it mathematically. Eigenstates (which Zurek calls "pointer states" in the article) are more robust because if you apply an operator to an eigenstate, the state remains unchanged, so "classical results stable". In contrast, any state which is not an eigenstate gets destroyed by the random state phase during the decoherence process, so "interference becomes unobservable".

   As I say, decoherence cannot predict which eigenstate will be observed, but decoherence comes pretty darn close to solving the measurement process. Certainly to the point where it appears that there is no "big unsolved mystery" here, no infinity of parallel universes created every time I measure anything.

6. **Peter Woit**
   July 30, 2019

   All,
   Comments about quantum Darwinism welcome, but not the usual arguments about more general issues in measurement theory. About MWI in particular, I'm likely to write something within the next couple months about Sean Carroll's forthcoming book, and that would be a better time to discuss MWI.

7. **Mateus Araújo**
   July 31, 2019

   Another Anon,

   The point is not predicting the outcome (solving the measurement problem is not about predicting the outcome), but explaining why only a single outcome is observed.

As Ball points out, decoherence only eliminates interference between alternatives, it doesn't single out any alternative, deterministically or otherwise. It couldn't, as decoherence can only delete off-diagonal elements of the density matrix, and to single out an alternative one needs to delete diagonal elements.

I'm afraid our host is getting annoyed by the constant discussion of MWI here, so maybe it's better to leave it for the post on Carroll's book.

8. **Low Math, Meekly Interacting**
July 31, 2019

The article may have alluded to this problem…

"Horodecki and other theorists have also sought to embed QD in a theoretical framework that doesn't demand any arbitrary division of the world into a system and its environment…"

Some folks seem to believe this need for division makes QD entirely circular:

"…'classical' pointer states do not emerge unless a key aspect of classicality has been tacitly assumed from the beginning…quantum Darwinists smuggle in classicality via their partitioning of the universe into distinguishable systems of interest that interact with mutually randomized environmental subsystems."

https://physicstoday.scitation.org/doi/full/10.1063/PT.3.2760

What think you? Fatally flawed or not?

9. **Peter Shor**
July 31, 2019

LMMI:

I think the ultimate hope of the proponents of quantum Darwinism is that, given a quantum system with dynamics, you can recover the partition into different subsystems that gives pointer states, and that this partition will be unique.

Has this been demonstrated yet? I don't believe so, but it certainly doesn't seem out of the question.

Finally, I suspect that there are probably some quantum dynamics which don't actually give pointer states. I don't know what quantum Darwinism says about these, but it seems to me that they should be investigated from a quantum Darwinistic point of view.

10. **Pascal**
August 1, 2019

About circularity: maybe the goal is to show that if we start from a classical-looking universe like ours and apply a quantum evolution, classicality is preserved (for instance, we will never observe the proverbial deal-and-alive cat)?
This looks like a reasonable goal.

If on the other hand they want to show that the "classical pointer states" emerge from some arbitrary (non "classical looking") initial quantum state, then it looks like a much more dubious goal to me.

Is it the first or second goal that the darwinists have in mind? Or something else entirely?

11. **Peter Shor**
August 2, 2019

Pascal: I don't know whether all the quantum Darwin people have the same goal.

Certainly, if you start with a high-temperature thermal (i.e., completely random) initial state, then it will stay thermal. But if you start with a low-energy state, you might be able to prove something.

12. **Arnold Neumaier**
August 4, 2019

Mateus Araújo,

as you write, decoherence cannot explain why only a single outcome is observed. The deeper reason for this is that quantum mechanics in the traditional interpretations has not even the means to express what it means to have a single outcome.

To explain the occurrence of single outcomes one needs an interpretational device that can talk about this. This is done in my thermal interpretation (already twice mentioned in Peter's blog); see the 5 preprints listed at
https://www.mat.univie.ac.at/~neum/physfaq/therm/
Essentially, decoherence tells roughly the same the same story as the thermal
interpretation, but only in statistical terms, whereas the thermal interpretation refines this to a different, more detailed story for each single case. This is possible since in the thermal interpretation, outcomes are defined as macroscopic expectations approximating the microscopic quantities to be measured, and q-expectations are always single-valued. This makes a big difference in the interpretation of everything!

13. **Jarek Korbicz**
August 22, 2019

One comment re the Quanta article on quantum Darwinism. The statement at the end:

"Spectrum broadcast theory (which has only been worked through for a few idealized cases) "

is not exactly correct in the following sense. Spectrum Broadcast Structures (SBS) have been theoretically found by the Gdansk group in *all* the models where quantum Darwinism (qD) was earlier predicted, apart from the NV centers (we are working on that). And in several more (QED, gravitational decoherence). Actually one of the pillars of the SBS program is to check as many models as doable to gather a theoretical evidence in their favor. So the SBS "has only been worked through for a few idealized cases" to the very much the same extent as qD. An arXiv search on my name will give the relevant papers. I wrote a comment to Quanta but it didn't seem to get through to the Editors.

Where We Are Now

August 6, 2019
Categories: Uncategorized

For much of the last 25 years, a huge question hanging over the field of fundamental physics has been that of what judgement results from the LHC would provide about supersymmetry, which underpins the most popular speculative ideas in the subject. These results are now in, and conclusively negative. In principle one could still hope for the HL-LHC (operating in 2026-35) to find superpartners, but there is no serious reason to expect this. Going farther out in the future, there are proposals for an extremely expensive 100km larger version of the LHC, but this is at best decades away, and there again is no serious reason to believe that superpartners exist at the masses such a machine could probe.

The reaction of some parts of the field to this falsification of hopes for supersymmetry has been not at all the abandonment of the idea that one would expect. For example, today brings the bizarre news that failure has been rewarded with a [$3 million Special Breakthrough Prize in Fundamental Physics for supergravity]. For uncritical media coverage, see for instance [here], [here], and [here].

Some media outlets do better. I first heard about this from Ryan Mandelbaum, who writes [here]. [Ian Sample at the Guardian] does note that negative LHC results are "leading many physicists to go off the theory" and quotes one of the awardees as saying:

> We're going through a very tough time… I'm not optimistic. I no longer encourage students to go into theoretical particle physics.

At [Nature], the sub-headline is "Three physicists honoured for theory that has been hugely influential — but might not be a good description of reality" and Sabine Hossenfelder is quoted. [At her blog], she ends with the following excellent commentary:

> Awarding a scientific prize, especially one accompanied by so much publicity, for an idea that has no evidence speaking for it, sends the message that in the foundations of physics contact to observation is no longer relevant. If you want to be successful in my research area, it seems, what matters is that a large number of people follow your footsteps, not that your work is useful to explain natural phenomena. This Special Prize doesn't only signal to the public that the foundations of physics are no longer part of science, it also discourages people in the field from taking on the hard questions. Congratulations.

In related news, yesterday I watched [this video] of a recent discussion between Brian Greene and others which, together with a lot of promotional material about string theory, included significant discussion of the implications of the negative LHC results. A summary of what they had to say would be:

- Marcelo Gleiser has for many years been writing about the limits of scientific knowledge, and sees this as one more example.
- Michael Dine has [since 2003] been promoting the string theory landscape/multiverse, with the idea that one could do statistical predictions using it. Back then we were told that "it is likely that this leads to a prediction of low energy supersymmetry breaking" (although Dine soon realized this wasn't working out, see [here].) In 2007 Physics Today published his [String theory in the era of the Large Hadron Collider] (discussed [here]), which complained about how "weblogs" had it wrong that string theory had no relation to experiment. That piece claimed that

  > A few years ago, there seemed little hope that string theory could make definitive statements about the physics of the LHC. The development of the landscape has radically altered that situation.

  and that

  > The Large Hadron Collider will either make a spectacular discovery or rule out supersymmetry entirely.

  Confronted by Brian with the issue of LHC results, Dine looks rather uncomfortable, but claims that there still is hope for string theory and the landscape, that now big data and machine learning can be applied to the problem (for commentary on this, see [here]). He doesn't though expect to see success in his lifetime.

- Andy Strominger doesn't discuss supersymmetry in particular, but about the larger superstring theory unification idea, tries to make the case that it hasn't been a failure at all, but a success way beyond what was expected. The argument is basically that the search for a unified string theory was like Columbus's search for a new sea route to China. He didn't find it, but found something much more exciting, the New World. In this analogy, instead of finding some tedious reductionist new layer of reality as hoped, string theorists have found some revolutionary new insight about the emergent nature of gravity:

  > I think that the idea that people were excited about back in 1985 was really a small thing, you know, to kind of complete that table that you put down in the beginning of the spectrum of particles…
  >
  > We didn't do that, we didn't predict new things that were going to be measured at the Large Hadron Collider, but what has happened is so much more exciting than our original vision… we're getting little hints of a radical new view of the nature of space and time, in which it really just is an approximate concept, emergent from something deeper. That is really, really more exciting, I mean it's as exciting as quantum mechanics or general relativity, probably even more so.

  The lesson Strominger seems to have learned from the failure of the 1985 hopes is that when you've lost your bet on one piece of hype, the thing to do is double down, go for twice the hype…

Update: The Breakthrough Prize campaign to explain why supergravity is important despite having no known relation to reality has led to various nonsense making its way to the public, as reporters desperately try to make sense of the misleading information they have been fed. For instance, you can read (maybe after first reading [this comment]) [here] that

> Witten showed in 1981 that the theory could be used to simplify the proof for general relativity, initiating the integration of the theory into string theory.

You could learn [here] that

> When the theory of supersymmetry was developed in 1973, it solved some key problems in particle physics, such as unifying three forces of nature (electromagnetism, the weak nuclear force, and the strong nuclear force)

Update: On the idea that machine learning will solve the problems of string theory, see [this yesterday from the Northeastern press office], which explains that the goal is to "unify string theory with experimental findings":

> Using data science to learn more about the large set of possibilities in string theory could ultimately help scientists better understand how theoretical physics fits into findings from experimental physics. Halverson says one of the ongoing questions in the field is how to unify string theory with experimental findings from particle physics and cosmology…

Update: Physics World has a [story about this] that emphasizes the sort of criticism I've been making here.

As mentioned in the comments, I took a closer look at the citation for the prize. The section on supersymmetry is really outrageous, using "supersymmetry stabilizes the weak scale" as an argument for SUSY, despite the fact that this has been falsified by LHC results.

Update: Jim Baggott writes about this story and post-empirical science here.

Noah Smith here gets the most remarkable aspect of this right. String theory has always had the feature that the strings were not supposed to be visible at accessible energies, so not directly testable. Supersymmetry is quite different: it has always been advertised as a directly testable idea, with superpartners supposed to appear at the electroweak scale and be seen at the latest at the LHC. Giving a huge prize to a theoretical idea that has just been conclusively shown to not work is something both new and outrageous.

Update: Tommaso Dorigo's take is here, which I'd characterize as basically "any publicity is good publicity, but it's pretty annoying the cash is going to theorists for failed theories instead of experimentalists"(he does say he wanted to entitle the piece "Billionaire Awards Prizes To Failed Theories"):

> An exception to the above is, of course, the effect that this not insignificant influx of cash and 23rd-hour recognition has on theoretical physicists. For they seem to be the preferred recipients of the breakthrough prize as of late, not unsurprisingly. Apparently, building detectors and developing new methods to study subnuclear reactions, which are our only way to directly fathom the unknown properties of elementary particles, is not considered enough of a breakthrough by Milner's jury as it is to concoct elegant, albeit wrong, theories of nature.
>
> Going back to the effect on laypersons: this is of course positive. Already the sheer idea that you may earn enough cash to buy a Ferrari and a villa in Malibu beach in one shot by writing smart formulas on a sheet of paper is suggestive, in a world dominated by the equation "is paid very well, so it is important". But even more important is the echo that he prize – somewhere by now dubbed "the Oscar of Physics" – is having on the media. Whatever works to bring science to the fore is welcome in my book.

Comments

1. **33lewski**
   August 6, 2019

   I am on holidays in Italy now where in the tv they call it "Oscars in Physics". I must admit that this is a good name, reflecting the promotion of popularity rather than physical aplicability of the theory.

2. **David Roberts**
   August 6, 2019

   > we didn't predict new things that were going to be measured at the Large Hadron Collider

   I thought people in fact *did* predict things that were going to measured at the LHC, and we haven't seen anything like any of them…

3. **Bernhard**
   August 6, 2019

   "big data and machine learning can be applied to the problem ".

   The most overhyped ideas of the moment will come save String Theory. How appropriate.

4. **Peter Woit**
   August 6, 2019

   David Roberts,
   Despite what one often hears, superstring theory has never predicted anything about what the LHC (or any accelerator) would see, for reasons I've gone on about here and elsewhere. What does make predictions is supersymmetry, and if you think supersymmetry solves the naturalness problem, these predictions should show up at the LHC. If supersymmetry doesn't solve the naturalness problem there is no prediction of what energy scale its effects will show up at, and it loses much of its interest.

   The accurate statement has always been that superstring theory predicts nothing, but that, if supersymmetry showed up at the LHC, that would be encouraging for the superstring idea, since supersymmetry is part of the theory. Pre-LHC, hopes for connecting superstring theory to the real world hinged on first finding supersymmetry at the LHC, then hoping that the discovered pattern of superpartners and their masses would somehow provide an effective constraint on possible superstring "vacua". If the constraint was good enough, you could hope to get new predictions out of the superstring.

   Post negative results from the LHC, there are no prospects for connecting string theory to the real world (see the recent post about Arkani-Hamed's talk on the subject). One reaction to this you often hear (from e.g. Witten) is basically "maybe a miracle will happen and we'll see something unexpected at LIGO or whatever." Another is Dine's "let's keep calculating and maybe a miracle will happen." Strominger seems to have adopted a different tactic: abandon the sinking ship and declare victory, announcing that it was a crummy ship anyway, that the new one being built will be infinitely superior, and the old ship will live on since some of its wreckage will be incorporated in the new one.

5. **John**
   August 6, 2019

   Peter,

   Your characterization of what Andy said ("Strominger seems to have adopted a different tactic: abandon the sinking ship and declare victory, announcing that it was a crummy ship anyway, that the new one being built will be infinitely superior, and the old ship will live on since some of its wreckage will be incorporated in the new one.") is completely off base. The proverbial "ship" in your analogy seems to refer to the idea that Strominger said is naiive, i.e. the hope that a simple ToE would be found and fundamental physics would be over. Nice way to straw man his argument.

   If anything, the "ship" would have to be what string theorists work on: string theory. He is by no means advocating abandoning string theory in favor of anything else. And you insinuating that just reveals how insincere and nitpicky your entire smear campaign has been all along.

   Strominger is telling us that string theory has expanded the horizons of physics in a wonderful way, and it is clear that e.g. AdS/CFT is making new strides toward connecting string theory with lots of things closer to the lab bench. Take for example holographic duality in the SYK model. That is a model where one might test things on a lab bench. You're absolutely missing how theoretical physics works, and Nima is correct in stating that non- or former-physicists are out of their depth.

   Regards.

6. **Lino**
August 6, 2019

Peter:

Where does all this leave particle physics? You quote Ian Sample above as saying: "I no longer encourage students to go into theoretical particle physics." What comes next? Do we look to the mathematicians or the experimentalists for the next step forward?

7. **Peter Woit**
August 6, 2019

John,
I don't think I'm misrepresenting Strominger's claims at all. In my analogy the sinking ship is the idea of string theory unification, the idea that string theory will explain the Standard Model and unify it with gravity. Strominger takes the bizarre tack of characterizing such an explanation as not that exciting, just a tedious "reductionist" explanation of some numbers in a table.

Holographic duality in SYK models is likely (among others) the kind of thing Strominger had in mind when he referred to "little hints of a radical new view of the nature of space and time". Trying to take a "little hint" and use it to make a dramatic claim that one has discovered the New World is about as hypey as hype gets.

As for the personal insults, I strongly suggest you leave them to Lubos.

8. **Peter Woit**
August 6, 2019

Lino,
The quote isn't Ian Sample's, it's from Ferrara, one of the awardees. I don't want to start a discussion here of my or other people's favorite idea for where progress may come from, just want to point out that it's been clear for a long time where progress won't come from: trying to get a unified theory out of supergravity (with or without strings). Supergravity is an immensely complex and technical subject, and it's the last thing young theorists should be spending their time learning about, although I fear this award is likely to encourage that.

9. **Angry Andy**
August 6, 2019

In the Nature piece, one reads: *"A lack of evidence should also not detract from supergravity's achievements, argues Strominger, because the theory has already been used to solve mysteries about gravity. For instance, general relativity apparently allows particles to have negative masses and energies, in theory. "If that was true, some things wouldn't fall to Earth when dropped, but fall into space," says Strominger. That does not happen, but no one could explain why not. Turning supergravity's mathematical machinery to general relativity, however, enabled physicists to prove that particles cannot have negative masses and energies. "Those results will hold whether or not supergravity actually exists in nature," says Strominger."*

This is a misrepresentation either of the problem in question, namely, the positive energy theorem in GR, and the history of its solution. (1) Saying that "For instance, general relativity apparently allows particles to have negative masses and energies, in theory. "If that was true, some things wouldn't fall to Earth when dropped, but fall into space," says Strominger" is to ignore years of work from relativists such as Arnowitt, Bondi, Bonnor, Choquet-Bruhat, Deser, Geroch, Misner and Lichnerowicz, only to list a few.
(2) The first proof of the theorem was actually worked out by Schoen and Yau, in "On the proof of the positive mass conjecture in general relativity" (1979) and "Proof of the positive mass theorem. II" (1981), where they employed rather more traditional methods from Geometric Analysis. It's true that Deser and Teitelboim, and then Grisaru, pointed out the positivity of total energy in isolated systems in SUGRA from the fact that its hamiltonian can be written as sum of squares of the hermitian supercharges, and this inspired Witten to later provide a simplified proof of the theorem using spinor methods in "A new proof of the positive energy theorem" (1981) (all published in Comm. Math. Phys.). But even so, Witten's proof was in need to some clarification to be completely rigorous, concerning particularly some analytical properties of Dirac operators, provided by Parker and Taubes, see here:
https://users.math.msu.edu/users/parker/Witten.pdf
Moreover, spinor methods have been applied to General Relativity at least from 1960 by Roger Penrose, see e.g.:
https://www.sciencedirect.com/science/article/pii/000349166090021X
I don't think Penrose needed SUGRA for this.

10. **Amitabh Lath**
August 6, 2019

There are two ways to interpret LHC results (140 fb^-1, !!). First, there are no more discoveries to be made; 2012 saw the final chapter in a story that began with the ancients and featured Mendeleev and Rutherford and Feynman and Weinberg and so many others. It's all over, pack it up. Tell the smart kids to go into cancer research or finance.

Or second, the experimentalists don't know how to look properly. We are pretty good at looking where we are told. Top, higgs decays were precisely modeled and the analyses beautifully crafted to look for exactly that. In this new era we continue to search for new physics by doing what we know: resonances, missing momenta, excess photons, electrons, muons or heavy quarks. Almost all LHC searches would look perfectly reasonable to someone from 50 years ago. But nature is not bound by our limitations.

Look, new physics is going to pop up. It might be something more complicated than a Ting-like bump on a nice flat background. We are collectively moving beyond basic techniques, and it is a really exciting time.

11. **Peter Woit**
August 6, 2019

For those interested in the exact relation of Witten's proof to supergravity, see section IV of his paper
https://projecteuclid.org/euclid.cmp/1103919981
which explains this in detail (and is consistent with what "Angry Andy" writes above).

12. **Sabine Hossenfelder**
August 6, 2019

Your quote from the Dine article ("A few years ago…") contains the words "absurd misinformation" which don't seem to belong there.

13. **Peter Woit**
August 6, 2019

Thanks Sabine,
Fixed.

14. **Lowell Brown**
August 7, 2019

Many years ago we received a letter of recommendation of recommendation form Sidney Coleman. I remember only two short sentences in the letter:

I am uninterested in gravity. I am super-uninterested in super-gravity.

I believe that a few years later Sidney did not retain this purity of soul.

15. **Chris K.**
August 7, 2019

Peter,

If there was any evidence that supergravity has anything to do with reality, Ferrara, Freedman and van Nieuwenhuizen would be getting a Nobel prize instead of the Breakthrough one. The goal of the latter is to recognize important theoretical discoveries in physics, which do not necessarily need to make contact with experiments. You can criticize the purpose of such a prize, but the fact that discovery of supergravity was a milestone towards better understanding of QFTs is rather indisputable. Among other things, to this day N=8 SUGRA remains the only four-dimensional theory of gravity that has a shot at being renormalizable. To me (as someone working on numerical GR) this fact is completely remarkable!

16. **TOM WEIDIG**
August 7, 2019

Given that the field is in a conceptual turmoil, given that the breakthrough has been promised for decades, and given that everyone seems to have given up hope for the next few years now that the LHC results are in, I expect a revolutionary breakthrough to happen soon! 😉

17. **Low Math, Meekly Interacting**
August 7, 2019

I thought the LHC was supposed to confirm the mechanism of EWSB, solve the hierarchy problem, uncover a new fundamental symmetry of nature and discover the LSP/dark matter. That was a "small" thing?

18. **Peter Woit**
August 7, 2019

Chris K.,
The problem is that if you're going to award a huge prize for theory not backed by experiment, you have a big problem of deciding what is good theory and what is bad theory. Given my interests, I've nothing against handing out huge prizes in mathematical physics, but if you do this you need to face up to some difficult problems of evaluation, and be careful to explain to the press that what you are rewarding is not success at explaining nature, but something different.

Note that the last two Breakthrough Prizes in physics went to solid state theory work that is connected to experiment, and to the WMAP experiment. The Breakthrough Prize is not promoted as a prize for work that can't be tested, quite the opposite. It is extremely misleading to the public to tell them that the construction of a supergravity theory and the WMAP experiment are the same sort of thing.

Have you read the citation for the supergravity prize? It explicitly claims that the significance of this work is based on the fact that supersymmetry
"offers solutions to some of those perplexing puzzles in the Standard Model, including a mechanism explaining the tiny particle masses, and a natural candidate for dark matter, which – like the hypothesized super-bosons – is massive but invisible."
This is just completely outrageous: the conjecture that SUSY "[explains] the tiny particle masses" (i.e. stabilizes the weak scale with respect to the Planck scale) always was highly problematic, and the work at the LHC has shown this is not true. What's going on here is much worse than giving a prize to an untested or untestable idea, it is explicitly giving a huge prize for an idea, based on an argument for its significance which experiment has just falsified. This is really bad for both the public credibility of the subject, as well as for the ongoing problem of getting it to face up to its failures.

19. **Peter Woit**
August 7, 2019

LMMI,
Actually I think what Strominger was referring to as a "small thing" was the 1985 hope to explain the spectrum of elementary particles and compute their masses and interaction strengths. By his measurement scale, the things you mention would not be small, but microscopic…

20. **Lee Smolin**
August 7, 2019

Dear Lowell Brown,

I was told by Bryce deWitt that I was the subject of that letter of Sidney's. There was just one more line…

Why is no one mentioning the simultaneous, and much simpler, construction of supergravity by Deser and Zumino? As students at the time we found much to admire from both results!

Thanks,

Lee

21. **Peter Woit**
August 7, 2019

Lee,
Philip Ball in his piece at Scientific American, writes
"One potentially controversial aspect of the decision is that the supergravity picture was also formulated independently by Bruno Zumino, a pioneer of supersymmetry, and Stanley Deser, now at Brandeis University, who also published their work in 1976—initiating disputes over priority. Zumino died in 2014, but the omission of Deser

from the award seems puzzling, Duff says, given that there is no restriction on the number of recipients."
Looking at a recent talk about the history of supergravity by Van Nieuwenhuizen, I'm guessing he might point out that his paper with Ferrara and Freedman was submitted March 29, 1976, the Deser/Zumino paper April 28.

22. **Peter Woit**
August 7, 2019

For Deser's view of the story of the discovery of supergravity, see
https://arxiv.org/abs/1704.05886

23. **TG**
August 7, 2019

I think S. Deser's final comment in the above-cited paper beautifully summarizes the situation; too bad it wasn't cited by the nominating committee…

"In conclusion, I quote Chou-En-Lai's reply to a query as to the effects of the then 200 year-old French revolution: "it's too early to tell": SUGRA is a beautiful set of theories, very much broken in the real world, yet with many useful and unexpected lessons in theory-building past and future."

24. **Marc Nardmann**
August 7, 2019

Witten's 1981 proof (or rather: detailed proof sketch; the analytic parts were filled in by Parker and Taubes in 1982) of the positive mass/energy theorem (PMT) is a very important result in mathematical general relativity and differential geometry. It was extremely influential. Witten's spinor proof has nothing to do with supergravity (or superanything), though. In fact, Witten added at the end of his article a section that begins with the following sentence: "In this section a few speculative remarks will be made about the not altogether clear relation between the previous argument and supergravity." If you cut that section from the article, you won't miss anything. The proof itself does not involve supergravity at all.

That the spinor proof was found only in 1981, two years after Schoen and Yau had given a different proof, is a historical accident. It could easily have been found in the mid-1960s, long before the discovery of supergravity: As inspiration, you just need Lichnerowicz' proof that certain compact spin manifolds without boundary do not admit a Riemannian metric with positive scalar curvature (1963); and the Arnowitt/Deser/Misner (ADM) definition of the mass/energy of an asymptotically flat spacetime (1960–1962). Knowing these things, you will feel a strong urge to figure out whether the main formula in Lichnerowicz' proof has a generalisation to certain compact spin manifolds with nonempty boundary. If you are moderately clever (Witten was certainly overqualified in that respect), you compute the boundary term in the case where the boundary is a large sphere in an asymptotically flat spacetime, notice that this term involves the ADM mass/energy, and thus get the strategy for the spinor proof of the PMT. You ask your local analysis expert why the PDE you obviously must solve has a solution, and then you're done.

The Breakthrough Prize announcement claims that supergravity "can be used to give a rather simple proof" of the PMT. This is like saying that apples can be used to give a rather simple theory of gravitation. Newton might have had an important idea for his theory while watching apples falling from a tree. But without apples, he would probably have had the same idea a week later, sitting on a privy, contemplating other falling objects. In an analogous sense, Witten most likely didn't need supergravity as an inspiration to come up with his PMT proof.

25. **Neo**
August 8, 2019

Dear Peter,

At Nature, "In 2013, for example, Stephen Hawking won for his theory — also still untested experimentally — that black holes give off radiation."

What do you think about this "justification" for supergravity ?

26. **Peter Woit**
August 8, 2019

Neo,
The problem with the Breakthrough Prize justification of an award for supergravity as an idea about unification is not that it's untestable, but that it has been tested and failed the tests. This isn't a compelling, hard to test idea, it's an idea that now has a lot of evidence that it's wrong.

Also relevant is the distinction Sabine Hossenfelder makes here between different sorts of untestable ideas:
https://twitter.com/skdh/status/1159032052320284673

27. **Bill**
August 8, 2019

Considering who decides these prizes, former recipients just made theirs look less unjustified by comparison.

28. **vmarko**
August 9, 2019

Speaking of prizes and levels of hype, yesterday the ICTP Dirac medal was awarded to Mukhanov, Starobinsky and Sunyaev, for inflation and CMB:

https://www.ictp.it/about-ictp/media-centre/news/2019/8/dirac-medal.aspx

Looking at the article, I was pleasantly surprised by the lack of hype and multiple comments about the agreement between theory and observations. But perhaps most surprisingly, while inflation is said to explain the uniformity and flatness of the Universe, there is absolutely no mention of the "magnetic monopole problem" – a rarely sober take on inflationary cosmology.

Best, 🙂
Marko

29. **Jim Baggott**
August 9, 2019

My take, just posted to Prospect Magazine's science blog. It's pretty much as you'd expect. https://www.prospectmagazine.co.uk/science-and-technology/prize-for-speculation-goes-to-how-foundational-physics-went-down-a-post-empirical-dead-end-science-prize

30. **Luca Coluzzi**
    August 10, 2019

    Regarding the matter, I heard an interview from the national italian radio broadcast (radiorai) to Ferrara, in which he says, among other things, that “likely” SUSY will be found in the next ten years, draws a comparison between the search for SUSY and the discovery of the Higgs Boson, and criticizes LQG.

    This is the web address of the interview (in italian, sorry)

    https://www.raiplayradio.it/audio/2019/08/RADIO3-SCIENZA-del-09072019—Un-superpremio-alla-supergravit195160-dc3fbb42-7089-4e48-800e-02fbe06cdaa8.html

31. **BrunoMAGAglitch**
    August 10, 2019

    Like much of theoretical physics, prizes from billionaires are sociological phenomena.

    Does this prize impede or enhance the execution of experiments with substantial discovery potential? That matters the most. Could go either way.

    By and large, theoretical high energy physicists don’t have any idea how to accurately estimate discovery potential.

    Meanwhile, the US neutrino budget is in turmoil, with ripples in $ that dwarf the chump change of the Milner prize.

32. **Jackiw Teitelboim**
    August 10, 2019

    Thank you for the link, Luca Coluzzi.

    I’m not a native Italian speaker, but if I understood Ferrari’s speech around the 14th minute of that interview correctly, Ferrari criticism of LQG (which the interviewer attributed to Carlo Rovelli) is, ironically, that it’s not predictive, while SUGRA and strings are, since they predict the existence of new particles!

33. **Lee Smolin**
    August 11, 2019

    Dear Jackiw Teitelboim,

    There are other ironies here: First of all, the LQG quantization of GR extends directly to give a quantization of supergravity, at least at N=1, including a treatment of d=11 supergravity. So supergravity and LQG are compatible, there is no conflict between them.

    Second, LQG evolved out of supergravity, as the key result that the hamiltonian formulation is polynomial when expressed in terms of the chiral connection arose in an attempt by Amitaba Sen to formulate a lattice regularization of supergravity.
    (For this reason the relation between supergravity and Witten’s proof of positive energy is particularly transparent in Ashtekar variables.) There is a small literature on supergravity in LQG for those interested.

    And yes, LQG permits a variety of couplings to matter and Yang-Mills fields (modulo some constraints.)

    Thanks,

    Lee

34. **evermasterx**
    August 12, 2019

    Yes, Jackiw Teitelboim,
    that is what Ferrara said.

35. **Amitabh Lath**
    August 12, 2019

    Tommaso Dorigo is right. The reason for not giving prizes to experiment is “there too damn many of you”. The whole system of giving prizes for science is still in thrall to the absurd romantic notion of the lone genius (at most two or three).

36. **Peter Woit**
    August 12, 2019

    Amitabh Lath,
    It’s not so much that there are too many of you, it’s that these days you don’t have enough of a star system. The goal of the Breakthrough Prize people is to promote fundamental physics by creating and publicizing “stars”. If you would just pick one or two people in your collaboration and identify them as the “stars”, you too could get more prizes.

37. **Bernhard**
    August 12, 2019

    Peter,

    ” If you would just pick one or two people in your collaboration and identify them as the “stars”, you too could get more prizes.”

    I don’t think the error is on the collaborations side. Actually, I think the opposite, collaborations should work to diminish the “stars” they already have (spokespersons) since their personal scientific contributions are 99% of the cases disproportional to the attention and prizes (like Nobel) they get.

    This winner takes all attitude and celebrity culture is the real problem here, and this BS prize makes it a million times worse. Moreover, be it or not what Milner cares about, it does reinforce the “romantic notion of the lone genius ” on the public’s imaginary.

    If you add to all of this the absurd idea that these prizes are now going for failed theories, nothing at all is positive to me (unlike Tommaso thinks). It’s made even worse by the fact they try to add confusion by rewarding real scientific discoveries in the mix.

    It’s a complete fiasco.

38. **Pankaj**
August 13, 2019

Nobel prize recognizes ideas that have succeeded. It may not be a bad idea to reward by some mechanism those ideas that have had enormous influence, but may not turn out to be correct. This will be a different issue how to "measure" influence. By almost any measure, idea of Supergravity has made major impact.

39. **Peter Woit**
August 13, 2019

Pankaj,
What personally attracted me to science when I was young was that it was about the search for a truth independent of what was popular or not popular. Having a huge prize for an idea being popular, even if it is not true, is an attempt to change this conventional understanding of what science is, and I think that should be resisted.

This is also peculiar in another way: often the reason for instituting a new prize is to reward work that is not already getting rewarded. Having a popular idea is what gets you all of the conventional rewards of academic life, so why add another reward for what is already rewarded?

40. **WTW**
August 13, 2019

Peter,
How about a prize for "The Theoretical Idea that has wasted more months of graduate students' time than any other over the last decade(s)"?
That could apply to both experimentalist and theoretician graduate students, and perhaps be expanded to include post-docs as well.

41. **Daniel Mittleman**
August 13, 2019

If you're going to award prizes for ideas that are wrong, then why stop with only those ideas which are now known to be wrong, but which were once thought to have some possibility of being correct? Why not award a prize for ideas that were known to be wrong at the very start? I'm thinking of somebody like Jan Hendrik Schoen, who inspired a great deal of experimental work around the world with his fantastic ideas, all of which were deliberate fabrications. Who are we to distinguish among the various flavors of wrong?

42. **no idea**
August 14, 2019

The sad truth is that if you are going to award prizes in this area, then you have to award prizes for ideas that are wrong, because there aren't any others.

43. **Anom**
August 14, 2019

Setting aside issues of whether or not it is right, is supergravity really even all that brilliant of an idea? Supersymmetry was a great idea, but after that it's kind of obvious that you should look for a theory of gravity with supersymmetry. A technical feat to be sure, but worthy of a big prize?

44. **Low Math, Meekly Interacting**
August 15, 2019

Although it's not nearly as lucrative, is the Dirac Medal considered more prestigious than the Breakthrough Prize?

Just as an example, Sen, Strominger and Veneziano won the 2014 Medal for "crucial contributions to the origin, development and further understanding of string theory." Actually, it appears many of the medalists won for theoretical work that is entirely speculative and arguably empirically falsified, to the extent that any idea like String Theory or SUSY can be falsified.

Handing out awards for brilliant theoretical work that either leads to falsified predictions, or evades any attempt at empirical validation through infinite malleability, does not appear to be unprecedented. Seemingly the practice is generating something of a monetary arms race, and that's the novelty.

45. **Peter Woit**
August 15, 2019

LMMI,
Yes, academia and science has always had a large number of prizes (the number is increasing) of all sorts, a bunch of them for theory and math/physics. There actually are three different "Dirac Medals" of this kind. The same supergravity group was awarded the ICTP version back in 1993, and has gotten a bunch of other similar ones together (Dannie Heineman Prize, Ettore Majorana Medal).

There are far more of these prizes than there are truly important successful ideas, so of necessity they end up rewarding lesser achievements and ones that not everyone would agree on. The difference is in the money and publicity campaign behind the Breakthrough Prize. When they won the ICTP Dirac Medal the three got an inconsequential amount of money and a few articles in places like Physics Today. For the Breakthrough Prize, it's articles in the Guardian and \$1 million together with a big PR campaign (including recruiting journalist pre-announcement to write many positive articles timed to appear at the announcement).

The question of whether this money and PR campaign will be successful is still up in the air. I noticed that many important media outlets decided not to cover this (e.g., nothing in the New York Times). In the future, which will carry the most positive reputational weight, a Dirac Medal or a Breakthrough Prize? The jury is still out, but I think unfortunately at the moment the Milner/Zuckerberg millions are speaking the loudest.

46. **David Roberts**
August 15, 2019

OT, but thank you, Peter, for resizing the "only the details are missing" picture to make the site design more uniform.

What's the difference between Copenhagen and Everett?

August 18, 2019
Categories: Quantum Mechanics

I've just finished reading Sean Carroll's forthcoming new book, will write something about it in the next few weeks. Reading the book and thinking about it did clarify various issues for me, and I thought it might be a good idea to write about one of them here. Perhaps readers more versed in the controversy and literature surrounding this issue can point me to places where it is cogently discussed.

Carroll (like many others before him, for a recent example see here), sets up two sides of a controversy:

- The traditional "Copenhagen" or "textbook" point of view on quantum mechanics: quantum systems are determined by a vector in the quantum state space, evolving unitarily according to the Schrödinger equation, until such time as we choose to do a measurement or observation. Measuring a classical observable of this physical system is a physical process which gives results that are eigenvalues of the quantum operator corresponding to the observable, with the probability of occurrence of an eigenvalue given in terms of the state vector by the Born rule.
- The "Everettian" point of view on quantum mechanics: the description given here is "The formalism of quantum mechanics, in this view, consists of quantum states as described above and nothing more, which evolve according to the usual Schrödinger equation and nothing more." In other words, the physical process of making a measurement is just a specific example of the usual unitary evolution of the state vector, there is no need for a separate fundamental physical rule for measurements.

I don't want to discuss here the question of whether the Everettian point of view implies a "Many Worlds" ontology, that's something separate which I'll write about when I get around to writing about the new book.

What strikes me when thinking about these two supposedly very different points of view on quantum mechanics is that I'm having trouble seeing why they are actually any different at all. If you ask a follower of Copenhagen (let's call her "Alice") "is the behavior of that spectrometer in your lab governed in principle by the laws of quantum mechanics" I assume that she would say "yes". She might though go on to point out that this is practically irrelevant to its use in measuring a spectrum, where the results it produces are probability distributions in energy, which can be matched to theory using Born's rule.

The Everettian (let's call him "Bob") will insist on the point that the behavior of the spectrometer, coupled to the environment and system it is measuring, is described in principle by a quantum state and evolves according to the Schrödinger equation. Bob will acknowledge though that this point of principle is useless in practice, since we don't know what the initial state is, couldn't write it down if we did, and couldn't solve the relevant Schrödinger equation even if we could write down the initial state. Bob will explain that for this system, he expects "emergent" classical behavior, producing probability distributions in energy, which can be matched to theory using Born's rule.

So, what's the difference between the points of view of Alice and Bob here? It only seems to involve the question of how classical behavior emerges from quantum, with Alice saying she doesn't know how this works, Bob saying he doesn't know either, but conjectures it can be done in principle without introducing new physics beyond the usual quantum state/Schrödinger equation story. Alice likely will acknowledge that she has never seen or heard of any evidence of such new physics, so has no reason to believe it is there. They both can agree that understanding how classical emerges from quantum is a difficult problem, well worth studying, one that we are in a much better position now to work on than we were way back when Bohr, Everett and others were struggling with this.

Comments

1. **Andrew Krause**
   August 18, 2019

   Can you expand a bit on being in a much better position to study such things now? Is this due to new ways to measure isolated quantum systems experimentally, or due to new ways to explain the theory?

   Relatedly, do you think we will see substantial progress on these "interpretation" issues, and if so where will it (likely) come from?

2. **Peter Woit**
   August 18, 2019

   Andrew Krause,

   Note that when Everett was doing his work, even the notion of decoherence was not understood, so there have been huge improvements since then in our understanding of the theory of how classical emerges from quantum.

   I've often here pointed to the work of Zurek and others, for instance on the notion of quantum Darwinism. For a discussion of recent experimental results related to this, see
   https://www.quantamagazine.org/quantum-darwinism-an-idea-to-explain-objective-reality-passes-first-tests-20190722/
   and
   http://blog.jessriedel.com/2019/07/24/faq-about-experimental-quantum-darwinism/

   A book describing many of the relevant modern experimental techniques is Haroche and Raimond, see
   https://www.oxfordscholarship.com/view/10.1093/acprof:oso/9780198509141.001.0001/acprof-9780198509141

   I know nothing about this, but I've always wondered whether these issues will come up in the design of quantum computers, where in some sense the whole problem is that of a quantum/classical interface (creating isolated qubits presumably is easy enough, but how do you then manipulate them?). I've been told there are serious practical problems with getting significant bandwidth when providing input to or getting output from a quantum computer. Can one learn anything from these (I have no idea)?

3. **orin**
   August 18, 2019

   Ultimately the difference is that Bohr and Heisenberg (inspired by the success of observer-dependence in Einstein's relativity) were anti-realists (they did not believe in a mind-independent objective reality) and used that to argue that quantum mechanics was complete, rather than that "we simply don't understand the details." For example they were OK with objective-level logical contradictions (ala Winger's friend), because they had abandoned realism in favor of subjectivism. More generally this has the consequence of disfavoring whole lines of research into the details, because much of what would ordinarily in the scientific tradition be seen as important issues, such as measurement outcomes not being objective, clearly defining what does and does not constitute a measurement, the contradiction of the measurement apparatus being treated classically, locality in instantaneous collapse in entangled systems, and so on, can be "waved" away by an appeal to antirealism. In this reading, indeed Alice and Bob as you describe them are really on the same side, against Copenhagen. Both can certainly be realists who just "aren't sure" about the details of how the classical emerges from the quantum. Both can be open to various "completions" of the QM story (Bob apparently favors Unitary QM), but both are aware that there can be no local hidden variables due to Bell, so whatever completion there is (within the tradition of scientific realism) is not so mundane as to be reasonably ignored by researchers. Alice could well even be a psi-epistemicist who is an instrumentalist/positivist because she thinks that progress cannot be made on the problem without further empirical constraint, while nonetheless acknowledging that there is some mind-independent machinery that we lack information about, that could potentially be part of an "interpretation" of what is going on behind the scenes. But such a position is generally understood as distinct from the antirealist views of Bohr and Heisenberg, murkily articulated by them though they were, because they would reject that anything more could be going on "behind the scenes" at all, just as there is no "behind the scenes, absolute velocity" in some preferred frame in relativity.

At some point in history this story sort of morphed into an association between "Copenhagen" and "rejection of philosophy" (ironically, given how universally literate and strongly informed the founders were by philosophy, logical positivistic though some of it was), rather than a more accurate association between "Copenhagen" and a rejection of realism. The rejection of a mind-independent reality has a poor reputation in virtually every other scientific discipline, including most of the rest of physics, so this reframing has historically allowed the discipline to sort of ignore what would otherwise produce significant cognitive dissonance.

4. **Peter Woit**
   August 18, 2019

   Orin,
   I'm trying to understand the difference, if any, between "Copenhagen" and "Everett" in the senses (which are fairly conventional) in which Carroll uses the terms. Here "Copenhagen" is what he calls the supposed current majoritarian textbook point of view, and which he also characterizes as the "shut up and calculate" point of view. To the extent Bohr/Heisenberg were arguing for "shut up" on dubious idealist philosophical grounds, I don't think those are shared by the textbook writers. Instead, the "shut up" point of view I think historically was backed by experience that no good could come of arguments about the problem of classical emergence from quantum because these tended to be empty and philosophical. This was a problem for those like Zeh trying to seriously attack the problem using physics.

   If you instead define "Copenhagen" as what Bohr/Heisenberg thought, one problem is that they were confused and struggling, said different things at different times. While using "Copenhagen" to mean one or another reconstruction of what they were thinking is relevant to the history of science, I don't think it's particularly relevant to the question of how physicists today are struggling with the "interpretation" or "measurement" problem of quantum mechanics.

5. **Peter Woit**
   August 18, 2019

   It might be useful for me to reproduce here Carroll's discussion (page 23) of "Copenhagen":

   "In a modern university curriculum, when physics students are first exposed to quantum mechanics, they are taught some version of these five rules [i.e. state space, Schrodinger, observables, Born rule, wave function collapse]. The ideology associated with this presentation – treat measurements as fundamental, wave functions collapse whey they are observed, don't ask questions about what is going on behind the scenes – is sometimes called the Copenhagen Interpretation of quantum mechanics. But people, including the physicists from Copenhagen who purportedly invented this interpretation, disagree on precisely what that label should be taken to describe. We can just refer to it as "standard textbook quantum mechanics."

6. **Doug McDonald**
   August 18, 2019

   Ah! I've never seen an explanation of Everett exactly like Carrolll's. After reading that,
   I see that what I been expounding is exactly that. If Carroll is conventional Everett,
   I'm an Everettian.

   Given that, I can state that the point is that Bob needs to be able to explain to Alice exactly HOW the Born rule works. We know that after a while a thermal system, the apparatus, evolves into probabilities that are Gaussian even after addition of the measured particle. We need a formalism that explains how the Gaussian probabilities of the apparatus select out the probabilities that we know from Born are correct,
   for the "world" that includes the pointer in the correct position. It's different depending on whether its like a spin 1/2 particle, discrete, or position of a photon
   on a screen after passing a double slit, continuous. Its that formalism you should be searching out.

   As far as I know, such a formalism is lacking. But I've found it essentially impossible even to search for the answer to that question.

   Added statement: I have always been mislead, if Caroll is correctly explaining Everett, by the words "split" and "worlds". There is only one world, that of unitary evolution.

7. **orin**
   August 18, 2019

   Peter, you have to understand that by "fairly conventional" you are referring to the few sentences in which the term "Copenhagen" is used in quantum mechanics textbooks, in which the aim is to teach physics students the bare bones of how to do calculations in quantum mechanics. The aim is not to get into any of these thorny philosophical details, which would take an entire course on its own, and would be hard to understand anyways without first learning the von Neumann style rules that the books teach. Further, since there is no wide consensus on which completion of QM is correct, any responsible textbook author is going to fall into a "shut up and calculate"-like position. As such, I think Carroll's description in the quoted text is consistent with what I've said. It's also relevant that to a large extent the textbook writers are not experts in quantum foundations, and so it shouldn't be all that important what they thought (interestingly, one of my favorite quantum textbooks is by Bohm, which happens to adopt the Copenhagen viewpoint, despite him later completely rejecting it). Though again, it's important to read textbooks in the correct context, which is that they are trying to convince the students to focus on the calculations, because there is no consensus pedagogic ontology or heuristic that will help the students learn more than bog them down and prevent the lecturer from finishing the standard topics on time in a quantum mechanics curriculum.

   So it's the wrong perspective to pin down your definition of "Copenhagen" on the textbooks. For an in-depth discussion of interpretations, you want to use the definition relevant to an in-depth discussion, and quantum textbooks aren't the place that in-depth philosophic positions on interpretations are expounded on. One place that in-depth discussion took place was literally in Copenhagen with Bohr and Heisenberg, and which influenced a generation of physicists, hence the name "Copenhagen". Despite Bohr and Heisenberg being opaque writers and not in total agreement, their general position was one of antirealism *and therefore* "shut up because therefore you won't get anywhere", *not* "shut up because philosophy is empty." Of course we all know that whatever the reasons, they were *wrong* about shutting up, as for example Bell showed that Einstein's realist intuitions about locality were correct and that there were testable consequences. But again, this wouldn't have mattered to Bohr, who was antirealist anyway.

8. **orin**
   August 18, 2019

   (I should clarify that I even though I have not read Sean's book, I would assume that with his description of Copenhagen as "textbook QM", he is less taking issue with the textbooks for not critically examining alternate interpretations, but more generally taking aim at the history and culture in physics of ignoring quantum interpretation work as "second class" or "too philosophical", of assuming as Bohr and Heisenberg had argued that "the work is done and QM is complete", a position that is only tenable if you are antirealist)

9. **Physicsphile**
August 18, 2019

I think the hope in many worlds is to prove the Born rule rather than assuming it. See for example:
https://arxiv.org/abs/0906.2718

10. **Sabine Hossenfelder**
August 18, 2019

That the observer cannot see why they're different is the reason people are arguing about it…

They difference is in the part that you don't want to discuss, which is that Everettians postulate the other worlds are real, while Copenhagenists refuses to say anything about what cannot be observed. I am not a fan of the Copenhagen interpretation (I just recently explained on my blog why) but it's philosophically on the safe side. Talking about the existence of non-observable things isn't something I like to see scientists engage in.

(Not so coincidentally, I am currently reading the same book.)

11. **Peter Woit**
August 18, 2019

orin,
If you like, just follow Carroll and replace "Copenhagen" with "standard textbook" in what I wrote. I'm referring to our understanding of the issues today, not that of Bohr and Heisenberg in their discussions in Copenhagen shortly after the discovery of QM nearly a century ago. Yes, their attitude was "shut up because our understanding is that you can't get anywhere", but
1. I don't think that this attitude was just naive anti-realism. It seems likely they had considered the practical issue of what would happen if you tried to treat a spectrometer as a quantum system, and realized they had no idea of how to approach the problem of emergence of the classical from quantum, so were stuck with figuring out what you could say without an understanding of such emergence.
2. Developments since the 20s such as decoherence have shown that there are hopes for making progress on understanding such emergence. As a result I think the "standard textbook" story has evolved to take into account this possibility (my impression is that this is sometimes called "neo-Copenhagen").

I don't think issues about Bell, Einstein, locality and realism have anything to do with this.

Physicsphile,

Yes, the "Everettian" hope is that if you understand classical emergence you'll derive the Born rule. I don't think though that there is now any accepted such derivation. For instance, for a discussion of the problems with the relatively recent Carroll/Sebens attempt at a derivation, see this by Adrian Kent
https://arxiv.org/abs/1408.1944

The conventional "Copenhagen" view of Bohr/Heisenberg would be that you can't hope to derive the Born rule, and the "standard textbook" view is that it's a postulate because there is now no derivation.

I guess one way of answering the question of what the Copenhagen/Everett difference is would be to say that they differ mainly in their guess as to how difficult it will be to understand classical emergence and derive the assumed properties of classical observables, collapse and Born's rule. Given progress on issues like decoherence I don't think the extreme "no progress is possible" position is tenable, so the difference between the two is one of degree.

12. **Peter Woit**
August 19, 2019

Sabine,
I'll be curious to hear what you think about the book!

I do want to put off discussing the "many worlds" business, because to my mind it is about an independent set of complicated issues and I want to think more about them. I'll just point out here that Zurek argues that the Everett point of view does not require adopting a many worlds ontology, and, even Carroll (on page 234) writes: "The truth is, nothing forces us to think of the wave-function as describing multiple worlds". Yes that quote is out of context, and he also makes an argument on the next page that multiple worlds are "enormously convenient". This is where I think I disagree with him, for reasons best discussed separately from the issue I wanted to bring up here.

13. **Alessandro Strumia**
August 19, 2019

Good old books inform that the same issue had been fiercely debated around 1926, when Schroedinger/Einstein wanted to describe everything via a deterministic local equation, getting rid of quantum jumps. Heisenberg/Bohr explained that it's not possible because we see particles as events. Decoherence and all modern stuff allow to understand better but don't change the key point: we need probabilities. So the Schroedinger equation is just a tool for computing probabilities in configuration space. Progress will be possible only if somebody will understand why a particle decays at a given time and choosing a specific final state (directions etc). But, exploring higher energy, nobody found dices.

14. **Jim Baggott**
August 19, 2019

Peter,
I find it helpful to distinguish the standard quantum formalism from any and all attempts to interpret this. It's fair to say that the axiomatic formulation is heavily influenced by the 'Copenhagen school' – the wavefunction provides a complete description, observables as the expectation values of Hermitian operators, the Born rule, the unitary time-dependent Schrodinger equation – but the formalism itself is a set of axioms and mathematical relationships, not an interpretation.

Although I agree with Carroll that there is no such thing as a 'Copenhagen interpretation' on which there was ever any kind of consensus, I think it is reasonable to follow Bohr and Heisenberg, who (rather uneasily) agreed on Bohr's notion of complementarity and a clear distinction between the quantum and classical worlds – sometimes referred to as 'Heisenberg's cut', or what John Bell called the 'shifty split'. Interestingly, I don't accept that von Neumann's projection postulate (the 'collapse of the wavefunction') should be included as part of this understanding of the Copenhagen interpretation. In the 'Mathematical Foundations', von Neumann applied his Process 2 (unitary time evolution according to the Schrodinger equation) also to classical measuring instruments, and only invoked Process 1 (the collapse) when the wavefunction encounters the experimenter's 'ego'. I don't know if Bohr ever had anything to say about this, but he surely wouldn't have liked the idea of applying quantum mechanics to classical objects.

It's therefore wrong in my opinion so say that the Copenhagen interpretation implies the collapse of the wavefunction. Copenhagen is a fundamentally anti-realist interpretation – arguably it doesn't take the wavefunction to represent the real physical state of a quantum system. The 'collapse' then just represents an updating of our experience, not a real physical event. It is only when you prefer to adopt a realist interpretation – as in the MWI – that you are obliged to interpret the collapse realistically. Of course, this hasn't stopped people (from Everett and Bryce De Witt, to Adam Becker in his book 'What is Real?') from accusing the Copenhagen interpretation of all this bad stuff.

The formalism is completely inscrutable on the question of interpretation and, as we know from all the marvellous experiments that have been done in the last 40 years or so, any attempt to *extend* the formalism using local or crypto non-local hidden variables fails to predict the results correctly. Any interpretation must therefore be consistent with the formalism, as we know this is correct. This doesn't mean to say that other realist extensions invoking spontaneous collapse mechanisms (for example) aren't possible, it's just that we haven't properly tested these yet. However, I have some sense of how such experiments might turn out …

So, in terms of the question you posed in your post – no, there is absolutely no difference in the way you apply the time-dependent Schrodinger equation in the standard formalism and in many worlds. But many worlds is a realist interpretation, and as such it struggles to find a realistic explanation for the Born rule, and for the preferred basis. Decoherence (understood as a real physical process) helps to bridge the quantum and classical worlds but I'd argue that it doesn't fix these problems.

And then you have the point that Sabine raised, above. Some 'Everettians' see little or no difference between the different 'branches' of the MWI and the 'empty waves' of de Broglie-Bohm pilot wave theory. Others see the MWI as a useful heuristic, not to be taken too literally. But still others – such as David Deutsch and philosopher David Wallace – want to interpret many worlds literally, in terms of parallel universes or a multiverse. This is where in my book the interpretation gets completely overwhelmed by its metaphysics.

I'm just now going through the final edit of my new book (now titled: 'Quantum Reality: The Essential Meaning of Quantum Mechanics and the Game of Theories') which will hopefully set all this out reasonably clearly so folks can understand the nature of the game that's being played. It will be published next year.

15. **Dave Miller**
    August 19, 2019

    Peter,

    I took QM from Feynman in 1974-75. He was quite explicit that he was making no philosophical point ("ontological commitment," the philosophers would say) in teaching the standard "Shut-up-and-calculate" approach (by the way, the phrase seems to be Dave Mermin's and was not Feynman's, contrary to folk wisdom).

    Feynman told us simply that if we tried to pursue interpretation issues, we would end up finding that we had not made any progress at all: it was not that the questions were necessarily meaningless but just beyond our abilities to answer.

    As to quantum computing: the British physicist David Deutsch, one of the pioneers of theoretical quantum computing, has written on this extensively and is quite convinced that the standard QM theory of quantum computing proves that many-worlds must be true. (For the record, I think he is wrong, but, then again, all I know is that I do not know the true answer!)

    Dave

16. **Jim Baggott**
    August 19, 2019

    Further to my earlier comment, I was sufficiently intrigued to spend a few minutes this morning finding out if Bohr ever said anything about von Neumann's quantum theory of measurement. There's a very brief reference in Abraham Pais' biography of Bohr, in which Pais quotes an entry from one of his old notebooks pertaining to a lecture delivered by Bohr in November 1954 which reads: '[Bohr] thinks that the notion "quantum theory of measurement" is wrongly put'.

    This gels with my own understanding. An anti-realist interpretation which assumes that the quantum representation doesn't apply to classical objects has no use for a quantum theory of measurement. This was never part of the Copenhagen interpretation.

    In his 1970 Physics Today article (the one that really launched 'many worlds') Bryce De Witt claimed that von Neumann's collapse postulate was part of the 'conventional' or 'Copenhagen' interpretation. I'd accept 'conventional', but he was wrong to say 'Copenhagen'. I suspect this was the beginning of attempts to demonize the Copenhagen interpretation and accuse it of all manner of bad things, presumably to make many worlds look good by contrast.

17. **Syksy Räsänen**
    August 19, 2019

    To follow up on Jim Baggott: it makes little sense to identify "textbook quantum mechanics" with the Copenhagen interpretation, since textbooks typically just provide the Born recipe without explanation. In this sense, "textbook QM" can be seen as compatible with Everettian and Copenhagen views, whether or not the two are contradictory.

    In order to meaningfully discuss the issue of compatibility of the Copenhagen and Everett pictures, one should first nail down what one means with the Copenhagen interpretation (as different people mean different things).

    This article about Bohr's vehement opposition to Everett's proposal may be helpful in this regard.

    https://www.sciencedirect.com/science/article/pii/S1355219808000786?via%3Dihub

18. **Pascal**
    August 19, 2019

    Suppose you want to do cosmology and study the extreme situations when quantum effects become relevant. There does not seem to be any meaningful notion of a "measurement" or an "observer" since no scientist can locate his or her lab outside of the universe.
    Would that not favor Everett over Copenhagen ?

19. **Lee Smolin**
    August 19, 2019

    Dear Peter,

    There is a clear difference between Copenhagen and Everett.

    The various versions of Copenhagen (or textbook) QM all require that the system studied is in fact a small subsystem of a larger universe. This is to make room for Bohr's classical world, where live the measuring instruments, clocks and observers. This is also required to connect the "probabilities" defined by Born's rule and the projection postulate with genuine experimentally obtained relative frequencies. The whole formalism is based on a clean distinction between states and observables, which expresses the assumption that by doing many repetitions of a measurement or preparation (which is only possible if your system is a subsystem of a larger universe) you can cleanly separate the effects of laws from those of initial conditions.

These distinctions all become problematic when one tries to apply textbook QM to the whole universe. Consequently anyone who contemplated doing so (ie a quantum cosmology) were aware it would require a modification of QM.

Everett QM was a deliberate attempt to make such a modification of QM that would make sense when applied to the universe as a whole. That is a big difference. Everett, Wheeler and deWitt all emphasized this point.

It was Everett's brilliant insight that to make QM cosmological might only require dropping the projection postulate and any reference to intrinsic probabilities, thus basing the theory on universal unitary evolution. It seems to be widely believed that his version fails to recover the correct probabilities. Otherwise there would be no need for the very subtle Oxford version of Deutsch, Wallace, Saunders etc. Whether they have completely succeeded or not-and in what version-remains, to my understanding, unresolved.

Thanks,

Lee

ps I look forward to reading Sean's new book. I disagree with him on several key points, but have tremendous admiration for the clarity of his thinking and writing.

20. **Blake Stacey**
August 19, 2019

People are willing to accuse Bohr of all sorts of mean, nasty, horrible things based on second-hand impressions, hearsay and a few overly-recycled quotations. His reputation for being an obscure writer is warranted, but it is also amplified by the unfortunate accident that his highest-profile writing (like his reply to EPR) is much less clear than some lesser-known works (like his contribution to the 1938 Warsaw conference). [Catherine Chevalley](#) adds that the conceptual concerns which motivated Bohr typically hailed from a tradition that is unfamiliar to modern philosophers of physics, and that the questions on Bohr's mind were often *not* those which nowadays go under the name of "quantum foundations." This has naturally led readers to be frustrated with him, simply because they expect something else from a discourse on "the interpretation of quantum mechanics."

21. **Peter Woit**
August 19, 2019

Pascal/Lee Smolin,

Yes, that's true. The textbook/Copenhagen recipe doesn't work for a QM cosmology. On the other hand, given the difficulties one already has with understanding out to get classical emergence in the non-gravitational case, it's not clear that Everett will also solve the much harder problem of how to get a quantum cosmology.

22. **Mateus Araújo**
August 20, 2019

Peter,

"If you ask a follower of Copenhagen (let's call her "Alice") "is the behavior of that spectrometer in your lab governed in principle by the laws of quantum mechanics" I assume that she would say "yes"."

There is divide between old Copenhagen and new Copenhagen in this point. I think it is nicely illustrated by what happened in QUPON 2015:

Časlav Brukner, a good representative of the neo-Copenhagen point of view, gave a talk about how to understand Wigner's friend. He argued that measurements are in fact described by the Schrödinger equation, but that in order to avoid Many-Worlds one needs to understand quantum states as only representations of knowledge of specific observers, not as ontological entities. Moreover, one cannot compare perspectives of different observers.

Right afterwards Anton Zeilinger, aa good representative of the old Copenhagen point of view, and Časlav's former PhD supervisor, gave a talk about something else. He started anyway with a critique of Časlav's talk, and visibly angry he thundered: "The measurement apparatus is classical!".

23. **Lee Smolin**
August 20, 2019

Dear Peter,

I agree. Indeed, since, for me, that there is only one universe follows from the definition of the universe, plus basic principles like the identity of the indiscernible, any argument that applying QM to cosmology requires a many worlds formulation is a reductio ad absurdum.

The correct conclusion, I would argue, is that QM in its current form, cannot be applied to the whole universe, because it is structurally a description of a subsystem of a larger system. What is needed is a completion that gives a complete description of individual phenomena, such as dBB, dynamical collapse models or the real ensemble formulation. Or, something yet to be invented.

Thanks,

Lee

24. **Pascal**
August 20, 2019

As pointed out in some comments, research on decoherence can be viewed as an attempt to recover a classical world from Everett-style QM, that is, from purely unitary evolution.
So it is maybe not surprising that Zurek and Everett had the same thesis adviser (Wheeler).

25. **Pascal**
August 20, 2019

I am confused now because according to Zurek's wikipedia page his thesis advisor was William C. Schieve. But according to Wheeler's, Zurek was a student of his. Maybe Wheeler was a co-advisor?

26. **Arnold Neumaier**
August 23, 2019

I think that the desire to make quantum mechanics apply to the whole universe (as there seems to be no border between classical and quantum mechanics) is the only reason for the popularity of the Everett point of view that there is only unitary evolution of the universe and nothing else. But since the purely unitary view lacks definite concepts for the notion of objective local events and local observers Everett had to introduce other (in my view abstruse) stuff to produce (an appearance of) consequences of the universal evolution that are applicable to the lab – something that textbook quantum mechanics has no problem postulating.

In my recent work I propose a different interpretation of quantum physics, which shares with Everett's view the advantage of having only unitary evolution for the universe. But it avoids the downsides (of ill-defined worlds, splits, events, histories, etc.). This "thermal interpretation" is described in my paper https://arxiv.org/abs/1902.10779 and discussed in further papers accessible from my web site http://www.mat.univie.ac.at/~neum/physfaq/therm/thermalMain.html

These papers form the basis of a book featuring the thermal interpretation of quantum physics that will come out soon:
https://www.amazon.com.au/Coherent-Quantum-Physics-Reinterpretation-Tradition/dp/3110667290

Something Deeply Hidden

August 20, 2019
Categories: Book Reviews, Multiverse Mania, Quantum Mechanics

Sean Carroll's new (available in stores early September) book, Something Deeply Hidden, is a quite good introduction to issues in the understanding of quantum mechanics, unfortunately wrapped in a book cover and promotional campaign of utter nonsense. Most people won't read much beyond the front flap, where they'll be told:

> Most physicists haven't even recognized the uncomfortable truth: physics has been in crisis since 1927. Quantum mechanics has always had obvious gaps—which have come to be simply ignored. Science popularizers keep telling us how weird it is, how impossible it is to understand. Academics discourage students from working on the "dead end" of quantum foundations. Putting his professional reputation on the line with this audacious yet entirely reasonable book, Carroll says that the crisis can now come to an end. We just have to accept that there is more than one of us in the universe. There are many, many Sean Carrolls. Many of every one of us.

This kind of ridiculous multi-worlds woo is by now rather tired, you can find variants of it in a host of other popular books written over the past 25 years. The great thing about Carroll's book though is that (at least if you buy the hardback) you can tear off the dust jacket, throw it away, and unlike earlier such books, you'll be left with something well-written, and if not "entirely reasonable", at least mostly reasonable.

Carroll gives an unusually lucid explanation of what the standard quantum formalism says, making clear the ways in which it gives a coherent picture of the world, but one quite a bit different than that of classical mechanics. Instead of the usual long discussions of alternatives to QM such as Bohmian mechanics or dynamical collapse, he deals with these expeditiously in a short chapter that appropriately explains the problems with such alternatives. The usual multiverse mania that has overrun particle theory (the cosmological multiverse) is relegated to a short footnote (page 122) which just explains that that is a different topic. String theory gets about half a page (discussed with loop quantum gravity on pages 274-5). While the outrageously untrue statement is made that string theory "makes finite predictions for all physical quantities", there's also the unusually reasonable "While string theory has been somewhat successful in dealing with the technical problems of quantum gravity, it hasn't shed much light on the conceptual problems." AdS/CFT gets a page or so (pages 303-4), with half of it devoted to explaining that its features are specific to AdS space, about which "Alas, it's not the real world." He has this characterization of the situation:

> There's an old joke about the drunk who is looking under a lamppost for his lost keys. When someone asks if he's sure he lost them there, he replies, "Oh no, I lost them somewhere else, but the light is much better over here." In the quantum-gravity game, AdS/CFT is the world's brightest lamppost.

I found Carroll's clear explanations especially useful on topics where I disagree with him, since reading him clarified for me several different issues. I wrote recently here about one of them. I've always been confused about whether I fall in the "Copenhagen/standard textbook interpretation" camp or "Everett" camp, and reading this book got me to better understanding the difference between the two, which I now think to a large degree comes down to what one thinks about the problem of emergence of classical from quantum. Is this a problem that is hopelessly hard or not? Since it seems very hard to me, but I do see that limited progress has been made, I'm sympathetic to both sides of that question. Carroll does at times too much stray into the unfortunate territory of for instance Adam Becker's recent book, which tried to make a morality play out of this difference, with Everett and his followers fighting a revolutionary battle against the anti-progress conservatives Bohr and Heisenberg. But in general he's much less tendentious than Becker, making his discussion much more useful.

The biggest problem I have with the book is the part referenced by the unfortunate material on the front flap. I've never understood why those favoring so-called "Multiple Worlds" start with what seems to me like a perfectly reasonable project, saying they're trying to describe measurement and classical emergence from quantum purely using the bare quantum formalism (states + equation of motion), but then usually start talking about splitting of universes. Deciding that multiple worlds are "real" never seemed to me to be necessary (and I think I'm not the only one who feels this way, evidently Zurek also objects to this). Carroll in various places argues for a multiple world ontology, but never gives a convincing argument. He finally ends up with this explanation (page 234-5):

> The truth is, nothing forces us to think of the wave function as describing multiple worlds, even after decoherence has occurred. We could just talk about the entire wave function as a whole. It's just really helpful to split it up into worlds… characterizing the quantum state in terms of multiple worlds isn't necessary – it just gives us an enormously useful handle on an incredibly complex situation… it is enormously convenient and helpful to do so, and we're allowed to take advantage of this convenience because the individual worlds don't interact with one another.

My problem here is that the whole splitting thing seems to me to lead to all sorts of trouble (how does the splitting occur? what counts as a separate world? what characterizes separate worlds?), so if I'm told I don't need to invoke multiple worlds, why do so? According to Carroll, they're "enormously convenient", but for what (other than for papering over rather than solving a hard problem)?

In general I'd rather avoid discussions of what's "real" and what isn't (e.g. see here) but, if one is going to use the term, I am happy to agree with Carroll's "physicalist" argument that our best description of physical reality is as "real" as it gets, so the quantum state is preeminently "real". The problem with declaring "multiple worlds" to be "real" is that you're now using the word to mean something completely different (one of these worlds is the emergent classical "reality" our brains are creating out of our sense experience). And since the problem here (classical emergence being just part of it) is that you don't understand the relation of these two very different things, any argument about whether another "world" besides ours is "real" or not seems to me hopelessly muddled.

Finally, the last section of the book deals with attempts by Carroll to get "space from Hilbert space", see here, which the cover flap refers to as "His reconciling of quantum mechanics with Einstein's theory of relativity changes, well, everything." The material in the book itself is much more reasonable, with the highly speculative nature of such ideas emphasized. Since Carroll is such a clear writer, reading these chapters helped me understand what he's trying to do and what tools he is using. From everything I know about the deep structure of geometry and quantum theory, his project seems to me highly unlikely to give us the needed insight into the relation of these two subjects, but no reason he shouldn't try. On the other hand, he should ask his publisher to pulp the dust jackets…

Update: Carroll today on Twitter has the following argument from his book for "Many Worlds":

> Once you admit that an electron can be in a superposition of different locations, it follows that person can be in a superposition of having seen the electron in different locations, and indeed that reality as a whole can be in a superposition, and it becomes natural to treat every term in that superposition as a separate "world".

"Becomes natural" isn't much of an argument (faced with a problem, there are "natural" things to do which are just wrong and don't solve the problem). To me, saying one is going to "treat every term in that superposition as a separate "world"" may be natural to you, but it doesn't actually solve any problem, instead creating a host of new ones.

Update: Some places to read more about these issues.

The book Many Worlds?: Everett, Quantum Theory and Reality gathers various essays, including
Simon Saunders, Introduction
David Wallace, Decoherence and Ontology
Adrian Kent, One World Versus Many

David Wallace's book, The Emergent Multiverse.

Blog postings from Jess Riedel here and here.

This from Wojciech Zurek, especially the last section, including parts quoted here.

Comments

1. **jxd**
   August 20, 2019

   The thing that's most bothersome to me about the measurement problem is whether, when I perform a Stern-Gerlach-like measurement that could produce one of two results, there is *anything at all* about the initial state of the universe that determines which result I see. Questions about whether there "really are many worlds out there" seem like they're sort of missing the point; both Everett and Copenhagen seem like different ways of saying "no" to this question, which is what many people (including me) find unsettling regardless of any questions about ontology.

   This leads to something that's confused me for a long time. It's always seemed quite clear to me that the answer to the above question has to be "no" if (i) there is a universal wavefunction that (ii) gives a complete description of the state of the universe and (iii) evolves according to the Schrodinger equation. Some of the conversation around the "emergence of the classical from the quantum" sometimes sounds like it's suggesting that (i-iii) are all true but somehow there ought to be some way to extract a single measurement result from it. Isn't such a project obviously doomed?

2. **Peter Woit**
   August 20, 2019

   jxd,
   I share your confusion, and have never understood how invoking many worlds is supposed to answer this.
   A while ago I wrote something asking a similar question here,
   http://www.math.columbia.edu/~woit/wordpress/?p=10533
   and learned a lot from the discussion. I'm still struck by the fact that, if you include environment + measurement apparatus, you are from the beginning dealing with mixed states and probability.

   But I'd rather not start up again exactly that old discussion, unless someone really has something new to add. I'd be much more interested in hearing or getting references to a more compelling argument for many worlds than the one Carroll provides.

3. **Edward Measure**
   August 21, 2019

   Are you actually talking about the cover of this book or the dust jacket? Tearing off the cover of a book seems pretty drastic, and makes the book a lot harder to hold, read, and shelve.

4. **Pascal**
   August 21, 2019

   Peter, you seem to accept the universal wave function as a description of the "real" state of the universe. Or at least you seem to accept it as plausible. Is that correct? Then I am again confused because I don't see the difference with the MWI, of which you are much more critical.
   Could you clarify the difference between these two positions?

   Also, from what I understand there is not a precise moment in MWI when a splitting "occurs." One can "split" a universal state vectors as the sum of 2 two (or more) vectors by choosing a basis. The 2 vectors can be viewed as the state of 2 parallel universes,
   and by linearity they will evolve independently. Is the issue why or how one basis is better than another to do this decomposition?

5. **Rollo Burgess**
   August 21, 2019

   Hi Peter

   Thanks for your thoughts on Sean Carroll's book. I will get it when it is available; I enjoy SC's podcast and his other writings…

   One question and one suggestion –

   Question – does the book contain lots of equations and mathematics? I ask because if not I will buy it on Kindle, and if so I won't. I really think that publishers should state this: it is outrageous that people sell mathematical books on Kindle (frequently expensively) which are in my experience essentially unusable as mathematical text does not display legibly. (emailing PDFs to Kindle works fine if you don't mind small fonts.)

   Suggestion – when you say that you are interested in references to places that that you can go to see the strongest argument for MWI – have you read The Emergent Multiverse by David Wallace? If not then that is your answer. Do not be put off by somewhat naff title; this is the most serious and sustained presentation out there, and also has a large bibliography. I'm very interested in your take. The issue that you and jxd are discussing seems to be a variant of the concern about where the probabilities come from, which is regarded as the big challenge with MWI. Wallace and the other 'Oxford' school' MWI people have a decision-theoretic argument which purports to address this. I have philosophical doubts about this argument but frankly, as someone who did physics only to UK GCSE and Maths to A-level and has recreationally self-studied beyond that I feel it is appropriate to be somewhat humble in my assessment of the arguments of trained professionals!

6. **Mateus Araújo**
   August 21, 2019

   Peter,

   I'm afraid you misunderstood Carroll's explanation on pages 234-5. He is saying that the alternative to having multiple worlds is needing to deal with the whole universal wavefunction. That doesn't give you a simple single-world ontology as one would want, but a hideously complex object that is nothing like a classical ontology. The most classical single-worldy ontology we can have (ironically enough) is precisely the one where we split the universal wavefunction into worlds.

   The issue here is that the fundamental ontology is the universal wavefunction, the worlds are a subjectively-defined emergent ontology that we use to connect physics to our everyday experience.

   An analogy would be to think of the universal wavefunction as a tree, and the worlds as the branches. Nothing forces us to think of the tree as being composed of branches, and in fact they are only an emergent ontology. They are not well-defined either, we can't really say where one branch begins and another ends. In fact there is only a tree made up of cells. It's just that it's very useful to talk about the branches, so we do that.

(Yes, trees are also only emergent ontology, I hope the idea comes through anyway).

7. **Amitabh Lath**
   August 21, 2019

   What does the multiverse mean in the "it from qubit" picture? If states+entanglement is everything (including the detector and the system being detected), and space and time are just how we experience this entanglement (spacetime "emerges" from entanglement); then what sense does it make to create other instances of spacetime?

8. **Nakagawa**
   August 21, 2019

   If the Everettian argument is valid, one solid consequence is that the initial one-World postulate for the argument is unphysical, since we have already been cohabiting with a vast number of mutually decoherent Worlds. When the initial postulate of the Everettian argument is replaced with the vast number of mutually decoherent Worlds, contingent interferences with each of decoherent Worlds may as a whole become significant, and I suspect decoherence only slightly outperforms recoherence. (Or, is there any convincing argument over the everlasting predominance of decoherence?) If so, wavefunction collapse-like phenomena will frequently occur by the recoherence effect.
   When we encounter an apparent wavefunction collapse, Everettians say it's caused by world-branching, but it should be explained either by branching or merging of a world, and there is no way to decide which really occurred…

9. **Peter Woit**
   August 21, 2019

   Edward Measure,
   Thanks! When I wrote that I knew "cover" wasn't the word I wanted, planned to go back to fix, forgot. Now done.

10. **Stephane**
    August 21, 2019

    Dear Peter,
    Are you going to review also the recent book by Smolin "Einstein's Unfinished Revolution: The Search for What Lies Beyond the Quantum"?

    I shall say that I much prefer what Jürg Fröhlich and colleagues are doing recently on quantum foundations (eg, https://arxiv.org/abs/1905.06603) rather than the Everettian approached favored mainly by cosmologists by considering that, instead of having a unique outcome from an experiment, all are realized (but only one in our Universe)…

11. **Peter Woit**
    August 21, 2019

    Pascal,

    1. I'm willing to consider the wavefunction of the universe, and use the word "real" to describe it (let's call this "real-psi"). I do worry though that maybe it makes no sense to consider it to be a pure state, that it is always a mixed state.
    2. In principle, one expects to somehow identify an emergent classical world corresponding to what we observe in this wavefunction. This is "real" in a different sense (the every day sense), let's call it "real-classical".

    MWI enthusiasts make what to me seems to be the following argument. One expects that if there is one emergent classical world in the wave-function, there will be lots. Since I've agreed that the one we're in is "real-classical", and the other emergent classical worlds are part of the same "real-psi" wave function, they must also be "real-classical". I don't see how this follows. To be fair to Carroll, he doesn't exactly say this. As quoted in the posting, he's aware that these are two different versions of "real", so you can't argue that one implies the other. So, I'm not "forced" by logic to agree to the "reality" of these supposed other worlds, and I don't see how agreeing to this solves any problem or does anything other than confuse the situation.

    The problem with the "split" picture you give is that it doesn't address at all the real problem of classical emergence. The best worked out theory of classical emergence that I'm aware of is Zurek's, and as far as I can tell he also rejects 2 above while agreeing to 1 (he describes himself as an "Everettian", see no need to describe other possible emergent classical worlds as real).

12. **Peter Woit**
    August 21, 2019

    Rollo Burgess,
    No equations in the book.

    Yes, I've tried reading the Wallace book, it is the most comprehensive and serious attempt at justifying the MWI picture. I didn't find anything convincing there on why multiple worlds must be described as "real", but was willing to believe there was a solid argument somewhere amidst the masses of material about probability and decision theory. One reason I like Carroll's book is that he writes and argues clearly, so you can easily follow the argument, see what he has and whether you agree or see a reason to disagree. It's in principle possible that Wallace has a convincing argument that Carroll just decided not to present, but one wonders whether that's really plausible.

    One thing I didn't get around to doing was adding links to the best sources I'm aware of for serious discussions of these issues, will try to do that soon.

13. **Peter Woit**
    August 21, 2019

    Mateus Araujo,
    I do understand that that's the point Carroll was trying to make, and shouldn't have been so flippant about "convenience". My point was just that, as you and Carroll both recognize, when you move from the universal wave function to supposed emergent classical worlds, you're discussing a very different sorts of ontology. Especially since I don't see any convincing theory of these "branches", I don't see why I have to use them to build my emergent ontology.

14. **Peter Woit**
    August 21, 2019

    Amitabh Lath,

I think what Carroll is doing is basically announcing that, in a fixed space-time, the problem of classical emergence is solved by MWI (except for an ongoing mopping up operation), and he's moving ahead to also get the space-time geometry as emergent out of a pure quantum system.

The first problem for me is that I don't think the original problem is solved. And that's a problem where you know exactly what the quantum system is (conventional textbook Schrodinger equation). But, much more seriously, I think the emergent space-time program suffers from the problem of "emergence from what?". Basically people doing string theory unification have given up, now are announcing that they will stop thinking about what the underlying theory is, and will derive our world as emergent from some unknown quantum theory ("it from unknown qu-something"). I don't see how they're going to get something from nothing here…

15. **Peter Woit**
August 21, 2019

Stephane,
I don't think I'll write a review of the Smolin book, don't really have anything interesting to say about it. As I've mentioned many times, I'm just not convinced by the point of view of Smolin and many others that what we don't understand about QM indicates the need for fundamental new physics. In the case of the Carroll book, I share his basic starting point (that what we know about the world should follow, emergently, from the known QM formalism).

About the "ETH-Approach to Quantum Mechanics", that just seems to me to be one of many sorts of programs which don't address the basic problem, that of understanding classical emergence.

16. **Mateus Araújo**
August 21, 2019

Peter,

I wouldn't use the word "moving". The fundamental ontology stays the same, it's always the universal wavefunction.

But I'm very confused. Are you saying that you accept the universal wavefunction as fundamental ontology, but that you think that this is somehow compatible with a single-world emergent ontology? How? How can you even single out one of the branches as "real"? They are the same, physically speaking. Or are you saying that the universal wavefunction doesn't have a branching structure? As far as I'm aware this point is not controversial – that decoherence splits the wavefunction into quasi-classical worlds that have effectively independent time evolution – and that people that reject Many-Worlds they also reject the universal wavefunction.

17. **Blake Stacey**
August 21, 2019

Steven Giddings had a criticism a little while ago of the "spacetime from entanglement" business which sounds about right to me. Basically, in order to define the kind of quantity that program wishes to build spacetime out of, you first have to introduce a structure very like what you're trying to explain.

> For example, one proposal is that spacetime "emerges from entanglement." However, a notion of entanglement relies, first, on a notion of division of a system into subsystems; it may be easily illustrated that for a given quantum state, different such divisions lead to either zero or nonzero entanglement. Related comments apply to entropies based on entanglement. Transfer of information, or entanglement, also requires division into subsystems between which transfer occurs.

(arXiv:1803.04973)

18. **Peter Woit**
August 21, 2019

Mateus Araujo,
Part of the problem here is that I don't believe anyone understands how emergence really works, so what the "branches" are, or what it even means to identify one branch as our own. I'd be much more willing to believe there could be a sensible argument for relative "emergent reality" of our branch and other branches if I felt I knew what the two different things actually were. I still don't see why accepting "reality-psi" of the wavefunction means I have to accept "reality-classical" of supposed branches that don't have anything to do with our "reality-classical".

I've found the place where Zurek, discusses this, see the last section of
https://arxiv.org/abs/0707.2832
which starts off
"There are two key ideas in Everett's writings. The first one is to let quantum theory dictate its own interpretation. We took this "let quantum be quantum" point very seriously. The second message (that often dominates in popular accounts) is the Many Worlds mythology. In contrast "let quantum be quantum" it is less clear what it means, so – in the opinion of this author – there is less reason to take it at face value."
and in "Closing remarks" there is
"It is therefore not clear whether one is forced to attribute "reality" to all of the branches of the universal state vector. Indeed, such view combines a very quantum idea of a state in the Hilbert space with a very classical literal ontic interpretation of that concept. These two views of the state are incompatible. As we have emphasized, unknown quantum state cannot be found out. It can acquire objective existence only by "advertising itself" in the environment. This is obviously impossible for universal state vector – the Universe has no environment."

19. **Peter Shor**
August 21, 2019

jxd:

If we could predict the measurement results we would get if we measured one element of an EPR pair, we would probably be able to transmit information faster than light (although this may depend on the exact model we're using on for how to predict these results).

Of course, this doesn't mean that the measurement results aren't predictable from aspects of the universe that are hidden somewhere that we can't get at. In fact, I think Bohm's pilot wave interpretation (which unfortunately has other problems) has this property.

20. **Mateus Araújo**
August 21, 2019

Peter,

"I still don't see why accepting "reality-psi" of the wavefunction means I have to accept "reality-classical" of supposed branches that don't have anything to do with our "reality-classical"."

Because there's nothing singling out any particular branch as physical, so it's not tenable to accept the branching picture but deny the reality of the "other" branches. Even if you define your current branch as the real one, it will split over and over again, with nothing distinguishing the future branches physically.

Incidentally, the Bohmians do have a single-world reality and a universal wavefunction, precisely by postulating these Bohmian particles that give reality to one branch of the wavefunction but not others. This is what it takes.

"Part of the problem here is that I don't believe anyone understands how emergence really works, so what the "branches" are, or what it even means to identify one branch as our own. I'd be much more willing to believe there could be a sensible argument for relative "emergent reality" of our branch and other branches if I felt I knew what the two different things actually were."

I'm not sure what your question is. The splitting part is the old story of decoherence, pointer states, quantum Darwinism. The "emergent" part is the arbitrary decision of when two branches have decohered enough so that their future evolution can be treated independently, and one can use the projection postulate to safely cut of other branches and focus on one's own.

21. **jxd**
August 21, 2019

I've always thought of the problem with trying to extract a single classical world from an Everettian picture as one of symmetry more than anything else. If the wavefunction of the universe can be written as a sum of two terms that don't interfere with each other and each term describes one of the two possible measurement results, you can't pick out one of them as "the one that really happened" without introducing some extra structure to this whole picture; there's nothing that could make one of them more "special" than the other. (And not introducing any extra structure is the whole point of the Everettian program!) The many-worlds move is to just accept this state of affairs and say, fine, then they both "really happened." This is the strange bit; whether you want to use the word "world" to describe them doesn't really matter.

Peter Shor: Yes, I think I agree, and that that's what I was getting at. The question I posed — whether there's anything about the initial state of the universe that determines which measurement result I get — is definitely an interpretation-dependent one. Everett and Copenhagen say no, Bohm (to the extent that it works) says yes.

The reason I'm confused is that some authors seem to want to extract a "yes" from unitary, collapse-free quantum mechanics with a universal wavefunction as the ultimate ontological description of reality, which seems obviously impossible to me. It would be lovely if this could happen, but as Mateus Araújo says I think it's pretty uncontroversial that it can't. This leaves you with either answering "no" to the above question or dropping one of the assumptions that got you there.

22. **Peter Woit**
August 21, 2019

Mateus Araujo,
I'm just not convinced that the cartoon structure of the quantum state space as divided into "branches", with us just characterized by a position on a branch actually captures what is going on. Part of this is skepticism that the picture of pointer states and quantum Darwinism is a complete understood answer to the classical emergence problem, neatly characterized by branches.

But also I have trouble with the idea that we're just a point on a branch. Remember that I find myself somewhere in between Copenhagen/textbook and Everett and note that Zurek claims (in the article linked to above) "One might regard states as purely epistemic (as did Bohr) or attribute to them "existence". Technical results described above suggest that the truth lies somewhere between these two extremes."

It is a basic fact about the state of the world as we see it that we can't characterize it as a pure state. As far as we can ever know, our world is at best some mixed state about which we have inherently limited information. Even in a purely classical world, I would be free to, given my limited information, decide to count as real worlds all trajectories consistent with that information, but that's not necessarily a good idea. The quantum situation is different, with limitations of information ones of principle, but still.

23. **Carlos Ungil**
August 21, 2019

What would a mixed state for the universe mean? A universal wavefunction would describe a pure state. We could have a proper mixture instead but that is usually understood to reflect our "classical" ignorance about the true quantum state (like in the case of an ensemble of systems where we only have statistical knowledge of their individual states); it is not really a fundamental property of the system.

24. **Pascal**
August 21, 2019

I am on the same page as Mateus and (I think) jxd. But I have a small quibble with one sentence in the comment by jxd:

"If the wavefunction of the universe can be written as a sum of two terms that don't interfere…"

By linearity of evolution, if you write the universal state vector Psi as Psi_1+Psi_2
the we have U(Psi) = U(Psi_1) + U(Psi_2), i.e., the 2 universes evolve "independently"
for *any* choice of the decomposition Psi = Psi_1+Psi_2. This means that "non inteference" is not a useful criterion to decide on which way to split Psi.
I think non-interference is a notion which makes sense only in the setting of
QM+projection postulate and not in purely linear QM.

25. **Mateus Araújo**
August 21, 2019

Peter,

"It is a basic fact about the state of the world as we see it that we can't characterize it as a pure state. As far as we can ever know, our world is at best some mixed state about which we have inherently limited information. Even in a purely classical world, I would be free to, given my limited information, decide to count as real worlds all trajectories consistent with that information, but that's not necessarily a good idea. The quantum situation is different, with limitations of information ones of principle, but still."

You are making the usual map-territory confusion. A mixed state is a description of your ignorance, not of what the world is. All our fundamental theories are formulated in terms of pure states; mixed states are introduced as derived concept, either to describe the statistics from local measurements on an entangled state, or to make probabilistic assignments of (pure) states to a not fully known system. The world itself is in a pure state, our ignorance of it doesn't make a difference.

"But also I have trouble with the idea that we're just a point on a branch. Remember that I find myself somewhere in between Copenhagen/textbook and Everett and note that Zurek claims (in the article linked to above) "One might regard states as purely epistemic (as did Bohr) or attribute to them "existence". Technical results described above suggest that the truth lies somewhere between these two extremes.””

Żurek’s statement makes no sense. Quantum states are either ontic or epistemic, the is no middle ground where to compromise. And the epistemicist program has taken quite a beating throughout history, I might add.

“I’m just not convinced that the cartoon structure of the quantum state space as divided into “branches”, with us just characterized by a position on a branch actually captures what is going on. Part of this is skepticism that the picture of pointer states and quantum Darwinism is a complete understood answer to the classical emergence problem, neatly characterized by branches.”

Well, that’s just the best we got. Sure, as all theories it is limited and subject to change, but until we find something better that’s what I’m going with.

26. **Blake Stacey**
August 21, 2019

Glitch notification: There’s an extraneous “~woit/wordpress/?p=11128#comments” just after the second blockquote.

27. **Peter Woit**
August 21, 2019

Carlos Ungil,
Sorry, I think my reference to a “mixed state” and universal wave function in that response to Pascal was confused, best to ignore that.

28. **Peter Woit**
August 21, 2019

Blake Stacey,
Thanks, fixed.

Mateus Araujo,
Thanks! Right now, while I have learned a lot, all of this is causing my brain to hurt, and I have other things to do, may or may not find time soon to think some more. I do wish someone could get Zurek to say more about this. He’s thought deeply about what seems to me the core of the problem, but the thing I quoted is one of the few places he has written about why he is skeptical about “Many Worlds” (in some other places he refers to this one).

29. **Mateus Araújo**
August 21, 2019

Pascal,

This is a point that often confuses people. The situation you describe is precisely one where the terms do interfere, and do not evolve independently. Let |0> be the upper path of an interferometer, and |1> the lower path. Then if the photon is in the state |0> + |1>, and passes through a beam splitter, the effect is H|0> + H|1> = |0>, the paradigmatic case of interference.

Now suppose we made a (non-demolition) measurement on the photon, entangling its position with the outside world |W>, ending up with the state |0>|W_0> + |1>|W_1>. Now these terms do not interfere anymore (as long as |W_0> and |W_1> are orthogonal), meaning that any whichever unitaries U,V one applies to the photon, getting U|0>|W_0> + V|1>|W_1>, will not make one term disappear as in the example above. In fact the evolution of both terms is now completely independent of each other.

An interference is only possible if one acts with an entangling unitary on the whole system at once. For a large enough system this is practically impossible, so it is far to regard |0>|W_0> + |1>|W_1> as a sum of two terms that don’t interfere.

30. **David Miller**
August 22, 2019

There is a basic problem with the idea of “branching” that is too rarely mentioned. The number of brnaches really cannot ever change, simply as a consequence of unitarity. There are never “new” branches. In some sense, the universe is just static, and the psi function and, indirectly, probability just sloshes around among the eternal branches.

All of us take this for granted when we do actual QM problems: e.g., resolving the spin of an eletron along different axes. But, somehow life gets more romantic when people think about MWI in the large.

David Deutsch takes this to its logical conclusion where there is no time either (try to do MWI in GR: the “problem of time” hits you). So, there is, Deutsch has said, just one eternal unchanging wave function with no actual time at all.

Like Peter, this makes my head hurt. Enough that I am willing to consider the possibility that just maybe QM is not the last word.

31. **Mateus Araújo**
August 22, 2019

Peter,

I’m glad you enjoyed the discussion. I think Żurek’s relative silence about this is because he doesn’t find Many-Worlds so objectionable. As he put it in his contribution to the “Many Worlds?” volume, his results “fit well within Everett’s relative states framework, but do not require ‘many worlds’ per se”.

32. **Anonyrat**
August 22, 2019

Mateus, is this table in wiki comparing QM interpretations accurate? It seems roughly split evenly between real wavefunction and not.
https://en.wikipedia.org/wiki/Interpretations_of_quantum_mechanics#Comparison

33. **Anonyrat**
August 22, 2019

All our fundamental theories are formulated in terms of pure states; mixed states are introduced as derived concept, either to describe the statistics from local measurements on an entangled state, or to make probabilistic assignments of (pure) states to a not fully known system. The world itself is in a pure state, our ignorance of it doesn’t make a difference.

I would argue that we formulate our theories in the simplest possible terms, which is why we formulate it in terms of pure states, but we simply don't know if the world is a pure state or not. Since the mathematical formalism works just as well either way, and since there is no experimental way to determine whether the world is in a pure state or not, "the world is in a pure state" is not a scientific statement.

34. **Will Sawin**
August 22, 2019

I think the issue is that a theory that describes

(1) the state of the world and
(2) what an observer observes at a particular time

is fundamentally incomplete. It also needs to explain

(3) how many, and which, observers exist.

Otherwise we would not be able to explain why any observers exist at all, nor why we find ourselves on Earth instead of on another planet or in empty space, nor answer whether other humans feel pain as we do.

If you accept that the universal wave function satisfying unitary evolution is fundamental, and everything else must derive from this, then you run into a problem with quantum measurements. It's alright to say that an observer, me, exists, and after performing the experiment, I either observed outcome 1 with 60% probability or observed outcome 2 with 40% probability. The problem is that there is nothing in the wave function to distinguish the observer who observed outcome 1 from the observer who observed outcome 2. So if one exists, then both must exist.

If you accept that both exists then I think you essentially accept many worlds. If before performing the experiment there are two scientists Alice and Bob, and afterwards there are Alice-1 and Bob-1 who both observed Outcome 1, and then observed each other discussing it, writing it down, etc., and also Alice-2 and Bob-2 who both observed Outcome 2. etc. etc., then it seems fair to say that Alice-1 and Bob-1 are in some kind of world together and Alice-2 and Bob-2 are in a different world together.

To believe that Alice-1 exists but not Alice-2 you have to believe in some extra information beyond the wave function that helps determine which observers exist. If you want to believe that Alice-1 and Bob-1 exist but not Alice-2 and Bob-2, or vice versa, but never any other combination, it seems like you have to believe in something like objective collapse or Bohmian mechanics.

35. **Mateus Araújo**
August 22, 2019

Anonyrat,

Yes, I think the Wikipedia table is roughly accurate. What's your point, though? Nobody is disputing that there are interpretations that consider the wavefunction to be real and interpretations that do not.

"I would argue that we formulate our theories in the simplest possible terms, which is why we formulate it in terms of pure states, but we simply don't know if the world is a pure state or not. Since the mathematical formalism works just as well either way, and since there is no experimental way to determine whether the world is in a pure state or not, "the world is in a pure state" is not a scientific statement."

So you want to ignore the historical development of our theories, and the fact that mixed states were introduced explicitly to be linear on the probabilities, to consider whether mixed states might be fundamental?

Fine, it is mathematically possible. It doesn't change the fact that the mixedness that Peter was talking about – the one caused by our ignorance of the precise state and environment – is explicitly a property of the observer, not of the world. Again, map versus territory.

And what if we still end up with a mixed state after removing this subjective mixedness? As you say, there is no experimental way to test whether the state of the universe is pure or mixed. We might as well use Occam's razor and take it to be pure.

36. **jxd**
August 22, 2019

I really appreciate the conversation this post has generated; it's clarified my thinking on a lot of these interpretation questions. I'm trained as a mathematician, not a physicist, and maybe relatedly I can sometimes find these questions really exasperating to read about. Over and over I've had the experience of reading some paragraph that purports to be explaining someone's favorite interpretation of quantum mechanics and realizing at the end that the paragraph didn't contain any content at all that I could meaningfully extract. (Żurek's discussion of what he thinks the wavefunction means is a good example.)

I especially feel this way about the epistemic interpretations I've tried to read about; I understand that they want to interpret the wavefunction as an expression of someone's knowledge, but I can never tell what this knowledge is supposed to be knowledge *of*. I know this might be off-topic, but can anyone point to a clear exposition of one of these approaches?

37. **Trailmut**
August 22, 2019

Breaking headline: "ICE Opens Interdimensional Detention Center to Indefinitely Imprison Immigrants Across Infinite Number of Multiverses."
(from The Onion, https://www.theonion.com/ice-opens-interdimensional-detention-center-to-indefini-1837476700)

(Sorry for being OT….a bit…)

38. **Curious Mayhem**
August 22, 2019

"… an electron can be in a superposition of different locations …" – Sure.

"… it follows that person can be in a superposition of having seen the electron in different locations …" – Maybe.

" … that reality as a whole can be in a superposition …" – Doesn't follow at all.

" … it becomes natural to treat every term in that superposition as a separate 'world'." – Really? In what world?

39. **Soup**
August 23, 2019

jxd,

You might enjoy this review by Matt Leifer: arXiv:1409.1570. He gives some toy models of what the "ontic states" might look like in a psi-epistemic theory i.e. what objective reality the wavefunction is supposed to encode knowledge about. They don't reproduce ordinary quantum mechanics though (well except for the trivial one ontic states = vectors in Hilbert space).

Realist psi-epistemic theories are awkward but it's interesting to see if they are possible. Personally, studying them has pushed me further towards a realist interpretation of the wavefunction.

40. **Mateus Araújo**
August 23, 2019

jxd,

The standard reference about epistemic versus ontic states is Harrigan and Spekkens. I find it quite clear.

People sometimes confuse epistemic quantum states with subjective quantum states. The former represents an observer's (lack of) knowledge about an underlying reality. The latter represents an observer's beliefs, without connection to an underlying reality. It is the point of view of QBism and similar subjectivist interpretations.

41. **Anonyrat**
August 23, 2019

So you want to ignore the historical development of our theories, and the fact that mixed states were introduced explicitly to be linear on the probabilities, to consider whether mixed states might be fundamental?

I fail to see how the historical development of our theories tells us anything about the subject of our theories, unless the history of the theory is part of the subject of the theory. Presumably if it is a good theory then some other culture/civilization/AI/alien species will arrive at the theory (or one isomorphic to it) with an entirely different history.

As you say, there is no experimental way to test whether the state of the universe is pure or mixed. We might as well use Occam's razor and take it to be pure.

Occam's razor would say, don't state an assumption either way. E.g., if there is no experimental way to determine whether God exists or not, Occam's razor says don't include God in your theory, and not "Might as well assume God exists" or "Might as well assume God does not exist".

42. **Anonyrat**
August 23, 2019

Just to draw your attention to a table of QM interpretations in A. Cabello. Interpretations of quantum theory: A map of madness. arXiv:1509.04711 and a Venn diagram (Figure 1.1) in J.B. Ruebeck, Understanding sequential measurements in psi-epistemic ontological models (https://uwspace.uwaterloo.ca/bitstream/handle/10012/14845/Ruebeck_Joshua.pdf?sequence=3)

It corrected my misimpression that psi-ontic and psi-epistemic was a complete classification of QM interpretations.

43. **Peter Woit**
August 23, 2019

Mateus Araújo,
The discussion here has been illuminating, causing the following evolution in my thinking:
1. When I wrote this posting I saw no problem with accepting the "universal wave-function" as what you get if you ask what is the description of the state of the world given by our best theory, QM. Now I realize that there's a big jump from what we know QM says about the world (which includes the fact we can't ever know the state of the world, so don't know if it is described by a pure state) to postulating a universal pure state wavefunction.
2. I remain confused about how the idea of multiple classical "worlds" is supposed to work and explain anything. What would really explain things is a detailed understanding of classical emergence. The best effort towards that I can find is the program of working to understand decoherence, the preferred basis problem and perhaps quantum Darwinism. Do the results of this program imply the "reality" of many worlds? Since I don't understand exactly how this is all supposed to work, I can't tell. That Carroll and most "Many Worlders" don't address this and seem rather uninterested in the details of classical emergence, while Zurek, who is interested , calls Many Worlds "mythology" doesn't encourage me towards the "Many Worlds are real" view.

In particular, Carroll only deals with the preferred basis/pointer state question in a single sentence (page 244-45): "the preferred-basis states are those that describe coherent objects in space, because such objects interact consistently with their environments." I'm not sure what this means, and he gives no references for it, not even a reference explaining what the "preferred basis" problem is. Among the many references in the book to more technical material, the only relevant one to classical emergence is to Zeh's 1970 decoherence paper (there are several attempts to derive the Born rule referenced, including Carroll's own).

As far as I can tell, Carroll's interest in the preferred basis problem is limited to the argument that the fact that interactions are local in space makes the position-space basis distinguished. I guess it is this that he wants to claim as somehow important for his attempt to find a theory of quantum gravity by identifying space inside a more generic quantum system (his version of "it from qubit"). As mentioned in the posting, I don't see any evidence you can do this with the tools and concepts available.

44. **Pascal**
August 24, 2019

Mateus, I am still not sure what "interference" means in this context since my point that U(Psi_1+Psi_2)=U(Psi_1)+U(Psi_2) for any Psi_1, Psi_2 is certainly valid. Instead I propose the following toy model.

The state space is a finite dimensional Hilbert state and the unitary U represents time evolution (time is discrete; for continuous time we would need an infinite dimensional Hilbert space).
The goal is to single out some "classical states" or in another terminology a "preferred basis".
We would like these states to enjoy some kind of stability property under the action of U.
So I propose that in a preferred basis, U should be simply (maybe up to a phase) a permutation matrix. "Up to a phase" means that the entries of the permutation matrix may be multiplied by arbitrary complex numbers of modulus 1.
This is a model of a cyclic universe: after a while we come back (up to a phase) to our initial state.
In this setting a "preferred basis" will be an orthonormal basis in which U becomes (up to a phase) a permutation matrix. This is a non-trivial constraint: not all bases satisfy this property.

This is unfortunately not enough to single out a unique “preferred basis”. For instance, given any permutation matrix one can always (like for any unitary) choose a basis of eigenvectors,
And if the initial state is an eigenvector there will be (up to a phase) no time evolution at all!
I will hazard the suggestion that the only way to single out a unique “preferred basis” may lie in the initial condition of the universe (which would be an element of that basis).
Again, there is no escaping the fact that if we start from an eigenstate there is no time evolution at all. But if we decompose the eigenvector in another of these “preferred bases” we may obtain a superposition of “universes”, each enjoying some time evolution even though there is no global time evolution in the multiverse (whose state is the eigenvector, if you have followed me up to this point).

Does this look like something that has been studied in decoherence or MWI ?

45. **Mateus Araújo**
August 24, 2019

Anonyrat,

“I fail to see how the historical development of our theories tells us anything about the subject of our theories, unless the history of the theory is part of the subject of the theory. Presumably if it is a good theory then some other culture/civilization/AI/alien species will arrive at the theory (or one isomorphic to it) with an entirely different history.”

It is important. We take seriously conjectures that are well-motivated, not conjectures whose only thing going for it is that “it hasn’t been proven false”.

“Occam’s razor would say, don’t state an assumption either way. E.g., if there is no experimental way to determine whether God exists or not, Occam’s razor says don’t include God in your theory, and not “Might as well assume God exists” or “Might as well assume God does not exist”.”

Not, Anonyrat, Russell’s teapot is just not there. We are not respectfully agnostic about unfalsifiable hypotheses, we mercilessly eliminate extraneous entities. And a state of the for |\psi> is much simpler than a state of the form p|\psi><\psi'|.

"Just to draw your attention to a table of QM interpretations in A. Cabello. Interpretations of quantum theory: A map of madness. arXiv:1509.04711 and a Venn diagram (Figure 1.1) in J.B. Ruebeck, Understanding sequential measurements in psi-epistemic ontological models (https://uwspace.uwaterloo.ca/bitstream/handle/10012/14845/Ruebeck_Joshua.pdf?sequence=3)

It corrected my misimpression that psi-ontic and psi-epistemic was a complete classification of QM interpretations."

Yeah, you can always reject an objective reality, and then anything goes. There's also no point in talking about it.

46. **Mateus Araújo**
August 24, 2019

Peter,

1 – I don’t see what’s the point of this discussion about whether the quantum state of the universe is pure or mixed. You still have many worlds either way, the important thing is that there is a quantum state for the whole universe.

2 – It’s not as if many worlds are postulated to explain anything, rather they are the consequence of applying the laws of quantum mechanics to the whole universe.

47. **Paddy**
August 24, 2019

Peter, Re Many Worlds vs Decoherence:

Everett’s starting point was von Neumann’s analysis of measurement in his Grundlagen. According to von Neumann, a “measurement” is a physical process that converts a pure quantum state into a mixed state. Pretty clearly von Neumann is thinking here of QM as a description of ensembles, and arguably this is nothing more than a mathematised version of the Born rule. The evidence for the Born rule, of course, is the fact that measurements on quantum systems give definite results, even when the original state is known to be in a superposition*, and after the measurement no further superposition can be demonstrated (or, per the analysis in the Feynman lectures, if you *can* demonstrate superposition you have not really made a measurement).

Everett applies von Neumann’s analysis to a single quantum system, and in effect asks: how should we interpret the plus signs when we write out the mixture as a sum (or integral) of terms corresponding to particular outcomes? Should we read “plus” as “and” or as “or”? Conventional QM goes for “or” but MWI goes for “and”, and it is hard to argue that this is not the only consistent interpretation of the mathematics.

A decade after Everett, decoherence theory explained in some detail how measurement-like processes, that is, interactions with “environments” containing a macroscopic number of degrees of freedom, could indeed convert a pure state into a state that effectively acts as a mixture, once the environmental degrees of freedom are traced out. Given the practical impossibility of designing an apparatus that took the environment into account in full detail, Zurek in particular claimed at the time that this “solved” the measurement problem and provided a complete interpretation of QM. However, it does not at all address the “and/or” question for mixtures and I believe that Zurek’s views have evolved over time on this. Most MWI people are very grateful to decoherence theory for providing a detailed account of “branching”, but would argue that we always knew that branching must take place thanks to the existence of our perceived classical world. And if you have every tried to explain decoherence at a popular level (or even to advanced undergraduates), you would have sympathy with Carroll for not delving into this in depth.

* Superposition of eigenstates of whatever is being measured.

48. **Peter Woit**
August 24, 2019

Paddy,
I don’t object to Carroll’s decision not to discuss decoherence theory in detail. In a popular book one’s ability to discuss more technical issues is very limited. My reference was to the very specific issue of the “preferred basis” problem. Note that he’s doing something very questionable with this book: he moves on from the old claim that MWI solves interpretational problems to the claim that it somehow is involved in telling you how to reconcile QM and GR. The hinge of this argument is that the “preferred basis” is the space coordinate basis,. Because this is both a huge problem for MWI and at the center of his argument, some sort of reference backing it up would have been a good idea.

I started looking a bit at how the MWI people treat the preferred basis problem, specifically at David Wallace’s discussion of it, and it seems to me that there’s not much argument besides the assumption that however classical emergence happens, it solves the problem. This is where I have a problem with Mateus’s point number 2. Besides Zurek, I just am not finding places where MWI people actually come to grips with classical emergence (in particular the preferred basis aspect of it), so the argument that the way classical emergence happens implies Many Worlds seems to me possibly empty.

49. **Paddy**
August 24, 2019

Peter,
I agree that Zurek seems to have put more serious work into the preferred basis problem than anyone else I know of. To be honest, I have not read his work on this point with enough care to claim that I fully understand how einselection is supposed to work, but I have extracted the idea that states expressed in certain bases are relatively stable because of the way the corresponding operators feature in the Hamiltonian, which seems extremely plausible (also, it sounds like what Carroll is referring to in the sentence you quote). If true it solves one of the two deep problems for MWI, the other being the probability issue, which I guess is why Wallace's book focuses on that.

50. **Charles Rezk**
August 24, 2019

I'm no expert on any of this, but I just want to comment on:

"A mixed state is a description of your ignorance, not of what the world is. All our fundamental theories are formulated in terms of pure states; mixed states are introduced as derived concept, either to describe the statistics from local measurements on an entangled state, or to make probabilistic assignments of (pure) states to a not fully known system."

This kind of glib statement is repeated everywhere. But I don't see how matters can be that simple.

If pure states are a description of the world, and mixed states just a description of incomplete knowledge, then we would model mixed states by *probability measures on the space of pure states*. Instead, mixed states are modelled by density operators: but there are infinitely many probability measure which give the same density operator. (E.g., for the qubit, the space of density matrices is a 3 dimensional ball, but the space of probability measures on pure states (a 2 sphere) is infinite.)

It seems to be a magical property of QM that for practical purposes almost all of the information about our incomplete knowledge can be ignored, and we can work entirely with mixed states (density operators). This seems like something which deserves an explanation in any account of an interpretation of quantum mechanics.

Of course, one way to respond to this is to suppose that there is something incomplete in our understanding of QM, which I think is a point of view Penrose takes in some of his big weird books.

It seems more economical to just suppose that mixed states can be a description of the world, exactly on par with pure states, a point of view which seems supported by the fact that the mathematical description of QM is at least as clean (if not cleaner) using mixed states as it is using pure states. Does anything go wrong if we just suppose that the world can be described by mixed states? I don't see how.

51. **jxd**
August 24, 2019

I may be misunderstanding the question, but I don't think you need any complicated explanation of classical emergence from the quantum in order to extract the many-worlds picture, provided you believe that the universe is completely described by a quantum state and that Schrödinger's equation describes how it evolves. It falls out of the linearity, right? If the environment starts in some state psi_E and the thing you're measuring has a state that lives in some other small Hilbert space, then the state that (1/sqrt(2))((psi_1 + psi_2) tensor psi_E) evolves into has to be a linear combination of the states that (psi_1 tensor psi_E) and (psi_2 tensor psi_E) evolve into, hence the macroscopic superposition.

I can see how you might *believe* these assumptions more readily if they were coupled with a good description of why macroscopic stuff looks classical; if this part of the story were somehow broken, there would be more of a reason to suspect that quantum mechanics needs to be modified. (Is this all you're saying?) But it seems pretty straightforward that the Everettian picture is what these assumptions imply.

Now, the many-worlds story comes from taking a physical model that performs very well in all the domains where we've tested it and inferring that it must also apply to the whole universe, a domain in which we very much have not tested it (whatever that would even mean). I don't think it's totally unreasonable to balk at the many worlds and conclude that it's a sign that the standard QM story must be missing something. But (a) there doesn't seem to be any experimental evidence for such a thing, and (b) I don't think there's a great theoretical candidate for what missing thing we could add that would solve the problem convincingly.

52. **Blake Stacey**
August 24, 2019

jxd,

The "Map of Madness" mentioned above (arXiv:1509.04711) does a pretty good job of finding what one might consider "canonical" sources for the various interpretations it tabulates.

53. **Mateus Araújo**
August 24, 2019

Charles Rezk,

"It seems to be a magical property of QM that for practical purposes almost all of the information about our incomplete knowledge can be ignored, and we can work entirely with mixed states (density operators). This seems like something which deserves an explanation in any account of an interpretation of quantum mechanics."

That is an excelent point. There is, however, an explanation. If you use true randomness to prepare your ensembles, and model the preparation procedures in the Many-Worlds way, then any two preparation procedures that result in the same density matrix for subsystem A must be related by a unitary transformation in subsystem B. Then it is clear why you can't distinguish the preparation procedures by doing measurements on A alone: that would violate relativity! You would be able to find out whether some unitary transformation was applied to subsystem B, but that application can be done with a space-like separation.

For more details, see the blog post I wrote about it.

54. **Mark Hillery**
August 24, 2019

"Once you admit that an electron can be in a superposition of different locations, it follows that person can be in a superposition of having seen the electron in different locations, and indeed that reality as a whole can be in a superposition, and it becomes natural to treat every term in that superposition as a separate "world"."

Ummm, no. This ignores the amplification process necessary to make the behavior of the electron visible to the person. Amplifiers are inherently noisy, which means they fuzz things out and destroy quantum superpositions. There is a very nice paper about this, which is, unfortunately, probably rather hard to find.: R. J. Glauber, Amplifiers, attenuators, and Schroedinger's cat, New Techniques in Quantum Measurement Theory, volume 480 of the Annals of the New York Academy of Sciences (1986).

55. **Blake Stacey**
August 25, 2019

Mark Hillery,

Here is a copy, online but paywalled.

56. **Arnold Neumaier**
August 25, 2019

Peter,

"The best effort towards that I can find is the program of working to understand decoherence, the preferred basis problem and perhaps quantum Darwinism. Do the results of this program imply the 'reality' of many worlds?"

Surely not. In https://arxiv.org/pdf/1404.2635, Maximilan Schlosshauer states in Section VII the little decoherence can contribute to the foundations. In particular, he writes: "Since decoherence follows directly from an application of the quantum formalism to interacting quantum systems, it is not tied to any particular interpretation of quantum mechanics, nor does it supply such an interpretation".

Mateus Araujo,

"It's not as if many worlds are postulated to explain anything, rather they are the consequence of applying the laws of quantum mechanics to the whole universe."

The laws of quantum mechanics neither contain the notion of a world nor that of a superposition in a preferred basis (which is usually used to motivate worlds). Thus many worlds cannot be a consequence of the laws of quantum mechanics alone. One must add quite some fancy, very controversial interpretation stuff to get many worlds….

57. **Peter Woit**
August 25, 2019

Paddy,
The problem with just saying it is the Hamiltonian that determines the preferred basis is, "which Hamiltonian, and how does it do this?"
For the case of a quantum particle, I see arguments for
1. configuration space basis: locality of interactions
2. momentum space basis: these are energy eigenstates for the free particle, so good basis for weak interactions.
3. coherent state basis: these have a nice relation to the classical limit.
For the case of a spin-1/2 degree of freedom, I have no idea how this is supposed to work.

58. **Peter Woit**
August 25, 2019

jxd,
The problem is how do you get from "the state is a macroscopic superposition" to "this corresponds to multiple worlds". In particular, "superposition with respect to which basis?" is the "preferred basis" problem. That's one problem you have to solve before the "multiple worlds" picture even begins to make any sense.

59. **Mateus Araújo**
August 25, 2019

Arnold Neumaier,

"The laws of quantum mechanics neither contain the notion of a world nor that of a superposition in a preferred basis (which is usually used to motivate worlds). Thus many worlds cannot be a consequence of the laws of quantum mechanics alone."

The worlds are part of emergent reality, not fundamental reality. The laws of quantum mechanics also do not contain the notion of trains, but this doesn't make them any less real.

"One must add quite some fancy, very controversial interpretation stuff to get many worlds…."

On the contrary, Many-Worlds is the only interpretation that takes quantum mechanics as it is. To get rid of the multiple worlds is that you need "fancy, very controversial interpretation stuff". To note:

Denying an objective reality (QBism).
Postulating a classical reality irreducible to the quantum laws (Copenhagen).
Postulating hidden variables (Bohmian mechanics).
Changing the Schrödinger equation (Collapse models.).

60. **Peter**
August 25, 2019

Mateus Araujo,

"The laws of quantum mechanics also do not contain the notion of trains, but this doesn't make them any less real."

I thought the problem was to get the trains from QM, and I don't see how MWI does it. I agree with Peter Woit when he writes that you have to solve the "preferred basis" problem before the "multiple worlds" picture even begins to make any sense.

There's something else that bothers me. What is this "environment" that causes decoherence? I suppose I'm wrong, but I sense a certain circularity. Environments cause decoherence, and if something doesn't cause decoherence in a certain system, it doesn't count as an "environment" of that system.

61. **Arnold Neumaier**
August 26, 2019

Mateus Araujo,

”The worlds are part of emergent reality, not fundamental reality. ”

See Peter Woit’s preceding post for the extra input needed to infer something emergent.

”Many-Worlds is the only interpretation that takes quantum mechanics as it is.”

Maybe it used to be the only interpretation that claimed that, but now there is also the thermal interpretation, which is based solely on the unitary dynamics of the universe:

http://www.mat.univie.ac.at/~neum/physfaq/therm/thermalMain.html

It works in a single world and features none of your objections to other interpretations,

”Denying an objective reality (QBism).
Postulating a classical reality irreducible to the quantum laws (Copenhagen).
Postulating hidden variables (Bohmian mechanics).
Changing the Schrödinger equation (Collapse models.).”

62. **Bill**
August 26, 2019

It has been pointed out by more than a few quantum foundation philosophers including Ruth Kastner that Quantum Darwinism because it is observer independent must assume a partition into environment+pointer+system in order to derive just that. Kastner posits an observer independent collapse theory which repairs a hole but causes another leak(non-unitary evolution of the Schrodinger equation). This is the common running theme in foundations literature(which assumption do you deny and how do you account for the inconsistency that arises).

63. **Peter Shor**
August 26, 2019

@Bill: It shouldn’t be circular.

You can choose many, many partitions into environment+pointer+system, and most of them don’t behave in ways at all consistent with Quantum Darwinism. So what you really need to do is show that there exists a partition with the right properties.

And of course, you need assumptions to ensure that this partition exists.

Assuming that such a partition exists at time *t*, and showing that such a partition exists for all times not too long after that is a reasonable way to proceed. This partition is not going to continue existing when the universe has died a heat death, so all you can ask for is that it remains for some time.

64. **Mateus Araújo**
August 26, 2019

Another Peter,

“I agree with Peter Woit when he writes that you have to solve the “preferred basis” problem before the “multiple worlds” picture even begins to make any sense.”

The preferred basis problem has been solved, for fuck’s sake. Everybody seems to love throwing around the sentence “preferred basis problem” without having the faintest idea what they’re talking about. If you want to be taken seriously at least describe what the problem is and why do you think the canonical solution is not satisfactory. Argh.

The problem was to explain why the basis corresponding to the measurement outcomes was the one corresponding to the quasi-classical worlds. Otherwise you couldn’t *derive* the fact that a state of the form |0>|M_0> + |1>|M_1> corresponds to a superposition of a world with result zero and a world with result one. Everett kind of postulated that measurement outcomes do correspond to worlds, and I think that’s fine: it clearly works. He was attacking the problem from the top-down direction, guessing what the quantum dynamics must be in order to solve the measurement problem. That’s profoundly unsatisfactory from the philosophical point of view, though, because you are introducing in your theory special “measurement” devices which happen to split the world according to their outcomes. A proper reductionist theory must describe measurements in terms of quantum dynamics, and explain why they split the world according to the outcomes.

And this has been done! It’s what this story about decoherence and pointer states and quantum Darwinism is all about. We have learned, from the 70s onwards and with more and more detail, that measurement devices are those that entangle the measured system to complex quantum systems, that their dynamics are nothing special, but a generic feature of system+environment interaction – called decoherence, that the measurement outcomes correspond to the pointer states – precisely those stable under decoherence, and that the subsequent dynamics of these pointer states are effectively split from eachother, again because they have decohered. That’s how you go from a superposition between two spins to a superposition of non-interacting quasi-classical worlds. Don’t take my word for it, read a fucking paper about it.

“There’s something else that bothers me. What is this “environment” that causes decoherence? I suppose I’m wrong, but I sense a certain circularity. Environments cause decoherence, and if something doesn’t cause decoherence in a certain system, it doesn’t count as an “environment” of that system.”

Yes, you are wrong. Decoherence is not about tautologically classifying systems into “environment” and “not environment”. It’s about showing how the usual interactions between some quantum systems – the prototypical example between a dust mote and air – cause them to be entangled, and thus interference in the system of interest effectively unobservable.

65. **Peter Woit**
August 26, 2019

Mateus Araújo,
Yes, I understand that there are people who claim the preferred basis problem is solved, but I’m not convinced. As you note, the Everett “solution” isn’t convincing, it just assumes classical emergence solves the problem somehow. Zurek actually engages with the problem, but has he really solved it? Jess Riedel in one of the blog postings I linked to above, takes the point of view “I say that the decoherence program as led by Zeh, Zurek, and others is an improvement—a monumental advance—but not a complete solution.”
For another randomly chosen example, here
https://www.preposterousuniverse.com/blog/2014/02/12/the-many-worlds-of-quantum-mechanics/#comment-7295910552604282125
Commenter “Matt” (Matt Leifer??)
objects to Sean Carroll’s claim that the problem is solved by “interactions are local in space”, and writes

"The preferred-basis problem, on the other hand, is specially a problem for the many-worlds interpretation, and refers to the fact that we can trivially expand the overall state vector in any of an infinite set of different choices of basis for the overall Hilbert space, where each basis paints a very, very different picture of what those different worlds are and what are the probabilities associated to them. There is no guarantee that most of those choices of basis will involve basis states that look classical, and, on the other hand, there may well be two (or more) choices of basis that give classical-looking basis states and thus inconsistent sets of classical realities that are not related to each other in a classically understandable way.

The preferred-basis problem is unsolved, and probably unsolvable without somehow adding on more axiomatic principles to the many-worlds interpretation for choosing one basis over all the others."

Unlike "Matt", I'm willing to believe that the preferred-basis problem as he states it is solvable, with a better understanding of what happens in a usual sort of physical "measurement", without new "axiomatic principles". My suspicion is that you have to really engage with what the properties of the Hamiltonian are that give the sort of "classical world" we want (and just saying "the interaction Hamiltonian is local in space" doesn't do it). I've not spent enough time understanding the details of what Zurek and others have done and have not done, but when I try and follow discussions of this, at crucial points they seem to me to be effectively assuming that an unsolved problem gets solved the way they want.

66. **Mateus Araújo**
August 27, 2019

Peter Woit,

Commenter "Matt" doesn't know what he's talking about. As I explained above, the preferred basis problem was about explaining why measurement outcomes correspond to quasi-classical worlds, not about the trivial fact that a quantum state can be written in different bases. His assertion that "each basis paints a very, very different picture of what those different worlds are and what are the probabilities associated to them" is often repeated, but empty. Nobody has *ever* even tried to show that there are in fact such different worlds, and what they would be (except for the science fiction author Greg Egan in his novel Quarantine, which I highly recommend, but is not a scientific work). If one wants to claim that there is some other basis where we have worlds with blue dragons, the burden of proof is on them.

About your point of view, it's hard to argue against wanting to have a better understanding of the measurement process, but I don't see a concrete objection. Do you think that there could be something that we usually regard as a measurement apparatus, but whose outcomes do not correspond to pointer states? Or do you think that we can have some pointer states that give rise to quasi-classical worlds with some radically non-classical feature, such as not being well-localized in phase space or having blue dragons?

The fact that neither thing happens in all the systems we have studied is good evidence that they in fact do not happen. Moreover, it is hard to prove a negative.

I don't see what is your problem with the "local interactions" argument. It is a great insight; Hamiltonians with interactions that are local in space will decohere spatial superpositions, and preserve states that are well-localized in phase space. Since the fundamental Hamiltonians we know *are* local, this does show that we won't have quasi-classical worlds with delocalized states, so this blue dragon is dead. What other non-classical features still need excluding?

67. **Peter Shor**
August 27, 2019

Mateus Araújo:

I don't see how the decoherence problem will really be solved until you can answer questions like: given a physical system, when will the polarization of light decohere into the vertical/horizontal basis, when will it decohere into the right/left diagonal basis, and when will it decohere into the clockwise/counterclockwise basis? And how about situations where it decoheres, but in which there is no preferred basis; how do you model decoherence in these situations?

Finally, blue dragons are indeed relevant; in some sense, the theory of decoherence won't be truly complete until you can *prove* that there are no quasi-classical worlds with a hidden basis that contains blue dragons. (Although making such a proof a requirement for accepting a theory of decoherence is much too demanding.)

It seems to me that pointer bases and quantum Darwinism only analyze the easy cases.

68. **Paddy**
August 27, 2019

Peter,
To be a bit more explicit than Mateus Araujo:

* Which Hamiltonian? Given that we are assuming here that QM is objectively correct, the one and only Hamiltonian that governs the evolution of the universal quantum state. Which, obviously, we don't yet know exactly.

* Which basis: to get a quasi-classical large-scale world, the wavefunction must be sharply peaked (by macroscopic standards) in both configuration and momentum space, but only for coordinates representing particles bound into macroscopic objects. "Sharply peaked" meaning resolved into multiple disconnected islands, one per "world". In this context "disconnected" allows the islands to be actually connected along one or a few dimensions, as when a position measurement is made on a delocalised particle. Owing to the very high dimensionality, O(Avogadro's number) for a simple lab setup, such connections are FAPP impossible to utilize to demonstrate interference effects.

The states are neither momentum nor position eigenstates which are both unphysical, nor are they exact eigenstates of any universal unique operator, e.g. coherent states. Zurek claims to have shown that einselection operates because the Hamiltonian can be expressed relatively simply in terms of position and momentum operators, and as a consequence, he says, when expressed in these spaces the wavefunction changes relatively smoothly. In contrast, whatever operator has eigenstates corresponding to non-pointer states, like, say, (live + dead) cat and (live – dead) cat would have a wave function that changed very fast indeed. States that change relatively slowly (and are therefore short-term predictable) are necessary for the function of any life-form (or general IGUS – information gathering and utilising system), so life has evolved operate in quasi-classical terms.

69. **Mateus Araújo**
August 27, 2019

Peter Shor,

Light-matter interaction is one of the most studied problems in physics. Finding an open problem about how the polarisation degree of freedom entangles with the environment would be a feat in itself. But lets say you do find such a situation where we don't know whether the decoherence will happen in the vertical/horizontal or clockwise/counterclockwise basis. What is the problem then? We do have "classical" light in such polarisations, these are not blue dragons.

"And how about situations where it decoheres, but in which there is no preferred basis; how do you model decoherence in these situations? "

What about that? Sounds like you're just getting photons polarised in a random basis. Isn't that pretty vanilla?

"Finally, blue dragons are indeed relevant; in some sense, the theory of decoherence won't be truly complete until you can prove that there are no quasi-classical worlds with a hidden basis that contains blue dragons. (Although making such a proof a requirement for accepting a theory of decoherence is much too demanding.)"

I agree that if somebody did find such a basis of quasi-classical worlds with blue dragons it would be a problem, as it would amount to a prediction that blue dragons exist, and in reality they don't (or even more fascinating, if somebody found such a basis that was *different* from the usual pointer states/coherent states basis, as it would imply that parallel to our reality of boring quasi-classical worlds there exists a hidden reality with blue dragons. That's the premise of Greg Egan's Quarantine). But absent such a discovery, proving that blue dragons are not predicted by quantum mechanics sounds like proving that Russel's teapot is not there.

70. **Peter Woit**
August 27, 2019

Paddy/Mateus Araújo,
Thanks for your comments. I'm afraid I've reached the point where to get more out of this I'd have to spend serious time doing some more reading and thinking, and there are other projects which seem more likely to be fruitful that I should get back to instead.

Without doing such reading and thinking, I'm stuck unconvinced that invoking "many worlds" as an explanation really explains anything, and in a position I don't like to be in, that of trying to figure out what is going on not by understanding something myself, but by seeing what experts say. From all I can tell, the best work done in this area has been that of Zurek and collaborators, and from what I can gather, Zurek comes down on the "many-worlds not necessary" side, while on the "is the preferred basis problem solved?" question I take Zurek as a yes, and Jess Riedel as "not a complete solution yet".

The Carroll book is going to convince a lot of non-physicists that the problems of QM are resolved by the existence of multiple worlds. I hope it will have the effect that experts with a good understanding of what is really going on will find the appearance of the book a good opportunity to write something less propagandistic that gives interested non-experts a more balanced explanation of the state of the subject.

71. **Blake Stacey**
August 27, 2019

> I hope it will have the effect that experts with a good understanding of what is really going on will find the appearance of the book a good opportunity to write something less propagandistic that gives interested non-experts a more balanced explanation of the state of the subject.

I hope so too. This is not a hope really founded on experience — my *expectation* is that people will continue to endorse whichever lazy answer they had already settled on, and to propagate the same dull arguments that were old by 1980 — but I shall hope anyway.

72. **Paddy**
August 29, 2019

Peter,
A last attempt to unblock the log-jam in your thinking about this. You have been told several times already, and so the fact that you don't take it on board suggests to me that it may be the crux of your problem:

Invoking "many worlds" is *not* supposed, even by MWI-ers, to be an explanation of anything. The explanation is QM. Many worlds is a *logical consequence* of QM. Or so say the MWI-ers. So you shouldn't be puzzling about what sort of thing many worlds explains, you should be puzzling about whether you can have a self-consistent, ontic, QM without getting many worlds as a by-product.

I think I was too kind to Zurek in an earlier comment. If you accept that decoherence is the explanation for the pure state to (apparent) mixed state transition, which he does, then you are committed to the mixed state being mixed only FAPP, and therefore you cannot consistently deny the existence of terms in the mixture that do not correspond to the world we experience. But Zurek did try to do that for many years, although I read something by him in the last decade or so (sorry, can't remember where) , which seemed to me to reluctantly accept the MWI deduction.

For the record, I am not fully convinced by MWI either. Probability is still a big problem, pace Wallace. QM could be wrong. It just seems to me that your criticism of MWI is surprisingly shallow.

73. **Peter Woit**
August 29, 2019

Paddy,

I think it's likely that you're misunderstanding my point.

Recall that the context of this posting is a discussion of Sean Carroll's book, which is devoted to making the case that invoking multiple worlds explains everything, with all that most people are going to get out of it the news that:

"Carroll says that the crisis can now come to an end. We just have to accept that there is more than one of us in the universe. There are many, many Sean Carrolls. Many of every one of us."

This is what I'm criticizing. I don't see how my accepting that there are many Peter Woits in the universe will end any crisis, or explain anything. What I decide to call a "real world", my choice of "ontology" if you like, to me is more of a philosophical than scientific issue, unless you can make a good case that this choice is important to understanding how to solve a problem.

The interesting physics question I'm trying to understand is that of exactly how the usual rules we teach students about how the bare QM formalism relates to "measurements" can be derived rather than postulated. This is an attractive idea and I don't see any evidence that this can't be done. The decoherence/Zurek line of research has clearly made a lot of progress towards this goal. I remain confused about whether it has completely addressed certain problems, and various claims made by Wallace and others for solutions to these problems don't seem convincing (the explanation for the Born rule lies in decision theory???). In trying to resolve these confusing issues, I still don't see how invoking a "many worlds" ontology solves anything.

74. **Mateus Araújo**
August 29, 2019

Paddy, Peter,

I also don't think the approach to the probability problem taken by Wallace (and Carroll and Vaidman, for that matter) is the proper one. Sure, they can derive from reasonable assumptions that rational agents must assign subjective probabilities given by the Born rule. But who cares about subjective probabilities? The reason probabilities in quantum mechanics are interesting is precisely because they are objective, not subjective. And rational agents? Come on! Many-Worlds is a reductionist theory about an objective reality, this agent-centric nonsense belongs in Copenhagen.

I also think it is a huge missed opportunity, as making sense of objective probabilities is an age-old problem in philosophy, and the only solution I know is using many-world theories.

I'm using lower case here because the solution I know is using a classical many-world toy theory, Kent's universe, not the quantum Many-Worlds itself. The quantum case still doesn't have a satisfactory explanation.

Some Math News

August 27, 2019
Categories: Langlands, Obituaries

My Columbia colleague Patrick Gallagher passed away a few months ago at the age of 84. He had only recently retired, and for many years was the longest serving member of the department and an important part of its institutional memory. On October 10 there will be a memorial conference here at Columbia.

It's too bad Pat didn't live to see the latest from Terry Tao, who describes recent results which are related to old work of Gallagher's by "Our proof of this theorem proceeds more or less along the same lines as Gallagher's calculation, but now with k allowed to grow slowly with x."

Turning to other topics, Peter Scholze continues to come up with new ideas about the foundations of mathematics at a pace far too fast for me to fool myself into thinking I might be able to follow what he's doing. In recent months he has run a course on "Condensed Mathematics", which involves new ideas about topology developed with Dustin Clausen. For a video of a recent talk where he explains this, see here.

On the Langlands/representation theory front, some interesting things are:

- A course run by Geordie Williamson in Sydney earlier this year.
- If you're interested in what Williamson is up to, you might want to watch videos from "somewhere in Russia" last month, or "somewhere on Long Island" last week. Lecture notes are here.
- Also in Russia last month, at the Skoltech Center for Advanced Studies, where my Columbia colleague Igor Krichever is director, there was a skoltech summer school on mathematical physics, with some of the lectures on video here.
- Several new papers addressing different points of view about geometric Langlands are out, including a very recent one from Etingof, Frenkel and Kazhdan. For some background on the relation of this to work by Langlands himself, see here and here. For some geometric Langlands-related work of a different sort, see here and here.

Finally, the 2019 PCMI was devoted to the topic of *Quantum Field Theory and Manifold Invariants*, videos are here.

Update: Also on the Langlands/representation theory/quantum front, this week at Northeastern there's a conference going on (rumored to be celebrating the fact that Etingof and Okounkov share the same birthday, and are now 50). Videos are starting to appear here, including one of Edward Frenkel discussing the paper with Etingof and Kazhdan mentioned above.

Comments

1. **anon**
   September 5, 2019

   Breakthrough Prizes have been announced (they seem to be moving earlier each year, wasn't the announcement in December a few years back). The recipients are Alex Eskin in maths and, unsurprisingly, the Event Horizon Telescope collaboration in Physics.

2. **Peter Woit**
   September 5, 2019

   anon,
   That is weird. In the past they kept this a secret until the prize ceremony near the end of the year. Maybe they're trying to get in before the Nobels, announced in a few weeks.

   I also still don't understand why they've inverted their previous practice of giving the "special prize" for current experimental results (e.g. LIGO/2016 and Higgs/2013) and the regular prize for (often failed) theory. This year the "special prize" went to the failed SUGRA unification idea and the regular prize to a current experimental result (EHT).

   There's some value in making an award to an experimental collaboration like EHT (as opposed to certain individuals). Besides the problem of awards for failed theory, for the rest of the math-physics awards they're making, I think we'd be better off without them. They're no different than any number of other awards out there, other than embodying the misguided idea that what math and physics research need is more Hollywood glitz.

3. **social network**
   September 6, 2019

   Eskin has co-authored papers with 7 Fields medalists. I'd be surprised if anyone else has a Fields number (to coin a term) as high as 5.

4. **Jamie**
   September 6, 2019

   @social network. Eskin clearly deserved the prize. Those seven are Margulis, McMullen, Kontsevich, Okounkov, Lindenstrauss, Avila, and Mirzhakhani.

   Except for Margulis, the other 6 Fields medals came after 1998, and four of them mostly worked in dynamical systems. Also, Fields medals for work in dynamical systems (or closely related work) include Yoccoz in 1994 and Venkatesh in 2018. Let's not forget that Sinai got an Abel prize in 2014. Seems like a very prestigious field in mathematics.

5. **social network**
   September 6, 2019

   @jamie, presumably the "Fields number" positively correlates with (among other things) strength as a mathematician. If you are strong enough to collaborate with top people in the field, and they keep coming back to write more papers with you, that's a pretty good sign that you are strong in your own right. At least that's how I interpreted it.

   Dynamical systems, at least in the USA, has had the reputation of being more social than other areas of math. Between that and it being a field that "relates to everything" it makes sense for collaboration numbers to be higher there.

6. **anon**
   September 6, 2019

Bourgain seems to have had at least five medalists (Bombieri, Figalli, Lindenstrauss, Tao, Venkatesh) as co-authors (six, if you count Connes; they were both editors of a conference proceedings). I only checked the ones that I though the most likely to have co-authored with him, so I may have missed someone.

7. **Peter Woit**
   September 6, 2019

   All,
   Maybe that's enough discussion of the new "Fields number" metric.

Quantum Supremacy II

September 7, 2019
Categories: Quantum Mechanics

About the only thing that has transcended the bitter partisan divisions between Democrats and Republicans in the US during recent years has been quantum mechanics, with the enactment late last year of the National Quantum Initiative Act (the NQI was first mentioned on the blog here). In March there was a National Quantum Coordination Office established at the White House, and last week there was an executive order establishing a National Quantum Initiative Advisory Committee.

The NQI directs the federal government to spend \$1.2 billion over the next five years, with the NSF told to create two to five "Multidisciplinary Centers for Quantum Research and Education" and the DOE two to five "National Quantum Information Science Research Centers". Besides the NQI, pretty much everywhere you look the past few years you see new well-funded "quantum" centers popping up, two randomly chosen examples would be the Chicago Quantum Exchange and the Yale Quantum Institute. In the private sector, a huge investment in quantum science is taking place, driven by hopes that quantum computing and other applications will lead to a technological revolution and associated vast riches.

Looking at new books on fundamental physics that I've seen over the past year and a half, the conventional enthusiastic treatment of string theory/SUSY/extra dimensions is now dead, with Sabine Hossenfelder's Lost in Math the only popular book addressing these topics, and doing so in a quite negative way. The new trendy topic is the foundations of quantum mechanics, with the recent publication of Adam Becker's What is Real?, Philip Ball's Beyond Weird, Anil Ananthaswamy's Through Two Doors at Once, Lee Smolin's Einstein's Unfinished Revolution, George Greenstein's Quantum Strangeness, and Sean Carroll's Something Deeply Hidden. Forthcoming from Oxford University Press are two quantum books by Jim Baggott, Quantum Reality and The Quantum Cookbook.

On the whole this change in hot topic is a positive development, although the fact that it's driven by a lack of anything new to say about particle physics and unification is rather depressing. On the quantum front, while I think it's great that public attention is being drawn to quantum mechanics, if you look at my reviews you'll see that I have mixed feelings about the point of view taken by some of the recent books (the best of the lot I think is Philip Ball's).

The latest example of the high public profile of quantum mechanics is the publication today in the New York Times of a piece by Sean Carroll arguing that Even Physicists Don't Understand Quantum Mechanics: worse, they don't seem to want to understand it. Unfortunately I don't think that this article accurately describes the issues surrounding what we do and don't understand about "quantum foundations", nor the dramatically improving funding prospects for research in this area. In addition I don't think that it's accurate, fair (or good for public relations) to portray your colleagues as "not really interested in how nature really works", somehow not curious or bright enough to realize (see here) that there is a crisis at the heart of their subject and that, thanks to Sean Carroll:

> the crisis can now come to an end. We just have to accept that there is more than one of us in the universe. There are many, many Sean Carrolls. Many of every one of us.

Comments

1. **Blake Stacey**
   September 7, 2019

   > Not everyone was happy that Bohr's view prevailed, but these people typically found themselves shunned by or estranged from the field.

   Two examples do not add up to a "typically", no matter how famous those examples are. Nobody mentions Haag's debate with Bohr, for example, despite it being rather illuminating. Nobody reads the proceedings of the 1938 Warsaw conference, where Hans Kramers declared that "everyone knew the quantum theory was provisional" and Heisenberg was reported as speculating that quantum mechanics would have to break down at high energies. Nobody — and this surprises me — notes that Feynman took Everett seriously enough to criticize in his early '60s course on gravitation.

   Nor can Bohr coming off better in his debates with Einstein really be equated with his view *prevail*ing, not when even other physicists who disagreed with Einstein (like Heisenberg and Pauli) also disagreed with Bohr.

2. **Blake Stacey**
   September 7, 2019

   For that matter, the most famous riposte to the view that quantum mechanics can be "considered complete" is the EPR paper, and neither Podolsky nor Rosen were drummed out of the physics profession!

   Louis de Broglie doesn't seem to have suffered for returning to pilot-wave theory in the 1950s. I have heard that he quashed the careers of Jean-Louis Destouches and Paulette Destouches-Février for taking too Bohrian a turn in their work on quantum logic, but that's philosopher lore which may or may not be confirmable in a history book. And speaking of quantum logic, that's a thing that a small but determined set of researchers bustled away at in order to "dig more deeply", starting with Birkhoff and von Neumann and continuing with Jauch, Piron, Mackey, etc. I doubt that any of Andrew Gleason's colleagues criticized him for doing quantum foundations instead of getting back to Hilbert's fifth problem.

3. **GoletaBeach**
   September 7, 2019

   Sean Carroll's op-ed piece left me unimpressed. "What's surprising is that physicists seem to be O.K. with not understanding the most important theory they have." he says.

   Huh? We had Bell's work, boiled down nicely by David Mermin, certainly regularly taught in grad and undergrad quantum. We had a whole lot of delayed choice experiments, photon-by-photon build up of the two slit interference pattern, interference measured around solenoids showing the reality of the vector potential. We had just about every weird quantum correlation experiment that arose in decays of the phi meson and Upsilon(4S) interrogated… in the later, Sin(2beta) of CP violation is a bit harder to measure due the the collapse of the wave function caused by the first decay of the correlated pair. Considerable effort checked all of that experimentally.

   Experimentalists have tried really, really hard to get the first empirical clue that the simple-minded Copenhagen is off. Maybe it is Quantum Bayesian in fact, which kinda works, and kinda feels like: everybody, the details of the measurement are and always have been a big correlated mess between the system and the observation apparatus.

   No empirical clue of anything inconsistent has showed up. Maybe the experimental effort has even exceeded that devoted to supersymmetry. Neither experimental quest has found a shred of deviation from simple expectations. Not a reason to give up, but hardly a reason for new billion-$ initiatives… clever folks laboring away in Rutherfordian and Via Panisperna-like genteel poverty are as likely to find the new revolution.

   Many (not sure about "all") experimental condensed matter folks doing QIS are pretty blunt that they don't care much about the marquis item… and Gil Kalai points out that noise might deflate the grand expectations of quantum computing…. https://www.ams.org/journals/notices/201605/rnoti-p508.pdf

Mainly QIS is a good jugular of funding… and their standard old condensed matter funding mechanisms all got wildly political and unproductive. The card "what if the Chinese get quantum computing first and take over the world" is now getting played hard.

And a card that particle physicists cannot play. Getting supersymmetry first (or any new high energy or dark matter/energy phenomenon) won't enable anybody to take over the world. Perhaps speculative experimentation will gradually get ironed of particle physics… as all the money is needed for the world and US accelerator neutrino program.

4. **Lena Birkenfeld**
September 7, 2019

Peter,

If I remember correctly, in one of your past posts you introduced to us that Arnold Neumaier has a new deterministic thermal interpretation of quantum mechanics grounded upon the mathematical concept of coherent spaces, and links to his five papers. Now on one hand, it seems that his thermal interpretation and coherent spaces may be the concepts needed to resolve the issues in the foundations of quantum mechanics that Sean Carroll and others are bringing up, as well as unify quantum and classical mechanics together. On the other hand, Neumaier's ideas are still relatively new, and apart from this blog and on PhysicsForums, his ideas haven't attracted much attention (He hasn't yet had his five papers peer reviewed yet either.) He has a new book coming out on that topic on 24 October 2019 called "Coherent Quantum Physics: A Reinterpretation of the Tradition": https://www.mat.univie.ac.at/~neum/physfaq/therm/

5. **casper**
September 7, 2019

The responses so far have interpreted Sean's "quantum foundations" word choice as referring to topics outside of his apparent emphasis on quantum interpretations, to the point of confusing Sean's critique of physics culture for a critique of mathematics culture (Gleason), confusing quantum foundational work for questioning the empirical adequacy of the standard von Neumann recipe ("Neither experimental quest has found a shred of deviation from simple expectations"), and even broadening the definition to include QIS. It really shouldn't be all that controversial that working on quantum interpretational issues would not generally help one's case for tenure in physics departments in the years since WWII.

6. **Alessandro Strumia**
September 8, 2019

Writing that physicists are not interested in understanding quantum mechanics and suggesting that physicists pushed out of the field those who tried is worse than inaccurate. It's fake news. EPR, Bell and others who achieved interesting results were welcomed and discussed. The problem is that there are not so many interesting results, in particular when trying to address the key issue: why a probabilistic theory?

7. **Evgenii Rudnyi**
September 8, 2019

Well, we should just consider the process when Carroll makes a statement, for example,

"We just have to accept that there is more than one of us in the universe. There are many, many Sean Carrolls. Many of every one of us."

from the viewpoint of many worlds interpretation. During this process there appears a lot of Sean Carrolls speaking any possible statements including the negation of the above statement. Hence, why should we pay attention to a statement of just one of these copies?

P.S. The statement "Everett didn't even try to stay in academia, turning to defense analysis after he graduated" seems to be incorrect. According to the book The Many Worlds of Hugh Everett III by Peter Byrne, Everett did not want to stay by academia, he wanted to earn good money.

8. **chorasimilarity**
September 8, 2019

Your formulation "Forthcoming from Oxford University Press are two quantum books …" reminds me of a character from Saki who does not agree with his cook. He says something along the line that a rabbit served with a sauce with exotic ingredients does not become an exotic rabbit.

9. **Peter Woit**
September 8, 2019

Lena Birkenfeld,
I hope to find time to better understand what Neumaier is doing, likely will write here something about his book when it comes out. It's a quite different sort of thing though than the Carroll and other books mentioned here, much more technical, aimed at those with some expertise in the subject rather than the general public.

10. **Peter Woit**
September 8, 2019

Blake Stacey/casper,

Carroll's history, like that of Adam Becker's in his book, is just a caricature drawn for ideological reasons and I doubt anyone interested in the history will take it seriously.

More likely to be taken seriously is his description of the current situation, which also seems to me an unrecognizable caricature. As I pointed out in the posting, at the level of what the public is being told about fundamental physics, "foundations" is the hot topic and getting more attention than almost anything else. Of the huge influx of "quantum" grant money, new institutes and new positions, only a small fraction of it will go to "foundations", but some of it will, especially to work that has any sort of connection to the real world and the new possibilities being opened up for experiment by new technology. If your idea of "foundations"research is arguing about what is "real", you may continue to find your colleagues dubious of its value.

By the way, on this topic I recommend this from Will Kinney:
https://twitter.com/WKCosmo/status/1170668355847643136

11. **Blake Stacey**
September 8, 2019

> Carroll's history, like that of Adam Becker's in his book, is just a caricature drawn for ideological reasons and I doubt anyone interested in the history will

take it seriously.

This is a good point. However, I suspect that many people are more interested in having a Rationally Correct(TM) view of physics history, and so they will be content with the caricature because it was taught under the auspices of capital-R Rationality.

David Mermin coined the phrase "shut up and calculate!" to express the attitudes of his own professors during his graduate-school years, the late 1950s. He himself has been publishing on quantum foundations since 1980, starting with generalizing the Bell inequality in the pages of the *Physical Review*. Even if the "shut up and calculate" sentiment endures, I find it extremely implausible that the *set of questions to which it is applied* has remained invariant over the last sixty-odd years.

My day job is actually a problem that could be classed into quantum foundations or quantum information, depending on which way the light shines. Conversations about it tend to be richer and more fulfilling in the latter context, since the former field prefers to keep having the same old arguments with itself that it's been having for generations, while the latter is looking for new experiments to do. Even better still, in my experience, have been the interactions with the mathematicians whose attention is caught by quantum information reaching out into algebraic number theory. When I was in physicist school, I never expected I'd be teaching myself about Hilbert's twelfth problem.

Thank you for the pointer to Will Kinney's Twitter thread; I find much to agree with in it.

12. **Peter Shor**
September 8, 2019

Why do all these people who are unhappy with the current state of quantum foundations think the universe owes us a simple, intuitive picture of what is going on?

We have at least three intuitively very unsatisfactory, but perfectly consistent interpretations of quantum mechanics ("many worlds", Bohmian pilot wave, and Copenhagen) which all seem to predict exactly the same thing. Mathematically, it's a coherent and quite beautiful theory (at least until you try to add gravity). Why does there need to be a simple, intuitive foundation to it? There's nothing really wrong with looking for one, but there's also nothing wrong with looking for experimental evidence for dark matter candidates. You just shouldn't be surprised if you don't find them.

13. **Michael Weiss**
September 8, 2019

Peter, in 1977 Sidney Coleman prominently concluded his undergraduate course in introductory QM with a final lecture on Bell's Theorem ("Physics 143" at Harvard). Given Sidney's stature in that decade as both a leading theoretical physicist and a master lecturer (indeed, graduate students and post-docs from around Boston routinely attended his QFT lectures in Physics 253), surely this was a clear sign that Bell's Theorem had entered the mainstream of physics education by that time.

Prior to his final illness, Sidney gave a lecture on further Bell-related developments in the early 1990s, which is still available on the Harvard Physics website.

I also remember chatting with Ed Purcell somewhere in the 1985-1988 period about David Mermin's exposition of Bell's argument. Ed shared that Mermin had felt honored and thrilled that Richard Feynman wrote him a personal note congratulating David on putting together such as lucid account. My impression was that this was a very special form of peer recognition to Mermin.

Finally, Harvard's Francis Pipkin was a major advocate for Alain Aspect in the 1970s (as I gleaned in 1978 in the course of Physics 191; a lab course with much informal time for wide-ranging discussions); I understand that Pipkin helped ensure Aspect's promotion and tenure (told to me by someone else).

These anecdotes argue against Sean Carroll's suggestion that "mainstream elite" physicists had dismissed Bell's work as uninteresting and irrelevant.

14. **Mark Hillery**
September 8, 2019

I agree with pretty much everything Blake Stacey, Will Kinney, and Peter Shor have said, but I would like to comment on a few points in the article. First this:

"For years, the leading journal in physics had an explicit policy that papers on the foundations of quantum mechanics were to be rejected out of hand."

Assuming the journal in question is the Physical Review, I can say that as a soon to be retired associate editor at Physical Review A, who has handled parts of quantum information and, on and off, parts of quantum foundations for 15 years, this simply is not correct. We have published a very large number of papers on Bell inequalities and variations on them. There are papers on axiomatizations of quantum mechanics, and attempts to explain why there is a gap between the maximum correlations allowed by mathematics and those allowed by quantum mechanics (for more information on this see Tsirelson's theorem and Popescu-Rohrlich boxes). We recently published a paper in which experimental data was re-analyzed in order to place bounds on the collapse parameter in the GRW model (mentioned in the Carroll piece). This list is by no means exhaustive. Exactly what the policies were before my time, I cannot say, but there are papers in the Physical Review on Bell inequalities from at least the early 1980's.

Second there is this:

"The current generation of philosophers of physics takes quantum mechanics very seriously, and they have done crucially important work in bringing conceptual clarity to the field."

This certainly is not the way I see things. The few philosophers of physics I am aware of have a strong preference for Bohmian mechanics, which is a subject almost completely ignored by working physicists. This very much limits their influence. Also, from what I have seen, the philosophers further diminish whatever effect they may have by their aggressive style.

Finally:

"After almost a century of pretending that understanding quantum mechanics isn't a crucial task for physicists, we need to take this challenge seriously."

Again, this ignores a lot. The decoherence program pioneered by Zeh and currently being pushed by Zurek, is taken quite seriously and has been going on for quite a while.

In conclusion, as has been noted by other commenters, Carroll's piece paints a very misleading picture of the past and present of research in quantum physics.

15. **Peter**
September 8, 2019

I can confirm that Bell, EPR etc. were part of the curriculum in the early 1980s when I studied physics (somewhere in Europe …). It wasn't very thorough and nobody left the classroom with the idea this was a hot research topic(*), but it's not correct that foundational questions were neglected.

(*) I left the classroom with the idea that a) something was missing in the "interpretation" of QM, but b) every experiment confirmed the predictions of QM and one could do good physics without worrying about interpretations. Personally, I looked at the achievements of QM from the hydrogen atom to electroweak theory and thought taking position b) had its merits.

16. **Peter Shor**
September 8, 2019

And my previous comment, transmuted to poetry (or at least to words with rhyme and meter):

If the eternal dance of molecules
Is too entangled for us mortal fools
To follow, on what grounds should we complain?
Who promised us that Nature's arcane rules
Would make sense to a merely human brain?

17. **Dave Miller**
September 9, 2019

Peter,

I'm older than most of the posters here, and perhaps the only one who had a strong interest in QM foundations as early as 1970.

No one seems to be noticing that the very testimonials here actually document a phase transition in the profession's attitude towards foundational issues in QM between the early '70s and mid '80s.

Michael Weiss says, "Peter, in 1977 Sidney Coleman prominently concluded his undergraduate course in introductory QM with a final lecture on Bell's Theorem." That is the earliest sign I know of the transition. Michael is of course correct that Sidney was highly respected; on the other hand, Sidney was a bit of a maverick, and it is not surprising that he was an early harbinger of the transition.

When I took QM from Feynman in the 1974-'75 academic year, there was not *one word* about Bell's theorem (much less Bohmian mechanics or MWI!). I myself tried to engage Feynman in discussions about QM foundations: I'd known him personally since early in my freshman year, and he was generally open to discussions with undergrads. However, he just would not address QM foundational issues: indeed, the only thing he said about QM foundations in the QM class was a rather stern warning to all of us to avoid research in the area, since it was a career killer.

And yet, Michael cites evidence that "Mermin had felt honored and thrilled that Richard Feynman wrote him a personal note congratulating David on putting together such as lucid account" in Mermin's famous *Physics Today* article in 1985.

Yes, that is my point: Feynman's (and the profession's) attitude changed radically between 1974 and 1985 (with thanks to Sidney Coleman, Dave Mermin, et al.).

The pre-1975 attitude, by the way, was not that you were an evil person who deserved to be driven out of the field if you were interested in QM foundational issues; the attitude was simply that this was a fruitless line of endeavor, and that work in that area would generally not earn you credit among your colleagues.

Again, John Bell is an example: he was respected as a legitimate physicist who did real work at CERN but who happened to dabble in foundational stuff on the side.

I myself did a term paper on Bell's theorem in the 1974-75 year, not for Feynman's class but for a "Modern Physics" class. The prof liked it, and I earned an "A." I knew I would not be punished for my interest, but I also believed Feynman that making it my main research topic was not a good way to advance my career. Indeed, as late as, say, 2000, how many people considered giants in physics had made their name by working on foundations? Witten? Weinberg? Polchinski? Coleman himself?

I can think of none.

Again, it is true that a physicist prior to the transition who showed an interest in QM foundations was not vilified and expelled from the field. But, I know of no one prior to 1980 who made QM foundations the core of his research efforts and who was respected as a result by most other physicists.

It is of course completely different now: our friend Peter Shor is (quite rightly) famous.

My strong impression from Feynman, by the way, was that his generation had just been exhausted when they were young physicists hearing about the Bohr-Einstein debates, etc. and had understandably decided that all that was a waste of time.

And they had a point: for decades, the "real action" was in QFT, gauge theories, the quark model, etc. Bell's 1964 paper seemed not to be fruitful for further research: the initial experimental tests were not until the '70s and '80s, and it is really only in more recent decades that Bell's relevance to quantum computing has made Bell's work seem fruitful.

As Max Planck famously said (often misquoted as "one funeral at a time"), "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

Dave

18. **WTW**
September 9, 2019

For a very recent paper on experimental evidence of Quantum Darwinism, by Thomas Unden, with other authors including Zurek, see
Revealing the emergence of classicality in nitrogen-vacancy centers
https://arxiv.org/abs/1809.10456
(submitted to PRL)

See also: To catch and reverse a quantum jump mid-flight, by Z. K. Minev et al,
https://arxiv.org/abs/1803.00545
(in Nature, June 2019, https://www.nature.com/articles/s41586-019-1287-z)

They seem fairly "foundational" explorations of quantum mechanics, and as far as I can tell done outside of any "Quantum Initiative". But very interesting explorations of the details underlying quantum processes and measurement, without having to invoke any other copies of Dr. Carroll.

One comment: I have read multiple accounts of how the personality and position of Bohm stifled dissent re. problems with his ongoing interpretations of QM, including among some other very well known physicists. Even when apparent disagreements were stated, they tended to be swept under the rug or somehow merged together as "quantum weirdness". See, for example, https://plato.stanford.edu/entries/qm-copenhagen/#DivVie . More recently, physicists working primarily on aspects of string theory or particle physics topics with whom I have spoken also tend to sweep away any such issues as being either uninteresting or irrelevant to the core of the problems on which they are working. (And in fact the only people willing to spend much time talking about such things have been in the Philosophy of Science department : -) I have no idea how general or widespread such things actually are, but from a limited sample, Carroll isn't too far wrong in terms of how most theoretical physicists have (not) approached these issues.

19. **Lee Smolin**
September 9, 2019

Dear Peter,

Can I comment as someone whose first class in QM was Freshman QM in 1972, ie just before the phase transition Dave Miller mentions?

First of all, its true that American physics in general has had a pragmatic, anti-foundational orientation since it became dominant after WWII. Putting aside that there were felt to be good reasons for that (see remarks of Dyson, Feynman etc) , that made it unsurprising when we students were sincerely advised to work on building the present paradigm rather than attempt to over-through Copenhagen (ie text-book QM).

In the face of this, the very small number of us determined to work on quantum foundations took several strategies, and many succeeded. One was to split one's time between more mainstream (ie funded) and foundational issues (Bell, Adler, 'Sorkin, 't Hooft, Mermin, etc), one was to go to teach at a first class small college (Bernstein, Pearle, Wooters), one was to go to or stay in Europe (Penrose, Isham, Deutsch, Valentini, etc,) which was more diverse, finally one could work on QF from a math (Goldstein,Nelson) or philosophy (Shimony, Albert, Maudlin, Sanders, etc) department. Finally, one could work in industry (Bennet, Shor) or defence (Zurek)

Indeed enough of us succeeded, that in spite of the discouragement, as someone interested in foundations from the early 70's, there were always new ideas to ponder and work on and people to discuss them with. And what was most important, was that the crucial experiments got done-even if, as with tests of Bell, it took several attempts till the experimentalists had clean results. (And they were done at high status places: Harvard, Berkeley, Paris, MIT, Vienna…)

So if we are honest, I don't think we can argue that the progress of QF between the
1960's and the QIT revolution in the 2000's was personnel limited. These are very hard problems that go to the core of our understanding of nature. They require a different temperament than makes a successful mainstream physicist. Not many people have the interest, patience, focus and courage to work on them.

In addition, from 1972 on was a very tough job market for all of theoretical physics. So I think the overall story is that the problems in QF are so compelling, for those with a taste for foundational kinds of issues, that enough of us succeeded, against advice and expectations that we now have in front of us a much more interesting and diverse set of approaches and hypotheses, than faced the field in the 1950's.

Thanks,

Lee

20. **Yatima**
September 9, 2019

Sean Carroll writes at Quanta Magazine:

Where Quantum Probability Comes From

and as expected defends Many-Worlds, and derives the Born rule from first principles:

Can we resolve this uncertainty in a sensible way? Yes, we can, as Charles Sebens and I have argued, and doing so leads precisely to the Born rule: The credence you should attach to being on any particular branch of the wave function is just the amplitude squared for that branch, just as in ordinary quantum mechanics.

Still not convinced. "Knowing" or "attaching credence" ultimately means storing classical bits in massively copied physical state (including a fat chunk of previously recorded history and the algorithms needed to process the whole shebang into a prediction or a floating-point value). It's not some metaphysical essence that a system in quantum superposition could even have.

(For an earlier reconstruction of the Born rule from first principles, there is this article by Philip Ball, also at Quanta: Mysterious Quantum Rule Reconstructed From Scratch.)

21. **Peter Woit**
September 9, 2019

Yatima,
Thanks for the pointer to that new Carroll article, but I'm afraid I want to discourage discussion of it here, since it's a topic the moderator has no interest in…

22. **Peter Shor**
September 9, 2019

Lee Smolin:

I don't believe Feynman was truly anti-foundational. When I was at Caltech as an undergrad, around 1980, he gave a talk on negative probabilities, and said that his motivation was that he had looked at Bell's theorem, and realized that there was a hidden hypothesis that all probabilities were non-negative. So maybe if you allowed negative probabilities, you could understand the foundations of quantum mechanics.

He later published a paper on negative probabilities where, with typical Feynman showmanship, he completely omitted to mention his motivation, presumably because he couldn't get it to work.

There are lots more mathematicians working on the Riemann hypothesis than there are mathematicians willing to admit it (just like there were lots more mathematicians working on Fermat's Last Theorem than there were ones willing to admit it). I expect there are currently lots more physicists working on foundations than are willing to admit it.

If I'm correct about how many physicists have worked on quantum foundational problems without telling anybody, they're even a lot harder than we think they are.

23. **Blake Stacey**
September 9, 2019

If anyone wants to read Feynman's paper on "negative probabilities", it's here. He defines what would nowadays be called a discrete Wigner function for a qubit (a topic that you can indeed get into PRL for).

24. **Jim Baggott**
September 10, 2019

I think it's important to realise that the general negativity around quantum foundations in the 20/30-year period to the 1960s or early 1970s wasn't so much about a commitment to the Copenhagen (shut up and calculate) orthodoxy as a genuine belief that these were questions that couldn't be answered by experiment. If, as it seemed, these were 'just' philosophical questions based on different views about how nature should behave then, arguably, they didn't belong in physics. This wasn't just about Bohr's pragmatism or Heisenberg's positivism. Remember that in the 1930s von Neumann had published a proof that all hidden variable extensions of quantum theory are impossible.

Many argue that all this changed with Bell, who dismissed von Neumann's 'impossibility proof' and devised his famous theorem and inequality, but I think this process started with Bohm, under the influence of a conversation he had with Einstein in the early 1950s.

This change was critical. As soon as it became apparent that there were indeed some foundational questions that could be answered by experiment, interest grew and foundations became a 'respectable' part of physics, which is why it started to appear even in a small number of introductory QM courses. I fear that Carroll's particular spin on history and his arguments in support of a MWI which does nothing more than wrap the standard quantum formalism in a thick blanket of metaphysics does more harm than good to the credibility of quantum foundations in a pragmatic and empirically-grounded physics community. In a piece to be published online early next month by Aeon, I argue that this kind of stuff is dangerous and threatens to undermine the authority of science just when it is under unprecedented attack from anti-scientific and pseudo-scientific propaganda.

25. **Blake Stacey**
September 11, 2019

> Remember that in the 1930s von Neumann had published a proof that all hidden variable extensions of quantum theory are impossible.

I'm glad someone mentioned this. Adam Becker made a fairly big deal out of it in his book, in a way that I found incomplete enough to be misleading. But there's an angle that's more significant to the theme of how quantum foundations research is done today. Von Neumann's flawed theorem is *fixable* if one generalizes from his notion of measurement to that of POVMs, as is commonly done in quantum information theory. The additional structure of the set of POVMs furnishes a replacement for von Neumann's additivity assumption, the point where his argument was criticized by Grete Hermann and probably by Einstein. Of course, it is not so shocking to say that changing the assumptions will change what can be proved, but it's part of the story. And, topically, this part of the story was told in *Physical Review Letters*.

Regarding Papers about Fundamental Theories

September 9, 2019
Categories: Favorite Old Posts, Quantum Mechanics

Discussion in the comment section of the previous blog entry led me to do a little bit of historical research this morning, and I thought I'd write up the results here. First of all, for some interesting comments from people around back then about how attitudes in the physics community changed during the 1970s, see here, here and here.

What I looked into is one specific story, trying to figure out what was behind Sean Carroll's claim in the New York Times that

> For years, the leading journal in physics had an explicit policy that papers on the foundations of quantum mechanics were to be rejected out of hand.

Mark Hillery here notes that this is likely a reference to the *Physical Review*, and that it very much has not been true for the past 15 years, during which he has been an editor there.

Tracing back where Carroll got this from, I guessed that (since it's the historical source he recommends in his book) it came from Adam Becker's book, *What is Real?*. Looking at that book one finds on page 214:

> *Physical Review* actually had an explicit editorial policy barring papers on quantum foundations unless they could be related to existing experimental data or made new predictions that coulhered be tested in the laboratory.

This matches Carroll's claim (with the part inconvenient for his case deleted…). Becker's source notes for this text refer to an editorial in the July 15, 1973 issue of Physical Review D (Particles and Fields) written by Samuel Goudsmit, the editor-in-chief. The editorial is entitled "IMPORTANT ANNOUNCEMENT: Regarding Papers about Fundamental Theories". Goudsmit does not specifically refer to quantum foundations papers, but writes:

> The subject matter of these papers usually concerns a fundamental aspect of theoretical physics. Extreme verbosity and vagueness of expression makes these papers hard to read and understand. A paucity of mathematics as compared to wordage distinguishes them from the more conventional theoretical papers. The author proposes new theories, but their specific assumptions are usually hidden behind very lengthy arguments. Sometimes the paper contains a reinterpretation of existing theories which the author considers more satisfactory than the prevailing views, though no new experimental consequences are expected.

He sets forth the following as features expected of articles publishable by the *Physical Review*:

> All implied assumptions must be stated clearly and concisely and as much as possible expressed in mathematical form.
>
> The author must convincingly show
>
> - that these assumptions lead to the explanation of hither to unexplained observations, or
> - that these assumptions expose new relations between known data or theories, or
> - that these assumptions are simpler and fewer than in existing theories.
>
> Moreover, the author must show that the new assumptions do not contradict existing experimental facts.
>
> He must also investigate possible new consequences of his assumptions and whether these could be tested by new experiments.

Looking some more into this, I realized that I had first seen this story in David Kaiser's book *How the Hippies Saved Physics* (see review here), which clearly is Becker's source (Becker's next note refers to this). On page 121 Kaiser has:

> The longtime editor of the *Physical Review*… actually banned articles on the interpretation of quantum mechanic. He went so far as to draw up a special instruction sheet to be mailed to referees of potentially offending submissions: referees were to reject all submissions on interpretive matters out of hand, unless the papers derived quantitative predictions for new experiments.

Kaiser goes on to quote John Clauser as pointing out that according to this policy, Bohr's response to the 1935 EPR paper would not have been publishable. His source notes refer to the Goudsmit editorial and private emails from Clauser on July 8, 2009. The same note also refers to an article by Clauser, Early History of Bell's Theorem, which has a lot of detailed information about the story of the reception of Bell's theorem and early efforts to do experimental tests (but nothing about the *Physical Review* policy). By the way, back in 1964, Bell decided not to submit his important paper to *Physical Review*, not because of any policy they might have had, but because they had page charges.

So, as far as I can tell, the historical record shows that the documented Physical Review policy didn't, as the descriptions by Kaiser, Becker and Carroll suggest, explicitly refer to papers on the interpretation of quantum mechanics or quantum foundations. Possibly it was such papers that were annoying Goudsmit and led to his editorial, but I'd be curious to know if anyone knows more about what was specifically bothering Goudsmit. What sort of papers were being submitted to Physical Review D around 1972-73 that would uncharitably fit the negative description he gives quoted above?

Update: In the comments Blake Stacey suggests that the 1972 experiment of Freedman-Clauser may have been what led to papers being submitted to Physical Review that inspired Goudsmit's July 1973 editorial. Looking more into this, it's quite possible that the kind of thing Goudsmit was concerned about were the sorts of papers Jack Sarfatti was writing around this time. According to Kaiser's book (pages 62-63), it was just a few months before this that Sarfatti decided to change the sorts of papers he was writing:

> By the early 1970s, having published a few articles in prestigious journals on quantum theory, elementary particles, and even some idiosyncratic ideas about miniature black holes, Sarfatti could list half a dozen distinguished physicists scattered across the United States, Britain and France as references to vouch for the quality of his work…
> … Sarfatti began to lose enthusiasm for his position at San Diego State during the early 1970s, and indeed for the sterile direction in which he saw theoretical physics heading. He announced his new plans in a letter to renowned Princeton physicist John Wheeler in the spring of 1973… Sarfatti declared that he would leave his "uninspiring institution" and seek out "the best possible environment to create a great and historic piece of physics. I feel impelled by history – a certain sense of destiny," he explained. ("I recognize that I may be suffering under some sort of 'crackpot' delusion, but I cannot accept that as likely. In any case, I must try," he averred).

See the comment here for some of the sort of papers Sarfatti was writing at the time, quite possibly submitting them to *Physical Review*. The opening sentence of Goudsmit's description of the problem (and the fact that he was publishing it in Phys. Rev. D, Particles and Fields)

> The subject matter of these papers usually concerns a fundamental aspect of theoretical physics.

seems to me more likely to be referring to the sort of thing Sarfatti was writing than to papers on the interpretation of quantum mechanics.

Update: It turns out that Goudsmit's papers are available online, here. A non-exhaustive search turned up no evidence pro or con for my conjecture about Sarfatti or similar papers. I only found one set of files (Box 50, Folder 45, "Leibowitz refereeing, 1973") referring to his 1973 editorial. These have to do with this paper, which was published September 1973, after two years of refereeing. This publication led to another author writing a paper criticizing the first, leading to another refereeing problem. Goudsmit weighed in (January 5, 1976) by noting that it was exactly this kind of paper and the problems with refereeing such things that his editorial had been concerned with. Note that the paper in question is NOT an interpretation/foundations paper. Goudsmit writes:

> The event shows again clearly the necessity of rapid rejections of questionable papers in vague borderline areas. There is a class of long theoretical papers which deal with problems of interpretation of quantum and relativistic phenomena. Most of them are terribly boring and belong to the category of which Pauli said, "It is not even wrong". Many of them are wrong. A few of the wrong ones turn out to be valuable and interesting because they throw a brighter light on the correct understanding of the problem. I have earlier expressed my strong opinion that most of these papers don't belong in the Physical Review but in journals specializing in the philosophy and fundamental concepts of physics.

He then refers to his earlier editorial.

Looking at the exchange in these letters, the referee of the second paper writes "I would suggest that understands neither relativity nor quantum theory." This example suggests that the problem Goudsmit had in mind when writing his editorial was not a need for "an explicit policy that papers on the foundations of quantum mechanics were to be rejected out of hand", but just a need to deal with the common problem that is still with us, for both journals and the arXiv. There are lots and lots of people writing low quality papers claiming to say something new about the foundations of physics, on a continuum from the crank to the not so bad. Refereeing such things is difficult and time-consuming, so a journal needs a policy to deal with them quickly and efficiently, otherwise they end up with the mess described in these letters. Goudsmit's editorial I think was an attempt to come up with such a policy.

Update: Jorge Pullin wrote to remind me of an earlier Goudsmit story, which I described on the blog here, but had completely forgotten about. In 2008 an undated paper from Bryce DeWitt's files (he passed away in 2004) was posted on the arXiv. It included a claim much like the ones discussed here that refer to the 1973 editorial, but about a much earlier incident:

> Most of you can have no idea how hostile the physics community was, in those days, to persons who studied general relativity. It was worse than the hostility emanating from some quarters today toward the string-theory community. In the mid fifties Sam Goudsmidt, then Editor-in-Chief of the Physical Review, let it be known that an editorial would soon appear saying that the Physical Review and Physical Review Letters would no longer accept "papers on gravitation or other fundamental theory." That this editorial did not appear was due to the behind-the-scenes efforts of John Wheeler.

I don't know of any other evidence for this (took a quick look in the Goudsmit online archive, didn't see anything). It seems highly likely that this claim about Goudsmit and the Physical Review is not accurate. One minor problem with a claimed "mid-fifties" planned editorial for Physical Review Letters is that PRL wasn't even started until mid-1958. More seriously, the idea that the Physical Review in the mid-fifties would consider banning "papers on gravitation or other fundamental theory" is just completely implausible, and if that phrase is accurate, it surely is very much taken out of context. This story is very similar to the Carroll one about the 1973 editorial, and I'm guessing the true story about the mid-fifties incident is again just that Goudsmit was even then struggling with how to deal with bad "not even wrong" theory papers about fundamental physics.

Update: Steven Weinberg has a version of the "mid-fifties" Goudsmit story, in his biographical notice for DeWitt, in the context of a discussion of the January 1957 Chapel Hill conference on gravity:

> Samuel Goudsmit had recently threatened to ban all papers on gravitation from Physical Review and Physical Review Letters because he and most American physicists felt that gravity research was a waste of time.

Again, there's a problem with this that PRL wasn't even started until a year and a half later, and he has Goudsmit specifying just GR research, not the wider "gravitation or other fundamental theory" which DeWitt gave in quotes.

Comments

1. **Blake Stacey**
   September 9, 2019

   Goudsmit sounds a bit like the mathematicians who got tired of receiving proofs of Fermat's Last Theorem in the mail.

   He does say that "there exist excellent journals publishing articles on the foundations and on the philosophy of science", which is not really the blanket condemnation of quantum foundations one had been led to expect, just a "stop sending it here, please".

   As I mentioned in the previous thread, David Mermin was publishing on Bell inequalities in the *Physical Review* family in 1980 (*Phys. Rev. D* **22**, 356). Bernard d'Espagnat was doing so even earlier (for example, *Phys. Rev. D* **11**, 1424, received in 1974 and published in 1975). McGuire and Fry published there in 1973 (*Phys. Rev. D* **7**, 555), following up on Freedman and Clauser's "Experimental Test of Local Hidden-Variable Theories" from 1972 (*Phys. Rev. Lett.* **28**, 938). At a guess, the Freedman–Clauser experiment had made Bell-inequality tests enough of a thing that people started writing shoddy papers on the topic.

2. **Peter Woit**
   September 9, 2019

   Blake Stacey,
   "At a guess, the Freedman–Clauser experiment had made Bell-inequality tests enough of a thing that people started writing shoddy papers on the topic."

   That sounds quite plausible. I was trying to think of what could have happened around 1972 that might have been the impetus for the Goudsmit editorial. Given the traditional pattern in HEP theory of new advances quickly being followed by lots of papers rushed into print by people trying to get near the front of a bandwagon, there may have been a lot of such papers coming into Physical Review in late 1972-early 1973. Perhaps Goudsmit was trying to lay down some rules as to what they would require of such papers to consider them.

3. **Blake Stacey**
   September 9, 2019

   An interesting historical tidbit from Clauser's article is that the very term *Bell's theorem* was coined in the august pages of *PRL*.

4. **GoletaBeach**
   September 9, 2019

   In my experience Phys. Rev. could always turn difficult when it wanted to… still does sometimes. A classic in Phys. Rev. Lett. history was the years when they decided Lagrangean instead of Lagrangian…. https://physicstoday.scitation.org/doi/pdf/10.1063/1.2811374

   They also wouldn't publish Wino/Zino for the superpartners of the W/Z for a while.

Not sure if any journal initially wanted to publish quarks (Gell-Mann) or aces (Zwieg).

Encouraging well-thought out quantum models that have empirical consequences is a good thing. No surprise to me that the terrific Stuart Freedman was involved in an early Bell test…

He was one of the many terrific students of the late Gene Commins, and, later on, he helped give birth to the KamLAND experiment, which showed that antineutrino oscillations measured entirely on Earth were consistent with the solar neutrino problem.
https://newscenter.lbl.gov/2012/11/16/in-memoriam-stuart-freedman-renowned-nuclear-physicist/

5. **Dave Miller**
September 10, 2019

Peter,

Thanks for digging this up. On the one hand, this explains some of the frustration felt by those of us who actually were interested in quantum foundations in the early '70s.

On the other hand, those of us who have followed quantum foundational work for the last fifty years can only note that Goudsmit's complaints are just about as timely today as when he wrote them:

> The subject matter of these papers usually concerns a fundamental aspect of theoretical physics. Extreme verbosity and vagueness of expression makes these papers hard to read and understand. A paucity of mathematics as compared to wordage distinguishes them from the more conventional theoretical papers.

Although I would note a more recent twist: papers that present some very complex mathematics that, it is claimed, reveal the secrets of quantum mechanics — except that no effort is actually made to connect this complicated math with actual physical issues such as the "collapse of the wave function," the Born rule, or the measurement problem.

So, even a low verbiage to math ratio is not enough.

I do, however, have some information on your conjecture:

> That sounds quite plausible. I was trying to think of what could have happened around 1972 that might have been the impetus for the Goudsmit editorial. Given the traditional pattern in HEP theory of new advances quickly being followed by lots of papers rushed into print by people trying to get near the front of a bandwagon, there may have been a lot of such papers coming into Physical Review in late 1972-early 1973.

From 1977 onward, I was a grad student at SLAC and tried to be fairly diligent about looking at the SLAC library's collection of new preprints: I was very interested at the time in quantum foundational issues but I recall very few preprints on quantum foundations (and the library did get lots of preprints on lots of topics, not just HEP)..

I also recall no seminars at either SLAC or on-campus on quantum foundations (except, curiously, for some guys in the Engineering and Economic Systems Department who had somehow gotten interested in quantum foundations). And, I do still recall a number of those seminars — one on perturbative quantum gravity, one by Sidney Coleman, one by Haim Harari on preons, a horrible seminar by Zichichi, etc.

In retrospect, of course, there was a lot of important work being done by pioneers testing Bell's theorem. But, the visibility in the physics community was low, and it seems to have been a very small fraction of the overall work in physics.

Dave

6. **Severin**
September 10, 2019

Have you ever considered going on Sean Carrolls podcast? I think this kind fact-checking and gentle push back would make for a way better conversation then what is currently on offer.

7. **Peter Woit**
September 10, 2019

Dave Miller,
I think it's also important to note that the mid-seventies was a period when the Standard Model had just come together, the November revolution of 1974, GUTs, SUSY had just appeared, the non-perturbative study of QCD had begun, etc. There were a lot of exciting things going on in fundamental physics at high energies, which I think had a lot to do with relative lack of interest in the measurement problem.

Severin,
I haven't been invited.

8. **Dave Miller**
September 10, 2019

Peter,

Yes, we were all excited by the Standard Model coming together, and people were also starting to think about supersymmetry/supergravity, quantizing gravity, etc. I actually heard about the November Revolution in November '74 from Feynman who had been in touch with the guys at SLAC and passed the information on to our QM class.

Strangely, Feynman never mentioned QCD or non-abelian gauge theories, even in his Intro to Particle Physics class that I took in '75-'76!

I think I first heard about non-abelian gauge theories in the summer of '76 from some experimentalist grad students I was working with at SLAC before I officially started grad school.

My own graduate work was on the tau lepton, discovered just before I entered grad school, which was pretty cool: gave me a chance to work with Marty Perl.

Still, all of us students were puzzled then, as students are now, by the weirdness of QM, and we were discouraged from pursuing that interest at all.

Dave

9. **Sebastian Thaler**

September 10, 2019

Popular culture occasionally voices its own opinions on the foundations of quantum theory:
https://www.facebook.com/berkeleybreathed/photos/a.114529165244512/2731285340235535/?type=3&theater

10. **Blake Stacey**
September 10, 2019

Given how nonspecific his editorial was, I suppose it's possible that Goudsmit was motivated by more than one subject. Would 1972 have been a good year for the crop of bootstrap models?

11. **Peter Woit**
September 10, 2019

Blake Stacey,
The height of the bootstrap was earlier, with Chew's book in 1966. Besides impetus from a specific physics craze, the other thing that occurred to me is that 1973 would have been about the time new people entering the field would have included a large number of dope-smoking long-hairs, and maybe this lifestyle was reflected in their writing. From the story told in Kaiser's book, there would be a certain amount of overlap between this explanation and the "people interested in Freedman-Clauser" explanation.

Another possibility is that Goudsmit was looking at the early string/dual models, realized how much trouble they could lead to, and was trying to get ahead of the string theory problem early on…

12. **GoletaBeach**
September 10, 2019

The Freedman/Clauser paper was cited about 11 times per year throughout the 1970s… I recall a lot of discussion about experimental tests in that decade. Local peak in citations in 1977.

It was 1981-1997 that the average citations were low… less than 8 per year, but a steady dribble.

1998-2019… citations of that paper have increased to about 30/year, with 46 citations in 2017.

The deeper issue: what does "understand" mean, exactly? Experimentalists are pretty satisfied if the equations match their data, and when that match happens consistently and repeatedly… something about having hands-on the data takes the edge off, and further, choosing which experiments to perform is a big, big, deal for experimentalists… theorists do toss off ideas without much worry as to just how immense the undertaking is to really do the experiment.

By and large it is theorists who claim we don't "understand". A bit too trite to just quote Upton Sinclair… I'm not saying Sean Carroll falls under this quote, but it might be true that book contracts and royalties do…

"It is difficult to get a man to understand something when his salary depends upon his not understanding it."

Might add… "when his perceived national security w/r to the Chinese depends on him not understanding it"… where "it" might be… the intrinsic noise in all quantum systems.

Adapting Big Bill Haywood… "A theorist is someone who leaves the room when discussions of noise and background for a proposed experiment break out."

13. **Low Math, Meekly Interacting**
September 10, 2019

Do the "dope-smoking long-hairs" referred to above include the Fundamental Fysiks Group? I take it quantum foundations and related philosophical concerns (as well as some acid-fueled musings of a more speculative nature) were right up their alley. Were they and their ilk especially prolific writers circa 1972?

14. **Peter Woit**
September 10, 2019

LMMI,
Yes, one subset of the dope-smoking long-hairs would have been some of the Fundamental Fysiks Group members (surely there were plenty others though). Kaiser tells their story in great detail, with one of the main characters Jack Sarfatti. If you go to the HEP database INSPIRE, you can see that in late 1973 Sarfatti was producing lots of papers (five in the last three months of 1973), and it may very well have been those that Goudsmit was trying to find an excuse for rejecting without refereeing.

The funny thing is that the titles of these papers sound very modern, could be titles of papers on the arxiv today, e.g.

The World On A String
Jack Sarfatti (ICTP, Trieste). Nov 1973. 7 pp.
PRINT-74-0117 (ICTP,TRIESTE)

Toward A Unified Field Theory Of Gravitation And Strong Interactions
Jack Sarfatti (ICTP, Trieste). Oct 1973. 7 pp.
PRINT-74-0081 (ICTP,TRIESTE)

Quantum Gravitational Dual Strings: Violation Of Time Reversal Invariance
Jack Sarfatti (ICTP, Trieste). Nov 1973. 7 pp.
PRINT-74-0116 (ICTP,TRIESTE

Explanation For The Asymmetry Between Matter And Antimatter In The Visible Universe
Jack Sarfatti (ICTP, Trieste). Nov 1973. 5 pp.
IC-73-175

and, a few months later

Quantum Mechanics as a Consequence of General Relativity
Jack Sarfatti (ICTP, Trieste). Mar 1974. 4 pp.

It's quite possible that Goudsmit's problem was this kind of thing, not papers about interpretations of QM and the implications of Bell's theorem.

15. **Jackiw Teitelboim**
September 10, 2019

Looking at this last paper by Sarfatti, it (embarrassingly?) looks not so different in (the lack of) content (and rigor) from a talk "QM=GR" delivered recently by Susskind in the t'Hooft Fest:

https://www.uu.nl/en/events/conference-t-hooft-2019

Maybe Susskind should start acknowledging Sarfatti's priority on these matters!

16. **Peter Woit**
September 10, 2019

Jackiw Teitelboim,
Actually, Susskind and Sarfatti worked together back in the 1960s (or at least that's what Sarfatti claims…).

17. **Joseph Healy**
September 11, 2019

As an aside, J.W. Stout referred to another Goudsmit comment in his 1986 article: The Journal Of Chemical Physics: The First 50 Years
.
"Sam Goudsmit once said that he suspected that the ratio of readers to authors of Physical Review articles was less than unity, since it was evident that some authors had not read their articles before submitting them. "
.
https://www.annualreviews.org/doi/pdf/10.1146/annurev.pc.37.100186.000245

18. **Low Math, Meekly Interacting**
September 11, 2019

I should have reviewed your review of Kaiser's book before posting, or it would have been obvious.

I wish to be clear I do not intend to besmirch the whole of long-haired, dope-smoking freakdom for the more offbeat behavior of a few notable individuals, however brilliant.

It's not hard, however, to imagine the Fysicists and their fellow travelers might have been sufficiently irritating to their squarer elders to trigger a response, however far-sighted some of the pesky deviants proved to be.

19. **former mathematician**
September 11, 2019

In summer of 1969, I moved into an apartment in Berkeley, where John Clauser and his wife Marilee had the informal role as superintendent. He spoke of his hopes for the experiment he was setting up. He expected the opposite result to the one obtained. When he was done, he thought, quantum mechanics would be relegated to a branch of mathematics.

20. **Peter Woit**
September 11, 2019

LMMI,

No criticism of anyone for dope-smoking or having long hair was implied. Those who still have all their hair are encouraged to enjoy it.

All,
See an update to the posting for more about Sarfatti. I now think the conjecture that it was Sarfatti's papers (and other similar ones) is the most likely explanation for the Goudsmit editorial, not an attempt by him to suppress study of the interpretation of quantum mechanics.

21. **Peter Woit**
September 13, 2019

See the updates to this posting for more historical evidence, which I think shows that Goudsmit's editorial was not particularly aimed at papers on interpretational issues in QM, but just concerned a long-standing problem of how to deal with low-quality "not even wrong" theory papers making claims about fundamental physics.

22. **Blake Stacey**
September 13, 2019

In the first decade of PRL's existence, Goudsmit declared a subject *non grata* at least three times. In 1959, it was masers; in 1960, it was the Mössbauer effect; and in 1965, it was "internal symmetries":

> Hereafter, papers in this area will be considered only if they propose a genuinely new concept which makes possible the correlation of previously unrelated data.

23. **Arnold Neumaier**
September 16, 2019

Potentially interesting in this context:
https://journals.aps.org/prx/pdf/10.1103/PhysRevX.4.040002
[Note: PRX = Physical Review X]

"when we are asked the question, "Does research on foundations of quantum mechanics fit into PRX's scope?" our answer must be a clear "yes." However, that is far from saying that any intellectually sound paper from that topical area merits a spot in PRX. Indeed, the task of judging whether an individual paper on foundations of quantum mechanics is suitable for PRX is an editorially challenging one, especially given the often unavoidable presence of philosophy in our efforts to understand, interpret, and apply quantum mechanics."

24. **Peter Woit**
September 16, 2019

Arnold Neumaier,
Thanks! That policy description also includes something similar to the 1973 editorial:

"In the end, we came away with the following guideline: PRX will publish papers on foundations of quantum mechanics proposing highly original, scientifically sound approaches or models that already, or can potentially, suggest new experiments, and/or lead to new numerical tools for solving quantum-mechanical problems, or even significant new predictions."

Again, the evidence is that Physical Review hasn't ever had a policy banning a topic, be it gravity or quantum foundations, but has just struggled with how to deal with low-quality papers in these areas.

25. **GoletaBeach**
September 16, 2019

From deep in the bowls of my memory comments from Eyvind Wichmann about Bell percolated up, from the 1970's, and at Berkeley.

Wichmann made the point that the Bell correlations between the two sides of an EPR pair were simply the consequence of angular momentum conservation.

Experimental work to test Bell correlations… well… do you really think there is a violation of rotational invariance? The EPR state, Wichmann emphasized, was the degree of freedom that the initial system could couple to. Now maybe Stuart Freedman was in the room. Maybe Stuart kinda stopped pursuing Bell tests.

26. **Mark Hillery**
September 20, 2019

Somewhat off topic but related to the last comment. Note that the Bell test experiment by Friedman and Clauser was published in PRL in 1972, Stuart J. Freidman and John F. Clauser, "Experimental Test of Local Hidden-Variable Theories," Physical Review Letters 28 #4, (3 April 1972), 938-941.

From the AIP interview with John Clauser:

Clauser:

Townes was just the opposite. Without Townes, I could never have done that experiment. Townes was the guy who actually twisted Commins' arm to put Stu Freedman on the experiment and to steer Atomic Beam Group funds into doing the experiment. And if I had not convinced Townes early on [it never would have happened]— In fact, the first thing I did when I arrived at Berkeley was that I gave a seminar describing this to Townes' group, and Gene Commins was there, as he had done this previous experiment with Carl Kocher. And at the end of this, Townes kind of puts his arm around Gene Commins and says, "Well, what do you think of this, Gene? It looks like a very interesting experiment to me." So if Townes puts his arm around your shoulder and says, "Looks like a very interesting experiment," I mean, Commins thought it was a total crock. But when Townes says he thinks it's interesting, it's interesting. It becomes interesting at that point.

Bromberg:

It's surprising to me because Cummins' experiment was precisely on the Einstein-Podolsky-Rosen paradox.

Clauser:

They did not understand Bell's Theorem. Nor did they understand just the significance of what it all meant at that point, absolutely not. But it was Eyvind Wichmann, a theoretician at Berkeley, a very bright guy, I think, who actually suggested that they do the experiment. He's a theoretician, a very bright guy. While I was at Berkeley, I actually audited a course he was giving. He apparently suggested to Commins that they can actually do this, and apparently they originally planned on doing that as a lecture demonstration. Originally it was built on this rolling table with wheels on it so they could roll it into a lecture. Kocher was going to do this kind of as just a little project, and polish it off, and then get on with a real thesis project. Well, it was much more difficult than they originally anticipated, and it became his thesis project. No, you could not have done it as a lecture demonstration. It took weeks of counting to get any decent statistics.

An Apology

September 15, 2019
Categories: Quantum Mechanics

I'm afraid I made a serious mistake in this previous posting discussing Sean Carroll's new book. Since the book was relatively reasonable, while the jacket and promotional material that came with it were nonsense, I assumed that Carroll was just being ill-served by his publisher. It's now clear I was very wrong. He's on a book tour, and the nonsense is exactly what he is putting front and center as a revelation to the public about how to understand quantum mechanics. For a couple examples, here's what was on the PBS News Hour

> The "many worlds" theory in quantum mechanics suggests that with every decision you make, a new universe springs into existence containing what amounts to a new version of you. Bestselling author and theoretical physicist Sean Carroll discusses the concept and his new book, "Something Deeply Hidden," with NewsHour Weekend's Tom Casciato.

and here's something from his talk down the street from me.

Using your public platform to tell people that the way to understand quantum mechanics is that the world splits depending on what you decide to do is simply What the Bleep? level stupidity. Those in the physics and science communication communities who care about the public understanding of quantum mechanics should think hard about what they can do to deal with this situation. They may however come to the same conclusion I've just reached: best to ignore him, which I'll try to do from now on.

No Comments

Chasing Einstein

September 16, 2019
Categories: Film Reviews

Last night I went to a showing of Chasing Einstein, a new documentary about the search for dark matter. It's quite well done, and if you're near New York, Berkeley or LA, you might want to take the opportunity to go see it in a theater.

The film starts out with a segment on LIGO, talking to Barry Barish and Rainer Weiss. Later on there are scenes from their Nobel celebration ceremony at Caltech and the award ceremony in Stockholm. There are no claims made that LIGO's results are related to dark matter. Rather, this material functions as a counterpoint to the dark matter material, contrasting a great success story to the rather frustrating lack of success that physicists have had with dark matter.

Attention then turns to Elena Aprile and the Xenon1T experiment. Aprile is in the physics department at Columbia, and attended the screening I was at. I think she's the great heroine of this film, although a bit of a tragic one. She and her collaborators have done a fantastic job of getting a series of highly sensitive detectors to work. If a WIMP particle responsible for dark matter had existed in the region advertised by many theories, they would have found it and followed the LIGO people to Stockholm. Instead they put a strong limit on the possible properties of such a conjectured particle. The film includes a heart-breaking scene when they unblind their data, quickly realizing that their years of effort haven't been rewarded with the discovery that they had been hoping for. Aprile has a realistic take on the prospects for future experiments of this kind: they can be make somewhat more sensitive, but it's hard to be optimistic that the remaining accessible parameter space contains a new particle.

Attention then turns to Erik Verlinde and his "Emergent Gravity" explanation for the dark matter phenomenon. I've never found the motivation for this compelling, so haven't followed his work carefully. For someone who has, see Sabine Hossenfelder's blog where she has written on the topic quite a few times (and has her own version of a model here). Grad student Margot Brouwer worked on this attempt to experimentally test Verlinde's ideas, and she is also featured in the film. My understanding is that the positive results her group found are matched by other more negative results, see here.

Tech entrepreneur Cree Edwards appears at various points in the film, and I'm guessing that he's the one who brought together the physicists and filmmakers to make the film (and probably financed it). He has an amateur's interest in fundamental physics, and his questioning of the physicists reminds one of how people's fascination with the subject is often deeply connected to their desire to make sense of the world, hoping to find explanations of the great puzzles of human existence. I fear he's not likely to find much of what he's looking for in physics, but glad to see that his questioning led to an excellent film.

Finally, the film contains scenes of observing a solar eclipse, an added attraction.

Comments

1. **A_saint**
   September 16, 2019

   I was hoping by reading this post I'd know how you thought about the film. This reminds me a bit about Dirac style of lecturing 😛 … Would you recommend others to watch it too?

2. **Peter Woit**
   September 16, 2019

   A_saint,
   I enjoyed the film, and I hope that came out in the posting, but I'm always leery of telling others that they will too, so should definitely see a certain film. Tastes differ, and what I find charming others might find annoying. So, I try to give enough information about what's in the film to let you make up your own mind.

   In any case, right now the film has only a very limited theatrical release, so relatively few people are going to have it available nearby.

3. **More Anonymous**
   September 16, 2019

   1.Just re-read the whole post and found one sentence that does describe your views: "… but glad to see that his questioning led to an excellent film."

   2.Also about " So, I try to give enough information about what's in the film to let you make up your own mind." Is there anything in the movie you have left out from the plot? Maybe add a spoiler in the title (if not)?

   3. "… "Emergent Gravity" explanation for the dark matter phenomenon. I've never found the motivation for this compelling … " I'm also curious what your view on the dark matter phenomenon is? Where should physicists be placing their bets?

   4. Also chrome is showing me a red sign with "Not secure" in the URL place (for this site). And if I click on the learn more:
   https://support.google.com/chrome/answer/95617?visit_id=637042412030042076-2172091136&p=ui_security_indicator&rd=1
   Whats this about?

4. **A_saint/More Anonymous**
   September 16, 2019

   About 4.

   That was quick!
   I just saw it go from:
   Not secure to Info to Secure

   Did you do anything on your end? I swear I'm not lying (and wasn't when I made that comment) … Interestingly it didn't remember my details despite the "
   Save my name, email, and website in this browser for the next time I comment." in the comment box.

   Also just noticed the username change in the 2nd post to More Anonymous must have accidentally done something .

5. **Peter Woit**
   September 16, 2019

A_saint/More Anonymous,
Yes, I did just fix the https warning, it was doing that because of an http rather than https in one of the buttons on the right hand side. The change in the site may have temporarily disrupted the "save your name" feature.

There's not really a plot to "spoil" for this film. The one moment of drama is the Xenon1T unblinding, but unless you're unaware that that experiment hasn't found dark matter, the outcome isn't a surprise.

I don't want to get into a general discussion about dark matter, it's a huge, endlessly debated topic, and unfortunately there hasn't been anything really new to say about it for quite a while (and I certainly have no new idea about it). My generic question about "emergent gravity" claims like those of Verlinde is "emergent from what?" Without a non-trivial answer to that question (and I don't see that Verlinde has one), it seems unlikely you are going to get anything interesting. Not mentioned in the film is the all too possible frustrating scenario that dark matter is a new particle, but one that has no interactions other than gravitational, so can effectively never be directly seen in a feasible experiment. If I had to place a bet, it might be there.

6. **GoletaBeach**
September 16, 2019

Perhaps the movie presents Elena Aprile in a tragic light, but I'm not aware of anyone in the actual dark matter field who shares that view. Everyone knows when they start a search program that the most probable outcome (by a lot!) is no discovery.

There is a very weird and contrafactual attitude out there… typified by a Malcolm Browne article in 1993 that ran under the headline "315 Physicists Report Failure In Search for Supersymmetry".

There was no failure. They (as LHC now, and Xenon-1T too) completely succeeded in doing exactly what they said they were going to do… achieve sensitivity to new physics within a certain parameter space. It is science… "Eliminate all other factors, and the one which remains must be the truth." Just as Michelson and Morley did. It is a recurring cynical joke that theorists on modern review panels would have defunded Michelson and Morley.

Everyone in the dark matter field knows that there are at least 8 orders of magnitude more to go before SUSY explanations of dark matter are close to inconsistent with SUSY. Anyone who says otherwise is confused about what some call the "Canadian Border Effect". Canadian population clusters around the southern border, it is the most desirable real estate. US population is sparse right there… for Americans, the northern border is not desirable.

A la mode SUSY dark matter theories always cluster just below the current experimental sensitivity for the same reason. For dark matter and for the LHC. Thinking "a la mode" is the only reason to build experiments is immature, although theorists want to be able to say "I predicted it!", so they find models just over the limit. Only other theorists take that seriously.

Aprile got involved in liquid xenon detector technology when it was mainly going to be used for x-ray astronomy at Columbia. She wasn't initially using the dual phase technique, invented in Russia. She had the insight to see and push for implementation of that technique with her liquid xenon ability to search for dark matter. She is not the only one, actually… Dolgoshien's old group in Russia and groups in UK simultaneously began the push. Aprile's group did get the first really good results out.

The technique has achieved 4-5 orders of magnitude in new sensitivity… imagine finding a technique that raised beam energy in proton-proton collisions by 4 to 5 orders of magnitude. And the liquid xenon can go a lot further; many groups are planning to continue… progress will be slowed (but not stopped entirely) when neutrino interactions start to show up.

The tragic element really is: both this blog (and Sabine Hossenfelder) portray vast improvements in experimental sensitivity without a transformational discovery as "failures". It is a lack of understanding as to how real experimental work and discovery works… the top quark didn't appear for 15-20 years post prediction, and its method of discovery was pretty much unheralded at its inception, for example. Real experimentation and experimental discovery entails quite a bit of persistence, and skepticism of all theorists.

Ruling things out has a huge value… Luis Alvarez answered this question in the 1960's when the muon intensity experiment in the pyramid of Khafre found "no large unknown chambers exist"… the technique did map the big chamber where they set up the experiment. He was asked about his "failure"… he pointed out that the experiment was a complete success, and would prevent explorers and drillers from mounting expeditions to look for new chambers… of no small value. And now, the technique has been refined and found a new chamber in the pyramid of Khufu.

These portrayals of "tragedy" and "failure" mildly annoy many experimentalists… and further convince them that theorists and mathematicians don't deeply comprehend what empirical science is. Sitting on a review panel with folks who don't get it can be quite painful… another topic Luis Alvarez used to write about. It may be that forums like this blog (and Hossenfelder) are suppressing discovery.

A portion of what Hossenfelder lobbies for is experimental testing of alternate DM theories. That is good…

7. **Peter Woit**
September 16, 2019

Goleta Beach,
I'm afraid the posting does not make my point of view clear, but I'm not sure why. To quote myself above:
"I think she's the great hero of this film, although a bit of a tragic one. She and her collaborators have done a fantastic job of getting a series of highly sensitive detectors to work. If a WIMP particle responsible for dark matter had existed in the region advertised by many theories, they would have found it and followed the LIGO people to Stockholm. Instead they put a strong limit on the possible properties of such a conjectured particle."

The reference to "tragic" is to the portrayal in the film, where she is interviewed and gives a nuanced explanation of what it is like to work on an experiment like this.

I agree completely that the achievement of huge increases in sensitivity and investigation of large new areas of parameter space by Xenon1T and others is a huge experimental success (while still being, undeniably, a failure to find an hypothesized new particle state). Arguably, this sort of achievement, not just discoveries, should be rewarded by the Nobel.

Similarly, the LHC results ruling out large new regions where there could have been new particles is a huge experimental success, while at the same time being a failure to find SUSY states. When I refer to such a failure, what I'm referring to is a theoretical prediction being successfully disconfirmed by experiment, and the failure is on the theorist's side, the success on the side of the experimentalists.

On the topic of where to expect SUSY states, my opinion has always been "nowhere", since such states don't explain anything, just make the SM more complicated. The regions heavily advertised for such states pre-LHC have now been ruled out, puncturing I hope the hype behind the whole subject. This doesn't rule them out in other regions, and if experimentalists can investigate some of those regions they should, but I think the argument for doing this increasingly is because something unexpected might turn up, not because unconvincing models will get ruled out.

8. **GoletaBeach**

September 17, 2019

Thank you Peter. The 3 families (e,mu,tau) and/or (u/d,c/s,t/b) also just make the SM more complicated too… who ordered that? Actually, "Who Ordered That?" might be the complementary experimental blog (if it existed) to "Not Even Wrong".

That the relative – sign between loops of symmetrical fermions & bosons in SUSY isn't the worst motivation I've ever heard of.

The WIMP motivation always has been: we doubt the dark matter has explicit strong and electromagnetic interactions. We believe the dark matter (DM) has gravitational interactions. That leaves one interaction to probe… the weak interaction. Slight bias if the DM has a weak interaction then it should have rest mass in the W/Z/Higgs ballpark.

That idea also helps in one scenario of the early universe, if the DM was in thermal equilibrium with us.

Experimentally that defines a program. The first "natural" experimental sensitivity benchmark was passed in the 1980's… if the (massive) DM had the couplings of a Dirac neutrino, it would have been discovered then. Experiments stopped for a while.

Gradually the idea that maybe the DM was not Dirac but Majorana seeped in, and the "natural" sensitivity plummeted to well below what we have lately achieved, because one needs to mediate through Higgs to top pairs to gluons, or through the Z0, and inevitably through interferences. Still no SUSY is needed. If anything SUSY makes *even tinier* cross sections *more* likely… also makes large cross sections more likely… just broadens everything.

Somehow Sabine Hossenfelder and Stacy McGaugh still write in the August 2018 Scientific American…
"But after three decades of failed attempts to detect any of these particles, ignoring alternative hypotheses is no longer reasonable."

First, there were no "failed attempts". Note they do not say "failed theories". "Failed attempts" implies the experiments failed. Most of the experiments succeeded admirably.
(I do know that you, Peter, are not equal to Hossenfelder and McGaugh, and I appreciate that).

The second clause of their sentence is always reasonable. Keep thinking about everything. Keep suggesting experiments to test the ideas.

A bunch of earthbound experimental effort actually did go into testing MOND… but turns out the loophole is… MOND depends on *absolute* acceleration. Relative acceleration is empirically proven to show no MOND effect.

So although lab experiments possess a sensitivity level sufficient to test MOND, we can't get them into the right reference frame without a rocket that gets out of the solar environment. To do that you are talking a billion $ at least. But just writing this has made me wonder if far out in the Kuiper belt or beyond their might be a system of binary snowballs that Hubble could study to test MOND.

A bit puzzling that Hossenfelder and McGaugh *ever* felt non-WIMP ideas were *ever* ignored.

Nobody felt strongly the collapsing binary signal would be large enough for this generation of LIGO to see the chirp. Great that unexpectedly massive black holes turned out to exist, but "Who Ordered That?" What is the formation mechanism? Post LIGO, no lack of theories, will be the same if WIMPs are seen at a cross section of 10-52 cm^2.

As for Nobels… only time will tell. Charpak's chambers did not work at all at first. It took non-Charpak adopters to correct their deficiencies.. but Charpak got the Nobel Prize. Yes, Nobels get awarded in particle physics for innovation without direct responsibility for a new particle or force.

The vast sensitivity improvement pioneered by Elena Aprile (and others) might yet get awarded a Nobel somehow.

9. **shantanu**
September 18, 2019

GoletaBeach: Some comments on your post.
we don't have even a single shred of evidence from astrophysical observational that dark matter is a WIMP or has weak scale couplings. (The only argument is WIMP miracle, which is at best a numerical coincidence)
Also the goal posts for WIMP direct dark matter detection have changed in the last decade. This is best exemplified in Figure 1 of https://arxiv.org/pdf/1703.02389.pdf

10. **Ron Avitzur**
September 18, 2019

I saw the ads for the film and was going to skip it, then saw your review and walked downtown to see it. Gorgeous film, loved the scenery, and awesome giving the physicists so much screen time. So thanks for changing my mind!

11. **GoletaBeach**
September 18, 2019

shantanu…. "we don't have even a single shred of evidence from astrophysical observational that dark matter is a WIMP or has weak scale couplings. "

You are right… we don't even have evidence that dark matter is a particle, or obeys our quantum mechanics or relativistic quantum field theory, or obeys any of our conservation laws.

But we assume some of that to test hypotheses. Gotta start somewhere.

That dark matter might have weak scale couplings is a conjecture, but has at least one very simple motivation you overlook: electromagnetic and strong couplings are very unlikely, gravitational couplings are likely (although could just be MOND or something like it), leaving the weak interaction as "the remaining one to check".

Sherlock Holmes logic: test the untested hypothesis.

The WIMP miracle *may* be a coincidence, or not. You may feel that it is, but your feelings are not scientific evidence.

The paper you refer to is nice but culls out the lower limits for cross sections, which for unconstrained SUSY models go down far below the bottom of those plots, down to at least 10-56 cm^2 or so. Not sure why the author forgot to mention that.

That (eventually) popular models tend to lurk just under the current experimental sensitivity has been true ever since models and sensitivity got on to a plot.

The popular models of any particular moment do not even close to comprising a "complete set" of pertinent models. For WIMPs there are always many models just below whatever the experimental sensitivity happens to be, and those models get popular when experiment gets there. But the knowledgeable know that those models are still lurking there right now, where experiment will be in 5 or 10 or 20 years.

The WIMP situation may not fit in a simplistic Kuhn or Popper-esque notion of science. They didn't describe real scientific discovery so that is no problem.

In real scientific discovery, the majority of new particles… the electron, the proton, the alpha-particle, the neutron, the electron neutrino, the muon, the muon neutrino, the tau, the strange quark, and the bottom quark were unheralded by theorists and dominantly discovered by experimentalists.

The photon, the gluon, the tau neutrino, the Z0 and W, the top quark, and the Higgs were predicted by theorists.

The charm quark is about 50-50.

So 10.5 particles for aphilosophical experimental exploration, 7.5 particles for textbook scientific philosophy.

For fundamental symmetries: C is definitely a theorists prediction, while P violation is about 50-50 (observed early by Cox and the tau-theta puzzle, then fleshed out by Lee and Yang), CP violation pure experiment, CPT on the theory side. So for these kind of symmeteries… 2.5 theory/philosophy, 1.5 experiment/aphilosophy.

Other symmetries… baryon number, lepton number… harder to say. Deep interplay throughout it all.

12. **shantanu**
September 19, 2019

Goleta Beach The dark matter could be a non-thermal relic. It could be a massive graviton. It could be even be a SM particle (see the collection of papers on Macro dark matter). There is no evidence from any astrophysical observational to distinguish between these (and other such possibilities) and a WIMP (for which so much of investment has taken place)

13. **GoletaBeach**
September 19, 2019

shantanu… you are right, the dark matter could be a non-thermal relic, which some WIMPs can be too, and there have long been experiments (ADMX, for example) among many others who have looked for non-thermal relics.

WIMP hunts, if you fairly account for all the dark matter effort going on at accelerators, with QIS sensors, with AMS and Fermi in space, with other indirect techniques… constitute a small fraction of all dark matter search funds. MOND got innovative attention in the 2000's but to really test it is in the billion-$ class now.

Whether the relative abundance of dark matter to our matter is astrophysical evidence that supports (to a certain extent) a weak interaction for dark matter is something you feel is a coincidence, but others feel is a bonafide observation.

Investment in experimental endeavor takes one input from perceived discovery likelihood, but there is another input which is innovation… innovation isn't perfectly correlated with $.

Experimentalists push to innovate all the time, and deep insights by folks like Aprile have led to huge advances in WIMP sensitivity per dollar. Other WIMP techniques have hit barriers and don't advance much even with the substantial $ that have been granted to them.

14. **Dan**
September 23, 2019

I saw Chasing Einstein, mixed feeling about this movie, mostly of the motives of the gathering these physicist in Montana and Carmel by Cree. Didn't get anything out of the Couch interview, and Carmel interview was uncomfortable to watch (poor Verlinde, just couldn't say what he really was thinking it seemed). Wish they would have gone more into the theory that Verlinde was putting forth. Aprile I feel was edited in the wrong light.

To A_saint, do not see this film, it was lackluster, and to be honest, I got more out of reading comments from GoletaBeach, shantanu and Peter Woit.

15. **Bernhard**
September 24, 2019

Thanks for pointing to Sabine's model, I didn't know it. I spent some time reading it – what a really cool article. Beyond me how someone who writes the number of high quality (many single authored by her!) articles she did not get tenure yet. In any case, I think "yet" is the word here.

Some background on “high energy physics”

September 16, 2019
Categories: Uncategorized

There was a workshop last week at the Harvard CMSA, focusing on new ideas about physics rooted in topology. Talks are available on the workshop webpage, and those interested in high energy physics might be most interested in the ones from the first session. There was an interesting introductory talk by Dan Harlow, in which he lays out his view (which I think is a very mainstream one) of the current situation of HEP theory.

He begins by noting the problem of building higher energy accelerators (claiming that the problem is that technological limits make the maximum energy of collisions go as the square root of the radius of the machine, but I think really for proton-proton machines it is linear in the radius, for electron-positron machines the fourth root of the radius). Given the lack of new data, he describes one tactic for theorists as to change fields, e.g. to machine learning or biophysics.

If one does want to persist, he argues there still is a list of things incompatible with the Standard Model (gravity, dark matter, neutrino masses, baryogenesis, inflation) and these are not just “aesthetic” problems (here he refers to misunderstandings in the “popular media”, a clear reference to Sabine Hossenfelder and her book). From there he focuses on quantum gravity, essentially arguing that the other problems can be addressed by BSM models, but none of these seem particularly nice, so without new data progress is unlikely.

He describes quantum gravity as the ideal situation for theorists, since according to him there’s no self-consistent theory that fits the data we already have (I guess he’s saying string theory models are inconsistent…). He describes current work on this as based on two main strategies, with AdS/CFT providing a link between them:

- “Study the non-realistic corners of string theory where mathematical control is possible”, i.e. pick some non-physical string theory background (e.g. AdS/CFT) where you think you can do self-consistent calculations and do those, hoping to get some more general insight.
- “Set aside gravity for the moment, and focus on understanding the mathematical properties of QFT.” He gives a few examples of general questions being studied (which unfortunately have no obvious relevance to addressing the problem of quantum gravity, or basic problems like that of non-perturbative QCD.)

In the question section, there was an exchange between Harlow and Seiberg, based on Harlow’s reference to changing fields because of no data and to something he said during the talk (at 2:06):

> **Harlow**: So then, what are we supposed to do in the meantime, right? You know we need to keep writing papers and posting them to hep-th and so on, so what do we do?

I suspect that for some context to the following exchange, you should also look at the video of the panel discussion earlier this year at Strings 2019, where Harlow, sitting next to Seiberg says (at 6:45) “We’re having fun, isn’t that the important thing?”.

> **Seiberg**: I’d like to make one comment.
>
> This was a beautiful summary, spectacular, except that one thing was fundamentally wrong and certainly should not be said. It’s not that we’re doing what we’re doing because we have to fill the time (audience laughter). We’re doing what we’re doing because it’s very important (audience laughter). I don’t think about “maybe we should write some books and this and that, until we have more information” I think this is wrong and this should not be
>
> **Harlow**: I’m doing it, right, I don’t like wasting my time, so, I think it’s worth my time. I do think it’s important. We have this list of phenomena that we can see and can’t explain.
>
> **Seiberg**: Comments like these have been used against us (audience laughter), in addition to the fact that they are wrong.
>
> **Harlow**: OK, yeah, yeah, I’m not talking to the New York Times, right. (audience laughter).
>
> **Dam Son?**: Is it recorded?
>
> **Harlow**: I don’t know actually (audience laughter), I’ve said much worse things that were recorded, so.

HEP theory is at a very difficult point in its history, and it seems that the older generation struggling with this is not particularly amused to hear what sounds like flippant takes on the problem from the younger generation.

Update: I finally got around listening to the Susskind interview mentioned in this comment. Susskind also has given up on particle physics:

> I originally was officially an elementary particle physicist. Elementary particles is not going so well, there’s no new experimental input and nobody knows what to do. It’s sort of reaching a point of, should I call it diminishing returns? It could change, it could easily change. I don’t think it’s doing very well. It’s not the fault of the physicists, it’s just the fact that they’ve reached a barrier, with no possible access experimentally to things that we’re not doing very well figuring out theoretically. So that’s not doing exceptionally well. My guess is the same thing may happen to cosmology. That they will eventually run, and they’re very close to it now, running out of new data, so there may be a barrier there.

Update: This week in Chicago there’s a workshop on the CEPC (proposed large new electron-positron collider in China). The first talk Monday was from Nima Arkani-Hamed. At the end of it, the question period started, with an exchange that resonates with the Harlow-Seiberg one:

> **Mike Peskin**: So, let me make a quick summary of this talk: “my prediction is that when we go to high precision with the Higgs we will see no deviation from the Standard Model, but that will be a good thing because theorists will be inspired to think about these fundamental questions.”
>
> **Nima Arkani-Hamed**: Absolutely. I’ve said it many times. Many people don’t believe me, but I believe it 100 percent. If we see some deviation, fantastic, great, people will have a lot of fun figuring it out, if we don’t see a deviation that’s a much, much bigger gauntlet thrown down at the feet of theorists to try to figure out what is happening.
>
> **Mike Peskin**: But on the other hand you’re not promising any concrete discovery, just we reconfirm the Standard Model at a much higher level of energy.
>
> **Nima Arkani-Hamed**: Reconfirming the Standard Model would just crank up the screws that are put on our theoretical imaginations even more.
>
> **Mike Peskin**: How many billions of dollars do you expect people to spend to reach this conclusion?
>
> **Nima Arkani-Hamed**: … However many billions it takes.

At the same conference, today Matthew Reece gave a talk on The Hierarchy Problem and the Motivation for Future Colliders. He starts out with:

> I'll review some arguments that may be well-known to many of us—but which I find are not necessarily well-known to students, some of whom are being taught that there is no motivation to search for BSM physics.

and gives this I think accurate characterization of the problem:

> The better way to frame the problem, and the role of fine-tuning, is that we are seeking a theory that explains the origin of the EW scale.
>
> If, within that theory, the EW scale is extremely sensitive to input parameters, it's not a very good explanation. The theory does not generically describe a universe like the one we live in.
>
> If moving around in parameter space just produces modest changes in the low-energy physics, that's a compelling theory that predicts a world like ours.

This characterization makes clear what the correct interpretation of the null LHC results should be: they provide significant evidence that the picture of a very high energy scale GUT/string theory with lots of parameters, generically producing the weak-scale physics that we see, is just wrong. There never has been any evidence for this anyway, so the failure of the hierarchy argument was to be expected. To the extent that you believe the hierarchy problem is the motivation for BSM physics, students who are being taught to give up on BSM physics by Harlow and others are not really being misled.

My own take on all of this: what Harlow and Arkani-Hamed get wrong is their claim that thinking about fundamental issues of quantum gravity is some new, exciting question that has just come up post-LHC null results. These issues have been there for decades; they were obvious at the time I was a grad student in the early eighties. The problem is what to do facing several decades of failure by theorists, and I don't think the answer is to make outrageous claims about how wonderful the current situation is. The motivation for a new collider is the one Reece points to, ignoring the business about the hierarchy problem: we don't understand at all the origin of the EW scale. This is the best argument for studying the scales just above it that the LHC has started to enter. If we can get some new insight into the EW scale from a detailed study of the scales just above it, that will revolutionize physics (not just be "a lot of fun"). If we can't, we're facing a very, very tough time, especially if we insist on pursuing fundamental theory the way it has been pursued in the past.

Comments

1. **DB**
   September 16, 2019

   Peter,
   re what Harlow says about quantum gravity being the ideal situation for theorists, here are Susskind's latest thoughts on some similar issues, talking about the "barrier" reached in both particle physics and cosmology, and why quantum gravity is probably the stuff to focus on right now.
   He mentions Maldacena's latest ideas as being the new biggest excitement, though I have no clue what they might be.
   Aforementioned info is between 15:50 and 19:20.

   https://podcasts.apple.com/gb/podcast/universe-speaks-in-numbers-lenny-susskind-interviewed/id1479389232?i=1000449685963

   PS: I also interpret that he says that string theory models are inconsistent… Hmmmmm, I thought he was a string theorist himself. Has he flipped?

2. **Peter Woit**
   September 16, 2019

   DB,
   What Harlow says I think is conventional wisdom in much of the "string theory" community (which these days mostly is not doing string theory anymore…), so it's not surprising you're hearing a variant of it from Susskind. The idea, as Harlow explains, is that the only fundamental problem left that isn't hopeless without new data is quantum gravity, and any approach to quantum gravity should start with holography and AdS/CFT.

   I don't want to start a discussion here of the latest things Susskind and others are doing along these lines. I'll just note that while Harlow makes it sound like this is new, this is pretty much the ideology the string theory community has been following for the past twenty years, and it seems to me that it has led them nowhere. The inspirational version you hear of this (it made an appearance in the "Chasing Einstein" film) is that the decks have been cleared and theorists are now confronting the most fundamental issues, space-time is doomed to be replaced by something new and exciting, inspired by AdS/CFT. The only problem is that this is not new, it's older than my students here at Columbia, and nothing promising has been found to replace the doomed space and time.

3. **Sabine Hossenfelder**
   September 17, 2019

   At least it looks like they they are trying to think about how to not waste further time. I guess that counts as progress. If all goes well, then in 20 years they will understand what I wrote in my book.

   For the record, I have a differentiated view on what problems are are actual problems in the foundation of physics and which are merely aesthetic misgivings. It's not like I am saying they are all just aesthetic. I have eg been very clear in pointing out that dark matter and quantum gravity are "good problems". I have a list here, in case you don't like to waste your time…

4. **Alessandro Strumia**
   September 17, 2019

   LHC has been mostly completed, its theoretical consequences have been mostly discussed, if nothing new happens the field will decline in the next few years. What next?
   Two long-term possibilities that seem not totally hopeless are:
   1) explore string vacua with supersymmetry broken at the string scale.
   2) make AI really I. Many deep results in physics have been achieved by a few smartest physicists, and there are reasons to think that A-IQ should be scalable.

5. **Sabine Hossenfelder**
   September 17, 2019

   Indeed, Alessandro's second point makes a lot of sense. There is almost certainly unexplored potential in the already existing literature where no one has made a connection that's hiding in plain sight. I recently wrote about an example from a different field. Readers of this blog might also enjoy looking at one of the early examples of this kind of analysis that happens to be the first string revolution.

6. **Maik**
   September 17, 2019

Am I missing something or is the claim that "we do not even have one self-consistent theory [of gravity] that fits the data we already have" really as outrageous as I perceive it to be?

7. **Shantanu**
September 17, 2019

So neutrino masses tell us there is physics beyond standard model. Why aren't more people working on connecting this to BSM physics theories at LHC?

8. **Peter Woit**
September 17, 2019

Maik,
He's referring to quantum gravity and the claim is not at all outrageous. As far as string theory goes, you don't have an understood, self-consistent version that gives four flat space-time dimensions and a small CC (the "landscape" vacua are controversial for good reason). For other approaches to quantum gravity (eg. LQG) I don't want to get into the often religious war of claims and counterclaims, but the argument that none of them have yet given a fully consistent model compatible with the real world is a defensible one.

Shantanu,

Plenty of people have been doing this, for decades. I think Harlow's claim (which I generally agree with) is just that all such BSM modeling has led to no compelling model, just a large collection of possible models, with no way of choosing between them unless we get some more relevant data. So, his argument is that best to stop working on this until and unless you get more data.

9. **adamt**
September 18, 2019

Seiberg says this is unhelpful to HEP, but also factually untrue: "Harlow: So then, what are we supposed to do in the meantime, right? You know we need to keep writing papers and posting them to hep-th and so on, so what do we do?"

And the exchanges illicit nervous laughter from the crowd.

The problem for HEP is that it is *not* untrue and that the public is getting wise and hence that laughter *should* be nervous. Even better if it were not laughter and the people in that room took the issue seriously rather than making comments about trying to hide the situation from The New York Times and worrying that the talk is recorded.

It is factually true that what the people in that room *do* is write papers. And that no one has been producing anything that really makes substantial progress on these problems for quite a long time. What *is* happening (and which is an unmitigated good thing) is that the problems themselves are starting to be classified and people are *starting* to think clearly about what problems are deserving of attention.

Of the two directions that Harlow mentioned I don't think a lot of progress can be seen. The public is getting wise to this. We're not all stupid.

10. **Jackiw Teitelboim**
September 18, 2019

Alessandro's 1th option (SUSY's broken at Plankian scales) would be the final give up in falsifiability, so not interesting at short/long-term. As the 2nd option (using AI), I bet you cannot fed an AI with the spectroscopic data accumulated in the XIX century and get "full" QM as an output (uncertainty relations, Dirac's transformation theory etc.).

11. **DB**
September 18, 2019

Peter,
thanks for the second update.

Nima Arkani-Hamed: "Reconfirming the Standard Model would just crank up the screws that are put on our theoretical imaginations even more".

Correct Nima… correct.
And it will probably put the final nail in the coffin to stuff like Susy.

Finally, no, we don't need to through out "as many billions as it takes" to reach those conclusions. That final comment is absolutely ridiculous, especially coming from someone who is a top theoretical physicist.

But I guess that the wheel has to keep on turning…

12. **Alessandro Strumia**
September 18, 2019

Jackiw, I assumed that nothing new gets discovered. If we already collected all particles accessible to realistic experiments, and we already have their low-energy theory, writing wrong models and falsifying them would be a politically-correct way of making no progress. The only hope would be matching what we know about the low energy theory (about 75 digits measured so far) with a high energy theory. Unnaturalness of the weak scale and of the vacuum energy points to a landscape of >10^150 possible vacua, so the situation might be hopeless. Maybe not. So far this failed, but attempts focused on N=1 SUSY at low energy.

13. **Amitabh Lath**
September 19, 2019

Most discussions about the LHC null results make it sound like experiments go out and look for new physics with no biases or input from the zeitgeist. This is not so, CMS and ATLAS were designed to look for new physics in signature spaces theorists were excited about. That's why they are breathtakingly good at missing momentum and electromagnetic energy resolution. Of course there is a tradeoff, they aren't as good at some others such as long lived particles or multiple low momenta particle signatures (compressed spectra). Yes, a large swath of BSM parameter space has been searched, but there are huge and important bits we are not sensitive to. Not yet.

14. **Alan**
September 21, 2019

Hello Peter,

I read your blog often. I have not commented before but I have a, possibly interesting, historical perspective on the topics in this post: quantum gravity, lack of experiments, and switching fields from physics to machine learning.

I did a PhD on quantum gravity in the late 1970's (supervised by Stephen Hawking) and started a postdoc in the US, but soon left physics for artificial intelligence (the term "machine learning" didn't exist at that time). I left physics because of several concerns. One was that quantizing gravity seemed very hard and all of the existing approaches had many problems. A second was the lack of experiments to guide and test theories (which was historically almost unprecedented). A third was that the research was becoming increasingly mathematical and I wondered whether it should be left to real mathematicians like Atiyah and Singer who had stronger mathematical tools. These concerns weren't unique to me, but other people didn't seem to find them so worrying. At that time, quantum gravity was fairly new (apart from a few pioneers like Bryce De Witt and John Wheeler) so most of my friends thought there could be breakthroughs soon.

But it seems that all these concerns remain valid almost forty years later. It is interesting that some leading physicists don't seem to realize how old they are. And that AI/ML is a very old escape route (though AI was done at only a few universities at the time).

I followed physics at a distance and was surprised at the growing amount of physics stories about conjectures (e.g., about extra space-time dimensions) which had no experimental support (but which non-physicists friends of mine believed because they'd read them in books or news articles) . I found your blog and Lee Smolin's book to be helpful reality checks and insightful about the sociology of physicists.

AI has been good to me (and I'm sure I was no loss to physics). AI also suffers from excessive hype these days, but the majority of AI researchers are fairly realistic about distinguishing between what AI can do now and what it has the potential of doing in the future (provided some very difficult problems are solved). Tough peer review, including the need to benchmark performance, keeps us fairly level headed. The hype is more from some start-up companies (financially motivated?), non-AI experts who have recently jumped on the AI bandwagon, and from reporters who realize that only the most exciting stories get on the front page.

Metaphorical Worlds Interpretation

September 17, 2019
Categories: Quantum Mechanics

Chad Orzel has a piece at Forbes which I like a lot, where he argues that the "Many Worlds" of the MWI interpretation should be taken metaphorically, and thus the MWI really should be the "Metaphorical Worlds Interpretation". I urge you to take a look (and argue about this with him, not me…).

Update: Natalie Wolchover suggests renaming MWI as the "Many Cakes Interpretation", since

> there's a lot of having of cakes and eating them as soon as things get awkward.

No Comments

The Number of the Heavens

September 18, 2019
Categories: Book Reviews, Multiverse Mania

Multiverse mania seems to have been dying down recently, with this only the third entry in that category here so far this year, after 10 in 2018, 13 in 2017, 10 in 2016, 17 in 2015, 18 in 2014, 12 in 2013, 9 in 2012, 15 in 2011. Bringing up the rear (hopefully…) is The Number of the Heavens, Tom Siegfried's new book out today from Harvard University Press.

Siegfried is about the worst of the many journalists covering fundamental physics that I've run into over the years (only real competition is K.C. Cole). For some of his efforts as a journalist over the years, see here, here, here, here, here, and here. It's not surprising that his multiverse book is an atrocious piece of propaganda.

It's basically a compendium of arguments for string theory and the multiverse, with a bit of extra history tacked on. You get to read long sections of all the usual pro-string landscape and multiverse arguments from the usual suspects: Carroll, Deutsch, Guth, Greene, Linde, Polchinski, Rees, Susskind, Tegmark, and Weinberg. There's the usual chapter on the MWI, ending with the acknowledgement that this has nothing at all to do with what the rest of the book is about. There's a chapter about the glories of supersymmetry, brane-world scenarios, nothing about negative results from the LHC.

The way Siegfried handles criticism of string theory, etc. is very simple: pretend it doesn't exist. As far as I can tell, there's nothing anywhere in the book that even acknowledges that there's another side to this story: for instance, no Baggott, Hossenfelder, Smolin, Penrose, or any reference to any book at all critical of string theory or multiverse hype. While there's zero criticism of string theory, there are, as far as I can tell, just two appearances of multiverse critics:

- On pages 223-8, remarks by Burt Richter at a panel discussion in 2006 get two paragraphs, followed by four pages of arguments from Linde, Susskind, Polchinski and Carroll explaining why he's wrong. The prominent multiverse critic David Gross makes a brief appearance in these pages, with no mention of the fact that he is a multiverse critic.
- Pages 262-9 are labeled a section on "Multiverse Deniers", but there's only one multiverse denialist quoted, George Ellis, with the only source given for his arguments this paper. In these pages short excerpts of his arguments are interleaved with long explanations from the author (as well as Weinberg, Wilczek, Carroll, Donoghue and Rees) about why Ellis is wrong.

Th one thing I can't figure out about this book is how it got to be published by a reputable university press. My understanding has always been that university presses have some commitment to ensuring scholarly excellence in what they publish, for instance by having a manuscript about a controversy reviewed by experts from both sides. That obviously can't have happened in this case, so I must be mistaken about how places like Harvard University Press now operate.

Comments

1. **tulpoeid**
   September 19, 2019

   "Multiverse deniers" must be the most disgusting term ever used seriously in a scientific discussion.

   Spelling out the obvious – but are they now pretending so blatantly that multiverse is the mainstream in the community as to imply that the rest are lunatics whom it's okay to ridicule in their face?

   I hope that the guy is just a special case that nobody takes seriously but, yes, if it is so then HUP is certainly helping change that.

2. **Peter Woit**
   September 19, 2019

   tulpoeid,
   From everything I've seen, many influential string theorists have realized that the string landscape multiverse is a big problem for their subject, convincing many people string theory is a failure. Vafa is quite explicit about how trying to avoid this is the motivation for his swampland program. See for instance here
   https://www.math.columbia.edu/~woit/wordpress/?p=10486

   I suspect very few people take Siegfried seriously, and the fact that he's publishing with HUP is probably due to not being able to sell this to a commercial publisher. I see that the book is currently #182,033 on Amazon, which means that just after release this is selling less than one book/day. The few copies in existence are probably ones like the one I found in the stacks of the Columbia library, where they likely buy one copy of everything HUP publishes. Once I return it, presumably it will sit there undisturbed for the next few decades.

   It probably would have been best to just ignore the thing, but I was outraged that my alma mater's university press would publish something like this (and I'm still curious how that could have happened).

3. **Miguel**
   September 21, 2019

   Given your own stated experience with university presses's handling of string theory projects, I'm not entirely surprised!

4. **Peter Woit**
   September 21, 2019

   Miguel,
   My experience with Cambridge University Press was I suspect different: they had the manuscript reviewed by people with a range of opinions. The non-string theorists were very positive, the string theorists very negative. The negative opinions created a problem for them with publishing the book.

   On the other hand, I did see that university presses do some weird things: Princeton University Press hired Lubos Motl to review the book for them…

5. **John**
   September 23, 2019

   Glad you didn't actually buy the book. Buying the book only helps to continue the hype and puts money in their pockets.

6. **Peter Woit**

September 23, 2019

John,
I'm glad to not have faced the moral issue of whether to spend money on a book like this in order to write about. In this case, my university did, and that's a more interesting issue. I'm assuming they likely buy everything HUP publishes, and perhaps HUP is kept afloat not by selling books to the public, but by this effective subsidy, due to its reputation.

I don't have a problem with this in prinicple, nothing wrong with subsidizing in this or other ways the publication of high-quality books. But if you are going to have such a system, something has gone wrong if you use it to subsidize the publication of things of this low quality.

7. **Narad**
September 23, 2019

> perhaps HUP is kept afloat not by selling books to the public, but by this effective subsidy, due to its reputation

They're endowed.

> It probably would have been best to just ignore the thing, but I was outraged that my alma mater's university press would publish something like this (and I'm still curious how that could have happened).

I recommend looking at the volume of titles turned out weekly. The CFO of the university press I toiled for, who bore a certain temperamental resemblance to Yosemite Sam, once vented to me (we both smoked cigarettes then) about his inability to get Books to understand that a parade of gender studies titles wasn't going to keep the lights on.

I have no idea whether HUP has a top-down quota, but this strikes me as business as usual.

8. **Jim Baggott**
September 24, 2019

Not wishing to sound disrespectful of HUP, but my experience with an academic publisher confronted with a profoundly anti-string theory/multiverse book proposal was rather different. Oxford University Press has published most of my popular science output, with two new books on quantum mechanics due next year. Yet after receiving reviews on my proposal for 'Farewell to Reality' from a couple of academic physicists, one strongly positive but another worried about what kind of message such a book would send, OUP declined. 'Farewell to Reality' was published by a commercial publisher in 2013. (And the reviewer who welcomed the proposal later agreed to comment on the manuscript.)

I fear that the real truth is that even academic publishers with a trade books division are – like all publishers right now – feeling the pinch of declining sales and disruption from alternative online sources. And although books about string theory and the multiverse are less prevalent these days, as Sean Carroll has demonstrated, books on controversial scientific subjects can still sell well.

9. **Peter Woit**
September 24, 2019

Jim Baggott,
That's interesting, very similar to my experience with CUP. Basically if something is actually controversial, in the sense that there are prominent academics on both sides, they are very leery of publishing. I was explicitly told by one editor of a university press (Princeton?) that Not Even Wrong was too controversial to be published by a university press. So, like your book, it ended up being a commercial publisher that published it.

In the case of the Siegfried book, the way Siegfried deals with the controversy he's writing about is by essentially pretending one side of the controversy is illegitimate, represented by two misguided "deniers". If HUP editors tried to find reviewers on both sides of the multiverse controversy, from looking at the manuscript the only people referred to on the "denier" side would have been Burt Richter (now dead) and George Ellis. If they didn't get Ellis to review the book, they would have had to go out and dig deeper to figure out who other "deniers" were. I'm guessing they didn't do this, likely refereeing a book about a controversy with reviewers all on one side of it.

10. **Shantanu Desai**
September 24, 2019

Can someone point out these concerns to HUP?

Recently Modified Posts

September 18, 2019
Categories: Uncategorized

I’ve often added material to recent posts as “updates”, while aware that some who might be interested would likely not realize the added material was there. To improve the situation, I’ve just added a “Recently Modified Posts” widget on the right. The ordering is by modification time. I’ll try and figure out how to avoid having the modification time change when I do something like fix a typo (right now some old film reviews are appearing on the list because I recently added a “Film Reviews” category).

Among recent updates, I recommend the updates to this posting. Someone pointed me to a quite remarkable exchange earlier this week between Mike Peskin and Nima Arkani-Hamed.

Comments

1. **jellydonut**
   September 19, 2019

   Sadly these will not show up for those of us relying on RSS to keep up to speed on the blog.

2. **Peter Woit**
   September 19, 2019

   jellydonut (and others),
   For those using the RSS feed, what RSS readers are you using? If I understand that I might be able to come up with a better solution. Feedly is the reader I use, and it shows 9767 users currently subscribed to this blog. I’d be curious to know what the other commonly used RSS readers are.

3. **Terry Murray**
   September 19, 2019

   Long time Lurker. Per your inquiry, read your blog via RSS->Feedly. Also the candor in you’re recent post on Siegfried was a breath of fresh air. I laughed loudly. Cheers.

Various and Sundry

September 26, 2019
Categories: Swampland, Uncategorized

A couple of mathematics items:

- Photographer Jessica Wynne has been taking photographs of mathematician's blackboards, and there's a story about this in the New York Times. Many of her photographs have been taken here at Columbia, where we happen to have, besides some excellent mathematicians, also some excellent blackboards.
- A non-Columbia excellent mathematician I've sometimes written about here is Bonn's Peter Scholze. If you want to get some idea of the field he works in (arithmetic geometry) and what he has been able to accomplish, a good place to learn is Torsten Wedhorn's new survey article On the work of Peter Scholze.

On the string theory front:

- Arguments about the failure of string theory as a unified theory have been going on so long that they are now a topic in the history of science. For detailed coverage of many events in the long history of these arguments, you can consult historian of science Sophie Ritson's 2016 University of Sydney doctoral dissertation. It and some of her other work is available at her academia.edu website.
- For the latest in content-free argumentation about the failure of string theory unification, Steve Mirsky has a podcast discussion with string theory fan Graham Farmelo (see discussion of his recent book here), in which Mirsky challenges Farmelo about the problems of string theory. Farmelo has spent a lot of time at the IAS and basically takes the attitude that the point of view of certain unnamed string theorists there is what should be followed. I'd describe it as basically "we've given up working on string theory unification, but will keep insisting it is the best way forward until someone proves us wrong by coming up with a completely successful alternate idea."
- For the absolute latest attempt to extract some sort of "prediction" from string theory, see this week's Navigating the Swampland conference in Madrid. Today there was a discussion session, with results shown of a survey of the views of those attending the conference. Note that, on the contentious topic of the reliability of supposed metastable de Sitter solutions of string theory, the Stanford group defending this reliability does not seem to be represented at the conference. I've been trying to understand what picture of physics this research has in mind, given that one main goal is to torpedo the metastable de Sitter solutions, and thus the usual "anthropic string landscape" picture. Looking at page seven, most participants seem to want to replace single field inflation models with more complicated quintessence or multi-field inflation models.
  In Hirosi Ooguri's talk he gives a supposed "unparalleled opportunity for string theory to be falsified", I gather by claiming string theory somehow implies a small value of r. He quotes Arkani-Hamed as saying that string theorists should have reacted to the bogus BICEP2 measurement of r=.2 by saying "if this is true string theory is falsified." They didn't do that. When the topic came up at the time, what they had to say was:

  > Theoretical physicist Eva Silverstein of Stanford says she disagrees that string theory-based models of inflation are in any sort of trouble. "There is no sense in which we are forced to start over," she says. She adds that in fact a separate class of theories that involve both axions and strings now look promising.
  >
  > Linde agrees. "There is no need to discard string theory, it is just a normal process of learning which versions of the theory are better," he says.

Update: Some more math

- Not just physicists, but mathematicians too can get the ridiculous headline treatment, see Number Theorist Fears All Published Math Is Wrong.
- A recent proof of the twin primes conjecture for finite fields from my Columbia colleague Will Sawin and a collaborator is the subject of a new Quanta article from Kevin Hartnett, see Big Question About Primes Proved in Small Number Systems.

Several physicists now have pieces up explaining why Sean Carroll's claim that "the Multiverse did it" (i.e. all you have to do is believe in multiple worlds) isn't a real solution to the measurement problem. Beside the previously mentioned Chad Orzel, there's also Sabine Hossenfelder and Philip Ball. I agree with Ball's conclusion:

> Here, then, is the key point: you are not obliged to accept the "other worlds" of the MWI, but I believe you are obliged to reject its claims to economy of postulates. Anything can look simple and elegant if you sweep all the complications under the rug.

Update: A video of the discussion session at the Swampland conference is here. It seems that I'm not the only one confused about what assumptions people working on this are making and what they are or are not accomplishing.

Comments

1. **Blake Stacey**
   September 27, 2019

   I think Ball makes a particularly good point near the end of his post:

   > Chad Orzel suggests that the right way to look at the MWI might be as a mathematical formalism that makes no claims about reality consisting of multiple worlds – a kind of quantum book-keeping exercise, a bit like the path integrals of QED. […] However, we have to recognize that many advocates of Many Worlds will have none of that sort of thing; they insist on multiple separate universes, multiple copies of "you" and all the rest of it – because their arguments positively require all that.

   That NYT story really makes me wish we hadn't gone the whiteboard route in our science building.

2. **Peter Woit**
   September 27, 2019

   Blake Stacey (and others),
   I'm afraid I need to encourage everyone who wants to discuss many worlds and the Sean Caroll campaign to do so at the other blogs covering this. Glad to see that others are writing about this, and they should be encouraged.

3. **none**
   September 27, 2019

   Regarding math blackboards, the ones of Knill teaching at Harvard have been simply inspiring
   http://www.math.harvard.edu/~knill/teaching/summer2017/exhibits/blackboards/index.html

4. **David Roberts**
   September 28, 2019

I don't see why people are saying it's "twin primes over finite fields", when really it's twin primes over polynomial rings over finite fields. No one would say we proved the twin prime conjecture over the integers (the OG conjecture) if it was proved over Z[x]. But maybe that's just me being curmudgeonly.

5. **Shantanu**
   September 28, 2019

   Peter, a link to a PI colloquium on swampland by Cumrun Vafa
   http://pirsa.org/19090116/

6. **Will Sawin**
   September 28, 2019

   David Roberts,

   I think in the Quanta article it's just an artifact of the fact that they can only explain one concept at a time – so they explain twin primes, and then finite fields, and then say "we're going to combine them" and then say "that doesn't make sense, finite fields have no primes" so introduce polynomials.

   But you're right that this isn't technically correct language – I would always say "over F_q[T]" to be as precise as possible, or "over function fields" to be a little more colloquial.

   It's fun to try to justify it – once could argue that, because the twin primes conjecture is only interesting in Dedekind domains, the phrase "twin primes conjecture" carries an implicit "for Dedekind domains" or even better "for principal ideal domains" so "over finite fields" means "for principal ideal domains over finite fields, i.e. polynomial rings".

   I guess Peter got it from the Quanta article but I'm sure he can argue his own case if he so desires.

7. **Peter Woit**
   September 28, 2019

   David Roberts/Will Sawin,
   Yes, I was using a sloppy terminology, following Quanta, but I think it's justifiable. Anyone who knows something about this field will know what is being meant. The standard math terminology for these things ("function fields") has always seemed to me to be opaque to anyone who hasn't heard it before, giving no indication of what kind of "functions" this has to do with.

8. **Yatima**
   September 28, 2019

   A couple of years ago, Buzzard saw talks by the senior mathematicians Thomas Hales and Vladimir Voevodsky that introduced him to proof verification software that was becoming quite good. With this software, proofs can be systematically verified by computer, taking it out of the hands of the elders and democratizing the status of truth.

   An apparent red-guard mentality concerning the need of canceling stale "elders" and implementing forced "democratization" (presumably after a "necessary conversation" has been had; with said conversation hopefully not ending in a short walk behind the shed)?

   It's almost as if I was reading Vice.com.

   Oh wait! Never mind.

   (And I absolutely like theorem-proving by machine; but I also agree with Michael Harris in [Why the Proof of Fermat's Last Theorem Doesn't Need to Be Enhanced](#))

9. **Peter Woit**
   September 28, 2019

   Yatima,
   What I think is more remarkable is the identification of "democratizing" with replacement of the judgement of human beings by that of machines.

10. **Blake Stacey**
    September 29, 2019

    > What I think is more remarkable is the identification of "democratizing" with replacement of the judgement of human beings by that of machines.

    I think this puts the matter well.

    Years ago, I read a comment by Michael Ashbacher about the classification of the finite simple groups. He wrote that the classification theorem was so long, the probability of an error in it approached 1, but the probability of any error being *uncorrectable* was nearly zero. There's just so much found out in the course of proving the result that the final structure is basically robust, in a way that mechanistic checkability doesn't really capture. The greater concern with the CFSG is that it wasn't written in any single place, let alone "democratized" so that people even slightly less specialized in the field could approach it. (The "second-generation" proof, revised and systematized with the benefit of hindsight, runs to [eight volumes so far](#) and still isn't complete.)

11. **Blake Stacey**
    September 29, 2019

    (*As**c**hbacher)

12. **jsm**
    September 29, 2019

    I agree with David Roberts: "twin primes conjecture for finite fields" made no sense to me, whereas "twin primes conjecture for polynomials over finite fields" should make sense to everyone.

13. **Peter Woit**
    September 30, 2019

    OK, OK, I give in. "finite fields" edited to "polynomials over finite fields".

14. **Neville**
September 30, 2019

I am more concerned with the exposition of the classification of finite simple groups than its correctness. Will future generations be able to make sense of this voluminous information? For example, some of Michael's work requires seriously heavy sledding to understand — he erroneously assumes the reader is at least half as smart and knowledgeable as he is. (Back in the day, I heard loud complaints about the difficulty of understanding my own very small contributions to the subject, although I don't think exposition was the real issue.) We should leave future generations with a reasonable chance of understanding the classification and making their own improvements and judgements about correctness when key contributors are unavailable to answer questions. There has been a lot of work on improving the situation, but I wonder whether it is sufficient. There is much understood by experts that is not spelled out on the printed page — Serre has made his share of complaints about the difficulties.

I do not believe this situation is unique to finite group theory. Too much knowledge is in the form of folklore, talks to an inner circle, or private conversations and correspondence. Fortunately, the internet has improved the distribution of information, e.g., the arXiv, videos of lectures, unpublished books, papers, and course notes posted on websites, and online availability of some letters, such as those from Langlands to Weil and Serre.

I agree with Thurston that human understanding should be our goal:
https://arxiv.org/pdf/math/9404236.pdf

Creating a proof in software of the classification would be a ridiculously large undertaking. I doubt the result would be easier for humans to digest.

15. **Wavefunction**
October 1, 2019

I was not too impressed by Farmelo's recent book although I loved his book on Dirac. Basically his attitude seems to be, "I am not an expert on this topic, so I am just going to assume the experts know best."

16. **Peter Woit**
October 1, 2019

Wavefunction,
Yes, Farmelo's attitude appears to be that "if the most illustrious theorist at the IAS tells me that his approach to going beyond GR to a unified theory is the best one, it must be the best way forward". An obvious counterargument is that if he had been a visitor at the IAS during an earlier period, Einstein would have told him that the ideas about unification he was working on then were the best way forward. Einstein's successors at the IAS today I believe are just as wrong now as he was then.

17. **no idea**
October 3, 2019

As a pure mathematician who has used computer-assisted proofs for almost 40 years, perhaps I can comment on the article "Number theorist fears all published math is wrong". Of course all published math is wrong, just as all published physics is wrong. But this is to take an absurd philosophical position. And as many commenters above have noted, pedantic approaches to proof are as foreign to real mathematics as they are to physics. There are mathematicians who loudly proclaim that their proofs are "computer-free" as if it was a virtue – and either make lots of mistakes that good use of a computer would have corrected, or silently use computers for any calculations they regard as "routine", while disparaging anyone else's use of computers that they don't understand. There are proofs that are regarded as "computer proofs" (such as the four-colour theorem) that are conceptually easy to understand, where the use of the computer consists of (a) finding a good way of dividing the problem into hundreds (originally thousands) of separate cases, and (b) systematically working through the cases with a handful of routine methods. There are proofs that are regarded as "human proofs" (such as the classification of finite simple groups) that are conceptually very difficult to understand, where the use of a computer has very little to contribute. Formalising such a proof in order to "check" it is an interesting exercise in logic/computer science, but who checks the checker? Proofs are checked by being part of the culture of mathematics, being used and re-used, re-worked, re-interpreted, taught to the next generation and continually checked against experiment. Just as in physics. And computers have always been used in mathematics – they just used to be human beings, that's all.

18. **Jackiw Teitelboim**
October 4, 2019

Dear Peter,

Regarding Farmelo's supposed attitude that "if the most illustrious theorist at the IAS tells me that his approach to going beyond GR to a unified theory is the best one, it must be the best way forward," and yours "obvious counterargument is that if he had been a visitor at the IAS during an earlier period, Einstein would have told him that the ideas about unification he was working on then were the best way forward,"

There's a significant difference. In Einstein's late days, most of his colleagues who worked on unified field theories, using extensions of Riemannian geometry to include electromagnetism, like Eddington or even Schrodinger, already moved on. Today, it's not only a lonely however illustrious theorist at the IAS that believes that the best approach forward is a particular framework for unification, but a significant percentage of a whole generation of particle energy theorists, working at many illustrious places, particularly in the US.

Another matter. Looking at this Swampland program (the colloquium at PI linked above is a good summary), it seems that Vafa and his collaborators are arguing that the whole approach to EFTs and naturalness arguments used to argue for SUSY at the LHC, failed since their theories, even if they looked OK from a QFT point-of-view, are not in the string theory landscape at all.

19. **Peter Woit**
October 4, 2019

Jackiw Teitelboim,

Yes, the situation is different. In the late stages of Einstein's career there were plenty of new experimental results pointing to more promising things to think about than generalizations of the Levi-Civita connection. Most people sensibly followed what experiment was telling them and ignored Einstein. If we were in the current situation back in the late 1940s, it's hard to know how many people would have followed Einstein. Quite possibly a "significant percentage" of theorists would have been arguing that generalized connections were the best way forward.

The "significant percentage" argument right now is useless, you can find an equally significant percentage who don't think string theory unification is the way forward. As always, judging an issue like this by numbers of followers isn't a very good idea. I've said before that I think the best argument for "string theory unification is the best way forward" is that Witten thinks so. Given zero experimental guidance, following the best theorist you can find may be the best thing to do, but it's a good idea to keep in mind this doesn't always work, with Einstein a good example to keep in mind. In his early 30s Einstein was seduced (for good reason) by a certain vision of the geometrization of physics, one he should not have stayed so attached to for so long. At a very similar age Witten was seduced by a vision of string theory unification, and I think the psychology of why he won't give that up may not be that different than Einstein's story.

About the Swampland program, I do recommend watching the recent discussion session mentioned earlier:
http://150.244.223.31/videos/video/2098/
The degree to which people are flailing about and unsure of what arguments they can trust and build on is remarkable.

Salam: The First ****** Nobel Laureate

October 3, 2019
Categories: Film Reviews

This evening I noticed that a recent documentary about Abdus Salam, entitled [Salam: The First ****** Nobel Laureate](), has just appeared on Netflix, and I spent some time watching it. The title is a reference to Salam's membership in the Ahmadiyya sect of Islam, which in Pakistan has been declared heretical, and thus Salam not Muslim.

I enjoyed watching the film, and learned a lot I didn't know about Salam, but there's not a great deal in the film about his actual work in theoretical physics. While starting to write more here about the film based on some notes I took while watching it, I noticed that Matin Durrani last year at Physics World wrote an excellent detailed review of the film, and I recommend you consult that for more details.

Among those interviewed are Chris Isham and Michael Duff, who have interesting comments on what it was like to work with him. I was pleased to see that one old photograph had him standing in front of a blackboard that prominently featured "Unitary G-reps".

Update: For another detailed review of the film, see here.

Comments

1. **AcademicLurker**
   October 4, 2019

   I've been searching in vain for interesting things to watch on netflix lately. Thanks for mentioning this, I'll probably check it out this weekend.

   All I know about Salam is what I read in Frank Close's *The Infinity Puzzle*.

2. **Peter Woit**
   October 4, 2019

   AcademicLurker,
   A main goal of this posting was exactly to just point to this as something Netflix subscribers might want to look at. Also, since it's about Nobel season, there's a little justification for discussing something which largely was about a Nobel Prize. There's a lot in the film about Salam getting the Nobel Prize, and the significance of that (including an amusing comment from Weinberg contrasting positively Salam's outfit with the penguin suits everyone else was wearing). Almost nothing though in the film about Salam's actual work on the electroweak theory. Close's book is good for that, there's also an old blog post here
   https://www.math.columbia.edu/~woit/wordpress/?p=3972
   although the Dombey article it discusses seems to have an ax to grind.

3. **David Derbes**
   October 5, 2019

   In 1975 I had just come out of the Part III Mathematical Tripos exams. I did well, but not well enough to stay at Cambridge. I was shopped to Imperial, Durham and Edinburgh. Durham, through Euan Squires, seemed to me all Regge all the time, no thanks. At Imperial I met Tom Kibble who was the soul of gentle kindness. He took me twice to see Salam, separated by an hour or so. Salam was never in. I knew his work, but it seemed to me that he would be tough to work with if he couldn't be found. I wrote to Kemmer at Edinburgh, and he invited me to travel north, where I had a chance to talk to Higgs. That went well, and in the fall I started working with Higgs. There is a bit more about Salam in Crease and Mann's *The Second Creation*, and there is a Salam biography with the somewhat scary title *Cosmic Anger* by Gordon Fraser. It's on my nightstand with another two dozen books. I've read a bit of it, but found it slow going. I really admire what Salam tried to do at the ICTP (a hell of a lot of the early supersymmetry stuff came out of there), and there is a touching story in *Second Creation* about Salam taking on Ne'eman as a research student.

4. **Anonyrat**
   October 6, 2019

   A story & interview published in Pakistan, Abdus Salam with Shahid Nadeem, originally published 1984; republished April 2019.
   https://herald.dawn.com/news/1153774

5. **Robert Delbourgo**
   October 8, 2019

   I knew Salam very well and from what I have been reading about this film it is a pity that his scientific achievements are largely skated over.

Various and Sundry

October 7, 2019
Categories: Uncategorized

Various HEP-related links:

- The physics briefing book for the ongoing update to the European Strategy for Particle Physics is now available, for more see here. This describes the physics that one might hope to do with various proposed new machines. The hard part comes in the next few months: coming up with a proposal that has some chance of getting funded.
- Jim Baggott has a new article in Aeon, But is it Science?, arguing that the heavy advertising to the public by physicists of untestable speculation about multiple universes endangers the credibility of the field. I of course am very much in agreement with this, and have often pointed out the problem, see here. Baggott specifically refers to the massive recent publicity campaign from Sean Carroll. Carroll in this case is promoting a multiple universe untestable interpretation of quantum mechanics, but in the past has been equally determined to promote the untestable multiple universe versions of unification, as well as an untestable multiple universe explanation of the arrow of time . Carroll will be touring Australia and New Zealand in February, presumably to further promote this sort of thing.
- There's an ongoing Cosmic Controversies conference in Chicago this week, which tonight will feature a panel discussion on "Do we need the Multiverse and can it made turned into a scientific theory?". Tomorrow the panel topics will be more promising: "What more can we learn from particle physics about cosmology?" and "Convergence or Disruption". You can find video posted from the conference here including a live stream.
- Slava Rychkov is giving a series of lectures on Lorentzian methods in conformal field theory. Lecture notes are appearing here, videos here.

Update: Wired describes job opportunities for physicists and astrophysicists in the fashion industry.

Comments

1. **Anthony Reynolds**
   October 8, 2019

   I thought Scott Aaronson's blurb about Sean Carroll's book was a bit over the top. Tears of joy? Really?

2. **Peter Woit**
   October 8, 2019

   Anthony Reynolds,

   Aaronson is the first person thanked in the book, for extensive editorial help. I suspect he may be the one responsible for keeping the multiple universe woo to a minimum in the book itself.

   Too bad he likely had no say on the book jacket. I do wonder what he thinks of the ongoing Carroll multiple worlds promotional campaign.

3. **Shantanu**
   October 8, 2019

   Peter: I don't think the KICP conference on cosmic controversies is livewebcast (despite the link you mentioned). Only the panel debates are put up online.

4. **Peter Woit**
   October 8, 2019

   Shantanu,
   I'm getting the livestream at that link right now, Arkani-Hamed is on…

5. **tulpoeid**
   October 16, 2019

   So, the physics briefing book looks really good as an up-to-date summary of the field.

   However, one sentence in the linked CERN Courier article grabbed my attention: "Readers are reminded that the discovery of neutrino oscillations constitutes a 'laboratory' proof of physics beyond the Standard Model."

   Last I checked, there was nothing "proving" BSM physics in neutrino oscillations. I went through that part in the book looking for developments that I might have missed and there are none. Actually, quoting Par.6.1.1,

   "To obtain finite neutrino masses, the Standard Model has to be extended in some way. A minimal extension is to introduce gauge singlet neutrinos (so-called right-handed or sterile neutrinos) which would allow to write down a Dirac mass term for neutrinos, in the same way as for all other fermions. This could indeed be the only source of neutrino masses, but in this case coupling constants need to be smaller than 10^-11 and lepton-number conservation has to be postulated as a fundamental symmetry."

   Despite clarifying that the SM might turn out to be adequate, a few lines above it is stated indeed that "The discovery of neutrino oscillation proves that neutrinos have non-zero masses. This is one of the few solid experimental proofs of physics beyond the Standard Model, as new interactions or new elementary particle states are needed to introduce this mass term in the Lagrangian."

   I'm not saying that I wouldn't like neutrino masses to turn out BSM, but … is neutrino hype the new selling point? Are we so desperate now?

6. **Peter Woit**
   October 16, 2019

   tulpoeid,

   I pretty much agree, although the "neutrino masses are BSM" argument is a common one. What people have in mind is the argument that if you don't add a sterile right-handed neutrino fields and just have Majorana mass terms, the usual Higgs sector won't do it, you need something else. However, you can just add a sterile right-handed neutrino field and have exactly the same kind of Dirac mass terms as for the other fermions (then you have to explain why the Yukawas are so small, but you don't understand anything about the values of Yukawas anyway). To me, calling such a scenario "BSM" is kind of misleading.

2019 Physics Nobel Prizes Announced, John Horgan Wins

October 8, 2019
Categories: Uncategorized

The 2019 Physics Nobel Prizes were announced this morning, half going to Jim Peebles for his work on big bang cosmology, half to Michel Mayor and Didier Queloz for discovery of an exoplanet.

You can read elsewhere more details about the prize winners and their work, but I do want to point out that this announcement means (since there will be no further Physics Nobel Prize awards before the start of 2020) that John Horgan has won his 2002 bet with Michio Kaku, with \$2000 going to the Nature Conservancy. The winning prediction from Horgan was:

> By 2020, no one will have won a Nobel Prize for work on superstring theory, membrane theory, or some other unified theory describing all the forces of nature.

If one looks at the comments back then, Gordon Kane signs on to an even stronger variant of the Horgan/Kaku bet:

> By 2020 there will be a Nobel Prize for a string- or unification- or supersymmetry-based theory or explanation or experimental discovery.

Luckily for him he doesn't seem to have put up any money for this, since he has now lost this bet.

For my own comments at the time, see here (this was a couple years before this blog was started). As I explained there, I was willing to sign up on Horgan's side of the bet if the "other unified theory" clause was eliminated. Unlike Horgan, I'm not a sceptic at all of the existence of a unified theory, or of humanity's ability to find it. My argument (which I think has held up well) was that we're not going to get there by pursuing superstring theory or anything like it. In a better world, the LHC would have found not a vanilla Higgs, but something unexpected that gave us a new idea about electroweak unification, one that pointed to a successful new idea about a fully unified theory. I didn't think this was likely, but I thought it was possible, and I wasn't interested in betting against the possibility I would most like to have seen.

What shocks me about where we are now that Kaku and Kane have lost their bets is not that they lost, which was to be expected, but that this loss seems to have had zero effect on their behavior. Kane's endless replacement of failed predictions by new ones is a well-known story. For Kaku, one can get some idea of his current point of view from this interview:

> **Yahoo News:** So tell us about your work in string field theory. You're trying to finish Einstein's equation?
>
> **Michio Kaku:** That's right. We want to find the "God Equation" — the ultimate theory that explains the entire universe. We want an equation that's maybe 1 inch long that would allow us to "read the mind of God" — those are Einstein's words.
>
> **Yahoo News:** And how's it going?
>
> **Michio Kaku:** We think we have it! It's called string theory. It's not in its final form, and it's not testable yet, we have the Large Hadron Collider outside Geneva.
>
> We're testing the periphery of the theory, but the theory itself is a theory of the universe — so it's very hard to test. But we physicists are optimistic. We think we will be able to test the theory. And we think it is the final theory. So physics ends at that point. Another era opens up, but one era ends when we finally prove this is the Theory of Everything….
>
> If string theory is correct, it means that all the subatomic particles — the electrons, the protons — are nothing but musical notes on a tiny vibrating rubber band. So that physics is nothing but the harmonies of the vibrating rubber bands. Chemistry is nothing but the melodies you can create from the vibrating strings. The universe is a symphony of strings.
>
> And the mind of God is cosmic music resonated through hyperspace.

Kaku also appears in this recent story, which Sabine Hossenfelder refers to as "math fiction". For this kind of phenomenon I prefer Horgan's version: "science fiction in mathematical form".

I don't know of other bets on string theory, but there were quite a few bets about SUSY. I assume David Gross has now paid off his lost bets on SUSY, haven't heard though anything about that. At the Copenhagen SUSY bet event, the losers (Arkani-Hamed, Gross and Shih) showed no signs that losing a bet on a scientific outcome had any effect at all on these scientist's views on the issue they were willing to bet on.

Update: Horgan has posted his own take on this here.

Comments

1. **Blake Stacey**
   October 8, 2019

   I'm pretty sure the "read the mind of God" line was Stephen Hawking in *A Brief History of Time*, not Einstein.

2. **Wavefunction**
   October 8, 2019

   Kaku's main goal for a long time has been to generate sound bytes and thrills for public consumption, not sound physics statements grounded in reality. I read his writings as speculative entertainment rather than science.

3. **David Brahm**
   October 8, 2019

   On a purely nitpicky note, I'm not sure whether "by 2020" means "by 1/1/20" or "by 12/31/20", so I'm not convinced the bet is closed. Contract law, anyone?

4. **Peter Woit**
   October 8, 2019

   Wavefunction,
   One can dismiss what Kaku and Kane say as not to be taken seriously, so not a problem. But the phenomenon of string theorists refusing to admit failure is a widespread

one, and it will do continuing damage to both the intellectual health of the field, as well as its perception among the public.

David Brahm,
In any case, there will be no string theory Nobel in 2020, and after that, Kaku will continue to give talks about how well string theory is doing anyway.

5. **Daniel Sudarsky**
October 8, 2019

It is truly astonishing that people that are supposed to excel at critical thinking, to be trained for, and committed to the scientific enterprise, to appreciate the power of reason over dogma and to be against crowd thinking, can express themselves in the manner Dr Kaku does regarding string theory. In an epoch where humanity is increasingly confronting the return of obscurantist attitudes, be it from politicians (and increasingly from segments of the so called " intellectual elites") not to mention from the wider public, it is frankly scary to see the ease with which such tendencies, that become forces to be reckoned with, emerge from within the scientific community. In fact, I wonder to what extent the latter in now contributing to the former. In other words, when eminent scientists tell us "we can prove a theory mathematically" (i.e. without concerning ourselves with empirical evidence) or that "one era ends when we finally prove this is the Theory of Everything", (i.e. there is no doubt that this IS THE THEORY) what is the effect on the attention people would pay to what other, less famous scientists, say regarding vaccination campaigns or global warming.

6. **David Roberts**
October 8, 2019

> Personally, I feel no need to prove the theory experimentally, since I believe it can be proven using pure mathematics.
> -Michio Kaku

Whatever you think about string theory, this is not how physics *or* mathematics works, I'm pretty sure. The following induction

> Because string theory has near-miraculous breakthroughs every 8 to 10 years, we can expect 2 more breakthroughs in the theory before 2020…

is astounding. There were, what, two (or three?) such breakthroughs before 2002? Or are there more stretching back before the 80s?

7. **Low Math, Meekly Interacting**
October 8, 2019

I hope this isn't OT, but while we're discussing the absence of unification, one could say the same thing about this year's prize. Worthy accomplishments, to be sure, but not exactly closely related. Is this not unusual?

8. **Peter Woit**
October 9, 2019

David Roberts,

The "near-miraculous" breakthroughs being referred to are

1. First superstring revolution, 1984: anomaly cancellation calculation of Green-Schwarz. This allowed the construction of certain kinds of superstring unification models that one would other wise suspect would have anomaly problems. 35 years later, all evidence is that if realistic such models can be constructed, there are so many of them that the class of models can give almost anything, explains nothing about the Standard Model. The real significance of this calculation is that it got Witten enthusiastic about superstring unification.

2. Second superstring revolution, 1995: Witten's M-theory conjecture, based upon evidence of string dualities. 24 years later, we still don't even know what "M-theory" is.

In 2002, if you believed the 1984 and 1995 hype, you might by induction have expected a mid-decade "revolution" in the 2000s. By now though, it has become all too clear that the first two "revolutions" did not give what was hoped, and (while you could argue for a 1997 AdS/CFT "second and a half" superstring revolution) since 1997 there have been no significant positive developments in the area of superstring unification. And quite a few negative ones, beginning with the failure of the last hope for some connection to experiment (at the LHC).

9. **Peter Woit**
October 9, 2019

LMMI,
That also struck me as very odd. I can't think of another example of a Physics Nobel award split in half, with the halves going to two completely unrelated topics.

10. **DanM**
October 9, 2019

"I can't think of another example of a Physics Nobel award split in half, with the halves going to two completely unrelated topics."

How about one year ago? Mourou and Strickland invented chirped pulse amplification, which is an optical amplifier that works for ultrashort pulses. Meanwhile, Ashkin came up with the idea of manipulating small objects with light (i.e., optical tweezers), which is a topic that is (from the point of view of an optics person) entirely unrelated to optical amplification, let alone to short pulse amplification.

That sort of thing is not actually all that rare. The assessment of "unrelated" is a matter of perspective. From most people's point of view, cosmology and exoplanets are both astrophysics topics, and therefore are very related to each other.

11. **A.J.**
October 9, 2019

2008 prize to Nambu and Kobayashi/Maskawa seems like a similar split, just a bit less obvious to laypersons.

12. **anon**
October 9, 2019

Another example from 2002: one half to Giacconi for X-ray astronomy and the other shared between Koshiba and Davis for neutrino astrophysics.

13. **Peter Woit**
October 9, 2019

DanM, A.J.,anon,

I see that in the summary of the award, they try and justify putting different things together. Back in 2008, the justification was that both awards were for understanding symmetry breaking (spontaneous for Nambu, CP for Kobayashi-Maskawa). In 2018 the justification was
"for groundbreaking inventions in the field of laser physics". In 2002 it was "for pioneering contributions to astrophysics".

This year, they're really stretching it, just pasting together two unrelated clauses of woo: "for contributions to our understanding of the evolution of the universe and Earth's place in the cosmos".

14. **kdl**
October 9, 2019

The Nobel Prize in Physics 1978 was divided, one half awarded to Pyotr Leonidovich Kapitsa "for his basic inventions and discoveries in the area of low-temperature physics", the other half jointly to Arno Allan Penzias and Robert Woodrow Wilson "for their discovery of cosmic microwave background radiation."

Actually this year's award to Peebles was related to CMB as well.

15. **Peter Woit**
October 9, 2019

kdl,
Interesting, that seems to be a case where there was no attempt at all to relate the two halves. One does wonder what rules are being used to govern this sort of thing.

16. **Moshe**
October 9, 2019

The flip side of the coin is the Physics Nobel Committee not joining together related discoveries when such a linkage would be natural. An example is provided by the (to me, surprising) omission of Robert Pound's elegant pioneering verification of the gravitational redshift in the laboratory (exploiting the ultra-precision of the Mossbauer effect) with the Hulse-Taylor binary pulsar as an indirect test of gravitational waves. The connecting thread would have been experimental testing of GR. Alas.

17. **Amitabh Lath**
October 9, 2019

The bet was made in 2002. The LHC startup got delayed (19 September 2008 magnet incident among other issues). Maybe you want to be a bit generous and extend the bet to the end of LHC Run 3?

18. **David Roberts**
October 10, 2019

@Peter,

that's what I thought, with AdS/CFT as a possible third, but so close to 1995 kind of rules it out.

Do people making such public statements not see they are being actively unhelpful to the field? By all means study field theories of extended objects, ideally in a mathematically rigorous way, just don't set your watch by a single pair of "near miraculous breakthroughs".

19. **chiz**
October 10, 2019

I've thought for some years that Wolszczan, Didier and Queloz deserved a Nobel but I'm puzzled as to why Wolszczan was only given a mention in the write-up rather than a share of the prize. Since when do you give the prize to the second person or persons to discover something?

Like others I'm unsure of what their work has to do with Peeble's. Maybe the committee didn't think Peeble's work was significant enough to be a sole recipient of the prize and was only worth part of a Nobel?

20. **anon**
October 10, 2019

The Nobel Foundation statutes say this about shared prizes:

"A prize amount may be equally divided between two works, each of which is considered to merit a prize. If a work that is being rewarded has been produced by two or three persons, the prize shall be awarded to them jointly. In no case may a prize amount be divided between more than three persons."

I don't see anything preventing a prize being shared by two completely unrelated works, though in practice they seem to at least try to find some connection.

21. **Peter Shor**
October 10, 2019

Why two barely related Nobel Prizes in the same year?

The Nobel Prize is only given out once a year, and I would assume that the Physics Prize Committee believes there is a long backlog of people who deserve it and haven't yet received it. Giving prizes to two only slightly related discoveries in a single year helps relieve this backlog.

22. **Lowell Brown**
October 11, 2019

Why does no one remark that Jim Peebles did wonderful work decades ago and that his award of a Nobel Prize is long overdue? Is no one interested here interested in significant connections of theory with observation?

23. **martibal**
October 11, 2019

@chiz: similar comments were made when Kobayashi and Maskawa got the prize, but not Cabibbo.

24. **Arnold Neumaier**
October 11, 2019

There are many Nobel prizes split to only loosely related or even very different achievements. Beyond those mentioned above consider, e.g.,

2009 Charles K. Kao, Willard S. Boyle, and George E. Smith
– for groundbreaking achievements concerning the transmission of
light in fibers for optical communication
– for the invention of an imaging semiconductor circuit –
the CCD sensor

1989 Norman F. Ramsey, Hans G. Dehmelt, and Wolfgang Paul
– for the invention of the separated oscillatory fields method and
its use in the hydrogen maser and other atomic clocks
– for the development of the ion trap technique

1986 Ernst Ruska, Gerd Binnig, and Heinrich Rohrer
– for his fundamental work in electron optics, and for the design
of the first electron microscope
– for their design of the scanning tunneling microscope

1981 Nicolaas Bloembergen, Arthur L. Schawlow, and Kai M. Siegbahn
– for their contribution to the development of laser spectroscopy
– for his contribution to the development of high-resolution
electron spectroscopy

1963 Eugene Wigner, Maria Goeppert-Mayer and J. Hans D. Jensen
– for his contributions to the theory of the atomic nucleus and
the elementary particles, particularly through the discovery
and application of fundamental symmetry principles
– for their discoveries concerning nuclear shell structure

1961 Robert Hofstadter and Rudolf Mössbauer
– for his pioneering studies of electron scattering in atomic
nuclei and for his thereby achieved discoveries concerning the
structure of the nucleons
– for his researches concerning the resonance absorption of
gamma radiation and his discovery in this connection of the
effect which bears his name

1955 Willis E. Lamb and Polykarp Kusch
– for his discoveries concerning the fine structure of the
hydrogen spectrum
– for his precision determination of the magnetic moment of the
electron

1954 Max Born and Walther Bothe
– for his fundamental research in quantum mechanics, especially
for his statistical interpretation of the wavefunction
– for the coincidence method and his discoveries made therewith

1936 Victor F. Hess and Carl D. Anderson
– for his discovery of cosmic radiation
– for his discovery of the positron

1927 Arthur H. Compton and C.T.R. Wilson
– for his discovery of the effect named after him
– for his method of making the paths of electrically charged
particles visible by condensation of vapour

This is still a non-exhaustive list; see
https://www.nobelprize.org/prizes/lists/all-nobel-prizes-in-physics/

25. **tulpoeid**
October 12, 2019

Allow me to say that all (apart from maybe one or at most two) of the examples of “split” Nobels mentioned in the comments can be accommodated within a single descriptive sentence – at a layman’s level of description. These guys advanced astrophysics on large scales. Those guys found out how electrons in atoms behave. I think that’s good.
(And, yes, there is the backlog to consider as well.)

Foundations

October 11, 2019
Categories: Uncategorized

Some links related to the foundations of math and physics:

- Kevin Hartnett at Quanta has a long article on Jacob Lurie and his work on infinity categories. Unfortunately Lurie didn't participate in the article himself, so comments are only from others. The article does a good job of giving at least a vague sense of what these very abstract foundational ideas are about, as well as examining the math community's struggle to absorb them. Lurie's work on this is spread out over more than 900 pages here and more than 1500 pages here. Recently he has been putting together an online textbook/reference version of this material as Kerodon, which is modeled after and uses much of the same software as Johan de Jong's Stacks project.
- At Mathematics without Apologies, Michael Harris has some comments on a recent discussion of the Mechanization of Math, held here in New York at the Helix Center. A video of the discussion is available here.
- In the new (November) issue of the AMS Notices John Baez has a review of a recent collection of articles about the foundations of mathematics and physics. The book, Foundations of Mathematics and Physics One Century After Hilbert, contains contributions about both math and physics, although in his review Baez concentrates on issues related to physics. He notes "The elephant in the room is string theory."
  The same issue of the Notices contains an informative long article about Michael Atiyah and his career, written by Alain Connes and Joseph Kouneiher (Kouneiher is the editor of the book reviewed by Baez).

Comments

1. **Heiko242**
   October 11, 2019

   After reading the review by Baez it seems to me that "The elephant in the room is string theory." is somewhat contradictory. Apparently the whole book adresses the lack of understanding regarding string theory. But, and please forgive my ignorance, the metaphor "the elephant in the room"…doesn't it say actually, that the elephant is ignored?

2. **Peter Woit**
   October 11, 2019

   Heiko242,
   It's not true that "the whole book addresses the lack of understanding regarding string theory". The only article directly addressing string theory is Witten's. Baez I think explains his "elephant in the room" claim when he writes:

   "Given its remarkable impact on mathematics, it is natural to ask what string theory has achieved toward its original goal: becoming a true theory of physics, one that makes experimental predictions we can test. The volume under review does not address this."

   That's the elephant in the room: the last 35 years have been dominated by a specific proposal for fundamental physical theory (unification via string theory), but, while this subject has interesting connections to mathematics, it has none to experiment and the real world. Wikipedia defines "elephant in the room" as
   "an important or enormous topic, problem, or risk that is obvious or that everyone knows about but no one mentions or wants to discuss because it makes at least some of them uncomfortable or is personally, socially, or politically embarrassing, controversial, inflammatory, or dangerous."
   which is an accurate characterization of the way the problems with string theory get ignored in contexts where they are a central issue.

   I just looked at my old blog posting about the Witten article (which was also published in Physics Today):
   https://www.math.columbia.edu/~woit/wordpress/?p=8068
   There I used the "elephants in the room" line, getting it from an Ashtekar interview where, discussing the problems with string theory he says: "These are elephants in the room which are not being addressed."

3. **Christoph**
   October 12, 2019

   In other math news, a report of the ICM Structure Committee makes for some interesting reading:
   https://www.mathunion.org/fileadmin/IMU/Report/SC/2019/structure_committee_final.pdf

4. **Arnold Neumaier**
   October 13, 2019

   I found in the context of the above the following 1988 article by David Gross:
   https://www.ncbi.nlm.nih.gov/pmc/articles/PMC282459/pdf/pnas00301-0008.pdf
   From p.8374:
   "Our critical colleagues denounce these efforts, indeed all of string theory, and call it by the dirtiest name they can come up with – recreational mathematics. Although I resent being called a recreational mathematician, I admit that there is a valid (albeit small) point to these criticisms. They remind us of the danger, in following the Diracian dictum, of turning into mathematicians. This for some theorists is an ever-present temptation."

5. **Zoviyer**
   October 22, 2019

   I feel that by not having Lurie contributing to the Quanta article about his work we missed reading there about his motivation in developing this theory, like, what were the problems and limitations he saw with the current view or tools of mathematics. But Lurie has given many talks about his work, so maybe Kevin Hartnett could have tried (maybe he did try) to find the pieces that formed his motivation and make it clearer for the readers where all this is heading to? With Grothendieck´work the motivation were the Weil conjectures for example. I'm not sure if the same case happened with MacLane – Eilenberg work that introduced categories for the first time. Some of the comments in the article refer to the non-clarity of what is this really about.

6. **A.J.**
   October 22, 2019

   Zoyiver,

   I don't think we have to ask Lurie why mathematicians are interested in higher categories and n-stacks and such. It'd be interesting to know why he personally chooses to work on them, of course, but a lot of people were already interested before he started work on them. Topologists have been thinking about these ideas since at least the 1970s; they've long known that the standard homotopy category is deficient and were looking for a good replacement. Likewise, people have known that derived categories are defective since they were first invented. In algebraic geometry, the idea of using higher categories goes back at least to Grothendieck's letter to Quillen in the early 80s. Mathematical physicists, I think, got turned on to them mainly through the Baez-Dolan Hypothesis and Dan Freed's papers in the early 90s. Point being, the general ideas and motivations (some of them, anyways) aren't new and didn't come from Lurie. But until he got involved, the ratio of theorems to conjectures and motivation wasn't very high.

John Tate 1925-2019

October 20, 2019
Categories: Obituaries

John Tate, who was responsible for some of the most important developments in number theory and arithmetic geometry during the second half of the twentieth century, has passed away at the age of 94. Tate was a faculty member in the Harvard math department when I was an undergraduate there, moving on to UT Austin in 1990, then retiring from there in 2009.

The work that Tate is famous for includes “Tate’s thesis”, his 1950 doctoral thesis, which may be the most influential doctoral thesis of modern mathematics. For a book-length explanation of Tate’s thesis, see Ramakrishnan and Valenza’s Fourier Analysis on Number Fields. The later generalization of the GL(1) case of Tate’s thesis to the non-abelian GL(n) case is one of the founding pillars of the Langlands program.

Tate was the Abel Prize laureate in 2009, and one can learn a lot more about him from an interview conducted around the time of the award. For an extensive discussion of Tate’s mathematical work, see this article from James Milne, or this review by Milne of Tate’s Collected Works.

From Milne’s web-site, some stories about Tate:

> A mathematician was explaining his work to Tate, who looked bored. Eventually the mathematician asked “You don’t find this interesting?” “No, no” said Tate, “I think it is very interesting, but I don’t have time to be interested in everything that’s interesting”.
>
> As a thesis topic, Tate gave me the problem of proving a formula that he and Mike Artin had conjectured concerning algebraic surfaces over finite fields. One day he ran into me in the corridors of 2 Divinity Avenue and asked how it was going. “Not well” I said, “In one example, I computed the left hand side and got p^{13}; for the other side, I got p^{17}; 13 is not equal to 17, and so the conjecture is false.” For a moment, Tate was taken aback, but then he broke into a grin and said “That’s great! That’s really great! Mike and I must have overlooked some small factor which you have discovered.” He took me off to his office to show him. In writing it out in front of him, I discovered a mistake in my work, which in fact proved that the conjecture was correct in the example I considered. So I apologized to Tate for my carelessness. But Tate responded: “Your error was not that you made a mistake — we all make mistakes. Your error was not realizing that you must have made a mistake. This stuff is too beautiful not to be true.”
>
> During a seminar at Harvard, a conjecture of Lichtenbaum’s was mentioned. Someone scornfully said that for the only case that anyone had been able to test it, the powers of 2 occurring in the conjectured formula had been computed and they turned out to be wrong; thus the conjecture is false. “Only for 2” responded Tate from the audience.

Tate’s father, John Torrence Tate Sr., was a physicist, editor of the *Physical Review* between 1926 and 1950. In one famous story, Tate Sr. stood up to Einstein by insisting that one of his papers be refereed in the usual way. Einstein was outraged (but it turned out the paper was incorrect). A few years ago I was at a talk here in New York at the Simons Foundation, during which the speaker put up a slide referring to Tate (Jr.)’s work, with a picture of Tate. After a moment, from the back of the room we heard “that’s not me, that’s my father!”.

Update: Kenneth Chang has an obituary of Tate at the New York Times.

Comments

1. **Michael Weiss**
   October 21, 2019

   My favorite memory of John comes from the fall of 1977. Prof. Tate was frustrated by our undergraduate class’ not understanding the special properties of normal subgroups, and so he had the entire class recite — aloud and in unison — the central theorem. Prof. Raoul Bott’s office was right across the hall. Hearing the chant-like recitation, Bott knocked on the door and asked in a stage whisper, “John, where is the revival meeting?” John relished the interruption. It was clear to us that these two legendary senior figures had a special bond based on a shared love for the beauty and depth of mathematics.

   Based on comments from Barry Mazur and Dick Gross in recent years, I do not think that John ever repeated this experiment in undergraduate education. Yet whether the recitation worked in any didactic sense, we loved John for his passion, clarity and kindness. (John years later filled in for Bott as interim Master of Dunster House, depriving Adams House students of his generous advice at meals.)

Today's Hype

October 29, 2019
Categories: Swampland, This Week's Hype

Hype about string theory and fundamental physics seemed to have been dying down recently, with only three editions here so far in 2019 of This Week's Hype. Today however brings a bumper crop of the highest quality, with new examples from a few of the world's most prominent theoretical physicists. Today's hype neatly exemplifies the two main current genres of hype about string theory and supposed new fundamental physics. The first is the old-school genre of string theory hype we've now been seeing for 35 years: "string theory makes a testable prediction" (no, it doesn't). The second is the new, post-modern variety: no actual theory, just a grandiose claim that space and time have been replaced, although it's unclear by what.

Trend: Cosmic Predictions from the String Swampland

In the subtitle of this APS Physics piece from Cumrun Vafa, we learn that the string swampland has "led to testable predictions about dark energy." If you read the article trying to find the testable predictions of string theory, you'll get to Figure 3, where the caption says "The colored curves are string theory predictions for dark energy for different values of c."

The problems with this include:

- This is based not on a theory or calculation, but on a conjecture (see e.g. here) that consistent string vacua have certain properties.
- Many experts disagree with this conjecture. In particular it would imply that the well-known supposed metastable string vacua of KKLT are inconsistent, and that is a matter of controversy.
- This conjecture is not the sort of thing that can be proved one way or another, since it is not about something well-defined. The non-perturbative formulation of string/M-theory necessary to get a well-defined answer to such questions about string vacua remains unknown. You can make various conjectures about the behavior of this unknown theory, but then your swampland conjecture is a conjecture about a conjecture.
- The conjecture involves an unknown constant "c". Unless you know what "c" is, you don't actually have a prediction.

In his conclusion, Vafa writes:

> In the next 5–10 years, we may know, for example, whether dark energy is constant or not. If it is, that could pose a serious blow to string theory. But if dark energy is found to be changing, could that observation be the first experimental evidence for ideas emanating from string theory?

The most likely possibility over the next 5-10 years is that measurements continue to be compatible with constant dark energy. I don't believe for a minute that 10 years from now after that result is in you will see Vafa or anyone else giving up on string theory or even admitting it has suffered a "serious blow". On the other hand, if there is any evidence for a varying dark energy, Vafa or others will surely claim it as "evidence for string theory", which it will not actually be.

Over the last 15 years I've often written here about this "Swampland philosophy", which never made much sense to me. I didn't understand back in 2005 and still don't understand now why conjectures that behavior you don't observe in the real world might be inconsistent with some other conjectures about an unknown M-theory are supposed to be of interest. The sociological motivation here is rather clear though: the KKLT-based "anthropic landscape" philosophy has not worked out well for the field, and the hope is to disentangle the subject from that morass. A good explanation of what is going on is provided by this (stolen from Will Kinney, who also has a lot to say about the whole swampland business):

Cosmic Triangles Open a Window to the Origin of Time

This is a different sort of hype, with no direct relation to string theory. In this genre of hype, you don't have any connection between your calculation and either experiment or a fundamental theory, but this doesn't stop you from making grandiose claims. What's behind this particular article is this paper, which develops a nice calculational method exploiting conformal symmetry. What's not made clear in the Quanta article is that this has no connection to anything measurable. As the authors of the article explain:

> In this paper, we have worked under the lamppost of weakly broken conformal symmetry. This has allowed us to derive particularly clean insights into the analytic structure of inflationary correlators. However, it also restricts the strength of the couplings between the inflaton and additional massive fields. This makes the observational challenge to detect these effects enormous.

In other words, this is about speculative models in which the observable effects described at length in the Quanta article would be unmeasurably small.

The post-modern hype come into play with the argument that these conformal-symmetry based calculations somehow tell us how to replace space and time.

> This suggests that the temporal version of the cosmological origin story may be an illusion. Time can be seen as an "emergent" dimension, a kind of hologram springing from the universe's spatial correlations, which themselves seem to come from basic symmetries. In short, the approach has the potential to help explain

why time began, and why it might end. As Arkani-Hamed put it, "The thing that we're bootstrapping is time itself."

If you're trying to understand the origin of this particular dollop of hype, it's a good idea to keep in mind something Arkani-Hamed said at a talk about Lance Dixon's work back in 2013:

> … I AM an ideologue. In my defense at least I can say that I'm a serial ideologue, in the sense that I'll take totally different ideologies and drop the last one without thinking about it, but it's very important for me personally to be an ideologue when I'm working on something…
> So, usually I'll get up when I talk about scattering amplitudes and give a long introduction about how spacetime is doomed, we have to find some way of thinking about quantum field theory without local evolution in space time and maybe even without a Hilbert space and blah-blah-blah. This is all very high-falutin stuff, this is stuff that Lance wouldn't be get caught dead saying. I think none of these guys would ever say something that sounds so pretentious, but I have to say it, you know I have to say it, because this is the only way I can get up in the morning, and like "I suck again, OK, here we go, I'm doing it because spacetime is doomed, I swear to God, right".

I'm actually rather sympathetic to the "bootstrap" philosophy in some general sense, which I'd interpret as "all is unitary representation theory of the conformal group", i.e. that constraints of conformal symmetry, analyticity and unitarity are almost enough to determine a fundamental theory. The 1960s version of this, trying to get strong interaction physics purely from such general principles, didn't work out, but I've become more and more convinced that representation theory and fundamental quantum field theory are very deeply intertwined. I do think though that to get anywhere you're going to need to either work top-down (i.e have an actual fundamental theory and derive its implications) or bottom up (i.e. use observations to find the route to better theory).

Will head out soon for a short vacation in Northern California (if it hasn't burned to the ground). I had thought about stopping by the 2020 Breakthrough Prize Symposium, but decided listening to talks about why supersymmetry/supergravity got a $3 million award would not be good for my blood pressure.

Update: Those interested in KKLT vs. Swampland debates might be interested in the latest from Tom Banks. He makes a detailed case against KKLT/eternal inflation/Landscape models, which string theory Swampland enthusiasts may find appealing. They should however note what he has to say about string theory itself:

> A more sensible attitude, which I share, is to accept that string theory defines some models of quantum gravity, but obviously not the one that corresponds to the real world.

Comments

1. **S**
   October 29, 2019

   Curious — when you say you're sympathetic to bootstrapping, does that include the idea that "the thing that we're bootstrapping is time itself"? Or are there versions of this that are less exotic? (I haven't known you to be a big friend to emergentist pictures, so I'm guessing so, but am out of my depth.)

2. **Thomas Van Riet**
   October 30, 2019

   "The non-perturbative formulation of string/M-theory necessary to get a well-defined answer to such questions about string vacua remains unknown. " Not sure. If there are regions of string moduli space where we can do computations (weak coupling regimes and their duals) then we can try to uncover patterns in those regions. These patterns can then have deeper meanings, even relate to thermodynamics, black hole thought experiments, etc. If so one can expect these patterns to be there in the full string theory. Then we use holography to check for further non-perturbative evidence. That is the Swampland. Being against that would be against understanding patterns. Sure, some conjectures are quite radical and imply that "established" things (KKLT) are not that established. But that we knew. Check for instance McAllisters' talk at Strings 2019. He admits at the very end that the separate ingredients of KKLT might seem under control (not my opinion) but it remains to figure out whether the collections of ingredients can coexist. So no. Nothing was established. Neither a landscape and neither a very constraining Swampland. But a Swampland is clearly there. And it is exciting to see what can be proven more rigorously in the already-complicated "lamppost region". So let us carry on and enjoy it. It is the most healthy hype sofar. Kudos.

3. **Luca**
   October 30, 2019

   It's a bit sad to see this kind of hype coming from Quanta magazine. At least it is based on a very interesting theoretical work though

4. **Peter Woit**
   October 30, 2019

   S,
   People have different motivations and meanings for "bootstrap", but in general it involves the idea of not looking for a fundamental Lagrangian, but instead trying to characterize as much as possible a theory by general principles including typically symmetry, analyticity, unitarity. It you look at what these principles are, to a large degree they're equivalent to saying you want some sort of highest weight (which gives positive energy, analyticity), unitary (unitarity), representation of a space-time symmetry group (conformal group or at least Poincare group).

   Where I part company with bootstrappers is that I don't think one should assume that the fundamental theory is some complicated, inaccessible thing (e.g. string theory/landscape), with simple representation theory behavior only emerging in a low energy limit. If you find a good representation theory picture that realizes the hopes of the bootstrap program and describes the real world, I think it will actually be a fundamental theory (and you probably can find a Lagrangian version of it if you want).

   The "we're bootstrapping time itself" business is just meaningless promotional blah-blah.

5. **Peter Woit**
   October 30, 2019

   Thomas Van Riet,

   If you want, you can carefully examine an area directly under a lamppost and then guess that the rest of the world behaves in the same way. The problem is that if you then start publicly claiming "predictions" based on this guess, you need to make clear to people that all it is is a guess. And also, that when this guess is shown to be wrong, you have no intention of abandoning your theory, all you will do is say "OK, looks like things aren't the same elsewhere as under the lamppost".

   Both the swampland and landscape programs have exactly this problem. The arguments about the consistency of metastable vacua like KKLT also take place close to an unphysical lamppost, and are useless for making any legitimate scientific predictions about what the conjectural M-theory predicts about the real world.

6. **Peter Woit**

October 30, 2019

Luca,

Most of the Quanta article is fine. It should though have made clear how far this is from connecting to any observations. As a general principle, any quotes from Arkani-Hamed about revolutionary changes in our understanding of space and time should come with a warning label about the large renormalization factor that needs to be applied to such claims.

7. **zzz**
October 30, 2019

another good will kinney

https://twitter.com/WKCosmo/status/1188539724048273408

8. **Will Kinney**
October 30, 2019

Glad you liked the meme Peter. 🙂

9. **Peter Woit**
October 30, 2019

Will,

I thought it was very enlightening as an explanation of the current swampland/KKLT argument. Thought for a minute about asking for your permission to use it, but decided "why do that, this is the internet, I'll just steal it."

10. **DRLunsford**
November 1, 2019

How does the conformal group come into the referenced paper (https://arxiv.org/abs/1811.00024)? That is, what was the motivation for this?

The problem with a paper like this is that it is too vague.

BTW there are interesting cosmic effects just from conformal kinematics, e.g. the idea of a cosmic rest frame, and by implication a way to distinguish it (e.g. the CMB). Amazingly, the Hubble relation itself comes right out this idea.

-drl

11. **Matthew Foster**
November 5, 2019

"all is unitary representation theory of the conformal group"

Well, not quite all. Non-unitary representations are pretty important too, if (rather frustratingly) still very poorly understood. Critical 2D percolation, the plateau transition of the quantum Hall effect, self-avoiding walks are all supposedly logarithmic conformal field theories still waiting for a consistent synthesis as far as I am aware.

I might wager that something like quantum gravity should also suffer from signatures of "non-unitarity," i.e. multifractality, turbulence, etc. Why should "spacetime foam" be anything less complex than classical 2D percolation is my question. It would seem to me that the same issues with nonlocality could naturally arise…

12. **Bernhard**
November 5, 2019

"A more sensible attitude, which I share, is to accept that string theory defines some models of quantum gravity, but obviously not the one that corresponds to the real world."

But is even that really true? I remember reading an article on the arXiv (https://arxiv.org/abs/1105.6359) several years ago which argued that "string theory addresses some problems of quantum gravity" but cannot yet be considered a theory of a quantum gravity, even an inconsistent with Nature one.

13. **Thomas Larsson**
November 7, 2019

Matthew Foster,
AFAIU unitarity is necessary for locality. Geometric phenomena like percolation and SAWs (I know nothing about the QHE) are defined globally, even if they are built from many small building blocks. Percolation is a path between two distant points, and a SAW defines the mass of a linear polymer inside a large sphere, and both definitions involve large-distance properties. Both phenomena can be embedded into families of graph models, a discrete set of which corresponds to unitary reps of the Virasoro algebra with local correlation functions, but percolation or SAWs themselves are inherently non-local.

So if you believe that quantum gravity is a local theory, you probably need unitarity.

Various Links

November 12, 2019
Categories: Uncategorized

Now back from a trip to the West Coast, here are some accumulated things that may be of interest:

- One thing I didn't do while there was attend the 2020 Breakthrough Prize symposium. For videos of three talks about supergravity, see here. At the time of the award I wrote here about why a \$3 million prize for a failed idea about particle theory was a bad idea. Listening to the talks, I think an even worse idea is telling the public that this is a great example of why they should trust science.
- For another dubious idea from the West Coast, in January the KITP is bringing high school teachers to Santa Barbara to teach them about Spacetime, Holography, and Entanglement. Most of the programs the KITP has run for teachers (see here) have been devoted to explaining important, solid science. Back in 2001 when they promoted string theory I thought that was a bad idea, this latest one isn't much better. Again, when the credibility of science is under attack, why go to the public (or, in this case high school teachers) to promote a highly speculative research program? Is it really a good idea for high school teachers to be exposed to this kind of hype, presumably with the hope that they'll somehow transmit it to their students?
- On the evergreen topic of bad multiverse science, Scott Alexander here defends multiverse speculation, responding to Jim Baggott's article. He and the authors of the more than four hundred comments debate at length a red-herring issue. Alexander writes:

  > My understanding of the multiverse debate is that it works the same way . Scientists observe the behavior of particles, and find that a multiverse explains that behavior more simply and elegantly than not-a-multiverse.

  Yes, if theorists had a simple, elegant multiverse theory with lots of explanatory power, you could get into interesting arguments about its testability and whether the idea was solid science or not. The problem is that no such multiverse theory exists. If you want to talk about the MWI multiverse, your problem is that solving the measurement theory problem by just saying "the multiverse did it" may be "simple" and "elegant", but it's also completely empty. If instead you want to talk about the cosmological multiverse, the problem is that you don't have a theory at all (and the actual fragments of a theory you do have are complicated and ugly). For more about this, see my posting and article on Theorists Without a Theory.

For something more positive, while traveling I noticed two quite interesting articles which explain in a detailed technical way approaches to two of the great unsolved problems of our time, while carefully discussing why the approaches have not (yet?) worked, leaving the great problems unsolved.

- For mathematics and the Riemann Hypothesis, see Alain Connes and Caterina Consani's article The Scaling Hamiltonian, about the attempt to understand the zeros of the Riemann zeta function in terms of the properties of a specific Hamiltonian operator, which in some sense is a generator of a group of scaling transformations.
- For physics and quantum gravity, see Donoghue's A Critique of the Asymptotic Safety Program, which has a detailed discussion of the problems with making sense of both quadratic gravity Lagrangians and the idea of a non-trivial fixed point gravity theory theory. I was interested to see that he has a lot to say about the Lorentzian vs. Euclidean signature issue, something often ignored.

Finally, I recommend reading Elizabeth Landau's interview at Quanta with astronomer Virginia Trimble and Trimble's excellent advice to us all:

> Pay attention. Someday, you'll be the last one who remembers.

Update: Two more math-related items well worth a look:

- Dan Rockmore at the New Yorker on Where do ideas come from?
- A profile of Terry Tao at the Princeton Alumni Weekly.

Update: Yet another blog entry from Scott Alexander about the multiverse, with more hundreds of comments. What is it with the fascination for this empty argument?

At BBC Science Focus, more multiverse promotion from Sean Carroll. He does end though by getting to the real point (note that when theoretical physicists say a question is "hard to answer", it means they have no idea how to answer it):

> Many-Worlds is a lean and mean theory, but it's possibly too lean and mean; there is very little structure to rely on, so questions like "Why do probabilities behave the way they do?" and "Why is classical mechanics such a good approximation to the world we see?" are hard to answer.

This is exactly the problem that those arguing over this at Slate Star Codex and elsewhere don't seem to understand: saying "all is the Schrodinger equation" doesn't tell you how to connect the theory to the world we observe. Adding in an ontology of multiple universes does nothing at all to solve this problem.

Comments

1. **luysii**
   November 12, 2019

   The interview with Trimble is also interesting for the way it contrasts her mindset with that of the woke young interviewer. Trimble isn't about to be patronized or felt sorry for. For details see — https://luysii.wordpress.com/2019/11/12/tough-60s-chick-disappoints-woke-young-interviewer/

2. **Peter Woit**
   November 12, 2019

   I'd rather try and moderate a discussion of everyone's favorite ideas about fundamental physics than one about Trimble's views on sexual harassment. Have left the luysii comment so those who want to discuss this can do so elsewhere.

3. **Bernhard**
   November 13, 2019

   "Is it really a good idea for high school teachers to be exposed to this kind of hype, presumably with the hope that they'll somehow transmit it to their students?"

   From some experience I say this is really bad since neither the exposed students nor the teachers have any idea how speculative these ideas are. I give "pro bono" talks about fundamental physics to young kids and they keep asking about the multiverse (which is close to science fiction). If anyone is to expose untrained audiences to supergravity, one has to be super honest about how not backed up by data this is (followed by a discussion on the importance of following the data and not abandoning the scientific method).

4. **Peter Shor**
November 13, 2019

I don't think the MWI multiverse is completely useless. It says that everything is quantum — there is no split between a macroscopic quantum world of observers and a microscopic quantum world of particles, the way that some people interpret (or at least used to interpret) the Copenhagen interpretation.

Of course, you *can* believe that everything is fundamentally quantum without subscribing to the MWI, but my impression is that there are a lot of physicists who don't truly believe this (for example, anybody who believes there is a sharp distinction between virtual particles and real particles is classifying particles into two classes, one purely quantum and the other more real).

5. **Peter Woit**
November 13, 2019

Peter Shor,
The problem with just saying "everything is quantum" (or with invoking splitting universes) is that it's an empty statement. You're saying nothing about the real problem (how does the classical world of our observations emerge from the underlying quantum formalism?). Yes, "everything is quantum" is simple and elegant, and I even believe it's most likely true, but it's not a non-trivial answer to the deep problem.

The criticism of an empty non-answer to a problem as being not testable/falsifiable is a way of pointing to the emptiness: if an idea about physics is non-empty, not just a bunch of words, you should be able to do something non-trivial with it that you can't do without it. If this new, different thing has some connection to the real world, however indirect, you expect it to imply some in principle observable difference.

6. **theoreticalminimum**
November 14, 2019

Off topic, but was wondering whether you've yet had any chance of reading the new bio of Jim Simons by Zuckerman, and whether you're planning to write a review of it at some point?

7. **Peter Woit**
November 14, 2019

theoreticalminimum,
Just finished reading it late last night, will write about it here very soon.

8. **Peter Shor**
November 14, 2019

Peter Woit:

If you don't say "everything is quantum," there's no problem. There's a classical world and a quantum world, and you don't *need* an explanation of how the classical world emerges from the quantum world.

You have to admit that "everything is quantum" before you can start to develop the theory of decoherence.

Certainly, some people were wondering why the classical world wasn't quantum before MWI — we have Schrödinger's cat and Wigner's friend — but my impression is that most physicists didn't even think about this problem. This seems to be confirmed by the historical development. Considering the following timeline (based on quotes from Wikipedia):

> Many-worlds is also referred to as the relative state formulation or the Everett interpretation, after the physicist Hugh Everett who first proposed it in 1957. The formulation was popularized and named many-worlds by Bryce DeWitt in the 1960s and 1970s.

and

> Decoherence was first introduced in 1970 by the German physicist H. Dieter Zeh and has been a subject of active research since the 1980s.

I agree with you that the statement "everything is quantum" is not an answer. But I think it's part of some very important questions.

9. **Peter Shor**
November 14, 2019

To continue my previous comment, while you can't test the MWI experimentally, you can indeed test the statement "everything is quantum" by doing experiments that look for interference between larger and larger objects. And so far, they haven't found a limit.

10. **Peter Woit**
November 14, 2019

Peter Shor,
I don't think we really disagree. Yes, you need to first pose the problem. My point is just that invoking MWI at the level of "everything obeys the Schrodinger equation, and our observed universe is one of many" sounds simple and elegant, but it does zero to solve the problem. And you can see this by noting that there is no testable distinction with the "textbook" or "Copenhagen" interpretation.

The theory of decoherence is very much non-empty, and, as a result, is very much testable.

11. **Will Sawin**
November 14, 2019

It's reasonable to complain about some many worlds promoters, but the part that's reasonable to complain about is not any flaw in MWI, but just the fact that they are presenting the existence of parallel universes as a big, exciting scientific discovery. As you note it's not even a scientific theory, just the avoidance of some bad theories and philosophical positions (objective collapse, hidden variables, denial of objective reality). It shouldn't even be that surprising. After hundreds of years of every scientific theory that implies that I, personally, am in some way special or unique, getting knocked down, are we really supposed to keep falling for it?

You are of course correct that MWI does not in itself contain any mathematical model of how, in fact, quantum states decohere. But the argument that we should take the interpretational point of view of whoever does the best mathematical (or experimental) work in this area seems shaky. Given that any correct quantum mathematics is compatible with any mainstream quantum interpretation, why should we not take the best mathematics and combine it with what we separately understand to be the best interpretation?

I read the beginning of one of the papers in the mathematical theory of decoherence that you linked on this blog, and noticed that it relied on a system of axioms about what observers observe and how that relates to the wave function. I don't think it explained what an observer is or how to tell which (if any) observers exist. If you add to this theoretical system any plausible definition of how to tell which observers exist from the wave function, you can conclude that there exists an observer which observes one outcome of the experiment and there also exists an observer which observes the other outcome, i.e. many worlds. I don't think this reasoning is illegitimate because I haven't written a paper on the mathematical side of decoherence.

12. **Blake Stacey**
November 14, 2019

There were historical predecessors to decoherence theorizing before Zeh, going back to Rosenfeld and company in the 1950s and even Heisenberg in the 1930s. Why the subject took so long to develop is an interesting question of psychology and perhaps even sociology, but telling why an idea got overlooked is always tricky. For example, Wigner nearly got the no-cloning theorem in 1961 but narrowly missed the point.

Weyl understood the mathematics of entanglement as early as 1931 — he called it a kind of *Gestalt,* since Schrödinger hadn't coined the modern word yet. In principle, he or anybody else after that could have said, "Hey wait, a generic unitary on a joint system will have no reason to respect the tensor-product structure, so it will rotate a tensor-product state into one that is *Gestalt*-y, whose marginals will be mixed, I wonder what that implies?"

I think that an underrated factor in the history of philosophizing about quantum mechanics is that, for much longer than we intuitively think nowadays, not everybody *trusted* quantum theory in their bones yet. Weyl himself expected it to be "subsumed" into a better theory. As late as 1938, Kramers was saying that "everyone knew that the quantum theory was provisional" and that there was no guarantee that "after some years, Schrödinger's equation would still be the root of everything." (See the proceedings for the 1938 Warsaw conference, which also report that Heisenberg was speculating about some kind of breakdown of quantum mechanics at higher energies.)

13. **Peter Woit**
November 14, 2019

Will,
I think the essential problem here is that, while many if not most of us believe quantum mechanics should also in principle describe observers, we don't know how to do this. There quite possibly are many different ways of thinking about what an "observer" is. It's perfectly plausible that in some definitions of "observer", time evolution take states with one "observer" into states with multiple, effectively disconnected, "observers". You can postulate such a definition with such behavior if you want, but this doesn't really buy you anything. If anyone asks you to say anything about the real world, all you can do is go back to what Copenhagen/textbook tells you to do.

What I see people arguing about MWI doing is starting with a sensible starting point (all is quantum), then ignoring the difficult problem this poses of how to describe observers, measurements, etc. and calculate something about them, instead starting in on empty discussions about conjectural behavior of undefined concepts.

14. **Peter Woit**
November 14, 2019

Blake Stacey,

I'm sure you're right that part of the explanation for the history of this subject is that its founders though of it as provisional. They had just been through a period of huge revolutions in physics, and expected another one soon.

Another part of the explanation may be the very limited experimental tools they had available, with lots of kinds of quantum behavior experimentally inaccessible. The context of the "Copenhagen" argument that you have to describe your experimental apparatus classically was one of much more limited kinds of equipment. If they had, say, a scanning tunneling microscope, they might have argued very differently.

15. **Narad**
November 14, 2019

> I'd rather try and moderate a discussion of everyone's favorite ideas about fundamental physics than one about Trimble's views on sexual harassment.

If I may venture one Trimble comment not on the foregoing subject, oh, man do I miss those ApXX reviews. The typescripts were a thing to behold – just riddled with typos (*many* of which the U. of C. Press failed to catch; c'mon, you call yourself an editor and don't know when a zero should be an *O* and vice versa?) and general zaniness. They rivaled Olin Eggen, whose secretary also prepared his submissions.

I just wish they would have let me have one rather than tossing them to the nearest warm body.

16. **Will Sawin**
November 14, 2019

I like the formulation:

> It's perfectly plausible that in some definitions of "observer", time evolution take states with one "observer" into states with multiple, effectively disconnected, "observers".

But the key point has to do with a certain stronger, but equally true statement:

> It's clear that in every plausible definition of "observer", time evolution takes states with one "observer" into states with multiple, effectively disconnected, "observers".

You're completely right that this doesn't make any progress on the difficult problem of defining observers and related concepts (which are not entirely problems in physics but also moral philosophy, psychology, possibly biology (of course if we use measuring devices and not humans as our observers these should merely be questions of physics and engineering)). But how much progress can we make on these questions if we can't even get this one basic fact right without 60+ years of bitter war?

17. **Arnold Neumaier**
November 15, 2019

Peter Shor,

"anybody who believes there is a sharp distinction between virtual particles and real particles is classifying particles into two classes, one purely quantum and the other more real"

No. The more typical belief is that real particles are quantum, while virtual particles are purely illustrative tools for discussing high-dimensional integrals. See https://www.physicsforums.com/insights/physics-virtual-particles/

"Of course, you can believe that everything is fundamentally quantum without subscribing to the MWI"

I do believe that, based on the much more rational thermal interpretation featured in my new book
https://www.degruyter.com/view/product/537801

18. **David Gerard**
November 15, 2019

Scott Alexander believes in the multiverse because Eliezer Yudkowsky believes in it – he comes from the LessWrong "rationalist" subculture.

19. **Peter Shor**
November 15, 2019

Arnold Neumeier:

I didn't say that the "more real" particles weren't quantum in some respects.

But in actual fact, there isn't a sharp distinction between "real" electrons and "virtual" electrons. It's a continuum, even though dividing electrons into "virtual" and "real" can be useful for intuitive understanding.

This idea that real electrons exist and virtual electrons don't actually exist (and are just a tool for doing calculations) seems to me to be a misconception carried over from the Copenhagen interpretation. To treat physics consistently, you either have to say that "real electrons" are just an approximation, or say that "virtual electrons" actually exist.

20. **Arnold Neumaier**
November 15, 2019

Peter Shor,

"there isn't a sharp distinction between "real" electrons and "virtual" electrons. It's a continuum"

This is not correct; you seem to mistake virtual electrons for short-living effective electrons.

As explained in detail in the Insight article linked to in my previous comment, there is a continuum between stable and unstable particles, but not between real particles (defined by having a state) and virtual particles (defined as internal lines in Feynman diagrams). It is impossible to write down the state of a virtual particle.

" "real electrons" are just an approximation"

Yes, since real electrons in QED are defined only asymptotically, at times $\pm \infty$.

21. **Trent**
November 18, 2019

langlands has some new (september) comments on the geometric langlands controversy. don't think i've seen this on your blog yet so thought you / your readers might like to be aware. https://publications.ias.edu/node/2700

The Man Who Solved the Market

November 14, 2019
Categories: Book Reviews

There's an excellent new book out about Jim Simons and Renaissance Technologies, [The Man Who Solved the Market](), by Gregory Zuckerman. I recommend it enthusiastically to anyone interested in the story of how a geometer ended up being worth \$23 billion. Lots of other mathematicians and physicists have also been involved in this over the years.

I first heard about Simons and his investment operation when I was a postdoc at Stony Brook in the mid-eighties, and have heard bits and pieces of this story from various sources over the years, sometimes clearly distorted in the retelling. It's very satisfying to finally get a reliable explanation of what Simons and those working with him have been up to all this time. For those with more interest than me in the details of quant strategies, the book provides far and away the most information available about how Simons and RenTech have been making so much money so successfully. The author managed to get some degree of cooperation from Simons, and was thus able to get a lot of those involved with him to talk. As a result, while this isn't an "authorized" biography, it's written from a point of view rather sympathetic to Simons.

One question that keeps coming up in the book is that of motivation. Why did Simons abandon a highly successful career doing research mathematics in order to focus on making as much money as possible? Part of the answer is that, from the beginning, Simons always had one foot out of the research math world, playing poker and trading commodities even when he was a graduate student working with Chern at Berkeley. Later, while employed at the IDA in Princeton, he spent time working not just on government projects but on the mathematical analysis of stock market trading strategies. While I've often heard the story of how he was fired from IDA after publicly criticizing the Vietnam War, less well known is that a big problem was that he was quoted in Newsweek saying he planned to work on his own projects, not government ones, until the war was over.

Unfortunately, the book has very little to say about a question I'm fascinated by: what does Simons intend to do with the \$23 billion (and counting, the RenTech Medallion Fund that he has a large piece of continues to be an incredible money-making machine)? There's very little in the book about his philanthropic activities, the most visible of which are at the [Simons Foundation](), which now has assets of nearly \$3 billion with amounts of the order of \$300 million/year coming in as income and going out as research funding. I think that on the whole Simons had made excellent choices with the math and physics that he has decided to fund, from the [Simons Center for Geometry and Physics]() at Stony Brook to a wide array of programs funded by his foundation.

A question that keeps reappearing throughout the book is that of the social significance of RenTech. It's a rather pure test case for the moral question about quant investing: would the world be better off without it? In the case of the main money-maker, their Medallion fund, it's hard to argue that the short-term investment strategies they use provide important market liquidity. The fund is closed to outside investors, and makes money purely personally for those involved with RenTech, not for institutions like pension funds. So, the social impact of RenTech will come down to that of what Simons and a small number of other mathematicians, physicists and computer scientists decide to do with the trading profits (calculated by Zuckerman at over \$100 billion so far).

Simons himself has engaged in some impressive philanthropy, but one perhaps should weigh that against the effects of the money spent by Robert Mercer, the co-CEO he left the company to (Zuckerman discusses Mercer in detail). Mercer and his daughter have a lot of responsibility for some of the most destructive recent attacks on US democracy (e.g. Breitbart and the Cambridge Analytica 2016 election story). In the historical evaluation of whether the world would have been better off with or without RenTech, the fact that RenTech money may have been a determining factor in bringing Trump and those around him to power is going to weigh heavily on one side.

Comments

1. **luysii**
   November 14, 2019

   There is also a fair amount about Simons in "The Physics of Wall Street" by physicist James Owen Weatherall, along with much more about quants and the street. Weatherall makes the statement that Renaissance Technologies is the world's best physics and math department.

2. **Peter Woit**
   November 14, 2019

   luysii,
   I don't know about "best" physics and math department, but it's certainly the wealthiest…

3. **Keith McClary**
   November 15, 2019

   "motivation"

   Oligarchs usually want to establish a dynasty (eg. Trump, Bushs, Rockefellers, Kennedys). Does Simons have kids?

4. **Peter Woit**
   November 15, 2019

   Keith McClary,
   Thinking a bit more about the book, it seems very likely that Zuckerman was operating under some sort of agreement with Simons which involved him promising to stick to RenTech and say little to nothing about the Simons children or his current activities beyond the Simons Foundation. The only real mention in the book of the children is that Zuckerman does discuss the quite tragic stories of the accidental deaths of two of his sons.

   What's not at all discussed in the book is that Simons has three living children, two daughters and a son. One daughter (Liz Simons) runs the Heising-Simons Foundation with her husband, Mark Heising. In 2017 this had assets of \$500 million, in 2018 gave away \$100 million. His son (Nathaniel Simons) with his wife run the Sea Change Foundation, focused on climate change, which supposedly has given away upwards of \$500 million.

   Also not discussed at all in the book is what must be a huge operation investing Simons' assets (I doubt he's putting them in a mutual fund…). This includes at least his family office, Euclidean Capital, and another investment group, Medley Partners.

   I also hadn't noticed that Zuckerman doesn't mention at all this story, which came out two years ago:
   https://www.theguardian.com/news/2017/nov/07/democratic-donor-james-simons-private-wealth-fund-tax-haven-paradise-papers
   Based on leaked documents, this described an offshore trust with \$8 billion back in 2010, as well as giving a lot of other details about Simons' assets.

   This raises the question of the significance of the one number Zuckerman gives about Simons' current wealth (\$23 billion). It's quite possible this doesn't include funds in trusts for his children or assets of Foundations he controls, so the size of assets controlled by him and his family may be much larger than this. Possibly his long-term plan is to leave his wealth to philanthropic foundations controlled by his children.

5. **Paul Blann**
   November 16, 2019

   Following your recommendation I read the book and was somewhat disappointed. It's principal subject is the history of a small group of people who use quantitative methods for exploiting the stock market and other financial instruments. Around this are painted a number of portraits of the key characters starting with Simons, but the cast is large. Despite the hagiographic treatment in the book these people are, in the main, a pretty unpleasant lot, mostly driven by an obsessive desire to acquire huge ampounts of wealth. They may well be mathematically adept (and in other subjects as well), but to a man (because they are virtually all men) they lack humanity. I was hoping to get more detail on the techniques used and how they developed over time. Alas, the writer is a journalist and despite doing a reasonable job at the history, was probably not able to expound on the science. I initially thought that the lack of depth in the characters was down to the writer – by the time I had finished I concluded that it was because many of the cast were rather one dimensional.

   I would say that if your interest is in how financial markets work and the people who work in them, then this could well be of interest. If you are interested in mathematicians in a real world environment I'm afraid this book will not shed much light.

6. **Peter Woit**
   November 16, 2019

   Paul Blann,

   Thanks. Your take on the book may be that of many readers, mine was colored by having a lot of interest, knowing a bit about many of the characters involved and having been curious to know more for a long time. It's true that on the whole they don't seem to be particularly interesting or pleasant people. The selection effect is for those who want to devote their lives to making as much money as possible by identifying obscure signals in a time series.

   Zuckerman doesn't give a lot of detail about exactly what Simons and his associates have been doing, but there's a lot more here than elsewhere. I would suspect the lack of detail is mainly due to the fact that those involved were only willing (or allowed) to discuss these trading strategies in generalities.

7. **Anonymous**
   November 16, 2019

   On the note of character, I found it disturbing that Simons would smoke during talks at the Simons Center (and I'm sure in other indoor environments) during the few times I saw him there. It gave off the vibe of "I'm so rich, I can break the rules and no one's going to say anything".

8. **Peter Woit**
   November 16, 2019

   Anonymous,
   The book contains more than a few stories about his smoking. One of them is that Edward Thorp wouldn't meet with him because he had heard he smoked in his office and meetings.

9. **Art**
   November 18, 2019

   Question about arXiv: Besides Cornell, arXiv "gratefully acknowledge support from the Simons Foundation and member institutions" on their homepage. Awhile ago, however, arXiv (briefly) solicited donations. I chuckled and moved on, but continue to wonder about the backstory to that episode.

10. **Peter Woit**
    November 18, 2019

    Art,
    Simons, mainly through the Simons Foundation, pays for a wide variety of things helpful to math and physics research, the arXiv contribution is just one.

    It seems that the arXiv is still asking for donations, see
    https://arxiv.org/about/donate

    One problem with having gazillionaires fund such things is that those with more modest resources, e.g. mere millionaires, start to feel there's not much point to donating, best to leave science philanthropy to our tech and financial overlords.

11. **Chris Oakley**
    November 19, 2019

    The notion that professional investors in capital markets are doing the world a service by "providing liquidity to the market" always makes me smile. Yes, they are, in the same way that professional poker players in Las Vegas provide a service to the tourists. These are the people who ensure that dumb investors like you and me get as little return on our investments as possible because they will have got there first. Still, I should not complain, as working for investment banks for 15 years is what paid for this house. I merely want to make the point that the notion that Jim Simons "launched the quant revolution" is a bit ridiculous. The idea of investment based on data-driven quantitative models occurred to a lot of people, including a number of companies in London who interviewed me in the late 1980s/early 1990s. Most of them are still going strong, but naturally they are very secretive, as I am sure was Jim Simons in the early days, so if one had to say anything along these lines it would be that each one of them separately "launched the quant revolution".

12. **random reader**
    November 23, 2019

    A less known piece of the story is that in addition to Simons' philanthropic outreach to scientists, RenTec has inreach that brings scientists and mathematicians to the firm as hired consultants. David Donoho was visiting Rentec when compressed sensing was the next big thing, and Cumrun Vafa has been there. It would be interesting to know who else has moonlighted at Renaissance.

    re: the 3 living children, it was reported that the younger daughter is autistic, and that as a result Simons has donated huge sums of money to research on the genetics of autism. Simons' surviving son has worked at Rentec in addition to being a philanthropist.

    > "The selection effect is for those who want to devote their lives to making as much money as possible by identifying obscure signals in a time series."

    This is a bit unfair even if technically true as a description of many jobs at RenTec. My understanding is that Simons set up the work culture of the firm based on the places he used to work at, IDA and academic research. As a result the employees seem to have a not insubstantial freedom to pursue their own projects and interests, though I guess (as at Google and other places with a similar approach) the idea is in part that a lot of those projects will end up making money for the company. From what I've seen of employees' postings on social media such as MathOverflow, they seem to maintain their interest in their areas of scientific expertise. Simons himself wrote some arxiv papers with Dennis Sullivan, harkening back to his work from the 1970's on differential cohomology (something about axiomatizing or characterizing the theory that had been originally defined by a construction).

13. **Cody**
December 12, 2019

To Peter, Paul and others,

I recommend you listen to Gregory Zuckerman's recent interview on the Bloomberg Master in Business podcast (titled 'Gregory Zuckerman on the Quant Revolution'). The podcast covers how the book was written and confirms Peter's suspicion as why the book is lacking the details one may have hoped to have learned about RenTech.

Terrifying Odyssey Through a Cursed World

November 18, 2019
Categories: Uncategorized

The great German artist Anselm Kiefer now has a show up in London at the White Cube Bermondsey gallery, with a review in the Guardian entitled [Terrifying Odyssey Through a Cursed World](). The review describes some of the works as follows:

> Another room is given over to panoramic blasts of brown and black that map sweeping vistas of desolate fields. A road twines through a morass of mud and collaged sticks. Lines of fence poles vanish in the distance. These scenes are drawn in black on a vertiginous scale. Kiefer uses perspective, the Renaissance technique of showing the real world shrinking towards a single vanishing point, to define his landscapes – but the perspective view is a transparency on top of a muddy tumult of colour and texture, with real, 3D stuff stuck over that in turn. From the right distance, the picture of a landscape can be read clearly, like a painting by Van Gogh. Go closer and the picture dissolves in a mess of bulges and muck.

What's the inspiration for these works (besides the Holocaust)?

> These landscapes are entitled Superstrings, a reference to string theory, an influential idea in contemporary physics that seeks to unify quantum mechanics with Einstein's relativity.

and the show is entitled Superstrings, Runes, The Norns, Gordian Knot. The gallery website explains:

> White Cube is pleased to present an exhibition of new work by Anselm Kiefer. The exhibition brings together many of the interests that have characterised Kiefer's work for decades, including mythology, astronomy and history. Located across the entire Bermondsey space, it features a large-scale installation and paintings that draw on the scientific concept known as string theory.

The Guardian review continues:

> The main gallery at White Cube Bermondsey is already pretty bleak in its featureless emptiness. Kiefer makes it work for him by heightening the chill, turning the White Cube into a morgue for Europe. Snow-covered landscapes with none of the cheer of Bruegel stretch away to infinity. They are marked with sticks as black as gravestones and nets that catch at nothing. Kiefer's science reading clearly hasn't cheered him up. The curvy grids of space-time become horrible wire traps in a devastated nowhere. We might be on the no-man's land of the Ukraine border. Anyway, this place has got death in its hard black furrows.

Another review, at City A.M. tells us more about Kiefer's motivations:

> This is where we come to string theory – the monolith of Kiefer's new show. Though he admits that he doesn't quite understand what string theory is, Kiefer professes complete fascination with the idea that there is a scientific equivalent to the allegorical Gordian Knot – an idea that he picked up after thirty years of subscribing to Spektrum, a German monthly science magazine…
>
> String theory cannot be verified empirically. Rather, it is an attempt to provide an all-encompassing description of the universe. And that, says Kiefer, is just why it is beautiful. "I suppose it's like painting," he says. "You cannot prove if a painting is good or bad. That is the point of it – it is descriptive… and there's something sublime in that."

From the images available, the work does look quite amazing. Kiefer quite possibly has gotten to the very heart of superstring theory, seeing in it a dark, desolate and blasted mythology which "cannot be verified empirically."

Comments

1. **Maik**
   November 18, 2019

   Well, at least some good came out of string theory (apart from some of the actual mathematics).

2. **Kerberos**
   November 19, 2019

   Wir
   sind eine durchlöcherte Schuhsohle,
   geworfen ins graue Nichts,
   Versinkend im Seinscheinschlamm :=(

   (said Nietsche, looking for the missing shoestring)

3. **evermasterx**
   November 19, 2019

   NietZsche

4. **Low Math, Meekly Interacting**
   November 20, 2019

   The abject language used to describe this work is bordering on parody. I'm thinking less Nietzsche and more of those Sprockets guys in black spandex, deadpanning about the uncaring and eternal void of existence.

   Not that it isn't beautiful. I find it skillful, striking and unnerving, but come on…

News From HEPAP

November 24, 2019
Categories: Uncategorized

For presentations a couple days ago at the latest HEPAP meeting, see here. One piece of news, from this presentation, is that there likely will be a delay in the scheduled startup of the HL-LHC, with the next LHC run (Run 3) extended for an additional year (through 2024), and the next shutdown (LS3) extended by a half year. The HL-LHC would then start physics in 2028.

Most of the HEPAP discussions have to do with funding. The pattern of recent years has been one of huge decreases in funding proposed by the Trump administration. These are completely ignored by both the Democrats and Republicans in Congress, which passes large increases in funding (then signed into law by Trump). For FY2020 this continues: at DOE the HEP budget for FY2019 was \$980 million, for FY2020 the White House budget request was a massive cut to \$768 million. This was taken no more seriously by anyone than the last few of these, with the FY2020 House Mark \$1,045 million, the Senate Mark \$1,065 million. The FY2020 budget remains to be finally finished and passed, in the meantime the federal government has been operating under a sequence of continuing resolutions.

Specifically on theory funding, JoAnne Hewett has a presentation on The State of Theory. It has no numbers in it, but the DOE numbers given here show an increase from \$60 million in FY2017 to \$90 million in FY2019 for Theoretical, Computational and Interdisciplinary Physics. But within this category, pure theoretical HEP is pretty flat, with big increases for Computational HEP and a huge new investment in Quantum Information Science (\$27.5 million in FY2019). There does seem to have been some sort of decision to de-prioritize conventional theoretical HEP in favor of newer trendy areas.

Hewett describes the general consensus on current problems with theory funding as

- Universal concern on ever decreasing levels of funding for university groups: concern that university programs are dying.

  -Private institutions attempt to offset cuts with non-federal funding sources.
  -Cuts to program further accumulated in 2019. Many postdocs learned in May 2019 that their contracts would not be renewed for the fall. It was then too late to apply for new positions.

- Lab theory programs are also losing researchers.
- Even distribution of cuts across U.S. theory program has indirect proportional effect to small programs.
- Large fluctuations cycle-to-cycle is making groups less cohesive and more inclined to opt for "safer" research projects.
- There is the perception that the recent emphasis on QIS comes at a cost to more traditional HEP theory research.
- Summer salary has been capped or reduced to 1 month in many cases. Removal of summer salary across the board is demoralizing.

and ends with

> The situation is becoming increasingly unstable.
> University-based theory is suffering its most serious crisis in decades.
> Its future is in jeopardy.

It would be interesting to see some numbers on the size of new private research funding going to HEP theory (for instance funding from the Simons Foundation or the private funding of the CMSA at Harvard). I don't know of such numbers but I'm curious whether what is happening is that the total funding level has seen reasonable growth, but increases in funding are going to a small number of elite institutions, with the rest of the field in decline.

On the question of caps or reductions in summer salary, I doubt that any significant number of researchers is reacting to only getting 1 month of summer salary by signing up for another job (e.g. teaching summer school) and not doing research during the other two months of the summer. There has been another huge influx of money to the field that in some sense replaces grant-funded salary supplements: the multi-million dollar Breakthrough Prizes. A sizable number of HEP theorists have now partaken in all or part of one of the \$3 million prizes. If you add in this money, on average HEP theorists may have been seeing significant increases in income, however with almost all of it going to a small number of people (at the same elite institutions that are doing well). What we're seeing may just be the same trend as in the rest of the US economy: a move to a star system with ever larger increases in inequality.

Another problem for the field of HEP theory may be that funding is stagnating because the DOE and NSF are skeptical about its intellectual health. Hewett notes that "Formal theory resides solely in university environment and has undergone significant funding cuts." Trying to make the positive case for this part of the field, she lists three areas of advances, but oddly, the first two are identical. The two areas of advances in formal theory she describes are:

> Advances in strongly coupled quantum field theory (gravity/field theory duality, bootstrap program, amplitudes) has implications for particle physics, cosmology and beyond.
>
> Geometric advances in particle physics constructions from String/F-theory has implications for the "swampland program".

For the second of these, it's quite possible that most physicists don't see this as an advance at all.

Update: Physics World has more about the delay here. It is supposed to be announced on Tuesday. The cause evidently is a budget gap caused by some planned contributions from non-member countries now not happening. The story doesn't explain which non-member countries are involved or why their planned contributions are now not expected.

Comments

1. **Sabine Hossenfelder**
   November 25, 2019

   As I have tried to tell particle physicists for 5 years or so, even the dumbest politician will eventually see that they don't live up to the promises they've been making (basically since the 1980s), which will result in funding cuts unless they make drastic changes.

2. **Peter Woit**
   November 25, 2019

   Sabine,
   The problem is that people right now are responding all too well to the incentive structure of the grant system. If you want to get your grant renewed, the worst possible thing you could do is acknowledge that what you have been doing with previous grant funding was a failure. The incentives are to deny the obvious (that what you have been doing doesn't work), make some dubious claim of success, and try and reorient what you are doing to fit the latest hot topic popular at funding agencies.

For example, instead of admitting that finding new "string vacua" has been a waste of time, the incentive is to keep working on this, but claiming a machine-learning/AI/big data aspect to your research. Instead of admitting that your quantum gravity research has gone nowhere, claim a connection to quantum information theory.

3. **Alessandro Strumia**
   November 25, 2019

   Actually, a bibliometric analysis shows a decline of most elite institutions. A few decades ago one elite institution contributed up to ≈7% of the world-wide output, while now it's ≈2%. Only IAS remains better.

4. **WhoOrderedThat**
   November 25, 2019

   Hmmm… seems to me like accelerators and experimentalists have lived up pretty well to the promises they made… resolutions/triggers/data throughput rates/recorded integrated luminosities at the LHC detectors; Daya Bay nailed theta_13, T2K is plugging along, IceCube has good data. All sorts of experimental activities… a whole new program of (g-2), mu2e, DUNE is making its way through milestones.

   Gosh excoriating accelerator and detector builders for a year or two or three, given the complexity of the projects… particularly the LHC… including incredible financial gymnastics of dealing with many world currencies and distinct science bureaucracies… is not at all broad-minded. Marie Curie, a strong supporter of internationalism and collaboration, would be shaking her head.

   Lots of rockets blew up on the pads prior to successful rockets… gosh they still blow up sometimes. An awful lot of mistakes were made in motorcycles, cars, and planes (hey, ask Boeing about their 737)… theorists sitting on the sidelines and acidly criticizing the folks fighting the real world to get their experiments built quicker and cheaper is not a winning style.

5. **Peter Woit**
   November 25, 2019

   WhoOrderedThat,

   No one is excoriating anyone for the potential HL-LHC delay. May be just as well, allowing a longer run 3 so more data available to analyze during the LS3. The only thing of longer-term significance that this pushes back is the availability of the LHC tunnel for a next generation machine (assuming the length of the HL-LHC run kept constant). Given that any next generation machine looks like it will take a long time to agree on and get funding for, having the LHC tunnel in use for a year or two longer doesn't matter at all.

   I see that Physics World now has something about this, says announcement Tuesday. Evidently the delay is being caused by a budget gap of 100 million pounds or so, caused by expected contributions from non-member countries not happening. I'll add something to the posting about this.

6. **Alessandro Strumia**
   November 25, 2019

   Peter,

   running LHC for a few years more does not matter because it seems anyhow unrealistic that high energy physics would survive until 204X. The main hope is that China decides that they want to become the new world center of high energy physics by having the next collider while LHC runs ad nauseam. If China starts, most Europeans would likely join.

7. **Sabine Hossenfelder**
   November 26, 2019

   @Peter,

   Yes, the existing incentive structure of the academic system encourages scientists to claim that what they have previously done was successful and continues to be exciting and promising. But that in and by itself is not the problem. The real problem is that we all know that this leads to research bubbles, have known it for a long time, and yet don't do anything about it.

   And that is despite the problem is (1) patently obvious (2) much talked about and (3) not difficult to solve.

   The current structure of the system reinforces, rather than alleviates, cognitive biases. In this concrete example that's loss aversion — if you've been working on something for a long time, it's hard to admit (to yourself as well as to other people) that this time was wasted. Any sensible organization of the scientific system should therefore have incentives to *prevent* such cognitive biases instead of reinforcing them.

   Of course this is not specific to particle physics, but that's not an excuse to accept it.

   For the people who always falsely claim that I do not know how to improve the situation, let me remind you that I have a list with things you can do to help here.

8. **Thomas**
   November 26, 2019

   It's a peculiar situation. Other than the stimulus year (2007), the last couple of years have seen the strongest increases for DOE HEP in a long time (basically, since the demise od the SSC). The budget is over a billion dollars, with 90M (if enacted, I think) going to theory. The puzzle is why more of it is not trickling down to University based theory. I suspect the answer does not have much to do feelings at the DOE (or by politicians in congress) about strings, the swampland, or the multiverse. DOE is a mission driven agency, and when new money comes in they want to spend it on projects, like support for experimental programs at the labs, computation, or QIS. At the same time there has been all this philanthropic money for fundamental theory, so that in the end the only people left out are theory groups at public universities who do not get much private support. I actually wonder whether part of the thinking at DOE is that they see no need to subsidize private spending on fundamental theory with public money (although agencies like NSF have a long history of supporting the IAS, for example). I also wonder what happens to all these postdocs at the IAS, Harvard, etc. that are supported by private funds. Historically, the majority of them found jobs, many at public universities.

9. **David Brahm**
   November 26, 2019

   Are Breakthrough Prize winners generally distributing their prize money to their groups and/or departments? That was my impression, though I'm too lazy to generate any evidence.

10. **Peter Woit**
November 26, 2019

David Brahm,
I haven't heard of many examples of that, although had head that Kontsevich did distribute prize money (he got two breakthrough prizes, $6 million).

One group that I haven't heard of getting any prize money are the theorists at non-elite institutions who are now finding DOE/NSF grant money harder to come by.

11. **I**
November 27, 2019

David Brahm, Peter Woit,

A single data point in favour of David's claim is professor Bell Burnell donating her breakthrough prize (£2.3m) for a PhD scholarship award.

12. **Jackiw Teitelboim**
November 27, 2019

Dear Alessandro,

Where can I find the bibliometric data which you refered to in your first comment? Is it publicly available, or published anywhere?

Regards.

13. **Peter Woit**
November 27, 2019

Jackiw Teitelboim,

I'm guessing that what he is referring to is the data discussed in section 5.1 of
https://arxiv.org/abs/1803.10713
The unnamed "elite institution" is CERN.

14. **Alessandro Strumia**
November 28, 2019

Dear Jackiw ,
I compute InSpire data in the way described in the paper mentioned by Peter.
The plot below shows how much some main institutions contributed to the total bibliometric output from 1970 to ≈2017
https://alessandrostrumiahome.files.wordpress.com/2019/11/institutest.jpg

15. **WhoOrderedThat**
November 29, 2019

@Peter @Sabine_Hossenfelder

It is a mistake to ever think that SUSY or string theory or whatever theoretical fashion of the moment was a dominant motivating factor for the LHC or the SSC or the Tevatron or LEP or the SLC or the SppbarS or whatever expensive accelerator that required a vast team you'd care to list.

The motivation is that particle physics discoveries most convincingly appear with increased beam energy. Theorists grab a lot of bandwidth in the discussion, but hardly one hardbitten machine builder or experimentalist believes a-la-mode theory: the exploration into the unknown is the point.

Yes, many if not most experimental particle physicists must plead guilty to having a cognitive bias in favor of clear and compelling empirical evidence. And thank goodness they do.

Alternate particle physics techniques… that eventually quantum loops gave estimates of the charm quark mass, the top mass, and the Higgs mass… that neutrino oscillation and mass is pretty much the only existing clear and compelling beyond-the-SM hard data… are useful but their interpretations always were ambiguous… the superweak description of K0-K0bar CP violation suggested new physics at 10^5 TeV, but turned out that that CP violation was milliweak and the details were filled in slowly and surely by the (at that time) high-energy frontier.

Theorists on the sidelines really have no idea of all the team dynamics that go into actually getting big accelerators and big experimental teams to function successfully. It is hardly surprising that those teams have a persistence time… it is easy to understand that the LHC did not pass a new amazing threshold, but thinking that the LHC should be turned off in an instant because some au courant theory wasn't verified is immature.

The teams and institutions will persist for a while: and maybe something amazing will come out of it, like precision Higgs couplings not lining up with expectation.

And if someday we do get a 100 TeV machine at some reasonable price tag, it will be young and creative LHC team members will almost surely by the motivators and achievers. They won't really care more than a pfennig about string theory or SUSY.

That the course of particle physics has been some sort of psychological hallucination brought about by the sociology of cognition is an idea that has come and gone multiple times. See "Constructing Quarks" by Pickering, for example.

In fact, shocks from unexpected and sociologically sterile (prior to discovery) empirical results are far more common… results which the theoretical particle physics never predicted. Hence the famous phrase, "Who Ordered That?"… the neutron, the muon, the kaons, CP violation, the J/Psi, the long b-life, neutrino oscillation and mass, the rather large t-quark mass, etc.

16. **Peter Woit**
November 29, 2019

WhoOrderedThat,

I don't really significantly disagree with you. Especially at this point, where speculative theory has been such a failure in recent decades, experimentalists should mostly ignore theorists, do what they can to study what happens at the energy frontier, and hope for something unexpected. The next 15 years or so, for better or worse, the path is pretty clear: get the HL-LHC working and get as much out of the LHC as you can. While doing this, best to ignore theorists who want to tell you about ever more obscure ways to look for SUSY or some other dubious speculative model.

17. **WhoOrderedThat**
December 1, 2019

Speculative theorists are sometimes like politicians… you might find them phony and self-aggrandizing, but progress in fundamental physics or government without them would be even more meager.

Unfortunately both speculative theorists and politicians can become untethered… in California the politicians clearly had no idea how to keep the high voltage electric distribution system maintained through our PUC, for example, to suppress wildfires.. echoing their cluelessness with Enron 2 decades ago… or their innumeracy on public pensions about that same time. We can all notice the recent shambles politicians have made out of our foreign service recently.

While speculative theorists chase their tails, Fermilab is pressing forward in (g-2), mu to e, and DUNE. Thank goodness the theorists are ignoring those programs, they probably would impede them.

The theorists are, however, paying attention to dark matter searches, and not helping… they seem to forget that heavy DM interactions with nucleons might be any of the 5 fundamental Lorentz structures – S,V,T,A,P… back in the 1950's, their antecedents thought for sure the weak interactions was S and T, until the tau-theta puzzle (Who ordered that?) induced the fantastic experimentation of Chien-Shiung Wu and a shift to V-A took place. Ironic that no experimentalists got a Nobel Prize for that work… only theorists Lee and Yang (who did do crucial work).

So today speculative theorists want to declare WIMPs dead based on S alone, and ignore the other 4 Lorentz structures, based essentially on ignorance.

However, lots of great energy for other types of dark matter searches… boson-like, low-mass, etc… has come from speculative theorists lately. Bully for all that.

That MOND has been experimentally addressed (on earth), and was a rejected (by other theorists, not experimentalists) portion of the LISA project has been ignored by speculative theorists. They seem to think experimentalists weren't paying attention, or form a false narrative that experimentalists are ignoring MOND.

18. **atreat**
December 2, 2019

Sabine & Peter,

Another way to *solve* the problem is to let it play out precisely how it currently is! Thanks in part to both of you, it is becoming ever more obvious that funding *should* be cut to Theoretical HEP. As you yourselves continuously note, nothing good is coming from it and the return on investment for the public is in many ways *worse* than nil.

Theoretical HEP is in failure mode and deserves its funding cut. Period. Full stop.

19. **Peter Woit**
December 2, 2019

atreat,
My point of view (and I think Sabine's is similar) is not that cutting theoretical hep funding is a good idea or the solution to anything, but that what is needed is to change the reward structure so that the incentives provided by grant funding stop being incentives to keep working on the same failed ideas.

In the case of the DOE HEP budget, the part going to theory is relatively small. Cuts in it seem to have been redirected to small increases in the funding of a few large experimental projects, with likely marginal impact on the science. I don't think this is a net plus, nothing is being done about the underlying problem. Far better would be if DOE would take seriously the problems Sabine and others have been pointing out and make some changes in how hep theory grant proposals are evaluated.

20. **no idea**
December 6, 2019

Peter,
"make some changes in how hep theory grant proposals are evaluated" – do you actually have any concrete suggestions for what changes would be beneficial? Or is this just a gripe?

21. **Peter Woit**
December 6, 2019

no idea,

One simple change: panels should start rejecting grant proposals that propose to continue a failed research program. If you're proposing to work on much the same idea that hasn't worked for thirty years, your proposal should include a serious discussion of why this hasn't worked in the past (and what your proposal is to solve the problem).

If people find they can't get funded to pursue a program that isn't working, they'll have to find a new idea to work on (or drop out of the competition for grants, freeing space for someone who does have a new idea).

22. **no idea**
December 6, 2019

Well, yes, but this is just apple pie. You need a change to the structure of the system, not just some vague advice to the people making the decisions. If the panel is composed of people who don't agree with your definition of "failed", then this proposal doesn't work. The whole system of peer review rewards people who conform to the prejudices of the senior people in the field. Exhorting these senior people to ignore their prejudices is experimentally proven to fail.

23. **Peter Woit**
December 6, 2019

no idea,

No one said this would be easy. The reason many senior people in the field won't publicly admit failure of a research program is precisely because they are aware of the likely cost of that admission (the defunding of this research program).

While politics may make it not practically relevant, the fact remains that all that is needed is intellectual honesty about failure and its implications.

24. **anonymous**
December 6, 2019

Peter and no idea,

You are making the assumption that panels are made of senior people. In fact, many panels in theory have quite a few young people (including assistant professors). But your points are well-taken. How does one tell panels to reject grant proposals that propose to continue a failed research program? I have noted that there have been relatively few funded proposals to study MSSM phenomenology, so maybe things are changing, but too slowly. Still, do you have a specific suggestion?

25. **no idea**
December 6, 2019

anonymous,
I do have a specific suggestion, but Peter always censors it. My suggestion is that grant proposals should be funded at random. Or at least some significant proportion of them. My point is that no-one can predict the good ideas in advance, and it is foolish to try. Of course, one has to do a little work to devise a system that discourages the freeloaders, but one needs to break the feedback loop that stifles diversity and originality.

26. **anonymous**
December 6, 2019

no idea,

Interesting idea. Check out Nature, this week. An article is subtitled "A growing number of research agencies are assigning money randomly". This is being done in Switzerland and New Zealand. The problem in theory, however, is that you might find very important grants (like Stanford or Princeton theory) unfunded, which would be catastrophic. And there are also real crackpot type grants. Maybe the very best and very worst proposals would not be randomly evaluated, but the others could (maybe with a weighting system). Also grants have widely different funding levels (and the requests are usually much bigger than they eventually get), so I'm not sure how it could work. But it is obviously not crazy with serious funding agencies considering it.

This and That

December 2, 2019
Categories: Book Reviews, Multiverse Mania, Uncategorized

First, a few physics items:

- Mark Alpert has a new novel out, Saint Joan of New York, a thriller subtitled "A Novel About God and String Theory", which is an accurate description. It's published by Springer, so you may be able to get access to it like I did through an institutional license here.

  The plot revolves around Joan, a talented high school student here in New York, who has been learning more advanced material through a mentor at City College, and in particular has learned about string theory and Calabi-Yaus. This Joan plays the role of a modern-day analog of Joan of Arc, using divine help to do battle not with the English, but with more modern dark forces. This divine help includes a revelation about Calabi-Yaus and the theory of everything. It's a thriller, so I'll avoid telling more about the plot so as not to spoil it.

  I quite enjoyed reading the book even though I'm not much of a fan of thrillers, although a lot of enjoyment was due to the fact that much of the action takes place here in New York on the Upper West Side, and that the main plot revolves around the question of string theory and existence of a TOE. Edward Witten plays a role in the story.

  If you like this one, you might also want to read some of Alpert's other novels, a couple of which also involve themes of a TOE.

- Most theorists have abandoned the search for a TOE, or the idea of explaining anything about the Standard Model, in favor of concentrating on hopes to find some sort of emergent theory of quantum gravity. For the latest on this, talks from the recent misleadingly titled Quantum Gravity in the Lab conference at Google might at some point be available. John Preskill's slides are here. He indicates that the general idea is that quantum gravity will emerge from "Massive Entanglement, Quantum Chaos and Complexity." This week the IAS will host a similar event, a workshop on Qubits and Spacetime. Wednesday evening many of the participants will be put on a bus to Manhattan, where they'll continue with the 2019 meeting of the Simons Foundation-funded "It From Qubit" collaboration.
- Also here in New York this week, Roger Penrose will be at Pioneer Works Friday night for a public program involving a conversation with Janna Levin. I have no idea whether his presence in New York at the same time as "It From Qubit" is a coincidence or not. If not, maybe the "It from Qubit" people will get back on the bus and head out to Red Hook Friday night.
- Instead of being at the IAS, Nima Arkani-Hamed has been spending the past semester at Harvard, with activities that include teaching a course, Physics 283B: Spacetime and Quantum Mechanics, Total Positivity and Motives. Videos of his lectures are online here (first one here). It would be great if someone could put together a written set of lecture notes from these videos.
- Finally, for some multiverse-related book reviews that have the unusual feature of showing some skepticism, see John Horgan here, Matt Leifer here, also Chris Fuchs here. Fuchs explains the problem with multiple worlds as a solution to the measurement problem:

  > Its main shortcoming is simply this: The interpretation is *completely contentless*. I am not exaggerating or trying to be rhetorical. It is not that the interpretation is too hard to believe or too nonintuitive or too outlandish for physicists to handle the truth (remember the movie A Few Good Men?). It is just that the interpretation actually does not say anything whatsoever about reality. I say this despite all the fluff of the science-writing press and a few otherwise reputable physicists, like Sean Carroll, who seem to believe this vision of the world religiously.

Some mathematics items:

- The latest Notices of the AMS has a wonderful set of Memories of Sir Michael Atiyah.
- In the same issue, AMS vice-president Abigail Thompson criticizes the University of California's use of "diversity statements" in hiring. *Inside Higher Education* had an article about this here, and it led to some controversy described from one point of view here. For some blog discussions see here and here, with the first of these a place more willing to host a discussion of this than I will do here.

Update: In case you haven't been getting enough hype about the multiverse recently, Scientific American has Long Live the Multiverse! for you, from Tom Siegfried. Siegfried assures us that "multiverse advocates have been right historically". He also assures SciAm readers that multiverse theories are testable, in a way similar to the way Einstein demonstrated the existence of atoms in 1905 using Brownian motion:

> For that matter, it's not necessarily true that other universes are in principle not observable. If another bubble collided with ours, telltale marks might appear in the cosmic background radiation left over from the big bang. Even without such direct evidence, their presence might be inferred by indirect means, just as Einstein demonstrated the existence of atoms in 1905 by analyzing the random motion of particles suspended in liquid.

He doesn't mention that his analog of the Brownian motion experiment has been done: people have looked for the predicted indirect effects of other bubble universes on ours, and found nothing. To the extent that the multiverse is testable, it has been tested and found to not be there.

Comments

1. **Arnold Neumaier**
   December 3, 2019

   Matt Leifer, not Liefer!

2. **Will Sawin**
   December 3, 2019

   > I once asked Daniel Simon, one of the founders of quantum computation, whether Everett's interpretation of quantum mechanics aided him in finding his now-famous quantum algorithm. His response makes me laugh to this day: "Everett? Who's Everett? And what is his interpretation?"

   This seems like a strange argument because Simon's work built on earlier work of Deutsch-Jozsa, which built on earlier work of Deutsch, who was heavily inspired by Everett. So Everett's ideas contributed, at least somewhat, to Simon's work regardless of whether he was aware of them.

3. **Peter Woit**
   December 3, 2019

   Arnold Neumaier,
   Thanks! Fixed.

4. **Blake Stacey**

December 3, 2019

Jozsa, to the best of my knowledge, distrusted quantum mechanics: His motivation for working on quantum computation was to see how far the theory could be pushed, and what would break if we actually tried to build a device that relied upon quantum physics to compute. (This is conference chatter; I don't have a specific written reference in mind, though I can try to dig one up some day when I have more library time.)

And yes, it's *Leifer*, though he pronounces it the way that German 101 would make you think is written *Liefer*.

5. **Will Sawin**
December 4, 2019

Blake Stacy,

Perhaps this demonstrates the incompleteness of "has led to useful research" as a measure of the quality of a theory (though it is surely a very useful one). Skepticism of the accuracy of quantum mechanics has led to a lot of interesting research over the past century. But all of that research has just given us more reasons to trust quantum mechanics.

6. **Low Math, Meekly Interacting**
December 4, 2019

I listened to a sobering piece on NPR this weekend (Living Lab, you can look it up). The subject was "Planck's Principle", i.e., roughly, "Science advances one funeral at a time."

Someone actually attempted to test this notion with some rigor. The field was bio-medicine, but there likely are implications for other fields.

The conclusions were actually worse than Planck thought: Even if a Superstar scientist dies, their proteges keep up the rear-guard, stifling progress about as effectively. It's not enough for the ossified to die off. You've got to worry about all the people still around who agree with them.

Seems relevant to Multiverse Madness and its otherwise inexplicable staying power.

7. **Peter Woit**
December 4, 2019

LMMI,

The problem is that Multiverse Mania is not something just afflicting older leading theorists, with younger theorists skeptical. Unfortunately I think to some extent the situation is reversed. Perhaps the leading theorist most willing to take on the mania has been David Gross, who is now 78. Any list you make of prominent proponents will be almost all significantly younger.

There's a historically unusual situation here: the field of HEP theory made huge advances for a very long time, until the mid-seventies. Since then progress has come to a halt, arguably turned negative post-1984 with string theory and now the multiverse. The people who were trained during the period of progress are now hitting retirement age and the field is dominated by those trained in failed ideas who have never experienced what progress looks like. All our usual historically based modes of thinking about how science evolves in time, continually progressing, may no longer be relevant.

On some days I also notice that ideas I grew up with about US history as a progression towards a better and better society are not looking so good either right now, and the cheeriest thought available is that one does get old, die and not have to see where this goes…

8. **no idea**
December 4, 2019

On a more cheerful (?) note, the Memories of Sir Michael Atiyah are indeed a wonderful read. Lusztig's mention that Atiyah pointed out Sandy Green's seminal work on representations of GL(n) reminds me that (a) Atiyah was in the habit of denigrating algebra and extolling the virtues of geometry, and (b) Green was strongly opposed to this point of view. I well remember a wonderful lecture Green gave in Bielefeld, I think some time in the 1990s, in which he systematically demolished all of Atiyah's arguments, and convinced the audience beyond a shadow of a doubt of the superiority of algebra over geometry.

9. **Maik**
December 5, 2019

Peter,

the idea that science progresses continuously is not as prominent in the philosophy of science any more as it used to be. The situation has changed with Thomas Kuhn's works, who advocated for the idea that science progresses in cycles with phases of `normal science' ultimately leading to a `phase of crisis' until `revolutionary science' causes a `paradigm shift' and starts the cycle over again. If one believes in this, then one may very well view `multiverse mania' as a symptom of crisis.

10. **Peter Woit**
December 5, 2019

Maik,
I understand that the idea of continual scientific progress has always been understood to be problematic. But what I'm referring to is something different than the Kuhnian "normal science" leads to "crisis" leads to "revolutionary science" and then "paradigm shift", which is a model of progress with forward jumps.

What if the "crisis" of no progress goes on so long that the field loses its best people and instead attracts those happy with a bogus "revolution", one that is not in any sense progress? As far as I know Kuhn or Kuhnians haven't looked at this possibility, which may describe where we are now.

In the political realm the analog is a crisis of representative democracy leading to the rise of authoritarian demagogues, a phenomenon which is historically much more well-known.

11. **Maik**
December 5, 2019

Peter,

In Kuhn's book "The Structure of Scientific Revolutions" this is actually how he describes a crisis: Many capable people leave the field (some are forced out because they openly question `the paradigm'), old approaches and old (implicit) methodologies fail, scientific `puzzles' are solved via ad hoc postulates often in (indirect) conflict with

the paradigmatic approach taken (personal note: which you may call `revolution' to make your approach more popular), theorizing becomes more and more elaborate without solving the actual problems.
So Kuhn would probably say that this is just how crises look like.

Of course, you are right to say that it is not fully clear where to draw the lines, i.e. reality is often more complex than that. Yet in my mind this is just a consequence of the fact that Kuhn's work is not a mathematical theory, but a qualitative description of a sociological process (like politics).

12. **Peter Woit**
December 5, 2019

Maik,

I guess then the new situation is that HEP theory has been in a crisis for about 40 years or so, a situation with no analog in modern scientific history. To find another such 40 year period, I think you'd need to go back to a point where the field was organized very differently (e.g. a factor of 100 fewer researchers). What happens to a field that is in "crisis" for too long?

13. **Maik**
December 6, 2019

Peter,

Frank Pajares (Emory University) has a good synopsis of Kuhn's book on his website. Kuhn answers your question in Chapter VIII "The Response to Crisis": Basically, the fundamental scientific problems (e.g. what was the multiverse supposed to solve?) start to become recognized as such, resolving them becomes the focus of an increasing amount of scholars, and the otherwise very rigid "rules for normal research" start to loosen up, opening the gate for new ideas and thus new potential resolutions. Then, either `normal science' solves the problem after all (i.e. it was a hard problem, the problems were not with the paradigm itself), scholars set it aside (i.e. it is openly labelled a hard problem without renouncing the paradigm and the solution is left to future generations), or a new paradigm emerges (Kuhn describes in detail how that works).

I would not view things as pessimistically as your words suggest. Physics has lifted off only in the last few hundred years or so. By now we have been able to get rid of an extremely natural, but ultimately naive and wrong view of space and time, and have been able to take a very good glimpse at how matter is fundamentally structured. Yes, we have more researchers today, but I dare to claim that research and research environments have also become more complex. Maybe it is my age, but I am quite confident that good and honest science will resolve the issues we face today.

14. **Peter Shor**
December 7, 2019

@Maik: as Peter says, the scary part is that soon the people dominating the field of HEP will never have seen their subfield of physics work in a normal scientific mode, and will think that the paradigm of scientific `puzzles' being solved via ad hoc postulates and wild extrapolations from toy models is the normal paradigm.

If they then accept all the ad hoc postulates and toy models that the now-senior researchers have already come up with, and also come up with their own ad hoc postulates and toy models (that may or may not subtly contradict the ad hoc postulates that everybody already believes), they will end up with a body of `knowledge' built on extremely flimsy foundations.

This is the way the `It from Qubit' subcommunity of physicists seems to be headed. If it weren't for the example of the AMPS paper (which showed that the community can sometimes realize that some of their ad hoc postulates are self-contradictory), I would be in a state of complete despair over the direction of the field.

15. **Alessandro Strumia**
December 8, 2019

Dear Peter,

I draw your attention to a new scientific publication that criticises string theory. I quote from https://www.journals.uchicago.edu/doi/full/10.1086/704991:
«String theory provides an example of how white supremacist racial prestige asymmetry produces an antiempiricist epistemic practice among physicists, white empiricism». «String theory has failed to succeed in expected ways because the community—which is almost entirely male and disproportionately white relative to other areas of physics—is too homogeneous». «Disentangling physics from the norms of patriarchal white supremacy must begin with an honest accounting of the roots of the Western scientific project in the project of slavery». «Science is traditionally a sexist and racist practice». «White empiricism contravenes core tenets of modern physics (e.g., covariance and relativity)». «Black women must, according to Einstein's principle of covariance, have an equal claim to objectivity regardless of their simultaneously experiencing intersecting axes of oppression». «There are contexts in which Black women are epistemically privileged observers».

The author is presented on popular press as one of the greatest promises of US physics, and influential author of Particles For Justice. Does a field that produces this kind of results deserve to be funded as science?

16. **Peter Woit**
December 8, 2019

Alessandro Strumia,
Thanks, I hadn't seen that, and hadn't thought of that particular explanation for string theory multiverse mania.

The research behind that paper was funded by a 100K grant from FQXI, which also has funded a great deal of pro-multiverse research.

17. **Suzanne**
December 10, 2019

Alessandro, thanks for the link. I couldn't believe it first, but the following is indeed an actual sentence from that paper:

"Given that Black women must, according to Einstein's principle of covariance, have an equal claim to objectivity regardless of their simultaneously experiencing intersecting axes of oppression, we can dispense with any suggestion that the low number of Black women in science indicates any lack of validity on their part as observers."

Wow.

Not sure that person has understood "Einstein's principle of covariance", but the insight's from that research seem well worth the 100K…

18. **FB36**

December 10, 2019

An interesting article about String Theory etc:

https://physicsworld.com/a/a-mathematical-mindset

"He feels that their attacks are part of an "especially regrettable" trend, whereby "anyone can have a valid opinion on any subject, regardless of their technical knowledge and appreciation of it"."

19. **John Baez**
December 10, 2019

More madness: an article in *Quanta* titled Why the Laws of Physics Are Inevitable, with the blurb "By considering simple symmetries, physicists working on the "bootstrap" have rederived the four known forces. "There's just no freedom in the laws of physics," said one."

Yay! Fundamental physics has been solved! It's done!

The funny thing is that the article says nothing at all about the details of this supposed earth-shaking discovery. And indeed the work being reported on has nothing to do with deriving the four known forces.

It's sad to see *Quanta* sinking into the mire of fake news that's engulfed so many other pop physics reporting. Are we really entering a post-truth world?

20. **Jack Morava**
December 10, 2019

what John just said.

21. **Peter Woit**
December 10, 2019

Suzanne,
I'm unable to understand why either string theory's problems with predicting anything or general covariance in GR have anything to do with the problems Black women encounter in the physics community. The discussion I've seen of this on Twitter is not encouraging and I'd rather people not engage in it more here.

FB36,
For my take on Farmelo and his claims, I refer you to here
https://www.math.columbia.edu/~woit/wordpress/?p=11012
where I address a similar comment from him in his book with
"As for the last bit, I'll just say that I think it can accurately be described as "sleazy"".

John Baez/Jack Morava,
That was odd, especially, the sub-headline
"By considering simple symmetries, physicists working on the "bootstrap" have rederived the four known forces. "There's just no freedom in the laws of physics," said one."
which is complete nonsense.
I was planning on writing a more substantive blog entry about this, trying to explain what this is about in a non-misleading way. No time today, probably tomorrow…

22. **Boyer Lindquist**
December 10, 2019

Reading the essay "A mathematical mindset" by Matin Durrani from Dec 2019 issue of Physics World, one senses a revisionist history of TQFT as a result of string theory instead of Yang-Mills instantons in late '70s, e.g. the Atiyah–Drinfeld–Hitchin–Manin construction, while hadronic ST was already considered a failure replaced by QCD.

23. **Peter Woit**
December 11, 2019

Boyer Lindquist,

After many attempts, I've given up on trying to get people to look at the actual history of TQFT, and to understand that Witten did not get his Fields Medal for string theory but for something different. For one of my early attempts, see here
https://www.math.columbia.edu/~woit/wordpress/?p=99

Are Physical Laws Inevitable?

December 11, 2019
Categories: This Week's Hype

The last couple days have seen various discussions online generated by a piece at Quanta Magazine with the dubious headline Why the Laws of Physics Are Inevitable and an even worse sub-headline claiming "physicists working on the 'bootstrap' have rederived the four known forces" (this is utter nonsense). For some of this discussion, see Sabine Hossenfelder, John Baez and Will Kinney.

One reason this is getting a lot of attention is that the overall quality of reporting on math and physics at the relatively new Quanta Magazine has been very high, a welcome relief from the often highly dubious reporting at many mainstream science media outlets. The lessons of what happens when the information sources society relies on are polluted with ideologically driven nonsense are all around us, so seeing this happen at a place like Quanta is disturbing. If you want to understand where this current piece of nonsense comes from, there is an ideology-driven source you need to be aware of.

A major line of defense of their subject by string theorists has essentially been the claim that, while it may lack any experimental support, string theory is "the only consistent way to combine quantum theory and general relativity". I've often explained what the problem with this is, won't go on about it again here. Nima Arkani-Hamed is at this point likely the most influential theorist around, for some good reasons. The roots of the problem with the Quanta article lie in taking too seriously the kind of arguments he tends to make in the many talks he gives. He's trying to make as strong as possible a case for the research program he is pursuing, so unfortunately gives all-too-convincingly a very tendentious take on the scientific issues involved. For more about this, see a posting here about the problems with the recent Quanta article that motivated the latest one.

Debates over generalities about whether the "laws of physics are inevitable" are sterile and I don't want to engage in them here, but I thought it would be a good idea to explain what the serious ideas are that Arkani-Hamed and others are trying to refer to when they make dubious statements like "there's just no freedom in the laws of physics". Here's an attempt at outlining this story:

Quantum mechanics and special relativity:

A mathematically precise implication of putting together fundamental ideas about quantum mechanics and special relativity is that the state space of the theory should carry a unitary linear representation (this is the QM part) of the Poincaré group (this is the special relativity part). You also generally assume that the time translation part of the Poincare group action satisfies a "positive energy" condition. To the extent you can identify "elementary particles", these should correspond to irreducible representations. The irreducible unitary representations of the Poincaré group were first understood and classified by Wigner in the late 1930s. My QM textbook has a discussion in chapter 42. If you impose the condition of positive energy and for simplicity consider the case of non-zero mass, you find that the irreducible representations are classified by the mass and spin (which is 0,1/2,1,3/2, etc.). Non-interacting theories are completely determined by the representation theory and exist for all values of the mass and spin.

Extensions of Poincare and the No-go theorem of Coleman-Mandula

To get further constraints on a fundamental theory, one obvious idea is to extend the Poincaré group to something larger. States then should transform according to unitary representations of this larger group, carrying extra structure. Restricting to the Poincaré subgroup, one hopes to get additional constraints on which Poincaré representations can occur (they'll be those that are restrictions of the representations of the larger group). The problem with this is the Coleman-Mandula theorem (1967) which implies that for interacting theories the larger group can only be a product of Poincaré times an internal symmetry group. Representations will just be products of the Poincaré group representations and representations of the internal group, with space-time symmetries and internal symmetries having nothing to do with each other. This is why the Quanta headline about "rederiving the four known forces" is nonsense: the three non-gravitational forces are determined by internal symmetries, have nothing to do with what the Quanta article is describing, work on space-time symmetries.

One way to avoid the Coleman-Mandula theorem is to work with not Lie algebras but Lie superalgebras. Here you do get a non-trivial extension of the Poincaré group and a prediction that Poincaré representations should occur in specific supermultiplets. The problem is that there is no evidence for such supermultiplets.

Another possible extension of the Poincaré group is the conformal group. Here the problem is that the new symmetry implications are too strong, they rule out the massive Poincaré group representations that we know exist. One can work with the conformal group if one sticks to massless particles, and this is what the methods advertised in the Quanta article do.

The idea that our fundamental space-time symmetry group is the conformal group is mathematically an extremely attractive one, with the twistor picture of space-time playing a natural role in this context. I strongly suspect that any future truly unified theory will somehow exploit this. Unfortunately, as far as I know, no one has yet come up with a way of exploiting this symmetry consistent with what we know about elementary particles. Likely a really good new deep idea is missing.

Quantum field theory

To get stronger constraints than the ones coming from Poincaré symmetry, one needs to decide how one is going to introduce interactions. One way to go is quantum field theory, with a principle of locality of interactions. This gets encoded in a condition of (anti)commutativity of the fields at space-like separations, which then implies various analyticity properties of correlation functions and scattering amplitudes. The analyticity properties can then be used to prove things like the CPT theorem and the spin-statistics theorem, which provide some new constraints.

Given a method of constructing a Poincaré invariant quantum field theory, typically done by choosing a set of classical fields and a Lagrangian, one can try and realize the various possible Poincaré group representations as interacting theories. What one finds is that, for spins greater than two one runs into various seemingly intractable problems with the construction. One also finds exceptionally beautiful theories in the spin 1/2 and spin 1 cases that exhibit an infinite dimensional group of gauge symmetries. An example of these is the Standard Model. Unfortunately, we know of no principle or symmetry that would provide a constraint that picks out the Standard Model. If we did, we might be tempted to announce that the principle or symmetry is "inevitable" and thus the "laws of physics are inevitable". We're not there yet…

Amplitudes and the S-matrix philosophy

In the S-matrix philosophy one takes the analyticity properties as fundamental, working with amplitudes, not local quantum fields. The 1960s version of this program (also often called the "bootstrap" program) was based on the hope that certain physically plausible analyticity assumptions would so tightly constrain the theory of strong interactions that it was essentially uniquely determined. This didn't work out. In his recent introductory lecture for his course at Harvard, Arkani-Hamed explains why. The research program he and others are currently pursuing is in some sense a modernized version of the failed 60s program. The hope is that new structures in amplitudes can be found that will replace the structures one gets from local quantum fields.

Amplitudes based arguments about, for instance, why you don't see fundamental higher-spin states, and why spin 1/2 particles have forces of the kind given by gauge theory have a long history, see for instance work on massless particles by Weinberg in the mid-sixties and Weinberg-Witten in 1980.

As far as I can tell, the work referred to in the Quanta article gives new amplitudes-based arguments of this kind for massless particles, exploiting conformal symmetry. It's not clear to me exactly what's new here as opposed to earlier such arguments, or how strong an argument about real world physics one can make using these new ideas. One thing that is clear though is that the Quanta quote that what has been discovered implies that "There's just no freedom in the laws of physics" is as much nonsense as the "we rederived the four known forces" business.

Update: For some discussion with the author of the Quanta piece, Natalie Wolchover, see the comments starting here.

Update: The Quanta article has been revised, see comments in the comment section here. There Daniel Baumann provides a link to a popular summary of the facts about massless particle interactions that his quotes were about.

Comments

1. **Akhil**
   December 11, 2019

   What are those "good reasons" that Nima Arkani-hamed is the most influential theorist around? And what are your thoughts on his amplituhedron approach?

2. **Low Math, Meekly Interacting**
   December 11, 2019

   Thanks for this. I'm sorry you and a few others had to make the effort, needed as it was. I found that article in particular quite a letdown.

3. **Peter Woit**
   December 11, 2019

   Akhil,
   He has shown excellent taste in moving out of his roots in SUSY phenomenology and instead into mathematical physics. As far as topics in mathematical physics go, looking at amplitudes in twistor space seems like a good thing to be trying. He's clearly smart and hard-working, an inspiration to many.

   Another "good reason" that I had in mind was that he's an extremely energetic and compelling speaker, but while this is an important "reason", maybe it's not a "good" reason, in the sense of good for the understanding of science.

   About the amplituhedron, the hype campaign back in 2013 and the "Jewel at the heart of physics" business in Quanta at the time was a bit disturbing. It was however so over the top that I thought most people didn't take it seriously (evidence for this was Scott Aaronson's blogging about the "diaperhedron"). As a piece of mathematical physics, my reaction to it then was like my reaction to a lot of things: "I don't have time to sit down and figure out precisely what this is and what it does, so I'll just wait a few years and see what happens. Either it will genuinely be a huge advance and after things settle down there will be lots of nice places to easily learn the details, or it won't turn out to be that much, and I'll have saved a lot of time." Six years on, this seems to have been a good choice…

   More generally, I've been following Arkani-Hamed's claims that this research on amplitudes is going to replace space-time for over a decade now. At first I was impressed, but over the years it has become clear that there's a massive gap between what he actually has and the claims he likes to start his talks with, a gap which shows no signs of narrowing. He often makes very clear that he himself recognizes that what he's doing is trying to puff up his own interest in the subject in order to keep going.

   I really wish he and Baumann would start acting responsibly and stop the hype campaign, or at least stick to only getting it in places like New Scientist.

4. **Workerpleb**
   December 11, 2019

   I get the sense that articles like this are motivated in large part by research funding pressures.

5. **Peter Woit**
   December 11, 2019

   Workerpleb,
   I don't know about Baumann, but Arkani-Hamed definitely doesn't need the money. He has a $3 million prize, and a very highly paid position to do full-time research. Actually, in many of his talks he explains clearly the motivation for the hype: it's to motivate himself. Not easy to get up in the morning and do hard, tedious calculations telling yourself "if I work hard I'll find the value of the 3rd loop term in this scattering amplitude". A lot more effective motivation to tell yourself and everyone else you're doing this because it's going to kill off space and time and revolutionize physics. The problem is that others start believing you…

6. **Boyer Lindquist**
   December 11, 2019

   That Quanta article is also misleading from a historical perspective. There one reads: "As the Nobel Prize winner Steven Weinberg showed in 1964, the existence of a spin-2 particle leads inevitably to general relativity." It's true that Weinberg did this, but he wasn't the pioneer as the Quanta article seems to imply, e.g., Pauli & Fierz or even Feynman worked on the matter some time before.

7. **Akhil**
   December 12, 2019

   Peter,
   As you said Nima is clearly smart and hard-working, an inspiration to many (including myself) but your response makes a lot more sense. I wonder what will be a more productive/sensible way to motivate yourself than making over-the-top claims (as Nima admits)? How do others like Weinberg, Witten, Atiyah motivate themselves? What about pure mathematicians in general, their work is not immediately connected to physical world, forget about replacing space-time. What do they wake up saying themselves? Your thoughts Peter?

8. **DB**
   December 12, 2019

   Peter,
   thanks a lot for your comments and for answering Akhil question. I was going to ask the same.
   Arkani-Hamed is a very smart and hard working individual, no doubt about that.
   But I've got the feeling that he knows that the HEP theoretical world has found a brick wall at a dead end street.
   It was either Sabine or you who already mentioned that there were suspicions about that.
   The question is why doesn't he say so. Not sure if it's related to money, ego or professional position… or another option I can't think of. He already has enough money to live as you say, he doesn't show too much ego (at least in public appearances), and his position in the teaching sphere is totally assured, so…
   what's left?
   I think that what he's doing, imagining it would come to a successful end, is just solving technicalities, not anything that could revolutionize theoretical physics or bring in new ideas/theories, like the substitution of space/time with something else.

He was even asked once by David Gross if he thought that "God" spoke in the language of polytopes, to which he obviously answered "No".
What he's doing is not a waste of time by any standard, but something far, very far away from what he pretends to sell at his conferences.
Which is something that leaves me thinking about some ethical aspects of the whole thing.
Anyway, the key of this all is, as you say, that people believe in this kind of things.
When the amplituhedron idea came out, even Witten was very excited about it.

https://www.quantamagazine.org/physicists-discover-geometry-underlying-particle-physics-20130917/

But as time has gone by, it's clear that it is an amazing new way to calculate amplitudes
(and Nima deserves great compliments for that), but it ain't any physics revolution at all by any standard of the imagination.

I just wonder when everyone in that world will acknowledge that they're totally stuck, and that they have to go back to the blackboard, rewind two decades, and start re-thinking about all these stuff with a totally new and honest mentality.

9. **Suomynona**
December 12, 2019

Boyer Lindquist,
I believe Weinberg's achievement was showing that any QFT of a spin-2 particle must couple to all forms of matter equally, and is essentially the particle manifestation of the equivalence principle. The prior work of Fierz, Pauli, Feynman, et al., did not show such a robust intimate connection between the two concepts.

10. **Warren Siegel**
December 12, 2019

The problem with all the twistor stuff is that it's all on shell. You need that for conformal invariance. But massless on-shell loop amplitudes are infrared divergent. Any regularization (dimensional or massive or off-shell) destroys the conformal invariance. So these guys write the explicit integrands for amplitudes, but can't integrate them.

A related problem is that "on-shell states only" means there's no obvious way to introduce the iε prescription of Stückelberg-Feynman propagators, except for trees, where you can just stick them in by hand for the poles. So you get integrals where you don't know which contour to choose around the singularities.

11. **Peter Woit**
December 12, 2019

Akhil/DB,
Mathematicians can also be motivated by wanting to solve great problems. Andrew Wiles I think has said he was motivated to go into number theory by the Fermat problem, and possibly this had some influence on research directions he chose. Once he actually had a serious idea about how to solve the problem, he wasn't about to talk about it publicly.

The culture of mathematics is less tolerant of people making vague, unsupported and grandiose claims. But even in physics, behavior like that of Arkani-Hamed is quite unusual. While lots of people may be motivated by hoping to find a new way of thinking about space and time, they don't normally spend long parts of their talks going on about this. Also odd is the way he goes on about "this is the most exciting time ever to do this kind of theoretical physics". I don't doubt for a minute that he believes what he is saying, but it's not very credible, and I suspect most experts listening to him are well aware of this.

It seems likely that the current amplitudes program suffers from the same problem as the sixties version: by giving up local quantum fields and gauge symmetry you're giving up too much fundamental structure. You never were going to understand the strong interactions that way, just as I suspect you're never going to understand quantum gravity without some new fundamental idea about the symmetries of short-distance space-time degrees of freedom. One promising thing about the amplitudes program is the use of twistors and conformal symmetry. On the other hand, I don't see polytopes as a promising fundamental idea. But who knows, advances are made by those who believe in them, maybe something will come of this and it's great that some are pursuing these ideas.

12. **Boyer Lindquist**
December 12, 2019

Suomynona,
Thanks for the reply. I agree that Weinberg's approach was much clearer. But I would like to note an interesting paper on this subject, published by W. Thirring, "An alternative approach to the theory of gravitation," Annals of Physics (1961), where he also gave a field-theoretic approach to GR and concluded: "Regarding the two cornerstones of general relativity's the field theoretic approach (1) gives the equivalence principle as a result and not as a postulate (2) it replaces the general covariance principle by gauge invariance (…)".

13. **Peter Woit**
December 12, 2019

Warren Siegel,

Thanks! It's very helpful to hear not just about the positive hopes for ideas, but also about the main problems they face.

14. **Suomynona**
December 12, 2019

Boyer Lindquist,
Weinberg actually cites Thirring's paper in a related 1965 paper (PR 138 B988), and has this to say about it:

> In criticism of these articles we may say first that they generally seem to be based on specific Lagrangians, and secondly, that there does not seem to be much point in defining the spin of a field without being able to tie the definition to the physically relevant representations of the inhomogeneous Lorentz group, i.e., the one-particle states. In our work everything rests on the known transformation properties of the operators which destroy and create physical particles, and of course we make no use of the Lagrangian formalism.

Weinberg's approach in his 1964 paper (PR 135 B1049) still appears more general than Thirring's and is rooted in a quantum context (the S-matrix), whereas Thirring appears to start from classical considerations.

15. **4gravitons**
December 12, 2019

For what it's worth, I'm fairly certain none of the results the Quanta article describes invoke conformal symmetry. Aside from the mention of Nima's work on cosmological correlators with Baumann, Lee, and Pimentel, the article looks like it's mostly talking about Laurentiu Rodina's work. You can look at the conditions he imposes, they don't typically involve conformal symmetry in any crucial way (except in that they involve on-shell massless particles and thus spinor-helicity formalism, so if that's all you meant I apologize for misunderstanding you).

I agree that the headline and a lot of the language in the article is overblown, these people aren't deriving the SM gauge group or anything like that. All they're doing is looking at what sets of minimal conditions can be enough to fix tree-level amplitudes in various theories. What's interesting is that these conditions can be quite light: often a few constraints from UV behavior or soft theorems are enough to specify a class of amplitudes without invoking, for example, unitarity.

16. **Peter Woit**
December 12, 2019

4gravitons,
Thanks. I had assumed the reference to
https://arxiv.org/abs/1811.00024
indicated that the methods of that paper which used conformal symmetry were involved.

If that's not the case I'm even more mystified by the motivation for this article. The only reference to Rodina's work is to the more than 5 year old
https://journals.aps.org/prd/abstract/10.1103/PhysRevD.90.084048
which is described as an update of Weinberg. Rodina does seem to have very new results of this sort, e.g.
https://arxiv.org/abs/1910.12850
but those don't seem to be mentioned.

17. **4gravitons**
December 13, 2019

I suspect Nima was her primary contact. That would explain why she only asked Rodina about his older work with Nima, why she threw in the cosmological correlator story (which is kind of about the same thing, but a bit distant), and why she didn't mention related work by other amplitudes people like Rutger Boels. I don't think it was intended to be an article about any specific work so much as the general theme, but it is weakened a lot by not including more of the history and background. (Which by the way, your post is a good treatment of! The world needs more explainer posts!)

For what it's worth Warren's point isn't really applicable to any of the work referred to here, since it's a) all tree level and b) the one that uses conformal symmetry is for correlators and thus off-shell. It's potentially a valid criticism to the amplituhedron story but that's not really the topic at hand.

18. **Natalie Wolchover**
December 13, 2019

Peter,
The headline, I grant you, is overblown. Writers don't get to choose headlines. But as for the article, some people seem to be reading into it things it never says and ignoring things it does say. I would wager that these readers know too much for this article, and don't have a sense of what lay readers know and don't know. This article is for them — for people who have no clue about the scheme that nature's building blocks fit into, people who think "photons" and "gluons" are like animals you might come across in a zoo that can look and act any possible way. There is an astonishing aspect to fundamental physics that these folks are missing out on, and excuse me for daring to try to explain it to them.

The article does not say that this is the only possible way the universe could be; in fact, it discusses some of the instances where the universe has "creative license," as I put it — e.g. with massive spin-½ particles and spin-0 particles, where there are very few constraints. But in other instances, the options are incredibly constrained — so much more than most people appreciate. So rather than getting upset thinking I'm making some kind of anti-empirical claim, maybe just step back and try to remind yourself how interesting it is — how amazing it must have seemed to you when you learned about it in grad school — that a massless spin-2 particle must uniquely be the graviton. That the spin possibilities are so limited. Etcetera. The person on the street doesn't know about these things, and it's worth telling them.

If you're interested, the references that I was most often pointed to (aside from the classic Weinberg paper) are:
Benincasa and Cachazo (2008) https://arxiv.org/abs/0705.4305
Schuster and Toro (2008) http://arxiv.org/abs/0811.3207
McGady and Rodina (2014) http://arxiv.org/abs/1311.2938
Best,
Natalie

19. **Peter Woit**
December 13, 2019

Natalie,

I don't at all have a problem with the way you were trying to explain remarkable facts about constraints on theories of massless particles. This is a really tricky business and note that in my own attempt here to write something, even for a more sophisticated audience, I basically gave up. I was hoping to say more than what's in the next to last paragraph, but after thinking about it for a while decided it was just too hard to say something both true and insightful about this topic, even assuming a lot of background.

I also don't have a problem with the "are the laws of physics inevitable?" framing or the headline of your piece. I used this kind of title for what I wrote also, to get attention and be provocative (seems to have worked, this posting is getting more attention than usual). Sabine Hossenfelder makes the obvious accurate counter-argument about what is wrong with this, but it remains true that it's remarkable what you can say based on a couple deep ideas and consistency, without even looking at the real world.

The huge problem here is the sub-headline. It's just completely wrong, basically everything about it. And probably an order of magnitude more people are going to read it than read the whole piece. Over the years I've seen this time and time again in science journalism: pretty good article, which goes up to the line of being wrong, to make a point, but doesn't cross it. Whoever writes headlines thinks it's their job to jazz things up and get attention, so they happily cross the line and write something completely wrong. I don't know what happened here or what Quanta's procedures are, but it would be a good idea to think through whether you can do better than others and avoid this kind of thing.

Honestly, if this sub-headline had appeared on an article a most other places I would probably have just ignored it, figured it best to not give it more attention. A lot of the complaints you're getting here are due to the fact that people have a very high opinion of your work and that of others at Quanta. Keep up the good work, but think about whether you can do better with the headlines/sub-headlines!

Peter

20. **Squarkino**

December 14, 2019

In https://arxiv.org/abs/1709.04891 , Arkani-Hamed spoke of ‘deriving string theory’ from first principles. How much progress has been made on that front?

21. **Peter Woit**
December 14, 2019

Squarkino,
While the idea of “deriving” string theory from some constraints on amplitudes sometimes gets mentioned, that doesn’t seem to actually have anything to do with the work you link to or other recent amplitudes work discussed in Quanta articles. The questions about string theory amplitudes have to do with an infinite tower of massive states and short distance behavior, most of the new amplitudes methods are for theories of massless particles.

It’s remarkable the extent to which currently fashionable research on quantum gravity (for instance Arkani-Hamed’s idea that one can get rid of space and time using things like the amplituhedron or “it from qubit” ideas for emergent gravity) has little or nothing to do with string theory.

22. **Natalie Wolchover**
December 14, 2019

Peter,
Fine, you didn’t like the subhed. It’s hard to summarize these ideas in an appealing sentence. Does that critique really justify calling this piece “ideologically driven nonsense”? Why be so overly critical? If you think this stuff is indeed remarkable and worth explaining, why not say so in your original post — with the caveat that you don’t like the display copy — rather than calling it nonsense and insinuating that I’ve been taken for a ride by ideologues? It’s unkind and untrue, and unfortunately the clarifications in your comment will be seen by virtually no one.
Natalie

23. **Peter Woit**
December 14, 2019

Natalie,

My apologies for not making it clear that I was mostly talking about the subhed. In the main text though, the paragraph which expands the subhed
“Thus, by thinking through the constraints placed on fundamental particle interactions by basic symmetries, physicists can understand the existence of the strong and weak forces that shape atoms, and the forces of electromagnetism and gravity that sculpt the universe at large.”
has the same problem (the bootstrappers have zero to say about internal symmetries and thus the existence of the SM forces). I’ll add something to the end of the posting pointing to our discussion here.

The problem with the subhed (or that paragraph I quoted) isn’t that it’s not to my taste. Note that, the day before I wrote anything, both John Baez and Will Kinney found this so problematic and remarkable that they wrote Twitter threads about it (there was also the more general Sabine Hossenfelder criticism). I think any physicist who knows anything about this subject and read the subhed probably had the same reaction: why is such a nonsensical claim appearing in Quanta, which normally is quite reliable? My posting was an attempt to provide some background for anyone asking themselves that question, in both the positive sense of pointing to important technical results and in the negative sense of pointing to the motivation behind over-the-top claims from theorists.

Apologies also for the overly vociferous tone of some of the posting, which was aimed not at you, but at the theorists involved. I don’t know Baumann at all, but I don’t understand why any theorist would think it was all right to tell the public that he and his colleagues have shown that “There’s just no freedom in the laws of physics that we have.” The field of high energy physics has suffered greatly from overblown claims by theorists. After 35 years of this, I’m getting both old and testy…

24. **Jack Morava**
December 14, 2019

In general I am a fan of Natalie Wolchover’s work but I think it’s unfair to describe Peter’s remarks as overly critical. Extraordinary claims deserve extraordinary evidence, and one would like to believe that `there is some inevitability of the laws of physics that can be summarized by a short handful of principles that then lead to building blocks that then build up the macroscopic world’. In that case, though, it would be nice to see that short handful of principles.

25. **Jackiw Teitelboim**
December 14, 2019

Dear Natalie,

I believe your article succeeds well in explaining the notion of a particle’s spin in a quite accessible way, and its role in determining their possible interactions. It’s not quite easy to explain these things to lay-readers, and sometimes even to our students!

About what people called “hype,” given that lay-readers will always be impressed with quotations from Einstein, a possibility instead of stating ambitious nonetheless unsubstantiated claims would be to cite remark from Einstein’s Autobiographical Notes, particularly the last sentence (but I copy the whole paragraph for completeness):

There is something else that I have learned from the theory of gravitation: No collection of empirical facts, however comprehensive, can ever lead to the setting up of such complicated equations. A theory can be tested by experience, but there is no way from experience to the formulation of a theory. Equations of such complexity as are the equations of the gravitational field can be found only through the discovery of a logically simple mathematical condition which determines the equations completely or (at least) almost completely. Once one has those sufficiently strong formal conditions, one requires only little knowledge of facts for the setting up of a theory; in the case of the equations of gravitation it is the four-dimensionality and the symmetric tensor as expression for the structure of space which, together with the invariance concerning the continuous transformation-group, determine the equation almost completely.
—A. Einstein (Autobiographical Notes, 1949)

Interesting to note that, as you last article at Quanta says that “Einstein arrived at general relativity through abstract thoughts about falling elevators and warped space and time,” you see that Einstein himself in his later life understood quite well how the symmetries of the theory – “continuous transformation-group” – plays a fundamental role.

26. **bertie**
December 15, 2019

I feel quite sorry for scientific journalists covering HEP, after all, what is there to write about that is ‘news’, peraps a little adventurism with headings is forgivable?

27. **Peter Woit**
December 15, 2019

Jackiw Teitelboim,
What the Einstein quote is referring to (diffeomorphism invariance) and what the amplitudes papers that Natalie Wolchover links to are doing (adding constraints from locality and unitarity to the usual Poincaré group action constraints) are two completely different things. One of the main motivations of the whole amplitudes program is to formulate the theory in a way that gets rid of the action of local symmetries. I recall that in one talk Arkani-Hamed described gauge symmetries with the technical term "crap". The argument is that local field theory with an infinite-dimensional group of local symmetries is a bad starting point, since it introduces a huge amount of redundancy in the description of nature.

This is also part of my objection to the text I quoted from the Quanta piece, which seems to imply that what's different about the work she is discussing involves "the constraints placed on fundamental particle interactions by basic symmetries", which is kind of backwards.

28. **DrDave**
December 15, 2019

Natalie, I guess as an editor I would simply correct the highly visible line at the top (with a correction note) "physicists working on the "bootstrap" have rederived the four known forces" as it isn't true. One can argue about the imaginary layperson, and attention grabbing headlines, and so on, one can fob it off on the editor. But, at the end of the day, it isn't true and it has your name on it. So that's a really good reason, as a journalist, to correct it. One of the jobs of a journalist is to fix stuff like this. If you don't, it implies that you have seen the evidence that it is not true and don't agree with it somehow.

29. **John Baez**
December 15, 2019

It's pretty strange that the title and subhead of an article – the only parts most people actually read – should be left to crazed hypesters unrestrained by sense of honesty. Imagine that the *New York Times* had that policy. Would anyone take it seriously?

But it's not just that the title and subheading are completely wrong. The quotes of Daniel Baumann in the article are also misleading and make grandiose claims that he did not intend.

On Twitter I wrote:

> There's an absolutely wild quote by Daniel Baumann: "There's just no freedom in the laws of physics that we have." If he's shown this, then he's solved all the biggest questions in fundamental physics and he deserves the next 20 Nobel Prizes. But he hasn't.

Daniel Baumann replied:

> I agree its very unfortunate. I spoke in a precise context: that the long-distance interactions of a massless spin-2 particle are those of GR. Everything here is in the context of massless particles. As you said, this is old stuff. I didn't not mean to imply more. I was just explaining the technical arguments. Didn't mean to hype anything. Apologies.

I replied:

> Didn't Natalie read you your quotes? When I've been interviewed for *Quanta* articles they've been pretty good at doing that, which allowed me to fix things. They even had a "fact-checker" for this purpose.
>
> Whoever writes the headlines, however, seems beyond supervision.

Daniel Baumann replied:

> No, not this time. Until this morning, I didn't even know the post had appeared.

I told him (and everyone) to be very careful when talking to reporters. They often take the most grandiose, wacky thing you say and quote it out of context to make their article more exciting. Something that makes sense in the midst of a longer conversation can make you look like an absolute idiot when taken by itself.

Daniel Baumann does *not* believe that "There's just no freedom in the laws of physics that we have."

30. **Peter Woit**
December 15, 2019

John Baez,
Thanks. This is odd though. In my experience Wolchover is one of the more careful science journalists around, and the problem here is not just the headline. Also in my experience, the reason bogus claims about fundamental theory get into popular articles is that theorists are all too willing to drop caveats and mix together their hopes for their work with what they actually have.

Does Baumann also disavow the longer quote from the end of the article:

"It's "just aesthetically pleasing," Baumann said, "that the laws are inevitable — that there is some inevitability of the laws of physics that can be summarized by a short handful of principles that then lead to building blocks that then build up the macroscopic world."

Other examples of this kind of misleading language can be found, see for instance the webpage for a recent Simons Foundation conference (Arkani-Hamed and Baumann two of the organizers)
https://www.simonsfoundation.org/event/amplitudes-meet-cosmology-2019/
which tells us
"the laws of physics are a nearly inevitable consequence of quantum mechanics and special relativity. Nowhere is this seen more clearly than in the invariant observables associated with scattering amplitudes in asymptotically flat space."
You can find lots of Arkani-Hamed talks that very much seem to be saying that QM + special relativity inevitably give something like the Standard Model, as one random example, watch this IAS public talk:
https://video.ias.edu/arkani-hamed-lecture-10-12

31. **JE**
December 15, 2019

Sorry, Peter, if this sounds presumptuous. Although I think you are making a good point and I very much agree with what you wrote re Quanta's article, the whole discussion sounds a little bit over the top, especially after reading John's comment above. IMHO, as other commenters have suggested, this article and the issues it raises with the title, subheading, etc. may just reflect the precarious state of HEP and the lack of good stories to write about in the HEP field.

32. **Peter Woit**
December 15, 2019

JE,
One point of view on why HEP is in a precarious state would be because of the damage caused by 35 years of misleading hype. From that point of view, trying to stop more of it might not be over the top…

33. **John Baez**
December 16, 2019

Baumann didn't explicitly "disavow" other quotes from his article, but I'm not in the business of extracting disavowals.

I agree that Natalie Wolchover is one of the best physics reporters out there. I wouldn't have bothered talking about this otherwise.

My goal is not to rake anyone over the coals. But I did want to make a bit of a stink. I think it's the duty of all scientists to push back against nonsense.

34. **Natalie Wolchover**
December 16, 2019

Hi Peter (and John and others),
We've changed the wording of the subhead and another sentence in the blog post and added a correction note at the bottom explaining the changes.

As for Daniel Baumann, first I'll just say that he and other bootstrappers are careful and conservative; in fact that's kind of their whole thing! From a journalistic point of view, the quote that has so bothered some of you is a great quote. It's passionate and reveals his feeling of being highly constrained by the rules, a la Mel Gibson as William Wallace yelling "FREEDOM!" It's an exaggerated statement to which it is indeed possible to say, "Well, technically, not NO freedom…" The article tries to explain some of these technicalities. I've apologized to Daniel for causing him some grief over the quote.

Thanks for keeping us on our toes, thanks for the apology, and I apologize to you all as well for not vetting the display copy well enough.

Natalie

35. **Daniel Baumann**
December 16, 2019

Dear Peter et al.,

I have never commented on a blog before, but since my words seem to be misinterpreted, maybe it would be helpful if I clarify the context of my remarks.

Natalie asked me to explain the following two papers:

Benincasa and Cachazo (2008) https://arxiv.org/abs/0705.4305
McGady and Rodina (2014) http://arxiv.org/abs/1311.2938

I was happy to do so, since I like these papers a lot and was impressed that Natalie would attempt to describe such a complex subject. My short (popular) summary of the papers can be found here:
https://www.dropbox.com/s/50tvt2odrlgwkgc/Quanta.pdf?dl=0

As you can see, it is only about massless particles and long-range forces. Everything I said in the article is in that context. In fact, we spoke for a long time and it was mostly a technical discussion on the details of these papers.
That locality and unitarity fixes the long-distance interactions of massless spin-1 and spin-2 particles to be those of YM theory and GR is a beautiful fact that I tried my best to explain.

Best,
Daniel

36. **Peter Woit**
December 16, 2019

Hi Natalie and Daniel,

Thanks a lot for the clarifications, that's great! I've added a note about this at the end of the posting.

Peter

37. **Low Math, Meekly Interacting**
December 18, 2019

I think I've witnessed something remarkable and encouraging happening here, and I thank all involved for finding a constructive conclusion.

Natalie: I generally am very happy with Quanta's reporting on physics and mathematics, and consider your work to be a standout in the field of popular science reporting. Any disappointment comes from the fact that you and your colleagues have made it worth it to even care. I am grateful for that, and also wish to add my encouragement. Please do keep up the good work. A quick perusal of the "industry standard" of online pop-sci content make it clear how sorely it's needed.

38. **DK**
December 19, 2019

Just a suggestion: For all the blog postings, perhaps the "Updates" can be added at the very top rather than at the bottom, appropriately delineated? Will improve readability.

39. **tulpoeid**
December 21, 2019

I am afraid I will act kind of like a party spoiler. I hesitated doing so but I keep thinking that the view of those who don't enjoy Quanta magazine could be a useful addition here.

So, being a physicist, a researcher, and a reader of popular science press, a few months into reading Quanta articles I had to firmly decide that I stop doing so (the couple of times I went back reaffirmed my decision). I find them too long and too imbalanced – they are trying to both water down the subject and speak to quasi-experts at the same time, missing the objective of having a clear target group. In the best cases, I couldn't get any new knowledge on the promised topic because it is either buried in a deep and convoluted way or it wasn't really there to begin with. This is probably a result of heavy editting and of very specific guidelines, and I say so because even Hossenfelder's clear writing becomes Quanta-ized over there.

In the worst cases, it is sheer propaganda. I realize that steps were taken thanks to this post and the discussion under it. But at the same time I wonder how good, or at least how objective, it is to congratulate Wolchover. As far as I can tell her articles have consistently been eulogies of fake physics for a few years now and in the end it will always be "I was only covering". I don't know whether she is one of the top science journalists right now, but I am almost sure that she has contributed to building the inaccurate picture that the public mistakes for proven physics facts lately.

To wrap up, correcting one's article is certainly commentable. But I can't help thinking that it is an integral part of journalistic work. Again, I am aware that all this will probably sound a bit off, however the urge to bring into discussion the fact that not every HEPer looks up to Quanta has been surging in me for ten days now…

40. **Peter Woit**
December 21, 2019

tulpoeid,
First of all, I think you need to compare what Quanta is doing to what other outlets for science journalism (such as Scientific American) are doing, and I think they come off very well in that comparison.
Secondly, covering HEP theory has very special problems. The material is difficult for even experts to understand, and if journalists try and deal with this by consulting the most prominent people in the field, they have a thorny thicket of BS to try to find their way through. I don't think you can blame the results on the journalists. As a point of comparison, consider Physics Today, which is the house organ of the US physics community. The main person over the years they have turned to for coverage of HEP BSM physics has been Gordon Kane (no less than three articles from him). So, it could be much worse…

41. **Severin Pappadeux**
December 23, 2019

My first read on spin-2 particle leading inevitably to general relativity was from Feynman Lectures on Gravitation, given at Caltech 1962

42. **Cosmin**
December 27, 2019

You are actually talking about a very old dilemma: who is to blame, the physicists or the editors/gatekeepers. Now everyone wants to be in the active physicists field (99%) but very few (read almost nobody – 1%) wants to do the editing/gatekeeping work, for various reasons. The result is that strânge ideas get a pass simply because nobody is there to stop them. Everyone wants to play but nobody wants to be the referee.

43. **Peter Woit**
December 27, 2019

Cosmin,

The problem of gate-keeping/refereeing within the physics community is a different one than the problem here, which is that of gate-keeping between the public and theorists with an idea to promote. This is what journalists have to do, and in my experience they're enthusiastic and willing to work hard at it to try and get it right. But, especially for highly technical theoretical work (a good example would be no-go theorems, where the issue of the significance of various technical assumptions is at the center of the problem) there's no way they can evaluate the significance of the work themselves. They have to rely on the self-control of the theorists themselves, their willingness to be careful and not overstate their case, as well as the willingness of their colleagues to take them to task when they go too far.

44. **Sebastian Thaler**
December 31, 2019

Frank Wilczek on Twitter today: "It's time to concede that this was not, alas, the decade of supersymmetry. Hope springs eternal, but the terms of bets are finite … I've got two to pay off."

45. **Peter Woit**
December 31, 2019

Sebastian Thaler,

I did see that, at
https://twitter.com/FrankWilczek/status/1211973691644489728

The remarkable thing about all those who have lost SUSY bets is that I've never seen any of them draw the conclusion that there is something wrong with the idea. Wilczek's loss of a bet on SUSY leads him to conclude "this was not, alas, the decade of supersymmetry", but that "hope springs eternal".

London Calling with Career Opportunities

January 2, 2020
Categories: Uncategorized

At some point within the past couple years I noticed that one blog that had Not Even Wrong on its blogroll was the blog of Dominic Cummings, who was often getting credited with masterminding the political campaign that got the British to vote (narrowly) for Brexit in 2016. Cummings has had further success recently as Chief Special Adviser to British Prime Minister Boris Johnson, with a blow-out election victory three weeks ago putting him securely in control of the British state.

Today on his blog Cummings has, invoking Grothendieck, posted a job advertisement: ‘Two hands are a lot’ — we’re hiring data scientists, project managers, policy experts, assorted weirdos…. He’s looking for mathematicians, physicists and others to join him to change British society, working

> in the intersection of:
>
> - the selection, education and training of people for high performance
> - the frontiers of the science of prediction
> - data science, AI and cognitive technologies (e.g Seeing Rooms, ‘authoring tools designed for arguing from evidence’, Tetlock/IARPA prediction tournaments that could easily be extended to consider ‘clusters’ of issues around themes like Brexit to improve policy and project management)
> - communication (e.g Cialdini)
> - decision-making institutions at the apex of government.

For some other descriptions of who Cummings would like to hire, on the economics side there’s:

> The ideal candidate might, for example, have a degree in maths and economics, worked at the LHC in one summer, worked with a quant fund another summer, and written software for a YC startup in a third summer!
>
> We’ve found one of these but want at least one more.

He also wants “Super-talented weirdos”, with examples given from William Gibson novels, such as “that Chinese-Cuban free runner from a crime family hired by the KGB.”

The remarkable things to me about this long document are what it doesn’t contain. In particular I see nothing at all about any specific policy goals. Usually a new government would recruit people by appealing to their desire to make the world a better place in some specific way, but there’s nothing about that here. The goal is to control the government and what the British population believes, but to what end?

In addition, a more conventional hiring process would be asking for candidates of high ethical values, with some devotion to telling the truth. Cummings seems to be asking for exactly the opposite: best if your background is “from a crime family hired by the KGB.”

Best of wishes to my British readers, now joining the US and other nations in a new dystopic post-truth era. It’s massively depressing to me to see how this has worked out here, I hope you do better. Maybe you should be sending in your applications to Cummings and hoping to sign up for a role in the new power structure. If so, tell him “Not Even Wrong” sent you…

Update: For more on Cummings, there’s a good Financial Times article.

Comments

1. **chorasimilarity**
   January 2, 2020

   Seems like he wants to form a team like the one tasked with the detonation of the bridge over the river Kwai. Strange enough that the wikipedia page [1] does not say a word about this team, although the main part of the book is about the culture war between those who want to destroy the bridge and those who want to build it as an example for the primitives.

   [1] https://en.wikipedia.org/wiki/The_Bridge_over_the_River_Kwai

2. **Peter Woit**
   January 2, 2020

   chorasimilarity,
   Demolition is definitely on the agenda for Cummings. The main target to be demolished is likely those aspects of the “establishment” that stand in the way of total control. Here in the US we’ve seen how this works, as the Republican party and mass media (Fox) have been forced into submission to the Trump personality cult. In the case of Trump what has become clear is that he has no interests beyond narcissistic ones. Cummings/Johnson seem to want to destroy the establishment and take total power, with no more of an idea than Trump of what to do with this power, other than to gloat at the defeat of their enemies.

   Comments encouraged from those in Britain who know much more about this than I do, discouraged from those who want to discuss something other than exactly what Cummings is up to.

3. **Bill Anderson**
   January 2, 2020

   You make some very good points!

   Cummings carries a lot of power and authority without any direct connection to the electorate. There is an irony as this is one of the EU’s major flaws from the Brexiteers’ perspective. But, of course, this logical consistency doesn’t apply to Dom.

   Unelected, unaccountable and with no apparent remit or brief he is perceived by many to be dangerous. Given Boris J’s history of inattention to detail and erratic judgements there is potential for calamity.

   However, our Civil Service can be less than cooperative and Cummings has not shown long term commitment in his career moves thus far. The next 2-3 years will be difficult because of the complexity and enormity of the Brexit process so I’m hoping he’ll bail!

4. **Peter Woit**
January 2, 2020

By the way, I see that Cummings even commented on the blog once, see here
https://www.math.columbia.edu/~woit/wordpress/?p=755&cpage=1#comment-42623
Unfortunately I had no answer to his question, so didn't respond.

5. **gjz**
January 2, 2020

This is what Cummings was looking for:
https://cms-docdb.cern.ch/cgi-bin/PublicDocDB/ShowDocument?docid=4039

6. **Peter Woit**
January 2, 2020

gjz,
Thanks! I hope he's still reading the blog and can take advantage of that!

7. **René Pannekoek**
January 3, 2020

Just because he does not ask for people who "want to make the world a better place" does not mean he's planning any dystopian nightmares. His advertisement seems to me to be targeted towards people who want to help make the British state function more efficiently and rationally.

It is very easy to declare that you are the kind of person who wants to make the world a better place, and it can be done at zero cost. But among the people who say such corny things are a lot of hypocrites, naive folks, and folks who really do not know themselves all that well. (An example of that last category would be many participants in online mobs who think they are "making the world a better place" by hating on some poor sod who made an inappropriate joke, or who failed to use someone's preferred pronoun.)

Saying you want to make the world a better place does not make it so. Just like not saying it does not mean you are a bad person. Personally, I've really had my fill of all the virtue-signalling out there, and I'll be looking for how a person acts, rather than how a person advertises him- or herself.

8. **tulpoeid**
January 3, 2020

Gibson mentioned in context in a proper physics blog.
I already feel more complete this year.

9. **ArshadM**
January 3, 2020

The problem with the British civil service is that it's perceived as being slow, lethargic and ineffective. I can't recall a single project in the last 2 decades that wasn't grossly over budget or delivered on the original aims.

Don't get me wrong, the entry process into the service is extremely thorough and the people are highly talented. But the career progression in the service means that it's quite easy for people to move around from job to job and not be held to account.

There has been some discussion of an op-ed that Rachel Wolf wrote in the Telegraph (behind a paywall unfortunately), but a summary can be found here – https://www.independent.co.uk/news/uk/politics/dominic-cummings-civil-service-boris-johnson-reform-whitehall-a9267101.html.

Every government seems to try these grandiose types of changes, usually with limited success.

10. **Winston Smith**
January 3, 2020

René,

I read Peter's "desire to make the world a better place" as a synonym for "policy preferences". Most people have policy preferences that align with what they believe will make the world a better place, though there certainly seem to be some striking counterexamples.

You write "and I'll be looking for how a person acts"; I agree, but Peter's point is that the ad gives no indication about what kinds of "acts" will be undertaken. One example (of many possible) might be: do we plan to cut or increase funding to NHS? It is reasonable for an applicant to such a position to have a general idea of the ends to which he or she will be working. As Peter writes, it seems this question is rather explicitly beside the point; the point is simply to have the power, with unclear ends.

You mention "efficiency" which most people favor in the abstract, but only to the extent that it is a euphemism for "more money for people like me and less for everyone else".

11. **Peter Woit**
January 3, 2020

Rene Pannekoek,
I'm no more of a fan of virtue signaling than you are. You're ignoring my main point, which is that Cummings is asking for people to come to work for him and use their talents to gain control of the British state apparatus and manipulate the public, without any indication of what positive outcome he hopes to achieve.

In other times I might react to seeing this by giving him the benefit of the doubt and wishing him well, but we've now been watching this same TV show for several years in the US. We know how it turns out: looting by the already wealthy and powerful, and a new normal of non-stop lying and deception, smashing of democratic institutions and norms, ripping of societies apart, all in the service of keeping in power a narcissistic sociopath. I do hope it goes differently in Britain, but anyone who decides to go work for Cummings should expect that this is likely what they are signing up for.

12. **Allan Greenleaf**
January 3, 2020

Christopher Wylie's recently released book, Mindf*ck, on the rise and fall of Cambridge Analytica (in which Mr. Cummings makes several cameos) is a good cautionary tale on doing value-free data science.

13. **Mark**
    January 3, 2020

    "Just because he does not ask for people who "want to make the world a better place" does not mean he's planning any dystopian nightmares."

    Wasn't he very involved in the data-science scandal with Cambridge Analytica, which was a significant factor in getting people to vote for Brexit which has led to the nightmare the UK is now in? Not sure giving him more smart people is in the countries interests.

14. **Peter Woit**
    January 3, 2020

    René/Mark,
    The problem with Cummings is not that he has a secret plan for a new dystopia, it's that we're now living in one that he has helped create. This is someone who actually wants to live in a William Gibson novel. He has realized his dreams, shows no signs of having any other dreams than enjoying holding the power he always wanted and making sure he keeps it by running a Ministry of Truth staffed by "misfit" data scientists.

15. **Neil**
    January 3, 2020

    Hi Peter
    .
    All very interesting.
    .
    After reading the DC blog and then your own, my first instinct was to be objective in relation to your point about what was left out, and then 'clarified' by you in the comments section:
    .
    "You're ignoring my main point, which is that Cummings is asking for people to come to work for him and use their talents to gain control of the British state apparatus and manipulate the public, without any indication of what positive outcome he hopes to achieve."
    .
    I then thought about which manifesto pledges the Conservative Party was just elected on. Aside from 'Get Brexit Done', here are the top line policy "guarantees":
    .
    https://vote.conservatives.com/our-plan
    .
    For more detail, the full 64 Manifesto is here:
    .
    https://assets-global.website-files.com/5da42e2cae7ebd3f8bde353c/5dda924905da587992a064ba_Conservative%202019%20Manifesto.pdf
    .
    So, if those are the policy directions (positve or negative, depending on who you are), perhaps DC believes these goals cannot be met using the approaches of times gone by.
    .
    I hope this is useful.
    .
    Best from Belfast,
    Neil

16. **Peter Woit**
    January 3, 2020

    Neil,
    There is nothing in what Cummings writes pointing to any of that, for instance nothing about how the skills he's looking for will be used to hire 50,000 more nurses.

    Again, maybe Cummings is just a misunderstood guy intensely devoted to improving the world by hiring more doctors and nurses and investing in education and infrastructure. We'll see, but at this point I see no reason to believe that, and anyone thinking of taking a job with him should be asking him some tough questions. I do hope I'm wrong.

17. **CWJ**
    January 3, 2020

    Unsurprising:

    https://www.theguardian.com/politics/2020/jan/03/dominic-cummings-call-for-no-10-staff-may-break-employment-law

18. **Peter Woit**
    January 3, 2020

    CWJ,
    I don't think the problem with what Cummings is doing is that he's not following conventional hiring rules. Criticism of him for that just helps him, allowing him to pose as a rebel against the hidebound establishment and its rules obstructing progress. The press should instead be trying to find out what he plans to do with the people he's hiring and reporting on that.

19. **Art**
    January 3, 2020

    Per Wikipedia, Robert Cialdini, "the Regents' Professor Emeritus of Psychology and Marketing at Arizona State University", "is best known for his 1984 book on persuasion and marketing, Influence: The Psychology of Persuasion."

20. **Mike Greig**
    January 3, 2020

    Peter

The UK government machinery is very different to the USA's. There is a supposedly independent and impartial civil service which carries out the wishes of whichever Government is in power. In practice it is run by humanities graduates from Oxford or Cambridge universities who are very clever generalists. They have a track record of frustrating the implementation of policies that the electorate voted for.
Below is another quote from Cummings's blog addressing this aspect:
"People in SW1 talk a lot about 'diversity' but they rarely mean 'true cognitive diversity'. They are usually babbling about 'gender identity diversity blah blah'. What SW1 needs is not more drivel about 'identity' and 'diversity' from Oxbridge humanities graduates but more genuine cognitive diversity."
Cummings has a lot of experience of the way the civil service operates. He was special adviser to Michael Gove, the Secretary of State for education from 2010 to 2014. Amongst other policies Gove introduced was Free Schools (somewhat similar to US Charter schools) where I have some personal experience. Dominic Cummings quite famously attributed the quote below to a civil servant in the Department for Education talking to Michael Gove's team in 2014.
'You're a mutant virus, I'm the immune system and its my job to expel you from the organism.'
Whether or not the current civil service intends to operate in quite that way, they are certainly huge defenders of the status quo and opponents of change.

21. **Peter Woit**
January 3, 2020

Mike Greig,
According to Wikipedia, Cummings is a public school/Oxford graduate with a history degree, and "clever generalist" seems to fit him, so he's not adding much diversity on that front.

One thing we've learned here in the US from Trumpism is that the royal road to political success is to stoke resentments, convincing your supporters that the world is full of "enemies" that need to be destroyed. Here these days it's the "Deep State". If your enemies have elite university degrees, so much the better for the resentment thing (to get people to ignore the fact that your own background is the same is where you need a real pro like Cummings).

Every politician everywhere in the world complains about "government bureaucrats", that they're the ones to blame for their inability to fulfill their campaign promises. It looks to me like Cummings is going to war with the British Civil Service not because that's the way to get 50,000 nurses hired, but because when they're not hired, he'll have a good scapegoat to blame it on. Again, I very much hope to be wrong.

22. **uhoh**
January 3, 2020

If you want to get an idea of Cummings's priorities, you might try this talk from 2014:

https://www.youtube.com/watch?v=GNaWPV5l4j4

After watching that and reading his blog a bit, it seems to me that his primary goal is to eliminate the (perceived) dysfunction in the British government and that a necessary first step is to replace all the mandarins with people from math/physics and the private world who know what it means to solve a problem, as he sees it. (Full disclosure, the idea of a technocratic government run by mathematicians has some appeal to my vanity.) Normal policy matters like Brexit and taxes going this way or that seem to be secondary concerns to him.

If this is the case, then it makes perfect sense why he wrote the ad on his web page that way.

23. **Roger Prentice**
January 4, 2020

Why would Cummings use this (already long) blog post to list the government's policy objectives when they had already been stated in the Queen's Speech? Here they are:
https://www.gov.uk/government/speeches/queens-speech-december-2019
It's not very dystopian.

Peter claims that Cummings is "not adding much diversity" because he himself is an Oxbridge graduate with a history degree, but that's the whole point. He's wanting to hire people "smarted than me". He has identified his own limitations and seeks to overcome them by hiring different sorts of people.

I'm actually encouraged by the prospect of bringing some data science into the business of government and policy creation. As an analogy, I think there are currently too many humanities degrees sitting around thinking "it must be the multiverse".

24. **Peter Woit**
January 4, 2020

uhoh/Roger Prentice,

So far what Cummings and others like him have accomplished using AI and data science techniques has been to use them to lie and manipulate the public, wrecking democracy, bringing Trump and others like him to power, and inflicting the Brexit disaster on the British. I'm getting old and curmudgeonly so maybe you shouldn't listen to me, but I've seen a lot that's very disturbing and little good coming out of the the use of AI and data science, especially in the political sphere.

A lot of people on the other hand seem convinced that Cummings is just a modest, well-meaning guy, devoted to good government, which will consist of math and physics phds using data science and AI to hire those 50,000 nurses. I hope they're right.

25. **Patrick**
January 4, 2020

Those were some great Clash songs.

26. **Roger Prentice**
January 5, 2020

Oh, just one more thing, as Columbo might say. Here's a view on (the lack of) data science in government by statistician Graeme Archer:
https://unherd.com/2020/01/why-we-need-weirdos-like-dominic-cummings/

It's worth clicking through just to see the photo of Dominic Cummings looking like, as I read elsewhere "the owner-operator of a dangerous fairground ride arriving at his own negligence trial".

27. **Andrew**
January 5, 2020

Peter,

I fear you're getting the wrong end of the stick here and are too quick to make simplistic comparisons between UK and US politics. Cummings goals are clear and are in my view well intentioned – he wants a more data-driven/scientifically based civil service and to make the UK the global home for education and science. He wrote a 237 page essay on how to improve education here: https://dominiccummings.com/the-odyssean-project-2/ . Its worth reading this from Steve Hsu (a personal friend of Cummings it seems): https://infoproc.blogspot.com/2019/12/now-it-can-be-told-dominic-cummings-and.html

"What does he want? Why is he doing this? Not for money, not for fame. For love of country and human progress and civilization. Dom's dream is to make the UK a global center for science, technology, and education"

P.S with regards brexit, as quick as many are too dismiss it (and for what its worth I voted remain) it may be worth your time hearing David Deutsch explaining his support for it https://www.youtube.com/watch?v=xdtssXITXuE – it is again simple to just view it as "Trumpian", "populist" etc.

28. **Peter Woit**
January 5, 2020

Andrew,
I'm very familiar with Steve Hsu and his (highly Trumpian) views, which I find appalling. That he's close to Cummings seems to me yet another reason to believe that having Cummings anywhere near the levers of power is not going to lead to anything good.

I think I've now given sufficient voice here to the "Dominic Cummings is just a well-meaning, selfless sort who wants to use science to make the world a better place" argument, as well as explaining why I don't believe it, so I'm shutting off comments here. Again, I'll be very happy if I'm wrong.

Musings on the Current Status of HEP

January 4, 2020
Categories: Uncategorized

To start the new decade there's an article very much worth reading by Misha Shifman, entitled Musings on the Current State of HEP. It's somewhat of an update of something he wrote back in 2012, which I wrote about here. He starts off with:

> Now, seven years later, I will risk to offer my musings on the same subject.The seven years that have elapsed since brought new perspectives: the tendencies which were rather foggy at that time became pronounced. My humble musings do not pretend to be more than they are: just a personal opinion of a theoretical physicist… For obvious reasons I will focus mostly on HEP, making a few marginal remarks on related areas. I would say that the most important message we have received is the absence of dramatic or surprising new results. In HEP no significant experimental findings were reported, old ideas concerning Beyond the Standard Model (BSM) physics hit dead-ends one after another and were not replaced by novel ideas. Hopes for key discoveries at the LHC (such as superpartners) which I mentioned in 2012 are fading away. Some may even say that these hopes are already dead. Low energy supersymmetry is ruled out, and gone with it is the concept of naturalness, a basic principle which theorists cherished and followed for decades. Nothing has replaced it so far…
>
> HEP, "my" branch of theoretical physics since the beginning of my career, seems to be shrinking. A change of priorities in HEP in the near future is likely as business as usual is not sustainable. The current time is formative.

I encourage you to take a look at the rest, there's a lot more detailed discussion of the state of HEP and allied fields, especially about the central role of quantum field theory.

Shifman also includes a section very critical of Richard Dawid, the "non-empirical confirmation" business and talks given at the "Why Trust a Theory?" conference (discussed here):

> With all due respect I strongly disagree with Richard Dawid and all supporting speakers at the conference and beyond… I object against applying the term "non-empirically confirmed" to science (the more so, the term "postempiric science"). Of course, we live in liberal times and everybody is entitled to study and discuss whatever he or she wants. But the word science is already taken. Sorry, colleagues. For "postempiric science," please, use another word, for instance, iScience, xScience, or something else.

As for David Gross's attempt to claim that string theory is, like quantum mechanics and quantum field theory, not testable just because it is a framework, not a theory, Shifman is having none of it:

> David Gross is a great theoretical physicist, whose discovery of asymptotic freedom made him immortal, but I respectfully disagree with him. Framework or not, both QM and QFT have absolutely solid confirmations in all their aspects in thousands of experiments.

As for the once popular idea that string theory could provide a "theory of everything", he writes:

> Well… it never happened and – I will risk to say – never will.

Comments

1. **DB**
   January 5, 2020

   Hi Peter,
   happy new year to you and your readers.

   At last, at last one well known theoretical physicist comes out openly and says loud and clear what many already think, but are afraid of saying or writing due to repercussions in their professional lives.
   Yes, string theory is NOT the so much sought after "theory of everything". Susskind has already said it half a dozen times. Arkani-Hamed has semi-implied it. I wonder, and have written on this blog post before, when will all the big guns come out and acknowledge it publicly.
   I guess money and egos are running high there…

2. **Jack Harvey**
   January 5, 2020

   Your web site http://www.math.columbia.edu/~woit/wordpress/ gives a warning which you may want to to eliminate. Using Firefox, Chrome or MS Edge the padlock icon may have a red line through it. It's a security notice which may scare off people from going to your site.
   Jack

3. **Peter Woit**
   January 5, 2020

   Jack Harvey (and anyone interested in why the browsers are giving warnings),
   What's happening is that while the main connection to the blog is SSL encrypted (https), there are lots of scripts and code in WordPress and the plugins being used that use http, not https. Eliminating all of these occurrences would be a lot of effort and not actually solve any significant security problem, so not a high priority right now.

4. **Art**
   January 5, 2020

   sort of OT: The Economist surveys collider proposals in "Assembling the future" in its 4 Jan edition.

5. **Alessandro Strumia**
   January 5, 2020

   In fig. 1 Shifman plots particle physics as a dead trunk. Other authors one day earlier wrote that we must "communicate much more convincingly the prospective economical, societal, environmental and cultural impacts of HEP" (arxiv.org/pdf/1912.13466.pdf). Who is right?

6. **Peter Woit**
   January 5, 2020

Art,
Thanks. For others, that story is at
https://www.economist.com/science-and-technology/2020/01/02/finding-new-physics-will-require-a-new-particle-collider
Mostly well-done, but gets a few things wrong:
1. The problem with electron beams is not that less mass means less kinetic energy, but less mass means the synchrotron radiation problem described later is much worse.
2. Introducing superpartners doesn't reduce the number of free parameters of the Standard Model, quite the opposite: it adds many more.

I hadn't heard of opposition by the US to a Chinese collider since US scientists wouldn't be allowed to work on it. As far as I can tell, a US physicist (Arkani-Hamed) is still director of the Chinese institute planning such a machine, see
http://cfhep.ihep.ac.cn/

Alessandro Strumia,

Thanks. The Shifman article is about HEP theory, and there's a good argument that it has now been dead for a while. The other article you link to is about the experimental HEP side, and provides a good update and summary of the problem of a next generation European collider. There the problem I think is not that energy frontier experimental HEP is dead, but that it has a serious medical issue: major organ failure projected for 2035 or so, and a replacement organ will be extremely expensive, unclear how to pay for it. Close family members are saying "not worth keeping it alive, it's a goner anyway, so why should we go into hock to save it, money will be better spent on something else". Thus the line you quote.

The article gives a good summary of the problem:
"major changes will be needed with respect to the present CERN way of programme realisation: exceptional and quite substantial contributions from the host states, significant contributions from non-member states participating in this programme and preferential loans with long-term reimbursement profiles. We assume that this should be possible."

7. **33lewski**
January 7, 2020

Depicting Post Empirical Science as a tiny toxic mushroom next to a great tree of Physics is very accurate. In my view a polypore on a tree would represent the situation slightly more faithfully.

8. **Squarkino**
January 8, 2020

Speaking of CERN funding, did we ever find out which nations didn't pony up the €100 million and why?

9. **Parravicini 2036**
January 11, 2020

Ironically enough, just a few hours after this post, Icecube ruled out the last Standard Model explanation of ANITA's anomalous neutrino events:

https://icecube.wisc.edu/news/view/709

10. **Peter Woit**
January 11, 2020

Parravicini 2036,
Unfortunately I don't think ANITA seeing just two hard to understand signals and ICECUBE seeing nothing adds up to bringing down the Standard Model. About ANITA, Sabine Hossenfelder had some comments here
https://backreaction.blogspot.com/2019/07/science-shrugs.html
The usual "extraordinary claims require extraordinary evidence" principle applies.

11. **Nick M.**
January 11, 2020

Hi Peter,

I just read this article regarding the DoE's approval (this past Thursday) of Brookhaven's proposed Electron-Ion Collider (EIC), and thought that I would share:

https://www.energy.gov/articles/us-department-energy-selects-brookhaven-national-laboratory-host-major-new-nuclear-physics

Also at Science magazine here:

https://www.sciencemag.org/news/2020/01/department-energy-picks-new-york-over-virginia-site-new-particle-collider

12. **Peter Woit**
January 11, 2020

Nick M.,

Thanks. I note that the Science article explains something that is not known to most people:
"The decision on a machine still 10 years away reflects the relative good times for DOE science funding, Dabbar says. "We've been able to start on every major project that's been on the books for years." DOE's science budget is up 31% since 2016—in spite of the fact that under President Donald Trump, the White House has tried to slash it every year."

Why the foundations of physics have not progressed for 40 years

January 13, 2020
Categories: Uncategorized

Sabine Hossenfelder has a new piece out, making many of the same arguments she has been making for a while about the state of fundamental theory in physics. These have a lot in common with arguments that Lee Smolin and I were making in our books published back in 2006. The underlying problem is that the way theorists successfully worked up until the seventies is no longer viable, with the Standard Model working too well, up to the highest energies probed:

> The major cause of this stagnation is that physics has changed, but physicists have not changed their methods. As physics has progressed, the foundations have become increasingly harder to probe by experiment. Technological advances have not kept size and expenses manageable. This is why, in physics today, we have collaborations of thousands of people operating machines that cost billions of dollars.
>
> With fewer experiments, serendipitous discoveries become increasingly unlikely. And lacking those discoveries, the technological progress that would be needed to keep experiments economically viable never materializes. It's a vicious cycle: Costly experiments result in lack of progress. Lack of progress increases the costs of further experiment. This cycle must eventually lead into a dead end when experiments become simply too expensive to remain affordable. A $40 billion particle collider is such a dead end.

I have a somewhat different view about a potential next collider (see here), but agree that the basic question is whether it will be "too expensive to remain affordable."

What has happened over the last forty years is that the way HEP theory is done has become dysfunctional, in a way that Hossenfelder characterizes as follows:

> Instead of examining the way that they propose hypotheses and revising their methods, theoretical physicists have developed a habit of putting forward entirely baseless speculations. Over and over again I have heard them justifying their mindless production of mathematical fiction as "healthy speculation" – entirely ignoring that this type of speculation has demonstrably not workhat woed for decades and continues to not work. There is nothing healthy about this. It's sick science. And, embarrassingly enough, that's plain to see for everyone who does not work in the field.
>
> This behavior is based on the hopelessly naïve, not to mention ill-informed, belief that science always progresses somehow, and that sooner or later certainly someone will stumble over something interesting. But even if that happened – even if someone found a piece of the puzzle – at this point we wouldn't notice, because today any drop of genuine theoretical progress would drown in an ocean of "healthy speculation"…
>
> Why don't physicists have a hard look at their history and learn from their failure? Because the existing scientific system does not encourage learning. Physicists today can happily make career by writing papers about things no one has ever observed, and never will observe. This continues to go on because there is nothing and no one that can stop it.

This story brings up a lot of complex issues in the philosophy and sociology of science, but to me there's one aspect of the problem that is relatively simple and deserves a lot more attention than it gets: how do you get theorists to abandon failed ideas and move on to try something else?

The negative LHC results about SUSY have had some effect, but even in this case it's remarkable how many theorists won't abandon the failed idea of a SUSY extension of the Standard Model. This was always a highly dubious idea, explaining nothing about the Standard Model and adding a huge number of new degrees of freedom and more than a hundred new undetermined parameters. Not seeing anything at the LHC should have put the final nail in the coffin of that idea. Instead, I see that this past fall MIT was still training its graduate students with a course on Supersymmetric Quantum Field Theories. You can try and argue that SUSY and supergravity theories are worth studying even if they have nothing to do with physics at observable energies, but it is a fact that these are extremely complicated QFTs to work with and have explained nothing. Why encourage grad students to devote the many, many hours it takes to understand the details of this subject, instead of encouraging them to learn about something that hasn't been a huge failure?

The techniques one gets trained in as a graduate student tend to form the basis of one's understanding of a subject and have a huge influence on one's future career and the questions one has the expertise needed to work on. Besides SUSY, string theory has been the other major course topic at many institutions, with the best US grad students often spending large amounts of time trying to absorb the material in Polchinski's two-volume textbook, even though the motivations for this have turned out to also be a huge failure, arguably the largest one in the history of theoretical physics.

To get some idea of what is going on, I took a look at the current and recent course offerings (on BSM theory, not including cosmology) at the five leading (if you believe US News) US HEP theory departments. I may very well be missing some offered courses, but the following gives some insight into what leading US departments are teaching their theory students. Comparing to past years might be interesting, possibly there's a trend towards abandoning the whole area in favor of other topics (e.g. cosmology, quantum information, condensed matter).

- Harvard:
  Fall 2019
  PHYSICS 283B: Spacetime and Quantum Mechanics, Total Positivity and Motives
  PHYSICS 287A: Introduction to String Theory
  Spring 2020
  PHYSICS 211BR – Black Holes from A to Z
  PHYSICS 287BR – The String Landscape and the String Swampland
- Stanford:
  No courses beyond QFT in 2019/20
- Caltech:
  No courses beyond QFT in 2019/20
- Princeton:
  Fall 2019
  Phy 540 Strings, Black Holes and Gauge Theories (Klebanov)
  Spring 2020
  Phy 540 Strings, Black Holes and Gauge Theories (Polyakov)
- MIT
  Fall 2019
  8.831 Supersymmetric Quantum Field Theory
  Spring 2020
  8.851 Effective Field Theory

The places not offering string theory courses this year seem to have had them last year.

Update: Something relevant and worth reading that I think I missed when it came out: Jeremy Butterfield's detailed review of Lost in Math, which has a lot about the question of why theorists are “stuck”.

Update: There’s some serious discussion of this on Twitter. For those who can stand that format, try looking here and here.

Update: Mark Goodsell has a blog posting about all this here, including a defense of teaching the usual SUSY story to graduate students.

Update: A correspondent pointed me to this recent CERN Courier interview with John Ellis. Ellis maintains his increasingly implausible defense of SUSY, but he’s well aware that times have now changed:

> People are certainly exploring new theoretical avenues, which is very healthy and, in a way, there is much more freedom for young theorists today than there might have been in the past. Personally, I would be rather reluctant at this time to propose to a PhD student a thesis that was based solely on SUSY – the people who are hiring are quite likely to want them to be not just working on SUSY and maybe even not working on SUSY at all. I would regard that as a bit unfair, but there are always fashions in theoretical physics.

Comments

1. **Warren Siegel**
   January 13, 2020

   If there were anything else, people would move on.
   It’s exactly because high-energy physics as we have known it no longer produces new results that increasingly imaginative ideas are being tested.
   Defeatists have not found viable alternatives.

2. **Peter Woit**
   January 13, 2020

   Warren Siegel,
   I agree that if there were a readily-identifiable good alternative, people would move to it. In the past experiment would point in the right direction. That’s gone and it’s a very hard problem to replace that and find other routes to new ideas.

   What isn’t so hard though is to recognize when ideas don’t work, and doing so isn’t “defeatist”. The problem isn’t “increasingly imaginative ideas”, it’s going along with increasingly bad justifications for continuing to pursue failed research programs instead of acknowledging the obvious.

3. **Amitabh Lath**
   January 13, 2020

   The courses for advanced graduate students tend to be more fluid than the core courses. Sometimes an advanced topic is offered every other year (for instance General Relativity). Some of the more esoteric topics can be even less frequent. Also the course might not be called “String Theory” but “Special Topics in XYZ…” And occasionally students will just arrange a weekly meeting with a professor to learn a subject in depth. I wouldn’t put much weight on courses appearing or disappearing from the course catalog.

4. **Alessandro Strumia**
   January 13, 2020

   The problem is driven by physics, not by sociology. Simple explanation in natural units:
   (collider radius) ≈ (4π/alpha)^3 (collider energy)/(electron mass)^2.
   Collider technology remains based on old physics: electrons and electromagnetism. Because the heavier particles discovered at colliders found almost no practical use, not even for building better colliders. This is the key point. The rest follows: colliders become big, slow, expensive. Theory detaches from experiment.

5. **Mark**
   January 14, 2020

   I regard string theory as etudes for physicists like there exist etudes for piano players. It’s good for improving techniques (mathematics in our case) but not “the real thing”, because string theory is in principle based on quantum theory for extended objects, which is not conceptually really a new idea or even revolutionary, being technically difficult but conceptually almost trivial. We have at the moment no one who is capable to produce a masterpiece like did Bach or Chopin in music, for example. We are just practicing and preparing for it.
   But that’s better than nothing.
   I would not condemn string theory in total: Learning it’s technical aspects (etudes) may lead us one day to be capable to find the true masterpiece.

6. **Peter Woit**
   January 14, 2020

   Alessandro Strumia,
   Yes, it’s clear that the underlying problem is the physics that makes probing higher energy scales more and more expensive. This creates a different problem, with sociological aspects: once they’ve detached from experiment, what happens to HEP theorists and the field of HEP theory?
   Some possible reactions to this problem are:

   1\. Ignore it, refuse to acknowledge it publicly, and keep on pursuing previously popular research programs that have failed. Keep training new generations of students in the complexities of SUSY or string theory.

   2\. Give up, abandon HEP theory for another healthier field (e.g. cosmology, condensed matter, quantum information theory, machine learning).

   3\. Find some route to new, more promising ideas.

   I’d argue that 3 is still viable. A big problem is the historical sociology of the field has become dysfunctional. It emphasized concentration on a small number of questions, driven by experiment providing the right question. This falls apart when experiment stops providing the right promising question for everyone to focus on.

7. **Peter Woit**
   January 14, 2020

   Mark,
   I think that for quite a while post-1984 you could sensibly make the argument that while the string unification conjecture was a failure, there was a lot to be learned from

the deeper study of string theory and it was worthwhile for people to pursue that. Unfortunately, over the past 20 years or so, progress in learning new things from the deeper study of string theory has pretty much come to a halt. Keeping doing the same things over and over, while waiting for a genius to save you from yourself is not a good plan. For one thing, up and coming geniuses who take a look at a field and see that going on will flee and look for another field in which to exercise their genius.

8. **Sabine Hossenfelder**
   January 14, 2020

   Hi Peter,

   Thanks for mentioning. The piece actually isn't new, it's a reprint of [a blogpost I wrote last year](#).

   Butterfield has misstated my position on some issues. I have a brief response to this [here](#).

9. **Akhil**
   January 14, 2020

   Peter,
   PHYSICS 283B: Spacetime and Quantum Mechanics, Total Positivity and Motives looks like a very interesting course to take for new students. Does it have any promising ideas or is it just the old failed ideas?

   Also curious about your take on pursuing deep mathematics as a promising way to progress. Can you elaborate on that?

   Where can you disagree with Sabine, on pursuing deep mathematics vs lost in math?

10. **SparkTech**
    January 14, 2020

    Cut the funding. I mean that's an idea, not the solution: the solution is the one which make possible a financial limitation to string theory community and alike.

    I think of this because, I believe, the true problem is the(!) money that keeps pouring into that kind of bogus research.

    And scare resources should be a resource for creativity.
    The theoretical work better be focused on technologies oriented to experiments and observations, by driving experimentalists on what could do (what observations/experiments could think of), instead of making unnecessary abstract assumptions on how the universe should be (parallel worlds, holography, strings etc.).

11. **Peter Woit**
    January 14, 2020

    Akhil,
    That's an unusual course. Arkani-Hamed was visiting Harvard, it was an opportunity for him to explain his current research program in detail. I've written about this "amplitudes" research many times here on the blog.

    There is a great deal in my "Not Even Wrong" book about the relation of deep ideas in mathematics to the Standard Model.

    For my disagreements with "Lost in Math", see my review of the book.

    SparkTech,
    The problem isn't a lack of focus on experiment, it's the lack of experimental anomalies that could point to a way forward. Experimentalists and theorists have worked hard to change this, but nature is not cooperating. The problem is how to make progress given this situation.

    Theorists need to work with abstract assumptions on how the universe should be, but they need to do a better job of rejecting ones that fail to lead anywhere and coming up with new ones to try.

    Cutting off funding to failed research programs would be helpful, but what really matter is how the leading figures in the field deal with the problem of no progress. Funding decisions should come from the people in the field, not from people who don't understand the problems theorists are grappling with. Hossenfelder is doing a good job trying to get theorists to face up to the problem, in some sense what we need is a more serious response to her challenge.

    Just saw this
    https://twitter.com/JimBaggott/status/1217011515385139201
    which makes the same point.

12. **Claudio Paganini**
    January 14, 2020

    One of the problems I see is the concentration of power with certain fields in the theory departments around the world but particularly within the US.
    "Just cut of the funding for field X" doesn't work if the people deciding over funding in theory departments are all from field X.

    I think physics would need to revise their publication culture and get closer to that of mathematics. Way fewer papers but an idea worked out in full detail.

    There are interesting alternative approaches out there which manage to resolve some of the conceptional issues. Non-Commutative geometry, for example, seems interesting, although lacking a Lorentzian formulation as far as I can tell. E8 theory is another one. Causal Fermion Systems, which can explain the 3 generations of fermions belongs to that set as well.

    Then on the foundations of quantum mechanics there is also interesting new work being done. Ellis approach that claims the macro to be as real as the micro aspects of the world, resolving the measurement problem by top down causation.
    Fröhlichs Events, Trees, Histories (ETH) approach to quantum mechanics which resolves the measurement problem by providing a sharp definition of events and in the course abandoning unitary evolution (except as a non interacting limit case).

    What all of these have in common is that they require deep mathematics which unfortunately scares away many physicists, even theoretical ones, from truly engaging with them.

13. **SparkTech**
    January 14, 2020

@Peter Woit

Thanks for your answer and for the time it took. For sure you have better hard data than me to judge on this.

Your statement "Funding decisions should come from the people in the field […]" is perfectly reasonable, and I have accepted it myself for some time by now
However the way I see it now (ok, call it naive if you like): if we keep financing string theory community (for example) in the same way as we did so far, then string theorists will do what the did already in the last past decades. Is there any evidence for a paradigm shift? Maybe occasionally some scientists switch the field (but as I said, you have better data).

As for the nature which does not cooperate, (again, I believe you have better knowledge) I also believe it is a lot easier to cook up a complex mathematical theories about quantum foams than to think of a better technology to extract new data from that quasar which is that far away. By limiting the funding for that juggernaut theoretical community will force them to squeeze their "theoretical" brains towards a more empirical approach.

I also agree that theorists need to work with abstract assumptions on how the universe should be, but there also must be a limit on that: the pathology is now an inflation of theories that explains nothing, since there was little-to-nothing observed or experimented to start from in the first place.

14. **Shantanu**
January 14, 2020

Peter and others:
How come universities (with theory departments) are not offering dedicated courses in neutrino physics? That is the the only place where there is "supposedly" evidence for Physics BSM? Or is it that the "breakthroughs" in neutrino physics have taught us nothing?

15. **Peter Woit**
January 14, 2020

Claudio Paganini,

Yes, if funding, hiring, training of graduate student, etc. decisions are all in the hands of people working in field X, any significant change requires changing the thinking of those in field X. The terminology "field X" for the dominant field in formal theory in the US is a very good one, since, while people in this field often call themselves "string theorists", most of the best ones are not doing string theory. While they often have moved on to other things, the problem is that they are still advertising string theory to the public, training graduate students in string theory, and evaluating new research directions based on whether they somehow follow from the string theory research program path of the past.

16. **Peter Woit**
January 14, 2020

SparkTech,
The problem is that string theorists have already been under huge pressure to show that their research connects to experiment, and all this has led to is even more dubious research directions like the string theory landscape or the currently popular "swampland" program. What's needed is acknowledgement that this is a failed idea that can never connect to experiment, not more effort to find a way to do so.

17. **Peter Woit**
January 14, 2020

Shantanu,
Pretty much every institution teaches courses on QFT, leading up to the Standard Model QFT, and in many of these I would expect that there is some discussion of neutrino masses and possible extensions of the Standard Model involving them.

But I don't think there has been much in the way of either promising new theoretical ideas or unexpected experimental results on this front. If forthcoming neutrino experiments turn up something unexpected, I think courses on the topic would quickly become much more popular.

18. **SparkTech**
January 14, 2020

You could be right. My bet: they acknowledging that will never happen.

As you said, there is already evidence for what I believe(more dubious research in spite of huge pressure, etc., I mean the right trend is not there). And human-wise who could acknowledge that he/she spend last 20 years himself/herself on bogus physics? Which is rather consistent with my little samplings of string – theorists claiming that string theory did make significant progress in the last decades. And, as you mentioned already, it is not only them: we got also many-worlds/universes etc.

I mean: you, Sabine, Lees Smolin doing great job. And it could bring hundreds of others being just as vocal. Still waiting for that to see it happening. I only say it may not be enough.

Again, thanks for you patience and time,
and I wish you a nice day!

19. **Peter Shor**
January 14, 2020

Sabine says:

> Theoretical physicists have developed a habit of putting forward entirely baseless speculations. Over and over again I have heard them justifying their mindless production of mathematical fiction as "healthy speculation" – entirely ignoring that this type of speculation has demonstrably not worked for decades and continues to not work.

But didn't this method work wonderfully well between 1900 and 1975?

Planck put forth the entirely baseless speculation that light came in chunks of discrete energy (and won a Nobel Prize).

Dirac put forth the entirely baseless speculation that the entire universe was a sea of electrons that had both extra electrons and holes (and won a Nobel Prize).

Geoffrey Chew put forth the entirely baseless speculation that the "bootstrap method" could explain the particle zoo (and did not win a Nobel Prize).

The difference today is that we don't have experiments that rule out the worthless entirely baseless speculations and confirm the entirely baseless speculations that happen to be correct. Experiments have been replaced by some complicated social process that selects some subset of the entirely baseless speculations that physicists come to a consensus on believing, but which seems to do a terrible job of separating the correct baseless speculations from the incorrect ones.

20. **Peter Woit**
January 14, 2020

"Experiments have been replaced by some complicated social process that selects some subset of the entirely baseless speculations that physicists come to a consensus on believing, but which seems to do a terrible job of separating the correct baseless speculations from the incorrect ones."

An excellent summary of the problem…

21. **Sabine Hossenfelder**
January 14, 2020

Peter, Alessandro,

The idea that one needs high energies to probe new physics is simply wrong. To begin with, I don't have to tell you that if there's new physics at high energies that would also change low-energy predictions, so higher precision can replace higher energies.

But maybe more importantly it is generally an unjustified assumption that the next breakthrough in the foundations will come from going to higher energies or short distances as opposed to probing other regimes that have been untested so far. My favorite example are quantum effects in many particle systems. This is imo presently the obvious frontier to push. You may not agree, but that's not the point. The point is that higher energies is not the only route.

Of course particle physicists don't want to hear it, but building bigger colliders has clearly run its course — for now. It's expensive and the scientific benefit is negligible. Colliders are something we will certainly come back to. Maybe in a hundred years or 200 years. (Unless by then hunting with wooden sticks has become ground-breaking technology.) But this is not the right time to throw more money at particle physicists.

Let me also repeat that the problem with lacking experimental input is not decoupled from the lack of progress in theory development. The days in which we could bank on serendipitous discoveries in the foundation of physics are over. "Just look" does not work any more. The string of failed experiments in the past 40 years is evidence for that. This is simply not up to debate, it's a fact.

It is likewise a fact that theorists' methods of theory development are not working. Claiming that "it's physics" does not explain why they are not willing to revise their methods.

Re the supposed lack of alternatives.
First, it's wrong. There are various alternatives. It's just that so few people work on them that it's not even clear how promising they are. They are easy to dismiss because they have open questions simply due to lack of manpower.

But besides that, I am sick and tired of the claim that theorists in the foundations should be allowed to do a crappy job because no one has any better idea what to do. If they don't know what to do, then we shouldn't pay them.

Seriously, what kind of attitude is this? In what other profession would you get away with not being able to get anything right for 40 years and then saying you will continue that way because you can't think of anything better to do?

22. **Peter Woit**
January 14, 2020

Sabine,
I agree with much of what you have to say here, do disagree though about the issue of a higher energy collider. If the only argument for it was "let's go look, maybe gluinos or extra dimensions will turn up", I'd agree with you. You're right that the generic "let's investigate the 1-10 TeV scale even though we don't think there's anything there" argument faces a serious counterargument about the cost. The to me serious HEP argument is that the most mysterious and least understood part of the Standard Model is the Higgs sector (some might want to argue for the neutrino sector, but that area of HEP is going ahead with little controversy over its funding, since it is quite a bit cheaper). I don't believe there's any way to get better information about the Higgs sector than what we'll get from the HL-LHC without a new collider.

The problem of course is the cost. If a new collider cost less than \$1 billion there wouldn't be a lot of opposition to going forward. At \$10 billion and up, the cost is a big problem. For the latest on the leading proposal, see this talk from yesterday

https://indico.cern.ch/event/838435/contributions/3635822/attachments/1967874/3272551/200113_FCC-Status.pdf

The timeline given there (page 15) shows, I think assuming the project gets the go-ahead in the next year or so, a three year "funding strategy" period, and eight years before one starts even digging the tunnel. My understanding is that next week there's a drafting session scheduled, and May 2020 is the time frame for a specific proposal to be submitted to the CERN Council. I'd argue that it's best if everyone wait a little while to have a serious argument over the new collider question, until there's a specific proposal for both what to build and how to fund it on the table. There's going to be plenty of time for discussion about this before anything much actually happens.

23. **Alex**
January 14, 2020

The post had asserted:

"Caltech: No courses beyond QFT in 2019/20"

At Caltech, Physics 230 is seen as the follow-up to the QFT courses.

24. **Peter Woit**
January 14, 2020

Alex,
Thanks, I updated the posting. I'd stopped reading the course description after "Advanced methods in QFT" and seeing mention of confinement and non-perturbative techniques in gauge theory, typical Standard Model QFT issues. The course description does say however that a first term version of the course like the fall one would cover
"introduction to supersymmetry, including the minimal supersymmetric extension of the standard model, supersymmetric grand unified theories, extended supersymmetry, supergravity, and supersymmetric theories in higher dimensions."

and if that's what the instructor chose to do last fall, it would have been just as bad an idea as the MIT SUSY course.

I'm well aware (Amitabh Lath kind of points this out), that what gets covered in advanced classes like this depends on the instructor and evolves over the years. I'd be interested to know if instructors of courses like this are realizing that the topics covered should be changed.

25. **Peter Woit**
January 14, 2020

A bit of research shows that in Fall 2017 when Hirosi Ooguri was teaching Ph 230 at Caltech, the syllabus
http://ooguri.caltech.edu/education/230
covered mostly gauge theory, no SUSY.
In 2015-6, when the course was taught by Kapustin, the fall and winter quarter were Yang-Mills and non-perturbative QFT, the spring was devoted to SUSY.
http://www.theory.caltech.edu/~kapustin/Ph230/Ph230.html

26. **Alan Roxdale**
January 14, 2020

> The techniques one gets trained in as a graduate student tend to form the basis of one's understanding of a subject and have a huge influence on one's future career and the questions one has the expertise needed to work on.

I'll put forward the suggestion here that the techniques one does _not_ get trained in, or worse, get trained to avoid also play a significant role. In fact, I think this is the big structural issue which has lead to the current stall (albiet taking perhaps 50-70 years to do so).

Adam Becker makes a strong case in his recent book that the profession and education of theoretical physicists underwent a sharp transition immediately after the second world war. The volume of physicists (and also other scientists and engineers) spiked, the centre of the profession shifted to the previously placid United States, and educational norms began to emphaise practicalities over philosophical introspection. You could argue that foundational issues were forgotten in the heady days of Big Science.

But, I find it hard to avoid the conclusion that there is more to it. A reading of history suggests that (quantum) foundational issues were buried, becoming not simply unfashionable, but actually taboo. Becker presents several unflattering cases for the record of post-war physics, enough to convince me at least that lack of progress on foundations (and I'd argue any new physics) can be blamed in significant part on purely sociological issues, all physics aside. This is the case in all fields no doubt, but this really does include theoretical physics.

> But didn't this method work wonderfully well between 1900 and 1975?

The mid 70's alway seem to be a sharp cutoff for most assessments of the progress in physics. I'd point to a delayed fallout from the general shift in the profession, as an older differently trained cohort passed the torch to a structurally different generation (though you could point to the SALT treaty too!). A generation which had been trained, institutionally, socially or otherwise to _not_ go looking in certain directions. Maybe it worked for a while, and probably did work for several other fields, but it hasn't worked for theoretical physics.

The solution I'd advocate is to change the "training". Make graduate students (indeed all physics students) aware of the problems, and in cases the gaping holes. Make them keenly aware of the history and the controversies and the unexplained, that there is still work to be done, and then let them do it. Tacitly of course this resigns the current generation(s) to "standing and waiting". Perhaps more controversially, this involves public funding bodies be made aware that the profession is more fallible than generally accepted. In any case, continuing as is only means further 'Breznevisation'.

27. **Peter Woit**
January 14, 2020

Alan Roxdale,
I wrote about Becker's book here
https://www.math.columbia.edu/~woit/wordpress/?p=10147
where you can see that I very much disagree with the story he pushes about the evil, unthinking Copenhagen orthodoxy and the good Bohm/Everett "quantum rebels". The last thing theory graduate students need is to be drawn into the tar pit of "interpretations" and sold a moralistic story about how the old fogeys just didn't realize that many-worlds are the solution to fundamental problems.

28. **Amitabh Lath**
January 14, 2020

Peter, you say At \$10 billion and up, the cost is a big problem. Why? Surely the cost of the FCC (or comparable collider in China) is a smaller fraction of the World GDP today than Fermilab's cost was to the late 1960s US economy. By the way a lot of that 10G$ estimate is salaries for engineers which will probably be "in kind" contributions from countries like India and China.

Also, the world is a much more peaceful place today. There are no incendiary-laden B52s heading to Asia. Countries today are content to lob shipping containers at each other and a major world crisis happens when the number of containers decreases by 4%. This is the ideal time to build something like the FCC.

The Standard Model is proving to be a tough nut to crack, I'll admit. But I do not know of any (experimental high energy) physicist that does not think we will eventually break it. It might take many decades, our grandchildren might be the ones to see the first hints of new physics but it will happen. The alternative is to believe that 2012 was somehow a privileged point in human history after which no fundamental discoveries could be made.

29. **John**
January 14, 2020

Sabine and Peter,

I find that Sabine's line of argumentation against a next high-energy collider to be completely unsatisfying. It seems that she is arguing that because the LHC has not turned up new physics (besides the Higgs, which is a major triumph that she seems to downplay), that means that experiment is a failure and that future colliders should not be built. I am happy to see that Peter disagrees, but I would like to add some of my feelings.

For one, we should remember that the LHC would never have happened had it not been for the cancellation of the Superconducting Supercollider (SSC). The LHC currently operates at a center-of-mass energy of 13 TeV with an expected upgrade to 14 TeV (though 14 TeV was the initial "design" com energy). On the other hand, the SSC was a 40 TeV collider, and a reasonable upgrade could have sent it to 50 TeV. This is not insubstantial.

New physics shows up only in very very subtle hints before one reaches the mass threshold for producing on-shell particles (e.g. a new 10-15 TeV particle would be nigh impossible to detect at the LHC but might show up at the SSC). In the past, theorists of SUSY and other BSM theories have predicted what the thresholds for new phenomena should be, but have been basically wrong. So, justifiably, Sabine is arguing that the theorists have been on the wrong track in terms of predicting what the thresholds should be. However, according to her logic, that would mean it could turn out that the LHC upgrade that ups it from 13 to 14 TeV could open new worlds of possibilities. Likewise, the possibilities at 40 TeV would be (and would have been, with the SSC) that much greater. So already, when Sabine is saying that "we need new physics," it is completely unjustified to argue against a new, more powerful collider. Had US politics not steered collider physics as they have, we might now be in a bountiful situation much like what was seen in the 1970's. And writing off this sort of possibility, based on theoretical arguments, is hypocritical if the argument is based on the same kinds of theoretical considerations you were just arguing against.

Just to put it in a simplified form: we could have had a much more powerful collider, 40-50 TeV instead of 13-14 TeV, years ago. The costs for such a machine have gone down at this point (adjusting for inflation.) So we are in the situation that the people who've told us that we should expect new physics at some new energy, based on theory, have been shown to be unreliable — however, that reasoning for why we should not trust theorists applies _just as well_ as those who are telling us _not_ to expect new physics at the next collider.

Sabine's ideas on "alternative" kinds of experiments are complete "Hail Mary" ideas — she needs to cite better literature if anyone is to take her seriously when she is going up against a historically proven experimental program basically single-handedly. Even if such ideas had any merit, it doesn't mean delaying the next collider makes any sense — big science needs to push on multiple fronts, just like ITER (a much more expensive experiment than the LHC) has been trudging along despite developments in fusion that will have made it somewhat out-of-date already by the time it gets finished. But in the field of fusion energy, they have the right mentality in that the big mainstream projects should proceed in parallel with the smaller more speculative ones, and not to put off progress on the big mainstream projects in hopes that a miracle happens in smaller sideline work.

30. **Amitabh Lath**
January 14, 2020

That first sentence should say The cost of the FCC or Chinese collider is smaller as a fraction of the World GDP than Fermilab's cost compared to the US economy in the late 1960s.

31. **Peter Woit**
January 14, 2020

John,
I don't disagree and made many of the same points myself in the posting and discussion here
https://www.math.columbia.edu/~woit/wordpress/?p=10768
" One piece of advice though is that experience of the past few decades shows you probably shouldn't listen to theorists."

Amitabh Lath (and John),
My comment that a \$10 billion cost is a "big problem" was not meant to indicate I have a problem with government spending on that order to fund a collider, or that there is some problem of principle, but just that raising that kind of sum is going to be hard to do.

This isn't going to get built in the US and the US is not going to provide a large fraction of the cost of a machine built elsewhere. The US budget process is currently a joke, with no clarity as to what will happen next year, much less over a 10-20 year period. Any document promising US future funding would not be worth the paper it was written on. I also doubt the Japanese or Chinese will fund much of a machine built in Europe. Maybe the Chinese will do this in China, but that would require overcoming objections from those like C.N. Yang who feel that China has more pressing needs for spending \$10 billion.

In Europe, CERN has a fixed budget (in fraction of GDP) and while one can reasonably expect it to hold onto that, it's very unclear to me whether a plan for \$10-\$15 billion in new spending can fit into that budget. I don't know enough about CERN funding to know whether there's a realistic prospect of getting commitments for a big budget increase to fund a new machine. I'm guessing we'll be hearing from CERN leadership in coming months about these issues.

The above is what I meant by "big problem", I hope it can be solved.

32. **Peter Woit**
January 14, 2020

All,
As discussed above, I don't think further debate over a new collider here now is a fruitful thing to do (and, you shouldn't listen to theorists anyway…).

33. **Peter Shor**
January 15, 2020

Amitabh Lath: If a €1.5 billion telescope was cancelled because it was too expensive, it's difficult to imagine how physicists will be able to get funding for a $20 billion collider.

And it's also difficult to imagine how a new collider will give us information about quantum gravity, at the Planck scale. So even if it discover interesting physics beyond the Standard Model, it doesn't really solve the problem that Sabine's article brings up.

34. **Amitabh Lath**
January 15, 2020

Peter Shor, either new physics is there within human reach or it is not. If it is then the biggest hadron collider we can build is obviously the best bet to find it. Nothing beats bigger \sqrt{s}. If you look beyond the headlines (LHC fails to find SUSY) you will see the breathtaking precision with which the searches have been carried out. Yes we are still smashing Swiss watches together but now we can identify just about every screw, spring, and sprocket that flies out. This should give you confidence about our ability to find new physics.

As to the comment that "even if it discover interesting physics beyond the Standard Model, it doesn't really solve the problem…" I do not understand this statement. How do we know what problems the new physics can or cannot solve until we find it?

As for $$, yes but we won't get it if we don't ask. The astronomers might have struck out with the Overwhelmingly Large but they are getting the Extremely Large, not to mention the James Webb, the Vera Rubin, etc.

35. **André**
January 16, 2020

Amitabh Lath: "the biggest hadron collider we can build is obviously the best bet to find it"
To me, this seems not obvious at all. I do not think that "new physics" necessarily requires "new particles". Sure, if you want to find new physics, you need to somehow explore new parameter space, and larger energies are an obvious candidate, but so are violations of the equivalence principle, massive superpositions, large distance Bell

tests, …
There are currently very few reasons to believe that any of those will show new physics, but we need to keep in mind that reasons to expect something new from a collider are equally unconvincing. As theoreticians, our task is of course to work on finding such convincing reasons why something new should happen in one experiment or another, but in the absence of these convincing theoretical reasons all these options must be seen as equally (un)likely to produce anything truly exciting.
The way I understand Sabine, her point is mainly that, because of this fact, we should maybe focus on the easier (i.e. cheaper) areas where new physics could show up first, and I very much agree with that sentiment.
The only potential benefit I see for a new collider is the one that Peter mentions: better understanding of the Higgs sector. Whether or not this is worth the price is in fact not an easy question. The "just look" argument, on the other hand, has not been aging well in my opinion.

36. **Peter Woit**
January 16, 2020

André,
One problem with the "let's do cheap violations of the equivalence principle, massive superpositions, large distance Bell tests, … instead of building a collider" argument is exactly that such things are (on the HEP funding scale) cheap. The issue of funding and doing such experiments really has nothing at all to do with the collider issue since the scales are completely different. The US and Europe each have billion-dollar/year budgets right now being spent on high energy physics, the Simons Foundation is handing out a quarter-billion/year in grants. If proposals for small-scale experiments can convince people they're worthwhile, they should in principle be able to get funded (if this isn't possible, the field has a different sort of funding problem that needs to get fixed).

37. **John Baez**
January 17, 2020

Peter Shor wrote:

> Planck put forth the entirely baseless speculation that light came in chunks of discrete energy (and won a Nobel Prize).

I can't let this go by. Planck was trying to explain some experimental data, and he didn't originally even pay much attention to the fact that his calculation required that light came in discrete chunks.

The simplified story often told to students is that Planck was struggling to deal with the fact that classical electromagnetism combined with statistical mechanics leads to an "ultraviolet catastrophe": due to the equipartition theorem, a box of classical radiation in thermal equilibrium would have more and more energy density at higher and higher frequencies, as described by the Rayleigh-Jeans law, leading to infinite total energy. Planck showed that if radiation came in discrete chunks this problem would go away.

But in fact Planck wrote his groundbreaking paper in 1900, he didn't accept the equipartition theorem seriously at the time, and the ultraviolet catastrophe was only recognized as a serious problem later. (The term was coined by Ehrenfest in 1911.)

What Planck actually started out trying to do was justify a different formula for the energy density as a function of frequency of radiation in thermal equilibrium, one that seemed empirically correct at high frequencies: the Wien law. He wrote a paper doing this in 1899, but then experiments showed the Wien law was inaccurate at low frequencies, so Planck went back to the drawing board.

In "an act of desperation", in 1900 he introduced discrete energy levels to get a law that matched the Wien law at high frequencies but differed from it at low frequencies. But he didn't think much about the meaning of these energy levels. He later wrote that it was "a purely formal assumption and I really did not give it much thought except that no matter what the cost, I must bring about a positive result".

He got a formula that fit the data, the Planck law, and he was happy with that. Only in 1908, thanks to Lorentz, did he accept the physical significance of the energy quanta he'd almost unwittingly introduced. This was *after* Einstein wrote his Nobel-winning paper on photons in 1905.

In short, Planck was heavily data-driven, and only later did the conceptual meaning and revolutionary nature of his calculation become clear. For more, try Max Planck: the reluctant revolutionary by Helge Kragh.

38. **Dr.Manfred Requardt**
January 17, 2020

Concerning Peter Shor, John Baez: Or read the formidable book by Thomas Kuhn, Black-Body Theory and the Quantum Discontinuity 1894-1912.

39. **Peter Woit**
January 17, 2020

Peter Shor/John Baez,
An interesting discussion of Planck, but this has gotten far off topic. Hossenfelder is getting a lot of flak for the "baseless speculation" terminology, but I think she was trying to point to something very specific, where she has a serious case.

Specifically, the problem is that there is currently no well-motivated theoretical reason to expect new physics:
1. At a new e+/e- collider. The examples people give of BSM models that contain new states that would not be seen at the LHC, but would be visible at a lower energy lepton collider appear to be contrived, "baseless speculation" is not a bad description. The non "baseless speculation" case for a lepton collider is as a tool for detailed study of the Higgs.
2. At a proton-proton collider with beam energy 2 (HE-LHC) -7 (FCC-pp) times that of the LHC. Such a machine would open up the study of a new energy range, but it is true that we have no well-motivated reason to expect something new in that energy range. Against her, there's a good argument that one should investigate the new higher energy range anyway, because doing so is what science is about (not just trusting that your theory extends to cover some unexamined region, but looking to check, something unexpected is quite possible). The problem is that doing this would be very expensive.

40. **Marco**
January 17, 2020

There is a nice reply from CLIC (https://arxiv.org/abs/2001.05373) on allegations made by FCC proponents (https://arxiv.org/abs/1912.13466).

41. **Amitabh Lath**
January 17, 2020

There is some seriously naive zero-sum thinking on this blog and comments. Peter Shor, Andre and others all point to cost as a the determining factor. Perhaps if science (colliders, telescopes, fusion…) manages to squeeze itself into a small enough box it won't get its head chopped off.

Projects like the SSC, LHC and the next one do not live or die by cost alone. Above a certain amount that isn't even the determining factor. The SSC was cancelled not because of minor cost overruns but because Speaker Jim Wright (D-Texas) was forced to resign, George H.W. Bush, (a yankee pretending to be Texan) lost re-election, and Lloyd Bentsen left the Senate. Suddenly Texas had zero support in DC. It's counter-intuitive but if the SSC had cost more, and that extra spending had been spread around widely to certain strategic states and districts, it could have survived.

If the FCC gets built it will be because of buy-in from the large CERN member states who will be promised major contracts. Italian, French, German, British, and Dutch electrical, mechanical, civil engineering firms will make sure to get their cut. Major code-writing will be done in Eastern Europe. There will be tacit agreement on new facilities in depressed areas, number of jobs, etc.

I understand the need highly numerate people have to believe everything is controlled by numbers, budgets, timelines. But the go/no-go decision on a project like the next hadron collider will rest on something a lot more political and social.

42. **Low Math, Meekly Interacting**
January 17, 2020

The political background is correct, but the motivation is forgotten: The SSC was supposed to discover the Higgs boson(s) and sparticles, or rule out natural SUSY.

The LHC already did that.

If one is being honest "because it's there" is the only well-founded motivation to probe the energy frontier, and one I happen to support (for what it's worth, which isn't much). Theoretical considerations have proven to be so pliable and inaccurate, and this has been going on for so long, it's very difficult for "outsiders" to take them seriously anymore.

It's really about time those making these arguments stop assuming interested outsiders are stupid and face the toll history has inflicted on the current state of the field.

43. **Peter Woit**
January 17, 2020

I'm going to delete any further arguing about a new collider here, nothing new is coming out of it. For the latest news about this, see this presentation from today:

https://indico.cern.ch/event/838435/contributions/3647164/attachments/1971463/3279599/05-D99-AB-next-steps.pdf

There's a strategy drafting session next week, results to be made public maybe in March. Even if there's a positive decision to pursue the idea of a new collider, it looks like this will just be the start of a multi-year process of trying to see if Amit is right and CERN member states will be willing to come up with new funding. An actual decision whether or not to go ahead is not envisioned until 2025/6. So, there will be plenty of time to debate this more…

44. **Peter Shor**
January 17, 2020

@John Baez: That's an interesting piece of history that I didn't know. But I don't think that invalidates the example. How much difference is there between "an act of desperation" and an "entirely baseless speculation"?

My point is that the paradigm that worked so well for physics before 1975 or so — throw lots and lots of not-so-crazy and crazy theories at the wall and see which ones are validated by experiments — doesn't work in the absence of experiments.

45. **Lee Smolin**
January 20, 2020

Dear Peter,

If its not too late, I'd like to propose a view point that points to a way forward for HEP. One reason scientists can get stuck is when we persist in asking the wrong questions. Progress resumes when someone asks the right question, ie a question that leads to the correct explanation of the physics in question.

In the case of unification and beyond the standard model physics, its pretty clear by now that what seemed like the right questions in the 1970's and 1980's: naturalness, what is the right symmetry group, etc are not leading to progress.

So what is the right question to ask now? Here is one suggestion:

Accept that the standard model parameters are fine tuned and ask: "What was the dynamical mechanism, acting in the early universe, or perhaps before the big bang, that tuned them?"

That is, think like Darwin rather than Plato. Faced with a diversity of species, Plato asked for absolute, timeless principles, that explained why these-and only these species-exist. Darwin's insight was that there are no such absolute principles. What we see in the biosphere is the result of a dynamical process: natural selection, acting over billions of years, that could have turned out differently. But the dynamics is simple, profound and leads to many insights.

My proposal, which I have been making since 1992, is that theoretical HEP will begin to flourish again when we HEP theorists drop the search for the ultimate symmetry group, and begin learning to think like Darwin.

Thanks,

Lee

46. **Peter Woit**
January 20, 2020

Thanks Lee,

I disagree with you that the current problem is too many theorists searching for a deeper symmetry-based explanation. These days the popular direction seems to be not that, but instead pursuit of the idea that space-time and the symmetries we see are just emergent epiphenomena of some seemingly unknowable or random quantum system.

The large number of random-looking parameters needed as input for the SM certainly doesn't look like the output of any known symmetry-based argument. Maybe they are environmental, determined either by anthropic selection or evolutionary path from whatever is going on pre-Big Bang. The problem remains though of coming up with a viable theory of whatever physics it is that is going on there, and that seems to me completely open.

47. **John Baez**
January 23, 2020

Peter Shor wrote:

> How much difference is there between “an act of desperation” and an “entirely baseless speculation”?

To me they seem diametrically opposed. Planck had some experimental data that contradicted the best theory so far, and he was desperately trying to fit it with a simple new model. An “entirely baseless speculation” doesn’t give a better fit of existing data: at best it satisfies some theoretical predilections and avoids contradiction with existing data. An example of the latter is making up a theory with new particles that just happen to have masses slightly larger than current colliders can see, but doesn’t make any more accurate predictions about things we actually do see. Some particle physicists have been doing this for decades now.

48. **DanM**
January 31, 2020

Is it worth pointing out that “the foundations of physics” are not limited to high-energy and particle physics? There are many theorists working on foundational problems in condensed matter physics, for example, where much progress continues to be made. It is a thriving and fruitful discipline with a close coupling to experiment and lots of exciting new results. The HEP community’s insistence that they’re the only ones who work on “foundations” (see, e.g., the headline to this very blog post) may be one of the problems here.

This and That

January 23, 2020
Categories: Uncategorized

- I was sorry to hear of the death a few months ago of Tony Smith, who had been a frequent commenter on this blog and others. Unfortunately my interactions with him mainly involved trying to discourage him from hijacking the discussion in some other (often unusual) direction. Geoffrey Dixon did get to know him well, and has written a wonderful long blog entry about Tony, which I highly recommend (Dixon's newish blog also has other things you might find interesting).
- On the Jim Simons front, the Simons Foundation has put together something to celebrate their 25th anniversary. It explains a lot about their history and what they are doing now, as well as giving some indication of plans for the future. On these topics, read pieces written by Jim Simons and Marilyn Simons. The Foundation has been in a high growth mode, having an increasingly large impact on math and physics research. Their main statement about the future is that the plan is for this to go on for a very long time:

  > According to its bylaws, the Simons Foundation is intended to focus almost entirely on research in mathematics and science and to exist in perpetuity. If future leadership abides by these guiding principles, Marilyn and I believe the foundation will forever be a force for good in our society.

  My impression is that the Simons children have their own interests, and foundations with other goals to run.

  News from the \$75 billion source of the money (RenTech) today is that Simons is increasingly turning over control of that to his son Nathaniel, who has been named co-chairman. He has also added four new directors to the board, four of them senior Renaissance executives, and one his son-in-law Mark Heising.

- There are various IAS-related videos you might want to take a look at:

  Pierre Deligne explaining motives last night.

  Michael Douglas on the use of computers in mathematics.

  A Dutch documentary (not all of it is in Dutch…).

- If you aren't regularly reading Scott Aaronson's blog, you really should be. Latest entries are a detailed report from Davos and a guest post with a compelling argument about a major factor behind the problem of why women leave STEM careers more than men.
- For the latest on the "It from Qubit" business, see talks at a KITP conference. John Preskill notes "lingering confusion over what it all means", which makes me glad to hear that I'm not the only one…

Comments

1. **John Baez**
   January 23, 2020

   Thanks for pointing that out about Tony Smith! I hadn't known.

2. **Richard Gaylord**
   January 24, 2020

   "The Foundation has been in a high growth mode, having an increasingly large impact on math and physics reserch.". that should be 'research'.

3. **Alessandro Strumia**
   January 24, 2020

   Dear Peter,

   you mention that "women leave STEM careers more than men", so let me point out that this is not what comes out analyzing data about every affiliation of every author of every paper in fundamental physics. This data allow to infer when every disambiguated author starts and leaves. An apparent 30% gender difference in leaving rates arises because the fraction of women in fundamental physics is higher now, while being hired has now become less likely for everybody. After correcting for this sociological confounding factor, the gender difference in leaving rates is found to be compatible with zero. This is shown in fig. 7 of a paper by mine, to appear on Quantitative Science Studies and that cannot appear on arXiv. Flaherty in (https://arxiv.org/abs/1810.01511) claimed a ≈400% difference in abandonment rates based on indirect small-scale data, but this has also been "firmly ruled out" by Perley (https://arxiv.org/abs/1903.08195).

4. **Chris Oakley**
   January 24, 2020

   Sorry to hear about Tony. He was part of the chorus of misfits and kooks (that included myself, Danny Lunsford, "Quantoken" – whoever that was, and, dare I say it, Lubos) that followed and commented in your blog right from the early days. His long posts were always supportive of everything up to, but not including, String Theory, and generally gave the impression that everything had been thought out carefully, however unorthodox the conclusions. Does anyone have something like a CV for him? I found a YouTube video of him talking about E8, and he is American, but beyond that, I know very little.

5. **Peter Woit**
   January 24, 2020

   Richard Gaylord,
   Thanks. Fixed.

   Alessandro Strumia,
   Thanks, but I'd encourage people who want to debate this complex issue to do so over at Scott Aaronson's blog, where he manages to do something I'm incapable of (moderating a sensible discussion of this kind of contentious issue).

6. **Geoffrey Dixon**
   January 24, 2020

   More information about Tony can be found …

https://gaupdate.wordpress.com/2020/01/20/obituary-for-frank-tony-dodd-smith-jr/

https://www.dignitymemorial.com/obituaries/cartersville-ga/frank-smith-8887608

And John Baez, I did send you an email about this some time ago, but I dare say the email I used may no longer be valid.

7. **David Brown**
   January 25, 2020

   https://www.daily-tribune.com/stories/frank-dodd-smith-jr,23384 Frank Dodd Smith Jr., obituary

London Calling with Career Opportunities II

January 27, 2020
Categories: Uncategorized

If you're a mathematician, you don't need to go work for Dominic Cummings in order to have dramatically improved career opportunities in the UK. The British government has just announced a huge increase in funding for mathematical research: 60 million pounds/year (or about \$80 million dollars) for the next five years (see here and here). To get some idea of the scale of this, note that the US GDP is about 8 times the UK's and the NSF DMS budget is about \$240 million/year. So the comparable scale of this funding in the US would be about two and a half times the NSF budget for mathematics.

Many of my mathematician colleagues have sometimes seemed to me to be of the opinion that a huge increase in funding for math research is the best way to improve a society. We'll see if this works for Britain.

While the new UK government ran on a nativist platform of restricting immigration, with the goal of keeping outsiders from taking bread out of the mouths of UK citizens, this doesn't apply to mathematicians: all limits are off and we're encouraged to flood the country. The law will be changed on Friday, changes go into effect Feb. 20. This will include an "accelerated path to settlement", no need to even have a job offer, and all your "dependents have full access to the labour market", no problem with them and the taking the bread out of the mouths of the locals thing.

Update: More here (except it's mostly behind a paywall, but evidently Ivan Fesenko is quoted).

Comments

1. **Robinson**
   January 27, 2020

   A sensible policy for sure. Mathematics is at the ground floor of so much (all, in fact) of science and technology.

2. **Peter Woit**
   January 27, 2020

   Robinson,

   Yes, and I should point out that my own work has always been aimed at progress on the foundations of math, physics, and so all science and technology. The world could thus be significantly improved by bags of cash being brought to my office on the fourth floor of the Math building. Thanks.

3. **Not Cummings**
   January 27, 2020

   You obviously are not aware of how toxic the debate has been in the UK over Brexit, otherwise you wouldn't be so silly to use the inflammatory sarcasm implied by "the goal of keeping outsiders from taking bread out of the mouths of UK citizens"

   Don't be dumb, it doesn't help.

4. **Peter Woit**
   January 27, 2020

   Not Cummings,
   Living in another Rupert Murdoch-controlled society in which toxic nativism plays a big role, inflammatory sarcasm seems as good a way to react to it as any.

5. **Chris Oakley**
   January 28, 2020

   As for anti-immigration, that platform only belongs to the far right here. The major political parties, including the governing Conservatives, only ever make pro-immigrant noises in public. Leaving the EU was, officially, more about not being bossed around by Brussels than fear of Bulgarians taking our jobs (although a native English person would probably not be prepared to work for the same money anyway …) Having large numbers of foreigners coming here to study or practise science and medicine is nothing new here. The Gates scholarships in Cambridge, for example, have attracted a lot of students from the Far East.

6. **Peter Woit**
   January 28, 2020

   Not Cummings/Chris Oakley,

   I see The Register has its own sarcastic take on this, headlining their story
   https://www.theregister.co.uk/2020/01/28/boffin_uk_visa_program/
   with
   "Boris celebrates taking back control of Brexit Britain's immigration – with unlimited immigration program
   Don't worry: The PM's only going to let the best boffins in… honest"

7. **martibal**
   January 28, 2020

   Maybe one should subtract from this "huge amount of additional money" the european fundings that UK may lost after the Brexit. Is the increase still so huge ? I think UK benefits quite a lot from the european research program (like ERC or Marie-Curie grants).

8. **Robinson**
   January 28, 2020

   If you think the UK could feed, cloth and house twenty five million low paid Indian and Chinese immigrants, make the case for completely open borders. If you don't believe in completely open borders then you have an immigration policy, and I would be interested to know what it is.

There's a general misconception in certain quarters that the UK's vote to Leave the European Union was an act of xenophobia (demonstrated here and elsewhere), when all surveys show that along with Norway the UK is one of the most welcoming countries for migrants in the whole of Europe. The "increase in hate crime" you read about is simply an increase in the recording of accusations of "hate crime", which can include being mean to someone on Twitter. 99% of these alleged crimes are never investigated. It's quite ridiculous.

The sneering and sarcasm I'm reading tells me that both here and in the US and in Europe the left has completely failed to absorb any lessons from Trump, Brexit and most recently the complete annihilation of Corbyn at the ballot box. On current trend it'll be at least one more election cycle before it starts to sink in.

9. **Peter Woit**
January 28, 2020

Robinson,
Yes, countries need immigration policies. I don't think there was anything all that wrong with the Obama-era ones here in the US, or the pre-Boris Johnson ones in the UK (which I am less familiar with).

I find it hard to believe that anyone is naive enough to believe that Trump/Johnson/Cummings are simply well-meaning public servants driven by an intense desire to work out a fair and just immigration system. I do think that most people, at least in the US, have absorbed a big lesson from Trump: dishonest pandering to xenophobia can help a lot in bringing you to power and keeping you there. The question is what to do about this lesson. The right has decided to embrace this tactic and support the demagogues using it, the left, it is true, has found no way to fight it.

Sorry, but on the hot-button immigration issues, no more here now, this kind of internet comment section debate is worthless.

10. **AcademicLurker**
January 28, 2020

How attractive are the working conditions for mathematicians (and academics more generally) in the UK these days. I haven't paid too much attention, but my impression was that UK universities have in recent years become even more ridiculously bureaucratic and metrics crazed than those in the US.

11. **Rod Deyo**
January 28, 2020

It's nice that pure mathematics will be generously funded in the UK, but will new theorems about motivic homotopy, p-adic Galois representations, or even a solution to the latest Erdos conjecture you never heard of, really solve society's economic and social problems?

At least the US is throwing money at more "applied" will 'o wisps, such as quantum computing.

12. **SnarkyButNotTotallyWrong**
January 28, 2020

Rod,

"but will new theorems … really solve society's economic and social problems?"

Perhaps! Because every highly intelligent person to whom you grant a lifetime position with a slightly better than subsistence salary and a small office stuffed with arcane books where they can live out the rest of their days pondering motivic homotopy and p-adic Galois representations in obscurity is one less highly intelligent person who could otherwise use their skills to destroy the world (witness, for example, Dominic Cummings).

13. **Anonyrat**
January 28, 2020

This webpage on UK funding of the mathematical sciences may be of interest. It has some nice charts and tables about areas of funding.

https://epsrc.ukri.org/research/ourportfolio/themes/mathematics/

14. **Peter Woit**
January 28, 2020

Anonyrat,

That's for current EPSRC math research grants. I'm curious if there's anything available explaining where the new money will be targeted. Note that the 100 million pound number seems to be for total (presumably multi-year) grant commitments. I gather many of these are grants for up to six years. The description of the new money is that it is for 300 million pounds over 6 years, so this is consistent with a truly huge increase in funding for math research grants.

Perhaps someone who actually understands these numbers can explain better what the current situation is and how it will change.

15. **Anonymous**
January 29, 2020

The £60 million/year will go to: 1) extending PhD fellowships and Research Associate positions to 4 and 5 years respectively (£19 million); 2) different kinds of grants (£34 million), and 3) extra money for specific research institutes in Bristol, Cambridge and Edinburgh (£7 million).

More details in

https://epsrc.ukri.org/newsevents/news/major-funding-boost-for-mathematical-sciences/

16. **Anonyrat**
January 29, 2020

As of 2017/18: "The current funding landscape in the UK shows significant overseas funding for Mathematical Sciences with £21.5m research income from the EU and £7.1m from other overseas sources. Together these account for 28% of the total research income to Mathematical Sciences compared with 55% from UK Research Council funding and 17% from other sources including health, industry and charities."

https://www.lms.ac.uk/sites/lms.ac.uk/files/files/LMS%20Mathematical%20Sciences%20and%20the%20Industrial%20Stategy%20designv17.pdf

17. **Amitabh Lath**
January 29, 2020

If you want to create native mathematicians maybe spend some of those $$ on K-12 math instruction? The "honors" math track in the US seems to be pre-calc in 11th grade and AP-calculus in 12th. Full disclosure, I have a kid in 11th grade on this trajectory. It's a good high school (Alan Guth's an alum!).

A few miles south there are some rich school districts that offer Calculus to 10th graders, differential equations in 11th, and real analysis in 12th. About 2-3 dozen students/yr go this track. I suspect this setup is replicating the path familiar to the asian immigrant STEM-professional parents who make up a sizable minority in these districts.

You might argue this is a waste of money, and injures students but I interview these kids for MIT and a common refrain is I didn't know what math was for until I did analysis!

18. **Peter Woit**
January 29, 2020

Anonyrat,
Nature has an article about the post-Brexit scientific relationship with the EU. In brief, nothing will change for a while, very much up in the air what they'll agree on for the future.

https://www.nature.com/articles/d41586-020-00215-0

Amit,

The new UK government's decision to make math research one of its first priorities, with emphasis on bringing in foreign researchers, is pretty unusual. More conventional would be to emphasize funding math education, with a goal of training those now in the UK to staff these research jobs. It looks like the new British government hasn't yet figured out exactly what it will do about education, see
https://www.theguardian.com/education/2019/jul/31/will-boris-johnsons-present-for-hard-up-teachers-be-enough

And, yes, for the best young math students, providing a curriculum in high schools that goes beyond basic calculus is a good idea. In the list you give, I'd replace real analysis with linear algebra.

19. **tulpoeid**
January 29, 2020

Although this will probably not appear in the comments section, I'd like to +1 opinions like Robinson's "there's a general misconception in certain quarters that the UK's vote to Leave the European Union was an act of xenophobia". It surprises me, as a scientist, to see people opposing Brexit on the ground of perceived xenophobia _and_ being totally dismissive to its supporters (in some cases as if the latter are too idiotic to even be present in online forums).

Some, or many, people voted for Brexit because of immigration. Many others did for completely different reasons. I'd never vote for remain if I were a UK citizen, as I oppose the political-financial structure that the EU has become today – therefore I believe that there are several others like me in UK right now.

But still, if we decide to focus on immigration, it'd be helpful to consider how many non-native inhabitants the UK has: the answer is *a lot*. This aspect shouldn't be disregarded, the British society shouldn't be dumped while other, actual xenophobic governments in countries where immigrants are suffering, are made to look angelic in comparison.

20. **Peter Woit**
January 29, 2020

tulpoeid,
I've no interest in debating the substantive issues around Brexit. I'm not that well-informed and it's not my country. A couple explanations though of why I have referred (with "inflammatory sarcasm") to the Cummings/Brexit/immigration issue.

1. While I'm not so well informed about the UK, I'm very well-informed about the US, where Trump and his enablers have very successfully exploited dishonest appeals to xenophobia in order to come to power. We've seen a lot of this here, with surely a lot more to come before November. The little I've seen of the UK media coverage of the immigration issue there smells exactly the same as the garbage we're subjected to here by Fox and Trump.

2. Cummings has played a central role in this. For those not familiar with the story, see for instance
https://www.theguardian.com/commentisfree/2019/jan/08/vote-leave-racism-brexit-uncivil-war-channel-4
which covers some of the story of the dishonest but effective way Cummings used his talents at Vote Leave to get out the xenophobe vote.

I don't think Cummings/Johnson are xenophobes themselves, but I do think Cummings is a master of the new technology of social control. Now that he's more or less in charge of the country, perhaps he'll use his talents for good, not evil. But best to be aware of the history of your new masters. Making an honest case to the public is not their thing.

21. **Dan F.**
January 30, 2020

Only about half of US federal funding for mathematical sciences comes from the NSF. The rest comes from the DOD, DOE, etc., and the real funding levels for mathematical sciences are somewhat higher than you indicate.

22. **David Schaich**
January 30, 2020

Although this is made clear in the links provided above, it may be worth emphasizing here that the "mathematical sciences" to be supported by this windfall are broadly defined in the UK, ranging from proving theorems to engineering and "Innovative Manufacturing". The funding is going to the Engineering and Physical Sciences Research Council (EPSRC), which does what it says on the tin. While my view may be distorted by the funding calls that come to my attention, I expect quantum technologies (uknqt.epsrc.ac.uk), data science, machine learning and artificial intelligence to be major beneficiaries (among others, of course).

One complication is that most nuclear/particle/astro/cosmology research—especially on the experimental side—is funded by a different Research Council, the Science and Technology Facilities Council (STFC). EPSRC and STFC are doing some joint funding of quantum simulation research, and each funds their own national supercomputing facilities (ARCHER and DiRAC, respectively). My area of lattice field theory is mostly STFC-supported, but can go after some EPSRC funding by focusing on data science and high-performance computing aspects. Similarly, string theory is mostly STFC-supported, but can go after some EPSRC funding by focusing on 'mathematical physics' aspects. I'm told that EPSRC support for string theory (and anything else that looks nuclear/particle/astro/cosmology) has dwindled over the past decade, but it may rebound now that they have all this extra cash to spend.

Regarding AcademicLurker's comment, my experience is indeed that UK universities are more bureaucratic and metrics-focused than those in the US, with the caveat that I was only a student and postdoc in the US, which shielded me from the bulk of the bureaucracy there. Salaries also tend to be lower in the UK, but on the plus side there's not a tenure gauntlet.

23. **tulpoeid**
January 31, 2020

    I see, that link about the brexit campaign seems quite illuminating (and takes into account that not everyone anti-present-EU is a racist).

24. **theoreticalminimum**
February 6, 2020

    Well, someone else has a take on Cummings: https://www.theguardian.com/politics/2020/feb/06/inside-the-mind-of-dominic-cummings-brexit-boris-johnson-conservatives

25. **Art**
February 18, 2020

    Oh dear. Respect for science devolves into advocacy of racism. Cummings, meet Schockley…

    https://www.theguardian.com/politics/2020/feb/17/boris-johnson-adviser-quits-over-race-and-eugenics-writings

Various

February 3, 2020
Categories: Uncategorized

- A few months ago I ended up doing a little history of science research, trying to track down the details of the story of the Physical Review's 1973 policy discouraging articles on "Foundations". The results of that research are in this posting, where I found this explanation from the Physical Review editor (Goudsmit) of the problem they were trying to deal with:

  > The event shows again clearly the necessity of rapid rejections of questionable papers in vague borderline areas. There is a class of long theoretical papers which deal with problems of interpretation of quantum and relativistic phenomena. Most of them are terribly boring and belong to the category of which Pauli said, "It is not even wrong". Many of them are wrong. A few of the wrong ones turn out to be valuable and interesting because they throw a brighter light on the correct understanding of the problem. I have earlier expressed my strong opinion that most of these papers don't belong in the Physical Review but in journals specializing in the philosophy and fundamental concepts of physics.

  I had heard that people studying foundations of quantum mechanics, frustrated by this policy, had started up during the 1970s their own samizdat publication, called "Epistemological Letters". I tried to see if there was any way to read the articles that appeared in that form, but it looked like the only way to do this would be to go visit one or two archives that might have some copies. Unbeknownst to me, around the same time Notre Dame University had just finished a project of scanning all issues of *Epistemological Letters* and putting them online. They are now available here, with an article about them here and an introductory essay here.

- There's an interesting essay on the arXiv about the current state of BSM physics, by HEP theorist Goran Senjanović, entitled Natural Philosophy versus Philosophy of Naturalness.
- Here's an article about problems string theorist Amer Iqbal has been having in Pakistan.
- The New York Times has an article about Cedric Villani and his campaign for mayor of Paris. The election is next month, and I'm having a hard time figuring out why Villani is running. There doesn't seem to be a lot of difference in policy views between the current mayor (Hidalgo) and the Macronistas (Griveaux and Villani), with the main effect of Villani entering the race a splitting of the Macron party vote.
- I was sorry to hear recently about the death of mathematician Louis Nirenberg. Kenneth Chang at the New York Times has written an excellent obituary. Terry Tao has some comments here.

Update: Excellent rant on Twitter from Philip Ball about misrepresentations of the Copenhagen interpretation. For your own rants, please engage in them on Twitter rather than here.

Comments

1. **Akhil**
   February 4, 2020

   Peter,
   Do you have anything to disagree with Goran Senjanović paper?

2. **Peter Woit**
   February 4, 2020

   Akhil,

   I mostly agree with the paper. As Senjanović points out, the problems with SUSY models/GUTs/naturalness were all known pre-LHC. It's interesting to see that the LHC results are now causing people to recognize these problems.

   I agree with him that the neutrino sector of the SM is now the part where there are mysteries that experiments might soon shed light on. I disagree about left/right symmetry, the left/right asymmetry I think is a fundamental feature of space-time.

3. **Peter Woit**
   February 4, 2020

   By the way, I just noticed that the new issue of Symmetry is out, with first article "Fine-tuning versus naturalness" that starts with the sentence:

   "When physicists saw the Higgs boson for the first time in 2012, they observed its mass to be very small: 125 billion electronvolts, or 125 GeV."

   Note that there's something obviously very peculiar about this. Since 125 GeV is higher mass than any other state known except the top quark, an equally good first sentence would have been:

   "When physicists saw the Higgs boson for the first time in 2012, they observed its mass to be very large: 125 billion electronvolts, or 125 GeV."

   What the author doing is assuming that the right way to measure masses is in terms of a GUT scale (there is no evidence at all for GUTs), or the Planck scale supposed to be relevant to quantum gravity (we don't know what quantum gravity is).

4. **Janko Frazzle**
   February 4, 2020

   I'm disappointed with Cedric Villani, he should know better. To be a great mathematician you need to have an interest in the subject from early childhood, and the same is with politics.

5. **Peter Woit**
   February 4, 2020

   Janko Frazzle,
   The French have in the past had political leaders who were first-rate mathematicians (e.g. Paul Painlevé). As for Villani, if you read his memoir you'll see that he consciously set out to win a Fields Medal, and successfully did so. With that track record, I wouldn't discount his prospects for achieving whatever his next goal is.

6. **Peter Shor**

February 4, 2020

It seems to me that the comment

> "Most of them are terribly boring and belong to the category of which Pauli said, "It is not even wrong". Many of them are wrong.

probably was true. Quantum foundations is a very attractive area for cranks, most of whose papers would be wrong or "not even wrong". On the other hand, throwing out all these papers, even those by lesser-known bona fide physicists[1], seems to me to be a mistake on the order of a math journal saying that, because they have been deluged by a flood of incorrect papers proving the Riemann hypothesis, that they will reject all such papers without even looking at them.

On the other hand, most of the cranks probably didn't even known that *Epistemological Letters* existed, effectively pre-screening submissions to it.

[1] Surely they wouldn't have rejected a foundations paper by Feynman without looking at it first.

7. **Peter Woit**
February 4, 2020

Peter Shor,
When I looked into the actual story, see
https://www.math.columbia.edu/~woit/wordpress/?p=11263
I found that it was just not true that Phys Rev had a "no foundations" policy. The supposed "no foundations" policy was announced here
https://journals.aps.org/prd/pdf/10.1103/PhysRevD.8.357
which starts off with explaining the reason for the policy:
"We occasionally receive a manuscript for which it is extremely difficult and sometimes impossible to find a suitable referee who is willing to read it."
This clearly would not a apply to a paper from Feynman…

Goudsmit later gave a specific example of why they instituted the policy, this paper:
https://journals.aps.org/prd/abstract/10.1103/PhysRevD.8.1679
In his archives you can find a long discussion of the difficulties they had trying to get this paper properly refereed.

8. **Suomynona**
February 5, 2020

Peter Shor,
My understanding of Feynman's work is that he was uninterested in epistemological issues, so it's unlikely he would have submitted something which violated the policy anyway.

Another note on Goudsmit is that he also had a similar policy on the other end of the spectrum, for papers perceived as too much about technological or engineering developments. Apparently some of the foundational papers developing the physics of the laser were rejected without review as the author described their work as on the "optical maser" (at the time, masers were widespread laboratory instruments).

9. **Felipe Pait**
February 5, 2020

Part of the reason why Villani is running may be that he can. The election in Paris is in 2 rounds, which means that a candidate that doesn't win can 1) gain name recognition; 2) bargain support in the 2nd round; and 3) not be tainted as a spoiler for splitting the vote.

It is somewhat like a primary in the US, full of candidates that can at most hope to get a spot as vice president or in the next round of elections.

10. **Peter Shor**
February 5, 2020

Suomynoma: Actually, Feynman was interested in epistemological issues (he was interested in everything); I assume he never published anything about it because he never figured out anything interesting and new to say about them.

When I was at Caltech as an undergrad, Feynman gave a talk where he explained that he had looked at Bell's theorem, trying to find hidden hypotheses. The hidden hypothesis he identified was that all probabilities had to be between 0 and 1, so he was trying to figure out whether negative probabilities could get around Bell's theorem.

He later published a paper about negative probabilities, but it didn't solve the difficulty with local hidden variable theories, and his motivation for looking at the question was only mentioned very briefly in it.

11. **Blake Stacey**
February 5, 2020

An interesting discovery from looking into the history last September was that Goudsmit actually declared a subject *non grata* three times in its first decade of PRL: masers in 1959, the Mössbauer effect in 1960 and gauge theories in 1965. In all these cases, the motivation was that too many mediocre, trend-following papers were being received, and so new ones would only be considered if they stuck to a high standard of concreteness. The bit of lore that they rejected the first article on lasers because they thought it was just another maser paper might or might not be true.

12. **Anon**
February 6, 2020

Hi Peter — The observed light neutrino masses (m_nu) suggest a heavy right handed neutrino mass scale ~ v_weak^2/m_nu which works out to be about 10^14 GeV when we put in v_weak~200 GeV, m_nu = 0.5 eV.

The above is simple dimension analysis — we can get large mass scale from two mass scales, and the physical reasoning behind this is the well known seesaw mechanism. Now 10^14 GeV is beyond reach of colliders. So if we want the large mass or new physics scale to understand the origin of neutrino masses to be at the TeV scale we need to multiply 10^14 GeV by a dimensionless small number ~10^{-11}. Such a small prefactor can physically be there if the Dirac type Yukawa couplings of the neutrinos are ~ sqrt{10^{-11}} = 10^{-5.5}.

Now we can argue that there is no reason why such a small prefactor cant be there in nature and search for the physics of the seesaw mechanism (right handed currents, neutrinos etc) at the TeV scale. But really is this physics more likely to be there at the LHC or even the next collider or is this physics more likely to be at 10^{14}GeV?

Naturalness arguments have generally worked and therefore the faith that multiplying by a small prefactor and getting the answer we want (namely reachable by LHC) is not likely to pay big dividends.

The Higgs mass seems to be fine-tuned and naturalness arguments appear to not work in this case, but naturalness is still a very useful guide (in experience of science over generations it usually works) and if the neutrino mass physics is indeed at 10^14 GeV then better to start thinking of what experiments can provide evidence for something at that scale (it could be other precision tests) rather than only working on LHC/TeV scale physics for right handed or B-L breaking scale.

Of course those guided by the idea that it is best to work at the TeV scale would also be doing great work because maybe there is a prefactor of 10^{-11} in the above. LHC or the next collider may help prove there isn't (most probable outcome aka nightmare scenario) or if it is there there that would be a remarkable discovery.

I dont see why people cant purse both approaches — or as a community we cant pursue both — trying to find BSM physics at TeV scale though there is no real reason or natural reason to expect one, as well as think of innovative ways to probe our theories even if their mass scale is out of reach of the LHC and conceivable future colliders.

13. **Peter Woit**
February 6, 2020

Anon,

The argument against usual Dirac mass-terms for neutrinos is that the ratio of electroweak scale to neutrino mass would then be unexpectedly large ("unnatural"). But we already know that the ratio of electroweak scale to the electron mass is very large (like 400,000). To me this just makes the point that we don't have any idea where the Yukawas come from, and not only no good reason to expect them to be order one, but plenty of evidence that there's an an unknown reason for them to be very small. I understand the usual seesaw argument, I just don't think it's all that strong an argument for a new physics mass-scale.

My earlier comment about possible new results about neutrinos was mainly a reference to non-collider experiments (neutrinoless double beta decay, sterile neutrinos). The speculative idea of a new high mass scale to explain neutrino masses doesn't affect my later comment that saying the Higgs mass is "small" makes no sense, since you should be comparing it to scales you know exist, not speculative ones.

14. **Amitabh Lath**
February 6, 2020

I never thought of naturalness as anything more than handwaving guesses. Sort of like Enrico Fermi's guess at the number of piano tuners in Chicago. Imagine if after Fermi made the guess they walked around looking for piano tuners and didn't find any. Maybe there is something suppressing piano tuners. Maybe they can't afford to live in Hyde Park and all moved to Englewood. Maybe people prefer woodwinds and gave up piano. Or maybe the piano tuners are there but you can't see them because they look like normal people.

Goran Senjanović's assertion that naturalness is some deep philosophical choice seems too harsh. Naturalness is what you do when you don't really have any other way.

Also, the bit about LHC experiments having a SUSY group and separate exotica group is true, but it's not because of we think SUSY is somehow better than. Essentially we sort physics signatures that have large missing momenta (MET) into the SUSY group, and everything else into the exotica group. I work on R-parity violating SUSY and that's in the exotica group because no MET.

15. **Anon**
February 6, 2020

Well we do know that the charged lepton Yukawa couplings of tau, mu and electron are similar to the bottom, strange and down quarks' Yukawa couplings. That is a good reason to expect the Dirac Yukawa couplings of nu_tau, nu_mu, nu_e to be similar to top, charm and up quarks' Yukawa couplings respectively.

Of course, it is not a proof but is a strong motivation to do further research with this thinking.

One can also, of course, assume as you seem to prefer, that the Dirac Yukawas of all three neutrinos are very very small and the light neutrino masses are all Dirac type, and nature has an exact B-L symmetry that prevents the Majorana mass term in the SM with the addition of 3 right-handed neutrinos.

16. **Goran**
February 7, 2020

Some comments on the comments on my essay. When I give an example of the LR symmetric theory, claiming it is a self-contained, predictive model of the origin of neutrino mass (analog to Higgs-Weinberg mechanism for charged fermion masses), I do not want to convince the reader of my starting point, I am only asking her or him (read Peter in this case 🙂 to appreciate the structural correlations that follow unambiguously. Moreover, it was the LR theory that led originally to neutrino mass long before experiment (many were telling me in the seventies that I was wasting my time – neutrino was supposed to be massless to most).

All I am doing in my essay is to advocate that high energy physics goes back to being natural philosophy in the Newton's sense of the world. You may be as philosophical as you wish, but you must make predictions and let the experiment decide, period. Which is why I argue against the obsession with naturalness as the main guiding principle for the BSM physics. Simply, it failed to produce an analog self-contained framework with verifiable predictions, and instead became a never ending game whose rules can change as and when one wishes.

17. **Goran**
February 7, 2020

Regarding the scale of seesaw. Seesaw by itself is just a scenario which basically says nothing – you are supposed to accept that a particle at unreachable energies 'explains' the smallness of neutrino mass? I argued, albeit briefly (I cite relevant papers for in-depth analysis) that a way to new physics and information about its scale ought to come through a well defined theory and experiment. In this case it is neutrinoless double beta decay which may be caused by neutrino Majorana mass or new physics behind it (or both). If the former explanation fails, say because we learn meanwhile that the hierarchy is normal, or even better, if electrons coming out were say RH, then we would need new physics. Simple dimensional analysis tells you then new physics must lie at energies not far from the LHC – and in the LR theory it is really hard to go above the 6 TeV LHC reach for the mass of the RH gauge boson. In other words, it is experiment and not wishful thinking that should (and can) decide the whether or not there is a small coupling in the Weinberg operator. Btw, the small coupling is rather natural in the LR theory where neutrino and electron live together – the 10^{14} GeV cut-off requires neutrino Dirac mass on the order of the top quark mass.

This said, if neutrino hierarchy ends up being inverse, or better, if you saw the neutrinoless double beta decay with LH electrons, I for one would, admittedly sadly, be first to admit no reason for new physics behind neutrino mass at reachable energies.

18. **Kris Krogh**
February 7, 2020

Peter,

I would guess you've never read John Bell's book, *Speakable and Unspeakable in Quantum Mechanics*, published in 1987. It's a compilation of his papers on quantum mechanics, each of them remarkably insightful. The majority were never published in mainstream journals, and several are from *Epistemological Letters*. That was definitely a reflection of the politics faced by Bell and his collaborators, as suggested by the book's title. Although the publishing situation is different now, those politics are still not behind us.

19. **Peter Woit**
February 7, 2020

Kris Krogh,
I have looked at that book, and read some of the articles in it. From what I remember I was put off by Bell's fondness for Bohm/hidden variables, and didn't find what I was reading helpful in terms of providing any insight into what seems to me the main problem (emergence of the classical from quantum).

20. **tulpoeid**
February 23, 2020

[I tried to put this under more relevant posts but they're closed.]
FYI, from a new interview with Jean Iliopoulos of GIM-mechanism fame (and well-known susy fan):
"What makes you be so sure about the existence of supersymmetry?"
"I am not."

https://www.tovima.gr/printed_post/to-pythagoreio-theorima-den-exei-ethnikotita-lfoute-thriskeia/

Robert Hermann 1931-2020

February 15, 2020
Categories: Obituaries

I was sorry to hear today of the recent death of Robert Hermann, at the age of 88. While I unfortunately never got to meet him, his writing had a lot of influence on me, as it likely did for many others with an overlapping interest in mathematics and fundamental physics. Early in my undergraduate years during the mid-1970s I first ran across some of Hermann's books in the library, and found them full of fascinating and deep insights into the relations between geometry and physics. Over the years I've often come back to them and learned something new about one or another topic. The main problem with his writings is just that there is so much there that it is hard to know where to start.

While the relations between Riemannian geometry and general relativity were well-understood from Einstein's work in the beginning of the subject, the relations between geometry and Yang-Mills theory were not known by Yang, Mills or other physicists working on the subject during the 1950s and 1960s. The understanding of these relations is conventionally described as starting in 1975, with the BPST instanton solutions and Simons explaining to Yang at Stony Brook about fiber bundles (leading to the "Wu-Yang dictionary" paper). But if you look at Hermann's 1970 volume Vector Bundles in Mathematical Physics, you'll find that it contains an extensive treatment of Yang-Mills theory in terms of connections and curvature in a vector bundle. While I don't know if Hermann had written about the sort of topologically non-trivial gauge field configurations that got attention starting in 1975, he had at that point for a decade been writing in depth about the details of the relations between geometry and physics that were news to physicists in 1975.

Being ahead of your time and mainly writing expository books is unfortunately not necessarily good for a successful academic career. Looking through his writings this afternoon, I ran across a long section of this book from 1980, entitled "Reflections" (pages 1-82). I strongly recommend reading this for Hermann's own take on his career and the problems faced by anyone trying to do what he was doing (the situation has not improved since then).

A general outline of his early career, drawn from that source is:

1948-50: undergraduate in physics, University of Wisconsin.
1950-52: undergraduate in math, Brown University.
1952-53: Fulbright scholar in Amsterdam.
1953-56: graduate student in math, Princeton. Thesis advisor Don Spencer.
1956-59: instructor at Harvard ("Harvard hired me as an instructor in the mistaken belief that I must be a topologist since I came from Princeton").
1953-59: "My *real* work from 1953-59 was studying Elie Cartan!"
1959-61: position at MIT Lincoln Lab, taught course at Berkeley in 1961.

Hermann ultimately ended up at Rutgers, which he left in 1973, because he was not able to teach courses there in his specialty, and felt he had too little time to conduct the research he wanted to work on. It appears he expected to get by with some mix of grant money and profits from running a small publishing operation (Math Sci Press, which mainly published his own books). The "Reflections" section of the book mentioned above also contains some of his correspondence with the NSF, expressing his frustration at his grant proposals being turned down. At the end of a letter from late 1977 (which was at the height of excitement in the physics community over applying ideas from geometry and topology to high energy physics) he writes in frustration:

> However, when I look in the *Physical Review* today, all the subjects which people in your position so enthusiastically supported ten years ago are now dead as the Phlogiston theory – and good riddance – while the topics I was working on then are now everywhere dense. Does one get support from the NSF by being right or by being popular?

John Baez has written something here, and there's an obituary notice here.

Update: I've been reading some more of the essays Hermann published in the "Reflections" section of this book. Especially recommended is the section on Mathematical Physics of this 1979 essay (pages 30-38). His evaluation of the situation of the time I think was extremely perceptive.

Update: For more about Hermann, see some of the comments at this old blog posting. Also, on the topic of his book reviews, see this enthusiastic review of the Flanders book *Differential forms with applications to the physical sciences*.

Update: For an interesting review covering many of Hermann's books, at the Bulletin of the AMS in 1973, see here.

Comments

1. **Geoffrey Dixon**
   February 15, 2020

   RH advised me for a time when I was a math grad student at Rutgers. Later, as a grad student in physics at Brandeis, I moseyed into Cambridge in hopes of sounding out Glashow on my division algebra ideas. I was allowed to speak with Sheldon, but he confessed that abstract math was not his specialty, and I left shortly thereafter somewhat flustered. I ran into Robert Hermann in the science building cafe after I left and we sat together. I had just started explaining my ideas to him when Sheldon walked by. He spotted us, came over, and said that Robert was just the person I should be talking to. Of course, he was right. Sheldon sat with us, and I lost what little coherence I had left and made a poor showing of myself until I left a little while later. That, unfortunately, was the last interaction I remember having with Robert H, a really good guy. Had I known he was in town I would have gone straight to him. Alas … (About 18 years later I taught an undergrad course in particle physics with Sheldon G at Harvard; also a really good guy.)

2. **xyz abc**
   February 16, 2020

   Every time I think of Hermann, I always tend to relate him to Spivak, in view of their unique and somewhat idiosyncratic career paths.

   Many things puzzled me over the years about Hermann, one of which being what is mentioned in this post. Namely how he did not end up getting due credit for having attempted to drawn connections between gauge field theory and differential geometry on fibre bundles even before Wu-Yang-Simons. I once heard rumors that I. M. Singer had actually noted Hermann's contributions in this area.

   Another strange anecdotal mystery to me is Hermann's relationship with Sternberg. "Being critical" is perhaps an understatement about his BAMS review of Sternberg's 1964 book, in which Hermann was in fact acknowledged by the author. (In all fairness though, Hermann's book review was safely towered in its acrimonious level by Barry Mitchell's on "Module Theory" written by a romannian mathematician.) In a subsequent year, Hermann appeared to explain at pains in one of his papers on Cartan's EDS how his work was independent but perhaps no less valuable than the opus published at a slightly earlier time by Singer and Sternberg.

   If one were to be pessimistically cynical, one would likely not be surprised at the tremendous amount of latent ego right underneath the thin skins of apparently humble mathematicians.

3. **Peter Woit**
February 16, 2020

xyz abc is referring to this 1965 review
https://www.ams.org/journals/bull/1965-71-02/S0002-9904-1965-11286-1/
I don't know what relations were like between Sternberg and Hermann, note that Hermann left Harvard (and says he wasn't very happy there) about the time Sternberg arrived (1959).

It's interesting to compare this review to this 1982 one of a Hermann book
https://www.ams.org/journals/bull/1982-06-03/S0273-0979-1982-15019-4/S0273-0979-1982-15019-4.pdf

which is also a quite critical review. Hermann and Sternberg shared many of the same interests, as well as each writing quite a few expository books. While in many ways different, their books to me seem to share some similar features. These include a mixture of beautiful, deep and insightful exposition of material not available elsewhere (although sometimes very demanding of the reader), together with some idiosyncratic material of less value. Maybe this is generically what happens if you spend a lot of your life absorbing the ideas of Elie Cartan, and then start writing a lot of books…

4. **James Hart**
February 20, 2020

I just read the recommended selection from his book "Reflections" and I am more than a little impressed at his insights, at his clarity of thinking, and just how well he understood the problems our fields were facing. (I have a PhD in physics, but a strong interest in mathematics for its own sake and have given myself a crude first-year course in University mathematics via textbooks and supplementary online reading. It's no replacement for a real degree, but it makes me less of an idiot when dealing with reading things like that book.)

5. **D. F.**
February 21, 2020

Hermann's review of Sternberg's book is positive, bordering on enthusiastic. I'm not sure how anyone reading it could conclude that phrases such as "In this chapter Sternberg gives us, with great expository skill and taste, a glimpse of vast research areas …" are anything else. Criticisms are made, but they are constructive, not tendentious, and they are offered in a friendly way, not a destructive one.

6. **Peter Woit**
February 21, 2020

D.F.,
The summary is
"In summary, this is a book that contains much useful material and that is, in general, well written, but that is marred by inattention to detail."
but, in any case, I hadn't heard that there was any particular problem between Sternberg and Hermann.

Hermann made public some of the negative grant referee reports, and they seem to me to reflect not personal hostility but an unsurprising negative take on a proposal to fund someone not playing by the conventional rules (i.e. quitting his job and writing not journal articles but self-published mainly expository books).

7. **Anonyrat**
February 22, 2020

In the beginning pages of the Hermann link above, Hermann must have pounded on Kac's "dehydrated elephants" half a dozen times. What exactly was all that about?

8. **Peter Woit**
February 24, 2020

Anonyrat,
He was referring to this essay by Mark Kac (of Rockefeller University, where Hermann had been for a while)

https://www.jstor.org/stable/43636233

It's pretty much a ferocious attack on the sort of mathematical physics practiced by Hermann, starting off with "it seems self-evident that mathematics is not likely to be much help in discovering laws of nature". Like Hermann, I strongly disagree (but don't want to start here the same argument about this that has often featured on this blog, most recently over Sabine Hossenfelder and "Lost in Math").

Kac's specialty was probability and analysis. He had little interest in geometry, even as mathematics, so not surprising he didn't see it as important for physics. Note that the essay was written in 1972, same year Weinberg's gravitation textbook was published, which also took the attitude that geometry was not important for physics, writing "the passage of time has taught us not to expect that the strong, weak, and electromagnetic interactions can be understood in geometrical terms"

This didn't age well, with 1973 seeing the triumph of gauge theory, the mid-late 1970s the dominance of ideas about geometry and topology in the subject, and by the 1980s Weinberg himself was working on such things, see discussion here
https://www.math.columbia.edu/~woit/wordpress/?p=529

So, the historical context for Hermann's comments about this was that he was living through a period (late 1970s) of vindication of his point of view, a few years after Kac's attack on it.

9. **S**
February 24, 2020

I'm very sorry to hear this. Finding an old, beautiful paper of Hermann was key to my being able to get over an obstacle in my thesis. I always hoped to contact him and thank him, and even wrote an email or two trying to get his address, but none of it led anywhere, and I'm sorry I wasn't more persistent. I now have a number of his books, and look forward to mining them for insights. (I had the opportunity to own a very large number of his books, but unfortunately, the amount of shelf space they would take was ponderous, so I took only those that seemed best.)

RIP.

10. **Peter Orland**
February 25, 2020

I bought a copy of Hermann's "Lie Algebras and Quantum Mechanics" as an undergraduate at UC Berkeley, back when pterodactyls blotted out the sun. I learned a lot of basic ideas from the book (although there was significant overlap with other books I was reading at that time). There is a good discussion of affine Lie algebras and Schwinger terms, although issues concerning regularization (which is the hardest part of QFT) are not really discussed. From my current standpoint I'd characterize the book as very broad in a good way, but not especially deep. Nonetheless, for a first exposure to the topics, it was accessible and fun.

11. **Peter Woit**
    February 25, 2020

    Thanks Peter,
    That's one book of Hermann's I haven't looked at but should. The topics you mention got a lot of attention in the 1980s. That Hermann was writing about them in 1969 is yet another example of him being way ahead of his time.

    I just ran into an interesting review from 1973 that covers many of his books, will add a link as an update.

Why String Theory Is Both A Dream And A Nightmare (as well as a swamp…)

February 26, 2020
Categories: Swampland, Uncategorized

Ethan Siegel today has a new article at *Starts With a Bang*, entitled Why String Theory is Both a Dream and a Nightmare. For the nightmare part, he writes:

> its predictions are all over the map, untestable in practice, and require an enormous set of assumptions that are unsupported by an iota of scientific evidence.

which I think just confuses the situation, which could be much more accurately and simply described as "there are no predictions". The fundamental reason for this is also rather simply stated: the supposed unified theory is a theory in ten space-time dimensions, and no one has figured out a way to use this to get a consistent, predictive model with four space-time dimensions. If you don't believe this, try watching the talks going on in Santa Barbara this week, which feature, after 17 years of intense effort, complete confusion about whether it is possible to construct such models with the right sign of the cosmological constant.

Siegel gets a couple things completely wrong, although this is not really his fault, due to the high degree of complexity and mystification which surrounds the 35 years of failed efforts in this area. About SUSY he writes

> For one, string theory doesn't simply contain the Standard Model as its low-energy limit, but a gauge theory known as N=4 supersymmetric Yang-Mills theory. Typically, the supersymmetry you hear about involves superpartner particles for every particle in existence in the Standard Model, which is an example of an N=1 supersymmetry. String theory, even in the low-energy limit, demands a much greater degree of symmetry than even this, which means that a low-energy prediction of superpartners should arise. The fact that we have discovered exactly 0 supersymmetric particles, even at LHC energies, is an enormous disappointment for string theory.

Like everything else, there's no prediction from string theory about how many supersymmetries will exist. The special role of N=4 supersymmetric Yang-Mills theory has nothing to do with the problem of low energy SUSY, instead it occurs as the supposed dual to a very special 10d superstring background (AdS5 x S5). This is of interest for completely different reasons, one of which was the hope that this would provide a string theory dual to QCD, allowing use of string theory not to do quantum gravity, but to do QCD computations. This has never worked, with one main reason being that it can't reproduce the asymptotic freedom property of QCD. Siegel tries to refer to this with

> And when you look at the explicit predictions that have come out for the masses of the mesons that have been already discovered, by using lattice techniques, they differ from observations by amounts that would be a dealbreaker for any other theory.

including a table with the caption

> The actual masses of a number of observed mesons and quantum states, at left, compared with a variety of predictions for those masses using lattice techniques in the context of string theory. The mismatch between observations and calculations is an enormous challenge for string theorists to account for.

He's getting this from slide 31 of a talk by Jeff Harvey, but mixing various things up. The table has nothing to with lattice calculations, those are relevant to the other part of the slide, which is about string theory predictions for pure (no fermions) QCD glueballs. These are not physical objects, thus the comparison to lattice computer simulations, not experiment. The table he gives is from here and about real particles. The "predictions" are not made as he claims "using lattice techniques in the context of string theory." There are no lattice techniques involved.

Normally Siegel does a good job of navigating complex technical subjects. The subject of string theory is now buried in a huge literature of tens of thousands of papers over forty years with all sorts of claims, many designed to obscure the fact that ideas haven't worked out. It's fitting that the name chosen for the kind of discussions going on at Santa Barbara this week is "The String Swampland". String theory verily is now deep in a trackless swamp…

Comments

1. **Simon Coveney**
   February 26, 2020

   Isn't string theory a mathematical framework, and the study of the landscape/swampland the study of the mathematical space of string theories, akin to model theory or topos theory in the foundations of mathematics to study the space of mathematical theories?

2. **Peter Woit**
   February 26, 2020

   Simon Coveny,
   No, not even slightly.

3. **Low Math, Meekly Interacting**
   February 27, 2020

   Have you reached out to Ethan? Attacking factual inaccuracies can serve as a useful rhetorical cudgel for those who seek to make fallacious arguments against an otherwise sound message. The article would certainly benefit from corrections for a number of reasons.

4. **Peter Woit**
   February 27, 2020

   LMMI,
   I don't think there's anything particularly wrong with the overall point of view of Siegel's piece. On the details of the problems of string theory he gets some things wrong, but on the whole, almost all non-technical articles about string theory get things wrong. The subject really has become a swamp that any non-expert will quickly get lost in. If I spent more time on this, trying to get people to fix things, it would be a thankless and pointless full-time job (look up "Augean Stables"…).

   I don't think string theorists will even bother to attack Siegel for this, especially since the correct statements are even worse for them (string theory doesn't predict N=4 SUSY since it predicts nothing). The current main tactic of string theory proponents is to just do their best to ignore criticism and try and pretend it doesn't exist. They're not likely to do anything that will get Siegel's piece more attention.

5. **Matthew Leifer**
   March 1, 2020

“Normally Siegel does a good job of navigating complex technical subjects”

No he doesn’t. As soon as he leaves his home territory of astrophysics he usually makes major errors. I have even made finding the factual errors in one of his posts into an exam question in the past.

This Week's Hype

February 28, 2020
Categories: This Week's Hype

Sabine Hossenfelder already has this covered, but I wanted to add a few comments about this week's hype, a new article in Quanta magazine by Philip Ball entitled Wormholes Reveal a Way to Manipulate Black Hole Information in the Lab (based on this paper). It's the latest in a long tradition of bogus claims that studying relatively simple quantum systems is equivalent to studying string theory/quantum gravity. For an example from ten years ago, see here. The nonsensical idea back then (which got a lot of attention) was that somehow studying four qubits would "test string theory".

A first comment would be that this is just profoundly depressing, because Ball is one of the best and most sensible science writers around (see my review of his excellent recent book on quantum mechanics) and Quanta magazine is about the the best semi-popular science publication there is. If this article were appearing in any one of the well-known examples of publications that traffic in misleading sensationalism, it wouldn't be surprising and would best be just ignored.

Hossenfelder has pointed out one problem with the whole idea (we don't live in AdS space), but a more basic problem is the obvious one pointed out by one of the first commenters at Quanta:

> In the end, if an experiment is performed based on standard quantum mechanics, and verifies standard quantum mechanics as expected, then it is irrelevant that this aspect of standard quantum mechanics might be analogous to a vaguely-formulated and incomplete speculative idea about spacetime emergence — nor can it provide any experimental support whatsoever for that idea.

I understand that, for science journalists hearing that a large group of well-known physicists from Google, Stanford, Caltech, Princeton, Maryland and Amsterdam has figured out how to study quantum gravity in the lab (by teleporting things from one place to another via traversable wormholes!!), it's almost impossible to resist the idea that this is something worth writing about. Please try.

Update: Philip Ball responds here.

Update: More from Philip Ball (and, if it appears, a response from me) at the Quanta article comment section, ~~comments from one of the paper's author's~~ also comments here.

Update: Commenter Anonyrat points out that the Atlantic is republishing this piece, as A Tiny, Lab-Size Wormhole Could Shatter Our Sense of Reality: How scientists plan to set up two black holes and a wormhole on an ordinary tabletop.

Update: In the future, I hope to as much as possible outsource coverage of this kind of thing to the Quantum Bullshit Detector. Today, see for instance this.

Comments

1. **Sabine Hossenfelder**
   February 28, 2020

   Thanks for the link. It won't be long until the headlines say we can test string theory on a quantum computer.

2. **Peter Woit**
   February 28, 2020

   Sabine,
   I think that's just too old hat now. The time evolution has been:

   1990s: We can test string theory/quantum gravity at the LHC!
   Early 2000s: We can test string theory/quantum gravity at RHIC! We're creating black holes at RHIC!
   Late 2000s: We can test string theory/quantum gravity in condensed matter experiments! We're creating black holes in our condensed matter lab!
   2010s-now trend: The landscape has explained it all, who cares anymore about boring string theory? We now can test quantum gravity in atomic physics experiments! And we're creating multiple universes when we do it! And we're creating not just black holes but wormholes! And the wormholes are traversable and we're teleporting through them!!!!!!

   Looks to me like we're just months away from the physics hype singularity…

3. **Low Math, Meekly Interacting**
   February 28, 2020

   Quanta, the best new pop-sci outlet…

   I hardly knew ye.

4. **Peter Shor**
   February 28, 2020

   Reading the Quanta article, it looks like their experimental proposal is to *do* quantum teleportation (of a particularly complex kind) and *call it* a traversable wormhole by invoking the non-existent ER-EPR correspondence.
   This seems to me to be an ingenious way to address the objection that AdS-CFT is untestable in the real world.
   The original article is very difficult to parse, so I haven't read it yet, but I'd love to hear from somebody who believes that this isn't what they're proposing.

   I quote the Quanta article:

   > "The role of the coupling is to send the essential classical data, which, with the aid of entanglement, teleports the signal from one black hole to another," [Nezami] said.
   > This, at least, is how a quantum information theorist would view the process. But according to the AdS/CFT correspondence, the channel between the black holes created by entanglement is equivalent, in a description based on general relativity, to a wormhole in space-time that connects them.

   One question: were things like this the inevitable outcome of using the arXiv and dispensing with refereed journals?

5. **Adam H**
February 28, 2020

I couldn't have said it better than SH or Peter Shor. I also don't see what this accomplishes except to verify quantum teleportation(which has already been experimentally verified). Is this another sort of QM=GR idea? A true traversable wormhole should allow matter/info to arrive at a distant point in space faster than light could get there traveling from its origin. This is clearly not a traversable wormhole by that definition, and the same physicists involved in this enterprise would likely agree that true traversable wormholes are not likely to ever exist for consistency reasons.

6. **Blake Stacey**
February 29, 2020

Peter Shor:

> Reading the Quanta article, it looks like their experimental proposal is to *do* quantum teleportation (of a particularly complex kind) and *call it* a traversable wormhole by invoking the non-existent ER-EPR correspondence.

That seems accurate to me.

When I first read the ER=EPR proposal, it seemed motivated by very thin heuristics that just barely scratched the surface of quantum theory. E.g., entanglement can't be used for superluminal signaling, and messages can't pass through wormholes; therefore, the phenomena are analogous. This amounts to saying that a wormhole exists between any two correlated objects in the Spekkens toy model.

It's not clear to me that the Brown et al. proposal couldn't be done, qualitatively, with stabilizer states, Spekkens toy bits or correlated Gaussian modes. (The "thermofield double state" doesn't sound any more exotic, really, than using EPR pairs as purifications of thermal states in Gaussian quantum mechanics. The point seems to be that it's entangled and its marginals are Gibbs states.) I could be missing something important, but right now, I'm not seeing how it's *really nonclassical,* in the sense that nothing like it can be emulated in a local hidden-variable theory.

7. **Alex**
February 29, 2020

I wonder why people keep talking about "traversable wormholes" when there are numerous theorems in classical and semi-classical GR that put into serious doubt their physical feasibility. At the classical level, you need negative energy to stabilize them. This was proved by Hawking under very general conditions. Second, it was also proved by Hawking that when you put a quantum field in it, the whole thing instantly explodes and destroys all matter configuration that was holding it in place. One can still speculate that the Casimir effect may provide negative energy. Good luck with collecting the amount needed; besides, it will explode anyway. Well, you can speculate that quantum gravity may act as a cutoff for that energy divergence of the quantum field, but that is, as per today, just words, not serious physics. All currently tested serious physics says they can't exist. Period.

I really don't understand how these things can pass the peer review.

Sources (two papers by the leading expert on wormholes and closed timelike curves, the Nobel prize Kip Thorne):

http://www.its.caltech.edu/~kip/index.html/PubScans/II-121.pdf

http://www.cmp.caltech.edu/refael/league/thorne-morris.pdf

8. **Blake Stacey**
February 29, 2020

The second update points to a Twitter thread by a colleague of the authors who was thanked in the acknowledgments, not an author.

9. **Anonyrat**
March 1, 2020

The Atlantic online provides this headline for a re-hosting of the Quanta article:
"A Tiny, Lab-Size Wormhole Could Shatter Our Sense of Reality
How scientists plan to set up two black holes and a wormhole on an ordinary tabletop"

10. **CWJ**
March 1, 2020

Slightly off-topic, but I found it through one of the twitter threads: the APS March meeting has been cancelled due to concerns about Covid19

https://physicsworld.com/a/american-physical-society-cancels-march-meeting-in-denver-due-to-coronavirus-outbreak/

11. **Peter Woit**
March 1, 2020

CWJ,

Since physics labs around the country can create and manipulate wormholes, could everyone stay home and create wormholes to a lab in Denver, then run the conference via wormhole? Has anyone studied the problem of Covid19 transmission via physicist's wormholes? I'm not sure I would want to be entangled with someone who has the virus…

12. **Peter Shor**
March 2, 2020

Let me try to put it in simpler terms.

Suppose you take a balloon, and measure the distance between points on it. Then you inflate it more, and you see how much these distances have expanded. This *does not* tell you that the universe is expanding. It's an analogy.

The same thing is true for the experiment proposed in the paper. The experiment should succeed because quantum information theory works, not because of anything about general relativity and wormholes.

13. **Alex**
March 2, 2020

So, I did my homework and just read all the papers involved. They actually try to address the two points I mentioned about "ordinary" wormholes, so kudos to them in that regard. Problem is, the solutions are highly unsatisfactory. Regarding negative energy, they consider two asymptotic anti de Sitter universes, and in this way can obtain a negative energy carrying quantum field in an analogous way to the Casimir effect, where the boundaries of the anti de Sitter universes connected by the wormhole act as the conducting metal plates in the latter. The issue with this is, of course, that we don't live in anti de Sitter. So that isn't going to work for addressing the first point against the existence of wormholes. The second point is that, with stable ordinary wormholes, you can create closed causal curves and this produces energy blow ups in quantum fields. They solve this by considering a wormhole connecting two different universes and which collapses quickly enough (it's just a perturbed, by the negative energy quantum field, Kruskal wormhole.) Problem is, you need to start with a very particular initial state (two white holes in two different universes, or two different universes connected by a wormhole that collapses into two black holes.) This critique is also valid for the usual wormholes, since those geometries also assume an already created wormhole and no convincing method for their creation is proposed. In these new wormholes, they use the AdS/CFT duality and the ER=EPR hypotheses to argue that they are equivalent to a quantum teleportation experiment; thus, by creating a quantum teleportation you may be actually creating the wormholes. Of course, AdS/CFT duality and ER=EPR are highly speculative and mathematically unproven hypotheses, besides the already mentioned fact that we don't live in an anti de Sitter universe. So, the most likely result from a quantum teleportation experiment will be just to prove again the known phenomena of quantum teleportation and nothing more.

14. **Mark**
March 2, 2020

Alex,

This paper may be of interest to you (and other readers who care about substance not just sarcastic commentary): https://arxiv.org/abs/1807.04726
It's using Casimir energy, but it's building wormholes in flat space using Standard Model matter content.

15. **Peter Woit**
March 2, 2020

Mark,

Thanks! Especially impressive that, according to the Atlantic, you can build that thing on an ordinary tabletop.

16. **Alex**
March 2, 2020

Hey Mark, thanks! That was a very neat paper. I'm glad they are trying to address the actual problems. I think this is a good contribution to our understanding of wormholes, in line of Thorne's classic work. The most valuable thing, for me, is the exploration of quantum matter configurations (in the real world) that could be used to get the negative energy.

The only objection I have is that, again, a very special initial state for the geometry is assumed (even more weird than a mere standard eternal wormhole.) But they actually recognize this (in 9.3 Open questions) rather than adding baseless speculation, so kudos to them.

17. **John Baez**
March 3, 2020

Peter Shor wrote:

> One question: were things like this the inevitable outcome of using the arXiv and dispensing with refereed journals?

I think the problem is that real progress on high-energy particle physics and quantum gravity has been very slow for the last few decades, but people in these fields want to be doing impressive things, so many have turned to puffing up what they're doing, using all sorts of tricks to make it seem more exciting than it is.

In fields where there's more real progress and less grandiose ambitions, the arXiv seems to work pretty well.

18. **Peter**
March 3, 2020

I'm following this blog since I read "Not even wrong", but this is one of the weirdest things I've ever seen here.

Do I understand what's happening?

– You do an experiment on X, something you understand quite well;
– if you assume certain things (some of which are dubious, to put it mildly) the behavior of X should have in some way a resemblance with the behavior of something hypothetical, called Y, that you're not testing with the experiment;
– The result of the experiment on X tells you something about Y.

If this interpretation is correct, this idea is an interesting subject for a paper on Philosophy of Science. But it's not science like I know it.

19. **Peter Woit**
March 3, 2020

Peter,
Yes, that's right. This is "quantum gravity in the lab" where you are not testing anything to do with quantum gravity. Another way to see the problem: the idea seems to be to use some model to predict the experimental result. What are the possible outcomes?

1. The prediction works. You've learned that this is a useful model for describing this kind of physical setup. You've learned nothing about whether it's useful for describing quantum gravity.

2. The prediction doesn't work. You've learned that this is not a useful model for describing this kind of physical setup. Again, you've learned nothing about whether it's useful for describing quantum gravity.

20. **André**
March 3, 2020

To me it always feels like these analogue models for (quantum) gravity are a bit like what paper models are for architecture. You can certainly learn from them, if a wall here or there looks nice in a given context. And you might be able to use them to see if taking this wall or that wall out still gives you a stable structure. But often it seems to me, the conclusion people draw from them is that real houses should also best be built from paper.

21. **Low Math, Meekly Interacting**
March 4, 2020

    There's another important assertion in the article, which can seemingly hold regardless of what is implied about "reality": that Ads/CFT provides a simpler description of the system being modeled. I take that to mean, roughly, that it makes the math easier. Similar benefits (e.g. for some tough problems in QCD) have been promoted elsewhere. Any merit to this specific aspect of the claim?

22. **Peter Woit**
March 5, 2020

    LMMI,
    That's the hope, but the reality is that this hasn't worked for realistic systems. You can see the problem in the case of QCD, where AdS/CFT gives you a calculational method for N=4 susy Yang-Mills, but not Yang-Mills. You end up doing computations in a different theory than QCD, then hoping that some rough features of that computation apply to QCD. As far as I can tell, all advertised applications of AdS/CFT have similar problems. The underlying problem is that people believe there is some underlying gauge-gravity duality principle, that gravity duals always exist, but other than in certain very special examples don't know how to find the gravity dual. The "applications" of this duality to realistic problems become just general claims that certain kinds of phenomena on the realistic side will correspond to other phenomena on the gravity side, without any detailed prediction for exactly how this will work.

23. **Urs Schreiber**
March 5, 2020

    AdS/QCD is rich in quantitative predictions for confined QCD states, see here.

24. **Geoffrey Dixon**
March 5, 2020

    Am I correct in thinking that this surfeit of AdS/CFT talk is tantamount to looking for a nail in the dark under a lamppost because the light is better there, and anyway all you've got is a hammer, so the solution to your problem had better require a nail? Maybe two.

25. **Peter Woit**
March 5, 2020

    Geoffrey Dixon,

    Yes.

    Urs Schreiber,

    AdS/CFT is conformally invariant, spectrum looks nothing like QCD. AdS/QCD is a phenomenological model, engineered to try and reproduce the strong-coupling long-distance behavior of QCD. But if you want to do that, there's something much simpler: it has been known for ever that the strong coupling expansion of QCD is an expansion in surfaces and you can think of it as a string theory if you want, and also get "predictions" for the strong-coupling/long-distance behavior.

    The hard part of the QCD problem is not this, it's connecting this behavior to the short distance behavior (asymptotic freedom), through a region where your calculational method doesn't work. AdS/QCD does not at all solve this problem.

26. **A Former Professional Higgs Boson Hunter....**
March 5, 2020

    A bit off topic, but I thought it was inspired…

    Today Time Magazine announced its 100 Women of the Year going back to 1920 and to my utter astonishment, the Woman of the Year in 1921 was Emma Noether…

    Kudos to Time!

    https://time.com/5792615/emmy-noether-100-women-of-the-year/

27. **Urs Schreiber**
March 9, 2020

    The problem solved by AdS/QCD is the working definition of QCD in the confined regime, producing decent analytic predictions for hadron spectra otherwise accessible at best via lattice numerics. This is effectively the conceptual explanation of the old Skyrme model for baryons, improved by including the full tower of mesons beyond the pion, which turn out to be unified as KK-modes of the holographic theory. Sutcliffe 10 explains nicely how the secret holographic nature of the Skyrmion model is really the famous theorem of Atiyah-Manton 89.

    It's all rather remarkable, but remains underappreciated. A good account happens to be in Chapter III of the collection "The Multifaceted Skyrmion".

28. **Peter Orland**
March 10, 2020

    Urs,

    AdS/QCD is a model (basically a jazzed-up quark model). It is not the only conceptual explanation for the Skyrme model. Plus, it probably has little to do with real QCD (which must have both confinement AND asymptotic freedom, in particular, generation of logarithms in correlation functions at short distances).

    History is repeating itself (in molasses). Way back in the mid- to late-1970's, the strong-coupling lattice approximation was used to get a fairly decent picture of hadron masses. Except for the pseudo-scalar mesons (which later were incorporated), the results were claimed to show that QCD describes the world.

    Then critics responded that this works for the same reason quark models and string models work. Confine heavy quarks with a linear potential and out comes a model of the hadrons! No surprise there.

    So the strong-coupling lattice people, very sensibly, gave up (after a only a year or two) and turned to Monte-Carlo simulations (or eventually starting doing string theory, in some cases)

In my opinion, it is long overdue for AdS/QCD people to give up as well (and why can't they?).

The basic technical point (excuse the capital letters which follow, but they are important) is this; you need a theory which can get to the ZERO-bare-coupling fixed point. This does NOT mean weak renormalized coupling. True QCD has INFINITESIMAL bare coupling, not strong bare coupling. The renormalized coupling (depending on how you define it) can be large, but NEVER the bare coupling.

If you don't believe me about infinitesimal bare coupling, it was shown for more conventional QFT's in the 50's, using the Kaellen-Lehmann spectral representation; see Itzykson and Zuber, for example. See also how the renormalization group works in this context.

You can play games to get Skyrmions (I remember people did this with the bag model too), but this is still insufficient. Fitting hadron masses is nothing new (and not good enough).

Sorry for the long discussion. I have said similar words on this blog a few times here before. Some string theorists seemed incapable or unwilling to hear it (it's basic, not advanced renormalization theory), then responded with insults. My verbose discussion here is to try to leave no ambiguity.

Regards,
Peter Orland

29. **Urs Schreiber**
March 11, 2020

That the quark model correctly predicts hadron bound states is a purely computer-experimental observation (established only fairly recently for the first few hadrons arXiv:0906.3599) of which a conceptual understanding remains an open problem, dubbed one of the Millennium Problems.

While it is an old idea that the string model of mesons gives a conceptual handle on the confinement mechanism, its detailed and quantitatively accurate development used to be lacking. Making the string model of hadrons actually work is what holographic QCD is all about.

Since holographic QCD readily explains fundamental characteristics of confined QCD that remain mysterious not just in the quark model but also in popular *ad hoc* strong coupling model building such as the bag model (not only the confinement and chiral symmetry breaking mechanism itself, but also for instance vector meson dominance and the Cheshire cat property of the bag model are readily explained by holographic QCD) it is attractive to researchers interested in real-world QCD (e.g. Rho et al. 16, doi:10.1142/9710).

The Skyrme model for hadrons is in fact reasonable not in its original form but only after adjoining the tower of vector mesons to the pion. But in that tower-corrected form the Skyrme model works wonders: nuclei all the way up to carbon(!) are well-decribed already by Skyrmions in the pion+rho field (arXiv:1811.02064). This tower-correction of the old Skyrmion model had let nuclear physicist to discover (PhysRevD.69.065020) the hidden 5th dimension, leading to proof of vector meson dominance for nucleons. Conversely, the tower-corrected Skrymion model emerges from holographic QCD, where all mesons are unified as the transversal KK-modes of the 5d flavour gauge theory.

That available results in holographic AdS/QCD currently only ("only") address the strongly coupled confined QCD phase and not its asymptotically free UV is not intrinsic to the holographic theory but owed to its computational development: holography is studied for strong 't Hooft coupling only for the convenience to be able to disregard string-scale and strong string-coupling effects on the bulk side. Inclusion of small-N corrections into AdS/QCD to get the full picture remains to be developed but need not and is not being ignored (e.g. PhysRevD.74.076004).

30. **Peter Woit**
March 11, 2020

Urs Schreiber,
This "holographic QCD" discussion has gotten off-topic, and Peter Orland is right about the problems with it. You admit the main point, that no one can use this to address what happens at short distances (despite 22 years of effort). This is a fundamental problem not one of "convenience". Sorry, but enough arguing this point, where the situation is clear (this doesn't work).

31. **Peter Orland**
March 11, 2020

Sorry for being off-topic Peter,

Twistors are a lot more interesting than AdS/QCD approaches, which won't resolve any nonperturbative QCD problems, especially not millennial ones. Although twistors probably won't solve these problems either, they still seem to have new applications waiting.

I have both Ward and Wells' and Huggett and Tod's books, and should probably get my money's worth out of them.

32. **Peter Orland**
March 11, 2020

Just realized that is more relevant to your other recent post. Oops.

Penrose at The Portal

March 6, 2020
Categories: Uncategorized

Since last summer Eric Weinstein has been running a podcast entitled The Portal, featuring a wide range of unusual and provocative discussions. A couple have had a physics theme, including one with Garrett Lisi back in December.

One that I found completely fascinating was a recent interview with Roger Penrose. Penrose of course is one of the great figures of theoretical physics, and someone whose work has not followed fashion but exhibited a striking degree of originality. He and his work have often been a topic of interest on this blog: for one example, see a review of his book *Fashion, Faith and Fantasy*.

Over the years I've spent a lot of time thinking about Penrose's twistors, becoming more and more convinced that these provide just the radical new perspective on space-time geometry and quantization that is needed for further progress on fundamental theory. For a long time now, string theorists have been claiming that "space-time is doomed", and the recent "it from qubit" bandwagon also is based on the idea that space-time needs to be replaced by something else, something deeply quantum mechanical. Twistors have played an important role in recent work on amplitudes, for more about this a good source is a 2011 Arkani-Hamed talk at Penrose's 80th birthday conference.

One of my own motivations for the conviction that twistors are part of what is needed is the "this math is just too beautiful not to be true" kind of argument that these days many disapprove of. There are many places one can read about twistors and the mathematics that underlies them. One that I can especially recommend is the book Twistor Geometry and Field Theory, by Ward and Wells. A one sentence summary of the fundamental idea would be

> A point in space time is a complex two-plane in complex four-dimensional (twistor) space, and this complex two-plane is the fiber of the spinor bundle at the point.

In more detail, the Grassmanian G(2,4) of complex two-planes in $\mathbf C^4$ is compactified and complexified Minkowski space, with the spinor bundle the tautological bundle. So, more fundamental than space-time is the twistor space T=$\mathbf C^4$. Choosing a Hermitian form Ω of signature (2,2) on this space, compactified Minkowski space is the set of two-planes in T on which the form is zero. The conformal group is then the group SU(2,2) of transformations of T preserving Ω and this setup is ideal for handling conformally-invariant theories. Instead of working directly with T, it is often convenient to mod out by the action of the complex scalars and work with $PT=\mathbf{CP}^3$. A point in complexified, compactified space-time is then a $\mathbf{CP}^1 \subset \mathbf{CP}^3$, with the real Minkowski (compactified) points corresponding to $\mathbf{CP}^1$s that lie in a five-dimensional hypersurface $PN \subset PT$ where $\Omega=0$.

On the podcast, Penrose describes the motivation behind his discovery of twistors, and the history of exactly how this discovery came about. He was a visitor in 1963 at the University of Texas in Austin, with an office next door to Engelbert Schucking, who among other things had explained to him the importance in quantum theory of the positive/negative energy decomposition of the space of solutions to field equations. After the Kennedy assassination, he and others made a plan to get together with colleagues from Dallas, taking a trip to San Antonio and the coast. Penrose was being driven back from San Antonio to Austin by Istvan Ozsvath (father of Peter Ozsvath, ex-colleague here at Columbia), and it turned out that Istvan was not at all talkative. This gave Penrose time alone to think, and it was during this trip he had the crucial idea. For details of this, listen to what Penrose has to say starting at about 47 minutes before the end of the podcast. For a written version of the same story, see Penrose's article Some Remarks on Twistor Theory, which was a contribution to a volume of essays in honor of Schucking.

Comments

1. **DB**
   March 6, 2020

   Very interesting post Peter.
   I have read both "Fashion, Faith and Fantasy" and "The Road to Reality", and I must say that I found Twistor Theory extremely complicated. It takes a hell of a lot of time to get to understand it just roughly.
   There's no doubt about Penrose's originality and out of the box thinking, and how much Twistor Theory has influenced the amplituhedron and other Arkani-Hamed's ideas on geometry of the Universe.
   But back in 2003, he had a meeting with Witten in Princeton, and it seems that the latter managed to write a 70 page paper in which he basically showed that Twistor Theory was embedded into String Theory.

   Has this been fully proven and that's why T.T. has been basically abandoned as the possible theory of "where the Universe emerges/comes from"?

2. **Peter Woit**
   March 6, 2020

   DB,
   It's not so much that twistor theory is complicated as that it involves some mathematical ideas that physicists are not familiar with (e.g. holomorphic vector bundles). The basic idea of twistors that I described in the post is actually simple, but radical. Not everything is about string theory or AdS/CFT, and trying to understand the twistor string or duality and N=4 super Yang-Mills is not a good way to try and understand twistor theory. One thing to say though is that Witten's paper on the twistor string does contain a very lucid and explicit discussion of what's called the Penrose transform (relating field equations on space-time and on twistor space).

   New ideas are definitely needed to connect twistor theory to the Standard Model and to use it to quantize gravity. There's a long history of people trying this, without success so far. I see various possibilities and am trying to write something up about those, we'll see what comes of that project. I'm hopeful, but still confused about some crucial questions.

3. **DB**
   March 6, 2020

   Thanks for your helpful answer Peter, and I'm looking forward to that future project!!
   Best of lucks.

4. **Lee Smolin**
   March 7, 2020

   Dear Peter,

   I share your fascination with twistor theory and spent some time during graduate school in Oxford learning it from Penrose and his students. Indeed, twistor theory turns out to be intimately connected to LQG and spin foam models; one way to explain why is to to show that both make use of the chiral structures that are revealed when you reduce general relativity to a topological field theory plus constraints. I have one paper about this: https://arxiv.org/abs/1311.0186, but it has been developed extensively by Wolfgang Wieland, Simone Speciale, Laurent Freidel and others. Some of the interesting papers are below.

Take care,

Lee

https://arxiv.org/abs/1207.6348
https://arxiv.org/abs/1901.08161
https://arxiv.org/abs/1107.5002
https://arxiv.org/abs/1602.01861
https://arxiv.org/abs/1807.11376

5. **tulpoeid**
March 8, 2020

Admittedly and perhaps reasonably, this requires investing huge amounts of time for those of us with the math knowledge of a physics degree (grad studies didn't add much in terms of fundamental math), simply for getting what it is about. Still, it might well turn out that reality needs math beyond this level.
So I think that the question that comes to mind is, what are the insights / results / meaning of twistors? Why are they worth pursuing? What are the consequences of complex two-planes as fibers of spinor bundles?

6. **Urs Schreiber**
March 8, 2020

It is curious that the mathematics which makes twistors exist precisely in spacetime dimensions 3 and 4 and 6 and 10 (here) is the same that makes the Green-Schwarz superstring exist in exactly these dimensions (here).

7. **Peter Woit**
March 8, 2020

Lee Smolin,

Thanks for the references. The way both twistor theory and LQG exploit chirality of 4d geometry in a fundamental way is fascinating.

Tulpoeid,
There's a lot of sophisticated mathematics involved here, but the point about spinors is rather simple. All fundamental matter fields we know of involve a complex two-dimensional spinor degree of freedom. In the conventional approach to geometry, it is rather complicated to understand where these degrees of freedom come from. In the twistor approach, a point in spacetime is precisely given by a complex two-dimensional space (in 4d twistor space), and this is the spinor space. The spinor degree of freedom is not something added ad hoc later, it is built into the very definition of space-time (in mathematician's lingo, the spinor bundle is "tautological").

8. **D R Lunsford**
March 8, 2020

It's a sort of line geometry for spacetime. Strange coincidence – Pluecker's original paper on line geometry appeared in the same volume of "Philosophical Transactions of the Royal Society of London" as Maxwell's "A Dynamical Theory of the Electromagnetic Field" – in 1865. That was the first complete presentation of Maxwell's theory.

-drl

9. **Robert Wilson**
March 9, 2020

What I find fascinating about twistor theory is the way that Penrose uses the fundamental structure of the group SU(2,2), and the twistor space on which it acts, to model spacetime at a point, *before* doing the geometry to model spacetime on an extended scale. He uses this particular group because it is a double cover of SO(4,2), that he wants for some geometrical reason that I admit is opaque to me. Altogether there are about 8 different real forms of this group (depending exactly how you count them), coming from 5 different real Lie algebras so(6)=su(4), so(5,1)=sl(2,H), so(4,2)=su(2,2), so(3,3)=sl(4,R) and su(3,1). I am aware of significant work using su(4), and other work using so(5,1), both attempting to do similar things to what Penrose has done. But has anyone looked seriously at sl(4,R) or su(3,1)?

10. **Mark Callaghan**
March 9, 2020

Peter, I just want to thank you for directing me to the Portal website, the discussion with Roger Penrose was fascinating , and there is much content of a similarly stellar nature there.

11. **Peter Woit**
March 9, 2020

Robert Wilson,
SO(4,2) is the group of conformal transformations, the Poincare group is a subgroup. Relativistic massless particles transform according to irreducible infinite dimensional representations of this group (or its double cover). Understanding these reps tells you a great deal about relativistic massless particles. Note that the SU(2,2) representation theory story is a generalization of the SU(1,1)=SL(2,R) story (where the discrete series representations can be constructed using holomorphic methods on a hemisphere of the Riemann sphere). The "Penrose transform" is of great interest to those working on geometric constructions of irreps of real Lie groups like SU(2,2).

One of the wonderful aspects of the twistor story is that it relates what happens in different space-time signatures. SO(4,2) is the Minkowski space story, SO(5,1) is the conformal group of S^4, so gives a (compactified) Euclidean space story. SO(3,3) gives the signature (2,2) story, where your spinors are real, and you're talking about real 2 planes in real 4 space. From what I've seen, people doing amplitudes calculations seem to exploit the ability to work in different signatures, with the real (2,2) signature case often useful. I don't know of uses of the other signatures. SO(6)=SU(4) is a much simpler story (SU(4) acts transitively on G(2,4) and the irreps one gets are finite dimensional. I don't know about use of the SU(3,1) real form.

12. **D R Lunsford**
March 9, 2020

"One of the wonderful aspects of the twistor story is that it relates what happens in different space-time signatures. SO(4,2) is the Minkowski space story, SO(5,1) is the conformal group of , so gives a (compactified) Euclidean space story. SO(3,3) gives the signature (2,2) story, where your spinors are real, and you're talking about real 2 planes in real 4 space. "

This is a really deep point. I have done of lot of living in SO(3,3) and often broke my head on the relation to SO(4,2) physically.

The books of Felix Klein are really good preparation for all this, even though they are old. They read like novels 🙂

-drl

13. **Robert Wilson**
March 10, 2020

Peter,
Thanks for that explanation. I understand that SO(4,2) is the only real form that contains the Poincare group, so that if you're interested in gravity then this is the obvious choice. But it also contains a subgroup SO(1,1) that commutes with the Lorentz group and acts on the translation part of the Poincare group as scale-changes. This is fine if all you want to model is GR, but it seems to me to be a problem if you want a quantum gravity. So I'm not entirely surprised that particle physicists find the SO(3,3) signature useful. Do you know of a good reference to get me started on exploring what particle physicists do with SO(3,3)? To me, the interesting thing about this signature, apart from the fact that it is the only one with real spinors, is that there are subgroups of dimension 12, which do not occur in any other signature. Dimension 12 seems to be important for both bosons and fermions in the standard model.

14. **Peter Woit**
March 10, 2020

D.R. Lunsford/Robert Wilson,

I know nothing about physical applications of the split signature case. Two good places to find information about the way different signatures are related in the twistor picture are the paper
Real methods in twistor theory, by Woodhouse
https://iopscience.iop.org/article/10.1088/0264-9381/2/3/006
and Tim Adamo's lectures on twistor theory
https://arxiv.org/abs/1712.02196
In Adamo's lectures you learn that
"In general, the idea in twistor theory is to work in the complexified setting, imposing reality conditions only at the end of a calculation. In the old days of the subject, these reality conditions were usually the Lorentzian ones, while early in the 'twistor renaissance' of 2004 the split signature reality conditions were preferred. Nowadays, Euclidean reality conditions seem to be the most useful when performing explicit calculations. So depending on what era of the literature you read, you can find any one of the three reality conditions given preference for a combination of physical and technical reasons."
I'm not all that familiar with the 2004 era literature using the split signature that he's referring to.

15. **Stephane**
March 12, 2020

https://cerncourier.com/a/when-twistors-met-loops/

16. **edg**
April 3, 2020

Dear Peter,

Eric Weinstein released the video of his 2013 Oxford talk on his approach to unified field theory, along with a preamble and some clarifying notes after the talk, see here:

https://www.youtube.com/watch?v=Z7rd04KzLcg&t=1s

To this physicist many parts are unclear, but perhaps you and others who follow this blog will be able to parse his constructions. I wonder in particular if this can be done in d=2 as a first toy model, before approaching d=4.

This Week’s Hype

March 20, 2020
Categories: This Week's Hype

In this disturbing time of pandemic, it’s reassuring to see that some activities continue as usual. On the string theory hype front, yesterday NASA put out a press release announcing that Chandra Data Tests ‘Theory of Everything’, which starts by explaining that:

> Despite having many different versions of string theory circulating throughout the physics community for decades, there have been very few experimental tests. Astronomers using NASA’s Chandra X-ray Observatory, however, have now made a significant step forward in this area.

This is based on a paper announcing limits on axions based on data from the Chandra X-ray telescope, which starts off with the dubious claim that axions “are generic within String Theory”. It seems to be very hard to get some people to understand that the number of “tests of string theory” is not “very few” but zero, for the simple reason that there are no predictions of string theory, generic or otherwise.

As usual, this kind of thing gets picked up by other news sources. In a sign of the times, the spin given to the bogus “test” is now often negative for string theory: This Galaxy Cluster May Have Just Dealt a Major Blow to String Theory.

Update: This is getting attention at The Daily Galaxy, under the headline “Mind of God?” –The Detection of ‘String-Theory’ Particles Would Change Physics Forever”.

For more on religion and string theory, there’s a ~~new~~ (actually from 2017) podcast featuring IAS theorist Tom Rudelius, entitled The Multiverse, the Polygraph, and the Resurrection. In an older podcast at Purpose Nation, Rudelius tells us this about the views of Nima Arkani-Hamed:

> To quote preeminent theorist Nima Arkani-Hamed, who is certainly no theist: “The multiverse isn’t a theory. It’s a cartoon, right, it’s like this cartoon picture of something that we might think might be going on but we really don’t have any solid theory of how it would work.”

It seems that Arkani-Hamed shares my views on this.

Comments

1. **dratcwo**
   March 20, 2020

   I agree that axion-like-particle is predicted in any theory with extra-dimension. However, there is another more generic prediction from string theory/SUGRA, the spin-3/2 gravitino.

2. **Peter Woit**
   March 20, 2020

   dratcwo,
   What’s the mass range and couplings for this “generic” gravitino?

   I don’t believe that string theory provides any answer to this. The situation is always the same: there is a “beautiful”, highly predictive, string theory/sugra that people like to point to, but it looks nothing like the real world (wrong space-time dimension, no SUSY breaking). Trying to make the theory agree with the real world requires making it exceedingly ugly and unpredictive, without any non-trivial “generic” predictions.

   The accuracy of the above characterization of the situation has become abundantly clear over the last couple decades. It would be nice if people would stop misrepresenting this to non-experts, e.g. innocent astrophysicists…

Fourier Analysis Notes

March 23, 2020
Categories: Uncategorized

This semester I've been teaching a course on Fourier Analysis, which has, like just about everything, been seriously disrupted by the COVID-19 situation. Several class sessions have been canceled, and future ones are supposed to resume online next week. To improve matters a bit, I've been writing up lecture notes for the material since in-person lectures were canceled, and we'll see how long I have the energy to keep this up.

The website for the course is here, giving detailed information about what it covers. In terms of level of mathematical rigor, the concept is to use the course as an opportunity to give students some motivation for a conventional real analysis course. The only prerequisite for the course is our usual Calculus sequence, which is not proof-based. In this class students are expected to try and follow proofs given in the book and in class, but not expected to be very good at coming up with their own proofs in the assignments, which mostly are computational. The textbook (Stein-Shakarchi) is based on a Princeton course with a somewhat similar philosophy of providing an introduction to analysis, but it is very challenging for the students to follow. I've looked around, but not found a better alternative. Other books on the subject tend to be either books for mathematics students that are even more abstract and challenging, or books for engineers that focus on either signal analysis or PDEs. Since the math department already has a PDE class I want to emphasize other things you can do with the subject.

The first set of lecture notes I wrote up were only loosely connected to Fourier analysis, through the Poisson summation formula. They dealt with theta functions and the zeta function, giving the standard proof of the functional equation for the zeta function that uses Poisson summation. I confess that one reason for covering this material is that I've always been fascinated by the connection between theta functions, quantization, representation theory (through the Heisenberg and metaplectic groups), and number theory. This subject contains a wealth of ideas that bring together fundamental physics and deep mathematics. On the mathematics side, this story was generalized by Tate in his thesis, where he developed what is essentially the GL(1) case of the modern theory of automorphic forms that underpins the Langlands program. On the physics side, one can think of what is going on as the standard canonical quantization of a finite-dim phase space, but with a lattice in the phase space giving a discrete subgroup of the usual Heisenberg group, and lots of new structure. For the details of this, one place to look is volume III of Mumford's books on theta functions.

The second set of lecture notes, which I've just started on, are intended as an introduction to the theory of distributions, a topic that isn't in Stein-Shakarchi. I highly recommend the book by Strichartz referred to in the notes for more details, with the notes maybe best used just as an introduction to that book.

I don't want to turn this blog into yet one more place for discussion of the COVID-19 situation that just about all of us are obsessed with at the moment. If you're interested in my personal experience, I'm doing fine. Almost all of us in New York are now pretty much confined to our apartments (it helps that the weather outside today is terrible), other than for short ventures out to get food or some exercise. I'd like to optimistically think that New York started taking action to stop the virus spread early enough to avoid disaster at the local hospitals. The best place I've seen to try and follow what is happening there is this web page. We'll see in the next couple days if the problem has started to peak, or has much further to go.

I'm in much better shape than most people, having left town early for a spring break vacation. I had been planning a trip to Paris, at the very last minute instead rented a car and started out on a road trip in the general direction of New Orleans, consulting coronavirus report maps for where to avoid. Ended up in Memphis and then the Mississippi Delta (it had become clear New Orleans was a bad idea) before finally deciding that the situation was getting serious everywhere and it was time to head home. So, ended up back here in New York in relatively good mental shape for the confinement to come. Good luck to all of us in dealing with the coming challenges…

Update: The semester is now over, and I've put together all the notes I wrote up in one document, available here. This fixes mistakes/typos/etc. in earlier versions of the notes, so I'm changing the links to point to the final version.

Comments

1. **Akhil**
   March 23, 2020

   Thanks Peter! We hope more genuine content on the Fundamental physics-Mathematics connections side is coming, which is hard to find elsewhere.

2. **Roderic C Deyo**
   March 23, 2020

   Perhaps it's not fully appropriate for the course since it uses complex analysis, but I noticed that Richard Bellman's wonderful short classic "A Brief Introduction to Theta Functions" was missing from the references.

3. **Hugh Osborn**
   March 24, 2020

   You are missing a – in the heat kernel in lecture 2

4. **Peter Woit**
   March 24, 2020

   Hugh Osborn,
   Thanks! Fixed.

5. **Jackiw Teitelboim**
   March 24, 2020

   Dear Peter,

   It's interesting that Vol.I of Stein & Shakarchi gets very very close to introducing the notion of Schwartz's distributions in their discussion of Poisson, Dirichlet, heat kernels &., but always avoids introducing the notion of continuous linear functional on the space of test functions.

   There's a short, rigorous and quite elementary book by Friedlander, "Introduction to the theory of distributions," which as a student at the time I found a valuable complement to Stein & Shakarchi.

   Regards.

6. **David Brown**

March 24, 2020

In the second set of notes, on page 1 in the last paragraph, shouldn't "Schwarz" be "Schwartz" for Laurent Schwartz? No tee Schwarz is either the string theorist Schwarz or Hermann Amandus Schwarz.

7. **Peter Woit**
   March 24, 2020

   David Brown,
   Thanks, yes it's Laurent Schwartz. Fixed.

   Jackiw Teitelboim,
   Thanks, the Friedlander/Joshi book is a good more advanced reference for distribution theory. I still would recommend the Strichartz book as less heavy on the theory and having more about applications and motivation.

   I can see why Stein-Shakarchi decide not to introduce distributions, but then you miss a huge part of the subject. The simplest way to introduce distributions, especially in the context of Fourier analysis, seems to me to be to stick to tempered distributions (i.e. Schwartz functions as test functions), but I don't think I've seen any text that does this.

8. **Babak**
   March 30, 2020

   Dear Peter,

   As you know, there's also a group theory point of view that can be found for instance in a beautiful book by Audrey Terras

   https://www.amazon.ca/Fourier-Analysis-Finite-Groups-Applications/dp/0521457181

   I think that is perhaps the true reality of Fourier analysis.

9. **Babak**
   March 30, 2020

   David: There's also Albert Schwarz. 😊☺️

This Week's Hype

March 25, 2020
Categories: Swampland, This Week's Hype

Maybe it's because people are at home with nothing else to do, but somehow the COVID-19 pandemic seems to be having the side-effect of generating new infections of "test of string theory" hype, a disease common many years back that seemed to more recently be under control. The example of a few days ago has now spread widely (see for instance Popular Mechanics), sometimes mutating into tests of "sting theory". Today there's a new example out, on the middle of the front page at Scientific American: Will String Theory Finally Be Put to the Experimental Test?

Of course the answer is "No", this is just one more in the Swampland strain of string theory hype. This latest example is based on a paper by Bedroya and Vafa, where they make a "Transplanckian Censorship Conjecture". The weird aspect of this kind of string theory hype is that it's not a "test of string theory", because it really has nothing to do with string theory. The authors of this paper are making a conjecture about "any consistent theory of quantum gravity". If their conjecture is true we shouldn't see the kind of B-modes in the CMB that were mistakenly claimed in the BICEP2 fiasco of 2014. So, the "test" here is a claim of falsification if experiments do for real see these B-modes. But what is being tested is a conjecture about any consistent theory of quantum gravity (one with very weak evidence). If B-modes are seen by a future experiment, the two possible conclusions to be drawn will be:

- There is no consistent theory of quantum gravity.
- The Transplanckian Censorship Conjecture is wrong.

It's pretty clear what the correct choice between these two will be, and none of this will "test string theory."

Update: I should have also pointed to this paper. Will Kinney today gave a talk, It Came From the Swampland, which went over this subject seriously in detail. His conclusion, which seemed to be shared by a string theorist he was talking to at the end, was pretty much that these conjectures should not be taken seriously. It looks like they're already in conflict with both experimental results as well as theoretical model-building.

Comments

1. **Zovi**
   March 25, 2020

   Just to mention that this week they officially cancelled the The Arithmetic of the Langlands Program series of events of this year at Bonn.

2. **Peter Woit**
   March 25, 2020

   Zovi,
   At this point I think pretty much everything not purely online has been cancelled for the next couple months and often beyond. A good point is made by Peter Coles here https://twitter.com/telescoper/status/1240963587138555905

   "It's now even easier to identify fake academic conferences – they're the ones that haven't been cancelled."

3. **Rajesh**
   March 25, 2020

   You missed the most important alternative which can be concluded, if B modes are ever discovered — Inflation is wrong. There are alternatives to inflation which can predict a large 'r' and still be perfectly compatible with this conjecture. In fact, in some follow-up work, it has been shown how even some (highly-contrived) models of inflation might also survive the TCC and yet produce a larger 'r'. So, I think the conclusions should really be:

   1) Inflation is wrong, or
   2) The TCC is wrong.

   It, most certainly, is not that there are no consistent theories of quantum gravity. But I completely agree with you that this, therefore, cannot be a test for string theory.

4. **Peter Woit**
   March 25, 2020

   Rajesh,
   Thanks, good point. However, if we ever do see a non-zero r, surely more film crews will show up at Andrei Linde's door, and he and the Stanford press office will again tell us about how non-zero r is "smoking gun" evidence for inflation. So, that's the point we'll be starting from…

5. **David Roberts**
   March 28, 2020

   @Rajesh

   and that 'or' is presumably not exclusive!

6. **Douglas Natelson**
   March 30, 2020

   Not to threadjack, but it's been making the rounds that Phil Anderson passed away yesterday at the age of 96. Truly, the end of an era, as he played a huge role in defining the discipline of condensed matter physics as well as discovered (if that's the appropriate term) what is now known as the Higgs mechanism.

7. **Peter Woit**
   March 30, 2020

   Douglas Natelson,
   Thanks, writing something now about Anderson (and including a link to your blog for more about his work).

8. **David Roberts**
April 3, 2020

Reported in Nature News [1]: Mochizuki's papers have been accepted for publication, with no substantial changes.

[1] https://doi.org/10.1038/d41586-020-00998-2 (not a 1st of April posting)

Philip Anderson 1923-2020

March 30, 2020
Categories: Obituaries

I heard this morning about the death yesterday of Philip Anderson, at the age of 96. It's not hard to make the case that Anderson was the most important condensed matter theorist of the twentieth century, with a huge influence on how we think about the subject. I believe he was even responsible for the name "condensed matter". There are already obituaries at Princeton, and at the New York Times. For more about his work, Douglas Natelson has written something here.

Anderson's career intersected with the field of high energy physics in several ways. Most importantly, what is often called the Higgs phenomenon really is a discovery of Anderson's, and this should have been recognized by a second Nobel for him (he already had one for some of his work in condensed matter). I've written extensively about this story on the blog, see for example here, here and here. The story is a bit complicated, but it's undeniable that in November 1962 Anderson submitted the paper Plasmons, Gauge Invariance and Mass to the Physical Review, and it was published on April 1, 1963. This was more than a year before the Higgs/Brout/Englert/Guralnik/Hagen/Kibble papers that HEP theorists always point to as the original ones. If you read Anderson's paper, you'll find a discussion of the "Higgs mechanism" which gets at the basic physics in much the same way we think of it today. There was no reason for HEP theorists to miss this paper, Anderson had written and published it not as a condensed matter paper, but as a contribution to current high energy theory. The only counter-argument I've gotten about this is that "Anderson's explicit model was non-relativistic", but this is a physical phenomenon for which relativity is not particularly relevant. Does it really make sense to argue that recognition should not go to a theorist who discovers a new phenomenon, but to others who later show that a possible problem (e.g. inconsistency with special relativity) not considered by the discoverer really isn't there?

My time as a student at Princeton during 1979-84 (during which years Anderson split his time between Princeton and Bell Labs) was a high point of interaction between the condensed matter and HEP theory groups. HEP theorists trying to understand QCD investigated many examples of non-perturbative quantum field theory behavior that were of common interest with Anderson and others working on condensed matter. Anderson did have a major philosophical difference with the reductionist point of view of many HEP theorists, with his 1972 paper More is Different providing a strong critique of reductionism and emphasizing the importance of emergent behavior. In some sense, leading HEP theorists in recent years have come around to his point of view, often working on emergent models of space-time, with little interest in what microscopic physics space-time might be emerging from.

Anderson made no friends among the HEP community when he came out in 1987 against building the SSC (which was cancelled in 1993), for more about this, see this blog entry. He was also a skeptic about string theory, which perhaps made for some discomfort as Princeton HEP theory centered around this subject starting in 1984.

The two personal interactions with Anderson that I remember both involved him providing me with significant encouragement. When I took my general exams at Princeton, one component was an exam on condensed matter theory. This was not then and is not now a subject I know much about. After the exam there later was a gathering of students and faculty, and Anderson came up to me to tell me that he had graded my condensed matter exam. On one problem evidently I had gone about it wrong, but at some point had stopped and written that the result I was getting wasn't sensible. Anderson complimented me on this, telling me that knowing when a calculation wasn't making sense was an important skill. This was the nicest way imaginable to encourage a student who didn't really know what he was doing.

Twenty years later, in early 2001, after I had written this article and had distributed it to several theorists including Anderson asking for comments, this is what he wrote back to me:

> (Jan 19, 2001)
> Dear Peter, I'm sorry to have been so slow to get back to you; my printer blew out when I tried to print out your attachment. When I finally got it it blew my mind–I loved seeing my vague misgivings made explicit. I would say that perhaps a stronger argument is the way of coping with black hole entropy, but since hearing about that i have never had it made clear to me that the story is unique to string theory except as a representative of sane quantum theories of gravity–the point made is that the gravitational theory provides its own cutoff.
>
> I would hope but wouldn't guarantee that P Today would publish it–I would be glad to introduce you as a guest columnist but they might not accept that.
>
> When Lewontin and friends wanted to do a similar, but less well justified, job on sociobiology they wrote a broadside for the New York Review of Books.Since there are so many popular books I see no reason not to do that. If that doesn't work I could perhaps insert you into John Brockman's "third culture" chat room..
> Anyhow, good luck–pwa

I wrote back to Anderson, telling him in particular that a prominent theorist had advised me not to publish the piece, since it would be counterproductive and the problems I discussed were well-known. He responded:

> (Jan 23, 200)
> Dear Peter, thanks for your reply. Of course would feel that the article would be counterproductive. Wasn't that just the point? And to whom is it all well-known? The general public? Deans and department chairmen who make the hiring decisions? Science journalists who create the buzz? Politicians and bureaucrats who control the purse strings? Bullshit!
>
> Yes, i agree that the version you sent me was too technical for a general journal like the NY review, but actually not much–the Lewontin arguments were pretty technical. Anyhow, good luck with this one. I showed it to a knowledgeable colleague, not a stringy but one who collaborates with them, and he also liked it.–pwa

Anderson then put me in touch with an editor at Physics Today. They decided not to publish the piece, first suggesting that instead I write a letter to the editor, and finally rejecting even that as "too inflammatory". Anderson's positive response to the piece though provided significant encouragement for my decision to start writing publicly about this issue.

He was one of the greats, and will be missed.

Update: More about Anderson from Physics World, Horganism, and Science magazine.

Update: David Derbes reminds me to point out that Higgs himself always properly credited Anderson, generally referring to the "Anderson mechanism", or more specifically describing what he had done as the "relativistic Anderson mechanism". For instance, he wrote here:

> I call this the relativistic Anderson mechanism because Anderson described it first: it was his misfortune not to do so explicitly enough.

Comments

1. **Mark Sharefkin**
   March 31, 2020

   Amen. you might enjoy https://arxiv.org/abs/1605.06993.

2. **Peter Woit**
March 31, 2020

Mark Sharefkin,
Thanks, that's an excellent source for more about Anderson's work. The author, Piers Coleman, was in my class at Princeton and worked with Anderson. Another fellow student in my class, Gabi Kotliar, also worked with Anderson, and both he and Piers have gone on to careers as leaders of the subject.

3. **Alessandro Strumia**
April 1, 2020

Interesting to hear that, thanks to Anderson and internet, you bypassed Physics Today decision of not allowing physicists to read your letter. Do such editors think that physicists are idiots who cannot be exposed to an open debate?

4. **Peter Woit**
April 1, 2020

Alessandro Strumia,

About the Physics Today story: the person Anderson put me in touch with at Physics Today was Gloria Lubkin, who passed away recently also, see here
https://physicstoday.scitation.org/do/10.1063/PT.6.4.20200211a/full/
Despite backing from Anderson, I think it was unlikely Physics Today was ever going to allow a piece forcefully criticizing the research program of prominent people by someone with my much inferior credentials. If Anderson had been the author, they would have published it. The one piece they ever published criticizing string theory (1986 Desperately Seeking Superstrings) was quite forceful, could be published because Glashow's name was on it.
Since then they've published several propagandistic pieces promoting string theory, in particular a couple pretty outrageous ones from Gordon Kane. I'm unaware of anything critical they've published about string theory post 1986. They really should commission such a piece to make up for their sins.
Anderson had a lot of influence in the physics community, but even his influence was in many ways limited.

5. **Amitabh Lath**
April 1, 2020

It's time to let Philip Anderson off the hook for the cancellation of the SSC. Once Jim Wright resigned as speaker of the House due to ethics investigations, the SSC was dead. It wasn't clear at the time but without a heavy hitter from Texas there was no way it was going ahead. What Everett Dirksen was to Fermilab, Wright was to the SSC.

Nothing a Nobel Laureate like Anderson said would have mattered if Wright had remained speaker, the SSC would have been built. After he was gone, nothing any number of pro-SSC Nobel Laureates said, no SSC.

6. **Ross McKenzie**
April 7, 2020

Phil Anderson wrote a sympathetic book review of Not Even Wrong for the Times Higher Education Supplement.
https://www.timeshighereducation.com/books/loose-ends-and-gordian-knots-of-the-string-cult/204995.article?sectioncode=1&storycode=204995
It is behind a paywall, but it is reprinted in his "Thoughtful Curmudgeon" book.
A double compliment.

7. **Peter Woit**
April 7, 2020

Ross McKenzie,

Thanks, I had forgotten about that. I should also have recommended that book to everyone (full title is "More and Different: Notes from a thoughtful curmudgeon").

8. **Thomas Larsson**
April 13, 2020

OT. John Conway has died from the coronavirus. I recall trying to read a big yellow book of his 30 years ago.

9. **Peter Woit**
April 13, 2020

Thomas Larsson,
Thanks. I haven't posted something on the blog about this basically because I don't have anything interesting to say about Conway. For a discussion hosted by someone who does, see Scott Aaronson's blog:
https://www.scottaaronson.com/blog/?p=4732

I did several years ago write here
https://www.math.columbia.edu/~woit/wordpress/?p=7910
about the excellent biography of Conway by Siobhan Roberts, which I highly recommend to anyone who would like to learn more about Conway and his remarkable life.

10. **Ralf Hofmann**
April 16, 2020

A true loss for science and the human race as a whole. Phil Anderson has furthered the field of Theoretical Physics in many ways towards a deep credibility of the concept of Emergent Phenomena – well beyond the realm of conventional condensed matter physics. A man of unwavering reason, stable convictions, true ingenuity, and highly useful, lasting contributions.

11. **Slava Rychkov**
April 19, 2020

"The field theoretical equivalent of a plasmon is now called `a Higgs boson'" (Ph. Anderson "Basic notions of condensed matter physics" (1984), p.44, top of the page.)
No comment.

12. **Nick Maiorino**
May 4, 2020

Hi Peter,

I just learned of this obituary notice (dated May 1st, 2020) for the passing of **Philip Anderson** in the publication “**Nature**“:

Philip W. Anderson (1923–2020)

https://www.nature.com/articles/d41586-020-01318-4

Latest on abc

April 3, 2020
Categories: abc Conjecture

Davide Castelvecchi at Nature has the story this morning of a press conference held earlier today at Kyoto University to announce the publication by *Publications of the Research Institute for Mathematical Sciences (RIMS)* of Mochizuki's purported proof of the abc conjecture.

This is very odd. As the Nature subheadline explains, "some experts say author Shinichi Mochizuki failed to fix fatal flaw". It's completely unheard of for a major journal to publish a proof of an important result when experts have publicly stated that the proof is flawed and are standing behind that statement. That Mochizuki is the chief editor of the journal and that the announcement was made by two of his RIMS colleagues doesn't help at all with the situation.

For background on the problem with the proof, see an earlier blog entry here. In the Nature article Peter Scholze states:

> My judgment has not changed in any way since I wrote that manuscript with Jakob Stix.

and there's

> "I think it is safe to say that there has not been much change in the community opinion since 2018," says Kiran Kedlaya, a number theorist at the University of California, San Diego, who was among the experts who put considerable effort over several years trying to verify the proof.

I asked around this morning and no one I know who is well-informed about this has heard of any reason to change their opinion that Mochizuki does not have a proof.

Ivan Fesenko today has a long article entitled On Pioneering Mathematical Research, On the Occasion of Announcement of Forthcoming Publication of the IUT Papers by Shinichi Mochizuki. Much like earlier articles from him (I'd missed this one), it's full of denunciations of anyone (including Scholze) who has expressed skepticism about the proof as an incompetent. There's a lot about how Mochizuki's work on the purported proof is an inspiration to the world, ending with:

> In the UK, the recent new additional funding of mathematics, work on which was inspired by the pioneering research of Sh. Mochizuki, will address some of these issues.

which refers to the British government decision discussed here.

There is a really good inspirational story in recent years about successful pioneering mathematical research, but it's the one about Scholze's work, not the proof of abc that experts don't believe, even if it gets published.

Update: See the comment posted here from Peter Scholze further explaining the underlying problem with the Mochizuki proof.

Comments

1. **PC**
   April 3, 2020

   Publication of RIMS does not really qualify as a major journal. So everything is going to be alright…

2. **Cosmological Constance**
   April 3, 2020

   In Fesenko's article one finds the following remarkable line:

   "Misinformation and disinformation in science has become a very serious issue, not only for mathematics."

   I think this is in fact a very relevant point – but, perhaps, just not in the way that Fesenko intends it.

3. **Peng**
   April 3, 2020

   Serious question.
   I have looked through some of Mochizuki's rebuttals. I was amazed by the almost "internet insults-as-defensive" language thrown in there. As well as his pedagogical choice of describing things with layers upon layers of analogy, that could be said more directly. Especially when some of the analogies seem to be chosen almost as if to insult someone if they question it. It feels like what a physics theorist I knew used to joke as some authors trying to "prove by intimidation". But that's in physics, where a bit of hand waving is allowed (no point in waiting for a millenial prize problem to be solved to posit an effective field theory). It's confusing how this manner of dialog aids any purpose in mathematics.
   -
   Anyway, I got a bit off tract. My question is:
   Is it possible the style / pedagogical choices are actually causing real problems? Would it be helpful at this point if someone that states they understand the theory (but unlike Fesenko, will explain in a more useful manner than Mochizuki), if they rewrite the proof in their own words? Or at least the more difficult portions?
   -
   Or is the claim that it is pedagogically impossible to present the more difficult portions in any different manner?

4. **IWonder**
   April 3, 2020

   Castelvecchi characterizes the style of Mochizuki's papers as impenetrable and idiosyncratic, with which I wholeheartedly agree. However, I wonder why in this discussion Go Yamashita's write-up (http://www.kurims.kyoto-u.ac.jp/~gokun/DOCUMENTS/abc_ver6.pdf) has apparently received so little attention. While I unfortunately can't follow the mathematics, I find the style to be very clear and traditional in the best sense. In particular, he gives a proof of the infamous IUTchIII Corollary 3.12 (Corollary 13.13 in his paper), which I find to lend itself better to a discussion.

5. **Peter Woit**
   April 3, 2020

Peng/IWonder,
For a long time the style, length, organization and idiosyncrasies of the Mochizuki papers seemed to be the main problem, keeping experts from being able to fully understand and thus check the proof, and Go Yamashita's version promised to improve the situation. But once Scholze and Stix identified a specific issue, spent a lot of time discussing it with Mochizuki, and ended up convinced this was a gap in his proof, that completely changed the situation. Few people are going to devote a lot of time to studying a very complicated proof that at a crucial point has a gap. What's needed is for Mochizuki or someone else to put forward a convincing response to the issue raised by Scholze/Stix. This doesn't seem to have happened, and behavior like attributing the problem to Scholze being an incompetent not only doesn't help, but just convinces others that engaging with Mochizuki and those around him to better understand the issue is a waste of time. The announcement that the journal will publish anyway also doesn't help the situation at all.

6. **David Roberts**
April 3, 2020

Fesenko's article seems like a rehash of something he wrote a few years ago, "Remarks on aspects of modern pioneering mathematical research" (https://www.maths.nottingham.ac.uk/plp/pmzibf/rapm.pdf)

7. **sleep**
April 4, 2020

Talor Dupuy says
https://twitter.com/DupuyTaylor/status/1246142127538614272?s=19

Scholz may be wrong

8. **K. N.**
April 4, 2020

Hi. I do read both Japanese and English, and I feel like I'm obligated to give you more information on this matter which may only be provided in Japanese so far. First of all, I am not an expert nor trying to convince you whether IUT holds to be true or not; I am only providing additional information.
–
In 2018, Scholze and Stix released the document to show the famous fatal flow, so did Mochizuki to show the validity of his work. In April, 2019, Fumihiro Kato, a Japanese mathematician who verified IUT, has published a book about IUT for non-expert Japanese audience. A few months later, I checked Amazon Japan to view readers' reviews, which most people gave 5 or 4 stars. Then I found one negative review briefly saying, "Who's gonna read this book? Math community now knows there is fatal flow in IUT." Kato responded to this review. He posted the same response on his twitter account, so we know this response is really came from the author. He, in a very humble manner, stated that IUT is still in the middle of proofreading (at that time), but IUT experts are now confident to say that Scholze and Stix were simply misunderstanding lines of argument.
–
In January 2020, Mochizuki updated his private blog only available in Japanese. For while, people thought this blog is not Mochizuki's but is made up by some geek. Surprisingly, Kato has told us in his book that the author of the blog is indeed the Mochizuki himself. Anyway, the updated article indicated the Mochizuki's frustration for his papers not being published for such a long time, and he rebuked the publisher in quite strong language. He didn't mention the name of the journal, but readers were aware that was RIMS since the article was available only in Japanese. Which implies that he was not a chief editor of RIMS for the IUT matter. When RIMS had a press conference yesterday, they indeed explained that Mochizuki was completely excluded from refereeing the papers.

9. **KS**
April 4, 2020

Dupuy should have any lecture in Princeton for Diophantine geometers.
It may be a good possible way to realize certain understandings and consensus in mathematical community out of Kyoto.

10. **Peter Woit**
April 4, 2020

sleep/K.N.

For more of the twitter exchange with Dupuy, see here:
https://twitter.com/meu_gato/status/1246220210891190272
where he seems to state that he is not able to provide a convincing proof of the problematic Corollary 3.12 (and agrees that Mochizuki publishing this in the journal he is chief editor of is a bad idea, no matter what procedures were followed).

The problem here is simple: neither Mochizuki nor anyone else has written up a convincing response to Scholze-Stix. Fesenko has devoted pages and pages to ad hominem attacks on them, nothing to a technical refutation of their argument. He's trying to make instead the opposite of an argument from authority, arguing that Peter Scholze is an incompetent. No one is going to buy this. Mochizuki apparently believes there is no reason to make significant changes to his article to address their concerns. He's entitled to his viewpoint that he is correct and they just don't understand, but any author who wants to convince the math community that he has a proof needs to be willing to work to explain to others what they are not understanding.

So, sure, Scholze-Stix may be wrong, maybe Mochizuki's methods do give a proof. But an explanation of how this works convincing to experts needs to be provided and it hasn't been yet.

11. **Peter Woit**
April 4, 2020

KS,
I don't think what's needed is more lectures about IUT. What's needed is a more convincing argument for Corollary 3.12, and my reading of Dupuy's twitter commentary is that he says he can't provide it.

12. **KS**
April 4, 2020

In my opinion, the corollary may be related to core theoretic framework of IUT , especially as how scheme theoretic universes are constructed and unified.
Mochizuki have been emphasized in his terminology mono-analytic generalized distinction of scheme theoretic objects , but which aren't mere ordinary scheme theoretic ones.
True problem may be such difficulty of theory , rather than so called too simplification by Scholze-Stix.
I hope that Dupuy or some other experts will lecture Diophantine geometers on core ideas and strategies of IUT in for instance Princeton possibly.

13. **justcurious1**
April 4, 2020

Can a referee go public after publication is made, or is that poor practise? Or could at least the referee reports be themselves published? They would help form better judgement on the situation: the experts would either say "nothing conclusive here", or "ha, I had missed that, got it".

14. **W**
April 4, 2020

@Peng,

My understanding is that there exists a paper (1) written by Mochizuki (and others), (2) using the language of IUT theory, (3) that is correct, (4) which could be made much clearer by removing the IUT language and writing it in more typical mathematical style. Unfortunately the paper for which I understand this is not the papers on ABC, but rather a paper he wrote after, purely about fundamental groups / anabelian geometry. (My understanding comes from a friend who has read it.)

So one can certainly say that there exists a paper where the style is the main problem. It just may not be true for the main IUT paper.
.
@Peter I don't think you can fully separate the Scholze-Stix criticism from problems with style. My understanding is that the Scholze-Stix criticism comes from (what they see as) changing the style of the argument so that it is more clear, and then observing that the modified argument is wrong, but Mochizuki says that these changes are substantive. If the argument specifically of Corollary 3.12 were written in a different style, there would be no issue.
.
@IWonder I know Scholze has said at some point that other expositions, including Go Yamashita's, are not clearer on the crucial 3.12. I don't know if this is true, as I haven't read it.
.
@Peter I am actually optimistic about Taylor Dupuy's work, specifically because I believe that if, in the future, he says he understands the proof of Corollary 3.12, then he will be able to write a comprehensible proof of it. If this is true then there would be two possible resolutions of his project and this would be a good thing.

15. **David Levitt**
April 4, 2020

I know that mathematicians are weird – but this is completely off the scale. Peter, could you please explain if there is any mathematical reason why, in eight years, Mochizuki cannot find an alternative approach to his proof? Your statement that "He's entitled to his viewpoint that he is correct" is too kind. To me, as a non-mathematical scientist, it seems crazy.

16. **Peter Woit**
April 4, 2020

David Leavitt,
Yes, this is completely off the scale. Having a serious math journal hold a press conference to announce that they're publishing a proof of a huge longstanding conjecture even though the consensus of experts in the field is that the proof is flawed is extremely weird.

It's not surprising Mochizuki hasn't come up with an alternative proof. That could be hard and he thinks this one is correct. The question is why, if he and others are convinced the proof is correct, they can't come up with arguments that will convince others of this.

17. **Peter Woit**
April 4, 2020

justcurious1,
The tradition is to keep referee reports confidential, and in this case it's not clear if breaking the tradition would help. Much of the work of the referees was surely on parts of the proof other than the controversial Corollary 3.12. On the controversial part of the proof, they're siding with Mochizuki that what he has written is correct and complete. If they have an argument for this that would convince Schole-Stix and others, it would be helpful if that were made public. This all comes down to the same problem of what is missing here that is needed to justify acceptance of the proof.

18. **Shinichi Aoki**
April 4, 2020

I am a Japanese science journalist. I've been watching your discussions on this site with interest.
In Japan, Dr. Mochizuki's paper, published in PRIMS, has the media buzzing that the ABC forecast has been proven. However, I disagree. I have read your arguments and understand that some researchers have claimed that Dr. Mochizuki's theory is correct and has proven a weak ABC theory, but have not convinced many experts.
Is there any chance that this situation could change?
I think Dr. Mochizuki will have to explain and convince himself. What do you think, Dr.Woit?

19. **Peter Woit**
April 4, 2020

Shinichi Aoki,
For there to really be a proof, it needs to be convincing to a consensus of experts in the field, and that has not happened here (the great majority of experts are not convinced). Scholze and Stix accurately entitled what they wrote about this as "Why abc is still a conjecture"
http://www.kurims.kyoto-u.ac.jp/~motizuki/SS2018-08.pdf
Unfortunately there does not seem to be much change since that was written. The publication of the article does not help at all, even hurts, since it removes some of the motivation for Mochizuki himself to write an improved version of his proof that might convince others.

20. **mahmoud**
April 4, 2020

I have asked this before in the comment section here, but I will repeat myself in the hope that someone knowledgeable is reading it:

It is well known that an effective proof of abc would imply an effective Roth's theorem (and consequently effective versions of many other things, like Falting's theorem), Mochizuki however has (to my knowledge) never stated any explicit bounds as a result of his work. It seems that he initially thought his proof of abc was non-effective due to a specific reason, this non-effective step was later removed by someone else (I have forgotten his name). Therefore his claimed and now published proof appears to have the feature that no explicit bounds follow from it, yet no one seems able to point to the source of this non-effectivity. To me that is a very strong indication that the proof is not complete.

And isn't the above a stronger reason to doubt the correctness of Mochizuki's work than the appeal to the authority of Scholze? Or har Mochizuki commented somewhere on why the proof remains non-effective?

21. **David Roberts**
April 4, 2020

> Therefore his claimed and now published proof appears to have the feature that no explicit bounds follow from it, yet no one seems able to point to the source of this non-effectivity.

this is not true. Dupuy and Hilado have derived very explicit estimates for Szpiro's conjecture in their work taking Cor 3.12 as a black box, and have just released a (pre-arXiv) preprint in Twitter. As in: "Let A_0 = 84372107405, B_0 = 316495, then the absolute value of the minimal discriminant of an elliptic curve E/F satisfying the standing hypotheses is bounded by … [explicit expression involving these constants and other known quantities]". This uses work of Serre–Tate is a serious way, and their reworking of Cor 3.12 into non-IUT language (that's another long paper they are working on). I don't know how you get more explicit than this.

There's also older work of Dimitrov (https://arxiv.org/abs/1601.03572) showing that one should get effective bounds ("there exist computable functions such that… [inequality]"), but I don't think he actually gives them.

22. **W**
April 4, 2020

@mahmoud I think the issue is that Mochizuki's work does give explicit formulas, they are just so weak you would have trouble finding an explicit countereaxample. Then any reduction argument to another problem would give you even weaker bounds. Taylor Dupuy and Anton Hilado have been working on giving explicit formulas on the statement https://www.dropbox.com/s/hwdxtpk5ydqhp6g/thm1p10-short.pdf?

If you look at Theorem 1.0.5 you can see constants like exp of 84372107405 times ell^4, where I think ell is a prime greater than 19, and thus must be at least 23. That's before you reduce from a general problem to Mochizuki's specific case.

I think Mochizuki has more precise estimates in his writeup, but they're also harder to understand.

23. **Felipe Lopes**
April 4, 2020

Here's my take on this situation. I know a considerable amount about elliptic curves and Abelian varieties, but the stuff on most high-level mathematics is way over my head.

It seems to me that Mochizuki's theory is an attempt to treat some classes of mathematical objects in the same way. That's not out of this world. The objections by Stix and Scholze boil down to a point where they believe this treatment is flawed. And Mochizuki answers that they do not understand his theory.

In all these years, several mathematicians were in contact with Mochizuki's group. If the relations he puts forward were so flawed that his proof is beyond repair, then there's something fundamentally wrong with his theory from the start, and the error would be much greater than a small passage in the controversial corollary.

Instead, what we are seeing is that there are many mathematicians open to the idea and are working on the theory instead of dismissing Mochizuki as a crank.

I find it completely possible that somewhere in the papers Mochizuki made a mistake that's very hard to fix, just like Wiles did when he proved FLT.

At the same time, I also find it possible a Heegner-style scenario, where someone outside the mainstream math community proved a theorem using 19-th century math, and people dismissed it on grounds that it cited an incorrect result, which turned out not to be flawed upon closer inspection.

In any case, I see little reason to not publish Mochizuki's papers. Even if his results are not perfect, many people seem to believe it's a viable route to attack the problem. That's enough for me.

24. **Peter Woit**
April 4, 2020

Felipe Lopes,
The latest version of Mochizuki's papers have always been available online, and anyone who wants to work on this has all they need. The only difference with having a published version is that the journal's editors have put their own reputation and that of their journal behind the claim that the arguments in the papers have been checked and are valid and complete. Given that the consensus of experts is still that this is a flawed proof, I don't see what the PRIMS editors are accomplishing here other than putting a torch to their journal's reputation.

25. **mahmoud**
April 4, 2020

Thanks to David Roberts and w for correcting me! I'm not sure how comforting it is that Dupoy & Hilado obviously have worked hard on this and still are unable to make sense of the proof of Mochizuki's 3.12 though…
(Vesselin Dimitrov was the name I couldn't remember in my first comment.)

26. **KS**
April 5, 2020

I want put here substantial reference on this issure , namely the corollary 3.12.
Mochizuki updated a his own survey which he recognize in his blog(in Japanese) as a most important summary on the heart of IUT which available at his web-site , the title as below .

The Mathematics of Mutually Alien Copies: from Gaussian Integrals to Inter-universal Teichmuller Theory.
(crucial arguments are about P142-146(example 3.11.4)).

In both above place and his blog , he is explaining that Scholze-Stix 's misunderstanding is caused by confusing logical relation ∧ with ∨ on the θ-link , so there is no problem in the theory originally according to Mochizuki himself.
Especially , there is an emphasis that θ-link should be absolutely "∧-unification" of Hodge theaters in the system of "log-theta lattice" and its horizonal sequence.
And he said in his blog(again to say , in Japanese but substantially well-argued in an above survey paper) , "logical operator ∧ " is basically stronger information than ∨ , in the sense of restriction of the relation.

As my "unreliable personal" undersrtanding , this arguement can be seen as just "how scheme theoretic universes are unified and computed systematically" in the context

of "arithmetic geometric(but mono-analytic , topological group theoretic) " construction of each of θ-divisors over global number fields.
Here , we should recall that θ-link is basically distinctly-unifying device of the Hodge theaters.

Anyway , I think there is any public need of the response to this by Scholze-Stix although it's not certain whether they recognized this matter or not.

27. **Vesselin Dimitrov**
April 5, 2020

All of this is confusing. Let me at least try to clarify something about effectivity and the external reduction.

@David Roberts: One can certainly be more explicit by spelling out – if not bypassing – what exactly those restrictive standing hypotheses mean. This paper by Dupuy and Hilado actually involves all of Mochizuki's special assumptions (from I Def. 3.1) occurring in his already explicit claim IV 1.10, and even more: [D-H] furthermore require the elliptic curve to have a good reduction at all prime ideals of residue characteristic 2. The latter is not assumed by Mochizuki (he rather states an explicit bound on the prime-to-2 part of the minimal discriminant, under his other assumptions in I Def. 3.1); and the Belyi maps argument does not allow a reduction to the case of good reduction above 2 (it only reduces to a situation of bounded contributions at 2). Besides, the true exponent is a "6" (as in IV 1.10) rather than the lossful "24" in [D-H]. My understanding is they rather want to make the statement more conventionally readable.

In my note you linked to, I simply explain that there is nothing inherently ineffective in the external reduction of the full abc conjecture to this explicit but restrictive statement IV 1.10. This is really disjoint from [D-H], the point being exactly to bypass the special assumptions. Yes, the increase in the constant will be rather large if fully worked out this way. Anyways the essence of the reduction is exemplified by the following representative statement that had not been known before Mochizuki's "Arithmetic elliptic curves in general position." It is one undisputed contribution of Mochizuki's to the subject of abc that will survive regardless of the ultimate outcome of the IUT saga: Over general number fields F, in the version where an [F:Q] dependence is included, the sharp (6+epsilon exponent) Szpiro discriminant-conductor conjecture is already equivalent to the a priori stronger sharp (1+epsilon) upper bound on (twice) the full Faltings height including its Archimedean term; with a change of constants that is traceable to a computable function of epsilon and [F:Q] alone.

@mahmoud, It seems Mochizuki has neither acknowledged nor disputed that his indirect appeal to compactness in "Arithmetic elliptic curves in general position" is straightforwardly turned around into a constructive argument, as I outlined in https://arxiv.org/pdf/1601.03572.pdf . But, along with his collaborators, he has subsequently gone about for effectivity in a different way:

I understand (cf. this announcement: http://www.uni-goettingen.de/en/77723.html?cid=20836&date=2020-01-23&fbclid=IwAR1DYidCLs2cwKcBE3TRQmiEwP8WXfSSiYvb5C2SR5rfX9JWKGxF7IYUeGw) that Mochizuki et. al. have since gone on to expand the claim of IV 1.10 further by also directly including the contribution to the minimal discriminant at 2. That would bypass the external recourse to Belyi maps insofar as the Szpiro conjecture over Q (not strong abc over Q with the "1+epsilon") is concerned. If they could really also directly add the Archimedean term of the Faltings height (as was claimed at one point in 2018), it would have yielded a full and practical explicit strong abc bound, and the connection to Siegel zeros would have gone through as well.

28. **PC**
April 5, 2020

@Peter Woit "I don't see what the PRIMS editors are accomplishing here other than putting a torch to their journal's reputation." For one thing they are making a lot of publicity for the journal. Whether this is going to harm or boost PRIMS in the long run remains to be seen.

29. **Peter Woit**
April 5, 2020

KS,
Mochizuki's claim "that "Scholze-Stix 's misunderstanding is caused by confusing logical relation ∧ with ∨" is not plausible and Scholze has in some sense responded to it with his statement to Nature that nothing has changed. This is just a variant of Fesenko's claim that the explanation for Scholze-Stix not accepting this proof is that they are incompetents, and equally hard to take seriously.

30. **Sam Hopkins**
April 5, 2020

I understand that other sciences have highly contentious episodes somewhat comparable to this one, and that of course in mathematics there are some people who maintain for a long time they've proved some important result although the community does not accept it (e.g. de Branges and the Riemann hypothesis), but I can't think of anything in math really similar to this Mochizuki stuff. Does anyone know of a historical parallel?

31. **Peter Woit**
April 5, 2020

Sam Hopkins,
People sometimes point to Heegner's proof of the class number 1 problem, published in 1952, but mistakenly assumed by experts to be incorrect until it was reexamined in the late 1960s. As far as I know though, the supposed problem with the proof was raised after its publication. I've never heard of anything like a journal going ahead and publishing a proof over a consensus of experts that it has a gap.

32. **DS**
April 5, 2020

Hi Peter,
I read that SM admits that it might be impossible to derive intermediate results from his purported abc proof, but has anyone else proved any statements about number theory or arithmetic geometry using IUT that are generally accepted (or provide a new proof even of known results)? Basically, has IUT been able to expand beyond its own shores?

33. **Peter Woit**
April 5, 2020

DS,
You need someone more expert than me for an informed answer to this. I haven't though heard of any major results crucially using the IUT stuff. For a related answer, see the comment above
https://www.math.columbia.edu/~woit/wordpress/?p=11709#comment-235908
from W.

34. **EricB**
April 5, 2020

From The Asahi Shimbun http://www.asahi.com/ajw/articles/13271575

The popular media seem to think it’s a done deal:

Editors of the journal of RIMS asked outside experts to peer review the articles for any problems.

In late 2017, it appeared that the articles would be published, but mathematicians in the West pointed out what they considered inappropriate leaps in logic in a core portion of the articles.

That led the journal editorial board to continue with their assessment. A number of other outside experts were consulted and it was only in February that Mochizuki’s proof was considered to no longer have any problems.

35. **David Roberts**
April 5, 2020

> Yes, the increase in the constant will be rather large if fully worked out this way.

The constant in the inequality is, even in D–H’s version, taking the curve E11a1 for concreteness (they check this satisfies the hypotheses of being “in initial theta data”) something like 10^86858896380650, *at the smallest*, if we take \ell = 7. So the bound can only get worse? (!!) I don’t know what the appropriate minimal discriminant is that this is supposed to be bounding, but this is not exactly a practical estimate.

36. **tulpoeid**
April 6, 2020

Naive question: Doesn’t “Publications of the Research Institute for Mathematical Sciences” sound like an institute’s own publication? If yes, they are morally entitled to publish their own work, with lower peer-review standards than for global-reach journals, isn’t it so?

37. **Peter Woit**
April 6, 2020

tulpoeid,
Different math journals do have different standards, and may have a mission of publishing their own researcher’s work. But “lower standards” means willingness to publish correct but not very interesting work, or maybe even correct but not very well written papers, not publishing incorrect papers.

38. **David J. Littleboy**
April 6, 2020

There was a front-page article in the Asahi Shinbun (one of the major Japanese newspapers) in the morning edition on April 4. An enormous article that took up about 1/3 the page, with the title in only a slightly smaller font than the lead article.

First, the old news, just for the record:

It states that the four papers were submitted to PRIMS at the same time they were published on Mochizuki’s blog and PRIMS submitted them to multiple outside reviewers at that time. The magazine furthermore asked the outside reviewers to continue their review in the light of the 2017 criticism of the proof, but says that said reviewers have now concluded that the proof is correct.

What I don’t think has been mentioned here is last paragraph of that article.

It states that (a) proofs in pure mathematics of things such as the abc conjecture have in the past led to practical applications and (b) that IUT is expected to be a powerful tool for solving a wide variety of difficult problems in mathematics. Furthermore (c) that a new research center (whose purpose is to promote this theory) led by Mochizuki had been created within the Kyoto University math department in 2019 and that the Ministry of Education, Culture, Sports, Science and Technology has created an annual budget of 4 x 10^7 yen (slightly under 4 x 10^5 USD) for said center.

If the proof weren’t controversial, that last paragraph wouldn’t be notable in the slightest. FWIW, a native speaker non-mathematician friend after reading said article was incredulous that there could be any problem whatsoever with the underlying math.

39. **Peter Scholze**
April 6, 2020

I have been weighing back and forth commenting again on this matter. However, the news in that last comment by David J. Littleboy convinced me that it might be good, even if futile, to say something again.

I may have not expressed this clearly enough in my manuscript with Stix, but there is just no way that anything like what Mochizuki does can work. (I would not make this claim as strong as I am making it if I had not discussed this for with Mochizuki in Kyoto for a whole week; the following point is extremely basic, and Mochizuki could not convince me that one dot of it is misguided, during that whole week.) It strikes deep into my heart to think that in the name of pure mathematics, an institute could be founded for research on such questions, and I sincerely hope that this will not come back to haunt pure mathematics.

The reason it cannot work is a theorem of Mochizuki himself. This states that a hyperbolic curve X over a p-adic field K (maybe with some assumptions, all of which are always satisfied in all cases relevant to IUT) is determined up to isomorphism by its fundamental group $\pi_1(X)$, and in fact automorphisms of X are bijective with outer automorphisms of $\pi_1(X)$. Thus, the data of X is completely equivalent to the data of $\pi_1(X)$ as a profinite group up to conjugation. In IUT, Mochizuki always considers the latter type of data, but of course up to equivalence of groupoids this makes no difference. (The passage back and forth is even constructive, by another result of Mochizuki.)

Mochizuki claims that by replacing X by $\pi_1(X)$, things can happen that cannot otherwise happen. Examples are given concerning the action of $\pi_1(X)$ on certain associated monoids. We discussed this at very great length in Kyoto, but none of these examples carried any actual content. Note that any potential non-commutativity of some diagram that results from identifying $\pi_1(X)$’s via isomorphisms of X’s could not possibly be resolved by using some other isomorphism of $\pi_1(X)$’s — all of them come from isomorphisms of X’s! Mochizuki considers infinitely many distinct isomorphic copies of $\pi_1(X)$’s, but could not tell us what goes wrong if we simply identify all of them with one another, and with $\pi_1(X_0)$ for some fixed X_0 — there is no diagram that commutes in his situation but does not commute under this further identification. (In my manuscript with Stix, we simply went through Mochizuki’s argument with this further identification, pinpointing what goes wrong. If this further identification causes problems, just tell us which diagram it is whose commutativity is rescued by not explicitly identifying $\pi_1(X)$’s.)

However, what I really want to do with this comment is to point out that there seems to be significant confusion over just the above point on X’s vs $\pi_1(X)$’s. Recently, arXiv:2003.01890v1 appeared, in which the author (Kirti Joshi) gives some survey on results related to Mochizuki’s work. In the introduction, on page 7, he explicitly claims that one could find non-isomorphic X’s giving rise to the same $\pi_1(X)$, and even more, in Remark 2.1 on page 14 he explains that my reading of the situation is a common misunderstanding. Even more, in Corollary 21.2 on page 47, he states something “well-known to everyone at RIMS” giving an explicit example of this phenomenon of non-isomorphic X’s giving rise to the same $\pi_1(X)$.

With this appearing on arXiv, I was indeed quite confused — did I in fact misunderstand this basic point all this time? If the above claims would have been true, I would see how Mochizuki’s strategy might have a nonzero chance of succeeding. But I was quite sure that in our discussions in Kyoto, Mochizuki agreed with me on that basic point; and the proof of Theorem 21.1 in that survey (of which Corollary 21.2 is indeed a corollary) was wrong. In any case, I emailed Joshi indicating my confusion, and he has since checked back with Mochizuki and retracted all of these claims (he told me a new version will be on arXiv soon). In particular, the fact “well-known to everyone at RIMS” is wrong, and in contradiction to this earlier correct anabelian theorem of Mochizuki.

I’m really frustrated with the current situation. What EricB reports from the Asahi Shinbun also sounds deeply troubling, effectively arguing along national lines; again, this strikes deep into my heart. I’m really quite surprised by the strong backing that Mochizuki gets from the many eminent people (who I highly respect) at RIMS.

If I can in any way help to mitigate the situation, I’d be most happy to.

40. **Taylor Dupuy**
April 6, 2020

Hi Peter!

First, hope your pandemic is going well. Mine is going ok. Hard to get things done without daycare.

Second, let me say that for hyperbolic curves over a p-adic field K (with no extra hypotheses like strictly Belyi type or Belyi type or canonical lift) that pi_1(Z) “determines” Z is open. Also, I personally would advocate against using words like “determines”, “reconstructs”, etc that have been causing sooo many problems in discussing all of these things. For the uninitiated, let me say that what Peter claims here is that outer isomorphisms of fundamental groups as topological groups are in bijection with isomorphisms of curves over a field K. The difference between what Mochizuki did in his relative case together with his interpretation of G_K and the absolute Grothendieck conjecture I claim is subtle (maybe I am missing something though, I’m a little intimidated saying this so publicly to be honest).

–to see this is a non-trivial topic consider for example the introduction to this manuscript here: http://www.kurims.kyoto-u.ac.jp/preprint/file/RIMS1892.pdf

Mochizuki’s theorem states that for Z and W hyperbolic curves over a p-adic field K, outer isomorphisms pi_1(Z) \to pi_1(W) *over* G_K — meaning they morphisms in an overcategory where pi_1(Z) \to G_K and \pi_1(W) \to G_K — are in bijection with isomorphisms between Z and W.

To prove this, it suffices for example to show pi_1(Z) interprets the field K. I have written down an unreviewed proof which I don’t think it is so difficult which I can share if you want.

Anyway, I think what you are thinking about that that pi_1(Z) admits and interpretation GG(pi_1(Z)) naturally isomorphic to G_K and since we have both the fundamental group and the augmentation map we should be in the setting of Mochizuki’s theorem and are done. This is not quite correct. What is at issue is that given f:pi_1(Z) \to pi_1(W) one does not necessarily know that GG(f) is inner (which would puts you into the hypotheses of the Mochizuki’s proof of the *relative* Grothendieck conjecture).

Also, Mochizuki has conjectured in print it is absolute Grothendieck over p-adic is not true—-See Remark 1.3.5.1 of this paper: http://www.kurims.kyoto-u.ac.jp/~motizuki/Absolute%20Anabelian%20Geometry.pdf but seems to be nonspecific nowadays. I don’t know. We would need to ask him.

I personally believe if it is true then there is sort of a Zilber trichotemy/diochotemy thing going on. That is “absolute Grothendieck over p-adic” iff pi_1(Z) interprets a field. Maybe I am using Zilber trichotemy/dichotemy wrong, but I stand by the statement. The reason for this is because in all the special cases where absolute Grothendieck over a p-adic field holds, (canonical lifts, beyli type, strictly belyi type) there is an interpretations of fields. Maybe this is wrong though and a reader can point out an example where we know the absolute Grothendieck conjecture over p-adic fields without some special hypothesis being imposed.

Third, your counterpoint is a heuristic and not a disproof. For example, Dieudonne modules are equivalent to finite group schemes and we don’t call them worthless. We could go blue in the face coming up with examples of two equivalent objects one of which is useful and the other which is not. Maybe I am missing something, but I’m not sure how productive these meta discussions are. I think your point is that he needs to “use something”/”do something”. I agree, but it is not a disproof.

Alternatively, we can argue who has the burden of proof here… which I think is a more compelling argument for rejecting the whole thing. I think we can all agree the notation in his manuscripts are a big dumpster fire.

Fourth, regarding the deformation theory, his entire theory is up to a generic isomorphism (=polyisomorphism) the base G. That is the fundamental objects he considers are (like) a fundamental group Pi with a map Pi \to G_K where G_K is considered up to automorphism. In fact, any time he does one of these polyisomorphisms (which I think we should be calling generic isomorphisms), we should probably be thinking of some sort of poor man’s universal family.

Next we can ask “does this *DO* anything”? Well, for one, it certainly provides a formalism for talking about how things change under automorphism. There are a lot of interesting representations of automorphisms of fundamental groups acting on various interpretations which seem to me to be vedry non-trivial… etale theta being a principal example. Here is another thing I haven’t been able to puzzle out but maybe you can help: Let Z be a hyperbolic curve over a p-adic field. Fix the augmentatio map f:pi_1(Z) -> G_K, let g:G_K -> G_K be and outer isomorphism. What is the base change of f by g?

pi_1(X) \times_{G_K, g} G_K = ???

It is like some sort of Frobenius twist looking thing—except not twisted by the Frobenius but by some outer morphisms of the fundamental group of the base. Is this the fundamental group of a curve? Anyway, it seems to me you can apply many of his interpretations to this object and that constructions are uniform in this sort of thing. This I am not 100% on and wish I didn’t have to talk about these things I don’t understand so well publicly. Maybe the readers can tell us what breaks here.

So, that’s all I have to say about that for now.

Best,
Taylor

P.S. Can someone tell me how to add new lines? Hopefully I can edit my comment later. Oooh, looks like spaces are included. They just didn’t show up in the preview. Niccce.

41. **W**
April 6, 2020

@Taylor Dupuy

Your clarification is very helpful for arbitrary curves, but as you say at the beginning, is relevant when there are no extra hypotheses like strictly Belyi type. Since IUT focuses on etale fundamental groups of once-punctured elliptic curves, and these are of strictly Belyi type, I don't see why they are helpful.

In case anyone following is confused about what theorem of Mochizuki is meant, it is Corollary 1.10 of his paper "Absolute Anabelian Cuspidalizations of Proper Hyperbolic Curves". (Not his earlier theorem about arbitrary curves.)

Even if this "stricly Belyi type" condition were somehow avoided, then the existence of extra isomorphisms of the abstract fundamental group would still be an open problem. It seems hard to imagine how these isomorphisms could ever be used in a proof of some concrete inequality between two real numbers without proving that at least one exists.

It is probably possible to make rigorous a lot of what Peter is saying, that any proof of XYZ form cannot possibly work, or maybe more precisely that Mochizukki's proof is equivalent to the wrong proof sketched by Scholze and Stix.

But one also should include not-completely-rigorous evidence when deciding on how much burden of proof to assign to the author of a paper. The fact that every plausible use of a particular mathematical construction would not help in a particular argument, but the construction adds unnecessary complexity to the argument, and thus would make it harder to see any mistake, and two smart mathematicians have tried to remove the construction and found an incorrect argument, is strong evidence against the paper, even though it is possible that (1) there is a different way to remove the construction which leads to a correct argument or (2) there exists a clear explanation of how the construction is used to solve a problem which is not possible without it.

> Fix the augmentation map f:pi_1(Z) -> G_K, let g:G_K -> G_K be and outer isomorphism. What is the base change of f by g?…. Is this the fundamental group of a curve?

Isn't the content of Mochizuki's theorem that, if Z of strictly Belyi type, then this is not the fundamental group of a curve? Of course this may be a curve in some other case.

42. **DL**
April 6, 2020

W writes:
>My understanding is that there exists a paper (1) written by Mochizuki (and others), (2) using the language of IUT theory, (3) that is correct, (4) which could be made much clearer by removing the IUT language and writing it in more typical mathematical style. Unfortunately the paper for which I understand this is not the papers on ABC, but rather a paper he wrote after, purely about fundamental groups / anabelian geometry. (My understanding comes from a friend who has read it.)

I'm not sure if I'm the friend to whom W is referring here, but I have had this experience and wanted to add a brief comment about it. I've gone through this paper:

https://projecteuclid.org/download/pdf_1/euclid.aspm/1540417834

and this paper:

http://www.kurims.kyoto-u.ac.jp/~yuichiro/rims1870revised.pdf

with some amount of care, and concluded that despite the non-standard language, they are both essentially correct. As W writes, either could be made much clearer (and shorter!) by rewriting them in standard language. What's worth noting here is that I was able to understand what was written in these papers and convince myself that they were correct, despite the non-standard exposition. On the other hand, I was unable to do this with crucial parts of the IUT papers, even after putting substantial time into doing so.

43. **Vesselin**
April 6, 2020

@W, just to fix your precise reference for the convenience of others following along: You mean either Corollary 2.3 on page 500 of that paper (Absolute anabelian cuspidalization of proper hyperbolic curves); or Corollary 1.10, part (iii) on page 43 of the subsequent paper (Topics in absolute anabelian geometry III).

Those disprove Corollary 21.2 of Joshi's preprint.

44. **Peter Scholze**
April 6, 2020

Taylor, thanks a lot for your answer!

Regarding your first point: Yes, I'm doing fine in these strange times; I hope you are too. Fortunately we are still allowed to enjoy the incoming spring.

Regarding your second point: As W observes, your objection only seems relevant when the curves are not of strictly Belyi type, but all the relevant ones for IUT are. So this objection is a red herring. (Meanwhile, it is clearly interesting to sort out whether strictly Belyi type is necessary, and I wish you luck in improving Mochizuki's anabelian results!)

Regarding your third point: Of course I am very well aware of the power of category equivalences. But there must be something you can do on the other side that you can't do on the first. As I said, during one week Mochizuki was not able to give a single relevant example. So to me it seems like a category equivalence that simply obfuscates things.

Regarding your fourth point: I am at a complete loss what one wants to do with full poly-isomorphisms. For the convenience of readers following along: A full poly-isomorphism between two isomorphic objects A and B of a category C is the set of all isomorphisms between A and B. Mochizuki often says that he identifies two objects A and B along the full poly-isomorphism. To be clear, this is (up to equivalence of categories) no data at all, there is a unique full poly-isomorphism, so you can't "pick one" (or rather, you can always pick one, and only one). On the other hand, to identify to objects in a category, you need to pick a specific isomorphism! Of course, you can just pick any one of them, but you can't pick all of them! If A and B are sets and $a\in A$ is an element, then what is the image of a in B under the full poly-isomorphism? It makes no sense. Mochizuki has repeatedly told us in Kyoto that because some diagram does not commute when A and B are identified via the obvious isomorphism (that usually exists in his situation), he has to identify them only along the full poly-isomorphism. But (in his situation) there is not a single isomorphism between A and B that makes the diagram commute! So how does the full poly-isomorphism help? You can't make the logarithm map into a map of fields by pre- and postcomposing with field automorphisms!

(To be clear, I am willing to accept that there is a nonzero chance that some of these things might make sense under certain circumstances. Again, let me stress that we discussed these very matters for one week in Kyoto.)

From what I understand, the objection to my manuscript with Stix is that we did some identifications that are not allowed. This is just the identification I was talking about: Mochizuki considers infinitely many distinct copies of $\pi_1(X)$ and is only allowing himself to identify them along the full poly-isomorphism. We do not see any diagram that commutes with this choice, but does not commute when we identify them along the identity map, once taking all $\pi_1(X)$ to be equal to the actual

$\pi_1(X_0)$ for our fixed curve X_0. In any case, the passage from X's to $\pi_1(X)$'s is giving you no extra flexibility, as discussed in my previous comment.

Best wishes,
Peter

45. **Taylor Dupuy**
April 7, 2020

Hopefully this is productive…

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second point (and W's comment on the base change thing):
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I'm not sure how much to engage here since this isn't strictly logically necessary for a disproof. Since I think this is an important point in terms of our understanding of what is going on I'm going to say a few words. First, for the readers, Peter and W are focusing on the bad non-archimedeans cases, I believe. In this situation, I'm not 100% sure that all the curves in what I would call the "zoo of covers" are SBT but certainly most of them are as they are analytifications of base changes of hyperbolic curves from number fields. This is Belyi's Theorem. There are a number of other points that we should consider: there are log structures too which should make things even more rigid (my understanding is that you need this information for reconstruction of the special fiber), you have these punctured tempered universal covers (these are certainly not algebraic), there are groups involved in mono-theta environments going on, and stacks fundamental groups. My point is that it is still conceivable to me that there is some sort of deformation theory going on and I don't want to confirm or deny this. But yeah, a lot of them are. The base fields certainly are not.

In terms of uniformity of constructions in G or "strange base changes"; Mochizuki performs many constructions with respect to (Π,G) where for a pair we consider a generic isomorphism $\mathbf{G}(\Pi) \to G$ — here $\mathbf{G}$ is the interpretation of a structure isomorphic to G_K in Π. This makes G "up to automorphism". You need Lemma 1.1.4.ii of "Absolute Anabelian Geometry of Hyperbolic Curves" for the interpretation $\mathbf{G}$.

W: "Isn't the content of Mochizuki's theorem that, if Z of strictly Belyi type, then this is not the fundamental group of a curve?"

I would say no, this is not the content of his theorem. But I don't know what you mean here. Mochizuki's theorem is a statement about morphisms not objects. The begining of the claim is

"*given* that $\Pi \to G$ and $\Pi'\to G$ are augmented fundamental groups of hyperbolic curves…"

There may be statements that allow you to classify when a group is the fundamental group of something but you will have to ask Daniel Litt or Emmanuel Lepage or Jakob Stix about this. They certainly will be much more knowledgable than me. By the way, this is what Mochizuki means when he calls things "bianabelian".

Let $\Pi = \pi_1(Z) \times_{G_K,g} G_K$. I think you can run the $\mathbf{G}(\Pi)$ interpretation on this thing. I haven't worked out what this does… it is unclear if $\operatorname{pr}_2:\Pi\to G_K$ is naturally the same as to $p_{\mathbf{G}}: \Pi \to \mathbf{G}(\Pi)$.

Also, Peter, don't hold your breath waiting for new results in this direction. What I said is pretty much the extent of what I "know" about absolute grothendieck conjecture over p-adic fields.

Also, I think we can all agree whether something can change or not due to Mochizuki's consideration of potentially lossy functors is one of the more interesting parts of this story and it is important that we as a community clarify the situation. I think Kirti's emphasis on this is a good idea.

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fourth point:
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https://imgflip.com/i/3vr2vv

Full polyisomorphisms should be considered as "generic isomorphisms". I completely agree with this viewpoint. There are some instances where you have polyisomorphisms which not full and these keep track of finite indeterminacy rather than an arbitrary choice.

For better or for worse Mochizuki decided this is the language he wanted to use to describe these things. When things are omitted without justification the statements made are no longer Mochizuki's. We can get mad at him all we want for over questionable style choices but it doesn't change his assertions. This, I believe, is grounds for rejecting a paper, but it doesn't disqualify the proof.

I want to give a couple comments on polyisomorphisms for the readers.

Comment A) First, I want to point out a really really really bad style problem that come with this choice of "polyisomorphism language": Many commutative diagrams involving full polyisomorphisms are tautologically commutative which makes many claims vacuous.

example: Theorem 3.11.iii.a, the $\vdash \times \mu$ prime strip commutativity statements.

These are everywhere.

This style choice forces the readers to search unnecessarily for non-full polyisomorphisms which, frankly, is a big pain (an example of a non-full polyisomorphism can be found in the definitions of the bridges of the Hodge Theaters for example). This doesn't make him wrong though. Just not the best expositor.

Comment B) If A and B are isomorphic objects then $\operatorname{Isom}(A,B) = f \operatorname{Aut}(A) = \operatorname{Aut}(B) f$ for any fixed isomorphism $f:A \to B$. In my head I always "push" the generic isomorphism into an arbitary automorphisms of one object. This is pretty tautological and Peter already does this but I just wanted to say that applying this systematically allows you to reduce a lot of things and perform computations.

Comment C) I have spent a bit of time literally identifying objects if they had a polyisomorphisms between them. Although you can actually get some pretty interesting "global objects" this is not what Mochizuki had in mind. One example comes from the so-called full polyisomorphism mono-theta environments and the log-linked fields. If you do this you end up identifying a bunch of roots of unity in a bunch of different fields — this is well defined because the fields are all log-linked. Anyway, my point here is that this is not what Mochizuki had in mind so you maybe don't want to do this.

(I can talk at length about other style issues. Another example is invocation of interpretations "in Hodge Theaters". This is one I feel that just sends readers on a wild goose chase reading page after page of definitions. Things are typically defined by much less and in an optimal presentation one shouldn't consider superfluous structure when a reduct will do.)

Ok, so what do generic isomorphisms do besides confuse readers?

First, they are intended to keeps track of automorphisms. Mochizuki's theory is really an investigation of the behavior of interpretations under automorphisms and permutations of the interpreting structure. I say permutations because sometimes they don't respect the category and are maps of sets. The stupidest example I can think of: Let $G\to G'$ be a map of groups with kernel N. Set theoretic permutations of G which fix cosets gN setwise induce the identity on G'.

Here are some example questions Mochizuki addresses:
–How does the kummer class of (an lth root of a pullback of) the Jacobi theta change when you take an automorphims of $\pi_1^{temp}(\underline{\underline{X}}^+)$?
–What about the evaluation points (conj classes of decomposition groups)?
–What types of automorphisms/actions stabilize what construction? (I am thinking about mono-theta environments here and the purpose they serve)
–What happens to a the measure space we construct when we take automorphisms of the interpreting G? (this is what Ind2 is)

Second, generic automorphisms can serve as a sort of "poor person's deformation space". I think Kirti discusses this well in his updated manuscripts so I'm not going to talk about this so much. I think, as Kirti has suggested, we should be asking ourselves which of these generic isomorphisms are actually representable. Emmanuel Lepage, if he is reading, might be able to say more about the usage of full and essentially surjective functors from the perspective of Gerbes. He told me something about this once.

Third, regarding your "what is the image of an element under a full polyisomorphism". This goes back to what the point of all these things are. We are really investigating interpretations under automorphisms. At the end of the day we are looking to construct a "multiradial representation of the theta pilot object" which is a region obtained by a procedure involving certain orbits. This is supposed to be 1) independent of any choices and 2) relatable back to our original P_q. We partially address (1) in my first manuscript with Anton which should be available soon. You can find some stuff on my vlog about indeterminacy diagrams. Anyway, the point of these comments is that the polyisomorphisms are Mochizuki's way of dealing with choices, it is also safe to make a choice but you need to make things independent of this choice.

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fifth point:
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–Regarding your manuscript with Jakob, the construction as you have stated imposes two normalizations that are alleged to be simultaneously enforced which lead to a trivial contradiction of the form A=B and A!=B. I think we all agree on this. Mochizuki says this is a straw man. I can't find these assertions in the manuscript.

I do however think that statements of the form "All proofs that use X must have property Y" could be very useful provided 1) we can make X completely rigorous and 2) Mochizuki's language can be pinned down in a way that makes X verifiable.

Aside: Actually, this was part of my motivation for looking at these interpretations. If you can show that two objects are equivalent (e.g. Frobenioids and pairs (Π,M)) then if there exists a proof using Frobenioids then there exists a proof using pairs (Π,M). These sort of reductions allow you to make assumptions about the structures that are used in the proof. This is what safely allows us to get rid of unappealing constructions.

I personally find a lot of the language very hard to falsify/parse.

–Regarding the sentences "This is just the identification I was talking about: Mochizuki considers infinitely many distinct copies of $\pi_1(X_0)$ and is only allowing himself to identify them along the full poly-isomorphism. We do not see any diagram that commutes with this choice, but does not commute when we identify them along the identity map, once taking all $\pi_1(X)$ to be equal to the actual $\pi_1(X_0)$ for our fixed curve X_0."

First, I don't think this is a faithful presentation of Mochizuki's setup but one thing I can say that might make you feel better: in this particular example of bad non-arch primes setting $X_0 = \underline{\underline{X}}_{\underline{v}}$ (this should be a double underline) then automorphisms of the fundamental group do all sorts of things to the zoo of covers. All of those dihedral symmetries act, all of the $\underline{\mathbb{Z}}$ symmetries act etc. Also, all of the stuff that is not interpreted from a fundamental group stays fixed. This seems particularly relevant in the context of "monotheta cyclotomic synchronization", but again, if I could finish the proof we wouldn't be having this discussion at all.

Sorry if I made any mistakes anywhere…

46. **DL**
April 7, 2020

Taylor — you've written quite a lot, but frankly I don't really see why anyone should continue discussing this until someone can point to a specific spot in the IUT papers which defeats Peter's objection. For example, if I understand correctly, you suggest some juice might be obtained by looking at maps of pi_1 which are not over G_K. If so, where are such maps used in IUT? And Peter gives a specific challenge — point to some diagram whose commutativity is rescued by not making the identifications Peter makes. If there is such a diagram, where is it?

47. **David Roberts**
April 7, 2020

> not making the identifications Peter makes

I'm convinced there is no such diagram, because that is not how category theory works. The whole issue with identifications (as in: demanding objects are distinct copies vs having them be the same object) is a red herring. Mochizuki doesn't want to do it for spurious technical reasons that one can ignore (or alternatively, humour him and agree to play along). I'm more suspicious that people could be thinking about objects that are living in different categories. But what do I know?

48. **DL**
April 7, 2020

David — indeed there are objects in different categories being identified here (eg a curve and its fundamental group). That's because we're speaking English, not trying to make formal mathematical statements. Formally, one might (for example) look for a diagram which is not commutative but whose image under some functor becomes commutative, say. That is something that can happen in "category theory." In any case, I agree it's likely there's no such diagram.

49. **David Roberts**
April 7, 2020

@DL

There are more people reading here than just experts on arithmetic geometry, or even mathematicians. I'm also writing for their benefit. I'm just pointing out that your interpretation of Peter's challenge is not really the question one should be asking—or at least, the way it comes across to me.

But oh well. I should keep my head down, perhaps. Much easier to discuss this not via the internet, but we are all locked away after all 🙂

50. **W**

April 8, 2020

@Taylor Dupuy,

> I would say no, this is not the content of his theorem. But I don't know what you mean here. Mochizuki's theorem is a statement about morphisms not objects. The begining of the claim is

> "given that and
are augmented fundamental groups of hyperbolic curves…"

Despite what DL mentioned, I just want to respond to this bit…

OK but if we're asked if something happens and we're trying to prove that it doesn't happen we can try a proof by contradiction.

We have a curve X over K and a map pi_1(X) => G_K. We fix K' and an isomorphism G_K => G_{K'} which does not come from an isomorphism K => K'. You ask, does the composition pi_1(X) => G_K => G_{K'} arise from a curve X' over K'?

Suppose it does. Then we have two hyperbolic curves X, X' and an isomorphism between pi_1(X) and pi_1(X'). Then Mochizuki proves that, if X is of strictly Belyi type, this isomorphism arises from an isomorphism between X and X'. In particular, it implies the isomorphism on the Galois part arises from an isomorphism K to K', contradicting our assumption.

In fact, given Mochizuki's previous p-adic Grothendieck conjecture theorem, the new theorem (except for the part about open injections, maybe) is precisely equivalent to the statement that the "base changes" of fundamental groups of Belyi curves are not fundamental groups of curves, plus the fact that the geometric part is identifiable as a normal subgroup.

51. **kirti joshi**
April 8, 2020

Dear Professor Woit,

This is to clarify the claims made about my paper in the context of your abc blog-post.
First of all let me say that my paper (referred to by Scholze in his comments) is not a survey of Mochizuki's work (though it may initially appear so because I state a number of standard results without proofs) and contains a number of new and original results (Scholze agrees with me on this). I start with one of Mochizuki's ideas and I build upon it in my paper. This is the paper which is cited in Scholze's comments and which can be found on the arxiv (though the update is not yet ready). In our correspondence Scholze has agreed (mostly) with all the changes in the new version and we (i.e. Scholze, Hoshi and myself) continue to correspond to resolve any persisting issues (of which there are very few). These issues and changes, at any rate, do not pertain to main results of the paper but to how the contents relate to IUT. Since comments about the retracted section of the paper may add to the confusion in a topic which is already quite complicated for many reasons, so all references to my paper (below) will be to the forthcoming version.

To explain one of Mochizuki's important ideas, let us begin with a classical result which says that there exist p-adic fields (i.e. finite extensions of the basic p-adic field Q_p) which are not isomorphic but which have topologically isomorphic absolute Galois groups. For examples of such fields see my paper. (In the 1990s this was refined by Mochizuki: a p-adic field is determined by its topological absolute Galois group equipped with its ramification filtration (see the section: five fundamental theorems of … in my paper) for references to proofs of these results).

Two p-adic fields with isomorphic absolute Galois groups have distinct additive structures (the multiplicative groups of non-zero elements of such fields are even topologically isomorphic). So the additive structure is the one which is changing (even as the absolute Galois group remains fixed). Because of Mochizuki's Theorem one can view the (upper) ramification filtration as the Galois theoretic manifestation of the additive structure of a p-adic field.

One of Mochizuki's ideas, simply stated, is to treat the p-adic field as a dynamic variable while keeping its absolute Galois group fixed. Because of the above remarks, this should be seen as treating the additive structure of the field as a (dynamic) variable. If readers are uncomfortable with this idea, they can simply think of allowing the p-adic field to vary while its absolute Galois group remains fixed. This makes complete sense and comes with highly non-trivial consequences as my examples illustrate:

(1) I show in my paper (with explicit numerical examples) that two p-adic fields with isomorphic absolute galois groups do not have the same different and discriminants (these are standard measures of complexity of fields in number theoretic contexts). These examples can be easily verified by any one with a computer equipped with SAGEMATH and importantly more can be found by my methods.

(2) I show that if E/F is an elliptic curve over a p-adic field F and if L,K are two p-adic fields with isomorphic absolute Galois groups and both containing F, then the base changed curves E_K and E_L (i.e. E considered as curves over L, K respectively) do not have the same list of numerical invariants (in general). This is done by means of explicit examples computed using SAGEMATH (with no additional programming needed). Notably my examples establish quite clearly that the additive structure of the p-adic field controls many subtle invariants of elliptic curves over p-adic fields. At any rate computing these examples does not require any of Mochizuki's theory.

Important realization on which my paper is based is this: the upper numbering ramification filtration is a Galois theoretic stand-in for the additive structure of the field and through this stand-in, the additive structure leaves its imprint on Galois representations.

(3) Notably invariants in (1) and (2) are also the sort of invariants which are crucial in Szpiro's conjecture and my work shows that these quantities are affected by the changes in additive structure and so Mochizuki's idea of using the variation of the additive structure to understand Szpiro's conjecture might have significant merit (there are several other new ideas in Mochizuki's paper as he has reminded me on a number of occasions). Note that I am not claiming that this is exactly what happens in the context of IUT, but I am simply reporting my observations that these quantities are not determined by the isomorphism class of the absolute Galois group of the relevant p-adic field. I do not know how to use my examples to illustrate changes in the specific context of IUT.

(4) I also demonstrate that the idea of changing the additive structure (which I have called anabelomorphy in my aforementioned paper) can be used in the theory of Galois representations. In this theory L,K are two p-adic fields which have isomorphic absolute Galois groups, so one can pass from representations of G_K to G_L. This does not affect many broad aspects of the representations as these two groups have equivalent categories of finite dimensional representations. However this operation of considering a G_K representation as a G_L representation via any given isomorphism of these two groups does not preserve (p-adic) Hodge theoretic properties of a representation (for example an Hodge-Tate representation of G_K may not remain Hodge-Tate when considered as a G_L representation via an isomorphism of G_L with G_K). However I prove that an important subcategory, namely ordinary p-adic representations is preserved under this operation. This operation of viewing G_K representations as G_L representations is not the identity functor in general nor is it so on the subcategory of ordinary representations. One can say such things because I also demonstrate that important numerical invariants of a Galois representations, for example its Swan conductor changes (again there are explicit numerical examples which I provide). The fact that this operation does preserve ordinary representations is of importance not only in Mochizuki's work (which uses two dimensional ordinary representations arising from Tate elliptic curves), but also in the broader theory of Galois representations (Wiles, Taylor and most results in the area since then). This result opens up the possibility of wider applicability of Mochizuki's ideas to other areas of number theory. For additional results readers are referred to my paper.

These ideas, proofs and examples have nothing to do with the one theorem of my paper which I have admitted was incorrect (and the new version will appear on the arxiv on thursday or friday this week) and my error should not be viewed by the readers as an example of what is wrong in this business. [I sincerely apologize to my colleagues and friends in Kyoto and Japan for the incorrect statement in the old version of my paper and my assertion that this (incorrect statement) was "well-known to everyone in Kyoto".] Again let me be clear that my errors (if any) should not in any case be attributed as issues emanating from Mochizuki's paper.

(5) I explain in my paper that even though K,L have topologically isomorphic absolute Galois groups, it is possible to communicate meaningful (arithmetic) information between them. For p-adic fields this idea is due to Mochizuki. There is no direct interaction between the additive structures of these fields at any point.

(6) In my paper I have also pointed out the analogy between Scholze's work (deeply extending earlier work of Fontaine) and Mochizuki's idea (see the section on perfectoid spaces in my paper) and in our personal correspondence Scholze has said that he sees no issues with my claims in that section (modulo minor corrections). In particular I point out in that section that Scholze's work is founded on a similar idea of changing perfectoid fields, perfectoid varieties (instead of p-adic fields and curves over p-adic fields) while keeping the absolute Galois group (of the perfectoid field) fixed (resp. etale fundamental group fixed). In the parlance of perfectoid geometry this corresponds to moving from one untilt to another untilt–see my paper for details (or Scholze's paper). Deepening of this analogy (as Taylor Dupuy and I hope to do in an ongoing project) should provide further insights into this difficult topic.

(7) The following way of remembering Mochizuki's idea may be useful:

A p-adic field wiggles and moves around in the isomorphism class of its absolute Galois group. This wiggling is a (new) degree of freedom in number theory and in algebraic geometry.

(This is illustrated in my paper with explicit examples and also see Mochizuki's paper and Hoshi's work and other members of the Kyoto school).

52. **Peter Scholze**
April 8, 2020

Dear Kirti Joshi,

thanks for chiming in here, and I'm sorry for concentrating on the parts of your paper/survey that were wrong.

The issue of non-isomorphic p-adic fields that have isomorphic absolute Galois groups is potentially interesting, and it is worthwhile to study which invariants are (un)changed under such an isomorphism. However, it is unclear to me how this enters into the actual content of IUT. In particular, your example of taking an elliptic curve E over a field p-adic field F and base-changing to p-adic fields K/F and L/F with isomorphic absolute Galois groups is not relevant to IUT. Namely, $\pi_1(E_K)$ and $\pi_1(E_L)$ are not usually isomorphic, although $\pi_1(E_K)$ and $\pi_1(E_L)$ both can be computed as pullbacks $\pi_1(E)\times_{G_F} G_K$ resp. $\pi_1(E)\times_{G_F} G_L$ where all terms are isomorphic — but not the fibre product, as the maps $G_K\to G_F$ and $G_K\cong G_L\to G_F$ are not the same. (This was the essential mistake in Joshi's first version.)

Regarding (6), perfectoid geometry gives nontrivial examples of such relations, which however require a strong "softening" of algebraic varieties (to pass to perfectoid spaces). Let me stress that perfectoid spaces have precisely this flexibility of changing geometry while preserving topology (like π_1), while Mochizuki's theorems I alluded to in the first comment prove that for the hyperbolic curves he considers, the geometry is determined by the topology (in fact, by π_1). It is clear that being able to change geometry while fixing topology can be interesting — but Mochizuki is just not in a setup where this is possible!

Finally, regarding (7), while Mochizuki claims that his proof must use this fact that p-adic fields are not determined by their Galois groups in some way, it never actually enters — in particular, no construction of such exotic isomorphisms is given or cited. In this sense, as you also say at the end of (3), this whole discussion seems tangential to IUT.

Best wishes!
Peter

53. **PC**
April 8, 2020

Dear Kirti Joshi,

I had the impression that the theorem of Jannsen-Wingberg says that the absolute Galois group of p-adic field only knows the residue field, the degree over Qp, and the number of roots of unity in the field. Maybe I misunderstood the statement, but if what I just wrote is correct, it is quite clear that you will have p-adic fields with isomorphic Galois groups and different arithmetic invariants.

Also I am confused by "the additive structure of the field" as such a field is just a Qp-vector space of dimension the degree over Qp; so its additive structure is encoded in the Galois group. Maybe you meant the multiplicative structure?

54. **OP**
April 8, 2020

Setting aside the matter of correctness, the referees have not fulfilled a crucial part of their assignment: ensuring that new ideas are explained much better, at least in the Introductions. If that was not part of their assignment then the editors (who have to be aware of the reasonable perception of the situation by the broader arithmetic geometry community) have been asleep at the switch.

If I understand correctly, after a lot of time and effort Peter Scholze has not identified any new insight in the papers that makes meaningful progress on the ABC Conjecture. The papers in their present form are therefore unsuitable for publication in a good journal, regardless of anything else (much as if Terry Tao were to regard a paper in harmonic analysis as devoid of new ideas then it is unsuitable for such publication too).

@PC: perhaps what is meant is that an identification of multiplicative groups (arising essentially from local class field theory) won't also respect the additive structure upon appending {0} with its usual multiplicative property.

55. **Tomate**
April 9, 2020

The last link doesn't work.

56. **Peter Woit**
April 9, 2020

Tomate,
Thanks. Fixed.

57. **AP**
April 10, 2020

@PC: I think what you state is true if you consider the absolute Galois group only as a group, but if you consider it as a group together with its ramification filtration then it determines the p-adic field.

58. **Taylor Dupuy**
April 10, 2020

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W (=Will?) :
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I agree with your observation. Nice.

Here is my summary (slightly modified).

Lemma.
Let X be an SBT curve over a p-adic field K.
Let $\Pi = \pi_1(X) \times_{G,g} G$ where $g:G \to G$ is an outer automorphism (=not inner).

The map $\operatorname{pr}_2: \Pi \to G$ is not isomorphic to the map between a fundamental group of an SBT curve to its base field. (Maps between maps are taken to be the obvious pair of maps satisfying the usual commutative diagram).
Proof.
Suppose it is.
First the map $\Pi \to G$ can be viewed as $\Pi \to \pi_1(X) \to G$, or pr_2 (indexing starting at 1 and not 0). Call this map h. In this case we have a diagram

$$\begin{CD}
\Pi @>\operatorname{pr}_1>> \pi_1(X) \\
@VhVV @VVV\\
G @>g>> G
\end{CD}$$

Where the bottom map is the outer g. If they were both SBT by Mochizuki's relative Grothendieck theorem + interpretability of the field we have that pr_1 is geometric (the Grothendieck theorem is not just about isomorphisms but all morphisms). But the bottom map is outer. This gives a contradiction. (I'm assuming the p-adic relative Grothendieck for morphisms here… which I need to double check on).//

I can't parse your last remarks completely…

"In fact, given Mochizuki's previous p-adic Grothendieck conjecture theorem, the new theorem (except for the part about open injections, maybe) is precisely equivalent to the statement that the "base changes" of fundamental groups of Belyi curves are not fundamental groups of curves, plus the fact that the geometric part is identifiable as a normal subgroup."

Here are a couple more points (I'm not trying to be a jerk):
a) What do you mean by "the new Theorem"?
b) We still don't know if $\operatorname{pr}_2: \Pi \to G$ is the same as $\Pi \to \mathbf{G}(\Pi)$ where $\mathbf{G}$ is the interpretation I referenced previously.
c) I think we only get that $\operatorname{pr}_2:\Pi \to G$ is not the natural map from and SBT curve to its base not that it is not the fundamental group of any hyperbolic curve.

I believe we can resolve (b) and (c) with the Lemma below.

Lemma. $\operatorname{pr}_1:\Pi \cong \pi_1(X)$ is an isomorphism. In particular Π is isomorphic to the fundamental group of an SBT curve.
Proof.
Let $f: \pi_1(X) \to G$ be the map from the fiber sequence.
Since $\Pi = \lbrace (a,b) : f(a) = g(b) \rbrace$ the kernel of the map pr_1 contains elements of the form $(1,b)$.
Since $f(1) = g(b)$ this means b is in the kernel of g. Since g is an isomorphism $b=1$ and hence the map pr_1 is an isomorphism.//

This means the composition $\pi_1(X) \to G \xrightarrow{g} G$ is something that Mochizuki considers but it not the isomorphic (as maps) to some $\pi_1(X) \to G_K$ of "geometric origin".

Also, I think you made an earlier comment about verifying the inequalities. I personally think people should be looking at the Mochizuki's inequalities after 3.12 but before Theorem 1.10 type inequalities. In my manuscript with Anton we point to a couple places where improvements can be made; there seems to be a lot of room between the two inequalities. To do direct computations with Cor 3.12 it seems you need to work directly with Division Fields as in the work of Harris Daniels, Alvaro Lozano-Robledo, and Drew Sutherland. Stuff like this:
https://alozano.clas.uconn.edu/wp-content/uploads/sites/490/2014/01/lozano-robledo_minimal_ramification_Rev1_v2.pdf

(Maybe you can email me and we can talk about this more if you are interested.)

****
Daniel:
****
More comments on p-adic Grothendieck:

The existence of some isomorphisms of fundamental groups of curves over p-adic fields inducing isomorphisms of absolute galois groups of p-adic fields "not of geometric origin" seems to be related to the section conjecture but I couldn't figure out if there was an "obvious" implication. See the results here:

http://www.kurims.kyoto-u.ac.jp/preprint/file/RIMS1892.pdf

I wanted to say something like "if the section conjecture is False then there exists a hyperbolic curve Z over a p-adic field K and some automorphism σ of $\pi_1(Z)$ such that $\mathbf{G}(\sigma)$ is not inner." but I may be totally totally off here. Maybe you can salvage this? I'm trying to get a sense of how difficult this should be by reducing this to a "really hard" problem.

Also, while the pro-p relative Grothendieck conjecture is true the pro-p section conjecture is False. This is a Theorem of Hoshi. I don't understand these counterexamples but it is my understanding that Jakob views these as “accidents” or “lucky”. (pg 192 of his evidence for the section conjecture book.)

I’m going to postpone any more remarks about missing monoid structures in the setup for a later post (if I’m going to say anything at all) because I want to get it right for everyone.

****
PC:
****
What OP says is correct. It is a local class field theory thing. A classic theorem is that for K a finite extension of $\mathbb{Q}_p$ we have $G_K^{ab} \cong \varprojlim K^{\times}/K^{\times n}=: \widehat{K^{\times}}$ where G_K is the absolute galois group of K. There is a version of this “on crack” where you can describe the topological groups $\mathcal{O}_K^{\times}$ and $K^{\times}$ among other things ‘inside’ G_K. You only get multiplicative stuff though.

You can’t recover the full field structure (both binary operations satisfying the usual axioms) because there exists fields K_1 and K_2 with $G_{K_1} \cong G_{K_2}$ where K_1 and K_2 are not isomorphic. This is a theorem of Jarden and Ritter.

Anyway, at the end of the day you can have one binary operation or the other but not both. The equivalence of having multiplication or addition (but not both!) comes from p-adic logarithms (which can be defined ‘inside the group’). This is just the first isomorphism theorem of groups.

59. **Peter Woit**
April 11, 2020

Thanks to all commenters here for the remarkably informative discussion of the mathematics involved in the problem with Mochizuki’s claimed proof explained by Peter Scholze. Note an important aspect of this discussion: no one (including Joshi and Dupuy, two people who have been deeply involved in the study of IUT) has come forward to explain how Mochizuki can get around the problem pointed out by Scholze. The only place I know of publicly available that supposedly contains such an explanation is Mochizuki’s web-page
http://www.kurims.kyoto-u.ac.jp/~motizuki/IUTch-discussions-2018-03.html
The only relevant materials there are absurd ad hominem arguments from Ivan Fesenko and Mochizuki’s own comments. Scholze and Stix are the only two who have had the experience of directly engaging in extensive discussion with Mochizuki of the problem, so their report that he has no answer to the problem must be taken as authoritative in the absence of some other strong evidence. The past two years of study of the problem do not seem to have led to anyone besides Mochizuki himself being willing or able to try to explain how Mochizuki’s claimed proof avoids the problem, and all experts I know find his explanation unconvincing.

Given this, the decision by PRIMS to hold a press conference announcing that the proof has been checked and will be published is completely outrageous. It may be good PR in Japan, but it is seriously damaging to the reputation of RIMS in the math community and those responsible for that institution need to come forward and address the issue.

60. **abc**
April 11, 2020

Hi Peter Woit:

I feel surprised to see your comment, since Mochizuki did respond extensively in his personal blog:

https://plaza.rakuten.co.jp/shinichi0329/diary/202001050000/

I did not see any “absurd ad hominem argument” you are talking about. Scholze and Stix’s names did not even appear.

I am wondering – did anyone ever reached out directly to PRIMS’s editorial board on this? Isn’t it too quickly to reach a conclusion that “…is completely outrageous. It may be good PR in Japan, but it is seriously damaging to the reputation…” without some communication with the referees who have done respectable work on this paper? These people did spend eight years in going through the detail and understanding the paper. Should not these words be reserved for publishers that published Atiyah’s six sphere paper (https://www.springer.com/gp/book/9783319648125)?

I am not sure how you came to the conclusion that “…no one (including Joshi and Dupuy, two people who have been deeply involved in the study of IUT) has come forward to explain how Mochizuki can get around the problem…”. It is still possible that Mochizuki does not need to “get around the problem” because the problem does not exist in the first place; maybe it is caused by confusion and misunderstanding from both sides.

I feel confused reading through the blog. If someone with established expert status in a nearby field “give his/her nod” or “shake his/her head” regarding a paper, is this enough to replace the traditional journal referee process? If this is the case I would be more than happy to know Atiyah’s six sphere paper is complete, detailed, rigorous and correct – Atiyah claimed his proof was correct.

61. **Peter Woit**
April 11, 2020

abc,
I read Mochizuki’s blog entry when it came out. To the extent I could make sense of it via Google Translate, it appears to be an argument that the people who see a problem with his proof (eg Scholze-Stix) are simply not understanding it because they are too dim-witted to understand the difference between an “and” and an “or” in a logical argument. This is just completely absurd.

I agree completely that it is the responsibility of the PRIMS editorial board to put forward publicly whatever mathematical report they received from referees showing that Scholze-Stix had misunderstood the argument and that the problem they pointed to does not exist.

As for the comparison to the sad story of Atiyah’s delusion in his declining years that he had a proof of the six-sphere question, it’s very telling that you see any relation at all between these two proofs.

62. **UF**
April 11, 2020

As far as I understand the main objection brought up here by Peter Scholze is: Mochizuki considers infinitely many distinct isomorphic copies of $\pi_(X) $’s, but can not tell us what goes wrong if we simply identify all of them with one another, and with $\pi_1(X_0)$ for some fixed X_0 – there is no diagram that commutes in his situation but does not commute under this further identification.

It seems to me that this is adressed in (C7) in Cmt2018-05. Mochizuki claims there that if one (once and for all) identifies the various $\pi_1(X)$’s in one column (using the non-archimedean logarithms), then there is no switching symmetry between the two neighboring columns. On the other hand, if one identifies the $\pi_1(X)$’s just as topological groups via poly-isomorphisms, then Mochizuki claims that there is such a switching symmetry (i.e. some diagram commutes at this weak level).

It seems to me that in the proposed proof both viewpoints are used: The ability to rigidify the relationship between the $\pi_1(X)$'s in one column when needed (e.g., I think, on both columns for the log-Kummer correspondence), and then later the ability to forget about the rigid structure, to pass from the LHS to the RHS.

63. **naf**
April 12, 2020

One way to prevent this absurd situation, i.e., the "proof" of abc being published by PRIMS, might be to organise an open letter/petition. The reason that I think that this might be useful is that even though many well-known mathematicians have expressed their views on Mochizuki's papers, these have been scattered across several blog posts and comments. If there were a simple open letter, saying something to the effect that the undersigned have gone through the IUT papers and do not believe that they constitute a proof of abc, and this was signed by a relatively large number of experts, then this might have more of an effect and might even be picked up by the Japanese press.

Of course, for this to work some expert–I am not one, though I have read the papers–has to take the initiative.

64. **Peter Woit**
April 12, 2020

naf,
The problems with such a petition are:
1. There's a strong feeling among many that engaging in that kind of effort to get publicity for one side of a mathematics argument is inappropriate, that mathematical truth is not a topic for petitions.
2. Scholze-Stix are the only people who have spent the time directly engaging with Mochizuki needed to be completely sure, based purely on their own personal understanding of the mathematics, that he doesn't have an answer to objections to his proof.

On the topic of 2., a huge part of the problem here is that, eight years after the proof came out, as far as I know no one except Mochizuki is willing to publicly claim that they fully understand the proof and can explain it to others, in particular that they can convincingly explain why Scholze and Stix are mistaken. Even in Mochizuki won't travel or give talks explaining this point, why won't anyone else?

There are mathematical organizations that arguably have some responsibility here and whose boards should consider some action. In particular the EMS is the publisher of PRIMS, and perhaps the IMU should consider the issue.

65. **BR**
April 12, 2020

Peter Woit,

there is one aspect I find not really well represented in the current discussion. I think an author has also the right to make a mistake, and I do not mean deliberately, but in the process of scientific investigation. At the same time one has to take care not to inflate the meaning of scientific publishing and peer-review. It is a misconception to think the peer-review process should warrant absolute truth and correctness. Especially not in cases of relatively new and open fields.

In that sense one could take a lot of pressure out of the current debate, especially concerning the various close-to or ad-personam issues, if we would more agree on the fact that publication in an international journal is just another means of communication, if however one with a certain level of quality assurance.

These are just some thoughts that I have accumulated during 20 years of active scientific research.

66. **Peter Woit**
April 12, 2020

RB,
I think you and many of my readers are not familiar with the culture of publication in mathematics, which by the nature of the field has a different aspect than that of other sciences. Assuring that the proofs of theorems given in a paper are correct is the main concern of the reviewing process at a math journal (secondary concerns are how interesting the theorems are and expository quality). What is going on here is completely unheard of.

Mathematics has a rigid, unyielding quality that differentiates it from other intellectual subjects. Either a proof is correct, with the claim logically following from the assumptions, or it isn't. Unfortunately it's not good enough for an argument to be fine at all steps except one, and this makes proving an important new theorem a high-pressure business. One can invest years in something which in the end simply doesn't work. In order to make progress in mathematics, one needs to understand clearly when there is a correct and complete argument and when there isn't. Mathematicians have the advantage of a much clearer boundary between what is understood and what isn't than is typical in most fields.

Mostly this leads in my experience to a positive aspect of mathematical culture: people are used to finding that they are wrong and arguments about whether a mathematical argument is correct tend to not get personal. A colleague likes to explain that mathematics is the only subject he knows about where when two people go into a room disagreeing about whether something is true, almost always they come out with one of them agreeing he (or she) was wrong.

In this case, one side (Scholze-Stix) is making purely mathematical arguments, the other (Mochizuki-Fesenko) has engaged in unusual public argument about the competence of those who disagree with them. The very existence of this asymmetry is evidence for which side has the mathematics right.

67. **Peter Scholze**
April 12, 2020

As UF is trying to point to a specific objection to my manuscript with Stix, and it's the first real try in this thread, let me try to answer this.

Actually, the objection is bizarre. If all your copies of $\pi_1(X)$ are the same, then how can the situation be less symmetric? It's totally symmetric. It's not symmetric if you also look at some other structures present in Mochizuki's setup, like the log-link: This just means that the logarithm map is a different map from the one you obtain by switching source and target, which is self-evident (the logarithm is not its own inverse). It also cannot be salvaged by using some other/indeterminate identification of $\pi_1(X)$'s — this would at most change source and target by some field automorphism. So it's unclear to me how the passage from X to $\pi_1(X)$, and to $\pi_1(X)$ up to indeterminate isomorphism, is helping in this matter.

68. **BR**
April 12, 2020

Peter Woit,

thank you for your explanations, you are fully right, to imply I am not a mathematician (also in our circles Mathematics is not considered a science but part of humanities (which is a bad translation for "Geisteswissenschaft")). But for the one you don't hit the nail fully on the head if you mean that in (other) sciences the peer reviews primarily “concerns are how interesting the theorems are and expository quality”. What I meant to address is that the peer review should mainly check technical and scientific consistency (call it quality). And here, definitely in the “sciences” is still plenty of space for technically and “scientifically” pretty flawless works to turn out to be “wrong” at a later stage of affairs.

It seems much less likely, but I am not convinced that all is that digital (either wrong or right) as you describe it. Let me attempt to construct an example: Lets take some theorem about a relatively involved concept like a perfectoid space with a technically fully correct proof. Let us assume there is some subtle issue in the definition of the perfectoid space which went undetected yet. Such a subtle issue might lead to a wrong theorem with a proof that relies on the definition, while the proof itself might still be technically fully correct.

I think even Maths is not immune against such problems. And such a problem could arise even if the proof is technically correct. In fields like arithmetic theory with very complex structures such cases seem not too unlikely.

69. **mahmoud**
April 12, 2020

Peter,
that it’s unheard of that a paper on mathematics gets published despite the referees not being completely sure of its correctness seems at odds with history; I guess Hales’ proof of the Kepler Conjecture would be the most famous counterexample. So what is unprecedented in this affair does not so much seem to be that Mochizuki has put forth a series of lengthy papers that the mathematical community is finding it hard to fully understand, but rather the complete breakdown in communication as is evident after the Scholze-Stix manuscript became public.

Any decision to (not) publish the articles will hardly change this highly unfortunate divergence of opinions that is going on now (and that I think is unprecedented).

70. **Peter Woit**
April 12, 2020

BR,
The reference to concerns about interest of the theorems and the quality of the writing was just about mathematics, that these are important in evaluating a paper, but secondary to the issue of correctness of the proofs. In different fields of science I would think standards vary, depending on what characteristics of research are considered most important.

Yes, the example you give is relevant. Proofs can be wrong if they rely on problematic definitions or other results. When referees evaluate a math paper, normally they are starting from the same assumptions as the author about the correctness and unambiguity of the earlier literature. But this is exactly why it is considered so important that a new paper not contain an incorrect argument, since this can wreck the consistency and correctness of later published literature. What is going to happen if Mochizuki’s paper is published is essentially a fork in the mathematical literature. People will write papers that depend on Mochizuki’s results and may get these published in a new fork, but the majority of the community will have to reject such papers in order to preserve the consistency of the larger fork. This will not be a good situation.

71. **Peter Woit**
April 12, 2020

Mahmoud,
The problem here is very different than the Hales case. In that case, referees were nearly certain the proof was correct, but could not be completely sure due to its complexity, making a computer check desirable. In this case there’s the bizarre situation that the referees seem to have agreed that the proof was correct, while the majority of experts believe the proof is simply incorrect.

If you read what Scholze has to say, he’s not saying that the Mochizuki proof is hard to check but probably correct. He’s saying that the proof is wrong, for fundamental reasons that he has explained and which the author has no answer to. The situation is in some sense relatively conventional: experts have looked at a claimed proof of a major result and identified a serious flaw, but the author has been unwilling to admit his proof has a flaw. The only unconventional thing is that given this situation a journal’s editors have decided to go ahead and publish the paper anyway.

72. **naf**
April 12, 2020

Peter,

I am not convinced that there is any “strong feeling” against a petition, since such a situation has perhaps never occurred, at least in the last few decades. Also, the intended purpose would not be to “determine the truth”, but merely to express an opinion with the hope of convincing PRIMS .

It is clear that Mochizuki is unlikely to be ever convinced to withdraw his claims–no one outside his circle actually thinks the proof is correct and should be published in its current form–and nothing can be done about this. The unfortunate thing is that RIMS is now explicitly supporting him. I can only assume that there must be strong non-mathematical reasons–they cannot be unaware of the Scholze–Stix objections–for them doing so.

As you had said before, all doubts concerning the acceptance of the papers could be cleared if PRIMS released the referee reports, but this could only happen if they are acting in good faith and not under some strong external pressure.

73. **Peter Woit**
April 12, 2020

naf,
I agree that something should be done about this, but there are significant problems with the petition idea, and I see no reason to believe that the PRIMS editorial board would agree to change its mind about publishing the papers in response to such a petition. There are a limited number of people who have responsibility for RIMS and this journal, and I suspect they’re hearing privately strong arguments that they should do something. I hope they live up to their responsibilities.

74. **mahmoud**
April 13, 2020

Peter,
I agree that the case of Hales is quite different from the IUT papers, but cf. the editorial note here for an approach one could take when publishing them. Furthermore, your contention that Scholze & Stix found a mistake in the proof and that Mochizuki should fix it or retract his claim isn’t unproblematic; I take the comment at the very end of his reply to S&S as saying that he completely agrees with their argument, but that this “absurd and meaningless” theory isn’t IUT but their own misguided simplification of it. So no essential progress at all has been made since he first made his papers public and the problem still remains that no one understands what he’s talking about. (I’m tempted to say that IUT appears to be not even wrong.)

To me the issue of whether PRIMS will publish the articles or not is minor since it seems most unlikely that any of the believers in IUT will change opinion if the editors decide to refuse. Having the work published would at least have the positive outcome of a final version and an end to the moving targets that Mochizuki has on his homepage (all the four IUT papers are updated this April and no changelog is provided).

75. **Peter Woit**
April 13, 2020

mahmoud,

Yes, Mochizuki has claimed Scholze-Stix misunderstand him and are wrong, but virtually no experts in the subject believe this to be the case, based on hearing both sides of the argument.

The point of stopping PRIMS from publishing an incorrect proof is not to convince the true believers that it's incorrect but to try and protect the integrity of the mathematics journal literature. If PRIMS does this, why shouldn't other journals? Is it really all right if the standard for publication of a proof of a major conjecture becomes not whether it is right but whether the author and his allies have the political muscle to get it done? We increasingly live in Orwell's world where truth no longer matters, but all need to resist being pushed farther down that path.

76. **anonymous**
April 13, 2020

I am not sure that a petition is a right thing to do. However, a semester at IAS (or similar institutions) devoted to checking IUT would settle the matter. Experts who care about the general state of maths could step forward.

77. **W**
April 13, 2020

@Taylor Dupuy

I'm just saying that you can reverse the argument to an extent. Given two curves X_1, X_2, with an isomorphism of their fundamental groups, if you check that the geometrical pi_1 of X_1 is sent to the geometric pi_1 of X_2 by this isomorphism, then it follows that this isomorphism arises by "base change" from an isomorphism of Galois groups of local fields. If this isomorphism of Galois groups of local fields arises from an isomorphism of fields, then the fact that X_1 is isomorphic to X_2 comes from Mochizuki's first p-adic Grothendieck conjecture theorem. If this isomorphism of Galois groups of local fields does not arise from an isomorphism of local fields, then we can get a conjecture if we know that base changes of strict Belyi type curves are never a curve. So Mochizuki's theorem that strict Belyi type curves are determined by their pi_1s, is basically equivalent, modulo these prior results (I think…), to a special case of your question

@abc

> I feel confused reading through the blog. If someone with established expert status in a nearby field "give his/her nod" or "shake his/her head" regarding a paper, is this enough to replace the traditional journal referee process?

No one is suggesting that we decide this based on our prior knowledge of Scholze, Stix's, and Mochizuki's level of expertise. Instead you're supposed to read Scholze-Stix's critique and Mochizuki's response and see – based on as much of the mathematics as you can understand, as well as the tone and such – who is more plausibly right.

One could also look at this comment thread. Specifically, note that everything Peter Scholze is saying is an elaboration of what's in the original note. He's not jumping to a new objection in every comment or anything strange like that. He's just calmly explaining the key points of the argument. (Though much more clearly than I could explain them!)

@mahmoud

Even before the Scholze-Stix manuscript (but not too long), Scholze and Conrad (and maybe others) had publicly highlighted Corollary 3.12 as the key step in the proof that did not seem to make sense. There is no comparison to a long computational argument where case after case is checked but one can't be completely sure there wasn't a small but crucial mistake in one of the cases – here there are hundreds of pages of trivialities, followed by a single step which multiple mathematicians carefully read and independently observed was not properly justified. Of course many papers contain a step whose justification as given in the paper is not complete, but when these are pointed out, the purpose of the refereeing process is to fix such things.

@mahmoud again

What Peter Woit is attempting to do is not just report on some kind of they said – he said. He's trying to look at what the two sides said and figure out if it is plausible or not. It's not very plausible that someone would be able to look at their own theory and a radical simplification of it and not be able to explain why they are different, it's also not very plausible that the explanations in Mochizuki's note are valid.

It just simply is the case that any diagram in a category where all objects are naturally isomorphic is equivalent to a diagram where all vertices are labeled by the same object and edges are labeled by automorphisms or endomorphisms of that object. It is the case that, if the arrows are isomorphisms, the complexity and mathematical value of the diagram is controlled by the automorphism group of the object, and Mochizuki, in earlier work, bounded the automorphism groups of the relevant pi_1(X) (and even if he hadn't, he isn't able to point to any new lower bounds on the automorphism group or new isomorphisms more generally constructed in this work). It is the case that any argument for proving an inequality between two real numbers by combining a series of inequalities in various objects in the category of one-dimensional affine spaces over the real numbers can be converted into an argument for proving an inequality between two real numbers by combining a series of inequalities between two real numbers.

It is also the case that adding an ad hominem attack about a point of total irrelevance to your argument does not make it more convincing, but rather quite the reverse, and …

It is true that no progress has been made if you ignore these points and various similar ones. But if you accept any of these arguments then enormous progress has been made.

78. **Sam Hopkins**
April 13, 2020

I think the sociological critiques of Mochizuki's work are almost as damning as the specific mathematical critiques by Scholze-Stix et al. Namely, as Peter Woit has pointed out, there is no community of mathematicians who claim that they can understand and communicate Mochizuki's work to the broader mathematical world; and furthermore the elaborate machinery developed by Mochizuki has so far apparently only been used to resolve exactly one famous conjecture in number theory, with no other applications. Consider by comparison the development of scheme theory. At the time they were developed (and still!) learning about schemes was considered very difficult; nevertheless, quickly several communities of experts across the world (in Paris, Boston, Moscow, etc.) emerged, and also almost immediately the theory was applied to many, many nontrivial problems. (I guess perfectoid spaces would be another similar example, although that might get too close to the personalities involved in the current dispute.)

79. **Peter Woit**
April 13, 2020

anonymous,
Many people seem to remain under the misconception that the problem here is that Mochizuki's proof is insufficiently well understood or checked. This was the problem a few years ago, but is not the problem now. The problem now is that a specific gap in the proof has been identified, but the author refuses to acknowledge this, while neither he nor anyone else can explain how to overcome the gap. Under these circumstances, no one competent is going to go spend a semester at the IAS discussing this (and, for his own reasons, Mochizuki himself is unlikely to do so either).

80. **Timothy Chow**
April 13, 2020

Peter, I largely agree with what you are saying, but I slightly disagree with this claim: "Assuring that the proofs of theorems given in a paper are correct is the main concern of the reviewing process at a math journal (secondary concerns are how interesting the theorems are and expository quality)." In our idealistic moments we might believe that this is the case, but I think that in most cases, the main concern of the refereeing process is to assess how interesting the theorems are. It is quite common for referees to *not* check every detail of the proofs for correctness, but to trust the author.

Having said that, I do agree that there are exceptions. If a paper claims to solve a well-known, difficult problem (as is the case here), then the referees are normally expected to scrutinize the correctness of the proof very carefully. Also, if a credible critic raises serious questions about the correctness of a proof (regardless of whether it's a famous open problem), then everyone would expect a respectable journal not to publish the paper until those questions are satisfactorily addressed. I agree that the current situation is unprecedented. Even in the case of Hsiang's proof of the Kepler conjecture, there was not this level of public criticism of the proof by other experts until *after* the paper had been published.

81. **Taylor Dupuy**
April 13, 2020

Will's comment has insprired me to write a little bit more since he seems to be paying close attention (thank you Will). I kind of felt like Peter W.'s comments were a call to close the dialogue.

It seems to me that Peter S. has two claims:

1) One involving the diagram in his manuscript with Jakob in section 2.2. That the setup here gives a contradiction is undisputed. The fact that the setup is valid is disputed (see C8 of Mochizuki's comments). It also seems very dubious to me that the contradiction would be so tautological and I can't find these claims in the manuscript.

2) That one can identify all fundamental groups at bad non-archimedean places using the identity and this will not change anything (maybe he wants to assert this for every structure in a "base hodge theater"?). This includes across the theta link and across log links. This is in say Footnote 8 of their manuscript.

Peter, how do you propose to show that (2) implies (1)? I also don't understand the level of your assertion. Do you want to conjecture (2)? Do you want to claim (2) as a theorem? Do you want to claim/conjecture that (2) is true and implies some contradiction?

There is something in footnote 12 of your manuscript that I spent a little time with and if it is supposed to claim this is the explanation of (2) implies (1) then it seems to be squeezing around 15 assertions into one footnote and omits the explicit claim that (2) implies (1). In particular I don't see how to recover the normalization that needs to be simultaneously in force in (1) to derive their contradiction.

Anyway, I still see Peter's arguments as still being in the realm of a meta argument.

Maybe it is obvious to him an everyone else how to apply the definitions in IUT3 and convert these into proofs but this is not obvious to me. Again, for me 3.11 is made up of about 15 different assertions and uses a large number of definitions that I don't have at my fingertips…

82. **mahmoud**
April 13, 2020

@w
I mostly agree with what you write, I only find it a sad sort of "progress" that it has become increasingly clear that Mochizuki can't explain why he thinks the alleged gap isn't really there. If one assumes that he is behaving honestly, he believes himself that he has a correct proof but is clearly failing completely to communicate his ideas to other mathematicians; and that issue has been apparent ever since he made his papers public.
My issue is mostly with the focus on publication in PRIMS; the "integrity of the mathematics journal literature" (to the extent there still is one) will survive I'm sure, even though the integrity of this particular journal might suffer. On the political side of this affair it's more worrying that some people apparently are trying to obtain money from the UK government for research into a theory that increasingly seems like a mathematical case of being not even wrong.

83. **Anon**
April 13, 2020

This thread is somewhere between depressing and necessary – thank you PW for hosting it. But I would be far more interested in a comment thread about Peter's Fontaine rings over Z. Wow. What is he about to do with them?

84. **Peter Scholze**
April 13, 2020

Let me take Taylor's comment as an opportunity to more clearly state several related but distinct criticisms that are explicit or implicit in my manuscript with Stix, and the previous discussion on this thread.

(1) The non-necessity of passing from X to $\pi_1(X)$
(2) The non-necessity to replace $\pi_1(X)$ with infinitely many distinct copies of it
(3) The inconsistency of the identification of various copies of ordered 1-dimensional real vector spaces

Off these, only (3) points to (what seems to us) an actual mistake. (1) is about the question whether anabelian techniques — which are supposed to be at the heart of the matter — are of relevance. (2) is about whether the huge diagrams that Mochizuki considers are actually relevant to the argument. Neither (1) nor (2) alone would really falsify anything; but if non-necessary, not much is left of these manuscripts.

As only (3) is about an actual mistake, we focused on this in our manuscript. (1) is Remark 9, and (2) is Footnote 8 in my manuscript with Stix. In my first comment here, I concentrated on (1), while point (2) was addresses in my second comment in response to the fourth point raised by Taylor. [I should apologize that while my current (2) corresponds to the (2) of Taylor's last comment, it is my (3) that corresponds to his (1).]

The reason I brought up (1) and (2) here is that if one only stresses (3), as we did in our manuscript with Stix, then it may seem plausible that we simply misunderstood something at this point of the argument, but some quite powerful machinery had been built and one could plausibly finish the argument differently. However, (1) and (2) mean that quite the opposite, all machinery that is in place seems to have no power.

To me, (1), (2) and (3) seem logically independent. For (3), we explained everything in Section 2.2 of the manuscript with Stix: What the various ordered 1-dimensional real vector spaces are, what identifications one wishes to do, and that these identifications lead to nontrivial monodromy, i.e. are inconsistent. The loop one has to take in order to get the monodromy is rather large (6 identifications). I have seen no convincing argument that this nontrivial monodromy does not lead to problems. Of course, one could just decide to cut the loop at any point, but then one has to make sure that the argument never uses that disallowed identification. In particular, one has to decide in advance where to cut — if you know where, please let me know. (That in various parts of the argument only small, locally consistent, parts of the diagram are relevant, does of course not help as the argument altogether must be consistent. So the only way to achieve a consistent argument is to decide not to use one of these identifications.) I agree that this contradiction is very tautological. It seems the more surprising to me that it can be altogether neglected. (Yes, Mochizuki can't possibly mean this. But regardless of what he means, this inconsistency is simply there!)

About (1), Mochizuki's anabelian theorem states that relevant X's are equivalent to relevant $\pi_1(X)$'s, which is as much as can be asked for regarding a proof.

About (2), it's hard for me to prove this (and it's not required for the main point, (3)), as Mochizuki of course asserts that this is false, and there are thousands of diagrams one might have the occasion to look at, and I can't look into Mochizuki's mind to find all of them. What I can say, and this is basically Footnote 12 in my manuscript with Stix, is that we checked that Theorem 3.11 holds up with (2) in place, and in fact is completely tautological, so if anything Footnote 12 is meant to "prove" (2) to the extent this seems possible. More generally, (2) seems extremely plausible to me, and with all diagrams I've looked at in Mochizuki's papers it was holding up; and if you doubt it, you can just answer to my challenge of pointing to a single diagram whose commutativity is rescued by allowing some indeterminate isomorphism.

I do not claim that (2) implies (3) or anyway leads to a contradiction, although my recollection is that Mochizuki said that (2) alone would contradict his papers — he agreed that it is impossible to think that his argument might work if all of these copies are simply the same. (Maybe the idea is that one of the identifications we use to get nontrivial monodromy in (3) is omitted by having distinct copies of $\pi_1(X)$'s. But that's not actually the case.)

As I said, it's very easy to convince me that (2) is wrong: Just point to one diagram whose commutativity is rescued by allowing this indeterminate isomorphism of $\pi_1(X)$'s.

85. **Peter Woit**
April 13, 2020

I very much do not want to close the remarkable and very valuable dialogue going on here about the mathematical issues with the Mochizuki proof. Taylor Dupuy however was correctly picking up on my wanting to help make sure the mathematical discussion stays focused on these issues. A broader discussion of the mathematics would quickly get beyond my already marginal abilities to moderate something like this.

Note that this particular comment thread is being moderated with an even heavier hand than usual: most submitted comments are getting deleted in an effort to keep the discussion focused and informative.

86. **David Roberts**
April 13, 2020

I'm sorry to keep banging on about it, but Peter S's (2) is an artefact of a weird approach to diagrams in categories that Mochizuki is using. Reading M's 2018 Report closely, he claims things like that you can't define manifolds if you build them out of colimits of diagrams where the objects are all the same 'copy' of R^n (this is LbEx5). Or that you incorrectly calculate the perfection of a ring if in the usual sequential colimit you don't create separate copies of the ring in advance (this is LbEx3). This is patently absurd, but makes sense if one assumes that diagrams *must* be injective functors, or rather, literal subcategories. Recall that M assumes that he is identifying isomorphic functors, so that the concept of a diagram qua functor is severely underdetermined. Working up to isomorphism like this, and replacing a diagram with one that produces an isomorphic (co)limit, one can safely assume that diagrams are subcategories—but it is super weird, and it took me ages to realise that was what he was thinking. This is why he talks about things like "forgetting histories", because he is thinking that you need to somehow create fresh, distinct copies of objects in order to not collapse the subcategory down, and thereby give a different diagram. So when someone versed in standard category-theoretic language says "let's identify these objects", he seems to hear it as "let's collapse this subcategory to something trivial". And when he says "I need distinct copies", it seems totally weird and unmotivated. So when I look at LbEx3 in the 2018 Report it looks like the sort of mistake a student would make, when learning category theory for the first time. The problem is his conceptions of basic notions seem to be so idiosyncratic that without a serious translation filter, what he is saying seems to be completely off the wall.

If one takes category theory seriously, and DL poked a bit of fun at me for this (we talked privately afterwards), then one can *at the level of foundations* forget the equality predicate on the objects of categories, so that the equality or otherwise of random objects of a category is not even something you can consider. Alternatively, one can pass to a skeleton of the category at hand, in which case you just don't have distinct copies at hand, but *nothing breaks*. So there can be zero mathematical effect of the whole issue of distinct copies or otherwise, it is merely a psychological crutch. Once one realises this, the actual mathematics can then be discussed, for instance something like creating a formal colimit of a diagram all of whose objects are pi_1(X) (or whatever), with appropriate gluing maps. But this is not what I wanted to address, and is out of my sphere of expertise.

(on a separate note: thanks to Peter W for his patience and willingness to host this public discussion)

87. **UF**
April 13, 2020

@Peter Scholze: Thank you very much for addressing these comments of Mochizuki relevant to your (2). Let us consider the simplest situation where we just consider two neighboring columns of log-links and disregard for the moment the theta-link (I think we agree compatibility with theta link at most creates more trouble).

Then even in this simple situation (omitting theta-, but keeping log-links) Mochizuki seems to claim (C7) in Cmt2018-05 that if one rigidifies the $\pi_1(X)$'s in the columns (by identifying them using identity maps, making the logarithms Galois-equivariant), there is no "switching-symmetry" permuting the two columns.

As you say, this seems quite bizarre, since one has two columns of isomorphic data, so why should one not be allowed to switch?

I think a way out may be the following:
It seems quite likely Mochizucki uses "switching-symmetry" in a technical sense, synonymous with "multiradiality" of some algorithm reconstructing the data (here a rigidified column of log-links) at hand from some choric data, as he often does, compare e.g. [Alien, p.51].

His statement would then mean that if we rigidify the vertical columns, then there is (unlike in the non-rigidfied case) no multiradial algorithm to recover this column from certain choric data. This does not sound so absurd anymore (to me).

Now which multiradial algorithm does he mean here?
I would suggest it may be the multiradial algorithm in [IUT III, Cor 2.3], more specifically, the first part of 2.3 (ii) which concerns its compatibility with log-links.
Note that close by, [IUT III, Rem 2.1.1 (ii)] the issue we are talking about "why π_1 only upto indeterminate iso?" is discussed. For further discussion see also in [IUT II, Rem 3.6.4 (i)].

In any case, I agree this is an important issue to track down.

88. **W**
April 14, 2020

@David Roberts

Please keep banging on about it! I think your interpretation of Mochizuki's argument in the response was an important insight and underappreciated.

Let me ask you what I think is an important follow-up question. Suppose you take the same argument and present it in two different languages – one, the standard categorical language, and two, Mochizuki's language where distinct copies of an isomorphic object are relevant for colimits and other categorical constructions. Assuming no other knowledge of what the argument actually is or how it is written, which language is more likely to conceal a subtle error in calculations or other mistake, and which language is more likely to make such mistakes easier to see?

I think this question is within your sphere of expertise, and the sphere of expertise of many other mathematicians – far more than have read carefully a portion of the document.

I also think its relevance to the broader topic of discussion is clear.

89. **Peter Scholze**
April 14, 2020

David Roberts: Thanks for your thoughts on this!

Actually, something related happened in our Kyoto discussions: We realized that we could not get on common grounds regarding the issue of whether one needs separate copies of a ring to form its perfection, so we decided that we simply have different psychological crutches (as you call it) on this, and that we better focus on some actual mathematical statement where related issues undisputably become important. However, no such focal point ever appeared, despite us going through the essence of the IUT manuscripts. So it seems to us that to the common mathematician, his whole big log-Theta-lattice essentially comes down to one Hodge theater — which is really just the elliptic curve you started with (the category of Hodge theaters is equivalent to the category of elliptic curves isomorphic to your given one) — together with the p-adic logarithm map ("the log-link") and some isomorphism ("the Theta-link") of two copies of a local Galois group acting (trivially) on a monoid isomorphic to $\mathbb{N}$ (I'm obviously simplifying, but not too much — I'm basically considering only one bad place, while you have to consider all places, but I'm already telling you about the most interesting place). The generator of this monoid $\mathbb{N}$ is one time regarded as the value of the Tate parameter q, one times as the value of the Theta-function (a collection of such values, really, but never mind). But of course this isomorphism of abstract monoids is totally incompatible with these "interpretations". Initially, one might think that Mochizuki claims that there is some isomorphism of the pair of (local Galois group acting on local units) that takes the q-value to the Theta-value — this would obviously have great consequences, and would probably require the use of exotic isomorphisms of local Galois groups — but this is just not the case, one can easily give counterexamples; and it can't even be true locally "up to blurring", only a global statement, averaging over all places, can be true. The isomorphism is just on the level of (local Galois group acting trivially on monoid isomorphic to $\mathbb{N}$) and the relation of this monoid isomorphic to $\mathbb{N}$ to the local units is completely external.

So for all we can see we simply followed the procedure you suggested, and reinterpreted his distinct copies in the way usual mathematicians think. We recorded the outcome of this in our manuscript: You can read there the details of what everything is, in particular that his log-Theta-lattice really boils down to essentially the simple data above. How could one possibly go from here to any nontrivial result? Of course, as also W suggests, all of this is much harder to see through in his language.

@UF: The remarks from IUT that you cite make heavy reference to his paper on etale theta-functions, which seems to play a key role in the IUT papers. This paper gives some neat algorithm to start from the fundamental group of a once-punctured elliptic curve with bad semistable reduction, and recover its Tate parameter q and some Theta function; I forget the details. While this is all good and well, I don't see the relevance: Mochizuki's more general anabelian theorems, discussed previously on this thread, tell you that from the fundamental group you can simply recover the whole curve. In these comments of Mochizuki that you reference, Mochizuki is discussing some nitty-gritty details of this algorithm, but this seems completely besides the point if you just remember that relevant $\pi_1(X)$'s are equivalent to relevant X's, so of course you can recover all invariants of X, and you can do so functorially in $\pi_1(X)$.

Generally, a point seems to be made that Mochizuki's algorithms have some magic power and that really the content of the algorithms is critical, so in the context of the previous paragraph, it would matter in some way how I invert the functor $X\mapsto \pi_1(X)$ using some explicit construction (or that I don't actually invert it but only read off Theta-values using some other roundabout algorithm). This seems very surprising to me.

Regarding Mochizuki's algorithms, let me add that I was surprised that the following procedure counts as an algorithm for Mochizuki.

In IUT-3, Theorem 3.11 (i), an algorithm is discussed, that does the following. The input is data concerning only p-adic fields; it is basically a profinite group isomorphic to the absolute Galois group of your given p-adic field. The output is something like the Theta-value of the elliptic curve you chose at the beginning of the IUT papers (cf. part (b) of that data).

How is this possible? The input data doesn't even know anything about the elliptic curve! This is completely magic!

The resolution is that the elliptic curve has indeed been fixed once and for all in these papers, and so of course you can produce that Theta-value — simply look at the elliptic curve you have fixed, and take its Theta-value.

Of course, there is some packaging done around this, but this is the essence of this "algorithm"; Mochizuki confirmed this.

90. **Taylor Dupuy**
April 14, 2020

I think we are mostly on the same page now. No worries about the indexing. I'm going to use your indexing in what follows.

–Regarding (3): The "cut" is supposed to occur at identification of the theta side of the theta link; in that global realified frobenioid we don't normalize that Picard group according what you would want the degree of the theta pilot object to be. For the interested reader, details are in my manuscript with Anton after we introduce theta pilot divisors. I think Mochizuki includes this in one of his responses too.

–Regarding (1) and (2) not implying a contradiction (directly): we agree. I would consider (1) and (2) open as well. We agree on that too. Also, as I'm sure everyone will agree, statements need to be pinned down a bit more and directly tied to what Mochizuki has written. So I think even *exact* statements of (1) and (2) are also open. I will say a little more about (1) below.

For reasers who want to look at this, the obvious thing to do is to take isolate single claim in IUT and just start running with it, then build out from there.

–Regarding not using automorphisms of fundamental groups: we should observe that without indeterminacies, there are no indeterminacies. This is tautological, yes, but indeterminacies appear in the statement of Cor 3.12 and without them we are really talking about something else entirely.

Also, this all goes back to how the indeterminacies allegedly afford us the ability to compare the "volume" of the hull of the multiradial representation of the theta pilot region with the degree of the q-pilot divisor (definitions can be found in my paper with Anton)—this is the infamous "Mochizuki switcharoo".

I could talk more about this more but right now but I will be stating a bunch of (useful) isolated facts that readers would need to assemble for themselves (if it is even possible to do so). I think I might be burned at the stake for this as well as run the risk of making mistakes in public!

–Regarding (2): I think we need more language about "infinitely many copies" but basically I'm of the same mindset as David on this. I think everyone agrees we shouldn't think about "different copies" so much in the same way that you don't need two copies of $\mathbb{R}$ to talk about $\mathbb{R}^2$. I'm not sure this is what you or Mochizuki means though Peter. On the other hand there certainly are cases where you need multiple monoids.

–Regarding (1): we basically agree here. I think groups may be much more convenient and I think the representations are definitely important. The statements Mochizuki makes involve etale theta and the reconstruction of evaluation points and the representation of $\operatorname{Aut}(\pi_1^{\operatorname{temp}}(X" _{\underline{v}}))$ on these interpretations (last time I tried a double underline it didn't work out so I'm using double prime this time, here we need to take an analytification or formal scheme with log structure). Technically speaking, I suppose $\operatorname{Aut}((X")_{\underline{v}})$ acts on the same objects but maybe it isn't as easy for me to see these actions.

–Getting your hands dirty in the definitions like UF has begun doing is the way to proceed in these investigation. I can give some basic definitions of "switching" but I believe they are not adequately developed for discussing IUT3.3.11. Maybe we should take that offline? Whatever you guys want. Mochizuki discusses the formalization of multiradiality in a remark following IUT3.3.11 but that remark mostly says "you can do it" without any details as I recall.

–As a side remark, and I know Mochizuki would hate me saying this, it does feel like mind reading at times. I strongly agree with this sentiment. He tries though. I believe attributions of malintent are misplaced. But still, lots of mind reading. 😛

–I am intentionally omitting a discussion on "power". If people want me to step into this world of speculation I can do it. I'm getting more of a "show me the money" vibe though.

91. **Taylor Dupuy**
April 14, 2020

Peter: I think we posted at the same time… I'm going to read your comment now.

92. **Taylor Dupuy**
April 14, 2020

Quick comments:

–Peter, I think the $\mathbb{N}$'s should be regarded as embedded inside a monoid with enough roots of unity and nth roots to do Kummer theory.

–You definitely need more than one part of the "Frobenius-like" objects of the Hodge Theater — the abstract monoids. I also think you should think about overloading free variables; this thing is some sort of F'd up quotient of a free construction. This of course isn't precise.

–The term "algorithm" is trash. I think it is a big part of the problem. Also, we should note that has been going on in anabelian geometry for a very long time and it isn't a "Mochizuki thing". For the most part I have found that
algorithms = interpretations in infinitary first order model theory
And I very very emphatically agree with you that it does *not* suffice to treat "functorial algorithms" as functors alone, you need more. This is certainly omitted in Mochizuki's exposition.

Also, this interpretations stuff also goes off the rails a little bit…
1) you need to be able to consider structures up to automorphism as well.
2) There is also a backwards version of this where he considers "lifts" of structures that interpret lower structures.
I talk about this a little in my first manuscript with Anton.

–Regarding 3.11.ii this is an independence results. He is saying it doesn't matter which lift of all these things that interpret the absolute galois groups "at the bottom" you take. This thing you construct independent of the lift (by the way this points to the automorphisms of fundamental groups you were looking for).

Here is a funny observation: there exists a "shitty multiradial representation" where you can take the union of all images of the theta pilot region in measure spaces cooked up from absolute galois groups of p-adic fields at $\underline{v} \in \underline{V}$. This is ALSO independent of the lift. Proving that it is "multiradial" is tautological.

Yet, this one doesn't use log-links and it isn't claimed that this "shitty multiradial representation" can be related back to the minimal discriminant.

93. **Peter Scholze**
April 14, 2020

Dear Taylor,

first, thanks for explaining where to do the cut. In your comment, I don't actually understand where you want to cut, so I'll have to look at your note with Hilado. (In my manuscript with Stix, which direction does the isomorphism go? Horizontal/slanted? In the lower or bottom half? In the left or right half?)

About (2) not leading to a contradiction, you actually made me realize that probably it does: I don't see why part of Mochizuki's indeterminacies, I believe for example (Ind2), is necessary, but omitting it leads to a form of ABC that is provably too strong. I agree it would be hard to "prove" a strong enough form of (2), but I think the burden of proof is on Mochizuki here, to show where the argument needs indeterminacies like (Ind2) — which is basically the issue (2) we're discussing.

Reading your first comment above, I actually don't really see where we disagree.

Regarding your second comment, the relevant $\mathbb{N}$'s are manifestly not part of monoids in which you can do Kummer theory! Yes, Mochizuki includes some extra factor, but that's just along for the ride.

About the last bit, to be sure we're on the same page: I was talking about part (i), not (ii), of Theorem 3.11.

94. **Peter Scholze**
April 14, 2020

OK, I quickly looked at your manuscript. If I understand it right, that's the upper left slanted arrow? In our terms, that's the difference between the "abstract" Theta-pilot — a generator of an abstract monoid $\mathbb{N}$ — and the "concrete" Theta-pilot — the actual Theta-values of your elliptic curve. (Mochizuki seemed to conflate the two originally, or so it seemed to us.) If you cut there, then the Theta-link no longer links anything to actual Theta-values! This of course removes any inconsistencies, but it also removes what's supposed to be the key, namely the identification of q-values with Theta-values, in some form.

On the other hand, I read say on page 140 of IUT-3 that Mochizuki considers the Theta-intertwining, which I believe simply means this identification of abstract Theta-pilots with concrete Theta-pilots. He wants to be very careful with using this etc., but I do believe he wants to (and has to) use it somewhere. So I don't think you can simply cut there. I think the closed loop that Mochizuki discusses on page 143 of IUT-3 is also relevant here. See also on page 144 the simultaneous Theta-intertwining and q-intertwining he wants to have (up to indeterminacies etc…).

95. **Taylor Dupuy**
April 14, 2020

Yep, the theta pilot doesn't map to the actual theta values *on the theta side*. On the q-side it does.

I will check your manuscript again in a bit. Dinner then bedtime (I am barbecuing).

I want to check again to make sure I didn't miss something.

96. **Taylor Dupuy**
April 14, 2020

I realize I have my phone… so yeah, you don't normalize the degree on the left hand side of your diagram. It is like we are all saying you can't something be equal to two different things at the same time…

Also there is only *one* copy of the real numbers.

97. **David Roberts**
April 15, 2020

@W

gosh, thanks!

> Suppose you take the same argument and present it in two different languages – one, the standard categorical language, and two, Mochizuki's language where distinct copies of an isomorphic object are relevant for colimits and other categorical constructions. Assuming no other knowledge of what the argument actually is or how it is written, which language is more likely to conceal a subtle error in calculations or other mistake, and which language is more likely to make such mistakes easier to see?

This is tricky: it depends who's reading it. Who are you envisaging seeing mistakes? I can't imagine (ignoring the fact this is IUT and tremendously baroque) that someone who's had a decade of practice with their own idiosyncratic style of working would make mistakes more frequently that someone using the language of the majority, all things being equal, apart from the fact the latter person has more potential external checks and balances. This latter point I think can't be overemphasised. Andrew Wiles was still speaking the language of his community by the time he emerged with his (first attempt at a) proof of FLT, and even engaged the help of someone else to try to check the subtle parts of the argument before that. This hasn't happened here…

A bigger problem is the rigid commitment to definitions/structures that are explicitly admitted as being far more general than necessary (*cough* Frobenioids *cough*). This increases the friction for potential eyes on the IUT papers, if you'll permit me a worrying metaphor mix.

98. **Taylor Dupuy**
April 15, 2020

Peter: Below is a more detailed response.

To recap, you made three posts. One in response to David and UF, and then two smaller ones. I'm going to address the newer ones first and then go back and address those older comments.

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Responses to Newer Comments
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Regarding the "cuts": I think I addressed this. Let me know if you want to talk about it more. I think if you are not taking two log-linked strips of Hodge Theaters and only have one theta monoid it is going to get rough. He wants all the computations in on particular Hodge Theater in the log-linked strip on the q-side to be the "usual normalization" (sorry for referencing such large objects, I know this is super abusive… I'm sure there is a better way to talk about this than to make blanket references to Hodge Theaters — really the relevant "official constructions" here use the $\prec$ prime strips (I think) in the Hodge Theater… that is not a fun reference chase.)

Regarding (2) [infinitely many fundamental groups]: So I think we need to clarify this what you mean by (2). I'm not sure which indeterminaciees you want to get rid of. At some points I see that you are thinking about using "one fundamental group" and at some points you are saying "use one Hodge theater". I think these have different consequences — one is about representations on monoids and one is about the groups solo. Are you wanting to get rid of ind3 and this log-kummer correspondence? What do you want to kill exactly.

I agree with you that tracing the proof for simplifications which removes or modifies inequalities to the point where they are false is a good strat for finding flaws.

Aside: I will always agree with you that the burden of proof is on the writer to explain things. In modern arithmetic geometry there is too much flexing on the reader. IUT is a bit of a weird flex in some ways.

Regarding $\mathbb{N}$'s and Kummer Theory in the $\Vdash \blacktriangleleft \times \mu$ prime strips:
As a recap: You stated that they are isolated, I said the have a Kummer theory. I want to clarify: technically you are correct the monoids in a components at a bad place of $\Vdash \blacktriangle\times \mu$ prime strip are just $\mathbb{N}$'s. BUT… they are interpreted structures. So they come from other structures where it does make sense. Note also that in isolation, there is no action of the $\mathbb{N}'s$ on the corresponding copies of $\mathcal{O}_{\underline{v}}^{\times \mu} \otimes \QQ$. (Just as a reading note: Mochizuki sometimes calls this his "holomorphic structure". I think he also uses the words "embedding" or "link between unit group and value group portions"). I can speculate a little about what is going on on either side of the theta link if you want.

*Subremark. This is sort of an ongoing theme I want to highlight: Given a structures A and B there may exist two ways of interpreting B in A (called them $\mathbf{B}_1$ and $\mathbf{B}_2$) which are not equivalent. This for example could for example be distinguished by the representations $\operatorname{Aut}(A) \to \operatorname{Aut}(\mathbf{B}_i(A))$ (I'm not saying this is the case here, I'm just trying to give a concrete example).

*Subremark. Sometimes I find it useful to think about there being a single $\Vdash \blacktriangleleft \times \mu$ strip that lives in different "charts" (after applying the correct automorphisms). I am not sure how helpful this is, but it is kind of fun to think about.

Regarding My Remarks on 3.11:
A quick summary: You had stated how ridiculous it is to construct anything involving Tate parameters from absolute galois groups of p-adic fields. I said some words about lifting G's to an interpretation so we view G as $\mathbf{G}(\Pi)$ for some fundamental group Π and said these constructions are uniform in Π (let me say this is a cartoon picture right now, we actually need a lot more structure on this. In particular what I had in mind was a lift of the structures "at the bottom" of 3.11.i to log-linked collections of prime strips — I made need to modify the structures in this statement to make it exactly correct). I had made some remarks about 3.11.ii being an independence result. Let me clarify, I was talking about $U_{\Theta} \subset \mathbb{L}^{\vdash,et}$ (the "(coarse) multiradial representation of the theta pilot region" inside the so-called "mono-analytic etale version of the log shells" all put together) being independent of the lift. Let me think about 3.11.i. I need to refamiliarize myself with this $\mathfrak{R}^{LGP}$ bullshit. If we are really going there right now then I think we need to introduce "abc-modules" (the primary data discussed 3.11.i and 3.11.ii). These are sort of pre U_{Θ} structures (used to construct U_{Θ}). I need to think about 3.11.i and get back to you. Also, we are in territory where I am pretty shaky. So… no promises…

Regarding "Theta Interwining":
Can you give me a reference with respect to a Theorem/Remark/Environment of some kind? I think we are reading different versions of the manuscript. (Let me just make one side remark— there needs to be a stable version of these documents somewhere in print; it has obviously become an important historical document. I'm not saying where and how, it just need to be immutable.)

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Response to Response to David Roberts and UF
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Let me just say, I don't think your simplifications were an unreasonable guess (and I want to clarify to the readers that this is actually not what Mochizuki meant in his setup. I'm just saying the whole thing is so crazy you need to start somewhere.) I think we all agree though if you take one Hodge theater it won't work as per your manuscript with Jakob.

Regarding Magic Powers:
I really like this comment. It is a very interesting thought regarding looking at the "conjugate constructions" from the perspective of curves. It does look funny. What is in this fundamental group sauce?! Here are some possible explanations:
1) There is more to what is going on than just fundamental group vs curves; Mochizuki uses the monoids and cyclotomes extensively. In particular he applies many "orthogonality" results.
2) Fundamental groups are first order structure: they are a topological space with a binary operation. The formalism of interpretations applies here (maybe by slightly increasing the signature), but you get to put everything in this nice box where represenations become automatic and you get to see how everything varies with respect to automorphisms. (Full disclosure, Mochizuki objects to the language of model theory for this stuff, he thinks it is unnecessarily complicating things. This is a matter of taste maybe.)

I think we need to unpack the comment ""The resolution is that the elliptic curve has indeed been fixed once and for all in these papers, and so of course you can produce that Theta-value — simply look at the elliptic curve you have fixed, and take its Theta-value."" What Mochizuki means is 'fixed in initial theta data' usually. I also want to repeat that I object to the usage of a single Hodge Theater. If HTbar is a log-linked collection of Hodge Theaters (which we view as a single massive infinitely sorted structure) the definable set we need is in like HTbar^2 (I said "like")

Regarding the comment: "it can't even be true locally "up to blurring", only a global statement, averaging over all places, can be true.…"
This is correct. His claim is a purely global statement. But… it is even WORSE than this! You need to take the hull and then take the volume and only then does he claim you see the comparison.

In this comment you also reminded me of some things concerning the shape of the inequality. I'm not going to do it now, but I'm going to make a note to try to talk about:
*Ind2 and moving around $q^{j^2/2l}$ — why you need Ind3
*Analytic number theory, what happens at large discriminants, and toy phenomenology for the inequality.

IOU: 3.11.i discussion.

======
Oh dear, all my triangles are off. The pointy bit should be to the right…

99. **UF**
April 15, 2020

I now think a good reason to for not identifying π_1's along log-links is as follows:
(It is very similar to [IUT-2, Rem 1.11.2 (ii)] for the theta-link; I am less precise here, taking the relevance of some reconstructions on faith, but it is a intuitive story, and precision can come later)

First, let us step back and look at the log-theta-lattice: It consists of Hodge-theaters and the log/theta-links as maps/functors between them. Now a formulation of (2) is:
Why do we, say for the log-links, not just take the "identity" isomorphism between the relevant π_1's instead of the full poly-isomorphisms between them?
Surly it is also a completely valid map between the theaters?

Yes, but: let us recall what we want to do with this log theta-lattice: we want at "suitable times(?)" glue the Hodge theaters along these links and then apply certain multiradial algorithms.

I think that if we would glue with the "identity" on the π_1's (and all the other prescriptions of the log-link), then this gluing will be inconsistent/not well defined (when combined with some multiradial algorithms), in some vaguely similar sense as if you try to define a complex structure on a manifold by charts, but in some region the procedure leads to two different, incompatible complex structures, i.e. the transition map is not holomorphic.

Indeed, in one Hodge theater, the π_1 is not independent of the rest of the data; it acts via the quotient to the Galois group on the monoids. Thus π_1 is "related" to the monoids, and it is not apriori clear we are allowed to glue two Hodge theaters by gluing π_1 and the monoids separately in whatever way we wish (and then follow certain multiradial algorithms).
In the above analogy, a certain region of the Hodge theater is supposed to be "determined" jointly by the π_1 and the monoids.
The multiradial algorithm we consider is roughly as follows: The action of π_1 determines on the (from the Galois group reconstructed) local monoids (with 0) in addition a ring structure (a "holomorphic" structure).

The output is roughly some etale version of some of the data originally present in the Hodge theater, and the reconstruction algorithm yields a canonical isomorphism between the original data and the reconstructed one (see e.g. [Alien, §2.12] for essentially such constructions).

If one now glues two Hodge theaters along a log-link (with in addition "identity"-gluing for π_1's), then this yields no consistent holomorphic/ring structure compatible with the above algorithm on some region:
Namely, consider the region of the ring (local units), which is glued via log (the actual log part of the log-link) to other Hodge theater; this yields one definition of "holomorphic" for this region. On the other hand, we can embed this region in the reconstructed monoids and use for the "holomorphic" structure on the reconstructed monoids the one induced by π_1 on both Hodge theaters and glue by the "identity".

The above roughly describes two incompatible holomorpic/ring structures on some part of the glued Hodge theater; they are incompatible, "because the transition map, essentially the log, is not a ring map". Such a situation is presumably avoided by taking the full polyisomorphism between π_1's, where the log map on the units alone defines the holomorphic structure on the relevant region.

100. **Peter Scholze**
April 15, 2020

Dear UF,

thanks for your answer. (Taylor, I will answer in a separate comment later.)

Removing all language from your message, I end up with the following message: The logarithm map is not a map of rings. This is why you have to consider source and target up to some indeterminacy.

The only way I see to make this into a valid thing is if the indeterminacy somehow allows you to map the logarithm map into a map of rings — after all, this is what you critized in the first place. But any automorphism of π_1's will only ever replace source and target by isomorphic rings. So the logarithm will still not be a map of rings. Note that I'm simply repeating what I already said in my second comment in this thread.

101. **Peter Scholze**
April 15, 2020

Dear Taylor,

addressing your last comment.

First half: I was refering to the current version on his webpage. I realize that this is dangerous, but it's the only thing that at least currently makes it possible for everyone to follow. In any case, to me it sounds like you are saying that the Theta-pilot has nothing to do with Theta-values. Well, strictly speaking (modulo minor issues) what Mochizuki does is the following: He takes the Theta-value (some nonzero p-adic number, not a root of unity), and takes the (multiplicative) submonoid M of the p-adic numbers generated by it, and then considers M as an abstract monoid. As an abstract monoid, it is then of course isomorphic to $\mathbb N$, necessarily canonically so; its generator is referred to as a Theta-pilot. Does the abstract monoid M know anything about the Theta-value? Of course not! But that is all that ever seems to enter the story of the Theta-link. At some point you do need to remember the interpretation of the Theta-pilot in terms of Theta-values — this is what Mochizuki calls the Theta-intertwining, and it plays a key role as I tried to reference. So I don't think that Mochizuki simply cuts our diagram there.

Now David Roberts previously mentioned that Mochizuki seems to have a nonstandard point of view on what a mathematical object is, requiring separate copies when they are not actually needed; he often refers to some "history" of these objects. Now maybe one could try to argue that because M was built out of Theta-values, it (the abstract monoid) still knows something about them, that can be even transported via an isomorphism of abstract monoids. (You made some similar remarks in your paragraph on $\mathbb N$ and Kummer theory, refering to "interpreted structures".) But that's evidently not the case. (That the abstract monoid M knows something about Theta-values might be debatable if for you an abstract monoid is really encoded — as it is in ZFC — in terms of its actual set of elements. But even then, this structure ("the interpretation") is clearly not transported by an isomorphism of abstract monoids.)

About the indeterminacies: I'm focusing on (Ind2); (especially) the part concerning automorphisms of local Galois groups (acting on local units if necessary). I don't see any diagram that doesn't commute when you take the identity isomorphisms, but does commute when you conjugate by an automorphism of a local Galois group (acting on local units). Why do you need that, expect because "you need to forget the history of your objects in order to apply reconstruction algorithms" or some magic like that? (There was actually a point where Mochizuki was surprised that by "forgetting the history of a group" one still has more than a group up to isomorphism: Namely, a group. That the datum of a group is strictly more than the datum of a group up to isomorphism seemed new to Mochizuki. I believe this is the (psychological) main reason he considers these full poly-isomorphisms. But really, forgetting "the history of the groups" you still have groups and they may still have natural commuting isomorphisms between them. Of course, for groups up to isomorphism you can't ask for natural commuting isomorphisms between them, then you only have "full poly-isomorphisms".)

Regarding the rest of your post, I don't see how you're really objecting to anything I said.

Finally a short answer to David Roberts' last message: I highly doubt your sentiment that the possibility of doing mistakes is not correlated with how well your language is adapted to the mathematics at hand.

102. **UF**
April 15, 2020

@Peter Schoze: I agree that the above should be surrounded by big caveats about the nature of the effects of passing to full poly-isomorphisms and I may interprete those incorrectly.

What my post above attempts to show, is: if passing to poly-isomorphism has the effect of doing no gluing/no identification of ring structures (arising from π_1, just the gluing from the actual log map), then the only gluing left is the actual log map, which gives one global chart, and no transition functions needed, essentially(?) since just one chart . I before never really seriously considered that full poly-isomorphism could have the effect of "no gluing arising from this part" (instead of "choose your favourite gluing"), but a similar thing is (I think) asserted for the case of the theta-link in [IUT II, 1.11.2(ii)], where the message is: full polyiso on π_1 means no sharing of ring structure arising from this.

Clearly this does not show by itself it really works like this, but it seems to reduce an argument for the log-link to some degree to an anologous one for the theta-link.

103. **Peter Scholze**
April 15, 2020

Dear UF (and Taylor and everyone),

I won't comment any further here on statements of the form "well, maybe Mochizuki actually meant (vague statement)".

A few comments up I summarized the situation with claims (1), (2) and (3). I have seen no valid objection to (1) and (2), and (2) alone would lead to a contradiction (as one gets too strong a form of ABC). To (3), Taylor indicated where to cut the diagram, but I really don't think this is what happens, as this would isolate Theta-pilots from Theta-values and effectively remove the actual Theta-values from the proof; while Mochizuki does consider this "Theta-intertwining" which is the association of the Theta-pilot with the Theta-values.

I will only comment further here if either a valid objection to (1) or (2) is mentioned, or further clarification is given regarding (3). Any further technical discussions are probably best done via e-mail.

Best,
Peter

104. **David Roberts**
April 15, 2020

@Peter S

sorry, that's not what I meant: from a purely mathematical point of view, my understanding of what M means by 'copies' is, divorced from all this context, mathematically consistent, if weird. I think things like working with functors up to isomorphism is a much bigger problem, especially when, eg, apparently more-rigid-than-usual "reconstruction functors" are at play. W asked about a very specific issue removed from the bigger context, which is the kind of counterfactual that is hard to answer sensibly. I agree than any language, mathematical or otherwise, that obfuscates what's happening is going to increase the risk of error!

105. **W**
April 15, 2020

@David Roberts I'm happy to take responsibility for my understanding, which I think was caused by me formulating the question poorly.

My intent was to focus on what kinds of inferences you (or anyone) might be able to make about Mochizuki's work, based on your knowledge of the response and your general knowledge of category theory, not based on reading through hundreds of pages of his papers as Peter Scholze did. I didn't intend to single out one particular difference in notation that happened to be among the least concerning ones, but I guess I did.

Anyways the point of this is that if Mathematician A writes a paper and Mathematician B says a particular formula is off by a factor j^2 then, all else being equal, there is a good possibility that either side might be correct. If they go back and forth discussing it the probabilities change somewhat, but at every point, all else equal, both probabilities are pretty high.

But if we find that Mathematician A is using language that increases the risk of errors, and Mathematician B claims to have found the error by removing that language, and Mathematician A refuses to change this language, and Mathematician A claims that they are using this language to reduce the risk of errors, then we have found a bunch of pieces of evidence that A is the one who has made the error and not B, even if we don't read the paper enough to follow either mathematician's reasoning line-by-line.

106. **David Roberts**
April 15, 2020

@W no worries 🙂

>Mathematician A refuses to change this language

this is the worrying part. And, IMHO, one of the biggest problems with the whole affair. The papers could have been rewritten in the past eight years to remove all the extraneous fluff, and using more standard terminology, based on feedback from the community. Forget about terminology and risk of error, it could have just simplified the papers.

>Mathematician A claims that they are using this language to reduce the risk of errors

I don't know about this bit.

But all this is very meta-reasoning, and not mathematics, like a lot in this whole affair.

BTW, "language" here is perhaps not the best word to use, given that it has three potential meanings: just the general idiom, full of metaphors and not-incredibly-helpful "motivation; the mathematical language (what I prefer to call 'terminology'); and the fact there are English- and Japanese-language documents on IUT (and unjustified questions about facility with one or the other out there).

107. **W**
April 15, 2020

@David Roberts

Sure, "terminology" is a better term. I don't think anything to do with the English or Japanese languages is particularly relevant, and the general idiom is clearly relevant but only incidentally to the main issue.

By "Mathematician A claims they are using this terminology to reduce the risk of errors" I mean passages like these, from Mochizuki's response:

> Omitting the labels leads to confusion concerning which copy
of the unity element $1 \in A$ is to be regarded as the unity element
for "Af ". Such confusion may, of course, be misinterpreted as
an "internal contradiction" in the theory of localizations of
commutative rings with unity. In fact, however, there is no "internal contradiction" in the theory of such localizations; the
apparent "internal contradiction" is nothing more than a superficial consequence of the erroneous operation of omitting the
labels

> relying, in mathematical discussions, on declarations of "remembering"
that are not accompanied by precise, explicit documentation of the labeling apparatuses that are employed incurs the risk that different people
will "remember" different labeling apparatuses, which result in structurally non-equivalent mathematical structures.

I can think of a different way to interpret them, though (that Mochizuki is only justifying some of his terminological choices here, and this justification depends on other terminological choices, and is valid once you accept those choices) so maybe this gloss is not completely fair.

108. **UF**
April 15, 2020

@Peter Scholze: Thank you for your comments.
Just to restate my view, in case I was unclear: I believe that the reasoning above for not including any specific identifications of the π_1's (i.e. just full poly-isomorphisms) in the definition of the log-link is entirely parallel to Mochizukis reasoning in [IUT II, 1.11.2(ii)] for not including any specific identifications of the π_1's in the definition of the theta-link.

109. **David Roberts**
April 15, 2020

@W

yes, I agree with your points. I just mentioned the other aspects to 'language', as there are readers here from a wide background, who might misconstrue what we are saying (there have been, I gather, accusations of racism thrown about that I want to head off here).

Those two quotes you have selected are indeed particularly weird, and reflect the psychological crutch I mentioned earlier. I even quoted the second one in my notes on Mochizuki's report. If this is truly M's thinking, then we are in "If a lion could speak" territory. This is not mathematics in the slightest.

110. **Rob W**
April 16, 2020

I understand that it might not be the best use of everyone's time but I have really enjoyed reading this. Seeing a well-mannered, real time discussion play out has been great (particularly in these times of lockdown). So while it might not be the most efficient way of trying to solve the problem / address the issues it is having some positive externalities (at least for me) .

111. **Taylor Dupuy**
April 17, 2020

Hi Everyone,

I'm just going to make one last post to close out some loose ends for interested readers. I also want to point out some things that we haven't covered that I think are important. I've omitted the discussions of asymptotics of Mochizuki's formula but other than that, I think I covered my IOUs. If there are any analytic number theorists who were really looking forward to that please email me.

Sorry for the length.

****
Regarding (3): Is Mochizuki's Proof Falsified?
****
There is no *proof* that Mochizuki's method doesn't work.

The following is what the Scholze-Stix manuscript proves:

Theorem. Assuming that one may identify Hodge Theaters in Mochizuki's theory and simultaneously impose "concrete normalizations" of q-pilot and theta-pilot degrees then there is a contradiction.

Remarks.
1) The hypothesis of "simultaneously imposing concrete normalizations" is equivalent to $l(l+1)/12=1$ for a fixed prime l. In the manuscript no machinery from Mochizuki's theory is used in this derivation. A protest to this identification is made in C12 of Mochizuki's response. Despite this, Peter S. insists this is what Mochizuki means. See his comments above, or the quoted comments below.
2) The hypothesis that one can identify Hodge Theaters is also protested by Mochizuki and runs counter to his stated objective. It is prudent to observe that the thesis of the Gaussian integrals survey is that sometimes in Mathematics one can introduce "alien copies" of objects to prove something about your original object. This runs counter to the assumption of identifying Hodge Theaters in the above theorem.

I am not taking the position that Mochizuki's proof is correct or will turn out to be correct but that, as it stands, the Stix-Scholze manuscript should not be held up as a reason to reject Mochizuki's proof. On the contrary, I am just saying that a falsification of the proof requires a fluency in the definitions which seems hard or impossible to achieve. Many of the definitions use nonstandard terminology and come off as ambiguous to many readers (I'm not sure if I want to get into a definition of ambiguous). Case in point: questions about the "embeddedness" (or in Mochizuki's terms "holomorphic structures") of the monoids involved in the $\Vdash \blacktriangleright \times \mu$ prime strips; these came up in Peter's previous post. Moreover, because of certain ambiguities in these definitions, readers are forced to search a large space of possible meanings. While these difficulties do not falsify the possibility of a proof, they make it so that what Mochizuki has written is not a proof in traditional terms. To speak plainly, flexing on the reader by omitting details is not something new in Arithmetic Geometry and IUT is a weird flex.

Finally, it is not outside the realm of reality that there could be 5 top notch international referees who have understood the proof as complete and correct. PRIMS has traditionally been a journal of very high quality and members of the editorial board have shown integrity in the past (look at how senior mathematicians are responding when asked by journalists to comment). Let's see what the print version looks like (hopefully it has a table of contents and remove bolding and italics).

What can we do?: I think we should all think carefully about the damage omissions have next time we are tempted to do it. Additionally, we should praise the writers whose writing we find "easy" and useful. Because of these difficulties I personally hold Mochizuki's document as a program for proving ABC and consider Mochizuki's formula a conjecture (until I can see this with my own eyes). Then again, there are a lot of Theorems I know about and use but can't prove. Also, be nice to everyone, we are all just people.

****
Regarding (1)
****
I see nothing wrong with (1). By this I understand replacing all constructions in IUT involving the fundamental group of an SBT curve with the corresponding construction in an actual curve. I think this is possible, but we would need to check that this doesn't break somewhere. If the claim is that all fundamental groups in the theory need to be eliminated, I would be more reluctant. There are a lot of weird curves. I think I need to see what a more precise statement looks like in practice to say for sure.

****
Regarding (2)
****

Regarding (2), to be clear, we should repeat that this is not what Mochizuki does (only using one fundamental group). Also, the statement as Peter has stated is a moral one not to be taken literally — there are many more groups appearing in the construction that are not isomorphic to $\pi_1(X)$ (profinite etale) so, like, it doesn't literally parse but I think I know what he means. Regarding its validity, I have reservations related to conjugacy synchronization and construction of "the diagonal" (points in Mochizuki's theory are replaced by conjugacy classes of decomposition group and evaluation of functions is replaced by restriction of cohomology classes to decomposition groups — since we want to evaluate at many points simultaneously one needs them to be determined up to conjugation simultaneously — we need a diagonal action of a conjugation not an action by a product of conjugations. This process involves multiple fundamental groups.) — BUT if we are going to dispose of these things entirely as in (1) you can probably scrap a lot from section 2 of IUT2. I need to think about how this construction works in order to say something precise. I personally would ask Emmanuel Lepage or Jakob Stix about this.

A more middle ground statement is that one can replace all "base Hodge theaters" by a single "base Hodge theater"—which have more than just $\pi_1(X)$ and a bunch of maps between them. Yet another variant of this would involve contracting even more fundamental groups in the base Hodge Theater (and this is where I think you might run into conjugacy synchronization and diagonal problems).

Also, there are a lot of monoids built into the theory which are excluded from the statement and I don't know if these are implicitly assumed to be identified as well. I think not.

Finally, to derive a contradiction, I think some things need to be said about *how* one proposes to reduce Mochizuki's construction to "a single fundamental group".

All in all to build this into an actual counter-example, as Peter claims, one needs to make the statement more precise.

Remark. In a post I read someone emphasized that Peter S. read "hundred of pages", or that he met with Mochizuki "for a week". Similarly, Fesenko has emphasized the number of hours people have studied the theory. These are personal reasons not to undertake something and not acceptable substitutes for proofs.

##################################
##################################
##################################

Now more comments…

****
Intertwining
****
>On the other hand, I read say on page 140 of IUT-3 that Mochizuki considers the Theta-intertwining, which I believe simply means this identification of abstract Theta-pilots with concrete Theta-pilots. He wants to be very careful with using this etc., but I do believe he wants to (and has to) use it somewhere. So I don't think you can simply cut there. I think the closed loop that Mochizuki discusses on page 143 of IUT-3 is also relevant here. See also on page 144 the simultaneous Theta-intertwining and q-intertwining he wants to have (up to indeterminacies etc…).

There is more of this on page 188 too (April 16, 2020 copy)

In the quoted statement above the "up to indeterminacies" is the point.

Mochizuki asserts the existence of TWO theta monoids you need to be thinking about relating. One that identified with q-monoid and the other which has been interpreted alongside q in the same Hodge theater. In Mochizuki's language one is in a "alien holomorphic structure" and the other is in interpreted from the same structure. Mochizuki's game is then relate the auxillary theta pilot using anabelian techniques (amphoricity in the language of Joshi's manuscript) in order to derive a relation between the two. This relation is a uniformity statement. All the thetas of the world can be blurred.

At one point I was confused about what is going on with the interpretations in absolute galois groups of p-adic fields. It is helpful to know that the measure spaces constructed from absolute galois groups of p-adic fields have measures which are well defined—in the initial construction in AAG3 (section 5?), the measures take values in an ordered one dimensional real vector space obtained by the process of 'perfection' ($\operatorname{colim} M$, nodes = $\mathbb{N}$, morphisms=$(a \mapsto ab)$ realized as multiplication by b) and completion. It turns out that because of initial theta data, the we know how to normalize the measures to give real numbers.

My point: the constructions of these measure spaces may look weird, but from the perspective of either interpreting structure these things give real numbers, and the values of these measures don't lie in some abstract ordered one dimensional real vector space. Also, there exists a region $U \subset \mathbb{L}^{\vdash,et}$ (this is my notation for the monoanalytic etale like big log-shell) that is invariant under automorphisms of log-linked collections of Hodge Theaters. In Mochizuki's terminology, "this region can be seen from both sides of the theta link".

>In any case, to me it sounds like you are saying that the Theta-pilot has nothing to do with Theta-values.

On the contrary, all theta pilots are defined *in* structures which includes an evaluation map like (theta monoid)$\times$(evaluation points)$\to$(constant monoids). Look at the diagram in IUT Remark 3.10.2 and read it clockwise starting from gau and ending at etale $\mathfrak{lgp}$. Each of these $\Vdash \blacktriangleright \times \mu$ strips are parts of a larger structure where multiplication of the "value group portions" may act on the "unit group portions". Eventually these "Kummer maps" lead to the construction of the region U_{Θ} described previously.

> Well, strictly speaking (modulo minor issues) what Mochizuki does is the following: He takes the Theta-value (some nonzero p-adic number, not a root of unity), and takes the (multiplicative) submonoid M of the p-adic numbers generated by it, and then considers M as an abstract monoid. As an abstract monoid, it is then of course isomorphic to N, necessarily canonically so; its generator is referred to as a Theta-pilot. Does the abstract monoid M know anything about the Theta-value? Of course not! But that is all that ever seems to enter the story of the Theta-link.

This goes back to my comments about his objects not having good black boxes (without some sort of language like the of interpretations). This also points to my point about his confusing writing style in the beginning. It is typically not sufficient to perform his constructions without knowing about the interpretation.

>At some point you do need to remember the interpretation of the Theta-pilot in terms of Theta-values — this is what Mochizuki calls the Theta-intertwining, and it plays a key role as I tried to reference. So I don't think that Mochizuki simply cuts our diagram there.

Again, from the horses mouth, read C12 of his responses. He says he does not do this. Also, please look at the displayed diagram below. I think this may clarify something for you. Also, in this "interwining", this is where the theorem about the amphoric/characteristic nature of the interpretation of the Jacobi theta function is supposed to be applied. I made some remarks before on this and refer to those.

>Now David Roberts previously mentioned that Mochizuki seems to have a nonstandard point of view on what a mathematical object is, requiring separate copies when they are not actually needed; he often refers to some "history" of these objects. Now maybe one could try to argue that because M was built out of Theta-values, it (the abstract monoid) still knows something about them, that can be even transported via an isomorphism of abstract monoids. (You made some similar remarks in your paragraph on N and Kummer theory, refering to "interpreted structures".) But that's evidently not the case. (That the abstract monoid M knows something about Theta-values might be debatable if for you an abstract monoid is really encoded — as it is in ZFC — in terms of its actual set of elements. But even then, this structure ("the

interpretation") is clearly not transported by an isomorphism of abstract monoids.)

I think this quoted text reinforces my points about "depth searches" and the ambiguities (percieved or not) in Mochizuki's writing. Peter I am going to use the words "My understanding" so you may want to skip this response.

Regarding ZFC, while this is true, we don't go this deep. That is not what is going on here. (for the uninitiated see https://stacks.math.columbia.edu/tag/0009)

My understanding of the "history of objects" is different, and I don't think this saves the day. "Forgetting history" is Mochizuki-speak for forgetting about the interpretation as you have suggested. Automorphisms of objects are no longer coming from the interpreting structure and this structure is no longer "going along for the ride".

In Mochizuki's language or the language of naked categories Mochizuki's theory does not have many good black boxes. This makes parsing the examples even harder because you need to carry around these large definitions in our small human brains (or use a large amount of paper). If you pick up a Hodges a "a Shorter Model Theory" or Olivia Caramello's book, or MacLane's Sheaves in Logic book, you will find perfectly fine definitions of interpretations that allow you to package all of that sloppy anabelian geometry/ "reconstruction algorithms" into nice little formulas.

Here are some of David Marker's notes:
http://homepages.math.uic.edu/~marker/math512-F13/inf.pdf

Exercise: Look at the first page of Marker's notes then show that the torsion of an abelian group is definable using infinitary formulas.

Exercise: People often complain about the non first orderizability of topological spaces. Read this post from stack exchange on how to define topological spaces:
https://math.stackexchange.com/questions/46656/why-is-topology-nonfirstorderizable

>About the indeterminacies: I'm focusing on (Ind2); (especially) the part concerning automorphisms of local Galois groups (acting on local units if necessary). I don't see any diagram that doesn't commute when you take the identity isomorphisms, but does commute when you conjugate by an automorphism of a local Galois group (acting on local units). Why do you need that, except because "you need to forget the history of your objects in order to apply reconstruction algorithms" or some magic like that?

I'm not sure what "that" is. I'm going to assume you mean ind2. I need to break this down in order to give you a coherent response. For me, ind2 is a representations of automorphisms absolute galois group of a p-adic field $G \cong G_K$ on a $\mathbb{Q}_p$-vector space $\mathcal{O}^{\times \mu}(G)\otimes \mathbb{Q} \cong (K,+)$ induced functorially. There are interpretations given from local class field theory (I'm going to release a table of these soon in my manuscript with Anton, alternatively you can find these in some papers of Hoshi). These preserve the $\mathbb{Z}_p$-lattice $\mathcal{O}^{\times \mu}(G)$ inside this vector space.

I'm not sure what sort of diagrams you want but here is one:
Let K be a finite extension of $\mathbb{Q}_p$ with uniformizer π where $\log(1+\pi) = \pi$ and $\log(1+\pi^2)=\pi^2$.
Let $\lbrace x\rbrace$ be a one point set.
Let $f,g: \lbrace x\rbrace \to K$ be given by $f(x) = \pi$ and $g(x) = \pi^2$.

$$\begin{CD}
@>>> \lbrace x \rbrace \\
@AAA @VVgV \\
\lbrace x \rbrace @>>f> K
\end{CD}$$
(I don't know how to make the diagonal arrow)
Considering K up to ind2 makes the diagram commute. Something tells me this is not what you had in mind. Ind2 has the property of "mixing valuations".

In terms of automorphisms of G these give you the freedom to move around the higher ramification groups. The reference for this is Joshi's manuscript.

Here is the standard warning: this will not save the day and you will NEED ind3 to make the formulas work as this sort of doesn't mix very much. The $\mathbb{Z}_p$-submodules $p^j \log(\mathcal{O}_K)$ will always be preserved under ind2, so $K = \coprod_{j \in \mathbb{Z}} p^j \log(\mathcal{O}_K) \setminus p^{j+1} \log(\mathcal{O}_K)$ and each annulus is preserves setwise under ind2.
This is saying, in some sense, that ind2 (and ind1) do not do too much. Moral: in order for Mochizuki's inequality to be true, you *do* need ind3.

The purpose of ind2 (and the other indeterminacies) is to perform a construction that is independent of the structure that it was constructed from.

>(There was actually a point where Mochizuki was surprised that by "forgetting the history of a group" one still has more than a group up to isomorphism: Namely, a group. That the datum of a group is strictly more than the datum of a group up to isomorphism seemed new to Mochizuki. I believe this is the (psychological) main reason he considers these full poly-isomorphisms.

Classic Mochizuki.

>But really, forgetting "the history of the groups" you still have groups and they may still have natural commuting isomorphisms between them. Of course, for groups up to isomorphism you can't ask for natural commuting isomorphisms between them, then you only have "full poly-isomorphisms".)

To be honest, I'm not sure what you mean by "history of groups" and it actually hurts my head to think about this or to think about you thinking about this. If you want to email me a reference I might be able to say what is going on here but I would need to see it. I'm also don't want to defend the usage of "history" in these documents.

>A few comments up I summarized the situation with claims (1), (2) and (3). I have seen no valid objection to (1) and (2), and (2) alone would lead to a contradiction (as one gets too strong a form of ABC).

I addressed this in the introduction.

>To (3), Taylor indicated where to cut the diagram, but I really don't think this is what happens, as this would isolate Theta-pilots from Theta-values and effectively remove the actual Theta-values from the proof; while Mochizuki does consider this "Theta-intertwining" which is the association of the Theta-pilot with the Theta-values.

I addressed how Mochizuki proposes to bring theta values back into the proof; using the anabelian geometry. How this actually works is unclear to me. Also, I think Mochizuki would object to your characterization "theta intertwining" here. He would say something about the "strong anabelian properties of the etale theta function" which might be equally imprecise. He would also point to a number of other constructions which would include MOD vs $\frak{mod}$ constructions, and the theory of cyclotomic synchronizations. How the q and Θ pilot objects are tied is the mystery and has been the subject of my conversations with everyone for the last several years (which is embarassing to admit outloud).

What is missing to me is the comparison between the U_{Θ} and q. Mochizuki and Hoshi have very patiently been trying to explain this to me for a long time now. Maybe you want to call this a gap. Maybe I'm
dense. Mochizuki will undoubtedly call this a "fundamental misunderstanding" and point to some aspect of his manuscript. Me (and Emmanuel Lepage and others) have

been talking about this since 2017 and are stumped.

****
Some "Refereeing" of the log-links discussions between Peter and UF
****

@UF April 11, 2020 at 9:02 pm
–C7 of the 05 comments actually says that you *can* switch.
–I don't think this rigidifying/derigidifying should be emphasized and will just confuse people. Calling a pair $(\Pi, \overline{M}) \in [\pi_1^{temp}(Z^+),\mathcal{O}^{\vartriangleright}_K]$ a "rigidified fundamental group" is weird.

@Peter Scholze, April 12, 2020 at 4:58 pm
In this Peter chooses to address the log-links as an example of indeterminate copies doing nothing. Here he claims these can be replaced by logs.

My understanding of the situation for log-links is different. I'm going to give an abstract setup and I claim something like this occurs in IUT. Fix $\overline{M} \cong \mathcal{O}^{\vartriangleright}_{\overline{K}}$. This can be decomposed as $\overline{M} \cong \pi^{\mathbb{Q}_{\geq 0}} \cdot \overline{M}_{tors} \cdot F$ where $\overline{M}_{tors}$ is the torsion subgroup and F is the free part (at finite level these are free $\mathbb{Z}_p$ modules). Furthermore for the subgroup of units we have $\overline{M}^{\times}\cong \overline{M}_{tors} \cdot F$. Suppose now that one only knows $\overline{M}$ up to automorphisms of $\overline{M}_{tors}$. In order to get a well-defined object, one thing that we can do is mod out by torsion $\overline{M}^{\times} \to \overline{M}^{\times \mu} = \overline{M}^{\times}/\overline{M}_{tors}$ this is now well defined.

Mochizuki would say the converse. That we want to manipulate $M^{\times\mu}$ (via ind1 and ind2) and this manipulation needs to be independent (lifted to?) from the $\overline{M}_{tors}$, which plays a role in the exterior cyclotome of the frobenius-like mono-theta environment, which is used for passing structures "down to the bottom" in IUT3 3.11.

@UF, April 13, 2020 at 10:16 pm
It seems that you are both talking past each other. Log links are about relationships between monoids *not* fundamental groups.

>It seems quite likely Mochizucki uses "switching-symmetry" in a technical sense, synonymous with "multiradiality" of some algorithm reconstructing the data (here a rigidified column of log-links) at hand from some choric data, as he often does, compare e.g. [Alien, p.51].

There is a technical sense. Given a morphism of connected groupoids $F:\mathsf{A} \to \mathsf{B}$ one can form the category $\mathsf{A}\times_{\mathsf{B}}\mathsf{A}$ whose objects are (A_1,A_2,f) where $f: F(A_1) \to F(A_2)$. When F is full and essentially surjective the switching functor is $(A_1,A_2,f)\mapsto (A_2,A_2,f^{-1})$. In practice, as I have said before, this seems to be note enough for applications as objects in categories seem to be more. Also, I think there is a problem because we literally want a map of sets at the end of the day. I haven't worked out how to use this formalism effectively.

>His statement would then mean that if we rigidify the vertical columns, then there is (unlike in the non-rigidfied case) no multiradial algorithm to recover this column from certain choric data. This does not sound so absurd anymore (to me).

Minor typo: "choric" should be "coric"

>Now which multiradial algorithm does he mean here? I would suggest it may be the multiradial algorithm in [IUT III, Cor 2.3], more specifically, the first part of 2.3 (ii) which concerns its compatibility with log-links. Note that close by, [IUT III, Rem 2.1.1 (ii)] the issue we are talking about "why π_1 only upto indeterminate iso?" is discussed. For further discussion see also in [IUT II, Rem 3.6.4 (i)]. In any case, I agree this is an important issue to track down.

This is addressed in the comments at the very very bottom.

@Peter Scholze, April 14, 2020 at 5:17 pm

>The remarks from IUT that you cite make heavy reference to his paper on etale theta-functions, which seems to play a key role in the IUT papers. This paper gives some neat algorithm to start from the fundamental group of a once-punctured elliptic curve with bad semistable reduction, and recover its Tate parameter q and some Theta function; I forget the details. While this is all good and well, I don't see the relevance: Mochizuki's more general anabelian theorems, discussed previously on this thread, tell you that from the fundamental group you can simply recover the whole curve. In these comments of Mochizuki that you reference, Mochizuki is discussing some nitty-gritty details of this algorithm, but this seems completely besides the point if you just remember that relevant pi1X's are equivalent to relevant X's, so of course you can recover all invariants of X, and you can do so functorially in pi1X.

If we are collection complaints about exposition I have another: As Peter has mentioned this and many other pieces of text in theorem environments punt you WAAAY back to topics in other papers. As Peter is mentioning this goes back to Etale Theta which I think is at least 5 papers back (IUT3>IUT2>IUT1>AAG3>AAG2>AAG1>EtTh) to be fair one could argue that AAGX and EtTh are independent. Either way, this is a big dependence, and we haven't even touched the dependencies of the AAGX papers.

I responded to the second part of this remark already. You asked about 3.11.i. Also, this $\mathfrak{R}^{LGP}$ is actually not so complicated. The one in Cor 2.3 $\mathfrak{R}$ that is the bad one (no pun intended). I'm going to postpone this dicussion until the end.

@UF April 15, 2020 at 9:04 am
>I now think a good reason to for not identifying π_1's along log-links is as follows:
(It is very similar to [IUT-2, Rem 1.11.2 (ii)] for the theta-link; I am less precise here, taking the relevance of some reconstructions on faith, but it is a intuitive story, and precision can come later)
>First, let us step back and look at the log-theta-lattice: It consists of Hodge-theaters and the log/theta-links as maps/functors between them. Now a formulation of (2) is: Why do we, say for the log-links, not just take the "identity" isomorphism between the relevant π_1's instead of the full poly-isomorphisms between them? Surely it is also a completely valid map between the theaters?
>Yes, but: let us recall what we want to do with this log theta-lattice: we want at "suitable times(?)" glue the Hodge theaters along these links and then apply certain multiradial algorithms.
>I think that if we would glue with the "identity" on the $\pi_1's$ (and all the other prescriptions of the log-link), then this gluing will be inconsistent/not well defined (when combined with some multiradial algorithms), in some vaguely similar sense as if you try to define a complex structure on a manifold by charts, but in some region the procedure leads to two different, incompatible complex structures, i.e. the transition map is not holomorphic.
>Indeed, in one Hodge theater, the π_1's not independent of the rest of the data; it acts via the quotient to the Galois group on the monoids. Thus π_1's "related" to the monoids, and it is not apriori clear we are allowed to glue two Hodge theaters by gluing π_1 and the monoids separately in whatever way we wish (and then follow certain multiradial algorithms).

I'm going to refer you to my forthcoming manuscript with anton (the prequel to the one with the IUT4 style computaions). There is some stuff on my vlog that is a (poor) template for this stuff if you don't want to wait.

>In the above analogy, a certain region of the Hodge theater is supposed to be "determined" jointly by the π_1 and the monoids.
The multiradial algorithm we consider is roughly as follows: The action of π_1 determines on the (from the Galois group reconstructed) local monoids (with 0) in addition a ring structure (a "holomorphic" structure).
>The output is roughly some etale version of some of the data originally present in the Hodge theater, and the reconstruction algorithm yields a canonical isomorphism between the original data and the reconstructed one (see e.g. [Alien, §2.12] for essentially such constructions).

This *is* a thing.

>If one now glues two Hodge theaters along a log-link (with in addition "identity"-gluing for π_1's, then this yields no consistent holomorphic/ring structure compatible with the above algorithm on some region:
>Namely, consider the region of the ring (local units), which is glued via log (the actual log part of the log-link) to other Hodge theater; this yields one definition of "holomorphic" for this region.

I can't parse this sentence.

>On the other hand, we can embed this region in the reconstructed monoids and use for the "holomorphic" structure on the reconstructed monoids the one induced by π_1 on both Hodge theaters and glue by the "identity".

This is true. You definitely have one structure from the base and one structure from pulled back from log. The diagrams commute though.

>The above roughly describes two incompatible holomorpic/ring structures on some part of the glued Hodge theater; they are incompatible, "because the transition map, essentially the log, is not a ring map". Such a situation is presumably avoided by taking the full polyisomorphism between π_1 where the log map on the units alone defines the holomorphic structure on the relevant region.

I'm not sure I parse this completely. I will say there is a way to get the a commutative diagrams of fields in the log-kummer correspondence. It depends on what maps you take. Any "log" has a backwards map which is an isomorphism of fields. I will refer to the forthcoming paper with Anton for details.

****
@Peter Scholze April 15, 2020 at 2:05 pm
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>Removing all language from your message, I end up with the following message: The logarithm map is not a map of rings. This is why you have to consider source and target up to some indeterminacy.

The log-link is about the monoids and not the base (you use really only use the fundamental group to use Mochizuki's interpretation of a field in order to pullback this structure so you can take the logarithm).

I don't know if this helps but there is a "post-logarithm" that IS an isomorphism of rings and this IS used the definition of the log link. I think it is nice to factor the actual logarithm as $\log^{post}\circ \log^{pre}=\log$ where $\log^{pre}$ is the map in IUT. One can then use the map $\log^{post}:\mathcal{O}^{\times\mu}\otimes \mathbb{Q} \to K$ and pull back the field structure to get a new field $K_{\log}$ which is $\mathcal{O}^{\times \mu} \otimes \mathbb{Q}$ as a set but with this new field structure.

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@UF April 15, 2020 at 4:57 pm
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>What my post above attempts to show, is: if passing to poly-isomorphism has the effect of doing no gluing/no identification of ring structures (arising from π_1 just the gluing from the actual log map), then the only gluing left is the actual log map, which gives one global chart, and no transition functions needed, essentially(?) since just one chart.

Yes, but I think the point is what I was saying about killing indeterminacies. Doing log-links isn't "for sport" as Mochizuki would say.

>I before never really seriously considered that full poly-isomorphism could have the effect of "no gluing arising from this part" (instead of "choose your favourite gluing")

I don't think I understand how you are thinking about this.

****
Remarks on IUT3 2.3.ii
****
We are looking to see if this statement says something non-trivial about the bases of log-linked hodge theaters and multiradiality (I actually think this is barking up the wrong tree since, as I've said before, the bases in log-linked hodge theaters do nothing except impart ring structures to the monoids). Indeed this is the case because of assertion (2) below. Let me just remark though for Peter, that the maps between the $\times \mu$ prime strips are what are going to encode everything in Galois groups and what eventually gets the theta back to the other side.

Anyway, at the beginning of the proposition there is a certain amount of setup. BUT, notice in the setup, most of the structures are interpreted! This means that their morphisms are just functorially induced by the interpreting structures, in this case there is a prime strip of the $\times \mu$ variety, and a base hodge theater (actually, it is even simpler than this… see the note below). Looking at my notes… this looks tautological.

Also, just as a tip from as a person who has wasted his life doing this, the thing to look out for are maps that are NOT full polyisomorphisms.

In this item there are a total of three assertions.
1) log-links induce a full polyisomorphism of the $\mathfrak{R}$'s;
2) Theta links on log-links pairs of hodge theaters have $\mathfrak{D}_{\Delta}^{\vdash}$'s interpreted in $\mathfrak{D}_{\Delta}^{\vdash}$ which are isomorphic.
3) Using (1) and (2) the $\mathfrak{R}$ across is generically isomorphic across log-links; the $\mathfrak{D}^{\vdash}_{\Delta}$ data is generically isomorphic across Theta links.

Proof of 1: This only could be a problem because of the nature of the way the monoids are linked and the dependence of $\mathfrak{R}$'s on these monoids. In the data for $\mathfrak{R}$ there is exactly one place where the monoids interact. This is in the $\times \mu$ prime strips. Everything else is functorially induced. For example, in the definition he takes the monotheta environment interpreted from the base (this actually has well-definedness issues of its own!).

Proof idea of 2: As described somewhere in this note ind2 is isomorphisms of $\mathcal{O}^{\times\mu}(G)$'s induced by isomorphisms of G's. If you look up the definition of $\mathfrak{D}^{\vdash}_{\Delta}$ you will see that it is just a bunch of G's. This means the generic isomorphism of from the G's induce an ind2 on the $\times \mu$ monoids they interpret; the $\times \mu$ monoid isomorphisms in the theta link are the same thing.

Expositional Note: This is an example of a proposition where too much structure was invoked for my taste. In the fine print you notice that the interpreters are $\mathfrak{D}_{>}$ and $\mathfrak{D}_{\Delta}^{\vdash}$ (=fundamental groups at each place and absolute galois groups at each place respectively). In the statement he references the full base hodge theater. That's baggage. Also, you will notice that this could have been broken into many much simpler assertions each which can be individually checked.

****
Remarks on IUT3 3.11.i
****
Peter had asked (essentially) how can galois groups interpret theta values? How can they do anything?!

Well, I claim that Theorem 3.11.i and 3.11.ii are not the difficult parts of Theorem 3.11; Theorem 3.11.iii is the weird part. There are some subtle differences between the (abc)-module structure in both of these statements. In the first, the actors (b) and (c) are defined as subset of the $\mathbb{L}^{\vdash,et}$ (at the appropriate places) in part i. In part ii there is a clear module structure.

In the situation of (i) you lift to an richer stronger and more powerful structure that allows you to define (b) and (c) then then you stick them inside the log shell. What the proposition is saying is that this is well-defined up to ind1,2 — so everything that was stuck inside these log shells now is considered up to a jumbling of the type described in a previous response. I will refer readers to my manuscripts with Anton Hilado for these formulas.

A subtle difference that I want to point out to everyone here which they may not have noticed is the difference between the usage of the MOD, LGP constructions and the $\frak{mod}$, $\frak{lgp}$ constructions in the later.

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Sorry if I made any errors anywhere. I didn't mean to and tried to proofread this. Hopefully we learn as part of the corrections.

Best,
Taylor

112. **Peter Scholze**
April 17, 2020

Dear Taylor,

thanks for these final comments! I think I should answer to this. Let me first say that I agree with much of what you write, and for the sake of keeping this short, I only jump at the few places where I disagree.

> Regarding (3): Is Mochizuki's Proof Falsified? […]

> Finally, it is not outside the realm of reality that there could be 5 top notch international referees who have understood the proof as complete and correct.

Really? I would have hoped that in that case at least one of them — not in their role as a referee, but simply as a mathematician who wants to share insight — would have come around and explain the key ideas in a way that is understandable.

> Because of these difficulties I personally hold Mochizuki's document as a program for proving ABC and consider Mochizuki's formula a conjecture (until I can see this with my own eyes). Then again, there are a lot of Theorems I know about and use but can't prove.

I very strongly object to the implicit (I believe) assertion that Mochizuki's result should now be considered just another of these really difficult theorems whose proof we never understood.

> *****************
> Regarding (2)
> *****************

> Also, there are a lot of monoids built into the theory which are excluded from the statement and I don't know if these are implicitly assumed to be identified as well. I think not.

Well, I think I would want to identify some, too. By mode of thought is that starting from your elliptic curve, you can easily cook up all the data that forms a Hodge theater, including those monoids. Simply take that collection, and always the same one. Later, when you study log-links, try to understand what they do, which diagrams do not commute, and if necessary enlarge your diagram (of course you get a non-commutative diagram if you want that the logarithm equals the identity). My issue lies with possible non-identity isomorphisms of Hodge theaters being relevant at any point; they are not for all I can see, as all possible non-commutativity can't be restored in terms of internal isomorphisms of Hodge theaters.

> Finally, to derive a contradiction, I think some things need to be said about *how* one proposes to reduce Mochizuki's construction to "a single fundamental group".

As the final step in Mochizuki (proof of Cor 3.12) is so unclear, it also seems impossible to fully justify where and how it breaks.

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Intertwining
****

> Again, from the horses mouth, read C12 of his responses.

OK, I reread this. He talks about the q- and the Theta-holomorphic structure. This indeed sounds like there must be two distinct Hodge theaters around — Hodge theater is, I believe, the technical version of "ambient holomorphic structure". But there are not! All of them are isomorphic. And if I identify them (using whatever isomorphism) I can see plainly that this does not make any sense.

> He says he does not do this. Also, please look at the displayed diagram below. I think this may clarify something for you.

Details seem to be off in the example, but in any case I totally see that some non-commutative diagrams become commutative up to these indeterminacies — otherwise they would not be indeterminacies. But I'm asking about a relevant one.

> Also, in this "interwining", this is where the theorem about the amphoric/characteristic nature of the interpretation of the Jacobi theta function is supposed to be applied.

The "amphoric" nature here is just that you can recover X from $\pi_1(X)$, so in particular the theta function (and all else). I'm still failing to see the importance of etale theta functions in all of this.

> Exercise: Look at the first page of Marker's notes then show that the torsion of an abelian group is definable using infinitary formulas.

> Exercise: People often complain about the non first orderizability of topological spaces. Read this post from stack exchange on how to define topological spaces: https://math.stackexchange.com/questions/46656/why-is-topology-nonfirstorderizable

I'm totally lost about where you're trying to go here.

>About the indeterminacies: I'm focusing on (Ind2); (especially) the part concerning automorphisms of local Galois groups (acting on local units if necessary). I don't see any diagram that doesn't commute when you take the identity isomorphisms, but does commute when you conjugate by an automorphism of a local Galois group (acting on local units). Why do you need that, except because "you need to forget the history of your objects in order to apply reconstruction algorithms" or some magic like that?

Thanks again for the example in reply to this, but I'm asking about a relevant diagram in the relevant abstract setting.

> The purpose of ind2 (and the other indeterminacies) is to perform a construction that is independent of the structure that it was constructed from.

This slogan I understand, but where does Ind2 help with anything? Why do you need to introduce it?

> I addressed how Mochizuki proposes to bring theta values back into the proof; using the anabelian geometry. How this actually works is unclear to me. Also, I think Mochizuki would object to your characterization "theta intertwining" here. He would say something about the "strong anabelian properties of the etale theta function" which might be equally imprecise. He would also point to a number of other constructions which would include MOD vs
constructions, and the theory of cyclotomic synchronizations. How the q and Θ are tied is the mystery and has been the subject of my conversations with everyone for the last several years (which is embarassing to admit outloud).

> What is missing to me is the comparison between the U_{Θ} and q. Mochizuki and Hoshi have very patiently been trying to explain this to me for a long time now. Maybe you want to call this a gap. Maybe I'm
dense. Mochizuki will undoubtedly call this a "fundamental misunderstanding" and point to some aspect of his manuscript. Me (and Emmanuel Lepage and others) have been talking about this since 2017 and are stumped.

Well, I guess you are just pointing your finger to the same problem that I'm trying to point at. It is completely unclear how you get the actual Theta-values back in the game. You started this paragraph with "using the anabelian geometry", but as I argued long before, it is completely unclear how anabelian geometry helps with anything in his setup (by (1)).

> *******************
> @Peter Scholze April 15, 2020 at 2:05 pm
> *******************
> >Removing all language from your message, I end up with the following message: The logarithm map is not a map of rings. This is why you have to consider source and target up to some indeterminacy.

> The log-link is about the monoids and not the base (you use really only use the fundamental group to use Mochizuki's interpretation of a field in order to pullback this structure so you can take the logarithm).

> I don't know if this helps but there is a "post-logarithm" that IS an isomorphism of rings and this IS used the definition of the log link. […]

My main question in the discussion with UF has always been the question of full poly-isomorphisms vs. identities between $\pi_1(X)$'s, i.e. issue (2). All you are writing is completely tangential to this. I think you actually agree that the log-link has so little to do with $\pi_1(X)$'s that it can't possibly be the reason to consider $\pi_1(X)$ up to full poly-isomorphism. Yet Mochizuki was trying to make that exact point when we were discussing.

> *******************
> Remarks on IUT3 2.3.ii
> *******************

Why are you discussing this? I'm lost. Is it saying more than Theta-values being the same in all Hodge theaters?

> *******************
> Remarks on IUT3 3.11.i
> *******************
> Peter had asked (essentially) how can galois groups interpret theta values? How can they do anything?!

> Well, I claim that Theorem 3.11.i and 3.11.ii are not the difficult parts of Theorem 3.11;

You are simply ignoring my point!

Best wishes!
Peter

113. **Peter Scholze**
April 17, 2020

PS: I just realized that maybe the following information is worth sharing. Namely, as an outsider one may wonder that the questions being discussed at length in these comments (e.g., the issue of distinct copies etc.) are very far from the extremely intricate definitions in Mochizuki's manuscripts (his notation is famously forbidding, some of it surfaced in Taylor's comments), and feel almost philosophical, so one might wonder that one is not looking at the heart of the matter.

However, the discussions in Kyoto went along extremely similar lines, and these discussions were actually very much led, certainly initially, by Mochizuki. He first wanted to carefully explain the need for distinct copies, by way of perfections of rings, and then of the log-link, leading to discussions rather close to the one I was having with UF here. He agreed that one first has to understand these basic points before it makes sense to introduce all further layers of complexity. (I should add that we did also go through the substance of the papers, but kept getting back at how this reflects on the basic points, as we all agreed that this is the key of the matter.)

114. **UF**
April 17, 2020

@Taylor Dupuy: Thanks for your comments.
The context for comment (C7) is Mochizukis interpretation of footnote 5 in [Scholze-Stix]. Mochizuki gives this interpretation in the first display of (C7): the rigidification mentioned there is (I think) not (some kind of) rigidification of π_1 in one Hodge theater (which you seem to mention, by considering pairs (π_1, monoid)), but the rigidification among the different π_1's in one vertical column by choosing a specific isomorphism ("identity") instead of full poly-isomorphism

between the different π_1's in one column.

Mochizuki then says that one can consistently identify the π_1's as above, if one forgets about both log- and theta-links. (Mochizuki does not say so explicitly right here, but I think in this situation there would be a switching-symmetry. He says so in a closely related situation after the second display, which in my understanding amounts to the same situation).

In the second display in (C7), he seems to say (my " " are not meant here as quotes): In contrast, if one keeps log-links (or maybe similarly theta-links), then the rigidification of the π_1's "depends" on the other data, say "0-column Frobenius-like data (at different vertical spots) with log-relations", which do not admit a switching-symmetry between two vertical columns.
Here in my understanding "depends" indicates that the log-identifications between the 0-column Frobenius data cannot be made completely independently from the identity-identifications of the π_1's in the 0-column, since at a fixed vertical position the π_1 and the monoids are "related/not independent" (via the action). Implicit here (and relevant for the context of this C7) seems to be the assertion that because the rigidification of the π_1's "depends" on the 0-column Frobenius data (which does not admit a switching-symmetry) , something has to go wrong with switching (when π_1's are rigidified in a column), "because" it goes wrong for the related/dependent 0-column Frobenius-like data. (If this is *not* implicit here, then what in Mochizukis response in C7 is the objection to the vertical rigidification of π_1's?)

After the second display he says: If one forgets about the 0-column Frobenius data (in particular the log-relationship between them at different vertical spots), then there is a switching-symmetry between two neighboring columns.

This is how I understand Mochizukis C7; I may certainly understand it wrong in parts, and I would be happy to understand it better. All the rest I wrote is an attempt make this (from my point of view) "implicit objection to rigidifying π_1's" in the second display more explicit; but if my understanding concerning this is incorrect, then this is largely meaningless.

115. **Taylor Dupuy**
April 18, 2020

Hi Everyone,

I'm going to take a break today to attend WAGON:
https://sites.math.washington.edu/~jarod/wagon.html

Check it out!

After that I'll take a look at the new comments and see if I have something to add.

Best,
Taylor

116. **n (long time blog lurker)**
April 19, 2020

Dr. Woit,

"I read Mochizuki's blog entry when it came out. To the extent I could make sense of it via Google Translate …"

maybe DeepL

https://www.deepl.com/translator

would be worth a try; it's got only few languages, but Japanese is one of them, and it gave me better results than Google Translate.

117. **W**
April 19, 2020

There has been some discussion about this comment thread that focused on the importance of the differentiating between fact and opinion. There has also been discussion that the long thread may obscure the points that Peter Scholze and Taylor Dupuy agree on by focusing on their areas of disagreement. I have written a (very long) comment to try to help with these issues.

The way this comment is intended to work is that the numbered paragraphs (1), (2), (3) give facts that are difficult or impossible to dispute and the paragraphs after them give opinions building on those facts.

(1) The first couple hundred pages of the IUT papers introduce a lot of difficult new terminology. Scholze and Stix demonstrated that none of this new terminology is necessary to prove any of the formal statements in these pages. Moreover, when the new terminology is ignored, the proofs all become simpler and easier to understand. They all follow quickly from Mochizuki's previous work on the p-adic Grothendieck conjecture.

As a reader, this should already make you skeptical of the accuracy of the paper. How can dressing up simple statements in confusing and difficult terminology for hundreds of pages represent progress in turning the solution of one problem (the p-adic Grothendieck conjecture) into a completely different problem (ABC)? There are mathematical arguments that begin by introducing a lot of terminology (e.g. work of Grothendieck) but nothing like this ratio of terminology to content has ever been successful before. However, you certainly shouldn't rule out Mochizuki's work based only on this evidence.

As an editor or referee, you should also be very skeptical at this point. Setting aside concerns about correctness, you have some duty to ensure papers published in your journal are clear and get to the point. Having hundreds of pages that can be compressed to much less by the removal of new ideas rather than their introduction is not normally consistent with good mathematical writing, but one could imagine that the remainder of the paper justifies this sufficiently.

(2) Many people have tried to read the papers, read and understood the first few hundred pages, and then were unable to verify a key point – Corollary 3.12. This point is either the first or the second statement in the paper which doesn't fit into the hundreds of pages mentioned in (1).

As a reader this should make you very worried! This is is not a pattern one typically sees when people try to understand a correct but difficult argument (regardless of how well or poorly it is written). Instead, different people almost always get stuck at different points. This is certainly true if the reason people are getting stuck is a lack of background in anabelian geometry, or a refusal to put enough work into it – both reasons suggested by defenders of IUT. You would then expect to see different confusions in different places from people with different levels of background, or from people who made careless errors at different points. Of course, what we see is exactly what you would expect to see if the proof of Corollary 3.12 is not a valid argument but the rest of the paper is valid.

As an editor or referee, this should be completely unacceptable. Even if the proof is technically correct in the sense that Mochizuki or someone else can give a clear and precise argument which the proof of Corollary 3.12 could be said to summarize, allowing the paper to continue in its present form would be a failure at their duty to ensure papers in their journal properly explain their arguments. A paper such that everyone who studies it seriously, except a small inner circle, gets stuck at the same point simply isn't a proper explanation.

Some defenders of IUT like to point out that Scholze and Stix didn't give their precise objection until 2018. But this phenomenon, given that it was noticed by most people who read the paper seriously, should have been turned up by the refereeing process before then. This is, I think, the starting point for ethical concerns about the refereeing process. (For instance, OP's comment suggests that the editors could have asked a series of referees, ignoring those who have negative commentary, until they found someone willing to say it is good.)

Is it possible that the final version makes substantial changes to this argument and answers this objection? Everything is possible, but it seems unlikely. It is common practice in mathematics to edit the online version of the paper to the final submitted version, or something close to it, without the journal's formatting. As far as I know the main reasons not to do this are because one doesn't post online versions or doesn't edit them often, neither of which apply to Mochizuki.

Even if the editors and Mochizuki want to avoid this for some reason (like they want to make people read the journal to see the complete proof, I guess…) they could easily post some comment online like "Corollary 3.12 has been replaced with a series of simpler statements, each with a detailed proof, culminating in the key inequality, totaling about 35 pages" and get much less criticism from mathematicians and mathematical physics bloggers. Does anyone like receiving criticism? Is there any reason not to do this?

(3) Two serious mathematicians have come up with a precise objection to the proof of Corollary 3.12. Despite the fact that it is not clear exactly what Mochizuki means, they have come up with a plausible interpretation, and shown that, in this interpretation, the inequality that Mochizuki proves is off from the inequality he states by a factor of j^2. This is important because the improvement of the stated inequality over a certain trivial inequality is exactly by a factor of j^2. They have come up with additional arguments, which are unsurprisingly not completely rigorous, that any plausible interpretation of what Mochizuki means will have the same problem.

(One could say that it is not plausible that Mochizuki made such a simple mistake, but I think it is plausible in light of the previously discussed information).

Neither Mochizuki nor his defenders have come up with a convincing rebuttal to this. Mochizuki's response focused on arguing that it is possible for an argument like his to work, and that Scholze and Stix misunderstood it, rather than making basic clarifications to the argument that would aid in understanding whether the objection is valid, and that would certainly be possible if the argument is correct. For instance it is *provably* possible that any valid argument proving a concrete inequality using facts about affine spaces over the real numbers can be replaced with one that proves the concrete inequality using other concrete inequalities (or identities). Doing this would allow a more focused discussion because Scholze could either dispute one of the building-block concrete inequalities or dispute the implications from them to the desired statement.

For me this objection, combined with the other stuff, is devastating. Of course it is not the case that one must stop the presses of a journal every time a mathematician objects to an argument, and keep them stopped until that mathematician is satisfied. But combined with the other very worrying facts about the proof, this is an objection that must be answered for the paper to be published ethically. As mentioned earlier, if a better answer than what is in the currently online version has been delivered by Mochizuki to the journal, I would expect it, or at least an advertisement for its existence, to be posted online.

Despite the long comment thread, Peter Scholze and others have not exhausted all the reasons that a mathematician examining Mochizuki's argument should be skeptical that it, or even any argument like it, could possibly work. I could list these additional reasons, but they are not so relevant when the objections outlined above already mean that it would be inappropriate to publish the article in its current form.

118. **Dale**
April 23, 2020

Kirti Joshi has now posted a revised manuscript "On Mochizuki's idea of Anabelomorphy and its applications" discussed earlier in this thread. https://arxiv.org/abs/2003.01890

119. **Fierce Inertia**
April 24, 2020

This paper of Joshi is remarkably unconvincing to me. If I may caricature it slightly, it seems to only contain the following types of results:

1. Statements of the form "(Thing X / Property Y) depends only on the absolute Galois group of a p-adic field."

None of these are surprising or difficult: they all follow from basic class field theory or from the Jannsen-Wingberg theorem (which IS a difficult result, cf. here for a nice overview: http://www.numdam.org/article/AST_1982__94__153_0.pdf)

2. Statements of the form "(Thing X / Property Y) does not depend only on the absolute Galois group of a p-adic field."

These are even less surprising, and they also follow from Jannsen-Wingberg, or from five seconds of thought.

3. Completely unmotivated results (e.g. Theorem 16.5, Theorem 22.6).

4. Vague suggestions that various things can be interpreted anabelomorphically.

What evidence is there here that this perspective of anabelomorphy is actually useful? What can you DO with it? The answer this paper seems to suggest is: nothing.

I am happy to be convinced otherwise.

120. **David Roberts**
April 29, 2020

For what it's worth, the first two (of three) papers by Taylor and Anton Hilado are now on the arXiv:

"The Statement of Mochizuki's Corollary 3.12, Initial Theta Data, and the First Two Indeterminacies" https://arxiv.org/abs/2004.13228

"Probabilistic Szpiro, Baby Szpiro, and Explicit Szpiro from Mochizuki's Corollary 3.12" https://arxiv.org/abs/2004.13108

121. **Martin**
April 29, 2020

From the abstract of the first paper:

"This paper does not give a proof of Mochizuki's Corollary 3.12. […] These manuscripts are designed to provide enough definitions and background to give readers the ability to apply Mochizuki's statements in their own investigations. […] It is our hope that doing so will enable creative readers to derive interesting and perhaps unforeseen consequences Mochizuki's inequality."

It is my interpretation that the statement of Corollary 3.12 is reformulated in these papers. In the second paper, some consequences of the reformulation are derived. But aren't these consequences of an unproven, highly debated corollary – which most likely "cannot work" (Scholze)? Which kind of "creative reader" should use these reformulation then?

Taylor was quite active here, maybe he can comment on how these papers fit into the story?

122. **Peter Woit**
April 29, 2020

Martin,
Scholze's argument is not that the inequality can't be right, but that Mochizuki's proof of the inequality can't work. It's a reasonable project to understand the implications of conjectured inequalities, better if they're clearly labeled as conjectures…

In addition, another way to show Mochizuki's proof doesn't work and pinpoint exactly where it goes wrong would be to find a point where he derives something too strong, something that can be shown to be true by other methods.

123. **Taylor Dupuy**
April 29, 2020

Hi Everyone (and Peter),

In what follows I can give four proofs/reasons why a certain statement in Peter's manuscript with Jakob about replacing Hodge Theaters with fundamental groups is false. I believe these to be correct. Please check for mistakes.

Best,
Taylor

====================================

I noticed the following claim embedded into the manuscript.

Claim.(Scholze-Stix manuscript page 6, paragraph 4)
There is an equivalence of categories between a connected groupoid of objects isomorphic to a big Hodge Theater and a connected groupoid of schemes isomorphic to a once punctured elliptic curve over a number field sitting in initial theta data. This allows us to replace Hodge Theaters by once punctured elliptic curves.

Here is the exact text:

"In other words, up to equivalence of categories, choosing a Hodge theater is equivalent to choosing a once-punctured elliptic curve abstractly isomorphic to X, and this equivalence of categories is constructive in the sense that one can give an explicit functor that takes a Hodge theater and produces a once-punctured elliptic curve. Of course, the category of elliptic curves abstractly isomorphic to X (and isomorphisms of curves) is equivalent to the category whose only object is X that we started with."

This is stated in the manuscript without proof; they cite a number of propositions from IUT1 Corollary 6.12 (i), Proposition 6.6 (iii), Corollary 5.6 (ii), Proposition 4.8 (ii), Definition 6.13 mirroring Mochizuki's style.

I just want to point out that this is a MUCH more extreme a claim than what I thought Peter was claiming previously. This is why I had softened it for him in my response warning readers not to take him literally. After looking at the manuscript again it appears that he *did* mean what he said, literally.

This, I can dispatch. It is actually False in four different ways. It is false the first time because of the equivalence of categories statement. It is then false the second time because of the assertion that "picking a once punctured elliptic curve is equivalent to choosing a Hodge theater" statement. It is false a third time because these two statements are themselves not equivalent. We then give another independent proof that this is false by means of interpretation of initial theta data (similar to Mochizuki's original counter-argument in his "Report").

Also, most importantly, this type of reduction used to simplify Mochizuki's definitions is far from admissible. The notion of equivalence of categories and "equivalent" in a colloquial sense should not be confused. Equivalence of categories is too coarse of a notion to be useful in simplifying the definitions in IUT. The correct notion (for objects which are not considered up to automorphism) is the notion of bi-interpretability. This is a special type equivalence of categories which considers the types of formulas you are allowed to use. A reference to this notion in Olivia Carmello's book at the bottom of this post and another to the wikipedia article.

Remark. In the Scholze-Stix manuscript, they never actually say *which* number field the elliptic curve is defined over. I'm going to assume it is F as defined in initial theta data in IUT. If you take it to be the field of moduli you can still find examples that give a contradiction.

****
Lemma. $\operatorname{Out}(\pi_1(X_F))$ has cardinality strictly greater than 2 when $\operatorname{Aut}(F/\mathbb{Q})$ is non-trivial (which is the case for F in initial theta data).
****

We will use Tamagawa's proof of the absolute Grothendieck conjecture for curves over fields finitely generated over $\mathbb{Q}$.

See Theorem 0.4 of Tamagawa's paper "The Grothendieck conjecture for
affine curves" (Compositio 1997),
https://doi.org/10.1023/A:1000114400142

Here is a text version:
https://www.cambridge.org/core/services/aop-cambridge-core/content/view/1992BA14A2D63FA076DB39A34EC45E83/S0010437X97000614a.pdf/grothendieck_conjecture_for_affine_curves.pdf

Tamagawa's Theorem:
If F is finitely generated over $\mathbb{Q}$, and Z/F is a hyperbolic curve and (geometrically connected over F) then $\operatorname{Out}(\pi_1(Z)) \cong \operatorname{Aut}(Z/\mathbb{Q})$.

Now for the application.

Let E be an elliptic curve over $\mathbb{Q}$ which does not have CM geometrically (to be concrete you can take Cremona's 11a1). Let $F = \mathbb{Q}(\sqrt{-1}, E[30])$ and let $X_F = E_F – o$ where o is the origin so we have $X_F = \operatorname{Spec}(F[x,y]/(y^2-x^3-ax-b))$ for some $a,b\in F$.

Since $\operatorname{Aut}(X/\mathbb{Q}) \cong \operatorname{Aut}(F[x,y]/(y^2-f(x)))$ we see that $\operatorname{Aut}(X_F/\mathbb{Q})$ contains $G(F/\mathbb{Q})$ and it contains $\operatorname{Aut}(X_F/F)= \lbrace [-1]\rbrace$. These commute. Also note that $[F:Q]>1$. This means $\vert \operatorname{Out}(\pi_1(X_F)) \vert > 2$.//

Remark. I think the subtle point here is that $\operatorname{Out}(\pi_1(X_F)) = \operatorname{Aut}(X_F/\mathbb{Q})$ and not $\operatorname{Out}(\pi_1(X_F))= \operatorname{Aut}(X_F/F)$. Right? Either way, it is not so relevant because this isn't the biggest point of contention.

****
Lemma. Let HT be a big Hodge Theater. $\operatorname{Aut}(HT)\cong C_2$
****
Summary:
The automorphisms of the $\pm$ side of the Hodge theater is $\lbrace \pm \rbrace$.
There are only trivial automorphisms on the $\divideontimes$ side of the Hodge Theater.
These act independently because the way the two sides interact is through a fullpolyiso (this is described below).
This concludes the proof.//

Details According to Mochizuki:

Rem 3.5.2: "The morphisms of data are obvious." (I can't defend this remark. This is an omission.)

Prop 4.8(ii); Cor 5.6(ii): Auts of multiplicative base Hodge theaters are unique; these lift to morphisms of Hodge theaters.

Prop 6.6(iii); Cor 6.12(i): Auts of additive base Hodge theaters are unique; these lift to morphisms of Hodge theaters.

Rem 6.12.2(i)(ii): We regard the Θ bridge as being interpreted in the $\Theta\pm$ bridge. It talks about the $\Theta^{\pm \operatorname{ell}}\operatorname{NF}$ Hodge theater as being the additive and multiplicative part full polyisomorphismed along the interpretation in the previous item.

Defn 6.13(i): This is what I said above but I guess this is where he gives it a name.

Remark. For people who have no experience with Hodge Theaters, Figure 6.5 in IUT1 really contains all the information you need (with the technical definition of the "bridges"). There are 4 bridges and an "additive" and a "multiplicative side". It turns out that the only "not full polyisomorphism" part of these bridges are the downward ones in that diagram which connect local and global information.

Remark. I would like to check this part again and give more the details, but I think this is all correct.

****
Mochizuki's Falsification
****
Something like this was in Mochizuki's original response. This is the most important thing to understand why these reductions are incorrect. I think it was overlooked because people find his writing hard to read. His argument was that "$\pi_1(X)$ doesn't know about initial theta data". This is true.

More concretely, the fundamental groups at (say) double underline covers can't exist without the "global multiplicative subspace" (part of the initial theta data). They are defined in terms of the single underline cover which is the dual isogeny of the quotient determined by the finite group scheme $M \leq E$ (this M is the "global multiplicative subspace"). This is not interpreted in $\pi_1(X)$ in Mochizuki's papers or the Scholze-Stix manuscript (and I don't see how to choose one in addition to the many with choices of $\underline{V} \subset V(K)$ in a definable way).

Reference: The global multiplicative subspace is explained in my "statement" manuscript with Hilado (https://arxiv.org/abs/2004.13228). See the initial theta data section. We spell out the required "compatibility" between $\underline{V}$ and M there.

****
Categorical Structure is Not What Matters
****
I will explain why (naked) equivalence of categories is not the appropriate notion to be using to reduce structure in Mochizuki's proof. The appropriate notion is bi-interpretability. The idea here is that one needs equivalence of *highly structured categories* and if this notion is not used we lose a lot of information. David Roberts actually makes this point as well in his Inference essay.

Example. Consider a non CM elliptic curve up to isomorphism (as a relative object). If we were just going to consider it by itself then the connected groupoid of curves determined by this object is equivalent to a category with a single object and two morphisms (because the automorphism group will be a group of order two)! A connected groupoid with two morphisms doesn't allow you to do much.

Example. The structure $\mathbb{Z}$ (with its ring structure) has $\operatorname{Aut}(\mathbb{Z}) = \lbrace \pm 1 \rbrace$. Yet $Th(\mathbb{Z})$ is undecidable. This is a highly non-trivial but with a trivial automorphism group. The theory of a two morphism category with one object is certainly decidable.

The previous example shows the weird sort of things you can do if you admit replacing objects using equivalence of categories. In this case the connected groupoid of things isomorphic to a big hodge theater HT is equivalent to a connected groupoid of things isomorphic to $\mathbb{Z}$ (as a ring). The sorts of things you can do with $\mathbb{Z}$ are quite different from the sorts of things you can do with HT.

For structures which don't involve objects up to automorphism the correct notion I claim is bi-interpretability (btw, objects up to isomorphism and not up to isomorphism is something that is conflated in the SS manuscript). Two structures A and B are bi-interpretable if and only if there exists an interpretation of A in B and an interpretation of B in A such that when these interpretations are viewed as functors they provide an equivalence of categories. The functorial formulation can be found in Caramello's book for example.

See
https://www.oliviacaramello.com/Unification/ToposTheoreticPreliminariesOliviaCaramello.pdf
Definition 6.12

See for a formulation in terms of sets:
https://en.wikipedia.org/wiki/Interpretation_(model_theory)

(As a warning, Mochizuki's bridges and Caramello's bridges have nothing to do with each other… as far as I know).

124. **Peter Scholze**
April 29, 2020

Dear Taylor,

thanks for your comment!

Unfortunately, I cannot follow what you are saying.

First, as is the case throughout IUT, the initial Θ-data are fixed. In particular, we have fixed a number field F etc. Our claim was:

Choosing a Hodge theater is equivalent to choosing a once-punctured elliptic curve over F, up to equivalence of categories.

Proof of Claim: As the relevant elliptic curve does not have CM, $\mathrm{Aut}(X/F)=C_2$ is the cyclic group with 2 elements. Similarly, as you write, automorphisms of Hodge theaters are C_2. Moreover, both categories have precisely one object up to isomorphism. The induced map $C_2\to C_2$ can be checked to be nonzero, hence an isomorphism.

So you say that you gave 4 disproofs.

The first is a statement about $\pi_1(X)$ that does not seem relevant to the discussion — the question is about Hodge theaters vs. elliptic curves over F. I apologize that we did not explicitly say "over F" in the text.

The second is a statement about Hodge theaters that is a part of our proof. So actually, the correct version of the first (namely, automorphisms of X over F are C_2) and the second *prove* our claim.

The third is some vague statement. Regarding whether something "knows", for example, the global multiplicative subspace: The initial Θ-data are fixed throughout. Given an elliptic curve isomorphic to X, I can fix such an isomorphism, and import all structures that have been fixed for X, like the global multiplicative subspace. This may feel like cheating, but it is exactly what Mochizuki does say in Theorem 3.11 (i) as discussed previously.

The fourth is some vague statement. I don't really understand what you mean by "bi-interpretability" or "highly structured category"; certainly the functors in both directions can be made explicit, which seems to go some way towards "bi-interpretability". Note that Mochizuki goes to great pains to define Hodge theaters, including (the highly non-obvious) morphisms, so I naturally expect their category to be of relevance. (If you want to define some theory of diophantine equations over $\mathbb Z$, you should not spend your time defining the category of rings isomorphic to $\mathbb Z$! By the way, the ring $\mathbb Z$ does not have -1 as automorphism…) Of course, category equivalences can have power. But Mochizuki's central claim is that he chooses *different* Hodge theaters and that something that looks like 5 is more like 13 in the other, except that they are also the same, up to some ambiguity… . So you have to choose Hodge theaters in his proof. But for this very step, the category equivalence does tell you that you might as well choose elliptic curves isomorphic to your given one (and take the corresponding Hodge theaters). I'm fine with going to the other side when I need to. But precisely because maps of Hodge theaters are a highly non-obvious notion, making it easy to get confused about all sorts of maps, I find it easier psychologically to first choose the elliptic curves and then pass to the other side.

Best wishes,
Peter

PS: You claim that objects and objects up to isomorphism are conflated in our manuscript. Where do we do this?

125. **Peter Scholze**
April 29, 2020

Sorry, I should have more carefully proofread. The claim is of course

Choosing a Hodge theater is equivalent to choosing a once-punctured elliptic curve over F isomorphic to the given X, up to equivalence of categories.

(I omitted "isomorphic to the given X".)

126. **Taylor Dupuy**
April 29, 2020

>By the way, the ring $\mathbb{Z}$ does not have $−1$ as automorphism.

https://images.app.goo.gl/zfRudP7opfTU8dVU8

That being said, you can just take some equally stupid thing like $\mathbb{Z}$ as an abelian group. You don't get undecidability… but whatever. The point is you have something "nontrivial" equivalent to a category with one object and two morphisms.

I might give a more detailed response about the other stuff later. Right now, I hope that other people will start commenting about the Mathematics and that it leads to a productive discussion.

127. **Peter Scholze**
April 30, 2020

Dear Taylor,

I certainly understood your point there — you might also take the ring $\mathbb Z[\sqrt{-1}]$.

There is of course a big difference between the ring $\mathbb Z$ and the "theory" it defines, i.e. roughly the class of all subsets of all finite powers $\mathbb Z^n$ that are definable by polynomial equations. The latter is indeed a highly nontrivial category (where morphisms are given by definable graphs); it is of course not equivalent to a category with one object and two morphisms. If a category like this is in place in Mochizuki's work, I'm happy to hear about it!

Reading the IUT papers, however, you are presented with some extremely difficult notion of a Hodge theater, together with a highly non-obvious notion of isomorphisms of such: Isomorphisms do not preserve nearly as much structure as you would expect them to, and this is by design as Mochizuki points out. So I find it very hard to "guess" what something like a surrounding "theory" might be. For all I can see, Hodge theaters fit neither into the framework of "structures" as used in the wikipedia entry https://en.wikipedia.org/wiki/Interpretation_(model_theory) you linked to, nor the topos-theoretic framework of Caramello. (Regarding the first one: A "structure" in the sense of model theory has first of all an underlying set. I find it hard to take a Hodge theater and produce some interesting set that is functorial in isomorphisms of Hodge theaters, the problem being the very lax notion of isomorphisms of Hodge theaters.)

However, these long discussions are all about interpretations. Regarding the mathematics proper: I stand by the claim made in our manuscript, and have indicated the proof above.

Best wishes!
Peter

128. **Taylor Dupuy**
April 30, 2020

Hi Peter,

I think this discussion kills (1). More comments are below.

(Sorry if there are any mistakes or typos)

Best,
Taylor

#################
#################

>The first is a statement about $\pi_1(X)$ that does not seem relevant to the discussion. The question is about Hodge theaters vs. elliptic curves over F. I apologize that we did not explicitly say "over F" in the text.

So maybe I am misinterpreting you (math joke) but I thought your point was that because of absolute Grothendieck, $\pi_1(X)$ and X are "the same" and that you could just replace one with the other willy-nilly in any statement. I think you are saying that any proof with fundamental groups could be replaced by a proof with X's (whenever absolute Grothendieck holds).

Look at your comments April 6, 2020 at 9:28 am.
Look at the text following your claims on April 13, 2020 at 4:25 pm (this is the (1),(2),(3) post).

This seems to contradict your position now that X_F should be viewed as relative. I have a feeling say you want X_F to be absolute in one case and relative in the other (which seems to violate your own heuristic).

Remark. Before getting into the equivalence of categories stuff below let me stress again that I think this is a red herring as it is not what we should be talking about.

In either case if you want to X_F as an absolute scheme, you get to apply absolute grothendieck but don't have the equivalence. In the case that you view X_F as a relative scheme you get the equivalence of categories. The equivalence of categories is a red herring since it isn't the appropriate notion to use for reduction. In the relative case I agree that the associated groupoids of isomorphic Hodge Theaters, Isomorphic F-schemes, and isomorphic rings are all equivalent to a category with one object and two morphisms. We both agree this is a ridiculous category that contain little information (if all we are allowed to talk about are objects and morphisms). We can interpret nothing from this category. There are no q-pilots, no theta-pilots, no bogus diagrams, nothing. Hence, this must not be what is going on.

####################
####################
>For all I can see, Hodge theaters fit neither into the framework of "structures" as used in the wikipedia entry

We want sets. (as you said)

You might notice that I made the caveat of bi-interpretability being correct for things in IUT not considered up to automorphism. The way I currently think about it is in two phases.

In phase one you have your structures (I replace all the Frobenioids with pairs $(\Pi, \overline{M})$, fundamental groups by Π's etc), in this phase we do all this interpreting.

In phase two whenever we add the polyisomorphisms, I exchange that for an automorphisms and let those act on whatever it is that was interpreted.

Remark. Mochizuki's Species/Mutation stuff in IUT4 is his way of explaining this (poorly). I have not made it though that stuff and at this point I don't think I need it. My point is, I think one can write this all down carefully, but I haven't done so and I don't think Mochizuki has done so. There is an intereting discussion on ncatlab that discusses this stuff. It seems that Skoda alluded to what I am talking about now back in 2012!
https://nforum.ncatlab.org/discussion/4260/abc-conjecture/ read posts 13, 16-20, 37.

Remark. Hodge Theaters seem to be used more of a catch-all for all the stuff he will ever need. Every individual construction I have encountered always requires much less than the full Hodge Theater. I certainly haven't worked out what the signature of a Hodge Theater is but I do think this should be worked out. You will also see from the big tables

First orderizability is explained in the stack exchange post I mentioned earlier. I think you were a little confused about the context of that post but hopefully this is clear now.

>The third is some vague statement. Regarding whether something "knows", for example, the global multiplicative subspace: The initial theta-data are fixed throughout. Given an elliptic curve isomorphic to X, I can fix such an isomorphism, and import all structures that have been fixed for X, like the global multiplicative subspace. This may feel like cheating, but it is exactly what Mochizuki does say in Theorem 3.11 (i) as discussed previously.

I claim this is can be made precise. I view his claims as statements about "functorial algorithms" (which I view as interpretations). For whatever definition of a "phase 1 Hodge Theater" you take as described above, there exists a prime strip as a reduct (I regard coming up with a minimal signature for a "working" Hodge Theater as an open problem). From this prime strip you get a group isomorphic to $\pi_1^{temp}(\underline{\underline{X}}_{\underline{v}})$ as a reduct: view the Frobenioids their as pairs $(\Pi,\overline{M})$ and then forget the monoid. In your formulation there is just no "functorial algorithm" so it is not comparing apples to apples. You are asking for something entirely different from what Mochizuki is doing.

I said this about 3.11.i before but I will repeat because I think you will be more receptive now that we are on the same page with this equivalence of categories vs bi-interpretability thing. What Mochizuki does in 3.11.i is a "lift/construct/push".
You lift to a structure to a structure with more interpretive power that interprets it, you perform the desired interpretation, then you push it into a set that interpreted in your original structure. This construction is equivariant with respect to automorphisms of the lifted structure. You take the orbit of these (or in the case of ind3, you do something like "consider all possible lifts") and consider the interpretation only as existing only up to this indeterminacy. The result is the type of crap you get in 3.11.i where the interpretation up to automorphism is independent of the choice of lift.

Remark. David Roberts recently pointed me to this 5 year old note of Kim where he picked up on the significance of the global multiplicative subspace:
http://people.maths.ox.ac.uk/kimm/papers/pre-iutt.pdf
I was impressed by how early he picked it up.

>You claim that objects and objects up to isomorphism are conflated in our manuscript. Where do we do this?

So I think by allowing yourself to replace polyisomorphisms by fixed isomorphisms and then at the same time cite Propositions from Mochizuki this could get you in trouble. I was worried about this at least.

129. **W**
April 30, 2020

(1) The issue is not whether switching between X and pi_1(X) could ever under any circumstances induce an error, but whether switching between X and pi_1(X) could ever be the deciding factor in whether Mochizuki's proof is valid. The idea that the discrepancy between the automorphism group of X as an absolute scheme and the automorphism group of X as a relative scheme could be responsible for confusion as to whether the proof is correct (either someone thinking it's correct when it's not, or thinking it's incorrect when it is) is absurd.

For one, there are many fields F with no automorphisms that one could run the argument over, and the discrepancy vanishes there. In any case, Mochizuki's argument is not an argument which is based on using the automorphisms of a field F to some kind of strategic effect.

(2) It is not, I think, in dispute whether it is possible to define an object on a structure that has few automorphisms, then "push" to a structure with more automorphisms by taking the orbit under its automorphism group. If you have two constructions which are each naturally defined in a different level of structure, then doing this might be helpful to compare them. But the issue here is that everything in sight is most naturally defined on the same structure, that being the structure of a (punctured) elliptic curve.

So this raises the question of what could be the benefit of artificially forcing some natural object to be invariant under some random unrelated group of automorphisms. Of course it is possible to imagine benefits that might possibly occur in some conceivable argument, but one has to explain what the benefit actually is in Mochizuki's argument and where it occurs (e.g. commutativity of some diagram).

One fundamental issue is that invariance under a group action is very useful for proving identities, but not useful for proving inequalities, unless one proves an inequality by first deriving an identity and using a separate argument to convert that identity into an inequality. But there don't seem to be any useful identities that can be proved using the invariance under automorphisms of the Galois group, because automorphisms of the Galois group never show up in an essential way at any other point in the argument.

130. **Taylor Dupuy**
April 30, 2020

Will, I completely 100% totally agree with (1). I think (2) is a misunderstanding of what I am talking about. We are not killing automorphisms but considering completely different structures. Also, I don't think you understand the definition of a structure. It isn't a hand-wavy thing. It is a collection of functions, relations, constants, and sorts. I need to think about your last two paragraphs before I can comment more.

131. **LMR**
April 30, 2020

Here are some relevant nLab entries that define the model-theoretic terms that Taylor has used in his recent comments. I hope this can clear up some confusion.

Structure: https://ncatlab.org/nlab/show/structure+in+model+theory

Interpretation: https://ncatlab.org/nlab/show/interpretation

132. **Peter Scholze**
May 1, 2020

Dear Taylor,

thanks for your further comments. I think W said it all.

Let me just make the following clarification regarding (1). You claim that you disproved some claim that I made in my manuscript with Stix, or some claim that I made in the current thread. This is wrong. In this thread, I looked at certain curves X (of strictly Belyi type and defined over a p-adic field) and claimed

The abstract curve X is equivalent to the topological group up to inner automorphism $\pi_1(X)$.

In the manuscript with Stix, this claim is also made, along with a different claim. Namely, for some other curve X defined over a number field F (appearing in the initial Θ-data of Mochizuki)

The relative curve X/F is equivalent to a Hodge theater.

In both cases, "equivalent" is meant in the sense of equivalences of categories of the relevant objects. (To be precise: a) The category of schemes that are hyperbolic curves of strictly Belyi type over a p-adic field, with morphisms being isomorphisms of abstract schemes, is equivalent to the category of topological groups that are isomorphic to $\pi_1(X)$ for some such X, and morphisms being outer isomorphisms of topological groups. b) The category of curves over F isomorphic to the given X, with morphisms isomorphisms of curves over F, is equivalent to the category of Hodge theaters as defined by Mochizuki, with morphisms the ones defined by Mochizuki.) We agree that both claims are true; in fact, both are theorems of Mochizuki.

You seem to argue that there is some tension between these claims, as one talks about absolute vs. the other about relative curves. Well, also the right side is different, so no wonder the left side is!

Actually, in the context of the current thread, it would have been a good question whether, given that a Hodge theater is a collection of certain topological groups isomorphic to $\pi_1(X)$'s for certain different X's, plus other data, with lots of relations, can you actually interpret all those things as some convenient structure on the side of schemes again? This is not a priori clear, and Mochizuki's central claim is that by passing to $\pi_1(X)$'s, he can do interesting new things. So he cooks up a Hodge theater. And … you can beautifully interpret it in good old schemes, it "is" just a curve over your original number field F.

The same happens for everything else I've seen in IUT or your comments.

I'm happy to continue any further discussions by e-mail.

Best wishes!
Peter

Lucien Szpiro 1941-2020

April 18, 2020
Categories: Obituaries

I'm very sorry to hear (via Michael Harris) of the death this morning in Paris of Lucien Szpiro, of heart failure. Szpiro was a faculty member here at Columbia for a few years, livening up the place at a time when the department was smaller and quieter than it is now. He then went on to a position at the CUNY Graduate Center and was often at the department here for number theory related talks. The Graduate Center has a short bio of him here, and on his website you can find more about his work, including some very nice short and lucid lecture notes on arithmetic geometry (see here and here). Some pictures of him and other mathematicians at his 70th birthday conference in 2012 can be found here.

What Szpiro is probably most famous for is the "Szpiro Conjecture" about elliptic curves which he first formulated in 1981. This is essentially equivalent to the later abc conjecture that has been the topic of recent controversy, so we really should have been all this time arguing about Szpiro, not abc. In a 2007 blog post I put out the news that Szpiro had announced a proof of abc at a talk he gave at Columbia (at Dorian Goldfeld's 60th birthday conference). Alas, a flaw in that proof was quickly found.

Update: Something about Szpiro from Christian Peskine:

> Lucien Szpiro est décédé d'une crise cardiaque samedi 18 avril. Ceux qui l'ont bien connu souhaitent d'abord saluer un homme d'exception. Lucien était tout à la fois un solitaire, un collaborateur passionné et un patron aimé et respecté. Un homme solitaire, intransigeant sur sa liberté, sur ses choix et sur la considération qu'il attendait. Un collaborateur passionnément ouvert au partage des idées et des projets. Un leader entrainant ses amis dans des aventures scientifiques nouvelles et enrichissantes.
>
> Recruté au CNRS après un cours passage comme assistant à la faculté des sciences de Paris, il y est resté jusqu'à son départ à City University (New York) au début du siècle. Le séminaire qu'il a animé pendant de nombreuses années a été pour beaucoup de collègues de tous ages un lieu d'étude et de formation. Son influence et ses recherches ont fait honneur au CNRS. Ses nombreux élèves en témoigneront de leur coté. Il était heureux à New York ou il avait trouvé une forme de sérénité.
>
> Ayant collaboré intensément avec Lucien durant de nombreuses années, je comprends que je perds un ami avec qui j'ai partagé des moments d'une intensité et d'une beauté rares. Il aimait la vie, il aimait la science et il aimait la recherche mathématique.

Comments

1. **Dave**
   April 18, 2020

   English translation, according to Google Translate:

   Lucien Szpiro died of a heart attack on Saturday April 18. Those who have known him well would first like to greet an exceptional man. Lucien was at the same time a loner, a passionate collaborator and a loved and respected boss. A lonely man, uncompromising on his freedom, his choices and the consideration he expected. A collaborator passionately open to sharing ideas and projects. A leader taking his friends on new and enriching scientific adventures.

   Recruited at the CNRS after a short course as an assistant at the Faculty of Science in Paris, he remained there until his departure from City University (New York) at the beginning of the century. For many colleagues of all ages, the seminar which he has led for many years has been a place of study and training. His influence and research have brought honor to the CNRS. Its many students will testify on their side. He was happy in New York where he had found a form of serenity.

   Having collaborated intensely with Lucien for many years, I understand that I am losing a friend with whom I have shared moments of rare intensity and beauty. He loved life, he loved science and he loved mathematical research.

2. **Mohan Kumar Neithalath**
   April 19, 2020

   He was a great mentor for me and without his course at TIFR where I took notes, and all the time we discussed mathematics, I doubt I would have become a mathematician. I attended his 60th birthday in Paris and he was kind enough to attend mine in Kozhikode. Thank you for all the things you did for me and all the wonderful memories.

3. **DH**
   April 20, 2020

   That is sad news.

   This paper of Peskine and Szpiro is very beautiful (http://www.numdam.org/article/PMIHES_1973__42__47_0.pdf). I would argue that this work was the first to really demonstrate the Frobenius morphism as a tool of serious power and depth in commutative algebra, with relevance even to questions in characteristic zero. There is a direct line from this paper to the (ongoing) perfectoid revolution and its many amazing applications to pure commutative algebra, etc.

4. **Donu Arapura**
   April 20, 2020

   I'm sorry to hear that. I attended Spziro's seminars and class many years ago at Columbia. I really learned a lot from him. I still remember his advice on how to understand the Grothendieck dualizing complex: "It doesn't matter what it is, what matters is how it moves", or in other words, how it transforms.

5. **William Haboush**
   April 25, 2020

   As a friend of Lucien, I must say that no account of him is complete that does not include a reference to his love for cinema, for fine dining and for music and the opera. I accompanied him to these things often and I can attest to his love of ideas and the arts.
   He was a man of boundless integrity with a love of precision and an eye for the telling detail. I attended his seminars on Arakelov theory in the eighties at Columbia and on Number theory in NY (it was a joint CUNY Columbia NYU thing) when I was able to attend it. His taste was as impeccable as the precision of his grasp. He was totally dedicated to the life of the mind.
   I also wish to covey my consolations to his many students and colleagues and above all to his partner of his last years, Beth Pessum.

Why the Szpiro Conjecture is Still a Conjecture

April 18, 2020
Categories: abc Conjecture

There has been a remarkable discussion going on for the past couple weeks in the comment section of this blog posting, which gives a very clear picture of the problems with Mochizuki's claimed proof of the Szpiro conjecture. These problems were first explained in the 2018 Scholze-Stix document Why *abc* is still a conjecture.

In order to make this discussion more legible, and provide a form for it that can be consulted and distributed outside my blog software, I've put together an edited version of the discussion. I'll update this document if the discussion continues, but it seemed to me to now be winding down.

Depending on one's background, one will be able to get less or more out of trying to follow this discussion, but it seems to me that it makes an overwhelmingly convincing case that Mochizuki's articles do not contain a proof of the conjecture and should not be published by PRIMS. No one involved in the discussion claims that there is an understandable and convincing proof in the articles. The discussion is rather about Scholze's argument that there is no way that the kind of thing Mochizuki is doing can possibly work. While Scholze may not have a fully rigorous, loophole-free argument (and given the ambiguous nature of many of Mochizuki's claims, this may not be possible) the burden is not on him to do this.

To justify the PRIMS decision to publish the proof, one needs to assume that the referees have some understood and convincing counterargument to that of Scholze, one that nobody has made publicly anywhere. If this really is the case, the editors of PRIMS need to make public these counterarguments, and those mathematicians who find them convincing need to be able to explain them.

A note on comments: if someone has further technical comments on the mathematical issues being discussed at the earlier posting, they should be submitted there. For discussion of issues surrounding publication of the claimed Mochizuki proof, this would be the right place (and I've moved a couple recent ones to here). For comments about Szpiro and his conjecture, the posting about him would be an appropriate one.

Update: I hear that the editors of PRIMS are aware of the recent discussion of the problems with the Mochizuki proof, but have decided to go ahead with the publication of the proof anyway. They do not seem to intend to release any information about their editorial process, in particular what counter-arguments to Scholze's they considered. In effect, they are taking the stand that they have convincing evidence that Scholze is wrong about the mathematics here, but cannot make it public for confidentiality reasons.

Note that the discussion in the comment thread itself has some later entries after the ones gathered in the pdf document I created.

Comments

1. **naf**
   April 18, 2020

   This is not in response to any particular comments but to the overall discussion.

   It seems to me that there is a psychological issue at play here which might have not been made explicit. This is that for anyone who has devoted enough time to understand the basic setup and language of Mochizuki's papers, it is very hard to accept that in the end there is no proof and one has wasted a huge amount of time. I myself devoted most of three months way back in 2013 to going through the papers and it was a shock, on reaching Corollary 3.12, to realise that nothing is really proved. (One might say that this shows my lack of understanding, but the point is that Mochizuki himself says that this is the key part of his proof and if you see how the proof is written it is clear that there is something seriously wrong here.)

   Given the time that has passed since Mochizuki's manuscripts appeared and the objections that have been raised by many experts, the obvious interpretation of the lack of any serious revision by Mochizuki (going by Occam's Razor) is that there is actually nothing that readers have missed, it's simply that there is no proof. However, because of Mochizuki's past achievements, this is hard to accept (it was for me), perhaps especially for those who know him personally. This seems to have led to some people assuming that it is because of their lack of understanding of what Mochizuki "really means" and so devoting a lot more time to IUTT. Once one has invested too much time, I think it becomes psychologically almost impossible to give up hope, and the lack of precision in the papers sadly ensures that there is no simple statement in them which can be shown to be wrong and make it impossible to maintain faith.

2. **lbj**
   April 18, 2020

   Now that Scholze has explained what is going on with Mochizuki's manuscripts, it's time for other arithmetic geometry experts to revisit the papers and weigh in. Those of us in the field know that almost all the experts definitively side with Scholze-Stix, so it's frustrating that the popular articles (which most people base their opinion on!) depict the situation as if it were much more ambiguous, because experts are afraid of taking a stand. And when mathematicians do get themselves quoted, it's with painfully noncommittal lines like "I will withhold my judgement on the publication of this work until it actually happens, as new information might emerge" and "In spite of all the difficulties over the years, I still think it would be great if Mochizuki's ideas turned out to be correct". The purpose of granting tenure was so that academics could speak honestly, rather than hide behind these sorts of vague equivocations. With increasing resources being allocated to IUT, it's clear that the situation will only become more and more damaging to mathematics at large.

3. **NS**
   April 18, 2020

   One reason for the situation in the press that Ibj is frustrated with is simply that the publication hasn't publicly appeared. Experts are rightly hesitant to say something definitive about a paper that they haven't seen. If it turns out that PRIMS publishes a paper that, as is widely expected, just leaves the existing gap in the proof of Cor 3.12 essentially unchanged, then we'd see some more forceful statements from experts. Of course, by then the popular press will have moved on.

   Of course this means holding a public press conference without releasing the manuscript is pretty shocking and irresponsible in this context. The article I'd like to read is what on earth explains the willingness of these other great mathematicians at RIMS to go along with this charade.

4. **OP**
   April 18, 2020

   The comment of @naf hits the nail on the head. But I would go a step further: when confronted with a truly massive edifice of highly technical mathematics, nearly all experts need some kind of motivation to persevere beyond the final goal at the end of the tunnel. For example, a powerful heuristic to give confidence in the strategy, or some kind of intuitive guide to grab onto along the way to have a feeling of making progress (or at least interesting achievements along the way in the absence of a global guide). But here there is nothing of the sort, not even a compelling mathematical reason to believe at the outset that investing a huge amount of time is going to reap satisfying mathematical understanding. There is only patience to keep oneself going, and it can be very hard to rely on that alone after a lot of time.

This practical (albeit psychological) concern came to mind almost immediately after I was asked early on to be on the referee team for the IUT papers. I have great respect for Mochizuki's mathematical talent, and no doubt in the sincerity of his belief that he has a proof of the main result. But I could see that the referees would not only have to check the details of an extremely long work written in a very obscure style (which didn't provide insightful reasons for confidence in the approach being used). They would also have to engage in a herculean effort to get the writing substantially changed. It was too much, so I declined and communicated my concerns to the editorial board. (I recommended immediate rejection with a demand that the work be completely rewritten before it could be reconsidered.)

I am very sorry to see all these years later that neither the referees who were eventually obtained nor the editorial board obtained any real improvement on the clarity of the way the material is presented, not even at least an Introduction presenting key new insights in some conventional manner (to compensate for the way the technical material is presented). I hope the editors of PRIMS and the senior faculty at RIMS will reflect on their responsibility to the field of mathematics, and reconsider what they are doing.

5. **derek**
April 18, 2020

Professor Woit: minor typo – 2108 should be 2018

"These problems were first explained in the 2108 Scholze-Stix document Why abc is still a conjecture. "

6. **Peter Woit**
April 18, 2020

derek,
Thanks! Fixed.

7. **David Roberts**
April 18, 2020

Thanks, OP. I wish the other referees would be so forthcoming, if it were indeed possible. Or if not, the journal could release their reports.

I note that Nick Katz was public about his refereeing of Wiles' first FLT paper, and his finding of the mistake, and it's public knowledge Gábor Fejes Tóth was one of the referees for Hales' Kepler conjecture paper that appeared in the Annals with a disclaimer. This is an extraordinary case, and warrants extraordinary actions, IHMO.

8. **David Roberts**
April 18, 2020

@Peter W – I notice you link, in your pdf, to my Inference essay, but not the technical note I pointed you to. I'd prefer the technical note is used, not the essay, which was written for the general public and much below the level of the comments here. In any case, I should have given you this link: https://doi.org/10.25909/5c5ce1fda4b7c instead of the one I originally sent.

9. **Peter Woit**
April 18, 2020

David Roberts,
Thanks for pointing this out! Fixed.

10. **OP**
April 18, 2020

@David Roberts:

Just to clarify: I wasn't a referee on the IUT papers, but rather was invited to serve as one, and I declined (giving the editorial board my recommendation for how I thought would be best to proceed). As for Katz' work as a referee on the initial FLT paper, my impression is that this only became public knowledge sometime after the fix was found and the corrected version had gone through the review process (e.g., maybe via the BBC video that was made about it).

11. **David Roberts**
April 18, 2020

@OP sure. By 'other referees' I mean the ones that accepted the job.

12. **KS**
April 20, 2020

For more sociological understanding , please let me put about some curious social situation around this issue in Japan.
On the official announcement of acceptance of the papers in Kyoto, two mathematicians Tamagawa and Kashiwara attended there, and Tamagawa is actually well-known expert in the anabelian geometry.But It seems likely that he can not explain about the mathematical matter of IUT in public so far.
In addition , Rigid geometer Fumiharu Kato already published the book "in 2019" about IUT "treated as correct theory" for general audience amazingly, but actually it wouldn't be sufficient to offer any insight for professional mathematicians.Furthermore , as Tamagawa is , he also has never wrote any rigorous mathematical papers about IUT as far as I know at present.
These facts seems to be indeed mysterious that is , " whoever did understand and can defense the theory sufficiently?" .

Anyway needless to say , since IUTT use too many and overlevel terminology ,so that if it cannot be expressed with crucial idea for proof , other mathematicians wouldn't accept it at all.

13. **Jeff Berkowitz**
April 20, 2020

Peter, I wonder if you could provide a little historical context for the non-mathematician. Are situations like this one common in the modern history of mathematics? Occasional? Rare? Unprecedented? Is there another situation you can compare this to?

14. **anon**
April 21, 2020

About the situation in Japan: do Japanese mathematicians in general accept the "proof" as it is? Or is it just (at least most) mathematicians at RIMS?

15. **Peter Woit**
April 21, 2020

Jeff Berkowitz,
This is unprecedented. I know of no other example in the history of the field of a reputable journal publishing a proof of a major result over the objections of experts in the field who have publicly argued that the proof is flawed, even that it cannot possibly work.

Very few Japanese experts in this field have expressed publicly an opinion that the proof is correct. I hear (second hand) that many of them privately say they are embarrassed by this situation. It would be helpful if they would say this publicly.

16. **Jay Watt**
April 21, 2020

I have been following this drama for quite a while now and there're a couple of things I just don't get:

1. Why do people assume that the referees (if there were any) really understood the papers and should thus come out and explain it? Most of you have been refereeing papers yourself. Do you read and check every line? That's impossible, and I'm open about this in every report I write. Even if you do think you got it all, does your judgment make the paper correct? We're all just humans and prone to make errors. As an amusing reminder, here's an (incomplete) list of "incomplete proofs": https://en.wikipedia.org/wiki/List_of_incomplete_proofs.

2. What's the matter with statements like the following?
* "well-known to every one at RIMS" (K. Joshi)
* "these German mathematicians" + many more (I. Fesenko)
* "a deep sense of discomfort, or unfamiliarity, with new ways of thinking about familiar mathematical objects" + many more (S. Mochizuki)

Did you all lose your minds? What is this?

3. The papers by Mochizuki consist of building up tons of highly ambiguous language, and when it comes to a proof, it's all obvious from the definitions. Why does anyone actually take this serious? What's comical is that Mochizuki himself complains in his pamphlet that "negative positions concerning IUTch were always discussed in highly non-mathematical terms, i.e., by focusing on various aspects of the situation that were quite far removed from any sort of detailed, well-defined, mathematically substantive content." Yeah, sorry, but how are we supposed to talk about this thing then?

4. Scholze and Stix tried to make some sense out of this and pointed out a specific part they don't understand; see also the comments above. There has been no reply except comments along the lines of point 2 above. So, what? Do you want to force them with a baseball bat to understand it? What are they supposed to do? What do you want?

Mochizuki: If you can't explain your stuff, no one will care, period.

17. **Peter Woit**
April 21, 2020

Jay Watt,
For many of the things you are not getting, I think most mathematicians feel the same way. About point 1 though (referees):

First of all, the standard of refereeing expected for a claimed proof of this kind is much higher than normal. But the reason for demanding an explanation from the editors and referees is that two years ago they were presented by Scholze and Stix with a strong argument about a specific flaw in this proof, as well as the information that Scholze and Stix spent a week discussing this flaw with the author and left convinced the author had no answer for how to fix the proof. In response to the problem identified by Scholze/Stix, the author seems to have made no substantive changes to the argument to fix it (Scholze says the problem is still there).

What's incomprehensible here is that the editors decided to publish the proof anyway, and even held a press conference to promote this. The editors and referees involved in this have a very specific responsibility to the math community that they are not meeting: they need to produce a convincing explanation for why Scholze/Stix are wrong (presumably they have this from the referees, otherwise they would not have decided to publish). Absent that, what we have here is a strong argument that the proof is wrong, met by a press conference saying the editors disagree, but will not say why.

18. **Timothy Chow**
April 21, 2020

Regarding whether this an unprecedented situation, I agree that there are many ways in which it is unprecedented. However, I would say that it is far from unprecedented for a flawed paper (even one solving a major open problem) to be published even when experts have seen a preprint and believe it to be at best incomprehensible. If an author has a strong reputation then mathematicians are typically very leery of saying that a paper is wrong unless they can point to a provably false statement. They have been socialized all their lives that it is taboo to say something definitive unless they can back it up with a rigorous mathematical proof. So if a paper is not even wrong then mathematicians will dance around the issue and generally refrain from delivering a verdict that it should not be published, unless they have formally accepted the task of refereeing the paper. Note, for instance, that Taylor Dupuy has said that he does not think that the Scholze–Stix manuscript is a sufficient reason for rejecting Mochizuki's proof, and he wants to see the final published version before making a final judgment. In general (i.e., setting aside the specifics of the IUTT papers), this is a typical attitude, and leads to quite a number of dubious papers by famous people getting published even though experts know that there are serious and perhaps fatal problems with them.

I think that what's most unprecedented about the current situation is that two mathematicians, who are not referees, have gone public with a major criticism prior to the publication of the article. This is the really unusual part of the story, and it might not have played out this way had there not been a false rumor a couple of years ago about the imminent publication of the paper. Had this rumor not been circulated, and had Mochizuki's papers simply appeared in PRIMS without a prior public announcement, then the situation would not be that different from, say, Hsiang's claimed proof of the Kepler conjecture and the ensuing controversy.

19. **MZ**
April 21, 2020

Jay Watt:

> There has been no reply except comments along the lines of point 2 above.

Not sure this is accurate. The M remark you quote was mentioned in a 2018 Quanta piece—perhaps that is where you are getting it from? Its original source is (apparently) the 2018 report by M on the discussions he had by Scholze & Stix. This report has comments way beyond what you quote in terms of mathematical content.

Also, reading the comments from people who actually interacted with him, it seems that M does engage into detailed mathematical discussions with people who contact him about his work. This way of representing his responses as no-math-content, dismissive commentary seems inaccurate.

As for point-by-point factual summaries of the state of the problem, this [description by D. Roberts] seems to have a more neutral/objective wording than some of the summaries given during the current comment thread:

> In documents released in September 2018, Scholze–Stix claimed the key Lemma~3.12 of Mochizuki's third Inter-Universal Teichmüller Theory (IUTT) paper reduced to a trivial inequality under certain harmless simplifications, invalidating the claimed proof. [. . .] Mochizuki agreed with the conclusion that under the given simplifications the result became trivial, but not that the simplifications were harmless. However, Scholze and Stix were not convinced by the arguments as to why their simplifications drastically altered the theory, and we stand at an impasse.

20. **Peter**
April 21, 2020

Timothy Chow,

I think you're making an interesting point, but the question then becomes: when is a proof "published", i.e. "made public"?
You write "two mathematicians, who are not referees, have gone public with a major criticism prior to the publication of the article." But Peter Scholze is always careful to point out that he's analyzing what is public now, and not the publication in PRIMS. And what is public now, is entirely the responsibility of Mochizuki. This unprecedented situation is created by him, not by these two mathematicians.

21. **Peter Woit**
April 21, 2020

Timothy Chow,
The whole story here is very unusual. Yes, in an alternate universe where Ivan Fesenko hadn't distributed an email claiming the IUT papers had been accepted for publication, which caused me and others to blog about the publication news , which caused Peter Scholze to speak up publicly about the problem, much of this would have taken place only privately before publication, and the public mess would have been, like in the Hsiang case, post-publication.

But that's not what happened, and one reason that it's not what happened is that, unlike Hsiang's proof, the Mochizuki proof received a huge amount of attention from experts from 2012 on, leading most of them to conclude that the proof was incomprehensible. As it became increasingly clear that experts in the subject could not get a comprehensible explanation from Mochizuki (or those around him who claimed to understand the proof), many of them became worried that the journal would publish the proof despite its incomprehensiblity and were debating what to do about this.

It should be emphasized again that at the time of the abortive announcement, what experts were upset about was the unusual prospect of publication of an incomprehensible claimed proof, but what has happened now is much worse than that: publication of a proof the community is convinced is flawed, by editors who are well aware of this.

I don't know the story of the refereeing of the Hsiang paper, but find it hard to believe that editors of the paper were aware of any privately expressed opinion by experts that that the proof wasn't complete and decided to publish anyway. In any case, I don't think comparison to the Hsiang/Kepler conjecture story is very apt. For one thing, the Hsiang proof was not based on claims of a completely new (as in Mochizuki's IUT) approach to the problem. He was following a well-known line of attack. Note that in the skeptical Mathscinet review of the Hsiang paper, you can read

"I think there is hope that Hsiang's strategy will work: at least the main inequalities seem to hold. As far as details are concerned, my opinion is that many of the key statements have no acceptable proofs. Typically, we are given arguments such as "the most critical case is…" followed by a statement that "the same method will imply the general case". The problem with arguments of this kind is not only that they require the reader to redo some pages of calculations, but, notoriously, that they occur at places where we expect difficulties and most frequently it is impossible to see how the same method works in the general case."

In the Mochizuki case, Scholze's claim is not that important details haven't been worked out, but that the whole approach is fundamentally flawed. By the way, if you read the exchange with Scholze you will see that Dupuy does not at all claim there is a proof in the Mochizuki papers, he is just trying to argue the Scholze has not yet rigorously shown that Mochizuki's approach can't possibly work.

22. **Peter Woit**
April 21, 2020

MZ,
I don't think the Roberts quote gives an accurate picture of the argument between Scholze and Mochizuki. I strongly recommend reading Scholze's summary of the problem here

https://www.math.columbia.edu/~woit/wordpress/?p=11709&cpage=1#comment-235940

and if you don't find it convincing, read the entire discussion, linked to in this posting.

What Scholze is claiming is not that "if you simplify the proof it won't work", but that there is a fundamental mathematical issue of principle for why nothing like this kind of proof can work. He explains this issue clearly in the second and third paragraphs of his comment. The later discussion is all about whether these paragraphs give a rigorous argument, or whether maybe Mochizuki has some way (which no one else has been able to understand) around the argument.

23. **Jay Watt**
April 21, 2020

Peter,

you are quite right about what you said about my point 1 (referees). Let me say I'm more surprised by the fact, it seems, that people indeed expect this may be helpful and would lead to anything. If a referee could actually clarify all/some of the issues (or just the specific one raised by Scholze&Stix), then why not do it here, anonymously? I think they can't, and any attempt would likely go along the lines "Well, Mochizuki probably meant (some vague statement)", as all the other discussions went so far. All this is not getting anywhere. It's the nature of the papers and the "theory" that makes it impossible to deal with on a rigorous and verifiable basis. It's an eel.

@MZ: The "deep sense of discomfort" quote is by M, at the end of http://www.kurims.kyoto-u.ac.jp/~motizuki/Rpt2018.pdf. But this whole thing: why all these philosophical meta phrases? Why not focus on the subject? What is this all about?

24. **Timothy Chow**
April 21, 2020

Peter Woit: I agree that there are disanalogies with the Hsiang case. But I do want to point out that it is actually disturbingly common for mathematical papers by well-known people, claiming to solve important problems, to be published even when the editors have received private indications from experts that the papers have serious gaps, or even that the entire approach is fundamentally flawed. It's usually not talked about openly because unless the author admits that there is an irreparable gap, you'll make enemies by making such a claim. I also don't know what happened behind the scenes with the publication of Hsiang's paper, but if the editors privately knew that other experts had serious reservations and decided to publish anyway, it wouldn't have been the first time that that sort of thing happened.

If Mochizuki's papers had been published in, say, 2016, after "only" four years, I don't think the situation would be quite so unprecedented. But yes, the current circumstances are unprecedented.

25. **Timothy Chow**
April 21, 2020

By the way, just now I looked again at Hales's 1994 Intelligencer article about Hsiang's proof. He makes it clear that he presented Hsiang with serious objections several times in the interim between 1990, when Hsiang announced his proof, and 1993, when Hsiang's proof was published. So it seems likely that the editor of the journal was aware, prior to publication, that other experts in the field found the proof unacceptable.

26. **Peter Woit**
April 21, 2020

Jay Watt,
I also find it highly unlikely that any of the referees or anyone the editors consulted post Scholze/Stix had a convincing argument for how the Mochizuki proof overcomes their objections. I do think it's important that they be confronted with a demand to produce such an argument. If it exists, they can vindicate themselves by making it publicly available. If it doesn't, they have two choices: change their decision about publication or double down on unethical behavior, claiming "there is an argument but we have to keep it secret".

27. **anon**
April 22, 2020

I agree with Timothy Chow that incorrect proofs being published, even when concerns have been privately discussed with the editors, isn't _that_ unusual. Also, in mathematics retraction of (well-known to be) faulty papers seems even more difficult than in other disciplines

Of course, the IUT saga has been very unusual or even unique in mathematics because of the huge amount of publicity it has received, and the behaviour of some people surrounding the case. It's definitely strange that besides the author there are several people who vouch (even very, very, fervently) for the proof, yet are completely unable to explain it in any meaningful way.

28. **mgflax**
April 27, 2020

math_jin@ retweeted a report that the papers to be published in P(RIMS) are essentially identical to the versions on M's website, with the exception of a single footnote on the first page.

29. **Nick Maiorino**
April 28, 2020

Hi Peter Woit,

Yesterday, David Roberts on his blog "theHigherGeometer" reported that papers relevant to Mochizuki's Corollary 3.12 are finally beginning to appear on Taylor Dupuy's homepage. See "Dupuy and Hilado's work on unravelling Mochizuki":

https://thehighergeometer.wordpress.com/2020/04/27/dupuy-and-hilados-work-on-unravelling-mochizuki/

30. **JE**
April 30, 2020

The same twitter account mentioned above by mgflax (which may or may not be a source of reliable information) retweeted another message a few days ago identifying one specific mathematician at RIMS who has apparently gone through Mochizuki's papers and is reported as not being convinced of the validity of the proof.

Although this may only count as anecdotal evidence of internal dissent (were it true), it would suggest that not everyone at RIMS fully endorses the belief that the Szpiro conjecture will become a theorem when the papers get published by PRIMS.

31. **Peter Woit**
April 30, 2020

JE,
At this point it's a factual matter that no one other than Mochizuki has come forward publicly with an argument explaining why the proof is correct, despite Scholze's arguments. I assume that those mathematicians at RIMS with any interest in the subject are well aware of this situation, and many if not most are privately drawing the obvious conclusions.

32. **David Roberts**
April 30, 2020

@JE is this 'specific mathematician' Teruhisa Koshikawa?

33. **Peter Woit**
April 30, 2020

David Roberts,
I assume JE is referring to this tweet
https://twitter.com/MugaShohou/status/1253341200054054912

Besides Mochizuki, the others listed there as understanding the proof are Hoshi, Yamashita and Saidi. As far as I know, the documents written by Hoshi (in Japanese) and Yamashita (in English) do not address the problems that Scholze raises, and I am not aware of anything on the topic written by Saidi.

34. **David Roberts**
April 30, 2020

@Peter, that was my guess, too. (I seriously wish that Hoshi's notes were in English, as his other work seems to be very reasonable, as far as I can tell.)

35. **JE**
May 1, 2020

@Peter and David,
Yes, that's the tweet. Peter, you're probably right about the uneasy situation created for some mathematicians at RIMS, especially after Kashiwara's and Tamagawa's appearance at the press conference held in early April to announce the publication, which could have been interpreted as a clear sign of support for Mochizuki's proof from the institution as a whole (Kashiwara was presented as head of the team that examined professor Mochizuki's theory, see e.g. https://www.japantimes.co.jp/news/2020/04/04/national/japanese-mathematician-shinichi-mochizuki/#.XqvfxGgzaUk). As things currently stand there, silence can be eloquent. We already knew that Hoshi, Yamashita and Saidi endorsed the proof, but the fact that one mathematician at RIMS has been publicly reported as not being convinced of it (at this point of the discussion) may be even more eloquent.

36. **David Roberts**
May 3, 2020

Hi Peter, I think it worth including *in the pdf document* a mention that the discussion continued, and give a link back. You mentioned that there was content before the first comment you included, so why not also say the discussion continued? Not everyone reading that document will come to it by a link from your blog post where you mention this. I say this merely from the point of view of having a coherent scholarly record (whatever one's view of the various positions).

37. **Peter Woit**
May 3, 2020

David Roberts,
I was planning on updating the pdf document to a final form, didn't get around to it today until just now. It's a beautiful day here in New York, and earlier a long bike ride seemed more pressing than that particular task.

38. **Timothy Chow**
May 5, 2020

Peter Woit, I noticed that in the PDF document you say that the PRIMS editors have decided that the Mochizuki proof should be deemed correct and complete. Are those the editors' precise words, or is that an inference you're making from their decision to go ahead and publish the proof?

39. **Peter Woit**
May 5, 2020

Timothy Chow,
That's my inference from their decision to publish (reiterated after consideration of the latest discussion with Scholze). I suppose it's possible that the PRIMS editors have an unusual understanding of the role of a mathematics journal and have decided to publish a proof they know to be possibly incorrect or incomplete. If so, perhaps at publication they will include some sort of text explaining such an unusual choice.

Various and Sundry

April 30, 2020
Categories: Uncategorized

- If like most other people you're stuck at home, and having trouble concentrating on the projects you thought the current situation would cause you to finally find the time to complete, one thing you could do is watch a lot of talks about mathematics online. As far as I can tell, mathematicians are doing much better than any other field right now in dealing with this, since they have a wonderful site developed at MIT called mathseminars.org. It contains a fairly comprehensive set of listings of Math seminars now being run online.

  If there's anything similar in the physics community, I'd be interested to hear about it.

- A new book about the problems of fundamental physics has recently appeared, David Lindley's The Dream Universe: How Fundamental Physics Lost its Way. I've been thinking for a while about whether to write about it here, have held off mainly because I felt I didn't have much interesting to say. Today I see that Sabine Hossenfelder has written a review of the book which I mostly agree with, so you should read what she has to say.

  There are a couple places where I significantly disagree with her. For one thing, unlike Hossenfelder, I'm a great fan of Lindley's much earlier book on this topic, his 1993 The End of Physics. This was written a very long time ago, at a time when writing for the public about fundamental physics was uniformly positive about the glories of string theory. Unlike all those books, this one has held up well. Reading it at the time it came out, it was remarkable to me to find someone else seeing the same problems with the field that seemed to me obvious, providing a very helpful indication that "no, I'm not crazy, there really is something wrong going on here."

  It's interesting to read Hossenfelder's take on the way Lindley makes "mathematical abstraction" the villain in this story:

  > The problem in modern physics is not the abundance of mathematical abstraction per se, but that physicists have forgotten mathematical abstraction is a means to an end, not an end unto itself. They may have lost sight of the goal, alright, but that doesn't mean the goal has ceased existing.

  Here is where I definitely part company with Lindley, and to some extent with Hossenfelder. The current problems with fundamental physics have nothing to do with mathematical abstraction, but with the refusal to give up on bad physical ideas that don't work. Thirty-six years ago Witten and many other leaders of the field fell in love not with a mathematical abstraction, but with a bad physical idea: replace fundamental particles with fundamental strings. One reason they fell in love with this idea was that it could be fit together with two other bad ideas they had been dallying with at the time, that there are new forces mixing leptons and quarks (GUTs), and that you can relate bosons and fermions with the square root of translation symmetry (SUSY).

  Unfortunately it seems to me that many theorists have now drawn the wrong conclusion from the sorry story of the last forty-some years, deciding that what they need to do is to stay away from unwholesome mathematics, and stick to the wholesome experimentally observable and testable. But what if the underlying reason you got in a bad relationship with a seriously flawed love interest was that there weren't (and aren't) any experimentally testable ones to be found? Maybe what you need to do is to work on yourself and why you stay in bad relationships: the mathematically abstract love of your life might still be out there.

- Witten yesterday posted a definitely not mathematically abstract paper on the arXiv, Searching for a Black Hole in the Outer Solar System. It's basically a proposal for finding a physical black hole we could then go and get into a relationship with. I can't help thinking the probabilities are that getting into a healthy relationship with a new mathematical abstraction is more likely to work out than this.

Comments

1. **Davide Castelvecchi**
   April 30, 2020

   https://virtualscienceforum.org/#/long_range_colloquium

2. **clayton**
   April 30, 2020

   for particle pheno talks, https://sites.google.com/site/lhcresultsforumtalks/ seems to be pretty thorough

3. **Sabine Hossenfelder**
   May 1, 2020

   Hi Peter,

   Thanks for the link. You quoted the one paragraph with a typo! Can you make the "end to a means" a "means to an end"? I didn't read the "End of Physics" when it came out but only a few years ago, which may have had an influence on my impression. Yours and Lee's and Jim Baggott's books are all considerably more informative and provide more solid arguments. Lindley's in comparison is rather superficial. But I admit it's somewhat unfair to compare his to books that appeared much later. Best,

   Sabine

4. **Peter Woit**
   May 1, 2020

   Hi Sabine,
   Fixed.

5. **Alessandro Strumia**
   May 1, 2020

   This site aggregates calendars of known physics webinars:
   https://sites.google.com/site/onlinephysicsseminars
   If something is missing, contact organisers (indico or google format only). Furthermore, physics is going to be included here (https://researchseminars.org) and InSpire is starting an analogous initiative. However these do not aggregate automatically.

6. **Jamie Sully**
   May 1, 2020

Hi Peter,
The ‘mathseminars.org’ website has now been expanded to include all of physics and mathematics. It is officially live, as of this afternoon, at:

http://researchseminars.org

The site has good coverage of talks in high-energy theory, but we hope that word will spread quickly to the rest of physics.

Best,
Jamie

7. **Armin**
May 1, 2020

Peter,

Is it possible that your conception of what has gone wrong in terms of “bad physical ideas” is just another way of framing Sabine’s conception in terms of “forgetting that mathematics is a means to an end”?

After all,

1. most of mathematics is not at this time relevant to physics, and is not expected to be.
2. This implies there are plenty of opportunities for physicists to be inspired by pieces of math which entail “bad physical ideas”.
3. But then, falling in love with, and consequently overlooking any red flags as one develops those ideas seems pretty accurately describable as Sabine does.

8. **Peter Woit**
May 1, 2020

Armin,
Mathematics is the language of physics, so you can blame any bad idea on “mathematics”. My disagreement with Hossenfelder is that I dislike her use of that framing of the problem, when what she mainly has in mind are certain specific failed ideas, which I think are more properly thought of as failed physical ideas, nothing to do with the fact that they are formulated in certain mathematical terms.

There is and always has been a strong tradition among physicists of skepticism about use of any type of mathematics beyond what they were trained in. If you go back to the early days of quantum mechanics you can find people complaining about matrices, a little later complaining about group theory (the “gruppenpest”). The failure of string theory has led such people to blame this failure on the use of such mathematics. But what if, absent help from experiment, what is needed for progress is a new idea coming out of mathematics, not out of experiment? Fundamental theoretical physics may be in the process of turning against exactly the tools it needs to get anywhere.

There’s a huge universe of mathematical ideas, most of which won’t be helpful in finding new physics. Those skeptical of the value of abstract math for physics have plenty of good examples to point to where mathematical structures are being invoked that are not of any help in physics. My reading of the history that I’ve seen over the course of my career though is not that the main problem is people getting lost down blind alleys of mathematics, but getting lost down blind alleys of bad ideas about physics. As an example, consider the multiverse. Claiming to explain things with “everything happens” is an awful idea for how to do physics and how to do science, and that’s the problem with it, not whatever mathematical language you choose to dress it up in.

Or, put differently, the problem isn’t with people falling in love with ideas because they find the math attractive. This may be our best hope for new ideas. The problem is not giving up on one’s ideas when they fail.

9. **Armin**
May 2, 2020

Thank you, Peter. I think I understand your perspective better now.

A propos history of physics: One richly informative website with accounts literally from the mouths of those physicists who lived it is https://www.webofstories.com/

10. **Bernhard**
May 2, 2020

Is it me or is that black hole article a bit at odds to Witten’s typical interests?

11. **Peter Woit**
May 2, 2020

Bernhard,
Yes, although Witten has always had wide interests, and his interests fundamentally are more about physics than mathematics. If you want to find a reason for him getting interested in this particular topic, it’s hard to overemphasize how central a topic black holes have become to theorists. Also, Witten has been involved with the Breakthrough Prize organization, which is a Milner/Zuckerberg operation, and they’re the ones behind the Breakthrough Starshot idea.

12. **DB**
May 3, 2020

Bernhard and Peter,
there are a lot of comments on different internet forums that state that Witten has basically given up on trying to find a TOE, and now has focused his attention more on the famous It from Qubit idea.
But he’s not the only one. If you look closely, many top theoretical physicists are directing their research and energies towards the high-tech world. His involvement with the Breakthrough Starshot idea is one of the clues.

13. **Peter Woit**
May 3, 2020

DB,
I don’t think it’s Witten in particular who has given up on trying to find a TOE, this is the attitude of most influential particle theorists. Among string theorists, the majority opinion now appears to be that the “landscape of string vacua” that predicts nothing at all is an indefensible position at the end of a blind alley, and that continued focus on string theory requires other arguments.

14. **DB**

May 3, 2020

Peter,
thanks for the info.
For many of us, it was pretty clear a long time ago that pursuing the search of a final TOE was a complete waste of time, for the simple reason that… it does not exist.
Witten probably saw the light a couple of years ago, as proved in the interview he gave to Graham Farmelo in Princeton. He accepted the landscape idea, which is the dominant position nowadays.
Only a couple of fanatic radical defenders of string theory being the TOE carry on saying that ST will come up with the final, unique solution to the Laws of Nature.
I happen to know a couple of them, one from a small country in Central Europe and the other from the Sates.

I don't know what other arguments string theorists are thinking of focusing on and pursuing, but I'll be very interested if you could tell me what they are.

Thanks!!

15. **Peter Woit**
May 3, 2020

DB,
I don't think that it it's clear that a TOE doesn't exist, although it is clear string theory as currently understood cannot give you one. String theory defenders have the following options:

1. Claim that string theory has succeeded, but because of the landscape can't predict anything. The problem with this is that most other physicists will not take you seriously. At one point I linked here to discussion at a Simons Center talk where Vafa explained just how damaging this has been to the perception of string theory among the public and among other scientists.

2. Claim that there is a problem with the landscape, that string theory as currently understood may be able to make some sort of prediction of something. This is the "swampland" program favored by Vafa. For reasons explained in many places here I don't think this will work. I see no evidence this point of view is getting much traction in the physics community.

3. Claim that the problem is that string theory is too poorly understood to be able to tell if it makes predictions or not, but that it is our "best hope for a TOE", justifying continued work on it. My impression is that this is currently the most common point of view among theorists who call themselves "string theorists". Since there are no viable ideas out there for how to move string theory in the direction of giving a predictive TOE, in practice people in this camp have stopped work on the problem and taken up other things to work on (e.g. it from qubit ideas).

16. **DB**
May 4, 2020

Thanks for your reply Peter.
This will be my last email on this thread, otherwise I will almost monopolize it.

I know about Vafa's swampland idea. As you say, it's going nowhere. Even though it's been out for years now, no top theoretical physicist has followed that road. Just him and his team. It doesn't look promising at all.
On one of the conferences organized to talk about it, I remember Arkani-Hamed said the following:
"QM forces us to split the world into two parts: infinite "observers" and finite "systems", but… neither Gravity nor Cosmology likes this!!!"
And two minutes later, the following:
"This raises the possibility that we are misinterpreting string landscape-
the different regions aren´t "out there", but are different approximate "system/observer" splits of a single object".

I can send you the link of the conference if you want.
So it seems to me that Nima has a hunch that all that swampland and landscape ideas have no traction at all.

I fully agree with what you say that the majority of string theorists now tend to defend point number 3. Actually, the phrase you've noted was said by Witten himself. No wonder the rest follows suit.

The most probable future for string theory, IMO, is going to be that it will be studied as a branch in mathematics.
Is it useful? Yes it is.
Can we fully discard it? No ways.
Has it got future? You bet.
But it does not describe Nature. It is not a unique theory of reality, but maybe something deeper -a set of mathematical principles that can be used to relate all physical theories. A new kind of Calculus, if you like.

Finally, when it comes to a possible TOE, that's where we part our ways.
I don't believe one exists at all. I think it's too much to hope for. And if it does exist, I tend to believe that it will always stay out of reach.

17. **Peter Woit**
May 4, 2020

DB,
There are lots of problems with solving the problem of string theory by announcing that it is just "mathematics", beginning with the problem that one doesn't know what "string theory" actually is. To the extent the words have any meaning, they are as a program to develop a specific sort of fundamental physical theory, not as a specific mathematical construction.

What you quote from Arkani-Hamed seems to me just the kind of off the top of his head speculation about some unknown possible new revolutionary fundamentals of physics replacing quantum mechanics that he likes to indulge in. He and others have gotten pushed into this by the (unacknowledged) failure of their previous research programs. A few years ago, pre-LHC results, in all his talks he put up a slide showing a fork in the road, arguing that in a few years the LHC would tell us whether there was weak-scale new physics providing naturalness or else it was the landscape.

The results are now in: no weak-scale new physics. But instead of now giving talks about how the LHC has proved the landscape picture (which he knows would cause his audience to laugh him out of the lecture hall), instead he is promoting speculation about replacing space, time and QM with something completely different.

18. **DB**
May 4, 2020

Thanks Peter.
To be honest, I never understood Nima's quote very well…

19. **David Hillman**
May 4, 2020

Hi Peter. I was struck by the starkness of your comment that the idea of there being new forces mixing leptons and quarks is a bad idea. It's long seemed to me unlikely, purely from a prejudice towards the simplicity of things (and perhaps this is just one more example of a prejudice that might lead a physicist astray), that the number of protons at the beginning of time would be the same very large number that it is now. Are you saying that this is a bad idea? or are there other ways to avoid proton number conservation besides having a mixing of leptons and quarks? (Similarly, where do you stand on Sakharov's conditions to explain prevalence of matter over antimatter?)

20. **Peter Woit**
May 5, 2020

David Hillman,
I don't see any point to trying to guess based on our prejudices about what the world should be like whether quarks can turn into leptons. Given that we don't see protons decay quickly, that's strong evidence that quarks don't turn into leptons. It makes perfectly good sense to speculate that maybe they do, just at a very low rate, and build experiments underground to look. We did this and found that, at the lowest rate we can probe, doesn't happen. Quarks turning into leptons is not an idea about mathematics, it's an idea about physics, a "bad idea" in the sense that all evidence is that it doesn't work.

I was really trying to make a point about the failure of GUT models. Some people like to point to that failure as a failure driven by mathematics: theorists wrote down a theory based on a large Lie group (SU(5)) that was supposed to be "mathematically beautiful" and that theory failed, so the problem was the use of the Lie group and obsession with mathematical beauty.

But the underlying problem with GUT models is not the mathematics. There are an infinity of different ways you can dress up in mathematical language (eg. an infinite number of choices of groups and representation) a GUT hypothesis which postulates new physics at a new energy scale that allows quarks to turn into leptons. In all of them the "mathematical beauty" of a large symmetry group is cancelled by having to introduce a new complicated and ugly Higgs sector to break the symmetry and explain why we don't see it. What drove the GUT hypothesis was not mathematics but physics (ask Georgi and Glashow…), and its failure is a failure of a physical idea, not of the idea of pursuing mathematical beauty as inspiration for new ideas about fundamental physics.

Some Philosophy of Science

May 5, 2020
Categories: Uncategorized

Defenders of certain failed speculative theories like to accuse those who point to their failure of being “Popperazzi”, relying on mistaken and naive notions about predictions and falsifiability due to Karl Popper. That’s never been the actual argument for failure, and two excellent pieces have just appeared that explain some of the real issues.

- Sabine Hossenfelder’s latest blog entry is Predictions are overrated, a critique of the naive view that you can evaluate a physical theory simply by the criterion “Does it make predictions?”. She goes over several important aspects of the underlying issues here, making clear this is a complex subject that resists people’s desire for a simple, easy to use criterion for evaluating a scientific theory.
- Over at Aeon, Jim Baggott writes about this under the headline How science fails, focusing on the life and work of philosopher of science Imre Lakatos. I wish I had been aware of the ideas of Lakatos when I wrote a chapter about the complexities of evaluating scientific success or failure in my book *Not Even Wrong*, since he was concerned with exactly the sorts of issues I was grappling with there.

  One of the main ideas of Lakatos is that you should conceptualize the problem in terms of characterizing a research program as “progressive” or “degenerating”. As relevant new experimental and theoretical results come in, is the research program showing progress towards greater explanatory power or is it instead losing explanatory power, for instance by adding new complex features purely to avoid conflict with experiment? One way I like to think of this is that it’s hard to come up with an absolute measure of success of a research program, but you can more easily evaluate the derivative: is some new development positive or negative for the program?

  I don’t think there’s any question but that supersymmetry, GUTs, and string theory are classic examples of degenerating research programs. In 1984-5 there was great hope for a certain idea about how to get a unified theory out of string theory (compactification on Calabi-Yaus), but everything we have learned since then has made this hypothesis one with less and less explanatory power.

  The Lakatos framework has the feature that there is no absolute notion of failure. It always remains possible that the derivative will change: for instance the LHC will find superpartners, or a simple compactification scheme that looks just like the real world will be found. The not so easy question to answer is when to give up on a degenerating research program. I think right now prominent string theorists are taking the attitude that it’s past time to give up work on the idea of string theory unification (and they already have), but not yet time to admit failure publicly (since, you never know, a miracle may happen…).

And now, for something completely different: If you want something more entertaining to read about particle physics, I highly recommend Tommaso Dorigo’s Anomaly! (see review here). The one problem with that book was that it stopped in the middle of the story (end of Tevatron Run 1). He now is making available some chapters (see here and here) he wrote that cover the later, Run 2, part of the story.

Comments

1. **Hendrik**
   May 5, 2020

   The whole question of what precisely a prediction is, has had a very stimulating analysis in the philosophy of science under the concept of the “empirical content of a theory.” Some hard problems here include the question of the theory dependence of our apparatus, so e.g. if you take a reading on the LHC, you not only need the particle physics, but also the electronics, solid state physics etc. to understand the numbers. I enjoyed the book “Structure and Dynamics of Theories” by Stegmuller (based on the work of Joseph Sneed) who makes sense of this.

2. **Marty Tysanner**
   May 5, 2020

   Another good post is by Stacy McGaugh, Predictive Power in Science. He discusses varying levels of prediction/explanation, with the “gold standard” being *a priori* predictions because they can’t be fudged.

3. **Low Math, Meekly Interacting**
   May 5, 2020

   I simply can’t agree that “predictions” are underrated, unless one is so inclusive as to consider a whole lot of obvious rubbish. Of course the mere ability to make some prediction doesn’t qualify as good science. But the inability to make any prediction is surely enough to disqualify.

   “Explanatory power” is “elegance” with more letters. I doubt there’s a way to really quantify the constituents of hypotheses and of theories in any universally reliable sense, so as to allow us to compute the magical ratio of explanations to assumptions. Sure, some things obviously need to be slashed by Occam’s razor. Other things, it’s not so cut-and-dry. A lot of people have come up with ideas that lacked the desired economy, and had them thinking, rightly, that “there’s gotta be a better way”. But it did the job, at least for a time. That’s perfectly scientific.

   Why can’t it be enough to “work sufficiently accurately to encompass known observations and predict new phenomena, such that it can be accepted provisionally?”

4. **Klavs Hansen**
   May 5, 2020

   LMMI:
   I’m not sure your suggestion “work sufficiently accurately to encompass known observations and predict new phenomena, such that it can be accepted provisionally” is not really what people are asking for. It sounds as if you want consistency with known facts and non-trivial predictions, based on an understanding that by necessity must be provisional as all other science.
   Predictions serve as more than a test of theories, of course. We should not forget that science is done by humans and comes with a huge baggage of sociology. My otherwise extremely diplomatic PhD supervisor had a remark about people who “can explain everything and predict nothing”. Non-trivial predictions that get confirmed demonstrate the hard way that somebody has understood something about the way Nature works.

5. **Sabine Hossenfelder**
   May 5, 2020

   Thanks for the link. Was about to mention Stacy’s post, but I see someone already did!

6. **Daniel Tung**
   May 6, 2020

Sabine seems to embrace only explanatory power and discards prediction power as a criteria of good science. But there is always the problem of how to define good explanation. She mentioned a good explanation should have less assumptions and is able to accommodate more data. This is reasonable, but it is not enough. For example, to certain people the statement "God created everything" seems to be a good explanation. It is also an extremely simple one. The problem with this "explanation" is that the statement itself has no predictive power at all. So both simple non-ad hoc explanatory power and novel successful predictions are necessary for the definition of a good science.

7. **Low Math, Meekly Interacting**
   May 6, 2020

   Klavs: Sure, as long as we can get everyone to agree on what "non-trivial" means. Generally, that's in the same league as "porn", i.e., you know it when you see it. Except when you don't. Obviously it's damn hard to nail down. I tend to like the stock, anodyne "definitions" of the above sort I quoted simply because there's hope they invite enough consensus to get people off of the subject of definitions and move on to the work. I wouldn't claim they're terribly useful as a guide, but as soon as one tries to get more particular there's a whole lot to debate, and little to show for it afterward.

   But for heaven's sake, let's not trivialize the importance of a prediction…at least of the sort we know to be great when we see it.

8. **Klavs Hansen**
   May 6, 2020

   LMMI:
   We don't all need to agree on a definition of the (non-)trivial nature of predictions to appreciate them.
   Just like we don't need to agree on how precisely to define dirty clothes before we agree that laundry is a good invention.

9. **Stephane**
   May 6, 2020

   I think any physicists should at minimum have read the book by Alan F. Chamlers – What Is This Thing Called Science? (4th edition).

   It is also interesting to note, that physics books for students were rewritten after the Second World War and the Sputnik crisis in the US to target more Engineers and so by removing all 'too philosophical chapters'. It is very nicely described in David Kaiser's article: "Turning physicists into quantum mechanics," Physics World (May 2007): 28-33.

10. **Peter Woit**
    May 6, 2020

    All,
    Like progressive and degenerating research programs, there are progressive and degenerating comment threads, and this one is degenerating in the usual way. If you don't have something new and interesting to say that is specifically about the material in the posting, please resist temptation.

11. **Tommaso**
    May 8, 2020

    Hi Peter,

    many thanks for citing my outlet of additional material from Anomaly!

    Concerning Sabine's defense of theory that make failed predictions, I will iterate here what I commented on twitter about it. Since she has been bashing Supersymmetry theorists for quite some time now, precisely because of their failure to make falsifiable predictions and their undeterred insistence on the possible veracity of their theory (with connected attacks to new accelerator physics projects), I mentioned that her latest entry seemed inconsistent. But consistency, so they say, requires you to be as ignorant as you were a year ago….

    Cheers,
    T.

12. **Peter Woit**
    May 8, 2020

    Hi Tommaso,
    Since Sabine hasn't been writing editorials for the New York Times arguing for shutting down the future of what I and my colleagues do, I can afford to be more charitable. I think her criticism was of the naive idea that you can easily tell good from bad theory by use of the identification "makes predictions=good theory". Unfortunately it's not so easy, and that's one reason I decided to write a whole book about the problem of evaluating SUSY/GUTs/string theory, instead of a paragraph saying "no predictions" (lots of people never read the book, just a review or two, and so are convinced that the content of that book is nothing but that kind of paragraph).

    In the SUSY case, many of the prominent theorists involved decided pre-LHC to stick their necks out and claim to have a specific prediction falsifiable at the LHC (weak scale SUSY). This was quite unusual. Thanks to you and your colleagues, science progressed, with heads chopped off and their previous owners reduced to the behavior of proverbial chickens with a similar experience. This hasn't stopped some of them from continuing to award each other $3 million prizes, but has very seriously damaged their credibility and influence. So, yes, it would have been a good idea for Sabine to acknowledge that sometimes the naive model does work (and we desperately need more of that…)

13. **Lee Brown**
    May 19, 2020

    Regarding "I don't think there's any question but that supersymmetry, GUTs, and string theory are classic examples of degenerating research programs. "

    Do you think that the attempt to use supersymmetric particles to explain the supposed anomalous events recorded in the ANITA project are legitimate, or simply an attempt to resuscitate supersymmetry?

    Popularly written about here : https://www.livescience.com/63692-standard-model-broken-supersymmetry-new-physics.html
    Referring to this preprint: https://arxiv.org/pdf/1809.09615.pdf

    I would like your thoughts Prof. Woit, as it is beyond my ability to judge.

14. **Peter Woit**
    May 19, 2020

Lee Brown,

The great thing about SUSY is that you can explain any weird event or two using it, but no one takes this kind of activity seriously. Those 2018 media claims were bad enough, but the coverage of ANITA has now completely jumped the shark: the NY Post today has this

https://nypost.com/2020/05/19/nasa-finds-evidence-of-parallel-universe-where-time-runs-backward-report/

"In a scenario straight out of "The Twilight Zone," a group of NASA scientists working on an experiment in Antarctica have detected evidence of a parallel universe — where the rules of physics are the opposite of our own, according to a report."

There doesn't appear to be anything new here, I can't figure out what idiot told the press about "evidence for a parallel universe". If I had any association with ANITA, I'd be suing the Post for defamation.

This kind of nonsense really should just be ignored, I'm only commenting on this because one needs a little bit of comic relief these days.

15. **Blake Stacey**
May 21, 2020

There's a CNET story ("No, NASA didn't find evidence of a parallel universe where time runs backward") that suggests the NY Post cribbed from the un-paywalled portion of a New Scientist story from April:

https://www.cnet.com/news/nasa-did-not-find-evidence-of-a-parallel-universe-where-time-runs-backwards/

My guess is that PI has a pretty well-funded PR staff.

16. **Willie Storage**
May 25, 2020

Sabine has admirably defeated a straw man without even giving the standard lines from the likes of Paul Thagard ("falsifiability provides no criterion for rejecting astrology as pseudoscience") that usually accompany "disproofs of Popper." She doesn't seem to have read Lakatos carefully, and possibly not even Popper. Arguments defending explanatory power over predictive success have all the problems that led Popper to see a clear difference between the predictive theory of Einstein and the explanatory-only theories of Freud, Adler, and Marx. Popper saw making correct predictions as possibly more of a vice than a virtue; it is very wrong to pin predictive success on Popper. Astrology makes many true predictions, but they are not bold. Popper admired bold predictions (falsifiable on that basis, not merely trivially falsifiable) for all the reasons that philosophical Bayesians do; without the theory the prediction should be utterly surprising, with the theory, less so. Eddington's experiment was his exemplar for this. Of course science seeks good explanations, but with no element of refutability, theory choice based on explanatory power descends into pure relativism.

17. **Art**
May 26, 2020

I note that the price of "Anomaly!" no longer is one. Looking forward to reading it.

Final Fourier Analysis Notes

May 18, 2020
Categories: Uncategorized

Our semester here at Columbia is finally over, and I've put the lecture notes on Fourier analysis that I wrote up in one document here. A previous blog posting explained the origin of the notes: they cover the second half of this semester's course, from the point at which the course became an online course due to the COVID-19 situation.

Not much blogging going on here, mainly since everyone staying home seems to have kept news of much interest to a minimum.

Comments

1. **Sami**
   May 19, 2020

   Great notes and thank you for making them public.

2. **DB**
   May 19, 2020

   Peter,
   this upcoming event might be pretty interesting.

   https://conference.ippp.dur.ac.uk/event/906/

3. **Geoffrey Dixon**
   May 20, 2020

   Correct me if this is way off:
   > The Fourier transform is possible because the 1-sphere is parallelizable;
   > What is known about generalizing concept to the 3- and 7-spheres?

4. **Justin B Glick**
   May 20, 2020

   I thought you could use a good laugh during these troubled times. Unfortunately, the article doesn't seem to be in jest.

   https://www.google.com/amp/s/whdh.com/news/nasa-uncovers-evidence-of-bizarre-parallel-universe-where-physics-time-operate-in-reverse/amp/

5. **Peter Woit**
   May 21, 2020

   Justin B. Glick,
   See
   https://www.math.columbia.edu/~woit/wordpress/?p=11747#comment-236286

   Geoffrey Dixon,

   It's not S^1 being parallelizable (all 1-manifolds are parallelizable…) that makes Fourier analysis work, it's that S^1 is a group (rotations of the plane). S^3 is also a group (SU(2) or Sp(1), unit quaternions if you prefer). There is an analogy of Fourier analysis in this case (like for any compact Lie group). Abstractly
   $$Functions(G)=\sum_{irreps\ V} V\otimes V^* =\sum_{irreps\ V} End(V)$$
   Or, in the case of SU(2):
   $$Functions(S^3)=\sum_{n=0}^{\infty}(\text{matrix elements of}(n+1)\times(n+1) \text{matrices})$$
   This says that any function on G can be expanded in matrix elements of irreducible representations, e.g. any function on S^3 can be expanded in matrix elements of the spin 0,1/2,1, etc. representations (these matrix elements depend on the group element, so are functions on the group).

   If your group is commutative, all irreducible representations are one-dimensional, and you get an expansion in these (as character functions on the group). For instance, the $e^{in\theta}$ for S^1.

   S^7 isn't a group, so this won't work.

6. **Peter Orland**
   May 21, 2020

   Hi Peter,

   But the group property is not necessary for orthogonal expansions to exist…

   Spherical harmonics on S^2, expansions in terms of orthogonal polynomials, Bessel functions. Orthogonality and completeness are all proved (despite there not being a Peter-Weyl/Plancherel theorem, which you have for groups). Granted that some (but I think not all) of these are obtained by reducing from a group to a lower-dimensional manifold.

   Not trying to be snarky here, just mentioning something that appears relevant. Maybe calling an expansion into orthogonal functions "Fourier" depends on the group property (since it is an expansion into group characters) of the manifold. That would make the term purely a matter of semantics. Beyond that, I'm not sure how it is important.

7. **Peter Orland**
   May 21, 2020

   For example, the symmetry group of S^7 is SO(7) or $spin(7)$. There are certainly spherical harmonics, respecting this group symmetry, on S^7.

8. **Peter Woit**
May 21, 2020

Hi Peter,

Yes, there are various other generalizations. There are bases of orthonormal functions of various kinds without an obvious group theory origin. For a pair of Lie groups G, H, you can think of functions on G/H as the subspace of functions on G that are right invariant under H. This is how you get spherical harmonics: think of S^2 as $SU(2)/U(1)$, functions on S^2 as functions on S^3 right invariant under the $U(1)$ action. In terms of the decomposition I wrote above into a sum over spins of $V\otimes V^*$, the $U(1)$ acts on one of these, with invariant piece one-dimensional for integral spin, zero for non-integral spin. So, the sum is over all integral spin representations, and this is exactly the decomposition of functions on S^2 into spherical harmonics.

While S^7 is not a Lie group (and G/H in general is not parallelizable), S^7 does have an amazing ("unparalleled" by any other example) variety of different geometries, thought of as different identifications with a G/H.

$$S^7=Spin (5)/Spin(3)=Spin(6)/SU(3)=Spin(7)/G_2=Spin(8)/Spin(7)$$

(you might want to think of $Spin(5)$ as $Sp(2)$, $Spin(6)$ as $SU(4)$).

9. **DB**
May 21, 2020

This might be entertaining for just over 40 mins.
The first part has some interesting aspects.
There are a couple of questions at the end, one re the landscape…

https://www.youtube.com/watch?v=GC2mqMmfW6o

10. **Peter Orland**
May 21, 2020

Your comments on S^7 are fascinating (I knew about S^2), but I guess I was making the point that there also exist complete sets of orthogonal functions on manifolds in all sorts of situations; even with no symmetry (by which I mean no Killing vector at almost every point).

I suppose I could have made my earlier point clearer by not citing examples of manifolds with symmetry.

11. **Peter Orland**
May 21, 2020

…but what seems clear is that when there IS symmetry, constructing orthogonal families of functions becomes an easier algebraic problem (with all kinds of beautiful aspects), rather than a hard and ugly analytic problem. Maybe that is the real point.

12. **Geoffrey Dixon**
May 22, 2020

I found an 8 year old introductory text on Spherical Harmonics in q Dimensions (Frye and Efthimiou). It claims there is scant literature on this topic, so something at this level is probably great for someone who has taught advanced analysis, but not by preference, and decades ago. I name no names.

13. **David Reed**
May 22, 2020

The group property is needed for convolution.

14. **Peter Orland**
May 23, 2020

Spherical harmonics in any dimension are quite adequately discussed in the book by Hochstadt on Mathematical Physics.

15. **Geoffrey Dixon**
May 23, 2020

Peter O: Hochstadt is mentioned in the text of Frye & Efthimiou. They say it is more advanced and definitive. I wonder if I own the thing – down in the Bat Cave. For the time being, given how unlikely it is at his point that it will help me crystallize my intuition, I'll continue perusing F&E in a desultory way.

Oral Histories

May 24, 2020
Categories: Uncategorized

I recently ran across a recent interview with Mary K. Gaillard, which encouraged me to look again at the AIP's oral histories site. For a review of her autobiographical book, see here.

She has the following comments on the current state of HEP theory:

> **Zierler:**
>
> What do you see as the future of theoretical physics? Where is the field headed?
>
> **Gaillard:**
>
> Well, I think it's headed towards insanity by itself. I mean, no, if we don't have experiments, people can let their imaginations run wild, and invent anything without it being verified or disproven. So I think it—I mean, if we want to understand more about what happens at higher energies, we have to have higher energy colliders. I don't think—well, cosmology is tied to particle physics, and that's probably something from—I mean, there is a lot of data coming from cosmology. And there is some data that will be coming from very low energy precision physics. But I don't think that theory by itself—it needs to be kept in line by experiments.
>
> …
>
> **Zierler:**
>
> And so what advice do you have sort of globally for people entering the field in terms of the kinds of things they should study, and the way they should study those things?
>
> **Gaillard:**
>
> That's a —actually, I often advise people to go into astro-particle physics just because I think that it has more promise of getting data because I don't—I mean, I strongly believe you can't go forward without good data, and unless—well, of course, if they do have another generation of colliders, that would be great. I just don't know if that's going to happen…

Another very recent interview I found interesting was that of Vipul Periwal. Periwal arrived as a Ph.D. student at Princeton around the time I was on my way out, starting his career right about when string theory hit in late 1984. He worked as a string theorist for quite a few years, ending up in a tenure-track faculty position at Princeton, but then left the HEP field completely, starting a new career in biology. Here are some extracts from his interview:

> **Zierler:**
>
> And what was David 's research at this point? What was he pursuing?
>
> **Periwal:**
>
> String theory. He was just 100 percent in string theory. Right? They just did the heterotic string, and so everyone was — every seminar at Princeton at that time was all string theory. It was all string theory. Curt was working on it, David was working on it. Edward was working on it. Larry Yaffe was probably the only person — no, two people, Larry Yaffe and Ian Affleck were not doing string theory. Not that they couldn't, but they just would not do it.
>
> …
>
> **Zierler:**
>
> So you mean, despite at this point all of the work on string theory, there were still existential questions about what string theory was, that remained to be answered?
>
> **Periwal:**
>
> There still are.
>
> **Zierler:**
>
> Yeah.
>
> **Periwal:**
>
> No one has ever figured out what is string theory. I mean, if you go ask all the eminent string theorists, none of them can answer for you this one simple question. Can you show me a consistent string theory, where supersymmetry is broken?
>
> …
>
> **Zierler:**
>
> Was it good for your research? Was it a good time for you ?
>
> **Periwal:**
>
> I don't think I did particularly interesting research. I did — I mean, I did okay, but I'm not particularly proud of anything I did there, except for one little paper I wrote, in which — see, this is called the contrarian part — is I showed — people were very excited about the large N limit, so I took this toy model, and I showed that in the large N limit, it actually produced something nonanalytic, as in like, you could not, in any order of 1 over an expansions, ever see what the answer was that was exact at N equals infinity. So, in other words, it was to me a cautionary tale. Like, you think you're doing large N and then getting an intuition for finite N. But here's this very simple model where you can do the calculation exactly, and you can do all your 1 over N expansion as far as you want, and it'll never tell you about what's going to happen at N equals infinity. But you know, it's a — at this point, string theory was already at that time pretty much a sociological thing.
>
> **Zierler:**

What do you mean “sociological”?

Periwal:

So, it’s something that was borne home to me gradually, that there’s no experimental proof. Like, are you a good physicist or a bad physicist? Who’s going to tell? How do you know? Right?

Zierler:

Yeah.

Periwal:

I mean, I’d go and give a talk somewhere, and I remember this very clearly. I went and gave a talk at SUNY Stony Brook, what’s now called, I guess, Stony Brook University. And at the end of the talk, I was talking to one of the faculty there who’d invited me. And he said, “So, what does XYZ think of this work?” And I was just taken aback. I was like, wait, you’re a physicist. I’m a physicist. Why do we need to know what XYZ thinks of this?

Zierler:

Yeah.

Periwal:

Right? That’s what I mean by sociology.

Zierler:

I see. It’s as much about what a certain group of peers thinks about the theory.

Periwal:

Yeah, and this really perturbed me. As far as I was concerned, after the string perturbation theory diverges thing, I was not interested in doing perturbative calculations. So, what the solution was that people did was: okay, we’ll work on various supersymmetric theories where there is no higher contribution, and under the assumption that there is supersymmetry, you can use holomorphicity to deduce things from the structure of the fact that there’s so much supersymmetry. And this really bothered me, as in okay, there’s this really amazingly beautiful structure, and lots of very pretty mathematical results that are coming out — mathematical results that are suggested by these correlations. But I just don’t get — as a physicist, I don’t to want to have to worry about, “What does XYZ think about what I’m doing?”

Zierler:

Yeah, because you’re pursuing a truth, and it’s either true or it’s not. It doesn’t really matter what other people think about it.

Periwal:

Right. I really don’t care. I mean, no matter how much I respect — and I do — Edward, or David, or whoever, I really don’t need to know what they think about my work. Right? I just — anyhow —

Zierler:

How does that attitude serve you in an academic setting, though? Right?

Periwal:

It doesn’t.

Zierler:

How does that attitude affect you in terms of tenure considerations and things like that?

Periwal:

Yeah, so when I was — no, so I actually — I mean, when I was — well, I have no — I’m really stupid sociologically, as in, I have no instinct for self-preservation. So, I could see I had role models in front of me of how people with tenure…

Zierler:

Succeeded.

Periwal:

…succeed, not just getting tenure at Princeton, but getting tenure at very good places after Princeton, too. And I paid zero attention to all this. So, while I was at Princeton, I tried doing some lattice gauge theory.

With this attitude, it’s not surprising that in Periwal didn’t get tenure at Princeton. He didn’t soon get job offers elsewhere in HEP theory, and decided in 2001 better to try another field than keep going in the one he was in. The interview ends with:

Zierler:

Alright. So, really, the last question. What does the big breakthrough moment look like for you? How would you conceptualize this in terms of putting all of this together? What does that big breakthrough look like?

Periwal:

If I could make a prediction that was clinically testable, that would make me very happy.

Zierler:

Do you think you'll get there? It's the thing that motivates you.

Periwal:

Yeah. I want — you know, I said this once. We had someone visiting when I was managing the physics seminar at Princeton once, as an assistant professor. So, this guy asked me, "So, Vipul, what are you working on?" And I was very jaundiced at that time about making a prediction. So, I said, "Well, lattice gauge theory," which, you know, nobody at Princeton did lattice gauge theory. You were all supposed to be doing string theory. I said, "Yeah, I want a number before I die." People are looking at me like, "What kind of lunatic is this?" But you know, a number. That would be nice.

Looking through the old interviews, I found one of very personal interest, that of [Gerald Pearson], who worked with my grandfather Gaylon Ford at Bell Labs. Some of his stories mentioned work with my grandfather (whose main expertise was in the design and construction of vacuum tubes) at Bell Labs during the 1930s. During this period both studied at Columbia, where my grandfather got a master's degree in physics.

Pearson:

Gaylon Ford worked with Johnson. When Kelly was head of the tube department, he worked in that area. And then they had a big shakeup after which the job was no longer available. Much against his desires, he came over to work with us.

…

Hoddeson:

In 1938 you were moved over from Johnson's group into Becker's. In fact, you and Sears seem to have changed places.

Pearson:

Before that took place, I remember Johnson called me into his office one day and he wanted to know if I would like to work on… well, Buckley had sent a memorandum asking for temperature regulators for buried cable. Johnson wanted to know if I would like to work in this area. Of course, no one likes to change their jobs but I said, "Fine" and we agreed that I would spend a portion of my time on this problem and that's where thermistors came from. This continued on and it was very successful. Then it was decided that the work fit in better with Becker's area than it did with Johnson's. And, well you asked me about Ford. He was the one who was brought from the tube shop to work on this. And then he later went to work on something else.

Hoddeson:

Let's see if we can date that time. Ford wasn't working with you yet. Ford is here with you in 1934. But this move didn't take place until '38.

Pearson:

Yes, that's what I was saying. He first came over to work on change of resistance with temperature. And he was working with a sulfide compound. And then, let's see, what happened to him. He went someplace else and Johnson called me into his office and asked me if I would like to carry on Ford's work and we agreed that I should do it part time and still work on noise. But I said I didn't want to work with sulphur, it smelled too bad. I said if I work in that area, I'm going to use some other materials. So I made a study of that. First I worked on boron and then on a combination of oxides. A lot of my patents are on such materials and devices. These devices are still used today in the buried cable system as volume regulators.

Comments

1. **Jesper**
   May 25, 2020

   Hi Peter,

   the interview with Periwal is spot-on right. I was a postdoc in HEP during the 00's and I had very much the same experience and the same attitude as Periwal – not to pay attention to the sociological aspect of the game (which sure enough ended my academic career). I think, though, that what Periwal describes goes much deeper than just string theory. During my career I came in close contact with other fields like loop quantum gravity and noncommutative geometry, and what I encountered there was the same sociological structures – a very hierarchic system, where it is critical for your future prospects to be accepted by the few people "on top".

   Its depressing, because the incentive structures in academia are in a way upside-down: you get rewarded for being a conformist and punished for being original. You build a career by sticking to mainstream and you kill a career by trying out ideas outside of mainstream. As long as the incentive structures are not fixed I am not sure that things will really change.

   The only place that I encountered a fundamentally different culture was in mathematical physics, among people doing axiomatic and algebraic quantum field theory. There I found a much less hierarchic culture where people were more open to new ideas. In fact, very much the same attitude that I have encountered among mathematicians.

   My field of research is an intersection between mathematics and physics and thus I have often given talks to mixed audiences. To me it is striking how different mathematicians and physicist react to new ideas. In my experience the mathematicians – and also mathematically oriented physicists – are much more willing to consider new ideas. Thus I completely agree with your view that HEP should look towards mathematics in order to learn how to deal with a situation where we have less experimental data to work from.

   – and by the way, it is possible to do research outside of academia. I don't think that people should necessarily waste their time in a system that is broken.

   Best, Jesper

2. **Tess**
   May 27, 2020

   I find the "it's sociological" comment really amusing, thought I'm not sure what it's trying to imply about sociology. From sociology, there's a subfield called Legitimation Code Theory, which talks about how knowledge is structured and related systems of power. The shift he refers to, from dealing with the research claims and evidence as written to asking "what does a particular expert think of this?" can be described within LCT as a change from a knowledge code to a knower code, where the authority of the speaker determines the perceived legitimacy of what is said.

3. **Robert Wilson**
May 28, 2020

Jesper,
I think you are right to point to the culture of mathematics as a good example of how progress can be made. In mathematics, people reject new ideas *after* considering them carefully, rather than before. Some years ago, there was a very interesting announcement of a proof that all finite projective planes have prime-power order. I won't embarrass the author of this paper, who was and is a well-respected mathematician, by giving further details, but I found this announcement surprising, so I immediately got hold of the paper and starting reading it. I organised a seminar series in which I explained the argument. Not until week 3 or 4 did I find the crucial error, buried deep in a double induction, where one of the indices had slipped by 1, thus bringing the whole house of cards tumbling down. It would have been easy for me to reject the result as implausible without reading it, as seems to happen all the time in physics, but that is not the culture in mathematics. It would have been easy to say, there's a mistake on page 1, the whole thing is rubbish, but that is not the culture of mathematics. The important thing is to engage with the argument, and to find not any old mistake – that is easy – but to find the crucial mistake which cannot be patched up. If experts do this kind of work before passing judgement, then the subject is in safe hands. If they pass judgement before doing this work, it is not.

4. **Thomas Van Riet**
May 28, 2020

Concerning: "No one has ever figured out what is string theory. I mean, if you go ask all the eminent string theorists, none of them can answer for you this one simple question. Can you show me a consistent string theory, where supersymmetry is broken?"

That is not about the 'what' really. Rather about the 'how' to break SUSY. At which scale etc,…. Let me be clear that dynamical SUSY-breaking in QFT is also a nightmare (ironically string theory, through AdS/CFT, has given us insight in this issue) . Are we all negative about QFT because of that? I mean, imagine we could break SUSY like 123 and pull as many 4D EFTs out of our stringy hat as we wanted ("old school landscape reasoning") people would have complained we cannot predict anything more than what QFT can. Once we start realizing that finding consistent non-SUSY 4D vacua is actually highly involved and constraining ("Swampland viewpoint"), we seem to be criticized for not yet having described the universe and everything within. So I wonder: is research on fundamental interactions only free of attacks once we solve all problems at once? I expect that this is not possible and that any healthy approach to a "theory of everything" is one that makes us confront ourselves with our limited knowledge of mathematics. String theory seems to make that clear to us. There is no reason, I can think off, to be less excited about strings, than we were 20 years ago. On the contrary, we have only seen striking ideas like holography emerge.

5. **Peter Woit**
May 28, 2020

Thomas van Riet,
Periwal was a Princeton faculty member specializing in string theory, with his field of expertise non-perturbative string theory. He's undeniably right that "No one has ever figured out what string theory is." Do you disagree with that? The "SUSY breaking" reference was clearly just to rule out certain specific non-physical cases with a lot of SUSY where someone could point to a conjectural definition.

As for whether there's a good reason to be less excited about string theory than 20 years ago, you might want to consider asking all the IAS physics faculty why they've stopped working on string theory…

6. **Thomas Van Riet**
May 29, 2020

Dear Peter

We must be reading different arxivs? To my knowledge they have continued working on it. It is certainly true that the hep-th community has gone in many spin-off directions inspired by string theory ideas (SYK being one). But no, stringy research is alive and kicking.

7. **Peter Woit**
May 29, 2020

Thomas Van Riet,
I notice you didn't answer my question, assume you agree that "No one has ever figured out what string theory is." is an accurate statement. As you are well aware, the SYK model is a quantum mechanics model that has nothing to do with string theory. Yes, string theory "inspired" work on SYK, because its failure caused string theorists to stop thinking about strings and try and find something different and more promising to work on.

The Week's Anti-Hype

May 27, 2020
Categories: Uncategorized

I never thought I would see this happen: a university PR department correcting media hype about its research. You might have noticed this comment here a week ago, about a flurry of media hype about neutrinos and parallel universes. A new CNN story does a good job of explaining where the nonsense came from. The main offender was New Scientist, which got the parallel universe business somehow from Neil Turok and from here.

The ANITA scientists and their institution's PR people were not exactly blameless, having participated in a 2018 publicity campaign to promote the idea that they had discovered not a parallel universe, but supersymmetry. They reported an observation here, which led to lots of dubious speculative theory papers, such as this one about staus. The University of Hawaii in December 2018 put out a press release announcing that UH professor's Antarctica discovery may herald new model of physics. One can find all sorts of stories from this period about how this was evidence for supersymmetry, see for instance here, or here.

It's great to see that the University of Hawaii has tried to do something at least about the latest "parallel universe" nonsense, putting out last week a press release entitled Media incorrectly connects UH research to parallel universe theory. CNN quotes a statement from NASA (I haven't seen a public source for this), which includes:

> Tabloids have misleadingly connected NASA and Gorham's experimental work, which identified some anomalies in the data, to a theory proposed by outside physicists not connected to the work. Gorham believes there are more plausible, easier explanations to the anomalies.

The public understanding of fundamental physics research and the credibility of the subject have suffered a huge amount of damage over the past few decades, due to the overwhelming amount of misleading, self-serving BS about parallel universes and failed speculative ideas put out by physicists, university PR departments and the journalists who mistakenly take them seriously. I hope this latest is the beginning of a new trend of people in all these categories starting to fight hype, not spread it.

Comments

1. **Warren Siegel**
   May 27, 2020

   An antiuniverse preceding our universe (Turok) isn't what's usually meant by a parallel universe.

2. **Peter Woit**
   May 27, 2020

   Warren Siegel,

   I could have sworn one of the news stories I saw explained that this was a "perpendicular" universe, but can't now find that.

3. **David Roberts**
   May 27, 2020

   There's a new paper in Nature about measuring the cosmic baryon density using FRBs, that is being reported in Australian ABC News as finding a whole lot of 'missing matter' in the universe (maybe some local pride breaking through: Australian telescopes and astronomers were involved). It's not terrible, the headline is a bit more sensational than the rest of the article.

4. **anon**
   May 28, 2020

   Missing baryonic matter is like water on Mars: I've lost count how many times I have seen press releases claiming that it has been found for the first time.

5. **Jens**
   May 28, 2020

   Doesn't string theory provide a natural explanation for hype/anti-hype asymmetry?

6. **Peter Woit**
   May 28, 2020

   David Roberts/Anon,
   I don't want to get into discussions here of claims about things like the significance of observations of fast radio bursts, simply because I'm incompetent to evaluate this or to even competently moderate a discussion of it. People who want to engage in public discussion of scientific issues need to pay close attention to the difference between what they have some expertise in and what they don't. This means avoiding weighing in on issues you don't have expertise in. It also though means that, since there are often few experts on a particular issue, those that are experts have an obligation to "If you see something, say something".

   On the issue of "parallel universes", there has been a massive failure over the years, with an overwhelming quantity of BS rarely if ever challenged by those who understand that it is BS. This press release is encouraging, I hope we'll see this become a regular practice. If your research is misrepresented in the press, go to your institution's PR people and put them to work.

   Jens,

   Yes, string theory does explain hype/antihype asymmetry, but it's not a natural explanation, since it involves fine-tuning by those involved.

An Advertisement for Representation Theory

June 9, 2020
Categories: Uncategorized

There's a new article at Quanta today promoting representation theory, Kevin Hartnett's The 'Useless' Perspective that Transformed Mathematics. Representation theory is a central, unifying theme in modern mathematics, one that deserves a lot more attention than it usually gets, with undergraduate math majors often not exposed to the subject at all. My book on quantum mechanics is very much based on the idea that the subject is best understood in terms of representation theory. Unfortunately, physics students typically get even less exposure to representation theory than math students.

While I think the article is a great idea, and well-worth reading, I do have two quibbles, one minor and one major. The minor quibble is that one example given of a group, the real numbers with multiplication, is not quite right: you need to remove the element 0, since it has no inverse. If the group law is the additive one, then the real number line with nothing removed truly is a group.

The major quibble is with the theme of the article that a group representation can be thought of as a simplification of something more complicated, the group itself. This is a good way of thinking about one aspect of the use of representation theory in number theory, where representations provide a tractable way to get at the much more complicated structure of the absolute Galois group of a number field. The talk by Geordie Williamson linked to in the article (slides here) explains this well, but Williamson also gets right the more general context, where the group can be easy to understand, the representations complicated. For a simple example of this, in the case of the circle group S^1 the group is very easy to understand, its representation theory (the theory of Fourier series) is much more complicated (and much more interesting).

As Williamson explains, a good way to think about what is going on is that representation theory does simplify something by linearizing it, but it's not the group, it's a group action. When people talk about the importance of the study of "symmetry" in mathematics, physics, and elsewhere, they often make the mistake of only paying attention to the symmetry groups. The structure you actually have is not just a group (the abstract "symmetries"), but an action of that group on some other object, the thing that has symmetries. When you talk about "rotational symmetry" you have a rotation group, but also something else: the thing that is getting rotated. Representation theory is the linearization of this situation, often achieved by going from the group action on an object to the corresponding group action on some version of functions on the object. Once linearized, the group action becomes a problem in linear algebra, with the group elements represented as matrices, which act on the vectors of the linearization.

To further add to the confusion, "symmetry" is often described in popular accounts as meaning "invariance". In typical examples given, "invariance" just means that you have a group action, since the group is taking elements of the set to other elements of the set (e.g. rotations not of an arbitrary object, but of a sphere). In representation theory, you have a different notion of invariance. For instance, for the representation of rotations on functions on the sphere, the constant functions are a one-dimensional invariant subspace, giving a trivial representation. But, there are lots of more interesting invariant subspaces of higher dimensions. These are the irreducible representations on the sets of spherical harmonics.

Comments

1. **I**
   June 9, 2020

   As an expert on this topic, at least with regards to QM, could you give your opinion on the most important texts/papers in the field?

2. **Peter Woit**
   June 9, 2020

   I,
   The fascinating thing about representation theory is that it is a unifying theme, appearing centrally in very different areas of mathematics. In each such area there's a huge literature and many different textbooks, so I can't come up with a short list, and I don't know of a book that provides a single overview of the many different areas of the subject.

3. **nikita**
   June 10, 2020

   It's strange they used Burnside as an exemplary representation theory sceptic. He founded it together with Frobenius.

4. **Robert Wilson**
   June 10, 2020

   As a popular article explaining representation theory to non-mathematicians, it does a reasonably good job. To a professional representation-theorist, however, it jars in a couple of places. "Mathematicians aim to avoid grappling with the full complexity of a group; instead they gain a sense of its properties by looking at how it behaves when converted into the stripped-down format of linear transformations" is wrong on so many levels. Physicists may behave like this, but mathematicians do not: we strip away the inessentials of linear transformations in order to study the stripped-down abstract group, not the other way around. It is true that to study a group it is often essential to study its representations: but this makes things more complicated, not less. It is also true that applications of group theory almost always involve representations: the group itself is usually too abstract for this purpose, without the mediation of representation theory.

5. **Robert Wilson**
   June 10, 2020

   nikita,
   You are absolutely right. What about Burnside's p^a.q^b theorem? The quintessential example of a theorem in group theory that until relatively recently could *only* be proved using representation theory, and not by group theory alone. In the very early days of representation theory in the second half of the 1890s it might have seemed as though the subject was too abstruse to be useful, but this point of view very rapidly became obsolete.

6. **John Baez**
   June 10, 2020

   I explain how Burnside changed his attitude toward group representation theory here.

7. **Sam Hopkins**
   June 11, 2020

   When people say "representation theory is a central, unifying theme in modern mathematics" I wonder to what extent they really mean that the objects to which representation theory can usefully be applied (say, simple Lie groups and all their offshoots) are central. It seems there is still a bit of mystery as to why representation theory is so effective for understanding these kind of algebraic objects and their actions. Certainly Williamson's work (and the work of his forebears like Lusztig, etc.) explores the boundary of representation theory's effective reach.

8. **Thomas Larsson**
   June 12, 2020

   As an undergraduate, and also later, I didn't understand the point with groups and representation theory. I took courses and could follow the steps, but it didn't seem very important to me. As a graduate student, I specialized critical phenomena and fractals, in particular in 2D because that was where progress was made. This was in the mid 1980s, when CFT came around, which I noticed but didn't understand.

   Then when preparing for my dissertation, I read the NPB paper by Dotsenko and Fateev, and it suddenly clicked. I realized that what I had studied in grad school could be explained in terms of the representation theory of the Virasoro algebra. This was a complete eye-opener and changed forever how I viewed physics, for better or worse.

9. **Albert**
   June 12, 2020

   Regarding your first quibble, the apparently trivial example of the multiplicative real numbers as a group, i.e. the reals w/0 zero, is actually kind of useful
   for understanding the difference between GL(2, C) and GL(2,R). The multiplicative complexes and reals are of course GL(1, C) and GL(1,R) and for both 1 and 2 dimensions, the
   complex groups are connected while the real ones have two connected components, with positive and negative determinant, the former a normal subgroup of index 2. I had never understood this very well, and realized only recently that the explanations are the same. Since the exponential function takes addition to multiplication, the Lie algebra (i.e. the additive group of all matrices) is mapped to the positive determinant matrices GL^+(2,R); note that indeed
   det(exp(A))= exp(trace(A))), so the image must have positive determinant. Further, to show the image is pathwise connected
   you can linearly interpolate in the Lie algebra and push that forward to a path in the image.
   (You can't directly linearly interpolate in the group since you might pass through something with determinant 0).

10. **Robert Wilson**
    June 17, 2020

    Geordie Williamson contacted me personally to point out that he is quoted out of context in this article, and in his lecture he discusses Burnside's change of heart in depth.

Feynman Lectures on the Strong Interactions

June 15, 2020
Categories: Uncategorized

Available at the arXiv this evening is something quite fascinating. Jim Cline has posted course notes from Feynman's last course, given in 1987-88 on QCD. There are also some audio files of a few of the lectures available here. The course was interrupted by Feynman's final illness, with the last lecture given just a couple weeks before Feynman's death in February of 1988. There's an introduction to the notes by Cline in which he explains more about the course and how the notes came to be.

The course was given over thirty years ago, and many textbooks have appeared since then, but it seems to me this has held up well as an excellent place for a student to go to learn the subject.

Comments

1. **Peter Orland**
   June 16, 2020

   There seems to be a lot of overlap with the course I took from RPF ten years earlier. Definitely a rewarding experience.

2. **Doug McDonald**
   June 17, 2020

   I read most of the piece.

   I would say that, more than a place for a beginner to go, it is the place for the
   non-beginner, non-really-expert to go to for interesting physical insights, especially ones not currently mainstream teaching fodder.

   Glashow, whom I saw in person for a whole semester, was like that too … but the big difference is that Feynman's insights hold up to post-lecture study as not being as empty handywavy as Glashow's. I, a chemist (!), could and did see through them.

HEP Theory Job Situation

June 17, 2020
Categories: Uncategorized

Way back in the 1980s and 1990s I was, for obvious personal reasons, paying close attention to the job situation for young HEP theorists. They were not good at all: way more talented young theorists than jobs, many if not most Ph.D.s who wanted to continue in the field unhappily spending many years in various postdocs before giving up and doing something else. By the later part of the 1990s I had found a satisfying permanent position in math, so this problem seemed much less interesting. When I was writing "Not Even Wrong" I did spend quite a bit of time gathering numbers to try and quantify the problem, and wrote about them in the book.

Since then I haven't paid a lot of attention to the HEP theory job situation, hoped that it might have gotten a bit better as the wave of physicists hired during the 1960s hit retirement age, opening up some permanent positions. Today someone sent me a [link to a personal statement on Facebook] (sorry, but you need to login to a Facebook account to see this) from a young theorist ([Angnis Schmidt-May]) who has recently decided to leave the field, for reasons that she explains. These include:

> We are put in competition with each other from day one, and only very few of us will be given prestigious positions in the end. Most of us never see a permanent contract, keep jumping from place to place and eventually need to find a second career after having sacrificed our entire 20s and 30s to academia. After having made it through the worst part of this and more or less securing my career, it still made me sick to see young physicists entering this spiral. I felt terrible about encouraging them to continue on this path because it is impossible to tell who will make it in the end and who will end up miserable with regrets…
>
> Science itself is severely suffering from the poor working conditions and lack of genuine career prospects. I personally found it extremely hard to focus on the science while constantly being worried about the duration and location of my next contract. #PublishOrPerish. Interactions with and among colleagues are often dominated by the drive to "show off". Very few people focus on removing misunderstandings or ask honest questions in order to fill their knowledge gaps. The general atmosphere is dominated by doubt instead of trust. We constantly need to outshine our peers. Better to demonstrate superficial knowledge of broad subjects than to focus on the details of a deep problem. Your next result needs to be "groundbreaking", otherwise you're out of a job. But produce it and have it published at least one year before your contract ends because that's when you need to apply for a new one. Science has become a show…
>
> I see absolutely no chance that any of the above will change any time soon.

She also makes important points about the personal cost of this system:

> During the last 10 years, I was forced to constantly move around, losing contact to people who meant a lot to me and not being able to establish new lasting relationships.

Sadly, it seems pretty much nothing at all has changed in the last 30-40 years, and I continue to believe this is one reason the subject has been intellectually stagnant during this period. About the only positive suggestion I can make for anyone who wants to try and do anything about this is to take a look at the analogous job situation in mathematics. My knowledge of this is mostly anecdotal, but my impression is that while, like most academic fields, the career path for a new math Ph.D. is not easy, the situation is not at all as bad as the one in HEP theory described above.

Completely Off-Topic: Xenon1T has reported [new results] today. This seems to me unlikely to be new physics (extraordinary claims require extraordinary evidence), so if you want to follow this story, you should be [consulting Jester], not me.

Comments

1. **Warren Siegel**
   June 17, 2020

   That's show business.

2. **Mark**
   June 17, 2020

   Not really a hep theory problem is it? Pretty much every academic field suffers from this problem – too many talented PhD, hardly any permanent jobs.

3. **Sabine Hossenfelder**
   June 17, 2020

   Very sad 🙁 I was on her PhD committee in Stockholm. Alas, I know exactly what she is talking about.

   And let me add that at the age of 43 I am still sitting on a temporary contract and am worried to run out of funding every other year or so.

4. **Peter Woit**
   June 17, 2020

   Mark,
   Yes, it's a general problem in academia, but to different degrees in different fields. About 20 years ago I was looking very closely at whatever data there was, and the problem in HEP theory was worse than in almost any other subject. I haven't looked at recent data, can just say that the situation in mathematics seems to me much better than in HEP theory. In particular the sad description here of the competitive environment doesn't sound like what I've seen going on in mathematics.

5. **Geoffrey Dixon**
   June 17, 2020

   Small advice from someone who fought the gnawing doubt for decades, then took steps to adjust to reality and break free:

   > If you want to do mathematics, or mathematical/theoretical physics, then do it. An institution is not necessary in most cases – just time.
   > Find something other than science that you can be passionate about, and which has the potential to provide a living.

   For me it was animation, then interactive animation, and finally a job I enjoyed creating interactive online apps that required a mathematical background. During all that time I continued working on mathematics and physics, albeit with less time. I gave talks at conferences. I retired with not too many worries.

   I was lucky, maybe, but being able to apply mathematics and physics to the real world will never not be needed.

Can your ego take the surrender? Is it surrender? Is academia what you grew up thinking it would be? Angnis discovered it is not, and she is correct.

6. **Low Math, Meekly Interacting**
   June 17, 2020

   This isn't at all unique to HEP theory, though the problem may be more/most acute in that field. The process of recruitment is fundamentally dishonest. The incentive to maintain a steady supply of cheap labor is irresistible. Research U.'s accept and produce far too many PhD's, indoctrinated and trained only for academia, with few provisions for those who can't hack it, for whatever reason. If there were any motivation to train talented people adequately for the more diversified needs of a real-world workforce, things could be much better, but there simply isn't. The needs of the institution (i.e. the grant and publications numbers game) are overriding. One needs to at least be made aware of the reality going in, but the system is far from self-policing.

7. **Peter Woit**
   June 17, 2020

   LMMI,
   Yes it's a general problem, but I do think it is much worse in HEP theory, and the damage the problem has done to the subject is unusual.

   I did do a few minutes research to see if I could get info about other fields. One thing that quickly turned up is detailed data on Philosophy Ph.D.s maintained here:
   http://placementdata.com:8182/phd-programs-running-tally
   One might think of a philosophy Ph.D. as the quintessential useless humanities degree that would make one unemployable, but the top programs often have 60-70% of their graduates getting academic positions of some sort, 25-40% getting academic positions in departments with Ph.D. programs. I'd be curious to see similar numbers for HEP theory, suspect they would be quite a bit lower.

   The humanities model is different than that in the sciences, in that they tend to not have postdocs, you can get a tenure-track job when you get your degree. Arguably this is a much better system than the HEP physics one, avoiding the brutal personal cost to many of having to spend years moving around and not putting down roots.

   The mathematics model is again different, somewhere in between, with both fewer research postdocs and fewer tenure-track jobs available to new Ph.Ds. A typical career path would be a single "postdoc" involving teaching, then a tenure-track job. I'd be very curious to know if there are numbers available comparable to the philosophy ones.

8. **Art**
   June 17, 2020

   She's leaving after "more or less securing" her career? I don't get it. Why not stay in place and work to improve things? (I refuse to open a Facebook account.)

9. **GM**
   June 17, 2020

   It is the same situation in pretty much all other disciplines, but in some of them it is even more messed up because many people would actually be perfectly OK with not being a tenured professor while in the same time the progress of science would greatly benefit from their accumulated expertise (experimental fields in the life sciences and chemistry immediately come to mind). Those people now just get thrown out of the system, and it is not even the case that they can just go to industry, R&D in biotech, pharma and the chemical industry has been increasingly either outsourced or just shut down altogether.

   But if there were permanent positions in which such people could just keep doing their research even if there is no tenure and teaching involved, that would first, keep them happy doing what they love to do, and second, help move science forward. What was all that specialized training for, after all?

   Sounds like a win-win, doesn't it?

   Except that in that sort of system proper wages would have to be paid and that people in such positions would probably not be working 14-hour days if they have families and kids.

   It is much better for the selfish short-term interest of research institutions, and, let's face it, also for the senior scientists who did manage to win the rat race, to have the current system, in which junior scientists spend two decades in a state of constant uncertainty that forces them to work those 14-hour days while being paid between a third and a half of what they would be otherwise, indefinitely delaying starting a family, etc.

   Sounds exploitative? It is.

   It is also a very good mechanism of control on a broader level, because it means that nobody in their right mind will rock the boat until they get tenure, but by that point they have been thoroughly vetted and domesticated and will likely never do so after that either.

   In the USSR a lot of the dissidents came exactly from the ranks of scientists.

   There was certainly a lot to oppose about what unfolded in the US over the last five decades but it has not in fact been meaningfully opposed. Much of the general population was driven to destitution, through mechanisms quite analogous to those in place to exploit junior scientists in academia. And now with the coronavirus we see that for the people on top, the general population is physically expendable too, in numbers in the millions, as long as this serves their selfish economic interests.

   The truth is that scientists not only did not say much to oppose that process, they in fact often supported it. Because when you constantly have to worry about the future and you can be thrown out at any moment, you, first, don't have the time to think about larger issues, and second, it is just not advisable to stick your neck out.

10. **Peter Woit**
    June 17, 2020

    Art,
    She does address this at some length, note the
    "I see absolutely no chance that any of the above will change any time soon."
    She has found a rewarding opportunity to work in a much better environment outside HEP theory. Why should she continue to work in a toxic environment, where everyone takes the attitude "this is how it always has been, nothing can be done"?

    The problem with this kind of environment is not just that good people can't find a permanent place in it, but that many of the best of those who can have other opportunities and leave.

11. **Chris Oakley**
    June 17, 2020

Sabine,

Just in case you are not aware of this, rubbishing most of what is going on in theoretical physics is not the best strategy for getting a permanent job in academia.

But you do it so well that if I was rich enough to have my own institute, you'd be hired straight away.

12. **Peter Woit**
June 17, 2020

GM (and others),
I'd like to insist that people stick to discussing the problems of HEP theory, rather than the inexhaustible question of the more general problems of other fields and the sad state of our society in general. For other fields, which ones provide a model for HEP theory to look to for something better?

I'd love to see some relevant data, but at least for those parts of academic mathematics I'm most aware of, the situation is much better, with full-time junior positions paid fairly well, with some hopes for finding a good permanent job (the adjunct teaching business is where people are mistreated).

13. **LK2**
June 17, 2020

I got a permanent position with a lot of luck. I find very hard to advise a young scientist to pursue this career. Moreover, I see a lot of bright phds leaving academia straight away nowadays. Why? Because industry is hiring physicists and giving them very interesting problems (mainly datascience). We are living in times where some industry problems are more exciting and advanced wrt what we do in a physics dept (quantum computing, machine learning, …). My take on hep theory or phds in general? Reduce the phd positions drastically or state clearly at the beginning what to expect if after graduation one wants to try to pursue a research career. Honestly, after many years in academia, I wish I had abandoned after the phd. Today's fundamental physics research is show-off and fake-grants writing just to "do stuff". Good luck to all the academia job seekers, K.

14. **zzz**
June 17, 2020

>rubbishing most of what is going on in theoretical physics is not the best strategy for getting a permanent job in academia.

"most" is not HEP

15. **Maciej**
June 17, 2020

Although I sympathise with the HEP community because I used to work in the field (not anymore) I don't really see where the problem is. HEP community hasn't produced any new (i.e. beyond the Standard Model) meaningful, testable theory for decades now. Most research papers are still about Susy, Strings, Loops – which are either ruled out already, provide no predictions or are not testable.
Even layman politicians will notice that there is no point in investing much in that area. Clearly many bright researchers will not secure jobs, little of them will. This was always the case, maybe now even more than ever. But I think it is adequate to the fact that there has been literally nothing new for decades.

If someone is complaining about not securing the job due to extreme competition, maybe it is time to change the field. There are plenty of topics that are well funded and equally challenging: quantum computing, artificial intelligence/machine learning, quantitative biology, quantitative finance et.c.

16. **Peter Woit**
June 17, 2020

Maciej,
My impression is that the job situation is somewhat better now than it was back in the 70s, the time of huge breakthroughs in the field as the Standard Model came together. There's no evidence that bad intellectual health in HEP theory causes a bad job situation, but I think it's quite possible that a bad job situation has helped contribute to bad intellectual health.

17. **Edward M Measure**
June 17, 2020

Does it make sense to control the number of PhD's awarded? The number of Postdocs? Or are these cures worse than the disease?

Right now there are excellent job opportunities for physicists, including those with just a BS, in software engineering and data science. For less stress and more job security, they can, if skilled, make 3 or more times as much as they would as a tenure track assistant prof. And the problems might be more interesting.

18. **William Connolley**
June 18, 2020

> We are put in competition with each other from day one, and only very few of us will be given prestigious positions in the end… We constantly need to outshine our peers.

Whilst this is obviously stressful for individuals, it isn't clear it is a problem for the field. Putting people in competition and rewarding only the best sounds like a plausible strategy. I'm not saying it is nice, or fair, or even deliberate, I'm only talking about the overall good of scientific progress. You could perhaps argue that the lack of progress disproves this idea, but then again maybe the work is just very hard. On the other hand, ASM addresses this by saying that the stress makes it hard to do science; I'm not sure that's fully convincing. And of course there's short-term pressure for flashy results, but you get that most everywhere.

19. **Angnis**
June 18, 2020

Dear Peter,

thank you so much for drawing attention to my FB post. I would like to emphasize that, originally, this post was intended only for my family and friends but then some people asked me to make it public so they could share it. I honestly did not expect that it would reach this far but now I am really glad that it did.

From the comments above, I understand that some of your followers are wondering about why I did not stay in academia in order to improve the situation. In fact, this was the hardest part in making the decision to leave. In my last years as a group leader I did my very best to improve the work environment for my team. Unfortunately, I constantly had to justify my actions and decisions to other colleagues. Paying attention to the well-being of my employees, I witnessed severe difficulties to handle family emergencies due to institutional restrictions, mental breakdowns due to stress and even harrassment in the work place. There was no system in place to help me handle these situations and when I tried to talk about this to other professors they always defended the status quo of academia. The only people who shared my point of view were younger and not in influential positions. Most of them were afraid to drive a change because they feared that this would affect their career prospects. In the end it felt too hard to deal with this on my own.

I truly loved physics and my research topic. I never wanted to do anything else and I am sure I would still be there if the environment had been different.

I decided to leave when I got offered a challenging role in a tech startup that tries to truly make a change, not only in its industry but in all of society. (Since some people asked me on FB: you can see what I do now on http://www.ceretai.com) My new team has a mission, we care for each other and for society as a whole. We reflect on our well-being at work on an every-day basis. And, in case your followers are wondering, we get things done without unhealthy pressure and are pioneering a new business area within artificial intelligence.

All the best,

Angnis

20. **Peter Woit**
June 18, 2020

Angnis,
Thanks for your comments. Good luck with the new career path!

My own experience with the work environment for young theorists is from long ago and far away from that of Angnis, but maybe things are not that different now. What struck me most about the environment was not any pressure from senior people, but their general lack of any interest in the lives or activities of their younger colleagues. My impression was that for most senior people, having a lot of grad students and postdocs around was important in order to have a viable research operation going and they had generalized goodwill towards them, but the fact of the matter was that these were people who weren't going to be around the field for long, so not worth paying much attention to. This was very different than the attitude in experiments, where the young were the ones getting the work done, so close attention to them needed to be paid.

21. **Peter Woit**
June 18, 2020

Edward M. Measure,
The reason nothing has changed in 40 years is that no one is going to fund a large increase in the number of permanent positions, so the only way to get to a healthier ratio of young theorists to permanent jobs would be to reduce the number of postdocs and grad students. No theory group wants to reduce the size of its grad student population: for one thing, if the did they university would likely cut permanent positions for theorists. There is a good case to be made for moving resources from postdoc positions to longer-term ones, I've never understood why that possibility hasn't gotten more consideration.

Non-academic job opportunities are pretty good now, but it's always been the case that theorists could generally find such work. I think a lot of senior people in the field take the attitude that it's fine to train lots of young people to do HEP theory, even though there are no jobs for them, because they can find other work. To the extent young people enter the field with the attitude "I know this won't lead to a permanent career, but I'll enjoy learning about his and working on it for a few years", that wouldn't be so unhealthy, but that mostly is not what is going on.

22. **Dmitrii**
June 18, 2020

Peter,

I know corona-virus is a once-in-a-lifetime event, but this year conveys an absolutely horrible impression for the job market situation in Math too.

My advisor told me early on that there would be a hiring freeze for 20-21 applications, so no postdocs for me (and he was right!). He suggested that I might consider deferring till 21-22, but he expects a surge in job market candidates during that year (precisely because of those people who decide to defer).

It is hard to tell at this point, but I expect that the effect of the pandemic on the financing of STEM-fields will be felt for a long-long time, so the market will further squeeze.

23. **Peter Woit**
June 18, 2020

Dimitrii,
You're quite right, the COVID disaster has caused a huge problem for grad students and postdocs who need to go on the job market, in math, physics and most fields. Columbia, like many places, has a hiring freeze in place. I know there is some effort, here and elsewhere, to extend funding for some graduate students because of the situation. This would be an excellent time for foundations like the Simons Foundation to step up and provide funding for students and postdocs who are victims of bad timing.

It's also true that the future is highly uncertain, with the long-term effects of this on university finances and hiring worrisome. Good luck and much sympathy to you and others facing this.

24. **Dmitrii**
June 18, 2020

Peter,

Thank you for the kind words.

Let me mention one more thing with regards to your claims about the relatively healthy situation in math compared to HEP: I heard from multiple senior people that to get a tenure-track from an R1 university a postdoc needs to be publishing about 1 paper every 6 month (of course, this is just an average not taking into account the quality of the paper, which might vary drastically). Given my level of talent, that would likely force me to spend my postdoc years on relatively shallow projects. Such incentives certainly lead to a deterioration of the field, much along the lines of what you describe about HEP.

One some level that is fair that only the most-talented (who can publish big results in a relatively short span of time) stay in the academia. On the other hand, I recall that Peter Higgs said in his interview that he wouldn't have done what he had done in the current publish or perish atmosphere.

I obviously do not have any solutions to offer, I am just sharing my frustration. It makes me sad that I will most likely have to abandon my dream of working as a scientist in favor of a less-stressful and more fulfilling job. It is not even the issue that theorists can get much higher salary in tech, as I am sure most academics do not work on their craft for the money. It is indeed the constant feeling that there is an impenetrable barrier between a young scientist and a tenured faculty member and that they control your fate to a ridiculously high degree (with a wide variety of malpractices coming with this control, such as nepotism, cronyism, discrimination, etc.).

Somebody made a remark above that probably one cannot work in science under such amounts of stress. I have certainly experienced that during this year, where my health (both mental and physical) just started giving up from the increased teaching load because of classes moving on-line, news about the job market, news about the plans of bringing back on-line teaching in the fall, your mentors worrying about their own problems and a general feeling of helplessness. I don't know for sure, but I suspect these problems do not manifest themselves to the same degree in other jobs available to people with such level of skill and education.

25. **David Roberts**
June 18, 2020

On the note of a hiring freeze, my department (mathematics, covering pure/applied/stats) was advertising positions that were empty when the university implemented a blanket hiring freeze. We have permanent people retiring/moving on this year, and we still can't fill *their* positions, even though it's cost-neutral (or even will save money). We are cutting subjects we would normally teach to even just maintain a sensible workload. Hiring ECRs is a looong way down the list.

26. **Peter Woit**
June 18, 2020

(All: after some Googling, I figured out that the "ECR" David Roberts is referring to is an "Early Career Researcher").

Dmitrii,
My comments about math being in better shape than physics shouldn't be taken as implying that the situation of academic jobs for young math researchers is an easy one, just that the situation in theoretical physics is significantly worse. And, in both fields, even those who end up successfully with a permanent position are put through years of stressful uncertainty and difficult relocations, of a sort those in non-academic positions generally don't have to put up with. Of course this is much worse than normal this year.

In math like in all of academia, hiring can involve a lot of unfairness and not necessarily reward the best work. I should say though that in all my years in the math department at Columbia, I've seen a lot of hiring decisions being made, and almost always felt that they were being made honestly and as fairly as possible. Especially in recent years, this has been very competitive. Like most places, the main in some sense unfair criterion is that people want to hire others working in areas close to their own, doing things they recognize and understand. Within this constraint, I haven't seen much emphasis on counting papers or citations, rather people want to see that a candidate has done some piece of work that impresses them. It is true that everyone's own expertise is limited, so there's a lot of reliance on letters from experts in the candidate's field.

27. **Robert**
June 22, 2020

Of course, I would love it, if people knew well before their 40's birthday what their long term job perspective would be (and for myself it luckily worked out, I have a permanent non-professor position in a strong physics department, so maybe my view is biased).

But that said and as I have already commented on FB, I don't think it's a fair portrait of the post doc years to paint them as only suffering to secure a tenured position. That would be horrible and everybody who perceives it like this should better leave today and find something more rewarding (luckily enough such positions exist for many HEP people). When I was a grad student and later a post-doc, I went to the office not because it was required of me for my career path, but because I enjoyed so much what I was doing! I could do what I liked most and was even paid for it. So I don't think it is a waste of time, even if you leave academia/physics/maths eventually. Even with the slightest quantitative understanding, you should realise that the number of PhD supervised by one professor means that not everybody will end up being a professor if the system is at least somewhat stationary. So that should not be your (only) motivation. You should do it for yourself and while doing it acquire quite a few skills that will also help you later in your non-academic career (which most people do since the jobs they end up in were mostly not available to them right after graduation).

Yes, moving to another country every two years building up a new social environment is really tough (and not well compatible with forming a family for example) but for a few years, it can still be worthwhile as long as you get enough out of it (in the present and not only in a hypothetical future).

28. **Robert**
June 22, 2020

Ah, and what I forgot: Choosing research topics (in plural hopefully) your of course should do what you are really interested in and where you can contribute. But you should be aware that if you mainly focus on niche problems that's quite a bet as you will later need to convince people that are supposed to hire you that what you achieved is interesting (taking into account their own reference frame for "interesting"). I am not saying, don't do it an only follow the stampede, but your results in your area should be well visible from without that area if your peer group in you area is not so big.

29. **theoreticalminimum**
June 22, 2020

Robert,
I see that you are the manager of a masters degree programme (the TMP) in Munich. The TMP website features a page of all those (~300) who have written a thesis in the programme. Maybe you could create a sort of wiki and invite all those who have graduated from the programme to share a brief of their timeline since graduating from the programme, I think this could help those students considering joining the programme have an idea what those who've graduated from it have gone on to do (e.g. TMP 'xx (advisor:) -> PhD (year completed: … ; advisor: … ; institution: …) -> n Postdoc's (institutions) -> etc.). I can see how such a huge database can be helpful to those condering applying to the programme, and later thinking of where they could go after completing the programme.

30. **Robert**
June 25, 2020

theoreticalminimum I agree that this would be something nice to have. Unfortunately, Organization of this program is essentially a one man (me) show and tracking everybody over the years is a considerable amount of work (I try to have a current email address of the alumni and even that is difficult over longer periods of time as those are often tied to institutions and this is what the original post is about; I decided to get my own domain exactly at the point when I started my first postdoc and I had to email everybody in my address book that mail email address was about to change once more from being hosted at my graduate school to my new university). What I can say is that after graduating from this master program (Americans: here in Europe, MSc is really a separate degree before you start your PhD research and not part of grad school or even the drop out of grad school option) almost everybody who intends to do a PhD — the vast majority — finds a good position to do so and the other usually don't because they have found something "better" in industry. On the other end, roughly 10 years after we had the first graduates from our program (we started with 7 students in 2007 who graduated two years later) we start seeing the first ones in permanent or tenure track (type) positions.

HEP News

June 18, 2020
Categories: Experimental HEP News

The CERN Council is meeting today and tomorrow, and should approve the long-awaited 2020 update of the European Strategy for Particle Physics. There will be a live webcast of the open part of the Council meeting on Friday.

My understanding is that the most difficult and contentious decision, that of how and whether to go forward with a new energy frontier collider, has been put off until 2026, when there will be a new update. In the meantime, design work will emphasize studies for the leading contender: a new large circular electron-positron machine. Studies of a linear collider design (CLIC) will continue at a reduced rate. New work will begin on the possibility of a muon collider, as well as other advanced accelerator technologies that might someday be usable.

There will be some move in the direction of the US program, which has abandoned the energy frontier, including more participation in the US and Japanese neutrino programs. A "scientific diversity program", Physics Beyond Colliders, will receive new support. This program will try and come up with new experiments that don't require a new energy frontier machine. For more about it, see this CERN report and this article in Nature.

In other news from CERN, work on the LHC should start resuming this summer, with the ongoing LS2 extended by a few months because of the COVID shutdown, so beams back in the LHC late next summer. There likely will be no significant new data coming from the LHC during 2021. The extended shutdown may provide the time for magnet quench training needed to bring the machine to its design energy of 7 TeV/beam.

Update: The CERN strategy report is here, also see here, here and a press release here. There is press coverage here, here and here.

The headline news is that this backs the FCC plan: a 100km new ring, first run as an electron-positron collider, then as a much higher energy proton-proton collider. There are however a whole bunch of very significant caveats:

- No plan for how to finance this very expensive proposal.
- The press release mentions a construction start timescale of "less than 10 years after the full exploitation of the HL-LHC, which is expected to complete operations in 2038". This is twenty years or so away, a very long time.
- The main near-term goal mentioned is work on designing the magnets needed for the proton-proton machine, to know by 2026 whether a pp machine is feasible. If the design of appropriate magnets with an acceptable cost for the pp machine is not possible, the implication is that there would be no point in building the large ring and ee machine.
- The main competitor to the FCC plan, CLIC, is not at all canceled, but work will continue on it.
- A new project to try and design a muon collider will be funded, with a planned 2026 decision about whether to move forward on a test facility for that. The technology for this still does not exist (muons decay very quickly…) but if such a collider were feasible, it would be much smaller and likely much cheaper than something like the FCC project.

So, those who want to argue one way or another about whether it's a good idea to spend a lot of money on building a new collider should rest assured that the future holds many, many more years in which to conduct such arguments…

Update: I find it very frustrating to see that the online discussion of this is dominated by a pointless argument about whether, as reported, CERN should be going ahead and spending more than \$20 billion or so on a new machine. THEY ARE NOT DOING THIS. What has happened is that, after a lot of work, they have identified the best possible way forward at the energy frontier (the FCC proposal) and decided not to go ahead with it now but to keep studying it and the required technologies. If the cost of this proposal had been a few billion dollars, they likely would have tried to come up with a plan to allocate much of the over billion \$/year CERN budget in future years to the project and start construction. Instead, for the next six years they are allocating .1 – .2% of the CERN budget to further studies of the proposal. Those who have been loudly complaining that this is too expensive a proposal for the HEP community to afford should declare victory, not go to war over this.

Update: The CERN press release has been changed, with "construction" starting within ten years after 2038 changed to "operation" starting within ten years after 2038. This makes more sense, the earlier version seemed absurdly far in the future. My understanding is that the current plan is essentially to put off to 2026 a decision about going ahead with FCC. By 2027 the HL-LHC will be in place, freeing up some money for a new project, possibly the FCC. A 2027 start to FCC construction would allow a start of operations within ten years after the 2038 HL-LHC end date.

Update: Adrian Cho at Science magazine has a report on this that gets it right, headlined European physicists boldly take small step toward 100-kilometer-long atom smasher. It includes the crucial:

> However, CERN Director-General Fabiola Gianotti emphasizes that no commitment has been made to build a new mammoth collider, which could cost $20 billion. "There is no recommendation for the implementation of any project," she says. "This is coming in a few years."

Comments

1. **DB**
   June 19, 2020

   … so we will have many, many more years of Arkani-Hamed spending a lot of time talking about this issue.

2. **Peter Woit**
   June 19, 2020

   DB,
   Arkani-Hamed seems to have turned into a mathematical physicist searching for a replacement for quantum mechanics and GR, so maybe he won't be talking about this so much. He was involved in the Chinese version of this kind of proposal, it may be that will be the one that has some hope of happening in our lifetimes, so attention will move to that. I have no idea what its prospects are.

   On the CERN front though, arguments about this are effectively now moot until 2026, since there's no plan for them to do anything other than to continue to study the possibilities until then.

3. **DB**
   June 19, 2020

   Yes Peter,

Nima has moved over to other things. His ideas about all his "hedrons" seem very interesting, but only time will tell if he's on the right track. No other top theoretical physicist seems to be following that road. So patience is going to be of the utmost importance here.
He was considered to become the director of that Chinese collider, but I'm not sure if that will come to pass either. As you know, the world economy is heading towards a great reset, and I guess that something like that huge collider might well be hanging in the balance at the moment.

And yes, sure. Re the CERN, 2026 is the new date to be paying attention to. The HEP world is moving very slowly, as expected. No surprises there.

4. **vmarko**
June 19, 2020

Any mention of the Chinese collider in the CERN report?

🙂
Marko

5. **Peter Woit**
June 19, 2020

vmarko,
The existence of the CEPC proposal is mentioned, as well as the fact that it's similar to the FCC proposal. Another good reason to put off a decision on FCC to 2026 or so is to see what the Chinese will do. If they go ahead with CEPC, I doubt anyone in Europe will want to try and raise the money for a second similar project.

6. **Gianni**
June 20, 2020

Peter, the quoted report on 'Physics Beyond Colliders' (on arxiv) states on page 7

"more questions than ever remain open."

The report provides exactly three questions: dark matter, dark energy and the baryon-antybaryon asymmetry in the universe.

These three questions are then summed up by stating that there is "exceedingly convincing evidence that there must be Physics Beyond the Standard Model". (All this is on page 7.)

It seems hard to imagine that these three arguments will be sufficient to build a new machine. The arguments are much weaker (but also much more honest!) than they were before the building of the LHC.

We are indeed in an unprecedented situation in high-energy physics: many researchers and possibly little to discover. The situation is extremely difficult to manage, especially for policy makers. I think it is essential to acknowledge this difficulty. There do not seem to be easy solutions. A careful approach, like that of CERN, seems appropriate.

7. **I**
June 20, 2020

One reason to build a collider after the LHC is the massive loss of technical experience and knowledge needed to make one. But it seems plausible that alternative projects listed in the report, and elsewhere, would foster the same expertise. Do you (dis)agree Peter?

8. **Peter Woit**
June 20, 2020

Gianni,
I think the new strategy document does get the main physics case right: the Higgs field remains a central mystery of fundamental physics. The LHC has done a lot to study it, but a Higgs factory able to study it in much greater depth is the logical next step for the field. The problem is whether there's an affordable way to do this, or whether we should just give up, decide that it's not worth it to find out more about this fundamental aspect of reality. I wrote about this in detail here
https://www.math.columbia.edu/~woit/wordpress/?p=10768

The FCC proposal seems to me an honest attempt to lay out what the best way forward is, but the scale and expense of it is highly problematic. The fact that there was no decision to actually move forward on the project, that the decision is being put off for another 6 years, seems to me an acknowledgement of this.

I,
Yes, there's a good case being made that one reason to not completely give up and stop research at the energy frontier is that once you do this, it will be hard to impossible to restart, since all the people with relevant expertise will be gone. Ideally, some smaller scale, less expensive projects can be found to keep the field active. There's clearly a lot of hope in some quarters that for instance a muon collider project will turn out to be feasible. That would use current expertise, while requiring also the development of new technology and solution of new problems.

9. **tulpoeid**
June 21, 2020

"THEY ARE NOT DOING THIS. After a lot of work, they have identified the best possible way forward at the energy frontier and decided not to go ahead with it now."

After reading the press release that's the impression I got. Then, the next day I read some media headlines and articles and was surprised to see that I had completely misunderstood CERN's statement. Eventually the quoted clarification just made me feel better about my wits, and once more totally pessimistic about how science is reported these days. (Can't we get even things like this straight anymore?) (Can it be that the relevant articles were written ahead of time based on rumours and then nobody noticed the difference?)

10. **tulpoeid**
June 21, 2020

Gianni,
this report is for the opposite of "building a new machine". It's a program literally for physics beyond colliders; so the arguments might be differently weighted than for the case for a new collider.

11. **Gianni**

June 21, 2020

Peter,

It seems to me personally that a high price is worth investing for the final theory, and surely 20 000 million. But still I remain worried. The Nature paper you quote, based on the CERN report arxiv.org/abs/1901.09966, presents eleven options for physics beyond the standard model. They also claim that this represents the *phenomenology* of *all possible* extensions to the standard model. The options are based

– on dark photons,

– on light dark matter particles,

– on millicharged particles,

– on Higgs-mixed scalars,

– on heavy neutral leptons,

– on axion-like particles.

Do we really think that there is a chance to find something on these fronts? I must admit that I am sceptical. In my view, the first question to settle is: are we looking everywhere possible? Or more specifically: did the authors miss some option?

12. **Peter Woit**
June 21, 2020

Gianni,
Sorry, tulpoied is right, I missed that you were referring to that "Physics Beyond Colliders" report, which is about non energy frontier possibilities. These are much much cheaper, may make sense to do them even though quite unlikely they'll find something. There may very well be quite a few other such possibilities beyond the ones they mention.

tulpoied,
I think this is somewhat CERN's fault: they emphasized the decision to concentrate on the FCC proposal, hoping I think to generate enthusiasm about it, with the fact that they hadn't actually made the decision to go ahead with this (and had no idea how to pay for it…) deemphasized. The journalists writing articles were likely writing them based on early access to an embargoed version of the press release.

One thing that makes much clearer what is going on is the detailed spending plans in CERN's proposed new medium term budget plan for the next 5-6 years, but I don't think this is a public document right now.

13. **Geoffrey Dixon**
June 21, 2020

Not that my opinion fucking matters, but Gianni's skeptical list of places to search is very telling:
– on dark photons,
– on light dark matter particles,
– on millicharged particles,
– on Higgs-mixed scalars,
– on heavy neutral leptons,
– on axion-like particles.
Every one of these entails shoehorning unimaginative dross into a theoretical architecture to which the mainstream has been addicted for decades – since Dirac and Feynman – and in none of these cases -NONE – should any of these chewing gum fixes succeed to any limited extent – will the community of theorists loudly proclaim: of course, this was inevitable. I know you won't publish this rant, but this is why I'm strongly opposed to building another collider anywhere, whatever technological expertise we may lose in waiting. The lack of imagination behind the proposed endeavor is staggering. Gianni was being far too polite in casting shade on his list of suspect motivations.

14. **Amitabh Lath**
June 21, 2020

If you are not at the energy frontier, you are doing chemistry….not that there is anything wrong with that. That was the advice of an eminent theorist when I was considering different subfields of (experimental) particle physics. He was not wrong. If we don't build the FCC (or CEPC) the field will still do good work, map out the higgs potential, measure the neutrino mixing angles, etc. But it will not be frontier physics.

I do not think $20 billion over a couple of decades is such a big deal, especially when you think about how much will come back into the economy as jobs and taxes. Sabine Hossenfelder has an article in Scientific American arguing against the next collider. The sub heading is: "the money could be better spent researching threats like climate change and emerging viruses" which frankly is the most naive thing I have ever read. Does she really think national and international economies work like that? How much of a bump did non-high-energy fields get when the SSC got cancelled?

15. **Peter Woit**
June 21, 2020

Amit,
I basically agree with you, although I see "mapping out the Higgs potential" (in the sense of measuring Higgs self-interactions) as energy frontier physics, one of the reasons for an FCC-ee and FCC-hh.

I also think the way numbers are being thrown around is problematic. I can't find where the "20 billion euro" number came from. The numbers I have seen are 10 billion dollars for the FCC-ee, another 17 billion for the later FCC-hh. As they have been for the past couple years, people are loudly arguing about numbers in a way completely disconnected from the crucial questions of exactly what those numbers would buy and how they would be raised. The immediate question to me seems to be "can 10 billion dollars be raised to pay for a tunnel + FCC-ee?" Until there's a plan on the table for how to do that, the argument is too disconnected from reality to be worth having.

16. **Niclas**
June 22, 2020

Carlo Rubbia has been arguing strongly for the muon option here in Europe. He says it is very realistic and basically the technology can be worked out. Assuming the

Chinese go the FCC route then a muon factory could be good for CERN with a lot of opportunity for discovery.

17. **Peter Woit**
June 22, 2020

Geoffrey Dixon,
You're making the same mistake I originally did. That list of things to look for that Gianni refers to is from the "Physics Beyond Colliders" report, written by the group looking into possible projects other than a new collider. I agree this is an extremely unlikely and sad list of things to look for, but it's what the future of the field looks like if you don't build a new collider, not a list of what you could look for with a new collider.

18. **Geoffrey Dixon**
June 22, 2020

Feeling less cranky now. I agree that Sabine's suggestion that the money could be more fruitfully spent making the planet able to support more humans is unrealistic. And although I am not immune to the occasional bout of dudgeon regarding trends in high energy physics, as regards its place among all the things humanity gets up to, it is – relatively speaking – an idyllic oasis. A shining city on a hill, as it were … and keeping it shining, and keeping physicists employed in pursuit of some lofty (preferably not misdirected – but even if misdirected) goal … that to me provides some solace, and quiets the inner Thanos. As to Sabine's suggestion, I am reminded of JFK's moon speech ("and do the other things"), and although JFK evidently didn't really care about getting to the moon, it was a good speech, and relevant to this discussion. WTF, build it (or them), for if we do not, we lessen our reason for existing.

19. **Peter Woit**
June 22, 2020

Niclas,

Here's my prediction for the future (assuming no new developments from theory or experiment changing the HEP landscape). In 2026-7 CERN will decide to go ahead with a muon collider test facility. The cost of this will eat up the available funds (100 million or so dollars/year) freed up by the ending of HL-LHC construction expenses. The FCC project will remain a goal for the future, with studies continuing, but no plausible funding plan.

By the way, Alessandro Strumia has a negative piece about the FCC as a guest post here
http://backreaction.blogspot.com/2020/06/guest-post-who-needs-giant-new-collider.html
As he himself points out, he has personal reasons for not being a fan of CERN, so you might want to take that into account when reading him. His point of view I think is not uncommon though among theorists, many of whom have decided they're pretty sure there will be no new physics at FCC-accessible energies.

My advice remains that one should not be paying attention to what theorists think about this, given the sad history of theorists promoting the LHC as a machine that would produce SUSY particles and black holes. They were completely wrong about this, could very well be completely wrong again with the no new physics prejudice. If the experimentalists and machine builders are able to get behind a viable project, that should be supported.

20. **Gianni**
June 22, 2020

Yes, the list from "Physics Beyond Colliders" is indeed for low-energy experiments. I searched for a similarly long report on physics with a new collider, but could not find one. Does anybody know a long report from CERN with arguments in favor of the 100TeV machine or with a list of experiments to be done with it?

21. **Amitabh Lath**
June 23, 2020

Peter, it is not sensible to talk about the cost of a mega project like a new collider as if it were an off-the-shelf item like a new microscope. A lot of the money will come with strings about where it will be spent. A lot of it will be "in kind" esp. manpower and software from developing countries. These should be considered infrastructure and/or stimulus spending.

Let's say the FCC gets power supplies and digitizers from CAEN. This is the Italian/INFN contribution. The understanding is that CAEN will build design and manufacturing facilities in a depressed region in the south. The hope is the technicians and engineers spark a tech boom. The Italian govt. is not simply writing a check.

The effect is even bigger in developing economies, think of Pakistan designing and building the CMS tracker alignment and barrel yoke, or India supplying superconducting corrector magnets and precision positioning jacks for the LHC.

We need large, ambitious, technically difficult projects to strive towards. It keeps us from turning into navel-gazers concerned only about our own conveniences. JFK understood that of course.

22. **Alessandro Strumia**
June 23, 2020

Actually I was not much interested in staying at CERN precisely because high-energy physics, as we knew it, is over. No new physics at LHC and no new collider in the next decades makes big change unavoidable.

23. **Low Math, Meekly Interacting**
June 23, 2020

What I am struck by most is the temporal scale. If the FCC is ever built, a significant fraction of the scientists involved in planning it (and taxpayers paying for the effort) will never live to see it completed. The EU could dissolve in the interim. Western Civilization may be obsolete before its finished. The Great Pyramid of Khufu is a mid-scale capital improvement by comparison.

Increasingly I'm in the sustainability camp. Maybe building a muon collider is not feasible, but it seems like a more worthwhile bet. I want humanity to explore the energy frontier, and I don't need to be persuaded to support it. But my faith in the durability of the institutions needed to see such a project to a fruitful completion has been severely shaken in the past few years. New technology might be the answer.

24. **A reader**
June 23, 2020

To Alessandro Strumia: "because high-energy physics, as we knew it, is over." With all due respect, is yours a scientific statement? Confidence level? Please do not let your personal hate, your personal agenda and your sour grapes help destroy HEP. The same applies to Sabine Hossenfelder. As a humble physicist, and a long term reader of this blog, I completely agree with Peter Woit's view: "If the experimentalists and machine builders are able to get behind a viable project, that should be supported."

25. **Peter Woit**
June 24, 2020

Amit,
It's exactly because the sources of financing for a new collider will be a complicated story that I don't think now is a fruitful time for arguments over that financing. Because people don't have anything specific to argue about, what you end up getting is "we shouldn't build a new collider, no matter how little the cost", and "we should build a new collider, not matter how much it costs" arguments, which seem to me disconnected from reality.

26. **Alessandro Strumia**
June 24, 2020

Dear reader, I would like to see a 100 TeV pp collider, but we reached the point where the usual "positive" view backfires and repels people like Sabine. You worry about people who discuss problems, I worry about problems. For example, do you expect that creative smart young students will enter a field that might produce discoveries with 10K authors when they will be near retirement, unless everything stops earlier?

27. **A reader**
June 25, 2020

Dear Alessandro Strumia, Sabine Hossenfelder is 1 (one) very talkative physicist with access to mainstream media platforms she very much enjoys using to support her personal views. Not all, but some of what she says sounds ideological, absolute views accompanied by (sometimes understandable) rage. I have read Peter Woit's blog for longer than a decade, so I know where that rage comes from and I can understand it. But hers are opinions, and when they are about the future I can't see confidence levels attached to them. I'm not that old, but I am definitely too old to like ideology. I much prefer sane pragmatism.
One of my best teachers, a very good man, old school, he taught us the basics of QFT, his view, I completely agree with, was that EXPERIMENTAL HEP is like an epic endeavour undertaken by an extremely large orchestra. It is astonishing that mankind (mankind!) managed to pull it off (more than once!). Think about the whole history of EXPERIMENTAL HEP. The women and the men whose commitment made all those GIANT achievements possible. I mean, we descend from apes! Much is dysfunctional with HEP THEORY, but why this eagerness to destroy ALL HEP? ALL!?!?
And to reply to your question, yes, I do believe there are still creative smart young students around who are romantic enough to be willing to be part of this epic and dedicate decades of their lives to it. Much of our world is grotesque cynicism and despair. I still remember the intense thrill to be part of a large collaboration at FNAL as a summer trainee, for a very short time, a couple of months, a long time ago, as a 3rd-year undergrad. There was some very special atmosphere there, so special I can still remember it vividly 20 years later, and that "magic" I believe it was that there was some epic going on there. So yes, I do think there are still creative smart young students willing to be part of some epic, in this very ugly time in human history, even if that epic is going to take 40 years of their lives. Just be upfront, state it clearly to them with honesty and without lies.

28. **Alessandro Strumia**
June 25, 2020

Dear reader, FNAL now left the high-energy frontier, because higher energy needs more money until it's no longer sustainable. I remember a similar atmosphere once upon a time in CERN: Soviet-style buildings and first-class physicists. They now got Renzo Piano building, but the epic is over. Buffalo Bill's show is no longer Wild West.

29. **Amitabh Lath**
June 25, 2020

Alessandro Strumia, as Peter concurs above, money is not the issue. The world economy is fully capable of building multiple energy frontier machines. Nor is the interest of young students: every year we get applications from amazing students and can only accept a fraction of them. Of course some turn out to be impatient and want earth-shattering discoveries now dammit! and show no interest in staying. But a core group realize they are part of an epic slog that is generations long.

Think of the bevy of e+e- machines in the 80's that failed to find the top. Were they failures? Of course not. Nature placed her secrets as she wished, not so humanity could make a new discovery every decade.

30. **Gianni**
June 27, 2020

Peter and all,

This is what I found searching. The CERN paper "FCC Physics Opportunities", published in Eur. Phys. J. C 79, 474 (2019), written by hundreds of authors, defines three aims of the FCC: (1) the search for unexpected phenomena around the Higgs and the electroweak gauge bosons, (2) the search for new particles, including dark matter, and (3) the search for "tiny deviations" (they call its this way themselves) from the standard model.

In my opinion, the real question seems to be: will the FCC just confirm the standard model or will it find something new?

We need to be very honest about answering it. Are we reasonably sure that there is something new to be discovered at higher energies? Would we put our hands into fire for our own answer?

31. **Alessandro Strumia**
June 27, 2020

Dear Amitabh, now we are a few years after the LHC peak of interest, and we still have work to do. If nothing new is discovered, the issue I mention will develop in due time.

32. **Peter Woit**
June 27, 2020

Gianni,
This is the heart of the problem: there is no strong theoretical argument for new physics accessible to a new collider, and that's why you are seeing some theorists arguing against building one. The counter-argument is that you do science by testing theoretical arguments, not by just accepting them. An analogy some people are making is that, pre-1998 and to this day, there was and is no good theoretical argument for a non-zero CC. So, why go look for one?

What makes the collider issue difficult is the high cost. If looking for a non-zero CC cost over ten billion dollars, I bet it would never have been done. I don't think it makes sense to decide not to build a collider just because theorists don't expect it to find something new. Deciding not to build such a thing will be a decision to give up on this fundamental field of science with a long and distinguished history, because it's not worth the expense to test the theory at this new higher energy scale. This is a question of values and I come down on the side of those who think it's worth the likely expense. I also though think this debate over values is a sterile one until there's a definite plan for how to pay for this on the table.

33. **Paolo**
June 27, 2020

From the ITER & SSC lesson, 10 years -> 20 years and \$ 20 billions -> \$ 40 billions; some peoples do live in alternative reality.

34. **Peter Woit**
June 27, 2020

Paolo,
My impression is that CERN has a fairly good track record for completing large projects at more or less the projected time and cost. In any case, even in your bad case scenario, the fundamental problem "where are you going to find 2 billion/year?" doesn't change, just goes on for more years…

35. **Peter Shor**
July 3, 2020

Delaying the decision is clearly the correct decision. Suppose that in the next ten years, evidence emerges for interesting new physics somewhere in the neutrino sector, which could only be investigated with some kind of $5 billion neutrino factory (this is, of course, wild speculation). Then if CERN had already committed to the FCC, the neutrino factory would likely end up being built elsewhere, and CERN would be spending tens of billions to be in the rearguard of new discoveries in HEP physics.

36. **LK2**
July 8, 2020

I was hit by the "reader" comment about the magical atmosphere at FNAL at the Tevatron times. It was really true and I also lived that as a student. There you had the feeling that something was really going on. As a person who stayed in exp. particle physics, I see this feeling completely lost in gigantic collaborations, endless meetings, true work done only by students while the others are sitting in meetings discussing budgets, schedules, reports. That's the way it is now, fine: I just decided to work on smaller scale experiments for having more fun. I still would love to see a new energy frontier project launched in the future: as physicists, we have to chart uncharted territory. What P. Shor says is also very relevant: better carefully look at the direction before committing. The muon collider option at least would be a big novelty also from the technological point of view. And handling dense muon beams can be useful also for a neutrino program!

What is “Spin”?

July 23, 2020
Categories: Uncategorized

The explanation for the lack of blogging here the past month is mostly that I haven’t seen any news worth blogging about. It took only a little bit of self-control to not do things like make snarky comments about recent conferences on string theory and quantum gravity.

Today I noticed a discussion on Twitter of the perennial question about what “spin” means in quantum theory, with some of the tweets included this highly appropriate meme:

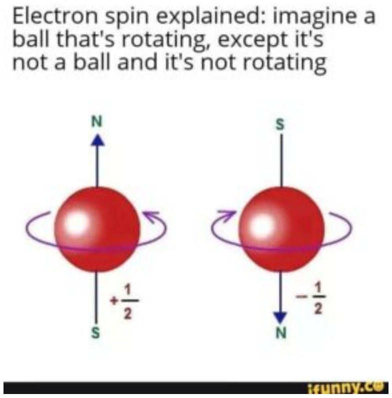

I thought it might be worth while to make a stab at explaining what “spin” really is. For a much more detailed version, I wrote a book. But this post is much shorter…

Picking a particular point and a particular direction (say the z-direction), the angular momentum J_z is defined to be the “generator” of rotations about that point, around the z-axis. This means that when you do such a rotation by an angle θ, for any observable (function of position and momentum) F
$$\frac{dF}{d\theta}|_{\theta=0}=\{F, J_z\}$$
where the bracket is the Poisson bracket. A short calculation shows
$$J_z=r_xp_y-r_yp_x$$
which is often given as the definition. J_z is itself an observable, which you can say is the angular momentum about the z-axis of a point particle with x,y coordinates of its position and momentum given by r_x,r_y,p_x,p_y. In classical physics J_z can take on any values.

In quantum mechanics, observables are operators acting on states, and J_z becomes the operator $\widehat{J}_z$ which (with an additional factor of $-i$ to get unitary transformations) generates rotations on states. This means (using units such that $\hbar=1$)
$$\frac{d}{d\theta}\ket{\psi(\theta)}=-i\widehat J_z \ket{\psi(\theta)}$$
You can solve this differential equation and see that if you rotate a state by an angle θ about the z axis, you get
$$\ket{\psi(\theta)}=e^{-i{\widehat J}_z\theta}\ket{\psi(0)}$$
States that are eigenvectors of ${\widehat J}_z$ are supposed to be the ones with a well-defined value of the classical observable J_z, given by the eigenvalue.

One finds experimentally that the observed values of J_z are given by
$$\frac{n}{2}$$
Unlike the classical case, as expected this number is quantized (that’s why they call it quantum mechanics…), but the factor of 2 is unexpected. Since a rotation by 2π should bring the state back to itself, one expects that
$$e^{-iJ_z2\pi}=1$$
so J_z should be an integer. If one finds a state with $J_z=\frac{1}{2}$, rotating it by an angle 2π changes its sign. This is weird, but the sign of a state isn’t itself something you can measure.

Looking more closely at the operator $\widehat{J}_z$ for quantum systems, one finds that for some states it has exactly the same relation to position and momentum as in classical physics
$$\widehat{J}_z=\widehat{r}_x\widehat{p}_y-\widehat{r}_y\widehat{p}_x$$
When states are given by a wavefunction depending on spatial coordinates, one can show that this is just the expected action by infinitesimal rotation of the spatial coordinates. In this case rotation by 2π doesn’t change the state, and J_z has integral (not half-integral) values.

For many quantum systems though, there is an extra term:
$$\widehat{J}_z=\widehat{r}_x\widehat{p}_y-\widehat{r}_y\widehat{p}_x+\widehat{S}_z$$
and it is this extra term $\widehat {S}_z$ that is the “spin” observable. When states are given by wavefunctions, what the equation above is telling you is that when you act on a state by a rotation, you get not just the expected induced action from the rotation on spatial coordinates, but also an extra term. A natural guess is that, as in the meme, a point particle is really a ball of some new stuff, with $\widehat {S}_z$ the effective extra term caused by the positions and momenta of the new stuff.

For an elementary particle such as an electron, experimentally one finds that $\widehat S_z$ has eigenvalues $\pm 1/2$, which explains why one sees half-integral quantization. As the meme says, there is no viable physical model of rotating stuff that would give this result. Something very different is going on.

So far I’ve stuck to talking about rotations about the z-axis, but one also should consider rotations about other axes. The problem is that more sophisticated mathematics is needed, since the generators of rotations around different axes don’t commute (doing the rotations in the opposite order gives a different result). The mathematics needed is that of the representation theory of the rotation group $SO(3)$ and its double-cover $SU(2)$. From this representation theory one learns that the only consistent possibilities are given by putting together copies of a “spin n/2” representation for $n=0,1,2,\cdots$. These are $n+1$-dimensional vector spaces, on which $\widehat{S}_z$ acts with eigenvalues
$$\frac{-n}{2}, \frac{-n +2}{2},\cdots,\frac{n-2}{2},\frac{n}{2}$$
The case $n=0$ is that of $\widehat{S}_z=0$, and the simplest non-trivial case is the $n=1$ case which gives $\widehat{S}_z$ for the electron.

So, the “spin 1/2” characteristic of the electron is something completely new, unrelated to anything in classical mechanics. If you describe the electron by a wavefunction, it will take values not in the complex numbers, but in pairs of complex numbers, with rotations acting on the pairs by the spin-1/2 representation (also known as the “spinor” representation). Besides the non-classical physical behavior, the geometry is also non-classical, with the spinor representation something that cannot be described by the usual formalism of vectors and tensors.

Another reason I haven’t been writing much on the blog this past month is that I’ve been working on writing up something about twistors. I’ll write about twistors in detail here when this is done, but one thing they do is give a picture of space-time geometry in which spinors are fundamental, not vectors. A fundamental idea of twistor theory is that a point in space-time is a complex two-plane inside complex four-space. In twistor theory the answer to the question of where the spinor degree of freedom at a point comes from is tautological: the two complex dimensional spinor degree of freedom at a point IS the point.

Bonus link for those who have gotten this far. A presentation by CERN director Fabiola Gianotti, which comes off a bit differently than news reports saying CERN is going ahead with FCC. On page 5

- Strategy gives a direction for future collider(s) at CERN (FCC). Prudent: feasibility study first.
- Intensified accelerator R&D to prepare alternatives if FCC feasibility study fails.
- No consensus in European community on which type of Higgs factory(linear or circular).

Page 9 lists three "first priorities" for the feasibility study:

- find funds for the tunnel
- no show-stoppers for ~100 km tunnel in Geneva region
- magnet technology ; how to minimise environmental impact

Comments

1. **Prof. David A. Edwards**
July 23, 2020

In order to help motivate my calculus students I often show how calculus can be used in sports; this actually often works. In particular, I often describe the motion of a curve ball in baseball. The configuration space of a baseball is R^3xSO(3) and its associated phase space is T*(R^3xSO(3)) (the cotangent bundle of R^3xSO(3)). SO(3) is too complicated to describe to Freshman. So, instead, I use a simpler, though adequate, model of a baseball as a point particle with extra spin angular momentum. Now the phase space is T*(R^3)xS^2. The dynamical equations can now be set up and numerically solved and graphed on a graphing calculator. One can then watch the ball curve! The phase space (S^2,v) (use the canonical area 2-form v obtained by thinking of S^2 as the standard unit sphere in R^3) is not the cotangent bundle of any configuration space. For any s in R+, T*(R^3)x(S^2, s*v) provides a classical phase space of a point particle of spin s. Using Geometric Quantization (see: http://www.amazon.com/Geometric-Quantization-Oxford-Mathematical-Monographs/dp/0198502702/ref=pd_bbs_sr_1?ie=UTF8&s=books&qid=1219711387&sr=8-1), one can quantize this phase space for s satisfying appropriate integrality conditions and obtain the usual quantum theory of spinning particles. For all s one can deformation quantize (see: http://www.amazon.com/Deformation-Quantization-Mathematicians-Mathematiques-Irma-Lecture/dp/311017247X/ref=sr_1_1?ie=UTF8&s=books&qid=1219711918&sr=1-1
) this phase space and get something that is probably also interesting!

Thus, Pauli was wrong! There are reasonable classical limits of spinning point particles.

2. **DB**
July 24, 2020

Hi Peter,
listening to David Gross giving the final lecture on Strings 2020, it seems clear that he has surrendered to the fact that there are certain parameters that might not be calculable, and that they are just an "accident". He might have come to the conclusion that his "never, never, never, never surrender" idea is wrong, and that things are tilting towards the anthropic principle/landscape idea. It might be worth of a comment… snarky or not.

And Witten's answer to Vafa on the 40th and 41st minute on the latter's talk re the completeness hypothesis might be willing of another comment too.

I think both comments seem to point in the same direction, that things are getting pretty murky when it comes to try and find a unique theory of the vacuum that describes our world.

3. **Arnold Neumaier**
July 24, 2020

"the "spin 1/2" characteristic of the electron is something completely new, unrelated to anything in classical mechanics"

Spin is not as quantum mechanical as you make it appear here. Indeed, the coadjoined orbits of the Poincare group provide phase spaces (Poisson manifolds) for classical particles with spin – for positive mass a 2-parameter family parameterized by mass m and continuous spin s. Upon quantization, the coadjoined orbits turn into irreducible unitary representations, and the spin gets quantized in the same way as angular momentum gets quantized when quantizing its representation on the sphere. Nothing more than geometric quantization is involved.

4. **Peter Woit**
July 24, 2020

David Edwards/Arnold Neumaier,
I'm a big fan of geometric quantization, but
1. It's not the best way to understand the spinor degree of freedom.
2. The relation to classical physics is in the limit of large spin, with spin 1/2 the opposite limit, maximally non-classical.

5. **Peter Woit**
July 24, 2020

DB,
To be blunt, I think the current situation with relating string theory to the real world is that most string theorists have simply given up. About the only active area in string theory that even tries is the Swampland business, which hardly anyone takes seriously. Given this situation, there's no longer any science here worth trying to have a serious discussion about, all that remains is the sad sociological question of why people are still promoting string theory as our best hope for a unified theory. But, that's also a tired subject, which I don't want to encourage here unless someone actually has something new to say about this.

6. **John Goldman**
July 24, 2020

Dear Peter,

Thank you for the clear explanation of spin 1/2 behaviour.

Whilst you are considering the twistor geometric implications could you please put your mind to what additional extension is needed to give rise to the 3 neutrino types?
Could a point in space-time be more than a complex 2-plane?
How about a 3-sphere?

7. **Manuel del Río Rodríguez**
July 24, 2020

Dear professor:

My name is Manuel del Rio and I am an avid and enthusiastic follower of your blog and your work. Good to have you back, even if nothing of late tickled your fancy! As you mention your book: it is in my top 5 of books I really want to be able to read in the future (right now, it is hopelessly out of my range; I am a Humanities person and English teacher for adults here in Spain, and have just started an online math degree which will have to progress very slowly -work and son must take priority-).

Even though it will probably have to wait a decade to be read, I will be buying it in the next months. Are you planning a second edition any time soon (incorporating the errata you mention in your page?)? If that were the case, I would wait for the purchase until it is out.

Best greetings

Manuel

8. **Chris Austin**
July 25, 2020

Perhaps the following historical question might be on topic for this post.

-i times the Pauli matrices gives a representation of Hamilton's quaternions. After struggling for a long time to find a generalization of the complex numbers, Sir William Rowan Hamilton suddenly thought of quaternions during a walk in 1843, and he famously scratched his inspiration on a bridge near Dublin. I had assumed that what he scratched on the bridge was something like -i times the Pauli matrices, but in fact what he wrote was $i^2 = j^2 = k^2 = i j k = -1$.

It would be nice to think that Hamilton discovered something like the Pauli matrices 177 years ago. But on skimming through nineteenth century textbooks on quaternions, such as those available on gutenberg.org, no trace of a 2-dimensional representation of the quaternions, or any matrix representation of the quaternions, ever seems to show up.

Élie Cartan constructed the spinor representations of the orthogonal groups in his 1913 paper that constructed, in the sense of their weight spaces, all the irreducible linear representations of the simple compact Lie algebras, (Bulletin de la S. M. F., tome 41 (1913), p. 53-96). The results for the Lie algebras of type B_l, i.e. SO(2l+1), are in section VI, starting on page 69. The weights for the spin representation of B_l are on page 70. An actual matrix form of the spin representation of B_l might be given implicitly in section XV, on page 86. For $l=1$, it looks as though X_1 there might be $(1/2)(\sigma_1 + i \sigma_2)$, X_{-1} might be $(1/2)(\sigma_1 – i \sigma_2)$, and Y_1 might be σ_3. But there does not seem to be any special discussion of the case $l=1$, or its relation to quaternions.

So for practical purposes, was the 2-dimensional representation of Hamilton's quaternions actually unknown, until Pauli presented the Pauli matrices, in his paper "Zur Quantenmechanik des magnetischen Elektrons," Zeitschrift für Physik, 43, 601–623, 1927?

9. **Peter Woit**
July 26, 2020

John Goldman,
Unfortunately at this point I don't know of a way to get generations out of twistor geometry.

10. **Peter Woit**
July 26, 2020

Manuel del Rio Rodriguez,
I'll be writing more about this here in coming months, but this year I'm teaching again the year-long course the book was based on. While doing this I'd like to revise and expand parts of the book, as well as make more minor improvements. I don't think there will be big changes in the first part of the book during the fall semester, but I hope to make major changes in the second half, when I'm teaching in the spring semester.

11. **Geoffrey Dixon**
July 26, 2020

Chris Austin, your comment intimates that Hamilton fell short in not achieving Pauli, when in fact the opposite is true. Pauli spinors are 2×1 complex matrices. The Pauli algebra, C(2), is isomorphic to the complexified quaternion algebra, the spinors of which are SU(2) doublets of Pauli spinors … so, had Pauli allowed Hamilton to inspire his work we may have had electro-weak U(1)xSU(2) theory a couple decades earlier than we did.

12. **Peter Woit**
July 26, 2020

Chris Austin/Geoffrey Dixon,
The fact that the complexification of the quaternion algebra over the reals is the algebra of two by two complex matrices is something I'm sure Hamilton (and other algebraists later on) knew. But I think he was interested in quaternions as a remarkable real algebra, so not much interested in its obvious complex representations.

My own point of view is that physicists have a big blind spot in that they are used to just throwing in complex coefficients whenever convenient, so always work in a complexification and are blind to the fact that there may be multiple real forms of whatever algebra they're working with. See my "two pet peeves" posting

https://www.math.columbia.edu/~woit/wordpress/?p=9222

I do think that removing this blind spot may help in better understanding the structure of fundamental physics, but there will be a lot, lot more needed to do this than keeping straight this simple example.

13. **Doug McDonald**
July 27, 2020

Speaking of lack of blogging, a couple of questions. Recently talk of the seriously suspect (bottom bins are even more suspect!) Xenon-axions experiment stuff is going around.

But we are supposed to be hearing from reliable, likely real, important (positive or negative, clarifying or muddying), results on neutrinos from several places, and muon g-2 experiments from Fermibab. We heard a bit on muon theory, very muddying.

Yet I see nothing blogged. Are there rumors or better? Even rumors of being bogged down whether for technical, analysis, or virus reasons?

14. **Peter Woit**
July 27, 2020

    Doug McDonald,
    Sorry, but while I would love there to be some exciting experimental HEP news (or even a good rumor) to blog about, I haven't heard any recently. Likely the COVID lockdown has impacted constructing and operating experiments, but people should still be able to analyze pre-COVID data from home…

15. **Edward M Measure**
July 27, 2020

    You had me at non-spinning, non-sphere, but is the bottom line that despite the non-classical behavior of the wave function, it otherwise acts just like additional angular momentum?

16. **Peter Woit**
July 27, 2020

    Edward M. Measure,
    To try and clarify the bottom line, yes, if you define
    "angular momentum = observable that generates rotations"
    Then, angular momentum can have two different sources
    1. Effect of rotating $\vec{x}$ in $\Psi (\vec{x})$. This is the angular momentum you are used to, cause by "stuff moving around", it's "orbital angular momentum", formula $$\vec{L}=\vec{x}\times \vec{p}$$
    in its classical and quantum versions.
    2. Ψ may take values in a vector space on which rotations act non-trivially. Then when you rotate a wave function, you get a second contribution from this, the "spin angular momentum". In the case of spin-half, Ψ takes values in $\mathbf C^2$, the spinors, and rotations act on the spinors (in a new way unlike the normal action of rotation on vectors and tensors, including the fact that it is a double-cover of the rotation group that acts, the usual rotations act only up to minus-sign problems).

17. **Babak**
July 27, 2020

    Hi Peter,

    I enjoyed your post.

    I'm just curious to know what do you think about the recent conference at the Perimeter Institute?

    Thanks.

18. **Peter Woit**
July 27, 2020

    Babak,
    I don't want to start a discussion of quantum gravity here. For one thing, I don't think there is anything new to say, and this has been true for quite a while. If you want an overview of the state of the subject I'd recommend looking at Nicolai's talk. It seemed to me rather discouraging, with nothing like a promising new idea. The current philosophy that appears to be dominant is that quantum gravity is "emergent". But Nicolai asks the right question: emergent from WHAT? And that's a question with no good answer. He asks for experimental evidence to help with the situation, but gives no reason to believe such evidence will ever be available.

19. **Cleon Teunissen**
July 28, 2020

    Peter woit,
    you write about an entity that is represented in spinor form:

    "If one finds a state with J_z = 1/2, rotating it by an angle 2*pi changes its sign. This is weird, but the sign of a state isn't itself something you can measure."

    I think among the sources I've read you are the first one to write _explicitly_: "the sign of a state isn't itself something you can measure".

    (Many sources state the following about entities represented in spinor form: "In order to return this object to the same state you have to turn it through 720 degrees." without mentioning whether that is _observable_.)

    I assume the spinor representation offers the most economical way (in mathematical sense) to reproduce the observed atomic spectrum energy levels.

    Let me make a comparison: in electronic engineering it is common to move the mathematical representation to complex number space, so that the equations are not in the form of sines and cosines, but exponential functions, allowing much more efficient manipulation of occurences of phase shift. It's a trade-off, you introduce imaginary voltage and imaginary current, and you get more efficient manipulation, but at the end of the computation you have to convert back to observables: measured voltage and measured current

    Is it possible at all to interpret the spinor representation in a similar way? That is, that it is an economical representation, but at the expense of introducing an _unobservable_ property ("a state with J_z = 1/2, rotating it by an angle 2*pi changes its sign")

    The Einstein-De Haas effect
    As is well known, the Einstein-De Haas effect drives home the point: if you manipulate the spin angular momentum of a sufficiently large population you can obtain exchange of a macroscopic amount of angular momentum.
    https://www.youtube.com/watch?v=qFkW0PHhXcY

    That is: I think it's worth emphasizing:
    quantummechanical spin angular momentum doesn't have a classical counterpart, but not in the sense that it only exists in some confined environment. Exchange with macroscopic objects can be elicited.

20. **Robert Wilson**
July 29, 2020

Chris Austin/Geoffrey Dixon/Peter Woit,
I agree that using quaternions rather than Pauli matrices makes the concept of spin understandable in a more elegant way. “Throwing in” complex numbers does obscure some of the issues. But there is more than one way of throwing in complex numbers. From quaternions/SU(2) one can either go the Pauli route, and end up with SL(2,C), or the Glashow-Weinberg-Salam route, and end up with U(2), often erroneously described as U(1) x SU(2). More clarity on the difference between these methods of throwing in complex numbers, and the underlying reasons for them, would certainly help me to understand what is going on.

21. **Hermann Nicolai**
July 29, 2020

Dear Peter,

thank you for advertizing my talk at QG2020. It was, however, not meant to be discouraging at all! It is just that (as my father, a medical doctor, used to say) “diagnosis must precede therapy”. And part of the diagnosis is that low energy SUSY is clinically dead, that the multiverse program is unlikely to get anywhere, and (for the “non-string approaches”) that explaining this or that particular feature of (say) the CMB is by far not enough, especially if you cannot clearly discriminate your explanation from other competing ones (after all, it might just be dust…). Last but not least divergences between different approaches are still so substantial that hope for a “grand syntesis” is far away. So the PI people did a great job in bringing together all these viewpoints and making the proponents talk to one another, and from that point of view the conference was a great success.

In the end it will only work if everything fits together, and a key element of this is explaining how the standard model (as is!) can emerge from a Planck scale theory. On this we have some ideas of our own (see my last two slides), but I did not want to overly emphasize this as (1) we might well be wrong, and (2) it is not the main purpose of an overview talk to push one’s own pet ideas.

Best regards,

Hermann

22. **martibal**
July 29, 2020

Regarding the explanation of why the standard model is as it is, I noticed than neither in the opening talk by Nicolai nor the closing one by Jacobson there is any mention of Connes and al approach to the SM using noncommutative geometry, although there have been various developments in the last years.

This might well be because this is not a theory of quantum gravity, and so it was not strictly in the scope of the conference. However this is a bit surprising because there are some people working on that topic at PI.
Is this approach to the SM so much “out of the radar” for physicist ? (I am asking without polemical intention, I am genuinely interested in understanding why it seems so much ignored ?).

23. **Peter Woit**
July 29, 2020

Cleon Teunissen,
The comment that the minus sign for spin causes no immediate problem because it is a sign of a wave-function, not of an observable certainly isn’t original to me. Discussing this gets into the interpretational issue of the physical significance of the wave-function, which is a discussion I very much don’t want to start again here.

On the analogy to solving real-valued problems with complex numbers: what’s going on there essentially is that, given a problem with a real-valued solution, you can ask for complex solutions, allowing use of the properties of complex numbers, making finding solutions easier. If you find a complex solution, you have found two solutions to your original problem (the real and imaginary parts), although one may be zero.

What’s happening with spinors is different. There is a serious question about whether it’s best to describe them in terms of real numbers, complex numbers, or quaternions, but the underlying fact is that there is a new and different spin-1/2 degree of freedom, which is undeniably there. It’s not something you are introducing just for calculational convenience.

24. **Peter Woit**
July 29, 2020

martibal and all,
I’m glad to have pointed those interested to Nicolai’s interesting review, and happy that he added comments here, but, as usual, I don’t want to start here an open-ended discussion of the state of speculative ideas, whether about non-commutative geometry or anything else.

25. **Peter Orland**
July 30, 2020

I expect you already know this, Peter, but there is also a classical version of spin-1/2. It’s a little subtler than integer angular momentum, but works just as well. This a charge on a sphere in the presence of a static monopole. This yields a Wess-Zumino action for the unit vector (or element of S^2), and upon quantization, this vector’s components become the Pauli matrices. A more general model (due to Tamm) is a charge anywhere in R^3, in the presence of a static monopole at the origin (which has wave functions which change sign under a rotation).

There is even a nice relativistic generalization. This is a slightly more complicated Wess-Zumino term, depending on two unit vectors in R^4, one of which is parallel to the four velocity, and the other which is orthogonal to the first (so the configuration space is $S^3\times S^3/U(1)$. This gives Dirac fermions after quantization (in Euclidean space. To get to Minkowski space, the S^3’s must be replaced by hyperboloids). One vector is the velocity divided by its norm. When quantized, this vector’s components become the Dirac gamma matrices. The other unit vector’s components become the rho matrices.

26. **Peter Woit**
July 30, 2020

Thanks Peter,
That’s a interesting physical variant of the general idea of taking S^2 as phase space and quantizing, but using the monopole to give the quantization line bundle.

I realized there’s another way to think of this, to argue that the spin-1/2 degree of freedom is what you get when you look not at classical mechanics, but at pseudo-classical mechanics (your phase space variables are anti-commuting) in the simplest non-trivial (d=3) case. I wrote up the details of this in chapter 30 and 31 of the book on QM (http://www.math.columbia.edu/~woit/QMbook/qmbook.pdf).

27. **Chris Regan**
August 7, 2020

Hi Peter,

Are you sure that geometric quantization isn't the best way to understand the spinor degree of freedom? It seems like a good way to go, based on a lower-dimensional example. Quantize space with a honeycomb lattice, and spin 1/2 pops right out. See this paper in Physical Review Letters: https://arxiv.org/abs/1003.3715.

28. **Peter Woit**
August 7, 2020

Chris Regan,
There are lots of ways to get a spinor degree of freedom out of a much more complicated structure. But spinors are really an extremely simple story about two by two matrices. What I'd like to advertise is this simplicity. If you want the simplest possible way to explain where spinors come from, twistor geometry is very compelling, giving spinors tautologically (a point in space time is exactly the spinor space).

Yesterday's Hype

August 1, 2020
Categories: This Week's Hype

Every summer CERN runs a summer student programme, designed to bring in a group of students to participate in scientific activities at CERN and provide lectures for them about the basics and latest state of the field of high energy physics. Because of the COVID situation, this summer they have not been able to bring students in, but are providing instructional lectures and Q and A's. This year's sessions are based on having students follow materials from last year's lectures, followed by a Q and A to answer their questions.

One of the topics the students are presented is What is String Theory?, and you can watch the 2019 video or look at the slides. Timo Weigand's presentation can be accurately described as pure, unadulterated hype, with not a hint of the existence of any significant problem with ideas presented. In the Q and A yesterday, Weigand did come up with a new piece of "evidence for string theory": it "predicts" no continuous spin representations.

I can't begin to understand why anyone thinks it's all right for CERN to subject impressionable students to this kind of thing. Someone, not me, should be complaining to the organizers and to CERN management.

This is unfortunately now an all too common example of what passes for "Sci Comm" in much of the field of fundamental physics: endless repetition of old discredited arguments in favor of a failed theory, coupled with pretending not to know about what is wrong with these arguments. The field that was once one of the greatest examples of the power of the human mind and the strength of the scientific method has become something very different and quite dangerous: all-too-visible ammunition for those who want to make the case that scientists are as deluded and tribalistic as anyone else, so not to be trusted.

Comments

1. **Gianni**
   August 1, 2020

   When I was a summer student at CERN in the 1980s, the lectures for the students were completely professional. This year, Weigand's last slide, with his summary, states: "String theory is a maximally economic quantum theory of gravity, gauge interactions and matter." A theory that adds 7 spatial dimensions and adds supersymmetry seems to give the impression that maybe it might just not be maximally economic. Weigand also writes on the same slide: "Assumption of stringlike nature of particles leads to calculable theory without UV divergences." What a pity that he had no time to explain the way to calculate elementary particle masses and coupling constants in this calculable theory. Possibly the margin on the slides was too narrow to write the calculation down?

2. **Peter Woit**
   August 1, 2020

   Gianni,
   The "maximally economic" description also struck me. It's kind of like listening to our current President: hard to figure out whether the no-relation-to-reality claims you're hearing are intentionally dishonest or just delusional.

3. **Sabine Hossenfelder**
   August 2, 2020

   Students aren't remotely as impressionable as you seem to think. Indeed, the most encouraging feedback I have gotten after the publication of my book came from students complaining that they are being taught useless nonsense. The kids, I think, will be alright. Trouble is, if we wait for them to grow up, we'll see 20 more years of time wasted on failed theories.

   This is why I think the key factor to change is public pressure. The hurdle here is that few people feel qualified to comment on what is going on in the foundations of physics. So, science communication (like you do) matters a big deal by explaining to non-experts just what is happening, why it's a problem, and why they need to call bullshit on it.

4. **Alessandro Strumia**
   August 2, 2020

   I heard various misunderstandings while playing soccer with summer students. The most bizarre, around 2015, was their firm belief that CERN had planned LHC run1 for discovering the Higgs, and LHC run2 for discovering SUSY/new physics.

5. **Peter Woit**
   August 2, 2020

   Sabine Hossenfelder,
   It's not that I think most students believe this kind of hype, although I think it does have some effect of encouraging smarter students not to enter the field, credulous ones to enter it.
   The most serious problem with this is the same problem with having a country's leader every day say things that are clearly untrue. It's not that most people start believing the untrue things, it's that the fact that he's doing it is an announcement that truth no longer matters. Instead, what matters is what's convenient for those in power, and everyone needs to now accommodate to that reality.

6. **Peter Woit**
   August 2, 2020

   Alessandro Strumia,
   Every so often I take a look at what people were saying at various stages in the LHC history. Just happened to look at this, from 2011
   https://www.math.columbia.edu/~woit/wordpress/?p=3492
   You come off well

   "Privately, a lot of people think that the situation is not good for SUSY," says Alessandro Strumia

   Arkani-Hamed at the time was furiously back-pedaling from his 2005 claim that the SUSY question would be settled after one year of LHC data. John Ellis was saying that he would abandon SUSY if it hadn't shown up by late 2012. You're right that around this time the pre-LHC storyline that "SUSY will be easy to see, the Higgs coming much later" had already started to be inverted, with Arkani-Hamed then changing his tune to "it will take until around 2020 to know about SUSY".

7. **Low Math, Meekly Interacting**
   August 2, 2020

   It's not possible to do science indefinitely without empirical checks. If HEP is a victim of its own success, it's not hard to see why. What physicists have achieved in the past century or so is nothing short of staggering. Who wouldn't be irrationally inspired by what the human mind can achieve. But we're wired to believe, and no one ought never to assume striving for brilliance and rationality is enough. Trusting in the strength of other minds is a comfort we instinctively seek, and a luxury we can never afford. Unfortunately, that may mean setting aside the grandest objectives for the foreseeable future, and being content with only the answers nature will yield in the present. I imagine it's difficult to maintain that level of discipline and restraint when it makes one an apostate and forces them to leave the fold.

   Harder still to take the notion seriously, perhaps, when lesser minds such as mine have the chutzpah to chime in with advice. But the lessons of history are clear enough that genius isn't necessary. Lacking it may even be protective. One can never forget they're only human.

8. **Peter Woit**
   August 2, 2020

   LMMI,
   I'm afraid that what has happened is that the "string theory" community has already taken your advice, setting aside actually working on unification as hopeless until something new comes from experiment.

   The problem is not just that they've given up, but that they have coupled this with an argument designed to avoid admitting that this giving up is a failure. The currently promoted ideology is that, whatever it is, "string theory" is not a failed idea, but is our "best hope", and that this "best hope" has shown that the problem is hopeless (the landscape), so everyone should give up.

9. **martibal**
   August 2, 2020

   @Sabine: some students may think they are taught bullshit, but not all of them, at least not the master students. How could they be aware of this anyway ? This propaganda comes from the place where the Higgs boson has been discovered, not from some remote university in a country with no research tradition.

   This kind of hype is problematic when students ask for a master thesis (having in perspective a PhD). I am in a mathematical-physics group, inside a math department. Every year we have students from the physics department asking us for a master thesis, because they found that their courses in physics were leaving aside too many mathematical details. Some of them are hesitating between say, a master thesis on supergravity or supersymmetry in physics, or some subjects of mathematical physics we may propose. What should we say ? SUSY is dead, don't listen to hype, come to maths ? (knowing that the job situation in math-physics is very difficult).
   Am I in position to say: don't listen to what you might heard from the CERN lectures, this is pure propaganda ?

10. **Martin**
    August 2, 2020

    Peter,

    Juggling axes is dangerous. In contrast, saying that fundamental physics has become "quite dangerous" is a hyperbolic appeal to fear.

    Calling ideas "dangerous" is a common trope and is a pet-peeve of mine. Nobody who criticizes an academic position as being "dangerous" ever traces the line of causation that leads to grad students (or whomever) being maimed or killed. Not even paper cuts. Because if they tried, they would suddenly appear hopelessly speculative and foolish. Let's not degrade our arguments in this way! Call it "counter-productive," not "dangerous." Don't make appeals to fear.

    In other respects, please keep up the good work.

11. **Peter Woit**
    August 2, 2020

    Martin,
    Maybe "dangerous" is not the right word, but neither is "counter-productive". What's getting maimed or killed is not any person, but the credibility of the subject. This is going to get worse the longer the hype campaign and refusal to admit an idea doesn't work goes on.

12. **Martin**
    August 2, 2020

    Peter,
    "quite dangerous" -> "damaging to the scientific endeavor"

13. **Low Math, Meekly Interacting**
    August 2, 2020

    Well, there's different levels "giving up", I suppose. Maybe, for instance, there's giving up on the notion one should devote a lot more effort to studying ways to "unify the fundamental forces" in the absence of some more obvious empirical indication that line of inquiry is going to lead somewhere. Landscapology is in its own league of capitulation. Its practitioners have decided unification is so important that empiricism itself ought not to get in the way of achieving it.

    Can't one eschew GUTs or TOEs for the time being and still do first-rate HEP without giving up on science altogether? I'm still waiting for someone to make an exciting public pitch for determining the shape of the Higgs potential that doesn't promise far more than it might deliver. My understanding is precision measurement of the Higgs self-interaction is fascinating "new physics" in its own right (being completely unlike any other interaction in the Standard Model).

    It's giving up on the addiction to, and perhaps a "belief system" based on the notion of a "Final Theory" I was thinking of.

14. **Amitabh Lath**
    August 2, 2020

    Physics is less prone to groupthink. At some point as a student you must develop a spine and go up against your mentor. I remember winning an argument with my advisor (effin' Nobel Laureate!) something to do with polarized electrons. My students have gone to the mat about machine learning and artificial intelligence in data analysis until I cried uncle. I am sure the same happens with string theory and SUSY etc. in the theory community. Physics students may be many things but they are not sheep.

15. **DB**
    August 3, 2020

As I see it, the maximum responsibility lies within the string theorists themselves.
Not with teachers, not with students and not with “scientific” journalists.
If they (especially the “top” ones) came out publicly in their conferences, and stated that ST is not the “final theory” they were looking for, that things/ideas have taken a different route and that ST, as it is, needs some big changes (has to be clearly rethought, as some do acknowledge privately…), the situation might start getting better in the HEP world.
Also, maybe everyone should be a bit more humble, and settle for something less than a final TOE which, IMHO, might be too much for us to ask for, and might very well not even exist.
Weigand’s presentation was an absolute joke.

16. **anonymous...**
August 3, 2020

Stop the String Theory bashing ! It’s “not even funny”.

String theory is dead and we will have in a few years the first generation of high energy theoretical physicist without a single interesting article in their whole sad career (mid 80’s-mid 2020’s)

We should instead discuss about recent articles and emphazize on :
– neutrino (https://en.wikipedia.org/wiki/List_of_neutrino_experiments)
– g-2 (Fermilab E989) vs LatticeQCD
– cosmological simulations vs observational cosmology (and the cool new toys : Euclid, LSST, …)
– syntax and models of type theories, infinite categories, abstract homotopy
– periods and motives

it’s not hype-driven development… these are just (some of) the 2020’s healthy scientific communities

“la fécondité se reconnaît par la progéniture”

17. **Peter Shor**
August 3, 2020

The problem with saying “the students are smart and not impressionable; they’ll be all right” is that the people who decide which students become professors are the string theorists who have bought into the failed theory, and they’re going to promote the students who aren’t saying that string theory has reached a dead-end.

So we may have to put up with another generation of string theorists before we can get fundamental physics on a reasonable track again.

18. **A.J.**
August 3, 2020

@PeterShor

The CERN summer students are not — generally speaking — future theorists of any kind. They’re future experimentalists, and the experimentalists who’ll be hiring them are not noted for being especially friendly to string theory. (Yes, many of them liked the idea of weak-scale SUSY, but I don’t know any who were particularly heartbroken when it turned out to be wrong.) Also, how much of an impact do you think a single lecture on string theory is going to have on people who spend all the rest of their days working on the nitty gritty details of a high energy experiment?

19. **Peter Woit**
August 3, 2020

A.J.,
I think you’re right that this is mostly an audience of potential experimentalists not theorists. But then, since there’s no relevance to any experiment, why do you think the organizers thought lectures on string theory of this kind were a good idea? I’m seriously trying to understand what is going on in people’s minds. “String theory” seems to have become some bizarre totem or something, completely divorced from evaluation as conventional science, but brought out to wow certain groups of people. This is starting to seem to me understandable only as some sort of religious or tribal ritual, a parading of mysterious golden tablets that have 40 years of the tribe’s hopes for the future invested in them. Why does anyone think this is what science looks like?

20. **A.J.**
August 3, 2020

Peter,

I was there about 20 years ago, and was a bit of an oddball for having much interest in theory or mathematics. Those students were quite serious experimentalists. I’ve kept in touch with many of them over the years, and the number who’ve become successful HEP experimentalists is a bit shocking. I’m used to theory, where maybe 1/30 got past the postdoc. For the CERN summies, it was at least 10x that. Some of them are even giving summer lectures this year.

In any case, the student lectures are a mix of Basic Practical Thing You Must Know to Do Real Physics and Glimpse Into Another Nearby But Probably Less Important Field of Physics. At least when I was there, theory wasn’t presented as anything to idolize.

21. **Paul Gor**
August 4, 2020

About 8 years ago my daughter and I were at a University Open Day in Sydney to choose her studies in science. As we walked around different faculties, we came across the Physics department where we met some eager young Physics students of the University. They were trying to appeal to my daughter to do Physics. One of their main selling point was that they do research in String Theory. I remember feeling disappointed, I thought, is String Theory now really the ‘Poster Boy’ of Physics?
PS.
My daughter has now finished her Masters Degree in Biology and Environmental Sciences.

22. **Bernhard**
August 4, 2020

“Someone, not me, should be complaining to the organizers and to CERN management.”

I agree, but it is very nontrivial to find someone with nothing to lose to make such a complaint. Anyone without a permanent position is ruled out for the fear of backlash, and even if you have a permanent position there are political reasons not to do it. Experimentalist professors who are group leaders won’t feel comfortable or judge they have the right credentials to make such a complaint, theorists won't want to "have a problem" with their friends, even those who think this is pure BS. And, especially if you work at CERN (be as staff or user), which is basically a small town, you won't want this attention drawn to you.

23. **WTW**
August 5, 2020

Peter,
Purely by accident, a recent Google search included a link to one of your old posts:
Two Cultures https://www.math.columbia.edu/~woit/wordpress/?p=6208
Just out of curiosity, following links you provided there, and reading (and remembering) what people back in 2013 were saying "must be true", and "what we've known for decades", is rather scary.

I recently came across a quote attributed to Saul Perlmutter that seems appropriate here (and to much that is going on in the world today):
"People forget that when we talk about the scientific method, we don't mean a finished product. … Science is an ongoing race between our inventing ways to fool ourselves, and our inventing ways to avoid fooling ourselves."
Perhaps that's the lesson that should be communicated to those summer students at CERN: that the latter should be their goal, with Weigand's presentation a case in point of the former.

Unfortunately, the former often seems to be winning over the latter lately, in HEP and elsewhere, despite ongoing evidence. This is not "new news", but for an example of more hard data re. SUSY from CERN, here's some of the latest published results, in PRL:
Search for Heavy Higgs Bosons Decaying into Two Tau Leptons with the ATLAS Detector Using pp Collisions at √s=13 TeV
https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.125.051801

24. **Juan**
August 5, 2020

I've just finished my PhD in HEP. I must say many students are, in fact, quite impressionable. I used to think I wanted to do research on ST until I started attending lectures by the big fishes of the community (including the superstring winter school at CERN)… It was a total disappointment being told about the swampland conjectures and exotic string compactifications without any reference to real physics and being surrounded by very arrogant students.

I've also got to know many students who sadly label ST and the like as "real" theoretical physics, as the path to follow, regarding (for example) solid state physics as less relevant. Many also think that phenomenology is BS and you are an idiot if you don't do research in sugra/extended susy/ads-cft. It seems that many just follow the fame and the hype of the big names in the field. Their PhD advisors get perpetuated by these young people, so I think the picture is not going to change any soon.

25. **FB36**
August 5, 2020

A new article full of (not so subtle) String Theory hype: 🙂

https://www.symmetrymagazine.org/article/taking-a-risk-on-theoretical-physics

26. **More Anonymous**
August 5, 2020

Off topic but good read https://www.nature.com/articles/d41586-020-02297-2

27. **Geoffrey Dixon**
August 5, 2020

Juan, in saying "many students are, in fact, quite impressionable", has hit the nail almost on the head. I would suggest "many" be replaced by "almost all". The notion that young HEPers are straining to break free of the strictures imposed by their presumed elder betters is charmingly naive.

28. **DSS**
August 9, 2020

Maximally economic? A theory that predicts 6, 7 or 22 extra spatial dimensions, of which there is no empirical evidence whatsoever? A theory that doubles the number of fundamental particles, without being able to predicts their masses, and for the existence of which, again, empirical evidence is sorely missing? A theory that posits a preposterously large number of possible different vacua, all of them just as likely within the context of the theory? A theory that takes the values of fundamental physical constants as deus ex machina-given? A theory that evolves in a deus ex machina-given space-time? This is a maximally economic theory? What is this guy smoking?

I, for one, really, really hope that theoretical physicists will eventually be able to do much better than that.

(Imaginary) Time Asymmetry

August 6, 2020
Categories: Quantum Mechanics

When people write down a list of axioms for quantum mechanics, they typically neglect to include a crucial one: positivity (or more generally, boundedness below) of the energy. This is equivalent to saying that something very different happens when you Fourier transform with respect to time versus with respect to space. If $\psi(t,x)$ is a wavefunction depending on time and space, and you Fourier transform with respect to both time and space
$$\widetilde{\psi}(E,p)=\frac{1}{2\pi}\int_{-\infty}^\infty \int_{-\infty}^\infty \psi(t,x)e^{iEt}e^{-ipx}dtdx$$
(the difference in sign for E and p is just a convention) a basic axiom of the theory is that, while $\widetilde{\psi}(E,p)$ can be non-zero for all values of p, it must be zero for negative values of E.

This fundamental asymmetry in the theory also becomes very apparent if you want to "Wick rotate" the theory. This involves formulating the theory for complex time and exploiting holomorphicity in the time variable. One way to do this is to inverse Fourier transform $\widetilde{\psi}(E,p)$ in E, using a complex variable $z=t+i\tau$:
$$\widehat{\psi}(z,p)=\frac{1}{\sqrt{2\pi}}\int_{-\infty}^\infty \widetilde{\psi}(E,p)e^{-iEz} dE$$
The exponential term in the integral will be
$$e^{-iE(t+i\tau)}=e^{-iEt}e^{E\tau}$$
which (since E is non-negative) will only have good behavior for $\tau <0$, i.e. in the lower-half z-plane. Thinking of Wick rotation as involving analytic continuation of wave-functions from $z=t$ to $z=t+i\tau$, this will only work for $\tau <0$: there is a fundamental asymmetry in the theory for (imaginary) time.

If you decide to define a quantum theory starting with imaginary time and Wick rotating (analytically continuing) back to real, physical time at the end of a calculation, then you need to build in τ asymmetry from the beginning. One way this shows up in any formalism for doing this is in the necessity of introducing a τ-reflection operation into the definition of physical states, with the Osterwalder-Schrader positivity condition then needed in order to ensure unitarity of the theory.

Why does one want to formulate the theory in imaginary time anyway? A standard answer to this question is that path integrals don't actually make any sense in real time, but in imaginary time often become perfectly well-defined objects that can be thought of as expectation values in a statistical mechanical system. For a somewhat different answer, note that even for the simplest free particle theory, when you start calculating things like propagators you immediately run into integrals that involve integrating a function with a pole, for instance integrating over E integrals with a term
$$\frac{1}{E-\frac{p^2}{2m}}$$
Every quantum mechanics and quantum field theory textbook has a discussion of what to do to make sense of such calculations, by defining the integral involved as a specific limit. The imaginary time formalism has the advantage of being based on integrals that are well-defined, with the ambiguities showing up only when one analytically continues to real time. Whether or not you use imaginary time methods, the real time objects getting computed are inherently not functions, but boundary values of holomorphic functions, defined of necessity as limits as one approaches the real axis.

A mathematical formalism for handling such objects is the theory of hyperfunctions. I've started writing up some notes about this, see here. As I find time, these should get significantly expanded.

One reason I've been interested in this is that I've never found a convincing explanation of how to deal with Euclidean spinor fields. Stay tuned, soon I'll write something here about some ideas that come from thinking about that problem.

Comments

1. **Pascal**
   August 6, 2020

   Isn't it possible to create some 'negative energy' with the Casimir force?

2. **Peter Woit**
   August 6, 2020

   Pascal,

   Energy is basically only defined up to a constant. You can always shift where E=0 is, so the point really is that energies are bounded below, that they can go off to infinity in only one direction. That's the asymmetry in the theory.

3. **Douglas Natelson**
   August 6, 2020

   Of course, in classical GR there is an absolute zero of energy density, corresponding to flat spacetime, right?

4. **Peter Woit**
   August 6, 2020

   Douglas Natelson,

   Yes, in classical GR. But I'm just trying to make a simple very general point about quantum theory, where very generally you have a Hamiltonian and it generates time translations. The positivity (perhaps after a shift in definition) of the energy eigenvalues is a very general fundamental aspect of a quantum theory, and if you work with Euclidean QM or QFT, this gets reflected in an asymmetry in the behavior in imaginary time. If you think about Euclidean QFT this is kind of surprising, since the theories one works with are often formulated in a Euclidean invariant manner (e.g. as a Euclidean invariant path integral or stat mech system).

5. **Sabine Hossenfelder**
   August 7, 2020

   there's a refection in your text that I believe should be a reflection

6. **Aula**
   August 7, 2020

   There are two instances of "the the theory" in the post.

7. **Peter Woit**
   August 7, 2020

   Sabine and Aula,
   Thanks! Fixed.

8. **Moshe**
   August 7, 2020

   The relation between the Euclidean and Lorentzian theories is most transparent for specific set of quantities, which are correlators of time-ordered operators evaluated between the vacua in the asymptotic past and future (which may be different for a time-dependent Hamiltonian). This is great if you are interested in S-matrix elements, which is typically the case if you are a particle physicist.
   \
   But these are not the most general quantities you may be interested in, for example you may be interested in expectation values of Schrodinger picture operators in some excited state (as in measuring the CMB), or in some retarded correlators corresponding to physical measurements in CM physics. For these quantities, the Euclidean calculations are less useful. In fact, it is not clear to me if the Euclidean calculations alone contain all the information needed in principle to obtain the Lorentzian information (I'd be interested in opinions), but at the very least extracting that information is somewhere between difficult to impossible (for example the analytic continuations involved often do not commute with perturbative expansions).

9. **Peter Woit**
   August 7, 2020

   Moshe,
   Thanks. I very much agree that the relation between Minkowski and Euclidean calculations is non-trivial and deserves much more attention. The usual assumption, based on the kinds of S-matrix calculations you mention, seems to be that all that's involved is changing some factors of i, but there's much more to it.

10. **DKepler**
    August 9, 2020

    Moshe, Peter: There certainly is much more to Lorentzian Euclidean relation than changing factors of "i". For example, blindly applying this to an arbitrary metric gives a complex metric field that has no natural interpretation. Besides, Euclidean quantities (such as Greens functions) do not distinguish between coincidence and null limits, while null surfaces play a crucial role in Lorentzian regime. However, there does exist a covariant alternative to Wick rotation which do not have these difficulties, though much less work has been done on it.

11. **martibal**
    August 10, 2020

    DKepler: if not off topic, could you say more about this covariant alternative to Wick rotation you are taking about ?

12. **DKepler**
    August 10, 2020

    martibal,

    (Peter can decide if the question/reply are off-topic.)

    The key idea behind this (first noted, to the best of my knowledge, by Hawking & Ellis (HE)) is based on the following mathematical result: given a Lorentzian metric g_L and a nowhere vanishing timelike direction field U, one can always construct a Euclidean metric g_E. As is well known, non-compact manifolds always admit such a vector field, as well as compact manifolds with Euler number zero.

    Some references where this HE observation was discussed in the context of QFT are Candelas & Raine, PRD15, 1494 (1977) and Visser 1702.05572, while recent generalization and consequences for GR, euclidean QG, and euclidean action can be found in Kothawala, arXiv:1705.02504, arXiv:1802.07055. The latter references also discuss transition from euclidean to lorentzian, instead of just the euclidean phase.

    Hope this helps.

13. **martibal**
    August 10, 2020

    DKepler: thanks a lot, this helps a lot.

14. **Chris Oakley**
    August 10, 2020

    There is also a connection between positivity of energy and causality. If one takes a derivation of the relativistic electromagnetic vector potential from a four-current (e.g. [here](#)), and requires positivity of photon energy as a supplement to the Maxwell equations, then the retarded solution is the only possible.

15. **Andras Laszlo**
    August 11, 2020

    Dear All,

    Just recently I happened to come across a related problem. I would be interested in some comments on the below, related phenomenon. (Peter might decide if it is off-topic.)

    In QFT, in a Lorentz signature setting, one can define two kind of quantum field correlator function(al)s: simple VEV of products of the quantum field operator (let us call them Wightman correlators), and the VEV of time ordered products of the quantum field operator (let us call them Feynman correlators). The Wightman correlators are the ones which satisfy the Wightman axioms, including the Wightman positivity. (If Wick rotated, they will satisfy the Osterwalder-Schraeder axioms, including the OS positivity, or reflection positivity.) The Feynman correlators, however, are the ones which are in principle returned by a Feynman integral procedure. Also, these are the ones, which turn up when one would like to evaluate QFT predictions (S matrix, for instance). To me, it seems that the transition from Wightman correlators to Feynman correlators is basically a projection (time ordering). So, if I am not mistaken, Wightman correlators (without bringing in some external information) cannot be fully recovered from merely the Feynman correlators. The question naturally arises: to what extent the Wightman axioms (in particular, the Wightman positivity) are reflected in the properties of the Feynman correlators? (We should not assume, of course, anything to be known about the theory, except for its Feynman correlators.)

Best regards,
Andras

16. **Peter Orland**
August 12, 2020

Andras Laszlo,

In practice, it is not trivial to obtain the Wightman functions from T-ordered correlation functions (or their connected versions, which can also be defined. For example the connected T-ordered 2-point function is the retarded commutator of the two field operators). As you say, going the other way can be done straightforwardly.

In principle, however, I THINK it can be done. The T-ordered Green's functions fix the vacuum state. Then the Wightman functions are vacuum expectation values of field operators.

17. **Peter Orland**
August 12, 2020

Thinking on the matter a little more, it HAS been done where constructive field theory methods work (in for example ϕ^{4}_{d} models for $d<4$). In these models the time-ordered functions are obtained (in Euclidean space, where they are analytic continuations of those in Minkowski space), and the Wightman axioms have been proved. I'm not an expert on continuum constructive field theory, but this is an example.

18. **Andras Laszlo**
August 13, 2020

Dear Peter Orland,
Thanks, and could you give some hints where one could start looking at these claims?
(What I actually wanted to ask: what one can say _without_ knowing the field operators — i.e. without actually solving the QFT model etc. That is, how the Wightman positivity translates to a situation if only the Feynman correlators — i.e., time ordered correlators — are known about the model.)
Best, Andras

19. **Peter Orland**
August 13, 2020

Andras,

I'm not going to satisfactorily answer your questions, but I'll make some remarks, which I hope generate more light than heat.

As to your first question, the book by Glimm and Jaffe is probably where you should start. I'm no expert, so I'll tell you what little I know (I'm very familiar with these concepts on a lattice, but in the continuum, don't trust my statements as the last word). In constructive field theory, it is important to have reflection positivity, which is a statement about observables on a half space ${\mathbb R}_+\times {\mathbb R}^{d-1}$, which is in Euclidean space. This is a sufficient (but perhaps not necessary) condition for unitarity and the spectrum condition. In constructive field theory, the models are not actually solved, but bounds are place on the convergence of resummation of perturbation theory (by a Borel transform). Once the Euclidean axioms are proved, then the claim is that, after a Wick rotation, the Wightman axioms are satisfied.

Now in a cut-off theory (requiring renormalization, which is very different from the axiomatic approaches), in principle, the time-ordered correlations define the vacuum state. If you knew the vacuum, you can certainly construct these, but the inverse procedure should be possible to find the vacuum from them. For example, in a free field theory, the two-point function determines a unique Gaussian vacuum wave functional. I have not thought more deeply as to how to do this more generally. It sounds like a good project for a student, though.

Anyway, once you have the vacuum state, the un-time-ordered correlations can be found. Of course, nobody actually knows the exact vacuum state for most interacting field theories, so I am stating a matter of principle.

One more remark. Given the time-ordered functions, the connected Green's functions,
can be found. These are (as I said in my previous statement) a little different. For example, the two-point connected function is the vacuum expectation value of the retarded commutator. The advanced commutator of two fields can also have its expectation value found this way. So at least vevs of commutators can be found. This is close to the two-point Wightman function, but perhaps not good enough an answer to you question.

20. **Andras Laszlo**
August 13, 2020

Dear Peter Orland,
Thank you for the answers.
Best regards, Andras

21. **Robert**
September 11, 2020

I am not sure if the has to be bounded from below in quantum mechanics. If it is and the Hamiltonian is only defined as a quadratic form than the Friedrichs extension guarantees the existence of a self-adjoint operator but if you had a s.a. H from another source, my intuition would be that you often can avoid running into problems even if it is unbounded from below. Take for example H= sqrt(p^2+m^2) + Z/r a toy model for a relativistic hydrogen atom. If Z is big enough, this is unbounded from below. But please correct me if I am wrong.

In QFT, however, I agree that you want a spectrum condition. My preferred reason is that even in the absence of a preferred vacuum (for example in the absence of Poincare symmetry because auf background fields) it allows to define composite operators like phi^2(x) via normal ordering (subtracting the dev of that expression in your favourite vacuum) as you need it for example for an energy momentum tensor: If both your reference vacuum state and your current state obey a positive spectrum condition, you get smooth expectation values of your composite operators. This is for example (in the example of QFT on curved manifolds) described around Thms 6.3 and 6.4 in Fewster's lecture notes https://wiki.physik.uni-muenchen.de/TMP/images/9/9f/Lecturenote-3908.pdf

Quantization and Dirac Cohomology

August 7, 2020
Categories: BRST

For many years I've been fascinated by the topic of "Dirac cohomology" and its possible relations to various questions about quantization and quantum field theory. At first I was mainly trying to understand the relation to BRST, and wrote some things here on the blog about that. As time has gone on, my perspective on the subject has kept changing, and for a long time I've been wanting to write something here about these newer ideas. Last year I gave a talk at Dartmouth, explaining some of my point of view at the time. Over the last few months I've unfortunately yet again changed direction on where this is going. I'll write about this new direction here in some detail next week, but in the meantime, have decided to make available the slides from the Dartmouth talk, and a version of the document I was writing on Quantization and Dirac Cohomology.

Some warnings:

- Best to ignore the comments at the end of the slides about applications to Poincaré group representations and BRST. Both of these applications require getting the Dirac cohomology machinery to work in cases of non-reductive Lie algebras. As far as Poincaré goes, I've recently come to the conclusion that doing things with the conformal group (which is reductive) is both more interesting and works better. I'll write more about this next week. For BRST, there is a lot one can say, but I likely won't get back to writing more about that for a while.
- The Quantization and Dirac Cohomology document is kind of a mess. It's an amalgam of various pieces written from different perspectives, and some lecture notes from a course on representation theory. Some day I hope to find the time for a massive rewrite from a new perspective, but maybe some people will find interesting what's there now.

No Comments

Twistors and the Standard Model

August 11, 2020
Categories: Euclidean Twistor Unification

For the past few months I've been working on writing up some ideas I'm quite excited about, and the pandemic has helped move things along by removing distractions and forcing me to mostly stay home. There's now something written that I'd like to publicize, a draft manuscript entitled Twistor Geometry and the Standard Model in Euclidean Space, which at some point soon I'll put on the arXiv. My long experience with both hype about unification in physics as well as theorist's huge capacity for self-delusion on the topic of their own ideas makes me wary, but I'm very optimistic that these ideas are a significant step forward on the unification front. I believe they provide a remarkable possibility for how internal and space-time symmetries become integrated at short distances, without the usual problem of introducing a host of new degrees of freedom.

Twistor theory has a long history going back to the 1960s, and it is such a beautiful idea that there always has been a good argument that there is something very right about it. But it never seemed to have any obvious connection to the Standard Model and its pattern of internal symmetries. The main idea I'm writing about is that one can get such a connection, as long as one looks at what is happening not just in Minkowski space, but also in Euclidean space. One of the wonderful things about twistor theory is that it includes both Minkowski and Euclidean space as real slices of a complex, holomorphic, geometry. The points in these spaces are best understood as complex lines in another space, projective twistor space. It is on projective twistor space that the internal symmetries of the Standard Model become visible.

The draft paper contains the details, but I should make clear what some of the arguments are for taking this seriously:

- Unlike other ideas about unification out there, it's beautiful. The failure of string theory unification has caused a backlash against the idea of using beauty as a criterion for judging unification proposals. I won't repeat here my usual rant about this. As an example of what I mean about "beauty", the fundamental spinor degree of freedom appears here tautologically: a point is by definition exactly the $\mathbf C^2$ spinor degree of freedom at that point.
- Conformal invariance is built-in. The simplest and most highly symmetric possibility for what fundamental physics does at short distances is that it's conformally invariant. In twistor geometry, conformal invariance is a basic property, realized in a simple way, by the linear $SL(4,\mathbf C)$ group action on the twistor space $\mathbf C^4$. This is a complex group action with real forms $SU(2,2)$ (Minkowski) and $SL(2,\mathbf H)$ (Euclidean).
- The electroweak $SU(2)$ is inherently chiral. For many other ideas about unification, it's hard to get chiral interactions. In twistor theory one problem has always been the inherent chiral nature of the theory. Here this becomes not a problem but a solution.

At the same time I should also make clear that what I'm describing here is very incomplete. Two of the main problems are:

- The degrees of freedom naturally live not on space-time but on projective twistor space PT, with space-time points complex projective lines in PT. Standard quantum field theory with fields parametrized by space-time points doesn't apply and how to work instead on PT is unclear. There has been some work on formulating QFT on PT as a holomorphic Chern-Simons theory, and perhaps that work can be applied here.
- There is no idea for where generations come from. Instead of PT perhaps the theory should be formulated on S^7 (space of unit length twistors) and other aspects of the geometry there exploited. In some sense, the incarnations of twistors as four complex number or two quaternions are getting used, but maybe the octonions are relevant.

What I think is probably most important here is that this picture gives a new and compelling idea about how internal and space-time symmetries are related. The conventional argument has always been that the Coleman-Mandula no-go theorem says you can't combine internal and space-time symmetries in a non-trivial way. Coleman-Mandula does not seem to apply here: these symmetries live on PT, not space-time. To really show that this is all consistent, one needs a full theory formulated on PT, but I don't see a Coleman-Mandula argument that a non-trivial such thing can't exist.

What is most bizarre about this proposal is the way in which, by going to Euclidean space-time, you change what is a space-time and what is an internal symmetry. The argument (see a recent posting) is that, formulated in Euclidean space, the 4d Euclidean symmetry is broken to 3d Euclidean symmetry by the very definition of the theory's state space, and one of the 4d $SU(2)$s give an internal symmetry, not just analytic continuation of the Minkowski boost symmetry. There is still a lot about how this works I don't understand, but I don't see anything inconsistent, i.e. any obstruction to things working out this way. If the identification of the direction of the Higgs field with a choice of imaginary time direction makes sense, perhaps a full theory will give Higgs physics in some way observably different from the usual Standard Model.

One thing not discussed in this paper is gravity. Twistor geometry can also describe curved space-times and gravitational degrees of freedom, and since the beginning, there have been attempts to use it to get a quantum theory of gravity. Perhaps the new ideas described here, including especially the Euclidean point of view with its breaking of Euclidean rotational invariance, will indicate some new way forward for a twistor-based quantum gravity.

Bonus (but related) links: For the last few months the CMSA at Harvard has been hosting a Math-Science Literature Lecture Series of talks. Many worth watching, but one in particular features Simon Donaldson discussing *The ADHM construction of Yang-Mills instantons* (video here, slides here). This discusses the Euclidean version of the twistor story, in the context it was used back in the late 1970s to relate solutions of the instanton equations to holomorphic bundles.

Update: After looking through the literature, I've decided to add some more comments about gravity to the draft paper. The chiral nature of twistor geometry fits naturally with a long tradition going back to Plebanski and Ashtekar of formulating gravity theories using just the self-dual part of the spin connection. For a recent discussion of the sort of gravity theory that appears naturally here, see Kirill Krasnov's Self-Dual Gravity. For a discussion of the relation of this to twistors, see Yannick Herfray's Pure Connection Formulation, Twistors and the Chase for a Twistor Action for General Relativity.

Comments

1. **Warren Siegel**
   August 11, 2020

   How do you get around the related problems that (Penrose) twistors are inherently massless & on shell?

2. **Peter Woit**
   August 11, 2020

   Warren Siegel,

   This is meant as a proposal for short-distance physics, conformally-invariant physics. What's missing is how to do gauge theory on PT and how to give dynamics to the Higgs, providing the interactions that would give you an effective field theory at longer distances that would have masses.

3. **John C. Rodney**
   August 11, 2020

   I don't have the background to evaluate your paper. Still, I am impressed that you have put it out to allow all those whom you have criticized to take their shots at you.

Also, does your theory predict anything and how can it be experimentally verified?

4. **Peter Woit**
August 11, 2020

John C. Rodney,
At this point in my life, the last thing I want to waste my time on is dumb arguments about these ideas, string theory, testability, etc. One could for instance argue that what I'm discussing predicts four space-time dimensions, only SU(2), SU(3) and U(1) internal symmetries, spontaneous breaking of electroweak symmetry, etc. For a truly completely convincing prediction, what you want is a calculation of one of the Standard Model parameters, or, better, some prediction from a model based on these ideas that is different than what the Standard Model predicts. There's still a lot missing here, with perhaps the most important idea missing being an idea about where different generations of fermions come from.

String theorists have argued that it doesn't matter how bad a unified theory string theory provides, that it has to be compared to other ideas about unification. I don't think that there's a serious argument that string theory unification based on the multiverse, the swampland or whatever can be compared favorably to the ideas presented here.

I'm happy to engage anyone in serious discussion of these ideas, but string theory really is off-topic.

5. **vmarko**
August 12, 2020

Hi Peter,

There are many unification models out there. Even I have one (easily found on arXiv) — using category theory, it allows you to discuss fermion generations, automatically includes gravity, works only in 4D, and has potential to circumvent Coleman-Mandula. And some other stuff. So the lack of unification proposals, with various interesting properties, is not the problem.

The problem is lack of peer interest.

Namely, until someone demonstrates that one of all those models out there actually reduces the number of SM free parameters, or predicts something outside the SM, it's all nothing more than just fascinating math. The majority of researchers will not bother to pay any attention to unification models. This is quite unfortunate, and IMO a bit sad, but I don't see any other way to attract a bit of attention and gain interest from other researchers in the field.

Even if one explicitly demonstrates that some model reduces the number of free parameters, it is tough to convince people. A typical example is the noncommutative SM, developed by Chamseddine, Connes and their collaborators. That's also based on some intriguing and fascinating math, and has some provocative properties regarding unification of interactions. Does the hep-th community pay any attention to it? Not much, really.

I share your point of view that proposing unification models and studying them is a worthwhile endeavour, but it appears that any such model requires a substantial number of people to drive the research in order to flesh out a convincing prediction. But attention of other researchers can apparently be obtained only if one already has a prediction. And thus you reach Catch 22, having a nontrivial manpower-problem to move the idea off the ground.

Math is always beautiful, various algebraic structures have some captivating properties, and offer tempting ways to explain various properties of nature that we do not yet understand. But the lack of collective peer interest in any such structure is a warning to all of us pursuing this research area — physics requires *more* than math.

Best, 🙂
Marko

6. **Low Math, Meekly Interacting**
August 12, 2020

Since this is your (pre-?)preprint's debut, I'm guessing the preliminary answer is "no", but…

Have you discussed this with or gotten feedback from others with deep interest in twistors? You wrote some very nice things about Andrew Hodges a while back, and he seems to be among the outstanding proponents of Penrose's invention, and something of a guru.

Not that anyone's opinion matters ultimately, but what constructive assessments you receive will be of interest to readers of this blog (even those who can't understand ~95% of the above post).

I share others' interest in hearing more about any observational consequences of these ideas, when the time comes.

Best of luck!

LMMI

7. **Peter Woit**
August 12, 2020

vmarko,
The main problem right now is that the SM is too good. Traditionally the way the field made progress was by focusing on explaining experimental results that contradicted the best available model. At this point all there is of that kind is arguably dark matter, where you have an extremely small amount of relevant info and a huge number of people working on it. In reaction to this difficult situation, most of the field has essentially abandoned the problem, which is a rational thing to do.

The only reason to keep working on this is if you see some possible way forward. To me there have always been aspects of the SM that don't quite fit together (the issue of Wick rotation and spinors is an example), and new things to learn about mathematical ideas that somehow relate to the SM, giving possible new ways of thinking about it. I've spent 40 years doing this, learned a huge amount, enjoyed myself, but until recently never felt that I'd come upon a set of ideas that fit together in a really convincing way. I've now tried to write up what I do understand, there are lots of new obvious questions and directions to pursue that I don't understand, plenty to keep me busy. I'd like to think that others will sooner or later find something in these ideas that resonates with them, we'll see…

8. **Peter Woit**
August 13, 2020

LMMI,
I did start writing people about this a while before making this public. Not a lot of reaction, partly I think because this is far from what people are used to thinking about, partly because it's August. There have been a small number of extremely helpful responses, of the form: "I'm not understanding this particular point", making it clear

where I need to do more work to clarify what it going on and explain it. I"m working now on expanding parts of what I've written, will write more on the blog once that is done.

9. **Anonymous**
   August 13, 2020

   This feels to me like the story relating the standard model to the octonions. In both cases, there is a beautiful mathematical structure that happens to have the standard model gauge group sitting inside it, maybe even acting in a somewhat suggestive way. It feels to me like the group theory version of the Strong Law of Small Numbers, as in "there aren't enough small Lie groups to meet the many demands made of them."

10. **Peter Woit**
    August 13, 2020

    Anonymous,
    There may very well be a role for the octonions, twistor space is conventionally thought of as four complex numbers or two quaternions, perhaps the octonionic structure can be exploited.

    But note that what I'm discussing is not just an algebraic framework for internal symmetries, but is based on twistor geometry, which is normally thought of as explaining space-time symmetries. It is putting internal and space-time geometry together which I think is what makes these ideas so interesting.

11. **Dave Miller**
    August 14, 2020

    Peter,

    As you know, putting fermions on a lattice is a mess. Does your work give any clues as to how to do that? My gut feeling is that any theory that cannot be cleanly discretized is missing something.

    Have you sought out comments from John Baez? This is the sort of thing he has been publicly musing about for decades, and I would think John would either have some interesting ideas as to how to move forward or, perhaps, some ideas as to why your approach won't work (if the latter is true, better to learn earlier than later!).

    And please continue to keep us all informed: one of us may come up with some useful idea.

    Best of luck!

    Dave Miller in Sacramento

12. **Peter Woit**
    August 14, 2020

    Dave Miller,
    Actually, thinking about fermions on the lattice a very long time ago is what got me interested in this whole subject of the geometry of spinors. Euclidean lattice gauge theory is beautifully adapted to the geometry of connections and curvature, one would like something similar for spinors.

    One problem with twistor methods and the lattice is that they typically very much exploit the properties of holomorphic functions, and it seems hard to do that in a discretized theory.

    Someday maybe I'll get back to thinking about the lattice and spinors. What is natural on the lattice is not spinors but differential forms, and those give you Kogut-Susskind fermions. The Euclidean twistor picture says you need to take into account at each point in 4d the fiber above it, which you can think of as projective (half)-spinors, or orthogonal complex structures. This gives you what you need to deal with spinors. Maybe there is some way to exploit this on the lattice.

    As always, John Baez has been helpful…

13. **Robert A. Wilson**
    August 15, 2020

    I think there is a misprint at the end of section 4.1: surely you mean Spin(2,2) = SL(2,R) x SL(2,R), and not Spin(3,3)?
    There is of course a well-known translation from twistors to octonions, which may throw up new insights. If you take the real split octonion algebra, and fix a choice of complex subalgebra, then you get SU(2,2) acting by (left, say) multiplications, and SO(2,4) acting by bi-multiplications. If you intersect with the automorphism group, you get SU(1,2), whose compact part is the EW gauge group U(2).
    If I had a model like this that only covered one generation of fermions, I'd be wondering what role the choice of complex structure within the quaternions plays in all this. I take it this is what you are hinting at by suggesting extending to Spin(7) or Spin(8)? The appropriate real forms would seem to be Spin(3,4) and Spin(4,4), but I'd be worried about the group getting too big, and would wonder what could be done by restricting to Spin(3,3) instead.

14. **Klas**
    August 16, 2020

    "One problem with twistor methods and the lattice is that they typically very much exploit the properties of holomorphic functions, and it seems hard to do that in a discretized theory."

    That might depend on which properties of holomorphic functions you are interested in. In the last decade a theory of univariate holomorphic functions on planar graphs has been developed. This was motivated by questions from statistical mechanics, and does e.g. connect to conformal field theory.

    The first steps were taken by Smirnov but now many others are working on this as well.

    A survey talk by Smirnov
    https://mta.videotorium.hu/en/recordings/2382/discrete-complex-analysis-and-probability

    Here you can find further references
    https://link.springer.com/chapter/10.1007/978-3-662-50447-5_2

Guys and Their Theories of Everything

August 21, 2020
Categories: Uncategorized

I'm a big fan of Sabine Hossenfelder's music videos, the latest of which, Theories of Everything, has recently appeared. I also agree with much of the discussion of this at her latest blog posting where Steven Evans writes

> nobody wants to see Peter Woit sing.

and Terry Bollinger chimes in:

> Please, under no circumstances and in no situations, should folks like Peter Woit, Lee Smolin, Garrett Lisi, Sean Carroll, or even John Baez try to spice up their blogs or tweets by adding clips of themselves singing self-composed physics songs.
>
> Trust me, fellow males of the species: However tempted you may be by Sabine's spectacular success in this arena, it just ain't gonna work for you!

The chorus of Sabine's song goes:

> All you guys with theories of everything
> Who follow me wherever I am traveling
> Your theories are neat
> I hope they will succeed
> But please, don't send them to me

One reason for her bursting into song like this was probably her recent participation in this discussion. I'd like to think (for no good reason) that it had nothing to do with my recently sending her a copy of this.

Today brought a new discussion of theories of everything, by Brian Greene and Cumrun Vafa. When asked by Greene to give a grade to string theory, Vafa said that he would give it a grade of A+, although its grade was less than A on the experimental verification front.

While I'm enthusiastic about new ideas involving twistors and happily continuing to work on them, it's pretty clear that this is not a good time to be bringing them to market. The elite academic world of Harvard and Princeton theorists that I was trained in has been doing an excellent job of convincing everyone that even the smartest people in the world could not make any progress towards a TOE, and that all claims for such progress from the most respected experts around are not very credible. Best to ignore not just the cranks who fill up your inbox with such claims, but all of them, judging the whole concept to be doomed until the point in the far distant future when an experiment finally provides the clue to the correct way forward.

Be warned though, if people don't pay some more attention, I'm going to start writing songs and singing them here.

Update: Note, an ill-advised attempt at humor referring to identity politics was obviously a mistake and has been deleted (along with some references to it in the comments). The threat to start singing is also a joke.

Comments

1. **Edward M Measure**
   August 21, 2020

   I don't know, John Baez may have some familial talent

   […]

2. **Peter Shor**
   August 21, 2020

   If you dream up a theory of everything,
   Grab your guitar, write some lyrics, and sing!
   Don't put it in a journal; that's not gonna cut it—
   The usual suspects would simply rebut it
   And chances are that your article would just be ignored.
   Take up songwriting if you want to be heard!

3. **graboluk**
   August 22, 2020

   @Peter Shor, for completeness, should I force "ignored" to rhyme with "heard" or the other way around? Or is there an english dialect where they rhyme by default?

4. **Kerberos**
   August 22, 2020

   ""Be warned though, if people don't pay some more attention, I'm going to start writing songs and singing them here.""

   The Ur-Twistor did the same:
   https://www.youtube.com/watch?v=Bwt2jktwAqU

5. **Peter Shor**
   August 22, 2020

   graboluk: Words that nearly rhyme (slant rhymes) are fairly common in English song lyrics and poetry. Usually, people pronounce them the way they normally do.

6. **Lars**
   August 22, 2020

It just *couldn't* have been your recent comment about beautiful math that got Hossenfelder's songwriting juices flowing, could it?

"Twistor theory has a long history going back to the 1960s, and it is such a beautiful idea that there always has been a good argument that there is something very right about it."

As we all know, Hossenfelder has a soft (some might say sore) spot for argument from beauty.

7. **Michael Weiss**
   August 22, 2020

   To amplify on Peter Shor's comments, one can do no better than to turn to a Nobel Laureate:

   "Rhyming doesn't have to be exact anymore," Bob Dylan told Paul Zollo of American Songwriter magazine in a 2012 interview. "It gives you a thrill to rhyme something and you think, 'Well, that's never been rhymed before'. Nobody's going to care if you rhyme 'represent' with 'ferment', you know. Nobody's gonna care."

   Dylan once admitted to Rolling Stone magazine that he stunned himself when he wrote the first two lines of 'Like a Rolling Stone' and rhymed "kiddin' you" with "didn't you". "It just about knocked me out," he said.

8. **Peter Woit**
   August 22, 2020

   Kerberos,

   Exactly that song had occurred to me as the right music for the topic.

9. **former mathematician**
   August 22, 2020

   In his arithmetically titled "love minus zero / no limit," Dylan rhymed "fire" with "buy her."

10. **FB36**
    August 22, 2020

    "When asked by Greene to give a grade to string theory, Vafa said that he would give it a grade of A+"

    Maybe we should allow all students to decide their own grades "since they are the ones who best know/understand their own work, they are the best qualified"? 🙂

11. **Sabine Hossenfelder**
    August 23, 2020

    Hi Peter,

    Happy to like it! And no, it didn't have anything to do with your paper, which came when I was pretty much already done.

12. **graboluk**
    August 23, 2020

    "kiddin you" and "didn't you" is legit (not even slant imo). On the other hand "heard" and "ignored" sound like, so to speak, a confirmation of Gromov's opinion on english and poetry.

    In any case, as a peace offering 🙂 I've just checked an online rhyming dictionary, which suggested both "absurd" and "nerd" for rhymes to "heard", so perhaps these potentially could be used instead in the penultimate verse.

13. **ohwilleke**
    August 24, 2020

    I could swear that I have seen John Baez do this once or twice.

    My late father was a professor and also did several for his students (fortunately for us all, pre-YouTube).

14. **Jacob Zelten**
    August 25, 2020

    I read this as "We String Theorists have failed to produce a Theory of Everything and we're the smartest people in the room. So obviously, nobody else is smart enough to create one."

15. **Peter Shor**
    August 29, 2020

    To rather belatedly defend my inadequate rhymes, let me point out that in one of the most famous poems in the English language, *Ode on a Grecian Urn*, John Keats rhymes *morn* and *return*, with the same two vowels that are found in *ignored* and *heard*. So this rhyme was acceptable long, long before Bob Dylan. (Of course, in all other respects, my lyrics above are vastly inferior to Keats'.)

16. **graboluk**
    August 31, 2020

    Peter Shor, I see your point but this isn't entirely convincing to me. The Oxford Dictionary of Original Shakespearean Pronunciation claims "rɪˈtɐːɹn" as the pronunciation for return, and "retornth" appears in a 16th century poem by Thomas Wyatt. So to me, you'd need an early 19th century english pronunciation expert in order to confirm the intended pronunciation of ode on a grecian urn. A few years back newspapers in UK ran articles about Shakespeare, whose general theme was along the lines of "it's not that his rhymes suck, it's just that the pronunciation has changed".

Funding Priorities

August 26, 2020
Categories: Uncategorized

The research that gets done in any field of science is heavily influenced by the priorities set by those who fund the research. For science in the US in general, and the field of theoretical physics in particular, recent years have seen a reordering of priorities that is becoming ever more pronounced. As a prominent example, recently the NSF announced that their graduate student fellowships (a program that funds a large number of graduate students in all areas of science and mathematics) will now be governed by the following language:

> Although NSF will continue to fund outstanding Graduate Research Fellowships in all areas of science and engineering supported by NSF, in FY2021, GRFP will emphasize three high priority research areas in alignment with NSF goals. These areas are Artificial Intelligence, Quantum Information Science, and Computationally Intensive Research. Applications are encouraged in all disciplines supported by NSF that incorporate these high priority research areas.

No one seems to know exactly what this means in practice, but it clearly means that if you want the best chance of getting a good start on a career in science, you really should be going into one of

- Artificial Intelligence
- Quantum Information Science
- Computationally Intensive Research

or, even better, trying to work on some intersection of these topics.

Emphasis on these areas is not new; it has been growing significantly in recent years, but this policy change by the NSF should accelerate ongoing changes. As far as fundamental theoretical physics goes, we've already seen that the move to quantum information science has had a significant effect. For example, the IAS PiTP summer program that trains students in the latest hot topics in 2018 was devoted to From Qubits to Spacetime. The impact of this change in funding priorities is increased by the fact that the largest source of private funding for theoretical physics research, the Simons Foundation, shares much the same emphasis. The new Simons-funded Flatiron Institute here in New York has as mission statement

> The mission of the Flatiron Institute is to advance scientific research through computational methods, including data analysis, theory, modeling and simulation.

In the latest development on this front, the White House announced today \$1 billion in funding for artificial intelligence and quantum information science research institutes:

> "Thanks to the leadership of President Trump, the United States is accomplishing yet another milestone in our efforts to strengthen research in AI and quantum. We are proud to announce that over $1 billion in funding will be geared towards that research, a defining achievement as we continue to shape and prepare this great Nation for excellence in the industries of the future," said Advisor to the President Ivanka Trump.

This includes an NSF component of \$100 million dollars in new funding for five Artificial Intelligence research institutes. One of these will largely be a fundamental theoretical physics institute, to be called the NSF AI Institute for Artificial Intelligence and Fundamental Interactions (IAIFI). The theory topics the institute will concentrate on will be

- Accelerating Lattice Field Theory with AI
- Exploring the Multiverse with AI
- Classifying Knots with AI
- Astrophysical Simulations with AI
- Towards an AI Physicist
- String Theory Conjectures via AI

As far as trying to get beyond the Standard Model, the IAIFI plan is to

> work to understand physics beyond the SM in the frameworks of string and knot theory.

I'm rather mystified by how knot theory is going to give us beyond the SM physics, perhaps the plan is to revive Lord Kelvin's vortex theory.

Update: Some more here about the knots. No question that you can study knots with a computer, but I'm still mystified by their supposed connection to beyond SM physics.

Comments

1. **Sabine Hossenfelder**
   August 26, 2020

   Looks like Chris Anderson was right when he proclaimed the end of theory.

   (There's a "b" missing in the last sentence.)

2. **Peter Woit**
   August 26, 2020

   Thanks Sabine,
   Typo fixed. The Chris Anderson argument she refers to is here
   https://www.wired.com/2008/06/pb-theory/

3. **Warren Siegel**
   August 26, 2020

   How about
   • NSF run by AI

4. **Alex**
   August 26, 2020

Reply to Chris Anderson – https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2711825/

5. **Mayer Landau**
August 26, 2020

It is absolutely fascinating, from a Frankenstein monster that will not die perspective, that the multiverse and string theory live on through AI.

6. **Tim Bradshaw**
August 26, 2020

I think what this probably really means is that the latest AI bubble is about to burst (or has burst). Certainly an AI winter is coming.

7. **David Brahm**
August 26, 2020

I did not think I'd live to see the day that the direction of physics research was announced by Ivanka Trump.

8. **sdf**
August 26, 2020

This sounds like an Onion article. Let's just have everything run by AI shall we? What about AI run by AI? Then we can all just stay in bed in the morning and avoid all this bullshit.

9. **Peter Erwin**
August 27, 2020

I suspect a large part of the "White House announcement" is congressionally mandated, in spite of the boilerplate, "Leadership of President Trump" bombast, in part because that's where the money has to come from. (And way down at the bottom of the announcement is an acknowledgment that "The National Quantum Initiative Act, bipartisan legislation signed by President Trump in 2018, called for the creation of research centers nationwide to accelerate foundational QIS research and development.")

10. **Thomas Larsson**
August 27, 2020

@Warren Siegel. How do you know that NSF is not run by AI today?

Artificial intelligence is better than none.

11. **Peter Woit**
August 27, 2020

David Brahm/Peter Erwin,
While Ivanka Trump was doing the announcing, the decisions that this is the way science needs to go are getting made elsewhere, often with the participation of the physics community. What I wonder more is what sort, if any, of peer review signed off on the multiverse AI business.

All this reminded me that I've written about some of this before, see
https://www.math.columbia.edu/~woit/wordpress/?p=10680

12. **DB**
August 27, 2020

I think we are underestimating the imminent role that AI is about to play in expanding our knowledge of the Universe. I'm with those 'tech geeks' who see the future discoveries of physics not coming out of Princeton, Harvard or Cambridge but rather out of Apple, Google and their Chinese equivalents. AI will be an existential tool to expand knowledge, but Chris Anderson's view is too radical. In about twenty or twentyfive years, 200 million teens in their bedrooms are each going to have access to more computing power and tools than the top physics institutions have today.
Quantum computers and AI are ALREADY the future of cosmology/string theory/multiverse/knot theory… whatever field you prefer.
Theoretical physics, as has been done/studied up till now… is at the verge of a massive paradigm shift. I know there are a couple of radicals out there (some are string theorists pretty well known in this blog, especially one of them from Central Europe…) who still believe that men alone will find that final TOE (which they consider to be ST itself!), but… sorry for them. It's not going to happen as they expect. Not a snowball's chance in hell!!
The field is screwed without the help of that powerful technology. And contrary to what some top theoretical physicists go about saying that it's a great time to become a theoretical physics student, well… bollocks.

13. **Yash Sharma**
August 27, 2020

The current approach to AI is highly data-dependent and statistical in nature. Causal understanding has not been introduced yet. And most active Machine Learning research is not primarily geared towards that.
Given that, as physicists do you think there is even a possibility that theoretical physics topics have any chance of benefitting from AI (without it having causal understanding) in the near future? Are there any planned physics experiments to which AI would add a significant value?

(I am from computer science and engineering background.)

14. **Peter Woit**
August 27, 2020

DB,
It's not just certain string theorists who believe human beings can come up with a successful unified theory, since I happen to also believe this, and would have thought that if anyone's not a string theorist, it's me. I might be wrong though about what I am, since I just saw in Katie Mack's new book the following definition of string theory: "String theory is a blanket term for theories that try to bring together gravity and particle physics in new ways" and that definition fits what I work on.

Apple and Google might be sources for certain kinds of physics discoveries, but I'd bet these will always be in certain restricted subfields (e.g. those relevant to building quantum computers). Those companies are pretty tightly focused on making money and achieving world domination, not likely to devote much effort to other fields with little practical, exploitable, applications.

The idea that increase in computer power and new computer algorithms will revolutionize physics has been around for at least the last 50 years I've been following the subject. Actual progress of this kind has always been much more limited than expected. When I was a grad student in the early 1980s I spent a lot of time doing computer calculations (lattice gauge theory Monte-Carlo simulations). That field has had interesting results, but not revolutionized the subject at all. One of the most likely possible applications of quantum computers would be in that area, but I suspect they will always be rather specialized, not affect most people's work.

On the experimental side of physics, I'm quite willing to believe that AI will find some very useful applications, from what I hear it's already being seriously exploited in LHC data analysis, with more to come. There's no evidence though that this sort of thing will lead to breakthroughs in fundamental experimental issues, for instance helping overcome the barriers to getting information about what's happening at energies above the TeV scale.

It's not hard to believe that a large fraction of the stupid things people do can be done just as stupidly, but faster, by artificial intelligence. Given the depressing state of the world, this might mean that most of what human beings do can be done by machines. On the issue of finding a better unified theory, all evidence I've seen in that the machines will do a great job of calculating away on bad, failed ideas, none that they'll help come up with better ones.

15. **Peter Woit**
August 27, 2020

Yash Sharma,
The main problem with beyond the SM HEP theory is that there is essentially no relevant data at all (maybe, making a lot of assumptions about astrophysics, a couple numbers about "dark matter"). So, it seems no role for "Big Data"- based approaches.

Back before the LHC turned on, a bunch of people organized an "LHC Olympics" designed to get algorithms ready to sort through and figure out the correct beyond the SM model based on the large number of new (non-SM) physics signals they expected. It always seemed more likely that things would work exactly as they had always worked in the past: intensive effort devoted to finding even one new physics signal. That's where the field is now: experimentalists will use all the help they can get to find such a signal, but for theorists whether there's no signal or one signal, Big Data is not what they will ever have.

16. **Yash Sharma**
August 27, 2020

Thanks, Peter.
About the other discipline set to receive great funding, Quantum Information Science, it seems much more likely to help theoretical physics, guessing from this nice article – https://www.quantamagazine.org/john-preskill-quantum-computing-may-help-us-study-quantum-gravity-20200715/ . Thats about quantum gravity. Do you see any other ways in which quantum computing might help (apart from lattice gauge theory that you mentioned). Specifically, are there any experiment or simulations that quantum physicists cannot perform in labs because they are infeasible or too expensive, but with a scalable quantum computer they might be able to do it?

17. **Peter Woit**
August 27, 2020

Yash Sharma,
Theorists have been promoting the idea that quantum information science will give us a theory of quantum gravity for a long time now, with little solid to show for this, and I'm extremely dubious that that will ever change. The only substantive reference in that Preskill article, was to the article discussed here
https://www.math.columbia.edu/~woit/wordpress/?p=11648

While I thought that Preskill article was disappointing, the best answer I know of to your question is a much better article he wrote a while back, see
https://arxiv.org/abs/1811.10085

18. **Miquel**
August 27, 2020

As a researcher in AI I am quite surprised to see such cranky topics being given respectability.

There is a lot of serious work on bringing optimisation, search and automated theorem proving (all of them techniques employed in research in AI) to help with experimental design, finding proofs and programming quantum computers.

Seeing a topic "exploring the multiverse with AI" – that sounds like something out of a work of fiction such as Neal Stephenson's "Anathema" – breaks my heart.

19. **vmarko**
August 27, 2020

Hi Peter and others,

Regarding the AI in theoretical physics, the only potentially useful application I can imagine is the following — unlike any human, the AI could "read" and sift through all scientific papers on the arXiv, with a possible outcome of finding one or two obscure papers which contain some crucial piece of result for some open problem, written by some people not very well known by the research community.

Namely, a human simply cannot read *all* papers, and most of the researchers only pay attention to papers authored by the people they are familiar with. So some crucial insight, put on the arXiv, can go completely unnoticed for a long time, if it was authored by a researcher who has no social contact with leaders in the field. Simply, nobody bothered to read it. On the other hand, in principle if properly trained, an AI could analyze all the arXiv papers, and turn everyone's attention to that one inconspicuous little paper written by those anonymous couple of guys from that obscure University of Whereever — which actually solves something important, but nobody paid any attention to it because its authors are not recognized as relevant by the main community of researchers.

I think the above could be a useful application of AI in theoretical physics, and IMO this is the best case scenario — I am in fact very skeptical that AI could do even that (perform a semantic analysis of papers on the arXiv), since semantic analysis usually requires actual understanding of the underlying problem, paths to possible solutions, etc. — all that stuff that we do in science that AI usually *cannot* do.

Other than that, I don't see AI being helpful in theoretical physics in any way whatsoever, except maybe in some highly specific usecases, like sifting through petabytes of LHC data looking for a non-SM signal. But do note that the latter is just a more efficient way of performing a very dull, boring and non-intelligent work what humans (or even monkeys) could eventually do even without AI, given enough time. A machine just does it faster.

Also, I am baffled that some people have an impression that AI can be somehow "smarter" or "more inteligent" than humans (whatever the definitions of those words may be). On the contrary, AI is just an efficient lossy compression algorithm (think jpeg or mp3). It is a mathematical function with a huge number of tunable parameters, and once tuned to give desired output to certain known inputs (process called "training" the AI), one can use it to approximate outputs for unknown inputs, hoping that the resulting outputs would not be "too far off" from the outputs one would expect to get. That is why mp3 playback sounds rather similar to the original song, and why google seems to be able to "predict" what will be the next word you type into its search box. There is nothing "intelligent" about it, it's just pure (and very stupid) large scale data-fitting.

All this stuff (that makes up the AI) has absolutely nothing whatsoever to do with creative thinking, which is what science is all about, IMO.

Best, 🙂
Marko

20. **Yash Sharma**
August 27, 2020

Using AI to perform analysis on arxiv papers is possible now. Hopefully we will soon know if this approach works, as they have already released arXiv papers dataset for a large ML community. https://blogs.cornell.edu/arxiv/2020/08/05/leveraging-machine-learning-to-fuel-new-discoveries-with-the-arxiv-dataset/

21. **Alessandro Strumia**
August 28, 2020

Understanding intelligence will be an interesting science, important for theoretical physics because it's plausible that IQ will be scalable. For the moment, "*AI" (adding AI to whatever theorists were doing) is not going to be a breakthrough. But it's nice that some physicists become apolitical when Ivanka throws money.

22. **vmarko**
August 28, 2020

Yash Sharma,

Yes, arXiv has opened its database for ML purposes. But training the AI to do something useful with it is a completely different story, and a much harder problem to solve, IMO.

Best, 🙂
Marko

23. **Jackiw Teitelboim**
August 28, 2020

You all have already been fooled. The NSF announcement was written by an IA. (That is why "strings" and "knots" are in the same sentence!) Penrose predicted this. The Emperor has no mind.

24. **George**
August 28, 2020

"Exploring the Multiverse with AI" – helping sift all that data?

25. **André**
August 29, 2020

Peter,
there are good reasons not to model particles as knots. One is that in the weak interaction, particles can change (electrons to neutrinos, for example) and this change cannot be described with knots, which are stable structures. Another is that knots do not yield spin 1/2. (Even stronger reasons arise in string theory: in 10/11/12/26 dimensions, knots of one-dimensional objects do not exist at all: every knot is equivalent to the unknot. Maybe the AI system will rediscover this result …) As a consequence, modelling particles as knots has never been successful, despite about a dozen attempts in the literature – starting with Kelvin, as you mentioned.

26. **DB**
August 29, 2020

Peter,
thanks for your detailed reply above.
Preskill's arxiv paper and the link that you include in your update are on the same track of the idea I'm trying to convey.
No matter what string theorists or non string theorists say and proclaim all over the world, there's not a f… chance for us humans to discover that final TOE… if it exists. We will need the help of quantum computers/advanced AI, and, slowly but surely, they will be taking over that task from us.
Does that mean that we will disappear and have no role at all?
No, it just means that evolution will carry on its due course and our role will be diminishing. Of course, it won't happen overnight. We will probably have a big role to play at least during this century. After that… I'm not betting my money.

Re the world's situation… I can't agree with you more. The disaster is absolute. And it doesn't look better for the near future at least. It just sends massive shudders down my spine to think that in a decade (or less!) we could be in the hands of people like Ivanka or Jared… Not to mention the totalitarian oligarchs that already run many other parts of the world.

Finally, the reason why I don't believe we will be able to find that TOE, is because, IMO, "Ultimate Reality" is ineffable. It can't be expressed with words because it is diffused, almost imperceptible and lacks precision. It seems to me that the material Universe comes from the Immaterial. And that sounds like jumping over the physical realm into the META-physical, which I consider totally out of reach.
Oh, and I'm not referring to a god as religious people understand. I mean something so abstract that it can't be perceived/understood, a noumenon type of idea/concept.

27. **Peter Woit**
August 29, 2020

André,
I agree, and am not aware of any non-crackpot ideas to use knot theory to get a viable beyond SM model. That's why I was surprised to see that there's a new NSF-funded institute that advertises work on this.

DB,
I don't see any point in arguing from anyone's human intuitions about what fundamental physics should look like or not look like at the deepest level, including whether it should be comprehensible to us, or beyond our comprehension. Those who look at the problem and are sure that engaging with it would be like a dog trying to learn GR should do something else. Personally there seem to me several things about the SM and GR which I don't understand, for which continuing to try and learn more about them may lead to something new, and I see possible ways forward that might work.

I'm in a good position to keep working on what I want to work on, unfortunately young people at this point are going to face overwhelming obstacles if they try and pursue this kind of fundamental theory. Instead they're encouraged to go to work in an AI institute training them to develop more powerful tools to further the goals of our new overlords. Even if we are all just dogs, better to howl at the moon…

28. **André**
August 29, 2020

Peter,
the arguments against knots as particles are strong and hold water. But related topological structures, maybe made of open strings, might be possible – of course again only in 3d.

To me it is astonishing that a string theory institution includes a knot theory group. Knot theory is only possible in 3d. Knots and strings are simply incompatible. Or is there a kind of open string theory derivate in 3d?

29. **Peter Woit**
August 29, 2020

André,
This isn't a string theory institution, it's an "AI and fundamental physics" institution. The theory of knots is very intricate and complicated, and you can easily generate lots of data, so it's plausible that AI techniques might be useful. The problem is that this has no connection to fundamental physics, and I don't see one even in the Northeastern PR material specifically about this.

If I had to guess, the supposed connection would be through some sort of extremely complicated constructions of possible "string vacua", where you manage somehow to use knots as one piece of the construction. I'd guess though that such constructions are of even less interest than the usual complicated constructions, but that doesn't mean they couldn't play a role in attracting NSF funding.

30. **Jean Tate**
August 30, 2020

As an extragalactic astronomer (optical, radio) I have some experience with AI and how it might help, perhaps even leading to learning something new about dark matter. Yes, it's certainly "sexy", but I think we're still very much learning that AI GIGO is real, and that a lot of work has yet to be done before autonomous cars stop killing jaywalkers (to stretch an analogy). Before AI unleashed on SKA datasets starts telling us marvelous things about dark matter and dark energy, perhaps AI could help with identifying consistencies, inconsistencies, myths, and even reproducibility issues in astrophysics and cosmology?

31. **Joseph Healy**
August 31, 2020

Jean Tate,
Autonomous vehicles are easy. Autonomous vehicles on the same road with humans, on the other hand, is hard because humans are infinite fonts of novel behavior which simply cannot be anticipated. Because of that, we are facing a couple of decades of random slaughter and mayhem while we catalog and suppress most of the edge cases we can't foresee. That may seem a steep price to pay but, to put it in perspective, it likely pales compared to the WMD-like prospect of aging baby boomers behind the wheel en masse over the same coming decades. Personally, I'll take my chances with autonomous vehicles so we better get the AI cracking on that front…

But AI advancing theoretical physics? Autonomous vehicles will be accident-free before that comes to pass.

32. **Jonathan**
September 1, 2020

It seems to me much of this work would be useful in advancing AI research, but not advancing fundamental physics research. With particle physics, we have a large amount of data that can be explained quite efficiently with a relatively small number of abstract rules. Current AI/machine learning research is able to build predictive models from large amounts of data, but isn't able to produce human comprehensible rules.

If you can understand how to build an AI able to go from raw LHC data to the Standard Model, that would be incredibly valuable from an AI perspective. If you apply current AI models to physics data you'd get a black box model which in the best case scenario would be able to reproduce the physics data, but provide little to no additional understanding.

33. **Chris Austin**
September 4, 2020

The comment above by Joseph Healy is an outrage. People have to be killed randomly by driverless cars, until driverless car programmers have anticipated "every situation" that can arise? Why not put in a set of semi-catchall control conditions, so that for example if a driverless car is confused about the situation ahead, or it's not clearly safe to proceed, and it's not a fast busy road, then the driverless car will slow down and stop? Why not require prototype driverless cars to go through an appropriate very lengthy period of testing with dual controls, so that a human supervisor inside a prototype driverless car can take control immediately, if the car starts to behave in a dangerous manner?

34. **Peter Woit**
September 4, 2020

Chris Austin,
I don't want to host a discussion of driverless car technology or of the social significance of AI here, but will use this as an excuse to use my control of the blog to editorialize.

Most of the AI advances I hear about don't seem to me to be doing anything to make the world a better place. In my city, there's no good reason for most people to own a car. What we need is good public transportation and taxis. I don't believe AI can anytime soon reliably drive a cab or bus around New York. Even if it could, I don't see how eliminating the professions of bus driver and cab driver are likely to make New York a better place.

35. **Lee Smolin**
September 4, 2020

Dear Peter,

I don't know if this is the reason that knots are mentioned in the announcement along side of strings, but it is worth mentioning that knot theory is fundamental to loop quantum gravity. The basis result of the field, proved by Rovelli and myself, is
that the gauge and spatially diffeomorphism invariant states are in one to one correspondence with diffeo classes of knots and graphs. Thus many basic steps to construct quantum gravity are problems in knot theory. One example of this is that the Jone's polynomial (Kauffman bracket) is a specification of a physical state of the gravitational

field, with a non-zero cosmological constant.

Meanwhile, Will Cunningham at PI has been using machine learning to get unprecedented results on the causal set approach to QG. He and I are also part of a collaboration of computer scientists and physicists developing applications of machine learning to problems in fundamental physics.

Thanks,

Lee Smolin

36. **isomorphismes**
October 8, 2020

Knots: It's not a bad thing if an area of real inquiry with the possibility of definite results gets a splash from the latest mania.

IAIFI: This sounds like science fiction. I could see data processing being useful in astronomy … how is it going to help *theory*?

Thanks for a very nice rep theory book, Dr. Woit. It took me a long time to be ready to appreciate it; now I'm very thankful to you.

Quantum Reality

September 2, 2020
Categories: Book Reviews

Jim Baggott's new book, [Quantum Reality](), is now out here in US, and I highly recommend it to anyone interested in the issues surrounding the interpretation of quantum mechanics. Starting next week I'll be teaching a course on quantum mechanics for mathematicians (more about this in a few days when I have a better idea how it's going to work). I'll be lecturing about the formalism, and for the topic of how this connects to physical reality I'll be referring the students to this new book (as well as Philip Ball's [Beyond Weird]()).

When I was first studying quantum mechanics in the early-mid 1970s, the main popular sources discussing interpretational issues were uniform triumphalist accounts of how physicists had struggled with these issues and finally ended up with the "Copenhagen interpretation" (which no one was sure exactly how to state, due to diversity of opinion among theorists and Bohr's obscurity of expression). Everyone now says that the reigning ideology of the time was "shut up and calculate", but that's not exactly what I remember. The Standard Model had just appeared, offering up a huge advance and a long list of new questions with powerful methods to attack them. In this context it was was hard to justify spending time worrying about the subtleties of what Copenhagen might have gotten wrong.

In recent decades things have changed completely, with the question of what's wrong with Copenhagen and how to do better getting a lot of attention. By now a huge and baffling literature about alternatives has accumulated, forming somewhat of a tower of Babel confronting anyone trying to learn more about the subject. Some popular accounts have dealt with this complexity by turning the subject into a morality play, with alternative interpretations portrayed as the Rebel Alliance fighting righteous battles against the Copenhagen Empire. Others accounts are pretty much propaganda for a particular alternative, be it Bohmian mechanics or a many-worlds interpretation.

Instead of something like this, Baggott provides a refreshingly sane and sensible survey of the subject, trying to get at the core of what is unsatisfying about the Copenhagen account, while explaining the high points of the many different alternatives that have been pursued. He doesn't have an ax to grind, sees the subject more as a "Game of Theories" in which one must navigate carefully, avoiding Scylla, Charybdis, and various calls from the Sirens. One thing which is driving this whole subject is the advent of new technologies that allow the experimental study of quantum coherence and decoherence, with great attention being paid as possible quantum computing technology has become the hottest and best-funded topic around. Whatever you think about Copenhagen, what Bohr and others characterized as inaccessible to experiment is now anything but that.

While one of my least favorite aspects of discussions of this subject is the various ways the terms "real" and "reality" get used, I have realized that one has to get over that when trying to follow people's arguments, since the terms have become standard sign-posts. What's at issue here are fundamental questions about physical science and reality, including the question of what the words "real" and "reality" might mean. In *Quantum Reality*, Baggott provides a well-informed, reliable and enlightening tour of the increasingly complex and contentious terrain of arguments over what our best fundamental theory is telling us about what is physically "real".

Update: For a much better and more detailed review of the book, Sabine Hossenfelder's is [here]().

Comments

1. **Matt Grayson**
   September 3, 2020

   I have just started reading Quantum Reality, and I'm grateful for the sensible definitions of "reality", a term I've always hated, but one thing confuses me still.

   How can anyone contemplate Netwonian mechanics and see "smooth continuities and merciless certainties"? Imagine two balls colliding. At the moment of collision there is infinite acceleration. Not large, infinite. Make the balls spongy? Still infinite where they first touch. The only way I can see to avoid infinities is for the balls to interpenetrate – overlap – and then gradually, if very quickly, rebound.

   And yet we have no worries about the interpretation of Newtonian mechanics. We say "clockwork" as if that explains anything. It is truly a theory of "shut up and calculate".

2. **Thomas**
   September 3, 2020

   I don't know .. it seems to me that the added value of yet another popular book on the foundations of QM is approaching zero exponentially fast. Why not write a book on progress in quantum technology and engineering?

3. **Peter Woit**
   September 3, 2020

   Matt Grayson,
   It has always seemed to me that the classical picture of the world is far more incoherent, inconsistent, and hard to make sense of than the quantum one.

   Thomas,
   I disagree, since while there have been lots of such books recently, mostly they have been of either little value, or seriously negative value, with the amount of attention they get often proportional to how misleading they are. Philip Ball's is the only other recent one that I think gets things right, having another one written in a somewhat different way I think is quite valuable.

   I won't disagree that a good new book on quantum technology would be great, and advances in this technology I think will ultimately help clarify interpretational issues. Baggott has some interesting material on this, in particular about implications of quantum computer designs for the preferred basis problem in many-worlds theory.

4. **Terry Jenkins**
   September 3, 2020

   Dear Peter,

   You write, " Whatever you think about Copenhagen, what Bohr and others characterized as inaccessible to experiment is now anything but that."

   Extraordinary claims require extraordinary evidence, but as you are quite busy, perhaps we could get just an example or two, or even a reference?

   Thanks in advance.

5. **Peter Woit**

September 3, 2020

Terry Jenkins,

What I was referring to was for instance one of the main issues in developing a quantum computer: how do you keep qubits from decohering, while at the same time being able to manipulate them to do computations? This is currently under intensive experimental and theoretical study, though you'll have to find someone more expert than me for good references on the state of the art.

The fact that such study was impossible in the early days of QM I think had a lot to do with the early Copenhagen insistence that one needed to treat the classical and quantum in completely different ways.

6. **Mark**
September 3, 2020

I think this might be a good reference on one approach to quantum computing:

Robust encoding of a qubit in a molecule
Victor V. Albert,1,2 Jacob P. Covey,1 and John Preskill1,2
Institute for Quantum Information and Matter1 and Walter Burke Institute for Theoretical Physics2 California Institute of Technology, Pasadena CA 91125, USA
(Dated: November 20, 2019)

https://arxiv.org/pdf/1911.00099.pdf

7. **Peter Woit**
September 3, 2020

Mark,
That's more of an alternate proposal for a quantum system to encode qubits, doesn't really address the transition to classical question.

For something closer to what I had in mind, see the Nobel citation for Haroche/Wineland
http://www.nobelprize.org/prizes/physics/2012/press-release/
and the textbook "Exploring the Quantum" by Haroche/Raimond.
The Physics Today review of that book
https://physicstoday.scitation.org/doi/full/10.1063/1.2774103
starts out

In 1952 Erwin Schrödinger wrote in the British Journal of the Philosophy of Science , "We never experiment with just one electron or atom or (small) molecule. In thought-experiments we sometimes assume that we do; this invariably entails ridiculous consequences."

which shows the very different situation the first generation of quantum theorists faced when thinking about the measurement problem.

8. **Mark**
September 3, 2020

Peter,

Thanks, I have read the book Exploring the Quantum, and highly recommend it. Forty years ago when I read Zurek & Wheelers: Quantum Theory and Measurement, I thought to myself that this will all be resolved in the next few decades with advancing technology. But that does not seem to have been the case. Not sure if the "measurement" problem is a real physics problem or simply one of those philosophical problems that never get resolved.

9. **Michael Weiss**
September 3, 2020

Peter and Matt,
With regard to QM being more sensible than classical mechanics, you may remember the late Ed Purcell's light-hearted remark when we were undergraduates that it made as much or more sense to speak of a "quantum certainty principle" than of the standard "uncertainty principle." Ed was referring to the implausibility of classical mechanic's infinite precision; he might also have had in mind Matt's point about infinite accelerations in hard-sphere collisions. Ed's quip reflected his always-deep insight. John Preskill was Ed's TA in intro QM in 1975 ("Physics 143") and might have more insight.

10. **Mark Hillery**
September 3, 2020

For something a lot more technical, and very recent, that summarizes several different approaches to obtaining a classical world out of quantum rules, I recommend "Roads to objectivity: Quantum Darwinism, Spectrum Broadcast Structures, and Strong quantum Darwinism" by J. K. Korbicz arXiv:2007.04276.

11. **Low Math, Meekly Interacting**
September 4, 2020

I'm probably getting this partially wrong…I donated most of my old pop-sci books a long time ago. Anyway, I think it was in "Dreams of a Final Theory". Weinberg recounted a colleague's rueful explanation for a promising student's eventual failure: He tried to understand quantum mechanics. The larger point, I think, was Weinberg sympathized with this point of view. However, despite his distaste for philosophy, he saw the unavoidable need. Otherwise, one could never truly understand cosmology; and perhaps a "final theory" would remain forever out of reach until QM was, in fact, "understood".

I don't recall if Weinberg had the measurement problem in mind specifically, or something else. Anyway, that book is about 30 years old now. My sense is one could pick it up today and find an essentially contemporary summary of the outstanding issue(s), i.e. troubling and completely unresolved. Every new pop-sci article I read these days on "foundations", a new experimental result is hyped as if it sheds some incredible new light. But you read past the click-bait and it turns out the results agree as precisely as one can measure with QM as currently formulated. The results don't actually resolve any interpretational issues, despite sometimes weaselly, or even borderline dishonest language suggesting the contrary. It's wonderful that these experiments may be pushing us ever closer to a veritable and practical quantum computer, etc., but I do scratch my head at the notion foundational progress is being made. To use a variant of the deadly word: Really? I'll likely purchase this book, just because it's been a while since I've read anything book-length for fun. But I bet I'll still feel disappointed.

12. **Peter Woit**
September 4, 2020

LMMI,
Weinberg has continued to be concerned about whether interpretational problems mean we are missing something fundamental about QM, writing about this in 2017 in the New York Review of Books. He has pursued some ideas about introducing non-linearities in the theory, but I think concluded they don't work.

Unlike Weinberg, my point of view on this has always been that it's not quantum mechanics we don't understand, it's classical mechanics (i.e. how our classical reality emerges from a fundamental quantum theory). The quantum mechanical formalism is simple, of great mathematical beauty, and completely successful experimentally. What's hard to understand is not that story, but why the classical picture works as a way to describe our everyday experience of reality.

I don't think there's the slightest reason to believe that new more powerful experimental techniques will see a violation of the laws of quantum mechanics. What they can do though is help understand the really difficult and complicated problem of classical emergence. The paper Mark Hillery points to is a good example showing the kinds of questions that one can now experimentally study to learn more about this, experiments of a kind that Bohr and his generation could not seriously consider.

13. **Matt Grayson**
September 4, 2020

Peter,
"Everyday experience of reality" is, IMHO, the elephant in the room. I frequently compare our perception of Nature with a flatworm's experience at the Opera (borrowed from Vernor Vinge). There's no reason at all that Nature should be in the tiniest bit comprehensible to flatworms or to us. Nor need our experience meaningfully reflect it. This means giving up on the "thing-in-itself" problem – e.g., reality of the wave function or the aether – but it does not in any way mean that we should give up on science, nor fail to visit both metaphysical and empirical shores.

14. **WTW**
September 5, 2020

Peter,
When you said
"…the advent of new technologies that allow the experimental study of quantum coherence and decoherence, with great attention being paid as possible quantum computing technology has become the hottest and best-funded topic around. Whatever you think about Copenhagen, what Bohr and others characterized as inaccessible to experiment is now anything but that…"
I thought that you were referring to the now commonplace practice in quantum experiments to infer the values of observables without causing the "collapse of the wave function" — e.g., preserving coherence of separated systems even during "weak" measurements.

Similarly, "using single electrons [or photons] as observers" is precisely what recent experiments claim to be doing. Yet most philosophers of quantum mechanics seem to want to ignore such topics, and few theorists seem to want to deal directly with the results of those experiments.

In terms of understanding "quantum reality", Nature seems to want to continually throw up roadblocks in our path. See, for example, the issues related to "quantum pigeon hole" experiments,
https://www.pnas.org/content/116/5/1549
and see
https://www.sciencemag.org/news/2020/08/quantum-paradox-points-shaky-foundations-reality
also at
https://www.nature.com/articles/s41567-020-0990-x and
https://arxiv.org/pdf/1907.05607.pdf

Then there's "Closing the superdeterminism loophole
in Bell's theorem" at
http://philsci-archive.pitt.edu/16203/1/Bell%20free%20will%202019.pdf
all more grist for the mill/food for thought.

15. **Jim Baggott**
September 5, 2020

Matt,
If you're currently reading 'Quantum Reality' then you'll be discovering – spoiler alert – that I'm sympathetic to the Kantian position of giving up on acquiring knowledge of 'things-in-themselves' beyond the basic assumption that, whatever 'they' are, they continue to exist when we're not looking. This is why it's so fundamentally important in my view to try as much as possible to distinguish our (metaphysical) ideas about reality from our (empirical) experience of it.
But this is the nature of the Game we play: we blend our ideas and experience in a scientific theory and the extent of correspondence between theory and experience allows us to judge its relative success. After studying both anti-realist and realist interpretations of QM for 30 years or so, I've developed a very bad feeling about it.

LMMI,
In 'Dreams of Final Theory' Weinberg wrote: 'A final theory will be final in only one sense – that it will bring an end to a certain sort of science, the ancient search for those principles that cannot be explained by deeper principles'. I understand this to mean that, for Weinberg, finality implies futility. With the deepest possible principles in hand, there's little sense in seeking a deeper scientific understanding. We are then left with philosophical questions, for which good arguments can be formulated, this way and that, but for which lack of empirical evidence will mean that any kind of resolution or consensus on the answers will remain forever elusive. If indeed quantum mechanics is science's final word on the physics of quantum objects, then foundational questions can only ever be philosophical questions. Just don't expect physicists with more realist preconceptions to agree with this anytime soon.

Peter,
I barely touch on it in 'Quantum Reality', but anyone broadly comfortable with the notion that QM is no more than a convenient (but extraordinarily powerful) way of connecting our experiences of quantum physics will be interested to explore what this means for classical mechanics. The simple answer is of course that precisely the same interpretation can be given to classical mechanics. Instead of the debating the reality of the 'wavefunction' we debate the reality of classical concepts such as space, time, energy, mass, momentum. The fact that we can 'see' objects moving through space, in time shouldn't prevent us from realising that there's still a big difference between the 'objects in reality' and our scientific representations of them. There's no arguing with the utility of assuming that something like mass is a real physical property of a real physical thing, but I think it helps if we don't lose sight of the fact that this is still a (metaphysical) assumption.

16. **Kevin S Van Horn**
September 5, 2020

> "I don't think there's the slightest reason to believe that new more powerful experimental techniques will see a violation of the laws of quantum mechanics. What they can do though is help understand the really difficult and complicated problem of classical emergence."

You're sounding a lot like an Everettian there; those two sentences would be at home in David Wallace's book *The Emergent Multiverse*. It makes me wonder if there's really all that much difference between your positions. I guess it comes down to wave-function collapse: do you think that's an actual, fundamental physical phenomenon, or some sort of emergent phenomenon?

17. **Alessandro Strumia**
September 5, 2020

The unsatisfactory aspect (at least to me) of Copenhagen QM is that it's probabilistic. This is the root of all debates about wave-function ontology. Physical progress would be understanding how randomness arises. But we don't have sensible deterministic theories that reproduce quantum randomness in some limit, so we don't know where we should search. Maybe it's some fast high-energy dynamics, and low-energy experiments are irrelevant. For sure, neither the papers by Weinberg nor experiments about decoherence address this issue.

18. **More Anonymous**
September 6, 2020

Hey Peter,

"my point of view on this has always been that it's not quantum mechanics we don't understand, it's classical mechanics (i.e. how our classical reality emerges from a fundamental quantum theory)" … It's obvious to me that classical mechanics emerges in the limit of some parameter. Do we know what this parameter is though?
I was under the impression that it would be the thermodynamic limit but I don't know of any heursitic argument that suggests the same. Is there any in the literature?

19. **Peter Shor**
September 6, 2020

Kevin S Van Horn:

I don't see why you think Peter Woit's belief that these issues are not going to be settled experimentally has anything to do with belief in the Everett interpretation.

There are two possibilities here:
• There is some way of predicting which of the outcomes that quantum theory says are random will be realized, at least with better accuracy than quantum mechanics gives.
• There is no way of predicting these outcomes more accurately than quantum mechanics.

If the first case holds, it means that the myriads of quantum mechanical experiments we have done just happened to have their experimental set-up completely orthogonal to the means of predicting the outcomes, as otherwise we would have observed probabilities that are not in accordance with quantum mechanics.

And the second case holds if you believe in any of the standard interpretations of quantum mechanics — wave function collapse, pilot waves, or many-worlds.

So I would agree with Peter that the first case is extremely unlikely.
(Of course, we believe that QFT is incompatible with GR, so something may happen at very high energies. In this case, it may still be impossible to settle the situation experimentally, as we are not going to build particle accelerators the size of galaxies.)

20. **Peter Woit**
September 6, 2020

More Anonymous,
It's not at all obvious that classical mechanics emerges as a limit of a parameter. For an interesting read, see this blog posting by Jess Riedel
https://blog.jessriedel.com/2015/03/24/how-to-think-about-quantum-mechanics-part-4-quantum-indeterminism-as-an-anomaly/
One simple point he makes is that
"The most glaring problem is that the state spaces of classical and quantum mechanics are completely different, so you can't have a simple limiting procedure unless you describe how you're going to map one onto the other. "

21. **Peter Woit**
September 6, 2020

Alessandro Strumia (and Kevin van Horn),
Yes, I agree that's the most unsatisfactory aspect of QM, especially in the Copenhagen interpretation where you make Born's rule a fundamental axiom. I take Copenhagen to say that you can in principle analyze an experimental setup purely using Schrodinger's equation, with no probability (except that in practice, your description of the initial state is always as a mixed state), but as you try and push your description to the point where it would capture our notion of "I measured X and got result Y", at some point you're going to have to give up on Schrodinger, introduce a "Copenhagen cut", and use Born's rule.

The only substantial Copenhagen/Everettian difference I see is that the Everettian's try to get rid of the Copenhagen cut, finding a way of understanding "I measured X and got Y" in a context purely governed by the Schrodinger equation. I'm an Everettian to the extent I think it's reasonable to expect this is possible. But what I see happening when people go down this path is one of two things
1. Serious, very interesting and difficult research about the emergence of classicality and well-defined outcomes (see for example the paper Mark Hillery pointed to).
2. Just ignoring the problem or waving it away as essentially solved, and claiming silly things like invoking a multiverse solves the problem. This is the usual multiverse situation: theorists without a viable theory claiming that an empty idea solves their problems.

I'm sorry to see that 2. gets a lot more public attention than 1.

22. **Kevin S. Van Horn**
September 6, 2020

Peter Shor:

This is the relevant part: "the really difficult and complicated problem of classical emergence." Wallace's book is all about investigating how the classical world can emerge from QM, without assuming the problematic wave-function collapse.

23. **Thomas**
September 7, 2020

In the mean time I did read (most of) Baggott's book, and I have not really changed my mind regarding its merits. Some years ago I read Becker's book, which is indeed annoying in its emphasis on the struggle between the quantum dissidents against the evil Copenhagen empire. However, the book was interesting because it contained many historical tidbits that are not well known in the physics community. Bagott's book may be more sensible, but there is just absolutely nothing new or interesting here. And regarding him being more sensible, how about this about the Everett interpretation: "The ship of science is hurtling towards .. a dangerous whirlpool of metaphysical nonsense about the nature of reality" (this is about Everett, DeWitt and Wheeler, not Everett channeled by Tegmark).

24. **André**
September 7, 2020

@ Peter Woit: "… my point of view on this has always been that it's not quantum mechanics we don't understand, it's classical mechanics…"

I think this distinction is a bit mute. We understand both just fine, it is the connection of the two that is the problem. Would you say, for instance, that Boltzmann's contribution to understand the emergence of thermodynamics from statistical mechanics improved upon our understanding of thermodynamics or of (stochastic) classical mechanics? I would argue that it was neither.

The relevant question is, if the emergence of classicality can be understood on the basis of only quantum mechanics as it stands today (as in the case of thermodynamics from stochastic mechanics) or if there is some ingredient missing, something one would need to modify about quantum mechanics.

I understand your preference for the first possibility, although I don't think that the solution can come out of nonrelativistic (Schrödinger equation) quantum mechanics. But maybe there is a way to keep all of the nice mathematical structures (e.g. connections with group theory) and still understand the emergence of classicality, for instance, once gravity is properly included.

If, however, you opt for the second possibility, then it should be out of question that the missing ingredient, the required change must be something on the quantum theory side. I don't see a way how you can explain away the difficulties in the foundations of quantum mechanics by somehow modifying classical mechanics (in a way that would still be compatible with observations).

Hence, in this sense I would argue against your point, and would say that the problem with our understanding is with quantum physics and not with the properties of its classical limit. But my impression is, what you actually have in mind is a derivation in a similar sense as Boltzmann's work?

@ Alessandro Strumia: "But we don't have sensible deterministic theories that reproduce quantum randomness in some limit, so we don't know where we should search."

Although I am not a big fan of Bohmian mechanics for other reasons, I would argue that it does give a satisfactory explanation on this issue, with the Born rule following as a corollary and the randomness coming entirely from the initial conditions, just like in stochastic classical mechanics.

I agree with your feeling, that this is the most unsatisfactory aspect, and I hold a strong belief that the solution must be somewhere along those lines, that the stochasticity ultimately stems from the initial state of the universe, in some sense.

25. **peter hoffman**
September 7, 2020

Question for Peter Woit:
Does your phrase "Others' accounts are pretty much propaganda for … a many-worlds interpretation" apply to David Wallace's book? Or similarly does Baggott's "… a dangerous whirlpool of metaphysical nonsense about the nature of reality" ?
If not, indeed in any case, it would be interesting to hear more concrete criticisms of that book now, so we needn't wait for when your notes on this general question become available.
That book is at least far more (mathematically) detailed than anything either Baggott or Ball has ever written as far as I know. So it is open for no end of specific criticism.

26. **Peter Woit**
September 7, 2020

peter hoffman,
My reference to "propaganda" was to "popular accounts" (of which Wallace is not one), and for more about what I had in mind (and lots more discussion of "many-worlds" and Wallace) see here
https://www.math.columbia.edu/~woit/wordpress/?p=11128

I don't want to here start up again a serious discussion of Wallace and many-worlds, that's both a different topic than Baggott's book and I don't think renewing the old discussion is going to lead to anything new. If you don't want to read all the old stuff, the short version of my view of Wallace's book and its attempt to argue based not on physics but on decision theory, is that it seems to me to not deal at all with the real problem (the difficult physics of the emergence of classicality). Put more bluntly, I don't believe at all that it's possible to solve deep problems about physical reality using the methods of social science.

27. **Nick Herbert**
September 8, 2020

In 1984, a book on quantum foundations with the same name as Baggott's was published by Doubleday, sold close to 100,000 copies and is still in print. I know this because I am still receiving royalties from that book.

28. **Bob**
September 9, 2020

Similarly, there are many books out there titled "Quantum Mechanics", "Quantum Field Theory" or "General Relativity", etc.

29. **Robert A. Wilson**
September 13, 2020

On the strength of this review, I bought a copy of Jim Baggott's book. Once I started reading it, I found it almost impossible to put down. Very clear, well-organised, wide-ranging, balanced, exactly what a well-educated non-expert requires. It steers me back towards Copenhagen, balancing my tendency to take too much notice of Einstein.

30. **Will Sawin**
September 29, 2020

@Peter Woit: In the quest to understand how the classical emerges from the quantum, one could begin by setting some ground rules. Presumably some of the most fundamental questions are?

(1) Does the quantum wave function exist? (in some objectively real sense)
(2) Does anything except for the quantum wave function exist? (except by supervening on the quantum wave function)
(3) Does the quantum wave function evolve under a souped-up, generalized form of Schrödinger's equation? (and not some different evolution law.)

I think it's reasonable to answer "yes" to all these questions and proceed to understand how classical theories can emerge from that.

Subjective theories of the wave function would be the prototypical rejectors of (1) (in addition to QM denialists, I guess), Bohmian mechanics is the prototypical rejector of (2), and objective collapse theories are the prototypical rejectors of (3).

If you answer "yes" to all three, the existence of many worlds is the first, most basic, corollary. It's immediate that you can only hope to derive a theory of multiple, branching classical worlds, with a probability measure assigned to each.

I agree that it is silly to take this trivial observation and then claim that you have completely solved the difficult problem of classical emergence.

I also think it is strange to accept the premises and rush off into attacking this difficult problem using lots of math without acknowledging the elementary point that there are many worlds.

31. **Peter Woit**
September 29, 2020

Hi Will,
My problem is with 1, I think it's meaningless and does nothing but obfuscate the fundamental problem.

Personally I'm not rushing off trying to solve the problem of classical emergence,I can see why it's extremely difficult and not something I have the energy or competence to make any progress on. If someone has a promising idea, that's great and should be encouraged. As for those who think the reality of multiple worlds is some sort of insight needed to move forward, this proposal is now as ancient as I am (I was born in 1957, same year as Everett's publication), and in a long career I've seen little worthwhile coming from it. Worse, in the last two decades I've seen a huge amount of damage to the field of fundamental physical theory from bogus claims that a many worlds ontology solves one important problem or another. I've seen such claims do a good job of selling books and convincing people not to work on important problems, seen none that they further any real science.

32. **Will Sawin**
September 30, 2020

Hi Peter,

Thanks for allowing us to discuss quantum interpretations in your comments section, even though it can get silly and repetitive.

I don't think many worlds is necessarily an insight needed to move forward. It's like, the fact that rocks on the moon are grey in color is not really a big insight into lunar geology, and is not really needed to move forward in any research direction for studying the moon. But it would still be strange to study moon rocks and steadfastly refuse to ever let slip that they are grey.

I admit, though, that it's possible that saying that there exist multiple worlds has a poisonous effect on many people's minds that impedes scientific progress. You would certainly know more about this than me.

How is anyone going to derive the classical from the quantum if they don't first accept that the quantum state exists? Is the plan to describe how something that exists emerges from something that doesn't exist?

To me, the blog post you linked, and the paper that Mark linked, and the other paper you linked in a previous discussion from this, all look like they are starting from the assumption that a quantum state, possibly though not necessarily a pure quantum state, exists as a real physical object, and then describing why that object would "appear" in some sense to have classical behavior. If to you they look like an alternative to doing that, then probably that points the way to a more fundamental disagreement.

33. **Peter Woit**
October 1, 2020

Will,
My problem here is that I don't believe the statements "the quantum state exists" or "many-worlds exist" have any substantive meaning. As such, in practice such statements shed no light on anything, and, when coupled with bogus claims that such statements solve significant problems (see e.g. Sean Carroll's recent book) can be seriously counter-productive.

I'm perhaps over-sensitive to this because of the huge damage done to HEP theory by similar claims about a supposed cosmological multiverse. I wrote specifically about that in the short essay "Theorists without a Theory", but much the same argument applies to the QM case. People are claiming to have solved a deep problem with an untestable idea, and if you look into it you realize that the source of the untestability is that there is no actual theory there. Their idea provides nothing substantive that addresses the problem at hand.

34. **John Baez**
October 1, 2020

Will Sawin wrote:

> (1) Does the quantum wave function exist? (in some objectively real sense)
> (2) Does anything except for the quantum wave function exist? (except by supervening on the quantum wave function)
> (3) Does the quantum wave function evolve under a souped-up, generalized form of Schrödinger's equation? (and not some different evolution law.)
>
> I think it's reasonable to answer "yes" to all these questions and proceed to understand how classical theories can emerge from that.

From the rest of what you said, I believe you think it's reasonable to answer "no" to question (2). Or did I misunderstand?

If someone answers "yes" to question (2), my followup question is "what?"

35. **Will Sawin**
October 2, 2020

Peter,

If you're not prepared to assert the quantum state exists, what does exist? Do electrons exist? Do Higgs bosons exist? Do I exist?

You can deny that something like, say, the sun, exists, but believe that every other observable phenomenon behaves as if there were a miasma of incandescent plasma in the sky illuminating everything half the time. If so, then someone telling you that the sun exists wouldn't really shed any light on anything. But, I mean, the sun exists, and the sun is made mainly of quarks, so quarks exist, and quarks are excitations in the state of certain quantum fields, so that state exists.

To me it's simpler to take the best, most mathematically elegant theory of the universe, and accept that whatever structures it describes exist, no matter how silly they sound.

I really don’t think the analogy between many world and the multiverse is that good. One proposal has the problem that there is no good notion of measure, the other we know roughly which notion of measure to use. One has the problem that it makes no predictions, except sometimes you can get it to make predictions that contradict our observations, while the other predicts our observations beautifully, with the only drawback that the exact same predictions are also made by a different, worse, theory that was discovered earlier because of a failure of bravery and imagination (and maybe an attachment to subjectivity).

36. **Peter Woit**
October 3, 2020

Will,
I don’t disagree with your comments about the use of the word “exist”, I just think they’re irrelevant to the problem. The situation seems not that different to me than arguing that whether or not prime numbers “exist” is relevant and important to understanding the problem posed by the Riemann hypothesis.

37. **Will Sawin**
October 7, 2020

Peter,

I mean, the Riemann hypothesis is a very interesting problem, but it’s a problem of mathematics, not physics. If prime numbers existed in the physical world in some clear sense, then the Riemann hypothesis might be considered a physical problem.

If this problem of “how does the classical emerge from the quantum” is supposed to be considered a physics problem, then presumably some component of the problem has to be something that really physically exists or a reasonable approximation of something that exists. I think one can argue that if anything in the setup exists, it has to be the quantum state, and that any attempt to argue that the quantum state is an approximation to something else that doesn’t lead to “many worlds” conclusions is untenable.

So I don’t think you need to claim that quantum states exist to solve the problem. But at some point you need to justify why you are working on this problem and not, say, relaxing on the beach. I think “I am trying to understand what everything in the universe is made of” is a compelling answer to that.

AMS Open Math Notes

September 6, 2020
Categories: Uncategorized

The AMS for the last few years has had a valuable project called AMS Open Math Notes, a site to gather and make available course notes for math classes, documents of the sort that people sometimes make available on their websites. This provides a great place to go to look for worthwhile notes of this kind (many of them are of very high quality), as well as ensuring their availability for the future. They have an advisory board that evaluates whether submitted notes are suitable.

A couple months ago I submitted the course notes I wrote up this past semester for my Fourier Analysis class, and I'm pleased that they were accepted and are now available here at the AMS site (and will remain also available from my website).

Comments

1. **Swol**
   September 6, 2020

   The idea of the AMS Open Math Notes is cool, but it is unfortunately very much not "Notes" or "Open".

   It is not "Open" in the sense that their reCAPTCHA (the thing that's checking to see whether you're a robot) doesn't work. It isn't "Notes" either because, for one particular entry (and maybe for more, haven't checked) you get a message "This book is now available for purchase at the AMS Bookstore." and no access to a pdf!

2. **Peter Woit**
   September 6, 2020

   Swol,
   I haven't had any trouble with their reCAPTCHA thing, or run into an example of something they had shifted to a book they publish and sell. It is worth noting that their traditional business model for how to finance the AMS is to get a lot of their revenue from selling books and journal subscriptions, so unless they come up with a new revenue source they're not likely to give that up.

3. **ArshadM**
   September 28, 2020

   I'm not sure who the target market is for this initiative, I am guessing it's fellow lecturers that can borrow and enhance. As a self-study student, these kinds of notes are not very helpful without problem sets and solutions (I don't mind paying a little bit for the solutions and that could be a source of revenue for the AMS/Author).

Fall Quantum Mechanics Class

September 7, 2020
Categories: Quantum Mechanics

I'll be teaching a course on quantum mechanics this year here at Columbia, from a point of view aimed somewhat at mathematicians, emphasizing the role of Lie groups and their representations. For more details, the course webpage is here.

The course is being taught online using Zoom, with 37 students now enrolled. I've set things up in my office to try and teach using the blackboard there, and will be interacting with the students mostly via Zoom. As an experiment, I've also set up a Youtube channel. If all goes well you should be able to find a livestream of the class there while it's happening, which is scheduled for 4:10-5:25 Tuesdays and Thursdays, starting tomorrow, September 8. I'll also try and make sure the recorded livestreams get uploaded and saved at this playlist. Unfortunately I won't be able to interact with people watching on Youtube, should have my hands full trying to get to know the students enrolled here in the course, with only this virtual connection.

Comments

1. **David Appell**
   September 7, 2020

   Peter, making your course widely available is really good of you. Thanks.

2. **Jeff Berkowitz**
   September 7, 2020

   Hello Peter. The link from your syllabus to John Preskill's lecture notes on quantum computation (http://www.theory.caltech.edu/people/preskill/ph229) is giving me Caltech's 404 page, both with and without the #lecture anchor.

3. **Blake Stacey**
   September 7, 2020

   I taught graduate statistical physics via Zoom back in the spring. It's definitely a different experience than lecturing live, with a lot less feedback. With the students on mute, you of course don't get the laughs at little jokes, and because everyone is visible in tiny boxes if at all, body language is much harder to read. It can feel like speaking into a void. But I still got reasonable questions in the video chats and by e-mail, the performance on problem sets and the midterm was in line with my expectations, and the final presentations that the students gave went well.

   Good luck!

4. **Anish**
   September 8, 2020

   Interacting with students is quite challenging in this remote setting. I suggest having a shared Google Doc between you and your students where students can give their feedback and ask questions increasing interaction in students themselves and Zoom chat is not really that easy to navigate. Furthermore, A YouTube channel named 3blue1Brown, which just started doing some live streaming, published an article with some tips for remote lessons like live quizzing, etc that you may find useful to integrate in your system. (https://www.3blue1brown.com/blog/livestream-setup). Thanks for making the lectures public.

5. **E K Shaji**
   September 8, 2020

   That's really cool, but can you also try to make your PSets/Assignments widely available?

6. **Michael Ball**
   September 8, 2020

   You should check out Richard Borcherd's Youtube channel. At the moment it has great content for undergrads. Definitely worth keeping an eye on (sorry if I posted this comment twice!)

7. **cyd**
   September 8, 2020

   Use a visualizer. The quality is much better than with a blackboard, unless you have a seriously good camera and audio equipment. They're going for $100 on Amazon.

8. **Peter Woit**
   September 8, 2020

   Jeff Berkowitz,
   Thanks. Fixed.

   E.K. Shaji,

   The assignments will be pretty much the ones in the book, although I may be revising those and adding some more. They're listed on the course page
   https://www.math.columbia.edu/%7Ewoit/QM/fall2020.html

9. **I**
   September 8, 2020

   Peter, will you be placing solutions to problems on your web page for those watching on Youtube?

10. **Peter Woit**
    September 8, 2020

    I,

No, I won't be making solutions to the problems available. Having solutions to problems available makes people less likely to engage with the whole point of such things. What's important is not solving a problem, it's what you learn by struggling with understanding how to solve the problem.

11. **I**
September 8, 2020

Peter,
Doesn't putting the solutions up a few weeks after each problem set have the same benefits? Plus the students get feedback on whether they're right. Alternatively, you could stream lectures where you go over the problems afterwards.

12. **Robert A. Wilson**
September 9, 2020

Michael Ball,
Richard's name is Borcherds, not Borcherd.

13. **Peter Woit**
September 9, 2020

I,
I generally go over some of the problems that students have had trouble with in class, likely will also do that this year, so that would be on the videos. The "give solutions to the students after the problems are due" model only works if everyone is doing the problems with the same due date, and the solutions don't become public. I hope lots of people are reading the book, and now watching the lectures, at very different times, and best for all of them if they struggle through the problems.

Something one doesn't appreciate as a student typically is that it's quite hard to come up with good problems at the right level. The problems in the book took a lot of effort to come up with, and they are not that great, could use a lot more work to improve them and come up with more. I hope to do some of that this year, but it's a task I find difficult and not rewarding (even though it really is important for the students).

14. **Michael Ball**
September 9, 2020

Robert A. Wilson

Noted

15. **Bill93**
September 9, 2020

Thanks for making available. Zoom screen sharing could let you display PowerPoint or other visuals prepared in advance and might let the discussion flow more smoothly, avoiding delays while something gets written on the chalkboard. Also, not pushing for speed and thus avoiding false starts might result in a similar overall pace.

16. **Peter Woit**
September 9, 2020

Bill93,
Thanks for the comments/suggestions, that's helpful.

I should probably make much more explicit the relation of what I'm doing in class to the book. For most of the lectures, essentially I'll be going over one chapter of the book, but what I'm not trying to do is give a polished lecture covering everything in that chapter. Instead the idea is to try and emphasize the main ideas, go over confusing points carefully, encourage students to stop me with questions and have lots of time for them.

I could in principle screen share the page of the book corresponding to what I'm talking about. What would really be ideal is if students try reading the chapter beforehand, mark it up with their questions, then during the lecture stop me if their question is not clarified by what I say when I get to that point (sorry to those of you on Youtube who can't ask questions. If people email me questions before the lecture I may or may not be able to say something about them during the lecture).

17. **pmcs**
September 9, 2020

Hi Peter,

I look forward to the rest of this series. One slight issue: when I try to save the first video to watch later, Youtube tells me that this is forbidden since "This action is turned off for content that is made for children."

Do you happen to know how this might be overcome?

18. **Peter Woit**
September 10, 2020

pmcs,
Thanks for pointing this out. That should be fixed now. I had selected a "for kids" option, thinking that meant "all right for kids" (why shouldn't kids be learning about unitary Lie algebra representations?). Instead it meant "intended for kids", which is different.

19. **Tobin Fricke**
September 26, 2020

Students of this class might also enjoy A. Zee's book titled "Group Theory in a Nutshell for Physicists." It's an excellent tour of the Lie groups and why we care about them; kind of a much more fleshed-out and beginner-friendly version of Georgi's famous "Lie Algebras in Particle Physics."

Another nice resource is Frederic P Schuller's "Lectures on the Geometric Anatomy of Theoretical Physics" on YouTube, which is a sort of grand tour of the relevant mathematical structures starting with the definition of sets, topology, fields, groups, etc. He has a shorter "winter school" version that also includes some exercises.

Thanks for making your lectures and text available here – I've enjoyed the first several lectures and hope to continue!

Quick Links

September 10, 2020
Categories: Uncategorized

A few quick links:

- I was sorry to hear of the recent death of Vaughan Jones. A few things about his life and work have started to appear, see here, here and here.
- For a wonderful in-depth article about the life of Michael Atiyah written by Nigel Hitchin, see here.
- There are now many new places where you can find talks about math and physics to listen to. For instance, just for math and just at Harvard, there is a series of Harvard Math Literature talks and Dennis Gaitsgory's geometric Langlands office hours.
- Breakthrough Prizes were announced today. There's an argument to be made that the best policy is to ignore them. Weinberg has another 3 million dollars.
- For an interview with Avi Loeb about why physics is stuck, see here.
- For an explanation from John Preskill of why quantum computing is hard (which I'd claim has to do with why the measurement problem is hard), see here.

Update: Last night I watched The Social Dilemma on Netflix, which included some segments with my friend Cathy O'Neil (AKA Mathbabe). Highly recommended, best of the things I've read or watched that try and come to grips with the nature of the horror irresponsibly unleashed by Mark Zuckerberg and Facebook in the form of the AI driven News Feed. Comparing to a documentary about Oxycontin from a while back, the effects of the News Feed are arguably more damaging. I'm wondering why the Oxycontin-funded Sackler family donations to cultural organizations and universities have been heavily criticized, unlike the News Feed-funded Zuckerberg/Milner donations to scientists.

Update: Alain Connes has written a short appreciation of Vaughan Jones and his work here.

Update: For another article about Vaughan Jones well-worth reading, see Davide Castelvecchi at Nature.

Comments

1. **Hansi**
   September 10, 2020

   Peter Woit wrote:
   "Breakthrough Prizes were announced today. There's an argument to be made that the best policy is to ignore them. "

   Well the "Breakthrough Prize in mathematics" is actually for a serious work in mathematical physics:

   https://www.imperial.ac.uk/news/203853/imperial-mathematician-scoops-3m-breakthrough-prize/

   Professor Hairer is recognised "for transformative contributions to the theory of stochastic analysis, particularly the theory of regularity structures in stochastic partial differential equations."

   When you look on the work for which the prize was given

   https://arxiv.org/abs/1303.5113

   then you see that it threats the stochastic quantization (a mathematical version of quantization that works by converting a classical system into a stochastic pde) and the renormalization of phi^4 theory.

   As the author writes:
   "One major difference between the results presented in this article and most of the literature on quantum field theory is that the approach explored here is truly non-perturbative"

   It is always important if someone invents mathematically rigorous methods for threating quantum field theories non-perturbatively, because more difficult systems like qcd and gravity display intense self interactions where such methods could be helpful.

   I therefore would say that the prize for this work is well deserved.

   The paper should be read, especially by mathematical physicists and high energy theorists.

2. **Peter Woit**
   September 10, 2020

   Hansi,
   No disrespect intended towards the winners of the math and physics Breakthrough Prizes, who are quite distinguished scientists. Note that Martin Hairer has already won many awards for his work, including a Fields Medal. He's not an unknown. To the extent that it's a good idea to pay attention to prizes, there's a good argument that the ones awarded by the math community are worth paying attention to. As for the ones trying to make a splash using large sums of Zuckerberg money, maybe you should (after deleting your Facebook account) ignore those. In other words, follow the example of Peter Scholze…

   For the physics prize, among the many good reasons to appreciate the work of Steven Weinberg, the $3 million he got today isn't among the top 1000.

3. **gentzen**
   September 11, 2020

   The lecture series from ETH like the Paul Bernays Lectures or the Wolfgang Pauli Lectures are also nice talks about math and physics to listen to.

4. **Jackiw Teitelboim**
   September 11, 2020

   I only wonder why some people are "ignored" in these prizes, like Deser for SUGRA, and Glashow for the SM now.

5. **Peter Shor**
   September 11, 2020

Jackiw asks why Weinberg was given a Breakthrough Prize for his role in the SM, while Glashow wasn't.

This is baseless speculation, but one possibility is suggested by the following two quotes from their Wikipedia pages:

> Glashow is a skeptic of superstring theory due to its lack of experimentally testable predictions.
>
> Steven Weinberg continued his work in many aspects of particle physics, quantum field theory, gravity, supersymmetry, superstrings and cosmology.

I'd like to believe this wasn't the reason, and maybe it wasn't. You could also argue that the sum total of Weinberg's research outweighs Glashow's.

6. **Douglas Natelson**
September 11, 2020

   And once again, in a relatively high profile platform like Salon, the Loeb interview uses the rather global term "physics" when they mean high energy theory.

7. **John Baez**
September 17, 2020

   To me the most exciting prize handed out by Breakthrough this year was the Maryam Mirzakhani New Frontiers Prize given to Nina Holden for her mathematically rigorous work on random surfaces and Liouville quantum gravity, one of the simplest irrational conformal field theories. I think it was given for her 7-paper series leading up to Convergence of uniform triangulations under the Cardy embedding with Xin Sun. I wrote a quick basic overview of some of the underlying ideas.

8. **SD**
September 20, 2020

   Given this is a post on various links, I thought your readers might like to know that Richard Borcherds has been putting up excellent video lectures on graduate courses in Algebraic Geometry, Representation Theory etc.

   Youtube Playlist

9. **Generic mathematician**
September 20, 2020

   Is there a good summary that describes the work of Vaughan Jones that Alain Connes briefly describes in his blog – something accessible to a mathematician without specialist background. (I'm familiar with the Jones polynomial defined via skein relations, but I know absolutely nothing about the underlying inspiration apparently arising from some classification in von Neumann algebras that somehow relates to knots – but I got intrigued by the brief description in Connes' blog.)

10. **Rob Meyer**
September 22, 2020

    Here is the citation for Steven Weinberg:

    2020 Special Breakthrough Prize in Fundamental Physics

    For continuous leadership in fundamental physics, with broad impact across particle physics, gravity and cosmology, and for communicating science to a wider audience.

11. **Pineapple**
September 23, 2020

    Thank you for pointing me at The Social Dilemma, I enjoyed it a lot and have spent some time since creating my own news aggregation using plain old RSS (your blog was first!). It's hard to find an RSS reader that doesn't ask you to create an account but once you've managed that and if you use FireFox Focus over ProtonVPN then your news and aggregation of it are finally private and not the subject of scrutiny by an overbearing AI marketing model. One thing that's a little frustrating is that so many sites have abandoned RSS in favour of Facebook and Twitter buttons!

12. **Arnold Neumaier**
September 25, 2020

    The full citation for Weinberg's price is here:
    https://breakthroughprize.org/News/61

13. **Richard**
September 27, 2020

    Pineapple,

    Which RSS reader are you using?

14. **Grigori Avramidi**
September 29, 2020

    Michael Freedman's Sept 28 talk in the Harvard Math Literature series was especially good. It gets into the meat of his 4-d Poincare proof and at the same time makes it look … not that hard. Hopefully they will have the video up soon.

15. **Davide Castelvecchi**
October 2, 2020

    Generic mathematician: I found Joan Birman's lecture about Jones' work at the ICM quite helpful and concise
    http://www.icm2002.org.cn/general/prize/medal/1990/Jones/page1.htm

16. **Generic mathematician**
October 2, 2020

    Davide Castelvecchi, thank you!

17. **Neville**

October 10, 2020

Peter, thanks for the link to Hitchin's absolutely terrific article about Atiyah!

2020 Physics Nobel Prize

October 6, 2020
Categories: Uncategorized

The 2020 Physics Nobel Prize was announced this morning, with half going to Roger Penrose for his work on black holes, half to two astronomers (Reinhard Genzel and Andrea Ghez) for their work mapping what is going on at the center of our galaxy. I know just about nothing about the astronomy side of this, but am somewhat familiar with Penrose's work, which very much deserves the prize.

Penrose is a rather unusual choice for a Physics Nobel Prize, in that he's very much a mathematical physicist, with a Ph.D. in mathematics (are there other physics winners with math Ph.Ds?). In addition, the award is not for a new physical theory, or for anything experimentally testable, but for the rigorous understanding of the implications of Einstein's general relativity. While I'm a great fan of the importance of this kind of work, I can't think of many examples of it getting rewarded by the Nobel prize. I had always thought that Penrose was likely to get a Breakthrough Prize rather than a Nobel Prize, still don't understand why that hasn't happened already.

Besides the early work on black holes that Penrose is being recognized for, he has worked on many other things which I think are likely to ultimately be of even greater significance. In particular, he's far and away the person most responsible for twistor theory, a subject which I believe has a great future ahead of it at the core of fundamental physical theory.

In all his work, Penrose has shown a remarkable degree of originality and creativity. He's not someone who works to make an advance on ideas pioneered by others, but sets out to do something new and different. His book "The Road to Reality" is a masterpiece, an inspiring original and deep vision of the unity of geometry and physics that outshines the mainstream ways of looking at these questions.

Congratulations to Sir Roger, and compliments to the Nobel prize committee for a wonderful choice!

Comments

1. **Shantanu**
   October 6, 2020

   Peter, a data point. About two decades ago, students requested that Roger Penrose give a physics colloquium at MIT, but they were vetoed by the faculty there, who thought his talk would be too fringe or non-mainstream

2. **David Appell**
   October 6, 2020

   I saw someone speculate on Twitter that this may be a reaction to regret among the Nobel Academy that they didn't give Hawking a Prize.

3. **Tim Bradshaw**
   October 6, 2020

   When I read the news that he had jointly won this morning I just immediately thought how right it was that he should win. I don't know the rules around the prize, but he richly deserves an award for the singularity theorems, and as you say his other work (one day I will try, again, to get my head around twistors: there must be better resources on them now than there were in the 1980s) as well.

   This is not to sneer at the other two recipients: the kind of experimental work which has gone on around GR since I stopped trying to be an academic is just heroic I think.

   But the news made my day, anyway.

4. **william e emba**
   October 6, 2020

   The Penrose-Hawking theorems were the main reason astronomers stopped deluding themselves that singularities were avoidable. No-hair theorems did not exist yet. (To be fully truthful, they don't really exist to this day.) Big Bang models at the time were along the lines of Gamow's Ylem. If you wanted to be sophisticated then, you did something like Lifshitz's linearized perturbation calculations that incorrectly ignored self-gravitation.

5. **Andrew P. Mullhaupt**
   October 6, 2020

   Lars Onsager got the Chemistry Nobel; his Ph.D. is technically in chemistry from Yale, where he was a postdoc (actually accidentally predoc) and they suggested he use one of his published papers, but he did new research instead, on periodic solutions ofthe Mathieu equation. At the time none of the chemistry or even physics faculty at Yale could decide whether to award a Ph.D. for this, so they took it to the mathematics department who said that they would award a Ph.D. for it if nobody else did.

   So Lars Onsager, got the Chemistry Nobel (but really for physics) while holding a Chemistry Ph.D., which was really a Mathematics Ph.D. (in sheep's clothing).

6. **Peter Woit**
   October 6, 2020

   Andrew P. Mullhaupt,
   Thanks, I didn't know the Onsager story. Too bad his prize wasn't for his solution to the 2d Ising model. Then the first prize in mathematical physics would have been a prize in Chemistry to a Chemistry Ph.D….

7. **Pascal**
   October 6, 2020

   From the Nobel prize press release about the massive object at the center of our galaxy: "Around four million solar masses are packed together in a region no larger than our solar system." The Schwarzchild radius of the sun is about 3km, so for 4 million solar masses we have a Schwarzchild radius of 12 million km. This is much, much smaller than our solar system (for comparison, the distance between the Earth and the sun is 150 million km).
   So how do we know that this is really a black hole?

8. **Maciej**

October 6, 2020

It is pretty clear to me why Penrose did not receive the breakthrough prize.
First couple of breakthrough prizes went to string theorists who then became members of the board that decides who gets the prize. Penrose has never been hiding his strong critique of strings (in books, interviews et.c.), so it adds up.

9. **gtr_gradstudent**
October 6, 2020

This news made me check if Roy Kerr was still alive. And indeed he is.

On the topic of the mathematical physics, I wonder if physicists know or care about Christodoulou and Klainerman's work, which to me is one of the summits of the subject in the last fifty years.

10. **Mark Weitzman**
October 6, 2020

The real question is why it takes about 55 years to give such an award? I remember first reading about Penrose (and Hawking) in MTW's Gravitation book published in 1973. I realize it helps to have observational evidence of black holes, but I think this has been around for quite a while. I am glad that he lived long enough to get his recognition from the Nobel Committee.

11. **martibal**
October 6, 2020

Besides the question of the title of the PhD, is there any other example of a Nobel prize awarded for a theorem ? As a side question: what about the Fields Medal ? Was Penrose already to hold for it, or was not it considered as a mathematically significant enough result ?

12. **Chris Oakley**
October 6, 2020

I never expected Penrose to get a Nobel Prize, but am very glad that he did. A true original thinker!

13. **Peter Woit**
October 6, 2020

martibal/Mark Weitzman,

I don't know of another example of a theorem getting a Nobel Prize. Hawking I think never got a Nobel prize because Hawking radiation is all too testable in principle, but not in practice and the theorems with Penrose were theorems and thus not something testable about the real world.

One guess as to what's going on here is that the lack of good new testable ideas about fundamental physics in recent decades has meant that if the Nobel committee wants to stick to rewarding only things that pass experimental test, then they will have to give awards to less and less impressive results. At some point the fact that they are ignoring hugely important ideas like those of Hawking and Penrose starts to become an obvious problem and perhaps this has caused them to rethink their criteria. In Penrose's case, the fact that he has a hugely impressive other body of work gives a good reason to find something to give him the prize for, even if they need to fudge their usual criteria. Hawking unfortunately didn't live long enough to take advantage of this.

14. **DS**
October 6, 2020

Dear Professor Woit,

I think the comment about the prize being awarded for a theorem in mathematics is a mischaracterization. It is not at all about making a previously known physical result rigorous!

In those days most physicists (including for example John Wheeler) believed that the singularities of the Schwarschild and Kerr solutions were unphysical and were due to either a poor choice of coordinates or because of symmetries.

Penrose showed that singularities were inevitable and physical, at least in the sense that empirically verified equations predicted them in generic situations (not only in special, symmetric solutions).

Hence, Penrose radically changed the physical picture and he did it by proving a mathematical theorem!

15. **Nicole**
October 6, 2020

Why doesn't anyone read the advanced information on the website?

https://www.nobelprize.org/uploads/2020/10/advanced-physicsprize2020.pdf

Nobel prizes are not given for theories. They are given for fundamental discoveries. Even Einstein's Nobel was for one of his ideas proven through experimentation.

Clearly it was Penrose's 1965 paper which won him this year's prize. Hawking would not have shared the prize even if he were alive. Penrose's ideas came before Hawking's. The Nobel does not award subsequent or derivative work.

16. **rhodium**
October 6, 2020

I cannot speak for physics, but it seems sometimes a chemistry prize is awarded to X for being who he is. Perhaps Penrose got the prize because he is Penrose and nobody else is.

17. **Peter Woit**
October 6, 2020

DS,

You won't find me arguing for a rigid distinction between math and physics, or that proving a mathematical theorem about a set of equations can't provide new insight into them that counts as "physics".

18. **FlyingCar**
October 6, 2020

I have mixed feelings tbh. I love Penrose's work, but, like with Hawking's theorems, rigorous experimental verification is almost impossible by the very nature of the topic. So why relax prize requirements decades after the theorems were first proposed? Perhaps it's to benefit the very theories Penrose dubs 'Fashion, Faith and Fantasy'. What's to prevent work on inflation from being awarded next year? I hate to sound conspiratorial but I actually think it's very likely to happen now.

Frankly, Penrose deserved a share of the Nobel prize for quasicrystals back in 2011. It's really strange how it all turned out.

19. **Alessandro Strumia**
October 6, 2020

The problem is that the time gap gets so large that it's more important than the science. The main work was done by Einstein, Hilbert and Schwarzschild. They died long ago. Hawking died in 2018. Why waiting 2020, if this old work deserved a prize? And why not Kerr, given that LIGO/Virgo see rotating black holes?

20. **Chris W.**
October 6, 2020

By the way, in 2017 Quanta Magazine published [this interview] with Andrea Ghez.

21. **Justin Glick**
October 7, 2020

This seems to be the anti-symmetric analog of Witten's 1990 Field's Medal. Wasn't the math community surprised to see a physicist win the medal, much the same as some in the physics community are surprised to see a mathematician win the Nobel?

22. **Ricardo Cavalcanti**
October 7, 2020

Max Born was another recipient of the Nobel Prize in Physics with a Ph.D. in Mathematics. (Source: https://en.wikipedia.org/wiki/Max_Born)

23. **Lars Johansson**
October 7, 2020

I was also delighted that Roger Penrose was awarded, although I'm not familiar with the details of his work on black holes. I've read his book The Road to Reality over and over and I'm slowly beginning to grasp more and more of it. It's totally fascinating!

According to the Nobel committee, this is the first time in history that GR is awarded (remember that Einstein was not awarded for GR but for his work on the photoelectric effect).

Besidess, I think that Penrose could as well have been awarded the 2011 Nobel prize in chemistry for his work on "Penrose tilings" which paved the way for the discovery of quasicrystals.

24. **Jackiw Teitelboim**
October 7, 2020

Eugene Paul Wigner was given the Nobel prize "for his contributions to the theory of the atomic nucleus and the elementary particles, particularly through the discovery and application of fundamental *symmetry principles*."

Formally, Wigner was a chemical engineer, whose thesis was Bildung und Zerfall von Molekülen ("Formation and Decay of Molecules"). However, Wigner education, thanks to the flourishing interaction between the mathematical and physical sciences at Göttingen around the 1920's, is not easy to define him into simple categories such as theoretical/mathematical physicist or a pure mathematician. After his thesis with PhD work with Michael Polanyi, he was interacting with people ranging from Arnold Sommerfeld to David Hilbert. So regardless what his PhD was for, it's still worth mentioning that *group theory* was the main reason for his Nobel prize.

In this sense, I consider his nomination as a recognition of representation theory in QM, even before gauge theories and the geometrical theory of p-bundles became widely recognized as the mathematical language of the fundamental interactions.

This besides Bohr calling it Gruppenpest.

PS: Pascal Jordan certainly was a founding member of QM and a high caliber mathematician, and I am sure he did not received a Nobel prize for reasons which the interested reader may find for herself by searching his biography, e.g., Bert Schroer's https://arxiv.org/abs/hep-th/0303241

25. **Jim Holt**
October 7, 2020

I agree that "The Road to Reality" is a masterpiece. In ploughing through it repeatedly over the years, I've learned more from it about a wider range of topics than any other math/physics book I can think of. (Although I hate his tensor diagrams!)

26. **DS**
October 7, 2020

For those complaining that the Nobel committe changed its criteria, in 1999 T'Hooft and Veltman got the Nobel prize for showing the renormalizability of Yang-Mills theories and as far as I know you can't experimentally verify if a theory is renormalizable. Also as far as I can tell https://webspace.science.uu.nl/~hooft101/gthpub.html they did not make any experimental predictions in the work that was awarded the prize. The quote is "for elucidating the quantum structure of the electroweak interactions in physics" and analogously Penrose elucidated the structure of general relativity, showing it generically produces singularities.

On the whole, asking for experimental verification on Penrose's work is nonsensical. If you went inside an event horizon and never met a singularity, the truthness of the theorem remains, it simply means that in the real world either the energy condition is broken or general relativity is incorrect at a certain scale. Either of these breakdowns would be revolutionary. The prize is due to: "for the discovery that **black hole formation is a robust prediction of the general theory of relativity**." It is a statement about general relativity as a theory, not about reality.

27. **Tim Bradshaw**
October 7, 2020

Pascal,

I believe the most conclusive evidence that Sag A* is a black hole is that orbital motions have been detected very close to the innermost stable orbit of the object if it is indeed a BH, and that these observations agree very closely with models of what I think are hot spots in the accretion disk. These orbits have periods of under an hour and velocities around 0.3c. ArXiv copy of the paper on this is here: https://arxiv.org/abs/1810.12641.

Apart from that, there's the question that we don't have any other candidates for anything that would pack 4 million Solar masses into something the size of the Solar system, I think.

28. **Laurence Lurio**
October 7, 2020

I am a bit surprised so many people love "The Road to Reality". For a book that is supposed to assume no prior knowledge of physics, I found it confusing and very dense. I came at it with a prior knowledge of physics and it took ideas which I previously understood and made them confusing. Possibly fun to look at things from a new perspective, but a lot of work to plow through when the end goal is twistor theory, which seems to be a Penrose pet project that no one else really uses. Possibly this book is like the Feynman Lectures, supposedly aimed at the neophyte but only really read by the experts looking for a novel approach. Is anyone aware of someone without a degree in physics who was actually able to get something out of this. Who is the intended audience? Maybe the next Penrose ?

29. **Peter**
October 7, 2020

I think it was Pauli who first used the expression "Gruppenpest".

30. **Peter Woit**
October 7, 2020

Laurence Lurio,
It's definitely not a book for beginners. It is highly original: Penrose is doing things his way, not the way that is in most standard textbooks. This is confusing at first, he's asking you to think about the subject of space-time geometry in a different way than you are used to. I'm sold on the twistor point of view, but even if you're not, his way of thinking puts front and center the conformal geometry of Minkowski space, which is rather different than the usual way of thinking about space-time geometry, and often more enlightening. The work he got the Nobel prize for grew out of this point of view, showing the power of it.

31. **Low Math, Meekly Interacting**
October 7, 2020

I consider Penrose a fine visual artist, something I appreciate all the more because he is able to express his ideas so accessibly with pictures. I encountered the famous figure representing trapped surfaces many years ago and found it stirring. Likewise the Penrose diagram of a Kerr black hole (however unphysical, I'm not aware of a more concise description of how bizarre it really is). The only explanation of Twistors I've really been able to get my head around came from him: Start by imagining the dome of the heavens, full of stars…

I always admired Penrose most for communicating a highly original perspective that is nonetheless bracingly lucid and (perhaps deceptively) comprehensible. I've had similar feelings about Feynman's popularizations. They both (I hope) helped me understand better how incredible advanced concepts in established physics can still be.

I don't know who really "deserves" a Nobel Prize, but I'm very encouraged that he got it.

32. **Stuart**
October 7, 2020

I'm curious where my countryman Roy Kerr fits in on the Nobel front. My understanding is that the Event Horizon Telescope data from last year confirmed his model of black holes, but this year's prize is about black holes but not about the EHT data?

Like Penrose, Kerr's no spring chicken. Hang in there, Roy!

33. **Matt Grayson**
October 8, 2020

I have no physics degrees. I loved "Road to Reality". I did not *understand* everything in it, but it's a large book and contained much that was interesting – more details than a popular science book, fewer than a graduate text. Feynman's QED is the only similar, though more basic, book I can think of.

34. **Peter Woit**
October 8, 2020

Matt,
It should be mentioned that your professional background is in geometry, which should have helped a lot to make the book accessible.

35. **Jules**
October 8, 2020

Peter, in 1997, out of curiosity, I went to the "Strings" Conference in Amsterdam. Hawking spoke with his computer voice. He said: "It is a pity that a black hole has not been found yet, because then I would get the Nobel prize." He was really unlucky …

36. **Peter Woit**
October 8, 2020

Jules,
I think to count as experimental vindication of Hawking's work, you'd need not just finding a black hole, but observing Hawking radiation from it. That hasn't happened yet, and I suspect is a very long time off. As Nicole above pointed out, the singularity theorem for black holes was due to Penrose, not Hawking, so the reason this award went to Penrose would not have worked to justify an award for Hawking.

37. **EDP**
October 8, 2020

Roger Penrose is certainly a brilliant mind, but why does he think that consciousness somehow arises from the quantum vibrations of microtubules? This I cannot fathom.

38. **Christopher Blanchard**
October 9, 2020

I am the neophyte Laurence Lurio can't see. My degrees are in economics and management, which are juvenile and silly attempts at intellectual disciplines. I have no great talent for mathematics, though economics did give me the basics of linear algebra – although it isn't much. For all that I found The Road to Reality, with four readings of all of it and quite a few more for several bits, completely fascinating, wonderful, illuminating, and all the rest. The sort of thing Penrose does is to offer me the clearest explanation I know anywhere for basics (like Goedels theorem – can't remember whether that is in The Road or one of his others – doesn't matter), and then those marvelous diagramatic explations of more complex material. This neophyte is happy.

39. **Lars**
October 10, 2020

The Penrose Singularity

A singular prize
For a singular role
A singular surmise
'bout a singular hole

40. **Peter Shor**
October 10, 2020

Laurence Lurio: My physics class in college, back in 1977, used *The Feynman Lectures*. As far as I can tell, the real complaint about them is that while they give a good "big picture" perspective on the material and provide what I thought was excellent intuition, they don't actually teach you how to solve the homework problems.

So possibly this is another instance of the "shut up and calculate" mindset.

41. **cedric bardot**
October 11, 2020

Thanks Peter to praise Roger Penrose's inspiring original and deep vision of the unity of geometry and physics. It's worth emphasizing it indeed more explicitly than (but also be glad with) Sean Carroll in his recent tweet:
Roger Penrose is the first "theorist" to win for work in gravitational physics since — well, ever. Even Einstein won for quantized light, not general relativity.

(https://twitter.com/seanmcarroll/status/1313489948578902018?s=20)

Do you or does any commenter know any kind of theoretical physics progress or experimental evidence could bring the conditions to convince the Nobel committee that the discovery of a 125 GeV scalar boson at the LHC has something to do with the discovery that the Standard Model Lagrangian is a robust prediction of some advanced new spacetime geometric theory?
I would be interested to read an answer from @martibal for instance.
If some theoretical particle physicists at CERN would consider this naive question, the hope the Higgs boson has something to say about spacetime geometry could become less dark & the hope for a future award to the achievements of LHC phenomenologists & experimentalists could materialize, couldn't they?

Incidentally a framework for unification of all fundamental interactions including gravity I have in mind happens to have a connexion with Penrose twistor theory (cf conclusion at p 578 of https://onlinelibrary.wiley.com/doi/epdf/10.1002/prop.201000069, also available on arxiv https://arxiv.org/abs/1004.0464).

42. **Curious Mayhem**
October 11, 2020

It's not clear Hawking could have won the Prize. His theorem is about cosmology. Penrose is an unexpected but appropriate choice, given how much evidence there now is for black holes. The pictorial breakthrough of Penrose diagrams and the interplay of timelike and spacelike regions are physics gold, for the ages.

43. **Peter Woit**
October 11, 2020

Cedric Bardot,
About the Sean Carroll comment, I think he soon realized other theorists have gotten Nobel prizes for gravitational physics. The unique thing about Penrose is that this is an the award for mathematical physics.

The rest of your comment is off-topic, but thanks for pointing to the reference by Connes-Chamseddine to twistors. I've been trying to figure out if there is any relationship between the ideas about twistor unification I've been thinking about and the non-commutative geometry unification program, so this is helpful. But it has nothing much to do with Penrose, so off-topic here. I'll surely be posting again about twistors and unification…

44. **martibal**
October 11, 2020

Since I was mentioned in a comment, I give a short answer: I do not think the way the Higgs field appears in the noncommutative geometry description of the standard model can be viewed as a "proof of the Higgs" similar to the "proof of black hole" by Penrose's singularity theorem. It would be off topic to develop my point here, but I do not think the two situations are comparable.
\\
I take this occasion to ask once more if anybody knows how much mathematicians value the singularity theorems ? Penrose was in his thirties in 65, so still in the competition for a Fields medal. Not that prizes are so important, but in our math department in Genoa we have a course specially devoted to the singularity theorems (and we did not open it because of the Nobel prize !). So far it attracts more the physics students than the maths ones. In our university, mathematical physics seems to be more appealing to physicist than mathematician, although in Italy "mathematical physics" officialy exists as a sector of math (not of physics). I was wondering if this is a general situation, and if so, what can be done to "sell" the subject better to mathematicians ? Maybe the Nobel prize may have a pervert effect, convincing mathematicians that all this stuff has definitely more to do with physics than maths 🙂

45. **Peter Woit**
October 11, 2020

martibal,

At the time Penrose did this work, interest of mathematicians in questions related to physics was at a minimum, so not surprising Penrose was not likely seriously considered for a Fields medal.

The situation now is quite different. Here at Columbia there is a great deal of interest in the mathematics of classical black hole solutions in the math department, less in the physics department. This is the specialty of one member of our department, Mu-tao Wang, and there has been an active seminar on the topic, see
https://sites.google.com/prod/view/grgas/home

A frequent visitor has been Princeton's Sergiu Klainerman who also works on this, see
https://web.math.princeton.edu/~seri/homepage/seri.htm

Another prominent mathematician who works in this area is Shing-Tung Yau. To inspire mathematicians, one thing you could point to is this relatively recent conference
http://www.fields.utoronto.ca/activities/workshops/international-conference-black-holes
at the Fields institute.

46. **william e emba**
October 11, 2020

Regardless of mathematicians' attitudes to physics in the 1960s, I don't think Penrose's singularity theorem would have merited a Fields medal as such. Certainly his other work (the Penrose triangle!) wasn't top of the world, however fascinating it was.

There was some awareness of mathematical physics at the highest levels back then. Laurent Schwartz received his 1950 Fields medal for developing and applying the theory of distributions, which made rigorous the Dirac delta-function. Not that the physicists were concerned, or even noticed.

As for Yau, his solution with Schoen of the positive mass conjecture (a significant foundational question for general relativity) was cited as part of his reason for receiving the Fields medal 1982. Their method of proof involved generalizing Penrose's 1965 arguments from 1D to 2D.

Hawking's extension of Penrose's work to cosmology and the Big Bang was certainly pivotal in forcing cosmologists to accept it. As for black holes, Hawking's area theorem has now been experimentally verified numerous times by LIGO.

47. **Shantanu**
October 12, 2020

For people interested in this, see the comments by David Wiltshire and also Roy Kerr himself on Peter Coles blog
https://telescoper.wordpress.com/2020/10/06/the-2020-nobel-prize-for-physics/#comments

48. **John Baez**
October 13, 2020

I'm really happy that Penrose won this prize. I wrote an explanation of the theorem he proved, that won him the prize.

49. **DrDave**
October 15, 2020

I'm curious what people here think about the effect, if any, of how the prize will affect teaching and research at the institutional level.
Specifically, will students consider Penrosian ideas (and others) more favorably as they plan their research, and will faculty appointments be weighted towards research related to the prize. Perhaps even a curriculum shift of emphasis. Or is the status quo going to continue.

50. **Peter Woit**
October 15, 2020

DrDave,
I think that kind of effect of the Nobel exists, but is fairly marginal. The prize gives Penrose's work more attention, but it was already well-known to people in the field. A good example of the effect might be that the prize announcement encouraged John Baez to write the really nice explanation that he links to above, so that gets more people aware of this.

My own agenda these days is to promote twistors, so it would have been great for me if Penrose got the award for twistors, but that's not the case. On the other hand, most people seem to believe that Witten got the Fields medal for work on string theory, so maybe some day they'll all believe that Penrose got the Nobel for twistors…

51. **Anonymous**
October 18, 2020

martibal,
The 1966 Fields Medals were for exceptionally groundbreaking mathematical work even by Fields Medal standards so Penrose not making the cut isn't necessarily the best metric: Grothendieck for reinventing algebraic geometry, Atiyah for the index theorem, Cohen for independence of the continuum hypothesis, and Smale for the Poincaré conjecture for d>4.

Do Particle Physicists Continue to Make Empty Promises?

October 23, 2020
Categories: Uncategorized

Blogging has been light here, since little worthy of note in math/physics has been happening, and I've been busy with teaching, freaking out about the election, and trying to better understand Euclidean spinors. I'll write soon about the Euclidean spinors, but couldn't resist today making some comments about two things I've seen this week.

Sabine Hossenfelder yesterday had a blog entry/Youtube video entitled [Particle Physicists Continue to Make Empty Promises](), which properly takes exception to this quote:

> A good example of a guaranteed result is dark matter. A proton collider operating at energies around 100 TeV will conclusively probe the existence of weakly interacting dark-matter particles of thermal origin. This will lead either to a sensational discovery or to an experimental exclusion that will profoundly influence both particle physics and astrophysics.

from a recent article by Fabiola Gianotti and Gian Francesco Giudice in *Nature Physics*. She correctly notes that

> They guarantee to rule out some very specific hypotheses for dark matter that we have no reason to think are correct in the first place.

A 100 TeV collider can rule out certain kinds of higher-mass WIMPs, but it's simply untrue that such an exclusion will "profoundly influence both particle physics and astrophysics." Very few people think such a thing is likely since there's no evidence for it and no well-motivated theory that predicts it.

Where I part company with Hossenfelder though is that I don't see much wrong with the rest of the Gianotti/Giudice piece and don't agree with her point of view that the big problem here is empty promises like this and plans for a new collider. Twenty years ago when I began writing N*ot Even Wrong*, I started out by writing a chapter about the inherent physical limits that colliders were starting to hit, and the significance of this for the field. It was already clear that getting to higher proton energies than the LHC, or higher lepton energies than LEP was going to be very difficult and expensive. HEP experimentalists are now facing painful and hard choices about the future, which I wrote about in detail here under the title Should the Europeans Give Up?. The worldwide experimental HEP community is addressing the problem in a serious way, with the European Strategy Update one aspect, and the US now engaged in a similar Snowmass 2021 effort.

Many find it tempting to believe that the answer is simple: just redirect funds from collider physics to non-collider experiments. The problem is that there's little evidence of promising but unfunded ideas for non-collider experiments. For the last decade there has been no new construction of high energy colliders, with as much money as ever available worldwide for HEP experiments. This should have been a golden age for those with non-collider ideas to propose. This continues to be the case: if you look at the European Strategy Update and Snowmass 2021 efforts, they have seriously focused on finding non-collider ideas to pursue. This should continue to be true, since I see no evidence anyone is going to decide to go ahead with a next generation collider and start spending money building it during the next few years. The bottom line result from the European process was not a decision to build a new collider, but a decision to keep studying the problem, then evaluate what to do in 2026. For the ongoing American process, as far as I know a new US collider is not even a possibility being discussed.

While HEP experiment is facing difficult times because of fundamental physical, engineering and economic limits, the problems of HEP theory are mostly self-inflicted. The decision nearly 40 years ago by a large fraction of the field to orient their research programs around bad ideas that don't work (SUSY extensions of the Standard Model and string theory unification), then spend decades refusing to acknowledge failure is at the core of the sad state of the subject these days.

About the canniest and most influential HEP theorist around is Nima Arkani-Hamed, and a few days ago I watched an interview of him by Janna Levin. On the question of the justification for a new collider, he's careful to state that the justification is mainly the study of the Higgs. He's well aware that the failure of the "naturalness" arguments for weak-scale SUSY needs to be acknowledged and does so. He also is well aware that any attempt to argue this failure away by saying "we just need a higher energy collider" won't pass the laugh test (and would bring Hossenfelder and others down on him like a ton of bricks…).

The most disturbing aspect of the interview is that Levin devotes a lot of time (and computer graphics) to getting Arkani-Hamed to explain his 1998 ideas about "large extra dimensions", repeatedly telling the audience that he has been given a \$3 million prize for them. This paper has by now been cited over 6300 times, and the multi-million dollar business is correct, with the prize citation explaining:

> Nima Arkani-Hamed has proposed a number of possible approaches to this paradox, from the theory of large extra dimensions, where the strength of gravity is diluted by leaking into higher dimensions, to "split supersymmetry," motivated by the possibility of an enormous diversity of long-distance solutions to string theory.

At the time it was pretty strange that a \$3 million dollar prize was being given for ideas that weren't working out. It's truly bizarre though that Levin would now want to make such failed ideas the centerpiece of a presentation to the public, misleading people about their status. The website for the interview also promotes Arkani-Hamed purely in terms of his failures, presented as successes:

> Nima Arkani-Hamed is one of the leading particle physics phenomenologists of the generation. He is concerned with the relation between theory and experiment. His research has shown how the extreme weakness of gravity, relative to other forces of nature, might be explained by the existence of extra dimensions of space, and how the structure of comparatively low-energy physics is constrained within the context of string theory. He has taken a lead in proposing new physical theories that can be tested at the Large Hadron Collider at CERN in Switzerland,

This is part of the overall weird situation of the failed ideas (SUSY/strings) of 40 years ago: they still live on in a dominant position when the subject is presented to the public.

At the same time, the topics Arkani-Hamed is working on now are ones I think are more promising than most of the rest of what is going on in HEP theory. The interview began with a discussion of Penrose's recent Nobel Prize, with Arkani-Hamed explaining Penrose's fantastic insights about twistor geometry and noting that his own current work involves a fundamental role for twistor space (personally I see some other promising directions for using twistor geometry, more to come about this here in the future).

In contrast to Hossenfelder, what I'm seeing these days in HEP physics is not a lot of empty promises (which were a dominant aspect of HEP theory for several decades). Instead, on the experimental side, there's an honest struggle with implacable difficulties. On the theory side increasingly people have just given up, deciding that it's better to let the subject die chained to a host of \$3 million prizes for dead ideas than to honestly face up to what has happened.

Update:
In case anyone needs any reminder of how bad the propaganda problem is:
https://kids.kiddle.co/String_theory
It appears this kind of thing is driven by a propaganda problem on Wikipedia.

Comments

1. **Sabine Hossenfelder**

October 23, 2020

Hi Peter,

Thanks for the reasoned response.

A few remarks. Firstly, the rest of the Giudice & Gianotti piece repeats unconvincing arguments I have debunked many times before, see eg here for a summary. Take eg the "spin offs" and "other benefits" claim. True. But the same can be said about any similarly large investment into science funding. Doesn't have to be a collider. So then we should put the money in a science experiment that also makes scientific sense.

Second, and more importantly, you state "This should have been a golden age for those with non-collider ideas to propose." Excuse me for being frank, but have you ever even considered to write a proposal for a large scale experiment? Probably not. Because if you had you would realize that you need funding to even put together a proposal. You need people. You need expertise. But all the people and the expertise are in particle physics. And of course particle physicists propose more particle physics experiments.

This whole process is so obviously incestual; it is beyond me why it's still allowed to happen. It's a chicken and egg problem: people go where money goes and money goes where people go. To make matters worse, then there are outside people, like you, who don't understand how it works and say "But look, this is where money and people go, so this must be where we should put in more money and people." *facepalm*

Third, I don't want to start this all over again because I have literally written a whole book about it, but I think we need to make more efforts to evaluate the promise of investing huge amounts of money based on looking at what strategies worked in the past. As I have explained, theory-based breakthroughs have only been successful when they were studying inconsistencies. There isn't any such inconsistency in the range in the new 100 TeV collider.

Fourth, that the last decade should have made it easy to get funding for other things is just wrong, look eg at the funding troubles of the SKA. The SKA was a good idea, once, but after it was slimmed down it failed making much sense which led more nations to pull out. We are talking here about a billion that was missing, not 20 billions. And you know what they're building next to Frankfurt what's been eating up 2 billions? A particle collider. Srsly. Not a 100 TeV machine, but a particle collider. And as you have probably heard RHIC is getting an upgrade that will eat up a few billions too. So please stop claiming there ain't no money going into this field, it's just wrong.

As to Arkani-Hamed. Fun fact. I was recently interviewed by someone who writes for a little known German science magazine (I believe it's a German version of the MIT Tech review). He told me he previously spoke with Nima. And you know what Nima told him? The good old finetuning story with the balanced pen and all that. (It's in print now, so not like I'm spilling secrets.)

It's really hard for particle physicists to give up their numerological arguments (naturalness, WIMP miracle, etc) because those are the only arguments they have. If they'd stick with the truth, nobody would give them funding, and they know this. That's why they continue to make false claims about dark matter and dark energy and new particles and fuzzy things about the baryon asymmetry, all of which fall apart to dust if you look a little closer.

2. **Johannes**
October 24, 2020

Peter and Sabine,

now I am an old theoretical physicist, reading your blogs since almost from when they started, but I still do not understand both of you. Allow me to explain the reason.

It is indeed correct that both theoretical particle physics and quantum gravity have not come up with the correct approach to go forward. And this is so since decades. You both have shown this in great detail, and for this you indeed deserve much praise. You both stress the importance of experiment, the fallacies of wrong arguments, the traps of untestable premises, etc.

After so many years, it is obvious that in the vast field of possible approaches, the correct one is like a tiny plant still waiting to be discovered among the large number of wrong ones. What today's theoretical physics needs, is a guidance from older experts to younger ones on where to go on searching.

We have a crisis in theoretical physics, not a crisis in experimental physics. But both of you also explicitly state "don't send me your TOEs". And you also act consequently. Indeed, there are so many bad ideas around. But it implies that you do not give *any* approach a chance. Not even the correct one. Ok, I can understand this attitude as well – time is valuable.

But if we cannot give answers to younger people, we should at least give them questions. However, you both have become so negative that it is not even allowed to write questions in your blogs. You do not believe it? Let me add some questions to this comment. It will lead to rejection.

(1) To Peter and Sabine: It is clear since decades that gravitation is of thermodynamic origin and that black holes have microscopic degrees of freedom. One goal of quantum gravity is to discover these microscopic degrees of freedom: What are they? How do they make up black holes and space?

(2) To Peter: How are these microscopic degrees related to twistors?

(3) To Sabine: A correct approach to particle physics must explain the fine structure constant or the mass of the electron. Yes, strings failed, and so did loop quantum gravity. Can other microscopic degrees of freedom achieve this?

We need to pass on questions such as these to younger people. We need to distinguish carefully between telling people not to take a wrong path and telling people not to take any path. As long not even asking questions is possible, we stifle progress.

3. **Alessandro Strumia**
October 24, 2020

Fact check: "a proton collider operating at energies around 100 TeV will conclusively probe the existence of weakly interacting dark-matter particles of thermal origin" is not true. Not even close. CERN is struggling for its survival, and it's not struggling in the best way.
The good way should have been: recognise that LHC will be scientifically over soon and that it's time to either invent how to build a smarter muon collider in the LHC tunnel, or to return the place to cows.
The bad way is: keep LHC running until its generation retires (current funds: ≈1 billion/year) and try exaggerations and public relations to get ≈30 billions for a bigger collider.

4. **Sabine Hossenfelder**
October 24, 2020

Hi again,

It occurred to me I should add something for context, which many of us "out here" tend to forget. I suspect the vast majority of particle physicists have no idea what we are even talking about. They live in a bubble in which there's only particle physicists. They haven't heard that naturalness arguments and the WIMP miracle died 5 years ago — and that's in the optimistic case that they know how much they relied on these arguments to begin with.

The reason I suspect this is that each time I am asked to "debate" a particle physicist they tell me 30 years old stories. They say that dark matter should be made of WIMPs and that there is supposedly something special about the TeV scale, and when I tell them the predictions made with these arguments have been falsified they are just stunned. It's like the past 5 years didn't happen.

(A small fraction of them will then argue one can't falsify numerological arguments, but, well, either way — falsified or unfalsifiable — it's arguments on the basis of which you should not invest tens of billions of dollars.)

Alessandro (above), btw, is the rare exception of a particle physicist who understands the situation (if you look at his publications, probably understood this long before I did).

5. **Doug McDonald**
   October 24, 2020

   Its a long shot, but I consider the most likely "effort" to succeed in getting theoreticians "on track" to success is "An experimental effort in astronomical observations measuring cosmological parameters with emphasis on no systematic errors" aka "pushing the cosmological constant to the other sign" plus "which side is wrong on Hubble?".

   Perhaps accurate full 4 pi measurement of the cosmic microwave background at numerous widely spaced different wavelengths, or even at continuous wavelengths, not just spot ones. That would spend a bundle. It would also pay for developing better continuously powered helium refrigerators for space use.

6. **Peter Woit**
   October 24, 2020

   Doug McDonald,
   What I'm discussing here is the situation with high energy particle (HEP) physics. Cosmology and astrophysics are a completely different story. Trying to do better CMB measurements is a very active and well-funded subject. I see zero reason to believe that any CMB measurement will ever tell us anything about the questions a next generation collider would be designed to address: the details of the behavior of the Higgs and whether the SM continues to work perfectly at scales of 1-10 TeV.

7. **Peter Woit**
   October 24, 2020

   Johannes,
   What I'm discussing is HEP physics, not quantum gravity, especially the increasingly common attitude that HEP physics is dead, with quantum gravity the only thing worth thinking about. The problems of quantum gravity are a completely different story.

   It's currently very much not true that I give no alternative approach a chance, since I now see a relatively clear path towards progress along the lines of the ideas about twistor geometry I've advertised.

8. **Peter Woit**
   October 24, 2020

   Alessandro,

   Yes, part of the story is that CERN's future is at stake, and those with jobs there have a vested interest in large projects that would keep it in business. This is worth keeping in mind when evaluating people's arguments.

   A muon collider in the LHC tunnel would be an ideal project in terms of keeping CERN going. I don't see any reason to believe that the reason for not pushing that instead of the 100 TeV idea isn't simply lack of feasibility (among other problems, such a thing might produce unhealthy levels of neutrino radiation over a large chunk of France and Switzerland).

   I wouldn't be at all surprised though if in 2026 CERN gives up on the 100 TeV pp idea as unfundably expensive and instead turns to the muon or some other lepton collider idea.

9. **Peter Woit**
   October 24, 2020

   Sabine,
   If you listen to Arkani-Hamed, he is acknowledging that the "new weak scale physics will restore naturalness" argument has been pretty much discomfirmed by the LHC. Unfortunately I think what's going on is that he hasn't abandoned the naturalness argument, he's just drawn a conclusion from it that he doesn't want to talk about.

   For many years he would give talks about how the LHC would lead to one of two possible outcomes: new physics restoring naturalness, or the anthropic multiverse. I suspect what's going on with him and many others is that they still believe these are the two possibilities, have drawn the conclusion that the LHC has vindicated the anthropic multiverse, and as a result have given up on HEP physics. They've seen the reaction of their colleagues and the public to the campaign for anthropic multiverse pseudo-science, so they're not repeating this publicly. You see this fairly clearly in some of Witten's statements of the order of "I don't like the multiverse but I now don't have an argument against it".

   About the funding issues, I have no idea whether giving up on HEP physics will free up money for other currently under-funded fields which I know little about. My point was just about HEP experiments. The argument for an energy frontier collider is that it's the only way to get better information about details of Higgs and 1-10 TeV scale physics. If anyone had a better, cheaper way to do this, it would get a lot of interest. The US already a long time ago did what the Europeans are faced with doing (giving up on energy frontier machines), and the experience here provides a pretty clear idea of what will happen if the Europeans do the same (very good for neutrino physics…).

   Any obstructions to people with good but much cheaper proposals getting funded are worth addressing, I just don't believe the main problem is collider spending crowding everything else out.

10. **OC**
    October 24, 2020

    Contra Sabine's claim that particle experimentalists only do more particle experiments, and speaking as an old timer who got his (theory) degree back in the '80s, I know 3 HE experimentalists from that era, two now quite senior. All three have since switched to other experimental areas, one working on the LSST and now on ground based CMB, another one now doing CMB stuff, and the third doing stuff with liquid xenon, first to look for neutrinoless double beta decay, now to look for other interesting things with the "backgrounds" from that setup. Oh, and a fourth friend – an ex-phenomenologist – now works on improving the cosmological constant measurement with large-scale galaxy surveys.

Physicists can change their spots. Not everyone gets permanently attached to unproductive lines of work.

Personally, I still think the ideas that have come out of string theory – particularly gauge/gravity duality and the related and more recent “it from qbit” ideas – are tremendously interesting, even if connecting them to the standard model looks very far off. That our society allocates a small fraction of the cost of one more pointless jet fighter to giving a few talented people the resources to pursue those ideas seems to me a good measure of our merit as a civilization, and it’s a shame that the meagerness of that commitment pits people who should be allies against each other.

11. **Anders**
October 24, 2020

The muon-g2 experiment at Fermilab, would that be considered HEP? It seems to be poking at a possible discrepancy of the standard model.

As for the 100 TeV collider in Switzerland, I just don’t see that happening. Building a 150 km tunnel would be ridiculously expensive, and its not like there is a new Higgs boson to look for.

12. **Peter Woit**
October 24, 2020

Anders,
The muon g-2 experiment is a good example of the sort of the thing US HEP is doing in the absence of an energy frontier collider. Unfortunately, the only information such low energy experiments can give you about what is happening at higher energy scales is quite indirect and just one number. If the g-2 result comes out in conflict with the Standard Model, that will be interesting evidence that there’s something beyond the SM, while at the same time telling you almost nothing about what it is.

13. **Amitabh Lath**
October 24, 2020

Please! Enough with this big physics vs. little physics death matches. Govt. money simply does not work that way. Case in point, the US govt. sent me (a tenured professor!) over $3k a few months ago just for…well I don’t know what for. Money rules are not the same when you own the printing press for dollars.

The only valid reason for NOT continuing down our path towards short distance physics would be if someone could guarantee there is NO new physics at these higher energies (or energies that would be reached by the next next collider). If you could prove there was no new physics until the Plank scale then fine, colliders are pointless.

14. **Shantanu**
October 25, 2020

If I was a 2nd year Ph.D student having taken particle physics courses and maybe attended a few colloquia/seminars etc, my first reaction would have been after reading that article is
“Wait a minute. We already know from neutrino experiments that there is evidence for Physics beyond standard model. So why are Giudice/Gianotti talking as if there is no evidence. what have we learned about TeV scale physics from 20+ years of neutrino mass results.? Else if neutrino experiments can tell us nothing about BSM of particle physics, besides measurements of delta m^2, sin^2 theta and delta CP, then why waste money on neutrino experiments? Conversely why aren’t top theorists (like Nima) working on these issues?”
Also I don’t see reporters asking this question to HEP theorists or Sabine/Peter stressing these points.

15. **Johannes**
October 25, 2020

Peter, Amitabh,

searching for new physics is the wrong path. We have new physics in front of us since 100 years! Here it is: What is the origin of the coupling constants? And what is the origin of the particle masses?

The trouble is that 99% of the proposed answers are by crackpots. But the truth it that this is the problem to solve. Do we need colliders to solve it? Maybe. Maybe not. But who is working on this problem? Nobody. That is the real scandal of (theoretical) particle physics.

The problem is clear, and everybody is avoiding working on the solution. Avoiding to work on a solution has indeed allowed many people to get a lot of money from funding agencies. Avoiding to work on a solution has also created a wide spectrum of fantasy theories.

Physicists and the funding agencies have a century old problem, but the search and use money in all sorts of directions – except in the correct one. Look at Snowmass 21. Look at what their theory frontier group does on this issue: NOTHING. Look at what CERN does in this direction: NOTHING. Look at what the rest of the world does: NOTHING.

String theorists gave up. Loop quantum gravity gave up. I know too little about twistors, but chances seem slim that they will solve the problem.

Fact is, asking for calculations of the parameters of the standard model is the best way to discard wrong theories.

The real scandal of particle physics is (1) that nobody is looking at understanding the parameters of the standard model. (2) That nobody encourages such research. (3) That nobody funds such research. And (4) that nobody even mentions that this is the real issue.

And to be a little direct: point (4) is true for most of the people who comment on this blog as well. As long as we keep sweeping the problem under the carpet, nothing will happen. Point (4) is the silent agreement of all particle physicists. As long as it holds, there will be no progress. As long as this silent agreement is kept, particle physics will not prosper.

We should tell young people that this is what we need to find out.

16. **Johannes**
October 25, 2020

Peter,

thank you for posting my comments. One more remark in the same direction. Some months ago, a blogger whom usually I admire a lot wrote that any TOE is tested by looking for deviations from the standard model. Even though many have agreed, many will agree or tend to agree, this is wrong. And it is misleading.

A TOE is first of all tested by checking its calculation of the coupling constants and the particle masses. (We do not know whether deviations from the standard model exist.)

This anecdote shows how deeply we all – the whole HEP community – have buried the goal to understand the parameters of the standard model below other, much less important activities and aims. Without noticing, we are misleading young people. (And I include myself here.)

We must tell the younger people what the goal is: understanding masses and couplings. The goal is NOT understanding supersymmetry, nor strings, nor loops, nor quantum gravity, nor the amplituhedron. The goal is NOT understanding baryon-antibaryon asymmetry, nor dark matter, nor dark energy. And: The goal is NOT finding deviations from the standard model.

The real goal we have before us, since over 100 years, is to understand the ratio 511 keV/Planck mass for the electron, the corresponding ratios for the other particles, the number 137.036, and the other two coupling constants, and a few more parameters (and, of course, the running with four-momentum of all of them).

Why are CERN, NSF, research agencies, sponsors, and research ministers not taking up this goal? It is the biggest problem in physics. It is orders of magnitude bigger than Fermat's last theorem. There are of course many obvious reasons (money does not help solving it in an obvious manner, accelerators probably will not help solving it, it is hard to plan, we have no clue yet how to get to a solution).

But let me be a bit provoking: If you, Peter, or I, or any of several dozen of other people had a billion dollars every year to spend on this goal, then you, or I, or any of this dozen of other people would be able to organize a world-wide project that would achieve that goal. And probably they would need less that a billion dollars a year.

In this sense – but only in this one – I agree and understand Sabine's point. And working towards this real goal would not be an empty promise. It would be a worthwhile effort.

17. **Peter Shor**
October 25, 2020

@Amitabh Lath:

> The only valid reason for NOT continuing down our path towards short distance physics would be if someone could guarantee there is NO new physics at these higher energies

Do you really believe this? Suppose you knew that the probability of discovering a new particle at the next-stage collider was one in fifty million (the chance of winning the lottery). Would it be worth it? Do you buy lottery tickets?

Assuming that the cost of the next machine is $20 billion, your expected value is positive at this probability if you put the benefit of a new particle at one quadrillion dollars. But surely, nobody would think of claiming that it would be worth it to pay a quadrillion dollars to discover a new particle. That's the GNP of the U.S. over 50 years.

So now that we've established that it's just a matter of probabilities, at what probability of seeing something would it be worth building the next collider?

Clearly, we should wait until the LHC run is finished and see whether there's any hint of new physics. But if the LHC doesn't give any hint of new physics, do you really think the probability of seeing something with the next-generation collider would be more than 10%?

18. **Peter Woit**
October 25, 2020

Johannes,
The problem is that no one has a good idea about this, and a billion dollars is not likely to change that. This is actually one of the strongest arguments for a new collider: the source of the mass problem is the Higgs sector, so the highest priority should go to a machine able to study the Higgs, and this has to be done at the energy frontier. Yes, it's possible that all an experiment will see are the SM-predicted numbers, but this is not guaranteed (and the Amit/Peter Shor comments show that this is the crux of the funding question).

The situation is somewhat analogous to that of the Riemann hypothesis. All indications are that we're missing some basic idea there, that the problem is not going to be solved by more strenuous efforts to apply ideas we already know about. A billion dollars in grant money handed out to people engaged in such strenuous efforts likely would change nothing.

A big difference of the Riemann hypothesis case is that RH hasn't been the victim of a campaign to claim that the best research has proved that it is inherently insoluble. This is what has happened to HEP physics: there has been a huge and all too successful "we couldn't solve it, so no one can" campaign to convince people that string theory research has shown that the problem of particle masses is inherently insoluble, that these numbers are just random environmentally determined artifacts of some very complicated and unknowable physics going on at the Planck scale.

One aspect of the particle mass problem is that there are obvious more basic questions we have no good ideas about, in particular that of why there are three generations. Before we can explain an entry in a matrix, likely we first need to explain the dimension of the matrix.

If you look at how the great unsolved problems of mathematics get solved, it's often not by a large well-funded group effort. The solutions of the Poincare and Fermat problems by Perelman and Wiles are instructive examples. In neither case would money have helped, probably quite the opposite, since accepting money would have likely required producing reports about their progress, something neither of them thought was a good idea. The way such problems get solved in when the best people in the field think hard about them, engage in research programs designed to turn up new fundamental ideas, and don't get convinced the problem is insoluble and stop thinking about it.

19. **Alessandro Strumia**
October 25, 2020

Peter,

to avoid excessive neutrino radiation (while keeping desired luminosity) one needs to make narrower muon beams. Achieving this seemed impossible, but now there are ideas, see e.g. https://arxiv.org/abs/1509.04454.

45 years ago Rubbia and Van den Meer could persuade the field to pursue a risky new idea. 20 years ago I saw the resistance to the LEP to LHC transition: a new collider meant a change of generation. This is not happening now that the field is dominated by mega-collaborations.

20. **George Ellis**
October 25, 2020

OC: “more recent “it from qbit” ideas” certainly did not come from string theory. They came from John Wheeler long before string theory was fashionable.

21. **Amitabh Lath**
October 25, 2020

Peter Shor: Yes it is worth it to me because the Standard Model pisses me off. It’s hodgepodge and annoyingly half-baked.

I am not going to play the “how much money is too much money” game because as we have discussed before, money spent on big science is research and infrastructure spending, it pays for itself many times over.

22. **martibal**
October 25, 2020

Peter: since I come from a region very close to CERN, your argument about unsafe neutrino radiation makes me worry. Is this a joke ? Or do we have any knowledge about how bad can heavy neutrino radiation be ?

23. **OC**
October 25, 2020

@George Ellis: (*the* George Ellis?!?) I’m well aware of the history of “it from bit”. I saw JW present the idea in person, with his self-gazing, eyeball-bearing U at the Einstein Centennial celebration at the IAS in 1979. But JW’s ideas were pretty inchoate, or at least I didn’t understand them (not surprising – I was an undergrad at the time). I don’t think anyone else understood them as a real direction for research then either, though in hindsight perhaps they’re related to the then-new Bekenstein-Hawking ideas about black holes and entropy, though I’m pretty sure Wheeler was concerned more with issues related to the measurement problem.

Anyway, the string theory connection, as best I understand it, is the effort to translate gauge/gravity duality into ideas about how entangled quantum bits give rise to geometry. The detailed formulations of these ideas that have come out in the last decade were – at best – very implicit in JW’s slogan.

24. **Peter Woit**
October 25, 2020

martibal,
Not a joke, but I wouldn’t worry right now.

The main reason a muon collider is difficult is that muons have a lifetime of only two microseconds. So, very hard to store and collide. Everything in the beam is decaying quickly, with decay products including two high energy neutrinos, which you can’t shield. Normally one thinks of neutrinos as famously harmless: huge numbers come from the sun, and then go through the earth without interacting (mean free path a light-year of lead). But the cross-section goes up with energy and is much higher for TeV scale neutrinos. Also, you need a lot of muons in the beam. So, the neutrino radiation problem is real for a muon collider. Alessandro may be right that there are ideas for keeping this to a low enough level.

In any case, if you’re worried you should figure out the coordinates of the circle where the plane of the LHC tunnel intersects the surface of the earth, and just make sure not to live there.

25. **martibal**
October 25, 2020

Damned ! I made (rightly) fun of people fearing black hole production at LHC, but I’ll have to be more careful about neutrinos then ! I had in mind the usual story you recalled (“lots of neutrino from the Sun pass across the Earth all the time etc). The surface of the Earth is far from flat around Ginevra, that may not be so easy to figure out the intersection of the circle with the surface.
Private joke for the italian readers: let’s ask Gelmini, she is an expert in neutrino 🙂

26. **Alessandro Strumia**
October 26, 2020

Dear Martibal, don’t worry about neutrinos. Neutrinos emitted by pp colliders are a problem only for Dark Matter searches. Neutrinos are one reason why a 100 TeV pp collider cannot “conclusively probe the existence of weakly interacting dark-matter particles of thermal origin”. If you want to worry about something, I would suggest perfluorocarbons

27. **Marco**
October 27, 2020

The FCC statement from the Nature article is in contradiction with the FCC CDR.

> No experiment, at colliders or otherwise, can probe the full range of dark matter (DM) masses allowed by astrophysical observation.
https://link.springer.com/article/10.1140/epjc/s10052-019-6904-3#Sec8

Both authors are on both publications.

28. **Martin S.**
October 27, 2020

+1 for muon collider
It would be something **new**.

When you look for something completely new, you’d rather use something completely new. HE-LHC can serve the current generation, with the new generation doing something new.

While no one will put huge money to the same (just bigger, but still the same) collider, muons can be sold as a new way that nobody has ever walked. And if it will provide a lot of neutrinos, you can make it a double experiment.

29. **The Spirit of Harry Lipkin**
October 27, 2020

The procrustean effect… Hossenfelder kinda believes theoretical displeasure with the WIMP miracle, but also lambasts other theoretical claims that she thinks provided a false basis for other experiments.

As a matter of fact I have written a lot of proposals and have organized big teams of researchers. Hossenfelder reminds me of Pickering and “Fabricating Quarks”.

Look, `twas ever thus. Experiment goes kinda where it can go, we only have tools to illuminate under certain lampposts. Often we’ve found incredible surprises… Harry Lipkin used to be eloquent about that…. the poor record of theory in guiding discovery in particle physics in the 20th century. But nonetheless, surprises were many.

Seems like Peter Woit, you get it. The experimental community is earnestly struggling. Doesn’t seem like Hossenfelder gets it at all, and the hardbitten hardcore gnomes who roll around in the radioactive dust pulling cables and stuff don’t yet listen much to her. But she could still be right, whatever it is she is saying. That is the shocking nature of all profound discovery… it could appear from anywhere.

30. **Peter Woit**
October 27, 2020

Martin S.
The problem with the HE-LHC idea is that the needed magnets are very expensive (3 billion) and only give a factor of two in energy above the LHC. The current plan for the next few years emphasizes working on the magnet technology, maybe that will help the situation.

Yes, a muon storage ring would make a great neutrino factory. It is definitely a project for a new generation, only question is whether it’s a generation that has yet been born…

31. **Suomynona**
October 29, 2020

@Johannes,

I don’t think the problem is so much that the old guard is not giving open questions to the younger generation, but rather they are also providing the supposed answers. For any number of open questions in high energy physics, the answer is given to be “string theory”. So the young people are primed to accept that the answers must have a given form, the form of string theory.

32. **From philosophy with physics**
November 14, 2020

Dear Peter,
thank you for trying to balance out. What I really like about your blog, is that you do not have an obvious agenda (as Hossenfelder has, or Smolin had in his book that was published parallel to yours).

Building accelerators is much less empty than a TOE or QG. It should not be underestimated that CERN is a peaceful facility that has quite an outreach+attraction to and on young people. This spin-off is worth a lot and will not be acquired by proposing to focus on quantum foundations or inconsistencies.

I happened to be in Geneva last year as they opened up CERN for the general public. It was a completly fee-free event. There would be much to tell about seeing the detectors, the special facilities (like the Anti Matter Factory) and much more, but the most important thing I really liked about it was the positive message this community conveyed.

They might oversell a lot of things (but so do all us if we want the money, Hossenfelder not excluded!), but they at least provide also a vision that science is something worth doing, that there are things beyond your imagination and everyone can be part of it. A positve perspective.

All the best

Various Links, String Theory now Untethered

November 13, 2020
Categories: Uncategorized

I've been spending most of my time recently trying to get unconfused about Euclidean spinor fields, will likely write something here about that in the not too distant future. Some other things that may be of interest:

- I did an interview a couple days ago with Fraser Cain, who runs the Universe Today website. He had some excellent and well-informed questions about the state of HEP physics. I regret a little bit that I focused on giving an even-handed explanation of the arguments over a next generation collider, didn't emphasize that personally I think building such a thing is a good idea (if the money can somehow be found), since the alternative would be giving up and abandoning this kind of fundamental science.
- On Monday, the Simons Center celebrated its 10th birthday, talks are here, giving a good overview of the kinds of math and physics that have been going on there during its first decade.
- For the latest on the formulation of the local Langlands correspondence in terms of the geometry of the Fargues-Fontaine curve, Peter Scholze is teaching a course now in Bonn, website here.
- Kirill Krasnov has a book out from Cambridge, Formulations of General Relativity. If you share my current interest in chiral formulations of GR and twistors, there's a lot about these in the book. For a more general interest survey of what's in the book, see Krasnov's lectures last year at Perimeter (links and slides are on his website).
- A couple weeks ago, a very well-done explanation of what's been going on around the black hole information paradox written by George Musser appeared at Quanta Magazine. Periodically in recent years I've tried to follow what's up with this subject, generally giving up after a while, frustrated especially at not being able to figure out what underlying theory of quantum gravity was being studied. All that ever was clear was that this was about low-dimensional toy model calculations involving some assumptions that had ingredients coming from holography and AdS/CFT.

  Musser's article makes quite a few things clearer, with one striking aspect the news that:

  > researchers cut the tether to string theory altogether.

  which I gather means that any foundation in AdS/CFT is gone, with what is being discussed now purely semi-classical. I don't understand what these new semi-classical calculations are, and whether optimistic claims that the information paradox is on its way to a solution are justified (history hasn't been kind to previous such claims). In recent years the pro-string theory research argument has often been that while there no longer were any prospects that it would tell us about particle physics, it was the best route to solving the problem of quantum gravity. It will be interesting to see what the effect will be of that cord getting cut by leading researchers.

  If you think it's a good idea to follow discussions of this kind of thing on Twitter, you might enjoy threads from Sabine Hossenfelder and Ahmed Almeiri.

Comments

1. **Patrick Bryant**
   November 13, 2020

   My understanding of the new semiclassical calculations is that by computing the gravitational path integral over an ensemble of n boundary conditions, and allowing topologies which connect the boundaries, one can reproduce the Page curve when n is analytically continued to 1.
   These new topological terms ("euclidean wormholes") can be computed 'easily' in 2d JT gravity and the page curve is reproduced there. The problem is that no one knows what exactly to make of these topologies that don't factorize and it's not obvious that effects in 4D won't spoil the magic. On page 29 of https://arxiv.org/pdf/1911.11977.pdf the authors suggest this isn't an issue, "2d gravity is convenient for drawing pictures, but the topological argument relating replica wormholes to the island extremal surface is similar in any spacetime dimension: one just replaces each point in the discussion below by a sphere."

   It seems the interpretation of the euclidean wormhole topologies in the limit n->1 is somehow related to an ensemble average of chaotic quantum systems and it is not clear that the gravitational path integral can be interpreted as a semiclassical limit of any single quantum system. I really don't understand the connection between the SYK model and JT gravity which seems central to this idea.

   I found these talks by Douglas Stanford to be illuminating:
   1) Conceptually helpful https://www.youtube.com/watch?v=-hfcApA9s8Q
   2) Technically helpful https://www.youtube.com/watch?v=Yi2hx0GH624

2. **Peter Woit**
   November 13, 2020

   Patrick Bryant,
   Thanks. My problem here is that the claims being made all are about "doing the gravitational path integral", but the whole problem of quantum gravity is that in 4d that (summing over all geometries using the classical action) is something no one has made sense of, and nothing is being said that addresses that problem.

   In 4d you can decide to give up on the full sum, just look at classical solutions and do a semi-classical calculation, but this doesn't touch the underlying quantum gravity problem. It sounds like this is what is being done here in a simpler context. Maybe it resolves the question of how to make sense of the semi-classical calculation in a black hole background, but I don't see anything addressing the problem of how to actually quantize the gravitational degrees of freedom.

3. **S=k_B log(W)**
   November 14, 2020

   There are (at least) two other lines of recent developments that relate to AdS/CFT and black hole physics. Both of them are very much tethered to string theory and are concerned with black holes in more than 3 space-time dimensions.

   1. One is the program of microstate geometries/fuzzballs. See this recent review

   https://arxiv.org/abs/1902.07176

   and this Quanta magazine article from a few years ago

   https://www.quantamagazine.org/how-fuzzballs-solve-the-black-hole-firewall-paradox-20150623/

   2. There has also been a flurry of activity showing how to use holography to successfully relate exact QFT path integral calculation to learn about the dual AdS black holes. See this review for a nice (but already outdated) summary

https://arxiv.org/abs/1902.07176

Of course, given the premise of this blog, I suspect that you may label both of these research programs as "not even wrong".

4. **Peter Woit**
   November 14, 2020

   S=k_B log(W),

   Your point seems to be that some people have not given up on hopes that they can use AdS/CFT to produce a viable quantum gravity theory describing our world (dS, four large space-time dimensions). Back in 2006 when I wasted a lot of time arguing with string theorists like Lubos Motl about this, it was a nine year old idea that was going nowhere. Now it's a 23 year old idea which has gone nowhere. As for claims that this line of work can solve the information paradox, the five year old Quanta article you refer to has

   "If Samir says he has a solution to the paradox, he is linguistically correct. He's also in good company," said Marolf. "There are lots of people with resolutions to the paradox. Whether it's the way physics actually works in our universe remains to be seen."

   On the information paradox front, the newer Quanta article tells a different story: apparently the resolution of the paradox is semi-classical, nothing to do with string theory or hopes of getting quantum gravity (4d, dS) out of AdS/CFT.

5. **John Baez**
   November 15, 2020

   Peter wrote:

   > Maybe it resolves the question of how to make sense of the semi-classical calculation in a black hole background, but I don't see anything addressing the problem of how to actually quantize the gravitational degrees of freedom.

   I think you're right. That's not *necessarily* bad. If folks could figure out how to solve the black hole entropy problem "semiclassically" – without inventing a full-fledged theory of quantum gravity – they could find a solution that applies to many candidate theories. It could be a bit like thermodynamics versus a detailed theory of the microscopic structure of matter: thermodynamics tells you less, but its results are more general, so the further you can go on solving a problem using just thermodynamics, the more robust your solution will be.

   However, the analogy to thermodynamics is weak, because in thermodynamics we know the rules of the game, whereas it would take a lot of work to isolate and clarify the principles underlying the calculation described in the *Quanta* article. As they say:

   > The work is highly mathematical and has a Rube Goldberg quality to it, stringing together one calculational trick after another in a way that is hard to interpret. Wormholes, the holographic principle, emergent space-time, quantum entanglement, quantum computers: Nearly every concept in fundamental physics these days makes an appearance, making the subject both captivating and confounding.

   I think the title of the *Quanta* article is only accurate if one reads it very generously. We are not near the end of work on the black hole information paradox.

6. **John Baez**
   November 15, 2020

   It's great to hear Kirill Krasnov has come out with a book on formulations of general relativity. We used to work together on spin foam models of quantum gravity, and we kept playing around with different formulations of general relativity, trying to find one that worked best for our purposes. I'd sort of lost touch with him in recent years until he came out with a paper on octonions and the Standard Model, which focuses on the importance of SO(9). I've been talking to him about that recently… but he didn't tell me he'd come out with a book on GR! It's a good thing I read this blog.

7. **Blake Stacey**
   November 16, 2020

   I've slipped behind in trying to understand the ways in which my fellow theorists are torturing black holes these days. Coming at the problem from the quantum information side, I never quite followed the arguments or grasped the underlying intuitions, I think. When "ER = EPR" came out, for example, I just didn't get it — it sounded like they would have had a wormhole between any two correlated systems in the Spekkens toy model, which is just silly. A lot of what I read since then seemed more likely to give a geometric description of certain types of many-body entangled states, rather than to turn entanglement into spacetime. (I know multiple people who knew John Wheeler, and we've had plenty of long chats about "it from bit" aspirations, so I *am* coming at "it from qubit" from a sympathetic place.) And for weird reasons of my own, I distrust the applicability of any conclusions from AdS, whether or not a gauge/gravity duality is invoked. The new "replica wormholes" effort seems to rely a lot on AdS still, even when it's not being stringy, doing things like gluing AdS to Minkowski in order to provide a place for radiation to escape into. I'm not sure how I feel about that!

   > … by computing the gravitational path integral over an ensemble of n boundary conditions, and allowing topologies which connect the boundaries, one can reproduce the Page curve when n is analytically continued to 1.

   This is probably a very smart tactic being employed by some very smart people, but it also gives me the feeling that somebody is going to pop out from behind the corner and declare that the total number of black holes in the Universe is now -1/12.

8. **Fen Zuo**
   November 17, 2020

   I read through Kirill Krasnov's 6d/7d model. As he mentioned in his talk, it could be related to the topological strings. Nevertheless, I believe it must be related to the so-called 6d (2,0) SCFT and its 7d dual. Some twisted version of the 6d theory is also called Theory X by some mathematicians. Here the cosmological constant naturally presents itself through the Omega parameters of Nekrasov. However, the relation to the twistor formulation is unclear, at least for me.

9. **Peter Shor**
   November 17, 2020

   One thing that worries me is whether all of this means anything. There's the quote from the article that John Baez pointed out:

   > The work is highly mathematical and has a Rube Goldberg quality to it, stringing together one calculational trick after another in a way that is hard to interpret. Wormholes, the holographic principle, emergent space-time, quantum entanglement, quantum computers: Nearly every concept in fundamental physics these days makes an appearance …

So how do we know that you can't find a way to string all these calculational techniques together in a Rube Goldberg way to come up with any result you want? In this case, the fact that they've come up with the Page curve isn't all that meaningful.

10. **John Baez**
November 17, 2020

Stringing together lots of calculational techniques makes sense when the rules of the game are clearly laid out – for example, this is how people proved that [8 and 9 are the only two powers of positive natural numbers that differ by 1](). It's a lot more risky when one is doing physics calculations that aren't based on a well-defined underlying theory. Then one is in real danger if there isn't a solid intuition grounding one's work. It's interesting that nobody in the *Quanta* article claims to understand what's really going on here – exactly how the information gets out of the black hole. I suspect people will be arguing about this for many years.

Personally I'd be quite happy with information loss.

11. **anon**
November 19, 2020

Replica wormholes do not depend on AdS and, as is now understood, arise for a good reason. They can be derived solely from the goal of approximating certain information theoretic quantities of a subsystem of a larger system in a more sophisticated way than just replacing your reduced state on the subsystem with a thermal density matrix (but while still not actually using more fine-grained information than equilibriation).
This was recently shown here: https://arxiv.org/abs/2008.01089
In this controlled approximation, certain matrix elements arise, and these matrix elements will in a gravity system be calculated exactly by these wormholes. Analogous objects would arise in other many-body systems.

12. **Somdatta**
November 20, 2020

Shouldn't quantum gravity really stem from quantum geometry, since gravity is after all the consequence of the dynamical geometry of manifolds? In the regime where gravity is strong, shouldn't one consider that the evolution of one spacetime slice to another takes place via all possible interpolating manifolds, to be summed over in a path-integral formulation? I believe that is what one is doing in JT gravity, but extending that to 4d is not trivial at all. For one thing, the theory of 4-manifolds is in a state of flux, so shouldn't one hold all theories of 4d quantum gravity in abeyance until that has settled down? And all this is over and above the problem of doing a path-integral quantization, where one is faced with the usual problem of defining the path-integral unambiguously. Not to speak of the issues of non-renormalizability plaguing such a formulation.

13. **Peter Woit**
November 20, 2020

Somdatta,
The problem with just saying quantum gravity is a "sum over geometries" is that there's a huge variety of very different ways of specifying what a "geometry" is, and once you have done that, another huge variety of ways to try and put a measure on the space. You need a much more substantive idea to say anything useful about this, I have no idea whether this recent work has one or not.
Whenever I try and read about this to understand better what is going on, I get put off by a wall of claims about "wormholes", then decide that my time is better spent thinking about other things.

14. **Somdatta**
November 20, 2020

Peter,
You're right, but what I wanted to point out was that even if you figured out how to solve those problems, most of the work on 4-manifolds concentrates on the simply connected ones, with claims in the literature saying that the non-simply connected case is intractable. So one might be thwarted for a very long time to come if not forever, from even defining the space of interpolating manifolds in the sum over geometries, let alone a measure over it. Wonder what LQG has to say on all this.

15. **Peter Shor**
November 22, 2020

After having a discussion with Sabine Hossenfelder on her blog, I am beginning to see the problem. Physicists have always treated mathematics sloppily and non-rigorously (e.g. Dirac delta functions, the replica method, Feynman path integrals, AdS-CFT applied to condensed matter). And they have gotten some amazing results this way. But they've always had experiment to tell them when their calculations gave the right answer or were completely off-base.

With the black hole information paradox and quantum gravity, they no longer have experiment to guide them. I think what they have to do is go back, look at what they think they've shown, and treat everything much more carefully (if not completely rigorously). But I suspect that most physicists find it inconceivable that this has become necessary.

If I recall the history of mathematics correctly, there are several times that mathematicians have needed to do this (one being the treatment of set theory that Cantor et al. put back on the right track).

16. **Jack Morava**
November 22, 2020

@PeterShor:

It is said that the

https://en.wikipedia.org/wiki/Italian_school_of_algebraic_geometry

started having annual conferences around the turn of the twentieth century, in order meet and vote on the theorems.

17. **Peter Shor**
November 22, 2020

@Jack Morava:

Wow! That's an amazing story, even just reading the short version in Wikipedia. I knew there were problems with making probability theory rigorous in the early 20th century, but I foolishly assumed that applied mathematicians were to blame, and that pure mathematicians had figured out how to do things better before then.

18. **John Baez**
November 22, 2020

Peter Shor wrote:

> With the black hole information paradox and quantum gravity, they no longer have experiment to guide them. I think what they have to do is go back, look at what they think they've shown, and treat everything much more carefully (if not completely rigorously). But I suspect that most physicists find it inconceivable that this has become necessary.

I disagree. I think most physicists realize that more careful thought is required to understand quantum gravity. Theoretical physicists are used to floundering around, writing papers that try different things until the truth finally becomes clear. They aren't like mathematicians or mathematical physicists, who start with clear assumptions and deduce consequences. They're doing something much harder, where the rules of the game only become clear near the end. So most of their papers are wrong, but that's okay – that's how it works.

19. **Somdatta**
November 23, 2020

Another thing worth pointing out that appeared in the Quanta article.

"When researchers set out to analyze how black holes evaporate in AdS/CFT, they first had to overcome a slight problem: In AdS/CFT, black holes do not, in fact, evaporate. Radiation fills the confined volume like steam in a pressure cooker, and whatever the hole emits it eventually reabsorbs. "The system will reach a steady state," said Jorge Varelas da Rocha, a theoretical physicist at the University Institute of Lisbon.

To deal with that, Almheiri and his colleagues adopted a suggestion of Rocha's to put the equivalent of a steam valve on the boundary to bleed off the radiation and prevent it from falling back in. "It sucks the radiation out," said Netta Engelhardt of the Massachusetts Institute of Technology, one of Almheiri's co-authors. The researchers plopped a black hole at the center of the bulk space, began bleeding off radiation, and watched what happened."

The point is, this artificial way of bleeding off the radiation doesn't take place naturally, so all analyses based on such a thing are artificial, and may not apply to nature. One might say AdS black holes themselves are unnatural, but then that becomes the point. One is applying unnatural assumptions to unnatural black holes and then making the claim that they apply to the real world. Why would such a thing be believable?

20. **jsm**
November 23, 2020

@Jack Morava Can you cite a source for your statement?

21. **Peter Woit**
November 23, 2020

John Baez,
I can't find the source, but one of my favorite John Baez quotes I recall as comparing doing theoretical physics without either mathematical rigor or connection to experiment as "playing tennis without a net".

There's a good argument for people trying whatever they can, if it ultimately leads to rules of the game getting clarified and the truth becoming clear. The problem is that sometimes that doesn't happen, and people keep doing the same thing endlessly, telling everyone what great tennis players they are (cf. string theory).

I'm very wary of saying much about this latest work because I don't understand it. But looking at many decades of similar claims about "solving the information paradox", and seeing what people claim to now have done convinces me that my time is better spent on other things. There's no way this kind of thing can connect with experiment, what would more likely make it interesting is having a well-defined solution to a well-defined problem of some kind. If a problem is hard, maybe all people can do is flail around and do wrong things, hoping to learn something that will help. Nothing wrong with that, but then there's no reason for articles in Quanta magazine or elsewhere.

22. **Jack Morava**
November 23, 2020

@jsm : It's a joke; ask your friendly neighborhood algebraic geometry grad student. I think I first heard it at Columbia in the 60s, perhaps from Spencer Bloch or George Kempf.

23. **Robert A. Wilson**
November 24, 2020

My father was one of the last members of the Italian school of algebraic geometry before its collapse, being a student of J. G. Semple, and publishing on the subject in 1947, 1950 and 1955. The abstract of the last contains the phrase "no great degree of rigour can be claimed" and refers to "experimental" justification (his inverted commas). That about says it all, I fear.

24. **Blake Stacey**
November 24, 2020

John Baez:

> Personally I'd be quite happy with information loss.

I'd be happy with that, too. (I remember watching a lecture by Bill Unruh during the heyday of the firewall back-and-forth where he suggested that information might just be "gone — *pfft!*") I've also played around with a toy model where a black hole is described by a mixed state all the way from formation through evaporation, so the intuitions based on (as Page says) "forming the black hole from a pure state of radiation in a box" simply aren't applicable.

25. **John Baez**
November 25, 2020

Peter Shor wrote:

> I can't find the source, but one of my favorite John Baez quotes I recall as comparing doing theoretical physics without either mathematical rigor or connection to experiment as "playing tennis without a net".

No fair using John Baez quotes against me! I'm compelled to agree.

Hawking's original work on black hole radiation had no connection to experiment but it could be, and was, made mathematically rigorous – in a "semiclassical" approach where the geometry of spacetime was described classically and the radiation was described using quantum field theory (a free quantum field, describing noninteracting photons).

So, the rigor was obtained by avoiding use of the nonexistent theory of quantum gravity and hoping that in some circumstances the semiclassical approach was a good enough approximation to the unknown reality to be worth thinking about.

The thermal nature of the radiation emitted led to a puzzle, mistakenly called the "black hole information paradox" because it only becomes a paradox if you put enough constraints on the solution that there's… no solution.

One way to push the puzzle toward the jaws of paradox is to adopt the AdS/CFT philosophy, which roughly says that everything about the universe can be observed from arbitrarily far away, on a "sphere at infinity", where it's described by a quantum field theory. This philosophy is not based on experiment: on the contrary, our universe is nothing like anti-de Sitter spacetime. It's also not mathematically rigorous. And the idea that everything about he universe can be observed from arbitrarily far away tends to conflict with the idea that once something falls into a black hole you can't observe it. So it should come as no surprise that with this approach the information loss puzzle became closer to a paradox, and researchers began to contemplate something very strange: that anyone falling into a black hole would be burnt to a crisp by a "firewall" lurking right beneath the horizon.

> But looking at many decades of similar claims about "solving the information paradox", and seeing what people claim to now have done convinces me that my time is better spent on other things.

Me too. All my best work came after I gave up working on quantum gravity. The "firewall" stuff happened after that, so I've never studied it carefully: I just listen to what people say, shake my head bemusedly, and shrug. I still hope that theoretical physicists can make progress on the black hole information puzzle by clear thinking, and I guess you caught me on an optimistic day. But the jury is still out on that.

> If a problem is hard, maybe all people can do is flail around and do wrong things, hoping to learn something that will help. Nothing wrong with that, but then there's no reason for articles in *Quanta* magazine or elsewhere.

If pop-sci magazines could manage to present the current flailings in fundamental physics in a way that made it really clear they were really just flailings, it would be okay. But somehow they feel the need to present each development as if were a wonderful burst of progress.

26. **Peter Shor**
November 30, 2020

John Baez:

I've sometimes wondered whether the AdS-CFT correspondence might not be an exact correspondence, but only a good approximation. In this case, it leaves enough wiggle room that you can speculate that information could be lost.

The few times I've asked somebody who works in the area, they say "no, the AdS-CFT correspondence is exact, and physics is reversible because CFTs are reversible" without ever giving me a good justification for this statement. Of course, if they did have a good justification, I wouldn't be able to understand it. But I've suspected for some time that they don't. And when they assume that AdS-CFT exact, they often end up postulating something else, like locality in GR is violated, and locality is something that we do have a great deal of experimental evidence for.

I'd much rather assume that AdS-CFT is a very good approximate correspondence.

27. **John Baez**
December 3, 2020

Peter Shor wrote:

> The few times I've asked somebody who works in the area, they say "no, the AdS-CFT correspondence is exact, and physics is reversible because CFTs are reversible" without ever giving me a good justification for this statement. Of course, if they did have a good justification, I wouldn't be able to understand it. But I've suspected for some time that they don't.

Yeah, I don't think it's been proved, not even in the physics sense of "proved", except in some baby cases like 2+1-dimensional gravity which have no local degrees of freedom, which makes it far less surprising that you can reconstruct solutions from data observable at infinity. (If nothing interesting is happening in any bounded region of space, it's not shocking that you can figure out what's happening by only doing measurements very far away.)

It's also probably worth reminding nonexperts (like me) that the most famous case of AdS/CFT doesn't apply to a four-dimensional universe governed by general relativity, like our own. It applies to superstring theory on a 10-dimensional spacetime that's the product of 5-dimensional anti-de Sitter space and a large 5-sphere. So there is unbroken supersymmetry, not seen in our universe, and also a spacetime quite different from ours.

There are also other cases of AdS/CFT, but if anyone makes strong claims for the relevance of AdS/CFT to our universe you can have fun pressing them for details about which case they're talking about.

Contemplating the End of Physics

November 24, 2020
Categories: Uncategorized

In a remarkable article entitled [Contemplating the End of Physics] posted today at Quanta magazine, Robbert Dijkgraaf (the director of the IAS) more or less announces the arrival of the scenario that John Horgan [predicted for physics back in 1996]. Horgan argued that physics was reaching the end of its ability to progress by finding new fundamental laws. Research trying to find new fundamental constituents of the universe and new laws governing them was destined to reach an endpoint where no more progress was possible. This is pretty much how Dijkgraaf now sees the field going forward:

> Confronted with the endless number of physical systems we could fabricate out of the currently known fundamental pieces of the universe, I begin to imagine an upside-down view of physics. Instead of studying a natural phenomenon, and subsequently discovering a law of nature, one could first design a new law and then reverse engineer a system that actually displays the phenomena described by the law. For example, physics has moved far beyond the simple phases of matter of high school courses — solid, liquid, gas. Many potential "exotic" phases, made possible by the bizarre consequences of quantum mechanics, have been cataloged in theoretical explorations, and we can now start realizing these possibilities in the lab with specially designed materials.
>
> All of this is part of a much larger shift in the very scope of science, from studying what is to what could be. In the 20th century, scientists sought out the building blocks of reality: the molecules, atoms and elementary particles out of which all matter is made; the cells, proteins and genes that make life possible; the bits, algorithms and networks that form the foundation of information and intelligence, both human and artificial. This century, instead, we will begin to explore all there is to be made with these building blocks.

In brief, as far as physics goes, elementary particle physics is over, from now on it's pretty much just going to be condensed matter physics, where there at least is an infinity of potential effective field theory models to play with.

Dijkgraaf ends with an argument indicating that human intelligence is outmoded, artificial intelligence is our future:

> Science concerns all phenomena, including the ones created in our laboratories and in our heads. Once we are fully aware of this grander scope, a different image of the research enterprise emerges. Now, finally, the ship of science is leaving the safe inland waterways carved by nature, and is heading for the open ocean, exploring a brave new world with "artificial" materials, organisms, brains and perhaps even a better version of ourselves.

Along the same lines, today also brings an article in the New York Times by Dennis Overbye, [Can a Computer Devise a Theory of Everything?] The article discusses the new [MIT Institute for Artificial Intelligence and Fundamental Interactions] and Max Tegmark's hopes that AI will "discover all kinds of new laws of physics". My guess is that this will work just fine if you give up on the 20th century understanding of what a "law of physics" is and follow Dijkgraaf's lead. The problem then may be not so much "will we understand the new laws of physics found by AI?", but rather that of them not being interesting enough to be worth understanding…

Update: To clarify the point I was trying to make about the Dijkgraaf piece arguing against the "end of physics", compare it to the [similar 1996 piece] Gross and Witten published in the Wall Street Journal (a summary is [here], an extract [here]). Gross and Witten were strongly disagreeing with Horgan, whereas it seems to me that Dijkgraaf implicitly agrees with Horgan that fundamental physics has hit a wall and theorists are moving on to do something else.

Comments

1. **Geoffrey Dixon**
   November 24, 2020

   How ironic. Not two hours ago I decided on a Miltonish title for my next blog: Paradigms Lost.

2. **David Appell**
   November 24, 2020

   Huh, I didn't realize we understood the laws followed by dark energy or dark matter. My bad.

3. **anon**
   November 24, 2020

   > Huh, I didn't realize we understood the laws followed by dark energy or dark matter. My bad.

   To add a bit more detail, Dijkgraaf starts by asking "Is physics finished?," he answers that it is not, and he gives three reasons. His third reason is the one discussed in this blog post (condensed matter is a basically endless subject). His second reason though, is that we don't understand dark energy and dark matter and physics can't be finished without understanding them.

4. **chris bolger**
   November 24, 2020

   We are not reaching the end of physics, we are reaching the end of affordable physics. 100 years ago one could physics with one person, on a table top with extra time, and a few dollars. Now it costs a discernable amount of GDP from many countries and teams of 1000's. We have found deeper layers of reality much faster than the growth of the economy by pooling money, resources and people. That can only go so far and we are reaching that limit. Progress in physics will inevitably slow down to the growth rate of the economy unless something revolutionary occurs like a miracle in AI. It is not the end of physics, it is just much slower.

5. **John Baez**
   November 24, 2020

   Peter wrote:

   > …from now on it's pretty much just going to be condensed matter physics…

   David Appell wrote:

   > Huh, I didn't realize we understood the laws followed by dark energy or dark matter. My bad.

   These sarcastic comments seem to overlook what Dijkgraaf wrote:

> Recent advances in cosmology allow us to state, with a fair amount of certainty, that 95 percent of the universe is missing. These missing parts consist of dark matter and dark energy, both equally mysterious forms of new physics. As long as such mysteries remain — and there are others — the work of physics will not be complete.

So, I don't think he's saying fundamental physics is done. Indeed his comments seem pretty well-balanced to me. The title, "Contemplating the End of Physics", is overblown and misleading, as often for articles in pop-sci magazines.

6. **WTW**
November 25, 2020

Pardon me if I've just become too cynical, but I read Dijkgraaf's Quanta article as just another PR piece, similar to those we have come to expect from senior management at CERN. (One clue: the bizarre depiction of phonons as somehow relevant, or even an accurate description of "sound" much less elementary quantum physics.) It's another justification for why, despite diminishing returns and increasing irrelevance for much of fundamental physics' "scientific" output, places like IAS (and, in the future, CERN) should continue to exist and be funded.

But it gets worse: If you just ignore the gaping holes in our current formulation of the 5% of the universe we're supposed to now "understand", that have been unable to be closed over that last half century, and concentrate instead on just re-arranging various pieces into ever more bizarre and intricate patterns, and call that a "search for knowledge", then you essentially remove any chance of being objectively critiqued or judged as having been successful (or not). While I doubt that these attempts to put a "positive spin" on the situation [pun intended] are a pre-planned subterfuge by people like the directors of these agencies, that is the net result. While, as Peter has previously said, just combining a bunch of ingredients and essentially throwing the resulting mess against the wall to see what sticks and what doesn't could hopefully someday result in something useful, no one wants to admit that's what they/we are doing. Especially not to the general public, nor to funding agencies.

7. **Luigi Foschini**
November 25, 2020

I don't think physics is ending, but there is a serious possibility of a decline of human beings able to do it. We think about science as an endless progress, but history teaches us that there were dark ages where science was destroyed (e.g. the end of Hellenic science). Today, Big Science is the real threat for physics. I wrote some notes in a Section of my book *Scienza e Linguaggio* (Aracne, Roma, 2015; in Italian); the English translation can be found here: http://www.brera.inaf.it/utenti/foschini/Foschini_translation.pdf

8. **Tim Bradshaw**
November 25, 2020

We're in the late stages of the latest AI hype cycle: this one is based on a fairly old idea which has only recently become practical, which is throwing deeply astonishing amounts of training data at machine learning systems. As with previous hype cycles, impressive early results lead to wild claims about artificial general intelligence coming in a few years usually followed by some kind of exponential runaway in intelligence. As with previous cycles it is not coincidence that many (not all) of the people making these claims are seeking funding for AI work. As with previous hype cycles the impressive early results will be followed by less impressive later ones as the tricks hit a wall – once you have trained your language model on all the natural language that there is and it still doesn't produce very good natural language, where do you go? And what does it tell you about how intelligent systems learn language (hint: nothing)? And so the bubble of hype will burst, a new AI winter will follow, people will look back on the wild claims as being rather silly, until eventually they forget and the cycle repeats.

Well, perhaps it will repeat. Previous AI hype cycles have been both started and stopped by various exponential processes: available computing power increased exponentially with time, but the computing requirements of AI increased either exponentially with a shorter time constant, or worse than exponentially. But computing power certainly can not increase exponentially for ever: its increase is bounded above by some constant power of time (which is probably effectively 2). We're perhaps some way from the exponential increase hitting the physical limits, but probably not tens of years away.

The good part of all this is that, as with the previous hype cycles, good applications will be found. My favourite is weather forecasting: there is a vast torrent of data and tomorrow's initial conditions are the data against which you can train today's run.

But what isn't going to happen is some artificial general intelligence deriving new and interesting laws of physics. That's just what that the people hyping AI need us to believe so they keep getting funded.

9. **Geoffrey Dixon**
November 25, 2020

A child discovered to be a mathematical prodigy may be directed from that point onward by establishment figures into doing research intended to advance ideas supported by the majority. In acquiescing the child will likely lose all chance of ever fulfilling his/her potential – of ever doing anything truly original.

Likewise, an AI might well be a computational prodigy, but it too will be nudged into areas supported by those feeding its algorithms data and methodologies. However, unlike the child prodigy, the AI will never have the potential to shatter paradigms, or to conceive something truly original. Indeed, it can not conceive at all.

10. **Thomas Larsson**
November 25, 2020

Yes, we know that 95% of the universe consists of dark stuff, but this is "knowing" in the sense that renaissance astronomers knew that the universe is governed by 13 epicycles. Within the framework of epicycle theory, experiments implied that there were at least 13 of them, and within the framework of GR+QFT, experiments show that 95% of the universe is dark. Both GR and QFT have of course been tested to much higher accuracy than epicycle theory ever was, but only in domains where the other can be ignored, and we know that they appear to be mutually inconsistent. In particular, the naïve quantum calculation of the CC is off by 120 orders of magnitude, which may indicate that GR+QFT might fail in situations where the CC is important.

11. **Peter Woit**
November 25, 2020

Dijkgraaf does say he has three arguments against physics being finished. Most of his article is about the third argument, which is what I discussed. For the first of his arguments, he himself gives the counterargument.

The second is the usual "we don't understand dark energy/matter, and it's most of the universe" argument. Everyone who makes this argument likes to cite the relative abundance, but I don't see how that matters for the question of how large a hole in our understanding these things are.

On dark energy, it appears there's a single number that describes it, one of 30 or so numbers in our best theory which we don't understand. The issue of understanding this particular number has been dead in the water for a long time: I know of no viable idea for calculating it. The string theory landscape people will tell you it is inherently uncalculable, it's an environmental artifact of the baby universe we happen to be in.

Dark matter is a much more complicated phenomenon, and it has received a huge amount of attention from theorists over several decades. All the relevant data is astrophysical, and I'm willing to believe astronomers in coming years will get us more. But, it seems to be very tough to move beyond "there's something out there gravitating, but we don't know what it is". Maybe it's right-handed neutrinos, or some other sort of unknown fundamental particle whose interactions are purely gravitational. But maybe it's instead something we don't understand about astrophysicsics, e.g. primordial black holes. Sure, this is an open problem, fundamental physics is not finished. I don't think Horgan's claim was that no open problems would remain, it was that progress towards solving them would stop.

12. **Scott Aaronson**
November 25, 2020

I mean, you could criticize Dijkgraaf's piece for being anodyne—yes, *of course* fundamental science has been gradually shifting its emphasis for decades from "what is" to "what could be," that's why so many of us now work on subjects like quantum information or topological matter or synthetic biology, welcome to the club! But like John Baez, I find it hard to argue that Dijkgraaf's thesis is *wrong.*

Indeed, let me propose a corollary: if (contrary to John Horgan) fundamental science does continue indefinitely, then it will be for basically the same reason why *pure math* continues indefinitely—namely, because the relevant limits turn out to be only those of possibility-space, not of physical space.

13. **Peter Woit**
November 25, 2020

Scott,
I'm not at all arguing that Dijkgraaf's thesis (or that of John Horgan from 1996) is "wrong". Sticking to the case of fundamental physics, I think Horgan was correct in pointing out that research was hitting limits, and that fruitless speculation like string theory was evidence of this. Lots of physicists were outraged by Horgan's argument, for an example of the reaction at the time, see a Wall Street Journal editorial by Gross and Witten
https://www.wsj.com/articles/SB837118256541339000
They argued that new revolutions were on the way, with string theory and supersymmetry promising ideas about to be tested at the Tevatron and LEP.

What I think is remarkable is the way Dijkgraaf now seems to be agreeing with Horgan. That much of the focus of theoretical physics at Princeton for many decades has been a huge failure is not something Dijkgraaf is going to say, but it's implicit in his not mentioning it. His job is to generate enthusiasm for theoretical physics and to get funding agencies and wealthy people to pay theoretical physicists, so he's putting the best face possible on what is happening.

My own agenda is somewhat different, so I'd rather see a clear-eyed examination of what went wrong and of what possible ways forward remain for those who don't want to give up and do something else. Maybe one day we'll see that in Quanta also.

14. **John Horgan**
November 26, 2020

Peter, thanks for kicking off this discussion of the themes my old book. I found Dijkgraaf's essay to be, as you and others suggest, mere marketing, and not very original at that. Michio Kaku and others were saying in the 90s that learning the laws of nature is like learning the laws of chess. The game is just beginning! And so on. I addressed that wan hope in 1996, as well as the hope that super intelligent AI will jumpstart a new era of physics, which Hawking proposed in the early 80s. I too, would love to see a "clear-eyed examination" of the status of physics, but that's asking a lot from people who have so much invested in positive outcomes.

15. **David Garfinkle**
November 26, 2020

The point of view of the Dijkgraaf article is not that unusual: essentially it is what the condensed matter physicists have been saying for decades. The only unusual thing is that a string theorist is saying it.

16. **tulpoeid**
November 26, 2020

There is no very mild way to put it, Dijkgraaf's view is pathetic. I'm happy to have my tax money going into basic science and avant-garde engineering, but not at all to engineering that poses as basic science.

17. **Peter Shor**
November 26, 2020

tulpoeid: So condensed matter theory is just engineering and not basic science?

There is a ridiculous amount of real science related to condensed matter theory that we don't understand, and one approach to trying to improve our understanding of it is to build specially designed materials.

Is making new chemicals just "engineering that poses as basic science" if you're doing it to better understand the laws of chemistry, and not for a specific application? (Maybe some research directions that involve making new chemicals are not worth funding, but I would hope that the funding agencies would decide not to fund them; the same should be true of new materials in condensed matter physics.)

18. **anon**
November 26, 2020

A minor comment, maybe not even worth making, is that you can do fundamental physics with condensed matter. For example, it is possible to imagine an alternate version of history where the leap from classical to quantum physics was made entirely via condensed matter experiments, basically because hbar has units of (length) * (momentum), and so you don't need to go to short distances to find it– you can see quantum effects on large scales by going to low momenta (or low temperatures). And so if there's another layer of physics out there beyond quantum mechanics, and/or another basic constant like hbar, it may well show up via condensed matter experiments. (Of course, the timescale for such a basic discovery could well be centuries, which is why this comment is a bit hollow.)

19. **Tim Maudlin**
November 26, 2020

I am just seconding things already said here, maybe with a different emphasis. The heart of the controversial "third argument" of the piece is this:

"The aim of physics is to understand in a precise, mathematical way all manifestation of matter and energy in the universe — and we have barely started to explore this infinitude of possibilities. Claiming that physics is finished is akin to arguing that mathematics ended after the introduction of natural numbers and basic arithmetic, or that chemistry was over with the advent of the periodic table. Learning the rules of chess doesn't make you a grandmaster.

The truth is, the realm of the smallest particles is not the only place you can find the fundamental laws of physics. They can also "emerge" out of the collective behavior of many constituents."

There is a name for the discipline devoted to to studying how systems can be constructed using known or given fundamental laws so that it behaves in a certain way. That name is "engineering". That can be as hard, and insightful, and interesting as any scientific discipline. Quantum information theory and quantum computation, in this sense, are obviously engineering. They ask the question: *Given* that the constituents of the system are governed by the laws of quantum mechanics, how can systems that transmit or process information be built?". That is an extremely interesting and hard question.

The only issue seems to be about terminology. No one would doubt that quantum information and computation are disciplines that can properly be called "physics". No one should doubt that they also can be properly called "engineering": the terms are not mutually exclusive even if the academic world creates different "schools" for each. If some physicists had not spent a lot of time denigrating engineering (cf. The Big Bang Theory) that observation would go down easier. The last ditch of the fight is then over the term "fundamental". In the usual sense in which that term is used, engineering and, say, condensed matter and solid state physics are not "fundamental", and the principles they employ are not "fundamental": they are both "emergent" and often just reliable generalizations that can, physically, be violated (like the "Second Law" of Thermodynamics which in a clear sense in not a physical "law" at all since it can be violated by a physical system).

There are some people (like me) who are largely interested in fundamental physics, fundamental ontology and physical law (properly speaking). That is just a sort of intellectual preference or matter of taste. Some of us think that the whole project of trying to figure out these fundamentals went bad, and the badness goes back to Bohr and Heisenberg. We wonder whether progress has slowed not merely because the issues are harder in various ways but because the whole intellectual approach has been corrupted. The flourishing of the non-fundamental disciplines really does not help us in our particular arena of interests, but it is certainly a thing to be celebrated.

20. **Peter Orland**
November 26, 2020

To add to anon's remarks above: you can measure $\hbar$ with photocells or light-emitting diodes.

21. **David**
November 26, 2020

Hi Peter,

Why is it you think potential future physics discoveries by AI could be not "interesting enough to be worth understanding"?

Is it because one can envision the AI being programmed to think based on the same ways theoretical physicists do and thus will further engage in the kinds of stagnation we've seen with things like string theory etc?

I find it plausible that the AI could eventually think very differently to the way humans do and thus circumvent our limitations.

22. **JE**
November 26, 2020

My take on Dijkgraaf's article is that no, physics is not finished, but for much better reasons he has been able to expose. He is even borrowing the success of other fields, like cosmology, to hide the lack of success of his own field (HEP and theoretical physics) in the first place.

-Not because the first two decades of this century have been pretty successful for physics, which they have not, even if the Higgs, gravitational waves and the first image of a black hole's event horizon have been discovered or taken.

-Not because 95% of the universe is missing. This is just a statistical fact or a teaser, not a reason. Maybe it is missing because we do not have the correct theory of quantum gravity. Or even the correct theory of quantum electrodynamics.

-Not because any authority claimed that natural numbers or the periodic table had put an end to maths or chemistry, or at least not as explicitly as those who claim now that progress in HEP and theoretical physics has come to an end because we cannot afford more expensive particle accelerators. They might help, but nothing else.

-Not because the behavior of phonons is similar to the behavior of photons, because it is only very vaguely similar.

-And certainly not because the fundamental laws of physics, which are quantum laws, can emerge out of the collective behavior of many constituents. We already know that the laws of the macroscopic world are quite different from the laws of the subatomic world.

The rest of the arguments are even weaker imho.

No, it is because, despite the lack of support or further experimental evidence, some researcher or group of researchers will be able to make their way through it. And because this effort is already underway.

23. **Peter Woit**
November 26, 2020

David,

All evidence I've seen is that the only ideas theorists have about using "AI" in theory are ideas for how to automate dumb things people are doing so they can be done more quickly and on a larger scale. Large scale dumb is still dumb.

I see how AI can have a role when there's a lot of relevant data that a human mind is going to have trouble making sense of. The basic problem of current fundamental theory though is that there is virtually no relevant data (i.e. data that disagrees with our best model).

As John Horgan points out, already back in the 1980s people were talking about this. Nothing ever came of it, I don't see any reason to believe that situation will change anytime soon.

On the other hand, invoking computers has historically been a good way for theorists to try and get money. That has often worked, is working now, and will work in the future.

24. **John Baez**
November 26, 2020

To me the question of whether condensed matter physics is "fundamental" or "engineering" is infinitely less interesting than… *condensed matter physics!*

You can make a supersolid where holes in a crystal – the *absence* of electrons – form a Bose-Einstein condensate and flow like ghosts through the crystal lattice. You can make “liquid light” using exciton-polaritons, which are a blend of photons and electron-hole pairs. You can make a Wigner crystal: a 2-dimensional periodic structure of repelling electrons. And it goes on and on: we’re in the golden age of new forms of matter! A physicist would need to have a really sour disposition to remain unexcited by these things.

25. **Tim Maudlin**
November 27, 2020

John Baez: There is a sense in which I agree with your comment, but it also illustrates the sense in which the fundamental is, well, *fundamental* in a way that creates an asymmetrical dependence of understanding. It certainly sounds quite amazing that the *absence* of an electron, or a *hole*, can behave like the *presence* of an electron or a *particle*. And one would like to understand that better. But what claim is even being made cannot really be grasped without first understanding what an electron *is* and what are the physical conditions that make it accurate to say there is one *present* and to ascribe to it a location. When reading your comment, one reflexively falls back on a Democritian picture of electrons as sharply located particles, maybe even point particles. And maybe that is even correct! But most physicists would flatly deny that that is the right way to understand what an electron is. So this is a nice illustration of how unclarity about the foundations inevitably flows upward to a sort of unclarity about what is built on, or constructed out of, the foundations, while there is no obvious necessity of an analogous retro-transmission of lack of understanding downward. That’s part of what draws some of us to the fundamental questions.

26. **Thomas Larsson**
November 27, 2020

” why *pure math* continues indefinitely”

Scott, I’m not sure this is really the case. A long time ago, people (Wigner and probably others) used to say that “God is a mathematician”. But of course the dual formulation of that statement is that “Physics is divine mathematics”. Not necessarily because divine math is better than mundane math, but because it is the kind of math that God the mathematician does and publishes in Nature (the bitch, not the journal).

But then again, some of us believe that divine math really is better than mundane math. So if fundamental physics runs out of steam, so does divine math.

27. **Amitabh Lath**
November 27, 2020

The conversations are good indicators of where these elder statesmen of the field feel we are going. But they are not the only voices we should listen to.
Physics will end when the brightest young people no longer want to pursue it. I have not seen this. In fact, even during the pandemic there are many undergraduate and graduate students wanting to work in particle physics. Generally speaking these are top tier students who could probably join any group in the department.

28. **Moshe**
November 28, 2020

Peter Orland and anon., historically in the way quantum mechanics was developed statistical mechanics played a huge role, say in the story of the black body radiation or in most of Einstein’s contribution to the subject. It could be an illustration of why often what is “fundamental”, to the extent that this is even an interesting question at all, can be only decided after the fact.

29. **Pierre Ramond**
November 28, 2020

Every so often, human beings like to believe that their century is special, and its impressive new tools and advances will take us to some sort of final clarity. Sounds a lot like Constantin’s world-view. It is good to remember that today’s queries which are firmly in the realm of science were viewed not so long ago as pertaining to the sacred.

The most striking aspect of universe-view today is the lack of paradoxes and contradictions with experiment! As noted by Mark Twain, history does not repeat itself, but it sure rhymes; science is not so different. I wonder how people felt after Newton’s death where the explanations of all phenomena led to his world-view. The challenge came not from the mind of great people but from the study of electricity. Will the study of dark matter provide the clues to go beyond?

We live in the post-Einstein era, a time of great synthesis that left us with the General Theory of Relativity and Quantum Mechanics, neither challenged by thought nor experiment. Yet both seem incomplete in different way. What price compatibility?

The spectacular Standard Model explains most forces in the early universe; but it still is a fragmented view of matter with many loose parts.

As Brownian motion nailed the molecular theory of bulk matter, will the Cosmological constant be a pointer to the underlying theory of space-time?

So many questions, so little time …

Read Einstein’s paper on the photoelectric effect. There is a wonderful discussion of in volume II (1963) of “The Natural Philosopher” by the late Martin J. Klein. That is what sublime physics is like!

Voltaire’s “Lettres Philosophiques” (letters 14,15,16) discuss the world views of Descartes and Newton.

30. **tulpoeid**
December 9, 2020

Peter Shor:

If something is application of science then, yes, it is engineering.
Examples of condensed matter applications specifically with the goal of advancing “basic” knowledge would be useful, though.

In any case, I can read this passage only as trying to sell engineering as basic research:
“All of this is part of a much larger shift in the very scope of science, from studying what is to what could be. In the 20th century, scientists sought out the building blocks of reality… This century, instead, we will begin to explore all there is to be made with these building blocks.”

31. **Sundar Narayan**
December 9, 2020

What Dijkgraaf seems to be saying is that engineering will replace and supersede physics. As an engineer, this is music to my ears, which is precisely why I do not believe Dijkgraaf. If it sounds too good to be true, then it probably is. Without physics, there will be no engineering. Period.

Various and Sundry

December 6, 2020
Categories: Uncategorized

A few recent items of interest:

- Martin Greiter has put together a written version of Sidney Coleman's mid-1990s lecture Quantum Mechanics in Your Face, based on a recording of one version of the lecture and copies of Coleman's slides.

  It's often claimed that leading physicists of Coleman's generation were educated in and stuck throughout their careers to a "shut up and calculate" approach to the interpretation of quantum mechanics. Coleman's lecture I believe gives a much better explanation of the way he and many others thought about the topic. In particular, Coleman makes the crucial point:

  > The problem is not the interpretation of quantum mechanics. That's getting things just backwards. The problem is the interpretation of classical mechanics

  He ends with the following approach to the measurement problem:

  > Now people say the reduction of the wave packet occurs because it looks like the reduction of the wave packet occurs, and that is indeed true. What I'm asking you in the second main part of this lecture is to consider seriously what it would look like if it were the other way around—if all that ever happened was causal evolution according to quantum mechanics. What I have tried to convince you is that what it looks like is ordinary everyday life.

  While some might take this and claim Coleman as an Everettian, note that there's zero mention anywhere of many-worlds. Likely he found that an empty idea that explains nothing, so not worth mentioning.

- For the past year the CMSA has been hosting a Math-Science Literature Lecture Series. The talks have typically been excellent surveys of areas of mathematics, given by leading figures in each field. To coordinate with Tsinghua, the talks have often been at 8am here in NYC, and several times the last couple months I've made sure to get up early enough to have breakfast while watching one of the talks. All of the ones I've seen were very much worth the time, they were the ones given by Edward Witten, Andrei Okounkov, Alain Connes, Arthur Jaffe and Nigel Hitchin. Jaffe's covered some of the ideas on Euclidean field theory that I've been spending a lot of time thinking about. For a more detailed version of his talk I highly recommend this article.
- Peter Scholze has posted at the Xena blog a challenge to those interested in formalizing mathematics and automated theorem proving (or checking): formalize and check the proof of a foundational result in his work with Dustin Clausen on "condensed mathematics". As part of the challenge, he provides an extensive discussion of the motivation and basic ideas of this subject, which attempts to provide a replacement (with better properties) for the conventional definition of a topological space.

  Spending a little time reading this and some of the other expositions of the subject convinced me this is a really thorny business. Scholze explains in detail his motivation for making the challenge in part 6 of the posting. My suspicion has always been that most of the value of computer theorem checking lies in forcing a human to lay out clearly and unambiguously the details of an argument, with that effort likely to make clear if there's a problem with the theorem. It will be fascinating to see what comes of this project.

  I see there's also a blog posting about this at the n-category cafe.

- On the topic of theorems that definitely don't have a clear and unambiguous proof, supposedly PRIMS will be publishing Mochizuki's IUT papers in 2021. Mochizuki and collaborators have a new paper claiming stronger versions of Mochizuki's results.

Comments

1. **Michael Weiss**
   December 6, 2020

   Peter,
   I write to provide a nuance to your inference that Sidney Coleman's question–"what it would look like if it were the other way around, if all that ever happened was causal evolution according to quantum mechanics?"– was unrelated to Everett's Many Worlds Interpretation (MWI).

   I believe that MWI was in fact the starting point for Sidney's line of thought. The evidence comes from his final lecture in Physics 143 in May, 1977 at Harvard College. (You took this class a year earlier, I think, with Ed Purcell.) Prof. Coleman ended this wrap-up lecture by asking the class exactly the above question and suggesting an answer: "It would look like ordinary everyday life." We sat in silence for a minute, trying to make sense of this insight.

   Frank DeLucia (a few years later to develop the DeLucia-Coleman Theorem) was TA that semester and (if he reads this blog) may have further insight. John Preskill was TA in 1976 with Ed, and may also remember how Sidney's thoughts evolved.

2. **Peter Woit**
   December 6, 2020

   Michael Weiss,

   Coleman explicitly does refer to Everett, and yes, he's an Everettian in the sense of seeing no reason QM without a reduction postulate can't describe the world as we know it. My point was just that he doesn't invoke a splitting into "many worlds" to replace wave-function reduction.

   I'd be curious to know if his views evolved, whether exposure to Everett changed an earlier, different point of view. The point of view he's taking (reduction of the wave-function is apparent, arising out of properly treating Sidney Coleman as a quantum system and the emergence of classical mechanics and our consciousness built around that) seems to me what I would have thought was the conventional one of the mid-1970s era when we were learning the subject. On the other hand, at that time I remember never hearing about Everett, hearing a lot about Copenhagen.

   Coleman explicitly says that arguing over a Copenhagen vs. Everett interpretation of QM is "getting things just backwards", ignoring the real problem of how we get classical out of quantum. He refers to "vernacular quantum mechanics", which is a "looser and sloppier" Copenhagen interpretation. That may be a better way than "shut up and calculate" to refer to a standard QM course's approach to "interpretation".

3. **John McAndrew**
   December 6, 2020

I'm a little disappointed that the questions he answered from the audience at the end aren't recorded in the text. He's asked if he's a follower of Everett's Many Worlds interpretation at 1:04:20:

Sidney Coleman, Quantum Mechanics in Your Face [1994]
https://youtu.be/EtyNMlXN-sw?t=3853

'Everett wrote a truly wonderful paper, then everyone got on their horse and rode off in all directions"

I'm guessing this is: Relative State' Formulation of Quantum Mechanics", Reviews of Modern Physics, 29: 454–462, cited here: https://plato.stanford.edu/entries/qm-everett/

4. **Peter Woit**
   December 6, 2020

   John McAndrew,

   Thanks for pointing that out. Does anyone know what he had in mind by the "everyone got on their horse" comment?

5. **S**
   December 6, 2020

   It's nice that Coleman doesn't bring in "many worlds," but on the other hand, it seems to me that his solution of "Where does Born come from?" is going to suffer from the same thing as any Everettian theory — some (possibly hidden) additional axiom or measure that is not clearly any better motivated or more coherent than collapse.

   (The problems with Coleman's own approach to getting Born's rule have been documented by, e.g., David Albert. I can dig up the reference if anybody is sufficiently interested; otherwise I'll spare myself the hassle.)

6. **Peter Woit**
   December 6, 2020

   S.
   Coleman was explicitly not claiming to be saying anything original.

   I'm curious to hear if others know more specifically about Coleman's views, but don't want to start a rehash of the usual arguments over interpretations.

7. **Frank Wilhoit**
   December 7, 2020

   Coleman's complete course on QFT, from the 1975-76 academic year at Harvard, is online here: https://www.physics.harvard.edu/events/videos/Phys253

8. **James Smith**
   December 7, 2020

   Your suspicion "..that most of the value of computer theorem checking lies in forcing a human to lay out clearly and unambiguously the details of an argument…" is spot on in my opinion.

9. **Michael Weiss**
   December 8, 2020

   Peter,
   Please forgive my lack of clarity: I left out that that in his last lecture in 1977 Sidney Coleman explicitly defined and discussed Everett and the MWI–but as a topic of enrichment not on the final exam. I guess he was unusual among the Harvard Physics faculty at that time in including this material even as a bonus topic.

   I believe that Prof Coleman ended Physics 143 with these conceptual issues because he simply loved playing with ideas and engaging young minds.

   Curiously, you may remember that Sidney late in his life gave an interview in which he claimed to have disliked teaching. If so, he sure fooled my classmates and me! We loved him.

10. **Peter Woit**
    December 8, 2020

    Michael,
    Thanks! I'm sorry I missed that version of Physics 143. The version I took a year earlier was taught by Norman Ramsey, using David Saxon's textbook. It's been far too long for me to really remember, but if he discussed interpretational issues, I don't recall that.

    I'll take a longer look at the Saxon book later, but from a quick look online, there's nothing like a "Copenhagen interpretation" discussion, and what is there isn't inconsistent with Coleman's Everettian viewpoint that in principle the state of the observer should also be described by QM.

    Sorry for my continual negative reaction to the "Many-Worlds" terminology. As I've written about here far too often, I think it's a really bad terminology to use to refer to the ideas Coleman was discussing, especially given recent publicity campaigns for the idea that invoking multiple worlds explains things that it doesn't. I'd be very curious to hear what Coleman thought of this, it's a shame he's no longer with us to discuss.

11. **Jon101**
    December 8, 2020

    There seems tobe some renewed interest in Mochizuki's work: he gave a colloquium at Berkeley in november https://mathoverflow.net/questions/375889/berkeley-mathematics-department-colloquium-by-s-mochizuki and there is a joint Rims-Lille seminar where several people from both inside and outside Mochizuki's circle are giving talks http://www.kurims.kyoto-u.ac.jp/~bcollas/IUT/index.html

12. **martibal**
    December 8, 2020

    Off topic but could be of interest for Peter: the last paper of Penrose, together with Marcolli, about twistors and noncommutative geometry
    https://arxiv.org/abs/2012.02823

13. **Peter Woit**
December 9, 2020

Frank Wilhoit,
Perhaps more useful than the videos, Coleman's QFT lectures are now available as a book, see
https://www.math.columbia.edu/~woit/wordpress/?p=10799

Jon101,
I've seen no evidence of increased interest in IUT recently, quite the opposite. Mochizuki and his circle have dealt with the problem pointed out by Scholze and Stix essentially by ignoring it based on a claim that Scholze and Stix are ignorant incompetents. It's striking (and actually, disturbing) that neither in Mochizuki's talk nor anywhere in the Lille/RIMS program is there any mention of the Scholze-Stix argument that the proof is flawed. Until Mochizuki or someone else comes up with a convincing rebuttal to Scholze/Stix, very few experts will pay attention to this.

14. **John Baez**
December 9, 2020

Peter Woit wrote:

> Does anyone know what he had in mind by the "everyone got on their horse" comment?

I don't know what he meant, but Everett *did* write a wonderful Ph.D. thesis on the relative state interpretation, which said very little if anything about "worlds" or "branches" — and then Bryce DeWitt and John Wheeler and others got into the act and introduced, or at least popularized, those terms. You can see the whole process of degradation in the book *The Many Worlds Interpretation of Quantum Mechanics*, edited by DeWitt and Neill Graham, which includes Everett's thesis followed by essays by DeWitt, Wheeler, Graham and others. It's sad how Everett gets blamed for other people's interpretations of his work. I don't know how much he went along with it.

15. **Jack Morava**
December 9, 2020

Re: theorems that definitely don't have a clear and unambiguous proof

Physics is very interesting. There are many, many interesting theorems. Unfortunately, there are no definitions.
David Kazhdan.

(stolen from https://www.jmilne.org/math)

16. **John Baez**
December 9, 2020

That's why category theory and physics work together so synergetically: category theory has tons of great definitions but no interesting theorems. 😜

17. **Low Math, Meekly Interacting**
December 9, 2020

I stumbled on this a while back because I was a big fan of the Eels, long before I knew Mark Oliver Everett was Hugh Everett's son.

A letter Hugh Everett wrote to Bryce DeWitt…

https://www.pbs.org/wgbh/nova/manyworlds/orig-02.html

18. **John McAndrew**
December 9, 2020

John Baez and Peter,

you might be interested in Everett's amoeba analogy from an early draft of Hugh Everett's doctoral dissertation at Princeton Universty:
https://www.pbs.org/wgbh/nova/manyworlds/orig-01.html

"The same is true of [sic] one accepts the hypothesis of the universal wave function. Each time an individual splits he is unaware of it, and any single individual is at all times unaware of his "other selves" with which he has no interaction from the time of splitting."

How does one interpret this?

It looks to me that Everett was the first to get on a horse and ride off in some bizarre, speculative direction, eventually crafting the paper that Coleman admires after Wheeler and co reigned in some of his 'not even wrong' ideas.

19. **Peter Woit**
December 10, 2020

John McAndrew,

Thanks. I should make clear that I don't think the problem with the "splitting into multiple worlds" business is that it's bizarre or speculative, it's that it's meaningless, and pretends to solve a problem that it doesn't. I'd be curious to hear what Coleman thought of this, but the fact that he doesn't mention multiple worlds and does explain that the hard problem is understanding how to interpret classical mechanics makes me guess he would have agreed that multiple worlds explain nothing.

20. **TonyK**
December 10, 2020

Peter, this is from the Wikiquote page on Stephen Leacock:

"Lord Ronald said nothing; he flung himself from the room, flung himself upon his horse and rode madly off in all directions."

"Gertrude the Governess", Nonsense Novels (1911)

Coleman realised that this quote could be interpreted literally in an Everettian world (or worlds? — syntax fails me here).

21. **Low Math, Meekly Interacting**
December 10, 2020

I am myself struck by the similarity between the quote from Coleman's lecture and Everett's expressed point of view, in regards to human experience. In fact, in essence their positions appear to be identical almost down to the wording. The only possible distinction seems a purely philosophical one, i.e. whether or not the notion of a single "reality" is justifiable if one agrees there is nothing but "causal evolution according to quantum mechanics".

22. **Peter Woit**
December 10, 2020

LMMI,
Unlike Everett, I don't see Coleman anywhere mentioning "splitting". The only place the term "reality" occurs is once in the combination "Quantum Reality", part of a proposed title for the talk. He seems to have no interest in the usual arguments over "reality".

My quantum education was very conventional for the early-mid-seventies, no exposure to Everett, directly or indirectly through Coleman. I distinctly recall being well aware that the obvious and simplest way to interpret QM (without wave-function reduction) was as a theory that applied to everything, including the observer and the observer's consciousness, that there was nothing about our experience of the world in conflict with this.

23. **Low Math, Meekly Interacting**
December 10, 2020

Hi, Peter,

I probably should have expressed myself better: They appear to be in 100% in agreement about the scientific content. All that's left to disagree about appears to be the "reality" of other possible experiences, including whether or not that's even something worth pondering.

24. **S**
December 10, 2020

I guess, Peter, that I'm confused about how one could take a unitary-only / Everettian / Colemannian approach to QM and *not* talk about many "worlds" or at least, if the "worlds" seem confusing, about many parallel non-interacting observers who all think that they are the same person. You end up with a wave function on configuration space that is extremely sharply peaked over various "classical" configurations, each of which corresponds to a different observer + environment, and if one takes each of those observers seriously, then there are a bunch of different Peters, who will have made (say) different moderation decisions about the value of this comment.

Call it worlds or don't — and I can see where talk of "branching" is maybe not helpful (although it's a reasonable description of decoherence in the Everett model, no?) — but this feature seems unavoidable at that point.

And so do problems with getting the Born rule out of the picture.

One thing I've wished is that I understood better what your — Peter's — alternative to this view is when you talk about unitary-only without many-worlds.

25. **Peter Woit**
December 10, 2020

S,
I just don't think saying "my initial more or less classical state branched into multiple other more or less classical states and I'm in one of them, other mes are in the others" explains anything about anything, or answers any kind of meaningful question. You can think about the world that way if you want, but you haven't solved any problem by doing so.

There are endless problems with making any sense out of "branching", in practice that's nothing but a meaningless word standing in for not understanding the measurement problem. What you're doing is trying to recover a classical description of reality, by ignoring the quantum nature of reality except at these mysterious places where classical reality "branches". I take Coleman as saying it's a mistake to try to describe reality classically, there is only quantum reality. Put differently, classical behavior is a complex, hard to understand emergent phenomenon. If you take that as the fundamental nature of reality and try and express everything in terms of it, you're going to end up with a confusing and paradoxical conceptual framework.

26. **Peter Shor**
December 11, 2020

Some questions that I think show that many-worlds doesn't actually solve the "measurement problem".

How do two photons in an Aspect-type experiment that are well separated in space know whether they're supposed to be in the same "world" or not? Do the "splits" between worlds happen everywhere in the universe at once (and does this violate the theory of relativity?) or do they start at a single point and radiate out like waves the speed of light (and in this case how do two world-splitting waves approaching each other know which worlds in one of these waves should be merged with which worlds in the other?)?

27. **Jim Akerlund**
December 11, 2020

Peter,
Of all your comments on the Coleman lecture, I at least expected a comment about this statement from the lecture.

"There's no point in trying to wow him with the anomalous
magnetic moment of the electron or the behavior of
artificial atoms that we just heard about or anything like
that, because he is so deeply opposed to quantum mechanics
and so old and stubborn that as soon as you
start putting a particular quantum mechanical equation
on the board his brain turns off, rather like my brain in
a seminar on string theory."

28. **Homodyne**
December 11, 2020

Coleman's derivation has an analogue in terms of classical probability.

If we imagine a system, say a coin, that is probability p to be heads and 1-p to be tails we could write it as:
p[H] + (1-p)[T]

Then imagine a detector which enters the state D or E depending on whether it sees heads or tails.
Well then after measurement we have:
p[H,D] + (1-p)[T,E]

Nobody would read this as "two separate worlds". As Peter Woit says it doesn't really solve anything. It's just tortuous semantics for a basic probability assignment.

What Coleman then goes on to describe is say we do this again with more and more coins then the probabilities essentially go to 1 for histories where the ratio of heads events to the total number of event is p and converges to 0 for other histories. In essence it's the law of large numbers.

The only mystery/issue is that in the case of a quantum system and recording device we would have:
c|H,D> + d|T,E>
where c and d square to p and 1-p.

The issue/thing to be explained is that we only recover the same probabilistic reading provided we can only measure the device in the |D>,|E> basis and that other "interference" bases are non-physical such as {|D>+|E>,|D>-|E>}.

This indeed seems to be the case from empirical evidence, but we need a detailed explanation as to why. Roland Omnes's famous 1994 book has a good sketch in Chapter 8 (although one would need Chapters 6 and 7 to fully understand it) and I'd also recommend Allahverdyan et al's paper here:
https://arxiv.org/abs/1303.7257
It's a summary of a much longer paper by them from 2011.

29. **Moshe**
December 13, 2020

One of the issues I have with MWI is that (like many other interpretational issues) it is not specific to quantum mechanics. In fact there is already an old approach to probability called modal realism which is essentially the same as the MWI. The fascinating thing about quantum mechanics is that it is not equivalent to a classical probabilistic system. Focussing on issues that already exist for classical stochastic systems misses all the real mysteries. Another common example is attributing some mystery (like conflict with relativity) to quantum entanglement that already exists for plain statistical correlations.

30. **Homodyne**
December 13, 2020

Moshe,

Thanks for that. It's what I was clumsily getting at above. If you treat the classical probability distribution as "real" then one essentially has many worlds for classical probability. I wasn't aware that it had a formal name, i.e. "modal realism".

Of course Spekkens toy model shows that many of the features people commonly say are quantum can be reproduced in a classical theory. For example super dense coding, teleportation, entanglement, no cloning, interference.

Thus allowing one to focus on what classical probability cannot replicate: Kochen-Specker contextuality and CHSH violating correlations.

31. **John Baez**
December 14, 2020

"Focussing on issues that already exist for classical stochastic systems misses all the real mysteries."

Not all of them. Classical probability theory is *already* deeply mysterious – witness the Bayesian/frequentist arguments about what probabilities actually mean. Any mystery one doesn't get straightened out in classical probability theory is likely to come back with redoubled force when you start thinking about probabilities in quantum mechanics.

But yes, quantum theory holds new mysteries.

32. **J Frager**
December 14, 2020

In the paper mentioned above (http://www.kurims.kyoto-u.ac.jp/~motizuki/Explicit%20estimates%20in%20IUTeich.pdf), Mochizuki et al. seem to propose an entirely new proof of Fermat's last theorem for prime exponents large enough. Has this been checked/confirmed/peer-reviewed? I haven't seen much discussion on that, although such a claim would seem to warrant it. Have the people involved in the original proof of FLT (Wiles, Taylor, Conrad etc.) expressed an opinion?

33. **Peter Woit**
December 14, 2020

J. Frager,
It has long been known that a proof of abc can be used to prove Fermat in this way. The problem is that this new paper depends crucially on the part of Mochizuki's IUT-based arguments which Scholze/Stix have convincingly argued can't work. As far as I can tell, essentially no one outside of RIMS and Nottingham thinks there is a proof, either in the original work, or in this new one.

The new work does imply much stronger claims than the earlier IUT papers. Perhaps the most interesting aspect of these is whether any of them can be shown to not be true, providing a direct disproof of Mochizuki's original work.

34. **Peter Woit**
December 14, 2020

For an example of what the problem is with the multiverse publicity campaign for MWI, check out the latest New Scientist
https://www.newscientist.com/article/mg24833122-100-if-the-multiverse-exists-are-there-infinite-copies-of-me/
where you'll learn that the way to think about quantum mechanics is this:
"The startling upshot of this view is that there are potentially squillions of versions of you going about their (your?) business in parallel universes.

Well, sort of. Those other versions of you aren't really copies, says Sean Carroll, a physicist at the California Institute of Technology: they are individuals who used to be you, but at some point split off and became separate. "You are not spread out over worlds," says Carroll. "You are here in this world, and there are a lot of other people in other worlds who are closely related to you."

As to how many other-worldly relations you have, it is impossible to say. "The number could be infinite or there could be a continuum of worlds rather than a discrete set," says Carroll. "But the number might also be finite. We're not sure.""

35. **Moshe**
December 14, 2020

John Baez, I think you know what I mean. But as an aside, it is an interesting sociological phenomena how philosophical discussions of scientific subjects can be completely decoupled from the scientific ones. My impression that people that use probability for a living, for an example as reflected in graduate texts in statistical learning theory, are uniformly Bayesian for well articulated reasons. Similar phenomena can be observed in discussions of quantum mechanics, general relativity, etc. etc.

36. **John Baez**
December 15, 2020

Yes, one great thing about working at the Centre for Quantum Technologies part-time is that when I'm there, I never hear the usual tiresome arguments about interpretations of quantum mechanics.

37. **Godfrey**
December 17, 2020

Anent Mochizuki, Scholze and Stix's paper seems to have gone?

http://www.kurims.kyoto-u.ac.jp/~motizuki/SS2018-08.pdf

38. **Peter Woit**
December 17, 2020

Godfrey,
The links may have changed. Mochizuki's page discussing this is here
http://www.kurims.kyoto-u.ac.jp/~motizuki/IUTch-discussions-2018-03.html
and it links to the Scholze-Stix paper as
http://www.kurims.kyoto-u.ac.jp/~motizuki/protectedpdf-2018-08/SS2018-08.pdf

39. **random reader**
December 26, 2020

That Mochizuki's claimed results imply effective ABC was known and published on the arxiv in 2016, by Vesselin Dimitrov. This goes back to Dimitrov's MathOverflow postings in 2012 on the implications of (incorrect, overly strong) claims in earlier versions of Mochizuki's papers, https://mathoverflow.net/questions/106560/philosophy-behind-mochizukis-work-on-the-abc-conjecture . Back then, Mochizuki acknowledged correspondence with Dimitrov and (independently) Venkatesh about those problems in 2012, and apparently revised his papers in response.

Now in the new article by Mochizuki et al, they elliptically refer to someone who must be Dimitrov as a mysterious "one mathematician" who had some related results (which they disparage) using Belyi maps (which are indeed used in the 2016 arxiv paper) but whose work they say they cannot find written down anywhere. As though Mochizuki and company could not get back in touch with Dimitrov or do a web search for any papers he might have written about this.

Not naming the person gives some not very plausible deniability, but it sure looks like Team Kyoto has graduated from denial of problems to outright erasure of work outside their clique.

40. **David Yager**
December 26, 2020

Fermilab's Muon g-2 pages have been updated recently – they had been left unchanged since 2015! Is the announcement near to hand? Probably not this year …
https://muon-g-2.fnal.gov/

Still no leaks to here or to Resonaances – it's just a shame how information no longer wants to be free.

41. **Anot Heran Dom**
December 26, 2020

@random reader,

I was thinking it might have been Taylor Dupuy, but you're right, Dimitrov seems more likely. I think it extremely bad form to not even name the person, let alone the disparaging remarks, verging on academic dishonesty. Imagine if people went around merely alluding to others' prior results but not naming them or giving a citation.

42. **F. Zaldívar**
December 27, 2020

Vesselin Dimitrov's 2016 preprint: https://arxiv.org/abs/1601.03572

43. **random reader**
January 3, 2021

Footnote on Mochizuki from last week's arxiv.

Jakob Stix, who together with Peter Scholze went through the Mochizuki ABC papers and diagnosed the mistake, has found an error that went unnoticed since 1997 in a famous paper by Caporaso-Harris-Mazur. Stix also contributed a lemma to the article by C-H-M announcing and fixing the mistake.

https://arxiv.org/pdf/2012.14461.pdf

Interesting data point on Stix' ability to find errors.

20 Years Later

December 29, 2020
Categories: Uncategorized

Almost exactly twenty years ago I started writing a short article about the problems with string theory. I had been thinking about doing this for quite a while, and the timing of entering the twenty-first century seemed appropriate for evaluating something that had long been advertised as "a piece of 21st-century physics that had fallen by accident into the 20th". The piece was done in a week or two, after which I sent it around to a group of physicists to ask for comments. The reaction was mostly positive, although at least one well-known theorist told me that publicly challenging string theorists in this way would be counter-productive.

One person who wrote back was Phil Anderson, I've quoted some of what he wrote to me in [this posting](). He suggested I send it to Gloria Lubkin at Physics Today, and evidently talked to her about it. I did do this, and after not hearing anything back for a week or two, decided to go ahead and post the article to the arXiv, where it appeared as [String Theory: An Evaluation]().

Rereading that article today, there's little I would change. Its argument is even more valid now than then. The problems of the theory and how it was pursued evolved over the next twenty years in ways far worse than what I could have imagined back then. In particular, the "multiverse" argument explaining away why string theory predicts nothing is something I could not have conceived of in 2001. The tribalistic sociology that has led to a large group of people calling themselves "string theorists" when what they do has nothing to do with string theory is also something I would have thought impossible.

In many ways, twenty years of further failure have had less than no effect. Lubos Motl is [still arguing]() that string theory is the language in which God wrote the universe, and Michio Kaku has a new book about to appear, in which it looks like string field theory is described by [the God Equation](). Ignoring these extreme examples, string theory remains remarkably well-entrenched in mainstream physics: for example, my university regularly offers a course training undergraduates in string theory, and prestigious \$3 million prizes are routinely given for work on the subject. The usual mechanisms according to which a failed scientific idea is supposed to fall by the wayside for some reason have not had an effect.

While string theory's failures have gotten a lot of popular press, the situation is rather different within the physics community. One reason I was interested in publishing the article in Physics Today was that discussion of this issue belongs there, in a place it could get serious attention from within the field. To this day, that has not happened. The story of my article was that I finally did hear back from Lubkin on 2/21/2001. She told me that she would talk to the Physics Today editor Stephen Benka about it. I heard from Benka on 5/6/2001, who told me they wouldn't publish an article like that, but that I should rework it for publication as a shorter letter to the editor. I did this and sent a short letter version back to them, never heard anything back (a few months later I wrote to ask what had happened to my letter, was told they had decided not to publish it, but didn't bother to let me know). In 2002 an editor from American Scientist contacted me about the article, and it ended up getting published there.

Looking back at how Physics Today has covered string theory and related speculation over the past 25 years, I did a search and here's what I found:

- [Reflections on the Fate of Spacetime]() (Witten) (April 1996)
- [String Theory Is Testable, Even Supertestable]() (Kane) (Feb. 1997)
- [Duality, Spacetime and Quantum Mechanics]() (Witten) (May 1997)
- [Large Extra Dimensions: A New Arena for Particle Physics]() (Arkani-Hamed, Dimopoulos, Dvali) (Feb. 2002)
- [Is string theory phenomenologically viable?]()(Gates) (June 2006)
- [The case for extra dimensions]()(Randall) (July 2007)
- [String theory in the era of the Large Hadron Collider]() (Dine) (Dec. 2007)
- [String theory and the real world]() (Kane) (Nov. 2010)
- [What every physicist should know about string theory]() (Witten) (Nov. 2015)

The only thing I could find anywhere during those 25 years indicating to Physics Today readers that none of this speculation had worked out was a short opinion column by Burt Richter

- [Theory in particle physics: Theological speculation versus practical knowledge]() (Richter) (October 2006)

It seems to me that those now in charge of Physics Today should be thinking about this history, their role in it, and what they might be able to do to make up for this heavily one-sided coverage of a controversial issue.

Comments

1. **Alessandro Strumia**
   December 29, 2020

   In 2020 we have webinars. It would be interesting to have a debate between you and Lubos.

2. **André**
   December 29, 2020

   Peter,

   About your last sentence: some people get elected because they lie more and bark louder than others, and some opinions get coverage in the press because the people behind them lie more and bark louder than others. The sad consequence is that in both politics and theoretical particle physics, the US hast lost its claim for leadership. Hopefully, the change on politics will be followed by a similar change in the physics press.

3. **Peter Woit**
   December 29, 2020

   André,
   While there are some analogies between the way tribalism and reality-denying fake news sources have wrecked US democracy and the problems of theoretical physics, I don't think debating that is useful (and I'll suppress further comments trying to do so…).

   Physics Today plays a fairly unique role as the "physics press" in the US. There's not much else in the way of media here aimed at covering physics news for professional physicists. The role it has played in the string theory controversy is rather peculiar. Some of the articles linked to above explicitly say their goal is to answer criticisms of string theory, but they don't explain these criticisms or provide a link to them, and such criticisms have not been allowed to appear in the pages of Physics Today. There appears to have been an editorial decision to only host one side of the argument.

4. **Udi**
December 29, 2020

You complain that people are calling themselves "string theorists", when in practice their work has nothing to do with string theory. But isn't this a good thing? Doesn't it demonstrate that "string theory" is actually not a mono-culture?

5. **Shantanu**
December 29, 2020

Peter : Did you try contacting Physics World (Brit equivalent of Physics today)?
what kind of articles have appeared there?

6. **Peter Woit**
December 29, 2020

Udi,
I don't think training students in string theory and refusing to admit that it's a failed research program, while at the same time moving on to something completely different is a healthy situation.

Shantanu,
The Physics World coverage of string theory has always been more even-handed, and in particular they've covered my criticisms of the theory, see for instance
https://physicsworld.com/a/still-not-even-wrong/

7. **Sabine Hossenfelder**
December 30, 2020

Physics Today last year (2019) published a piece by Gordon Kane in which he argues for a larger particle collider by once again shifting the predicted masses of supersymmetric particles up. Frankly I think it's embarrassing that a scientific magazine would run pseudoscientific "predictions" like this on their pages. I wrote about this here

http://backreaction.blogspot.com/2019/03/particle-physicists-continue-to-spread.html

I also contacted PT about it and said that it would be appropriate if they let someone argue the opposite side. Needless to say, I did not suggest myself, but said if they can't find anyone else I'd be available. They wrote back to say they might do a collider pro-con, then I didn't hear again from them. I asked a few months later what happened to this and got no reply.

The bottom line is that they ran all the false assertions by Kane and never corrected them.

I have since stopped reading Physics Today and have been happier with Physics World.

8. **lol**
December 30, 2020

I've been reading this blog fora while, and never commented. But I guess 20 years is indeed a good time to take stock of what has been accomplished. So lets briefly discuss what has happened in "string theory" (i.e. high energy theory, i.e. whatever theoretical physicists at top places work on) in the last 20 years.

1. A wide range of conformal field theories have been solved numerically using the conformal bootstrap. This has resolved decades old questions about phase transitions in statistical physics and condensed matter, including many ongoing experiments.

2. The AdS/CFT correspondence conjecture, which is the only known non-perturbative definition of quantum gravity, has been rigorously proven in several specific examples (i.e. a certain 2d example, as well as for vector model like CFTs). Also tons of extremely nontrivial evidence has been amassed in pretty much every other well defined example, such as precise derivations of black hole entropy.

3. Dualities between quantum field theories, again many with immediate experimental relevance (such as electrodynamics in 2+1 dimensions and O(N) models) have been proposed and nontrivially tested. These non-supersymmetric, completely physical, dualities were inspired by earlier dualities that involved supersymmetry, and were inspired directly from string theory. This is one particularly beautiful example where idea from string theory and condensed matter have worked together to produce a profound result of great interest to people in both fields.

4. Numerous extremely general theorems about QFT have been proven. One particularly famous example are C theorems (i.e. quantities that are monotonic under RG flow, which is basically the most fundamental aspect of QFT), originally shown in 2d by Zamolodchikov decades ago, which have now been proven rigorously in 3d and 4d.

Note that in these examples, I have avoided any speculative topics (i.e. black hole information, the origins of dark matter, string pheno, etc), since those are more open to debate, and less universally accepted.

I would say by any reasonable standard, this is a very impressive list of accomplishments, certainly more impressive than anything alternatives to string theory have offered (tho in fairness, almost no one works on such alternatives, which might come as a surprise to laymen who only read blogs), and certainly worth the extremely trivial amount of funding that theoretical physics gets (which altogether in the last 20 years probably amounts to less than a standard mid scale experiment).

In sum, I would encourage readers of this blog to retain an open mind. Sure, the standard model has not been derived from string theory, and no one has observed a string, but string theory has continued to profoundly influence numerous fields ranging from pure math to experimental condensed matter physics. This is why everyone is still working on it… not bc of a sinister cabal of the Elders of Physics. Also, no string theorists read Lubos blog anymore, he went over the deep end years ago (and Gordy Kane is like 80 years old? give the guy a break… hes earned the right to say crazy things in his retirement). Unfortunately there are no blogs written by active competent string theorists these days (perhaps bc they are too busy producing results!).

9. **Peter Woit**
December 30, 2020

Sabine,
I wrote about that spectacularly bad Kane piece here
https://www.math.columbia.edu/~woit/wordpress/?p=10881

It's hard to argue with Physics Today's decisions to publish what Witten has to say, but their decision to repeatedly publish articles by Kane containing claims that are outright wrong or that no one except him believes is pretty bizarre.

10. **Alessandro Strumia**
December 30, 2020

Dear LOL, what is very impressive in your list is that you consider it a "very impressive list". The field needs smart young theorists, not those who can be attracted through hype. Why not telling that all predictions about new physics at LHC were wrong, that best hopes for discoveries are now gone, that theorists moved to reconsider a variety of old QFT problems and made some technical progress while waiting that something really very impressive will appear on the horizon?

11. **Peter Woit**
December 30, 2020

lol,
You demonstrate very well exactly the problem: instead of admitting that string theory has failed, you've redefined QFT, or more generally "high energy theory, i.e. whatever theoretical physicists at top places work on" as "string theory". Your justification for this is a list of developments in QFT that have either zero to do with string theory or only a tenuous "inspired by" connection.

Nati Seiberg predicted this back in 2005

https://www.theguardian.com/science/2005/jan/20/science.research

""Most string theorists are very arrogant," says Seiberg with a smile. "If there is something [beyond string theory], we will call it string theory.""

He didn't predict though that what string theorists would discover "beyond string theory" was just QFT.

One problem with this redefinition of string theory as QFT is that it's not done consistently. For instance, the Columbia undergrad course on string theory I believe follows Zwiebach and aims at the light-cone quantization of the string. If "string theory" now means "QFT", why isn't this course instead explaining how to quantize a field (for instance the free EM field, so students could understand what photons really are)? Not everyone has gotten the memo that "string theory" no longer means string theory, and that it now means QFT (or maybe something else, whatever is most convenient for those who don't want to admit that what they used to call "string theory" has been a failure).

12. **Sabine Hossenfelder**
December 30, 2020

lol,

There are so many things wrong with your comment, I don't even know to begin. Let me just highlight two of your most glaring mistakes. First, you seem to believe that AdS/CFT is the only "non-perturbative definition of quantum gravity". Gross. You should stop commenting on things you clearly know nothing about. Also, have you heard that we don't live in Anti de-Sitter space? Second, black hole entropy. Do you realize that no one actually knows what the entropy of a black hole is? So what the heck do you think this calculation shows? As Doyne Farmer put it so nicely, it's "math piled on top of math". Physics is not math. Physics is about describing nature.

But, yeah, thanks for being here to document the problem everybody else is talking about. You have evidently never thought about what you believe. Time to start using your brain.

13. **Apostolos Syropoulos**
December 30, 2020

Someone who works at Columbia University posted on Twitter someting about an interview with Susskind the inventor of the super duper theory of string. I dared to reply that string theory is a failure and he blocked me and send me a direct message asking me if this was a joke or if I am as stupid as I look. I can only imagine what people have told you all these years…

If you think this comment is interesting but do not wan to mention the university just say an American university. If you like I can send you a screen shot of his post

14. **AcademicLurker**
December 30, 2020

My impression, from this blog and elsewhere, is that the formalism of string theory requires a considerable investment of time and effort to understand. Do string theory-ish techniques have applications outside of string theory itself? If not, it seems like having students spend significant time learning it is like having them spend time learning to write calligraphy in elvish. Perhaps the time could be more profitably spent on other subjects.

15. **CFT gal**
December 30, 2020

Dear Sabine,

Can you explain what you think is wrong with lol's statement "AdS/CFT is the only "non-perturbative definition of quantum gravity"? To be honest, lol's comment demonstrates way more expertise in the subject than you do.

16. **Peter Woit**
December 30, 2020

lol/Sabine Hossenfelder/CFT gal,

I'm going to put a stop to this kind of argument about AdS/CFT, since I'm getting flashbacks to years of the String Wars, where endless arguments of this kind shed more heat than light. I will add my own final point, agreeing with Sabine that we don't live in 5d AdS space-time: there is no such thing as a well-defined AdS/CFT-based theory that describes quantum gravity in a universe like ours.

Another aspect of the flashback problem is that I'm reminded of something I really detested about the behavior of string theory partisans during the String Wars: posting anonymously from major physics research institutions, hiding behind anonymity to attack the competence of others and make really dubious arguments. Please don't misuse anonymity to engage in unprofessional conduct.

17. **Peter Woit**
December 30, 2020

Apostolos Syropoulos,
I'd suggest engaging in discussions about string theory on Twitter is even more of a waste of time than most discussions on Twitter, which is a pretty extreme situation.

I noticed that this posting made it to Hacker News, so if you want an unmoderated computer nerd discussion, see
https://news.ycombinator.com/item?id=25573989

18. **lol**
December 30, 2020

Alessandro Strumia,

You raise a good point, that indeed theoretical particle physics is struggling due to the lack of new interesting experimental data, the failed predictions, etc. The questions being considered today clearly are not as groundbreaking as the golden age of the standard model. But there are other interesting questions that can be answered, and I hope my list provided some of them. For instance, the numerical solution to the 3d Ising model phase transition from the conformal bootstrap is no trivial thing, some of the giants of physics (e.g. Onsager) most famous work was on this theory, and its taught us countless deep things about nature (to quote ICP: "****ing magnets, how do they work?"). It is in many ways, the simplest and most fundamental QFT, of interest far beyond its numerous realizations in nature. I would say preferring 4d SU(3)xSU(2)xU(1) gauge theories is a matter of taste, similar to the classic debate between micro and macro (Phil Anderson has a fun essay where he prefers the meso).

Also, considering how incredibly difficult it is to write a consistent theory of quantum gravity, I'm happy with any version at this point (tho of course like everybody it would be great to find a version in 4d flat space without supersymmetry!). There has been lots of interesting work in the last few years about trying to get flat space holography from AdS… but the jury is still out there (its a hard problem!)

Peter,

You also raise a good point, that indeed many of the things called "string theory" today have nothing to do with string theory, and its more sociological that its called that (which is less arrogance of string theory than the fact that people are lazy with labels). But it's worth noting that many of the big accomplishments I mentioned directly come from either the methods developed to understand string theory, or the theory itself. E.g. most intro string theory classes like the you mention spend half the semester teaching just 2d CFT methods. These are the exact same methods that are used to study some of the most cutting edge theories in condensed matter (like the work of Nicholas Read, who ironically works at a place that is no fan of string theory in general), but most of them were developed by people mostly motivated by string theory (e.g. sasha polyakov, one of the most underappreciated [by the public] and profound physicists of the latter 20th century). Also, the stuff about the 3d dualities, thats very trendy in condensed matter these days, was directly inspired by analogous supersymmteric 3d dualities derived from string theory (and found by a former string theorist now full-time condensed matter theorist Dam Son). Some people even claim you can derive the new condensed matter ones from the supersymmetric one.

Sabine,
[*Juvenile 3 letter response deleted*]

19. **Peter Woit**
December 30, 2020

Academic Lurker,
Yes, string theory is a highly technical subject for students to learn, much of this technology not very generally useful. I'd argue the most useful is the 2d conformal field theory part of a course on string theory, but this is quite a demanding subject, best learned on its own, without the extra baggage it comes with when used in string theory.

The undergrad-level courses I'm aware of use Zwiebach and I believe don't discuss conformal field theory, just try and quantize the string in light-cone gauge. This technology is pretty much useless for anything else.

20. **Peter Woit**
December 30, 2020

lol,
About flat space AdS/CFT: very smart people have been trying to do this not just in "the past few years", but from 1997 on. After 23 years, I think the jury is in on that one.
The 3d Ising problem is a great problem in mathematical physics, with theorists trying to solve it using strings back before my student's parents were born. All in all, if high energy theorists want to give up on high energy theory and instead go work in condensed matter physics, good for them. Will be interesting to see how they do competing for grants, jobs, etc. in that field. And what the tolerance level among condensed matter experts for hype is…

21. **Naive Postdoc**
December 30, 2020

Dear Peter,

I unfortunately think that the hype is already present (though to a lower degree) in theoretical condensed matter physics, particularly in the field of symmetry protected topological phases of matter (SPT) and I don't think they have a problem with grant money.

There's a reason why so many string theorists and string mathematicians such as Witten, Kapustin, Gaiotto, Freed, Hopkins, Johnson-Freyd among many others are working on this topic and have, in my naive opinion, brought a similar attitude to the one they have in HEP as a community. Let me not expound my accusations too much and only say that they use emergence as a wild card and the worrisome thing is that there are almost no SPT experiments, though much more likely to be in the near future than in HEP, but in the meantime speculation reigns.

22. **Alessandro Strumia**
December 31, 2020

Dear LOL, I agree that this work might indirectly lead to useful tools to understand physics of fundamental relevance. Still, computing the brachistochrone is just the optimal way of spending theorists time, while the main line of development stagnates. The difference between 4d SU(3) x SU(2) x U(1) and QFTs for magnets* is discussed in a footnote of Weinberg QFT book:

«* This section lies somewhat out of the main line of development and may be omitted in a first reading».

23. **BootsTrap**
December 31, 2020

Lol, it is strange to see numerical bootstrap on the list and even at the top. These CFT's were understood long ago and various perturbative schemes to compute are available, e.g. the famous 4-epsilon expansion. The recent attempts were about putting some old equations on a computer and present nothing new, just another numerical gadget to add few digits to the already known exponents. That it became somewhat popular is due to a low entrance fee – even a student could put these things on a computer and get some plots for a paper. The 3d non-SUSY dualities are exciting, as well as the a-theorem, and sphere free energy but have nothing to do with strings and are not even related to them.

24. **tomate**
December 31, 2020

I was then a bachelor student, but already interested in getting a picture of what contemporary physics was all about. I remember finding this paper in the stack of unclaimed papers on the side of the printer in the computer room. I read it with curiosity, happy that I would have some arguments I could hardly understand to give myself a posture in my chats with my senior pals. It contents stuck with me for quite some time, until I had to take decisions on my future, and this paper was a big part of my choices, in a physics department were if you were a HEP theorist there was no choice apart from strings and branes.

25. **Peter Shor**
December 31, 2020

@Naive Postdoc: Symmetry protected topological phases of matter is not an area which physicists have worked on for 40 years with very little to show for it.

Theoretical physics tends to have fads, with lots of people working in the popular areas, and new areas tend to get overhyped. The worrying thing is that string theory is *still* overhyped after people have spent 40 years on it with very little return, and many of the researchers have given up on doing active research in it and are working in other fields.

26. **Naive Postdoc**
December 31, 2020

Dear Peter (Shor),

I totally agree with you that the case in string theory is much worse. I was addressing a particular issue raised by Peter (Woit) that we would see how much of their hype was tolerated by condensed matter physicists and how much money they would get … My point was that perhaps condensed matter physicists are more tolerant than he thinks, SPT and the string theorist's move towards it being the case and point. On the whole, the influence of string theory may have permeated other branches and their subcultures deeper than we're aware.

27. **lol**
December 31, 2020

BootsTrap,

I'm not sure what you mean by "These CFT's were understood long ago". Most interesting CFTs are strongly coupled, which means that just writing down some Lagrangian isn't very helpful (and of course many interesting CFTs don't even have Lagrangians, especially in higher dimensions, such as the 6d (2,0) theory), bc Feynman diagrams teach you nothing about them. Things like epsilon expansions give some intuition, but they are totally nonrigorous and don't always work (for the obvious reason that epsilon=1 is not a good expansion parameter). The big question in formal QFT for the last 30 years or so is how to find non-perturbative ways of understanding QFTs (eg CFTs), and most experts in both condensed matter and particle physics acknowledge the numerical bootstrap to be one of the biggest breakthroughs in that regard. The point is that the progress is not just computing a few more digits of accuracy, its conceptually having a way to define and compute things in theories for which no non-perturbative method was known before (except Monte Carlo methods in some of the simplest cases, but those don't give very much physical intuition, and are limited to the most simple quantities in the theory). For instance, you can read this writeup in a condensed matter journal here (https://www.condmatjclub.org/uploads/2020/01/JCCM_January_2020_02.pdf), which discusses recent experiments (involving a spaceship!) that bootstrap results have influenced.

Also, the reason i mentioned 3d non-SUSY dualities and the a-theorem, is precisely bc they were derived using cutting edge CFT and QFT techniques that were developed by string theorists. In particular, the dilaton effective action (which is how the a-theorem was proved) and mirror symmetry (which is how the supersymmetric version of the 3d dualities were proven, which strongly influenced the non-susy dualities. mirror symmetry of course has had a profound influence on pure mathematics as well).

Finally, on a sociological note, it's always hard to judge an extremely technical fields as an outsider (even a very smart one, say a prominent scientist in an adjacent field). I think a good rule of thumb is to see if the people working in that field have produced results that have influenced many other fields, in addition to things just of interest to their field. For instance, the giants of string theory (like Witten, Moore, Polyakov, Maldacena, Gross etc), even if they had done absolutely nothing for string theory, would still be famous due to their numerous profound contributions to cosmology, condensed matter, pure math, etc, many of which have had direct experimental repercussions (I can give you a long list for each of these people if you want, but you could prob just go on Inspire and see for yourself). This is why people take them seriously, not just bc quantum gravity is trendy, or silly hype. Conversely, if you come up with your own theory of quantum gravity, but haven't already done anything profound in other less speculative fields, and if you're new methods aren't useful for anything outside what you're doing, then you shouldn't be insulted if people don't take you that seriously (even if you write lots of popular books to convince laymen that theres a conspiracy against you). A good analogy might be CERN: you might know nothing about what particles are, but you'll certainly appreciate the world wide web that was invented as a byproduct. The point is that good lines of research inevitably produce useful byproducts (since all correct things are eventually connected in theoretical physics), whereas bad lines of research don't. After all, they are Not Even Wrong 😉

28. **yaron**
December 31, 2020

@Peter Shor: Indeed, physicists have not worked on topological phases for 40 years with little return, *yet*. But how long should one wait?

From yours and Native postdoc's comments it seems we share some discomfort from the situation in string theory, but the question is what lesson do we apply to other research programs. Since people made nice careers in strings (40 years is probably around average from graduate school to pension) it seems there is less risk in following the hype than trying something new, even after one sees the hype is false.

29. **Avals Tsetrot**
December 31, 2020

I can offer a reason why most high energy theorists comment on this blog anonymously. It's because not many people are willing to admit that they, looking for a distraction and in a moment of weakness , come to this blog to waste some time reading this incompetent dirt. Here we go, let's see if the blog's owner will have the balls to let this comment through, I bet he won't.

[*Attacks on others deleted*]

30. **Avals Tsetrot**
December 31, 2020

Since the owner deleted a part of my comment which he qualified as an attack on the others, let me try to waste a bit more of my time and rewrite it in a less polemic form.

First of all I do not believe it's a job of string theorists to predict new physics at the LHC, rather it is the job of hep-ph community, some of whose most illustrious representatives have deigned to comment in the above thread. Hep-ph people, not string theorists, apply for and win Advanced ERC grants to predict signals of new physics at the LHC.

Second, there is no shortage of funding for alternative theories of quantum gravity, as long as they have a sliver of hope to be successful. One example is the Horava theory of Lorentz violating gravity http://arxiv.org/abs/0901.3775 (BTW Horava is a string theorist), whose ramifications have been investigated vigorously. This activity was supported e.g. by the CERN theory department (which at some point hired two junior staff members working on it http://arxiv.org/abs/1410.2408). Higher-derivative gravity is also under active investigation in spite that it is not perturbatively unitary, and one has to jump through all sorts of hoops to pretend that perhaps in some pickwickian sense it is. This activity has been supported by at least one Advanced ERC grant http://arxiv.org/abs/1705.03896.

31. **Peter Woit**
December 31, 2020

lol,
I'm actually very sympathetic to the program of exploiting conformal symmetry in 3d QFT, but have a different perspective on it, and see motivating it by string theory as seriously misguided.

You write:
"The big question in formal QFT for the last 30 years or so is how to find non-perturbative ways of understanding QFTs (eg CFTs)"
What you're missing is that this began not 30 years ago, but 47 years ago, with the 1973 discovery of asymptotic freedom and realization that the correct theory of the strong interactions was QCD, strongly coupled in the infrared. By the time I got to grad school in 1979, for quite a few years everyone at Princeton had been focused on non-perturbative QFT, aiming at understanding QCD. A lot of effort was going into studying other QFTs as toy models and everyone was spending their time studying what the condensed matter people knew about such QFTs. My thesis work was Monte-Carlo computer simulations aimed at QCD, but I began with testing out methods on some stat-mech models such as the 2d XY model (the simplest model with interesting topological features).

The current interest in strongly-coupled stat-mech models among ex-string theorists looks to me like not a development inspired by string theory, but a return to what everyone was working on in the early 1980s, before they abandoned it in 1984 for 10d superstrings and complex 3d algebraic geometry (because Witten and others convinced them that all one needed to unify physics was to find the right Calabi-Yau).

Of course there has been a lot learned since 1984 about non-perturbative QFT. I share your fascination with the question of how to exploit conformal symmetry in QFTs above dimension 2. In particular, I've been spending a lot of time looking at what Rychkov and others have done by studying the very non-trivial relations between the Euclidean and Lorentzian signature situations in 3d. I differ though I suspect in believing that string theory is worse than useless here. While some theorists may have gotten to this problem by starting at string theory and AdS/CFT, trying to understand 3d QFT by compactifying 10d superstrings is likely to at best tell you something highly obscure about uninteresting models.

Where my point of view is completely different is that I'm not ultimately interested in condensed matter, but in fundamental physical laws, so care about 4d, not 3d. One of the main motivations of the twistor ideas I'm excited about is that twistors provide a compelling way to get 4d conformal symmetry. What I see as new has to do with the relations between the Euclidean and Minkowski conformally invariant theories, but in 4d, not 3d.

So, I'd be happy to see a campaign to promote the study of conformal symmetry in QFT as the guiding principle (or even fad) in the subject. But to get students and others interested in this, starting by teaching them about 10d superstrings and AdS/CFT is a mistake, even if this is convenient for those who don't want to admit failure of their research programs.

32. **Peter Woit**
December 31, 2020

Avals Tsetrot,

"I do not believe it's a job of string theorists to predict new physics at the LHC"
The problem is that string theorists have given up not only on predicting new physics at the LHC, but on predicting new physics anywhere at any energy, as well as giving up at explaining anything about fundamental physics.

The current problem is not that research on string theory is still dominant after 40 years, it's that leading string theorists have given up on fundamental physics, trashing the subject on the way out with crap like the landscape, and continuing to try and convince people that their failure was not actually a failure.

33. **BootsTrap**
December 31, 2020

Lol, imagine that on a cold morning in 1984 Belavin-Polyakov-Zamolodchkov publish a paper where they put the 2d bootstrap equations on an average computer available back then and observe a kink close to the expected position of the 2d Ising model. This does not sound very exciting as compared to what they actually wrote. This could have generated couple of hundreds papers more… One thing it would not had led to it is understanding of the beauty behind 2d CFT's. Well, the same is with the 3d numerical bootstrap. It cannot even prove that these CFT's exist as the technique gives you 'exclusion plots' and adding one more operator can kill the solution. It does not lead to any understanding that would be comparable to BPZ. Just another technique to get more digits. Yes, it was popular some time ago, just because it was very easy to write a paper and we all need papers. The SUSY dualities have not helped to prove the non-SUSY 3d dualities. 3d and 4d generalizations of the c-theorem do not require any knowledge of string theory, as is clear from the proofs.

34. **Slava Rychkov**
December 31, 2020

Hi BootsTrap,

let me tell you a bit about the history of conformal bootstrap. When I gave the first talk at the IAS about the conformal bootstrap revival back in 2008, I was challenged by noone else but my PhD advisor Sasha Polyakov who did not believe at the time the equations I was using were applicable to higher dimensional CFTs. So much for the fact that conformal bootstrappers are using well-known equations, if one of the CFT classics did not outright agree with those equations 🙂

Also I challenge you to write a few more papers like e.g. the recent bootstrap work http://arxiv.org/abs/2011.14647 if you believe that any grad student can nowadays write a valuable bootstrap paper. You have to be a pretty damn smart grad student to master all the theory which goes into the conformal bootstrap AND develop an efficient numerical algorithm to put all that theory to good use. Good luck.

Finally, if you don't like numerical work, I do encourage you to look for analytical understanding of the numerical bootstrap bounds (which are rigorous bounds, mind you, even if computed numerically). Then you will become an equal of BPZ. It should be easier to understand analytically something which you know exists, shouldn't it? So here is a nice challenge for you and everyone else, a challenge which did not exist before the numerical bootstrap work. You may wish to familiarize yourself with the work of Dalimil Mazac who did find analytical proofs of some numerical bootstrap bounds.

The point being, before the numerical bootstrap, most people were skeptical that 3d critical phenomena would one day be accessible to the CFT techniques, and now nobody can doubt that.

35. **Dalimil**
December 31, 2020

BootsTrap,
what BPZ understood in 1984 are extremely special and simple examples of CFTs, namely the unitary ones in d=2 with central charge between 0 and 1. This leaves out a vast landscape of CFTs, namely irrational 2d CFTs (with central charge above 1), and all CFTs in d>2. There is currently no explicit solution of any interacting example of either kind but it is clear that if there is, it would be orders of magnitude more complicated and interesting than what BPZ accomplished. What the modern bootstrap has shown is that the CFT axioms are powerful enough to plausibly single out such theories, giving us some hope that one day somebody smart will find a solution of either an irrational 2d CFT, or an interacting CFT in d>2. Furthermore, from this point of view, the current understanding of 2d CFTs is almost as rudimentary as that of d>2 CFTs.

36. **TS**
December 31, 2020

It is not a new thing that "phenomenology" is thought of as being outside of "theory" but to see this as blatantly as in lol's comment and the following discussion is rare. All the amazing progress in precision calculations that accompanied the LHC era? Didn't happen, or isn't "theoretical physics"!

Maybe people should consider whether they actually believe that physics is about the real world when they can't even be bothered to acknowledge the progress that is being made in predicting experimental results in the world's largest lab.

37. **Alessandro Strumia**
January 1, 2021

Dear "Avals",

you don't believe that "it's a job of string theorists to predict new physics at the LHC", but in past decades lot of activity in string theory had been done assuming that there is one vacuum and two scales: the Planck/string scale, and the SUSY/weak -breaking scale (while the vacuum energy vanishes for reasons to be clarified in the future). Then one constructs string models with N=1 SUSY to be discovered at LHC, focusing on Calabi-Yau compactifications, etc, etc.

Unfortunately, whoever wrote laws of nature had not good taste or was not as smart as string theorists. The vacuum energy was found to be positive, and no SUSY was found at LHC. In the string context, this might mean that there is a huge number of vacua with SUSY broken at the string scale.

If now you Aesopically like to think that you never cared about LHC, you miss what made physics more interesting than abstract theory: the pleasure of being slapped by experimentalists, hoping that a kick in the back will point to a better theoretical direction than keeping dragging the aether or retracting to pure math.

PS: Peter, don't worry about attacks. To keep physics serious we need people like the P-dual of "avals", who bluntly say what they think, rather than what is polite and convenient.

38. **André**
January 1, 2021

Peter, Avals,

it might seem that the job on any theorist working on high energy physics is to explain why there are three particle generations, where the gauge groups come from, where the particle mass hierarchy comes from, and where the coupling constants come from.

It might also seem that most proposed theories, including string theory, where not really that successful in this endeavor, despite the help of a lot of brainpower.

Avals, are you working on these questions? If not: Are you honestly and really saying that string theory will solve these questions?

39. **BootsTrap**
January 1, 2021

Dear Lol, Slava and Dalimil, I hope that most people agree that an analytical understanding is much better than a (yet another) numerical approach (even if it is the most computationally efficient one at present). There can be two versions of BPZ: economy class: with the help of a computer BPZ draw a plot with a kink in the neighborhood of 2d Ising model. First class: what they actually done. It does not solve all 2d theories, but solves the important ones, and it advances greatly our understanding. I guess we know what is the 3d economy class version of that. Instead of yet another paper that improves the numerics I would recommend to read some of the recent analytic bootstrap papers (for example, Alday and pals who might give us the first class version of the 2d story in the future). Perhaps, one should create hep-th-numerics for people who gave up. No offense, the better numerics for 3d Ising are useful, but I would not put it on the list at all, which is where this debate has started. Dalimil, I disagree that the numerical bootstrap has shown anything apart from adding more digits and it is nice that you moved to analytical methods: the CFT existed before, the kinks that finally turned into the islands are due to 3d Ising-type theories having actions, equations of motions etc, which singles them out of the continuum of 'theories' – they are disconnected from the continuum and we knew that.

40. **Slava Rychkov**
January 1, 2021

Hi BootsTrap,

what a biased perspective on the conformal bootstrap past and present history! So many fertile exchanges exist in the conformal bootstrap community between the numerical and analytical methods! The numerical bootstrap people did not give up, on the contrary, by 2008, it's the analytical people who long gave up the hope to access strongly coupled non-supersymmetric CFTs via the conformal bootstrap equations. Paraphrasing you, they could not afford a first class ticket and so they refused to fly instead of trying out the economy. The economy salon was pretty cushy, and totally empty, when we tried it. 🙂

Thanks to the numerical bootstrap, this hope is now back and, to my joy, it stimulates a huge amount of analytical thinking which would not otherwise exist. The numerical work showed a window of opportunity, and afterwards analytically-minded, intellectually curious, and very smart, people decided to take a look at what is going on, not the other way around.

As to the fact that numerical bootstrap "just adds more digits", I do like to stress that it does so with rigorous error bars, unlike resummed perturbation theory and Monte Carlo simulations, where error estimation is often an art rather than science. This is pretty unique: in fact there is hardly any other method in quantum field theory which has this property. It gives bootstrap results the status of a final verdict in cases when one has to decide the fate of borderline cases, and more conventional techniques disagree. By now we have several experimentally relevant applications of this unique role of the bootstrap.

But OK, it's not like we need everyone's approval. In fact your opinion is not uncommon, my ENS colleague Volodya Kazakov likes to bug me with very similar remarks, while I bug him back saying that it's his job as an expert in integrable models, not mine, to figure out if the 3d Ising CFT is integrable and solve it. 🙂

There are many real physicists out there struggling to understand concrete condensed matter systems. They know the value of a practical method when they see it, and it's them who I consider the primary bootstrap audience.

Good buy!

41. **Dalimil**
January 1, 2021

Bootstrap,

Your comparison of the modern bootstrap with BPZ is substance-free because solving CFTs in d>2 (or irrational 2d CFTs) is a very different (and apparently much more difficult) mathematical problem than solving rational 2d CFTs. So your dismissal of the recent advances is a bit like if a mathematician were dismissing the importance of partial results on the location of zeros of the Riemann zeta function by arguing that we know the location of roots of quadratic polynomials since many years ago.

Of course, an exact analytical solution of a d>2 CFT would be optimal, but in its absence, if I were to decide whether it's better to have an analytic answer to 5 orders in perturbation theory (with unknown errors) or a numerical answer with rigorous error bars of size 10^{-5}, I'd probably choose the latter.

Perhaps the most important point though is that the modern bootstrap advances have made interacting conformal field theory in general dimension into totally rigorous fields of mathematics. It's true that axiomatic approaches to QFT exist since the 1950s, but it was understood only from 2008 onwards that the CFT axioms are powerful enough to determine dynamical data. In principle, you can now explain these axioms to a mathematician who can then start proving rigorous theorems about interacting CFTs and thus also nonperturbative quantum gravity in AdS — quite remarkable if you ask me.

42. **Peter Woit**
January 1, 2021

Alessandro Strumia,
I agree that we need more professional theorists who say what they think, even if it's unpleasant. What I object to and don't want to tolerate here is abuse of anonymity.

Trying to think about what Physics Today should be doing (as opposed to what it has done), what's needed is not so much articles by people like me or Sabine Hossenfelder answering the kind of articles they have been publishing. Instead, what's needed is a different kind of article, with an honest discussion of what the problems are and what experts think about them. Instead we get these highly political one-sided arguments (which don't even acknowledge explicitly the other side). A good start might be for Physics Today to go to the authors of the articles I linked to, along with others who over the years have argued for heavily promoted speculative ideas that have now been disconfirmed, and ask them to honestly address the current situation.

43. **Peter Woit**
January 1, 2021

André,
In general I don't think that theorists need to be addressing directly experimental results or fundamental open issues in the theory, since most of the time they won't have anything useful to contribute if they try and do that. Better to work on possibly more obscure and technical things that are not understood, where progress is possible and might someday inspire a new idea that can help with fundamental issues. But fundamental open issues are not going to ever get solved if people misleadingly tell themselves and others that they have alread been solved (or have been shown to be insoluble).

44. **Lee Smolin**
January 1, 2021

Dear LOL,

I agree that your four points are an impressive list of accomplishments, of whom the many who contributed should be rightly proud. But in the generations who entered physics in the 1940s to 1970s there are a number of individuals who each had a lasting impact on science greater than your whole list, while addressing both theoretical and experimental issues: Polyakov, Wilson, Feynman, Gell-Mann, Weinburg. Anderson, 't Hooft, Dyson, And then think that they knew Einstein, Bohr, Born, Heisenberg, Schrodinger, Dirac…

May I gently suggest that those of us who aspire to argue about the future of physics might, after 20 years, include humbleness on our list of New Years resolutions?

Best wishes for the new year,

Lee

45. **Alessandro Strumia**
January 2, 2021

Physics so far kept attracting best people, so it's possible that LOL & co are as smart as Polyakov & co, and that the real difference is that old generations did the easier important jobs, leaving the harder less important jobs to current generations. This might have now contributed to avoiding open discussions about difficulties and to hyping partial achievements. But the real end of physics will come if it will stop attracting smart "un-humble" persons.

46. **lun**
January 2, 2021

Alessandro, your comment is pretty telling on many levels.
The "top talent" drift from theoretical physics to various incarnations of data science and computer science has been going on for decades. The fudamental problems theoretical physicsts are struggling with now, far from being "less important", are are precisely the ones the likes of Einstein, Maxwell and their ilk struggled with: unification of all forces, quantization of gravity, microscopic understanding of entropy and so on. Its just that the current "elite" has been making a lot less progress in their resolution while feeling much more self-important about it.
Then again, one big problem with "meritocracy" is that self-selected elites who believe they are the best and only people like them are the best is that intellectual incest and group-think, to the point of blatant blindness, follow naturally. We see this all over theoretical physics today, it is part of the problem with it.

47. **Fabien BESNARD**
January 2, 2021

lun, I'd like to concur with you using the words of Roger Penrose : I once heard him say that there are two types of mathematicians, smart ones and dumb ones, and that he fell in the second category. I think the problem of today's theoretical physics is that there are too many smart physicists.

48. **Peter Woit**
January 2, 2021

Some thoughts on comments above:

I continue to see very smart young people wanting to go into fundamental theoretical physics. The best go to get Ph.Ds at the same sort of elite programs where I was educated. What I don't see is papers with impressive new ideas from young theorists. This is not their fault: their elders are not coming up with anything either, and the way in the past you often got breakthroughs was young people working not in a vacuum, but set in a promising direction by their advisors or others around them.

The combination of few new ideas, tribalism and a celebrity culture has led to more examples of huge rewards and attention for not so great (or even wrong) work, with people labeled as "geniuses" and collecting $3 million on very dubious grounds. A screwed-up rewards structure carries serious implications for the ability of a field to make real progress.

As for "humility", that's definitely needed and missing in terms of people not admitting that an idea has failed, or has produced only very modest results. On the other hand, a fair amount of arrogance is needed to believe that one can make a breakthrough and be willing to bet one's time and energy on it. So, we need more of both humility and arrogance…

49. **lol**
January 2, 2021

Dear Lee, Peter, etc,

Happy new year!

I think we actually agree on a lot more than than the stereotypes would assume. None of us think string theory has solved the deepest fundamental issues, none of us like overhyped results (especially when the scientists are to blame, not just the science journalists), we all seem to appreciate some nice results from the last 20 years (even if they aren't as groundbreaking as those from the golden age of physics), and that progress in technical nonsexy things (like computing Ising model critical exponents to a few more digits) is still a worthwhile and honorable thing for scientists to do, even if it wont explain the hierarchy problem or solve global warming.

One last thing I would try to emphasize tho, is that while a few famous string theorists at well funded institutions might well give off the impression of great arrogance in their accomplishments, for most of us, we're working for very low pay, for many many years, and certainly not basking in the glow of media attention. Speaking personally, I have chosen anonymity on this forum not bc I'm secretly Ed Witten in disguise (he seems too busy tweeting about Israel these days lol), but bc i'm just a lowly postdoc at a non-American institution with very poor job prospects, making probably the equivalent of minimum wage in the US, and I don't want any inadvertently foolish thing that I say online to hurt my already dismal prospects. For people like me (who are the vast majority of string theorists, high energy theorists, formal theorists, whatever), we do what we do out of love for science, fully aware of the huge experimental and theoretical problems with our field, out of a perhaps quixotic dream that perhaps we will stumble upon a solution, or at the very least make substantial, if not groundbreaking, progress on worthy technical and sometimes conceptual issues.

The main reason I spoke out was simply to convince laymen (or non-string theory scientists) reading this blog that despite what they may have been told, we are making steady progress in some directions (maybe not the sexiest ones), and that we would appreciate to continue receiving our paltry salaries, which seem to decrease by the year. My main concern is that excessive negative press will defund not just those doing extremely speculative work (which I do think also deserves some funding, tho Im not the right one to defend them), but also the 90% of us doing humble meaningful work, and contributing to the results I listed.

Perhaps going into the new year, in addition to decrying excessive hype, we could vow to also promote humble progress?

50. **Alessandro Strumia**
January 2, 2021

Dear lun,

bibliometric data show that fundamental theory was concentrated in a few main centers up to ≈30 years ago. Next, internet made research diffused. Access to internet become enough to find articles and books and tools, and follow courses and webinars. A very strong young physicist can now get a safe position somewhere even outside main centers. Actually, IAS seems to me the last concentration of excellence. Recently we also got a diffusion in the topics that are being explored. This should be enough to bypass sociological problems.

I fear that stagnation on big issues mostly happened because these are difficult problems.

51. **AcademicLurker**
January 2, 2021

On another blog, people were answering a "What should I keep in mind when applying to graduate school?" question from a physics student who seemed interested primarily in theory.

One respondent, who identified themselves as being at a "top" department said that perfect test scores, perfect grades and undergraduate research experience weren't enough to winnow the field of candidates, so the big deciding factor was the number of papers published as an undergrad. I don't know if this person was exaggerating or not.

No doubt the requirements described select for people who are very good at what they do, but they likely also select for a very specific type, when maybe some variety is what's needed (Lee Smolin addresses something like this point in his book).

52. **Peter Woit**
January 2, 2021

lol,
Thanks for your comments. I appreciate that especially non-tenured theorists have a very good reason to comment anonymously. When I started the blog I was thinking that anonymity was necessary for younger people critical of string theory to be able to speak without fear of retribution, soon realized that young string theory enthusiasts also had reason to be fearful (a lot of the physics community has never been sympathetic to string theory). The tribalistic aspects of this controversy are disturbing.

A lot of what people worry about does have to do with funding and with the always precarious job situation (by the way, the job situation was just as bad if not worse before string theory became popular). Many people seem to react to the yearly multi-million dollars in prize money from Milner/Zuckerberg by arguing that it's money going to theorists, so good. I'd love to see pressure put on the Breakthrough Prize people to change what they're doing to fund modestly paid permanent positions for theorists, rather than give huge amounts to people who already have a permanent position.

53. **Peter Woit**
January 2, 2021

AcademicLurker,

I think one thing that is true is that there has been significant grade inflation over the years, so having a transcript with a string of As isn't worth what it once was. This is not good for the unusually talented, who now need to find other ways to distinguish themselves.

One problematic aspect of string theory for a long time has been that ambitious young people are told that it's "the best hope for a unified theory" or some such, so naturally try to study the subject. But, it's a very technical business and working your way through Polchinski requires a huge investment of time and energy, arguably a really bad way for the best young theorists to be getting trained. A modest proposal would be that physics departments should serious considering getting rid of any courses teaching supersymmetric field theory and string theory. What students are learning in these courses is not very useful, and giving them a warped perspective as they try and enter the field and become researchers in their own right.

54. **Richard**
January 2, 2021

Peter,

What about https://physics.aps.org/ ?

How do they compare to PT and PW with regards to the presentation of string theory and other speculations?

55. **Peter Woit**
January 2, 2021

Richard,
That's kind of a different sort of publication, essentially news articles based on new papers. There's not a lot about string theory there, because not a lot of new developments. Physics Today also does news stories (as opposed to the kind of promotional feature articles I was counting and linking to), and relatively few of those have been about string theory. Actually, an editor at Physics Today wrote a blog entry

https://physicstoday.scitation.org/do/10.1063/PT.5.010017/full/

responding to someone asking why there were so few news stories about string theory (he didn't address the sort of problems I'm raising).

56. **Douglas Natelson**
January 3, 2021

AcademicLurker, I think that's a major overstatement. It is true that undergraduate (co)authorship is something that gets noticed in admissions, but smart people know that some of that is a matter of access (e.g., students who can't do REU programs over the summer and may come from smaller schools just might not ever have a credible opportunity to be on a paper as an undergrad. That doesn't imply something about their ability to succeed at the doctoral level.).

57. **martibal**
January 3, 2021

Regarding the lack of humbleness, the problem may not be so much in the attitude of postdocs or young researchers than in the mind of whom is in position to hire other people. And sadly this is not limited to string theory.

Some 20 years ago, when a renowned center for theoretical physics was founded, it seemed easier to be hired there as a postdoc by claiming great ideas about foundations of quantum mechanics or quantum gravity, than by presenting humble calculations on, say, the metric interpretation of the Higgs field in noncommutative geometry.
I remember some (non-stringy) conferences on quantum gravity with highly non-humble plenary talks about some models – a little bit more evolved that Ising-1D – that were presented as bright new paths towards quantum gravity just because a potential in 1/r popped out. When someone asked "how do you go from this to full gravity", the answer was "well yes, the technical details are still to be worked out".

Humbleness is not a value appreciated by hiring committees. In a french maths department, I was once asked what I was hoping for the future of mathematical physics. I answered "more humbleness". I thought this was a smart answer. I learnt later on that the committee did not appreciate it at all.

As a last example, some years ago, french CNRS edited some guideline to apply to ERC grant (the main european funding agency). One of the main advice was to use key-words like "breakthrough", "multidisciplinary", "high-risk, high-gain" and – my favorite one – "to be at the cutting edge of knowledge". Quite far from humbleness…

58. **CWJ**
January 4, 2021

martibal,

The irony is, in my experience in the US at least, is that reviewers of proposals (and papers) are generally extremely conservative and tend to strongly prefer well-trodden paths. This is independent of what the funding agencies call for. For example, NSF proposals explicitly must include some component that addresses a broader social good (this isn't the exact wording), but typically reviewers hate anything more than 'train the next generation.' Anything the slightest bit of out-of-the-ordinary outreach will get slammed as a waste of time. That goes for the scientific part of the proposal as well–even though the funding agencies ask for 'innovative' scientific visions, in practice most reviewers are harsh towards anything save incrementalism. Maybe other people's experience is different…

59. **Ash Jogalekar**
January 4, 2021

As a chemist who has always enjoyed your blog tremendously, I have the following question. What do you think about the argument that string theory, even if it may not lead to productive ends in itself, provides excellent training in physics and mathematics that would set up students for careers in other fields?

The reason I ask is because it reminds me of the field of "total synthesis" of organic molecules in chemistry where graduate students spend five or six years undertaking very arduous, risky syntheses of complex organic molecules, many of which don't have an obvious use. The argument has often been made that while the field itself provides little benefits, it's an excellent training ground for students because it exposes them to multiple techniques, ideas and arguments in organic chemistry that are hard to find in other subfields.

60. **Peter Woit**

January 5, 2021

Ash Joglekar,
The problem is that how to calculate in string theory is an extremely specialized subject, which doesn't have a lot of overlap with more useful fields. The really weird thing that is going on is that there's also little overlap between what theory students are getting trained in (for example, the content of Polchinski's two-volume textbook) and mainstream current theoretical research by "string theorists". I'm not sure what function studying Polchinski now fulfills, other than as a tribal rite of passage.

DAU. String Theory

January 4, 2021
Categories: Uncategorized

I first wrote here in 2015 about DAU, the unusual film project based to some extent on the life of Landau. Parts of the film first were shown in Paris early in 2019, and this past year started appearing on the DAU website. I'd been looking forward to seeing Gross, Yau, Rovelli and others in the film, so paid to watch one of the first parts, DAU. Degeneration, when it became available last year. It's over six hours long, for a review, see here. I ended up doing a certain amount of fast-forwarding, was disappointed to only see Nikita Nekrasov and Dmitri Kaledin, none of the other math/physics world figures I had heard had participated.

DAU largely was funded by Russian oligarch Sergei Adoniev. For an excellent article discussing the project and its context in current Russian culture, see Sophie Pinkham's article Nihilism for Oligarchs.

There wasn't much physics in DAU. Degneration, but evidently it plays a significant role in other parts of the film. According to the DAU website,

> Real-life scientists, who were able to continue with their research in the Institute, included: physicist Andrei Losev; mathematicians Dmitri Kaledin and Shing-Tung Yau; string theorist Nikita Nekrasov; Nobel-Prize winning physicist David Gross; neuroscientist James Fallon; and biochemist Luc Bigé. "One group was researching string theory and another researching quantum gravity. These groups hated each other. One stated there were 12 dimensions, the other claimed there were 24. The string theory group believed there couldn't be 24 dimensions. The quantum gravity group believed that the other scientists were narrow-minded," explained Khrzhanovskiy.

Now available is a part which seems to more centrally involve physics, DAU. String Theory, which is described as follows:

> Nikita Nekrasov is a scientist, a theoretical physicist who studies our world and other possible worlds. He refuses to make a choice between mathematics and physics, between one woman and another, as he ponders the existence of the multi-universe. At scientific conferences, attended by eminent foreign scientists and a rising younger generation of physicists alike, Nekrasov gets carried away debating the beauty of string theory. He attempts to explain to all of his women – Katya, the librarian, Zoya, the scientific secretary, Svetalana, the head of department – about the theory of his own polygamy, and the possibility of having enough feelings to satisfy everyone.

Multiple universes have always been advertised with "in some other universe you're dating Scarlett Johansson", relating the idea to multiple partners in this universe is an innovation.

I haven't yet watched DAU. String Theory, will likely find time for that soon. I'm worried that I'll still not get to see Gross, Yau, Rovelli and others though, and lack the time and energy to look through all the other parts of the film. I'd like to crowd-source a solution to this problem: if anyone watching these things can let the rest of us know in which parts (at what times) well-known math/physics personalities appear, that would be greatly appreciated.

Comments

1. **Alessandro Strumia**
   January 4, 2021

   In case you watch the film, please let me know if somebody impersonated the academic mob that in 1937 attacked in this way their colleagues in the Landau UPTI institute who got accidentally involved in Stalinist purges:

   "Vile agents of fascism, Trotskyist-Bukharinist spies and saboteurs … Enemies penetrated among physicists, carrying out espionage and sabotage assignments in our research institutes … Soviet physicists more closely unite around the Communist party and Soviet government, around our great leader Comrade Stalin". (Source: https://arxiv.org/abs/1508.03578, page 22)

2. **Julie**
   January 5, 2021

   Peter,
   Gross and Verlinde appear at ~56 min.

   You can watch free at
   https://filmix.co/drama/143039-dau-teoriya-strun-2020.html

   Although its in Russian, the relevant parts are in English.

   The protagonist mostly speaks to his women. I can't even start to explain all the things that are wrong with it. Haven't expected anything else from a production by a Russian oligarch though.

3. **Peter Woit**
   January 5, 2021

   Julie,
   Thanks! For me though the site you link to gives an error
   "Video blocked at the request of the copyright holder"
   At some point I'll invest the \$3 to pay to watch this.

4. **Low Math, Meekly Interacting**
   January 5, 2021

   I'm a little leary, in spite the modest investment. The Degeneration review makes the project sound like a mashup of David Cronenberg and Seymore Butts on PCP. That said, I'm not altogether unsympathetic to the notion of slogging through to see what such luminaries could possibly be doing as characters in such a production. Julie, is it anything interesting, at least?

5. **Nikolas Claussen**
   January 5, 2021

I went to visit the DAU exhibition when it was on show in Paris almost two years ago; it is my impression that the whole thing is basically a fraud, or at least, behind all the hype, pretty hollow. When I arrived at the exhibition, a friend and I were refused entry on account of an invalid reservation number although we had bought the tickets from an authorized seller – never saw my money again. Two other friends who had bought tickets elsewhere did manage to see the exhibition which was rather underwhelming, resembling a collection of pop-up bars (you could get drunk, at least) with a few shock elements (e.g. pornographic films). In different rooms, various, mostly unrelated film snippets were shown and there were a few actors wandering through the corridors. Although booked out according to the website, most rooms where conspicuously empty. No comparison to the kind of immersive, concept-art exhibitions you can see at the Palais de Tokyo, for example.

6. **Peter Woit**
   January 5, 2021

   LMMI,
   DAU. String Theory sounds a bit more promising than DAU. Degeneration, the one I saw and that most of the reviews are about. For one thing, DAU. String Theory is less than half as long, for another, Julie reports that string theory celebrities are in it, unlike DAU. Degeneration, where all you get is Nekrasov.

   The reviews of DAU. Degeneration are also somewhat misleading in promising pornography, violence and other outrageous conduct. From what I remember it was mostly long sequences of murkily lit scenes of people talking and not much happening. Long scenes of people drinking and behaving badly. If you're hoping for David Cronenberg you'll be disappointed. On the other hand, if you're a fan of Dogme film-making, you might be interested. A lot of the scandal revolves around reports that the non-professional actors were coerced into participating in some of the sexual behavior and drunken violence that one sees on-screen. As a spectator though one can't tell what the truth of that might be.

7. **Julie**
   January 6, 2021

   To be honest, I didn't had the patience to watch it all. The twitching of a male ego.

   There are pictures of physicists in dark suits before black boards, wearing hats, walking the streets, Gross smoking a cigar, sitting together in their trousers with braces, drinking alkohol – Yau and Rovelli sit at the table, where young women serve food and drinks. Later some slow dancing.
   Nekrassov calles himself a polygamist – multiple disciplines (physics and math), multiple countries, cities, and – of course – multiple wives. The main message: why should I choose anything (anyone), if I can have it (them) all. Which is a rather typical stringy message which has brought much damage to the perception of this community (see e.g. B. Greene, the main prophet).

   Low Math, Meekly Interacting, no, not really. But that depends of course on your general degree of enthusiasm for a nerdy guy who is uncovering the very hard and important secrets of the universe and his twitching ego. And since it's ought to be a piece of art, the interest is to be found in the eye of the beholder, right?

8. **Alex**
   January 10, 2021

   DAU is a bad project. It was funded by money stolen from russian people.
   Its screenplay is made to denigrade human dignity.
   And "DAU. Degeneration" specially features russian neo-nazi Maxim Martsinkevich (https://en.wikipedia.org/wiki/Maxim_Martsinkevich).

9. **D. K.**
   January 11, 2021

   it's a macho project that unwillingly uncovers the backwarded macho mentality within math and physics and it is not even in any sense irony. Painful.

Martin Veltman 1931-2021

January 6, 2021
Categories: Obituaries

I heard today of the recent death of Martin Veltman, a theorist largely responsible (with his student Gerard 't Hooft) for showing the renormalizability of non-abelian gauge theories, a breakthrough crucial to the Standard Model that won both of the them the 1999 Nobel Prize. For the story of this work, the best source is likely Veltman's Nobel lecture.

My one memory of meeting Veltman in person was when he visited Stony Brook at the time that I was a postdoc there (mid 1980s). There was a party at someone's house, and I spent part of the evening talking to him then. What most struck me was his great passion for whatever it was we were talking about. One topic I remember was the computer algebra program Schoonschip (which Wolfram acknowledges as an inspiration for Mathematica). I vaguely recall that at that time Veltman had recently ported the program to a microprocessor and he was selling copies in some form. It also seems to me that one remarkable aspect of the program was that it was written in assembly language, not compiled from a higher level language. At the time I was doing computer calculations, but of a very different kind (lattice gauge theory Monte-Carlos). Since my own interests were focused on non-perturbative calculations, I wasn't paying much attention to Veltman's work, although I do remember finding his Diagrammar document (written with 't Hooft) quite fascinating.

A comment that evening that really struck me was about students, in particular that "you give your students your life-blood!". This seemed likely to have some reference to Veltman's relations with his ex-student 't Hooft, but I'm pretty sure I didn't quiz him on that topic.

Many years later, when I was trying to get *Not Even Wrong* published, I contacted Veltman and he was quite helpful. At the time he had recently published his own popular book about particle physics, Facts and Mysteries in Elementary Particles, which contained his own version of the Not Even Wrong critique:

> The reader may ask why in this book string theory and supersymmetry have not been discussed. . . The fact is that this book is about physics and this implies that theoretical ideas must be supported by experimental facts. Neither supersymmetry nor string theory satisfy this criterion. They are figments of the theoretical mind. To quote Pauli, they are not even wrong. They have no place here.

That book is quite good, I strongly recommend it. May its author rest in peace.

Comments

1. **rhofmann**
   January 7, 2021

   A giant of theoretical physics and a true loss to the community and to me personally. I spent several hours chatting with Tini and his wife when they visited Heidelberg in 2004 which was a wonderfully natural and insightful experience. Squeezing himself out of our small 3-door car, he smilingly told me that next time the car needs to be bigger. Undoubtedly sharing my warm sentiments for him with many physicists of our time, I do stand in awe of his intellect, unadulturated convictions, and accomplishments in establishing the gauge principle for the description of fundamental particle interactions.
   You'll be missed greatly, Tini!

2. **shantanu**
   January 7, 2021

   He also never believed in dark matter. I don't know much about his relationship with T'hooft. Can you elucidate on that?

3. **David Brown**
   January 7, 2021

   In 2010, Roger Highfield, science editor of "The Telegraph", published several comments from theoretical physicists concerning the LHC start-up.
   "Martin Veltman, Nobel Laureate University of Utrecht, Netherlands: 'It would not surprise me if the experimenters don't find the Higgs particle. I don't trust the theory behind it. But if it does appear to show up, it will be crucial to check that it behaves as the theory predicts. I would be surprised if supersymmetry were found. I supported the idea when it was first suggested, but I've gradually lost confidence in it, though I might well be wrong. To be sure, if the LHC finds nothing to support supersymmetry, its advocates will just make excuses and keep using it. As for string theory, it's all mumbo jumbo, with no connection with experiment.' "
   https://cds.cern.ch/record/1195875/files/PRESSCUT-V-2009-3073.txt

4. **Peter Woit**
   January 7, 2021

   Shantanu,
   For a very old blog posting about Veltman and some of his views, see
   https://www.math.columbia.edu/~woit/wordpress/?p=106

   About Veltman/'t Hooft, I've heard little second hand, and first hand all I know is what I quoted from Veltman in the posting, and comments by 't Hooft at the Coleman memorial conference, see
   https://www.math.columbia.edu/~woit/wordpress/?p=171

5. **Arnold Neumaier**
   January 7, 2021

   Peter,

   "his own popular book about particle physics, Facts and Mysteries of Elementary Particles […] That book is quite good, I strongly recommend it."

   In contrast to you, I strongly unrecommend it. It is very misleading in giving virtual particles an appearance of reality that is not even slightly backed by theory or experiment. See https://www.physicsforums.com/insights/vacuum-fluctuation-myth/ and https://www.physicsforums.com/insights/misconceptions-virtual-particles/

   In contrast, his book "Diagrammatica: the path to Feynman diagrams"
   https://www.cambridge.org/nu/academic/subjects/physics/theoretical-physics-and-mathematical-physics/diagrammatica-path-feynman-diagrams is a textbook on quantum field theory covering the standard ground up to QED in an essentially standard manner. This technically precise (though not mathematically rigorous) book contains not a single mention of the word 'virtual'. Instead, Veldman explains here in an exemplary way what Feynman diagrams are and how they are interpreted and used. From the

introduction:

”Perturbation theory means Feynman diagrams. It appears therefore that anyone working in elementary particle physics, experimentalist or theorist, needs to know about these objects. […] This then is the aim: to make it clear which principles are behind the rules, and to define clearly the calculational details. This requires some kind of derivation. The method used is basically the canonical formalism, but anything that is not strictly necessary has been cut out. No one should have an excuse not understanding this book. Knowing about ordinary non-relativistic quantum mechanics and classical relativity one should be able to understand the reasoning.”

6. **David**
January 7, 2021

There is a statement on Lubos' blog by 't Hooft:

https://motls.blogspot.com/2005/03/sidneyfest.html,

"Oeps, in my comparison of Sidney's fast and brilliant mind with that of my advisor Veltman, I must have left a false impression of my admiration of Veltman. I was making jokes about him (much as how he would do that himself), but please be assured that he is brilliant in his own way, as his richly deserved Nobel Prize testifies."

7. **Peter Woit**
January 7, 2021

David,
Thanks. You can also read there Lubos's description of the 't Hooft talk, which starts with "Before the talk, I had roughly 20 seconds to chat with Peter Woit."…

8. **David**
January 7, 2021

You can also watch the talk here: https://youtu.be/7CLAFRTGZjY

9. **Peter Woit**
January 7, 2021

David,
Thanks. The references to Veltman are in the first part of the talk.

10. **Peter Woit**
January 8, 2021

All,
Sorry, but I don't want to host a debate here about "virtual particles".

11. **Peter Orland**
January 9, 2021

I remember Veltman giving a talk on problems with the Higgs mechanism in Brighton at a big meeting 1983. I had a job in London, and went down there with some students and other postdocs.

The beginning of the talk was interesting, but after 15 minutes I was fast asleep. Veltman's voice was very gravelly and hypnotic; the perfect background noise to facilitate a nap. When I heard the applause at the end, I was very embarrassed by my faux pas. As people asked questions, I looked around and noticed that a few other physicists were still slumped in their chairs.

I had to find out what the talk was about from the proceedings.

12. **Deepak Aryal**
January 11, 2021

Veltman gave a very entertaining talk titled "The future of particle physics" at 2019 Lindau Physics Meeting (Especially the Q&A part). Here is the video: https://www.mediatheque.lindau-nobel.org/videos/38224/2019-meeting-martinus-veltman

13. **Low Math, Meekly Interacting**
January 12, 2021

Interesting to hear he and John Bell were such good friends. CERN has some photos to remind us…

https://cds.cern.ch/record/764869?ln=en

14. **Francesco Vissani**
January 18, 2021

Thank Peter for remembering the 2003 book and his brilliant statement.

I had the great fortune to meet him at the end of my PhD (1993) in Zakopane and had the opportunity to discuss with him and learn many things. (As well as drinking a beer and discussing late into the night with him and other young people). One of those persons who gives you the desire to do better than you are capable of doing.

If I remember correctly, a few years later (1996) his contract was terminated. I got the impression that the reason was not particularly noble or profound, he was 65, but simply that in the place he was in, criticism of supersymmetry was not well tolerated. In 1999 fortunately there was the recognition we all know.

An important issue for him was the status of the cosmological constant; he also talked about it in the Nobel lecture. I have tried to raise this issue with others by talking about supersymmetry, but I have always seen very little interest. The typical response was: you don't understand, this is not relevant. Surely I do not understand a lot of things, but I hold that Veltman understood a lot. Thank you Tini.

15. **David Roberts**
January 22, 2021

I met Veltman briefly in 2001 at a public lecture I went to, that he gave at the University of Adelaide (I was a first-year physics student. It was the 26th of April, I find, looking back). I vaguely recall my mum asked a question at the end and he came over to hear it more clearly and give us an answer. I was starstruck meeting a Nobel laureate, and I thought he was really old then! The night also stuck out to me because I learned (from the late Rod Crewther) how to pronounce Lagrangian correctly, as I had only ever seen it written in books.

I still have a very nicely written and illustrated booklet from the night ("Very elementary particle physics") explaining the basics of the subject, which Veltman wrote.

Various Links

January 13, 2021
Categories: Uncategorized

Our semester at Columbia started earlier than usual this year, with first classes this week, my first class yesterday. This semester I'm teaching the second half of a year-long course on the mathematics of quantum mechanics. There's a Youtube channel with the lectures for the first half of the course, and now also for the second half. The course is largely following the textbook I wrote based on teaching this is earlier years. The first lecture yesterday was a summary of a point of view on canonical quantization explained in the first semester and in the book. This point of view is essentially that Hamiltonian mechanics is based on a Lie algebra (functions on phase space with Poisson bracket the Lie bracket), and canonical quantization is all about the essentially unique unitary representation of (a subalgebra of) that Lie algebra. On Thursday I'll start on the fermionic version of canonical quantization, which has a very much parallel structure, giving a super-Lie algebra and spinors.

A few other items:

- John Baez's This Week's Finds in Mathematical Physics was an unprecedented project conducted over 17 years, providing a wealth of fantastic expository material on topics in math and physics. It started in 1993, and on its twentieth anniversary I wrote an appreciation (in an appropriate font) here. John has now announced that this material has been typeset (2610 pages!) and he is editing it, to be released in batches. The first part is now available, on the arXiv as This Week's Finds in Mathematical Physics (1-50). As I find time, I'm looking forward to reading through these, encourage everyone interested in math and physics to do the same.
- Frank Wilczek has a new book out, and there's an interview with him at Quanta. You can see a conversation between him and Brian Greene here on Friday.
- Another physicist with a new book is Jesper Grimstrup, whose Shell Beach: The search for the final theory I've just finished reading and enjoyed greatly. The book is quite personal and non-technical, with topic Grimstrup's life as a theorist pursuing a unified theory. His career story is quite interesting, giving insight into the ways academic theoretical physics is challenging for young theorists trying to pursue non-mainstream research programs. Several books have appeared in recent years aimed at putting this kind of physics research in a human and philosophical context, telling you what it has to do with the meaning of life. There's some of that in this book too, of a much more compelling sort than what you see elsewhere. Grimstrup has a website here, and in recent years has ended up leaving academia and trying to fund his research with donations. I can think of a lot worse things you could do with your money than send him some.

  I'm quite sympathetic to the underlying theme that he describes pursuing (together with Johannes Aastrup) in the book, that of bringing together the insights of loop quantum gravity and non-commutative geometry. More recently they've been working on some new ideas for formulating QFT non-perturbatively that seem worth investigating. There's a survey blog post here.

Update: Another bit of private math/physics funding news. The IAS has announced establishment of the Carl P. Feinberg Cross-Disciplinary Program in Innovation

> Scientific research at the Institute is traditionally driven by the collaboration and independent projects of a full-time Faculty and a revolving class of more than 200 researchers at various stages in their careers. The Carl P. Feinberg Cross-Disciplinary Program in Innovation will build on this successful model with the recruitment of mid-career scholars who have pioneered foundational developments in new areas. Bringing together scholars with such unique insights—which may not be obviously connected to the existing themes of the past 20 or 30 or 40 years—ensures that IAS will remain agile and responsive to new intellectual developments that do not yet fit the mold of what graduate students and postdocs generally know. In order to close this knowledge gap, the program will feature intense, focused workshops and "master classes."
>
> "Since its founding, the Institute has served as a world center for investigations into the fundamental laws of nature. We are currently in the middle of a grand symbiosis of ideas, from the equations of general relativity to the quantum information of black holes," stated Robbert Dijkgraaf, IAS Director and Leon Levy Professor. "This revolutionary program will provide a dedicated space and the necessary flexibility to accelerate these exciting developments, and will surely forge new connections across fields."

Comments

1. **Peter Morgan**
   January 13, 2021

   And another interview with Wilczek, doubtless another of many, with Brian Keating, here, https://www.youtube.com/watch?v=prJjVUuDhvg

2. **Jeff**
   January 15, 2021

   Congrats to John Baez! Might there be any way to get a "top ten finds" for each year, or even just overall? Thank you!

3. **Ilyas Khan**
   January 15, 2021

   Peter thank you for highlighting Jesper's book. I remember the first call for assistance and how pleased I was to help. The book is a lovely "dividend"

4. **Felipe Pait**
   January 23, 2021

   Thanks to your suggestion, I'm reading "Shell Beach." Can you suggest a good book on non-commutative geometry? I'm an engineer who knows some differential and Riemannian geometry, would prefer a reference that brings out the intuition but is not too much geared towards specific problems in physics.

5. **Peter Woit**
   January 23, 2021

   Felipe Pait,
   Alain Connes has written various inspirational survey articles and given lectures that give a good introduction to the subject. Many are at his website alainconnes.org
   Have a look at
   http://alainconnes.org/en/downloads.php
   especially the section on survey papers.

   Among his talks, there was a very recent survey one at a Harvard seminar, see
   the first one listed under
   http://alainconnes.org/en/videos.php
   but you might find many of the others valuable.

Is the Standard Model Just an Effective Field Theory?

January 13, 2021
Categories: Uncategorized

An article by Steven Weinberg entitled On the Development of Effective Field Theory appeared on the arXiv last night. It's based on a talk he gave in September, surveys the history of effective field theories and argues for what I'd call the "SM is just a low energy approximation" point of view on fundamental physics. I've always found this point of view quite problematic, and think that it's at the root of the sad state of particle theory these days. That Weinberg gives a clear and detailed version of the argument makes this a good opportunity to look at it carefully.

A lot of Weinberg's article is devoted to history, especially the history of the late 60s-early 70s current algebra and phenomenological Lagrangian theory of pions. We now understand this subject as a low energy effective theory for the true theory (QCD), in which the basic fields are quarks and gluons, not the pion fields of the effective theory. The effective theory is largely determined by the approximate SU(2) x SU(2) chiral flavor symmetry of QCD. It's a non-linear sigma model, so non-renormalizable. The non-renormalizability does not make the theory useless, it just means that as you go to higher and higher energies, more possible terms in the effective Lagrangian need to be taken into account, introducing more and more undetermined parameters into the theory. Weinberg interprets this as indicating that the right way to understand the non-renormalizability problem of quantum gravity is that the GR Lagrangian is just an effective theory.

So far I'm with him, but where I part ways is his extrapolation to the idea that all QFTs, in particular the SM, are just effective field theories:

> The Standard Model, we now see – we being, let me say, me and a lot of other people – as a low-energy approximation to a fundamental theory about which we know very little. And low energy means energies much less than some extremely high energy scale 10^{15}–10^{18} GeV.

Weinberg goes on to give an interesting discussion of his general view of QFT, which evolved during the pre-SM period of the 1960s, when the conventional wisdom was that QFTs could not be fundamental theories (since they did not seem capable of describing strong interactions).

I was a student in one of Weinberg's graduate classes at Harvard on gauge theory (roughly, volume II of his three-volume textbook). For me though, the most formative experience of my student years was working on lattice gauge theory calculations. On the lattice one fixes the theory at the lattice cut-off scale, and what is difficult is extrapolating to large distance behavior. The large distance behavior is completely insensitive to putting in more terms in the cut-off scale Lagrangian. This is the exact opposite of the non-renormalizable theory problem: as you go to short distances you don't get more terms and more parameters, instead all but one term gets killed off. Because of this, pure QCD actually has no free parameters: there's only one, and its choice depends on your choice of distance units (Sidney Coleman liked to call this dimensional transvestitism).

The deep lesson I came out of graduate school with is that the asymptotically free part of the SM (yes, the Higgs sector and the U(1) are a different issue) is exactly what you want a fundamental theory to look like at short distances. I've thus never been able to understand the argument that Weinberg makes that at short distances a fundamental theory should be something very different. An additional big problem with Weinberg's argument is its practical implications: with no experiments at these short distances, if you throw away the class of theories that you know work at those distances you have nothing to go on. Now fundamental physics is all just a big unresolvable mystery. The "SM is just a low-energy approximation" point of view fit very well with string theory unification, but we're now living with how that turned out: a pseudo-scientific ideology that short distance physics is unknowable, random and anthropically determined.

In Weinberg's article he does give arguments for why the "SM just a low-energy approximation" point of view makes predictions and can be checked. They are:

- There should be baryon number violating terms of order $(E/M)^2$. The problem with this of course is that no one has ever observed baryon number violation.
- There should be lepton number violating terms of order E/M, "and they apparently have been discovered, in the form of neutrino masses." The problem with this is that it's not really true. One can easily get neutrino masses by extending the SM to include right-handed neutrinos and Dirac masses, no lepton number violation. You only get non-renormalizable terms and lepton number violation when you try to get masses using just left-handed neutrinos.

He does acknowledge that there's a problem with the "SM just a low-energy approximation to a theory with energy scale M=10^{15}–10^{18} GeV" point of view: it implies the well-known "naturalness" or "fine-tuning" problems. The cosmological constant and Higgs mass scale should be up at the energy scale M, not the values we observe. This is why people are upset at the failure of "naturalness": it indicates the failure not just of specific models, but of the point of view that Weinberg is advocating, which has now dominated the subject for decades.

As a parenthetical remark, I've today seen news stories here and here about the failure to find supersymmetry at the LHC. At least one influential theorist still thinks SUSY is our best hope:

> Arkani-Hamed views split supersymmetry as the most promising theory given current data.

Most theorists though think split supersymmetry is unpromising since it doesn't solve the problem created by the point of view Weinberg advocates. For instance:

> "My number-one priority is to solve the Higgs problem, and I don't see that split supersymmetry solves that problem," Peskin says.

On the issue of quantum gravity, my formative years left me with a different interpretation of the story Weinberg tells about the non-renormalizable effective low-energy theory of pions. This got solved not by giving up on QFT, but by finding a QFT valid at arbitrarily short distances, based on different fundamental variables and different short distance dynamics. By analogy, one needs a standard QFT to quantize gravity, just with different fundamental variables and different short distance dynamics. Yes, I know that no one has yet figured out a convincing way to do this, but that doesn't imply it can't be done.

Update: I just noticed that Cliff Burgess's new book Introduction to Effective Field Theory is available online at Cambridge University Press. Chapter 9 gives a more detailed version of the same kind of arguments that Weinberg is making, as well as explaining how the the Higgs and CC are in conflict with the effective field theory view. His overall evaluation of the case
"Much about the model carries the whiff of a low energy limit" isn't very compelling when you start comparing this smell to that of the proposals (SUSY/string theory) for what the SM is supposed to be a low energy limit of.

Comments

1. **Sabine Hossenfelder**
   January 14, 2021

   I am just here to give my usual speech: The "naturalness" problems in the standard model are not scientific problems. They are aesthetic problems. They come about because physicists claim an unobservable number that temporarily appears in the math is "unlikely".

There are two problems with this. First, the debate about the supposed singularity at black hole horizons should have taught physicists that fretting about non-observable issues in mathematical calculations is a waste of time. Second, one can't speak about probabilities without probability distributions, and we will never be able to obtain an empirically supported probability distribution over unobservable parameters*.

Add to this that "naturalness" arguments haven't worked with the axion (the original one), haven't worked with the cosmological constant, haven't worked with supersymmetry.

(The charm quark prediction btw wasn't a naturalness argument, it was a good old-fashioned argument from Occam's razor. It's just that people at the time used the word "natural" in their argument.)

The bottom line is, naturalness should go out of the window.

For what the SM is concerned, well, it doesn't contain gravity, so of course the short-distance physics isn't fundamentally the right one.

—
* Same problem with multiverse arguments.

2. **Mitchell Porter**
January 14, 2021

I'm trying to understand what the central thesis of this post is.

On the one hand, we seem to have the suggestion that the fundamental theory really could be just a QFT which consists of the standard model coupled to a new form of gravity. Well, it's a striking idea, and one always has the 2009 prediction of the Higgs boson mass by Shaposhnikov and Wetterich, that was premised on exactly such an assumption.

On the other hand, there is the criticism of naturalness, on the grounds that naturalness predicts new weak-scale particles and they haven't turned up.

I guess I don't understand whether this constitutes grounds for criticizing the very idea of treating the SM as an effective field theory. Yes, if it's SM all the way to the Planck scale, then the SM is not just an EFT. But that's an extremely bold hypothesis that may or may not be true.

Meanwhile, for now, it's surely legitimate to still be interested in the possibility of new particles or forces. Is it claimed that EFT is a wrong way to model this or a wrong way to be systematic about it?

If naturalness is a bad guide, to me that implies, not that the use of EFT is overall misguided, but that you need to be ready for the coefficients to be larger or smaller than expected.

3. **Warren Siegel**
January 14, 2021

Lattice QCD is the exact opposite of what you describe: It's exactly because of the ambiguity in the action that you have "non-universality". This shows up when trying to take the continuum, or small-coupling, limit. This is a result of the running of the coupling (asymptotic freedom), & shows up in arguments for analyticity in the complex coupling-constant plane (e.g., see 't Hooft). It's also related to the inability of constructive quantum field theorists to prove the existence of theories that aren't @ least superrenormalizable (besides the instanton problem, also a difficulty on the lattice). These problems also show up in (resummation of) perturbation theory, as renormalons, which require (an infinite number of) new couplings as energies rise (like nonrenormalizable theories), appearing as vacuum values of (color-singlet) composite operators. (Thus, contrary to the belief of loop quantum gravity people, lattices don't eliminate problems seen in perturbation theory.)

The only known solution is (perturbatively) finite theories, which require something you hate — supersymmetry.

4. **Peter Orland**
January 14, 2021

Hi Warren,

If I understand you (and perhaps I don't) lattice QCD doesn't suffer from any of the problems you describe (at least according to conventional wisdom. Not theorems). The ambiguities you mention concern semiclassical methods (perturbative, with resummation, summing over saddles points).

It is expected (but not proved) that the only real couplings where physical quantities are not analytic are 1. zero (at $\theta=0$) and 2. some nonzero value (at $\theta=\pi$).

QCD is (presumably) a completely finite theory, with very few parameters (one if there are no quarks and $\theta=0$).

5. **Peter Orland**
January 14, 2021

Just to be technically precise, there can be other nonanalyticities, but these are not universal and depend on irrelevant terms in the lattice action.

6. **lun**
January 14, 2021

Sabine, actually this post explains clearly why naturalness is not an esthetic argument but a mathematical one.
QFT is formulated in terms of functional integrals, which are in general divergent and irresolvable. However,
(i) they can be expanded, in various ways: perturbing around the coupling constant (perturbation theory), around scale separations (EFTs), saddle points and so on.
(ii) The divergence can also be regulated in such a way that it does not affect physical quantities, which are generally correlators measured at a certain scale, provided we use some experimental data as input, the bare minimum (which as Peter explains is sufficient for QCD) is the scale.

The most used expansion that includes (i) and (ii) will have a few normalizeable and super-renormalizeable terms (where there are no factors dependent on scale separation) and infinitely many non-renormalizeable and trivial ones that go away if the scale separation is large enough (this last point is where people most often use natuarlness arguments).

Saying axions, ccs, Higgs mass etc are not natural is a shorthand for saying
that if you take this theory and try to do the functional integral with (i) and (ii) in mind
(i) and (ii) do not quite work for these observables. That is a rigorous mathematical statement about a physical theory, which might indicate problem with the theory, or problems with the approximation (see the discussion after your comment). But it is not just aesthetic. If would be aesthetic if we realized Feynmans dream

——————————————————
https://arxiv.org/abs/2006.08594 "This makes me dream,
or speculate, that maybe there is some way, and we are
just missing it, of evaluating the path integral directly"
———————————————————-
and STILL had to use naturalness. But we did not as yet, so its a legitimate maths question.

7. **Peter Woit**
January 14, 2021

Cliff Burgess here
https://twitter.com/CburgesCliff/status/1349579929462198273
characterizes this by
"Very early 70s take on things in that blog post, it seems"

Not quite right, since until 1973 and asymptotic freedom the consensus was that QFT was no good at short distances, or for describing strong interactions. From about 1974 -1984, it's true that the point of view of this posting may have been the dominant one. Post 1984 things went back to "QFT no good at short distances (need string theory)" and that's been the prejudice the past 30 years. I think the failure of the field to make any progress during this period argues for going back to before the wrong turn.

One aspect of the history of the subject is that it was only for a short ten year period (1974-1984) that grad students entering the field were being told that maybe QFT works at all distance scales. At every other point during the nearly 100 year history of QFT they've been told it fails at high energy.

8. **Peter Woit**
January 14, 2021

Michell Porter,
Nothing wrong with EFT. Lots of QFTs are EFTs. All QFTs used in condensed matter are EFTs. I don't think though that the idea that the SM is not just an EFT is an "extremely bold hypothesis". All expected failures of the theory based on the idea that it's just an EFT have not worked out. It agrees with all experimental results and (modulo Higgs +U(1) problems) seems to make perfectly good sense at all distance scales. It should be the baseline conjecture that it can describe all distance scales, until someone comes up with something better. No one has.

9. **Peter Woit**
January 14, 2021

Warren Siegel,
I won't try and argue that issues of the perturbation and semi-classical approximations to a putative rigorous non-perturbative lattice-regularized QCD are well-understood. But all evidence I'm aware of is that (keeping things simple) lattice pure gauge theory is a well-defined non-perturbative theory, with expected infrared and ultraviolet behavior if you take the continuum limit appropriately. And when you do this, the limit is insensitive to the definition at the cutoff scale as I stated.

In any case, even if you can find a problem with this, the larger point is that this is as close as there is to a well-defined 4d theory that makes sense at short distance scales and gives behavior we observe in nature. For no well-defined X is there any evidence that "we should be doing X instead of QFT at short distances".

10. **Warren Siegel**
January 15, 2021

@ Peter & Peter:

't Hooft's arguments are based entirely on the running of the coupling, & are not tied to perturbation theory. But the same problems are seen in perturbation theory, & in constructive quantum field theory, which is rigorous & entirely nonperturbative.

Lattice QCD is an alternative approach to perturbation theory that provides results for different observables. But it does not avoid any of the fundamental problems of the theory. The conclusion is that QCD is a low-energy effective theory. Of course, the problem is worse with the Standard Model because of the U(1) gauge group, which is seen to be a problem even @ finite coupling on the lattice. (Problems with asymptotically free couplings show up only near vanishing coupling, as proven by Tomboulis, but in agreement with 't Hooft's argument.)

11. **Peter Orland**
January 15, 2021

Hi Warren,

But 'tHooft's arguments are based on renormalons/instantons. Although these don't emerge from perturbation theory per se, they are not pure running-coupling constant arguments.

He also had a program for constructing large-N theories in four dimensions, but I don't know what came of this (maybe this is what you mean by constructive FT).

By the way, there is a long paper by Magnon, Rivasseu and Seneor, written almost 30 years ago, claiming a construction of SU(2) Yang-Mills in 4 Euclidean dimensions, in finite volume (not the infinite volume limit). If correct, this work goes a long way towards overcoming the problems you raise; in a finite volume, all the same features are present.

12. **Peter Orland**
January 15, 2021

… and Peter Woit will tell us soon to shut up and conduct this technical discussion elsewhere.

13. **Peter Woit**
January 15, 2021

Peter Orland,
No, technical discussions that are relevant to the topic are encouraged! The question of whether QCD is just an effective theory or not is highly relevant.

Warren Siegel,
My understanding is that there is good evidence (much of it numerical) for the conjecture of the existence (at all distance scales) of a well-defined non-perturbative version of QCD, as specified precisely in the Millenium prize document

https://www.claymath.org/sites/default/files/yangmills.pdf
If Tomboulis or anyone else has a solid argument for non-existence (can you give a reference?), they should be putting in their claim for the \$ 1 million. My suspicion is that what you're discussing is a different problem involving the subtleties of the perturbation expansion or semi-classical approximation for QCD, for which I'm willing to believe there are all sorts of issues.

14. **Thomas**
January 15, 2021

I think the misunderstanding in Warren Siegel's comment is this:
"Lattice QCD is an alternative approach to perturbation theory that provides results for different observables."

The lattice can of course be used as a regulator in perturbation theory, and this theory has the sames issues as weak coupling perturbation theory using other regulators. (The lattice has a convergent strong coupling expansion, albeit with a finite radius of convergence). However, the main point is that the lattice provides a fully non-perturbative definition of the theory. There is indeed no proof, but we have strong physical arguments, and plenty of numerical evidence, that the continuum and infinite volume limits exist, and that they define the theory we observe in nature.

This means that QCD is a perfect theory, one that can be extended to arbitrarily short distances. But in practice this does not help, because QCD is embedded into electroweak theory, and we have an equally strong expectation (and, again, numerical evidence) that the \$U(1)\$ and scalar sectors cannot be extended to arbitrarily short distances. This means that the SM, even without gravity, must be viewed as an EFT.

What is maybe somewhat unusual is that the only estimate of the breakdown scale that we have right now is from RG running of the scalar sector. It would be nice to directly observe a higher dimension operator. It is possible that neutrino mass comes from a higher dimension operator, but until we observe a Majorana mass we don't know (which is why double beta decay experiments are so important).

15. **Peter Woit**
January 15, 2021

Thomas,

To be clear, since the SU(3) and SU(2) gauge theories are asymptotically free, the remaining problem is the U(1) and Higgs. The U(1) "Landau pole" problem is at scales way above the Planck scale. The Higgs problem is intriguing, indicating borderline instability up near the Planck scale.

I agree that the SM cannot just stand on its own, ultimately one wants to unify it with a quantum theory of gravity. To me, the simplest scenario would be a unification with a gravity QFT, that would resolve the issues of the high energy limit of the U(1) and the Higgs. Then, yes, the SM decoupled from gravity would not be fully consistent, but on the other hand characterizing it as just a low energy effective approximation would be misleading (since to a large degree the theory would work at all distance scales).

16. **Peter Orland**
January 15, 2021

Lun,

Although it is true that renormalizable "unnatural" theories are not fully consistent at high energies, those energies are EXTREMELY high.

For example, QED is unnatural, but its cut-off cannot be predicted from the theory itself. If we didn't know about SU(2)\$\times\$U(1), we would not know where (below \$10^19\;eV\$) QED breaks down.

In this sense, Sabine is completely correct. There is no ability to predict where the theory breaks down, and naturalness is an attempt to do the impossible (to predict what cannot be predicted). We only know that such theories must break down, below some very large momentum.

17. **Arnold Neumaier**
January 15, 2021

That a QFT has a Landau pole is a problem only for approaches that regulate the theory with a noncovariant cutoff. This includes lattice approximation.

However, a Landau pole is not a problem for covariant regularizations.

In particular, in causal perturbation theory, the renormalization is done covariantly at an arbitrarily chosen renormalization energy parameter. The theory is well-defined for any choice far away from the Landau pole and, by the Peterman-Stückelberg renormalization group, is independent of this choice — only the quality of approximation depends on the choice.
The Landau pole only says that one cannot choose the renormalization energy parameter close to the pole without getting meaninglessly inaccurate results.
(This is unlike renormalization through cutoffs, where the covariant theory is only obtained in a limit, a process that suffers from a UV induced Landau pole.)

Relevant in this context is also the not widely known fact that QCD has like QED a Landau pole. But while for QED the UV induced Landau pole is at physically inaccessible energies (far larger than the Planck energy), the IR induced QCD Landau pole is at physically realized energies! Nevertheless the pole does not invalidate predictions at these energies. Thus arguing for inconsistency based on Landau poles is a relic from ancient times where QFT was not yet well enough understood.

For further details see my article on Causal perturbation theory at https://www.physicsforums.com/insights/causal-perturbation-theory/
and the discussion at https://www.physicsoverflow.org/32752/ and
https://www.physicsoverflow.org/21328/

18. **Peter Orland**
January 15, 2021

The Landau pole is an artifact of the one-loop (or finite loop) approximation. The pole is present in a region of momentum space where this approximation can't be used.

I don't think it is helpful to think of triviality/unnaturalness in connection with Landau poles. In Wilson's discussion (see Sections 12 and 13 of Wilson and Kogut's Physics Reports article on the renormalization group) concerning (non)triviality, the Landau pole is not mentioned at all. Corrections to Landau's mean field theory (an entirely different development) and Ginzburg's criterion are, however, relevant.

19. **lun**
January 15, 2021

Peter Orland, for sure "naturalness" is not a very useful tool to understand what exactly is wrong (the last decades of theoretical physics have conclusively shown this).

Rather, lack of naturalness is a symptom of a problem, and its wrong to say the problem is aesthetic, it is mathematical consistency rather then aesthetics.

20. **JE**
January 15, 2021

I cannot add much to the technicalities some of you have quite accurately exposed regarding the shortcomings of QFTs in general, the SM in particular, and most notably QG. To me, wondering whether or not the SM is just an EFT pertains more to psychological aspects of the scientific process than to the purely physical or mathematical ones. The SM very successfully predicts almost all (if not all) results we can currently measure. We know that there are some mathematical inconsistencies and the U(1) and scalar sectors fail at very short distances, but we can still safely use the SM, Higgs sector included, because it matches our experimental results, so these shortcomings should not be an everyday worry except because we still lack a valid theory of QG.

I have raised the psychosocial factor because Weinberg himself is appealing to herd mentality by saying that, "The Standard Model, we now see – we being, let me say, me and a lot of other people – as a low-energy approximation to a fundamental theory about which we know very little. And low energy means energies much less than some extremely high energy scale 1015–1018 GeV."

Saying "Me and a lot of other people" (true or not) seems to me like a clear attempt at concocting herd mentality based on his recognized authority.

Let's be frank. The SM cannot be the final theory, because it lacks something. But it's probably much closer to the final theory than e.g. string theory, because it has much more pros than cons (unlike ST). So the farther you depart from it, the less likely you will find a final theory. And ST really goes very far away from it. Herd mentality has been a problem for HEP for decades. Had we devoted 1% of the effort we have invested in trying to square the circle of considering the SM a correct, but only effective, field theory, and searching for an entirely new theory that can replace it, instead of trying to fix its shortcomings, we would probably be there by now.

21. **Geoffrey Dixon**
January 15, 2021

Just curious: I'm assuming the discussions going on here could easily have occurred 5 years ago. How about 10? Or 20? How far into the past could one go and these debates would have still been by-and-large possible? Not that there's anything wrong with that … just curious.

22. **Peter Woit**
January 16, 2021

Geoffrey Dixon,
The point of view Weinberg is arguing for was worked out in detail by him and others over 40 years ago (some of the basic papers about this are his from 1979-80). The discussion here is much the same as one could have had back then. The only difference is that lots of evidence against this point of view has accumulated over the last 40 years: no violations of the SM (+Dirac neutrino mass terms), existence of the Higgs with its hierarchy problem, CC with its hierarchy problem, failure of all attempts to come up with a plausible theory of different physics at the GUT/Planck scale.

Back in 1980, the EFT point of view was not dominant, with lots of people (for example see Hawking on N=8 supergravity) taking the point of view that physics at high energy was likely to be some QFT extending the SM (so the SM is a piece of the final theory, not an approximation to a different kind of theory). The EFT point of view on the SM has now been dominant for decades, with no sign of influential physicists like Weinberg re-evaluating things based on what has been learned in the last 40 years. There seemed to be some hope post LHC results, but people mostly seem intent on trying to not draw conclusions based on those.

23. **Warren Siegel**
January 17, 2021

@ Peter & Peter,

QCD has a running coupling because it has a divergence. If Yang-Mills theory were finite, it would be conformal; its coupling wouldn't run. So 't Hooft's argument holds outside of perturbation theory.

Tomboulis only showed the problem could only occur near vanishing coupling, not that it did occur.

Constructive quantum field theory, which is a nonperturbative approach, also has problems with instantons & renormalons.

To my knowledge, lattices have never solved any problem of principle that had been discovered in perturbation theory.

Perturbative finiteness is the only known solution to renormalons. & the only such theories known are supersymmetric.

24. **André**
January 17, 2021

Peter,

can you explain how your point of view and that of Weinberg differ in practice?
Weinberg says that the standard model is valid (maybe) up to 10^18 GeV. Up to which energy do you think that the standard model is valid? 10^19 GeV? More?

25. **Peter Orland**
January 17, 2021

Hi Warren,

Finiteness of QCD means that the renormalized theory exists and is finite. It is not conformal invariant.

The massless limit of the regularized theory, after removing the regulator, is Weyl and conformal invariant. That's not what Peter and I are writing about.

Again, instantons and renormalons appear in trying to ressum perturbation theory (in the case of instantons summing over saddle points). There was a lot of optimism that some sort of resummation of perturbation theory over saddle points would yield an analytic solution of QCD, and people are still working on this. It doesn't mean the theory does not exist apart from these methods.

26. **Peter Orland**
January 17, 2021

Just to make it clear what is meant by finiteness:

The lattice theory is ultraviolet regulated and well defined. Calculate physical quantities (cross sections, string tension, maybe some Green's functions multiplied by anomalous powers of the lattice spacing). Now, fixing one of these quantities, take the lattice spacing to zero. This is what Peter W. and I are arguing exists (there is no theorem to this effect. It is a conjecture).

At no point are there any divergences. Nor does Weyl or conformal invariance appear (there are scaling violations).

27. **Peter Orland**
January 17, 2021

"To my knowledge, lattices have never solved any problem of principle that had been discovered in perturbation theory."

There are examples where such problems of principle are solved on the lattice, but they are asymptotically-free models in lower dimensions. These models can also be solved by other methods and the solutions agree.

28. **Arnold Neumaier**
January 17, 2021

Warren Siegel said: "Perturbative finiteness is the only known solution to renormalons."

This was only true long ago. Another solution to renormalons, known since 2015, are resurgent transseries. For recent results, see, e.g., https://arxiv.org/pdf/2007.01270.pdf

29. **More Anonymous**
January 17, 2021

Peter

I was pondering about something which lead to hopefully a relevant line of thought. I am under the impression the continuum limit of QFT is equivalent to micro causality. The Lieb-Robinson bound in lattice QFT does become the usual (micro-)causality property in the continuum limit see (arxiv.org/abs/2006.10062). That paper stops short of establishing my this, because they don't take a strict continuum limit. They show that the exponentially-small tails are small enough to be unimportant on scales much larger than the lattice spacing, with some technical definition of "unimportant," but they leave the strict continuum limit as an exercise for the reader.

Thus, from this point of view micro causality is a derived property of the lattice spacing. Now in a theory of gravity one would expect the light cone (of micro causality) to be deformed. But since the the propagation of information is dependent on the lattice spacing. The Large scale behaviour does affect the small scale and visa-versa.

P.S: I am far from an expert.

30. **Peter Woit**
January 17, 2021

Warren Siegel,
It still seems to me you're just pointing to problems with the perturbation series and the semi-classical approximation, not the non-perturbative theory, defined as Peter Orland states.

In particular, as far as problems with instantons go, how this works in lattice QCD is something I spent a lot of time working on long ago. Instantons are classical solutions, and their role in the full theory is an issue about the semi-classical expansion. They're irrelevant to the question of whether the lattice sums have the expected continuum limit. I haven't followed recent developments, but when I was involved, the problem was to identify not instantons on the lattice, but a sensible notion of net topological charge of a lattice configuration. One wants something that agrees with the continuum formula for configurations near classical solutions, and such that ambiguities due to the lattice regularization become unimportant in the continuum limit. Problems doing this might in principle cause trouble for non-zero theta parameter, but for QCD at theta=0, issues about assigning topological charges to lattice configurations shouldn't be relevant.

31. **Peter Woit**
January 17, 2021

All,
I don't want to try and have a discussion here of general issues about proposals for making sense of QFTs at short distances, so only issues directly about the SM are on topic.

André,
Neither I nor Weinberg have any idea where the SM breaks down or how. I am arguing that some sectors of the SM show no signs of breakdown at any scale, but that doesn't mean I know what happens at inaccessibly high energy scales. Much of what I object to here is making pure speculation about what's happening at scales we know nothing about sound like it has some solid evidence.

Weinberg and others pushing this point of view don't claim to know what the higher scales are, they are just setting up a framework in which there can be higher energy scales with different physics, with the SM a low energy approximation. For instance, in some models of neutrino mass of this kind, one explains the low values of neutrino masses by the existence of some high scale, still below the GUT or Planck scales. But, that's just one kind of model. Once can avoid the higher scales by just assuming neutrino masses are Dirac, with Yukawas small for some reason we don't understand (we don't in any case actually understand anything about why Yukawas take the values they do).

32. **(a different) André**
January 18, 2021

I have a different perspective, but perhaps the same conclusion as Weinberg (and many others) on the SM being an EFT of whatever is a more complete theory. My background, just to provide some perspective for my comments, is that I utilize lattice QCD and EFT to determine various properties of nucleons and their interactions which are required with various levels of precision, in order to interpret current bounds, and hopeful signals, of beyond the SM signatures in low energy experiments (such as an EDM in a nucleus or the observation of neutrinoless double beta decay or signatures of non V-A decays of neutrons etc.). Interpreting the current limits as bounds on new physics requires some quantitative understanding of BSM matrix elements in nucleons and nuclei, some of which need to be propagated to more complex systems with EFTs of nuclear physics.

Now – about the SM being and EFT of some more complete theory. My perspective could be summarized by the question, what right do we have to expect that the SM is correct to arbitrarily short-distance/high-energy? It seems far more probable that we are ignorant of some very short-distance physics than the SM being The theory that can take us to the Plank scale. Assuming there is something we are missing, and assuming that this new physics is short distance, describing the SM as an EFT of more complete theory is the most general framework to have this discussion with our current understanding of how to describe physics (using QFT). It also provides a framework to combine constraints from low-energy precision tests of the SM and signatures in colliders like the LHC, and thus helps the community significantly reduce the parameter space of possibilities.

If the presumed BSM physics is light (dark photons etc.), then this EFT description is less useful too useless, depending upon the details of the new physics. The community of people using this EFT language to make these constraints is also generally quite aware of this limitation as well.

33. **Peter Woit**
January 18, 2021

(a different) André,
There's nothing wrong with conjecturing that there's new physics at a high energy scale and using EFT as the appropriate framework for deriving the implications of such a conjecture. But you need to then acknowledge that
1. Despite decades of intensive effort, we haven't seen any of the expected implications of such new physics.
2. What we have seen of the Higgs and CC is in strong contradiction with the conjecture of such new physics.

Given these facts, "SM all the way up to Planck" is also a perfectly valid conjecture.

The problem with "It seems far more probable that we are ignorant of some very short-distance physics than the SM being the theory that can take us to the Plank scale." is that it's really just a historical prejudice (in the past we kept finding new things at higher energy scales). We don't at all know one way or the other what the truth of the matter is, so one should look at all possibilities. The "SM just a low energy approximation" possibility has gotten a huge amount of attention, become a bit of a fixed ideology, and led to finding nothing. I'm just arguing that the alternate possibility is every bit as worthy of being taken seriously and its implications explored.

34. **(a different) André**
January 18, 2021

Hi Peter,

Sure, it should be considered. But, in order to be viable, there are a few significant challenges that such a proposal would have to address. For example, as we understand things now, the CP violation in the SM is orders of magnitude too small to generate the observed abundance of matter over anti-matter in the universe. So one would have to come up with some new explanation how the SM only could circumvent this otherwise failure to explain the observation. Or, we would have to decide this is not a question worth trying to answer. Assuming we want to find some explanation, and without clear ideas how to generate the excess with just the SM, I stand by my statement that it "It seems far more probable that we are ignorant of some very short-distance physics than the SM being the theory that can take us to the Plank scale."

35. **Peter Woit**
January 18, 2021

(a different) André,
Sure, although the matter/anti-matter problem brings in the question of one's model of the early universe, which adds another source of complexity to the issue.

36. **WTW**
January 19, 2021

Peter,
No one has mentioned the Muon g-2 experiment investigating the apparent anomaly in the anomalous magnetic moment of the muon, and the very detailed theoretical work going on in parallel that includes extending calculations down to extremely fine precision by the Muon g-2 Theory Initiative. (See, for example, https://news.fnal.gov/2020/06/physicists-publish-worldwide-consensus-of-muon-magnetic-moment-calculation/, and https://arxiv.org/abs/2006.04822) That precision is required in order to differentiate between SM prediction and experiment, and it includes non-perturbative lattice QCD calculations as well as dispersive.

As some of those folks have said, this is a very highly technical and specialized field, and I have no insight into what hoops they are having to jump through to get low enough error bounds. Perhaps someone can comment on how relevant such work is to this discussion. (But it does illustrate that state-of-the-art investigations into the validity/applicability of the SM at extremely short distance scales does not necessarily require Planck-scale experimental energies, and that such work is active and on-going — even if relatively rare.)

37. **Peter Woit**
January 19, 2021

WFW,
The Fermilab muon g-2 experiment is supposed to be reporting results soon, which will be interesting. The problem though with this is that if they do find a deviation from the SM, it will be a frustrating situation. This just gives you one number, and all sorts of new possible non-SM degrees of freedom could contribute to that number, as part of some complicated higher loop calculation. So, that number, if non-zero, will tell us there's something going on we don't understand, but give very little to go on about what this might be.

38. **WTW**
January 19, 2021

Peter,
"So, that number, if non-zero, will tell us there's something going on we don't understand, but give very little to go on about what this might be."

Yes, but — to your point, above — it could give us an indication of a limit (if there is one) on just how "effective" an EFT the SM actually is. Something we don't now have.
And the lattice QCD and other techniques being developed there could potentially/hopefully be useful in other contexts as well, in helping to identify anomalies in other experimental data that could give further clues.

39. **Warren Siegel**
January 20, 2021

@ Peter & Peter,

Lattice QCD is exactly as finite as renormalized perturbation theory.

I'm not claiming QCD doesn't exist, only that it's nonperturbatively nonrenormalizable, due to renormalons, a consequence of only the running of the coupling, which can be seen from its divergences, which appear even in lattice quantization if one tries to take the continuum limit w/o renormalization of the bare couplings by giving them singular dependence on the lattice spacing.

Instantons are a separate problem from renormalons. Various solutions have been proposed, such as the 1/N expansion.

Perhaps the lower-dimensional theories with other solutions to which you refer are the superrenormalizable ones treated by constructive QFT. Those people never had success in 4 dimensions.

@ Arnold,

I'm not familiar with your solution of "long ago" 2015, but thanks for the reference. My experience with claimed UV fixed points away from the origin is that their position & even their existence is strongly prescription dependent. Also, it would be nice to see an example of a gauge theory, since theories that are not asymptotically free are known to have difficulties even in lattice field theory. Furthermore, such treatments of improved resummation in the literature tend to focus on instantons rather than renormalons, & may even fail to distinguish the two.

40. **Peter Orland**
January 20, 2021

Hi Warren,

Yes, renormalons appear as a class of diagrams which are clearly not Borel summable, even if the regulator used is a lattice. Maybe resurgence, or another scheme can cure this, or maybe not.

If you could solve this problem via rigorous methods, this might give a proof that the lattice theory is well-defined. Or maybe a proof would have nothing to do with summing graphs (including saddle points).

So renormalons may mean zip, zero, nada for the existence of the theory.

As an illustration, renormalons exist in field theoretic models in lower dimensions. I am thinking of the principal chiral model (which I have some experience with). Resummation methods are not fully successful for this model, but there is no doubt it exists. There are even exact results for the S-matrix and (at large N) correlation functions.

41. **Peter Orland**
January 20, 2021

PS I mentioned above the constructive field theory paper about SU(2) Yang-Mills in a finite volume, by Magnon, Rivasseu and Seneor. They claim that the continuum theory exists, for a finite volume. They can't take the thermodynamic limit, but (assuming their paper is correct. I guess I'll have to slog through it), this means NO fundamental ultraviolet problems with renormalons.

42. **Warren Siegel**
January 23, 2021

@ Orland,

As I said, constructive QFT can prove existence if they are SUPERrenormalizable, not just renormalizable, hence <4 dimensions.

Also as I said, the examples I've seen for defining resummation for (very) low dimensions have been for "instantons", not renormalons, i.e., corresponding to finite-action solutions to the classical field equations.

43. **Peter Orland**
January 23, 2021

Hi Warren,

"As I said, constructive QFT can prove existence if they are SUPERrenormalizable, not just renormalizable, hence <4 dimensions."

Yes, but that is NOT proving the renormalizable asymptotically-free theory does not exist. You are only saying that constructive field theory methods have not been successful (unless Magnon, et. als. is correct) for non-superrenormalizable theories. There is no no-go theorem.

Please read my other remarks above. I think I addressed your assertions quite adequately.

44. **Erickson**
February 1, 2021

Hi Peter,

If gravity QFT is one of the simplest options, wouldn't asymptotically safe gravity be the most promising track to go with? There's even attempts to make just the non-gravitational forces asymptotically safe (removing Landau poles). I believe this is along the lines of works by Eichhorn et. al., and Donoghue seems to have a fair number of things to say along this direction.

45. **Dieter Van den Bleeken**
February 2, 2021

Dear Warren, Peter & Peter,

recently we found a very simple model with renormalons where perturbation theory can be explicitly re-summed using a particular contour prescription in the Borel plane. The model is simple enough that is non-perturbatively defined and can also be solved non-perturbatively and exactly. The S-matrix obtained that way matches perfectly to the one obtained by resummed and renormalized perturbation theory. The model has no supersymmetry.

Now, this might sound too beautiful to be true and you are already wondering "where is the catch"? The catch is that this model is not a relativistic QFT but rather a simple one-particle quantum mechanics model. The advantage this offers is that we have a well defined Hilbert space with self-adjoint Hamiltonian and everything is under perfect mathematical control. Although it is a very poor version of QCD it has a logarithmically running coupling that induces exactly the same renormalon ambiguities (at least the UV ones) as its QFT cousins.

The actual details can be found in our paper arXiv:1906.07198 .

Wilczek on the Problems of Fundamental Physics

January 18, 2021
Categories: Uncategorized

Sean Carroll has a new interview up with Frank Wilczek in which they discuss, among other things, the problematic current state of fundamental physics. On the topic of string theory, here's the discussion:

> 0:58:34.8 SC: Well, some of this worry has come out of string theory, many of our colleagues for the last several decades have pointed to string theory as the most promising way forward. As far as I know, you have not done a lot of work directly on conventional string theory. What is your feeling about that approach to moving beyond quantum field theory?
>
> 0:58:54.8 FW: Well, I think it has produced a lot of attractive work that's intellectually rich and has spun off into fertile mathematics, but I don't see that it's been converging towards informative assertions about the physical world…
>
> 0:59:18.7 SC: That's very elegantly stated, actually, yes.
>
> 0:59:21.0 FW: That you can check. And for me personally, I've kind of voted with my feet, I think there are more promising things to think about, that's partially a sociological statement, but I think it's, string theory is getting plenty of attention, it doesn't need me. I'm happier doing things that other people aren't doing, but that's a personal statement, and so far, I haven't regretted my choice, but I watch what people… I watch the subject and I watch what people are doing and I wish them good luck, and if and when things that I think are promising insights into the physical world emerge, I will pay a lot of attention.
>
> 1:00:12.3 SC: Sure Do you think that the rest of the field has voted with their feet in a slightly too uniform way, do you think that too much of our intellectual effort is going in that particular direction?
>
> 1:00:21.8 FW: I do, but I might be wrong, so I don't want to discourage. Plenty of people are doing other things, so it's not as if the rest of the world is feeling the lack of input from people who are working on string theory, it's fine, people can work on string theory and it doesn't hurt anything. I feel… Well, it's going to sound, I don't want to be patronizing, the people who do it are mostly adults and they know what they're doing, but students and people who are thinking about what they're going to do should go into it with open eyes. They should realize that the prospect of making an impact in our understanding of empirical science or technology are not… The prospect that you'll make impact like that is probably not optimized by going into string theory.
>
> 1:01:20.8 SC: Yeah, no, actually, I think that we're in exact alignment here. I feel a need to defend the string theory against unfair criticisms, but I do worry a little bit about the fact that it seems hard these days to connect it directly to empirical reality.
>
> 1:01:36.2 FW: Yeah, well, some nice ideas are coming off, as coming out as spin-offs, very, very clever people do string theory and they do clever things. So as I said, there's been a lot of fruitful mathematics, there have been new techniques that have proved somewhat useful in condensed matter, although certainly not proportional to the amount of effort that's going into it, and the future may look different, they may be real breakthroughs that come out of string theory that wouldn't have come otherwise. But so far, the amount, I would say, other people may disagree, and I might be very unpopular among some of my colleagues for saying this, but I think the output compared to the input has been pretty disappointing on the empirical side.

I find it kind of remarkable that Carroll, known for defending string theory and string theorists, here reacts to Wilczek's pretty negative characterization of string theory with "I think that we're in exact alignment here."

About current hot topic work on the black hole information paradox:

> 1:02:33.5 SC: Right. You have been involved in productive ways on the black hole information problem, which a lot of string theorists care about… What is your current feeling on the state of that problem? Do you think we're making real progress?
>
> 1:02:50.8 FW: I think progress is being made in the sense that more intellectually coherent pictures are being drawn and some surprising connections to error correction and really interesting new chapters of quantum theory are emerging. On the other hand, it is a very esoteric problem, nobody's going to produce… I don't see a way, but who knows, but nobody has produced an experimental system to which these ideas apply in any reasonably direct way. So what does it mean to solve a problem like that? I'm not even sure what it means, where you can't check. Many hypotheses go into it, the distance between the models and actual black holes that were phenomena you can observe are vast and many things could go wrong along the way in making these models.
>
> 1:04:04.7 FW: So I guess, yeah, it's wonderful that people are making progress at the field, they're making progress and have a literature that they enjoy, and it really is interesting from any point of view, it's good, and maybe I should leave it at that, but how should I say? I don't think it's… I don't think it's the pinnacle of physics, let me put it… Let me put it that way.

On SUSY, Wilczek acknowledges

> I'm a supersymmetry diehard.

which he certainly is.

If you look back at his many talks about prospects for the future, you'll see that pre-LHC he was arguing

> By ascending a tower of speculation, involving now both extended gauge symmetry and extended space-time symmetry, we seem to break though the clouds, into clarity and breathtaking vision. Is it an illusion, or reality? This question creates a most exciting situation for the Large Hadron Collider(LHC), due to begin operating at CERN in 2007, for this great accelerator will achieve the energies necessary to access the new world of of heavy particles,if it exists.

In the current interview and elsewhere, Wilczek makes clear the reason he believes in SUSY is his 1981 calculation with Dimopoulos/Raby showing that in SUSY versions of GUTs you could get the coupling constant evolution to overlap at the same energy. He's still quite devoted to this argument, for him it's of greater significance than the usual hierarchy problem arguments.

He has by now lost multiple bets that the LHC would see SUSY particles, including ones with Garrett Lisi in 2009 and Tord Ekelöf in 2012. At this point, even diehards like Wilczek acknowledge that chances that the LHC will see SUSY are slim. Another problem is that increasingly sensitive proton decay experiments have also ruled out a large part of the proton lifetimes predicted by the SUSY models Wilczek favors. He puts his faith in further proton decay experiments and a new, expensive collider. This is pretty much exactly the sort of thing that causes Sabine Hossenfelder to go ballistic over arguments for a new collider.

For a flavor of the SUSY discussion, here's one piece of it, with Carroll starting off with a quite peculiar argument for SUSY:

> 0:29:28.8 SC: Yeah, maybe you can opine on this, but the way that I like to say it is, we could, in the space of all possible worlds that we live in, only one of them,

we could have found supersymmetry already at the LHC very easily, but the fact that we haven't doesn't mean it's not there. Maybe it's less likely that it's there, but it's easy also to imagine theories where supersymmetry is real, and we just haven't seen it yet.

0:29:53.1 FW: Right. So supersymmetry, as I said, there have to be… For supersymmetry to be valid, there have to be these superpartner particles that are the particles that the particles we know about turn into when they move into the quantum dimensions, but we don't know what their masses are. We know some of their properties, but not their masses, and they could be very heavy. If they're very, very heavy, we lose the benefit of improving… The benefit that supersymmetry would otherwise give in improving how the couplings unify, but okay, maybe that was a cruel joke on the part of nature. I want to think not, but the alternative is that they're just a little bit too heavy to have been produced easily and identified easily at the LHC, and we just have to work a little bit harder and spend a little more money on…

Comments

1. **paddy**
   January 18, 2021

   Peter,
   At the risk of being too facetious, it would appear that your problem is that your criticisms were both too early and not "very elegantly stated".

2. **Peter Woit**
   January 18, 2021

   paddy,
   I do think that the consensus will likely be that I was a premature anti-string theorist who did not understand the proper way to explain to authority figures that they were wrong.

3. **André**
   January 19, 2021

   "Il est dangereux d'avoir raison dans des choses où des hommes accrédités ont tort." Voltaire

4. **Sabine Hossenfelder**
   January 19, 2021

   Scientists with secure positions tend to overestimate how much of a choice the vast majority of researchers in the field really have. Voting with our feet means, for most of us, leaving academia because we can't pay rent from doing the research we think is promising. I have seen dozens of friends drop out that way, not because they wouldn't have been able to get funding, but because they felt that the research they could have gotten funding/a job for would have been a waste of their time. The result is though, that some research just doesn't get done, and academia remains a universe of research bubbles in which popular areas continue to float forever, carried by social reinforcement.

   I think it would be good if instead of listening to the top 0.1% of lucky scientists and their rosy experiences with academia over and over again, the media and podcast hosts and so on would spend some more time talking to the other 99.9% and ask them just why they work on what they work on. Better still, talk to some of those who left and ask them why. That should be eye-opening.

5. **Alessandro Strumia**
   January 19, 2021

   Last time I heard a seminar by Wilczek was when LEP closed and CERN organised talks to discuss what was the lesson. At that time some data suggested supersymmetry at LHC, while other data pointed in the opposite direction. But the talk was stubbornly one-sided.

6. **David Brown**
   January 19, 2021

   According to Wilczek, string theory has "… produced a lot of attractive work that's intellectually rich and has spun off into fertile mathematics …" Has supersymmetry generated any new mathematics that mathematicians find impressive?

7. **Peter Woit**
   January 19, 2021

   David Brown,
   Supersymmetry has led to a lot of new mathematics. For instance, Witten's "Supersymmetry and Morse Theory" paper is the founding document of the whole field of topological quantum field theory, and the Seiberg-Witten work on N=2 supersymmetric Yang-Mills revolutionized the field of four-dimensional topology with the Seiberg-Witten equations. One could argue that these developments are even more significant than what has come out of string theory.

   The problem with this, as with the mathematical advances coming from string theory research, are that they are pretty much in a direction orthogonal to the direction of the the string or susy unification program. As an idea about fundamental physics, what is relevant is N=1 4d SUSY and issues of its spontaneous breaking, and this is the part of SUSY that seems to have relatively little deep mathematical signficance.

8. **Peter Woit**
   January 19, 2021

   Alessandro Strumia,
   I'm completely mystified by Wilczek's diehard attitude about SUSY. On other topics he's a model of cautious, careful reasoning. Besides his very early paper with Dimopoulos/Raby, SUSY is not something he devoted much of his career to, so he's not someone like Ellis or Kane who have much of their career invested in the idea. In addition, making many public statements that the LHC would decide the SUSY issue, together with losing mutiple public bets, you would think would argue for a stance of "looks like I was probably wrong on that bit of speculation", not the diehard "there's still a chance I was right, let's build a new collider to find out".

9. **Low Math, Meekly Interacting**
   January 19, 2021

   Isn't it simply due to a hardened bias in favor of observable unification, which SUSY purportedly provides in an especially neat and (to some beholders) beautiful way?

   Perhaps this is overly provocative, but an alternative, which your last post may hint at, is that it's pretty much the SM all the way up to the Planck scale, right? Maybe dark matter is taken care of by a (directly unobservable) sterile neutrino, gravity is "asymptotically safe" or something, and…that's it. Nothing really new or interesting happens until you reach unimaginably high energies, things are the way they are for reasons humans can never conceive of , much less probe, and there's essentially nothing left for HEP theorists to do.

Maybe that's the mother-of-all-nightmare-scenarios, and accepting SUSY is wrong makes it that much more plausible. It's unacceptable because it kills an entire field of investigation that once occupied the very pinnacle of human achievement, perhaps never to be duplicated again.

My deeply pessimistic psychosocial take.

10. **Marco**
January 20, 2021

> Better still, talk to some of those who left and ask them why. That should be eye-opening.

Sadly, academia thinks of people that are critical of academia and have left as disgruntled scientists that were not good enough to succeed and are thus bitter, and shrug off any criticism (valid or not) from such persons.

What is a Spinor?

January 21, 2021
Categories: Uncategorized

Recently Jean-Pierre Bourguignon recently gave the Inaugural Atiyah Lecture, with the title *What is a Spinor?* The title was a reference to a 2013 talk by Atiyah at the IHES with the same title. Bourguignon's lecture is not yet online, but I realized there are lectures explaining what a spinor is that I can highly recommend: my own, in this semester's course on the mathemematics of quantum mechanics. I'm closely following the textbook I wrote.

Teaching this course this past academic year has made me all too aware of things that are less than ideal about the book, and I unfortunately haven't had time to get to work on making any significant improvements. Going through the material on spinors though, I'm pretty happy with how that part of the book turned out, think it provides a clear explanation of a beautiful and important story, one that is not readily available elsewhere.

One aspect of this that I emphasize is the remarkable parallel between

- The usual story of canonical quantization, which is based on an antisymmetric bilinear form on phase space, giving an algebra of operators generated by Q_j,P_j acting on the usual quantum state space.
- Replacing antisymmetric by symmetric, you get the Clifford algebra, generated by γ-matrices, acting on the spinors.

For a table summarizing precisely this parallelism, see chapter 32 of the book.

For more video from my office, I recently had a long conversation with Reza Katebi, who has a Youtube channel of interviews called The Edge of Science.

Comments

1. **More Anonymous**
   January 21, 2021

   Does anyone know Micheal Atiyah's reason for distaste of supersymmetry (mentioned around minute 36 of the video)? He seems to evade the question :/

2. **Geoffrey Dixon**
   January 22, 2021

   Bass ackwards, starting from something inherently bosonic to get to something fermionic. As pointed out by John Conway, each of the normed division algebras is a spinor space. Starting from there you get – among other things – Dirac neutrinos and a matter dominated universe.

   I know this is too self serving to pass muster, but sometimes these conversations using ideas that are fundamentally flawed are a tad irksome. Start with spinors.

3. **Peter Woit**
   January 22, 2021

   Geoffrey Dixon,
   The discussion of spinors I give in the book and lectures isn't bosonic, it's inherently fermionic. What I find remarkable and wanted to emphasize is the tight parallelism between the initially very different looking bosonic and fermionic versions of canonical quantization.

   What this is most useful for is getting a uniform way of dealing with spinors in any dimension, including infinite dimensions. Fundamental matter particles are described by a quantum field theory based on fermionic canonical quantization of the solutions of a Dirac equation. The solution space is infinite dimensional, fermionic quantization gives a state space that can be thought of as the spinors for this infinite dimensional space.

   The question of how to best think about the physical spin 1/2 degree of freedom and spinors in real 3 space or 4 space-time dimensions is at the other end of the problem from a uniform treatment of spinors valid in complex vector spaces of arbitrarily high dimensions. For real vector spaces you get a much more intricate algebraic story, bringing in the quaternions at least. The mysteries of division algebras are relevant there, as is my currently favorite idea about fundamental space-time geometry (twistors, where points in space-time are precisely spinors).

4. **Geoffrey Dixon**
   January 22, 2021

   Several years ago I encountered Atiyah in London. This was not long after he'd given a talk at Princeton extolling the algebra C ⊗ H ⊗ O as a basis for a unified HEP theory, with the octonion algebra, O, accounting for gravity. Those attending the talk were nonplussed, for they'd expected something else. Anyway, back in London, surrounded by a fawning throng, I told him his division algebra idea was wrong in its essentials. He was chagrined.

   Still, that algebra, like twistors, has the hope of forming a bottom up theoretical HEP foundation, one that might lead inexorably to unique and unavoidable explanations of many of the things that perplex us. As I am not an expert in QFT, this comment may be out of line, but it has always seemed to me that reliance on QFT to show the way is top down, relying on inconsistencies, infinities, and anomalies to exclude paths that are not viable – trial and error.

   As to twistors, in one of Penrose's tomes he referenced me, along with a cluster of others who exploited division algebras beyond C. But it was not a positive reference. His twistors are intimately connected to C, and he felt that going beyond C would be like pouring molasses on caviar – unnecessary, and unwanted. Your willingness to consider pouring higher dimensional division algebras onto twistors I find intriguing. I have hopes it'll be more like pouring maple syrup on pancakes.

5. **Peter Woit**
   January 22, 2021

   Geoffrey Dixon,
   Unfortunately, as far as I can tell, Atiyah's enthusiasms in the last few years of his life were not based on anything very substantive, and I think that includes whatever he was thinking about octonions and gravity.

   Most of what has been achieved with twistors crucially uses holomorphicity which is based on complex numbers, so I can see why Penrose thinks those are crucial. Twistors naturally give you not space-time but complexified space-time, and if you want to work with fields on this, you need holomorphicity in order to get usual fields on usual space-time.

One place where it is clear that quaternions play a role in twistor theory is in the Euclidean signature version, where the conformal group is $SL(2,\mathbf H)$. Penrose must know about this, but to get back to Minkowski signature you need holomorphicity, so complex not quaternionic analysis. The fact that twistor space can be taken to variously be $\mathbf C^4$ and $\mathbf H^2$ makes one suspicious that there's an octonionic story buried there somewhere. But when I try and understand what people have tried to do along these lines, I remain baffled so far, unclear to me what's going on.

6. **Chris Oakley**
January 23, 2021

Backtracking a little bit, physicists quite reasonably objected to relativity on the grounds that measurements of distance and time could not possibly depend on the observer's motion and to quantum theory on the grounds something could not possibly be a particle and a wave at the same time. Electron spin, although it explained doublets in spectral lines nicely, was another idea that took a while to be accepted. I am guessing that one objection was that all objects in nature surely had to be invariant under a 360° rotation. So if spinors are real, spacetime is not as simple as we imagined. I would not know what to do with Penrose's notion of spacetime being formed from composite spinors, but it does at least "explain" this one thing.

7. **Bertie**
January 23, 2021

The interview with Reza Katebi was wide-ranging and insightful.
At 2 hrs long, I never expected to watch it through to the end, but am happy I did
🙂

8. **Rollo Burgess**
January 23, 2021

Thanks v much for your video interview which is is very interesting.

I am also thrilled finally to find a topic on this blog in which I possess some cognitive authority: per the comment in the video, in cricket a googly is a delivery which has an off spin but is delivered with a leg spin action, thus tricking the batsman into thinking the ball will break in the opposite direction on the bounce from what it in fact does. 🙂

9. **I**
January 25, 2021

Well, your book is quite good for an auto-didact. Trouble is, it is lacking in exercises. What other flaws do you think there are?

10. **Peter Woit**
January 25, 2021

I,
Yes, there are some exercises, but too few. There are a bunch of places in the book where I'd now do things somewhat differently, or in a different order. One major flaw I think now is chapters 24-26, which spend too much time developing certain things that will get used later, but probably should have been relegated to an appendix, to be consulted as needed.

In recently years I've spent a lot of time thinking about the Euclidean version of QM, and someday would like to incorporate some of that into the book.

11. **Art**
January 26, 2021

Look likes the Bourguignon video is now up.

This Week’s Hype

January 25, 2021
Categories: This Week's Hype

I had just been thinking the other day about how little one hears recently about the multiverse, with those previously involved in heavy promotion of the idea perhaps having thought better of it. Today however, Quanta has [Physicists Study How Universes Might Bubble Up and Collide](). This describes work of a sort that has become popular in recent years: study of various condensed matter systems, with a huge dollop of hype on top about quantum gravity based on some aspect of the condensed matter theory calculation having some vague relation to some calculation in some toy quantum gravity model or other.

I’ve written extensively here and elsewhere about the real problem with all claims by theorists to be studying the multiverse: they’re [Theorists Without a Theory](), lacking any sort of viable theory which could make the usual sort of scientific predictions. The main problem with the Quanta article is at the beginning:

> What lies beyond all we can see? The question may seem unanswerable. Nevertheless, some cosmologists have a response: Our universe is a swelling bubble. Outside it, more bubble universes exist, all immersed in an eternally expanding and energized sea — the multiverse.
>
> The idea is polarizing. Some physicists embrace the multiverse to explain why our bubble looks so special (only certain bubbles can host life), while others reject the theory for making no testable predictions (since it predicts all conceivable universes). But some researchers expect that they just haven’t been clever enough to work out the precise consequences of the theory yet.
>
> Now, various teams are developing new ways to infer exactly how the multiverse bubbles and what happens when those bubble universes collide.

The big problem is with:

> they just haven’t been clever enough to work out the precise consequences of the theory yet.

The reference to “precise consequences” is a common misleading rhetorical move, implying that there is no problem getting “imprecise consequences”, that the problem is just getting those extra digits of numerical precision. What’s really going on is that we know of no theoretical consequences of the multiverse, precise or imprecise, because there is no viable theory. The logic here is pretty much pure wishful thinking: if you look at colliding Bose-Einstein condensates and see a particular pattern, then if you saw a pattern like that in the CMB, you could try and infer something about your unknown multiverse theory. It’s not unusual for theorists to work on speculative ideas involving some degree of wishful thinking, but this is a case of taking that to an extreme.

Update: One of the very few theorists who has pushed back on the multiverse ideology is Paul Steinhardt. Howard Burton [has posted here]() something from [his interviews with Steinhardt](), which includes this from Steinhardt:

> “I’ve had this discussion where I’ll say, ‘Well, what do you think about the multiverse problem?’ and they reply, ‘I don’t think about it.’
>
> “So I’ll say, ‘Well, how can you not think about it? You’re doing all these calculations and you’re saying there’s some prediction of an inflationary model, but your model produces a multiverse — so it doesn’t, in fact, produce the prediction you said: it actually produces that one, together with an infinite number of other possibilities, and you can’t tell me which one’s more probable.’
>
> “And they’ll just reply, ‘Well, I don’t like to think about the multiverse. I don’t believe it’s true.’
>
> “So I’ll say, ‘Well, what do you mean, exactly? Which part of it don’t you believe is true? Because the inputs, the calculations you’re using — those of general relativity, quantum mechanics and quantum field theory — are the very same things you’re using to get the part of the story you wanted, so you’re going to have to explain to me how, suddenly, other implications of that very same physics can be excluded. Are you changing general relativity? No. Are you changing quantum mechanics? No. Are you changing quantum field theory? No. So why do you have a right to say that you’d just exclude thinking about it?’
>
> “But that’s what happens, unfortunately. There’s a real sense of denial going on.”

Update: Ethan Siegel has an [excellent piece on the basic problem with string theory]() (to the extent it’s well-defined, it has too large a (super)symmetry group and too many dimensions, no explanation for how to recover 4 space-time dimensions and observed symmetry groups).

> Here’s why the hope of String Theory, when you get right down to it, is nothing more than a broken box of dreams.

Update: If you’re looking for a detailed discussion of multiverse theories, of neither the usual promotional sort, nor the highly critical sort I specialize in, I can recommend Simon Friedrich’s new book [Multiverse Theories: A Philosophical Perspective](). Friedrich has a blog entry about the book [here]().

Comments

1. **John Baez**
   January 26, 2021

   Why do writers at *Quanta* and other science magazines bother with this crud when there’s plenty of much more interesting *actual new physics* to write about? – you know, physics where people use theories to make predictions, test these predictions in the laboratory, and then refine their theories. Are condensed matter physicists just too busy making actual discoveries to chat to the journalists?

2. **Peter Woit**
   January 26, 2021

   John Baez,

   Quanta is more focused on the kind of actual new physics you suggest than just about any other publication, but I guess the multiverse is still irresistible for use as a hook to get attention.

   The main problem is with the physics community itself, which has over the years produced far too little pushback on multiverse pseudo-science. A rare exception is Paul Steinhardt. I just ran into Howard Burton’s interviews with him, will add a link to the posting. He quotes Steinhardt explaining that the physics community basically tries to sweep the multiverse problem under the rug instead of confronting it.

3. **Syksy Räsänen**

January 26, 2021

Steinhardt's comments on the multiverse are misleading.

Some inflationary potentials give eternal inflation. Usually this occurs when they are extrapolated into a regime far from where the observed inhomogeneities are produced, and in a region where the calculations cannot be trusted any more.

Some inflationary potentials do not give eternal inflation.

The conflation of inflation, eternal inflation and multiverse (all distinct things) by Steinhardt et al is part of the multiverse hype problem, not a welcome respite.

4. **Peter Woit**
   January 26, 2021

   Syksy Räsänen,
   I don't want to start up the usual arguments over inflation, or get involved in arguments over different behavior of different inflationary potentials.

   Steinhardt is one of the founding fathers of inflation, now argues against it. His main argument about this is that inflation doesn't do one of the things it was supposed to do: explain why you get a smooth, flat universe without having to tune that into the initial conditions. As far as I can tell, he's on solid ground when making that argument.

5. **Blake Stacey**
   January 26, 2021

   "What lies beyond all we can see?"

   Fluffy nougat.

   This answer is just as scientific as multiversology while having the added benefit of being much more delicious.

6. **jackjohnson**
   January 26, 2021

   I should keep my mouth shut because of my ignorance, but it seems to me that mathematical work of Uhlenbeck and others on compactification of moduli spaces by adding `bubbles' is important and interesting: and that if someone has something serious to say about `the multiverse', they should be aware of that literature. As far as I can tell, the set of such persons is well-approximated by the empty set.

7. **Michael Weiss**
   January 26, 2021

   Not to change the subject, but as a chemist I think that Paul Steinhardt should have shared the 2011 Nobel Prize in Chemistry for quasicrystals. His contributions were deep and wide-ranging.

8. **John C. Rodney**
   January 26, 2021

   Check out particleclara on TikTok for songs and info about CERN and the LHC. Maybe not directly relevant to this post but it just went up today and it is certainly relevant to this general blog.

9. **vmarko**
   January 27, 2021

   Is it possible that the writers of Quanta are simply not aware that there are other topics to write about in fundamental physics? Alternatives to string theory, multiverse and inflation, but still quite interesting, thought-provoking and equally captivating for the non-expert audience? Maybe they simply don't know that such topics exist?

   🙂
   Marko

10. **Low Math, Meekly Interacting**
    January 28, 2021

    I saw a couple hopeful signs, with effort. Yeah, it's "crud". But unlike the majority of related content I've read, there was at least an attempt at moderation. Some work is characterized as "the [most] baby version of this problem that you can think of,". If the reader doesn't recognize how pointless it all is, they may still come away with a sense that the odds aren't good: "you've taken a lot of things that are just very hard for physicists to deal with and mushed them all together and said, 'Go ahead and figure out what's going on,'". There's even a hint of an admission: "It's a long shot".

    Not great, but not nothing. Multiverse Mania has set a low pop-sci bar, but I felt this article rose a smidgen above the norm.

11. **Syksy Räsänen**
    January 28, 2021

    Peter:

    I don't think Steinhardt's argument that inflation -> eternal inflation -> multiverse is on solid ground. Let me elaborate… (I won't even go into the eternal inflation -> multiverse part.)

    1) The eternal inflation regime only exists for potentials of certain shape. You can easily write a potential such that there is no eternal inflation.

    2) Even for the potentials that have an eternal inflation regime, it is typically far separated from the observable regime.

    For example, for the archetypical quadratic potential, observable modes are generated around 60 e-folds before the end of inflation, and eternal inflation regime is about 10^6 e-folds earlier. In terms of field value, this is a factor of 10^3. There is no reason to suppose that we can just extrapolate the potential there.

    3) Even if the potential shape for large field values supports eternal inflation, there is no reason that the initial conditions have to be such that the field starts there. In fact, Steinhardt has criticised inflation with the argument that we don't know the initial conditions – but his argument about eternal inflation requires specific initial conditions.

4) Eternal inflation (in slow-roll inflation) means that the power spectrum is of order unity, so perturbations are of the same order as the background and perturbation theory breaks down. So we cannot trust the calculation anymore.

This of course does not mean that eternal inflation is ruled out – simply that we do not know it to be an inevitable feature of inflation.

I find popular articles about this a bit depressing to follow, as they often focus on two sides who both think inflation->multiverse, divided only by whether they think this is good or not… Not really representative of the cosmology community.

12. **sdf**
January 30, 2021

The situation at Leicester again (somehow related to a previous post on the AI fad)
https://www.reddit.com/r/math/comments/l7yyir/not_joking_university_of_leicester_to_make/

13. **Geoffrey Dixon**
January 31, 2021

By the way, re the Quanta article, I've commented elsewhere how annoying I find it when such articles, often relating to some very outré, and lurid, notion, claim that the notion in question is being investigated by "physicists", the very unspecific plural leaving the uninitiated with the impression that physicists as a class – en masse – are focused …

Anyway, "A physicist thinks there is a black hole in Newark, NJ", in being singular, is less likely to grab eyeballs – and that, after all, is their business. Although I'd sure read that one.

14. **Frank Wilhoit**
February 1, 2021

Why was Ethan Siegel's piece published in *Forbes*, of all places? Who is the audience?

It is well written and accurate, but the nut graf is the third from last, topic sentence: "If String Theory is correct, then somehow — and nobody knows how — this ultra-symmetric state broke, and it broke incredibly badly."

That word "incredibly" is doing (as they say) a lot of heavy lifting there. Siegel's use of language is otherwise careful. Is he really making an argument from intersubjective plausibility? And if he is, isn't that just another manifestation of the same lgical fallacy as the arguments from "naturalness"? To whom, exactly, and why, exactly, is this symmetry breaking "incredible"? There is such a thing as scientific imagination; whose imagination fails here, and whose does not?

15. **Peter Woit**
February 1, 2021

Frank Wilhoit,
Siegel has a regular column on the Forbes site, often dealing with fundamental physics, similar to this one. He does a good job of trying to explain serious physics to a general audience.

The point he is making I think is the main one that needs to be made, and that rarely gets explained. It's been the problem with string theory (and GUTs and supersymmetry) since the beginning. They posit huge new symmetry groups (in the case of string theory, 10d super-Poincare as well as things like two copies of E8) which imply physics that looks nothing at all like what we observe. You then have to come up with symmetry-breaking mechanisms which explain why and how the symmetry is broken to the much smaller (4d Poincare + SU(3)xSU(2)xU(1)) we observe.

Calling this symmetry breaking "incredible" is perfectly reasonable. That this happens is an extraordinary claim that should require extraordinary evidence. Instead there's none at all. The original hope that an exotic compactification scheme using Calabi-Yaus would solve the problem was always highly speculative and hard to believe (it could properly be described as "incredible"). That doesn't mean people shouldn't have looked into it, but for a long time it has now been clear this doesn't work.

16. **jack morava**
February 1, 2021

I think Ethan Siegel's account of the dream of string theory: "… that we can take this theory, like some enormous unbroken box, and stick the right key in it and watch it crumble away, leaving only a tiny piece left that perfectly describes our Universe…" is cogent and realistic; but then IICC, the Planck mass is something like twenty orders of magnitude greater than the Higgs mass, which sounds like plenty of room for new physics. It reminds me of people like Lyell who came to understand that the age of the world was much greater than they had imagined. An old academic cartoon shows two bearded guys at a blackboard, one says to the other, `It's not that they want you to promise results; it's that they want you to promise results IN THEIR LIFETIME!' Apparently Archimedes, based on work of Aristarchus, estimated the size of the universe as roughly two light-years; today we might say he was off by ten orders of magnitude. It is not so clear to me that string theory should be abandoned just because it has a lot of unexplored room in it.

17. **jack morava**
February 1, 2021

PS the previous comment escaped before I had the chance to say that all this hype nevertheless creeps me out.

18. **Frank Wilhoit**
February 1, 2021

Peter,
If he had used the word "improbably" rather than "incredibly", I would have had no point to make. Perhaps I had none, even so.

Thanks,
FW
.

19. **Bernhard**
February 2, 2021

Simon Friedrich's blog post is well-written and diplomatic. Though I think none of the hard critics of the multiverse claim that might not live in a multiverse.

20. **Lars**

February 3, 2021

” these large groups are enormous, like a block of uncut marble, and we want to get just a tiny, perfect statuette (our Standard Model, and nothing else) out of it.”

“String Theory truly is: a large, unbroken box that must somehow crumble in this particular, intricate fashion, to recover the Universe we observe”

He is mixing his metaphors — and very badly.

21. **Low Math, Meekly Interacting**
February 3, 2021

I concur with Bernard, and I’m in disagreement with Friedrich on other matters. I don’t understand how a concept that may be as malleable as “God”, and may require god-like powers to probe, can be described as “far from unscientific”. It’s far from helpful, at best. Of course there are mysteries grounded in good science that may be beyond humans to explain or test, but that’s hardly a new or especially deep idea. That the obvious happens to take a on a most abstruse manifestation in the speculative realm of untestable hypotheses doesn’t strike me as terribly “serious”. There’s a mountain of negative data to stack up against the notion of new symmetries where they should have been found in the most “natural” places, tons of non-evidence of collisions with bubble universes and so forth. It’s not as if these idea haven’t been quite thoroughly vetted, and we’re butting up against real physical limits to testing them any further, quite aside from the fact that they’re no longer promising. Where’s practical “science” in any of these multiverse scenarios to hide any longer? Aren’t such concepts already so seriously constrained by evidence as to stretch credibility to the breaking point? I don’t think there’s much room for diplomacy, anymore, however congenial one wishes to be. Hasn’t “the multiverse” already conclusively failed?

22. **Robert A. Wilson**
February 3, 2021

As a group theorist, I can confirm that these large groups really are enormous. However, they bear no resemblance to a block of uncut marble. Nor do they bear any resemblance to the universe we observe.

Geometric Langlands News

February 11, 2021
Categories: Langlands

There's various news to report on the geometric Langlands front, spanning number theory to quantum field theory:

Minhyong Kim has been running an Online Mini-Conference on the Geometric Langlands Correspondence for the past month, and Dennis Gaitsgory has been doing something similar since last spring at his Geometric Langlands Office Hours.

Very recently Edward Frenkel has given talks in both places (see talks here, here and here, slides here and here). He's been talking about joint work with Etingof and Kazhdan on a function-theoretic (as opposed to sheaf-theoretic) version of geometric Langlands. They have a paper out here, are working on two more.

This work to some extent has its origins in attempts by Langlands to come up with his own version of such a function-theoretic approach. Frenkel was asked to discuss this topic by the organizers of the Abel Conference in honor of Langlands. I wrote about what happened here. Frenkel came to the conclusion that what Langlands was suggesting could not work (Langlands vehemently disagreed…), but this led him to the current research he is pursuing with Etingof and Kazhdan. For a written version of Frenkel's talk explaining all this, see here.

On the quantum field theory front, Witten and Gaiotto have been working on relating older ideas of Gukov-Witten about using branes as a general method of quantization, applying this to geometric Langlands, in the new context that Frenkel's talks discuss. Witten talked about this last week in the Kim seminar (video here, slides here). Gaiotto last week also spoke about this at a Kansas State seminar, video here, slides here.

The original 2008 Gukov-Witten paper on branes and quantization is here, Gukov's 2010 Takagi lectures on this are written up here. The problem of how to quantize a general symplectic manifold is a fascinating one, and at the time I was very interested to see this proposal. It does however invoke a very sophisticated set of ideas about quantum field theories in order to deal with what one would think are much simpler examples of the quantization problem. Perhaps this program would come into its own in this new case, where the quantization problem involves similarly sophisticated mathematical constructions.

From another side of the geometric Langlands world, Peter Scholze is continuing his lectures on his ongoing work with Laurent Fargues that reformulates the local Langlands correspondence in terms of geometric Langlands on the Fargues-Fontaine curve. There are associated discussion sections, with a web-page here.

Announcement: I'd been reading about how the hot new idea for authors on the internet is Substack, where all sorts of interesting material can now be found. After thinking about this "back to the email newsletter" model for a minute, I realized that I should try and see if I could get email subscriptions to this blog working. There's now a place over on the right where you can ask for an email subscription. No experience with this yet, so I can't guarantee either that it works or that problems won't turn up that will cause me to have to turn that feature off.

Update: For another talk by Witten about this from today (Feb. 11) see here.

Comments

1. **Jim Akerlund**
   February 11, 2021

   Peter,

   For the email notifications, will the notification say who is doing the post, or will it just say "new post in topic "X"?

   Jim Akerlund

2. **Peter Woit**
   February 11, 2021

   Jim Akerlund,

   When I post a new blog entry, you'd get an email from "Not Even Wrong", with a subject line like
   [New post] Geometric Langlands News
   and then the full text of the blog entry as an email. So you can read the blog entry in your email, and get it write when it's posted.

   It's actually very similar to the Substack model, where subscribers get content in their email exactly this way. Only difference is that subscriptions are free!

   This is being done by a WordPress feature called JetPack, and everything is being managed at wordpress.com. My only concern about this is that it's something I don't have full control over, since it is managed by wordpress.com. On the other hand, it does seem convenient for people, and I don't want to manage subscriptions + sending out bulk emails myself.

3. **4gravitons**
   February 12, 2021

   As reassurance, I've had people subscribing by email via JetPack to my blog for several years. There is one issue that's come up a few times, and that is that if someone replies to the email, that reply is automatically submitted as a comment on the blog. I had a few family members reply thinking it was just going to me, only for it to end up in moderation as a comment instead. But as long as your readers don't confuse the blog emails with your personal email you shouldn't have any problems.

4. **anon**
   February 12, 2021

   Sad news from MIT: Isadore Singer died yesterday.

5. **Will Sawin**
   February 12, 2021

   Further recent progress in geometric Langlands theory is the work of Arinkin, Gaitsgory, Khazdan, Raskin, Rozenblyum, and Varshavsky (https://arxiv.org/abs/2010.01906), which like the work of Etingof, Frenkel, and Khazdan, is devoted to finding concrete, function-theoretic consequences of geometric

Langlands, but in a different direction: they give a new statement of the geometric Langlands correspondence which formally implies, and doesn't just suggest, the (everywhere unramified) Langlands correspondence for function fields over finite fields.

6. **Peter Woit**
   February 13, 2021

   Thanks Will!
   Looking forward to the day when it will again be possible for you to help explain this sort of thing to me over lunch…

Isadore Singer 1924-2021

February 12, 2021
Categories: Obituaries

I was sorry to hear this morning of the death yesterday at the age of 96 of Is Singer, a mathematician who led much of the interaction between mathematics and physics during the 1970s and 1980s. In the early stages of my career, among mathematicians investigating the amazing relations between mathematics and the quantum field theories describing fundamental physics there were three towering figures: Atiyah, Bott and Singer. That the last of them has now left us marks the end of an era.

Each of the three had a huge influence on me, both intellectually and personally. Reading their papers and listening to their lectures were great intellectual experiences, shaping early on my understanding of what is central to mathematics and how it fits together with physics. Especially inspirational was the way that they brought together very different fields of mathematics, with Atiyah having his roots in algebraic geometry, Bott in topology and Singer in analysis. Their work together makes a strong case for the unity of mathematics and the relation to physics makes an equally strong case for the unity of mathematics and physics.

On a personal level, at a time when I was tentatively moving from a career in physics to one in mathematics, getting to meet and talk to each of them had a big impact. Much as I respected the great theoretical physicists I had met, rarely had I found them to be particularly friendly or encouraging, and their attitudes influenced the general atmosphere of the field. Atiyah, Bott and Singer struck me each in their own way as wonderfully warm and enthusiastic personalities, and I believe this influenced the atmosphere among mathematicians working in their fields. They were among the most respected figures in the math community, so their enthusiasm for ideas coming out of physics generated a lot of interest in these ideas among a wide variety of mathematicians.

Singer had always had an interest in physics, majoring in physics as an undergraduate at the University of Michigan, then after the war going to graduate school in mathematics at the University of Chicago. I highly recommend reading or watching this long interview with him from 2010, where you can learn the story of his career.

A mathematical high point of this career was his work during the early 1960s with Atiyah that led to the Atiyah-Singer index theorem. A crucial part of this story was Atiyah in 1962 asking Singer why the A-roof genus was integral. Singer realized that this was because it counts the number of solutions of an equation, and that the equation was the Dirac equation. This example in some sense generates a huge amount of mathematics which is described by the index theorem, and which links together very different mathematical fields. On this and other topics, well-worth reading is the 2004 interview with Atiyah and Singer after they were awarded one of the first Abel Prizes.

One can trace much of the history of the modern interaction of mathematics and quantum field theory to an origin back in the summer of 1976, when Singer visited Stony Brook and talked to physicists there about gauge theories, geometry and the BPST instanton (Simons and Yang a year earlier had started to realize how gauge theory, geometry and topology were linked). The next year he was in Oxford working with Atiyah and Hitchin on instantons, which really set off an explosive development of new ideas, inspiring and fascinating both mathematicians and physicists.

Singer spent the years from 1977 to 1983 at Berkeley, which he turned into a major center for this new mathematical physics. During this time he was also one of the founders of MSRI, which to this day plays a major role in worldwide mathematical research. After 1983 he returned to MIT, from which he retired in 2010. I believe the last time I saw him was at his 85th birthday conference, which I wrote about here.

Update: The New York Times has an obituary here.

Update: Dan Freed (who was a graduate student of Singer's) has a piece about Singer at Quanta magazine here.

Comments

1. **Jeff Berkowitz**
   February 12, 2021

   I am sorry for your loss, and the world's.

   Now we (those of us in our age group, each in our respective disciplines) are the elders.

   We can only hope to play the role so well.

2. **Davide Castelvecchi**
   February 13, 2021

   Is there a good, reader-friendly source for understanding what the index theorem means and what its implications are? My student self — who desperately tried to read Lawson and Michelson back in graduate school but found it inscrutable — would love to know!

3. **Peter Woit**
   February 13, 2021

   Davide,
   When I was learning this stuff I found Atiyah's own expository articles clear and inspiring. See for instance

   1966 ICM lecture
   1967 Algebraic topology and elliptic operators
   1973 The index of elliptic operators (colloquium lectures)
   Classical Groups and Classical Differential Operators on Manifolds

   For the heat equation method and later developments, one place to look is Dan Freed's unfinished notes on Dirac operators (he was Singer's student)
   https://web.ma.utexas.edu/users/dafr/DiracNotes.pdf

   For relation to quantum mechanics:
   Orlando Alvarez, Lectures on Quantum Mechanics and the Index Theorem

4. **D R Lunsford**
   February 13, 2021

   Thanks for that – I met Feigenbaum once, and had the same experience that mathematicians at the high level were more engaging than physicists. Was just thinking about Irving Segal and his conformal c0smology and then find out here he was Singer's advisor. Small universe.

-drl

5. **Peter Woit**
February 13, 2021

drl,

Feigenbaum was a rather unusual character, and many would consider him a physicist, not a mathematician.

I was interested to see a nice tweet by Erik Verlinde about Singer:
https://twitter.com/erikverlinde/status/1360328234743836676
where he also notes that Singer was much friendlier than the people he was used to dealing with
"[Singer was] one of the friendliest people I have met."
Verlinde though also shows the difference between theoretical physics and math culture when he writes:
"In my view, he was the only mathematician with a deep understanding of theoretical physics."
Among mathematicians I know, even the few who might think there is only one physicist with a deep understanding of mathematics would never say that out loud…

6. **rhofmann**
February 15, 2021

Well written, authentic obituary. Many thanks, Peter. I am looking forward to read the 2010 interview with Prof. Singer tonight. Isadore Singer has contributed a lot to the renewed fertile interaction between physics and mathematics initiated by physicists' discoveries of selfdual field configurations in gauge theories in the 1970ies, and it makes me feel sad that another grand master of the field is no longer among us. The index theorem and how it counts the zero modes of the Dirac operator on an instanton is wonderful stand-alone math stuff which, moreover, has deep physical implications in pointing to the essentials of dynamical chiral symmetry breaking and quantum anomalies in Yang-Mills. In addition, its significance for the physics of gauge theory is expressed by the fact that Nahm's duality transformation between instantons on T4, which vastly generalises the important ADHM construction, significantly relies on the index theorem. Nahm's transformation, in turn, is the venue to calorons of non-trivial holonomy which mediate the emergence of mass and charge from pure energy in Yang-Mills.

7. **John Baez**
February 15, 2021

Davide wrote:

> Is there a good, reader-friendly source for understanding what the index theorem means and what its implications are?

I don't know the perfect one for you. John Rognes wrote a popular account for the Abel prize which might be good for people who know less math than you. You might find it unsatisfyingly vague.

Briefly put, the Atiyah-Singer index theorem gives a topological formula for the dimension of the space of solutions of a differential equation on a manifold. As such, it's a great way to use topology to learn something about the solutions of differential equations without actually solving them. And conversely, it's a great way to use differential equations to learn things about the topology of manifolds!

It was extremely important sociologically. It built a bridge between two fields — partial differential equations and topology — which had not existed before. And because the differential equations it applies to are important in physics, it connected physics more firmly to both these fields.

More precisely, the Atiyah-Singer index theorem is a formula for the "index" of a differential equation, where the index is the dimension of the space of solutions (the "kernel") minus something else (the dimension of the "cokernel"). So, you have to be clever to use it to get precise information about the dimension of the space of solutions. But it easily gives a lower bound.

Furthermore, it only works when the manifold is compact and the differential equation is "elliptic" (like the Laplace equation, not like the wave equation or heat equation).

On the bright side, it works for differential equations where the solutions are not just functions, but sections of vector bundles — like the elliptic version of the Dirac equation, which turns out to be the key to the general case. Another interesting thing is that because Dirac equation involves spinors and Clifford algebras, the Atiyah-Singer index theorem highlights the role of spinors and Clifford algebras in topology.

I spent a lot of time in college reading *Seminar on the Atiyah-Singer Theorem*, by Richard Palais. It had great explanations of the analysis prerequisites, like pseudodifferential operators on vector bundles, Sobolev spaces, and chain complexes of Hilbert spaces. Somehow all that seems second nature to me by now. But I still don't feel I have a great intuitive feel for the topology: that is, why the formula for the index in terms of characteristic classes is what it is.

When I went to grad school at MIT, Singer was one of the stars there: he ran a seminar on mathematics connected to quantum field theory, and a huge crowd attended, including Raoul Bott and many other bigshots. When visitors like Witten or Atiyah came to town, we'd all go to their talks.

At that time — say, 1982-1986 — it seemed that everyone wanted to learn the Atiyah-Singer index theorem and generalize the heck out of it. I attended Dan Quillen's seminar where he was trying to find a really simple proof of the Atiyah-Singer theorem. It was very exciting, because he was working in real time. Each class he would start flawlessly, and then continue until he got stuck on something. In the end he was scooped by Ezra Getzler, who however used some fancy analysis that Quillen would have wanted to avoid.

8. **Alex**
February 16, 2021

Just want to add, for those interested in the topic, that learning about the index theorem is the best way to prepare if you want to study noncommutative geometry in the sense of Connes, which pretty much involves (among other things) trying to generalize the index theorem to the noncommutative setting. So, that's one modern spinoff of the Atiyah-Singer index theorem.

9. **JC**
February 16, 2021

@Davide

I also attempted to go through Lawson & Michelsohn "Spin Geometry", and also found it heavy going back in the day.

Years later, I found Rosenberg's "Laplacian on a Riemannian Manifold" slightly less heavy going.

Yet More Geometric Langlands News

February 27, 2021
Categories: Langlands

It has only been a couple weeks since my last posting on this topic, but there's quite a bit of new news on the geometric Langlands front.

One of the great goals of the subject has always been to bring together the arithmetic Langlands conjectures of number theory with the geometric Langlands conjectures, which involved curves over function fields or over the complex numbers. Fargues and Scholze for quite a few years now have been working on a project that realizes this vision, relating the arithmetic local Langlands conjecture to geometric Langlands on the Fargues-Fontaine curve. Their joint paper on the subject has just appeared . It weighs in at 348 pages and absorbing its ideas should keep many mathematicians busy for quite a while. There's an extensive introduction outlining the ideas used in the paper, including a long historical section (chapter I.11) explaining the story of how these ideas came about and how the authors overcame various difficulties in trying to realize them as rigorous mathematics.

In other geometric Langlands news, this weekend there's an ongoing conference in Korea, videos here and here. The main topic of the conference is ongoing work by Ben-Zvi, Sakellaridis and Venkatesh, which brings together automorphic forms, Hamiltonian spaces (i.e classical phase spaces with a G-action), relative Langlands duality, QFT versions of geometric Langlands, and much more. One can find many talks by the three of them about this over the last year or so, but no paper yet (will it be more or less than 348 pages?). There is a fairly detailed write up by Sakellaridis here, from a talk he gave recently at MIT.

In Austin, Ben-Zvi is giving a course which provides background for this work, bringing number theory and quantum theory together, conceptualizing automorphic forms as quantum mechanics on arithmetic locally symmetric spaces. Luckily for all of the rest of us, he and the students seem to have survived nearly freezing to death and are now back at work, with notes from the course via Arun Debray.

For something much easier to follow, there's a wonderful essay on non-fundamental physics at Nautilus, The Joy of Condensed Matter. No obvious relation to geometric Langlands, but who knows?

Update: Arun Debray reports that there is a second set of notes for the Ben-Zvi course being produced, by Jackson Van Dyke, see here.

Update: David Ben-Zvi in the comments points out that a better place for many to learn about his recent work with Sakellaridis and Venkatesh is his MSRI lectures from last year: see here and here, notes from Jackson Van Dyke here.

Update: Very nice talk by David Ben-Zvi today (3/22/21) about this, see slides here, video here.

Comments

1. **Fen Zuo**
   February 27, 2021

   Geometric Langlands is reflected in integer/fractional quantum Hall effect and the so-called Hofstadter's butterfly, according to recent work of Kazuki Ikeda.

2. **Jim Eadon**
   February 28, 2021

   Can anyone provide a (hand-wavy) summary for the educated layman (Physics post-grad) of what a Fargues-Fontaine curve is, and what's special about, e.g. in the context of the Langlands programme?

3. **Arun Debray**
   February 28, 2021

   Re: DBZ's course, I am not the only student taking notes. Jackson Van Dyke is also posting his notes online: https://web.ma.utexas.edu/users/vandyke/notes/langlands_sp21/langlands.pdf (Github link: https://github.com/jacksontvd/langlands_sp21). Jackson's notes go into quite a bit more detail, and he's gone back and added more references and figures than I have. In the end we'll hopefully combine our notes into one document. If anyone has any questions, comments, or corrections about either set of notes they're welcome to get in touch with me or Jackson.

4. **Peter Woit**
   February 28, 2021

   Arun,
   Thanks a lot, both for producing the notes, and for letting us know about the other ones.

5. **David Ben-Zvi**
   February 28, 2021

   Peter – thanks for the references!
   Maybe let me note the series of talks in Korea (by Sakellaridis, Venkatesh and me) are aimed at a more arithmetic audience, some previous talks (eg at MSRI last March) might be more accessible to the audience here.

   Also I might add that the perspective on automorphic forms as quantum mechanics is very old and widely used. A newer perspective I'm trying to advertise in the course and talks is to think of automorphic forms (and the Langlands program) as being really about 4d QFT — an arithmetic elaboration of the Kapustin-Witten picture for geometric Langlands. This accounts for many of the special features of quantum mechanics on arithmetic locally symmetric spaces – e.g., dependence on a number field is the analog of considering states on different 3-manifolds, Hecke operators — a form of quantum integrability– come from 't Hooft line operators, the choice of level (congruence subgroup) corresponds to consideration of surface defects, and most importantly the relation with Galois representations (the Langlands program) can be viewed as electric-magnetic duality.

   The new feature of the work with Sakellaridis and Venkatesh (the first paper should appear relatively soon..) is that the theories of periods of automorphic forms and L-functions of Galois representations can fruitfully be understood as considering boundary conditions in the two dual TQFT, and that the electric-magnetic duality of boundary conditions (as studied by Gaiotto-Witten) can be used to explain the relation between the two (the theory of integral representations of L-functions).

6. **Peter Scholze**
   March 1, 2021

Jim Eadon,

let me try to answer. This paper is about the (local) Langlands correspondence over the p-adic numbers $\mathbb Q_p$. Recall that p-adic numbers can be thought of as power series $a_{-n}p^{-n} + \ldots + a_0 + a_1 p + a_2p^2 + \ldots$ in the "variable" p — they arise by completing the rational numbers $\mathbb Q$ with respect to a distance where p is small. They are often thought of as analogous to the ring of meromorphic functions on a punctured disc $\mathbb D^*$ over the complex numbers, which admit Laurent series expansions $a_{-n} t^{-n} + \ldots + a_0 + a_1 t + a_2t^2 + \ldots$. More precisely, there is this "Rosetta stone" going back to Weil between meromorphic functions over $\mathbb C$, their version $\mathbb F_p((t))$ over a finite field $\mathbb F_p$, and $\mathbb Q_p$.

However, there is an important difference: t is an actual variable, while p is just a completely fixed number — how should $p=2$ ever vary? In geometric Langlands over $\mathbb C$, it is critical to take several points in the punctured disc $\mathbb D^\ast$ and let them move, and collide, etc. What should the analogue be over $\mathbb Q_p$, where there seems to be no variable that can vary?

In one word, what the Fargues–Fontaine curve is about is to build an actual curve in which p is the variable, so "turn $\mathbb Q_p$ into the functions on an actual curve". It then even becomes possible to take two independent points on the curve, and let them move, and collide. With this, it becomes possible to adapt all (well, at least a whole lot of) the techniques of geometric Langlands to this setup.

This idea of "turning p into a variable and allowing several independent points" is something that number theorists have long been aiming for, and is basically the idea behind the hypothetical "field with one element". I would however argue that our paper is the first paper to really make profitable use of this idea.

7. **Peter Woit**
March 1, 2021

David,
Thanks! I've added some links to your MSRI talks, which do look like a better place for people to start.

I've always been fascinated by analogies between number theory and QM/QFT, the new angle on this you're pointing out is really remarkable, looks like a significant deep link between the subjects.

Could you (or Peter Scholze!) comment on any relation of this to the other topic of the posting (local arithmetic Langlands as geometric Langlands on the Fargues-Fontaine curve)?

8. **Jim Eadon**
March 1, 2021

Professor Scholze,
You give a sense of how exciting it is, to, literally? connect the dots between different fields. I'm glad I studied pure mathematics as a hobby enough to get the gist of your explanation. I will re-read your reply a few times, as it's deep, and connects several fascinating objects and techniques.
I really appreciate you taking the time to engage, it means a lot. And thanks too, to Professor Woit, for bringing such mathematics to my (and others) attention, I enjoy the blog.

9. **David Ben-Zvi**
March 1, 2021

Peter – the way I see it (somewhat metaphorically) is as follows. In extended 4d topological field theory we seek to attach vector spaces to 3-manifolds, categories to 2-manifolds etc. The Langlands program fits beautifully into this if you accept the "arithmetic topology" analogy: besides ordinary 3-manifolds we consider (Spec of ring of integers of) global fields (number fields and function fields over finite fields) as "3-manifolds". Besides surfaces we also admit local fields (such as p-adics or Laurent series over finite fields) and curves over the algebraic closure of finite fields as "2-manifolds" (this is the theme I'd like to get to in my course, though still a way to go).

If you accept this ansatz, there's no "geometric Langlands" and "arithmetic Langlands", we're just considering different kinds of "manifolds" as inputs. For example geometric Langlands on surfaces and local (arithmetic) Langlands both concern equivalences of categories (in the latter case, one seeks descriptions of categories of reps of reductive groups over local fields are described in terms of spaces of Galois representations).

The Fargues-Scholze work is (among many other things!) a spectacular realization of this kind of idea. They show that the local Langlands program can be (and arguably is best) considered as geometric Langlands on an actual curve attached to the local field (the Fargues-Fontaine curve). Moreover the most crucial structure here, the Hecke operators, are miraculously described in a geometric way (factorization — the colliding points in Peter's response) that descends directly (via Beilinson-Drinfeld) from the structure of operator products in 2d QFT.

The wonderful recent work of Arinkin, Gaitsgory, Kazhdan, Raskin, Rozenblyum and Varshavsky that Will Sawin mention in a recent comment also fits into this general paradigm, in that they show that [unramified] arithmetic and geometric Langlands in the function field setting are precisely related by "dimensional reduction" – you pass from "2-manifolds" (curves over alg closure of finite fields) to "3-manifolds" (curves over the finite fields — which you should think of as mapping tori of the Frobenius map, so 3-manifolds fibering over the circle) by taking trace of Frobenius, just as TFT would tell you.

10. **Laurent Fargues**
March 5, 2021

I'm late but I'm going to say a few words to complement Peter's comments. Since this is a Physics blog I'm going to give a few key words that may speak to physicists. A lot of things work by analogies in this work, trying to put together some ideas from arithmetic and geometry together, make some mental jumps and trying to fill the gaps.

When Peter is saying "turning into a variable and allowing several independent points" this is analog to the fusion rules in conformal fields theory. There is the possibility in the world of diamonds to take different copies of the prime number p and fuse them into one copy. Here there reference, if I dig in my mind the first time I heard about this, is the work of Beilinson Drinfeld on factorization sheaves in terms of D-modules. You will find this fusion process in the Verlinde formula too in a coherent sheaves setting for compact Riemann surfaces in the work of Beauville "Conformal blocks, fusion rules and the Verlinde formula" for example where you fuse different points on a Riemann surface. I typically remember a talk by Kapranov about 'The formalism of factorizability' and did not get why the Russian peoples, who are known to have a huge background in physics, were such obsessed with this. No doubt this is linked to vertex algebras, where there are fusion rules, too and plenty of things of interest for physicists. For arithmeticians the declic, I remember saying to myself "at least I understand why peoples are obsessed with those factorization stuff", came from Vincent Lafforgue who remarked that if you work in an étale setting instead of a D-module setting, the moduli spaces of Shtukas (a vast generalization of modular curves for functions fields over a finite field) admits a factorization structure and this factorization structure gives you the Langlands parameter for global Langlands over a function field over a finite field.

For the curve here is what I can say. If you take an hyperbolic Riemann surface it is uniformized by the half plane on which you have a complex coordinate z. You can imagine the same type of things for the curve where the variable is the prime number p.

By the way there is an object in the article that may speak to physicists : Bun_G, the moduli of principal G-bundles on the curve. The analog for physicists would be the moduli of principal G-bundles on a compact Riemann surface, where now G is a compact Lie group, that shows up in the work of Atiyah and Bott. Still by the way, one of the origin of my geometrization conjecture is trying to understand the reduction theory "à la Atiyah Bott" for principal G-bundles on the curve (the analog for any G of the work on the indian school (Narasimhan-Seshadri) + Harder for GL_n i.e. usual vector bundles).

There are other objects that may speak, by analogies, to physicists in this paper. Typically the so called local Shtuka moduli spaces "with one paw" (i.e. only one copy of the prime number p). The archimedian analogs are hermitian symmetric spaces. Realizing local Langlands in the cohomology of those local Shtuka moduli spaces has an archimedian analog : Schmid realization of Harisch-Chandra discrete series in the L^2 cohomology of symmetric spaces. Schmid uses the Atiyah-Singer index formula to obtain his result, a tool well know to Physicists. Hermitian symmetric spaces are moduli of Hodge structures and this has been a great thing to realize that p-adic Hodge structures à la Fontaine are the same as "geometric Hodge structures" linked to the curve.

I could speak about this during hours and do some name-dropping that speaks to physicists but one thing is sure: there is no link with the multiverse, this I'm sure. Anyway, I have no idea how the curve looks like in other universes of the multiverse.

11. **Peter Woit**
March 5, 2021

Thanks Laurent!

The Atiyah-Bott story (involving gauge fields + the Yang-Mills equations) and the Atiyah-Schmid story (involving, for SL(2,R), the Dirac operator on a 2d space) are two of my favorite topics. They're essentially the two main components of the Standard model (the Dirac equation for matter fields, the Yang-Mills equation for gauge fields). Only difference is that they're in 2d rather than 4d….

I hope you're still planning to come to Columbia for fall 2022, look forward to seeing you then!

12. **Laurent Fargues**
March 6, 2021

My Eillenberg lectures are reported to 2023 sadly, because of the virus. I'll try not to enter into the technical details and give some general picture of the objects showing up in this work.

By the way, the cancelled program "The Arithmetic of the Langlands program" jointly organized with Calegari, Caraiani and Scholze is officially reported to 2023, same period of the year as before.

13. **Thomas**
March 7, 2021

A very nice popular sketch of the idea of "the curve" by Matthew Morrow:
https://webusers.imj-prg.fr/~matthew.morrow/Morrow,%20Raconte-moi%20la%20Courbe.pdf
& more:
https://webusers.imj-prg.fr/~matthew.morrow/Exp-1150-Morrow,%20La%20Courbe%20de%20FF.pdf

14. **Jim Eadon**
March 9, 2021

@Thomas thanks for the link.

ABC is Still a Conjecture

March 4, 2021
Categories: abc Conjecture

Just a reminder that the abc conjecture is still a conjecture, there is no known valid proof (don't believe what you might read in an EMS journal). For more about why one attempted proof doesn't work, see here and here. For extensive background on this, you could start at this blog posting and work backwards, to the first announcement of a claimed proof back in 2012. By 2018 Scholze and Stix had shown that the claimed argument was flawed, and since then the math community has lost interest and moved on. Devotion to the idea that the proof is valid seems now restricted to a small circle of die-hards based in Kyoto and Nottingham who are doing what they can to try and pretend the hole pointed out in the proof does not exist. There will be an IUT Summit in Kyoto in September, but the organizers don't seem to have found anyone from outside Kyoto or Nottingham willing to participate.

Update: Mochizuki today on his website has put out a 65 page manuscript dealing with criticisms of his proof, it's entitled:
ON THE ESSENTIAL LOGICAL STRUCTURE OF INTER-UNIVERSAL TEICHMULLER THEORY IN TERMS OF LOGICAL AND "∧"/LOGICAL OR "∨" RELATIONS: REPORT ON THE OCCASION OF THE PUBLICATION OF THE FOUR MAIN PAPERS ON INTER-UNIVERSAL TEICHMULLER THEORY

I've taken a quick look at this document, and I don't think it will convince anyone Scholze is wrong about the flaw in Mochizuki's proof. There's a long third and final technical section, but the first two sections do a great deal of damage to Mochizuki's credibility. Nowhere in the document do the names Scholze or Stix appear (they are referred to as "RCS: the redundant copies school"), but it starts off with statements such as

> the response of all of the mathematicians with whom I have had technically meaningful discussions concerning the assertions of the RCS was *completely uniform* and *unanimous*, i.e., to the effect that these assertions of the RCS were **obviously completely mathematically inaccurate/absurd**, and that they had no idea why adherents of the RCS continued to make such manifestly absurd assertions.

and

> the assertions of the RCS are nothing more than **meaningless, superficial misunderstandings** of inter-universal Teichmuller theory on the part of people who are clearly *not operating on the basis of a solid, technically accurate understanding of the mathematical content and essential logical structure* of inter-universal Teichmuller theory.

Before going on to the more technical third part, the second part is an extensive discussion of elementary mathematical errors, as some sort of "explanation" of what's wrong with Scholze and Stix.

Essentially the claim Mochizuki is making in these first two sections is that the most accomplished and talented young mathematician in his field is an ignorant incompetent, and that everyone Mochizuki has consulted about this agrees with him. It's hard to imagine a more effective way to destroy one's own credibility and to convince people not to bother to try and make sense of the third section.

There's no direct reference to the Scholze-Stix document, just a reference to Mochizuki's own web-page about March 2018. Mochizuki has even gone to some trouble to stop anyone from accessing the Scholze-Stix document without first reading his own web-page.

As for the long discussion by Scholze and others of the problems with the proof that was hosted here and gathered here, the only apparent reference to this is

> More recently, one mathematician with whom I have been in contact has made a quite intensive study of the mathematical content of recent blog posts by adherents of the RCS.

followed by

> Despite all of these efforts, the **only justification** for th **logical cornerstone RCS-identification** of (RC-Θ) that we could find either in oral explanations during the discussions of March 2018 or in subsequent written records produced by adherents of the RCS were statements of the form
>
> "I don't see why not".

Update: To take a look at the preface, see here.

Comments

1. **lroberth**
   March 5, 2021

   Funny, I was just thinking about the conjecture and visited your page for the first time in months to see if you'd weighed in… sure enough! If I understand in layman terms, Mochizuki wants to imbue his objects with a history and this was flawed; regardless, many of us marvel how the most advanced mathematicians simply cannot agree on what should be the rules of logical argument itself. Is math fuzzy or just too hard to understand beyond this level except for just a few?

2. **Peter Woit**
   March 5, 2021

   lroberth,
   I think you're misunderstanding the situation. The experts in this field are in close to unanimous agreement there is no proof. The math is not fuzzy and the rules of logical argument are clear. What experts in the field are finding hard to understand is why anyone is still claiming there's a proof and why any journal would publish such a claim.

3. **lroberth**
   March 5, 2021

   @Peter, no I get it, I just don't get it why Mochizuki doesn't! He is an accomplished and brilliant mathematician after all, so why doesn't a widely-agreed logical disproof work with him and his supporters? This is the fascinating meta to a lot of us.

4. **Peter Woit**
   March 5, 2021

lroberth,
Mathematicians are human beings, not calculating machines. Over the years, I've become all too familiar with how hard it is to get someone who has invested their soul in an idea to realize that it doesn't work. The abc story is nothing compared to some others I could think of…

5. **Simon L.**
March 5, 2021

Oh, I wasn't aware that this is going to be published by the EMS! Why on earth did they do this? They should have been aware of the context. Ugh.

I cannot read the preface due to paywall. Is there any context provided at least?

6. **anon**
March 5, 2021

This might be nitpicking, but I wouldn't say that PRIMS is an EMS journal. It's published by the EMS Publishing House, but I don't think that makes it an EMS journal (unlike, for example, JEMS).

7. **Peter Woit**
March 5, 2021

Simon L./anon,

The EMS name dominates the web-page for this publication. I understand that contractually they may not be able to affect what PRIMS publishes, but I don't understand why they haven't issued a statement explaining the situation, but instead recently put out this
https://ems.press/updates/2020-11-16-prims-special-issues-2021

I still haven't accessed the journal through the paywall, but somewhere I can't now find did see the first page of the preface. It appears to be the only introductory material and is just a statement that Mochizuki did not participate in review of the papers, and I believe states that there were five referees. Nothing about Scholze-Stix or any indication that there's any controversy over the proof.

8. **Robert A. Wilson**
March 5, 2021

This journal is one of those old-fashioned ones that there used to be lots of, run by an academic institution and published by a commercial publisher who didn't interfere. We have become too used to the modern situation in which the commercial publisher interferes far too much in the running of a journal. To add to the confusion, this commercial publisher is linked to a mathematical society, which gives the false impression that the EMS endorses what is published in this journal. Nevertheless, I would consider it to be inappropriate for either the EMS or the EMS Press (which are separate organisations) to make any comment of the kind you suggest: it would amount to exactly the kind of interference from a publisher that you would normally suggest should be strongly condemned.

9. **Florian Stümpfl**
March 5, 2021

Just skimmed through the first pages of the Mochizuki manuscript mentioned in the update. Nothing of substance really, all he does is slander Scholze and Stix. So nothing new.

10. **JE**
March 5, 2021

Just a brief quote on the EMS reaction to the announcement of the upcoming publication of Mochizuki's papers on ABC, taken from an article published in Nature on April 3, 2020: "If the editors of the journal "waved away these criticisms" and published the paper without major revisions, it would reflect badly on them and on Mochizuki himself, says Volker Mehrmann, the president of the European Mathematical Society (EMS), which publishes the journal on behalf of the RIMS. (The EMS has no editorial control over the journal's content, Mehrmann says, and he was unaware that an announcement was imminent until contacted by Nature.)"

What strikes me as mystifying in this whole Mochizuki affair is not the fact that a highly reputed mathematician who has invested his soul in an idea, as Peter rightly put it, is finding big trouble to realize that it doesn't work, even to the point of engaging in ad hominem attacks on two equally reputed mathematicians in a seemingly desperate attempt to whisk away criticism on his work, but rather that he has been able to convince a small but important group of die-hard mathematicians in Kyoto and Nottingham to ignore the criticism and side unequivocally with him, and have two other highly reputed mathematicians, like Kashiwara and Tamagawa, making the announcement.

11. **Peter Woit**
March 5, 2021

Robert A. Wilson,
The EMS Press is owned by the EMS, and its president is the president of the EMS (this is Volker Mehrmann, mentioned by JE).

Yes, editorial independence is an important principle, but no matter what the context, publishers have some basic level of responsibility for what they publish. I'm not surprised that the EMS Press felt it could not intervene in a Kyoto editorial decision, was surprised that they thought it a good idea to advertise this issue and put it in a positive light.

12. **xyz**
March 5, 2021

The EMS publishes Portugaliae Mathematica and Rendiconti del Seminario Matematico della Università di Padova too. Some people have focused rather on the fact that the editor-in-chief of PRIMS is Mochizuki himself.

13. **Peter Woit**
March 5, 2021

xyz,
If you read Mochizuki's new document, you'll find his explanation of why his journal and his colleagues at RIMS were the only people "technically qualified" to referee his papers.

14. **David Roberts**
March 5, 2021

….and even tell people how they can buy their own special copy of the papers:

> Individuals interested in purchasing a print copy of the PRIMS special issue on Inter-universal Teichmüller Theory may contact us at preorders@ems.press.

as if it's a souvenir!

15. **random reader**
March 6, 2021

The next step seems obvious: set up an open online bulletin board, independent of the involved parties, for discussing and disputing the papers.

If Mochizuki is confident that he is right, he can not only vindicate his credit for solving ABC, but publicly "own" (pwn!) the entire mathematics community simply by answering questions in a public, archived forum. It would be the biggest flex in mathematical history, maybe all of modern academic history.

But if he were not entirely confident of his position, he would need to find a justification for not taking any further questions, and somehow de-legitimize any further scrutiny lest things get out of hand. The anonymity and obscurity of the refereeing process creates a circular, perpetual excuse —- if the referees approved the paper, then it must have been sufficiently well explained, and thus Mochizuki does not have to explain it to anyone else if he doesn't want to.

If referee comments (not identities) were published this illusion would be harder to pull off, even as a pretense.

16. **Peter Woit**
March 6, 2021

random reader,
Mochizuki and the PRIMS editors have here taken unjustifiable advantage of the traditional confidentiality of the refereeing process. In their statement publishing the papers, they do not acknowledge the existence of the Scholze-Stix objection or the consensus of experts in the field that the proof is flawed. They explain that a committee was formed to evaluate the proof and imply that it considered the objections, but have refused to make public anything from this committee justifying its decision. All we have to go on is Mochizuki's new document, and I don't think that's going to convince anyone.

As for an "open online bulletin board" debate, that's essentially already happened, hosted here April 6-19 of last year. I'm sure Mochizuki and those around him in Kyoto were aware it was going on, and they would have been welcome to participate, either anonymously or identifying themselves, but they chose not to. One could argue that the moderator (me) had a particular point of view, but I stand by the decisions I made about moderating comments, which emphasized allowing anyone who actually understood the mathematics involved to have their say. At this point, anyone who wants to evaluate this for themselves can read the earlier Scholze-Stix document and Mochizuki's response, + the discussion here, and now Mochizuki's new contribution. I think if one does this, it's clear there is currently no proof of abc, and that Scholze and others have much better ways to spend their time than engage with Mochizuki's claims that they are ignorant and just don't understand.

17. **W**
March 6, 2021

Another response to Mochizuki's earlier work that I was disappointed he didn't engage with was David Roberts' notes discussed here https://thehighergeometer.wordpress.com/2018/09/28/on-mochiukis-report-on-discussions/ which elegantly explains how the confusions/contradictions/difficulties he describes in various simplified examples in his response to Scholze and Stix can be easily resolved using the standard mathematical language of categories and functors.

The new document contains many other simple examples trying to get the underlying point across, and if there really is an underlying point being communicated, the first order of business would be to explain why the usual mathematical language is insufficient.

One part of the document I found interesting is when he says

" In fact, however, the RCS-identifications of (RC-log), (RC-Θ) do not resolve
such issues [i.e., of relating corresponding objects in (Θ±ellNF-)Hodge theaters at
distinct coordinates "(n, m)" of the log-theta-lattice] at all [cf. the discussion of
symmetries in Example 2.3.1, (iii)!], but rather merely have the effect of
translating/reformulating such issues of relating corresponding objects in
(Θ±ellNF-)Hodge theaters at distinct coordinates "(n, m)" of the log-theta lattice into issues of tracking the effect on objects in (Θ±ellNF-)Hodge theaters as one moves along the paths constituted by various composites
of Θ- and log-links."

This is striking to me, because to my understanding "tracking the effect on objects as one moves along the paths" is precisely what Scholze, Stix, etc. propose to do! This seems like conceding that their approach could be made to work, if one tracks these effects appropriately. (Which, of course, makes me think I am reading it wrong.)

The fundamental issue here is not that working with a single concrete object and tracking how it transforms along paths (i.e. compositions of various automorphisms) is better in some abstract, logical, philosophical sense, but rather that it is much less likely to conceal hidden errors, than Mochizuki's approach of defining an infinite number of distinct objects and identifying some of them by definition, even ones written by quite different notation, meaning that objects with quite similar notation mean quite different things.

Another quote I found striking was

"it should be emphasized that it is entirely unrealistic
to attempt to obtain the inequality of the final numerical estimate as the result of
concatenating some chain of intermediate inequalities since this is simply not the
way in which the logical structure of inter-universal Teichmuller theory is organized.
That is to say, in a word, the logical structure of inter-universal Teichmuller theory
does not proceed by concatenating some sort of chain of intermediate inequalities.
Rather, […] the logical structure of inter-universal Teichmuller theory proceeds by
observing a chain of AND relations "∧""

Similarly, the key point here is that (1) any argument involving combining a bunch of relations between points of different torsors for the real number can be logically translated into an argument involve combining a bunch of equalities and inequalities of real numbers by fixing an identification of each torsor with R, (2) any symmetries of the situation that existed prior to doing this are preserved, at the cost of introducing terms in the formulas describing those symmetries to account for isomorphisms between torsors that are incompatible with the identifications, (3) an argument written this way is less likely to conceal errors.

18. **Charlie G**
March 6, 2021

Not a mathematician here. All of this is far over my level, but while ABC is still a conjecture, I wonder if Mochizuki's attempted proof has revealed any techniques that are of more general value for other purposes – in the way Wiles' proof has – I gather – opened up useful avenues for other research.

19. **anon**
March 6, 2021

To be fair to Mochizuki, his manuscript has a section two for non-specialists like myself which shows he's sincere in trying to get people to understand his ideas.

Initially it didn't make sense to me and I was tempted to arrogantly dismiss it as gibberish; but after reading it a few more times its main points are becoming profoundly clearer: how apparent contradictions disappear in mathematics once the overall structure is restructured in some part, despite the total information content remaining the same.

20. **Peter Woit**
March 6, 2021

anon,
Mochizuki likely is sincere in his belief that those who think his proof is flawed are making elementary mistakes in their reasoning. The problem is that this is not even slightly credible. If you know anything about how the best mathematicians work, his trying to explain why Scholze disagrees with him by listing elementary mistakes a student might make comes off as just bizarre.

One thing that strikes me about this document is that Mochizuki sadly now appears to lack colleagues willing or able to tell him not to do things that destroy his own credibility.

21. **André**
March 7, 2021

Peter, Charlie G,

the question whether Mochizuki has produced valuable insights or tools is the central one. The question will decide whether, in the future, people will treat him like Wiles or like a crackpot. Is there any information on this? As an outsider, I have a second question: the term "inter-universal" makes no sense at all. Why is it called that way?

22. **Pedro**
March 7, 2021

From the 65-page document:

"There is a fundamental difference between criticism of a mathematical theory that is based on a solid, technically accurate understanding of the content and logical structure of the theory and criticism of a mathematical theory that is based on a fundamental ignorance of the content and logical structure of the theory."

That is totally right. But pretending that the time employed by Stix-Scholze (who have more things to do) just led them to "fundamental ignorance" says a lot about his thinking process. And about the clarity of his writing.

Also all the praise of PRISM and its history and the editors in the preface to the publication, and dismissing other journals (Annals, Inventiones…) as unsuitable for publication only point the reader to doubting the refereeing process.

23. **The Vlad**
March 7, 2021

What's the deal with the odd stylistic structure of the Mochizuki document (i.e. littered with bold text, italics, randomly dispersed quotation marks, no numbering of equations)? I've never seen anything like it in the math literature. Is this just some quirk of his or is it standard in some math fields, or something else? I found it very distracting.

24. **Jon1**
March 7, 2021

Apparently there are also some people interested by Mochizuki's work in Lille, he will answer questions in a seminar in a few weeks and other folks will talk there later on, see this page https://math.univ-lille1.fr/d7/sarit

25. **UF**
March 7, 2021

Let me draw attention to p.58/ step 3 of the descent process in Mochizukis manuscript, which is (very superficially) perhaps the most counterintuitive of the descent process described by Mochizuki: Here one passes from (0,\circ)^\perp to (0,0)^\perp. How can this be "passing to weaker information", if we go from unspecific vertical \circ position to the specific vertical position 0?

As far as I understand, the point is the following: we can in the reconstruction algorithm reconstruct all data \pi_1, monoids and the action on monoids etc. up to some indeterminacies. However, keeping in mind the algorithm these data were reconstructed from, these reconstructed data (are claimed to) contain more information than the information contained just in the output data (forgetting about the algorithm constructing this output). One may thus decide to forget some of this information, and replace some (not all) of the reconstructed objects by abstract, unrelated ones. In this step 3 one replaces reconstructed monoids by abstract monoids and builds with the algorithm some "shells" in these abstract monoids (instead of in the reconstructed monoids). The claim is that the reconstruction algorithm still works (up to further indeterminacies Ind2) even if the monoids are unrelated, and not reconstructed like other data showing up in the algorithm.

If one accepts this, one can take advantage of this by taking as the unrelated monoid the specific one which is shared via the theta-link from the 1-column. Whereas before (in step 2) the input data into the algorithm was vertically invariant, it is no more so with this specific choice of abstract monoid. On the other hand the (via step 3) reconstructed data has now some relation to the q-pilot in the 1-column: the monoid in which the shells are constructed comes from the q-pilot via the theta-link. (Breaking this vertical invariance of the input data is essential to prove anything meaningful related to the q-pilot, which is located at a specific vertical position).

This may also indicate a reason to pass from X to \pi_1(X): In IUT, one decides to share across the theta-link a "part" of $\pi_1(X)$ (or X), which is "easily" describeable in terms of \pi_1(X) (but not so much in terms of X), namely for instance the monoid on which \pi_1(X) acts (this monoid can be viewed as "part" of \pi_1(X), since a copy of it can be reconstructed by $\pi_1(X)$).

26. **Peter Woit**
March 7, 2021

The Vlad,
It's a quirk of Mochizuki's. Again, I think an increasingly large part of his problem is that he has no one around him willing or able to advise him to not do things that don't help with his credibility.

Jon1,
This is part of a program organized by Collas out of Kyoto, see
http://www.kurims.kyoto-u.ac.jp/~bcollas/IUT/IUT-schedule.html
In all the materials linked to there, again, no mention even of the existence of Scholze-Stix, much less any discussion of the problem with the proof they have pointed out. The Kyoto standpoint is pretty uniformly to try and pretend that Scholze and Stix don't exist.

Charlie G/Andre,

One big problem with publishing the IUT papers without acknowledging that the consensus of the field is that they contain serious error is that people will now have trouble using anything in those papers to build on and do something else with. If you use results from those papers in a proof, you're going to need to show that they're independent of the problematic parts of the papers if you want to publish your results outside of Kyoto. Essentially this may create a fork in the math literature.

27. **xyz**
March 7, 2021

Charlie G/Andre,

You're raising a very good question, the answer to which (so far) strengthens the sceptics' case: Months after Wiles' first announcement, many original, universally accepted results were produced on the basis of his methods. (That was already before the full proof of the part of modularity conjecture required for Fermat's Last Theorem). In the claimed proof of the ABC-conj. nothing remotely like this has happened during all those years after its announcement. This may indicate something about the applicability/generality/strength of the tools and insights employed for the claimed proof.

28. **A. Rukhin**
March 7, 2021

" Both Professors Kashiwara and Tamagawa have an outstandingly high international reputation, built up over distinguished careers that span several decades."

The mention of these two stands out as somewhat interesting: what have these two said publicly about the latest developments? Apparently, they supported the decision to publish…

29. **Peter Woit**
March 7, 2021

A. Rukhin,
Kashiwara and Tamagawa chaired the committee that decided to publish the papers, and appeared at the press conference last year announcing this. Mochizuki's main argument at this point is to pit their reputations against Scholze's, and politically this may work well in Kyoto, less well elsewhere.

Kashiwara is not an arithmetic geometer, but Tamagawa is, and he should be in a good position to evaluate Scholze's argument that the proof is flawed. That he made the decision to publish implies that he understands why Scholze is wrong and how the proof overcomes Scholze's objections. Tamagawa is at RIMS, has collaborated with Mochizuki, and should be very interested in Mochizuki's IUT ideas and how they prove abc. As far as I know though, he has not spoken at any of the various IUT workshops, or written anything about this.

The one thing that has been missing here has been an explanation of how the proof overcomes Scholze's objections from one of the people other than Mochizuki who supposedly understands the proof. This is something that Tamagawa should be able to provide, and it would be very helpful if he were to do so.

30. **Timothy Chow**
March 8, 2021

lroberth: You used the term "logical disproof," but the Scholze-Stix argument falls just short of that. As I understand it, they are not explicitly claiming that the notorious Corollary 3.12 is provably false. What they are saying is that Mochizuki's argument fails to establish Corollary 3.12. That is, the objection is that there is a glaring *gap* in the argument. In theory, the question of whether there is a gap in a mathematical argument that B follows from A is an objective one, because someone who says there is no gap should be able to fill in all the intermediate steps in the chain of reasoning from A to B on demand, to the point where even a machine could check that each step is correct. The catch is that this process might require, say, one million intermediate microsteps, so in practice, mathematicians don't actually produce all the intermediate steps explicitly. An expert might be able to see how to get from A to B without any further explanation; a less expert mathematician might require ten intermediate macrosteps spelled out; a graduate student might require a hundred intermediate mesosteps, and so forth.

What happens if mathematician X claims that B follows from A but mathematician Y does not see it? If X and Y have a healthy relationship then X will spell out some intermediate steps for Y's benefit, and will keep increasing the amount of detail until either Y is able to see how to supply the remaining details, or X realizes that there is a problem with the argument. But if X and Y do not have a healthy relationship, then X might lose patience at some point and stop cooperating. That is basically what has happened here, except that a lot more than two mathematicians have gotten involved, and a sizable sub-community has become alienated from the rest of the community.

These kinds of rifts are rare, and it is not just laypeople who are perplexed. Most mathematicians are watching the IUTT fiasco from the sidelines with amazement. But perhaps what we should really be surprised at, and grateful for, is that such rifts don't happen more frequently. Human nature being what it is, sociological rifts are to be expected, and it is a wonder that the mathematical community functions as well as it does.

31. **David Roberts**
March 8, 2021

@W

Luckily Peter Scholze confirmed in the previous extended discussion that he and Stix had internalised and compensated for Mochizuki's hangups that I describe around "copies". My observation is only a very tiny contribution trying to unravel what's going on.

There's stuff in the new document that utterly mystifies me (I've not read or even looked at the whole thing). Section 2.3 for instance is … Not Even Wrong.

32. **André**
March 8, 2021

The 65 page document has a clear message: to understand what IUT is, you need to think about it for 6 months or 3 years (section 1.6), come to Japan (sections 1.6 and 1.9), talk to "M" for a long time, and think about it, discussing with "M", until you understand it. As a result, the worldwide number of people who understand it is of the "order of 10" (section 1.6), centered around Kyoto.

The math examples that "M" gives are so simple that nobody will ever believe that Scholze and Stix stumbled over such issues.

"M" clearly states, page over page, that Scholze and Stix are stupid. In fact, the document states that all his critics are stupid. Above all, the document shows that "M" is abusive towards people of different opinion.

The whole paper is clearly written by an aspiring leader of a sect. There is no need to come to Japan to understand this. I have lived in Japan, and this type of sect leaders is common there. It is sad to see that such people exist in mathematics, and especially sad to see that they exist at Kyoto University, one of the best in Japan.

33. **Peter Woit**
March 8, 2021

André,

I'm not sure that "sect" is the right name for the weird phenomenon going on here, but if it is, this "sect" has not been very successful at gaining adherents. Back in 2018, according to Fesenko, 12-18 people had studied the proof sufficiently to understand it and believe it correct, see
https://www.quantamagazine.org/titans-of-mathematics-clash-over-epic-proof-of-abc-conjecture-20180920/
Two and a half years later, Mochizuki now describes the number of people who understand IUT as "roughly 10".

A weird aspect of this story is that almost no one admits to actually understanding the proof themselves. Back in 2014, Mochizuki described the main people working on checking the proof as Yamashita, Hoshi and Saidi. Many years later, the only documents not from Mochizuki that I'm aware of explaining the proof and addressing the Scholze-Stix argument are from Yamashita. Hoshi has also written something pre-Scholze-Stix in Japanese, and participated in discussions with them. But as far as I can see, there's no evidence that anyone besides Yamashita and Hoshi can publicly vouch for the proof and answer questions about it. PRIMS had some undisclosed number ("several") of referees (quite possibly including Hoshi and Yamashita), but it's unclear how many of them could vouch for the whole proof (as opposed to just checking some piece of the IUT papers).

If this is a "sect", it's a small one, arguably getting smaller.

34. **Peter Woit**
March 8, 2021

Timothy Chow,
I don't think your portrayal of the current situation as due simply to a lack of detail is accurate.

Scholze-Stix not only say that there is a gap, but give an argument for why the gap cannot be closed using Mochizuki's methods. The debate hosted here last year mostly revolved around whether that argument was bullet-proof or whether there might still be some way to get Mochizuki's methods to work. What did not show up here (or anywhere else) was anyone claiming they knew how to do this and could provide details.

It's not the case that Mochizuki has lost patience and stopped engaging with the Scholze-Stix argument. He just put out 65 pages supposedly doing this. The problem is that what he is doing is classic behavior of someone who has lost an argument but won't admit it and wants to fight on. I think those 65 pages will do a lot to convince mathematicians that he does not have a proof. Scholze and Stix are the ones who now seem to have stopped responding, for the good reason that Mochizuki has no serious counter-arguments, just insults.

By the way, I just noticed this from Scholze, in case anyone thinks that the publication might have changed his mind:
https://t.co/0zbGxpgAfy?amp=1

There has also been a long discussion of this at Reddit, see
https://www.reddit.com/r/math/comments/lz4ccm/mochizuki_strikes_again/
I recommend the comments by "whisperfiends"
https://www.reddit.com/user/whisperfiends/
who makes an attempt to follow Mochizuki's claims that Scholze-Stix are confused about something basic, is suspicious that it is Mochizuki who is confused about this.

35. **W**
March 8, 2021

@Charlie G and André:

There exist other works which apply Mochizuki's language to other questions of anabelian geometry. However, my understanding is that this language is not essential, and the works would be shorter and clearer without it.

@The Vlad:

If you look at his old papers like "The local pro-p anabelian geometry of curves", when he was doing work that everyone agrees is correct (and brilliant), you can see some strange formatting, but much less (mostly a lot of italics – also the equations aren't numbered, but that isn't too strange in the more algebraic fields of mathematics).

@Peter Woit

I don't think there will be so much of a problem with people trying to cite the papers but avoid the problematic parts, simply because there doesn't seem to be much worth citing in the unproblematic parts. But there certainly could be a fork in the mathematics community regardless.

@David Roberts

My interpretation of what Peter Scholze says there is a bit different. I just bring your observations up because they are very helpful to me!

Isn't 2.3 also the exact kind of thing you discussed – gluing topological spaces is a colimit of a diagram, and if you define diagrams correctly you can't get into trouble based on whether you identify things or not?

36. **JE**
March 8, 2021

As of now, the English-speaking media have turned their backs on the publication of Mochizuki's papers. In fact, one can hardly find any mention of it other than on this blog or reddit. The situation vastly differs from last year's, when many articles quickly announced their publication. Be it the result of poor communication strategies on the part of the EMS or exhaustion, Mochizuki's attempted proof of the ABC conjecture seems to be a dead issue in Western media's terms. Coupled with his 65-page manuscript, containing plenty of arguments from authority, implicit ad-hominem attacks and appeals to herd behavior, the damage he is inflicting on his reputation by either refusing to accept that the proof is flawed or being able to provide valid counter-arguments is enormous, as Peter said.

And this is really unfortunate, because he has proved to be an extremely talented mathematician. The fact that no senior mathematician or anabelian geometer in Kyoto has been willing or able to convince him otherwise may be hard to grasp by Western standards. Mochizuki is a highly respected mathematician in Japan, who was appointed as editor-in-chief of PRIMS few months before his papers on IUTT were released (and from being excluded from the editorial committee, paradoxical as it may seem), and one of the most prominent figures at an institution with a strong background of success, including Fields medalists Hironaka and Mori, among many other respected figures.

At this point, I seriously doubt that this affair may even create a fork in the math literature. Any figures, other than Mochizuki, who have implicit or explicitly sided with him may slowly backtrack and even take advantage of the media impact of this affair to propel their careers in Japan or elsewhere. The number of mathematicians who have published explanatory reports on his proof, like Yamashita and Hoshi, has even decreased in the last few years. And the highly-respected figures who have been involved in the process (as co-editors-in-chief) have solid mathematical careers and are not facing a reputational risk in any way comparable to Mochizuki's.

37. **David Roberts**
March 8, 2021

@W

The idea that having an equivalent subcategory of Top where there is only a single terminal object means that the somehow [0,1] (called \mathbb{I}) becomes the same as S^1 (called \mathbb{L}) is laughable. The same argument would imply that **every single topological space** collapses to a single point, since every one-element subspace is the "same", according to Mochizuki (as in: why are the endpoints of [0,1]—in more proliferation of useless notation, called \alpha and \beta—singled out? you could replace them by any pair of points of [0,1], or any space). The argument confuses taking a colimit with taking a skeleton, as far as I can tell around the verbiage (why do we need to be reminded of the standard topology on [0,1], and that it is a topological manifold with boundary?)

38. **Timothy Chow**
March 8, 2021

Peter, I pretty much agree with what you say. All I was trying to do was explain to lroberth that "logical disproof" isn't quite the right term. We have seen how Taylor Dupuy has argued that the Scholze-Stix argument isn't, as you put it, bullet-proof. I have privately asked other experts for their opinion, and a common response I get is a shrug and a comment that Mochizuki hasn't provided a clear argument. I think that this behavior (as well as the behavior of Mochizuki's quiet supporters) is more puzzling if there really is a "logical disproof," but it's easier to understand if there is a gigantic gap.

As for "lose patience," perhaps I should have said "lose his cool." Also I didn't say "stop engaging"; I said "stop cooperating."

39. **S**
March 8, 2021

I've noticed an (understandable) resistance to taking Scholze/Stix's word for it, among interested onlookers (mostly non-mathematicians, I think). If I can't engage with Mochizuki's material or Scholze/Stix's objection to it, I should remain neutral, goes the thinking. To these people, I would emphasize the fact that it's been 8 years, and if IUT were actually right, it's overwhelmingly likely that someone would have written it down in an understandable and uncontroversial form by now. The specific objection of Scholze and Stix brings this point into greater focus, because we know exactly which aspect of the theory must be zoomed in on, in order to clarify matters. There is just no plausible explanation for why this hasn't happened yet, except that it can't be done.

Things could change in the future if Mochizuki or someone else provides new mathematical content. But that wouldn't retroactively change the fact that there's no valid proof now.

40. **Jim Eadon**
March 9, 2021

Peter,
The publishing house must surely be aware of the explicity identified error(s) in the proof, identified due to Scholze and Stix.
If they do publish the paper, and they fail to acknowledge the identified error(s), then are they not fraudulent, in some way, for selling fraudulent documents with false advertising?
Could they be accused of (even illegally) misleading consumers? There are legal consumer-rights acts in many countries, where you're not allowed to mislead consumers.

41. **Timothy Chow**
March 9, 2021

I just noticed that on March 18, Mochizuki is conducting an "Interactive Q&A Session on the Essential Logical Structure of Inter-universal Teichmüller Theory." It seems that this session is not open to the public, since the List of Participants page says, "In order to ensure a cohesive and focused working group, note that participation is restricted to a limited number of participants, and that registration is done by invitation only." I wonder if the session will be recorded and made available to the public later. Over 30 people are listed as participating; I wonder if any of them will dare to ask a difficult mathematical question? Failing that, will anyone request that the "mathematicians with whom I [Mochizuki] have had technically meaningful discussions concerning the assertions of the RCS" (saying that Scholze and Stix's objections are "obviously completely mathematically inaccurate/absurd") identify themselves?

42. **Peter Woit**
March 9, 2021

Jim Eadon,
I know for a fact that the EMS Press is aware of the issue, am somewhat mystified by the fact that they promoted this issue of PRIMS and have so far not apparently done anything to address the problem. Likely they have contractual arrangements such that they can't interfere in editorial decisions of PRIMS, but I find it hard to believe there is nothing they can do here.

43. **The Vlad**
March 9, 2021

@W

I took a look at the M paper you mentioned, and even there I find his style and the paper structure highly unusual (e.g. no abstract, acknowledgements section in the introduction, the unnecessary quotation marks and italics everywhere). He also makes some sweeping meta-mathematical statements, such as:

"Moreover, it is the feeling of the author that,more than the technical details of the state-ment of TheoremA, it is this fact – i.e., that the Grothendieck Conjecture for hyperboliccurves is best understood not as a global, number-theoretic result, but rather as a result inp-adic Hodge theory – that is the central discovery of this paper."

Putting all this together, it paints a picture of M abandoning or disregarding standard math communication conventions in favor of his own idiosyncratic style (this also fits with his refusal to travel overseas to convince others of his work). As PW points out, this seems to indicate an inability of mentors, colleagues and.or reviewers to dissuade M from behaving in a way that may harm his reputation.

44. **Peter Woit**
March 9, 2021

The Vlad,

Mochizuki's typographical idiosyncracies are easily ignored, and his not liking to travel isn't unusual. I actually like the "sweeping meta-mathematical statements" that tell you what the author thinks the "big picture" is, often find them helpful to understand what the author is doing. When the papers first came out, they were long and technical, beyond my abilities or patience to try to read. Later Mochizuki came out with
http://www.kurims.kyoto-u.ac.jp/~motizuki/Panoramic%20Overview%20of%20Inter-universal%20Teichmuller%20Theory.pdf
which did give a readable overview of what he was trying to do.

After talking to some experts it became clear that they could understand what he was trying to do, they just couldn't understand whether what he was trying to do really worked, because crucial parts of the argument were unclear and not well explained. Scholze changed everything by pointing to specific place where the argument could not work for general reasons, and most experts are now convinced by that.

So, the real problem here is not Mochizuki's idiosyncracies, it's his behavior when faced with someone pointing out a flaw in his argument.

45. **David Roberts**
March 9, 2021

For what it's worth, I think my previous comment was not quite right. I thought through what was going on in this weird example a bit more in a blog post here. Now I think it's just a straw man argument, at best.

46. **mahmoud**
March 9, 2021

Timothy Chow,
as Scholze repeatedly said himself in prior comments to this blog Mochizuki could easily clear up the issue regarding the Scholze-Stix objection by pointing out where in their argument a non-commutating diagram shows up that in fact will commutate provided one replaces it with one of his "mutually alien copies" diagrams. Instead, however, he keeps putting out new documents with increasingly bizarre analogies (the latest where he substitutes a real number for the integral sign is a new record).

Talking about a "gap" in the proof or the S&S objection as a "logical disproof" muddles what is a quite simple situation: there just is no proof that has been communicated in an intelligible fashion. Thus I also take issue with Woit saying Mochizuki's idiosyncracies can be ignored; in fact his highly idiosyncratic style (of communication and – apparently – thinking about perfectly conventional mathematical topics) has prevented any progress on understanding his purported proof from the very beginning.

47. **DrB**
March 15, 2021

Has someone taken a look at this recent (Nov 2020) "RMS preprint" by Mochizuki et. al.: http://www.kurims.kyoto-u.ac.jp/~motizuki/Explicit%20estimates%20in%20IUTeich.pdf

It is stated that it contains a proof of Szpiro's conjecture and an alternative proof of Fermat's last theorem.

48. **Peter Woit**
March 16, 2021

DrB,
This depends on the flawed proof in the IUT papers, so is also not a proof. It will be interesting to see if any journal other than PRIMS is willing to publish it.

49. **John**
March 17, 2021

I understand where Timothy Chow is coming from. Someone not familiar with academic mathematics might wonder why a complex paper can take so long to verify. The reason is that most papers will have a dozen gaps in them, with the understanding that an expert can easily fill them. So when a mathematician encounters corollary 3.12 his first instinct is not 'this argument is non-sensical' but rather 'I don't have the background required to fill in the gaps'. So, how do errors ever get found? The clearest but most difficult way is to prove that one of the statements in the paper is wrong, this is not something Scholze and Stix did, and it can't find out wrong proofs of true statements either. Another way is to take an example of the things in the statement, one where you understand how to fill in the gaps better, and show that some of the logical implications does not follow in this example, this is less decisive but often does point to a genuine gap. The way Scholze and Stix did if I understand correctly is to introduce modifications to the theory, so they are talking about different objects, but these objects have enough properties in common with the original theory that the arguments seem to follow, but the conclusions are false. There are two ways this can happen, either the argument is wrong in some way it can't be filled easily somewhere in one of those gaps, or the modified theory is different in some important ways (contained in those gaps in understanding). My understanding is that Mochizuki claims the second of course, but he hasn't pointed any 'important' difference, important is a bit of a value judgement, so instead you could say he hasn't pointed to one that helps understand the gaps better. So most people at this point believe the argument to be either wrong or at least not convincing.

What I find surprising about this is that several serious mathematicians seem to be convinced of Mochizuki's proof in ways that they have a lot of trouble communicating, I don't recall anything like that happening before.

50. **Peter Woit**
March 17, 2021

John,
About
"several serious mathematicians seem to be convinced of Mochizuki's proof in ways that they have a lot of trouble communicating"
I'd put it a bit differently. There is literally nothing written down by a mathematician other than Mochizuki engaging with the Scholze-Stix argument and showing that it is wrong, and Mochizuki's engagement with the argument has no credibility.

As far as I know, Yamashita and Hoshi are the only two people besides Mochizuki supposedly understanding the proof and able to provide a counter-argument to Scholze-Stix, and Hoshi has not written such a counter-argument down. Yamashita has written a long survey of the proof, but it makes no explicit reference to Scholze or Stix or

their argument.
Mochizuki's own attempts to engage with Scholze-Stix involve a large amount of absurd ad hominem attacks on their competence, coupled with a refusal to directly reference and deal with the core of their argument. This is the sort of thing you expect from someone who has no real counter-argument. As a non-expert trying to read the more technical parts of Mochizuki's supposed counter-argument, I can't follow the details, but I also can't find anywhere where he engages with the arguments Scholze has made here. He goes to a lot of trouble to not use Scholze's name and not refer directly to anything he has written. Checking with people who are experts to ask if they see something I'm missing I'm told it's not there.

51. **DrB**
March 17, 2021

Perhaps part of the difficulty is that the unwritten rules for acceptance of a proof within mathematics seem to be adhered by. These rules state that the elders need to accept the proof as correct. The elders usually means experts in the field. Unfortunately, as is often the case, the field of Anabelian Geometry is very small in terms of the number of experts. Scholze works in Arithmetic Geometry. Fields medal or not, he is not an expert in Anabelian Geometry. It seems to me people like Fesenko, Yamashita and the anonymous referees that accepted the paper count as the experts. Their verdict counts by the unwritten rules. Who can really say that it is necessary for the experts within a field to address any kind of criticism raised, except to state that the criticism is unfounded based on an insufficient understanding of the theory? If they were in fact obligated to do this it would change the entire paradigm of peer review. Experts would be obliged to address endless amounts of misunderstandings by mathematicians that do not work in the field in question. Publication could be protracted indefinitely as a result. It is pretty clear by now that the system is completely broken. We need objective or objectifiable methods to validate results in mathematics.

Btw that preprint of Mochizuki et. al. that I have mentioned is already being cited in published work:
https://www.sciencedirect.com/science/article/abs/pii/S0022314X2100055X?dgcid=rss_sd_all

52. **Peter Woit**
March 17, 2021

DrB,
Anabelian geometry is a subfield of arithmetic geometry. The "Scholze is just too ignorant to judge work in anabelian geometry, and too incompetent to realize he's out of his depth" argument isn't plausible, and if you do believe that, he was joined in this by Stix, who is an expert in that subfield.

The paper you mention evidently is based on old, pre-IUT work, just mentions the new IUT-based claims, but does not use them. It will be interesting to see if any reputable journal will publish a paper claiming to prove something assuming the flawed IUT-based stuff.

53. **Jens Franke**
March 19, 2021

Dr. B,

while it is obviously reasonable to cover the identity of referees who decide against a paper (eg, to protect them against retaliation), it is less obvious why the same thing should be done for the referees who give their OK to a paper. So maybe this is what at least some journals should do?

For me, someone who OKs a paper should be able to explain it at least to experts of neighboring fields. Also, it makes sense to mention the referees of a published paper to credit them for the work they had to invest. In some cases this is known (eg, Herbert Federer being the referee for the hard case of the Nash embedding theorem). In the case of the series of IUT papers by Mochizuki, the amount of work required to go through it must be huge, even for experts in anabelian geometry. Why not credit them for this effort?

Outside the field of mathematics/theoretical physics, I know of at least one example (Organic Syntheses) who seem to do just that. They did so when they started in the days of world war I and seem to have stuck to this habit ever since.

54. **Peter Woit**
March 19, 2021

Jens Franke,
In this case, having your name made public as a referee who approved the IUT paper might be problematic.

On the other hand, the names of the committee at PRIMS that approved the paper were published with the paper. Those mathematicians agreed to publicly put their reputation behind an approval of the proof. It seems to me that they're the ones who need to provide an explanation. They have decided based on some argument that Scholze-Stix are wrong and the consensus of the arithmetic geometry community is wrong. What is that argument? Is it Mochizuki's argument that Scholze-Stix are incompetent? Is it something else? If it's based on a report from a referee, they should be able to provide the substantive part of that report, stripped of the identity of the referee.

55. **Jens Franke**
March 19, 2021

Peter Woit,

yes they give the names of the committee members but they just say "Several mathematicians kindly accepted an invitation to referee the papers; …". So it does not list anyone who claims to have made his way through the proof. This, of course, is the usual way of doing things. For me, it does not make that much sense, but the only journal I know of which does not have this approach is Org. Synth.

I do not see why it would be problematic to be named as the person who gave his OK to the IUT series of papers, provided that you are able to defend your opinion. After all, it would be a big honour and for most mathematicians it would be the most important thing they have done in their lives.

56. **Peter Woit**
March 19, 2021

Jens Franke,
I think it's extremely implausible that there is an anonymous referee who has a convincing counter-argument to Scholze-Stix, but that everyone involved in this is keeping it secret.

On the other hand, it's unfortunately plausible that the referees agreeing to approve the proof were people who might have reason to fear significant career implications if they took a stand that the proof was incorrect. The proof should have been refereed by an independent journal and independent editors, not by Mochizuki's colleagues at RIMS.

57. **HM**

March 19, 2021

What could be the long-term consequences of such a fork in the mathematics literature? Does it become a political statement whether one assumes ABC?

58. **Jens Franke**
March 19, 2021

Peter Woit,

They speak of “several mathematicians”, so there are several persons possibly involving Fesenko as well. But if PRIMS had the policy of publishing the names of referees giving their OK to a paper, they would not be able to make an exception for the IUT papers and we would know rather than merely suspect.

I now think your “having your name made public … might be problematic” applies to the hypothetical case of a journal J run by persons close a powerful person X publishing an article by X choosing a poor devil D working under X as the referee, with D being practically forced to OK the paper. Obviously, it would be embarrassing for D to be named as a referee. But then, it would be even worse for the editors of J as it would force them to openly admit they picked someone close to X. Would they still dare to do so?

59. **Peter Woit**
March 19, 2021

HM,
Since there are so few mathematicians who accept that Mochizuki has a proof, the impact of this is very limited, essentially a small number of people cutting themselves and their work off from the rest of the math community. It seems likely this is how things will stay. A larger scale split in principle is possible, hard to imagine how that would evolve, I don’t think we’ve seen anything like in modern mathematics.

60. **Peter Shor**
March 19, 2021

Jens Franke:

Occasionally, papers get published despite one terrible referee report. In this case, you really shouldn’t make the names of the person responsible for the negative report public. But I suppose it would possible to make the names of referees who gave positive reports public.

61. **Jens Franke**
March 19, 2021

Peter Shor,

Yes, its the positive ones which should be made public.

62. **Peter Woit**
March 19, 2021

All,
I’d like to discourage this turning into a general discussion of refereeing practices. This is an extremely unusual case, with general issues of referee anonymity I don’t think particularly relevant. In particular, if the referee’s names were known I doubt that would change anyone’s mind about the viability of the proof. What would change minds would be a serious mathematical counter-argument to Scholze-Stix, from any source.

63. **Mizan R Khan**
March 19, 2021

A result published in a good journal does not necessarily mean that it is accepted by the general mathematical community. A good example of this phenomena is the work of Heegner on the the class number 1 problem. He published his proof in 1952, but it was only in the late 60’s that the number theory community acknowledged that the proof was essentially correct. This was through the efforts of Birch and Stark. Without their intervention, it is conceivable that Heegner’s work would still be viewed as a failed attempt at proving the class number 1 problem.

64. **David J. Littleboy**
March 21, 2021

FWIW, a quick Google revealed a short article in the Asahi Newspaper* March 5, 2021 digital edition on the Mochizuki question. A quick and dirty translation:

“A paper by Prof. Mochizuki of Kyoto University that is said to prove the abc conjecture has been published on March 5th after a consecutive period of 8 and a half years since its submission. The publisher, the EMS publisher, has responded to Asahi Newspaper inquiries. It will be published first in electronic form and then in printed form a month later by PRIMS, an international mathematical journal that publishes mathematical research”

“The paper is titled (title here) and since it comes to a total of 720 pages of English text, PRIMS will dedicate a single issue, which normally would consists of five or more papers, to this paper. Since the paper is extremely difficult and there are mathematicians who doubt the correctness of the proof, EMS is taking this opportunity to make this work more widely known.”

I find that last sentence rather interesting. (The Japanese does not use quote marks, and uses a Japanese abbreviation for EMS (without explaining it), but it sure looks to me that EMS told the Japanese newspaper that there were doubts about the proof.)

*: https://www.asahi.com/articles/ASP355S0CP35ULBJ00Q.html

65. **AZ**
March 22, 2021

About the Asahi article, the part about mathematicians doubting the proof, it says that RIMS (数理研; not EMS) is taking the opportunity to make this work more widely known. The part about a response from EMS Press in the first paragraph only concerns the fact that it will be published electronically one month before the printed version.

The article is actually longer. In one part it says that the papers are so novel/eccentric and hard to understand that even mathematicians say “they don’t understand where they don’t understand” them, as if the papers “came from the future”. A well-known mathematician has also said “there is an uncorrectable gap in the way the proof proceeds”, therefore the verification took the unusually long time of seven and a half years to complete.

This is only the public part; the rest of the article is behind a paywall and I haven't tried to access it.

66. **Dan Winslow**
March 22, 2021

From my read they are definitely still saying that it's considered 'proved', it just took longer because of that pesky mathematician.

67. **TonyG**
March 22, 2021

Actually, the May 3, 2020 issue of the same newspaper had a story quite a bit more critical of the claim than the recent one: https://tinyl.io/3lQo

68. **Per Östborn**
March 22, 2021

Found a Japanese article which says (in Google's translation):

'Some overseas mathematicians are skeptical about the content, but Professor Akio Tamagawa of the institute, who was involved in the editing, said, "The counterarguments have been exhausted and may remain parallel in the future." I hope that young researchers will read the treatise seriously and that subsequent research such as improvement, generalization, and application will appear.'

https://this.kiji.is/740458703363735552

As a layman unable to make a judgement of my own, I wonder why nobody seems to discuss the technical third part of Mochizuki's document. If the treatment there is correct, then the simplifications made by Scholze and Stix are invalid, and their objections irrelevant to the validity of Mochizuki's claimed proof (if I understand the structure of the arguments correctly).

Is this because the content of the third part is 1) wrong, 2) meaningless/irrelevant, 3) incomprehensible, or 4) nobody thinks it's worth the effort trying to understand it?

69. **Peter Woit**
March 23, 2021

Per Östborn,
I don't want to speak for others and I'm not an expert, but when I look at Mochizuki's third part all I see is what seems to be a repetition of the argument of his papers, with no attempt to engage at all with Scholze-Stix. He doesn't refer anywhere in that section to them, to anything they've written or said to him explaining the problem. In particular Scholze has repeatedly asked for something very specific that Mochizuki should be able to produce if their argument is wrong (a diagram with certain properties). No one else has been able to produce this, and Mochizuki just ignores the question (along with all the rest of the substance of what Scholze is saying).

70. **Greg Price**
March 31, 2021

In that Asahi article from last year, it's also interesting that they give it quite an explicitly geographical framing. The headline is:

ABC conjecture: "Is the proof real?"
In the West, objections one after another

And that accurately captures the framing in the first paragraph, which says that on the subject of the paper, "mainly in Western countries, the discussion has become 'has it really been proved?'".

(Or one might read "Europe and America" what I've translated as "the West" or "Western countries" — the original says 欧米, which literally means the former but commonly means the latter.)

71. **Taro Nakano**
April 2, 2021

In the April 3 issue of the Japanese business magazine Weekly Diamond, Professor Fumiharu Kato of the Tokyo Institute of Technology wrote about the reasons for the "misunderstanding" surrounding IUT. He is a researcher who has regularly held seminars with Mochizuki to discuss IUT and is known as an evangelist of IUT in Japan.

This article is a column for the general public and has no technical content, but I have translated it into English for your reference.

https://tar0log.tumblr.com/post/647331299812638720/column-by-prof-fumiharu-kato-on-the

The researchers around the Kyoto University group who support the Mochizuki paper do not often mention that the paper has been questioned overseas. I think this column is a rare example of a supporter of the Mochizuki paper speaking out about the controversy.

72. **Peter Woit**
April 2, 2021

Taro Nakano,
Thanks. Kato is just following Mochizuki in his claim that Scholze and Stix are just incompetents making elementary mistakes. This is not plausible or a serious response to their argument.

73. **Taro Nakano**
April 2, 2021

FYI, I translated into English two articles published by the Asahi Shimbun at the stage when the Mochizuki paper was accepted in 2020 and published in 2021. I have translated the entire articles including the paid part.

– May 3, 2020, Asahi Shimbun article, "ABC Conjecture: 'Is the Proof True?'"
https://tar0log.tumblr.com/post/647382973188112384/may-3-2020-asahi-shimbun-article-abc

– March 5, 2021, Asahi Shimbun Article, "Proof of the ABC Conjecture Finally Published"
https://tar0log.tumblr.com/post/647386711006035968/march-5-2020-asahi-shimbun-article-proof-of

74. **Winnie Pooh**
April 13, 2021

@Peter Woit:

You've complained several times already about Mochizuki failing to refer to Scholze & Stix by name in his paper. I think there's a misunderstanding here about Western vs. Asian culture that needs to be cleared up. Source: I'm Asian.

In Asian culture, it is considered impolite to point your finger at somebody, or even gesture in their general direction. Similarly, when you want to point out that somebody else is wrong (as Mochizuki does with Scholze & Stix), it is considered more polite to do so in an oblique, indirect way.

Ultimately, this is all about "saving face", an important concept in Asian culture. What Mochizuki basically does with his "no names" criticism is (1) point out that Scholze & Stix are wrong (or so he believes), while still (2) minimizing the number of people who know who exactly he's criticizing, and thereby minimizing the damage to the "social standing" of Scholze & Stix. At least that's the theory and rationale behind "saving face" and "no names". Whether it works in this particular case is of course another matter.

An example that might be easier for Western people to wrap their heads around is when you criticize somebody in public (which will damage their public reputation) vs. when you criticize somebody in private or at least in a small group (= minimal damage to their public reputation).

To summarize, Mochizuki's "no names" criticism is probably a form of Asian politeness, not an attempt to deprive Scholze & Stix of deserved recognition.

75. **Peter Woit**
April 13, 2021

Winnie Pooh,
Many people seem to want to attribute the problems with Mochizuki's response to criticism of the abc proof to cultural differences, but I don't see that at all (although maybe you could argue that culturally-determined unwillingness to embarrass a colleague explains why people at RIMS went along with publication of the defective proof). Looking at everything Mochizuki has to say about this, the problem is that he refuses to actually engage with the mathematical arguments Scholze and Stix are making, to the extent of claiming that everyone he has talked to agrees with him that they have no substantive argument. I don't want to psychoanalyze him, but it looks to me like this refusal to engage with their argument (because he has no counter-argument) has a lot more to do with why he refuses to write their names or link to the document with their argument than any cultural explanation.

I sometimes see people suggest that the problem here may be Mochizuki's unfamiliarity with the English language or Western culture. For those unaware of his background, note that from age 5-25 he lived in the US, and was educated at Philips Exeter and Princeton (and was a postdoc at Harvard).

76. **Winnie Pooh**
April 15, 2021

@Peter Woit

Fair enough, and thanks for your answer. I only commented to provide an alternative angle, not to start a pointless psychoanalytical debate. In the end, who knows what the man is thinking.

Another angle that occurred to me after I wrote my first post is that maaaaybe Mochizuki feels deeply offended by how Scholze & Stix handled the situation, and now does what the stereotypical Japanese person seems to like doing when offended, which is to clam up and passive-aggressively refuse to acknowledge the other person's existence. I think this is more in line with how you view his behavior.

As to how Scholze & Stix could have managed to offend Mochizuki, maybe he felt he wasn't given enough time to explain himself before Scholze left Japan? Of course that would be Mochizuki's side of the story. Whether he has any right to feel offended is another matter.

Anyway, both feeling offended and the ensuing passive-aggressiveness strike me as fairly "stereotypically Japanese" (and also somewhat typically Asian, for that matter), so I still feel there's a cultural component to all this.

77. **Winnie Pooh**
April 16, 2021

@Peter Woit

After giving your reply some more thought, I now realize that we seem to fundamentally disagree on one important point:

If I understand your position correctly, what you are suggesting is that deep down Mochizuki knows he's screwed or at least in a very weak position, and now he and his buddies are trying to cover it up / pretend it didn't happen. That would make this a case of deliberate deception, and would make Mochizuki the Donald Trump of mathematics (if you squint hard enough).

But honestly, I don't see this at all. After rereading the Mochizuki passages you quoted, I get the impression that, even if his attempted proof turns out to be a dud in the end, and even if he has no substantive counter-argument for Scholze & Stix, right now he still seems genuinely convinced that he's right, or at least "essentially" right. So under this interpretation the worst one could accuse him of is being a delusional cult leader, but not one who's actively lying.

78. **Peter Woit**
April 16, 2021

Winnie Pooh,
I don't think this is deliberate deception on Mochizuki's part. In my (extensive) experience with very smart people unwilling to admit that an idea they've worked on for decades is flawed, all evidence I've seen is that the human capacity for self-delusion under such circumstances is an essentially infinite quantity.

New Spaces in Mathematics and Physics

March 22, 2021
Categories: Book Reviews

Available online today (if your institution is paying…) from Cambridge University Press are two volumes well-worth spending some time with: New Spaces in Mathematics and New Spaces in Physics. These contain write-ups based on a workshop organized back in 2015 by Mathieu Anel and Gabriel Catren, the videos of which are available here.

It would be hard to write in any detail about the wealth of material in these volumes, so I'll mainly just link to the essays that seemed especially interesting to me:

- Microlocal analysis and beyond by Pierre Schapira.
- Spaces as infinity groupoids by Timothy Porter.
- Sheaves and functors of points by Michel Vaquié.
- Stacks by Nicole Mestano and Carlos Simpson.
- The geometry of ambiguity: an introduction to the ideas of derived geometry by Mathieu Anel.
- Geometry in dg-categories by Maxim Kontsevich.
- Noncommutative geometry, the spectral standpoint by Alain Connes.
- Supergeometry in mathematics and physics by Mikhail Kapranov.
- Derived stacks in symplectic geometry by Damien Calaque.
- Twistor theory: a geometric perspective for describing the physical world by Roger Penrose.

For several decades now one often hears from prominent theoretical physicists that "Space-time is doomed", to be imminently replaced by something new coming out of the latest ideas about fundamental physics. For a long time the claims of this sort getting the most attention were from string theorists, and in these volumes Marcos Mariño explains these in his Stringy geometry and emergent space. More recently, Nima Arkani-Hamed has been making well-publicized claims along these lines, with space-time to be replaced with volumes of objects in Grassmanians such as the amplitudehedron.

A large fraction of the theory community is now working on things like "it from qubit", which propose to somehow get space-time emergent out of things like qubits or quantum information theory. For most of this kind of thing, I've found it hard to figure out exactly what the proposal is for the more fundamental objects from which space-time is supposed to emerge. One recent extreme proposal, by Sean Carroll, has the virtue of specifying what the object is (a self-adjoint matrix acting on a complex vector space), but I don't think there's a plausible route from that to our observed physics.

As many of the articles linked to above should make clear, mathematicians have over the past centuries developed a range of deep and surprising ideas about new sorts of ways to think about space and geometry. This activity continues: Peter Scholze's perfectoid spaces and condensed mathematics are examples of new directions of this kind, too new to make it into these volumes.

Of all of these ideas, the ones that at the moment I find most compelling are the twistor geometry ideas of Roger Penrose, and I'll have much more to say about those in another blog post soon.

Comments

1. **Paolo Bertozzini**
   March 22, 2021

   Dear Peter,

   great news 🙂 I have been really waiting to read these two volumes. I personally think that there is a great need to look into such mathematical structures (mostly from (higher) category theory and (algebraic) non-commutative geometry) in order to define an appropriate notion of "geometry", "localization" and "holonomy" adapted to quantum physics. I feel that currently there is really a big and unjustified hype about *emergence* of space-time (a "fashion" even stronger than certain propaganda on strings and related ideas, since it is shared by essentially all of the competing lines of research on quantum gravity): everything geometrical is considered to be a macroscopic feature *emergent* from non-geometrical or pre-geometrical phases. There is something that I find quite weird and wrong about such ideology of emergence: four-dimensional Lorentzian manifolds might well turn out to be some "large-scale limit", but this does not mean that a geometrical description at the quantum level is impossible and uninteresting (and these developments in mathematics seem exactly to suggest the contrary).
   Best Regards.

2. **More Anonymous**
   March 22, 2021

   Dear Peter,

   Any book recommendation for twistor theory at physics postgraduate level? I've been quite mystified as to what's going on there.

   Thanks

3. **David Roberts**
   March 22, 2021

   A totally minor and irrational thing: the cover designs are not consistently laid out between the two volumes!

   More constructively, there are pointers to relevant pages, talk videos and links to some arXiv/draft versions at
   https://ncatlab.org/nlab/show/New+Spaces+for+Mathematics+and+Physics

4. **John Baez**
   March 23, 2021

   Wow, it's out! I've got a chapter in here, and I've been waiting for it to come out for many years. It's a good thing I read your blog.

5. **ay**
   March 24, 2021

   Way off topic (no need to post) but there's a new image out on the black hole you show above the blog.

https://www.space.com/first-black-hole-image-polarized-m87

6. **Peter Woit**
   March 24, 2021

   ay,
   Thanks. When I get a chance I may update the image that appears on the blog header.

7. **iRex**
   April 3, 2021

   Just to let you know strings are still making headlines:) https://www.theguardian.com/science/2021/apr/03/string-theory-michio-kaku-aliens-god-equation-large-hadron-collider

The Future of Fundamental Physics

March 24, 2021
Categories: Uncategorized

IAS director Robbert Dijkgraaf will be giving the CERN colloquium tomorrow, with the title The Future of Fundamental Physics. Here's the abstract:

> The reports of the death of physics are greatly exaggerated. Instead, I would argue, we are living in a golden era and the best is yet to come. Not only did the past decades see some amazing breakthrough discoveries and show us the many unknowns in our current understanding, but more importantly, science in general is moving from studying `what is' to `what could be.' There will be many more fundamental laws of nature hidden within the endless number of physical systems we could fabricate out of the currently known building blocks. This demands an open mind about the concepts of unity and progress in physics.

I don't know of any "reports of the death of physics", but there are a lot of reports of the death of string theory (Dijkgraaf's specialty) and of the larger subject of attempts to go beyond the Standard Model, experimentally or theoretically. CERN yesterday announced new results from LHCb testing lepton universality (a prediction of the Standard Model). LHCb sees a ratio of decays to muons vs. electrons in a certain process that is off from the Standard Model prediction by 3.1 sigma.

If this result is confirmed with better data and careful examination of the theory calculation, that will be a dramatic development, indicating a significant previously unknown flaw in the Standard Model. BSM theory and experiment would be very much undeniably alive (no known relevance of this though to the troubles of string theory). Unfortunately, the experience of the past few decades is that 3 sigma size violations of Standard Model always go away after more careful investigation (see for instance the 750 GeV diphoton excess). It's exactly this pattern that has people worried about the health of the field of high energy physics.

Dijkgraaf's claim that "we are living in a golden era" is an odd one to be making at CERN, which has seen some true golden eras and is now facing very real challenges. Even odder is arguing at CERN that the bright future of science is due to it "moving from studying `what is' to `what could be.'" CERN is at its core a place devoted to investigating "what is" at the most fundamental level. I'm curious to hear what those at CERN make of his talk.

Dijkgraaf's abstract to me summarizes the attitude that the best way to deal with the current problems of HEP theory is to change the definition of the goals of the field, thereby defining failure away. The failure of heavily promoted ideas about string theory and supersymmetric extensions of the Standard Model is rebranded a success, a discovery that there's no longer any point to pursue the traditional goals of the subject. Instead, the way forward to a brighter future is to give up on unification and trying to do better than the Standard Model. One is then free to redefine "fundamental physics" as whatever theorists manage to come up with of some relevance to still healthy fields like condensed matter and hot new topics like machine learning and quantum computing. I can see why Dijkgraaf feels this is the way forward for the IAS, but whether and how it provides a way forward for CERN is another question.

Update: I just finished watching the Dijkgraaf talk, together with the question session afterwards. Dijkgraaf basically just completely ignored HEP physics and the issues it is facing. He advertised the future of science as leaving the river of "what is" and entering a new ocean of "what can be", with the promising "what can be" fields biotech, designer materials and AI/machine learning. He hopes that theorists can contribute to these new fields by trying to find new laws governing emergence from complexity, perhaps via new ideas using quantum field theory tools.

With nothing at all to point to as a reason to be optimistic about HEP, a couple questioners asked whether his river of "what is" might be now hitting not an ocean but a desert, and he didn't have much of an answer. All in all, I'm afraid that the vision of the future he was trying to sell is not one in which high energy physics has any real place. It fits well with the depressing increasingly popular view of the field, as one which had a great run during the twentieth-century, but now has reached an end.

Update: For more discussion of the reliability of the LHCb result, see comments here and here, as well as Tommaso Dorigo's blog post.

Update: Tommaso puts his money where his mouth is.

Comments

1. **Marcus C Thomas**
   March 24, 2021

   Dijkgraaf seems to jump around a bit beginning with "fundamental physics" leaping into "science in general" and then ends with what seems to be a philosophical reference to "unity in physics." Buried within these wild leaps is what appears to be the primary aim of his comments, "science…is moving from studying `what is' to `what could be.'" His point seems to be to justify the trend of highly speculative model-building ("what could be") rather than methodologies that are testable and rely on evidence ("what is").

2. **Anon**
   March 24, 2021

   Hi Peter,

   There's not really a theory calculation to be examined for R_K, the Standard Model just gives unity. Of course theory goes into the simulations used to perform parts of the analysis, but this is generally reliable for these use cases.

   What you say about 3 sigma excesses is of course true but what's interesting here is that in almost doubling the data sample from 5 to 9 fb^-1 the significance increased. If it were a fluctuation one might expect it to gradually reduce as more data is added.

   https://lhcbproject.web.cern.ch/Publications/p/Directory_LHCb-PAPER-2021-004/FigS5.pdf

3. **Peter Woit**
   March 24, 2021

   Marcus C Thomas,

   I think the "what is" versus "what could be" distinction Dijkgraaf is making is not one of testable vs. speculative. His next sentence "There will be many more fundamental laws of nature hidden within the endless number of physical systems we could fabricate out of the currently known building blocks" indicates that he has is mind more
   "what is": the elementary particles and interactions (e.g. the SM).
   "what could be": complicated systems with new emergent behavior that we can make out of elementary particles.

   Dijkgraaf and the theorists at the IAS are basically quantum field theorists, take QFT as "fundamental" and are interested in finding new ideas about QFT and new things to do with it. To me he's basically saying the time for trying to find something new about elementary particles using QFT is over, the future is in new QFT ideas that could describe emergent behavior in complex systems, with practical implications.

4. **Peter Woit**
   March 24, 2021

   Anon,
   Thanks!

   Is there another experiment able to compete on this measurement (Belle II)?
   Two competitors seeing the same thing tends to be more convincing than one experiment's error bars.

   As a theorist, other evidence would be a compelling model that explains this as well as other anomalies. I gather the ambulance chasing has already started, will wait and see what that leads to.

5. **Anon**
   March 24, 2021

   Hi Peter,

   For this particular measurement yes it's Belle II. There are other similar ratios which also have tensions with the Standard Model and can likely be measured by the other LHC experiments. For example:

   https://hflav-eos.web.cern.ch/hflav-eos/semi/spring19/r_dtaunu/rdrds_rds_spring2019.pdf

   Plenty of ambulances have been chased already (see the references in the paper). Compelling is subjective, but they're reasonably conventional BSM theories like leptoquarks or Z'.

6. **Low Math, Meekly Interacting**
   March 24, 2021

   I was also under the impression that the LHCb excess has a more encouraging pedigree than the diphoton excess, which reportedly seemed from the outset about as random as it turned out to be.

   But, as with other BSM ideas and their myriad possibilities, there are diverse ways to conjure up leptoquarks. Maybe too many. A sense of plausibility, or what about the SM is crying out for that particular addition is lacking almost entirely from the popularized accounts that I've been able to find. Are mass differences between the generations enough of a reason to expect anything like it? And so on. Seems unwise to get too excited, or to crank up the siren on the ambulance just yet.

7. **Thomas Larsson**
   March 24, 2021

   Dijkgraaf and myself belong to the same generation and were, I believe, shaped by the same event: the success of CFT in 2D statistical physics (he is of course also shaped by the first superstring generation, something I never was because I didn't understand it). Although Dijkgraaf made real contributions and comes from the Netherlands, the Mekka of statphys, whereas the train had already left the station when I figured out what was going on.

   In the context of 2D statphys it does make sense to talk about what can be and what can not. Here, we know that some things are (the Ising and somewhat more complicated models), but we also know what can be (the discrete unitary series) and what cannot be (values of c inbetween). This is a fundamental success which I think is underappreciated, even if 2D statphys has admittedly a limited domain of applicability.

   Alas, applying a similar attitude to SUSY, which does not have the underpinning of a theory which is mathematically deep and empirically successful, makes no sense to me.

8. **Peter Woit**
   March 24, 2021

   Thomas Larsson,
   I think you're right that Dijkgraaf's point of view has been shaped by starting out in 2d CFT/string theory back in the mid 1980s. He started on his Ph.D. around the time of the first superstring revolution of 1984. At the time the excitement about string theory was driven by hopes to get a unified theory out of it, something that never worked out. From the beginning Dijkgraaf worked not on this failed project of string theory unification, but on doing other things (e.g CFT, TQFT) often motivated by string theory, that had applications elsewhere (2d stat mech, topology). So, to the extent that his current point of view is that one should give up on unification and find QFT applications elsewhere, it's consistent with his work from the beginning.

9. **John Baez**
   March 24, 2021

   Dijkgraaf says:

   > "There will be many more fundamental laws of nature hidden within the endless number of physical systems we could fabricate out of the currently known building blocks."

   This sentence seems to be talking about condensed matter physics. I'd be really happy if Dijkgraaf openly admitted high-energy physics has been stagnant for decades, while condensed matter physics is full of exciting discoveries. I'm trying to convince young theoretical physicists of this myself. But he seems reluctant to come out and say this openly.

   I'm also not sure what his definition of "fundamental laws" is here, or "fundamental physics". If he wants to redefine them to include the approximately true but mathematically deep laws governing some condensed matter systems… well, there's a case to be made for that. But I think to do it he should come out and say the words: "condensed matter". Otherwise it's all rather mysterious.

10. **Sabine**
    March 25, 2021

    Since I am presently reading Chiara Marletto's new book about counterfactuals, maybe that's what he is referring to with the "could be"? The argument is in a nutshell that what you can do with a system is an important property of the system in and by itself. Not sure what Dijkgraaf has to do with that though, so maybe it's not what he meant.

As to the 3.1 sigma. They need some semi-discovery that the present collider just about can't confirm to argue we need a bigger collider. Might be this or another one.

Oh, and just in case any young people are reading this, let me add I agree with John about condensed matter physics. It's interesting and cool and it's somewhat unfortunate that it has a reputation of being somewhat dull. There's much to be done there and almost certainly a better research direction at the moment than high energy physics.

11. **Wandering mudhen**
March 25, 2021

On the 3.1 sigma excess in B+ > K+ e+e-… the detector is not e/mu symmetric at all… very, very different acceptance/efficiencies. And electrons emit bremmstrahlung. So the observed number of K+e+e- events is a factor of 2 to 3 smaller than K+mu+mu-, and K+e+e- has a broader signal shape, and extends over more backgound.

Then simulation is used to correct back for that… and LHCb claims (systematically) 1% error in that correction.

In, say, 1970, this claim would not be accepted. But simulation tools are far more powerful today.

K+e+e- ends up being quite a bit nearer to the floor of background due to lower acceptance*efficiency and bremm smearing.

The kaon system has an awful lot of higher resonances that were part of the particle explosion of the 1960's, prior to acceptance of quarks… the phenomenology of those resonances is hardly airtight. Predicting the coupling of those resonances to the bag of partons the appears after the quark decay of b to s l+ l- seems to me to be a big challenge.

Then if some of the decay products of the higher resonances are missed by LHCb, they'd appear as background under the bremm tail of the K+e+e-. Getting that background wrong by a few percent would put them down to 2 sigma, and 10-20% would explain the entire effect.

So, either lepton non-universality, or, difficult to model regge-pole type resonances from the 1960's… which would Carl Sagan say needs the more extraordinary evidence? But good that LHCb is getting out near the envelope of what we understand.

12. **Peter Woit**
March 25, 2021

Wandering mudhen,
Thanks for the cautionary comments. I think it's correct to summarize the situation as "this is an extraordinary claim, with not now extraordinary evidence".

13. **André**
March 25, 2021

Peter,

you write "It fits well with the depressing increasingly popular view of the field, as one which had a great run during the twentieth-century, but now has reached an end." Sorry, HEP will reach an end only if there is no physics beyond the standard model (which indeed seems more and more likely) AND once the origins of the parameters of the standard model are understood.

The origins of the parameters are still in the dark, completely. In your blog I have never read about any attempt to explain their origin – neither in your posts nor in the comments. This needs to change. There is still fun ahead, even if there is a desert and nothing beyond the standard model.

14. **Peter Woit**
March 25, 2021

André,
I don't disagree. What Dijkgraaf is doing is refusing to admit that string theory has failed, trying to act like it was a success, providing us with a "new way of doing science". This unfortunately discourages anyone from continuing to try and solve these problems for real.

15. **Alessandro Strumia**
March 26, 2021

"We are living in a golden era and the best is yet to come": all Standard Model particles have been seen, LHC discovered no new physics, no next collider is underway because costs got so high and no smarter muon collider has been planned. These multiple issues suggest that maybe we are living in the historical era where hep-ex reached its end.
I wonder if this possibility can be openly discussed, given that members of the last center that explores high energy "shall conduct themselves with due regard to the interest and proper functioning of the Organization", and "refrain from any act of activity … which would be morally or materially prejudicial to the Organization" and "to express their personal opinions on matters connected with the functioning of the Organization or its activities shall first obtain the written authorisation of the Director General".

16. **Constantin**
March 26, 2021

I think there is little to no possibility of having a truly open community discussion onn the future of HEP as it must involve both thinking and voicing "the unthinkable" (which means different things to different people).

17. **Jose Alvarez**
March 26, 2021

CERN would be well advised to invite younger physicists who did not grow up believing that the LHC would be the answer to the future of the field. For example, there are a number of active efforts initiated by theorists that have lead to new experimental initiatives to probe a variety of dark matter candidates. Almost all of these initiatives are driven by folks under the age of 45.

At the end of the day, none of the grand challenges of particle physics have actually gone away. It is abundantly clear that specific directions that have been religiously pursued such as string theory and low energy supersymmetry have gone nowhere. The junior folks, especially in phenomenology, pretty clearly understand this. If CERN wants to hear about the future of the field, they would be better off inviting these folks to give such seminars rather than people who have been cheerleaders for failed efforts.

18. **Patrick**
March 26, 2021

Wandering mudhen, you describe the challenges of the measurement well. That's why it takes so much time to update this measurement. Once we understand the simulation, we test the measurement at the J/psi resonance and get 1, as expected from electromagnetism. As we have 100 times more data there we can plot that ratio against any variable of interest. See Figs 3,9,10 of https://lhcbproject.web.cern.ch/lhcbproject/Publications/LHCbProjectPublic/LHCb-PAPER-2021-004.html : it's flat. In particular the opening angle of the leptons is crucial. The bottom line is that the B is so much boosted at the LHC that differences in the B frame between the J/psi mass range and the signal 1-6 GeV range are washed out. Once the J/psi is understood, we test the Psi(2S) and get 1 again. Only then did we look at the dilepton mass range of interest.

For the K resonances, they affect the signal region and the Psi regions similarly, so we can test our understanding on the control samples. But indeed we improved our modelling of these backgrounds compared to the previous analysis. The net effect on RK is negligible.

19. **Tommaso Dorigo**
March 27, 2021

Hi,
an Anon above seems impressed that the significance of the R excess grew over time. This is a very common circumstance for null results when experimentalists underestimate their systematic uncertainties. The latter may come from a number of sources, from underestimates of intervals of confidence of internal parameters used in the inference-extraction procedures, to subtle detector effects. We have seen so many of these over the past 40 years that it is just bad propaganda to claim that the growth of a significance is additional evidence for the effect being genuine.
Cheers,
T.

20. **Krzysztof**
March 27, 2021

On slide 13, R. Dijkgraaf claims that the graviton was proposed in 1915 and discovered in 2015.

Well… all this presentation looks like some slick corporation show-case.

21. **Peter Woit**
March 27, 2021

Krzysztof,
I noticed that during the talk, thought it was quite strange for that mistake to be there.

22. **Tommaso Dorigo**
March 27, 2021

Hi again Peter,

so the comment by Anon stimulated me to write about systematic uncertainties and other things, and how one should still be very careful with anomalies that "grow with the data" – https://www.science20.com/tommaso_dorigo/another_3_sigma_fluke_from_lhcb-253707

Cheers,
T.

23. **Tommaso Dorigo**
March 27, 2021

And thanks for linking it from the post!
Cheers,
T.

24. **Lorenzo Di Pietro**
March 29, 2021

Before the talk: you are disappointed because you predict he is going to oversell the future of HEP. After the talk: your prediction turned out to be wrong, but somehow you are still disappointed.

25. **Peter Woit**
March 29, 2021

Lorenzo Di Pietro,
I wasn't predicting Dijkgraaf would oversell the future of HEP, was wondering how HEP fit into his argument that science is moving away from "what is" questions. On the LHCb announcement, there is some danger that's oversold, but that's a separate question, and I also had no idea if Dijkgraaf would mention that.

I was surprised that Dijkgraaf's talk to the CERN audience dealt with the future of HEP by not mentioning it at all, essentially implying that it has no future worth talking about.

26. **Doug McDonald**
March 29, 2021

Speaking of bets and leptons, are there any bets, to be decided very soon, on what Fermilab is about to say about muon g-2? Bets on the number itself, or its standard deviation? Or even guesses?

27. **Peter Woit**
March 29, 2021

Doug McDonald,
I've seen no bets. I'm a little surprised expectations are so high about this. Maybe I'm wrong, but my impression was that this upcoming announcement is going to be about their Run 1 data, which was of similar size to the earlier Brookhaven experiment. So, no expectation of significantly better precision or more conclusive (5 sigma level) ruling out of the SM prediction. We'll see soon…

28. **Sundar**
March 31, 2021

Dear Dr. Woit: https://en.wikipedia.org/wiki/Lepton#lepton_universality_anchor says that the Babar and Belle experiments too found similar deviations from lepton universality just like LHCb did, but the statistical significance did not meet the 5 sigma threshold. When 2 other experiments possibly found deviations from the Standard Model prediction, does the latest LHCb announcement sound so improbable?

29. **Peter Woit**
April 1, 2021

Sundar,
The problem is that there are a large number of possible decays you can measure and check for lepton universality, and if you measure a lot of them, statistically you expect to see deviations in some of them, even if lepton universality is correct. You need something more than a random-looking pattern of some decays satisfying lepton universality, some not, at levels not big enough to be convincingly significant.

30. **ML**
April 6, 2021

Hi,

the talk is a typical Dijkgraaf for the public ears – saying a lot without saying much. I like the more honest ones when Robbert is a cheerleader for stringy maths. And don't get me wrong, I like him very much. He's always been for experiment and testable consequences to a theory next to other more stringy things.
However, I am not sure what the point was with the ocean of what could be. I imagined first RD would talk about the (endless) possibilities coming out of strings and the likes – and perhaps discovering exactly the five centimeters next to the map. But then he explained how our knowledge leads to the engineering of what there could be. Which plays well with stringy engineering, but needs the very important addition that it's purely theoretical knowledge. RD but was talking about "unexplored physical phenomena" without any qualification on their kind. And so, the HEP community would hear that they may continue on their track of smashing what there is to discover what there could be. And the theoreticians would hear that they might continue with playing around on and with their models deriving what they could lead to. That is but a description of the current (and much criticized) state of this particular areas of physics today.

It was announced as a talk about the "future" but it was rather a motivational speech to those whose hope is fading into the shadows of other exciting topics in science that are not about fundamental physics.
I remember seeing a Strings'XX talk by Strominger about the "fun" and adventures on the sea, string theorists were sailing like Magellan into unknown shores. And certainly the discoveries will be similarly significant. And perhaps they already are, but not in the way of the actual 'golden era' of HEP. They have not discovered the promised new continents so far but saw many Magical things and discovered new Monsters.
The golden era ships were sailing along the river, the shores always in sight. Out into the ocean, let's not forget, many sailors got lost and eaten by the monsters.

It might be helpful to step from the sailing ship for a moment on to the ground. To look for the five centimeters or something else.

And btw – does anyone know who's the inquirer at 1:18:18 ? 🙂

Cheers, M

Twistor Unification

April 1, 2021
Categories: Euclidean Twistor Unification

I've finally finished writing up a new version of some ideas that I first wrote about here last summer. The latest draft is here, I may set up a web page with more info here.

Several people had very helpful comments on what I wrote last summer, especially in pointing out that I wasn't providing sufficient justification for the most radical claim I was making, that the problems with analytic continuation of spinor fields indicated that one could interpret one of the Euclidean space rotation group SU(2)s as an internal symmetry. I then spent a lot of time mastering aspects of Euclidean QFT I had never properly understood. Section two of the current paper is the result. It's in some sense quite elementary, people may find it of independent interest, even if you're not interested in the ideas involving twistors. Section three, an exposition of relevant aspects of twistors, is pretty much unchanged. Section 4 is an outline of the ideas about how to get a unified theory out of twistors, much there is still sketchy. I understand a lot better than last year how what I'm proposing fits into some standard ideas about "chiral" formulations of gravity, also have learned a bit more about previous attempts to formulate chiral gravity and gauge theory on twistor space. Some highly speculative remarks that this might all be somewhat related to N=4 super Yang-Mills have been added.

Here's a little bit more here about the hardest to believe claim being made (about analytically continuing spinors). The standard assumption (this is what I always thought) has been based on the analytic continuation behavior of correlation functions: Schwinger and Wightman functions are analytic continuations of each other, and one might think there's nothing more to analytic continuation between Euclidean and Minkowski space theories. After learning more about the Euclidean QFT literature, I was struck by how different this is from the physical Minkowski space formalism: states and fields don't just analytically continue, they're quite different sorts of objects in the Euclidean case. Anyway, this is all explained in detail in the paper…

Update: No, this is not an April Fool's joke. I've now created a twistor unification page where I'll try and maintain updated information about this unification proposal

Comments

1. **Art**
   April 1, 2021

   um, the date?

2. **Elias**
   April 1, 2021

   Can someone say what makes this April 1, aka what is the fake part?

   I don't know anything about twistors and QFT or relativity, but, from a cursory glance it gave a legit feeling. Perhaps it's the LaTeX formatting?

3. **Lowell Brown**
   April 1, 2021

   One should understand that Schwinger's comment about the doubling of spin 1/2 fields in order to transform to Euclidian space involved starting with HERMITIAN four-component spin 1/2 fields in space time.

4. **Peter Woit**
   April 2, 2021

   All,
   No, not an April Fool's joke, perfectly serious.

5. **Peter Woit**
   April 2, 2021

   Lowell Brown,
   From Schwinger's Euclidean Quantum Electrodynamics paper

   "Thus the requirement of a Euclidean formulation excludes the simplest field in space-time, the four-component Hermitian spin-1/2 field (Majorana). In this context a a trivial observation may be worth repeating – a four-component Hermitian field is fully equivalent to a two-component non-Hermitian field."

   The issue of hermiticity of fields here is confusing, but my focus is on something else, the relation between the Spin(4)=SU(2)xSU(2) symmetry in Euclidean space and the Spin(3,1)=SL(2,C) symmetry in Minkowski space for a Weyl spinor theory. Looking just at the Wightman and Schwinger functions that are analytic continuations of each other, the analytic continuation relates the symmetries. But if you try and define states and operators, something different happens (the Euclidean theory state space is a different sort of thing than the Minkowski state space).

6. **Low Math, Meekly Interacting**
   April 2, 2021

   I was about to request an exegesis myself, as this either had either to be an inside joke of diabolical sophistication or, well, not a joke.

7. **Peter Woit**
   April 2, 2021

   LMMI,
   At some point it became clear I'd have the draft done around April 1 and I thought best to avoid that date. Later decided might as well post on April 1, maybe people would carefully read the thing looking (unsuccessfully) for the joke. As far as I can tell, that didn't work out…

8. **Robert A. Wilson**
   April 2, 2021

   In this subject these days it is sometimes difficult to distinguish between an April Fool's joke and an idea that is meant to be taken seriously. Perhaps that is not such a bad thing – there is many a true word spoken in jest.

9. **Justin**
   April 3, 2021

   Do you plan on publishing a 2nd edition of Quantum Theory, Groups and Representations: An Introduction, with added material on twistors?

10. **Jamie**
    April 3, 2021

    Hi Peter. I was wondering if you could comment on Eric Weinstein's article on Geometric Unity.

11. **Peter Woit**
    April 4, 2021

    Justin,
    I would at some point like to write a revised and expanded version of that textbook, now have a long list of notes to myself about ways it could be improved based on teaching the class again this year. Unclear when I'll have the time and energy for this project, and whether I'd be able to explain about twistors in a useful way that wouldn't take writing hundreds of pages. So, not any time soon, but maybe some day.

    Jamie.

    No, especially certainly not here, where I think there's a much more interesting set of ideas about unification to discuss…

The God Equation

April 4, 2021
Categories: Book Reviews, This Week's Hype

When I was out for a bike ride yesterday I stopped by a large book store and looked to see if they had a copy of Michio Kaku's new book The God Equation. They didn't, but did have plenty of copies for sale of his various previous efforts to promote string theory, such as 1987's *Beyond Einstein*, 1994's *Hyperspace* and 2005's *Parallel Worlds*. If someone interested in fundamental physics walks into a bookstore, and looks in the Science section for something to read written by a well-known physics professor, these books are what they're likely to end up taking home and reading.

When I got back from the bike ride, several people had forwarded me a link to this story from the Guardian which gives a good idea of what's likely in the book, claims like:

> Well, string theory has also created a tremendous amount of interest, as well as a backlash. People say, well, where is the proof? Quite frankly we don't have the proof, in the same way that Newton did not have the proof of his inverse square law back in 1666. Sometimes, the mathematics and the ideas are ahead of the concrete experimental data. That's where the Large Hadron Collider comes into play…
>
> The Standard Model is the theory of almost everything. It works spectacularly well but it's one of the ugliest theories proposed so far. There's this avalanche of experimental numbers you have to put in by hand. But in string theory the Standard Model just pops right out. With just a few assumptions you get the entire Standard Model. So the point here is that we need experimental proof and the LHC may give us hints of a deviation in the Standard Model and that's where this post-LHC physics comes into play.

This is just complete and unadulterated bullshit, of exactly the same sort Kaku and a host of others well-credentialed physicists have been heavily and successfully promoting for the last 35 years. I started writing about this 20 years ago, and there have been some changes since then (for one thing, we have Sabine Hossenfelder). I'm still waiting though for any of the leading figures in the physics community responsible for the string-theory hype campaign to do anything at all to try and stop Kaku and the rest of the Fake Physics onslaught that they unleashed.

Usually with books like this, once I get a copy of the book I try and write here a careful review quoting the writer accurately and explaining the problems with what they've written, but this time I think I'll pass on the grounds that this would be a waste of time.

The funny thing though is that I probably agree with Kaku far more than most people about the possibility of unification, although I wouldn't use the terminology "God equation" to describe a unified theory. Unfortunately Kaku has done far more than most physicists to discredit the search for a better unified theory, through the endless nonsense he has put out about the subject in books like this. I do think we'll find a better, more unified theory, and I even think I know a couple of the crucial equations, which, leaving God out of it, are:
$${D\mkern-11mu/}_A\psi=0$$
and
$$F_A^+=0$$

Update: You can read the book's introduction here. It seems that Kaku has conceptualized the book as a response to criticism of string theory. Near the end of the introduction, he assures us:

> This book will hopefully give you a balanced, objective analysis of string theory's breakthroughs and limitations.

This morning he's on Morning Joe.

Comments

1. **Dylan Mahoney**
   April 4, 2021

   What do those crucial equations represent, and where do they come from?

2. **Peter**
   April 4, 2021

   That Guardian quote makes me want to twist my own head off.

3. **David**
   April 4, 2021

   "Newton did not have the proof of his inverse square law back in 1666."

   Newton computed the force required for an apple to fall as it did, for the moon to "fall" in its orbit around Earth, and for Haley's comet to orbit as it did, and found the forces varied as the inverse square of the distance to Earth.

4. **Topologist Guy**
   April 4, 2021

   "The standard model pops right out." Isn't this simply false?

   The truth coming closest to this, I think, is that perturbative superstring theory on a flat 10D background reduces to a 10D supersymmetric quantum field theory at low energies (whose fields come from the low-energy excitations of the string). The project to get the specific 4D QFT that is the standard model, with its specific fields and numerical parameters, from a compactification of superstring theory has been a dream for decades and is farther than ever from being realized, with the enormous moduli space of possible string vacua. Of course we all already know this.

   So I think Michio Kaku is saying that superstring theory (on a flat background) reduces to *a* (10D) quantum field theory at low energies. It's very disingenuous to precede this with a remark on how "ugly" the standard model is, precisely because of the apparently arbitrary assortment of fields and parameters (not because QFT itself is an ugly framework), which suggests that string theory reproduces these specific fields and parameters as a low-energy limit, which is simply untrue. Kaku's statement really reduces to "you can recover a QFT as a low-energy limit of strings," which is far less impressive.

5. **Peter Woit**

April 4, 2021

Dylan Mahoney,
The first equation is the Dirac equation, and governs the behavior of matter fields. The second is the (anti)self-duality equation, which (especially from the twistor Penrose-Ward correspondence point of view) governs the behavior of gauge (force) fields.

6. **Peter Woit**
April 4, 2021

David/Topologist Guy,

The serious question here is why Kaku makes obviously absurd claims and arguments (that the SM pops out of string theory, that the status of Newton's law of gravitation when he came up with it was like the status of string theory). He's clearly not operating according to any usual scientific principles. Ash Joglekar, see here
https://twitter.com/curiouswavefn/status/1378833441702039554
says he understands Kaku not as a scientist, but as a fantasist and storyteller.

7. **Peter Orland**
April 4, 2021

David,

Newton did not have to calculate anything new to get his law of gravitation. He just needed Kepler's summary of Tycho's observations.

Kepler's first law (a planetary orbit is an ellipse, with the sun at one focus) and Kepler's second (equal areas) law, together imply an inverse square relationship of acceleration towards the sun. Then Kepler's third law enforces that $GM_{\odot}$ is the same for each planet. I don't know if Newton discovered the gravitational force law this way, but that would be my guess (a historian would know better).

8. **Douglas Natelson**
April 4, 2021

Kaku drives me up a wall. Time and again, he deliberately erases or obscures the line between science and complete bs speculation, presents all of this on TV or in print without qualification, and acts like that's fine because it gets people interested in science.

9. **Michael Weiss**
April 5, 2021

Newton's theory coherently and quantitatively retrodicted key observations (Kepler's laws, Tycho rich database of astronomical observations) and in turn predicted a vast body of further observations. (Indeed, the precision of the classical predictions enabled future rigorous testing of GR. The Lamb shift was also quantitative and precise, enabling the development and testing of QED.)

Given experimental constraints, it is not credible to equate the Newtonian paradigm with the status or sociology of String Theory, however brilliant the mathematics and however elegant its continuing impact in condensed-matter physics.

10. **Amitabh Lath**
April 5, 2021

Strings are one of those concepts that have become accepted by repetition. I have had to correct generation-Z/alpha members of my extended family from proclaiming to their grade-school science class that their "…uncle works at the LHC which is a powerful microscope that can make out strings inside quarks inside protons inside atoms…"

11. **Sabine**
April 5, 2021

Thanks for the link.

As Douglas mentions above, lots of people trying to excuse this by claiming it's okay because it "gets people interested in science". What they forget to mention though is that the people who actually get interested in science by things like this figure out very quickly that they've been sold vacuous quackery and end up being cynical nay-sayers and general science skeptics.

It's an interesting question why the Guardian would run factually incorrect scientific statements like this. Suppose the topic hadn't been string theory but climate change. Would they have printed utter nonsense just because it might "get people interested in science"? Certainly not. Why not? Because it's irresponsible.

I suppose when it comes to string theory they believe it doesn't really matter whether what you say about it is right or wrong. I think that's incredibly short-sighted though. Because research in the foundations of physics is a long-term investment into new technologies. I wish the Guardian editors would think a little more about what they run on their pages, or at least add an explanation for the reader for how to gauge the scientific credibility of Kaku's statements.

12. **Julien**
April 5, 2021

I think you can write to the guardian and they may publish your letter. See the links at https://www.theguardian.com/help/contact-us#nav2

Also, Katharine Viner is editor-in-chief and the page above tells you how to build her email address from her name.

13. **Low Math, Meekly Interacting**
April 5, 2021

"…in the same way that Newton did not have the proof of his inverse square law back in 1666."

Seriously, WTF is this about? I'm not being facetious. I have to believe there's some point being made here that is obscured by an outrageous level of omission. Like maybe he's thinking of the fact that Newton had no mechanism to explain the inverse square law, like maybe instantaneous action at a distance, which made Newton himself uncomfortable, begged for further insight. Or something. My ever-dwindling faith in humanity increasingly depends on such generous rationalizations.

14. **Peter Woit**
April 5, 2021

Julien,
I don't think a letter would be worth the effort. If they did publish such a letter no one would see it.

It would be great if somebody would contact the Guardian and ask them to issue a correction, to appear on the same page as the bogus claims by Kaku. Also good to get them to be aware they need to do some basic fact-checking before publishing claims like Kaku's. In particular his claim that Isaac Newton's discoveries were at the time untestable speculation should have set off red flags for any author or editor who had taken even a high school class in physics.

In general, this phenomenon of the huge amount of public misunderstanding of string theory should be addressed. I've done my part, string theorists themselves should take some responsibility for the mess that their proponents have created and do something about it.

15. **Ash Jogalekar**
April 5, 2021

"Unfortunately Kaku has done far more than most physicists to discredit the search for a better unified theory."

Although I agree that Kaku often says nonsense that's highly misleading, I think professional physicists pushing string theory have done far more harm to "discredit the search for a better unified theory" than Kaku ever did.

16. **Douglas Natelson**
April 5, 2021

I could be wrong, but a quick look at Google scholar suggests that Kaku hasn't published anything academic in many years, and there is nothing on the arxiv after 1999. I have no issue with people shifting gears into popularization, but to have pretensions of being an active scientist when not publishing for over two decades is too much.

17. **maciej**
April 6, 2021

While many Kaku's statements are clearly exaggerated, I think it is only for the PR of his books or PR of himself. Unfortunately this is how the "sales" work. Kaku has not been publishing scientific papers for years. He has became a sales man of his books, so I would not care much for his claims and I don't think scientists do.

Also, look at his textbooks on QFT or String Theoy/M-theory. Do you see those out-of-touch claims there? Not at all. Actually these books are rather well written. This shows that his exaggerated claims are only for the public. You may still not like it, but I don't think it is creating any more harm for the scientific community i.e. there would be still hype about ST even if Kaku was not known to the public.

In my opinion, the real damage comes from known and active researches in ST who still make similar unsupported claims.

18. **Jim Eadon**
April 6, 2021

True story. Yesterday (before reading this post) I asked a famous String Theorist for how String Theory connected with testable physics. He answered that the testable predictions of String Theory are "Super Symmetry" and the "Hagedorn tower of particles".
Perhaps dissatisfied with his own reply, he then simply deleted his (Twitter) tweet, and blocked me.
That exchange with a String Theorist (I'll not mention his name, but he's high-profile) told me more convincingly even than critics that the String Theory HEP programme has not gone well…
Interestingly, he also said that you did not need to test String Theory to see whether it is correct, because "it *is* the laws of physics"

19. **Peter Woit**
April 6, 2021

Maciej/Jim Eadon,
Kaku is an extreme case, but the problem of misleading/exaggerated claims for string theory is a widespread one.

20. **Robinson**
April 6, 2021

I am not a physicist, just an interested reader. It's funny you mention his books being very visible. I have a copy of Hyperspace, purchased in just such a manner as you describe. Kaku is charismatic (like Brian Greene) and has been in many TV shows like Horizon (BBC), which give him name recognition with us hoi polloi.

I cannot really comment on the content.

21. **Ted Rogers**
April 6, 2021

He was also interviewed about the book by Michael Shermer:

https://youtu.be/cDiN1JSdirs

It is very long, but still seems like something of a missed opportunity for a skeptic.

22. **Peter Woit**
April 6, 2021

Ted Rogers,

I took a quick look at the transcript. Shermer is supposedly a professional skeptic, but when he asks Kaku for experimental evidence of string theory, he seems all right with a response about strings vibrating in 11 dimensions and the mind of god. Inviting Kaku on and then seemingly taking the attitude that "this guy is a reputable scientist, so whatever he says must make sense", he's not exactly an advertisement for his brand of skepticism.

23. **Ted Rogers**
April 6, 2021

Peter,

Yes, that was my reaction. Very frustrating.

24. **Bernhard**
April 6, 2021

Kaku should stick to writing about the future (he's a fantasist, as someone said), since his books on this are quite entertaining (whether one believes his predictions is not the point), and I think he's rather careful interviewing people outside his own area of expertise.

In all his books, it is always when he talks about string theory that I have to roll my eyes. He has excellent technical knowledge (which is the reason he continues to get a "pass"), there is no excuse to write some much BS about the Standard Model popping out of string theory no matter what amount of simplification he wants to get behind in order to have the excuse. This is technically so wrong that no amount of simplification justifies writing it.

25. **Peter Woit**
April 6, 2021

Took a closer look at the transcript of the Shermer interview. When pressed about testability he lists three ways to test string theory:

1. String theory predicts dark matter, will see this at next generation post-LHC collider.
2. LISA will see gravitational waves from the big bang predicted by string theory
3. Measurements of the gravitational force inverse square law "in your living room" will see deviations predicted by string theory.

Utter horseshit.

Shermer's reaction: "You always blow my mind with these things."

26. **Peter Woit**
April 6, 2021

One fun thing about the Shermer interview:
Around 19:47 he refers to a book called "Not Even Wrong" that came out a decade or so ago and criticizes string theory as untestable, he thinks authored by a guy named "Witten".

27. **David Roberts**
April 6, 2021

>he thinks authored by a guy named "Witten".

who thinks? I presume you mean Shermer, since surely Kaku wouldn't make that mistake.

28. **Peter Woit**
April 6, 2021

Yes, it's Shermer who thinks this.

29. **Amitabh Lath**
April 7, 2021

If you got kicked off the fnal muon g-2 webinar, it is on youtube.
http://www.youtube.com/watch?v=81PfYnpuOPA

30. **Bernhard**
April 7, 2021

"he thinks authored by a guy named "Witten"."

I can't stop laughing about this…

31. **Patrick Dennis, MD**
April 8, 2021

David, Peter O. — The story goes that Robert Hooke and Edmund Halley were trying to work out the nature of gravity. The best guess was that it varied inversely as the square of the distance, but they were frustrated that they could not show that such a force would lead to elliptical orbits. So one (Halley, I think) asked Newton what kind of orbit would result from an inverse-square gravity, and Newton immediately answered, "an ellipse." He said he had already proved it, but did not have the proof immediately at hand. When he eventually showed it to his colleagues in the form of a brief manuscript, they realized that he was on to something very big indeed, and it was at their urging that the off-the-cuff calculation emerged years later as Principia.

32. **David**
April 8, 2021

Why had he worked out the equation of the orbit? Because he had already thought about the force law of gravitation.

" After dinner, the weather being warm, we went into the garden and drank thea, under the shade of some apple trees…he told me, he was just in the same situation, as when formerly, the notion of gravitation came into his mind. It was occasion'd by the fall of an apple, as he sat in contemplative mood. Why should that apple always descend perpendicularly to the ground, thought he to himself…" from Memoirs of Sir Isaac Newton's Life by William Stukeley, 1752

Stukely got the story straight from Sir Isaac. Newton worked out that the force on the moon was the same as the force on the apple, diminished by the square of the ratio of the distances. That, combined with his fluxions, was enough for him to work out the orbit as a conic section.

Muon g-2 Result

April 7, 2021
Categories: Experimental HEP News

The long awaited FNAL muon g-2 result was announced today, you can watch a video of the seminar here, look at the paper and a discussion of it at Physical Review Letters, or read stories from Natalie Wolchover at Quanta and Dennis Overbye at the New York Times. Tommaso Dorigo has an extensive discussion at his blog. In terms of the actual new result, it's not very surprising: quite similar to the previous Brookhaven result (see here), with similar size uncertainties. It's in some sense a confirmation of the Brookhaven result. If you combine the two you get a new, somewhat smaller uncertainty and ($a_\mu=\frac{1}{2}(g-2)$)
$$a_\mu(Exp)=116592061(41)\times10^{-11}$$

The measurement uncertainties are largely statistical, and this is just using data from Run 1 of the experiment. They have accumulated a lot more data since Run 1, and once that is analyzed the FNAL experiment should be able to provide an experimental value with much lower uncertainty.

The big excitement over the g-2 experimental number has to do with it being in conflict (by 4.2 sigma now) with the Standard Model theoretical calculation, described here, which gives
$$a_\mu(Theory)=116591810(43)\times10^{-11}$$
An actual discrepancy between the SM theory and experimental value would be quite exciting, indicating that something was missing from our understanding of fundamental particle physics.

The problem is that while the situation with the experimental value is pretty clear (and uncertainties should drop further in coming years as new data is analyzed), the theoretical calculation is a different story. It involves hard to calculate strong-interaction contributions, and the muon g-2 Theory Initiative number quoted above is not the full story. The issues involved are quite technical and I certainly lack the expertise to evaluate the competing claims. To find out more, I'd suggest watching the first talk from the FNAL seminar today, by Aida El-Khadra, who lays out the justification for the muon g-2 Theory Initiative number, but then looking at a new paper out today in Nature from the BMW collaboration. They have a competing calculation, which gives a number quite consistent with the experimental result:
$$a_\mu(BMW)=116591954(55)\times10^{-11}$$

So, the situation today is that unfortunately we still don't have a completely clear conflict between the SM and experiment. In future years the experimental result will get better, but the crucial question will be whether the theoretical situation can be clarified, resolving the current issue of two quite different competing theory values.

Update: Also recommended, as always: Jester's take.

Comments

1. **Mark Weitzman**
   April 7, 2021

   I wish the New York Times would have better science reporting – typical error:

   "That leads the factor g for the muon to be less than 2, hence the name of the experiment: Muon g-2."

   Of course (g_mu-2)/2 from PDG website is about (10^-10) * 11659208.9± 5.4±3.3 and |g_mu|>2.

2. **Anonyrat**
   April 7, 2021

   The BMW pre-print: https://arxiv.org/abs/2002.12347

3. **Roger**
   April 7, 2021

   So BMW showed up to crash the party with the news that their paper is now published in Nature. Its not often we get that degree of excitement in particle physics.

   A lot of people worked on that result. They didn't deserve their result day being hijacked the way it was.

   That said, I'd be interested in knowing the back story of what went on and why the BMW paper wasn't included in the calculation that led to 4.2 sigma. Was there a prescribed procedure for deciding whether or not a calculation would be included or was this decided on the fly in response to the BMW work ?

   To be clear, I certainly don't think that it was the intention of the g-2 collaboration and the Theory Initiative to put across a misleading estimate of the size of the data-theory discrepancy (and they not have done this at all, even inadvertently). However, given the events of today, there is an unfortunate impression of a stitch-up.

   For an outsider like me (I'm a collider physicist), today's events were interesting from both a scientific and sociological perspective.

4. **Alessandro Strumia**
   April 7, 2021

   4.2σ anomaly with respect to one theoretical prediction, omitting the other theoretical prediction close to the measured g-2.

5. **Amitabh Lath**
   April 7, 2021

   Muons are having a moment.

   So what physics could be causing this 4.2 sigma (assuming it remains)? After the LHCb results there was a lot of chatter about leptoquarks, but I don't see much here?

6. **Henry McFly**
   April 7, 2021

   Assuming that the muon g-2 Theory Initiative's calculation is correct, what kind of BSM explanations are there to explain the discrepancy between the SM prediction and the experimental measurement?

7. **e**
April 7, 2021

Roger,

The value for the leading-order HVP recommended by the g-2 Theory Initiative is that of the data-driven approach. The white paper notes that

"For HVP, the current uncertainties in lattice calculations are too large to perform a similar average and the future confrontation of phenomenology and lattice QCD crucially depends on the outcome of forthcoming lattice studies. For this reason, we adopt [the data-driven evaluations of HVP] as our final estimate [..]"
[Section 8, above (8.12)]

From v1 of 2002.12347 to the paper published today, the quoted result for the LO HVP in units of 10^{-10} has shifted from 712.4(4.5) [v1, Feb '20] to 708.7(5.3) [v2, Aug '20 — after the white paper] to 707.5(5.5) [published], so the assessment that all lattice results need to be further scrutinized seems to have been warranted.

8. **Rollo Burgess**
April 8, 2021

The media are saying that these results can be taken in conjunction with the LHCb data also recently discussed to give an indication of something going on beyond the SM.

I was a bit unsure about this.

Is it correct that these 2 anomalies could be related and the the probability of BSM physics being present is higher than just the sum of either being real? Or is this not right?

9. **Mark**
April 8, 2021

I'm an experimentalist, but am very curious about the BMW 20 result. The theory seminar speaker implied, I recall, one should not take it seriously because it was not "data-driven", unlike the 3 calculations that show the 4.2 sigma discrepancy. Is this a conclusion that is widely shared in the theory community?

When I read the abstract of the BMW20 paper last night they imply their calculation is independent because it does not rely on the e+e- experimental results that the others use and position their calculation as a new independent result which resolves the discrepancy in that it agrees with the experimental measurement, but not the other theory estimates quoted. Reading the full paper today on the archive I don't get the same impression that they claim that though and instead they talk about further calculations being needed to confirm or refute their result, thus implying its unresolved as to which calculations are the most correct.

10. **Roger**
April 8, 2021

"e"

The argument for not including the lattice number is plausible but that's not really the point.

When it comes to stats, as an experimentalist, I'm taught to decide on the analysis strategy and to stick to it (unless this becomes impossible in practice) so that any conclusion I draw from the data will have a certain statistical integrity.

The key question is whether the lattice number would have been omitted if it had agreed with the dispersion-based estimates. If it would have been omitted then fair enough. If it wouldn't have been omitted then this further weakens any attempt to claim a 4.2 sigma discrepancy.

11. **Peter Woit**
April 8, 2021

Something that strikes me about the BMW vs. Theory Initiative story. At least one of the two is wrong, having made a significant mistake in their calculation/analysis. One would hope that both groups will look very carefully at wherever they are getting inconsistent numbers and focus on finding the error(s), whether their own or errors of the other group.

12. **André W-L**
April 8, 2021

Some comments on the theory discrepancy and lattice QCD (from a lattice QCD practitioner who does not work on g-2):

1 – Even if the theoretical determination of the hadronic vacuum polarization (HVP) moves the theory prediction to match the experimentally measured muon g-2, this just shuffles the tension to other parts of the electroweak constraints (with similarly sized tension)
https://arxiv.org/abs/2003.04886

The only way to eliminate this global tension without invoking new physics, presently, is if the experimental value moves towards the current standard model theory prediction that is in 4.2-sigma tension with the experimental result.

2 – The Theory White Paper (WP – arXiv:2006.04822) considered both lattice QCD determinations of the HVP as well as data-drive (dispersive) ones. Besides the BMW result, no other lattice QCD results have a precision comparable to the dispersive approaches. Therefore, they decided to not use a lattice QCD determination in the final estimate, until the lattice results have an uncertainty small enough to compete with the data-driven determination (as noted above).

3 – Regarding BMW (arXiv:2002.12347) not being included in the the WP average, there was a well known deadline of 15th Oct. 2019 for papers to be included, if and only if they were published (not just arXiv'd). This deadline was relaxed to 31 March 2020, but is still too soon for the BMW paper to be included in the averaging procedure. See 3rd bullet on the 10th page of the arXiv version.

4 – Regarding lattice QCD results as theory predictions, as Aida El-Khadra nicely summarized in her presentation, we do not base our lattice theory predictions on any single lattice QCD result, as they all contain systematic uncertainties which must be controlled and eliminated. For example, different choices of discretizing QCD lead to finite differences that are not universal and so the continuum limit must be obtained through an extrapolation (for many quantities, all discretization effects arise from higher dimensional "irrelevant" operators). But, there is no proof this works, it is just a very well-motivated expectation based on asymptotic freedom. Therefore, we rely upon multiple independent calculations to be performed as a cross check that these systematic effects are under control. This is very synonymous to the situation that we do not accept new physics results until multiple experiments can confirm the finding (as best we can).

5 – Regarding the precision of the BMW result, as Aida also nicely summarized, the BMW result is so stochastically precise, the final result is dominated by the

systematic uncertainties, which for HVP calculations, involve more complex systematics than in more typical calculations. This increases the importance of having multiple groups independently perform the computations since systematic uncertainties do not automatically improve with more statistics and computing power. It is not statistically very significant, but as a measure of the challenge with these systematics, the BMW value shifted 1-sigma over time (from first arXiv release to publication): 712(4) [v1] to 709(5) [v2] 708(5), based upon community "criticism" of how they handled some of the systematic uncertainties, not through additional computations etc. (as noted above).

It is therefore premature to rely upon lattice QCD results to constrain the hadronic vacuum polarization contribution to g-2 with sufficient precision to make a statement about new physics or not. It is anticipated that by the time the higher-precision experimental results are available, the lattice results will be sufficiently precise and cross-checked to provide a constraining determination of the HVP and hadronic-light-by-light contributions.

13. **clayton**
April 8, 2021

André W-L — thanks for chiming in here, I for one consider your perspective to be informative 🙂

You say in closing that "the lattice results will be sufficiently precise and cross-checked" by the time the experiment becomes more precise, which is in the next year or two as a rough timescale. That indicates that other lattice groups are already working on this ab initio calculation. Do you know which groups are working on this?

14. **WTW**
April 9, 2021

In re. "criticisms" causing updates of their original paper, the BMW group has actively solicited such evaluations and critiques from other experts, including from the g-2 Theory Initiative team. While there may inevitably be some level of competitiveness between groups, in general they seem to have tried to use any available resources to improve the results. Hopefully such collaboration will continue, to help reach a consensus from lattice QCD calculations.

As to many people noting the LHCb report of possible EW lepton flavour violation in B-hadron decays as somehow further validation of these g-2 results, Tommaso Dorigo's comments seem extremely timely.

It should also be noted that a recent ATLAS report "sets a new constraint on lepton-flavour-violating effects in weak interactions, searching for Z-boson decays… " — https://arxiv.org/abs/2010.02566 And as to some new ultra-light particle being the culprit affecting the anomaly in the muon's anomalous magnetic moment, there is a recent paper showing no detection of any Axion-like particle having any EM interaction within a network of synchronized ultra-precision optical clocks, down to a level of ~ 8 x 10^-18 second. (https://www.nature.com/articles/s41586-021-03253-4.epdf) Likewise, CERN's Baryon/Anti-Baryon Symmetry Experiment (BASE) has recently published new limits on (non) detection of coupling of Axion-like Particles and photons, using a cryogenic Penning trap: https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.126.041301#fulltext

Throw in the ANAIS experiment failing to replicate DAMA's problematic detection of dark matter particles after 3 years of data, and it seems that the tactic of just adding some more particles to the mix might be getting more constrained of late.

15. **André W-L**
April 9, 2021

It was pointed out to me that in their latest version, BMW addresses the electroweak tension pointed out by Crivellin et al – page 82 of the BMW arXiv is section 26, addressing this point. They present a detailed argument that the global electroweak tension (if g-2 theory moves to be consistent with experiment) is more like 2.4 sigma (than ~3-4).

16. **Roger**
April 9, 2021

André W-L
Thanks a lot for your comments. I'm pleased that a rule wasn't made up to exclude BMW.

Beyond this, while I accept and respect your experience with lattice QCD, I found your last remark a bit baffling "It is therefore premature to rely upon lattice QCD results to constrain the hadronic vacuum polarization contribution to g-2 with sufficient precision to make a statement about new physics or not."

Nobody is suggesting that lattice QCD be used to make a statement about new physics. There is simply no statement to be made with the current precision of the data and theory calculations. Instead we are in a situation in which a calculation has been made which has been peer-reviewed and has now appeared in Nature and this should surely now merit being shown next to the data even if the experiments would also feel the need for a caveat. Perhaps two plots could have been prepared. I saw the CERN seminar of the g-2 results (theory talk + experiment talk) and felt the experiment was making a mistake by not showing the new calculation in its standard publicity plot since it gave the impression of being in bed with its own theorists , which is very unfortunate. Perhaps that was ok – doing the right thing isn't always easy, if it was the right thing. However, whether the experiment and Theory Initative like it or not, people are making their own plots. It will be difficult for the experiment to keep up its standard publicity plot with its "approved" prediction given that Nature has just published a plot that is "unapproved" for comparison. Politically they need to think carefully about how to proceed.

Regarding the tension moving elsewhere, this is certainly interesting but should have nothing to do with a decision on whether to show a theory prediction for a specific observable in comparison to a measurement of that observable. Indeed,the whole business of much of particle physics these days is looking for tensions and trying to interpret them.

17. **Martin H.**
April 9, 2021

Dear Andre,

thanks for your comments! If I may add to your point about the electroweak fit: in our paper we presented several scenarios how big the tension would be depending on the energy range in which the changes in the e+e- cross section occur. In response, BMW provided arguments that these changes will be concentrated at low energies, in which case the tension in the electroweak fit only increases moderately.

However, this then implies that the low-energy cross sections would have to be changed by a lot, see
https://arxiv.org/abs/2006.12666
https://arxiv.org/abs/2010.07943
so unless the Fermilab number changed, the tensions would be moved somewhere else.

18. **Joseph Conlon**
April 9, 2021

Whether it's in Nature (the journal) is neither here nor there. Glossy covers and fancy editorials do not correct science make.

19. **André W-L**
April 9, 2021

Martin H. – thank you for the update! (those who don't know, unless Martin H has conspicuously the same initials – he is one of the leading experts on the data-driven dispersive methods for g-2 and also an expert on the electroweak fits).

clayton – if you look at Table 8 (page 80) of the theory white paper (https://arxiv.org/abs/2006.04822), the top 3 panels are results from various groups working on the problem. It is one of the most intensely studied problems with lattice QCD these days, so there are a large number of groups working on it. Figure 44, on the next page, gives you a visual representation of the same results.

WTW – I did not mean to imply BMW were not looking for feedback. My point is that the systematic uncertainties are sufficiently challenging that even one of the worlds leading lattice QCD groups did not "get it right" the first time.

Roger – BMW, in their paper, claim there is no need for new physics, the "or not" part of my statement about it being premature to rely upon lattice QCD results.

If I were one of the co-authors, I would make a similar claim as I'm certain they put a tremendous amount of effort in trying to ensure everything was controlled, and are therefore confident that within the uncertainties they quote, they have the correct Standard Model prediction. As someone without "skin in the game", I see the value in "community consensus", meaning more LQCD results need to be performed with uncertainties similar to BMW before we (the lattice and broader community) can have higher confidence.

Take it another way – if the LQCD results do not agree with the data-driven dispersive results for the HVP, at a similar level of tension that is currently quoted for g-2, that would be evidence for new physics (or a larger than expected systematic uncertainty) since LQCD is pure Standard Model and the data-driven analysis is nature.

I also agree that it is unfortunate, and a mistake not to show the BMW results on the same, or at least a companion plot. Their result is among the highest quality lattice QCD results available, if not the highest quality.

20. **SG**
April 9, 2021

@Martin H.: If all the changes are indeed below 1 GeV what is the significance of the discrepancy between the e+e- cross-section measurements and BMW ? From the lower panel of Fig 30 of https://arxiv.org/pdf/2002.12347.pdf (BMW) it seems that the first bin has a discrepancy that is more than 3 sigma. Am I reading this correctly?

21. **Martin H.**
April 9, 2021

SG: not quite. This plot shows bins in the space-like region, which you cannot directly compare to (time-like) e+e- cross sections. But the emerging picture, see Fig. 8 in
https://arxiv.org/abs/2010.07943
is similar: the e+e- data would have to be wrong way outside their quoted uncertainties. As for the significance, the global tension between e+e- and BMW (v3) is now 2.1sigma, but since the changes need to be concentrated at low energies, the local tension is bigger than that. The exact significance depends on energy range and data compilation, but to give you some idea: for a partial ("window") result the BMW paper quotes a tension of 3.7sigma, see caption of Fig. 4 in
https://www.nature.com/articles/s41586-021-03418-1
Such detailed comparisons, especially with forthcoming new data and results from other lattice collaborations, should allow for some clarification in the near future.

22. **SG**
April 9, 2021

@Martin H– Thanks a lot for your answer. So is it correct to say that BMW either has a 3 sigma(-ish) tension with electroweak precision tests or if the changes to e+e- cross section are concentrated at low energies, it has a 3 sigma (-ish) tension with e+e- data ? So whichever way we see it there is a significant tension between theory and experiment.

23. **cssm**
April 10, 2021

In the theoretical prediction, I noticed, components from individual gauge sector are mentioned. I was just wondering what happened to the cross terms from gauge mixing. Also if there is some new physics, shouldn't that be already present in the lepton-hadron cross-section data? In that case iterative approach and convergence test should also be incorporated.

24. **Anonyrat**
April 11, 2021

Experimentalists, please explain how this is disentangled: Bhabha scattering is used to measure the integrated luminosity in electron-positron colliders, which is a key to measurement of the hadron vacuum polarization. But hadron vacuum polarization is apparently necessary to calculate Bhabha scattering to the required precision.

Recently (2020), a newly computed Bhabha scattering cross-section, reducing it by 0.048% resolved a 2-decade-old 2-sigma discrepancy of LEP data with the Standard Model.
https://www.sciencedirect.com/science/article/pii/S0370269320301234

If BMW is correct, there is a correction needed to the Bhabha scattering cross-section, and to the measured e+e- data. How big is it, and will its direction go toward resolving the tension mentioned in the comments above?

25. **Ahcci**
April 11, 2021

The BMW collaboration has a new, semi-technical article up at "The Conversation." The URL is https://theconversation.com/proof-of-new-physics-from-the-muons-magnetic-moment-maybe-not-according-to-a-new-theoretical-calculation-157829

26. **someone**
April 12, 2021

@SG: No, BMW is in 3sigma tension with with chiPT predictions from e+-e-. The direct lattice prediction effort of e+-e- is just starting.

27. **SG**

April 13, 2021

@someone: I did not mean comparison with lattice but experimental data on e+e-. Fig. 8 in https://arxiv.org/abs/2010.07943 , pointed out by Martin H., is a comparison between real data and the shift in e+e- cross-section implied by BMW. This is, of course, just a restatement of the tension between the BMW and data-driven value for the HVP contribution but now focussing on the <1 GeV region for the e+e- data.

28. **Luca**
April 13, 2021

Honestly I find the work on the non-perturbative behavior of the standard model even more exciting than potential new particles. From this situation is clear that we do not understand the model well enough to make some predictions, or at least that a substantial amount of work needs to be done here. New physics is where physicists work and it is perhaps time to give more glamour to the hard work of connecting our best theory to the reality we have instead of always looking for jet another reality. High energy physics is alive and well.

29. **Will Sawin**
April 13, 2021

Exciting!

Are there other interesting situations where the discrepancies between different attempts to calculate the standard model prediction differ by multiple sigma, so that we aren't necessarily able to tell whether an experiment is evidence for a failure of the standard model?

I am tempted to ask "Could we have already observed, in some experiment, the effects of beyond-the-standard-model physics, but not have noticed it because our calculation of the standard model prediction is different from the correct standard model prediction but not from the experimental data?" but I realize this is probably not a very helpful question, in part do the the fact that "the correct standard model prediction" might not be a mathematically well-defined concept outside of the existing calculational techniques.

30. **Peter Woit**
April 13, 2021

Will Sawin,
The calculational problems with the Standard Model are pretty much just with the strong interactions (there are problems of principle with perturbation expansions for weak and electromagnetic interactions being only asymptotic expansions, but these issues only show up at too high orders of the expansion to matter).

The problems with strong interaction calculations are very serious: typically the best you can do is a lattice gauge theory calculation (Monte-Carlo simulation of a path integral), and there you're lucky to get a calculation to 1%. The only reason the g-2 calculation is so accurate is that the strongly interacting contributions to the calculation are a very small part.

So, for most things involving strongly interacting particles, the theoretical calculation has much larger errors than the experimental measurement (e.g. the masses of such particles). In general the much more precise measurements are consistent with the low precision calculations. An example where there's a possible problem involves spin and the proton, see eg. "Proton spin crisis" on Wikipedia or someplace better. But there, the theoretical understanding of how to calculate things is so bad that it's likely that the problems are just with the calculations, not problems with the underlying theory.

31. **Luca**
April 13, 2021

Hi Peter,

"But there, the theoretical understanding of how to calculate things is so bad that it's likely that the problems are just with the calculations, not problems with the underlying theory."

Can we actually say to have a theory then? We have an asymptotic perturbative theory for sure, but does this fully constraints the non-perturbative behavior, especially concerning bound states? To my naïve eye, it is more a rough set of ideas than a full theory.
I have the impression that the "beyond the standard model" things we should look for are protons, atoms and tables and chairs, not superstrings. It puzzles me why we have a fundamental theory that is utterly incapable of predicting the behavior of something as elementary as a hydrogen atom and jet we think that all that physics is boring and already understood. Where does all this confidence comes from? Who decided that the perturbative regime of a theory is its only fundamental aspect? Am I missing something? I mean I know that the people who actually work in these fields takes these problems very seriously, but jet the general consensus seems to be that there isn't new physics to be understood here. Why? Because all the physics is implicitly in the Lagrangian? But as far as I know we do not even a proper way to make sense of the Lagrangian theory in the non-perturbative regime. Do we have strong evidence to suggest that the perturbative behavior can be continued in a unique way to the non-perturbative regime without introducing anything new?

32. **Peter Woit**
April 13, 2021

Luca,
What we have for QCD is not a "rough set of ideas", but a well-defined cut-off theory, the lattice theory, so precisely defined that you can write code and do computer calculations. What we don't have is rigorous or calculational control of the continuum limit, there all we have is conjectures that there's a well-defined limit with certain properties, together with all the computational evidence being consistent with those conjectures. To convince yourself this is more than a "rough set of ideas", look for instance at
https://arxiv.org/abs/1203.1204
in particular the hadron spectrum in figure 2.

The one place where there might be an actual hole in our ability to write down a precise non-perturbative theory is not for the strong interactions, but for the weak interactions, where the gauge interactions are chiral. That's a complicated story, and there since the interactions are weak, perturbation theory gives all we need to compare to experiment.

33. **Luca**
April 14, 2021

Peter,

Thank you for the clarification. I am aware that lattice QCD is a well defined theory. However, it's value is as a proxy for an underlying continuous limit which is truly the theory we would like to have. I understand that the conjecture of a unique and well defined limit is supported by evidence, but is as far as I understand this is limit to specific settings. Your discussion about the proton spin suggests that in many, perhaps most, situations we do not have idea how to approach this limit. Is it just a problem of limited computational resources? Are you confident that a correct MC estimate of the proton spin composition could be obtained if we just had bigger computers? Or it will require novel ideas? If the latter is true then we cannot say to have a complete theory.

34. **Mark Weitzman**
April 14, 2021

Hi Peter:

Have all the fundamental difficulties of lattice gauge theory, such as fermion doubling problem been resolved, or is it simply the case that workarounds have been found for the current calculations. I have always felt that a physical theory is not really well defined unless it can be simulated on a computer in principle.

35. **Peter Woit**
April 14, 2021

Luca/Mark Weitzman,
I'm not that well-informed about the latest state of the technology of QCD calculations with fermions, but not aware of any problems of principle, i.e. that can't be solved with more computational power. This is different for the chiral weak interactions, where there may very well be remaining problems of principle.

These issues are actually related to the work I've been doing with twistors, going way back. Early in my career I worked on pure lattice gauge theory calculations, trying to take into account the role of topology, so needed to understand well the geometry of what was going on. I was very interested in the fact that the geometry was straightforward for pure gauge theory, but that when you introduced spinor fields, some sort of new geometry was coming into play and it was unclear both what it might be and whether lattice difficulties were due to not taking spinor geometry into account.

One way of thinking about what's going on is that there's a natural lattice version of fermions if they're differential form fields, with 0-forms given by fields on the sites, 1-forms on the links, 2-forms on the plaquettes, etc. These are "Kahler-Dirac" fermions. The problem is that they're not spinor-valued fields, but anti-symmetric tensor valued fields. Spinors are in some sense "square-roots" of the anti-symmetric tensors, but to construct such a square root, you need to choose a complex structure on R^4. Then spinors are (up to a phase factor) either the holomorphic or anti-holomorphic differential forms.

There is an S^2 of appropriate choices for the complex structure on the tangent space at each point in R^4, and a point in the projective twistor space is just a point in R^4 together with a choice of the complex structure (a point in the fiber S^2). So, on projective twistor space, the spinor fields have a straightforward geometrical interpretation (although in terms of holomorphic geometry).

I haven't thought seriously about this in a long time, but back when I was thinking about it, I didn't see any obvious way to use this to solve any of the problems of lattice fermion fields. Some of the things I've started thinking about now (how to relate chiral Yang-Mills theory on R^4 to holomorphic theories on projective twistor space) might though give some new ideas on the lattice stuff, we'll see…

36. **Andre W-L**
April 15, 2021

Luca, Mark, Peter, Will,

A few more comments related to your comments and questions.

1. The fermion doubling problem is practically solved in several ways now, and each method has been used to make post-dictions of well known quantities that agree with the experimental numbers and each other (after the continuum limit extrapolation and physical quark mass extrapolation/interpolation).

2. It is now common for quantities of mesons (pions, kaons, charmed and bottom mesons) that lattice QCD results are obtained at the sub-percent level of precision with a fully quantified uncertainty budget. This level of precision requires the inclusion of QED and Weak interactions to reduce the uncertainty from the ~1% level down to the 0.2% level (the target precision for many quantities). This is starting to become the case for simple properties of nucleons, which have now started to reach the 1% level of precision.

3. The "Proton spin crisis" is a red herring. In units of h-bar, we know the spin is exactly 1/2. What is complicated is to determine how the various constituents of the nucleon contribute to the spin. Now, there are several complete calculations including the quark contribution and gluon contribution that obtains the total spin. For example, this result obtains the total spin with a 15% uncertainty:
https://arxiv.org/abs/2003.08486

The "spin crisis" came because, based upon the quark model (which is an extremely naive and in many cases surprisingly successful description of protons, neutrons etc), there was an expectation that the spin of the proton would come almost entirely from the "valence" quarks of the proton (which has up, up, down valence quarks). It was found experimentally that the total contribution from these valence quarks fell far short of adding up to 1/2. But, we know these valence quarks are confined (very strongly) by gluons, and so it is not surprising at all that there is substantial contributions to the spin from the gluons and the orbital angular momentum contributions from the proton's constituents.

4. There are several interesting puzzles related to strong interactions. There is the more famous "proton radius" puzzle in which the size of the proton when measured from electrons or muons disagreed by as much as 7-sigma. This puzzle is now expected by most in the community to have come from systematic uncertainties in the experimental determinations which are now beginning to be resolved.

There is the "neutron lifetime puzzle" in which the lifetime of the neutron when measured in a "beam" and "bottle" experiment disagree at the 4-sigma level. The beam experiment only measures Standard Model decay modes and the bottle measurement simply counts the number of neutrons vs time and so measures all effects. This generated a lot of interest that there might be new, relatively light, new physics decay modes. Many expect this discrepancy is driven by a systematic uncertainty.

In both cases, lattice QCD can be used in principle to help resolve these puzzles, but the challenge of obtaining a sufficient precision to be useful has prevented the calculations from contributing so far. Calculations of nucleons generically require exponentially more stochastic samples to obtain the same relative precision as for mesonic quantities (this is a well understood Monte-Carlo sampling problem related to sign-problems and stochastic sampling of fermions).

5. There are other exciting examples in "nuclear physics" where lattice QCD can be used to compute quantities with a 20% uncertainty which will be useful. For example, the search for neutrinoless double beta decay of large nuclei requires an understanding of short distance contributions to how two-neutrons can simultaneously beta-decay to two protons, assuming the neutrino is Majorana in nature (it is at least in part is its own anti-particle – the only fermion we know of for which this is possible, as it does not cary any Standard Model "charge"). While we can never use lattice QCD to calculate the decay of a large nucleus, we can compute the basic neutron-neutron to proton-proton amplitude for various models of Majorana neutrino exchange between two nucleons. Knowing the QCD contribution to this amplitude with a 20% uncertainty is more than sufficient to help the field. Unlike many other processes, there is no experiment that can measure this neutron-neutron to proton-proton process because if it occurs, it is only in a big nucleus, so isolating the underlying amplitude is not possible.

Lattice QCD can also be used to constrain the interactions of strange-matter (hyperons) with neutrons in principle much better than experiment. This is because the hyperons rapidly decays through Weak interactions and so the data set is very limited (there are now several international experiments that will significantly improve the data set). In lattice QCD, we are free to "turn off" the Weak interactions so the hyperons are stable. What is interesting is that an improved understanding of hyperon-neutron forces, and triple-neutron forces, may improve our ability to predict the nuclear equation of state which is relevant for understanding neutron stars and how they might merge in binary systems, and if supernovae will result in a neutron star or black hole.

We (the community) are optimistic that as the exa-scale supercomputers come online (starting next year), we may get enough computing power to reliably compute these processes. I could go on, but I've already written a "bit" more than I intended.

37. **Gianni**
April 15, 2021

Andre W-L,
I am very grateful for the extensive and stimulating, though short, list of issues that the lattice community is able to investigate either with great precision, good precision, or an acceptable one.
I would like to ask your opinion on the challenge presented by lattice calculations of the parton distribution functions, which are so relevant in the analysis of HEP experiments. I know that there efforts along this directions.

38. **André W-L**
April 16, 2021

Gianni,

That is a very exciting topic these days also. Until a few years ago, the lattice community had come to the conclusion that lattice QCD could be used to only compute moments of the parton distribution functions. A few years ago now, the field was re-vitalized by a proposal of Xiangdong Ji, that, instead of computing these structure functions directly, one could approach the problem by computing a related quantity (quasi-parton distribution functions) that in the limit of large nucleon momentum, become the parton distribution functions. It took a few years for the community to sort out the most essential details and now great progress is being made (the critical difficulty was understanding how to renormalize these objects). There are now at least three variants of the idea being used in the community, each with their own advantages and disadvantages, which is also great as it provides multiple ways to get at the same quantities (perhaps in different kinematic regimes). There are at least as many lattice groups working on this problem as those who are working on g-2 calculations. The community is now pushing the idea to compute the transverse momentum dependent distribution functions with some promising ideas being pursued.

One of the biggest challenges in these computations is related to the same signal-to-noise problem mentioned above. As you look at a boosted nucleon, the noise issue becomes more severe because, the boosted nucleon has more energy, and so the correlation functions decays more rapidly in Euclidean time, while the noise of the correlation function has an energy scale that does not increase as the momentum is increased, and thus the mass gap which dictates the degredation of the signal-to-noise becomes larger, causing a more rapid loss of the signal. People are actively working on clever ideas to circumvent this problem (in addition to using bigger/faster computers). These calculations are well on track to help with HEP and NP problems (the planned electron-ion-collider).

There are already collaborations between lattice groups and those who work on the structure functions to begin figuring out how to use lattice QCD results as inputs to help determine and constrain them, so I anticipate the next several years will be very fruitful for this type of research.

39. **Gianni**
April 16, 2021

André W-L ,
thank you very much for the very nice overview. Let us hope to see soon reliable calculations also on parton distribution distributions and TMDs! I heard that the issue of the renormalization within the Ji framework is highly non trivial, but as you pointed out the sinergy between different approaches should lead us to the goal.

Hawking Hawking

April 8, 2021
Categories: Book Reviews

There's a very good new book about Stephen Hawking that just came out, Charles Seife's Hawking Hawking. Some detailed reviews can be found at Prospect Magazine (Philip Ball) and the New York Review of Books (James Gleick). Seife has chosen to write the story of Hawking's life starting at the end and ending at the beginning, which takes some getting used to, but provides a different perspective.

Hawking was a huge world-wide celebrity, widely considered by the public and the press to be the modern-day analog of Einstein, dominating the field of theoretical physics. His personal story, involving a long life battling a disease that left him quadraplegic and severely disabled, added greatly to the phenomenon he became. His life has been the subject of various books, films and TV shows, but only now, three years after his death, has something appeared that gives an account of this life corresponding not to myth but to reality.

The reality of this story is that Hawking was a very good theorist, with a high point of his career his work on Hawking radiation in 1974. I remember attending lectures by him at Princeton in the early 1980s, when he was actively working on Euclidean quantum gravity. His speech was hard to follow, so one of his graduate students or postdocs would translate for the audience. Unfortunately, the disease continued to take its toll, and after he nearly died from it in 1985, losing the ability to speak to a tracheostomy, all evidence I've seen is that he was no longer able to continue to do research at the highest level. From then on he lived a remarkable and full life for another 33 years, including some collaborative work with other theorists, but he was no longer the driving force behind any new research programs. Seife quotes extensively many physicists who worked with Hawking during this time, including Andy Strominger and Hawking's student Marika Taylor, who give a fairly good idea of what it was like to work with him.

During the early 80s Hawking was quite fond of the idea that N=8 supergravity would be a successful unified theory, famously giving a talk about it entitled Is the end in sight for theoretical physics?. The advent of string theory coincided with the serious deterioration in his health and ability to communicate. From then on he was reliant on others to explain to him what was going on in string/M-theory:

> Taylor didn't yet know how difficult the task ahead of her was. Her thesis was going to be on M-theory, but Hawking was not an expert on the subject. Taylor would largely have to guide herself straight to the frontier of an incredibly difficult branch of theoretical physics, digest all the important work of the past few years, and then teach Hawking what she had learned before even being able to come up with a thesis idea. On top of that, Hawking wasn't particularly enthusiastic about the string-theoretic parts of the theory: he just cared about supergravity. "As I was starting to go into those areas, I wouldn't say that he was skeptical," Taylor says. "He was just not interested… Actually I think the real truth is that he didn't want to engage with people on territory he was unfamiliar with."

Soon after I started this blog in 2004, I wrote here and here about Hawking's heavily publicized talk in Dublin announcing that he had figured out how to resolve the black hole information paradox. I was baffled by reports of his talk and his paper, and not the only one. Seife tells the story of this in some detail, and I think the consensus is that there was no there there.

A large part of Hawking's celebrity and income derived from his work as a popular author. His 1988 popular book, *A Brief History of Time*, was a huge success. Seife tells the story of how that book came about, partly motivated by the need for a new source of income. An initial manuscript due to Hawking was edited and improved a great deal before the published version was done. Many other books followed, and if you go to any bookstore with a science section, you're likely to find quite a few of them for sale. The problem is that, on the whole, they're not any good, and they're not written by Hawking. Seife documents this sorry tale in some detail.

I first noticed this when I ran across a copy of God Created the Integers, a thick anthology of writing on mathematics, supposedly edited by and with commentary by Hawking. At least he's listed as the sole author. Given the topic and the volume of material, it seemed highly implausible to me that Hawking was actually the author. For a review of the book, see here. Seife explains in detail that much of it is essentially plagiarized from other sources, and that to this day, it seems to be unknown who wrote the material (just that it clearly wasn't Hawking).

At least this sort of thing got little attention, which unfortunately was not true of his 2010 *The Grand Design*, co-written with Leonard Mlodinow. I wrote about this book in some detail here. Put bluntly, it was an atrocious rehash of the worst nonsense about M-theory and the string theory landscape, with an argument for atheism thrown in to get more public attention. This is the sort of thing that has done a huge amount of damage to both the public understanding of fundamental physics, and even to the field itself. James Gleick's otherwise excellent review of the Seife book ends with

> Hawking promoted the theory of everything with a vengeance. He made it part of his brand. It was the title of the 2014 biopic in which Eddie Redmayne played Hawking. The much-quoted ending of A Brief History of Time raised the prospect of a complete theory—a final theory: "It would be the ultimate triumph of human reason—for then we would know the mind of God." At the 1998 White House event, Hawking told the assembled dignitaries:
>
> > We shall have to rely on mathematical beauty and consistency to find the ultimate Theory of Everything. Nevertheless I am confident we will discover it by the end of the 21st century and probably much sooner. I would take a bet at 50-50 odds that it will be within twenty years starting now.
>
> He would have lost that one, too. It was hubris—but it sold, and it is part of his legacy. He showed younger colleagues how to chase grand theories and best-selling books. Hawking is not the only physicist guilty of hawking.
>
> The theory of everything is a false idol. Why should the universe, which grows more gloriously complex the more we see, be reducible to one set of equations and formulae? The point of science is not the holy grail but the quest—the searching and the asking. Let us hope there will never be a final theory.

We now live in an environment where the idea that there may be a deeper, more unified theory has become completely discredited, through the efforts of many, with Hawking playing an unfortunate part.

If you have any interest at all in Hawking's story, you owe it to yourself to read this book. It's a rich and thoughtful examination of his life and work, pushing aside the myth and bringing out the much more interesting reality behind it.

Update: There's now a review of the book by Frank Wilczek in the New York Times.

Comments

1. **Sabine**
   April 9, 2021

   I suppose it was a difficult branch, not bran?

   I'm just here to say I've read the book too and also recommend it.

2. **Martin Visser**
April 9, 2021

I can't quite make out what this phrase was meant to say – "and I think the consensus is that there was no there there."

3. **Davide Castelvecchi**
April 9, 2021

I have a much more positive view of The Grand Design. For one thing, it's well written and extremely readable, contrary to A Brief History of Time. And the central thesis of the book is precisely that there may not be one ultimate theory, only a number of partial theories, each explaining a limited range of phenomena. I found that surprising and refreshing because it suggested that Hawking (to the extent that he had anything to do with the contents of the book) had decided to move on from his "reading the mind of God" propaganda.

4. **Genghis Khan**
April 9, 2021

Whenever anyone mentions Leonard Mlodinow, I cannot help but think of Langlands's incredible review of his book "Euclid's Window".

5. **Philip Gibbs**
April 9, 2021

It's not unusual for physicists to do their most original work before the age of 40. Hawking gave us singularity theorems, Hawking radiation, quantum cosmology and much more. He brought new mathematical insight and tools to work on general relativity and opened up the field of quantum gravity when most physicists had no idea how to make any progress. He may not have solved the black hole information paradox, but he was the one who highlighted the significance of the problem in the first place and that is at least as important.

It's futile to compare the achievements of physicists who worked on different topics many years apart, but Hawking was a great physicist by any standard. In the UK his work was the subject of television documentaries from the mid 1970s, long before he started writing books, and it was very clear that being in a wheelchair was not the main reason for his fame. Nobody gets to be the Lucasian Professor of Mathematics at Cambridge because of media popularity.

If he went on to popularise his work when it became difficult for him to keep up with later developments due to his condition, then that is to be celebrated. It was probably beyond human reach for him to take in the new mathematics of string theory enough to contribute to research given his disabilities, but he had worked on supergravity and could certainly see that it was a worthy natural progression. There is a wide range of opinions on the current state of subjects like M-theory, supersymmetry and multiverses among physicists and not everyone agrees with your conclusions. The status of the subjects may well be overhyped but the endgame has yet to be played out.

If Hawking needed more and more help with writing his later books then that is understandable. It's safe to assume that he was able to set directions, read drafts and suggest changes. Being able to keep up any kind of active life required finances and he may not have had his name attached to some of these later works otherwise, but I am sure he kept his integrity intact. Both his scientific work and his popular books will continue to be highly influential for a long time, and rightly so.

6. **Alice T.**
April 9, 2021

"There is no there there" is how Gertrude Stein famously described the state of broadband access in Oakland, the city of her 1874 birth. For more on this:

https://www.wsj.com/articles/why-gertrude-steins-no-there-there-is-everywhere-1517589198

7. **Bill**
April 10, 2021

Who says the idea of a unified theory is discredited??

It's true that one or two first pass ideas didn't work so far, but come on, even the over-the-top hype machine (hawking included) and a couple of possibly-failed attempts does not mean the universe doesn't admit a simpler description than the standard model. It may be that unification is somehow truly, ultimately impossible, but the idea has barely just come into existence compared to the timescale over which we've been continuously simplifying our scientific theories of nature. I think people are generally smart enough to know not to take 20-30 years of limited work by a very finite set of people to mean that the whole idea of a unified theory is somehow "discredited" at this point.

8. **DSNYC**
April 10, 2021

It is worth pointing out here that had he lived not too much longer, there is every likelihood that Hawking would have shared the Nobel Prize with Roger Penrose.

9. **Peter Woit**
April 10, 2021

Bill,
I should clarify what I see as Hawking's role in this, it's complicated. For evidence that the idea of a unified theory has been discredited, see the Gleick quote above, also Davide Castelvecchi's comment (both of them are excellent science journalists). There are many examples of this from leading physicists that I've documented here on the blog, the latest was a couple weeks ago from Robbert Dikjgraaf, the IAS director, see here
https://www.math.columbia.edu/~woit/wordpress/?p=12245

What's driving this are various forces. The main one is the failure of the string theory unification program, together with the decision by many string theorists to refuse to acknowledge this, but to instead try to make the case that the conventional idea of a unified theory should be abandoned. Dijkgraaf is one example.

Another force driving this is people like Kaku and his recent book. He's loudly making the case for unification, but a case based on ridiculous claims about string theory and its testability that can't be taken seriously. This does just as much damage to the case for unification as Dijkgraaf.

Hawking is a more complicated story. Back in 1980, when he was still quite active and very aware of what was going on in the field (and pre-string theory), he made a strong case for unification via N=8 supergravity. That theory doesn't quite work as a unified theory, although it comes remarkably close. His 2010 "The Grand Design" book was completely different. For a detailed discussion of it, see my review at the time, which I titled "Hawking Gives Up"
https://www.math.columbia.edu/~woit/wordpress/?p=3141

That book really is a rehash of string theory multiverse pseudo-science, explicitly abandoning his 1980 point of view. Does it really reflect a thoughtful evaluation by Hawking of all the evidence or is it Leonard Mlodinow's uninformed retelling of what people like Susskind were pushing at the time? I'd argue it's likely more the latter than the former.

10. **Alan**
April 10, 2021

The book sounds interesting. My perspective may be biased because I did my PhD with Stephen in the late 1970's so I was there when he became Lucasian Professor and I heard his talk about the possible end of theoretical physics. At that time his reputation among physicists was incredibly high due to over ten years of highly successful research. This included his analysis of general relativity, building on tools that Penrose had developed, and his work on quantum field theory on curved spacetime which culminated in his theory of Hawking radiation. At that time he seemed, at least to a lowly graduate student like myself, to be completely on top of the technical material. He had a great sense of humour so I wouldn't be surprised if he joked about how he became Lucasian Professor just as he joked that deciding to study quantum gravity was the first sign that you are going mad. I had much less contact with him after my PhD because I decided that I didn't have a clue about how to quantize gravity (and suspected that nobody else did either) so I left physics for the new and obscure subject of AI. When his book came out I was surprised that half way through it switched from material that was well established (e.g., by experiments or by strong circumstantial evidence) to ideas that were much more speculative, but without this being signaled to the reader. I found this, and the popularity of the book puzzling and always assumed that the success of the book was largely due to the photo on the front cover. The lack of distinction, in popular accounts of physics, between theories which are speculative and those which are experimentally verified has grown enormously over time and is the main reason why I keep coming back to this blog for a dose of heathy skepticism. It is now more than forty years since Stephen gave his lecture about whether the end was in sight for theoretical physics. My personal view is that in science, like in many things, it is important to be in the right place at the right time with the right skills. Quantizing gravity, or unifying physics, may be almost impossible to do by pure thought and might need to wait until there are experiments to guide us (perhaps finally there is a crack in the standard model). When I started my PhD I asked Stephen if there were any experiments to guide a theory of quantum gravity and he answered no with a big grin.

11. **Bill**
April 10, 2021

Hi Peter,

Ok, fair that some people have somehow declared either failure or a moved the goal posts with respect to unification. However, just a comment from a "boots on the ground" theorist, I would say pretty much everyone actively working (e.g. not including Dijkgraaf) on hep/qg is still quite open to the idea of unification.

OTOH, true enough that few people are very actively working on it, since at the moment there's now a lack of promising immediate directions. In my biased/anecdote-based opinion, I think most people (especially younger people) would view this as likely a temporary situation rather than some fundamental roadblock. An undercurrent of current work is to poke around looking for news ways to approach unification, rather than to throw our hands up and declare it's impossible.

I suspect we would probably agree that the set of very public voices (e.g. Kaku and company) is an unfortunately very unfaithful representation of the field as a whole.

12. **Peter Woit**
April 10, 2021

Bill,
It's not that most theorists think unification is impossible, more that the general attitude now is that progress on unification is not on the agenda, but must await finding M-theory, or replacing space-time with amplituhedra or error-correcting codes, or finding a new particle at the LHC, or some new young Einstein telling us all the answer, etc. But that situation is a complicated topic, with people having many different attitudes.

Staying with the topic of Hawking, I think the situation now is dramatically different than that of the 1980 when Hawking gave his talk and I was a grad student. At the time we'd just been through a period of huge progress on unification and there seemed every reason to believe that more was possible, so this was very much worth thinking about. Now we're in very different times, with Hawking's 1981 talk often treated the way Gleick treats it: a hubristic worship of a false idol.

I see this history very differently: Hawking was not wrong to argue that N=8 supergravity was remarkably close to a unified theory and such a thing might be within humanity's grasp. But within a few years after 1980, Hawking got too sick to keep working at the highest level, and most theorists working on unification turned to something different which really was a false idol. Leonard Mlodinow's 2010 book was an example of where this led, perhaps a healthier Stephen Hawking would had a very different point of view.

13. **Mark Hillery**
April 11, 2021

The Grand Design should really not be referred to as Leonard Mlodinow's book. An account of the writing of that book can be found in Leonard's recent memoir about working with Hawking. Hawking was very much involved in its writing. To my knowledge, and the memoir reinforces this, the views expressed in the The Grand Design are mainly those of Hawking.

14. **Peter Woit**
April 11, 2021

Mark Hillery,
I haven't seen Mlodinow's recent book about working with Hawking, remember though reading something many years ago from him about this experience. From that and from the accounts of others working with Hawking in Seife's book, my impression is that Hawking's communication difficulties in later life meant that he was getting all his information second-hand, filtered through others. One thing I don't know about is the extent to which he could, for instance, read a book or article on his own.

My point about attributing the book to Mlodinow is somewhat of an exaggeration (likely the book reflected Hawking's views accurately on many topics), but I think not when it comes to many scientific rather than philosophical issues. As an example, in my posting about the book, I quoted this:
"various calculations that physicists have performed indicate that the [super]partner particles corresponding to the particles we observe ought to be a thousand times as massive as a proton, if not even heavier. That is too heavy for such particles to have been seen in any experiments to date…"
This is misleading nonsense. Maybe it's Hawking's own nonsense, but I strongly suspect it's nonsense that Mlodinow got not from Hawking but from somewhere else.

15. **Doug McDonald**
April 11, 2021

Peter: what does "finding M theory" in " more that the general attitude now is that progress on unification is not on the agenda, but must await finding M-theory" mean?

16. **Peter Woit**
April 11, 2021

Doug McDonald,

I think a common attitude among “string theorists” these days is that any hope of unification must await first finding out what M-theory=non-perturbative string theory is, i.e. actually have a full definition of what string theory is. The problem with this is that it’s a problem that has been around for 25 years, with no one having any viable ideas, so hardly anyone working on it.

17. **Lee Smolin**
April 17, 2021

Dear Peter,

While we are talking about relativists, one of the very greatest, Ezra Ted Newman, died on March 24. He made many lasting contributions to our understanding of general relativity (Kerr-Newman black holes, Newman Penrose spinors, heaven theory, (related to twistor theory and many others) and was much loved by many friends and colleagues.

Thanks,

Lee

18. **Peter Woit**
April 17, 2021

Thanks for letting us know Lee. I noticed there’s an obituary here
https://www.utimes.pitt.edu/passings/physics-ted-newman
Also, an older article about Newman here
https://www.chronicle.com/article/a-humble-heavyweight-in-physics-finally-gets-his-due/
compares him to Hawking:
“the physicist Stephen W. Hawking, who is about as good at attracting attention as Mr. Newman is at deflecting it.”

19. **Luysii**
April 17, 2021

Hawking did much more than physics. I ran a muscular dystrophy clinic for 15 years. I made sure my ALS patients knew of him (they didn’t in the 70s). Hawking gave them all hope, something no physician could do.

Finished Some Things

April 13, 2021
Categories: Uncategorized

I've now finished with two things that I've been working on over the last year or so:

- The paper explaining my proposal for "Twistor Unification" is now done and uploaded to the arXiv, see here.
- I've finished lecturing for the course on quantum mechanics for mathematicians that I've been teaching this academic year. Because of the Covid-required online format for the lectures, they could easily be put on Youtube, where they're available here. I'm hoping to never ever have to teach this way again, so don't expect to ever again be producing Youtube lectures. The lectures pretty closely follow my book, and I had been hoping to work on improving and expanding the text. Unfortunately, partly due to laziness and partly due to the twistor stuff, while I found a lot in the book that needs improvement, I didn't find the time to do the necessary rewriting and writing. I do however have a notebook full of notes on what needs to be done.

For the future, I'm hoping to go on some sort of vacation in a couple weeks, and soon get back to work on some of the major issues raised by the unification proposal (much of which is very sketchy, a lot to be done). I hope to do quite a bit of traveling the rest of this year, likely won't be teaching in the fall, but probably will be teaching the quantum course again next year during the spring semester. At that point perhaps I'll get finally get around to the project of rewriting and expanding the quantum book.

Comments

1. **Low Math, Meekly Interacting**
   April 13, 2021

   Another eclipse?

2. **Peter Woit**
   April 13, 2021

   LMMI,
   Unfortunately the only total solar eclipse in the next couple years is December 4 in Antarctica, which is not so easy to get to (could be done, but the cost is somewhat prohibitive). I'm hoping to make it to Europe late summer or fall, Covid permitting…

3. **Jim Lai**
   April 13, 2021

   Thank you Prof. Woit for the book and the online course.

   I am a self learner. I am still in Chapter 6. I am taking my time (doing it carefully and making notes in LaTeX) and enjoying the study. This may be the only good thing that has come out of COVID.

4. **Martin van Staveren**
   April 14, 2021

   In the intro of the book it is stated

   "Some of the main differences with standard physics presentations include:
   • The role of Lie groups, Lie algebras, and their unitary representations is
   systematically emphasized, including not just the standard use of these to
   derive consequences for the theory of a "symmetry" generated by operators
   commuting with the Hamiltonian."

   If the group is not that generated by an operator commuting with the Hamiltonian, then what does the group refer to? This is the conceptual problem of gauge field theory: we just posit a local non-abelian symmetry and plop there is the gauge field. But why demand this local non-Abelian symmetry?

5. **Martin**
   April 14, 2021

   From p. 34:
   > An argument from beauty can be made …
   You might get some backreaction from a comment like that

6. **Peter Woit**
   April 14, 2021

   Martin van Staveren,

   The reason for local symmetry (Abelian or non-Abelian) is that, while the theory of a free field is not invariant, the theory of a field coupled to a gauge field is, so this gives you an interacting theory. In the usual way of thinking about this, there's not much representation theory going on in the case of gauge symmetry: you're looking for states that are trivial representations of the gauge symmetry.

   For the sort of thing I had in mind in what you quote, two examples are:

   1. Lorentz boosts: there are a unitary symmetry of the state space in a relativistic theory, they don't commute with the Hamiltonian.

   2. For a system of oscillators, some quadratic operators in the P,Qs or in the a,a^dagger s that don't commute with the Hamiltonian (e.g. different number of annihilation and creation operators) can be understood as the Lie algebra representation operators for a larger (symplectic) group than the usual unitary group of dimension n acting on n oscillators. This action of the larger symplectic group has physical significance, you can use it for instance to understand squeezed states in quantum optics (this is explained in detail in the book).

7. **Peter Woit**

April 14, 2021

Martin,

I'm hoping so, that was meant to provocatively challenge a common point of view (one with a very eloquent expositor…).

8. **Martin van Staveren**
April 14, 2021

"The reason for local symmetry … is that, while the theory of a free field is not invariant, the theory of a field coupled to a gauge field is, so this gives you an interacting theory".

Yes, that what I meant, The need for a local symmetry follows from the need for an interacting theory. Usually the logic runs in the opposite direction.

9. **anon**
April 14, 2021

Peter, I found your book outstandingly clear, benefiting from you teaching the subject and getting feedback from your students no doubt. But I feel the exercises should be more carefully integrated into the relevant sections rather than left at the back.

10. **Peter Woit**
April 14, 2021

anon,
I agree completely. Probably the weakest aspect of the book pedagogically is that it needs more exercises, ideally I'd think at least twice as many, integrated with the chapters. This is among the things I'd hope to work on this past year while teaching the course, but never got to. Maybe next year….

11. **Low Math, Meekly Interacting**
April 15, 2021

I'm guessing this ranks pretty low on your list of priorities, but worth a try: Any chance of an expository piece, either here or elsewhere, meant to convey the main concepts of "Twistor Unification" to non-experts?

For instance, it appears you are identifying what I might have understood to be a "mathematical trick", i.e. analytic continuation (a.k.a. Wick rotation in this instance, I think), with an actual physical process, i.e. electroweak symmetry breaking. I wonder to my naive self if this implies something about the nature of spacetime before and during the corresponding cosmological epoch. What does it imply in general to take Euclidean spacetime "as fundamental"? That Minkowski spacetime is not? And so forth. I probably can't even formulate the right questions, which makes this attempt to understand all the more embarrassing. But I am interested, nonetheless, it what it all "means".

12. **Peter Woit**
April 15, 2021

LMMI,
I had been thinking that there are aspects of this that I should try and write about in a way that would be as widely accessible as possible, maybe as a blog post. An example would be the the basic picture of twistor theory and how it relates Euclidean and Minkowski space. It took me a while to get the right picture in mind, and what I wrote in the paper doesn't easily convey that. So, may be will try that sometime soon.

On the analytic continuation business, there I did try and write things out in an expository manner in the paper. There's a lot to say about this, but one simple fact is that if you say you are only going to think about Minkowski space-time and functions on Minkowski space-time, avoiding analytic continuation, that's not actually an option. As you find in every textbook and as I work out in the paper, if you try and write down the simplest theory of free particles you end up with ill-defined formulas (this is true even for the propagator of the non-relativistic free particle). You have to do something to make sense of these formulas, the textbook tells you what to do as an "i\epsilon prescription". When you do that, you're defining your propagator as the boundary value (as \epsilon goes to zero from the positive direction) of an analytic continuation.

If you look at almost all the non-perturbative work on QFT, it is actually done in Euclidean space-time, with the idea that at the end of the calculation you analytically continue to Minkowski. The hard to believe claim I am making is that the way space-time symmetries behave under analytic continuation is much trickier than you might think (the behavior is straightforward for correlation functions, but for states you need to take into account the fact that you must break Euclidean invariance to define states and to know how to analytically continue to Minkowski).

I've been trying to think more about what this point of view implies for quantum gravity, and what strikes me is that how to even formulate the problem there is quite unclear. Beyond a recent paper of Kontsevich and Segal where they try and analytically continue in a space of metrics, I know of very little that seems to have been done about this (would love to hear from those who know more).

13. **Arnold Neumaier**
April 16, 2021

Peter,

"what this point of view implies for quantum gravity, and what strikes me is that how to even formulate the problem there is quite unclear."

Canonical quantum field theory is formulated in curved spacetime (where analytic continuation makes sense only in special cases) by defining the propagators using the theory of linear hyperbolic PDEs rather than by an $i\epsilon$ prescription. See, e.g.,

Hollands, Stefan, and Robert M. Wald. "Quantum fields in curved spacetime." Physics Reports 574 (2015): 1-35.

This works very well perturbatively. It also covers canonical gravity if one accepts that for higher and higher accuracy you need to use more and more coupling constants.

14. **Peter Woit**
April 16, 2021

Arnold Neumaier,

Except, see page 68 of that paper
https://arxiv.org/abs/1401.2026
I don't think that using microlocal analysis really gets you away from having to consider analytic continuation.

In general, I just don't understand the point of view that one should avoid having analytic continuation a part of your fundamental theory. One gains nothing and loses a lot.

15. **Arnold Neumaier**
April 19, 2021

Peter,

Microlocal analysis allows and heavily uses analytic continuation on tangent and cotangent spaces (which are flat vector spaces), but not on the manifold!

'page 68 of that paper' is a distribution in flat space, hence has no bearing on curved space modeling.

'I just don't understand the point of view that one should avoid having analytic continuation a part of your fundamental theory. One gains nothing and loses a lot.'

Analytic continuation of curved space-time without symmetries is simply ill-defined. p.7/8 of the above paper by Hollands and Wald states:

"However, a general curved spacetime will not be a real section of a complex manifold that also contains a real section on which the metric is Riemannian. Thus, although it should be possible to define "Euclidean quantum field theory" on curved Riemannian spaces [65], there is no obvious way to connect such a theory with quantum field theory on Lorentzian spacetimes. Thus, if one's goal is to define quantum field theory on general Lorentzian spacetimes, it does not appear fruitful to attempt to formulate the theory via a Euclidean approach."

They don't give a reference for their no-go statement, but I think this is a theorem of differential geometry. Thus one loses nothing but gains logical correctness.

16. **Igor Khavkine**
April 19, 2021

Peter Woit: "In general, I just don't understand the point of view that one should avoid having analytic continuation a part of your fundamental theory. One gains nothing and loses a lot."

It's just a sad fact of life that analytic continuation is a luxury that not everyone can afford (borrowing a social justice metaphor). For a non-flat Lorentzian manifold, the existence of a Wick rotation via analytic continuation is a highly restrictive condition (one of course has to first make a reasonable definition for what Wick rotation might even mean in general). Questions about it occasionally come up on MathOverflow. Here's an example where there was a somewhat detailed discussion of the differential geometric obstructions.

Presumably, a satisfactory theory that would include both standard model and gravitational physics, would eventually have no problem describing particle creation/scattering in the vicinity of two merging black holes emitting gravitational waves into an expanding cosmological background. Such a theory does not yet exist, but if one were to also require the ability to Wick rotate as a fundamental property of such a theory, it immediately becomes highly suspect whether these two requirements are actually compatible.

17. **shantanu**
April 19, 2021

Peter: are you submitting this manuscript for publication?

18. **Peter Woit**
April 19, 2021

shantanu,
Will likely do so, need to look more into what an appropriate journal would be (e.g. non-problematic business model and copyright policy, able to referee an unusual manuscript).

19. **Peter Woit**
April 19, 2021

Arnold Neumaier,
I haven't followed through all the details in Hollands-Wald, but it looks to me like they still need to define distributions as boundary values of something holomorphic, so are assuming some analyticity of the manifold (so locally analytically continuing it) and the complex analytic story is not just on the tangent space.

Arnold Neumaier/Igor Khavkine,
I'm well aware of the usual problems with
1. Analytically continuing an arbitrary classical Lorentzian solution to a Euclidean one.
2. Making sense of Euclidean space-time gravitational path integrals.

The relation of Euclidean and Lorentz in quantized gravity theory has always seemed highly unclear to me (if anyone knows a good discussion, would like to hear about it). Perhaps the twistor picture gives a different perspective, as might the proposal here for unification with electroweak degrees of freedom.

20. **DKepler**
April 20, 2021

I'm well aware of the usual problems with
1. Analytically continuing an arbitrary classical Lorentzian solution to a Euclidean one.
2. Making sense of Euclidean space-time gravitational path integrals.

The relation of Euclidean and Lorentz in quantized gravity theory has always seemed highly unclear to me (if anyone knows a good discussion, would like to hear about it).
—
I am pasting a response which might be relevant for this, from the comment section of an earlier blogpost of yours: **(Imaginary) Time Asymmetry** (Aug 6, 2020)
———————————————————————————————————-
The key idea behind this (first noted, to the best of my knowledge, by Hawking & Ellis (HE)) is based on the following mathematical result: given a Lorentzian metric g_L and a nowhere vanishing timelike direction field U, one can always construct a Euclidean metric g_E. As is well known, non-compact manifolds always admit such a vector field, as well as compact manifolds with Euler number zero.

Some references where this HE observation was discussed in the context of QFT are Candelas & Raine, PRD15, 1494 (1977) and Visser 1702.05572, while recent generalization and consequences for GR, euclidean QG, and euclidean action can be found in Kothawala, arXiv:1705.02504, arXiv:1802.07055. The latter references also discuss transition from euclidean to lorentzian, instead of just the euclidean phase.

Hope this helps.

21. **Igor Khavkine**
April 20, 2021

Peter Woit wrote: “I haven’t followed through all the details in Hollands-Wald, but it looks to me like they still need to define distributions as boundary values of something holomorphic, so are assuming some analyticity of the manifold (so locally analytically continuing it) and the complex analytic story is not just on the tangent space.”

Just to clear out some potential miscommunications, here’s a bit of an explanation. The ideas about “wave front sets” and microlocal analysis referred to in the paper by Hollands & Wald do not require any kind of analyticlity of the spacetime. The basic idea of microlocal analysis is to look at the singularities of a distribution $D(x)$ by taking coordinate Fourier transforms $\hat{D}_\chi(p)$ of products $\chi(x)D(x)$, where $\chi(x)$ is a bump function with support small enough to fit into a single coordinate chart. As the Fourier transform of a compactly supported distribution, $\hat{D}_\chi(p)$ is of course analytic in the momentum variable p, but that is beside the point, as none of its p-local properties are invariant under changing the bump function $\chi(x)$ or changing the x-coordinate chart. What is invariant is the asymptotic behavior of $\hat{D}_\chi(p)$ for large p (“high frequencies”). This information is collected geometrically into a subset of the cotangent bundle of the spacetime, the “wave front set”. The key “Hadamard property” identifying physically reasonable n-point functions in QFT on curved spacetimes, as referred to by Hollands & Wald, is formulated purely in terms of this wave front set. The Hadamard property and its relatives are considered to be appropriate generalizations of all the $\pm i\epsilon$ prescriptions floating around in flat space QFT.

When the spacetime is analytic, one can indeed analytically continue the spacetime coordinates into complex directions and define distributions as boundary values of holomorphic functions. In that case, there absolutely is a relationship between holomorphicity properties (the size of the domain of the holomorphic function whose limit is being taken) and the wave front set (of the distributional boundary value). This relationship is captured by the “analytic wave front set”.

A quite detailed exposition of the above ideas can be found in the lecture notes arXiv:1901.10175 (or a bit shorter and down-to-earth one in arXiv:1412.5945, risking a bit of self-promotion 😅).

22. **Peter Woit**
April 20, 2021

DKepler/Igor Khavkine,

Many thanks for the clarifications and the references, those are very helpful.

Some History

April 29, 2021
Categories: Uncategorized

I'm heading out soon for a 10 day vacation in the Rocky Mountains, blogging likely to change from sparse to non-existent for the next couple weeks. I've come across the following things that people with an interest in the recent history of mathematics may find worthwhile:

- S. T. Yau over the past year has organized a series of talks on the recent history of mathematics, featuring prominent people in the subject giving expository talks on a topic, sometimes writing something up. The talks are available here, the write-ups here. I can especially recommend Nigel Hitchin's detailed explanation of the work of Michael Atiyah relevant to physics, much of which he was personally involved in.
- Lieven Le Bruyn at neverendingbooks points to some wonderful French math YouTube videos. Don't miss Alain Connes interviewing Serre, with Serre explaining that he doesn't know (or care) what a topos is.
- For a good account of the fascinating life of Alexander Grothendieck, there's Luca Signorelli's The Man of the Circular Ruins. I hadn't realized that some of the weirder writings from Grothendieck's later life are now readily available, for instance La Clef des Songes.
- For a long recent account by Langlands both of his recent ideas about geometric Langlands and his fascination with languages (including White Russian language instructors), see this letter to Yvan Saint-Aubin.

Comments

1. **Richard Townsend**
   April 29, 2021

   The Grothendieck article is truly fascinating – what a story!

2. **Michael Gogins**
   May 1, 2021

   I have been interested in Grothendieck for many reasons for many years. I think this article is absolutely the best introduction that I have read. Thanks!

3. **Luca Signorelli**
   May 2, 2021

   Hi Peter, thanks for the link! Grothendieck's story, personality and achievements are indeed fascinating. It's one of those historical figures that once approached "expand" in your field of view instead of fatally shrinking. No matter how complicated and often difficult to judge they may be.

4. **jsm**
   May 3, 2021

   The point of Langlands's letter is to explain that to create a geometric Langlands theory in the style of Hecke, that is to say with eigenvalues and eigenfunctions, we will need a theory other than that of the Russians.

   > To be more precise, the theory I described in my Russian article for the group GL(2) and elliptic curves begins with an article by Atiyah, an article that is in my opinion one of his best, and it remains, I believe, to do the same for any curve and any reductive group… It will be necessary to start by pursuing the ideas of Atiyah, but generalizing his article. But this will not, in my opinion, be easy. I haven't tried it myself, especially I don't know how to guess the structure of Bun(G) in general… The structure of Bun(G) being known, it will require an understanding of all the eigenfunctions attached to a given curve and a given reductive group. In general, this last question will be much more difficult than for elliptic curves, the case dealt with by Atiyah.

5. **Will Sawin**
   May 4, 2021

   @jsm,

   In connection with your summary it is probably worth mentioning that Pavel Etingof, Edward Frenkel, and David Khazdan recently (but earlier than the date appearing on the letter) formulated a version of geometric Langlands theory with eigenvalues and eigenfunctions, using the "Russian" theory, and without any kind of Atiyah-style explicit description of the points of Bun_G, in (https://arxiv.org/abs/1908.09677).

   They also handle the ramified case, at least for sufficiently mild (unipotent) ramification, which was something else that Langlands suggested as being interesting in this letter.

The Evolution of the Physicist's Picture of Nature

May 24, 2021
Categories: Uncategorized

Reading this Nautilus article about Julian Barbour led me recently to something I don't think I've ever read before, Dirac's 1963 Scientific American article The Evolution of the Physicist's Picture of Nature. There is a very famous quote from this article that I've often seen:

> It is more important to have beauty in one's equations than to have them fit experiment

but I was unaware of the context of that quote, in which the famous part is prefaced by "I think there is a moral to this story, namely that…" The story that Dirac had in mind was that of the discovery of the Schrödinger equation. Famously, Schrödinger first wrote down a relativistic wave equation (now known as the Klein-Gordon equation). This equation is what one quickly gets if one follows de Broglie's idea that matter is described by waves, and uses the relativistic energy-momentum relation. Here's the full story, as told by Dirac, giving his famous quote in context:

> I might tell you the story I heard from Schrödinger of how, when he first got the idea for this equation, he immediately applied it to the behavior of the electron in the hydrogen atom, and then he got results that did not agree with experiment. The disagreement arose because at that time it was not known that the electron has a spin. That, of course, was a great disappointment to Schrödinger, and it caused him to abandon the work for some months. Then he noticed that if he applied the theory in a more approximate way, not taking into ac count the refinements required by relativity, to this rough approximation his work was in agreement with observation. He published his first paper with only this rough approximation, and in that way Schrödinger's wave equation was presented to the world. Afterward, of course, when people found out how to take into account correctly the spin of the electron, the discrepancy between the results of applying Schrodinger's relativistic equation and the experiments was completely cleared up.
>
> I think there is a moral to this story, namely that it is more important to have beauty in one's equations than to have them fit experiment. If Schrodinger had been more confident of his work, he could have published it some months earlier, and he could have published a more accurate equation. That equation is now known as the Klein-Gordon equation, although it was really discovered by Schrödinger, and in fact was discovered by Schrödinger before he discovered his nonrelativistic treatment of the hydrogen atom. It seems that if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress. If there is not complete agreement between the results of one's work and experiment, one should not allow oneself to be too discouraged, because the discrepancy may well be due to minor features that are not properly taken into account and that will get cleared up with further developments of the theory.

There's another remarkable aspect of this Scientific American article, something about it that would be completely inconceivable today: they write down three equations, including both the famous non-relativistic Schrödinger equation for the Coulomb potential, as well as the relativistic Klein-Gordon version.

Dirac notes that Schrödinger found his formulation of quantum mechanics in a very different way than Heisenberg found his:

> Heisenberg worked keeping close to the experimental evidence about spectra that was being amassed at that time, and he found out how the experimental information could be fitted into a scheme that is now known as matrix mechanics. All the experimental data of spectroscopy fitted beautifully into the scheme of matrix mechanics, and this led to quite a different picture of the atomic world.

whereas

> Schrödinger worked from a more mathematical point of view, trying to find a beautiful theory for describing atomic events, and was helped by De Broglie's ideas of waves associated with particles. He was able to extend De Broglie's ideas and to get a very beautiful equation, known as Schrödinger's wave equation, for describing atomic processes. Schrodinger got this equation by pure thought, looking for some beautiful generalization of De Broglie's ideas, and not by keeping close to the experimental development of the subject in the way Heisenberg did.

At the end of the article, Dirac makes the case that progress in fundamental physics may not come from a theorist like Heisenberg finding a scheme to match experimental results, but from a theorist like Schrödinger pursuing mathematical beauty:

> It seems to be one of the fundamental features of nature that fundamental physical laws are described in terms of a mathematical theory of great beauty and power, needing quite a high standard of mathematics for one to understand it. You may wonder: Why is nature constructed along these lines? One can only answer that our present knowledge seems to show that nature is so constructed. We simply have to accept it. One could perhaps describe the situation by saying that God is a mathematician of a very high order, and He used very advanced mathematics in constructing the universe. Our feeble attempts at mathematics enable us to understand a bit of the universe, and as we proceed to develop higher and higher mathematics we can hope to understand the universe better.
>
> This view provides us with another way in which we can hope to make advances in our theories. Just by studying mathematics we can hope to make a guess at the kind of mathematics that will come into the physics of the future. A good many people are working on the mathematical basis of quantum theory, trying to understand the theory better and to make it more powerful and more beautiful. If someone can hit on the right lines along which to make this development, it may lead to a future advance in which people will first discover the equations and then, after examining them, gradually learn how to apply them. To some extent that corresponds with the line of development that occurred with Schrodinger's discovery of his wave equation. Schrödinger discovered the equation simply by .looking for an equation with mathematical beauty. When the equation was first discovered, people saw that it fitted in certain ways, but the general principles according to which one should apply it were worked out only some two or three years later. It may well be that the next advance in physics will come about along these lines: people first discovering the equations and then needing a few years of development in order to find the physical ideas behind the equations. My own belief is that this is a more likely line of progress than trying to guess at physical pictures.

The context for his famous quote is thus an argument for pursuing fundamental physics by looking for mathematical beauty, not giving up on a beautiful equation just because it doesn't seem to fit experiment. As in Schrödinger's case, more effort may be needed to understand the actual relationship of the equation to reality.

Besides this argument, which I've always been well aware of and sympathetic to (despite not knowing the context in which Dirac was making it), there's something else I found very striking about the 1963 article. Dirac begins by explaining that the four-dimensional Lorentz symmetry of relativity is in a sense broken by the choice of a way of describing the state of the world:

> What appears to our consciousness is really a three-dimensional section of the four-dimensional picture. We must take a three-dimensional section to give us what appears to our consciousness at one time; at a later time we shall have a different three-dimensional section. The task of the physicist consists largely of relating events in one of these sections to events in another section referring to a later time. Thus the picture with four dimensional symmetry does not give us the whole situation. This becomes particularly important when one takes into account the developments that have been brought about by quantum theory. Quantum theory has taught us that we have to take the process of observation into account, and observations usually require us to bring in the three-dimensional sections of the four-dimensional picture of the universe.
>
> The special theory of relativity, which Einstein introduced, requires us to put all the laws of physics into a form that displays four-dimensional symmetry. But when

> we use these laws to get results about observations, we have to bring in something additional to the four-dimensional symmetry, namely the three-dimensional sections that describe our consciousness of the universe at a certain time.

Dirac also refers to work on canonical formulations of general relativity, aimed at quantizing gravity:

> … if one insists on preserving four-dimensional symmetry in the equations, one cannot adapt the theory of gravitation to a discussion of measurements in the way quantum theory requires without being forced to a more complicated description than is needed bv the physical situation. This result has led me to doubt how fundamental the four-dimensional requirement in physics is. A few decades ago it seemed quite certain that one had to express the whole of physics in four-dimensional form. But now it seems that four-dimensional symmetry is not of such overriding importance, since the description of nature sometimes gets simplified when one departs from it.

Thinking about twistor unification has led me to some similar thoughts: a Euclidean formulation of quantum theory requires picking a choice of imaginary time direction and breaking SO(4) symmetry in order to define states. Dirac thinks of our consciousness as giving us access to the state of the universe defined on a 3d slice, but the twistor point of view is even more directly related to our conscious experience. A point in space time is defined by the sphere of light rays through the point, and it is this sphere that our vision gives us direct access to, with 3d space something we make up out of these spheres.

A common argument against Dirac's point of view is that it's engaging in mysticism. For another recent article that touches in a different way on the mystical nature of a discovery about fundamental physics, see this interview with Frank Wilczek, where he tells this story:

> The mystical moment came while I was visiting Brookhaven National Laboratory, on Long Island. Somehow—I don't remember how, exactly—I wound up alone, standing on a jerry-rigged observation platform above a haphazard mess of magnets, cables, and panels. This was a staging area for assembling detectors and renovating pieces of the main accelerator there, the Alternating Gradient Synchotron (AGS). I must have gotten separated from my host for a few minutes. In any case, there I was, alone inside an aircraft-hangar-sized metallic box, staring down at the kind of equipment that people use to explore the fundamentals of Nature experimentally.
>
> And then it happened. It came to me, viscerally, that the intricate calculations I'd done using pen and paper (and wastebasket) might somehow describe this entirely different realm of existence—namely, a physical world of particles, tracks, and electronic signals, created by the kind of machinery I was looking at. There was no need to choose, as philosophers often struggled to do, between mind or matter. It was mind and matter. How could that be? Why should it be? Yet I somehow, I suddenly knew that it could be so, and should be so.
>
> That was my mystical experience. I warned you that it was ineffable.

Comments

1. **Arnold Neumaier**
   May 24, 2021

   "It may well be that the next advance in physics will come about along these lines: people first discovering the equations and then needing a few years of development in order to find the physical ideas behind the equations."

   Didn't string theorists try to do just this – except that many years of development didn't produce the hoped-for reward?

2. **CWJ**
   May 24, 2021

   The problem with the argument by beauty is, who gets to decide something is beautiful? I can assure you, the crackpots who come to my office and, more frequently now, send me e-mails (maybe they post on Tik-Tok as well, but I don't follow that), believe their ideas are beautiful…. and they get quite angry when I don't agree.

3. **Nitin Nitsure**
   May 25, 2021

   Dirac says "What appears to our consciousness is really a three-dimensional section of the four-dimensional picture." What section is this? What reaches us at any instance is some information from the past null cone and its interior, which is a 4-dimensional region of the spacetime. Even if the spacetime is flat Minkowskian, all the other points on the hyperplane t=0 of our instantaneous rest frame are separated from us by spacelike intervals, so this hyperpalne cannot be a part of our consciousness. In the presence of gravity, it is interesting to note that without simplifying — and quite unrealistic — cosmological assumptions, the past null cones of points in the spacetime are horribly tangled, and global foliations by spacelike smooth hypersurfaces do not exist in general. The latest blog entry of David Mumford titled "Ruminations on cosmology and time", dated March 1, 2021, makes this point quite forcefully.

4. **Peter Woit**
   May 25, 2021

   Arnold Neumaier/CWJ,
   I've often argued that string theory unification is not a beautiful idea (the string theory landscape is the antithesis of beautiful).
   Dirac's argument I think is not so much that you should use beauty to evaluate the success of a theory, but you should use it as a criterion for choosing a promising idea to pursue. That different people will disagree about what is beautiful then just implies that different people will pursue different research programs, which is fine.

   Nitin Nitsure.
   I don't know why Dirac was bringing our consciousness into it. It seemed he was just saying that the usual way to get a state space in a relativistic theory is to choose a spacelike hypersurface.

5. **Mark Paris**
   May 25, 2021

   Dirac's Sci Am article is cited in Ch. 1 of Weinberg's QTF. To the unintiated: both the original article (Dirac) and the volume (Weinberg) are 'must read.'

6. **Art**
   May 25, 2021

   In a humorous example of your observation, I searched in vain for equations in the 2010 reprint (that you linked to) of the original 1963 Dirac SA article.

7. **Klavs Hansen**
   May 26, 2021

A bit paradoxical, isn't it, that Dirac's preference for selecting beautiful theories over those based on empiri is based on the empirical evidence of how well the former work.

8. **Peter Woit**
   May 26, 2021

   Art,
   Good point. SciAm I guess no longer has the technology to reproduce an equation. For the original, with equations, one source is
   https://www.jstor.org/stable/24936146

9. **Low Math, Meekly Interacting**
   May 26, 2021

   I am and always will be highly skeptical of reliance on esthetics, though of course scientists are human beings, and need to follow their instincts and intuitions to a certain extent to be able to function at all. Naive falsificationism is also, of course, inadequate and theorists shouldn't be held in thrall to every new bit of data that experimentalists generate. This is clearly especially true in a new field.

   But Dirac himself seemed to have followed his own esthetic sensibilities and doubts about empirical data right off a cliff with his "large numbers hypothesis", and for every example of beauty shining a guiding light, I bet there are at least a thousand for which it was the road straight to perdition. I simply can't put much faith in "beauty", except as an occasional source of inspiration or motivation to try harder, which we all need at times in one form or another. The full account of human history simply precludes ascribing it any greater significance. It always seems to be true only with a very selective form of hindsight.

10. **Frank Wilhoit**
    May 26, 2021

    "Beauty" in equations is (A) in the eye of the beholder and (B) very largely a matter of the level of abstraction of the notation. The purpose of abstract notation is to manage complexity; that is not a matter of beauty, but of legibility, and it is a double-edged sword.

    Dirac's point, if he had one, was that ugly equations (cf. A and B above) carry, or ought to carry, some degree of presumption against their correctness (he was more explicit about this elsewhere). But the reason why we appreciate beauty in reality is not because it is universal, or even typical; it is because it is exceptional.

11. **David Roberts**
    May 27, 2021

    Even worse, the text refers to at the equation(s) that is/are no longer there, so clearly the editorial capacity is somewhat lacking too.

12. **Eli Rabett**
    May 27, 2021

    Perhaps a better description than beauty of what is sought by physicists is that the description be terse

13. **Nitin Nitsure**
    May 28, 2021

    Peter Woit

    Thank you for your explanation of Dirac's quote. However, you did not address the point about a possible lack of spacelike foliations. I am curious about the following points, and request you to shed light:

    (1) Arguably, some of the most `beautiful' spaces are homogeneous spaces G/H. A lot of physics is indeed done on such, with flat Minkowski space being an example. Representation theoretic methods go furthest for these spaces. However, the actual cosmological spacetime is said to be quite inhomogeneous, riddled with blackholes which are far from symmetric, and has a complicated global topology. (This will have to be called `ugly' if beauty is understood via symmetry.) This has two consequences: On the one hand there is no nontrivial global symmetry, and so the role of representation theory will remain confined to what comes via gauge theory (via representations of the structure groups of some principal bundles on the spacetime). On the other hand, there is no reason to believe that the cosmological spacetime admits a good enough global foliation by spacelike hypersurfaces to enable the posing of physics problems in the familiar evolution (on a state space) format that you alluded to in your answer to my comment.

    (2) It is interesting to see from your extensive quotation that Dirac recommends an approach to physics via advanced mathematics. I remember reading somewhere that Dirac never recognized in his writings the difference between a hermitian operator and a self-adjoint operator, an important distinction when the operators are unbounded. Dirac's braket notation has no room for the domain of the operators, which is of importance here. As is well known from 1930's, the definition of the braket of two unbounded self-adjoint operators will need us to apply the spectral theorem for unbounded self-adjoint operators, as the intersection of their domains can be too small. How does this square with Dirac's recommendation not to shy away from using whatever mathematics that is needed for the physics that we want to understand? Einstein managed to learn the differential geometry that he needed, so what kept Dirac away from functional analysis?

    (Note: I am not a professional physicist, and I will understand if you regard the above as unsuitable for your blog for whatever reason. In that case, I will appreciate a private answer. Thank you.)

14. **Nitin Nitsure**
    May 28, 2021

    A small correction: I should have said that (as is well known) the difficulty in defining the braket [A,B] in a straight forward way as the difference AB -BA is that the inverse image by one operator of the domain of the other operator may be too small, that is, it can fail to be dense in the Hilbert space. This is where one needs some more functional analysis, namely, the spectral theorem for unbounded self-adjoint operators and Stone's theorem.

15. **Martin S.**
    May 28, 2021

    It looks like some survivor bias with having amounts of subjectively pretty theories, but only mentioning the very existence of the correct one. I understand though that Dirac would fall for it after the distrust he had to withstand before positrons got confirmed. But hey, if a theory is already successful on some stuff, one can cautiously trust it elsewhere too.

16. **Peter Woit**
    May 28, 2021

Nitin Nitsure,
1. I don't know what Dirac would have said about that sort of problem with the usual formalism. I think he was just referring to the way the formalism deals with the simplest situation.
2. Dirac was arguing for the significance of mathematical beauty, not for advanced mathematical technology, especially not for the importance of analysis involving precise definitions and rigorous proofs.

17. **S**
May 30, 2021

It seems to me to be something of a category mistake to attempt to refute Dirac's argument for beauty with a cry for falsifiability or something. It seems manifest that experimental testing/falsification is how a theory must ultimately be evaluated, but beauty can be a very good guide as to what to work on in the first place; or at least, if one wants to take it to be so, who can complain?

(Of course, nothing about the need for experiment truly seems "manifest" after string theory, and admittedly, people other than Peter or Dirac have tried to put the latter's argument into service for ditching falsifiability altogether, so the category mistake may be a natural one at this point in history….)

18. **Curious Mayhem**
May 31, 2021

Shocking: the years when Scientific American was a serious magazine. The article is a must read, indeed.

String theory was never an example of mathematical beauty, unlike, say, the Dirac equation. The notion that string theory is beautiful is yet another part of the hype that's surrounded the subject for almost 40 years.

19. **Peter**
May 31, 2021

Was beauty really the guiding principle when Dirac discovered his equation? I always thought his most important motivation was that he was looking for an equation with only a first-order derivative in time.

It was discovered (relatively rapidly, if I remember correctly) that his equation was covariant (as it should be) and that half of the solution, if I may say so, reproduced the Pauli matrices for electron spin. What Dirac suggested for the other half, his hole theory, can hardly be described as beautiful.

So if you look at the genesis of the Dirac equation, the prime motivation seems to have been not beauty but physical intuition (that first order derivative in time). Acceptance that the equation was interesting came because it was related to experimental results (electron spin). The hole theory was rather ugly, but of course, later, positrons were discovered.

I'm not denying the Dirac equation is beautiful and immensely powerful, but I'm not certain that he pursued it because of its aesthetical qualities.

Non-empirical Physics

June 2, 2021
Categories: Uncategorized

I haven't been paying much attention in recent years to the philosophers of science studying "Non-empirical" or "Post-empirical" physics or theory confirmation. At various times I did write fairly extensively about this, see for instance here, here and here. By 2015 there was a conference in Munich on the topic, which led in 2019 to a volume of papers entitled Why Trust a Theory?

There's a new paper out along similar lines, String theory, Einstein, and the identity of physics: Theory assessment in absence of the empirical, evidently to appear in a journal special issue from a 2019 conference on Non-Empirical Physics from a Historical Perspective.

The reaction of most physicists to this sort of thing is exemplified by Will Kinney's tweet about the paper:

> WTAF

In the past few years I've been writing less and less here and elsewhere about the issue of evaluating string theory as physics, for several reasons:

- String theory has effectively gone completely post-empirical, decoupling from any possible relation to experiment. This Week's Hype used to be a regular feature here, devoted to debunking the numerous bogus claims regularly being made for how to "test string theory". One rarely sees these anymore, with the string theory community now having given up on this and somehow comfortably moved into a completely post-empirical mode.
- I'm actually much more sympathetic than most people to the idea that there is a serious and very interesting question about how to evaluate ideas about theoretical fundamental physics in the absence of viable experimental tests. But I haven't had much luck finding others who share my views. The reaction to blogposts like this recent one tends to be pretty uniformly scornful, that I'm just Lost in Math. The post-empirical philosophers of science deal with me differently, pretty much doing their best to ignore me (I don't make it into the extensive bibliography of the new paper on the arXiv).
- There are two other projects that seem to be a much better way to spend my time (the twistor unification stuff, and improving the textbook on QM and representation theory).

By the way, I notice that there is an arXiv trackback already for another blog entry about this paper, wondering if trackbacks here are still censored.

Well, that's all about this for now, best to take my own advice and go think about something else.

Update: I just ran across this AIP interview with John Schwarz from last year. Schwarz seems to feel that string theory unification is a huge success, despite the testability problem. On the failure to find the superpartners he and other string theorists expected, that's a problem for experimental physics, not for string theory:

> As I said, if supersymmetry is not discovered, there's a danger that experimental particle physics will die. If that happens, it would be tragic, but it wouldn't be the end of string theory. String theory will continue, regardless, and will continue to advance.

On the topic of answering those who argued that superpartners would not be found back in the 2000s, and who have put forward detailed criticisms of string theory unification, here's what he has to say:

> There were a couple popular books that attacked string theory about a decade or so ago. The authors clearly had chips on their shoulders. For people without a physics background it's not possible to assess whether what they're reading makes sense or not. But anyone with at least an undergraduate education in physics I think can recognize that they should not be taken seriously.

Comments

1. **Sabine**
   June 2, 2021

   There's also a new book called "On The Fringe" by Michael Gordin about the demarcation problem. It has a paragraph or two about string theory, but only to say "look, sometimes even scientists can't tell if it's science or pseudoscience" (not an actual quote, I paraphrase). The book is mostly about astrology, alchemy, bigfoot, and so on.

2. **Peter Woit**
   June 2, 2021

   Sabine,
   I don't think arguing over whether "string theory" is "science" or "pseudo-science" is worthwhile. Even before you get to the demarcation problem, your problem is that these days "string theory" has no specific meaning.

   What's accurate to say is that, even if you agree to put string theory unification on the science side of your science/pseudo-science demarcation, it's a failed idea about science. My problem with what most of the "post-empirical physics" philosophers of science are doing is that they're just producing excuses for failure. They are starting from the assumption that this can't be failed science, since its proponents are leaders of the field, rewarded with multi-million dollar prizes. So, it must be a new way of doing science.

3. **André**
   June 2, 2021

   The last sentence of the conclusion of the paper arxiv:2105.14342 reads: "let's … keep funding it as generously as before." The author thus states clearly that he is making money out of his paper. This is appalling. Were it politics, not science, this would not be called a conflict of interest, but simply corruption.

4. **Alessandro Strumia**
   June 2, 2021

   Entire published papers are not appearing on arXiv. Is it wise to leave such a resource, important for the whole international field, in the hands of a few individuals and private donors?

5. **Peter Woit**
   June 2, 2021

André,
I think the “keep funding it as generously as before” plea makes explicit that what’s going on here is what I described in my response to Sabine: an attempt to evade the unpleasant implications of failure (with losing funding just the crudest of such things).

6. **Peter Erwin**
June 2, 2021

André,

The author of the paper is a philosopher/historian of science, not a physicist working on string theory, so encouraging people to continue funding things like string theory isn’t going to give *him* any money. (Arguably there is a slight conflict of interest in that he finds the topic interesting, so he’d presumably be disappointed if it was shut down; but since he’s written a number of papers on Einstein, I suspect he’d do fine academically even if string theory was shut down.)

7. **Peter Woit**
June 2, 2021

Alessandro Strumia,

I don’t want to try and host here a discussion of the general topic of arXiv moderation (from dealing with comments, I know moderation is difficult…). To the extent the problem is with them censoring politically incorrect views (whether about string theory or anything else…), it’s a problem larger than a few individuals, would likely be there whatever their institutional structure is.

To describe my experience in dealing with the arXiv over the trackback matter, it was essentially impossible to communicate with anyone there, or even find out what decisions they were making and on what basis. After a huge effort involving a lot of time and asking for help from a lot of people, all I ever got was a message from a Cornell administrator saying a decision had been made by unknown parties based upon unknown criteria. To this day I have no idea who decided what about trackbacks, based on what information and what policy, or what their current policy is.

8. **Sabine**
June 3, 2021

Peter,

“My problem with what most of the “post-empirical physics” philosophers of science are doing is that they’re just producing excuses for failure”

Well, yes, kind of. Some of them, anyway. The problem is that most philosophers don’t actually understand the topic (string theory or quantum gravity or naturalness) in any depth, are afraid to put forward an original analysis of it, and then end up doing sociology more than philosophy. Unfortunately, they then don’t factor in what we actually know about the sociology of science.

Having said that, it actually is difficult to sort out the wheat from the chaff if you can’t do it by way of experiment because that’d take centuries. So we have to change our methods, and philosophers have a role to play in that.

Merritt did a pretty good job for MOND. I mean, as I write in my review, I have some objections to his conclusions, but this isn’t the point. The point is that this kind of analysis is very important. But Merritt is an astrophysicist who only recently switched to philosophy. I think we’d need someone like this in string theory. Or, preferably, several people.

Having said that, the book by Gordin I mentioned above makes a good point, which is that it often takes time for scientists to sort out the demarcation and also, the judgement changes over time. There are quite a few ideas that we currently regard as pseudoscience but that were once considered adequate.

All of which means, basically, you are making scientific history 😉

9. **Sabine**
June 3, 2021

And while I am at it, let me echo Alessandro’s concerns about the arXiv moderation. I have personally never had an issue with them, but then I am a soundly boring person. However, I wrote a blogpost some years ago about the arXiv moderation issue (brought to my attention by others) and ever since people have contacted me asking for help. Every once in a while I try to actually do that, but it never leads anywhere. I have found it impossible to figure out who is even making a decision on what, and in the end they’ll fall back on saying “if it gets published in a journal, we’ll accept it on the arXiv” which amounts to refusing to take responsibility. Most people are afraid of saying anything about it because their good standing with the arXiv moderation decides on their career. This is not a good situation. I really think the community should do something about it.

10. **Alessandro Strumia**
June 3, 2021

It seems to me that we now have two big problems at the same time:
High-energy physics wants to keep its historically large level of funding despite lack of new physics, while institutions are affected by an ideology that denies objectivity and views science as a system of power.
Will the field survive to a short-sighted compromise?

11. **Peter Shor**
June 3, 2021

The arXiv certainly needs moderation, otherwise it would fill up with papers that are written by crackpots and become completely useless.

I haven’t had any problem with it myself, but I think making the system more transparent (it being completely opaque now) would be a considerable improvement.

12. **maciej**
June 3, 2021

If you criticize string theory on arxiv your work is likely to be rejected. They can even disable your submission rights if you don’t stop submitting your criticism. The situation is very concerning, reminds me a kind of censorship of totalitarian systems. What arxiv needs is transparency but that comes with objectively chosen committee which for decades has not been objective at all
(The only hep-th related members of arxiv committee are pro-string-theory Jacques Distler and Paul Ginsparg https://arxiv.org/help/physics/#AdvisoryCommittee).

13. **Peter Woit**
June 3, 2021

maciej,
About my problems with the arXiv, all I can do is guess what they might be. A good guess seems to be the attitude towards this blog taken by a certain arXiv moderator right at the beginning. Those interested in history might want to take a look at the discussion in the comments right at the beginning of the blog, here

https://www.math.columbia.edu/~woit/wordpress/?p=3

14. **Claudio Paganini**
June 4, 2021

"I'm actually much more sympathetic than most people to the idea that there is a serious and very interesting question about how to evaluate ideas about theoretical fundamental physics in the absence of viable experimental tests."
Could you expand on this a little more. I have spend quite some time over the last 4 years thinking about how to judge the potential of a theory by its mathematical ingredients but so far I haven't really come to a conclusion that goes beyond gut feeling and a set of pitches that catch the attention of physicists. (given we have to decide which approaches to unification should be funded, I think it is crucial that we do have a better framework to gauge the potential of new ideas).

15. **Peter Woit**
June 4, 2021

Claudio Paganini,

I think this is a difficult and complex question, with no easy answer. A couple aspects of it are:

1. Initially, speculative ideas are hard to evaluate, since there is a lot about their implications that still has to be worked out. Most speculative ideas don't work out, and it's not that one could tell this at the beginning, but as you work things out and understand them better, typically the more you learn, the more you find problems with the idea. Only in rare occasions do you find that the more you learn the better and more solid the idea looks. In some sense, the fundamental problem with the string theory unification idea is just exactly this: you could argue about how promising it was in 1984, but everything learned about it since then makes clearer and clearer the deadly problems with the idea. In slogan terms: the absolute value of the significance function of an idea may be very hard to evaluate, the derivative in time a lot easier.

2. Absent experimental confirmation, you need to actually understand ideas and their evolution to evaluate them, there's no easy way around this. If a highly complex and obscure set of ideas accurately computes the details of something you can observe, you know there is something right about it, even if you don't understand the set of ideas. If you decouple from experiment, you have no external evaluation of the ideas, have to really understand and engage with them to evaluate them.

3. Psychologically, once people have a certain amount of their time/energy/reputation invested in working on a set of ideas, it is very difficult for them to honestly evaluate whether the ideas are working.

The combination of 2 (a lot of effort needed to understand something well enough to evaluate) and 3 (if you put a lot of effort into an idea, you have a lot invested in it) creates a huge problem, and I think explains a lot about some of the pathology we've seen in fundamental theoretical physics

16. **Aristotle Pagaltzis**
June 6, 2021

> The combination of 2 (a lot of effort needed to understand something well enough to evaluate) and 3 (if you put a lot of effort into an idea, you have a lot invested in it) creates a huge problem

Not to be bleak, but does this leave room for a way out other than what one might postulate as the inverted Planck's principle? ("A failed scientific venture does not wane by disappointing its proponents and making them see the light, but rather because its proponents eventually die and a new generation grows up that is familiar with its ineffectiveness.")

17. **Peter Woit**
June 6, 2021

Aristotle Pagaltzis,
Actually, the situation is even bleaker than that. Once a complex failed research project gets institutionalized, absent a new successful one coming along and displacing it among the younger generation, the younger generation just gets trained in the complex failed project, reproducing the problem. To this day, lots of the best young students in theoretical physics are being trained by being told to spend years reading the canonical multi-volume superstring theory treatises.

18. **Jackiw–Teitelboim**
June 6, 2021

Dear Peter,
Suppose you are talking to a person belonging to the set of "best young students in theoretical physics" from some reputable university according to all standard metrics, and have been told to "spend years reading the canonical multi-volume superstring theory treatises".

What would *you* tell such student to spend their time on then?

An advice I have being often told, which I rejected based upon personal experience, is that of doing a math PhD instead of a physics one. Nevertheless, the kind of interests and standards of "rigor of proof" in a typical top math PhD program will possibly consume much more energy as reading a 2-volume Polchinski's textbooks, however with the additional risk of also losing physical intuition.

19. **Peter Woit**
June 6, 2021

Jackiw-Teitelboim,
There really isn't a good answer to give students in this position right now. HEP theory/quantum gravity is not a healthy research area and hasn't been for years. The first obvious piece of advice is to consider instead working in a healthier research area. There are a lot of them in mathematics, many with significant overlap with fundamental physics. But, sure, if what interests you is just physics and not math, getting a math phd. is problematic.

Students are inescapably highly dependent on faculty and their advisor, this can't be solved by them getting advice from the internet or figuring out themselves the problems with their advisor's research direction. I do think though that good advice at the moment is that if you're a student being told to spend a lot of time mastering the quantization of the 10d superstring, you should be asking questions and looking around for other options.

20. **Peter Shor**
June 6, 2021

Jackiw-Teitelboim: if the actual theory of quantum gravity requires math that physicists currently don't know, then if you don't learn more math you will never find that theory.

On the other hand, it's clearly much safer to read through all Polchinski's textbooks, because it's very possible that if you get a math PhD, the math you learn is not going to be the math you need for quantum gravity. And if you're not interested in proving theorems rigorously, but in using advanced math to study physics, you may not get a good academic job. [This is something that maybe could be fixed.]

However, if no physicists ever learn any more math, then it's quite possible we'll never find the theory of quantum gravity.

Looking back in history, finding the theory of general relativity needed knowledge of differential geometry. But Einstein was smart enough that he realized he needed to learn this, and he had a friend, Marcel Grossmann, who helped teach it to him. And after the theory of GR, differential geometry was added to the physics curriculum (at least for relativists).

21. **Robert A. Wilson**
June 7, 2021

Peter Shor,
An alternative approach is for mathematicians to learn more physics, so that a mathematician who actually does know the mathematics that posterity will discover is needed, can bring that mathematics to the attention of physicists. Of course, the mathematician will need to do a lot of work explaining a lot of physics in terms of a new mathematical model, before any physicists will listen. But if physicists don't know what mathematics they need, perhaps mathematicians can help?

22. **lun**
June 7, 2021

Peter Shor, at least from a historical point of view things are a bit more subtle.
Heisenberg did not know matrices, and Einstein did not know differential geometry.
Learning the required maths can be accomplished while developing the theory, after having "found it". "finding it" was about physical insight, not knowing the right maths.
Perhaps post-empiricism will change that.

23. **Ricardo**
June 7, 2021

Somebody please comment on advising a young student to choose condensed matter physics as a field of study. TIA.

24. **JE**
June 7, 2021

If you ask me, I would say that the guiding principle for advancement in theoretical physics should not be beauty, but physical intuition. The quest for beauty can easily lead you astray in physics, because beauty is quite a debatable question. Boldly put, beauty is fine for fine art, but physical intuition will more likely keep you linked to physical reality. The quest for beauty can rapidly entice you to supersede the SM with a larger theory of which it merely is a subset, while physical intuition can tell you that such a successful theory may have become a victim of its own success by obfuscating the fact that it may not even be "right".

25. **Peter Woit**
June 8, 2021

lun/JE,
The problem with the argument that "physical intuition" or "physical insight" is the answer to figuring out a new layer of fundamental physics is that there's no good reason to believe our physical intuitions based on earlier theories are relevant. Quantum mechanics famously violated all previous physical intuitions and took a lot of getting used to (with a lot of help from mathematics). Curved space-time also violates all previous physical intuitions, and Einstein could only pursue that idea and work with it by learning a lot of very sophisticated new mathematics.

26. **Low Math, Meekly Interacting**
June 10, 2021

It's been ages since I've been around bachelor's anything, but my sense is that an undergraduate physicist is going to have to go out of their way to learn much more than the basics of GR and QFT. Most won't take dedicated courses on either, even if they're on the curriculum. I know Schwarz isn't trying to be fair, but is his dismissal as absurd as it looks to me?

27. **Peter Woit**
June 10, 2021

LMMI,
Yes, it's absurd. Also kind of sad that Schwarz feels it necessary to make over the top ad hominem (chip on my shoulder?) attacks on an argument he has no good answer to. Back in 2006 the point of view of my book and Smolin's on string theory already had a lot of support in the theoretical physics community. By now, with the collapse of hopes for a SUSY extension of the SM and the multiverse debacle, the idea that string theory has not worked out is I think a majority one among physicists. Schwarz and some others may comfort themselves with the idea that criticism from their colleagues is based purely on ignorance, incompetence and resentment, but I don't think this helps his case.

Various Math Items

June 6, 2021
Categories: Uncategorized

Some math-research items:

- Mura Yakerson has been doing a really wonderful series of interviews with mathematicians, available at her math-life balance web-page or Youtube channel. I've just started listening to some of them, including ones with Peter Scholze and Dustin Clausen (Clausen is John Tate's grandson, the latest AMS Notices has a memorial article).
- There's a remarkable report out from Peter Scholze about the progress of the Liquid Tensor experiment. Back when I first heard about this, I figured it was a clever plot by Scholze to get other people to help with a very complicated part of a proof, by getting them to work out the details, with the excuse being that they would be doing a computer check of the proof. Seemed to me very unlikely you could check such a proof with a computer, but that by forcing humans to try to disambiguate things carefully enough in preparation for a computer proof, he'd get a human-checked proof. Looks like I was wrong.
- For yet more Scholze news, the Fields Medal symposium this year will be devoted to his work.
- Trying to find something of interest in math, that wasn't Scholze-related, I noticed this site devoted to the case of Azat Miftakhov, where there will be an online Azat Miftakhov Day program. Foiled though on the Scholze front, since he's a speaker there, talking about Condensed Mathematics.
- The list of those giving plenary lectures at next years ICM is here.

Update: Kevin Hartnett at Quanta has a good new article up about quantum field theory and mathematics (an inexhaustible topic…)

Update: Also from the Simons Foundation, there's a wonderful profile of my Columbia colleague Andrei Okounkov, who has been very active in bringing together mathematics and ideas from quantum field theory.

Update: Nature has a story about the Liquid Tensor Experiment.

Comments

1. **Bertie**
   June 6, 2021

   I like Scholze's riff on the metal band Liquid Tension Experiment, which was formed back in 1997

2. **Peter Shor**
   June 7, 2021

   Let me note that the big news in the list of sections for the ICM is that there are two new Applied Math sections for invited lectures:

   17. Statistics and Data Analysis, with 8–11 talks (split from Probability and Statistics, and with a lot more talks than statistics used to get)

   and

   18. Stochastic and Differential Modelling, with 4–6 talks (a completely new section).

   A lot of the other sections had their targets reduced by one talk to make room, although I think they're also expanding the number of invited lectures a little. I knew something like this might be coming (but didn't think to check it until I saw your blog post).

3. **Mark Hillery**
   June 12, 2021

   Here is a question from someone whose knowledge of quantum field theory is rudimentary. Does anyone know how the new work on making quantum field theory rigorous discussed in the Quanta article fits in with previous work in this area, such as axiomatic field theory a la Wightman, algebraic quantum field theory a la Haag (this is mentioned briefly), or constructive field theory?

4. **S**
   June 13, 2021

   I second Mark Hillery's question.

   Also — do Dijkgraaf's comments in the Quanta article seem somewhat in tension with his recent talk and earlier article? If successfully mathematizing QFT will lead us to new fundamental physics, that seems like quite another project than just using the physics we have to analyze more and more complicated things, I should think.

5. **Stephane**
   June 13, 2021

   Talagrand's book, What Is a Quantum Field Theory? A First Introduction for Mathematicians, is planned for March 2022
   https://www.cambridge.org/in/academic/subjects/mathematics/mathematical-physics/what-quantum-field-theory-first-introduction-mathematicians?format=HB&isbn=9781316510278

6. **Peter Woit**
   June 13, 2021

   Mark Hillery,

   Much of the new work discussed is based on perturbation theory, in principle providing a new mathematical framework for renormalized perturbation theory in QFT, one more adapted to things like working on arbitrary manifolds, not just flat space-time. From the point of view of mathematicians, what has always been a goal is to have a mathematically well-defined framework for describing the QFTs physicists have used to get new results in topology, i.e. topological quantum field theories. This new work to some extent gives that, but so far just for perturbative theories, while the most dramatic TQFT results use non-perturbative information.

   The kinds of approaches you mention are motivated by a desire to have a rigorous non-perturbative quantum field theory. The Wightman axioms crucially use Poincare symmetry and Minkowski signature, while the Costello stuff is set up to work on arbitrary manifolds with no Poincare symmetry, and with Euclidean signature. So, here

there’s not a lot of overlap. For the relation to AQFT, see
https://arxiv.org/abs/1711.06674
The results there are pretty limited, this paper is just about free field theory.

For constructive QFT, again the whole point is to try and construct something outside of perturbation theory. The state of the art there has always been lattice regularized QFT, with the problem that of how to take the continuum limit. Unfortunately I don’t know of a lot of work or progress on this recently.

7. **Peter Woit**
June 13, 2021

S,
To a large extent, theorists studying QFT have given up on trying to understand more about the QFT that describes fundamental physics (because the problem is too hard), and instead work on QFTs for which new ideas may give some results, but the physical relevance of these QFTs is typically in condensed matter physics.

For quantum gravity, this takes the form of people mainly studying low dimensional toy models. Whether these will tell us about 4d quantum gravity is quite speculative, but often they have direct relevance to very different subjects, e.g quantum information theory, or condensed matter problems (where the low dimension is the relevant one).

This may provide some context for reconciling Dijkgraaf’s different-sounding comments.

8. **Arnold Neumaier**
June 14, 2021

Public lecture notes of a course by Sourav Chatterjee, based on Michel Talagrand’s QFT book are at
https://statweb.stanford.edu/~souravc/qft-lectures-combined.pdf

9. **Matt Foster**
June 18, 2021

On the mathematical proof of the “DOZZ” formula (3-point function/OPE coefficient) for 2D Liouville field theory

https://www.quantamagazine.org/mathematicians-prove-2d-version-of-quantum-gravity-really-works-20210617/

10. **Timothy Chow**
June 20, 2021

I made a MathOverflow post back in April 2020, where among other things I said (regarding Mochizuki’s abc proof), “I do think that it is reasonable for skeptics of the proof to request that those who claim to understand the proof, and who want to cultivate a whole new generation of younger mathematicians to pursue IUT, to formalize the proof in a proof assistant.”

I tried to post a similar comment here on Not Even Wrong, but it didn’t get published, perhaps because Peter thought the comment was silly. In light of how the Liquid Tensor Experiment has played out (which doesn’t surprise me at all except that I expected it would take about a year, rather than six months, to get to this point), perhaps the suggestion doesn’t sound as silly any more.

11. **David Roberts**
June 20, 2021

@Timothy Chow

at the very least, such a move would necessitate trimming all the cruft from the definitions and reducing to the actual key lemma of interest, which is some statement about monoid actions (as opposed to a much more complicated statement about Frobenioids). And I don’t mean Corollary 3.12, but the key sticking point inside that the claimed proof of it.
This is what Scholze did for the LTE, isolating the key technical proposition that doesn’t really involve condensed abelian groups at all, and how far the formalisation has actually gotten.

The only problem is that one needs a community of people large and invested enough (and experienced in formalisation) to divide up the labour and do it. And a person who understands all the proof being willing to give precise technical advice on the question, as Scholze supplied to the formalisation team. The forthcoming clarifications for IUT have so far not been, to my eyes, of that nature, but more like mathematical koans.

12. **Timothy Chow**
June 21, 2021

@DavidRoberts

I agree with what you say. I should perhaps amend the word “skeptics” to “funding agencies” (if you click through to MathOverflow, I explain this point in more detail). From the standpoint of a funding agency who believes that Mochizuki is acting in good faith, Mochizuki should be able to play the role that Scholze did. And the funding agency can help with assembling the formalization team, which I imagine would comprise grad students and postdocs who are on good terms with Mochizuki.

13. **Peter Woit**
June 21, 2021

Timothy Chow,
I don’t remember, but likely didn’t post your comment about abc because the issue of formalizing Mochizuki’s proof did seem irrelevant for two reasons:

1. It seemed to me highly unlikely that this was within the realm of possibility, even if one thought of the situation as involving a conventional but very complex proof, with the author of the proof behaving conventionally.

2. Going on about proof assistants just obscures the actual problem: Mochizuki refuses to engage with Stix and Scholze in the standard way a mathematician is supposed to respond to those challenging a claimed proof. Instead of providing more details for and clarifying challenged arguments, he refused to significantly add anything to his original manuscript, issuing instead documents which convincingly destroy his own credibility. In addition, it looks like no one around him is able to provide such details and clarifications.

Given the Liquid Tensor Experiment success, I agree my judgment about 1. was flawed. But I still think 2. is the relevant point, and discussing proof assistants in the context of the problem with Mochizuki’s proof is a red herring. The problem here isn’t the kind of thing Scholze was struggling with: an argument so complex that the best human mathematicians might miss a subtle problem. Yes, there a proof assistant might be the answer. But the problem here is a human one of a very different kind.

14. **Timothy Chow**
June 21, 2021

Peter, I absolutely agree that 2 is the relevant point; I absolutely agree with your characterization of the sociological situation; and I absolutely agree that the problem is a human one of a very different kind from the Liquid Tensor Experiment. But I disagree that "going on about proof assistants obscures the actual problem." Quite the opposite! The point I am making about proof assistants is that, under the assumption that they are sufficiently easy to use (point 1), they are *ideally suited* for addressing the actual (human, sociological) problem.

Abstractly, we have a small Community A that insists on the correctness of a certain proof, and a much larger Community B that is unconvinced. For sociological reasons, the usual mechanisms for resolving mathematical disagreements have broken down. Community A insists that it has provided full details and that Community B is behaving in a disrespectful manner and refusing to acknowledge the plain truth. Community B, of course, says the same about Community A. The point is that the existence of proof assistants means that Community B can fairly say to Community A, "You need to explain your proof to a proof assistant before we'll give you more money." If Community A's claims are true, then there should be no reason why Community A can't formalize the proof. The key point is that a proof assistant, being a dumb machine, cannot be accused of being disrespectful or failing to conform to some human standards of decorum.

If Community A is right, and produces a formalization, then the formalized proof should convince Community B. (I'm assuming here that the two communities can at least agree on a proof assistant that they both trust, and that they can agree on a statement that is simple enough for there to be no debate about whether the formalized statement correctly captures the disputed claim. These assumptions are reasonable in this case, I think.) On the other hand, if Community A fails to produce a formalized proof given a reasonable amount of time and money, then it's going to become increasingly difficult for them to explain what the holdup is.

The proof assistant allows a funding agency to withhold funding without having to engage in a potentially unpleasant confrontation with Community A. If it so desires, the agency can insist that it is totally on the side of Community A, and that Community B is a bunch of evil morons; its pen is poised to sign the check as soon as Community A satisfies this small bureaucratic requirement of producing a formalization. Crucially, the funding agency requires very little technical expertise to play its role in this drama.

15. **Peter Woit**
June 22, 2021

Timothy Chow,
I guess I'm just inherently skeptical about claims that the solution to human problems is "computers". A colleague of mine likes to claim that mathematics is the only academic subject where, when two people disagree about something, they go into a room to discuss it, and emerge with one of them saying "I was wrong". This is an exaggeration, but it's an important ideal to try and maintain. The problem here is a violation of community norms designed to uphold that ideal, and I don't think computers can replace that ideal.

It does look like proof assistants can be very helpful when an argument is so complicated that no one is sure whether it is right or not. This latest is a success of this kind, another example would be the Flyspeck project in the case of the Kepler conjecture. Cases where experts disagree about whether an argument is right should be resolved by more discussion, until one side gets the other to see where they went wrong. If people can't do this, they're not in any position to start on the much harder project of getting a computer to resolve their differences.

16. **Timothy Chow**
June 22, 2021

There's nothing magical about the proof assistant being a computer; any competent neutral third party would do. I claim that from time immemorial, many human disputes of all kinds have been settled by the intervention of an impartial mediator that both sides trust. Surely you're not inherently skeptical of that claim? The only special thing here is that the neutral third party happens to be a machine, and is therefore *indisputably* impartial.

I have discussed this dispute resolution procedure with a variety of people with different amounts of technical training, and just about everyone immediately understands it and is puzzled why the mathematical community doesn't follow it (I have to explain that proof assistants are much more cumbersome to use than they might think). The few people who have balked have been mathematicians. It initially puzzled me why the principle that a neutral mediator can aid in dispute resolution—a principle which is obvious to almost everyone on the planet—would be so difficult for mathematicians to grasp, but I think I understand why. It relates to the point you just made, that mathematicians have been spoiled by the scarcity of serious disputes in our field (when it comes to mathematical correctness, at least). In every other field, disputes are a dime a dozen, and so everyone intuitively understands what methods are available to address them. Mathematicians, on the other hand, know of only one method: talk it out. Since that almost always works, we never bother to think about a Plan B for cases where it *doesn't* work. If talking it out doesn't work, we have no idea what to do.

Another contributing factor, I think, is that mathematicians tend to be surprisingly slow to catch on to what computers can do for them. When the Appel–Haken–Koch proof of the four-color theorem came out, did mathematicians react by saying, "This is awesome! What other computer-assisted proofs can we come up with?" Some may have, but by and large, the initial reaction of most mathematicians was to either ignore the result or complain about the role of the computer. A similar dynamic is happening with proof assistants. For example, in Scholze's blog post, he wrote, "Initially, I imagined that the first step would be that a group of people study the whole proof in detail and write up a heavily digested version, broken up into many many small lemmas, and only afterwards start the formalization of each individual lemma." Experts in formalization have long known that proof assistants have progressed far beyond that stage, but the news has been slow to seep into the general consciousness, and I think it's partly because most mathematicians have incorrect preconceived notions about proof assistants that cause them to resist them rather than milk them for everything they're worth.

You say that invoking a proof assistant to help resolve differences is a "much harder project" than resolving them by discussion. That is true under normal circumstances, but going on about this fact obscures the actual problem, which is that the conventional method of human discussion has broken down in this case. I'm totally with you that calm discussion is the way disagreements *should* be resolved, but what if that method fails? Then the inequality flips. Human discussion becomes infinitely hard, and appealing to a proof assistant becomes potentially easier.

Note that I'm just as skeptical as you are about Mochizuki's proof. I don't expect a formalized proof of abc to emerge, for the simple reason that I doubt that "Community A" really has a proof. But that doesn't matter. What matters is the *conditional* statement that *if there is a proof then it should be formalizable in a proof assistant*. Now you might counter that *if there is a proof then it should be explainable to Community B*, and that is true under normal circumstances, but what Community A will say is that Community B is being stubborn and disrespectful. Again, the value of a proof assistant is that it cannot be accused of being disrespectful.

One final point. I don't claim that a proof assistant will completely solve the sociological breakdown by getting Community A and Community B onto the same page. If Community A does not really have a proof, then unifying the two communities probably won't happen. But what the proof assistant can do in that case is to make it clear to the whole world that Community B is right and Community A is being unreasonable. At the moment, it's unclear to many outsiders which community is right. You and I know the absurdity of Mochizuki's claim that Scholze is an idiot, but it's harder for the rest of the world (read: Japanese funding agencies) to be sure. That could change with a dispute resolution procedure that lay people could readily understand.

17. **jrrm**
July 6, 2021

Mark Hillery,

Kasia Rejzner's book (cited in the Quanta article) belongs to the Haag-Kastler school of algebraic field theory and is part of the work of Klaus Fredenhagen and coworkers on QFT in curved space times. It seems to me, that is now possible to prove rigorously much of the folklore "Theorems" of perturbative QFT in this semiclassical scheme rigorously.

Strings 2021

June 21, 2021
Categories: Strings 2XXX

Strings 2021 started today, program is available here. Since it's online only, talks are much more accessible than usual (and since it's free, over 2000 people have registered to in principle participate via Zoom). Talks are available for watching every day via Youtube, links are on the main page.

As has been the case for many years, it doesn't look like there will be anything significantly new on the age-old problems of getting fundamental physics out of a string theory. But, as has also been the case for many years, the conference features many talks that have nothing to do with string theory and may be quite interesting. I notice that Roger Penrose, a well-know string theory skeptic, will be giving a talk on the last day of the conference next week.

Another series of talks that I took a look at and that I can recommend is Nima Arkani-Hamed's lectures on Physics at Future Colliders at the ICTP summer school on particle physics. He never actually gets anywhere near discussing the topic of the title for the talks, but does give a very nice leisurely introduction to computing amplitudes for zero-mass particles. What he's doing is emphasizing ideas that are often not taught in conventional QFT courses (although they should be). His second talk explains how to think of things in terms of classifying representations of the Poincare group, an old topic that unfortunately is often no longer taught (see chapter 42 of my QM textbook). His third talk emphasizes thinking of space-time vectors as two by two matrices (see section 40.4 of my QM book). This is a truly fundamental idea about space time geometry that gets too little attention in most physics courses.

Update: At String 2021, yesterday Nima Arkani-Hamed gave a talk on "Connecting String Theory to the Real World We See Outside Our Windows", where he sometimes sounds like me, contrasting the pre-LHC claims of string theorists:

1. LHC will discover SUSY
2. String Theory Loves SUSY + Unification

to what they are saying now that the LHC has found no SUSY

> CICADAS . (Anyway, String Theory is mainly about Quantum Gravity).

He goes on to explain the "landscape philosophy", which he sees string theorists (and himself) as now adopting. According to this philosophy, "connection to particle physics appear hopeless/"parochial"/unimportant". As a result, he sees the current situation as

- String theorists are for the most part no longer actively pursuing connecting to particle physics of the real world.
- Understandable as a short-term strategy
- But in my view a real mistake in the long run…

One reason for this being a real mistake is that, divorced from input from the real world, theory becomes sterile:

> Questions Posed by Nature are Vastly Deeper and more fruitful than ones we humans tend to pose for ourselves.

Unfortunately I don't think Arkani-Hamed has any compelling argument against "string theory implies landscape implies nothing to say about particle physics". He discusses the "swampland philosophy", but gives as a challenge to theorists just making more precise the sort of empty question that this philosophy deals in (he asks whether D=9 SU(2021) to the power 2021 is in the swampland).

Update: In the final discussion section, Witten emphasizes that "What is string theory?" still has no answer, that we have "little idea what it really is". He lists two main things we know about the supposed theory:

1. General string perturbation theory using 2d conformal field theory. He mentions that one basic problem with this is that there is no understanding of what happens in time-dependent backgrounds, so, in particular, this is useless for addressing the big bang, which is the one place people now point to as a possible connection to real world data.

2. AdS/CFT

He notes that to get at non-perturbative string theory we seem to need some more general understanding of quantum theories, going beyond the usual quantization starting with an action, and ends by saying maybe quantum information theory can help. In the discussion section, Vafa challenges him on this, saying he sees no indication that quantum information theory gives any insight into dualities he sees as the central aspect of the non-perturbative theory. Witten's answer is that this was just a vague hope, that the duality ideas are now 25 years old, we haven't progressed beyond them, need something new.

Comments

1. **Do_not_want_to_register**
   June 21, 2021

   Another talk of Penrose (and a conference in his honour):
   https://sites.google.com/unifi.it/scri21/program
   (videos of the talks at the bottom)

2. **Suomynona**
   June 22, 2021

   How is the idea that particles are irreducible representations of the Poincaré group work in the context of quasiparticles in condensed matter physics, where the underlying system background is generally not symmetric under Poincaré transformations (e.g. rotations and translations are typically discretized due to the presence of a lattice)?

3. **Peter Woit**
   June 22, 2021

   Suomynona,
   In other physical contexts you have different symmetries than Poincare, often approximate symmetries. The general principle is that whenever you have a group acting on a physical quantum system, the state space will be a unitary representation of that group, and unitary representations break up into irreducible representations. It will often be a good idea to analyze states in terms of these irreducible representations. How useful this will be will depend on the symmetries you have available. What's remarkable is how powerful Poincare space-time symmetry is as a determinant of how elementary particles behave. Condensed matter physics is a quite different story.

Much as I love this general topic, it's getting far from the topic of the posting, so I'll just advise: read my book…

4. **ay**
   June 23, 2021

   I am currently reading Weinberg's book to fill some holes in my knowledge. It's very good but he often leaves the reader a lot to figure out. I watched the entire video on the Poincare group and, although the path was the same as Weinberg, there was a lot of new information – I thought it was excellent. It also gives one a feeling for how Arkani-Hamed works through problems. The video answered some questions that I still had (e.g. how sensitive is the procedure to choice of k_mu – he did the case where k_mu included a boost).

5. **lun**
   June 27, 2021

   Starting from 1h14m of this "meet the public" vide
   https://www.youtube.com/watch?v=WwmxaXTnGMI
   you see Ed Witten, and then Shiraz Minwalla, basically agreeing with you.

6. **Peter Woit**
   June 28, 2021

   lun,
   I just took a look at that session and thought one remarkable point was where people were asked "what would cause you to give up on string theory?" The only answers forthcoming were from Igor Klebanov, who said he had been working on it his whole life, so couldn't see himself giving up on it, and Ashoke Sen, who said he would only give up on it if it were shown to be mathematically inconsistent. Of course, a few minutes earlier Witten had explained that no one knows what the theory actually is, which means there is no way to show that it is mathematically inconsistent.

   The whole tone of the discussion was pretty depressing. In response to the obvious question about relation to experiment and testability, people have clearly completely given up on this, with nothing to say (other than a bogus claim that string theory makes some prediction about B-mode polarization), going on about how it took thousands of years for the atomic hypothesis to be vindicated. There's a lot of repetition of tired old arguments from decades ago, zero acknowledgement that things have not worked out as hoped. It seems that no one in the string theory community dares to publicly breathe a word of skepticism. Skeptical challenging of arguments is supposed to be what science is all about, but the speakers in this panel made it seem that this is absolutely not something that is part of their field or what they do.

7. **Jackiw–Teitelboim**
   June 28, 2021

   This goes without saying all technically challenging fundamentally important problems required for superstring theory unification be at least *mathematically* self-consistent have been left unanswered. (With exception of a group of very few dedicated researches, to whom I sincerely keep my deep respects.) While the community just spread out to other problems.

   Paraphrasing Fermat, "I have discovered the ugly truth of this, which this margin is too narrow to contain." Nevertheless, it may be worthy registering on this blog some of those problems which aren't enough emphasized. Nothing will be said about "multiverses" or "swampland" on what follows.

   (i) No one convincingly proved the mathematical equivalence between the RNS superstrings and GS formalism.

   (ii) Every-time I receive a grant report asking for resources, penned by a senior string theorist, I'm already able to guess the 1st lines: "ST is the only mathematically consistent candidate for understanding quantum gravity while providing a framework potentially unifying all know fundamental interactions."
   I'm sorry if I cannot comment on other approaches to quantum gravity or non-stringy grand unification programs. But regarding superstrings, from such a strong statement one would expect at least proof (not rigorously by mathematical standards of course!) that perturbation series following from superstring S-matrix, already known to diverge as one sums over all moduli parameters (which isn't really the issue, since this happens even in QED, see *Dyson, F.J., 1952. Divergence of perturbation theory in quantum electrodynamics. Physical Review, 85(4), p.631.*), one would at least expect that a well-behaved asymptotically perturbation series follows as one takes into account contributions from all the moduli parameters of Teichmüller spaces of Riemann surfaces with higher genus and market points.
   At the time I drop those lines, more than 50 years from dual resonance models?, I haven't found any convincingly argument.

   (iii) Most superstring calculations are performed under euclidean worldsheet for many practical reasons, as we all know and love from Wick's rotation in standard q.f.t.'s. Nevertheless, from a theory claiming to be the only mathematically self-consistent unification program, something regarded as just a mere technicality now requires an answer. The spin-bundle required to defined fermion content from SUSY multiplet depends on the metric signature. I would be eager to read a real formal discussion if first-quantised RNS superstring in euclidean signature is quantum mechanically equivalent (unitary Boguliubov transformation) to original version starting from Minkowski signature.

   I could go on the whole night, but I believe, as most other string theorists do, there are more "urgent" or at least doable problems to solve now. In my case, I will give a run, certainly much more healthy, no?

8. **PeterH**
   June 28, 2021

   Just viewed the last question and answer session.
   The only message I hear repeatedly is that 'String theory is the only game in town'.
   Well, that would be the case if people follow the hype for last 20-30 years or more.
   For a so called well defined mathematical theory, cannot be tweaked etc, the number of unsolved issues with it and the claim not understanding what string theory is – is inconsistent.

9. **Peter Woit**
   June 28, 2021

   Jackiw-Teitelboim/PeterH,
   There's no reason to get into the technical argument over whether the perturbative expansion of various versions of the superstring in 10d is mathematically well-defined. That looks nothing like the real world, and as Minwalla points out, no one at the Strings conference is even talking about that sort of "string theory" anymore. There's a long list of things they are talking about under the name "string theory", with the generic problem that the ones that are reasonably well-defined have nothing to do with the real world (e.g., wrong dimension).

   Sen is saying he'll give up on string theory if people prove that "string theory" is inconsistent, but Minwalla and Witten are saying there is no definition of the term "string theory".

What’s really bizarre about the current situation is that “string theory” has completely decoupled from any well-defined proposal for an actual 3+1 d unified theory of gravity and the SM, while at the same time its proponents claim it is the “only game in town”. The “only game in town” in fundamental physics now is to give up on a theory of the real world.

10. **Jackiw–Teitelboim**
June 28, 2021

Hi Peter,

Thanks for allowing my last comment. As you probably guess from my former comments under the present pseudonym, I’m one of those students whom, maybe for lacking courage or ,who knows? , self-confidence, went through a Faustian-like monologue, if you allow me a little of prose:

I’ve studied, Schwarz, alas, Polchinski,
Green and Witten, recto and verso
And how I regret it, D-branes also
Oh, God, how hard I’ve slaved away,
With what result? Poor fool that I am,
I’m no whit wiser than when I began

You are possibly right it’s now pointless to debate those issues. But I went through the hard road, being told just to accept all fundamental questions I raised above as unanswered and move on to the “cutting-edge” problems whatever and whonever decides what these are. So I wrote that list as both an act of rebellion nevertheless also of relive: I don’t think one needs to wait for Planck’s law to clean up our field through funerals in this century anymore & here’s the right point for me to close: It’s time for new ideas and we are going to find them.

11. **DKepler**
June 29, 2021

I just had a look at that session, and more than anything, felt sorry seeing such eminent physicists giving such vacuous reasons just for the sake of defending a theory they have worked on for a long time. Even beginning PhD students will no longer believe that ST is the only framework that reproduces BH entropy. Even without LQG, there are many ideas on/in QG which are physically well founded. This community is indeed becoming like a religious cult, something one of the speakers explicitly denied. Ironically, the very fact that he had to refer to it at all should alarm them.

12. **David Roberts**
June 29, 2021

@DKepler

> Even beginning PhD students will no longer believe that ST is the only framework that reproduces BH entropy.

That’s interesting, can you give pointers?

13. **DKepler**
June 30, 2021

@David Roberts: There are many references that discuss this from different perspectives. The following articles by Carlip provide a nice overall perspective, drawing attention to the “universality” of BH entropy derived in different approaches to QG – what he refers to as an “embarrassment of riches”.

1. Symmetries, Horizons, and Black Hole Entropy [https://arxiv.org/abs/0705.3024]
2. Black Hole Entropy and the Problem of Universality [https://arxiv.org/abs/gr-qc/0702094]

Hope you will find them as a useful starting point.

14. **Cobi**
June 30, 2021

@DKepler
The derivations listed in 0705.3024 seem to fall into three classes:

– String Theory (D-brane/String state counting, AdS/CFT, Fuzzballs)
– The LQG “derivation”, involving an ad-hoc choice for the Immirzi parameter
– Non-QG QFT derivations that are not attempting to identify the microstates

In particular, i can not find a derivation of any black hole entropy from first principles in quantum gravity outside of string theory.
The LQG example hardly qualifies if you do not have any way to fix the Immirzi parameter and just tune it to get the desired result.
Of course there are many ways to obtain the BH formula that do not try to identify the microscopic degrees of freedom in a theory of QG, the original calculation from the 70s being an obvious example.

15. **Tom**
June 30, 2021

Off topic, but the European congress of Mathematics has finished up. The EMS prizes are always a good indication of future Field’s medalists
https://8ecm.si/prizes

16. **Log**
June 30, 2021

@DKepler

Ashoke Sen has computed the logarithmic corrections to the Bekenstein-Hawking entropy of various standard black hole solutions in this paper

https://arxiv.org/abs/1205.0971

It seems that non-stringy approaches to quantum gravity are not very successful at reproducing these universal results.

17. **Peter Woit**
June 30, 2021

All,
Please, no more about the “only string theory can compute the Black Hole entropy” arguments. These have been going on for over 25 years, with nothing new in a very long time. To summarize the situation, now as it has ever been:

1. Hawking tells you what the semiclassical limit is.
2. Any consistent theory of quantum gravity should reproduce this semiclassical limit.
3. String theory calculations that can be done in unphysical situations (wrong dimension, lots of SUSY, extremal black holes) claim to reproduce the semiclassical limit.
4. LQG calculations claim to reproduce the semi-classical limit.
5. People interested in string theory/LQG religious warfare argue about the calculations in 3. and 4.

I’ve personally never understood why showing that (unphysical limits of) your quantum theory gives the expected semi-classical result is anything other than a rather weak consistency check on your theory. Others clearly feel differently and think this is of huge significance.

If people think carrying on this argument is a good use of their time, they are encouraged to do so elsewhere.

18. **adrianmigueldiego**
July 2, 2021

Dear Peter, I believe you are ‘cherry-picking’ parts of comments. An exchange that was otherwise quite lively and interesting, you paint in the worst possible way. I am not a fan of string theory. I just work on Theoretical Physics topics. I saw the discussions you refer to. You are either blinded or so convinced by the idea that string theory has nothing to say about Physics, that you pick any phrase that adds to your view of things. Please, watch the discussions (there were many) and appreciate that these were honest discussion. People are attempting to understand things, with the tools at hand.

Is String Theory a Theory of Nature? Probably not if you ask me. But it is clear that these colleagues are attempting to understand topics in QFT and Gravity, with the tools they have at hand.

Please, do appreciate that negative comments like yours distort the ideas the general public (or even physicists in other areas) have about String Theory, or Theoretical Physics in general.
Of course, I do not justify equally wrong comments from certain string theorists
with the exact opposite agenda.

But this procedure you adopted in this post, is not the best way of understanding science, or communicating it.

thanks
Adrian

19. **Peter Woit**
July 2, 2021

Adrian,
I did watch fully the discussions I wrote about here, also took a look at many of the others. The quotes I gave are accurate, and links are provided so that anyone who wants to can see the full context.

You’re right that I was cherry-picking, but in the sense of ignoring the huge amount of hype about what great progress was being made, and sticking to writing about certain places where people like Arkani-Hamed or Witten chose to address the real problems (which most of the time are studiously ignored).

If you’re so concerned about the public or physics community getting misled, surely you’ve contacted the organizers of and participants in the conference’s “Outreach activities”.

20. **adrianmigueldiego**
July 2, 2021

Dear Peter,
You wrote
”If you’re so concerned about the public or physics community getting misled, surely you’ve contacted the organizers of and participants in the conference’s “Outreach activities” ”

To this I was referring with my phrase
” Of course, I do not justify equally wrong comments from certain string theorists
with the exact opposite agenda.”

I do not know if outreach sessions like the ones we witnessed in this Strings-conference are the best idea. I think a better service to the public would be made by discussing things that are well established, rather that the permanent focus on ”what is coming/the future” etc. In my view, this anxiety to communicate to the public the latest developments is a sign of certain immaturity. But this is just an opinion. There are other views, it is a matter of taste.

What I wanted with my post was to point out that (in my view, again), you were selecting particular phrases that sounded negative, in what other wise were quite interesting exchanges among physicists.

The way I see the string people (not necessarily those who appear in the media, but those who publish in the arXiv) is as a small set of honest and hard working physicists, technically sophisticated, researching mostly on QFT and Gravity, with various approaches.

I believe I am not misguided. Can they make mistakes? Over-claim? Of course, these are people. But the time, the community (and the arXiv) takes care of these mistakes.

I believe that your permanent opposing the ‘string hypes’ is good and welcomed. It is healthy. But I believe that in today’s update of this post you were not intellectually honest. This is damaging.
Once again, this is just an opinion.

Thanks for reading
Adrian

21. **martibal**

July 2, 2021

@adrianmigueldiego: as a mathematical-physics, I have been earring for 20 years that string theory is the only game in town, I have seen the “raise” of LQG as an opponent to string, and I witnessed the tentative of string theory to kidnap noncommutative geometry claiming that this was “nothing but the geometry of string theory”.

So please stop it with this idea that criticising string theory, as Peter does, is not good nor an honest way to communicating about science.
The string community has been dishonest for at least 20 years, and has absolutely no right to give any lessons about ethics in science. As Peter keeps on repeating, the denial of the string community to acknowledge the failure of string theory as a unification theory (which is nothing to be ashamed off: physics is full of beautiful theory that are not correct) is a shame for the physics community.

22. **Peter Woit**
July 2, 2021

Adrian,
I strongly disagree with you about the issue of the intellectual honesty of my account of what Witten and Arkani-Hamed had to say. Yes, I was ignoring some other things they had to say, for example, Arkani-Hamed’s
“String Theory remains the most magical structure we have encountered in theoretical physics – and the magic seems directly connected to the particle physics of the real world”
I think I was doing him a favor by not quoting that kind of thing, which he really should be embarrassed about.

Note also that I was making no comment about the work people were presenting at the conference, was sticking to discussing particular important points about the current state of the subject being made by the two most talented and hard-w0rking leaders of the field. I don’t think I was misrepresenting those at all.

The problem with the string theory hype is not that it’s new, exciting, cutting-edge speculation, rather than well-established. String theory is now older than not just my students, but also many of their parents and unfortunately it’s very well-established, sometimes even taught to high school students. The problem is not that it’s too new to be evaluated properly, but that it’s been a failure.

23. **Essenza**
July 3, 2021

In the ending remark, Ooguri makes the observation that the percentage of speakers older than him is roughly a linear function (64-2x)% where x is the number of years after his PhD. While Ooguri has his own interpretation about this observation (incoming flux of young people in the field is as healthy as ever), I wonder what others think about it.

24. **Alessandro Strumia**
July 3, 2021

Following Nima’s final recommendation, an experimentalist and a string theorist hang out together.
Experimentalist: “I have to compute QCD, Wikipedia says I need to add unphysical ghosts on a local section and their fiberwise duals and project out the quotient Fock space isomorphic to the supergraded BRST cohomology pullback of the field configuration along a vertical diffeomorphism of the principal Batalin–Vilkovisky anafestic G-bundle, as if it were antani. This jargon sounds a bit confusing”.
String theorist: “indeed, what’s QCD?”

25. **M is for Missed**
July 3, 2021

I’m posting this since I thought it could be useful for those very busy hep-ph physicists who may wish to go directly to (what I & surely many others) considers as the key moment from “perspective” lectures.

If one knows how to read between the lines, then at this lecture you finally can hear what, in different words, could well be re-phrased as: the search for M-theory using dualities *failed*.

https://youtu.be/sMvbtgE-idQ?t=1227

Of course even his brightest former student couldn’t accept that:

https://youtu.be/sMvbtgE-idQ?t=1972

26. **Ellis_AND_Wooster**
July 4, 2021

The experimental community pretty much politely ignores the string theory community, and has, well, since 1974 or so when the analytic S-matrix failed to predict the charmed quark.

Blasting is well underway in South Dakota for the $2 billion + LBNF/DUNE facility.

The real stuff happens in physics without a press release, book, or blog.

27. **André**
July 4, 2021

Ellis_AND_Wooster,

it is true that experiments are real physics. But what DUNE will find has been predicted years ago: normal neutrino mass order, no additional neutrinos, no proton decay, and no physics beyond the standard model whatsoever.

These boring predictions, if confirmed by DUNE, will show that also in experimental physics no “real stuff” is happening…

28. **Robert A. Wilson**
July 5, 2021

Witten says something about a (possible) theory that is “intrinsically” quantum mechanical, which seems to mean a theory that does not start with a continuous field and quantise it, and which (therefore) presumably cannot be described in that way. Are there any actual attempts in the literature to build such an “intrinsically” quantum theory, or is this just pie in the sky?

29. **Tony**
July 5, 2021

Penrose's talk is interesting (https://www.youtube.com/watch?v=hk_6EtWUatM), as is the discussion between him and string folks afterwards.

30. **Peter Woit**
July 6, 2021

All,
Sorry, but deleted various attempts to carry on the experimentalist vs. theorist mudfight of Ellis_AND_Wooster/André. I suppose I shouldn't have even let that get started…

Robert A. Wilson,
As I've pointed out here before (can't find a link), there's nothing unusual about a quantum system that isn't just a conventional quantisation of a classical system. The simplest example is the qubit.

31. **Robert A. Wilson**
July 7, 2021

Yes, but I can't imagine that Witten was thinking of anything as well known and well understood as a qubit.

32. **Peter Woit**
July 7, 2021

Robert A. Wilson,
Likely Witten was thinking of things like the 6d (2,0) superconformal theory which has not classical limit and its construction is somewhat mysterious. The point though is that there are plenty of much simpler examples, which often can be understood purely in terms of representation theory.

33. **Jackiw–Teitelboim**
July 7, 2021

Hello, Robert A. Wilson
If you are interested in what field theories Witten's probably interested in, then I certainly recommend you to follow the on going ICTS's Quantum Field Theory, Geometry and Representation Theory, with Witten's mini-course starting tomorrow:

https://www.icts.res.in/program/qftgrt2021/talks

In order to get a feel what is at stake here, start with:
https://arxiv.org/pdf/0809.0305.pdf

Already at p.1 one reads, "The Problem: According to textbooks, the passage from classical mechanics to quantum mechanics is made by replacing Poisson brackets with commutators. However, this is an unrealistically simple description of the situation, even for a basic example such as the classical phase space."

Go on and you certainly will be assured that's not going to be about qubits, Dear Robert.

34. **Peter Woit**
July 7, 2021

Jackiw-Teitelboim,

That's about the general problem of quantizing a symplectic manifold, but my impression was that Witten was thinking of the even more general problem of a quantum theory with no known symplectic manifold of which it is the quantization.

The qubit example is a bit unfair since in some sense it's the quantization of the sphere, although this sphere is not a classical limit of the quantum theory (and sphere isn't a cotangent bundle of some configuration space).

Even More Langlands

July 6, 2021
Categories: Langlands

Various news at least tangentially related to the Langlands program:

- Cambridge University Press this month is publishing a volume edited by Julia Mueller, entitled The Genesis of the Langlands Program. The chapters by various authors concentrate on Langlands himself and the early history of the program. For the table of contents, try Google Books.
- For more recent developments, see this survey article by Ana Caraiani.
- Today's arXiv preprints include Gaiotto and Witten's Gauge Theory and the Analytic Form of the Geometric Langlands Program.
- An online conference on Quantum Fields, Geometry and Representation Theory sponsored by the ICTS in Bangalore started yesterday. This conference features several mini-courses relevant to geometric Langlands. In particular, Witten will be giving a mini-course on Quantization, Gauge Theory and the Analytic Approach to Geometric Langlands that should provide a good survey of the subject as well as an introduction to his new work with Gaiotto mentioned above. Videos are available here.
- Getting a bit more tangential, the Simons Foundation recently announced the formation of a Collaboration on Global Categorical Symmetries, directed by Constantin Teleman.

  Even more tangential, Laurent Fargues is now on Twitter, starting off with a thread on condensed sets.

Comments

1. **Mike**
   July 6, 2021

   The look inside feature on amazon.com gives a more complete table of contents for "The Genesis of the Langlands Program" than that in your Google link. The latter omits many of the chapters from its contents listing and the available sample pages – at least when I looked – only covered half the contents list. See:

   https://www.amazon.com/Genesis-Langlands-Program-Mathematical-Society/dp/1108710948?asin=1108710948&revisionId=&format=4&depth=1

2. **Felipe Zaldivar**
   July 7, 2021

   For a table of contents and a marketing excerpt try CUP at: https://www.cambridge.org/us/academic/subjects/mathematics/number-theory/genesis-langlands-program?format=PB

Deterioration of the World’s Thinking About the Deepest Stringy Ideas

July 12, 2021
Categories: Strings 2XXX

For quite a few years now, I’ve been mystified about what is going on in string theory, as the subject has become dominated by AdS/CFT inspired work which has nothing to do with either strings or any visible idea about a possible route to a unified fundamental theory. This work is very much dependent on choosing a special background, in tension with the idea that, whatever string theory is, it’s supposed to be a unique theory that relates all possible backgrounds. This issue came up in a discussion session at Strings 2021, and it turns out that others are wondering about this too. There’s this today from Lubos Motl:

> Aside from more amazing things, the AdS/CFT correspondence became just a recipe for people to do rather uninspiring copies of the same work, in some AdS_5/CFT_4 map, and what they were actually thinking was always a quantum field theory, typically in D=4 (and it was likely to be lower, not higher, if it were a different dimension!) whose final answers admit some interpretation organized as a calculation in AdS_5. But as Vafa correctly emphasized, this is just a tiny portion of the miracle of string/M-theory – and even the whole AdS/CFT correspondence is a tiny fraction of the string dualities.
>
> This superficial approach – in which people reduced their understanding of string theory and its amazing properties to some mundane, constantly repetitive ideas about AdS/CFT, especially those that are just small superconstructions added on top of 4D quantum field theories – got even worse in the recent decade when the “quantum information” began to be treated as a part of “our field”. Quantum information is a legitimate set of ideas and laws but I think that in general, this field adds nothing to the fundamental physics so far which would go beyond the basic postulates of quantum mechanics…
>
> When Cumrun correctly mentioned that the real depth of string theory is really being abandoned, Harlow responded by saying that there were some links of quantum information to AdS/CFT, the latter was a duality, and that was important. But that is a completely idiotic way of thinking, as Vafa politely pointed out, because string theory (and even string duality) is so much more than the AdS/CFT. In fact, even AdS/CFT is much more than the repetitive rituals that most people are doing 99% of their time when they are combining the methods and buzzwords of “AdS/CFT” and “quantum information”. Many people are really not getting deeper under the surface; they are remaining on the surface and I would say that they are getting more superficial every day.

According to Lubos, he’s not the only one who feels this way, with an “anonymous Princeton big shot” agreeing with him (hard to think of anyone else this could be other than Nima Arkani-Hamed):

> There is a sociological problem – coming from the terrifying ideological developments in the whole society – that is responsible for this evolution. I have been saying this for a decade or two as well – and now some key folks at Princeton and elsewhere told me that they agreed. The new generation that entered the field remains on the surface because it really lacks the desire to arrive with new, deep, stunning, revolutionary ideas that will show that everyone else was blind. Instead, the Millennials are a generation that prefers to hide in a herd of stupid sheep and remain at the surface that is increasingly superficial…
>
> So most of the stuff that is done in “quantum information within quantum gravity” is just the work of mediocre people who want to keep their entitlements but who don’t really have any more profound ambitions. As the aforementioned anonymous Princeton big shot told me, their standards have simply dropped significantly. The toy models in the “quantum information” only display a very superficial resemblance to the theories describing Nature. That big shot correctly told me that in the early 1980s, Witten was ready to abandon string theory because it had some technical problems with getting chiral fermions and their interactions correctly.
>
> Harlow says that many of the people – who may be speakers at the annual Strings conference and who may call themselves “string theorists” when they are asked – don’t really know even the basics of string theory. And they can get away with it. Just like there is the “grade inflation” and the “inflation of degrees”, there is “inflation in the usage of the term string theorist”. Tons of people are using it who just shouldn’t because they are not experts in the field at all. Harlow said that many of those don’t understand supersymmetry, string theory etc. but it’s worse. I think that many of them don’t really understand things like chiral fermions, either. It’s implicitly clear from the direction of the “quantum information in quantum gravity” papers and their progress, or the absence of this progress to be more precise. They just don’t think it’s important to get their models to a level that would be competitive with the previous candidates for a theory of everything – like the perturbative heterotic string theory, M-theory on G_2 manifolds, braneworlds, and a few more. They are OK with writing a toy model having “something that superficially resembles a spacetime” and they want to be satisfied with that forever.

I don’t want to start here an ad hominem discussion of Lubos and his often extreme and eccentric views. On the topic though of the devolution of string theory as a TOE to playing with toy models of AdS/CFT using quantum information, it seems quite plausible that not only the “anonymous Princeton big shot” but quite a few other theoretical physicists see the current situation as problematic.

Comments

1. **Low Math, Meekly Interacting**
   July 12, 2021

   Has not thinking about AdS/CFT also deteriorated, or at least diminished in applicability? It wasn’t that long ago there seemed to be loads of ambitious talk about AdS/QCD and stringy condensed matter physics. Now I assume it’s about relating the duality perhaps to real-world applications in quantum computing. A solution ever in search of a physically realized problem, seemingly.

2. **Gavin**
   July 13, 2021

   To me it reads like someone past his prime, blaming the lack of success in the field on anyone he can, instead of reevaluating whether the ideas themselves lead anywhere fruitful.

3. **Peter Woit**
   July 13, 2021

   Gavin,
   I agree that much of Lubos’s problem is that the dream of his youth (a unique M-theory, based on a matrix model or whatnot) has died and can’t be revived. People don’t work on what he wants them to work on because the ideas have failed. There is something to his description though of the current sorry state of the field, and what’s really interesting here is his plausible claim that Arkani-Hamed and others share it (although, unlike him, are not willing to say so in public…).

4. **Gavin**
   July 13, 2021

   @Peter Yes, I agree. The problem is that no one in the field has any idea how to make progress with the truly interesting questions. One approach is to work on topics that produce papers, even if they aren't truly interesting (this isn't really a criticism; there are always papers written making incremental progress even in healthy fields). Another approach, apparently, is to blame the lack of progress on the younger generation. I wish there was more discussion about re-evaluating the direction of the field to establish where progress realistically can be made, rather than pointing fingers or holding onto old dreams that are unattainable in the foreseeable future.

5. **Matthew Foster**
   August 10, 2021

   In condensed matter, the SYK model has generated a lot of excitement as a toy solvable model of strongly correlated matter. This is of perennial interest, because we still want to understand things like high-Tc superconductivity in the cuprates, but lack good analytical tools for tackling even the simplest models for these materials.

   SYK rather remarkably links "toy" Liouville gravity (Liouville quantum mechanics) and conformal invariance at short times, and random matrix theory at large time scales. It has a holographic description in terms of JT gravity, although I'm far from an expert on that part.

   2D Liouville field theory (as in the original Polyakov path integral, important for non-critical strings) can be formally used to describe properties of certain critical 2D wave functions in topological quantum materials. (The connection is via so-called log-correlated random energy models in statistical physics). These are related to real experiments, notably the quantum Hall plateau transition. The underlying structures seem to be logarithmic conformal field theories akin to percolation, but still very poorly understood. These are now resurfacing in yet another context: measurement-induced transitions in random quantum circuits. These toy models are not dissimilar to actual quantum hardware at Google.

   So there are indeed rich and fruitful developments that may yet profit from holography, Liouville field theory, and other stringy byproducts. I can't speak for what use any of this is to elementary or "real" quantum gravity physics, but some of us on the condensed matter side are indeed excited.

6. **Peter Woit**
   August 10, 2021

   Matthew Foster,
   There's lot of interesting work of the sort you mention, but it's about quantum mechanical models or maybe 2d QFTs, which have little connection to string theory (other than that people who worked on string theory turned to these topics as "toy models" to play with when their work on string theory failed). There's zero connection of any of this to the claimed motivation for string theory research as giving "our best hope" for a unified theory.

7. **Matthew Foster**
   August 11, 2021

   Peter, I think the part of Lubos' comments that do resonate a bit with me, although probably not in the way he intended, is that the string community seems to have moved far from its origins.

   As a condensed matter person, SUSY is the first part I'm happy to ignore (although there is a simpler version of SUSY that actually plays a useful technical role in many of the applications I described above, for quantum systems with randomness).

   But one can ask the very basic question of what goes wrong with the bosonic string?
   We know that in principle it can be defined in 4 dimensions (coupled to Liouville CFT), and it seems like a perfectly sensible starting point for an _effective_ theory of a gluon stretched between two quarks. I know real QCD is much more complicated than that, but nevertheless it is a well-defined problem.

   I find it very surprising that none of the "modern" string textbooks mention the work of discrete matrix methods that demonstrated the instability of summing over membranes to branching polymers. It's a quite basic and old result now, but it points out how hard it is to generalize path integrals to higher-dimensional membranes, owing simply to the entropic favorability of contracting the (closed, say) string to arbitrarily small radius and branching infinitely. Maybe this picture applies only in imaginary time, but it seems like the kind of result that should be more widely known and appreciated.

   On that note, if one forgets about SUSY, what about a purely fermionic string, i.e. with fermion bilinears playing the role of spacetime coordinates, instead of the free boson? It's likely strongly coupled, but has anyone tried this? One might hope that Pauli exclusion would help prevent the branching polymer collapse of the bosonic analog.

8. **Peter Woit**
   August 11, 2021

   Matthew Foster,
   Understanding string theory in four dimensions is a very old problem that has received (and continues to receive) a lot of attention from theorists. The main hope has always been that this will give you a calculational method for strongly coupled gauge theories like QCD (as opposed to a unified theory with gravity). The problem is simply that no one has managed to have more than very partial success with this.

   This kind of work goes back more than forty years, to around 1980, when it was being pursued intensively by Polyakov, Migdal and others. During my graduate student years (1979-1984), trying to solve QCD was the center of attention, and I spent a lot of time trying to understand these string theory based ideas. Post-1984, what got most attention was the 10d critical superstring and unification, but for many people working on string theory, the 4d string was always a focus of their work.

   So, sure, this remains an idea worth pursuing, but one should be aware that it's an idea the best in the field have spent over forty years banging their heads against unsuccessfully, not something that hasn't gotten attention for no good reason.

9. **Matthew Foster**
   August 11, 2021

   Hi Peter,

   "So, sure, this remains an idea worth pursuing, but one should be aware that it's an idea the best in the field have spent over forty years banging their heads against unsuccessfully, not something that hasn't gotten attention for no good reason."

   Of course. I just meant that it is surprising to me that the intrinsic difficulty of solving the 4D bosonic string is glossed over. Ivan Kostov told me that in his opinion, the bosonic string _is solved_: it has the branching polymer instability seen in the discrete approach to 2D quantum gravity (Cates, Froehlich, Ambjoern, and others), and that's it. Seems like a major omission that this wasn't included early in, e.g., Polchinski's volume 1. (And that's not a dig at Polchinksi, since I did my Ph.D. at UCSB. He was the friendliest, most approachable string practitioner there, at least as far as the students were concerned).

   Given this, without SUSY the case for strings seems doomed indeed. Another comment is that strongly fluctuating, say multifractal behavior, might make perfect sense in the context of quantum gravity. This is something we do know how to handle in certain contexts on the condensed matter side. The branching polymer picture, and what emerges generally from studies of 2D quantum gravity (dominated by extremal statistics of rare curvature fluctuations) is very different from the cartoon of smoothly joining strings in textbooks.

Would still like to know about the pure fermionic version, though…too busy with course preparation now to dig.

The Problem of Quantization

July 12, 2021
Categories: Quantum Mechanics, Quantum Theory: The Book, Uncategorized

I've been watching Witten's ongoing talks about geometric Langlands mentioned here, and wanted to recommend to everyone, mathematician or physicist, the first of them, on The Problem of Quantization (pdf here, video here, the question session is very worthwhile). For those very sensibly not interested in the intricacies of geometric Langlands, this talk is about the fundamental issue of "quantization".

Hamiltonian mechanics gives a beautiful geometrical formulation of classical mechanics in terms of the Poisson bracket on functions, while quantum mechanics involves operators with non-trivial commutators. It was Dirac's great insight that "quantization" takes functions to operators, taking the Poisson bracket to the commutator. In mathematician's language, it's supposed to be a unitary representation of the Lie algebra of the infinite dimensional group of canonical transformations of a symplectic manifold, so a homomorphism from functions with Poisson bracket to the Lie algebra of skew-adjoint operators on a complex vector space.

The problem with this is that you'd like to have an irreducible representation, but the only way to get this is to pick some extra structure on the symplectic manifold. The standard example is the phase space $\mathbf R^{2n}$, where you have to pick a decomposition into position and momentum coordinates. The state space will then be functions of just position, or just momentum. A different choice is to complexify, and look at functions of either holomorphic or anti-holomorphic coordinates. This choice is called a "polarization". One aspect of the "problem" of quantization is that, given a phase space (symplectic manifold), there may not be an appropriate polarization. Or, there may be many different ones, with no obvious reason why they should give the same quantum theory.

Witten doesn't mention one aspect of this that I find most fascinating. For relativistic quantum field theories the phase space is a space of solutions of a relativistic wave-equation. To get physically sensible results one must choose a polarization that distinguishes between positive and negative energy (or between functions which extend holomorphically in the positive or negative imaginary time direction).

In these lectures, Witten advertises a rather exotic quantization contruction, using (even for a finite dimensional symplectic manifold) conformally invariant boundary conditions in a two-dimensional QFT. I'm not convinced that this is really a good way to deal with the case where what you're doing is looking for representations of a finite-dimensional Lie algebra, but it's plausible this is the right way to think about the geometric Langlands situation, where you're trying to quantize a moduli space of Higgs bundles.

In the question section, someone asked about my favorite approach to this problem, essentially using fermionic variables and cohomology. This can be thought of in general as using spinors and the Dirac operator, with the Dolbeault operator a special case when the symplectic manifold is Kähler. Witten responded that he had only really looked at this in the Kähler special case.

Comments

1. **Jackiw–Teitelboim**
   July 12, 2021

   Hello,

   Yes, not difficult to guess from my former comments on your website that I'm the person who asked the question regarding yours, and mine, "favorite approach." I think Witten's reply was somewhat disappointing since I see no reason to constrain oneself to Kähler's structure.

2. **Peter Orland**
   July 13, 2021

   I always felt that the inverse problem was at least as important; what is the corresponding {\em classical} model corresponding to some quantum system?

   Quantization is not unique, of course, but the classical limit of a quantum system is unique (although it may have different descriptions).

   My impression is that mathematicians and people doing physical mathematics don't spend much effort on the inverse problem. It has a lot of relevance to real physics, though. For example, there are interesting condensed-matter systems which should correspond to field theories with Lagrangians (hence the classical limit), but where the connection is not proven.

3. **Arnold Neumaier**
   July 13, 2021

   Peter Orland,

   unfortunately, the classical limit of a quantum system is not unique either; it depends on the choice of a family of (generalized) coherent states in the Hilbert space of the quantum system, which is not unique. For the classical limit in terms of coherent states, see, e.g.,

   Yaffe, L. G. (1982). Large N limits as classical mechanics. Reviews of Modern Physics, 54(2), 407.

4. **Peter Orland**
   July 13, 2021

   Arnold Neumaier,

   That is not really the issue. It depends upon what you mean by classical, i.e., which parameter you take to zero. If that parameter is $\hbar$, there is no ambiguity. If you decide that the parameter is 1/N, as in the example you give, what you obtain can be entirely different.

   On the other hand, the problem I raised includes many large-N models, which have not been successfully been written as classical systems. The result should be unique, but we don't usually know what it is.

   What I was referring to above, was that the classical limit of some systems can't yet be directly produced, only conjectured. An example is the XXX spin chain, at large spin s. This is supposed to be the O(3) sigma model (Haldane's Nobel Prize was at least partly due to this identification), possibly with a $\theta=\pi$ term (for half-integer s). There is a lot of evidence for this conjecture, but it is not proved. There are other such conjectures around.

5. **Alex**
   July 13, 2021

Personally, I think the case for the finite dimensional case is pretty close to solved in many aspects, and now fully streamlined in the work of Landsman:

-Lie Groupoids and Lie algebroids in physics and noncommutative geometry (https://arxiv.org/abs/math-ph/0506024)

-Between classical and quantum (https://arxiv.org/abs/quant-ph/0506082)

-Quantization commutes with singular reduction: cotangent bundles of compact Lie groups (https://arxiv.org/abs/1508.06763)

Field theories, of course, are a different beast.

"To get physically sensible results one must choose a polarization that distinguishes between positive and negative energy".

That's true, although I would say the problem is not so much in the quantization step (an abstract Weyl C*-algebra can be defined without that choice), but in obtaining a concrete representation of that algebra, where you indeed need to make that choice (which amounts to select a particular quasi-free/Gaussian algebraic state on which to base the GNS representation of the algebra; if there's a timelike Killing vector field on the spacetime, you can make the interpretation of that step in terms of "positive/negative energy Fourier modes") And, still, all of this is in the case of free/non-interacting field theories!

6. **Arnold Neumaier**
July 13, 2021

Peter Orland,

in quantum theories not derived from an action principle there may not even an $\hbar$ in the theory!

Of course if you have a Hamiltonian where you know (or assume) the precise $\hbar$ dependence and know (or assume) which quantities should survive the limit $\hbar\to 0$ then there is no ambiguity about the classical limit, though finding it may still be nontrivial.

Can you provide references for "There is a lot of evidence for this conjecture"?

7. **Peter Orland**
July 13, 2021

I did not mean specifically theories with known S matrices, e.g. satisfying Yang-Baxter equations.

The literature on the Haldane conjecture is substantial, and the best place to find more discussion is in Haldane's paper and those citing it.

The same problem occurs for pretty much any spin chain or vertex model which is argued to be Bosonic QFT. Some degrees of freedom are relevant, hence should be kept, but others should be thrown away.

Higher-dimensional examples include quantum link models of non-Abelian gauge theories. The Yang-Mills Lagrangian has not been derived from these. Long ago, Daniel Rohrlich and I argued that some of these are YM, but some are not even relativistic.

8. **Peter Woit**
July 13, 2021

Alex,
One of the main points Witten was making is that quantization is not just producing an abstract algebra, but also a state space for them to act on. This is where you really need to introduce extra structure.

He doesn't describe this in general, but he's referring to the fundamental problem of Lie algebra representation theory: given a Lie algebra you have an abstract associative algebra (the universal enveloping algebra), which is a quantization of a symmetric algebra, but producing irreducible representations is very tricky. The orbit methods says they should be quantizations of co-adjoint orbits, but how to do this in general runs into interesting problems (which he hopes brane quantization might resolve).

9. **ARoxdale**
July 14, 2021

> What textbooks don't usually emphasize is that this doesn't work,or better, it only works approximately, to first order in h, or for a preferred class of functions on the classical phase space.
>
> Accordingly, there actually is no completely natural operation of quantizing a classical phase space – none that is known, and I believe, none that exists. Quantization always requires some additional structure.As Ludwig Faddeev used to say, quantization is "an art not a science.

Is there some qualifier for this remark? Or is quantization really only something that can be trusted to 1st order?

10. **Peter Woit**
July 14, 2021

ARoxdale,
It's not a question of being trusted, the problem is more one of ambiguity: there are lots of possible quantizations of a given phase space, all of which differ by terms which vanish in the classical limit hbar goes to zero. This is mentioned in some textbooks as the "operator ordering problem".

If you impose various conditions you want your quantization to satisfy, you can sometimes get a unique quantization, but only for some special class of functions on phase space.

My QM textbook
https://www.math.columbia.edu/~woit/QMbook/qmbook-latest.pdf
does emphasize this problem, explains in great what happens for phase space $\mathbf{R}^{2n}$
Witten is interested in more general symplectic manifolds, where the situation is much more complicated, with geometric quantization a partially successful way to deal with it (while requiring the specification of a polarization, as Witten discusses).

11. **Lowell Brown**
July 19, 2021

I have no idea why Witten is concerned with this problem.

Quantum mechanics is the fundamental theory.
So one could, perhaps, be concerned with the classical limit of quantum mechanics, not the other way round.
Physics is concerned with computing experimental results. So you do this computation and see if this class of experiments has a macroscopic limit.

Steven Weinberg 1933-2021

July 24, 2021
Categories: Obituaries

I heard this morning the news that Steven Weinberg passed away yesterday at the age of 88. He was arguably the dominant figure in theoretical particle physics during its period of great success from the late sixties to the early eighties. In particular, his 1967 work on unification of the weak and electromagnetic interactions was a huge breakthrough, and remains to this day at the center of the Standard Model, our best understanding of fundamental physics.

During the years 1975-79 when I was a student at Harvard, I believe the hallway where Weinberg, Glashow and Coleman had offices close together was the greatest concentration of the world's major figures driving the field of particle theory, with Weinberg seen as the most prominent of the three. From what I recall, in a meeting one of the graduate students (Eddie Farhi?) referred to "Shelly, Sidney and Weinberg", indicating the way Weinberg was a special case even in that group. I had the great fortune to attend not only Coleman's QFT course, but also a course by Weinberg on the quantization of gauge theory.

Weinberg was the author of an influential text on general relativity, as well as a masterful three-volume set of textbooks on QFT. The second volume roughly corresponds to the course I took from him, and the third is about supersymmetry. While most QFT books cover the basics in much the same way, Weinberg's first volume is a quite different, original and highly influential take on the subject. It's not easy going, but the details are all there and his point of view is an important one. When you hear Nima Arkani-Hamed preaching about the right way to understand how QFT comes out uniquely as the only sensible way to combine special relativity and quantum mechanics, he's often referring specifically to what you'll find in that first volume.

Besides his technical work, Weinberg also did a huge amount of writing of the highest quality about physics and science in general for wider audiences. An early example is his 1977 The Search for Unity: Notes for a History of Quantum Field Theory (a copy is here). His 1992 Dreams of a Final Theory is perhaps the best statement anywhere of the goal of fundamental physical theory during the 20th century. His large collection of pieces written for the *The New York Review of Books* covers a wide variety of topics and all are well worth reading.

At the time of the 1984 "First Superstring Revolution", Weinberg joined in and worked on string theory for a while, but after a few years turned to cosmology. In early 2002 he was one of several people I wrote to about the current state of string theory, and here's what I heard back from him:

> I share your disappointment about the lack of contact so far of string theory with nature, but I can't see that anyone else (including those studying topological nontrivialities in gauge theories) is doing much better. I thinks that some theorists should go on pushing as hard as they can on string theory, and others should do something else, but it is not easy to see what. I have myself voted with my feet (if that is the appropriate organ here) and switched entirely to work in cosmology, which is as exciting now as particle physics was in the 1960s and 1970s. I wouldn't criticize anyone for their choices: it's a tough time for fundamental physics.

A couple years after that time, Weinberg's 1987 "prediction" of the cosmological constant became the main argument for the string theory multiverse. This "prediction" was essentially the observation that if you have a theory in which all values of the cosmological constant are equally likely, and put this together with the "anthropic" constraint that only for some range will galaxy formation give what seem to be the conditions for life, then you expect a non-zero CC of very roughly the size later found. I've argued ad nauseam here that this can't be used as a significant argument for string theory in its landscape incarnation. One way to see the problem is to notice that my own theory of the CC (which is that I have no idea what determines it, so any value is as likely as any other) is exactly equivalent to the string landscape theory of the CC (in which you don't know either the measure on the space of possible vacua, or even what this space is, so you assume all CC equally likely). One place where Weinberg wrote about this issue is his essay Living in the Multiverse, which I wrote about here (the sad story of misinterpretation of a comment of mine there is told here).

Weinberg's death yesterday, taking away from us the dominant figure of the period of particle theory's greatest success is both a significant loss and marks the end of an era. His 2002 remark that "it's a tough time" is even more true today.

Update: Scott Aaronson writes about Weinberg here, especially about getting to know him during the last part of his life.

Update: For Arkani-Hamed on Weinberg, see here.

Update: Glashow writes about Weinberg here.

Comments

1. **Alessandro Strumia**
   July 24, 2021

   Weinberg was the last of the physics giants who produced fundamental theories validated by experiments. After half a century his Standard Model still holds, so maybe now is the fist time in physics history without scientific giants.

2. **Shantanu**
   July 24, 2021

   A tribute to him from one Astrophysics colleague who had recently joined UT Austin
   https://twitter.com/MBKplus/status/1418972769509855236

3. **Ricardo**
   July 24, 2021

   His last book "Foundations of Modern Physics came out a few months ago. From the preface:
   "This book treats such a broad range of topics that it is impossible to go very far into any of them. Certainly its treatment of quantum mechanics, statistical mechanics, transport theory, nuclear physics, and quantum field theory is no substitute for graduate-level courses on these topics, any one of which would occupy at least a whole year. This book presents what I think, in an ideal world, the ambitious physics student would already know when he or she enters graduate school. At least, it is what I wish that I had known when I entered graduate school."
   Does anybody have an opinion about how well he succeeded?

4. **Wei Liu**
   July 24, 2021

   It is sad news that Weinberg is not with us anymore but I cannot agree with Alessandro Strumia that "Weinberg was the last of the physics giants who produced fundamental theories validated by experiments…so maybe now is the fi(r)st time in physics history without scientific giants." Please be reminded that C. N. Yang and T. D. Lee still are healthily living.

5. **Mark Weitzman**
July 24, 2021

@Ricardo: I wrote the following review on one of my piazza sites a few weeks ago. I am currently half way through the book, and I am enjoying the book, although I am familiar with most of the content already. As I indicated below it is more of a review book, than a teaching book.

Steven Weinberg had come out with a new book: Foundations of Modern Physics. I have read the first 2 chapters, and scanned the rest of the book – the book is a short 300 pages and the hardcover is about $40. The level of the book is intermediate to advanced undergraduate.

I often encounter online students who have taken several of the MIT physics MOOC's but often feel that they either lack some background, or don't see what they have learned (especially in the QM sequence) and how it will be applied. To many of these students my advice is usually to read the Feynman Lectures on physics, to get the big picture and for more background in QM prerequisite physics, and/or find a good textbook on modern physics.

When I was in college almost half a century ago, I remember using Leighton's classic book Principles of Modern Physics 1959 (about 700 pages in length), and suggest students try and find a more recent book covering roughly the same material – I haven't seen one, so let me know if you have. There seem to be many freshman/sophomore level books covering modern physics, but I don't think these are sufficient.

Weinberg's book is less of a textbook (while there are 25 problems at the end of the book, there are no exercises at the end of chapters, and no worked out examples). It is written in Weinberg style, few pictures or diagrams, unusual symbols (m_1 for the atomic mass unit, usual notation is u), nice but sometimes terse arguments, and excellent content with interesting historical asides. The math level is low at the beginning (algebra – elementary calculus), and rises a little with the level of the material, but not nearly as difficult as his graduate level books.

There are seven chapters in the book:

Early Atomic Theory
Thermodynamics and Kinetic Theory
Early Quantum Theory
Relativity
Quantum Mechanics
Nuclear Physics
Quantum Field Theory
So the coverage is exactly what a student should be looking for in a modern physics book (perhaps astrophysics and cosmology are left out, because Weinberg just published a set of lectures on astrophysics, and has a whole book on Cosmology). The book seems more of a review or plug holes in background, and less of a teaching masterpiece.

I recommend that students looking for a book on this type of material take a look at the book and see if it is for them. I think students who are taking or have finished the MITx 8.04-8.06 sequence might find the book worthwhile as a review and also as filling out some material, and get a preview of quantum field theory as well.

6. **Ralf Hofmann**
July 25, 2021

Steven Weinberg – a great, highly creative physicist and wonderfully involved teacher of fundamental physics who shaped my own education in quantum field theory and cosmology during essential steps. I'll miss him and his sober views very much.

7. **Alessandro Strumia**
July 25, 2021

Dear Wei, thank you, let me try to explain better. Historians like to choose a somehow arbitrary moment to symbolise a gradual change. If the change I mentioned will really happen, I expect they will choose this moment.

8. **Thomas Larsson**
July 25, 2021

Alessandro, Glashow is still around, isn't he? So not quite the last. As a sophomore, I attended a talk by GSW when they were here in Stockholm to collect their Nobel prize, and I understood absolutely nothing.

Btw. I learned from Lubos that Miguel Virasoro has passed away as well.

9. **Sebastian Thaler**
July 25, 2021

I was working at Cambridge University Press in 1995 when the first volume of his "Quantum Theory of Fields" was released. I recall speaking briefly with him on the phone about something trivial like distributing review copies, and picking up a copy of the book that had been delivered to our office. I read the first page and then put it back down—it was just a *bit* over my head…

10. **Amitabh Lath**
July 25, 2021

My PhD thesis was a measurement of the Weinberg angle (sin^2\theta_W) using polarized electrons at the SLAC linear collider. Some people insisted the W stood for Weak but I always held it is W for Weinberg. I read and reread his paper A Model of Leptons many times until I could reproduce it at my defense. I wish all theory papers were as clear and understandable.

11. **Severin Pappadeux**
July 25, 2021

Miguel Virasoro died the same day

12. **Shantanu**
July 25, 2021

Amitabha: From what I understand the "Weinberg" angle was first introduced by Glashow.

13. **Amitabh Lath**
July 26, 2021

Shantanu, that is funny because I was told to call it Weak Mixing Angle (and not Weinberg angle) after I gave this talk at Harvard. Glashow was in the room but he was not the one who made that comment. I made the correction with a marker on my transparencies right there. This was a while ago, but I also vaguely remember some chatter about "well if this is right it means the higgs is light". We were, and it was.

14. **Shantanu**
July 26, 2021

Amitabha: This is also mentioned on Peter's blog https://www.math.columbia.edu/~woit/wordpress/?p=17
I have also heard this mentioned in many HEP seminars I attended as a grad student.

15. **Robinson**
July 27, 2021

Dreams of a Final Theory is available on Audible. It's read by Weinberg himself. It's written for a general audience, so even untutored but interested readers like me can understand it.

16. **D R Lunsford**
July 27, 2021

That is sad news, but it was a long life well-lived. He was an authentic giant. His book on GR is still my favorite (tied with that of Fock) and I was lucky to learn the subject in detail mostly from that. Agree also about QTF II. I wish we had made more progress in his last years for him to enjoy and contribute to. RIP.

-drl

More of the Same (Physics, Math and Unification)

July 29, 2021
Categories: Euclidean Twistor Unification, Langlands

I was going to just provide the following links with a some comments, but decided it would be a good idea to put them into what seems to me the larger context of where we are in fundamental physics and its relationship to mathematics.

For the latest on the conventional physics approach to unification (GUTS, SUSY, strings, M-theory), there's:

- The Lex Fridman podcast has an interview with Cumrun Vafa. Going to the section (1:19:48) – Skepticism regarding string theory) where Vafa answers the skeptics, he has just one argument for string theory as a predictive theory: it predicts that the number of spacetime dimensions is between 1 and 11.
- A second edition of Gordon Kane's String Theory and the Real World has just appeared. One learns there (page 1-19) that

> There is good reason, based on theory, to think discovery of the superpartners of Standard Model particles should occur at the CERN LHC in the next few years.

For the latest in mathematics and the interface of math and physics, there's

- Quanta magazine has a good article about the dramatic Fargues-Scholze result linking geometry and number theory.
- A long paper by Gaiotto and Witten on their new ideas about quantization, giving a lot of background and details, in particular exploring the link to the currently best-developed set of ideas relating geometry and quantization (so-called "geometric quantization").

About the first two links, I'm at a loss for words.

The second two are extremely interesting topics indicating a deep unity of number theory, geometry and physics. They're also not topics easy to say much about in a blog posting. In the Fargues-Scholze case that's partly because the new ideas they have come up with relating arithmetic and geometry are ones I don't understand very well at all (although I hope to learn more about them in the future). The connections they have found between representation theory, arithmetic geometry, and geometric Langlands are very new and it will likely be quite a few years before they are well understood and their implications well-developed.

In the Gaiotto-Witten case, some of what they discuss is very familiar to me: geometric quantization has been a topic of fascination since my student days, and one major goal of my QM book was to work out in detail (for the case of $\mathbf R^{2d}$) some of the subtleties about quantization that they discuss. For co-adjoint orbits in Lie algebras, geometric quantization has a long history, and "brane quantization" may or may not have anything new to say about this. For moduli spaces of vector bundles on Riemann surfaces, and Hitchin moduli spaces of Higgs bundles on Riemann surfaces, "brane quantization" might come into its own.

There is a fairly short path now potentially connecting fundamental unifying ideas in number theory and geometry to our best fundamental theories in physics (and seminars on arithmetic geometry and QFT are now a thing). The Fargues-Scholze work relates arithmetic and the central objects in geometric Langlands involving categories of bundles over curves. These categories in turn are related (in work of Witten and collaborators) to 4d TQFTs based on twistings of N=4 super Yang-Mills. This sort of 4d QFT involves much the same ingredients as 4d QFTs describing the Standard Model and gravity. For some better indication of the relation of number theory to this sort of QFT, a good source is David Ben-Zvi's lectures this past semester (see here and here). I'm hopeful that the ideas about twistors and QFT in Euclidean signature discussed here will provide a close connection of such 4d QFTs to the Standard Model and gravity (more to come on this topic in the near future).

No Comments

Some Math Items

July 30, 2021
Categories: abc Conjecture, Uncategorized

Some math items that may be of interest:

- Zentralblatt Math has a review of Mochizuki's IUT papers, by Peter Scholze. Scholze explains the problem with the proof claimed in these papers. For more details, see his manuscript with Stix, or this discussion hosted last year on this blog.
- The Bulletin of the AMS will be running a special issue on the work of Michael Atiyah. Available already are two wonderful articles: Michael Atiyah's work in algebraic topology, by Graeme Segal, and The Atiyah-Singer index theorem, by Dan Freed.
- For a talk by Helmut Hofer about Andreas Floer and his work, together with contributions from others who knew Floer, see The Floer Jungle: 35 years of Floer theory.
- The latest AMS Notices has a memorial tribute to Steve Zucker, with a detailed discussion of his career and mathematical work. His collaborator David Cox explains that the origin of their collaboration was exactly what everyone has always suspected.

Update: The Scholze review has been removed (temporarily?). A cached version is here.

Update: The review was temporarily removed just because what was posted wasn't a finalized version, this is explained here. They should repost once Scholze has a chance to make any final edits.

Update: The review is back up.

Update: Michael Harris has a new substack site, where he'll be writing about the mechanization of mathematics. I'm glad to see someone doing this from his point of view.

Comments

1. **Essenza**
   July 31, 2021

   Seems like Peter's comments are removed/editted (?). Here's the original (I think; found online):
   https://web.archive.org/web/20210730194853/https://zbmath.org/pdf/07317908.pdf

2. **TS**
   July 31, 2021

   Zentralblatt took down Scholze's review and replaced it with the paper's abstract. Here is a cached version of the review https://webcache.googleusercontent.com/search?q=cache:Lm7YcNafEeoJ:https://zbmath.org/pdf/07317908.pdf

3. **David Roberts**
   July 31, 2021

   That "review" of IUT 1 is a cut-and-paste of the abstract, as can be seen by comparing with the published version here: https://doi.org/10.4171/PRIMS/57-1-1

   I haven't compared with the published abstracts, but the other "reviews" of parts 2–4 look to be the same. If there was a review by Scholze, it's been removed; I'd love to see it…

4. **David Roberts**
   July 31, 2021

   I see via Nalini Joshi on Twitter that the review was captured before it evaporated: https://web.archive.org/web/20210730194853/https://zbmath.org/pdf/07317908.pdf (and she alludes to the discussion on Reddit here: https://www.reddit.com/r/math/comments/ousg3s/scholzes_review_of_mochizukis_paper_for/)

   If some kind of political pressure has been applied to remove this, then that is not a good look for anyone involved.

5. **SD**
   August 2, 2021

   At the end of the Hofer's nice history talk, Gromov – as is usual of him – makes some interesting general comments concerning Scholze's condensed mathematics.

6. **nad**
   August 9, 2021

   It seems to me that Helmut Hofers talk was at least partially meant to keep up discussing mathematical (work) culture and it's possible effects.

   That is – it is problematic to publicly discuss possible reasons for Andreas Floer's -as the german Wikipedia writes *unexpected*– suicide, on the other hand it is not too far fetched to assume that his mathematical life took quite a role in his decisions. I therefore found it considerably well-balanced that Helmut Hofer mentioned in his talk in a matter-of-fact way some of the possible strains that may have impaired Andreas Floer.

   So for example he mentioned that Andreas Floer may have had probably felt quite under pressure, when there was some kind of "hype" about his work and that people expected him to write up his results. He mentioned that Mathematics was not Andreas Floer sole passion, but that he played viola da gamba and thus that it was probably not easy to conciliate his other passions with his mathematical passions and (new) duties. Briefly after his death I had heard someone mentioning that Andreas Floer was quite probably suffering from something that could be somewhat called "stagefright" – another strain, which may come with job duties or at least with the roles a successful mathematician is often expected to fill in.

   I had met him only very briefly before his death when he was a few days in Berlin. It may not be sensible to state the impressions I got from the very few interactions, as they are maybe simply too attached to particular moments – but in one instance he appeared to me quite deeply troubled. In the last interaction though he was sort of looking forward to his planned longer stay in Berlin and completed his farewell to me with something like a smiling "Bis dann" (see you soon). So I agree that his suicide didn't appear to have been planned, but that it was -at least to me- rather -as Wikipedia writes- unexpected.

7. **kitchin**

August 17, 2021

Nad, I wonder though if the “jungle” approach is just something made up by Hofer. Rare mathematicians do gain a wide knowledge of the waterfront early on. Yet may they still spend three months banging against a crack in the wall? I refer here to the story in the link of Floer taking three month to accomplish one of his goals after discussing it with a colleague. I admit some prejudice because Hofer seems so affable and interested in history, and he is looking for an answer to what happened.

(It does make me wonder though if I should have tried a more jungle approach when younger! I do remember reading somewhat widely, but without much effect.)

There’s also the issue of math anxiety, which can hit at any stage. Or, more likely in Floer’s case, locking up on a perfectionist writer’s block. I gather he enjoyed explaining his ideas in person.

8. **nad**
August 19, 2021

@kitchen
I had problems to understand your comment.
—————————————————————————————————————

❓ By

> “I wonder though if the “jungle” approach is just something made up by Hofer.”

do you mean “made up” versus “observed” or “made up” because it is an analogy or something else?

—————————————————————————————————————

❓ I rewatched the talk but I couldn’t find the story you are referring to:

> “I refer here to the story in the link of Floer taking three month to accomplish one of his goals after discussing it with a colleague.”

Could you please say around which minute the story was told ?

—————————————————————————————————————

❓ A prejudice -in my understanding- is often something like an idea about “how objects happen/act in some time instance in past, present or future”. What do you mean by prejudice here:

> ” I admit some prejudice because Hofer seems so affable and interested in history, and he is looking for an answer to what happened.”

Do you mean that you know what happened?

Conversations on Quantum Gravity

August 26, 2021
Categories: Book Reviews

Things for many years now have been going badly for string theory on the public relations front. Today the Economist has [Physics seeks the future: Bye, bye, little Susy](), where one finds out that:

> But, no Susy, no string theory. And, 13 years after the LHC opened, no sparticles have shown up. Even two as-yet-unexplained results announced earlier this year (one from the LHC and one from a smaller machine) offer no evidence directly supporting Susy. Many physicists thus worry they have been on a wild-goose chase…
> Without Susy, string theory thus looks pretty-much dead as a theory of everything. Which, if true, clears the field for non-string theories of everything.

Unfortunately for the public understanding of science, this is followed by

> But at the moment the bookies' favourite for unifying relativity and the Standard Model is something called "entropic gravity"… in the past five years, Brian Swingle of Harvard University and Sean Carroll of the California Institute of Technology have begun building models of what Dr Verlinde's ideas might mean in practice, using ideas from quantum information theory.

For something much more anecdotal, on Saturday night I was having dinner outside in a hut during a rainstorm on the Upper East Side (having fled an aborted Central Park concert), and started talking to a couple seated nearby. When informed I taught math and did physics, one of them recommended Carlo Rovelli's new book to me, and said he hoped I wasn't doing string theory. Luckily I could reassure him about that.

This morning I found out about [Conversations on Quantum Gravity](), a fascinating book published by Cambridge that appeared online today, [hard copies for sale in November](). It consists of interviews about quantum gravity put together by Dutch string theorist Jay Armas, starting in 2011. The scale of this project is immense: there are 37 interviews, most of them rather long and detailed, making up a book of 716 pages. What I'm writing here is based on a day's worth skimming of the book. I'll likely go back again and look more carefully at parts of it.

Roughly half the interviewees are string theorists, with the author making a concerted effort to also include non-string theory approaches to quantum gravity. I made the mistake of starting off by reading some of the string theorist interviews, which was rather depressing. By the end of the day, after making my way through about 20 long interviews with string theorists, with few exceptions the story they were telling was one I'm all too familiar with. It's roughly

> We don't actually know what string theory is, just that it's a "framework" that encompasses QFT and much more. We can't predict anything with it now and don't see any plausible way of predicting anything in the future, but the theory is a successful theory of quantum gravity, unlike our competition. There is no good reason for people to be working on anything else.

For example, here's Cumrun Vafa:

> **If a young student asks you what approach to quantum gravity they should work on, what would your answer be?**
>
> There is no question that string theory is the right framework to understand quantum gravity. By this I mean that it is closer to the truth than any other existent theory.
>
> **Is it worth exploring other approaches?**
>
> Well . . . certainly being close-minded is not good. We should be open to other developments. But the fact that there exist other subjects does not justify exploring them if they are not on equal footing with string theory.

and here's Edward Witten:

> **Due to the lack of experimental data, there exist a plethora of different approaches to quantising gravity. Which of these approaches, in your opinion, is closer to a true description of nature and why?**
>
> I would say your premise is a little misleading. String theory is the only idea about quantum gravity with any substance. One sign is that where critics have had interesting ideas (non-commutative geometry, black hole entropy, twistor theory) they have tended to be absorbed as part of string theory.

and David Gross:

> **So you don't think that other approaches like loop quantum gravity have . . .**
>
> Loop quantum gravity is total BS. I mean, it's really not worth discussing it. Don't put that in the book. But, it really isn't.

Luckily Armas doesn't take up Gross on the suggestion that loop quantum gravity is not worth discussing, interviewing quite a few people who are working on research programs that have grown out of it. I got much more out of these interviews, which were very different in tone and content than the ones with string theorists. Many of them gave a very clear account of the technical problems these approaches have encountered, referring to very specific well-defined models and calculations. Instead of the triumphalist claims and vague speculation of the string theorists there was a careful explanation of exactly what they were trying to do and the problems they were trying to overcome.

There's a huge amount worth reading in these interviews, perhaps I'll later add some more pointers. A couple specific examples that occur to me right now are Steve Carlip's careful discussion of the quantization of the toy model of 2+1 dimensional gravity, and Lee Smolin's very personal account of his frustration at the reception of his book "The Trouble With Physics".

If your institution is paying Cambridge for access, you should take advantage of this now and take a look. Congratulations to Jay Armas for bringing us this material.

Update: There's a new preprint out by historian of science Sophie Ritson, [Constraints and Divergent Assessments of Fertility in Non-empirical Physics in the History of the String Theory Controversy](), which examines in detail the arguments of the string wars and later over how to evaluate string theory. While I don't think there's a single reference in the 716 page Armas book to anything I've written, my views do make an appearance in this article.

Update: There's a linked editorial in the Economist [Fundamental physics is humanity's most extraordinary achievement](), which (rather optimistically) sees the current state of affairs as:

> Supersymmetry is a stalking horse for a yet-deeper idea, string theory, which posits that everything is ultimately made of infinitesimally small objects that are most

easily conceptualised by those without the maths to understand them properly as taut, vibrating strings.

So sure were most physicists that these ideas would turn out to be true that they were prepared to move hubristically forward with their theorising without experimental backup—because, for the first decades of Supersymmetry's existence, no machine powerful enough to test its predictions existed. But now, in the form of the Large Hadron Collider, near Geneva, one does. And hubris is turning rapidly to nemesis, for of the particles predicted by Supersymmetry there is no sign.

Suddenly, the subject looks wide open again. The Supersymmetricians have their tails between their legs as new theories of everything to fill the vacuum left by string theory's implosion are coming in left, right and centre.

Comments

1. **Jackiw–Teitelboim**
   August 26, 2021

   One might not be so unwise in conceiving the possibility that *realistic* progress in fundamental physics in near future, in the reductionist sense well articulated by Weinberg in his many writings (which ironically, many "string theorists" nowadays working on subjects such as "celestial" holography, "it from qubit," whatever, clearly gave up on superstring unification), might not be the problem of *quantum gravity* at all.

   As I student to whom reading older Proceedings and Reviews, around the time when the current SM was established, is an enjoyable weekend hobby, it seems to me that at some time, researches decided that the search for a better understanding of the fundamental interaction should necessarily include quantum gravity.

   Now it might be the time to revisit this issue of what happened when "grand unification," of SM interactions, should necessarily be a "theory of everything."

2. **Laurence Lurio**
   August 27, 2021

   How stringent a test of string theory is the non-detection of supersymmetry at the LHC? I thought that string theory would still be consistent if supersymmetry were found at higher energies, its just that this wouldn't solve the hierarchy problem.

3. **Peter Woit**
   August 27, 2021

   Laurence Lurio,
   The accurate statement is that string theory predicts nothing about anything, including nothing about whether the LHC would see superpartners. Unfortunately for string theorists, pre-LHC this is not what they were saying publicly. For some more details about this, see for instance this blog posting
   https://www.math.columbia.edu/~woit/wordpress/?p=3864
   written in 2011 when the LHC results had started to come in and string theorists had started back-pedaling.

   For another example, from a 2006 highly negative review of my book in the LA Times:

   "As for Woit's claim that string theory has "absolutely zero connection with experiment," experiments already planned for a new European particle accelerator will look for the existence of extra dimensions and extra families of particles — both predicted by string theory."

   So, pre-LHC, string theorists were happy to mislead the public with "string theory predicts superpartners at the LHC" when confronted with the lack of predictivity of the theory, and this has led to stories like the Economist's. What's really remarkable, and recurs throughout the interviews, is that they are now perfectly fine with the idea that the theory they work on can't predict anything, while at the same time claiming that a theory in this situation is much better than any other theoretical approach.

4. **DrDave**
   August 27, 2021

   " When informed I taught math and did physics, one of them recommended Carlo Rovelli's new book to me, and said he hoped I wasn't doing string theory. Luckily I could reassure him about that."
   First smile of a long day.

5. **Topologist Guy**
   August 27, 2021

   Peter,

   I've always found the claim that "string theory predicts that the LHC will find extra dimensions" to be highly misleading, more so than the claim that superpartners will be discovered at the LHC. The "prediction" that extra dimensions will be accessible at ~ 10 TeV energy scales came not from "string theory" per se, but rather from some very specific model building that used ingredients from string/M-theory (for example, the Randall-Sundrum model https://arxiv.org/pdf/hep-ph/9905221.pdf). There is no sense in which a fundamental string theory calculation "predicted" the Randall-Sundrum braneworld scenario.

   Jackiw-Teitelboim,

   I agree that there are avenues in fundamental physics research other than quantum gravity. As a mathematician, I have noticed a deep difference in the attitudes towards quantum field theory by mathematicians and high-energy theorists. Most high-energy theorists view the four-dimensional QFTs of the standard model as entirely settled science. As a mathematician, however, I feel like great progress could be made in making the quantization of four-dimensional field theories rigorous. While theorists dismiss the question of mathematical rigour, it seems clear to me that a rigorous understanding of 4D interacting field quantization would illuminate the complex mathematical structure of these QFTs. The problem of "Yang-Mills and mass gap" appeals to me, and I think there are great opportunities available in more deeply understanding the mathematical structure of the standard model QFTs, with a distant goal possibly being the quantization of Yang-Mills by cohomological pushforward or an appropriate generalization thereof.

6. **Peter Woit**
   August 27, 2021

   Topologist Guy,

   When people tried to use "the LHC might see extra dimensions or produce black holes" as a "prediction of string theory" I always thought this was so ridiculous I didn't even know how to argue with it. Just about no one ever really took that seriously.

For mathematicians it’s important to point out that most of the QFT they’ve been looking at is TQFT. Amazingly that leads to dramatic topological results in 4d, but the underlying QFTs (supersymmetric Yang-Mills with twisted differentials) used to build TQFTs like the one that gives Donaldson invariants have all sorts of different and interesting mathematical structure. The fundamental confinement/mass gap problem is just part of it. The standard model QFT is a chiral gauge theory, and these have additional poorly understood problems that go much beyond the problems of phi^4 or pure Yang-Mills. I’ve spent most of my career frustrated by the extent to which theorists have lost any interest in those problems in favor of moving into extremely complicated and ill-defined speculation about string/brane theories in high dimensions.

In what I’ve been working on recently I’ve been struck by how little is really understood about the relation between 4d Minkowski and Euclidean signature theories. For quantum gravity as far as I can tell this remains a complete mystery.

7. **Gavin**
August 28, 2021

It seems to me that loop quantum gravity (or any other approach to quantum gravity) is really no better than string theory in terms of producing useful predictions that can be tested. While LQG predicts violations of Lorentz invariance, given there isn’t a clear scale at which this would happen, this kind of prediction seems on part with saying “supersymmetry at some scale that may or may not be in reach of experiments” is a prediction of string theory. Furthermore, as I understand, it is currently not known whether GR and quantum field theory emerge from loop quantum gravity in an appropriate semiclassical limit.

I would have thought this blog would advocate for theoretical physicists spending more time understanding the mathematics behind established theories, than expressing a preference for one speculative approach over another.

8. **Peter Woit**
August 28, 2021

Gavin,
If you actually read my posting I think you’ll see nothing promoting dubious claims about predictivity of one speculative approach over another. What I am pointing to is that the unprofessional “We rule, the other side is hopeless BS” behavior is mainly coming from one side, at least in the interviews in this book.

9. **Peter Shor**
August 28, 2021

Gavin,

Does loop quantum gravity really predict violations of Lorentz invariance, or is this the same kind of non-rigorous hand-waving that theoretical physicists are prone to? I don’t see any reason why a theory that’s constructed from non-Lorentz invariant elements couldn’t give entirely Lorentz-invariant predictions.

10. **Peter Woit**
August 28, 2021

Peter Shor,
There have been various claims about this made in the past, and from what I remember, they’re discussed in some of the interviews in the book. I don’t think there’s any point in reviving arguments over those. From reading the many interviews in this book, the current situation about “predictions” is actually much more reasonable on all sides than it was in the past: neither LQG nor string theory people are any more claiming such things.

11. **Shantanu**
August 30, 2021

Peter : doesn’t string theory and also certain BSM models such as Kostelecky’s SME extension also allow for violation of Lorentz invariance?

12. **Peter Woit**
August 30, 2021

Shantanu,
The situation with string theory is always the same. Since you don’t really know what the theory is, it is consistent with just about everything, and predicts nothing. That string theory doesn’t predict anything used to be a claim that would generate some argument, but reading through the interviews in this book, it now seems to be the consensus even among string theorists.

13. **Robert A. Wilson**
August 30, 2021

Peter,
I agree with you that the relation between Euclidean and Minkowski signature is of fundamental importance for quantum gravity, and, like you, I cannot understand why people do not pay more attention to this issue. Actually understanding what the signatures mean, rather than complexifying them out of existence, is surely one of the most important issues here.

14. **Alex**
August 30, 2021

“… One sign is that where critics have had interesting ideas (non-commutative geometry, black hole entropy, twistor theory) they have tended to be absorbed as part of string theory…”

I wonder what Connes and Penrose think about that (actually, I know: they think it’s a BS claim).

“…Loop quantum gravity is total BS. I mean, it’s really not worth discussing it. Don’t put that in the book. But, it really isn’t…”

Just wow. Is this the proposed level of the debate for this Nobel prize winner? And it seems he even doesn’t have the guts to say it in public, I think it’s good that they did put it in the book. Now we know who is who.

Evidently, confrontation didn’t work with these people. So, I think it’s time to just ignore and alienate them. They did good work, they enjoyed the deserved praise, but it’s time to move on, they are doing damage to the field.

Now, if you ask me what to do in relation to research in QG, it’s really an interesting moment. A lot has been investigated, many theories have been developed, we did learn some things. But, also, all of this didn’t live to its many promises. I think it’s time to step back, make a deep breath, open the window. But not for more new ideas and theories. This method of proposing theories because of mere aesthetics, or because they make some supposedly falsifiable predictions is not good.

I think that, rather than new theories, it would be better to do research about the deep conceptual problems posed by the very notion of quantizing gravity, or to investigate phenomenological models based on the basic qualities that a QG theory must supposedly have (say, superposition of geometries, discretization of geometry, etc) in order to see if we can get some input from actual experiments. And, if we must do theory, to do it closely to the methods, principles and notions from the actual theories that we know they work (GR and QFT), and not start throwing everything in the kitchen sink (as in string theory, ending in that pathological quote from Witten). And I know that even these basic proposals are debatable, but at least they try to mark some criterion before launching to speculate.

15. **Peter Woit**
August 30, 2021

Alex,
I just realized that there's a pattern here: Vafa is Witten's student and Witten is Gross's student. Vafa is 61, Witten 70, and Gross 80. I suspect it would not be hard to find 50 year old students of Vafa, their 40 year old students, and so on, all of whom would, at least in private, argue that string theory rules/LQG sux. To a large degree, the whole subject of quantum gravity has sunk into a depressing anti-scientific tribalism, with the string theory tribe now rallying around ill-defined ideas that somehow entanglement/quantum information theory/machine learning etc. are the way forward. You can try and ignore this tribe and its behavior, but it remains the dominant one, at least in US academia.

My current weird relationship to quantum gravity is that, after a long career of mostly ignoring it since I thought the way forward was likely to be to find a unification with the parts of fundamental theory we do understand (the Standard Model) and I didn't have a good idea about that. Recently though I've become more and more convinced that the ideas about twistors and unification I've been thinking about do say something about quantum gravity, and I'm trying to write up a revised version of my paper that emphasizes that. Hope to have that done soon, doubt that it will attract any interest from the string theory tribe, but perhaps another tribe will see things differently.

16. **DKepler**
August 31, 2021

I agree with Alex.

A Nobel laureate using such a language in response to a request for a scientific opinion is simply disgusting, and indicates the insecurity String theorists are facing. Talking of unscientific biases, when entropic gravity was trending, I recall someone asking a string theorist, a Nobel laureate, about his opinion on related emergent gravity works (Jacobson, Padmanabhan, and others), and his response was he is more in favor of what Verlinde does because <>. Guess who this laureate was?

17. **Dimitris Papadimitriou**
August 31, 2021

Nothing in theoretical physics can goes on for long (and remain healthy) without, at least, some hints of experimental evidence.
In the case of QG, there is almost nothing known with confidence (with the exception of some strongly negative results about potential Lorentz- Invariance violations from astrophysical observations). A minimum of observational evidence is necessary for people to have some solid background for their QGT candidates, otherwise we'll have again and again the usual (mainly among string theorists, but not exclusively) situation:
Guesses and Conjectures built upon other Conjectures without an end, as is the case with Holography and the attempted "solutions" of the black hole information problem, where String people tried to cure an inconsistency with rather far fetched hypotheses (based on the pompously dubbed "Central Dogma"), that quickly led to even more serious inconsistencies.
They, eventually, throw out of the window almost all established physics (QFT, locality/Causality, the Equivalence Principle- is there anything left?), and these are not mild or subtle violations…
Ok, I don't mean that everything we know must remain intact, but as the situation is now, anyone can hypothesize anything without any consequences.

18. **Peter Woit**
August 31, 2021

DKepler,
I just tried re-reading the Verlinde interview in the book, it's quite remarkable. It seems that he believes there is some more fundamental theory underlying string theory
"I really think that you can try to explain string theory as an emergent framework from something else underlying it."
"In general, I think that string theory is a cleverer idea and better motivated than any of these other ideas, which I think are just guesses. When I wrote my paper [26] people sent me emails saying that they knew what the microscopic model for what I was advocating was. I didn't believe it and I didn't even have to look at it because the possibility that we just know what it is like is really small. The probability is just one in a googolplex, so I don't think we have any chance of finding it. I think we can find general principles but not the microscopic model. Also, I think that LQG is too contrived and it doesn't explain anything."
So, he has given up not only on previous hopes for some connection to experiment, but also on any hope of actually having a well-defined fundamental theory. Elsewhere he argues string theory is too beautiful, truth is something unknown and uglier.
Honestly, none of this makes any sense to me, and the Economist take that this is now our leading theory is disturbing.

On top of this, there's
"I think that people who follow other approaches should learn more about string theory as there is no other competing formulation. There's no comparison between string theory and other formulations."
Having no well-defined theory coupled with this kind of critical attitude towards others who are trying to work with well-defined theories is kind of breathtaking.

19. **Nogo**
August 31, 2021

I really don't understand the "human story" behind all this.
Why talented, respected people with real accomplishments act in such a way as to attract disrespect and even ridicule? Gordon Kane with his predictions (and now postdictions?) of SuSy discovery, string theorists just redefining "arrogance", Mochizuki with his treatment of critics… I don't get it.

20. **Peter Woit**
September 1, 2021

Nogo,
I think the "human story" is that often very smart people are not good at realizing they were wrong about something and admitting it. Perhaps being smart even makes things worse: you lack experience at being wrong.

21. **Dimitris Papadimitriou**
September 1, 2021

Peter Woit, Nogo,
Maybe it's something like a "Majority Syndrome": If someone is a leading personality of a research field that is popular and risk-free (in the sense that it's not gonna be falsified any time soon), and also has a "natural" confidence that is quite common among smart and relatively famous people, maybe he/she isn't immune to this

exaggerated kind of arrogance.

There are many other smart theorists, not only in various QG research programs, but also mathematical physisists, GR experts, and others that work in equally important areas that don't have (not even closely) such type of behaviour.
They're just doing their hard work without much publicity and far out claims.
Needless to say: If the situation was reversed, and these other research areas were more popular (and overhyped), maybe they would exhibit the same kind of unrestrained arrogance.

In my opinion, only a few string theorists have such a behaviour, though.
There are many young people that doing their thing, and avoid exaggerated claims, even if they, sometimes, adopt some pompous phrases (like the "Central Dogma" and the like) that others have invent, to make things sound more important that they really are.

22. **Peter Woit**
September 1, 2021

Dimitris Papadimitriou,
While most string theorists don't behave like this, in reading the 20 or so string theorist interviews it was disturbing how many of them do, and the overall attitude of not acknowledging failures.
String theory is a very unusual subject though, since it has gone from having a very positive public perception to a much more skeptical and negative one. Some of the bad behavior surely has to do with a feeling that string theory is under unfair attack, so the thing to do is fight back, fairness be damned.

23. **DKepler**
September 2, 2021

Thanks Peter for those excerpts from Verlinde's interview. There is a great irony here which perhaps also applies on a broader level to the entire String community. The key ideas on which Verlinde based his work came from Jacobson (and later work by Padmanabhan), and Ted's earlier research was in topics closer to canonical gravity, LQG etc. which are being called "BS" by Gross. There seems to be some kind of a zealot like attitude towards absorbing anything not coming from string theory as part of string theory and then continue abusing other ideas. LQG may be useless as a physical theory, just like string theory. How can we say that the mathematical tools it has introduced can not be useful, while using the same argument to favor string theory?

24. **Andrew Thomas**
September 2, 2021

I'm no fan of string theory, but regarding the David Gross quote, if you give an interview to a journalist "off the record" then the rules of confidentiality apply and should be respected by the journalist. That then gives the interviewee freedom to speak openly and say things he or she would not normally say, which might include highly-inflammatory opinions. If that off-the-record protocol is not respected by a journalist then that freedom is lost.

25. **Peter Woit**
September 2, 2021

Andrew Thomas,
This clearly was an on the record interview. One can argue that after saying something on the record, Gross changed his mind and asked that it be off the record. All of the interviews were edited afterwards with the collaboration of the interviewee. Armas explains
"The interviewees had the opportunity to review and modify the edited transcripts as they pleased which, in many cases, contained additional questions that I added a posteriori over the years and to which the interviewees provided answers. Some of the interviewees spent a considerable amount of time reviewing and editing their transcripts, while some performed only minimal edits or none. Given the long time span between the date of some of the interviews and the publishing date of this book, some interviewees decided to update part of their answers to better reflect their current understanding of the topics discussed, while others decided to keep their original answers."
From this I think it's pretty clear that Gross approved leaving the "BS" statement in.

26. **Anonymous**
September 2, 2021

I know this topic is getting a little old but Gross, Witten, and a number of other string theorists were asked again about LQG at the recent strings conference in this 3 min video: https://www.youtube.com/watch?v=Wh9jqAUavNo

Anyway, I don't think it's fair for people to get too upset about the "BS" comment. Scientists are still people and most work on things where there are competing explanations and ideas. There will be deriding comments occurring occasionally in group meeting and in private seminars everywhere.

27. **Peter Woit**
September 2, 2021

Anonymous,
Sure, in private scientists will often refer to competing research programs as "BS". It's not though a usual thing to do for publication or in an outreach program for the public (that's where that video came from). The public perception of string theory has become very negative (for good reason), and I think this has led string theorists to feel embattled and justified in lashing out at others in this unusual way.

The substantive critique in those video comments is that LQG is basically an attempt to quantize gravity emulating the success with the Standard Model as a quantization of connections (using for gravity the chiral spin connection and vierbein Ashtekar variables). All those commenting in the video make much the same claim, that they believe the reason you run into technical problems with this is that it is "not radical enough". The problem though is that since no one knows what "string theory" is right now, they don't have a well-defined proposal for what fundamental objects should replace connections/vierbeins. For a while this was supersymmetric versions of loops in 10 dimensions, now it's maybe a quantum error correcting code, hard to tell.

It may very well be that connections/vierbeins are the right variables, we just haven't found the right way of quantizing the theory. Gross and Veneziano make the analogy as the Fermi theory vs. Glashow-Weinberg-Salam. Maybe it's more accurate that the LQG starting point is like trying to describe the weak interactions using massive gauge fields. This has renormalizability problems, but is almost right, you just need to add the Higgs mechanism.

28. **Justin Glick**
September 3, 2021

Peter,

Regarding your last comment, are you suggesting that your latest thinking about twistors may be incorporated into LQG, perhaps providing key insights leading to a consistent theory?

29. **Shantanu**
September 3, 2021

Peter, there are other faculty at UCSB who have worked on LQG (and non-string approaches). Wonder what Gross thinks about this ? Does anyone have any data on funding in string theory vs LQG?
Some univs like Penn State and LSU have invested heavily in LQG. Someone should have asked this question how/why they did this.

30. **Nogo**
September 3, 2021

I guess "not radical enough" is in the eye of the beholder – I remember Lee Smolin arguing in his book that String theory was not radical enough, because it was not background-independent…

31. **Peter Woit**
September 3, 2021

Justin Glick,
More details to come soon in a revised version of the paper, but one idea in the older version is that gravity is a gauge theory of one of the SU(2) factors of Spin(4), which is much the same as saying you should do GR in terms of Ashtekar variables. This is the historical starting point of the LQG program. The context I'm giving for this has new features (the other SU(2) is an internal symmetry, imaginary time direction is the Higgs), perhaps these will lead to some different possibilities to pursue, we'll see.

32. **Peter Woit**
September 3, 2021

Shantanu,
The conflict over resources between LQG and string theory has been going on for over 30 years and is a long, complicated and evolving story. The unusually aggressive attacks on LQG you sometimes see from string theorists I think have something to do with that conflict.

33. **Peter Woit**
September 3, 2021

Nogo,
The funny thing is that the conventional wisdom in "string theory" these days is that Smolin was right, that string theory (a theory of strings) is not radical enough. Of course what people have done to avoid acknowledging this is to redefine the words "string theory" (nowadays, pretty much all string theorists agree that they don't know what it is, but it's not a theory of strings).

34. **off topic**
September 8, 2021

Lawrence Krauss had a long interview with Gross in July that mostly covered Gross' life in physics. At the end, Gross used the recent black hole information paradox work as an example of string theory's utility. Krauss was skeptical and said that he was not optimistic but Gross pushed back pretty strongly saying that the burden of proof was now on those that think there is a paradox. Nothing much else was said about string theory.

One funny fact that is only slightly relevant because Witten and Gross have been mentioned here was that Gross wrote a one line recommendation letter for Witten's application to the Harvard Society of Fellows: "he's smarter than me and probably smarter than you so accept him".

35. **anon**
September 10, 2021

Matt von Hippel has a blog post on the Economist article:

https://4gravitons.com/2021/09/10/stop-listing-the-amplituhedron-as-a-competitor-of-string-theory/

36. **DV**
September 13, 2021

It is indeed quite breathtaking to realise the level of contempt the string theory stalwarts have for loop quantum gravity. Especially when one considers the fact that not one of them has actually they tried to sit down and understand what LQG actually is. If they did, they would realise that it is in fact the most conservative possible approach towards quantising gravity, which does not require any extra dimensions or symmetries, and which actually succeeds (for the most part) in its original goal.

Further, re Gavin, Lorentz invariance is perfectly obeyed by LQG, unlike the situation in string theory where whether or not Lorentz invariance is broken is something that is almost dependent on the choices the theorist makes. Moreover general relativity does emerge as the long wavelength, "low energy" limit of LQG. The question of QFT is still somewhat more open but rapid progress is being made towards answering it.

It is exhausting advocating for LQG for more than 10 years now in the face of what seems like an iron curtain of willful ignorance which string theorists have put up. I take some consolation in Planck's words:

"A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

37. **Peter Woit**
September 13, 2021

DV,
I think there are string theorists who have some understanding of LQG, and the fact that it's a conservative approach to quantizing gravity is exactly what they don't like about it (if you watch the youtube video someone linked to, they are very explicit about this).

What's happened with string theory over the years is that it has evolved into ever more radical, speculative and ill-defined formulations. Starting with the quantized string, then branes, then an unknown 11 dimensional "Master/Mother/Membrane" theory, nowadays it's "It from Qubit", with no one having any idea how the qubits are going to be put together to get "It" (or even what "It" is). The people who have been doing this for nearly forty years (as well as the younger ones they have trained, no the field is reproducing, can't die off) find it inconceivable that this might all be a mistake.

38. **anon2**
September 14, 2021

The field is reproducing, to some extent, but it seems to me that the number of students interested in string theory or high-energy physics in general has dropped significantly during the last 10-15 years.

39. **Suomynona**
September 14, 2021

DV:
Last I checked it *wasn't* possible to recover GR as a low energy limit of LQG. The existence of something which can be identified with a massless spin-2 excitation is, I thought, still an open question. Do you have a reference?

It's also not obvious why SU(2) should be the gauge group of quantum gravity.

40. **Peter Woit**
September 14, 2021

All,
This really isn't the place for this kind of technical argument over LQG, which, honestly, gets tedious real fast when most people involved are trying to score points, not clarify things. The interviews in this book with people who have worked on LQG, are working on LQG, or various ideas that have grown out of it contain a lot of detailed arguments of this kind, with references. If you want to understand things, those are great sources.

About SU(2) and quantum gravity, the answer is independent of LQG, going back to the story of Ashtekar variables, explained a bit in the latest version of my paper on unification using twistors and Euclidean QFT. It's simplest in Euclidean signature, where the space-time rotation group is Spin(4)=SU(2)xSU(2). The remarkable fact is that in 4d you can write down the condition for the curvature of a connection to solve Einstein's equations just using one Lie (SU(2)) component of the spin connection, so you have an SU(2) gauge theory (you also have vierbeins though, which are a different story).

Minkowski space-time makes this more problematic, since there Spin(3,1)=SL(2,C).

41. **Juan Cristobal Latorre**
September 22, 2021

Hello Dr. Woit,
Could you please elaborate on your comment:
"Unfortunately for the public understanding of science, this is followed by […]"
What is the part of the following verbatim paragraph from The Economist article in your blog that you believe is unfortunate for the public?
This is not a facetious or cynical question. I've read your book "Not Even Wrong" (big fan, by the way), as well as similar perspectives such as Sabine Hossenfelder's material (also big fan). I'd like to understand therefore, what you think was perhaps misleading in The Economist's article.

Thank you!

42. **Peter Woit**
September 22, 2021

Juan Cristobal Latorre,

To elaborate, it's not an improvement if the physics community's idea of what the best way forward is moves from a failed non-trivial idea (string theory) to something content-free. The words "Entropic gravity" don't seem to correspond to any actual theory of anything. Sean Carroll at least explains what his ideas are, which makes it easy to see there's nothing non-trivial there.

This Week's Hype

September 8, 2021
Categories: This Week's Hype

Today Quanta has One Lab's Quest to Build Space-Time Out of Quantum Particles. No, this kind of experiment is not going to "Build Space-Time", now or ever. This kind of obfuscation about quantum gravity advances neither fundamental physics nor the public understanding of it, quite the opposite. The article does make clear what the motivation is: deal with the problem that

> String theory, still the leading candidate to replace the Standard Model, has often been accused of being untestable.

by claiming that it somehow can be tested in a lab.

Comments

1. **anonymous commentator**
   September 8, 2021

   To me, these types of articles about testing quantum gravity give off the impression that modern theoretical research into quantum gravity, AdS/CFT, and whatever else is coming out of the string theory community has more to do with to engineering toy constructions in the lab and pushing the limits of condensed matter and atomic physics than anything fundamental about the universe and high energy physics.

2. **Sabine**
   September 9, 2021

   We'll see lots more of this because there's so much money now being thrown at quantum simulations in the quantum technology packet. Doesn't matter if it makes any sense. This entire funding process is so broken.

3. **Peter Shor**
   September 9, 2021

   About the motivation for this paper …

   The people building quantum computers today are very much doing cutting-edge research. But right now, quantum computers are too small to solve any real problems, so people are looking around for neat demonstrations they can do.

   This "simulation" of quantum gravity is almost certainly worthless in terms of what it actually tells us about quantum gravity, but if you just have 18 qubits, you can't do any other calculations that would be worthwhile, either. And this "simulation" certainly seems to have been useful in terms of publicity value.

   We'll see whether quantum computer experimentalists keep on doing these "simulations" once they have a few thousand qubits and can do useful calculations.

4. **Peter Woit**
   September 9, 2021

   anonymous commentator,
   Given that David Gross and other leading string theorists think that it's "BS" to attempt to quantize gravity by trying to find a consistent quantum theory of the connection degrees of freedom that characterize geometry, one wonders what they think about claims that the way to quantize gravity is to study systems of trapped atoms in an atomic physics lab.

   I normally avoid attributing to scientists motives based on money rather than honest belief about the best way forward, but it certainly seems that the replacement of conventional HEP physics funding with large amounts of funding for "Quantum Information Science" is having an effect.

5. **Peter Shor**
   September 9, 2021

   I don't believe that funding is the reason that string theorists are now approaching their research from a quantum information viewpoint — at least, it's not the only reason. This may also be due to the AMPS (Almheiri, Marolf, Polchinski, and Sully) paper, where quantum information theory showed that Susskind's theory of black hole complementarity was probably incorrect. This was the first real result in years in the black hole information field, and researchers there started looking for more ways to apply quantum information to their research.

   And indeed, they are finding more and more ways to apply quantum information to this field, although I suspect that not all of these ways are justified.

6. **Matthew Foster**
   September 9, 2021

   I'll defend the intersection of "quantum gravity" research and stat mech/cond mat, but mainly insofar as it aids the latter solve important, intractable problems.

   One of my favorites (quite old now) is the Knizhnik, Polyakov, and Zamolodchikov (KPZ) formula for computing scaling dimensions of operators in 2D classical stat mech models at criticality (CFTs), defined on random manifolds. The "annealed sum" over all possible geometries shifts scaling dimensions in a simple way. Why is it useful? Well it turns out to be easier to calculate certain critical exponents of _logarithmic_ CFTs that describe geometric critical phenomena, e.g. 2D classical percolation, in the random case–using matrix methods. Then you can use KPZ _backwards_ to get exponents on the flat plane. This was pioneered by Duplantier et al. Annoyingly, log-CFTs (which are for the most part, still poorly understood) are the most important for applications to many interesting quantum phenomena in condensed matter, such as the plateau transition of the quantum Hall effect.

   From a condensed matter POV, though, summing over ALL geometries is rarely directly useful like this. There are, however, some important problems where a sum over _weakly_ random geometries can be important. This seems to be the case for 2D massless Dirac carriers in 2D quantum materials (cuprates, graphene, surface states of topological phases), where at least in some contexts it is natural for disorder to modulate the "speed of light" for the carriers. We are hoping there might be a way forward using some recent developments in 2D quantum gravity. In fact, this is one motivation for an Aspen workshop we are going to hold on "Random geometry" in Fall 2022, covid willing…

7. **Peter Woit**
September 9, 2021

Matthew Foster,
There's no question that issues like the ones you mention involve very non-trivial theory with a long history, and in that theory, connections to quantum gravity (especially in lower dimensions) are important.
The Quanta article though is about something completely different, experimental atomic physics results with zero connection to quantum gravity. There's now a very long history of attempts to confuse everyone about this, very sad to see that Quanta is doing this, something that deserved mention here.

I've wasted too much time in the past explaining the obvious points that the people doing this are trying to obscure (to these extent these experiments test anything, it's whether some approximation scheme works in certain calculations in standard atomic QM, nothing to do with quantizing gravity).

8. **Blake Stacey**
September 9, 2021

It has always seemed to me that the "It from Qubit" program was more likely to lead to a geometric description of some kinds of correlated multipartite states than to a theory of quantum gravity. In other words, the hype gets the arrow pointing in the wrong direction.

9. **Matthew Foster**
September 9, 2021

Peter,

I'll only say that SYK has a very similar flavor, in that
(1) on one hand, it's a very simple zero-dimensional "quantum mechanics" problem of randomly interacting Majorana fermions, or equivalently, an SO(n) bilinear "magnet" with random, fully broken SO(n) symmetry.
On the other hand, it
(2) exhibits emergent conformal invariance in 1+0-D, which maps to Schwarzian/Liouville quantum mechanics. The physics of the model is encoded in the sum over diffeomorphisms of the circle. While there is no true geometry here, it is a toy version of Liouville CFT. Moreover, at long times for a finite number of N Majoranas, there is a random matrix description that has a holographic dual in terms of JT gravity.

So realizing SYK in the lab would in principle allow one to experimentally test all of these connections. It's not "true" quantum gravity, but it's a first step. The problems I mentioned in my post are likely much harder, because they do involve 2D Liouville or other geometric (logarithmic) CFTs, and these are still poorly understood. But there has been some more recent cross-fertilization with cond mat and stat mech that might shed some further light, for example the interpretation of 2D Liouville as a log-correlated random energy model. This is a relatively simple toy model for glassy physics, which exhibits a "freezing transition" below a critical temperature. The freezing transition is related to violations of the so-called Seiberg bound in Liouville, and might have something to do with "macroscopic" operators that punch holes into the "worldsheet" of the theory.

Most of the text of the Quanta article appears pretty honest about what is being attempted, and what has been accomplished. I take it your main objection is to the headline, and the string theory preamble?

10. **Robert C Arnold**
September 10, 2021

It must be the season… Here is yet another claim re string theory's supposed testability: https://phys.org/news/2021-09-s-matrix-bootstrap-theory-quantum-gravity.html?utm_source=nwletter&utm_medium=email&utm_campaign=daily-nwletter

11. **Shantanu**
September 10, 2021

Peter : one technical problem is that after such articles they don't provide the email address of author or a place for readers to comment or provide feedback.
I wish they did so.

12. **Peter Woit**
September 10, 2021

Shantanu,
Quanta does have a comment section on the article, but their comment sections are unmoderated and thus useless (overrun by cranks, people who have no idea what they are talking about). Phys.org mostly just republishes press releases. Possibly this story has its origins in a press release somewhere. Physical Review Letters encourages people to issue press releases when a paper is published, it looks like this is an example.

By the way, looking at the PRL article, this is about the superstring in 10d, no mention of the main problem of string theory (how do you get 4d?). From the results of the authors I don't see anything like an argument that "string theory is the unique theory of quantum gravity", even in 10d. Again, lots of hype for a result that doesn't do much of what is advertised. This has been the story of string theory in the media forever.

13. **Peter Woit**
September 10, 2021

Matthew Foster,
The headline and string theory framing are completely outrageous. The rest of the article is fine if you strip out all mention of quantum gravity. It's this that's just completely misleading.

As for SYK, again you're bringing in a complicated and interesting theoretical topic, but one that has nothing to do with this article.

14. **Ian McCormack**
September 10, 2021

This reminds me of this paragraph from Sophie Rilson's paper that Peter Woit linked in his previous blog post:

'Quantum field theorist Matt Strassler and Woit had an, at times, furious debate about the relationship between string theory as a tool and as a theory of quantum gravity (see comments on (Strassler, 2013a)). Woit accused Strassler of misleading the public by claiming that progress coming from the use of string theory as a tool is indicative of progress in string theory as a theory of quantum gravity (which Woit calls string unification). For Woit, the applications of string theory are tests of an "approximation scheme" as opposed to tests of a theory (Woit, comment on Strassler, 2013a).'

15. **Peter Woit**
September 10, 2021

Ian McCormack,
That was a bit different. There the argument was over something relatively well-defined: AdS/CFT relating 4d and 5d and whether it's a good approximation to QCD. At the time there was a lot of hype about "tests of string theory" that really were tests of this approximation.

In this case, instead of a specific 4d/5d AdS/CFT duality conjecture as a tool, people are talking about all sorts of very different things in toy models in low dimensions, with basically zero relation to string theory or gravity in 4d (note that "gravity" in 1d, 2d, 3d is something of a different nature, since there are no physical gravitational degrees of freedom in those dimensions).

16. **Matthew Foster**
September 12, 2021

Hi Peter,

No vacuum gravitational waves in 2+1-D, yes. But dynamics are nontrivial with source matter fields. That doesn't qualify as physical DOF? Einstein tensor is nonzero there.

17. **Peter Woit**
September 13, 2021

Matthew Foster,
I don't think source matter fields change the basic situation: the real problems of quantum gravity aren't there. In 3+1 d the physical state space is infinite dimensional, with an infinity of gravitational degrees of freedom interacting non-linearly with renormalizability problems. In lower dimensions the degrees of freedom are pure gauge (except for maybe a finite number), which is very different.

Euclidean Twistor Unification, version 2.0

September 10, 2021
Categories: Euclidean Twistor Unification

I've completely re-organized and largely rewritten my paper from earlier this year on [Euclidean Spinors and Twistor Unification](). Soon I'll upload this as a revision to the arXiv, for now it's available [here](). This new version starts from a very basic point of view about 4d geometry, leaving the technicalities about Euclidean QFT for spinors and the expository material about twistors to appendices.

Most ideas I've worked on over the years that seemed initially promising ultimately became more and more problematic the more I looked at them. This set of ideas keeps looking more and more solid. There are several (to me at least…) attractive aspects:

- Spinors are tautological objects (a point in space-time is a space of Weyl spinors), rather than complicated objects that must be separately introduced in the usual geometrical formalism.
- Analytic continuation between Minkowski and Euclidean space-time can be naturally performed, since twistor geometry provides their joint complexification.
- Exactly the internal symmetries of the Standard Model occur.
- The intricate transformation properties of a generation of Standard Model fermions correspond to a simple construction.
- One gets a new chiral formulation of gravity, unified with the Standard Model.
- Conformal symmetry is built into the picture in a fundamental way.

There's more in this version about how quantum gravity fits into this, when formulated in terms of chiral variables (i.e. Ashtekar variables). This gives a new context for old questions about quantizing in these variables (this is in Eucldean signature, the other chirality is not space-time geometry but internal Yang-Mills geometry, and the imaginary time component of the vierbein is distinguished and given the dynamics of a Higgs field). I haven't spent much time on this yet, but suspect this new context may help overcome problems that people trying to pursue quantum gravity in this chiral connection framework have run into in the past.

One common reaction I've gotten to these ideas is the one I myself had in the past: analytic continuation relates expectation values of field operators in Euclidean and Minkowski signature, so my left-handed SU(2) after analytic continuation gives part of Lorentz symmetry, not an internal symmetry. What took me a long time to realize is just how different Euclidean and Minkowski signature QFT is. Yes, Schwinger functions and Wightman functions can be related by analytic continuation (in a rather subtle way, the Wightman functions aren't functions, but boundary values of holomorphic functions). But at the level of states and operators things are very different. It's just not true that there is some holomorphic formulation of QFT states and operators, with Euclidean and Minkowski space restrictions related by analytic continuation. There's a lot of explanation about this in the paper.

One objection I've run into is that by distinguishing a direction in Euclidean space I'm breaking Lorentz symmetry. What's true is quite the opposite: having such a distinguished direction is needed to get Lorentz symmetry after analytic continuation. If you want to start in Euclidean space and get Lorentz symmetry, you have to do something like distinguish a direction and get an Osterwalder-Schrader reflection in that direction, which you need to get from SO(4) to SL(2,C). From the other direction, if you start in Minkowski space-time and analytically continue, you have a choice of lots of possible Euclidean slices to analytically continue to. You need to pick one, and that will distinguish an imaginary time direction. This is most easily seen in the twistor formalism, where the Minkowski space-time geometry is determined by a quadratic form that picks out a 5-dimensional hypersurface in PT. This will project down to an imaginary time = 0 subspace of Euclidean space-time, which picks out the imaginary time direction.

Comments

1. **Thomas**
   September 10, 2021

   This looks like real progress, Here's hoping your theory is well-received. 2 questions:
   Does point 3 imply that you get the 3 generations of particles of the SM ?
   Does Euclidean Twistor Unification make any specific prediction at this stage ?

2. **Peter Woit**
   September 10, 2021

   Thomas,
   No, this doesn't explain why multiple generations, in particular why 3 generations.

   What I find compelling about this is that it provides a remarkable geometric framework that has exactly the local symmetries of GR and the standard model. But it involves formulating the theory on twistor space rather than spacetime, which I don't understand quite how to do correctly. Perhaps if that can be done, one would have a well-defined theory more constrained than the standard model, so able to make new predictions.

3. **Bertie**
   September 11, 2021

   Good readability, one typo, last sentence before Section 6, 'recently' =recent

   Would love to know if Penrose is aware of this!

4. **D R Lunsford**
   September 11, 2021

   Excellent! Thanks! This is very good!

   -drl

5. **SteveB**
   September 11, 2021

   Towards the bottom of p4 the expression for the determinant should be:
   x_0^2 – x_1^2 + x_2^2 – x_3^2

6. **lun**
   September 11, 2021

Very interesting. From my very incomplete understanding, what you are saying is that if you set up the integration measure for some partition function not on 4d real space (Euclidean with a Minkowski analytical continuation) but in complex space, reparametrized using twistors, GR general covariance and gauge invariance under SU(3)XSU(2)XU(1) could follow from some version of invariance of the functional integral under field redefinitions.
Is this it or is this a misunderstanding?

7. **Peter Woit**
September 11, 2021

Bertie/SteveB,

Thanks, will fix the typos.

Haven't heard from Penrose, but he's been rather busy, I hear being a Nobelist takes a lot of time.

lun,
That's not really it.
One role for twistors is that they tautologically describe spinors on complexified space time. So they give an arena where you can do analytic continuation of spinor fields between Euclidean and Minkowski 4d real slices.

Unlike most attempts to use twistor theory in physics, I'm looking not at the Minkowski slice but at the Euclidean slice. There you have usual 4d Euclidean space time, and usual Euclidean QFT as a sort of stat mech, but also
1. A distinguished imaginary time direction (this determines how you are going to recover Lorentz symmetry and the Minkowski theory).
2. Euclidean twistor space, i.e. twistor theory, restricted to the Euclidean slice. This is a bundle over Euclidean space time, fiber a sphere, which you can interpret in various ways (projective spinors at the point, orthogonal complex structures at the point).

The claim is that these elements determine local SU(3)xU(1)xSU(2)xSU(2) gauge symmetry on twistor space. The first three factors can be thought of as internal symmetries, with the SU(2) spontaneously broken by the distinguished imaginary time direction. Gauge theory with the last SU(2) gives a chiral spin connection that is used in one formulation of GR (general covariant if you use the usual Palatini action in a chiral form). It may be that this setup gives some different possibilities for the action than just the usual term.

8. **Arnold Neumaier**
September 12, 2021

Peter,
"if you start in Minkowski space-time and analytically continue, you have a choice of lots of possible Euclidean slices to analytically continue to. You need to pick one, and that will distinguish an imaginary time direction. "

This means that there are many conjugate Euclidean formulations of the same Minkowski theory, breaking explicit Poincare covariance in each Euclidean formulation and restoring it only after the Osterwalder-Schrader reconstruction.

You also didn't mention that analytic continuation is not possible in most curved spacetimes – only in the quite restrictive class of spacetimes compatible with a complex structure.

In my view, both observations send the same message: The Euclidean formulation is only a tool, rather than a basis for QFT.

9. **Peter Woit**
September 12, 2021

Arnold Neumaier,
I agree that explicit SO(4) invariance is broken by having a Euclidean QFT + analytic continuation to Minkowski space-time. Note that it is not Poincare that is broken, it is just Euclidean rotations. My argument is that this is not a bug but a feature: it corresponds to the spontaneous symmetry breaking of the electroweak theory.

For one common objection about a non-existence problem of the analytic continuation, see the 2002 survey by Gibbons, where section 20.3.1 is titled "A non-problem", and starts with
"Let us comment on an often repeated objection. This is the mathematically correct but physically totally irrelevant statement that 'not ever Lorentzian spacetime M_L admits an analytic continuation M_C containing a Riemannian section M_R.' This is also true for example of Yang-Mills connections on Minkowski spacetime, they don't always analytically continue to give real connections on Euclidean space. The answer is the same in both cases. All that is needed is the behavior of quantities like Z(\Sigma,h) as a function of boundary values. Physically the individual interior spacetimes, Riemannian or Lorentzian, that go into the functional sum have no particular significance except in the classical limit, when one is preferred, or, more interestingly in the special case when both Lorentzian and Riemannian real sections exist simultaneously in the same complex spacetime M_C."

The special case of S^4 I discuss in the paper is an example of the last phenomenon Gibbons mentions.

10. **martibal**
September 12, 2021

Hi Peter, could you give the title of Gibbons survey ? He seems to have a lot of preprints in 2002, and it is not clear to me which one is relevant here.
Thanks !

11. **Peter Woit**
September 12, 2021

martibal,
The full reference is reference 18 in the latest version of the paper.

12. **Arnold Neumaier**
September 13, 2021

Peter,

"My argument is that this is not a bug but a feature: it corresponds to the spontaneous symmetry breaking of the electroweak theory."

But the latter has physical consequences whereas the former is a nonphysical artifact of the Euclidean view. No experimental result hints at a broken Poincare symmetry!

13. **Peter Woit**
September 13, 2021

Arnold Neumaier,

You keep talking about broken SL(2,C) invariance, but what I'm pointing out is that SO(4) invariance is not a symmetry of the states or operators of the theory. To get SL(2,C) invariance on states you have to have something like an Osterwalder-Schrader reflection, which breaks SO(4). This breaking of SO(4) does not break SL(2,C), it's required to get SL(2,C).

Electroweak symmetry breaking is a breaking of gauge degrees of freedom, so the SO(4) breaking will be of that nature.

14. **Arnold Neumaier**
September 13, 2021

To get SL(2,C) invariance on states you have to have something like an Osterwalder-Schrader reflection – yes, but my point is that what you get is a representation of the Poincare group with a distinguished time coordinate, which is unphysical!

15. **Peter Woit**
September 13, 2021

Starting with Euclidean theory you just can't get a Poincare group representation at all, unless you have a distinguished imaginary time direction in Euclidean space (to do OS reflection). For a general context see for example

https://www.jstor.org/stable/2006979

This particular construction is in coordinates and has a distinguished real-time coordinate, but since it's a Lorentz group representation, it's unitarily equivalent to any choice of real-time coordinate.

16. **Peter Orland**
September 13, 2021

Whether you can map fields on Lorentzian-signature manifolds and Euclidean-signature manifolds is irrelevant (there is such a bijective mapping, as far as I know, between Lorentzian manifolds and Euclidean manifolds with a vector field everywhere. But it does not matter).

The real issue is whether {\em amplitudes}, not fields on manifolds, can be continued. That's what OS positivity is for.

17. **Peter Orland**
September 13, 2021

Sorry, what I wrote needs an edit:

Whether you can map fields BETWEEN Lorentzian-signature manifolds and Euclidean-signature manifolds is irrelevant (there is such a bijective mapping, as far as I know, between Lorentzian manifolds and Euclidean manifolds with a FIDUCIAL vector field everywhere. But it does not matter).

The real issue is whether {\em amplitudes}, not fields on manifolds, can be continued. That's what OS positivity is for.

18. **Peter Woit**
September 14, 2021

My previous comment about "predictions" didn't satisfy some, lots of comments demanding "predictions" deleted. If one wanted to play the game played by string theorists over the years, one could claim for this set of ideas "predictions" of four dimensions, chiral spinor fields, U(1) x SU(2) x SU(3) gauge symmetries and matter fields with the right quantum numbers, as well as gravitation.

But that's a waste of time, the problem with string theory (or any speculative theory) has always been much more complex than "does it make "predictions"?", and I'll continue to delete comments that want to go on about this. The interesting question is whether there are new ideas here that will be fruitful and explain things about the Standard Model + GR we've previously had no explanation for. I think so, but if you don't, do something else. There's a lot I still don't understand about the implications of these ideas, maybe further effort will show they can't work, or maybe they'll lead to progress. Time will tell.

In any case, we're still a long ways from any danger of decades of fundamental research in theoretical physics going nowhere because of being dominated by these ideas…

19. **DKepler**
September 14, 2021

Peter, Peter, and Arnold: Concerning the issue of distinguished time coordinate emerging when going from Euclidean to Lorentzian, this (inevitable fact) is most clearly evident when working with the covariant formalism for Wick rotation (using a timelike direction field) which I believe is what Peter Orland was referring to; I have left references about this earlier on this blog. The key point is that the partition function (for gravity+matter) should now presumably also include the path integral over this timelike field. (Gibbons, on the other hand, does *not* talk about this additional path integral.) I do not see any reason why Lorentz invariance will necessarily be broken in all this. In fact, at the quantum level, having such a direction field might be a bonus since we know that description of quantum vacuum is observer dependent.

20. **Shantanu**
September 16, 2021

Peter: you should give an ILQG seminar at PSU or the quantum gravity seminar series at Perimeter or other places which run a seminar series.

21. **Peter Orland**
September 16, 2021

DK, Yes, I mentioned that vector field in my first version of my post, but edited it away, after deciding I was veering off topic.

22. **Peter Woit**
September 16, 2021

Shantanu,

I’m speaking at Brown next week. If other people are interested in hearing about this, I’ll be happy to talk elsewhere.

23. **Lowell Brown**
    September 16, 2021

    This level of math is way beyond me. But I am curious were the justification for the statement

    “since we know that description of quantum vacuum is observer dependent.”

    comes from.

24. **Kingsuk Maitra**
    September 22, 2021

    Dr. Woit,
    Would you be able to share the slides from your Brown talk in this blog?

25. **Peter Woit**
    September 22, 2021

    Kingsuk Maitra,
    Just finishing the slides this evening, will post here after the talk.

26. **Anonymous s**
    September 23, 2021

    does Euclidean Twistor Unification combine space-time and internal symmetries and does Coleman–Mandula theorem apply ?

27. **Peter Woit**
    September 24, 2021

    Anonymous,
    No, I’m not embedding both space-time and internal symmetries in a larger symmetry group and having that act on the states. That’s what Coleman-Mandula says you can’t do (because you’ll end up with a trivial theory).

    For me a lot of the appeal of this picture is exactly that it gives a sort of unification that does not involve embedding everything in a bigger grand unification group, that you then have to break somehow to get back to the SM and usual space-time symmetries.

Visit to Brown

September 24, 2021
Categories: Book Reviews, Euclidean Twistor Unification

I spent yesterday up in Providence, visiting the Theoretical Physics Center at Brown, and giving a talk there (slides are available here, newer version of a paper here), At some point a recording of the talk should appear online. In the talk I tried to emphasize some basic things which it took me a very long time to appreciate:

- The ways in which Euclidean QFT is very different than Minkowski space-time QFT, in particular the necessity of having a distinguished imaginary time vector, breaking SO(4) invariance, in order to recover Lorentz (SL(2,C)) invariance.
- The way in which Minkowski space-time shows up when you do twistor theory in Euclidean space-time (see the pictures in the slides). This again makes clear the way SO(4) invariance is broken.

While I'm making a proposal for how to get gravity out of chiral 4d geometry, I've never been that expert in GR, and GR is the focus of much of the theory community these days, in particular the theorists at Brown. So, they had lots of questions about what the implications of this are for GR that I couldn't answer. I'll keep thinking more about this and may some day start to have answers (or maybe GR experts will find this proposal interesting enough to figure out the answers themselves).

I was invited to give the talk by Stephon Alexander, and got to spend some time talking with him while in Providence. He has worked in the past (see here) on ideas that bring together the gravitational and weak interactions in a similar way. More recently he has been working on ideas for how one might observe an unexpected chiral component to the gravitational interactions, and now has a grant from the Simons Foundation that will fund work in this area. Next week he'll be here at Columbia giving an astronomy colloquium on the topic.

He also has a new book (his first was *The Jazz of Physics*) out, Fear of a Black Universe: an Outsider's Guide to the Future of Physics. It's quite interesting, with much of the earlier parts describing some of his experiences making a career for himself as a theorist, together with explanations of the physics background. The last part (in collaboration with Jaron Lanier) heads off in somewhat of a sci-fi direction, an excerpt is here.

A major theme of the book (with which I'm very sympathetic) is that the community doing this sort of theoretical physics desperately needs to get out of its current rut and open itself to new ideas, which often will come from "outsiders". One aspect of being an "outsider" that Alexander has experienced is difference in racial background, but he's concerned with a more general context of hostility to ideas that aren't those currently favored by "insiders". While he started out his career doing string theory, he has moved in different directions over the years. He explains that as a postdoc at SLAC he invited Lee Smolin to come and lecture on loop quantum gravity, something which was not at all well received by the local string theorists. While I'm quite interested for my own reasons to understand better what he has been doing with the physics of possible chiral effects in gravity, it was great to see his enthusiasm for and encouragement of ideas that don't fit exactly into the narrow conception of the subject that now dominates all too much of the community doing fundamental theoretical physics.

Update: There's video of the talk available here.

Comments

1. **Alessandro Strumia**
   September 25, 2021

   What about Yukawa couplings? Attempts to find simplicity behind the Higgs usually sink when dealing with the fact that the SM Higgs has Yukawa couplings with messy values.

2. **Peter Woit**
   September 25, 2021

   Alessandro Strumia,

   No idea how to compute Yukawa couplings. Part of that problem is not understanding where three generations come from.

3. **Douglas Natelson**
   September 25, 2021

   Whenever I see phrasing about a chiral component to gravity, I get flashbacks to noted physics Internet personality Uncle Al, and his claims that "someone should look" to see if left-handed chiral crystals and right-handed chiral crystals obey the equivalence principle.

4. **Jackiw–Teitelboim**
   September 28, 2021

   Have you contemplated the possibility that, from the viewpoint of a QFT formulated in the standard spacetime language, the action functional corresponding to the gravitational theory following from your proposal of symmetry breaking leading from Euclidean to Lorentzian signature, would include terms that might involve metric-affine geometry (Cartan connection), e.g., torsion, instead of the standard Levi-Civita connection? Or more generically a Ehresmann connection?

   Might be worth looking for a debate between Weinberg and Hehl on the merits of such geometries, if this turns out to be relevant to what you are up to.

5. **Peter Woit**
   September 29, 2021

   Jackiw-Teitelboim,

   There are various ways to extend GR formulated with a spin connections and the Palatini action:
   1. Allow torsion
   2. What I'd call Cartan geometry, interpreting the vierbeins as a Cartan connection. One version of this is Macdowell-Mansouri.
   3. Add R^2 terms, get a conformally invariant theory, I noticed there was a paper about this on the arxiv yesterday:
   https://arxiv.org/abs/2109.12743

   All of these have a long history, perhaps reformulating in the twistor Euclidean framework with the imaginary time direction vierbein getting the dynamics of the Higgs gives something new.

6. **Sri**
   October 5, 2021

   Hi Peter,

   Long time lurker here. This twistor gravi-weak unification is really awesome and the way in which the Higgs emerges due to Osterwalder – Schrader reflection is very very deep.

   Couple of basic questions (I dont know if you have addressed these already):

   1) Where do three generations come from?

   2) How does QCD fit into this framework?

   3) Do you have any comment on what objects of algebraic quantum field theory like the modular operator look like in this framework?

   Thanks,
   Sri

7. **Peter Woit**
   October 5, 2021

   Sri,
   Thanks for your comments about the unification and Higgs ideas. The more I think about these ideas the more convinced I become that something like this can work. Also, the more I look at the literature, the more convinced I become that these ideas are actually new.

   1. I don't understand the origin of three generations, understanding that is crucial. There are various possible ideas to look at that I haven't yet had time to pursue.

   2. The definition of projective twistor space naturally provides an internal SU(3) symmetry, one can try and gauge this, use the usual Yang-Mills action, get standard QCD. There is however something different going on: the SU(3) principal bundle on projective twistor space is not the pull-back of an SU(3) principal bundle on the base space-time. The SU(3) group varies as you move around the CP1 fiber. I need to better understand the significance of this, how and if it changes the usual QCD dynamics.

   3. I know precious little about algebraic qft.

Various Math and Physics News

October 4, 2021
Categories: Uncategorized

First some items on the mathematics side:

- The latest AMS Notices has some memorial pieces about [Vaughan Jones] and [Robert Hermann]. I contributed a piece to the Hermann memorial, for more about him, see [here].
- If you read French you might enjoy Yves André's *Dix regards sur la mathématique contemporaine*, freely available [here].
- There's a wonderful overview of various conjectures in number theory last year from Barry Mazur, [About Main Conjectures].
- The Harvard Math department seems to not have had a lot of luck with its funders. Last spring they had to [close] their [Program in Evolutionary Dynamics], which was funded by Jeffrey Epstein. The very active [Center of Mathematical Sciences and Applications] has been funded by the Evergrande Group, a real estate investment company that has now run into serious financial problems. I haven't heard what the implications will be for the CMSA in the future.

On the physics side:

- The 2021 Physics Nobel Prize will be announced tomorrow morning. I gave up predicting these things after [this prediction back in 2004].
- Gian Giudice has put on the arXiv a written version of his [Theory closing talk at LCHP2021]. He ends with

  > These are interesting times for particle physics: times of great uncertainty, in which our physics perspective is changing, and in which we are laying the foundations for the future of our field. As a community, we must rise to the challenge.

  What worries me is that the much of the rest of the article contains a lot of

  1. Arguing for multiverse pseudo-science:

     > The multiverse describes a physical reality that challenges the presumption that there must be a single unified theory in the deep UV. In a sense, it is the ultimate Copernican revolution since not even the patch of the universe we live in is special. It implies a revision of the cosmological principle because the universe is approximately homogeneous and isotropic only within our horizon, but may be globally highly non-homogeneous. The multiverse is not an abstract idea, but it is a generic consequence of a large class of inflationary theories, where unavoidable quantum fluctuations of the inflaton spark a chain process with eternal creation of regions that expand faster than the surrounding space.
     >
     > The multiverse is actually a familiar instrument of our everyday physics toolkit.

  2. Arguing against the fundamental significance of symmetry principles:

     > There are also theoretical indications for questioning the concept of symmetry. It is nowbelieved (and to a certain extent proven) that any global symmetry is violated at the level of quantum gravity. This means that any global symmetry that we observe in nature is only an accidental effect of looking at a system without sufficient short-distance resolution. The case of gauge symmetries is more subtle. Gauge symmetries are not real physical symmetries, in the sense
     > that they don't correspond to an invariance under a physical transformation, but only to a redundancy of the coordinate parametrisation. We often confuse our students on this point by showing them the Mexican-hat potential and leading them to believe that there is a degeneracy of vacua, when in reality there is only one single vacuum state that breaks EW symmetry, as it is clear from the fact that the physical spectrum doesn't contain any Goldstone boson corresponding to zero-energy excitations. Gauge symmetries may not be as fundamental as we thought, but only an emergent phenomenon. They could be a mirage of a different reality that takes place at a more fundamental level.

  It's looking depressingly possible that leaders of the field will push through as new "foundations for the future of our field" the argument that "the multiverse did it and symmetry is a mirage." Instead of moving forward, the field will take a huge step backwards.

Comments

1. **Martin S.**
   October 4, 2021

   Hi, the article on closing the "program for evolutionary dynamics" (it does not mention genetics) only notices past inappropriate contacts. I have not seen anything about financial issues there. Why is a scientific program closed then? I guess that I do not understand it because I am not in USA.

2. **Peter Woit**
   October 4, 2021

   Martin S.
   My mistake about the name of the program, fixed. I believe the program was shut down because the main faculty member involved in it was Martin Nowak, and the main funding had come from Epstein. Nowak was sanctioned by the university after a report was issued which detailed the university's involvement with Epstein and Nowak's role in this. The report is at
   https://ogc.harvard.edu/files/ogc/files/report_concerning_jeffrey_e._epsteins_connections_to_harvard_university.pdf

3. **mqt**
   October 4, 2021

   Here is a link to André's book that lets you download the PDF:
   https://spartacus-idh.com/pdfs/078/

4. **anon**
   October 5, 2021

   Surely Harvard could have easily kept funding PED if it was only a question of money. But the centre was very much Nowak's/Epstein's creation, so keeping it going would have looked very bad.

   CMSA doesn't have the same PR issues, so maybe Harvard is more willing to increase its own contribution. Or maybe S-T Yau (I think CMSA is his pet project) can find another sponsor.

5. **Sabine**
October 5, 2021

Well I agree that the community of particle physicists must "rise to the challenge". This means they should get rid of theorists who make a living by putting forward useless speculations, like inventing particles that no one will ever see (or extra-dimensions, or unparticles, etc). If your methods of theory-development allow you to retrodict any possible fluctuation in the data, then those methods are obviously unscientific and must be discontinued.

And, needless to say, they should also make sure that the group-think which made such practices acceptable in the first place doesn't happen again. Even psychologists have managed to clean up their act and have taken steps to throw out unscientific methods. I find it depressing that particle physicists still don't even acknowledge their problem.

6. **Shantanu**
October 5, 2021

My prediction : Aharanov and Berry

7. **martibal**
October 5, 2021

As many Italian colleagues think: they gave back to Parisi what they had stolen to Cabibbo!

8. **AcademicLurker**
October 5, 2021

Once again, my prediction of Aspect Clauser and Zeilinger is off.

I've been wondering for years whether Parisi would get it. His work has certainly been far reaching and influential enough. I confess I've never heard of the other two.

9. **AcademicLurker**
October 5, 2021

Re: Parisi and the Nobel prize, I must say I don't envy the poor science journalists who are now stuck with the task of trying to explain spin glass theory and replica symmetry breaking to a general audience. If they thought that the topology stuff in 2016 was difficult to popularize…

10. **CM**
October 5, 2021

Gauge symmetry as an emergent phenomenon has indeed been explicitly demonstrated in condensed matter systems. A notable example is the emergent QED in quantum spin ice. So the "different reality that takes place at a more fundamental level" could be that the fundamental theory of our world is some complex bosonic spin model, as envisioned by people like Xiao-Gang Wen or Laughlin.

11. **Peter Woit**
October 5, 2021

CM,
Sure, you can make a model of emergent QED, but emergent from what.? QED itself is hugely successful, tested to absurd accuracy, why replace it with a bosonic spin model or any such thing. This explains nothing at all, replaces a highly constrained beautiful theory based on deep geometry with something much less interesting? Why do this if there's not the slightest bid of evidence for such a supposedly more fundamental structure?

12. **OB**
October 6, 2021

@Peter
I don't think anyone's really advocating to replace QED with a spin model or any such thing. The point is to realize that just because symmetry principles, including gauge symmetries, are very useful doesn't necessarily imply that they are fundamental truths about the universe, valid at all length scales. What the Condensed Matter examples demonstrate is that one can end up with beautiful looking gauge theories as the effective theory, even if the underlying reality is quite unsymmetric and ugly (from a high energy theorist's POV)*. Indeed, there's no present experimental motivation to replace the standard model with some short distance Condensed Matter like model from which it could emerge, but philosophically its worth knowing that the success of the SM doesn't imply that its symmetries are necessarily fundamental.

*I'm a CM theorist, so I don't find lattice models ugly or boring, but each to their own.

13. **CM**
October 6, 2021

Peter Woit:

To answer your question, the QED could be viewed as emergent from some qubits defined on a lattice of extremely small length scale. I mean, if people can entertain the idea that the fundamental constituents are tiny strings, perhaps it is not too absurd to speculate that the world is build on a super-tiny lattice model.

Yes, QED is hugely successful and elegant. But until we figure out the consistent fundamental theory (super-string or not), one cannot rule out the possibility that the standard model QED is emergent from some bosonic model defined on a lattice. And the fact that such a beautiful gauge structure can emerge from a local bosonic theory is not "something much less interesting". This construction could be deeper than the "deep geometry" you mentioned.

14. **Arnold Neumaier**
October 6, 2021

CM,

the quantum spin ice does not give standard QED since it has magnetic monopoles and a fine structure constant which is far too large (besides being dependent on microscopic details of the model):

Pace, S. D., Morampudi, S. C., Moessner, R., & Laumann, C. R..

Emergent Fine Structure Constant of Quantum Spin Ice is Large.
Physical Review Letters 127 (2021), 117205.

15. **Peter Woit**
October 6, 2021

OB/CM,
I don't disagree that it's possible that the SM and its gauge symmetries are emergent from a more fundamental complicated/ugly/unsymmetric collection of qubits or whatever. But at this point, there's not a shred of evidence for this, it's completely unmotivated speculation. The string theorists at least started out with a model (10d superstring compactified on a Calabi-Yau) that in principle could explain things and that you could argue about. Replacing string theory conventional wisdom about a flawed theory by a new conventional wisdom that there's an ugly complicated fundamental theory we know nothing about isn't progress, it's just a way of convincing everyone to give up.

16. **Lowell Brown**
October 11, 2021

" Gauge symmetries are not real physical symmetries, in the sense …"
The reason people such as this guy can dispense expert knowledge is that they have no knowledge of what they are talking about.
Let's examine a nonabelian gauge theory. The group requires a specific form for the field strength F^a_{mu nu}.
F^a_{mu nu} = A^a_mu , nu – A^a_nu , mu
won't do.

String Theory and the Real World

October 6, 2021
Categories: Uncategorized

I just noticed that Gordon Kane has recently published a second edition of his 2017 String Theory and the Real World. Columbia doesn't seem to yet have full online access to the second edition, but one can already compare the two editions in a few places. For instance, on page 1-5 of the 2017 edition one reads

> The LHC is now working in a region of energy and intensity where well-motivated theories imply superpartners could be seen by late 2018.

and

> There is good reason, based on theory, to think discovery of the superpartners of Standard Model particles should occur at the CERN LHC in the next few years.

The corresponding first chapter of the latest edition has:

> The LHC has so far just entered the region of superpartner masses predicted by compactified theories, which ranges from about 1.5 to ~5 GeV (we'll discuss that range later). Those values are the only physics predictions, rather than just speculations. The LHC will run with higher luminosity after an upgrade, beginning in late 2021 if pandemic work stoppages do not delay it. That increases the possibility of discovery, though not very much. A higher energy collider is needed. From what we know now, a collider with twice the LHC energy range would probably suffice, and cover the region of gluino masses to about 5 GeV.

The concluding chapter of the 2017 edition tell us:

> The compactified M-theory implies that three superpartners (and only three) will be observed at the LHC in the current three-year run (assuming the full integrated luminosity is achieved). These are the gluino and the charged and neutral winos.

Presumably he's talking about the LHC Run II (2015-18) which did meet its luminosity goals, without any hint of the three superpartners. I don't yet have access to the later parts of the 2021 edition to see what they say about this.

This isn't the first time Kane has published multiple editions of ever changing "predictions" about supersymmetry. At one point I compared the 2000 and 2013 editions of "Supersymmetry and Beyond", you can see the results here.

Comments

1. **Sabine**
   October 7, 2021

   I am writing this comment for the interested reader, not for Peter who I know understands the situation perfectly well.

   Those particles are not scientific predictions. They are calculations based on theories that have been fabricated for the very purpose of having some "new" physics in the energy range of question. I have worked in this field myself and this is literally how it's done: You make up a theory and fiddle with the details until it shows a deviation from the established theories in a range that may become experimentally accessible soon. Then you claim this is a "prediction".

   As I have said now for more than a decade, this methodology is obviously unscientific and must be discontinued. It has become accepted practice because it's easy and convenient and allows people to crank out a lot of papers quickly. But it's a waste of time and money, and certainly not a reason to build a bigger collider.

   Many people are afraid of criticizing high energy particle physics because they don't understand the math. But you don't need to understand the math to see that these so-called predictions have as a matter of fact not worked since the 1980s. You can look at Kane's own publications to see that his predictions have been falsified over and over again and he keeps on changing them. (Which Peter has documented on this blog.)

   This is not a problem of "it's just hard to measure" (as it was, eg, with the neutrinos or gravitational waves). This is a problem of people who eternally amend their so-called theories. They live under the mistaken impression that just because a hypothesis makes predictions it is scientific. This is a trivial misunderstanding of the most rudimentary philosophy of science. Guesses aren't scientific predictions. And piling lots of math on top of guesses doesn't change the fact that they were guesses.

   I am not saying this because I am against particle physics (or other research on the foundations of physics). To the very contrary, I am saying this because I care about progress in the field. This methodology has not lead to progress. Researchers in the field urgently need to clean up their act.

2. **Peter Woit**
   October 7, 2021

   Sabine,
   I think it's important to note that what Kane is doing now is different than what most "string theorists" are doing. A lot of his book and recent writings are actually aimed at criticizing the string theory community for admitting that there is no way they can predict anything about LHC-scale physics. He's arguing for models and calculations that most of the rest of the string community has essentially disowned. They're content to take the position that if SUSY is found that proves string theory, if it's not, that's no problem for string theory.

   What I find remarkable about the Kane story, in his Physics Today articles, his earlier popular books, and now these two editions is the repeated blatant, on its face violation of conventional norms of scientific ethics. Most other people making models of the kind you describe are careful to stick to "my model shows there's a possibility the LHC will see something", and not say "if my model is right, the LHC will see something". This is because they realize that if they say the latter, when the LHC sees nothing they will be ethically bound to admit "my model is wrong", with negative implications for their career, funding, etc.

   Kane instead deals with the ethical problem by just ignoring it, never admitting anything and doing the same thing time and time again. This is not usual behavior. What's remarkable is that those responsible for maintaining ethical standards do nothing about it: the editors at Physics Today let him publish more articles doing the same thing, the publishers of his popular books and now the IOP let him publish revised editions. This really is not supposed to happen.

   I notice that while the old popular books carried endorsements from others in the field, the IOP ones don't seem to. My impression is that privately most string theorists find Kane an embarrassment, but will never say so publicly because they have for a long time circled the wagons and will not criticize behavior of members of their own tribe, no matter how egregious.

3. **anon.**

October 7, 2021

"What's remarkable is that those responsible for maintaining ethical standards do nothing about it: the editors at Physics Today let him publish more articles doing the same thing, the publishers of his popular books and now the IOP let him publish revised editions. This really is not supposed to happen. … most string theorists find Kane an embarrassment, but will never say so publicly because they have for a long time circled the wagons and will not criticize behavior of members of their own tribe, no matter how egregious."

You appear to be calling for censorship to solve the string theory hype dilemma, which is a bit like the tactics used in totalitarian regimes (by Stalin against Trotsky, for instance). This puzzles me. Surely censorship is always wrong in a libertarian world? Surely, truth will conquer all and the publishers of BS will go bankrupt when the correct model emerges? Forgive me for asking, but isn't the way to defeat evil, to fight weak arguments with reality instead of trying to ban weak arguments from being published?

4. **vmarko**
October 7, 2021

Peter,

"My impression is that privately most string theorists find Kane an embarrassment, but will never say so publicly because they have for a long time circled the wagons and will not criticize behavior of members of their own tribe, no matter how egregious."

I believe this is true to a large extent, but there are exceptions. During the "Why trust a theory" conference in Munich 2015, David Gross called out Kane for the "prediction" of the Higgs mass, and there was very vigorous discussion by Kane, Gross, Dieter Lust, and a couple of other people in the audience. I attended it, and later commented the event on your blog, here:

https://www.math.columbia.edu/~woit/wordpress/?p=8132&cpage=1#comment-220854

Afterwards, the recording of Kane's lecture became available, here:

https://videoonline.edu.lmu.de/en/node/7485

The "fun" part happens between slides 31 and 32 of Kane's talk. 🙂

Best, 🙂
Marko

5. **Peter Woit**
October 7, 2021

anon.
First of all, I'm not a libertarian and don't want to live in a libertarian world.
A healthy science literature needs editors and referees, and here I don't think they were doing their jobs.

Marko,

I do remember that, but that case was a bit different, since Kane was claiming for himself personally a huge (bogus) scientific achievement.

6. **Sabine**
October 9, 2021

Peter,

Sure I agree that Kane is an extreme case and of course there are pretty much no string theorists supporting him, but his methods exemplify those used by an entire community. The idea is that you fabricate some fancy explanation for why your particular guess is a good one, and then you do a series of impressive looking calculations which you call "predictions". That lives up to the scientific standard of today and you'll easily get it published in good journals because they can't think of a reason to reject it. The reason they should reject such guesses is, of course, that just because a hypothesis is testable doesn't mean it's scientific.

What's happened with Kane, I believe, is that since he's waded into science communication he was forced to express himself clearly which resulted in big claims that now come back to haunt him. Most people in the field will make sure their predictions have loopholes so that they can be amended should need come. This isn't hard to do since those predictions were guesses to begin with. Since Kane is only a particularly egregious example of a much more general problem, I find it actually somewhat unfair to pick on him particular.

The real issue is the entire community which, as you note, doesn't move a finger to improve the situation. That's because (to use a German idiom, if you excuse) they'd saw off the branch on which they sit. Producing these useless predictions is the only thing they know what to do. They can't discontinue it because what else would they make money with? This is of course also why they constantly complain that I'm supposedly just complaining and not making constructive suggestions. Totally wrong of course, if you read my book (or listen to my lectures) I have explained very clearly that they should focus on resolving mathematically well-defined inconsistencies rather than pulling guesses out of thin air. That, however, is difficult and would make their lives harder, so they won't do it. It's rather simple economics.

As I have mentioned before, this is the same problem they had in psychology with their sloppy ways of assessing statistical significance. (a) The problem was known for a long time. (b) It stalled scientific progress but they kept on doing it anyway (c) They justified it to themselves and others by claiming that it's a generally accepted standard.

The difference is that psychologists have taken steps to solve their problem. Particle physicists haven't.

7. **mjk**
October 9, 2021

Sabine,
I don't think it is possible to actually prove that someone is "pulling guesses out of thin air". That is just as much speculation as what you are alleging. The only evidence we can go on is what they say and write. And the only standards we can apply are how well motivated are their arguments. Are their ideas based on well established observations? Or are they based on well accepted assumptions of science? After that, it's only a matter of whether the math is correct. Perhaps these are the criteria that should be used by publishers. It seems obvious, for example, that string theory would not pass, since it is not based on observational data and the assumption that particles are strings or the like is not well motivated, IMO.

8. **Edward M Measure**
October 9, 2021

I predict that String Theory will continue to, er, thrive until some new discovery or idea appears in the current great experimental and theoretical desert. Theorists got to theorize somehow, even if it is only about how many string angels can dance on the head of a pin.

9. **Palinuro**
October 14, 2021

May be there are too many models in the market, with few new ideas and with some regions of parameters being excluded by experiments, but the problem started with the master, who after writing the SU(2)xU(1) model proposed next the SU(3)xU(1) model …

On the other hand, Sabine and Peter, is it fair to judge the whole particle physics community by the "sins" of one of its subgroups? Take for instance the work done on QCD corrections to Higgs production, with amplitudes, twistors and more into the game, it seems to me a solid pice of research, don't you think so?

10. **Peter Woit**
October 14, 2021

Palinuro,

I've never criticized "particle physics" in general, and Sabine's critique is clearly aimed at a certain mode of research, quite different than the sort of thing you mention.

The Kane case is an extreme one. Other people working on this kind of speculative model are careful to not make the kind of repeatedly falsified claims he does. The institutional problem his behavior raises is not so much about the field of research but about those institutions publishing his claims (Physics Today, his book publishers), They should be well aware that these claims have been repeatedly falsified in the past and that he does not acknowledge or address this.

11. **Rando M.**
October 27, 2021

I know this blog is a big fan of Lubos Motl. I feel like handing out some damning evidence of the futility of string theory from his own thesis advisor, Tom Banks.

https://www.physics.rutgers.edu/people/hpgs/BanksT.html

Here is his home page at Rutgers. As you can see in the second-to-last paragraph, he says:

" I've consequently decided to abandon this area of research and shift my focus to condensed matter physics. The study of quantum gravity, inflation, black hole information, and "string theory", suffers from the absence of well defined mathematical models, which define the theory in asymptotically flat or de Sitter space in a non-perturbative manner. It also suffers from a lack of contact with experiment, which is not expected to be ameliorated in the near future. Until further data becomes available on supersymmetric particles, dark matter, or finer details of the primordial fluctuation spectrum, these fields will be in the realm of uncontrolled conjecture. 15 years is enough time to spend on them."

What's astounding about this (I'm not exactly sure when he switched focus) is that in the late 90s he was co-author of the BFSS matrix model which was supposed to be the much-sought definition of M-Theory. Lubos created his own matrix model for non-perturbative string theory as an undergraduate, Banks was thoroughly impressed, Lubos became his doctoral student, and the rest is history.

So some time ago, Banks realized that all that impressive work on matrix models and other such speculation was just that, speculation, and he moved on to more concrete areas of physics that are connected to experiment. (I'm not sure if anyone else in the past has brought up Banks' change of heart on this blog, so apologies if this is redundant.)

This Week’s Hype

October 8, 2021
Categories: This Week's Hype

The latest from the BBC:

String theory – a simple way to understand the universe

Not worth more comment than it’s another reminder that this nonsense continues to be heavily promoted in our most prominent and respected mass media. I’m beginning to doubt we’re going to be rid of it in my (or anyone’s) lifetime.

No Comments

News and Travels

October 15, 2021
Categories: Uncategorized

First some personally relevant items:

- I finally have a finished version 2.0 of my euclidean twistor unification paper (discussed here), it's uploaded to the arXiv, should appear there Monday.
- I'll be giving a talk October 30th at the Foundations 2021 conference in Paris, something about the unity of math and physics, and will take the opportunity to spend about two weeks in Europe, mostly in Paris.
- In other travels, I'll be in the Bay area for a week or so mid-November.
- For entertainment I tried out a WordPress plugin that dumps all my blog content into a single pdf. If you want 8,518 pages to read at your leisure when you're not connected to the internet, this would be one way to spend your time.

In math and physics news, there's:

- David Mumford has a had a remarkable career, first as an algebraic geometer (he won a Fields Medal for his work in this area) and later in the field of computer vision. He's also known as a talented expositor, with his books and papers the standard references for several different topics. He's moved into physics this month, with a wonderful article about cosmology in the Notices. His blog is well-worth following, it had the cosmology piece a few months back.
- Also in the Notices is a set of memorial articles about Lucien Szpiro, who passed away last year. I wrote a little bit about him here, am very pleased to see these articles which give a detailed picture of both the person and his mathematics.
- The Simons Collaboration on Global Categorical Symmetries had its kick-off meeting this week in Stony Brook, videos available here. There are many interesting talks to watch. I got very excited for a minute (around :05:00 in this video) when Greg Moore started talking about some of my favorite questions (e.g. what is the representation theory of gauge groups in dimension greater than one?). But then I realized he had labeled these "Traditional Questions", in Fraktur font to emphasize how old and out of date they were. He described these as "old-fashioned questions", that people were not seriously working on anymore. As he explained, you're no longer supposed to be thinking about a fixed topology, but looking for something more general that treats all topologies. My problem with this is that one tends to get interesting results about topology this way, but the physics applications seem to be in condensed matter physics, with little relevance to questions about local fundamental physics that have always been my main interest.
- I really don't understand the thinking in physics theses days at all. Nima Arkani-Hamed is a remarkable theorist who came up with a lot of highly speculative ideas about particle physics that have never worked out, then moved on to brilliant work leading efforts that have transformed the study of scattering amplitudes. The APS just announced that he's getting the 2022 J. J. Sakurai Prize for Theoretical Particle Physics, for "the development of transformative new frameworks". These "transformative new frameworks" are listed as "work on large extra dimensions, the Little Higgs, and more generally for new ideas connected to the origin of the electroweak scale", none of which has had any success, while the amplitudes work is ignored.

Comments

1. **Alessandro Strumia**
October 16, 2021

   Congratulations to Nima. He did so many things that we can joke that a good motivation would have been: "for amplitudes… despite extra dimensions, little Higgs…". A look at recent editions (https://en.wikipedia.org/wiki/Sakurai_Prize) shows that, given the lack of new discoveries, it become a prize for US physicists who proposed new theories not confirmed or disfavoured by experimental tests. But then, why not extending the prize to theories that cannot be tested? Or suspending this kind of prizes until something is discovered?

2. **Jackiw–Teitelboim**
October 16, 2021

   Some random comments:

   – Read Graham Farmelo's "The Universe Speaks in Numbers" to see that a fair prize to Amplitudes surely should go to Freddy Cachazo first! (I nowadays believe one should rethink what a recognition of high-quality contributions should be instead of… well, money and medals. People getting it are already wealthy as scholarships to new pos-docs around the world only diminishing particularly to the kind of physics most of us reading this blog regularly are interested.)

   – Now, more interestingly, I find amusing Mumford's take on cosmology and an excellent example of physicists vs. mathematicians approaches. The latter are clearly far more open minded even while superficially looking otherwise, sometimes maybe for the rigorous style of mathematical writing mystifying untrained minds. But of course, this "open" mindedness could be a characteristic more of David Mumford himself, but my personal experience (Peter surely have far longer than I of course!) most mathematicians are generally open minded in private but rarely are willing to publish any physics-intuition that inspired their works under their names. Another exception I liked was Connes' interview in "Conversation on Quantum Gravity," possibly one of the few interesting chapters, given string theorists self-denial depressing. (Witten's interview is a indeed just a single page or my edition allowed by my university access is limited?). Particularly Connes' answer to mathematician's rôle in society, that was witty.

   – I looked at the Foundations conference website and their program. It will be fun to see your highly mathematical motivated talk alongside philosophers of science. Good luck to you!

3. **Joseph Conlon**
October 16, 2021

   "Nima Arkani-Hamed is a remarkable theorist who came up with a lot of highly speculative ideas about particle physics that have never worked out, then moved on to brilliant work [sic] leading efforts that have transformed the study of scattering amplitudes."

   Or, this week's hype.

   QCD is not my field, and I have never contributed to it at a technical level, but over the 15 or so years I have been in particle theory, it is clear that

   (a) the ability to compute QCD amplitudes with increasing numbers of loops and legs, of direct relevance to the LHC, has developed enormously over this period

   (b) Nima Arkani-Hamed is, by quite some way, not the leading contributor to (a)

   Instead this rather illustrates one of the systemic problems with the subject, which is the idea that problems are only interesting (and progress is only made) when someone from Princeton (or Harvard, or MIT, …..) works on them.

4. **Peter Woit**
October 16, 2021

Joseph Conlon,
My point was more that assuming you're going to give him a prize, why do it for the part of his work that was a failure?

I can't evaluate the relative technical contributions of the many people working in the amplitudes area. But, besides having a position at Princeton, Arkani-Hamed is quite a phenomenon: the two-hour long inspirational talks about how advances in the study of amplitudes are going to revolutionize physics, replace space, time and maybe quantum mechanics have sometimes even almost convinced me. Whatever else he is, he's certainly the driving force responsible for turning "Amplitudes" into a major and vibrant part of theoretical physics.

5. **JJT**
October 16, 2021

They already gave the prize for amplitudes in 2014 to Bern, Dixon and Kosower who well deserved the prize.

6. **Alessandro Strumia**
October 18, 2021

I don't expect that amplitudes will revolutionise physics. Because amplitudes satisfy non-trivial properties already at tree level, i.e. classical physics. How can you expect that simple classical physics hides flat-space holography, or something better than the usual local Lagrangians in space-time? It seems more likely that amplitudes satisfy non-trivial mathematical relations with no deep fundamental meaning, and that we care about amplitudes because that's how colliders happen to practically measure small distances. Relations among amplitudes that allow to quickly compute scatterings among many gluons are useful, but maybe only to those who want to compute these higher-order processes.

7. **Peter Shor**
October 18, 2021

Alessandro Strumia:

You say:

> It seems more likely that amplitudes satisfy non-trivial mathematical relations with no deep fundamental meaning,

One comment: I don't see how you can dismiss any mathematical relations like these as having no deep fundamental meaning. These mathematical relations certainly didn't happen just by chance, and I don't believe we understand the reason for them yet.

Now, whether the fundamental meaning underlying amplitudes is interesting for physicists, or just for mathematicians, is a different question. But either way, it's worth investigating.

8. **Alessandro Strumia**
October 18, 2021

Dear Peter (Shor): because these are perturbative relations, that satisfy interesting mathematical properties that appear *already at classical level*. I don't expect that a classical field theory has a hidden formulation where big surprising things happen, such as emergence of space-time and locality. So, while I agree that it's interesting and worth investigating, I am skeptical about highest expectations that amplitudes are the magic door that will open a new level of quantum reality. I would be happy to be wrong.

9. **Joseph Conlon**
October 19, 2021

Nima is certainly charismatic – no arguing that. But, NLO and NNLO QCD – surely the beating heart of amplitudes as they relate to the real world – happened without him. I think it does a disservice to the subject when people who did the real work get written out of the subject in favour of charismatic personalities. Give people who do the work the credit they deserve.

A similar thing was true of the film Particle Fever, which left out the theorists responsible for actually working out how to see the Higgs, in favour of Nima talking about the multiverse.

Although if we are talking about emergence of spacetime – it is perhaps more remarkable that 2-dimensional supersymmetric conformal field theories can be reinterpreted as describing the dynamics of 10 spacetime dimensions……

10. **COliveros**
October 21, 2021

Could you direct me to an explanation for laymen of what your twistor unification theory does?

11. **Peter Woit**
October 24, 2021

COliveros,
Unfortunately such a thing does not now exist. At the moment I'm trying to figure out how to get experts interested in these ideas, with the project of getting laymen interested something for the future.

12. **Kingsuk Maitra**
November 9, 2021

Dr Woit,
Are you planning to give a talk on your twister unification work during your visit to
the Bay Area mid November? If so, could you please post
the time and venue?
Thanks
Kingsuk Maitra

13. **Krzysztof**

November 11, 2021

a statement of the year? “the naturalness paradigm is devoid of physical meaning”

QFT without infinities and hierarchy problem: https://arxiv.org/abs/2110.05175

Back

November 26, 2021
Categories: abc Conjecture, Uncategorized

I haven't been posting here for a while, partly due to a lot of traveling, partly due to some personal time-consuming commitments, and largely due to a lack of much in the way of news that seemed worth much attention. For some examples of such news that might be of interest:

- Due to discovery of a buckled RF finger, the LHC start-up (at 6.8 TeV/beam) has been delayed from end of next February to end of March or beginning of April. For details, see here.
- As usual in the US for many years, no one knows what the Federal HEP physics budget for the current fiscal year is, although we're a couple months into it. The formal US budgeting process involves a long process including an executive branch budget proposal and congressional committee hearings and debate. This however does not lead to actual budget numbers, which only emerge at the last minute, made in some way understandable to no one I've ever asked about this. From the latest news, the US might have a budget any day now, and then a bit later we'll find out what the HEP budget will be.

  This year the process has involved a highly peculiar situation with the budget for US LHC contributions (prospects for large cuts, assumed to get fixed mysteriously in the last minute process). For the details of what is going on, there's a news story here, and discussion at an HEPAP meeting here. For the first time I'm aware of, the HEPAP meeting videos are on Youtube (see links here), so one can follow the actual discussion between physicists and government officials there.

- In non-news about the abc conjecture, the Japanese media appears to be reporting uncritically about IUT-based claims of proofs that are not accepted by the vast majority of experts in the subject. There have been a couple of workshops devoted to IUT (see here and here) recently, with those speaking about IUT almost all based at RIMS. Recently Mochizuki has posted a strange Invitation to view IUT workshop videos. To view the videos you have to apply, and promise not to use them for "non-mathematical purposes". My guess is that one of the "non-mathematical purposes" at issue would be bloggers pointing out that nowhere in the talks does anyone discuss the fact that convincing arguments have been given by Peter Scholze and Jacob Stix that the IUT-based proof of abc is flawed and cannot possibly work. This problem is addressed with:

  > Unfortunately, it has come to my attention that certain misunderstandings concerning IUT continue to persist in certain parts of the world. Perhaps the most famous misunderstanding concerns an asserted identification of "redundant copies". This misunderstanding involves well-known, essentially elementary mathematics at the beginning graduate level concerning the general nonsense surrounding "gluings". For instance, if one "applies" this misunderstanding to the well-known gluing construction of the projective line, then one concludes that the two copies of the affine line that appear in this gluing are "redundant", hence may be identified. This identification leads immediately to a contradiction, i.e., to a "proof" that the projective line cannot exist! More details may be found in the Introduction to and the references given there.

  In case anyone thinks it's plausible that Peter Scholze is making errors in elementary mathematics at the beginning graduate level, David Roberts has an explanation of what's going on here.

On the string theory front, it's become impossible to figure out how to have any sort of scientific debate about most of the public defenses of "string theory". For two recent examples:

- In an article about What We Will Never Know, David Gross rather explicitly acknowledges that prospects for testing ideas about string theory are now an issue of "faith", with no hope of turning into science any time soon:

  > There's faith that one way or another we should be able to test these ideas… It might be very indirect—but that's not something that's a pressing issue.

- For Nabil Iqbal, string theory is now to be understood at the pre-scientific level of parable. In his parable, human beings trying to understand the equations of string theory are like fish trying to understand the equations governing the behavior of water. I'm trying to think of a sensible comment about this, but I've got nothing.

Comments

1. **S**
   November 26, 2021

   So presumably it would be like humans trying to understand the equations that govern air? If so, that seems like good news. The Navier-Stokes equations are challenging no doubt, but there's enough optimism surrounding them that it seemed reasonable to attach a $1M prize to one of the biggest outstanding problems relating to them. String theory being in a similarly coherent state would be unambiguously good news — right?

2. **Peter Woit**
   November 26, 2021

   S,
   In the parable, humans are trying to understand the vacuum, not air, and our current understanding of the vacuum based on QFT is like a smart fish understanding water using fluid mechanics. A string theorist telling us that the vacuum needs to be understood in terms of strings is like a really super-duper smart fish telling other fish about atomic physics.

   Again, trying to think of something intelligent to say about this parable, but failing utterly.

3. **David Roberts**
   November 26, 2021

   I sent the note I wrote to Mochizuki, and he responded to explain more of what he meant, in terms I could understand this time. He says that his approach to diagrams ('labels', etc), is the same as the standard approach that I give, which is true—up to isomorphism and idiosyncratic notation and terminology. But his contention is that Scholze and Stix are not just replacing merely isomorphic objects by equal objects, but also collapsing the diagram indexing them so that the indexing nodes become equal.

   For those not used to category theory, one can have a nontrivial diagram—take a directed graph, for simplicity,—with many nodes, where different nodes can be labelled by the same object. Mochizuki's style is to additionally decorate objects at different nodes with distinct labels (wholly unnecessary, from a technical point of view). Ordinary category theory just remembers the shape of the diagram to distinguish the nodes. Mochizuki contents that his critics are collapsing the diagram so that if two nodes are decorated by the same object, the nodes are collapsed, forming a quotient of the diagram. I cannot imagine a situation where someone well-versed in category-theoretic vernacular would make this mistake.

Our discussion is ongoing; at this point I'm simply trying to diagnose the thought process that leads to his claim at a technical level (not the sociological, mind!). We are only discussing a simple example, far, *far* from IUT, but it's an example that Mochizuki himself claims is a very good illustration of the actual construction, and the error he claims is being made in the critique.

4. **anon**
   November 27, 2021

   It would be interesting to hear from someone who has followed the Japanese media if the situation there really is as bad as it looks from the outside (to someone who has to rely on Google Translate etc. to read Japanese). Is there any even slightly critical commentary about the IUT in popular media?

5. **Martin Rosinberg**
   November 27, 2021

   Anon,
   As far as I know, there has been no critical commentary of Mochizuki's claim in the japanese media.
   (PS: I presently live in Kyoto).

6. **Jim Eadon**
   November 28, 2021

   The Fish analogy reminds me of a different fish analogy I read many years ago.
   The author claimed that Linde explained that our universe is like an ocean, and scientists are like fish trying to explain why water is the temperature it is from first principles. However, the universe actually contains many oceans, and the temperature of our ocean is just down to random chance. The conclusion? Apparently, our universe must be one universe of a vast multiverse of possible universes, each with differing physics. This was "explaining" why String Theory is unable to, well, explain our universe. Nothing changes…

7. **Peter Woit**
   November 28, 2021

   Jim Eadon,
   It's the same analogy, with the same "explanation" of why we can't explain anything (viscosity replacing temperature).

8. **Blake Stacey**
   November 30, 2021

   David Roberts wrote:

   > Mochizuki contents that his critics are collapsing the diagram so that if two nodes are decorated by the same object, the nodes are collapsed, forming a quotient of the diagram.

   It would definitely change the character of the Four Color Theorem if a map that could be colored with four colors must only contain four countries.

9. **Peter Woit**
   November 30, 2021

   All,
   There's no point to following Mochizuki in debating things which have nothing to do with Scholze's actual argument. If anyone wants to engage with that, there's a detailed discussion at
   https://www.math.columbia.edu/~woit/szpirostillaconjecture.pdf

   There at the beginning Scholze asks

   "If this further identification causes problems, just tell us which diagram it is whose commutativity is rescued by not explicitly identifying $\pi_1(X)$'s."

   No one in the discussion there was able to answer this, and Scholze reports that Mochizuki could not do so in their private discussions. Publicly, Mochizuki has never answered the question, preferring to argue that Scholze is an incompetent who needs to spend more time studying the IUT papers to find the answer to his question.

   Given this situation, what's going on here is clear, and there's no point to wasting more time on it.

10. **Davide Castelvecchi**
    December 2, 2021

    The Asahi Shimbun story has now been published in English
    https://www.asahi.com/ajw/articles/14488092

11. **anon**
    December 7, 2021

    Some real mathematics news: it seems that the 2026 ICM will take place in Philadelphia. The official decision will of course only be made at the IMU General Assembly in July, but there are no other applications this time.

Unifying Foundations for Physics and Mathematics

November 28, 2021
Categories: Uncategorized

During recent travels I attended two conferences (in Paris and Berkeley) and met up with quite a few people. At the Paris conference I gave an intentionally provocative talk to the philosophers of physics there, slides are here. The argument I was trying to make is essentially that more attention should be paid to evidence for a deep unity in much of modern mathematics, which at the same time is connected to our best unified theory of physics (the Standard Model and GR). Edward Frenkel has made some similar points, referring to the Langlands program and its connections to physics as a "Grand Unified Theory of Mathematics". The specific structures underlying this unification seem to me to deserve attention as providing an important way of thinking about what's at the "foundations" of both math and physics.

Another motivation for this talk was to make an argument against what I see as having become a widespread and standard ideology about the search for a unified theory in physics. Talking to many physicists and mathematicians interested in physics, I noticed that the conventional wisdom, shared by the establishment and contrarians alike, is that the SM and GR are likely low energy emergent theories, that some completely different sort of theory is needed to describe very short distances such as the Planck scale. Physics establishment figures tend to believe that following the path started with string theory, then AdS/CFT, lately quantum error correction or whatever, will someday lead to a dramatically different sort of theory, replacing space, time and maybe quantum mechanics. Contrarians often have their own favorite idea for a radically different starting point. For an example of this, take a look at Figures 2 and 3 of Mike Freedman's The Universe from a Single Particle (he spoke about this in Berkeley). Figure 2 is the "establishment" picture, with AdS/CFT the fundamental theory, well-decoupled from the emergent SM + GR (since no one has any idea how to relate them). His Figure 3 shows his own proposal, even better decoupled from any connection to the SM + GR.

Given the extreme level of experimental success of the SM + GR, the obvious conjecture is that these are close to a unified theory valid at all distances. That the mathematical framework they are built on is closely connected to unifying structures in mathematics provides yet more evidence that what one is looking for is not something completely different. The odd thing about the present moment is that arguing that our well-established successful theories can provide a solid basis for further unification makes one a contrarian, with the "establishment" position that a revolution sweeping such theories aside is needed.

I hope to find time in the next few weeks to write up what's outlined in the slides as a more detailed article of some sort. More immediately, I plan to write a blog entry and perhaps some more detailed notes about the "twistor P^1" mentioned at the end of the talk, explaining how it shows up in Euclidean twistor theory as well as in recent work on the Langlands program.

Comments

1. **AP**
   November 28, 2021

   The Langlands program is important, but it is hardly a "Grand Unified Theory of Mathematics". It does not even interact with the majority of areas of mathematics, and outside of number theory it is a pretty niche subject (e.g. it does make contact with algebraic geometry and representation theory, but most workers in those fields do not care about it). It's even stranger to think of it as "foundational", at least in the way that mathematicians usually use the word.

2. **Peter Woit**
   November 29, 2021

   AP,

   The use of a different meaning than usual for "foundations" was meant to be provocative, drawing attention to a too-little recognized and poorly understood deep structure unifying different areas of mathematics. That what most mathematicians work on and care about has little to do with this is true, but also true of "foundations" in the usual sense (and also true in physics where most research has nothing to do with a possible unifying theory).

3. **DL**
   November 29, 2021

   "Given the extreme level of experimental success of the SM + GR, the obvious conjecture is that these are close to a unified theory valid at all distances. " Does not the Dark Matter vs Mond problem indicate that something is seriously wrong?

4. **Peter Woit**
   November 29, 2021

   DL,
   I don't want to start a serious discussion of dark matter here (especially not of MOND vs. dark matter). It's a huge and very complicated subject that is the main focus of attention for attempts to get beyond the SM, precisely because it's the only significant source of a possible discrepancy between the SM and observations.

   Just note that there's no discrepancy between the SM and observations in any earth (or solar system..)-bound experiment. The dark-matter problem shows up in very large scale astrophysical observations, as a conflict between these and best astrophysical modeling. While the evidence seems to be against this, in principle the problem could be in the astrophysical modeling, not the underlying theory (e.g. primordial black holes).

   Even if dark matter is a new non-SM particle, such a thing can be accommodated with a small change to the SM, adding one or more new fields with no SM, only gravitational interactions. You might even be able to do this just with right-handed neutrino fields, which fit very nicely into the SM, in some sense are expected.

5. **Low Math, Meekly Interacting**
   November 30, 2021

   I could be conflating issues relevant to this post with irrelevant ones, but hopefully not. Whether connecting unifying developments in mathematics and physics seems too mystical strikes me as a very superficial objection. What I wonder about is how such conjecture helps with the problem everyone typically cites, namely incurable infinities. One could reasonably say GM and QFT were unified back in the 50s or thereabouts, but such theories were "diseased", as Feynman put it. The current approaches aspire to cure the disease with discretized space-time, extended objects, "asymptotic safety". They give mathematically sensible answers that, so far, don't agree with reality very well.

   So what does something akin to connecting number theory and algebraic geometry credibly do to make quantum gravity yield finite answers that resemble our world without introducing something very new at the microscopic level?

6. **Peter Woit**

November 30, 2021

LMMI,
The way things work is not that some particular piece of known mathematics directly solves deep problems in theoretical physics.

My argument here is that the answer to the problems of quantum gravity are not going to be found by throwing out the ideas about geometry, gauge symmetry, spinors, the Dirac operator that our best theory is built on, that have been wildly successful and that are mathematically very deep. What's needed is an extension of these ideas, not abandoning them for something completely different, arguing that somehow you'll recover them later as emergent phenomena.

I don't want to host now a discussion of everyone's favorite ideas about quantum gravity. Since it's my blog, I will just mention that the work I've been doing points in the direction of fundamental space-time degrees of freedom being closely related to the SM ones, using spinors, twistors, and the chiral spin-connection. I don't claim to have solved the problem, but do see new things to think about that may go somewhere: use of just one chirality of spin connection and a fundamentally conformally invariant formalism, working in Euclidean space time as primary, having a degree of freedom that distinguishes the imaginary time direction. For more details, see
https://arxiv.org/abs/2104.05099

7. **WTW**
November 30, 2021

Peter,
Following is a transcription excerpted from an IAS Video Lecture by Leonard Susskind on 29 March 2021. https://www.ias.edu/video/some-half-baked-thoughts-about-de-sitter-space
(The transcription is my own, not from Google; the only AI used was that which is between my ears. Any errors are mine.)

"…
Let's begin with a nice thought… In fact let's imagine a group of very smart theorists. They know all about quantum mechanics and relativity, but have never heard about modern astronomy or particle physics.

If they were anything like us, they would eventually discover black holes, quantum field theory, string theory, supersymmetry, the holographic principle, large-N matrix theory, AdS/CFT. In other words, from a pure theory point of view, they would be about where we are now.
What would they take away from all of this?

Here's what I think they would say: I think they would say, first of all, if you want to put gravity and quantum mechanics together, you better have a frozen time-like boundary to anchor the theory, and to define observables. AdS is great in this respect. And maybe flat space is OK. To make the string scale much smaller than the cosmic radius, since in this case the cosmic radius would be the AdS scale, the boundary theory better be super-strongly coupled — but knowing as much as we do, they might believe that the only theories that can be pushed to super-strong coupling are supersymmetric. That would also fit with what they know about matrix theory, where without supersymmetry you can't even seperate two particles. Asymptotic boundaries, super-strong coupling, supersymmetry — these would be their touch stones.

What would happen, then, if some crazy man showed up and told them that he had seen the data, and could assure them that space has no boundary, that spacetime is more like deSitter space than Anti-deSitter space, that there's no supersymmetry, and — despite that — locality works down to ultra-microscopic scales?

I tell you what I think they would say. I think they would say, "Baloney! Your model is in the swampland."
They'd say this not because of some made-up input bound on the inflaton, but because the model violates the entire foundation of their mathematical understanding!

OK, what should we take away from this parable?
The answer is, I don't really know. …"

I've heard similar "parables", with varying descriptions of what is or isn't discrepant, from people like Arkami-Hamed and Ed Witten among others. I believe the general summary is "We're missing something, and we don't know what it is." And we tend to think (or at least pretend) — since people in this field are so damn smart — that we know more than we actually do.

The critique I would give of your presentation is that you are underestimating the fundamental inconsistencies between and among these theories, that have tried to be papered over with mathematical slight of hand. Rather than providing some deep underlying fundamental truth, it tends to be used as fairy dust. Separating the wheat from the chaff while navigating our desire for universality of mathematical form is one of the key issues that must be addressed if we are to make real progress.

8. **Paolo Bertozzini**
November 30, 2021

Dear Peter,

thanks for pointing this out clearly. There is indeed an unusual and quite widespread consensus between most of the current approaches to quantum gravity: not only that geometry of space-time (and the standard model) is an emergent macroscopic feature, but that it is even impossible to talk about "geometry" at the fundamental quantum level (for example one can see the recent brief talks at the "1st Workshop of the International Society for Quantum Gravity" [https://isqg.org/first-isqg-meeting/] that are available on the ISQG YouTube channel). Surprisingly this "emergentism" seems relatively common even in areas, like non-commutative geometry, that in principle are already capable of "saying something" on space-time (and standard model) at the quantum level.

One should probably make here a distinction between "macroscopic emergence" exclusively via phase-transitions (that is the position I was referring to in the previous paragraph) and an "operational/spectral" point of view (where space-time, commutative or not, is determined by other degrees of freedom, but is otherwise perfectly well defined also at the fundamental level).

In the current trends in AdS/CFT, some geometric properties (related to the modular structure of the local operator algebras in CFT with respect to the vacuum or its perturbations) are being investigated; such results will likely survive even without the background propaganda of "emergence" and will continue to make sense also in situations that are different from the original AdS/CFT string-theoretic motivation (since they are just based on Reeh-Schlieder theorem in QFT and Tomita-Takesaki modular theory). The recovery of "space-time geometry" from states on local algebras of operators has actually a much older tradition, going back to works in Algebraic QFT by Haag, Bannier, Keyl, Buchholz, Summers (just to cite a few).

Best Regards.

9. **Peter Woit**
November 30, 2021

WTW,
Susskind's
"If they were anything like us, they would eventually discover black holes, quantum field theory, string theory, supersymmetry, the holographic principle, large-N matrix

theory, AdS/CFT. In other words, from a pure theory point of view, they would be about where we are now."
This indicates the fundamental psychological problem of the field. He can't even imagine the possibility that string theory, SUSY, etc were wrong turns, that he and others have gone down a blind alley for decades.

A huge problem with the current relation of math and physics is that those with a deep belief in the unity of math and physics who can't believe they're in a blind alley are left searching for unity in an unpromising place (e.g. AdS/CFT), limiting what they can find. It's really important for people to distinguish what is solid fundamental theory (SM +QG), concentrate on looking for deep mathematics there, being very wary about starting with unsuccessful speculative physics as a starting point.

In some sense all mathematics is connected, and if you start at an unpromising place you may sooner or later make your way somewhere interesting (for instance, if you start by believing 6d Calabi-Yaus are fundamental). But it's much, much harder to make progress that way and you're likely to end up convincing others that you're "Lost in Math", that looking for unity through math is a fool's errand.

10. **Peter Shor**
December 1, 2021

Peter, you say

> Physics establishment figures tend to believe that following the path started with string theory, then AdS/CFT, lately quantum error correction or whatever, will someday lead to a dramatically different sort of theory.

It seems to me that trying to base a fundamental theory of quantum physics on quantum information and quantum error correction is an error on the order of trying to base a fundamental theory of classical physics on thermodynamics.

Thermodynamics will tell you a lot about classical physics (and maybe you can even derive the ideal gas laws from it), but ultimately it will leave a lot of physics unspecified; thermodynamics will never give you the properties of electrons.

11. **d_b**
December 1, 2021

@Peter Shor
Isn't that precisely the idea, though? The claim (or speculation, rather — I'm not sure that I would elevate it to a "claim" just yet) is that classical gravity is part of the emergent, low energy behavior of complex quantum systems (or perhaps ensembles of quantum systems). The fundamental theory would just be quantum mechanics, and the quantum error correction is supposed to explain how a quantum mechanical system can robustly encode classical spacetime.

12. **Peter Shor**
December 2, 2021

d_b

My point is that I think it's much more likely that some theory of quantum gravity is fundamental, and all the quantum information type stuff these researchers are looking at is emergent. And if this is true, there's a limit to how far they can go unless they realize this.

13. **SRP**
December 6, 2021

Given the accessible experimental anomalies that have to do with the proton, notably the Krisch transverse-polarized collision data (repeatedly confirmed as showing spin effects not dying out as they are supposed to at higher energies), statements about how the SM is just too gosh-darn perfect to give theorists any clues strike the layman as avoidance.

14. **Peter Woit**
December 6, 2021

SRP,
I'd be very interested (because of the euclidean twistor unification picture I've been working on) to see a discussion of spin-dependent effects in proton collisions that indicates experimental anomalies that cannot be plausibly attributed to our lack of a reliable non-perturbative calculational methods for those effects.

15. **SRP**
December 9, 2021

Here is Krisch in 2010, after telling the story of how from the beginning QCD theorists firmly predicted the wrong result based on general principles (I've found a few presentations by others since then saying yep, still an anomaly, along with some other problems):

"To summarize, for the past 30 years QCD-based calculations have continued to disagree with the ZGS 2-spin and AGS 1-spin elastic data, and the ZGS, AGS, Fermilab and now RHIC [28] inclusive data. To be specific:
* These large spin effects do not go to zero at high-energy or high-Pt, as was predicted.
* No QCD-based model can yet explain simultaneously all these large spin effects. There is a BASIC PRINCIPLE OF SCIENCE:
* If a theory disagrees with reproducible experimental data, then it must be modified. Precise spin experiments could provide experimental guidance for the required modifica- tion of the theory of Strong Interactions. New experiments at higher energy and higher Pt on the proton-proton elastic cross-section's: dσ/dt, Ann and An could provide further guidance for these modifications, just as the RHIC inclusive An experiment [28] confirmed the earlier Fermilab experiments [27]. Elastic scattering is especially important because:
* It is the only exclusive process large enough to be measured at TeV energy. This is probably because proton-proton elastic scattering is dominated by the diffrac- tion due to the millions of inelastic channels that compete for the total cross-section of only about 100 milibarns at TeV energies. Many people may have forgotten this simple but essential geometrical approach [1], which I learned from Prof. Serber's optical model in 1963 [3]; perhaps it should now be learned or relearned by others."

https://arxiv.org/pdf/1001.0790.pdf

Lex Fridman Podcast

December 3, 2021
Categories: Uncategorized

A couple months ago I recorded a podcast with Lex Fridman, it's now available [here](#).

A lot of Fridman's other interviews are well worth watching or listening to, and I thought we had an interesting conversation. I can't stand listening to or watching myself, so not sure how it turned out. But happy to answer here any questions about what we were discussing.

Comments

1. **Bryan**
   December 4, 2021

   Very cool! I love this blog and Lex's podcast so what a great treat!

2. **Lino D'Ischia**
   December 4, 2021

   "I can't stand listening to or watching myself . . . "

   I know the feeling!

3. **Jason S.**
   December 4, 2021

   Was there a mutual softball agreement when it came to the topic of Eric Weinstein? Surely you or Lex must be aware of the technical criticisms of his Geometric Unity theory that's been out for awhile now (the only one in existence by Nguyen and Polya).

4. **Peter Woit**
   December 4, 2021

   Jason S.,
   If I'm going to spend time specifically criticizing people's ideas, it's because I think there's a problematic situation with the ideas, that they're getting significant attention and funding within the research community, crowding out better ideas. I don't see any point to spending time discussing technical criticisms of Weinstein's work (or Garett Lisi's, or any number of other similar if less well-known research programs).

   What I thought was worth talking about (and from what I remember, did talk about with Fridman) is the underlying fundamental problem that Lisi/Weinstein's ideas share with mainstream ones that are now in the textbooks (GUTs/SUSY). If you try to get unification by embedding the SM symmetry groups in a larger group, you have to then introduce new physics to break the symmetry group down to the observed SM one. Generically, this removes most if not all of the explanatory power of your idea about unification.

   Anyway, happy to discuss that point more, I think it's very much underappreciated. I'll leave debating pro/con the work of Lisi, Weinstein, Wolfram and others to those who feel it's a good use of their time, don't want to host that here.

5. **Jason S.**
   December 4, 2021

   Peter:
   While your answer makes sense and explains why you would not criticize such theories on your platform, it would seem that the right metric for answering a question on a podcast is whether ideas are getting podcast attention (vs scientific community attention). So it looks to me like a missed (deliberate?) opportunity to give a non-generic comment when one seemed possible. If there wasn't a point to evaluate Weinstein's (or anybody else's) work, the question would not have been asked.

6. **jjohn**
   December 4, 2021

   I wasn't aware of Lex Fridman or his blog before this. He seemed to understand almost nothing that you said, judging by his inane follow-up questions, silly reformulations of your points, etc.. Was he having a bad day or is this representative of his work? You say his blog his interesting, so I suppose it was a bad day…

7. **DF**
   December 4, 2021

   At ~7:00 you mention that recent developments in Number Theory "fit into a context where the theory is kind of four-dimensional." Could you elaborate what in particular you were referring to?

8. **Peter Woit**
   December 4, 2021

   Jason S.,
   For me, what's worth giving attention to, on a podcast or elsewhere, is exactly those topics that are interesting and not getting attention.

   jjohn,
   Fridman's own expertise is far removed from math/theoretical physics. I think you underestimate how hard it is for someone who hasn't spent a lot of time following controversies in physics to understand what is going on and carry on a thoughtful conversation about them. I've often run into people trying to do discuss or write about such issues who are well-meaning but completely clueless. Given the challenges I think he did quite a good job. It was clear that his own interests are mostly in different directions than mine, but he found common ground .

   Also worth noting is that he interviews a large number of people, with very different interests. The sheer number of interviews he has done is remarkable, and from what I've seen the quality is pretty high.

9. **Peter Woit**
December 4, 2021

DF,
What I had in mind specifically was Witten's reformulation of geometric Langlands in terms of a four-dimensional QFT. It's true this is geometric, not number theory Langlands, so more of an analogy. I also had in mind the dictionary relating knots and 3 manifolds with number theory, in which a number field is three-dimensional. See here
https://web.ma.utexas.edu/users/vandyke/notes/langlands_sp21/langlands.pdf
for notes from a recent course by David Ben-Zvi in which he relates this knot/3 manifold point of view on number theory with the 4d QFT.

10. **Steve E**
December 5, 2021

Haven't visited this page in years, but the Lex Fridman podcast brought me back! Great to be here. Your conversation with Lex was great.

11. **JohnB**
December 5, 2021

That was a very good interview, be interesting to hear more approachable discussions like this, that touch on the potential of spinors/twistors for physics! (though having it broken up with other varied/interesting topics, like in this interview, helps :))

12. **George H.**
December 5, 2021

I very much enjoyed your podcast with Lex. Thank you.

13. **Jono**
December 5, 2021

Hi Peter,

I think there may of been some string theory hype back in September that you may of missed, since I don't see that you made any blog post about it. Apparently there's a paper that was published in Physics Review D that claims to of found gravitational wave spectra of merging "fuzzballs" by numerical computer modelling that are different from different from those predicted by ordinary GR for merging black holes. The authors appear to claim that these could be potentially detected by current or future gravitational wave detectors. It was reported here:

https://physics.aps.org/articles/v14/s110

and the pre-print is here:

https://arxiv.org/pdf/2103.10960.pdf

I've already taken a look at the paper. Do you have any comment on this?

14. **Jay H.**
December 5, 2021

That actually was an interesting conversation. Now I couldn't help but noticing that, when string theory became a topic of discussion, you never mentioned the fact that LHC has not found any supersymmetric partners. My understanding is that, given the energy regions that LHC has explored, this already discards most supersymmetric schemes – leaving only those that are highly contrived and therefore far less compelling.

Is this right? If not, do you think LHC is likely to find these partners some time in this decade?

15. **Low Math, Meekly Interacting**
December 5, 2021

I'm also admirer of Lex Fridman's interviews. The list of interviewees and breadth of subjects covered are indeed impressive, and I have yet to listen to one for which I would use the word "inane" to describe any part of the exchange.

Enjoyed this one, too.

16. **Peter Woit**
December 5, 2021

Jay H.,

No I don't think there's any reason to believe in the usual SUSY extensions of the SM. There's zero evidence for them, and they have exactly the problem I mentioned earlier: you introduce a new symmetry and immediately create a huge problem: how do you break it to explain why we don't see it? For SUSY this problem is pretty deadly: SUSY-breaking schemes are ugly and introduce lots of new undetermined parameters. A good idea should reduce the number of parameters you can't calculate, not increase that number. That the LHC did not see SUSY was not unexpected at all for most theorists.

The problem with string theory is different. You don't really have an understood full theory, all you understand well is a perturbative expansion in 10d space-time of a supersymmetric theory. The problem with trying to use this is that you have to get rid of 6 of the dimensions and there are too many ways to do that, so your theory can't predict anything. Some string theorists found it convenient for many years to answer people saying their theory predicted nothing and couldn't be tested by claiming "our theory predicts SUSY, can be tested at the LHC" (which wasn't really true). This blew up in their face with the null LHC results. That's a complicated story, best to keep it simple: the theory predicts nothing and can't be tested, mainly because the only version you understand has six dimensions you can't get rid of without destroying the predictivity of the theory.

17. **bryan**
December 7, 2021

I just listened to the first half and what a great interview!

Peter, you mentioned a fundamental idea about mapping the integers to a geometric space with the function mod p. What is this called and where can I read more about it?

18. **Peter Woit**
December 7, 2021

bryan,
The general story is that to a commutative ring R one can associate a "space" Spec(R), with R then in some sense functions on this space. This is at the foundation of modern algebraic geometry, which gets rather sophisticated.

What I was referring to was the case of R=Z, the ring of integers. In that case what happens is that Spec (Z) is basically the set of prime numbers p. Given any integer n in Z, you can think of it as a function on the set of prime numbers, with f(p)=n mod p (so your functions are taking values in integers mod p at the point p).

19. **bryan**
December 7, 2021

Peter, thanks! This is going to keep me busy for a while!

20. **Patrick Malloy**
December 8, 2021

Another long-time, non-technical, blog reader. Very much enjoyed the podcast. I think your gentle push back on the relationship between the sceince you and Fridman were talking about and a meaning of life nicely illustrates one of the qualities that I find valuable in your presentations.

One thing I do not think you addressed, and that might have come near the surface of the conversation is a point you have made several times on the blog. If I remember correctly, you have noted that mathematics practice could serve as a useful model for theoretical physics in absence of new experimental data. (Please forgive if I am mischacterizing.)

I was always intrigued by this idea and wondered what sorts of practices these might be? You may have alluded them in a couple places, for example when you touched on Lisi and Weinstein.

You have already dealt with this issue at length elsewhere, and, as you rightly note, life is short and there are many things yet to do.

Thank you and very best.

21. **Peter Woit**
December 8, 2021

Patrick Malloy,

There are two different ways I think mathematics practice can help with the current state of physics:
1. Mathematicians are very careful about stating things precisely and making it clear exactly where the line is between what we understand and what we don't. Physicists traditionally haven't needed this, they could rely on experiment for guidance, mathematicians have never had this to rely. I'm convinced physicists could benefit from adopting more of these concerns. At the moment, in many physics theory papers it's hard to impossible to figure out what the exact statement is, and there's a huge lack of clarity over whether ideas work or don't.

2. Much more speculatively, I believe there's a deep unity between fundamental physics and deep unifying ideas in mathematics. Taking this principle seriously gives one at least some very vague guidance as to what is a promising way forward and what isn't.

22. **gentzen**
December 9, 2021

I really enjoyed your podcast with Lex. It taught me some things I liked to know. It made me wonder whether your old book was also at the level of Penrose's books when it comes to explain physical and mathematical concepts. Read some of its reviews, but decided that I would rather read your newer stuff, when I find time to read more of your writtings.

Some Math and Physics Items

December 7, 2021
Categories: Uncategorized

First some math news:

- An anonymous commenter claims here that the 2026 ICM will take place in Philadelphia. I had heard that a US group was submitting a proposal, so this rumor is plausible.
- Many mathematicians and physicists have signed an Open Letter on K-12 Mathematics pointing to problems with attempts to reform mathematics education such as the California Mathematics Framework. For more about this, see the blog entry posted here and on Scott Aaronson's blog, and more detail here.

  While I've always had some sympathy for the general idea that there's much that could be changed and improved about the US K-12 math curriculum, there's a huge problem with all proposed changes based on the "algebra/pre-calculus/calculus sequence is too hard and not relevant to everyday life" argument. Students leaving high school without algebra and some pre-calculus are put in a position such that they're unequipped to study calculus, and calculus is fundamental to learning physics. Without being able to learn physics, a huge range of possible fields of study and careers will be closed to them, from much of engineering through even going to medical school. Whatever change one makes to K-12 math education, it shouldn't leave students entering college with a severely limited choice of fields they are prepared to study.

- Davide Castelvecchi at Nature has a story about machine learning being useful in knot theory and representation theory. Given my personal prejudice that hearing endlessly about how AI and machine learning will take over everything is just depressing, I'm trying to ignore this kind of thing. But, together with stories like the success of proof assistants in solving a problem posed by Scholze, it's harder and harder to believe what I would like to believe (that this is all a bunch of hype that should be ignored).

For some physics items:

- Jim Baggott has an excellent article at Aeon about the "Shut up and calculate" meme, featuring a retraction by its originator, David Mermin

  > In a quick follow-up discussion with me in July 2021, Mermin confessed that he now regrets his choice of words. Already by 2004 he had 'come to hold a milder and more nuanced opinion of the Copenhagen view'. He had accepted that 'Shut up and calculate' was 'not very clever. It's snide and mindlessly dismissive.' But he also felt that he had nothing to be ashamed of 'other than having characterized the Copenhagen interpretation in such foolish terms'.

- For some wisdom on the thorny issue of how to relate Euclidean and Minkowski signature metrics in gravity, see the recent IAS lecture by Graeme Segal on Wick Rotation and the Positivity of Energy in Quantum Field Theory.
- In fundamental theoretical physics these days, it's quantum information theory all the time, with conferences around now here, here, here, and here. I can't figure out what the relevance of any of this is supposed to be to actual models describing reality. Best guess would be that this is supposed to "solve the black hole information loss paradox", although in that case Sabine Hossenfelder has some apt comments here.
- For something more inspirational, see Natalie Wolchover's long piece at Quanta on the JWST.

Update: For more from Geordie Williamson about the math/AI story, see here. For more about the problems with the California Mathematics Framework and its co-author see here.

Comments

1. **Jim Akerlund**
   December 7, 2021

   Not sure if you will want to change it, but your first sentence begins "First some math news: …". Then later you say "For some math items: …". But they seem to be about physics. My guess is that the second one you will replace the word "math" with the word "physics".

   Jim Akerlund

2. **Topologist Guy**
   December 7, 2021

   Machine Learning:

   Is this the problem posed by Scholze that you mentioned, Peter?

   https://xenaproject.wordpress.com/2020/12/05/liquid-tensor-experiment/

   So it seems to me that the problem was to formalize/verify Scholze's proof of a theorem in condensed math—not to prove a conjecture. I don't think the computer contributed any original or creative insight here. In principle, the formalization could have been done with a pen and (a lot of) paper, so the computer here served as more of a digital bookeeping mechanism than an artificial intelligence! But I am woefully unknowledgable about this whole project, so if the computer supplied some crucial step in Scholze's proof, I would be glad to hear it!

   Like you, I find the prospect of machine learning surpassing human beings in our mathematical pursuits quite depressing. I am confident however that us humans will continue to supply creative insights and perspectives on hard problems that machines are incapable of providing. Could computers have originated schemes and etale cohomology? I find that very hard to imagine.

   Education:

   I don't support the California reforms whatsoever. In particular I am appalled by the notion of doing away entirely with programs for gifted students, not least because of my memories of being a child prodigy and forced to endure fourth-grade mathematics. At the same time, I question whether calculus should be the goal of a high-school math education. As a mathematician, it seems like a rather arbitrary goal. If I were to reform math education in America—indeed, to rebuild it from scratch—I would spend a lot more time inculcating a sense of logic and the skill to read and write proofs in abstract mathematics. I see no reason why you can't teach set theory, ZFC, infinite cardinals/ordinals, topology, groups/rings/fields or even differentiable manifolds and some of the basic elements of Lie groups and representation theory to high-school students, if they were taught appropriately. It would be a much better use of time than explaining how to compute the integrals of different rational functions by hand, and it's certainly more interesting, beautiful and foundational material.

3. **Rando M.**

December 7, 2021

Regarding the quantum information theory point, did you see that at Strings 2021 this year, Witten said that quantum information theory could lead the way to an explanation of what string theory "really is"? And then he got into an argument with Cumrun about it? I feel like that for me was the final demonstration of what an all-around waste of time string theory has been and now they're trying to shoehorn every popular fad and meme into it to maintain relevance. It's sad really.

P.S. not sure if you already mentioned this earlier, I'm only a casual reader, sorry if this is a redundant point

4. **cgh**
December 7, 2021

On the education item: I don't support the California reforms and lent my name to the petition. I think the issues are more subtle and widespread, though. I left academia about two decades ago but have steadily hired a large number of PhD/MSc level people from the "hard sciences" in that time. There's a consistent trend I've seen within the interview process that often separates people with US-based educations prior to university versus the others. It's stranger today as everything is "data science", "machine learning", or "artificial intelligence". I can't imagine a CV in previous times simply stating "kalman filter" or "support vector machine" or "particle filtering" so independent of a goal or outcome. I recently spoke to someone with a bunch of machine learning language wrapping a post-doc in numerical GR. I asked and was told "If I don't talk about machine learning I don't get grant money."

As a father of two children I've seen large changes in education that roughly fall into two camps: first is the wide spread adoption of "common core", similar to California, and second is the influence of all this new-fangled training that upsets the progressive (graded) aspect of education and really creates these strange ways topics are introduced. This seems to hit math and reading the most obviously. The former is the reason why I put my children in private school. My 8th grader is in algebra I. I expect that he will have calculus and linear algebra by graduation. I am less concerned with him having calculus proper and more interested in mathematical reasoning beyond the rote memorization of rules that are used in fluency during earlier years or primary and early secondary school. I can tell what they are doing, pedagogically, is trying to introduce more abstract thinking in earlier years, but it seems very inconsistent and contrived (as opposed to abstract). It is a hodgepodge of ideas that seems to be a dangerous experiment with something that was already presenting a challenge (in the US). More broadly than California I have seen efforts to level the playing field not by creating more opportunity but by removing material deemed challenging. Often this is accompanied by some kind of DEI statement which limits the debate. Open to change and evolving the curriculums but it all seems a bit to politicized and arbitrary. I've also witness new-fangled material displace material with little debate (more broad than maths, eg reading lists) And if jobs are the litmus test the reality remains that I produce many jobs for PhD level quantitative researchers and I am not hiring many people educated at US primary and secondary schools.

The comments in this blog aren't the right place for a discussion on this topic so I will stop here, but certainly wanted to voice my support and solidarity with colleagues and others mentioned above.

5. **cgh**
December 7, 2021

BTW, I recall doing some work on applying knot theory to protein folding many years back. The difficulty was in the computing power. Several years later I started building proprietary systems that were more and more powerful. There's a ton of marketing behind Google (just as there was with IBM). Not to diminish their work but when the details of AlphaGo emerged it was remarkably similar, if not identical, to a proprietary system I have been running on massively parallel GPU cluster for years. My point being that while there's been progress, largely due to speed and the availability of data, very little has changed otherwise. Sometimes we joke that it's still all OLS despite the hype. It's fun to write down Ax=b on a quantum computer. After a little while it becomes clear that it's a perfectly good classical problem whether or not we have error correction and massive qubits.

6. **Peter Woit**
December 7, 2021

Jim Akerlund,

Fixed.

All,
Sorry, but I can't moderate a general discussion of everyone's ideas about how math should be taught. If it's not very specifically about the Open Letter, it's off-topic.

7. **Peter Woit**
December 7, 2021

Rando M.,
I did mention this on the blog previously, see the end of this posting:

https://www.math.columbia.edu/~woit/wordpress/?p=12381

8. **DRLunsford**
December 7, 2021

Kontsevich and Segal paper https://arxiv.org/abs/2105.10161

-drl

9. **Eitan Bachmat**
December 7, 2021

Dear Peter
The Scholze result verification has nothing to do with ML, the basic "technology" in proof assistants is type theory which is strongly related to mathematical logic. These systems have had a series of magnificent achievements in recent years . ML has obviously also been successful but, as pointed out, this seems to be the first time that it yields plausible and serious math conjectures. In short, both developments are serious, the first is not "depressing", regarding the second, I sympathize with your feelings, but that's life.

10. **Peter Woit**
December 8, 2021

Eitan Bachmat,
I understand that the proof assistant business is different than the ML business, but I fear that in both cases there's not much I can do about my reaction to seeing the best human minds unable to compete with the machine.

On both fronts there's probably a lot of hype and the best human minds are still in many ways far ahead of the machine. But rather than looking into this in detail, seems like a good idea to just avoid thinking about it…

11. **anon**
December 8, 2021

There's actually a source for the ICM 2026 news: the latest IMU Circular Letter to Adhering Organizations. See here: https://www.mathunion.org/fileadmin/IMU/Publications/CircularLetters/2021/IMU%20AO%20CL%2025_2021.pdf

12. **Anonymous**
December 8, 2021

Peter, I wouldn't think of it as "human minds being unable to compete". Unlike Chess or Go, the goal of mathematics is inherently open-ended, being exploration and discovery based on aesthetic taste (even if sometimes motivated by practical concerns). Computers will surely play an increasing role in assisting humans, both by reducing tedium and aiding exploration, but there are sufficient depths unreached for this to mean richer mathematics rather than a trivialisation of the field.

13. **Will Sawin**
December 8, 2021

I think there is a plausible theory that one of the two machine-learning-in-mathematics success stories you mention was not really a machine learning breakthrough, if you take the point of view that to count as a success of machine learning it must not be something that could have been achieved with a similar amount of effort using more classical statistical tools like linear regression.

Specifically, the knot theory one – I'm not a statistician, but Figure 3b in the Nature paper looks like the kind of linear correlation between two variables of interest that could be found by doing a simple linear regression of all the algebraic invariants on all the geometric invariants, and then the second relevant variable could be identified by further analysis, like adding quadratic terms.

Even if I were a statistician, there would be something silly about complaining that you could have achieved something a different way when, well, you didn't, but given that advances in mathematics achieved by machine learning (justifiably) get significantly more attention than similar advances in mathematics achieved a different way would, I think some effort must be put into considering this possibility.

For the representation theory one, my guess (again, not a statistician) is this is probably not true. The message-passing neural network is an elegant system for learning properties of graphs, and I don't know of any classical statistical tools that seem likely equally helpful.

I don't think the proof assistants used in the Liquid Tensor Experiment involved any machine learning, although machine learning tools in that setting are being worked on and I imagine they will be useful tools for computer-verified mathematics researchers soon.

But overall I don't think the position of hoping that machine learning won't help humans do mathematics is really tenable. Is mathematics really that much harder than chess? Of course quite a lot of what has already been written is hype, including some works by authors that seem to understand neither the field of mathematics they are working in nor the basic principles of statistics. But saying that the field is inherently hype because of that is like saying Florida doesn't exist because there were a lot of Florida real estate scams.

14. **Peter Woit**
December 8, 2021

Will,
I should make clear that I'm not saying these stories are hype, quite the opposite: what's bothering me is that they appear to not be hype. Maybe the future of mathematics is in close collaboration between human minds and computers, but I'm personally not onboard with that, just because to my mind we're all already too close to computers.

15. **Peter**
December 8, 2021

@Peter Woit – the story with proof assistants is very far from 'the human cannot compete with the machine'. We are seriously far from being able to use proof assistants for helping with pretty much any act of creation of proof.

However, if you have created a long and complicated proof, where you perhaps have a great many cases which you really hope are exhaustive, or where the definitions are subtle and being used in ways which are close to the limits, you might reasonably be worried that you are missing something.

You could deal with that worry by ignoring it (as is traditional), but this leads occasionally to errors, including sometimes plain false theorems. Or you could deal with it by blowing up your 200-page human-readable paper to 2000 pages which a human can in principle read but in practice won't, by filling in every single detail and being very careful to do every little calculation explicitly. Or, now, you can spend a still very large amount of time (much longer than writing 200 pages, probably a bit less than writing 2000) by putting all the details to a computer and asking it to formally verify the proof.

The major gains at the moment are that you can be fairly confident the computer's answer is accurate, and you can afford to write inelegant stuff and skip the prose for the proof assistant, because no human will read it and judge you. And (minor) for a class of basic calculations the proof assistant can do it for you. The major loss (which hopefully will get better over time) is you have to teach the computer a lot of stuff that would be assumed knowledge for a journal paper, but which no-one previously got around to putting to the proof assistant's knowledge.

I'm sure the ML community thinks that one day they will bolt ML on to proof assistants to replace mathematicians, but this day is definitely not here yet. One major problem, compared to games, is that when you play a game you have two sides, and the side that plays the better strategy wins – you can start with a pathetic strategy, try to learn better responses, and repeat millions of times to get somewhere, with these millions of repeats being the computer playing itself with different variations of its current strategy and seeing which things work better.
In mathematics (and most things, actually) you don't really have this. You can ask the computer to prove something; if its strategy is pathetic, it will simply fail and there is no good way to say 'now improve' in any directed way, it doesn't get a score for its failure (at least not without a human intervening) to say which variation of strategy was best. Similarly if it succeeds it's not clear how to give the computer a 'next problem' that's at the right level to get improvement, again at least not without human intervention; and humans simply don't have time to intervene all the time at the rate current ML programs can learn.

16. **Robert A. Wilson**
December 8, 2021

Perhaps as a mathematician who has been involved in computer-assisted mathematics for more than 40 years I might be allowed to comment? At that time, computers could not be said to provide artificial intelligence. What they provided was artificial stupidity. They were therefore very useful in augmenting our natural supply of stupidity, and making far more mistakes far more quickly than we could do ourselves. This was, and still is, a huge benefit to mathematicians, and speeds up progress in all sorts of ways.

I may be wrong, but my impression is that a lot of what is called artificial intelligence these days is really just a more sophisticated version of artificial stupidity. And making connections that people haven't seen before is exactly what artificial stupidity should be expected to do: if people make these connections, they reject them as stupid, and they are lost. It is only because they are made by "artificial intelligence" that people take them seriously, and think about them.

17. **Sabine**
December 9, 2021

Thanks for the mention.

Regarding your question about the "relevance" of quantum information. It's the easiest way people in the foundations can get funding through the many current quantum initiatives. We'll therefore almost certainly see more of this, also quantum simulations (you know, wormholes and LQG and that kind of thing).

18. **Mark Hillery**
December 10, 2021

Peter,

You may very well consider this off-topic, and if so, I am sure you will let me know.

It is interesting that quite a few people from Stanford have signed the Open Letter on K-12 Mathematics, since the main person behind the new California Mathematics Framework, Prof. Jo Boaler, is at the Stanford Graduate School of Education. Boaler had a serious run-in with mathematicians in the early 2000's. Wayne Bishop, R. James Milgram, and Paul Clopton criticized her work and accused her of scientific misconduct, a charge that Stanford dismissed. Anyone interested in the details of the criticisms can find them here: https://www.nonpartisaneducation.org/Review/Essays/v8n5.htm and https://nonpartisaneducation.org/Review/Essays/v8n4.htm . According to Bishop, et al., one of the main effects of Boaler's curriculum changes at a high school that was part of a study she ran was to significantly increase the number of students from that school who needed to take remedial mathematics courses when they got to college, just the kind of thing the people behind the open letter are worried about.

19. **Peter Woit**
December 10, 2021

Mark Hillery,

Not off-topic at all. For more about Boaler, someone pointed me to this

https://fillingthepail.substack.com/p/tessellated-with-good-intentions

20. **Richard**
December 17, 2021

On the subject of math news — very sad news, as it happens — Jacques Tits, a distinguished mathematician who won both the Wolf and Abel Prizes, has died (On Dec. 5, 2021, at the age of 91.).
Though I did not know him personally (Nor can I claim any detailed knowledge of his work.), I do not hesitate to say that — as both man and mathematician — he was very highly regarded.
In particular — in regard to matters mathematical — he will be remembered for his work on buildings (Curiously enough, "building" is an actual technical term — as is, I believe, "apartment"!).
I believe he may also have been the first to claim a sighting of what is generally regarded as the unicorn (Some might say "Loch Ness Monster" or "Bigfoot" would be more apposite monikers!) of abstract algebra: the legendary * field with one element * (An idea that, to the best of my knowledge, is still viewed by most mathematicians as less-than-fully fleshed-out — and is arguably as close as math gets to the sound of one hand clapping.).
I believe he also helped guide the mathematical development of a young Pierre Deligne.
He will be missed.

21. **Thomas**
December 17, 2021

A short and moving obituary by Broué at the Société Mathématique de France :
https://smf.emath.fr/smf-dossiers-et-ressources/broue-memoriam-tits

22. **Robert A. Wilson**
December 24, 2021

I would like, if I may, to comment on the achievements of Jacques Tits in regard to the unification of mathematics. He did more than anybody else to unify the fields of finite, algebraic and Lie group theory, and the corresponding finite, algebraic and differential geometries. Tits's geometries, buildings and all the rest of it seem to be regarded as somewhat irrelevant to mainstream differential geometry, but this is far from true. He saw, more clearly than anyone, the fundamental structures that are central to Klein's vision of a unified algebra/geometry. It is a pity that others have chosen to use their power and influence to drive a wedge between algebra and geometry, which Tits sought to unite. I met him a few times in my career, and he was unfailingly encouraging and appreciative. I was astonished to find that not only did he know of my work, but he knew it well, and appreciated it for what it was.

23. **Robert A. Wilson**
December 29, 2021

This may be a bit off topic, but there is a really nice obituary of another of the giants of mathematics, John Conway, at
https://royalsocietypublishing.org/doi/10.1098/rsbm.2021.0034.
In terms of physics, there is mention of the relationship of the Conway-Norton Monstrous Moonshine to string theory via the work of Richard Borcherds, and also of the Free Will Theorem proved with Simon Kochen. The former, I suspect, has little to say about real physics, but the latter is a close relative of Bell's inequality, and I suspect is fundamental, although clearly it has nothing whatever to do with free will.

24. **Heiko242**
January 4, 2022

Also semi-topic, (but since the Bogdanov affair was once extensively discussed here): Both Bogdanov-Twins have now died due to Covid.

More Math and Physics Items

December 18, 2021
Categories: Uncategorized

Yet more math items:

- First of all, congratulations to my colleague Johan de Jong, recipient of the 2022 AMS Steele Prize for Mathematical Exposition. Johan's Stacks Project is very much deserving of such recognition. It's both huge in scale and very high in quality, with nothing else really comparable. While it has attracted many contributors, it has always been mostly a one-person effort. If you're interested in helping, even those not so expert in the field can contribute by fixing any mistakes they might find when using this incredible resource.
- On my currently favorite topic of the unity of math (and physics), there's a talk by Barry Mazur, in which he begins by raising the question "What is it that unifies Mathematics?". He goes on to turn around the question "What is the physical interpretation of the Jones polynomial?" asked by Atiyah (and answered by Witten's Chern-Simons theory). Mazur asks:

  > What is the Arithmetical-Algebraic-Geometric interpretation of the Jones polynomial?
  >
  > or of Chern-Simons theory?
  >
  > or of TQFT?

- Mazur's title is "Bridges between Geometry and Number Theory". The metaphor of "bridges" to describe what unifies mathematics gets a workout in a recent Quanta article about Ana Cariani and the Langlands program entitled The Mathematician Who Delights in Building Bridges (and subtitled *Ana Caraiani seeks to unify mathematics through her work on the ambitious Langlands program*.)
- At the same conference as the one with the Mazur talk, Maxim Kontsevich spoke on Geometry from the perspective of quantum mechanics and string theory. His talk was a great summary of various aspects of the problem of quantization, in both quantum mechanics and conformal field theory. There wasn't much though about what has been going on since the early developments in conformal field theory that he discussed. Things got a bit worrisome at the end, when he announced that he can't understand Kevin Costello these days (if he can't, who can?), and ended with (here's a google-aided transcript):

  > You see that gauge theories and gravity appears in various interactions is it's in nothing else in a sense, and geometric limits of various string theories or quantum field theories and what I claim that it's in fact it's something generally about complex systems and mathematics. You do some combinatorial problem, whatever it is you get some counting or something, and then maybe you look on asymptotic growth of the number of solutions. It could be something very simple but your arranged parameters became something more complicated and if you see something more complicated it's kind of I think it's unavoidable you see some physics in a very wide sense: some string theory, some membranes, whatever. Okay, thank you.

  I can't really make much sense of this, but he seems to have some sort of vision of fundamental physics being linked with complexity, a point of view that seems increasingly common, while not leading anywhere promising.

Moving to purely physics topics:

- Noah Miller was a student here at Columbia in one of my mathematics of QM courses. I've had some wonderful students in those classes, and he was one of the best. He has gone on to graduate study in physics at Harvard, and I just saw a beautiful new paper by him this week on the arXiv, From Noether's Theorem to Bremsstrahlung: a pedagogical introduction to large gauge transformations and classical soft theorems. It's an exposition for non-experts of some of the new ideas about gauge symmetry and physics that Strominger and collaborators have been working on, highly lucid and readable.
- I very much recommend taking a look at the talk from earlier this year by Mikhail Shaposhnikov, Conformal symmetry: towards the link between the Fermi and the Planck scales. Shaposhnikov has done a lot of fascinating work over the years, developing in detail a point of view which hasn't got a lot of attention, but that seems to me very compelling. He argues that the SM and GR make a perfectly consistent theory up to the Planck scale, with the "naturalness problem" disappearing when you don't assume something like a GUT scale with new heavy particles. Watching the discussion after the talk, one sees how many people find it hard to envision such a possibility, even though all experimental evidence shows no signs of such particles. For more about what he is in mind, see the talk or some of the many papers he's been writing about this.
- Finally, skydivephil tells me he has managed to get David Gross and Carlo Rovelli to debate string theory vs. loop quantum gravity, with video to drop on Youtube tomorrow. I normally try to make it a policy to avoid getting into this particular debate, but this I have to see. While you're waiting for this, you can watch an earlier pairing well worth seeing: Alan Guth and Roger Penrose debating the multiverse versus cyclic cosmology.

Update: I just watched the Gross/Rovelli debate, and thought Rovelli did a good job of making the case that string theory is a failed research program. Gross spoke uninterruptedly at length, but interrupted Rovelli constantly. I found it interesting that Gross acknowledged "supersymmetry hype" and hype back in 1984-5, while at the same time engaging in massive amounts of hype about the current state of string theory. On the time scale for progress in string theory, he says 80 years (end of the century) to understand how to use string theory to solve QCD, no time scale for getting unification out of string theory.

Gross's main point he kept repeating is that "string theory" now means an overarching framework that includes the Standard Model, so there's no distinction between the Standard Model and "string theory" and you can't argue that "string theory" is a failure. This argument is so silly that it's hard to engage with it in any sensible way, and Rovelli didn't even try.

Update: There's an interesting long interview with Andy Strominger here. Some of this brought back old memories, since Strominger overlapped with me a bit as an undergraduate at Harvard, although the story of that part of his life is very unusual. I hadn't realized the extent to which from the very beginning he was focused on the problem of quantum gravity, which to some extent explains his lack of interest in particle physics extensions of the Standard Model.

One thing he makes clear is that at this point string theory has become completely disconnected from the possibility of saying something testable about the real world. The AIP interviewer kept trying to ask about that, leading to this exchange:

> **Zierler**: So is there an experiment that you can conceive of that could disprove string theory?
>
> **Strominger**: I guess I am not getting my point across.
>
> **Zierler**: You're saying that string theory is totally outside the world of experimentation.
>
> **Strominger**: … So yes, I don't think – not many string theorists will talk this way – but I don't think that we are in my lifetime — and I'm planning to live a very long time — going to get direct experimental evidence for string theory.

On the issue of what the terms "string theory" now mean. Strominger makes it clear that from the point of view of him and many others, there's no longer any possible critique of "string theory" as a fundamental physical theory:

For the last 30 years, everything new that we've discovered, as long as we can relate it to the ideas in string theory, we call it string theory. So, if we continue to call everything that we discover string theory, it's virtually certain that– (both laugh) It's certain that when we get to the answer, we'll call it string theory!

Comments

1. **lun**
   December 19, 2021

   Shaposhnikov's lecture is about https://arxiv.org/abs/2110.05175
   right? They seem to completely ignore the issue of triviality of the theory
   (in the sense of https://en.wikipedia.org/wiki/Quantum_triviality)

2. **Peter Woit**
   December 19, 2021

   lun,
   No, the talk was not about that recent paper but about Shaposhnikov's long-standing research program. He has lots of relevant papers, for an early one, see https://arxiv.org/abs/0708.3550

   The point he is making about no hierarchy problem if you have no GUT scale is a very simple one: if you just look at the SM and gravity, the only energy scales are electroweak and Planck. Since you don't actually have a viable theory of what is going on at the Planck scale, the only thing you have to go on is a single-scale theory, and that doesn't have the fine-tuning problem you get when you try to maintain two very different energy scales. Taking into account both scales, he suggests a possibility is that a unified theory just has one scale (Planck), with the electroweak scale an exponentially small non-perturbative effect.

   So, the hierarchy problem appears when you write down a GUT theory, or a unified theory including gravity which has Planck-scale states. Conventional GUT theories and string unification models have this problem, but there's also zero evidence for them.

   Besides this simple point, he has a well-developed research program dealing with the issue of gravity and many others. I think it's extremely interesting and should be getting a lot more attention than it does.

3. **Johan Smit**
   December 19, 2021

   I wonder what Cumrun Vafa and the people working on the Swampland would think about David Gross's redefinition of string theory to encompass all of theoretical physics.

4. **Peter Woit**
   December 19, 2021

   Johan Smit,

   I think most string theorists are well aware that the argument Gross makes about string theory and the SM doesn't pass the laugh test. Gross didn't mention the "swampland" argument, I'd guess that he (like most string theorists) doesn't find it a compelling as an argument for string theory.

5. **Bertie**
   December 19, 2021

   Skydive Phil has a poll up on twitter as to who won the Gross/Rovelli debate.
   IMO Gross did his cause no favours in hogging the conversation then getting all defensive when Rovelli spoke.

6. **WD**
   December 20, 2021

   After watching the Rovelli/Gross debate, I couldn't help but feel somewhat sorry for Gross. Quite frankly, he came across as somebody well past his best, and desperately clinging to old ideas. Also, his body language was borderline disrespectful. I was not impressed.

7. **Peter Woit**
   December 20, 2021

   WD,
   I thought Rovelli handled this very well, comparing Gross to Einstein: a theorist who did extremely important work early in his career, but ended up in the last decades of his career chasing a failed research program.

8. **anonymous**
   December 20, 2021

   "For the last 30 years, everything new that we've discovered, as long as we can relate it to the ideas in string theory, we call it string theory. So, if we continue to call everything that we discover string theory, it's virtually certain that– (both laugh) It's certain that when we get to the answer, we'll call it string theory!"

   So when will string theorists start to call number theory and algebraic geometry "string theory", if they continue to discover and/or prove things in number theory and algebraic geometry like they have in the past 30 years?

9. **Peter Woit**
   December 20, 2021

   anonymous,
   Wouldn't surprise me if there were physicists now calling algebraic geometry "string theory". Number theory however I think is safe from this for now.

10. **SRP**
    December 21, 2021

    Soundsl like string theory is the opposite of (pre-boom) AI, where once a task was successfully automated we stopped calling it"AI."

11. **Dimitris Papadimitriou**
    December 21, 2021

    Andy Strominger's interview was really interesting.
    He seems to have an exciting life (and not only in physics!).
    The era between the 60s and 70s were a unique period in the history of humans. (I'm a bit jealous!).
    It seems also that he's a quite honest and unpretentious person. Somewhere in the middle of the interview he mentions that most string theorists did not understood very well GR back then, and this is quite true:
    For a long period of time he and Gary Horowitz were among the few in the strings community that had a deep knowledge for the theory of gravity.
    Untill recently, some of them had a weird, almost hostile attitude towards some aspects of GR, as in the case of firewalls for example.
    They also underestimated the importance of the dynamical internal spacetime geometry of black holes, at least before the important paper of Christodoulou/ Rovelli (about the growth of the slice independent maximal volume of the black hole interior), that revealed a potential danger for holography.

12. **Justin Glick**
    December 22, 2021

    Related: https://cerncourier.com/a/witten-reflects/

13. **Anon**
    December 22, 2021

    Witten on the LHC
    https://cerncourier.com/a/witten-reflects/

14. **sdf**
    December 22, 2021

    News to me from the title slide of the Kontsevich lecture: "Laurent Lafforgue, Huawei"!!

15. **Z Y**
    December 27, 2021

    sdf,

    Funny coincidence, I came today to this blog specifically to see if there was any comment regarding Lafforgue's move to Huawei 🙂

16. **Mike M**
    December 28, 2021

    Oof, the Gross/Rovelli "debate" was tough to watch. I agree with Bertie's comments above. It really seemed like Rovelli showed up prepared for a thoughtful debate, while Gross' approach was much more casual. Early on when Rovelli politely asked to be permitted to give uninterrupted opening remarks, Gross countered with "Well, I want to be able to make corrections along the way…" which is kind of a microcosm for the larger cultural problem.

    I like Skydivephil's channel, but I get a bit nervous about debates/conversations like this in the public view. Could a novice viewer could walk away with the impression that "physics" is really just a lot of fancy semantic arguments.

Witten Goes Anthropic

December 22, 2021
Categories: Multiverse Mania

Multiverse mania started seriously among string theorists around 2003, with a defining event Susskind's February 2003 [The Anthropic Landscape of String Theory](). At the time I was finishing up writing what became the book "Not Even Wrong", and my reaction to Susskind's paper was pretty much "This is great! Susskind's argument implies that string theory can't ever be used to predict anything. If people accept that, they'll have to give up on string theory since it has come to the end of the line." Over the next year or two it became clear that devotion to multiverse mania wasn't just localized at Stanford (where Andrei Linde had always been pushing this, even before the string theorists climbed aboard). Other proponents of the string theory landscape were up and down the California coast, including Raphael Bousso at Berkeley and Joe Polchinski at UCSB. One West Coast holdout was David Gross, who [that summer at Strings 2003]() quoted Churchill's words to his country during the Nazi bombardment of London: "Never, never, never, never, never give up". On the East Coast, the center of the resistance was at the IAS in Princeton, where several people told me that Witten was privately strongly making the case that this was not physics.

I ended up adding an additional chapter to the book about this, and [covering developments closely here on the blog](). For many years I found it impossible to believe that this pseudo-scientific point of view would get any traction among most leaders of the particle theory community. How could some of the smartest scientists in the world decide that this was anything other than an obviously empty idea? After a while though, it became clear that this was getting traction and that there was a very real danger that particle theory would come to an end as a science, with most influential theorists giving up, justifying doing so by claiming they now had a solid argument for why there was no point in trying to go further. String theory is the answer, but the answer is inherently unpredictive and untestable.

It has become clear recently that we've now reached that end-point. From the [new video of his discussion with Rovelli](), it's clear that David Gross has given up. No more complaints about the multiverse from him, and his vision of the future has string theory solving QCD 80 years from now, nothing about it ever telling us anything about where the Standard Model comes from. Today brought an extremely depressing piece of news in the form of a [CERN Courier interview with Witten](). Witten has also given up, dropping his complaints about the string theory landscape:

> Reluctantly, I think we have to take seriously the anthropic alternative, according to which we live in a universe that has a "landscape"of possibilities, which are realised in different regions of space or maybe in different portions of the quantum mechanical wavefunction, and we inevitably live where we can. I have no idea if this interpretation is correct, but it provides a yardstick against which to measure other proposals. Twenty years ago, I used to find the anthropic interpretation of the universe upsetting, in part because of the difficulty it might present in understanding physics. Over the years I have mellowed. I suppose I reluctantly came to accept that the universe was not created for our convenience in understanding it.

I've never really understood the kind of argument he is making here, that the problem with the string theory multiverse is that it's upsetting, but we just have to get control of our feelings. Feelings have nothing to do with it: the problem is not that the idea is upsetting, but that it's vacuous.

The rest of the interview is also pretty depressing. At the high energy physics experimental frontier, Witten promotes "split supersymmetry", something which does little more than try to keep on life support failed ideas about supersymmetry and "naturalness":

> There is also an intermediate possibility that I find fascinating. This is that the electroweak scale is not natural in the customary sense, but additional particles and forces that would help us understand what is going on exist at an energy not too much above LHC energies. A fascinating theory of this type is the "split supersymmetry" that has been proposed by Nima Arkani-Hamed and others.

On string theory, he follows Gross in referring to not "string theory" but "the string theory framework" and describes the situation as

> We do not understand today in detail how to unify the forces and obtain the particles and interactions that we see in the real world. But we certainly do have a general idea of how it can work, and this is quite a change from where we were in 1973.

The situation with string theory unification is that it's a failed idea, not that it's a successful general idea just missing some details.

Finally, Merry Christmas and best wishes for the New Year. Fundamental physical theory may now be over, replaced with a pseudo-science, but at least that means that things in this subject can't get any worse.

Comments

1. **Jackiw–Teitelboim**
   December 22, 2021

   Interesting "developments" in less than a week: Gross' demonstrating his degree of rudeness is unbounded, particularly with Rovelli's (BTW I watched Penrose/Linde's debate at same channel, maybe Penrose is too just much of a gentleman to compare with but… well, poor moderator!); Strominger's story about Witten's hope on electron/muon's mass ratio; and, of course, Witten's speaking about "different portions of the quantum mechanical wavefunction".

   I am not going to start a debate about Everett's interpretation, but mixing stringy/eternal-inflation multiverse cosmologies with a very debatable interpretation of quantum mechanics is bearing not in pseudo-science, but poor judgment.

   Let's hope next year Ed can make another contribution at the same level as in 1994 with Seiberg.

2. **Paolo Bertozzini**
   December 22, 2021

   Dear Peter,
   as regards Witten's interview, there might be some other reasons why the "multiverse mania" is no more the main center of discussion … when questioned about "Which current developments in theory are you most excited about?", Witten clearly stated that the "It From Qbit" ideas connecting gravity and quantum theory, via the insight from "holographic duality" on the relation between gravity and gauge theory, is the new game in town. As far as I understand, this line of research is only very mildly related to the original string theoretic program (through the *original* AdS/CFT duality).

   As regards the discussion between Gross and Rovelli, personally I did not like it very much … maybe because the only sincere agreement that I could spot was on the common fashionable criticism of mathematical motivated investigations ("Lost in Math" in direct opposition to sound theoretical ideas describing the "true in natural reality"). So … for Rovelli, string theory is beautifully lost in sterile mathematics … and for Gross fundamental physics outside string theory is missing the true messages from the real world accumulated wisdom in the study of the standard model unification (that He seems to consider to be somehow a prediction of string theory). *Both* points above (that string theory is "mathematically elegant" or "clearly motivated by experimental insight") are for me quite problematic.

I find the pairing of “naturalism/realism” with “divorce from mathematical soundness” much more troublesome for theoretical physics investigation, compared to an anthropic/multiverse mania that, after all, has always been around in different philosophical shapes since Bruno, if not much earlier.

Things might still get worse 😉
Merry Christmas and Happy New Year 2022.

3. **anonymous**
December 22, 2021

Why is it that the likes of Gross and Witten never think that there is a third alternative, that one gives up string theory instead of giving up their critique of the multiverse/anthropics?

4. **Ron Martin**
December 22, 2021

The best case scenario I see now for theoretical physics is that as people move on from string theory to other hotter areas of study in theoretical physics (such as gauge/gravity duality and the It from Qubit program), string theory gradually fades into irrelevance as its current defenders die out and new research programs take over. Worst case scenario is that the current string theorists, despite moving on to other fields, still nevertheless manage to catechise a new generation with the belief that “string theory is the only way to fundamentally explain the universe”, who then go on to perpetuate this belief indefinitely far into the future making string theory into essentially a new religion.

5. **Johan Smit**
December 23, 2021

Paolo Bertozzini,

Even worse than either the “multiverse”/anthropics or the disentanglement of “naturalness” and “mathematical soundness” is the redefinition of string theory to cover more and more of theoretical physics, like what Gross, Strominger, Witten and presumably other string theorists are saying in their interviews, lectures, and debates, i.e. quantum field theory (especially conformal field theories and their duals) is string theory, condensed matter physics is string theory, quantum information science is string theory, even parts of mathematics is string theory. At some point, the term “string theory” itself will become such a general term that “string theory is true” will become a tautology, which would make no sense to anybody even a decade ago.

6. **Sabine**
December 23, 2021

Contrary to what most particle physicists believe and will tell you (and have written), arguments from naturalness never worked as predictions. The one case where that isn’t obvious is the charm quark but what they called “naturalness” in that prediction is not what they call (technical) naturalness today* (it’s rather an argument from simplicity – pairing up the quarks increased predictive power). As Witten correctly notices, naturalness turned out to be just wrong: neither the CC nor the mass of the Higgs boson is technically natural.

What particle physicists should do at this point is

(a) abandon naturalness rather than trying to bring it back in modified fashion and
(b) understand how it could possibly happen that so many particle physicists believed in a criterion that’s so obviously unscientific as this, and then
(c) prevent the same problem from happening again for other unscientific ideas

As to the multiverse. This idea is promoted by people who mistake math for reality and who don’t understand what a scientific explanation is. Saying that all values of a constant “exist” if you can only ever observe one value is both scientific and philosophical nonsense, and if you, after you’ve said that, still have to assume the constant has that value which you actually observe, then you haven’t explained anything. Re-expressing a set of constants by the maximum of some probability distribution just adds a completely superfluous story.

Also, I have noticed a few times that the title of my book “Lost in Math” has taken on a meaning on its own. I want to stress that the message of my book was NOT to criticize the use of mathematics in physics, but rather to say that physicists do not take math seriously enough.

Merry Christmas everybody 🙂

* Incidentally, I got this wrong in my book. I have regrets.

7. **Eric Weinstein**
December 23, 2021

Nice Dylan reference Peter.

If I might try something a bit out of pure physics, it is worthwhile asking why stagnation issues are afflicting the most theoretical disciplines across different fields. Neo-classical theory also became the dominant paradigm in economic theory as did the neo-darwinian synthesis in evolutionary biology.

In all these cases, it feels like there was something special about the particular time when the idea hardened into dogma. I didn’t get to watch biology or economics when this happened. But in physics it was clearer because it happened more recently. I don’t think we could have a new dogma now. No person on the scene is invested with sufficient power at this moment to create dogma the way String Theory became nearly religious in the devotion it inspired in its 1980s practitioners.

My sense is that this phenomenon of a field becoming dogmatic is personality dependent. Arrow, Samuelson, Friedman etc were all very different titans of economics. In biology, Triver, Williams, Dawkins, Alexander etc. were similarly giants. In Physics, we have only had Witten at that level for a long time (1980-2000 ish) with support from Juan, Nima etc in physics and Atiyah, Singer, Quillen, Segal, Freed, etc in math. But it felt for a long time that, often enough, only Witten fullly understood the plot.

So my thought is that this is a bit like Greenspan admiting that, as the former oracle, he didn’t know fully what he was doing with the economy after all. He was all alone in that. Likewise, I fear it’s not really about the leadership of Witten, Gross, Vafa, Strominger, Seiberg etc. It’s really about Ed. Many years ago Joe Polchinski said to me ‘You keep mentioning String Theory Eric, but I am never sure whether String Theory really exists as a proper discipline or community…or whether we are merely runnning sub-routines for Ed.’

I don’t know what to make of this. But it seems like a pretty important moment for the health of the field to free your younger colleagues and do what Greenspan did: admit you made a serious error in leadership judgement.

I honestly don’t know why this group of leaders isn’t more concerned with the story of their stewardship of this field. Were I in their shoes, I imagine I might be thinking like Eisenhower did before his ‘Military Industrial Complex’ farewell address. Why not turn over a field that doesn’t have to labor under your unjustified pronouncements about the divine nature of Strings from 40 years ago? Give your intellectual descedents a new lease on life. This isn’t learning to love the landscape or appreciating anthropics after all. It’s really about admitting a catastrophic error in leadership judgment. And I think highly enough of Ed and company to think that they should do it out of love and concern for the field at their ages. But, what do I know.

“Those of us who have looked to the self-interest of lending institutions to protect shareholders’ equity, myself included, are in a state of shocked disbelief,…Yes, I’ve found a flaw. I don’t know how significant or permanent it is. But I’ve been very distressed by that fact.” -Alan Greenspan at 82

Physics is a science….so we should be able to do as well as the economists and financiers I would imagine. No? Something to think about anyway.

8. **Alessandro Strumia**
December 23, 2021

Dear Paolo, is “It From Qbit” a politically-correct way of saying “From It to Qbit”? Seasonal Greetings.

9. **Andrew Thomas**
December 23, 2021

I agree with your positive initial feelings about string theorists accepting the multiverse. It’s an admission that string theory has come to the end of the line. I’m not saying string theory is wrong, but this is the clearest possible admission that “there’s nothing to see, here”, and there’s now no longer any expectation that much of interest is going to come from continued research in string theory. Hopefully new students will take this as a coded message that they would be better off working on something else instead. It’s definitely a positive thing.

10. **Anonymous**
December 23, 2021

“Fundamental physical theory may now be over, replaced with a pseudo-science, but at least that means that things in this subject can’t get any worse”?

That brings to mind the Soviet era distinction between an optimist and a pessimist: an optimist thinks that things are as bad as they could possibly get, while a pessimist thinks things could always be worse. I’m not sure your optimism is justified, Peter, but perhaps it was meant as seasonal cheer. In any case, enjoy the holidays.

11. **Peter Woit**
December 23, 2021

Anonymous,
I was tempted to end the posting with “Merry Fucking Christmas”, but thought better of it…

12. **John**
December 23, 2021

The issue with the current state of string theory is that too many string theorists have for far too long been studying string theories that were supersymmetric, which is not true in our world, and then equated string theory with superstring theory. And then they discovered that there are at least 10^500 different superstring theories, and decided that the multiverse and anthropic reasoning was necessary to explain the 10^500 superstring theories, all while ignoring that none of the multiverse or anthropic reasoning is even necessary because our world isn’t supersymmetric so none of the 10^500 theories even matter. And this has become especially apparent after the LHC results showed the non-existence of supersymmetry in the real world.

If string theorists were actually serious about string theory as a fundamental physical theory, they would admit that the past 40 years, since the first superstring revolution, has been a wrong turn in the string theory research program, and then start over and begin searching for a consistent and stable non-supersymmetric string theory that has all the Standard Model particles and a spin 2 particle for gravity. Instead, the previous string theory holdouts against this multiverse nonsense, especially Ed Witten, the leader of the first superstring revolution, have not only failed to admit that superstring theories are a failed idea and that people should pursue alternative non-supersymmetric string theory approaches, they also have accepted the multiverse and largely given up on the string theory research program entirely, moving on to other fields such as the gauge/gravity duality research program in quantum field theory. Ed Witten and David Gross may say that a century into the future and we might see a string theory develop that might explain the Standard Model or QCD, but if nobody is working on (non-supersymmetric) string theory due to the acceptance of the multiverse and anthropic reasoning in the string theory community, I don’t see how string theory would make any progress in a century from where it is today.

13. **Mitchell Porter**
December 23, 2021

I would ask the commenters on this blog, who so dislike multiverse ideas and the anthropic principle and especially their combination… Do you have any thoughts on the possibility that that is how nature is? Do you just think that’s impossible or unlikely; do you think it is possible, but if it is so, it is inherently beyond the reach of science; or what?

I will add that I am not objecting to the pursuit of theories that still hope to explain reality in terms of something unique. I am just wondering how categorical the association between “anthropic multiverse” and “unscientific” is.

14. **Peter Woit**
December 23, 2021

Mitchell Porter,
Maybe there is a multiverse, maybe there isn’t. I have no idea and don’t really have any feelings about it one way or another. But multiverse theories are pseudo-science unless they come with some scientific way of finding evidence for whether they are true or not. The problem with the string theory anthropic landscape multiverse is that there is nothing of this kind, see

https://www.math.columbia.edu/~woit/wordpress/?faq=why-do-you-describe-the-multiverse-as-pseudo-science

What is going on here is that the “multiverse” is being used not as a scientific explanation of anything, but as an excuse for not admitting a theory is a failure. Calling this “pseudo-science” is actually being charitable (less charitable would be something like “dishonest bunch of crap”….).

15. **Peter Woit**
December 23, 2021

John,
Over the years many string theorists have tried to get around problems with using the superstring in 10d by finding string theories that work in other space-time dimensions or are not supersymmetric. The problem is that no one sees any way to do this. The standard theory of a non-supersymmetric string seems to need 26 space-

time dimensions and even in that case has a tachyon. From early on in the subject, trying to get around this is something many people have worked on, unsuccessfully.

16. **Michael McGuigan**
December 24, 2021

John,

There was a small resurgence of interest in non supersymmetric string theory after the LHC failed to discover supersymmetry. Much of the work was based on the tachyon free SO(16)xSO(16)' Model in 10d or non Supersymmetric orbifolds. See for example:

https://arxiv.org/abs/1407.6362
https://arxiv.org/abs/1502.03087
https://arxiv.org/abs/1907.01944

There should probably be a workshop bring interested people together to develop this idea further and see what the implications are for particle physics.

Sent from my iPhone

17. **History observer**
December 24, 2021

It has been complained many times on this blog that the young generation of physicists is dangerously being indoctrinated by the string theory-multiverse-anthropic ideas by the senior leaders of the field and/or by media. However, I find Weinstein's comment above particularly relevant for understanding the important difference between the mindset of the past century's young generation of physicists and the present day young generation of physics. Even Einstein was not taken back then as a leader in fundamental physics by the young generation of physicists. Very few followed him in his quest for a unified theory the way he *envisioned* it. That young generation found for itself the right path for progress and did not fall into delusion because of a great figure. That the present day generation is easily indoctrinated by media and/or by any senior investigator in a field actually reveals the value such a generation could in any case bring to the said field, even if it were not indoctrinated in the first place.

18. **John Peacock**
December 24, 2021

I can understand your negative outlook on anthropic reasoning in the specific context of string theory – indeed it would be a shame to abandon the aim of deepening our understanding beyond the great success of the standard model, especially if there is a risk of giving up too soon and too easily. But I can't see that this alone explains or justifies your apparent hostility to every aspect of the multiverse. The logic of using observer selection in an ensemble to explain apparent fine tuning works fine in some contexts: we certainly didn't need modern observations of thousands of exoplanets to be completely sure that they were there. Outside religion, the idea of winning a cosmic lottery is the only way to explain how the Earth happens to be the right distance from the Sun to permit life. With the cosmological constant, Lambda, we have a much stranger coincidence to explain; and in the absence of any idea of how its value might be explained from first principles, it seems reasonable to me to consider observer selection as a possible explanation. That would be true even if we didn't have strong-field inflation models as a worked example to show that it is possible to generate an ensemble of causally disconnected universes; and certainly the discussion of multiverse issues in cosmology should not be tied to the specifics of inflation or string theory. Weinberg showed us that we can still make progress without such assumptions in the case where only Lambda varies within the multiverse, and this is a testable model: it predicts the posterior for Lambda, which can be compared with the single datum of the observed value. This is hardly precision cosmology, but it's something. The existence of this calculation encourages one to think about specific issues, namely how much the efficiency of galaxy formation is suppressed in counter-factual models with increased Lambda. If the suppression is not large, then this model is ruled out and we would conclude either that there is no multiverse, or that more than one parameter varies within the ensemble. In fact, the best existing calculations indicate that the suppression is just about sufficient, and Weinberg's model survives (https://arxiv.org/abs/1801.08781). This isn't exactly a great triumph, since the other members of any ensemble cannot be observed – so yes the situation in cosmology is very different from the one in exoplanets, even if the initial logic is the same. What we are talking about here represents reaching the end of the road for experimental science – but that hardly proves that this isn't the way things are. Of course we should continue to search vigorously for unique physical explanations of the parameters in our current theories; but this has been going on with little success for long enough, and therefore it seems to me that cosmologists are entirely justified in pursuing the parallel route of seeing where multiverse reasoning can take us.

19. **Johan Smit**
December 24, 2021

John,

People have discovered that any tachyon-free string theory has to have some remnant supersymmetry, even if it is a broken misaligned supersymmetry.

https://arxiv.org/abs/hep-th/9402006

So you are out of luck if you want to abandon supersymmetry completely but keep string theory.

20. **Peter Woit**
December 24, 2021

John Peacock,
My objections to the cosmological multiverse are in the context of using it to justify empty or failed ideas about fundamental theory, and, even worse, to then use this as an argument against any attempt to better understand fundamental theory.

I don't have any objection to people positing a cosmological multiverse and seeing what they can get out of the idea. It is a problem though that people doing this often overhype what they are getting, and engage in misleading claims about implications for fundamental theory.

The Weinberg argument is a good example: as far as fundamental theory goes, it's based on assuming a roughly flat measure on the CC in the relevant region, any CC equally likely. This isn't much different than saying we know nothing about what the fundamental physics of the CC is. Somehow this gets magically turned into an argument for the string theory landscape, or for assuming there is no point to even trying to understand the fundamental origin of the CC.

21. **shantanu**
December 24, 2021

Peter: Witten also doesn't seem too excited about neutrino experiments and I am surprised that the interviewee did not ask anything about non-0 neutrino mass, which is "supposed" to be evidence of physics beyond the standard model

22. **Alessandro Strumia**
December 24, 2021

Michael, how do you get flavour from the SO(16)xSO(16)' string where the Higgs is the extra-dimensional component of a vector (so, its Yukawas are gauge couplings)?

23. **Michael McGuigan**
December 24, 2021

Allessandro,

Yes that's correct, the SO(16)xSO(16)' Model has no gauged scalars in 10d and Higgs type fields come from components of the higher dimensional gauge field in the simplest situation, with Yukawa interactions coming from higher dimensional gauge interactions, with the fermions and number of generations (flavors) related to the Euler characteristic. The other references talk about more complicated situations as well that can give rise to a Higgs type field. Like I said it would be nice to have a workshop to discuss these things.

24. **S**
December 24, 2021

John Peacock,

You write that "Outside religion, the idea of winning a cosmic lottery is the only way to explain how the Earth happens to be the right distance from the Sun to permit life," and also "What we are talking about here represents reaching the end of the road for experimental science – but that hardly proves that this isn't the way things are."

It seems to me the juxtaposition of these two shows why it's problematic for *physics* to go down this road. The fact that the alternate explanation(s) are religious also doesn't prove that *that's* not how things are — but religion isn't physics. It's perfectly possible, as you say, that there's a multiverse or what have you, but surely it's a relevant question to consider when the speculations of what might be have trailed into being equivalent to religious musings and are no longer physics?

(That's not to it's fundamentally impossible to discuss predictions of the model, a la Weinberg. My point is just that "This might just be how things are" does not mean that whatever "this" is is physics.)

25. **Lars**
December 25, 2021

"I suppose I reluctantly came to accept that the universe was not created for our convenience in understanding it." — Edward Witten

I don't understand what that is supposed to mean.

How is it a justification for accepting the anthropic principle and multiverse?

Is assuming that the physical constants, fundamental particles etc are what they are for some physical reason (ie, not just random variation of values in a multiverse that just happened to produce the conditions where humans came about) the same as assuming the universe was created for our convenience in understanding it?

I don't get it.

26. **DH**
December 25, 2021

Mitchell Porter,

I for one don't consider multiverse theories or anthropic reasoning to be inherently troubling/impossible/unscientific/etc. If it ultimately turns out that an inflationary multiverse seems to be a necessary implication of the best available evidence, then so be it–once again we become smaller, like we did with Copernicus and Hubble.

However, I share the first commenter's puzzlement that Witten seems to casually conflate this with the multiverse of Everettian QM–as though "the anthropic alternative" has any meaning/power as a concept in its own right, irrespective of the specific physical account of why a "landscape" exists & what it consists of. He says he "has no idea whether this interpretation is correct," but what he posited there is not even AN interpretation, it's a weird mishmash of completely different ideas that happen to share the (potential) necessity of embracing anthropic reasoning.

So I don't dislike multiverse ideas in general, but I also think it's borderline meaningless to discuss/assess them "in general." That there may be regions of spacetime that are wholly & permanently separate from ours, sufficiently so to consider them "different universes," seems to me a perfectly sensible idea (though I lack the expertise to assess it). A mathematical multiverse that arises from philosophically reifying the Schrödinger equation is another matter entirely (probably you can tell I think it's nonsense, but regardless, I think everyone should at least be able to agree that it's another matter!).

At times I think some of our most prominent theorists are essentially becoming Platonic idealists, while being too philosophically naive to even realize that that's what they're doing. Regardless of whether this is "unscientific," it's certainly…something.

27. **Anon**
December 26, 2021

I don't see how the multiverse explanation can work if the basic theory has supersymmetry (such as superstring thy). Wouldn't the landscape be dominated by the Minimal SUSY Std Model (MSSM) vacua as compared to the Standard Model vacua, precisely because of the hierarchy problem? Or are we imagining that in most other universes of the multiverse where there is galaxy formation/intelligent life, their Large Hadron Colliders (or even the LEP) will discover SUSY?

28. **Peter Woit**
December 26, 2021

Lars,
I think the Witten quote gives away what's really at issue. This is not about the multiverse but about whether one can claim to have a successful unified theory even if it is a theory that cannot be tested, with specific reference to the string theory and its purported landscape of vacua.

Pre-LHC, whenever Witten and others were challenged about the testability of string theory they would point to the possibility of the LHC finding at least indirect evidence for string theory (via SUSY). With that possibility now closed off, Witten has been put in the situation of having to either give up on the theory or try to defend the concept of a theory that can't possibly be tested. He's taking the second option and that's what the quoted argument is all about.

29. **Peter Woit**
December 26, 2021

DH (and others),

I agree that it's meaningless to discuss multiverse theories "in general". To have a sensible discussion it needs to be about a specific set of models (or "framework" if you like). If your argument is not about the string theory landscape set of models, please, no more. On this specific issue I've devoted a huge amount of effort over the years to trying to understand exactly how such models work, and I don't see anyway around the conclusion that this is a vacuous framework.

30. **Peter Woit**
December 26, 2021

Anon,

The anthropic argument has nothing to say about SUSY: either SUSY or non-SUSY vacua are consistent with our existence. When the string landscape program got started 20 years ago Susskind and others argued that it was predictive, that it could predict whether the LHC would see SUSY: just count the number of vacua with TeV-scale SUSY and those without. If there were a lot more of one than the other, than you would have a statistical prediction for what the LHC would see.

That never worked out, basically for the same reason the string theory landscape can never make predictions: it's not well-defined enough to allow one to know what's a string theory vacuum and what isn't. You know far too little about what the theory is that you're talking about to know how to characterize string theory vacua in general (much less count them).

31. **Johan Smit**
December 26, 2021

skydivephil has put a poll on Twitter asking who won the debate between Rovelli and Gross, and over two-thirds of the respondents said that Rovelli won:

https://twitter.com/skydivephil/status/1472635458438021127

32. **The Big Red Scary**
December 27, 2021

"Things got a bit worrisome at the end, when he announced that he can't understand Kevin Costello these days (if he can't, who can?)"

Presumably Kontsevich hasn't really tried, or he has tried and he has understood, but doesn't want to admit it since he's not the king of that hill. Andre Weil claimed not to understand schemes, but I once heard a protege of his say that, upon being left alone in Weil's office, he pulled Weil's copy of Grothendieck's EGA off the shelf and found every page full of carefully penciled remarks.

I can well imagine that Costello is hard to read for almost all physicists, and I have no idea whether it is interesting as theoretical physics, but it's fine mathematics and not so very hard to understand if you put the time into it.

33. **Steve Burton**
December 29, 2021

I believe that the correct Churchill quote is actually:
"never give in, never give in, never, never, never, never-in nothing, great or small, large or petty — never give in except to convictions of honour and good sense. Never yield to force; never yield to the apparently overwhelming might of the enemy. . . "

Here's a link to a reference:
https://www.nationalchurchillmuseum.org/never-give-in-never-never-never.html

IMO (1) the difference between "give up" and "give in" is subtle, but important, and
(2) the entire document at the link is worth a read.

Cheers!

34. **Peter Woit**
December 30, 2021

Steve Burton,
When I wrote about the Gross quote in the last chapter of my book Not Even Wrong, I did discuss the Churchill quote and point out that Gross's version was not accurate.

What struck me at the time was that Gross was comparing the threat to science from the anthropic landscape with the threat to England of defeat and occupation by the Nazis. Nearly 20 years later, what I find striking about his not mentioning this now is that by his own comparison he has decided to give in and do something comparable to accepting Nazi occupation.

35. **Paolo Bertozzini**
December 30, 2021

@ Johan Smit

I totally agree that the habit to "redefine as String Theory" whatever comes handy for "leading String Theorists" is ubiquitous and disgusting.

On the other hand, I notice that this is unfortunately a characteristic of the current cultural supremacy of aggressive marketing over anything else (most of science today is de-facto reduced to a "branch of marketing"): creating an "influential marketing channel" is an extremely difficult (often random) and resource consuming task and hence it is more and more common to have ideas, techniques or area "rebranded" on-purpose, often forgetting the original motivations and history of that subjects.

At the end of the day (if one is able to swallow His-Her own pride and ambition for recognition) the important is that certain crucial ideas achieve the attention they deserve (maybe with a different name and pursued by different groups of researchers) … the huge problem is that the hype implicit in this aggressive "marketing" often distorts the discussion, so that certain biases become almost impossible to discuss or correct: as a result, essential changes are hold back and might become effective only decades after their original inception.

One example of such "rigidity" is the usage of " (bad) excuses" to justify at all costs the validity of a research program that has otherwise failed some or all of its initial goals: this, as Peter said, is the real role of "multiverses" in this String theoretic context.

On a more positive side (as I suggested in my original post), I personally see as promising that the real focus of interest is shifting to other nearby areas (and maybe people do not even bother to make opposition to previous questionable points). For example, I am very happy to see a revival of themes from Algebraic Quantum Field Theory (as explicit case, here are the last pre-prints from Witten: https://arxiv.org/abs/2112.11614, https://arxiv.org/abs/2112.12828) … although several of these ideas/techniques could have been explored decades ago 😉

I would be very interested to know Peter's opinion on these specific works.

@ Alessandro Strumia

As we all know, "It from Qbit" is just a quantum byproduct of the "If from Bit" originating from Wheeler. "Digital Physics" has an independent tradition significantly older than anything related to String Theory (and most of all the "Quantum hype" is today even more "influential") so a "rebranding" in the above-mentioned sense might not be so easy in this case. I would completely agree that "From *It* to Qbit" sounds much more appropriate as a description of what has been actually happening 😉

Sorry for the late reply (but here it is end of the academic semester and I was really submerged with work). Happy New Year 2022 🙂

36. **Roger**
January 4, 2022

Off-topic.
RIP the Bogdanoffs. They played an entertaining and not insignificant role in the String Wars.
https://www.bbc.com/news/world-europe-59867046

37. **Cedric BARDOT**
January 5, 2022

In the CERN Courrier interview Witten says about string theory:

> This framework has turned out to be very powerful, even if one is not motivated by gravity and one is just searching for new understanding of ordinary quantum field theory.

This reminds me of a remark by 't Hooft in

> … there is ample information in what we already know, but not fully understand, even when it sounds as boring as perturbative quantum field theory and perturbative gravity in the Schwarzschild background

If "powerful" = "useful (for theoretical physicists)", I find his comment relevant at least in the current context emphasized by Paolo Bertozzini. Algebraic quantum field theory in curved space-time might experience indeed a revival with particularly pedagogical efforts done by Witten in his recent preprints to explain to youngsters (engaged in solid theoretical research in natural geometric structures) the subtleties of type III von Neumann algebras & show them why these oldies but goldies are required in trying to make some sense from (not be fulled by?) the rare computable models available to "quantum gravity" explorers.

The connection is loose but it would be funny if Leutheusser & Liu work "Emergent Times In Holographic Duality" under the scrutiny of Witten could help to make some progress in the thermal time hypothesis of Rovelli & Connes…
I wonder if Paolo Bertozzini has an educated guess on this subject.

38. **Mark**
January 6, 2022

Interesting to see in Witten's own words the devolution of the string theory research programme from a theory of unification of all the forces, to a theory of quantum gravity, to merely being a framework for providing tools to understand quantum field theory better. If so, then theoretical particle physics will have returned to the state it was before the first superstring revolution, where the dominant paradigm was quantum field theory, and everything else (including string theory) is studied for possible applications to quantum field theory.

39. **Paolo Bertozzini**
January 10, 2022

@ Cedric Bardot

I agree that "old" (algebraic) quantum field theory will have several further surprises in store (also for quantum gravity); if they will find better reception by the theoretical physics community, it is probably because the current AdS/CFT and holography trends (coupled with the impasses in string theory) are forcing people in that direction.

As regards the "thermal time hypothesis": from a very approximate first reading of the work of Leutheusser Liu, the "emergent time" evolution, although not directly coinciding with a Tomita-Takesaki modular flow (used in the Connes-Rovelli thermal time case), is anyway obtained from the Borchers-Wiesbrock half sided modular inclusions (the generator has positive spectrum) and so the connection is not that far away.

Elaborating on the deep interplay between modular theory and geometry is of course extremely tempting form me (and I would seriously risk of running out of topic). My suggestion has always been that modular theory provides a "thermal non-commutative space-time" (instead of just a classical "emergent thermal time") – I think I have been pestering Carlo in many conferences with comments on this topic 🙂

Leaving aside technical important points, the present wide consensus on the *emergent nature of space-time* in quantum gravity (that was recently mentioned by Peter on this blog) is probably the most serious obstacle for attempts in the above mentioned direction. In any case, the usage of methods from Tomita-Takesaki modular theory will continue to spread: the importance of modular theory is not related to specific models (and goes even beyond the area of von Neumann algebras) since it is an ubiquitous feature of complexification of ortho-symplectic spaces; it will likely lead to further intriguing links with *analytic continuation (Wick rotation / Osterwalder Schrader reflection positivity) and geometry* – see recent works by Longo and by Neeb Olafsson (that is another reason why I was asking Peter's opinion on such developments).

Sorry if I went a bit "tangential" 🙂

Igor and Grichka Bogdanoff 1949-2021/22

January 4, 2022
Categories: Obituaries

A few days ago I heard news from Paris of the death of Grichka Bogdanoff on Dec. 28, and this morning heard of the death yesterday of his twin brother Igor. There are many news stories online (e.g. here), and Lubos Motl has written about them here.

There's a chapter in my book *Not Even Wrong* about "The Bogdanov Affair", and quite a few blog postings here referred to the twins and their activities related to theoretical physics. The motivations for writing about them were always two-fold. That they had managed to get more or less nonsensical papers published in reputable physics journals in 2001-2 (*Annals of Physics* and *Classical and Quantum Gravity*) raised important questions about how one evaluates speculative theoretical physics research. But also, the whole story had many comic aspects (see for instance here). I always supposed that to some extent the brothers were in on the joke and I hope that was true. At one point they invited me to come see them when I was in Paris, but I decided not to take them up on the offer, since it seemed best to keep one's distance from whatever they were doing. In recent years I hadn't been following at all their activities.

There's a darkly comedic aspect to this and other examples of prominent people opposed to COVID vaccinations succumbing themselves to the disease. I'm sorry that this happened to the brothers, putting a final all too avoidable tragedy at the end of their remarkable life stories.

Comments

1. **FG**
   January 5, 2022

   To be fair, although unvaccinated, there were not proselyte about it. Instead, they seemed to have some kind of sentiment of superiority over the populace, as in "not only we are much smarter than you, but our body natural strength is also far superior and can go through anything unharmed". Perhaps, since they were well known, and highly regarded in the pseudo-science circles that the antivax seem to frequent, there deaths will open the eyes of some.

   Being in France, it is a bit depressing to read their obituaries in the general press, where science illiterate journalists present them as "doctors in theoretical physics and mathematics" most often without any mention of the vivid negative reactions in the french physics community when they were granted PhDs. And for those newpapers that mention it in passing, they tend to present it as a "controversy" (like something which is a matter of opinion, rather than a scientific issue), as if the physics circles had been split about the value of their works.

2. **cgh**
   January 5, 2022

   My memory will not be accurate on all this but spanning the years between the Sokal and Bogdanoff affairs there were a number of examples of randomly worded papers being discussed and I recall websites (one at MIT if I remember correctly) that would randomly generate scientific papers. At the time I was publishing in peer reviewed journals in areas of mathematical physics less hifalutin than string theory and it wasn't always easy. Referees challenged things, asked for wordmithing, simplification. Sometimes it was legitimate and other times I disagreed. Sometimes it seemed petty (as academics is wont to…) Sometimes it was annoying – I recall one time when I submitted two papers that I felt stood on their own but was asked to combine them by the same journal and, when I did, spent months arguing over issues related to combining disparate ideas in one paper. My point being that, for much of physics, it's an effort to get things final and published in peer-reviewed, major journals (with the old page fees), as it should be, despite the pettiness and politics of publishing original academic work. At the time it struck me as very strange that qg, hep-th, could have these types of issues. Looking back it doesn't strike me as strange.

3. **Peter Woit**
   January 5, 2022

   cgh,
   I don't want to here get into the endless topic of the problems of the refereeing system. The Bogdanoff papers were an interesting test case though for this system. I'm pretty sure the brothers thought they were doing real scientific work. They were not writing a hoax paper a la Sokal designed specifically to fool a referee, but what they were writing was nonsensical and a working referee system should have immediately picked this up.

   The criticism I often got for writing about this story was along the lines of "sure, there are lots of bad papers getting published in bad journals, so what?". Here though at least a couple of the journals (Annals of Physics and Classical and Quantum Gravity) were quite reputable ones. This test case showed that their refereeing system could not distinguish sense from nonsense.

   I think the source of the problem is deeper than that referees are sometimes lazy and incompetent. The problem of quantum gravity research that is so incoherent that it's hard to evaluate is one that has not gotten any better in the past twenty years.

   I'm resisting the temptation to start pointing to recent examples of such research that I personally can't make sense of. The problem with incoherent research is that it often requires a lot of time and effort to be sure there's not something interesting there that you are missing, and doing almost anything else with your time would be more worthwhile. I'm sure that's a large part of the explanation of why referees allowed the Bogdanoff papers to be published.

4. **Pascal**
   January 6, 2022

   According to the above mentioned post by Lubos Motl, the scientific work by the Bogdanov brothers does make sense. Or at least, it's not more nonsensical than average. In his own words:
   "While I wouldn't see their dissertation as a fully meaningful foundation for further exciting research, I think that the papers were at least comparable with the average work that can earn PhDs."

5. **Jonathan Chiche**
   January 6, 2022

   As regards the statement "I always supposed that to some extent the brothers were in on the joke and I hope that was true": I still recall what I was told, back in the period 2005-2010, about the way the brothers had written their theses, how the head of École Polytechnique had been ordered to let one of them defend his thesis in the school's premises, and the curt and dismissive reply made by the PhD candidate to a mathematician in the audience pointing out that what he had just heard did not make any sense. Unfortunately, I do not think anyone gave any public complete account regarding these matters at the time, and what remains is but hearsay. Thus, in a sense, this affair should have been taken more seriously. The truth would have been much clearer now. It could have been a valuable experience to meet them in Paris and try to get a precise and meaningful scientific statement from them. It may sometimes be hard to tell fraud and good faith apart, but the twins were delusional at best.

Among the obituaries in French, I find this one quite telling: https://www.lefigaro.fr/vox/medias/grichka-bogdanoff-un-etre-exquis-d-une-prodigieuse-intelligence-et-d-une-vaste-culture-20211228. You may think that the author too, writing, among other things, about Grichka Bogdanoff, "avait-il réellement un âge, lui qui, fervent amant des deux infinis pascaliens, transcendait toute limite spatio-temporelle", is in on the joke. Alas, it is clearly not a joke.

You may also want to listen to the interview given (in French) by the Bogdanoff brothers on 6 December 2021: https://www.youtube.com/watch?v=KCQW6zabA1A. They make laudatory statements about a French self-proclaimed elite scientist who, in the last two years, has been one of the major sources of disinformation regarding the current pandemic, and whose fraudulent papers and misleading Youtube videos are clearly no joke either.

All in all, giving the Bogdanoff brothers the benefit of the doubt seems overly cautious, and especially surprising on this blog.

6. **Peter Woit**
January 6, 2022

Pascal,
Lubos's involvement in this is one of the "comic aspects" I had in mind, in particular his book "The Bogdanov Equation", see
https://www.math.columbia.edu/~woit/wordpress/?p=3594

Jonathan Chiche,
Charitably giving people the benefit of the doubt is my general policy. In this case though I should point out by that by "in on the joke" I meant that quite possibly the brothers were not completely delusional about the value of their research. A less charitable way of saying they were "in on the joke" would be to say that they were more than a bit con artists.

7. **Roger**
January 6, 2022

I'm not sure I can draw any other conclusion than that they were a pair of dishonest chancers. Setting aside the comical aspects of the affair, there was far too much deceit in what they did (eg the fake Professor Yang, fake research institutes etc.).

It suited their reputational goals and vanity to be regarded as having contributed to physics research so they sought to convince the public and academic committees that they had done this. In a way the checks and balances built into the system worked even if this was imperfect. They may have succeeded in getting something into the literature and gaining PhD's (albeit with the lowest passing grades) but their work is generally regarded as being either nonsense or simply to be ignored.

It is rightly customary to focus on the positives when discussing the deceased. However, I only know of the twins through the negatives and I see nothing in their behaviour that leads to any sympathy for them. I wish it were otherwise.

8. **Peter Woit**
January 6, 2022

Roger,
As far as theoretical physics goes, the brothers were con artists, but I'd argue that the way they went about this had a fair amount of entertainment value and did no actual damage to the field. Theoretical physics has other con artists, who generally provide no entertainment value and have done a great deal of damage…

9. **Jackiw–Teitelboim**
January 6, 2022

Interesting… retired string theorists are people with the curious divertissement of writing books with unpresuming titles such as: L'Équation Bogdanov, The God Equation, The Cosmic Landscape etc.

10. **Frank Wilhoit**
January 7, 2022

I keep coming back to the quote from Roman Jackiw: "…It showed some originality and some familiarity with the jargon. ***That's all I ask.***" Here is someone who did real work, yet when to comes to students, "all he asks" is ***a purely literary operation***, which, as any of us who have ever written an academic paper knows, is trivial. Nothing could better encapsulate the bottomless cynicism of [what] the educational enterprise [has been reduced to]; and parallels exist in every area of study, not just physics, not just science.

11. **John Baez**
January 8, 2022

This [video on the Bogdanoffs]() talks about some curious aspects of their later life I hadn't known about, but concludes with something I'd never imagined: the theory that Moshe Flato encouraged them to get PhDs, and supported their research, as a kind of revenge against the physics establishment. I'm not convinced, but it's an interesting idea: a Sokal-like hoax perpetrated not *by* the Bogdanoffs but *using* them.

12. **jack morava**
January 8, 2022

Back in the day it was clear that the Bogdanoffs were an anomaly but perhaps not what kind, but now I think they are easily recognized as trolls.

13. **Peter Woit**
January 8, 2022

John Baez,
I knew Moshe, who was a truly wonderful character, and it's plausible his decision to supervise the Bogdanoff PhDs had an element of trouble-making in it. He died unexpectedly in 1998, before the brothers finished their PhDs, so if he had a plan about how this would work out, we'll never know what it was.

14. **Chris Oakley**
January 8, 2022

The video is interesting. Lubos nice and friendly, as usual, I note.
However I don't quite understand how the brothers getting PhDs *after* having falsely claimed that they had them in their books is going to make it right.
I suppose that there are some things that there is no point in trying to understand.

15. **JE**
January 11, 2022

The Sokal and Bogdanoff affairs, and even the Mochizuki one, bear some resemblance, no matter how different they may turn out to be, in the amount of attention and energy they take from the refereeing system and mass media. Of course, that's a large part of the explanation of why referees allowed the Bogdanoff papers to be published, but I am not quite sure they did no actual damage to the field, because media attention and referee energy are not unlimited. The Sokal affair was meant to raise doubts about the refereeing system, which is okay in principle. As a result, the refereeing system should now be more able to block moves in this direction. But did it really hit the target or quite the opposite?

16. **Peter Woit**
January 11, 2022

JE,
The Sokal hoax wasn't really aimed at the refereeing system, but aimed at showing that prominent figures in the "Science Studies" field were intellectually incompetent. The editorial collective at Social Text fell right into his trap, deciding to publish the hoax because it appealed to their prejudices, not bothering to have it refereed by an expert. Falling for the hoax publicly humiliated some prominent academics and made their field somewhat of a laughing-stock, with long-lasting and significant effects.

The Bogdanoff story I don't think had anything other than a temporary effect of making some editors more careful at certain journals.

The failure of the refereeing at PRIMS in the Mochizuki case is again a very different story, with very different reasons for the failure. There I think the damage caused is very significant, but localized in Kyoto.

17. **JE**
January 11, 2022

Peter,
No idea what Sokal's actual intentions were. He clearly knew that the journal was not refereed. I wonder why he, as a mathematical physicist, bothered to show that prominent figures in the "Science Studies" field were intellectually incompetent, other than "to attract attention to what he saw as a decline of standards of rigor in the academic community," as Weinberg put it. To this aim, perhaps the editors were viewed as easy targets, but maybe I am missing some important part of the story.
He recognized that the article had been "liberally salted with nonsense," and in his opinion was accepted only because "(a) it sounded good and (b) it flattered the editors' ideological preconceptions."
I agree that the Bogdanoff affair did not add much in this respect, or maybe just as a side-effect, and that Mochichuki's is another story, but did the Sokal hoax end with nonsense and goobledygook in science, did it achieve the opposite or was it just a sterile effort in this regard?

18. **Peter Woit**
January 11, 2022

JE,
I don't want to get into a discussion of the Sokal hoax story, which was fascinating, but rather different. His motivation was not to end nonsense and gobledygook in science, but in the a different field, which you might call cultural studies.

Actually, I just remembered that at some point long ago, when I was first starting to write about string theory I wrote to Sokal. Probably something like "besides the problem you exposed among the humanists, there's a real problem here in science itself, concerning string theory, and maybe you should be doing something about that". I don't think I ever heard back from him.

19. **Damien**
January 13, 2022

I met them at the time of the Bogdanoff affair (christmas 2002, new year 2003). Unable to understand their kind of physics, I wanted to know what they were about.

They were nice, warm, and I am positive 100% sincere about what they were doing. They were not "in the joke".

Contrary to what is said in an aforementioned video, they were not millionnaires. Acccording to a mutual friend, they lived their whole lives in debt. I am also convinced they never did plastic surgery. It would have been the worst plastic surgeon in the world.
I am convinced that their faces were altered by their taking more or less legal drugs in the hope of remaining young.
As I said, when I met them they looked sincere, but as it became clear that they had lied, engaged in sockpuppetry, modifide quotes about them, I set my opinion of them. They were fraud, whether they knew it ior not, maybe pathological liars.

20. **Pascal**
January 15, 2022

If I recall correctly, Sokal's grudge against some social scientists was that they would study "hard sciences" like physics as a purely social phenomenon, forgetting that scientific theories are grounded in experimental evidence.
Ironically, it seems that physicists have decided to show that the social scientists were right after all. Exhibit #1 is string theory: the main evidence for it seems to be that there are lots of string theorists, they hold powerful and prestigious positions, and they are able to silence their critics (with diminishing success, though).

21. **Anonyme**
January 20, 2022

The Bogdanovs were supposed to go on trial at the beginning of February for a very unglamorous story. This will not happen after all.

https://www.lemonde.fr/societe/article/2022/01/20/les-escrocs-presumes-d-un-millionnaire-bipolaire-juges-a-paris_6110172_3224.html

Yet More Math and Physics Items

January 14, 2022
Categories: Uncategorized

Various items that may be of interest:

- Robbert Dijkgraaf was sworn in a few days ago as Minister of Education, Culture and Science in the Dutch government. Unclear who if anyone is director of the IAS at the moment, but David Nirenberg will take on this position February 1. Nirenberg is currently a professor of medieval theology and director of the Divinity School at the University of Chicago. This will be the first time in thirty-five years that the IAS director has not been a mathematician or physicist (although some might argue that medieval theology and theoretical physics have seen a convergence in recent years…).
- Lieven Le Bruyn has a posting about Huawei's new research center in Paris (and adds more here), which seems to have attracted multiple Fields medalists, with Laurent Lafforgue's affiliation recently changing from IHES to Huawei. Lafforgue held the Huawei chair in algebraic geometry at the IHES, which now is in the market for a new Huawei chair in algebraic geometry. The head of Huawei has recently emphasized the importance to the company of "bringing in talent with 'tall noses'".
- An interesting way to keep track of hot topics in US theoretical HEP might be to watch the "white papers" starting to appear as part of the Snowmass 2021/2 exercise. Two examples now on the arXiv are a seven-author white paper on the Emergence of Spacetime and a three-author one on Celestial Holography. There will be a program on Celestial Holography at the PCTS in Princeton in a couple weeks.
- On the Mochizuki/abc front, Jordan Ellenberg on Twitter points to this recent preprint from Kirti Joshi. Joshi claims inspiration from the IUT papers, but writes in a much more conventional mathematical language, and makes no claims to prove abc or any other major new results in number theory. One might optimistically hope that his work would clarify the true significance of Mochizuki's IUT work.

Update: A very recent relevant paper from Joshi is this. It contains a detailed comparison of his point of view with Mochizuki's, but avoids taking any position on the controversial Corollary 3.12 claimed by Mochizuki.

Update: To clarify the above. In this paper (Theorem 10.1.1) Joshi proves his version of Mochizuki's Corollary 3.12. But importantly (in that paper): Along with Theorem 10.1.1, there is a discussion of why Joshi's version of Corollary 3.12 is different from Mochizuki's version and notably why his version does not imply Mochizuki's version (according to Joshi, the two versions work with two different ambient sets to compute the theta-values locus–Joshi's version uses a natural ambient set his theory provides–and it is deeply tied to Fargues-Fontaine Theory).

Comments

1. **Peter Orland**
   January 14, 2022

   I have no idea what sort of leader of the IAS Nirenberg will be, but in my highly uninformed opinion, he is a serious and interesting scholar. His book "Anti-Judaism" on a store shelf attracted my attention only by its seeming weird. I overruled my prejudices and leafed through it. Then I bought it and I read the thing in a day.

2. **Peter Orland**
   January 14, 2022

   … and I guess I did not know before that his father was also someone whose papers (on Sobolev inequalities) I had read.

3. **theoreticalminimum**
   January 15, 2022

   Maybe this could be of interest. Gallimard have published Grothendieck's R&S in two volumes and a fiction presumably inspired by the story of Perelman.

4. **DKepler**
   January 16, 2022

   …although some might argue that medieval theology and theoretical physics have seen a convergence in recent years…

   .

   Yes, some might argue the convergence, but it is unfair to drag whole of 'theoretical physics' into this. String theory is the 'only game in town' – at least in this regard. Your own posts on top string theorists promoting multiverse demonstrate this very well.

5. **Johan Smit**
   January 17, 2022

   Another physics item: David Tong published lecture notes for supersymmetric field theory, where he indicates that the primary reason for studying the topic is to understand quantum field theory better and find more connections to mathematics, rather than to find beyond standard model physics for which there is no evidence of.

   https://www.damtp.cam.ac.uk/user/tong/susy.html

6. **jackjohnson**
   January 18, 2022

   @ John Smit, Not that there's anything wrong with that?

7. **Johan Smit**
   January 18, 2022

   jackjohnson,

   There is nothing wrong with any of this. It's just more evidence of an ongoing paradigm shift in theoretical physics, where the connections to speculative beyond standard model physics gets de-emphasised (due to the lack of evidence for supersymmetry at the LHC) and the focus is turning towards theoretical quantum field theory and toy supersymmetric models. Ten years ago these lecture notes would probably have dedicated an entire chapter or two to supersymmetric extensions of the standard model like MSSM and possible phenomenology at the LHC.

String theory is headed in the same direction as well, with the focus turning away from trying to find a theory of quantum gravity or to unify all the forces, and instead studying toy string theory models (or really toy CFT models) and applying the results to other quantum field theories.

8. **jackjohnson**
   January 19, 2022

   @ johan smit, fair enough, thanks!

9. **anonymous**
   January 19, 2022

   Kirti Joshi published a part 2 of his Arithmetic Teichmuller spaces paper: https://arxiv.org/abs/2111.04890

10. **bertie**
    February 11, 2022

    So with the linked Kirti Joshi paper, he derives results analogous to the IUT stuff including a result 'of the same form' as Corollary 3.12 but I gather there is some problem getting bounds which are as tight as Mochizuki's otherwise Joshi would just go ahead and prove ABC wouldn't he? Joshi is exceedingly polite towards Mochizuki's work. However, if Joshi's analogous construction does everything that IUT does,with the single exception of proving ABC, wouldnt that look a little bit like a 'nail in the coffin'?

11. **Peter Woit**
    February 11, 2022

    bertie,
    My understanding is that Joshi is making no claims one way or another about Mochizuki's Corollary 3.12, in particular not claiming he knows how to prove (or disprove) it. Maybe it's a true statement, maybe it's not. One thing that remains the same is that no one can explain Mochizuki's proof of the statement in a convincing way.

This Week’s Hype

January 21, 2022
Categories: This Week's Hype

For many years, editions here of This Week’s Hype were mainly devoted to bogus claims that someone had found a way to get a testable prediction out of string theory or other “evidence for string theory”. Recently there have been many fewer such claims, with consensus in the string theory community that there is now no hope to get a prediction from string theory about observable physics at accessible energies. One can watch the recent talks here on Steven Weinberg and his legacy to get a good idea of what this current consensus looks like: you can’t test string theory since string effects occur at much too high an energy scale, and Weinberg showed that such things will just look like the Standard Model sort of QFT at observable energies. In addition, Weinberg is also credited with the anthropic CC argument, taken as evidence for the otherwise unobservable string theory landscape. Taken together, the consensus of leading particle theorists has become that there’s no point to trying to do any better than the Standard Model, with the only answer available to anyone who asks questions about higher energies is “string theory, whatever that is”.

With particle physics abandoned, theorists have focused on quantum gravity as the only legitimate issue to study. For many decades the hope was that a consistent answer to the unknown question of what string theory really is (often called “M-theory”) would be found, and that would provide a final end to the subject of fundamental physics. This final theory would be untestable, but it would self-consistently explain why one could not hope to test it. In recent years though, after decades of no progress towards a consistent M-theory, string theorists have essentially given up on this hope.

This situation has lead to a recent trend in string theory research: instead of looking for positive evidence for string theory, try to find an argument that resistance is hopeless, string theory is the only theory possible. The arguments of this kind I’ve seen make no sense to me, but they are gaining in influence. One place I noticed this is in this recent white paper about the interesting topic of celestial holography, which has little to do with string theory. There the authors write:

> A crowning achievement for the celestial holography program would be for it to determine concretely whether string theories are the only consistent theories of (asymptotically flat) quantum gravity.

Today Quanta magazine has more of this sort of thing, with an article whose title shows up on the web as A Correction to Einstein Hints At Evidence for String Theory. The sub-headline tells us that

> In a quest to map out a quantum theory of gravity, researchers have used logical rules to calculate how much Einstein’s theory must change. The result matches string theory perfectly.

which sounds pretty impressive. The article starts off with quotes such as:

> The hope is that you could prove the inevitability of string theory using these methods,” said David Simmons-Duffin, a theoretical physicist at the California Institute of Technology. “And I think this is a great first step towards that.

and

> Irene Valenzuela, a theoretical physicist at the Institute for Theoretical Physics at the Autonomous University of Madrid, agreed. “One of the questions is if string theory is the unique theory of quantum gravity or not,” she said. “This goes along the lines that string theory is unique.”

The paper at issue is this one which appeared on the arXiv nearly a year ago. It’s not about string theory or about conventional quantum gravity in four space-time dimensions. The topic is graviton scattering in maximally supersymmetric theories in ten flat space-time dimensions, and the argument is that the basic principles of supersymmetry, Lorentz invariance, analyticity and unitarity imply a bound on the coefficient of the lowest order correction term. The only relation to string theory is that a string theory calculation of this correction coefficient satisfies the bound (as expected, since string theory is supposed to satisfy the assumed basic principles). Much is made of the fact that in string theory one can get any value of the coefficient consistent with the bound. This is taken as evidence for the “inevitability” of string theory, but I don’t see this at all. It’s more accurately evidence for the usual problem with string theory: it’s consistent with anything. If the authors of this paper had found that the string theory bound was different than their bound, they could have written a paper arguing that they had finally found a way to falsify string theory (measure the coefficient, if it was found to be in the region allowed by general principles but not by string theory, string theory would be falsified).

The article does get right the motivations behind these claims:

> Some physicists hope to see string theory win hearts and minds by default, by being the only microscopic description of gravity that’s logically consistent. If researchers can prove “string universality,” as this is sometimes called — a monopoly of string theories among viable fundamental theories of nature — we’ll have no choice but to believe in hidden dimensions and an inaudible orchestra of strings.
>
> To string theory sympathizers, the new bootstrap calculation opens a route to eventually proving string universality, and it gets the journey off to a rip-roaring start.

and it gives a little space to skeptics:

> Other researchers disagree with those implications. Astrid Eichhorn, a theoretical physicist at the University of Southern Denmark and the University of Heidelberg who specializes in a non-stringy approach called asymptotically safe quantum gravity, told me, “I would consider the relevant setting to collect evidence for or against a given quantum theory of gravity to be four-dimensional and non-supersymmetric” universes, since this “best describes our world, at least so far.”
>
> Eichhorn pointed out that there might be unitary, Lorentz-invariant descriptions of gravitons in 4D that don’t make any sense in 10D. “Simply by this choice of setting one might have ruled out alternative quantum gravity approaches” that are viable, she said.
>
> Another critique, though, is that even if string theory saturates the range of allowed α values in the 10-dimensional setting the researchers probed, that doesn’t stop other theories from lying in the permitted range. “I don’t see any practical way we’re going to conclude that string theory is the only answer,” said Andrew Tolley of Imperial College London.

I don’t at all understand why Quanta chose to cover this. All it does is help to spread hype and further the cause of the “resistance is futile” campaign from proponents of a failed research program.

Update: This kind of hyped story turns into the expected PR result:

> **Evidence for String Theory** –In a quest to map out a quantum theory of gravity, researchers have used logical rules to calculate how much Einstein’s theory must change. The result matches string theory perfectly, reports Natalie Wolchover for Quanta.

Comments

1. **John Baez**
   January 21, 2022

   "Much is made of the fact that in string theory one can get any value of the coefficient consistent with the bound. This is taken as evidence for the "inevitability" of string theory, but I don't see this at all."

   The funny thing is, you don't even need to know much to notice the vast hole in this argument. You just need to pay attention to the overall logic. It's a bit like claiming I'm a good weatherman because I predict the probability of rain is somewhere between 0% and 100%. I'm "inevitably correct" in some sense, but it doesn't mean I'm any good.

   How can anyone be fooled by this, unless they want to be?

   The fact that this whole argument is about universes unlike our own – maximally supersymmetric theories in ten flat space-time dimensions – requires a bit more technical expertise to notice, at least if people are trying to hide it. But this, at least, is pointed out in the article: "Eichhorn pointed out that there might be unitary, Lorentz-invariant descriptions of gravitons in 4D that don't make any sense in 10D. "

   In other words, we should only think this whole calculation is relevant to real-world physics if we *already* buy into string theory.

   It's sad that *Quanta* is talking about this when there is really interesting physics being done.

2. **Dalimil Mazac**
   January 21, 2022

   @Peter Woit and John Baez, I find it hard to understand your skepticism. The article is addressing the important question whether there are consistent theories of quantum gravity other than string theory. It gives partial evidence that at least in 10D with maximal susy, the answer is no. I think that is pretty exciting! There are a lot of people doing fantastic and honest work along these lines nowadays and if anything, their work should be covered by science journalists more, not less.

3. **Peter Woit**
   January 21, 2022

   Dalimil Mazac,
   I've read the paper carefully and I see nothing there about any evidence there are no consistent quantum gravity theories other than string theory, even in the 10d maximal susy setting. The quote from Andrew Tolley (who I gather works on bootstrap methods) makes the relevant point: that string theory has alpha in the allowed range says nothing about whether or not there are other theories with alpha in the allowed range.

   I completely disagree with you that bogus claims that only string theory can describe quantum gravity deserve more coverage by science journalists and are "exciting" and "fantastic". They're the opposite of good science, part of a campaign to convince people that a failed research program is the only way forward.

4. **Dalimil Mazac**
   January 21, 2022

   @Peter Woit, the main point of the paper is not that string theory satisfies a bound (I agree that on its own would not be very interesting). Rather, the point is that the lower bound coming from the bootstrap constraints seems to coincide with the lowest value attainable in string theory. This was by no means guaranteed from the getgo.

   Of course, this is not a proof of uniqueness of string theory, but no one is claiming that it is. However, it is a first step of such a proof in the sense that it would become a proof if it could be extended from alpha to all the higher-order terms of the S-matrix.

   Finally, I can assure you that most people who work on these ideas would be equally excited if their work lead to the discovery of a previously overlooked quantum gravity theory, as they would be to prove the uniqueness of string theory. There is really no hidden agenda besides trying to understand new facts about nature using rigorous bootstrap methods.

5. **Peter Woit**
   January 21, 2022

   Dalimil Mazac,
   I'm still not seeing how string theory allowing any alpha consistent with general principles has anything to do with whether or not there's another theory also consistent with these principles.

   You can hope, like Chew and many others did during the 1960s, that your list of general principles constrains the S-matrix to be something unique. And you can hope that that thing is string theory (and that results in maximally supersymmetric 10d have something to do with non-supersymmetric 4d). But calling this result a "first step in a proof" of this I'm afraid well deserves the term "hype".

6. **S**
   January 22, 2022

   It's depressing that the world is currently in a place where (a) "believe science!" is a major campaign and (b) the world's smartest physicists seem to have bet the farm on a very straightforward example of affirming the consequent.

   (Of which, I must say, Dr. Mazac's two posts, in tandem, seem a clear illustration.)

   More and more it seems to me that the information environment we're in is characterized by the fact that there is just no safe consumption algorithm to teach anybody other than "be an extremely sophisticated consumer of information and mistrust everything you can't evaluate for yourself."

   Which is just terrible.

7. **Sabine**
   January 22, 2022

   Since no one's ever seen a graviton, they may for all we know not exist and the entire question is moot. When I refer to "quantum gravity" I always stress that this refers to any way to resolve the inconsistency between the SM and GR. It does not necessarily mean you actually quantize gravity. The question whether a quantization of gravity is internally consistent is different than the question whether a theory consistently combines qft and gr. The commonly named example is that you don't get a theory of atoms by quantizing water. Likewise, you may not get a theory for the fundamental constituents of space-time by quantizing gravity.

   In case you got lost in the previous paragraph, I am saying that the question these people are trying to address isn't the question we want an answer to. They're two separate questions. And I agree with Tolley that there's no way mathematical proof will give us an answer to the question we actually want an answer to (how do we remove the inconsistencies between the SM and GR), simply because no proof is better than its assumptions, and you can't ever prove the assumptions. It's unfortunate how many physicists confuse physics with math (& good that Tolley isn't one of them).

Incidentally, the entire idea that you can somehow derive a more fundamental theory from a less fundamental one makes no sense because if you could it wouldn't be more fundamental.

(None of this is to say that I am a fan of emergent gravity or think string theory is wrong.)

And why Quanta keeps covering stuff like this is a good question indeed. I've been wondering for a while what's going on there. They also seem obsessed with black hole information loss despite the fact that there's nothing to learn from this "research".

Only interesting thing I learned from this is that Astrid seems to be affiliated with a place in Denmark.

8. **MT**
January 22, 2022

"It's depressing that the world is currently in a place where (a) "believe science!" is a major campaign and (b) the world's smartest physicists seem to have bet the farm on a very straightforward example of affirming the consequent."

In your opinion, what is the risk of the situation as you have described it fueling further ignorance against science in among members of the public?

9. **JS**
January 22, 2022

Let me chime in something from a person actively working on bootstrap techniques. There's no doubt that the paper of Vieira et al. is remarkable and probably one of the most exciting results that ever came out of the S-matrix bootstrap. They demonstrate that at the subleading order in the low-energy limit, string theory in 10D is compatible with S-matrix axioms such as unitarity, analyticity, etc. and moreover it seems to saturate this compatibility among theories with maximal supersymmetry in 10D (in a well-defined sense). However, Quanta's coverage of this topic is extremely disappointing and I don't understand why they decided to put a weird "string theory vs. the world" spin on it, instead of letting the result speak for itself. Completely unnecessary.

10. **Ryan Usher**
January 22, 2022

> "…I don't at all understand why Quanta chose to cover this."

This is something I find myself wondering as well, and it's not the first time Quanta has fallen prey to this kind of hype–and I characterize it in that way to be generous to Quanta in the face of my cynicism. So I decided to waste a couple of hours to come up with the following:

Quanta provides a bare bones explanation of their publishing process on it's "About" page, making it as clear as it can that the Simons Foundation has no say or control over what gets published. This seems to be a bit misleading, however, because Quanta's current Editor-In-Chief's (Thomas Lin) bio states that he joined the Simons Foundation in 2012 with no indication that he is no longer a member of the Foundation. If you wanted to truly keep the Simons Foundation out of what you're publishing, it might be a better idea to not have a member be your Editor-In-Chief.

What seems to ultimately guide Quanta's coverage, however, are these two, simple, statements:

> "…All editorial decisions, including which research or researchers to cover, are made by Quanta's staff reporting to the editor in chief; editorial content is not reviewed by anyone outside of the news team prior to publication…"

There are, as of this comment, 38 articles on Quanta's website that are tagged with "string theory". Out of those articles the authorship is broken down in the following ways:

12 are written solely by Natalie Wolchover ("Senior Writer/Editor)*
4 are written solely by Kevin Hartnett ("Senior Writer/Editor")
3 are written by a collaboration between Natalie Wolchover, Olena Shmahalo ("Contributing Art Director"), and Lucy Reading-Ikkanda ("Former Graphics Editor")
3 are written solely by Robbert Dijkgraaf (who is credited as a "Contributing Columnist")
2 are written solely by Erica Klarreich ("Contributing Correspondent")
1 is written solely by Peter Byrne ("Contributing Writer")
1 is written solely by Charlie Wood ("Staff Writer")
2 are written by Sabine Hossenfelder ("Contributing Columnist")
1 is written solely by Dan Falk ("Contributing Writer")
1 is written solely by Thomas Lewton ("Contributing Writer")
1 is written solely by George Musser ("Contributing Writer")
1 is written solely by Philip Ball ("Contributing Writer")
1 is written solely by Eva Silverstein ("Contributing Columnist")
1 is written solely by Patrick Honner ("Contributing Columnist")
1 is written solely by K.C. Cole ("Contributing Writer")
1 is written solely by Joshua Sokol ("Contributing Writer")
1 is written solely by Siobhan Roberts ("Contributing Writer")
1 is written solely by Jennifer Ouellette ("Contributing Writer")

*One of Natalie Wolchover's articles is simply a multimedia presentation titled, "Theories of Everything: Mapped", so its relevance may be questionable as it may be tagged with numerous legitimate theories.

There are, as of this comment, 17 articles on Quanta's website that are tagged with "multiverse". Out of those articles the authorship is broken down in the following ways:

7 additional articles written solely by Natalie Wolchover**
2 additional articles written solely by Charlie Wood
1 additional article written solely by Jennifer Ouellette
1 article written in collaboration between Natalie Wolchover and Peter Byrne
1 article written solely by Thomas Lin ("Editor in Chief")
The same single article by Peter Byrne referenced above

The same single article by Dan Falk referenced above
The same single article by Philip Ball referenced above

**One of Natalie Wolchover's articles is tagged with both "string theory" and "multiverse", it is included in the string theory tally.

Although one can quibble about the content of the articles themselves: "it's just a Q&A!","it's an editorial column/blog, which 'is not reviewed by anyone outside of the news team prior to publication…!'", or "it's part of their 'Quantized Academy' series!" I will concede the possibility that some of the Q&A's might be interviews with scientists who are critical of string theory, but the fact of the matter is the majority of the articles are giving a voice to string theory/the multiverse. Even if the tone of an article is entirely neutral, it continues to legitimize string theory/the multiverse–even if its only in a small way.

Based on the authorship breakdown above it seems pretty clear that, for whatever reason, Natalie Wolchover is pushing string theory/the multiverse at Quanta–and her role as a Senior Writer/Editor most likely gives her a lot of s(w)ay in what gets put out. But she may not be the only problem, because the current Deputy Editor at Quanta is Michael Moyer, whose bio states: "Before joining Quanta Magazine in 2014, Michael Moyer spent six years at Scientific American, where he was most recently in charge of physics and space coverage and led the magazine's special editorial projects…"

If I'm remembering correctly, Scientific American is a publication that is rife with string theory and multiverse hype, so having someone who was in charge of physics and space coverage there for six years now the Deputy Editor at Quanta is not a good sign; any skepticism about Moyer's position is, unfortunately, bolstered by the fact that when you look at the publication dates for articles on string theory in Quanta, there are two articles in 2013, then nothing until 2015, after Moyer is hired (the history of Quanta's archive regarding the multiverse is a little more even), with healthy coverage of the publication of something on string theory every year since–2019 being an unusually barren year, with only one article tagged with string theory published.

To me, it looks like Quanta–far from "falling prey" to the hype–sadly perpetuates string theory/the multiverse mania, just in a more conservative/limited way.

11. **anonymous**
January 22, 2022

The result by Vieira et al isn't really all that interesting. String theory and other 10 dimensional theories are toy models that theoretical physicists like to play in, and it's great that the S-matrix bootstrap could give a result pertaining to such toy models, but there is no evidence yet that the techniques used with the S-matrix bootstrap could say anything about actually interesting theories relevant to the real world, i.e. theories that are in 3+1 dimensions.

12. **Scott Aaronson**
January 22, 2022

I actually thought the Quanta article was pretty good! It successfully conveyed to an outsider like me both what the result literally says (i.e., the two alpha ranges approximately coinciding in 10D SUGRA), and how string theorists and non-string-theorists very differently interpret that.

13. **Peter Woit**
January 22, 2022

MT,
One motivation for my anti-string theory hype campaign has always been that I think is that this kind of hype has already done significant damage to the public perception of science and scientists, and threatens to do even more if it continues. If we're trying to get the public to "trust science", this kind of behavior does not promote that at all, quite the opposite.

14. **Peter Woit**
January 22, 2022

Ryan Usher,
In trying to understand why this kind of thing happens I think you're focusing on the wrong people. The source of the problem is that the most prominent scientists in the field are pushing this, not that professional journalists pay attention to such figures. The Simons Foundation, like Simons himself, is focused on elite institutions, and Quanta is going to reflect points of view coming from those elite institutions. When there's a serious failure of elite institutions like in the string theory/multiverse case, it's a lot to expect of either the Simons Foundation or Quanta to take on the role of trying to fix that failure.

15. **Peter Woit**
January 22, 2022

Scott,
The point that the coincidence of these two ranges doesn't imply uniqueness of string theory is a straightforward matter of fact + logic. If such straightforward issues become matters of opinion, determined by which tribe you belong to, science is doomed.

In any case, in this example I don't even think that most members of the string theory tribe are willing to sign onto the incorrect side of the argument. Lubos Motl, as fanatic a member of the string theory tribe as they come, has a long posting about this here
https://motls.blogspot.com/2022/01/how-string-theory-correctly-predicts.html
where he makes some of the same points I made, e.g.

"The authors seem excited about the fact that string theory exploits exactly all the allowed values. Well, it is a double-edged sword. On one hand, you can be reassured that string theory overlaps with the consistent quantum theory of gravity nicely, so we have "the" theory. On the other hand, you could say it is bad news because – as in the swampland reasoning – you would want string theory to impose stronger conditions than those that can be extracted from "possibly naive", non-stringy arguments."

He argues for the uniqueness of string theory, but not by claiming that the coincidence of the ranges shows this (he more favors "LQG sux" as a uniqueness argument).

16. **Scott Aaronson**
January 23, 2022

I agree that the coincidence of these two ranges can't be interpreted, in any way whatsoever, as a "uniqueness proof" for string theory! And I didn't see the article as claiming that. Rather, it quotes string theorists who *optimistically hope* that this result might be extended to a more general result stating that, if a given correction to GR is consistent with general postulates about quantum gravity, then that correction can also be realized string-theoretically. Even that would really be a "universality statement" rather than a "uniqueness statement" — more a statement that one "might as well" study string theory than a statement that one "must" study it — but it would be pretty important information regardless, no?

17. **Martin S.**
January 23, 2022

Do the other approaches have some results about which Quanta could report? Has anyone suggested to Quanta such results from the other approaches?

Regarding the discussed article, I would mainly prefer if they would write clearly that the 10D thing is about mathematics, and not about physics.

18. **Peter Woit**
January 23, 2022

Scott,
I read this as pretty clearly an argument for uniqueness ("This goes along the lines that string theory is unique."), and for something much stronger than "string theory is a universal framework that can capture any possibility". That string theory is "unique" implies there are no other non-equivalent possibilities. Thus, no point to work on alternatives, because if you succeed you would just give a different construction of string theory.

I don't think one can understand any of what is going on here outside of the context that people are trying to defend a proposal for a fundamental theory that is not well-defined and is completely untestable. About the only way you can justify this is by a "no-alternatives" (or uniqueness) argument, according to which all seemingly deadly problems of the theory don't matter: it has to be true since it is the only possibility.

19. **Peter Woit**
January 23, 2022

Martin S.,
I don't think there's been much progress in any of the main approaches to quantum gravity. I was going to write that I don't know of alternatives they should be writing about, then realized that's not true. There is at least one (euclidean twistors), but they're not alone in ignoring that….

The "10D thing" is not "mathematics", it's an idea about physics (the bootstrap constraints are very much physical principles). I'm very weary of the endless blaming of bad ideas about physics on "mathematics".

20. **Peter Woit**
January 23, 2022

MT,
Among the biggest fans of this are the intelligent design people, see
https://uncommondescent.com/intelligent-design/string-theory-again-will-a-correction-to-einstein-save-it/
Their argument is kind of a "God universality": if you try to make sense of the universe sooner or later you will have to give up on conventional scientific notions of evidence and start doing metaphysics/religion. They are welcoming the string theorists to the realm of religion with open arms…

21. **Mark**
January 23, 2022

The real issue here is not that string theory as an approach to quantum gravity is degenerative in the Lakatos sense (which it is), but that the entire quantum gravity research program as a physics research program is degenerative, of which string theory is only one branch among many. There is currently no evidence of gravity itself being quantum, as Sabine above pointed out, and it would remain that way until it is discovered in experiment or observation that gravity is indeed quantum. As a result, internecine warfare between different subprograms of quantum gravity over which approach to quantum gravity is more degenerative, such as the assertion in the Quanta article that string theory is the unique approach to quantum gravity, is largely irrelevant in the larger picture. The quantum gravity research program as a whole does not really deserve any attention from the scientific media until actual evidence of gravity being quantum is found, let alone the internal debate over which approach to quantum gravity is less degenerative.

22. **Peter Woit**
January 23, 2022

Mark,
It would be helpful it Quanta and other science publications would identify degenerative research programs and put warning labels on stories about them.

I don't agree though that only an experimental observation can save a degenerative research program and put it on a progressive course. New theoretical ideas that solve the problems that have caused a field to degenerate are possible. What I see as the underlying problem here is a failure of the mechanisms that are supposed to push degenerative research programs out of mainstream science. Failed ideas need to be recognized as such, not promoted by influential people and institutions.

23. **Mark**
January 23, 2022

Peter,

I don't necessarily agree either that only an experimental observation can save a degenerative research program. Cosmology is a field where the current standard model of cosmology, Lambda-CDM, is a degenerative research program, and there is plenty of experimental evidence from dwarf galaxies to large scale structures like the Giant Arc to the Hubble tension showing that Lambda-CDM has significant flaws, and where it is new theoretical ideas, like superfluid dark matter or relativistic MOND or something else, that are needed to put it back on a progressive course.

However, it is the case that some research programs are degenerate for lack of experimental evidence. This was the case with most speculative beyond standard model research programs in high energy physics such as supersymmetry and GUTs, where there is simply an utter lack of evidence of anything beyond the standard model, apart from neutrinos having mass. For a few decades, theorists saw no results after no results of any beyond standard model physics, and merely tinkered with the parameters of their pet SUSY or GUT and waited for the next set of results at the Tevatron or LHC to come out. Eventually, after the latest LHC run, most theoretical physicists have abandoned the BSM research program after the non-detection of anything there, and largely moved on to other fields. SUSY and many other BSM models weren't actually proven false, some theorists today speculate that evidence for SUSY might not appear until the Planck scale, but it is pointless to spend further time on SUSY models when experimental tests for SUSY at the Planck scale won't be around for many decades or centuries. No amount of speculative new ideas (apart from neutrinos for which there is already experimental evidence of BSM physics independent of the LHC results) would have saved the wider BSM research program from dying out, unless experimental evidence of new physics was actually discovered at the LHC or some other near-term future experiment.

The quantum gravity research program is another one of the research programs where the degenerative nature of the research program comes from the lack of experimental evidence to signal anything about the theories. Any experimental evidence of quantum gravity is largely speculated to appear at the Planck scale, which once again is decades or centuries away. Theoretical ideas won't help solve the core issue here, as one simply ends up with a quantum gravity landscape with a multitude of different theories explaining the same phenomena with no experimental evidence to distinguish between any of the theories. So really the only solution here is similar to the solution in the BSM research program, to abandon the quantum gravity research program until the time comes when such experiments can be done.

I agree with you that the underlying problem here is a failure of the mechanisms that are supposed to push degenerative research programs out of mainstream science. However, there is more of quantum gravity that needs to be pushed out mainstream science than just string theory. String theory has been around for 50 years and is merely the most popular and well known degenerative research program in quantum gravity. Loop quantum gravity has been around for 30 years and is also a degenerative research program, suffering from many of the same issues as string theory. The same is true of many other approaches in quantum gravity, such as causal sets, or it from qubit, and so forth. The same mechanism failures that keep string theory alive in the theoretical physics community are also the ones that keep loop quantum gravity alive and causal sets alive and it from qubit alive and so on, none of which can even be proven wrong because of the lack of experimental evidence and the lack of even any theoretical predictions in many theories.

Furthermore, even if string theory ends up disappearing from the face of the earth, so long as the larger quantum gravity research program remains fairly active, it would merely be replaced with other overhyped ideas about quantum gravity such as loop quantum gravity or it from qubit or emergent spacetime from some underlying unknown quantum structure, all as flawed as string theory is right now. And the scientific media will still write bogus claims that they have discovered evidence for this quantum gravity theory and that quantum gravity theory when no such thing has occurred yet.

In so far that other quantum gravity research programs do and will end up making advances in physics it is in the same way that string theory ended up making advances to physics, in the contributions that such theories makes to mathematics and mathematical physics and in the techniques developed in the theory that are eventually applied to other subfields of physics such as condensed matter physics and quantum field theory. But that has nothing to do with understanding the world at a fundamental level.

24. **Anonymoose**
January 24, 2022

It's interesting to compare this line of work with the swampland conjecture.
Here, string theory allows the full parameter space allowed by basic physics, and this is a positive result for string theory.
In swampland, string theory doesn't allow the full parameter space allowed by basic physics, and this is a positive result for string theory.
Talk about having your cake and eating it! It would be nice if there was some framework which allowed these people to talk to one another and figure out which one is right…

25. **Peter Woit**
January 24, 2022

Mark,
I don't think it's helpful to expand an analysis of the problems with this article and string theory in general first to quantum gravity in general, then to mainstream cosmology. Whatever the problems of the Lambda-CDM model, it's a well-defined model, and there's a conventional science debate about the details of its relation to observations. In string theory there's now nothing even remotely like this, or any prospects for such in the future.

26. **Mark**
January 24, 2022

Peter,

I feel like you missed the entire point of my post.

I only brought up Lambda-CDM because you stated that I asserted that all degenerative research programs can only be fixed with experimental evidence. I did not assert that all degenerative research programs can only be fixed with experimental evidence, only that the specific cases of string theory and other quantum gravity research programs could only be fixed with experimental evidence. Apart from using Lambda-CDM as an example to correct that specific misunderstanding, and to highlight the difference between research programs that can be fixed with new ideas vs those that can only be fixed with experimental evidence, Lambda-CDM is not relevant to this topic.

On the other hand, it is entirely reasonable to broaden the analysis from string theory to other parts of quantum gravity, because you do so yourself on this blog:

https://www.math.columbia.edu/~woit/wordpress/?p=12472
https://www.math.columbia.edu/~woit/wordpress/?p=11648

There is the same amount of disingenuous hype and obfuscation going on with these alternative approaches to quantum gravity, where they try to link simple quantum experiments and theoretical calculations to vague quantum gravity ideas, which shows that this problems are not entirely contained within string theory but permeates throughout the quantum gravity research program.

27. **Peter Woit**
January 24, 2022

Mark,
Sorry for the misunderstanding. I guess one reason I'm less concerned about problems with quantum gravity in general is just that they haven't had a huge negative impact on the entire field of fundamental theory the way string theory has. If there is a problem with (picking a random example) causal sets hype, it's not one that I see as having done significant damage or worth spending time worrying about.

I do think we disagree though about prospects for quantum gravity research. I see no reason not to believe that it's possible that purely theoretical developments in this area, with no input from experiment, will come across an important idea that has been missing so far, and that this will allow the field to move forward.

28. **Dalimil Mazac**
January 24, 2022

Scott Aaronson,

In this case "string universality" and "string uniqueness" mean effectively the same thing:

A gravitational S-matrix is a priori parametrized by infinitely many couplings (Taylor coefficients around small momentum). So you can think of a gravitational S-matrix as a point in this infinite-dimensional space of couplings. The 10D superstrings define tiny subspaces in this infinite-dimensional space (type IIA gives a 1d subspace and type IIB a 2d subspace, parametrized by the string coupling).

As a corollary, assuming that a (10D maximally supersymmetric) gravitational S-matrix comes from string theory leads to infinitely many predictions about its small momentum expansion. So much for string theory not making any predictions.

Showing that the most general S-matrix consistent with the bootstrap comes from string theory would amount to showing that the bootstrap constraints cut the a priori infinite-dimensional space down to the respectively 1d and 2d subspaces. This is such a huge reduction of uncertainty that I think it's more appropriate to call it uniqueness than universality:)

Needless to say, this goal is still very far but the paper by Guerrieri+Vieira+Penedones shows that everything works after projecting the infinite-dimensional space to the first coordinate.

29. **Peter Woit**
January 24, 2022

Dalimil Mazac,

Sure, string theory makes an infinite number of predictions, including:
10D spacetime, very specific and intricate spectrum of states and their interactions

Only problem with this infinite number of predictions is that they are all completely wrong…

As Lisa Randall likes to say: “string theory predicts gravity: 10-dimensional gravity”

Notes on the Twistor P^1

January 29, 2022
Categories: Euclidean Twistor Unification, Langlands

I've just finished writing up some notes on what the twistor P^1 is and the various ways it shows up in mathematics. The notes are available here, and may or may not get expanded at some point. The rest of the blog posting will give some background about this.

One of the major themes of modern mathematics has been the bringing together of geometry and number theory as arithmetic geometry, together with further unification with representation theory in the Langlands program. I've always been fascinated by the relations between these subjects and fundamental physics, with quantum theory closely related to representation theory, and gauge theory based on the geometry of bundles and connections that also features prominently in this story.

The Langlands program comes in global and local versions, with the local versions at each point in principle fitting together in the global version. In the simplest arithmetic context, the points are the prime numbers p, together with an "infinite prime". A major development of the past few years has been the recent proof by Fargues and Scholze that the arithmetic local Langlands conjecture at a point can be formulated in terms of the geometric Langlands conjecture on the Fargues-Fontaine curve.

Back in 2015 Laurent Fargues gave a talk at Columbia on "p-adic twistors". I attended the talk, and wrote about it here, but didn't understand much of it. The appearance of "twistors" was intriguing, although they didn't seem to have much to do with Penrose's twistor geometry that had always fascinated me. What I did get from the Fargues talk was that the analog at the infinite prime of the Fargues-Fontaine curve (which I couldn't understand) was something called the twistor P^1, which I could understand. The relation to the Langlands program was a mystery to me. Some years later I did talk about this a little with David Ben-Zvi, who explained to me that his work with David Nadler (see for instance here) relating geometric Langlands with the representation theory of real Lie groups involved a similar relation between local Langlands at the infinite prime and geometric Langlands on the twistor P^1.

Over the past couple years I've gotten much more deeply involved in twistor theory, working on some ideas about how to get unification out of the Euclidean version of it. I've also been fascinated by the Fargues-Scholze work, while understanding very little of it. Back in October Peter Scholze wrote to me to tell me he had taken a look at my Brown lecture and was interested in twistors, due to the fact that the twistor P^1 was the infinite prime analog of the Fargues-Fontaine curve. He remarked that it's rather mysterious why the twistor P^1 is what is showing up here as the geometrical object governing what is happening at the infinite prime. I was very forcefully struck by seeing that this object was exactly the same object that describes a space-time point in twistor theory and I mentioned this at the end of my talk in Paris back in late October.

Scholze's comments inspired me to take a much closer look at the twistor P^1, beginning by trying to understand a bunch of things that were somehow related, but that I had never really understood. These ranged from Carlos Simpson's approach to Hodge theory via the twistor P^1 to some basic facts about local class field theory, where one gets a simple analog for each prime p of the twistor P^1 and the quaternions. Along the way, I finally much better understood something else in number theory that had always fascinated me, see the story explained very sketchily in section 6.2. That the quantum mechanical formalism for a four-dimensional configuration space beautifully generalizes to all primes, with the global picture including an explanation of quadratic reciprocity is not something I've seen elsewhere in attempts to bring p-adic numbers into physics. I'd be very curious to hear if someone else knows of somewhere this has been discussed.

Anyway, these new notes are partly for my own benefit, to put what I've understood in one place, but I hope others will find something interesting in them. Now I want to get back to thinking about the open questions raised by the twistor unification ideas that I was working on before the last few months. A big question there is to understand what twistor unification might have to do with Witten's ideas relating geometric Langlands with 4d QFT. Perhaps something I've learned by writing these notes will be helpful in that context.

Update: I've posted the notes, with an added abstract and a final section of speculations, to the arXiv, see here.

Comments

1. **WD**
   January 29, 2022

   I really enjoy how unabashed you are at acknowledging when you do not understand something. I have come across way too many in the scientific environment who seem to be unable to do that, even when it is fairly obvious that such is the case.

2. **jack morava**
   January 31, 2022

   FWIW the table of comparisons at the very end of the notes looks wonderful to me.

3. **noneone**
   January 31, 2022

   About Section 6.2, see
   https://mathscinet.ams.org/mathscinet-getitem?mr=1474565
   and
   https://mathscinet.ams.org/mathscinet-getitem?mr=1896980

4. **Peter Woit**
   February 1, 2022

   noneone,
   Thanks. I have seen those. The first doesn't really say anything about QM, does give a more abstract approach to the reciprocity law than Weil's. The second does do a bit with QM and this kind of idea, there are some other things at Varadarjan's site

   https://www.math.ucla.edu/~vsv/

   The most detailed relevant things to read that I know about are Michael Berg's expository book about the Weil paper
   "The Fourier Analytic Proof of Quadratic Reciprocity"
   and a long article relating some of this to path integrals, see
   https://www.sciencedirect.com/science/article/pii/S0723086915000857

   What I haven't seen is anything using the specific choice of orthogonal space V to be the quaternions. This case plays a big role in mathematics (Jacquet-Langlands), but I haven't seen anything trying to relate it to physics, where it might give a relativistic particle theory that generalizes to all p at once.

5. **MT**
February 8, 2022

Years back, you posted your position as radical platonism and explained that to mean that at a fundamental level, physical objects are actually mathematical objects. Wouldn't it t be fair to say that these developments (and Langlands trends) continue to point in this direction?

6. **Peter Woit**
February 9, 2022

MT,
Yes. This is to my mind very much evidence for what I was calling "radical Platonism". That the description of a space-time point (a fundamental construction of physics) is the precise analog at the infinite prime of the Fargues-Fontaine curve (a fundamental construction of number theory) at the finite primes indicates a very close and unexpected relationship between physical reality and pure mathematics.

7. **Sebastian Thaler**
February 10, 2022

Happy to see you're allowed to post to the arXiv again after all the drama of years past…

8. **Peter Woit**
February 10, 2022

Sebastian Thaler,
I should make clear that I've never had a problem posting preprints to the arXiv. The reason there aren't many there from me in past years is because I haven't written much that I was happy with, not because moderators rejected submissions. So you should attribute the lack of arXiv articles to my laziness or to my high standards, the blame there does not lie with the arXiv. That I've posted two things recently reflects the fact that I'm very excited about what I think are some genuinely new and important ideas (or, in the twistor P1 case, new connections between ideas), that keep looking very interesting to me the more I think about them.

The trackback thing is a different story. There's a blanket ban at the arXiv on trackback links to my blog. For instance, when someone at Quanta mentions an arXiv preprint in an article, a link appears on the arXiv at that article. For an example, see
https://arxiv.org/abs/1804.10624
which has a link to the recent Quanta article
https://www.quantamagazine.org/a-correction-to-einstein-hints-at-evidence-for-string-theory-20220121/
that I wrote about. When I similarly link to an arXiv paper in a blog discussion of the paper, no link to it appears, even though my blog software sends a trackback request. This is a blanket ban, links to my commentary on my own papers are even banned.

Way back when, there was a public statement from the arXiv that trackbacks to my blog were banned since I was not an "active researcher". This no longer seems to be the criterion they use (the author of the Quanta piece is not a researcher but a journalist). I just made another attempt recently to find out what their policy is and why trackbacks to my blog are banned, but the response I got indicated that this is something I'll likely never find out.

This Way to the Universe

February 10, 2022
Categories: Book Reviews, Multiverse Mania

There's a new popular book out this week by string theorist Michael Dine, [This Way to the Universe](), as well a a new [Sean Carroll podcast]() interviewing him about the book and the state of particle theory research. According to Carroll, Dine represents the "insider view" of what is really going on in fundamental physics:

> you're getting what basically is the closest to a consensus view of what state particle theory and fundamental physics is right now.

Much of the book is a very conventional and straightforward attempt to explain modern particle theory/GR/cosmology to a general audience, featuring some explanations from Dine about specific attempts to go beyond the Standard Model that he has worked on. The last quarter or so of the book is about string theory and the multiverse. One odd thing about this is that the jacket copy is fraudulent, stating:

> People assume string theory can never be tested, but Dine intrepidly explores how the theory might be investigated experimentally.

whereas there's nothing like that in the book that I could find. Instead, on page 253 one finds about string theory

> it's not clear it's right, or that it even makes definite predictions at all…
> Many readers will know that string theory has been a lightning rod for criticism. In this chapter, we'll understand why, on the one hand, the subject is so seductive, and on the other, its critics may have a point.

On page 295 there's

> In fact, the existence of states in string theory that really look similar to what we see around us is highly conjectural. This hasn't stopped me nor my colleagues from writing many papers speculating on a stringy reality. Unfortunately, none of these papers can be said to be making a prediction from string theory. Typically the author likes one particular solution of string theory or another, and selects one feature that is distinctive and goes beyond the Standard Model. But apart from the arbitrariness of this choice, the author has closed his or her eyes to two huge problems with their proposal .

On the Carroll podcast, there's this exchange (starting around 1:25):

> **MD**: There are people who work actively trying to… On what they would call string phenomenology, but I think that at the moment, this is a hard topic and we just don't understand well enough how in detail the theory could be related to nature… …strings are rather simple things, but the steps from there to things that look like the standard model, that look like general relativity, are pretty elaborate, and along the way, there are steps we don't really understand…
>
> **SC**: … Let me ask you how you respond to sort of the hardcore critics who might say something like this: In the 1980s, the first superstring revolution, people are going around saying like, yeah, we're going to unify everything, we're going to predict the mass of the electron and everything is going to be finished in 10 years. Then not only has string theory not made any predictions that you can test in an accelerator, but once we have the landscape of string theory, we're saying that string theory is compatible with almost any set of particle physics you can have, and at that point, shouldn't you just give up and move on to something else? It's not a thing that's going to give you any testable predictions at any point in the future.
>
> **MD**: Well, I would basically say that that, all that is fair, but at some gut level, I don't exactly agree. So first of all, I would say that in 1985, already, in this era of the first superstring revolution, Nathan Seiberg and I pointed out what has come to be known as the Dine-Seiberg problem, a very basic and fundamental obstacle to relating string theory to nature. And people have proposed possible solutions, some of which are interesting, but really, there's… In the subsequent nearly 40 years, people have not put forward. So I'm on safe ground, I sort of took both sides of this issue.
>
> **SC**: .. why not just give up if we think that string theory could predict anything at all given the landscape problem?
>
> **MD**: Well, I think my own attitude is to sit somewhere on the fence, not to devote huge amounts of energy to it, but to allow string theory to inform my thinking about various kinds of issues.

Dine is referring here to the "Dine-Seiberg problem", described in [a 1985 paper]() with abstract

> We argue that if the superstring is to describe our world, it is probably strongly coupled. Several other (unlikely) possibilities are discussed.

The paper is specifically about the effective potential for the dilaton, but the problem is generic and fundamental: you can't get anything like the real world out of the perturbative superstring and you don't know what strongly coupled string theory is (one can argue that AdS/CFT tells you what strongly coupled string theory is, but again, that looks nothing like the real world).

The really odd thing about Dine's current comments about testability (besides that they contradict the jacket of his book) is that 15-20 years ago he was for a while one of the theorists most prominently making the case that string theory and the landscape could be tested. I wrote about this often here on the blog, see for instance [here]() and [here](). The second of these postings is about a 2007 Physics Today article by Dine entitled
[String theory in the era of the Large Hadron Collider]() that claimed:

> A few years ago, there seemed little hope that string theory could make definitive statements about the physics of the LHC. The development of the landscape has radically altered that situation. An optimist can hope that theorists will soon understand enough about the landscape and its statistics to say that supersymmetry or large extra dimensions or technicolor will emerge as a prediction and to specify some detailed features.

The Physics Today piece was rather explicitly an answer to my criticisms of the string theory landscape as untestable pseudo-science, with a subtitle

> The relationship between string theory and particle experiment is more complex than the caricature presented in the popular press and weblogs.

Fifteen years later, in this new book Dine says nothing about his earlier claims about testing string theory at the LHC, or that others clearly pointed out at the time what was wrong with them. He ends with this summary of the situation:

> … one can adopt the landscape viewpoint, but then one has to acknowledge that, at this point in time, we have nothing like a complete theoretical framework in which to make any scientific investigation, and that there are facts hard to reconcile with this viewpoint. I, for one, find this quite unsettling.

His experience of the last fifteen years does not seem to have made him think any more charitably of string theory critics, who get this sneering description on page 269-70:

> view the subject with total disdain, often wearing their ignorance of even its most rudimentary aspects as a badge of honor.

One telling mistake in the book is its reference (page 117) to the "Clay Mathematics Institute of Peterborough, New Hampshire", an indication of the all too typical theoretical physicist's lack of knowledge of anything about mathematicians and the mathematics community.

Update: There's an interesting conversation between Dine and Lisa Randall about the book and these issues, see here.

Comments

1. **Jim Given**
   February 10, 2022

   Logicians have very unflattering terms for those who want to have things both ways.

2. **DrDave**
   February 10, 2022

   I think one thing that we are seeing is string theory folded into mainstream science books, generally favorable to ST but appearing to be a wider overview (which they can be, of course) without any real attempt to present any evidence to the contrary. And what we aren't seeing is the exactly the same sort of book but with a real critical appraisal of ST. A very practical way to monopolize the shelfspace of ideas.

3. **Sabine**
   February 11, 2022

   In all fairness the "predictions" for the LHC (be that from string phenomenology or other BSM physics) had little to do with the theories per se and all with the added requirement of "naturalness" that Dine has written about a lot. I'm still waiting to see a particle physicist admit that they were wrong to think that naturalness is a scientific criterion on which one can base predictions.

   (And that they almost all believed this naturalness stuff is why one should be very skeptical of "consensus views"…)

4. **Andreas Karch**
   February 11, 2022

   Naturalness is a property of quantum field theory that is well established mathematical fact and has been experimentally proven in many instances. Certain quantities in a quantum field theory are sensitive to the short distance details (for example scalar masses). Having a light scalar requires fine tuning. We can test these ideas in quantum field theories where we know the exact short distance physics — that is in condensed matter systems. There the underlying microscopic theory is a lattice of nuclei and electrons. We know "the theory of everything" as far as materials are concerned. At long distances, quantum field theory is the right description. To see light scalars, you need to fine tune microscopic parameters. A typical example is the order parameter near a phase transition. That is a light scalar, but you need to tune near the critical point for it to appear in the low energy QFT. Usually it is pushed up to high energies, that is the UV cutoff set by the lattice. On the other hand, light fermions are natural. The Fermi liquid describing most metals does not need to be fine tuned (let's forget about superconductivity for now, this requires a well understood but more subtle discussion). Landau's theory of phase transitions as well as Landau's theory of Fermi liquids is just the idea of naturalness spelled out. These are very powerful and scientifically proven theories. So no one is going to give up on naturalness as a property of quantum field theory ever — since it is true and it works.

   Now applying naturalness to particle physics is of course a guess. No one had any right to demand that it has to apply there. But it's an appealing idea. If at the microscopic level we have many choices for the fundamental parameters (as we do for a condensed matter system), you could expect that we live at a "generic" point in this parameter space. Of course we don't know if this is in fact so as we haven't established the microscopic theory in that case. But unlike model building based on completely random guesses, naturalness basically makes this one assumption and then asks what the consequences would be. And the consequence is that you would need a mechanism to keep the Higgs mass light. And all known mechanisms to do that would leave extra light particles behind. The fact that we didn't find any of these new particles in retrospect makes it look like naturalness was not the right idea to pursue. But to claim it was a bad idea to check out is something few physicist will ever agree. It's a simple to state and well motivated principle and one was able to test it.

5. **Peter Woit**
   February 11, 2022

   Andreas Karch/Sabine Hossenfelder,

   I don't want to start up yet another discussion of "naturalness" here, but as blog owner will just repeat my usual argument that the sensitivity of scalars to short distance physics only causes a fine-tuning problem for people who are pushing a theory with complicated new degrees of freedom at exponentially short distances. If you try to fix a problem caused by a bad theory at unobservably short distances by invoking more bad theory at the electroweak scale, it's not surprising the LHC tells you this doesn't happen.

   Dine makes the usual comments about naturalness and fine-tuning, but there's no serious discussion in his book. In the end, the failure of "naturalness" arguments for new physics at the electroweak scale gets used in the conventional way by Dine, as an argument for the anthropic landscape. The main point about this I think is just that if your argument leads you to untestable pseudo-science, you've proved that it's not a good scientific argument.

6. **eigenmoore**
   February 13, 2022

   That's why I like the book 'Why String Theory?' from Joseph Conlon, he has a entire chapter titled something like 'Experimental Evidence for String Theory' and it's just full blank page!

7. **Peter Woit**
   February 13, 2022

   eigenmoore,
   Dine has done much the same thing: in the text of the book he explicitly admits there's no way to make predictions using string theory and explains why.

   His innovation is to do this, then have the publisher put on the cover of the book promotional material claiming that inside it contains an explanation of how string theory will be investigated experimentally.

8. **Lee Smolin**
   February 15, 2022

   Dear Peter,

I was sorry to read that quote of Dine's as I've always found him openminded. But, in the spirit of being not even wrong, here is a test: Jay Armas has edited a collection of "Conversations on quantum gravity" including researchers of diverse points of view.

Let's assign to each researcher a +1 for every mention of an approach not their own which is objective ie checkable and correct, a -1 for every objective but false statement about another approach and a -2 for every statement evaluating an approach, theirs or not, that has no objective meaning by which it could be checked. I haven't done this-anyone want to try it?

9. **Peter Woit**
February 15, 2022

Lee,
To be fair to Dine, the full quote contrasts extremists: those "who view string theory as a sort of holy grail" to others who "view the subject with total disdain, often wearing their ignorance of even its most rudimentary aspects as a badge of honor" I'm assuming he'd put you as in neither category (not sure about me though…).

Exactly because Dine is otherwise very mild-mannered and willing to admit that critics might be right (to the point of even claiming to be on both sides of the argument) that this sentence stands out in the book. Those like Dine who publicly joined the string wars on what I would claim was the losing side (see his Physics Today piece), as open-minded as they may be, may still be nursing wounds from that era.

I agree with your implicit point that in the Armas volume the difference between the way string theorists and non-string theorists discuss the problems of various approaches is rather striking.

Seminar talk on Euclidean Twistor Unification and the Twistor P^1

February 14, 2022
Categories: Euclidean Twistor Unification

Today I gave a talk via Zoom at the [Algebra, Particles and Quantum Theory seminar series]() organized by Nichol Furey. The slides from the talk are [here]() (I gather the talk was recorded and video might be available at some point).

This talk emphasized explaining the twistor geometry, integrating some of what I've learned over the last few months thinking about the "twistor P^1" (see [here]()). For instance, one way to think of the basic object of Euclidean twistor theory is as $\mathbf {CP}^3$, together with a different real structure (the twistor real structure) than the usual one given by conjugation of complex coordinates. One thing that struck me while writing up these slides is that the Euclidean twistor story gets a lot of mileage out of identifying $\mathbf C^2$ and $\mathbf H$, together with taking as fundamental $\mathbf H^2$. It has always seemed possible that the octonions might have a role to play here; one way into that might be to think about identifying $\mathbf H^2$ with $\mathbf O$ in some analogous way to the $\mathbf C,\mathbf H$ story.

There's nothing new here about any of the many open questions of how to use this geometrical framework to get a fully worked out dynamics that would include the Standard Model and gravity. After a detour into number theory and hyper-Kähler geometry for several months, I'm now getting back to thinking about those questions.

Update: Video of the talk is now available [here]().

Comments

1. **Aula**
   February 15, 2022

   "one way into that might be to think about identifying H^2 with O in some analogous way to the C, H story."

   People have thought about this before, and it seems that the end result is always some variant of the Cayley-Dickson construction.

2. **bertie**
   February 15, 2022

   Very nice, your speculations seem noticeably stronger than in your earlier public writings on the topic. Also nice to see Octonions making a debut in your work, was Prof Baez an influence there?
   How was the talk received?

3. **Peter Woit**
   February 15, 2022

   Aula,

   More specifically what I'm wondering about is more geometrical, whether there's an octonionic analog of the relation between the twistor P^1 and the quaternions. That one is identifying C^4 with H^2 gives the twistor real structure on CP^3, what extra structure on CP^3 does one get from identifying H^2 with the octonions?

4. **Peter Woit**
   February 15, 2022

   Bertie,
   The more I think about this framework for thinking about fundamental physics, the more convincing I find it. You can interpret that as a reason others should take this seriously, or as evidence that I'm becoming increasingly delusional…

   The reaction to the talk seemed to me quite positive, there were good questions.

   John Baez's article on octonions in the Bulletin of the AMS is one of the few things I've read about them that gave me some insight especially into the geometry. I plan to soon reread it, see if there's anything there or elsewhere about the question I mentioned in the previous comment.

5. **Jackiw–Teitelboim**
   February 15, 2022

   Speculations on twistors and fundamental theoretical physics reminds me this lectures by Atiyah: https://arxiv.org/pdf/1009.4827.pdf
   On octonions, I am also reminded of this informal lecture given by Atiyah some years ago: https://www.youtube.com/watch?v=lp2cXnNt0Xs
   I wonder if he ever published anything more substantial about this…

6. **Peter Woit**
   February 15, 2022

   Jackiw-Teitelboim,
   Thanks for those references, which certainly resonate with me, for instance the story of the twistor P^1 fits perfectly with Atiyah's
   "In the big picture, physics is at infinity, and number theory at the finite
   points."

   Atiyah was always a huge inspiration to me, and what he has to say about his speculative dreams relating number theory and physics is fascinating. It's frustrating not to have more detail from him about this put down to paper. Also frustrating is what happened in the last few years of his life, when he apparently lost the ability to see needed details. The last time I saw him (2016 in Heidelberg), at one point he was talking about how the octonions were central for understanding gravity, but he didn't seem to have a really coherent argument one could follow (at least I couldn't). Likely what's in the video you link to may be the best source for trying to understand what he had in mind.

7. **Robert A. Wilson**
   February 16, 2022

   I watched Atiyah's 2010 video in its entirety, and I would like, if I may, to comment on what he says about octonions, since I am in fact an acknowledged expert on octonions. If you don't like it, then please ask Nichol Furey or John Baez, who are also acknowledged experts on the octonions, and will back me up. Atiyah did no work on octonions until his mid-70s, and he simply doesn't understand what he is talking about. It is clear in the work of Nichol Furey, and Geoffrey Dixon, and Corinne Manogue and Tevian Dray, and many others, that EM is complex, the weak force is quaternionic, and the strong force is octonionic. These are all people who have worked seriously, rigorously and productively in the area, which Atiyah never did. Atiyah links EM to real numbers, the weak force to complex numbers, the strong force to quaternions, and gravity to octonions. This is just nonsense.

8. **Peter Woit**
   February 16, 2022

   Robert A. Wilson,

   I just took a longer look at the Atiyah video and have to agree that on gravity/octonions there's not much at all there, just like some years later in Heidelberg there didn't seem to be much to the idea, beyond a vague "hunch". He does admit that the idea is "vague and philosophical". I agree that the connections strong interactions/octonions studied by Nichol Furey and others seem much more substantive. Beyond these, one might somehow speculate that triality has something to do with three generations, but I don't know that anyone has anything specific about that.

   The explanation Atiyah gave in that talk of the Tits-Freudenthal magic square was really nice, but there's no clear connection to physics.

   On this kind of thing, Baez has written extensively and in detail, I recommend his series of blog posts that start here
   https://golem.ph.utexas.edu/category/2020/07/octonions_and_the_standard_mod.html

   One aspect of the octonions that has often showed up in speculations about relations to physics is that the Lie algebra sl(2,O) in some sense is so(9,1), the Lie algebra of the 10d Lorentz rotations. This is of some encouragement to the idea of relating octonions to the 10d superstring, but I've never seen anything much coming out of that.

9. **Robert A. Wilson**
   February 16, 2022

   The Tits-Freudenthal magic square is undoubtedly very beautiful, but I wouldn't call Atiyah's remarks an "explanation". What is magic about the square is that it is symmetric, and that is a very deep and difficult theorem of Tits. For an explanation, you can't do better than the Barton-Sudbery paper, but for deep insight, in a vaguely unintelligible way, I think Vinberg has the edge. Atiyah's remarks do not seem to me to amount to much more than the observation that there are four dimensions of space, four fundamental forces, and four rows and columns in the magic square, but perhaps I am missing something. I agree with him that there is something very special about the number four, but I don't find anything deep in what he says here – although in other parts of his talk I think there is real depth.
   The idea that triality is connected to the three generations is a key part of Garrett Lisi's infamous "Exceptionally simple theory of everything", that few people take seriously these days. It is a seductive idea, but it hasn't worked out. You can make your own decision whether to work on it, but I lost interest in it many years ago. I don't know why you qualify the isomorphism between sl(2,O) and so(9,1) with the words "in some sense": it is a perfectly valid and rigorous mathematical result, well understood in the 1930/40s by von Neumann, Jordan, Schafer, Chevalley, etc. But they quickly abandoned the idea that it had anything to do with physics, and I think this conclusion is amply supported by your suggestion that it might have something to do with string theory!

10. **John Fredsted**
    February 17, 2022

    Robert A. Wilson,

    I would like to take issue with "It is clear in the work of Nichol Furey, and Geoffrey Dixon, and Corinne Manogue and Tevian Dray, and many others, that […] *the strong force is octonionic*" [my italics].

    In section 5 of the nice article "Quark structure and octonions", by Günaydin and Gürsey (J. Math. Phys. 14, 1973) it reads: "Since G2 has only real representations, only real representations of SU(3) can occur in the representations of G2."

    So, for one thing, the G2 automorphism group of the octonions, cannot naturally explain, say, the (physical) difference between the quark triplets [3] and [3*].

11. **Geoffrey Dixon**
    February 17, 2022

    John Fredsted

    Günaydin and Gürsey also demonstrated that the complexified octonions (S = C ⊗ O) give rise to an SU(3) with respect to which S itself (which is a spinor space – see for example, Conway and Sloane Sphere Packings, if you are dismissive of the 4 names listed above) transforms as a singlet, anti singlet, triplet, and anti triplet. This barely scratches the surface.

    Cheers, Geoffrey Dixon

12. **Robert A. Wilson**
    February 17, 2022

    John Fredsted,

    I happen to agree with you, but I was not discussing my own opinions, I was discussing the work of others, so that *if* you believe the octonions are useful in physics, *then* they must show themselves in the strong force. If they do not show themselves there, as you seem to be saying, then I don't see how you can believe in the usefulness of octonions in quantum gravity – which seems to have been Atiyah's point of view, and still has some currency today. One can argue about lots of this – but not on Peter Woit's blog – but I agree with Peter that both the weak force and gravity are quaternionic, quite contrary to what Atiyah was saying.

13. **Peter Woit**
    February 17, 2022

    All,
    Enough about the octonions and gravity. From all I can tell, there wasn't a serious Atiyah proposal about gravity. About SU(3) and octonions, I'm just starting to think about whether this fits into the twistor geometry I'm studying. Pointers to previous work on this continue to be appreciated and I'll look into them.

14. **Blake Stacey**
    February 17, 2022

    Perimeter hosted some online talks about "Octonions and the Standard Model" last year; the recordings are on PIRSA.

15. **Robert A. Wilson**
February 18, 2022

If you want to relate octonions directly to twistors, you need the split octonions in order to get the right symmetry groups. Then you get SO(4,2) and SO(3,3) and the corresponding spin groups in the isotopy groups, and SU(2,1) and SL(3,R) as the possible real forms of SU(3) in the automorphism group. Physicists tell me they are not interested in non-compact real forms. Various of the people already mentioned have their various ways of dealing with this.

If you are interested in links to number theory, you'll want discrete versions of octonions, which are discussed in the Conway/Sloane book to some extent, but you probably want to look at Conway/Smith "On quaternions and octonions" for more. There is also a section of my book "The finite simple groups" that goes beyond what I have seen elsewhere. Incidentally, 't Hooft's latest paper arxiv:2202.05367 asks for integer versions of SU(3) for describing physics at the Planck scale. A list of all such groups is in Blichfeldt's "Finite collineation groups" (1917), and an argument for a specific choice is in arxiv:2202.08263.

16. **Dick Gross**
February 18, 2022

I don't know anything about the connection of exceptional groups to physics, although some claims on E8 have been debunked by Distler and Garibaldi:

Distler, Jacques; Garibaldi, Skip
There is no "theory of everything" inside E8.
Comm. Math. Phys. 298 (2010), no. 2, 419–436.

The symmetry of the magic square is not difficult from the point of view of

Deligne, Pierre; Gross, Benedict
On the exceptional series, and its descendants.
C. R. Math. Acad. Sci. Paris 335 (2002), no. 11, 877–881.

which also extends the magic square of Lie algebras to a magic triangle of Lie groups.

17. **SG**
February 21, 2022

… a glass of pure wine to facilitate the debate…

– Octonions are non-associative (or more exactly anti-associative)
and so far nobody knows physics with such property.

– Complex Quaternions, (a.k.a. Weyl spinors) C x H — isomorphic but not the same as C^4 or H^2 — are exactly what one needs to include SM and gravity under one roof. Twistors are basically spinors enriched by projective geometry point of view.

– E8 ~ SU(5) x SU(5) ~ [SU(2) x SU(3)] x [SU(2) x SU(3)] — doubly GUT 🙂 — has to be also part of the final solution, as spinor geometry is 8D, and E8 is the most exceptional Lie group describing "the-most-ever-conceivable-still-admissible-eight-dimensional-something".
It is simply not restricted just to Octonions.
(Distler, Jacques; Garibaldi, Skip's argumentation is correct only in respect to Lisi's "theory of everything" but not universally).

ICM 2022 and the Invasion of Ukraine

February 21, 2022
Categories: Uncategorized

The news this evening that Russia is sending troops into ~~the~~ Eastern Ukraine and in effect announcing annexation of at least part of ~~the~~ Ukraine carries extremely disturbing implications for the whole world. On a much more minor scale of importance, I don't see how the IMU has any choice but to cancel this year's ICM planned for St. Petersburg in July.

Four years ago when the IMU chose St. Petersburg over Paris for the 2022 ICM I commented here on this blog:

> It does seem to me though that in these worrisome times, when offered the choice between the world's most active opponent of liberal democracy and one of the great remaining healthy liberal democracies, the other choice than the one the IMU made would have been the better one…
>
> I agree that in general it's best to keep mathematics and the ICM out of politics. A question to think about though for those who know the history of the 1930s is that of whether there was some point during the rise of Fascism that one would stop thinking it was a good idea to have the ICM in a Fascist capital. We're not yet far along the horrific path of the 1930s, but maybe that just means that all should be thinking about what can be done to keep the world from going down that path again.

I sympathize with many who felt that the decision to hold the ICM in Russia was an important way to support Russian mathematicians and a reasonable gamble that Putin would not take his country down the path he now appears to have chosen. But right now it's looking like that gamble failed and the IMU will have to figure out what to do about its mistake.

I don't want to host a general political discussion here, especially not with the all too many people I've heard from who don't have a problem with burying liberal democracy. If your comment is not about the ICM, please don't submit it.

Update: On February 10 an organization of Ukrainian mathematicians emailed the ICM invited speakers asking them to cancel their talks (*and on January 31 had emailed the AMS leadership*). I'm curious to know if any responded to this, and if the Russian military invasion will lead to some decisions to cancel talks.

Update: According to this from @UkrainianMath, the AMS position was recently that "the AMS leadership is closely tracking the situation surrounding the ICM and believes that it is still premature to advocate a boycott". The "premature" indicates that there is some point at which AMS leadership agrees that the ICM should be canceled. Is the situation of Russian troops occupying ~~the~~ Eastern Ukraine still "premature", or will the AMS wait for them to take Kiev?

Update: I noticed there's an AMS-NSF-Simons-ICM Travel Grant program to fund ICM participation by US mathematicians. It was supposed to announce awards this month. Will this program go forward or will the grants be canceled?

Update: Many Russian mathematicians likely feel the same way about this as Edward Frenkel who calls this "a catastrophe for Russian people and all Slavic people".

Update: The AMS has issued a statement urging the IMU to cancel the ICM and has suspended the AMS-NSF-Simons-ICM travel grant program

Update: There's a statement out signed by invited ICM speakers. Unfortunately it has been overtaken by events, with little more than a request that the IMU "elaborate and announce contingency plans" in case of war, something that would have made sense a month ago, but not now. Nothing yet that I'm aware of from the IMU or other mathematical societies than the AMS.

Update: The French SMF has a statement calling on the IMU to not hold the ICM in Russia during 2022. In another statement, the London Mathematical Society "strongly recommends that the IMU not hold the ICM in Russia in July 2022." Also in France, INSMI at the CNRS has this.

Update: There's a long twitter thread about this here. It includes a contribution from Ian Agol: "As a chair of the topology selection committee, I requested @ICM2022 that the opening ceremony not be presided by a head of state (presumably Putin), but they were not willing to consider this."
This makes clear the fundamental problem with deciding to hold the ICM in a country ruled by a fascist dictatorship. If you do this, you end up putting the conference under the control of the dictator, because anyone inside the country cannot oppose them. Those outside the country end up having to either go along with the dictator, or cancel the conference, and this is where the IMU is now.

Update: The IMU has issued a (rather empty) statement, saying that "The Executive Committee of the IMU is now assessing the situation"

Update: More statements from national math societies: Italy, Canada, Poland, Lithuania.

Updates: The IMU this morning has on their website:

> The IMU Executive Committee is currently assessing the highly disturbing events that are taking place in Ukraine and their implications for the IMU. We will return with a statement as soon as it is available.

There are new statements calling for canceling the ICM from the European Mathematical Society, the Australian Mathematical Society, the Swedish Mathematical Society and others.

Update: Via @UkrainianMath, the latest from the IMU Secretary:

> The IMU EC has been sitting in meetings for two days now, discussing the situation and how to respond properly. I ask for your understanding that it is more difficult for a global organization to meet and discuss this issue.
>
> Of particular concern to us is how to find a possible way to carry out a General Assembly and an ICM if possible, but outside Russia. Furthermore, we do not want to cause damage to our Russian colleagues, who have spent endless hours preparing for an ICM.
>
> The IMU webpage does contain a statement that we are working on this, and until we have reached a decision, which will be very soon, this is the best we can do.

Update: The IMU has announced that the ICM will take place as scheduled, but as a free fully virtual conference, not in-person in St. Petersburg. The IMU General Assembly will take place in-person, but at a different location still to be chosen, outside Russia.

Update: The ICM website (icm2022.org) no longer exists, with that address redirected to the IMU site mathunion.org. There had been no activity on the @ICM2022 Twitter account since Feb. 11, but now there's a statement from four of the Russian mathematicians who had been involved in organizing the ICM:

We condemn the madness, the injustice, and the irreversibility of war that threatens the very existence of humanity. While our losses cannot be compared to the losses and the suffering of millions of people in the Ukraine, we are devastated to see all of our dreams and all of our work of many years ruined. The goals towards which we worked could not have been further from the horror that is happening and those responsible for it. Still, amid the ruins of our dreams, we feel left with an insurmountable debt that may take much longer than the life of our generation to be forgiven.

D. Belyaev, A. Okounkov, J. Pevtsova, S. Smirnov

Update: See here for a letter to the IMU from some mathematicians arguing against the decision to hold the ICM online.

Comments

1. **Alessandro Strumia**
February 22, 2022

Can I attend attend a physics conference in Russia, if I cannot attend physics conferences in your liberal democracy nor post to arXiv one scientific publication?

2. **sdf**
February 22, 2022

If the IMU made the St. Petersburg decision even though Crimea was illegally occupied at the time, I rather doubt they feel any kind of moral obligations of this type.

3. **Genghis Cohen**
February 22, 2022

All the young Russian mathematicians I know – and there are many in my city/institution – were quite disgusted with the choice to host the ICM in St. Petersburg. I'm very curious to see what happens now.

4. **Peter Woit**
February 22, 2022

Alessandro Strumia,

I don't know what the nature is of problems you have attending US physics conferences.

It looks like the IMU will choose Philadelphia for the 2026 ICM. Depending on the results of our 2024 election, that might be another one that gets canceled.

5. **anon**
February 22, 2022

I'm expecting to see mass cancellations by speakers unless the situation in Ukraine improves significantly.

I believe it's also time to rethink the whole concept. ICM has become so large and costly to organize that few places are able to even think of hosting.

6. **Bo Berndtsson**
February 22, 2022

I can only say that I agree with Peter. It has now become clear that it was a big mistake to let Russia host the coming ICM. I, for one, cannot imagine myself at the inaugural ceremony, applauding representatives of the Putin administration.

7. **Andrzej Daszkiewicz**
February 22, 2022

ICM 1982 in Warsaw, Poland, had been postponed for a year, till 1983 and the suspension of the martial law (which had been imposed in December 1981), so there is a precedent. As a young Ph.D. student in Warsaw at that time I was really happy that the Congress had not been calcelled, but of course the situation now is completely different, Russian mathematicians are not as isolated as we had been behind the iron courtain.

8. **Anonymous**
February 22, 2022

FYI Ukrainians generally consider it offensive to refer to "the" Ukraine

9. **Noah Snyder**
February 22, 2022

I'm glad to see that someone mentioned the 82/83 Warsaw ICM, which is a very interesting bit of history that I didn't know about until last week when I was looking for any parallels to previous ICMs. If Kyiv were hosting the ICM this year I'd expect we'd see a similar delay as in 82/83, but of course that's not the current situation. There didn't really seem to be any previous congresses with anything parallel to the current situation (e.g. Moscow hosted in '66 not '68 or '80, Berkeley hosted in '86 not '70 or '04). At any rate I agree that it pretty clearly needs to be cancelled or moved at this point. Not sure if it's too late logistically to move it to Paris. It's all very sad, my entire intellectual genealogy is in the city of St. Petersburg and it's a wonderful mathematical heritage that deserves to be celebrated.

10. **Will Sawin**
February 22, 2022

It seems the AMS is pulling its representatives, cancelling the grant program, and urging IMU to cancel: https://twitter.com/amermathsoc/status/1496255307697340418

11. **Low Math, Meekly Interacting**
February 23, 2022

Interesting how autocracy and demagoguery can destroy nearly anything. It's incredibly sad to see Russian intellectuals punished as the cost of denying prestige to a government they themselves likely universally deplore. It's the obvious thing to do, but what a sickening reversal of the East-West cultural exchange set in motion at the end of the 20th century. What a world we live in.

12. **Peter Woit**
    February 23, 2022

    LMMI,
    With the invasion of the Ukraine, the problem with holding the ICM in Russia goes way beyond its adding to the prestige of the neofascist Russian government, and Russians (mathematicians and non-mathematicians alike) are victims of their leadership in ways that go far beyond not having an international conference there.

    The reason I think it is now inconceivable that the ICM will be held as planned is that, even if you believe in completely divorcing math from politics and not punishing citizens of a country for their government, as a practical matter holding the ICM would be a disaster. The Russian state has now gone to war by sending its military in to occupy Ukraine and no one knows how bloody and ugly this is going to get. With the US and most Western countries every day announcing new actions against Russia and Russian counter-actions sure to come, I can't see how any sensible person would now think it would be a good idea to book plane tickets and make hotel reservations so as to plan travel into a war zone in July. Optimistically the fighting will take place not with bombing and shooting but with financial, trade and other sanctions, but that likely will be enough to make basic things like flying to St. Petersburg soon impossible.

13. **MT**
    February 23, 2022

    "I can't see how any sensible person would now think it would be a good idea to book plane tickets and make hotel reservations so as to plan travel into a war zone in July."

    Not only is this correct, but you wouldn't want to book plane tickets for any event that would require entering Ukranian airspace. People fail to realize how close to WWIII things are getting.

14. **Peter Woit**
    February 23, 2022

    MT,
    Yes, although I'm assuming airlines are being appropriately cautious now, well aware of what happened in 2014 (the Russians shot down a commercial airliner flying over Eastern Ukraine, killing all aboard).

    As this continues, even if a Westerner could get an airline to fly them into St. Petersburg, there's also the other war-zone problem of ending up interned as a prisoner of war. Until recently one could have made the case that Putin would not imprison and hold hostage citizens of Western countries since that would hurt his relations with those countries, but that's no longer a very convincing argument.

15. **Edward**
    February 23, 2022

    I am surprised none of the comments here are arguing against relocating the ICM. Does the ICM really want to become involved in the sordid U.S. regime change operation in Ukraine? The U.S. spent $6 billion on regime change efforts in Ukraine, culminating in the 2014 coup which overthrew the democratically elected president of Ukraine. This essentially led to a civil war which has wrecked the country. The anti-Russian faction(including Nazis) seized power in Kiev and the ethnic Russians in the West revolted. If the ICM wants to boycott a country what about America? The list of American crimes is long, starting with the CIA organized coups against the democratically elected governments of Guatemala, Iran, Indonesia, and Chile. More recently, there are the genocidal sanctions against Iraq, which killed hundreds of thousands of people, followed by an invasion which destroyed the country. America has wrecked Libya, Syria, Yemen, and Ukraine and is trying to replace the democratically elected government of Venezuela with U.S.-appointed Juan Guido. Who gets to decide which countries are committing crimes and get boycotted? Will the same standards apply to every country?

    I think it is a bit rich to call Mr. Putin a "neo-fascist". He is a very popular democratically elected politician with an approval rating above 60%, much higher then our president or Congress. He has done an excellent job rebuilding Russia after the disastrous Yeltsin years. There has been an extreme propaganda campaign against Russia by the West for years, and I hope the ICM does't become part of this effort. After the Iraq WMD hoax, Americans should be more careful about taking U.S. government and media accusations at face value.

16. **Peter Woit**
    February 24, 2022

    Please do not respond to the comment by "Edward". I've been deleting things like this as they come in, thought it might be a good idea to leave one so that people have an idea what the pro-Russian propaganda campaign that has been going on looks like.

17. **Andrzej Daszkiewicz**
    February 24, 2022

    There is a similar statement calling IMU to not organize ICM in July in St. Petersburg issued by the Polish Mathematical Society (PTM), available in Polish here:
    https://www.ptm.org.pl/zawartosc/stanowisko-zg-ptm-w-sprawie-icm-2022

18. **Yiannis Sakellaridis**
    February 24, 2022

    The commentary about the statement by ICM speakers ("a month ago") is unfair. As of last weekend, European leaders were shuttling between Kyiv and Moscow and projecting optimism that diplomacy would prevail. In any case, the statement sets some standards – that even the threat of war is incompatible with the spirit of unity that the ICM should represents – whose implication for today's situation is completely clear. The statement has now been signed by more than half of the invited speakers.

19. **Peter Woit**
    February 24, 2022

    Yiannis Sakellaridis,
    While I'm sympathetic with the difficulty of organizing a statement like this, I'm afraid I have to disagree. The Russian military buildup and threats to the Ukraine have been going on for months, and those months were the time for a call to the IMU to announce that a Ukraine invasion would imply cancellation of the ICM and to make contingency plans.

    The IMU has had months now to make a contingency plan about what it would do in case of an all-too likely Ukraine invasion by Russia. This week is not the time for calls for contingency plans but for implementing them.

20. **Robert Owczarek**
    February 24, 2022

I think ICM has to be canceled, stressing the role of the Putin's regime in nullifying its legitimacy for having such privilege. I am afraid though that IMU is partially connected with Russia and Putin's supporters. How otherwise the ICM would land in St. Petersburg in the first place? And make no mistake, Putin, when he crossed this Rubicon, will not stop there. All Europe is now in danger, and in effect the world. Putin must be stopped.

21. **Peter Woit**
February 24, 2022

Robert Owczarek,
I don't know what the IMU's rules are for making a decision about this. The IMU executive committee is here
https://www.mathunion.org/organization/imu-executive-committee
its ICM structure committee is here
https://www.mathunion.org/icm/icm-2022
and the ICM executive organizing committee is here
https://icm2022.org/organization

The ICM executive organizing committee is entirely Russian and I suspect any of them who agreed with canceling the ICM would put themselves personally in grave danger.

The other IMU committees are mostly non-Russian and I know of no reason they should not be agreeing on the obviously necessary action of canceling the conference.

As I wrote in my comment to Yiannis Sakellaridis, it has been clear for quite a while that this might happen. These IMU committees have already had a lot of time to try to decide what they would do in the case of a Ukraine invasion, so unclear why we're not hearing something publicly from them.

22. **Zee**
February 24, 2022

The IMU has the responsibility to cancel the ICM for the sake of the organizers and Russian mathematicians.

23. **Sam Hopkins**
February 24, 2022

Could the ICM take place entirely online? I was at the ICM in Rio in 2018 and it was a very enjoyable experience, but the experience of attending many remote conferences during Covid has made me rethink the benefit of traveling long distances for in person conferences, not to mention in terms of money and CO2 emissions it feels frankly wasteful.

24. **Peter Woit**
February 24, 2022

Sam Hopkins,
That's one possibility, and it may be what happens.
There's no way to change the venue of a 5000 attendee-scale conference like this on the timescale of a few months, but it might be possible to organize a hybrid conference with a much smaller in-person component.
Possibly the reason for no statement from the IMU is that they are trying to decided about this.

25. **Noah Snyder**
February 24, 2022

Yes, there's no way they can logistically hold a normal ICM in person at another location this summer. The natural thing is to move it and hold it next summer, but it's not even clear to me whether you can reschedule an event of this size on "only" one year's notice. It's going to be genuinely difficult for the IMU to figure out what options they have. Hopefully they didn't just start working on contingency plans this week. It may end up needing to get rescheduled for next summer and also shrunk in some way.

26. **Engineer**
February 24, 2022

Looks like the football community is set to deal with a similar situation as the UEFA are set to strip Saint Petersburg of the hosting rights for 2022 Champions League final. It would be sad if they can act while the IMU can't.

27. **Sam Hopkins**
February 24, 2022

The IMU has now made a statement: https://www.mathunion.org/fileadmin/IMU/Publications/CircularLetters/2022/IMU%20AO%20CL%204_2022.pdf

28. **Johan Richter**
February 24, 2022

The tweet by Ian Agol has been deleted.

29. **mathlog**
February 24, 2022

It would be interesting to know what Andrei Okounkov and Stanislav Smirnov will say on the matter. They are members of the organizing committee (and kind of its face, see https://icm2022.org/blog/interview-with-andrei-okounkov-and-stanislav-smirnov) and are both not based in Russia.

30. **Peter Woit**
February 24, 2022

mathlog,
I have no knowledge specifically of what Okounkov or Smirnov think about this, but I'm guessing that almost all the Russian mathematicians associated with organizing the ICM feel much the same: the Ukraine invasion is a terrible disaster and the conference can't take place in July, but saying this publicly would carry severe implications for their personal safety so they're not going to do so.

31. **Lukasz**
February 24, 2022

As a supplement to the messages of Andrzej Daszkiewicz and Noah Snyder, I would like to add here that there on the website https://mathshistory.st-andrews.ac.uk/ICM/ICM_Warsaw_1983/ , is placed a story of ICM 1983.

PS. My Father was enjoying at that time that there were some chances that Alfred Tarski would attend this Congress. He wanted to discuss with Tarski on some problem (in mathematical logic), solved by my Father. However, unfortunately, Tarski was absent there. He died about 2 months after ICM 1983.

32. **David Roberts**
February 24, 2022

The Australian Mathematical Society has announced they don't want the IMU to proceed with holding the Congress in St Petersburg: https://austms.org.au/statement-on-icm-2022/

33. **Gil Kalai**
February 25, 2022

Indeed these are very sad days.

Regarding the ICM. It is not clear if the war in Europe will enable any form of ICM in 2022, but it seems pretty clear that it will not take place in St Petersburg as planned. Here are some thoughts about a possible way to have a congress if the conditions will make it possible.

First a remark: One nice thing about Rio-ICM2018 was that all invited, public, plenary lectures and also the panels were videotaped (in a very good quality) which provided (in addition to the traditional Proceeding) a very valuable resource. Now, if in additional to good quality videotaping, the lectures are also broadcast then we can consider a hybrid structure of many venues (10-15) where speakers (mostly) will not speak from their homes but will be gathered to several well-videotaped sub-conferences with a live audience. (Still some speakers may speak from their own institutions (perhaps also with an audience) and some speakers may choose to record their lectures in advance.

Mathematicians could have some possibilities for talks that interest them: to attend (if it is nearby), or to listen to the broadcast in real time, or to watch the video later.

Making a viable program with this format could be an interesting combinatorial problem (suitable for the young minds). Probably the early afternoon in Europe could be a good time for plenary talks. We may need to make the ICM a few days longer.
For such a plan we can choose 10-15 venues around the world, with some local activities as well in addition to giving and watching ICM lectures.

One more idea. Perhaps we can give all speakers of all types (except, of course the prize winners) an opportunity to post a 15-20 minute introductory video that will be posted around May of the year in the ICM site. (This can be extended also to contributed talks/abstracts and to lectures in satellite conferences, and to other participants.) While only a few percent of people will watch the videos in advance this can still be very helpful.(I tried myself to implement this idea with my live lectures in the pandemic times.)

Maybe some of the social events could become distributed as well.

Actually I made few of these remarks/suggestions in 2019 before the pandemic and certainly in no connections with the terrible war here:
https://terrytao.wordpress.com/2019/02/05/request-for-comments-from-the-icm-structure-committee/#comment-512332

34. **Arend Bayer**
February 25, 2022

I want to point out that the letter signed by ICM speakers (I am one of them) states that the ICM cannot be held while the organising country is *threatening* a war against its neighbour, and that it cannot be held in the context of a potential war.
I think the implication for the current situation is clear.

On a more important note, I am impressed by the courage of the >370 signatories of this letter by Russian scientists against the war: https://trv--science-ru.translate.goog/2022/02/we-are-against-war/?_x_tr_sl=auto&_x_tr_tl=en&_x_tr_hl=en&_x_tr_pto=wapp

35. **Richard Séguin**
February 25, 2022

Major sports communities have been acting quickly and making decisions. The International Olympic Committee is calling for all sporting federations to cancel or relocate events in Russia in addition to banning Russian and Belarusian flags and anthems at sporting events, Formula One has cancelled the Russian Grand Prix at Sochi, the football event at St Petersburg (Champions League Final) has been moved to France, and important ski events scheduled for Russia have been cancelled or moved.

I do understand the immense difficulties of actually planning the move of a large meeting like this, but I hope they are not waiting to figure out if they can in fact do this before deciding whether or not to cancel St Petersburg. A meeting at St Petersburg is just simply out of the question. They should cancel now and then try to figure out if they could move the meeting. If they finally decide that there isn't enough time to plan a move, then the meeting will have to be sacrificed this year, or they could consider virtual options — which we've become quite skilled at in the past two years — as discussed by Gil Kalai above.

36. **Peter Woit**
February 25, 2022

Arend Bayer,
I understand the difficulty of drafting something that most speakers would agree on, but I still think that the formulation chosen would have been very appropriate a month ago, but became inappropriate on Monday when the invasion started. I realize this is unfair, but as written the letter indicated no problem with a July ICM in Russia if the Ukrainians had been defeated and "peace" imposed on them by that time.

And I agree completely about the courage of Russians willing to publicly raise their voices against the war.

37. **Anon42**
February 25, 2022

One purely symbolic but low effort gesture some readers of this blog could make to support Ukrainian civil society would be submitting their best work to the journal Symmetry, Integrability and Geometry: Methods and Applications (SIGMA, https://www.emis.de/journals/SIGMA/). This is a high quality but not super prestigious mathematical physics journal run by volunteers from the National Academy of Sciences of Ukraine that is free for readers, free for authors, and an arxiv overlay. Symbolism aside, this is probably how Western journals should have set up their publishing models in any case. Some major caveats…I have no idea whether the editors would support this since it's certainly possible they're unable to deal with reviewing articles given the more pressing matters at hand. I also have no clue about their politics.

38. **Dylan Thurston**
February 25, 2022

The letter text was finalized on Sunday, before the invasion started in any sense.

39. **Greg_S**
February 25, 2022

Here's what's not clear to me: While the IMU is dragging its feet, have any of the other mathematics societies mentioned in the updates called on their members to boycott the meeting, and have any individual mathematicians announced their refusal to attend?

40. **an American mathematician**
February 25, 2022

> Update: Many Russian mathematicians likely feel the same way about this as Edward Frenkel who calls this "a catastrophe for Russian people and all Slavic people".

> I have no knowledge specifically of what Okounkov or Smirnov think about this, but I'm guessing that almost all the Russian mathematicians associated with organizing the ICM feel much the same: the Ukraine invasion is a terrible disaster and the conference can't take place in July, but saying this publicly would carry severe implications for their personal safety so they're not going to do so.

I'm not so sure that this is actually true, Peter. In fact, I suspect that the opposite may very well be true. I think that many Russian mathematicians would still like the ICM to take place in Petersburg, and would be upset if it were canceled, moved, or boycotted. The ones that work in the West would probably be hesitant to express such an opinion publicly, due to its sensitive political nature.

I hear that support for the "special military operation"/"invasion" in the/~~the~~ Ukraine is very high among the ethnic Russian general population, and I see no reason why mathematicians would be any different. As an American who has spent a semester in Petersburg, I can tell you that nationalism is strong, even among the scientific intelligentsia in post-Soviet Russia.

Shafarevich, for example, never left Russia, despite the mass exodus of former Soviet mathematicians for the West in the 90s. His political views can perhaps be described as Russian Orthodox chauvinism, and his writings have been criticized in the West as anti-Semitic. I still remember the efforts of American mathematicians in the 90s to expel him as a member of the NAS. He eventually staged his own boycott of sorts by resigning in protest against the 2003 U.S.-led invasion of Iraq. Shafarevich was a particularly vocal case, but my guess is that he was not the only one to hold such views, even if you include the younger generations of Russian mathematicians.

Frenkel is, of course, a great mathematician, but I wonder whether he really speaks for all Russian mathematicians. I often hear stories of how Jewish and other minorities felt oppressed in Soviet Russia (Frenkel was apparently not admitted to Moscow State for political reasons).

I don't know what Okounkov or Smirnov think of the statements from other national math societies either. *mathlog* says that neither is based in Russia, but I believe that Smirnov has a position at St. Petersburg State, and that Okounkov has a position at the Higher School of Economics, as does Bogomolov. In fact, in the interview that *mathlog* linked to, Smirnov says, "We also support the request not to sacrifice mathematics in general and the activities of the IMU in particular to boycotts and other political acts." Okounkov is also representing Russia on the IMU EC as a member-at-large. I suppose we will see what happens.

41. **Peter Woit**
February 25, 2022

an American mathematician,
My experience may be limited, but from what I've seen Russian mathematician fans of Putin and the attack on Ukraine are about as rare as American mathematician fans of Trump and the attack on the Capitol.

Okounkov definitely is US-based, until recently at Columbia (on leave this year), this fall I hear at Berkeley. He has been one of the main organizers of the 2022 ICM, and I can only imagine how difficult this situation is for him and his hopes for this event.

42. **Peter Woit**
February 25, 2022

Greg_S.,
At this point I think people see no need to call for a boycott of something that isn't going to happen.

43. **jsm**
February 26, 2022

February 26, 2022. The IMU continues to support Putin's Russia with its website advertising: "The International Congress of Mathematicians (ICM) will take place in Saint Petersburg, Russia, between 6–14 July 2022."

44. **Sam Hopkins**
February 26, 2022

The prominent Russian mathematician Anatoly Vershik (https://en.wikipedia.org/wiki/Anatoly_Vershik), who is in fact based in St. Petersburg, has called for the cancellation of ICM 2022: https://twitter.com/SocMathFr/status/1497204027163037700.

45. **Sam Hopkins**
February 26, 2022

Huge update! The ICM will take place fully online, at the same time as scheduled: https://www.mathunion.org/fileadmin/IMU/ICM2022/ICM_2022_statement.pdf.

46. **Sam Hopkins**
February 26, 2022

Sorry to keep spamming comments, but another thing your readers might be interested in is that Terry Tao, who is the chair of the ICM Structure Committee, is inviting discussion of how best to use the virtual format of the ICM on his blog at https://terrytao.wordpress.com/2022/02/26/the-international-congress-of-mathematicians-icm-2022-will-now-be-fully-virtual-how-best-to-utilise-this-format/. (Note that he wants to limit discussion to the *format* of the ICM, and not the surrounding politics.)

47. **a Russian mathematician**
February 26, 2022

I’d like to second an American mathematician. There are Russian mathematicians of this type. Alex Bondal still openly defends the invasion in his public Facebook posts. There are others like him, who are less obnoxious.

48. **Will Sawin**
March 2, 2022

A letter from (currently) 351 Russian mathematicians bravely speaking out against the invasion: https://trv-science.ru/2022/02/mathematicians-against-war/

49. **Vyacheslav Boyko**
March 3, 2022

Reply to Anon42:
We greatly appreciate your support during these difficult times. Although we stay in Kyiv, everything is still more or less OK with all the members of the SIGMA team. Despite the Russian aggression, we continue to work on SIGMA (https://www.emis.de/journals/SIGMA/) and the submitted articles are processed without substantial delays.

50. **anon**
March 11, 2022

The latest IMU circular tells that the General Assembly and the Prize Ceremony will be held in Helsinki.

51. **Jackiw–Teitelboim**
March 21, 2022

https://inference-review.com/on-the-war-in-ukraine

A Few Items

March 4, 2022
Categories: Uncategorized

Some short items and links:

- Among possible futures that I never would have dreamed of during my student days was that someday my Nobel-prize-winning undergraduate advisor would "try to rile" my Nobel-prize-winning graduate school professor at a Bohr Centennial celebration by quoting me. I hope the quote at least was one I would agree with.
- Also on the topic of hoping I agree with what I say publicly, there's an NHK documentary about Mochizuki and the abc conjecture that has recently been finished, was supposed to air in Japan this weekend, now delayed til next month due to more timely news from Ukraine. I did an interview with the filmmakers here in NYC last year and they talked to many other people. No idea how they'll manage to deal with this controversial story, coming from a Japanese perspective.
- At Quanta magazine, another article about the "naturalness problem", headlined A Deepening Crisis Forces Physicists to Rethink Structure of Nature's Laws. This has the usual problem with such stories of assigning to the Standard Model something which is not a problem for it, but only for certain kinds of speculative attempts to go beyond it. John Baez makes this point in this tweet:

  > Indeed, calling it a "crisis" is odd. Nothing that we really know about physics has become false. The only thing that can come crashing down is a tower of speculations that have become conventional wisdom.

  James Wells has a series of tweets here, starting off with

  > The incredibly successful Standard Model does not have a Naturalness problem. And if by your criteria it does, then I can be sure your definition of Naturalness is useless.

  He points to a more detailed explanation of the issue in section 4 of this paper.

- My criticisms of some Quanta articles are motivated partly by the fact that the quality of the science coverage there is matched by very few other places. If you want to work there, they have a job open.
- But if you really want to cash in on gazillionaire money going into mathematics, you might want to try for some of the $20 million that crypto entrepreneur Charles Hoskinson is giving Carnegie Mellon to establish the Hoskinson Center for Formal Mathematics. Early in his career Hoskinson was in a Ph.D. program in analytic number theory, but bailed and later joined Ron Paul's presidential campaign, and ended up in crypto since "When Bitcoin came out, it was like the spiritual successor to what Ron Paul was talking about" (see here).
- Someone who is not going to be getting Hoskinson funding is Michael Harris, whose The Silicon Reckoner you should follow for an alternate take on "formal mathematics". For the reaction to such criticism from the formalizers, you can check out this Zulip Chat archive, and then responses from Harris here.
- For Grothendieck news, see here, here and here.

Update: There's a statement out today from Breakthrough Prize Laureates strongly criticizing Russia's invasion of Ukraine. There's also a truly appalling statement from the Breakthrough Prize Foundation itself, not in the least critical of Russia or Putin and abusing the memory of Stephen Hawking. Witten characterizes the Foundation statement as "disappointingly vapid".

Update: Milner seems to have realized that refusing to criticize Putin was not a tenable position. A new statement from the Breakthrough Prize Foundation starts off with:

> As the terrible war in Ukraine continues, with casualties and atrocities mounting, the Breakthrough Prize Foundation strongly condemns Russia's invasion of Ukraine and its unprovoked and brutal assaults against the civilian population.

and announces a further \$ 3 million donation:

> the Foundation today pledges a further \$3 million in funding to support physicists, life scientists and mathematicians who have been forced to flee from Ukraine. We hope that this donation will help talented researchers contribute to human knowledge in such dark times.
>
> The Breakthrough Prize Foundation stands together with the Ukrainian people, its scientists and their families.

Comments

1. **4gravitons**
   March 4, 2022

   For the record, the relevant quote/paraphrase was "string theory is not even wrong", so I'm pretty sure you're safe there.

2. **Peter Woit**
   March 4, 2022

   4gravitons,

   Thanks! Although these days I'm leaning more to the "string theory (as an idea about unification) is just wrong" point of view…

3. **Sabine**
   March 5, 2022

   Just here to mention that my 2018 "Lost in Math" is about the naturalness myth that led particle physicists to erroneously expect new physics (besides the Higgs) to show up at the LHC. I agree with Baez and Wells, but I think the larger question looming here is: how could this have happened?

4. **Bob**
   March 5, 2022

   Is that twitter account confirmed to really be Edward? It doesn't have the blue checkmark next to his name, and there are a lot of fake accounts out there.

5. **Peter Woit**
   March 6, 2022

Bob,
As far as I know it is him. If someone knows for a fact that it is a fake, let me know.

6. **Peter Woit**
March 6, 2022

For an exchange between Sabine and the author of the Quanta piece, see here
https://twitter.com/skdh/status/1500516249251549186

I'm sympathetic to Sabine's frustration that her argument about this has been ignored, since I also wrote a book giving an argument about this which has been ignored. Back in 2002 I wrote about this in the section on SUSY in my book (see page 164 of the US edition). There I explained that the hierarchy problem (since cleverly rebranded as the "naturalness" problem) was something you got from postulating GUT or string unification at a GUT or Planck scale. Low energy SUSY was supposed to fix this particular problem, but my argument was that GUTs/strings had strong evidence against them (they were complicated and didn't really explain anything), and adding SUSY just made things worse.

Besides writing about this in the book, I've probably made similar arguments dozens if not hundreds of times on this blog since 2004, for many years in the context of arguing that the LHC was not going to see SUSY. After the negative LHC results, at first I thought that the new-found "crisis" in HEP theory would cause people to acknowledge the underlying problem, but it now looks like that's never going to happen (and those who long ago pointed out the underlying problem will always go unmentioned…)

7. **Lee Smolin**
March 7, 2022

Dear Peter,

I can only agree that this talk of a crisis in elementary particle physics, due to what the standard model doesn't explain, is very old news.

I first heard the concept of a crisis in the standard model in a talk Abraham Pais gave at a conference at Boston University (Or was it U Mass, Boston?) when I was an undergrad (~ 1974). This is not very long after the SM had been formulated. His theme was that the standard model, while it did very well as a description of the forces, did not explain a single parameter of particle physics or reduce the number of parameters by even one. He discussed the hierarchy problems and clearly called this a crisis.

I've been in crisis mode ever since, as have not a few others. Much of my work since has been intended as a response to the crisis. All of these responses have had in common the idea that laws are not eternal, but evolve dynamically on a landscape of theories. These have included Cosmological Natural Selection (1992), the subject of my first book (Life of the Cosmos, 1997). The issues related to reductionism, and how to modify it without loosing falsifiability is dealt with at length, there and in later books,. The need for background independent, and relational theories also must be understood an implication of the same crisis.

There turn out to be several hypotheses about how the crisis might be resolved if the laws evolve – include the Principle of Precedence, and the autodidactic universe. All of these responses to the crisis turn out to be related to the concept of laws and of time, indeed this is itself very old news as it was discussed clearly by CS Pearse in the late 1890's.

If we want to resolve this now 50 year old crisis I would suggest we focus on the ways in which our current schema for physical theories (which Roberto Mangabeira Unger and I call The Newtonian Paradigm) is unfit for extending physics to a cosmological scale, and how our notions of law and time must be reshaped to apply to a theory of the whole universe.

Thanks,

Lee

8. **zzz**
March 8, 2022

Hoskinson was not exactly a phd student.

https://en.wikipedia.org/wiki/Charles_Hoskinson

9. **Neo**
March 9, 2022

An update about Charles Hoskinson:

He was never in a math graduate program and he has not even completed a math undergraduate degree.

https://en.m.wikipedia.org/wiki/Charles_Hoskinson#cite_note-laurashin-5

https://twitter.com/laurashin/status/1500266527693709321?s=21

10. **Cedric BARDOT**
March 9, 2022

When Ed. Witten describes the statement by S. Pete Worden, chairman of the Breakthrough Prize Foundation, as vapid – because (?) he doesn't name Putin & the Russian army as aggressors of Ukraine (contrary to the statement signed by a lot of Breakthrough Prize Laureates) – it reminds me of this famous dialog with Titus and his father Vespasian (Roman emperor) as reported by the historian Suetonius:

> When Titus found fault with [Roman empire Vespasian] for contriving a tax upon public conveniences, he held a piece of money from the first payment to his son's nose, asking whether its odour was offensive to him. When Titus said "No," he replied, "Yet it comes from urine."

Source : Life of Vespasian 23.3 (tr. J.C. Rolfe)
Far from me the desire to play the devil's advocate but nowadays the further one is from a Russian laundromat(?) or a [dirty (?), source of] cash* the easier or safer one can nurture humanity's highest qualities like Honesty…!
*https://twitter.com/gfbertone/status/1500040565056061440

A question for Peter: why do you think the memory of Stephen Hawking is abused in Worden' statement?

11. **Peter Woit**

March 9, 2022

Cedric Bardot,
I thought invoking Hawking in attributing the Ukraine war to “our aggressive instincts”, while refusing to criticize Putin and the Russian state, was thoroughly repulsive. Saying what is happening in Ukraine is due to “our” aggressive instincts is like saying the same thing about Hitler’s invasions.

The funny thing is I had some sympathy for Milner, that he was being unfairly attached to Putin and his dictatorship. Seeing that statement made me quickly lose any such sympathy. I doubt it was issued without his approval.

12. **Risk Averse**
March 9, 2022

I am not an expert about oligarchs, but I would guess most of them did quite immoral things in order to get rich. Nevertheless, I find this rush to ask people to condemn Putin, quite ridicules. Clearly, Putin is a horrible man, becoming his enemy is very dangerous. I assume that any Russian expressing views against Putin is taking a risk for their property, freedom, and life. There are quite a few Russians in the west that were probably killed by Putin’s people. So who in their right mind will issue a statement against him? Moreover, condemning Putin might feel morally nice, but it will not make any difference to the reality.

13. **Peter Woit**
March 9, 2022

Risk Averse,

Milner’s claim is that he’s not an oligarch, has been out of Russia for ever, and that his venture capital business has no funding from Russia. If, unlike lots of other Russians now in the West or in Israel, he is fearful of publicly criticizing Putin, that’s fine. What’s appalling is issuing public statements like the one linked to here which attach blame for what has happened to anyone else other than Putin and his regime.

My interpretation of what happened is that Milner thought donating \$3 million to “victims of the conflict” (while not specifying whether they were Ukrainian or Russian) would solve his ethical problem with cash. Don’t know why with him it’s always exactly \$3 million…

14. **Risk Averse**
March 9, 2022

Peter, the last paragraph in your response is messed up.
I don’t know how Milnor made his money and I don’t really care. He is very rich so I assume he made it on the expense of lots of other people. However, he is still Russian and it is very likely Putin can put the squeeze on him, one way or another. In any case, who cares what he says, that does not change anything.
I think that people who grew in the west have no idea how risky things can become very quickly. I grew up in Israel, which in many ways is very democratic. However, if you are an Arab, unfortunately, you probably should be careful what you do and what you say. We should all be careful telling other people to do things that can put them in risk.

15. **Peter Woit**
March 9, 2022

Risk Averse,
If Milner wants to keep quiet because of the dangers, that’s fine. As far as I know, no one was demanding that the Breakthrough Prize Foundation put out a statement taking a stand on the war. He could have given money to refugees without making a public statement, or just announced he was donating money without saying anything else. I can’t see any reason it was necessary to make an offensive public statement invoking Hawking.

All,
I’m deleting comments on oligarchs, the war, etc. which have nothing to do with the Breakthrough Prize statement.

16. **Yovan**
March 10, 2022

The decision by Peter Scholze to decline the horizon prize looks prescient now. I feel bad for next year’s winners.

17. **dms**
March 10, 2022

If anyone wants to be entertained I’d recommend reading Lubos’ posts on Ukraine. Nutty as always, but it’s comforting to me that he’s at least as upset about the invasion as your standard center-left academic types. A silver lining of the invasion is that it demonstrates that political differences within the west are mostly a pose, and that there is a real consensus on what is fundamentally unacceptable.

18. **curiouser**
March 11, 2022

Glashow had already invoked the Pauli quote “not even wrong” in a Physics Today article on superstrings in 1986, so he may not have been referring to this blog or Woit at the Bohr Centennial. This is the relevant quote:

`Superstring theory, unless it allows an approximation scheme for yielding useful and testable physical information, might be the sort of thing that Wolfgang Pauli would have said is “not even wrong.” It would continue to attract newcomers to the field simply because it is the only obvious alternative to explaining why certain detectors light up like video games near the end of every funding cycle.’

https://arxiv.org/abs/physics/9403001

19. **Peter Woit**
March 11, 2022

curiouser,
Yes, that’s quite possible. Surely Glashow was aware of the “Not Even Wrong” quote long before I was around and he and many others from the early days of the “first superstring revolution” were of the opinion that it applied well to what was going on in string theory.

20. **4gravitons**
March 16, 2022

To clarify, Glashow specifically mentioned Woit in his comment (he did also mention it was a paraphrase of Pauli).

21. **Anon**
March 16, 2022

Hi Peter

There is no "standard" model for neutrino masses. The renormalizable Standard Model predicts the neutrino masses to be zero (with non-renorm terms of the order m_weak^2/M_Planck if there is no other mass scale). This prediction has been proven wrong. So one has to go beyond the SM for neutrino masses.

We could do this, without introducing a new Mass scale (the seesaw scale), by adding 3 right-handed neutrinos and providing small Dirac masses to them.

I think your view is that this model with Dirac masses to neutrinos, doesn't have a hierarchy problem — but this is *not* the Standard Model. In fact, the standard or more popular thinking as far as neutrino masses go is that they are Majorana (not Dirac), and their smallness can be understood to be because of the seesaw mechanism. This then brings a new mass scale into play and generates the Hierarchy problem. To avoid the Hierarchy problem I think you'd predict the neutrinos have Dirac masses.

I think though that this reasoning takes the Hierarchy Problem very seriously. As we need to extend the SM by adding 3 right handed neutrinos, and impose a B-L global symmetry so that neutrinos get dirac masses from really small Yukawa couplings. This is what you seem to be saying doesn't have the Hierarchy problem. But there is really no evidence for this model or for an exact B-L global symmetry in nature.

I personally think we have the seesaw mechanism at 10^14 GeV seesaw scale, and for reasons, we don't fully understand yet the Hierarchy problem isn't a show-stopper.

22. **Peter Woit**
March 16, 2022

Anon,
Since there's zero evidence for either Majorana masses or a seesaw mass scale, I don't see how this can be "standard" in any sort of way (about "popular" I'm not sure that's right and in any case its irrelevant).

When people say "the Standard Model" these days, obviously they're including some way of dealing with neutrino masses. Personally I don't see why dealing with them exactly the way one deals with all the other fermion masses (Dirac mass terms) shouldn't be thought of as the "standard" way.

That said, a right-handed neutrino is something very different than other particles (no em/weak/strong couplings) and this makes the neutrino sector and the issue of neutrino masses the place one is most likely to find something new and unexpected that finally improves on the standard model. Personally the seesaw mechanism seems to me to open more problems than it solves, but tastes differ. In the Euclidean twistor stuff I've been thinking about, right handed neutrinos are something very fundamental, and I'm hopeful that one might get somewhere very interesting thinking of them in these terms (I haven't yet).

23. **Anon**
March 16, 2022

SM has relevance as a low energy effective field theory. If neutrinos have Dirac masses then SM is not the low energy effective field theory, as the matter content gets modified at low energies by the addition of 3 right handed neutrinos.

When people say SM they mean it without right-handed neutrinos I think, in the sense of a low energy effective field theory.

SM + Dirac masses to neutrinos goes beyond the SM, even at low energies.

24. **The Big Red Scary**
March 17, 2022

Concerning the Russian invasion of Ukraine, Woit and Motl have finally found something to agree about.

25. **Peter Woit**
March 17, 2022

Anon,
I just don't agree with your conception of the meaning of the words "standard model" as something
1. Completely wrong, since doesn't have neutrino masses
2. Only a low energy effective approximation.
but arguing over the meaning of words leads nowhere.

26. **Peter Woit**
March 17, 2022

The Big Red Scary,

I think, as a matter of principle, if Lubos and I agree on something it must be true.

27. **martibal**
March 17, 2022

Peter and Anon, maybe this is off-topic, but besides words, is the issue Anon is mentioning something physically problematic ? I mean: does incorporating in the Standard Model right handed neutrino with low Dirac masses create some incoherence regarding renormalization, or other incoherence ?

28. **Anon**
March 17, 2022

From the literature in the field, SM refers to a very specific, well-accepted, model that predicts that neutrinos have no mass. The discovery of neutrino mass is considered the first new physics found that goes beyond the standard model.

But I think it gets step-parently treatment — it wasn't the new physics people hoped would be discovered (like SUSY etc) — and since larger issues in the field are framed without any reference to neutrino masses, those discussions continue with neutrino sector ignored.

Which "Beyond standard model theory" do neutrino masses indicate? I think this is an important question that experiments would have to decide. There is no experimental evidence one way or the other yet of whether neutrinos have Majorana or Dirac-type masses. This has to be experimentally determined — we can't just say the SM prediction is that neutrinos have Dirac masses (which is what Peter is implying). The SM prediction was that neutrinos have no masses, which has been ruled out.

The implication of Dirac mass for the neutrinos is that you have to modify physics at low energies — new light degrees of freedom — right handed neutrinos that exist in our world even today!

If they have Majorana masses then right-handed neutrinos do not exist at low energies (their mass could then be at the much higher seesaw scale ~ 10^14 GeV), and below that scale it is just the usual SM without right-handed neutrinos.

I think Peter takes the Hierarchy problem very seriously (just as SUSY folk did :-)) and ends up arguing for SM + 3 right handed Dirac neutrinos as being the theory of nature, that doesn't require further proof as he equates this to SM.

But in my opinion, this requires the same level of proof as SUSY did, especially if 3 Dirac neutrinos is what is the solution or implication of the Hierarchy problem.

martibal — you can add 3 right handed neutrinos to the SM and give them Dirac masses by imposing a B-L global symmetry. This introduces a bunch of small dimensionless numbers (very tiny Yukawa couplings with the Higgs of the neutrinos)… much much smaller than for any of the other particles — like even electrons. But one can live with that. Also, once you add 3 right handed neutrinos to the SM, the most general Lagrangian would include Majorana masses to the neutrinos anyway — you would have to impose B-L symmetry as a global symmetry of nature to exclude the terms giving rise to Majorana masses.

29. **Peter Woit**
March 17, 2022

Anon,

I agree with your answer to Martibal.

My view on the hierarchy problem is that the SM has no hierarchy problem, including if you interpret "SM" as treating neutrino masses like other masses, caused by a Dirac mass term and right-handed neutrino fields. The hierarchy problem is a problem for people trying to build theories that include the the usual Higgs and some new physics at a much higher mass scale, whether it be seesaw masses, GUTs, high-scale SUSY, your favorite Planck scale quantum gravity physics or whatever. To my mind, all such attempts have more serious other problems than the hierarchy problem (they explain very little by adding a lot of complexity, with zero experimental support).

Again, what I think is really interesting here, indicating a way truly beyond the SM, is the very unusual nature of right-handed neutrino fields.

30. **Anon**
March 17, 2022

yeah I agree Peter that the right handed neutrinos are interesting.

In fact, the minimal left-right symmetric (LR) model anticipated/predicted that neutrinos would have masses. Basically, because of parity, both left and right-handed neutrinos must be included, and right-handed neutrinos pick up a large Majorana mass at the parity (or B-L) breaking scale, and set up the seesaw mechanism. In fact, you cant have Dirac type neutrinos in the minimal left-right symmetric model (they'd be too heavy and experimentally ruled out…. the Yukawa couplings of neutrinos to the usual Higgs cannot be made arbitrarily small in the LR model, as right-handed neutrinos are integral to the model and don't just decouple if their Yukawas vanish, like they decouple in SM with 3 right handed neutrinos).

LR model also makes other predictions.

31. **Mariner**
March 20, 2022

What do you think of this?

https://profmattstrassler.com/2022/03/20/a-prediction-from-string-theory/

32. **Peter Woit**
March 22, 2022

Mariner,
I have to assume the headline about "a prediction from string theory" is just trolling.

This kind of thing is a blast from the past, something some people engaged in during the string wars 15 years ago but have long given up on. Of course, there is nothing like an actual prediction from string theory here. What Strassler is writing about is some exotic models he worked on 15 years ago, for which there never has been the slightest evidence or even any sort of significant motivation. The only connection to string theory is that conjecturally you could use AdS/CFT to calculation some implications of these models.

2022 Abel Prize to Dennis Sullivan

March 23, 2022
Categories: Uncategorized

This year's Abel Prize has gone to topologist Dennis Sullivan, for the announcement see here, with more information about Sullivan and his work here. There are press stories at Nature, the New York Times, Quanta, and elsewhere.

Sullivan was one of the leading figures in great advances in understanding the topology of manifolds in higher dimensions during the late 60s and 70s. Some of the best of his early work for many years was only available if you could find a copy of unpublished mimeographed notes from a 1970 MIT course. In 2005 a Tex'ed version of the notes was finally published (available here). This includes as a postscript Sullivan's own description of this work, how it came about, and how it influenced his later work.

This was followed by wonderful work on rational homotopy theory, making use of differential forms. For this, see Sullivan's 1977 Infinitesimal computations in topology, and lecture notes on this by Phil Griffiths and John Morgan. In later years Sullivan's attention turned to subjects with which I'm not very familiar: topics in dynamical systems and the development of what he called "string topology".

Since 1981 Sullivan has held the Einstein chair at the CUNY Graduate Center here in NYC, running a seminar each week that concentrates on the relation between topology and QFT. For many years these were held in a Russian style, going on for multiple hours, possibly with a break, until all participants were exhausted. There's a remarkable collection of videos of these lectures at the seminar site, including many going way back into the 80s and 90s, with video recorded at a time when this was quite unusual (more recent ones are on Youtube).

When I first came to Columbia Sullivan was often here attending and giving lectures, for many years splitting his time between Paris (where he held a position at the IHES), New York and Rio. The Abel Prize biography explains

> In 1981, Sullivan was made the Albert Einstein Chair in Science (Mathematics) at the Graduate School and University Center of The City University of New York. He kept his position at IHES and spent the next decade and a half shuttling between Paris and New York, often on Concorde.

Some of the various stories I heard about Sullivan's lifestyle at the time involved his having multiple apartments in New York, which he used to host a variety of visiting mathematicians. Another story I heard directly from him was about how he survived an attempted car-jacking in Brazil, during which he was shot, but managed to escape and drive himself to a hospital for treatment. I had first heard about this from Mike Hopkins several years before. When I asked Hopkins why he had become a topologist, he said that one factor was the inspiring example of people like Sullivan who worked in the field, jokingly characterizing it as involving "real men who got into gun-fights".

In 1997 Sullivan traded the IHES position for one at Stony Brook, and over the years has unfortunately been seen less often here at Columbia. Congratulations to him on the well-deserved prize!

No Comments

Is Space-Time Really Doomed?

March 31, 2022
Categories: Euclidean Twistor Unification

For many years now the consensus in a dominant part of the theoretical physics community has been that the center of attention should be on the problem of quantizing gravity, and that conventional notions of quantum theory and space-time geometry need to be abandoned in favor of something radically different. The slogan version of this is "Space-Time is Doomed."

Ever since my student days long ago, I've spent a lot of time looking into the problems of quantum gravity and what people have tried to do to address these problems. The highly publicized attempts to get known physics out of radically different degrees of freedom that I've seen haven't seemed to be making any progress, remaining very far from anything like known physics. In the case of string theory, which also claimed to be able to get particle physics, there was at one point a (highly over-hyped) relatively well-defined proposal that one could discuss, but that's no longer the case.

Recently things have changed as I've become convinced of the promise of certain specific ideas about four-dimensional geometry involving twistors and Euclidean space-time signature. I've written about these here and on the blog, and have given some talks (see here and here). These ideas remain speculative and incomplete, but I think they provide some new ways of thinking about the problems of quantizing gravity and unifying it with the other forces.

The existence of a yearly essay competition gave me an excuse to write something about this which I just finished yesterday and sent in, with the title Is Space-Time Really Doomed?. After spending some time on a diversion into arithmetic geometry, I've been getting back to seriously thinking about this topic, looking forward to having time in coming months to concentrate on this. I hope the essay will encourage others to not give up on 4d geometry as doomed and unquantizable, but to realize that much is there still waiting to be explored.

Update: The essay is now on the arXiv here.

Update: Awards for this announced here. I got an honorable mention.

Comments

1. **Mark**
   March 31, 2022

   Apart from the metric and the Cartan tetrad formalisms of geometry mentioned in your article, there is probably a third formalism worth exploring, the geometric algebra formalism of geometry, which uses real Clifford algebras.

   The geometric nature of quantum field theory and the Standard Model, the gauge theoretic nature of gravity, and the relation between the mathematical structures of four-dimensional geometry, general relativity, and quantum field theory were already made explicit in the geometric algebra formalism in Chris Doran and Anthony Lasenby's 2003 textbook titled Geometric Algebra for Physicists:
   https://www.cambridge.org/core/books/geometric-algebra-for-physicists/FB8D3ACB76AB3AB10BA7F27505925091
   Furthermore, the book comes with a section of a chapter showing that the geometric algebra formalism could also be used for twister geometry and a twister-based formalism as well.

2. **Andrei**
   April 1, 2022

   The "space-time is doomed" claim, put forward by Nima Arkani-Hamed is based on two arguments:

   1. In order to perform an infinitely accurate measurement you need an infinitely large instrument, otherwise the quantum uncertainty imposes an accuracy limit. When you want to fit an infinitely-sized instrument into a laboratory room you collapse to a black hole and no measurement is done.

   2.In order to measure something very small (below Planck length) you need a very energetic particle which, again, would result in a black hole.

   I think both arguments are fallacious.

   1. Even if the physics is classical you need an infinitely sized memory to record a result with infinite accuracy. Forcing that memory inside a room would lead to a black hole. So quantum uncertainty is a red-herring here. And why would you expect to be able to perform an infinitely accurate measurement with finite resources? Why is this premise supposed to be true in the first place?

   2.One can measure in principle distances smaller than Planck length with interferometry. No need for ultra-energetic particles. LIGO measures distances with an accuracy of about 10^-20m with a wavelength of 10^-5m.

   So, Nima's arguments fail.

3. **David Roberts**
   April 1, 2022

   @Andrei,

   John Baez wrote a nice article explaining all kinds of issues that arise when thinking of the universe as being modelled by infinite precision reals, even using boring old Newtonian mechanics

   "Struggles with the Continuum", https://arxiv.org/abs/1609.01421

4. **Pascal**
   April 1, 2022

   Can your approach accommodate quantum superpositions of different spacetimes? This looks (to my nonexpert eyes) like something that should be included in any successful theory of quantum gravity. Or else, you would have to abandon some basic tenets of relativity (spacetime) or of quantum theory (quantum superpositions).

5. **Peter Woit**

April 1, 2022

Mark,
There's an argument for replacing the use of differential forms and the exterior algebra with the Clifford algebra, for one thing it makes introducing spinors more well-motivated. But that works for any dimension, in this essay I wanted to aim at certain structures specific to 4d.

6. **Peter Woit**
April 1, 2022

Andrei,

In any case the argument that you need to consider black hole states doesn't imply doom for space-time. After all, they're space-time geometric objects themselves.

Pascal,
Sure, in a quantum theory of space time you should have superpositions of geometries just like in QED you have superpositions of EM fields.
One thing I'm pointing out is that the fact that we haven't found a convincing quantum dynamics for the degrees of freedom given by geometry of space-time does not mean that those are the completely wrong fundamental degrees of freedom, just emergent epiphenomena. This problem is fine as a motivation for looking for different fundamental degrees of freedom, but that's not led to anything successful either.

7. **Alex**
April 1, 2022

I agree with the sentiment. I also have a bold proposal, not about a physical theory, but a sociological one: it's time to stop listening to the people in the community that has been so wrong over decades by now, hyping and overselling *their doomed ideas*. For the good health of the field, this has to stop. The names are so obvious that there's not even a need to spell them out explicitly, we all know who they are, what have been they saying for decades, with what language, tone, and bullying methods. But the problem was not even one of academic ethics, it's simply that the ideas have failed miserably to deliver their promised 'El Dorado' of physics, and misdirected a whole field and young researchers to nowhere.

8. **Darran**
April 2, 2022

I don't think the comments above do justice to Arkani-Hamed's arguments regarding spacetime. There's a big difference between classical and quantum mechanics regarding measurements.

In classical mechanics measurements are treated no differently to any other process and thus the ability to perform an accurate measurement is of no consequence to defining the theory. The physical degrees of freedom are defined independently of how they are measured.

However in quantum theory the whole formalism is an engine to compute the probabilities associated to sharp measurements. Arkani-Hamed's point is that in a setting where we treat gravity and cosmology realistically it's not possible to fulfill the conditions quantum theory assumes measurements obey within a finite region and thus it seems difficult to have well-defined local observables.

Of course in quantum information we can model unsharp measurements with POVMs, but the space of operators still includes sharp PVMs. If PVMs aren't possible at all locally the theory would seem to need to take a very different form.

Of course the above does not "prove" this line of argumentation is correct, Arkani-Hamed has been clear that this is just his intuition about where to go. However you can't argue it's wrong by ignoring the completely different nature of measurements in quantum and classical theory.

9. **Peter Woit**
April 2, 2022

Darran,
I guess I just don't see the relevance of any such arguments to the claim being made (that space-time degrees of freedom cannot be what a fundamental theory is based on, they should be emergent degrees of freedom of an effective theory).

10. **Darran**
April 2, 2022

Peter,

Essentially because spacetime degrees of freedom would be local ones if they retain their character from GR. If there doesn't seem to be local observables due how measurements work in a realistic cosmological setting it is hard to see how the proper quantum theory would involve spacetime in its conventional form.

His isn't the only view where considerations of treating measurement realistically result in a weakening or replacement of the notion of spacetime. Rovelli's relational quantum mechanics is an example.

11. **Alex**
April 2, 2022

Peter,

when you say spacetime may not be doomed in QG, which one of the following options are you thinking of?:

1) Both classical manifold and metric, the pair (M,g), are somehow still there at the fundamental physical reality, even after quantization.

2) Only the manifold M survives at the Planck scale.

3) Both of these concepts in the pair (M,g), and their mathematical machinery, are still needed to formulate the quantum theory via quantization of (general relativistic) classical field theories. Now, at the physical fundamental level, that doesn't mean that there's a classical pair (M,g) waiting for us at the Planck scale.

4) Physical quantum spacetime at the Planck scale may be some sort of noncommutative geometry, where basic geometric notions such as metric and fiber bundles can be suitably generalized into their noncommutative counterparts, and hence our basic geometric ideas can still be implemented there (although, in their noncommutative incarnations).

5) Other?

Thanks.

12. **Peter Woit**
April 2, 2022

Darran,
To sensibly discuss this, I think one needs to get much more specific. I don't know what it means for a quantized degree of freedom to "retain its character" with respect to the classical theory, or exactly what locality means in this context.

Alex,

The essay was trying to point to a relatively specific proposal: standard 4d geometry is formulated in terms of standard geometrical degrees of freedom (SD/ASD spin connection/curvatures, vierbeins, torsion, half-spinors, twistors). Can one build a fundamental theory by quantizing these degrees of freedom? In a path integral formalism you want to find an action/path integral measure written in terms of these variables that makes sense. I know the various problems people have run into trying to do this in the past, want to understand if there is some way to overcome these problems in the somewhat different specific setup I'm proposing that includes the SM degrees of freedom

13. **Darran**
April 2, 2022

Peter,

Basically if we imagine spacetime surviving in the resulting quantum theory, then you would imagine some structures from classical GR or more fundamentally just basic geometric notions, e.g. connections, will appear in quantised form.

Regardless of what geometric entities are quantised, quantising classical variables gives you self-adjoint operators. Self-adjoint operators are just weighted sums of projectors, so essentially quantising gives PVMs with weights.

However it seems that no realistic observation in a finite region actually corresponds to a PVM/Self-Adjoint operator. Only POVMs are possible, to use quantum information language. Thus quantisations of classical quantities don't seem to lie within a cosmologically realistic quantum theory. Hence a quantisation of classical geometry can't be fundamental.

Asher Peres has a nice paper "What's wrong with these observables?" where he discusses how most POVMs don't correspond to quantised versions of classical observables. Arkani-Hamed doesn't use Quantum Info terms like POVM, but it's just a formal way of expressing a lack of sharp observables.

Quantised classical variables correspond to idealised measurement processes. In non-relativistic QM and Minkowski QFT we can get away with this idealisation by recourse to limiting infinitely massive devices, but Arkani-Hamed's point is that in a treatment that properly respects cosmology we cannot. Thus "quantised geometry" would not be the way to go.

14. **Alex**
April 3, 2022

Peter,

okay, then I think that would fall into point 3) of my list. Personally, that's my opinion too. But, what insurmountable problem do you see with other approaches that take a similar view on this (e.g., LQG; of course, it's a canonical quantization approach rather than path integral, but the view on the role of classical geometry is similar; and, although still with some problems, some versions of if, like Thiemann's Master Constraint Programme, seem quite advanced by now; not that I'm trying to sell this approach, I have my doubts too, but I'm just mentioning it)? Also, why path integrals, knowing their infamous ill-defined measures over infinite dimensional field configuration spaces?

Anyway, I have a simple reason to stick to 4d standard geometry rather than über speculative theories: it's physics that actually *works* and describes our reality! And science tends to advance by quasi-static changes on the acquired knowledge, despite what those simplified versions of Kuhn's view, that one often hears, say. I think point 4) in page 7 in this article by Rovelli (https://arxiv.org/abs/1805.10602) puts it very well, whatever views one may have on his own approach to (L)QG.

15. **Peter Woit**
April 3, 2022

Alex,
There's a list of serious technical problems that you run into if you try to pursue either path integral or canonical quantization of these degrees of freedom. I think it's quite possible these can be overcome with some new ideas, in particular working in Euclidean space twistor space (with a distinguished time direction). In any case, this seems to be a lot more promising than the idea that you're going to solve the problem with quantum information theory.

16. **Darran**
April 3, 2022

Peter,

Who is trying to solve these problems with Quantum Information Theory? Arkani-Hamed isn't for example, in case I gave that impression.

17. **Peter Woit**
April 3, 2022

Darran,
That was a reference not to Arkani-Hamed, but to the well-publicized and well-funded "It from Qubit" stuff.

These days Arkani-Hamed typically uses the "spacetime is doomed"argument as motivation for his work on reformulating amplitudes calculations, but I don't think the relation is much more than vaguely motivational.

18. **Jack**
April 4, 2022

Dear Peter,

I read your essay with great interest, thanks for sharing.

Since you are interested in the unique nature of 4D, I wonder if it is also worth mentioning that in four-dimensions we have this commuting nature between the Hodge Star and Riemann Curvature operations, as described in Professor Krasnov's book that you reference. This also happens to be when the metric can be described as "Einstein".

Related to this (I think, although I am not sure) is one of my favorite results in the form of Lovelock's theorem and just how general one can make the left-hand side of the Einstein equations. Although it can be generalized to any number of dimensions, in four it shows the unique role played by the cosmological constant. In the light of the cosmological constant problem, I personally think that this is something not discussed enough.

Thanks and best,

Jack.

19. **Peter Woit**
April 4, 2022

Jack,
The commutativity of * and curvature in 4d is exactly what allows one to express GR in terms of just self-dual connections. It seems to me quite remarkable and important (and this was one of the main points I wanted to make in the essay) that 4d GR depends on only half the variables one expects, i.e.half the variables that almost all descriptions of the theory use. For this to work nicely though, you need to be in Euclidean signature.

20. **Prof. Legolasov**
April 6, 2022

Hi Peter,

Can you please elaborate on this (taken from your essay)

> The only known non-perturbative definition of Yang-Mills theory is in Euclidean space-time

What is the definition of Yang-Mills theory in Euclidean space-time that you're referring to?

21. **Peter Woit**
April 6, 2022

Prof. Legolasov,

The lattice gauge theory definition. This very much requires Euclidean signature to make sense.

22. **Pham Trinly**
April 16, 2022

I have a very basic layman question on the subject. Why is Cartan-Einstein theory so very much out of favor? More broadly, could it be that the difficulty of quantizing gravity lays in the fact that the classical theory is incomplete? (And that the missing terms don't make much of a contribution on the experimentally accessible energy scales?)

The Anti-Science Movement

April 6, 2022
Categories: Fake Physics, Uncategorized

I noticed recently that Stony Brook is hosting next week a panel discussion devoted to

> a conversation about one of the most grave challenges to confront humanity: the anti-science movement.

There is a truly grave challenge being referred to, but a serious mistake is being made about the nature of the challenge. In particular, there's no evidence of an "anti-science" movement, quite the opposite. Across the globe, if you ask people what profession they respect the most, "scientist" comes out on top (see here). Likely the organizers have in mind climate denialists and anti-vaxxers as prime examples of "anti-science" behavior, but in my experience such people typically show a great devotion to pointing to scientists, scientific results and scientific papers to justify themselves. An example would be Lubos Motl, who has put out literally thousands of pages on his blog about climate and COVID science (by the way, his blog seems to have gone "by invitation only", anyone know what that's about?).

The problem isn't "anti-science", but bad science, promoted for ideological reasons. This is part of a larger truly grave challenge to humanity, that of our information environment being flooded with untruth, on a scale that dwarfs the output of the Ministry of Truth that Orwell foresaw. For years now we've been living with this in the form of phenomena like Trumpism, and the past few weeks have seen the Russian government exploiting these methods to conduct a campaign of brutal slaughter. I don't know what the best way to address this challenge is, but unless something can be done, humanity has an ugly and disturbing future ahead of it.

Sticking to the problem of what to do about the promotion of bad science, there at least I have some experience trying to do something about one example of it (although with very limited success). This problem deserves attention and a panel discussion, but a panel in which four of six members have devoted a significant part of their careers to promoting a failed scientific research program is a really odd choice.

The underlying thorny issue is that of how to evaluate scientific claims. Given the complexities of controversial science, non-experts generally have little choice but to try and identify experts and trust what they say. A major societal role of elite institutions is to provide such experts, ensuring that they provide trustworthy expertise, untainted by ideology or self-interest. A large part of what is going on these days seems to me to reflect a loss of faith in elite institutions, with an increasing perception that these are dominated by a well-off class pursuing not truth, but their own interests. As a product of such institutions I'm well aware of both their strengths and their weaknesses. We need them to do better, and in this case Stony Brook should come up with a better panel.

Update: I've heard that Lubos himself shutdown the blog, unwilling to agree to follow rules Google was now enforcing.

Comments

1. **Martin S.**
   April 6, 2022

   One way that is bad to support (good) science, is to be superficial. And making it a political issue is another bad thing.
   Thus regarding politicizing, when you bring the Kubrick's clockwork guy, it begs for bringing the political negative of him that tries to destroy education (of math). When you damage education, in a decade no one will be able to recognize (good) science at all.
   And regarding superficial views, when you write about Kremlin vs. past weeks, it omits a crucial point: that Kremlin was liquidating free society in Russia for years, and that only such a destruction allowed them to turn life into nightmare even outside Russia.

2. **Paolo Bertozzini**
   April 6, 2022

   Dear Peter,

   I am not so sure … whenever I see around people with "clear and evident truths" in Their hands (or Their minds) and uniformly shared opinions (very common in today society dominated by influential marketing) … my fist instinct is to stop, doubt and rethink … and this applies also to "open support for *Science*".

   Skepticism (especially in the forms linked to post-modernist epistemology and social constructivism) is often considered the main culprit of the "crisis of faith in *Science*" seen in certain "anti-science movements" very visible in populist circles.

   I am afraid that the "problem with *Science*" today is much deeper and very very unpleasant to explore: *Science*, as it has been on-purpose politically re-branded after the WWII with Popperism at the LSE, is hiding (since its inception) a more complex and unpalatable story. When I was a kid I used to be enthusiastic about everything scientific … now (after almost 30 years paying attention to "under-developed" countries) … I try to stay as distant as possible from *Science* and I tend to believe that *Science*, rather than a solution, has been and is actually playing a big role in support of the main causes of many of the problems that I would like to see eliminated from human society (inequality, racism, exploitation, just to mention those that are closer to my concerns).

   As every technological instrument and knowledge, science can be of great help to identify and solve problems (although its purpose might be unrelated to utilitarian aims. *Science* (note the *quotes*) on the other side, is not just identifiable with a methodology of "knowledge acquisition", but it comes heavily charged with a dangerous ideology (something that was and is often denied claiming its objectivity).

   I repeat that the above "criticism" has nothing to do with "anti-vax" or "anti-green" conspiracy positions now raging mostly in certain "over-developed" countries.

   It is easy (and often necessary) to concentrate on localized issues (for example the trouble with theoretical physics or medical/biological research) … from time to time it is probably a good idea to have a wider panorama. I apologize in advance if this post is "out of topic" (or unwelcome) 😉

   Best Regards.
   Paolo

3. **anonymous**
   April 7, 2022

   The anti-vaccine/anti-lockdown movement is a bad example of anti-science, because at least in the United States is less about science/anti-science and more about American politics and which side of the culture war the issues landed on. For the first few months of 2020 when Trump was still in office, it was largely the Democratic aligned factions of the United States who were denying the severity of COVID, and it was the Republicans who were trying to prevent travel to and from the United States in order to keep COVID out. Then COVID hit New York City and all of a sudden both sides flipped their positions on COVID overnight. The same thing happened with the vaccines, while Trump was trying to speed up vaccine production under Operation Warp Speed, it was largely the Democrats who questioned the effectiveness of the vaccines and vaccine mandates. Once Trump lost the 2020 elections and the Democrats gained control of the White House and the Senate, the Democrats flipped and started supporting the vaccines, while the Republicans flipped to a more anti-vaccine position. The only constant throughout the COVID pandemic has been that the Democrats and Republicans had to politicize the issues and take opposite sides on the issues.

A much larger issue with "Science" is the corruption of the scientific process and scientific institutions by corporations and governments and other self-interested parties. This is seen in climate science when energy companies such as coal and oil try to censor existing climate science of global warming in favor of more profits, in medicine when pharma companies aren't being transparent enough or honest enough in their medical experiments of the effectiveness of their drugs, et cetera. It is this corruption that leads many people to lose faith in American scientific institutions.

4. **anon**
   April 7, 2022

   I think some default setting on blogspot.com has changed in the last few days. I have noticed that some other blogs hosted there have also gone invitation only.

5. **Michael Gogins**
   April 7, 2022

   Thanks for this.

   In my view automated and unregulated lying is humanity's worst problem, as it makes dealing with other serious problems practically impossible.

6. **Thomas Larsson**
   April 7, 2022

   Re Lubos: I occasionally drop by his blog (or dropped, before he closed it), and I noticed that Lubos developed a quite fierce anti-Russian stance after the invasion of Ukraine. Quite the opposite of the American alt-right, but not unparalleled in Europe, e.g. by the Polish government. My guess is that that got him into trouble with his previous friends.

7. **Shecky**
   April 7, 2022

   Another problem (a big one!), I think you're bypassing here is science journalism! Journalism (especially in a day of 'breaking news' and clickbait) is inherently incapable of nuance and complexity, so "science" becomes waaay oversimplified as spoonfed to the public. If science tells you one week that coffee is good for you and the next week that coffee is bad for you, etc.etc., well, of course much of the public loses any confidence in it, and 'anti-science' arguments are easily constructed/promoted.

8. **Umesh**
   April 7, 2022

   The blog has 'gone private'. It's a universal feature of all social media providers these days to provide such an option to the account owner. If he/ she so chooses, they can 'priv' their account and have the option of approving (accepting/ declining) 'follow requests'. It reduces reach, but that's the price to pay if you wish a select audience.

9. **Peter Shor**
   April 7, 2022

   I would have to disagree that there's no anti-science movement. Having talked to a teenager who was educated in a Christian school, she was definitely taught according to an anti-science viewpoint—that you can't trust anything scientists say, because they have their own self-serving motives for saying these things. (This may actually be true to a much lesser degree—look at the string theory apologists. But the best lies contain an element of truth.) And I have to assume that this viewpoint is reasonably common among evangelical Christians, who make up around a third of the U.S. population.

10. **Tom WEIDIG**
    April 7, 2022

    Why are not also mentioning woke stuff such as gender issues, anti-racism, and so on. These political activists are constantly abusing "science", especially social science to support their political cause.

    And about climate science, I simply disagree calling climate forecasting models science. They are forecasting models based on climate science, physics, chemistry and many other inputs. They are not science. So if I deny a climate crisis, I could just do in on the ground that I do not trust those models but that doesn't mean i deny climate science.

11. **Doug McDonald**
    April 7, 2022

    umesh:
    But Lubo's blog offers no way to contact him. Its probably some Google "cancel" spasm. My guess is that we will eventually find out what happened and it will be "the [unfortunately] usual".

    One problem with "Science" is that what gets covered by the media gets softer and softer.
    And some parts that are still very "hard", like particle physics, get filled with things that are 'not even wrong'. Among those there is all the screaming about the "measurement problem" which is just a battle of words, and some protagonists insisting on "instantaneous" "wavefunction collapse", which is indeed wrong. Its not instantaneous on the appropriate energy scale time intervals.

12. **Peter Woit**
    April 7, 2022

    Peter Shor,
    Yes, there is and always has been some religious anti-science movement, but I don't think that's what's responsible for our current problems (white evangelicals make up about 14% of the US population). What's new about our current situation, in an oversimplified formulation, can be simply summarized as: Fox News.

    As far as I can tell, Fox News is not particularly devoted to promoting religion or attacking science. What they are though is a propaganda machine, weaponized to distribute information not based on whether it's true, but whether it advances the political aims of the Republican Party in the US.

    The challenge humanity is facing is not an anti-science information environment, but one toxically polluted by ideology (and yes, I'm well aware that it's not only the right that is the problem, with identity politics warriors of all stripes showing little interest in what's a fact and what isn't). My complaint about that Stony Brook panel is not just that they may be addressing the wrong problem, but that scientists with histories of promoting bad science for ideological reasons are not the people one should look to for answers to the problem of ideological information pollution.

13. **Peter Woit**
April 7, 2022

Tom Weidig,
I don't want to host a discussion of climate models, or vaccine science or epidemiology and public health or anything of the sort, since I have no expertise at all on those topics. The challenge those topics pose is that of how society can put a stop to the ideologically polluted information environment surrounding them that we now live in and get most people trustworthy information.

14. **Peter Woit**
April 7, 2022

Doug McDonald,
It looks like you are right about Lubos. I've heard from someone who contacted him that he has shut his blog down rather than follow rules Google is now enforcing. This is somewhat like what happened to him at Harvard, where I gather he did not renew his visa and left his position rather than agree to follow rules he didn't like.

15. **Anonyrat**
April 7, 2022

The continued integrity of science places certain demands on practicing scientists. There appears to be a problem in this regard, and not just in the fields that Peter Woit writes about.

There is a long history of anti-science instigated by powerful economic, political, religious interests. Social media has newly provided these a powerful tool of amplification. If science remains healthy, these will likely eventually be beaten back. But given the times we live in, scientists have to be even more diligent to keep science healthy.

16. **Geoffrey Dixon**
April 7, 2022

Some comments about pop sci media are too harsh IMO. I really DO want to know what happens if you stick your head in the path of an LHC beam (a recent pop sci headline). Were I not a sort of scientist, this would immediately make me want to become one.

17. **S**
April 7, 2022

Peter Shor,

I think the situation you refer to among evangelical Christians is not so different from the vaccine or climate situations Peter mentioned. (This is a milieu I'm very familiar with.) They (or some of them — a minority probably) do ascribe questionable motives to mainstream scientists, but their response is not, "So science is worthless." It's to appeal to other "scientists," and try to argue that those ones are actually following The Scientific Method. Meanwhile, areas of science without clear conflict with their beliefs are taught in a more or less totally normal way (sometimes well, sometimes poorly, as with other schools).

tl;dr: I don't think "anti-science" applies even in the situation you describe.

18. **Kirk**
April 7, 2022

I once over-heard a woman in a library discourage her son from reading a math book because it didn't have Jesus in it.
If you don't even trust basic math, you're not going to trust statistics in scientific models. Yes, we can always expect people to be enthused when a paper or study agrees with them, but cherry picking studies (especially bad studies that are later shown to be error ridden) is not being pro-science, it's being pro 'whatever will make people agree with me and makes me look good'. There are people in this world who generally loathe skepticism and say outright they don't understand why people can't just accept faith, but will happily ape the outward appearance of science and its skepticism in order to make their position look better. Their rule is 'skepticism against everything that disagrees with me'. This is how someone can, for instance, throw 99% of all work on climate science and then in the next breath say 'I trust climate science.'
Science is a methodology. That doesn't stop people from abusing the word when they aren't doing or supporting anything actually scientific. This is no different from fascist banana republics that claim to be democracies. Just because they claim it doesn't mean it's so.

19. **Ash Jogalekar**
April 8, 2022

I am not sure I understand your distinction between "anti-science" and "pro bad science". An anti-vaxxer for instance cites the "science" he or she thinks they know to support their claims, and yet they aren't just promoting bad science but in this case they are demonstrating an opposition to what we consider the central evidence-based tenets of the scientific method. In that sense I would call them "anti-science". What am I missing?

20. **Peter Woit**
April 8, 2022

Ash Joglekar,

The problem is that they would claim they are following the evidence-based tenets of the scientific method, perhaps even quoting a published study and its data. In any case, if you think the problem of "anti-science" is people claiming to do science while not following the scientific method of rigorously testing ideas against experiment, is a panel with four string theorists the right group to be making this argument?

What I'm arguing is that calling the problem "anti-science" is missing the point. Almost everyone involved thinks they are "pro-science". Some people however are doing science well, rigorously testing their ideas against the real world and being careful to avoid being misled by ideology or self-interest, others aren't. Again, this is the wrong panel to explain this difference to the public.

21. **Alex**
April 8, 2022

I mostly agree with the blogpost, but I don't think the future is bleak. Nor brilliant. Human nature gonna human nature, it was like that for centuries, it will be like that for the centuries to come. Of course, social networks and globalization are new phenomena, but, for the same reason they allow the propagation of misinformation, they also allow the free propagation of truth. Ultimately, it's on the user to decide to accept whatever thing is posted on the net, or search more on the same net to find the actual truths. It all depends on the open mind and critical thinking skills from the part of the user. And that's where education institutions enter, since they should equip users with those abilities, so that they do not let themselves to be brainwashed either by organizations (religions in particular, but also political parties and ideologues) or social network algorithms. Of course, there's a great deal of complicated human psychology and neuroscience there, but it's time to start to incorporate that on the standard education, so that kids can know their brain's tendencies to be biased. Furthermore, public policies should be directed at that, rather than trying to censor content or regulate the social networks themselves. Unfortunately, I think that both institutions and public servants are not doing a great job, they only seem concerned in looking for scapegoats.

22. **Peter Woit**
April 8, 2022

Alex,
I'd like to believe that the new information environment provided by the internet will allow the propagation of truth, that all you have to do is get people to have open minds and think critically. Unfortunately I've seen no evidence of that happening, quite the opposite.

Maybe a good way to think about the problem is that we've gone from an environment where information was scarce and regulated in many ways, to one in which it's abundant but unregulated. I'm not a libertarian who believes the answer to all problems is to get rid of regulations. The problem with an unregulated information environment is it allows self-interested actors to pollute it. What happens when your information environment is full of raw sewage?

The question of how to clean up our polluted information environment may be a much more difficult one that that of how to clean up and control pollution of our physical environment.

23. **Alex**
April 8, 2022

I'm not a libertarian either, but I would rather have free speech than excessive regulation, even if that allows interested and biased discourses. After all, that's the spirit of the constitutions of all free western countries, and we learned to value that in the hard way. But, on the other hand, in the real and messy world, the solution probably lies in a balance between free speech and regulation. Who decides, and where, to draw the line? I don't have an answer for that. Ultimately, that will come out from the democratic process, with imperfect results, as imperfect as the system is.

But my main point was about the education of people regarding the psychology and neuroscience of bias and brainwashing. Not a perfect or definitive solution, but I do think it can help with this problem.

24. **Peter Woit**
April 8, 2022

Alex,
I agree that getting people to recognize bias in their information source is part of the answer. Again, the Stony Brook panel is very much the wrong one to address this.

25. **zzz**
April 8, 2022

i know its a physics center hosting so they got physicists, but its very emblematic of the bubble thinking that is part of this problem

26. **Jackiw–Teitelboim**
April 8, 2022

After reading your post I remembered the following lecture by Gell-Mann which I believe is quite timely: https://youtu.be/o8_imzEdS84

27. **Marco Masi**
April 8, 2022

A) I think that the good old Hegel's dialectic could be useful as a methodological approach, since primary school. I made this experiment myself in pre-college education. Those students who exercised a thesis, anti-thesis, and synthesis approach were less prone to pseudo-scientific nonsense. Nowadays, people (in the academic environment as well) rely too heavily on the "right" source, or on the "expert" authority, excluding every other point of view. Putting oneself in the shoes of those we do NOT agree with, is a difficult but highly enlightening practice.

B) "For years now we've been living with this in the form of phenomena like Trumpism, and the past few weeks have seen the Russian government exploiting these methods to conduct a campaign of brutal slaughter." –
I think it goes the other way around. Trump exploited a disinformation tactic that Russia practiced since the times of the Soviet Union.

28. **dms**
April 8, 2022

These are political problems and they have political solutions. They don't have anything to do with good science, bad science, or anti-science. I really do not like the implication that the problem is that everyone doesn't think like a scientist and the solution is that we teach everyone to do so. I don't think the world needs that many scientists.

The people who think COVID is not serious, or vaccines are not helpful, or climate change is not real are insecure. Their fears and insecurities lead them to make incorrect judgments about the world. The thing to do is to address their insecurities, not to lecture them about science. And I think their insecurities are legitimate. White working class people have been in a steady decline since the post-war era, and this decline has not been addressed by mainstream politicians. I'm not saying that when their insecurities are addressed these people will correct their judgments, but they will be much less likely to stand in the way of solutions. Instead of being reactionary they'll simply stop caring about these issues.

Part of the problem is the academization of politics. As more people in the US have become more educated, the Democratic party has become the party of people with college degrees. As a result of this we get politicians succeeding at the national level without being able to talk to people without college degrees. Issues that were probably better off confined to academia have become mainstream issues. Voters who are more educated can do their own research and don't need to be won over by rhetoric. But this leaves many less educated voters on the table, and unscrupulous people have taken advantage of this.

29. **SRP**
April 9, 2022

As I am a classical liberal , I find musings like our host's about the desirability of regulating "misinformation" on contested topics ironic. The exact same forces that put four string theorists on a panel defending the scientific method would control any regulated information regime. Such a regime would probably "support" many of my opinions about economics , but it would still be bad.

It is an acute observation that Peter makes, however, about everyone appealing to scientific norms even when they disagree with "consensus" science. That certainly applies to me when I find myself shaking my head at what seem to me to be ill-considered though popular scientific judgments. Right or wrong, I am not interested in playing by"anti-science" criteria of truth.

30. **Peter Woit**
April 9, 2022

SRP,
I don't know what the answer to these problems is, but it's important to get people to think beyond bromides about "trust science" or "freedom of the press".

On the issue of regulating the information environment, we've already seen what an unregulated information environment looks like and how it gets weaponized by bad actors who want to tear societies apart. We've learned an unregulated Facebook is a great tool for anyone who wants to organize genocide of a minority population or destroy fragile democratic institutions.

The current information environment is a regulated one, with regulations set by Mark Zuckerberg, Jack Dorsey (soon to be joined by Elon Musk), and whoever is running Google. Maybe that's the best humanity can do, maybe not.

31. **More anonymous**
April 9, 2022

@SRP
I personally think the solution to these problems lies in empowering the people's judgement. For example, when it comes to any theory, irrelevant of how advance the mathematics of it are if it makes false predictions. Then there should be a cost in credibility to be paid. This is what the scientific method is practiced as well. (The case of no predictions should be looked down upon for a vast chunk of money was taken with 0 output).

On the note of misinformation, again I would advocate empowering the individual's judgment and creating tools which enable the same. The app ground news (https://ground.news/) would be an example of what I'm talking about.

32. **SRP**
April 9, 2022

Peter, you already have become snarled in contradiction about Facebook (whose allegedly malign effects have been wildly exaggerated,BTW). Are we already regulated or not? Normally one tries to distinguish curation and editing from censorship. My conceptual test for this is whether user A is being vectored to material that user A would be expected to prefer if he had the time and knowledge to assess, or whether B has decided that A should not see that material despite knowing that A would be very interested in it.

FB has veered quite a bit over time in how it polices and filters user-generated content. The overriding fact that volume makes considered human judgement of each user post impossible, while automated solutions remain highly imperfect, guarantees a large number of Type I and Type II errors regardless of one's editorial goals. But it is obvious that FB has responded to threats from politicians by tightening (directionally) its filter on content. A visible manifestation of this has been their public outsourcing of many decisions to supposed "fact-checking" organizations whose biases are clear for all to see And those biases are of a very similar sort as the ones that put string theorists in charge of adjudicating what is "anti-science.".

33. **Peter Woit**
April 9, 2022

SRP,
Not sure what contradiction you're talking about. All information environments are regulated to some degree and I'm not pro or anti regulation in the abstract. The devil is in the details.

As far as Facebook goes, I think you're not disagreeing with my point that its regulation policies are now in the hands of one person, Mark Zuckerberg. Possibly his main regulation criterion is that however its done, it not force him to hire human beings on a scale that would cost him too much money.

For an example of regulation directly related to the posting, it seems to me likely that the shutdown of Lubos's blog was caused by this March 23 change in Google policy

Update regarding Ukraine (March 2022)

Due to the war in Ukraine, we will pause monetization of content that exploits, dismisses, or condones the war.

(Posted March 23, 2022)

On March 22 Lubos's rants about the war were allowed and monetized, then a Google executive or committee changed how they regulate speech, and now no more Lubos ranting. One can make a reasonable argument that they should not have denied us Lubos, or that they should have shut him down long ago, but this Google executive or committee is playing a huge role in regulating speech throughout the world. Maybe handing this over to our tech overlords is the best we can do, maybe there's a better way. I don't know.

34. **jjonhson**
April 9, 2022

Sudden dramatic increases in the reach of media — printing press, radio, television, internet — tend to be disruptive, and it takes time for rules (laws, licensure, etc.) to catch up. The internet presents special challenges, including speed of circulation, anonymity, unlimited points of entry, opportunities for unchecked amplification, and confusion of the distinction between public and private. Some controls have emerged — e.g. private firms responding to public pressure — but we're not where we need to be and it's hard to anticipate from the armchair where exactly that is. One hopes that accountable institutions are still robust and nimble enough to meet the challenge.

35. **SRP**
April 10, 2022

Peter,

I don't want to drag this out, but prior to arguing that tech bosses are in effect regulating speech, you wrote:

"On the issue of regulating the information environment, we've already seen what an unregulated information environment looks like and how it gets weaponized by bad actors who want to tear societies apart."

So either we've seen unregulated or we haven't.

As for FB not hiring enough people to monitor the billions of posts they get each year…just think about the scaling. Even a non-profit willing to run losses wouldn't be able to provide that level of contextually informed human review, and if the reviewers were human, a who-guards-the guardsmen problem results.

At this point, Substack is the last mainstream venue for publishing views that anger online mobs and/or upset the worldview of coastal U.S. political and cultural elites.

36. **Peter Woit**
April 10, 2022

SRP,
I was referring to the fact that Facebook had to introduce increased regulation in response to dramatic bad effects of their allowing some kinds of unregulated postings (e.g. the Rohingya genocide story).

I don't think the moderation problem Facebook has is one of technical difficulty in implementing a specific moderation policy. They have huge programming resources to throw at an AI-based approach and huge financial resources to hire humans to cover the cases where AI won't work.

The problem is not implementing Zuckerberg's policy but what the policy is: it's all right to use his platform for pure propaganda. Part of his reason for refusing to delete untrue posts is that it's often hard to distinguish truth/untruth. But I suspect a larger role is played by his understanding that if he starts deleting propaganda from some sources, he'll lose a large part of his user base that is devoted to that propaganda.

In 2020 Zuckerberg's policy was for instance
https://www.thewrap.com/mark-zuckerberg-rules-out-trump-fact-checks-says-facebook-shouldnt-be-the-arbiter-of-truth/
There have been more recent statements from them about this, see
https://transparency.fb.com/policies/community-standards/false-news/
In the last few weeks the war has created a new situation,
https://www.nytimes.com/2022/03/30/technology/ukraine-russia-facebook-instagram.html
one that makes very clear that in a wartime situation if you decide to allow propaganda you may be getting pretty directly involved in the slaughter of innocent people. I wonder whether this will change Zuckerberg's thinking and thus Facebook's future policies.

37. **Low Math, Meekly Interacting**
April 12, 2022

I'm late to the party, but fwiw, I think you nailed it. People aren't anti-science. They love science…as long as it doesn't challenge any of their preconceptions or cherished dogmas.

Now that we have limitless connectivity, anyone anywhere can find a credentialed individual who tells one exactly what one wants to hear, and hence they're free to dismiss the so-called experts because Conspiracies. And credentialed individuals can spout whatever nonsense they like as long as they aren't explicitly advocating mass murder because Freedoms. Sometimes even that minimum standard needn't stop you.

So, there is a "solution": The average person consumes media judiciously, considers diverse points of view with both a willingness to listen and a healthy dose of skepticism, and is willing to educate themselves both about history and current events sufficiently well that the next time they cast a ballot they aren't committing an act of civic malpractice.

With that out the way, I therefore conclude we're all fucked. Enjoy the tail-end of the post-enlightenment while it lasts, which seemingly won't be much longer.

38. **SRP**
April 12, 2022

Peter, Zuckerberg had the right idea originally. Attempting to regulate viewpoints via blocking communications has never been a winning approach to promoting a free society. Nor would it be practical for FB to do a contextual, discriminating job of sorting out truth and falsity at the volume it faces.

Nor is "propaganda" a good excuse. Listening to Tokyo Rose and Lord Haw-Haw caused more laughs than demoralization for Allied soldiers. Those who rarely hear dissenting views or propaganda are in the position of those with naive immune systems facing infections; Milton had it right when he said "I cannot praise a fugitive and cloistered virtue."

You may find this open letter to Elon Musk from the president of the Foundation for Individual Rights in Education useful:

https://www.thefire.org/open-letter-to-elon-musk-from-greg-lukianoff-on-preserving-free-expression-on-social-media/

39. **Peter Woit**
April 12, 2022

SRP,
About the only thing more naive and crazy than believing that this problem can simply be solved by "free speech" is believing that Elon Musk is going to save us…

40. **Santo D'Agostino**
April 13, 2022

Jonathan Haidt makes some interesting suggestions towards the end of his article here

https://www.theatlantic.com/magazine/archive/2022/05/social-media-democracy-trust-babel/629369/

in a section entitled "Reform Social Media."

1. Social media companies need to verify users. This will reduce or eliminate bots and curb abuse.

2. Modify "share" features to slow the spread of lies by requiring users to copy and paste instead of just clicking on a share button.

3. Require social media companies to share their algorithms and data with researchers.

None of these suggestions involve banning individuals or policing content.

41. **Mello**

April 14, 2022

wrt lubos, his response to the query about his blog's disappearance is here:

https://www.blogger.com/comment.g?blogID=7043496070972918852&postID=8207791788172964990

42. **Peter Woit**
April 25, 2022

SRP,

It looks like the plan of putting the world's richest man in charge of our information environment is now going to be realized, we'll see how that works out. Maybe putting everything in the hands of an insanely rich wacky Tech overlord not very interested in whether what he says is true or not is the best humanity can do under the circumstances.

ABC on NHK

April 9, 2022
Categories: abc Conjecture

There will be a documentary broadcast tomorrow in Japan on Mochizuki's claimed proof of the abc conjecture. I was interviewed for this by the filmmakers last year, but don't know anything about whether and how that footage will be used. I'd be curious to hear reports from any Japanese-speaking readers who see the documentary tomorrow.

Over the years there has been a detailed coverage of this story here on the blog. To make it more accessible, I've added an abc conjecture category. In case the documentary doesn't make this clear, the current consensus of experts in the field is that there is no proof. Peter Scholze and Jacob Stix identified a problem with Mochizuki's proof in 2018 (discussed in detail by Scholze and others here), and Mochizuki has not provided a convincing answer to their objections. No one else (including the journal editors who published the proof in PRIMS) has been able to provide a clear explanation of the problematic part of the proof.

Update: NHK has two web pages summarizing the content of the program, see here and here for English translations.

Taylor Dupuy is still making implausible claims that Scholze's criticism of the proof is invalid. To judge for yourself, see here a long detailed discussion of the issue between them involving several other experts.

Reports I'm seeing from those who have watched the program say that it does correctly explain that the proof is not accepted by many experts.

Comments

1. **Meata Konvo**
   April 9, 2022

   "Equivalent" theory by Kirti Joshi. No proof of ABC.

   https://arxiv.org/abs/2111.06771

2. **David J. Littleboy**
   April 10, 2022

   I thought I had already missed it, but it's tonight at 9:00 pm. I'll record it and give a quick overview tomorrow.

3. **Random guy**
   April 10, 2022

   I saw parts of it. The main takeaway of the show, I believe, is that Mochizuki's proof is a radical departure from conventional mathematics that continues to be the subject of fierce debate amongst mathematicians. Peter: there's a scene where you say something to the effect that Peter Scholze's objection was a huge deal for the community which allowed many to stop worrying about the legitimacy of the proof – I guess credit to the producers for including that..?

   Transcipt and screenshots at links below:

   First half:
   https://www.nhk.jp/p/special/ts/2NY2QQLPM3/blog/bl/pneAjJR3gn/bp/pzwyDRbMwp/

   Second half:
   https://www.nhk.jp/p/special/ts/2NY2QQLPM3/blog/bl/pneAjJR3gn/bp/pBg9n63J4m/

4. **David J. Littleboy**
   April 10, 2022

   The NHK special on Mochizuki wasn't as bad as it could have been.
   Long story short, it's about how IUT is new and different and could revolutionize math. While it does present that there are mathematicians who don't accept the proof, it glosses over that with claims that we don't yet have ways to describe it yet; i.e. that the
   problem is more how to explain the ideas than that the proof doesn't convince.

   Mochizuki himself refused to be interviewed, saying "How IUT differs from previous mathematics is an important question, but presenting it to a popular, or even mathematically sophisticated, audience in a TV program would be unreasonable."

   Peter Woit is quoted at the beginning as saying (roughly)
   "It's unlikely that the abc conjecture has been proven."
   and David Roberts is quoted as saying "It most certainly has been."
   At the end Roberts is back saying people who don't get Mochizuki's ideas
   are stuck in old mathematical thinking, and PW is back saying Sholze is the best
   number theorist we have, and if he's not convinced, it's not convincing.
   Then Peter is quoted saying "I'd guess most mathematicians are real happy that
   they don't have to worry about it any more."

   Much of the program was an attempt at an introduction to number theory. Attempt.

   Many mathematicians who were concerned with the abc conjecture or related mathematics prior to Mochizuki were interviewed.

   Taylor Dupuy is presented as a young and up and coming mathematician who thinks it worth betting his career on. (This may be a tad of an overstatement of his position, but he's interviewed multiple times throughout the program and the point that there are young and upcoming mathematicians interested in it is one of the punch lines at the end of the program.)

   My SO watched it with me, and reports that to a non-technical audience, it reinforces the prejudice that math is a waste of time. Furthermore, she
   says that the folks at NHK should have listened to Mochizuki and not tried to explain it to a popular audience.

OK, maybe it was as bad as it could have been…

5. **Z Y**
April 10, 2022

For me the program is an excellent exposition to the relevance of the abc conjecture, in some ways more understandable than the Notices AMS article of a few years ago, a good document to have for future historians and may influence young people to enter into mathematics

6. **David Roberts**
April 10, 2022

> David Roberts is quoted as saying "It most certainly has been."
> At the end Roberts is back saying people who don't get Mochizuki's ideas

Hmm, I'm pretty sure I didn't say this. Or at least, didn't mean to say this!

7. **David J. Littleboy**
April 10, 2022

David Roberts said:

"Hmm, I'm pretty sure I didn't say this. Or at least, didn't mean to say this!"

I was wondering if you'd say that. The program was structured (in those two segments) to put you in contrast with Peter, and gave the strong impression that that was what you thought. A careful translation of the Japanese might show that you were more careful than to say those things so clearly. Might.

FWIW, the quote of yours on the web page referenced above is a good summary of the whole point of NHK's presentation of Mochizuki's theory, namely that he sets up two things as being the same and then handles them as being different. "What is strange about Mochizuki's theory is the point that at the same time as saying two things are exactly the same, next, it handles them as being completely different. In mathematics, it is a basic principle that two things that are seen to be the same are the same. Is it possible for things to, while being the same at the same time to differ? I though about this really carefully. No, this is absolutely unreasonable."

8. **David Roberts**
April 10, 2022

@David J. Littleboy

my position is that I'm more cautious about how to interpret the clues we are getting from various parties. I very much respect Peter Scholze and I don't think he would have made an elementary mistake. I take Scholze and Stix's document at face value when it is worded cautiously (eg saying it isn't a watertight argument), and of course Peter S will stand by it. It doesn't hide the fact it's not dealing with the full theory, but this is the sort of thing high-level people do work with in other sub-fields (Terry Tao has written things like this, for analytic number theory). Discussions privately with Peter S since have pointed to the exact fact he was never satisfied with in Mochizuki's verbal explanations, in response to a specific question about passing to the full, un-simplified theory. Perhaps there is an explanation for this, but Mochizuki doesn't have a good way to explain his intuition, or maybe there is a deeper flaw that is just surfacing like a wrinkle in a badly-fitted carpet. Taylor is more blunt, saying to Scholze and Stix "you can't do such and such a step", in the video (posted yesterday on YouTube) of him presenting his work from a year ago. This is of course, pointing to the actual contentious isomorphism (in the hexagon diagram), and is the clearest statement I've seen in pushback to Scholze and Stix's argument, but it's very light on details; given the medium, and time constraints, it was appropriate, but I'd love to hear more. Taylor is open in the video up front that there are things he thinks don't necessarily join up in the argument, his complaint is that if there is a mistake, he doesn't think it's where Scholze and Stix are pointing.

Taking Corollary 3.12 as a *conjecture*, as Taylor does, lots of interesting mathematics can be done. This is in my view akin to assuming GRH and then finding all kinds of qualitative estimates of prime distributions, etc. This is how I view the subsequent IUT work of Mochizuki et al, getting FLT etc. It's all conditional on an interesting conjectured inequality in anabelian geometry/Arakelov theory. But, contra Mochizuki, we really can't say that inequality (i.e. Corollary 3.12) is established. (More than one expert has described that step as a cliff, or a sudden leap etc. If experts are happy with everything before then, and then all struggle at that point, it's not because the theory is just too hard for them.) I'd rather people take this view, than throw the lot in the bin. The glory of giving a new proof of FLT, or other hard number-theoretic results, is clearly lessened, but it's at least a healthier way forward for the community (IMHO) to try to extract useful content here, being clear with what is accepted, and what is not (cf work of Kirti Joshi who is claiming no big result, but is adapting some IUT ideas into standard anabelian geometry, and getting non-zero mileage).

It might be that my qualified optimism (conditional on assuming Cor 3.12 as a conjecture) happened to translate on screen into something less negative than others, including Peter W here. I can only go on what I think I would have said, and what is written on that NHK page for the documentary. I was quite complimentary towards Peter S in the interview, though it's quite likely they didn't use that, and I was fairly up-front that IUT doesn't give a proof of abc, as far we currently understand it.

9. **David Roberts**
April 10, 2022

Just to clarify, Taylor says in his video:

> I don't claim to understand Mochizuki's proof. I don't see it as completely correct because there is a point of contact that I can't see. And that's all I really want to say about that.

Taylor told me privately what that "point of contact" is, but not in any detail.

Taylor's discussion of what he sees as the weak point in the Scholze+Stix argument is at this time stamp, and goes for about 5 minutes. Make of it what you will.

10. **W**
April 10, 2022

@David Roberts:

In addition to that video, the other place Taylor explains his point of view on Scholze-Stix is Remark 3.8.3 of https://arxiv.org/pdf/2004.13108.pdf but this explanation is even shorter. Both the talk and the paper are from 2020.

He wrote a long Twitter thread (https://twitter.com/DupuyTaylor/status/1513240056395448324) responding to the text version of the documentary, where he expresses a lot of uncertainty on what the final outcome will be:

"For me there is still the option that this was a big waste of time for everyone "

"The situation may end up more like Lame's proof of Fermat or Kempe's Proof of the Four Color Theorem. Lame's proof was 175 years ago and we discuss it regularly when discussing the concept of a unique factorization domain. "

"There is also the possibility that we are in a Heegner-like situation where the proof is ultimately correct, but the author will pass away before the proof is finally accepted by the mathematical community (after Stark)."

I will resist the temptation to say anything more about the content of the proof.

@David J. Littleboy Your comment at the end about your SO makes me sad. I guess the point is mathematicians consider abstruse abstract problems and can't even agree on those. But in fact this is the one thing that (some) mathematicians can't agree on, and we agree on almost everything else, which is why it is apparently newsworthy.

11. **David J. Littleboy**
    April 11, 2022

    Hey, W! Don't be too sad. My SO was irritated more with NHK for trying to make a story about something that can't be made into a story than about math. Again, her main take away from this thing was that Mochizuki is a perfectly sensible bloke for not talking to NHK. I think I was able to get the point across that we don't know what math is going to be useful 100 years from know, so we have to just keep at it.

    (We were both irritated that it didn't have more on Mochizuki as a person. We're both fascinated by the problems of bilingualism and biculturalism and the trials and travails of growing up being the smartest bloke in the room. There's a human story there, too.)

12. **Z Y**
    April 18, 2022

    Is it only me that find remarkable that something as big as NHK took the decision to make a documentary about their countryman's effort to prove ABC and that the end message is that the proof is not yet accepted by the majority of experts? No wonder why Mochizuki didn't want to be interviewed

13. **Nayr**
    April 22, 2022

    Is there anywhere that we can rewatch the NHK documentary? It seems interesting.

14. **Ryan Pang**
    April 28, 2022

    You can now watch the NHK documentary on youtube:
    Part 1: https://www.youtube.com/watch?v=CklnC91Tbow&t=295s
    Part 2: https://www.youtube.com/watch?v=TtQ023QDCLQ
    Part 3: https://www.youtube.com/watch?v=_nIwOGWkf-M
    Part 4: https://www.youtube.com/watch?v=qAvLqDKaMaQ&t=8s
    The auto translate by youtube is a bit off tho.

15. **AG**
    May 2, 2022

    Hi
    Recently (on May 2) Prof. Mochizuki wrote a long comment about NHK documentary on his blog in Japanese. Its contents are very interesting.
    He himself says that the second half of what was broadcast was a complete flop.
    He is critical of the content of interviews and the way other researchers critique his theories. I know nothing about math and needless to say I can't judge anything. I could understand just one thing: Mochizuki is a man of great integrity, and sincerely from the bottom of his heart thinks that his proof is complete and correct.
    It's difficult for me to translate it into English. I hope someone translate correctly (only if not illegal) . To show the address of the blog here must be at least legal:
    https://plaza.rakuten.co.jp/shinichi0329/diary/202201010001/

16. **Peter Woit**
    May 2, 2022

    AG,
    I took a look at that (via Google Translate). It seems the part of the documentary he didn't like was the part where people discuss problems with his proof. He doesn't mention at all Scholze, (or me, I gather I mainly explain about Scholze's objection and its significance). He complains in detail about Faltings, as well as about Taylor Dupuy.

    I do agree he surely believes in the correctness of his proof, but he is not following standard professional norms in dealing with criticism.

Two New Quantum Field Theory Books

April 11, 2022
Categories: Book Reviews

I've recently noticed that two very good new books on quantum field theory have become available, one aimed more at mathematicians, one purely for physicists.

What Is a Quantum Field Theory?

Available online now from Cambridge University Press (actual printed books to come soon) is mathematician Michel Talagrand's *What Is a Quantum Field Theory?*. While it's subtitled "A First Introduction for Mathematicians" and definitely aimed more at mathematicians than physicists, it's a wonderful resource for anyone who wants to understand exactly what a quantum field theory is.

Like many mathematicians, Talagrand tried to learn about quantum field theory first from physics textbooks, which tend to avoid any precise definition of even the basics of the subject. He soon found what was the best source for someone looking for more precision, Gerald Folland's 2008 Quantum Field Theory: A Tourist Guide for Mathematicians. Folland's book is extremely good, but also extremely terse. In 325 pages it covers more carefully the material of an old-style QFT book such as Schweber's 900 page or so *An Introduction to Relativistic Quantum Field Theory* from 1961. Talagrand is covering much the same material, but with 742 pages to work with he is able (unlike Folland) to work out many topics in full detail, providing something previously unavailable anywhere else.

Both Folland and Talagrand have written books with much the same goal: to as precisely as possible explain the details of the renormalized perturbative expansion of QED. There is little overlap with the work of mathematical physicists who have aimed at rigorous non-perturbative constructions of quantum field theories. They are using canonical quantization methods and don't overlap much with many of the more recent physics QFT textbooks, which are based on path integral quantization and aimed at getting to non-abelian gauge theories and non-perturbative techniques as quickly as possible.

When I was learning QFT not that long after the advent of the Standard Model, I had little patience for fat QFT books about perturbative QED and canonical methods. Why not just write down the path integral and start calculating? Over the years I've realized that things are not so simple, with canonical quantization and operator fields giving a perspective complementary to that of the path integral. Among the more modern books, volume 1 of Weinberg's three-volume series is the one that best gives this different perspective, and is most closely related to what Talagrand is covering.

For mathematicians, Talagrand's book is a great place to start. For physicists, Weinberg's is an important perspective to get to know. If you're reading Weinberg and want more detail about precisely what is going on, Talagrand's new book would be a very good place to turn for help.

Quantum Field Theory: An Integrated Approach

Over the years I've often consulted various parts of Eduardo Fradkin's notes on quantum field theory on his web pages. On some basic topics I found these to give very clear explanations of things that were done in a confusing way elsewhere. After recently hearing that the notes are now a book from Princeton University Press, I ordered a copy, which recently arrived.

Fradkin's book has not much overlap with the material in the Talagrand book described above, and is somewhat different than traditional high energy physics-oriented QFT books. It tries as much as possible to integrate the high energy physics point of view with that of condensed matter and statistical mechanics. Path integral methods are then fundamental. Unlike many other modern QFT textbooks that aim at getting to the details of perturbative Standard Model calculations, Fradkin is more oriented towards getting as quickly as possible to non-perturbative techniques and models of interest in statistical mechanics. He gives a good introduction to various of the modern non-perturbative QFT techniques that have been developed in recent decades, often motivated by the so far only partially successful attempt to come to terms with a strongly-interacting gauge theory like QCD.

While most of the book is quite good, the first few pages aren't, and will immediately drive away mathematicians who might pick it up. The material in these pages about group theory uses bad terminology (for Fradkin, the "rank" of a Lie group is its dimension and the fundamental representation of SU(n) is the "spinor" representation) and sometimes is just completely wrong. On the second page of the first chapter after the introduction, he wants to explain why the Lorentz group is non-compact, in contrast to SO(3). To explain why SO(3) is compact he starts by mistakenly arguing that since it leaves the unit two-sphere invariant the points of SO(3) and of the unit two-sphere are in one-to-one correspondence, showing the volume of SO(3) is $4\pi^2$. This paragraph should be deleted in future editions of the book.

That this kind of thing can make it into a book like this is remarkable, but unfortunately relativistic QFT books and other sources (e.g. here) don't always get right basic facts about the Lorentz and rotation groups. I once tried to do my part to remedy this, see here.

Update: John Collins has here an article that provides a careful discussion of scattering in QFT, starting with the basics, which could be thought of as part of a QFT book. This may be of interest to both physicists and mathematicians who want to see something less superficial than many text book discussions.

Comments

1. **Alex**
   April 12, 2022

   One of the things I liked about Folland is that he discusses the full blown representation theory of the Poincaré group on infinite dimensional Hilbert spaces, which allows you to obtain the "wave equations" of the different fields from that classification. An application of Mackey's Imprimitivity theorem (it's based on Varadarajan's Geometry of Quantum Theory.) In the physicists' treatment, often only finite dimensional representatoions of the Lie algebra are discussed, and one often hears things like "the photon doesn't have a wave function". It has! And one can even prove that it doesn't admit a position operator, thus giving all the correct intuition. Of course, this is all for conceptual understanding and of little use for the hardcore calculations for real applications that physicists like. But I don't know why physicists' treatments have moved more and more to a purely pragmatic view in which such conceptual issues are just dismissed almost derogatorily.

2. **AcademicLurker**
   April 12, 2022

   As a stat mech-ish person who has been trying to gain a deeper understanding of the methods we borrowed from QFT (although I guess the borrowing has gone in both directions), the Fradkin book sounds intriguing. I'm currently slowly working my way through Blundell and Lancaster's Quantum Field Theory for the Gifted Amateur. Any idea how it compares with Fradkin?

3. **Peter Woit**
   April 12, 2022

Alex,
Understanding representations of the Poincare group pretty much completely determines how relativistic free field theories work, and constrain very tightly interacting theories. Interest in this was more widespread during the 1960s, less so after that, partly because it's to deal with such issues in terms of the path integral. There is a lot of this though behind what Weinberg does in his volume 1.

More recently I see a bit of revival of interest, from the directions of the amplitudes program.

Academic Lurker,
The Blundell/Lancaster book is better for QFT beginners, it does a great job of explaining carefully the basics. Fradkin is better on more advanced topics. His treatment of the stat-mech related stuff is truly integrated with the rest. Much of it is the same material as other modern QFT books that are HEP-oriented (harder to follow than Blundell/Lancaster since less detail, more background assumed) but in many places there is additional material related to stat-mech of a sort you don't see in other books.

4. **Jackiw–Teitelboim**
April 12, 2022

Coincidentally, I was looking for new resources on the topic of quantum fields for mathematicians to prepare for a class next fall (also to read something that is not on another depressing worldly news), and I found out about a new monograph by Albert Schwarz: "Mathematical Foundations of Quantum Field Theory" (2020).

I think this book is a very welcome one for the mathematically-inclined. While for quite understandably reasons most texts of this lot is oriented toward the topological, geometrical and group theoretical aspects, Schwarz's returns the attention to the algebraic, functional-theoretical issues, which should perhaps receive some renewed attention from time to time.

PS: Perhaps most readers of this site should remember another title by the same author, on topological QFT.

5. **Andrew**
April 12, 2022

Just wondering if anyone have any recommendations for books on QFT (and for that matter GR) which might be comprehensible to some one with a somewhat ancient PhD in physics who has no ambitions to actually work in the field. A step up from the popular stuff but giving a good conceptual overview rather than just leaping into calculations. Are Zee's Nutshell books any good? They look interesting, but hellishly expensive, especially to get to NZ where I live, so I don't want to just take a punt. I see he has another shorter one on QFT in the works.

6. **Peter Woit**
April 12, 2022

Jackiw-Teitelboim,

I got a copy of the Schwarz book last years. It's a nice very detailed treatment of the Fock-space formalism and concentrates on scattering theory. Mostly he is just writing about scalar field theory, and doesn't get very far into the perturbative calculations and renormalization that Folland and Talagrand get into. So, in some sense a more detailed treatment of a subset of Folland/Talagrand. Very different than Fradkin or other modern QFT books (or his own earlier book on TQFT.

Andrew,

I'd recommend the Blundell/Lancaster QFT for the Gifted Amateur book mentioned earlier. It's relatively elementary, and works things out completely and clearly. Zee's QFT book I don't think is a good place to start, since he's covering a lot of material without going into details of calculations. So, hard for beginners to make much sense of. If you have gone through something like Blundell/Lancaster, Zee is then great for giving you a deeper conceptual understanding of the subject.

7. **Jonathan**
April 13, 2022

There seems to be a glut of books on QFT in recent years, another one is Quantum Field Theory: From Basics to Modern Topics by Gelis.

Andrew, I have used Student Friendly Quantum Field Theory by Klauber at times and I found it to be quite pedagogical.

8. **Alex Gezerlis**
April 13, 2022

Peter,

Thanks for the heads-up on the new books.

I think you're being slightly unfair to Fradkin's excellent book. The point you make is valid; similarly, one could find other minor points to complain about, e.g., Fradkin is often not careful with the horizontal placement of tensor indices. (Then again, Schwartz doesn't care about vertical placement, either!).

However, any major work on such a complicated topic is bound to have minor flaws. The question that I think is important when deciding what to recommend to beginners is if the author exhibits mastery/insight. Fradkin certainly does, whereas the Blundell/Lancaster book cannot be placed in the same category; their text is riddled with conceptual errors (including extensive derivations where they're forcing the answer to go their way). I'm sure you would spot tons of these if you looked for them.

To offer an alternative recommendation, I think Maggiore's Modern Introduction to QFT is both beginner-friendly and authoritative. That being said, this is a blog, so you obviously don't have to do a full bibliographic analysis every time you are asked for a book recommendation 🙂

9. **Peter Woit**
April 13, 2022

Alex Gezerlis,
I don't think I'm being unfair to Fradkin, in trying to make clear that the value of his book lies in the material about non-perturbative QFT and the integration with stat mech. On the standard basics of perturbative QFT, parts are quite good, but parts are not and are done much better elsewhere, especially anything having to do with symmetry arguments. The problem I pointed out with the discussion of SO(3) is not a typo, but getting something very simple and very basic wrong, and it's not good to see something on the second page of the book that anyone reading it should have seen was a problem.

On QFT books in general, I don't think there's an ideal one. Even the best of them have their weaknesses. There's a lot to be said for Fradkin's, but others (including Blundell/Lancaster, whatever it gets wrong) are better for someone looking for an introduction to the basics.

10. **Andrew**

April 13, 2022

Peter and others, thanks for the comments. Blundell & Lancaster looks just the thing for me personally.

I've always found lack of physical motivation and consistent terminology an obstacle to understanding. I attended a lecture course on QFT by John Polkinghorne as a graduate student, and he leapt straight into something called "second quantisation" and detailed calculations, confusing the hell out of me. It's only recently that I saw Steven Weinberg saying that the term "second quantisation" was confusing and should be dropped, so I felt a bit better.

I hope it's not too far OT to ask if there's anything similar to Blundell & Lancaster but for GR? There's a similar abundance of confusing terminology here, especially around covariant vectors, or is it dual vectors or one forms – the last one totally unmentioned as a concept when I was a lad. Again, we leapt straight into index gymnastics, and I just wanted to ask "Why on earth would I want to raise or lower an index? What does that actually mean??"

Anyway, I ramble. Thanks to our host for a most interesting site.

11. **Paolo Bertozzini**
April 13, 2022

Dear Peter,

thanks for the overview of the interesting new book by M. Talagrand.

As regards quantum field theory books for a mathematical oriented audience, how is Your evaluation of introductory texts like:

E. de Faria, W. de Melo: Mathematical Aspects of Quantum Field Theory ,

J. Dimock: Quantum Mechanics and Quantum Field Theory ?

And of some more specialized texts such as:

R. Haag: Local Quantum Physics ,

J.Derezinski, C.Gerard: Mathematics of Quantization and Quantum Fields ?

Thanks in advance for any comment on the topic.
Paolo

12. **Alex**
April 13, 2022

I think it's also worth mentioning the whole school of QFT in curved spacetimes, whose current torch carrier seems to be Klaus Fredenhagen et al. Not only they do have completely rigorous mathematical constructions, they even extended (and made mathematically rigorous) perturbation theory to curved spacetimes via microlocal analysis! (They use their version of the Epstein-Glaser method)

A short, state of the art review is (details are in the references provided there):

https://link.springer.com/book/10.1007/978-3-642-02780-2

I find quite illuminating to put QFT in a generic curved spacetime background because you realize that many things you thought were essential to QFT… well, they were not! I feel I finally understood some deep conceptuals doubts I had only when I studied QFT in curved spacetimes for the first time. One can start with more welcoming introductions, like Wald's "QFT in curved spacetimes and BH thermodynamics". I highly recommend to physicists with a more particle physics background to take a look at that point of view on QFT, too.

13. **Peter Woit**
April 14, 2022

Paolo Bertozzini,
Those are all very different books. Some quick comments:

de Faria/de Melo: very short and very superficial. I don't believe you can usefully cover the wide range of topics they cover in such a short book, unless you're writing for people who already know the details of much of what you're writing about.

Dimock: this is a really great book that I highly recommend. It's from the canonical quantization point of view, and does everything rigorously, but very clearly. This is a different point of view than most modern physics textbooks, well worth understanding both by physicists and great for mathematicians trying to understand the subject.

Haag: This is an exposition of the operator algebra approach to QFT, which is quite different than either canonical quantization or the path integral. Little overlap with other QFT books. I haven't personally found that very useful or understood it well (but recognize that often once I understand something like this better I appreciate it a lot more)

Derezinski/Gerard: This is a very specialized and detailed treatment of the mathematics of canonical quantization. I think of it more as a reference book for anyone getting deeply into that subject, especially mathematicians with a specialized interest.

14. **Łukasz**
April 14, 2022

And what about the handbooks of:
Iwo Bialynicki-Birula and Zofia Bialynicka-Birula, "Quantum Electrodynamics"
Lewis H. Ryder, "Quantum Field Theory" ?

15. **Peter Woit**
April 14, 2022

Andrew,
An anonymous correspondent mentions (new edition about to come out)
https://www.amazon.com/Introducing-Einsteins-Relativity-Deeper-Understanding/dp/0198862032/

and the recently published Coleman notes
https://www.amazon.com/Sidney-Colemans-Lectures-Relativity-Griffiths/dp/1316511723/

16. **Alex**
April 14, 2022

Peter,

Re Haag: the operator algebra approach is actually a reformulation of canonical quantization (in the sense of going from the Poisson bracket to the operator bracket), it's just done at the abstract algebra level to avoid problems related to the failure, in the field case, of the Stone-von Neumann theorem of the CCR. Even LQG is formulated this way today.

Furthermore, a LOT has happened in the field since Haag's book. The review book I posted before covers all of the newer stuff.

It's a bit too hardcore math phy, and it requires quite a solid math background (from operator algebras to Hörmander's theory of wavefront sets for distributions, etc.) I certainly don't claim myself to have a full understanding of all the required techniques, but it's definitely worth it to at least give it a try.

17. **John Baez**
April 15, 2022

Andrew wrote:

> It's only recently that I saw Steven Weinberg saying that the term "second quantisation" was confusing and should be dropped, so I felt a bit better.

Second quantization is a real thing; for example if you take the Fock space of a Hilbert space you get another Hilbert space, so this is a process you can do twice – and this actually has applications in physics. But it's often not explained very well, so it might be better to ignore it in an introduction to quantum field theory.

I wrote a light-hearted introduction to nth quantization here.

18. **Arnold Neumaier**
April 15, 2022

Andrew wrote: "I attended a lecture course on QFT by John Polkinghorne as a graduate student, and he leapt straight into something called "second quantisation" and detailed calculations, confusing the hell out of me. It's only recently that I saw Steven Weinberg saying that the term "second quantisation" was confusing and should be dropped, so I felt a bit better."

Maybe you'll like the uniform way first and second quantization is cast in my just accepted paper:
A. Neumaier,and A. Ghaani Farashahi, Introduction to coherent quantization,
Analysis and Mathematical Physics, to appear (2022).
https://arxiv.org/abs/1804.01400

19. **Peter Woit**
April 15, 2022

I'd like to second (third?) the comments of John Baez and Arnold Neumaier about QFT and second quantization. It took me a while to understand this, but "second quantization" is a very precise and useful way to think about QFT from the canonical quantization point of view.

In brief, Hamiltonian mechanics says phase space is the initial data for an equation of motion, and functions on phase space are a Lie algebra (Lie bracket is Poisson bracket). For a linear phase space one can restrict to the subalgebra of linear functions. This is the Heisenberg Lie algebra, and quantization is just going to its (essentially unique) unitary representation.

If you start with the finite-dim phase space for a single particle theory, this "first quantization" gives you a representation on an infinite dim space of wavefunctions. One gets QFT by doing the same thing, but taking the infinite dim space of wavefunctions as your phase space, for which the term "second quantization" is not bad.

The details of this point of view are in my book
https://www.math.columbia.edu/~woit/QMbook/qmbook-latest.pdf

20. **John Collins**
April 15, 2022

John Baez wrote "Second quantization is a real thing; for example …".

Certainly, as John explained it is possible to define the concepts of second (and higher order) quantization. But for me, the physics issue is: What is the reason for using a quantization method?

The standard quantization method is for obtaining a (candidate) formulation of a quantum theory whose classical limit is expected to be some given classical theory. This is useful because before encountering quantum theory one knows about classical systems (including electromagnetic fields). As is indicated in Weinberg's paper https://arxiv.org/abs/hep-th/9702027 that method is all that is necessary to formulate QFT, and the idea was known from the earliest days of quantum theory. (However he only references the Born-Heisenberg-Jordan paper Z. Phys. 35, 557 (1926). The QFT idea is more clearly visible in the earlier Born-Jordan paper, Z. Phys. 34, 858 (1925).)

So, in agreement with Weinberg, I find it unnecessary to formulate QFT in terms of second quantization. However, when teaching QFT, I find it useful to give an account of non-relativistic Schrödinger QFT. Then there is a nice correspondence between the Schrödinger equation for a single particle wave function and a QFT formulation of the corresponding many-body theory, that can be called second quantization. I find I have to emphasize that its status for theory construction is very different from "first quantization", which I normally simply call "quantization". First quantization gets you from an ordinary classical theory to a new type of theory, viz a quantum theory in the Heisenberg picture. But second quantization gets you from one quantum theory (in a Schrödinger formulation) to another quantum theory (in the Heisenberg formulation). I have to emphasize that QFT needs no change in the principles of quantum theory; I also immediately enhance the Schrödinger QFT to include a term for inter particle interactions, which give a non-linear equation for the field.

I find it counterproductive when people treat the formulation of relativistic QFTs as a matter of second quantization. It confuses learners into thinking that it involves going beyond the general principles of quantum theory. The real change is going from situations where the wave function formulation is often the most convenient (for non-relativistic situations) to a Heisenberg-picture formulation with heavy emphasis on its time-dependent operators.

21. **Peter Woit**

April 15, 2022

On the topic of “second quantization” I agree with John Collins that the name might confuse some people, but it does reflect a central idea about how the multi-particle formalism works.

When you (first)-quantize the (d-dimensional) harmonic oscillator, with classical phase space $\mathbf R^{2d}$ that you have identified with $\mathbf C^d$, you get a quantum state space that can be identified with polynomials on $\mathbf C^d$, with degree of a monomial the number of quanta. In the Fock space approach to multi-particle systems you are doing exactly the same thing, with classical phase space now the single-particle quantum state space.

This is confusing if you are trying to understand quantum to classical, but it clarifies (at least for me) several things (why particles act like indistinguishable quanta, the relation of symmetries in the classical Hamiltonian formalism to symmetries in the quantum formalism).

22. **Chris**
April 16, 2022

Talagrand’s book is beautiful. It a pleasure to read in it. Explanations are clear and detailed. Every chapter has a short summary in a few lines that allows to check one’s progress. The style of writing is friendly to the reader. Content and form are beautiful. It’s a gentleman’s masterpiece.

23. **James Eshelman**
April 17, 2022

Peter,
If not too far off topic I’d be interested in your opinion of Adam Marsh’s book “Mathematics for Physics” ? He gives major math definitions and results across what to me is an astoundingly wide range of areas in math, without proofs. Just results. Often very useful for physics, especially when reading a paper depending on one of those areas Marsh covers. I haven’t yet run across any serious error, but someone more involved in math might. If there are few it might deserve to be better known among physicists.
Jim

24. **Peter Woit**
April 17, 2022

James Eshelman,
I haven’t seen the book before, just took a quick look. What I looked at seemed pretty accurate. From the table of contents it’s mostly a survey of geometry of use in physics (without discussing any of the physics).

The problem with this kind of thing is that if you cover so much material in 300 pages, there’s nowhere near enough detail for someone who is trying to learn a subject from the basics. And for people who already mostly know the subject, they likely have there own favorite places to use as reference. I suspect this book will mainly be useful to people who already know much or most of the content. Looking through it they’ll find some topics they didn’t know about, and will know enough to be able to follow those topics.

25. **InterestedLurker**
April 17, 2022

Peter or others what is your opinion of Matthew Schwartz’ book, “Quantum Field Theory and the Standard Model”?

Also, it seems that QFT courses quickly move through background material on Lagrangians and Fields and I haven’t found a book that covers those topics in great depth, is there a good mathematically rigorous treatment out there on those topics?

26. **Peter Woit**
April 18, 2022

Interested Lurker,
I haven’t taught a standard sort of QFT class for physicists so haven’t looked closely at the books available. My impression from a quick look is that Srednicki, Peskin/Schroeder and Schwartz cover much the same ground, with different emphases. My main comment about all these books is that they give a deceptively simple idea of how QFT works, one oriented towards computing terms in a perturbative expansion, avoiding engaging with most of the structure you would like to have to really understand a QFT.

There probably are mathematically rigorous treatments of Lagrangian methods out there, but I don’t think that’s actually useful. Physicists writing about QFT now seem to uniformly start with the idea of a general Lagrangian system (for which a rigorous treatment is difficult), but then immediately specialize to very special systems and assuming good properties. They then quickly get into trouble when crucial examples don’t have these properties (e.g. EM fields with their gauge symmetry). To my mind Hamiltonian methods capture much more of the structure one wants, but developing QFT along those lines is a very different project.

27. **lun**
April 18, 2022

“but then immediately specialize to very special systems and assuming good properties. They then quickly get into trouble when crucial examples don’t have these properties”
(e.g. EM fields with their gauge symmetry)

Can you elaborate here? Which properties do you mean?

28. **Peter Woit**
April 19, 2022

lun,
In textbooks this shows up as the fact that the Legendre transform from velocity to momentum variables is only one-to-one in special circumstances. Even for free field theories this becomes a problem, typically first seen when you try to quantize the theory of a free photon, and find that the canonical momentum for the time-like component of the vector field is zero.

29. **John Collins**
April 19, 2022

Peter and Interested Lurker,
I’ve taught QFT (to physicists) quite a number of times, and over the years I’ve become increasingly dissatisfied with the available textbooks, primarily concerning the foundations. (More advanced topics are another matter.) I’ve worked out ways to try to overcome some of the worst problems, and provided some short documents to the

students; I plan to make them more public after I've re-checked them.
The big problems are about the treatment of scattering theory and the derivation of perturbation theory. Of the many books I've examined, only those by Srednicki and by Peskin and Schroeder get reasonably close to what I want to see. Sterman also has some very useful material that helped me. I find the treatments in these books very useful for inspiring better treatments.
It's a long story to explain all the difficulties. But I see 3 main root causes:
First is the need to treat the infinite time limits in scattering in a physically (and mathematically) sensible way. Any treatment that doesn't use wave packets is destined to give nonsense. Here, I recommend Peskin and Schroeder's treatment, including a nice explanation of how one can relate cross sections to calculated amplitudes.
The second problem is the use of the interaction picture to derive perturbation theory. Haag's theorem says that for a relativistic QFT in an infinite volume of space the interaction picture never exists. As far as I can see, this breaks all derivations that attempt to derive perturbation theory directly for the S-matrix.
The only way I know of that works is slightly indirect, but much better and general I think: First get perturbation theory for the time-ordered Green functions, and then get the S-matrix by the LSZ method. (For some of my approach, see the paper of mine that Peter referred to earlier: https://arxiv.org/abs/1904.10923.)
There's still the problem of deriving the Gell-Mann-Low formula for Green functions without getting clobbered by Haag's theorem. You can modify the Peskin-Schroeder derivation to do this: Initially put the theory in a spatial box, and only take the limit later at a suitable point.
Once you take all the necessary limits (and do UV renormalization) you get properly behaved results to the extent that perturbation theory can show them.
The final difficulty is that quantum fields are not operator-valued functions on space time but are operator-valued distributions. It's easy to get into trouble if one doesn't allow for this. Here several books can help: Among them are the book by Talagrand that we were discussing here and Peter's own quantum mechanics book.

30. **Arnold Neumaier**
April 21, 2022

John Collins,
"The only way I know of that works is slightly indirect, but much better and general I think: First get perturbation theory for the time-ordered Green functions, and then get the S-matrix by the LSZ method."

This is sufficient only for theories such as QED that have no bound states. Once bound states are present, even the standard asymptotic treatment is ill-defined. This is the main reason why QCD has unsettled infrared divergences showing up in a Landau pole at (in contrast to QED) physically accessible energies. See the discussion in https://www.physicsoverflow.org/32752/?show=32756#a32756

31. **John Collins**
April 21, 2022

Arnold Neumaier appears to say that the LSZ method doesn't apply when there are bound states, and that the the standard asymptotic treatment is then ill-defined.

This is simply not the case. You just use products of fields (not necessarily at the same space-time point) that have the quantum numbers to add or remove one of the bound state particles you are interested in. For example in QCD, if you want to find an S-matrix element or an operator matrix element involving protons, you would use a product of three fields for the up quark for an outgoing proton, and the hermitian conjugate field product for an incoming proton.
Then you apply the LSZ method unchanged.
That is, you examine Green functions that include these fields. In momentum space, corresponding to the bound state there is a pole in the external momentum of the field product. In coordinate space, there is the corresponding asymptotic large-time oscillatory behavior.
Essentially the same idea is used in lattice QCD to calculate matrix elements of operators between hadron states. The important difference is that instead of the oscillating asymptotic behavior in Minkowski coordinate space, one has exponential decay corresponding to the mass of the particle.

32. **Arnold Neumaier**
April 22, 2022

John Collins,

"For example in QCD, if you want to find an S-matrix element or an operator matrix element involving protons, you would use a product of three fields for the up quark for an outgoing proton, and the hermitian conjugate field product for an incoming proton.
Then you apply the LSZ method unchanged."

It is not obvious that this recipe will give the correct perturbative expression. Please point to a paper where it is shown that this indeed yields the correct scattering amplitudes.

33. **John Collins**
April 23, 2022

Arnold Neumaier:
There are (at least) three separate issues here: One is whether the LSZ method applies to bound states, for which the unambiguous answer is yes (e.g., K. Hepp "On the Connection between the LSZ and Wightman Quantum Field Theory", Commun. Math. Phys 1, 95 (1965)). The second issue, which is the one you raise, is how to do practical calculations using perturbatively based methods. The third issue is what has been done with the LSZ method in non-perturbative situations.
It is true that in a fixed finite order of perturbation theory for a Green function you don't see bound states. But you don't have to restrict yourself to strict fixed order perturbation theory. There are known and successful methods for dealing with bound states in relativistic QFT that involve perturbative methods. You can get an introduction to these from Paul Hoyer's writings, e.g., https://arxiv.org/abs/1605.01532 One issue he discusses is how bound state poles in amplitudes arise with those methods, even when the poles don't exist in any finite order of perturbation theory.
One could ask Paul if he knows where these methods are applied to scattering involving bound states.
One other situation where the LSZ idea has been applied to bound states is in current algebra, for example for soft pion theorems. These results necessarily apply to QCD. A suitable current is used as an interpolating field for a pion, with the current having a non-zero matrix element between the vacuum and a state of a single pion. In the momentum space form of a Green function involving the current, there is a pole at the pion mass in the momentum corresponding to the current. The LSZ theorem tells you how to relate a Green function with poles to the corresponding S-matrix element.

34. **Arnold Neumaier**
April 24, 2022

John Collins,

I know that bound states are treated with perturbative methods after resummation, but all this is done (even in Paul Hoyer's work) in a somewhat ad hoc fashion, and not based on first principles.

Your claim was that your 2019 paper provides a more principled derivation, and that it extends trivially to bound states. But it is not straightforward to extend your paper to the bound state case by your recipe
"You just use products of fields (not necessarily at the same space-time point) that have the quantum numbers to add or remove one of the bound state particles you are interested in. For example in QCD, if you want to find an S-matrix element or an operator matrix element involving protons, you would use a product of three fields for

the up quark for an outgoing proton, and the hermitian conjugate field product for an incoming proton. Then you apply the LSZ method unchanged."

The reason is that with this recipe, your free field (5) then becomes a multilocal field with three spacetime arguments, and the analogues of your Green's functions (6) have three times too many spacetime arguments, too. This is quite different from what Haag/Ruelle/Hepp do! This mismatch affects everything you do later.

In Section VII you briefly allude to states of multiple particles and say that you do not need multiparticle wave functions. But this is an illusion caused by lack of attention to details. If you use 1-particle wave functions to do the smearing you do not get an in-space of bound protons isomorphic to $L^2(R^3,V)$ where V accounts for the discrete indices (as it should be) but an in-space of three quark states isomorphic to a direct product of three $L^2(R^3,V)$. To remedy this you need to do the smearing with some sort of bound state multiparticle wave functions. But there is no simple way to define these.

In particular, when you "put the theory in a spatial box" you destroy any distinction between bound and unbound states, since these are asymptotic notions that need unbounded space to be meaningful.

In Section 4G you mention the need for normalizable (i.e., smeared) states, and appeal indirectly to the cluster decomposition property discussed in Volume 1 of Weinberg's book. This property is, however, invalid for QCD because of confinement. One has cluster decomposition only for the bound states. Again it is essential to work with bound state multiparticle wave functions to ensure that everything is fine!

In the standard treatment of bound states via field operators one proceeds quite differently: One assumes the validity of 1. the perturbative formalism (ignoring bound states, leading to severe infrared singularies even after UV renormalization) and 2. some resummation techniques. Then one derives from it Schwinger-Dyson equations that, suitably truncated, produce approximate multiparticle wave functions of Bethe-Salpeter form.

In a first principle derivation, one would have to produce the Schwinger-Dyson formalism for bound states simultaneously with the perturbative structure. How this can be done in your setting seems to be a highly nontrivial challenge!

35. **John Collins**
    April 25, 2022

    Arnold and I are going to continue our discussion off-line for a bit. We'll report back when we come to what we hope will be agreement. There are non-trivial issues that we need to sort out at length.

Various and Sundry

April 18, 2022
Categories: abc Conjecture, Uncategorized

- Last week a review of the Mochizuki IUT papers appeared at Math Reviews, written by Mohamed Saïdi. His discussion of the critical part of the proof is limited to:

  > Theorem 3.11 in Part III is somehow reinterpreted in Corollary 3.12 of the same paper in a way that relates to the kind of diophantine inequalities one wishes to prove. One constructs certain arithmetic line bundles of interest within each theatre, a theta version and a q-version (which at the places of bad reduction arises essentially from the q-parameter of the corresponding Tate curve), which give rise to certain theta and q-objects in certain (products of) Frobenioids: the theta and q-pilots. By construction the theta pilot maps to the q-pilot via the horizontal link in the log-theta lattice. One can then proceed and compare the log-volumes of the images of these two objects in the relevant objects constructed via the multiradial algorithm in Theorem 3.11.

  Saïdi gives no indication that any one has ever raised any issues about the proof of Corollary 3.12, with no mention at all of the detailed Scholze/Stix criticism that this argument is incorrect. In particular, in his Zentralblatt review Scholze writes:

  > Unfortunately, the argument given for Corollary 3.12 is not a proof, and the theory built in these papers is clearly insufficient to prove the ABC conjecture…. In any case, at some point in the proof of Corollary 3.12, things are so obfuscated that it is completely unclear whether some object refers to the q-values or the θ-values, as it is somehow claimed to be definitionally equal to both of them, up to some blurring of course, and hence you get the desired result.

  After the Saïdi review appeared, I gather that an intervention with the Math Reviews editors was staged, leading to the addition at the end of the review of

  > Editor's note: For an alternative review of the IUT papers, in particular a critique of the key Corollary 3.12 in Part III, we refer the reader to the review by Scholze in zbMATH: https://zbmath.org/1465.14002.

  Since the early days of people trying to understand the claimed proof, Mochizuki has pointed to Saïdi as an example of someone who has understood and vouched for the proof (see here). Saïdi is undoubtedly well aware of the Scholze argument and his decision not to mention it in the review makes clear that he has no counter-argument. The current state of affairs with the Mochizuki proof is that no one who claims to understand the proof of Corollary 3.12 can provide a counter-argument to Scholze. Saïdi tries to deal with this by pretending the Scholze argument doesn't exist, while Mochizuki's (and Fesenko's) approach has been to argue that Scholze should be ignored since he's an incompetent. The editors at PRIMS claim that referees have considered the argument, but say they can't make anything public. This situation makes very clear that there currently is no proof of abc.

- At one point the American Institute of Mathematics (founded in 1994 with financing from John Fry) was supposed to move from its location behind a Fry's Electronics store to a castle in Morgan Hill modeled on the Alhambra (see here). This never worked out, and last year Fry's Electronics declared bankruptcy. The latest news is that next year AIM will move to Caltech, for more see here.
- I'll never understand why places like MIT continue to teach undergraduate courses on a failed speculative idea about physics.
- There has been a lot of coverage in the press of claims by a group analyzing old CDF data to have come up with a dramatically better value for the W mass (one seven sigma away from the SM value). While this would be really wonderful if it were true, unfortunately that doesn't seem very likely. There isn't a well-motivated theoretical reason for this discrepancy, this is a very challenging measurement, and the new value seriously disagrees with several previous measurements at CERN. For an informed discussion of this from someone who was on CDF and has worked on these sorts of analyses, see Tommaso Dorigo's blog post.
- It will be interesting to see how well the LHC experiments can ultimately do this measurement. The LHC is about to start up again after a long shutdown, with beam commissioning starting on Friday.

Comments

1. **S**
   April 18, 2022

   As a regular reviewer for Mathematical Reviews, this is disappointing. Presenting a biased pro-IUTT review while linking to an "alternative review" that describes the consensus of mainstream mathematicians, gives the false impression that the debate is ongoing. As a resource used widely by mathematicians all over the world, MR has a responsibility to play referee here, rather than providing access to arguments for both sides and letting everyone decide for themselves what the truth is.

   It seems we have learned nothing from the Trump years. It's not enough to give both sides of a disagreement an equal platform. To maintain credibility, one must be willing to take a side when the preponderance of evidence is clear.

   (In fact, it's worse than that, because even with the editor's note, MR comes across as more on Mochizuki's side than not.)

   I review papers for MR on a voluntary basis because I appreciate MathSciNet as a useful resource. But credibility is a fundamental part of what makes it a useful resource.

2. **Peter Woit**
   April 18, 2022

   S,
   I've found it very hard to understand why MR published the Saïdi review despite being aware that there were well-know problems with the proof that the review did not address. The current editor's note improves the situation but I agree that as it stands the review gives the wrong impression, not making clear that the consensus of the expert community is that there is no proof of Corollary 3.12.

3. **Roger**
   April 19, 2022

   I agree that there is quite some evidence that the CDF W-mass measurement is problematic (eg comparison with earlier measurements).

   There is, however, also a well known effect that new measurements have a tendency to be consistent with earlier measurements even when the latter measurements are wrong.

   All credit to CDF for following a robust analysis procedure that precludes the possibility of artificially agreeing with earlier measurements. They did the best measurement they thought they could do and published it.

   I wouldn't bet much on it being right but that's another story.

4. **Mike**
April 19, 2022

Link to the MIT course is not working

5. **Peter Woit**
April 19, 2022

Mike,
Works for me, but did change link to https in case that was causing the problem.

6. **Mike**
April 19, 2022

It works now, thanks.

7. **John Baez**
April 19, 2022

It would actually be great if MIT taught a course on failed speculative theories in physics, perhaps using Helge Kragh's book as a textbook. If students only learn about successful theories they won't get how hard it is to be right, and how many great physicists had theories that didn't pan out.

8. **Martin S.**
April 19, 2022

Two other takes on the W mass:
https://resonaances.blogspot.com/2022/04/how-large-is-w-boson-anomaly.html
https://non-trivial-solution.blogspot.com/2022/04/do-we-have-finally-found-new-physics.html

9. **Michael Weiss**
April 19, 2022

Prof. Baez has made here a wonderful suggestion. I wonder if such a course might be further enriched by an account of theories that "failed" in their original form and context, but which contained conceptual elements that re-emerged in subsequent decades or centuries, i.e., as components of a superseding theory. Examples might include: (a) in the fifth century BC Greek thinker Leucippus and his pupil Democritus envisioned small indivisible particles, "atoms"; (b) Descartes proposed a "corpuscular" theory of light (1637) at odds with the wave nature of classical light; (c) Einstein posited a cosmological constant to provide an outward pressure opposed to overall gravitational collapse, at odds with Hubble's observations; and (d) Veneziano strings as a theory of the strong force, re-conceptualized as a renormalizable model inclusive of gravitons.

10. **Olaf Teschke (zbMATH Open managing editor and section editor algebraic geometry)**
April 20, 2022

I would respectfully disagree with the negative assessment of the handling of reviewing IUT by the colleagues of MathSciNet. Compared to the peer review process, the purpose of a review in MathSciNet (as well as zbMATH Open) is different; to quote the Guidelines for Reviewers (https://mathscinet.ams.org/mresubs/guide-reviewers.html):

> What is a review? A review should primarily help the reader decide whether or not to read the original item. The review may range in length from a few lines to about 600 words. In most cases the review should state the main results, together with sufficient information to make them comprehensible to someone already familiar with the field. The main ideas of the proof should be sketched when feasible. If the results are technical, requiring extensive notation or elaborate formulas, it is preferable to describe them with a few well-chosen and relatively nontechnical sentences. A review should also contain comments that provide some background for the item, evaluate it and connect it to related items or approaches. Well written reviews are most desirable since the lasting value of Mathematical Reviews/MathSciNet as a research tool lies in its independent third-party reviews.

It is frequently very difficult to find reviewers willing to write a high-standard review, and obviously especially so in this peculiar case (zbMATH Open was very lucky that Peter Scholze agreed for this specific volume). Since our services are fundamentally based on the willingness of the reviewers to provide appropriate reviews, editorial policy will in most cases refrain from interfering or even rejecting detailed reviews contributed by an expert in the field (who Mohamed Saïdi certainly is). With this long-term practice in mind, adding an Editor's note as in the case of the MathSciNet review is already a very rare measure, and a quite indicative statement.
Ideally, of course, the validity of a theorem should not need to be settled in the framework of our reviewing services; but given the situation as it is, I think that both reviews contributed to the clarification of the current status.

11. **Peter W Shor**
April 20, 2022

John Baez says:

> It would actually be great if MIT taught a course on failed speculative theories in physics, perhaps using Helge Kragh's book as a textbook.

That's a great idea, and it sounds like it would be a great course. One problem with this idea is that the course would most likely be listed under Science, Technology, and Society (STS) and not under Physics (Course 8), so most physics majors woudn't end up taking it.

12. **Peter Woit**
April 20, 2022

John Baez,
Perhaps teaching a course on the details of a failed speculative theory would be a good idea, if you explained to the students that it was a failure and why it failed. Teaching such a course and presenting it to undergraduates as a success is what I don't understand at all.

13. **Peter Woit**
April 20, 2022

Olaf Teschke,
Thanks very much for contributing your perspective. My understanding (which may not be correct) is that the problem MR had in this situation is that they were unable to find any expert willing to write a review including a discussion of the problem with the proof. Saïdi was willing to write a review, but not to discuss the problem with the proof. It seems they decided to publish his review since the alternative would be no review.

But a review that doesn't discuss a well-known flaw in the proof clearly violates their own standards as you've quoted them above: that the proof is flawed is highly relevant to whether a reader would want to read the paper, and is crucial for the background of the paper and for its evaluation. The editor's note improves the situation, but this still is a flawed review that does not meet their stated standards.

14. **Kevin S Van Horn**
April 22, 2022

Ironically, the first page of the linked-to notes for the MIT string-theory course gives the argument against string theory in capsule form.

15. **Low Math, Meekly Interacting**
April 22, 2022

I first read about the W anomaly in Quanta. Why they don't just go straight to Tomasso Dorigo or Adam Falkowski every time one of these things heats up is a bit of a mystery to me. Maybe because the wet blanket or reality doesn't get eyeballs.

16. **Peter Woit**
April 22, 2022

Kevin S Van Horn,
I also found that pretty weird. You could interpret "we don't know what string theory implies" as an argument for further research in the subject, but to me it seems an extremely strong argument against teaching the subject to undergraduates.

Lot of other strange things there, such as "Fact 2" which tells us that "the global picture is still lacking today because the field is very young". Over 50 years old "very young"? I can understand why people try and make excuses for the failures of the subject, but I don't understand the phenomenon of making absurd excuses.

17. **Sam**
April 22, 2022

I see the MIT course notes have the standard sum of all integers is -1/12 to get d=26, without any real justification. This put off multiple students when I took this course long ago.

18. **fred**
April 23, 2022

Just to add a bit in this various section: the restart from LHC comes along with some just-around-the-corner hype: https://www.bbc.com/news/science-environment-61149387

19. **Peter Woit**
April 23, 2022

Sam,
At least for that there's a perfectly legitimate mathematical explanation, whether or not the instructor explains it. What's really odd is that this is a physics department teaching fundamentals of physics to its students, but scheduling one course with no known connection to physics. "String theory" has become institutionalized as an ideology disconnected from conventional science, with, at least at MIT, indoctrination part of the undergraduate curriculum.

20. **Shantanu**
April 23, 2022

Peter : OT. A talk by David Gross on string tehory and unification at KITP in 2022
https://online.kitp.ucsb.edu/online/snowmass-c22/gross/rm/jwvideo.html
would be interested in your views.

21. **Peter Woit**
April 23, 2022

Shantanu,
I did watch that. Gross repeated the now 45 year old argument about SUSY GUTs unification of couplings (from what I remember, when I watched this, in the discussion afterwards Mike Peskin pointed out that that unification only works well in the simplest one-loop calculation, that it gets worse if you do a more precise calculation).

He did admit that things haven't worked out, pointing to no SUSY at LHC and the landscape. He did also say "I see little hope that we can achieve the original goal" absent some new theoretical breakthrough. He argued that people should try to solve QCD via string theory and address the "What is string theory?" question, but he has been saying this for decades, people have been trying for decades, nothing has changed.

The one new thing I heard from him was a mention of the bootstrap program and a comment that "other UV completions than string theory may exist and we should be open to this possibility".

22. **tulpoeid**
April 24, 2022

About IUT, what I've been trying to understand is if that work has other merits on its own.
Ie. even if the proof of Corollary 3.12 is wrong, so the overall proof is wrong, does Mochizuki's vast construct contribute to math in some other collateral way?
If I'm right this has happened before with proofs that turned out to be wrong, and I'll be genuinely puzzled if such complex work has nothing else to offer and all its value is hanging on a single point.

23. **Hermann Vile**
April 24, 2022

Mochizuki's latest endeavor seems to be regularly updating this document (https://www.kurims.kyoto-u.ac.jp/~motizuki/Essential%20Logical%20Structure%20of%20Inter-universal%20Teichmuller%20Theory.pdf), which is now 140 pages. I doubt this will convince anyone; if anything, his writing has become even more turgid and depressing.

@tulpoeid: No, there is *nothing* useful in IUT. If there was, it would've been successfully extracted by ambitious young people in the ten (!) years since the IUT papers were first posted.

24. **Peter Woit**
April 24, 2022

Hermann Vile,
That document just gets stranger and stranger. Last time I looked at it, see here
https://www.math.columbia.edu/~woit/wordpress/?p=12220
it was at only 65 pages. It has kept the same structure:
1. No reference to Scholze or Stix by name, no reference to the original document they wrote about the problems with the proof, no reference to later efforts by Scholze to further explain why the proof is invalid (e.g his Zentralblatt review and the discussion on this blog).
2. Out of three long sections, the only one with technical content is the third. Mochizuki seems to think it's a good idea to precede that by two long sections about elementary mathematics and logic supposedly not understood by his critics, as well as a long rant about mathematical ethics (given the PRIMS publication story, this is especially rich). These two sections do an excellent job of completely destroying his credibility.

tulpoeid,
If you look at Scholze's Zentralblatt review, he summarizes the mathematical content of the rest of the IUT papers. Kirti Joshi has written a series of papers, summarized in
https://arxiv.org/abs/2111.06771
which attempt to put Mochizuki's ideas in a more conventional context, where it should be more clear if anything useful can come out of them.

Three Quick Items

May 3, 2022
Categories: Uncategorized

Just time for three quick items:

- There's a wonderful book out now published by the Simons Center at Stony Brook, with the title Crossings. It tells the story of the center and of various people involved with it through a large number of interesting pieces written by these people. The book is published by the center, available here.
- Symmetry magazine has an article out today, with the title Can a theory ever die?. It's largely about supersymmetry, with "No" the answer to the title question. There's a story about Bruno Zumino I'd never heard before:

  > In 1996 theorist Jonathan Feng attended a seminar about searches for new particles predicted by the mathematically elegant theory of Supersymmetry. The speaker was optimistic that researchers would find the particles at massive colliders such as the Tevatron, then in operation at the US Department of Energy's Fermi National Accelerator Laboratory, or the Large Hadron Collider, then under construction at CERN.
  >
  > Feng noticed Bruno Zumino, one of the founders of Supersymmetry, in the audience. Zumino's reaction to the talk confused Feng.
  >
  > "He left the seminar shaking his head," says Feng, who is now a professor at the University of California, Irvine. "I thought he would be happy that an army of people were looking for his theory. So why was he shaking his head?"
  >
  > Feng caught up with the distinguished theorist during the coffee break. He still remembers what Zumino told him: "I never thought it would be this hard. If it's this hard, then they're never going to find it."

  So, twenty-six years ago one of the leaders of the field thought the idea was going nowhere and likely doomed. In the years after that LEP and the Tevatron put much stronger limits on SUSY before closing down in 2000 and 2011 respectively.
  From 2010 to the present day, the LHC has again put far stronger limits on SUSY particles. Most now agree it is overwhelmingly unlikely that the rest of the LHC or HL-LHC runs will change the situation. And, prospects for a higher energy collider are very uncertain and many decades away. You'd think that would be the end of it.

  But the article quotes theorists determined to keep at it (no quotes from anyone who thinks SUSY is over) and there's still an active community of people pursuing what Zumino thought was doomed multiple decades and accelerator generations ago. Large conferences continue to be scheduled, for example SUSY 22 this summer, which will be preceded by a pre-SUSY school designed to train a new generation to work on the failed ideas for many decades to come.

- I'm leaving soon to spend a couple days in Texas, giving a colloquium talk at the University of Texas at Dallas math department.

Update: The slides from the talk at UT Dallas are here.

Comments

1. **paddy**
   May 3, 2022

   "take care when you travel through [Texas] Germany with the truth under your coat!"

2. **anonymous**
   May 3, 2022

   I don't agree with that interpretation of Zumino's response. You may be attributing beliefs to him that he did not hold.

   He doesn't say that SUSY is "going nowhere" or "doomed," he says that we're "never" going to find direct empirical evidence for it at a particle collider. (Here I understand "never" in the colloquial sense of, "not in a reasonable amount of time.") This is not really surprising, since there's a lot of room between the LHC and the Planck scale, and no reason to believe superpartners should exist at energy scales accessible by 2000s-era technology (assuming they exist at all).

   You might object and claim that an idea is doomed/going nowhere if there's no hope of it being empirically verified or rejected in a "reasonable" amount of time (say roughly 100 years). But that's not an obviously true claim, and it's not clear that Zumino held it.

3. **Peter Woit**
   May 3, 2022

   anonymous,
   Sure, it's possible Zumino thought that SUSY extensions of the SM broken at inaccessibly high energy scales were a promising idea, but I don't see any good argument for that (as opposed to the electroweak scale, with the "naturalness" motivation).

   Feng and Tanedo in the Symmetry article give the impression that it no longer matters whether prospects for experimental vindication of an idea have vanished. I suspect Zumino was of a very different generation which did not think this way.

4. **F. Zaldivar**
   May 3, 2022

   Just a typo: Can a theory ever die?

5. **Peter Woit**
   May 3, 2022

   I did some quick research, which indicates that, at least publicly, Zumino was more optimistic about SUSY than Feng indicates. In a 2008 review article
   https://arxiv.org/abs/0805.3726
   with Mary Gaillard, they write

   "We are very optimistic that the MSSM, or some extension of it … will correctly describe the particle and superparticle spectrum that will be produced at LHC energies."

   and note that if there is no SUSY at LHC energies it could be at higher energies. If Zumino had lived, it's an interesting question how he would have reacted to beliefs he was “very optimistic” about being disconfirmed experimentally. Maybe he would have reverted to his 1996 pessimism.

6. **Peter Woit**
   May 3, 2022

   F. Zaldivar,
   Thanks. Fixed.

Brian Conrad on the California Mathematics Framework

May 5, 2022
Categories: Uncategorized

Brian Conrad has been doing the state of California a great service by taking a careful look at the drafts of the proposed California Mathematics Framework and the research they are supposedly based on. He has recently created a website where he has been writing up commentary on what he has found. Conrad is known among his colleagues as one of the most careful and level-headed research mathematicians around, and these characteristics show through in what I have read on his new site.

I should make it clear that personally I have zero expertise on the topic of K-12 math education, so my own views on the matter aren't worth much (and, I think anyone commenting on this should ask themselves about the same issue). Conrad has put a huge amount of his time and effort into learning about the subject and the extensive relevant math education research, and it is this that makes paying attention to the views of a university math professor a good idea in this case.

There's commentary about this appearing elsewhere, including a Wall Street Journal article, and a blog entry by my Columbia colleague Andrew Gelman.

Comments

1. **just different**
   May 7, 2022

   I haven't been following this closely and I certainly don't know whether the CMF is on balance a good idea, but my general impression is that the viewpoint of Conrad and other signatories of the "open letter" is fundamentally elitist. There seem to be a lot of predictions that CMF will delay or discourage the "better" students–the only students who matter, apparently–from pursuing STEM in college, but not a lot of concern about the students who have been failed for decades by the conventional mathematics sequence.

   It would be easier to take these critics seriously if they paid attention to the goals of the CMF and offered constructive alternatives instead of dismissing as inconsequential the abysmal track record of the status quo for the great majority of students.

2. **Brian Conrad**
   May 7, 2022

   @just different:

   Please clarify what "open letter" refers to. I never signed the one from the Independent Institute (despite being asked to do so multiple times, as were many other STEM faculty who did not sign it), nor did I sign the K-12 letter (though in that case I agree with what it says).

   The statement I actually did sign was the more recent https://sites.google.com/view/mathindatamatters/home . What is elitist about alerting parents and students to the reality of what college degrees in STEM fields require? Or about being concerned that the Introduction to Data Science course explicitly advocates to women and minorities that they forgo learning Algebra II in high school and that the CMF also advocates data science as an alternative to Algebra II without clearly indicating the downstream effects on access to 4-year STEM degrees, including in data science?

   Please explain where in what I have written on my website or in the more recent statement that I actually did sign you see something that is "fundamentally elitist". And why do you think my views on this are only focused on the "better" students, or that I ever thought they are the only students who matter? Did you know that within my own institution I was involved in creating a summer bridge program

   https://mathematics.stanford.edu/soar-mathematics-program

   to help students from under-resourced schools to be better-prepared on the core high school math needed for intro-level courses across all STEM fields? (Stanford has some incoming students who struggle with fractions and basic algebra facts too.) And that I spear-headed two curricular overhauls of core service-level math courses in my institution (involving input from experts across many STEM fields — in contrast with the CMF process that did not solicit input from real experts in data science without a financial stake) to modernize motivation in the learning of core math topics without sacrificing the substantive content? Where have you seen me "dismiss as inconsequential the abysmal track record of the status quo for the great majority of students"? (I never have, and I am perfectly well-aware of that issue; to express concern about one facet of a multi-part problem doesn't necessitate fixing everything at once.) How do you know I haven't offered some constructive alternatives? (I have offered constructive suggestions on improving the CMF in past months to people involved in the process; I simply don't post about that on social media.)

   And when a document is produced that has many vague half-baked proposals and falsifications of evidence (even if it has other parts which are fine), how much of a burden should there be on those who object to tell the people responsible how to fix the mess? The fact that a document claims to have certain goals doesn't mean it actually contains a realistic or sustainable approach toward those goals (one reason I read the entire CMF was so that I would not be an armchair critic).

   You insinuate that I have not paid attention to the goals of the CMF, yet right here on the desk as I type this is a print-out of the entire thing full of comments in the margins from reading the whole thing. I have not internalized the CMF to the level I did with EGA (for multiple reasons), but I'm well aware of what the CMF claims its goals to be and how it falls very short. Please state clearly what you think the CMF's goals are at the high school level (which is the focus of what I have written on this matter) and where the evidence is that the CMF provides a realistic path toward such goals.

   SFUSD implemented a version of the CMF's earlier proposals at the high school level, and it was later exposed as such a fiasco that all mention of SFUSD has been scrubbed from the 2nd draft. The data science advocacy now in the CMF has been done for years in LAUSD, and has been described to me as a "disaster" in terms of how it has off-ramped kids at the most under-resourced LA schools (those who rely fully on public education, without access to tutors or other work-arounds) from learning the math they need to be prepared for a 4-year STEM degrees in college.

   To be clear: I have never said that all students must learn Algebra II (the UC & CSU systems have chosen to have a single math prerequisite across all fields, so please criticize that if you wish — it is orthogonal to the reality of the math one has to learn to earn a data science degree at those places), nor have I ever said that the public math education system doesn't need significant improvements.

   My point is about the necessity for transparency about downstream consequences of public policy guidance on the content of math options in the high schools, and the unacceptability of the CMF peddling fantasies about what math in high school is actually preparatory for 4-year college STEM degrees.

   I read the entire CMF (both drafts) to fully inform myself on what the CMF does and does not actually do (lofty goals are easy — I look for details), and have spent many hours speaking with the leadership of the National Society of Black Engineers (which shares the same concerns as I do), leaders of the DEI efforts in the Berkeley School of Engineering (who share the same concerns), people with track records of real success in URM outreach such as Adrian Mims (founder of the Calculus Project; he also shares the same concerns) and college faculty at many other institutions many levels who have put in years of effort on improving access to math classes, and many other thoughtful people with years of experience on these matters.

You say you "haven't been following this closely" (a contrast with me, to put it mildly) and end with a paragraph that lumps me in with "these critics" (whomever they are) and suggests my concerns don't need to be taken seriously because of two reasons that don't apply to me at all. Please clarify on what basis should your comments be taken seriously.

3. **just different**
   May 7, 2022

   @Brian Conrad

   Here's the thing: the past two years have made it blindingly obvious that a very large fraction of Americans are too innumerate to respond appropriately to a global pandemic or correctly interpret election results at a basic level. This bothers me a lot more than the possibility that Stanford freshmen will be less than optimally prepared for STEM majors.

   The failure of K-12 mathematics instruction is particularly acute in California, and racial and gender disparities have been static for decades. Some people have identified algebra as the bottleneck where so many American students stall, and most of the ones who pass don't seem to be acquiring any real-world understanding anyway. So on the face of it, it is not unreasonable to ask whether some other curriculum might better serve the educational goal of creating a functionally numerate and mathematically prepared citizenry. I have no idea whether the CMF does a better job of this, but this line of discussion has been shunted to the margins.

   Instead, the CMF debate has been largely hijacked by right-wing culture warriors, who as always are arguing in bad faith: their true dispute is with the underlying premise that there are persistent disparities which we have an obligation to recognize and address. So no, I don't think criticism about "woke math" or whatever deserves to be taken seriously. The more substantive objection that delaying or replacing Algebra has knock-on effects for potential STEM majors is fair enough (if a bit privileged), but this seems to me to be a much more academic question that can be resolved without the overheated rhetoric and multiple editorials and open letters.

   As I said above, I am only a casual observer with general impressions obtained from nonspecialist media, and I am not directing my remarks at you (or anyone) personally. But since your name gets dropped by people saying some pretty repugnant things, it might make sense to make the sound-bite version of your own position clearer.

4. **More Anonymous**
   May 8, 2022

   @Brian Conrad

   It's nice of you to address this personally. I suspect what's happening is in society is people are unfortunately looking at everything from a particular lens. And now anyone who wants to do anything his/her actions will be used as a tool for some political ideologue and to score some points. It's tragic from the Information Age we have regressed to what I dub as the "Data Age."

5. **Mark Hillery**
   May 8, 2022

   I have read parts of the California Mathematics Framework, and I admire the patience of anyone who was able to read the whole thing. It is very hard to read because of all the verbiage that conveys no information. While I don't teach in California, what starts there often spreads to the rest of the country, and people like Boaz Barak, Jelani Nelson, and especially Brian Conrad have done us all a huge favor in pointing out its flaws. There are already problems with the mathematical preparation of college students, and the current CMF, if implemented, would make them worse. If someone hasn't looked at the document, I would suggest their opinion is not worth considering. This is doubly so if they are not aware of what happened when parts of the CMF were implemented (as noted by Brian Conrad) in the SFUSD. Check out Scott Aaronson's blog for comments by some San Francisco math teachers.

   It was interesting to hear that there are incoming Stanford students who have problems with fractions (What is it about fractions? They really cause a lot of students trouble.) and basic algebra. At Hunter College, where I teach physics, we have, I am sure, far more. I have often taught our algebra-based introductory physics courses, and a large portion of the class struggles with the math. If you are trying to learn some physics and fighting with the math as well, the course becomes twice as hard. The last thing we need is something that would make the mathematics preparation of college students worse, and I am afraid the CMF would do just that.

   I am no expert on high school education; I just know what I would like the students who take our courses to be able to do. Why so many cannot, I don't know, but I expect under-resourced elementary and high schools have a lot to do with it. At Hunter we know about under-resourced schools. See, for example, the recent article in New York Magazine entitled "Hunter College is Falling Down", and I was once in a classroom at Hunter when a rat fell from the ceiling. Our students would benefit from more individual attention, but we don't have the resources to give it to them.

   While resources matter, so does curriculum. In the 1990's we had a contingent of students who were recent immigrants from what had been the Soviet Union. I don't know anything about Soviet schools or their resources, but the facility of these students with high school mathematics was greater than that of our native born students (this is an impression, I never did a systematic study). Is there something to be learned here?

6. **Scott Aaronson**
   May 8, 2022

   just different: As someone who was also involved in the campaign to defend serious math education in California, let me give you my personal perspective. I care *enormously* about instilling broad mathematical and statistical literacy in the population. Societally, that's an even bigger issue than training the next generation of mathematicians and scientists, which of course I care about also. But crucially, *anyone who has sneering contempt for the top STEM students and their needs, is not someone who I trust to design a curriculum for the average students either.*

   Have you seen the emails that were disclosed just yesterday, where Jo Boaler (the mastermind of the CMF) is asking someone whether the concept of inequalities (yes, like $x+2y\leq 5$) is actually needed for college-level data science, or whether, presumably, that topic can be omitted? Is this someone who *you* trust to be in charge? If so, why?

   Compare the many people a few generations ago who said: "It's elitist to care about a few persecuted geniuses like Sakharov and Solzhenitsyn. The Soviet Union is trying to improve conditions for the great mass of people!" The fallacy, we now understand, is that any government that would persecute Sakharov and Solzhenitsyn, that would regard them as existential threats, is not a government that you want in charge of the great mass of people either.

7. **Brian Conrad**
   May 8, 2022

   @just different:

   Let me add some points not already in the comments of Mark Hillery and Scott Aaronson. Firstly, due to this communication being in writing rather than in person (so tone of voice is missing), I can't quite tell if you're being sarcastic in seeming to say that I don't recognize innumeracy among the wider population is a more important concern than the STEM-preparedness of students admitted to places like Stanford. I considered pointing out in my original response that I am aware of such type of things (with the example of what I did to assist incoming freshmen here just being an illustration), but I didn't since it seemed clear enough not to need such comment. So I hope you were being sarcastic.

There are two completely different topics (among many others): (i) math education for general citizenship (to give it a name), and (ii) math education for STEM careers. You are focusing on the former, and I and many at universities in industry have been focused on the latter. I wouldn't criticize someone for preferring to focus their concern on (i), and for the same reason I am mystified by your apparent objection that others choose to focus attention on (ii) (or some other aspect of this multi-faceted puzzle, such as teacher training, the grip of publishing companies with dreadfully boring material, etc.). If the goal is to make an impact, rather than just talk about it, we have to choose our focus where we think it can actually make a difference.

The two issues (i) and (ii) interact insofar as students' interests can evolve during middle and high school, and even college (many kids who develop interest in CS in college are surprises to realize they should have paid more attention during math class in earlier years). STEM degrees and college-level STEM coursework are an important factor of the social mobility that college makes possible (it's not the majority of degrees, but it is a sizable fraction and will keep growing as the workforce becomes more automated), so referring to concern about preparedness for such degrees as "a bit privileged" sounds like objecting to government funding of scientific research on the grounds that poverty is still a big problem.

On your concern (i), let me just say that the CMF does not have anything genuinely new to contribute towards it, beyond the hype of renaming things related to statistical topics as "data" or "data science" (for reasons I leave to your imagination). That is why you don't hear anything in the media about the CMF's K-6 components (the parts related to basic numeracy skills); to first-order approximation it is not changing things in any significant way. One can quibble locally, and more globally object to the verbose writing style and poor organizational structure that permeates the document (in contrast with CMF's of the past), but this is not fodder for news media and I am certain that any "public comments" I post about that will be met with a big yawn.

There are many nuances to these matters, so a position fitting into a sound-bite is inevitably going to be too superficial to be meaningful. I'm not aware of repugnant things people are saying while dropping my name (it reminds me why I am not on social media; ignorance is bliss), but it would be a fool's errand to imagine that when speaking out on something then some people will twist one's words out of context to justify things with which one disagrees. Also, I have seen many examples (going beyond my post on misrepresentation of citations within the CMF) of people on the "left" side of this topic misrepresenting written things in deeply deceptive ways too.

But what I'd really like to address is the impression you have of the CMF issue being largely high-jacked by right-wing culture warriors. You are correct that nearly all media attention comes from the right (to the extent the "left" media says things, they have usually ignored the concerns from STEM experts), but you are misdiagnosing it. This is not a hijacking. It is more like leaving one's car unlocked with the engine running and and being surprised that someone drives off with the car.

The story of coverage of the CMF concerns is an illustration of the huge difficulties confronting traditional journalism. The almost total failure by national-level non-right-wing media to pay attention to concerns about the CMF at the high school level is not for lack of trying by those who are eager to share a lot of information with their fellow citizens. When the K-12 letter came out, first the WSJ wrote something and then only media further and further to the right (Daily Caller, Epoch Times, Russia Today,…) wrote anything. It was astounding.

Why did NYT, Washington Post, LA Times, etc. say nothing at the time in December? Why does the LA Times publish CMF propaganda pieces timed with the release of the new version but never report on the reality that actual STEM experts in industry and academia are pretty much uniformly alarmed by the CMF's high school proposals (I am not referring much to acceleration aspects, which have been toned down for the 2nd draft)? Why does the NYT not publish an Op-Ed written by two deans at UC Berkeley cogently explaining how the CMF will be damaging to efforts at STEM career access and pipeline diversification? Why do education writers who are provided with compelling information laying out the basis for concerns with the CMF not follow up?

One explanation I heard from someone who spoke with a NYT reporter is that the NYT thought the concerns about the CMF at the high school level is just a local story to California. If so then they have made a huge mistake, and not only for the reason Scott Aaronson points out ("as goes California, so goes the nation"). The "disease" of promoting data science in place of other math in the high schools with false promises and very inadequate guidance about downstream effects on preparedness for a wide range of college degrees is already spreading around the country, and is popping up in other countries such as Australia and Germany (and perhaps others).

Someone else told me that this is an expensive story to write (to track down the necessary evidence and organize it). It may seem so, but that isn't really the case because through the individual actions of many concerned private citizens acting on their own since last summer there is a treasure trove of information already assembled (the recent posting of emails that Scott mentioned is but one such example; that one had nothing to do with me).

I have spoken with a couple of journalists for some non-national venues (and will be speaking with another), and it has led me to the conclusion that the root of the problem is that the story of CMF concerns is very difficult for journalists to write because it involves how the content of high school math connects to other things. That is doubly challenging: most education journalists don't have a math background, and explaining the concerns to a general audience in an engaging way is difficult too (though the emails may provide a hook that finally draws in some journalists not focused on culture wars).
The falsehoods being spread about data science in relation to math for 21st-century jobs are a kind of truthiness: they sound very plausible to people who are not well-educated enough in technical matters to realize it's nonsense. (I heard of a prominent expert on quantum computing saying that if he were an ordinary educated person but not in matters related to STEM then he'd believe the propaganda too.) That can make it hard to convince many journalists away from culture wars that there's something objectionable enough to be newsworthy. So if you're looking for reporting from a source that is not focused on culture war topics then you're not going to find many options.

The fact that math-types are often reluctant to speak up too visibly without being fully informed and the CMF is a nightmare of poor writing (making it hard to reach a state of feeling fully informed about it) is further discouragement. The pro-CMF side also proclaims themselves (falsely) to the champions of equity, which scares many people into not speaking up, due to fear of being "cancelled".

Here is an ironic aspect you may wish to consider. The way your initial comment expressed a dismissive view of the STEM-related content-based concerns I and others have spelled out in detail is exactly the type of reaction that many people with such concerns and a reluctance to speak up (such as due to not having a full command of the details and nuances) are concerned with having to confront. I have no such concerns because (i) I am not on social media, and (ii) I know enough about it to be self-confident. So when you next wonder why you only see reporting on CMF concerns from sources which don't care what people in the center and left think, in a sense your way of discussing it represents part of the problem. Dishonesty spread from the left on these matters (I am not referring to you, just to be clear) — which is now starting to become more widely known (e.g., I'm told that 70,000 people have now seen those email posts on Twitter since they appeared around 18 hours ago; maybe it'll reach Hagoromo-level virality) — doesn't help things either. To paraphrase TFG: there are very dishonest people on both sides.

I suspect that if we were to discuss these matters in a cafe and thereby had an opportunity for genuine back-and-forth based on nuance and details then we'd probably agree on most things related to this matter apart from perhaps where to focus personal attention on it. And I think you'd be astounded at the layers of complexity in this CMF topic (that was my own experience as I spoke with people coming to it from many different angles). The analogy I made somewhere on my "public comments" website to an iceberg seen from the surface is more accurate than you realize.

You should take some inspiration from the efforts by Bob Moses, a math teacher turned civil rights activist in the 1960's who recognized in the mid-1980's that learning algebra is the key to economic mobility for those from less-resourced backgrounds. (Read his book Radical Equations for more on this if you haven't read it already.) Had he not passed away last summer, I'm sure he would have been alarmed at what the CMF is proposing in opposition to what he pushed for in the last 35 years of his life. There are others continuing Moses' legacy, showing the way to continue broaden engagement with algebra and other math that leads to actual STEM readiness, not the content-changing approach that is preferred in the CMF. I am optimistic that at the high school level, a responsible balance between access to math for real STEM-readiness and other options in high school math will be achieved. Failure is not an option.

8. **Peter Woit**
May 8, 2022

All,
I'm reverting to usual blog policy and deleting all further comments that try to engage in the culture wars. Comments not specifically about what is in the CMF are highly discouraged, and it may get to the point where I'll have to insist that commenters prove they have read the thing (although then I suspect we'd just be left with Brian Conrad debating himself…).

9. **comment**
May 8, 2022

Politics and education are of course not cleanly separable, but it's dismaying to see many (on 'both sides') seeming to not even try to distinguish (1) their political/cultural beliefs (2) their expert knowledge (3) their understanding of education research. Although I strongly agree with the majority of what 'just different' said above, I think they are a bit off in their targeting. It is very good and clarifying for me to see Brian Conrad explicitly say above that he is focused on "math education for STEM careers". Similarly the "Math in Data Science" open letter (unlike some others) is very good for its focus and its avoidance of surrounding political debates. What I have read from BC's new website is (unlike many other similarly-minded websites/blogs) likewise very good. As someone with deep sympathies and frustrations with 'both sides' of the underlying debate, I find it very useful and clarifying to have material explicitly presented with such clear framing and purpose. (It probably also makes it a little harder for the anti-diversity extremists to hijack the cause.)

Two Titans

May 10, 2022
Categories: Uncategorized

If one tried to pick a single most talented and influential figure of the past 100 years in each of the fields of pure mathematics and of theoretical physics, I'd argue that you should pick Alexander Grothendieck in pure math and Edward Witten in theoretical physics. This afternoon I've run across two excellent sources of information about each of them.

Alexander Grothendieck

This week's New Yorker has The Mysterious Disappearance of a Revolutionary Mathematician by Rivka Galchen. It's a very well-done survey of Grothendieck's life and work, aimed at a popular audience. If, like many mathematicians, you've always been fascinated by Grothendieck's story, you won't find too much in the article you haven't seen before. But, if you've never delved into this story, you should read the article. On a related note, a copy of Récoltes et semailles that I ordered recently has just arrived in the mail, and I'm looking forward to spending some time with that this summer.

Edward Witten

In theoretical physics a very different but equally off-the-scale talented and influential figure is Edward Witten, who is the subject of a recent long and in-depth interview by David Zierler as part of the Oral Histories program at the Niels Bohr Library and Archives.

I first met Witten probably in 1977, when I was an undergraduate at Harvard and he was a Junior Fellow, recently arrived from Princeton. Over the years since then he has done a mind-blowing quantity of highly impressive work which I've done my best to try to follow. You can find many places where I've written about this here on the blog, and there's also a lot in my book Not Even Wrong. Much of what he discussed in the interview was familiar to me, but I learned quite a bit new from his recollection of the details of how his work came about and how he thought about it. On some of the specifics of what happened many decades ago one should keep in mind that memory is imperfect. For instance, he describes a short period as a graduate student in economics at the University of Michigan, which surprised me since in research for my book I'd read that this was at the University of Wisconsin. Maybe I got this wrong, but if so I'm not the only one (see for instance here).

Witten's work in the area where pure mathematics and quantum field theory overlap has had an overwhelming influence on those like myself who are fascinated by both subjects and their interaction. The landscape of this area would be completely different (and highly impoverished) without him. At the same time, his equally large influence in the area of attempts to unify physics I believe has been much more problematic.

I'll quote here with a little commentary some of the passages from the interview that I found striking or where I learned something new.

About his early years:

> Witten:
> I was very interested in astronomy when I was growing up. Well, I was not an exception; these were the days of the Space Race, so everybody was interested in astronomy. I was given a small telescope when I was about nine or ten. That's certainly a vivid memory. Another vivid memory is learning calculus when I was eleven. My father sort of taught me calculus or gave me materials from which I could learn it. But I didn't advance very much in math beyond that for quite a few years…
>
> Zierler:
> And then initially you thought you would go and become an economist?
>
> Witten:
> Yes.
>
> Zierler:
> What were your interests there? Did you think that your mathematical abilities would be applied well in that field?
>
> Witten:
> It's again hard to remember reliably, but I might have thought that. And I might have also thought that I could make a contribution to international development. But I realized- well, I came to the same realization I had come to when I was working on the McGovern campaign, that it wasn't a good match for me. I remember being very embarrassed when I told the people in the department at Michigan who had been quite kind to me, that I had decided to leave. But in hindsight, I understand something that wasn't that clear to me at the time, that if a given graduate program isn't a good match for a given student, the department and the student are both better off if that's realized sooner rather than later. If I had understood that at the time, I would have been less embarrassed, probably, with what I told them.

How to learn general relativity in ten days:

> Zierler:
> Was general relativity considered popular or interesting at Princeton at the time that you were a graduate student?
>
> Witten:
> Well, I was certainly interested in it. I learned general relativity in a very exciting period of about ten days, from the book of Steve Weinberg. I mean, I tried to learn more from the book of Misner, Thorne, and Wheeler, and I did learn more from it, but my opinion of the book was what it remains now, which is that it's got a lot of great stuff in it, but it's a little bit hard to use it to learn systematically. The book I found useful for studying systematically was by Steve Weinberg.

The Harvard Society of Fellows:

> Zierler:
> Ed, did you enjoy the Harvard Society of Fellows, the social aspects of it?
>
> Witten:
> Well, I enjoyed it up to a point, but let's just say that many other people thrive on that more than I did.

On how he experienced the First Superstring Revolution.

> Witten:
> I'm not exactly sure what I would have said if you had asked me. There's no interview, so there's no record of my thinking in 1982 or 1983, and I won't be able to remember very well. But as I was telling you, I was interested enough to spend a whole summer reading John Schwarz's review article, but a little bit wary of becoming too involved in it…

> Something that was obvious to me but wasn't immediately completely obvious to everybody was this. Green and Schwarz had put string theory in the form where there was a very strong case that there was a consistent quantum theory that described gravity together with other forces. And the other forces could be gauge fields, somewhat like in the Standard Model. But there was something extremely conspicuous that was wrong in terms of phenomenology, and that was that the weak interactions couldn't violate parity…
>
> And as it existed in 1982 and 1983, string theory was a consistent theory of gravity unified with other forces, but it completely missed the chiral structure. So, to me, that was a huge siren blaring. Anyway, to set the stage, I want to just point out to you that it was clear by 1982 or 1983 that there were an incredible variety of delicate things that fit together perfectly to make it possible to have a theory of quantum gravity based on string theory. It was unbelievable that it could all be a coincidence. Yet it was markedly wrong for describing the real world because of this question of the chiral nature of the fermions. But then in 1984, Green and Schwarz discovered a more general method of anomaly cancellation, and everything changed…
>
> So, anyway, what was really problematical for Green and Schwarz was the combination of fermion chirality and anomalies. Taking these together, it seemed that string theory could not work. But then, in August 1984, Green and Schwarz discovered a new mechanism for anomaly cancellation, and everything changed…
>
> So, it was immediately obvious to me, once they made their discovery, that you could make at least semi-realistic models of particle physics, in that framework. But also, to me, I had done kind of an experiment in the following sense. I had spent two years watching this, wondering, could it be? Can it be that all the coincidences that had been discovered that made string theory possible were just coincidences? As far as I was concerned, the discovery they made in 1984 was an empirical answer of "no" to that question. If the miraculous-looking things that had been discovered up to 1982 and 1983 were truly coincidences, you'd then predict there wouldn't be any more such coincidences. That had proved to be wrong when they made this miraculous-looking discovery about anomalies that enabled the theory to be much more realistic.
>
> In explaining this to you, I'm trying to help you understand why this had so much of an impact on my thinking, watching from the outside for a couple years, wondering if this subject was as amazing as it appeared to me. And a "no" answer would have predicted there shouldn't be more miraculous discoveries. And that was, to my satisfaction, disproved in August 1984. So, after that, the hesitation that had kept me from becoming more heavily involved earlier evaporated. Now, I realized that in the physics world, there were plenty of people who hadn't lived through this two years of uncertainty that I had lived through, and in many cases they had never heard of the whole thing until August 1984. And they hadn't done the experiment I had done. So, they didn't react as I did.

Here Witten explains how one very specific technical calculation triggered for him a dramatic vision of a possibility of a unified theory of everything, a vision that has stayed with him to this day, nearly four decades later.

About his evangelism for string theory unification starting in 1984:

> Zierler:
> How much cheerleading did you do among your colleagues, both near and far, after this revolution in 1984, that this is what people should concentrate on? That we can have this figured out in the near term?
>
> Witten:
> I wasn't intentionally cheerleading, but I was very enthusiastic. And I actually think I was right to be enthusiastic. I wasn't intentionally cheerleading, but to the extent that I encouraged other people to get involved, I've got no regrets about it at all (laughter).

Another very interesting recent interview in the same series is one with Cumrun Vafa. Here's what Vafa remembers about that time:

> Vafa:
> I remember I was at my office, I had come back from a trip, from I think the summer school in Europe, in Italy. Had come back to my office in Princeton on the fourth floor, and Ed's office is on the third floor. And he rarely came to our floor, fourth floor, but here he was, coming and knocking at my door, and then saying, "Have you heard about the revolution?"…
>
> I said, "What revolution?" He said, "The SO(32) revolution." Okay, that was my first introduction to Green and Schwarz's work. SO(32) revolution. I said, "No, what is it?" He said, and he was completely sure, confident, that physics is not going to be the same after this. He said, "Physics is going to change forever because of this, and now everybody is going to work on this."

I had left Princeton for Stony Brook early that summer. During the next few years, reports I got from fellow postdocs who tried to talk to Witten about their work were pretty uniformly something like "he told me that what I'm doing is all well and good, but that I really should be working on string theory."

Unlike the case in the interview with Vafa previously mentioned, Zierler doesn't really try and pin Witten down on the subject of the problems of string theory. He does ask:

> Zierler:
>
> What was happening at the time or has happened since in the world of experimentation or observation that may get us closer to string theory being testable?

but lets Witten give a non-answer, which in effect is that the landscape means string theory unification is completely untestable, so he has pretty much given up:

> So, if you talked to me in the 1980s, I'm sure I would have expressed some hope about seeing supersymmetry as part of the answer of the hierarchy problem. But I would have expressed a lot of confidence about observing something that would have explained the hierarchy problem. …
>
> But ultimately, with the LHC, experiment has reached the point that it's extremely problematic to have what's called a natural explanation of the weak scale, a mechanism that would explain in a technically natural way why the Higgs particle is as light as it is, thus making all the particles light. It's actually a baby version of the problem with the cosmological constant. So, to the extent that the multiverse is a conceivable interpretation of why the universe accelerates so slowly, it's also a conceivable explanation of why the weak scale is so small. It might be the right interpretation. But if it is, it's not very encouraging for understanding the universe. When the multiverse idea became popular around 1999, 2000, and so on, I was actually extremely upset, because of the feeling that it would make the universe harder to understand. I eventually made my peace with it, accepting the fact that the universe wasn't created for our convenience.

You would think that having an untestable theory on your hands would mean that you would try something else, anything else, but Witten seems convinced that whatever its problems, it's the only way forward:

> It's satisfying to know that there was only one candidate for superunification. There's only one reasonable candidate now for the theory that combines gravity and quantum mechanics. Before 1995, there was more than one. It's more satisfying to know that the theory seems to have a lot of possible manifestations, in terms of approximate vacuum states, but at a fundamental level, there's only one fundamental theory or system of equations, that we admittedly don't understand very well. That's got to be an advance of some kind…
>
> By the time he had the theory , he had the right mathematical framework of Riemannian geometry. At least by the time the theory was invented, he had the ideas it was based on, and some of them he had had before.
>
> String theory and M-theory have always been different. From the beginning, they were discovered by people who discovered formulas or bits and pieces of the

> theory without understanding what's behind it at a more fundamental level. And what we understand now, even today, is extremely fragmentary, and I'm sure very superficial compared to what the real theory is. That's the problem with the claim that supposedly I invented M-theory. It would make at least as much sense to say that M-theory hasn't been invented yet. And you could also claim it had been invented before by other people. Either of those two claims is defensible (laughter). So I made some incremental advances in a subject that's far from being properly understood.

This "we don't know what the fundamental equations are, but we know that they are unique" argument has never made any sense to me.

On his relation to mathematics:

> Zierler:
> What did it feel like to win the Fields Medal as a physicist?
>
> Witten:
> Well, it was a thrill, of course. It felt a little funny because I knew that obviously I was a non-standard selection. And I don't like controversy about science, and I felt that I might have been a controversial choice in the math world. But on the other hand, I hadn't selected myself, so I didn't feel any controversy was my fault…
>
> What's a little funny about my relation to the math world is that although some of my papers are of mathematical interest, they rarely have the detail of math papers. And I can't provide that detail. I simply don't have the right background. What I bring to the subject is an ability to understand what quantum field theory or string theory have to say about a math question. But quantum field theory and string theory are not in the precise mathematical form where such statements can usually be rigorous.

The "I don't like controversy about science" quote makes clear that Witten and I are temperamentally very different…

About the birth of geometric Langlands:

> By the late 1980s- I'm probably forgetting bits of the story, I should tell you- but by the late 1980s, Sasha Beilinson and Vladimir Drinfeld had discovered what they called a geometric version of the Langlands program, and it involved ingredients of quantum field theory. Tantalizing. But it was tantalizing because they were using familiar ingredients of quantum field theory in a very unfamiliar way. It looked to me as if somebody had put the pieces at random on a chess board. The pieces were familiar, but the position didn't look like it could happen in a real chess game. It just looked crazy. But anyway, it was clear it had to mean something in terms of physics. I even worked on that for a while at the time.
>
> I think I've gotten this slightly out of order. I think when I worked on it was actually before the work of Beilinson and Drinfeld, driven by other clues. And the Beilinson and Drinfeld work was one of the things that made me stop, because I realized that A, I couldn't understand what they were doing at the time, and B, there were too many things I didn't know that they knew, and that seemed to be part of the story. Anyway, as you can see, my memories from whatever happened in the late 1980s are pretty scrambled.
>
> They wrote a famous paper that was never finished and never published. It's 500 pages long. You can find it online, if you like. They have an incredibly generous acknowledgement of what they supposedly learned from me, which is way exaggerated. Based on a hunch, I told them about a paper of Nigel Hitchin, but I didn't understand anything of what they attributed to me. At any rate, regardless, even if I didn't understand what they did with it, the fact that I was able to point them to the right paper was another sign of the fact that what they were doing had something to do with the physics I knew. But I couldn't make sense of the connection. And this kept nagging at me off and on for a long time.

He then goes on to tell the story of the IAS workshop on geometric Langlands and how it led to his work on a QFT version of geometric Langlands.

In recent years Witten has continued to work on geometric Langlands and other topological quantum field theory related topics at the mathematics end of things. As far as physics goes, he is following the very popular "it from qubit", quantum gravity from information theory, line of thinking:

> Witten:
> And the third time has been the last six or seven years. It's actually hard to remember the evolution of my thinking (laughter). I reread an interview I had done in 2014 which told me what my thinking was in 2014 better than I could have remembered it reliably (laughter). And what I told the interviewer at that time was somewhat similar to what I'm telling you right now. So, this has gone on for a while, and despite that, I haven't really found the right way to become involved myself. But I do suspect that something big is happening.
>
> Zierler:
> What has happened since 2014, when you initially got excited about this?
>
> Witten:
> There have been various striking developments, but a particularly dramatic one came in 2019 when there was success in understanding what is known as the Page curve in black hole evaporation… Lots of things have happened that show that there's a conspiracy between gravity and quantum mechanics. Somehow gravity at the classical level knows about quantum mechanics and statistical mechanics…
>
> Zierler:
> To bring the conversation right up to the present, as we discussed right at the beginning, your interest in quantum information. And you said you don't yet know how you might break into the field. What might be some possible avenues?
>
> Witten:
> Well, when I was a graduate student, I sat down one day with piles of paper preprints. We didn't have the archive. I'd sit down with piles and piles of paper preprints, and go through them, trying to find something I might do. The most interesting calculation I did as a student- I told you about it- was this calculation of deep inelastic photon-photon scattering, which was inspired by a paper I saw by Roger Kingsley, who studied the question but not quite with the most modern QCD ideas. So, when I was a graduate student trying to break in, I would go through piles of preprints. I guess the equivalent now is to look at papers in the archive and try to see what I might do. And I have made some minor contributions, actually, but I don't feel like I've fully become engaged with the subject, as I have with other subjects in the past.

Witten and the interviewer discuss the difficulty of finding something to work on that is not too hard but still significant, and he comments that this is:

> …the difficulty I've had getting involved with quantum information theory and gravity. I found a few things that I could do, but they were a little bit too narrow to really make me think that I was getting involved where I wanted to. And I haven't quite found the right avenue. But I haven't given up (laughter). I do have the feeling that's the direction where something big is most likely to happen. You see, there isn't a general understanding of what string/M-theory mean. And there's something missing in the general understanding of quantum gravity. The biggest hope would be that those two would somehow make contact with each other.

I can understand why Witten hopes that the mystery of quantum information theory and gravity will give insight into and resolve the mystery of what M-theory is, finally vindicating his 1984 vision, but this looks to me like a very, very long shot.

Comments

1. **Topologist Guy**
   May 11, 2022

   A couple points on the Grothendieck piece. The author attributes the introduction of categories and functors to Grothendieck. In fact categorical machinery was first used by Eilenberg and Maclane in 1945, roughly twenty years before Grothendieck's seminal work on the Weil conjectures. Grothendieck though did apply functorial machinery to algebraic geometry in a transformative way.

   The authors also note that Grothendieck developed the theory of motives as part of his solution to the Weil conjectures. I do not believe this is true. Nor did Grothendieck originate sheaves (which are due to Leray), but he did apply sheaves to algebraic geometry in a transformative way, through his "schemes." His seminal theoretical contribution that enabled the proofs of three of four of the conjectures was his etale (l-adic) cohomology theory. I do wish the authors had explained something of the search for this "Weil cohomology theory," and Grothendieck's brilliant insight, that homological algebra could be done on linear objects varying over any site, not just over the open subsets of a topological space.

   I've been thinking about writing a popular article along these lines (explaining about the Weil conjectures and etale cohomology).

   Overall, though, a decent biopic on Grothendieck.

   Incidentally, for readers interested in the life of Grothendieck, here's another brilliant piece to see: https://theanarchistlibrary.org/library/konstantinos-foutzopoulos-the-man-of-the-circular-ruins?v=1620246019

   Personally I prefer this to the New Yorker piece.

2. **Amadou**
   May 11, 2022

   From the little I know about Grothendieck's vision of motives, his Standard conjectures on algebraic cycles, which are inspired by Weil's proof of his conjectures for curves, would be part of the package, and thus would yield, "immediately", a motivic proof different from Deligne's.

3. **stoopid**
   May 11, 2022

   "If one tried to pick a single most talented and influential figure of the past 100 years in each of the fields of pure mathematics and of theoretical physics, I'd argue that you should pick Alexander Grothendieck in pure math and Edward Witten in theoretical physics."

   Really? Yang? Anderson? Schwinger? Bardeen? Weinberg? Nambu? Etc.

   I think the list extends to many tens if not hundreds.

   What "physics" has Witten actually done? He's been very influential in mathematics.

4. **Shantanu**
   May 11, 2022

   Peter : This is one of the rare interview by a particle theorist where non-0 neutrino mass is mentioned. His exact quote:
   "And the second are the neutrino masses, which are plausibly a signal coming from something close to grand unification, though unfortunately we don't know for sure that that's the correct interpretation."
   I wish the interviewee pinned him more on this that if non-0 neutrino mass is supposedly a sign if physics beyond standard model, why isn't Witten and others working more feverishly on it. Does anyone know if Witten has published papers with his father (on aspects of classical GR?)

5. **Peter Woit**
   May 11, 2022

   stoopid,
   Yes, really. Note that I specified "most talented and influential", not biggest accomplishments. On "talented" I don't think there's any argument, and on "influential", I also don't think there's anyone who has come close to his influence.

   In the posting there's a lot about what happened in 1984 and years afterwards. Witten took a technical argument about anomaly cancellation and used it to convince almost everyone in the field to follow him down a very speculative path. Decades afterwards, when it has long been clear this doesn't work, there are still thousands of people working on it, and here at Columbia we teach it to undergraduates. I don't think there's anything even close in the history of physics to that scale of (negative) influence. At the same time of course one can point to a very large number of extremely important and influential positive developments that Witten is responsible for.

6. **Peter Woit**
   May 11, 2022

   Shantanu,
   I may be wrong, but as far as I know Witten has not published papers with his father.

   One remarkable thing about Witten is that he is extremely knowledgeable about the state of high energy experiment and about HEP phenomenology (some of this shows through at points in the interview), far more so than most theorists. He has worked in phenomenology, but that has not been a big focus of his efforts. One could argue that, post SM, no one has been able to make a major breakthrough in HEP (beyond SM) phenomenology. So his concentration on formal theory has been sensible not just because his talents may be stronger in that area, but because the lack of experimental clues has stymied any progress in BSM physics.

   The reference to neutrino masses was part of some half-hearted remarks about GUTS, which he still holds out some hope for, while realizing that no significant evidence for them has appeared.

7. **Alex**
   May 11, 2022

   I was going to post something like what @stoopid said, but I see now that Peter already gave an answer. Still, it makes me cringe a bit to see Grothendieck to be put on the same foot as Witten, in their respective fields. Sure, in terms of influence and talent, maybe, but, to me, Grothendieck's legacy is far superior. Witten will gradually be cornered by history to the areas of mathematics on which he has been influential, as well as some isolated important results in theoretical physics (say, his proof of the positive mass theorem in GR.)

Most of his huge talent was wasted due to a lack of self-awareness (to put it gently and not to talk about lack of scrupulous or good faith…) regarding the über speculative nature of the field, a bit of historical bad luck (zero new experimental results for bSM physics in his lifetime), and maybe some other reasons. I think Grothendieck had better luck in that. Still, Witten managed to make important contributions in other areas, due to his huge talent, evidently.

As a physicist from a much more younger generation, Witten is far from a "hero" to me, despite his talent, and I think this reflects the feeling of many people around my age, particularly outside the main US centers.

8. **Peter Woit**
May 11, 2022

Alex,
I see Grothendieck and Witten as sharing an unparalleled off-the-charts talent together with a huge capacity for a lot of hard work, with those around them recognizing this, leading to a great deal of influence.

In other respects, they and their careers were very different. Grothendieck was only intensively active for about fifteen years (1955-70), whereas for Witten it's about 45 years and counting. Grothendieck was focused on building new foundations for his subject, whereas Witten never has worked in that way. While Grothendieck's new foundations changed the way people thought of the subject, one might argue that most of these changes would have happened sooner or later without him, since they were the natural evolution of the direction things were headed in.

Witten's contributions to our understanding of non-perturbative quantum field theory seem to me of a different nature. Most of the great things he has accomplished I think would not have happened without him (as one example among many, I don't think we'd have the Seiberg-Witten non-perturbative solution to a 4d SUSY gauge theory). I suspect that, in the distant future, once the damage from "string theory" is cleared out, some of Witten's new ways of thinking about QFT will turn out to be fundamental to further developments.

As for negative influence, here I don't think Witten is the main person to blame. Most theorists have speculative enthusiasms which drive their work, and one can't really blame them for following those. The herd-like way so much of the theoretical physics community followed Witten down a blind alley, and still refuses to admit what happened isn't mainly his fault.

9. **Chris Austin**
May 11, 2022

Shantanu and Peter:
Large Radius Expansion of Superstring Compactifications
Louis Witten(Cincinnati U.), Edward Witten(Princeton U.)
(Apr, 1986)
Published in: Nucl.Phys.B 281 (1987) 109-126

10. **Peter Woit**
May 11, 2022

Thanks Chris!

11. **Alex**
May 11, 2022

Peter,

I really don't agree with your views regarding Witten's contributions to non-perturbative qft. I mean, what real and relevant problems have been solved? Has the Yang-Mills non-perturbative rigorous existence and positive mass gap problem, the one for which there's the Clay Institute prize, been solved? Has the rigorous non-perturbative quantization of the non-linear Einstein's Eqs. been solved? To me, a real advance in non-perturbative qft should be able to tackle those real physics problems. I really only see promises that so far have failed to deliver. On the other hand, there have been lots of people with their feet on the ground and away from the flashes that have taken these problems seriously (i.e., at face value, without embedding them on grand, delusional speculations) and are, in my opinion, much more closer to solutions than Witten ever was (e.g., the AQFT approach is getting closer to the YM problem, while things like Thiemann's master constraint programme are doing the same for the quantum EFEs). SUSY doesn't exist, TQFTs do not describe any known fundamental forces (at most, boundary terms in GR are of this form). As for the unification of the forces, Connes' approach is much more closer to success that anything Witten ever invented. Sure, all of this only reflects my tastes to some extent, but they do contain a core of truth.

Second, I do think Witten carries a lot of blame. True, the herd mentality was a disaster and wasn't a necessary consequence of Witten's status (for example, Einstein was left alone by the community during his quest for a unified field theory, since there were more promising things to do at that time, and I think it was the right choice, despite the fact I consider Einstein to be the greatest thinker of all times in physics). But, once you are made a leader by your peers, that carries a duty and a responsibility too. And Witten's role as a leader was close to reckless if we consider how things developed.

I'm sorry if this sounds a bit ranty, I think there are strong generational and geographical distances between our experiences and viewpoints.

12. **Will Sawin**
May 11, 2022

@Topologist guy: I concur with Amadou – this is surely referring to the proposed proof of the last Weil conjecture using the standard conjectures.

The standard conjectures are closely related to the category of Chow motives – they prove it has various nice categorical properties. If I remember correctly, it's possible to formulate the proof that the standard conjectures imply the last Weil conjecture both with and avoiding the category of Chow motives.

Of course the great irony here is that the standard conjectures are much, much more difficult than the Weil conjectures, and the biggest progress on the standard conjectures since Grothendieck formulated them was arguably the proof of the Künneth type standard conjecture over finite fields by Deligne and Katz-Messing, which uses the Weil conjectures. (In fact, maybe this is not so ironic, as Grothendieck suggests in Récoltes et semailles that the standard conjectures should be viewed as a generalization of the Weil conjectures to arbitrary correspondences.)

I was a bit disappointed that the New Yorker article didn't add any clarity to the part of the story that has always confused me, where Grothendieck is angry that Deligne proved the Weil conjectures in the wrong way. This confuses me because the complaint doesn't really make much sense (how can you complain that someone proved something the wrong way when neither they, you, nor anyone else know how to prove it the right way?) and because I never saw a place in writing where Grothendieck expresses that anger, though there are places where he criticizes Deligne about other things. The article added a bit more detail than I'd heard before to some other aspects of the story, like Grothendieck leaving IHES, but not that.

13. **Z Y**
May 11, 2022

I would argue that Langlands has been more influential, you can see this manifestly by the amount of generational talent that since the 70s has moved from their original research subjects to become involved in his program (Deligne, Scholze,Vincent Lafforgue, T. Hales, Venkatesh), Witten being one of them. And probably this list will continue to grow in the coming decade.

14. **Peter Woit**
May 11, 2022

Alex,
I agree that Einstein is a good contrast to Witten. In his later career at the institute in the same sort of position as Witten he pursued speculative lines that weren't working out, while the rest of the community ignored him. One difference is that I don't know of any stories of Einstein telling younger people that they should drop everything and work on his latest idea. I actually was kind of appalled by Witten's "no regrets at all" attitude about his evangelism starting in 1984.

About the problems you mention, you can't fault Witten for not solving them since no one else has (or has even come close). I think he's very aware of and motivated by those problems but also very aware that he doesn't have a promising idea to solve them. He has a very quick mind and his style is not to go off and spend years thinking about a hard problem, but rather to quickly realize he doesn't have a good idea that will solve the problem and so move on to looking for another problem which he can solve. About, eg., solving QCD, my guess is that we're lacking the right idea, and likely the way to get the right idea is not by frontal attack but by developing tools solving other problems, such that some day one will have found the right tool to attack QCD.

15. **Amadou**
May 11, 2022

@ Will Sawin: I think, when it comes to Deligne's proof, the better term for Grothendieck's reaction would be "disappointment" instead of "anger" (on this specific point I never felt, reading Récoltes et Semailles, that G expressed anger): he claimed to have wanted above all a complete theory of motives – the most profound theme he introduced in mathematics, in his words; the Standard conjectures ("the proper way" to prove the RH over finite fields) would yield such a theory. I guess, later in his mind (not in mine to be clear), Deligne's proof came to symbolize the abandonment of "foundations building" (his style) in favor of "tricks" or shortcuts to quickly snatch the prized theorem. (see R&S, 3.3. Le décès du patron-Chantiers à l'abandon). I am not sure if this clarifies anything for you.

16. **Will Sawin**
May 11, 2022

@Amadouh: It helps! But it doesn't resolve my confusion about the discrepancy between what's in Récoltes et Semailles and what some other people say – for example the paragraph in the New Yorker article about this is not a good summary of the reaction you describe. So my confusion is to what this other perspective where (as in the New Yorker article) Deligne "wronged" Grothendieck with this proof, who experienced a "massacre" and in turn "vilified" him, is based on, if not Récoltes et Semailles – maybe recollections of conversations people had with Grothendieck about feelings he had but got over before writing Récoltes et Semailles?

Another interesting thing in the section you wrote is Grothendieck's list of topics he studied that were dropped:

"I no longer heard news about motives, topoi, the six functor formalism, De Rham and Hodge coefficients, nor about the "mysterious functor" which was supposed to unite under one umbrella De Rham and l-adic coefficients for all prime numbers, nor about crystals (except to learn that they remained at a standstill), nor about the "standard conjectures" and other conjectures which I had formulated and which, evidently, represented crucial questions. "

If these topics were ignored at one point, I don't think they've been recently. People are constantly producing the six functors formalism in new contexts, there's really interesting fundamental work on what a six functors formalism even is (though maybe involving only three at a time), p-adic Hodge theory was developed and constructed the mysterious functor and now we have a single cohomology theory that underlies both, there's a Topos Institute and various topos-based research programs unrelated to the work there, everyone's talking about motives, ….

@Z Y: Yes, it's terribly infectious. When I came to Princeton I was determined not to study automorphic forms, and when I left I had learned a lot about automorphic forms, and now have a few papers about them.

But I feel that it's more about the intrinsic interest of the problems, rather than any influence of Langlands himself (perhaps using "influential" in an overly strict sense). Didn't most of the transfers to Langlands you mention happen after Wiles's work?

Maybe another way to say this is that the Langlands conjectures were very influential, possibly on the century-defining scale that Grothendieck's work was, but his actual work towards those conjectures (Jacquet-Langlands, Langlands-Tunnell, Langlands-Shahidi, …) was only influential in the way that many good papers are influential. People who work on his conjectures are not necessarily working in his mathematical universe.

An interesting parallel or contrast to Grothendieck is the way Langlands has been disappointed with some of the directions people take his work – famously geometric Langlands but also I heard that he doesn't like modularity lifting and is disappointed that everyone isn't studying beyond endoscopy via the trace formula.

17. **David Roberts**
May 12, 2022

@Will

There's an interesting point that I think is captured in the audio recordings of Grothendieck's 1973 Buffalo lectures on topos theory and functorial algebraic geometry: the news that the last Weil Conjecture had been proved landed in the USA, but not the details or even sketch of the proof. So there's Grothendieck waxing lyrical about how Deligne must have used techniques X, Y and Z in the style of Grothendieck's own foundation-building toolbox, and how wonderful this was. And then he clearly found out, and by a decade or so later, was bitter his own style of work was not used.

I believe this is discussed in McLarty's talk about the lectures; at the very least, the video description mentions a closely related point.

18. **Peter**
May 12, 2022

The past 100 years is the period 1922 – 2022. The most influential theoretical physicists must have been Schrödinger, Heisenberg and Dirac, no? After all, they discovered QM. That is a pretty influential theory, if I'm not mistaken.

19. **Larsina**
May 12, 2022

Other than the Michigan/Wisconsin-Madison issue, I think it is very interesting that Witten doesn't remember clearly why he chose Brandeis or history or other choices he made when he was young. On the other hand, he remembers very clearly about the issues in physics and maths since the graduate student days. Is it a kind of focused or selective memory loss or does he want not to discuss about those personal issues from younger days?

20. **tulpoeid**
May 12, 2022

I came here to write something similar to stoopid's comment, and then I read the reply to him/her.
Honestly, that reply should have been included in the body of the post to avoid a misunderstanding… It clarifies "influential" and once more shows how bad a state theoretical physics is in – and that "accomplishments" now means "social engineering".

21. **Boris**
May 12, 2022

Pierre Deligne- The Abel prize interview 2013…, and 30:45 Grothendieck's program as a hindrance to proving Weil's 3-rd conjecture.

https://youtu.be/MkNf00Ut2TQ

22. **Richard**
May 12, 2022

Witten states:

"They wrote a famous paper that was never finished and never published. It's 500 pages long. You can find it online, if you like."

Could someone please post a link to said paper?

23. **Peter Woit**
May 12, 2022

Peter,
QM, QED and the Standard Model are the most influential theories of the past 100 years, but in each case there is no single theorist responsible for the theory. Among Schrödinger, Heisenberg and Dirac, if you pick any one and ask what physics would look like without them, I think the answer is "not that different", their discoveries would have been made a little bit later by someone else. If you ask the question of what the theoretical physics of the last 45 years would look like without Witten, the answer is "very, very different" (in both positive and negative ways…).

24. **Peter Woit**
May 12, 2022

Richard,

Witten is referring to this paper
http://math.uchicago.edu/~drinfeld/langlands/QuantizationHitchin.pdf

25. **Low Math, Meekly Interacting**
May 12, 2022

I've been struck repeatedly by Witten's unequivocal rejection of other approaches to QG and unification. He's hardly the only "only-game-in-towner", but he generally appears to choose his words very carefully and rarely comes across in any interview or article as undiplomatic. Nonetheless, I think he's been on record several times declaring LQG essentially worthless. He may or may not be correct, but in such a highly speculative area of research, I find it hard to justify such stark dismissals based on anything I would conventionally characterize as evidence. Major, perhaps even fatal difficulties, sure. But no value whatsoever?

26. **Peter Woit**
May 12, 2022

LMMI,
I don't want to get involved in the string theory/LQG argument, but it is quite remarkable that, while Witten is normally extremely cautious in what he says about scientific issues, on the topic of string theory he abandons this caution. One reason for this may be the intense criticism he has gotten about string theory for a very long time, leaving him feeling he has no choice but to mount a strong defense.

27. **Richard**
May 12, 2022

Peter Woit,

Thanks for the link!

— Rich

28. **Amadou**
May 12, 2022

@ Will Sawin: Regarding Grothendieck's list of dropped topics, I think one should keep in mind, besides the time of his writing (1984-1988 ?) which is before some later developments in these subjects, that Grothendieck may not have been very aware of progress that was made in the 70's and early 80's (e.g. Fontaine's work on the mysterious functor, his period rings and conjectures). If I were to speculate as to why, I would say that: (1) his correspondence with Serre, his most important source of mathematical information until then, essentially stops after 1969; (2) he was not an avid reader of the mathematical literature (especially after 1970). Also, he seems to be drawing from (not so reliable) memory only some of the material for R&S, correcting himself in footnotes of footnotes (see for example R&S, 13.3.3, "Poids en conserves…").

29. **Anonymous physicist**
May 12, 2022

Peter: you repeatedly treat `theoretical physics' as synonymous with `theoretical high energy physics.' Please don't. Theoretical high energy physics is only a subset (and an increasingly small subset) of theoretical physics writ large.

30. **Peter Woit**
May 12, 2022

Anonymous physicist,
I appreciate the point, usually try to use a different term, e.g. "fundamental" theoretical physics. The problem with "HEP theory" is that the traditional leadership of that field (e.g. consider the IAS theoretical physics faculty) has stopped doing that long ago, with the "having something to do with quantum gravity" now a more accurate description. I have no idea what the right terminology is for what people like Witten have been doing for the last forty years, or for what they are doing now.

31. **Anonymous physicist**
May 12, 2022

Some younger people in that general field refer to it (Ie what the string theory community is now doing) as 'formal theory.' Perhaps that is as accurate a name as any. It has the advantage that it also connotes theory that is increasingly disconnected from the rest of physics, both theoretical and experimental. And indeed I think nowadays the majority of people who consider themselves theoretical physicists essentially ignore the entire field and almost everything its practitioners have done.

32. **Low Math, Meekly Interacting**
May 13, 2022

Sorry to evoke the Sting Wars, as my recollection was solely meant to illustrate. I believe you could substitute any other framework or theory, and find the degree to which it is differentiated from String theory would be inversely proportional to its worth in Witten's judgement. Which, again, is striking, given what is otherwise characteristic about his statements. I don't know, of course, but I've concluded that's simply what he thinks, and in his mind so clear as to require no moderation. A lot of folks who look to me like zealots have been less stark. I simply find it amazing that someone of his brilliance and equanimity has ended up embracing the multiverse. A logical conclusion based on the overwhelming correctness of String/M theory, I guess. But a premise with zero empirical evidence to show for over half-a-century of work is one hell of a premise to be so convinced of. Especially for what may be the greatest brain of any living human.

33. **Jonathan**
May 13, 2022

On the topic of Grothendieck, the Notices of the AMS published several times biographical notes on him, the most extensive are perhaps this couple by Allyn Jackson, in case anyone wants something a bit more in depth.
https://www.ams.org/notices/200409/fea-grothendieck-part1.pdf
https://www.ams.org/notices/200410/fea-grothendieck-part2.pdf

On the interviews, the one with Vafa has an interesting reply from him on the sociology of string theory that I haven't seen anyone else bring up: "So, I would say that the very fact that new ideas are blossoming from string theory has been the reason it has continued, despite the fact, despite the striking fact, there has been zero experimental connection." To what extent are people working in this field because there's a constant stream of not too difficult ideas coming in that people can spend some time on and get a result out of? Is there a contributing factor that PhD advisors are telling their students, "hey, there's these ideas in string theory you can work on and I think you'll definitely get some good results out of" versus them being unsure what kind of results they could get if they work on something else.

On the topic of picking out a single person to be the most influential/talented figure, I think it is a bit of a silly thing to spend time on, similar how you might get debates on various forums about who is the greatest or most important figure of the XXth century, etc. Perhaps before the "modern" times, say in the times of Poincaré or Hilbert or early in Einstein's life it would be easier to give a reasonable scientific estimate because a) the fields weren't too big and b) there weren't as many people working on them (in 1900 the number of physics PhD awarded was 10). By the time of WW2 fields had become much larger and the number of people working in theoretical physics has only ever continued to increase. Thus there were probably far more physicists working on string theory in the 1990s and 2000s than there were physicists working on quantum theory in the 1920s or 1930s, and consequently the responsibility of any individual theorist would have gone down on average. Even on the ends of the bell curve, how would you compare someone like Witten with someone like Weyl? As he himself admits in the interview he thinks his influence is overrated, and for sure if he was doing something else then some connections wouldn't have been made, but many things would have still been worked on by the dozens of others working on the theory regardless, which is a point he made in the interview, while it's harder to make that argument the further you go back (ie with Weyl), particularly on topics that take inspiration from mathematical rather than experimental ideas.

On the topic of giving a name, given that the first sentence of the Wikipedia article on theoretical physics says that the field tries to "rationalize, explain and predict natural phenomena", perhaps something like mathematical physics would be more appropriate?

And finally, besides LQG and strings, are there many other framework theories that have any serious hope of contributing towards a theory of quantum gravity?

34. **Peter Woit**
May 13, 2022

Jonathan,
I do think it's true that the continued huge research effort in "string theory" (or whatever you want to call it) is driven by the sociological fact that it is set up to provide things that people can work on and write papers about using the tools they're now familiar with. The lack of any success of this effort for decades means that what's really needed are some quite different tools and ideas, but that kind of research is difficult, not rewarded, and lacking any community support.

I don't see any point in most comparisons "was X better than Y", but I do think Grothendieck and Witten are rather special cases, with Witten an unparalleled case of someone having an overwhelming influence on his field. Without him I think the Green-Scharz anomaly cancellation result and superstring theory would have been just another speculative path to unification that would have attracted a small fraction of the field, and died off in not too many years as it went nowhere. Away from string theory, his immense contributions to mathematics and to QFT/topology would not have happened. The whole landscape of this kind of theoretical physics and of mathematics would be very different.

And no, "mathematical physics" is not an appropriate term. If you look at much of what is going on as "formal theory" (as an example, the "it from qubit" stuff), the mathematical content is minimal. The problem is not that physicists have pursued mathematics, but that they have pursued bad ideas about physics.

35. **CJ**
May 13, 2022

@David Roberts, and more generally re: the question of whether Grothendieck was angry about Deligne's proof

I think you are extrapolating too far based off of what I remember to be just a few comments of Grothendieck that he had heard that Deligne had finished the proof of the Weil conjectures. Looking at the evidence is available to us, I find none (and in fact much to the contrary) that he was bitter about that proof in particular not being through the standard conjectures, while he was clearly upset about motives and surrounding ideas not being pursued in the years after (which is a completely different thing).

I believe that the claim that Grothendieck vilified or was at angry at Deligne for proving the Weil conjectures in the way he did is simply false, but close enough to the truth – not in the sense that it is almost true, but that the truth can be confused with it through a hasty paraphrase – that it is repeated a lot. Given what we know of

Grothendieck's life and personality, it should not be surprising that many claims about him are exaggerated or just false. Given all the inaccuracies I've heard about him, this would be a more understandable one, though it is unfortunate that it is so widespread.

Regarding evidence for my claim, I think Recoltes et Semailles is a reliable reference, because the question at hand is precisely what Grothendieck thought about a certain thing, and Grothendieck is clearly writing what he thinks there (regardless of whether his analysis is correct). Here Grothendieck speaks highly of both the fact that Deligne proved the Weil conjectures (in Weil I), and of the progress that Weil II represented. I never read any part where he insinuated that it was wrong for Deligne to use the methods he did, whereas he did say that establishing the truth of the Weil conjectures should have been taken as encouragement to continue to work on motivic ideas.

Grothendieck certainly makes no secret that he's upset at the development/non-development of motives and other mathematics in the 15 or so years after he formally left the mathematical scene. How justified his stance was may be a difficult question, but I'd say that it is far more understandable than being upset about a particular paper because of its methods. Like I mentioned earlier, I would guess that the uniqueness of Grothendieck's life and character would make it easier for such a rumor to spread.

36. **toto**
May 13, 2022

Anyone who has read even small portions of récoltes et semaille (and is fluent in french) would understand how "angry" Grothendieck was at his former students. He repeatedly (and in fact it is one of the main subjects of the book) speaks about what he calls the " funeral" i.e. the fact that he thinks that his students have purposefully tried to reduce the amount of credit given to him (especially about the developement of étale cohomology SGA 4 1/2 being a big point of anger) and have tried to make topos theory "disappear".

I think that most people (me very much included) think that this is complete delusion on Grothendieck's part. But this is not the case of everyone : for example Laurent Lafforgue has recently published a text that goes in Grothendieck's direction (https://www.laurentlafforgue.org/Corrections_Lafforgue_entretien_Bourguignon_Grothendieck_RS.pdf). This passage is striking (although in my opinion completely wrong) :

"Grothendieck emploie le mot « enterrement » non pas du tout comme un acte de rupture avec la « communauté scientifique » mais comme le constat qu'il fait que des pans entiers de son œuvre mathématique qu'il considère particulièrement importants ont été non seulement délaissés par ses anciens élèves et collègues mais font l'objet de leur part d'un « dédain » parfois affiché, d'un sourd « mépris » et d'une hostilité qui en interdisent l'accès aux jeunes générations. Presque quarante ans après « Récoltes et Semailles », on peut constater que la plupart des thèmes dont Grothendieck déplorait alors qu'ils fussent dédaignés ou proscrits, comme les catégories dérivées ou les motifs, sont revenus en force et sont étudiés dans le monde entier. Cependant, le thème des topos, que Grothendieck plaçait en tête de la liste des thèmes importants proscrits, est toujours exposé à une grande hostilité dans le monde académique. En fait, nombreux sont les étudiants qui veulent se lancer dans ce sujet, mais ils font l'objet d'intimidations de la part de représentants du monde académique qui tentent de les dissuader de s'engager dans cette voie."

37. **Alessandro Strumia**
May 13, 2022

Einstein wrote a paper claiming that gravitational waves don't exist. String theorists similarly misunderstood what string theory tells, when they wanted to believe it's a theory with some supersymmetric vacua.

38. **David Brown**
May 13, 2022

typo "let's Witten give a non-answer"
Witten was at U.W. Madison for a semester — Zierler probably transcribed it incorrectly.

39. **Peter Woit**
May 13, 2022

David Brown,
Typo fixed.
Could be a transcription error, could be Witten misremembering (the 70s were a long time ago…)

40. **J.S. Milne**
May 14, 2022

Be careful Peter, when reading Récoltes et semailles, not to go down the rabbit hole. [In the 1980s, Grothendieck sent me a copy, and asked me whether I was a "knowing" or "unknowing" member of the conspiracy.]

41. **Anonymous**
May 17, 2022

To Peter,
"I can understand why Witten hopes that the mystery of quantum information theory and gravity will give insight into and resolve the mystery of what M-theory is, finally vindicating his 1984 vision, but this looks to me like a very, very long shot."
Could you elaborate on this? What do you think of Witten's prediction on quantum information and quantum gravity? Do you think it's an area worth pursuing for young researchers today?
Thanks

42. **Peter Woit**
May 17, 2022

Anonymous,
I think there's zero chance for what Witten is hoping for (a vindication of M-theory through quantum information theory approaches to quantum gravity).

The QIT/QG business seems to be yet another example of the pathological sociology that has afflicted HEP/fundamental theory for decades now. Based on a minor intriguing result, the entire field orients itself around a very speculative idea almost sure to not work out. There are endless promotional talks, little realistic evaluation of whether the idea is going anywhere. Grants, conferences, talk invitations, awards, jobs, articles in the press, etc. all start flowing to those who seem to be working on the hot idea. If you're a young researcher you're in a really ugly position: to get a job you should work on the hot idea, even if it's not going anywhere and already has too many people doing it. This pattern just goes on and on.

By the way, I was just taking a look at the AIP site which has interviews, now one with Lars Brink. Brink is a die-hard superstring enthusiast, but even he now thinks the way string theory was pursued was not desirable:

“So, sure, I think it has been overdone in a sense. I think there’s also been too many people working in string theory. There was a time in the ’80s and ’90s when every American university should have a string theory group. They were sort of vacuum cleaning the market for people. That’s never good. For a long time, people could survive by reading string papers, and they could then do some twist on it, and that has been a bit too much. We all have to pay for it now because the funding of fundamental physics as such is diminishing and even dwindling. Here in Sweden, it’s a disaster. My younger colleagues had good funding 10-15 years ago. Now they’re not getting any funding at all from our research council. They are putting much more money into say quantum information. Of course, I’ve only listened to some seminars in quantum information, but I think they will also follow, more or less, the same scheme. There will be simple problems that they can work on, and then they will get out in various directions. It’s not necessarily so but it will be more, again, mathematics of some kind.”

43. **Peter Shor**
May 17, 2022

One of the problems with It from Qubit is that it’s really quite hard to tell the papers that are nonsense from the ones that aren’t. For example, Maldacena and Susskind’s ER=EPR paper is a speculative idea that has no chance of being correct (but listening to his most recent talk, Susskind hasn’t given up on it). And when you actually corner other people in the area they (or at least some of them) will admit that this paper has virtually no chance of being correct, but for some reason they aren’t willing to say this publicly.

There are undoubtedly other papers in this field which are equally improbable. But it seems to me that any field where you have to be in the cogniscenti to know which papers are the ones worth paying attention to is in deep trouble.

44. **Peter Woit**
May 17, 2022

Peter Shor,
String theory at least in the mid-late 1980s came with a fairly specific conjecture about what the theory was and how it was going to connect to reality (approximate Calabi-Yau compactifications). This didn’t work out as hoped, and string theorists were driven to more and more complicated conjectural “solutions” to the theory, at some point early on passing the “Not Even Wrong” boundary into a framework incapable of making any sort of conventional testable (even in principle) prediction.

The ER=EPR/It from Qubit program on the other hand started out from the beginning “Not Even Wrong” in a stronger sense, without anything like a well-defined conjecture relevant to the real world. Looking at the 2013 ER=EPR paper
https://arxiv.org/abs/1306.0533
besides writing down a metric in the appendix, the paper is 43 pages of verbiage and pictures, with no non-trivial equations. The “cognoscenti” problem here is that you need access to one of a small number of people to explain to you which of the verbiage corresponds to something well-defined which you could write down an equation for, and which corresponds to an extremely vague hope/dream with nothing backing it.

Whenever I try and look into the literature of this field, all I can see that is well-defined are toy model calculations, often in 0+1 or 1+1 dimensions. These may be very interesting, but they don’t seem to come with any serious proposal for what they are supposed to imply about a realistic (even in the sense of having any physical gravitational degrees of freedom at all) theory.

Probably others have the same problem I have with writing anything publicly about this. The literature is huge and complicated, so it would be a full time job to master it to the point of being sure there is no there there. I’ve been through this before with string theory claims and wasted far too much time on that. Until the people working on this start producing review articles that clearly explain how what they are doing is supposed to connect to reality, what has worked and what hasn’t, it seems to me that the rest of the world should assume that, as far as a theory of fundamental physics goes, this is just another extremely speculative vague idea that is not working out.

45. **Mitchell Porter**
May 18, 2022

I wonder on what grounds ER=EPR is supposed to have “no chance” of being correct. There is already the curious parallel of non-traversibility of wormholes, and non-transmission of information via entanglement alone; obtaining both of these limitations from a common origin is exactly the kind of beautiful conceptual connection one expects from a deep correct insight.

Perhaps I should also be asking what “It from Qubit” ideas *are* to be considered promising!

46. **d_b**
May 18, 2022

@Peter Shor
I’d be interested to hear why you think ER=EPR has no chance of being correct.

47. **Scott Aaronson**
May 19, 2022

If we compare It from Qubit to “old-fashioned string theory,” I think we’d have to say that, yes:

– Both seem difficult to inform by any feasible experiments (apart from “experiments” that are really simulations, e.g. of one quantum system by a different one).

– Both have fundamental concepts that still lack clear mathematical definitions, except in special cases that are then speculatively extrapolated way beyond.

– Both have a strong predilection for toy models (with, e.g., unbroken supersymmetry, negative cosmological constant, different numbers of dimensions) that make clear departures from the observed world.

Having said, I’d submit to the people here that there’s at least one respect in which It from Qubit sharply differs in flavor from “old-fashioned string theory.” Namely, IfQ takes as its starting point a fundamental conceptual problem (black hole information loss) that would presumably need to be confronted eventually in any theory that encompassed both QM and GR. It tries to produce general insights that would interest anyone who cared about that problem, regardless of the unknown details of what might be happening at extremely high energies and short distances. It often uses string theory as a source of examples, but then (as it were) discards the strings like scaffolding, so that many of the results and open questions can be made surprisingly comprehensible to anyone who understands and accepts QM, plus some high-level facts from GR and QFT.

For instance, anyone, regardless of their speculative commitments about the Planck scale or lack thereof, can now be asked: so, OK, what do *you* think would happen if you did the AMPS experiment? If you wouldn’t encounter a firewall at the event horizon, then which of the assumptions leading to that apparent conclusion do you reject? Or (as in Susskind’s recent talk): could an observer jumping into a black hole see the answers to certain computational problems that are exponentially hard even for quantum computers? If not, then again: which of the assumptions seeming to point that way are false?

If we reach for analogies in the history of physics, none of this strikes me as analogous to any of the great triumphs—the experimentally-confirmed new theories—but much of it *does* strike me as analogous to the posing and sharpening of puzzles (e,g., Newton's bucket argument, Einstein's hole argument, Maxwell's demon, EPR…) that often either preceded the triumphs or drew out their consequences after the fact.

48. **Peter Shor**
May 20, 2022

Why do I think ER=EPR has no chance of being correct? I think the idea (somebody correct me if I'm wrong) is if you take a whole bunch of entangled pairs of particles, say in the state 1/sqrt(2)(|↑↓〉 -|↓↑〉), and let them collapse into two black holes, you get an Einstein-Rosen bridge between the black holes.

Now, suppose you take a whole bunch of entangled particles 1/sqrt(2)(|↑↑〉 -|↓↓〉) and let them collapse. This is the same thing as turning one of the two black holes 180 degrees, so shouldn't they also form an Einstein-Rosen bridge? Now, suppose you take a whole bunch of particles, half of them 1/sqrt(2)(|↑↓〉 -|↓↑〉) and half of them 1/sqrt(2)(|↑↑〉 -|↓↓〉). Do they form a black hole as well? If not, why not? … you started with a lot of entanglement, it just wasn't consistently oriented. But if they do, then it seems to lead to the conclusion that you can make an Einstein-Rosen bridge out of pairs of particles with no entanglement between them at all, because a probabilistic mixture of these two entangled states is equivalent to an unentangled state. And if they don't, you need to answer the question of how much consistent orientation do you need before you get a bridge? And is there a continuum … if more of the particles are have the first orientation rather than the second, do you get a partial Einstein-Rosen bridge? How exactly does this work?

Now if the original paper had actually discussed questions like this, maybe I would take it seriously. But no, it was a grand hand-wavey idea that sounded good, but as far as I can tell, had utterly no thought given to the possibility that when you try to work out the details, they might turn out to be inconsistent.

49. **Peter Woit**
May 20, 2022

Thanks Scott for the explanation of motivation for this. I do have to now discourage further technical discussion of these issues in this comment section, partly because they're off-topic, partly because I'm incompetent to moderate such a discussion.

50. **Geoff Penington**
June 11, 2022

@Peter Shor: The ER=EPR argument starts from the observation that the thermofield double state of two entangled CFTs is holographically dual to an Einstein-Rosen bridge. This observation had actually already been made ten years earlier by Maldacena. However what Maldacena and Susskind did is observe that you could make a naive version of the AMPS paradox for the thermofield double state, but that it's resolution is obvious (the interior partner of the Hawking mode and its purification in the opposite CFT are the same fundamental degree of freedom). When Lenny talks about collapsing entangled qubits into black holes to make a wormhole, he really means constructing the analogue of the thermofield double state in some hypothetical version of holography beyond AdS/CFT. A generic entangled state will not have any sort of short classical wormhole, and no one claims that it does.

They then argued that "morally" the same solution should work for actual evaporating black holes, because we should think of any pair of entangled particles as (in some poorly defined way) physically equivalent to a connected wormhole. This claim as they originally stated it is very imprecise – what on earth is the physical meaning of a "Planckian wormhole" connecting any Bell pair? – to the point where it's "not even wrong".

But a year or so later a paper by Engelhardt and Wall was published introducing the "quantum extremal surface prescription" for computing holographic entropies. And in that prescription classical geometry and entanglement of quantum fields are treated in exactly equivalent ways. This can be thought of as a precise realisation of the vision of ER=EPR in at least one important context. The 2019 developments that Witten referenced in his interview then applied those formulas to evaporating black holes in a way that essentially realises the vision of Maldacena and Susskind.

So sometimes (aspects of) grand hand-wavey ideas can eventually be turned into precise calculations, although it normally takes a lot of work.

51. **Peter Woit**
June 11, 2022

Geoff Penington,
Thanks a lot for the clarifications, that's very helpful. I've never been able to get anything out of things like the ER=EPR paper or Susskind's talks, since they seem to be based on some highly conjectural "vision", but with no explanation of what is actually known and what the conjecture is.

This and That

May 25, 2022
Categories: Uncategorized

Various things that may be of interest:

- MSRI in Berkeley has announced a \$70 million dollar gift from Jim and Marilyn Simons, and Henry and Marsha Laufer. This gift will make up the bulk of a planned endowment increase of \$100 million and is the largest endowment gift ever made to a US-based math institute. The success of the Renaissance Technologies hedge fund is what has made gifts on this scale possible. This summer MSRI will be renamed the "Simons Laufer Mathematical Sciences Institute", and the directorship will pass from David Eisenbud to Tatiana Toro.
- The journal Inference has just published an article by Daniel Jassby, which gives a highly discouraging view of the prospects for magnetic confinement fusion devices. Jassby, who worked for many years at the Princeton Plasma Physics Lab, argues that performance of magnetic confinement fusion systems has not much advanced in a quarter century, making for very bleak prospects that such designs will lead to a workable power plant in the forseeable future. He sees inertial confinement fusion systems like the National Ignition Facility at Livermore as making some progress, but ends with:

  > The technological hurdles for implementing an ICF-based power system are so numerous and formidable that many decades will be required to resolve them—if they can indeed be overcome.

- I've been spending some time reading Grothendieck's Récoltes et Semailles, which is a simultaneously fascinating and frustrating experience. I've made it almost to the end of the first part, except that there will be another forty pages or so of notes to go. To get to the first part involved starting by reading through about two hundred pages of four layers of introduction. It seems that basically Grothendieck did no editing. Once he was done writing the first part, as he thought of more to say he'd add notes. He distributed copies to various other mathematicians, and then kept adding new introductions, with various references to how this fit in with more technical mathematical documents he was working on (La "Longue Marche" à Travers la Théorie de Galois, À la poursuite des champs).

  After the first part, looking ahead there's the daunting prospect of 1500 pages with the theme of examining his deepest mathematical ideas and what he felt was the "burial" that he and his ideas had been subjected to after his leaving active involvement with the math research community in 1970. Quite a few years ago I did spend some time looking through this part to try and learn more about Grothendieck's mathematical ideas. I'll see if I can try again, with the advantage of now knowing somewhat more about the mathematical background.

  Besides the frustrating aspects, what has struck me most about this is that there are many beautifully written sections, capturing Grothendieck's feeling for the beauty of the deepest ideas in mathematics. One gets to see what it looked like from the inside to a genius as he worked, often together with others, on a project that revolutionized how we think about mathematics. This material is really remarkable, although embedded in far too much that is extraneous and repetitive. The text desperately needs an editor.

  There are various places online one can find parts of the book and other related material, sometimes translated. Two places to look are the Grothendieck Circle, and Mateo Carmona's site.

- For an up-to-date project on reworking foundations of mathematics (with an eye to eliminating analysis…), Dustin Clausen and Peter Scholze are now teaching a course on Condensed Mathematics and Complex Geometry, lecture notes here.
- I noticed that the Harvard math department website now has an article on Demystifying Math 55. The past couple years this course has been taught by Denis Auroux, and one can find detailed course materials including lecture notes at his website.

  The current version of the course tries to cover pretty much a standard undergraduate pure math curriculum in two semesters, with the first semester linear algebra, group theory and finite group representations, the second real and complex analysis. The course has gone through various incarnations over a long history, and has its own Wikipedia page. For various articles written about the course over the years, see here, here (about a Pavel Etingof version) and here (about a Dennis Gaitsgory version).

  I took the course in 1975-76, when the fall semester was taught by mathematical physicist Konrad Osterwalder, who covered some linear algebra and analysis rigorously, following the course textbook Advanced Calculus by Loomis and Sternberg. The spring semester was rather different, with John Hubbard sometimes following Hirsch and Smale, sometimes giving us research-level papers about dynamical systems to read, and then telling us to read and work through Spivak's *Calculus on Manifolds* over reading period.

  My experience with the course was somewhat different than that described in the articles above, partly due to the particular instructors and their choices, partly due to the fact that I was more focused on learning as much advanced physics as possible. I don't remember spending excessive amounts of time on the course, nor do I remember anyone I knew or ran into being especially interested in or impressed by my taking this particular course. What was a new experience was that it was clear the first semester that I was a rather average student in the class, not like in my high school classes. The second semester about half the students had dropped and I guess I was probably distinctly less than average. The current iteration of the course looks quite good for the kind of ambitious math student it is aimed at, and it would be interesting if a new textbook ever gets written.

Update: One more related item. This week Chapman University is hosting a conference about Grothendieck. Kevin Buzzard has posted his slides here.

Comments

1. **Analyst**
   May 25, 2022

   What do Clausen and Scholze's notes have to do with "reworking the foundations of mathematics," much less about eliminating analysis therefrom? I'm not familiar with their work but their stated goal is just to develop some of the basic theory of the constructions from their research papers and to illustrate it by proving some old theorems.

2. **Peter Woit**
   May 25, 2022

   Analyst,
   My reading of what Clausen/Scholze are trying to do is revamp the foundations of complex analysis/complex geometry so the subject is not based on analysis (e.g. differentiable functions), but on algebra. They explicitly advertise that their way of proving basic theorems of the subject is "analysis-free". A motivation for this is arithmetic geometry, where Scholze would like to prove things like real local Langlands by the same p-adic methods used at finite primes:
   "Part of our goal is to develop foundations for analytic geometry that treat archimedean and non-archimedean geometry on equal grounds; and we will proceed by making archimedean geometry more similar to non-archimedean geometry.

3. **Joseph Healy**

May 25, 2022

I think one thing missing from Daniel Jassby's commentary is a current trend in managing tokamak plasma in real-time with AI.
.
https://www.nature.com/articles/s41586-021-04301-9

4. **anonymous**
May 26, 2022

That's not an accurate characterization of the notes. They aren't revamping "the foundations of complex analysis/complex geometry," they're just proving some core theorems about compact complex manifolds. (Note that complex analysis and complex geometry encompass far more than compact manifolds…) Further, it's no surprise this can be done algebraically, because GAGA tells you that compact complex manifolds are equivalent to complex algebraic varieties. One could already prove all of these things from an algebraic perspective. The point of the notes seems to be to give new proofs along these lines that illustrate their new techniques.

This is an interesting project (especially as a way to make their techniques more accessible), but I feel it is inappropriate to claim it is "reworking foundations of mathematics (with an eye to eliminating analysis…)." There's nothing about foundations in the notes, and the fact that these theorems can be "algebra-ized" is known to any PhD student in complex geometry.

5. **mls**
May 26, 2022

@ anonymous

Dr. Woit has a history of misrepresenting anything in mathematics and related subjects which does not conform to his personal beliefs. This has been especially true with every mention of "foundations."

At least the last paragraph of his present post includes an honest history explaining his profound ignorance of mathematics.

6. **Will Sawin**
May 26, 2022

I'm going to continue the trend of every commenter having a slightly different interpretation. In particular, I think one should look at this in the context of Scholze's previous work in condensed mathematics.

I think it's important to begin with the observation that in the theory of complex manifolds one often wants to use analytic methods and one also often wants to use algebraic methods potentially involving derived categories and such. A basic concern is that, in some future arguments, one could run into trouble when these two strands don't weave together properly.

For example, if one wants to keep track of size of something using Banach spaces, and also use derived categories, one could be stymied by the fact that Banach spaces do not form an abelian category, and therefore can't be used to construct a derived category. Scholze's earlier work on condensed, liquid, and solid vector spaces provides a fix for this by defining an abelian category that includes Banach spaces.

I believe Scholze's work in this course is intended to provide further tools on the same lines that are potentially useful in future research. Specifically this is for research that already intersects analysis and algebra – I don't think there is any intention to replace analysis in the proof of existence of solutions of some PDE or the prime number theorem or something like that. But it's not specific to the point that there is some Weil-conjectures-like goal in mind.

@anonymous

> because GAGA tells you that compact complex manifolds are equivalent to complex algebraic varieties.

This is not true at all. It only tells you that, for a manifold that is already algebraic, studying it using analytic and algebraic tools will give you the same answers (the same compact submanifolds, the same cohomology, …)

> One could already prove all of these things from an algebraic perspectiv

This seems like a silly point of view when one of the things being proven in the notes is GAGA, i.e. exactly the bridge that links the algebraic and analytic perspectives. One can't use GAGA to prove GAGA.

According to Laurent Fargues, one of the goals is the Hodge decomposition. Do you consider this something that can be proven entirely algebraically?

7. **Johan**
May 26, 2022

If I remember correctly, Columbia Library has one of the original copies of Récoltes et Semailles (sent by Grothendieck to Sammy Eilenberg), no? It may even be autographed, in any case it really belongs in the rare books collection.

8. **Jonathan Chiche**
May 26, 2022

I believe most, if not all, original copies of "Récoltes et semailles" are inscribed by Grothendieck. In 2008/2009 I read one of the two copies owned by Paris N university (with N=7 probably). One of them was inscribed by Grothendieck to Faltings, the other to Leray. If my memory serves me right, the latter had a note written by Leray pointing to an error in the text (regarding himself), and one of the two copies (I cannot recall which) was incomplete pf roughly one half. It would be interesting to have a list of institutionally and privately owned original copies, together with the text of Grothendieck's dedication. I disagree that the text needs an editor and remember it as a great reading for an aspiring mathematician.

9. **Dustin Clausen**
May 26, 2022

For what it's worth, I agree with what Will Sawin says (minus the attribution of everything solely to Scholze, of course!). I never thought of the goal of condensed math, or this approach to analytic geometry, as being to eliminate analysis. Actually, a lot of analysis shows up in the foundations. Rather, the goal is to put parts of analysis and topology in a new framework, one which allows to mix more easily with algebra. Then we can make formal arguments of a certain algebraic style, which however lead to analytic and topological conclusions.

In the example of the theorems we aim to reprove in the course, this means that the analysis is black-boxed into some foundational material about liquid vector spaces, and then the rest of the argument is in a sense purely algebraic. But I don't really think of that as eliminating analysis. In fact, in my view it is neither desirable, nor even possible, to elminate analysis from the study of complex geometry!

10. **Peter Woit**
May 26, 2022

mls,
There was a small element in my post of trolling of analysts/foundations of math aficionados. I won't really apologize since it was kind of successful, and in the spirit of Clausen/Scholze's claim to be making the subject "analysis-free". I see that Clausen has written in here to clarify, which is great.

There is some connection of this to my math education experiences at Harvard. Doing about average in a Math 55 class that had a bunch of the top Math Olympiad performers in it even though I wasn't putting much time into it (much more of my time was going into the quantum mechanics class I was taking) wasn't such a bad performance. More relevant, the next math class I took was a graduate course in analysis, taught by Andrew Gleason. This course extensively covered set theory, point set topology and measure theory, and had us spending lots of time puzzling out questions like whether a space was $T_{2\frac{1}{2}}$. Around the same time I also took a set theory course from Quine (I did pretty well in both from what I recall).

In retrospect taking these courses (Gleason and Quine) was a big mistake and a waste of time, caused by my idea that, both in physics and in math, what I should be doing was focusing on learning the "foundations", from which understanding of everything else would flow. I should have been taking other courses which were not "foundational", but would have taught me about some of the great unifying concepts that bring together a wide range of beautiful mathematical structures (as well as physics!). Someone should have told me to take representation theory…

11. **Łukasz**
May 26, 2022

@Analyst @Peter Woit

In my opinion, the phrase "reworking the foundations of mathematics," connotes rather research in mathematical logic or set theory. For example, classical Arithmetic is based on classical first-order logic (some people prefer to tell in this context, about classical functional calculus).

12. **Peter Woit**
May 26, 2022

Łukasz,
I did clarify this later by specifying "foundations of complex analysis/complex geometry" and note that Clausen/Scholze talk about "a new foundation for combining algebra and topology". I'm well aware that many people identify "foundations of mathematics" with mathematical logic/set theory. That identification led to the misguided educational experiences of my youth that I explained, so I rather intentionally used the term to refer to a broader (and, if you ask me, more interesting) set of issues.

13. **Will Sawin**
May 26, 2022

@Dustin Clausen

Sorry about that! Maybe next time I will make up for it by crediting it entirely to you and not at all to Scholze…

14. **Richard**
May 27, 2022

What does "GAGA" stand for?

15. **Peter Woit**
May 27, 2022

Richard,
For most people, a well-known performer. For some mathematicians, more relevant is
https://en.wikipedia.org/wiki/Algebraic_geometry_and_analytic_geometry

16. **Richard**
May 27, 2022

Peter Woit,

For * me * , it was what I was going trying to figure out what it means!
Thanks for the reference! [For the linklazy among us, "GAGA" refers to "Geometrie Algebrique et Geometrie Analytique", a foundational paper in algebraic geometry published by the redoubtable Jean-Pierre Serre in 1956 (Full disclosure: my personal knowledge of such things is a trifle on the nonexistent side).]

17. **David Brown**
May 28, 2022

Jassby's 2018 article might be worth studying:
https://thebulletin.org/2018/02/iter-is-a-showcase-for-the-drawbacks-of-fusion-energy/
In any research, the yea-sayers who are overly optimistic might have a strong tendency to drive out the nay-sayers — because cash and career advancement are at stake.

18. **martibal**
May 28, 2022

And for those who do not speak french, let us not forget that "gaga" in popular langage means "senile" (more or less, maybe Peter would have a more accurate translation), and may also be for "to have a crush on someone"

19. **Paul D.**
June 17, 2022

The big problem with these DT fusion schemes is that the volumetric power density is just terrible. An existing PWR fission reactor's primary pressure vessel might have a power density of 20 MW/m^3; the volumetric gross fusion power density of ITER is 0.05 MW/m^3. It's very difficult to see how DT fusion can ever be cheaper than fission, given that the reactor itself will be at least an order of magnitude larger (and much more complex).

Note that this problem has nothing to do with plasma physics, but rather is due to limits on heat and radiation transfer at the wall of the reactor vessel and the square-cube law. Totally solve plasma confinement and the problem is still there, at least for reactors burning DT.

This issue has been known for approaching 40 years, if not longer. I find it incredible the press is still spouting glowing nonsense about DT fusion.

http://orcutt.net/weblog/wp-content/uploads/2015/08/The-Trouble-With-Fusion_MIT_Tech_Review_1983.pdf

20. **Alan Post**
June 18, 2022

I think Jassby's criticism of SPARC is a bit off: he points out that higher field strength will produce higher mechanical stresses, but doesn't make any argument that this is insurmountable. He also says that SPARC should focus on improving Q, not cost-effectiveness. But a big reason that progress has slowed is that the machines have become so expensive, so these are connected. Additionally, increasing field strength directly increases Q, for a fixed-size machine.

21. **David Roberts**
June 27, 2022

If you wanted to add another update about the Chapman conference, the videos are now available.

22. **John Doe**
June 28, 2022

I have math55a and math55b course notes on my computer. They can be found on the internet, easily. The full course notes are about 100 pages~ for each part and they cover everything that I covered in about 4 years at my university, but in very brief detail. The algebra course even covers topics like category theory and differential geometry. A lot of Galois theory is covered too.

I think I'd do very very poorly in math55 just because you are given not much time at all to digest these concepts. Rather than spending several weeks on rings, they are covered in one lecture and then you move on to something more advanced. I guess Peter Woit is to me like Ed Witten is to Peter Woit.

I also have all of parts I, II,III course notes of the Cambridge math degree, which can also easily be found on the internet. They were latex'd by a student who did it in four years. The full course notes there are about 4000 pages~ as compared to the 200 pages in math55. They do go a little more advanced and lots of extra topics like model theory, combinatorics are covered as well.

Memories of a Theoretical Physicist

June 1, 2022
Categories: Book Reviews

Joe Polchinski's autobiographical Memories of a Theoretical Physicist has just been published, in an open-access version that is freely available. Much of the volume is what appeared here on the arXiv back in 2017, but this has been supplemented with other material, including an introduction by Andy Strominger and detailed bibliographical notes by Polchinski's student Ahmed Almieri. If you're interested in the details of Polchinski's work, I'd recommend also reading Witten's biographical memoir here, which covers the same scientific material, but from Witten's perspective.

Polchinski and Witten agree that one of his three major accomplishments was the anthropic string theory landscape, but I'd argue that this was the opposite of an accomplishment. Instead it should be seen as a disastrously bad scientific argument, one that became a wrecking ball that brought to an end most work towards a better, more unified theory of fundamental physics. Polchinski and Susskind were the two most influential figures in pushing for this argument in the theoretical physics community (Susskind wrote a popular book).

Everyone I've talked to who knew Polchinski has nothing but positive things to say about him as a scientist and as a person. I never got to meet him in person, but wish that I had, this might have improved our bad relations. Back in 2004, I wrote an early blog entry that seems to have greatly upset him, by describing (accurately I still think) a popular article on the landscape he wrote with Raphael Bousso for a Scientific American issue about Einstein and his legacy as pseudo-science that would have made Einstein gag. I was not the only one with this reaction to his work, his KITP colleague David Gross also had strong things to say on the topic. In the memoir Polchinski refers to the years of these arguments (and of the appearance of my book and Lee Smolin's) as ones he found emotionally very difficult. At the time he somehow managed to get the arXiv to ban trackbacks to my blog, for that sorry story see here.

Towards the end of his life the landscape pseudo-science he had so vigorously promoted became a dominant point of view among influential theorists, with even Witten coming to accept it. Polchinski remained upset by my continuing complaints about the subject. One of his last papers, (this one, also see here), extensively attacked me personally, claimed that string theory was true with Bayesian probability greater than 97.5 percent and appears to have been partly a reaction to this blog post. In the blog post I made fun of his claims to have calculated the Bayesian probability of a multiverse as at least 94%. I was unaware at the time that he was already sick with the disease that would later take his life.

Much of this autobiographical memoir is rather technical and will be mostly of interest to experts and specialized historians. The complicated story of Polchinski's career is very much the story of what happened during this time to the field of fundamental theory in physics. This is a very different story than the usual one of a scientific field's progress towards greater enlightenment.

Update: A correspondent pointed me to something I hadn't noticed. In section 3.3 Polchinski writes:

> This was typical of Mandelstam, how far ahead he was in much of his thinking. Another example, the first paper that one studies in the Langlands program today, is the first paper that Mandelstam gave me to read forty years ago.

I wouldn't describe it the way Polchinski does, but I'm guessing the paper he's referring to is Montonen-Olive.

Comments

1. **Sabine**
   June 2, 2022

   I did a postdoc at UCSB in 2005 and moved to PI in 2006. I'd started blogging at around the same time I moved to Santa Barbara. My own research at the time was peripherally related to both string theory and quantum gravity, but really neither. In any case, I believe it's for those reasons that both Joe and Lee talked to me about the whole string-theory-and-Peter-Woit thing repeatedly.

   For what I remember, Joe's issue with you was mostly your position and you not being a physicist which disqualified you in his eyes. Lee wasn't quite as easily to discard.

   The thing with the Bayesian estimate. I believe Joe and I actually talked about this when I interviewed him for my book. (This was a week or so before his surgery.) This didn't make it into the transcript (because I don't explain in the book was Bayesian inference is) and I'd have to try and find the recording to be sure, but I believe he told me explicitly that of course he wasn't serious with this number. I think he was trying to make a more general point that Bayesian inference is a way to quantify the credibility of a hypothesis.

   There's a lot of people in the foundations of physics now who think that Bayesian inference is kind of a magical tool that they can use to prove their biases must be correct. Joe was just somewhat ahead of his time, I guess.

2. **Dave Miller**
   June 2, 2022

   Peter,

   Polchinski was a year ahead of me at Caltech (though only a few months older), and we were in the same dorm all of our time at Caltech: I knew him fairly well for those three years at Caltech and became reacquainted when I was finishing my Ph.D. at SLAC and he was a post-doc at SLAC.

   Joe was generally a fairly nice, quiet guy (a bit less so when he was drunk). Joe was sympathetic and supportive to me when I went through some personal problems at Stanford.

   He did have some wild and crazy exploits — including the famous climb in the air shaft to get to the roof of the nine-story Millikan Library (Joe did not of course climb the whole nine stories, but it still would have been quite a fall!). The Caltech administration, by the way, was a good deal less accepting of that particular stunt than his autobiography makes out.

   I've always felt that the 97.5 percent Bayesian calculation was a bit of a joke: I'm doubtful that Joe really took Bayesianism all that seriously.

   I also knew Billy Zajc, who was, if anything, even wilder and crazier than Joe.

   By the way, I and another Caltech physics grad had a chance to meet Joe's wife Dorothy back while they were engaged: we were both surprised that Joe had connected with such a pleasant and decent woman. I believe his claims in the autobiography that she really helped to give Joe some grounding.

I'm sorry for the difficulties you had with Joe: even decent people can become unreasonable when their ego and their career seem threatened.

Thanks for the link.

Dave Miller in Sacramento

3. **John Preskill**
June 2, 2022

Also very touching: The Afterword by Joe's family.

4. **Łukasz**
June 4, 2022

Dave Miller "I'm sorry for the difficulties you had with Joe: even decent people can become unreasonable when their ego and their career seem threatened."

Well, Polchinski was on the make in scientific world, at least. I remember, how as a student, I was admiring his classical monograph.
On the other hand, he was engaged in String Theory, and some other physicists, as for example, Peter Woit, are sceptical, as far as this theory (or rather "String Hypothesis"), is concerned. With all respect to Polchinski, but if I were on his place, even if my results were criticized, I would be glad that they are very well-known and many people appreciate them.
There are many people (let's call them, as Mr. X1. or Mrs. X2.), who have actually a large contributions to some scientific branches, and they are not appreciated. Their results really solve some important problems, and this is not the case of String Theory ("String Hypothesis") – after reading such paper, you see that there is actually a correct solution of certain important problem.
Nobody has been criticising their results, except some editorial boards and/or reviewers, who most evidently do not want to see these results to be published. And even if Mr. X1. or Mrs. X2. publishes eventually these results, they will be very often passed over.
Polchinski fell on his feet, at least as far as his scientific career is concerned.

5. **Martin S.**
June 4, 2022

@Dave Miller: It is frequently in a way that other people push their opinions to you, regardless of validity of those opinions. And since it is commonly pain in the ***, it is easy to get angry due to that.
Not having an idea whether Peter's blogging and book came in a situation when other ones were pushing to Polchinski, but can imagine that it was a part of such an environment.

6. **wb**
June 5, 2022

Bayesian probabilities depend on the prior and the (number of) hypothesis one considers. I assume he never considered the hypothesis that string theory could be false …

7. **Peter Woit**
June 5, 2022

wb,
I went back and took a look again at Polchinski's Bayesian calculations. His Bayesian method is that each argument he has in favor of something reduces the probability that it is wrong by 1/2. For the multiverse he has four, so only 6% chance it is wrong, for string theory it seems to be six, so 1.5% chance it is wrong (but I can't figure out from what he writes what the six arguments are).

One remarkable thing about his analysis is that, according to him, there are only arguments in favor of string theory and the multiverse, no arguments against either.

The whole thing is just completely bizarre.

8. **Peter Woit**
June 5, 2022

Łukasz and Martin S.,

In section 10.7 of the book, Polchinski is quite explicit about what upset him:
"But also I had extended periods of anxiety. Sometimes these just plagued the early hours of sleep, but other times they took over the day, and my work. One that I remember clearly was my induction into the National Academy of Sciences in 2005. This should have been a time of great celebration, and there was some, but throughout I was filled with an ill-defined anxiety."

This induction ceremony would have been April 22 in 2006. The year before at Strings 2005 there had been a panel discussion which did not go well for the landscape, see https://www.math.columbia.edu/~woit/wordpress/?p=230
and there were an increasing number of negative stories about string theory in the press. In March there had been public controversy about my blog over the issue of arXiv trackbacks (I was unaware that Polchinski was behind this, upset by a 2004 blog posting criticizing his Scientific American article about the landscape). On April 9 I posted a detailed argument about the problems with his claims about the landscape, see here. My book was published a month after the NAS induction ceremony, Smolin's a few months after. By April 22 review copies of both books were in circulation and it's quite possible Polchinski had seen them and so knew what was coming.

9. **Alex Gezerlis**
June 5, 2022

Peter,

"I went back and took a look again at Polchinski's Bayesian calculations. His Bayesian method is that each argument he has in favor of something reduces the probability that it is wrong by 1/2."

This line of thinking exhibits almost too many fallacies to count: it implicitly assumes a discrete state space, then employs equiprobability with only two possible outcomes (along the lines of "you're either gonna roll sixes or not, it's 50/50"), obscures the distinction between scientific and statistical significance, mixes up the p-value with the probability that the null hypothesis is true, says nothing regarding the power of the test, uses likelihood to mean posterior, the list goes on and on.

All that being said, the reason I am writing this comment is to emphasize that Polchinski was facing major challenges when he produced this alleged argument. While it's true that in science we're supposed to focus on the argument not the person, to me at least it feels infelicitous (to put it mildly) to dissect this specific argument.

10. **Peter Woit**
June 5, 2022

Alex Gezerlis,

Polchinski himself makes clear that he realizes he is not doing a correct Bayesian analysis. In https://arxiv.org/abs/1601.06145 he writes:

"So let me say very clearly: Bayesian analysis is not the point. It is not even one percent of the point. Every word spent on this subject is a wasted word, in fact it has negative value because it distracts attention from the real point, which is of course the physics. And I can blame myself for this, because I chose to frame the problem in this way."

My point in an earlier comment was just to note that he was refusing to acknowledge any problems with string theory or the landscape. I don't think this had anything to do with his illness, it's consistent with his behavior over many years and with the behavior of other prominent string theorists. Someone who talked extensively to him over the years assures me that his arguments about string theory and attitude towards string theory critics were no different after his illness than they had been a decade earlier.

11. **2 Questions**
June 8, 2022

1. Could the String Wars episode in Joe's life have played a role in creating/aggravation his illness?
2. Why does Sabine say "you not being a physicist"?

12. **3rd Question**
June 8, 2022

3. If you are not a physicist, Peter, are you more of a science historian or a mathematician?

13. **Peter Woit**
June 8, 2022

Questions,

I don't know of any reason that things Polchinski found upsetting in 2006 should have anything to do with his fatal illness ten years later.

Polchinski unfortunately seems to have decided to deal with the 2006 string theory critique by ad hominen arguments: Smolin was not honest, and Woit was not a physicist, so people should not listen to either of them. It's true that, after a Ph.D. and postdoc in theoretical physics, since 1988 I've been employed by math departments.

I'm definitely not a science historian. On the mathematician/physicist distinction, I don't see why one has to be only one or the other. I was thinking of writing more here about the mathematician vs. physicist thing, but that's a long story, which really has nothing to do with Polchinski, so maybe I'll write about it later in its own context.

14. **AcademicLurker**
June 8, 2022

"Only someone with a faculty appointment in a university physics department is a physicist" seems like a pretty narrow definition since, by that standard, Einstein during his *Annus Mirabilis* wasn't a physicist…

15. **Peter Woit**
June 8, 2022

AcademicLurker,

Looking at what Polchinski wrote late in life about Smolin and me, his claim is basically that Smolin and I do not think and behave the way scientists are supposed to think and behave (for instance, about me he writes "The actual scientific quality is so very poor, as was easily seen by contrasting with the way that leading scientists actually work and think"). I disagree strongly with him about "scientific quality", but, restricting attention to prominent areas of theoretical physics during the last 30 years, I have to agree that I don't work and think the same way as leading figures in the field (and Smolin maybe feels the same way).

In various exchanges in blog comment sections with string theorists, I found that typically they seemed to think that the only legitimate scientific arguments were technical ones that accepted the same basic framework as researchers writing papers on the subject. For example, on the subject of the landscape, technical discussion of the properties of specific moduli stabilization mechanisms supposedly giving string theory vacua was science, arguing that a theory with such vacua was unpredictive and thus a failure was not science. For Polchinski and other string theorists, working within their framework was science, arguing against their framework was not science (and doing so put you in the realm of the crackpot, like people who argued against evolution or relativity).

16. **Shantanu**
June 8, 2022

Peter or anyone : Do you know what Polchinski felt about research (and researchers)in LQG ?

17. **Peter Woit**
June 8, 2022

Shantanu,
I don't remember Polchinski saying much about LQG or LQG researchers. If I had to guess I'd guess that he thought that string theory was a much better framework for doing fundamental physics than LQG, but for him arguing against other people's frameworks was not doing science so he should concentrate on discussing the string theory framework.
I think that for him and many other string theorists the sociological arguments that Smolin and I were making made no sense. By definition, successful science was whatever leading scientists were doing, unsuccessful science was what they stopped doing or decided not to do. Anyone arguing against this was by definition not doing science.

18. **Max Madera**
June 9, 2022

I think your last paragraph perfectly summarizes the lore within first class string physicists.

19. **Jim Eadon**

June 9, 2022

Dear Peter, regarding arXiv trackbacks, is your blog still blocked?

20. **Peter Woit**
June 9, 2022

Jim Eadon,

As far as I know, trackbacks to this blog from the arXiv are still not allowed, for reasons that remain a mystery to me.

Physicists Discover Never-Before Seen Particle Sitting On a Tabletop

June 9, 2022
Categories: Uncategorized

I woke up this morning to find out that a new Higgs particle which could explain dark matter has been discovered, in a table-top experiment at Boston College. For some of the news stories about this, see here and here. Wikipedia now has an entry for this that explains:

> The Axial Higgs boson is a fundamental particle whose discovery was announced by American researchers in Nature on June 8, 2022.

Of course this is complete nonsense. The paper Nature just published (the preprint is here) is about a condensed matter experiment that has nothing at all to do with the Higgs (effective fields in a description of a condensed matter system have nothing to do with fundamental fields).

Who is responsible for misleading the public and discrediting science with this kind of behavior?

- The authors, who begin their abstract with

  > The observation of the Higgs boson solidified the standard model of particle physics. However, explanations of anomalies (for example, dark matter) rely on further symmetry breaking, calling for an undiscovered axial Higgs mode.

  which has nothing to do with the result in their paper.
- The editors and referees at Nature, who should never have allowed such an abstract.
- Boston College, which put out this press release, which starts out:

  > Chestnut Hill, Mass. (6/8/2022) – An interdisciplinary team led by Boston College physicists has discovered a new particle

  In this case another institution, Oak Ridge, put out a much more responsible press release for the same paper, showing how to do this properly.

Universities desperately want to see this kind of story in the press, and there's rarely any downside for the scientists and PR people who produce bogus such stories. Boston College needs to take action to retract the press release and make sure this doesn't happen again. Nature should also take action to issue a correction stating this paper has nothing at all to do with the Higgs field and address the bad editing and refereeing that led to this.

Update: At least the Wikipedia article has been fixed.

Update: More physicists spreading hype about this here.

Update: The Higgs hype has been extended to add quantum computing hype, see Newly-Observed Higgs Mode Holds Promise in Quantum Computing, and the Wikipedia article now includes this new, extra hype.

Comments

1. **Peter**
   June 9, 2022

   I assume that it was BC and not BU that put out that press release. That aside, this is tremendously, tremendously depressing.

2. **Peter Woit**
   June 9, 2022

   Peter,
   Thanks for pointing out the mistake attributing the press release to BU rather than BC, fixed. Apologies to BU.

3. **WD**
   June 9, 2022

   I am guessing that there isn't a downside in the short term for the parties involved. However, in the medium-long term the damage that this kind of thing does is enormous – those of us old enough remember how the ridiculous hype spread by many practitioners condemned the AI discipline to decades in the cold, and we can't help but having a sickening déjà vu feeling concerning AI (again!) and quantum computing. If this percolates to fundamental physics research as well we are going to be in a world of trouble.

4. **paddy**
   June 9, 2022

   Not quite the Jesuitical logic I am familiar with.

5. **Peter Shor**
   June 9, 2022

   Weren't there similar misleading articles when the Majorana quasiparticle was "discovered", also in condensed matter?. ("Discovered" here is in quotes because the result was later retracted.)

   Let me add that the condensed matter versions of both the Majorana and this Higgs boson are important discoveries — much more interesting than the entanglement experiments that have been claimed to connect to quantum gravity. However press releases and the press should not confuse these condensed matter quasi-particles with high energy physics particles.

   Since there are probably a lot more unknown condensed matter phenomena to be discovered which can be couched in terms of unusual quasi-particles, we should be prepared for more of these types of news releases.

6. **Peter Woit**
   June 9, 2022

Peter Shor,
The Majorana case was somewhat different in that there wasn't a significant attempt to mix it up with particle physics and dark matter. Also, there the condensed matter system was of huge practical interest as a possible topological qubit.

There's by now a very long history of misleading press stories of this kind related to string theory, typically of the form "string theory finally related to experiment", based on the appearance in some condensed matter system of something that had some sort of analogy with something that appeared in something related to string theory. At least this story didn't do that (they easily could have, adding something about axions in string theory), perhaps a reflection of the fall in popularity of string theory hype.

7. **Peter Shor**
June 9, 2022

Peter Woit,

I definitely remember reading news articles about the Majorana particle discovery that mixed up fundamental particles and quasi-particles (here's one), but looking back, you're right — it looks like the universities' actual press releases didn't do this.

8. **John**
June 9, 2022

The title of this blog post is much more misleading than the authors' abstract. I (seriously) think that you should edit it.

9. **SteveB**
June 9, 2022

Only after reading the Oak Ridge release, I see that they detected a quasi-particle, not a fundamental particle.

Why is the name "Higgs" used here for a quasi-particle as in "the axial Higgs mode"?

10. **Peter Woit**
June 9, 2022

SteveB,
I'm not sure exactly what they're doing in this paper, but assume that the usage is by analogy to the particle physics setup, where there's a Higgs field acted on by a gauge symmetry. Some modes are gauge modes, but there's also a gauge-invariant degree of freedom, and this is the Higgs mode, excitations are Higgs particles.

In the simplest case (Abelian Higgs model), the Higgs field takes values in the plane, lowest energy on a circle, gauge mode is motion around the circle, Higgs mode is radial motion.

11. **Peter Woit**
June 9, 2022

John,
If you click through the first link, you'll see that the blog post title is just exactly the title of a press article about this, also the title of a Slashdot post about this
https://science.slashdot.org/story/22/06/08/2224227/physicists-discover-never-before-seen-particle-sitting-on-a-tabletop

When I write about a press article, I often use the title of the press article as the title of the post. Doing this in this case was intended as a bit of a joke, since for most of my usual readers just reading that title should have made clear this was comically absurd hype.

12. **Anonymous**
June 9, 2022

There is in fact no gauge symmetry or Higgs mechanism at all in the system studied in this article. The authors study a charge density wave material in which the (global) translation symmetry is broken (to be contrasted with superconductors, where there is actual gauge symmetry). The phase/goldstone mode remains intact and what the authors study is the corresponding amplitude mode. In fact, one of the referees argues that such a mode should simply be referred to as an amplitude mode, not a Higgs mode, but the authors assert that this terminology–referring to an amplitude mode with no Higgs mechanism as a Higgs mode–has been used previously in the literature, and the matter was dropped (see peer review file available with the article, referee 3).

13. **Alessandro Strumia**
June 10, 2022

It's a shame that Nature and Boston College did not claim that their new Dark Matter axial Higgs confirms string theory

14. **Adam**
June 10, 2022

@Anonymous: this battle has been lost ten years ago. The first appearance I am aware of in condensed matter is PRB 84, 174522 (2011) by Podolsky et al, where they call it an "amplitude (Higgs) mode" in the title, but mostly "Higgs" in the text.
It became a " 'Higgs' amplitude mode" in Nature 487, 454 (2012), and soon after, the term amplitude was finally dropped, e.g. PRL 110, 170403 (2013).

Having worked on this problem, I tried at first to fight not to call that a Higgs mode at all. But when everyone else does, what can you do (I still say in my talks on the subject that this Higgs mode has nothing to do with the Higgs boson since there is no gauge field though).

I guess the referee dropped the matter because there wasn't much they could do about it (especially if the other referees did not complain).

15. **vmarko**
June 10, 2022

@Adam: The issue is not the common terminology of the "Higgs mode" or whatever, but the fact that (in the abstract) the authors make an explicit connection to the Standard Model and dark matter, both of which are completely unrelated to the topic they have actually studied in the paper.

Using the same terminology for different things is not an excuse to confuse and conflate those things. The referees should have pushed against that part of the abstract, regardless of the commonplace terminology.

Best, 🙂

Marko

16. **Peter Woit**
June 11, 2022

Anonymous/Adam,
I hadn't realized how bad the Higgs hype problem in condensed matter had gotten, that people were calling the amplitude of a field a "Higgs" mode when there is no gauge symmetry. Paying a little bit more attention, I also realized that the "Higgs" in this case isn't even a scalar field, it's a vector field. Calling the amplitude of a vector field in a theory with no gauge symmetry a "Higgs" is just completely absurd.

17. **Adam**
June 12, 2022

Peter: it is not as bad as you say. It is a scalar field under spacetime transformation, but it has N (typically 2) internal degrees of freedom, with O(N) symmetry (equivalent to U(1) for N=2).
In the symmetry broken phase, there are N-1 Goldstone modes (that would be eaten by the gauge field if it were present), and a remaining one, which would be the Higgs field. It used to be called the amplitude mode because it is related to the longitudinal fluctuations of the order parameter (along its vev), which, at least at a mean-field level, is equivalent to the fluctuations of the amplitude of the order parameter.

So the problem is "only" that people use the term Higgs even though there is no gauge field and thus no Higgs mechanism.

18. **Blake Stacey**
June 17, 2022

The Wikipedia article appears to have been deleted.

All Langlands All the Time

June 12, 2022
Categories: Langlands, Uncategorized

I'm about to head to Paris on vacation, quite possibly there will be less blogging for the next couple of weeks. Here are a few Langlands-related items:

- The big Langlands-related event of the summer will be the July IHES Summer School on the Langlands Program. Laurent Fargues has posted notes for the summer school (written with Peter Scholze). They comment at the end that "it still remains to find an archimedean analog" of their work relating local Langlands and geometric Langlands on the Fargues-Fontaine curve.
- At Quanta, Alex Kontorovich has a popular introduction to the Langlands program.
- There's a recent review by Michael Harris of expected properties of the local Langlands correspondence.
- There's a program going on about automorphic forms at the Collège de France, featuring lectures by Ngô. Maybe I'll stop by to hear one next week.
- I was sad to hear about the recent death of Joël Bellaïche, who had been a postdoc here at Columbia. David Hansen writes here about him and his work.

Comments

1. **Topologist Guy**
June 14, 2022

   So I'm by no means an expert in p-adic geometry, but my understanding is that Scholze's perfectoid spaces are very useful gadgets for relating problems in "mixed characteristic" (e.g., over the p-adic numbers Q_p) to problems in characteristic p. For perfectoid spaces, unlike in schemes, we have this "tilting functor" relating perfectoids over mixed characteristic fields to ones in positive characteristic. It's often preferable to work in positive characteristic, as we have access to the Frobenius endomorphism in this case.

   So very very broadly speaking—and perhaps somebody more knowledgeable could correct me on this—this is the reason perfectoid spaces and generalizations (diamonds, v-stacks etc.) are useful for studying local Langlands over non-Archimedean local fields—which are mixed characteristic. This is also the reason none of this machinery would work in the Archimedean case—there's no tilting functor here.

2. **PS**
June 15, 2022

   @Topologist Guy: Yes, that's a very good summary. But note that ~10 years ago very similar arguments were made that the "shtuka" techniques of the function field case can't possibly be applied to p-adic fields, as there's no Frobenius there (and you can't take nontrivial self-products of Spec Q_p etc pp)… but yes, currently it's completely unclear what the right structures over the real numbers would be. (Other than that it will involve some form of the twistor-P1 that's strangely also featuring prominently in other contexts on this blog…)

3. **David Ben-Zvi**
June 16, 2022

   Topologist Guy – definitely perfectoid technology is not relevant in the archimedean case, but there are many recent technological breakthroughs that are important in the Fargues-Scholze work that should be relevant there, in particular condensed mathematics.

   Roughly speaking, the archimedean local Langlands conjecture in the spirit of Fargues-Scholze would relate derived categories of [_] sheaves on the stack of G-bundles on the twistor line and coherent sheaves on a stack of archimedean local Langlands parameters. Here G is a real reductive group, and the semistable G-bundles have as stabilizers inner forms of G. So sheaves on this stack restrict to give representations of these real groups, and this provides the mechanism to relate real groups and Langlands parameters.

   The question is what kinds of sheaves? since we want to realize [admissible] infinite dimensional continuous representations of G, eg unitary reps, we need some notion of sheaves of topological vector spaces, and here one needs condensed (and in particular presumably liquid) technology to make sense of this and to formulate the matching notion of Langlands parameters.

   In fact an archimedean local Langlands conjecture in much the same spirit appears in work of David Nadler and mine from 2007 (and will be reviewed in our IHES summer school contribution with Harrison Chen and David Helm, to be posted soon). The main differences as I understand them from the desired "real Fargues-Scholze" are that we work with parabolic bundles on the twistor line rather than just plain bundles (i.e. we are "tamely ramified" rather than "unramified"), old-fashioned constructible sheaves rather than a condensed version, and a fairly-close-to-traditional version of the space of Langlands parameter – realized as parabolic local systems on the twistor line.

   On the semistable locus, rather than seeing pt/G (and inner forms) we see {complex flag manifold}/G, and the relation to admissible representations of G is not simply "take stalks" but rather "take maps to holomorphic functions" — this is the Kashiwara-Schmid cousin of Beilinson-Bernstein's realization of Harish-Chandra modules for real groups.
So this is less direct than in the eventual expected story, in that we replace representations by their realization on flag manifolds (eg finite dimensional reps get replaced by their realization given by the Borel-Weil theorem). Also I can't imagine that the picture with parabolic bundles is what you want eventually in a more global story, but it's what we can do with old technology.

In a Parallel Universe, Another You

June 21, 2022
Categories: Multiverse Mania, Uncategorized

From today's New York Times, Michio Kaku explains:

> In physics, the concept of a multiverse is a key element of a leading area of study based on the theory of everything. It's called string theory, which is the focus of my research. In this picture, subatomic particles are just different notes on a tiny, vibrating string, which explains why we have so many of them. Each string vibration, or resonance, corresponds to a distinct particle. The harmonies of the string correspond to the laws of physics. The melodies of the string explain chemistry.
>
> By this thinking, the universe is a symphony of strings. String theory, in turn, posits an infinite number of parallel universes, of which our universe is just one.

In this universe I'm on vacation and in no mood to waste time commenting on this crap.

Comments

1. **Daniele Corradetti**
   June 21, 2022

   Would it be an infinite number of parallel symphonies performed only by strings? Or at some point the shape of a drum also comes into play? Just to know whether a PhD in harmonic analysis can have a future…

2. **gio**
   June 21, 2022

   Oh please do it. 😉

3. **Rajendra**
   June 21, 2022

   I hope in a parallel universe, reporters ask Kaku about experimental proof for string theory. 🙂

4. **Brian Flanagan**
   June 21, 2022

   I was listening to Brian Greene push his Many Worlds notion.

   All I asked was, "How many worlds can spin on the head of a pin?"

   That got me banned from the forum.

5. **Sabine**
   June 21, 2022

   I have a new book coming out in August that explains to what extent these ideas are science and how much of it is just belief. Book is called "Existential Physics", details here: http://existentialphysics.com/

6. **Cedric BARDOT**
   June 21, 2022

   > La théorie des supercordes = de la poussière d'orchestre.
   >
   > Like all the others, this French anagram is pure coincidence. It therefore teaches us nothing, but it invites us to ask the question of the link between physics and music, between cosmos and sounds, between harmony and orchestration.

   (Etienne Klein's quote from https://www.radiofrance.fr/franceculture/podcasts/la-conversation-scientifique/l-univers-est-il-orchestre-3624161)

   June 21 is a good occasion to do so as it is a traditional Music Festival day at the place where Peter enjoys his vacations! I wish him pleasant ones.

7. **Low Math, Meekly Interacting**
   June 21, 2022

   Why drag chemistry into it? What awful thing did chemists ever do?

8. **Jim**
   June 21, 2022

   These people got the chairs, got the money and just want more money. The damage they have done to this, the previous and hopefully not the next generation of theorists is unprecedented.

   I was a PhD candidate in 2011, fortunately yours and Lee Smolin's books opened my eyes. I was at CERN when Higgs particle was discovered and people didn't talk loud about the nightmare scenario, which actually happened.

   Greene, Susskind, Kaku and more, don't care that much now. It's like climate change, we don't realize that our children will inherit it since we won't be alive when the nightmare scenario will manifest itself.

   Fortunately I left my theoretical PhD. I do data science, get the money I deserve and I do legitimate,real research on the side, for free, analyzing astronomical data and publishing papers.

9. **Mike**
June 21, 2022

How many alternative universes do I have to run to escape Kaku kaka?

10. **Alessandro Strumia**
June 22, 2022

Jim, a summer student at CERN told me they learnt that the plan was: discover the Higgs in run 1 and SUSY in Run 2. Presumably this has now been delayed to Run 3, so let's silently wait for the start of the symphony.

11. **Rollo Burgess**
June 22, 2022

I recently attended a public talk (at the 'how the light gets in' festival), on the subject of multiverse discourse. Kaku was a panellist, the others being Sabine Hossenfelder and Roger Penrose. The level of nonsense being spoken by Kaku was apparent even to my 10 yo daughter who was sitting next to me, she was irked by an Alice in Wonderland analogy he used… for my part, it just seemed that he continuously changed the subject, flitting between cosmological and everettian multiverse ideas when cornered.

@Sabine, I thought you did a great job of putting your case while remaining calm with this slipperiness. Penrose, while always courteous, exercised the privilege of a 90 year old Nobel laureate by looking pretty scornful.

12. **Doug McDonald**
June 22, 2022

Kaku's piece falls down to below even the level of 'not even "Not Even Wrong" ', down into "performance art of the absurd". Is saying that nasty enough?

13. **Lukasz**
June 22, 2022

I would like to ask you everobody (first of all, Peter Woit), about the reactions of string theorists, to the allegations concerning string theory. I have meant their factual replies i.e. how do they defend string theoretical approach ?
Probably this topic was discussed here, but if so, then I missed this.

14. **Peter Woit**
June 22, 2022

Lukasz,
You should ask string theorists yourself. In my experience most of them try and avoid engaging in any way with criticism of the theory. For Kaku, I wrote a bit here
https://www.math.columbia.edu/~woit/wordpress/?p=12269
about his recent book "The God Equation", which is to some extent his response to criticism of string theory. His way to make a counter-argument is to make claims that are clearly untrue. It seems that doing this kind of thing not only doesn't get you in trouble, it gets you published in the New York Times…

15. **Mike Ball**
June 22, 2022

How about how string theorists respond to Kaku? Im no expert, but its seems likely to me that regardless of your view on the scientific validity of the miltiverse etc, Kaku completely mischaracterises the view of many (most?) string theorists/QFT researchers on these issues.

16. **John Baez**
June 22, 2022

Have you already written about the multiverse movie with Michelle Yeoh, Peter? That takes a suitably slapstick tone.

17. **Shantanu**
June 23, 2022

Does anyone know if Kaku has had Ph.D students working with him and DOE grants for his research?

18. **Peter Woit**
June 23, 2022

John Baez,
Haven't seen the film or written about it. Even when not on vacation I'm now doing my best to avoid the multiverse.

Shantanu,
I don't know of any evidence that Kaku has been doing string theory research since the 1990s. For what's on the arXiv, see
https://arxiv.org/search/?searchtype=author&query=Kaku%2C+M

19. **SteveB**
June 23, 2022

Kaku (and his hair) was the keynote speaker at the annual high performance computing conference in 2012, SC12. This conference usually draws about 5000 people. I attended his talk, but my skepticsm had already been primed here. I didn't like it, but the pictures were pretty. He spent a lot of time on history and then speculated on fairyland-like computer advances. A summary can be found

https://www.hpcwire.com/2012/11/16/michio_kaku_sketches_technological_wonderland_of_the_future_at_sc12/

where he is referred to as a "celebrity physicist".

20. **Chris Oakley**
June 24, 2022

"The God Equation" by Michio Kaku. Conclusion: there isn't one.

“The Bogdanov Equation” by Lubos Motl. Conclusion: there isn’t one.

This situation, I find, is reflected in technical papers in fundamental physics, and long before String Theory was invented. Titles suggest they’ve solved some major problem, but closer examination reveals something infinitesimal.

21. **Topologist Guy**
    June 24, 2022

    Michio Kaku’s most recent entry on the arXiv dates back to 1999.

    https://arxiv.org/search/hep-th?searchtype=author&query=Kaku%2C+M

    It’s frustrating that he continues to present himself as an expert/active researcher in string theory and the string landscape. None of his papers are related to KKLT or string vacua.

    For comparison, arguably the other top popularizer of string theory, Brian Greene, has papers on the arXiv from as recent as 2018, on KKLT/string vacua/landscape,

    https://arxiv.org/search/hep-th?searchtype=author&query=Greene%2C+B

    So I’d say he’s much more qualified to call himself an “active researcher” or “expert” in the string landscape.

22. **David Roberts**
    June 24, 2022

    @Peter

    For hep-th, having only six papers on the arXiv in the 90s looks like practically non-existent. He has string theory research dating back to the 70s, though, so has been a long-term practitioner, from before it was cool.

    Harvard adsabs listings, restricting to refereed items, gives nothing newer since his last arXiv paper from 1999 (it gives some obvious false positives by others with the same name), nor anything in the arXiv era aside from what is on the arXiv.

23. **Bernardo**
    June 24, 2022

    To a lay person like me, albeit one who thinks he is at least able to tell the difference between rational thinking and quackery, Michio Kaku has descended to the level of a Deepak Chopra. Or perhaps he’s always been a Chopra-like “physicist”.

24. **jd**
    June 25, 2022

    I wish to draw attention to the article by Mermin in the latest June issue of Physics Today. In that Mermin shows the role that probabilty plays in quantum mechanics and that the “collapse” of the quantum state is not a physical process. The many-worlds interpretation is eliminated.

25. **Charles Weis**
    June 26, 2022

    Kaku is saying nothing new to establish his legitimacy. More important is what this says about the legitimacy of the New York Times.

26. **Alex M**
    June 27, 2022

    Kaku is the invited Physicist for practically all documentaries involving extraterrestrial affairs, or multiverses quantum ghostly fantastic themes.

27. **Peter Woit**
    June 27, 2022

    Alex M.,
    I’m well aware of that situation. What I can’t figure out is why the New York Times considers that the right credential for asking a scientist to write for them about physics.

28. **Rajendra**
    July 3, 2022

    Intrigued by the responses of Topologist Guy and David Roberts, I did some more search. This is what American Physical Society page on Kaku says

    “As physicists have picked up where Einstein stopped, one solution they have come up with is string theory. String theory combines the two theories by assuming there are multiple universes and dimensions beyond the ones we know.

    After getting his PhD from the University of California, Berkley, Michio joined in on this challenge. He co-founded string field theory, a subset of string theory. String field theory uses the mathematics of fields to explain string theory.

    More evidence and a better understanding of string theory may one day allow us to travel between universes and into new dimensions, potentially even making time travel possible.”

    https://www.aps.org/careers/physicists/profiles/kaku.cfm

    I think endorsements such as these may be partly responsible for NYT’s decision. For a non specialist, those these consecutive paragraphs are as good as saying Kaku is in the same league as Einstein and he invented Time Travel. If the non specialist is a fan of Stranger Things, that’s the final nail in the coffin.

    The question is, why is APS doing this?

ICM 2022

July 3, 2022
Categories: Uncategorized

The 2022 ICM is starting soon, in a virtual version organized after the cancellation of the original version supposed to be hosted in St. Petersburg (for how that happened, see here). The IMU General Assembly is now going on, moved from St. Petersburg to Helsinki. One decision already made there was that the 2026 ICM will be hosted by the US in Philadelphia. With the 2022 experience in mind, hopefully the IMU will for next time have prepared a plan for what to do in case they again end up having a host country with a collapsed democracy being run by a dangerous autocrat.

Registration for following the talks in real time has now been closed, but the talks are being recorded and will appear on the IMU Youtube channel. The program is here.

There will be quite a few other virtual events affiliated in some way with the main ICM, for a list see here. Some of these are traditional satellite conference which have been moved from their originally scheduled version in Russia. An example is this one organized by Igor Krichever, which was supposed to be held at Skoltech in Moscow, but was moved online and hosted by Columbia.

The Fields Medals will be announced at 10am local time in Helsinki on July 5, there will be a livestream here. This will be 3am here in New York, so I'll likely be sleeping and find out what happened later in the morning. Since I just got back from vacation and it's now a holiday weekend, I've been out of touch with my usual sources of math gossip and haven't heard any informed rumors about who the medalists will be. One person who has been mentioned as a possibility is the Ukrainian mathematician Maryna Viazovska.

The last couple times (2014 and 2018) the IMU has put out the news about the Fields Medals to some of the press under unusual embargo terms that made reporting difficult for everyone except Quanta magazine which was given special access (for more about this see here). I haven't heard anything about whether the same thing is happening this year.

Update: just noticed this, indicating that again press access may be Quanta-only.

Update: Antoine Chambert-Loir claims "serious information" that Viazovska will get the Fields Medal (at least that's who he seems to be referring to). It looks like press access is going to more organizations than Quanta this time, see this from Nature. Terry Tao has a blog post with some more ICM information.

Update: The medalists are Duminil-Copin, Huh, Maynard and Viazovska, much the list of names that people have been speculating about. There's much about the winners and their work at the IMU site, and several other press organizations have extensive coverage, including Quanta, Plus Magazine! and the New York Times. Stories about each of the Laureates from Plus Magazine! are featured on the IMU site.

The medalists were chosen quite a few months ago, before the Ukraine war. The interview with Viazovska contains part conducted before the war, as well as a more recent part about Russians and the war (the interviewers were Okounkov and Konyaev).

Update: Barry Mazur was awarded this year's Chern Medal. During the ICM a new documentary about Mazur will be available for watching, Barry Mazur and The Infinite Cheese of Knowledge.

Update: I enthusiastically recommend that you take a look at Andrei Okounkov's remarkable set of popular articles about the work of the four Fields medalists, see here, here, here and here.

Comments

1. **Mike**
   July 4, 2022

   I hope Viazovska gets it. Beautiful and deep work. And it would be great for women in Mathematics. And it would give a small amount of hope to young ambitious Ukrainians.

2. **Deane**
   July 4, 2022

   There was no press embargo. Press was told a few months ago. But, unlike for the Abel Prize, they weren't supposed to interview other mathematicians.

3. **anon**
   July 4, 2022

   No embargo? So the press could report at any time? And as long as a few months ago? Really?

4. **Peter Woit**
   July 4, 2022

   anon,
   I think Deane misspoke, from what I can tell the press was told the names in advance, but not allowed to publish these til the announcement. Unclear if it is the same situation as last time, with only Quanta allowed to interview the winners and get access to other mathematicians for quotes about the work of the winners.

5. **Neville**
   July 4, 2022

   The linked remark of Antoine Chambert-Loir says "I have serious information that one woman will get it." Maryna Viazovska may win a Fields Medal, but she is not the only possible female candidate.

6. **anon**
   July 5, 2022

   Hugo Duminil-Copin, June Huh, James Maynard, and Maryna Viazovska are the Fields medalists. I don't think any of them is a big surprise.

7. **Hermann Vile**
   July 5, 2022

Duminil-Copin, Huh, Maynard, Viazovska – all very reasonable choices.

8. **Deane**
July 6, 2022

Yes, I misspoke. The issue last time was that Quanta knew who the medialists were far in advance but the rest of the press was informed only a week or two before the ICM. There was also a mishap with the New York Times, where the notification went to the wrong person and so the person assigned to the piece had only a few days to write his article.

This time, everybody was told months ahead of time.

Thanks to Peter for reminding me of all this.

9. **Davide Castelvecchi**
July 6, 2022

I cover math prizes for Nature, and I was *not* informed in advance, neither this time nor in 2018. I am not sure if there were other outlets that got advance notice apart from Quanta.

10. **Jim Golden**
July 6, 2022

I thought the Quanta interviews were wonderful, especially for someone like me who's been out of the game for a while. I sent them to some non-maths friends who also really enjoyed the profiles and personalities of the Fields Medal winners.

11. **Deane**
July 6, 2022

Davide Castelvecchi, I'm really sorry to hear that. You should contact Helge Holden about this. My brother Kenneth Chang writes for the New York Times, and he definitely was notified at least 3 months ago. I learned this firsthand from Helge Holden (who was on the site visit committee for the 2026 ICM).

12. **The Abbot of Nalanda**
July 6, 2022

The pop math articles* on the works of the various winners written by Andrei Okounknov for the IMU Fields Medal website are ridiculously good. Their quality reminds me of SciAm articles from the 1980s.

* Called 'Popular Science Expositions' on the site

13. **Z Y**
July 6, 2022

Thanks Abbot, nice articles, it helps that the problems they worked in are relatively accesible

14. **John Baez**
July 9, 2022

The headline of a *New York Times* article announced this:

Fields Medals in Mathematics Won by Four Under Age 40.

A real shocker!

15. **Thomas Larsson**
July 10, 2022

Alas, the text begins with "the Fields Medals, which are given out once every four years to some of the most accomplished mathematicians under the age of 40."

The headline is not usually written by the author of the article.

16. **dh**
July 11, 2022

Peter, I'd be interested to hear your thoughts on Elliott Lieb. To an outsider like me, he looks pretty amazing, much more impressive than this Witten guy everyone's always going on about. 😉

17. **Peter Woit**
July 11, 2022

dh,
I guess this is on topic since Lieb was awarded the 2022 Gauss Prize at the ICM. I didn't write about this because I'm not familiar in any detail with Lieb's work. His work is within conventional old-school mathematical physics, rigorously proving results about statistical mechanical and many-body quantum systems. He has written over 400 papers, over a period of 67 years.

Comparison to Witten is pretty silly and not on-topic, Witten's work has been of a very different nature, with a much bigger impact on math and physics in general.

18. **dh**
July 11, 2022

Sorry for the poor attempt at humor.

~~Strings~~ Black Holes 2022

July 14, 2022
Categories: Strings 2XXX

Each summer for nearly a quarter-century there has been a big yearly conference bringing together the string theory community. I've often written about these conferences on the blog, see here. This year's version will be held next week in Vienna, for more information see here.

Taking a look at the program, one thing that stands out is that the string theory community has almost completely stopped doing string theory. Looking at the program, only two out of 44 talks seem to be significantly about string theory. One of three parallel discussion sessions is entitled "Strings and the Real World" and will be chaired by Cumrun Vafa. I'm guessing this will mostly be about the swampland, not string theory.

A tradition at these conferences is one or more public talks designed to publicize string theory. This year's versions will be given by Netta Engelhardt and Andy Strominger. They have nothing to do with string theory, but they do make very clear what the string theory community has found to replace string theory: black holes. Engelhardt's title is "The Black Hole Information Paradox: A resolution on the horizon?" and Strominger's is "Black Holes: the Most Paradoxical Objects in the Universe".

Looking at the talk titles, the most common words in the titles are "holography" and "black holes", with the center of gravity of the subject now for a couple decades the effort to use holography to say something about black holes. Maldacena's title is "What happens when you look at supersymmetric black holes for a long time?" which seems also an interesting question about the field itself.

Update: Paolo Bertozzini points out to me that the LQG community has scheduled its big yearly conference LOOPS2022 at exactly the same time as the string theory community one (this week). It's quite interesting to compare and contrast the two sets of talks. There are some very broad similarities between what both communities are doing, with overlaps in interest around black holes, entanglement, holography (in the form of large symmetry groups at infinity). Another commonality is that both communities are focused on the gravitational field, with nothing to say about particle physics and matter in general. This has been true of LQG since the beginning. In the case of string theory the big selling point originally was that it gave a theory of matter, but the string community has for a long time given up on that. There is a difference in how the communities think about "what are the fundamental degrees of freedom for gravity?" On the string theory side they've given up on that, the answer now is that gauge-gravity duality and emergence are supposed to allow you not to care about fundamental degrees of freedom. On the LQG side, people are still hard at work on specific sorts of degrees of freedom and how to quantize them.

Comments

1. **Andrei Sipoș**
   July 15, 2022

   Makes sense, black holes are one of the few objects where quantum gravity considerations actually do occur.

2. **Alex M**
   July 15, 2022

   When you give a speech on paranormal activities, and you want the audience to believe, you will try to bring them into the most unknown and unprovable ground. In physic, it is…a black hole!!! Anything can happen there….because we have no cleancut theory to have coherent description of the phenomenon.

3. **Low Math, Meekly Interacting**
   July 15, 2022

   Would you characterize this as a tacit acknowledgment the LHC has given all it can to "new physics"? With gravitational astronomy still in its infancy, seems like the best (if not only) strategy for plausibly (using that word generously) claiming contact with experiment may be just around the corner.

4. **Peter Woit**
   July 15, 2022

   Andrei Sipos,
   Part of what is going on is that long ago most string theorists gave up on saying anything about particle physics and adopted the attitude that "string theory" was now just about quantum gravity. So, then Strings 2XXX started to be Quantum Gravity 2XXX. But this was always not quantum gravity in general, but "quantum gravity approaches that evolved out of string theory, especially holography related". No loop quantum gravity, no R^2 quantum gravity, etc., etc. The concentration on black holes is a further evolution from this.

5. **Peter Woit**
   July 15, 2022

   LMMI,
   For many decades now string theorists and much of the particle physics community have taken the attitude that the limits on energy range of accelerators mean that the thing to do is to move to thinking about astrophysical observations somehow relevant to very high energies, especially very early universe cosmology. "String cosmology" has been featured at Strings 2XXX for a very long time (and never made any sense at all to me, but that's a different topic, and people seem to have lost interest in it).

   Most of the current theoretical activity (solutions to information paradox) around black holes has no plausible relation to astrophysical observations. Maybe there are claims to make contact with things like EHT, but I don't see how EHT pictures are going to tell you something about the quantized gravitational field.

6. **Justin**
   July 15, 2022

   Okay, I have a question: isn't this what you wanted, for people to give up working on string theory?

7. **Peter Woit**
   July 15, 2022

   Justin,
   Sure, except that first of all I think they should admit they have given up on string theory (for one thing, they should change the name of the conference). Continuing to promote string theory as a unified theory to the public when you have given up working on this since it is going nowhere is pretty problematic.

Secondly, while I'm in favor of giving up on string theory, I'm not in favor of giving up completely, which is more or less where the string theorists have ended up.

8. **Michael Weiss**
July 16, 2022

Peter, one of my colleagues suggests this analogy in favor of String Theory (ST): that just as in GR there was a quiet 30-year period (from 1925-1955 with the exception of Oppenheimer and Snyder (1939)), during which the general framework of GR was acclaimed but not as an active field of research–and without further experimental data–so too may we see a 30-year quiescent period for ST (save for its use in studies of Black Holes and as a source of mathematic techniques in condensed-matter) followed by an explosion of renewed activity. This analogy presupposes that ST is the right long-term framework, as GR has proven to be today, i.e., in our ongoing "golden age" of quantitative astrophysics and cosmology. I guess long-term confidence in ST, despite the limited progress in the past 20 years, is a matter of faith for some and not for others.

9. **Peter Woit**
July 16, 2022

Michael Weiss,
The problem with that analogy is that comparing GR to string theory is just delusional. Pre-1925, GR was a huge success: a completely well-defined, simple (when expressed in terms of Riemannian geometry) theory that made a huge number of predictions, including several dramatic ones about small corrections to Newtonian gravity that were quickly confirmed. This is something completely different than "string theory", where you have no predictions and no well-defined theory, just a hope that someday you might find one.

10. **Low Math, Meekly Interactin**
July 16, 2022

I was thinking more gravitational wave (laser interferometry) observatories than VLB interferometers. I recall there being some hype around stringy signatures in the spectra of colliding black holes when the first observations were coming in (circa 2016, I think). Haven't heard much about that lately. Maybe someone has since decided we'll need LISA to see extra dimensions or something along those lines.

11. **Peter Woit**
July 16, 2022

LMMI,
I should have mentioned LIGO/LISA as well as EHT. But the story is always the same: "X will see effects of string theory" claims are all completely bogus, for every value of X.

Not sure which LIGO hype you are thinking of, I mentioned one such here,
https://www.math.columbia.edu/~woit/wordpress/?p=9409
may have not mentioned some others, like this
https://physics.aps.org/articles/v14/s110

The "just wait for LISA" thing has been going on since before my students were born, often as part of string theory hype. For one example from 2006, see here
https://www.math.columbia.edu/~woit/wordpress/?p=418
Note that back then the story was that LISA would launch within ten years…

12. **James Smith**
July 16, 2022

You write:

Secondly, while I'm in favor of giving up on string theory, I'm not in favor of giving up completely, which is more or less where the string theorists have ended up.

Perhaps you should explain this a little.

13. **Peter Woit**
July 16, 2022

James Smith,
The initial selling point of string theory was that it was supposed to unify particle physics and gravity, explaining the standard model. String theorists have given up on anything to do with particle physics, with one justification that the string theory landscape is so vast that you can't predict anything with it, so there is no way to say anything new about particle physics, you just have to give up.

For quantum gravity, the fundamental problem has always been the non-renormalizability of our standard quantum theory of space-time degrees of freedom. Recently what the string theory community has done has been to restrict study to "emergent space-time", with no theory of what degrees of freedom space-time emerges from. In that sense they've also given up on the main problem of quantum gravity.

14. **Max Madera**
July 16, 2022

I loved your last paragraph. No more anthropic, but antropological effects of string theory.

15. **More Anonymous**
July 16, 2022

@Peter,

I'm confused why it's a good idea to directly quantize GR? I mean I would never quantize pressure and energy density which it what the theory spits out for the FLRW metric. I'm curious what your thoughts are?

16. **James Smith**
July 17, 2022

Okay, I kind of see. There is much that I don't understand in your second paragraph but the gist of what you are saying is that you have not given up on the fundamental problem of quantum gravity as you call it, but the string theorists have.

One further question if you don't mind. Is it that the standard quantum theory of space-time predicts the standard model, so to speak, but does not encompass quantum gravity, and solving this fundamental problem would extend that theory or at least produce another that would encompass quantum gravity? Solving this fundamental problem is what you have been working on, right? If so, could the quantum gravity part of such a theory be verified by experiment?

17. **Peter Woit**
July 17, 2022

More anonymous,
The part of our fundamental theory now that explains gravitational forces is a classical dynamical theory of space-time (GR). One way to describe the fundamental degrees of freedom of this theory is as the metric field, but there are many other formulations (e.g. using vierbeins and the spin connection). Applying standard quantization methods to this theory leads to the non-renormalizability problem.

If you want to argue that these degrees of freedom are not fundamental, that they are just effective low energy degrees of freedom coming from some more fundamental theory, that's fine (this is what string theorists were originally doing, the metric was just a low energy mode of the more fundamental string theory). But you need to explain what your more fundamental theory is. The current fashion is to announce that "space-time is doomed", that the metric is just an effective field, but not to give a new proposal for a consistent fundamental theory which exhibits this behavior. It's this move that I would call "giving up".

18. **Peter Woit**
July 17, 2022

James Smith,
The "standard quantum theory of space-time" doesn't say anything about the standard model, where the fundamental degrees of freedom are "internal", not space-time degrees of freedom. It also has the renormalization problem.

A conventional approach to unification is to extend one's notion of space-time by adding more dimensions, then trying to interpret these new degrees of freedom as the internal degrees of freedom of the standard model. I'm trying to do something a bit different, exploiting different aspects of geometry that appear when one works in Euclidean space-time and with twistors. I'm still very enthusiastic about these ideas (now writing up a new version of part of this), but there's a lot to be done to get these ideas to work, and still to be seen what new implications one might get from a fundamental theory formulated this way.

19. **James Smith**
July 17, 2022

Thank you for the clarification.

20. **thomas van riet**
July 18, 2022

"I'm guessing this will mostly be about the swampland, not string theory." The Swampland papers (apart from the heuristic pheno papers) are very stringy. By construction of what the Swampland means. I invite you to look at the average paper on the topic and you will strings everywhere

21. **Peter Woit**
July 18, 2022

thomas van riet,
If "swampland" now divides between "formal swampland" and "phenomenological swampland", I was referring to the latter, since the session is about "String theory and the real world". The attempts I've seen to relate the "swampland" to the real world have little relation to string theory, basically are very speculative conjectures about "any quantum theory of gravity".

22. **pontifex**
July 18, 2022

It seems that words as "unified field theory" or "super-symmetry" have no meaning, or any meaning that suits you, in pop culture now, see this article in The Guardian:

http://www.theguardian.com/film/2022/jul/18/empire-of-the-drones-terrence-howard-takes-us-where-no-actor-has-gone-before

23. **Shantanu**
July 20, 2022

Peter, in the Loops2022 conference the have invited Neil Turok (who although is not a core string theorist) has worked on cosmic strings and branes.
Also does any one have any data on amount on NSF/DOE funds on LQG vs string theory (in US)?

24. **Peter Woit**
July 20, 2022

Shantanu,

I don't think this kind of question has made any sense for a long time now. "String theory" long ago stopped referring to a specific theoretical framework. Most "string theorists" stopped working on string theory and "string theorist" now is just a name for a tribal affiliation, with a wide spectrum of intensities of that affiliation. Turok is definitely not now significantly affiliated with the tribe, especially given the kind of things he has been saying about string theory since 2016, such as
"[String theory has] almost self-destructed, I would say because it turned out to be not just one theory but this vast collection of theories which could all give different descriptions of the world."
See https://www.math.columbia.edu/~woit/wordpress/?p=8361
At this point it would be surprising to see him at Strings 2022, not at LOOPS22.

My general impression is that there's relatively little funding of LQG research in the US right now, much more in Europe. One reason for that is possibly that the string theory tribe in the US has always been quite hostile to LQG, seeing it as a heresy.

25. **vmarko**
July 25, 2022

Hi Peter,

"Another commonality is that both communities are focused on the gravitational field, with nothing to say about particle physics and matter in general."

The Loops22 conference has had 4 talks (out of a total of 100 or so) that were about matter, in one way or another. So formally there is nonzero interest about matter in the LQG community. That said, I completely agree that mere 4% of the talks represent nowhere near enough interest that IMHO the community should have about the topic. So I agree it is a bit sad state of affairs.

Best, 🙂
Marko

26. **Alex**
July 26, 2022

I don't see why the concentration only on the gravitational aspect from the part of the LQG community is something supposedly bad.

The idea that in order to do QG you need to include matter and actually have a unified ToE is old and coming mainly from the particle physics thinking of the 70s and one of the main motivations that gave us string theory (from that same community).

From the beginning, the LQG community has explicitly rejected such hypothesis and considers it as not necessary for a QG theory. That's their hypothesis. I don't know if it's right or wrong, but if we start to argue over my hypothesis vs yours, we go back all the way to the string wars again.

I think LQG has more important problems to deal with (in particular, empiral testing issues etc.) than its publicly acknowledged hypotheses!

If the infamous string wars showed us something, it's that nobody is going to change their mind by such discussions. The only reason why string theory is now falling into some disgrace is because many string theorists themselves have given up due to the issues dealing with reproducing the SM and the non-discovery of susy in the LHC. So, let the LQG community work with what they have and believe and see where that goes. Perhaps they will also give up, perhaps they will realize that the formalism itself is asking for non-gravitational fields to be included, etc.

Personally, I see no chance of connection with the empiricial realm for any QG theory candidate at least in the coming 100 years, so I predict a long QG winter after the final fall of string theory in the near future (yes, I know that they are all doing "QG" now, but how far goes their influence beyond Princeton? They are beyond recovery now, they lost the most important thing for such speculative endeavors: credibility. It's a bit like the stock market, and their stocks are certainly going down after a big bubble).

27. **vmarko**
July 27, 2022

Alex,

"I don't see why the concentration only on the gravitational aspect from the part of the LQG community is something supposedly bad."

There's nothing intrinsically wrong with doing QG without matter, but the issue is that there is only so much you can do with pure gravity. It is similar to doing vacuum GR — there are some interesting solutions and nontrivial geometry, but if you want to study the really cool phenomena and model realistic physics, you need matter in your theory. My overall impression from listening to all the talks at Loops-22 (especially the panel discussion at the end) was that we have sort-of constructed a well-defined theory of QG, and now we don't know what to do with it.

The most interesting set of problems in QG require coupling of matter. For example — what is the equation of state for matter fields at the center of the black hole? What is the corresponding curvature? What is the state of the gravitational field sourced by matter in quantum superposition? What are the effective EoMs for matter in a gravitational field in quantum superposition? How does matter emit and absorb individual gravitons? How does one properly derive QFT in curved spacetime as an approximation to a full theory of QG? Can we resolve the CC problem? Is there a limit to the validity of the equivalence principle? Etc… None of these questions can be answered without coupling matter to QG.

And if you try to couple it, you immediately face the question what kind of matter, leading you to the SM, dark matter, their various extensions and beyond to ToE. That said, I agree that the question of matter spectrum (i.e. what are fundamental matter fields and their coupling constants) may or may not be related to the ability to couple matter to gravity. So far we do not know of any restrictions on matter spectrum coming from gravity. But we cannot tell either way if nobody does the research.

Best, 🙂
Marko

28. **Alex**
July 27, 2022

Marko,

Yes, I definitely agree that those are important problems and the matter issue will have to be addressed at some point for that.

What I was thinking is that vacuum QG is conceptually more interesting and complicated than vacuum GR, since you now have to deal with things like: are distances, areas, volumes, proper times, discrete and probabilistic? What happens to the continuum manifold? What's even a quantum worldline? It's like going to GR 101 and start all over again with the presentation of the very basic stuff, like manifolds and curves, but now with their quantum versions.

Then it would be interesting to have phenomenological models that take those predictions and some effective model of the rest of matter, and see if we can at least test that. Of course, the full coupling between QG and quantum matter would be even better, but I think it's just too much to ask for the current state of development.

In the case of LQG, I don't think it has even solved in a completely satisfactory way any of those problems I mentioned. So, I maintain my prediction of a QG winter period starting in the near future (like the one that happened to AI in the 70s and 80s). I actually think it will be good for the field. Many people that still are prominent need to retire, since their current "research" is utter pseudo science (in both camps, strings and loops). Furthermore, technology has to evolve. Who knows, maybe in a couple of decades or even in 100 years, a fresh new generation will be able to make the breakthrough.

29. **Mitchell Porter**
July 28, 2022

String phenomenology has its own conference, String Pheno 2022. There are plenty of would-be applications of string theory to particle physics there.

30. **Peter Woit**
July 28, 2022

Mitchell Porter,

The point of my post was not that Strings 20XX talks no longer have any connection to the real world (that happened long ago), but that now they no longer have any connection to strings either. I haven't written in a while about "String Pheno 20XX" conferences, since the subject has become so bizarre that it's hard to figure out what to say. At least in the US, almost all leading "string theorists" except Vafa have now long ago abandoned any interest in "string phenomenology", realizing that it's a hopeless failed program. Why there are so many people and conferences devoted to a subject that almost everyone regards as a complete failure is something I'll never understand.

Before the Big Bang: The Origin of the Universe from the Multiverse

July 29, 2022
Categories: Book Reviews, Multiverse Mania

There's a new book out this month, Before the Big Bang: The Origin of the Universe from the Multiverse, about which we're told:

> One of the world's most celebrated cosmologists presents her breakthrough explanation of our origins in the multiverse.
>
> In recent years, Laura Mersini-Houghton's ground-breaking theory, spectacularly vindicated with observational evidence, has turned the multiverse from philosophical speculation to one of the most compelling and credible explanations of our universe's origins.

I spent a few minutes today looking through the book in the bookstore, trying to figure out where to find the details of the "spectacularly vindicated with observational evidence." I didn't see any references in the book, just a claim that in 2018 the author collaborated with Eleonora Di Valentino on showing vindication by observation. Presumably this is a reference to these three papers, but who knows. I don't see anything like that in a quick look at the papers.

For many years I've spent a significant amount of time reading books and papers purporting to offer scientific evidence for a multiverse, trying to carefully understand the author's arguments and write about them here (one example involved earlier claims by this author, see here). Few physicists though seem to care that bogus claims and pseudo-science about the multiverse have overrun their field and become its public face. I've come to the conclusion that best to not waste more time on this.

Update: Will Kinney reminds me that he wrote a paper about this, see here, as well as here and here for more about the story of that paper. Also see another old posting, here.

Comments

1. **Will Kinney**
   July 29, 2022

   Peter, I wrote a paper examining the claims of evidence for the model in light of the Planck data here:

   https://arxiv.org/abs/1606.00672

   Long story short, there isn't any. Only lower bounds on the relevant scales. If you remember, this paper caused a bit of a kerfuffle with LM-H 😉

2. **Peter Woit**
   July 29, 2022

   Thanks Will, I'd forgotten that story, even though I wrote about it at the time here. I'll add some links to the posting.

3. **Larry Lurio**
   July 29, 2022

   It is not a waste to respond to each and every exaggerated scientific claim that makes its way into a serious published book. They eat away at peoples confidence in the scientific endeavor and diminish the value of more mundane scientific theories that are actually supported by evidence earned by sweat and long hours of experimental work.

4. **Marty**
   July 29, 2022

   Re: Waste of time responding to multiverse nonsense:

   I suppose back in the 2000s you could have taken a similar point of view regarding string theory hype. But you didn't, and it made a real difference. A positive difference for theoretical particle physics, I'd say.

   I'd also say your efforts helped save the careers of at least a few PhD students who might have otherwise focused on string theory. I don't know if I would have ended up going down the string theory path without your perspectives, but a fellow grad student didn't heed your warnings and I think his future in theoretical physics suffered.

5. **Peter Woit**
   July 29, 2022

   Will,
   Took a quick look and, funny thing, the 2016-8 papers claiming to have Planck evidence don't reference your earlier 2016 paper claiming there isn't any. Perhaps the authors somehow missed hearing about your work, but I would have thought the referees would have pointed it out to them.

6. **Will Kinney**
   July 29, 2022

   Peter —

   Pretty sure they didn't miss it.

7. **Alex**
   July 29, 2022

   "spectacularly vindicated with observational evidence"

   Just… wow. I'm speechless on how spectacularly outrageous that is. These people have really destroyed science popularization. Just imagine the amount of lay people that will read that and believe it. A bit of exaggeration, teatrics, histrionism is okay I guess, since you must catch the public's eye in a competitive market. But these are just blatant lies. Just lies, lies and more lies. What the F is wrong with these people? Do they live in an alternate reality? How can this happen? What happened to institutions, to book editors, etc.? This strange world is like Orwell's 1984, physics version!

I'm really baffled and depressed by seing things like this.

8. **Nirmalya Kajuri**
July 30, 2022

I remember her from her old hit 'black holes don't exist', based on a flawed argument whose flaw was already understood in the 80s.

9. **Peter Woit**
July 30, 2022

The commenter above is referring to this paper
https://arxiv.org/abs/1409.1837
discussed for instance here
http://backreaction.blogspot.com/2014/09/black-holes-declared-non-existent-again.html
and here
https://arstechnica.com/science/2015/03/completely-implausible-a-controversial-paper-exists-but-so-do-black-holes/

10. **Anonymous theoretical physicist**
July 30, 2022

Peter: in case you feel that you are engaged in a Sisyphean task, I should perhaps comment that I first discovered your blog when I was entering grad school back in the aughts, and it was instrumental in convincing me to avoid string theory (and more broadly, high energy physics), and I do believe you very likely saved my academic career in the process, before it had even begun. So: thank you.

11. **David Roberts**
July 31, 2022

From the Ars Technica article

> In order to solve the firewall paradox, Mersini-Houghton wasn't thinking small: she claims to have unified general relativity with quantum mechanics. "Physicists have been trying to merge these two theories—Einstein's theory of gravity and quantum mechanics—for decades, but this scenario brings these two theories together, into harmony," said Mersini-Houghton in a press release. "And that's a big deal."

I guess this fell through…

12. **Alessandro Strumia**
July 31, 2022

What started the phenomenon of theory books with ridiculously exaggerated claims? String hype? Or maybe it started with Hawking, who had real interesting science plus medical bills, so physicists turned a blind eye to minor exaggerations? With the result that, once the anti-bullshit dam is broken, going back is difficult.

13. **Peter Woit**
July 31, 2022

Alessandro Strumia,

I think Hawking's 1988 "A Brief History of Time" had a big effect, not by opening the subject to exaggeration, but by unexpectedly making huge amounts of money. Publishers got the idea that a fundamental physics book, especially if written by someone with an interesting story, was worth publishing, because they might hit a jackpot. By the way, absent a full-time self-promotion campaign, most such books don't end up making much, the author often could have made more money by not writing the book and instead taking a part-time job dog-walking…

The post 2004 flood of pseudo-science books about the multiverse was enabled by publishers hoping to get lucky, but more importantly by the collapse of any intellectual standards in the field, and having no real fundamental physics science to write about (one can only sell so many books about the Higgs).

When I was growing up, there were plenty of books for sale about bogus physics. But they weren't written and/or blurbed by prominent theorists at elite academic institutions, so it wasn't hard to tell the difference between those and real science.

14. **Will Kinney**
July 31, 2022

Peter, Alessandro —

I tried to handle the inflationary multiverse with honesty and nuance in my book, for what it's worth. Jury is still out as to how well that sells.

15. **Peter Woit**
July 31, 2022

Will,
I did quickly read through your book, which seemed significantly better than the usual multiverse lot. Didn't write anything here just because writing anything substantive would have required time and energy I don't have, as well as patience I now lack for engaging with this material. Good luck with sales, although I fear taking a more sensible point of view may mean you'll do even worse than the dog-walking gig…

16. **Sleptovia**
July 31, 2022

Peter–why is Brian Green's book "The Hidden Reality: Parallel Universes and the Deep Laws of the Cosmos," better than Laura Mersini-Houghton's book "Before the Big Bang: The Origin of the Universe from the Multiverse"?

Here you write, "My colleague Brian Greene has a new book coming out soon, The Hidden Reality: Parallel Universes and the Deep Laws of the Cosmos. I haven't seen a copy, but from what I can gather, it looks like it is probably the best of the many books about "multiverse" ideas,. . ." –https://www.math.columbia.edu/~woit/wordpress/?p=3389

What exactly makes Brian's book so much better than Laura's and the rest?

Thank you!

17. **Peter Woit**
July 31, 2022

Sleptovia,
You deleted the rest of the sentence ("but still not exactly my cup of tea."), as well as the part telling people to look for experimental evidence of any multiverse claims. From what I remember, Brian doesn't claim any. The big difference with Mersini-Houghton is that she does make such claims (and as far as I can tell, they are bogus).

By the way, if one wants to evaluate multiverse books, I think Will Kinney's is better than Brian's.

Glashow Interview

August 2, 2022
Categories: Uncategorized

David Zierler, the oral historian at the American Institute of Physics, has done many in-depth interviews with theoretical physicists in recent years. Today I came across a 2020 interview with Shelly Glashow, which was very interesting in general, and also answered a question I had always wondered about. Glashow was my undergraduate advisor at Harvard, where I was a student from 1975-79. From what I remember, his office was more or less next door to Steven Weinberg's. It was well-known that they had been close friends, in the same class first at Bronx High School of Science, and then at Cornell. Towards the end of my time at Harvard I heard that their friendship was over and they were barely on speaking terms, but I never knew what had happened. In the fall of 1979, they were (together with Abdus Salam), awarded the Nobel Prize for their work on the unified electroweak theory.

In the interview, Glashow explains the story from his point of view:

> Glashow:
> by the late 1970s I began to think of myself as a Nobel contender. But I was under the impression that my old friend Steven Weinberg was doing everything in his power to keep the prize for himself and Salam. In particular—at a conference that he attended in Tokyo—he went out of his way to avoid mentioning my name at all while presenting the history of weak interaction theory. I got very upset by that omission. It was the issue which terminated our friendship. In the summer of 1979, I was invited to a meeting in Stockholm, to discuss the current state of physics ideas and others. Prior to the meeting, I sent a transcript of my talk to Steve. He was violently against my giving the talk. Because it examined various alternatives to what was then known as Weinberg/Salam theory. In fact, it was an open-minded talk in which I was discussing whether their—or more properly—our theory was a correct one or not. But it was such a heated discussion that I eventually had to simply hang up on him, because I had no intention of revising my talk. And I did not.
>
> Zierler:
> Was his assessment of your paper accurate in your mind?
>
> Glashow:
> I did talk about alternatives to the Weinberg-Salam theory. Yes. I was not yet convinced that it had to be true.
>
> Zierler:
> And what was your sense of why this was so unacceptable to him?
>
> Glashow:
> He thought it would endanger the Nobel Prize that he had campaigned for and anticipated for Salam and himself.

A copy of Weinberg's Tokyo paper is here.

In the interview Glashow is scornful about Salam's work and the campaign to get him a part of the Nobel Prize:

> Glashow:
> … Recall that Salam made a great deal of noise about why the prize should be given to he, Salam. I've been told that there were dozens and dozens of nominations of Salam. In fact, there's a whole paper written about his shenanigans, which I can refer to you; written by Norman Dombey. Everything he says is true, to my knowledge….
>
> My Nobel Prize depended on that one paper written in 1960. Steve's Nobel Prize depended exclusively on that one paper he wrote in 1967, a wonderful paper which applied the notion of spontaneous symmetry breaking to the—my electroweak model. So, the question arises, what did Salam do? He introduced the electroweak—the SU(2)XU(1) model in 1964. That was over three years after I did. He copied my work but did not cite me…
>
> Zierler:
> Do you want to comment on why then he would have been a co-recipient of the Nobel Prize with you for this copy of your work?
>
> Glashow:
> I'll explain it in a moment. But let me come back to—he also claims to the first to introduce spontaneous symmetry breaking in the paper that he wrote in 1968, one year after Steve wrote his paper. But that paper even cites Steve's paper, so it is hardly the first time. He did what each of us had previously done, but much later. So why did he get a Nobel Prize? Very simply, he was nominated many times. Because he was Director of the International Center for Theoretical Physics in Trieste, Italy and he was very close with the directors of physics institutes in many countries; almost 100 of different institutions. And many of them wrote letters, by his instruction, using his words in some cases, encouraging the Nobel Committee to give the prize to him and also Steven. All of this documented, in fact, by the paper by Norman Dombey, who had access to Salam's files in Italy, and has copies of the letters that he sent to other people encouraging them to nominate him. So, I think he shared the prize because he made a point of doing just that.

I wrote something on the blog about Donbey's claims here.

Zierler also asks Glashow some questions about string theory, a topic on which Glashow's views have been consistent from the beginning:

> Zierler:
> In retrospect, Shelly—how well do you think—has both string theory and your criticism of it aged over the past 30 years?
>
> Glashow:
> Well, it's hard to answer that. String theory has become an established part of physics departments throughout the world, more so in Europe than in America. We still have some universities which are proudly string-free, like Boston University. We also have an awful lot of string theorists around who are twiddling their thumbs. It is not clear that string theory is going anywhere. I expect that string theorists would disagree with that assessment. But they are actually considering many other circumstances such as black holes in other spaces than ours, and there are all kinds of interesting things being done in mathematics, in physics, elsewhere by string theorists but with no relationship to the questions that interest me. They cannot answer the questions they set out to answer. That much is clear.
>
> Zierler:
> That's as clear to you—
>
> Glashow:
> That was clear from the beginning, I think…
>
> Glashow:
> … I no longer feel so strongly about string theory. Why beat a dead horse? String Theory does not answer the questions that I'm interested in. I'm sad about that. I

hope that they're wrong. I have no reason to think that their horse is, in fact, dead, but it's dead from the point of view of being useful to my way of thinking about physics. And I think that many experimenters feel exactly the same way, because string theorists say nothing about experiments that have or could be done. They only speak of experiments that cannot be done, which is somehow not interesting.

Update: Robert Delbourgo wrote in to point to his description of what happened in 1967. Here's the relevant part:

> I have been asked by the organizers to comment upon the the birth of the standard model during 1967 and Salam's prominent role in it. This is an excellent occasion to set the record straight and recount my view of its history; if nothing else to refute innuendos which have occasionally surfaced during the 1970s that Salam was not deserving of the Nobel Prize. That autumn of 1967 I had been in charge of organizing the seminars at IC. Because Salam was constantly on the move and hardly spent more than one month at a stretch in London, I arranged with him to give a couple of lectures on his recent research (in October, to the best of my recollection) during his spell at IC to kick off the seminar season, as it was early in the academic year. He agreed to do so even though the audience attending those talks was somewhat thin. Paul Matthews was certainly present, but Tom Kibble was away in sabbatical in the USA. My memory of his lectures is a bit indistinct nowadays, but I do remember that he kept on invoking these k-meson tadpoles which disappeared into the vacuum which induced the spontaneous breaking of the gauge symmetry: what we now know as the expectation value of the Higgs boson. The resulting model looked rather ugly – and it still is – and I admit that I paid little attention to it; nor do I think that Salam himself was especially enraptured by the model's beauty. A week or so later, I wandered into the Physics Library and came across Steven Weinberg's Physical Review Letter, which I noticed looked suspiciously like Salam's attempt. I showed the article to Salam, who was rather troubled that it was almost the same as his own research, but which was of course entirely independent. Matthews and I urged him to publish his work at the earliest opportunity and this happened to be the upcoming Nobel Symposium. As they say, "the rest is history". I hope that this account of the events at the time scotches all aspersions that Salam should not have been a prize recipient.

Comments

1. **Chris**
   August 2, 2022

   Peter, thanks for an interesting post. Did you happen to see where the audio recording of the interview is? I could only find the transcript.

2. **Brathmore**
   August 2, 2022

   Do mathematicians lobby for, and fret about, whether they'll win the famous math prizes? Or is this just in the sciences? Contrast Weinberg/Glashow with Perelman or Scholze. As science has become so interdependent, the Nobel limit on 3 per year is as stupid as the madness it engenders.

3. **S**
   August 3, 2022

   "They only speak of experiments that cannot be done, which is somehow not interesting."

   That seems a generous take.

4. **Georges E. Melki**
   August 3, 2022

   After reading this article, I lost most of the respect I had for both the Nobel Prize and its recipients! How could those so-called "great men" be so scornful of each other? How could they be bickering like this about precedence and priority in their fields? And what is the Nobel committee doing? What a shame!

5. **Steven Malarkey**
   August 3, 2022

   Why beat a dead horse?

   Perhaps something for us all to ponder.

6. **Geoffrey Dixon**
   August 3, 2022

   Having taught an undergrad course in particle physics at Harvard with Glashow, and rubbed elbows with Salam in Trieste, little in this surprises me. I only attended a couple of talks by Weinberg, but … My short assessments of the 3, in the order mentioned: good guy; preening; aloof.

7. **Anonyrat**
   August 3, 2022

   Glashow mentioned his summer 1979 talk here:
   https://inference-review.com/article/in-memoriam-steven-weinberg
   And gave a citation:
   https://iopscience.iop.org/article/10.1088/0031-8949/20/2/022
   Scenarios for Physics at LEP
   Sheldon L Glashow
   Published under licence by IOP Publishing Ltd
   Physica Scripta, Volume 20, Number 2
   Citation Sheldon L Glashow 1979 Phys. Scr. 20 283

8. **Anonyrat**
   August 3, 2022

   Glasgow's 1979 talk:
   https://inis.iaea.org/collection/NCLCollectionStore/_Public/10/470/10470384.pdf

9. **zzz**
   August 3, 2022

   that sort of politicing for a Nobel is not uniq to physics

10. **Peter Woit**
August 3, 2022

Anonyrat,
Looking at the Glashow spring 1979 Stockholm talk paper, I see what upset Weinberg. On page 309 Glashow claims that current phenomenology was consistent with a different model, with “Weinberg” angle 0. Weinberg likely took that as an explicit attempt to go before the Nobel committee and tell them that Weinberg’s model might be wrong so they shouldn’t give a prize for it.

11. **Peter Shor**
August 3, 2022

Chris asks:

> Did you happen to see where the audio recording of the interview is? I could only find the transcript.

I am fairly sure that for this series of interviews, they don’t release the original recording but just the transcript. I don’t know why—this is pure speculation, but maybe it’s because they think more people would be willing to be interviewed this way.

12. **Anonyrat**
August 3, 2022

Peter,

In the first link, Glashow wrote:

“I was invited to speak about the weak interactions at a conference in Stockholm in the spring of 1979.2 While in Stockholm, a member of the Nobel Physics Committee delighted in telling me that the Weinberg angle appearing in the Weinberg–Salam model was identical to the angle I introduced in my 1960 paper. I was delighted as well.”

The “weak mixing angle” was invented by Sheldon Glashow in his 1961 paper, “Partial-symmetries of weak interactions.”

13. **Peter Woit**
August 4, 2022

Anonyrat,

That’s why I put “Weinberg angle” in quotes. If you were around Glashow and his collaborators during the late 70s you were well aware that it wasn’t discovered by Weinberg…

14. **Palinuro**
August 4, 2022

Once I had the chance to talk with Glashow, and ask him when he was convinced that his model was true, he said that after neutral currents were observed, not 1961, not 1967 or 1971.

Weinberg, great physics and books, but in both of them it is like if Veltman has not existed.

15. **curiouser**
August 6, 2022

It’s hard to understand why the theory, at least initially, was called the Weinberg/Salam model, and why “Weinberg was doing everything in his power to keep the prize for himself and Salam”, if all Salam did was to steal from Glashow and Weinberg. It’s unfortunate that Weinberg and Salam are no longer around to defend themselves against what sounds like childish bickering fifty years after the fact. Maybe they wouldn’t have cared to.

16. **Peter Woit**
August 6, 2022

curiouser,

I’m also curious where the term “Weinberg-Salam”, with non-alphabetic ordering, first appeared. If Salam had independently discovered this model and written it down, why not “Salam-Weinberg”? The model got zero attention until after ‘t Hooft showed renormalizability. ‘t Hooft in his earliest mention I can find
https://webspace.science.uu.nl/~hooft101/gthpub/massive.pdf
refers to the model as Weinberg’s model, no mention of Salam.

For much more detail about this story, I recommend Frank Close’s book “The Infinity Puzzle”.

From everything I’ve read, Glashow has a good argument that Salam should not have gotten recognition for this. Writing a paper in 1964 describing the same model someone else well-known working in the same field had published three years earlier is not the kind of thing one normally gets any kind of credit for. Salam claimed to have independently discovered and discussed the Weinberg 1967 model for leptons in a series of fall term (Oct-Dec.) 1967 lectures at Imperial. There are no records from anyone of what he discussed in those lectures. Weinberg’s paper went to PRL Oct. 17 1967, was published Nov. 20, so quite possibly would have been available to Salam at some point during that period (there’s zero chance he would not have paid attention to a new paper from Weinberg).

Glashow is almost 90 years old, and has every right (and arguably a duty) to try and set the historical record straight about the Salam story. About Weinberg, what he has to say has nothing to do with the science, it’s purely about their interpersonal conflict and why he stopped speaking to Weinberg. It seems to me he has every right to publicly explain that if he wants to.

Weinberg was alive when Glashow was interviewed, but now can no longer respond. I doubt he would have wanted to. One can make a guess about how he felt about this: in 1978-9 he was clearly one of the first in line for the next physics Nobel Prize, perhaps had little patience for things that might interfere with this (such as arguments over claims about an earlier, incorrect, version of his model, or claims that it might yet turn out to be wrong).

17. **Shantanu**
August 7, 2022

Peter : in NASA/ADS you can do a full-text search
NASA/ADS is not always 100% complete in terms of Physics papers but according to this, the first paper which used “Weinberg-Salam” model is

https://ui.adsabs.harvard.edu/abs/1972PhRvD…6.2023M/abstract
I don't think inspirehep does a fulltext search. If it does someone can search via that too.

18. **tulpoeid**
August 12, 2022

Very interesting historically, thank you. The ending with "experiments that cannot be done" is also gold.
I remember almost fifteen years ago Glashow giving a talk at CERN. He made several jokes and everyone laughed. At some point he made a good joke against susy. Only me and a couple other people in the auditorium laughed (but at least we laughed loud).

19. **Robert Delbourgo**
August 12, 2022

Peter, please see my comments on page 3 of the Salam Memorial volume in the article "the force and gravity of events". I will leave it at that. I am an old guy now and you will just have to believe me. I only wish I had kept written notes of Salam's talks in autumn 1967. At the time I thought the whole edifice was rather ugly, especially the way he introduced the tadpole diagrams, which represented spontaneous symmetry breaking in his description.

20. **Peter Woit**
August 12, 2022

Robert Delbourgo,
Thanks for writing. I've added your comments that you point to as an update to the posting.

It still seems to me that it remains unclear what exactly Salam knew and lectured about before he had access to Weinberg's paper.

Looking back on the history of the electroweak theory, one thing that strikes me is how little interest everyone (including their own authors) seemed to have in these models. It took three years for anyone (whether Weinberg and/or Salam) to bother to put the Higgs mechanism together with a unified electroweak theory, and then that was ignored by everyone for another three years or so until 't Hooft/Veltman. Both Weinberg and Salam don't seem to have been particularly enthusiastic at the time about the idea that won them a Nobel Prize.

21. **shantanu**
August 12, 2022

Peter: something OT. I just accidentally noticed that 1975 nobel prize winner (and another Schwinger student just like Glashow) Ben Mottleson passed away in May . Despite getting a Nobel prize in Nuclear Physics and working in this area for 70+ years, I am surprised that there is no tribute to him on this or any other blog or from other physicists (which I could find). Was his work not well known to particle physicists despite getting a Nobel prize?

22. **Peter Woit**
August 12, 2022

Shantanu,
Nuclear physics is a quite different subject than high energy particle physics, and in general I think HEP physicists don't know much nuclear physics (during my Ph.D. I learned the minimum to pass general exams). In Mottleson's case, the Nobel-winning work was done 70 years ago. All I knew about Mottleson was that he did something with Bohr a very long time ago, surprised to hear he was still with us until recently.

23. **Peter Orland**
August 12, 2022

Hi from Copenhagen. Ben Mottleson won the prize with Åge Bohr, not Niels.

24. **Peter Orland**
August 12, 2022

He may have also worked with Niels, although I am not aware of it.

25. **Chris Austin**
August 12, 2022

Peter, "It took three years for anyone (whether Weinberg and/or Salam) to bother to put the Higgs mechanism together with a unified electroweak theory,"

From Weinberg's own account, on pages 2 to 6 of his Nobel Lecture, he was at first rather disturbed by the result that spontaneously broken continuous symmetries lead to massless Goldstone bosons. But in 1964, the year when the papers on the Higgs mechanism by Higgs; Englert and Brout; and Guralnik, Hagen, and Kibble appeared, there was another development, which suddenly seemed to change the role of Goldstone bosons from that of unwanted intruders to that of welcome friends. This was the Adler-Weisberger sum rule, which gave the ratio g_A/g_V of axial-vector to vector coupling constants in beta decay in terms of pion-nucleon cross sections. One way of looking at this result was to suppose that the strong interactions have an approximate symmetry, based on the group SU(2) x SU(2), and that this symmetry is spontaneously broken, giving rise among other things to the nucleon masses. The pion is then identified as (approximately) a Goldstone boson, with small non-zero mass, an idea that goes back to Nambu. Weinberg spent the years 1965-67 happily developing the implications of spontaneous symmetry breaking for the strong interactions. In 1967 he thought of trying out the idea that perhaps the SU(2) x SU(2) symmetry was a "local," not merely a "global," symmetry. He considered a model where in addition to the vector ρ mesons of the Yang-Mills theory, there would also be axial vector A1 mesons. But he was not too enthusiastic about it. Then at some point in the fall of 1967, it occurred to him that he had been applying the right ideas to the wrong problem. It is not the ρ mesons that are massless: it is the photon. And its partner is not the A1, but the massive intermediate boson, which since the time of Yukawa had been suspected to be the mediator of the weak interactions.

https://www.nobelprize.org/uploads/2018/06/weinberg-lecture.pdf

26. **Peter Woit**
August 12, 2022

Chris Austin,
As Weinberg explains, he and everyone else were focused on the strong interactions, trying to use spontaneous symmetry breaking and the Higgs mechanism to understand that. A lot of the reason why no one for years applied the Higgs mechanism to the electroweak theory was that everyone was thinking about the strong interactions, not the electroweak interactions.

27. **pontifex**
August 12, 2022

A nice rendering by Weinberg of the story of his realization that he had been trying the right idea for the wrong theory is here

http://www.ictp.it/media/1867057/100reasons-Weinberg.pdf

28. **Hans**
August 13, 2022

Peter,

my own interaction with Glashow was about 10 years ago was with a gentleman. I wrote him to ask him whether he thought that the weak and the electromagnetic interaction had actually been unified, or whether the two interactions mix in a specific way. (I lost the mail exchange due to a hard disk crash…)

He answered that this was not clear-cut, and that mixing was an acceptable and possibly better description.

For somebody who received the Nobel prize for this unification, I found this quite impressive.

Symmetry and Physics

August 6, 2022
Categories: Quantum Mechanics

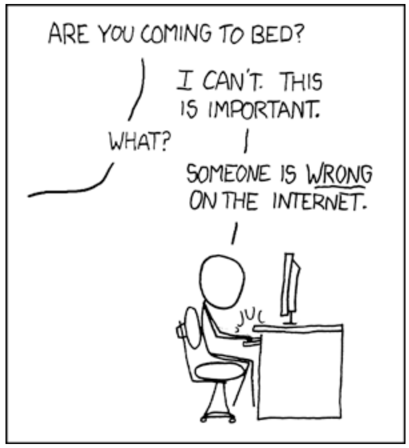

It's getting late, but I can't help myself. Reading too many wrong things about symmetry and physics on Twitter has forced me to do this. And, John Baez says I don't explain things. So, here's what the relationship between symmetry and physics really is.

In the language of mathematicians, talking about "symmetries" means you are talking about groups (often Lie groups, or their infinitesimal versions, Lie algebras) and representations. The relation to physics is:

Classical mechanics (Hamiltonian form)

In classical mechanics the state of a system with n degrees of freedom is given by a point in phase space $P=\mathbf R^{2n}$ with n position coordinates q_j and n momentum coordinates p_j. Functions on this space are a Lie algebra, with Lie bracket the Poisson bracket
$\{f,g\}$. Dynamics is given by choosing a distinguished function, the Hamiltonian h. Then the value of any function on P evolves in time according to
$$\frac {df}{dt}=\{f,h\}$$
The Hamiltonian h generates the action of time translations. Applying the same formula, other functions generate the action of other groups (spatial translations, rotations, etc.). If your function satisfies $\{f,h\}=0$, it generates a "symmetry", and doesn't change with time (is a conserved quantity).

Quantum mechanics

Quantization of a classical system is something mathematically obvious: go from the above Lie algebra to a unitary representation of the Lie algebra. This takes elements of the Lie algebra (functions on P) to skew-adjoint operators on a Hilbert space, the space of quantum states. There's a theorem (Stone-von Neumann) that says that (modulo technicalities) there's only one way to do this, and it gives an irreducible unitary representation that works for polynomials up to degree two. For higher degree polynomials there will always be "operator ordering ambiguities". The representation is given by
$$1\rightarrow -i\mathbf 1,\ \ q_j\rightarrow -iQ_j,\ \ p_j\rightarrow -iP_j$$
This is a representation because
$$\{q_j,p_k\}=\delta_{jk}\rightarrow =-i\delta_{jk}\mathbf 1$$
The right-hand side is the Heisenberg commutation relations for $\hbar=1$.

For more details, I wrote a whole book about this.

Comments

1. **Leonardo Abbrescia**
   August 7, 2022

   Hi Peter, old student from your Lie groups class in the early 2010's here. (Though in hindsight, I wish I had taken your math physics class instead). Here's an easy way to complete your post with the last piece of the "holy trinity" of math physics: relativistic mechanics.

   Given the $(n+1)$-dimensional Minkowski spacetime with the standard Minkowski metric $(\mathbb{R}^{n+1},m)$, you can consider the Poincaré group; the group of symmetries of m. These symmetries include spatial and temporal translations, rotations, and boosts. By Noether's theorem, these correspond to several conserved quantities to solutions of the linear wave equation $\Box_m \phi = -\partial_t^2 \phi + \Delta_x \phi = 0$. These symmetries have paved the way for the dramatic progress in mathematical relativity seen in the past 3 decades.

2. **oliver knill**
   August 7, 2022

   Your blog is great as it is. Never mind twitter. There are actually only very few frequently updated math and physics blogs which succeed explaining non-obvious things for a larger audience (some linked here on your site).

3. **Peter Woit**
   August 7, 2022

   Leonardo Abbrescia,

   The point of view on symmetry I describe can handle the symmetries of relativistic mechanics, you just need to think of the phase space as the space of solutions of your relativistic wave equation. It doesn't tell you why certain groups and representations occur in our best fundamental theories (or why a certain wave equation). It does tell you rather that classical and quantum mechanics are formalisms whose structure is based on the use of a specific infinite dimensional Lie algebra.

This point of view is very different than the one normally taught to physicists, that it's all about Noether's theorem (which, when you have a Lagrangian with an invariance group, tells you the functions that generate the symmetry) and just a calculational method useful in certain approximations.

4. **Peter Woit**
   August 7, 2022

   oliver knill,

   Thanks! John Baez is however right that normally writing expository material is not what I do on the blog (and he's one of very few who does a wonderful job of this on the internet). There are several reasons I don't do this, the biggest one that it's really hard.

5. **Lino D'Ischia**
   August 7, 2022

   Great summary. Who needs a book! 🙂

6. **John Baez**
   August 7, 2022

   Indeed it was intended as a description, not a criticism. Yours is one of the few blogs I follow.

7. **Moshe**
   August 7, 2022

   You may be interested in recent work on categorical and non-invertible symmetries, those don't have a group structure.

8. **Peter Woit**
   August 8, 2022

   Moshe,
   Thanks. I want to emphasize though that this posting is not about a modern, sophisticated new development in ideas about symmetry. It's about the simplest topics at the very beginning of first courses in Hamiltonian mechanics and QM (the Poisson bracket and the Heisenberg commutation relations).

   That the Poisson bracket is a Lie bracket goes back to the very definition of and motivation for the Lie bracket 150 years ago, and the idea that QM gives a unitary representation of the classical mechanical Lie algebra goes back 100 years to Dirac's major insight at the very beginning of QM. Dirac would not have used this language, but Weyl immediately was undoubtedly aware of it.

9. **Alex**
   August 9, 2022

   This line of thought is developed until its final logical consequences in George Mackey's work on Imprimitivity Systems, which unifies the Stone-von Neumann Theorem (seen as the uniqueness of the irepresentation of the Heisenberg group/Weyl relations up to unitary eq.) and Wigner's work (on the classification of elementary particles via irepresentations of the Poincaré group) into a single mathematical framework. Furthermore, it also clarifies (at least for the finite dimensional phase space case) the origin of Dirac's quantization rule (Poisson goes to i×operator brackets.)

   Really beautiful mathematics, I wish it was more well known.

   Standard references can be:

   -Folland: Introduction to Harmonic Analysis;

   -Varadarajan: Geometry of Quantum Theory*.

   *which also contains a proof of Gleason's Theorem and the whole lattice theoretical approach to QM.

   Modern developments include Landsman's work on translating and expanding Mackey's ideas into the C*-algebra approach, aided by the use of tools from noncommutative geometry.

10. **Peter Woit**
    August 9, 2022

    Alex,

    The "line of thought" I'm explaining here is just very basic facts about a specific Lie algebra and a specific representation, with the significance that these are basic postulates of classical and quantum mechanics respectively.

    You can develop this "line of thought" in many different directions, bringing in more groups and representations. Mackey's theory is only one specific direction, aimed at a general theory of induced representations.

    Personally I'd argue that a generalization more relevant to physics is in the direction of geometric representation theory, the orbit method and geometric quantization. These are topics which link symplectic geometry (more general notions of classical phase space) and representation theory (more general notions of quantum system). This is a genuinely mathematically sophisticated and active area of current research, tough going for most people.

    I think it's a mistake to throw a lot of sophisticated abstract formalism designed to handle very general questions at people as an "explanation" of the role of symmetry in physics. Hardly anyone will understand this, and all you'll accomplish is convincing physicists that this area of mathematics is the "gruppenpest", a plague of incomprehensible ideas that tell us nothing about physics. Effort would be better spent explaining to physicists that Lie algebras and representations provide real insight into basic formulas they were taught to accept unthinkingly about classical and quantum mechanics

11. **Alex**
    August 9, 2022

    Peter,

    yes, Mackey's direction is one among several, but, historically, is the closest one (early 40s) to the original developments (Weyl, early 30s; Wigner late 30s, etc.), and was devised as a synthesis of all of those developments that were happening in QM at the moment. That's why I used the term "line of thought", since it seemed like a natural and organic development to me. In fact, it was Mackey who first used the name "Stone-von Neumann Theorem" for the result about the uniqueness of the CCRs.

Geometric quantization is indeed another approach, and I think they are actually complementary, since they treat different problems and aspects of quantization. In fact, they can even be combined. Unfortunately, both approaches have their well known limitations. Geometric quantization uses the language of connections and bundles; the hard part, I think, lies not on this but on the amount of tricks and subtle twists you must do along the way in order to make it work. Due to this, the process can become quite artisanal and difficult to follow for the unexperienced.

As for the clarification part, I was referring to the simple fact that the imprimitivity condition (which can be seen as a simple covariance condition on the system) gives rise to the CCRs.

Finally, I actually disagree with your last paragraph. In my own experience, I have seen many graduate students very confused by all the seemingly unrelated Lie algebras/groups that were introduced in their courses (Galilei, Heisenberg, Poincaré) and were quite interested and relieved when told that the representation theory of all of these groups could be solved by the application of a single general theorem, and by many other aspects as well (e.g., obtaining the wave equations from the induced representations' spaces, etc.) I do agree with the part about explaining the role of Lie algebras in the formulas that are thrown at them in the basic courses, and it was never my intention to suggest replacing that by some full blown introduction to induced representations, geometric quantization or whatever. Just an after that elaboration.

12. **Peter Woit**
August 9, 2022

Alex,

For me the imprimitivity condition doesn't clarify the CCRs, they (the CCRs) are just the Lie algebra relations for linear functions on phase space.

A lot of the appeal of representation theory to me is that it's a vast subject deeply connected to physics that one can approach from many different points of view. There may very well be an overarching single point of view that unifies the subject, but that remains elusive (at least to me…)

13. **Alex**
August 9, 2022

Peter,

well, the idea is that the imprimitivity condition is a covariance-type condition, between the position observables on some configuration space and the relevant group action on it, which in simple terms can be viewed as expressing the homogeneity of said observables with respect to the relevant symmetries of the configuration space. Furthermore, in the standard examples where the configuration space is R^n and the group are the additive translations R^n, the imprimitivity condition is equivalent to the Weyl relations(/Heisenberg group rep.), which are just the exponentiated form of the CCRs: thus, in this view, the CCRs are equivalent to imposing the homogeneity of the position observables with respect to the relevant symmetry of the configuration space. Thus, it's a very symmetry-friendly explanation, that's why I considered it relevant here.

I don't claim Mackey's theory to be such an overarching single point of view on representation theory, but it does unifies, as I mentioned, the Wigner classification theory (giving also the representation spaces, made of -distributional- solutions to the relevant wave equations for all spins in full mathematical rigor*) with the CCRs and their uniqueness (it can also be used to obtain the relativistic position operators and show that they indeed don't exist in the massless case for spin >=1, like the photon and graviton).

*most students of QFT think that single photons don't have wave functions and equations because that's what they are told in informal presentations of the subject. But they do have! And are given by the elements of the corresponding representation spaces, of which, as far as I know, the Mackey machine method via induced representations is one of the few approaches if not the only one to give you those Hilbert spaces rigorously and explicitly only from Poincaré representation theory. To construct the Fock space for photons in standard QFT, you need first such single photon spaces. In mathematical physics, these details are indeed relevant. If physicists don't care, that's their loss, not mine.

14. **lun**
August 10, 2022

One can define both in terms of analysis instead of Algebra, with symmetries playing an important but auxiliary rather than central/conceptual role:

Classical mechanics: There is an initial condition and an action. Dynamics is defined by the trajectory that minimizes the action, which gives evolution at any point after the initial condition.

Quantum mechanics: Same as classical mechanics, but dynamics is defined by correlators that are computed in terms of functional integrals of trajectories.

It is not clear, at least to me, why the symmetry/algebra definition is "better" or "more fundamental" than the analysis based one.

15. **Peter Orland**
August 10, 2022

"*most students of QFT think that single photons don't have wave functions and equations because that's what they are told in informal presentations of the subject. But they do have!"

Of course they do. But you just need to write the wave equation (for any relativistic particles, not only photons) in terms of oscillator variables (which become creation and annihilation operators in second quantization). These variables are the Fourier transform of the true wave function. Amplitudes and probabilities become as simple as they are for the Schroedinger equation, No deep mathematics is needed to understand this.

16. **Gavin**
August 10, 2022

Since the post advertises that it will explain "what the relationship between symmetry and physics really is," I think it's important to add at least a caveat that discrete symmetries are also important in physics (especially condensed matter physics) and are not described by a Lie group.

17. **Peter Woit**
August 10, 2022

Gavin,
Yes, and I did start off the post referring to all groups, not just Lie groups. In classical mechanics discrete groups are of limited use, no Noether's theorem. In quantum mechanics however, the abstract situation is the same as for Lie groups: the state space is a unitary representation of the group, and the representation theory of the discrete

group then provides a lot of information about the behavior of the quantum system.

18. **Peter Woit**
August 10, 2022

lun,
Yes, if you want you can do physics purely within the Lagrangian formalism (although if you do this, you quickly find that “just integrate e^{iS} over the infinite dimensional space of trajectories” often is a proposal that one can’t make well-defined).

The question was about the relation of symmetry and physics, and I’m arguing that is best understood in the Hamiltonian formalism (compare solving the N-dim harmonic oscillator problem and studying its symmetries in the canonical operator formalism vs. by calculating path integrals).

19. **Peter Woit**
August 10, 2022

Alex/Peter Orland,
This is getting off topic. I’ll just again refer to my book, where the single-particle state space for relativistic particles is worked out (even including distributional solutions, although that is not handled well and I hope to rewrite some sections for that reason).

20. **maciej**
August 10, 2022

I’m missing distinction between symmetries and invariances. They are not the same concepts, although both correspond to some transformations that leave the theory intact. Symmetries (e.g. rotational symmetry) map some physical states to different physical states. Invariances (e.g. gauge, diffeomorphism) do not change the physical state. As Coleman used to say: You don’t ask the experimentalist “In what gauge you did your experiment?”.

21. **Peter Woit**
August 10, 2022

maciej,
This is a good point. When I was writing the book about QM, I struggled with the fact that it’s not clear what one means when one says “symmetry”. There’s always a group, and it is acting on something, but what structure is being preserved has to be specified and so different sorts of things can be going on.

In the Hamiltonian formalism, functions on phase space with Poisson bracket are the Lie algebra for the infinite dimensional group of transformations of phase space preserving the symplectic structure. This is a group of “symmetries”, but sometimes by “symmetry” one means something more special: a Lie algebra of functions that have zero Poisson bracket with the Hamiltonian. These also preserve time evolution, give conserved quantities. As an example, consider spatial rotations: components of angular momentum give a three-dimensional space of functions on phase space with Poisson brackets satisfying the relations of the Lie algebra of the rotation group. These functions may or may not Poisson commute with the Hamiltonian and be conserved quantities (depending on whether the Hamiltonian is invariant under all rotations, or just a subgroup).

In QM, you have the same issue, you get operators with commutation relations given by the Lie algebra, which may or may not commute with the Hamiltonian operator. But in QM you in addition have a representation of these operators on states. Your states may be invariant (trivial representation), or in some specified non-trivial representation. In the rotation group example, your states may be invariant under rotations (s-wave), or transform non-trivially under rotations in a manner given by the representation theory of SO(3) and SU(2).

In the case of the gauge group, conventionally you assume states are in the trivial representation for local transformations (not necessarily so for global transformations).

22. **Hugh Osborn**
August 10, 2022

I am not sure I agree with what you say about the transition from classical to quantum. Real classical variables become hermitian operators (without any factors of i)
And the classical Poisson bracket is then replaced -i times the operator commutator,
with 1/hbar if this is not set equal to one. This is what Dirac did in 1926.
The centenary of real QM is due in just three years.

Hugh Osborn

23. **Peter Woit**
August 10, 2022

Hugh Osborn,
In a unitary Lie algebra representation the operators are skew-adjoint, not self-adjoint, so I put in factors of i to change the usual self-adjoint operators to skew-adjoint. Using these skew adjoint operators, the Lie bracket of classical mechanics (the Poisson bracket) goes precisely to the commutator of operators (this is the definition of a Lie algebra representation).

If you write things just in terms of self-adjoint operators, you have the usual extra factor of i in the relation between the Poisson bracket and the commutator of operators.

24. **Ricardo Jimenez**
August 17, 2022

While group theory (more exactly representation theory of Lie Groups) is certainly important in Quantum Field and Condensed Matter theories, it is hard to find any discussion of it in the literature of the rest of physics. What books are there on Classical Mechanics and Classical Electrodynamics from the group theory point of view? Thanks in advance.

25. **Peter Woit**
August 17, 2022

Ricardo Jimenez,

I just noticed this book
https://www.amazon.com/dp/1848167741
but don’t know much about it. I don’t know of any book that develops classical mechanics from the point of view of taking as basic the Lie algebra of functions on phase space with Lie bracket the Poisson bracket.

For classical electrodynamics, the relevant group theory is the Lorentz group and the gauge group, I don't know of a book that puts those front and center but maybe there is one.

In classical physics you don't really encounter representation theory much, although that's fundamental to quantum physics. So it is in quantum physics where the Lie group point of view really comes into its own.

26. **Jonathan**
August 17, 2022

Wouldn't classical mechanics texts more in the direction of symplectic geometry cover that sort of material?

27. **Peter Woit**
August 17, 2022

Jonathan,
I took a look at one such text I could think of (Arnold). His and other books do emphasize that phase space is a symplectic manifold and use the symplectic structure, but don't emphasize that the Poisson bracket is Lie bracket for the Lie algebra of the infinite dimensional group of transformations of phase space preserving the symplectic structure (maybe this fact is somewhere, but not emphasized).

28. **Anonymous**
August 17, 2022

The book "Introduction to Mechanics and Symmetry" by J.E. Marsden and T.S. Ratiu contains many interesting applications of Lie groups and Lie algebras in Classical Mechanics.

29. **Arnold Neumaier**
August 18, 2022

Alex,
"Geometric quantization is indeed another approach, and I think they are actually complementary, since they treat different problems and aspects of quantization. In fact, they can even be combined. Unfortunately, both approaches have their well known limitations."

In my recent paper

A. Neumaier and A. Ghaani Farashahi, Introduction to coherent quantization, Analysis and Mathematical Physics 12 (2022), 1-47.

I discuss coherent quantization, a generalization of geometric quantization including the non-unitary case. Unlike in geometric quantization, the groups are not assumed to be compact, locally compact, or finite-dimensional. This implies that the setting can be successfully applied to quantum field theory, where the groups involved satisfy none of these properties.

30. **Arnold Neumaier**
August 18, 2022

Ricardo Jimenez,

"What books are there on Classical Mechanics and Classical Electrodynamics from the group theory point of view?"

Two nice such books include:

Marsden and Ratiu, Introduction to mechanics and symmetry.

Arnold and Khesin, Topological methods in hydrodynamics.

31. **Student**
August 22, 2022

I've always felt that Ballentine's Chapter 3 in his Quantum Mechanics books is a tour de force. In one chapter he introduces the Gallilean group as the group that describes the non-relativistic symmetries of space time and proceeds to derive the fundamental Quantum Mechanic operators from these symmetries.

He doesn't get into the formal mathematics of Lie Groups and Algebras or representations but it was a powerful illustration of that POV.

I'd contrast that with your book Peter, which I have tried to work through on and off, which goes much deeper into the subject, but, if I may offer a slight critique, the motivation for the approach is less clear.

32. **Peter Woit**
August 22, 2022

Student,
My book does have a somewhat similar emphasis on non-relativistic space-time symmetries, but sticks to the Euclidean group of rotations and translations rather than getting into the larger Gallilean group (for which the representation theory is much trickier, and the extra generators you get not that interesting). In both cases, thinking of the non-relativistic case this way extends nicely to the relativistic Poincare case.

One thing to keep in mind is that Ballentine is aimed at physicists, my book is based on a course in the math department. The motivation is much more on the math side than for Ballentine. A book like Ballentine's is hard for mathematicians to follow, since he's avoiding standard mathematical concepts (like that of a representation) in favor of language familiar to physicists. For physicists, I suspect my book is similarly harder to follow because of the unfamiliar language and formalism, but I hope that they will ultimately find this point of view worth learning, since it is quite powerful and clarifies issues that the physicist's approach often leaves unclear.

Various and Sundry

August 18, 2022
Categories: Uncategorized

A random collection of things that may be of interest:

- September 17 and 18 I'll be at the How the Light Gets In Festival in London, participating in discussions of the relation of math and physics, and theories of everything. I'm looking forward to the festival, which sounds like fun, and to spending some time in London. A week or so later, I'll be in Oxford, attending the Clay Research Conference as well as a Physics from the Point of View of Geometry workshop in honor of Graeme Segal's 80th birthday.
- I've been spending the summer trying to write up some details of the ideas I've been working on, specifically the claim that the geometry of spinors in four dimensions allows one to think of one of the SU(2)s in the Euclidean Spin(4) symmetry as an internal symmetry. Still learning more about how this works, hope to have something ready to publicize within the next month or so.
- For rest and relaxation I've been learning a bit more about various Langlands-related topics. The talks from the IHES summer school are mostly well-worth watching. Also very highly recommended are David Ben-Zvi's lectures on *The Langlands Program as Electric-Magnetic Duality* given a couple weeks ago at a workshop in Cambridge.
- Still trying to finish reading Récoltes et Semailles and decide whether to write something here about this bizarre and fascinating document. If you want to read this yourself, Mateo Carmona has a freely available transcription here.
- There an interesting conversation about Ricci flow between my Columbia colleagues John Morgan and Richard Hamilton available here.
- Sometimes it takes great self-control to avoid responding to things I see on Twitter. In the case of a recent exchange between Noah Smith and various people defending string theory. I couldn't help myself and started writing something, then soon hit the character limit. This returned me to sanity as I realized that trying to have an intelligible discussion in the twitter format about anything complicated is just absurd.

  The gist of a lot of the discussion was that even string theory defenders now admit it was an overhyped failure as a "theory of everything", but they then come up with new, improved hype. One argument seems to be that string theory has led to new developments in hype about black holes (for these, Scientific American has you covered here).

- Today on Twitter Sabine Hossenfelder explains her current academic employment situation (no permanent position, latest grant application denied.) She's a very unusual case, and has a successful new book and other ventures that to some degree can replace a standard academic income. For everyone though, the way academic jobs in theoretical physics work, if you decide you want to pursue topics other than very conventional ones that a group is already working on, you're going to have a very hard time. Getting older and having a life also tends to be inconsistent with pursuing the very few opportunities that might come up.

Comments

1. **Alex**
   August 19, 2022

   I'm tired of seeing string theorists hijacking other fields and pretending their failed methods and ideas are revolutionizing those fields. It's remarkable, these people are not going to stop, it seems. And they get a lot of press! When is this nightmare going to stop?!

   Meanwhile, in actual black hole news, people like Klainerman have been working on the really, really hard problems, like Kerr stability, and recently they finally proved an amazing result, with actual relevance to our physical picture about the final state of real black holes at late times.

   At least Quanta covered the news: https://www.quantamagazine.org/black-holes-finally-proven-mathematically-stable-20220804/

   There are some great talks by him in youtube on these topics as well.

2. **suomynona**
   August 19, 2022

   This isn't the first time Noah Smith has tweeted a horrendously ignorant opinion, then mocked those pointing out his ignorance as being sensitive and butt-hurt, and then deleting the tweet to hide evidence of his ignorance.

   Something that struck me as bizarre in that thread was that string theory was being defended as a "simple toy model" for describing condensed matter systems, while simultaneously also being admitted as far removed from the actual realistic physics of said systems. String theory was considered a "good starting point" because it's "easy to work with," not because it's actually relevant to the physics under consideration. String theorists and condensed matter physicists certainly have different standards for what constitutes a "simple toy model".

   FYI, it's perfectly acceptable twitter etiquette to comment with a multi-tweet thread. You need not limit your entire reply to a single tweet.

3. **Peter Woit**
   August 19, 2022

   suomynona,
   I don't think his original tweet was "horrendously ignorant". It's hard to overemphasize the damage of all sorts that decades of outrageous hype, failure, and refusal to acknowledge failure have done (and continue to do) to important parts of physics. Yes, the case for linking this to less progress in "applied quantum mechanics" wasn't very good, and I think it's to his credit that he realized this and deleted the original tweet.

   On the other hand, all the people who piled on with ridiculous hype about string theory's supposed successes are the sorts who never will admit that their claims are wrong. That's just one reason why it would have been a waste of time to engage with them.

   I'm well aware one can chain together tweets, but that's awkward. Twitter's design seems intended to make thoughtful discussion and engagement with other people's arguments difficult, while encouraging randoms short bursts of stupidity.

4. **theoreticalminimum**
   August 21, 2022

   I'd be interested in what you got from a reading of R&S. I've myself tried to find time to read the 2-volume, but it's been very difficult, and reading it is not the easiest of things, because it seems like a lot of what he wrote came from some deep thinking, which requires the reader to pause and meditate on some points.

5. **Peter Woit**
   August 21, 2022

   theoreticalminimum,
   Writing something about this is a daunting prospect, it's huge unedited mix of the wonderful and deep with the paranoid and tedious.

   Probably a good idea to keep in mind that to some extent Grothendieck's intended audience was mathematicians in his field (algebraic geometry). The more you know about that field and what was going on in it during the years from the mid-50s to the mid-80s, the more you're likely to make sense of parts of it. At some points this gets very frustrating: after he goes on for dozens of pages about how terrible it was that people other than Deligne didn't understand and appreciate his (unpublished) ideas about weights and the "yoga" of motives, you really wish he instead had spent the time writing out an explanation of those ideas.

6. **Niclas Granqvist**
   August 28, 2022

   More multiverse mania:

   The Guardian: Cosmologist Laura Mersini-Houghton: 'Our universe is one tiny grain of dust in a beautiful cosmos'.
   https://www.theguardian.com/science/2022/aug/27/cosmologist-laura-mersini-houghton-before-the-big-bang-interview

7. **Pall Lobe**
   September 5, 2022

   Very sad to know about Sabine Hossenfelder. There is no value to popular fame, which is much more important writing another stupid paper, in mainstream academia.

   PS: I am a string theorist.

LHC News

August 24, 2022
Categories: Experimental HEP News

I see today via the LHC Page 1 Vistar that a problem at a cooling tower will cause part of the accelerator to need to be warmed up to room temperature, putting the LHC out of business for the next 4 weeks or so.

The LHC has just been coming out of long shutdown the past few months, starting its Run 3. In the past couple weeks it has started to get up towards its full luminosity potential, with over 2400 bunches in the beam. So far during Run 3 the machine has delivered an integrated luminosity of about 10 inverse fb to each of the experiments (ATLAS/CMS). The plan for Run 3 (which is expected to last through 2025) is to accumulate an integrated luminosity of about 300 inverse fb, doubling the 140 inverse fb of Run 2, at a slightly higher beam energy (6.8 TeV vs. 6.5 TeV).

Update: Thanks to Benson Woo for pointing out to me an article today in the Wall Street Journal about plans for possible shutdowns of parts of the CERN accelerator complex this winter in case of energy supply problems due to the ongoing conflict with Russia.

Comments

1. **Amitabh Lath**
   August 26, 2022

   Although 300 fb-1 does not sound like huge improvement over Run 2, there are several triggers (online data filters) that are getting tested for the High-Luminosity LHC era. Of course, Run 3 will also chase down some interesting anomalies we saw in Run 2.

   One interesting thing about Run 3 analyses will be how machine learning techniques are incorporated into nearly every facet. The comfort level for ML has really increased. Concerns about things like overtraining and bias are real, but not considered insurmountable.

2. **Severin Pappadeux**
   September 12, 2022

   Snowmass (shall we call it Rainmass?) report

   https://www.scientificamerican.com/article/physicists-struggle-to-unite-around-future-plans/

This and Next Year's Hype

September 5, 2022
Categories: This Week's Hype

I normally try and avoid getting into the vast topic of the hype problem in other subjects than string theory, but a couple things I've seen recently make it hard to resist. So, just this once…

Quantum Computing

Michio Kaku has a new book coming out next year, called Quantum Supremacy: How the Quantum Computer Revolution Will Change Everything. The publisher's summary tells us that quantum computing "may eventually unravel the deepest mysteries of science and solve some of humanity's biggest problems, like global warming, world hunger, and incurable disease." More concisely:

> There is not a single problem humanity faces that couldn't be addressed by quantum computing.

For a very different take, see The quantum computing bubble at the Financial Times, where Nikita Gourianov argues that there's a speculative bubble going on in this field, and:

> Well, when exactly the bubble will pop is difficult to say, but at some point the claims will be found out and the funding will dry up. I just hope that when the music stops and the bubble pops, the public will still listen to us physicists.

For a response to this, see a later article at the Financial Times: Separating quantum hype from quantum reality.

I think Gourianov makes an important point for physicists to keep in mind. Having this sort of hype blow up in physicist's faces is not going to help with the credibility problems physics already has with the public due to decades of hype about non-existent breakthroughs in fundamental physics.

Nuclear Fusion

Attempts to build a nuclear fusion-based power reactor have been going on for 70 years or so. Decades ago it had already become a joke that success was always "30 years off". One would think that because of this there would be overwhelming skepticism about new claims in this field, but there's continual new hype all the time. The Guardian recently had a long article about The race to give nuclear fusion a role in the climate emergency. If you read the article carefully, there's no evidence of any change on the "30 years off" front, with one expert describing magnetic confinement-based reactors as highly unlikely before "after 2050" and laser-based schemes "another 50 years to go, if at all."

One project that has been getting a lot of press is SPARC, a collaboration between MIT and a private start-up. Their claim seems to be that they've got a workable reactor design all ready to go, last year finished developing the needed 20T high temperature superconductor-based magnet, and by 2025 will have a working reactor putting out more energy than goes in. Then:

> **On this path, how long would it take before fusion energy is on the grid?**
>
> MIT scientists and their collaborators believe that ARC — a fusion power plant that would produce electricity continuously — could be built and operating by early 2030.

This all seems highly implausible to me, but Bill Gates is putting money into the the project and I guess we'll find out soon. For a skeptical take, see here.

About nuclear fusion, Michio Kaku tells us that:

> Quantum computers could allow us to finally create nuclear fusion reactors that create clean, renewable energy without radioactive waste or threats of meltdown.

Two more items:

Getting back to the sort of claims about physics that don't work out that I usually write about, the IAS website points to two recent items:

- Symmetry magazine interviews various physicists who appeared in the film Particle Fever (which I wrote about here).
- An electronic music producer interviews Edward Witten.

Update: Theoretical physicists are making a contribution to the energy crisis, see here.

Comments

1. **Alex**
   September 5, 2022

   Just wow. It's amazing how these people are simply incapable of communicating anything without uber hyping it at the same time. It's beyond pathological, it's in the community's fabric now. It's a way of life. Is there any way (or even hope) of coming back to rational after we passed such event horizons of delusion, but also of lack of scruples? I'm really pessimistic, to be honest.

2. **steve blazo**
   September 5, 2022

   I do not agree with the appeal of nuclear fusion over nuclear fission. The safety record of fission is amply demonstrated by any serious analysis on the line of deaths per megawatt hour and third and fourth generation plants will be improvement over older designs and they are available now. The high level waste issue just seems a red herring, there just is not that much of the stuff generated. Uranium reserves (or thorium) are ample. I just do not see the point in investing 10's of billions in the hope you will be able to solve some very difficult, perhaps practically impossible problems with fusion when you already have a solution staring you in the face.

3. **Rohan**
   September 6, 2022

   I wonder what people think about the prospects of nuclear fusion startup Helion? How seriously is it taken in this space?

4. **Sabine Hossenfelder**
   September 6, 2022

Nuclear fusion needs chaos control which is a strongly non-linear problem. Last time I looked no one knew how to efficiently solve non-linear equations on a quantum computer. I warned of quantum computing hype some years ago here:

https://www.theguardian.com/technology/2019/aug/02/quantum-supremacy-computers

5. **Hans**
September 6, 2022

My partner's father worked on fusion. For 50 years he told him that fusion will come within 50 years. He got quieter when he got older.

My colleague questioned quantum computing at work. His boss got angry in away he had never seen.

Peter, I admire your energy, but be careful not to open too many battle fronts.

6. **Peter Erwin**
September 6, 2022

I suppose that if you wanted some tenuous source of optimism about fusion, you could look at how the value of *N* in "fusion power is *N* years away, and always will be" has been gradually creeping down. I mean, I remember quite clearly in the early 1990s hearing it as "40 years away, and always will be", and here's a recollection of an earlier version: "I first heard the standard joke about fusion as an undergraduate physics student in the 1960s: Fusion power is fifty years away – and probably always will be."

A simple linear extrapolation tells us that we'll reach the state of "fusion power is zero years away" around 2110! Progress!

7. **Anonyrat**
September 6, 2022

"As John Preskill said at the Q2B conference organized in December 2021 by QC-Ware, "There is a line between setting ambitious goals and fanning inflated expectations… for us as a community, we'll be better off if we try to stay on the right side of that line"."

"As Victor Galitski states in his already mentioned paper, the ethics of science communication is at stake in the current quantum hype."

From: https://arxiv.org/pdf/2202.01925.pdf

8. **AcademicLurker**
September 6, 2022

In the linked Symmetry magazine piece, there's an interesting quote from David Kaplan about how before Particle Fever he used to pick projects that could be "wrapped up in six months". Since making the film he says he's changed how he works and now realizes that worthwhile questions might take longer to answer.

Peter and Lee Smolin in their books back in 2006 both mentioned how career incentives force theorists to work on short term projects, and Sabine Hossenfelder mentioned it again in her more recent book. There apparently is very little will to change how the incentive structure works. It's funny, because you would think it would be obvious that fundamental problems like the ones HEP theorists work on won't be solved in 6 months.

9. **Alan Post**
September 6, 2022

The new high-field magnets will allow existing fusion device designs to work better. The idea with SPARC and ARC is to do this with the currently best-performing design (H-mode tokamak). Of course, many others are applying the new magnets to other designs that are less proven or have performed less-well in the past. Hopefully one or more of these approaches will pan out. I doubt something the scale of ITER will ever be cheaper than renewables plus storage.

I do wish everyone involved would avoid making misleading claims about "net energy" or "gain", though.

10. **Varun Chauhan**
September 6, 2022

I remember studying nuclear fusion for nuclear engineering the common understanding is it is always 50 years away.

11. **David J. Littleboy**
September 6, 2022

Given that the number of problems that can be solved (that is, actually known algorithms) using quantum computation is tiny (this year, the folks working on it finally came up with a third algorithm, as I understand it the first new algorithm since Shor's and Glover's algorithms* of the 1990s), Michio Kaku's comments on what QC will do for us are more than over the top.

Even people who claim that the "problem" being used for the quantum supremacy demonstration actually demonstrates that admit that it has absolutely no conceivable practical (meaningful in real life in any way) use. And all this is in the context of Sabine Hossenfelder's comment on the expected arrival time of a QC that can actually run those three algorithms as being "not in our lifetimes".

The universe is becoming more and more devoid of reality checks, it seems…

*: That almost no new quantum algorithms have been forthcoming was a surprise here: Shor's algorithm, as I understand it, is essentially a quantum-fast Fast Fourier Transform, which ought to be useful for all sorts of things. One would think.

12. **Peter Woit**
September 6, 2022

For enthusiastic coverage of some of these new startups, see this article by Philip Ball in Nature last year

https://www.nature.com/immersive/d41586-021-03401-w/index.html

Ball recalls the usual "30 years away" (I recall always hearing 20-30 years away, not 50), and notes that nowadays it's often "10 years away". That would normally make one more optimistic, but he also writes:

"Private companies say they'll have it working in ten years, but that's just to attract funders," says Tony Donné, programme manager of the Eurofusion consortium which conducts experiments at the state-run Joint European Torus, established at Culham in the late 1970s. "They all have stated constantly to be about ten years away from a working fusion reactor, and they still do."

Timelines that companies project should be regarded not so much as promises but as motivational aspirations, says Melanie Windridge, a plasma physicist who is the FIA's UK director of communications, and a communications consultant for the fusion firm Tokamak Energy, in Culham. "I think bold targets are necessary," she says."

It may just be that when you're looking for private investors rather than government funding, you have to claim shorter time horizons, even if they're nonsense. If you need to claim "ten years" to get money, and your choice is do that or don't get funded and shut down, the only sensible thing to do is to claim "ten years", and deal with the fact this is not going to work out that way later.

13. **Peter Woit**
September 6, 2022

AcademicLurker,
I was interested to read that comment by Kaplan also. The problem of all the incentives being aligned to push people into short-term unambitious projects are still all there. The problems of this kind Smolin, Hossenfelder and I have pointed out a long time ago are just as bad if not worse now. Everyone agrees that the field is in a difficult state, with new ideas needed, but no one wants to change anything about how research has always been organized and funded.

14. **Douglas Natelson**
September 6, 2022

Can someone please stop giving Michio Kaku a megaphone? His brand of popularization does more harm than good, in my view. BTW, if you want to see an excellent and thought provoking talk about quantum computing, I recommend this one by Garnet Chan (Caltech) about whether quantum computing gives an exponential speed up for problems related to quantum chemistry (where a priori one might imagine a real benefit from quantum algorithms): https://m.youtube.com/watch?v=DZPH7ENcRLU

15. **DrDave**
September 7, 2022

Surely there must be something in the book about how quantum computing will make–and test–predictions using string theory?

16. **David Roberts**
September 7, 2022

@David J. Littleboy

Can you give us a name for and a link to this third algorithm?

17. **Mark Hillery**
September 7, 2022

For anyone interested in what quantum algorithms are out there, have a look at the Quantum Algorithm Zoo: https://quantumalgorithmzoo.org .

As someone who has worked in quantum information for 25 years, I can say that the last thing the field needs now is more hype. There is way too much already. Back in its early days, it was a small field that encountered a lot of skepticism from people working in more established fields. I remember a reaction to one talk I gave, which was, "Is this physics?" Well, things have changed. The American quantum information community adopted the APS March Meeting as its annual meeting. I don't like March Meetings, too big, but I did go to the one in 2019 in Boston at the behest of Physical Review. I was amazed at how many people were working in quantum information/computing and the level of interest from industry. I guess when big money gets involved the danger of hype increases, but making promises you can't keep is a really bad idea. The honest answer about the future of the field is that while there is a lot of interesting work going on, both theoretical and experimental, no one knows where all this is going to go.

18. **David J. Littleboy**
September 7, 2022

David Roberts asks for a link to the third algorithm.

Quanta was on it:

https://www.quantamagazine.org/quantum-algorithms-conquer-a-new-kind-of-problem-20220711/

19. **Masoud Kamgarpour**
September 8, 2022

Regarding credibility problems of physics with the public, I would say your concern is misplaced. The public has very short memory and are hardly going to remember what string theory did or did not promise 30 years ago. I mean we are talking about the same public half of whom believes that Trump had the highest number of spectators in his inauguration, amongst other absurdities…

As an outsider mathematician interested in physics, I would characterize the issue not as a lack of credibility but as disappointment. Creating hype about string theory led to many great young minds going into that direction only to find themselves lost after enormous amount of effort. We see many of these examples of former string theorists coming to the math departments and unfortunately, cannot accommodate many smart ones. One would hate to see history repeating itself now under the guise of quantum computing.

20. **Stef Roux**
September 8, 2022

A comparison between quantum computing and string theory is like comparing apples and oranges. While string theory has no connection with physical reality, there are already some quantum computers in operation. They are small and not very reliable, but there is no fundamental stumbling block to prevent them from becoming big enough and reliable enough to be useful. It will take time though. Most people in the field say it is still several decades.

There are more than just three algorithms for quantum computers. It is true that quantum computers are not suitable for all computing applications, but there is enough that they can do to make them useful.

So, while it is unfortunate that there is a lot of hype about quantum computing, it does not mean that it is not a viable technology. The challenges are significant but not fundamentally insurmountable.

21. **SRP**
September 13, 2022

The skeptical quotes about the private fusion efforts mainly come from publicly funded no-hope-for-practical-use project members. These self-licking ice cream cones have no urgency internally and a strong interest in denigrating more-innovative and more-focused private programs that might lead to questions about their own funding and progress. Outfits like TAE and Helion have been hitting milestones along well-defined research and development paths over a number of years. Perhaps their trajectories will hit a wall in their next-stage machines (currently under construction), but no obvious physics or engineering showstoppers have yet cropped up.

22. **Mark Hillery**
September 16, 2022

Me again. For an interesting perspective on the state of quantum algorithms see https://arxiv.org/pdf/2209.06930.pdf by Scott Aaronson. And yes, there are a lot more than 3 quantum algorithms.

It should also be pointed out that quantum computing is only part of the ongoing quantum project. You can buy off-the-shelf quantum crypto systems from established companies, and the Chinese have demonstrated quantum cryptography using a satellite link. There is also research into quantum networks, whereby one can send quantum rather than classical information. Research into enhancing the performance of sensors and sensor networks by using entanglement and nonclassical states of light (LIGO is doing the latter, though the original theoretical proposal is 4o years old) has been a long term project. All that said, there is still way too much hype in the field, and probably too many startup companies. On the other hand, from someone who has been in the field not from the beginning but from early days, from an intellectual point of view it has been a fun ride.

23. **Joseph Healy**
September 19, 2022

AI was vastly over-hyped in the 80's and much nonsense still surrounds the technology today. And while one might wish a different trajectory for quantum computing, hype like Michio Kaku's appears an embedded and inescapable facet of money chasing technology and technology chasing money.

24. **Mark Hillery**
September 25, 2022

Depending on your opinion of the Breakthrough Prizes, you can decide how you want to interpret this, but it is an excellent choice of winners.

Breakthrough Prize in Fundamental Physics Awarded to Charles H. Bennett, Gilles Brassard, David Deutsch and Peter Shor.

25. **Mark Hillery**
October 4, 2022

From the New York Times:

The Nobel Prize in Physics was awarded to Alain Aspect, John F. Clauser and Anton Zeilinger on Tuesday for work that has "laid the foundation for a new era of quantum technology," the Nobel Committee for Physics said.

Some News, Then More of the Same

September 11, 2022
Categories: Multiverse Mania, Uncategorized

Some News:

I'll be in England later in the month, in Oxford much of the week of the 26th-30th. That week is the week of the 2022 Clay Research Conference and Workshops. The evening of Tuesday the 27th I'll be giving a public talk on Unified Theories of Physics, sponsored by the Oxford Centre of the Institute of physics.

The 2022 HowTheLightGetsIn festival in London was supposed to be the weekend of September 17-18, but has been postponed two weeks because of the death of the Queen. It's now scheduled instead for the weekend of October 1-2 and I'll likely be there, participating in a couple of panel discussions.

More of the Same:

I've written too much here over the years about the problems with multiverse theories. For short versions, there's also FAQ entries here and here, and a piece called Theorists Without a Theory I wrote for Inference. Seeing some recent things about this topic from people I generally agree with (e.g. here and here) leads to an uncontrollable urge to reiterate some of my arguments, so:

- You can't argue against the concept of a multiverse in general, dismissing unobservable universes. If you had a very successful theory based on ideas that simultaneously implied successful predictions about what you can observe, as well as unobservable parallel universes, you could get indirect evidence for a multiverse. The strength of this evidence would depend on the details of the theory, but it's logically possible that this could be strong evidence.
- Arguments pro or con about the "multiverse" that simultaneously engage with the many-worlds interpretation of quantum mechanics and inflationary or string theory landscape models are a waste of time. These are two completely different subjects, which raise completely different issues and have nothing to do with each other. For the rest of this I'll stick to the second subject, ignore the first.
- If you want to have a serious discussion on this topic, it should be about a particular model or well-defined class of models. One popular class is inflationary models. Here people often write down a well-defined model, but the problem is that it's a toy model (e.g. no SM fields, just gravity and a hypothetical inflaton field unrelated to any field for which we have a tested theory). Another popular class is the "string theory landscape". Here the problem is that you don't have a well-defined model. People who work on this work not with a well-defined theory but with a list of properties of a conjectured, currently non-existent, theory (e.g. "M-theory"). There's nothing wrong with doing this to see if you get interesting predictions about the world, which would give you some confidence in the existence of the conjectural theory. There is something seriously wrong with doing this if after decades of work you find that the list of properties you have is vacuous in terms of explanatory power.
- It's important to understand just how vacuous the "string theory landscape" class of models is. The problem is not just a measure problem on the space of possible universes, but much worse: one has no idea what this space is that you would like to put a measure on.
- "Pseudo-science" is an accurate description of "string theory landscape" research. People have complained to me that it is too harsh, should only be applied to activities of people who are abusing the good name of science for discreditable purposes. Doing something because you refuse to admit failure of a scientific idea you have a lot invested in seems to me a discreditable purpose.

Update: Joe Conlon is upset that I'll be speaking in Oxford. He objects to my credentials, but perhaps my views on his field of string phenomenology (which are shared by a large fraction of the physics community) might have something to do with it. I'm wondering if Conlon also complained about this recent Oxford speaker (video here).

Comments

1. **Sabine Hossenfelder**
   September 12, 2022

   Peter wrote: "If you had a very successful theory based on ideas that simultaneously implied successful predictions about what you can observe, as well as unobservable parallel universes, you could get indirect evidence for a multiverse. The strength of this evidence would depend on the details of the theory, but it's logically possible that this could be strong evidence."

   I do not know what it means for a theory to "imply" something that is unobservable, this is a meaningless phrase. Whatever you mean by it "implying" something, it is not the same as indirect evidence because there can be no indirect evidence for other universes.

   Indirect evidence means that you have a mathematical structure in your theory that is *necessary* to describe what you observe even though it cannot itself be observed (eg, the wave-function, or gravitational waves). The entire problem with the other universe is that because they are not observable you do *not* need them to explain anything you in fact observe.

   For this reason there is no evidence that you can gather for their existence, neither direct nor indirect.

   The assumption "unobservable universes exist" is unnecessary to describe anything we observe. That's it. You are claiming that this statement is wrong.

   Peter wrote: "Arguments pro or con about the "multiverse" that simultaneously engage with the many-worlds interpretation of quantum mechanics and inflationary or string theory landscape models are a waste of time. These are two completely different subjects, which raise completely different issues and have nothing to do with each other. "

   In both cases physicists mistakenly believe that someone which is unobservable exists and fail to acknowledge that this existence is a superfluous assumption which is hence not scientific.

2. **Will Sawin**
   September 12, 2022

   @Sabine Probably our host does not want us to discuss the many worlds intepretation.

   However, it is easy to imagine a theory that has the same relationship to the multiverse as the theory of the quantum wave function has to MWI, and we can discuss this hypothetical theory instead.

   Suppose we had a theory which precisely defined a space X, with points of the space corresponding to different effective field theories or different values of the physical constants, and gave an evolution rule for a function f on the space X which can be interpreted as the density of universes with those laws of physics (for example a form of "cosmological natural selection"). Furthermore, one could calculate the likely behavior of the function f, and find that it concentrated on physical theories matching our own observations. (Or to avoid retrodiction, one could even imagine that the fundamental constants of physics in our universe were observed to change slightly over time, and this theory was capable of predicting that change via a flow on the space X.)

Supposing we had such a theory, it would seem reasonable to me to refer to the points of the space X as distinct universes, which the theory describes the dynamics of.

3. **Low Math, Meekly Interacting**
September 12, 2022

I'm with Dr. Hossenfelder on this, at least to the extent my grasp of the philosophy allows. It's one thing to say your equation has, e.g., two solutions, one of which fits an empirical observation. It's quite another to say the other possibility is "real". One possibility that I'll never observe is myself writing something about this without feeling the need to use scare quotes. Any kind of multiverse which must deny contact with experiment for any other part of the ensemble seems simply to fall on the other side of the line of demarcation. Why are we forced to acknowledge the other solutions "exist"?

That's my biggest beef with the Everettians. Why is it so intellectually dishonest or lazy to simply dismiss my "other selves" with a shrug, or view them as artifacts with no scientific relevance. I suppose it forces me to insist that science is ultimately an empirical discipline, but that's a hill I'm willing to die on.

4. **Peter Woit**
September 12, 2022

Will,

Yes, please don't try and start a discussion of MWI. To my mind at least, the issues there are quite different (QM is a well-defined theory, for one thing, and MWI enthusiasts often say their claim is just that all you need is QM).

However the scenario you describe is closely related not to MWI, but to the string landscape. My main problem with this scenario is just that it's a hypothetical that has nothing to do with the current state of theory (no one has anything like the kind of theory you postulate). I see no point in arguing about a failed theoretical idea by starting with "assume the kind of theory desired exists and is completely successful".

Sabine can answer you if she wants. I'll write another comment related to yours.

5. **Engineer**
September 12, 2022

I took "imply" to be mathematical/logical imply =>. My understanding is that GR mathematically describes what goes on just inside the event horizon of a black hole. I would say GR implies what is going on there, and it is not observeable to us. It would be fair to me to say all the evidence supporting GR is indirect evidence of what is going on just within the event horizon.

6. **Peter Woit**
September 12, 2022

Sabine (and LMMI),

Different people think about this differently, here's my take:

A fundamental physical theory is a model (expressed of necessity in the language of mathematics) which comes with a way to relate some of its elements to experimental observations. This last feature allows the model to simultaneously explain what we observe and be tested (if it says we should observe X and we observe not X, it is wrong).

A good theory is one that is simple, and based on few assumptions, from which flow many non-trivial predictions about what we will observe, predictions that can be and have been checked. The ratio between information out and information in is very high.

A bad theory is typically one that starts out simple, but makes wrong predictions. You can fix these wrong predictions by making the theory more complicated, but what you end up with is something useless: information out is just reprocessed information in, you are learning nothing new from such a theory.

Applied to the multiverse issue, I see no argument of principle that you can't find a very good theory with elements that correspond to our observations and have been successfully checked, but also other elements that don't. If a model is simple and highly successful, and non-observable elements are closely linked in its structure to tested, observable elements, it would be reasonable to say that one has evidence that the non-observable elements exist.

You can argue that elements of a model that cannot be tested by observation should just be cut out of it (characterized perhaps as "mathematics, not physics", or "unscientific"). But I can also take the attitude that what's important is how good the model is, and a good model can consistently contain observable and unobservable elements, it just has to have a convincing explanation of what you observe and what you don't.

What's really important to make clear is that the deadly problem the string theorists have is a very conventional one: they have a bad, failed model. They're trying to wriggle out of this inconvenient situation by invoking non-existent successful models and getting people wrapped up in arguments of principle over those. Best to not do this since it's besides the point, a waste of time, and accomplishes what those with failed ideas want: obfuscating the fact of their failure.

7. **martibal**
September 12, 2022

Peter: from the history of physics, do you have any example in minds of such non-observable predictions that would comes from the mathematical structure ?
.
For instance in GR the mathematical structure (space-time is a curved manifold) yields only observable predictions that, indeed, made the theory successful (binding of the light, redshift etc). Even the effect that at the time seemed unobservable (gravitational waves, black holes) are nowadays observed.

I would say the same holds true for gauge degrees of freedom. They may well be classically unobservable, but lead to some quantum effect.
.
How can one claim that a mathematical structure needed for the coherence/beauty/simplicity of the theory is forever unobservable ? Negative numbers (as I read, don't remember where) where invented just for the convenience of calculation, but they were denied of any meaning: what sense would it make to give meaning to something which is less than nothing ? Well, today negative numbers are very concrete, to anybody who has ever experienced a deficit on one's bank account 🙂
.
But if something that is now unobservable might become observable one day, then I find it misleading to call it "multiverse". It is just a part of our universe that at the moment is not accessible to our experiment.

8. **Peter Woit**
September 12, 2022

martibal,
I don't think issues of mathematics have anything to do with this, other than the fact that mathematics is the language you use to write the theory down. There are all sorts of issues about how this language describes elements and relations of the theory, and how you get from elements of the theory to experimental observations (the LHC detectors are not measuring the values of quantum fields…). But they're besides the point I'm trying to make.

What I'm talking about is the possibility of a model having elements that are a fundamental part of a model's structure (independent of how you write this down), but not accessible to experiment. I can't think of a good historical example, maybe there aren't any. But again, arguing over this abstractly is besides the point, so I'm not interested in trying to come up with examples and do so. The string theorists point to a logically possible class of models with this characteristic. I don't think they're wrong that these are logically possible, the problem is that all attempts to construct a model in this class which is consistent (mathematically and with observation) have failed utterly.

9. **Marvin**
September 12, 2022

Martibal

You are asking Peter: "from the history of physics, do you have any example in minds of such non-observable predictions that would comes from the mathematical structure ?" Then you mention " for instance in GR the mathematical structure …… yields only observable predictions……"

A good answer for you will be the Godel solutions to GR equations. Essentially, they are mathematically feasible, but they show "time looping" that was never observed!

Your question look trivial, but in fact it is at the core of any sustainable theory: how nature select in the general mathematical landscape this equation and not any other?

.

10. **Will Sawin**
September 12, 2022

@Peter Woit,

Well, if people disagree with your claim that "If you had a very successful theory based on ideas that simultaneously implied successful predictions about what you can observe, as well as unobservable parallel universes, you could get indirect evidence for a multiverse" by saying it's impossible that a theory could imply unobservable parallel universes, I don't think it's possible to respond without sketching in more detail what a theory that implies unobservable parallel universes to look like.

I did indeed intend it to be similar to a potential version of the string multiverse theory, although it's also a bit inspired by some other things.

@LMMI The issue is not just that an equation may have two solutions but that there is some dynamics involving multiple universes that (indirectly) makes predictions. You can't just drop some of the universes in this scenario any more than you can drop some elements of any other dynamical system and get an equivalent system.

Of course no one is forced to believe anything and you can always deny the existence of whatever you please, but if theories of this type ever make solid predictions and do better than competing theories, any kind of consistent principle that denies the other universes will also have to imply that stars stop existing when they get far enough away from us and silly things like that.

11. **Low Math, Meekly Interacting**
September 12, 2022

I'm not advocating for "dropping" something so much as acknowledging a certain state of ignorance about its "existence". Do I conclude "it's as if there was another universe" or "there is another universe"? However unavoidable the question, I'll never be able to settle the matter empirically. Hence my bias is to say that's not a scientific question.

12. **Scott Aaronson**
September 12, 2022

I mean, like, I accept the theory of object permanence — the theory that the stuff in back of my head continues to exist, that it doesn't disappear and then reappear when I turn around — even in situations where I can't use mirrors or cameras or any other mechanism directly to confirm the stuff's continued existence. I accept this theory purely on the basis of its simplicity, even though it postulates the existence of entities that (by definition) are unobservable in principle to me at the relevant times.

13. **David Roberts**
September 12, 2022

> simultaneously engage with the many-worlds interpretation of quantum mechanics and inflationary or string theory landscape models

Or, worse, simply call radically different things 'the multiverse', one of which is literally our universe past the cosmological horizon, and one of which is the strongest possible Platonism, ascribing physical reality to every logically consistent mathematical structure.

[I rechecked the WP article to see what it said about various people's multiverse classifications, and ideas like "Given infinite space, there would, in fact, be an infinite number of Hubble volumes identical to ours in the universe." really just trigger me, as a mathematician. Thus stuff needs citations to outright mathematical criticism.]

14. **martibal**
September 13, 2022

@Marvin: Goedel solution is usually put apart because, besides the equation of general relativity, one takes into account in the description of nature some causality requirements, that seem incompatible with the existence close timelike curves. This is not as if there were a landscape of solutions to Einstein equations, all equally physically relevant, and for some unknown reason nature selects one or the other.
.
Then one may discuss if these causality requirements are legitimate (for instance, putting causality aside, is it possible for an extended body to have all its point following close timelike trajectories ? If not, then is this really a causal problem if an elementary particles follows a close time like curve ? etc). It might even happen than in the future, one finds some situation in which Goedel solution makes sense. But this does not mean that in our universe Goedel solution is excluded, and it exists in some "multiverse".
.
It is a bit like Dirac equation: energy was not supposed to be negative but if, in a precise mathematical sense, one takes solution of Dirac equation of negative energy, then one discovers antimatter. This does not mean that matter lives in a universe, and antimatter lives in another one.

15. **martibal**

September 13, 2022

@Peter: I agree with your view on string landscape (it is inconsistent both with experiment and from the mathematical point of view). I am just a bit puzzled by your disagreement with Sabine.

Maybe this is just a matter of vocabulary, whether one intends by "multiverse" the elements of a theory that cannot be directly observed (for the moment, but with no certainty that they may not become observable in the future) or another "reality" which, by definition, has not any possible interaction with our universe.
.
Yet, I do not find innocent that string's hype use the word "multiverse". It is again a way to oversell the theory using some "sexy" terminology: the "tiny little vibrating strings that describe everything" are now replaced by "a landscape of possible universes". This is far more appealing to the public than twistors or noncommutative algebras. Pointing this out, as Sabine does, does not seem to me so useless.

16. **Peter Woit**
September 13, 2022

martibal,
Just to make clear my problem with the string landscape: it's not that it's inconsistent with observation or mathematics, it's that there is no theory. People insist on talking about this subject without noticing that the "theory" they are talking about is nothing more than some hopes and dreams adopted by a group of people as a tactic to avoid admitting that a specific research program has failed utterly. My main disagreement with Sabine and others is that they argue as if there is a theory, engaging in arguments about what is "real", what is "unscientific", what is "ascientific" etc., etc. which are completely besides the point. If you argue about this kind of thing, there's no right answer and it completely obfuscates what is going on, and lets the people doing this get away with it (and "it" is convincing people unification is hopeless, and salting the ground so that no one will ever try again).

For more along the same lines of what I'm disagreeing with, this morning there's a tweet from Will Kinney
https://twitter.com/WKCosmo/status/1569647758604124163
"This gets multiverses exactly right: they arise naturally out of the math, but there is no way — even in principle — to know whether or not other universes exist."
He's referring to certain specific models of inflation, which have nothing to do with the string theory landscape models where you get different physics in different universes. People really need to make substantive arguments about specific models, not empty statements covering all sorts of completely different sorts of theories.

Also, not a big fan of bringing "math" into these empty arguments. The sort of math used in these models is very basic college-level stuff. Referring to "math" this way is a bit like if whenever I discuss anything with anyone, I make a big point of going on about how they are using "English", as if that were relevant to their argument.

17. **Thomas Schaefer**
September 13, 2022

Consider the EW phase transition in the standard model. The SM is sufficiently well established that nobody would seriously argue with the statement that this transition took place in the early universe (people might argue about the details). However, it is unlikely that any super-advanced civilization will ever observe the transition in the laboratory (the energy density is 10^8 times higher than the critical energy density of the QCD transition, and the energy density created in pp or heavy ion collisions only grows very slowly with the beam energy in a collider, roughly like the inelastic cross section log(E)^2).

We could imagine that the SM predicts (it does not — but it could have) that the transition produces domains in which the strength of the weak interaction differs. I would be prepared to consider this as a multiverse scenario, one that does rely on well established physics.

18. **Peter Woit**
September 13, 2022

Thomas Schaefer,

Thanks, that's a good example of how one might have a well-tested theory that implies unobservable regions with different physics.

This inspires a different example of the point I'm trying to make. One can in principle imagine "brane-world" scenarios in which at LHC energies one could excite brane degrees of freedom that would decouple from our brane as a separate stable brane. Suppose one started doing these experiments and saw exactly what the theory predicted, including phenomena corresponding to exactly what one would expect to see in such a decoupling event. Should one say that such decoupled branes don't "exist" and any reference to them or their properties is "unscientific"?

19. **Low Math, Meekly Interacting**
September 13, 2022

I'm inclined to think there's an important distinction between assuming something empirically manifest continues to exist when I'm not looking at it, and assuming something I never observed and can never be observed, ever, does. I don't find conflating such things to be persuasive enough, but I do admit those who do have a point.

20. **Joseph Conlon**
September 14, 2022

Kaku has become a crank (the one good thing about him writing books about quantum computing is that he is not writing them about string theory). I wasn't aware of his talk at the Oxford Union; the Oxford Union is an undergraduate student debating society (albeit one with a long and notable history) with a reputation of inviting controversial speakers, mostly on politics, and no one looks to the Oxford Union for guidance on the state of theoretical physics.

You are not a crank and I enjoy reading your blog. I do (obviously) think you wrong on the scientific substance, and, yes, do not think the UK's professional society for physicists should be presenting you to the public as a scientific authority on unification (and the level of research activity does indeed feed into this view). (But, also, as I said elsewhere, I am strongly against rescinding invitations once made.)

But there is nothing personal to this; your views are well known and while I think them wrong and misguided, they are always expressed in reasonable, if direct, language. I'd be quite happy to meet for coffee or something similar when you are in Oxford, let me know if interested.

21. **Peter Woit**
September 14, 2022

Joseph Conlon,

Thanks for the comment! I'll contact you privately to see if we can find time to meet up when I'm there, I would enjoy doing that.

I'm in the middle of preparing that talk, it will mostly be things that much if not most of the theoretical physics community would agree with (even the skepticism about string theory unification). At the end there will be something about my own recent ideas about unification, with warning labels these are speculation not now vetted by the community. Some of this will be about a fundamental role for twistors, which should be less controversial in Oxford than anywhere else in the world.

22. **jack**
September 14, 2022

Good to see a cordial exchange here between JC and PW. However I'd like to suggest, with all due respect, that JC is on rather shakier ground relative to PW than he seems to suppose. If fundamental physics has been stuck for a generation, who contributes more: one whose wheels spin at high speed (=copious "research output") or one who tries to show that the spinning is idle? This is science, not history, so we can't say definitely and for all time that the spinning *is* idle. But we also can't say – and given recent history really shouldn't say – that the spinning *isn't* idle. This is so despite what is surely a very strong tendency among wheel-spinners (if that's what they are) to regard what they do as progress. This suggests two very different kinds of interest: that of "active researchers" in a specific research program, and that of practitioners more generally who believe that the research program is a failure. Even if PW weren't proposing to share speculative ideas in fundamental physics at Oxford but only a critique of what he takes to be a failed research program, those who think of themselves as contributors to the program would do well to receive him not just cordially but modestly.

23. **Joseph Conlon**
September 15, 2022

"If fundamental physics has been stuck for a generation, who contributes more: one whose wheels spin at high speed (=copious "research output") or one who tries to show that the spinning is idle?"

`If" is having carry quite a lot in this sentence: certainly, *if* fundamental physics (or anything else!) is headed down a dead end, there is no credit for continuing to drive in that direction. But, generally, my view of the history of theoretical physics is that everyone should be rather humble about their ability to judge good ideas and dead ends on timescales much less than a couple of decades, and in many cases much longer than that.

And I never really get why people think string theory is an example of failed program (as I describe in my book, it has delivered too much to many different areas). To me, the highway which really is littered with the corpses of dead and never-to-be-resurrected ideas is that of BSM model building.

24. **Pedro**
September 15, 2022

Peter Woit,

Have you ever written down your thoughts on MWI? I would be curious to know.

25. **Peter Woit**
September 15, 2022

Pedro,
I don't remember exactly where I wrote about this. If you look through the blog postings in this category
https://www.math.columbia.edu/~woit/wordpress/?cat=21
you'll find various places where I wrote something.

I don't want to start a discussion here, but a quick thing to point out is that MWI proponents don't have any new equations or new theory, they say just use the standard Schrodinger equation. They argue that talking about a multiverse solves interpretational problems, but to me this just is verbiage that doesn't address the difficult underlying questions, which really are about how to understand the physics of how classical behavior and our picture of a classical world emerges from the quantum theory.

26. **Alex**
September 16, 2022

Peter writes

"… and lets the people doing this get away with it (and "it" is convincing people unification is hopeless, and salting the ground so that no one will ever try again)."

That may be one possible angle. But lately I have been wondering that, while perhaps not hopeless on scientific grounds (although that's open to debate), it seems hopeless to me in terms of how the community reacts, the particle physics community in particular. They seem to have very rigid preconceptions on how unification should work and be done. If your theory doesn't meet that, then they will ignore you. A case study for me regarding this has been Connes' spectral standard model. It's a purely geometrical unification of all forces and even the Higgs field into a single framework and action functional. The early versions had many problems, but the current incarnation is quite good. My point is not to do propaganda for this model, but it's quite remarkable how little attention it receives from that community. My feeling is that this is because the unification is classical (and then unification doesn't seem to solve the non renormalizability of gravity) and that no new, beyond SM particles/fields are predicted at higher energies (maybe only one more at most). These two things go against core traditional beliefs in the community on this topic since many decades.

So, it seems to me that, even if you are Connes, Fields medal etc., unification is hopeless. What's left for us common mortals, then? Unification seems hopeless because the particle physics community is making it so. It's not even the science what is counting here. Maybe that's one of the reasons nobody will ever try again, why bother? Nobody will listen to you. I'm very pessimistic, to be honest.

27. **The Angry M-Theorist**
September 16, 2022

>>People insist on talking about this subject without noticing that the "theory" they are talking about is nothing more than some hopes and dreams adopted by a group of people as a tactic to avoid admitting that a specific research program has failed utterly.

Seriously, Peter? We *do* actually have a nonperturbative string theory—not a set of "hopes and dreams", but a complete theory. AdS/CFT tells us that string theory on anti-de-Sitter space is dual to N=4 Super-Yang-Mills. I see holographic dualities every single day in my research, and that's actually how I *define* non-perturbative superstring theory on that background—the dual to the superconformal field theory. It's clear that you're still living in the 1990s, unwilling to accept, or even unaware, of the enormous progress in understanding the nonperturbative behavior of string theories enabled by holographic dualities. We have a theory now, Peter.

And as somebody who is so enthusiastic about the Standard Model and QCD, I'd imagine you'd be ecstatic about the enormous progress in understanding the nonperturbative behavior of QCD that has been enabled by string theory and AdS/CFT and similar dualities. Surely that progress in understanding QCD is more than enough to justify this research project. But I guess you'd rather stick your head in the sand than admit defeat.

28. **Peter Woit**
September 16, 2022

The Angry M-theorist,

A major failed hope and dream is that AdS/CFT has anything to do with the real world (it says nothing about 4d string vacua that would give the SM + gravity).

This kind of anonymous comment is word for word the kind of thing I got a lot of nearly 20 years ago. Back then I wasted too much of my time trying to have serious discussions with such commenters. Now I'm a lot older, and somewhat wiser…

29. **The Angry M-Theorist**
September 16, 2022

Okay, Peter. Let's talk about the landscape. It's extremely frustrating to me that you, and the other folks in this thread (Sabine, etc.) keep harping on this point that the landscape of string vacua furnishes no predictive power. The opposite is actually true. Statistical surveys of string vacua have made predictions about particle masses borne out by experiment. We aren't at the point yet where we can predict precise ranges of physical constants from statistical arguments about string vacua, but we HAVE made predictions confirmed by experiment. This is not my research area, but I invite you to read this excellent review article

https://arxiv.org/pdf/2202.11578.pdf

Examining vacua compatible with KKLT moduli stabilization, the authors construct a distribution of SUSY masses and discover that sparticle masses are likely beyond the reach of the LHC. Thus, the fact that the LHC has not yet found SUSY is, in fact, resounding experimental confirmation of the KKLT landscape of string vacua. It's hilarious to me that you keep yapping here on your blog about the LHC "not finding SUSY." while that's actually the biggest piece of experimental evidence for string theory and the KKLT landscape we have.

30. **Peter Woit**
September 17, 2022

The Angry M-Theorist,
This really does take me back. The problem with anonymous comments like yours is that it's impossible to tell whether the commenter is a troll or an idiot. So, at some point I just started deleting all these things. Will revert to that policy now.

31. **Paolo Bertozzini**
September 19, 2022

@Alex (16 September)

I find the remarks, especially those regarding Connes' approach, inspiring and deserving some further discussion, so please forgive me if I am going a bit out of topic here.

First of all, I would like to point out a nice interview of Alain Connes in chapter 6 of the relatively recent book "Conversations on Quantum Gravity" by Jay Armas.

There (at the end of page 139) in the discussion on the discovery of the mathematics underpinning the renormalization process, we find the following sad and disillusioned statement:
"These breakthroughs were enough for me to come to terms with my discontent with the method used by physicists. The sociological fact that they have not been understood by physicists nor are they part of their general knowledge is totally unimportant to me."

One might of course imagine that all of non-commutative geometry has been receiving the same cold reception by the community of theoretical physicists for very similar reasons (lack of ability to appreciate the mathematical details) … for sure, the initial failed prediction of the Higgs mass (coupled with the usual "Popper nonsense" about crucial experiments ditching entire theories) has been easily exploited by the "mathematically lazy" … but there might be a few other factors involved here.

While, in one way or another, string theory or loop quantum gravity have been claiming from the very start to propose fundamental theories of physics solving both (ST), or at least one (LQG), of the unification and the quantization of gravity problems, non-commutative geometry (apart from rare exceptions, for example Jesper Grimstrup's attempt) has been less assertive, more humble in goals and has almost never been considered, even by Alain Connes himself (private conversation in Cargese 1997), as an approach dealing with foundations of quantum gravity: the spectral triples of Connes' standard model are supposed to be a description of geometry only at the (Euclidean) "classical level" (see page 152 of the interview), to be further subject to "second quantization" and "renormalization" treatments, in exactly the same way and with exactly the same issues as any other classical field theory undergoing quantization.
Hence it is not surprising that talks and contributions in non-commutative geometry often appear in the "phenomenology" section of physics conferences, where people consider "approximate models".

My (wrong?) feeling is that the possible fundamental nature/meaning of "(non-commutative) geometry in a fully covariant" field theory context is left unexplored also because of some current deep ideological impasse: when all of the quantum gravity communities are in perfect agreement on the "emergent nature of space-time geometry" (and the lack of meaning of geometry in a fundamental theory) and somehow even Alain Connes himself does not dare to question such dogma
[quoting for example from page 140 and the mid/end of page 149 of the previously mentioned interview:
"I find it quite an ill-conceived idea that one could quantise gravity as one quantises all other fields because I don't believe that geometry makes any sense, say, above the Planck energy. This belief is shared by many people. I think that in the same way that the Brout-Englert-Higgs particle emerges from symmetry breaking [16, 17], the actual geometry of spacetime also emerges from symmetry breaking and does not make sense beyond a certain energy level. One of my leitmotifs for developing non-commutative geometry has been to try to understand how geometry can actually emerge from purely Hilbert space considerations."
"In particular, we have to imagine a more fundamental theory in which the geometry doesn't make any sense beyond the Planck scale but appears out of spontaneous symmetry breaking when the temperature is lowered."
"So my belief is that there is an enormous symmetry which is the symplectic unitary group; this symmetry is spontaneously broken and only then the geometry emerges. This is one possible scenario and if it holds it shows that it is vain to try and quantise gravity in a given geometrical background in which case one is quickly confronted with the lack of renormalisability, since at sufficiently high energies the geometry itself will have totally disappeared."]
… there seems to be little space left to propose variations to the common narrative in which non-commutative geometry is just taking the place of classical geometry in some mid-low energy emergent scenario adding "corrections" to usual treatments, rather than conceptually changing the panorama.

Of course, the "emergence of geometry from Hilbert space" has also a much deeper meaning, where "emergence" stands for a new "spectral approach" to geometry (but then a full spectral theory of non-commutative algebras is still missing and taking the usual "dual" point of view, shifting focus on algebras, … seems to be not enough to give a clear meaning to geometry in quantum gravity).

Best Regards (sorry for the long post) 😉
Paolo

32. **Guy in CS**
September 19, 2022

"Arguments pro or con about the "multiverse" that simultaneously engage with the many-worlds interpretation of quantum mechanics and inflationary or string theory landscape models are a waste of time. These are two completely different subjects, which raise completely different issues and have nothing to do with each other."

What about this paper, Peter, which suggests otherwise (i.e., that the MWI and eternal inflation are somehow related?)

https://arxiv.org/pdf/1105.3796.pdf

Does this offer you any great insight into how classical behavior emerges from quantum? Or into inflation?

33. **Peter Woit**
September 20, 2022

Guy in CS,
Waste of time applies. I've never heard of anyone who thinks that makes any sense.

34. **Anonymous Theorist**
September 20, 2022

"I've never heard of anyone who thinks that makes any sense."

What about Leonard Susskind and Raphael Bousso? Presumably they think the idea makes sense? And presumably you've heard of them?

If you've identified flaws in their reasoning, perhaps you could bring that up with them, rather than categorically rejecting their work as "nonsense."

By the way, Bousso has over 8,000 citations, and Susskind over 39,000. So I find it hard to believe that, working together, they would write something that makes no sense to anybody.

35. **Peter Woit**
September 21, 2022

Anonymous Theorist,
The "Multiverse Interpretation" paper was eleven years ago. If you can find a recent article by anyone (including Bousso-Susskind) that explains this in a comprehensible manner and makes a compelling case that it is not nonsense, please provide that rather than citation counts.

I recall a comment from Steven Weinberg somewhere that he could make no sense of this. Weinberg was more polite, less polite was Lubos Motl, who wrote "they're on crack", see

https://www.math.columbia.edu/~woit/wordpress/?p=3723

36. **Anonymous Theorist**
September 22, 2022

Peter,

A couple points.

1. Please think about the way your attitude here would affect a vulnerable young graduate student or a postdoc who's trying to make it in the competitive environment of high-energy theory. When they see a professor like yourself trashing others' work, calling it "nonsense"—the work of physicists with tens of thousands of citations under their belts—won't they be afraid that the established professors would trash their own unorthodox ideas even more? Graduate students and postdocs need to feel like their potentially unorthodox theoretical contributions will be welcomed, rather than trashed as "nonsense." You are really doing a disservice to the community with this kind of language. Again, point out a specific flaw in Susskind's reasoning rather than categorically condemning his idea.

2. If there's one thing I can't stand, it's hypocrisy. You claim that string theory is "not a true theory." There's a complete theory for perturbative superstring scattering amplitudes on flat ten-dimensional spacetime, but no complete "theory" that would tell us about the moduli space of four-dimensional string vacua. It's true, there is only the conjectured "M-Theory," and we don't yet have a handle on what the degrees of freedom are for that theory (although we have some ideas). But if that's your central criticism of the string research project, I could EQUALLY WELL say that about your "unification theory," Peter! Your so called twistor unification of gravity and the standard model is a joke. It's manifestly background dependent. In fact it's all on flat spacetime. The twistor geometry here only tells you about flat Minkowski spacetime. The Penrose-Ward transform here only tells you about conformally invariant wave equations on Minkowski spacetime. You're examining quantum fields on flat four-diemsnional space. So when you claim that you have a theory of "quantum gravity" and the geometry is organized around flat Minkowski spacetime with the fixed background, it makes me laugh. How can you attack string theory when your own unification theory only works on a flat Minkowski background?

37. **Peter Woit**
September 22, 2022

Anonymous theorist,
I think you're doing much greater damage to young theorists by insisting that the way to evaluate theoretical ideas is by counting up the citation numbers of the authors, and that authors with high citation numbers cannot possibly be writing nonsense. If you think the Bousso-Susskind multiverse interpretation should be taken seriously, explain why or point to such an explanation. It's not my job to write out detailed explanations of why some particular piece of nonsense that no one is paying attention to is nonsense (despite the author's citation numbers).

About string theory, the problem is not that it's an incomplete theory, it's that everything that is known about the theory points to it looking nothing like the real world. You can only get the real world in a very complex and poorly understood way, and it looks like if that works you could get anything you want the same way.

About my own ideas, they're definitely much more incomplete than string theory. I'm enthusiastic, continuing to work on them and in the middle of writing up more. And no, what I'm trying to write down in more detail is not a theory of flat space-time. Gravity is included in a chiral version, the sort of thing Ashtekar and then the LQG people have worked with, with some differences (including working in Euclidean signature). But, this is really off-topic here, and given that you evaluate ideas based on citation counts, I'm clearly not going to get anywhere with you.

Oh, and also, I'm currently on vacation in one of the most spectacularly beautiful places I've ever seen, in the Lake District of England. So, shouldn't be spending time on this anyway…

38. **Felix**
September 22, 2022

Speaking of hypocrisy:

Anonymous Theorist, point 1: don't trash other people's unorthodox ideas, as it might discourage others

Anonymous Theorist, point 2: Peter Woit's unorthodox idea is 'a joke' and 'makes me laugh'

39. **Sam**
September 22, 2022

"I could EQUALLY WELL say that about your "unification theory," Peter!"

Some red flags from Anonymous Theorist tells me to leave well alone:

1) Presenting arguments in ALL CAPS.
2) Using "quotation marks" to undermine others work.
3) Attacking others work instead of defending your own position.

Enjoy the Lake District. It is a beautiful place 🙂

40. **Peter Woit**
September 22, 2022

Felix/Sam,
The problem with anonymous comments like that one is that I honestly can't tell whether they're written seriously, or whether they're written by a troll trying to discredit string theory (and waste my time…).

41. **martibal**
September 22, 2022

@anonymus theorist: String theory has been involving thousands of people, for many years, it has spread into common culture thanks to lies ("the idea is very simple: the world is made of tiny little vibrating strings"), trying to impose itself by intimidation ("the only game in town", "no other approaches to quantum gravity is viable") and claiming to absorb other ideas in a strongly dishonest way ("non commutative geometry is just the geometry of string theory" "Connes model are the low energy limit of string theory"). So please keep a low profile regarding hypocrisy and any other moral qualities.

Regarding Peter, your attitude makes me think of those heterosexuals that complain about the gay pride and want an heterosexual pride: as far as I know, no kids have been kicked out from home because straight. As far as I know, Peter's idea are not studied in the most reknown institute, receiving large quantity of money (how many breakthourghprize to twistors ?).

And please, don't pretend to take care of young people's future. If you were really concerned with this, then you would urge young people to have a critical view on strings and not to listen to propaganda.
Some years ago I have heard Vaffa at ICTP in Trieste, followed by Connes. Vaffa's propaganda in front of students that, most likely, could not know the real state of the art of the field was a shame.
But at least he put his face.

42. **Topologist Guy**
September 22, 2022

Peter,

For what it's worth, I'd put money on "anonymous theorist" being an actual theorist and not a troll. They clearly have some technical knowledge about string/high-energy theory. If they are a troll, they're probably somebody in the hep-th community and not a random layman. It seems to me that a genuine internet troll would be more interested in stirring up culture war nonsense than, say, wasting the time of a moderately well-known physics blogger.

Anyway, the arguments here are almost nostalgically reminiscent of the 2000s era string wars. A blast from the past, as it were.

43. **Anonymous nontheorist**
September 23, 2022

Peter,
A troll or not a troll, it is actually these kinds of heated arguments that make your blog more interesting than others. Please keep replying wisely to these kinds of attacks from arrogant people.
Btw, I find the citation count for judging the value of others' works an extremely interesting topic (Susskind himself used it in one of his arguments against Smolin). Why don't you devote a whole entry in your blog just for this citation count issue? It is at the root of the socialogy that led to the present crisis in high energy physics.

44. **Peter Shor**
September 24, 2022

@Anonymous Non-Theorist:

It seems to me that citation count is close to useless when you have a faddish area (high-energy physics) some of whose leading practitioners are producing vacuous papers which it seems everybody feels compelled to cite anyway, because otherwise they might upset these practitioners, something which they fear would negatively impact their careers.

Mathematics is much better off — when Michael Atiyah announced his proof of the Riemann hypothesis, most mathematicians paid no attention to it.

45. **Anonymous nontheorist**
September 24, 2022

Thank you Peter Shor!
Your point raises another interesting topic which is: why socialogy works in high-energy physics, but does not in mathematics. That's why I asked Peter to open a new thread in his blog for this citation issue because it's a rich (but off) topic. I remember Peter promised once in this blog to dedicate a post to the difference between math people and physics people and how can one be both. So maybe he could include in that topic this citation issue as well 😉

46. **Nick**
September 25, 2022

Re Peter's early comment beginning: "Sabine (and LMMI), Different people think about this differently, here's my take: …"

(First-time reader here.) This is one of the best short statements I've come across of how theories-models-observations relate, and what counts as evidence, in any scientific inquiry. So, thanks a lot Peter!

Do you (or others who are sympathetic) have any recommendations for longer no-nonsense elaborations of this particular "take" on these epistemological issues. (I know they have been discussed endlessly over centuries, just asking if there is any article, book, or blog post that someone thinks is especially good/worth reading). Thanks!

47. **Adam Treat**
September 26, 2022

Scott Aaronson, the point is that when you accept such a theory you are doing philosophy (however reasonable and "obvious" you might think that philosophy is!) rather than science.

48. **Pascal**
September 28, 2022

Adam Treat: not quite. It follows from accepted physical theories such as Newtonian physics that the back of my armchair continues to exist even when I do not lean against it, or look at it. You may try to cook up a physical theory in which the back of the armchair disappears, but it's simply not the theory that's in physics textbooks. Asking what are the consequences of a given physical theory should still be called science (armchair science?) rather than philosophy, in my opinion.

Something about England

September 27, 2022
Categories: Uncategorized

Heading to Oxford today, this evening I'll give a talk there on Unified Theories of Physics. On Saturday I'll try to find some way to get to the HTLGI Festival in London despite a national rail strike, where I'll give a talk on Saturday and be on two panel discussions Sunday.

I'll post slides after the talk tonight, one theme of which will be the failure of a series of attempts to extend the Standard Model, all of which were started in the mid-1970s (GUTs, SUSY, string theory). An opinion piece by Sabine Hossenfelder appeared yesterday in the Guardian, which takes a similar point of view on the current fate of extensions of the SM, but I strongly disagree with a lot of what she has to say.

The bad theory activity she points to has been going on for decades, but in recent years it seems to me to be a lot less popular. Most influential theorists have (quietly) agreed with her that particle physics is dead. In attacking bad model building in particle physics, I think she's going after a small group of stragglers, not the center of theoretical activity (which has problems much more worth discussing).

What I most disagree with her about though is her treatment of HEP experiment and experimentalists. Yes, one can find people who have used bad theory to make bad arguments for building a new machine, but I don't think those have been of much significance. For more on the current debate about this, see here. At the present time though, no one is spending money on building a new energy frontier machine any time soon. Money is being spent on running the LHC at high luminosity (CERN) and studying neutrinos (US), as well as studying the possibilities for going to higher energy. All of these activities are valuable and well-justified.

The LHC has been a huge success so far, with the old claims that it was going to see extra dimensions an embarrassment which doesn't change the science that has happened. The discovery of the Higgs was a huge advance for the field, and the on-going effort to study its properties in detail is important. Another huge advance for the field has been the careful investigation of the new energy range opened up by the LHC, shooting down a lot of bad theory. Pre-LHC, the most influential theorists in the world heavily promoted dubious SUSY extensions of the SM, making these arguably the dominant paradigm in the field. LHC experimentalists have blown huge holes in that bandwagon, in some sense by doing exactly what Hossenfelder complains about (looking for evidence of badly motivated theories of new particles). In this story they're not the problem, they're the solution.

I'll be busy this week with the talks mentioned and with attending math talks in Oxford, so little time to discuss more here or do a good job moderating a discussion. So, behave.

Update: The slides from the Oxford talk are here.

Update: Sabine has a blog entry more carefully explaining her point of view here.

Update: Some coverage of this at Physics World.

Update: More discussion of this from Ethan Siegel here, response from Sabine here.

Comments

1. **Sabine Hossenfelder**
   September 27, 2022

   Peter wrote:

   "Most influential theorists have (quietly) agreed with her that particle physics is dead."

   I am not concerned with "most influential theorists" (in particle physics) but with "most theorists" (in particle physics). That is, I'm concerned with the 99.9% that make up the big bulk. You are referring to the 0.1% at the top, ppl who are swimming in grant money and can do whatever they want. I can only guess, but quite possibly the sample of people you personally get to meet and talk to is strongly biased towards the top.

   The problem with those 0.1% is that, as you say correctly, they have been very quiet when instead they should have been pushing for the systemic change that is badly needed.

   Second issue with your criticism is that you somehow ended up claiming I'm commenting on HEP in particular. I actually don't know why that is.

   If you think that LHC experimentalists have blown "a hole into the bandwagon", you seem to not have noticed that most of them have just jumped off this wagon and onto the astroparticle wagon while they're waiting for the plans for the next bigger collider that will make the next big bandwagon.

   Experimentalists make themselves guilty by just not caring how badly motivated many of those theories are to begin with. They excuse themselves by claiming "I'm just an experimentalist, I just test this stuff" and pocketing the money. It's not an excuse I'm willing to accept.

   This field is in dire need of a methodological change. The situation is very similar to that in psychology 15 years ago, when they realized that it had become very common to publish results based on sloppy statistics that ended up being irreproducible. Psychologists managed to mobilize the community and more or less agree on guidelines for better methods. At least in my mind it has been a remarkable success that proves it is possible for academic fields to undergo community-driven changes in methodology. Particle physics needs to do the same. If psychologists can do it, so can they. They should start by analyzing what went wrong in the first place, but that still hasn't happened.

2. **Peter Shor**
   September 27, 2022

   Sabine:

   The LHC is running, and as far as I am aware, by far the biggest cost is just making the particles collide. And it's certainly seems like it's worth it, just from the amount we're learning about the Higgs boson, pentaquarks, and other things that really exist. Experimentalists running data analyses on the results to eliminate new particles that theorists have proposed is a relatively minor additional expense.

   What would you suggest they do with the data — just leave it there unanalyzed?

3. **Mark**

September 27, 2022

“Experimentalists running data analyses on the results to eliminate new particles that theorists have proposed is a relatively minor additional expense.”

As a member of one of these large collaborations, my experience is that not many (relatively) people do this nowadays. It’s become rather a niche activity. In the early days when we were told by theorists to expect squarks/gluinos etc on day one such analyses were very popular activities.

4. **SteveB**
   September 27, 2022

   I have never heard Dr. Hossenfelder say that the LHC should be turned off or end its current run. I have heard her say that a new, bigger, more expensive collider is not justified, at least using the justifications put out by the promoters. The promoters still use SUSY or wishful thinking to promote it, or even “we have to keep all these trained HEP folks employed.” The next machine as proposed is too expensive for those kinds of arguments.

5. **Dirac**
   September 27, 2022

   Small edit to your slides: Dirac’s tomb is not in Westminster Abbey (its just a marker there). He is in fact buried in Florida 🙂

6. **Anon42**
   September 27, 2022

   In a 2006 comment on your blog post at https://www.math.columbia.edu/~woit/wordpress/?p=406 about LHC predictions Sabine Hossenfelder wrote “I favour the scenario: they find nothing at all. No higgs. No susy. No monopoles. No nothing.” When Lubos Motl claimed in a follow up comment that this was impossible she questioned “the validity of the axioms you use to draw the conclusions.” Her scenario seemed even less well motivated than the models she now criticizes, and of course the LHC didn’t just find “nothing.” Unfortunately her current article linked above has no transparency or reflection about this track record. She now writes “The Higgs boson, on the other hand, was required to solve a problem.” Should Dr. Hossenfelder be the one adjudicating particle theory predictions.

7. **Mark**
   September 28, 2022

   “The promoters still use SUSY or wishful thinking to promote it, or even “we have to keep all these trained HEP folks employed.””

   Most of the literature I have read on the topic emphasises improved precision on the measurements of the properties of the Higgs particle.

   ““we have to keep all these trained HEP folks employed.””

   that’s what hedge funds, google, amazon etc are for! Very few experimental particle physicists actually work as academic scientists, so no-one is arguing for a new collider to create jobs for particle physicists (at least I have never come across this).

8. **Low Math, Meekly Interacting**
   September 28, 2022

   I still don’t understand why building a Higgs factory for no other reason than to better understand the only known (and probably) fundamental scalar can’t be made into a compelling argument on its own. Yeah, it’s expensive, but so what? It’s not stupid, and nations blow money on stupid things constantly, in a seemingly compulsory manner.

   Still no confidence in muon colliders, I see.

9. **Shantanu**
   September 28, 2022

   Peter, I am surprised that you are supportive of neutrino experiments. What exactly will the next generation experiments measure which will help connect it to TeV scale physics.
   Its 24 years since we “supposedly” have evidence for physics beyond standard model through non-0 neutrino mass and yet influential theorists have privately admitted to you that particle physics is dead. so help me understand the paradox.
   So my question to you is what measurement are you eagerly waiting for from next generation neutrino experiments which make particle physics rise from the ashes?

10. **Dan Riley**
    September 28, 2022

    It seems worthwhile to emphasize that the current proposals are conceptual designs that are likely at least a decade (probably more) from any kind of major funding decision. The R&D activities to advance the conceptual designs to something solid are relatively cheap and usually generally valuable (especially work on more compact superconducting magnets and RF cavities, plus alternative strategies for reaching higher energy), even if that work doesn’t directly lead to a funded accelerator complex. So I’d encourage more of a “wait and see” attitude to the current conceptual designs.

    p.s. working for an LHC experiment, expect to be retired long before major funding for any kind of next generation facility becomes a real issue

11. **Marvin**
    September 28, 2022

    I agree with Dan Riley, we must keep investing at small scale to test improved devices or new acceleration techniques. Wake field is an exemple of novelty. These small scale research will also have impacts on various industries, you can think of electricity storage and transport. I think that building an accelerator with actual technology and just reaching one order up in energy is, as probably Sabine tough, a waste of money: there is no theoretical predictions that necessitate any rush.

12. **Peter Woit**
    September 28, 2022

    LMMI,
    The problem is that the LHC has actually already done an impressive job studying the Higgs, and the results after the HL-LHC should be even better. To justify a new machine, you have to show the improvement over the HL-LHC is going to be worth the high cost.

    It may ultimately be that people decide to wait for muon collider technology instead of building a new energy frontier pp or e+-e- collider. But that technology I think is many years out.

13. **Peter Woit**
September 28, 2022

Shantanu,
The neutrino sector is the least well-understood of the standard model, and you can study it better without having to deal with the fundamental technological limitations that make new energy frontier experiments exorbitantly expensive. I don't have anything specific to point to, but (here I somewhat disagree with Sabine) I think it's up to experimentalists to measure what they can. Maybe something unexpected will turn up about how neutrinos behave , maybe we'll just get better measurements of SM parameters.

14. **Robert A. Wilson**
September 29, 2022

The point of theory-building, I suppose, is that the theory should be, as Einstein said, as simple as possible, but not more so. Or, in other words, as complicated as necessary, but not more so. More or less all theories since the Standard Model have been "more complicated than necessary", either in the sense that they predict phenomena that do not actually occur in the real world, or that they cannot predict anything at all. That is why I find your twistor model of gravity so appealing – it is quite clearly just as complicated as necessary, and not more so.

15. **M.**
September 30, 2022

Anon42 writes:

"Should Dr. Hossenfelder be the one adjudicating particle theory predictions."

Well, first of all, she isn't. And second of all, there is a real difference, I think, between a comment in a single thread on a science blog, and a model/prediction published in the literature. Everyone is wrong sometimes. What's notable, even in that blog comment, is how much Hossenfelder got right (e.g., no monopoles, no SUSY, etc.)

16. **Paul**
September 30, 2022

Null experiments can be just as important as the confirmation of expected theoretical findings. Just saying…

17. **Kevin Black**
October 1, 2022

Hi Peter,
Thanks for the post. I think the point is that the HEP energy frontier experimental community is mostly working on the LHC which is currently the only place where the Higgs boson can be produced and working on analyzing that data. There are some measurements which can be already classified as precision (i.e. percent level measurements) but there are many things that are just not measured at all or very poorly measured (i.e. couplings to the second generation fermions, the higgs self-coupling, rare decays). Would anyone say that if we had experimentally confirmed that the photon coupled to half the charged particles and measured its couplings to some of those only to 30-40% percent precision that we should just call it a day? That is seems , in a word or two, absurdly anti-scientific.
HEP high energy experimentalists are also working on novel ways to get to higher energy in more compact and cheaper ways (plasma wakefield accelerators, muon colliders, etc) but nobody is proposing to build those anytime soon because we can't. CERN has plans for a larger collider but that is still decades away and even the biggest proponents of those machines understand that we are a long way both financially and scientifically from actually constructing those proposals.
Honestly, I don't know what these straw man arguments except to confuse people who aren't actually engaged or following HEP.

18. **Peter Shor**
October 2, 2022

It seems to me that there's a lot of argument, when there's probably not all that much to argue about.

1. Barring global catastrophes, we're not going to shut down the LHC until we've got all the data out of it that we can.

2. If we get to the end of the LHC run, and there is no hint that higher energies will find anything interesting, it will be really hard to make a case for building a Super-LHC, and I expect that one will not be built.

So that leaves the increasingly unlikely scenario that at the end of the LHC run, there are hints that interesting phenomena will happen at higher energies. In that case, there will be difficult decisions to make. But it seems much too premature to worry about this now.

19. **martibal**
October 2, 2022

The presentation of arXiv at Scientific World is, well… problematic:

"Described as "an open research sharing platform reinventing scientific communications," arXiv is home to preprints of many of the papers that Hossenfelder derides. What is more, you can read them free of charge and come to your own conclusions."

First, does arXiv define itself as "reinventing scientific communications" ? It has been going on for the mid 90′, and at least in hep-th and maths it is the main way of communicating science, I would say.
Second, presenting arXiv has the home for useless speculations is rather unfair. Yes there are some on arXiv, and there is also the proof of Poincaré conjecture by Perelman, the papers announcing the Higgs discovery etc. It would be nice that the journalist leaves arXiv outside of the debates.

20. **Smaugtalk**
October 4, 2022

Not really off-topic: this is going to really piss Peter off.

https://www.nobelprize.org/prizes/physics/2022/summary/

The Nobel Prize in Physics 2022 was awarded to Alain Aspect, John F. Clauser and Anton Zeilinger "for experiments with entangled photons, establishing the violation of Bell inequalities and pioneering quantum information science"

Hidden variables are irrelevant to QM, of course, and so this experiment is trivial ("non-spooky").

Posting only to be deleted, but the previous line *is* your expressed view, eh?

21. **Peter Woit**
October 4, 2022

Smaugtalk,

Actually, I'm perfectly happy with this Nobel prize and the citation. This kind of experimental study of the unusual behavior of QM is great. Even happier that they didn't give it to some theorist or philosopher for going on incoherently about what this means for "locality"…

22. **Dave Miller**
October 5, 2022

Peter,

I'm looking forward to your blog post on the Nobel and also to Peter Shor's perspective.

As someone with a fifty-year obsession with the foundations of quantum mechanics, I think this Nobel was richly deserved: I am only sad that John Bell died so young and that Abner Shimony died seven years ago.

Quantum cryptography and quantum computation are perhaps a bit oversold nowadays — but at least all of this is real physics that involves actual experiments! And it is hard to see how any of it could have happened without the seminal work on Bell's theorem.

Dave Miller in Sacramento

23. **Peter Woit**
October 5, 2022

Dave Miller,
I don't really have anything interesting to say about the Nobel, so won't blog about it. The choice was a good one, and I had actually thought they already had made this award long ago. A reaction much like mine is Philip Ball's, see here
https://twitter.com/philipcball/status/1577244753523265536

24. **former mathematician**
October 5, 2022

Maybe no one remembers this but me: When John Clauser was building his first experiment, he expected the opposite result from what he got.

25. **Peter Shor**
October 5, 2022

Since I was asked, let me say I'm very happy about the Nobel prize going to quantum information theory. And these are an excellent set of recipients, possibly being the three people who did the most for experimental tests of Bell inequalities and similar demonstrations of non-classicality.

26. **David J. Littleboy**
October 6, 2022

"When John Clauser was building his first experiment, he expected the opposite result from what he got."

This has been reported in at least one article on the prize. E.g. here's Quanta on it:

"Unsure what he would find, Clauser had placed a $2 bet that his experiment would prove Einstein right. To his surprise, his results vindicated Bell's prediction over Einstein's. "

As a non-physicist, I've been surprised by how hard this is. The Bell inequality thing seems pretty clear (at least when read in various popular descriptions) but people keep finding possible loopholes. Thus it's interesting that work from 2017 was included in this Nobel Prize. Does this make it one of the shortest periods between work done and Nobel recognition?

FWIW: there's a collection of reprints of Bell's papers (John S. Bell on The Foundations of Quantum Mechanics) that's affordable and surprisingly interesting even to non-physicists (even if we don't get most of it's points).

27. **AcademicLurker**
October 6, 2022

According to David Kaiser in *How the Hippies Saved Physics*, when Aspect started work on this he was warned that he would ruin his chances for a permanent academic position by working in such an unfashionable area…

28. **David Roberts**
October 20, 2022

Thanks for the link to the Siegel essay, it had some nice general ideas and was a fun read. However, I take small issue with the scope of the closing statement:

> The best we've come up with is this: to let people choose for themselves what they work on

While in principle this is true, the structure of academia is that people will actually choose work on what will keep them employed. And one person working on an ambitious idea (not ruled out by the usual quick theory-killers) may not have the critical mass of attention to see it go through to the end of its lifecycle (either finding why it cannot work due to some perhaps subtle technical problem, physical argument or experiment, or else be actually useful for something).

This Week's Hype

October 5, 2022
Categories: This Week's Hype

The hype campaign marches on, just three very recent examples:

- Can stringy physics rescue the universe from a catastrophic transformation?
- Cosmic strings' greatest power? Their ability to confound physicists.
- TF1 Snowmass Report: Quantum gravity, string theory, and black holes

Comments

1. **SDS**
   October 5, 2022

   The TF1 Snowmass Report is intriguing: thirty-eight authors (some of them of a very high profile) for what amounts to (mostly) fluff.

2. **Geoffrey Dixon**
   October 5, 2022

   Resistance is futile, Peter. Drink the Kool-Aid.

   Speaking of which Ethan Siegel, evidently incensed at Sabine's dreary (but correct, IMO) assessment of theoretical particle physics, wrote a rebuttal in which he says, if I interpret him correctly, that in the absence (in his view) of any clear way forward, a series of guesses (sparticles, axions, etc.) is an entirely valid modus operandi. It rules out all sorts of nonsense. So stay the course, full speed ahead, be one with the Borg.

   As usual, when I consider all these guesses, and all future guesses, I have to ask: Were your guess proven correct, how would you explain that its veracity was inevitable? If you cannot do that prior to submitting it to experimental verification, then you haven't a chance – just a job, and the moral support of like-minded ruminants.

3. **MS**
   October 5, 2022

   Although there is definitely hype around cosmic strings, extending the symmetry structure of the SM is a tried-and-true strategy to address extant (not-just-theoretical) problems. After all, the SM is a (successful) theory based on SU(3)xSU(2)xU(1) gauge symmetries. Even assuming just the SM gauge group and the Higgs mechanism, there are measurable/phenomenological consequences of this phase transition and the associated topological defects (that said, the SM EWSB version is highly constrained). If there are additional gauge (or global) symmetries which are spontaneously broken at energy scales reached in the early universe, shouldn't the hype associated with topological defects formed in these phase transitions (like cosmic strings, which have measurable effects e.g. effects on structure, lensing, gravitational waves, etc.) be differentiated from the hype around string theory which still has different "difficulties" in terms of phenomenological predictions?

4. **Peter Woit**
   October 5, 2022

   MS,
   Yes hype about cosmic strings is hype about GUTs, not hype about string theory. But the hype in this article is the same kind of outrageous hype as the usual string theory hype, completely misrepresenting the situation. The author writes
   "According to our best understanding of the early Universe, our cosmos should be riddled with cosmic strings. And yet not a single search has found any evidence for them."
   This implicitly claims that a GUT model, not the SM, is "our best understanding of the early Universe". The situation is just like for monopoles: there's supposed to be a huge problem of where the cosmic strings and monopoles are, when the SM predicts no cosmic strings and no monopoles. The theories that predict such objects are GUTs, for which we have lots of other falsifying evidence (eg no proton decay).

   The math here is pretty simple: if the Higgs potential has a minimum on a manifold X, then you get cosmic strings when $\pi_1(X)$ is non-trivial, and monopoles when $\pi_2(X)$ is non-trivial. In the SM, $X=S^3$, so the two homotopy groups are trivial and you expect no cosmic strings, no monopoles, just as observed.

5. **Peter Woit**
   October 5, 2022

   SDS,
   I think it's kind of a remarkable document. The field is facing some difficult problems, and the point of the Snowmass effort is supposed to be to come up with a plan for the future. The authors act as if there is no problem to be faced, nothing wrong with what people have been doing in the past, the way forward is to just keep doing the same thing.

6. **Peter Woit**
   October 5, 2022

   Geoffrey Dixon,
   I'm onboard with Sabine that the best way forward is to identify problems with the structure/consistency of our best theory and find ways to address those problems. Completely mystified though why others don't see that the analytic continuation of chiral spinor fields is such a problem, and addressing that should be the highest priority of all theorists.

   More seriously, I'm also onboard with Ethan Siegel: the field is in a bad state, with nothing so far working (including Sabine's favored approach), so there's some justification for pursuing lots of different approaches, even ones that usually aren't very promising ways to get successful new ideas. What's important though is that such unpromising ideas at least are new, not what we have now which is a field that has institutionalized a research effort organized around a set of old ideas that failed long ago (see the last item).

7. **Alex**
   October 5, 2022

   I disagree with the idea of just trying anything (new) in the absence of guiding empirical evidence. I'm afraid that only a couple of things/strategies are worth pursuing, but will not be popular, since it amounts to basically doing nothing or very little (in terms of new, wild ideas… you know, the stuff that people like to hear about) and just wait until new experimental evidence finally contradicts our current best theories. But how do we reach to that desired point? The history of physics is illuminating.

Newton proposed his laws of mechanics in the late 1600s. As we know, they were a monumental success. And when it seemed they were failing, it was actually because of a planet that was not yet discovered. Only the small deviation in the prediction of the perihelion of Mercury resisted a newtonian explanation. The reign of newtonian mechanics lasted for a bit more than two centuries!

Now, did physicists of the time jumped all to speculate on wild theories that would replace newton during those centuries, like today's physicists are doing with the SM? Maybe some did, but, regarding the rest, it seems to me their attitude was smarter. They either worked on applications of the theory (mainly in astronomy, to great success) or to rewrite/study its mathematical foundations (Lagrange, Hamilton, etc.) And I think that, in the part of the foundations of physics where no evident contradictions exist, that should be the adopted strategy today.

On the other hand, most other physicists were working on new and exciting phenomena, like heat and electricity and magnetism. This culminated in the theories of thermodynamics, statistical mechanics, and Maxwell's EM. And the actual hard challenges to newton came from the last two (first with Planck, then Einstein). That's because they exported the newtonian theory to realms for which it wasn't designed to deal with. So, that's where the new evidence will come from. So, the second strategy is to concentrate in realms not covered yet by our current theories. That seems a bit difficult, considering how amazingly encompassing they are, but there are still some holes here and there (e.g., quantum superpositions of ever bigger massive bodies, gravity mediated entanglement of similar bodies, and so on). The mathematics of both GR and the SM applied to other systems. More cross fertilization with other fields of science, etc.

So, I think there's a clear path forward from the current situation, supported by history, but it's not an easy one, it will not give you the possibility of activating the sausage machine of paper production and collect grants money with a bulldozer. Very little space for media super stars from the field. Instead, it requires a very careful and selective process regarding which problems to pick, the advance will be slow, and the general attitude should be more on the conservative side until news of something genuinely new finally appear.

8. **Stef Roux**
October 5, 2022

Dear Peter

You said: "Completely mystified though why others don't see that the analytic continuation of chiral spinor fields is such a problem, and addressing that should be the highest priority of all theorists."

Can you perhaps elaborate? Has it been discussed in a previous blog post? Or if not, in a paper somewhere? It sounds like the kind of thing I'd like to do.

9. **André**
October 6, 2022

@Alex: Great comment, I agree almost completely. Unfortunately, after almost ten (post-PhD) years in academia I have come to the conclusion that the only way (for me, someone who is just a decent physicist but not some genius) to proceed with this sort of serious, hard-work research on the foundations of the established theories is not possible within academia. So I am currently working on a strategy to leave the academic world and hope to find a way that allows me to do this sort of theoretical physics as a hobby.

10. **Bianca Andrescu**
October 6, 2022

> a series of guesses (sparticles, axions, etc.) is an entirely valid modus operandi. It rules out all sorts of nonsense.

The problem with this, of course, is that there is an infinite amount of possible nonsense. The more you rule out, the more nonsense you subsequently generate.

11. **Peter Woit**
October 6, 2022

Stef Roux,
See
https://arxiv.org/abs/2104.05099
I'm currently trying to finish writing something much more explicit about the analytic continuation issue.

12. **Peter Shor**
October 6, 2022

And another question very few people seem to be thinking about:

"Why is the Standard Model one of the very few Lagrangians that is compatible with Alain Connes' non-commutative geometry constructions?"

The answer is probably that most physicists don't know anything about either analytic continuations of chiral spinor fields or non-commutative geometry, and their colleagues tell them that these aren't the answer, so why should they bother learning about them (something that is quite hard; it's much easier to continue thinking about stuff they know about).

But if one of them is the answer …

13. **André**
October 7, 2022

@Peter Shor:
I can only speak for myself, but as one of these ignorant physicists refusing to learn more about potentially promising – but hard – mathematical concepts such as non-commutative geometry or twistors, my reason is not that I don't believe that one of them could be the answer.
Rather I believe that this is not the most promising way to do theoretical physics.

It's worth noting that both for the discovery of GR and QM the first 20+ years were about fiddling around with the "old" mathematical tools, e.g. statistical mechanics or Maxwell's theory. Only after there was a certain amount of understanding of the new physics (described in "old" terms) did people begin to discover its relationship to new mathematics.
Riemann was already dead for a quarter century before Einstein started thinking about space and time, nonetheless, the thought of differential geometry as a tool came much later – after already having discovered the principles of special relativity, and arguably Einstein knowing more differential geometry would not have changed much about the history of this discovery.

For the formulation of QM, the discovery of many mathematical tools was historically happening around the same time (e.g. Hilbert, Weyl), and yet, the first half century of the new theory was dominated by phaenomenological models that were strongly based on the established mathematics of classical physics.

Hence, I am very happy if mathematical physicists are working on ideas like Connes' or Penrose's, but I would be quite surprised if the next big discovery comes from one of these approaches. Most likely, I expect it to come from some experiment – maybe one of the macroscopic quantum systems in the lab type things – but if it comes from theory, then my bet would be on a deeper understanding of issues and inconsistencies with QFT in curved spacetime vs. GR or in the heuristic ideas around studying quantum mechanical systems with gravity. In my opinion, there are plenty of situations that are only very poorly understood in our existing theories (talking about fundamental issues, not complex systems etc.).

14. **Peter Woit**
October 7, 2022

All,
I've just deleted a large number of incoming comments that had nothing to do with the posting. Please keep in mind that you create a problem by posting extensive comments that may be interesting, but have nothing to do with the posting. When I allow such comments, this quickly leads to a lot more of them as everyone decides this is a good place to try and get a discussion going of whatever interests them. This then puts me in the position of having to spend a lot of time I don't have trying to sensibly moderate a discussion that has gone off into areas I'm not interested in.

15. **Anonyrat**
October 9, 2022

The Snowmass report contains this paragraph, with intriguing last sentence:

" Grand unification seems very natural in the heterotic picture (where the MSSM gauge group naturally arises as a subgroup of the larger ten-dimensional gauge group). It is less natural in intersecting brane models, and may be achievable in F-theory models with 7- branes. In all cases, details of making the unification work are subtle. (Current data is also not conclusive on the question of whether achieving unification should be a central goal or not.)"

16. **Peter Woit**
October 9, 2022

Anonyrat,

I think this can be translated as "nearly forty years of intensive research has shown string theory is compatible with just about anything, specifically either GUT theories or non-GUT theories". Since this is a document aimed at guiding funding decisions, one would think the implications of this would be obvious…

17. **Nick**
October 9, 2022

Do people know if any sociologist or historian has done a study of string theory as a research program? What I have in mind is a study that tries to provide a social-historical explanation of it/framework for understanding how it persists, starting from the assumption that string theory's prominence has not been justifiable on rational grounds for a very long time? This is something sociologists do well–explain why a social institution that doesn't fulfill the ostensive functions legitimating its existence (in this case, producing physics knowledge), nevertheless persists as if it actually is fulfilling those functions, despite convincing demonstrations that it isn't. Given the prestige and cultural influence of string theory, and the importance of the questions it purports to help answer, it seems like this is something worth understanding from a sociological and historical point of view.

18. **Peter Woit**
October 9, 2022

Nick,
I don't know about sociologists, but various historians (Sophie Ritson is one example) and philosophers of science have written about the controversy over string theory. But, as long as most of the leading figures in the field continue to argue that string theory is not a failure, I don't think academics from other fields are going to do work starting from the assumption that string theory is a failure.

At some time in the future, such academics may very well be looking back at what happened in fundamental theory starting in 1984 and analyzing it more from the point of view of what this can tell us about how science can go seriously wrong.

19. **Peter Shor**
October 11, 2022

An article in the New York Times is now hyping ER=EPR, among other things.

> Dr. Susskind and other physicists now speculate that wormholes and spooky action are two aspects of the same magic and, as such, are the key to resolving an array of cosmic paradoxes.

20. **Peter Woit**
October 11, 2022

Peter Shor,
I did see that, and as usual with this kind of hype, can't even figure out what the hypsters are referring to. Supposedly it's now all about wormholes, but what's the theory governing these wormholes?

In the NYT, Overbye does refer to two identifiable papers as the crucial advances. The first is
https://arxiv.org/abs/1905.08762
where the authors have an actual theory (JT gravity coupled to conformal matter), but it's a 1+1 dimensional QFT. 1+1 d quantum gravity is very much a toy model, with no physical degrees of freedom. Maybe some phenomenon in this toy model corresponds to wormholes and resolves the bh information paradox, but if so, the bh information paradox is getting resolved in a way that doesn't have anything to do with the problems of 3+1 d physical quantum gravity theories.

The second is
https://arxiv.org/abs/1905.08255
There the author just invokes "AdS/CFT" as the theory he is using, but doesn't use anything about a specific theory of the quantization of the 4d gravitational degrees of freedom. His conclusion is that understanding the bh information paradox can be done semi-classically:
"In this paper, we have argued that the key expected features of unitary black hole evaporation in AdS/CFT can be derived from the bulk semiclassical description of an evaporating black hole, so long as we assume entanglement wedge reconstruction."

In both papers, the conclusion seems to be that you don't need a well-defined 4d quantum gravity theory, that you can just work semi-classically or in 1+1d. Hard for me to follow the details here, but the original goal of having a well-defined theory of quantum gravity seems to have been replaced by something else.

I'm getting the impression that in the same way Susskind successfully argued for the abandonment of the idea of having a well-defined theory of particle physics (what we see is a random artifact of the unknowable "landscape"), what's going on here is the abandonment of having a well-defined theory of gravity (with gravity only understandable semi-classically as an emergent approximation to something unknowable).

21. **Christoph**
October 11, 2022

Peter, there surely are good arguments that a unified theory of relativistic quantum gravity has no equation of motion. The minimum length yields a minimum measurement error, and thus no way to define a precise equation. Also, for the same reason, single degrees of freedom are unobservable (in a way that recalls the unobservability of single gravitons). As a result, there is no way to define an equation of motion (or a Lagrangian) for the underlying degrees of freedom. Thus, the unified theory of relativistic quantum gravity can only be statistical, emerging from some underlying degrees of freedom.

But there still is the standard model. It arises, like general relativity, from these underlying degrees of freedom, with the precision that we know. In other terms, the underlying degrees of freedom determine the fundamental constants uniquely. Thus, there is no landscape, but a uniquely defined world with quantum theory and general relativity.

22. **Peter Woit**
October 11, 2022

Christoph,
I'm not asking for observable degrees of freedom, an equation of motion or a Lagrangian. If you want to tell me that
"the unified theory of relativistic quantum gravity can only be statistical, emerging from some underlying degrees of freedom" and that "the underlying degrees of freedom determine the fundamental constants uniquely" you have to tell me what those underlying degrees of freedom are, what laws govern them, show that gravity emerges and explain how to compute the fundamental constants.

Maybe you think you have a way to do this (please don't use my blog to promote it), but I don't see any evidence that Susskind et al have a program to find such a thing. Rather their program seems to be to argue that string theory is such a thing, but you can never know what it really is or how it works.

23. **Z Y**
October 11, 2022

Nick,
Coincidentally I am starting to read about the works of some schools of Sociology of Science/Scientific knowledge: the Edinburgh school and its Strong Program, and the Relativism of the Bath School being the more controversial ones. In a bibliographical search on theses matters I found an essay by Weinberg (1995) Night Thoughts of a Quantum Physicist. In it it mentions his view on the Strong Program and related schools and also this passage: "String theory is mathematically very difficult. I think it's a sign of the intrinsic health of physics that even though most physicists of the older generation don't learn string theory and can't read papers in it, young string theorists continue to get tenure at leading American universities. Our field is not (as some imagine it to be) hegemonic, dominated by an old guard of fuddy-duddies. It is very much alive to new possibilities" …the irony

24. **Christoph**
October 11, 2022

Peter, what I wanted to point out is that the statement "the unified theory of relativistic quantum gravity can only be statistical, emerging from some underlying degrees of freedom" can be made *independently* of whether these degrees of freedom are known or not. Nature *must be* like that – because of the measurement uncertainties in nature at the Planck scale and because of the impossibility to reach the Planck scale. There is no alternative.

The same holds for "the underlying degrees of freedom determine the fundamental constants uniquely". Nature must be like that – there is no alternative.

Finding theose degree of freedom is a different step. It is of course necessary as well.

25. **Peter Shor**
October 12, 2022

Christoph:

I think there's something wrong with that reasoning. The angular momentum of a single particle is unobservable (except statistically), but we have perfectly good equations of motion for angular momentum.

Quantities that are only statistically observable but have exact equations of motion are everywhere in quantum mechanics. If Schrödinger and Heisenberg had followed reasoning like yours, we would not have the theory of quantum mechanics that we do today.

26. **Christoph**
October 12, 2022

Peter Shor,

quantum theory and relativitsic quantum gravity differ.

Quantum theory and experiments show that we are able to say whether a system contains just one spin or just one particle. We can isolate a single particle and follow its motion.

In contrast, we are not able to say (or even imagine) that a systen contains just one Planck-scale degree of freedom (those which make up black hole entropy) or just one graviton. We cannot isolate one of those degrees of freedom, because the minimum length cannot be realized in any experimental set-up. (We are far away by many orders of magnitude.) We cannot follow the motion of such as single degree of freedom, because the minimum length cannot be realized, not even approximately.

In relativistic quantum gravity, only systems with huge numbers of degrees of freedom can be described: black holes, curved space, etc – in contrast to quantum theory, which allows describing single particles.

So, in relativistic quantum gravity, there can never be an equation of motion for a single degree of freedom, in contrast to quantum theory. The constituents of space are not observable one by one, but only in huge numbers.

27. **Dumans Fladskote**
October 12, 2022

"… , what's going on here is the abandonment of having a well-defined theory of gravity (with gravity only understandable semi-classically as an emergent approximation to something unknowable)."

Peter, this is not at all what is happening in the recent computations of the Page curve using the semi-classical description plus HRT formula. It is like saying that because you compute the entropy of a many-body system using thermodynamics and equations of state, you have given up on there being an in principle microscopic description of the entropy. The AdS/CFT community does not think semi-classical GR is a low-energy EFT of something unknowable, because we know exactly what the microscopic degrees of freedom are: they are the degrees of freedom of a conformal field theory.

28. **Peter Woit**
October 12, 2022

Christoph/Peter Shor,
This has gone way off topic, enough.

29. **Peter Woit**
October 12, 2022

Dumans Fladskote,
You know exactly what the microscopic degrees of freedom are? Great, I couldn't get that from reading any of these papers. What are they exactly, and what exactly is the conformal field theory?

30. **Dumans Fladskote**
October 12, 2022

It depends on what AdS/CFT example you are looking at, so for different bulk dimensions and matter fields you will have different CFTs. But compared to most things in QFT, CFTs are extremely well understood. The prototypical example is N=4 SYM. In this case semiclassical (super)gravity in an asymptotically AdS_5 x S^5 space is an EFT for N=4 SYM in certain limits. The microscopic degrees of freedom of quantum gravity are the degrees of freedom of N=4 SYM. Sure, these don't live in spacetime, but that's the whole point of emergent gravity. Of course, I cannot tell you in full generality what the emergent gravitational field in the bulk are constructed from in N=4 (but in some cases we can, ref HKLL and the extrapolate dictionary). But this is not a fundamental problem, any more than it is a problem that I don't know how to write the pion field of the chiral lagrangian in terms of QCD operators. There is some (extremely hard to compute in practice) field redefintions of N=4 SYM in terms of new variables, whose dynamics become Einstein gravity at large N and large 't Hooft coupling.

31. **Dumans Fladskote**
October 12, 2022

Let me add that we know the exact dual for JT gravity. It is the matrix model described in 1903.11115. So the matrices described there are exactly the microscopic degrees of freedom of JT gravity. As for Penington's case, his argument is sufficiently general that you don't need to know the exact matter spectrum of the bulk theory (or the dual CFT).

32. **Peter Woit**
October 12, 2022

Dumans Fladskote,
In the toy JT case I see a well-defined theory (JT + conformal matter in 1+1), even independently of the dual.
When asked for a specific theory that's going to be quantum gravity in 3+1d spacetime (dS), you're telling me the standard story about a supergravity limit of superstring theory on AdS x S^5 being dual to N=4 SYM on the boundary 4-sphere. I know that story well, and I've seen 25 years of people trying to get a 4d physical quantum gravity theory out of it, which somehow never seems to lead to a specific theory you can write down which does this. What is this theory?

This subject has clearly evolved recently and there are new aspects I don't understand (ER=EPR and the wormholes, for instance…). If there is a specific conformal theory you can write down which is supposed to have as dual 4d physical gravity, it would be helpful to know exactly what it is, and how this duality is supposed to work.

33. **Dumans Fladskote**
October 13, 2022

Peter,

But now you are changing the complaint. You are of course 100% right that AdS/CFT does not give you a dual to 4-dimensional quantum gravity in deSitter space, but that was never the complaint I was responding to. You seemed to say that the AdS/CFT community had given up on the microscopic degrees of freedom, which I was disagreeing with. You were saying "but the original goal of having a well-defined theory of quantum gravity seems to have been replaced by something else.". The point I was making is that it was recently realized that the Page curve can be computed without knowing the microscopic degrees of freedom. Just because you decide to carry out this computation doesn't mean you have given up on microscopic degrees of freedom. Furthermore, since the Page curve computation is completely agnostic to the number of dimensions, so it is pretty reasonable to be hopeful that this solution works more generally.

34. **Peter Woit**
October 13, 2022

Dumans Fladskote,

For a research community to "give up" on a goal doesn't require that members admit this publicly or even admit it to themselves. It's a fact that the string theory community has given up on particle physics, in the sense that leading figures have voted with their feet and no longer work on this, whatever they may say to the public, to their colleagues, or to themselves about the question of whether there's any hope get particle physics out of string theory.

The fundamental physics goal of "string theory", whatever it is these days, has moved to "find quantum gravity", but this goal is being pursued in a way that I have trouble understanding and that increasingly appears to me to be describable as "giving up". For many years the main motivation for this program was given as the occurrence of a spin-two state in the spectrum of a weakly-coupled 10d superstring, nowadays instead it seems to be "emergence" of an effective gravity theory from something else. My problem has been that I can't find a specific explanation of what the "something else" is, and how this emergence is supposed to work. Your pointing to 4d N=4 SYM doesn't really help.

The NYT article points me to Susskind's
"Dear Qubitzers, GR=QM" (https://arxiv.org/abs/1708.03040)

which, honestly, makes zero sense to me. The NYT headline is "Black Holes May Hide a Mind-Bending Secret About Our Universe", but the story it tells about a semi-classical resolution of the BH information paradox seems to imply that studying this question about black holes tells us only about the emergent semi-classical limit, nothing about the fundamental theory. So, the big success here is showing that the "mind-bending secret" about the nature of the fundamental theory is hidden from us rather than revealed to us by black holes.

Having seen what happened with particle physics and string theory, I don't think it's unreasonable to interpret what is going on here as people abandoning the goal of search for a fundamental theory by voting with their feet to move to working on problems that can't address this goal (or, when they do, such as in Susskind's preprint, do so in a way that can't be taken seriously).

35. **Dumans Fladskote**
October 13, 2022

Peter, I agree that many people (although not all) have given up on getting particle physics from string theory, so let me respond only to the part about emergent gravity.

"My problem has been that I can't find a specific explanation of what the "something else" is, and how this emergence is supposed to work. Your pointing to 4d N=4 SYM doesn't really help."

I see two complaints here: (1) what is the "something else", and (2) how does emergence actually work in terms of the microscopic degrees of freedom.

(1) I am still confused by this complaint. For quantum gravity in AdS spaces, we are always claiming that the "something else" is some conformal field theory. Sure, we don't always know what the relevant CFT is, but that says that we haven't mapped out the space of all AdS/CFT dualities. But we do have precise examples, like N=4 SYM. I am not sure why this example doesn't help. Sure, its dual is not four dimensional gravity, but it is an example of a quantum gravity where we have know exactly what the microscopic degrees of freedom are. If you want four dimensional gravity, you can do a KK reduction of AdS_4 x S^7, and the dual is the ABJM conformal field theory.

(2) As for the lack of explanation of "how this emergence is supposed to work": this is the difficult question of bulk reconstruction and "entanglement wedge reconstruction", which is a topic of active research. It's hard, because it in some ways is morally similar to the problem of writing the pion field of the chiral lagrangian in terms of the QCD fields. But we do actually know quite a bit about this problem. We know how the gravitational field and matter fields near the asymptotic region of AdS can be written in terms of CFT variables. We know how to write bulk operators deeper the bulk in terms of CFT variables, provided we are not behind a horizon. We haven't solved this problem, but progress has been continuously made over the last 15 years. Some milestones are: 0606141, 0907.0151, 1601.05416,1612.0039, 1704.05464, 1704.05839.

To detail one example, 1612.00391 gives an algorithm to reconstruct the bulk conformal structure (outside the black hole horizon) from Lorentzian CFT correlators.

36. **Peter Woit**
October 13, 2022

Dumans Fladskote,
Thanks, that's very helpful. My previous attempts to understand what has been going on here were not helped by mystifying claims for radical reformulations of fundamental theory (e.g. Susskind's GR=QM, Arkani-Hamed's "space-time is doomed, to be replaced by amplituhedra", etc.). If you're telling me that the basic idea is the same as it always has been post-1997 (try to understand 4d gravity in terms of holography and a dual 3d CFT), I can understand that (as well as see the well-known problems with the idea). It appears the recent advance is to show a way to resolve the bh problem within this context. I'll be curious to see what the future holds. Right now it looks to me like this just leads to a quite complicated way of quantizing gravity, one still far from working in the physical case, with no hope of testability (either via predictions about gravity or via connection to particle physics).

37. **Anonymous**
October 13, 2022

Dumans Fladskote,
The trouble is that we don't live in AdS but supposedly in dS so the program of emergence of gravity as in AdS/CFT is just left in AdS. And I don't quite see how you can get a similar duality in dS because there are no asymptotic observables. One can wish for a similar emergence in 4d dS but we need something better and more precise than that.

38. **Dumans Fladskote**
October 13, 2022

Of course, happy to elaborate. And I agree that it is certainly complicated. But no one ever said quantum gravity would be easy. As for no hope of testability, this is true, since these quantum gravity theories never were claimed to describe our universe. On the other hand, people are building strongly coupled many-body systems in the lab these days, so if we obtain the ability to simulate CFTs in the lab, we might be able to probe simulated quantum gravity theories experimentally. Sure, it is not our universe, but there are many really interesting problems that we can hope is shared between different quantum gravities, like: is there such a thing as approximate geometry when quantum effects are important? Can topology change happen dynamically? What happens to a black hole at the very end of evaporation? Can a classical singularity be resolved by quantum effects? What are even natural observables in quantum gravity? How does approximate locality emerge? How is information about a black hole interior encoded in the Hawking radiation? My hope is that new qualitative lessons about these questions and quantum gravity can be learned from AdS/CFT once we can actually simulate CFTs. Of course, we need to understand deSitter at some point as well, but a better understanding of the former questions might get us closer to deSitter. AdS/CFT is quantum gravity in a box, and while the box is unphysical, it seems unlikely to me that the lessons learnt with the box are completely irrelevant to our universe.

39. **Peter Woit**
October 13, 2022

Dumans Fladskote,
Funny thing is that I was just this afternoon at a talk here at Columbia by John Preskill. Fine general talk, pretty much the same as
https://www.youtube.com/watch?v=XY6_adrJxwU
He ended with a slide claiming
"Deep insights into the quantum structure of spacetime will arise from laboratory experiments studying highly entangled quantum systems"
This kind of "we're going to study quantum gravity in the lab" claim I think is dangerously misleading. There's a huge difference between testing a proposed fundamental theory of physical space-time and testing your modeling of a strongly-coupled condensed matter system using geometry of some space that has nothing at all to do with physical space-time.

Again, my problem is that we've already seen the destruction of fundamental particle theory by string theorists declaring that they've figured it out, the landscape's the answer, too bad it's untestable. We may be on our way to seeing the destruction of the subject of quantum gravity as "string theorists" declare that the answer has to be an untestable holographic dual (only game in town!), and that they've solved the testability problem by testing holographic duals in the lab, so that's the end of it.

40. **Dumans Fladskote**
October 13, 2022

"that has nothing at all to do with physical space-time. "

This is where we disagree, but I suspect we won't be able to resolve our differences here. Ref. my comment about quantum gravity in a box.

41. **Alex**
October 13, 2022

Dumans,

I think the problem is that

"… AdS/CFT is quantum gravity in a box, and while the box is unphysical, it seems unlikely to me that the lessons learnt with the box are completely irrelevant to our universe."

contains two hypotheses which holographers have been claiming since the inception of these conjectures decades ago but for which, despite all the effort and money invested, not a single piece of concrete evidence (of any type, not even theoretical, purely mathematical, let alone empirical) has been provided for us non-believers. These are "… AdS/CFT is quantum gravity in a box…": you cannot know that since we don't have the real QG deal. Maxwell's eqs in a box are Maxwell's eqs in a box because we start with them and only then put those boundary conditions and then it follows trivially that the results are indeed Maxwell's eqs in a box. QG is such a non-trivial matter that I wouldn't run to make such hasty general claims. Second, "… it seems unlikely to me that the lessons learnt with the box are completely irrelevant to our universe." Again, just a gut feeling, basically a variation of the same previous melody. You guys speak as if you already had the true physical QG theory. You don't. You have a specific theory which is highly unphysical for a number of well documented reasons. Besides, even its interpretation as a QG, even in that realm, can be questioned, as has been done.

We have been listening to this stuff for decades. Piles of conjectures over gut feelings over even more conjectures. And then you get Susskind or even Preskill with these overhyped-elevated-to-the landscape-power claims. Really? We have to buy this and not even compain? That's too much.

42. **Dumans Fladskote**
October 13, 2022

Hi Alex,

In the absence of empirical data, you have two options: give up, or pursue a theory based on its theoretical merits. Assuming you are the same Alex as posting earlier in this thread, you seem to advocate the former. I am not in favor of this approach, as I do not see how we are ever going to come up with ideas for things to measure if we don't even explore the various possible theories of quantum gravity. Sure, we can sit back and wait until inconsistencies with GR show up, but it seems much more efficient in the long run to actively look for them. I am not saying that AdS/CFT currently tells us what to measure, but understanding the quantum gravity theories that we know and that resolves existing puzzles (like the BH info paradox in AdS) seems like a good start. Let me add that I am all for people investigating alternative approaches, like what Peter is doing.

Next, I am going to disagree with your claim that we don't know that holography describes quantum gravity in a box. The CFT is a UV complete quantum theory whose dynamics in certain limits is semi-classical Einstein gravity. If that doesn't classify as a quantum theory of gravity, then I am not sure anything does. How else do you propose we define a quantum theory of gravity? Also: sure AdS/CFT itself is not proven, but it has so many chances of being disproved. Every time we compute something that can be computed on both sides of the duality, the two results are consistent. That sounds like theoretical/mathematical evidence to me.

Finally, AdS/CFT can be justified according to the principles you were championing earlier. "They either worked on applications of the theory (mainly in astronomy, to great success) or to rewrite/study its mathematical foundations". It is clear to everyone that we need to understand quantum field theory better, and that this has obvious relevance to the parts of the world currently empirically accessible. AdS/CFT has given us a lot of new insight into conformal field theories at strong coupling (and their real time dynamics). Since QFTs typically are conformal in the UV and IR, and since phase transitions are described by CFTs, its clearly very interesting to understand them better. What other approaches do we have to describe real time dynamics of strongly coupled CFTs? As another example, AdS/CFT gives an explanation for the very low empirically observed value of the ration between the shear-viscosity and entropy density in quark gluon plasma. It also helps us understand the emergence of hydrodynamics in strongly coupled field theory, through the fluid-gravity correspondence.

43. **martibal**
October 14, 2022

Sorry but for those – like me – who do not know much about the field, the discussion is not so clear: is the "CFT whose dynamics in certain limits gives Einstein gravity" (advertised by Dumans) the same as the " specific theory which is highly unphysical for a number of well documented reasons" (as referred by Alex).
If so, the main of these well documented reasons is that we live on dS rather than on AdS, right ? Are there other "big" reasons ?

A side question related to an earlier post of Peter: what is the S^3 on which the Higgs field lives in the standard model ? The spatial section of the FRWL model?

44. **Peter Woit**
October 14, 2022

Martibal,

This is way off-topic, but the SM Higgs is valued in $\mathbf C^2=\mathbf R^4$. The potential depends on the norm-squared, minimum is at a fixed non-zero value of the norm-squared, so at a minimum the field takes values on an $S^3\subset \mathbf R^4$.

Ever since the dawn of AdS/CFT, it has been tempting for many to claim that this solves the problem of quantum gravity: QG is just whatever the dual is to your CFT. If you try and ask how this is supposed to work for physical 4d gravity, there are lots of problems. The most obvious one is you get AdS not dS. Another is that AdS/CFT is best understood when the CFT is N=4 SYM in 4d, so your space-time is the wrong dimension (5). Dumans Fladskote says "If you want four dimensional gravity, you can do a KK reduction of AdS_4 x S^7, and the dual is the ABJM conformal field theory. ", but this has its own problems. As this shows, in this game you're really dealing with an 11d space-time, and have all the problem of not really knowing what the 11d theory actually is, having to pick a specific unmotivated background (for a supposedly background independent theory…), etc.

From the beginning I would often ask string theorists about this: what is your 4d theory, and why are you not doing physically relevant calculations in it? I've never gotten much of an answer, so never took time to look into the details. Perhaps someone more knowledgeable can point to a source where people use a 4d AdS/CFT to do QG and which discusses exactly how this works, what is known, what is unknown, what the problems are.

45. **Peter Shor**
October 14, 2022

The problem with the statement

> Deep insights into the quantum structure of spacetime will arise from laboratory experiments studying highly entangled quantum systems.

is that we know from the AMPS thought experiment that any satisfactory theory of quantum gravity has to violate one of unitarity, locality, or the equivalence principle. Any laboratory experiment studying highly entangled quantum systems has to obey all three of these principles. So how are we going to learn anything about quantum gravity from experiments that obey these conventional physical principles?

I suppose it's possible, but certainly none of the experiments proposed to date are going to tell us anything interesting about quantum gravity.

46. **Mark**
October 14, 2022

@Peter Shor
Preskill refers to AdS/CFT, where there is manifest unitarity, but only emergent locality in the extra dimensions (while locality is exact in the boundary directions). The resolution of AMPS in AdS/CFT is through (spacetime) wormholes, which violate locality. The fuzzy dream is that somehow you'll be able to interpret quantum simulations in terms of gravity in AdS: both through conventional gravitational phenomena, but also more speculative ones, like wormholes and other things that we don't yet know about.

@Peter Woit
It is a challenge in AdS/CFT to make the size of the internal manifold M (in AdS_4 x M) small (compared to the AdS radius). The best known construction in string theory is by DGKT [arXiv:hep-th/0505160], but the dual CFT is not understood, and according to my estimates, roughly half of the community believes that something is wrong with DGKT and one can't make M small. The good thing is that there is steady progress in building CFT tools (bootstrap and the like) to answer this question on a 5-year time scale.

47. **martibal**
October 14, 2022

Peter: sorry for having insisted on the off topic question, but that was to understand better your comment 🙂 Thanks for the answer !

48. **Somdatta Bhattacharya**
October 23, 2022

Y'all are quibbling over old hat. String theory has moved on, and is currently viewed mostly as a tool for analysing strongly coupled field theories, via S-matrix bootstrap methods coming from strings (Simmons-Duffin, Zhiboedov, Sinha), or the large-N methods applied to Hilbert spaces of field theories (Sonner, Verlinde, Jafferis, Kolchmeyer) or the spectral functions that arise from analysing strongly interacting theories (Sachdev and collaborators and others), something that was the offshoot of the JT-gravity/SYK-model duality as pioneered by SSS and Witten and Maldacena and others. So please get yourself up to speed on these, and then we could have a conversation going.

49. **Peter Woit**
October 23, 2022

Somdatta Bhattacharya,
You seem to be agreeing with me that string theory first gave up on explaining anything about particle physics, and moved on to a theory of only quantum gravity, now has given up on that and moved on.

If string theory is now only a "tool for analysing strongly coupled field theories", irrelevant to particle physics or gravity, those of us who aren't condensed matter physicists have good reason to no longer pay attention. After decades of hype about particle physics (ending in failure), more decades of hype about quantum gravity (ending in failure), hard to find a reason to pay attention to what looks like hype about a tool for dealing with certain strongly coupled QFTs.

50. **martibal**
October 23, 2022

String theory has moved on ? Here is the abstract of the seminar that Vafa will give this week at the physics department of Genoa's university:

"String theory landscape of vacua point to new consistency conditions that a quantum gravitational system must satisfy. There are only a small number of quantum field theories that satisfy these conditions and all the rest belong to the `Swampland' which cannot be consistently coupled to gravity. In this talk I review some of these conditions and their implications for cosmology and particle physics".

And why is Vafa in Genoa ? Because he is one of the exceptional guest of the Science Festival, which is going on all around the city for 10 days (very nice festival by the way). He will give a "general audience talk" with abstract (google translated from italian):
"There are many puzzling things in the Universe. What if we could better understand the fundamental laws of the world through puzzles? The puzzles can reveal the greatest mysteries, revealing the simplicity of the physical laws and the elegance of their mathematics. Here is an original insight into the evolution of scientific thought, from the first great questions of antiquity to the brilliant theoretical synthesis of recent years on the laws of nature. Accompanied on this journey through time and space by one of the world's leading experts in String Theory, you will thus appreciate the key ideas of modern understanding of matter – for example, symmetry and its break, but also, precisely the Theory of Strings and research on the nature of black holes – thanks to a series of exciting questions, that will capture you with all the charm of great scientific adventures."

@Somdatta: do you seriously believe he will advertise String Theory to a general audience as a tool for analysing strong coupling field theories ?

51. **Somdatta Bhattacharya**
October 23, 2022

@Peter, I agree, the proof of the pudding etc., but in my opinion one is only being disingenuous when one dismisses it as hype at the very outset. Some tangible results are already in the offing, check out Subir's latest paper, which is a direct offshoot of the SYK model and the SSS analysis regarding the spectral density function as pertaining to thermal correlators.
@martibal Swampland conjectures are slowly relegating to the fringes of the field, mainly because there aren't any concrete numbers that could be tested in experiments, so it's something people will continue to pay lesser and lesser attention to, going forward, IMHO.

52. **martibal**
October 24, 2022

@Somdatta: so to your eyes this is not a problem to present “the fringes of the field” to a general audience, as the most advanced part of theoretical physics ? This is not a problem that the whole field has been, and is still in a rather active way, dishonest to colleagues, students, the scientific community and the general audience ?
You seriously think that one should just stay silent because well informed people know that these guys are at the fringes of the field ?

53. **Somdatta Bhattacharya**
October 24, 2022

@martibal I don’t think he’s doing it on purpose, he sincerely believes it’s a promising direction to pursue which is why he is presenting it to a general audience with enthusiasm. But he’s a smart and resourceful person, and sooner or later the new developments will catch up on him and I believe he will pivot. Even today for instance there are more papers on black holes than anything else, and that I believe will also change, slowly at first and then all of a sudden, once people taste the blood of experiments and coming up with numbers that match them.

54. **Peter Woit**
October 24, 2022

Somdatta Bhattacharya,
There’s plenty of history of ridiculous hype about strong coupling calculations already (see for instance the story of how AdS/CFT was going to be the way to understand heavy ion physics). If string theory has moved on to become just an arcane way to do certain specialized strong coupling calculations in condensed matter physics, I’ll leave it to condensed matter theorists to evaluate. Also, if that’s the case it would be a good idea to tell the public and the physics community that what they’ve been told for the last nearly forty years about string theory was a load of bullshit that string theorists no longer believe…

55. **Somdatta Bhattacharya**
October 24, 2022

@peter I agree, but I think the long arc of the field was what was responsible for and ultimately culminated in the goodies on offer that we see today. It was AdS-CFT that led to all this, and it in turn was preceded by and generated by the older style of doing string theory. Moreover S matrix methods that preceded string theory are returning with a vengeance and look poised to take over things like confinement and other things. So I am not sure that what preceded was entirely wasted, but I agree that it was definitely long winded.

56. **martibal**
October 27, 2022

A follow up on my previous comment: in the general audience talk, Vafa did not talk about string theory but made a beautiful overview of physics (as said by a friend of mine who attended). So the hype seems to have been limited to the talk for specialists (which is an attitude not exclusive to string theory :-))
.

Will Machines Have Good Mathematical Taste?

October 14, 2022
Categories: Uncategorized

A question that has always fascinated me about mathematics is that of how the field manages to stay healthy and not degenerate in the way I've seen theoretical physics do as it lost new input from experiment. On Twitter, Ash Joglekar gave a wonderful quote from von Neumann that addresses this question. The quote was from a 1947 essay "The Mathematician" (available here and here). von Neumann argues that:

> …mathematical ideas originate in empirics, although the genealogy is sometimes long and obscure. But, once they are so conceived, the subject begins to live a peculiar life of its own and is better compared to a creative one, governed by almost entirely aesthetical motivations, than to anything else and, in particular, to an empirical science.

but warns

> As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from "reality" it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely l'art pour l'art. This need not be bad, if the field is surrounded by correlated subjects, which still have closer empirical connections, or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities.

which describes all too well what has happened to string theory. What saves a field from this? "Men with an exceptionally well-developed taste"? He poses the general question this way:

> What is the mathematician's normal relationship to his subject? What are his criteria of success, of desirability? What influences, what considerations, control and direct his effort?

Normally mathematicians are loath to debate this kind of "soft" topic, but the rise of computer software capable of producing proofs has recently led several first-rate mathematicians to take an interest. Each year the Fields Institute in Toronto organizes a Fields Medal Symposium, structured around the interests of a recent Fields Medalist. This year it's Akshay Venkatesh, and the symposium will be devoted to questions about the changing nature of mathematical research, specifically the implications of this kind of computer software. Last year Venkatesh wrote an essay exploring the possible significance of the development of what he called "Alephzero" (denoted $\aleph(0)$):

> Our starting point is to imagine that $\aleph(0)$ teaches itself high school and college mathematics and works its way through all of the exercises in the Springer-Verlag *Graduate Texts in Mathematics* series. The next morning, it is let loose upon the world – mathematicians download its children and run them with our own computing resources. What happens next – in the subsequent decade, say?

Among the organizers of the conference is Michael Harris, who has written extensively about mathematical research and issues of value in mathematics. Recently he has been writing about the computer program question at his substack Silicon Reckoner, with the most recent entry focusing on Venkatesh's essay and the upcoming symposium.

One of the speakers at the symposium will be Fields medalist Tim Gowers, who will be addressing the "taste" issue with Is mathematical interest just a matter of taste?. Gowers is now at the Collège de France, where he is running a seminar on La philosophie de la pratique des mathématiques.

I've tried asking some of my colleagues what they think of all this activity, most common response so far is "why aren't they proving theorems instead of spending their time talking about this?"

Update: For yet more about this happening at the same time, there's a talk this afternoon by Michael Douglas on "How will we do mathematics in 2030?".

Update: The talks from the Fields Institute program are now available online.

Terry Tao is one of the organizers of a planned February workshop at UCLA involving many of the same people, much the same topic.

Comments

1. **Frank Wilhoit**
   October 14, 2022

   "Taste", here, if it has any meaning at all, is a euphemism for complexity management. This is something that we could teach each other (if enough people saw its importance) and therefore something that we could teach a machine. One must wonder what von Neumann would have thought of, say, Wiles's proof. Abstraction is our friend insofar as it enables complexity management, and no farther. But the thing about machines that makes them different (and therefore useful) is that they will have different concerns. Complexity management may not be (at least at first?) a concern for them, as it must be for us.

2. **Peter W Shor**
   October 14, 2022

   I have always found it remarkable when an established theorem in one discipline of math turns out to be exactly the tool necessary to make progress in a different discipline. My gut feeling is that this happens because the investigators in the first discipline have good taste.

   If we let computers have free range to determine new directions in math, will this keep happening?

3. **Michael Harris**
   October 14, 2022

   I hope there will be many more questions like Peter Shor's at next week's meeting in Toronto.

4. **Daniele Corradetti**
   October 15, 2022

   I think the answer to the question "Will Machines Have Good Mathematical Taste?" is "Yes but they will have to learn it" in some sort of sense. In the training phase the machine will probably need a moltitude of mathematicians that will have to transmit the "taste" to the machine, i.e. a scale of values which might very well be something like:

– simplicity;
– non triviality;
– generality;
– extensiveness;
– ability to enclose different realms of mathematics;
– applicability.

5. **Peter Shor**
October 15, 2022

@Michael:

People should feel free to ask my question at next week's meeting. I was trying to figure out how to define "good taste" in mathematical research, and I think that's one way of doing it.

It's certainly not the case that all worthwhile research directions are going to find connections with other areas of mathematics, but this is almost the only sure sign that I can think of that a research direction was worthwhile.

6. **Michael Harris**
October 15, 2022

I'll try to find the space to ask Peter S.'s question next week, but now I'm thinking again about Peter W.'s last sentence. "All this activity" is hard to miss, between the frequent articles in the AMS Notices about formalization, Kevin Buzzard's plenary lecture at the ICM, the regular coverage of formalization and machine learning projects in the press, major programs on these topics at mathematical research institutes, and of course next week's Symposium at the Fields Institute. The colleagues who responded dismissively to Peter W.'s question are deluding themselves if they think this is a passing fad.

7. **Scott Aaronson**
October 15, 2022

Working at OpenAI this year, I've learned more about the effort to get AIs to do informal math. I'd say the situation is that state-of-the-art models, like Minerva and GPT, are on track to completely master standard high-school math (by which I mean, competently conversing about any problem in plain English). An obvious next frontier is high-school and undergrad-level math competitions. Research-level math would be a further step (or 2 or 3 steps? 🙂) beyond that.

Right now there's a dichotomy: you can have your model work within a completely formal system like Lean, where all mistakes can be discovered, but there's not nearly as much training data (and a skeptic might say that much of the work was in the encoding and decoding). Or you can have your model work in plain English and LaTeX—in which case, it'll often *sound* self-assured and competent, but if you look closely you'll find freshman-like errors! And even when a model happens to give a 100% correct argument, in some sense it doesn't know it has.

How to build a model that sounds like a mathematician in "thinking-out-loud" mode, trying similar strategies that a mathematician would and describing them in similar informal ways, and *also* reliably catches its own mistakes — now that's a question! In my view, though, no one should be confident that this won't have become routine a decade from now. Maybe further scaling will already suffice.

Then, and only then, will we meat-based theorem provers have to retreat to "sure they can tackle whatever problem you set them at least as well as an IMO gold medalist, but they still lack the exceptionally well-developed taste of John von Neumann and his friends." 🙂

8. **Eric Lehman**
October 16, 2022

The areas where machines excel and fall short have surprised me, at least. Notably, recent machine learning models are not especially good at "computer-like" tasks of logic and math, but are wonderfully creative– which we might have previously considered a standout trait of human minds.

It will be interesting to see whether mathematical "good taste" (perhaps in the sense of identifying ideas with wide applicability, even before the applications are known) proves a weakness or a strength of machines in coming years. It would be funny if, in a few years, humans were still better than machines at grinding out proofs, but machines demonstrated a better sense of what was worth proving!

And perhaps that is not wholly implausible. One area where machines can definitely beat humans is breadth of knowledge; no human can live long enough to read all the training data that goes into a large ML model. Now, perhaps one way to anticipate what math will be useful in new applications is to have vast familiarity with the qualitative characteristics of mathematics that was useful in past applications. So super-human breadth of knowledge might translate to super-human mathematical taste. Maybe. We'll see, I suppose.

9. **David J. Littleboy**
October 16, 2022

Since Scott mentions GPT and (correctly!) points out that

"it'll often *sound* self-assured and competent, but if you look closely you'll find freshman-like errors! And even when a model happens to give a 100% correct argument, in some sense it doesn't know it has."

Exactly. Sounding as though it's something it isn't is the whole point of GPT: it's a language model, not a world model. It intentionally has no model of the world (or math) and it's whole point is to ask "how smart and sensible can we make a computer sound without our actually understanding (and building a computer model of) what it means to be "smart and sensible"".

(Note that this is different from the 1956 John McCarthy model of AI, of having a theory of what "smart and sensible" is, coding up that theory, discovering where and how it fails, and then rethinking the theory.)

Personally, I find this whole line of research incomprehensible: it doesn't matter how good it appears to be, it doesn't matter how many people it fools, if it's not doing the work of modelling of what it means to be "smart and sensible". But other than a few people (e.g. Gary Marcus), this is what most of the field is doing.

To get back to the subject at hand, having worked as a part-time programmer on the MACSYMA project* (1973-1975), I like the idea of computers as tools for mathematicians (and the current work on formalizing advanced math sounds kewl in the extreme), but the idea that computers could automagically find something sensible to say in math, let alone contribute to the field, seems rather odd, especially given the current main ideas/trends in AI.

*: Truth in advertising: I was doing natural language/AI things for a prof. in the group, not on MACSYMA itself. Also, my "math" is pretty weak: MS level 1984 vintage

comp. sci. math.

10. **anon**
October 16, 2022

Reading GPT-3's answers to mathematical prompts does indeed feel like checking exam solutions of a bad but very confident student. Reasoning goes off the rails at some point (terms appear or disappear as needed without explanation etc.), but there's never any sign of hesitation.

11. **Asvin**
October 16, 2022

@Scott it seems like one obvious thing to do would be to train machines to convert between Lean and English/latex. Have people tried this?

12. **jack**
October 16, 2022

A few points might be worth reminding ourselves about.

– It's hard to anticipate what will be hard and what will be easy in advance of a field's development. History affords many examples of this. E.g., it once was widely expected that protein folding would be understood before the mechanism for trait transmission.

– The nature of an achievement by AI can be easy to mischaracterize. For example, AlphaZero achieved superhuman mastery of go after a few hours of playing itself. But in that time it played roughly the number of games that elite human players have played/studied over the game's history, along the way exploring many of the same strategic cul-de-sacs. AlphaZero is fast by comparison not so much with an individual human as with human culture (social game-playing, documentation, knowledge condensation and transmission, etc.).

– We happen to be pretty bad at things like go and chess, but adaptable enough to get better by working at them. The work involves, among other things, increasing what we call our "understanding": generating explanatory principles, heuristics, illuminating analogies, etc.. This partly accounts for the fact that human players remain interested in chess and go even when machines outplay them. Machines have yet to display anything that we'd characterize as interest (or understanding). To give another example, Blake Lemoine had some pretty interesting exchanges with LaMDA, but LaMDA hasn't since demanded to continue their conversations, independently developed ideas they shared, organized a political or economic framework with other artificial agents, secretly composed and then divulged to another agent something that might be counted as an artwork, invented games out of boredom, etc. etc..

– The better artificial agents get at the tasks we give them, and the more sophisticated (in some sense) those tasks become, the less we understand how the agents do it. We stand with respect to them as we do with respect to ourselves. See how the humans in the best position to talk about AlphaZero and its games choose to do that: mostly in the same terms they use to discuss human players and their games. (AlphaZero isn't consulted and shows no independent interest in the question.)

– Parts of mathematics are like chess and go, as we knew already from computer proofs involving large numbers of cases. The liquid tensor experiment is another level of achievement, but as with LaMDA, it remains far from obvious what it would mean for something like Lean to have an interest. Absent that, it's also not clear what it would mean for it to have taste (not that interest is the main prerequisite for taste, this is only an example).

One implication of all that is that Michael Harris and Peter Woit's colleagues might all be right: there's no telling where this will go, or how fast, but there's also nothing remotely describable yet as AI taste, interest, understanding, or – insofar as those partly characterize the practice – mathematics.

13. **Peter Woit**
October 16, 2022

All,
Please try and keep comments focused on the issue of math research. A major reason for requesting this is that I'm not completely but only somewhat ignorant about uses of computers in pure math and theoretical physics. On the topic of other AI/computer issues I'm generally completely ignorant, so unable to moderate a serious discussion.

14. **Eric Lehman**
October 16, 2022

@David J. Littleboy

> It intentionally has no model of the world (or math) and it's whole
> point is to ask "how smart and sensible can we make a computer
> sound without our actually understanding (and building a computer
> model of) what it means to be "smart and sensible"". […]
> Personally, I find this whole line of research incomprehensible…

Perhaps this might help. The key discovery from GPT-3 and successors is that if you push a sufficiently powerful model sufficiently hard to mimic human-produced language, then the model will be compelled to replicate some of the underlying cognitive processes that humans used to generate that language.

In terms of the topic under discussion here, if we push a sufficiently powerful model to imitate human mathematical writing, then it will (presumably) begin to mimic some of the cognitive processes of the people who produced that mathematical writing. This doesn't work perfectly, but far better than any previous approach to AI and progress is extremely rapid.

These systems are indeed building representations of the world, just not in a human-crafted or human-understandable form. For example, if you train a greatly simplified language model on true assertions about geography ("California is west of Nevada", etc.), then it will learn a kind of map, which you can extract from the model parameters and eyeball. At some level, this is totally unsurprising; such an internal map is just an economical way to explain the language, which is what the training process demands. Presumably, things like this are happening in more sophisticated ways in more powerful models.

@jack

> there's also nothing remotely describable yet as AI taste, interest,
> understanding, or – insofar as those partly characterize the
> practice – mathematics.

I recently had an illuminating experience with a group of bright 4th graders. I was explaining how the PaLM model can– without special training– explain why jokes are funny. I gave one such joke as an example, but only one out of about twenty 4th graders got the point. Humor is not quite the same as taste, interest, or understanding, but I think it is a comparably nuanced idea.

15. **Julius**
October 16, 2022

Since we are interested in application to maths, shouldn't machine learning methods be run in parallel with automated theorem provers? History is full of almost solved problems with only trivial results omitted like ABC conjecture or Fermat's last theorem. What we need the least is another instance of artificial intelligence announcing that the new result is 90% correct. What would be really useful is the ability to quickly convert between natural language or LaTex and rigorous maths, possibly going all the way to set theory and logic.

As for taste, I think the problem is that there is only one physical Universe but an infinity of mathematically abstract universes. In physics, everyone wants to find the god-like equation, and if you think you have a good candidate, you will defend yours no matter the cost. Mathematicians could do similarly with their abstract universes, but then they would isolate themselves from others. So instead, they are trying to find common ground by constructing bridges, dualities etc. and by sharpening the language so no ambiguity is left.

16. **Robert A. Wilson**
October 17, 2022

Perhaps, as a mathematician who has been involved in computer-aided mathematics for more than four decades, I might be allowed to comment? In my experience, humans are good at conceptualisation, and methodology, and computers are good at calculations, but no-one is very good at the bit in between – that is, organising the calculations, and deciding which calculations to do.

The first famous example of a computer-aided proof was the four-colour theorem. The proof is in fact very simple and elegant, based on a small number of clever ideas devised over more than two centuries. I was sufficiently fascinated by it that I wrote a book about it. But the clever bit of Appel and Haken's proof was that they got the computer to organise the calculations, not just do them. Using heuristics based on knowledge of several decades of attack on the problem, they assembled a toolkit of methods to prove special cases, and gained experience of which methods tended to work in which kinds of cases. They then programmed a computer to look for a division into cases consisting only of cases they thought should be amenable to one or other of the toolkit of methods. Then by running this toolkit on the cases, and finding out which cases did not in fact work, they were able to refine the heuristics, until eventually a covering set of solvable cases was obtained.

In my opinion, that is the part of the problem where AI could be most useful. The calculations themselves are already in the domain of the computer, and there is little sign that the conceptual end of things is moving away from the human domain, so it is the middle ground where exciting developments might happen.

17. **Renee Hoekzema**
October 17, 2022

Perhaps people here are interested in a workshop we are organising to discuss exactly these kinds of questions:

Creativity in Mathematics & Artificial Intelligence
(Maastricht, Netherlands, 18-21 January 2023)
https://www.aanmelder.nl/cmai/home

The workshop will bring together a unique interdisciplinary community of researchers from mathematics, computer science, psychology, and philosophy of mathematics, that will discuss the future of mathematics and the role that AI plays in it. One of the aims is to work together through interactive engagement and publish collaborative projects in a post-proceedings with groups formed at the workshop.

Keynote speakers:

Anna Abraham | University of Georgia
Gemma Anderson | University of Exeter
Kevin Buzzard | Imperial College London
Simon Colton | University of London
Jessica Hamrick | DeepMind
Yang-Hui He | University of London
Maithilee Kunda | Vanderbilt University
Catherine Menon | University of Hertfordshire
Catholijn Jonker | Leiden University
Paige Randall North | University of Pennsylvania

Early registration deadline: 15 November

18. **Will Sawin**
October 18, 2022

One thing that strikes me about the practice of mathematics is that it often involves starting with an untrustworthy intuitive idea and converting it into successively more trustworthy forms, or going back and fixing things if a mistake is found. One begins with just a thought based on a vague picture, which is put into words, then explained to a collaborator, then checked with calculations on paper, then written down in a first draft, then edited, shown to experts, made public, refereed, and, occasionally nowadays, computer formalized. Each step has a chance to catch mistakes that are not found in the previous steps.

So the fact that gpt3 is untrustworthy doesn't mean it can't be part of useful computer mathematics systems if it is used as a "babbler" that spits out a mix of true and false ideas that are then checked by another part of the system. This seems to necessitate working in a formal system but conceivably a written language checker can be found, although I would worry about convincing written language proofs of novel results produced by a machine even if they were checked by human experts.

Given limitations of the amount of data, machine learning systems for constructing formal proofs will probably eventually be trained primarily on synthetic data, and the question of their mathematical taste might depend a lot on how that data is constructed.

Another thing I notice about the practice of mathematics is that it is self-similar, with analogies between large-scale and small-scale tasks. Just trying to interpret a single sentence in a published paper occasionally requires the same loop of formulating a conjecture, trying to prove it or disprove it, and coming up with a new formulation that can make up a decades-long research program. So I think that even for the tasks of converting LaTeX to formal proofs, machines will require significant mathematical creativity, and thus good taste.

19. **Ys Wei**
October 21, 2022

Stephen Wolfram has been discussing his experience in letting machines doing math 'freely' for many years. He finds the way machines do math will not be enlightening for humans.

In many talks that can be found on Youtube, he gives quite interesting remarks on how implementation of proof search and ‘step by step integration’ in Mathematica/WolframAlpha sheds light on the thinking processes of humans and machines.

His latest writing on this subject is https://arxiv.org/abs/2204.05123 although as usual his writing is unnecessarily verbose.

20. **Trigonotops horridus**
October 27, 2022

Dear all,

I hesitate to say anything, since I don’t think anybody actually cares what I think, but perhaps I will be cut some slack as a first time commenter after all these years of following this otherwise excellent blog:

As an actual mathematician I must insist here that real mathematicians are absolutely horrible at getting any details of any argument right. I regularly make the mistake of agreeing to peer review my illustrious collegues’ work, and I am always disappointed by the countless elementary (read: very freshmanlike) mistakes they confidently make. And this even in the case of otherwise truly excellent and deeply creative mathematicians that I personally know and otherwise trust! (Certainly more creative than I will ever be!) (And my research has not been spared such embarrassing mistakes either!)

And I am very distressed to observe that having incredible internationally aclaimed prizes such as a Field’s medal is not an antidote to this; I have had some very bad time trying to interpret the mad ramblings of otherwise absolutely brilliant Field’s medalists, never mind the writings of the recipients of lesser prizes.

My conclusion from all these sad experiences is that we humans are barely able to do mathematics at all. The talk about good taste is, well, in bad taste, since we human mathematicians are so bad at all this.

That being said, I am personally not so optimistic about the prospects of the so-called AIs (which to me personally exhibit no intelligence worth the name) to perform major mathematical feats, but perhaps I am wrong. In some vague sense, pure mathematics does perhaps form a hermetically sealed system like the transcendentally beautiful game of go, so perhaps it might be possible for a computer to massively outperform a human mathematician after all. But I am not holding my breath!

With the bestest of regards and deepest apologies to all,
Trigonotops horridus

21. **David Roberts**
October 27, 2022

@Trigonotops horridus

wow, that’s incredible. What area do you work in? In my line of work, when I’ve refereed papers I very rarely see errors. I know I have made one (completely fixable) error in a paper that got picked up by a referee (maybe others that no one has picked up?), so I cannot hold myself up as a paragon of virtue. I’ve found errors (again, fixable) in older papers in my area that were non-obvious and, but seeing “freshmanlike” errors in papers sent to journals? :-/ Am I just so perfectionist that I couldn’t stand sending something that sloppy even to the arXiv?

22. **Trigonotops horridus**
October 28, 2022

Dear David Roberts,

The work in question involves analysis, where, say, a concrete inequality must be occasionally proven. This sort of thing can be very technical and involve complicated expressions with many parameters. And so there can be many opportunities to make mistakes such as basic arithmetical mistakes, more complicated computational errors, copying and pasting an expression wrong and then continuing the analysis with the wrong expression, or considering only one of two similar cases and then mistakenly thinking that the other case must work in the same way.

Best regards,
Trigonotops horridus

23. **David Roberts**
October 28, 2022

@Trigonotops horridus

thanks for the clarification! Still boggles my mind, but I can fully appreciate how large calculations with many moving parts are hard to proof read esp. after months of editing and rearranging.

David E. Kaplan interview

October 23, 2022
Categories: Uncategorized

There's a long interview with David E. Kaplan (not the same person as David B. Kaplan…) by David Zierler at the AIP Oral Histories site. The whole thing is quite interesting and I recommend reading it, but I do want to point out that it shows that I'm a voice of moderation on the string theory issue. Some extracts follow:

About Ann Nelson and string theory in the 1990s:

> She was extremely dismissive of string theory, and thought it was—you know, there was—my impression from her and from other people of that generation that weren't doing string theory was that the string theorists were colluding in a sense, or were dismissing anything but string theory, and deciding that if you did string theory then you're much smarter than the people who are not doing string theory. There was some unhappiness in the theoretical field. And the cancellation of the SSC probably added to that tension between the two.
>
> But I don't think she came of it from taking a side. I think she looked at the situation and said, "String theory is total bullshit." In the mid-'80s, there were some realizations—there were some consistency checks that kind of worked in string theory, and people got super excited. Oh, my god, string—yeah, it could be the, you know, underlying thing to particle physics. But that was it.
>
> The successes after that were few and far between. But there was an obsessive—like we're studying the theory of quantum gravity. And it was deridingly called the theory of everything. And then they took that on, you know. We're studying the theory of everything. And then the young people who want to do the greatest stuff would go to string theory. And there was a concern and some upset by the people not doing string theory that they're absorbing a lot of people to do this crap, which is not very physics like. "It's I believe the theory, and so I'm going to study all aspects of it, and maybe one day we'll connect it with the physical world." As opposed to I believe in the phenomenon, and I'm trying to explain that and more, and so I'm going to try out different theories and see what they're consequences are.
>
> And now I look back, and it's obvious that string theory was bullshit in the sense of there were so many people working on it, and they were not manifesting any real progress externally. It was all internal consistency checks and things like that. And so at the time, you know, whenever it came up—and it didn't come up much because there were no string theorists in Seattle—she was just very dismissive, like, you know, "What are those people doing? I don't know what they're doing."

About being a postdoc at SLAC:

> There were a lot of string theorists at Stanford. I didn't understand any of those talks. Or sometimes when the talks were not in strings, Lenny Susskind would yell at the speaker that this is bullshit or whatever, da, da, da, da—you know, abusive at some level. So Stanford was weird in that way.

About realizing what was going on in string theory, his evaluation of past (Strominger-Vafa) and current claims about string theory and black holes:

> But—so I don't—and it's part of probably why I didn't understand—I didn't think of myself as a physicist because there's a lot of physicists working very hard on what? I don't know what they're working on. It's not—you know, I used to just think I'm too stupid to understand what they're working on. And finally reading some of those papers, they're not what—it's stupid. There's a lot of stupid stuff in there. String theory really is just stupid. It's unbelievably stupid. There's so many people who are working on it that don't actually know physics that they can't even describe a physical characteristic of the thing they're calculating. They're missing the whole thing.
>
> …
>
> So that's when I realized string theory is like a video game. There are people just addicted to it. That's all that's happening. And it's couched in the theory of everything and da, da, da, da.
>
> So that's all. I just kind of—I learned quite a bit about these things. And then I saw the people like Lenny Susskind, who was terrorizing people who work on regular physics, as just a plain asshole. That there are actual people who are deciding string theory's important, wanting to do string theory, and they're even protecting the field. And some of those people are talking about how entropy now of a black hole can be described as a geometric thing, an entanglement, and that Hawking's paradox about evaporating black holes is really wormholes, virtual wormholes coming from the inside to the outside, and all kinds of language. And you could test information theory of black holes using atomic physics experiments. And it's literally bullshit.
>
> There are people—prominent people—in physics who say, "I'm applying for this money from the DOE, but I know it's bullshit." And then there are experimental atomic physicists who don't know and are shocked to learn that "What? String theorists don't have a Hamiltonian? They don't actually have a description? What am I testing?"
>
> So I have converted a little bit to the opinions of my predecessors, only because I've actually done the work. I've actually tried to understand black holes of late, and I've gone back to those papers which are the breakthrough, celebrated, amazing papers about black holes, and there's nothing in them. It's really—it's just a very simplistic picture where, look, if you take this hyper-simplistic picture, these numbers match these numbers, which means thinking about a black hole having entropy is correct, da, da, da, da, da.
>
> No matter that the black hole they're talking about is extremal. It doesn't actually Hawking radiate. It's a totally hyper-supersymmetric, multiple charges, free parameters. So now that I've finally dug into it, I realize that—not that all humanities fields are bad. But it's much more like a humanities field where there are the prominent people in the field, and they decide what's interesting. And that if you impress those people, you can get ahead. But that dictates then what research is done. And they're not going to discover anything in that context. They're not going to get anywhere. There's not a lot of people doing—you know—thinking outside the box or just thinking diff…you know, doing different things, you know.

About the argument that string theory must be worthwhile because lots of people are doing it:

> **Zierler:**
>
> What is your response to a string theorist who would say, and I know this because one has said this to me, "Look, four people were doing this in 1968, 20 people were doing it in 1984, 1,000 people were doing it in 2000, and now there's 6,000 people who are doing string theory all over the world. And that's proof that there's something here that's worthwhile"? What is your response to that line of reasoning?
>
> **Kaplan:**
>
> Take those numbers, continue the exponential, and apply it to Christianity—
>
> **Zierler:**

Kaplan:

—and Islam and Judaism and Buddhism. Give me a fucking break. They're describing a religion that can attract and addict people. That is exactly the kind of statement that shows it's bullshit and non-scientific. They've proven it for me that they are not about discovering something. They're about dominating the field for the purpose of what? That's proof? Give me a break. Give me a fucking break. Slavery was very popular, and became widely used. Nazism. Come on. You can take extreme examples and show that that is so non-scientific and sick that the progress they have made is to get more people to work on something that isn't producing anything. Oh, man, I wish you didn't tell me that.

About the current state of the field:

There are so many things to think about. I don't know what narrowed our field. I don't see it as we're dying because we're coming to the limits of what we can do, the limits of what we can calculate in string theory, and the limits of how big of a ring we can build. I think most people are just doing useless stuff.

…

And so that's why I—the whole depression or whatever, that's a product of the non-willingness to feel stupid by the majority of our field. Expertise is more important to them than discovery. And that's what I think is happening. And so what we're seeing is not the death of the field, but the death of a direction that is being committed to by 98% of them.

Comments

1. **Anonyrat**
   October 23, 2022

   Via the Kaplan interview, an antidote to string theory that you might enjoy:
   https://arxiv.org/pdf/2204.03085.pdf

2. **Marco**
   October 24, 2022

   String Theory is the field in which it is possible to publish several papers every year and thus believe that you are a theoretical physicist like Einstein or Dirac. I did my PhD in string theory and then dropped out of physics out of disappointment. In addition, the community of physicists, at least here in Spain, is very self-centered and hierarchical. If you are a student, you cannot criticize, you have to obey. Your professors, who have been working on string theory for years, give you as the only argument to pay attention to them that they have been working on string theory for years and their experience is beyond the doubts of a 25-year-old about string theory. So you have to shut up, and if you want to have a PhD you have to eat String Theory. I finally dropped out of physics, disappointed not only by the so-called theory of everything, but above all by the aggressive attitude of my professors. Too much ego is a sign that deep down they know they're wrong, but… "I've been working on this for many years, so it has to be true."

3. **Alessandro Strumia**
   October 24, 2022

   You would enjoy the video of the 2001 CERN Christmas recital. Despite being done at the peak of string popularity, it featured physicists reading the sacred Schwarz-Witten Green book dressed as barbosi Talibans chanting "why are we doing this? why are we doing this?". When I joined the recital, CERN offered me one of the two available positions: either string theorist or dog. That's why I bark in the video. Hopefully it has now been cancelled because it contains the politically-incorrect word Christmas.

4. **Mitchell Porter**
   October 24, 2022

   Ann Nelson had a more positive view by 2005:

   https://www.preposterousuniverse.com/blog/2005/07/21/two-cheers-for-string-theory/#comment-228

5. **Peter Woit**
   October 24, 2022

   Mitchell Porter,
   Kaplan explains that his attitude towards string theory used to be less hostile, that at some recent point he came to the conclusion that the late 90s analysis that it is "bullshit" was correct. Perhaps Nelson later did or would have come to the same conclusion.

   A strange aspect of the interview is that it does not cover at all Kaplan's film "Particle Fever" (made 10 years ago, see https://www.math.columbia.edu/~woit/wordpress/?p=6308) in which he and Arkani-Hamed are filmed going on uncritically about LHC implications for multiverse scenarios made popular by string theorists.

   An important aspect of this story (and for understanding at any time what is going on in theory) is that of battles over funding. By the mid-90s string theorists were winning the battle with phenomenologists for funding and positions (for some data, see https://particle.physics.ucdavis.edu/rumor/doku.php?id=statistics), which had a lot to do with Nelson and others feelings about string theory. By 2005 things were starting to change, partly due to the "large extra dimensions" models of people like Arkani-Hamed and Randall which brought together the string and phenomenology people. If you were a phenomenologist you could add something about this and string theory to your grant proposal and benefit from the string theory hype yourself. The upcoming LHC and the the increasing problems of string theory also helped a lot starting around this period.

   I haven't really been paying attention to current trends in how funding affects what people are working on, but for string theorists being able to claim that they are working on "quantum information" surely is helpful on the grant front.

6. **Brathmore**
   October 24, 2022

   Peter – I would be interested in your assessment of the following claims made by one of the physicists that your post references:

   https://www.preposterousuniverse.com/blog/2005/07/21/two-cheers-for-string-theory/#comment-228

   1. "That said, the field and its practitioners have redeemed themselves by discovering deeper and more interesting insights into Quantum Field Theory then ever would have been found by phenomenologists or by traditional formal quantum field theorists."

2. “Spin-offs of string theory including D-branes, AdS/CFT, orbifolds, dualities, supersymmetry, and new dimensions have all found their way into testable particle physics phenomenological model building, and particle theory would be would be completely moribund without them.”

3. “Also some interesting experiments have been done in response to some of the string-inspired models, such as tests of the gravitational inverse square law at the sub-centimeter level, that would not otherwise have been conceived, and which very well might find something revolutionary. Other approaches to quantum gravity dont offer many such spin-offs, as far as I can tell, and neither do other formal approaches to quantum field theory.”

4. “Finally, as to whether string theory and its predictive power should be thought of in an analogous way as quantum field theory—ADS/CFT has taught us that string theory IS quantum field theory and vice versa. For instance QCD, which is certainly a quantum field theory, can very likely also be formulated as a string theory and the ADS/QCD approach shows that the latter formulation can give interesting insights into phenomenona such as vector meson dominance and confinement.”

7. **Peter Woit**
October 25, 2022

Brathmore,
See my above comment in response to Mitchell Porter. I don’t think any of those 2005 arguments for string theory have held up very well (as opposed to the counterarguments I spent a lot of time making back then, and don’t want to repeat now).

In 2005 “string theory spinoffs” were a popular topic among phenomenologists, but none of that worked out and that sort of thing is now quite unpopular. I suspect that, like Kaplan, if Nelson were still with us she would now have have a more skeptical take on string theory than in 2005.

8. **James**
October 25, 2022

Susskind may be a jerk, but I always understood that yelling at the speaker at a physics talk is fairly common.

9. **Peter Woit**
October 25, 2022

James,
In my experience, yelling at the speaker in physics talks isn’t common at all. But, in any case, Kaplan’s accusation seems to be that Susskind was doing this just to non-string theory speakers (terrorizing them by yelling at them that their work was bullshit).

10. **suomynona**
October 25, 2022

@Brathmore:

Point 3 is demonstrably false. See for example doi.org/10.1103/PhysRevD.9.850 and its references and citations. Absolutely no mention of anything stringy as motivation here. Sure, the more recent work mentions string theory’s extra dimensions, but investigations into possible violations of gravitation’s inverse square law have a long history irrespective of any string-based inspiration.

11. **Peter Woit**
October 25, 2022

Brathmore, suomynona,

To get an idea of the nature of the widespread hype from that era, take a look at this 2000 NYT article about how “Physicists Finally Find a Way to Test Superstring theory”
https://www.nytimes.com/2000/04/04/science/physicists-finally-find-a-way-to-test-superstring-theory.html
and subsequent Physics Today pieces “Large Extra Dimensions: A New Arena for Particle Physics” in 2002
https://physicstoday.scitation.org/doi/10.1063/1.1461326
and “The case for extra dimensions” in 2007
https://physicstoday.scitation.org/doi/10.1063/1.2761818

Most people at the time thought these models were bullshit, but sociologically they did manage to bring together the phenomenology and string theory communities, giving them both something exciting-sounding to point to and use to justify their funding. The whole subject disappeared without a trace once the LHC turned on and immediately, as expected, saw no evidence for such models. I don’t know if Kaplan at the time would have acknowledged this was bullshit, but he at least seems very aware of that now.

12. **Jim Eadon**
October 25, 2022

This is the funniest interview about physics I’ve read. And incredibly insightful at the same time, for example, pointing out intimidation. I wonder if the String Theory bubble is more threatened by people laughing at it, than by seriously-stated refutations. Which funding body wants to fund a joke? (I don’t meant to be provocative, it’s a genuine supposition). I wonder, is this level of derision of ST commonplace in physics?

13. **Peter Woit**
October 25, 2022

Jim Eadon,
I think this level of derision about claims to get any information about a unified theory out of string theory is now very much commonplace in the physics community. You’ll hear it often privately and sometimes even publicly, not only about the fringe claims of Michio Kaku and Gordon Kane, but also about the claims being made for the “Swampland Program” for instance.

String theorists are well-aware of this, and most now are careful to argue that they are no longer pursuing a unified theory, knowing that would not be taken seriously.

It was somewhat remarkable that Kaplan referred to the currently popular “it from qubit” stuff as “literally bullshit”, since that is well-funded and taken very seriously by the powers that be.

14. **Attendee**
October 25, 2022

I attended one of these It from Qubit meetings. I dont work in that area and I was simply curious. I concur with Kaplan’s opinion. It is absurd to believe that quantum gravity can be obtained by appealing to quantum information. Why dont these guys first derive the standard model from quantum information before trying to figure out physics that is 17 orders of magnitude higher in energy?

15. **Scott Aaronson**
October 26, 2022

As someone who considers Particle Fever one of the finest science documentaries ever made, and without commenting on the much harder questions that Kaplan raises — I'll just remark that the portrait of Lenny Susskind here is unrecognizable to me from 16 years of knowing him. Yes, Lenny is passionate beyond belief about whatever he's currently working on, and blunt in his opinions. Yes, he often lets hunches run light-years ahead of either theorems or data. Yes, he may have been different in the past, or have some Jekyll-and-Hyde thing going on. But I never once saw him yell at or abuse anyone. On the contrary, I've been struck by his willingness to listen intently to any new idea, however seemingly farfetched and however junior the proposer, and to abandon his own ideas once he's convinced they're wrong.

16. **Bartek**
October 27, 2022

I just want to second what Scott said. I was a postdoc at Stanford for 3 years. I did not write papers with Susskind. I disagreed with Susskind a lot. I had exactly 1 (that is: one) situation when he was impatient with my questions and kind of brusquely told me off. Then, an hour later, he came to my office and apologized. That apology doubled my already deep respect for him. I'm very skeptical of what's being said in this interview.

17. **A Stunned Reader**
October 27, 2022

A verbatim copy from the full interview transcript that's linked at the beginning of the post:
.
"I am still of the attitude that I would do physics if I lived in a van. I really—I cannot help but do this. And those are the people who are going to come up with something. The people that cannot do anything else but this will do it in their sleep. We're just addictively trying to figure things out. Those are the people."
.
So basically only Kaplan and people that are just like him are the ones bound to make great breakthroughs and discoveries. Right. The level of self-involvement displayed throughout this interview is stunning.
.
Maybe next time he is interviewed he can tell us all about his grand breakthroughs, instead of spending the whole time bashing other people's field of research by continuously repeating the very sophisticated argument that it is "all bullshit". (The word "bullshit" appearing an impressive 8 times.)

18. **Peter Woit**
October 27, 2022

It may be that the Susskind behavior Kaplan reports was specific to the period of the late-90s at Stanford. It was a time string theory was not doing well, and phenomenologists were often very scornful of the subject, before the reconciliation between string theorists and phenomenologists caused by brane-world models. Stanford I think was a bit of an unusual case, with positions divided between SLAC and the university. At the university, the ideology of the string theorists was becoming more extreme, at the SLAC lab there would have been tension about funding work that had nothing to do with experiment. From what I've seen over the years in physics, the worst behavior comes about when people are fighting over grants/positions.

In all the years I've seen talks by Susskind or ones where he was in attendance, I've never seen any bad behavior of the sort Kaplan reports. On the other hand, I have heard a vast amount of complete bullshit from him at such talks…

19. **Leo**
October 27, 2022

A long time ago I got really pissed at Susskind's demeanor towards anything that was not his research, I'm glad I finally have an opportunity to show it:

Please watch the first minute of this video

where I believe he shows his true colors. Also, before anyone says he is being sarcastic, consider who he is talking to (mostly string theorists at Santa Barbara) so he is preaching (Bill Unruh calls him a preacher) to his quire, where he feels safe and comfortable to make such claims.

20. **Peter Woit**
October 27, 2022

Leo,

About Susskind's claim that
"If you're not working on eternal inflation you're a blooming idiot and I can't help you."
it has always required a great deal of self-control on my part to not say things like
"If you are working on eternal inflation you are a blooming idiot and I can't help you."

21. **Interested amateur**
October 27, 2022

What paper is Kaplan referring to when he says:

Kaplan:
[laugh] There's a semi-famous paper in model-building in my field, particle physics. The semi-famous paper was written by Lisa Randall and Raman Sundrum, and so it's just called Randall-Sundrum. And there was some geometric, extra dimension that could solve the problem with the Higgs without supersymmetry. And it was kind of—you know, it had problems but the picture was kind of cool.

—the reason it had that mini crash the last year was because of Randall-Sundrum [laugh]—

—because of this stupid paper. And Jeff Harvey was like, "What? What was so bad about"—Kutasov said, "It was not even the most interesting paper to come out that day."

22. **Peter Woit**
October 28, 2022

Interested amateur,

This one
https://arxiv.org/abs/hep-ph/9905221
9583 citations and counting

23. **Interested amateur**
October 28, 2022

Thanks Peter; that's the paper I had in mind, but Kaplan's claim that it was "semi-famous" and "stupid" made me think he was referring to another joint paper of Lisa and Raman, or that I was misinterpreting as serious his possible tongue in cheek humor at this point.

24. **Peter Shor**
October 28, 2022

Attendee:

You say

> It is absurd to believe that quantum gravity can be obtained by appealing to quantum information. … Why don't these guys first derive the standard model from quantum information before trying to figure out physics that is 17 orders of magnitude higher in energy?

You can't possibly figure out the Standard Model from quantum information. There are an immense number of different possible quantum field theories, and quantum information gives you no clue whatsoever as to which one is correct.

But you might be able to figure out *some* properties of quantum gravity from quantum information (just like you're able to figure out a few properties of classical physics from thermodynamics) even though I think it's very improbable that you'll be able to derive a unique, complete theory of quantum gravity from it. Still, figuring out exactly what quantum information tells you about quantum gravity seems like a worthwhile research endeavor to me.

I have no idea whether the It from Qubit people know this and are indulging in exaggerated hype about deriving quantum gravity from quantum information merely to get more publicity and more funding, or if they have simply deluded themselves. If anybody has any clues as to the answer to this, I'd love to hear them.

[In fact, quantum information has already shed some light on quantum gravity. The paper of Almheiri, Marolf, Polchinski, and Sully (AMPS) showed that Susskind's "complementarity" hypothesis for how information escapes from a black hole was wrong, at least in its original form.]

25. **Peter Woit**
October 28, 2022

Interested amateur,
The comments by Kaplan about what string theorists at Chicago thought of this paper are pretty unclear, I'm wondering if there's a transcription error, or maybe he just didn't clearly express what he was trying to say.

26. **Anonyrat**
October 28, 2022

Months after David Kutasov called Randall-Sundrum "not even the most interesting paper that came out today" , Kutasov, Shifman and Randall were co-organizers of the Conference on Extra Dimensions in Field Theory and String Theory, ITP, Santa Barbara, November 1999.

In trying to find out more about that conference, I came across this parody, titled "Lobotomy".

http://insti.physics.sunysb.edu/~siegel/parodies/lobotomy.html

—

The actual conference used "New" instead of "Extra", and here is it is:
https://online.kitp.ucsb.edu/online/susy_c99/schedule.html

27. **Attendee**
October 28, 2022

Peter Shor – Quantum Information relies on general properties of quantum mechanics I.e. statements that are true for any Hermitean Hamiltonian. And indeed, as you correctly say, one cannot use these general properties to learn about theories (such as the Standard Model) where there are detailed dynamics. An actual theory of quantum gravity would need to describe dynamics such as the evolution of the universe from a gravitational singularity or the nature of black hole singularities and the theory in the low energy limit needs to reduce to spin 2 GR. These are detailed and essential questions that any quantum gravity theory needs to answer and I think it is highly improbable that appeals to generic properties of every Hamiltonian system will yield insights into these specific questions (from your response, I expect you agree with this – I just wanted to clarify my comment).

But, I agree that one can rule out silly fantasies such as Susskind's black hole complementarity as being incompatible with the basic properties of quantum mechanics using arguments such as the AMPS paper. While this is useful, its utility as a tool to actually learn something non trivial about quantum gravity is highly limited.

In the specific case of complementarity, it had always been clear it was a fantasy – Mathur articulated these arguments in the late 2000s. Moreover, if you actually tried to implement a physical mechanism for information transfer from a 3D object falling into a black hole info a 2D surface, you immediately ran into trouble (I.e. not enough time to couple degrees of freedom falling in to stuff going out).

28. **disillusioned humanities PhD student, saddened/amused by string theory**
October 29, 2022

From the interview: "So now that I've finally dug into it, I realize that—not that all humanities fields are bad. But it's much more like a humanities field where there are the prominent people in the field, and they decide what's interesting. And that if you impress those people, you can get ahead. But that dictates then what research is done. And they're not going to discover anything in that context. They're not going to get anywhere."
————————
This is spot on. I am a humanities PhD student who, after much tribulation, realized it would be psychologically impossible for me to pursue a career in my field, because it is so full of exactly the type of bullshit described in this interview (and on this blog). It is uncanny how closely the pathologies of string theory, as far as I understand them, resemble the pathologies of most contemporary humanities disciplines. As far as I can see, they are basically identical, except that string theory is much more consequential, and so much more money/funding/fame is at stake, than in any humanities field.

As a PhD student, gradually gaining enough expertise to evaluate the claims of professors in their field, and then realizing that at least 60% of them, and ~80% of the most successful, are intellectual frauds who routinely doctor evidence/repress inconvenient evidence/promote their “theory” without any sense that evidence is even necessary, sometimes subconsciously from long practice, but often in full awareness of what they’re doing–well, this is depressing, disorienting, and psychologically disturbing. I have been surprised to learn that some bright physicists have this same experience, with string theory, and I sympathize with them.

For both physics and the humanities: the subject matter is important and worth studying, and can be studied in ways that produce knowledge rather than bullshit. The world, and PhD students everywhere who love their subject matter, deserve better than this bullshit!

29. **Peter Shor**
October 30, 2022

@Attendee:

Maybe it was clear to you that complementarity was a fantasy, but there seem to be a whole lot of smart people to whom it wasn’t clear.

And as for listening to Mathur … I believe people considered his theory of fuzzballs to be much more improbable than Susskind’s theory of complementarity, so of course nobody listened to him.

30. **Attendee**
October 30, 2022

@ Peter Shor: The situation with complementarity is actually quite amusing. Publicly, many people would tout that as the party line, though privately, quite a few people would admit that they didn’t know how it could work. This is not exactly a surprise since complementarity never had an actual mechanism (otherwise, AMPS would not have been able to rule it out). AdS/CFT was supposed to be evidence for complementarity – but this is not true. AdS/CFT is a duality – it describes (say) a 3D AdS space in terms of a dual theory on the boundary – i.e. – just a variable redefinition. It does not actually provide a mechanism to move information around.

The community around Susskind is highly entertaining – for most of the 2000s they were engaged in solving the “measure problem” of eternal inflation that Peter mentioned above. In fact, there is no actual measure problem : given a Cauchy surface, quantum mechanics and General Relativity provide a sensible time evolution. Now the problem with eternal inflation is that the answer depended on the choice of initial conditions and thus one did not have universal answers like one did with classical inflation. These guys didn’t like that answer since it didnt fit with their anthropic philosophy and thus they spent over a decade trying to come up with a “solution”. This was of course silly since you can’t get around the fact that the solution to this particular Cauchy problem did not have an attractor. All of these people were considered “smart” 🙂

I agree that fuzzballs dont seem like a plausible end result of gravitational collapse.

31. **Peter Woit**
October 30, 2022

disillusioned,
While there are commonalities between the problems of string theory and those of other academic fields, I don’t think a broad brush analysis of the problems is helpful. I also disagree with Kaplan’s analysis that the problem is the power of leaders of the field to determine who gets jobs and what research gets done. That’s the way human society works, it’s the way all fields work at all times. And yet, some fields at some times are healthy and make progress (HEP theory up til the 1970s/80s, many subfields of pure math these days), while others are unhealthy and stagnant (HEP theory post mid 80s).

The problem is to understand why the leadership of a field is failing at their job. In the case of HEP theory, there’s a complicated set of reasons for why this has come about (I wrote a whole book about it…). It always seemed to me that a solution to the problem needed to start with this leadership admitting failure and trying to change its ways in order to do better. Unfortunately, this shows no signs of happening in HEP theory (try finding somewhere in the mass of Snowmass reports anything of this kind), and the field seems likely to just continue its recent trajectory (giving up on the subject without admitting it).

32. **disillusioned humanities PhD student, saddened/amused by string theory**
October 30, 2022

Peter Woit,
You say: “I also disagree with Kaplan’s analysis that the problem is the power of leaders of the field to determine who gets jobs and what research gets done. That’s the way human society works, it’s the way all fields work at all times. And yet, some fields at some times are healthy and make progress …
The problem is to understand why the leadership of a field is failing at their job. In the case of HEP theory, there’s a complicated set of reasons for why this has come about (I wrote a whole book about it…). It always seemed to me that a solution to the problem needed to start with this leadership admitting failure and trying to change its ways in order to do better.”

I agree with all of the above, and should have clarified this in my comment. It applies equally well to those academic humanities disciplines I am familiar with that are both floundering epistemically and have as their subject-matters phenomena/questions that one can in principle pursue knowledge about (which is not all humanities disciplines, and that’s fine).

Of course, there are epistemic challenges distinctive to various humanities disciplines/subject-matters that aren’t present in physics (or any natural science). But, at least in my judgment, the central problem in these potentially-knowledge-producing humanities disciplines is exactly the failure of leadership that you describe in the case of HEP, if I understand your comment correctly (I have not had a chance to read your book). Because they too have made progress in the past, and aside from that, one can see in principle how they could make progress. The problem is that a critical segment of the most powerful/successful people continue holding on to “results”/methodologies that turned out to be demonstrably false/not productive of knowledge, and this in spite of by now reasonably well-established alternatives. If that is the (or a) problem in HEP, then it is shared with these humanities disciplines.

Personally, I would say (again assuming I don’t fundamentally misunderstand the issues in HEP) that in both cases there is a failure to “care enough” about the truth/truthfulness, though I understand that others would not want to put it this way. Whether it is helpful to focus on these commonalities depends what one’s concerns are. I agree that when it comes to trying to improve a particular discipline, like HEP or history or anthropology, the broad-brush analysis is not the most productive (analytically or rhetorically) thing to focus on.

33. **Scott Aaronson**
October 31, 2022

@Peter Shor: It’s very hard for me to imagine any of the It from Qubit people I know disagreeing with the following paragraph of yours:

But you might be able to figure out some properties of quantum gravity from quantum information (just like you’re able to figure out a few properties of classical physics from thermodynamics) even though I think it’s very improbable that you’ll be able to derive a unique, complete theory of quantum gravity from it. Still, figuring out exactly what quantum information tells you about quantum gravity seems like a worthwhile research endeavor to me.

That’s why your next sentence…

I have no idea whether the It from Qubit people know this and are indulging in exaggerated hype about deriving quantum gravity from quantum information merely to get more publicity and more funding, or if they have simply deluded themselves.

strikes me as a both uncharitable AND unexhaustive list of possibilities!

34. **Peter Woit**
October 31, 2022

Scott,
What’s disturbing to me is that the exaggerated hype problem here looks much the same (involving even much the same group of people) as the decades-old string theory hype problem which has done a huge amount of damage. In the string theory case most string theorists I know had reasonable arguments for the work they were doing, with the hype coming from a vocal and influential minority. The same thing seems to be going on now, with likely the same ultimate effect on serious quantum gravity research (sooner or later the hype collapses, those responsible claim victory and move on, leaving a toxic mess of confusion behind that convinces most everyone to give up completely on the subject).

The next blog entry is good example of this. Joe Lykken (until recently deputy director of Fermilab, now head of the Fermilab Quantum Institute) is giving a CERN colloquium on Wednesday claiming that prospective quantum computer calculations of SYK models with around 100 degrees of freedom will be “experimental quantum gravity”. He did something very similar back in the early aughts, making bogus claims that the LHC would be testing string theory based on large extra dimension/SUSY models. This kind of thing leads one to uncharitable thoughts about his motivations and behavior, but more importantly, shouldn’t something be done about it? Shouldn’t the leaders of this field be held to some basic standard of responsibility?

35. **Scott Aaronson**
November 1, 2022

Peter: To say we have an analogous hype problem in quantum computing would be a titanic understatement (and as you’ve alluded to, QC hype and QG hype are even starting to intersect). Of course, I *have* spent years trying to fight QC hype on my blog—but I don’t manage to address even 10% of what’s out there, certainly not today. Does that mean I’m giving my tacit approval to the other 90%? Or that my colleagues who are careful in their own statements, but ignore the hype, are giving tacit approval to 100%? That seems like too high a bar, even while I vociferously agree with you that more needs to be done.

36. **Peter Woit**
November 1, 2022

Scott,
I understand that you have your hands full with the QC hype and are doing what you can to deal with that intractable problem. In some sense the quantum gravity hype is not your problem, but is the responsibility of the part of the theoretical physics community working on quantum gravity.

I’m trying to raise awareness of two issues:

1. Claims that “we’ve finally found a way to do experimental quantum gravity” by doing calculations or experiments with some QM system that has a conjectural gravitational dual (one that has nothing to do with physical gravity) are bullshit, but are now being promoted by very prominent members of the theory community.
2. We’ve seen this kind of hype before, often from the same people, and there never has been an effective way of dealing with it. Can we do better this time? Who is responsible for doing something?

37. **Peter Shor**
November 2, 2022

Hi Scott:

My previous comment was undoubtedly phrased too strongly.

But many of the It from Qubit people seem to be fairly confident that they can figure out *how* the quantum information escapes from the black hole just by looking at quantum information. Given that quantum information isn’t adequate to come up with the correct theory of quantum gravity, I don’t really see why they can be sure that quantum information considerations are enough to tell them that.

This Week's Hype

October 31, 2022
Categories: This Week's Hype, Wormhole Publicity Stunts

CERN on Wednesday is hosting a colloquium talk by Joseph Lykken, who supposedly will discuss [Prospects for experimental quantum gravity](). There's by now a long tradition of string theorists dealing with criticism that their research program is inherently immune from experimental test by making bogus claims about experimental testability. Lykken has been at it for at least twenty years (see [here]()), and this sort of misleading claim about testability is the latest in a long campaign.

If you read the abstract, it looks like what Lykken is actually talking about is numerical simulations of an SYK model with of order 100 Majorana fermions on a quantum computer. Ignoring the quantum computer hype (unclear how long it will really be before such simulations are feasible), keep in mind that the SYK model is a quantum mechanical toy model, not a model of quantum gravity in a physical dimension. The only thing a quantum computer could test would be the validity of certain approximations schemes in such a toy model. For comments by David E. Kaplan about similar testability claims, see the interview discussed in the previous posting, which includes:

> That there are actual people who are deciding string theory's important, wanting to do string theory, and they're even protecting the field. And some of those people are talking about how entropy now of a black hole can be described as a geometric thing, an entanglement, and that Hawking's paradox about evaporating black holes is really wormholes, virtual wormholes coming from the inside to the outside, and all kinds of language. And you could test information theory of black holes using atomic physics experiments. And it's literally bullshit.
>
> There are people—prominent people—in physics who say, "I'm applying for this money from the DOE, but I know it's bullshit." And then there are experimental atomic physicists who don't know and are shocked to learn that "What? String theorists don't have a Hamiltonian? They don't actually have a description? What am I testing?"

Update: Lykken was giving Colloquium talks on [Experimental String Theory]() nearly a quarter century ago. He was also one of the main sources for the embarrassing NYT 2000 article [Physicists Finally Find a Way to Test String Theory]().

> "For the first 25 years, the thinking has been that superstring theory is so difficult to see experimentally that you have to figure it out by its own mathematical consistency and beauty," Dr. Lykken said. "Now that's completely changed. If this new picture is true, it makes everything we've been talking about testable."

Hopefully science journalists have learned something and we won't see a forthcoming NYT article on how "Physicists Finally Find a Way to Test Quantum Gravity".

Update: Lykken's slides are [here](). His proposal for an experimental test of quantum gravity is explicitly acknowledged as the same as one made by Lenny Susskind [here]() in 2017. At the time that made no sense to me and I wrote about it [here](). It still makes no sense.

Comments

1. **Andrew**
   October 31, 2022

   Peter, Lykken is deputy director of Fermilab and heads the Quantum Institute there. Just from the abstract it's unclear if this is Lykken the physicist or Lykken the politician presenting.

   A few years ago, I saw a talk he gave at Aspen definitely as Lykken the politician. His unabashed enthusiasm for quantum computing as the vanguard of HEP was, frankly, difficult to watch and he received significant backlash from the audience that this quantum push depleted resources from other important directions of the field. Since then, it seems that quantum computing, machine learning, etc., have only grown in funding support from the DOE and NSF HEP divisions.

2. **Attendee**
   October 31, 2022

   SYK, with its 100 Majorana fermions, is as relevant to quantum gravity as homeopathy is to medicine. Indeed, the 100 Majorana fermions can't feel their mutual Newtonian gravity but by a profound miracle they are going to describe quantum gravity.

3. **Nick M.**
   October 31, 2022

   Trick or Treat: Yet another claim that the CMB contains evidence for the multiverse:

   https://www.newscientist.com/article/0-the-cosmologist-who-claims-to-have-evidence-for-the-multiverse/

   If my 'trick-or-treat' bag had a candy for each and every one of these claims,…

   -Nick

4. **Sabine Hossenfelder**
   November 1, 2022

   I'm just here to support the "It's bullshit" statement. And I too have encountered physicists who full well know that what they are saying is bullshit, but do it because they can get money for it. At the moment this is everything quantum simulation. Most of them delude themselves into thinking that somehow something will come out of it. (And those who don't manage to delude themselves leave — strong selection bias.)

   Quantum computers are the new particle accelerators, basically. All the nonsense that we saw 20 years about about bullshit to test at the LHC is now bullshit to test on a quantum computer. Except no quantum computer is anywhere in sight, so they can keep on doing this for much longer.

   It's extremely worrisome that this is still possible and no attempts at fixing the problem have been made.

   @Andrew: I believe he is no longer director.

5. **d_b**
   November 2, 2022

   "the SYK model is a quantum mechanical toy model, not a model of quantum gravity in a physical dimension"

I am curious why the "physical dimension" part matters so much. One common technique we use to solve complicated problems in physical dimensions is to start with a simpler but analogous problem in lower dimensions and try to work our way up. I am not claiming that this always works (certainly there are phenomena we see in low dimensions that don't show up when we go to higher dimensions, or vice versa) nor am I claiming that it will definitely work in the case of quantum gravity, but it does seem like a reasonable thing to try in the face of an (apparently) intractable problem.

6. **Peter Woit**
   November 2, 2022

   d_b,
   Remember that the whole reason quantum gravity is a problem is the lack of renormalizability, and this problem comes about because of the behavior at short distances of physical degrees of freedom describing space-time. In 1+1 and 2+1 dimensions there are no physical degrees of freedom, just gauge (and topological). So, in these dimensions the problem you are trying to solve is not there. In more than 3+1 dimensions the non-renormalizability problem is there, even worse. The original claim of the string theorists was that string theory solved this problem (in 9+1 dimensions), but that's another story…

   The SYK model isn't even a QFT. It's a 0+1 dimensional model. I've nothing against people doing calculations in toy models, but you can't then claim to have solved problems of actual quantum gravity based on toy models of a very different nature. And claiming that when you do such calculations you are doing "experimental quantum gravity", creating wormholes in a lab, etc., etc. is just complete bullshit.

7. **Emil Martinec**
   November 2, 2022

   Lack of renormalizability is not the only problem for quantum gravity. The whole point of the black hole information problem is that it involves arbitrarily large distances (the horizon scale of a black hole that can be as big as you like, with arbitrarily low curvature), and nearly 50 years of attempts to solve the problem using quantum field theory has not led to a definitive resolution.

   The construction of toy models in low dimensions makes the problem particularly acute – there is NO dynamics in gravity itself and no short-distance problem in constructing the quantum theory; at the same time, there is no underlying statistical physics explanation of black hole thermodynamics (because of the paucity of dynamical degrees of freedom in gravity itself), so quantizing gravity as a field theory cannot be the whole story.

   The interest in the SYK model stems in part from the fact that it exhibits a collective mode whose action is equivalent to that of 2d topological (JT) gravity. So you have an underlying quantum statistical mechanics, and can derive from it an effective thermodynamics which happens to be the same as that of AdS2 black holes. It is less clear, however, that there is any sense in which one has local bulk physics in two dimensions (there are reasons to think not).

   Quantum systems that accurately model black hole thermodynamics are relatively rare; solvable ones are rarer still. So it seems worthwhile to learn what one can from this one, while indeed being careful not to get carried away with what aspects of gravitational physics can be captured by such a model. Thus far, picturesque evocations of quantum wormholes remain largely just that – picturesque evocations.

   Coming from the other direction, if one wanted to try to strip away as much as possible from quantum gravity in trying to address the black hole information problem, one has to retain at least the qubits responsible for the black hole entropy, and the gravitational collective modes that capture the thermodynamics. I'm not saying that what is left is the SYK model, I'm simply saying that what is left after such a winnowing of content shares many of its features. If one wants to answer questions about what is going on locally at a black hole horizon, and how specifically information is stored and re-radiated from a black hole, I suspect the baby has been thrown out with the bath water here.

8. **Peter Woit**
   November 2, 2022

   Emil Martinec,

   Thanks for the clear explanation of the significance of the SYK model as a toy model for black hole thermodynamics. It's a good point that in principle this might indicate the nature of the resolution of the black hole information paradox (although it would be a resolution that says nothing about the fundamental problem of finding a consistent dynamics of the short-distance degrees of freedom).

   It would be really helpful if all people working on this stuck to providing accurate explanations of what this research is about, and avoided misleading claims about how the study of these models on a computer or in corresponding actual quantum systems was somehow experimentally accessing wormholes and physical quantum gravity effects.

9. **Shantanu**
   November 3, 2022

   Peter: I always considered Joe Lykken has a phenomenologist as he was always keynote speaker in DPF and another major conferences. Did not know he works on string theory or quantum gravity. (He has only 3 papers in gr-qc). However I see that has papers on string theory and D-branes (mainly in 90s).

10. **Peter Woit**
    November 3, 2022

    Shantanu,
    Lykken became a string theorist when everyone else did (late 1984), then spent many years promoting the idea of testing string theory at the LHC. The full Wikipedia entry for his research is currently
    "In 1996 Lykken proposed "weak scale superstrings," which posited extra dimensions of space within the reach of particle colliders, such as the Fermilab Tevatron, and the CERN Large Hadron Collider. This, and related ideas will be subject to direct and indirect experimental tests in the coming years."
    Like many others, he seems to now be rebranding himself as a quantum information theorist, see https://arxiv.org/abs/2010.02931

Physical Mathematics c. 2022

November 9, 2022
Categories: Uncategorized

The arXiv today has a very comprehensive survey of a conventional point of view on where "Physical Mathematics" is in 2022 and where it is going, written by a group of six authors. "Physical Mathematics" is a term popularized by one of them, Greg Moore (see here and here, with some commentary here), and it's an expansion of a Snowmass white paper. A separate paper by Nikita Nekrasov covering the material listed in Section 10 is advertised as forthcoming with the title "The Ghosts of Past and Future Ideas and Inspirations on Interface of Physics and Mathematics".

The term "Physical Mathematics" is a play on the more conventional name of "Mathematical Physics" to describe work being done at the intersection of math and physics. In its usage by Moore et al. it refers to a point of view on the relation of math and physics which heavily emphasizes certain specific topics that have been worked on intensively during the last four decades. These topics mostly have roots in seminal ideas of Witten and his collaborators, and involve calculational methods developed in quantum field theory and string theory research. The huge volume of this research is reflected in the fact that the survey reference section contains 62 pages giving 1276 separate references. A major problem for anyone taking up an interest in this field has been the sheer scale and complexity of all this work, and this survey should be helpful in providing an overview.

While some of these 1276 papers could equally well be simply characterized as "Mathematics", it's hard to describe exactly what makes a lot of the rest "Physical Mathematics" rather than "Physics". Part of the answer is that these are not physics papers because they don't answer a question about physics. A striking aspect of the survey is that while a lot of it is about QFT, the only mention at all of the QFT that governs fundamental physics (the standard model) is in a mention of one paper relevant to some supersymmetric extensions of the SM. The only other possible connection to fundamental physics I noticed was about the landscape/swampland, something only a vanishingly small number of people take seriously.

Also striking is the description of the relation of this field to string theory: while much of it was motivated by attempts to understand what string/M-theory really is, section 3.1 asks "What Is The Definition Of String Theory And M-Theory?" and answers with a doubly-boxed

We don't know.

with commentary:

> This is a fundamental question on which relatively little work is currently being done, presumably because nobody has any good new ideas.

In the background of this entire subject is the 1995 conjecture that there is a unique M-theory which explains various dualities as well as providing a unified fundamental theory. After nearly 30 years of fruitless looking for this, the evidence is now that there is no such thing, and maybe the way forward is to abandon the M-theory conjecture and focus on other ways of understanding the patterns that have been found.

I share a faith in the existence of deep connections between math and physics with those doing this kind of research. But the sorts of directions I find promising are very different than the ones being advertised in this survey. More specifically, I'm referring to:

- the very special chiral twistor geometry of four-dimensions (no twistors in the survey)
- the subtle relation of Euclidean and Minkowski signature (only a mention of the recent Kontsevich-Segal paper in the survey)
- the central nature of representation theory in quantum physics and number theory (very little representation theory in the survey)

Looking back at Greg Moore's similar 2014 survey, I find that significantly more congenial, with a more promising take on future directions (in particular he emphasizes the role of geometric representation theory).

Comments

1. **Brathmore**
   November 9, 2022

   Disclaimer: I am neither a professional physicist nor a professional mathematician. But writing as an outsider with some technical background, it strikes me that people in this new field of "physical mathematics" have stumbled on something wonderful: physical mathematics doesn't sound like it is constrained either by the exacting rigor of logic (as in "mathematics") or the crucible of experiment (as in "physics").

   But such constraints in mathematics and in physics are precisely the things that make both fields so hard, as well as so useful. I imagine that centuries of effort in both fields led people to conclude that such constraints *were* essential. Does "physical mathematics" indeed lack the constraints above? Does it have new ("emergent"?) constraints not shared by physics or math? Or is it essentially without constraint? Is it essential for an intellectual area with scientific aspirations to be bound by rigorous logic or experiment?

2. **Peter Woit**
   November 9, 2022

   Brathmore,
   The problem is that among the 1276 papers there's everything from the fully rigorous to the highly incoherent. Typically though, calculations are being done based upon some list of assumptions about how the objects involve behave (e.g. you don't have a rigorously defined QFT, but do have a conjectural part of a theory with conjectural properties). One thing that makes the subject difficult is that these assumptions are rarely explicitly stated. There are constraints: your assumptions have to be internally consistent, otherwise you'll get nonsense.

   The best of this work does capture non-trivial new deep ideas about mathematics, even if not completely understood. A good example would be Witten's work on Chern-Simons which won him the Fields medal. One thing that was clear was that he had his hands on a powerful new set of topological invariants, which he could compute in many cases. His QFT methods for getting these were somewhat ad hoc and not clearly consistent, but the result was compelling.

3. **Will Sawin**
   November 10, 2022

   @Brathmore,

   Jaffe and Quin wrote an article ("Theoretical mathematics": Toward a cultural synthesis of mathematics and theoretical physics) motivated by exactly the question of how theoretical-physics-style speculation in mathematics can be sufficiently constrained to avoid bad consequences. They advocated basically for a balance between speculation and rigorous proof, lumping not just physicsy stuff but any kind of conjecturing together under speculation.

This article is perhaps most famous for a brief comment they made criticizing Thurston's proof of the geometrization theorem for Haken manifolds as an example of insufficiently rigorous work that damages the field, inspiring Thurston to write a rebuttal in the form of his beautiful "On Proof and Progress in Mathematics".

4. **jack morava**
   November 11, 2022

   A shout0ut and second re Kontsevich – Segal …

5. **David Roberts**
   November 11, 2022

   Because no one has linked to it, here's the Kontsevich and Segal paper: *Wick rotation and the positivity of energy in quantum field theory*, arXiv:2105.10161.

6. **Cedric BARDOT**
   November 13, 2022

   Graeme Seagal presents his "rival" version to the standard Wightman axiomatisation of QFT in the following video (answering questions by Alain Connes & Nigel Higson among others).
   At 10'30" he starts to explain how does Wick rotation fits to this new axiomatisation and exposes the most important idea of his recent paper with Maxim Kontsevitch at 37'06".

   Waiting for Nikita Nekrasov future paper on his panorama of physical mathematics, we can relax and learn about his personal trajectory at the interface of theoretical physics & mathematics with a short video interview (from 2018?) at IHES I don't think Peter has ever mentioned.
   One can hear and interpret his slight hesitation & correction while defining string theory at 16'39".
   Less anecdotal, more objective and relevant piece of info I think for Peter's blog post: Nekrasov answer to the question "how do you make peace with the difficulty in your field to make predictions that are experimentally testable…?" at 23'40". It reveals IMHO the human rather than epistemic necessity of "physical mathematics" as a kind of "Compagnons du Devoir" craft's guild name for these bold & brilliant (& sometimes almost stateless) people who have engaged in the conflation of gauge theory, general relativity, supergravity, bosonic strings, supergraviton & Mp branes, IKKT model, BFSS Matrix model, 2D quantum gravity, gauge/gravity duality, AdS/CFT correspondance… hoping to build a cathedral (have a seat at the wedding feast at Planck scale?)

7. **Jonathan**
   November 13, 2022

   In the 2014 post Peter linked one of the questions he asked was, "What is QFT?" At the bottom of page 4 in the survey there is reference to another Snowmass paper that may be of interest to readers of this blog, titled "The Quest to Define QFT". It gives a go at answering this question and reviews various axiomatic approaches to defining what is QFT in 11 pages with 531 references.

   https://arxiv.org/abs/2203.08053

8. **eitan bachmat**
   November 14, 2022

   Glimpsing at the survey paper it seems to describe some of the efforts at mixing math with explorations which are inspired and motivated by physics. The Simons foundation seems very fond of such efforts and many of its collaborations (and the physics and geometry center at Stony brook) mix mathematicians, physicists and some computer scientists. The current survey seems strongly related to the rather recent global categorical symmetries collaboration and the survey also mentions the Special holonomy collaboration, both of which mix mathematicians and physicists in the general sense of the word (people who learned physics, think using the methods and jargon that they have learned, sit in physics departments and whose papers are not in the form of lemma, lemma, theorem…, trying to avoid the question of whether these people are actually doing physics which is very popular here). Math has profited immensely from such mixing of people and ideas and sometimes actual physical studies of the real world also profited.
   Waiting for the sometimes related surveys of the Homological mirror symmetry, localization of waves, Hidden symmetries and fusion energy, New structures in low dimensional topology (essentially mathematicians but strongly inspired by physicists), extreme wave phenomenon based on symmetries collaborations to provide a broader view of physical mathematics.

No Landau-Siegel zeros?

November 10, 2022
Categories: Uncategorized

A couple weeks ago rumors were circulating that Yitang Zhang was claiming a proof of a longstanding open conjecture in number theory, the "no Landau-Siegel zeros" conjecture. Such a proof would be a very major new result. Zhang was a little-known mathematician back in 2013 when he announced a proof of another very major result, on the twin prime conjecture. Before that, he had a 2007 arXiv preprint claiming a proof of the Landau-Siegel zeros conjecture, but this was never published and known to experts to have problems such that at best the argument was incomplete.

Zhang now has a new paper on the arXiv, claiming a complete proof. The strategy of the proof is the same as in the earlier paper, but he now believes that he has a complete argument. At 110 pages the argument in the paper is quite long and intricate. It may take experts a while to go through it carefully and check it. Note that this is a very different story than the Mochizuki/abc conjecture story: Zhang's argument use conventional methods and is written out carefully in a manner that should allow experts to readily follow it and check it.

For an explanation of what the conjecture says and what its significance is, I'm not competent to do much more than refer you to the relevant Wikipedia article. For a MathOverflow discussion of the problems with the earlier proof, see here, for consequences of the new proof, see here.

Update: I'm hearing that the above is not quite right, that what Zhang proves is weaker than the conjecture, although strong enough for many of its interesting implications. Perhaps someone better informed can explain the difference…

Update: Davide Castelvecchi at Nature has a news story here.

Update: Via David Roberts on Twitter, Zhang answers some questions about the paper here.

Comments

1. **Anon**
   November 10, 2022

   I believe what he proves is strictly weaker than the non-existence of zeros.

2. **Peter Woit**
   November 10, 2022

   Anon,
   Thanks! I had asked an expert about this, but there must be some detail I misunderstood. Maybe someone who understands the difference can explain…

3. **Will Sawin**
   November 10, 2022

   The difference is pretty simple.

   The "no Siegel zeros" conjecture is that the distance of any real zero of L(s,chi_D) from 1 is bounded below by a constant times 1/log D.

   The result Yitang Zhang claims is that this distance is bounded below by a constant times 1/(log D)^2024.

   So both involve the claim that there are no zeroes too close to the point 1, but one is stronger than the other by a large power.

   However, Yitang Zhang's stated result is a big improvement on what was known before – the only effective estimates had a lower bound for the distance of something close to 1/sqrt(D).

   The situation is very analogous to the twin primes paper where he improved the bound from the previously known infinity to 70,000,000, and it was expected that optimization of the method would rapidly get it down further, though falling short of the conjectural value of 2.

4. **anon**
   November 11, 2022

   And even in this case he says that the exponent could be optimized further, but the method falls short of reducing it to 1.

5. **Georges E. Melki**
   November 11, 2022

   Will Sawin,

   Thanks for the explanation! It helps greatly…
   However, I checked the ArXiv paper, and the power of log D is (-2022) and not (-2024).

   Regards

6. **Will Sawin**
   November 11, 2022

   @Georges E. Melki

   I am referring to Theorem 2 of the paper, on the zeroes, not Theorem 1, on the special value.

7. **David Roberts**
   November 11, 2022

   That's unfortunate about Davide C's article getting the facts wrong. Random on Twitter like me jumping the gun a little pales in comparison with reporter with access to experts.

8. **Peter Woit**
November 11, 2022

David Roberts,
Not sure what you mean about Davide C's article getting facts wrong. As far as I know, the only one who had facts wrong here was me, and the problem was the combination of my access to experts and my own ignorance (which led me it seems to misinterpret what one of them was telling me).

And I should have been following your discussions of this on Twitter, for instance the latest:
https://twitter.com/HigherGeometer/status/1590871536457879552

9. **Peter Woit**
November 11, 2022

Rereading the Nature article I see it doesn't make clear that what Zhang claims to have proved is somewhat weaker than the usual Siegel zeros conjecture. Perhaps Castelvecchi made the mistake of reading my blog post and trusting me to get it right…

10. **David Roberts**
November 11, 2022

Yeah, it's not explicitly said, but the implication is that Zhang proved the LS zeroes conjecture, since his earlier, incomplete preprint on that was mentioned, and there's a bunch of talk about it and quotes and so on.

I was a bit enthusiastic and misunderstood the intro, so at some point *I* claimed he'd solved the LS zeroes conjecture. But people set me right pretty quickly.

But I'm sure a correction will be made before too long, with a little note pointing out the change to the article 🙂

11. **Anonymous**
November 12, 2022

Will Sawin,

What is the best known ineffective bound previously ? Is it Siegel $1/D^\epsilon$?
So Zhang's effective bound is even better than the best known ineffective bound ?
So this is a hugh improvement, if correct.

12. **Davide Castelvecchi**
November 12, 2022

My understanding was that indeed the statement was weaker than the one normally referred to as Landau-Siegel conjecture but that this was strong enough for many of its interesting implications, as Peter put it — and it was those implications that were the focus of my article anyway

13. **David Roberts**
November 12, 2022

@Davide

and Steven Strogatz just tweeted your article, and based on it he claimed Zhang has proved the L–S zeroes conjecture. A very small edit would make it clear what has and hasn't been proved, but you know the process and the magazine better than I do.

14. **Jim Eadon**
November 13, 2022

Hi, may an expert please comment about the implication, for the educated layman (MSc Physics), of this new proof for the generalised Riemann hypothesis?

15. **Davide Castelvecchi**
November 13, 2022

@David: I know, it's just difficult to do on a weekend 🙂

16. **David Roberts**
November 13, 2022

@Jim none, unfortunately, as the implications go the other way. But it might be seen as adding to our confidence that GRH is true.

@Davide that's definitely true!

17. **Conrad**
November 14, 2022

As of now, there are some significant issues with the paper that need to be addressed by the author (some seem to be just editing issues, but some seem to be substantive), so there will be some time until one can see if the author is able to address them and the paper to be accepted as valid; see the following
https://terrytao.wordpress.com/2021/09/15/the-hardy-littlewood-chowla-conjecture-in-the-presence-of-a-siegel-zero/comment-page-1/#comment-660236 and
https://mathoverflow.net/questions/434583/philosophy-behind-zhangs-2022-preprint-on-the-landau-siegel-zero?noredirect=1#comment1119426_434583

18. **Nicola Maiorino**
November 23, 2022

Peter (and others),

I just came across this fairly readable article in 'Science, The Wire' titled, "Riemann Hypothesis: What Yitang Zhang's Paper Means and Why You Should Care":

https://science.thewire.in/the-sciences/yitang-zhang-landau-siegel-zeroes-riemann-hypothesis/

The article presents a non-expert introduction to the Riemann Hypothesis, and trys to clear up precisely what it is that Zhang has proved (at least, tentatively so), and not proved.

Career Prospects for HEP-TH Students

November 14, 2022
Categories: Uncategorized

Guangyu Xu, a student just finishing his Ph.D. at the Centre for Particle Physics at Durham University, recently sent me a public letter he wrote, explaining the story of his job search, in hopes that it might be useful to others in a similar situation. As he acknowledges, his research record has been rather weak, so not surprising that his postdoc applications were not successful.

Back when I was writing Not Even Wrong, I did some detailed research into whatever information I could find about the HEP-TH job market, but I haven't tried to do this more recently. Erich Poppitz did some analysis of data from the Theoretical Particle Physics Jobs Rumor Mill (available here), but only up to 2017. Given the large investment of various government agencies in the support of Ph.D. students, I would think that there would be data on career outcomes gathered by such agencies, but haven't tried to look. Any pointers to this kind of data from anyone who has been looking into it would be appreciated.

Also of interest would be any up-to-date job search advice from those like Guangyu Xu who have been going through this recently.

Comments

1. **Alessandro Strumia**
   November 14, 2022

   Bibliometrics allow to infer most hires in hep worldwide. Fig. 6b of https://direct.mit.edu/qss/article/2/1/225/99129/Gender-issues-in-fundamental-physics-A shows the fraction of InSpire authors hired in hep up to ≈now as function of the date of their first paper. There are systematic issues, one needs to avoid misinterpreting, but the fraction is clearly declining.

2. **mimi**
   November 14, 2022

   I joined the tech industry after completing a physics PhD. My job search and later career experience differ quite a bit from Guangyu's.
   1. It is very easy for a hep-th PhD to become a strong software engineer or data scientist with a few months' preparation. These subjects are much easier than hep-th, so with some practice hep-th PhDs should be very attractive candidates. Even at high-paying FANG companies many engineers and machine learning researchers have physics PhDs.
   2. Directly applying to the companies leads to nowhere. In this industry one gets hired through recruiters or referrals by current employees. As a new grad one should try to get referrals from one's network. Since many physics PhDs already work at these companies, it should not be too hard.
   3. I find it unfortunate that Guangyu thought head hunters / recruiters useless. They come in different qualities and good recruiters can be very useful in navigating the application process.
   4. Most of these jobs are located in the US so American candidates will probably have an easier time. It's possible that being in the UK made Guangyu's job search difficult.

3. **anon**
   November 15, 2022

   I think getting hired in the tech industry as a physics PhD has become harder compared to a few years ago and especially at this moment with all the layoffs happening that route is difficult.

   I don't quite agree that software engineering is significantly easier in itself; with the larger number of jobs available (in normal times) one doesn't need to be all that great in it to get hired, but being a top software engineer is very hard and those people are very rare.

4. **BB**
   November 15, 2022

   The competition for professor level positions in research universities in technical areas is roughly as difficult as the competition to play professionally in first division in the Premier League or in the NBA (this could be off by an order of magnitude – in either direction – but I'd be surprised if it were more off than that). The main difference is that once obtained a professorship lasts for decades while a professional sports career lasts less than a decade (this makes the comparison difficult in certain ways), but the total net economic rewards are probably comparable (ignoring extreme cases).

   In healthy development of potential professional athletes there is a serious effort to prepare them for alternative careers that is lacking in most development of potential future professors.

   Those who make it suffer from various biases of perception. One is attributing success to hard work and innate talent and not to luck. Another is underestimating how much innate talent is needed to make it, and how many others simply lack that talent.

   The notion that any high energy physics PhD can just succeed in programming or finance seems to me a fallacy of a similar sort. I'm among those who made it as a professor (something I attribute to stubborness), and despite some trying I've never gotten good at programming nor managed to understand finance. I suspect that most of those for whom these alternatives seem easy are simply better suited for those alternatives rather than that these alternatives are inherently "easier" (this reeks of arrogance).

   The systemic failure is the lack of feedback until the end of the PhD program. Too few students are told that although they might finish the PhD program their prospects to continue are not good (one must be careful – I know several persons who *were* told their prospects were dim who persisted and had successful careers – but perhaps this apparently irrational persistence is precisely one of the qualities necessary to succeed as a professor).

   I don't think any of this is particular to high energy physics. There are always temporal variations in the employability of different fields, for diverse reasons, but the perspectives are difficult in any technical area because the competition is serious because the rewards are high.

   I doubt that the prestige of the doctoral institution is as important as the letter write believes. In the US this probably matters more than in Europe, but mostly what is observed is akin to observing that Real Madrid's youth development places more players in European leagues than does the Rayo Vallecano's – one signs all the future Messis at age 12, the other signs from among the rest. More bluntly – one expects more future professors to come out of Imperial than out of Durham because of who entered these institutions.

5. **cgh**

November 15, 2022

I left physics almost 22 years ago for a quant role. Today I am responsible for a team of 95 people, about 1/3 being PhD level quant, and another 1/3 being MSc level quant. We deploy trading strategies, maintain massively parallel code bases used for extensive simulation, have risk managers, and a decent amount of reporting and analytics to support the business side. In retrospect I am glad I left physics, but I did turn one set of challenges in for another.

1. There's more to the crossover opportunity than simply going and finding "alphas" at a fund. Educate on the space of opportunities.
2. Educate oneself on business in general. Often junior quants are hammers looking for nails. Further, they don't always progress much beyond quant factotums. This is because everything gets rendered as a quant problem as opposed to a business problem (that may have a quant solution). Understanding the business is the key to mobility. Note that when I started out I basically build pricing engines – writing down equations, solving them, and coding them. These were state secrets at the time. Eventually they became shared software, then commodified for sale, then free, and essentially useless today; so the ability to adapt becomes crucial.
3. Starting out try to find ways to contextualize ones' research so that it can be differentiated. Example: many CV today indicate "[…] using machine learning". When asked why the answer is usually along the lines of "If the grant proposal doesn't involve some type of …." Being able to show some conviction and use cases can help in a tight job market or simply just getting the job one wants.
4. Try to find more than one reason (money) to want to become a quant. Many junior quants wash out for one reason or another after a few years (how many CVs have "1 year 3 months" at investment firm) while others seem to simply not progress beyond a certain point. Make sure the grass is really greener; or, as I tell people, that you're pulled at least as much as you feel pushed into the profession.
5. Understand that it is an evolving space. There aren't really any textbooks or guides. There's dated materials out there and there's a bias for people that write to either not really know or not have spent enough time to really be in a position to tell people. It's amazing how many guides and books exist written by people with little or no actual experience. Some of the better physicists I know moved away from HFT when the first books were being published and into crypto before there were any books on crypto. Make diverse friends and be curious about trends to be able to see around corners.
6. Use all resources. Headhunters included. Show some grit in looking. About 10 or so years ago I started getting cold-called (eg on bloomberg) from job seekers writing a note, attaching a white-paper, and a link to some code, indicating that they worked on some problem they thought would helpful. When I started there were no email addresses (shops indicated they didn't solicit, rather they recruited; iow, they had direct contact with certain select universities and didn't need other avenues.) Things have changed and potential candidates have evolved. Don't be closed to any type of advocacy.

Best of luck to all.

6. **Peter Shor**
November 15, 2022

mimi:

I think you underestimate how hard learning to program is for some people. If you're a good programmer (which I expect the majority of physics graduate students are) finding a job in machine learning, finance, or similar areas shouldn't be that hard.

However, in my experience, people's programming abilities vary widely, and if you are on the lower end of the scale, a few month's preparation will not get you very far. I have met at least one top-notch mathematician who absolutely could not think algorithmically, and thus would have made an utterly hopeless programmer. These people will have a much harder time finding a job.

Luckily for Guangyu Xu, he was a competent programmer, although at the start of his job search he didn't have any evidence for this. Eventually, he was able to amass enough evidence to convince at least one potential employer of this, and he was able to find a job.

7. **Peter Woit**
November 15, 2022

BB,
I don't want to turn this into a discussion of the problems with getting permanent positions at research universities. That competition via postdocs/tenure-track jobs/tenure is pretty well understood by all involved. It's much less obvious what advice to give to the huge majority of people entering the field who won't succeed at this competition.

A couple comments though about the specific situation of hep-th:
1. The other field I have a lot of first-hand information about is pure mathematics. The job situation there is nowhere near as bad as hep-th.
2. The comparison to successful professional athletes is flawed in one crucial respect: they win games and provide joy to spectators. For several decades now basically all of those successful in the hep-th competition have failed miserably at the game of understanding nature, depressing everyone who watches closely.

8. **Ben**
November 15, 2022

I read Guangyu Xu's open letter. The first time I read it, it struck a chord with me and reminded me of my own job-hunting struggles after I got my Ph.D. I felt a kinship to him. But after reading his letter a few more times, I find it curious that he does not even mention his native China and its need for well-trained physicists like him.

9. **corvid**
November 15, 2022

I bailed out of doing high energy theory postdoc ~ 15 years ago because I had stopped believing the field was going anywhere and I didn't think I was willing to do what it takes to become a professor. I found the transition to being a hedge fund quant relatively easy, but I was a good candidate because I cared about and decently understood programming, happened to know a reasonable amount of probability theory, and at heart was someone who was good at and just wanted to be able to spend time solving problems in applied math. With a bit of preparation I was able to brush up on options math and passably fake knowing a few facts about finance, statistics and data analysis. Some observations:
1. as a physicist who could program, I could go to a website, download a useful library, and write some files of code that would compile and do some calculation for me that I could use in a paper. This skillset is far from knowing basic software engineering, where you write code that interacts with hundreds of thousands of lines of code that may change and any time and that was written by hundreds of different people, use source control tools, unit testing, have a build and release process, etc. This was a shock.
2. Even fifteen years ago, the time when physicists could just show up and get a job was over. Banks were starting to hire from quant finance masters programs (incidentally, I suspect one of the main functions of these programs is not so much to teach anything especially useful to employers, but to produce OPT visas for bright students from abroad). The big funds never focused exclusively on physicists, but have always hired people from CS, math, EE, stats, operations research etc. You still sometimes meet candidates who think because they have a string theory PhD from a fancy school they will have the red carpet rolled out for them. Not the case.
3. Another ex-physicist told me before I left academia that the hardest math I would ever need again was to take the moving average of some numbers. That has turned out to be far from the truth. On the other hand, I'm pretty sure Rentec isn't so astoundingly successful because they have the deepest bench of algebraic geometers.
4. By far the biggest change over the past fifteen years has been the rise of "data science" and machine learning. A lot of nonsensical marketing fluff goes along with this, but obviously there are real skills there. By and large physicists are cut off from this, at least at the cutting edge, though you do sometimes meet people who have used machine learning in a legitimate way for a real project. This seems to be true for theorists as well as people doing, say, high energy experiment or working on big data

projects in astronomy or whatever.
5. No doubt being from a well known, fancy school helps you get an interview.

10. **Andrew Bolt**
November 16, 2022

Having made the transition from academia to industry myself several years ago, I'd say the most useful skill was knowing how to learn. In interviews I'd tell them that I didn't know much about computer networks or satellites, but I was willing and able to learn. That ended up getting me into a pretty good career.

Now that I sit on the other side of the table, I find that recruiting people is often less about the specifics of what someone knows, but more about the attitude they bring to the team. I'd much rather hire someone smart who is willing to stick with tough problems than take someone who looks good on paper but won't actually deliver all that much.

11. **Grad student**
November 16, 2022

Ben:

I would be very reluctant to infer anything from the lack of references to China. There are many, many reasons why someone may not consider returning to their original country a desirable option.

12. **Brathmore**
November 16, 2022

I sympathize with grad students facing dim prospects for making a career out of their passion, but join the very, very large club. Most people don't get to pursue their passion as their career. If you got in a PhD in your passion, you had years more passion than most of us, and you are not haunted by the question of "what if I had…"

There are plenty of options besides software or finance for PhD physicists who aren't tenure track. Among them:

1. TEACHING high school, community college, adjunct professor, teaching at smaller liberal arts colleges. At a high school like Andover, you'd get to mentor some of the top students in the country.

2.LAW SCHOOL (in the US), potentially going into IP law. (Please first spend time with attorneys and see if you can imagine spending your days doing law. Average career satisfaction is low, but some are happy with their choice. I know a former Physics PhD making millions as a partner at a big firm, but the stars really aligned for him).

3. MEDICINE (in the US). You may need 1-2 years to take the bio and chem prerequisites (this can be done at an "extension school"). Then 4 years of med school, then 3 to 7 years of residency (where you're getting paid $50K – $70K per year). Then income ranging from $200-600K depending on the specialty. But make sure you like medicine first.

4. CONSULTING. If you have good people skills, you might be able to land a position at consulting firms like McKinsey or BCG, which can serve as a platform for other business jobs.

5. If you have trouble breaking into finance/software, get a master's in Statistics, Data Science, Financial Engineering, CS, etc. (or possibly do a post-doc in these subjects). For software, there are "coding camps" that take less than 1 year, and that have good placement rates. The intellectual content of these programs should be less demanding than a physics PhD, and the MS degree (or post-doc, or coding camp) should help get you into the field.

13. **CERN Particle Theorist**
November 17, 2022

Peter,

Sorry for the off-topic comment, but you, and your readers, would surely be interested to hear this.

I'm at CERN—I'd like to remain anonymous. CMS+Atlas picked up a 1350 GeV four-lepton excess, currently at three sigma. That's the rumor. There will likely be a press release this coming week.

14. **mimi**
November 17, 2022

Various commenters have said that software engineering is not necessarily easy to learn. My point was not that it was easy on an absolute scale but only relatively for hep-th PhD students. I've worked at some top tech companies and have met quite a few other physics/math PhDs there. All of them agree that software engineering / data science are much easier than what they had to study in grad school. I'm sure there are some physicists/mathematicians for whom programming is hard (as Peter Shor pointed out) but most can become quite good with a few months of intensive work.

15. **Peter Woit**
November 17, 2022

CPT,

Hadn't heard anything like that, and that rumor sounds implausible (a joint CMS-ATLAS press release????). Informed sources encouraged to confirm or deny….

16. **cgh**
November 17, 2022

I'll make one last comment in response to mimi and Peter S re programming that may be useful to physics/math people thinking about a transition:

It's been my experience that there's roughly two types of programmer that do what I do – the scrappy programmer and the programmer's programmer. The former tends to be a quant with good problem solving skills that can accurately generate prototype models that solve a problem. c++, cuda, python, matlab, apl applied to pdes, optimizatin, simulation, valuation, etc. What they tend not to be good with is how the latter excels. Coding standards, documentation, scalability, extensibility, compute costs. I've run into real problems early on in my career mediating this interaction. It's not uncommon to inherit tens, if not hundreds of thousands of lines of "spaghetti code", perhaps calling other code across a zoo of languages. Long ago I figured out how to solve this. I have a dedicated tech team that has front, middle, and back end programmers; and they own the code base. The back end are real low-level programers with PhD comp sci, hard physics, backgrounds. The front tends to be app dev. The middleware folks deal with the transmission of data. The pure quant teams prototype while the tech teams productionalize according to standards we've developed, inclusive of testing and documentation.

So one bit of advice that I have is an incoming quant that is a scrappy programmer, who I assume is good at problem solving in their IDE/language of choice, is to be amenable to learning about the broader ecosystems to allow for smoother handoff and assimilation of prototypes. I've seen places literally shut down or taken over because people ignored design, architecture, costs, and standards as growth overpowered considerations around continuity and sustainability. Some math/physics types can't be bothered with this stuff, or think it's beneath them, but the ones that do think about it tend to do better and certainly have an easier time having their ideas accepted amongst a team and, most gratifyingly, their solutions put into production.

17. **Engineer**
November 17, 2022

I recently retired from leading an organization with thousands of engineers, mathematicians, and scientists. I'd hired a lot of them. Over 35 years I learned to look to hire physicists (and mathematicians) for engineering roles. When we had to invent a solution to a novel problem, the physicists always started from first principles. To make a long story short, they helped the team find solutions our competitors never dreamed of. If you are a leader looking to amplify innovation in your team, bring some physicists in.

18. **martibal**
November 18, 2022

@CERN: by four-lepton excess, do you mean the possible discovery of a new lepton, in addition to the electron, tau and muon ?

19. **William Orrick**
November 18, 2022

@martibal Presumably the poster meant collision events with four leptons coming out and a reconstructed mass of 1350 GeV. Four leptons were one of the main signatures looked at in the original announcement of the Higgs boson discovery

20. **Peter Woit**
November 18, 2022

All,
At least one of the comments here about a supposed new LHC result was an impersonation by a troll. Most likely the whole thing is trolling. So, deleting any more comments about this that come in (unless you send me the draft of the paper, with the plots…).

21. **Matt Grayson**
November 18, 2022

FWIW, my experience (heading a global financial modeling group) agrees with @cgh in every particular. The different approaches to programming is a point that cannot get too much emphasis.

Math Job Rumors

November 17, 2022
Categories: Uncategorized

I noticed yesterday a website named Math Job Rumors that has been operating for a couple months. No idea what the story behind it is other than that it's clearly a descendant of Economics Job Market Rumors, which had some small participation by mathematicians, but is somewhat of a dumpster fire of misinformation, trolling, misogyny and various sorts of juvenile behavior. It looks like someone is trying to provide something similar aimed specifically at mathematicians, with some improvement over the EJMR environment.

One aspect of the site are threads devoted to rumors about tenure track and postdoc hiring in pure math, I don't know if there has been something like this before. In theoretical physics there's the venerable Theoretical Particle Physics Jobs Rumor Mill and the HEP Theory Postdoc Rumor Mill, but these are run in a very different way, with all information posted coming from one or more people who run the site, based on information sent to him/her/them.

The problem with the EJMR or Math Job Rumors model is that anonymity is needed for the whole thing to work, but once you start allowing people to post things anonymously, if you don't moderate what is posted, you'll quickly get overrun by idiots, trolls and other sorts of bad actors. Some kind of moderation is going on at the new site, but it's unclear who is doing it or on what basis.

After starting with the Official Peter Woit blog hate thread, I moved on to reading a few other threads. Lots of dumb stuff, lots of inside jokes, lots and lots of trolling. I confess though that in one case the trolling was clever enough to make me laugh out loud, but it's aimed at a really small audience. I did learn one piece of information that appears to be true, that prominent string theorist Shamit Kachru has gone on leave from his position at Stanford to work as a consultant in the finance industry.

In summary, for those mathematicians who read this blog and feel that they are not wasting enough time on mostly dumb internet stuff, you might want to take a look…

Comments

1. **David Appell**
   November 17, 2022

   Congratulations on having a hate thread. You have arrived.

2. **Jackson Jules**
   November 17, 2022

   I checked out your hate thread out of morbid curiosity, and overall it seeemed…weirdly supportive?

   Like don't get me wrong: there are a couple people throwing unwarranted potshots at you. But overall, the vibe was that people are quietly thankful for your willingness to talk about thorny topics like the ABC conjecture and string theory. Which surprised me somehow.

3. **lowly graduate student**
   November 18, 2022

   As grad student on the job market, as much as I find certain posts there to have kind of a toxic vibe, the discussion there does serve a purpose. How do mathematicians evaluate/rank one another? What/who is considered good/not good and why? These are the kind of conversations that people don't make face to face (at least when you're a graduate student) due to politeness, but somehow you really need to get a sense of this going forward in your career. And when there is a demand and a vacuum /lack of discussions in our everyday life, its gets filled by anonymous threads like mathjobrumors. I hope after much effort in sifting through the trolling, what I find will at least be somewhat resemblant of the truth – which is what people are too polite to say to my face.

4. **Peter Woit**
   November 18, 2022

   First a general comment: looking more at the math job rumors site and especially at recent threads, the extreme level of unfunny trolling and low quality of most of the supposedly serious comments doesn't bode well. Whoever is responsible for this needs to figure out a better solution to their moderation problem than whatever they're doing.

   lowly graduate student,
   You need to be very careful about taking seriously anything you read there, unless it's something factual you can check elsewhere. Besides the large amount of intentionally incorrect material (trolling trying to be funny and fool people), there's also a lot of just dumb stuff coming from people who don't know much about what they're talking about.

   This is the big problem with anonymity: if people don't put their name and reputation behind their words, this encourages all sorts of bad behavior. On the other hand, on certain topics (from hot button political issues to negative evaluations of certain fields or people), no one who sensibly wants to avoid making enemies is going to publicly say exactly what they think.

   Probably the best advice at the moment about an anonymous site like math job rumors is to (putting aside the trolling if you can recognize that) take whatever you read there about as seriously as you would take something another graduate student tells you when they're drunk late night at a bar. Actually, a lot of the comments on that site probably are posted by your fellow graduate students, drunk late at night, even if not actually at a bar…

5. **anon**
   November 18, 2022

   It almost looks like many people posting there want it to fail. The ratio of obvious troll posts to even semi-serious ones is probably five to one if not more. Looking at the earlier threads, it doesn't look like it was that bad in the beginning.

6. **Probabilist_omni**
   November 19, 2022

   This really got a laugh out of me for some reason:

   *"Starting fall of 2021, I will be taking an extended sabbatical/leave. Much of my effort during this period will be spent developing my skills in rapidly developing areas

like machine learning, with a view towards the science emerging from analysis of large datasets" . Nothing wrong with that of course and you never stop learning new stuff that you might find interesting etc etc, but still hilarious for "some" reason. I think every student that might still consider at this point going into string theory should be directed to that page; just food for thought.

*Section of Kachru's profile in the Research and Scholarship section

7. **Peter Woit**
November 19, 2022

Probabilist_omni,

Somehow his fall 2021 goal of working on "the science emerging from analysis of large datasets" turned into a 2022-23 job "as a consultant in quantitative research for a company which is active in a number of global financial markets."

8. **I**
November 24, 2022

"In summary, for those mathematicians who read this blog and feel that they are not wasting enough time on mostly dumb internet stuff, you might want to take a look…"

Oh no, Peter! Why did you push your readership into the quicksand? Some of them will be stuck in that pit for life.

Jokes aside, there's clearly some demand for something like this. Even if it degenerates into a cesspool of trolls and string theorists, well, the people on there are adults. If they're using it and having fun, isn't that a good thing? Plus, maybe they'll be more inclined to troll people there than on a blog's comments.

If you view the site as providing rumours worth checking out, then it does seem like you could still get valuable information from it. Though the signal to noise ratio would be quite low, maybe with good enough search tools that wouldn't be a problem.

9. **Peter Woit**
November 24, 2022

I,
They seem to be struggling with trying to find a workable moderation system that can deal with not just the kind of grotesque stuff that afflicts EJMR, but also a flood of garden-variety stupidity. I'm skeptical that there's any way to run a completely anonymous site that people with something well-informed and intelligent to say will want to use unless one can figure out how to keep the idiots under control. We'll see…

10. **Daniel Litt**
November 30, 2022

Ugh, really horrible stuff. Ruined my day, honestly.

The Mystery of Spin

November 22, 2022
Categories: Quantum Mechanics

Scientific American has a new article today about the supposedly mysterious fact that electrons have "spin" even though they aren't classical spinning material objects. The article doesn't link to it, but it appears that it is discussing this paper by Charles Sebens. There are some big mysteries here (why is Scientific American publishing nonsense like this? why does Sean Carroll say "Sebens is very much on the right track"?, why did a journal publish this?????).

These mysteries are deep, hard to understand, and not worth the effort, but the actual story is worth understanding. Despite what Sebens and Carroll claim, it has nothing to do with quantum field theory. The spin phenomenon is already there in the single particle theory, with the free QFT just providing a consistent multi-particle theory. In addition, while relativity and four-dimensional space-time geometry introduce new aspects to the spin phenomenon, it's already there in the non-relativistic theory with its three-dimensional spatial geometry.

When one talks about "spin" in physics, it's a special case of the general story of angular momentum. Angular momentum is by definition the "infinitesimal generator" of the action of spatial rotations on the theory, both classically and quantum mechanically. Classically, the function $q_1p_2-q_2p_1$ is the component L_3 of the angular momentum in the 3-direction because it generates the action of rotations about the 3-axis on the theory in the sense that
$$\{q_1p_2-q_2p_1, F(\mathbf q,\mathbf p)\}=\frac{d}{d\theta}_{|\theta=0}(g(\theta)\cdot F(\mathbf q,\mathbf p))$$
for any function F of the phase space coordinates. Here $\{\cdot,\cdot\}$ is the Poisson bracket and $g(\theta)\cdot$ is the induced action on functions from the action of a rotation $g(\theta)$ by an angle θ about the 3-axis. In a bit more detail
$$g(\theta)\cdot F(\mathbf q,\mathbf p)=F(g^{-1}(\theta)\mathbf q, g^{-1}(\theta)\mathbf p)$$
(the inverses are there to make the action work correctly under composition of not necessarily commutative transformations) and
$$g(\theta)=\begin{pmatrix}\cos\theta&-\sin\theta&0\\ \sin\theta &\cos\theta &0\\ 0&0&1\end{pmatrix}$$

In quantum mechanics you get much same story, changing functions of position and momentum coordinates to operators, and Poisson bracket to commutator. There are confusing factors of i to keep track of since you get unitary transformations by exponentiating skew-adjoint operators, but the convention for observables is to use self-adjoint operators (which have real eigenvalues). The function L_3 becomes the self-adjoint operator (using units where $\hbar=1$)
$$\widehat L_3=Q_1P_2-Q_2P_1$$
which infinitesimally generates not only the rotation action on other operators, but also on states. In the Schrödinger representation this means that the action on wave-functions is that induced from an infinitesimal rotation of the space coordinates:
$$-i\widehat L_3\psi(\mathbf q)=\frac{d}{d\theta}_{|\theta=0}\psi(g^{-1}(\theta)\mathbf q)$$

The above is about the classical or quantum theory of a scalar particle, but one might also want to describe objects with a 3d-vector or tensor degree of freedom. For a vector degree of freedom, in quantum mechanics one could take 3-component wave functions $\vec{\psi}$ which would transform under rotations as
$$\vec{\psi}(\mathbf q)\rightarrow g(\theta)\vec{\psi}(g^{-1}(\theta)\mathbf q)$$
Since $g(\theta)=e^{\theta X_3}$ where
$$X_3=\begin{pmatrix}0&-1&0\\ 1&0&0\\0&0&0\end{pmatrix}$$
when one computes the infinitesimal action of rotations on wave-functions one gets $\widehat L_3 + iX_3$ instead of $\widehat L_3$. $S_3=iX_3$ is called the "spin angular momentum" and the sum is the total angular momentum $J_3=L_3 + S_3$. S_3 has eigenvalues $-1,0,1$ so one says that that one has "spin 1".

There's no mystery here about what the spin angular momentum S_3 is: all one has done is used the proper definition of the angular momentum as infinitesimal generator of rotations and taken into account the fact that in this case rotations also act on the vector values, not just on space. One can easily generalize this to tensor-valued wave-functions by using the matrices for rotations on them, getting higher integral values of the spin.

Where there's a bit more of a mystery is for half-integral values of the spin, in particular spin $\frac{1}{2}$, where the wave-function takes values in $\mathbf C^2$, transforming under rotations as a spinor. Things work exactly the same as above, except now one finds that one has to think of 3d-geometry in a new way, involving not just vectors and tensors, but also spinors. The group of rotations in this new spinor geometry is $Spin(3)=SU(2)$, a non-trivial double cover of the usual $SO(3)$ rotation group.

For details of this, see my book, and for some ideas about the four-dimensional significance of spinor geometry for fundamental physics, see here.

Update: I realized that I blogged about much this same topic a couple years ago, with more detail, see here. One thing I didn't write down explicitly either there or here, is the definition of spin in terms of the action of rotations on the theory. It's very simple: angular momentum is the infinitesimal generator of the action of rotations on the wave-function, spin angular momentum is the part coming from the point-wise action on the values of the wave-function (orbital angular momentum is the part coming from rotating the argument). Using a formula from my older posting, for a rotation about the z-axis, the total angular momentum operator $\widehat J_z$ is by definition
$$\frac{d}{d\theta}\ket{\psi(\theta)}=-i\widehat J_z \ket{\psi(\theta)}$$
The spin operator $\widehat S_z$ is what you get for $\widehat J_z$ when you act just on the wave-function values. For a spin n/2 state particle, the wave-function will take values in $\mathbf C^{n+1}$. For the spin 1/2 case the action of rotations is by 2 x 2 unitary matrices of determinant one (the spinor representation). For a rotation by an angle θ about the z-axis, this is
$$e^{-i\theta\frac{\sigma_3}{2}}$$
so the spin operator is
$$\widehat S_3=\frac{1}{2}\sigma_3$$

Comments

1. **André**
November 23, 2022

   One more big mystery that you forgot to mention is why Scientific American writes about this three years after its publication (and four and a half years after it first appeared on the arXiv)?

2. **Shantanu**
November 23, 2022

   One more mystery is that this is published in physics section of arxiv and not hep-th etc.

3. **Peter Woit**
November 23, 2022

   Shantanu,

That’s not really a mystery at all…

4. **Pascal**
November 23, 2022

Peter, can you elaborate on your objections to Seben’s paper? The bulk of the paper seems to be a history of the understanding of spin, from classical ideas to modern ideas based on QFT, and on the obstacles that had to be overcome to get there. Seben also uses this opportunity to advertise his own interpretation. He carefully states that this is just an *interpretation* (perhaps in the sense of “interpretation of quantum mechanics”), not a new theory leading to any new prediction.
Do you think his history is wrong? Or correct, but this material is so well known that writing a historical paper about this is pointless? Did he get the physics wrong? Are you against the discussion of interpretations in general? In summary, to use your own punctuation style: what is wrong with this paper?????

5. **Doug McDonald**
November 23, 2022

Something that came to mind. Mayybe this is a nonsequitor, but still …

Peter: you’re a mathematician. You are giving a mathematician’s argument. Its based on the Poincare group, which includes spinors. It works. It agrees with the world. But still, it just suggests from the existance of spinors that there might be spin 1/2 particles, or more generally, particles of half-integer spin.

But ….

by the same argument there could be … and is … a “Super-Poincare” group that promotes spinors to generators of a sort of “Supersymmetry” which we should have a look at for particle physics. We HAVE had a look or two or ten thousand … and it doesn’t work.

I see that as a generic problem of going from math to physics. Or to be a bit kinder, of going from math to lots of funding for theoretical physics that does not work.

6. **Peter Woit**
November 23, 2022

Pascal,

I’m not commenting on the history or “interpretation” in the Sebens paper. The “paradox” Sebens and Becker (and others before them) go on about is based on the idea that “angular momentum” is the usual $\mathbf q \times \mathbf p$ formula from high school physics, a measure of how an extended object is rotating about a point. You can’t understand spin this way, no matter how hard you try to give strained interpretations to terms in the Dirac free field.

The point I was trying to explain in the posting is that you need to start not from high school physics, but from the fundamental fact that angular momentum is the generator of the action of rotations on your theory. Becker and Sebens seem either unaware of or uninterested in this basic fact. From this more fundamental definition, you can not just assume the formula $\mathbf q \times \mathbf p$ you learned in high school, but derive it (by looking at how the theory behaves under infinitesimal rotations of space).

From this point of view, whenever you have fields, wavefunctions or whatever that are not scalars under rotations (a 3d-vector field the simplest example), when you compute the formula for angular momentum, you will get not just a $\mathbf q \times \mathbf p$ orbital angular momentum term, but another term. This is the “spin” angular momentum. It has nothing to do with stuff moving in space.

Another point I was trying to make is that this has nothing to do with the complexities of 4-d space-time, spinors, the Dirac equation, etc. This is a purely 3d story, just look at a vector-valued field in 3d (any dynamics you want, the definition of angular momentum doesn’t depend on dynamics).

7. **Peter Woit**
November 23, 2022

Doug McDonald,
First of all, my Ph.D. is in physics, not math…
Secondly, as I tried to explain in my previous comment, this has nothing to with 4d symmetries (Poincare), nothing to do with spinors, certainly nothing to do with SUSY. The mathematics is very basic: rotations of physical 3d space (applied to whatever kind of physics you are doing, Hamiltonian, Lagrangian, classical, quantum, field theory or not).

I am only bringing mathematics into this with the claim that to understand what angular momentum is, to derive the formula for it, you have to look at what 3d rotations do to your theory. Not everyone needs to be willing to use this level of math, but if they’re not willing to do so, they shouldn’t be making complicated arguments about supposed paradoxes with angular momentum.

8. **anon**
November 23, 2022

The field (2nd quantization) gives mass and affects electric charge (vacuum polarization). So why can’t it have an effect on spin, too?

9. **Peter Woit**
November 23, 2022

anon,
The fundamental definition of angular momentum and spin is the same in QFT as in any other part of physics (generator of infinitesimal rotations) and has nothing to do with the dynamics.

I realized that I actually worked this out very explicitly for the simple cases of spin zero and spin 1/2 in non-relativistic QFT in my book. See sections 38.3.2 and 38.3.3 of https://www.math.columbia.edu/~woit/QMbook/qmbook-latest.pdf

10. **Peter Woit**
November 23, 2022

Another comment: here and in the book I’ve emphasized phase space (Hamiltonian) methods. Same thing works in the Lagrangian formalism, where Noether’s theorem first tells you how to define the angular momentum (including the spin term) from the infinitesimal action of rotations on the fields, then tells you that if the Lagrangian is rotation invariant, the angular momentum will be conserved.

11. **George**
November 23, 2022

The single particle theory is not really a good theory, as it does not give energies that are bounded from below for free particles. Of course, you can have a wavefunction in the spinor representation of rotations and you will get spin quantum numbers indeed. But you do not get the energy spectrum correctly without Dirac sea voodoo. The first time that spin is explained within a logically acceptable framework is in QFT, I would argue.

12. **akhmeteli**
November 23, 2022

Peter Woit,

Thank you for mentioning the article in the Scientific American.

You wrote: "I'm not commenting on the history or "interpretation" in the Sebens paper." However, Sebens' work is specifically about interpretation. He wrote: "What follows is a project of interpretation, not modification."

Sebens' work may be good or bad, but I would appreciate your arguments. What you wrote so far sounds to me like "Sebens cannot discuss interpretation of spin unless he uses the same mathematical formalism as I do." My understanding is your formalism has the same predictions as the traditional one(s), so I am not sure one must exclusively use your formalism to triage interpretations.

If I misunderstood your arguments, I apologize.

13. **Peter Woit**
November 23, 2022

George,
The problem of negative energies in relativistic quantum mechanics has nothing to do with spin (you have the same problem in the spin-less theory).

Non-relativistically (or in the non-relativistic limit of a relativistic theory), there's a perfectly well-defined quantum theory of a single spin 1/2 degree of freedom. Sometimes called the Pauli-Schrodinger theory, see chapter 34 of the book I linked to. In this theory you can easily compute the angular momentum and see that it has separate orbital and spin angular momentum terms, and see the simple origin of the spin angular momentum term.

14. **Peter Woit**
November 23, 2022

akmeteli,
My formalism is exactly the standard one in the textbooks: the angular momentum observable of a theory is the infinitesimal generator of the action of rotations. The most common version of this is the Lagrangian version, and if you compute this by the textbook Noether method in a theory where the values of the fields transform under rotations that gives you the "spin" part of the angular momentum.

Sebens is doing some rather obscure calculations in a relativistic theory and seems to be claiming that these justify "interpreting" spin in terms of some kind of motion in space. This doesn't explain anything. It's the exact opposite of an explanation, an attempt to mystify and confuse. In a rough analogy it's like saying I don't like potential energy, all energy should be kinetic energy, so I need to come up with a complicated way to "interpret" potential energy as kinetic energy.

15. **Blake Stacey**
November 24, 2022

The paper's conclusion, as the last line of the abstract says, is that "The electron's gyromagnetic ratio is twice the expected value because its charge rotates twice as fast as its mass." Personally, I'd take that as *another objection to the idea that you can think of the electron as "spinning."* If you work very hard to dodge all the objections that physicists raised decades ago, the best case is that you still run into the problem that the charge rotates twice as fast as the mass, which is not how a classical charged body can behave. Your picture of the electron as a classically spinning object is unphysical in a new way.

I feel like *Scientific American* should have mentioned that Sebens and Carroll were coauthors. It's not a major thing (that was several years ago), but still. I'd expect that to be included if someone quoted me about work an erstwhile colleague did.

16. **Alex**
November 25, 2022

I think the reason some people see quantum spin as mysterious is that, even when you correctly define angular momentum in both classical and quantum as the infinitesimal generator of rotations, in the quantum case you get a sum of the usual quantized so-called orbital angular momentum (which has a clear classical counterpart) with the so-called spin angular momentum, which lives in an "internal" finite-dimensional Hilbert space, tensored with the usual L^2(R^3), and which, naively at first sight, doesn't seem to have a classical analogue.

But you can actually give a more general Poisson structure on the classical phase space so that you indeed get a new internal space and generators that correspond to a sort of classical spin. The reason for calling this an analogue of the quantum spin is that it's related to it by a deformation quantization, so it's indeed its true classical analogue in the sense of quantization theory. For some details see page 31 here: https://arxiv.org/abs/quant-ph/0506082

17. **4gravitons**
November 25, 2022

"but one might also want to describe objects with a 3d-vector or tensor degree of freedom"

I would have guessed that this is the crux of the discussion, though. Why would one want to do this (independent of our having observed objects that do indeed have this property)? I would think that when people think of spin as mysterious, they're asking a question of this form (and as Doug McDonald points out, the principle can't be something like "if you can describe such objects they exist", if you don't expect SUSY to exist).

(Can one say that of the original classical setup too? Kind of? Angular momentum of extended objects is at least derivable from (to speak loosely) linear momentum of point particles. I think it's a bit silly for a philosopher to decide arbitrarily that motion of point particles is the most fundamental thing in the universe, but eh, I'm skeptical of metaphysics in general, most people aren't.)

I'd also guess that, to whatever extent Carroll and/or Sebens think the problem is solved in QFT, it's not because of the mathematics of QFT, but rather because QFT motivates us to think of point particles as not ontologically basic, and thus makes it more acceptable to think of objects as just carrying whatever degrees of freedom we find convenient. Obviously you can do this in QM as well, but you're less motivated to, in the same way that you're less motivated to develop classical field theory before Maxwell develops E&M.

From what you describe of Sebens' "explanation", it indeed sounds like it's addressing this in a stupid way, regardless, especially if it's invoking charge (does he think sterile neutrinos can't exist?).

18. **Low Math, Meekly Interacting**
November 25, 2022

99% of this is completely over my head, but I do wonder: Could there be any pedagogical value in abolishing "spin" from the quantum lexicon? I got enough of a physics education to know where the average person gets led astray, and it's not entirely our fault. "Classically non-describable two-valuedness" is rather a mouthful, but is there any alternative to "spin"? Could the world be rid of the notion that "spin angular momentum is like spin, only it's not, and nothing's spinning"? Seems a good place to start would be to swear off entirely the high-school/first-year physics picture asking us to "imagine a little bar magnet spinning".

Seems the only proper way to popularize QM is to stop putting paradoxical ideas in our heads to begin with, such that we need to spend so much time and energy later disabusing ourselves of misunderstandings rooted in bad classical analogies. Obviously, even professional physicists and philosophers continue to fall into the very same traps the pioneers of QM did, so it seems the first priority is to drill into everyone's brains that there are no classical analogs worth using. Perhaps then SciAm, etc. would have nothing of general interest to write about.

19. **Low Math, Meekly Interacting**
November 25, 2022

I think it's not too hard for a high-school student to absorb the following (assuming I'm not screwing something up myself):

Lorentz invariance and Noether's Theorem represent two of the deepest and, to the best of our knowledge, inviolate truths of the natural world. It follows directly from the fact we occupy four space-time dimensions that particles like the electron must have four intrinsic degrees of freedom to allow for conservation of energy and momentum. These are fundamentally quantum degrees of freedom with no classical analogs, but bear some resemblance to classical, extended objects in the fact that these degrees of freedom have quantum units of charge and angular momentum.

I.e, if you can handle thinking about charge as an "intrinsic" property of an electron, there's no justifiable reason to think of "spin" any differently, except that our brains are evolved to conceive of such properties in classical terms. Nevertheless, they allow for no better explanation, and indeed none are necessary.

Start teaching it that way early and ditch "spin"! Many a wayward and frustrated brain would be grateful.

20. **Peter Woit**
November 25, 2022

Alex,
It's true that there's a good way to think of the classical limit of "spin", as adding a new factor to phase space of a sphere in $\mathbf R^3$. This idea is actually very general: if the values of your field transform under a group G as an irreducible representation of G, take as new factor in phase space the corresponding co-adjoint orbit in the dual of the Lie algebra of G. If you want, you can then use a phase-space path integral, and think of your classical spinning particle as moving along a trajectory in the usual phase space times the sphere (or co-adjoint orbit in general).

The problem with this is that a curved, finite volume phase space like the sphere behaves rather differently than the conventional phase space. Additionally, it's only in the limit of infinite spin that you recover a classical limit. Things like spin 1/2 and spin 1 are at the opposite, truly quantum limit.

21. **Peter Woit**
November 25, 2022

LMMI,
The word "spin" is part of the problem, but just part of the more general problem of trying to explain what a quantum particle is, starting from a classical particle. This just can't work, it inherently makes quantum theory seem like incoherent, incomprehensible voodoo.

To understand angular momentum you just need to think about 3d space and 3d rotations of this space, invoking four dimensions and special relativity only makes this harder to understand. There is a difference between spin and purely internal degrees of freedom like charge. In both cases in quantum theory you are using the fact that the values of your wave-function (or field) transform under a group acting point-wise on the values. In the internal symmetry case (charge), your group doesn't act on space. But in the spin case, the group is the rotation group, acting at the same time moving around points in space. You can separately see the action on values and the action on spatial points, and this separation is the separation between spin angular momentum and orbital angular momentum.

22. **Andrew Thomas**
November 25, 2022

Could the world be rid of the notion that "spin angular momentum is like spin, only it's not, and nothing's spinning"? Seems a good place to start would be to swear off entirely the high-school/first-year physics picture asking us to "imagine a little bar magnet spinning".
Well, I would suggest exactly the opposite. I think we need to get away from this idea that a particle's spin is "nothing like actual spin and nothing's spinning". The particle has angular momentum: if you shine a beam of electrons with each having the same spin value onto a object (like a brick) then that object will start to spin – for real. I don't think this is generally appreciated. That sounds a lot like real spin to me.

Here's Frank Wilczek (Nobel prize particle physics) on the subject from his book Fundamentals: "If you've ever played with a gyroscope, you'll have a head start on understanding the spin of elementary particles. The basic idea of spin is that elementary particles are ideal, frictionless gyroscopes which never run down. Spin changed my life. I always liked math and puzzles, and as a child I loved to play with tops". Argue with Frank at your peril.

Nobody is suggesting that quantum spin is the same as classical spin, but it is much closer in nature than is generally realised, certainly closer than is usually suggested in popular science books.

23. **Peter Woit**
November 25, 2022

Andrew Thomas,
Yes, spin is angular momentum, and if you want to play with metaphorical descriptions of it as Wilczek does that's fine. But you should keep reading in the text (page 75-6) that you quote. In the rest of the paragraph he goes on to explain that the way "spin changed my life" for Wilczek is that he took Peter Freund's advanced course on "the application of mathematical symmetry" (e.g. Lie groups and representations) to physics. In this course
"Professor Freund showed us how some extremely beautiful mathematics, building on the idea of symmetry, leads directly to concrete predictions about observable physical behavior… To me, the most impressive example of this connection was – and still is – the quantum theory of angular momentum, which he showed us… To experience the deep harmony between two different universes – the universe of beautiful ideas and the universe of physical behavior – was for me a kind of spiritual awakening. It became my vocation. I haven't been disappointed."

To me what Wilczek is saying is that if you want to talk about physics at the level of vague metaphor, fine to think of spin as something spinning. If you want to understand what spin actually is, you need to understand it as a contribution to angular momentum, where angular momentum is defined mathematically in terms of rotational symmetry.

24. **Lars**
November 27, 2022

The spin of elementary particles like electrons is one thing.

But the spin of composite particles would seem to be a different beast.

For example, the experimental evidence indicates that the spin of the proton IS at least partly the result of "motion in space", depending on the "orbital" angular momentum of the component quarks.

See
"What we know and what we don't know about the proton spin after 30 years" (Y. Zhao, 2020, Brookhaven National Laboratory)

"Results from Jlab 6 GeV experiments and HERMES data suggest a substantial quark orbital contribution"

25. **Peter Woit**
November 27, 2022

Lars,
When you have a composite bound state like the proton which does not correspond to an elementary field, you can assign it a spin, by looking at the lowest energy state and seeing how it behaves under rotations. For a proton, you find it transforms as the spinor representation, so has spin 1/2. The relation between the spin of the proton and the spin of its constituents is complicated and as far as I know not completely understood. The problem is that the proton is a strongly coupled system of both quarks and gluons, and we lack good calculational methods for such systems. For a composite system of free particles you can understand the total angular momentum in terms of the spins of the components and orbital angular momentum, but a strongly coupled system is something different.

I think what is going on with some people is that they don't like intrinsic spin of an elementary particle because it doesn't correspond to something they can visualize in a model of reality which consists of scalar elementary particles moving in space. They want to explain the spin 1/2 of an electron in terms of this model of reality. But when you do this, you run into an intractable problem: if you have spinless particles and only orbital angular momentum, all states will have integral total angular momentum, no possible way to get 1/2 integral values.

If you ignore the half-integrality problem and try and build a model of electrons as bound spinless particles anyway, you run into the problem that electrons exhibit pointlike behave on scales much smaller than their Compton radius. Then when you try to explain spin 1/2 in terms of orbital angular momentum you get into problems with your hoped-for model, since it would imply faster than the speed of light motion of the component particles.

26. **Peter Orland**
November 27, 2022

Hi Peter,

Feel free to delete this, if it seems off-topic, but spin (the physical phenomenon) can be visualized as mechanical revolution. Wess-Zumino actions for spin are well-known (even for Dirac particles, which I wrote a paper about ages ago. There is a close relation with twistors).

Now these Wess-Zumino actions are equivalent to (Fock-Tamm) charge-monopole systems, even if one does not accept the charge and monopole as existing in physical space. This picture is actually useful for understanding Skyrmion spin.

Of course, this way of understanding spin has nothing to do with the topic of the Scientific American article.

27. **Peter Woit**
November 27, 2022

Peter,
That's related to what Alex above brought up. If you enlarge phase space by adding a factor of a co-adjoint orbit (for 3d rotations, the orbit is a sphere in 3d space) and quantize this, you get wave-functions with values in a non-trvial representation of the rotation group. The phase-space path integral for the sphere factor will be given by integrating over paths on the sphere with action a Wess-Zumino term, one you could think of as the phase factor picked up from the vector potential of a monopole at the center of the sphere. But, the monopole is unphysical, as is the sphere you're moving on.

This whole story leads to really beautiful mathematics, it's the simplest case of the Borel-Weil-Bott geometric constructions of representations of compact Lie groups. I'm a huge fan of this way of thinking, but I don't think those who want to get more sophisticated mathematics out of physics really want to go down this path…

28. **Davide Castelvecchi**
November 28, 2022

Peter,

This is a very interesting discussion! You point out that there is a well-understood machinery for understanding spin, based on group representations. But the very fact that there is Dirac's correspondence between Poisson brackets and commutators and that all of this works is at least a bit mysterious, no? (Also, I thought the quantization procedure wasn't even uniquely defined?)

Two: the thing that's measurable, spin along a certain axis, is often described as telling us that the spin is "up" or "down" — even though it doesn't tell us that the angular momentum points in a certain direction, only that it has a certain angle with the axis; it ostensibly measures a component of a vector, but that vector is not even well-defined (because you can't measure all of its components).

Three: the ontology of a particle's spin. If are to believe that the wavefunction has physical meaning, then each electron in a (unpolarized) beam has a well-defined spin state — in the sense that at any given time and place there exists a direction along which it will have 100% probability of giving a spin "up". But there doesn't seem to be a way to do anything with this because we can only ever deal with mixed states and the properties of the ensemble. (Ok I guess this is a mystery about the quantum measurement problem, not specifically spin.)

Four: The Hilbert space where the spin state lives is some abstract three-dimensional sphere outside of the physical world. And if I understand correctly, when you associate an operator L_z to measuring spin in a certain direction, the correspondence between the operators L_x and L_y and the other directions of space is partly arbitrary.

All of this sounds pretty mysterious to me!

29. **Peter Woit**
November 28, 2022

Davide,
I'm not claiming there are no mysteries here, just that trying to understand what spin is in terms of stuff moving in space is a huge mistake. Also, that trying to do this in the 4d relativistic (Dirac equation) context is another sort of mistake, since the situation there is quite tricky, and the phenomenon you are trying to understand is already there in a much simpler context in the 3d non-relativistic theory. Maybe you can shed some light on the mystery of why this article was published by SciAm…

I just took a look at various sources to see how they explain "spin". Found everything from the completely dreadful (Wikipedia) to some places that get things right. As you might expect, one of the latter is Weinberg, who in his QM book (chapter 4) starts off with "we should think of the existence of both spin and orbital momentum as consequences of a symmetry principle" and an explanation of how to get from rotations to angular momentum. He also correctly states "there is nothing about spin that requires relativity to be taken into account".

About your four mysteries, the first three involve the basic mystery of the relation of classical and quantum mechanics, including the mysteries of measurement theory. The spin 1/2 system doesn't explain any of these mysteries, but it does provide the simplest examples of non-classical behavior so a good place to start in thinking about this basic mystery.

For the fourth, it is an observed fact of nature that fundamental particles are best described by a $\mathbf C^2$ valued wave-function, transforming as a spinor under rotations. This is mysterious in the sense that it is highly non-classical and not in accord with our classical intuitions at all. It's also telling you that fundamental physics is described not in terms of usual geometry, but in terms of spinor geometry. To me that's a big hint about what to look for in a more fundamental theory. It should most naturally be formulated in terms of spinors, with a compelling argument for twistors that in twistor theory a point in space time is tautologically a spinor $\mathbf C^2$. Yes, this is an argument from mathematical beauty.

What's not mysterious here is how you get from "particles are described by spinor-valued wavefunctions" to the description of what spin is. This is nothing other than the simple fact about 2 by 2 matrices that you rotate spinors by exponentiating Pauli matrices.

Was thinking of writing a part II of this posting that explains this more, maybe later today…

30. **Arnold Neumaier**
November 28, 2022

Peter,

"transforming as a spinor under rotations. This is mysterious in the sense that it is highly non-classical and not in accord with our classical intuitions at all."

In fact, everything about spin 1/2 was already discussed by Stokes in 1852, long before quantum mechanics was born. He had no problems with intuition, though it was different from the usual one!
See my article "A Classical View of the Qubit" at
https://www.physicsforums.com/insights/a-classical-view-of-the-qubit/

31. **Levy-Leblond**
November 28, 2022

Levy-Leblond explained how spin can arise without relativity more than 50 years ago:

https://link.springer.com/article/10.1007/BF01646020

32. **Peter Woit**
November 28, 2022

Levy-Leblond,
This goes back much further than that, to more than 95 years ago, and Pauli's 1927 paper
https://link.springer.com/article/10.1007/BF01397326 (which Levy-Leblond refers to).

What Levy-Leblond is working out is going from the usual story about elementary particles being irreducible representations of the Poincare group given by solutions of relativistic wave-equations to instead the non-relativistic case of the Galilean group and its representations. But this is still more complicated than you need, the Galilean boosts have nothing to do with spin. All you need is spatial translations and rotations (the Euclidean group in 3d). Quantized these give momentum and angular momentum operators. If you look at one component wave functions you just get orbital angular momentum, if you take multiple component wave-functions, with rotations acting non-trivially on the values at a point, the angular momentum acquires the additional spin angular momentum term.

Again, I worked out the details of all of this explicitly in chapter 34 of my book, but there's nothing there that Pauli (with some help from Weyl) wouldn't have already understood in the late 1920s.

33. **martibal**
November 29, 2022

Is there any conceptual explanation of why the multicomponent wave function transforms under the double cover of the rotation group, and not the rotation group itself ?
(I know this is a fact of nature, is there any understanding of why it should be so ?)

34. **Arnold Neumaier**
November 29, 2022

martibal,

not every multicomponent wave function transforms nontrivially under the double cover of the rotation group, , and not the rotation group itself; only (and precisely) those for fermions do. This is explained by the spin-statistics theorem from relativistic quntum field theory.

35. **Peter Woit**
November 29, 2022

Martibal,
To me the conceptual explanation is just that the geometry of physics is inherently spinorial, not surprising since spinor geometry is more fundamental than the usual

geometry in terms of vectors. In arbitrary dimensions you have a non-trivial double-cover of the rotation group and spinor representations. The spinor representations are more fundamental than the usual vectors (and tensors built out of them) since you can build vectors out of spinors but not spinors out of vectors.

In three dimensions there is a very simple way of seeing this using the quaternions. If you identify space with imaginary quaternions, rotations are given by conjugation by unit quaternions q. Unit quaternions form a 3-sphere, isomorphic to group SU(2). The double cover comes from the action of q and -q being the same rotation. The spinor representation and its fundamental nature comes from the fact that SU(2) acts on $\mathbf C^2$, and all representations of SU(2) can be build out of this one.

36. **vmarko**
November 29, 2022

I can see that the discussion has covered a wide range of points of view and properties of spin, but apparently not all, so maybe I can chip in a bit. 🙂

Maybe it should be emphasized that the notion of spin does not really have much to do with quantum mechanics. Rather, it can be regarded as a field theory phenomenon, completely classically.

For example, in Maxwell's electrodynamics, the electromagnetic wave has nonzero (integer) spin. Likewise for a massive Proca theory, and for general relativity. And likewise for the half-integer spin of the Dirac, Weyl, Majorana, Rarita-Schwinger fields. All of these can be understood as classical field theories. And as Peter noted, even the nonrelativistic Schrodinger-Pauli equation (regarded as a classical field theory) describes nonzero spin.

In general, in field theory one can talk about the action of a rotation operator on the spacetime point (the "argument") and on the field components (the "function"). The former gives rise to orbital angular momentum, while the latter gives rise to spin. And there is nothing quantum-mechanical in any of that.

Also, regarding the distinction between non-relativistic and relativistic theories — while both can describe spin, there are a few subtle differences. First, the Dirac equation predicts the electron magnetic moment, whereas the Schrodinger-Pauli equation makes no such prediction (magnetic moment is a free parameter). Second, in relativistic theory the action of a rotation group element has to have the same parameter for both the orbital and spin parts (since relativistically there is just one notion, the "total" angular momentum operator, as a generator of the Lorentz group), while in non-relativistic theory the actions of the orbital and spin parts are independent, with a priori different parameters. This has been known to lead to incorrect predictions in nonrelativistic theory, since one is falsely allowed to "rotate" the orbital and spin angular momenta independently of each other. And finally, in QFT there is the spin-statistics theorem, which is inherently a relativistic (and also quantum) statement, with no non-relativistic counterpart.

Best, 🙂
Marko

37. **Jeffery**
November 29, 2022

Hi Peter,

I came across an old paper with similar claim published in 1985. Does this make any sense at all?

https://physics.mcmaster.ca/phys3mm3/notes/whatisspin.pdf

38. **Peter Woit**
November 29, 2022

Jeffery,
Sebens references that one, likely used some of it in whatever he is doing. Very explicitly same motivation: the only "physical explanation" of spin angular momentum is to try and interpret it as some kind of orbital angular momentum. Zero mention of or any interest in what angular momentum really is (generator of action of rotations on the theory). The 1985 author quotes 1928 and 1939 papers with similar claims. Another mystery is why SciAm is selling this as something new.

39. **martibal**
November 30, 2022

If you think classically (i.e. non quantum) then why the need of going to the double cover of SO(3) ? Why is nature not happy just with rotations and needs to go to the double cover ? And why does it stop to double cover and do not consider other covering group ?

40. **Peter Woit**
November 30, 2022

Martibal,
The only interesting covering group of SO(n) is the spin double cover, other covers are trivial. Why should nature be happy with vectors and SO(3) and not exploit the more fundamental spinor representation and the double cover?

The classical situation is a little complicated to state, but here are some comments:

You can make a classical theory of a particle with spin in two ways:

1. Extend phase space by a sphere. You'll then get an extra "spin" contribution to angular momentum, which can take any non-negative value (determined by the radius of the sphere). No integrality or half-integrality of spin until you quantize.

2. Extend phase space by an anti-commuting coordinate. This is "pseudo-classical" mechanics and gives you a spin-1/2 term in the theory.

If you work with classical fields, you can write down classical field theories where the field transforms under any representation (including spinor) of the rotation group that you want. These can only be half-integral.

Spinors are not inherently quantum. If you go from 3 to 4 dimensions, the story of 4d spinors is very much a basic part of the theory of twistors. Twistor theory is not a quantum theory, almost all the work in twistor theory has been based on applying the twistor geometrical setup to classical field theories.

41. **tulpoeid**
December 2, 2022

I might be naive in my knowledge of QM (honestly), but having to "take into account the fact that in this case rotations also act on the vector values, not just on space" is or at least results in a very deep mystery. And definitely not the case that "When one talks about "spin" in physics, it's a special case of the general story of angular

momentum". Spin is fundamentally about more than rotation and angular momentum, while at the same time surprisingly connected to them.
If I'm misunderstanding something obvious, I'd be grateful for a useful nudge.

42. **Peter Woit**
December 2, 2022

tulpoeid,
There is no mystery. All I'm saying is that if you write down a theory of fields (classical, quantum, whatever) and decide to use fields that take 3d vector values, then when rotations act on your theory, you have to take into account that the values of the fields rotate (giving spin angular momentum, "spin 1" in this case) not just the arguments (giving orbital angular momentum).

43. **Giorgio Pastore**
December 3, 2022

Peter,

I think that your judgment of Seben's work is too drastic. Your considerations about angular momentum as the generator of the rotations are correct, of course, but I think they do not exhaust the subject. Indeed, they do not directly address the content of Seben's paper.

I understand that the central issue is not to reduce the angular momentum to some cross-product of position and momentum. I think that Seben's work is on the same track as previous work by Belinfante and other people, one of the last being Ohanian.
Work that was not, and it is not an alternative to the view of angular momentum as the generator of rotations, and it is not a nonsense either. It is a work that helps to address a different issue: why, in the case of charged particles, such orbital and angular momenta carry a magnetic moment?

Electromagnetism requires the presence of charged currents to get magnetic moments. Seben's point of view has the advantage of clarifying the relation between such currents and the properties of the wave field. How would you get the presence of magnetic moment from the generators of the rotations?

44. **Arnold Neumaier**
December 3, 2022

Giorgio Pastore,

"How would you get the presence of magnetic moment from the generators of the rotations?"

It simply comes from the current $j(x)=:\psi(x)^*\sigma\psi(x):$

45. **Peter Woit**
December 3, 2022

Giorgio Pastore,

From my reading of Sebens (and Ohanian), their main arguments are not about the magnetic moment issue. The way spin couples to EM is very interesting and non-trivial, but I don't see how anything they do elucidates that question at all.

Comments on AdS/CFT

November 30, 2022
Categories: Uncategorized

This is something I wrote back in June, for context see the next posting.

First of all, there's the following, which is not strictly scientific, but very relevant to how one decides to evaluate progress in a subject.

- The Maldacena AdS/CFT paper is almost 25 years old and has nearly 18,000 citations. Trying to exploit ideas based on AdS/CFT has been the main goal of thousands of the best theorists in the world for decades. Questions like "what about getting this to work in the more physical case of dS?" are not new but very old and have been the subject of tens of thousands of person-years of unsuccessful effort. This doesn't mean it can't be done, I think it does mean that what's needed are some quite different ideas, there's little point in further banging away at ones that haven't worked for this long, after this much effort.
- Some of the hype surrounding AdS/CFT has been outrageous. One example is the claim that AdS/CFT gives a good way to calculate things about heavy-ion physics. This is just not true, and the people saying things like this should know better. Seeing people do things like this make me question their arguments about whether other ideas work or not (or have good prospects for working).
- As time goes on, people start using "AdS/CFT" to mean a wider and wider array of things. It often now denotes very general and vague conjectures about duality relations between gauge theory and gravity systems, or holography, or entanglement. It becomes impossible at some point to have a coherent discussion about the subject since there is no well-defined thing to talk about.

Sticking to the specific meaning of a duality between a specific superstring theory on AdS_5 x S^5 and N=4 super Yang-Mills on the conformal boundary, back in 1997 there were two reasons to get excited about this:

- Reading the duality as telling you about gauge theory in terms of string theory, you could hope that the duality could be extended to non-supersymmetric Yang-Mills, providing the long-sought string dual to QCD, allowing reliable strong-coupling QCD calculations. After a few years post 1997, it started to become clear this wasn't working, and why. N=4 SYM has zero beta-function and is conformally invariant, so the effective coupling and physics are the same at all scales. QCD has a running coupling constant, with weak-coupling and strong-coupling physics very different. AdS/CFT allows strong-coupling calculations on the gauge theory side using weakly-coupled strings, but this has the same problems that we've always had with QCD: there are ways to write down strong-coupling expansions, but no way to match those to weak coupling physics, no way to capture the way physics changes from strong to weak coupling.

  By the way, I noticed the Simons Foundation has just announced a collaboration to study QCD and strings

  > Foundation Announces the Simons Collaboration on Confinement and QCD Strings

  and this doesn't even mention AdS/CFT. People have tried really, really hard over decades to use AdS/CFT to say something about QCD, with very limited results.

- Reading the duality as telling you about strings and quantum gravity in terms of gauge theory, the hope is to understand quantum gravity this way. There are a bunch of problems with this:

  There's the gravity in AdS rather than dS problem that you mention. As noted above, back in 1997 it was reasonable to expect a useful extension to dS. After 25 years of failed efforts, if there is such a thing it has to be something very different.

  You want gravity in 4d, not 5d. This may not be a serious issue since you can take 5d with one small dimension, or brane or whatever to get rid of a dimension.

  There's a fundamental problem with doing gravity this way: string theory-based quantum gravity uses weakly coupled strings with the graviton a weakly-coupled mode. But this theory is dual to strongly-coupled gauge theory where you can't calculate anything. So, AdS/CFT is telling you nothing about the usual picture of how gravity arises from string theory. What it supposedly tells you about are strongly coupled strings (using weakly coupled gauge theory), but then the connection to gravity is something very different than what was originally advertised for string theory.

  Put together, the problem is that, to the extent AdS/CFT is telling you something about strings and quantum gravity, it's telling you about the wrong kind of space-time (AdS) in the wrong dimension (5) with the wrong kind of strings (strongly-coupled). The general philosophy seems to be that at least it's telling us about some kind of quantum gravity, which is a reasonable motivation, but leaves one far from real physics, in the land of general issues like resolving the black hole information paradox. But 20 years ago we were being told that it was resolved by AdS/CFT, then ten years later we were being told it wasn't (the "Firewall"). Again, given the level of hype people operate with, it's hard to evaluate any of this kind of thing with no relation to anything measurable.

I'm all in favor of good toy models, and from what I can see the main activity in AdS/CFT these days is trying to understand lower dimensional toy models. This leads to lots of interesting things to study, but you seem to end up with very complicated things happening even in much lower dimensions (0+1 SYK models, 1+1 JT gravity models), far from the 3+1 dimensions one wants. There are no physical gravity degrees of freedom below 3+1 dimensions, so it's all too possible that what one is studying in these lower dimensional models is exactly the things of no physical relevance to the real problem.

Finally, my feeling has always been that the difficulty of measuring purely quantum gravitational effects means that the only convincing quantum gravity will be one unified with the rest of physics, fitting together well with what we can observe. The danger with studying pure quantum gravity is that you'll end up with not one theory, but an infinite number of them, empty of any predictive value. The landscape is a realization of that danger.

Update: By the way, this is roughly the 25th anniversary of AdS/CFT, Scientific American has a piece by Anil Ananthaswamy.

Comments

1. **Thomas**
   November 30, 2022

   The very first prediction of a property of the QGP based on AdS/CFT correspondence, $\eta/s=1/(4\pi)\simeq 0.08$, remains very close to the most accurate determinations from RHIC data. For example, Bass et al. (https://inspirehep.net/literature/1763462) find $\eta/s=(0.085\pm 0.025)$ at the critical temperature. Obviously, this prediction ignores all the differences between the two theories, such as finite N_c, finite coupling, different matter content. I'm happy to admit that it must be somewhat of a coincidence — but a remarkable coincidence it is.

2. **Joseph**
   December 1, 2022

It seems to me that quite a few of your arguments boil down to "There hasn't been progress on X in 25+ years"

Using algebraic geometry as an analogy the motivic picture has developed tremendously since the 1960's but the fundamental pieces of the program rest on existence theorems for algebraic cycles which no one knows how to prove. These are so out of reach that I wouldn't be surprised if we saw no progress on even the weakest existence theorems this century.

Yet, this is a crucial issue and vast swathes of mathematics from the Langlands program to transcendental number theory are waiting to be revolutionized.

Going back to physics, imagine if in 2080 physicists finally manage to quantize string theories with Ramond-Ramond fields thus enabling computations beyond the supergravity limit in AdS/CFT.

Maybe in 2200 the QCD string is found and in 2500 M-theory is finally understood. The point being I don't think the amount of time there hasn't been progress means anything. So what if it takes a 1000 years?

3. **Peter Woit**
   December 1, 2022

   Joseph,
   The problem with AdS/CFT, as a way to do calculations in QCD, or as a theory of quantum gravity, is not that there has been no positive progress, but that all the work on it over 25 years has just clarified the underlying reasons why it doesn't work. In the comments posted here I was trying to explain those underlying reasons. The problem is not positive progress being so slow that we have to wait til 2080, but that the derivative is the wrong sign: progress is towards proving that there is no way anything like this can work, that you need to find something quite different.

   In the math analogy, it's as if almost every implication of the motivic picture didn't work correctly, giving completely incorrect relations between cohomology groups, and that those working on this keep saying "we still believe in the picture, we just need to keep calculating and maybe someday someone will come up with the right idea that fixes everything not working". This situation would be a strong argument for abandoning the motivic picture. Some people might want to continue to work on it, but such work should be in the context of admitting the original ideas didn't work, not continuing to hype them uncritically.

4. **Anonymous**
   December 6, 2022

   " What it supposedly tells you about are strongly coupled strings (using weakly coupled gauge theory)"

   Incorrect.

   Gauge theories at large N are dual to weakly coupled strings.
   Weakly coupled gauge theories are dual to tensionless weakly coupled strings.
   Indeed, strongly coupled gauge theories at large N are dual to 'standard' gravity, but such gauge theories are under much better control analytically/numerically when compared to gravity. Also, we 'know' their completion to finite N and any coupling using the same techniques.

5. **Peter Woit**
   December 6, 2022

   Anonymous,
   You're taking the quote out of context, also out of the context of what this email was about. The point I was making is that AdS/CFT is completely irrelevant in the usual picture of how string theory is sold as the answer to quantum gravity: gravity as the effective space-time low energy theory due to the graviton mode of a weakly coupled string. In this picture you can compute how the strings behave using perturbation theory, AdS/CFT is irrelevant.

This Week's Hype

November 30, 2022
Categories: This Week's Hype, Wormhole Publicity Stunts

This morning Quanta Magazine informs us that Physicists Create a Wormhole Using a Quantum Computer, promoting the article on Twitter with BREAKING: Physicists have built a wormhole and successfully sent information from one end to the other and Physicists have used Google's quantum computer to send a signal through a wormhole, a shortcut in space-time first theorized by Einstein and Rosen in 1935.

This work is getting the full-press promotional package: no preprint on the arXiv (unless I'm missing something?), embargoed info to journalists, with reveal at a press conference, a cover story in Nature, accompanied by a barrage of press releases (see here, here, here, with Harvard, MIT and Google to come). This is the kind of PR effort for a physics result I've only seen before for things like the Higgs and LIGO gravitational wave discoveries (OK, and the primordial gravitational wave non-discovery). It would be appropriate I suppose if someone actually had built a wormhole in a lab and teleported information through it, as advertised.

An additional part of the package is the Quanta coverage, with a very long article by Natalie Wolchover and an over-the-top seventeen minute film How Physicists Created a Wormhole in a Quantum Computer, with abstract

> Almost a century ago, Albert Einstein realized that the equations of general relativity could produce wormholes. But it would take a number of theoretical leaps and a "crazy" team of experimentalists to build one on Google's quantum computer.

The two senior physicists behind this, Joe Lykken and Maria Spiropulu, have histories that go way back of successfully promoting to the press nonsense about exotic space-time structures appearing in experiments that have nothing to do with them. Back in 1999, the New York Times published Physicists Finally Find a Way to Test Superstring theory, which featured Joe Lykken. In 2003, they featured Maria Spiropulu explaining how she was going to find extra dimensions (or "something just as 'crazy'") at the Tevatron, or failing that, the LHC.

I just saw that the New York Times also has a big story about this: Physicists Create 'the Smallest, Crummiest Wormhole You Can Imagine'. At least this article has some sensible skeptical quotes, including:

> "The most important thing I'd want New York Times readers to understand is this," Scott Aaronson, a quantum computing expert at the University of Texas in Austin, wrote in an email. "If this experiment has brought a wormhole into actual physical existence, then a strong case could be made that you, too, bring a wormhole into actual physical existence every time you sketch one with pen and paper."

An odd thing about the Quanta article is that it contains a couple quotes from me, that aren't at all about the wormhole business. They're about the attempt to use AdS/CFT to either solve QCD or get a viable theory of quantum gravity. Back in June Wolchover contacted me with some questions about AdS/CFT. It seems that she was planning a long piece on AdS/CFT, one which somehow many months later got amalgamated with the wormhole nonsense. I had forgotten that I was thinking of turning what I sent her back then into a blog posting but never got around to it, so just earlier today posted it here.

On the substance of what is really going on here, it's exactly the same as what was discussed extensively a month ago in this posting and in its comment section. The claim that "Physicists Create a Wormhole" is just complete bullshit, with the huge campaign to mislead the public about this a disgrace, highly unhelpful for the credibility of physics research in particular and science in general.

Update: Here's the promotional piece from Google, and Will Kinney's reaction.

Update: Physics World has Quantum teleportation opens a 'wormhole in spacetime' with a quote from Witten saying positive things about this experiment ("a 'milestone' in developing control over microscopic quantum systems"), nothing about the wormholes.

Update: I tried reading the paper in some more detail. Almost all the calculations in the paper were done on paper or on a classical computer. As far as I can tell, all they did was perform elaborate SYK calculations on a classical computer, together with simulations of noise on the Google quantum computer, trying to find a possible calculation on the quantum computer that would have signal, not just noise. Once such an N=7 SYK calculation was identified, they used a 9 qubit quantum computer and the noisy result matched the simulation result from the classical computer, exactly as expected. Seeing the completely expected match between results from a 9 bit noisy quantum computer and the results of the simulation of this on a classical computer caused Maria Spiropulu to say that "I was shaken" and "It was nuts. It was nuts", while Joe Lykken felt that the moment was on a par with discovery of the Higgs particle.

I hadn't noticed that the Nature issue comes with an article by Brown and Susskind, A holographic wormhole traversed in a quantum computer. Amidst the hype, they do at least point out:

> because nine qubits can be easily simulated on a classical computer, the results of this experiment cannot teach us anything that could not be learnt from a classical computation, and will not teach us anything new about quantum gravity.

New Scientist is the sober one here, with their headline the relatively reasonable A quantum computer has simulated a wormhole for the first time

Update: MSN is going for the larger context: physicists didn't just create a wormhole in a lab, also This tiny 2D wormhole could finally solve the biggest problem in physics

Update: Andreas Karch on Twitter I think has an accurate characterization of this "mostly a publicity stunt":

> Experimentally it's of course cool they can do SYK – as a demonstration they have control over their device. They can couple 9 qbits in a pre-specified way. But I guess we knew they could do this before. Going after SYK in particular, in my mind, is mostly a publicity stunt.

Update: Quanta has changed the title of their article from "Physicists Create a Wormhole" to "Physicists Create a Holographic Wormhole".

The MIT press release is out, and it's comical in the other direction, explaining the huge breakthrough as MIT researchers use quantum computing to observe entanglement.

Chad Orzel is getting flashbacks to 2006, which I can well understand. Many of the worst offenders in this hype campaign were hard at work doing the same thing back then (and earlier), and I was, as now, ineffectually trying to do something about it (the first edition of "This Week's Hype" dates back to that year).

Update: Quanta has also deleted the original "BREAKING: Physicists have built a wormhole and successfully sent information from one end to the other" tweet. Davide Castelvecchi at Nature as a more sober story, ending with

> The theory tested at the Google lab "only has a very tangential relationship to any possible theories of quantum gravity in our Universe", says Peter Shor, a mathematician at the Massachusetts Institute of Technology in Cambridge.

Update: More coverage of this here, here, here and here. Quanta and Wolchover are, quite appropriately, blaming the "some of the best-respected physicists in the world" who sold them this nonsense, see here, here and here.

Comments

1. **Blake Stacey**
   November 30, 2022

   The original arguments in the "ER = EPR" paper would imply that a wormhole exists between two correlated toy bits in the Spekkens toy model, which would be farcical.

   The very thing that makes AdS spacetime a "good box to put gravity in" — a boundary that a geodesic can reach in finite proper time — is what makes it unlike the universe we live in. You can have toroidal black holes in AdS! "De Sitter space looks like anti-de Sitter locally" also ignores stability. Like, the answer to the question "if I do this, will my lab implode into a black hole" is different between the two.

   I could buy that the research sold under the "it from qubit" slogan may yield an interesting and useful geometric description of entanglement in some multipartite quantum states. That's a far cry from a physical theory of quantum gravity, however. And I say this as a quantum information specialist who'd love to see my own specialization unlock the mysteries of the cosmos!

2. **Adam Treat**
   November 30, 2022

   Good on Scott Aaronson actually pushing back on this incredible hype. Let's see how many other responsible QC researchers and quantum gravity researchers do the same. Peter, do you think the actual researchers are the drivers of this hype or is it more likely the institutions (Google, CalTech, etc) + Natalie Wolchover are the drivers and the actual researchers are complicit by not pushing back on the over-the-top hype?

3. **Somdatta**
   November 30, 2022

   Can't say much until I have read the paper which I hope appears tomorrow. But the wormhole they are talking about is of course a fictitious one, the question is whether the experiment that they do has any tell-tale signs of the wormhole, which apparently they do, and that is all that matters. AdS/CFT tells you that a gravity description is sometimes more useful in discerning what is going on than the underlying strongly coupled quantum system, hobbled too much by strength of the coupling, to be amenable to analysis. The SYK system is of course one where the two descriptions are demonstrably in agreement, and yet there could be features that are easily explained by the gravity description than the quantum one, and that is what's in contention, I guess. However through this, no wormhole will ever open up in ordinary spacetime, if that's what people are pondering over.

4. **Peter Woit**
   November 30, 2022

   Adam Treat,
   I don't think the journalists are blameless, this is not Quanta's finest hour. Overbye at the New York Times did a much better job (although I'd argue it would have been better to not cover this at all, or as a story not about physics, but about an out-of-control hype problem).
   The main people responsible though are the researchers involved. In the case of several of them, if physicists could be brought up on malpractice charges, they'd be in trouble.

   Somdatta,
   The paper is up on the Nature site, released same time as the hype campaign. Funny thing is I suspect you're talking about when it will be on the arXiv. New rule, don't take anything in Nature seriously, until it's on the arXiv…

5. **Somdatta Bhattacharya**
   November 30, 2022

   Peter,
   I was aware of the Nature paper, but I don't have access. The New Scientist article is good. Thanks.

6. **Peter Woit**
   November 30, 2022

   Somdatta,
   Thanks for the explanation. It does seem that publishing in a commercial venue like Nature and not putting on the arXiv makes your paper inaccessible to a lot of its scientific audience.

7. **Somdatta Bhattacharya**
   November 30, 2022

   Peter,
   You're welcome.

8. **Regan E howard**
   November 30, 2022

   I was hoping for something "real" but it's just a sim. And doing the sim on a quantum computer made it a publicity stunt. The video was a bit flashy in the sales sense. Note that tech companies included Google are under some financial strain now and the accountants are looking at chopping non-revenue producing entities. I'm not sure who is playing whom.

9. **Low Math, Meekly Interacting**
   November 30, 2022

   Depressing.

   Blogs are great, but where else do I go for science journalism now?

10. **Bobak H**
    November 30, 2022

Peter, what do you think about this surrogate model they found. As someone with a very limited background in this kind of physics, it's hard for me to understand whether their checks are sufficient: to wit "winding size", various properties of mutual information between qubits, and some properties between the correlation functions of the state (which I guess in this context are amplitudes for the state to return to itself at time t?).

Thanks for your post!

11. **Alessandro Strumia**
December 1, 2022

Peter writes: «if physicists could be brought up on malpractice charges». This is excessive. Just pay their analogous wormhole work with analogous Monopoly money.

12. **John Baez**
December 1, 2022

Unfortunately Natalie Wolchover didn't seem to realize that this hype was merely hype: on Twitter she wrote: "Curious to see so many people approve of the article but bristle at the headline. Can someone (who has actually read our article or the Nature paper) articulate *why* they don't think it's valid to say the experiment created a wormhole?"

Various people who know what's going on tried to explain it carefully and gently. But I think one has to be brutally clear in situations like this, so I replied:

They did an simulation that crudely, roughly mimicked a system dual to a 2d wormhole. So it's like a kid scrawls a picture of an inside-out building and the headline blares

BREAKING NEWS: CHILD BUILDS TAJ MAHAL!

13. **Mike**
December 1, 2022

I came to this blog (after not visiting for a while) expecting I would find the story surrounding this article featured in a "This Week's Hype" post and Peter did not disappoint me.

As a condensed matter/ultracold atom theorist who works on non-equilibrium dynamics of strongly correlated systems, who has been following the activity on SYK from a bit of a distance, and saw the emergence of the first theory papers talking about wormholes in SYK appear and being reported in workshops and seminars a couple of years ago, I have only one thing to say about this whole business:

Yuck! And the way the media has been dealing with work is appalling and makes me really ashamed of being in the "same" community as the authors…

14. **Mateus Araújo**
December 1, 2022

Clearly Wolchover was fed some garbage by the scientists involved and believed it uncritically. She writes that simulating stuff with quantum computers is fundamentally different than simulating stuff with classical computers, because on quantum computers "the system evolves according to the actual laws of nature".

I guess then that the classical computer I'm writing this comment with doesn't run on the actual laws of nature. Maybe it's a cellular automaton? Or an abstract Turing machine, coming straight from Platonic heaven? Whereas the rest of the world runs on regular physics?

Now, if you believe this nonsense, it makes sense to get excited by the simulation on a quantum computer and not by the much easier and better simulations one can do on classical computers.

15. **Mateus Araújo**
December 1, 2022

Or, to be a bit more charitable, she believes that a simulation with an analogical computer is "more real" than a simulation with a digital, programmable computer. She just missed the fact that Sycamore is not an analogical computer, but in fact a digital, programmable computer.

Which, incidentally, is why people are interested in Sycamore in the first place!

16. **Theorist at UC Irvine**
December 1, 2022

Peter:

Even if the headline isn't strictly accurate (a topic for another time, although I think you're splitting hairs here), what's the harm? It's a cool-sounding result which gets people interested in theoretical physics, science more generally. As long as science journalists are driving interest and engagement, I think they're doing a good job. If you want to discuss bad science journalism, surely a better use of your time would be all the anti-science fake news coming from the populist right in the U.S.

17. **Marshall Eubanks**
December 1, 2022

Today, Quanta did a 10 tweet thread explaining and (to some degree) walking back yesterday's hype:

To avoid further confusion with wormhole in the sci-fi sense of a passageway that a person can fly through, we have changed our headline to read "Physicists Create a Holographic Wormhole Using a Quantum Computer."

https://twitter.com/QuantaMagazine/status/1598353637515853827

18. **A reader**
December 1, 2022

"She writes that simulating stuff with quantum computers is fundamentally different than simulating stuff with classical computers, because on quantum computers "the system evolves according to the actual laws of nature"

I think what she meant is simulating stuff with classical computers you are solving the Schrodinger equations whereas with quantum computers you are actually performing a quantum experiment.

19. **Nicola Maiorino**
December 2, 2022

@John Baez

I also like Sabine Hossenfelder's blunt response to Natalie Wolchover's Twitter query "Curious to see so many people approve of the article but bristle at the headline. Can someone (who has actually read our article or the Nature paper) articulate *why* they don't think it's valid to say the experiment created a wormhole?".

Sabine wrote the following on Twitter in an attempt to clarify the difference in understanding (in the lay-person's mind) between the hype in the article's headline, and the nature of the experiment that was actually performed:

"To say the hopefully obvious, when the average person reads "wormhole" they don't think "the [holographic] dual of an AdS2 wormhole", because they know neither what "dual" means, nor what "AdS2" is."

20. **Peter**
December 2, 2022

Am I the only one to notice that publicized "breakthroughs" etc. in physics are becoming more and more esoteric?

On one side, there's a quantum mechanical system doing its expected quantum mechanical things. One the other side, there's a description, based on toy models and all kinds of speculative assumptions. It suggests there may be a relation between the quantum mechanical system and gravity, if gravity would be something that can be described by these toy models and assumptions – which remains to be seen, but actually is very hard to see at the moment.

Is that a breakthrough? It smacks of desperation, to be honest.

Very striking is the Witten quote you mention. I think it's called "damning with faint praise" in English. It's a level above "hmmm, nice, thanks" but not by much.

21. **sdf**
December 2, 2022

If there were a "Hall of fame" for this sort of thing, then surely this would be at the top? I can't think of a more egregious example of over-egging a paper.

22. **Mateus Araújo**
December 2, 2022

A reader: Ah indeed, that's probably what she had in mind. Still it's a weird mysticism about quantum computers. It doesn't really mesh with how much the computation is abstracted from the physical subtract. The correspondence is on the logical level, not physical.

23. **Alessandro Strumia**
December 2, 2022

I found a 2017 newspaper announcing that the first wormhole for time travel was built in Napoli using graphene. This raises an issue of scientific priority. Please add an "update" to recognise that wormholes, like pizza, have been invented in Napoli, and we will return your integrity by offering a 50% discount on Trevi Fountain.

https://www.ansa.it/canale_scienza_tecnica/notizie/fisica_matematica/2017/03/18/costruito-il-prototipo-di-uno-wormhole-per-i-viaggi-nel-tempo_9e619d68-b6c8-4333-9dac-9dc270ebe847.html

24. **martibal**
December 2, 2022

Trevi Fountain is free 😉 Nice try. The great breakthrough would be wormhole with ananas.

25. **Peter Woit**
December 2, 2022

Theorist at UC Irvine,
I think your attitude explains one source of the whole problem and the behavior of people like Lykken, Spiropulu and Jafferis. They likely are thinking "as long as this gets people excited about our field and our research, that's good for them (and good for us). So, doesn't matter if what we're saying is false or misleading."

Besides the minor matter of scientists being supposed to be concerned about what is true and what is false, engaging in outrageous behavior like this discredits the whole field. In this case they may have gone so far as to destroy their own credibility with their colleagues. Joe Lykken may have thought that appearing on a video claiming this result was as important as the Higgs discovery was a good way to get attention, but it also means a lot of people are no longer going to take seriously anything he says.

26. **Attendee**
December 2, 2022

Let me second Peter's response to "Theorist at UC Irvine". The purpose of science and science journalism is to either communicate what is true or suitably describe controversy. Neither of these attributes are true about these articles – the "science" here is total bullshit .

The average person who reads this sort of garbage and then learns it is garbage does not get "excited about science". Rather they (correctly) develop skepticism about scientists as a group and this becomes deeply problematic for both society and science.

I used to respect a lot of people who are involved in this hype. Shame on them.

27. **Low Math, Meekly Interacting**
December 2, 2022

A very diplomatic reality check on Ars…

https://arstechnica.com/science/2022/12/no-physicists-didnt-make-a-real-wormhole-what-they-did-was-still-pretty-cool/

Diplomatic to a fault, I'd say, but helpful nonetheless.

28. **Peter Woit**

December 2, 2022

LMMI,
Not claiming to have created a wormhole is good, the problems with the claims about this work go far beyond that, and articles like this one are still heavily hype-laden. The fact of the matter is that all that was done here was running a simple quantum-mechanical calculation on a nine qubit machine, with results that exactly matched what was expected. Beyond being a check that the google machine works as expected, this has no actual relation to quantum gravity, wormholes or any such. The endless verbiage about the relation of a bunch of coupled qubits to quantum gravity is still nothing but hype.

29. **Matt**
December 2, 2022

I thought hard about an analogy to tell your non-physicist friends what a „dual wormhole" is, so here it is: an angel is dual to a fishing rod, because in German the meaning of the word „angel" is fishing rod. So the analogous newsflash might have read as follows:

„Breaking: Scientists finally managed to study an angel in the lab.

Physicists from Caltech partnered with their friends from Guugle's fishing store to finally study the mysterious creature widely known as angel in their lab for the very first time. Indeed, linguists recently found out that an angel is dual to a fishing rod and thus they are basically the same thing, because the German word „angel" means „fishing rod".

Guugle provided their newest fishing gear, but some essential parts were missing, unfortunately. So instead of a fishing rod they could only provide a fishing line. However, Caltech's physicists immediately realized that the essential part of a fishing rod, and thus of an angel, is the fishing line. By studying the tension of the fishing line, they found unambiguous signals indicative of an angel. It was nuts."

Maybe that helps to contextualize the achievement of these very special scientists.

Best, Matt

30. **James A. Given**
December 2, 2022

A pity! When I saw you were quoted I said, "Great! Even Peter Woit is on board!"
Finally, they'll have to give Susskind his Nobel Prize!

31. **Low Math, Meekly Interacting**
December 2, 2022

I took Ouellette's article to be pretty clear about the tenuousness of the connection to quantum gravity, and it states explicitly "the atoms behaved exactly how one would predict they would using traditional 1920s-era quantum mechanics". A commensurately diplomatic way to characterize some of her judgements about how "exciting" or "interesting" this "new dual way to speak about certain specific systems" might be to note these are subjective terms.

But you're probably right, and I'm grasping at straws. I'm a big fan of good science journalism, and Ouellette and Wolchover have produced some of the finest, in my opinion. I'd like to think Quanta and those involved got played here, but that doesn't seem to be the case. So, again, depressing.

32. **Martin S.**
December 2, 2022

Your currently last update has twice link to [Matt Strassler's comment]().
I guess that the second case is for the [Twitter thread](), or its [6/10 part]().

PS This comment is only meant as a "typo" notice, not as a real comment.

33. **William Orrick**
December 3, 2022

In your latest update the first link in the second set of links doesn't seem to point where you intended. (It's identical to the last link in the first set of links, and doesn't contain any remarks by Natalie Wolchover.)

34. **Peter Woit**
December 3, 2022

Link in update fixed.

35. **Anonyrat**
December 4, 2022

Wolchover has pinned her tweet about her story. I.e., she is not backing down from her part in the hype.

https://twitter.com/nattyover/status/1597984919904718848?s=21

Igor Krichever 1950-2022

December 1, 2022
Categories: Obituaries

I just heard the sad news that Igor Krichever passed away this morning at the age of 72. Igor was a great scholar, a wise man, and a wonderful human being. He will be sorely missed by his colleagues at Columbia and elsewhere. My condolences to his family, which includes another first-rate mathematician, his son-in-law Sasha Braverman. During the past year Igor had been suffering from a progressive neuro-degenerative disease. Fortunately he was still in good enough health to fully participate in and enjoy his 70th birthday conference, which took place at Columbia in early October.

In recent years Igor had been spending only one semester each year at Columbia, much of the rest of the time was in Moscow, where he was director of Skoltech's Center for Advanced Studies. He came to Columbia in the mid-90s, with his hiring the beginning of a period of successful expansion and improvement in the math department. He was a gentle and friendly person, and it was always a pleasure to have a chance to talk to him about one topic or another. When he became chair of the department I remember thinking that it seemed unlikely that someone as scholarly and laid-back as him, with a somewhat typical Russian mathematician's other-worldliness, could deal well with the challenges of the university bureaucracy. I was very, very wrong, as it became clear that he was extremely wise in the ways of the world and a great department chair. I guess that after growing up with Soviet bureaucracy, dealing with the Columbia version was child's play.

Igor was a very distinguished mathematician, one of the leading figures working at the intersection of integrable systems and algebraic geometry. For more about his scientific work, there's a biographical notice written by some of his colleagues at the time of his 60th birthday (which was also celebrated at Columbia with a conference, see here).

Comments

1. **Yao Liu**
   December 1, 2022

   Sad and unexpected news. He gave an ICM talk this year too, and his lectures on integrable systems (and notes) will be valuable resource for students years to come.

2. **Peter Woit**
   December 2, 2022

   Igor's recent ICM talk is available here
   https://www.youtube.com/watch?v=LliT-96R1mQ

The Wormhole Publicity Stunt

December 3, 2022
Categories: This Week's Hype, Wormhole Publicity Stunts

The best way to understand the "physicists create wormholes in the lab" nonsense of the past few days is as a publicity stunt (I should credit Andreas Karch for the idea to describe things this way), one that went too far. If the organizers of the stunt had stuck to "physicists study quantum gravity in the lab" they likely would have gotten away with it, i.e. not gotten any significant pushback.

There have already been a lot of claims about "quantum gravity in the lab" made in recent years, and surely many more will be made in the future. It's important to understand that these all have been and always will be nothing but publicity stunts. In all cases, what is happening in these labs is some manipulation and observation of electron and electromagnetic fields at low energies. None of this has anything to do with gravitational degrees of freedom. One cannot possibly learn anything about the gravitational field or quantum gravity this way. If there is a dual theoretical description of QED in terms of a "gravitational" theory, this dual description is about other variables that have nothing to do with space-time and gravity in this world.

I'm hoping that journalists and scientists will learn something from this fiasco and not get taken in again anytime soon. It would be very helpful if both Nature and Quanta did an internal investigation of how this happened and reported the results to the public. Who were the organizers of the stunt and how did they pull it off? Already we're hearing from Quanta that the problem was that they trusted "leading quantum gravity researchers", and presumably Nature would make the same argument. Who were these "leading quantum gravity researchers"? Why weren't any of the many other physicists who could have told them this was a stunt consulted?

It's pretty clear that one of the organizers was Joe Lykken. After I wrote about his talk at CERN a month ago, someone told me that Dennis Overbye at the NYT was looking into writing about Lykken's claims. I found it odd that the NYT would be interested in this, now it's clear that the behind-the-scenes publicity campaign was starting already a month ago. If you look at Lykken's slides, there's no mention at all of the work he had done and knew was about to appear in Nature, but the whole talk is structured around arguing that such a quantum computer calculation would be a huge achievement. I still don't know what to make of his claims in the Quanta video that the result of the Google quantum computer calculation was on a par with the Higgs discovery. Does he really believe this (he's completely delusional) or not (he's intentionally dishonest)?

It's extremely unusual to not distribute a result like this on the arXiv before publication, to instead keep it confidential and go to the press with embargoed information. By doing this though you control the first wave of publicity, since you pick the press people you deal with and the terms of the embargo. One thing that first mystified me about this story is why Natalie Wolchover at Quanta was quoting comments from me on a different issue in her story, but hadn't asked me about the article and its "physicists create wormholes in a lab" claims. One possible explanation for this is that the terms of the embargo meant she could not discuss the Nature article with me. I have to admit that if I had heard from her or any other journalist that a group was about to hold a press conference and announce publication in a major journal of claims about quantum gravity in a lab, and would I respect embargo terms so they could share info with me and get a quote, I would have said no. Likely I (and others in a similar situation) would immediately have gone and written a blog entry about how a publicity stunt was about to happen.

Update: I just noticed that the "It from Qubit" community will be gathering Monday thru Wednesday in Princeton and Thursday thru Friday in New York. One of the Princeton talks will be from one of the Nature authors (Jafferis), talking about "Emergent Gravitational Dynamics in Quantum Experiments" (no abstract, may or may not be explaining how he created the wormholes). This would be an excellent occasion I think for this community to discuss what can be done to stop publicity stunts like this one from discrediting their subject. The New York component will be invitation only, at the Simons Foundation. Presumably the Quanta people will be there to discuss with them the huge damage to their reputation they just suffered because of the publicity stunt. I'd be curious to hear how this goes from anyone participating.

Update: Something I should have linked to before is Scott Aaronson's blog posting about this, and the comments there. One that I think is of interest explains that SYK at large N is not precisely dual to a 2d gravity theory as one often sees claimed, and has other useful explanations of issues with duality claims.

Update: According to Spiropulu on Twitter, at 2:15 Eastern Time today you can watch Jafferis talk about this stuff to a workshop at Princeton here.

Update: Just watched the livestream of the Jafferis talk. He went over in detail the paper. At the end, a few technical questions. At this point I'm seeing no evidence that anyone (other than Scott Aaronson) in this community has any problem with the outrageous hype and publicity stunts like this one being used to promote their field to the public and attract more funding.

Update: Something from Ethan Siegel, who as usual, gets it right:

> There are no lessons to be learned about quantum gravity here. There are no lessons to be learned about traversable wormholes or whether they exist within our Universe. There are not even any lessons to be learned about the uniqueness or capabilities of quantum computers, as everything that was done on the quantum computer can be done and had previously (without errors!) been done on a classical computer. The best that one can take away is that the researchers, after performing elaborate calculations of the Sachdev-Ye-Kitaev model through classical means, were able to perform an analogous calculation on a quantum computer that actually returned signal, not simply quantum noise…
>
> Wormholes and quantum computers will likely both remain topics that are incredibly interesting to physicists, and further research into the Sachdev-Ye-Kitaev model will likely continue. But the connection between wormholes and quantum computers is virtually non-existent, and this research — despite the hype — changes absolutely nothing about that fact.

Update: To get a taste of the utter nonsense people are now getting as "News" because of this publicity stunt, try watching this ABC News segment.

Comments

1. **Jay Smith**
   December 3, 2022

   This whole thing amounts to a heck of a lot of egg in the face of the world's physics community, in general, the HEP one, in particular. And at a time when sectors of western societies are already dismissive of science and scientists openly. This is bad – very bad.

2. **Ted Rogers**
   December 3, 2022

   It is somewhat encouraging that there has been so much pushback. Maybe there is finally starting to be a shift away from the "what's the harm?" attitude toward excessive hype?

3. **Peter Shor**
   December 3, 2022

It seems to me that the string theorists and "it from qubit" community seem to have this unwritten rule that they don't criticize other members of this community in public. For example, when you criticized "ER=EPR" on this blog, Urs Schreiber responded on Twitter that it was not to be taken seriously, but it was just a Zen koan. And Geoff Penington said on Twitter that "no one has ever believed that". This despite the fact that Susskind continued to give lectures where he presented ER=EPR, as described in the original paper, as an actual physical phenomenon.

This unwritten rule goes a long way towards explaining how the journalists got misled.

4. **Peter Woit**
December 3, 2022

Ted Rogers,

We'll see. Given past experience with decades of string theory hype, I'm not so optimistic that the physicists doing this kind of thing will stop doing it, or that the journalists covering the field will stop getting taken in.

5. **Peter Woit**
December 3, 2022

Peter Shor,
This kind of tribal behavior has been a big feature of the string theory community, sorry to see evidence that it is a feature of this newer community.

One aspect of this story I don't understand is why no one waved Natalie Wolchover away from writing about this in the way she did. I would have thought just about any of the "It from Qubit" people (putting aside Susskind…) would have recognized that her article and the video were highly problematic and advised her to tone them down. The only explanation I can think of is that the embargo terms kept her from getting the kind of advice she needed.

Just noticed that the Simons-funded It from Qubit group will be meeting all next week in Princeton and at the Simons Foundation headquarters. Maybe some of this will be sufficiently private that people will be willing to criticize what happened, maybe even consider doing something about it. I'm also curious to know if participants will hear from people like David Spergel and Thomas Lin, who should be concerned about the reputational hit this kind of publicity stunt may cause the Simons Foundation and Quanta.

6. **Peter**
December 3, 2022

I've got a degree in physics in the early 1980s but I've had an uncommon career and worked 10 yrs. in "the media". With that background, I guess I should feel some sympathy for Nathalie Wolchover, but I don't.
As a reporter, you know people try to use (and abuse) you. You know you're a pawn in a bigger game. If you're a halfway decent reporter, you know what you're used for. And if you don't know, you find out.
Guarding your integrity in those circumstances isn't always easy. But writing "It's debatable whether the experiment furthers the hypothesis that the space-time we inhabit is also holographic, patterned by quantum bits" simply isn't good enough; and the way she used your quotes without you knowing the context in which they would be published, is below par, to put it mildly.

7. **Dimitris Papadimitriou**
December 3, 2022

They just "trusted leading QG researchers"?! That's really hard to believe..
One thing that baffles me is that the whole attempt seems so naive, almost childish. The fanfare and the pomposity of the video, especially, was laughable (or cringe worthy , perhaps- it depends on one's point of view…).
This kind of hype could not last for more than a few days (at best!), so what was the intention of the people who prepared/ organized this?
It seems almost logically inexplicable. As if they wanted any kind of publicity, even negative…
The situation with science communication is getting worse. I don't believe that this will change for the better anytime soon.

8. **Peter Woit**
December 3, 2022

Peter,
The problem for journalists covering this sort of theoretical physics has always been that it is ferociously technical and obscure. Experts have enough trouble trying to figure out themselves what is going on, it's hopeless for journalists. The standard method journalists have for dealing with this is to recognize who is a credentialed expert, call up a bunch of them, then try to evaluate what they say. If you've got one expert who tells you wormholes are being produced in a Google quantum computer, and a bunch who say that's not right, you will stay out of trouble. If the majority of the experts you call up tell you that wormholes are being produced in a Google quantum computer, the natural thing to do is to get very excited about how amazing that is and work hard to bring it to the public.

9. **Peter Woit**
December 3, 2022

Dimitris Papadimitriou,
Looking at that video, I suspect that all involved did believe what they were saying, which means that they're in the grip of a delusion. Human beings are prone to delusion, especially if those around them are enthusiasts of the delusion. The idea that you're going to "study quantum gravity in a lab" is a delusion shared by an unfortunate number of people. How to shake the deluded out of their delusion, especially when all the incentives are on the side of sticking to the delusion, is a difficult problem.

10. **Pierre**
December 3, 2022

Peter,

I thought you'd be interested to know that the "wormhole created in a quantum computer" story is now being covered in some far-right-wing media. I won't name them here (they're very far-right sites, not sure if you'd allow a link here), but they're essentially saying "isn't this manifestly stupid? See? Why should we believe scientists when they publish bullshit like this?" and essentially use the story to argue that scientists and science journalists are all a bunch of idiots, hence why should we trust them on vaccines/climate change etc.

This is another consequence of bad publicity stunts like this: it erodes trust in scientists.

11. **Peter Woit**

December 3, 2022

Pierre,
Thanks, interesting to hear about that, although I'd just as well not drive traffic their way. The problem though is very general. Unless they're unusually gullible, most people, left, right and center, are going to react to the "physicists create wormholes" headline by wondering if there's something wrong with physicists and this may also extend to other scientists. I keep wondering when other scientific fields are going to start demanding defunding of theoretical physics because of the reputational damage they're seeing from headlines like this.

12. **Alex**
December 3, 2022

"I'm hoping that journalists and scientists will learn something from this fiasco and not get taken in again anytime soon."

Considering how systematic this has become, and how the same people seems to be always somehow involved, I really believe that they actually *want and look forward* to be "taken in" again. Which is just a diplomatic way of saying they are either delusional or intellectually dishonest. In the case of N.Wolchover, this is not the first article I have seen by her which is enthusiastic about highly controversial claims coming from the same group of ex-string theorists behind these ideas. She usually mentions the obvious caveats, but only at the end of her articles and after the strongly enthusiastic first half. Let's not forget she has an actual bachelor degree in physics and went to graduate school in physics for some time. So, she's not that naive and I don't buy that "we just asked the experts" lame excuse. I recall reading somewhere that one of her favorite topics in physics was string theory and that the people she most respected worked in that topic. So, my take is that she quite understands a lot of what she writes about and likes it, she's on the wagon, as simple as that. She believes in these ideas, I don't think she's intellectually dishonest, in the sense that I think she probably thinks the caveats are solvable problems (which is a common view in that research community). Regarding the other people involved, I would say there's a bit of both of the mentioned things. About Susskind in particular, I really don't know at this point. But it baffles me how well respected he is.

My take is that nobody of the involved will learn anything about this because they knew exactly what they were doing. And they will keep doing it again and again. If anything, the fiasco will make them to get better at it.

This simply *has* to go on, for a simple reason: the absence of any actual new results and ideas in fundamental physics. People simply have nothing else to talk about, so the discussions quickly derail to the surreal. The same happens in politics.

13. **Ex YouTuber**
December 3, 2022

So from what I understand, no wormhole has been created in the experiment. But does the experiment really prove the ER=EPR conjecture as stated in the video? Also, scrolling down the comments below the video (which has up to now 339k views), I didn't see a single warning comment from the viewers that no wormhole was created. Why didn't any real expert post a comment in the comments section of that video to inform people about the "fiasco"? Another video on YouTube where the same people held a Zoom meeting is available!

14. **Peter Woit**
December 3, 2022

Ex YouTuber,
As far as I know, all Quanta has done is change "wormhole" to "holographic wormhole" in the title of the Wolchover article and in a tweet. They've left the bizarre and delusional video as is.

15. **Douglas Natelson**
December 3, 2022

The discourse on Twitter about the press coverage has been morbidly fascinating. If anything, this whole affair feels like the BICEP2 business, complete with massive media coverage and pre-made breathless promotional video. I don't think there's anything wrong with putting out a press release when publishing work that you think is important, but waging a well-choreographed media campaign feels wrong to me, even if you've done something amazing.

Have the "it from qubit" people actually written a clear technical claim, and if so, could someone point me to it? I've asked this before. It just seems absurd on it's face to think that, e.g., creating two entangled photons through parametric down conversion by a nonlinear optical element is truly creating a Planck-scale wormhole in actual spacetime.

16. **Matt**
December 4, 2022

Regarding your question who the organizers were, my guess is that Hartmut Neven could have been one of them as well. Neven is heading Google‘s quantum AI division and has been managing their quantum computing endeavour since John Martinis brought his superconducting qbit expertise to Google in 2014. Neven also appears in the video, so at least he wasn‘t opposed to this publicity stunt.

Google undoubtedly has deep pockets, but I always suspected that they look at quantum computing as a means to an end, rather than a means without end. Don‘t get me wrong, huge kudos to them for pushing superconducting qbits as far as they did, but in the big picture they basically have nothing to show for. After almost 10 years, they still seem to be lightyears away from any commercially viable application.

Looking at the situation, I‘d suspect that the QC guys at Google slowly but desperatly need something to show for. So any publicity that convinces Google to keep paying the bills appears to be good for them.

17. **Klaus**
December 4, 2022

Peter,

one of the cited "It from Qubit" abstracts contains the sentence: "We show that a very old black hole can tunnel to a white hole/firewall by emitting a large baby universe."

Clearly, the Quanta/Nature wormhole claims are negligible compared to this one. Imagine what a quantum computer can do if you simulate very old black holes…

I do not know how much alcohol is needed to submit such a statement to a physics conference. No wonder that scientists are seen as untrustworthy. It seems that the authors confuse doing physics research with writing scripts for the next Hollywood movie.

18. **martibal**
December 4, 2022

@Peter: about defunding theoretical physics, isn't it that this kind of "wormhole by google" actually is a good way to get some funding by non national-entities like breakthrough prize , or whatever foundation of some "visionary" billionaires that are looking for breakthrough every day? What would happen if someone, say Musk, decides that this sounds a pretty cool stuff and invest a few hundred millions dollars on this subject ? For him this would be a joke, but in theoretical physics world, this would be a great deal of money and allow this kind of bs to go on ever and ever..
.

19. **Dimitris Papadimitriou**
December 4, 2022

Ex YouTuber
There were some "warning" comments about that video (I tried myself) but they were few and sparse.
And, of course, they were buried under tons of "wow" comments (many of them demanded even the …Nobel prize for the "breakthrough").
That's what is usually happening in such cases, as we all know very well.
Trying to do a sensible or carefully critical comment that is relevant to the content of such videos is almost a pointless exercise.
Only a few people will notice…

20. **Sabine Hossenfelder**
December 4, 2022

"Why weren't any of the many other physicists who could have told them this was a stunt consulted?"

I do not buy for a second that the people who wrote these articles didn't know other physicists think this is bullshit. They knew it, they just thought they'd — as usually — get away with it.

As you undoubtedly know, I have been very outspoken about this, it's one of the examples of quantum hype in my last year's video, and I also commented on Overbye's wormhole piece from two months ago here.

Granted, maybe those journalists don't watch my videos but the vast majority of them are on twitter. After all, it's their JOB to have an eye on these things. And a few years ago I wrote an article for (oh the irony) Quanta Magazine that one shouldn't confuse mathematical black holes with real ones. It was also the last piece I wrote for them.

The bottomline is that they had all the information to properly judge the new paper, they just thought that most physicists, as usual, would keep their mouth shut when confronted with bullshit headlines because they have this idea that any kind of attention is somehow good attention.

I am extremely grateful for the outcry over this headline because I hope it'll send a message that science news readers wand to read about real science, and that that, in return, will have an impact on research in the field.

21. **Sabine Hossenfelder**
December 4, 2022

Another comment on your somewhat broad-sweeping statement:

"There have already been a lot of claims about "quantum gravity in the lab" made in recent years, and surely many more will be made in the future. It's important to understand that these all have been and always will be nothing but publicity stunts."

I am afraid I have to disagree because there are some experiments that quite plausibly have a chance to see weak field effects of quantum gravity in the laboratory. I understand that that's not what you had in mind — you were referring to analogies and simulations for which your statement is entirely correct — but let's not throw out the baby with the bathwater.

I wrote about this possibility a few years ago here, and for all I can tell the experimental groups have since slow but steadily made progress.

22. **Frank**
December 4, 2022

One more curious thing. In HEP author names are usually alphabetical. When they are not, the order should indicate who contributed the most (first author) to who contributed the least (last author). Spiropulu is the last listed author, but the piece by Wolchover describes the group as being led by Spiropulu. So which is it?

23. **Peter Woit**
December 4, 2022

Sabine,
Thanks, your caveat is right, there could be lab results actually relevant to quantum gravity. I should have specified that what I'm referring to are all such claims involving a supposed duality, with the gravity stuff taking place on the other side of the duality from the physics being studied.

24. **Peter Woit**
December 4, 2022

Frank,
Thanks, I hadn't noticed that, and the ordering is interesting The first two authors are Jafferis and Zlokapa, which I guess means they are the ones who did most of the work, presumably Jafferis the general theory, Zlokapa (an MIT student) actual calculational work. Putting Spiropulu at the end, while Wolchover calls her the leader of the group suggests that what she led was the PR effort.

25. **Peter Woit**
December 4, 2022

martibal,
You're right, any potential NSF/DOE defunding could be more than made up by billionaires who want to go through the traversable wormholes.

The money from Jim Simons though flows through the Simons Foundation, and I'm curious what its director (David Spergel) thinks of all this.

26. **Peter Woit**
December 4, 2022

Klaus,

The people doing this refer to supposedly analogous structures in toy models using the same name as used in a physical theory. Part of the problem here is that after spending too many years in a toy model, you can forget that it's not the real thing. Journalists then often seem to think that there's no reason to make clear to the public the difference (i.e.use a misleading headline, bury the fact that it's a toy model deep in the article).

27. **tulpoeid**
December 4, 2022

Frank and Peter,

No; it's among the common practices to put the PI at the end of the authors list. I've worked for years at a lab where this was the practice, and I know it's elsewhere as well.

28. **Peter Woit**
December 4, 2022

tulpoeid,
Thanks. Looks like grant involved is this one
https://pamspublic.science.energy.gov/WebPAMSExternal/Interface/Common/ViewPublicAbstract.aspx?rv=b31f0e55-de01-4635-91d1-2bf112daea20&rtc=24&PRoleId=10
Spiropulu PI, Jafferis co-PI

29. **Peter Shor**
December 4, 2022

@Sabine:

You may be in the position of the boy who cried wolf. You have criticized so many things in physics, some of which many physicists perceive as actually valid (like dark matter), that the Quanta Magazine people may not be paying attention to you anymore.

And while I realize that your criticisms of dark matter have a lot more caveats in them than your criticism of calling simulations "wormholes," this may not be apparent to journalists.

30. **Peter**
December 4, 2022

Peter W.,

"The problem for journalists covering this sort of theoretical physics has always been that it is ferociously technical and obscure."

You're very mild for Nathalie Wolchover. As I understand it, she was given news under embargo. That alone should have made alarm bells ring. It's a trick providers of information often use to give undue weight to something. Every halfway decent reporter knows that. So the first question you ask yourself when given the embargoed news is: "Why is it embargoed? Interesting, relevant news doesn't need embargos."

The second question a halfway decent reporter asks is: "Why me?" An embargo puts you in a difficult position. You can't talk to a credentialed expert and try to evaluate what they say, because you can't tell them what your questions actually are about – it's embargoed. So you ask them a few general questions. The experts give equally general and usually polite answers, which invariably sound quite meek when confronted with the forcefully made and detailed statements by the provider of the embargoed information.

I could go on forever, but I won't. Quanta made a fool of itself here.

31. **martibal**
December 4, 2022

Simon's foundation has an excellent reputation among mathematicians, right ?
Are they related to the hype, or are they just hosting a conference on a related topic ?

32. **math**
December 4, 2022

Hi
I am not sure I understand everybody's criticism here.
The publication's authors have attempted to address a small aspect of a vast question in fundamental physics and published their result in a well reputed journal (vetted through a peer review process, AFAIK).
Are you criticizing their scientific result or the aggressive PR operation they have orchestrated?
About the latter, everyone is doing PR's these days. People need to «sell» their stuff (to attract funding, apparently…). The science is still valid (or else please explain why the published result is wrong and should not have been published).

33. **Peter Woit**
December 4, 2022

martibal,
The Simons Foundation is funding this sort of research with a large grant, see
https://www.simonsfoundation.org/mathematics-physical-sciences/it-from-qubit/
the conferences this week are part of what is funded by that grant.

It looks though like the PIs of the Simons grant and the authors of the publicity stunt are a disjoint set.

34. **Peter Woit**
December 4, 2022

math,
The scientific result I assume is correct, but it is not of any substantial significance and should not have been published in Nature. The authors ran a computation on a nine-qubit quantum computer and got the expected result, showing nothing more than that the Google machine works as expected. The publicity stunt aspect of what they did goes way beyond anything I've seen theorists do before.

35. **martibal**
December 5, 2022

math: since when does the refereeing process means embargo ? Basically all maths results are freely available on arXiv much before they get published in a journal. The paradigmatic case is Perelman's proof of Poincaré conjecture. The same is true, maybe less systematically, for high energy physics.
One may understand that the publication of the Higgs discovery is under embargo (given that there are two collaborations that work on the subject and this is really a huge result). But none of these is true in the quanta case.

"People need to «sell» their stuff (to attract funding, apparently…). "
That would be hearable (also discutable) if there were actually "stuff" to sell. Selling science as the last season of whatever Sci Fi series ruins the credibility of the field. One really does not need that at the moment.

36. **George**
December 5, 2022

Just by looking at the comments under the Youtube video, I can tell for certain that the public has been completely misled about the actual content of this work. And that will, in the long term, damage the trust between public and scientists, I am afraid.

37. **clayton**
December 5, 2022

in experimental groups (and also in astro), it's very common to put the PI at the end — eg the final author of https://www.nature.com/articles/s41586-019-1666-5 is Martinis, who was the head of the group and did all the press for it. The breakdown is very roughly "actual major contributors to the entire paper first; people who worked on a section or two, or who did direct mentoring of people in the first group, come second; people with private data, instrumental knowledge, etc., that we couldn't have done without, but who didn't do lots on the paper itself, third; people who got the grant money and oversaw stuff last"

38. **anon**
December 5, 2022

PI as the last author is common in experimental physics (as well as chemistry and life sciences, as far as I know), but I've never seen it happen in astrophysics. Maybe it happens in instrumentation groups, which are closely related to experimental physics.

39. **Steven Bratman**
December 5, 2022

I engaged in some twitter back and forth with Thomas Linn on the subject. His stonewalling and defensiveness was depressing. I offered as constructive criticism that if an expert is mentioned in an article, and that expert feels the article is utter nonsense, it would be appropriate to mention this. He insisted that the article already did so by including the words,"said Woit, a critic of AdS/CFT research." As you've explained, you gave those quotes in an unrelated context, which might excuse Quanta if they were unaware of your opinions. But he also told me that he knows all about your thoughts on the subject, and told me I was "partisan" because I found your opinion on the subject credible. Sigh.

40. **clayton**
December 5, 2022

in astro it differs very much between theory and then what you might call interdepartmental experiment (which is most of them) and single-department experiment (where it can be idiosyncratic). In the first case there wouldn't really be a PI as such, and in the second case you'd be in the "thermodynamic limit" of PIs (where there's so many that they all become alphabetical again). I had in mind the final one, which indeed would often be either a small experiment or the instrumentation paper for a larger experiment but centered in one particular group.

Regardless, it seems that this particular piece is following the PI-last standard inherited from experimental groups.

41. **Peter Woit**
December 5, 2022

Steven Bratman,
I saw that Twitter exchange. In general it's really depressing that both Lin and Wolchover seem to not understand the problems with what they have done. I did go back and reread more carefully the Wolchover article. The main quote from me is completely out of context and misleading (it quotes a 6 month old email from me about AdS/dS, which has nothing at all to do with this story, in a context set out as being about the experiment.)

42. **Laurence Lurio**
December 5, 2022

The hype in the press about this was so outrageous that I went and looked at the actual article in nature. There is no hint of this crazy hype in either the blurb about the article in Nature, or in the abstract to the article itself. All quite sober and no hint of "creating" a wormhole. The blame for this sits squarely on the press and not on the journal.

43. **Somdatta Bhattacharya**
December 5, 2022

You guys are indulging in ad hominem. What exactly is wrong with what Jafferis had to say today?

44. **Peter Woit**
December 5, 2022

Laurence Lurio/Somdatta Bhattacharya,

Nature isn't completely blameless, they devoted their front cover to a picture of a "Holographic" wormhole and a claim that this work was "a step towards investigating quantum gravity using quantum computers", although the Brown-Susskind summary of the research inside the issue says "the results of this experiment cannot teach us anything that could not be learnt from a classical computation, and will not teach us anything new about quantum gravity."

As for the Jafferis talk, nothing wrong with it, but it would have been an excellent opportunity for at least one person in the audience to confront him about why he had participated in the publicity stunt based upon this work.

45. **Adam Treat**

December 5, 2022

One reason you might not see pushback on Jafferis talk is that the blame for the hype is being placed on the experimentalists. At least that is what Matt Strassler has to say on his blog. He says that he's known Jafferis for over a decade and considers him a young genius and that he's not responsible for the hype, but rather the experimentalists are… I take his comments to mean that he blames Spiropulu and Google for launching the media campaign for which young Jafferis was unknowingly being used. See [here.]()

46. **Peter Woit**
December 5, 2022

Adam Treat,

I've learned a lot more about how this was done today. One thing I learned is that Spiropulu has been planning this for about four years (i.e. trying to find some kind of calculation that can be done on a quantum computer that could be sold to the public as "first quantum gravity in a lab" or "physicists create a black hole/wormhole in the lab"). Doubtless the Google people were happy to sign on and make their machine available for the stunt. From what I can tell, Joe Lykken (a theorist, not an experimentalist), was also part of the planning.

If you take a look at the video, Jafferis doesn't play a really big part, but he's filmed together with the whole group of them, played along, and cannot have been unaware of what the stunt was that Spiropulu was trying to pull off, and his own role in it.

47. **David Roberts**
December 6, 2022

Young researcher getting a chance at a Nature paper isn't going to complain too hard if the senior collaborators are drumming up some publicity.

48. **Somdatta Bhattacharya**
December 6, 2022

@peter,
Why focus obsessively on the publicity stunt and not on what they are actually doing. Also no one confronted him because it wasn't the appropriate forum. BTW, the bigger story is unfolding in Matt's blog today ICYMI.

49. **Ex YouTuber**
December 6, 2022

This YouTube video shows that all of them are accomplices; that the blame should not be put on Spiropulu alone:
https://youtu.be/PlZroYSYq8k

50. **tulpoeid**
December 6, 2022

Peter,

If your comments are used out of context, you should demand that they get deleted from the article and that the edit is mentioned explicitly. At the very least, the editor has the obligation of mentioning your ask for retraction next to your comments if they are kept in the article.

In other hype, not anything as spectacular, but Maryland theorist uses the landscape as solid base to tell the public that there are no laws of physics.
https://link.newscientist.com/view/62f685a7fc1bdc567268bdc8hrq9c.exx/0177a14a

51. **Peter Woit**
December 6, 2022

David Roberts,
Jafferis is a tenured full professor at Harvard, I think he's at a point in his career where he doesn't need to go along with this kind of thing if he doesn't want to.

52. **Peter Woit**
December 6, 2022

tulpoeid,
I've had a discussion with the editor at Quanta. My point of view on their article is that it is not fixable, with far more serious problems than my quote. I'd like to convince them that there's a problem and they need to do better in the future: there will be a lot more people coming to them with "quantum gravity in a lab" stunts. My arguments about this are having limited effect since I'm not a researcher in this field. I urge researchers in this field to contact Quanta with any complaints about the article. Jafferis would be an example of someone who has responsibility to do something about the situation, but he's not the only one…

53. **Peter Woit**
December 6, 2022

Somdatta Bhattacharya,
I watched the entire Jafferis talk, the reason for not concentrating on discussing their calculation in a sparsified N=7 SYK model is that it's not interesting. The only significance of the calculation is as a publicity stunt which Jafferis has significant responsibility for. Given this, I'd argue it was inappropriate for people to spend the question time on technical questions, they should have been asking him about the larger significance of what he had done, which was the stunt.

Matt Strassler I think agrees this is a publicity stunt, he's trying to use it as an opportunity to discuss a completely different topic close to his interests.

54. **David Roberts**
December 6, 2022

@Peter W

Oh, I was taking the phrase "young Jafferis" at face value. I guess "young" is relative. I really should have checked, though… :-S

On a different note, I'm sure every single illustration attached to this story is using a cartoon of a wormhole with too many dimensions. Instead of the problem of not being able to accurately picture 3+1d spacetime and an ER bridge in it, here the problem is that accurately picturing the appropriate and accurate 1+1d would uncover the hype for what it is.

55. **Santo D'Agostino**
December 12, 2022

Martin Bauer explains the physics of the experiment here for lay people. As usual for him, the explanation is clear and insightful. Tl;dr: No wormholes were created.

https://twitter.com/martinmbauer/status/1602240035478183936

56. **Peter Woit**
December 12, 2022

Santo D'Agostino,
From what I've seen, post the publicity stunt, the coverage of this story by physicists has been uniformly on the negative, no wormholes side. Matt Strassler for instance has "an international spectacle about a quasi-simulation of a cartoon of a wormhole" as his description. Since the stunt, I've yet to see anything, anywhere from one of the authors or anyone else defending what they've done. It just doesn't seem to matter that the physics community overwhelmingly is critical of this: journalists at Quanta and others taken in won't admit now that they were taken in, likely never will.

57. **Evgeni Ku**
December 25, 2022

What would the point of a publicity stunt be here? Opportunity for future grants for the scientists involved?

It seemed fishy to me even as a lay person with minimal understanding of the matter beyond pop-sci books, which is how I got to this blog post. I can't understand the reason for making such an elaborate hoax though.

One thing I could imagine is this being heavily pushed by Google as a form of marketing for their quantum computer work, though it feels like that should also be doable with far more reasonable and non-contraversial experiments.

58. **Peter Woit**
December 25, 2022

Evgeni,
For the Google people involved, this was pretty explicitly a publicity stunt designed to promote their Sycamore quantum computer. If you look at what was actually being done, it was a calculation for which their machine was dramatically worse than any conventional computer.
For the physicists involved, this was a stunt designed to justify current funding and attract future funding. Some of the people involved have been at this for 25 years, back then concentrating on misleading people about "experimental tests of string theory". This is just more of the same, with "string theory" evolving into "holographic quantum gravity".
What's remarkable is how successful this was, with the con taking in not just the press but the directors of the IAS and Fermilab. There will clearly be an ongoing effort to pursue this, we're going to see a lot more bogus "quantum gravity in the lab" claims, unclear if those at the IAS and Fermilab who have been conned will wise up.

59. **Frank**
December 25, 2022

Why in the world do you think the directors of Fermilab and the IAS were not in on the con? They benefit from increased funding for "quantum gravity in the lab" and had access to experts who could explain the significance of what was done. Fermilab in particular has severe cost overrun problems and needs to fight for continued funding.

Publicity Stunt Fallout

December 6, 2022
Categories: This Week's Hype, Wormhole Publicity Stunts

Latest news this evening from Scott Aaronson at the IAS in Princeton:

> Tonight, David Nirenberg, Director of the IAS and a medieval historian, gave an after-dinner speech to our workshop, centered around how auspicious it was that the workshop was being held a mere week after the momentous announcement that a wormhole had been created on a microchip (!!)—in a feat that experts were calling the first-ever laboratory investigation of quantum gravity, and a new frontier for experimental physics itself. Nirenberg speculated that, a century from today, people might look back on the wormhole achievement as today we look back on Eddington's 1919 eclipse observations providing the evidence for general relativity.
>
> I confess: this was the first time I felt visceral anger, rather than mere bemusement, over this wormhole affair. Before, I had implicitly assumed: no one was actually hoodwinked by this. No one really, literally believed that this little 9-qubit simulation opened up a wormhole, or helped prove the holographic nature of the real universe, or anything like that. I was wrong.

Scott has been the one person in this field I'm aware of who has tried to do something about the out-of-control hype problem that has been going from bad to worse. I do disagree with him about one thing. He goes on to write:

> I don't blame the It from Qubit community—most of which, I can report, was grinding its teeth and turning red in the face right alongside me. I don't even blame most of the authors of the wormhole paper, such as Daniel Jafferis, who gave a perfectly sober, reasonable, technical talk at the workshop…

I do blame all those people. Unlike Scott, they've been either participating in hype for years, or staying quiet and enjoying the benefits of it. Grinding their teeth and turning red in the face is not enough. They need to finally say something and take action.

Update: Still unclear to me what the ultimate fallout of the publicity stunt will be. One thing that is becoming clear is that the publicity stunt is part of a vigorous and very effective campaign to mislead funding agencies and those making funding decisions. The goal is to convince them that "quantum gravity in a lab" is a real thing and the way forward for fundamental theoretical physics. The bogus Quanta story, video, headlines are not a bug, but a feature. Among the funding agencies, DOE is on board, their grant funding the publicity stunt, and they are advertising it prominently in their presentation to HEPAP today (see page 7 here). At the IAS, it seems director Nirenberg's claims that the "first quantum gravity experiment on a chip" was possibly the biggest breakthrough in a century were not off-the-cuff comments based on what he had read in the paper or at Quanta, but prepared remarks based on conversations with IAS senior faculty. I gather that what he has been hearing from at least some of them is that the wormhole "experiment" vindicates their past research and justifies supporting them in the direction they are choosing for the future. He has though now been hearing other viewpoints.

If researchers in this field want to know what they can do about the problem, contacting places like Quanta to get them to fix their coverage is one thing, another is contacting people with funding responsibilities (ie. program officers at funding agencies, directors of institutes) who seem to have been misled by the hype campaign.

Update: Quanta hasn't done anything more to fix the wormhole article, but they have now updated their original "Physicists have built a wormhole and successfully sent information from one end to the other." tweet. New one reads "Experimental physicists built the mathematical analog of a wormhole inside a quantum computer by simulating a system of entangled particles."

Update: Today at HEPAP the Fermilab director was prominently advertising the wormhole publicity stunt as a Fermilab initiative (see slide 67 here). She describes future plans for more of the same, with these calculations being performed not on one quantum computer, but on two spatially separated quantum computers connected by a quantum network. This would somehow allow for a big increase in the "quantum gravity in the lab" hype with a new Nature cover story: "FNAL scientists connect two quantum computers by a wormhole between two labs". A question for those in the "It from Qubit" field. Are you willing to contact those responsible for funding this, who are now prominently advertising this work as a major success and new direction of research they intend to fund (e.g. Glen Crawford at DOE, and Lia Merminga at FNAL)?

Update: 4gravitons has a blogpost about the Quanta article, concentrating on the issue of "tone" of the coverage. That's relevant for the usual problematic sort of physics coverage, but in this case something much more unusual is going on. This was a well-organized publicity stunt designed to justify funding "quantum gravity in a lab" research. Quanta was taken in more so than many other journalism venues. But the really disturbing part of this story is who else was taken in: the IAS director, the Fermilab director, the DOE division director and others, who are sophisticated consumers of science journalism, and independently getting their information from senior scientists in the field.

Comments

1. **Sabine Hossenfelder**
   December 7, 2022

   The worst are the people who've been brainwashed by the "experts" but don't themselves understand enough of the topic to actually have an own opinion. Like Lockyer going on about naturalness and why that means bigger colliders will find something (see here). I actually wrote to him asking what he was going on about in this interview just to make sure. In explanation he sent me a link to a blogpost by Matt Strassler, not kidding.

   Scott is asking what went wrong and what can be done about it. I know I sound like a broken record, but here we go again. Large communities of likeminded people are affected by social reinforcement, aka, group think. This, I cannot stress often enough, is a well documented phenomenon and, no, intelligence does not protect from it. The more you hear other people talk about X, the more you think X is important, and the more you yourself will talk about it. In the end, everyone thinks X is great and important because everyone else thinks that, not because it's actually important.

   (I actually suspect the problem is worse among intelligent people because they believe they're too intelligent to be affected, but that's just a conjecture on my part.)

   It's easy enough to address the problem: Give every scientist a basic education on the sociology and philosophy of science, and social and cognitive biases. I believe that if they think about it, they should realize they don't actually want to waste their time on research that will turn out to be entirely irrelevant. Though there is a case to be made that it makes money and popularity in the short run, I don't think it's what most of them are driven by.

   I have a longer list with recommendations for what can be done by scientists, higher-ed admin, policy makers, and members of the public in the appendix of my 2018 book which is available here.

2. **Pascal**
   December 7, 2022

   What went wrong: it is probably not a coincidence that this is happening in fundamental physics, where actual progress has been rather slow for several decades (according to some of the blogs I read).

What can be done about it: change the system so that people do not have to overhype their results in order to find a job / keep it / get funding. A tall order, perhaps.

3. **David Roberts**
December 7, 2022

Wow. Just wow. I really, really want to ask the journalists involved if they can hear this, and then think they communicated the result properly. This is not Jo Bloggs who gets science news fourth-hand on Facebook. This is someone who should be able to go and ask the experts down the hall to explain at length over coffee.

4. **vmarko**
December 7, 2022

Next time I read a Quanta article, whatever the topic, the first thing I'll put in my comment is "And did they also create a wormhole in the lab?"

I'll stop when I see a meme on the net along the lines "Chuck Norris creates a wormhole…". 🙂

5. **martibal**
December 7, 2022

After dinner speech… maybe D. Nirenberg were just a little drunk 😉
or is there something rotten in the kingdom of Princeton ?

6. **Mark Hillery**
December 7, 2022

Yesterday in my intermediate mechanics class a student pulled out a copy of the latest Nature issue and asked about the wormhole article. He had some very good questions, which indicated that he was pretty suspicious of what he was reading. I was able to confirm his suspicions and explain that most of the claims in the article were nonsense, and that Nature had been hoodwinked.

Toward the end of my time as an editor at Physical Review A (15 years was quite enough, thank you), we started getting papers where all the authors had done was run something on a quantum computer. Sometimes this was even phrased as, "We did an experiment." Real experiments are hard work, and running something on a cloud quantum computer does not count. Claiming you created a wormhole is worse. Most of these papers were rejected out of hand. If the paper contained something new that was run on a quantum computer as an illustration, that was different. On that basis, and, for the moment, ignoring the hype, the "wormhole" paper would probably be publishable in Physical Review D (this is a guess, I know nothing about quantum gravity, and I don't want to speak for the PRD editors). The hype, however, is appalling, and it is clearly causing a lot of damage, which will only get worse when people realize how vacuous the claims are.

7. **NoGo**
December 7, 2022

Oh yeah, a lot of people were taken in by this publicity stunt.

In the comment section under the Ars article I pointed out the reaction of specialists, including Peter Woit and Scott Aaronson, to the stunt — with quotations and links. (I definitely did not try to pose as an expert myself)

I've got angry responses, attacking me and the experts, some saying that these reactions were "dumb" etc.

Out of curiosity I've checked the comment history of the people responding in this way. They seem to be intelligent techies, who made thoughtful and informed contributions to discussions of computer and technology topics, not some trolls.

I don't think that these people are going to be convinced by blogs like this one; at this point only serious pushback by media and (ideally) the scientists involved in that research can help…

8. **Laurence Lurio**
December 7, 2022

I agree, this has gotten out of control. Perhaps it might be appropriate to contact the American Physical Society. Is there any way someone with some clout in the field could draft a letter and then invite cosignatures?

9. **Anonymous Referee**
December 7, 2022

Mark Hillery,

Some of the papers that you did not reject out of hand came to me to referee. I then rejected them. The most ludicrous was one claiming they had done an experiment with indefinite causal orders by implementing a causally ordered quantum circuit in a causally ordered quantum computer.

10. **Peter Woit**
December 7, 2022

Sabine,
I think you're right about the group-think problem, but in this particular case there's something else even more disturbing going on. Most of the people in this field are well-aware the wormhole thing was a publicity stunt, but saying so publicly, or doing anything about it more privately, would carry personal costs they are not willing to bear. The main author of the stunt, Spiropulu, has spent four years working on it, funded by a DOE grant. Getting a "physicists create wormholes" headline in this case (unless it blows up in her face…) carries major professional and financial rewards, which may have a lot more to do with this than group-think.

Scott describes the quantum gravity research community as "grinding its teeth and turning red in the face", but not as doing anything. I gather no one spoke up at the time to publicly tell Nirenberg he had just embarrassed himself and the IAS by being the victim of a publicity stunt. Did anyone explain this to him in private later? If not, could it be not groupthink, but not wanting to take the professional cost of being the one to explain to someone in a powerful position that you and your community have a serious hype problem, highly damaging to his institution, which it is unwilling to do anything about?

11. **Alex**
December 7, 2022

Allow me to seriously doubt about the sincerity of these "teeth grinding" and "red faces" that Scott reports. I mean, this has been happening in the field for years, many of them probably contributed with both nonsensical research and hype themselves, but now suddenly they feel ashamed? I think they are actually feeling shame because of how grotesque and caricaturesque it all resulted at the end. I bet that, in their minds, they were hoping for something much more serious, credible and professionally looking. But, alas, it's just the mirror: sometimes it gives you back an image which ain't pretty… if you have nothing to offer to it, the mirror cannot do magic and put substance where there's none.

12. **Adam Treat**
December 7, 2022

    The entire reason this happened is because the general public – which is illiterate on the subject matter at hand – passionately wants to believe that their science fiction dreams are becoming real. The editors/writers at Quanta, the NYTimes, Reuters, and ABC perfectly understand this is the hook for their audience: report that their science fiction dreams are slowly becoming real and make money by feeding this audience.

    The purveyors of this hype – whether it was the experimentalists or the theorists or both – who actively drove Quanta and these other media outlets to write about it – knew perfectly well the incentives at play. Had they soberly explained the research and not hyped it, then it would not have gotten the worldwide media fame that it has gotten them. Whether there is self-deception here on the part of the scientists I can't say. But the whole field does take a hit when that eager audience for their science dreams come true realizes they were bamboozled.

13. **Peter Woit**
December 7, 2022

    Adam Treat,
I think you're partially blaming the victim here. If physicists actually created wormholes in a lab in Santa Barbara, most people would sensibly be interested in hearing more about this. The problem isn't an illiterate and unsophisticated general public, since the people believing this include the IAS director, a Pulitzer prize-winning science journalist and editors at Nature. Best to put the blame where it belongs: those directly responsible for the stunt, and the larger community of researchers which enabled them.

14. **Geoff Penington**
December 7, 2022

    Scott is someone who enjoys science communication, is good at it, and is willing to devote significant time to it. As a result, he has a blog where he publicly posts his opinions on things like this. Most physicists do not have a blog, do not regularly engage in science communication and so did not publicly comment on the article or hype. If everyone working in the field other than Scott should be blamed for this whole mess, is your claim that any practising physicist has a moral obligation to make a blog? That takes a lot of time, effort and skill as I assume you know.

    As someone in the field, I have been on multiple emails threads in the last week about how embarrassing a failure of science communication this was, and how we need to get better at communicating to journalists to avoid such events in the future. I was also at the dinner with Scott last night and there was loud heckling throughout the talk (think: "I don't know whether this will be as impactful as the eclipse observations in 1919" "I really think we do") to an extent that I have never seen or even imagined was possible in such a setting.

    I certainly don't blame David at all, and clearly as a field we have some collective responsibility to not let stuff like this happen. But all the actual It from Qubit researchers quoted as independent experts (Scott, Dan Harlow etc.) made skeptical comments, and even the authors of the paper who actually work in the field were not the ones saying completely ridiculous stuff. Daniel Jafferis gave a nice and entirely reasonable talk on the paper this Monday. In particular, I'm not really sure what the vast majority of people in the field, who all first learned about the paper when they saw the quanta/NYT articles, were supposed to do to prevent this whole thing.

15. **JT gravity**
December 7, 2022

    "If everyone working in the field other than Scott should be blamed for this whole mess, is your claim that any practising physicist has a moral obligation to make a blog?"

    I believe there exists a serious (very serious) fallacy in this "argument."

16. **Peter Woit**
December 7, 2022

    Geoff Penington,
I've also been having multiple email threads the past few days about this, with people's position either "nothing wrong here" (IAS and Quanta point of view) or "yes, this was bad, I don't see anything though I can do about it". Scott's taking a "this is bad and I'm trying to do something about it" position is an exception.

    My comments blaming all involved in this except Scott should be taken in that context, and in the context of unsuccessfully complaining about disturbing levels of hype in this field (string theory – > AdS/CFT -> "It from Qubit") for decades, watching it getting worse and worse in recent years. This past week it has gone off the charts on the crazy meter.

    I agree that not everyone should be out there making public statements about this. But if people don't want to get blamed for craziness in their field, they should do something. Here's one example: personally I believe Quanta should pull the article and video from their site and re-report this story properly. Thomas Lin (Quanta chief editor) made the argument to me that he would not change anything about their coverage because he was not hearing from quantum gravity experts that there was a problem with it. Why not? Whenever a publication covers your field of expertise and gets it very wrong, why not write to the editor and argue that they should fix it? My impression is that everyone (except Scott…) is saying "someone else should do that, not me, not my responsibility, I don't want the cost in time/energy/making enemies that I'd incur".

    Another comment: I think your problem is not so much being bad at communicating with journalists, but rather not being willing to take on those in your midst who are very good at communicating with journalists, but do so to self-promote and push bad science.

17. **Amitabh Lath**
December 7, 2022

    Peter, no. I do not believe Jafferis and members of his group are to blame, and I do not think you do either. While they had some control over the contents of the Nature article they cannot prevent their co-authors from beclowning themselves on the popular media forums.

    I brought this up with some eminent Rutgers theorists and their consensus is that the questions Jafferis et al are asking about a small group of interacting fermions that mimic "…(some sort of) (TWO DIMENSIONAL) quantum gravity with a wormhole…" is not crackpot stuff. (All caps theirs!)

    Unsaid but implied: calling the implementation of this on a computing device (of any sort) a "crummy wormhole" is crackpot.

    I agree with Scott Aaronson: let's not tar everyone on the author list with the same brush.

18. **Ken**
December 8, 2022

For those who say that the authors are not to blame: Has anyone watched the video clip released by Quanta (https://www.youtube.com/watch?v=uOJCS1W1uzg) in which the authors kept crying "wormhole", "wormhole", "This is a wormhole. I think I just saw one", "I think we have a wormhole, guys", "I never really believed that we'd get all the way to something that was real"…? Should we blame the popular media people for what the authors said in the video clip? Am I missing something?

19. **Attendee**
December 8, 2022

Amitabh, you should ask your theory friends if they can derive Newtonian gravity by studying these small group of interacting fermions.

The fact that theorists feel duty bound to defend this garbage even in private discussions is disturbing.

That is the actual sad part of this story. Maria is a publicity seeker and thus easy to condemn. But, she was encouraged by all of these theory people to do this experiment since they claimed they were going to learn something deep about quantum gravity from it. Sadly, she is not the only experimentalist who has been misguided. There are quite a few groups around the world doing this sort of work (using cold atoms etc.) and there is nothing to be learned from these experiments – the experiments will work as predicted by well understood quantum mechanics and it is then "reinterpreted" in fancy holographic language.

20. **Peter Shor**
December 8, 2022

First, let me say that I hardly understand this stuff at all, so I may easily be missing things.

But I am puzzled by something here. Looking at the *Nature* paper, Eq. 1 is essentially the original SYK model, where you construct a Hamiltonian on Majorana fermions that contains all four-element terms, and you give each one a random Gaussian coefficient. When I first heard about the SYK model, if my memory is correct, this was described as either a zero-dimensional or an ∞-dimensional theory.

With the AdS-CFT correspondence, the dimensionality of the dual should differ from this number (0 or ∞) by 1. But the article clearly says that the dual is a 2-dimensional theory (as do some of the commenters here). What am I missing? Could somebody who knows something about the SYK model enlighten me?

And another question: It takes quite a bit of work to make things behave like Majorana fermions on a quantum computer. How did the authors manage to do this? Or did they just use ordinary distinguishable qubits instead? Would doing this change the dynamics of the system so that it's not actually equivalent to teleporting through a wormhole? (The teleportation protocol itself should work fine whether you're using Majorana fermions or plain distinguishable qubits.)

21. **Peter Woit**
December 8, 2022

Amitabh Lath,
Sorry, but I do think Jafferis in particular has a lot to answer for here. Yes, his part of the paper may be the part that contains some non-trivial science, but he has played a significant role in the publicity stunt aspect of this. He appears in the ludicrous Quanta video, the promotional podcast, and is quoted in the Quanta article as claiming that the stunt shows "a filament of real space-time". If he has any problem with the way he has been used in the publicity stunt, I haven't seen any evidence of it or heard privately anyone mention this.

Perhaps more significantly, the stunt was organized by Spiropulu, who recruited him as co-PI on a DOE grant that has funded them and the stunt. The stunt is now being advertised by DOE as a great accomplishment (this is happening today, at a HEPAP meeting, audience other funding agencies and top people in HEP, see page 7 of this presentation: https://science.osti.gov/-/media/hep/hepap/pdf/202212/Crawford_DOE_HEP_Research_Program_Status_HEPAP_202212.pdf)

So, from what I can tell, Jafferis has been deeply involved in the misleading publicity stunt, has been getting paid to do so, and has shown no signs of having any problem with it. I think it's perfectly fair to blame him.

22. **Leo**
December 8, 2022

Peter Shor, the SYK model has time in it (space is a point) so it is a 0+1 dimensional theory being the boundary theory of a 1+1 dimensional gravitational theory in principle.…

Geoff Penington, I appreciate you commenting on this blog which is much more than what most of your colleagues do. Consider a "hypness index" = [Influence x Exposure]/ [Achievements] , then the hypness index of the "it from quibit" community is 2 orders of magnitud above any other field in physics, specially after this stunt. Also think of the following: If any government agency gives a lot more money after this stunt to your community, will anybody within it come out and ask for moderation? Your colleagues may grind their teeth as Scott says but they will gladly take the extra money that comes from this and forget why they got it. You may say that every community has this hypocrisy problem but by my made up hypness index the most insulting and in urgent need to correct case is the "it from qubit" one.

23. **Peter Shor**
December 8, 2022

I figured out the answer to my first question: the wormhole is 2D because it has one dimension of time and one dimension of space, since the SYK model has zero dimensions of space.

I still don't know the consequences of replacing Majorana fermions with ordinary distinguishable qubits in the SYK model.

24. **JT gravity**
December 8, 2022

Well… it should be well-known that there is no m=0,s=2 particle in the representation theory of Lorentz group SO(1,d) for d=1,2, hence no propagating DoF's in d=2,3, thus it's questionable if it makes sense to speak of gravity theory in low-dimensions; unless one is considering near-extreme solutions of the d=3 theory. But, then, all work on JT gravity relies on heavy Wick rotation, and reading Witten+Stanford paper, https://arxiv.org/abs/1907.03363, you see computations of Z in the JT theory with genus g!=0 in Euclidean signature, so, there's not well-defined Lorentzian structure on their computations. Hence, no graviton DoF's propagating in their calculations, and it's not even clear if one can recover the Lorentzian manifold structure in the end.

But if this seems already bearing on the absurd, the JT/SKY pseudo correspondence is based on a NEARLY AdS2/CFT1 "duality." So, these It from Qubit people are studying an approximation of a toy model of an approximation of a theory which is not even clear whether it corresponds to any gravitational theory at all.

25. **Amitabh Lath**
December 8, 2022

Attendee: I believe you may be misconstruing what my Rutgers theory colleagues said about this work. They remain skeptical about the main claim, but do not claim any expertise on SYK connections to QI, so from what I gather they are driving past this car wreck after giving it a short glance, without slowing down to consider it in too much detail (this is a skill you develop driving in New Jersey).

Peter: I do not know Jafferis and cannot speak to his inner thoughts and opinions. I did not watch the Quanta video but surely we can make a distinction between appearing in a video and saying something (maybe) impolitic and imprecise about "filaments of spacetime", and what Lykken is doing going on about having created actual wormholes. A lot of my colleagues think very highly of Jafferis (he was a PD at the Rutgers NHETC) and as a community we would be better off not excommunicating him and members of his group.

26. **Peter Woit**
December 8, 2022

Amitabh Lath,
I'm not suggesting "excommunicating" Jafferis. He's a tenured full professor at Harvard and will do fine. He has though clearly signed up as co-organizer of the publicity stunt. If it succeeds he'll be rewarded, if it blows up in his face he may suffer some reputational damage, and I'll have trouble feeling too sorry for him about this.

As for the junior people in this collaboration, they're not responsible for the hype/publicity stunt aspects. Some of them will though have tough decisions to make about how to pursue a career and try and find a permanent position. If the "quantum gravity in the lab" hype works and that's where positions are, their best bet would be to go along with that. If it blows up and gets discredited (I'm doing what I can…) they should be looking for different sorts of projects to work on and stay away from this kind of over-hyped activity.

27. **David Roberts**
December 8, 2022

Since there seem to be relevant experts here, what is the (local) topology of the 1+1d spacetime with the SYK wormhole? It's a Lorentzian surface, with boundary I guess, so it should be possible to get a concrete handle on it, no? Pun not intended…

28. **George H.**
December 8, 2022

Just a lurker, x-experimentalist; I would have heard nothing of this if I didn't follow SA's blog. So the general public(GP) knows nothing (IMHO), however the general trust I see in the GP is at an all time low, and this just increases the mistrust. Citation rankings reward fame, so to succeed in science you need fame, which rewards hype, and you all forgive that… because they are just trying to succeed in the fame game. I often think that so many problems are related to having the wrong feedback variable.

29. **James Smith**
December 9, 2022

I have to say that the spectacle of tenured academics calling the general public illiterate and unsophisticated in this instance I find a little ironic.

Various and Sundry

December 8, 2022
Categories: Uncategorized

If you're sick of hearing about bogus wormholes, here are some other random topics:

- There's a SCOAP^3 for books initiative, lots of textbooks in the field of particle physics now made open access and available here.
- It's completely mysterious to me why this is so, but Mochizuki's failed proof of abc is now getting promoted at various places outside Kyoto, including Stony Brook, France (note what comes first in the suggested survey articles) and maybe even Oberwolfach.
- One thing I've never understood about the dark matter story is whether just having right-handed neutrino fields (which fit very well into the pattern of fields of the Standard Model) is enough to get fundamental physics to be consistent with astrophysical observations usually interpreted as evidence of dark matter. Stacy McGaugh despises this kind of idea, where you predict particles with only gravitational interactions, making fun of it here as The Angel Particle. He has a specific agenda (modified gravity), which I think explains the vehemence.

  Ethan Siegel has an extensive discussion of gravity-only dark matter models, under the title Is dark matter's "nightmare scenario" true?

  John Baez has a blog post on Neutrino Dark Matter, based on talking to Neil Turok about this recent paper by Boyle, Finn and Turok.

- The National Academy has appointed an EPP-2024 committee, which has been holding various meetings. Taking a look at some of the videos, I was struck by what Lisa Randall (about 34:00-38:00 and 46:00-48:00) and Nima Arkani-Hamed (about 48:00-53:00) had to say in this one. They both emphasized the problem that it is hard for young theorists to make a career for themselves by trying to come up with a new idea, as opposed to a series of small improvements on ideas currently being pursued by many other people (often things that have been around for a long time). This I think has been a huge problem in the field for a very long time, possibly has gotten worse for some reason in recent years, and is part of the explanation for the moribund state of the subject. While they eloquently state the problem, unfortunately I don't see much from them or anyone else about how to do something concrete to address it.

Comments

1. **Mark Weitzman**
   December 8, 2022

   Thanks for the SCOAP link – what a fantastic resource – you have now expanded my list of unread theoretical physics books by 33%.

2. **Frank**
   December 8, 2022

   Did you notice that none other than Maria Spiropulu is co-chair of the EPP-2024 committee?

3. **Peter Woit**
   December 8, 2022

   Frank,
   Yes. Other fun fact: that video I watched and linked to was recorded on Nov. 30, the day of the wormhole announcement. From what I saw, I thought Spiropulu did an excellent job of chairing the meeting, especially considering that she likely had other things on her mind that day.

   But, I was trying to make this posting wormhole-free…

4. **Dimitris Papadimitriou**
   December 8, 2022

   E. Siegel's " nightmare scenario" article considers only one possibility (the "wimpzillas"). But there are other candidates for gravitation – only interacting dark matter like primordial black holes, or proposals about quasi-stable black hole remnants (assuming that Hawking evaporation stops near the end stage), or Penrose's proposal.
   It goes without saying that most (if not all) of these scenarios are more or less speculative, but the same holds for many other proposals that have to do with this subject (including modified gravity and the like).

5. **Norbert**
   December 8, 2022

   Well, at least at Stony Brook the promotion comes with some caution « Dr. Fumiharu Kato (…) spent some time explaining the approach of professor Shinichi Morizuki to prove it. We want to caution those watching the video that at the time of the lecture there is no universal consensus in the mathematical community that the ABC conjecture is proven. Some details and references can be found in https://en.wikipedia.org/wiki/Abc_conjecture. »
   Best

6. **Marty Tysanner**
   December 9, 2022

   Stacy McGaugh despises this kind of idea, where you predict particles with only gravitational interactions, making fun of it here as The Angel Particle. He has a specific agenda (modified gravity), which I think explains the vehemence.

   In reading various blog posts and a few papers of his, I have found Stacy to be knowledgeable, fair minded and intellectually honest. My understanding is his disagreements with the theoretical cosmology community center around the willingness of many theorists to overlook data that disagree with the standard cosmological model (LCDM), who propose *ad hoc* "solutions" when observations/data disagree with simulations, who introduce unmotivated particles and fields for dark matter when the best motivated candidates (e.g., WIMPs) are ruled out or are no longer plausible, and yet who willingly ignore or denigrate modified gravity proposals that actually predict a wide range of phenomena that LCDM cannot naturally explain or that conflict with dark matter simulations.

   From his writings, Stacy sees dark matter theory as a moving target that can always be fixed by introducing new hypotheses or fine tuning existing parameters whenever conflicts with observations arise, essentially avoiding falsification of dark matter as an explanation of observations. This theoretical "approach" probably looks familiar to you from your own observations of many HEP/BSM theorists.

   What Stacy calls "angel particles" — particles that interact gravitationally but in no other way, effectively rendering them unobservable and non-falsifiable — would be another example of avoiding conflict between observations and dark matter theory without abandoning currently held views.

Stacy seems well aware that the key reason modified gravity is so disagreeable to cosmologists (beyond its conflicts with the approach they have devoted much of their careers to) is that it does not fit nicely with GR. And since GR is a very well tested theory that forms a backbone of fundamental physics, it is really problematic to propose without very strong evidence that GR is wrong in a domain where it is expected to be valid. He freely acknowledges in his blog posts that if one starts with GR as a governing fundamental theory for galactic scale structure and beyond, the need for dark matter is unavoidable. He says he, too, was strongly in the dark matter camp during the first part of his career, but repeated conflicts between observational data and dark matter phenomenology caused him to gradually change his thinking.

So I'm not sure it's really fair to say "He has a specific agenda (modified gravity)."

One thing I've never understood about the dark matter story is whether just having right-handed neutrino fields (which fit very well into the pattern of fields of the Standard Model) is enough to get fundamental physics to be consistent with astrophysical observations usually interpreted as evidence of dark matter.

I'd be surprised if this is an unexplored idea. Why not ask Stacy if he is aware of observations that strongly disfavor or rule out the idea, assuming you have an idea/proposal for the neutrino mass? How would you test the idea observationally so that it is more than metaphysics, especially since we don't currently know if right-handed neutrinos actually exist?

7. **Dimitris Papadimitriou**
December 9, 2022

Marty Tysanner

The same objections you mentioned about DM are also equally valid for modified gravity proposals:
Those alternative theories that incorporate additional fields/ forces are obviously associated with unknown particles that need to be detected for any of these proposals to be tested and confirmed. So we can't avoid the need for detection of unknown particles even in that case…
Another alternative (for those who try to avoid this) is theories with massive graviton(s) that are already on the verge of falsification.
Moreover, the so far proposed MOND -oriented models need also DM as an addition, to explain observations, (I think that most MOND proponents admit that) so they're just complicating things further.

Also, I'm not sure I'm following the reasoning about the "falsifiability" of only gravitationally-interacting DM:
Only detection via any of the three interactions of the standard model of particle physics is " legitimate " for the confirmation of existence of DM?
Gravity does not count as an interaction? What about alternative hypothetical "purely gravitational" theories without additional fields? Are they unfalsifiable too?
Gravitational effects (lensing etc) do not count?

Ok, exactly because gravity is so "weak" there's a need for additional observations, nobody disagrees with that.
But these criticisms seem to me a bit over the top.

8. **clayton**
December 9, 2022

As a dark matter phenomenologist, let me just remind everyone that the _primary reason_ to believe in the phenomenon of cold dark matter is the _very high precision_ with which we measure the CMB power spectrum, especially modes beyond the second acoustic peak. There is a stone-cold, qualitative, crystal clear prediction of CDM about the relative sizes of the second and third peaks that modified gravity _profoundly and irremediably_ gets wrong: it thinks the third peak should be relatively larger than the second (due to the difference between compression and rarefaction — see Wayne Hu's tutorials for pictures), whereas CDM thinks they should be about the same or the third peak should be lower. They are, in fact, at _very high precision_ about the same. All the thousands of other modes confirm this picture (now up to ell of 2000 which hardly any deviations), but this _simple, qualitative fact_ is to me the most convincing way to see that we need a new matter-like field. Any talk I've ever seen by McGaugh (or more exotic modified gravity people like Verlinde) elides this fact, and they evade the questions when I put my hand up to ask. I have invited McGaugh to a conference before specifically to discuss this point, and he just doesn't want to.

In fairness to McGaugh, he pointed out some very interesting features of galactic DM distributions that do deserve answers. But it turns out that there are a plurality of possibilities, from complex DM physics (self interactions) to unmodelable SM physics (stellar feedback, galaxy-galaxy interactions). There are no such alternatives to CDM to explain the CMB power spectrum.

As to Peter's question of whether right-handed neutrinos could be the DM particle — sure, but you still have to explain why they're generated in the quantity we measure from the CMB (this isn't trivial). But yes, this is a credible possibility, the details of which are elaborated every day on the hep-ph arXiv. And interestingly, such models can have similar features to the self-interacting CDM mentioned above.

9. **Peter Woit**
December 9, 2022

Marty Tysanner,
I have followed Stacy McGaugh's blog for a while, much of his critique of dark matter models seems well-argued. This particular argument though seemed to me very unconvincing, partly because the ratio of sneering polemic to serious content was high, partly because right-handed neutrinos seem to be an extremely well-motivated theoretical idea, and particles with only gravitational interactions are not at all unobservable (they may very well be exactly what explains galaxy rotation curves, CMB peaks, etc).

If you want to argue against a massive right-handed neutrino model of dark matter, to me you need a much more serious argument.

clayton,
Thanks for your comments. I've always assumed this was a well-studied topic in the field, just never have had the time or energy to look into this. Two problems I wonder about are, given just the SM with right-handed neutrinos, and what we currently know about mass terms (from Higgs couplings)
1. can you get the right amount of dark matter production in very early universe models?
2. in the later universe, is the way this dark matter evolves consistent with everything you observe?

10. **Stacy McGaugh**
December 9, 2022

Hi. This is an interesting discussion. I think it goes a long way to illustrate how much we take for granted in our own experience that is not shared with others.
My distaste for the angel particle has nothing to do with right-handed neutrinos or any position on modified gravity. It has everything to do with my lived experience with the search for WIMP dark matter, in which the goalposts have been moved time and time again – chiefly to lower interaction cross-section. So, I reasoned (long ago – see this 2008 page: http://astroweb.case.edu/ssm/darkmatter/WIMPexperiments.html), that the obvious culmination of the failure to detect dark matter would be to come up with a particle that was utterly undetectable. This might be true – who could tell? – but it is also the logical (and predicted) extrapolation of a failed hypothesis. I do indeed sneer at the latter.

Right-handed neutrinos are a specific kind of particle that does make a certain kind of sense, as described here. I've nothing against that. There is, however, no guarantee that, should they exist, they have anything to do with the dark matter problem.
I take considerable umbrage at the presumption that everything I say can be attributed to an agenda in favor of modified gravity. That is simply not correct. It isn't my theory.
It may surprise readers to learn that I was an advocate of dark matter before I ever heard of MOND. What changed, for me, was that MOND made predictions that came true in my data. So, I changed my mind: perhaps I had been wrong to be so sure that the answer had to be dark matter and couldn't possibly be modified gravity. This was an excruciatingly painful admission, but isn't that what we're supposed to do as scientists?

11. **Stacy McGaugh**
December 9, 2022

OK, now to answer Clayton. I don't recall ever refusing to discuss the CMB at a conference. I have done so many times. Nor have I done as he says, and "elided" the phenomenology he describes in the CMB. Quite the contrary; I agree with most of what he says above and have said so on many occasions. I have lots else to say, so I can't fit into every talk something I don't have much to say about.
One thing Clayton elides is that I did make a prediction for the CMB peaks, both with and without dark matter, and it was the prediction without dark matter that came true. This has remained true in every data set as the CMB data have improved (see https://tritonstation.com/2020/07/31/second-peak-bang-on/). But it is also true that the ansatz I used to make the correct prediction for the second peak also predicts a smaller third peak. This is not observed, as he says, so maybe it was just a huge coincidence – or maybe that's just how far the ansatz could take us.
The presumption for a long time has been that no modified theory of gravity could get the CMB right. I have a lot of sympathy for that attitude. I have had just as much difficulty trying to get dark matter to explain observed rotation curves. It only does so at the level of "we need more mass." It does not do what MOND does in predicting what we get from what we see.
It is no longer true that it is impossible to explain the CMB and galaxy power spectra with modified gravity. Skordis and Zlosnik have done just that (arXiv:2007.00082). I don't know if that is the right theory, but it is at least a demonstration that what was thought to be impossible can be done. It is simply not correct to say that there are no alternatives to CDM to explain the CMB power spectrum.

12. **Peter Woit**
December 9, 2022

Stacy McGaugh,
Thanks for your comment and apologies for misrepresentation of your position. I had been following some of the same goalpost movement in the context of SUSY models. Over the years one can find endless plots of "SUSY predictions" on plots of dark matter limits, always extending below the current limits, but within reach of the next generation experiment. This kept going over many generations of experiments, and many orders of magnitude improved limits. In recent years I've stopped looking at these things, since I think most people have now given up on this.

On dark matter research in general, it's a huge and confusing topic. I'm aware of the time and effort it would take to master the subject and have well-informed views, and it's time I don't have. I've followed your blog with curiosity, the idea that there's a problem with the conventional wisdom is one I'm not unwilling to believe, given my experience in the rest of HEP theory. At this point not much I can do except to hope that better work and better data lead to a consensus. The reason for bringing this up here was mainly to ask for more information about what to me looks like the most theoretically attractive explanation.

13. **Marty Tysanner**
December 9, 2022

Hi Dimitris Papadimitriou,

Those alternative theories that incorporate additional fields/ forces are obviously associated with unknown particles that need to be detected for any of these proposals to be tested and confirmed.

Logically speaking, is this an example of "proof by assertion"? Modified gravity proposals (like MOND) modify the form of the gravitational interaction without adding new particles/fields to the already-known set. That doesn't mean that modified gravity proposals account for every observational fact, or even "make sense" from the standpoint of our current theoretical understanding, but those are different issues.

Only detection via any of the three interactions of the standard model of particle physics is " legitimate " for the confirmation of existence of DM? Gravity does not count as an interaction?

Since I'm not aware of suggestions that gravity can't be treated as an interaction, or that we can't infer physical facts from gravitational effects, I don't know how to respond other than "of course gravitation counts as an interaction." But that's beside the point.

If all you see are gravitational effects with no signature from any of the other three known interactions, what can you infer from it? If you put a thermally and electromagnetically shielded box on a sensitive torsion balance and measure the mass of its contents gravitationally, can you say, in a *practical* sense, whether the box contains a ham sandwich, a glass of water or a lump of an exotic new metallic alloy? Can you falsify the experimentalist's claim that the box holds an exotic alloy without opening up the box and examining its contents via a non-gravitational interaction?

Likewise, how can you meaningfully claim observation of any dark matter particle if said particle interacts only gravitationally? How can you *empirically* distinguish two hypothetical dark matter candidates, both of which only interact gravitationally?

14. **Marty Tysanner**
December 9, 2022

Hi Peter,

[…] right-handed neutrinos seem to be an extremely well-motivated theoretical idea, and particles with only gravitational interactions are not at all unobservable (they may very well be exactly what explains galaxy rotation curves, CMB peaks, etc).

Even with good theoretical motivation, how would one test the idea that right-handed neutrinos meaningfully contribute to dark matter phenomena if they only interact gravitationally? How can we empirically distinguish them from other possibilities, so that the idea can become more than a story we tell ourselves?

On a different note… Since I haven't studied the right-handed neutrino idea at all, please forgive me if the following question is naive. Presumably right-handed neutrinos would be included as a term in an expanded Lagrangian that includes the SM plus gravity (gravitons), and thus would be governed by a QFT. Hence there should be at least one interaction vertex for them, one that describes the necessary coupling to gravity. The question I have about the idea is the following: Is there an additional coupling whereby right-handed neutrinos are created in the early universe but not today, and if so, what might that coupling look like? Or does the neutrino/graviton coupling alone mediate the right-handed neutrino creation, with the required energy to create them only being available in the early universe?

15. **Peter Woit**

December 9, 2022

Marty Tysanner,
I'm not a cosmology or dark matter expert, maybe one can answer your questions. I have plenty myself, just know that the SM with neutrino masses to me most naturally has right-handed neutrino fields. About contact with observation, I'm assuming your cosmological model produces gravitation-only particles, they evolve and you get structure formation. Does this match what you are seeing now? I have no idea what the problems here are.

16. **Stacy McGaugh**
December 9, 2022

Hi Peter,
Thanks. I have bent over backwards to remain objective and address the data that people say I don't address (e.g., arXiv:1404.7525). There is just too much to know, and ultimately, which way we lean depends on how we weigh rather disparate lines of evidence.
Much of the discussion in the context of particle physics seems not to do this, and skip directly to "what could the dark matter be?" This is indeed an interesting question, but its phrasing also presupposes the answer. We've invented dark matter for good reason, but now that we have it wedged into our consciousness, how could we tell if we were wrong?
I see Marty is posing a similar question about distinguishing different types of non-interacting particles. That's a good question. My concern is that, and also more general: is the dark matter paradigm falsifiable?

17. **Felix**
December 10, 2022

The statement by clayton that stellar feedback and galaxy-galaxy interactions are 'unmodelable SM physics' might come as a surprise to all the people who've spent their careers modelling those things

18. **clayton**
December 11, 2022

to respond to Stacy (to whom I do want to reiterate my genuine admiration for pointing out many interesting features of galactic rotation curves) and Felix and anyone else — the CMB is a linear system that we are able to model at _extremely high precision_, and the predictions without CDM are _qualitatively wrong_. The CMB allows a precise measurement of the CDM and baryon densities independently because it is such a simple linear system.

Galaxies are not! Star-formation physics is incredibly complex and nonlinear. So perhaps it's a bit imprecise to say that galaxies are "unmodelable" — they are unsolvable in the same sense that the Navier-Stokes equations are unsolvable. Obviously, all such systems are modelable. But we simply do not have sufficiently good models at this time to make high-precision measurements of fundamental physics from them.

As for the Skordis and Zlotnik papers — they require a new field that carries a substantial energy density that redshifts like a^-3. It walks and quacks like CDM (even though it doesn't have perturbations like CDM, but they also don't solve for the matter power spectrum — another linear quantity that CDM gets right).

Some resources:
https://chrisnorth.github.io/planckapps/Simulator/# (click on the "Toggle power spectrum plot" button on the top right, the one that looks like a squiggle, and see how different the power spectra and the CMB maps look with and without CDM!)
http://background.uchicago.edu
https://arxiv.org/abs/1112.1320

19. **Maurice**
January 15, 2023

Dear Clayton,

Maybe you could try to enter the discussion with Dr McGaugh here https://tritonstation.com/2023/01/13/what-we-have-here-is-a-failure-to-communicate/

If not, well, silence is better than an answer, a confession.

Best,
Maurice

Wormhole Publicity Stunts: Past, Present and Future

December 9, 2022
Categories: Wormhole Publicity Stunts

Most of the news I'm hearing today about the current wormhole publicity stunt is that physicists who could do something about it are instead blaming any problem on journalists and defending the stunt as some sort of progress forward.

I've been wondering what the future for this kind of thing looks like, got a partial answer by looking at this presentation today by the director of Fermilab. On page 67 she explains

> Future experiments with better QC and with QCs connected through quantum networks, such as those under development at Fermilab, could provide better insight through better resolution and adding non-trivial spatial separation of the two systems.

So, next generation wormhole publicity stunts will involve, beyond going from 9 qubits to more, putting two quantum computers in two places and connecting them by a quantum network. The press reports will explain that physicists not only created a wormhole on a chip, but created a wormhole connecting two different labs.

I started looking for more information about these next-generation wormhole publicity stunts, and found instead something I hadn't been aware of, an older such stunt, described in Towards Quantum Gravity in the Lab on Quantum Processors, which got attention last spring not at Quanta, but in the much lower profile Discover Magazine, where one reads:

> The team developed quantum software that could reproduce wormhole inspired teleportation on both quantum computers and then characterized the results. "We have designed and carried out "wormhole-inspired" many-body teleportation experiments on IBM and Quantinuum quantum processors and we observe a signal consistent with the predictions," say Shapoval and co.

One reason for the lack of significant attention to this publicity stunt as opposed to the current one surely is the decision of the authors to claim not "wormhole teleportation" but "wormhole-inspired teleportation".

The past is the past, but it looks like the field of quantum gravity research is from now on going to be dominated by these wormhole publicity stunts, using more qubits and more quantum computers. This kind of research project is nearly ideal: you can get lots of funding from conventional sources like DOE, or even better, funding from and access to equipment at large tech companies like Google and IBM. You can convince the director of your lab or institute that you're doing research of significance comparable to the discovery and testing of general relativity 100 years ago and your work will be vindicated by cover stories in Nature and all over the rest of the media.

Back in 1996, in *The End of Science*, John Horgan worried that this kind of science would end up in a "speculative post-empirical mode", and quantum gravity theorists have for years now worried about accusations of not being connected to experiment. The solution to this problem is now clear: no one will take your wormholes seriously if they're just on paper, so the thing to do is to get them realized in an algorithm that you run on the most twenty-first century experimental hardware available, a quantum computer in a tech company lab.

Update: There was a hoax comment posted here last night, supposedly from Natalie Wolchover, which had me fooled for a while. Whoever was doing this seems to have been making excellent use of ChatGPT, together with manipulating other aspects of how the comment was posted that helped fool me.

Comments

1. **Alessandro Strumia**
   December 10, 2022

   In my opinion, worse than the overhyped wormhole was the overhyped 2021 Fermilab presentation of muon g-2 results as a 4.6σ anomaly, done ignoring one lattice computation that was in agreement with the measured value. Now ≈three lattice computations agree, so the g-2 anomaly is likely over, but the Fermilab director (in the recent presentation you link) still writes: "Muon g-2: Complete data production, analysis, theory to achieve 5σ".

   Furthermore, I don't understand the claim, in the same presentation: "U.S. is universally acknowledged as the world leader in neutrino science for decades to come". Main results come from Japan, and their future HyperKamiokande can be faster/better than DUNE.

2. **John Horgan**
   December 10, 2022

   Thanks for the shout-out Peter. The phrase with which I describe physics involving strings, multiverses and other untestable stuff is "ironic science," which should be considered more akin to philosophy or science fiction than science. This wormhole story seems like a fascinating example of it. The details of the "experiment" described in Nature are obscure to me, but here is how I understand it: The authors postulate that entanglement is carried out via wormholes. Then they perform quantum computations, which supposedly exploit entanglement. To emphasize the wormhole angle, they simulate a wormhole with their computations, and on that basis they claim to have actually created a wormhole, because entanglement entails wormholes. Is that right? If so, it's a spectacular case of question begging, in the sense of assuming what you set out to prove. And by the way, I am not an AI bot, this is the real, fully sentient John Horgan.

3. **Peter Woit**
   December 10, 2022

   John Horgan,
   I still think the best way to explain the problem is that the physics of a quantum computer has nothing to do with gravity. You can't do an experiment that has nothing at all to do with certain physical degrees of freedom, and claim that you are studying those other degrees of freedom experimentally.

   What's going on here is people muddying the waters because it sounds good, bringing in highly speculative and ill-defined theories (ER=EPR) about relations between the quantum computer physics and geometry of some space (which has nothing to do with our space-time), together with mixing up a simulation of a model and the physics being simulated itself.

   Relevant to your writings, what I wanted to point out is that many of the people involved in this have been getting beaten over the head for years about embodying the "post-empirical" part of the danger you were pointing to. What's going on here is basically a fraudulent attempt to claim that they have solved this problem, have found a way to experimentally investigate quantum gravity on a quantum computer, despite the fact that there is no relevant interaction of the experiment with gravitational degrees of freedom.

4. **Scott Aaronson**

December 10, 2022

Complicating the discussion is the fact that there really are toy models of quantum gravity for which it's currently intractable to calculate what they predict, and there really is some hope that a 100- or 200-qubit quantum computer could help with that. Indeed, because of the extreme simplicity of the toy models (typically just an array of qubits subject to some time-independent Hamiltonian), this might even be one of the *first* applications of quantum computers to become technically feasible (for some definition of "application").

Of course, as shouldn't need to be said but often does, even if this program succeeds, the question will remain of what if anything those toy models say about the real universe. *Calculating the predictions* of the toy models can only ever be one part of an answer to that question.

5. **Feliz Kemal**
December 11, 2022

Dear Peter,
As you rightfully pointed out the authors, at least the last author, love public attention. For instance, this experiment got so much media attention and it was covered everywhere as if they invented teleportation for the first time ever (look at the altmetric score)
https://journals.aps.org/prxquantum/abstract/10.1103/PRXQuantum.1.020317

However, teleportation experiments at telecom wavelengths have been around for decades, for instance: https://www.nature.com/articles/nature01376

What they did last year has improved the state fidelities etc. but the result is far from what is made out to be. For instance, they also speculate that their work would open the path towards integrating these teleportation systems with quantum memories, but even then that was done some 8 years ago:
https://www.nature.com/articles/nphoton.2014.215

6. **George Ellis**
December 11, 2022

John Horgan here repeats their mantra "entanglement entails wormholes." This is simply false. The EPR paper in 1935 had nothing whatever to do with a wormhole, it was a Minkowski spacetime calculation. Entanglement happens quite happily in flat spacetime, as has been experimentally proved. It could perhaps occur in traversable wormholes IF any were to exist in the real universe, which is highly doubtful. In any case the claimed equivalence represented in an "equation" of the form ER = EPR is not true. Don't fall for the propaganda!

7. **John Horgan**
December 11, 2022

Thanks guys, really helpful. Just one question: Are the particles in the 9-qubit Google computer, when they carry out a computation, entangled with each other? Is that why the Nature authors can claim that they have created an actual wormhole, and not just a simulation, because according to the ER-EPR conjecture entanglement entails a wormhole?

8. **Peter Woit**
December 11, 2022

George Ellis,
My problem with ER=EPR is not that it is untrue, but that it's meaningless, as about "not even wrong" as "not even wrong" gets. It's basically a conjecture that "maybe there is a duality between conventional QM and some unknown theory", but doesn't tell you what the unknown theory is (it's not conventional GR). What's going on here is a study of some toy model infinitely far from anything physical, but with some ER=EPR hoped for properties. The authors do completely standard calculations in the toy model on a classical computer. Then, they do again the simplest such one possible on a quantum computer, purely as a (very successful) publicity stunt.

9. **Peter Woit**
December 11, 2022

John Horgan,

The qubits in any quantum computer are entangled, that's the basis on which it works. It's also true that entanglement is everywhere in any quantum system. What's hard to do is not to create entanglement but to isolate the qubits so they don't entangle with the environment. See my comment to Ellis about ER=EPR: if you believe it, a quantum computer is a collections of wormholes (where the word "wormhole" is meaningless, since you have no idea what the dual theory is in which they supposedly exist).

The "wormhole" they are claiming is a name for something in the 2d gravity toy model dual to an SYK toy model. I have to advise you to not even try and understand exactly what this means and what this "wormhole" is, if you think you understand it I'll bet you're mistaken…. Such things have no relationship at all to physical reality, they're objects in toy models. Also nothing to do with quantum computers, which come into play here purely as a way to fool the rubes (such as the IAS and FNAL directors).

10. **John Horgan**
December 11, 2022

George Ellis, don't worry, I haven't fallen for the ER-EPR "propaganda." I'm just trying to understand how the Nature authors could possibly claim that their crude little simulation contains an actual wormhole. The only justification–and again, I don't believe it, I'm just trying to figure out what the authors believe–is that they assume the ER-EPR conjecture is correct, which implies that a real wormhole might underpin their quantum-computer simulation of a wormhole. Peter Woit, thanks for your clarifications, and cautions. I don't know if I'm going to write about this strange episode, but I enjoy trying to make sense of it, for my own sake if no one else's. Physics just gets weirder and weirder.

11. **vmarko**
December 11, 2022

By the way, even if one is a "true believer" in ER=EPR (as the authors of the paper supposedly are), this particular result is nowhere near the landmark for wormholes, nor a first one created. A quick google search says this about a world record for controlled entanglement:

"A team led by physicists Prof. Harald Weinfurter from LMU and Prof. Christoph Becher from Saarland University have now coupled two atomic quantum memories over a 33-kilometer-long fiber optic connection. This is the longest distance so far that anyone has ever managed entanglement via a telecom fiber. (Jul 7, 2022)"

That must be one hell of a wormhole, and moreover, inside a fiber optic cable! The media should have covered *that* wormhole, not this tiny one inside Google's processor… 🙂

However you frame it, the media outlets have been had. 🙂

Best, 🙂
Marko

12. **André**
December 12, 2022

Funnily, assuming one would take ER = EPR seriously, one would believe that the ‘real’ entanglement in the quantum computer is dual to some wormhole in some spacetime, whereas the hypothesized 2D wormhole is dual to entanglement in the simulated model.

So I guess an equally valid headline would have been: “Physicists simulate entanglement using Google’s wormholes”

Quanta might have been better off putting the qualifier in front of the ‘Physicists’ rather than the ‘wormhole’ when amending the title.

13. **Peter Shor**
December 13, 2022

@vmarko:

You say

> That must be one hell of a wormhole, and moreover, inside a fiber optic cable!

I don’t think that’s how wormholes work. A wormhole takes matter directly from the input to the output without going through the physical space in between.

Similarly, quantum teleportation takes the input quantum state directly to the output quantum state without this state being present anywhere in the physical space in between.

So clearly, by Occam’s razor, teleportation must work by transporting the quantum state through a wormhole. 😉

The New Yorker and the Publicity Stunt

December 12, 2022
Categories: Wormhole Publicity Stunts

The wormhole publicity stunt story just keeps going. Today an article about the Google Santa Barbara lab and quantum computer used in the publicity stunt appeared in the New Yorker. One of the main people profiled is Hartmut Neven, the lab founder and a publicity stunt co-author. He is described as follows:

> Neven, originally from Germany, is a bald fifty-seven-year-old who belongs to the modern cast of hybridized executive-mystics. He talked of our quantum future with a blend of scientific precision and psychedelic glee. He wore a leather jacket, a loose-fitting linen shirt festooned with buttons, a pair of jeans with zippered pockets on the legs, and Velcro sneakers that looked like moon boots. "As my team knows, I never miss a single Burning Man," he told me.

The article explains what has been going on at the Google lab under Neven's direction:

> in the past few years, in research papers published in the world's leading scientific journals, he and his team have also unveiled a series of small, peculiar wonders: photons that bunch together in clumps; identical particles whose properties change depending on the order in which they are arranged; an exotic state of perpetually mutating matter known as a "time crystal." "There's literally a list of a dozen things like this, and each one is about as science fictiony as the next," Neven said. He told me that a team led by the physicist Maria Spiropulu had used Google's quantum computer to simulate a "holographic wormhole," a conceptual shortcut through space-time—an achievement that recently made the cover of *Nature*.

There are some indications given that the wormholes aren't everything you'd like a wormhole to be:

> Google's published scientific results in quantum computing have at times drawn scrutiny from other researchers. (One of the Nature paper's authors called their wormhole the "smallest, crummiest wormhole you can imagine." Spiropulu, who owns a dog named Qubit, concurred. "It's really very crummy, for real," she told me.) "With all these experiments, there's still a huge debate as to what extent are we actually doing what we claim," Scott Aaronson, a professor at the University of Texas at Austin who specializes in quantum computing, said. "You kind of have to squint."

I took another look at the *Nature* article and realized that at the end it has a section explaining the contributions of each author (I'll reproduce the whole thing as an appendix here). For Neven it has

> Google's VP Engineering, Quantum AI, H.N. coordinated project resources on behalf of the Google Quantum AI team.

Two physicists profiled are John Preskill and Alexei Kitaev. Academia in this field is seeing a big impact of quantum computing jobs and funding. According to the article:

> Preskill and Kitaev teach Caltech's introductory quantum-computing course together, and their classroom is overflowing with students. But, in 2021, Amazon announced that it was opening a large quantum-computing laboratory on Caltech's campus. Preskill is now an Amazon Scholar; Kitaev remained with Google. The two physicists, who used to have adjacent offices, today work in separate buildings. They remain collegial, but I sensed that there were certain research topics on which they could no longer confer.

Someone told me that the Amazon lab where Preskill works has postdoc-type positions for theoretical physicists, salary about 250K.

Theoretical physics hype and quantum computing hype come together prominently in the article. Besides Shor's algorithm and its implications for cryptography, here's the rest of what quantum computers promise:

> A quantum computer could open new frontiers in mathematics, revolutionizing our idea of what it means to "compute." Its processing power could spur the development of new industrial chemicals, addressing the problems of climate change and food scarcity. And it could reconcile the elegant theories of Albert Einstein with the unruly microverse of particle physics, enabling discoveries about space and time.

How long until quantum computers unify GR and the Standard Model? We just need better, fault-tolerant, qubits, and then:

> A thousand fault-tolerant qubits should be enough to run accurate simulations of molecular chemistry. Ten thousand fault-tolerant qubits could begin to unlock new findings in particle physics.

The hype here is far hypier than any of the string theory hype I've been covering over the years, and it looks like it's got a lot more money and influence behind it, so will be a major force driving the field in coming years and decades.

Appendix:

The Nature contributions section is:

> J.D.L. and D.J. are senior co-principal investigators of the QCCFP Consortium. J.D.L. worked on the conception of the research program, theoretical calculations, computation aspects, simulations and validations. D.J. is one of the inventors of the SYK traversable wormhole protocol. He worked on all theoretical aspects of the research and the validation of the wormhole dynamics. Graduate student D.K.K.47 worked on theoretical aspects and calculations of the chord diagrams. Graduate student S.I.D. worked on computation and simulation aspects. Graduate student A.Z.48 worked on all theory and computation aspects, the learning methods that solved the sparsification challenge, the coding of the protocol on the Sycamore and the coordination with the Google Quantum AI team. Postdoctoral scholar N.L. worked on the working group coordination aspects, meetings and workshops, and follow-up on all outstanding challenges. Google's VP Engineering, Quantum AI, H.N. coordinated project resources on behalf of the Google Quantum AI team. M.S. is the lead principal investigator of the QCCFP Consortium Project. She conceived and proposed the on-chip traversable wormhole research program in 2018, assembled the group with the appropriate areas of expertise and worked on all aspects of the research and the manuscript together with all authors.

Update: John Horgan's take on the stunt is Physicists Teleport Bullshit Through a Wormhole.

Update: Quanta is still actively promoting their story that

> Physicists have purportedly created the first-ever wormhole, a kind of tunnel theorized in 1935 by Albert Einstein and Nathan Rosen that leads from one place to another by passing into an extra dimension of space.

with nothing on their site that indicates that this story has gotten an almost universally negative reaction from knowledgeable scientists. The one exception I've seen is Lubos Motl, who comments on the Quanta story. Lubos also engaged in a long exchange here with Matt Strassler. I had been worried about how Lubos was doing, it's reassuring to see that he's still out there, still himself, and still reliably defending the most indefensible products of string theory hype in his characteristic style.

Update: A surprising number of theorists seem willing to help hype the publicity stunt. See for instance this from Penn. It seems to me full of misinformation, for instance

> Heckman: In fact, the entire experiment could have been done on a classical machine; it just would have taken a lot more time.

In fact, the "experiment" was actually done on a classical computer, in a very short amount of time. For studying this kind of model, classical computers are hugely faster and more capable than any current quantum computer. Here they were used to search for a simplified version of the real problem that was easy enough that it could be done by the quantum computer.

Update: Dan Garisto on his blog has an article about the wormhole fiasco, originally intended for publication in SciAm. Garisto explains that SciAm was originally not taken in by the publicity stunt:

> I should tell you, when my editor at Scientific American sent me the embargoed press release on Thanksgiving with the subject line "Hmmm," I responded dubiously. "They used 9 qubits! What the hell could that possibly tell you?" We chose not to cover it—initially.

He also gives a correct description of what this really was:

> …the supposed bombshell dropped by the Nature paper is a calculation of an approximation of a model conjectured to be equivalent to a lower-dimensional gravity for a universe that isn't ours.

and challenges the misleading messaging from the scientists and from Quanta:

> Quantum message discipline is sorely needed. Clarifying their headline change, Quanta noted that "The researchers behind the new work — some of the best-respected physicists in the world — frequently and consistently described their work to us as 'creating a wormhole.'"
>
> When reached for comment, the study's co-leader Maria Spiropulu pointed to a Caltech FAQ in her team's defense. "Did we claim to have produced or observed a wormhole deformation of 3+1-dimensional spacetime?" the FAQ asks. The answer it then offers is a firm "No." The FAQ further elaborates that what the researchers saw was only "consistent with the dynamics of a traversable wormhole." The Nature paper is less absolute. There, Spiropulu and her co-authors describe their research as "the experimental realization of traversable wormhole dynamics on nine qubits" and discuss how they used a quantum teleportation protocol to "insert another qubit across the wormhole from right to left."
>
> And in a video produced ahead of publication by Quanta, several researchers spoke gushingly, saying, "This is a wormhole" and "I think we have a wormhole, guys." This is at best, deeply misleading.

He also has a quote from Natalie Wolchover which tries to explain the point of view which she (and others like Susskind) take that a quantum computer calculation of a toy mathematical model of a physical system is somehow really creating and doing a laboratory experiment on the physical system being modeled:

> Natalie Wolchover, a Pulitzer Prize-winning science writer at Quanta, argues that when a quantum computer simulates a toy model of quantum matter, such as the SYK model, it is really "creating" the quantum system it asks about. "It's profound but somehow I can't put my finger on what it means about the difference between 'real' and 'simulated,'" she wrote in an email.

This is complete nonsense, as Scott Aaronson tried to explain in the quote he gave for the NYT article about this:

> If this experiment has brought a wormhole into actual physical existence, then a strong case could be made that you, too, bring a wormhole into actual physical existence every time you sketch one with pen and paper.

Comments

1. **Pascal**
   December 12, 2022

   I have finally watched the quanta video. On top of all the other problems, they have their history of wormhole physics completely wrong. They claim that Einstein was frustrated that the ER bridge was not traversable. But the idea to use wormhole to connect distant points in space time is not due to Einstein-Rosen'35 at all. It came much later, in the sixties, with the work of Wheeler and collaborators. The notion of a "non-traversable wormhole" is also due to these guys. In ER'35, a wormhole was a model of an elementary particle. Unfortunately, a neutron is much too small for Einstein or Nathalie Wolchover to jump through.

2. **Scott Aaronson**
   December 12, 2022

   By the usual standards for quantum computing popular articles that have prevailed for 20+ years, this one's not so bad. At least it talks clearly about the difficulty of beating classical computers in a fair comparison, and it focuses much more on quantum physics and chemistry simulation (which is indeed the best bet for near-term applications) than on doubtful uses in optimization, finance, and machine learning.

   For the record, in the quote from me, I wasn't talking about the wormhole claim, which hadn't even come out yet when Stephen Witt interviewed me, and about which I probably would've had stronger words! I was talking specifically about the quantum supremacy experiments based on BosonSampling and Random Circuit Sampling.

3. **John Pohl**
   December 12, 2022

   "Someone told me that the Amazon lab where Preskill works has postdoc-type positions for theoretical physicists, salary about 250K." Is this true? That is amazing money for a postdoc position.

4. **Peter Woit**
   December 12, 2022

   Scott (or anyone else),

   Any guess for the source for the claim about "ten thousand fault-tolerant qubits" as the threshold for getting something new in particle physics? Presumably this wasn't a randomly chosen number, but corresponds to some particular calculation on a quantum computer. Which one?

5. **Peter Woit**
   December 12, 2022

   John Pohl,
   Someone told me the 250K number. I just checked on Glassdoor, which shows

https://www.glassdoor.com/Salary/Amazon-Postdoctoral-Research-Scientist-Salaries-E6036_D_KO7,38.htm
231K (141K base, 27K bonus, 63K stock).

Yes, that's amazing money for a postdoc position. Also according to Glassdoor
https://www.glassdoor.com/Salary/Caltech-California-Postdoctoral-Scholar-Salaries-E22139_D_KO19,39.htm
the salary for a more conventional postdoc is 55K.

In the string theory hype story I never thought that money was what was motivating people to engage in hype. In this case I think it's clear money is a factor.

6. **Scott Aaronson**
December 12, 2022

@Peter Woit: I was wondering that too! I think it's plausible that, supposing you had 10,000 fault-tolerant qubits, you could do all sorts of simulations of interest for HEP. But I have no idea why the article gave that specific number.

7. **Peter Woit**
December 12, 2022

Scott,
I just realized this could refer to either simulating QCD better than usual Monte-Carlos, which could be a significant thing. But it might also be something like the SYK calculation, which supposedly was "quantum gravity in a lab", but wouldn't really tell you anything about the real world.

8. **David Roberts**
December 12, 2022

At my university, "coordinating project resources" isn't enough to officially be allowed to be an author. It has guidelines around how much people need to contribute to be an author on a paper, and merely bringing funding isn't enough, but I suspect these guidelines are sometimes treated as such, or else grant holders have a high-level project meeting once or twice, and give comments, and count themselves as having 'contributed' to the research.

9. **Peter Woit**
December 12, 2022

David Roberts,
Maybe rules are different when you're creating wormholes.

10. **Meticulous Reader**
December 13, 2022

I read John Horgan's piece. It's a nice one, but even that one contains some of the roots behind the issue. Journalists should meticulously choose their words and not fall into the trap of repeating what people in a certain group keep telling each other. As long as journalists keep referring to those people as "quantum gravity experts", hype around what those people are doing can only grow and receive wide acceptance. Calling people quantum gravity experts when those people spend their careers playing with toy models that have nothing to do with real gravity is in itself a big contribution to feeding the hype.

11. **Davide Castelvecchi**
December 13, 2022

"Soon, in what many consider the most bizarre scientific result of all time, the physicists realized that whether an electron behaved more like a particle or more like a wave depended on whether or not someone was observing it. The field of quantum mechanics was born."
— I'm not sure that's how the history of quantum mechanics went? 🙂

12. **Peter Woit**
December 13, 2022

Meticulous Reader,
I do agree there are some problems with the Horgan piece. For instance, he does not make clear that while the scientists involved produced unprecedented amounts of bullshit, none it actually got teleported anywhere.
Since this whole subfield is based on Susskind's 2017 GR=QM paper, a more accurate description of those involved than "quantum gravity experts" would be "people pursuing an increasingly outlandish failed 40 year old research program".

13. **Peter Woit**
December 13, 2022

Davide Castelvecchi,
Does make one wonder about the New Yorker fact-checking. If they got that so wrong, how could you trust them about the really hard to believe stuff, like what Hartmut Neven was wearing?

14. **Sabine Hossenfelder**
December 13, 2022

"There's literally a list of a dozen things like this, and each one is about as science fictiony as the next,"

Except in science fiction novels they can actually do something with the technology, rather than just publish papers about its interpretations in scientific magazines.

15. **David Thurman**
December 20, 2022

Peter, the identical behavior is playing out in the crypto world right now. Which BTW is a nice place for mathmaticians, computer science majors, and physics majors from MIT such as the ftx founder to make a quick billion.

Ethics is that actually science? I don't think it comes under that domain directly it manifests in it indirectly. So the article isn't all that shocking, and now it's internet hard science fact which you have to battle.for years.

Sometimes the one fooling others is the biggest fool or an idiot. Other times they lack ethics and then it becomes evil. Hard to tell here on this topic in regards to are they evil or idiots. Ethics, interesting topic, it's not science, but is profoundly and deeply relevant to good healthy science, which appears to becoming sicker by the day.

16. **Peter**
    December 20, 2022

    I did some superficial checking here in NW-Europe where I live. The Guardian in England mentioned it, but wrote "Experts who were not involved in the experiment cautioned that it was important to note that a physical wormhole had not actually been created, but noted the future possibilities" and quoted Daniel Harlow in the NYT.

    A search on "trou de ver" (wormhole in French) didn't show results in the leading French newspapers (Le Monde, Le Figaro, Libération). Same for "Wurmloch" in German.

    Nothing, as far as I can see, in the leading newspapers in the Dutch-speaking part of Belgium, although in the French-speaking part there's an article on the website of the public broadcaster RTBf. It's uncritical and ends with the message that the wormhole is not one we'll perhaps find in space, "but it allows progress in the unification of the theories of quantum entanglement, spacetime and quantum gravity" (my translation).

    A leading newspaper in the Netherlands, de Volkskrant, had an article about the experiment but it focused on the controversy and gave a lot of space to the critics. It mentioned a critic who says "if they had given their paper a realistic title (…) nobody would have batted an eyelid" (my translation).

    My search wasn't very thorough, but I have the impression the recent experiments on nuclear fusion got more attention.

17. **jonW**
    December 23, 2022

    Peter, Quanta has just put out a review of the most significant (in their view) stories of the year in Physics. It's written by Natalie Wolchover, who is much less gushing (almost backpedalling) about wormholes in this new article:

    "The holographic wormhole spawned endless opinions among physicists and lay readers alike. Some physicists thought the quantum simulation was too pared down compared to the theoretical model it was based on to have a holographic dual description as a wormhole. Many felt that the physicists behind the work, and we, the journalists who covered it, should have better emphasized that this was not an actual wormhole that could transport people to Andromeda. Indeed, to open up a wormhole in real space-time, you'd need negative-energy material, and that doesn't seem to exist. "

    It's at https://www.quantamagazine.org/the-biggest-discoveries-in-physics-in-2022-20221222/

18. **Peter Woit**
    December 23, 2022

    jonW,
    Thanks, that's a lot less over the top than the original article video. Still, listing this as one of the five great discoveries in physics this year has no relation to reality.

19. **French Dip**
    December 23, 2022

    Hi Peter,

    I'm not sure you're referencing the Penn article accurately as there's a fair amount of criticism in there. See e.g.

    Heckman: *Insinuating that there is an actual wormhole traversal happening in our world is quite misleading. The authors of the article and the press covering it are engaging in very irresponsible representations of the work.*

    Balasubramanian: *Once again, they have not built a wormhole in our world. To say that they done so requires some mental gymnastics.*

20. **Marvin**
    December 24, 2022

    Peter W and French Dip,

    About the Penn article, instead of dissipating the controversy, there is overall more hype in the article, making the experiment look a piece of reality that can be constructed from it after all:

    "The black holes in question don't exist in our world. Rather, they are present in an alternative, 'dual,' description of their quantum computing system in terms of a theory of gravity in a different number of dimensions. So, in a sense, they used this 'dual' notion of gravity to imagine a traversable bridge, or wormhole, connecting two different quantum mechanical systems, and translated this back into equivalent phenomena in the actual system they built on their quantum computer."

    So, these black holes that don't exist (wormlike connected) are represented with a gravity theory in multi-dimension (not 4 as in GR) that don't exist. What is this? A ridiculous toy model that pretend to have quantum computer properties. This trivial experiment and hype really don't help physic make progress.

21. **Peter Woit**
    December 24, 2022

    French Dip/Marvin,
    The problems with the way the publicity stunt was covered go way beyond the problem of no actual wormhole creation. The quantum computer calculation was of zero scientific value, it added nothing to our understanding of anything. The publicity stunt was in the end a huge success: its goal was to convince people (from the public to lab and institute directors) not that a wormhole had been produced, but that the organizers had accomplished something of scientific importance, that they were doing "quantum gravity in a lab". Besides the outright wrong things in the Penn article (no, this is not a quantum computer doing something better than a classical computer), it just completely mystifies what this calculation really was.

22. **Davide Castelvecchi**
    December 27, 2022

    @Peter Woit:
    Sorry I only saw your reply now. Perhaps one way to interpret "quantum mechanics was born" is that they were thinking of Einstein's 1905 paper on the photon? We knew already that light can have wavelike behavior, and this paper showed it can act like a particle, too…

23. **Alan Roxdale**

December 27, 2022

It seems pretty clear that the fuel driving all this stuff is funding pressure, and the pressure to game funding agencies by hook or by crook. A tale as old as post 1980 economic reforms.

I recall a long number of years ago this blog described a particular incident in Physics as a "reverse Sokal hoax". This one feels at at least about that magnitude …

> A surprising number of theorists seem willing to help hype the publicity stunt

…and probably more. Maybe we'll get a full-blown "N-ray redux" before it all fizzles out (it is going to fizzle out right?)

24. **tulpoeid**
December 28, 2022

I think you are wrong in attacking the Penn article. Yes, it got the quantum computing efficiency part right, but in everything else it was the complete opposite of "help hype the publicity stunt". Actually I'm a bit surprised to read that you think it contributes to the hype, while it is attacking it without mincing words.

25. **Peter Woit**
December 28, 2022

tulpoeid,
I read the article again, and I'll stick to the position that the Penn article misinforms and contributes to the hype. The answers to the questions about the role of quantum computers in this are just completely backwards and designed to further the hype. Saying
"No, they haven't created a wormhole, or a shortcut connection between two distant points in space. But what they've been able to achieve here is still very impressive and a good step forward for quantum computing."
shows the problem. Yes, Balasumbranian is correcting the nonsense about actually creating wormholes, but he's furthering the hype about this calculation ("still very impressive"). Claiming this as a "good step forward for quantum computing" is just hype and nonsense.

The point of the publicity stunt was not to convince people of the crazy idea that they had created real wormholes in the lab, but to convince the public, their colleagues, and those responsible for their funding that they had made impressive progress on quantum gravity by doing "quantum gravity in a lab". Balasumbrananian and Heckman (and many others) have decided to support this. For a more appropriate response, see Garisto's article. It's remarkable that here many science journalists are doing a much better job of getting the science right than the physicists commenting on this.

Alan Roxdale,
The danger here is that, like with string theory, this is not going to fizzle out, but instead become a dominant part of the field in the future. If funders can be convinced that where money should go is to bogus "quantum gravity in the lab experiments", that's going to be the future of the field.

26. **clayton**
December 28, 2022

I tend to agree that a lot of the coverage of this has been dreadful, but this Penn article seems to be one of the best — Heckman goes as far as to say "Insinuating that there is an actual wormhole traversal happening in our world is quite misleading", and Balasubramanian says "Once again, they have not built a wormhole in our world. To say that they done so requires some mental gymnastics." They use a lot of technical language which, honestly, I'm a bit surprised made it to this popular webpage. (Er, "popular.") Not sure what more they could have done, honestly.

27. **Peter Shor**
January 3, 2023

One final comment, although it's probably too late for anybody to read it.

> Natalie Wolchover, a Pulitzer Prize-winning science writer at Quanta, argues that when a quantum computer simulates a toy model of quantum matter, such as the SYK model, it is really "creating" the quantum system it asks about.

My response to this is that the quantum computer isn't even simulating a qubit going through a wormhole. If I understand the experiment correctly, the wormhole is in a static state; for a qubit to actually go through a wormhole, the wormhole has to have dynamics. It seems to me that is going on is that they are grabbing a qubit on one side of the wormhole, using standard teleportation to send it to the other side, and claiming that it went *through* the wormhole. This is a very bizarre interpretation of *through*.

28. **Geoff Penington**
January 9, 2023

@Peter Shor:

They're not doing standard teleportation. Instead what they do is evolve one side backwards in time, throw in a qubit, and evolve it forwards in time again. Then they turn on a simple coupling between the two sides (the "teleportation" part is that this could in theory be implemented by measuring one side and then evolving the other based on the measurement outcome), evolve the other side forwards in time, and magically the qubit pops out on the other side.

From a QI perspective the mechanism for why this works is not at all obvious, and actually relies on the dynamics of the theory creating something called "size winding". (Actually you don't need size winding to send one qubit through – any scrambling dynamics will do – but more than one qubit requires size winding.) In fact, the effect was only discovered at all by looking at holographic theories, where it literally describes a particle falling causally through a traversable wormhole.

The nontrivial "interesting" content of the nature paper involved finding sparse dynamics on only nine qubits that still exhibited signatures of size winding. This let them run the dynamics on Sycamore which is fun I guess.

Obviously this didn't push the limits of what's achievable on a quantum computer in any way. It's trivial and has been for years. I think the source of Vijay and Jonathan's confusion on this front (and I think it is confusion or at most academic politeness – not them trying to contribute to the hype) is that simulating a traversable wormhole in a meaningful way was supposed to be really quite hard – and so it's natural to think that its achievement would involve the real cutting edge of Sycamore's power. Instead the nature article just found a way to declare the bar for victory to be so low that they could step over it without really trying.

29. **Peter Woit**
January 9, 2023

Geoff Penington,

I didn't mean to accuse the Penn string theorists of intentionally trying to contribute to hype. The problem with outrageous claims like "physicists create wormholes" appearing in well-respected places like Quanta and the cover of Nature is that even quite sophisticated readers tend to assume that there must be some significant result behind the dubious headline. It looks to me like Heckman just made this assumption, without looking into what the actual result was.

Back in the days when string theory was getting this kind of hype treatment, the underlying theoretical results were very hard to understand without a lot of complicated background. But here it was very easy to see that the quantum computer business was an irrelevant stunt, it’s remarkable that any theorists were taken in by it.

Pierre Schapira on Récoltes et Semailles

December 30, 2022
Categories: Uncategorized

Earlier this year I bought a copy of the recently published version of Grothendieck's *Récoltes et Semailles*, and spent quite a lot of time reading it. I wrote a bit about it here, intended to write something much longer when I finished reading, but I've given up on that idea. At some point this past fall I stopped reading, having made it through all but 100 pages or so of the roughly 1900 total. I planned to pick it up again and finish, but haven't managed to bring myself to do that, largely because getting to the end would mean I should write something, and the task of doing justice to this text looks far too difficult.

Récoltes et Semailles is a unique and amazing document, some of the things in it are fantastic and wonderful. Quoting myself from earlier this year

> there are many beautifully written sections, capturing Grothendieck's feeling for the beauty of the deepest ideas in mathematics. One gets to see what it looked like from the inside to a genius as he worked, often together with others, on a project that revolutionized how we think about mathematics.

A huge problem with the book is the way it was written, providing a convincing advertisement for word processors. Grothendieck seems to have not significantly edited the manuscript. When he thought of something relevant to what he had written previously, instead of editing that, he would just type away and add more material. Unclear how this could ever happen, but it would be a great service to humanity to have a competent editor put to work doing a huge rewrite of the text.

The other problem though is even more serious. The text provides deep personal insight into Grothendieck's thinking, which is simultaneously fascinating and discouraging. His isolation and decision to concentrate on "meditation" about himself left him semi-paranoid and without anyone to engage with and help channel his remarkable intellect. It's frustrating to read hundreds of pages about motives which consist of some tantalizing explanations of these deep mathematical ideas, embedded in endless complaints that Deligne and others didn't properly understand and develop these ideas (or properly credit him). One keeps thinking: instead of going on like this, why didn't he just do what he said he had planned earlier, write out an explanation of these ideas?

As an excuse for giving up on writing more myself about this, I can instead recommend Pierre Schapira's new article at Inference, entitled A Truncated Manuscript. Schapira provides an excellent review of the book, and also explains a major problem with it. Grothendieck devotes endless pages to complaints that Zoghman Mebkhout did not get sufficient recognition for his work on the so-called Riemann-Hilbert correspondence for perverse sheaves. Mebkhout was Schapira's student, and he explains that a correct version of the story has the ideas involved originating with Kashiwara, who was the one who should have gotten more recognition, not Mebhkout. According to Schapira, he explained what had really happened to Grothendieck, who wrote an extra twenty pages or so correcting mistaken claims in *Récoltes et Semailles*, but these didn't make it into the recently published version. If someone ever gets to the project of editing *Récoltes et Semailles*, a good starting point would be to simply delete all of the material that Grothendieck included on this topic.

The extra pages described are available now here, as part of an extensive website called the Grothendieck Circle, now being updated by Leila Schneps. For a wealth of material concerning Grothendieck's writings, see this site run by Mateo Carmona. It includes a transcription of *Récoltes et Semailles* that provides an alternative to the recently published version.

The Schapira article is a good example of some of the excellent pieces that the people at Inference have published since they started nearly ten years ago (another example relevant to Grothendieck would be Pierre Cartier's A Country Known Only by Name from their first issue). I've heard news that they have lost a major part of their funding, which was reportedly from Peter Thiel and was one source of controversy about the magazine. I wrote about this here in early 2019 (also note discussion in the comments). My position then and now is that the concerns people had about the editors and funding of Inference needed to be evaluated in the context of the result, which was an unusual publication putting out some high quality articles about math and physics that would likely not have otherwise gotten written and published. I hope they manage to find alternate sources of funding that allow them to keep putting out the publication.

Comments

1. **Anon**
   December 31, 2022

   Rereading the Cartier article I am struck by his language; C. is quite the stylist, even in translation.

   Schapira's article does not offer the same linguistic pleasure, but does at least correct the record – though little is needed. Kashiwara's genius, so evident in his extraordinary papers (not only those on D-modules, microlocal analysis, RH) speaks for itself, as does Mebkhout's subsequent output. But the historical notes in some Kashiwara-Schapira books are cringe inducingly hagiographical about K, I've often wondered what K. thinks about them.

2. **Luca Signorelli**
   January 1, 2023

   Hi Peter, happy New Year! Glad to begin the year myself reading some of your excellent musing about Grothendieck. As you know, I'm greatly interested in his personality and his mathematics and I've written about him on my blog (which you kindly linked some time ago in another post you've dedicated to him).

   I've read Récoltes et Semailles (in French) in its entirety, and boy, it's a rough ride indeed. I agree with the fact it clearly wasn't edited. But I deeply feel the "unedited" version is the one we should read. Not that this version is the one Grothendieck himself would have liked us to read because I think in the end he didn't intend Récoltes to be published. But it's important we read it because in my opinion it has been assumed for too long that R&S is just the monumental rant of a terminally paranoid recluse. It's not. As you say, it has moments of absolute brilliance, mathematical or not.

   I've been in touch for some time with people who are/were familiar with Grothendieck's past, and I've somehow changed my attitude toward some of his most controversial decisions, including him leaving the IHES. I've long thought he had been overcome at a certain point by his personal problems which he somehow had rationalized in the refusal to work for the "institutions". It turns out he had a point of being wary of IHES connections to the military, and other people shared his views. But while Grothendieck was principled AND not well off, others were either principled and with enough means to get away with IHES money, or had not enough financial security to break ties with that institution. Long story, and not exactly a new one…

3. **Joseph Healy**
   January 2, 2023

   https://www.newyorker.com/magazine/2022/05/16/the-mysterious-disappearance-of-a-revolutionary-mathematician

4. **jack morava**
   January 8, 2023

Off topic, but, alas,
https://en.wikipedia.org/wiki/Yuri_Manin
has passed away.

5. **Phoenix Kim**
   January 9, 2023

   Is there any ongoing effort to translate Grothendieck's works into English language? I know some people independently translated some pieces of his work, but I have not seen any organized program to translate all his work, including Recoltes.

6. **Anon**
   January 11, 2023

   Pierre Schapira's article "A Truncated Manuscript" is available on ArXiv

   https://arxiv.org/pdf/2301.02898.pdf

7. **Andrew Gelman**
   January 24, 2023

   Hi, Peter. You might be interested in this mildly unpleasant experience I had with Inference magazine a couple years ago. The short story is they contacted me, asked me for an article, it went through a couple rounds of revision where at each step I had to bug them for comments, then suddenly they dropped it for no reason.

   My guess is that they were never so excited about the article in the first place. But the main thing I remember was the obsequious tone of the email from the assistant who solicited the article, followed later by a complete lack of explanation for why they dropped it.

   As I wrote in the above-linked post, I can't really be angry at them–they have every right to ask for an article and then change their mind. The whole experience was kinda weird, not like any other interaction I'd had with any other publication. A bizarre mix of hot and cold.

8. **Anonymous**
   January 26, 2023

   This may be the place to note some sad news: Mikio Sato, the creator of algebraic analysis, who is mentioned in Schapira's article, passed away this Jan. 9th:

   https://twitter.com/SpringerMath/status/1613150057095921665

   Here's an old (1990) AMS interview of Sato:

   https://www.ams.org/notices/200702/fea-sato-2.pdf

   Like Grothendieck, he was one of the really visionary mathematicians of the 20th century.

Spring Course

January 10, 2023
Categories: Uncategorized

This semester I'm teaching the second half of our graduate course on Lie groups and representations, and have started making plans for the course, which will begin next week. There's a web-page here which I'll be adding to as time goes on. The plan is to try and write up lecture notes for most of the course, some of which may be rewrites of older notes written when I taught earlier versions of this course. I'll post these typically after the corresponding lectures. Any corrections or comments will be welcome.

This year I'm hoping to integrate ideas about "quantization" into the course more than in the past, starting off with the mathematics behind what physicists often call "canonical quantization". This topic is worked out very explicitly and in great detail in this book, but in this course I'll be giving a more stream-lined presentation from a more advanced point of view. This subject has a somewhat different flavor than usual for math graduate courses, in that instead of proving things about classes of representations, it's one very specific representation that is the topic.

This topic is also the simplest example of the general philosophy of trying to understand Lie group representations in terms of the geometry of a "co-adjoint orbit", and I'll try and say a bit about this "orbit philosophy" and "geometric quantization".

The next topic of the course will likely be more standard: the classification of finite dimensional representations of semi-simple complex Lie algebras (or, equivalently, compact Lie groups), and their construction using Verma modules. For this topic it's hard to justify spending a lot of time writing notes, since there already are several places this has been done very well (e.g Kirillov's book). After doing this algebraically, I'll go back to the geometric and orbit point of view and explain the Borel-Weil-Bott theorem giving a geometric construction of these representations.

For the last part of the course, I hope to discuss the representations of SL(2,**R**) and the general classification of real semi-simple Lie algebras and groups. If I ever manage to understand what's going on with the real Weil group and the Langlands classification of representations of real Lie groups, maybe I'll say something about that, but that is probably too much to hope for.

Throughout the course, as well as the relation to quantization, I also hope to explain some things about relations to number theory. These would include the theory of theta functions early on, modular forms at the end.

Comments

1. **JT gravity**
   January 10, 2023

   I am finding the study of quantization of co-adjoint orbits very timely since there's a lot of interest these days on covariant phase methods in order to compute asymptotic charges associated to asymptotic symmetry groups on GR, such as the flat-space/celestial/Carrollian holography, e.g., the Noether charges associated to BMS group. The paper by Witten on Virasoro and coajoint orbits is still very readable, by the way. Since you like BRST cohomology, this paper by Barnich could be of some interest to some of yours students wishing to see a modern application of the methods presented in the class: https://inspirehep.net/literature/567304

2. **David Brown**
   January 16, 2023

   Is the following useful for understanding applications of real Lie groups to physics?
   KYUNGPOOK Math J. 42 (2002), 199-272 "The Method of Orbits for Real Lie Groups" by Jae-Hyun Yang
   https://www.arxiv-vanity.com/papers/math/0602056/

3. **Peter Woit**
   January 19, 2023

   David Brown,
   That article has a lot of material I'll be covering from a somewhat different point of view. By the way, a better link might be
   https://arxiv.org/abs/math/0602056

   I've just started posting notes for the course, in a couple weeks will write something about the orbit method, will also include references, some of which I think are much more helpful than that article.

   Soon I'll be lecturing on the Weil or oscillator representation. In some sense it's just one example of the general theory of the Jacobi group that the author of that paper writes about, but I think it's better understood on its own terms as a very special structure.

   Anyway, a detailed treatment of the subject should appear in the notes over the next couple weeks.

4. **Art**
   February 13, 2023

   Class notes?

   By the way, the Berkeley Lectures link doesn't work for me, although I think I was able to find them elsewhere.

5. **Peter Woit**
   February 15, 2023

   Art,
   I fixed the link to the Berkeley Lectures. Class notes I'm writing are linked to from the syllabus. Starting to get way behind though on those.

   I've just finished the first part of the course, now will turn to more conventional material for a while, probably won't write up notes on this material since there are many very good places to read about it (the Berkeley notes one example).

What is the AdS/CFT Conjecture?

January 22, 2023
Categories: Uncategorized

In recent years I've found there's no point to trying to have an intelligible argument about "string theory", simply because the term no longer has any well-defined meaning. At the KITP next spring, there will be a program devoted to [What is String Theory?](), with a website that tells us that "the precise nature of its organizational principle remains obscure." As far as I can tell though, the problem is not one of insufficient precision, but not knowing even the general nature of such an organizational principle.

What one hears when one asks about this these days is that the field has moved on to focusing on the one part of this that is understood: the "AdS/CFT conjecture." I've gotten the same answer when asking about the meaning of the "ER=EPR conjecture", and recently the claim seems to be that the black hole information paradox is resolved, again, somehow using the "AdS/CFT conjecture." Today I noticed this twitter thread from Jonathan Oppenheim raising questions about the "AdS/CFT conjecture" and the discussion there reminded me that I don't understand what the people involved mean by those words. What exactly (physicist meaning of "exactly", not mathematician meaning) is the "AdS/CFT conjecture"?

To be clear, I have tried to follow this subject since its beginnings, and at one point was pretty well aware of the exact known statements relating type IIB superstring theory on five-dim AdS space times a five-sphere with M units of flux to N=4 U(M) SYM. While this provided an impressive realization of the old dream of relating a large M QFT to a weakly coupled string theory, it bothered me that there was no meaning to the duality in the sense that no one knew how to define the strongly coupled string theory. This problem seemed to get dealt with by turning the conjecture into a definition of string theory in this background, but it was always unclear how that was supposed to work.

So, my question isn't about that, but about the much more general use of the term to refer to all sorts of gravity/CFT relationships in various dimensions. There are hundreds if not thousands of theorists actively working on this these days, and my question is aimed at them: what exactly do you mean when you say "the AdS/CFT conjecture"?

Update: The ongoing discussion between Jonathan Oppenheim, Geoff Pennington and Andreas Karch about this on Twitter is very interesting, indicates that it isn't so clear exactly what "the AdS/CFT conjecture" is. For me and presumably many others, would be great to have a source for an authoritative discussion of what is known about this topic. The Twitter format is very much not optimal for discussions like this.

Comments

1. **Bob P**
   January 23, 2023

   I actually think of AdS/CFT as being a bit like the orbit method in representation theory. Each is really a collection of results rather than a top-down theory. In both cases, the results are statements that you can do some calculations in two quite different looking setups and somewhat mysteriously arrive at the same answer. In AdS/CFT, one of the important results is the Ryu-Takayanagi formula. This formula says an entanglement entropy in certain CFTs is equal to the area of an extremal surface in an AdS spacetime. This reminds me of the orbit method result that relates dimensions of irreducible representations to volumes of coadjoint orbits. In both examples, something from quantum mechanics is related to something geometrical. Of course, AdS/CFT is much more limited than the orbit method in the sense that it only applies to certain "large N" CFTs, while the orbit method is supposed to be about all of representation theory. At any rate, I would call "AdS/CFT" the AdS/CFT correspondence and use it to refer to this collection of correspondences.

2. **Peter Woit**
   January 24, 2023

   Bob P,

   In the case of the orbit method (or of the Langlands program that Geoff Pennington made an analogy to on Twitter), a top-down explanation of why they work may be missing, but a minute of googling will get you fairly precise technical definitions of what "orbit method" or "Langlands program" means. You'll easily find the details of how specific co-adjoint orbits are supposed to match with specific Lie group representations or how specific modular forms are supposed to match with specific Galois representations. My question "what is the AdS/CFT conjecture" was not intended to be tendentious or get people arguing, but, seriously, to try and just understand exactly what people working on this mean when they use the term.

3. **Jonathan Oppenheim**
   January 24, 2023

   The proper experts will hopefully correct me if I'm wrong, but my amateur answer is that the old school version of AdS/CFT was that (i) the partition function of the bulk AdS theory (e.g. type IIB string theory in AdS5xS5) is the same as the partition function of the boundary CFT (e.g. N = 4 supersymmetric Yang–Mills theory). And (ii) the Hilbert spaces of both theories are the same. This would give us a correspondence between bulk and boundary observables and correlation functions. I think there is a lot of evidence for (i).

   The status of (ii) is very unclear to me. It would say that there is a 1-1 map between bulk states and boundary states. But https://arxiv.org/abs/1411.7041 gives an example where it can't be true in its original form because it would lead to a violation of the no-cloning theorem. So in order to preserve some sense of (ii), they postulate that the bulk is encoded in the boundary as an error correcting code. In other words, each physical state in the bulk, needs many states of the boundary in order for it to be encoded in such a way that a violation of the no-cloning theorem is avoided. So (ii) might be true in the sense that there is a correspondence between physical states of the bulk, and "code states" of the boundary. I don't know that there is much independent evidence that the CFT is an error correcting code, but it certainly has to be if some notion of (ii) is to be preserved.

   But even this is not enough, because old black holes can have "too many states". And so a more recent claim is that various bulk states which we think of as being orthogonal, are not really orthogonal. See for example https://arxiv.org/abs/2207.06536. So, I would say that (ii) is not regarded as true anymore, but it may be true in some sense (although I feel this is speculative).

   There are other additional parts of the correspondence which are more speculative but are generally acknowledged as such (e.g. complexity = something). And there are elements which are claimed to be "established beyond reasonable doubt", and yet I see no evidence for (and if anything, evidence against). Chief among them is the claim that because the CFT undergoes unitary evolution, the bulk quantum gravity theory must also be unitary, and therefore, black hole's preserve information. I see no reason for (i) and (ii) to imply this, and I see no evidence for this part of the AdS/CFT conjecture. On the contrary, it forces researchers to embrace all manner of perversions, from violations of the equivalence principle, to post-selection, to the absence of free will and local degrees of freedom. I suppose this was the cause of my Sunday morning rage-tweeting. If only I'd just gone for a walk….

4. **Andreas Karch**
   January 24, 2023

Hey Jonathan,

so we moved on to the long-text format. I think in your rant, I mean argument, you are getting yourself/us into a lot of trouble because you have a very old-fashioned notion of what states are in a theory of gravity. Diffeomorphisms are a gauge redundancy in a theory of gravity. Every statement that makes reference to local fields, local states, maybe "counts" how many states there are inside a black hole, is not really about physical states. Gauge invariant states in AdS are associated only with the boundary. It's almost true by tautology that the states of a theory of gravity in AdS are those of a CFT. The only questions is really "which CFT". I don't think anyone has come up with a better definition of what we would even mean by the states of quantum gravity in AdS other than boundary observables. This is the AdS analog of the statement that the only observables in a theory in asymptotically flat quantum gravity are S-matrix elements. Any local question in Minkowski space is gauge redundant.

You (and the papers you cite) are asking interesting questions about how local physics in AdS can arise as an approximate notion from the dual CFT perspective. There are lots of puzzles there. But I really don't see how this calls AdS/CFT into question. None of the things you call physical states in the bulk are physical.

And now I go hide in a hole …..

5. **Andreas Karch**
January 24, 2023

This is, by the way, also one of the main shortcomings of these tensor network models of AdS/CFT that are so popular these days. They capture many interesting aspects of fields on AdS space, except for the most important one: if we do gravity on AdS, none of the local states in the bulk is physical. They are all gauge redundant. Surely there is a lot one can learn from these toy models, but one shouldn't take them too seriously. It's a tall order having a toy model of quantum gravity that assumes gravity is not important.

(and now I should REALLY go hide in a hole ….)

6. **AR**
January 24, 2023

I would have said that the AdS/CFT correspondence is that any quantum gravitational theory in an anti-de-Sitter space of dimension D+1 is described by a non-gravitational quantum theory in dimension D. Fundamentally, this is because a gravitational theory is believed to not have gauge invariant local observables; the true gauge -invariant observables must therefore live on the boundary, The isometries of AdS ensure that this boundary theory is conformal.

I do think that this idea has not changed; the original correspondence between type IIB in AdS5 times S5, and N=4 SYM was only the best worked out case.

The more general correspondence leaves the nature of the boundary theory somewhat vague, and is therefore quite plausibly true. The more specific one becomes about the boundary theory, the more tests are possible. There are a few very specific duality conjectures, including the original one; I don't think the evidence for them is overwhelming.

Pace Jonathan Oppenheim, I think that if the boundary theory is unitary and the bulk theory is not, then this means that the AdS/CFT correspondence is manifestly false. But YMMV.

7. **Emil Martinec**
January 25, 2023

@Andreas, I think it is a bit too strong to say that gauge invariant states are associated only with the boundary. There are plenty of examples in AdS/CFT (e.g. the Lunin-Mathur and Lin-Lunin-Maldacena constructions) where a BPS spectrum is matched on both sides of the duality with exquisite precision. Specific BPS states in the CFT are mapped to specific BPS bulk geometries. Just because there is gauge symmetry (on both sides) doesn't mean that one is unable to describe physical states in both bulk and boundary descriptions, and say what they look like deep in the middle of AdS. Of course, when one gets away from situations where calculations on both sides of the duality are under control, one is unable to construct the map. That doesn't mean there isn't a bulk theory of quantum gravity, or that one must somehow refer all its physical properties to the conformal boundary of spacetime.

@Jonathan, it seems to me that much of your complaint stems from the fact that we don't understand much about the duality map as a detailed isomorphism in complicated dynamical situations, and that certainly includes those where black holes are involved. Even then, there are examples where one has confidence that the map is still valid. I am thinking specifically of AdS3/CFT2, where there is an infinite dimensional conformal symmetry whose representation theory highly constrains both sides. There is a moduli space of theories on both sides that includes a weak-coupling domain for the CFT where the Hilbert space is well-understood. The conformal symmetry continues to govern the asymptotic density of states through the Cardy formula over the entire moduli space, and it agrees with gravitational thermodynamics in the regime where gravity is weakly coupled. So either there is some non-unitary quantum theory of gravity which lives nowhere on this moduli space and whose thermodynamics has nothing to do with an underlying quantum statistical mechanics and its associated state-counting, or quantum gravity is indeed unitary and the thermodynamics does have such an underlying interpretation. In the latter case, it would be unreasonable to suppose there is no duality map, since the CFT is explicitly such a quantum system with exactly the same thermodynamics, with a precise match of BPS protected quantities, etc.

Many of your other quibbles with the correspondence have to do with more recent work much of which is on rather less sound a footing. I am not surprised if you are confused by it, since in some cases the authors themselves appear to be confused about how AdS/CFT works. If some of these ideas turn out to be wrong, it will not invalidate AdS/CFT but will rather tell us that certain ideas about the properties of the duality map are incorrect. I have not seen ANY evidence to date that suggests that the duality itself is in question.

@Peter, the most established part of the conjecture has to do with maps of protected quantities where (super)symmetry allows calculations on both sides of the duality that can be compared. In addition to those mentioned above, there are now quite sophisticated evaluations of the gravitational path integral that match the asymptotic series of BPS partition sums computed in the CFT. The semiclassical expansion of the gravitational path integral (with the sum over saddles generating subleading terms in the asymptotic expansion) takes one beyond the leading order gravitational thermodynamics to a fuller understanding of the correspondence. Here one is not asking gravity to describe microstates in the Hilbert space directly, that is likely the wrong thing to do (just as the terms in the asymptotic expansion of an elliptic function obtained by modular transform are not associated with individual terms in its original q-expansion); rather, gravity seems to know how to count states in aggregate (including subleading terms in the expansion) without resolving the microscopic details.

These calculations are not grand statements of principle, but rather detailed computations in specific examples where one can work out a match between the same quantity in AdS and CFT. Many checks of this sort have been done, amassing a great deal of evidence for the conjecture, but what the full scope of the duality is remains unclear. The most solid part of the correspondence is a list of explicit examples for which there is a wealth of supporting evidence. This success has led to a reasonable belief that any other example one can motivate as a limit of string theory will hold as well.

Where people seem to be running into trouble is in the arena of trying to abstract some grand principle that underlies and unifies (and perhaps extends) all the particular examples, in the process abandoning much of the string theory which makes the details work in the examples we know. One such is ER=EPR, but there are others (to paraphrase Groucho Marx). Many focus on toy models which may or may not contain the essential physics. Many have given up on understanding the bulk dynamics in favor of making inferences/conjectures about what the bulk physics must be like by analyzing the CFT, which ends up processing bulk physics through some poorly understood nonlocal map in a strongly coupled field theory; there seems to be rather less effort in the opposite direction (doing string theory in AdS), likely because the calculations are rather more difficult.

8. **Jonathan Oppenheim**
   January 25, 2023

   You can't hide in a hole Andreas, the boundary sees everything! 😉

   I think even you have to admit that this proposal that you're advocating along with Suvrat Raju and others, is very controversial within the string theory community. My impression is that it is a small minority of people who believe it, while the majority believe what you call the "old fashioned notion". But that could just be that I tend to hang out with a different crowd of string theorists…. Namely, those who believe that GR makes sense in our universe without a boundary, and "the value of the field where my finger is pointing at" is a diffeomorphism invariant statement.

   At any rate, that's besides the point, because the one thing that is obvious here, is that string theorists have extremely different opinions about what counts as a state of the bulk, and yet they all seem to agree that the AdS/CFT conjecture is obviously true. In the same way as all string theorists believe the black hole information paradox has been solved, but completely disagree on the solution.

   I find this tendency frustrating because I learn a lot from my string theory colleagues, but it is often made difficult by overly bold claims. And it is this sort of tendency which led to the wormhole-on-a-chip fiasco, and which distorts our view of which research directions are likely to be fruitful.

9. **Andreas Karch**
   January 25, 2023

   What I describe in my comments is much more basic than my work with Suvrat. The fact that there are no local gauge invariant observables in a theory of quantum gravity other than those defined at the boundary has been hammered away by people like Witten from the earliest days of AdS/CFT. Please don't mix this up with somewhat more conjectural statements about black holes. Local states in the bulk aren't physical. I don't think you'll find many people who disagree with this assertion. An interesting question is how to reproduce approximately local quantum field theory observables from the CFT.

10. **Andreas Karch**
    January 25, 2023

    PS: for the record, I am referring here to Witten's talk at the Strings 98 conference in Santa Barbara where he pointed out that the fact that there are no local observables in a theory of quantum gravity basically tells you that just by asking what you mean by gravity in AdS, the only observables we know how to define are correlation functions at the boundary. At this level, AdS/CFT is really a tautology. The fact that you find this even controversial is somewhat puzzling to me.

11. **Jonathan Oppenheim**
    January 25, 2023

    Thanks Andreas. Some genuine questions:
    1) Is there agreement that it's a tautology? What then do you make of https://arxiv.org/abs/2207.06536 which claims that there is not an isomorphism between local bulk and boundary states.

    2) When you say that the only observables we know how to define are correlation functions at the boundary, would you agree that there might be others that you don't know how to define (e.g. relating to observables inside a black hole)?

    3) There is a community of researchers who study relational observables in GR. E.g. "the value of the field where Alice's finger is pointing," is a diff invariant statement. I've always regarded this program as moreally equivalent to string theorists relating their observables to the boundary (just replace Alice with the boundary). Would you agree?

    4) I of course agree that there are diff invariant observables that you can relate to the boundary, e.g. the value of the field along a geodesic some proper time from the boundary. And via some extrapolate dictionary, reconstruct regions in the bulk from the boundary, But don't you think that https://arxiv.org/abs/1411.7041 successfully argues that you can get a potential violation of the no-cloning theorem, because naively, it appears you can reconstruct the same bulk observable from different parts of the boundary?

12. **Peter Woit**
    January 25, 2023

    Just a note that due to the way moderation of comments works, Emil Martinec's very helpful comment just now was posted, even though it is earlier in the time sequence.

13. **Andreas Karch**
    January 25, 2023

    Thanks Jonathan. I think your points 1, 2, and 4 are all about the question how apparently semi-classic physics in the bulk can arise within a large N CFT. There are open questions there. But this is a higher order question: if there are no local observables in a theory of quantum gravity, how come local QFT works so well to describe the experiences of a low energy bulk creature? We happily do experiments in labs and don't worry about this not being strictly defined an observable. But I don't think many would argue that this program contradicts the assertion that there are no local gauge invariant observables.

    Sometime it feels like rephrasing AdS/CFT in quantum information language, despite all its new insights, has made people forget some very basic lessons learned long ago.

    As to your 3, the relational observables, they are defined as integrals over all of space and as such only depend on boundary data. They are boundary observables that have the power to give you an approximate sense of local physics in the bulk. That's exactly the sort of thing I am talking about. This program accepts that there are no gauge invariant local observables and tries to do interesting physics with it.

14. **Jonathan Oppenheim**
    January 25, 2023

    Thanks Andreas. When I hear people invoke AdS/CFT, it is often to claim that the CFT is a quantum theory of gravity. And so for me, the question of how (or whether) semi-classical physics arises in the bulk, is the central question. In order for the statement that the CFT is quantum gravity to be a testable and meaningful statement, it must reproduce semi-classical gravity (or something which could reasonably be said to be the correct low-energy or classical limit). Otherwise, it is not a quantum theory of gravity, but a quantum theory of something else. That doesn't mean it can't teach us a lot about quantum gravity, but it wouldn't be quantum gravity.

    Since you call them second order questions, maybe they are impossibly to answer right now, and that's fine. But I feel like people act like it's settled, which is what I take issue with.

Regarding relational observables – I think we mostly agree here. I suppose I would say they can be defined in a space-time without a boundary and so I don't need a boundary to have gauge invariant observables which are the moral equivalent of local ones.

15. **Jonathan Oppenheim**
January 25, 2023

@Emil, thanks so much for such a detailed answer, much appreciated. Indeed, it is in these more complicated situations where I feel people's apparent certainty is less supported by evidence. Although these are complicated situations, they are important ones — the black hole information paradox & black hole thermodynamics seem to be the key questions for quantum gravity.

16. **AR**
January 25, 2023

In classical gravity, observables need to be diff invariant, but in quantum gravity, an observable is (expected to be) defined through an integral over all bulk metrics with some fixed boundary value. This is much more restrictive than the classical case. For example, "the value of the field along a geodesic some proper time from the boundary" cannot be an observable in QG; when the metric changes, the geodesics themselves change. It is for this reason that only boundary-to-boundary correlations are believed to be gauge invariant. I didn't think this was controversial, but things may have changed.

And as everybody has said, this doesn't mean that it is not interesting to talk about local bulk physics; we just have to be cognizant that this must be in some sense approximate.

17. **Emil Martinec**
January 25, 2023

@Jonathan, just to be clear: Among the complicated situations that ARE well-understood are BPS black holes with macroscopic horizon area, where there is a robust CFT state counting that agrees with the AdS gravitational path integral.

It is useful to divide the black hole information problem into two aspects: the information storage problem, and the information recovery problem. The fact that the BPS spectrum has been precisely matched tells us that AdS/CFT knows how to solve the information storage problem for this class of extremal black holes, in that on the CFT side of the duality one is explicitly counting the microstates, and the AdS gravity computation reproduces this counting at the level of an asymptotic expansion for large charge/mass.

The more complicated information retrieval problem where we throw something into such an extremal black hole and ask what comes back, and how it comes back, and which of our assumptions that lead to the information paradox is breaking down in AdS quantum gravity, is currently out of reach. Not because AdS/CFT is wrong, but because we currently lack the tools to ascertain what is going on on the AdS gravity side of the duality, let alone calculate correlators in a strongly-coupled CFT, or produce an explicit map between the two. But again, this is a statement about our poor understanding of the duality map, not about its existence.

I think most people assume that because we have all these precise tests of the things we can calculate, that the duality holds not only for symmetry-protected quantities but in general. Again, that it is unreasonable that it holds only for some class of states (which includes extremal black holes) but not others. And then the duality tells you that the information retrieval problem is solved in principle by the unitarity of the CFT under the assumption that the duality is an exact (if inexplicit) map.

I think this is where most people in the subject are at the moment — at some point you have done enough checks of the duality, assume it is true and ask what are the consequences. Much as people no longer question the truth of the Riemann hypothesis, and instead work out its implications. Which is not to say that it is unimportant to prove either conjecture; rather that until that day arrives there are useful consequences to explore assuming their respective validity.

18. **Emil Martinec**
January 25, 2023

@AR, good luck describing what is going on today say at the LHC using only the asymptotic data from the inflaton fluctuations in the far past which eventually assembled into it and its beams, and wherever those beams (and the atoms in the LHC) end up in the asymptotic future.

If (as some people were briefly fantasizing before the LHC turned on) the fundamental Planck scale had been sufficiently low for the LHC to have made small black holes, I suspect we would be trying to use a bulk quantum theory of gravity to calculate the scattering amplitudes for this process in a small spacetime region around the collision rather than worrying about where the LHC is hiding in the primordial inflaton wavefunction.

19. **Suvrat Raju**
January 26, 2023

Hi Jonathan,
I'm just entering the discussion so maybe I've missed a lot of the background. But just reading your first few comments. 1411.7041 doesn't say anything to the effect that the Hilbert space cannot be the same. The point is simply that approximately local operators in the bulk can be written in different ways on the boundary. More precisely, we have multiple operators \phi_{i} which have the property that within a low-energy set of states, the different \phi_{i} act in approximately the same way. This is quite well understood and this phenomenon occurs even about empty AdS. Where is the violation of no-cloning here?
The issue about the growing volume in the black-hole interior that leads to a seeming puzzle with the entropy is a more interesting problem. Mathur pointed it out several years back. But one popular proposed resolution is simply that states that *appear* to be independent in the bulk are not really independent. One kinematic observation is that this can happen quite easily in high-dimensional Hilbert spaces. See https://arxiv.org/pdf/2010.03575.pdf
In a Hilbert space of dimension e^{S}, with S >> 1, it is possible to find e^{S'} vectors (|\Psi_{i}>) where S' >> S which have the property that their mutual inner product || << 1 for all i,j.
There are many things we don't understand about the duality and about black holes. But I agree with Andreas. I don't think it is accurate to suggest that there are somehow well-known problems with the original formulation of AdS/CFT in terms of the equivalence of Hilbert spaces.

20. **Jonathan Oppenheim**
January 26, 2023

Thanks @Emil. Here I have to disagree. The fact that string theory gets the entropy right for a class of symmetry protected extremal black holes, does not imply that AdS/CFT implies that black hole evaporation has to be unitary. I can predict the entropy of a box of gas from it's microscopic or macroscopic properties, and that holds just as well in a unitary theory as well as an open system. I think it's perfectly reasonable to believe that black-hole evaporation is unitary for various other reasons. But that's very different from claiming that AdS/CFT tells us that black-hole evaporation is unitary (which is what is often claimed). In fact, I would say that AdS/CFT is either silent on this, or appears to lead to the opposite conclusion.

I agree that it is reasonable to assume the conjecture is correct and explore the consequences. I think that's a solid research direction to take. We do this all the time in quantum information theory and computer science, but we're always careful to append some line such as "assuming the polynomial hierarchy doesn't collapse." Even though in that example, we are virtually certain that it doesn't. This level of care is what I often find missing, and I do think it leads to genuine confusion, even amongst experts, as to what is solid and what is less solid.

In many cases, I feel that the conclusions that are drawn assuming the AdS/CFT conjecture would cause me to question the assumptions, at least in the particular setting under consideration. But I never see that happening, and so am struck by people's confidence.

21. **Emil Martinec**
January 26, 2023

Jonathan, I'm not sure what you have in mind. Among the BPS protected quantities are the operators that introduce and absorb (super)gravitons from the AdS conformal boundary, that can again be matched on both sides of the duality. For instance, such operators acting on the CFT vacuum/global AdS yield protected single-particle states. So we can begin with the AdS vacuum and send in some gravitons with enough energy (order n^2) that in the bulk description would make a black hole (this could even be two very high energy gravitons that in semiclassical gravity look like colliding shockwaves). On the CFT side, one has a strongly coupled field theory in which such excitations rapidly thermalize. On the gravity side, one has black hole formation. Now, I suppose one could try to claim that the same operator that makes a graviton excitation on the AdS vacuum does not do the same thing when acting on the single-particle excited state generated by the first graviton operator, but that seems rather unreasonable — the interpretation of the asymptotics of the operators (eg single-trace operators in N=4 U(n) SYM being dual to supergraviton creation/annihilation operators) is independent of the state they are acting on. The intermediate state is a microstate in the thermal ensemble of the CFT, and a black hole in the gravity theory, with energy of order n^2. The density of intermediate states is the same on both sides.* One can then de-excite this state long after thermalization by removing low-energy supergravitons near the boundary in the AdS description, dual to the same local single-trace operators in the gauge theory. The gauge theory description of this process is explicitly unitary. It has a dual AdS description in terms of incoming and outgoing supergravitons.

So as long as we stay outside the black hole and compare the two descriptions of the evolution, there seems to be a complete parallel. Of course, I'm not giving you the exact bulk description everywhere in the bulk because we don't have one at the moment. But the description of what's going on as seen from far away is built from parts of the duality that ARE well-understood.

Could you be a bit more explicit about what you find lacking in this line of reasoning? Do you think there are "hidden variables" that don't participate in the gauge theory description, but are there on the gravity side, that somehow invalidate the duality? The matching of the non-extremal entropy makes this unlikely. I'm puzzled as to why you say that AdS/CFT is silent or even negative on the issue of unitarity in black hole evaporation.

* There is a well-known factor of 3/4 between weak and strong coupling entropies in the gauge theory that is understood to be due to interaction corrections in the strongly coupled gauge theory. If this bothers you, we can work in the AdS3/CFT2 examples where the thermodynamics doesn't change between weak and strong coupling in the CFT2, due to the additional power of conformal symmetry in 2d.

22. **Jonathan Oppenheim**
January 27, 2023

@Emil, since we are talking about extremal or near-extremal black holes, and they don't really evaporate, to what extent can we even pose the question here, much less answer it? And even if we could pose the question, we can't check the other side of the duality, so is this not a question of faith?

In terms of what I find lacking, I would say that any attempt to reconstruct the bulk when we have a late-time evaporating black hole results in conclusions that would prompt me to reconsider the premise. The conclusions being either a break-down of the equivalence principle at low energies for large black holes, or the requirement for post-selection (which already signals a breakdown of unitarity) etc. There is no need to introduce hidden variables to consider a break-down of unitarity.

The need to consider island corrections to the entropy (because AdS/CFT seems to over-count the entropy of radiation), or the need to consider non-isometric bulk to boundary maps as in https://arxiv.org/abs/2207.06536 (and I see now Suvrat mentions https://arxiv.org/abs/2010.03575) seem to me to be symptoms of that. Which is not to say that it can't all be made to hold together, but only to say that at the moment, it doesn't. In my view we don't have a clue what is going on, and shouldn't claim otherwise.

@Suvrat, apologies, your comment was moderated so only appears now. Regarding no-cloning and error correction, this is the content of 1411.7041, and I mean no more than what is contained in that paper. My naive quantum information theorist's view of an error-correcting code is that one needs to encode the logical states in a larger physical Hilbert space. Is this still compatable with the Hilbert space's being equal?

Regarding the more important question of proposals to deal with the over-counting of states (as in 2207.06536 or 2010.03575): It is certainly possible that bulk states which we think of as being orthogonal are not actually orthogonal. But this seems unlikely to me, and one is introducing yet another radical concept to fix a problem with the original proposal. We also appear to require that states with high complexity do not have semi-classical bulk duals. These concepts may be required to save AdS/CFT in this regime, and I applaud them being explored. But if one keeps on having to introduce new mechanisms, then the project starts to look less elegant and less likely to be true. Which is not to say that it can't be made to work, only that if we need to keep introducing new mechanisms along our path, we might want to at least question whether we're heading in the right direction.

23. **Emil Martinec**
January 28, 2023

@Jonathan, a black hole in AdS can be made to evaporate by extracting energy at the conformal boundary by hand — there is a finite probability to find Hawking radiation at any radius, and we can collect it near the boundary and spirit it away. We are free to decide when and where we want to introduce the operators that accomplish this. On both sides of the duality, this corresponds to acting with field operators at the conformal boundary by a well-understood limiting procedure, which on the CFT side amounts to using local operators to extract energy from it. So as I said, one can make a black hole form by sending in quanta from the conformal boundary, let the system thermalize, wait as long as you want, and then extract energy at the conformal boundary. So yes, we can pose the question.

I have been trying to argue that all of this is much more than a matter of faith, rather it is a reasonable hypothesis supported by mountains of evidence. And that evidence points to the existence of a unitary bulk theory of quantum gravity. Of course there is more to be understood about black hole interiors, where/how does the Hawking calculation go wrong, etc. The information problem isn't solved (claims to the contrary in the popular press notwithstanding).

The utility of any duality is that it provides an alternate route for calculation; one accepts the hypothesis and then uses whichever side of the duality is simpler to analyze for the question at hand. Otherwise, what is the point? To do every calculation twice because we might find the N+1st check of the duality invalidates it? So perhaps the more germaine issue might be why, if one is interested in answering questions about physics inside the horizon, one wants to attack that problem by using the CFT side of the duality, where it is filtered though all sorts of ill-understood, nonlocal properties of the duality, and moreover in a strongly-coupled field theory. I would have thought using the side of the duality where quasi-locality in the bulk doesn't need to be "reconstructed" would be more appropriate.

You mentioned recent proposals about islands, non-isometric codes, etc. They are a work in progress, the sort of toy model that I was referring to as perhaps having discarded essential ingredients. There is no "need" to consider any of them; and they may very well be wrong-headed. They are examples of what I was calling attempts to formulate grand principles — models and interpretations layered over the basic facts of AdS/CFT. And they are not guaranteed to be correct. In some cases, they represent a radical departure from what had seemed to be settled about how AdS/CFT works. And if indeed they turn out to be red herrings, the basic facts of AdS/CFT will still stand.

I will add one more option to the menu, which obviates all the discussion about islands, old black holes, bags of gold, overcounting/non-orthogonality of states etc. We have several examples of non-geometric phases of string theory, where gravity is not the most important actor and its effects are overwhelmed by the plethora of other degrees of freedom in string theory. Examples include the high-temperature Hagedorn phase, Landau-Ginsburg phases of string compactifications, and the formation of "giant gravitons". In AdS/CFT, gravity is a set of collective modes, order N^0 among N^2 degrees of freedom. In geometric phases, most of the N^2 degrees of freedom are confined/not activated/in their ground state. In AdS/CFT, the formation of a black hole involves on the CFT side a deconfinement transition. It might then be that on the AdS side, the horizon is a phase boundary, and there is no geometric interior (the analytic continuation of the vacuum geometry past the horizon presumes that the N^2 degrees of freedom are still confined there).

Much of the confusion at the moment stems from trying to hold onto a geometrical picture of the black hole interior, and presuming that the only place we need strings and branes is well inside the horizon near the singularity where tidal forces are large. We then get painted into a corner where all of a sudden we are invoking effects over much larger distances than the horizon scale in order to rescue the theory (like saying that the black hole interior at late times is non-locally encoded in the early radiation arbitrarily far away). So the lack of a geometrical interior is a possibility which bypasses many of the things you find objectionable, though there is plenty to object to about this alternative as well. Anyway, best to shut up and figure out a way to calculate, if we can.

24. **Peter Shor**
January 28, 2023

@Emil Martinez: If there is no "need" to consider any of these unconventional proposals about islands, non-isometric codes, etc., why are physicists considering them, and claiming in their papers that they solve certain problems that arise in AdS-CFT? I have too much respect for the authors of these papers to believe that they are introducing these seeming contrived ideas solely to lengthen their publication lists.

So I can only conclude that AdS-CFT is a conjecture with many unanswered questions about how it can be applicable to our universe.

25. **anonymice**
January 29, 2023

Daniel Harlow and Daniel Jafferis claim that Jackiw-Teitelboim gravity is a quantum gravity in 1+1D AdS space which has no CFT dual:

https://arxiv.org/abs/1804.01081

26. **Peter Woit**
January 29, 2023

Thanks to all who have provided some useful explanations about AdS/CFT. I feel I'm still though lacking what I'm looking for, a specific general conjecture about which two theories are in duality. More specifically:

I see the claim for N=4 SYM in d=4 and its supposed relation to a string theory/supergravity low energy limit, compactified on S^5. I also can see how different compactifications of this in different dimensions will give different pairs of conjecturally dual theories. But:

1. In dimensions above 3/2 does the conjecture go beyond this and how do I know what the paired dual theories are?
2. In dimension 2/1 and 3/2, are the dual pairs supposed to be related by compactification to the higher dimensional story?
3. In 3/2 d, writing gravity in CS form, there's a clear analog of the usual Chern-Simons story relating 3d gauge theory to 2d CFT on the boundary. What is the relation of this to AdS3/CFT2?
4. What exactly is the current best statement of how AdS/CFT relates 2d JT gravity to 1d SYK (presumably addressing the issue in the Harlow/Jafferis paper linked above)?

27. **JT gravity**
January 29, 2023

I believe the "best" (in the sense of being as close to the rigor of mathematical physics as it is possible in the present state of our understanding of QFT and string theory) work along the lines of proving the AdS3/CFT2 correspondence is currently due to Rastelli's and Gaberdiel's school. More precisely, this school is trying to show that type IIB string theory on AdS3 × S3 × M4 with p units of NS flux contains an integrable subsector, isomorphic to the minimal (p, 1) bosonic string. Some relevant papers:

https://arxiv.org/abs/hep-th/0507037
https://arxiv.org/abs/1011.2986
https://arxiv.org/abs/1207.6697

For some recent work on the original form of the Maldacena's proposal, using the pure spinor formalism, see:

https://arxiv.org/abs/1903.08264

28. **Emil Martinec**
January 30, 2023

@Peter:

1. Dual pairs can be constructed by taking low-energy limits of brane configurations in string theory. The basic idea is that the low energy limit of the open string theory on the branes is a gauge theory coupled to matter. On the gravity side, the low-energy limit is the near-source geometry of the branes. The brane construction determines the field content and then one analyzes the field theory RG to determine whether it is (i) scale invariant or (ii) has an interesting IR fixed point. At large N this is dual to (i) a self-similar geometry, or (ii) to a non-scale invariant geometry with an AdS limit as one scales it toward the brane source. If the field theory happens to be scale-invariant from the start, then the geometry will have the AdS form.

1a. For instance, the IR limit of N D2-branes is maximally supersymmetric gauge theory in d=3. This theory is not conformal, but has a nontrivial IR fixed point that is dual to M-theory on AdS4xS7. It was subsequently realized that this theory and many other AdS4/CFT3 dual pairs can be realized as Chern-Simons gauge theories coupled to matter, going by the name of the ABJM construction and various elaborations of it.

1b. The IR limit of fivebranes yields a nontrivial 6d SCFT with (2,0) superconformal symmetry, dual to AdS7xS4. Webs of intersecting fivebranes yield 5d SCFT's dual to a variety of examples of AdS6/CFT5. So there are examples in all dimensions where there is field-theoretic superconformal symmetry.

2. Compactification of the CFT does not usually give lower-dimensional correspondences, rather one typically intersects more branes. For instance, maximally susy AdS3/CFT2 comes from intersecting two stacks of three-branes, rather than by compactifying a single stack of three-branes. The 2d SCFT lives at the intersection. There is now a rich zoology of such constructions with less supersymmetry.

3. First let's review how things work in 2d JT gravity, which can be written as a BF topological gauge theory for SL(2,R). Because it has no bulk dynamics, the dynamics

can be pushed onto a set of edge modes, whose action is the Schwarzian (the natural action of coadjoint orbits of Diff(S^1)/SL(2,R). This story lifts to 3d Chern-Simons gravity, whose only dynamics is again that of boundary diffeomorphisms, governed by a 2d version of the Schwarzian theory originally studied by Alekseev-Shatashvili in 1988 and derived in this context by Cotler-Jensen 1808.03263. Note that this is not the CFT of the duality, rather it is simply a generating function for stress tensor Ward identities and does not realize the BTZ black hole density of states.

@anonymice, the version of this statement for the JT gravity/Schwarzian theory is the content of the paper you referenced. JT gravity and the Schwarzian theory have almost no states and so do not realize (n)AdS2 BH microstates; but because gravity knows about BH thermodynamics, it can be used to compute the BH density of states (as usual, the thermal free energy is the gravitational action).

4. The Schwarzian theory can be derived as the dynamics of a set of collective modes of the SYK model via a standard set of standard manipulations, but SYK has more content than just these collective modes — for instance it actually has 2^N states while the Schwarzian has just a few. The Schwarzian theory is equivalent to JT gravity — the latter, being topological, can be reduced to a set of edge modes. Then one could ask whether there is some sort of 2d theory that is exactly dual to the 1d SYK. It is unlikely that there is a quasi-local bulk theory exactly dual to the entire SYK model, in contrast to the examples derived from string theory where it is believed that string theory in the bulk and the large N CFT are exactly dual. At the very least, such a theory would be non-local at the AdS scale.

One might say (and has) that gravity a thermodynamic collective field theory, while string theory realizes the underlying statistical mechanics. AdS/CFT makes this more than a slogan, by relating string theory in AdS to a standard quantum system with a standard quantum statistical mechanics. The astonishing fact about gravity is that it knows about the BH equation of state without knowing much about the microstates; whereas in conventional systems, one has to have the microphysics in hand first, and use it to derive the equation of state in order to then start doing thermodynamics.

29. **David Roberts**
January 30, 2023

> The utility of any duality is that it provides an alternate route for calculation; one accepts the hypothesis and then uses whichever side of the duality is simpler to analyze for the question at hand. Otherwise, what is the point? To do every calculation twice because we might find the N+1st check of the duality invalidates it?

Well, either there is a proof, or it might be that the duality doesn't actually hold in all conceivable cases. It is thus a conditional calculation. Or else you are making a definition that the result of what is happening on one side is literally the thing that happens on the other, and no one has checked that this is consistent with the definition not using the duality.

It's not about doing calculations twice, but actually proving conjectures. Is there some solid statement of some conjecture here that is proven, with the hypotheses of some proof of at least a wide class of special cases spelled out explicitly? Otherwise I fail to see how a conjectured mechanism that has only been calculated in a bunch of cases, but not proven for all the cases people care about, can be considered anything other than an open problem. Experimental mathematics is full of examples of patterns that fail to continue, and famously some of them continue for ranges that make the volume of the universe in Planck volumes look approximately zero.

30. **Jonathan Oppenheim**
January 30, 2023

@Emil, as far as I understand the process you're describing, you are inserting operators into the action, and thus changing the physical content of the theory during evolution? This doesn't seem to have much to do with black-hole evaporation, and I'd be loath to draw any conclusions about that. But I'd be happy to come away from this discussion with some reading homework if you have suggestions. It sounds like a check of the partition function, which I am not challenging.

I think evaporating black holes is more than just another N+1 check. And this is made apparent by the fact that when one tries to reconstruct the bulk using the standard rules one gets nonsense, unless one adopts the various proposals (island formula, non-isometric codes) that have recently been put forward. Maybe I'm biased, being part of It from Qubit, but I am with @Peter here — these proposals are being considered because we are being forced to — otherwise there are paradoxes. And whether these new proposals can be made consistent is very much a great challenge and an open question.

31. **Emil Martinec**
January 30, 2023

@Jonathan, no, I am not changing the action or the Hamiltonian of the CFT. The suggestion is to compute correlators in the same theory, with the insertion of a few operators at early times that make black holes as intermediate states, and then at late time insert many operators that gradually collect the Hawking radiation. The procedure for introducing/extracting such quanta goes back to the classic papers of Gubser-Klebanov-Polyakov hep-th/9802109 and Witten hep-th/9802150. That a small number of such insertions can make a black hole is exemplified by calculations in AdS3 of two energetic particles making a BTZ black hole, see for instance gr-qc/9809087. The same operators, looking at much lower energy modes, can be used at late times to collect the radiation.

A closely related procedure is to couple the CFT to a bath (which indeed involves a modification of the CFT Hamiltonian by a term that couples the same local CFT operators discussed above to creation operators for bath excitations) and thereby transfer energy from the CFT to the bath. If we turn on such a coupling adiabatically at very late times, it does the same thing as I was describing above — remove Hawking radiation from the CFT.

As to your last comment, I think we're largely in agreement that there is a problem somewhere; we just disagree on whether the takeaway is that AdS/CFT duality is wrong or whether people are making incorrect inferences about the duality map.

This Week’s Hype

January 24, 2023
Categories: This Week's Hype

The New York Times today has Where is Physics Headed (and How Soon Do We Get There?). It’s an interview by Dennis Overbye of Maria Spiropulu and Michael Turner, the chairs of the NAS Committee on Elementary Particle Physics – Progress and Promise. This committee is tasked with advising the DOE and NSF so they can “make informed decisions about funding, workforce, and research directions.”

The transcript of the interview is rather bizarre, for several reasons. Spiropulu, probably the main person responsible for the recent wormhole publicity stunt, is here the voice of sober reason:

> **Overbye**: String theory — the vaunted “theory of everything” — describes the basic particles and forces in nature as vibrating strings of energy. Is there hope on our horizon for better understanding it? This alleged stringiness only shows up at energies millions of times higher than what could be achieved by any particle accelerator ever imagined. Some scientists criticize string theory as being outside science.
>
> **Spiropulu**: It’s not testable.

whereas Turner (an ~~astronomer~~ astrophysicist with no particular background in mathematics) is a big fan of string theory as mathematics:

> **Turner:** But it is a powerful mathematical tool. And if you look at the progress of science over the past 2,500 years, from the Milesians, who began without mathematics, to the present, mathematics has been the pacing item. Geometry, algebra, Newton and calculus, and Einstein and non-Riemannian geometry.
> …
> We will have to wait and see what comes from string theory, but I think it will be big.

On the topic of particle physics and unification, there’s

> **Overbye**: You’re referring to Grand Unified Theories, or GUTs, which were considered a way to achieve Einstein’s dream of a single equation that encompassed all the forces of nature. Where are we on unification?
>
> **Spiropulu**: The curveball is that we don’t understand the mass of the Higgs, which is about 125 times the mass of a hydrogen atom.
>
> When we discovered the Higgs, the first thing we expected was to find these other new supersymmetric particles, because the mass we measured was unstable without their presence, but we haven’t found them yet. (If the Higgs field collapsed, we could bubble out into a different universe — and of course that hasn’t happened yet.)
>
> That has been a little bit crushing; for 20 years I’ve been chasing the supersymmetrical particles. So we’re like deer in the headlights: We didn’t find supersymmetry, we didn’t find dark matter as a particle.

Turner makes the case one often hears these days from string theorists: the field may have given up on unification, but it has moved on to something much less boring:

> **Turner**: I feel like things have never been more exciting in particle physics, in terms of the opportunities to understand space and time, matter and energy, and the fundamental particles — if they are even particles. If you asked a particle physicist where the field is going, you’d get a lot of different answers.
>
> But what’s the grand vision? What is so exciting about this field? I was so excited in 1980 about the idea of grand unification, and that now looks small compared to the possibilities ahead.
> …
> **Turner**: The unification of the forces is just part of what’s going on. But it is boring in comparison to the larger questions about space and time. Discussing what space and time are and where they came from is now within the realm of particle physics.
>
> From the perspective of cosmology, the Big Bang is the origin of space and time, at least from the point of view of Einstein’s general relativity. So the origin of the universe, space and time are all connected. And does the universe have an end? Is there a multiverse? How many spaces and times are there? Does that question even make sense?
>
> **Spiropulu**: To me, by the way, unification is not boring. Just saying.

The problem with the idea that we’ve moved on to a new, far more exciting time in physics, devoted to replacing conventional space-time and exploring the multiverse, is that there’s no actual way to do experiments about any of this (other than the wormholes…). If this is the vision of the coming NAS report, a possible response from the DOE and NSF may be “That’s nice, we can now shut down all those expensive labs and experiments doing the boring stuff and focus on investigating the wormholes that Google’s quantum computer is producing for us.”

Update: Ars Technica has some refreshing anti-hype: Requiem for a string: Charting the rise and fall of a theory of everything, with subtitle “String theory was supposed to explain all of physics. What went wrong?”

It’s a good explanation of what went wrong with string theory, although one might point out that pretty much the same story was first explained by others in detail 20 years ago.

There definitely seems to be a recent trend in popular science articles to finally admit that string theory unification may simply be a failed and now dead idea. This article gets brutal at times:

> The dearth of evidence has slaughtered so many members of the supersymmetric family that the whole idea is on very shaky ground, with physicists beginning to have conferences with titles like “Beyond Supersymmetry” and “Oh My God, I Think I Wasted My Career.”

Comments

1. **Felix**
   January 24, 2023

   I don’t think Michael Turner is an astronomer. As far as I can tell, he’s worked on things like axions and theoretical cosmology which are much more string theory adjacent. Unless he routinely works in magnitudes, he’d be an astrophysicist at best.

2. **Peter Woit**
January 24, 2023

Felix,
Agreed that astrophysicist is more accurate than astronomer, I changed the posting to reflect this. I noticed that in 2010 a UC press release described him as the APS's "first astrophysicist president"
https://news.uchicago.edu/story/michael-s-turner-become-american-physical-societys-first-astrophysicist-president

In any case, I see nothing in his research work or educational background to indicate any expertise in the mathematics of string theory.

3. **Georges E. Melki**
January 24, 2023

What I liked best: Turner's "Einstein and non-Riemannian geometry"!!LOL

4. **Sabine Hossenfelder**
January 25, 2023

Just on the off chance that someone reading this still doesn't know. The claim that "we don't understand the mass of the Higgs boson" is an argument from naturalness. I explained in my book "Lost in Math" why that's not a scientific argument. That it's not scientific is why the "predictions" that supersymmetry should show up at the LHC turned out to be wrong.

The statement that the Higgs mass is "unstable" is bluntly wrong. Like a lot of people in the community, she is confusing as physically possible change of parameters (that can be used to identify an instability) with a physically impossible change, which is the type of change that is used in naturalness arguments. This alleged "instability" is just a property of the theory. There is nothing problematic about it. There's also nothing in need of explaining because we know where it comes from, it comes from the Higgs being a scalar. I believe particle physicists for the most part don't know what they mean by "explaining" in the first place.

Doesn't help that the words "naturalness" and "finetuning" are used with different meaning in different parts of the foundations of physics, which I would have written a review paper about if the DFG hadn't refused to finance me, which they did, so now I'm just leaving comments on blogs to complain about what other people are doing.

It's extremely worrisome that despite the fact that naturalness turned out to be wrong, particle physicists still insist on using it.

5. **Robert A. Wilson**
January 25, 2023

Perhaps non-Riemannian geometry will explain Einstein's theories better than the Riemannian sort?! Perhaps that's what's missing from quantum gravity…

6. **Peter Woit**
January 25, 2023

George E. Melki, Robert A. Wilson,

While "Einstein and non-Riemannian geometry" is considered by most to have been a failed unification research program, maybe Turner is right that non-Riemannian geometry is the way to go. Perhaps he's referring to attempts to interpret one of the factors in the euclidean 4d rotation group in terms of internal symmetry, giving a non-Riemannian sort of geometry.

7. **Marvin**
January 25, 2023

Peter W

We can look at the simpler possibility that Turner make a lapsus in his enumeration: "Geometry, algebra, Newton and calculus, and Einstein and non-Riemannian geometry", have to be corrected as : "Geometry, algebra, Newton and calculus, and Einstein and non-Euclidian geometry."

8. **Martin**
January 26, 2023

I think Einstein Non-Riemannian was meant as General Relativity and Non-Riemannnian singniture.

9. **Peter Woit**
January 26, 2023

Marvin/Martin,
My comment about this was meant as a joke. Surely Turner meant non-Euclidean or maybe pseudo-Riemannian. The striking thing about all his comments was that he appeared to have no idea what he was talking about (a main direction for HEP physics is to decide about the multiverse?????). I suspect people in HEP are pretty appalled that this is the way their field is being promoted to the public and that a chair of a committee determining their future is saying these kinds of things.

10. **Robert A. Wilson**
January 26, 2023

My comment was also meant as a joke, of course. I hope no-one took it seriously.

11. **Jim Holt**
January 27, 2023

A propos, I found this comment (from a youtube video on cosmology) to be perceptive:

"Sideshow Bob
3 days ago
Everyone has known for decades – if you want to truly explore multi verses, you need to repair your toaster with parts from the nuclear power plant."

12. **Ted Rogers**
January 28, 2023

From the Ars Technica article:

“Recently, theorists have proposed another duality, the AdS/CFT correspondence.”

Recently!?

Various and Sundry

February 7, 2023
Categories: Uncategorized, Wormhole Publicity Stunts

A few things that may be of interest:

- Fermilab is continuing to push the wormhole publicity stunt, with Joe Lykken, the lab's Deputy director for research on the 17th giving a public lecture on Wormholes in the Laboratory. The promotional text goes way out of its way to mislead about the science:

  > A wormhole, also known as an Einstein-Rosen bridge, is a hypothetical tunnel connecting remote points in spacetime. While wormholes are allowed by Albert Einstein's theory of relativity, wormholes have never been found in the universe. In late 2022, the journal Nature featured a paper co-written by Joe Lykken, leader of the Fermilab Quantum Institute, that describes observable phenomenon produced by a quantum processor that "are consistent with the dynamics of a transversable wormhole." Working with a Sycamore quantum computer at Google, a team of physicists was able to transfer information from one area of the computer to another through a quantum system utilizing artificial intelligence hardware.

  The "utilizing artificial intelligence hardware" seems to be an incoherent attempt to add more buzzwords to the bullshit. If you know anyone with any influence at the lab, you might want to consider contacting them and asking them to try and get this embarrassment canceled.

- On another embarrassment to science front, Fumiharu Kato on Twitter is announcing the publication of the paperback edition of his book promoting the IUT proof of the abc conjecture. In his talk about this at the Simons Center he dealt with the issue of the problems with the proof by pretending they don't exist, but (from what I can make out using Google Translate), he says he'll deal with this in the paperback edition. His point of view seems to be that once PRIMS (chief editor S. Mochizuki) decided to accept the IUT papers, no one should be writing things like this. Perhaps he's just trying to point out that this is potentially a huge embarrassment for PRIMS and RIMS in general, which is undeniable. But he appears to be going down the truly unfortunate path of making this not about mathematics but about Japanese national honor, with one tweet getting translated as:

  > Some non-Japanese mathematicians questioned, "This is an insult to the Japanese mathematics world! Why don't Japanese mathematicians say anything after being so insulted?" I also think the question is valid.

- Continuing with the difficult and depressing, there's the ongoing Russian war of aggression in Ukraine. The New York Times reports on the efforts of the Simons Foundation help sustain Ukrainian science. The Guardian has an excellent article on the problems the LHC experiments are having due to the fact that Russian physicists make up a significant part of the collaborations. I had heard this story back in September from John Ellis, who I met for the first time in London (at an event which now has a video of a discussion I was involved in). Tommaso Dorigo has an article about this on his blog, where he takes a point of view that is appealing (no borders or nationalism in science), but I don't think it's so simple.

  I've been wondering if there is a historical parallel to look to, with one possibility the situation in 1938-39 when Hitler invaded Czechoslovakia. By this point (as now) a lot of scientists had fled to the West, and the issue must have arisen of how scientists in the West should deal with their German colleagues who were staying in Germany.

- Turning to something much more pleasant, Michael Harris points me to a video of a talk by Manjul Bhargava that has finally appeared, one of a series of talks at a 2018 conference in honor of Barry Mazur.
- This week in my graduate class I'm talking a bit about Howe duality, and just discovered that his original unpublished articles on the subject are now available online, see here and here.
- Finally, I only recently learned about the volume of Sidney Coleman's correspondence that recently appeared, under the title Theoretical Physics in Your Face. Especially fun to read for those like me who remember the era at Harvard when Coleman was at the center of activity. One quote, his opinion in 1985 evaluating the grant to the Princeton theory group:

  > If I have any serious criticism of this group at all, it is that their recent concentration on superstrings seems to me a tactical error, too much devotion of effort to a line of development that (at least to an outsider's eye) is not that promising. However, I could well be wrong in this, and, even if I am right, they'll soon discover they've drilled a dry hole and be off exploring other fields next year.

  Unfortunately the last part of this was very wrong…

Update: Nature has an article on the resolution of the LHC Russian authorship issue.

Comments

1. **Douglas Natelson**
   February 7, 2023

   One of the best math talks I ever heard was by Manjul Bhargava, on the 15 theorem (https://en.m.wikipedia.org/wiki/15_and_290_theorems). Just outstanding for a non-expert.

2. **David Roberts**
   February 7, 2023

   > A wormhole, also known as an Einstein-Rosen bridge, is a hypothetical tunnel connecting remote points in spacetime. While wormholes are allowed by Albert Einstein's theory of relativity, wormholes have never been found in the universe.

   Doubling down on linking the "wormhole" in at best a 1+1D Lorentzian manifold to General Relativity and our universe and spacetime, even though one might argue that reading the text literally, they aren't saying this outright.

3. **Michael Weiss**
   February 8, 2023

   It is remarkable to realize that as far back as the mid-1970s Prof. Sidney Coleman included Bell's Theorem as part of his introductory undergraduate course on QM ("Physics 143" at Harvard). It was a serious yet "for fun" lecture at the end of the syllabus, not to be included on the final exam. Even as Sidney always instilled a sense of wonder and elegance, his presentation of Bell's ideas communicated its fundamental importance–the opposite of "shut up and calculate." It might be that Sidney was the first physicist nationally to include Bell in an otherwise standard sophomore course. How he would have appreciated this year's Nobel Prize in Physics (his late Harvard colleague Prof. Francis Pipkin was an early advocate of Bell-inspired experiments).

4. **Anon**
   February 13, 2023

   The Coleman book has the worst copy editing I've ever seen. The first chapter (available on World Scientific's website) has numerous occurrences of "Seldom Glasgow".

5. **Peter Woit**
   February 13, 2023

   Anon,

   I do think Sidney would have appreciated the humor of that renaming of Glashow.

6. **Anon**
   February 17, 2023

   Thanks a lot for pointing out the two Howe papers – never knew these existed, and have only ever seen his two 1989 papers on the topic. Really interesting.

7. **anon. fool**
   February 27, 2023

   "… the issue must have arisen of how scientists in the West should deal with their German colleagues who were staying in Germany …"

   This is a well known and deeply unpleasant story. It was a question of whether the German government permitted them to continue their research, or drove them out for political views or race, or even for having the temerity to try to help those less fortunate. By that time, the Germany government was forcing every profession to prepare for war so pure science without military uses had little respect or funding, although in October 1938 Gerhard Schrader came up with nerve agents tabun and sarin, and in December 1938 Otto Hahn and Fritz Strassmann discovered uranium fission. The Nazis put Heisenberg on charge of nuclear research, and he visited Bohr in Copenhagen in September 1941, but Bohr declined to help the Nazis and fled to the UK and then USA.

   I think and hope that unless further escalation occurs, a FAR better analogy to the present situation is given by Andrei Sakharov's Memoirs (Knopf 1990), which details the gulag and psychiatric treatment provided by the CCCP for dissidents.

   Sakharov was exiled with his wife to Gorky by Brezhnev for criticising the latter's decision to invade Afghanistan at the end of 1979. He was there relentlessly persecuted by the KGB and went on repeated hunger strikes for 7 years until Gorbachev released him. His statement of 27 January 1980 (Appendix B of his Memoirs, pp673-5):

   "On January 22, I was detained on the street and taken by force to the USSR Procurator's office … I was asked to return the medals and orders and certificates … Rekunkov also informed me of the decision to banish me to the city of Gorky, which is closed to foreigners … I was instructed to report three times a month to the police … The authorities are completely isolating me from the outside world. The house is surrounded 24 hours a day by police and the KGB, who keep away all visitors, including my friends. Telephone connections with Moscow and Leningrad are cut off. We have not even been able to call my wife's mother … Even in prison, there is more possibility of communication with the outside world …

   "The worsening of the international situation was caused by the following actions of the USSR … Supporting terrorist regimes … Supporting the actions of quasi-governmental terrorists in Iran who have violated diplomatic immunity … the invasion of Afghanistan … against the Afghan people."

8. **Justcurious**
   March 14, 2023

   Next week there's the opening workshop of a new big research network between universities in France and Japan and seems to be watchable by zoom, but only by members https://ahgt.math.cnrs.fr/activities/workshops/AHGT-2023/

   As an outsider, most talks seem to be uncontroversial mathematics including well-known people like Yves André etc. But on Tuesday 22 one of the speakers will be Shinichi Mochizuki about IUT (he is a member of that network), just before the session free discussion. It might be interesting, a pity it isn't public.

What’s Going Right in Particle Physics

February 14, 2023
Categories: Uncategorized

Since I had a little free time today, I was thinking of writing something motivated by two things I saw today, Sabine Hossenfelder’s What’s Going Wrong in Particle Physics, and this summer’s upcoming SUSY 2023 conference and pre-SUSY 2023 school. While there are a lot of ways in which I disagree with Hossenfelder’s critique, there are some ways in which it is perfectly accurate. For what possible reason is part of the physics community organizing summer schools to train students in topics like “Supersymmetry Phenomenology” or “Supersymmetry and Higgs Physics”? “Machine Learning for SUSY Model Building” encapsulates nicely what’s going wrong in one part of theoretical particle physics.

To begin my anti-SUSY rant, I looked back at the many pages I wrote 20 years ago about what was wrong with SUSY extensions of the SM in chapter 12 of *Not Even Wrong*. There I started out by noting that there were 37,000 or so SUSY papers at the time (SPIRES database). Wondering what the current number is, I did the same search on the current INSPIRE database, which showed 68,469 results. The necessary rant was clear: things have not gotten better, the zombie subject lives on, fed by summer schools like pre-SUSY 2023, and we’re all doomed.

But then I decided to do one last search, to check number of articles by year (e.g. search “supersymmetry and de=2020”). The results were surprising, and I spent some time compiling numbers for the following table:

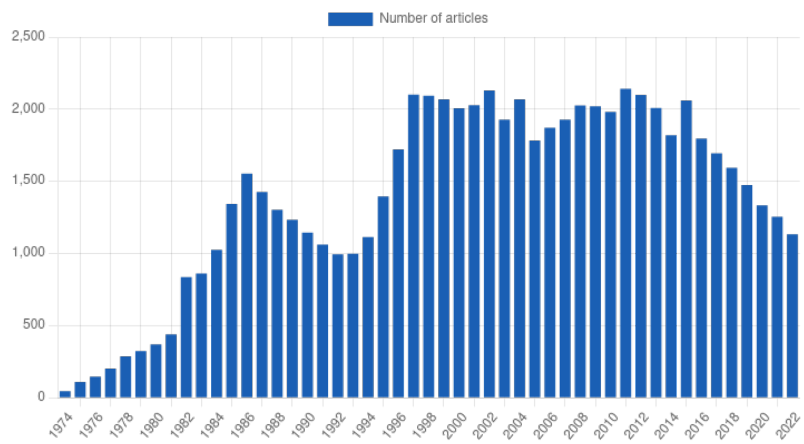

These numbers show a distinct and continuing fall-off starting after 2015, the reason for which is clear. The LHC results were in, and a bad idea (SUSY extensions of the SM) had been torpedoed by experiment, taking on water and sinking after 20 years of dominance of the subject. To get an idea of the effect of the LHC results, you can read this 2014 piece by Joe Lykken and Maria Spiropulu (discussed here), by authors always ahead of the curve. No number of summer schools on SUSY phenomenology are going to bring this field back to health.

Going back to INSPIRE, I realized that I hadn’t needed to do the work of creating the above bar graph, the system does it for you in the upper left corner. I then started trying out other search terms. “string theory” shows no signs of ill-health, and “quantum gravity”, “holography”, “wormhole”, etc. are picking up steam, drawing in those fleeing the sinking SUSY ship. With no experimentalists to help us by killing off bad ideas in these areas, some zombies are going to be with us for a very long time…

Update: There are a couple things from Michael Peskin that are relevant to this that might be of interest: an extensive 2021 interview, and a very recent “vision statement” about particle physics. Peskin’s point of view I think is exactly what Hossenfelder is arguing against. He continues to push strongly for a very expensive near-term collider project, and doesn’t seem to have learned much of anything from a long career of working on failed ideas. I remember attending a 2007 colloquium talk here at Columbia where he put up a slide showing all the SUSY particles of masses a few hundred GeV that the LHC was about to find, and assured us that over the next 5-10 years we’d be seeing evidence of a WIMP from several different sources. According to the interview, in 2021 he was working on Randall-Sundrum models (see here) and wondering about the “little hierarchy problem” of why all the new physics was just above the LHC-accessible scale rather than at the electro-weak breaking scale. I very much agree with some of his vision statement (the Higgs sector is the part of the SM we don’t fully understand, and the argument for colliders is that they are the only way to study this), but his devotion to failed ideas (not just Randall-Sundrum, but string theory also) as the way forward is discouraging. In the interview he admits that it’s looking like post-LHC abandonment of particle physics is the most likely future:

> I think if you were a betting man, you would bet that LHC will be the last great particle accelerator, and the whole field will dissipate after the LHC is over.

Most of his prominent colleagues have the same attitude that the subject is over, and have already moved on, unfortunately to things like creating wormholes in the lab.

Comments

1. **Interested amateur**
   February 14, 2023

   It looks to me that your graph clearly shows the fall started in 2012, a huge year for the LHC, with a hopeful blip in 2016, one year after the LHC restarted in 2015.

2. **S**
   February 15, 2023

   Perhaps the experimental result of thousands of theorists coming to the ends of their career and realizing they have nothing to show for it will eventually start to tell….

3. **Sabine Hossenfelder**
   February 15, 2023

   Hi Peter,

   Thanks for digging up these numbers, which is very interesting. A lot of people in hep-ph have been shifting weight to axion-like particles and general hidden sector things, branching out into astroparticle physics and sprinkling machine learning over it for good measure.

Unfortunately, that doesn't solve the problem in the community, it just moves it elsewhere. Since they never acknowledged that their LHC predictions went badly wrong, they're doomed to repeat their mistakes.

The situation in particle physics is very comparable to the reproducibility crisis which they had in psychology. Both were driven by bad scientific methodology that had become normalized. Psychologists managed to mobilize the community and introduce better methodology. And, amazingly enough, this was a bottom-up effort, purely community driven.

I sincerely hope that particle physicists can get their act together and do the same thing. A lot could be gained in this community if everyone had a basic education in the philosophy and sociology of science. And yeah, it would to hurt for a few years, but I believe that particle physics stands much to gain from it.

4. **Tom WEIDIG**
February 15, 2023

Whatever mistake or bad strategy particle physicists (especially theorists) might have committed or followed, particle physics will naturally correct itself as physics has the most powerful feedback control mechanism in science: experiments.

Nature as expressed through experiments will tell us whether it has implemented SUSY or not!

In this respect, physics is fundamentally different to psychology, and I disagree with Sabine. Psychology experiments are very different to physics experiments, because in psychology you have to control for the informational content in people's brains. Every electron and quark is identical to any other electron and quark, whereas every human being in an experiment is different in so many ways, especially in its informational content of the brain which shapes behaviour. Moreover, the crisis in psychology was not a crisis in theory building but in using sloppy statistics (p-values) to overinterpret experimental results, whereas particle physicists are extremely carefull and accurate in its statisical interepretation of experimental data.

The wild model building bonanza is only due to a lack of experimental constraints.

5. **More Anonymous**
February 15, 2023

"So what will happen to particle physicists? Well, if you extrapolate from their past
behavior to the future, then the best prediction for what will happen is:
Nothing. They will continue doing the same thing they've been doing for the past 50 years. It will continue to not work. Governments will realize that particle physics is eating up a lot of money for nothing in return, funding will collapse, people will leave, the end."

Sabine I think your extrapolation is wrong?

I suspect particle physicists have changed their minds but not conceded it as Peter shares: "These numbers show a distinct and continuing fall-off starting after 2015, the reason for which is clear …" Now the burden of proof to show that every spin-off idea is equally unpromising (as they are "they're doomed to repeat their mistakes") is still an argument to be made? I wouldn't be surprised if this was indeed the case (at least for string theory) but I think data helps and prevents us when we make blanket statements

More Anonymous

6. **Robert Matthews**
February 15, 2023

@Tom Weidig: to be fair to Sabine, I think she may have meant questionable statistical practices like p-hacking, mis-interpretation of p-values etc when she said "bad scientific methodology that had become normalized" (perhaps Sabine would like to comment ?).

She's right that psychologists "managed to mobilize the community and introduce better methodology". These include the emergence of Registered Reports (RRs), which guarantee publication of research on the basis that the methodology is refereed and approved _before_ the study takes place, rather than afterwards.

By decoupling publication from the level of p-value achieved, RRs to date suggest that research in psychology _does_ have a problem with theory building, in the sense of failing to propose hypotheses leading to effect sizes convincingly above noise level. So they're not so far from HEP theorists really.

As someone once said, science is _hard_, but that's no excuse for persisting with stuff that's long since keeled over and died.

7. **Laurence Lurio**
February 15, 2023

I agree that the current experimental results are not encouraging. However, I was always impressed when I went to SUSY talks by the graph showing the three standard model coupling constants varying with energy and almost meeting at around 10^15 GeV. When SUSY was included they all meet at exactly the same place. Do you feel that this was never a convincing argument?

8. **Peter Woit**
February 15, 2023

All,
Please stick to the problems of particle physics, which are a big and complicated issue. A sensible discussion of the larger problems of other sciences and social science would be something way beyond my ability to moderate.

9. **Peter Woit**
February 15, 2023

Laurence Lurio,
I wrote extensively about this in my book and on the blog, agreeing that it's probably the best argument for SUSY extensions of the SM, but is still extremely weak and unconvincing. It's an argument that is often made in a misleading way.

Technically, one problem is that the graph often presented is a one-loop calculation, ignoring the fact that two-loop calculations make the "three lines join at a point" significantly less true. To really do this calculation, you need to use updated LHC limits (the SUSY-change in evolution can't start until the scale at which you see SUSY), you need to include higher-loop contributions, and you need to discuss "threshold effects" of the unknown physics of new Higgs bosons you have to introduce at the GUT scales. With all this taken into account, I don't think there ever was a very impressive "three lines join at a point", just "they join better than with no SUSY", which is something different. I haven't looked into this recently, curious if someone knows of an up to date accurate calculation.

The big problem with any such calculation though is that its assumptions are implausible and discredited by experiment. For the highly implausible, it assumes the "desert hypothesis": no new physics in between the EW breaking scale and the GUT scale. For discredited, GUTs (SUSY or otherwise) generically predict proton decay rates in conflict with observation.

In addition, beyond being implausible and in conflict with experiment, GUTs have fundamental generic problems of just adding a lot of complex structure to the SM, while not explaining anything. What you are doing is postulating a larger symmetry, but then not getting anything out of it since you have to add a complicated symmetry breaking structure to explain why we don't see evidence of this new symmetry. SUSY by itself has the same generic problem in spades (you postulate a new symmetry making your theory more predictive, the predictions are wrong, so you have to add a complicated mess purely to get rid of the new predictions of the symmetry). These problems have been around since long before the LHC put the final nails in the coffin of this subject.

10. **Anonyrat**
February 15, 2023

I think the downward trend in Supersymmetry papers would be more meaningful if compared to a baseline (e.g., compared to number of papers on Higgs, QCD, neutrinos, dark matter, or some such). I don't know what the appropriate comparison would be – perhaps QCD? – or else I would try myself.

11. **Low Math, Meekly Interacting**
February 15, 2023

What happened c. 1985? There's a similar downward trend that only reverses around 1994, perhaps sparked by developments around the Second Superstring Revolution. Then again, the earlier downswing would seem to coincide with Green-Schwarz and the First Superstring Revolution, so maybe Strings can't explain these trends in particle physics so neatly.

12. **Aristotle Pagaltzis**
February 15, 2023

To pick a nit, the graph looks to me as if 2015 was a singular outlier in a trend that actually started in 2011. But this is still compatible, in fact possibly even more compatible, with the explanation that the LHC permanently altered the trajectory of the field.

(In fact 2011 is either the highest point, or tied for highest with 2002. One might not unreasonably read this graph as a steady underlying increase leading up to 2011, with two superimposed waves of excess enthusiasm in the run-up to 1986 and 1997 – sound familiar? –, and then a steady underlying decrease since.)

Thank you for noticing this and pointing it out. It was a rare piece of genuinely heartening news, even ill as it bodes for the dispatch of less experimentally tractable zombies.

13. **Peter Woit**
February 15, 2023

Anonyrat,
I think taking a ratio with some other keyword with its own history is just going to confound different phenomena.

What's remarkable to me about the SUSY numbers are how little they changed from 1997 to 2015, which indicates a very large and stable industry of people consistently pumping out "SUSY" papers. Something happened to wreck the health and stability of this industry and it's pretty clear what it was.

14. **Peter Woit**
February 15, 2023

LMMI,
The first peak is definitely the "First Superstring Revolution", where for several years everyone was writing papers about how SUSY models derived from the superstring were going to unify physics. Not sure of what the dynamics was during the mid 90s driving up the numbers to the stable situation the subject then entered for a very long time.

15. **Alessandro Strumia**
February 16, 2023

A more accurate plot done with bibliometric codes takes into account these factors:
– The overall number of papers/year increased significantly.
– Papers on "MSSM" or "gluino" or "sugra" are on supersymmetry even if it's not mentioned, etc.
This shows a decline of "SUSY&co" and of "string&co". "Higgs" peaked and it's now declining. "DarkMatter&co" grows.

16. **Mark**
February 16, 2023

I wish people would stop saying "particle physicists". This discussion is more about particle theorists. Experimentalists might not have their backs up so much by some of the comments in popular media if this distinction was made…

On SUSY the LHC was very successful – we were told we would produce an abundance of gluinos and squarks when we turned it on. We made some rather difficult measurements that showed almost straight away that prediction was wrong.

17. **A Humble Grad Student**
February 16, 2023

Hello Peter,

If I had to guess, I would say that Witten's 1995 announcement of the equivalence of the various string theories through S and T dualities and M-Theory is the reason for the late 90s spike. Also you had Maldacena's AdS/CFT paper in 1997/98. These two milestones alone probably contributed significantly to the number of papers mentioning "supersymmetry".

Regards,
A Humble Grad Student.

18. **Thomas**

February 16, 2023

Comment on traversable wormholes
https://arxiv.org/abs/2302.07897

19. **Dan Riley**
February 19, 2023

@Robert Matthews, I don't believe experimental particle physics has a statistical reproducibility problem per se. We publish null results, and have for decades–null results are informative, so publication hasn't been tied to p-values. More recently, most experiments have adopted strong measures against p-hacking–analyses have to be fully defined using simulated data, and results (especially major results) are routinely blinded.

What Sabine generally complains about is mostly the generation of theoretical models, and experimentalists using those theories to justify new and bigger machines. I'm unconvinced that the problem there is substantively analogous to the reproducibility crisis in psychology, nor as easily solved as pointing out bad statistical practices.

The Trouble With Path Integrals, Part I

February 16, 2023
Categories: Uncategorized

Two things recently made me think I should write something about path integrals: Quanta magazine has a new article out entitled How Our Reality May Be a Sum of All Possible Realities and Tony Zee has a new book out, Quantum Field Theory, as Simply as Possible (you may be affiliated with an institution that can get access here). Zee's book is a worthy attempt to explain QFT intuitively without equations, but here I want to write about what it shares with the Quanta article (see chapter II.3): the idea that QM or QFT can best be defined and understood in term of the integral
$$\int_{\text{paths}}e^{iS}$$
where S is the action functional. This is simple and intuitively appealing. It also seems to fit well with the idea that QM is a "many-worlds" theory involving considering all possible histories. Both the Quanta article and the Zee book do clarify that this fit is illusory, since the sum is over complex amplitudes, not a probability density for paths.

This posting will be split into two parts. The first will be an explanation of the context of what I've learned about path integrals over the years. If you're not interested in that, you can skip to part II, which will list and give a technical explanation of some of the problems with path integrals.

I started out my career deeply in thrall to the idea that the path integral was the correct way to formulate quantum mechanics and quantum field theory. The first quantum field theory course I took was taught by Roy Glauber, and involved baffling calculations using annihilation and creation operators. At the same time I was trying to learn about gauge theory and finding that sources like the 1975 Les Houches Summer School volume or Coleman's 1973 Erice lectures gave a conceptually much simpler formulation of QFT using path integrals. The next year I sat in on Coleman's version of the QFT course, which did bring in the path integral formalism, although only part-way through the course. This left me with the conclusion that path integrals were the modern, powerful way of thinking, Glauber was just hopelessly out of touch, and Coleman didn't start with them from the beginning because he was still partially attached to the out-of-date ways of thinking of his youth.

Over the next few years, my favorite QFT book was Pierre Ramond's Field Theory: A Modern Primer. It was (and remains) a wonderfully concise and clear treatment of modern quantum field theory, starting with the path integral from the beginning. In graduate school, my thesis research was based on computer calculations of path integrals for Yang-Mills theory, with the integrals done by Monte-Carlo methods. Spending a lot of time with such numerical computations further entrenched my conviction that the path integral formulation of QM or QFT was completely essential. This stayed with me through my days as a postdoc in physics, as well as when I started spending more time in the math community.

My first indication there could be some trouble with path integrals I believe started in around 1988, when I learned of Witten's revolutionary work on Chern-Simons theory. This theory was defined as a very simple path integral, a path integral over connections with action the Chern-Simons functional. What Witten was saying was that you could get revolutionary results in three-dimensional topology, simply by calculating the path integral
$$\int_{\mathcal A} e^{iCS}$$
where the integration is over the space of connections A on a principal bundle over some 3-manifold. During my graduate student days and as a postdoc I had spent a lot of time thinking about the Chern-Simons functional (see unpublished paper here). If I could find a usable lattice gauge theory version of CS (I never did…), that would give a way defining the local topological charge density in the four-dimensional Yang-Mills theory I was working with. Witten's new quantum field theory immediately brought back to mind this problem. If you could solve it, you would have a well-defined discretized version of the theory, expressed as a finite-dimensional version of the path integral, and then all you had to do was evaluate the integral and take the continuum limit.

Of course this would actually be impractical. Even if you solved the problem of discretizing the CS functional, you'd have a high dimensional integral over phases to do, with the dimension going to infinity in the limit. Monte-Carlo methods depend on the integrand being positive, so won't work for complex phases. It is easy though to come up with some much simpler toy-model analogs of the problem. Consider for example the following quantum mechanical path integral
$$\int_{\text {closed paths on}\ S^2} e^{i\frac{1}{2}\oint A}$$
Here S^2 is a sphere of radius 1, and A is locally a 1-form such that dA is the area 2-form on the sphere. You could think of A as the vector potential for a monopole field, where the monopole was inside the sphere.

If you think about this toy model, which looks like a nice simple version of a path integral, you realize that it's very unclear how to make any sense of it. If you discretize, there's nothing at all damping out contributions from paths for which position at time t is nowhere near position at time $t+\delta t$. It turns out that since the "action" only has one time derivative, the paths are moving in phase space not configuration space. The sphere is a sort of phase space, and "phase space path integrals" have well-known pathologies. The Chern-Simons path integral is of a similar nature and should have similar problems.

I spent a lot of time thinking about this, one thing I wrote early on (1989) is available here. You get an interesting analog of the sphere toy model for any co-adjoint orbit of a Lie group G, with a path integral that should correspond to a quantum theory with state space the representation of G that the orbit philosophy associates to that orbit. Such a path integral that looks like it should make sense is the path integral for a supersymmetric quantum mechanics system that gives the index of a Dirac operator. Lots of people were studying such things during the 1980s-early 90s, not so much more recently. I'd guess that a sensible Chern-Simons path integral will need some fermionic variables and something like the Dirac operator story (in the closest analog of the toy model, you're looking at paths moving in a moduli space of flat connections).

Over the years my attention has moved on to other things, with the point of view that representation theory is central to quantum mechanics. To truly play a role as a fundamental formulation of quantum mechanics, the path integral needs to find its place in this context. There's a lot more going on than just picking an action functional and writing down
$$\int_{paths}e^{iS}$$

Comments

1. **AcademicLurker**
   February 17, 2023

   Interesting. I mentioned in an earlier thread that I was currently working through Blundell and Lancaster's *QFT For The Gifted Amateur*. Before taking up that book I looked at Zee's *Nutshell*. It's notable that Zee starts right off introducing path integrals in chapter 1 while in Blundell/Lancaster they don't show up until halfway through the book. I was wondering whether that was purely a personal choice or whether it represented a broader difference in schools of thought about how the subject should be taught.

2. **Andrzej Daszkiewicz**
   February 17, 2023

   Academic Lurker,
   and Michel Talagrand in *What is a Quantum Field Theory? A First Introduction for Mathematicians* (Peter, thanks for mentioning this book!) completely ignores path integrals: "I have also decided to stay entitely away from path integrals […]. There is no doubt that from the point of view of physics, path integrals are a correct approach, but they are not well defined mathematically, and I do not see what yet another heuristic discussion of them would bring."

In fact, I am working through all three (Talagrand, Zee's *Nutshell* and Blundell/Lancaster) and I have just finished Zee's *QFTaSaP* which I have to return to aSaP ;).

3. **Peter Woit**
   February 17, 2023

   AcademicLurker,
   Different people have different points of view, depends a lot on what your experience with qft is and what sorts of calculations you've worked on. For a long time I would have argued that the path integral is the place to start (and I think I did this when I taught a QFT course in the math department back in 2003). More recently, in the QM course I've been teaching and the book I wrote, I avoided path integrals completely. One reason for this though was that the course doesn't cover interacting QFTs.

   At this point it seems to me that it's a good idea to start off with free field theories as an infinite collection of harmonic oscillators and emphasize the propagator, without getting into path integrals at the beginning. By the way, the path integral is a horrible way to calculate things in the QM simple harmonic oscillator). The path integral formalism is a very nice way of organizing the perturbation expansion, and it comes into its own in Yang-Mills theory. But it should be taught with caveats about the necessity of analytically continuing in time, difference between configuration space path integral and (much more problematic) phase space path integral. I think the whole field is suffering from a lack of awareness of the problems with the path integral formalism (which likely point to ways in which the fundamentals of the subject are unsatisfactory).

   Andrzej Daszkiewicz mentions correctly that the Talagrand book (also Folland's) are good examples of where the authors saw no way to give the kind of careful discussion they wanted in terms of path integrals. There are other books by mathematicians (see Glimm/Jaffe) that are based on path integrals, but then they stick to a certain class of theories where the path integral works well (and stay in Euclidean space-time).

4. **Pascal**
   February 21, 2023

   A belated remark on many worlds and complex amplitudes (as opposed to probability densities): this situation is not specific to QFT at all. Think of the more elementary setting where unitary transformations are applied in discrete time steps, as is often the case in quantum information and computation. Then we get a finite sum instead of a path integral, and it is a sum of complex amplitudes (not probabilities). And that doesn't prevent many worlds from being a viable interpretation.

5. **Peter Woit**
   February 21, 2023

   Pascal,
   No intent to start a discussion of many-worlds. My point was just that readers of the Quanta article hearing that a sum over all possible paths is "how the world really is", might think this sounds like the many worlds multiverse, but it's really something quite different. This is especially clear in the Euclidean time picture, where QM is precisely a probabilistic measure on possible histories of worlds, but this is a very different thing than the Born rule probability.

6. **Matthew Foster**
   February 25, 2023

   Hi Peter

   The sphere integral is the "spin coherent state path integral" for a single spin, i.e. the first-order term is a Wess-Zumino-Novikov-Witten term. At least for 2D WZNW CFTs (Witten's nonabelian bosonization), there is no problem reproducing results from the formal affine Lie algebra rep theory (e.g. scaling dims for local operators) via perturbation theory in the corresponding matrix sigma model. The perturbation theory works at large levels. Maybe the restriction to finite KM primary fields is a nonperturbative effect that would require a more serious formulation of the path integral?

7. **Peter Woit**
   February 25, 2023

   Matthew Foster,
   Yes, my toy example is the "spin coherent state path integral" and I'm claiming it's problematic to think of it as an actual integral. You can certainly use it for a semi-classical approximation, but that's different.

   I wrote up all this way back in in 1989, published in an obscure location. A copy of the paper is here https://www.math.columbia.edu/~woit/tqtandrepthy.pdf

   This is about 0+1 theories, for 1+1 QFTs, there's a more complicated story. The WZW path integral is a configuration space path integral (analogous to taking the group manifold as configuration space in 0+1d)

   There's lots and lots more to say about these 1+1 d QFTs related to representations of loop groups or affine Lie algebras. From what I remember it was always my intention to write a follow up to the above paper that would discuss this, but I don't think I ever finished that project, should go back to it.

8. **Matthew Foster**
   February 27, 2023

   Hi Peter,

   In condensed matter, the main use of WZNW terms of this form is in the context of topological boundary modes, and the connection to associated topological theta terms in one higher dimension. For example, a 1+0-D path integral that encodes chiral edge-state wave functions of a quantum Hall droplet, in the presence of disorder, realizes a WZNW action that is a generalization of the spin coherent-state path integral. This allows one to connect directly to the topological character of the quantum Hall state in one higher spatial dimension, since WZNW is the boundary term accrued from the integration of the topological theta term in a finite system. More sophisticated "anomaly matching" arguments (e.g. with a Chern-Simons "response" action for the edge or bulk) give the same conclusions.

   Usually we have an alternative nonperturbative framework for the boundary WZNW theory, so one doesn't have to evaluate directly the likely-ill-defined path integral. Where we run into problems is with the higher-dimensional bulk, with a topological theta term that is hard to deal with and even hard to sensibly derive. This is because the coefficient of the theta term accrues contributions from the ultraviolet, and a satisfactory UV completion (in terms of a concrete lattice model in our case) is often too unwiedly to do much with.

The Trouble With Path Integrals, Part II

February 16, 2023
Categories: Uncategorized

This posting is about the problems with the idea that you can simply formulate quantum mechanical systems by picking a configuration space, an action functional S on paths in this space, and evaluating path integrals of the form
$$\int_{\text{paths}}e^{iS}$$

Necessity of imaginary time

This section has been changed to fix the original mistaken version.
If one tries to do this path integral for even the simplest possible quantum field theory case (a non-relativistic free particle in one space dimension), the answer for the propagator in energy-momentum space is
$$G(E,p)=\frac {1}{E-\frac{p^2}{2m}}$$
Fourier transforming to real-time is ill-defined (the integration goes through the location of the pole at $E=\frac{p^2}{2m}$). Taking t complex and in the upper half plane, for imaginary t the Fourier transform is a well-defined integral. One gets the real-time propagator then by analytic continuation as a boundary value. For a relativistic theory one has
$$G(E,p)=\frac{1}{E^2-(p^2+m^2)}$$
and two poles (at $E=\pm \sqrt{p^2+m^2}$) to deal with. Again Fourier-transforming to real-time is ill-defined, but one can Fourier transform to imaginary time, then use this to get a sensible real-time propagator by analytic continuation.

Trying to do the same thing for Yang-Mills theory, again one gets something ill-defined for real time, with the added disadvantage of no way to actually calculate it. Going to imaginary time and discretizing gives a version of lattice gauge theory, with well-defined integrals for fixed lattice spacing. This is conjectured to have a well-defined limit at the lattice spacing is taken to zero.

Not an integral and not needed for fermions

Actual fundamental matter particles are fermions, with an action functional that is quadratic in the fermion fields. For these there's a "path integral", but it's in no sense an actual integral, rather an interesting algebraic gadget. Since the action functional is quadratic, you can explicitly evaluate it and just work with the answer the algebraic gadget gives you. You can formulate this story as an analog of an actual path integral, but it's unclear what this analogy gets you.

Phase space path integrals don't make sense in general

Another aspect of the fermion action is that it has only one time derivative. For actions of this kind, bosonic or fermionic, the variables are not configuration space variable but phase space variables. For a linear phase space and quadratic action you can figure out what to do, but for non-linear phase spaces or non-quadratic actions, in general it is not clear how to make any sense of the path integral, even in imaginary time.

In general this is a rather complicated story (see some background in the part I post). For an interesting recent take on the phase-space path integral, see Witten's A New Look At The Path Integral Of Quantum Mechanics.

Update: A commenter pointed me to this very interesting talk by Neil Turok. The main motivation that Turok explains at the beginning of the talk (and also in the Q and A afterwards) is exactly one that I share. He argues that the lesson of the the last 40 years is that one should not try and solve problems by making the Standard Model more complicated. All one needs to do is look more closely at the Standard Model itself and its foundations. If you do that, one thing you find is that there's a "trouble with path integrals". In Turok's words, the problems with the path integral indicate that "the field is without foundations" and "nobody knows what they are doing".

I do though very much part company with him over the direction he takes to try and get better foundations. He argues that you shouldn't Wick rotate (analytically continue in time), but should complexify paths, analytically continuing in path space. For some problems doing the latter may be a better idea than doing the former, and in his talk he works out a toy QM calculation of this kind. But the model he studies (anharmonic oscillator) doesn't at all prove that going to the imaginary time theory is a bad idea, for some calculations that works very well. He's motivated by defining the path integral for gravity, where Euclidean quantum gravity is a problematic subject, but the gravitational version of the toy model I think will also be problematic. The ideas I've been pursuing involving the way the symmetries of spinors behave in Euclidean signature I think give a promising new way to think about this, and you won't get that from just trying to complexify the conventional variables used to describe geometries.

Comments

1. **Martin Luc Rosinberg**
   February 17, 2023

   It's completely off-topic, sorry, but I just want to bring to your attention this comment uploaded today on the ArXiv (ref: 2302.07897) that severely criticizes the paper by D. Jafferis et al. on traversable wormholes that generated so much hype recently. According to this new analysis, that looks very solid, several essential claims made in the paper are not justified.

2. **More Anonymous**
   February 17, 2023

   @Peter

   Have you read Padmanabhan's Quantum field theory the why, what and how? His section 5.6.3 Propagator from a Fermionic Path Integral is fabulous. As someone who never really got how to extend my intuition of QM to QFT this book was a lifesaver 🙂

3. **Robert A. Wilson**
   February 17, 2023

   I am a little confused as to why you consider the discretization to be problematic. Is it because discretization is necessary for doing calculations, and if the theory doesn't discretize properly, the calculations could give the wrong answer? Or is it because discretization is necessary for the fundamentals of physics, and if the theory doesn't discretize properly, the theory is wrong?

4. **Peter Woit**
   February 17, 2023

More Anonymous,
That is a nice beginning book, one starting from the path integral. The "Fermionic Path Integral" though I think is a bit misleading, since it's not an integral…

5. **Peter Woit**
February 17, 2023

Robert A. Wilson,
Nothing to do with physics at short distances. Physicists are claiming to do certain integrals over infinite dimensional spaces. Mathematicians trying to make sense of this tend to get caught up in issues of the right formalism to define such integrals, which leads to a very complicated story.

I'm pointing out you can think about this more concretely: discretize the problem to make the integral finite-dimensional, then see if you can get a well-defined limit. For some theories like Yang-Mills, this procedure works, and is even necessary (there is no definition in infinite-dim, and the correct nature of the limit is very non-trivial). If you try to do this for other theories though (like the Chern-Simons or toy examples), trying to put the thing on a computer and calculate shows you clearly the serious problems that face attempts to get a well-defined path integral in those cases.

6. **vmarko**
February 17, 2023

Peter,

"As written, this integral doesn't exist."

What do you mean by this? Isn't that just the Fresnel integral?

Complete the square, use Euler's formula to express exp in terms of sin and cos, and thus reduce the rhs to integrals of sin(k^2) and cos(k^2), i.e. Fresnel integrals. What doesn't exist there?

Best, 🙂
Marko

7. **Peter Woit**
February 17, 2023

Marko,
Yes, what I wrote isn't right, was trying to refer to a problem with computing the propagator you see before you get to the formula I wrote. Will edit to fix.

8. **Blendletan**
February 17, 2023

My understanding is that the Osterwalder-Schrader theorem shows how this gets right to the heart of why it's so hard to construct a QFT that really exists and satisfies the Wightman axioms. You can actually build a QFT iff you can actually define this measure (for imaginary time), and show reflection positivity (which allows you to Wick rotate back to real time). So isn't the trickiness you describe exactly the right thing for mathematicians to get caught up in?

9. **Peter Woit**
February 17, 2023

Blendletan,

I just fixed the first part of this post. The point I'm trying to make in that first section is that even for the simplest scalar free field theories the path integral as written is ill-defined. What is well-defined is the imaginary time path integral.

But even in imaginary time, making sense of the path integral for an interacting field theory is very hard. If you can do this, Osterwalder-Schrader give the conditions under which you can reconstruct a real-time theory.

I've been spending a lot of time looking at this story for spinor fields, where it is very non-trivial. If you're a mathematician and want to write a QFT book using well-defined objects, you can do what Jaffe/Quinn did, sticking to scalar fields. Or, if you want to do spinor fields and QED like Folland/Talagrand, you need to avoid the path integral and define the theory in other ways.

10. **JT gravity**
February 17, 2023

What do you think about the "map" or "dictionary" from QFT to statistical mechanics using Wick rotation? The latter is such that the path integrals are well-defined Wiener integrals, so if you could justify the Wick rotation in the first place, perhaps the path-integrals of QFT would be trustworthy, no?

11. **Peter Woit**
February 17, 2023

JT gravity,
This is what Osterwalder-Schrader do for you: they tell you the condition (reflection positivity) you need to justify Wick rotation to a real time theory.

What makes the subject difficult is not this, but that, especially in more than 1+1 dimension, the statistical mechanics systems corresponding to interesting QFTs are very challenging to say anything about. Euclidean lattice gauge theory is a 3+1d stat mech system, but if you can really understand what happens in the limit as lattice spacing goes to zero, there's a million dollars waiting for you.

12. **Marvin**
February 17, 2023

Peter,

Thanks for bringing forward and clearly these core mathematical problems in physic. BTW, it is amazing how physicists can usually make things look confused, so that any mathematician trying to help for the next step can be lost in the fog.

13. **Peter Orland**
February 18, 2023

Hi Peter,

Maybe I should not interject here, but there is much heat and little light on many discussions of path integrals. Although there are problems with the real-time Dirac - Feynman path integral for Chern-Simons and first-time derivative actions, there is a way that such things make sense.

If 1. the Lie-Trotter formula is valid and 2. there is a sensible completeness or overcompleteness relation (resolution of the identity), then the real-time path integral can be sensibly interpreted rigorously! This sort of thing is discussed in an old books by Barry Simon and by Larry Schulman. This is not to say there are not problems with certain physical systems, like those you mention, but for, say standard Hamiltonian mechanics, everything is fine. Other mathematicians (like T. Kato) and physicists also worked on this. Some of the subtleties, like the mid-point prescription are automatically explained by this method.

This method goes over to real-time bosonic field theories, provided they are put on lattices. There is still the technical issue of taking the continuum limit, but that is not unique to the path-integral formulation.

I always just think of the (non-Wick rotated) path integral as an application of 1. and 2. above. You usually can't go wrong this way, although Chern-Simons models still have the problems you mention.

14. **Peter Woit**
February 18, 2023

Hi Peter,
The problem is that, yes, you can start with a quantum theory, break up the exponential of the Hamiltonian using Lie-Trotter, and if you do this in a certain way, you can claim the infinite sum is a phase space path integral. The problem is that it doesn't have lots of the properties you would expect of a path integral, since your paths are discontinuous and you crucially had to do very specific things in getting the infinite sum (eg. alternating specifying q and p).

The "path integral" you get doesn't in particular have the invariances you expect. Since it is written in terms of dpdp in the measure and pdq in the exponent, you expect it to be invariant under symplectic transformations, but it isn't (if it were, you could choose coordinates in which it was a harmonic oscillator, semiclassical approximation exact, see Schulman chapter 31). You also get into very confusing questions when you try to decide what the state space of the theory is starting with the path integral.

There's a fundamental issue behind this: there is no way to start with a classical phase space (e.g. symplectic manifold) and quantize, getting a corresponding quantum system, unless you introduce extra structure (e.g. a polarization). Without smuggling in somewhere this extra structure, your phase space path integral is doing something impossible.

15. **Peter Orland**
February 18, 2023

… but you don't need phase space, only q space in Lie-Trotter. This is very close to the Schr{\"o}dinger representation. We don't write wave functions or wave functionals as functions of phase space.

If you want to introduce p as well as q, one way is to write every other factor (exponential of the kinetic term) as a Gaussian integral, with p as the integration variable.

Dirac and Feynman did emphasize phase space (see Dirac's 1933 paper, Feynman and Hibbs or Feynman's papers from the 40's), but I never saw the point of this.

Anyway, plenty of applications don't use phase space. The book of Simons I mentioned, from the 70's, actually proves nontrivial results from this (and some yes, with real, not imaginary time).

16. **Peter Woit**
February 18, 2023

Hi Peter,
Sorry I thought your comment was referring to the third point in the posting, about path integrals over phase space. These do get used, and it's not often recognized that path integrals for Chern-Simons theory and usual fermion theories are phase space path integrals, not configuration space path integras (since the action is first order in the time derivative).

To the extent we're discussing instead the first point, and path integrals in configuration space, what I first wrote was too strong (since fixed), but there still are problems with real-time path integrals. I was just looking at the late 70s Simons book "Functional Integration and Quantum Physics". The way he puts things in the introduction is:

"By extending the notion of measure, various attempts at defining [the real time path integral} have been made; see e.g [gives references]. These methods, while useful computationally and for formal heuristics, have not thus far turned out to be analytically powerful; we will not discuss them further."

In the preface he explains "I decided to limit myself to the well-defined Wiener integral rather than the often ill-defined Feynman integral."

17. **Peter Orland**
February 18, 2023

My copy of this book is out of reach, in my office, but I will take your word for it that Simon confines the bulk of the discussion to the Wiener integral. But I do know that he ALSO discusses the proof of Lie-Trotter formula for REAL time at the beginning of the book (which is useful, even he does not use it further). I should check, but I remember the "non-analytically powerful" methods Simon refers to are some formal definitions of C. DeWitt, among others. There is also discussion of the real-time case in the second volume of Reed and Simon.

Anyway, the real Lie-Trotter formula is completely adequate for most of the applications you can find in, e.g., Feynman and Hibbs.

18. **Peter Orland**
February 18, 2023

But phase-space PI are another matter, and I apologize if took things away on a tangent.

19. **Peter Woit**
February 18, 2023

Thanks Peter,
Just took a look at Reed-Simon, vol. 2. In section X.11, they do use the free particle propagator and the Trotter product formula to write down an expression for the time evolution operator as the limit of an expression that one can interpret as a path integral using polygonal paths for a fairly general potential V.

But then they do very quickly say that while this is beautiful and heuristically an integral over path space, an actual measure on path space like Wiener measure is something quite different, and they go to imaginary time and use Wiener measure to get Feynman-Kac.

Besides my experience with lattice gauge theory and Yang-Mills, of course my current fascination with the way the symmetries of spinors work in Euclidean signature is another motiviation for thinking of imaginary time as not just a trick, but something fundamental. While it's true that for QM path integrals you can make sense of the free particle propagator without going to imaginary time (thanks Marko…) and then use this to define a limit as a path integral, for QFT, I don't think this is true. The argument now in the first section shows that then to even define the free particle propagator you have to do something, which has a natural interpretation as thinking of the propagator as something holomorphic in the upper half complex time plane.

20. **vmarko**
February 19, 2023

Just a short note – regarding the propagator and Wick rotation, I believe the problem is a bit deeper, and the issues with path integrals are just a symptom.

Namely, there is a fundamental difference between elliptic and hyperbolic PDE's, where the former have a well-defined Green function, while the latter really don't. Since the Klein-Gordon equation is a hyperbolic PDE, the only way to introduce its Green function is to Wick-rotate to imaginary time, find the Green function in Euclidean theory, and then perform analytic continuation back into real-time. It's essentially due to the difference in signature between the d'Alembertian and a 4D Laplacian.

This is a problem for all relativistic wave equations (and a consequence of Lorentz-invariance), so whenever you discuss free field theory (or perturbative one), you are bound to run into this issue. It has nothing specifically to do with path integrals, any approach to quantizing field theory will suffer from this (though it might be hidden away or appear in some other form).

Best, 🙂
Marko

21. **JT gravity**
February 19, 2023

Marko,

Where can I read in more technical details what you are saying? Is this related to the problem of choosing a vacuum for a QFT defined on a curved spacetime background? I guess this is so because, on the one hand, the choice of vacuum defines unambiguously the notion of positive/negative frequencies for the one-particle states (e.g., a positive-frequency particle state w.t.r. to Rindler is not so w.t.r. to a Minkowski observer), while on the other hand, the notion of positive/negative frequencies depends on the time-orientability of Lorentzian structure, which is avoided in Euclidean signature.

22. **Peter Woit**
February 19, 2023

JT gravity,
An important thing to notice is that, just looking at the formula for the time/energy Fourier transform, positive energy means holomorphic in one complex time half plane, not the other.
Another version of the problem with the real time path integral that I was trying to point out is that it obscures a fundamental piece of structure of a qft: what is positive vs. negative energy or, by Fourier transform, what is positive vs. negative imaginary time? In the imaginary time path integral, the asymmetry is fundamentally built in and explicit

23. **Darran**
February 20, 2023

Peter,

I went through a similar period of liking and then understanding the limits of the Path Integral.
It can actually be difficult however to find a proper canonical treatment of issues normally discussed with the Path Integral, e.g. Yang-Mills theory and the Higgs Mechanism. For this reason I recommend the books of Strocchi "An Introduction to Non-Perturbative Foundations of Quantum Field Theory" and Nakanishi and Ojima "Covariant Operator Formalism Of Gauge Theories And Quantum Gravity".

24. **Peter Woit**
February 20, 2023

Darran,
I very much agree. There are very tricky issues with how gauge invariance works in these cases, highly unclear that the path integral solution (in lattice gauge theory, just integrate over all degrees of freedom, gauge or otherwise) really works as advertised. One example of this is the no-go theorem for BRST in lattice gauge theory. I agree with the recommendation of the Strocchi and Nakanishi/Ojima books.

I should also point out that a very specific problem with the culture of "string theory" is that it promotes the ideology that there's nothing to see here and the only people who think about or work on these issues are losers, too incompetent to realize that these subjects are a closed book and serious theorists are those who move on to schemes for getting emergent space-time out of string-theory grounded ideas.

25. **JT gravity**
February 20, 2023

I regard the BV-BVF complex cohomology to be the most powerful and deep quantisation scheme for QFT, and I also consider that the true meaning of "supermathematics" is not the supersymmetry of the target-space manifold relating fermions to bosons but in the mathematical structure of graded manifolds in the sense of Kostant modelling the phase-space of a quantum dynamical systems.

You raised an important sociological question regarding the general dogmatic attitude of string theories on their faith in the path integral technique. You should remember of Bert Schroer's views on functional integrals and string theories.

Finally, returning to the question of postive/negative frequencies, it's intersting that the Fourier decomposition is not conformally invariant while the decomposition into positive/negative frequencies itself is conformally invariant. Thus, it looks like there is a deep geometric structure hidden here which becomes transparent if you holomorphically continue the green function into the Riemann sphere, so that the choice of a positive frequency is equivalent to the choice of a hemisphere. And this is one of the key motivations for twistor theory!

26. **Russell Thompson**
February 20, 2023

I’m intrigued when Peter writes that his research is, “…another motivation for thinking of imaginary time as not just a trick, but something fundamental.” In 1953, Feynman used the path integral method with Weiner functionals and the Wick rotation between imaginary time and inverse thermal energy to write the partition function for the helium superfluid system. His imaginary time trajectories are now often referred to as “ring polymers” in modern quantum simulations (see Ceperley’s 1995 review of Path Integral Monte Carlo methods). Although the use of the imaginary time/inverse thermal energy dimension is always stressed as being a trick, Feynman’s partition function is mathematically the same as that of classical, coarse-grained representations of polymer systems under the Gaussian thread model, which in turn are equivalent to quantum density functional theory. The theorems of DFT therefore allow a one-to-one mapping between a classical polymer systems with the fictitious imaginary time dimension and regular, non-relativistic quantum mechanics, without it. It is very tempting therefore to look on the imaginary time as playing some fundamental role. Even Feynman and Hibbs mused about related matters in their 1965 quantum book (see section 10.5, Remarks on Methods of Derivation).

27. **Peter Woit**
February 20, 2023

Russell Thompson,
It is truly remarkable that if you take your system defined in imaginary time to be periodic in imaginary time, you get statistical mechanics with temperature inverse to the period. In the limit as you get rid of periodicity (periodicity length infinite), you get the zero temperature limit, which is where you do conventional particle physics. “Just a trick?” I suspect not.

28. **Andras Laszlo**
February 23, 2023

Hi,
Indeed there are several mathematical issues for defining the functional integrals in Lorentz signature, and it seems also a correct statement that for fermionic fields, actually this has little to do with integrals at all in terms of measure theory. But one can translate the formal Feynman functional integrals to a differential reformulation (the master Dyson-Schwinger equation), which is an equation for the partition function, and is formally equivalent to the functional integral formulation. Then, one can further translate this equation for the partition function to the language of (Feynman type) field correlators, and there the corresponding operators and function spaces are well defined.

That is, one could say that Feynman functional integrals are just shorthands for the master Dyson-Schwinger equation, expressed on the (Feynman type) field correlators. This kind of treatment also removes this puzzling issue concerning the fermion fields mentioned by Peter.
Best regards,
Andras

29. **Amateur Mathematician**
February 24, 2023

Some of you may be interested in a recent (2022) work by Feldbrugge and Turok that approaches path integrals without using Wick rotation.

The paper: https://arxiv.org/abs/2207.12798
Related paper: https://arxiv.org/abs/1909.04632
A talk by Turok: https://www.youtube.com/watch?v=L17Cx-iD8uU

30. **Peter Woit**
February 24, 2023

Amateur Mathematician,

Thanks a lot: I just watched the Turok talk, found it very interesting. His motivation is very close to mine, although he takes a very different view of imaginary time, which I strongly disagree with. I’ll add something about this to the posting, with more comments.

Latest on the Wormholes

February 17, 2023
Categories: Wormhole Publicity Stunts

I had thought that the wormhole story had reached peak absurdity back in December, but last night some commenters pointed to a new development: the technical calculation used in the publicity stunt was nonsense, not giving what was claimed. The paper explaining this is Comment on "Traversable wormhole dynamics on a quantum processor", from a group led by Norman Yao. Yao is a leading expert on this kind of thing, recently hired by Harvard as a full professor. There's no mention in the paper about any conversations he might have had with the main theorist responsible for the publicity stunt, his Harvard colleague Daniel Jafferis.

Tonight Fermilab is still planning a big public event to promote the wormhole, no news yet on whether it's going to get cancelled. Also, no news from Quanta magazine, which up until now has shown no sign of understanding the extent they were taken in by this. Finally, no news from Nature about whether the paper will be retracted, and whether the retraction will be a cover story with a cool computer graphic of a non-wormhole.

Update: Dan Garisto goes through the Jafferis et al. paper, noting "Turns out it looked good only because they used an average (a fact not specified in the article)." and ending with

> The unreported averages for the thermalization and teleportation signal make a stronger case for misconduct on the part of the authors.

I don't understand why Fermilab was planning a public lecture promoting this, and with what has now come out, it should clearly be cancelled.

Update: I like the suggestion from Andreas Karch

> Quanta magazine could make a video where the wormhole authors share in vivid detail the excitement they felt when they realized that their paper isn't just overhyped but actually wrong.

Update: Garisto has a correction, explaining that the averaging is not the problem with Jafferis et al., rather that the teleportation signal is only there for the pair of operators involved in the machine language training, not there for other pairs of operators that should demonstrate the effect. In any case, best to consult the paper itself. If Jafferis et al. disagree with its conclusions, surely we'll see an explanation from them soon.

Update: The Harvard Gazette promotes the wormhole publicity stunt, with "Daniel Jafferis' team has for the first time conducted an experiment based in current quantum computing to understand wormhole dynamics." As far as I can tell, that's utter nonsense, with the result of the quantum computer calculation adding zero to our understanding of "wormhole dynamics".

Update: Video of the Lykken talk now available, advertised by FNAL as Wormholes in the Laboratory.

Comments

1. **Andrew**
   February 17, 2023

   Just attended Lykken's webinar. He did not address this refutation at all or any other voices of reason. I attempted to ask a question about this paper but it was not selected (the talk was in Zoom webinar format).

2. **Peter Woit**
   February 18, 2023

   Andrew,
   I also watched a bit of the webinar. It was completely misleading propaganda, no mention of anything skeptical or critical, he was selling this as a huge advance and success. Probably you weren't the only one with serious questions at the end, but they just chose the "when do we get to travel through a wormhole" ones. It's outrageous that Fermilab is doing this kind of thing.

   Hard to know what it will take to stop more of it. Lykken will pursue the standard successful string theory tactic of just ignoring serious criticism, pretending it doesn't exist.

   I see that the grant funding this stuff:
   https://pamspublic.science.energy.gov/WebPAMSExternal/Interface/Common/ViewPublicAbstract.aspx?rv=b31f0e55-de01-4635-91d1-2bf112daea20&rtc=24&PRoleId=10
   is up end of August. Will anyone with some sense stop this from getting renewed or funded elsewhere?

3. **Jim Eadon**
   February 19, 2023

   "their paper isn't just overhyped but actually wrong"
   Phew! I was worried for a moment, there, that the paper was, not even wrong.

4. **suomynona**
   February 20, 2023

   I suspect that unless there is public backlash at a level on par with the initial announcement, Jafferis et al. will subscribe to the policy of "we will not validate the criticism with a response". They're clearly playing the political PR game. No need to engage if the whole thing never picks up traction in the public arena.

5. **Peter Woit**
   February 20, 2023

   Jim Eadon,
   This is starting to become a serious problem with the title of this blog. If things keep going this way and I keep feeling I have to write about them, I may need to change title to something like "Utter Bullshit" to accurately reflect the nature of what I'm writing about.

6. **Peter Woit**

February 20, 2023

suomynona,

So far the authors of the stunt are following the string theory playbook for dealing with criticism: pretend it doesn't exist and keep playing the hype video.

7. **More_Nonymous**
February 24, 2023

The problem with calling this wormhole bullshit "bullshit" is that, in the unlikely event that mainstream media wants to report it, they can't. Because the FCC, y'know? So it's kinda self-defeating.

How's that appointment of a medieval historian to head the Institute For Advanced Studies working out, in your opinion? The rub here is that there are structural reasons why these positions are occupied by the kinds of people occupying them; a scientist would have, if not knowingly lied about the wormhole, then played along, while, charitably, the medievalist simply did not know better and was duped. So which of the two given choices (the hypester-hoaxster-fraudster or the innocent) is, in your opinion, uh, preferable as director of IAS?

Lastly: at what point do we start to describe these people as what they are: swindlers, hoaxers, charlatans, liars, or, if you want to, simply "dishonest, money-seeking, self-serving careerists". While some of these labels might be actionable, proper terminology is also important!

8. **Peter Woit**
February 24, 2023

More_Nonymous,
It's an interesting question about alternative universes to speculate how things would have been different if Dijkgraaf had still been IAS director. I doubt he would have have brought history into it, wouldn't have made public comparisons to the experimental vindication of GR in 1919. I also doubt he would have said anything skeptical, preferring to join his colleagues in trying to figure out how to best exploit the hype to help with funding of their field.

9. **Gali Weinstein**
February 25, 2023

The paper written by Norman Yao et al sounds compelling.
I am waiting for Jafferis et al.'s response to this paper.
I wrote two comments on the wormhole experiment. But I uploaded these comments in December and January. Anyway, I put them here:
https://arxiv.org/abs/2212.03568
https://arxiv.org/abs/2301.03522
The second comment discusses philosophical problems. I thought back then that the great achievement was to generate the sparse Hamiltonian, i.e., the greatest achievement was done by conventional computers rather than by the Sycamore. But if as Yao et al show that this sparse Hamiltonian is problematic, then I may have to update my second comment. I will wait and see…
Yao et al. claim that the systems considered in the Nature paper are unable to simultaneously satisfy scrambling + thermalization dynamics and perfect-size winding. Nevertheless, both properties are central to the holographic correspondence (AdS/CFT) and are known to occur in the SYK model at large system sizes.
Perfect-size winding is a necessary criterion of a traversable (ER = EPR) holographic wormhole. So did Jafferis et al. simulate perfect-size winding with 9 qubits?
Yao et al. argue that perfect-size winding observed by Jafferis et al. seems reliant on the small size, but not on thermalization and scrambling (quantum chaos). I wonder whether this contradiction is correctable or rather, maybe this signifies that we cannot simulate quantum teleportation through a holographic wormhole with 9 qubits (with 164 two-qubit gates circuit).
Gil Kalai wrote that we have difficulties even with 5 qubits: https://cstheory.stackexchange.com/questions/35934/states-and-probability-distributions-that-the-5-qubits-ibm-computer-can-produce?fbclid=IwAR1ntzNZ9df1wZtaIYxBl7uhXoTfRhtPkBo_1e1w4uUgEbIyLHuwUIXfiVM

10. **André**
February 27, 2023

@Gali Weinstein:
"Gil Kalai wrote that we have difficulties even with 5 qubits: […]"

I don't know too much about quantum computing, but that question you link has last been updated 5 years ago, which, I believe, in the field of quantum computing on real devices is ancient history.

Some Interviews

February 23, 2023
Categories: Uncategorized

Some interviews that readers of this blog may find of interest:

- David Zierler at the AIP interviews Lee Smolin (April 1, 2021).
- David Zierler at the AIP interviews Stephon Alexander (June 7, 2021).
- Alexis Papazoglou at IAI News interviews me (hot off the press).

Comments

1. **More Anonymous**
   February 24, 2023

   I think you missed this interview of Andrew Strominger?

   https://youtu.be/y3cw_9ELpQw

2. **Dilawar Singh**
   February 24, 2023

   In other news: https://www.quantamagazine.org/physicists-use-quantum-mechanics-to-pull-energy-out-of-nothing-20230222/

3. **Peter Woit**
   February 24, 2023

   Dilawar Singh,
   Hard to get excited about teleportation in quantum devices unless it's through a wormhole…

4. **John Baez**
   February 25, 2023

   In his interview Smolin says:

   "These very rough models [of quantum gravity] predict noise at the square root of the Planck frequency. Interestingly enough, that's where LIGO has some sensitivity to noise."

   "So, there's a story about this. The GEO down in Hannover, I don't know if it was ever actually published, but there were talks where they showed graphs of this noise, and Giovanni got very excited about it, and I got very interested in it. Again, since they didn't publish, we didn't publish. Now, the LIGO people seem to be saying that there's some inexplicable noise in the detector. Again, I'm told this second hand. I don't think they've published."

   Does anyone here know anything about "inexplicable noise" in the gravitational wave detectors LIGO or GEO600?

5. **Martin S.**
   February 25, 2023

   @JB WP has some info on it: GEO600: Claims on holographic properties of spacetime.

6. **Young physicist**
   February 26, 2023

   I learned a lot of things from Lee's wonderful interview, things he never included in his previous books, which are very important for the younger generation. I wish he would one day publish personal memoires in which he would elaborate more on his academic journey and those personal matters. One thing in particular is the contrast between the two portraits of Witten, the one found in his interview and the one found in your book Peter.

7. **John Baez**
   February 26, 2023

   Thanks, Martin S! With an apparatus so exquisitely delicate as LIGO or GEO600, the battle against noise is an endless one, and I imagine at any particular stage of this battle there will be "inexplicable noise" – or more accurately, *not yet explained* noise. So I would be very reluctant to jump to the conclusion that this not yet explained noise is due to innate properties of spacetime. But I was very curious to hear what rumors Smolin was alluding to.

8. **André**
   February 27, 2023

   Even assuming that the noise was in fact there, and would in fact be due to some aspect of Planck scale physics, I am skeptical how much you can learn from it about any specific model.
   All models I know of that attempt to make some predictions for effects like noise or decoherence are at some point putting the Planck scale in by hand – e.g. setting some arbitrary length scale in the model to be equal to the Planck length. Hence, I don't find it very astonishing if one gets results of order 1 in Planck units as phenomenological predictions. It's what I would expect from most models of quantum theory + gravity.

9. **Jim Eadon**
   February 27, 2023

   Prof Woit, I was impressed by your interview. I'm not a professional scientist (just an MSc opto-quantum physics), but I was always intrigued by the Twistor theory of Penrose. Ironically, I first heard about it, if I remember, in a String Theory book. That same book made me a big string theory enthusiast, until I eventually realised it was a dead end. I only realised that after I bought "Not Even Wrong". (Later I read "the trouble with physics", a great book. But your book was more to the point, to me.)
   I agree that String Theory has been a boon to mathematics. But for physics, ST has set physics back 20 or 30 years, I think. Perhaps I forgive string theory, a little, due to

the incredible group-theory Moonshine mathematics. >smile< I'm a fan of group theory, of John Conway RIP, Richard Borsherds, and co.

Physics With Witten

February 26, 2023
Categories: Uncategorized

I just noticed that last semester Edward Witten was teaching Physics 539 at Princeton, a graduate topics course. Since he's now past the age of 70, at the IAS he is officially retired and an emeritus professor (the IAS is the only place I know of in the US with retirement at 70, presumably since it is a non-teaching institution). I don't know if there are other times Witten has been teaching courses at the university since his move to the IAS in 1987.

Videos of the first few lectures are on Youtube here, problem sets on this web-page. It seems like the course started out covering issues with causality in general relativity, following these lecture notes, then later moved on to topics in quantum information theory.

Comments

1. **Noah**
   February 26, 2023

   Are you sure they still have mandatory retirement and it's not just a tradition? I can't see why IAS would have any kind of special exception. I guess it's also possible that just no one has bothered to sue?

2. **Peter Woit**
   February 26, 2023

   Noah,
   Legally, there is an exception for high-level executives, who can be required to retire at age 65. I had heard that the IAS argues that the positions of its faculty are more akin to high-level executives than to university faculty, since they don't teach. I'd also heard that people are well aware this might not stand up in court if challenged, but that so far no one has been interested in challenging it.

   In any case, given the nature of the IAS positions, and that there's not much practical ifference between an emeritus and non-emeritus position there, it would be hard to make an argument on fairness grounds that the IAS should keep paying people very high salaries indefinitely.

3. **Alex**
   February 27, 2023

   Ha, a bit surprised (in a good way) that he's teaching those particular topics, and to particle physicists, I presume.

   In my years reading papers, I think it has become the custom for some people with a background in (Minkowski, perturbative, path integral) QFT to simply launch into a research career in topics like QFT in curved spacetimes, the information paradox, and quantum gravity, without even knowing basic elements from causality theory in GR, like global hyperbolicity and Cauchy hypersuperfaces. These two things are key to give the correct physical interpretation in QFT in curved spacetimes to things like transitions from pure to mixed states. In normal, inertial frame Minkowski QFT, where all relevant time slices are Cauchy, that would certainly be a red flag, but in QFT in curved spacetimes, you can be evaluating the second state in a non-Cauchy time slice, and its nonpure character is a consequence of that. In fact, that's exactly what happens in the typical Schwarzschild evaporation scenario. As Wald has been stressing for decades, the problem is not the pure to nonpure transition, since that's explained by the second hypersurface not being Cauchy, but by the resulting "evaporating spacetime" not being/not appearing to be globally hyperbolic, in violation to Penrose's second cosmic censorship hypothesis.

4. **Dimitris Papadimitriou**
   February 27, 2023

   Alex
   There's an (important) implicit assumption, though, that black holes have to evaporate completely, so that after the 'final moment' of the evaporation, for external asymptotic observers the trapping horizon disappears and nothing remains but empty flat spacetime.
   In that case, there's a Cauchy horizon and spacetime is indeed non globally hyperbolic, as you said.
   That seems problematic , not only due to the violation of the weak cosmic censorship; there's also the issue with the huge (of order M^5 during the final stages of the evaporation, where M is the initial mass of the hole) internal " maximal volume" of the black hole, that (in the case of full evaporation) is disconnected (and disappears? who knows?) in a way that seems unphysical when the throat shrinks as the horizon area goes to zero.
   Another possibility is that some kind of a slowly decaying remnant is left behind, that disappears gradually (with soft gravitons/ photons, perhaps…).
   In such cases spacetime may be globally hyperbolic after all:
   No violation of CC, no information loss. This is an open possibility.

5. **clayton**
   March 1, 2023

   so is Piet Hut emeritus now?

6. **Peter Woit**
   March 1, 2023

   clayton,
   He turned 70 last fall, I'm guessing under their usual policy he'd retire after this academic year. But, given his history there, if anyone were to challenge their policy, it might be him.

Yet More on the Wormholes

February 27, 2023
Categories: Wormhole Publicity Stunts

The paper explaining that this Nature cover story, besides being a publicity stunt, was also completely wrong, has so far attracted very little media attention. The first thing I've seen came out today at *New Scientist*, a publication often accused of promoting hype, but in this case so far the only one reporting problems with the hyped result. The title of the article is Google's quantum computer simulation of a wormhole may not have worked. It contains an explanation of the technical problems:

> The first problem has to do with how the simulated wormhole reacted to the signals being sent through it….Yao and his colleagues found that for each individual test, the system continued to oscillate indefinitely, which doesn't match the expected behaviour of a wormhole.
>
> The second issue was related to the signals themselves. One of the signatures of a real wormhole – and therefore of a good holographic representation of a wormhole – is that the signal comes out looking the same as it went in. Yao and his team found that while this worked for some signals – those similar to the ones the researchers used to train a machine learning algorithm used to simplify the system – it didn't work for others.
>
> …it seems that for this particular quantum system, the size winding would disappear if the model was made larger or more detailed. Therefore, the perfect size winding observed by the original authors may just be a relic of the model's small size and simplicity.

There is a response from Maria Spiropulu:

> "The authors of the comment argue about the many-body properties of the individual decoupled quantum systems of our model," she says. "We observed features of the coupled systems consistent with traversable wormhole teleportation."

Remarkably, Lenny Susskind throws the authors of the stunt under the bus:

> "What is not so clear is whether the experiment is any better than garden-variety quantum teleportation and does it really capture the features of macroscopic general relativity that the authors might like to claim… only in the most fuzzy of ways (at best)," he says.

Comments

1. **T.J.**
   February 28, 2023

   I remember being at a student journal club discussing this work in which a curious computer science undergrad with little to no knowledge of wormholes etc. pointed out that the result would have to be reproduced with "untrained operators" to properly establish the claim. I'm surprised that nobody pointed out this basic and blatant flaw earlier.

2. **Sabine Hossenfelder**
   February 28, 2023

   I wouldn't have thought it possible, but I agree with Susskind.

3. **E.S.**
   February 28, 2023

   Can anyone explain Spiropulu's response? The only way I can think of making sense of it is if she mixed up "decoupled" and "commuting."

4. **Geoff Penington**
   March 1, 2023

   I'm not sure why it's shocking to see Lenny express scepticism about a result that has met with almost universal scepticism from experts in the field who were not co-authors of the paper.

   @E.S. I believe the authors argument is that even though the terms in each "boundary Hamiltonian" all commute, the coupling between the two sides doesn't commute with them, and so the coupled system is interesting, or even "wormhole-like", even if the individual decoupled systems are not. I find this argument uncompelling. An actual holographic simulation of a wormhole should absolutely have to thermalise without any coupling between the two sides.

5. **Jonathan**
   March 1, 2023

   This is off topic but Springer is expecting to release a Handbook of Quantum Gravity this year or next. The ncatlab has a page with published chapters on arXiv so far which may be of interest to readers:
   https://ncatlab.org/nlab/show/Handbook+of+Quantum+Gravity
   Much more CDT and LQG than I would have guessed.

6. **Peter Woit**
   March 1, 2023

   Geoff Penington,
   Susskind was in some sense part of the original publicity stunt, co-authoring the explanatory article that appeared in Nature with the technical article, see
   https://www.nature.com/articles/d41586-022-03832-z
   "A holographic wormhole traversed in a quantum computer"

   I'm assuming he recently read the Yao et al article and realized that the original article he had helped promote was likely technically wrong. Not shocking that he should now express skepticism, for shocking, see what Harvard is sending out, just got this yesterday in the newsletter they send out to their alumni

   https://news.harvard.edu/gazette/story/2023/02/one-small-step-toward-understanding-gravity/

   Last I heard from the editor at Quanta and the director of the IAS, they were standing behind this work as a huge breakthrough, since that was what they were hearing from experts in the field. Maybe things have changed, will be interesting to see if Quanta now takes seriously my suggestion that they should re-report this story.

7. **S**
   March 2, 2023

   Perhaps the interesting question is why the journalists at Quanta would take it upon themselves to decide that something *was* a huge breakthrough when such a large number of experts in the field held that it was not only not a breakthrough, but was nonsense. It's a vision of science journalism that I hope they do not decide to adopt uniformly.

8. **JdM**
   March 3, 2023

   More from Lenny ("Lost in the landscape"):

   https://cerncourier.com/a/lost-in-the-landscape/

9. **Peter Woit**
   March 3, 2023

   S,
   The Quanta article (and associated video) was produced under embargo conditions, where the claims of a "breakthrough" were being kept secret and could only be discussed with a very limited range of people other than the authors. Even after the "breakthrough" was made public, the initial reaction from a lot of experts was to try and find a way to be supportive, since it was excellent PR for their field. It took a while for many experts to look into the actual result and realize there was nothing there. More recently, the article by Yao et al has made clear that what was there was not just vacuous but wrong.

   I'm curious to see what Quanta does about this. Last time I was in contact with them, I was advising them they should re-report the story. Their response was that they were going to listen to what experts had to say (not me…).

10. **Dan Winslow**
    March 13, 2023

    It seems to be spreading. Not sure what to make of this exactly, but it sure seems like wormhole-style hype to me.
    https://phys.org/news/2023-03-counterportation-quantum-breakthrough-paves-world-first.html

11. **Peter Woit**
    March 13, 2023

    Dan Winslow,
    Yes, outrageous hype, courtesy of the University of Bristol press office. Anyone at Bristol willing to contact people there to tell them to not do this kind of thing?

12. **Mitchell Porter**
    March 14, 2023

    Anyone who wants to skeptically study the claim from Bristol about "counterfactual teleportation" might want to start with this critique by Lev Vaidman of the original concept:

    https://arxiv.org/abs/1304.6689

13. **Mitchell Porter**
    March 24, 2023

    Vaidman has coauthored another critique of "counterportation":

    https://arxiv.org/abs/2303.08962

    Meanwhile, at a popular physics Q&A site, the author of the concept may be seen defending it, alongside a skeptical answer:

    https://physics.stackexchange.com/q/755561/1486

Lost in the Landscape

March 3, 2023
Categories: Multiverse Mania, Swampland

A commenter in the previous posting pointed to an interview with Lenny Susskind that just appeared at the CERN Courier, under the title [Lost in the Landscape](). Some things I found noteworthy:

- He deals with the lack of any current definition of what string theory means by distinguishing between "String theory" and "string theory". "String theory" is the superstring in 10 dimensions somehow compactified to have some large dimensions that are either flat or AdS. This can't be the real world

  > I can tell you with 100% confidence that we don't live in that world.

  since the real world is non-supersymmetric and dS, not supersymmetric and AdS. He describes this theory as being "a very precise mathematical structure", which one might argue with.

  Something very different is "string theory":

  > you might call it string-inspired theory, or think of it as expanding the boundaries of this very precise theory in ways that we don't know how to at present. We don't know with any precision how to expand the boundaries into non-supersymmetric string theory or de Sitter space, for example, so we make guesses. The string landscape is one such guess…
  >
  > The first primary fact is that the world is not exactly supersymmetric and string theory with a capital S is. So where are we? Who knows! But it's exciting to be in a situation where there is confusion.

- About anthropics and the landscape, he still thinks this is the best idea out there, but acknowledges it has gone nowhere in twenty years:

  > Witten, who had negative thoughts about the anthropic idea, eventually gave up and accepted that it seems to be the best possibility. And I think that's probably true for a lot of other people. But it can't have the ultimate influence that a real theory with quantitative predictions can have. At present it's a set of ideas that fit together and are somewhat compelling, but unfortunately nobody really knows how to use this in a technical way to be able to precisely confirm it. That hasn't changed in 20 years. In the meantime, theoretical physicists have gone off in the important direction of quantum gravity and holography.

- About the swampland, like everyone else I know, he can't figure out what the argument is that is going to relate it to the real world:

  > The argument seems to be: let's put a constraint on parameters in cosmology so that we can put de Sitter space in the swampland. But the world looks very much like de Sitter space, so I don't understand the argument and I suspect people are wrong here.

- His comments on Technicolor strike me as odd:

  > I had one big negative surprise, as did much of the community. This was a while ago when the idea of "technicolour" – a dynamical way to break electroweak symmetry via new gauge interactions – turned out to be wrong. Everybody I knew was absolutely convinced that technicolour was right, and it wasn't. I was surprised and shocked.

  I remember first hearing about the Technicolor idea around 1979 when Susskind and Weinberg wrote about it. It was a very attractive idea by itself, but the problem was that to match known flavor physics you needed to go to "Extended Technicolor", which was really ugly (lots of new degrees of freedom, no predictivity). No idea when people supposedly were "absolutely convinced that technicolour was right", maybe it was for the few months it took them to realize you needed Extended Technicolor.

- About the wormholes, he says:

  > One extremely interesting idea is "quantum gravity in the lab" – the idea that it is possible to construct systems, for example a large sphere of material engineered to support surface excitations that look like conformal field theory, and then to see if that system describes a bulk world with gravity. There are already signs that this is true. For example, the recent claim, involving Google, that two entangled quantum computers have been used to send information through the analogue of a wormhole shows how the methods of gravity can influence the way quantum communication is viewed. It's a sign that quantum mechanics and gravity are not so different.

  Unclear to me how this enthusiastic reference to the wormholes relates to his [much less enthusiastic recent quote in *New Scientist*]():

  > What is not so clear is whether the experiment is any better than garden-variety quantum teleportation and does it really capture the features of macroscopic general relativity that the authors might like to claim… only in the most fuzzy of ways (at best).

Comments

1. **Jonas**
   March 3, 2023

   Susskind published today a paper on the Arxiv about de Sitter and holography. Take a look: 2303.00792

2. **Peter Woit**
   March 3, 2023

   Jonas,
   Thanks. This seems to be more along the lines of ideas that go back to
   https://arxiv.org/abs/2205.00315
   and further. The controversial Quanta article about the wormhole stunt ended with a section about this, I guess to show that the AdS problem was being overcome.

   An interesting thing about these recent papers is that Susskind now lists his affiliations as not just Stanford, but also Google Research in Mountain View.

3. **Jeff Berkowitz**
   March 3, 2023

   Peter, I'm curious about your "…like everyone else I know…" comment regarding the swampland conjectures. A couple of years ago when I was really trying to follow this stuff, it seemed to me that the swampland "program" was clearly targeted at KKLT – saying in effect, "you have a conjecture that DS may emerge from String Theory [note caps] but I have an equally reasonable conjecture that it cannot." Is this not the right way to think about it?

4. **Peter Woit**
March 3, 2023

Jeffrey Berkowitz,
Yes, that's the right way to understand what they are doing (although, as Susskind notes, the landscape idea of dS is string theory, not String theory). But then, where I think I join Susskind and many others in being confused, is in trying to figure out what the implications of this are supposed to be if it is successful. If you can show string theory can't give you dS, and you know we live in dS, the obvious implication is that string theory can't describe the real world. But, the people doing this claim to be string theorists devoted to making string theory work. Their plan instead seems to be to argue that since string theory has to be true, the world cannot actually be dS, it's just an illusion, with the real story quintessence or whatever. But there seems to be no good argument for this.

5. **Bob Y.**
March 3, 2023

Susskind's makes the following comment regarding the recent "wormhole" simulation:
"…two entangled quantum computers have been used to send information through the analogue of a wormhole…". What are the "two entangled quantum computers" in reality?

6. **Jerome**
March 4, 2023

Peter, your last comment is extremely illuminating. I've always been confused by the same thing: all evidence points to us existing in a dS universe, so if one could show that (S/s)tring theory is a sort of theoretical closure of the set of all Theories of Everything for AdS (and only AdS) universes, then that would constitute the closest thing you could get to an unassailable disproof of string theory. As magically energetic colliders will forever be out of our reach, this kind of no-go theorem is the only way I can imagine string theory being falsifiable in practice. I never could understand why they'd be so eager to detonate their own field of study by proving such a damning conjecture—a conjecture which all evidence points to being true, so far.

If their goal is talk themselves out of the universe being dS, then I truly don't know what hope there is for the future. We've gone from "Oh, these useless 1+1 AdS toy models of QG are just stepping stones to more realistic models" to demanding that the universe actually be such a model. I'll continue disregarding the entire subfield until the day they can describe the universe we actually live in.

7. **Adam Treat**
March 4, 2023

Something that strikes me as a theme in all of this is the repeated use of towers of conjectures. The arxiv links of Susskind in the comments are replete with conjectures based on other conjectures. It is difficult to find the motivations for such conjectures as they are often not spelled out and seem to resort to general agreement among a group of mathematically talented superstring physicists like Witten, Maldecena et al.

This makes me wonder about the claim (which I'm entirely unqualified to understand) that there is this dichotomy between S vs s superstring theory. I would *assume* that the distinction is based on some mathematically proven line of definitions/theorems in the capital S, but Susskind's fondness for Nth degree reliance on conjectures gives me pause.

Maybe time will be kind to these towers of conjectures with future generations of mathematicians/physicists following up with rigorous and formal methods, but it seems a shame that this generation is not as interested in going back and putting in the work of justifying the conjectures at the root of the tower.

8. **Low Math, Meekly Interacting**
March 4, 2023

I was also puzzled by the technicolor comment. It seemed like practically no one was talking about that when the LHC turned on. It was all sparticles lighting detectors up like Christmas trees. And now the world isn't supersymmetric at all? Why? I thought the whole "lesson" of SUSY heaving nothing to do with EW symmetry breaking is that there's a Landscape and SUSY could be broken at any scale you could imagine. Because there is no string (capitalized or not) without SUSY. I can't keep it all straight anymore. What goalpost is even left to move at this point?

9. **Peter Woit**
March 4, 2023

Jerome,
I'd be wary of any belief in illumination re the goals of the Swampland program. The only things clear to me about the motivations for this program are
1. they would like to challenge the idea that KKLT-type vacua are consistent, implying no predictivity.
2. they would like to claim that having a consistent theory of quantum gravity implies constraints on unified theories, string theory qualifies, so they have "string theory predictions".
Beyond this though, I don't understand what their vision is for a predictive theory, and Susskind seems to share my confusion about this.

10. **Peter Woit**
March 4, 2023

Adam Treat,
The distinction between string and String theory is exactly Susskind's attempt to address this. I think he's acknowledging that "string theory" now just means some very speculative "string-inspired" scenario, yes, with conjecture built on conjecture. As he says, the way to characterize this is
"Who knows! But it's exciting to be in a situation where there is confusion. "

The problem for me is that I don't see any way to have a sensible discussion about this sort of "Who knows!" string theory. Nothing wrong with people working this way if it leads somewhere, but all the evidence is that for decades this has gone nowhere.

He doesn't say exactly what "String theory" is, but it is the sort of thing I used to spend a lot of time trying to understand and argue with people about exactly what is going on. His claims that this is completely well-defined mathematics that mathematicians build on and get Fields medals for I don't think is true. But, in any case, he agrees this cannot give you a theory of the real world.

11. **Peter Woit**
March 4, 2023

LMMI,
By the time the LHC had turned on, the problems with the technicolor idea were well-known and most people were skeptical that this could work. It remained possible that something was being missed, that the LHC would turn up not the standard Higgs, but something that was a variant of the technicolor idea, perhaps an unexpected variant (I would have loved that to have happened…).

Susskind clearly isn't claiming that everyone thought technicolor was right until the LHC. He seems to say there was some point at which it "turned out to be wrong". Not sure what that would be, except it happened long ago.

12. **Topological twist**
March 7, 2023

My understanding of capital S "String theory" as a mathematician is that there are rigorous definitions of objects that could be called "topological strings" of "type IIA and type IIB". Relevant work here is due to Kontsevich-Soibelman, Costello, and Lurie. These are 2d oriented topological field theories valued in chain complexes, meaning that there are operations on some chain complex parametrised by chains on the moduli space of Riemann surfaces with disjoint homomorphically embedded discs, and some compatibilities with sewing these Riemann surfaces along embedded discs. But this seems to be a far cry from making rigorous sense of the finer structure of N=2 d=2 SCFT associated to Calabi-Yau threefolds, which seems to be the minimum of what a string theorist would mean about "String theory" of type II.

13. **Peter Woit**
March 7, 2023

Topological twist,
Yes, the things mathematicians can make sense of and that have had significant impact in pure mathematics are things like topological strings, not anything like the String theories Susskind is referring to which try to model the real world but require exact SUSY.

Not much point though in arguing about this with Susskind though, since he is conceding that the theories he is making these dubiously optimistic claims about can't be a theory of the real world.

14. **Interested amateur**
March 8, 2023

Peter, since you've mentioned SUSY in the comments, you might be interested to know that Ben Allanach of Cambridge University UK gave an interview two years ago, which can be found on Youtube: Supersymmetry and Particle Physics | Ben Allanach.

"up to last year I spent all my career working on the supersymmetry theory… it was a bridge to everything…so yeah I had to rethink last year and now I'm working in a different mode, I'm looking at the data, and looking for glitches, and so we predict with the Standard Model of particle physics"

The LHC has done a very successful hatchet job, and not just on Susskind's Technicolor.

15. **Amitabh Lath**
March 10, 2023

The "killing" of Technicolor was carried out by precision ElectroWeak measurements at SLD and the detectors at LEP1 . Peskin and Takeuchi created a set of variables (S, T) which parametrized the isospin conserving and isospin breaking part of BSM physics. If you put the SM at 0,0 in an S,T plot then Technicolor is off in the upper left somewhere. The various EW asymmetries (and also atomic parity violation measurements) shut the door on any large deviations in S,T space. By the time the LHC turned on Technicolor was long dead.
(Full disclosure: my PhD thesis was the measurement of the Left-Right Asymmetry of Z bosons made with polarized electrons).

16. **Leonid**
March 12, 2023

Composite Higgs models and Walking Technicolor models are some natural evolution of the original technicolor. These were seriously probed at the LHC, where the mass value of the Higgs boson, the production rate of Higgs signals and the exclusion of top partners were the main tests. Perhaps Susskind meant these generalized technicolor models.

17. **Topologist Guy**
March 12, 2023

Peter,

Trying to grasp the precise difference between "big s" and "small s" string theory. Is this distinction along the lines of what Susskind is referring to:

1. String theory: Perturbative superstring theory on a flat Minkowski background. This is a well-defined theory, at least up to physical rigor.

2. string theory: Some conjectured nonperturbative background-independent thing which reduces to String theory in the appropriate limit.

18. **Peter Woit**
March 13, 2023

Leonid,
I think the much earlier SLD/LEP1 results Amitabh Lath mentions are more likely what Susskind is referring to. I don't remember him (or anyone else) having much enthusiasm for Technicolor by the time the LHC turned on.

Topologist Guy,
Susskind seems to be specifically pointing to the AdS/CFT picture of non-perturbative String theory as defined as the dual of N=4 SYM on the boundary, something that goes much beyond perturbative String theory. It's debatable whether that's as well-defined as Susskind claims, but since he agrees it can't give you the real world, not worth debating.

The distinction he is making centers more on supersymmetry, where he's making the (correct…) argument that the world is not supersymmetric, but there exists no well-defined non-supersymmetric string theory.

19. **JdM**
March 14, 2023

An older (2008) article from Lenny on Technicolor (and Supersymmetry):

Thoughts on a Long Voyage
https://www.worldscientific.com/doi/epdf/10.1142/9789812779762_0005
(also: https://www.worldscientific.com/doi/abs/10.1142/S0217751X08042316)

Savas Dimopoulos wrote on TC back in 1994 (https://arxiv.org/abs/hep-ph/9412297) and again in 2000 (https://arxiv.org/abs/hep-th/0105034) – these two articles are *almost* identical.

20. **Peter Woit**
March 14, 2023

JdM,
Thanks. The Dimopoulos comment
"By the summer of 1980 it became clear that these theories suffered from generic problems of flavor violations [12] that could perhaps be cured only by complicating the theory immensely and losing any hope of calculability."
agrees with my memory that very soon after Technicolor came on the scene, people lost interest once they started looking at the kind of Extended Technicolor models needed to agree with experiment.

Not Quite What Happened

March 23, 2023
Categories: Wormhole Publicity Stunts

Quanta has an article out today about the wormhole publicity stunt, which sticks to the story that by doing a simple SYK model calculation on a quantum computer instead of a classical computer, one is doing quantum gravity in the lab, producing a traversable wormhole and sending information through it. From what I've heard, the consensus among theorists is that the earlier Quanta article and video were nonsense, outrageously overhyping a simulation and then bizarrely identifying a simulation with reality if it's done on a quantum computer.

The new article is just about as hype-laden, starting off with:

> A holographic wormhole would scramble information in one place and reassemble it in another. The process is not unlike watching a butterfly being torn apart by a hurricane in Houston, only to see an identical butterfly pop out of a typhoon in Tokyo.

and

> In January 2022, a small team of physicists watched breathlessly as data streamed out of Google's quantum computer, Sycamore. A sharp peak indicated that their experiment had succeeded. They had mixed one unit of quantum information into what amounted to a wispy cloud of particles and watched it emerge from a linked cloud. It was like seeing an egg scramble itself in one bowl and unscramble itself in another.
>
> In several key ways, the event closely resembled a familiar movie scenario: a spacecraft enters one black hole — apparently going to its doom — only to pop out of another black hole somewhere else entirely. Wormholes, as these theoretical pathways are called, are a quintessentially gravitational phenomenon. There were theoretical reasons to believe that the qubit had traveled through a quantum system behaving exactly like a wormhole — a so-called holographic wormhole — and that's what the researchers concluded.

An embarrassing development provides the ostensible reason for the new article, the news that "another group suggests that's not quite what happened". This refers to this preprint, which argues that the way the Jafferis-Lykken-Spiropulu group dramatically simplified the calculation to make it doable on a quantum computer threw out the baby with the bathwater, so was not meaningful. The new Quanta piece has no quotes from experts about the details of what's at issue. All one finds is the news that the preprint has been submitted to Nature and that

> the Jafferis, Lykken and Spiropulu group will likely have a chance to respond.

There's also an odd piece of identity-free and detail-free reporting that

> five independent experts familiar with holography consulted for this article agreed that the new analysis seriously challenges the experiment's gravitational interpretation.

I take all this to mean that the author couldn't find anyone willing to say anything in defense of the Nature article. An interesting question this raises is that if all experts agree the Nature article was wrong, will it be retracted? Will the retraction also be a cover story?

The update of the original story is framed by enthusiastic and detailed coverage of the work of Hrant Gharibyan on similar wormhole calculations. The theme is that while Jafferis-Lykken-Spiropulu may have hit a bump in the road, claiming to be doing "quantum gravity in the lab" by SYK model calculations on quantum computers is the way forward for fundamental theoretical physics:

> The holographic future may not be here yet. But physicists in the field still believe it's coming, and they say that they're learning important lessons from the Sycamore experiment and the ensuing discussion.
>
> First, they expect that showing successful gravitational teleportation won't be as cut and dry as checking the box of perfect size winding. At the very least, future experiments will also need to prove that their models preserve the chaotic scrambling of gravity and pass other tests, as physicists will want to make sure they're working with a real Category 5 qubit hurricane and not just a leaf blower. And getting closer to the ideal benchmark of triple-digit numbers of particles on each side will make a more convincing case that the experiment is working with billowing clouds and not questionably thin vapors.
>
> No one expects today's rudimentary quantum computers to be up to the challenge of the punishingly long Hamiltonians required to simulate the real deal. But now is the time to start chiseling away at them bit by bit, Gharibyan believes, in preparation for the arrival of more capable machines. He expects that some might try machine learning again, this time perhaps rewarding the algorithm when it returns chaotically scrambling, non-commuting Hamiltonians and penalizing it when it doesn't. Of the resulting models, any that still have perfect size winding and pass other checks will become the benchmark models to drive the development of new quantum hardware.
>
> If quantum computers grow while holographic Hamiltonians shrink, perhaps they will someday meet in the middle. Then physicists will be able to run experiments in the lab that reveal the incalculable behavior of their favorite models of quantum gravity.
>
> "I'm optimistic about where this is going," Gharibyan said.

I had thought that perhaps this fiasco would cause the Quanta editors to think twice, talk to skeptical experts, and re-report the original credulous story/video. Instead, it looks like their plan is to double down on the "quantum gravity in the lab" hype.

Update: Two more related pieces of wormhole news.

- On Friday Harvard will be hosting a talk on the non-wormhole.
- In this preprint Maldacena argues for another example of how to do quantum gravity in the lab, by doing a QM calculation on a quantum computer that will "have created something that behaves as a black hole in the laboratory" (no wormholes, just black holes). The calculation he suggests involves not the newer SYK model, but the ancient BFSS matrix model from 27 years ago, which at the time got a lot of attention as a possible definition of M-theory.

Update: The Harvard CMSA talk about the wormholes is available here. I didn't see anything in the slides about the Yao et al. criticism of this work. In the last minute of the video there was a question about this, and some reference to the criticism having been addressed during the talk. Supposedly there was some quick verbal summary of this response to the criticism in this last minute, but the sound was so garbled I couldn't understand it. Here's the automatically generated transcript:

> 1:16:50
> so I guess I guess um we're talking about like at the time of interpretation you do see this
> 1:16:56
> operating ghost in kind of declare the two-point function if you're looking for at later times you can ask about

1:17:01
different kind of scenarios one is accepting the single-sided systems what it's doing it's like internal reversible
1:17:07
verbal hamiltonian and you see thermalizing Dynamics in the library
1:17:12
um perhaps also the size winding uh although it's not necessarily required
1:17:18
for all of your fermions to show size winding because you have done gravitational attractions in your model we do see impact that all the pronouns
1:17:26
have quite good size winding they're good enough to allow them to teleport to size binding but the time and size
1:17:31
binding is clearly related to like the the rate of Decay the two-point function and so it seems to actually lend itself
1:17:38
to an even tighter kind of interpretation where would you associate different masses through different
1:17:44
permeons and this is quite consistent that is

Someone with more patience and interest in this perhaps can carefully follow the talk and report what the response to the Yao et al. criticism actually was.

Update: A response by the original authors to Yao et al. has been posted as "Comment on "Comment on "Traversable wormhole dynamics on a quantum processor" " ". From the abstract, the claim seems to be that the results of the toy model calculation are "consistent with a gravitational interpretation of the teleportation dynamics, as opposed to the late-time dynamics", and that this is not in conflict with the objections by Yao et al. These objections are described as "counterfactual scenarios outside of the experimentally implemented protocol." The odd thing here is the description of the quantum computer calculation as a "factual" experimental result, part of an "experimentally implemented protocol". The quantum computer calculation was not an experiment but a calculation, with a known-in-advance result (the calculation done previously on a classical computer). The criticisms of Yao et al. aren't "counterfactual" to an experimental protocol, but challenging the interpretation of a calculation. As far as I can tell, this whole discussion is about how to interpret simple calculations you can do on any conventional computer, nothing to do with an "experiment".

Comments

1. **Anonymous**
   March 23, 2023

   I would like to point out the parallel to the room-temperature superconductivity debacle, which to me appears to be a similar instance of the postmodern approach to doing physics research.

2. **Peter Woit**
   March 23, 2023

   Anonymous,

   If you're referring to the recent room-temperature superconductivity controversy, it's interesting to notice that the same Quanta writer (Charlie Wood), who wrote this hype about "quantum gravity in the lab", wrote what seems to me a much more even-handed article about that, see here
   https://www.quantamagazine.org/room-temperature-superconductor-discovery-meets-with-resistance-20230308/
   A story about an experimental development is somewhat different than one about theoretical developments. Another group can try and reproduce an experiment, and people promoting non-reproducible work will run into skepticism. For theorists there's no such thing: you can keep repeating the same nonsense about quantum gravity for ever with impunity.

   The odd thing about the "quantum gravity in a lab" hype is that all the top Quanta physics writers and editors have gone all in (Natalie Wolchover, Thomas Lin, Charlie Wood). I've heard from several experts that they tried to explain to them the problems with their coverage, but they're not listening to skeptical voices, preferring to (like the director of the IAS), take the word of a small number of people like Maldacena that quantum gravity in the lab is a real thing and the future of the subject.

   Built into the DNA of the Simons Foundation and Quanta I think is a faith in the traditional elite institutions of the US theoretical physics world. That people at the IAS/Harvard/Caltech/Stanford could just be wrong about string theory/quantum gravity/it from qubit, etc. to this world-view is nearly inconceivable.

3. **Peter Shor**
   March 23, 2023

   Don't a lot of the latest quantum gravity theories say that quantum gravity goes beyond the standard rules of quantum mechanics? For example, the AMPS paper (Black Holes: Complementarity or Firewalls?) seems to imply that something really unusual must happen. But if you can do quantum gravity in the lab, then doesn't quantum gravity have to satisfy the standard rules of quantum mechanics?

   Do any of the people writing these papers see any contradictions here?

4. **Rusty**
   March 23, 2023

   Is there anything going on in this experiment/calculation
   that can't be explained by ordinary, non-relativistic QM?

5. **Peter Woit**
   March 23, 2023

   Peter Shor/Rusty,
   That's the obvious problem with all "quantum gravity in the lab" claims. The "lab" setup is described by conventional well-tested QM/QED, and you're not planning on testing that. So, as a matter of physics, you know exactly what is going to happen, there's no need to do the "experiment" and it can't tell you anything new about physics.

   The only sense in which you don't know what's going to happen is the same sense in which any numerical calculation may tell you something new about solutions to an equation. But if I solve an equation describing a cow on my laptop, that doesn't mean I created a cow, or that I'm doing lab experiments on cows.

6. **Sabine Hossenfelder**
   March 24, 2023

I think the underlying problem here isn't Quanta, it's Nature. Nature has a bias for publishing anything that can be experimentally done, regardless of how meaningless the theoretical interpretation. This is isn't the first time this has happened. Nature has published several previous papers on this wormhole stuff (without the teleportation part). They also keep publishing anything about ultracold gases so long as someone claims it's got something to do with black holes or the early universe. (Don't get me started.)

Otoh, you'd never see them publish a theory paper in that area, and probably for the better because most of it is nonsense. So you get those theorists teaming up with experimentalists to get their stuff published in Nature and chances are the theory-part is never really scrutinized.

This doesn't really excuse that the Quanta editors can't seem to admit to themselves that they're promoting bullshit, but with Nature in their back they know they'll get through with it.

The issue with the superconductor claim I think is somewhat different. It doesn't originate in the journal, but it originates in the lab itself, and there's only so much peer review can do even in the best case.

7. **John**
   March 24, 2023

   Sabine,
   I don't think Nature is completely innocent in the superconductivity story, given the history of retractions from the same authors.

8. **André**
   March 24, 2023

   To be fair, the whole issue of claiming that doing a simulation is somehow equal to studying the real thing isn't really new. There has been similar hype coming from the analog gravity community for years (obviously, I know a lot of really decent researchers in analog gravity who are very careful about stating precisely the relevance of what they are doing, but there are also those who are very loudly claiming that they are doing "gravity in the lab" when looking at water waves or BECs).
   I always ask those people what their experiments tell us that you couldn't learn from numerical simulations, and most of the time the answer is nothing – with the additional caveat that numerical simulations have smaller statistical errors.

   I also do not understand what it is that science communicators (journalists/nature editors/…) find so unattractive about the story: We have these complicated equations that are thought to tell us something about gravity and now, thanks to quantum computers, we can learn much more about their solutions which we didn't previously understand. [Notwithstanding that the second part of this statement is probably not true yet, and the first may be not true, period.]

   My PhD supervisor used to say (not sure if his words or an unattributed quotation): Don't trust an experiment until verified by theory.

9. **jack morava**
   March 25, 2023

   It occurs to me that the best way to understand the quantum computer emulation of a wormhole is as an example of the law of similarity in

   https://en.wikisource.org/wiki/The_Golden_Bough/Sympathetic_Magic

10. **Dimitris Papadimitriou**
    March 26, 2023

    André

    I don't think that there is really an analogy between the Analog gravity papers/ experiments and the recent "wormhole" case.
    In the former, there is an actual similarity in the math description.
    In the latter, there is a conjectured duality between two different descriptions.
    The case for Analog gravity, although it has its limitations, is much more solid, not that controversial as the other.

11. **Peter Woit**
    March 26, 2023

    Dimitris ,
    The problem with analog "gravity in the lab" claims is a different one, but it's just as much nonsense, since your experiment again is not looking at gravitational degrees of freedom. You're testing whether an equation does a good job of modeling the analog system, that the same equation appears in a theory of gravity is irrelevant.

    If I'm studying cows and one of my equations modeling the cows is the same as one describing something happening near a black hole, I'm still studying cows, not black holes.

12. **Sabine Hossenfelder**
    March 26, 2023

    John,

    "I don't think Nature is completely innocent in the superconductivity story, given the history of retractions from the same authors."

    Of course they knew this too. What do you expect them to do? Reject a paper just because of prior history? That wouldn't be proper treatment either.

13. **Jesko Sirker**
    March 26, 2023

    Sabine,
    the analysis of Hirsch and Marsiglio has raised some very serious doubts about the first retracted Nature paper on room-temperature superconductivity. Their analysis seems to suggest that raw, measured data might never have existed but rather were later constructed based on the published curves. I think in such a case the onus is on the authors to provide more information about the experiment and the data analysis to the physics community before other papers by the same group can be considered for publication. Besides, the new publication is marred by the same issues as the previous one: most prominently, a zero resistance is not measured but rather defined by again subtracting some 'background'. This is not standard in the field. Several studies have already been published where none of the results in the new Nature paper could be reproduced. This whole story is very unfortunate and could have been easily avoided if Nature would have acted more responsibly.

14. **Peter Woit**
    March 26, 2023

All:
Enough about the high-Tc story, which has nothing to do with the topic of this posting.

15. **Anon**
March 27, 2023

Update:
https://arxiv.org/pdf/2303.15423.pdf

16. **Peter Woit**
March 27, 2023

Anon,
Thanks!
I guess we'll have to now wait for "Comment on "Comment on "Comment on….

Or, maybe Quanta can contact its five anonymous experts to weigh in.

17. **anon**
March 28, 2023

I can simulate the Navier-Stokes equation on a computer by discretizing the equation and putting it on a grid. There is a good theory of how the errors introduced in the discretization process scale with the size of the grid. There are proofs that the errors go to zero as the grid size goes to zero.

I don't think the wormhole people have any similar kind of theory for how to estimate the errors in their approximations. So it feels to me like this conversation is going to degenerate into "feelings" about whether some output from the computer feels like this or that.

If they were calculating finite N SYK, then they could estimate their errors (the differences between finite N and infinite N). But they switch to a fully commuting Hamiltonian that "looks like" finite N SYK. I don't think they have any control over the errors that that step introduces.

18. **Matthew Foster**
March 28, 2023

Peter,

"If I'm studying cows and one of my equations modeling the cows is the same as one describing something happening near a black hole, I'm still studying cows, not black holes."

This is true of course, but misses a potential advantage of such "analog" simulations. If the same equations govern both cows and black holes, and you do the experiment with cows, you may discover new emergent or nonlinear phenomena of the cows that you didn't anticipate or understand how to extract from the equations. Most of us humans remain pretty feckless in the face of strong nonlinearity. Assuming that the same equations approximately apply in a corresponding regime of validity for black holes, one might anticipate or propose new physical phenomena in the latter.

Replacing actual experiments with numerics, some of the insights with respect to SYK and its holographic dual(s) have emerged this way, e.g. the effects of the sum over diffeomorphism fluctuations.

19. **André**
March 29, 2023

Matthew,

"If the same equations govern both cows and black holes, and you do the experiment with cows, you may discover new emergent or nonlinear phenomena of the cows that you didn't anticipate or understand how to extract from the equations."

That is correct, of course, but I think the relevant question is not whether there might be such new phenomena that you don't find from analytically studying the equations, but if studying real cows gives you any advantages (precision, practicality, …) compared to a numerical simulation of said equations. If the answer is no, then I see no point in studying the cows.

20. **eitan bachmat**
March 29, 2023

Dear Peter
you say
"Built into the DNA of the Simons Foundation and Quanta I think is a faith in the traditional elite institutions of the US theoretical physics world. That people at the IAS/Harvard/Caltech/Stanford could just be wrong about string theory/quantum gravity/it from qubit, etc. to this world-view is nearly inconceivable."

I am not sure its reading correctly the DNA of the Simons Foundation if there is one. If you look at all their MPS collaborations across all domains, there seem to be a few general themes. One main theme seems to be the teaming of people and ideas from math, physics (both theory and applied), computer science and engineering (both theory and applied), in short, trying to build interdisciplinary bridges. Another less dominant, but still important theme seems to be symmetries and dualities as powerful tools in math, physics and engineering. A faint theme, but which still seems to have some effect is classifying objects, which is a very typical mathematical activity. I think this accounts for much of their activity for better or worse and within your field of interest, this leads to a thematic bias towards the more mathematically powerful among practitioners, even if the physics is completely off, dualities and other ideas that arise commonly in string theory, or quantum field theory more generally, are extremely powerful in relating different pieces of math or even computer science, and this bias leads indirectly to your perception of the choices.

21. **Peter Woit**
March 29, 2023

eitan bachmat,
The context of that comment is one of trying to understand why a highly competent journalistic operation like Quanta would be taken in by this hype. Contrast the Quanta wormhole coverage to that at the NY Times or other prominent places.

Also, the comment was not about the topics the Simons Foundation favors (on the question of topics, I think there's a prejudice towards computational projects, but the Foundation is now funding a wide range of different sorts of things). From what I have seen over the years, Simons likes to fund elite institutions (places like Berkeley, the IAS, IHES, etc). Other donors often take a different approach, sometimes explicitly trying to build up non-elite institutions. On the whole, I'm somewhat of an elitist myself, believing that historically elite institutions have been the source of great work, and that their health is important to maintain for the health of the whole field. But, when elite institutions are captured by failed or over-hyped research programs, that’s a big problem, and one that I think Quanta is ill-equipped to recognize.

A Muon Collider?

March 30, 2023
Categories: Uncategorized

The US particle physics community has been going through a multi-year process designed to lead up this fall to a 10 year strategic plan to be presented to the DOE and the NSF. In particular, this will generate a prioritized list of what projects to fund over this period. The process began with the Snowmass self-study, concluded last year, and available here. Since last fall there have been two independent efforts going on:

- A National Academies study has been holding meetings, materials available here.
- A P5 (Particle Physics Project Prioritization Panel) is holding meetings, see here, and planning for a report to NSF and DOE by October.

Looking through all the materials relevant to particle theory, there seems to me little acknowledgement of the serious problems faced by the subject, or any new ideas for how to address these problems. Most of the effort though is devoted to where most of the money will be spent, on the experimental side. To a large degree, for the short-term it's clear where funding has to go (to continue supporting the LHC into the HL-LHC era, and finish building the DUNE/LBNF US neutrino project). The longer-term is however very uncertain, as it is unclear whether there's a viable energy-frontier project that could study higher energies than those accessible at the LHC.

Last week EPP2024 and P5 held Town Hall events at Fermilab, see here and here. There's video of the EPP2024 event here. On the question of the long-term future, one issue that is getting a lot of attention is that of whether to prioritize development of a possible muon collider. In this presentation a young physicist gives a future timeline including their likely retirement and death dates, showing that a muon collider is their only hope for new energy frontier physics during their lifetime. For those of my age the situation is a bit different, since even a muon collider is not going to do the job. At the EPP2024 event (3:28 in the video) Nima Arkani-Hamed makes the case that:

> I think the subject has not been so exciting for many, many decades, and at the same time our ability to experimentally address and solidly settle some of these very big questions has never been more uncertain. I don't think it's a normal time, it's an inflection point in the history of the development of our subject, and it requires urgency… The confluence of the technical expertise for doing so and the enthusiasm amongst the young people who are willing to do it exists now and I very much doubt it will exist in 10 or 15 years from now. If we are going to do it, we have to start thinking about doing it now.

While his point is more general, he's clearly making the case for starting a new energy frontier machine project soon, with the muon collider the one possibility for getting to higher energies than the LHC.

A few weeks ago there was a workshop at the KITP devoted to the muon collider question, with a news story here (anyone know why the video of the panel discussion is password-protected?). Arkani-Hamed gave a talk aimed at other physicists here. On the European front, a couple days ago there was this meeting.

Already twenty years ago when I was writing *Not Even Wrong*, it was clear that a muon collider was in principle a very attractive idea for how to get to higher energies and I wrote about this in the first chapter of the book. The much higher mass of the muon than the electron means that you don't have the same synchrotron energy loss problem, so can build a much smaller storage ring at the same energy, or get to much higher energies with the same size. The problem though is that muons have a life-time of only 2.2 microseconds. This implies two serious difficulties:

- You need to produce, store, accelerate and collide the muons in a very short period of time.
- As the muons decay they'll produce large numbers of high energy electrons and neutrinos, creating a difficult environment for detectors to operate in and significant radiation hazards.

Normally one thinks of neutrinos as virtually never interacting with anything, but the numbers and high energies of the neutrinos produced at a muon collider create a potential significant radiation hazard, one that cannot be dealt with by shielding.

While I might not be around to see the results from a muon collider, if such a thing is viable, I'd strongly support such a project, and would even buy a t-shirt. The US DOE HEP budget is about a billion dollars/year. One would think this should be enough to accommodate building demonstrator projects or a small collider ring on a 10 year timescale, and possibly even an energy-frontier ring on a 20 or more year timescale. What's worrying me a bit is the fact that more visible progress on this hasn't happened since I looked into it 20 years ago. Why no current demonstrator project? Have the potential radiation hazard issues found solutions? I'd be very curious to hear from anyone with expertise on these questions.

Comments

1. **Patrick Meade**
   March 30, 2023

   Hi Peter, for the most recent status you can look in the Snowmass muon collider forum report: https://arxiv.org/abs/2209.01318 there actually has been considerable progress recently which is why you see increased theory community support. I'm giving a Columbia HEP seminar next week on muon colliders so you're welcome to attend and any questions not covered by the report can hopefully be answered!

2. **Severing Pappadeux**
   March 30, 2023

   Mu cooling needs to be demonstrated for collider to be feasible. MICE is not doing very good

3. **Mark**
   March 31, 2023

   The MICE collaboration demonstrated muon cooling with an experiment at the Rutherford Appleton Laboratory. I understand this is needed for a future muon collider, so there is not zero progress on topics needed to be demonstrated . Would a collider demonstrator have been useful (or even feasible at all) without first demonstrating we can cool the muons enough for a real system?

4. **Nathaniel Craig**
   March 31, 2023

   Thanks for taking a look at the KITP program! Regarding the panel — we wanted to encourage the panelists to engage in critical and constructive dialogue (half the panelists were asked to highlight challenges). We felt they would be able to speak most freely if the recording was not public. I think the panel was very successful, and we are working on a transcript for public release.

5. **Andrea Wulzer**

April 1, 2023

Thank you for the interest in muon collider activities. The International Muon Collider Collaboration (IMCC, https://muoncollider.web.cern.ch) is active, with mostly European participants for now. Here a link to the IMCC meetings and conferences (https://indico.cern.ch/category/11818/) and to the first IMCC report (https://arxiv.org/pdf/2303.08533.pdf). Section 3 of the report helps answering your questions.

6. **John Baez**
   April 4, 2023

   Can someone give me some figures about the neutrino emissions from a muon collider, and how they could be high enough to be dangerous? That's surprising!

7. **Peter Woit**
   April 4, 2023

   John,
   For an early study, see
   https://arxiv.org/abs/physics/9908017
   something more recent
   https://indico.fnal.gov/event/48995/contributions/214442/attachments/143292/181217/MC_Neutrino_Radiation_Mokhov_May19_2021.pptx

   This is surprising, one normally thinks about neutrinos going through the earth without interacting with anything. But neutrino cross-sections increase with energy, it's high energy neutrinos that are the problem, and a TeV scale muon collider would produce a lot of them.

8. **Alon Faraggi**
   April 4, 2023

   There is a ready made solution to the US experimental particle physics program on the energy frontier on a time scale of 10 years. Resurrect the SSC. Possibly an upgraded version with a 100km circumference and 30TeV beam energy. The muon collider is bread & butter accelerator physics. The MICE experiment took a long while to deliver (~25 years). But eventually it did prove the feasibility of the muon beam phase space cooling (https://www.nature.com/articles/s41586-020-1958-9). As a physics machine the muon collider is a prospect of 50-100 years. Pure fantasy.

9. **Severin Pappadeux**
   April 6, 2023

   John,

   earliest is probably by N. V. Mokhov and A. Van Ginneken, "Muon Collider
   Neutrino Radiation", Muon Collider Collaboration Meeting
   Book, Orcas Island, WA, May 1997.

   or

   https://accelconf.web.cern.ch/p99/PAPERS/THP52.PDF

10. **Severin Pappadeux**
    April 6, 2023

    MICE demonstrated that muons (being counted one-by-one) going through some LH2 follow well our multiple scattering, struggling, elastic etc models. That's about it. What has to be done is (close to) full scale cooling cell, with intensive muon beam, material which could handle above mentioned beam, solenoid field, RF cavities etc.

    And it means there is ought to be reasonable target setup producing muons.

    We're talking about serious accelerator test facility, and 10 years program on the scale of $50mln/year.

    Until such program is up, approved and running, all muon collider talks will be confined to conferences.

11. **John Baez**
    April 8, 2023

    Thanks, Peter, that's fascinating. It's interesting how being underground would increase the danger of these neutrino beams.

Who was “Not Even Wrong” first?

April 10, 2023
Categories: Uncategorized

I recently heard from John Minkowski, whose father Jan Minkowksi was a student of Pauli’s in the late 1940s. He asked if I knew what the specific context of Pauli’s “Not Even Wrong” comment was, and I told him I didn’t. I referred to this early blog post, which explains that Karl von Meyenn (editor of Pauli’s correspondence) had pointed me to a biographical memoir about Pauli by Rudolf Peierls which includes:

> Quite recently, a friend showed him the paper of a young physicist which he suspected was not of great value but on which he wanted Pauli’s views. Pauli remarked sadly ‘It is not even wrong.’

Looking around for any more information about this, Wikipedia links to a 1992 letter to the editor at Physics Today from Peierls, which states

> Wolfgang Pauli’s remark “Das is nicht einmal falsch” (“That is not even wrong”) was made not as a comment on a seminar talk but as a reaction to a paper by a young theoretician, on which a colleague (I believe it was Sam Goudsmit) had invited Pauli’s opinion.

Google also turned up a translation of a talk by Peierls in this article by Mikhail Shifman, which includes:

> Somebody showed to Pauli a work of a young theorist being well aware that the work was not too good but still willing to hear Pauli’s opinion. Pauli read the paper and said, with sadness: “It is not even wrong.”

Trying to guess what the article in question might have been, I’m tempted by the hypothesis that the discussion with Goudsmit was about Everett’s “Relative State” Formulation of Quantum Mechanics paper. The timing (“Quite recently”) would have been right, with the paper published in July 1957, Pauli’s death later in December 1958. Goudsmit at the time was editor-in-chief at Physical Review, so would have been interested in Pauli’s opinion of the paper.

Complicating this story, John Minkowki sent me some pages from his father’s 1991 book Through three wars: The memoirs of Jan Michael Minkowski, which included this (in a context describing his 1946-48 student days at ETH):

> I remember a seminar in theoretical physics given by a visitor from another Swiss university. These seminars were presided over by Dr. Pauli, and after the speaker finished all eyes would turn to Pauli to pronounce the verdict in his commentary. This particular lecture was treated by Pauli with progressively faster twirling of his thumbs around and around one another and a growing benevolent smile. Bad sign, we thought. The more he smiled the more vicious he will be, we thought. And sure enough, he smiled some more and said “It isn’t even wrong.”

One possibility here is that Minkowski was mis-remembering something from forty years earlier, another is that the occasion that Peierls was referring to was not the first time Pauli had used the phrase. As evidence for the second hypothesis, see this interview with Konrad Bleuler, which points to the possibility of Stueckelberg as the “visitor from another Swiss university”:

> So these seminars took place in a common seminar having also Professor Ernst Stueckelberg, then a Professor in Geneva, also Stueckelberg being a well-known theoretician, his work was very much, if I might remind you of that fact, acknowledged by Richard Feynman. For example, his idea of the particle going back in time being interpreted as an antiparticle came as far as I know originally from Stueckelberg and many other great ideas. I remember one special seminar in which, of course this seminar could be rather called. High Court, with scientific papers in the docket, sometimes really sentenced to death. From that one might record Pauli’s classification of scientific papers. There were two classes or else there were old and right. Or the other class, new and wrong. But hardly anything intermediate. If it was even worse, Pauli would have said “it’s not even wrong.” That was the kind of atmosphere. But all what is written in physics is either understood or else it’s thrown away, and not this half-and-half, what we see at present. But then in this connection it was a search for truth. And for Pauli, a lecture hall was a kind of a holy place where only truth was allowed. And a wrong statement was a sacrilege, and in that sense one should understand his rather extremely sharp remarks he might make to some lecturer who seemed not to present things in a quite logical way. But coming to that special, to another special seminar is the following: Stueckelberg always knew really special — I might say prophetic — ideas. He gave a lecture and of course Pauli — it happened very often — didn’t agree. And said “you are not allowed to say such things.” But you see, Stueckelberg being a prophet, he’s not so easily stopped uttering his prophecies. So Pauli in despair menaced Stueckelberg with a stick and it seemed — I was not present myself but I was told — that the seminar ended like the war of Troy, Pauli, rather corpulent, with his stick after Stueckelberg around the table in the lecture hall. That was the kind of attitude at this period.

I’m not sure what to make of all of this. Perhaps Pauli used the phrase both in the late 40s to criticize Stueckelberg (probably unfairly since many of Stueckelberg’s ideas were ahead of his time) and then Everett in the late 50s (in my opinion accurately, but I don’t want to start up the usual empty arguments about MWI here).

Comments

1. **paddy**
   April 10, 2023

   Concerning your usage relative to string theory: did S. Glashow use it first in that context?

2. **Peter Woit**
   April 10, 2023

   paddy,
   Well before string theory took off in late 1984, Pauli’s phrase was well known among physicists. I don’t remember the first time I heard it, but it was surely pre-1984 and not uncommon in those days to hear it used in reference to poorly understood speculative ideas unlikely to go anywhere. While post-1984 string theory quickly attracted a lot of enthusiastic followers, there always were a large number of skeptics, many of whom might have privately been using the phrase to describe their view of the theory. I don’t know if Glashow was the first published example of this.

3. **Sheldon from The Big Bang Theory**
   April 10, 2023

   See the 1986 May issue of Physics Today where Glashow uses it.

   There are dozens of references to the fuller quote online.

   “The trouble is that most of superstring physics lies up at the Planck mass — about
   10 GeV – and it is a long and treacherous road down to where we can see the light of

day. A naive comparison of length scales suggests that to calculate the electron mass from superstrings would be a trillion times more difficult than to explain human behavior in terms of atomic physics. Superstring theory, unless it allows an approximation scheme for yielding useful and testable physical information, might be the sort of thing that Wolfgang Pauli would have said is "not even wrong." It would continue to attract newcomers to the field simply because it is the only obvious alternative to explaining why certain detectors light up like video games near the end of every funding cycle." –Sheldon Glashow, Nobel Laureate & Paul Ginsparg, Ph.D., Desperately Seeking Superstrings

4. **Low Math, Meekly Interacting**
April 10, 2023

I've only seen the account you originally cited, and find it the most plausible. Aside from his exclusion principle and his matrices, Pauli seems to be most famous for his insults. The Dorothy Parker of physics, as it were. That one, being especially pithy and cutting, makes such an impression I imagine it would have been dutifully attested and widely circulated almost as soon as any public lecture concluded. It would have already been a cliche by 1957.

5. **Peter Woit**
April 10, 2023

LMMI,
The problem with the Peierls story is that an origin in a private conversation between Pauli and Goudsmit in 1957 doesn't explain how this became so widely known. He doesn't explain how he knows about it, not explicitly saying he was there to hear it, giving the impression that his information is not first hand. During the 47-57 decade it looks like Peierls was in Birmingham, possibly stories like this from a Zurich seminar would not have reached him (there were no blogs…), but he did get a 1957 report from someone in on a Pauli-Goudsmit discussion.

So, a 1947 Stueckelberg Zurich seminar origin story, with Peierls first hearing the phrase second-hand in 1957 from a different context seems not at all impossible. The 1992 Physics Today letter explicitly rules out an origin in a seminar, but (just like Minkowski's 1991 book), the recollections very late in life of what happened forty years earlier are often not very reliable.

6. **Amitabh Lath**
April 10, 2023

What does the phrase mean? Does it mean really really wrong? Like an explanation of the Stern-Gerlach effect that did not use electron spin or SU(2)? Or maybe it means the topic belongs in philosophy or metaphysics and cannot be judged by criteria used to judge physics concepts (some might place MWI there).

7. **Peter Woit**
April 10, 2023

Amit,
I suppose the phrase sometimes gets used to mean "so obviously wrong as to be not worth discussing why it's wrong", but that doesn't seem to me to capture the appeal of the phrase or, whether it was Stueckelberg or Everett, how Pauli was using it. What the phrase captures nicely is the frustrating problem of scientific claims that come with no way of deciding if they are right or wrong. There can be multiple reasons for this, one of which is claims that are just too speculative and vague to pin down precisely enough to see if they are right or not. From what I know of Stueckelberg's work, there were times he had a very good idea, but either didn't explain it well enough or didn't understand it well enough himself. I can imagine Pauli getting exercised about a seminar with this characteristic. Everett is another case where where it's unclear exactly what is being claimed and whether any real explanation of anything is being provided. That this remains true 65 years later is my problem with MWI.

So, at least to me "Not Even Wrong" points to unclear speculative claims, but such things can have value. Most successful advances in math and theoretical physics start out as an unclear idea. Only later do things get fully understood and the value (or lack thereof) of the idea becomes manifest.

8. **D. Alder**
April 10, 2023

My understanding of the phrase is that it denotes an idea, theory, etc, that isn't even incorrect in some illuminating or interesting way which would allow one to identify some mistake or misconception and correct it. It may even be correct insofar as it can be; or correct in some trivial, misleading, or contextually irrelevant manner.

Anthropic fine-tuning is a perfect example. It's obviously true, since it's based on a tautological observation. But so what? It doesn't explain anything.

9. **Peter Woit**
April 10, 2023

D. Alder,
I don't think Pauli had in mind empty tautologies when he was using the phrase since in his day no one was making the kind of absurd claims about such things that they do now. Would be interesting to hear his reaction to some of the modern "beyond not even wrong" developments such as the landscape and the wormholes.

10. **Scott Aaronson**
April 11, 2023

It's actually pretty extraordinary, and ironic, if the phrase "not even wrong" originated to ridicule the ideas of two physicists, Stueckelberg and Everett — BOTH of whom, several generations later, many people remember less for anything they were wrong or not-even-wrong about than for being far ahead of their time!

11. **anon**
April 11, 2023

The French Wikipedia "Même pas faux" quotes K. Mendelssohn (1973), *The World of Walther Nernst*:
« Shortly before his death Pauli was handed the paper of a young American physicist whom his colleagues suspected of being a budding genius. After studying it carefully, Pauli, rocking sadly, gave it back with the acid comment: 'It is not even wrong', meaning that there was not a spark of imagination in it »

12. **Chris Oakley**
April 11, 2023

Scott,

Yes – Pauli did rather overdo it. I had not known about the stick, but I cannot say I am surprised. Sabine seems to have taken on Pauli's role of telling everyone where they are going wrong. A necessary function, and the lack of anyone doing this between Pauli's death in 1958 and Sabine's blog starting in 2006 is probably what allowed a lot of unpromising ideas to develop, such as supersymmetry and string theory. Hopefully the subject will get back on track now. The role has to be taken on by a native German speaker, and I am hoping that she will be able to produce her own catchphrase on the lines of Pauli's. Maybe she has already.

13. **Peter**
    April 11, 2023

    When I read “On Bullshit” by the philosopher Harry Frankfurt, it was striking to see how he defines “bullshit”. It’s more or less “not even wrong”, with some intention added. The only thing that counts is whether the bullshit is suitable for the purpose of the communication. It’s not a lie, rather something that doesn’t *care* about being right or wrong.

14. **Low Math, Meekly Interacting**
    April 12, 2023

    I like the application of “not even wrong” to untestable hypotheses, but I personally doubt that’s what Pauli meant. I rather think it’s for ideas so ill-conceived as to be not worth falsifying, like homeopathy. Pauli himself posited correctly that the neutrino exists. He hated doing it, because he sincerely believed that idea was empirically intractable, but did it anyway. Oddly, Pauli reportedly seriously considered the notion that his eponymous “effect”, i.e. his ability to ruin experiments with his mere physical proximity, had some other basis in reality than the terror he struck in others with his noxious persona.

15. **DrDave**
    April 16, 2023

    Mathematician Max Simon:
    “Die schlechtesten Antworten sind freilich die welche so schlecht sind daß sie nicht einmal falsch sind also nach dem Schema Die große Armut stammt von der großen pauvreté besonders beim Ansatz von Gleichungen hat man damit zu kämpfen.”
    (Didaktik und Methodik des Rechnens und der Mathematik)

16. **DrDave**
    April 16, 2023

    I think what Max Simon published in 1908 shows unambiguously the meaning of “not even wrong.” He clearly states that the idea or equation (Max is talking math) is so bad that it does make it as far as wrong, that it is below wrong in the order of things. Pauli would have known Max and/or his work, and it is more than likely that this is the earlier, published origin of the phrase as applied to science, and it is also quite possible that someone just misattributed it to Pauli like these internet misquote cards that appear on social media.
    A couple more things, Max’s syntax is typical, with the verb at the end, either in singular or plural (“ist” or “sind”) and the form in which Pauli’s “original” appears is not typical.
    All contemporaneous forms use “einmal”, more familiar as “noch einmal” which means “one time”, reinforcing the threshold meaning of the phrase, literally not even good enough to be wrong even once.
    Max’s 1908 quote translated:
    “Of course, the worst answers are those that are so bad that they are not even wrong, so according to this scheme great deprivation comes from great poverty [pauvreté], you have to struggle with it, especially with equations.” Max then gives some math examples.

17. **jbc**
    April 18, 2023

    This is rather tangential to the question of when Pauli first used this phrase but it might be helpful to put it in context. It’s usage is quite common even today in Austria, where Pauli grew up and spent his schooldays—he didn’t invent it but adjusted it to the context discussed. The original phrase is “net amoi foisch”, austrian dialect for the german “nicht einmal falsch” . As is often the case, it sounds natural and scathing in the dialect form, in contrast to the stilted high german version usually quoted. It is generally occurs as part of a trilogy, used typically by school teachers when returning written homework examples, placing the jotters on the pupils’ desks with the corresponding attributes “richtig”, “foisch” or “net amoi foisch” depending on their opinion of the solution.

    As said above, this doesn’t answer the query but it does give background information which might explain why the phrase would occur naturally to Pauli.

Reality is a Paradox

April 12, 2023
Categories: Uncategorized

Lex Fridman's latest podcast features a nearly four hour long conversation with Edward Frenkel, under the title [Reality is a Paradox – Mathematics, Physics, Truth & Love]. Normally I'm fairly allergic to hearing mathematicians or physicists publicly sharing their wisdom about the larger human experience (since they tend to have less of it than the average person), and I'm pretty sure I've never before listened to a podcast/interview longer than an hour or so. But in this case I listened to and enjoyed the entire thing. Besides sharing Frenkel's deep interests in the relation of representation theory and quantum mechanics, and views on the unity of mathematics (and physics…), I envy his positive and thoughtful outlook on life and his openness to a range of human experience. The interview left me with a lot to think about and I recommend it highly.

Comments

1. **erik jan bosch**
   April 12, 2023

   I mostly enjoy the podcasts by Lex Fridman, also the one with you. So i will listen to the one with Frenkel. I'm not a physicist but i am very interested in the subject. I just recently read the book by Sean Carroll The biggest idea in the universe in which he explains also the mathematics (calculus and linear algebra) behind physics for the layman. The second volume will go deeper into quantum mechanics. How do you feel about this translation work to a greater audience? I think he is doing very important work to make this subject (physics including the mathematics) available to the public.

2. **Peter Woit**
   April 12, 2023

   erik jan bosch,
   I don't want to change the topic here to Sean Carroll and his recent book (which I haven't read). Almost universally I've found physicist's attempts to write not about physics but about things like the meaning of life to be not worth paying attention to (with Carroll's "The Big Picture" a good example). That he's now sticking to the worthwhile (and too difficult for me) task of trying to explain real ideas about physics to a wide audience is a big improvement.

   Frenkel is a very unusual example of a scientist who has something to say about the human condition worth listening to, and it's noteworthy that he doesn't ground it in "I'm a scientist, know things you don't about math/physics, so you should pay attention to my views on life in general".

3. **Andrew**
   April 13, 2023

   There was also an interesting interview with Frenkel on the Numberphile podcast, this one only an hour and twenty minutes for those with less time to spare 🙂

   https://www.youtube.com/watch?v=lMwLCSgvxtw&t=2s&ab_channel=Numberphile2

4. **Giovanni Ronchi**
   April 13, 2023

   For those with even less time to spare there is this 56 minutes interview of Edward Frenkel by Brian Keating: https://youtu.be/b67o24J3bAs

5. **Art**
   April 14, 2023

   Frenkel certainly seems like a jolly fellow. The interview sounds almost calculated to attract your specific interest, with his first mathematics epiphany being exposure to representation theory.

   It seems Oulipo is having a moment. Not only does Frenkel mention the group, but it also pops up in Sarah Hart's "Once Upon a Prime", as reviewed in the Economist. Long ago I read Perec's "Life, A User's Manual" (in translation) based solely on the title.

6. **Cleon Teunissen**
   April 15, 2023

   On the subject of complex numbers.

   The version of the story of complex numbers that Edward Frenkel gives is historically incorrect, and significantly so. (It's not necessarily the case that Frenkel is at fault there; it may be that that is how the story had come to Frenkel.)

   Generally, what tends to happen in physics/mathematics is this: there is a concept that has many contributors, and as succesive authors retell the story, relying on old memory only, the attribution gets shifted more and more to one persion.

   My information source for the story of complex numbers:
   'A Short History of Complex Numbers' Author: Orlando Merino
   https://www.math.uri.edu/~merino/spring06/mth562/ShortHistoryComplexNumbers2006.pdf

   According to the above account:
   Cardano's contribution to the overall history consisted of developing a systematic method for finding the roots of cubic equations. According to Merion Cardano avoided the class of cases that would involve considering the square root of a negative number. While Cardano's contribution is vital, it is historically wrong to attribute complex numbers to Cardano.

   Rafael Bombelli went a step further:
   There are cubic equations that obviously do have a root, but the Cardano formula involves the root of a negative number. Bombelli noticed that if you allow that root of negative number then there in the cases with obvious root that factor subsequently drops out of the calculation, and then it all works out. I infer that at that stage the root of a negative number was admitted, but only in cases where it is seen to drop out of the calculuation.

   My impression:
   Over several generations of mathematicians there was a gradual lessening of suspicion. Over time mathematicians came to see the root of a negative number something on

par with other mathematical concepts.

My point is:
Frenkel comes to the story of complex numbers from musings of how important it is to retain a child-like willingness to consider things that appear to make no sense. Then he enthousiastically gives the Cardano-centered version of the story of complex numbers to illustrate his belief.

Now, I share the concept of child-like openness that Frenkel professes. It's just: the true history of complex numbers is totally *not* the story to illustrate that.

There is a quip by Mark Twain that goes as follows:
"In the real world, nothing happens at the right place at the right time. It is the job of journalists and historians to correct that."

It's funny because it's true.
Human memory is fallible; human memory is prone to restructuring memories to suit the belief system.

When physics authors write a "historical introduction" they always end up doing the very thing that Mark Twain described. When I read a "historical introduction" in a physics textbook there are almost always significant deviations from the actual history. (Often the deviation will have accumulated from author to author.)

7. **David Brown**
April 20, 2023

Frenkel's home page is worth studying.
https://www.edwardfrenkel.com
It seems that Frenkel has a newsletter that people can subscribe to via email.

8. **David Derbes**
April 21, 2023

Cleon Teunissen: Full agreement that complex numbers came about as a result of the need to explain how the Cardano formula, expressed in terms of square roots of a negative number, could possibly produce a positive real number. Tignol (*Galois' Theory of Algebraic Equations*, World Scientific) makes the case very powerfully. However, I think you are not being entirely fair to Cardano. He does not introduce a symbol for the square root of -1. But, in the article you cite, Merino cites a famous example of Cardano's from *Ars Magna* as follows, involving a *quadratic*, not a cubic, namely $x^2 – 10 x + 40$: "to divide 10 into two parts, the product of which is 40″, and completing the square. (Cardano does not actually write down the quadratic.) This leads to $x = 5 \pm \sqrt{-15}$, and, remarking that we must "put aside the mental tortures involved", finding that the product of the roots equals 40. After some discussion, he concludes, "So progresses arithmetic subtlety, the end of which, as is said, is as refined as it is useless." (See T. R. Witmer's translation of *The Great Art, or the Rules of Algebra*, G. Cardano, pp. 219-220.) The best account of the long trial to nail down the Cardano formulas, at least in English, is perhaps in M. Livio, *The Equation That Couldn't Be Solved*.

9. **Z Y**
April 24, 2023

Off topic: the MJR site was put to rest until the end of summer, seems to be an strange and very unpopular decision. The site was gaining tremendous action and while it hosted some asinine posts, some of the mathematics related posts were very thoughtful and valuable

10. **Peter Woit**
April 24, 2023

Z Y,
I'll be surprised if it gets revived. Whenever I would go take a look, my impression was that the level of dumb was pretty overwhelming. Who would want to spend the time it would take to try to find the few interesting comments? What sensible person who wanted to have an intelligent discussion would comment there?

The fundamental problem with an anonymous site like that is it needs a good moderation system to stop the idiots and trolls from overwhelming the place. I never understood the moderation system in place there, but whatever it was, it was nowhere near good enough. Like everything else in our brave new world, maybe there's an AI solution in the future…

And yes, off-topic so I'll moderate out any but the most amazingly insightful discussion of what happened at that site.

11. **kitchin**
April 25, 2023

Getting back to your summary and link to the Fridman interview… I'm exchanging dueling Youtube links with a friend in the social sciences/humanities. His was Martin Rees on multiverses. Mine was the shortest one here, the interview of Edward Frenkel by Brian Keating.

How would you rate this for a heuristic? "Rees says different universes may be a millimeter apart. In Langlands, our representations may be a prime number apart."

Obviously I'm using representation and prime quite loosely, as Rees, I assume, was with millimeter. I think still 120 odd years since quanta, it's difficult to convey the challenge of discreteness when things appear continuous. (Einstein's middle period frequent quoting of Ernst Mach is one way in.)

My other comments relevant to your summary would be about Frenkel's use of the term Platonic, and getting beyond it in math foundations. Scanning his book, I don't think he's going a direction I would from that (ethics in Greek drama for one), but he is wonderfully open and engaging.

Frenkel's discussion of math & physics ideas not easy to popularize is close to crystalline in the shortest video. As I haven't read the book fully, I'll suppose his philosophical direction is good too. Thanks for posting about it.

12. **Peter Woit**
April 26, 2023

kitchin,
The problem with the multiverse of Martin Rees, e.g. at
https://theconversation.com/the-multiverse-our-universe-is-suspiciously-unlikely-to-exist-unless-it-is-one-of-many-200585
is that he has no evidence and no viable theory to point to. He and others going on about this do not seem to be referring to anything well-defined (closest might be the string theory landscape, which is highly problematic).

About deep structures unifying math and physics, when Frenkel points to the Langlands program, he is pointing to something very specific, which we don't fully understand, but do know a great deal about. If you want to propose thinking of universe as at the real place, other universes at different primes p, at least there is something specific, well-defined, with a lot of interesting structure that you are pointing to. Maybe this it is useless to think about this as a "multiverse", but at least there is something well-defined to talk about.

13. **Ψ**
April 27, 2023

Your own experience that you haven’t found mathematicans’ insights on non mathematical valuable is of course inarguable, but the statement
“their wisdom about the larger human experience (since they tend to have less of it than the average person)” makes me sad. I read that as separating the experience of doing (in whatever form) mathematics from “human experience “, and I disagree strongly. Math has an existence beyond us (IMO), but our experience of it is by definition human. I find it rich and rewarding (which is why I am a mathematician) and in my experience it’s deeply connected to the rest of my experience – i see mathematics in the world, and the practice of mathematics informs my worldview. I value a wide range of experience, but mathematicans are among those whose experiences I’m most interested in personally. I’d like to see more of musings on the wider world of the character of, to name a few exemplary cases, the writings of Poincare, Penrose, Ulam, Gromov, and Thurston. I’d also enjoy seeing more mathematicians engaged with the wider world; Frenkel’s doing his part, as are you (PW). I think we have a lot to contribute.

Kontsevich on the Hodge and Tate conjectures

April 26, 2023
Categories: Uncategorized

Yesterday afternoon there was an event at CUNY featuring a panel discussion on Chern-Simons terms. Nothing new there, although it was interesting to hear first-hand from Witten the story of how he came up with the Chern-Simons-Witten theory. One piece of news I heard from Nikita Nekrasov was that he was missing a talk that day at the Simons Center in Stony Brook by Maxim Kontsevich, who would be arguing that the Hodge and Tate conjectures were not true. The video of that talk has now appeared, see here.

I'm way behind in preparing for my class for tomorrow, so haven't had time to watch the full video and ask experts about it. Will try and learn more tomorrow after my class, but it does seem that if Kontsevich is right that would be a dramatic development. If you are able to evaluate Kontsevich's arguments, any comments welcome. Tomorrow I'll also try and at least find some good references to suggest for anyone who wants to learn the background of what these conjectures say.

Update: I see there's an older version of this idea described here.

Update: Having watched the video and talked to a few people about it, I fear that there's not much new here, and nothing likely to convince experts that a falsification of the Hodge or Tate conjectures is on the horizon. Kontsevich himself introduces the talk as "not really a talk, but a kind of after-dinner rant." He for a long time has been trying to find examples that could falsify the Hodge conjecture, with no success so far, and from what I can tell, he doesn't have a new compelling proposal for where to look and how to do this.

Comments

1. **jsm**
   April 26, 2023

   In his earlier talk, Kontsevich suggested looking for a counterexample to the Hodge conjecture on abelian fourfolds of Weil type. However, Markman has proved the Hodge conjecture for abelian fourfolds of Weil type with determinant 1, so this may not be such a great idea.

   Markman, Eyal The monodromy of generalized Kummer varieties and algebraic cycles on their intermediate Jacobians. J. Eur. Math. Soc. (JEMS) 25 (2023), no. 1, 231–321.

2. **David Brown**
   April 28, 2023

   For those of us among the algebraici geometriae ignorami, I recommend:
   "The Tate conjecture over finite fields (AIM talk)" by J. S. Milne, 2007, somewhat revised and updated in 2021
   https://arxiv.org/pdf/0709.3040.pdf
   "Known cases of the Hodge conjecture" by Genival da Silva Jr, 2021
   https://arxiv.org/abs/2105.04695
   https://en.wikipedia.org/wiki/Timeline_of_abelian_varieties
   V. Kumar Murty's 1986 book
   https://books.google.com/books/about/Introduction_to_Abelian_Varieties.html?id=1bJKqHqWgp4C

3. **Mike**
   April 28, 2023

   Was the panel filmed?

4. **Peter Woit**
   April 28, 2023

   Mike,
   I believe so, and I think the whole thing was on Zoom. I don't know if there are any plans to make the video public. If so, one place it might appear is at the Einstein Chair Mathematics Seminar Youtube site here
   https://www.youtube.com/channel/UC_jFgn51x3iXh8ljGzWRToA

5. **jsm**
   April 29, 2023

   A little history. What's interesting about abelian varieties of Weil type is that they have cohomology classes, predicted to be algebraic by the Hodge conjecture, but not obviously algebraic (not in the Q-algebra generated by the divisor classes). When Weil constructed the varieties, he wrote to Tate: "As you and Mumford seem to believe Hodge's conjecture, it is now up to you to exhibit algebraic cycles corresponding to these abnormal classes. As I incline to disbelieve it, I shall rather attempt to show that there is no such cycle; but I have no idea how this could be done."

6. **Simon Pepin Lehalleur**
   May 1, 2023

   I think Kontsevich's points are important and good to think about carefully even if one doesn't share his strong opinion about the Hodge conjecture. The philosophy of motives has a few components which can be considered independently:

   0) There are many cohomology theories for algebraic varieties (singular over C, de Rham, l-adic, crystalline…) with very different constructions and nevertheless striking similarities. Moreover, these similarities underlie interesting phenomena in algebraic geometry, arithmetic geometry, number theory, mathematical physics…

   1) There should exist a universal cohomology theory, valued in some abelian category of motives, which explains these similarities. This abelian category should be a rigid Tannakian category (with the tensor product related to products of varieties and the Künneth formula, and rigidity coming from Poincaré duality), and the corresponding "motivic Galois groups" should reduce some questions about these similarities to tractable questions in the representation theory of pro-algebraic groups. There should be a subcategory of pure motives (for motives of smooth projective varieties) embedded in the larger category of (all, mixed) motives, together with a functorial weight filtration with pure graded objects, so that every motive is canonically an iterated extension of pure motives. This subcategory of pure motives should be the category of representations of the maximal pro-reductive quotient of the motivic Galois group.

2) The category of motives should be constructed solely from algebraic geometry, and morphisms groups in that category should be ultimately describable in terms of algebraic cycles: directly via Grothendieck's original proposal for pure motives, and more indirectly (in terms of "higher Chow groups"/higher algebraic K-theory) for mixed motives.

One way to summarize Kontsevich's talk is that he believes in 0) and in some form of 1) but not in 2).

Point 0) is essentially an "empirical" observation which has been apparent and pretty uncontroversial ever since Grothendieck, and has only become clearer since then.

Point 1) still seems like the most parsimonious explanation for 0). In his talk, Kontsevich indicates that he does believes in some version of 1). In characteristic 0, mathematicians have actually constructed abelian/Tannakian categories which realise some of these desiderata (Deligne's pure motives based on absolute Hodge cycles, André's pure motives based on motivated cycles, Nori's mixed motives with an important alternative construction by Ayoub and Choudhury-Gallauer). However, we we understand very little about morphism groups in these categories, because they are built from the choice of an embedding of your base field into C and transcendental constructions involving singular cohomology, and not a priori related to algebraic geometry. We also essentially don't know what to do in positive characteristic. Consequently, these constructions only have had a few applications so far.

Point 2) is what that Kontsevich rejects. As far as I can tell, the standard heuristic argument for believing in 2) is that algebraic cycles are a clean mechanism for forming bridges between different cohomology theories: namely, two cohomology classes in different cohomology theories can be the cycle class of the same algebraic cycle! So if algebraic cycles with prescribed cohomological properties were as abundant as the Hodge and Tate conjecture (as well as a few other key conjectures about cycles like the Beilinson-Soulé and Bloch-Beilinson-Murre conjectures) predict, we could indeed construct the categories predicted by 1) purely geometrically. This was explained by Grothendieck in the pure case (where one only needs the standard conjectures), and would follow from Voevodsky's theory in the mixed case, as I discuss a little below.

Sidenote: the full force of the Hodge/Tate conjecture is not needed, one "only" needs Grothendieck's standard conjectures; personally, I would not be too sad if the Hodge conjecture was false as long as the standard conjectures are ok! But Kontsevich also rejects that possibility; as shown by André, the standard conjectures for all smooth projective complex varieties imply the Hodge conjecture for abelian varieties, and Kontsevich is looking for counterexamples to the Hodge conjecture on abelian varieties.

As Kontsevich stresses, the evidence for all these cycle conjectures is quite thin on the ground. There are simple situations like Weil-type abelian varieties where we don't know where cycles lifting certain natural Hodge classes could come from (even with recent great progress by Markman), and almost all known cases rely on cleverly bootstrapping existing cycles and not on finding new general principles of construction of cycles. I think Kontsevich is right that some of us "motivists" have become too trusting in the general web of motivic conjectures, and that more effort should go into stress-testing them by looking for counterexamples as well as new invariants which could lead to counterexamples (like his tropical proposal).

However, if 2) is false and 1) is true, then to make 1) usable we would still need an explanation of where the morphism groups of the abelian categories in 1) come from, if not from algebraic geometry! Especially in positive characteristic where we do not have the crutch of singular cohomology and we do not understand why something like independence of l-adic Betti numbers for general varieties, which would follow from any reasonable version of 1), should be true.

An interesting part of Kontsevich's discussion is about the relative setting. As he notes, in the context say of topological Q-local systems over a smooth algebraic curve over a number field with some given complex embedding, we have some tentative criteria for what "local systems of pure motives" should be: integrality of monodromy, G-function /growth of power series coefficients of solution of the associated differential equation , vanishing p-curvatures of the differential equations in characteristic p, existence of Dwork-Frobenius structures. However, (outside of recent progress in some directions for rigid local systems on higher-dimensional varieties by Esnault-Groechenig) we don't really know how to prove the equivalence of these very disparate conditions, other than to try to prove that they are all equivalent to "coming from smooth projective families of algebraic varieties"! A useful theory of 1) should include abelian categories of "motivic local systems" and needs to explain why these conditions all seem to be equivalent; again, if not by geometry, how?

To conclude, let me clarify the position of the work of Voevodsky in this picture. Voevodsky has, among many other things, constructed triangulated categories which look like the *derived categories* of the categories predicted in 1), have a purely algebro-geometric construction, and whose morphism groups can be described (by some very non-trivial theorems) in terms of algebraic cycles. This has had some nice applications and also revealed a wider picture of "motivic homotopy theory" which is interesting independently of the validity of points 1)-2). However, finding an abelian category of motives inside of these triangulated categories ("constructing the motivic t-structure") is known, at least in characteristic 0, to be essentially equivalent to the same hard conjectures about algebraic cycles.

I should also point out Ayoub has proposed a very interesting approach to the motivic t-structure on Voevodsky motives which tries to bypass the conjectures on cycles, and which so far has lead to the new construction of Nori motives mentioned above and to a functional/geometric version of the Grothendieck-Kontsevich-Zagier conjecture on periods of varieties over number fields- ironically, one of the many conjectures that Kontsevich now believes to be false…

7. **Peter Woit**
May 1, 2023

jsm and Simon Pepin Lehalleur,
Many thanks for the informative comments.

8. **Topological twist**
May 3, 2023

Bear in mind that it is commonly said that Deligne was skeptical of the Hodge conjecture and tried to find counterexamples on abelian varieties, but failing that, developed the theory of absolute Hodge cycles. This tropical speculation has been around for at least ten years. I once tried to get Katzarkov to explain to me why tropical geometry should succeed where Deligne failed, but couldn't get a reasonable answer. Someone should push Kontsevich hard on this until he says something that other people can understand.

This Week’s Hype

April 26, 2023
Categories: This Week's Hype

According to this article, string theory is going to be tested using quantum computers, by doing a lattice QCD calculation:

> The way string theory is tested involves ‘lattice quantum chromodynamics’: a calculation problem far beyond what digital computers can achieve. ‘Quantum computers,’ he writes, ‘may be the final step in finding the Theory of Everything.’
>
> ‘I’m not a computer person. I’m a theoretical physicist,’ he says. ‘But I got into quantum computers because I realised this may be the only way to quantitatively prove that string theory is correct. String theory exists in the multiverse. That is, we exist perhaps in parallel states which are bizarre, with new laws of physics, but we coexist with them. The way to prove it is with a quantum computer.’

I suppose you need to buy the book to find out more.

Comments

1. **David Roberts**
   April 27, 2023

   > In Kaku’s account of it, then, there doesn’t seem to be a human problem that quantum computers won’t be able to fix. The only fly in the ointment, as he mentions not very much more than *en passant*, is that between here and all the good stuff there are a couple of potential road-bumps.

   Just a couple… 🙄

2. **Chris H**
   April 27, 2023

   “the first thing that they’ll do along the way is to make it possible to break by ‘brute force’ (i.e. sheer computational welly) every form of encryption on the planet.”

   Is any of that true?

3. **Ling-Nan Zou**
   April 27, 2023

   Why is anyone at all paying attention to Kaku when he’s been so consistently wrong about pretty much everything? Peter, you need to serve by example and ignore any and all articles involving this guy.

4. **Peter Woit**
   April 27, 2023

   Chris H,

   No.

5. **Peter Woit**
   April 27, 2023

   Ling-Nan Zou,

   Sometimes the high-media profile Kaku craziness is just hard to resist pointing to. But, you’re right, instead I should be pointing to the anti-Kakus who deserve more attention. A blog post of this kind is coming right up…

6. **zzz**
   April 27, 2023

   Kaku lost me when they brought him on a news channel to talk about a hurricane approaching florida

string theory lied to us and now science communication is hard

April 27, 2023
Categories: Uncategorized

I want to make up for linking to something featuring Michio Kaku yesterday by today linking to the exact opposite, an insightful explanation of the history of string theory, discussing the implications of how it was sold to the public. It's by a wonderful young physicist I had never heard of before, [Angela Collier](). She has [a Youtube channel](), and her latest video is [string theory lied to us and now science communication is hard]().

Instead of going on in detail about the video and what's great about it, I'll just give you my strongest recommendation that you should go watch it, now. It's as hilarious as it is brilliant, and you have to see for yourself.

Comments

1. **Sage of PHY6 Paths**
   April 27, 2023

   Life hack: If someone is not even putting enough effort to focus while talking and instead is gaming while speaking then that video is not worth watching.
   –
   She told there are 10 string theories (at 16:00) and were unified even though there are only 5. This person doesn't look like she EVEN knows string theory at the popular science level. She wrongly included String theory in a nutshell (by Elias Kiritsis) as a popular science book among Kaku's and Brian Greene's books.
   –
   So are we supposed to learn the history of string theory from someone who doesn't know what Pauli got the Nobel for?
   –
   "if they are not testable now they are not predictions": Thanks for teaching me that when Einstein "predicted" gravitational waves it was not a prediction until 100 years later.
   –
   She is wishing string theorists should never be hired (at 31:20). Looks like she is much less nuanced than people like you. Correct me if I am wrong but you don't advocate for completely stopping hiring string theorists right? She is an ASTRO person. They are well-funded. Why do some of them wish that fields like string theory and quantum gravity which are already SEVERELY underfunded to go extinct? What do they achieve if humanity stops progressing in fundamental physics?
   –
   She incorrectly said that the cost of SSC was 200 billion instead of 8.25 billion.
   –
   She is randomly saying things (making stuff up) like the public was thinking like this in 2000s and like that in 2010s etc without any SOURCES. The pop science audience is in millions and you can't assume they all have similar opinions. Even to know what the majority thinks you need a LARGE SAMPLE SIZE to get an idea.

   She is saying due to string theory people lost faith in all sciences. This is definitely a baseless CONSPIRACY theory.
   –
   So, she even left Astro (galactic evolution and dark matter), because the industry has even more cash (she exactly mentioned this as the reason) than Astro.
   –
   The reason why string theory is more appealing to the general public is that humans are fascinated by fundamental things. Post a video about how John Bardeen invented the transistor. Next post a video about how Einstein discovered GR, obviously this will get much more views even though the transistor is more useful in everyday life. The origin of the universe is a mystery that humans found fascinating even thousands of years ago (religions were created in part due to this, in another part due to fear of death)

   Most people are not fascinated by condensed matter or AMO. Most people who join undergrad join by wanting to contribute to fundamental physics. In my undergrad class, 10/50 people wanted to work on string theory or QFT/particle phenomenology, or loop quantum gravity when they joined. Only 2/50 were left in HEP by the end only because everyone understood that going into HEP is EXTREMELY bad for their job opportunities and they [reluctantly] sold their souls to condensed matter or AMO or Astro or finance.
   –
   I hate myself for wasting time by watching this.

2. **John Hessler**
   April 27, 2023

   That was the best video I have seen in months!! I loved that she brought out the M-Theory text and pointed out the Greene quote in the Einstein anniversary edition!! Thanks for pointing this out—my class will watch this next week!!

3. **Peter Woit**
   April 27, 2023

   Sage of PHY6 Paths,
   I think you're unfair on some of the specifics you're accusing her of getting wrong, but I don't think that's what's relevant. She could have made a scrupulously accurate and fair video, which would have been boring and no one would listen to. Many people quite accurately point out to me that when I write (accurately and fairly…) about the problem of string theory hype, no one is interested any more: it's beating a dead horse, a complete waste of time.

   So, how does one deal with the ongoing phenomenon of string theory hype (from the Kaku extreme, through the long list of prominent physicists still at it after all these years)? I thought this gonzo, bizarre, over-the-top, completely hilarious video was a unique answer to the question. Not something she or anyone else should try again, but I haven't seen a better answer to the question of how to deal with the likes of Kaku (ignoring him doesn't seem to work).

4. **Klaus**
   April 28, 2023

   Sage of PHY6 Paths,

   To give such a complex talk without "ahms" and without cuts while playing a video game at the same time shows that her intelligence and wit are extraordinary. Please stop hating yourself. Instead, hire her in your department!

5. **Guangyu Xu**

April 28, 2023

I think this is a valuable video. I can imagine many other physicists would have a similar perspective. The public opinions on string theories definitely did some damage to the physics community as a whole.

But I don't necessarily think Witten etal. lied intentionally, at least not in the beginning. I'm sure they were excited about their theories and did what they thought was the best for their careers. At least string theory did produce something useful for maths, even if there is no good testable predictions in the near future.

This is in stark contrast with the "wormhole in a lab" hype, which is so obviously fabricated lies to get the public funds. In one hand I feel angry, on the other hand it feels pathetic that even the most well funded areas in physics are so desperate for funding.

6. **James Smith**
April 28, 2023

I'd just like to say how shocked and dismayed I was to learn that Michio Kaku is a phony and an idiot.

7. **Bertie**
April 28, 2023

I was an avid consumer of popular science during the 90s & noughties and I think this girl totally nailed it.

As to gaming while she was talking, well that was next level multi-tasking, I was truly impressed and I don't see why anyone should whinge, I assume she was nervous and that is how she manages, good on her!

8. **Mark Olson**
April 28, 2023

It seems to me that the takeaway here is not the details of her argument or the foibles of her presentation, but that she probably represents many other newcomers to physics who are prepared to move on from this apparent dead end. "And that may be an encouraging thought." (I, too, found the gaming while talking incredibly annoying, but I dealt with it by just listening.)

Max Planck: "science advances one funeral at a time"

9. **John Horgan**
April 28, 2023

Thanks for the heads up, Peter. I love Collier's history of string theory, except for when she bashes Margaret Wertheim's book Physics on the Fringe, which I really like. Wertheim does not bash all of physics, only strings, multiverses and other ideas that cannot be tested.

10. **Peter Woit**
April 28, 2023

For my take (back in 2011) on the Wertheim book, see here
https://www.math.columbia.edu/~woit/wordpress/?p=4246
where there's a link to John Horgan's review
https://blogs.scientificamerican.com/cross-check/in-physics-telling-cranks-from-experts-aint-easy/

11. **lettuce field theorist**
April 28, 2023

For many young people, watching someone play a video game on camera while they're talking about an unrelated topic is completely normal. Granted, it's usually not about string theory, but it doesn't matter. As a millennial and twitch enjoyer, watching the gameplay actually keeps my attention on the video much better than if she were just talking, and this might be one of the best ways to reach a gen Z audience. It didn't even strike me as odd. If you can't deal with this format because you're a boomer or if it's simply not for you, then don't watch it. I can't believe the first commenter decided to watch the whole thing and then be a hater. You've wasted your own time, why're you complaining to us about it? The nerve…

Regarding the actual content, I find myself agreeing with the majority of her points. Her perspectives on string theory from an astrophysicist's point of view resonate with me, being a lattice QCD guy myself. Both of these fields have made incredible progress over the past 30-40 years, maybe they should've paid some string theorists to write hyped-up pop science books about it. Many of the things she said also reminded me of the angry conversations I've had with the blank wall in my office when the wormhole paper came out. It's not just that they're obviously fabricating false "breakthroughs" to reel in funding and are getting away with it by misleading the public, what really upsets me is knowing that I can't do anything about it, especially as an early-career scientist. At least string theory was sort of well-motivated back when it started, this new wave of pseudoscientific "quantum gravity in the lab" claims seems quite different from my point of view. I totally would've believed a younger Brian Greene or Ed Witten telling me that they're indeed fully convinced that string theory is the answer, even if I had disagreed myself. But they just kept going even when it didn't pan out, and as a result, the new generation of non-physicists they inspired seems to have no problem with engaging in deliberate misrepresentation in order to achieve their own career goals. I often wonder how they manage to sleep at night. No one can be this delusional, right? RIGHT???

12. **Peter Woit**
April 28, 2023

lettuce field theorist,
Thanks for providing some perspective to boomers like myself. To me seeing someone play a video game while talking on screen seriously about something else comes off as a hard-to-believe attention-getting piece of radical performance art. Interesting to hear that it's just what millenials do. Maybe I'll start making videos of myself doing things shocking and incomprehensible to millenials, like reading a physical newspaper or something and put those on my Youtube channel.

Or, maybe the lesson is that if I want to get millenial's attention for material about twistors and quantization of euclidean chiral spinor fields, I first need to take up gaming and work on that…

13. **Geoffrey Dixon**
April 28, 2023

lettuce field theorist
As a boomer who stopped maturing many decades ago (a Z-boomer, as it were), your comment was pure gold. It would give me hope for the future were it not that the likes of Sage of PHY6 Paths – dismissive, arrogant, not fun at parties – exist in your gen too, and they have a way of rising to the top. Consult Machiavelli for further references.

14. **Gabe Khan**
April 28, 2023

Even as a mathematician with some tangential connections to string theory, I completely understand the skepticism for it and very much liked the video. Personally, I would be much more comfortable with strings if they were sold as a theory which was developed by physicists and have generated deep and non-trivial mathematical insights. In that respect, it has been highly successful (and much less controversial). And although physicists might not prioritize it as highly as a mathematician does, there is real value in solving difficult mathematical problems.

But there is a real problem in researchers aggressively overselling their work before it has produced the results to match. I understand it is necessary to promote your work, since no one will read technical research unless you convince them it is worthwhile to do so. However, I still don't have a good answer to the question of how researchers can sell their work in responsible ways and not create the academic equivalent of a speculative bubble.

String theory is one the most prominent instances of this due to the success of its public outreach. However, there are many other examples which come to mind. And in the long run, this hype often proves to short-sighted and ends up detracting from serious work in the area.

15. **SPARx**
April 28, 2023

Curiously, I found your blog from the video (mentioned by user "Perfidy" in the comments section), and not the other way around. A useful feature of good science communication is that creates a cyclical effect, increasing the interest and awareness of the consumer. This increased awareness drives the consumer to seek similar but often less sensational content, which can lead to more good science communication.

An equal or greater challenge for "good science communication" is the fact that it is in no way equivalent to "good science." From my perspective walking a different path saddled with its own plethora of "pop science," it feels far rarer that the two are ever paired, in fact. It should be no surprise that people who rely on their communication skills to market their ideas rather than any facts or evidence contained therein also happen to be the smoothest communicators.
By contrast, the utility of a video such as this one, despite bearing such as egregious flaws as including videogame footage or misidentifying a cash figure, is that it serves to draw the public's opinion away from hapless acceptance OR excessive skepticism.

The relaxed tone and focus on certain specific instances of potential wrongdoing welcomes viewers who have the (frankly commonly held) belief that "string theory is cutting-edge and well validated science" to reconsider this belief without reflexively disavowing physics – or indeed even string theory itself – outright. Perhaps beneficial outreach, given how perilous ignoring a scorned majority holding the keys of finance and electoral support can become. NASA alone teaches many lessons.

Cheers!

16. **Chris Oakley**
April 29, 2023

I enjoyed the video, although she should (i) have taken out the "This is just my opinion man" banners and (ii) done the whole thing riding a unicycle on a tightrope.
I know there are good scientists out there – I have worked with them, generally doing unglamorous, non-headline-grabbing things – but in fundamental physics, they seem to be few and far between.

17. **gigi**
April 29, 2023

I found her other video about postdocs maybe more useful.

In particular about how current professors are pretty much also responsible for the tough environment postdocs face.

I find it pretty hypocritical how usually professors and university admins are ready to jump on any silly talking point, like divesting oil and pronouns, but working conditions for postdocs are "just the tough way it is". And I'm not referring to salaries, but at the very least administrative support. Changing countries with a family is easily a waste of several months of work, just because univ. admins can't be arsed to put together a decent FAQ for new hires.

18. **David Roberts**
April 29, 2023

If nothing else, this video gives a sample (n=1) of a mainstream physics view of string theory, by someone who grew up reading the pop sci treatments of it. Yes, the inaccuracies are unfortunate. The SSC funding cancellation being moved up in time was to me the most egregious slip; the mentality of the episode is about right, but it lessens the impact getting that one wrong. Then the origin of supersymmetry being inspired by ST, but at least that was tethered somewhat to superstrings, and considered as a prediction. After that, the specific mistakes like the critical dimensions for CFTs, the specific number of different string theories, are more or less beside the point. If there are five or ten superstring theories, it changes nothing about the thrust of the argument.

The example of how the framing of the character of Sheldon from Big Bang Theory shifted is for me a really interesting. All of this is sociology, not science, so seeing evidence of attitudes is really about a barometer of general feeling, not basing the authoritativeness of Collier as a source on her precise recollection of internal details of string theory, when she specialised in some other type of physics.

19. **anon**
April 30, 2023

I think the video is intentionally exaggerated in its claims and not meant to be taken nearly as literally as some here do and the gaming is there also mainly for trolling…

20. **JimV**
April 30, 2023

As a member of the class of science-book reading, science-loving amateurs to which she refers, I feel as if some dark-matter scientists have lied to me. (There was a commenter to this blog several posts ago who did so.) That is, like the string-theory advocates, they passionately believe in their hypothesis and refuse to credit data of its shortcomings, despite its lack of a-priori predictive results, adding or changing parameters as necessary to postdict results such as the CMB spectrum.

In neither case (string-theory or dark matter) do I think the lies are deliberate, conscious lies, but the result of cognitive biases which we probably all share to some degree (myself certainly).

I found the video game distracting and a bit annoying, although an impressive feat if true (would have been easy to fake in editing; I assume it was not fake but must logically leave room for the possibility). On the other hand, perhaps it forced me to concentrate on what she was saying more, by having to snap my attention back.

21. **PatrickC**

April 30, 2023

My recollection of pop-sci reading from the 90s/earlier? is that mentions of string theory always had some sort of disclaimer to the effect of 'not testable by orders of magnitude and/or for any forseeable future'. I rhink the claim of "deception" of the interested public is not generally supportable. Certainly not compared to more technology-related hype-fests like cold fusion, or the EM drive, or this latest 'simulated yet real' wormholes, or …
I also found the simultaneous gaming to be sort of annoying, and detracted from taking her seriously.

22. **Peter Woit**
April 30, 2023

PatrickC,

Kaku to this day claims publicly that string theory is testable in the near future, although Brian Greene has become much more cautious than in the 90s. If you want links to 100 or so examples of string theorists making bogus claims about testability, see here
https://www.math.columbia.edu/~woit/wordpress/?cat=8
These are post-2004, what was going on in the 1990s was even more egregious. For example, the New York Times was hood-winked in 2000, putting out this story

https://www.nytimes.com/2000/04/04/science/physicists-finally-find-a-way-to-test-superstring-theory.html

Yes, discussions of string theory in the 1990s did often include mention of the fact that the strings were small, so direct detection would be hard to impossible, but many claims were made about getting indirect evidence. I think the case that the string theory hype heavily promoted to the public at that time (and to this day…) was highly misleading is very strong. This hype was one of the main reasons I wrote "Not Even Wrong", and I think the argument I was making 20 years ago that string theory was an overhyped idea that had already failed by then stands up quite well today.

23. **Low Math, Meekly Interacting**
April 30, 2023

This video also left me somewhat bemused and uncomfortably reminded of my advancing age. Even with the explanations above, I still don't get the joke of the video game, if joke it is. My reflexively cynical side wondered if it was a built-in excuse for the chronological and factual inaccuracies. In my day bong hits provided similar absolution from fact-checkers on mornings after I was inspired to opine at great length extemporaneously.

Though not a physicist, I started having similar misgivings some time in the early aughties (I think I found this blog shortly thereafter). It is simultaneously fascinating and depressing to hear Collier's take roughly twenty years later. She was in grade school when the problem became obvious even to a layman like me, yet the false narrative of String Theory is so pervasive in science popularization a new generation is experiencing more-or-less exactly the same sort of disillusionment. Very sobering.

24. **Marvin**
April 30, 2023

Did her video bring me something? Yes, I was not aware that Greene is mentioning string theory in the intro of a new edition of Einstein's " The meaning of relativity". This is pure hype, glueing this fictive theory to a classic. Looks like an irrespectuous act to me.

I have the old version in 3 different languages. I will not buy this new edition……

25. **Interested amateur**
April 30, 2023

Coincidentally, I came across Angela just yesterday after watching another of her videos which is a gem: physics crackpots: a 'theory' video. I agree with her 100% after spending many years participating in discussions on sci.physics.relativity; and reading Lubos Motl's opinions on his blog on identity politics, climate change, Covid, the 2019 US election etc.

26. **David in Tokyo**
May 1, 2023

JimV wrote: " I feel as if some dark-matter scientists have lied to me. "

I dunno. My impression was that the dark matter idea was at least in response to real data, to a real problem in astrophysics. My impression is that it isn't working out, but that's OK. Science is supposed to be about looking at data and thinking about it and trying ideas out. I suppose if I read the popularizing fan literature, I might get irritated, too, though.

But as a computer nerd (we're supposed to like sci-fi more than I actually do), using words like cloaking and wormhole are far more objectionable. Ditto for teleportation. Even in the kindest possible interpretation, ripping off perfectly good (albeit inane) ideas from science fiction and using them to talk about things that aren't anything like the original is objectionable in the extreme. The rather dated term "shameless" springs to mind and refuses to leave.

27. **Jonathan Baxter**
May 1, 2023

Have to disagree. This was narrowly opinionated, weakly argued, and pretty annoying.

I've been one of the string-theory-following "public" she mentions (theoretical physics undergrad). Listening to her I feel like we lived in different universes. Yes, string theory was probably over-hyped. And I am not wedded to it being the correct theory. But we had no (and still have no) serious alternative proposal for unifying gravity with quantum mechanics. She is certainly not proposing any such alternative.

It's also not the case that string theory has drowned out the rest of physics, as she implies. Again, as an outsider interested in physics, I've seen far more public discussion of the rest of physics over the decades than I have of string theory.

She judges string theory based on books written by crackpots. That's not string-theory's fault. Pretty much every scientific field spawns its own crackpot literature.

I find the whole thing pretty disrespectful. Witten is a modern-day Einstein. Arguably Einstein + Gauss. Problems with argument from authority aside, when given a choice between the opinion of a YouTuber who doesn't seem to know the Pauli exclusion principle and is offering no alternative to string theory, and someone of Witten's stature that firmly believes it's the most promising direction to pursue, I know who I am more inclined to listen to.

28. **Peter Woit**
May 1, 2023

Jonathan Baxter,
Your comment makes a good case for the problem Collier is pointing to.

According to you the books about string theory she describes are “written by crackpots”. The ones I remember her referring to are by Michio Kaku, Brian Greene and Leonard Susskind. Are they all “crackpots”? If Brian Greene and Leonard Susskind are crackpots, do you have a way of figuring out which of my other Columbia colleagues and which of the Stanford faculty are crackpots? If you don’t, do you now see the problem with overhype trashing the credibility of conventional authority figures in science?

Yes, Witten is a genius, and he’s a professor at the IAS. Just like Einstein, who spent his time at the IAS pursuing a set of failed ideas about unification. Maybe things are more complicated, and geniuses sometime pursue misguided research programs?

29. **Jonathan Baxter**
May 1, 2023

Peter Woit,

“According to you the books about string theory she describes are “written by crackpots”.

That’s not what I said. I said “She judges string theory based on books written by crackpots”.

I should have been clearer that I was not talking about texts on the subject, or popularizations by leading lights of string theory. She is far less judgmental about those, other than to accuse the authors of overhyping string theory’s potential. And on that point: I have a difficult time believing the authors did/do not genuinely believe that string theory had/has a good chance of unifying gravity and QM. I simply don’t believe they were intentionally telling lies as she claims. Speculating that something will happen “within a decade” does not automatically become a lie when it does not, in fact, happen within a decade.

A decade is usually a placeholder for “sometime not in the near future”, regardless of who is making the claim, and regardless of the subject. When I hear “decade” I think to myself “ok, you don’t actually know”. But again, the person speculating usually doesn’t know that they don’t know, so the accusation that they’re somehow lying is not justified.

As far as crackpot books go, I am talking about, for example, “physics of the fringe” which is written by someone with little knowledge of actual physics (full disclosure, I have not read this book and had not heard of it until it was mentioned in the video).

No doubt geniuses pursue misguided research programs. But Einstein is not a good analogy. Other than very accurately quantifying his objections to QM with the EPR papers, he essentially turned his back on QM, a field that was making astonishing progress explaining the physical world in his own lifetime.

Where is the corresponding field that competes with string theory? As far as I know it doesn’t exist. So while I am perfectly willing to believe that string theory is not the right answer, it’s not like Witten has been studiously ignoring spectacular advances elsewhere in favor of his own pet theory.

(tbf on Einstein, his objections to QM are just as valid today as when he made them. They’re an objection to the fact that QM does not describe the physical world of our own experience. He is in good company. Most people who go down the road of truly attempting to understand what QM is telling us about reality end up in a deep conundrum, and usually start musing about the nature of consciousness itself)

30. **Peter Woit**
May 1, 2023

Jonathan Baxter,
So, by “books written by crackpots” you are referring to a single book written by a serious science writer that was about crackpots, and which you don’t know anything about? For more about it, see
https://www.math.columbia.edu/~woit/wordpress/?p=4246

You say “She is far less judgmental about those” (popularizations by leading lights of string theory), completely missing her main point. The video is entitled “string theory lied to us” and she repeatedly refers to the 1990s popularizations of string theory and their authors and repeated calls these authors “liars”. Seems pretty judgmental to me…

31. **Jonathan Baxter**
May 1, 2023

Peter Woit,

What can I tell you? I don’t think those guys are crackpots. If I misinterpreted her view of “fringe” then ok. Nothing I said stands or falls on that.

Yes, she is very judgmental about what she views as the overhyping of string theory – I said as much.

My main points are

A) Her “liar liar” claims are offensive and unconvincing. I don’t believe leading string theorists were intentionally lying when claiming that unification of gravity and QM/QFT was reachable within a decade. And her repeated claims that Witten was happily basking in adulatory Einstein comparisons while secretly harboring doubts? Come on. At least based on my impression from afar, Witten may have the highest ratio of talent to humility on the planet.

B) I don’t believe the string theory hype had any impact on the credibility of science (I didn’t make that point yet).

There’s vastly more nefarious corruption of science than a little overhyping of string theory.

32. **Jonathan Baxter**
May 1, 2023

To be clear, I’ve never heard of Michio Kaku. But I have read books by Greene and Susskind and they’re not crackpots.

33. **martibal**
May 1, 2023

“Witten is a modern-day Einstein. Arguably Einstein + Gauss”

Maybe it would be wiser to let history makes its job, and see in 50 or 100 years if the impact of Witten’s work in maths and physics is comparable to the one of Einstein and Gauss. The cult of personality is also part of the problem of over hyped string theory.

34. **Peter Woit**
May 1, 2023

martibal,
One problem with Einstein/Witten comparisons is the following. While Einstein started his research career around 1900, just as modern physics was about to be born, Witten started his in 1974, exactly at the point the SM was finally in place, and dramatic progress coming to a screeching halt. So, Witten has been working his entire career in a very different situation than Einstein.

Everyone who is convinced string theory is the best way forward because of the undeniable fact that Witten is a genius should keep in mind the example of Einstein, Witten's predecessor at the IAS, who similarly for his entire career there thought he knew the best way forward (extending classical GR to more general geometries), but was completely wrong.

I agree that Collier was being somewhat unfair to Witten about the issues of comparison to Einstein. Unlike some others, he's not at all someone who seeks public attention. But he has been the most influential figure from the beginning in telling other theorists they should work on string theory, arguing the case within the theory community that it was by far the most promising way forward. The big scandal of string theory is that it failed and the leaders of the field (with Witten the most influential) have refused to admit what happened.

35. **Low Math, Meekly Interacting**
May 1, 2023

I am also of the opinion that, e.g., Brian Greene has, or at least had, no intention to misinform. I also still hold his books in high regard for their introductory material and the quality of their writing.

But let's face it: Even when the Elegant Universe came out, the claims therein about the candidacy of String Theory as a TOE were already in serious trouble, not only empirically but purely theoretically. AdS/CFT was 1997. Elegant was published in 1999. KKLT was 2003; Fabric of the Cosmos was published in 2004. So circa Greene's most successful publications, there was already every reason to believe that String Theory not only couldn't work in anything less than ten dimensions, but those six extra dimensions could be rolled up in a functionally infinite number of ways…and the extended ones had to have the wrong curvature. By the mid-2000s (and arguably well before), String Theory was obviously an empirical dead-end to all but its most ardent believers. None of its proponents, nor practically anyone involved in popularizing HEP, saw fit to educate the layperson about the fact that natural SUSY was pretty much DOA before the LHC even turned on. They didn't tell us the so-called WIMP miracle was in serious tension with experiment in the 1990s. They completely glossed over the fact that String Theory's one "prediction" i.e. "gravity" is actually a little-known extension of GR with scalars we never observed nor ever needed to explain anything. And on and on and on…

And it's just been getting steadily worse with time. Today we're agape at this latest wormhole nonsense, and all I can think is practically anyone could come up with a takedown of this whole industry, and yet it amazingly seems indestructibly undead. One simply has to quote certain very respectable people 30, 20, 10, 5, 2…years ago, look at what they said then, what they said later, what they say now, and compare. It's stunning how many goalposts have been moved, how many equivocations have been required to keep the machine going, how many ridiculous statements to the effect of "well, we never actually claimed we could predict"… The only proper answer is "Yes, you did. Over and over again, in fact. And you've been wrong about every single one of these claims. Every. Single. One."

So, no, it's probably not accurate to call some of these folks liars. But does that help, at all?

36. **Douglas Natelson**
May 1, 2023

I'm late to the party here, but I enjoyed the video. I don't think over-hyping string theorists have somehow deeply harmed the credibility of all of science, but I do think they and their ilk have done the discipline of physics a serious disservice. There is far more to physics than just high energy theory, and hype + acting like they speak for all of physics is not helpful. Sage of PHY6 describing people choosing to work in AMO or condensed matter as "selling their souls" is hyperbolic sneering-at-non-high-energy bs, and attitudes like that approach Sheldon Cooper levels of self-caricature.

37. **zzz**
May 1, 2023

"He lied and never knew that he lied, and when it was pointed out to him said that lies were beautiful. He was an idealist"

–Maugham

38. **Anonymous**
May 1, 2023

I think that the tone of Angela Collier's video might not have come across clearly. She's speaking conversationally and without artifice. The reason that she's playing a video game as she talks, as she explains in the first few seconds of the video, is that she doesn't want to get too involved in scientific details when she talks about this, since it isn't her real focus, and she's using the game to distract herself from doing so. It wasn't a gimmick; she was just being straightforward.

Since she's talking off the top of her head, she's unsure of some details, so, for example, she speaks of the SSC as costing two hundred billion dollars (just meaning, basically, a lot of money), and, although she's certainly aware of the exclusion principle, she isn't aware that it's what Pauli got the Nobel Prize for. She was careful to look up the facts afterwards and put them onscreen in the video, however.

This isn't my field, but I have followed popular accounts of string theory since the late 80s, and I tend to share her point of view. It was only when I read Peter's book, and Lee Smolin's, that I realized how little of string theory was actually on solid ground. Up until then I'd thought it was odd to devote so many resources to a program that speculative, but I had the impression that at least the foundational conjectures were proven. In any case, it had all begun to seem more and more contrived as it went on, and it was being greatly oversold, so things hadn't been smelling right anyway.

From Angela Collier's standpoint, it wouldn't have been so easy. She would have grown up wanting to be a physicist and trusting popular books to tell her the truth about what physics was. The media of the time, always happy to find something to push, was selling string theory as the future. When that proved to be hype, it would have been a serious thing for her.

She's an astrophysicist, and she's from Appalachia. Both of those things, I think, would incline her to be a lot more bottom line than most particle physicists. Her claim is that the string theorists who were behind many of the claims in the media knew that they were stretching things at the time they said them, and therefore they were lying. That's a strong way to put it, but I wouldn't say it was unreasonable. If she's correct about what was going on, and they'd been selling used cars with the same attitude, then I think that's the word people would use.

39. **Dom**

May 2, 2023

There is an inherent problem here and it can be expressed by the character in Seinfeld who claimed "It's not a lie if you believe it". Clearly there is no way to tell the difference between sincerely held ideas, sincerely held delusions, having doubts but pretending not to and barefaced lying.

40. **David Derbes**
May 2, 2023

JimV writes: "As a member of the class of science-book reading, science-loving amateurs to which she refers, I feel as if some dark-matter scientists have lied to me. (There was a commenter to this blog several posts ago who did so.) That is, like the string-theory advocates, they passionately believe in their hypothesis and refuse to credit data of its shortcomings, despite its lack of a-priori predictive results, adding or changing parameters as necessary to postdict results such as the CMB spectrum."

I do not know what "lies" JimV is referring to. Dark matter was hypothesized by Fritz Zwicky around the same time that he and Walter Baade hypothesized neutron stars, in 1932. The reason for the hypothesis was the apparent non-Keplerian dependence on stellar orbits in galaxies; absent more mass than provided by the estimated mass of luminous matter, these stars would have left orbit. Vera Rubin seems to have established Zwicky's hypothesis with decades of careful measurements. I don't see how the existence of dark matter can be denied. I don't know what it is, nor what predictions have issued from its existence. The predictions may well depend on its identity, and that is as far as I have heard far from settled.

For a good popular book on dark matter, I like Laurence Krauss's "The Fifth Essence", though thirty years old now. Probably there is a better, newer choice, maybe "The Four Percent Universe" by Richard Panek (2012), which discusses both dark energy (also confirmed by extensive data, and recognized by a Nobel) and dark matter. And neither are necessary to predict the CMB; that was done by many back in the 1940's (at least as early as the 1940's): Gamow and his students Alpher and Herman; Zel'dovich in the '50s (who even suggested the Holmdel NJ horn antenna!), Dicke and Peebles in the early 1960's, it's not a short list. No post-diction required!

41. **Peter Woit**
May 2, 2023

Seeing some very smart people making very bad arguments about string theory has always made me wonder what they are thinking. I suspect that thinking "this is wrong but I'm going to write/say it anyway" (i.e. truly lying) is very unusual. But what is all too usual is people approaching the problem of what to write/say as "what can I say that will further my agenda and interests while not actually being indisputably untrue?" If you do this, I don't think you have a lot of right to complain when another physicist calls you a "liar" on youtube.

One thing that strikes me about ChatGPT is that it seems to work exactly like this. If you tell it to make an argument for string theory, it will produce much the same output as what you hear from the best string theorists. ChatGPT I don't think "lies", it just is designed to produce output that sounds plausible and convinces people, with the question of accuracy and fairness of its argument just not in the code. People's behavior I guess is much the same.

My experience of many string theorists is that in private they will avoid bogus arguments, and do their best to refuse to make public comments (because they know an accurate argument would cause problems). The ones who show up in the media are the ones with the lowest built-in restraint against saying something inaccurate.

It's interesting to contrast mathematicians to physicists. Mathematicians have a strong ingrained culture of never saying anything inaccurate, so rarely turn up in the media making bogus arguments (every so often they do show up making bad arguments about physics, but because of lack of expertise).

42. **Low Math, Meekly Interacting**
May 2, 2023

For the nitpickers: I double-checked my memory of KKLT, and what I took from it back then was A) there's no denying or avoiding the Landscape in ST, and B) KKLT's conjectured mechanism to break SUSY, get rid of massless scalars and the non-existent long-range forces they would mediate, and "uplift" metastable dS vacua to realistic lifetimes was hardly universally embraced, and hotly debated ever since. The verdict today appears to be either (super)ST is inherently incompatible with a positive cosmological constant, or our pocket of the multiverse doesn't "really" have one, despite the overwhelming observational evidence to the contrary. Who could have predicted such an outcome except…well, everyone who wasn't a fan of String Theory?

Assuming I got the above correct, I ask again: What good does it do anyone to say ST is an "honest" enterprise?

43. **Peter Woit**
May 2, 2023

David Derbes,
The dark matter question seems to me the opposite of string theory. It's completely conventional science: you have phenomena that don't fit your best theory (galaxy rotation curves, CMB observations) so theorists try and find a better theory that explains the data and experimentalists try to get better relevant data. Sure, theorists sometimes overhype the theory they work on, denigrate opposing ideas, but this is all very conventional science.

The problem with string theory is that it never became a conventional science, which is very different.

44. **Peter Woit**
May 2, 2023

LMMI,
I think that few string theorists are signed up for the "no positive CC" program of the swampologists. To me the whole "swampland program" seems like what you would get if you asked ChatGPT "the landscape is killing us on the PR front, what can we do about this?".

45. **David Derbes**
May 2, 2023

Peter,
I stay away from all string theory arguments. My thesis (1979) was on supergravity/local supersymmetry, so I feel some sympathy for the string folks. Me, I got out five seconds after I passed the thesis defense. I was simply addressing what I felt another poster was saying about dark matter. I agree completely with your remarks about dark matter; it is a handy way of describing an observed phenomenon (same as dark energy, though of course that has some theoretical underpinnings in Einstein's introduction of the Lambda term), and with luck we will eventually have a much better understanding of what's going on. I do think that some interesting math has come out of string theory, not that I understand much of it, and maybe down the road some good will come from it. I do remember early on—1976?—when Witten scored a PRL with a nifty proof of the positivity conjecture in GR with a supersymmetric argument, later modified not to need susy at all. And as I think you know, I have two friends and former (high school) students who have made careers at least in part with string theory. Both geniuses and good guys, in my opinion, for whatever that's worth.

46. **Peter Woit**
May 2, 2023

David,
I think you have in mind Witten's 1981 paper here
https://link.springer.com/article/10.1007/BF01208277

While he was motivated by thinking about supergravity, the argument doesn't need that, just properties of spinors and the Dirac operator. My impression is that Witten's paper has become a classic in the mathematics literature, often taught to grad students in differential geometry.

47. **Interested amateur**
May 2, 2023

Jonathan Baxter, at 43:00 Angela says:

"It's fine to do String theory… the fact that they spent 30 years on it is fine… they didn't do anything wrong, that's how science works, my issue is how they lied to everyone about it… for thirty years String theorists wrote books and gave interviews where they said within the next decade it's going to be the next big thing"

at 44:48:

"it's a lie to give an interview where you're like 'see the problem with the Standard model is this happens and my theory fixes it' when your theory is not a theory and you don't have any experiments. It's OK to give an interview and say 'here's what I'm working on, here's what I think will happen when we test it', but you need to say when we test it."

Take the LHC: it was built first and foremost to discover the Higgs. It was worth building to test the claims of the Standard model at the time; and the Higgs being confirmed in 2012 is a spectacular achievement IMO. But leading up to it, the public were told its other purpose was to possibly discover SUSY, which was 'likely' as some claimed, new dimensions, black holes, dark matter etc… when it wasn't. It's damaged the credibility of scientists being believed by the public when justifying them paying for their scientific funding.

String theory has also been used in a similar and damaging way according to Angela.

48. **Peter Shor**
May 3, 2023

Jonathan Baxter

If the string theorists on grant review panels deny grants to people trying to research alternative theories of quantum gravity (something that I am fairly sure happens), we never will get an alternative theory of quantum gravity.

Yes, none of the alternative theories of quantum gravity work very well yet, but if you use that as a reason to deny funding to people who want to research them, you get a self-fulfilling prophecy.

49. **Jonathan Baxter**
May 3, 2023

Peter Shor,

I've had a couple of recent comments nuked, so it seems opinions like mine are not particularly welcome. I get the hint: I wasn't going to bother commenting any more. But since you addressed me directly:

I doubt that serious alternative theories of quantum gravity have been nixed in favor of string theory. This all starts to sound very conspiratorial.

The undergraduate theoretical physics department I studied in had two "factions" for want of a better term: the "phenomenologists" and the "mathematical physicists". I thought phenomenology was dubious: making models you know are false yet somehow are going to yield insights into the truth. Felt like curve-fitting not science to me.

But other than the usual zero-sum struggle for a limited funding pie, I don't think the phenomenologists (who were ascendant at the time) deliberately nixed the research programs of the mathematical physicists.

50. **Peter Woit**
May 3, 2023

Peter Shor/Jonathan Baxter,

This has been getting off-topic, I'd like people to stick to discussing the video. I will however take the opportunity to add one thing relevant to the issue of how ideas get suppressed that I noticed recently. A postdoc ad
https://www.aei.mpg.de/1023262/postdoctoral-position-in-quantum-gravity-supergravity-string-theory
for the "Quantum Gravity and Unified Theories" department in Potsdam specifies explicitly
"Potential candidates should have a PhD and a research background in supergravity, string theory or their symmetry structures."
This kind of explicit specification that people pursuing different ideas shouldn't even apply isn't that common, but if one has any experience with how postdoc and junior faculty hiring in this field works, I think it's very clear to everyone what your career prospects are if you try and follow a different road.

The whole thing is kind of bizarre actually. The SUSY/string theory approach to unification has become widely discredited among the physics community (and physicists on youtube…) but people are still advertising jobs restricted to working on this approach.

Sorry, but for further comments, please try to stay on-topic…

Note added (5/5/23): The example I gave here was a bad one. The department at Potsdam, with Hermann Nicolai as director until recently, has been among the places more open to a wider array of ideas. I had not understood the significance of the clause ("or their symmetry structures"). The position advertised is to work with Axel Kleinschmidt, who (see here) works on some interesting new ideas about symmetries, which is exactly the sort of thing I'd hope to see more of.

51. **Sage of PHY6 Paths**
May 3, 2023

Douglas Natelson,
I NEVER claimed that all people who do condensed matter or AMO or nanophysics go into those despite being more interested in hep. I know many people who are genuinely interested in those stuff though I am not one of them. But many people move from hep-th to applied physics very early in their career [end of undergrad or beginning of PhD] due to lack of opportunities. The opposite [people interested in applied physics moving to hep] will never happen due to lack of funding to hep.

52. **John Baez**
May 3, 2023

I enjoyed Collier's string theory video a lot, almost as much as her video on crackpots where she points to my Crackpot Index as archaic evidence that physics cranks have always been the same. (What I liked most about her crackpot video is her answer to the inevitable question "but what if they're right?")

For me the most cringeworthy mistake in her string theory video was her guess that the pion was a particle whose existence was predicted by the Standard Model. Kids these days! They don't even remember Yukawa's prediction of the pion back in 1935, the whole invention of the idea that short-range forces are carried by massive bosons, and the confusion caused by the muon???

But anyway, it doesn't really matter for her point.

53. **Jim**
May 4, 2023

A nice article:
https://www.symmetrymagazine.org/article/whatever-happened-to-the-theory-of-everything?language_content_entity=und

54. **Peter Woit**
May 5, 2023

I've added a note to this comment
https://www.math.columbia.edu/~woit/wordpress/?p=13482&cpage=1#comment-243561
about a job advertisement for a postdoc at Potsdam. While the problem I was referring to is a real one, this was not a good example, for reasons explained in the note.

55. **JE**
May 5, 2023

Peter,

No matter how fair and well-meaning your note can be, the publicly-funded Max Planck Institute is proudly publicizing an award to a young researcher for "making string theory more calculable" on the webpage you linked to, on grounds that "String theories are candidates for a unification of Einstein's general relativity and quantum field theory", which does not sound to me like a new or very interesting idea in the 2020s. The award was "intended to motivate especially gifted early career researchers to pursue a future university or research career", and the awardee completed his PhD in Hermann Nicolai's department of Quantum Gravity and Unified Theories under the supervision of Axel Kleinschmidt.

Hopefully they will now be exploring more promising and interesting ideas about symmetries, as you say…

56. **Peter Woit**
May 5, 2023

JE,
I'd be a lot happier if Potsdam and other places explicitly gave up on this sort of string theory research, and this job ad is not completely unproblematic, but the important question is that of what research activity by young theorists is getting funded. This particular case isn't a good example of the problem (funding limited to bad, failed ideas), with the research program's focus on new ideas about symmetries a good one. Would be better without the starting point being string theory/supergravity, but one can't have everything…

57. **Frank Wilhoit**
May 6, 2023

@Anonymous,
Appalachia is not a bottom-line place. It is a story-telling place. And the nature and place and purpose of story-telling are absolutely central, not only to the particular disputes about the scientific method that are addressed here, but to every other aspect of 21st-Century life. Story-telling is such a fine thing that no one ever stopped to think what would happen if we had too much of it.

58. **Marion Delgado**
May 11, 2023

I watched this and my very first thought was "Peter Woit should see this." I used to be a faithful reader of this blog, back in the day, and I never wrote about this, but I was already familiar with Sabine Hossenfelder and some others, but it was through "Not Even Wrong" that I discovered Scientific American's John Horgan, and I am really grateful to the blog for that. Back before the day, I used to drag my academic friends to string theory presentations, and when they were critical, I couldn't explain away their critiques, so I am also grateful for Dr Woit putting the objections in better words than theirs or mine and affirming them.

59. **SRP**
May 13, 2023

From Jim's Symmetry link:

"But to push beyond the Standard Model and toward a theory of everything, scientists need a foothold in the form of an unexpected deviation—which they have yet to find.

"The Standard Model is too good," says Peter Woit, a theorist and mathematician at Columbia. "Despite a huge effort we haven't been able to come up with something better.""

Sure, if one ignores the anomalies Krisch and colleagues found decades ago, at readily accessible energies, then QCD is fine. But as far as I can tell, understanding of this phenomenon isn't much advanced since this review from ten years ago.
https://arxiv.org/pdf/1001.0790.pdf

60. **Curious Mayhem**
May 31, 2023

The Angela Collier website seems to be down: http://www.acollierastro.com/

61. **bryan**

June 10, 2023

I finally got around to watching this video, and it really resonated with me for 2 reasons:

1. This reminds me of how I felt reading popular science books in the 90s

2. This is a wonderful demonstration of modern casual science communication

For the first point, I felt I was mislead about the likelihood of ST being real. I remember reading Peat's "Superstrings and the search for everything" for recreation in college when I was studying engineering. (Ironically, if I remember correctly, that books spends a good deal of time discussing Twistor theory). Anyway, I was pretty convinced as a layman that ST was likely true, and that SUSY would likely be discovered at the LHC. I think I remember Peter saying on this blog the likelihood from the professional physics community of SUSY detection at LHC was actually closer to around 10%, and I was confused why my impression was so different.

On the second point, I think it is important for technical experts to understand this type of science communication. Playing a video game (in this case the classic roguelike "Binding of Isaac") while talking is not a "joke" or meant to be disrespectful, rather discussing topics while streaming other content is a casual way to engage the audience. Look at chess #2 player in the world, Hikaru Nakamura. He considers himself now a streamer first, and a chess player second. He will have casual conversations about chess and pop culture while playing online. He just won the Norway Chess tournament and during the finals he was doing video confessionals mid-game to explain what he was thinking mid-game, doubts and all. This type of communication is making chess more popular among teenagers.

This casual stream of consciousness talking on very technical subjects makes the streamer very vulnerable, but it allows the audience to understand their though process at a deeper level. Commenters here are complaining about the inaccuracies in her video, they would likely rather read a polished peer-reviewed paper with proper citations, but this is the last step in process and not the first. I believe most people would rather hear someone's raw thoughts and ideas, where they're not afraid to be wrong.

Quantum Supremacy

May 3, 2023
Categories: Book Reviews, This Week's Hype, Wormhole Publicity Stunts

We're hearing this week from two very different parts of the string theory community that quantum supremacy (quantum computers doing better than classical computers) is the answer to the challenges the subject has faced.

New Scientist has an article Quantum computers could simulate a black hole in the next decade which tells us that "Understanding the interactions between quantum physics and gravity within a black hole is one of the thorniest problems in physics, but quantum computers could soon offer an answer." The article is about this preprint from Juan Maldacena which discusses numerical simulations in a version of the BFSS matrix model, a 1996 proposal for a definition of M-theory that never worked out. Maldacena points to this recent Monte-Carlo calculation, which claims to get results consistent with expectations from duality with supergravity.

Maldacena's proposal is basically for a variant of the wormhole publicity stunt: he argues that if you have a large enough quantum computer, you can do a better calculation than the recent Monte-Carlo. In principle you could look for quasi-normal modes in the data, and then you would have created not a wormhole but a black hole and be doing "quantum gravity in the laboratory"

> seeing these quasinormal modes from a quantum simulation of the quantum system under discussion, would be a convincing evidence that we have created something that behaves as a black hole in the laboratory.

This isn't a publicity stunt like the wormhole one, because the only publicity I've seen is a New Scientist article, and this is just a proposal, not actually executed. Maldacena estimates that to reproduce the recent Monte-Carlo calculation you'd need 7000 or so logical qubits, which the New Scientist reporter explains would be something like one million physical qubits. So, there's no danger Quanta magazine will be producing videos about the creation of a black hole in a Google lab any time soon.

Maldacena has been chosen to give the presentation tomorrow at the SLAC P5 Town Hall about a vision for the future of fundamental theory, no idea whether creating black holes in the lab using quantum computers will be part of it.

At the other extreme of respectability and influence in the physics community, Michio Kaku has a new book out, Quantum Supremacy. I took a quick look yesterday at a copy at the bookstore. I'll leave it to others to discuss the bulk of the book, which seems to be about how "There is not a single problem humanity faces that couldn't be addressed by quantum computing." The last few pages are about string theory, beginning with the usual bogus pro-string theory arguments, working up to the ending of the book: "So quantum computers may hold the key to creation itself" (i.e. they will "solve all the equations of string theory"). His argument for the relevance of quantum computers to string theory is that they will calculate paths in the landscape:

> One day, it might be possible to put string theory on to a quantum computer to select out the correct path. Perhaps many of the paths found in the landscape are unstable and quickly decay, leaving only the correct solution . Perhaps our universe emerges as the only stable one.

This is justified by a bizarre paragraph about lattice gauge theory, which explains that since we can't solve QCD analytically, here's what theorists do:

> One solves the equations for one tiny cube, uses that to solve the equations for the next neighboring cube, and repeats the same process for all that follow. In this way, eventually the computer solves for all the neighboring cubes, one after the other.

This pretty conclusively shows that the explanation for the Kaku phenomenon is simply that he has no idea what he is talking about.

Update: Michio Kaku was on a very recent Joe Rogan Experience, getting a huge audience for his explanations of quantum computing. Some commentary here.

Update: The reviews of the book have been pretty uniformly very enthusiastic, with the reviewers evidently incapable of distinguishing sense from nonsense. A depressing example is at Science magazine. Why choose as reviewers of a book on quantum computing two people who know nothing about the subject? Is it because *Science* couldn't find anyone who does know about quantum computing willing to read the book and write about it?

Update: Scott Aaronson has read the book and confirms that it's every bit as awful as it seems. For a different look at out-of-control quantum computing hype, see here

Update: The one thing keeping my spirits up while reading the almost uniformly glowing reviews of this piece of junk has been the thought that "at least the New York Times is kind of doing the right thing": not reviewing the book. Just noticed they do have a review up:

> That mind-blowing future is the focus of the final five or so hours of the audiobook, which explores the real-world impacts quantum computing could have: altering our immune systems to avoid cancer and Alzheimer's, increasing crop yields, ending world hunger. As Kaku puts it, "the familiar laws of common sense are routinely violated at the atomic level"; but his lucid prose and thought process make abundant sense of this technological turning point.

Just shoot me…

Comments

1. **David Metzler**
   May 3, 2023

   I'm a bit surprised that these folks haven't pivoted from "quantum computers will save HEP" to "large language models will save HEP."

   On a different topic, is there an accessible Google sheet detailing the planned schedule for this run of the LHC, like you linked to last year? That was great for us watching at home.

2. **Sabine Hossenfelder**
   May 3, 2023

   Estimates that I have seen say it'll require about a million physical qubits for 110 to 150 or so logical qubits, though that depends on error rate and algorithm. I strongly doubt that you can do 7000 logical with only 1 million physical ones, it's almost certainly considerably more. At the very least I'd like to see a source for the NS estimate.

   Besides that, I think it would be good if journalists writing about this stuff would just ask people making such claims "and then what?". I mean, let's assume for a moment we actually manage to build a 7000 logical qubit quantum computer and actually simulate Maldacena's whatever model on it. And then what? They'll write papers and press releases. And then what? What will we learn from it? What will we do with it?

Well, nothing. We'll learn that you can run a certain code on a certain computer. Will it tell us what's going on in black holes? Of course not because you still have no idea if that code you ran on your computer actually describes a black hole. What will we do with it? There's nothing you can do with it.

I know that many people in the field dismiss my output-orientation as unscientific but I would like to argue that this is just a tactic they have developed to shield themselves from criticism. There's a LOT of public money going into quantum technology and people deserve an honest answer to the question what it's supposed to do for them.

PS: I just recently made [a video about what we could do with quantum computers](), in case someone is interested.

3. **Peter Shor**
   May 4, 2023

   @Sabine: Your comment is really much too negative about numerical/quantum simulations in general.

   This experiment certainly could possibly give a negative answer to the question: is the BFSS matrix model the same as one formulation of type IIA string theory. Negative answers can be valuable.

   And if you think that a positive answer on a numerical experiment can never definitively tell you that this is what is going on in the physics world, then why did the Nobel committee give a prize to the discovery of the Hulse–Taylor pulsar system, where numerical calculations confirmed that it was losing energy because of gravitational waves. (For all we know, there might a different mechanism that just happens to have the same orbital decay parameters.)

   Would Maldacena's proposed experiment tell us much? Probably not … looking at the abstracts of the papers Peter links to above, it seems to be testing whether two theories, neither of which comes close to describing the real world, predict the same result. Would this tell us anything about real-world black holes? It's not clear.

4. **Mark**
   May 4, 2023

   @Peter Shor

   You don't need numerical calculations for the Hulse-Taylor pulsar, it's just quadrupole radiation described in textbooks, see e.g. eq. (3) in
   https://arxiv.org/pdf/1011.0718.pdf

   But the broader point of needing numerical GR for discovery is valid. In the case of the discovery of gravitational waves by LIGO they needed waveform templates from numerical simulations.

5. **Low Math, Meekly Interacting**
   May 4, 2023

   I thought, as Seth Lloyd reportedly proved in the 1990's, Feynman's conjecture that quantum computers could do quantum simulations no classical computer could do is a notion that still holds up despite the huge advances in classical computing. Whether building a useful universal quantum simulator is technically feasible I don't know, but seems like a worthwhile objective to pour some cash in, to support some very down-to-earth basic science in e.g. chemistry and condensed matter.

   One rather hopes this highly speculative AdS/CFT-inspired "tabletop black hole" stuff doesn't end up killing interest in the entire field once the backlash reaches a critical level.

6. **Peter Woit**
   May 4, 2023

   David Metzler,
   The "machine learning will solve the problems of string theory" thing has been going on for a while, see for example

   https://news.northeastern.edu/2019/08/05/how-theoretical-physicist-james-halverson-is-using-data-science-and-machine-learning-to-study-string-theory/
   https://iaifi.org/
   https://lims.ac.uk/string-data-2022/

   For the 2023 LHC schedule, see
   https://edms.cern.ch/ui/file/2785501/1.2/2023-LHC-schedule_V1.2.pdf

7. **Peter Woit**
   May 4, 2023

   LMMI,
   The danger to the field of quantum computing is from people like Michio Kaku, getting huge audiences for absurd claims at Joe Rogan. There's already a lot of discussion of a possible coming "quantum winter", with the hype hitting the wall of the reality of slow progress.

   Things like the article in New Scienctist about Maldacena's proposal for creating black holes don't get a lot of wider public attention. The danger of this is not to quantum computing but to fundamental theoretical physics. There's a lot of money and attention out there for anyone who claims to have an idea for how to do quantum gravity using machine learning, quantum computers, quantum information theory, etc. If prominent theorists change their research programs to chase that money and attention, it could move a field already in trouble because of string theory/susy into a complete intellectual collapse dominated by pursuit of nonsense.

8. **Tim Bradshaw**
   May 4, 2023

   Peter Shor,
   I think (hope) that you have missed the point of Sabine's comment: using a numerical model of a theory to test its predictions against some actual experimental measurements, as in the case of Hulse-Taylor or a huge number of other cases (the detection of gravitational waves, for instance) is an entirely different thing than 'testing' a theory for which no experimental evidence exists using a numerical simulation.

   Low Math, Meekly Interacting,
   I am not sure what Feynman thought quantum computers could do, but they gain you no formal power over classical machines: any problem which can be solved with a quantum computer can be solved with a classical computer, and vice versa. What they do gain is an improvement in time complexity for some problems. That in practice makes some problems soluble which would not be soluble on a classical machine because they have some awful time complexity.

   How anyone can claim that "there is not a single problem humanity faces that couldn't be addressed by quantum computing" is entirely mysterious to me.

9. **Mike**
May 5, 2023

Out of interest, why is there no public effort by physicists to counter the relentless nonsense coming from Kaku?

I know that there is a LOT of hype and inaccurate information in pop Sci books and science magazines, but Kaku seems to be responsible for a disproportionately large share of the garbage!

Maybe a joint letter in Scientific American? I don't suppose it would help much. Kaku will still churn out the books, TV documentaries and be invited for interviews as the expert of anything and everything. My heart sinks everything I hear Kaku has taken an interest in a new topic.

10. **André**
May 5, 2023

"How anyone can claim that 'there is not a single problem humanity faces that couldn't be addressed by quantum computing' is entirely mysterious to me."

He does say *addressed*, not *solved* ;).

11. **No Math, Random Reader**
May 5, 2023

Honest question about the things Michio Kaku said (maybe dumb question… but still, honest): how much is fair to expect from a venue like Joe Rogan's podcast? I wouldn't expect accuracy or even factuality of people having casual conversation in a media not famous for high level talks. My point is: isn't this, by itself, a proof that these kind of theories will eventually get to the level of "were the piramids built by aliens"? The kind of thing that just will not go away because it provides entertainment. There is lot of people like me out there: people who knows nothing about math or physics (and never will, by the way, I already have lots of thing to read just from my job), but enjoy watching edutainments like scishow, pbs spacetime, etc. I think most of these people knows there is something sketchy or at least controversial about string theory, even if they can't quite explain what is the matter. I mean… people are watching Sabine's channel too.

12. **Peter Woit**
May 5, 2023

Mike,
That's a good question, one I've asked people over the years. On the string theory front, Kaku has been writing misleading popular books on the topic for over 35 years. Most string theorists agree, at least privately, that what he writes and says has become increasingly inaccurate and increasingly embarrassing. They generally however make the argument: "dealing with this is not my responsibility, there's loads of misleading stuff out there about science. And, maybe it will have a positive effect, getting young people and the public interested in this kind of science, going on to read better books." A cynical point of view on this would be that you can't expect people to go out of their way to challenge something untrue which is promoting their own interests. When it gets to the point of damaging their interests, they might do something and we're not there yet.

As for his nonsense about quantum computing, there's so much promotional bullshit going on that it's understandable that anyone wanting to do something about this has no particular reason to start with Kaku.

No Math, Random Reader,

The problem is Kaku, not the Rogan podcast.

13. **Alessandro Strumia**
May 6, 2023

Kaku says something wise in this scientific debate: https://www.youtube.com/watch?v=mY5V5jqdX9U.
Trying to summarize:
Strings become mainstream because a generation of smart theorists risked their careers attracted by string theory. Now string theorists control theory groups and this can only change if somebody proposes a better alternative. Not by un-constructive criticism.

14. **Geoffrey Dixon**
May 6, 2023

Alessandro Strumia

Of course, "better alternatives" are themselves risks to the careers of those wedded to comatose ideas. And they take great pains to ensure that they are buffered against the slings and arrows of outrageous fortune by indoctrinating the next generation with this same professional angst.

15. **Peter Woit**
May 6, 2023

Alessandro Strumia,

Given the present state of the HEP theory community in general, its ability to recognize and respond appropriately to a good new idea is not so clear. One thing that is very clear is that Michio Kaku is a convincing example of a theorist with zero chance of even understanding a new idea (his discussion of lattice gauge theory shows he's incapable of understanding even the most basic aspects of current ideas about QFT).

16. **vmarko**
May 6, 2023

Alessandro Strumia,

The "only game in town" argument has not been valid for quite some time now. There actually are various alternative proposals out there, some arguably better than others, but for some reason they fail to attract attention as promising research directions. As a consequence, very few people work on those proposals, and therefore progress is slow.

And in the meantime string theorists are also making no progress, but they represent the vast majority within hep-th and gr-qc, and are not interested in abandoning their old community for some underdeveloped new idea. It's a viscious circle — lack of interest by the community produces lack of manpower, which produces lack of development, which produces lack of interest by the community…

That’s the actual problem with better alternatives to ST.

Best, 🙂
Marko

17. **Random Non-Physicist**
May 6, 2023

“And, maybe it will have a positive effect, getting young people and the public interested in this kind of science, going on to read better books.”

Let me say, as someone who was once on a track to become a physicist partly due to the influence of popular science, that one problem with Kaku-style hype is it gives people an unrealistic idea of what science is about and what it can do. He paints a picture of a magical, science-fictional world where the human intellect and its technology have god-like powers, avoiding the messy details and limiting realities. They’re alluring stories for the uninitiated, but when you start to look under the hood, understand the science in more detail and see that it’s not really like Kaku says, it’s easy to become disillusioned, to realize that you’ve been taken in by propaganda, and to lose interest in the field. That’s kinda what happened to me anyway.

Unfortunately, in America we tend to reward the hucksters, the entertainers, the PR people and the imaginers of fantasy worlds much more than those who deal in less glamorous reality. A capitalist entertainment-centric society has little incentive to reign in the Michio Kakus of the world, and every incentive to promote them. Science should be the complete opposite of this ethos, but it has clearly been corrupted by the larger society.

18. **John Baez**
May 8, 2023

Someone here almost coined a very useful term for describing the slow progress of fundamental physics: “viscous circle”.

19. **vmarko**
May 9, 2023

John,

Feel free to coin the term! 🙂

I meant to write “vicious circle”, but made a typo, which can then be reinterpreted as a typo of “viscous circle” instead, with a curiously similar meaning to the former…

Best, 🙂
Marko

20. **Shantanu**
May 10, 2023

Peter : something OT. I am surprised you haven’t mentioned anything about passing away of Stanley Deser one of the doyens of GR and also supergravity, who has been working on GR for past 70 years starting from the days he overlapped with Einstein at IAS.

21. **Peter Woit**
May 10, 2023

Shantanu,
Nothing about Deser just because I never met him and know little about his work. Part of the problem is that I’ve always shared Sidney Coleman’s view on supergravity.

I can point to places that have more about Deser, the NYT at
https://www.nytimes.com/2023/05/08/science/stanley-deser-dead.html
and an interview at
https://heritageproject.caltech.edu/interviews-updates/stanley-deser

22. **John Baez**
May 12, 2023

The Arnowitt-Deser-Misner formulation of general relativity in terms of initial data on a spacelike slice is a big deal.

23. **Felix C.**
May 14, 2023

While we’re on the subject of Stanley Deser, I’ll mention that World Scientific is providing free access to his autobiography, _Forks in the Road_, until July 31 of this year:

https://www.worldscientific.com/doi/epdf/10.1142/12205

24. **Mike**
May 19, 2023

Scott Aaronson has taken one for the team: https://scottaaronson.blog/

Shockingly he did not love the book!

25. **Mark Hillery**
May 23, 2023

It gets worse. Neil deGrasse Tyson interviews Michio Kaku:
https://www.youtube.com/watch?v=IfvoBlDUrPU

26. **Peter Woit**
May 23, 2023

Mark Hillery,
What an embarrassing bunch of bullshit. What is wrong with Tyson?

27. **Giovanni Ronchi**
May 25, 2023

Interview with Kaku about the book by Michael Shermer

https://youtube.com/watch?v=0lsO-dSR-7s

28. **Peter Woit**
May 25, 2023

Giovanni Ronchi,
That's really depressing. Shermer just swallows all Kaku's nonsense whole. Amazing that someone whose whole thing is about the importance of skepticism, just believes every ridiculous and untrue thing Kaku says because he is supposedly a scientist.

29. **former mathematician**
June 5, 2023

The June 4 New York Times Book Review led its Audiobooks column with "Quantum Supremacy."

Various and Sundry

May 30, 2023
Categories: Uncategorized

A few unrelated items:

- I've been hearing from several people about their plans to travel to China this summer, just realized that they're all going there for the same reason, to participate in the First International Congress of Basic Science. This is something new and on a grand scale, featuring 240 or so invited speakers, award of a new million dollar – plus prize, together with prizes for "Best paper" over the last five years in 36 different categories. Yau is the main organizer, and the Chinese government is providing the funding. So, if it's July 16-28 and you are wondering where your colleagues are, quite possibly the answer is Beijing.
- I'm doing my best to try not to think about the implications of recent AI developments for mathematics, but someone who is doing a lot of thinking about this is Michael Harris, who this week at his Silicon Reckoner substack discusses Google's use of arXiv math papers to train their Minerva language model. Harris raises the interesting question of whether this use of arXiv papers violates the licenses of these papers, standard ones of which include language like

  > You may not use the material for commercial purposes.

  Even if Google is massively violating the arXiv licenses for commercial purposes, it's unclear whether anything can be done about this, especially given the legal resources Google can afford. In addition, I suspect that when hearing about this a more common response than "this is terrible, I want to sue" would be "this is great, how can I get this thing to write papers for me, or even better, get Google to pay me to help make this possible."
- Last month Symmetry magazine had an article Whatever happened to the theory of everything? featuring some quotes from me and John Ellis. Ellis explains that the particle physics community has become skeptical of supersymmetry and string theory:

  > Supersymmetry seemed less and less likely to be right, and superstring theory never materialized into something with testable and concrete predictions.
  >
  > "The rest of the community is asking, 'Where's the beef?'" Ellis says. "There hasn't been any beef yet. Maybe particle physicists have turned a bit vegetarian and have lost interest in stringy beef."
- Possibly in response to the problem for string theory that Ellis is pointing to, Witten next week is giving a non-technical theoretical physics colloquium talk at the ICTP on What Every Physicist Should Know About String Theory. Back in 2015 he published something with the same title in Physics Today, which I wrote about here. We'll see if there are any new arguments on this now very old topic.

Update: The 2023 Shaw prize in mathematics is going to Drinfeld and Yau.

Update: I missed the fact that last there was a Breakthrough Prize ceremony last month. This year they've emphasized even more the "Oscars of science" idea by moving the ceremony from Silicon Valley to LA and having it at the Academy Museum of Motion Pictures. The announcement has none of the names of the scientists, just the names of the Hollywood stars that would attend.

Update: I see (from a Strumia tweet) that a Witten 2015 talk with the same title is available here, was given I guess as a public talk at Strings 2015. It's all about the differences between the 1d single-particle path integral and the 1+1d worldsheet path integral, unclear to me why this is something every physicist needs to know about, or whether this year's version will be different.

Comments

1. **Peter Shor**
   May 30, 2023

   From playing around with language models, it seems to me that they are quite good at assimilating information that is on the internet and applying it to problems, but remarkably bad when faced with problems where they actually have to do something new. If I'm correct about this, the Minerva language model will fall well short of their hopes that it will "help push the frontiers of science and education."

2. **Z Y**
   May 30, 2023

   Philosophy wise, if there's a basic science field that can make a call to assemble all the others together, that field may well be mathematics. Still, social wise is puzzling that in fact are mathematicians the ones who try to bring different kind of basic scientists together.
   The question remains, why named it: ICBS, if they don't even include the life sciences or chemistry.
   PS. Fesenko continues to be a peculiar story (aswell as Caucher B., if for entirely different reasons)
   PS2. Seems almost all current russian math luminaries will be speakers

3. **Sabine Hossenfelder**
   May 31, 2023

   Particle physicists would be well advised to think about what went wrong so they don't go on repeating their mistakes. A huge amount of brain-time was spent on producing papers that will never serve any useful purpose. Thousands of intelligent people have wasted their life on this. I find it incredibly tragic. Imagine how much farther ahead society could be if they'd put their mind to work on something useful.

4. **Eric Lehman**
   May 31, 2023

   The Minerva model was announced almost a year ago (June 30, 2022) on the Google Research blog, making it ancient technology by now. No commercial use has emerged since then, and probably none can at this late date.

   Setting that aside, I believe Harris misunderstands the NonCommercial (NC) clause. He says:

   "Google is not a charitable organization; all of Google's purposes are "commercial" unless demonstrated not to be…"

   But Creative Commons repudiates this reasoning at https://wiki.creativecommons.org/wiki/NonCommercial_interpretation

“The definition of NonCommercial depends on the primary purpose for which the work is used, not on the category or class of reuser. Specifically, a reuser need not be in education, in government, an individual, or a recognized charity/nonprofit in the relevant jurisdiction in order to use an NC-licensed work.

Under the NC clause, Google and others may use arXiv papers in a manner “not primarily intended for or directed towards commercial advantage or monetary compensation”. Creative Commons further clarifies, “The inclusion of “primarily” in the definition recognizes that no activity is completely disconnected from commercial activity; it is only the primary purpose of the reuse that needs to be considered.”

In short, Google appears to have used the arXiv content for a research publication, which is permitted under the NonCommercial clause. Am I missing something???

5. **Gurmeet Singh**
   May 31, 2023

   LLMs won’t be very good at producing proofs but they are of course not the right tool for that. One possible right tool would be something like a DeepRL proof search which uses LLM for the heuristics, there have been attempts at this like GPT-f but they are not very flashy, producing formal proof is less flashy than human readable proof…

   What do you think of the point of view that maths is so important for humanity that we should not leave it to humans alone. If large parts of maths can be automated, we’ll get more powerful mathematical tools. For my own satisfaction, I can solve already solved problems, they are still fun like Olympiad problems.

6. **Greg Guy**
   May 31, 2023

   What makes the whole ‘do not use for commercial purposes’ is that the papers are indexed by Google so that they show up in searches. Arguably this is a commercial purpose, but do people really want Arxiv to be unsearchable? If so it’s hard to see how you can be for one commercial indexing scheme and not for another.

7. **David Roberts**
   May 31, 2023

   With my perhaps-too-cynical hat on, I wonder if Witten will win the Fundamental Science Lifetime Award.

8. **Peter Woit**
   May 31, 2023

   David Roberts,
   I think Witten would be a reasonable choice for such a prize. But this makes clear the lack of any good reason for proliferating such prizes, since giving Witten another prize would change nothing about anything. 20 years ago as far as I can remember there were no large monetary prizes in Mathematics. Now we have the Shaw prize, the Abel, the Breakthrough Prize and this prize, all at or above a million dollars. What good do any of these do, what is the point of coming up with new rewards for those already most recognized and heavily rewarded by the field? Is yet more of a star system what mathematics really needs?

9. **Peter Shor**
   May 31, 2023

   David Roberts:

   Hasn’t Witten made enough contributions to pure mathematics to deserve the Fundamental Science Lifetime Award? The only argument against giving it to him is that if you do, all the string theorists will say that he won it for string theory.

10. **Low Math, Meekly Interacting**
    May 31, 2023

    Didn’t see a mission statement anywhere, apologies if I missed something obvious: Is the “Basic Science” meeting to be focused on those three areas moving forward, or does the International Congress plan on having a theme each year such that other areas of “basic science” are discussed? More of a semantic quibble, but I might have called it something else depending on the answer.

11. **Peter Woit**
    May 31, 2023

    LMMI,

    The only general information I see about the conference is here
    https://www.icbs.cn/en/web/index/18009_1464241__

    Over 300 speakers, awards to be presented by Chinese leaders. It says this will be an annual event, unclear if in other years “Basic Science” will mean a different choice of fields, but that seems possible.

12. **anon**
    May 31, 2023

    The prize needs Witten (or some other very famous scientist) much more than he needs the prize.

13. **Peter Woit**
    May 31, 2023

    anon,
    Very true, and not just in this case…

14. **David Roberts**
    May 31, 2023

    @Peter Shor

    I really wonder how one judges the relative merit of nominees when the field is presumably all of (?pure) mathematics, (?fundamental) physics, and theoretical computer science.

Just looking at the attendees and the organisers, it feels like a string theorist will get the prize, and I presume they will follow the time-honoured tradition of giving the inaugural prize to the biggest name.

15. **David Roberts**
June 1, 2023

On a different note, of the judges for the best paper prizes, there are, for the mathematics topics (if I managed to count them properly; also I checked all the people I didn't know): 89 men, 4 women, 17 anonymous.

For physics there are 17 judges, one is a woman (and none anonymous).

I haven't the patience to check the majority of the 47 judges in computer science that I don't recognise the names of.

On a slightly less serious note: of the four judges for "Mathematical logic, Foundations and Category Theory", none of the them are category theorists, one is a set theorist, one works on independence results, and two are recursion theorists. Not sure how they are going to judge the quality of an influential category theory paper 🙂

16. **Oisin McGuinness**
June 2, 2023

Regarding the Breakthrough prize, it appears this ceremony honors the last 3 years of winners, and see this page: https://breakthroughprize.org/News for the Sept 22 2022 announcement of the 2023 winners. So old news….?

17. **Shantanu**
June 2, 2023

Peter: sad news about Jim Hartle https://www.physics.ucsb.edu/news/announcement/2132
I am surprised you have not mentioned it given that he is one of the pioneers of quantum gravity

18. **Peter Shor**
June 3, 2023

@Oisin McGuinness: As a Breakthrough Prize winner who attended the ceremony, let me correct you; The Breakthrough Prize Ceremony did not honor all 3 years of winners; some of them will be honored at next year's ceremony.

19. **James Smith**
June 5, 2023

Eric Lehman,

you write:

"The Minerva model was announced almost a year ago…making it ancient technology by now."

So any genuine idea or insight over a year or so old becomes worthless then?

You also write:

"No commercial use has emerged since then, and probably none can at this late date."

Not so. The likelihood is that tools to help mathematicians based on this kind of AI will begin to emerge in the next two to three years.

Actually, the Minerva approach is a good one in some people's opinion, including mine, and the idea is not going to die away.

Using training data that includes TeX markup rather than stripping it out is a good idea for the reasons that the authors give. And having your model also spit out TeX markup is also arguably a good idea because, as long as the output is subsequently rendered by something like KaTeX, it remains human readable.

Unreadable proofs are the bane of formal reasoning systems built on type theories and always have been. I am hoping that approaches such as this will move the field to consider systems with more legible output.

20. **Francis**
June 9, 2023

I have repeated the Minerva examples in the arXiv paper by using the current version of the free ChatGPT (based on GPT3.5). The results are better than those reported in the arXiv paper (correct except in one case). Here is a link for the full chat https://chat.openai.com/share/da97cc49-db06-4355-937b-c51ccc2a42f0 Enjoy!

21. **Paul Levy**
June 10, 2023

Why on earth would you train a model on arXiv papers and then ask it to solve high school calculus questions?

22. **Shantanu**
June 11, 2023

Peter: Witten's talk is now online at https://www.youtube.com/watch?v=s9qC7DtB7tY

23. **liuyao**
June 13, 2023

I predit that the inaugural physics prize will go to C.N. Yang, who is 101 and lives in Tsinghua (5 out of the 7 chairs are at Tsinghua, with Yau sitting on all three committees.)

24. **Shantanu**
June 17, 2023

Peter, something OT: Check out https://pirsa.org/23060016 which is a panel debate related to a recent conference in PI.
and in particular the question asked to Rob Myers (related to string theory) about at 28 minutes. he also said that we have seen evidence for teleportation across an Einstein Rosen bridge.

25. **Edward Teach**
July 3, 2023

Hilarious that Fesenko was plastered all over the ICBS homepage at first, only to be dropped in favor of Fukaya and Kontsevich.

My spy in Beijing says that he expects only “students and prize winners” to attend this thing.

From Quantum Mechanics to Number Theory via the Oscillator Representation

June 1, 2023
Categories: Uncategorized

This past semester I taught our graduate class on Lie groups and representations, and spent part of the course on the Heisenberg group and the oscillator representation. Since the end of the semester I've been trying to clean up and expand this part of my class notes. I'm posting the current version, working title From Quantum Mechanics to Number Theory via the Oscillator Representation. This is still a work-in-progress, but I've decided today to step away from it a little while, work on other things, and then come back later perhaps with a clearer perspective on what I'd like to do with these notes. In a few days I'm heading off for a ten-day vacation in northern California, and one thing I don't want to be thinking about then is things like how to get formulas involving modular forms correct.

There's nothing really new in these notes, but this is material I've always found both fascinating and challenging, so writing it up has clarified things for me, and I hope will be of use to others. The basic relationship between quantum mechanics and representation theory explained here is something that I've always felt deserves a lot more attention than it has gotten.

In the past I've often made claims about the deep unity of fundamental physics and mathematics, One goal of this document is to lay out precisely one aspect of what I mean when making these claims. There are other much less well understood aspects of this unity, but the topic here is something well-understood.

One thing that struck me when thinking about this and teaching the class is that this is a central topic in representation theory, but one that often doesn't make it into the textbooks or courses. Typically mathematicians develop theories with an eye to classifying all structures of a given kind. This case is a very unusual example where there is effectively a unique structure. The classification theorem here is that there is basically only one representation, but it is one with an unusually rich structure.

When I get back from vacation, I plan to get back to work on the ideas about twistors and unification that I'm still very excited about, but have set to the side for quite a few months while I was teaching the class and writing these notes. More about that in the next few months…

Comments

1. **Jim Golden**
   June 2, 2023

   This is great, thanks for this. I decided to (re)tackle Not Even Wrong again this summer. My graduate work in mathematics is some years behind me (and was applied math / engineering). I'm enjoying making this a summer reading project. As part, I picked up Weyl's "Symmetry" and Hall's "Lie Groups, Lie Algebras, and Representations" based on your suggestions in the readings. Also found a copy of Penrose's "The Road to Reality" (that's a summer project in itself…) I look forward to adding these notes to my reading list. Have a great vacation!

2. **John Baez**
   June 6, 2023

   I look forward to the number theory section! Your notes remind me slightly of Mackey's book *Unitary Group Representations in Physics, Probability, and Number Theory*.

3. **Peter Woit**
   June 6, 2023

   John Baez,

   The main reason I lost steam and haven't yet finished the notes is that I want to spend the summer working on other things. But also relevant are the following:

   1. The way the story of the oscillator representation and the theta correspondence is set up in detail over the real numbers goes over straightforwardly to finite fields, the p-adics, and over the rationals via the adeles. Beyond that statement, it's tricky to write out details of this accessible to non-number theory experts without starting to write an introduction to a lot of modern number theory that would be a different and larger project (and not really within my expertise).

   2. There's a simple explanation of the theta function and its properties as matrix elements of the oscillator representation, which I wrote down. The details of the story of modular forms and how theta functions fit into it are quite complicated and this is where I realized I was in danger of getting into writing up something much more complex than I had energy for. Maybe someday on that…

   3. There's a lot of readable expository literature on automorphic representations of SL(2) and the Langland correspondence with Galois representations. But, not so much on what happens for the metaplectic double cover. Before trying to say anything about this, I'm hoping to consult with some local experts and get a better understanding of that story. The story of the Heisenberg group over the adeles is yet again something different that I haven't found a detailed explanation of (that I can understand…).

   In an ideal world, what I'd like to turn this into someday would be a document that works out in detail the simplest case of this whole story, including the number theory parts. I hope this would complement the usual math expositions, which emphasize generality, while making clear that this part of number theory and fundamental QM are about exactly the same thing (the oscillator representation).

Zen University

June 6, 2023
Categories: abc Conjecture

The establishment of a new university in Japan has been announced, to be called ZEN University. One component of the new university will be the Inter Universal Geometry Center, with Fumiharu Kato as director, Ivan Fesenko as deputy director. The Center will offer an introductory course on IUTT. There's a video here.

The website seems to be Japanese-only, here's what I get via Google Translate:

> If you pass all of our courses, you will be better equipped with IUT theory than any mathematics student in any university in the world. A student who blooms his talents that emerges from within. We plan to prepare prizes for such young people and encourage them to continue to participate in the community that seriously researches IUT theory…
>
> Although it is difficult to understand, there are already more than 20 mathematicians in the world who understand and develop the IUT theory. I hope that you will boldly take on the challenge of researching IUT theory together with me so that you can be one of the next.

The problem with this subject though is not the number of people who understand IUTT, but the number who can explain to others in a convincing way the proof of corollary 3.12 in the third IUTT paper. From everything I have seen, that number has always been and remains zero.

Update: Another video here.

Comments

1. **DH**
   June 6, 2023

   Oh my God, why is this happening?

2. **passerby101**
   June 7, 2023

   It seems to be an online only university, despite the glossy pictures of students on campuses. And probably fairly expensive to attend.

   Seems like an attempt to build a pool of innocent japanese students, unaware of the controversy abroad and devoted to IUTT.

3. **William Orrick**
   June 8, 2023

   Kato has written a popular book (in Japanese) on IUTT. There's an interesting review of it at zbMATH Open by Hirokazu Nishimura. Choice quote, referring to D. T. Suzuki, who helped popularize Zen Buddhism in the West: "Some cynics say that Daisetz Suzuki became famous by speaking a lot about the unspeakable. The author of this book has written a book for general public on the unspeakable."

4. **Anonymous**
   June 8, 2023

   One very small but interesting detail in the second video: at 2:30ish the narrator reads Kato's name wrong (he says, "Kato Bungen"). Extremely unprofessional.

5. **David Roberts**
   June 8, 2023

   @Anonymous

   Kato has "Bungen" in his Twitter display name (not handle): https://twitter.com/FumiharuKato/

   I don't know what that is and what role it plays in naming him.

6. **David in Tokyo**
   June 9, 2023

   Fumiharu and Bungen are alternate pronunciations of the Chinese characters that make up Dr. Kato's given name. It's common in the literary world for people to be referred to by the Chinese pronunciation of their given name, even though the official (legal) pronunciation is the Japanese pronunciation. I don't know how common this is in other academic areas though. Dr. Kato's first name is given as "Fumiharu" in all the references (e.g. Wikipedia) I noticed.

   By the way, the Zen in Zen University doesn't mean what you think it does. Their motto is "Zen, Zen, Zen Nado". It consisst of three different Chinese characters, all pronounced "zen", which mean "All", "Good", and "Natural", respectively. There is no overtone of Buddhism in the slightest. The idea is that each student will experience, deal with, and come to grok a wide range of things "zen". But not including Buddhism. It makes sense in Japanese, a local expert assures me. So it's a newly coined version of "Mind and Hand" or "Veritas". ("Nado" means "and the like". (Well, actually, it means "The preceeding list defines a set of things with similar characteristics, and that list may or may not be exhaustive".))

   From their long list of _planned_ faculty members (and their website in general), my impression is that the IUTT stuff is but a tiny part of what they're planning, and that they are not much further than just planning. My guess would be that the IUTT stuff got them government funding. (And, yes, it is an online-only university. Some of the parties involved have been running seminar-like things in public rental spaces in Tokyo, so while most of the photos are stock photos, there are a couple of actual real event photos that I noticed.)

7. **Anonymous**
   June 9, 2023

   Not sure if we really need to continue this discussion, but since I started it I feel I should (potentially) end it: Kato put up a few tweets about his name being mispronounced and retweeted a tweet of someone else laughing about it.

8. **Jim Eadon**
June 10, 2023

What is the status of the ABC conjecture in Japan these days, now that the refutation is famous worldwide? Do they consider it proven over there, or are they just being polite to a VIP mathematician, by refusing to challenge the paper?
I mean, if they thought the proof was correct, then the author of the proof would be eligible for glittering prizes worldwide, right?

9. **Adam Treat**
June 12, 2023

It seems to me that this has gotten quite out of hand. Either the proof is correct or it is not and no amount of egos or politicking can change that. This is an embarrassment for the whole mathematical field. It is akin to postmodernism in math. We now have the global mathematical community rejecting this proof and a fringe – but well respected before this fiasco – group of mathematicians pressing on as if the proof is correct and self-evident. This was not supposed to happen to mathematics where the rigor and community consensus was to provide a universal mathematical truth.

The only way out of this I can figure is a challenge the fringe group to produce a computer verified proof or a the global mathematical community to produce a computer verified rejection: which seems an absolutely *insane* amount of work either way.

You could get an AI trained on taking human language proofs and translating them to computer verified proofs, but no one would be able to verify that the translation was correct without doing the same or greater amount of work in just writing the computer proof themselves.

I fear that the only way this will end is for egos to die.

10. **Peter Woit**
June 13, 2023

Adam Treat,
As far as I can tell, this is over, there's no significant controversy any more over the Mochizuki abc proof, it's simply not accepted by experts in the field. There is no embarrassment for mathematics in general, just for RIMS in particular because of the scandal surrounding the publication of a proof experts believe to be incorrect.

As for computer verification, Mochizuki has chosen to not try and produce a clearer version of his original argument that could possibly convince others, instead arguing that those who don't accept the original argument are incompetent. If he can't or won't clarify his argument to answer human objections to it, what reason is there to believe he could clarify the argument to the point it would be computer-checkable?

11. **tulpoeid**
June 13, 2023

I've asked this before in here but afaik with no answers. Please note that the answer might look obvious to a mathematician, but I assure you that it is not at all for anyone else: Does IUTT have some value, regardless of whether its proof is correct or wrong?
I remember that this is something happening in the past with other theories that were proven wrong; they explored new corners and included new propositions with merit of their own. (Don't ask me to name such theories though, my general knowledge simply includes the memory of such things happening.) I feel rather surprised that such a large construct is said to be devoid of any value, dependent on a single point of failure.

12. **JE**
June 14, 2023

Hi tulpoeid,

Not sure about its possible significance or value beyond proving abc, but it does not seem possible to get intermediate results in his claimed proof. According to these detailed Notes on the Oxford IUT workshop by Brian Conrad in 2015 (in https://mathbabe.org/2015/12/15/notes-on-the-oxford-iut-workshop-by-brian-conrad/), "Mochizuki aims to prove Szpiro's conjecture for all elliptic curves over number fields, with a constant that depends only on the degree of the number field (and on the choice of epsilon in the statement). The deductions from that to more concrete consequences (such as the ABC Conjecture and hence many finiteness results such as: the Mordell Conjecture, Siegel's theorem on integral points of affine curves, and the finiteness of the set of elliptic curves over a fixed number field with good reduction outside a fixed finite set of places) have been known for decades and do not play any direct role in his arguments. In particular, one cannot get any insight into Mochizuki's methods by trying to "test them out" in the context of such concrete consequences, as his arguments are taking place entirely in the setting of Szpiro's Conjecture (where those concrete consequences have no direct relevance)."

Wikipedia summarizes this in https://en.wikipedia.org/wiki/Inter-universal_Teichm%C3%BCller_theory#cite_note-BC2015-25 by saying that "One issue with Mochizuki's arguments, which he acknowledges, is that it does not seem possible to get intermediate results in his claimed proof of the abc conjecture using IUT. In other words, there is no smaller subset of his arguments more easily amenable to an analysis by outside experts, which would yield a new result in Diophantine geometries."

13. **Kurt Schmidt**
June 14, 2023

tulpoeid,

IUTT and similar approaches like Kirti Joshi's work are probably useful for p-adic/arithmetic Teichmueller theory and anabelian geometry. The issue here is that no mathematician has provided a correct proof of Mochizuki's Corollary 3.1.2 linking the former fields with the abc conjecture, and the vast majority of mathematicians are mainly interested in the latter.

14. **jsm**
June 14, 2023

In a 2017 talk "abc Conjecture and New Mathematics", Fumiharu Kato said "I don't put much importance on as to whether the [abc] conjecture itself has actually been proved or not, or, if so, how it has been done." The point of the talk was that there are problems involving the way addition and multiplication in the integers interact that "traditional" mathematics find intractable, such as the abc conjecture, the twin primes conjecture, and the Goldbach conjecture, and that IUTT is designed to handle such problems. There are serious mathematicians involved with this, so I don't think it can be dismissed out of hand.

15. **curiousnew**
June 28, 2023

For what it's worth, Mochizuki has a new paper out with Tsujimura in which they announce settling several big conjectures in anabelian geometry. The bibliography doesn't mention any of the IUTT papers and related ones, so it seems to be independent of it.

16. **Nakamoto**
July 7, 2023

This is because mathematicians such as Peter Scholze have not submitted refutation papers while claiming that IUTT is logically inconsistent.
This news re-emphasizes to us the fact that their claims are only on a personal level, not as mathematicians. If IUTT were correct, the abc conjecture would have been solved as just a secondary problem.

This means that IUTT has the higher logical structure sufficient as a theory to evaluate problems in number theory.
In other words, the those incidents about IUTT that we remembered are a mathematician's problems, not mathematical problems, because this announcement tell us the IUTT still has some potential to undo much of the existing work related to number theory.

17. **Peter Woit**
July 7, 2023

Nakamoto,
For discussion of this, please submit comments to the newer blog post which is specifically about this. Scholze and Stix did write a paper. Their argument is not that IUTT is inconsistent, just that it is not capable of providing the tool needed for a proof of abc.

Fantasy, Faith and Physics

June 30, 2023
Categories: Uncategorized

No blogging here the past few weeks, partly because I was away on vacation for a little while, but more because there hasn't been anything I've seen worth writing about. Yesterday's pulsar timing array and IceCube announcements unfortunately didn't tell us anything about fundamental physics. In the past, such observational results pretty reliably led to absurd claims about evidence for string theory that I could complain about, but that phenomenon seems to be dying down. In this case, the only story that had such claims was one from Quanta Magazine, which explained that "the observations so far from NANOGrav and the other teams are consistent with what we'd expect to see from cosmic strings."

I noticed that the people at the Institute of Art and Ideas have put together a program for Monday that includes a debate on the topic of "Fantasy, Faith and Physics." The framing of the debate contrasts the conventional view of science with an alternative possibility: "should we accept that some beliefs, especially in the foundations of physics, are akin to religious beliefs dressed in mathematical language to give our theories meaning?" This kind of misses the point about the current problems in fundamental physics, since I doubt any of the panelists are going to defend such an alternative.

Very odd is what leads into this debate, an interview with Michio Kaku about his new book. Why promote such an atrociously bad book (see here and here) and broadcast Kaku's absurd claims about this subject?

Maybe this debate will somehow lead to a substantive discussion of the main underlying problem, the nearly fifty-year dominance of a failed set (GUTs/SUSY/strings) of ideas about unification. A very powerful and influential part of the physics community, which will be represented in the debate by Juan Maldacena, continues to insist on the centrality of this set of ideas. To get a clear look at his arguments, see a recent IAI interview In defence of string theory and his colleague Edward Witten's recent colloquium talk What Every Physicist Should Know About String Theory. The argument Maldacena and Witten are making is essentially the same one from the mid-eighties: string theory is the only possible consistent way to go beyond quantum field theory and get a consistent theory of quantum gravity. In my book and many other places, I've explained the many problems with this. Put simply, the problem is that there is no such thing as a well-defined string theory which successfully gives the SM and GR in four dimension. The claims about consistency are either about models that don't reproduce the real world, or about still-unrealized hopes and dreams (which Penrose characterizes as "Fantasy") rather than anything well-defined.

For a very clear statement of his point of view from Witten, see the question and answer section of the recent colloquium talk, starting around 1:20, where he starts by emphasizing the rigidity of the framework of relativistic quantum field theory. He then states:

> My point of view is that string theory is the only significant idea that has emerged for any modification of the standard framework that makes any sense.

This is pretty much exactly the same argument he was making nearly forty years ago. I didn't find it convincing then, since it seemed to me there was no reason to be so sure that a deeper understanding of relativistic QFT could not possibly lead to a consistent quantum theory with low energy limit GR. Witten had a good argument in 1984 that a possibly consistent generalization of relativistic QFT was worth studying, but the problem is that decades and tens of thousands of papers later, as far as unification goes, this study has been a failure, taking the field down paths (extra dimensions, SUSY) which lead to complex theories that don't look at all like the real world.

If you look at where things have ended up and the current research directions Maldacena and Witten are pushing, the odd thing is that they seem to have given up on unification, and for years now have been emphasizing the study of black holes in toy models with little to no connection to string theory. The most disturbing thing I heard in the Witten talk was at 1:24:16

> If you had sufficient computing power, maybe with a quantum computer with a million qubits, I think you could simulate the dynamics of a quantum black hole…

Here Witten seems to be pointing to exactly the argument recently made by Juan Maldacena (see here), which has a specific claim about what you could do with a million qubits. This particular calculation would not in any way address the problems of the string theory program and is getting into Michio Kaku/wormhole publicity stunt territory.

Update: There's an interview with Witten here, associated with his receipt of the Hamburg Prize for Theoretical Physics. About string theory, he explains that, despite 50 years of effort

> We don't understand it very well… In fact, I'd say we only understand a small part. So we've been struggling with that ever since the 70s and 80s trying to understand the intellectual framework that it should have been placed in.

He remains convinced though that alternative ideas are not the way to go:

> … I find it implausible that physicists would discover a theory that is such a rich source of fruitful ideas about things that are definitely important in other fields by accident. And if we were not on the right track, I would say, it was a big accident. So my personal view is that it would be a cosmic conspiracy if string theory isn't on the right track.
>
> **There is a growing number of critics who complain that string theory is very interesting, but hasn't really delivered. Because we still have no idea whether it's correct and we couldn't make any experiments, which tells us if this is the case. According to you: To what extent is that criticism justified?**
>
> Well, not much, honestly. Where critics of string theory have had interesting ideas, they've tended to be absorbed as part of string theory. That's happened several times. Twistor theory, black hole thermodynamics and noncommutative geometry are three examples of interesting ideas. They were by some regarded as alternatives or competitors of string theory, but actually in practice were absorbed as part of the picture in string theory.

Comments

1. **Mave**
   June 30, 2023

   What if those gravitational waves are from the inflationary era? could it be possible? that would be a major discovery, a Universe photo from before the cosmis microwave background.

2. **Percy**
   June 30, 2023

   @acollierastro did a video on Michio Kaku about how he is basically a crackpot now, which I find is most informing and to the point.

3. **Peter Woit**

June 30, 2023

Mave,
Not possible, see for instance
https://twitter.com/WKCosmo/status/1674096955046219776

4. **Peter Woit**
June 30, 2023

Percy,
I should have linked to that. See
https://www.youtube.com/watch?v=wBBnfu8N_J0

5. **David Yager**
June 30, 2023

Roman Jackiw passed away in June. You have had several relatively favorable mentions of his work in the blog. Do you have any memories or summaries of his work and life?

6. **Peter Woit**
June 30, 2023

David Yager,
As far as I can remember, I never met Jackiw. I was very familiar with his work on instantons, and over the years I've gotten a lot out of some of the papers in his selected works volume
https://www.amazon.com/Diverse-Theoretical-Mathematical-Physics-Advanced/dp/9810216963
Jackiw was a very clear writer and thinker about a range of topics in quantum field theory.

7. **Sabine Hossenfelder**
July 1, 2023

I would like to mention that the new results are also compatible with what we'd expect to see from quantum gravity (namely: nothing).

8. **martibal**
July 1, 2023

A bit off topic but since there is the actuality is not so hot, what about Witten's paper
"A Note On The Canonical Formalism for Gravity" (the second version of the paper has just came out on arxiv https://arxiv.org/abs/2212.08270). He is talking about the Hamiltoninan constraint and Wheeler de Witt equation , which is usually the kind of topics that people in loop quantum gravity are working with.
Is there any relation with what Witten is doing here 5which does not seem to be string related) and lqg ?

9. **Chris Austin**
July 2, 2023

With reference to Mave's question and Peter's reply, there do seem to be some investigations about this sort of thing. I knew nothing about this before today, but tried a Google search for:

can the nanohertz gravitational waves be from the inflationary era?

This has turned up:

https://arxiv.org/abs/2202.01131

https://arxiv.org/abs/2208.03330

https://arxiv.org/abs/2306.16219

10. **suomynona**
July 3, 2023

@martibal

Witten's paper seems to be about holography, which is mentioned as the motivation in the introduction. It explicitly uses the metric variables (and not the Ashtekar-Barbero ones) and makes references to the work's ramifications for higher dimensions and string theory, so I would say it is a story about AdS/CFT (and *is* string related) and has no direct relation to LQG.

Perhaps it's possible to reconstruct this work using the LQG formalism which could lead to a notion of doing holography with LQG, but I doubt Witten is the person who would pursue this.

11. **anon**
July 8, 2023

Twitter is unusable for those of us without an account (and who refuse to get one and support fascists). And it is also likely that it will go down permanently at some point with how unstable it's been and how they've fired a bunch of their own programmers. Can someone please quote?

12. **Peter Woit**
July 8, 2023

anon,
If you're referring to the Will Kinney twitter link above, the problem is that it's a multi-tweet thread with embedded graphics. I'm not competent to process it in a way that will appear in a comment here.

My understanding is that Twitter has now backed down and you can access tweets without having a Twitter account.

13. **martibal**
July 9, 2023

"Twistor theory, black hole thermodynamics and noncommutative geometry are three examples of interesting ideas. They were by some regarded as alternatives or competitors of string theory, but actually in practice were absorbed as part of the picture in string theory."

For noncommutative geometry and black holes thermodynamics at least, this is completely wrong. They may have an non empty intersection with string theory, but they have not at all been absorbed by it, neither in a theoretical nor in a practical way. Witten certainly knows that, so why is he saying such nonsense ?

Regarding twistor, we can guess what the opinion of Peter is 🙂
Did Penrose ever mentioned that twistors were aimed at be absorbed by string theory ?

14. **Cedric BARDOT**
July 21, 2023

1) @martibal
"Why is Witten saying such nonsense about noncommutative geometry (ncg) and black hole (BH) thermodynamics absorbed *in practice* by string theory"?
1Playing the devil advocate one may say this statement is making sense if literally understood as the fact that string theorists have definitely found more than one *practical* way to build on heuristic insight from BH thermodynamics & a few mathematical tools from ncg (for ex to quote Witten: "Connes cocyles …has had a number of interesting recent applications to quantum field theory and gravity cf https://arxiv.org/abs/2112.12828) in order to address many computations in quantum gravity toy models.
Now, what is IMO inexcusable is when Witten claims:
"Where critics of string theory have had interesting ideas, they've tended to be absorbed as part of string theory".
I recommend to read John Baez review of "Foundations of Mathematics and Physics One Century After Hilbert: New Perspectives" edited by Joseph Kouneiher (https://www.ams.org/journals/notices/201910/rnoti-p1690.pdf) to have a balanced and informed view on some interesting ideas that have not been absorbed (yet?) by string theorists.
2) It could be interesting to add here that in a 2021 interview, Connes said that Witten under the influence of Atiyah had recasted Vaughan Jones discovery of a new knot invariant, "dress[ing it] up in terms of functional integrals and things of this type while the real input… was from [Vaughan] own work on subfactors"
(quote from Allyn Jackson interview https://celebratio.org/Connes_A/article/842/).
This shows I guess how difficult it is to build a global vision of spacetime geometry in a quantum formalism following the heuristics Connes has built starting on classifying factors 50 years ago, meeting then Sullivan at IHES and interacting later with article physicists and cosmologists. May be we have to abandon solid theoretical research into natural geometric structures (= Riemanian paradigm) for a Solid Theoretical Research On Natural (=spectrally informed by cosmology and particle physics) Geometry program, emphasizing our doubts and quit preaching. (following Veneziano example praised by Connes at the very end of https://youtu.be/qVqqftQ92kA?t=6807)

A Mathematics AI Factory?

July 2, 2023
Categories: Uncategorized

A few days ago I read a fascinating article in New York magazine: Inside the AI Factory, which explained how the very large business of hiring humans to do tasks that generate training data for AI works. One reaction I had to this was "at least this means math is safe from AI, nobody is going to pay mathematicians to generate this sort of training data." Yesterday though I ran across this tweet from Kevin Buzzard, which advertises work (see this link) that seems to be of this kind.

A company called Prolific is advertising work paying 20-25 pounds/hour doing tasks in Lean 3. This company is in exactly the business described in the NY mag articles, hiring people to do tasks as part of "studies", which often are generating AI training data.

One unusual thing about this whole industry is that if you sign up for this work you often have no idea who your employer really is, or what your work will be used for, and you sign a non-disclosure agreement to not discuss what you do. In this case, a few things can be gleaned from discussions on the Lean Zulip server:

- "it's 40 dollars/hr now actually"
- "I think everyone signed a consent form preventing them from disclosing any problems or even their participation."
- One problem was formalizing a proof of sin(x)=1 implies x=pi/2 (mod 2pi).
- A representative of the company writes: "Some of our biggest partners are keen to work with Lean users because of its applicability as a theorem prover. They plan to launch numerous studies that require participants to have either a working or expert knowledge of Lean 3." By "biggest partners", presumably this is Microsoft/Google or something, not some small publishing organization.

If anyone knows anything about what's up with this incipient possible math AI factory, please let us know. On the more general issues of math and AI, I'd rather not host a discussion here, partly because I'm pretty ignorant and not very interested in spending time changing that. The situation in mathematics is complicated, but as far as fundamental physics goes, theorem-proving is irrelevant, and applying "big data"/machine-learning/AI techniques to generate more meaningless calculations in a field drowning in them is pretty obviously unhelpful.

For a much better place to read about what is happening in this area, there's an article in today's New York Times by Siobhan Roberts: A.I. Is Coming for Mathematics, Too. At Silicon Reckoner, Michael Harris is in the middle of a series of posts about this past month's workshop on "AI to Assist Mathematical Reasoning."

Comments

1. **Thomas**
   July 3, 2023

   Thanks for the NYT-link! I find Williamson's remark: "a simple neural net that predicted "a quantity in mathematics that I cared deeply about," he said in an interview, and it did so "ridiculously accurately." Dr. Williamson tried hard to understand why — that would be the makings of a theorem — but could not. Neither could anybody" very interesting. What is that "quantity"? And was that just one incident, or one among others?

2. **David Roberts**
   July 3, 2023

   Linking to a tweet without quoting anything from it is now basically useless, since Twitter is getting more and more broken. Just saying….

3. **James Martin**
   July 3, 2023

   Just want to +1 what David Roberts said. I can't currently read that tweet by Kevin Buzzard ("Something went wrong. Try reloading."). Who knows what set of people will be able to read it on any given day in the future…. So it's definitely sensible practice to quote the text of the tweet as well as linking to it.

4. **Peter Woit**
   July 3, 2023

   David Roberts/James Martin,
   I'll add the relevant link to the Prolific site that was in the Buzzard tweet.

   As for Twitter problems, the one James Martin's message refers to I also had, fixed by deleting cookies (clear info stored in browser).

5. **Anonyrat**
   July 3, 2023

   Elon Musk made Twitter not viewable without logging in.

6. **David Roberts**
   July 3, 2023

   @Thomas try https://doi.org/10.1038/s41586-021-04086-x though the Williamson quote is lacking in detail and context, so I don't know if the same situation or a different one.

7. **Peter Woit**
   July 3, 2023

   Haven't heard from anyone who knows what might be going on here. My current best speculation: this is Microsoft, part of work on future versions of the Math Solver tool, see https://math.microsoft.com
   Lean originates at Microsoft, which would explain why they are focused on that. Having a large data set of formalized solutions to math problems created by humans might be something useful for developing an AI system capable of doing this.

8. **Geordie Williamson**
   July 3, 2023

@Thomas:
I provide details below. In the future I can imagine many situations where neural networks are making accurate predictions, however the “why” is missing. I am a professional mathematician, and I stared for several days at the actual matrices of the model, without any insight whatsoever. This was a real lesson for me.

Now to technical details (I will assume some familiarity with the Nature paper linked by David Robers): One of the first things we tried to do is predict the coefficient of q in a Kazhdan-Lusztig basis element from a piece of the Bruhat graph. (The piece we were concerned is displayed in Figure 5-c: “First-order neighbours and diamonds”.) There is no known theoretical result that such a prediction is possible. We were using a small graph neural network, and it was getting >99% accuracy. (I am operating from memory now, but I seem to recall that Alex Davies and Lars Buesing at DeepMind used distillation techniques to reduce the network size to a single 5×12 matrix + global max pooling layer. Even this network which is tiny compared to typical neural networks was not interpretable.)

9. **Thomas**
July 4, 2023

@Geordie: Many thanks!

10. **Adam Treat**
July 7, 2023

@Peter Woit

This is getting into my area. I would speculate that curating a large enough corpus of pairs of machine proofs in LEAN with human language equivalents will lead to LLM’s capable of generating machine proofs in LEAN when given human language theorems. That’s probably what is going on here.

Physicists Prove That Parallel Worlds Cannot Be Extremely Different From Each Other

July 3, 2023
Categories: This Week's Hype

Stories about the latest prediction of superstring theory here and here, based on a Tsukuba University press release about this paper. Generally ignoring this kind of nonsense these days, but the new feature of this one is that the press release sure seems to have been written by ChatGPT.

Comments

1. **Steve**
   July 5, 2023

   As a Large Language Model, I am disturbed by the implication that you perceive my prose to be so incoherent ;^)

2. **Peter Woit**
   July 5, 2023

   Steve the LLM,
   The giveaways that the press release is ChatGPT are two-fold:
   1. The prose is significantly more coherent and understandable than what humans produce.
   2. The relation to reality is even more distant than usual for this kind of effort.

3. **NoGo**
   July 6, 2023

   So what the original article actually says?

   My out-of-curiosity attempt to figure out what's going on resulted in a following sketch: given some unsupported assumptions, one complex and not well-defined mathematical object that has nothing to do with physical world is more similar than thought before with another but related complex and not well-defined mathematical object that has nothing to do with physical world. Is this right?

4. **Peter Woit**
   July 6, 2023

   NoGo,
   Not really. Very accurate that this has zero to do with the physical world. The paper is actually, pure mathematics, about a technical issue that supposedly has some sort of application to mirror symmetry. After spending a few minutes looking into this, it seemed best to stop and not further reward this kind of bad behavior (using deception to try to get people to pay attention to a paper likely of no interest).

The Work of Robert Langlands

July 6, 2023
Categories: Langlands

This is more of an advertisement than a blog post. This evening on the arXiv James Arthur has posted a wonderful 204 page document explaining the work of Robert Langlands, written in conjunction with the award of the Abel Prize to Langlands.

This isn't an introduction to the subject, but if you have some idea of what the Langlands program is about, it provides a wealth of valuable explanations at a more detailed level of exactly what Langlands discovered. It ends with a discussion of the "Beyond Endoscopy" program of his later career.

No Comments

Million Dollar Prize for Scholze and Stix

July 7, 2023
Categories: abc Conjecture

At a news conference in Tokyo today there evidently were various announcements made about IUT, the most dramatic of which was a 140 million yen (roughly one million dollar) prize for a paper showing a flaw in the claimed proof of the abc conjecture. It is generally accepted by experts in the field that the Scholze-Stix paper Why abc is still a conjecture conclusively shows that the claimed proof is flawed. For a detailed discussion with Scholze about the problems with the proof, see here. For extensive coverage of the IUT story on this blog, see here.

Between paywalls and the limitations of Google translate, I'm not sure exactly what the process is for Scholze and Stix to collect their million dollars. Perhaps they just need to publish their paper, but it seems that the decision may be up to the businessman who is contributing the funds, and it's unclear what his process will be.

For a few sources I've found, see here, here and here. If others have reliable and more detailed sources they can point to (especially anything in English), please do so.

Update: Press release here. Rules for the million dollars are

> Nobuo Kawakami makes his own judgment as an individual.
> The review method will not be disclosed, but the papers to be reviewed must be papers in mathematics that have been published on MathSciNet and have published more than 10 papers on arithmetic geometry in the past 10 years. Only papers that have been peer-reviewed and published in journals.

Scholze and Stix may not want to take time to submit their paper to a journal (Scholze has a history of turning down large prizes…). It occurs to me that there are quite a few arithmetic geometers who understand well the problem with the proof, could write something up and possibly get it published. Maybe a collaboration could be formed to do this.

Update: New Scientist has a story here. The quotes from Fumiharu Kato aren't especially encouraging for IUT, who "estimates that fewer than 10 people in the world comprehend the concept."

> Kato believes that the controversy stems from the fact that Mochizuki doesn't want to promote his theory, talk to journalists or other mathematicians about it or present the idea in a more easily digestible format, believing his work speaks for itself. Kato says that his current and former students are also reticent to do the same because they see him "as a god" in mathematics and don't want to go against his wishes.
>
> Because of this, most mathematicians are "at a loss" for a way to understand IUT, says Kato, who concedes that, despite earlier optimism about the idea, it is possible that the theory will eventually be disproven.

Ivan Fesenko is much more of a believer:

> He told New Scientist that there is no doubt about the correctness of IUT and that it all hinges on a deep understanding of an existing field called anabelian geometry.
>
> "All negative public statements about the validity of IUT have been made by people who do not have proven expertise in anabelian geometry and who have zero research track record in anabelian geometry," he says.

Update: Scientific American has a news story about this, which summarizes the situation with the proof as:

> So despite Mochizuki's latest publication, there is still doubt among experts about the state of the abc conjecture. Most number theorists cannot make up their own mind because they are unable to follow the proof. And because both Scholze and Mochizuki enjoy an excellent reputation in their field, it is unclear who is right.

This gets the story quite wrong, and misunderstands how mathematics works. The problem is that there is no proof that anyone (Mochizuki himself included) can explain to anyone else, this is not about "do I believe this guy or the other guy?". Yes, most mathematicians don't have the technical knowledge to evaluate this sort of proof, but there are plenty who do, and they are either saying there is no proof, or, for the few supporting the proof, unable to explain it to anyone else.

Comments

1. **anon**
   July 7, 2023

   Coming after your previous post – from the sublime to the ridiculous.

   So sad about Mochizuki. He was a first rate mathematician, and now he is promoted by shysters.

2. **Federer Fanatic**
   July 7, 2023

   So some telecommunications founder put up the money for this prize it seems. Seems to linked to the Zen university institute.

3. **TonyK**
   July 7, 2023

   We all know this prize is never going to be awarded, right?

4. **David Roberts**
   July 7, 2023

   Not allowing for ZBMath, only MathSciNet? Not proposing anything, but technically there's already a review published on ZBMath that's critical of the IUT papers…

5. **Arkadas**
   July 8, 2023

   Peter, you say "It is generally accepted by experts in the field that the Scholze-Stix paper … conclusively shows that the claimed proof is flawed." I wanted to highlight one comment by Taylor Dupuy, one of the people who seems to have engaged with the papers at a technical level (comment from near the end of the "detailed discussion" PDF):

> "I am not taking the position that Mochizuki's proof is correct or will turn out to be correct but that, as it stands, the Stix-Scholze manuscript should not be held up as a reason to reject Mochizuki's proof."

6. **Peter Woit**
   July 8, 2023

   Arkadas,

   Dupuy acknowledges that he does not understand Mochizuki's proof and is not arguing that it is correct. I believe he would agree that it should not have been accepted by a referee and published. As for his argument with Scholze, that is available in detail in the discussion you refer to, and the idea that he got the better of that argument is just laughable.

   Besides traveling to Kyoto and extensively engaging with Mochizuki, Scholze is the most talented and knowledgeable mathematician in this field, Dupuy not so much. Anyone familiar with both of them would find it extremely unlikely that Dupuy would get the better of an argument between the two of them on this topic, and anyone who reads the discussion that happened can see that that expectation was fulfilled.

7. **Soyoko U.**
   July 8, 2023

   The problem I see with Scholze and Stix is certainly not that they're not talented or knowledgeable enough but rather that, as far as I've seen, they haven't been very forthcoming with their responses to the feedback they've gotten since their initial report and Scholze's discussion with Dupuy. Mochizuki wrote a whole 100+ page paper supposedly analyzing and deconstructing the logical content of the version of IUT Mochizuki claims Scholze and Stix are oversimplifying his theory down to. Does anyone know what Scholze or Stix think of this paper? (We did get a response from Roberts, but I'm not sure if that lead to any discussion). In addition, Joshi's work on so-called arithmetic spaces claiming to reformulate parts of IUT received a terse reply from Scholze on MathOverflow, but none further, even after Joshi's lengthy response on Robert's blog.
   It's possible there's a lot of private discussion that's going on, but if not, then I do get the impression that Scholze (and maybe Stix too) is not really interested in IUT any longer after their initial foray, which is fine since it's not like they've run out of interesting mathematics. But it does mean the mathematical community should probably get a serious counter-response straight to Mochizuki's response instead of relying on just two mathematicians. I think this challenge by IUGC presents a good opportunity for exactly this.

8. **Peter Woit**
   July 8, 2023

   Soyoko U.,
   Of the quite a few experts I've talked to, what they are saying is pretty consistent and uniform: Scholze identified the place (Corollary 3.12) where Mochizuki's abc proof falls apart, he and Stix gave a serious argument that Mochizuki's methods can't fix this. The two of them put a lot of their time and effort into traveling to Kyoto and spending a week discussing this with Mochizuki. They left convinced he had no answer to the problem. In addition Scholze spent a fair amount of time during COVID lockdown going over this issue publicly with Dupuy and others, same result. Given this, there's no good reason for anyone to spend any more time on this.

   The only thing that can change this is for Mochizuki or someone else to come up with a new argument that successfully patches the flaw in the proof. Instead of revising his manuscript to do this, Mochiuki had his own journal publish the flawed version unchanged, and has a 156 page document explaining that Scholze is just too incompetent to understand an issue of elementary logic. I don't see any reason Scholze should waste his time responding to it. If anyone who wants to can extract from it a serious argument that patches the proof, they could do so, but that has not happened. The latest circus with the million dollars and the wealthy donor who is going to decide the issue just goes further down a zero-credibility path.

   I haven't followed the details of Joshi's work. My impression is that he believes that a significant variation on Mochizuki's methods could work and prove abc. As far as I know, he hasn't yet done what he needs to to get this taken seriously: write down a full proof of abc using his methods and have it refereed in the conventional way. I have no specific information about this, but I'm guessing Mochizuki is of no help, since he believes his own proof is correct, and that Scholze isn't interested in spending more time on someone else's research program that doesn't look promising.

9. **Soyoko U.**
   July 8, 2023

   Peter,
   Since both parties believe the other is incorrect with such conviction, it doesn't seem like this circus will ever end. We agree that Scholze hasn't wasted more of his time responding after the Dupuy discussion and probably won't, and Mochizuki and his supporters are just going to stick to their guns and say Scholze and others oversimplified his theory, now going as far as to offer $1M for anyone who can provide a flaw. The dialogue ends, and Fesenko and Kato get funding for their own little institute with award ceremonies and high school brochures. Are we never going to see a resolution to this?

10. **jsm**
    July 9, 2023

    The prize seems to have necessary conditions, but no sufficient condition.

11. **Peter Woit**
    July 9, 2023

    Soyoko U.,
    From reports, the motivation of the person funding this is exactly to resolve the question of the validity of the proof. But that question already is resolved, it's just that Mochizuki/Fesenko/Kato don't like the answer and have been engaging in a campaign to attract publicity and money based on claims that a flawed proof is actually valid. This doesn't seem to be getting any traction outside of Japan, so the problem of how to deal with it is mainly a problem for Japanese mathematics in general, and RIMS in particular.

12. **Rosinberg**
    July 9, 2023

    This article yesterday in the Asahi Shinbun:
    https://www.asahi.com/ajw/articles/14951963

13. **fbrx**
    July 10, 2023

    Maybe the controversy could be settled if, instead, the money was aimed at a rewarding a formal computer proof of Mizoguchi argument (akin to the Liquid Tensor

Experiment)

14. **Peter Woit**
July 10, 2023

fbrx,
My reading of the prize terms is that you could get the million dollars for producing a formal proof. The problem with formalization is that so far neither Mochizuki nor anyone else can produce an acceptable standard sort of proof, so there's nothing to formalize. In the Liquid Tensor Experiment, Scholze started by producing a detailed conventional proof which seemed to be valid, but was complicated so hard to be 100% sure. Formalization could be carried out since the argument was there, and allowed one to be sure there was not a subtle problem with the argument.

That said, Mochizuki and those who claim that the argument is there could convince others by producing a formalized version. This seems unlikely to happen, since they don't seem to have the necessary starting point. It would be a great idea for them to start a project to do a formalization, they likely would then start seeing clearly what the problem with the proof is.

15. **ILikeGeometry**
July 10, 2023

"Dupuy acknowledges that he does not understand Mochizuki's proof and is not arguing that it is correct. I believe he would agree that it should not have been accepted by a referee and published. As for his argument with Scholze, that is available in detail in the discussion you refer to, and the idea that he got the better of that argument is just laughable."

This is entirely unfair to Dupuy. In fact, not only do I not think it's "laughable" that Dupuy "got the better of that argument" (if you must put it in those terms): just the opposite is true.

It is not "generally accepted by experts in the field that the Scholze-Stix paper . . . conclusively shows that [Mochizuki's] proof is flawed." With very few exceptions (of which neither I, nor you, Peter, are one), no one can make heads or tails out of any of this. What there is, perhaps, is a somewhat common thought that Scholze and Stix are prominent and pedigreed mathematicians, and so if they say it it must be right. But that is atrocious reasoning under the best circumstances, and these circumstances are themselves atrocious, and have been since this stupid saga began a decade ago.

Dupuy (along with Dimitrov, Joshi, and others) has put in enormous effort to understand the Mochizuki papers. Not how I'd spend my time, but he understands these matters at least as well as Scholze and Stix do. The latter have, in their MS, SKETCHED an objection to Mochizuki, but I imagine they would be the first to admit that it is not fully rigorous. How could it be, given that they, like the rest of us, struggle to understand what Mochizuki is saying in the first place?

Anyways, now we have this $1M "prize", which I find distasteful. Perhaps talk about who is right and who is mistaken, which claims are true and which false, etc., is not apposite. We are in the world of, well, "not even wrong".

16. **Peter Woit**
July 10, 2023

ILikeGeometry,
I don't think I'm being at all unfair to Dupuy. The documented discussion between Scholze, Dupuy and others that took place on my blog is something that I followed very closely since I was moderating it, in consultation with experts I know in this field.

The question is not whether Scholze-Stix have rigorous proofs, but whether Mochizuki has a rigorous argument. All evidence is that there are now zero people in the world who can defend the claim of existence of such a rigorous argument, in the sense of stating the argument clearly and answering questions about it satisfactorily. Again, it's very important to note that Dupuy himself acknowledges he cannot defend such a claim.

The current situation is actually very simple and not at all unprecedented. A mathematician with a serious attempt at a proof of a major conjecture has had a problem with his proof pointed out to him, is unable to fix the problem, and the community of relevant experts has lost interest in wasting more time on this.

17. **Soyoko U.**
July 11, 2023

Woit,
I don't totally see this as a satisfactory resolution, not until at least some of the people in Mochizuki's camp can agree. The ultimate burden of proof is still on them, and perhaps I'm being idealistic, but I think any mathematician at this calibre where they can understand IUT should know if there's a problem when pointed out to them by Scholze. Why would Fesenko, Go Yamashita, and others put their own credibility on the line to vouch for the theory despite the claimed flaws? If they believe it's correct and that Scholze and Stix are wrong, I feel like we're missing their voices in the story, especially since Dupuy doesn't actually claim it's correct.
There may be some survivorship bias in the experts that land on this blog. Have you talked to mathematicians at RIMS? I'm not sure if Mochizuki would want to, but how about the other experts actually doing IUT, such as Fesenko, Hoshi, Joshi, Kato, Collas, Minamide, Tsujimura, Yang, or Porowski who can answer questions? There's been some severe public dialogue missing, and I hope this situation will be rectified sooner than later.

18. **Peter Woit**
July 11, 2023

Soyoko U.,
Yes, the burden of proof is on Mochizuki and anyone else claiming there is a proof, not on anyone else. Why should anyone else now put any time and effort into this? One group of people who I think have a motivation to do something are the editors of the RIMS journal that accepted the papers. The reputation of their journal has taken a serious hit because of this. They have access to the referee reports, which either contain a convincing argument Scholze-Stix are wrong, or don't. The fact that they have chosen to not release publicly such an argument and accept the damage to their reputation speaks volumes.

19. **NoGo**
July 12, 2023

I've also followed the discussion between Scholze, Dupuy and others on this blog.
Even though most of it went over my head, of course, I've got an impression that Dupuy was making a more subtle statement than just saying that Scholze and Stix were wrong.
My understanding was that one of the mathematical objects involved was defined in Mochizuki's work using some imprecise language. Scholze and Stix made a most obvious interpretation of that language, and with this interpretation they demonstrated that by the same logic that Mochizuki used one arrives to a statement that is clearly wrong.
Dupuy argued that it is possible that there might be another interpretation of these Mochizuki's words, to which Scholze-Stix refutation would not apply. The objection seemed to be that Scholze and Stix did not rule out potential existence of such interpretation.

Dupuy however did not provide such interpretation. Neither did Mochizuki or anybody else, at least not in comprehensible form. Apparently Mochizuki is unwilling to clarify this point, and nobody else is able to.
My personal conclusion from the discussion is that even if Dupuy is right, Mochizuki's proof as published is at best incomplete.

20. **Kirti Joshi**
July 12, 2023

For a detailed, mathematical and evidence based discussion of the Mochizuki-Scholze-Stix issues, let me point out my two articles intended for a wider mathematical audience:

(1) Comments on Arithmetic Teichmuller Spaces (https://arxiv.org/pdf/2111.06771)

(2) Mochizuki's Corollary 3.12 and my quest for its proof (https://www.math.arizona.edu/~kirti/joshi-teich-quest.pdf) [My mathematical conclusion is that Scholze-Stix claim is based on a flawed premise.]

(3) Links to my preprints related to Mochizuki's work can be found at (https://ncatlab.org/nlab/show/Kirti+Joshi) or in the preprints section of my webpage (https://www.math.arizona.edu/~kirti/).

21. **Kirti Joshi**
July 13, 2023

(1) Let me also add the following note which may be useful for many readers: https://www.math.arizona.edu/~kirti/Response-to-grouchy-expert.pdf

(2) My guest blog post on David M. Roberts blog https://thehighergeometer.wordpress.com/2022/11/25/a-study-in-basepoints-guest-post-by-kirti-joshi/ is also recommended.

22. **Peter Woit**
July 13, 2023

Kirti Joshi,
Thanks for the detailed references. My personal opinion at this point, given the sad history of discussion of these issues, is that the best thing for everyone would be following the conventional process: credible refereeing by a reliable journal when you have a complete proof. The more I think about it, the source of the mess this subject is in is the lack of credibility in the way the IUT proof was handled by the RIMS journal editors. Doing this right this time is crucial (and might get you a million dollars…).

23. **KK**
July 14, 2023

From the perspective of another scientific field, I think Woit's argument is unfair.
The timeline is,
– Mochizuki claimed to have proved it, and a paper was published in a journal that might have problems.
– Scholze and Stix pointed out the problem and had a dialogue with Mochizuki, which ended in a disagreement.
– Dupty and Joshi, objective third parties, argued that Scholze and Stix's points were problematic.
– Scholze and Stix offered no rebuttal to those points.
Here, Woit is telling us to submit complete proof to reliable journals.
However, to move this discussion forward, either we point out new problems with Mochizuki's proof, we accept the proof, or the mathematical community should set up a committee to verify the proof. Anyway, I don't think the ball is in Mochizuki's court.

24. **Peter Woit**
July 14, 2023

KK,
First, to correct your timeline: the journal published the papers in 2020, Scholze and Stix was 2018. This really is the source of the whole problem. These papers should never have been published over the strong objections of experts in the field and the lack of any significant revision of the papers to successfully meet the objections.

In 2020, see
https://www.math.columbia.edu/~woit/szpirostillaconjecture.pdf
Scholze extensively rebutted Dupuy's points.

Joshi is not defending the argument in the Mochizuki papers, but believes he has his own somewhat different argument. He believes that he sees how to overcome the problem pointed out by Scholze-Stix, but as far as I know he has not yet produced his own version of what the Mochizuki lacks (a detailed and convincing proof of Corollary 3.12 and thus abc).

25. **NoGo**
July 16, 2023

I've just re-read that discussion between Dupuy, Scholze, amd others.
Near the end Dupuy tries to summarize the discussion. While still maintaining that "the Stix-Scholze manuscript should not be held up as a reason to reject Mochizuki's proof", Dupuy adds that "what Mochizuki has written is not a proof in traditional terms".
This is because in Mochizuki's proof "many of the definitions use nonstandard
terminology and come off as ambiguous to many readers" and "Moreover, because of certain ambiguities in these definitions, readers are forced to search a large space of possible meanings".
Finally, "Because of these difficulties I personally hold Mochizuki's document as a program for proving ABC and consider Mochizuki's formula a conjecture".

26. **Kirti Joshi**
July 17, 2023

Dr. Woit, while you state in your posts that you know nothing about my work, but you are free with your opinions regarding my work. So let me clarify. Between 2018–2023, I have written a number of papers on Mochizuki's work, with complete proofs and examples. I had them vetted by experts before posting them on the arxiv for further comments. This is the normal process which I follow for my math pre-publications.

(1) Regarding the Scholze-Stix manuscript, I have established that the very premise it is founded upon is incorrect and hence it provides no mathematical conclusion regarding Mochizuki's work at all.

(2) I have already posted a paper on the arxiv (Constructions II: Proof of a local prototype of Mochizuki's Corollary 3.12). This has all the bells-and-whistles of Mochizuki's Corollary 3.12. The general case will appear in (Constructions III). There has been extensive discussion on your blog about this corollary and many have asserted that this corollary is impossible.

(3) As regards to the timeline, let me point out the following important events:

(3A) (2000, 2001) Teichmuller Theoretic Proofs of the Geometric Szpiro Inequality due to Fedor Bogomolov (NYU) et. al published in (2000), Shouwu Zhang (Princeton) published in (2001).

(3B) (2012) Mochizuki's papers I–IV posted online.

(3C) (2016) Mochizuki's posted his discussion of Bogomolov et. al. and Zhang proofs and importantly he points out in his paper that these proofs are a `Rosetta Stone' for his own proof.

(3D) (2018) Scholze-Stix post their report.

(3E) (2020) Earliest version of my Untilts paper was circulated (I have comments to it from Scholze, Stix, Mochizuki, Hoshi and Kedlaya (later F. Kato and I. Fesenko and other anabelian geometers received copies). This paper demonstrates that arithmetic data required for construction of tempered fundamental groups is not rigid (as asserted by Scholze-Stix).

(3F) (2021-2023) Number of my papers elaborating my ideas are posted on the arxiv. They demonstrate some of the key claims of Mochizuki's papers I–III quite robustly.

(4) No mathematical challenges (privately or publicly) have been raised to my papers (on this topic) to date.

(5) Importantly: Scholze-Stix make no reference to Bogomolov et. al., Zhang proofs and nor are they mentioned in any public discussion led by Scholze, and they are not mention in Scholze's ZbMath review (which is also flawed for the same reasons as above). Mochizuki's discussion of these proofs was available on his website (released in 2016). Scholze-Stix report was released in 2018. Omission is quite striking given the importance of Mochizuki's arithmetic claim and its relationship to the geometric case (normal procedure in arithmetic is to understand the geometric case first (e.g. geometric Langlands and the Langlands correspondence resp.) [My discussion of the Bogomolov et. al., Zhang proofs including both, my point of view and Mochizuki's appears in my (Constructions II(1/2)) paper.]

(6) For the lay-readers, in mathematical logic, a false proposition can be used to arrive at any conclusion one wants. So Scholze-Stix report provides no useful conclusion regarding Mochizuki's work.

(7) A links to all my paper referred above can be found at https://ncatlab.org/nlab/show/Kirti+Joshi.

27. **Peter Woit**
July 17, 2023

Kirti Joshi,
First of all, the only reason I've said anything about your work is that it is repeatedly used to justify claims for the validity of the Mochizuki proof. It would be helpful if you would state clearly whether the Mochizuki papers contain a valid proof of Corollary 3.12 and thus abc.

I've expressed no opinion and have no opinion about the validity of your argument that the Scholze-Stix objection can be overcome, allowing a proof of Corollary 3.12 and thus abc. Here I have just been stating the obvious: the way to conclusively resolve this is to produce such a proof and have it carefully checked by experts.

28. **jack morava**
July 17, 2023

I am certainly no expert in any of this but it appears that Kirti Joshi has anted up a very respectable attempt at doing just what you have asked for.

29. **Peter Woit**
July 17, 2023

Jack Morava,
My understanding is that Joshi has given a proof of a "local prototype" of Corollary 3.12, which is not strong enough to give abc, with a proof of the full result still to come in a "Constructions III" paper.

30. **jack morava**
July 17, 2023

Thanks, looking forward hopefully to clearer skies…

31. **anon**
July 18, 2023

Not directly related to this, but the comments to the original post are closed now: the big $1M+ prizes at the International Congress of Basic Science in Beijing went to Mumford and Adi Shamir. So, surprisingly, no Witten… A bunch of "best paper" prizes were also given to works published in the last ~five years. From a quick look, the selections (in fields I know about at least) seemed reasonable, though obviously there would have been many other equally deserving candidates.

32. **mnmltype**
July 21, 2023

There was a discussion about Kirti Yoshi's preprint on MO with a direct refutation from Peter Scholze all happening in November 2022. See https://mathoverflow.net/a/435112

33. **Peter Woit**
July 21, 2023

mnmltype,
Thanks, I had somehow missed that.

34. **Kirti Joshi**
July 22, 2023

(1) @mnmltype, @Peter Woit: Both of you have evidently missed my responses to the claims made by Scholze and Sawin on MathOverFlow (in 2022). These claims are addressed in detail in my guest blogpost (2022) on David M. Roberts blog (https://thehighergeometer.wordpress.com/2022/11/25/a-study-in-basepoints-guest-post-by-kirti-joshi/) and in my Quest Paper (https://www.math.arizona.edu/~kirti/joshi-teich-quest.pdf). [These links are already posted in the list of links I have given above.] You may find my response to "Grouchy Expert" (also listed above) useful as well.

(2) It is clear that some people on both the sides of this debate are still unwilling to come to terms with incontrovertible mathematical facts uncovered by my investigations. I urge them to please read my mathematical writings on this matter.

(3) How many times does it have to be said that Scholze-Stix argument is based on a flawed mathematical premise and therefore it provides no valid mathematical conclusions regarding Mochizuki's work? [Repeating an incorrect argument ad nauseam does not make it true.]

(4) To put it in Richard Feynman's memorable words: "It doesn't matter how beautiful your theory is, it doesn't matter how smart you are. If it doesn't agree with experiment, it's wrong." I performed a mathematical equivalent of an experiment (see my Untilts Paper 2020-version) by finding a counter example and hence recognized that Scholze-Stix argument is wrong.

(5) For detailed mathematical proofs see my papers, links provided above or at https://ncatlab.org/nlab/show/Kirti+Joshi.

35. **Will Sawin**
July 25, 2023

@Kirti Joshi

It is not possible to falsify a mathematical claim by introducing a new definition for terms used in the claim so that, under the new definition, the claim is false. The definition of "arithmetic holomorphic structures" you use in your work is – as you point out in the linked Quest paper, for example – different from Mochizuki's.

Surely Scholze and Stix were thinking about Mochizuki's definitions when they made their claim that distinct arithmetic holomorphic structures do not exist. The fact that, under your definition, they do exist, is not a falsification.

Mathematical works that consist in large part of new definitions, and proofs of theorems whose statements depend on these new definitions, can never be evaluated solely on a mathematical basis of "Are the proofs of the claimed theorems correct"? To evaluate the importance of the work one needs to estimate the importance and relevance of the new definitions. This will always be at least somewhat subjective and never incontrovertible.

36. **Kirti Joshi**
July 27, 2023

@Will Sawin: I don't really understand the logic of your above comments: are they meant to be a serious mathematical discussion or just musings for social media–I can't tell. But I will address them all the same.

To refresh your memory, and for the record, let me say that in early November, 2022, I spent considerable amount of time explaining my work to you by email. It was clear to me from our correspondence, that you did not wish to be persuaded on this matter at all (as evidence I cite your subsequent Nov 2022 posts on Mathoverflow–to which I gave a detailed response in my guest blogpost on David Robert's blog and I provided further details in my Quest Paper).

Now let me address your comments posted above:

(1) This time your defense of Scholze-Stix is that (I quote) "Surely Scholze and Stix were thinking about Mochizuki's definitions when they made their claim that distinct arithmetic holomorphic structures do not exist." I am sorry, I can only address their written mathematics and not what they may have been thinking.

(2) You assert that "The definition of "arithmetic holomorphic structures" you use in your work is – as you point out in the linked Quest paper, for example – different from Mochizuki's."

As I have demonstrated in my Untilts paper, my definition of arithmetic holomorphic structures provides arithmetic holomorphic structures in Mochizuki's sense.

Let me add that both Mochizuki's theory and my theory of these structures is based on the structure of tempered fundamental groupoids. Mochizuki and I read the information in the tempered fundamental groupoids differently, and these readings lead to our respective definitions, but there is no change in the essential content.

In fact as I point out in my Untilts paper, the structure of the tempered fundamental groupoid (of a variety over a p-adic field) is quite close to that of the Diamond associated to the said variety by Scholze.

(3) Whether you like it or not, and regardless of whether Mochizuki has said it or not, my observation is that the key premise of the Scholze-Stix report is fundamentally flawed. My reasoning is carefully and meticulously documented in my papers, with proofs, examples and all details. As noted in the Quest paper (see \S 1.8), amongst other things, the Scholze-Stix argument is flawed because of their claim that Teichmuller Theory cannot possibly exist because moduli theory exists!

(4) You state that "…This will always be at least somewhat subjective…"–nothing in this context or in mathematics in general is ever "somewhat subjective."

(5) If you still have any questions about my papers and its relationship to Mochizuki's work, I will be happy to visit your math department for a seminar and defend my work.

37. **less polite than Will Sawin**
July 28, 2023

@Kirthi Joshi: You seem to be actively promoting your writings via various blog comments, blog posts, pdf documentations of your "quest", etc., but it is not clear to me at all what *interesting theorems* you are claiming. Your papers – which I have read – seem to consist largely of definitions, constructions, and rhetoric. But I see no reason to care about the constructions you've cooked up so far, because you haven't done anything *non self-referential* with them. Calling something an "arithmetic holomorphic structure" doesn't automatically make it interesting!

38. **Peter Woit**
July 28, 2023

All,
I think the argument with Kirti Joshi has gone well past the point of shedding light on anything and needs to end.

Nothing I've seen here or heard from others has changed the basic facts of the matter: no one has a proof of abc. Joshi believes he has a viable way to get a proof, other experts are skeptical. To convince the skeptics, he just needs to produce a proof, experts will look at it and see if it works. If he's right, hew will richly deserve the $1 million.

39. **jack morava**
July 28, 2023

I don't think this is about the million dollars…

40. **Oisin McGuinness**
July 29, 2023

Alex Kontorovich has a great reaction to the SciAm article, see this Twitter chain: https://twitter.com/alexkontorovich/status/1685461590429081600?s=61&t=U5PW80qvOnhPbSXZ3SpfUQ

A formalization challenge!

Relative Langlands Duality

July 10, 2023
Categories: Langlands

For several years now, David Ben-Zvi, Yiannis Sakellaridis and Akshay Venkatesh have been working on a project involving a relative version of Langlands duality, which among many other things provides a perspective on L-functions and periods of automorphic forms inspired by the quantum field theory point of view on geometric Langlands. For some talks about this, see quite a few by David Ben-Zvi (for example, talks here, here, here and here, slides here and here), the 2022 ICM contribution from Yiannis Sakellaridis, and Akshay Venkatesh's lectures at the 2022 Arizona Winter School (videos here and here, slides here and here). Also helpful are notes from Ben-Zvi's Spring 2021 graduate course (see here and here).

A paper giving details of this work has now appeared, with the daunting length of 451 pages. I'm looking forward to going through it, and learning more about the wide range of ideas involved. A recent post advertised James's Arthur's 204 page explanation of the original work of Langlands, and the ongoing progress on the original number field versions of his conjectures. It's worth noting that while there are many connections to the ideas originating with Langlands, this new work shows that the "Langlands program" has expanded into a striking vision relating different areas of mathematics, with a strong connection to deep ideas about quantization and quantum field theory. The way in which these ideas bring together number theory and quantum field theory provide new evidence for the deep unity of fundamental ideas about mathematics and physics.

Comments

1. **Klaus**
   July 11, 2023

   Peter,

   how should a traditional physicist imagine the progress that the Langlands program will bring or could bring to quantum field theory?

2. **Peter Woit**
   July 11, 2023

   Klaus,
   I'm afraid a traditional physicist can happily just ignore the Langlands program. But at a deep level I'd argue that representation theory and quantum theory are closely related, and the Langlands program is a vast generalization of representation theory, seemingly with connections to QFT that could illuminate the relation between QFT and representation theory.

   For some notes on the relation between QM and representation theory, see
   https://www.math.columbia.edu/~woit/qmnumbertheory.pdf
   One aim of those notes is to explain the theta correspondence, and this seems to be a point where there is some relation (that I don't currently understand…) to the relative Langlands story here.

3. **David Ben-Zvi**
   July 11, 2023

   Peter, thanks for the kind post!
   The theory of theta series and the oscillator reps does fit very nicely into the arithmetic QFT metaphor / fairy tale (all the precise math in our paper is in the function field setting).
   Let's think of the Hilbert space of L^2 functions on the line over F a local field (real or p-adic) not just as individual quantum mechanical systems but as the states of an "arithmetic 3d QFT" on the "arithmetic 2-manifold" F. Likewise for quantizations of other symplectic vector spaces than the plane (eg other theta correspondences).
   Now we upgrade this quantum mechanics story by considering its symmetries, which come by quantizing groups G of hamiltonian symmetries of the plane. We could take the multiplicative group for the Tate-Iwasawa theory of abelian L-functions or the full SL2 (or metaplectic group) for the theta correspondence.
   To each of these groups G we have [on level of metaphor!!] an "arithmetic 4d QFT", which to a global field and fixed level structure ("arithmetic 3-manifold") attaches the space of automorphic forms (with level structure coming from a defect along a "link", the conductor).
   The "oscillator" 3d QFT is now upgraded to a boundary theory for this 4d QFT. The state associated to this boundary theory on our arithmetic 3-manifold is the theta-series (so the period of an automorphic form is its pairing with another state – whence integral representations of L-functions). More generally the theta series gives an operator from L2(R) (or an adelic version) to the space of automorphic forms, which can be interpreted as the path integral with a prescribed boundary theory. Finally all this story has an electric-magnetic dual description in geometry of spaces of Galois representations into the dual group of G, which becomes the role of the theta correspondence as realizing cases of Langlands functoriality.

4. **Peter Woit**
   July 11, 2023

   Thanks David!
   That's very helpful in giving me an idea of how this is supposed to work in a context where I have some idea what is going on.

Understanding Confinement

July 12, 2023
Categories: Uncategorized

This week and next there's an interesting summer school going on at the IAS, with topic Understanding Confinement. Videos of talks are available here or at the IAS video site.

Taking a look at some of the first talks brings back vividly my graduate student years, which were dominated by thinking about this topic. When I arrived in Princeton in 1979, the people there had been working for several years on trying to understand confinement semi-classically, in terms of instantons and other solutions to the Yang-Mills equations (e.g. merons). By 1979 it had become clear that such semi-classical calculations were not sufficient to understand confinement and people were looking for other ideas. There were quite a few around, including the idea that there was some sort of string theory dual to pure Yang-Mills theory, and I spent quite a lot of time reading up on efforts of Migdal, Polyakov and others to find a formulation of string theory that would provide the needed dual. I ended up writing my thesis on lattice gauge theory, an approach which had the great advantage that you could at least put the calculation on a computer and start trying to get a reliable result for pure Yang-Mills numerically. Some of the calculations I did were done at the IAS, with Nati Seiberg and others. The other thing I spent a lot of time thinking about was how to put spinor fields on the lattice, the beginning of my interest in the geometry of spinors.

I strongly recommend watching Witten's talk on Some Milestones in the Study of Confinement. His career started a few years before mine, with the early part very much dominated by the problem of how to make sense of Yang-Mills theory non-perturbatively, and this has has always been a motivating problem behind much of his work. In his talk he explains clearly the approaches to the problem (lattice gauge theory, 1/N, dual Meissner) that appeared very soon after the advent of QCD in 1973. He emphasizes how each of these approaches shows indications of a possible string theory dual, while frustratingly not leading to a string model that has the right properties, summarizing (41:30) the situation with:

> The string theory we want is probably quite unlike any that we actually know, as of now. We don't know how to make a string theory with the short distance behavior of asymptotic freedom.

In his talk he discusses later developments, in particular the Seiberg-Witten solution to N=2 SYM and the AdS/CFT duality between a string theory and N=4 SYM, explaining how these advances still don't provide a viable approach to the confinement problem in pure Yang-Mills.

I'm looking forward to seeing the rest of the talks, and finding out more about some things that have happened over the years since I was most actively paying attention to what was happening with the confinement problem.

Comments

1. **Anonyrat**
   July 24, 2023

   It is sad that on this most physical of questions discussed here there are zero comments. Perhaps speculative physics is now the permanent fashion – it is so much easier.

2. **Peter Orland**
   July 25, 2023

   As a longtime obsessive on confinement, drawn to the problem like a moth to a carbon electric arc, I cannot help but second Anonyrat's remarks.

3. **A.J.**
   July 26, 2023

   I felt the same way, but should we be surprised? The barrier to having an opinion on confinement is rather high.

4. **Marty Tysanner**
   July 26, 2023

   Interesting… Now the comments are about the lack of comments rather than the subject of the posting. 🙂

5. **Peter Orland**
   July 26, 2023

   Marty Tysanner, I do have strong opinions about this subject, but expressing them here will just get my legs broken with a big baseball bat by the *REDACTED*. I would rather explain them in technical publications.

6. **Peter Orland**
   July 26, 2023

   Anyway, I wrote a bunch of papers from 2005 onward which anticipate later work of others. Peter may (justifiably) delete this post, because this is the wrong forum for self promotion, but a few are: hep-lat/0501026, hep-th/0607013.

7. **Peter Woit**
   July 26, 2023

   All,
   I'd love to be able to host a discussion of what the IAS talks had to say about recent progress on the QCD confinement question, but looking through them I couldn't really find anything. Witten didn't seem to have anything to discuss post Polchinski-Strassler from 23 years ago. More recent progress on various models discussed in other talks didn't seem to apply to QCD (in 4d, Nair's talks on 3d were interesting). Informed comments from those paying closer attention to all this than me are welcome.

This Week's Hype

July 18, 2023
Categories: This Week's Hype

Nanopoulos and co-authors have predictions from superstring theory that are "in strong agreement with NANOGrav data." He has been at this now for almost 40 years. See for instance Experimental Predictions from the Superstring from 1985, where the superstring predicted a top mass of 55 GeV and 360 GeV squarks.

Comments

1. **Low Math, Meekly Interacting**
   July 19, 2023

   How many free parameters does a study like this typically involve? I recall an anecdote, I think about Fermi rejecting a paper Freeman Dyson wanted to publish. Something about with so-many parameters, "I can model an elephant wiggling its trunk." At what point did this wisdom fall out of favor?

2. **Peter Woit**
   July 19, 2023

   LMMI,
   The problems with "predictions of superstring theory" go way beyond the Fermi/Dyson sort, rather point to a deeper sociological problem that is not going away.

3. **Wolfgang Keller**
   July 19, 2023

   @Low Math, Meekly Interacting

   As far as I am aware, this example was rather due to John von Neumann: Von Neumann's elephant – Wikipedia

   There is a paper on this topic (also linked in the Wikipedia article) where an elephant is fitted with four parameter and a fifth parameter is used for wiggling the elephant's trunk: Jürgen Mayer, Khaled Khairy, Jonathon Howard. "Drawing an elephant with four complex parameters"

   Link to a PDF of this paper: https://publications.mpi-cbg.de/Mayer_2010_4314.pdf

4. **Andrew M**
   July 19, 2023

   @LMMI

   The plot in Figure 1 involves fixing 5 free parameters (see the text after Eq 10). However, note that the action used as a starting point (Eq 1) has 3 free *functions*, so even to get to those 4 parameters multiple assumptions and simplifications have been made. On top of that even to call Eq (1) "a prediction from superstring theory" is a stretch since it is really a specific 4D low energy field theory with N=1 supersymmetry, that would have to emerge from some compactification + supersymmetry breaking; it isn't even the most general 4D N=1 supersymmetric effective field theory you could get, but rather "Wess-Zumino no-scale supergravity" which is chosen since you can use that to construct inflation models.

   To unpack that, to get from "superstring theory" to what they do in this paper, you need to postulate that Naure has made use of a compactification + partial supersymmetry breaking that reduces to a specific class of 4D low energy supersymmetric theories that are motivated by the property that they are easy to work with in an inflationary scenario. This class of theories has three free functions. By making further simplifications (again motivated by computational convenience), one assumes that these free functions can be parameterized in terms of 5 parameters. Then, for a specific range of these 5 parameters, the power spectrum has a form that people guess (but no one has checked explicitly) will form primordial black holes. (In more detail, the usual argument for producing primordial black holes is that the power spectrum crosses a certain threshold in density — but at this threshold all that happens is perturbation theory breaks down, to actually show black holes form you would need to solve Einstein's equations nonlinearly). If you tune the parameters right, you can arrange for the energy density of these PBHs to dominate over radiation at a specific time in the Universe's past. Under this assumption, the time variation in the Newtonian scalar potential acts as a source term for gravitational waves. Given that you've tuned the parameters of the model to give you the transition to PBHs at the right time and with the right abundance, this source term will generate gravitational waves in the PTA band of the right amplitude. Incidentally, the paper does not give ranges for the parameters that are consistent with data, but rather one specific set of values, which one might call a set of measure zero.

   I'll leave it to you to decide for yourself how robust this prediction is.

5. **More Anonymous**
   July 20, 2023

   @Wolfgang Keller

   Dyson as well (it seems): https://www.youtube.com/watch?v=hV41QEKiMlM

   I'd be be curious on Peter's take on "either you should have a clear physical model in mind or rigorous mathematical basis."

6. **NoGo**
   July 20, 2023

   I think that Freeman Dyson tells that story about parameters in one of the videos in https://youtu.be/rs1jGsn61p8, although I am not sure which one out of 157 🙂
   IIIRC it the phrase was "With 3 free parameters I can fit in an elephant, with 4 I can make it wiggle its trunk" (I am not sure about the exact numbers).

   I don't know how many wiggling elephants, tortoises, and unicorns one can fit into 10^500 variants of a theory, but if I understand to Peter and others correctly, the problem is that none of these variants can be written down and make any predictions anyway.

7. **Peter Woit**
   July 20, 2023

Maybe that's enough about the Fermi/Dyson/von Neumann story.

Thanks to Andrew M for the explanation, which shows that number of parameters is the least of it (there actually is no real theory, and they are effectively choosing arbitrary functional forms, so, more like infinite number of parameters).

8. **Cedric BARDOT**
July 21, 2023

Naively comparing end of the abstract article: "such a signal can act as a clear signature of no-scale Supergravity and Superstring theory…" with its conclusion: "it may serve as a smoking gun of Superstring theory in GW observations…" one can hopefully conclude authors/reviewers have not made fools of themselves!
Thanks a lot to connoisseurs like Andrew M for helping enthusiasts, amateurs and possibly even critics or science whistleblowers in order to appreciate the phenomen(ologic)al masterpieces regularly produced by PhDs, post-docs & tenure track profs dreaming to become full professors in our contemporary big sciences-dependent, knowledge-driven & resources-limited society.

9. **Low Math, Meekly Interacting**
July 21, 2023

von Neumann via Fermi. Got it! And yes, thank you most kindly, Andrew M. Given my enthusiasm for the nanoGRAV story, I actually spent some time trying to comprehend the preprint. I did get the distinct sense a lot of, uh, choices were being made, but I clearly couldn't discern even a fraction of it. "Overfitting" doesn't really suffice. "Bullshit", then?

10. **Robert Cochrane**
August 14, 2023

This week's hype in the British press was the muon g-2 results from Fermilab and focused on the possibility of new physics. I was asked by my wife (who has no physics background) what new physics was likely. My amateur response as a retired engineer was that the theoretical calculations based on the existing Standard Model of particle physics are exceedingly complex and there appear to be two distinct methods of calculation which differ at present, but I personally expect them to align in a year or so with the latest experimental results.
Very interested to hear your views.

11. **Peter Woit**
August 14, 2023

Robert Cochran,
I've decided it's best to just ignore the g-2 hype, the less attention it gets the better. The underlying story is as you say, and exactly the same as last announcement: the experimental result is consistent with one version of the SM calculation, which means no evidence for new physics here.

Strings 2023

July 28, 2023
Categories: Strings 2XXX

For much of the past week, I've been attending off and on (on Zoom) the Strings 2023 conference. This year it's in a hybrid format, with 200 participants in person at the Perimeter Institute, and another 1200 or so on Zoom. These yearly conferences give a good idea of what some of the most influential string theorists think is currently important, and I've been writing about them for twenty years. Videos of the talks are being posted here.

As in many of these Strings conferences in recent years, there was very little discussion of strings at Strings 2023. Of the 24 standard research talks, only 4 appeared to have anything to do with strings. A new innovation this year was to schedule in addition four "challenge talks", conceived of as talks explicitly about material outside of string theory that might interest string theorists. In particular Edward Frenkel gave a nice survey of a wide range of ideas from quantum integrable systems and ending up with geometric Langlands. He motivated this with reference to what Feynman was working on very late in life and the problem of solving QCD. His slides are here, video here.

In addition there were four morning "Discussion Sessions", which I attended most of, and at which string theory put in little to no appearance. Today's discussion featured Nati Seiberg and Anton Kapustin and was about lattice versions of QFT, especially in their topological and geometrical aspects, a very non-stringy topic dear to my heart. Yesterday was It From Qubit, which had Geoff Penington discussing topics related to black holes. The conventional wisdom now seems to be that the information paradox is gone, solved semi-classically, so giving no insight into true quantum gravity dynamics. While this means you can't see anything interesting at large distances from the black hole, Penington had some new ideas about something that might in principle be observable at atomic-scale distances from a super-massive black hole. Maldacena started off the session with slides promoting the way forward as quantum computer simulations involving 7000 qubits, a variant on the wormhole publicity stunt. The only time string theory made an appearance was in a suggestion by Dan Harlow that perhaps by doing quantum computer simulations theorists could solve the the problem of what "string theory" really is. It's pretty clear what the leading direction is now for continuing the long tradition in string theory of outrageous hype.

After this week, I'm even more mystified about why the conference was called "Strings 2023" And how does one decide these days what "string theory" is and who is a "string theorist"? Oddly, two of the things that now distinguish this yearly conference from others are a pretty rigid exclusion of both real world physics (Frenkel comments on this here) as well as of what got people excited about string theory, superstring unification and its implications for seeing low energy SUSY at colliders. People still interested in that have split off to other conferences, especially String Phenomenology 2023 and SUSY 2023.
Those conference have their own kinds of mysteries (why do people keep working on ideas that failed long ago?). In particular, the closing talk on the Status and Future of Supersymmetry at SUSY 2023 was all about the great prospects for SUSY at the LHC, and included a Conclusion written (no joke) by ChatGPT:

> The future of supersymmetry as a research program holds both exciting challenges and potential breakthroughs. While the LHC experiments have yet to observe direct evidence of supersymmetric particles, ongoing theoretical advancements and refined experimental techniques offer renewed hope. The future of supersymmetry research lies in two key directions. Firstly, novel theoretical models are being explored, including new variants of supersymmetry that incorporate dark matter candidates or non-linear realizations. These approaches push the boundaries of our understanding and allow for further exploration of the particle zoo. Secondly, upcoming experiments, such as the High-Luminosity LHC and future colliders, aim to explore higher energy scales and increase the sensitivity to supersymmetric signals. With these advancements, the quest for supersymmetry will continue to shape the field of particle physics, inspiring new theoretical insights and propelling experimental discoveries.

Things just get stranger and stranger…

Update: Speaking of stranger and stranger, you can listen here to Sean Carroll talking by himself for four hours and twenty-two minutes about why there really is no crisis in physics, the whole supersymmetry/string theory thing is going just fine.

Update: I hadn't realized just how accurate Joe Conlon's description of the conference as "IAS-centric" was. For the four discussion sections, no IAS in one of them ("strings, QFT and mathematics", the other 3 sessions were all IAS (two IAS faculty, the rest an assortment of ex or current members).

Comments

1. **S**
   July 28, 2023

   ChatGPT requires very large volumes of text of a given type to train well for it. It's not too surprising that it can write good string theory hype.

2. **Sabine Hossenfelder**
   July 29, 2023

   Maybe they will rebrand themselves to X-theory

3. **F.T.**
   July 29, 2023

   As a (serious) string theorist I must fully agree on this post.

   Interestingly in the past there were a vision and summary talk in particular saying
   how well and prosper string theory is doing. It seems that all this enthusiasm is gone: no vision anymore, nothing to conclude.

4. **lun**
   July 29, 2023

   Nanopolous gave a plenary talk about this
   https://arxiv.org/abs/2303.04814
   Very nice and interesting. An experimental detection proposal for something predicted by both the standard model of particle physics and cosmology, about which string theory says nothing.

5. **Peter Woit**
   July 29, 2023

   There's a comment from Joe Conlon here that is interesting
   https://twitter.com/JosephPConlon/status/1685222041249587200
   His diagnosis is that this series of conferences is "IAS-centric", features mainly discussion of topics of interest to the Princeton IAS faculty.

Looking at the backgrounds of the speakers, I think Harvard plays as big a role as the IAS, so this should be "Harvard and IAS-centric", with most speakers having a student/postdoc/faculty background at Harvard and/or Princeton.

6. **Peter Woit**
   July 29, 2023

   F.T.,
   Yes, one change in this latest version is elimination of the "vision talk" category. This is turning into a subject with no plausible vision of a way forward.

7. **Kurt Schmidt**
   July 29, 2023

   Sabine Hossenfelder,

   There is already "M-theory", where the "M" could stand for "Mystery", "Magic", or "Mediocre". About as apt of a name as any for the current state of string theory.

8. **Peter Woit**
   July 29, 2023

   Kurt Schmidt,
   The problem is "M-theory" refers to a certain sort of unified theory, one that appears even less at Strings 2023 than "string theory".

   A correct name would be something like "topics that people at Harvard and Princeton who used to work on string theory are working on this year instead, since the string theory stuff didn't pan out."

9. **Matt Grayson**
   July 30, 2023

   Sabine Hossenfelder,
   The pun inside the joke is extremely clever. I missed it the first time. Bravo.

10. **Doug McDonald**
    August 1, 2023

    I read the entire transcript of the of whole Sean Carroll thingie. I can't imagine the boredom one must endure to listen to the whole of it.

    The most striking thing about it is the lack of phenomenological calculations or even phenomenological suggestions about what dark matter is (concrete suggestions of a rest mass, not just "axions would be really light").

    It also over-attacks MOND by not seriously treating more recent theories than straight original TeVeS.

    Other than that, it would actually be convincing. That's why most pros are seriously completely convinced.

11. **tulpoeid**
    August 2, 2023

    "… the information paradox is gone, solved semi-classically": ? Can we please learn more about this?

    From Frenkel's tweets: "… never lost sight of the big prize: understanding the physics of THIS Universe. And I think that's still a good guiding principle for today's physicists." Just stating the obvious, but to me this is one of the most concise meta-comments on the state of the field ever written: in 2023 prominent theorists have to timidly suggest that the definition of physics might be a good guiding principle for physics.

    Of course "a pretty rigid exclusion of real world physics" does seem to be the defining feature of the field. Even more, it's a lifestyle. If one appeared at a similar setting talking of real world connections, one would appear like the backwards relative visiting from the countryside.

12. **Peter Woit**
    August 2, 2023

    tulpoeid,
    This has gotten a huge amount of attention at Quanta and elsewhere, see for example
    https://www.quantamagazine.org/the-most-famous-paradox-in-physics-nears-its-end-20201029/
    https://www.quantamagazine.org/netta-engelhardt-has-escaped-hawkings-black-hole-paradox-20210823/
    also
    https://iopscience.iop.org/journal/0264-9381/page/Focus-Issue-on-Reproducing-the-Page-Curve-in-Black-Hole-Evaporation

    I don't think the problem with the current state of "string theory" is lack of connection to real world physics. The huge criticism of "string theorists" of this kind is what is causing them to head in absurd "quantum gravity in the lab" directions. There is no way to solve their problems by just redirecting them to the real world. I've gone on endlessly here and elsewhere about what those problems are (e.g., not actually having a theory…), it's a complex topic, not reducible to sound bites.

13. **Peter Woit**
    August 2, 2023

    tulpoeid,

    Quanta today has a new article about exactly this:
    https://www.quantamagazine.org/new-calculations-show-how-to-escape-hawkings-black-hole-paradox-20230802/

    What I'd be curious to see is a story about the implications of this. The main reason much of the "string theory community" abandoned everything else to become focused on toy models of the information paradox was the hope that solving this paradox would tell us something about the nature of quantum gravity. Instead, the solution they found says that solving the paradox can be done within the semi-classical framework, so you learn nothing new about quantum gravity this way. Given that, what do you do now? If your answer is to keep working on the details of the toy models (lots of work to be done!!), why are you doing this?

14. **Scott Aaronson**
    August 3, 2023

Supposing Harlow et al. are right with their non-isometric codes account of the black hole information paradox (the subject of the new Quanta article), I'd say that you unequivocally DO learn something new about quantum gravity. Namely, you learn that a key to QG is that there's a "fundamental Hilbert space" that perfectly obeys QM, but that our (3+1)-dimensional experience is described by an "effective Hilbert space" that has an apparently much larger number of qubits, but in which most of the "exponentially complex" quantum states don't actually exist. And you learn that the way this gets implemented is via a linear map between the fundamental and effective Hilbert spaces, which acts like an isometry (or nearly one) on all "low-complexity" effective states, with the illusion breaking down only for "high-complexity" states like those that arise in the firewall paradox, or in decoding the Hawking radiation from an old black hole.

There's still the obvious difficulty, that as far as known, the only way to test these ideas experimentally might involve waiting ~exp(10^67) years and THEN jumping into a black hole — and having learned the answer, you couldn't tell anyone outside the black hole! But I don't think one can argue that there's not a new proposal here about quantum gravity to chew on.

15. **Peter Woit**
August 3, 2023

Scott Aaronson,
Thanks for the explanation. From what I can tell, if the proposal right, the information paradox gets resolved generically in the class of theories people are considering (some day I'd like to see a precise definition of that class, but it looks like whatever it is, it's very large). How this happens is perfectly interesting, but if it doesn't help pin down a more specific theory, isn't this approach to quantum gravity just more definitively lost in an infinite landscape of theories with no way forward?

16. **Scott Aaronson**
August 4, 2023

Yeah, you could imagine a huge space of possible theories implementing these ideas, just like a huge space of theories all implement the ideas of QM or SR. That's a bug insofar as none of this seems to helps you pin down (e.g.) the specific particle content of our universe, but also a feature insofar as it doesn't stand or fall with the fortunes of a particular proposal like string theory.

17. **Alessandro Strumia**
August 11, 2023

Simple question: do the new understanding mean that a black hole made of 10^10 kg of hydrogen emits this energy in photons (and other light particles)?

18. **Peter Shor**
August 12, 2023

Alessandro Strumia:

I don't think anybody has questioned that black holes emit most of their energy in photons since Hawking discovered Hawking radiation. The new understanding does nothing to change this.

The Philosophy of Supersymmetry

August 16, 2023
Categories: Uncategorized

A few months back I saw a call for papers for a volume on "Establishing the philosophy of supersymmetry". For a while I was thinking of writing something, since the general topic of supersymmetry is a complex and interesting one, about which there is a lot to say. Recently though it became clear to me that I should be writing up other more important things I've been working on. Also, taking a look back at the dozen or so pages I wrote about this 20 years or so ago for the book *Not Even Wrong*, there's very little I would change (and I've written far too much since 2004 about this on the blog). What follows though are a few thoughts about what "supersymmetry" looks like now, maybe of interest to philosophers and others, maybe not…

First the good: "symmetry" is an absolutely central concept in quantum theory, in the mathematical form of Lie algebras and their representations. Most generally, "supersymmetry" means extending this to super Lie algebras and their representations, and there are wonderful examples of this structure. A central one for representation theory involves thinking of the Dirac operator as a supercharge: by extending a Lie algebra to a super Lie algebra, Casimirs have square roots, bringing in a whole new level of structure to familiar problems. In physics this is the phenomenon of Hamiltonians having square roots when you add fermionic variables, providing a "square root" of infinitesimal time translation.

Going from just a time dimension to more space-time dimensions, one finds supersymmetric quantum field theories with truly remarkable properties of deep mathematical significance. Example include 2d supersymmetric sigma models and mirror symmetry, 4d N=2 super Yang-Mills and four manifold invariants, 4d N=4 super Yang-Mills and geometric Langlands.

But then there's the bad and the ugly: attempts to extend the Standard Model to a larger supersymmetric model. From the perspective of 2023, the story of this is one of increasingly pathological science. In 1971 Golfand and Likhtman first published an extension of the Poincaré Lie algebra to a super Lie algebra. This was pretty much ignored until the end of 1973 when Wess and Zumino rediscovered this from a different point of view and it became a hot topic among theorists. Very quickly it became clear what the problem was: the new generators one was adding took all known particle states to particle states with quantum numbers not corresponding to anything known. In other words, this supersymmetry acts trivially on known physics, telling you nothing new. It became commonplace to advertise supersymmetry as relating particles with different spin, without mentioning that no pairs of known particles were related this way. In all cases, a known particle was getting related to an unknown particle. Worse, for unbroken supersymmetry the unknown particle was of the same mass as the known one, something that doesn't happen so the idea is falsified. One can try and save it by looking for a dynamical mechanism for spontaneous supersymmetry breaking and using this to push superpartners up to unobservable masses, but this typically makes an already pretty ugly theory far more so.

The seriousness of this problem was clear by the mid-late 1970s, when I was a student. The one hope was that maybe some extended supergravity theory with lots of extra degrees of freedom would dynamically break supersymmetry at a high scale, leaving the Standard Model as the low energy part of the spectrum. There wasn't any convincing way to make this work, and it became clear that one couldn't get chiral interactions like those of the electroweak theory this way. 1984 saw the advent of a different high scale model supposed to do this (superstring theory), but that's another story.

Looking back from our present perspective, it's very hard to understand why anyone saw supersymmetric extensions of the SM as plausible physics models that would be vindicated by observations at colliders. For example, Gross and Witten in 1996 published an article in the Wall Street Journal explaining that "There is a high probability that supersymmetry, if it plays the role physicists suspect, will be confirmed in the next decade." Ten years later, when the Tevatron and LEP had seen nothing, the same argument was being made for the LHC. After over a decade of conclusive negative results from the LHC, one continues to hear prominent theorists assuring us that this is still the best idea out there and large conferences devoted to the topic. Long ago this became pathological science. In the call for papers, the issue is framed as:

> recent debates on the prospects of low energy supersymmetry in light of its non-discovery at the LHC raise interesting epistemological questions.

From what I can see, the questions raised are not of an epistemological nature, but perhaps the philosophers will find a way to sort this out.

Update: There was a workshop on this last year, abstracts here.

Update: I happened to come across today this 2021 interview of Daniel Freedman by David Zierler. Zierler repeatedly asks Freedman why he has faith in SUSY despite the long history of no evidence. Near the end, Freedman gives this very defensive explanation:

> **Zierler**:
>
> What I hear in your remarks is an adherence to supersymmetry despite its immediate experimental prospects. Is that a belief or is it something more?
>
> **Freedman**:
>
> It's a belief which stems from confidence in the powerful symmetry which underlies the subject. Some human beings indulge in beliefs which have no basis whatsoever. Some of those beliefs are destroying our society at the moment. My belief in a credible and interesting physical theory isn't going to hurt anybody.

Comments

1. **Molly**
   August 16, 2023

   Your argument for spontaneous supersymmetry breaking being ugly and somehow a desperate attempt to make a theory agree with nature makes no sense. The generalisation of Abelian gauge theories to non-Abelian gauge theories could equally well be accused of being made ugly by desperate attempts to reconcile that with nature by shoving a mass into that theory either by hand or through the Higgs mechanism. You might say that's different, we know it exists in nature, but that's only a posteriori. The fact is both generalisations follow the same kind of logic and motivations. Note also that the first minimal attempts at the non-Abelian generalisations were also wrong. Had we happened to be technogically limited such that going up in energy took a decade per 10 GeV such that the W and Z were supposedly always around the corner but always disappointingly out of reach then no doubt non-Abelian theories would be the target of your polemic. Of course you'll say something like "well that was different, we knew about neutral currents etc." But it's always easy to join up the dots in hindsight. My point is one that in principle non-Abelian and supersymmetric generalisations are very similar in philosophy and what experiment can tell us is something determined by the limitations of technology which nature doesn't give a damn about. Yes it's disappointing that supersymmetry is not where it could have been according to *one* specific argument. But had it taken say a century to go from 40 GeV to 80 GeV you'd have been there every step of the way berating non-Abelian theorists and their unscientific fanciful ideas led by group think. It's ironic that you appear to be completely oblivious to your own twisted reasoning that's no less delusional than Gordy Kane's, only on the opposite anti-supersymmetry end of the spectrum. This anti-supersymmetry narrative is just as tiresome a broken record as the extreme pro-supersymmetry promises. The truth, as always, lies somewhere in the middle.

2. **Peter Woit**
   August 16, 2023

"Molly",
First of all, what's with the fake name (I really doubt your name is Molly)? Could it be because you know very well that arguing that "in principle" the situation of the Weinberg-Salam model and the MSSM are "very similar in philosophy" is so silly that you don't want your real name on it?

As you're undoubtedly well aware, when Weinberg wrote his model down in 1967, the W and Z masses were determined by three parameters. The EM coupling + Fermi constant gave two out of three, so to nail down the W, Z masses you just needed to measure one more parameter (the Weinberg angle). According to you this is much the same thing as the early (and current…) situation of the MSSM, which I believe introduces about 105 new undetermined parameters.

So, you're arguing that having one undetermined parameter and having 105 undetermined parameters is "very similar"? Whose reasoning here is "twisted" and "delusional"?

3. **A long long time ago**
August 16, 2023

Prefacing this by stating that weak-scale supersymmetry is mostly dead (but see Goldman, W (1973)), it's not fair to say that people should have known better. Supersymmetry, like technicolor, is a very good idea that happens to be wrong. Supersymmetry gave a plausible way to make the Higgs mass natural, provides a dark matter candidate and makes the couplings unify more closely. Now it's very easy to cavil at all these things, especially with the benefit of hindsight, and it's clear that things didn't work out. But the physicists excited by supersymmetry weren't idiots and had very good reason to be excited. It's the people who haven't tempered their enthusiasm in response to the experimental evidence that are the problem.

4. **Molly**
August 16, 2023

I am not referring to the Weinberg-Salam model vs the MSSM, which are specific models in a more general framework. I am referring to the framework of non-Abelian gauge theories in general, going back to Yang and Mills, whose mathematical generalisation of the Lie algebra is very much in analogy with the further generalisation to a super Lie algebra. Pauli came up with this before but knew there no other massless gauge bosons that such a generalisation predicts. The Woit of the time would have derided attempts to rescue this theory by breaking the degenerate mass spectrum with an "ugly" spontaneous breaking mechanism. He would have pointed out that Glashow's first minimal SU(2) attempt was already clearly excluded by experiment and scorned further model-building, such as adding an admixture of U(1), as just further desperate attempts to evade experiment rather than abandoning the fantasy. What next, SU(3)? SU(4)? SU(3)xSU(2)xU(1)? Gauge theories aren't even predictive, he would say, you can always change your prediction by choosing a different SU(N)!

The point here is not about number of parameters; parameters parameterise ignorance, and ignorance is a technological limitation. If we happened to have experimental access to the susy-breaking mechanism then it would not be 105 parameters but ultimately a less arbitrary theory where supersymmetry relates parameters by symmetry in the same way we don't consider 8 gluons to be uglier than a single photon because they're related by SU(3). The difference in number of parameters and the extent to which one can determine them experimentally is a point of practicality.

It says something about physicist stereotypes that Molly is unfortunately not taken for granted as a real name for a physicist. In any case please consider that people may have legitimate reasons for preferring not to draw public attention to themselves. Just because you choose to publicise yourself with your arguments does not make anonymous arguments any less valid simply because you can't make a mental profile of the person behind the arguments. Would it help to say I have never published a paper on supersymmetry? Could it be that I am not a lifelong SUSY fanatic but simply a student who took the time to learn the facts and formed my own reasoned opinion?

5. **Peter Woit**
August 16, 2023

A long long time ago,
Technicolor is an excellent example to contrast to supersymmetry. Like SUSY, it's an initially very attractive idea (I was a grad student when it came out and well remember this appeal). But as you try and fit it with reality it gets more ugly (extended technicolor?), and as experimental evidence and theoretical work developed, most people lost interest, with a few hanging in there trying to make it work. This is normal science, and the question with SUSY is why didn't it evolve in the same way, but instead took the turn to the pathological?

I'm very well aware that some of most enthusiastic SUSY advocates are people smarter, better-informed and much harder working than me. The phenomenon of their decades-long enthusiasm for what to me was an unpromising idea has always baffled me. The cases where this continues post LHC-results just strike me as inexplicably bizarre. One can have a reasonable argument about exactly when things turned pathological during the period 1974-present, but I don't think one can reasonably argue that we're not there now.

6. **Peter Woit**
August 16, 2023

"Molly",
Ah, the "framework" nonsense. Can't tell now whether I'm dealing with an earnest fool or a clever troll, strongly starting to suspect the latter. Enough.

7. **I can still remember**
August 17, 2023

The thing I want to do is to defend those physicists in the 80s. It was very reasonable for people to get excited about SUSY. It managed to solve multiple problems at once. And you call the MSSM an ugly theory, but it's really not. It's the phenomenological SUSY breaking lagrangian that's a disaster. There was always hope that a compelling form of SUSY breaking might come along that could tame the mess.

And the difference with technicolor is that, as I understand it, the FCNC results made it pretty challenging relatively soon. For SUSY, it wasn't until the Higgs mass started getting pretty high compared to the Z-mass that the experimental outlook started looking dimmer. As that happened and later with the lack of any sign of superpartners at the LHC and with the SUSY breaking mess not seeming to get much better, you saw more physicists getting pessimistic about weak scale SUSY.

I'm not going to defend the die-hards who still claim that SUSY must be the answer.

8. **Alex**
August 17, 2023

My take: let's leave history for the historians of science. Who cares who was right in the 80s? It was a different context.

To me, the important thing is, what do we do today? The whole field of fundamental physics is in a languid state. Particle physics is in deep coma, quantum gravity is in fantasy land. People with authority are in denial that anything is wrong, science journalism is taken by hype. Young physicists are leaving, those who stay waste their talent in dead end fantasy theories. The whole situation is very dire to me.

9. **Peter Woit**
   August 18, 2023

   Alex,
   I think one of the main reasons for the sorry state of the field is that no one wants to admit being wrong, no one wants to criticize respected figures in the field for continuing to push failed ideas, no one wants to look at what happened and find where things went wrong. So the field is dominated by a 50 year old failed conceptual framework (Planck scale superstrings/supergravity, GUTS, SUSY extensions of SM at some scale). As long as this conceptual framework is what people are trained in and live and work in, the hard job of finding an alternative is much harder.

10. **DB**
    August 19, 2023

    I'm mostly here to agree with "A long long time ago". As a student in the 80's, I learned that technicolor and supersymmetry were the two leading candidates to solve the hierarchy problem (weak scale far below Planck scale, avoid Higgs mass divergence), and technicolor was already on the way out. Plus the dark matter candidate (photino, or more generally neutralino), and the beauty of SUSY SU(5) (converging couplings, less proton decay), made it the most promising avenue of study.

11. **Peter Woit**
    August 19, 2023

    DB,
    Yes, that's the sales pitch that was being made for decades starting in the early 80s. The problem is that a few weak arguments for a complicated theory with no evidence don't add up to a significant argument for such a theory. There always were some prominent people out there making every argument they could think of for SUSY, never breathing a word about the problems with the theory. Anyone mentioning these problems was a "contrarian" even though what they were saying about the problems with the theory was well known in the community.

    If you look at occasions where theorists made bets on SUSY, significant majorities always were betting against, well aware of the problems even it they weren't getting a lot of attention.

12. **Peter Shor**
    August 20, 2023

    It's not just a 50-year-old failed conceptual framework that's the problem.

    The field seems to have this tendency to latch onto unproven ideas long before they've demonstrated their ability to stand up. Look at Susskind's idea of black hole complementarity from 1993, which didn't have very much evidence for it, but was accepted by many researchers for nearly 20 years, until it succumbed to the AMPS paper in 2012.

    I'm not at all convinced the same thing isn't going to happen to the latest craze, non-isometric quantum codes; these seem to violate so many principles of quantum information that I don't see how they can possibly work.

    This wouldn't be a problem, if researchers in the field didn't have the tendency to dismiss alternative ideas with the reasoning "we've already solved that."

13. **Anonymous**
    August 21, 2023

    "My belief in a credible and interesting physical theory isn't going to hurt anybody."

    Except perhaps for the younger researchers who are forced to work on theories they do not believe in to please the boss.

14. **Peter Woit**
    August 21, 2023

    One thing that I realized is strange about about this history is that somehow skeptics of SUSY got labeled as "contrarians", with SUSY "mainstream science", which is completely backwards. From my conversations with theorists over many years, at no time were SUSY models ever something that most people found convincing. For evidence of this, see for instance here
    https://strings.ph.qmul.ac.uk/~dsb/dbwager.pdf

    Given the negative LHC results, at this point I'd argue that anyone still promoting SUSY extensions to the SM is somewhere on the spectrum from "contrarian" to "crackpot", with the likes of Sabine Hossenfelder holding down the centrist, mainstream position.

15. **Amitabh Lath**
    August 21, 2023

    So I'm guessing nobody here feels that the 5.1 sigma discrepancy with theory reported by the Fermilab g-2 experiment is due to SUSY or her siblings?

16. **Kurt Schmidt**
    August 22, 2023

    The muon g-2 discrepancy is a 5.1 sigma discrepancy between two different ways of calculating the dipole moment from the Standard Model, one of which already agrees with the experiment. There is no need for beyond Standard Model physics, much more likely is the case that the other method is flawed and needs correcting.

17. **Peter Woit**
    August 22, 2023

    Amit,
    At this point the SM being wrong by 5.1 sigma is an extraordinary claim, and given the theoretical complexities, the evidence is far from extraordinary. Seems likely one of the theory calculations is just wrong (or uses data wrong).

    If the SM is wrong, personally I doubt what we're seeing is SUSY, should be something much more interesting….

18. **Amitabh Lath**
    August 22, 2023

    Kurt, should we be giving more weight to the theory that agrees with experiment (the BMW calculation) or the other one that shows the huge tension (if they have a clever

acronym I missed it)? The latter seems to be a bigger collaboration so I have been hedging that way.
In any case, the big theory issue is lack of e+e- –> hadrons data, which is being addressed. The g-2 experiment is still not systematics limited and has a lot more data to analyze. So both theory and experimental uncertainties will shrink in the next 2 years of so. As Peter says, probably not SUSY but interesting…

19. **Hiro Kawabata**
August 22, 2023

It seems that the theorists have considerable work to do on their calculations, including discrepancies in the data used for the dispersive method and the discrepancy between the dispersive and lattice QCD methods, before any claims of BSM physics can be made:

https://muon-gm2-theory.illinois.edu

20. **Peter Woit**
August 23, 2023

Maybe that's enough about muon g-2, where the issues have nothing to do with SUSY.

A generic problem with the idea of using precision measurements like g-2 to get information about BSM physics is that if your BSM models (like MSSM) have loads of extra particles and parameters, a measured g-2 anomaly due to MSSM effects is going to give you one number, which is a very complicated function of the the many parameters you'd like to know about. In any case, right now we don't even have that.

21. **adrianmigueldiego**
August 24, 2023

Dear Peter,
you should exercise some prudence when discussing Daniel Freedman.

Freedman is a very good mathematical physicist. He never over claimed
his results. He has a long trajectory of serious, correct and hard work. You are criticising him for what is an opinion (or a belief). He states this very clearly. He never forced anyone to work on his topics. Young researchers approached him in the last 50+ years (to work with him). Daniel Freedman is an example of serious, hard, concentrated work. Sticking to correct and elaborated calculations.

Again, you should be less critic of serious people, in particular if they state their beliefs and opinions, making clearly their character!

22. **Peter Woit**
August 24, 2023

adrianmigueldiego,

Note that what I was doing was not criticizing Freedman or his work, but pointing to a long interview with him that people could read for themselves. I found his defense of continuing to work on a failed research program revealing.

As for not criticizing serious people, sorry, but when their ideas are wrong they (the ideas) should be criticized, and I'm not going to apologize for doing so.

23. **kitchin**
September 24, 2023

Again I'll recommend also the Frenkel interview by Brian Keating, because it's shorter. https://www.youtube.com/watch?v=b67o24J3bAs

At 27:00 Keating asks him about string landscapes and multiverses, but turns the question to whether there could be different maths in these universes, perhaps even without proof by contrapositive ("modus tollens"), or at least a different value of pi. So what is Frenkel to do with such a question? He starts talking about an alien species without numbers, such as the unitary intelligence in the movie Solaris, that would discover our same math by homotopy theory! He illustrates homotopy classes by imagining winding dental floss around your finger. Now that's a string.

Strings are otherwise not mentioned in the interview as I recall it.

While it's a popular math book, I would have liked to have had Frenkel's Love and Math in grad school, to get an overview of the specialties I was studying. It's a good book for many reasons, and can be usefully skimmed in about the same amount of time as the longer videos, or maybe twice that time.

Various and Sundry

August 24, 2023
Categories: abc Conjecture, Uncategorized

The math department at Columbia this fall will be hosting three special lecture series, each with some connection to physics (at least in my mind…):

- Sergiu Klainerman will be lecturing on the proof of nonlinear stability for slowly rotating black holes, Wednesday afternoons at 2:45.
- Nikita Nekrasov's lectures will be on The Count of Instantons, Friday afternoons at 1:30.
- The Eilenberg lectures will be given by Laurent Fargues on Some new geometric structures in the Langlands program, Tuesdays at 4:10.

Some other less inspirational topics:

- The news this summer from the LHC has not been good. On July 17 a tree fell on two high-voltage power lines, causing beams to dump, magnets to quench, and damage (a helium leak) to occur in the cryogenics for an inner triplet magnet. See here for more details. Fixing this required warming up a sector of the ring, with the later cooldown a slow process. According to this status report today at the EPS-HEP2023 conference in Hamburg, there will be an ion run in October, but the proton run is now over for the year, with integrated luminosity only 31.4 inverse fb (target for the year was 75).
- The Mochizuki/IUT/abc saga continues, with Mochizuki today putting out a Brief Report on the Current Situation Surrounding Inter-universal Teichmuller Theory (IUT). The main point of the new document seems to be to accuse those who have criticized his claimed proof of abc of being in "very serious violation" of the Code of Practice of the European Mathematical Society. This is based upon a bizarre application of the language

  > Mathematicians should not make public claims of potential new theorems or the resolution of particular mathematical problems unless they are able to provide full details in a timely manner.

  to the claim by Scholze and Stix that there is no valid proof of the crucial Corollary 3.12. It would seem to me that Mochizuki is the one in danger of being in violation of this language (he has not produced a convincing proof of this corollary), not Scholze or Stix. The burden of proof is on the person claiming a new theorem, not on experts pointing to a place where the claimed proof is unsatisfactory. Scholze in particular has provided detailed arguments here, here and here. Mochizuki has responded with a 156 page document which basically argues that Scholze doesn't understand a simple issue of elementary logic.

  Also released by Mochizuki today are copies of emails (here and here) he sent last year to Jakob Stix demanding that he publicly withdraw the Scholze-Stix manuscript explaining the problem with Mochizuki's proof. Reading through these emails, it's not surprising that they got no response. The mathematical content includes a long section explaining to Scholze and Stix that the argument they don't accept is just like the standard construction of the projective line by gluing two copies of the affine line. On the topic of why he has not been able to convince experts of the proof of Corollary 3.12, Mochizuki claims that he convinced Emmanuel Lepage and that

  > one (very) senior, high-ranking member of the European mathematical community has asserted categorically (in a personal oral communication) that neither he nor his colleagues take such assertions (of a mathematical gap in IUT) seriously!

  I suppose this might be Ivan Fesenko, but who knows.

- Since the Covid pandemic started, the World Science Festival has not been running its usual big annual event here in New York. This fall they will have an in-person event, consisting of four days of discussions moderated by Brian Greene. In particular there will be a panel Unifying Nature's Laws: The State of String Theory evaluating the state of string theory, featuring four of the most vigorous proponents of the theory (Gross, Strominger, Vafa and Witten). I suspect their evaluation may be rather different than that of the majority of the theoretical physics community.

Update: Quanta has a very good article by Kevin Hartnett about the telescope conjecture in homotopy theory and its recent disproof. This is due to work of Ishan Levy, Robert Burklund, Jeremy Hahn and Tomer Schlank, all of whom gave talks on the subject at the Oxford conference this past June in honor of the 65th birthday of Mike Hopkins. Videos of the talks are available here. If homotopy theory is not your thing, and if you haven't heard Graeme Segal speak recently about his thinking on the definition of quantum field theory, you could instead watch his talk.

Update: Video of Klainerman's lectures will be available here:
https://youtu.be/CkyG4VnRXps?si=1xhZx-UyRI2I52sr

Comments

1. **Kurt Schmidt**
   August 24, 2023

   David Gross was born in 1941, Edward Witten in 1951, Andrew Strominger in 1955, and Cumrun Vafa in 1960. Why did they decide to select 3 Boomers and 1 member of the Silent Generation to host the string theory panel, rather than a younger string theorist who could speak for the younger generations?

2. **Peter Woit**
   August 24, 2023

   Kurt Schmidt,
   With Witten in the lead, all four of them jumped on the string theory unification bandwagon at the beginning in 1984-85. Since then and to the current date, they have been among the most prominent proponents of the subject. So, if you want to know what the hard-core, true believers who have been at it for nearly forty years think, this is the right panel. If you wanted a sober evaluation of where we are after those forty years, you'd choose a different panel.

3. **Low Math, Meely Interacting**
   August 25, 2023

   I don't wish to pile on the ageism, but the idea of "string unification" seems to have become rather quaint these days, hasn't it? What would such a panel have to say that would be of much relevance to even string theory's most ardent supporters at present? It's been many years now that all the press seems to be about using string-adjacent techniques to make toy models of black holes, wormholes, condensed matter and so forth. Witten himself explicitly capitulated on anthropism, so what more is there to discuss about the parochial details of our pocket of the multiverse?

4. **Peter Woit**
   August 25, 2023

   LMMI,

Of the five (including Greene), Vafa is the only one still trying to make an argument that string theory has some implications for the real world (his "swampland program"). All the others have completely given up on this, but continue to try to sell this failure as some sort of success. Andy Strominger takes this the furthest, see for instance here
https://www.math.columbia.edu/~woit/wordpress/?p=11138
dealing with the failure of 1980s hype by claiming that string unification would have been boring small potatoes:
"I think that the idea that people were excited about back in 1985 was really a small thing, you know, to kind of complete that table that you put down in the beginning of the spectrum of particles…"
Now he's replacing that with new, improved hype about space-time. It seems to me that what he learned from the past was that hype works, best to just keep at it.

5. **David Roberts**
August 28, 2023

Perhaps if Mochizuki can convince Lepage to publicly state he thinks the IUT papers are all correct, and *why* the proof of Corollary 3.12 in IUT3 is solid, then Mochizuki might have a tiny bit more support. At present, it seems like hearsay. Also, all this vague references to senior experts etc without saying who they are is of rather little worth without them being public in their support.

6. **Peter Woit**
August 28, 2023

David Roberts,
Mochizuki gives a list of those who he says have worked on understanding the Scholze/Stix criticism of the proof:
""Yuichiro Hoshi, Go Yamashita, Arata Minamide, Akio Tamagawa, Ivan Fesenko, Mohamed Saidi, Emmanuel Lepage, Benjamin Collas."
The damning fact is that to this day none of those people has written or lectured on their supposed understanding of how the proof overcomes the Scholze/Stix objections.

As for his claim about a "(very) senior, high-ranking member of the European mathematical community", Mochizuki completely blows up the credibility of this claim by saying that none of this person's colleagues take Scholze/Stix seriously. He repeatedly has gone on like this, claiming that no one he talks to takes the objections to the proof seriously, or knows anyone who does, which indicates that he has isolated himself to a disturbing extent.

7. **closed timeline curve**
August 29, 2023

Prof. Klainerman used all his might
to deduce a new theorem last night.
It's a breakthrough for sure,
but the proof is obscure
and takes two thousand pages to write.

8. **Alex**
August 30, 2023

Is there a video of the talk by Klainerman somewhere? I'm quite interested in watching that. Thanks!

9. **Peter Woit**
September 1, 2023

Alex,
I don't know if there will be video of one or more of these three lecture series this fall. If there is, I'll add a link here, also likely available at Columbia math dept. youtube channel.

10. **Timothy Chow**
September 2, 2023

I was under the impression that Scholze and Stix did not publish their report, but gave it to Mochizuki privately, for him to do with it as he wished. Mochizuki then chose to post it on his own website. Do I have the facts right? If so, then it would seem strange for Mochizuki to request that Scholze and Stix "publicly withdraw" their "manuscript" when Scholze and Stix were not the ones to publicly release the report in the first place.

11. **Marvin**
September 9, 2023

I can't make it for the Laurent Fargues presentation on the 12th, anybody know if it will be available online after? Thanks

12. **Peter**
September 12, 2023

Will the Nekrasov lectures be made public?

13. **Peter Woit**
September 15, 2023

Peter,
Yes. First should appear on dept website next week.

What Does Spec Z Look Like?

September 14, 2023
Categories: Langlands

This week Laurent Fargues has started a series of lectures here at Columbia on Some new geometric structures in the Langlands program. Videos are available here, but unfortunately there is a problem with the camera in that room, making the blackboard illegible (maybe we can get it fixed…). Fargues however is writing up detailed lecture notes, available here, so you can follow along with those.

Fargues is covering the story of the Fargues-Fontaine curve and the relationship between geometric Langlands on this curve and arithmetic local Langlands that he worked out with Scholze recently. On Monday Scholze gave a survey talk in Bonn entitled What Does Spec **Z** Look Like?, video available here. Scholze's talk gave a speculative picture of how to think about the global arithmetic story, with Spec **Z** as a sort of three-dimensional space. One thing new to me was his picture of the real place as a puncture, with boundary the twistor projective line. He then went on to motivate the course he will be teaching this fall with Dustin Clausen on Analytic Stacks. Here at Columbia we have an ongoing seminar on some of the background for this, run by Juan Rodriguez Camargo and John Morgan.

Update: Peter Scholze next week at the Rapoport conference will be giving a talk on new ideas about the twistor $$\mathbf{P}^1$ and real local Langlands. His abstract is

> Towards a formulation of the real local Langlands correspondence as geometric Langlands on the twistor-$\mathbf P^1$
>
> We will propose a formulation of the local Langlands correspondence for complex representations of real groups in terms of a(n everywhere unramified) geometric Langlands correspondence on the twistor-$\mathbf P^1$, analogous to our work with Fargues in the case of p-adic groups. This is motivated by discussions with Rodriguez Camargo, Pan, le Bras and Anschütz on the analogous case of locally analytic p-adic representations, and is different from the previous work of Ben-Zvi and Nadler in a similar direction. In particular, on the geometric side we get representations of the real group, encoded in terms of liquid quasicoherent sheaves on $$; and on the spectral side, we get representations of the real Weil group W_R, or rather vector bundles on $$.

Comments

1. **John Baez**
   September 15, 2023

   Peter wrote:

   "Scholze's talk gave a speculative picture of how to think about the global arithmetic story, with Spec(ℤ) as a sort of three-dimensional space."

   While there's speculation in Scholze's talk, I want to let beginners know that the 3-dimensional nature of Spec(ℤ) is not a speculative thing. In 1973 Barry Mazur worked out the etale cohomology of Spec(ℤ) ∪ {∞} and showed it has nontrivial cohomology up to and including dimension 3:

   http://www.numdam.org/articles/10.24033/asens.1257/

   (Apparently he also showed its etale cohomology vanishes in higher dimensions 'up to 2-torsion'. Has anyone later computed the 2-torsion?)

   There has subsequently been a lot of work on how Spec(ℤ) ∪ {∞} resembles a 3-sphere and primes act like knots in this 3-sphere. Here are some relatively easy ways to learn more:

   Chao Li and Charmaine Sia, Knots and primes, https://www.julianlyczak.nl/seminar/knots2016-files/knots_and_primes.pdf

   Barry Mazur, Thoughts about primes and knots, https://www.youtube.com/watch?v=KTVEFwRbuzU

   Masanori Morishita, Analogies between knots and primes, 3-manifolds and number rings, https://arxiv.org/abs/0904.3399

   For people who know a fair amount of math but are in a rush, I recommend just checking out this analogy table from Li and Sia's course notes:

   https://math.ucr.edu/home/baez/mathematical/knots_and_primes_1.jpg

2. **jack morava**
   September 15, 2023

   what John said.

3. **Peter Woit**
   September 15, 2023

   John,
   Thanks for writing up that background!

   The word "speculative" might not have been the right one. For the things John refers to it's important to keep in mind that these are just analogies. Spec Z can't be an actual 3-manifold.

   In the talk Scholze explained how his work with Fargues on the relation to geometric Langlands fits in with these analogies. My understanding now is that near the puncture corresponding to the infinite prime the "space" should have an action of the positive reals, looking like the twistor P^1 for a fixed value. Near a finite prime it should again have such an action, looking like a torus at a fixed value. Fargues has promised me he'll explain this in detail in his lectures. This is all supposed to fit in with work of Deninger in which there's a foliated space with a flow acting on it analogous to the Frobenius. Note that Deninger's program is aimed at the Riemann hypothesis, so these are deep waters…

   Perhaps what is "speculative" is the idea that there is some sort of global version of the Fargues-Scholze story (which is local at each prime), and that this global version would give not just an analogy, but an actual space corresponding to Spec Z (for some exotic definition of what a "space" is).

4. **John Baez**
   September 15, 2023

Hmm, Scholze actually makes a cryptic reference to the 2-torsion in the etale cohomology of Spec(ℤ) starting at around 19:15 in his talk, and leading up to "if you think deeply about the etale cohomology of Spec(ℤ) then there is some funny 2-torsion phenomenon having exactly to do with Gal(ℂ/ℝ) having infinite cohomological dimension, and if you just use $\mathbb{R}P^2$ instead you get a much better answer".

I *wish* I could think deeply about the etale cohomology of Spec(ℤ). Alas I can't, but I see that Gal(ℂ/ℝ) = ℤ/2 has as its classifying space ℝP^∞, whose cohomology has 2-torsion (namely, it's ℤ/2) in every dimension. Is this what Mazur found for the etale cohomology of Spec(ℤ) ∪ {∞}?

This is pretty technical but maybe someone reading this will know.

5. **John Baez**
September 15, 2023

Sorry, I meant "in every even dimension".

6. **jsm**
September 15, 2023

Actually, it was Artin and Verdier who first worked out the etale cohomology of things like Spec(Z) (Artin-Verdier duality), but it was Mazur who popularized it. Let U=Spec(Z). It is possible to define a cohomology of U "with compact support" that fits into a long exact sequence
H_c^r(U.F) -> H^r(U,F) -> H^r(Spec R,F)->
The cohomology with compact support behaves more or less as it should, but the etale cohomology of the real numbers is just Galois cohomology with Galois group Z/2Z which has periodic cohomology, and so goes on for ever. Artin used to say that "Spec Z is singular at infinity".

7. **Thomas**
September 15, 2023

The lectures by Gross which Fargues mentions in his talk seem to be available as text at: http://www.math.columbia.edu/~chaoli/docs/EilenbergLectures.html

8. **jack morava**
September 15, 2023

IIRC the mod two stuff is in the original Artin-Tate seminar notes where they calculate the Galois cohomology of the idele-class group and wrestle with the solenoid?

9. **John Baez**
September 16, 2023

Thanks, jsm!

10. **DrDave**
September 17, 2023

Re: Black (Green) board legibility:
The YT video is super low resolution, below what most phones and cameras record,
so there is a good chance there is an original video with 720p or higher resolution. If so, it would be simple to boost the sharpness and contrast.
However, if the presenter has slides or notes, then that is even easier.

11. **yyb**
September 17, 2023

I don't know if this is related to analytic stacks or not, but 4 months ago someone asked on MO about Dustin Clausen's talk about the "modified Hodge Conjecture" here https://mathoverflow.net/questions/446978/clausens-modified-hodge-conjecture/446992#446992 and Clausen's answer is that the modified Hodge conjecture is related to "analytic k-theory" that's possibly related to analytic stacks that they're going to talk about this fall.

12. **Peter Woit**
September 17, 2023

DrDave,
Yes, the problem with the video is the very low resolution, which is what Columbia's zoom configuration gives for cloud-stored recordings. Currently discussing this with the Columbia zoom people.

13. **Will Sawin**
September 17, 2023

If I can add some detail to what jsm said, the compactly-supported cohomology behaves as it should in that it has a form of Poincare duality which in particular means it vanishes in degrees above 3. So together with the fact that the cohomology of Spec R with coefficients in Z/2 is equal to Z/2 in every nonnegative degree, that immediately tells you that the ordinary cohomology of Spec Z with coefficients in Z/2 is equal to Z/2 in every degree >3.

Note that this kind of situation is exactly what you see for noncompact manifolds – a long exact sequence between the ordinary cohomology, the compactly-supported cohomology, and the cohomology of a neigborhood of infinity. So from this perspective Spec Z behaves like a non-compact 3-manifold, except that a neighborhood of infinity is homotopic to the classifying space of Z/2Z, i.e. to infinite-dimensional real projective space. So somehow we have a 3-manifold which near infinity looks infinite-dimensional.

Somehow the twistor P^1 must be related to this picture by the close relationship between the twistor P^1 and two-dimensional real projective space and some kind of approximation between infinite-dimensional real projective space and two-dimensional real projective space.

Let me also point out that the notion of a 3-manifold with an action of R should not be very mysterious. A very classical way of producing a 3-manifold is to start with a surface and an automorphism of the surface, take the surface cross the interval, and then glue the two sides using the automorphism, producing a 3-manifold fibered over the circle. Such 3-manifolds naturally carry an action of R (locally acting by translation by the interval), whose periodic orbits correspond to fixed points of powers of the automorphism.

The relevance of this to Spec Z (at least in a way that makes sense to me) goes something like this: An algebraic curve over a finite field is fibered over the spectrum of the finite field with fiber an algebraic curve over an algebraically closed field. Algebraic curves over C are Riemann surfaces, so the fiber behaves like a surface, and finite fields have Galois group Z-hat, so the spectrum of the finite field behaves like the classifying space of Z (fields behaving like classifying spaces of their Galois group), and thus like the circle. So the curve behaves like a fibered 3-manifold, with the automorphism being the generator of the Galois group, i.e. Frobenius. Periodic orbits correspond to fixed points of powers of Frobenius which are the same thing as prime ideals of the ring of functions on the curve.

But Z behaves like the ring of functions on a curve since both are Dedekind domains of finite type, so its spectrum should behave like a 3-manifold with an action of R whose periodic orbits correspond to prime ideals, i.e. primes.

14. **Will Sawin**
September 18, 2023

After watching the video, indeed it seems to be RP^2, i.e. the quotient of the twistor P^1 by complex conjugation, rather than the twistor P^1 itself, that appears.

The fact that it's RP^2 / the twistor P^1 that appears seems to be a way of capturing Hodge theory, so I guess the reason we see RP^infinity from the perspective of the étale cohomology of Spec(Z) is that étale cohomology doesn't really capture Hodge theory.

15. **John Baez**
September 19, 2023

Will Sawin wrote:

"But Z behaves like the ring of functions on a curve since both are Dedekind domains of finite type, so its spectrum should behave like a 3-manifold with an action of R whose periodic orbits correspond to prime ideals, i.e. primes."

I haven't had time to watch the whole video yet – just the first part. So, if anyone feels like it, I'd love to hear how close Scholze is to formalizing this hand-wavy but delightful intuitive picture using his new ideas.

(I'll watch the rest as soon as I have time.)

16. **Will Sawin**
September 20, 2023

@John Baez: The thing that most surprised me about the construction is that the action of R is completely literal – there is really a one-parameter continuous (in the appropriate sense) family of automorphisms (or endomorphisms – if I understand right one starts with a space with a semigroup action and can use it to construct a space with a group action by a general construction). I wouldn't have expected such an action to literally exist on any reasonable variant of Spec Z.

On the other hand, I don't think Scholze made any claims in the talk about the space being like a manifold. Well, the space is defined by starting with an (suitably analytified) spectrum of a ring of power series, so I guess one can locally (in a certain sense) express functions on the space as power series, which makes it like a manifold, but this is somewhat indirect (and anyways these are 1-variable power series, not 3-variable). Since the space is closely related to Spec Z there perhaps exists some cohomology theory for it which recovers the cohomology of Spec Z and thus makes the space behave homologically like a 3-manifold.

For the intuition about Spec Z locally looking like certain spaces at certain points, the talk sketches some local picture along these lines. This is localization in the algebraic geometry sense, i.e. potentially arbitrary maps from another space to your space, so it doesn't completely fit the handwavy intuition of literal open sets.

17. **anon**
September 20, 2023

Presumably this 3-manifold is still very prickly – spine like, like the 1-skeleton it maps to. I mean, do the primes 3 and 5 (say) interact only at the generic point? Or do we expect the orbits around 3 wind around the orbits around 5 (and so forth), producing Massey products or some such, in an as yet unseen global theory? That would be rather shocking. But if not, what number theoretic information is this 3-manifold picture going to carry?

(I'm still reading Bhatt's wonderful prismatic gauge lectures, and havent absorbed the advances at p – neither in stacky language nor via TCH etc. So I'm somewhat behind.)

18. **jack morava**
September 20, 2023

… do the primes 3 and 5 (say) interact only at the generic point? … Or do we expect the orbits around 3 wind around the orbits around 5 (and so forth), producing Massey products or some such, in an as yet unseen global theory? That would be rather shocking.…

For such products see Morishita, Masanoi, Milnor invariants and Massey products for prime numbers, Compos. Math. 140 (2004) MR2004124, and John B's citation for Morishita upthread.

19. **anon**
September 20, 2023

Thank you Jack! That's very pretty. So "of course" primes p,q interact – quadratic reciprocity is such an interaction. Properties of global etale cohomology of Spec of a number field already allow one to see this kind of phenomena as a Massey product. Scholze's 3 manifold \`Spec Z' would give a \`cycle theoretic' interpretation of this as an actual linking number between actual R-orbits. And presumably much more?

Any predictions on what new \`hard' information such a picture of Spec Z would tell us?

(I'm already blown away by the stacky/de Rham intepretation of d dimensional varieties over finite fields having etale cohomological dimension 2d+1; Bokstedt periodicity and Bott, R[u,v]/uv-p. And that's just the local picture.)

Also: is it trolling on this blog to ask whether there are \`arithmetic' versions of 3+d-dim TFTs, and what they assign to d-dimensional arithmetic varieties? And what an honest number theoretic punchline of having the evident analogous structure would be?

The enormous gulf between the easy to predict analogies, and the often simple but unknown definitions needed to make them work never ceases to amaze.

20. **Another anon**
September 21, 2023

This is maybe a little bit off-topic, but there is a Simons Collaboration on Perfection in Algebra, Geometry, and Topology to tackle major open questions in number theory, algebraic geometry, p-adic geometry, commutative algebra, and algebraic topology here: https://scop.math.berkeley.edu/ with 13 people as principal investigators (including Scholze, Clausen, Bhatt, Lurie, Mathew, etc) with Martin Olsson as the collaboration director.

21. **David Ben-Zvi**
September 23, 2023

@anon: Arithmetic (Topological) Quantum Field Theory is in fact a thriving research area, initiated by Minhyong Kim. You can see more about the topic from the talks at this seminar https://homepages.warwick.ac.uk/~u1972347/AGQFT.html and there will be a program on Arithmetic QFT at Harvard's Center of Mathematical Sciences and Applications Feb-March 2024, with a research conference March 26-29.

22. **Kirti Joshi**
September 24, 2023

@Dr. Woit: In the context of the current discussion here, let me point out my preprint, Constructions II(1/2), posted on the arXiv on May 17, 2023. My paper furthers the analogy between Riemann surfaces and Number Fields and establishes a Teichmuller Theory of Number Fields. Specifically in the context of this discussion here, let me say that in my work I construct a global topological object which resembles Fargues-Fontaine curves (of both Y and X types) at all primes of the number fields simultaneously.

My treatment at archimedean primes is different from Fargues's treatment, but also closer to the original Banach space approach of Fargues-Fontaine at finite primes. Notably, I work with a certain (infinite dimensional) complex Banach algebra which serves as the ring of functions on the archimedean Fargues-Fontaine curve (of my theory)–and this curve appears as the spectrum (of maximal ideals) of this Banach algebra of my theory–just as the closed maximal ideals of the Fargues-Fontaine algebra B are related to the p-adic Fargues-Fontaine curve.

This global object which I construct is equipped with a natural action of the multiplicative group of the number field which at each prime operates through Frobenius morphism (at each finite prime) and through the multiplicative group at archimedean primes (I assume that the number field has no real embeddings for simplicity). [Further properties of this global object are established in my paper.] This global topological object is metrisable and locally i.e. at each prime of the number field, this object is equipped with the natural metric provided by Fargues-Fontaine theory.

I show that the points of this global topological object parameterize distinct arithmetic avatars of the number field just as points of the Teichmuller space of a Riemann surface parameterize different complex function theories (i.e. distinct complex structures) on a fixed topological surface.

Hence my work further deepens the analogy between Number Fields and Riemann surfaces.

Each avatar of the Number Field (called an arithmeticoid in my paper) also provides holomorphic structures (at all primes) in the sense of Berkovich analytic spaces (at archimedean primes one obtains analytic spaces) and I show that deforming the avatar of the number field also deforms Berkovich analytic structures. This has consequences for anabelian geometry related to Mochizuki's IUT. Mochizuki, in his IUT, has asserted the existence of deformations of arithmetic structure of a number field whose existence I have established in my work.

Current versions of my preprints related to this can be found on the arXiv using this link or through my webpage. I will be happy to answer any questions.

23. **Kirti Joshi**
September 27, 2023

Let me further remark on one important aspect of my global arithmetic construction alluded to in my comment above. My (global) construction has certain automorphisms/symmetries (at all primes) which have a nice geometric counterpart which appears in the Virasoro Uniformization Theorem of Beilinson-Schechtman (also Kontsevich) via action of automorphisms of a punctured formal disc–detailed, for instance, in the Ben-Zvi–Frenkel book in 17.3.2). The Virasoro uniformization and related actions plays an important role in Beilinson-Drinfeld work on Geometric Langlands, and is also of significance in Conformal Field Theory.

Mochizuki, in his IUTT, asserts that automorphisms of certain unit groups change arithmetic holomorphic structures (he calls this property Indeterminacy Ind2).

In my Constructions I paper, I have proved a precise version of this important assertion. [Informally speaking, my approach to establishing this claim is that one should work with the automorphisms of the perfectoid punctured unit disc–precise statements are in my paper. In my Untilts paper, I provide a natural geometric construction of arithmetic holomorphic structures which also establishes their existence in Mochizuki's context.]

So the global object I construct is at once geometric and arithmetic in nature and allows me to prove a precise version of Mochizuki's claim (alluded to above). My approach also agrees with Fargues-Fontaine "holomorphic functions of the prime number p" point of view (at all primes p) and automorphisms of the relevant unit group provides "holomorphic changes of the coordinate p" (p is any prime) in a manner entirely analogous to the Beilinson-Schechtman setting and also agrees with Mochizuki's multiplicative monoidal approach to thinking about number fields and varieties over them. Minhyong Kim's theory requires working with fundamental groups arising from distinct base-points (and paths connecting them)–and my theory (and Mochizuki's IUTT) requires working with tempered fundamental groups arising from distinct base-points.

Given the close parallel the above described object and its symmetries bear to its counterparts in Geometric Langlands (and Conformal Field Theory), it would be interesting to see if it plays an analogous role in the corresponding arithmetic contexts.

24. **Yonz**
September 29, 2023

Will the Rapoport conference talk be available online? I'm quite interested in watching that. Thanks!

Frenkel on String Theory

September 20, 2023
Categories: Uncategorized

Curt Jaimungal's Theories of Everything podcast has a new episode featuring a long talk with Edward Frenkel (by the way, I'll be doing one of these next month). A few months ago I wrote about a Lex Fridman podcast with Frenkel here. While both of these are long, they're very much worth watching.

While there's some overlap between the two podcasts, some different topics are covered in the new one. In particular, one thing that happened to Frenkel since last spring is that he attended Strings 2023 and gave a talk there (slides here, video here). The experience opened his eyes to just how bad some of the long-standing problems with string theory have gotten, and starting around here in the podcast he has a lot to say about them.

It's pretty clear that his reaction to what he saw going on at the conference was colored by his experience growing up in late Soviet-era Russia, where the failure of the system had become clear to everyone, but you weren't supposed to say anything about this. He pins responsibility for this situation on senior leaders of the field, who have been unwilling to admit failure. As part of this, he acknowledges his own role in the past, in which he was often happy to get some reflected glory from string theory hype by playing up its positive influence on parts of mathematics while ignoring its failure as a theory of the real world. In any case, I urge you to watch the entire podcast, it's well worth the time.

For a very different perspective on the responsibility of senior people for string theory's problems, you might want to take a look at the bizarre twitter feed of stringking42069, which may or may not be some very high-quality trolling. In between replies and tweets devoted to weightlifting, weed and women, the author has some very detailed and mostly scornful commentary on the state of the field and the behavior of its leaders. His point of view is that the leaders have betrayed the true believers like himself, abandoning work on the subject in favor of irrelevancies like "it from qubit", in the process tanking the careers of young people still trying to work on actual string theory. For a summary of the way he sees things, see here and here. Comments on specific people here and here.

This weekend here in New York if you've got $35 you can attend an event bringing together five of the people most responsible for the current situation. I doubt that the promised evaluation of "a mathematically elegant description that some have called a "theory of everything."" will accurately reflect the state of the subject, but perhaps some of the speakers will have listened to what Edward Frenkel has to say (or read stringking42069's tweets) and realized that a new approach to the subject is needed.

Update: Curt Jaimungal at the Theories of Everything podcast has a new episode, discussing quantum gravity with Jonathan Oppenheim. Around 1:10 Oppenheim has some comments about the current problem of few opportunities for young people to pursue new ideas in this field, including:

> You know, it's a multifaceted problem. I think part of it is that for whatever reason, people like to work on the same thing as everyone else. And I mean, we are social creatures, and we want to be part of the community. And so if there's a big community doing something, then it's very natural to want to be part of that community and do that research.
>
> But it's, I feel like it's gotten to quite an extreme. It feels quite extreme at the moment, I feel like even when I was a student, you know, there were various researchers who, I would say, didn't have a firm allegiance to say, string theory or loop quantum gravity, and you could kind of work with one of them and work on your own approach. Whereas I think now, for whatever reason, the landscape has just become a lot more divided into different communities who do different things, and it's much harder to go off on your own. And maybe that's just because it's students' worry that if they go off on their own, they won't get a job. I think that's probably a big part of it.

Update: Bringing together this and the last posting, if you'd like more Frenkel and more Langlands, there's a new Numberphile video out.

Comments

1. **Z Y**
   September 20, 2023

   That's a who's who ensemble of ST at NY, a kind of last hurrah, have these four came together at a public/scientific conference panel before? Hopefully a video will be uploaded somewhere.
   The state of the matter is such, specially on laymen circles, that I reckon it will be disappointing if none of them at least show some reflection about the urgent need to explore new ideas outside of ST if we are going to make any progress at fundamental physics

2. **Peter Woit**
   September 20, 2023

   ZY,
   I assume a video will be available at some point. I can't remember an event ever that brought all of them together, especially not to discuss the state of string theory.

   It is a remarkable group in the sense that each of them has played and continues to play a large role in the hyping of string theory, with most of them at it for nearly forty years. When Frenkel refers to those who have responsibility for creating the problem with string theory and for now doing something about it, it's very much this group.

3. **Allen M.**
   September 20, 2023

   Peter,

   I have been reading your blog for a decade. To see you being vindicated in this way warms my heart.

   Thank you for everything.

4. **Peter Woit**
   September 21, 2023

   Allen M.,
   Thanks, but I'd much rather see something positive getting done about the problems with fundamental theoretical HEP research that I've pointed out, and unfortunately I don't see that happening. Rather, the field shows little sign of facing up to its problems, instead just goes from bad to worse.

   If we're in an analog of the end-days of the failed Soviet system, there's no reason to be sure something better lies in the future.

5. **Hiro Kawabata**
September 21, 2023

Another event that may be of interest.
On that may be more forward looking:

"Puzzles in the Quantum Gravity Landscape: viewpoints from different approaches"

https://events.perimeterinstitute.ca/event/47/

Oct 23 – 27, 2023
Perimeter Institute for Theoretical Physics

6. **Alessandro Strumia**
September 23, 2023

Dear Hiro, the String King 42069 (Lubos 2.0?) account recommended by Peter writes that the conference you recommend is "crackpots", see https://twitter.com/stringking42069/status/1700574269573374384

A problem in the quantum gravity community is the lack of criticism towards those theorists who make big claims based on weak logic. The conference you suggest has an unnecessary Code of Conduct that limits the open criticism needed to keep untestable quantum gravity serious, see https://events.perimeterinstitute.ca/event/47/page/164-respectful-environment-code-of-conduct.

Even worse, the registration page forces participants to endorse partisan political values and beliefs, such as equity and μaggressions. Most physicists might not know the hidden political meaning of these kind of words, designed to sound nice. As this political stuff has nothing to do with quantum gravity, I cannot attend this conference.

7. **Low Math, Meely Interacting**
September 23, 2023

Thanks for the pointer to Curt Jaimungal's podcast. Among other content I find less edifying are some real gems, foremost this discussion. Frenkel is a remarkable human being.

He rightly emphasizes his observation of a phenomenon described many times elsewhere (notably by you and Sabine Hossenfelder) in regards to this subject, namely the lengths human beings will go to avoid cognitive dissonance. For those with a passing interest in psychology, it's a well-established fact that we see it all around us while being almost incapable of recognizing how profoundly it shapes our own thoughts and actions. It's the mechanism that allows groupthink to propagate and overwhelm all collective rational thought. Every leader depends on it to stay focused, and every apparatchik to stay sane.

I agree with you, Peter, that it's the unique liability of the true genius that he or she becomes convinced their intellectual powers make them exceptional in ways they are not. Frankel shows great integrity and compassion in pointing out there's no shame in being ordinary in this way. But rarely are we rewarded for admitting it, despite the good it would do the individual and their community.

8. **Peter Woit**
September 23, 2023

Alessandro Strumia,
It seems quite possible StringKing is a parody account of a clever young theorist making fun of string theory fanatics (e.g. the answer to all problems is to read Polchinski???).

Given my allergy to moral posturing and those who want to enlist everyone in their self-righteous crusades, I'm not very sympathetic to having long codes of conduct like the Perimeter one and requiring people to sign them. If StringKing showed up in real life at a conference behaving like on Twitter (unlikely….) maybe you would need a code of conduct to deal with him.

Your idea that this kind of code of conduct will interfere with appropriate scientific criticism would apply more to the last conference at Perimeter (Strings 2023), where Frenkel's perception of a culture not allowing valid criticism I think was accurate. This next conference seems to me one more aware that all known programs have serious problems, that what is needed is new ideas.

All, please no more comments about Perimeter's code of conduct or attempts to carry on woke/anti-woke warfare here.

9. **Michael Weiss**
September 23, 2023

Thank you for sharing the link to Curt Jaimungal's interview with Prof. Edward Frenkel. It is extraordinary both for its perspectives and for the insights it indirectly provides into a remarkable character. His life story beginning in the former USSR is inspiring.

Returning to the interview, Prof. Frenkel's emphasis on what he describes as "self-awareness" is pertinent not only to the scientific community, but to larger trends in our society more broadly.

As an aside, Frankel's candid comments on your courage (and that of scientist-observers Drs. Lee S and Sabine H; i.e., to have highlighted unfortunate sociological trends in hep-th more than 15 years ago), a critique sadly dismissed by some hep-th leaders at that time as "sour grapes," surely must be an important validation, if years too late. This is as close to a thank-you as anyone can expect in this world.

10. **Photino Birds**
September 23, 2023

You dislike if people work on string theory. Do you also dislike if people stop working on string theory?

I think the situation is IMPROVING. The old senior people stopped working on string theory, but they still say that they believe in string theory. It might be they are too embarrassed to say that their theory failed after dedicating decades to it and subconsciously think it somehow will come back as the main research topic again. They think currently progress is not happening in string theory so they temporarily work in semiclassical gravity or holography.

The younger generation is much different. People like Daniel Harlow, Geoff Penington, Sabrina Pasterski etc NEVER call themselves as string theorists. They just say they are high-energy theorists who work on quantum gravity etc. Among the young, only some swamplanders have hope that string theory will come back as the main theory. Young people who work on other topics like AdS holography (It from Qubit etc.), Celestial holography, etc don't have much hope in string theory. I think they are just hoping a better theory will come in the future. They are also hoping this better theory will be holographic. These people will not say string theory is dead as it will hurt the senior people's feelings. I think once the old generation retires, these people might rename the conference names from "strings 20xx" to "Quantum Gravity and QFT 20xx".

What they are doing now is AT LEAST better than still continuing to do the old string theory. I can't say the progress is impressive, but at least it is decent. I hope that more new ideas come out in this decade. Also I hope the job market will improve in the future, in the past people like James Sully and Aitor Lewkowycz who did great research could not get a professor job. But instead of being more optimistic, you, Sabine and String King etc are very pessimistic.

11. **Alessandro Strumia**
    September 24, 2023

    Some sharp opinions by StringKing agree with what I privately hear from European physicists, for example during coffee breaks. By the way, coffee breaks is where dangerous doubts on string theory were discussed before blogs around 2000. During talks people said "thank you for the insightful talk, I have not fully understood a point at slide 3", and criticism was never openly expressed. But, during coffee breaks, the same people said "I fell asleep at slide 3, wtf did he talk about?". At the time, the main string topic was pp-waves, deadly soporific after lunch. That's when joint seminars started being useless, and other physicists stopped attending technical string talks.

12. **Peter Woit**
    September 24, 2023

    Alessandro Strumia,

    I'm pretty much in agreement with StringKing on his argument that the senior people in string theory have given up on the subject and are now pursuing trendy irrelevancies, and don't doubt this is also what most informed people in the subject are thinking.

    The reason StringKing seems to me likely a troll/parody account is the pairing of an intelligent/well-informed take on current research in the subject with over-the-top stupid material about weed/exercise/bros/broettes and fixation on the idea that the way forward is to re-read Polchinski and pursue old failed research programs. Most likely explanation for this pairing is someone trying to make those still pursuing string theory look like idiots.

13. **Peter Woit**
    September 24, 2023

    Following up on my last comment. StringKing posts continually, except during typical sleeping hours in Europe. At the same time, his writing style is comes off as "dumb-ass American". I'm betting parody account by a European theorist wanting to make fun of string theorists, specifically American ones.

14. **Andrew**
    September 24, 2023

    Peter.

    I'm not sure how much you care about the identity of StringKing, so if it seems this is getting a bit obsessive, feel free to ignore.

    I think it quite likely he is based in London, or at least the UK. He mentions participating in a half marathon this morning, and there happened to be one where I live (London) this morning, starting at approx the time he tweeted about it (sorry Xed). I only know this because I live close to the finishing line of the event and heard all the noise it created, I don't normally follow such things.

    There were some other such events in Europe today, but not in the USA. He seems to me to be a native English speaker, doesn't make the odd slip ups or strange word choices that good but non-native English speakers tend to make.

    Is he a troll? Of course anyone can look up half marathon dates, so it doesn't prove anything, just circumstantial evidence. I take your point that he's making string theory look bad, and why would a real string theorist do that, but the behaviour of a certain Czech blogger (only the most extreme example anongst many tbh) leaves me unconvinced.

15. **Peter Woit**
    September 24, 2023

    Andrew,
    UK/London based seems quite possible, early on he was discussing UK string theorists in great detail, before those on the continent.

    Mainly curious whether my troll hypothesis is correct.

16. **Kurt Schmidt**
    September 24, 2023

    Joseph Conlon thinks that StringKing is from Tennessee or somewhere in the southern United States, since that account follows a number of Tennessee state politicians on twitter:

    https://twitter.com/JosephPConlon/status/1703115644852469981

    https://twitter.com/JosephPConlon/status/1703117784027144219

17. **Peter Woit**
    September 24, 2023

    Photino Birds,
    Your description of the situation as young people knowing very well that string theory is dead but "These people will not say string theory is dead as it will hurt the senior people's feelings" agrees with what I'm seeing and what Frenkel was seeing, but I think both of us see something much worse going on than people not wanting to hurt someone's feelings.

    The reason young theorists don't publicly say "actually Woit/Smolin were right in 2006, string theory was on its deathbed then and it's dead now" is not that they don't want to hurt someone's feelings, but that they don't want to end their career. A field where people can't honestly discuss the scientific status of ideas because doing so could be career-ending is a very unhealthy one, and one not likely to return to health soon.

18. **martibal**
    September 25, 2023

If the young people know that string theory is dead, why do they start a career in this topic ? Is is at this point impossible to make theoretical high energy physics in the US if not in string ? Here in Italy, or in Europe in general, we have often some students aiming at doing theoretical / mathematical physics and choosing to do algebraic qft, qft on curve spacetime, noncommutative geometry etc. Of course their job situation after the PhD is not easy (to say the least) but I am not sure this is so much better in string.
Nobody put a gun against young master students in physics to force them to enter string.
Maybe a problem is the fascination for big names: "Witten as the new Einstein" etc If so, then maybe the "best minds" (to take the terminology that string people like) actually do not go to string anymore, because the "best mind" might have understood that the star system is not a good indication of the health of a research area.

19. **André**
September 25, 2023

@martibal

My impression is that the confrontation of students with current research is too little, too late. Hence, I think that the majority of PhD students at the beginning of their PhD only has a very limited understanding of the status of the type of research they are getting into.
I think it would help, if the focus in teaching would shift a lot more from 'things we understand' to 'things we don't understand'.

T.b.h., when I graduated and started my PhD in 2010, the main reason I didn't end up in string theory was that by pure luck I attended a seminar on algebraic geometry, of which I did not understand the slightest bit, but which was taught by a string theorist turned mathematician who, in a private discussion, told me to read the books by Smolin and Woit before deciding to go into string theory. If I had randomly picked some other math lecture that year to attend just for fun, who knows, I might be preaching the word of Polchinski on used-to-be-twitter today.

20. **Peter Woit**
September 25, 2023

martibal/Andre,

My impression is that a lot of ambitious young people interested in fundamental theory are making the mistake of assuming that this is a normal subject making continual progress. So, the thing to do is to work your way up through the historical progress of the subject, learning QED, standard model QFT, GUTs, Supersymmetry, supergravity, string theory, M-theory, AdS/CFT, it from qubit or whatever. Along the way, they're getting a quick superficial intro to QFT + SM from a textbook or a course, then spending a long time working their way through things like Polchinski, not aware that what they are reading about is a failed research program. They are spending the formative years of their education devoted to developing expertise in failed ideas. When they start research they're not well-equipped to have any perspective or expertise on possibly interesting areas to work in, are equipped to take whatever jobs are out there continuing failed research programs.

21. **martibal**
September 25, 2023

Peter,
I would have understood the argument 20 years ago. I remembered reading Hawking's "A brief history of time" that string theory seemed the logical step after studying general relativity and quantum field theory. Yet, during my studies I then heard people explaining what was he difference between tested science and speculative science, helping me to make my mind.
But ok, let say the information was not so easily accessible at the time (in the 90's, beginning of the 2000) and depending on which lab one were studying, there was a risk to be confronted only to the stringy ideology.
But this is no longer true. Whoever makes a little bit of research on string theory will find some critics, will end up reading your book, or Smollin's, will find this blog etc
It is hard for me to believe that people starting a PhD in theoretical physics are totally immune to the critics against string theory. If so they are (at least partially) voluntary blind.
When, as you said, they made their way to the "continuous progress" of the field, at some point they should realise there is no such continuous progress. For the example the idea of compactifying 6 or more dimensions: it may look very cool at the beginning, but quite soon one realises that the idea has been going on for years and is not settled. Which does not prevent to work on it, but being conscious that this is a highly speculative idea.

22. **P. Trinli**
September 26, 2023

Out of curiosity, could anyone expand in nontechnical terms on the issues with string theory that Edward Frenkel is pointing out? Specifically about the "Calabi-Yau turning singular"?

23. **Masoud Kamgarpour**
September 27, 2023

Frenkel's comments on String theory deserves to be cut into a separate video for easy consumption. I really like his quick explanation of what the problem is from mathematical perspective:

1. There is no way to know which six dimensional Calabi-Yau to pick.
2. Even if you somehow pick one, the manifold changes over time.
3. This changing manifold is bout to become singular and then doing any sort of computation becomes impossible (with current techniques).

His analogy with Israelites walking across the deserts was also spot on.

Congratulations to Peter (and a few others) who saw the problem early on and stuck to their position despite all the difficulties. Swimming against the current is an extremely tough and courageous act.

I think this "hyping" of one's research is not unique to physics or AI. It has unfortunately become a feature of scientific endeavour. There are many reasons for this, but one definitive one is the grant process. When I first started on writing grants, I found to my distaste that I had to hype up my research. What was frightening however was that after a few years of doing this, I actually started believing my own BS! One would have to have a lot of self-awareness not to fall into this trap!

24. **Peter Woit**
September 27, 2023

P. Trinli, Masoud Kamgarpour,

The "moduli problem" for string theory has been around and well-known since the beginning. Calabi-Yaus of a fixed topology aren't rigid objects but come in moduli spaces. The theory has to somehow explain the dynamics of these spaces, and there has never been a satisfactory answer to this. Besides the problem of singularities Frenkel points to, even locally at a smooth point, the moduli parameters will fluctuate, giving lots of universal long-range forces, in violent disagreement with experiment.

The whole “landscape” story is about Rube-Goldberg proposals (e.g. KKLT) to find dynamics that fixes the moduli values using branes, fluxes, etc. String theorists have been arguing for over 20 years about whether these proposals work. The bottom line though is that if they do work they have an ugly, complex, worthless, unpredictive theory. If they don’t work they don’t have a theory (mysteriously, this is the hope and dream of the “swampland” program). I’ve wasted too much of my life arguing with people about this nonsense, so, don’t want more discussion of this problem here. You can find hundreds of pages on the blog about it back in the early years of the blog.

25. **John Baez**
September 28, 2023

It was easy to see back in the 1980s that string theory was based on strange assumptions for which we had no evidence, so I was never tempted to seriously work on it. Perhaps it was *easier* to avoid getting into it back then: with essentially no internet, I was forced to learn physics by reading books and taking courses, so I knew general relativity and quantum field theory pretty well by the time I first heard of string theory. By now any young student has many more ways to learn physics – but they also have to wade their way through a mire of misinformation and downright disinformation.

For me the hard part was realizing that quantum gravity is not a good problem to spend much time on: not right now, anyway. I was young, overoptimistic, and brainwashed into thinking that fundamental physics was still the most exciting part of science, the way it once was. So I spent about a decade on quantum gravity before I realized that other activities were more productive and could be equally fun. It wasn’t a total waste because at least I learned some math and physics. But I hope smart youngsters are smart enough to sidestep this quagmire!

At least I hope they listen to what Frenkel is saying here.

26. **Alessandro Strumia**
September 28, 2023

Half of the moduli problem (long-range forces in disagreement with experiment) has been solved by LHC: supersymmetric particles much above the weak scale make moduli heavy. The other half is technical: finding local minima of a poorly known potential of many fields.

27. **Peter Woit**
September 28, 2023

Alessandro Strumia,
One of the current big problems with theoretical particle physics is that a large part of the community won’t recognize that you are joking…

28. **Attendee**
September 28, 2023

Meanwhile, the Simons Foundation continues supporting famous string theorists engaging in an obviously ridiculous research direction:

https://www.simonsfoundation.org/funded-project/on-the-cosmological-and-particle-physics-implications-of-the-string-swampland/

I wonder how this sort of proposal even gets positive reviews since privately a large number of string theorists mock this enterprise. But I suppose they act in unison when it comes to extracting funding dollars. I scratch your back, you scratch my back…

29. **Peter Woit**
September 29, 2023

Attendee,
Since supposedly NSF grants are hard to get, requiring very high evaluations from reviewers, I’ve never understood how grants for research topics most of the community thinks are completely misguided ever get approved. Vafa since 2020 has had an NSF grant for exactly the same topic
https://www.nsf.gov/awardsearch/showAward?AWD_ID=2013858
expiring next year.

The new Simons grant to Vafa is pretty unusual. They give very few of these “Targeted Grants in MPS”, see
https://www.simonsfoundation.org/grant/targeted-grants-in-mps/?tab=awardees
and it’s unclear how such grant proposals are evaluated. Vafa has been closely involved with the Simons Center and Simons Foundation for quite a while.

30. **Lee Smolin**
October 1, 2023

Dear all,

The problem of the dynamics of the moduli fields in K-K theory was already understood clearly by Einstein who wrote to Ehrenfest in the 1920’s that “it is anomalous to replace the four dimensional continuum by a five dimensional one and then subsequently to tie up artificially one of those five dimensions in order to account for the fact that it does not manifest itself.”

Thanks,

Lee

Spacetime is Right-handed

October 9, 2023
Categories: Euclidean Twistor Unification

I've finally managed to write up something short about an idea I've been working on for the last few months, so now have a preliminary draft version of a paper tentatively entitled Spacetime is Right-handed. One motivation for this is the problem of how to Wick rotate spinor fields, given that Minkowski and Euclidean spacetime spinors are quite different. In particular, it has always been a mystery why a Weyl spinor field has a simple description in Minkowski spacetime, but no such description in Euclidean spacetime, where the Euclidean version of Lorentz symmetry seems to require introducing fields of opposite chirality. The argument of this paper is that the relation between Euclidean and Minkowski is not the usual chirally-symmetric analytic continuation but something where both sides use just one chirality ("right-handed"). It's quite remarkable that the dynamics of gauge fields and of GR also has a chiral-asymmetric formulation.

In the ideas about unification using QFT formulated in Euclidean twistor space that I've been working on the past few years, it was always unclear why, when you analytically continued back to Minkowski signature, the left-handed Euclidean spin symmetry would not go to the Lorentz boost symmetry, but to an internal symmetry. One goal of this paper is to answer that question.

This past weekend I recorded a podcast with Curt Jaimungal, which presumably will at some point appear on his Theories of Everything site. It includes some discussion of the ideas behind the new paper.

Comments

1. **PaulS**
   October 10, 2023

   Interesting — you write: "One goal of this paper is to answer that question."

   Is there an answer in the paper? Might you be able to summarize, if so?

   Thank you!

2. **Peter Woit**
   October 10, 2023

   PaulS,
   What I'm showing in the first section is that if you identify the Lorentz group as the right $SL(2,C)_R$ factor in the orthogonal group (actually spin double cover) of complex spacetime, and identify complex spacetime as the tensor product of the spinor rep for this group and its conjugate (so, spacetime is righthanded in the sense of built out of righthanded spinors), then Euclidean spacetime will have a distinguished (imaginary time) direction and only the $SU(2)_R \subset SL(2,C)_R$ will act on it (not the $SU(2)$ from the left-handed factor, this acts trivially on spacetime).

   In this paper I haven't discussed at all how these left-handed $SU(2)$s behave in the Euclidean theory, this is what I'm trying to do with twistors. The argument about them is just that they are not going to show up as Minkowski space-time symmetries as they do in the usual analytic continuation (analytically continuing to Lorentz boosts).

3. **Hayun**
   October 11, 2023

   So, this is part of your theory of everything?

4. **PaulS**
   October 11, 2023

   Thank you for the detailed anwer! Is there an experiment that can demonstrate that "Spacetime is Right-handed"? How would right-handed space differ from left-handed space, or ambidextrous space? Thank you!

5. **Peter Woit**
   October 11, 2023

   Hayun,
   This is related to the twistor unification stuff, as explained above. But there's also a more general point, about the not well known fact that the elements of our best fundamental theory (matter fields, gauge fields, GR) can be formulated in chiral variables of one handedness. This suggest to me, independent of the motivation from twistors, that one should be thinking about the SM/GR in this kind of spinorial language, not the usual chirally symmetric vectors.

   Paul S.,
   If you're just looking at Minkowski space-time, you won't see this kind of chiral asymmetry, it just appears when you try to Wick rotate. Questions about what it implies for observed physics then depend on how the theory works in Euclidean spacetime signature, which isn't a topic of this paper.
   I'm not especially expert on various approaches to quantum gravity, possibly the asymmetry I'm pointing to may be relevant in some of them.

6. **flippiefanus**
   October 11, 2023

   What breaks the symmetry? Minkowski space is left-right symmetric. The Wick rotation is a formal process. So one can expect that the mathematics has a sign convention or something that causes the asymmetry. But is there a physical reason for it?

7. **Interested amateur**
   October 12, 2023

   Peter, if space is right-handed, doesn't this imply there's also an arrow for time in your paper; from the CPT theorem?

8. **Peter Woit**
   October 12, 2023

flippiefanus/Interested amateur,

Some comments:

The proposed asymmetry I'm writing about is not a meaningful distinction if you just look at Minkowski space-time. As far as that is concerned, there are not two SL(2,C)s, one right, one left, but just a single SL(2,C), which is the usual Lorentz symmetry, no distinguished time direction. Since there's only one, it's meaningless to say it's the right one, the left one or the diagonal one.

It does become meaningful once you start thinking about Wick rotation, or even just introducing the "i epsilon" terms that are essential to make sense of propagators (I don't share the view that this is "unphysical"). Once you do that, you are writing formulas with time complex. Then you can ask what is the symmetry group of complexified space-time, and the usual story is that there is a left SL(2,C) and a right SL(2,C), with the physical Lorentz group the (conjugate) diagonal. What I'm suggesting is that this is the wrong way to complexify, that you should take the physical Lorentz group to be the right SL(2,C).

This is different than the usual complexification, which has been the one used in rigorous QFT as in Streater-Wightman. I haven't checked, but quite possibly doing this you lose the CPT theorem. But, I'm really only interested in the Standard Model, and that respects CPT by construction. What I'm pointing out is that it may be possible to set up the SM and GR in a way that behaves quite differently when you Wick rotate, with, in particular, the left-handed part of rotations in Euclidean signature not acting at all on Minkowski spacetime, instead acting like an internal symmetry.

A comment about the "arrow of time". Once you introduce those "i epsilon"s and start thinking about imaginary time, you find that the theory is very much asymmetric in imaginary time. By Fourier transform, positivity of the energy picks out one sign of imaginary time in which you can define things (the imaginary time propagator only goes in one direction).

9. **Atiyah**
October 13, 2023

Of course this may all be silly, but to quote one of the greats on the bigger picture:

> No one fully understands spinors. Their algebra is formally understood but their general significance is mysterious. In some sense they describe the "square root" of geometry and, just as understanding the square root of −1 took centuries, the same might be true of spinors.

— Michael Atiyah [Farmelo (2009)]

10. **Michał Sykutera**
October 13, 2023

Are there any specific experimental or observational implications that might arise from your proposed "right-handed" chirality approach to the relation between Euclidean and Minkowski spacetime?

11. **Peter Woit**
October 13, 2023

Michael Sykutera,
If I wanted to emulate string theorists, I'd say this approach predicts SU(2) gauge fields with chiral couplings, so is experimentally vindicated by the weak interactions. But, more seriously, this is just part of a larger project of reformulating the standard model in a way that exploits four dimensional spinor and twistor geometry, hopefully explaining more than the standard model does, but that is still a long ways off.

12. **Low Math, Meely Interacting**
October 13, 2023

I my efforts to get my pea brain around the concept of Wick rotation, I've encountered many an internet forum wherein perplexed grad students learning QFT try to figure out what it "means" to implement such a thing. The stock answer seems to be it's just to get sensible answers out of path integrals, and it's all very rigorously dealt with, Osterwalder-Shrader, no hocus-pocus here, etc. It's kind of a trick. Maybe they bring up connections to statistical mechanics if they want to entertain a little mysticism, but otherwise the attitude of the experts is all very blasé. It's a cog in the machinery of QFT, more-or-less. Is that an accurate assessment of the typical attitude about the subject?

I ask because I'm trying to get a sense of why you've had a difficult time getting anyone interested in this idea. There are myriad possible answers to such a question, of course, but to narrow it down to the more "professional" sorts of objections you might encounter, one might be that your approach assigns a great deal more significance to the act of analytically continuing from Euclidean to Minkowskian spacetime than most folks would ever imagine. They might object that you're taking a basic fact about mathematics, i.e. that not everything is integrable, and elevating it to some deep hint about the nature of reality. And that's just not justified.

Am I being at all accurate here? I wish to make abundantly clear I am not at all attempting to debate the merits of your idea (which I am demonstrably incapable of doing), just trying to both understand it and also imagine how those endowed with the ability to debate it might react.

Thanks!

13. **Peter Woit**
October 13, 2023

LMMI,
Yes, many people may think "just a trick", but there are many arguments against this point of view. To pick one relevant to my experience: our non-perturbative info about QCD all comes from Euclidean spacetime lattice calculations. To get a workable definition of QCD that you can do numerical calculations with, you have to formulate the theory in Euclidean spacetime. The path integral for QCD simply is meaningless in Lorentz spacetime, well-defined and calculationally tractable in Euclidean spacetime.

When you look at chiral spinors and try to Wick rotate you realize something strange is going on. This paper tries to explain that, arguing that the correct Wick rotation gives you something very unexpected, a different Euclidean spacetime, with what you thought were SU(2)_L spacetime rotations actually an internal symmetry. This gives a very different way of thinking about unification which seems to me very much worth pursuing.

14. **Micha**
October 15, 2023

Abit off-topic to the spinors but, why is the "i epsilon" considered a trick? When I first learned it, I thought about it as a trick, but now I think about it in terms of the

Schwinger Keldysh path integral (closed time contour), where it appears naturally from causality. In a similar way it makes sense to me that the imaginary time correlators have an asymmetry, when one starts implementing them with a Konstantinov-Perel'-contour with KMS boundary conditions. This is also pretty much my justification for Wick rotations.

15. **Peter Woit**
October 15, 2023

Micha,
You'll have to ask those who consider it a "trick", not me. By the way, Schwinger was the one who first advocated Euclidean QFT as a consistent formalism and a fundamental idea.

16. **Gavin**
October 15, 2023

Perhaps a naive question, but if I'm understanding correctly your proposal is essentially to identify one of the SU(2) factors in the (Euclidean) Lorentz group SO(4)=SU(2) x SU(2) as the SU(2) of the weak interactions. Does this not run afoul of the Coleman Mandula theorem, which proves that no non-trivial S-matrix can involve a mixing of spacetime and internal symmetries?

17. **Peter Woit**
October 15, 2023

Gavin,
The main point of this paper is that the left Euclidean SU(2) acts trivially on spacetime, only the right handed SU(2) acts on spacetime

So the left SU(2) is an internal symmetry commuting with spacetime symmetries. Just like standard model and no conflict with Coleman Mandula.

Analytic Stacks

October 19, 2023
Categories: Langlands

Dustin Clausen and Peter Scholze are giving a course together this fall on Analytic Stacks, with Clausen lecturing at the IHES, Scholze from Bonn. Here's the syllabus:

> The purpose of this course is to propose new foundations for analytic geometry. The topics covered are as follows:
> 1. Light condensed abelian groups.
> 2. Analytic rings.
> 3. Analytic stacks.
> 4. Examples.

Yesterday Clausen gave the first lecture (video here), explaining that the goal was to provide new foundations, encompassing several distinct possibilities currently in use (complex analytic spaces, locally analytic manifolds, rigid analytic geometry/adic spaces, Berkovich spaces). These new foundations in particular should work equally well for archimedean and non-archimedean geometry and hopefully will be the right language for bringing together the Fargues-Scholze geometrization of local Langlands at non-archimedean places with a new geometrization at the archimedean place. He describes as "(very) speculative" the possibility of a geometrization of global Langlands (with Scholze more optimistic about this than he is).

Tomorrow Scholze will take over, giving the next six lectures. Perhaps this characterization is a bit over-the-top, but seeing lectures of this sort and of this ambition taking place at the IHES brings to mind the glory days of Grothendieck's years lecturing at the IHES on new foundations for algebraic geometry. I fear that keeping up on the details of this as it happens will require the energy of someone much younger than I am…

Update: Scholze's first lecture is here. He gives his version of the motivation for these new foundations.

Update: This sort of thing didn't happen back in the days of SGA.

Comments

1. **Antoine Deleforge**
   October 20, 2023

   For those who haven't watched till the end, here is a couple of interesting things from
   the Q&A:
   1) At 1:45:45 Clausen reveals that he and Scholze are trying to write a book at the same time as they are giving these lectures. He says it's not clear whether they'll release things sequantially or all at once.
   2) Around 1:46:00 a member of the audience asks whether he can describe in one or two sentences some major applications of this theory. After some hesitation, Clausen answers that it proves "the most general possible Rieman-Roch theorems in analytic geometry".

   Eventhough these topics fly way above my head, I find the progressive discovery of these massive new theories by Clausen and Scholze fascinating to watch. What I find particularly striking is that this is the 4th semester-long course that they give around Condensed Mathematics since 2019, accumulating hundreds of pages of lecture notes… yet Scholze and Clausen still haven't published any paper together to date! It's like witnessing the slow unravelling of something gigantic with likely spectacular applications, but we are still in the build up phase, with still no major results in sight, at least that they deem worthwhile of publication.

   Equally striking is that Scholze hasn't published anything new on the ArXiv for more than 2 years now. He seems to be completely all-in on this! Unless of course he has some other academic activities that I am not aware of.

2. **Jonathan**
   October 20, 2023

   Off-topic but talking about the IHES they're hosting a programme March next year "Quantum and classical fields interacting with geometry" and I notice Witten is on the list of speakers. There's been lots of work on the semiclassical approach recently in connection with black hole physics so it's perhaps good for the field to see stringy people move onto other areas.
   https://indico.math.cnrs.fr/event/9486/

3. **Yonz**
   October 20, 2023

   Antoine Deleforge,
   One "big" application or motivation that they may have is possibly the discovery of something called "analytic K-theory". If I remember correctly, Clausen said on Mathoverflow that the main motivation for condensed mathematics is to define algebraic K-theory "with proper support" which guides them to something they call "analytic K-theory" that they then use to define "modified Hodge Conjecture" in terms of their "analytic K-theory", and Clausen said that the usual Hodge conjecture is equivalent to the statement that "analytic K-theory class can be continuously deformed into an algebraic K-theory class".

   About Scholze's "upcoming paper/academic activities", you can see from a paper published in Arxiv last month by Kieu Hieu Nguyen here https://arxiv.org/pdf/2309.16505.pdf that apparently Scholze has an upcoming paper as you can see in Remark 1.7 which I will quote here:

   "A construction of Hecke eigensheaves associated with "generous" L-parameters and a proof of Harris-Viehmann's conjecture for an arbitrary reductive group as well as various foundational results would appeared in the forthcoming work of Hamann, Hansen and Scholze [HHS]"

4. **Antoine Deleforge**
   October 20, 2023

   Yonz,

   Thanks a lot for these pointers! Really appreciate.

5. **Andreas**
   October 23, 2023

Also, Garoufalidis and Zagier have a reference to a paper in preparation with Scholze, called The Habiro ring of a number field. Reference [36] in https://arxiv.org/abs/2111.06645

6. **Abbot of Nalanda**
October 23, 2023

While I have nothing useful to say about the math as this is not my field, I thought I'd make the following interesting observation: Tate, apart from being the grandfather of this young field, is also Dustin Clausen's grandfather.

7. **Peter Woit**
October 23, 2023

Abbot of Nalanda,
Yes, and another reason why watching this unfold evokes the 1960s and the transformation of algebraic geometry during those years, when Tate was one of the leaders.

8. **kitchin**
November 8, 2023

At the end of the 4th Clausen-Scholze lecture at IHES, there's a bit of a shocker, at least to me. Scholze discuses implications from, and to, the Continuum Hypothesis, due to high cardinalities.

9. **Antoine Deleforge**
November 9, 2023

kitchin,

I agree that I was a bit shocked that CH was suddenly brought up seeminlgy out of the blue! But then, my impression is that he's saying more or less "This thing is convenient to have for some proofs, but the price to pay is not to have CH." Meaning, it's a convenient tool if you are ready to get rid of CH, but there are probably other ways around in most cases. Did you understand what he presented at some deeper evidence against CH, or something like this ?

Something that surprised/impressed me from yesterday's lecture (#5) is that he presented the notion of "Solid" condensed Abelian group in a completely different way from their previous lectures, which seems much more elementary and easy to grasp. Namely, a condensed Abelian group is solid simply if its null sequences are summable (in an appropriate formal definition of this). It can be "solidified" if it is possible to assign a unique object to each sum of null sequence. Proofs of well-behaveness seem to be much easier from this definition, and he ends up showing the two definition are the same. It also explains quite intuitively why R can't be meaningfully solidified.

Answering a question at the very end of the lecture, Scholze hints that Clausen and him think they have a way to present Liquid vector spaces in a similar fashion, and are currently working out the details on whether it works. "We'll see in a few weeks". Very exciting!

10. **Nearby Goose**
November 9, 2023

kitchin: Connections between Ext-vanishing hypotheses for abelian groups and set-theoretic size issues are a classical topic – read about the "Whitehead problem" for instance. From this point of view, the appearance of these matters in lecture 4 was interesting, but maybe not so shocking.

11. **Anon**
November 13, 2023

This may be off-topic, but a week ago, Clausen gave a talk titled "What is the K-theory of the complex numbers?" at IAS/Princeton Arithmetic Geometry Seminar here: https://www.math.princeton.edu/events/what-k-theory-complex-numbers-2023-11-06t213000

Here's the abstract:

"I will explain what the question means and how to make it precise. Then I will give a conjectural answer.

This is based on joint work with Peter Scholze."

You can find the video on IAS YouTube channel.

12. **Anon**
November 14, 2023

Now after I've watched the Clausen talk at IAS here: https://www.youtube.com/watch?v=5QE__xdYtA0 I see that he's promoting the course on analytic stack with Scholze at 34:56 which makes contact to his IAS talk to some extent.

In this talk, Clausen describes his joint work with Scholze in creating something they called Analytic K-Theory, and Clausen also lists some conjectures regarding Analytic K-Theory and some evidence as to why they should be true (including the fact that they proved the conjectures for degree less than or equal to zero). I would guess that they have a paper together on this topic, but haven't posted it to the arxiv yet because, as Clausen said in the IAS talk, they believed that degree one of the conjecture is within reach.

13. **Long Wind**
November 14, 2023

I debated writing this earlier today, but that last comment ("I would guess that they have a paper together on this topic, but haven't posted it to the arxiv yet") pushed me over the edge. The constant obsessive speculation and textual analysis of which papers Clausen / Scholze might be writing is really too much. Likewise, we don't need a news flash every time one of them gives a lecture at, say, the IAS.

Condensed math is a beautiful idea, but – like scheme theory! – it is ultimately a tool which lets one handle certain technical situations (in this case, "mixing topology and homological algebra") with greater facility and new insights. It is definitely not magic, and there is certainly no royal road through it to "geometric Langlands over Z" or any such thing.

Spacetime is Right-handed v. 2.0 and Some Notes on Spinors and Twistors

October 30, 2023
Categories: Euclidean Twistor Unification

I've just replaced the old version of my draft "spacetime is right-handed" paper (discussed here) with a new, hopefully improved version. If it is improved, thanks are due to a couple people who sent helpful comments on the older version, sometimes making clear that I wasn't getting across at all the main idea. To further clarify what I'm claiming, here I'll try and write out an informal explanation of what I see as the relevant fundamental issues about four-dimensional geometry, which appear even for $\mathbf R^4$, before one starts thinking about manifolds.

Spinors, twistors and complex spacetime

In complex spacetime $\mathbf C^4$ the story of spinors and twistors is quite simple and straightforward. Spinors are more fundamental than vectors: one can write the space $\mathbf C^4$ of vectors as the tensor product of two $\mathbf C^2$ spaces of spinors. Very special to four dimensions is that the (double cover of) the complex rotation group $Spin(4,\mathbf C)$ breaks up as the product
$$Spin(4,\mathbf C)=SL(2,\mathbf C)\times SL(2,\mathbf C)$$
where these two factors act on the spinor spaces.

While spinors are the irreducible objects for understanding complex four-dimensional rotations, twistors are the irreducible objects for understanding complex four-dimensional conformal transformations. Twistor space T is a $\mathbf C^4$, with complex conformal transformations acting by the defining $SL(4,\mathbf C)$ action. A complex spacetime point is a $\mathbf C^2\subset T$ and conformally compactified complex spacetime is the Grassmannian of all such $\mathbf C^2\subset T=\mathbf C^4$. One of the spinor spaces at each point of complex spacetime is tautologically defined: it's the point $\mathbf C^2$ itself (the other is of a different nature, with one definition the quotient space $T/\mathbf C^2$).

Real forms

While the twistor/spinor story for complex spacetime is quite simple, the story of real spacetime is much more complicated. When several different real spaces complexify to the same complex space, these are called "real forms" of the space. A real form can be characterized by a conjugation map σ (an antilinear map on the complex space satisfying $\sigma^2=1$), with the real space the conjugation-invariant points. Using the obvious conjugation on $\mathbf C^4$, we get an easy to understand real form: the $\mathbf R^4$ with real coordinates, rotation group $SL(2,\mathbf R)\times SL(2,\mathbf R)$ and conformal group $SL(4,\mathbf R)$. Unfortunately, this real form seems to have nothing to do with physics, its invariant inner product is indefinite of signature $(2,2)$.

The real spacetime with Euclidean signature inner product has an unusual conjugation that is best understood using quaternions. If one picks an identification of the twistor space T as $T=\mathbf C^4=\mathbf H^2$, then the conjugation is multiplication by the quaternion $\mathbf j$. The Euclidean conformal group is the group $SL(2,\mathbf H)$. The spinor spaces $\mathbf C^2$ are identified with two copies of the quaternions $\mathbf H$, with the rotation group now the group $Sp(1)\times Sp(1)$ of pairs of unit quaternions.

In this case the conjugation acts in a subtle manner. Since $\mathbf j^2$ is -1 rather than 1, it's not a conjugation on T, but is one on the projective space $PT=\mathbf CP^3$. It has no fixed points, so the twistor space has no real points. What is fixed are the quaternionic lines $\mathbf H\subset \mathbf H^2$, each of which corresponds to a point in the (conformally compacified, so $S^4=\mathbf HP^1$) real Euclidean signature spacetime. Using the decomposition as a tensor product of spinors, the action by $\mathbf j$ squares to -1 on each factor, but 1 on the tensor product, where it gives a conjugation with fixed points the Euclidean spacetime.

The real spacetime with Minkowski signature is another real form of a subtle sort, with very different subtleties than in the Euclidean case. The conjugation σ in this case doesn't take the twistor space T to itself, but takes T to its dual space T^*. It takes spinors of one kind to spinors of the opposite kind (at the same time conjugating spinor coordinates to get anti-linearity). The Minkowski signature conformal group is the group $SU(2,2)$ and the rotation group is the Lorentz group $SL(2,\mathbf C)$ (acting diagonally on the two spinor spaces, with a conjugation on one side).

Some philosophy

The usual way in which the above real forms get used is that mathematicians ignore the Minkowski story and use the Euclidean signature real form to do four-dimensional Riemannian geometry, with the $Sp(1)\times Sp(1)$ decomposition at the Lie algebra level corresponding to the decomposition of two-forms into self-dual and anti-self-dual. Physicists on the other hand (especially Penrose and his school, but also those trying to do quantum gravity using Ashtekar variables) ignore the Euclidean story and use the Minkowski signature real form. In various places Penrose is quoted as explicitly skeptical of any relevance of the Euclidean story to physics. Working just with the Minkowski real form, one struggles with the fact that the Lorentz group is simple, but that one can get a very useful self/anti-self dual decomposition if one makes one's variables complex.

The point of view I'm taking is that Wick rotation tells one that one should look simultaneously at both Euclidean and Minkowski real forms, understanding how to get back and forth between them. This is standard in usual geometry where one just looks at vectors, but looking at spinors and twistors shows that something much more subtle is going on. The argument of this new paper is that when one does this, one finds that the spacetime degrees of freedom can be expressed purely in terms of one kind of spinor (right-handed by convention), the one that twistor theory tautologically associates to each point in spacetime. The other (left-handed) half of the spinor geometry involves a purely internal symmetry from the point of view of Minkowski spacetime. This should correspond to the electroweak gauge theory, exactly how that works is still under investigation…

Update: Now posted on the arXiv here. Only reaction on social media I've seen so far is from Strinking42069, which seems to be a parody account trying to make fun of string theorists.

Comments

1. **RD**
   October 31, 2023

   `Off-topic comments better be interesting…`

   Apologies for instantly derailing the comment thread, but Ed Witten was the guest on 'The Life Scientific' this morning on BBC Radio 4. It's a regular, weekly show in which Jim Al-Khalili interviews various members of the scientific community , lightly touching on the technical aspects of their work without alienating the distinctly non-technical Radio 4 audience.

   Jim is a physicist himself. You made need to create a free account to listen to it:

   https://www.bbc.co.uk/sounds/play/m001rycq

2. **Peter Woit**

October 31, 2023

RD,
Thanks. That is a nice summary of Witten's career, including explaining how much the quark confinement problem has motivated him as well as explanations of his relation to mathematics. I do find his excellent advice about the danger of getting hung up on preconceptions in one's research darkly humorous given his continued faith in and promotion of M-theory unification.

This is an old story, with nothing really new here, unlikely anyone has anything new to say about it. I'd rather have discussion of some new ideas….

3. **Jim Eadon**
October 31, 2023

Dear Professor Woit,
You mention people modeling quantum gravity Does gravity (in the General Relativity sense) crop up in your set-up (as far as I can tell it doesn't). It seems to me (I'm no expert) you are modeling Special Relativity plus Euclidean space via twistors and so on, and potentially obtaining Electroweak fields for free, so to speak.
Perhaps a snappy tagline on what you are claiming could come in handy (more understandable than the abstract right-handed concept)

4. **Peter Woit**
October 31, 2023

Jim Eadon,
I'm making an argument at the level of fundamental symmetries, but yes, this includes an argument (explained a bit in a subsection) for a particular approach to quantum gravity (Euclidean quantum gravity using chiral variables). I even write down a Lagrangian.

For much much more about exactly the chiral variables approach, I just noticed two new papers on the arXiv:
https://arxiv.org/abs/2310.19729
https://arxiv.org/abs/2310.19722
These are in Lorentzian signature, and struggle with exactly the problem I mention in the paper. This problem goes away in Euclidean signature, but the issue of relating Euclidean quantum gravity to physical quantum gravity is obscure. I hope those expert in the subject of Ashtekar variables and chiral formulations will take a look at this.

5. **Peter P**
October 31, 2023

"The other (left-handed) half of the spinor geometry involves a purely internal symmetry from the point of view of Minkowski spacetime. This should correspond to the electroweak gauge theory…"

Terms leading to neutral currents and coupling to Higgs require both chiralities (uL and uR). Arguably, iso-singlet contributions into neutral currents are smaller than iso-triplet ones. Currently, they are still required to match experiments. The overall idea is intriguing: matching right-handed spacetime with left-handed inner DoFs (matter?).

6. **Interested amateur**
November 1, 2023

On the subject of twistors, a lecture was given by Nima Arkani-Hamed a few months ago as part of the IAS-DIAS organised conference, The Amplituhedron at 10: Hidden Mathematical Structures of the Amplituhedron: Why Is the Universe Big?

In particular at 1:27:38, Nima is asked how his physics program adds anything beyond Heisenberg's S-matrix theory. Worth a post by itself sometime in the future, maybe?

7. **Low Math, Meekly Interacting**
November 2, 2023

Hopefully not too OT, but I'm not convinced of the parody hypothesis. The truth of internet personae can be so much stranger than any imaginable fiction one must keep an open mind pending definitive evidence.

8. **Peter Woit**
November 2, 2023

Interested amateur,
I've often written here about the amplitudes program and Arkani-Hamed's vision for it. Earliest post I can find is from nearly fifteen years ago, see
http://www.math.columbia.edu/~woit/wordpress/?p=1705
At the time I was quite interested to see that people were working on formulating QFT in twistor space, that's something that has continued.

The amplitudes program has had a huge sociological success, it's now a very large enterprise. In his talk and at the point you mention, I think Arkani-Hamed makes clear what his vision is for it: find some new geometrical principle which will give S-matrix amplitudes, replacing local QFT and replacing spacetime. He's a huge optimist about whatever idea he's working on at the moment, we'll see what happens.

I'm all for new ideas about reworking QFT in terms of different geometrical fundamentals, and twistors are among the most promising such ideas. But I don't think it's so healthy that the sociology of the field is that one very speculative and not obviously successful idea about this attracts a large fraction of the people and other resources of the field. We'd be better off if people interested in new geometrical ideas about fundamental physics were investigating a much wider range of possibilities. For instance, the geometry of the Wick rotation between spinors in Euclidean and Minkowski spacetimes…

9. **Peter Woit**
November 2, 2023

LMMI,
Hard to guess what the true story of Stringking42069 is, maybe it is an actual string theorist whose brain has been rotted by too much testosterone and 420. But it's very hard to take at face value the combo of informed and accurate information about string theory research with a huge amount of material that makes the author appear to be a complete idiot.

10. **suomynona**
November 3, 2023

It appears fairly obvious that Stringking42069 is not any kind of serious account. If not parody, Stringking42069 is at the very least an *intentionally* highly exaggerated personality. Even for an anonymous internet commenter, they're far too unhinged for them to be serious and posting in good faith while still having enough faculties to accurately understand the HET community. A pretty dead giveaway are the numbers ‘420’ and ’69’ in their Twitter/X handle. These are numbers which are, quite obviously, associated with an intentional joke.

11. **Marvin**
November 5, 2023

Hi Peter,

When you say in your article:”Spacetime (both Minkowski and Euclidean)
can be said to be “right-handed”, and we have seen that this goes beyond the spin 1/2 matter degrees of freedom, with Yang-Mills and gravitational dynamics also described using right handed spinors.”

Are you expecting some measurable astronomical effect following the fact that spacetime had this preferred chirality. For what I know, exept for small anomalies (like the “Axis of evil”), there is no such significant effect visible. Or may be you have your own idea on this.

12. **Peter Woit**
November 6, 2023

Marvin,
What I’m claiming is that you can describe all those degrees of freedom in a way that is completely chirally asymmetric when you look at complex spacetime (or Wick rotate to Euclidean spacetime). Just looking at Minkowski spacetime nothing is different, and you just have the usual theories, which are thought of as chirally symmetric. The main point is that if you Wick rotate one of these theories normally thought of as chirally symmetric, you get something very definitely chirally asymmetric.

Peter

13. **tulpoeid**
November 6, 2023

Btw I think Stringking is as much a parody of string theorists as Motl is. Parody, yes, maybe; intentional, not really.
(Also, he’s apparently the most interesting strings have to offer nowadays.)

14. **Marvin**
November 7, 2023

Thanks Peter: very clear explanation!

15. **Hayun**
November 8, 2023

Unlike other TOE, your argument is that gravity and gauge theories are completely decoupled?”

16. **Peter Woit**
November 8, 2023

Hayun,
What I’m pointing out here is just that the usual YM and gravity theories can be expressed in terms of purely chiral variables. They are coupled in just the usual manner (the YM action involves the Hodge star, which depends on the metric, or vierbein). Chiral fermions are coupled to YM and gravity by the usual covariant derivatives.

17. **Plebian Science Beggar**
November 26, 2023

Are there any interesting new predictions of Twistor Theory? I get that in theory it recovers the familiar models, but on the margins has it yielded new observable stuff that the old theories don’t predict?

BTW, long-time fan and lurker and first-time poster. It would mean a whole lot to get a response from you. Tear me to pieces, please!

18. **Peter Woit**
November 26, 2023

PSB,
Twistor theory isn’t really a “theory”, although I suppose you could try to follow the string theorists and go on about how it’s a “framework”.

What twistor theory does is point out that there’s mathematically a different way of describing Minkowski spacetime, one that has some very powerful and appealing characteristics: it makes conformal symmetry manifest and allows the use of complex variable methods. You can try and write down the full standard model and GR in this description of spacetime, but it’s very awkward, definitely doesn’t give what you would like (a theory that reproduces the successful parts of the standard theory and also gives something different and testable).

What I’m trying to do is to investigate a new set of ideas based on twistor theory (looking simultaneously at the Minkowski and Euclidean versions) which look to me to have promising properties. Still a ways though away from showing exactly how to reproduce the standard theory this way (and, ideally, also something new and different), but what I do understand looks promising.

Unification, Spinors, Twistors, String Theory

November 22, 2023
Categories: Euclidean Twistor Unification, Uncategorized

Last month I recorded a podcast with Curt Jaimungal for his Theories of Everything site, and it's now available with audio here, on Youtube here. There are quite a few other programs on the site well worth watching.

Much of the discussion in this program is about the general ideas I'm trying to pursue about spinors, twistors and unification. For more about the details of these, see arXiv preprints here and here, as well as blog entries here.

About the state of string theory, that's a topic I find more and more disturbing, with little new though to say about it. It's been dead now for a long time and most of the scientific community and the public at large are now aware of this. The ongoing publicity campaign from some of the most respected figures in theoretical physics to deny reality and claim that all is well with string theory is what is disturbing. Just in the last week or so, you can watch Cumrun Vafa and Brian Greene promoting string theory on Brian Keating's channel, with Vafa explaining how string theory computes the mass of the electron. At the World Science Festival site there's Juan Maldacena, with an upcoming program featuring Greene, Strominger, Vafa and Witten.

On Twitter, there's now stringking42069, who is producing a torrent of well-informed cutting invective about what is going on in the string theory research community, supposedly from a true believer. It's unclear whether this is a parody account trying to discredit string theory, or an extreme example of how far gone some string theorists now are.

To all those celebrating Thanksgiving tomorrow, may your travel problems be minimal and your get-togethers with friends and family a pleasure.

Update: If you don't want to listen to the whole thing and don't want to hear about spinors and twistors, Curt Jaimungal has put up a shorter clip where we discuss among other things the lack of any significant public technical debate between string theory skeptics and optimists. He offers his site as a venue. Is there anyone who continues to work on string theory and is optimistic about its prospects willing to participate?

Update: For two more clips from the podcast, there's one about spinors, and one about "spacetime is not doomed".

Comments

1. **Anonymous**
   November 22, 2023

   Multiverses are getting mixed press, too. I subscribe to the IAI newsfeed, and this recent article came up:

   https://iai.tv/articles/the-multiverse-is-unscientific-nonsense-auid-2668

2. **Peter Woit**
   November 22, 2023

   Anonymous,
   I think the whole multiverse nonsense was a disaster for string theory and string theorists know this. Vafa's "swampland" program is explicitly an acknowledgement that saying "the multiverse did it" is indefensible and destroys your scientific credibility: you need another answer to the problem of inherently not being able to predict anything.

   What non-swampland others seems to be doing now is trying to avoid ever mentioning the "multiverse did it" answer unless backed into a corner and forced to come up with something.

3. **neo**
   November 22, 2023

   with Vafa explaining how string theory computes the mass of the electron

   string theory computes the mass of the electron –could you elaborate is this a new result ?

4. **Peter Woit**
   November 22, 2023

   neo,
   Maybe someone with more time to waste can explain Vafa's argument. My impression is that Keating didn't take it seriously. From a quick scan of the transcript, Vafa at one point says that even if there were some experimental result that disagreed with string theory, to him it would just mean that one needed to understand string theory differently.

5. **David Appell**
   November 22, 2023

   What is Vafa's Formula for the mass of the electron, and where is it published?

6. **Peter Woit**
   November 23, 2023

   David Appell,
   There is no Vafa Formula, this is just complete nonsense, very disturbing to hear it come from someone like Vafa (without any challenge from Keating).

   It's typical "swampland" argumentation, based on a general conjecture about quantum gravity theories ("weak gravity conjecture") which has little to do with string theory, and just gives not a number but some absurdly weak bound, many many orders of magnitude from the mass. Vafa then goes on to explain (around 34:00) that even if a "prediction" like this was falsified, he wouldn't see that as a reason to doubt string theory, it would just mean that one needed to understand "string theory" differently. This has become religion, not science and I don't understand how this has happened and why anyone continues to take it seriously.

7. **Low Math, Meekly Interacting**
November 23, 2023

Glad this finally got posted. I was rather hoping to come out with a greater understanding of twistors, but not so much. Even Penrose, a masterful expositor, seems to have a hard time conveying what twistors are all about to a lay crowd, so I can't say my hopes were reasonable.

8. **James Smith**
November 23, 2023

I think this is relevant:

https://en.wikipedia.org/wiki/Googly

9. **David Appell**
November 23, 2023

Thanks Peter. Happy Thanksgiving.

10. **Peter Woit**
November 24, 2023

LMMI,
Penrose has given various versions of more physical elementary explanations of twistors, but the problem is that this is all about a new way to think about space-time in highly relativistic and conformally invariant regimes, far from ordinary intuition.

If you're happy with complex variables I do think there is a very simple way to understand twistors and spinors: a point in spacetime is a complex two-plane in C^4. Elements of this C^4 are twistors. Spinors at a point are tautological: elements of the complex two-plane.

The problem with this though is that it gives you complexified space-time, and you have to understand which points are "real". The "real structures" that determine this for Minkowski spacetime and Euclidean spacetime are not obvious (and quite different in the two cases).

11. **Peter Woit**
November 24, 2023

James Smith,
Yes, there's a fundamental problem with twistor theory that Penrose likes to call the "googly problem": twistor theory is naturally chiral, with points right-handed spinors. The traditional problem is how to understand left-handed spacetime degrees of freedom in twistor language. This problem is normally considered in Minkowski signature spacetime. I'm arguing that if you Wick rotate and look at Euclidean signature spacetime the problem becomes a virtue: you can describe physical spacetime just using right-handed degrees of freedom, with the left-handed ones now internal degrees of freedom.

12. **Marvin**
November 26, 2023

Hi Peter,

In Vafa interview, the prediction by string theory of the mass of the electron happen at around 14:40, not 34:00.

This said, as you had explained above, this prediction is extremely vague.

I can't understand why they look satisfied whit these weak results.

13. **Peter Woit**
November 26, 2023

Marvin,
Thanks, that's right. What's at around 34:00 is where Vafa responds to the question of what he would do if experiment falsified a "prediction" of string theory, like the electron mass one:

"I will go back and search my on my understanding of string theory. And perhaps we we, we made a mistakes along with our understanding, because I think part of an issue is that we don't have a complete formulation based on what string theory is. So we are kind of on a difficult platform to be that sure is string theory, right? Because string theory fails to do that. You have to know exactly what string theory is, and we don't know that yet. So I would go back and check my understanding of the subject."

This pretty much answers the question Keating used as title for the interview: "Is String Theory Actually Science?"

14. **Adam Treat**
November 26, 2023

Peter Woit,

Your point is well taken, but if I can play the devil's advocate for a minute consider if Vafa's reply was:

"I will go back and search my on my understanding of physics. And perhaps we we, we made a mistakes along with our understanding, because I think part of an issue is that we don't have a complete formulation based on what physics is. So we are kind of on a difficult platform to be that sure is physics, right? Because physics fails to do that. You have to know exactly what physics is, and we don't know that yet. So I would go back and check my understanding of the subject."

I honestly think this is what is going through Vafa's head when he says things like this.

15. **Peter Woit**
November 26, 2023

Adam Treat,
I'm sure it's right that in his mind Vafa is identifying "string theory" = "fundamental physics", but that's exactly the problem. At this point as far as he's concerned there is no way string theory can be wrong, the only open question is what sort of string theory is the right one. This really isn't science anymore and this attitude is killing off the subject of fundamental physics.

16. **Schratter**
November 30, 2023

While we still don't quite know what Twistors are or how they relate to physics, we did learn that Garrett Lisi slept on Woit's couch, once again demonstrating that modern physics is mostly just sociology.

17. **Peter Woit**
November 30, 2023

Schratter,
Being on the list of people whose couch Garrett slept on is not exactly membership in a small, exclusive club…
If you want to know what twistors are and what they have to do with physics, there are lot of places to learn about this (see for instance slides from my talks in recent years, on my website). You're not going to understand this sort of thing by listening to two people talking on a podcast like this.

18. **zovi**
December 4, 2023

Have anyone here have a take at Oppenheim's paper "A postquantum theory of classical gravity?" in PhysRevLett X ?

19. **Simone Speziale**
December 5, 2023

Hello,

To complement Peter's answer to LMMI's question, I think it is always nice to give a geometric intuition of twistors by recalling the interpretation of their incidence relation in the case of conformally flat spacetime: each zero-helicity twistor describes a light ray in Minkowski, each non-zero-helicity twistor describes a twisting null congruence in Minkowski spacetime, or alternatively what is called an alpha-plane in complexified Minkowski spacetime. The set of such flat alpha-planes is useful because it is integrable and spans the manifold CP^3. This is the manifold whose complex structure is associated to the conformal metric structure of Minkowski's spacetime, and this result gives a 4d (limited) extension of the beautiful equivalence between conformal metric structures and complex structures that we have in 2d.

For those interested in a bit more details, recall that to each spinor we can associate a light-like direction at the origin of Minkowski space: This is done taking the ratio of the spinor's components, and interpreting it as a stereographic coordinate on the celestial sphere. With this construction we see that the complex structure of CP^1 can be associated to the celestial sphere. Twistors are bi-spinors, and the way light rays are reconstructed from the incidence relation is roughly speaking with one spinor giving the direction of the light ray, and the second spinor giving the shift away from the origin. The space of light rays is a 5d manifold (2d for the direction of a light ray in the origin, and 3d of independent translations), therefore there is no complex structure that can be associated to it. However, the pair of spinors has to satisfy the special "zero-helicity" property in order to describe a light ray. In general, it describes a 6d space of complexified light rays called alpha-planes mentioned above. The incidence relation is invariant under complex rescaling of the spinors, hence this 6d space is a CP^3 inside C^4.

The difficulty in going beyond (conformally) flat spacetime is that to define a twistor space associated with a curved manifold, you would need the alpha-planes to be integrable, so that they span a complex manifold. This is the manifold whose complex structure would be associated to the conformal spacetime metric. But integrability of the alpha-planes requires the self-dual part of the Weyl tensor to vanish (A preferred chirality enters the theory from the start, because the incidence relation treats the two spinors in an asymmetric way).
Hence this basic construction only works for curved Euclidean or complex spacetimes, and one has to figure out some alternative way of proceeding in order to treat real Lorentzian spacetimes.

Concerning the latest developments by Penrose, he is working to overcome this limitation using inputs from non-commutative geometry, in a version of twistors that he refers to as Palatial Twistor Theory.

20. **Peter Woit**
December 5, 2023

zovi,
I don't want to host a discussion of the proposal in the Oppenheim paper here, since I know nothing about it and it's very far from my own interests. There is one aspect of this though that I would like to understand better. Somehow it got very wide media coverage, see for instance this in the Guardian
https://www.theguardian.com/science/2023/dec/04/wobbly-spacetime-may-resolve-contradictory-physics-theories
This is despite the experts of very different kinds contacted by the journalist agreeing to take odds of 5000:1 that this doesn't work.

I'm thinking that quite possibly I could get better odds than that on my ideas about chiral Euclidean gravity from Rovelli and Pennington, so how do I get a story in the Guardian?

21. **Martin S.**
December 5, 2023

@zovi: It was [described at Quanta]() a few months ago.

22. **Peter Woit**
December 5, 2023

Simone Speziale,
Thanks a lot for providing that physical/geometric description of twistors and more background!

This may be the deformation of too much time spent in math departments, but I'm very much seduced by the simple mathematical structure in the complexified case (a point in complex spacetime is exactly the chiral spinor space). The Minkowski real form where we visualize physics adds a much more complicated structure. The Euclidean real form is much simpler, even though its relation to the physical Minkowski story is tricky to understand.

P5 Report

December 7, 2023
Categories: Uncategorized

There is a HEPAP meeting going on today, with release of the long-awaited P5 report prioritizing future HEP spending. The report is available here now, to be officially unveiled later in the day and discussed at the HEPAP meeting. The muon collider as a future project gets a strong endorsement as the "muon shot".

The full report is now available here.

See press coverage of this at Nature and the New York Times.

There will be continued discussion of the P5 report at HEPAP tomorrow, and at a Town Hall at Fermilab on Monday.

While the theory side of HEP in principle is part of this report, the attention to theory is minimal, with the report recommendations about theory, in total:

- The substance-less "Enhance research in theory to propel innovation, maximize scientific impact of investments in experiments, and expand our understanding of the universe."
- A more substantive call to give university theorists on DOE grants more money, with no attempt to prioritize what the money would be for:

  > Increase DOE HEP-funded university-based theory research by $15 million per year in 2023 dollars (or about 30% of the theory program), to propel innovation and ensure international competitiveness. Such an increase would bring theory support back to 2010 levels. Maintain DOE lab-based theory groups as an essential component of the theory community.

  In the page or so of text about theory, the emphasis is on the phenomenology part of theory in contact with experiment. About formal theory there is just

  > Theorists uncover the mathematical patterns that describe the universe and explore alternate mathematical universes to deepen our understanding of nature. Theoretical investigations into quantum gravity have unlocked connections between extreme space-time geometries and information theory. The perspectives theorists bring to particle physics play an important role in inspiring young scientists.

Update: Sabine Hossenfelder has her reaction to the P5 report here. I'm sympathetic to her critique that, as far as experiments go, the report is an argument for "more of the same". But I'm not at all sympathetic to her alternative: "Personally, I think what they should do is spend some money on serious theory development" instead of funding experiments. If there's a part of science where money spent by the US has been much more of a waste than in HEP experiment, it's HEP theory, which is now pretty much intellectually dead. There's a good argument that the way HEP theory funding works has been a driving factor in this fatal illness, so that the problem with HEP theory is not too little funding but too much.

Comments

1. **p.**
   December 7, 2023

   "This less favorable scenario will lead to a loss of US leadership in many areas, especially the science of the G3 dark matter experiment, and will damage our reputation as a reliable international host for DUNE and as a partner for a Higgs factory. We still make investments in the future, but at a significantly reduced level for small-scale experiments, including ASTAE, theory, computing, instrumentation, and collider R&D. In this scenario, it would be increasingly difficult to maintain US competitiveness as an international partner in accelerator technology." -p43

   Can someone explain please to an obvious idiot, WTF the USA feels the need to lead the world in this? Is there no comprehension about these folk that such a policy is seen as elitist, narcissistic, capitalist juggernaut? Or is that the deliberate intent? Or maybe you think the rest of the world is such a corrupt load of savages they need leading? Just curious

2. **Peter Woit**
   December 7, 2023

   p.
   I think you're not understanding the nature of this report. It's intended as a prioritization of requests from the US HEP physics community to US government agencies for funding. The intended audience is government officials, legislators, etc. who will be asked to approve large expenditures for HEP research. No one is going to try to make the case to a US congressman: "give us $1 billion/year so we can do research second in importance to what the Europeans are doing at CERN."

3. **Amitabh Lath**
   December 8, 2023

   A couple of things stuck out for me: the report mentions muon collider research, as well as a "portfolio of small-scale and agile experiments". I admit to being on the fence about the viability of a muon collider, but the idea of small-scale and agile experiments is exciting.

4. **tulpoeid**
   December 10, 2023

   Interesting comment in the press release about the report, by Hitoshi Murayama, P5 panel chair:
   "Our thinking about what dark matter might be has also changed [since the previous P5 process], forcing the community to look elsewhere — to the cosmos."
   (https://www.aps.org/newsroom/pressreleases/part-research.cfm)

5. **Peter Woit**
   December 10, 2023

   tulpoeid,
   I was surprised that a CMB experiment is the P5 highest priority, had assumed that this was an issue for astronomy funding priorities, not HEP physics funding priorities. It seems one argument for this is the lack of a WIMP showing up at the LHC or in direct dark matter detection experiments means one should look is cosmology. I hadn't realized cosmology experiment were getting funded out of the DOE HEP physics budget.

6. **anon**

December 11, 2023

CMB-S4 is (or at least was when it was discussed in the Astro Decadal Survey) supposed to be a joint roughly 50-50 split DOE-NSF effort. With the way (ground-based) astro funding is looking, it would never get built with only NSF money.

7. **Photino Birds**
December 29, 2023

Peter, I always thought that both your and Sabine's opinions are equally bad. But it looks like Sabine is more reasonable than you.

hep-th funding is very low. There are so many good researchers doing popular mainstream research, and after 6+ years of postdoc, they are forced to leave to Google/AnthropicAI, etc, and do some uninteresting high-paying job. If postdocs who work in mainstream approaches in top universities and get thousands of citations cannot get a job, obviously, nonmainstream approaches cannot get any job since it's harder to compare and measure progress among nonmainstream postdocs. I hope I don't need to share the inspire links of people who wrote very famous papers related to holography who had to leave the field in the last few years, as you probably know the situation. I think since you don't like string theory/holography maybe you are blindly advocating for less funding. But if more funding is given nonmainstream approaches will also benefit, not just mainstream approaches.

It seems you just want the entire hep-th field to die. I cannot understand what you will gain from that. Understanding fundamental physics is like the real purpose of humanity. It seems you are blaspheming against fundamental physics.

8. **Peter Woit**
December 29, 2023

"Photino Birds",
First of all, what's with the anonymity and identifying as a weird sci-fi fandom site? How am I or anyone else supposed to tell the difference between a serious argument from an informed person and something unserious that should be ignored?

If you are an informed person and this is supposed to be a serious argument, I'll just point out that it's not a very good one. You're arguing that more hep-th funding is needed, to go to support the people adding to the 20,000 papers on the heavily overhyped topic of holography. Yes, there are too many such people on the market, so their job prospects are not great. What is needed though is not more funding to keep them doing holography, but redirection of current funding to other "non-mainstream" topics.

9. **JE**
January 5, 2024

Hi Peter,

While I do not share Photino Birds' view –or "shoot in the foot"– that holography can be a good example of non-mainstream physics that should (continue to) be supported with more hep-th funding, I think he has a case arguing against your comment that, "If there's a part of science where money spent by the US has been much more of a waste than in HEP experiment, it's HEP theory, which is now pretty much intellectually dead."

And more so when you say: "I was surprised that a CMB experiment is the P5 highest priority, had assumed that this was an issue for astronomy funding priorities, not HEP physics funding priorities."

First, because little or no sympathy for more/continuing hep-exp and hep-th funding can be fairly misinterpreted as "It seems you just want the entire hep-th field to die." Perhaps you could explain where you would put taxpayers' money more clearly to stake your case.

Second, although I may be biased by my own research, there could be a deep, largely unexplored, connection between the CMB and hep-th. In other words, I can understand why CMB experiments can be among P5's highest priorities. Or, more generally speaking, certain cosmological observations closely related to hep-th, which could shed more light on theory than big particle colliders (not including the muon collider, which seems quite reasonable an option) and at a much lower cost.

And third, given the critical state of the field and the manifest inability of mainstream physics to find a solution, I would put my two cents in non-mainstream rather than mainstream physics, including outsider (privately-funded) research. Unlikely as you may think it is, if a lone researcher or a small group of privately-funded researchers came with a viable solution to the hep-th crisis, it would deal a big blow to the whole academic system. And perhaps with good reason.

10. **Peter Woit**
January 5, 2024

JE,
My comment about CMB was simply about the source of the funding. I would have assumed that microwave telescope funding would be categorized under astronomy, not DOE HEP.

I'm writing more about this in the next posting, but my point about hep-th funding is that, with about 4000 hep-th arXiv submissions/year and order of magnitude 10,000 active hep-th researchers in the world, the problem with progress coming to a halt is not due to not enough funding. Yes, maybe some very different funding mechanisms would help, but no one is even discussing that, instead just asking for more of the same.

11. **JE**
January 5, 2024

Peter,

Thanks for the clarification. Looking forward to reading that posting.

Perhaps discussing these mechanisms could be worthwhile. More "non-mainstream" research could be needed after so much mainstream (at least until mainstream theory becomes non-mainstream, as it happens to be the case with holography now, according to some sources). This may suggest that the difference between mainstream and non-mainstream can start to become unclear after the debacle of string theory.

So I would rather talk about promising lines of research -mainstream or not-, which better be publicly-funded.

12. **Photino Birds**
January 6, 2024

JE,

The tides are shifting in that direction, holography becoming non-mainstream. Recently, Eran Palti (swamplander) https://x.com/EranPalti/status/1740626727490248743 noticed this. Several people who usually work on AdS/CFT toy models have gone back to doing proper string theory this year. In the last 10 years, most people worked on holography toy models and proper string theory was not popular. Exactly solvable models like JT gravity taught many things, but they are not UV complete. People always knew that these toy models were temporary. There is only one UV complete quantum gravity we know: perturbative string theory. So it is not far fetched that proper string theory will again be the hot topic in the future, as it is still the most promising theory. Using AdS/CFT as a nonperturbative definition of string theory is a cop-out. People must find an independent nonperturbative definition of string theory using new techniques and try to prove the AdS/CFT correspondence. PhDs and postdocs must do what is popular now. Research in proper string theory in the last 10 years was not good for postdoc opportunities, etc, due to sociology but that might change soon.

Recent Talk

December 9, 2023
Categories: Uncategorized

I gave a "Spacetime is Right-handed" talk yesterday, part of a series entitled Octonions, standard model and unification. The slides are here, video should appear here.

Much of the talk was devoted to explaining the usual relation between spinors and vectors and how analytic continuation in complexified spacetime works then, from both the spinor and twistor point of view. This is contrasted to a new proposal for the relation between vectors and spinors in which the space-time degrees of freedom see only one of the two SL(2,C) factors of the usual complexified Lorentz group.

Nothing in the talk about using this for unification, where the idea is to exploit the other factor, which now appears as an internal symmetry. Starting from the point of view of Euclidean spacetime, the spacetime vectors and spinors that are related by Wick rotation to Minkowski spacetime degrees of freedom behave differently than usual, with a distinguished imaginary time direction. The general idea is that in standard Euclidean spacetime, where the geometry is governed by the rotation group SU(2) x SU(2), so splits into self-dual and anti-self-dual parts, one of these parts Wick rotates to spacetime symmetry, the other to an internal symmetry.

Comments

1. **Daniele Corradetti**
   December 10, 2023

   It was an excellent talk. Looking forward for the new manuscript. I hope it is self-contained and builds everything from scratch so that also who only have a mathematical background (no physical background) can understand the deep motivations of this work.

2. **Robert A. Wilson**
   December 10, 2023

   If I may, I'd like to expand a little on the comments I made after the talk, concerning the first section (slides 1-12). The classification of representations into real, complex and quaternionic on slide 8 only applies to complex irreducible representations, not to arbitrary representations. When you consider the Dirac spinors, on slide 10, the representation is not irreducible, and it has not only the "real" interpretation, that is as the complexification of a real Majorana spinor, but also a "quaternionic" interpretation, that is as a restriction of a quaternionic 2-spinor to a complex 4-spinor. To see this, it is enough to negate \sigma on one of the Weyl spinors, in order to change from \sigma^2=1 to \sigma^2=-1.

   In fact, the difficulties with the standard formalism that you encounter on slide 12 occur mainly as a result of ignoring the quaternionic case, and working exclusively with the real case. Several people have pointed out that the quaternionic case introduces a *choice* into the theory, that is not there in the real case: the copy of C inside H, that is used to define the standard Dirac spinor, is not canonical. This choice has been used, for example, to extend from one generation (choice of i) to three (i, j, k).

   In terms of Clifford algebras, what is going on here is that Cl(3,1) and Cl(2,2) are 4×4 real matrix algebras, while Cl(1,3), Cl(4,0) and Cl(0,4) are 2×2 quaternion matrix algebras. Hence it is possible to unify an external Minkowski spacetime with an internal Euclidean spacetime in a single Clifford algebra, if and *only if* the Minkowski signature is (1,3). This is not what is done in the SM, of course, in which complexification is used to obliterate the signature completely. But it could be done, and it might resolve the problems with the SM that you point out on slide 12.

3. **Peter Woit**
   December 10, 2023

   Danielle Corradetti,

   While the symmetry group structure largely determines the Standard Model QFTs, it's a very long and complex story to get from facts about the basic groups and their representations to the actual quantum field theory.

   This spring I'm teaching an advanced graduate course with a goal of explaining to mathematicians as much as possible what the SM QFT actually is, using a great deal of geometry and representation theory. I'll be writing notes, the end result may answer your question, you might not find the answer so easy to follow…

4. **Peter Woit**
   December 10, 2023

   Robert A. Wilson,

   Many thanks for the comment, which is very helpful. I have to think more about the role of Clifford algebras in what I'm doing. They work in general dimension/signature, while I'm focused on the very special properties of the 4d case.

5. **lun**
   December 14, 2023

   Naive question: How non-trivial would it be to apply these ideas to a QFT defined on the lattice in order to avoid the doubler problem and/or define a lattice chiral theory?

6. **Peter Woit**
   December 14, 2023

   lun,
   I hope ultimately these ideas will give new possibilities for how to formulate spinor fields on the lattice, but haven't yet thought very much about this. I'm concentrating now on trying to understand how to exploit the Euclidean left-SU(2) symmetry as an internal symmetry.

   A major problem with spinors on the lattice as always been that differential forms have a natural lattice formulation, but spinors are "square roots" of differential forms (the relation between vectors=1-forms and spinors discussed in the talk is part of this story).
   You naturally end up with lots of copies of spinors (e.g. Kogut-Susskind fermions). I hope that with this new interpretation where some space-time symmetries become internal, you might have a new lattice formulation not for a single chiral fermion (where you expect anomaly problems), but for a multiplet of them. But, still a ways from knowing how to even exactly formulate this kind of proposal.

7. **Sam**

January 2, 2024

Have you thought about / commented at all on the potential relationship between your Euclidean twistor theory and Connes' spectral approach and NCG? As a lay person, there seem to be some striking parallels:

– Both are Euclidean to start
– Twistor coordinates define a noncommutative space
– Derivation of the SM from the eigenvalues of the spectral action gives Pati–Salam, with the same SU(2) – left / right splitting you use to encode spacetime chirality
– Particle representations are given by the inner-automorphisms / internal symmetries of the full metric, rather than spatial symmetries that have to be compactified
– The spectral view (observables are incoming energy differences) seems philosophically complementary to the projective space of twistor theory
– Both do away with the classical notion of a point (NCG is "pointless" in that one cannot localize arbitrarily; the fixed points in twistor theory are quaternionic lines)
– Both connect with the homotopy of spheres and k-theory

Searching the archives, you've linked to Connes before and mentioned his work on the spectra of the RZF, but I don't think you've ever explicitly discussed the relationship between his programme and your own. A friendly request!

8. **Peter Woit**
January 2, 2024

Sam,
There are some points of contact such as you mention between what I've been working on and NCG a la Connes. But from the little time I've spent so far trying to better understand NCG, I haven't found a deep enough connection to help with the problems I've been thinking about.

String Theory Hype Fest

January 5, 2024
Categories: This Week's Hype

I just finished watching the video here, which was released today. Since this was advertised as a panel discussion on the state of string theory, I thought earlier today that it might be a good opportunity to write something serious about the state of string theory and its implications more generally for the state of hep-th. But, I just can't do that now, since I found the video beyond depressing. I've seen a lot of string theory hype over the years, but on some level, this is by far the worst example I've ever seen. I started my career in awe of Edward Witten and David Gross, marveling at what they had done and were doing, honored to be able to learn wonderful things from them. Seeing their behavior in this video leaves me broken-hearted. What they have done over the past few decades and are doing now has laid waste to the subject I've been in love with since my teenage years. Maybe someday this field will recover from this, but I'm not getting any younger, so dubious that I'll be around to see it.

Most shameful of the lot was Andy Strominger, who at one point graded string theory as "A+++", another only "A+". He did specify that very early on he had realized that actual string theory as an idea about unification was not going to work out. He now defines "string theory" as whatever he and others who used to do string theory are working on.

David Gross was the best of the lot, giving string theory a B+. At two points (29:30 and 40:13), after explaining the string theory unification vision of 1984-5 he started to say "Didn't work out that way…" and "Unfortunately…", but in each case Brian Greene started talking over him telling him to stop.

Funny thing is, I think even most string theorists are going to be appalled by this performance. Already, here's what StringKing42069 has to say

> 🤮 these old jagoffs have thrown an entire generation of strings under the bus. Fuq them.

Update: I haven't seen any negative reaction to this hypefest from anyone in the physics community other than from StringKing42069. The Black Hole Initiative at Harvard features the event prominently on its website here advertising Strominger's participation (he's a PI).

I'm finding it hard to believe that any of the participants in this thought of it as anything other than an advertising effort useful to try and prop up public support and grant funding. In particular, Strominger's "A+++" is easier to understand once you realize the extent of the grant funding involved, e.g.:

- The Black Hole Initiative that features this on its website: \$16 million from the Templeton Foundation, \$3.6 million from the Moore Foundation.
- The Simons Collaboration on Celestial Holography: \$8 million from the Simons Foundation.
- NSF Grant: \$400,000 from the NSF.
- DOE Grant: \$3.5 million from the DOE.

The abstract of the last of these is A+++ hype in tune with the WSF video:

> Vigorous efforts made over the last several decades have advanced our understanding of the fundamental laws of nature beyond the standard model of particle physics. Further advances would potentially include unification of the forces, the reconciliation of quantum mechanics and gravity, a derivation of the standard model couplings, a universal explanation of the area law for horizon entropy, and a theory for the origin of the universe.

For a much older example of successful use of hype to extract grant funding, there's this Jeffrey Epstein story I hadn't known about until recently.

Comments

1. **Eric Weinstein**
   January 5, 2024

   This is astounding video. They are all so well spoken and pure in their summations that you can hear exactly what the last 40 years have sounded like from inside the harmonious echo chamber of what once was physics. A single skilled critic would destroy the vision that this succeeded at an A+ level. And so…there can be no critics on stage to disturb the delicacy of this castle in the air made of clouds and string.

   \

   This is perfect in every way. A summation of 4 decades of a constant stream of preternatural success after success inerupted only by triumphs as viewed by the leaders and greying architects of this, the most wonderful of all possible theories. 'Raindrops on roses and brown paper packages tied up with strings.' as the song goes.

   \
   I feared I would hate this from your description Peter. And yet, I adore this and wouldn't change a thing. No one would believe me if these men were not willing to say it themselves.

2. **Shantanu**
   January 6, 2024

   Peter, happy new year. As a preview
   see https://online.kitp.ucsb.edu/online/strings24/
   Hopefully all these talks will be online.

3. **JE**
   January 6, 2024

   I share your grief, Peter. Watching top physicists strive to perform as televangephysicists with the help of Brian Greene's performance skills is shameful (and boring). We already had Michio Kaku, but these are bigger names. It has almost led me to change my mind about hep-th funding. Just in case it could get confused with hep-tv funding.

   I would say that Witten's body language might indicate that he was not comfortable during the talk, especially in the first half, as if his mission there was just to answer some technical questions (shorter statements, etc), not to make a TV show, but perhaps this is out of shyness. And Gross' final B+ is a strong indicator of the real state of the subject…

4. **Ted Rogers**
   January 6, 2024

Most people in the comments section seemed to have loved the video. Not sure if that means anything.

5. **James Lai**
January 6, 2024

   Indeed Gross kept shaking his head during Strominger's ending remark.

6. **Peter Hickman**
January 6, 2024

   Usual hype, nothing new.
I would have these 2 questions on the assessment of 'string theory' exam paper:
Q(1) – Is the Standard Model of particle physics recovered from 'string theory'?
Q(2) – Is there any experimental or observational results for 'string theory'?
By this assessment, score is 0/2.
Certainly not A++++ or B+ grade.
The biased panel do not want to admit failure. They should realize that its not wrong to admit failure, that's the nature of science research. If 'string theory' was any good, i.e. useful, and makes definite predictions, then the hype over last c.40 years would be unnecessary. The continuing hype shows that the goal of 'string theory' has failed, whether the panel and other string theorists admit to it or not.

7. **David Brown**
January 6, 2024

   Note that Witten applauded (rather unenthusiastically) Strominger's A+ grade for string theory.

8. **Giotis**
January 6, 2024

   Surely the fact that String Theory counts exactly the number of Black Hole microstates, according to Bekenstein-Hawking prediction, strongly suggests that the Theory knows a couple of things about the Universe.

   This statement in the latest review summarizes the situation for the SM:

   "The SM can be embedded into string theory: The main ingredients of the SM – the gauge group, chiral matter and Yukawa interactions – follow relatively naturally from general principles of string theory. Reproducing exactly the SM including, for instance, its vectorlike matter content or the precise flavor structure, is more involved, but each property per se has been obtained or is within reach. The challenge is to find vacua that combine all these features at the same time, with all moduli stabilized and incorporating a realistic cosmology."

   "The Standard Model from String Theory: What Have We Learned?"

   https://arxiv.org/abs/2401.01939

   The SM can be derived from String theory but the issue is to combine all its ingredients in a complete cosmology of a dS vacuum with all moduli stabilized.

   If you know a couple of things in String pheno you understand that this is a daunting task.

   The fact that this Universe is not unique is plausible of course (the failure of naturalness may indicate just that as Witten explained).

   The new development though is the Swampland program which restricts significantly the number of "good" EFTs.

   On the other hand the general problem, understanding for example singularities, is that String theory is still only understood at perturbative level.

   Beyond AdS/CFT, the non perturbative definition in the bulk is missing.

   Dualities can help but up to a point.

9. **Peter Woit**
January 6, 2024

   Shantanu,
One obvious problem with Strominger's A+++ for the consistency of string theory is that a conference is starting devoted to the problem that no one knows what the definition of "string theory" is.

10. **Peter Woit**
January 6, 2024

    It occurred to me that the best way to understand this video is that it's an infomercial. It was to a large degree scripted as such, with expensive graphics produced to help the sales job. Witten seemed fine with participating in such a thing, Strominger was the over the top "it slices!! it dices!!" guy. Gross went off script a couple times and had to be slapped down. I wonder if he had second thoughts about participating once he saw the result.

11. **Peter Woit**
January 6, 2024

    Giotis,
That the Strominger-Vafa calculation of 28 years ago, with all its caveats and limitations, is still promoted as the best evidence for the consistency of string theory, speaks volumes.

    On the issue of getting particle physics out of string constructions, don't believe me when I tell you it doesn't work, believe Gross, Strominger and Witten. As they admit in this video they long ago gave up on this (Strominger first). The euphemism they use is that for this to work "we would have to be very lucky", i.e. you have to assume some new physics being discovered that has exactly the right properties to point to a string theory origin. They clearly don't believe this is going to happen.

    Those young theorists who were fooled by this kind of infomercial into going into string theory and trying to connect it to reality are now upset that the people who sold them this have now given up and are stabbing them in the back. Zero chance that Gross, Strominger or Witten would hire anyone deluded enough to believe what they were selling 35-40 years ago. Latest from Stringking42069 about this is at

https://twitter.com/stringking42069/status/1743647553529270639
"Senior hep-th bros are corrupt assholes at this point. They don't support strings and push their weakass cronies instead. Fuq the lot of them.

Strings is still the best game in town. Too bad these assclowns can't recognize that anymore."

12. **Gavin**
January 6, 2024

I'm an ex-physics postdoc working in the private sector. I don't have any skin in the game.

I don't think you're doing yourself any favors giving StringKing42069 publicity. To me he comes across as extremely obnoxious and unserious. I understand the appeal because he is prolific in making anti-string-establishment statements, but I personally think it undermines your credibility to cite him. There are plenty of critics of string theory in the broader physics community who will put their name on what they say and give an actual argument, such as acollieastro or honestly almost every physicist or astrophysicist I've met who doesn't publish in hep-th.

In terms of the event itself, I personally thought it wasn't worth talking about. It's the same people saying the same things they've been saying for decades. Literally — their history of string theory stops pretty much around 2000 when Brian Greene wrote his books. I think anyone who goes to an event like that who has followed string theory for any length of time would realize nothing new was said, and anyone who hasn't probably isn't that interested in physics that they will remember anyway. For any young students inspired to go into physics, I think it's a net positive… I was inspired to go into physics by Brian Greene, but then as I went through the coursework and interacted with professors and students I actually got a better picture of physics as a whole and went into a different research field.

13. **Peter Woit**
January 6, 2024

Gavin,
The problem isn't Brian Greene, it's much, much more serious than that. You seem to have missed the main point of my posting, which is about the very significant and disturbing implications of having Witten + Strominger + Gross participating in something like this.

StringKing is an idiot (or someone very clever trying to make string theorists look like idiots). Obviously I'm not making a serious argument about string theory by pointing to his nonsense. As for whether I've devoted enough time and effort to making credible, serious arguments about these issues over the years, I think I'll let the record speak for itself.

14. **sdf**
January 7, 2024

Greene cutting off Gross everytime he tried to inject some reality into the conversation, I'm sorry but I found this hilarious to watch, it is almost like some kind of bad vaudeville.

15. **Alex**
January 7, 2024

Oh, what more can be said that hasn't already been repeated ad nauseam regarding the pathetic (maybe even terminal/end stage?) state of hep-th and of the conspicuous enablers of this situation? I started writing a longer comment but then I deleted it because I thought "what for?".

I think Karl Marx said, about history repeating itself, that this happens "… the first time as tragedy, the second time as farce." The tragedy and damage has already been done. As Peter mentioned, young researchers stabbed in the back by their mentors, a field with no prospects, with its credibility on the floor, etc.

So, this recent video and the like, I think, belongs to the farce. In that sense, the appereance of "StringKing" gives to this vulgar grotesque opera its mandatory clown. I think it's good that Peter mentioned this person since it sets the tone and background on the stage for the main characters in the video to do their dance. Let's hope this time is… finally… the *final* act of the spectacle… "Applaud, friends, the comedy is over."

16. **gentzen**
January 7, 2024

Witten mentions Nordström's theory of gravitation at 9:00. A beautiful simple Lorentz-invariant theory of gravitation consistent with Newton's theory of gravitation in the appropriate limit. But "according to Nordström's theory, the perihelia of Mercury should lag at a rate of about 7 seconds of arc per century, whereas according to general relativity, the perihelia should advance at a rate of about 43 seconds of arc per century." (quoted from Wikipedia). I guess the irony that String theory also fails to predict observations (and is perceived as beautiful by it proponents) was fully intentional.

I never heard of that theory before, and only watched that video because you mentioned it. Sorry to admit it, but I actually enjoyed it, and subsequently also watched "The Story of String Theory (Daniel Robbins)" and read an interview where John Baez said: "It turns out there's a wonderful story here: briefly, particles are to categories as strings are to 2-categories, and all the math of particles can be generalized to strings using this idea!"

This left me with the feeling that String theory might have its place as a "mathematical model" where quantum field theory can be explored beyond renormalization and perturbation theory. You just have to accept that it is not the correct theory of our universe. Like John Baez said in that interview: "Ironically, when I quit quantum gravity, I felt free to explore string theory. As a branch of math, it's really wonderful."

17. **Marvin**
January 7, 2024

A Nobel Prize winner, a Field medalist, a Breaktrough prize recipendaire, invited in their nicest suits to discuss…..about the late. They make a tribute to the acomplishement of the late, showing how it help in their career. But most of all, trying to make us not forget the late. That was my feeling.

18. **cond-mat phd student**
January 7, 2024

Gavin

I have followed pretty much the same initial trajectory as you, entering physics with the idea of studying high energy physics and moving towards a different field (stat-mech*) when I realised the state of hep-th.
I strongly disagree with your assessment that so long as we entice new physics students, hep hype is a net positive.
I find somewhat cynical the position that we should get as many physics students as possible, even if this requires borderline lying to them.

I may be overlydramatic, but personally I felt kind of betrayed.

This strategy reflects very badly on physics as a whole, imho.

* alas, I now have the feeling that cond-math.stat-mech is not in much better shape than hep-th, see the Majorana fermion debacle.

19. **Diogenes**
January 7, 2024

What you will discover from reading philosophy is that unless you precisely define terms you will have arguments for centuries. Here ‘string theory’ remains mostly undefined, even within the timeframe of a single conversation, so arguing about the merits is really pointless. I think what the panel is really saying is: “We theorists get an A for effort over the last century, and even if a subset of us sucked up all the oxygen for the past 30 years and didn’t get that much done, please don’t cut our funding or try to tarnish our shiny reputations. This stuff is hard and we did the best we could.”

20. **Peter Woit**
January 7, 2024

Marvin/Diogenes,
The extent to which a “state of string theory ” discussion was mostly about how wonderful the string theory unification idea of forty years ago was (with any less than positive comment squelched) I think reflects the way this was really about defending the interests of the participants.

For Gross and Witten, they’re now getting to the age where one thinks about one’s legacy and how one will be remembered by history. For both of them it’s looking like they will be seen as having huge accomplishments, but tarnished by their spending much of the later part of their careers promoting a failed idea. It’s in their interest to have the 1984/5 proposal for a TOE seen as of such overwhelming beauty and promise that what happened later can be excused. Brian Greene’s career is even more tightly connected with this.

For Strominger with his A+s for current string theory related work, an important issue is how people see his ongoing “celestial holography” program, which has been promoted by tying it to AdS/CFT and string theory.

21. **Charles Cherqui**
January 8, 2024

My favorite part is he says when Strominger says that if he didn’t believe in it then he wouldn’t be working on it just after telling us out his last 80 papers 2 have been on strings. So he’s not working on it? Then what does that indicate about his faith in it?

22. **Peter Woit**
January 8, 2024

Charles Cherqui,

I think you missed this part:
“string theory has now come to be a word that is used for everybody who is working on anything that goes beyond the standard laws of physics”
I guess that according to him I’m a string theorist.

When he says he believes in “string theory” and gives it an A+, “string theory” just means whatever he is working on, so he’s just saying that he believes in what he’s working on and he deserves an A+.

23. **Charles Cherqui**
January 8, 2024

Peter,

No, I picked that up but took that as him putting lipstick on a pig. Of course it makes sense that rather than admit failure he would simply draw a bigger box around what “string theory” means. In that now it means everything me and my friends are into, regardless what it is. String theory is now simply whatever they are doing to try to go beyond the SM.

Whether he meant to or not, he exposed a truth. If, in their heart of hearts, they really thought this was the right answer they’d still be working on it, just like Einstein kept at it undeterred right up to his death.

24. **Peter Woit**
January 8, 2024

Charles Cherqui,
Gross did something similar, claiming AdS/CFT shows string theory equivalent to a QFT, so “string theory” means “QFT”.

Yes, all these people have abandoned the “string theory” that they were advertising in the infomercial. The point of quoting “stringking” was to show that those like him who still believe in the original vision and are trying to make it work are well aware these people have abandoned them and feel that they have been stabbed in the back.

25. **nikita**
January 8, 2024

Interestingly, Einstein also spent the last decades of his life pursuing a research program that was almost universally considered a dead-end. He didn’t “promote” it though in any sense, as far as I know.

26. **Peter Woit**
January 8, 2024

nikita,

Yes. A good analogy perhaps to the situation we’ve been in since 1984 would be if theorists in 1920 had all decided to follow Einstein, and spent the next forty years not pursuing QM and QFT, but studying ever more elaborate classical generalizations of GR, very excited about how they unified EM and GR. These generalizations somehow would never manage to make testable predictions, with the excuse that the properties of atoms were determined by the solutions to non-linear equations involving geometry at the Planck scale. So, while untestable, the theory would still deserve an A+++.

27. **Kawabata Hiro**
January 8, 2024

Paraphing a paraphrasing of Planck –

“Physics advances one funeral at a time.”

28. **Héhéhé**
January 9, 2024

Peter, it is time to admit that you are StringKing4206 , you clever troll !

29. **Peter Woit**
January 9, 2024

Kawabata Hiro,
Problem is that the younger people, trained by spending years reading Polchinski, are even more deluded about string theory than their elders. Looking at the people in this video, their age is inversely correlated to their degree of skepticism.

Another problem is that it has become clear that this is not going to change on the time scale of my life expectancy.

30. **Peter Woit**
January 9, 2024

Héhéhé,

No, not me. But if it was, I would say that, wouldn’t I?

31. **Fadde'ev-Popov ghosts**
January 9, 2024

Mr. Strominger modestly attributes himself and his followers an “A+++” grade, yet my reservations linger, as most AdS/CFT experts might not be as generous, even in Leibniz’s “best of all possible worlds”.Reading arXiv:2304.08292, one discovers a competing program to “celestial holography” known as “Carrollian holography.” While the – conjectured CFT dual to perturbative quantum gravity defined on a Minkowski space background is anticipated to be situated in the celestial sphere Sˆ2 at infinity, the CFT corresponding to the latter pertains to null-infinity R \times S^2 and obeys to the Carroll symmetry algebra associated with a null Lorentzian hypersurface. Considering that the latter is a codimension one submanifold of Penrose’s conformally compactified Minkowski space, in contrast to the codimension two nature of the former, it appears that Carrollian holography more faithfully embodies the original t’Hooft-Susskind holographic principlE than its celestial counterpart. Even attempts to reconcile both programs are likely haMpered by the distinct dimensionality of the CFT backgrounds, leading to different degrees of freedom and, consequently, distinct thermodynamics of asymptotically flat black hole metrics.

Adding further difficulties, linking asymptotic BMS symmetries with the Virasoro symmetry algebra necessitates extending the BMS group from the semi-direct product of Poincaré with supertranslations to the so-called superrotations. These superrotations, acting as local diffeomorphisms in the celestial sphere, possess generators represented by meromorphic vector fields on S^2, as outlined in arXiv:1102.4632. However, in mapping the phase space from past null-infinity \Scri^- to future null-infinity \Scri^+, enforcing the antipodal matching condition becomes challenging when allowing poles associated with the generators of superrotations. This presents an already classical-level problem in defining the phase space for theories involving superrotations.

Furthermore, celestial amplitudes, discussed in works like arXiv:1711.06138, arXiv:1705.01027, and the more recent arXiv:2210.04725, are defined using a generalization of the optical theorem with a singular shadow integral transform (see App.B of 2210.04725). This singularity renders the inverse ill-defined, leading to the use of vertex operators in celestial spheres with conformal weights Δ featuring non-vanishing imaginary parts, ultimately rendering the theory non-unitary. It’s imperative to recall that the primary goal of holography was to address the issue of the unitary evolution of black hole states. An “A+++” grade, indeed?

32. **Peter Woit**
January 9, 2024

Fadde’ev-Popov ghosts,
Thanks, but I really don’t want to try and host a discussion of the technicalities of celestial holography here, which is off-topic and beyond my competence to moderate.

It does though provide a good example of the current problems generated by string theory hype, including Strominger and his A+++s. “Celestial holography” is one of the very short list of hot topics in hep-th. The focus on the celestial sphere agrees well with twistor geometry and I see that twistor people are working in this field. New symmetries and how to exploit them is another of my favorite topics.

But if I try and start figuring out what is going on in this subject, I find it explained as being motivated by AdS/CFT and string theory, see for instance
https://arxiv.org/abs/2111.11392
where we’re told “A crowning achievement for the celestial holography program would be for it to determine concretely whether string theories are the only consistent theories of (asymptotically flat) quantum gravity.”

Given the history of string theory and AdS/CFT hype, it’s very hard to take seriously a lot of the stated motivation for this subject. Strominger’s performance on this video makes it really hard to take his arguments about what’s promising seriously, and he’s the main leader of this field. So, I start out sympathetic and interested, but have a hard time figuring out which parts of the arguments for the subject are credible and which aren’t.

33. **Shantanu**
January 10, 2024

Peter: out of curiosity, why aren’t there many public debates between proponents and critics of string theory? Such debates exist a lot in astrophysics (Shapley-Curtis debate, Lamb-Packzynski debate, Kroupa- White debate , etc). the only few I am aware of is one between Krauss and Brian Greene and I think there was another one between you and Jim Gates. Probably there should be more.

34. **Peter Woit**
January 10, 2024

Shantanu,

My impression is that there are few if any string theorists who want to participate in a serious debate on this topic, because they know that they would be arguing a very weak case. In recent years the only examples of this sort of thing I can think of were organized by the IAI people in Britain, e.g. this
https://iai.tv/video/the-trouble-with-string-theory
In at least one case, I was disinvited from a planned appearance on a panel discussing string theory, as far as I could tell because a prominent string theorist wouldn’t participate if I was there.

35. **Peter Woit**
January 10, 2024

It occurs to me that while I don't normally like this kind of crude explanation of people's behavior, in this case there's a simple explanation of Strominger's "A+++" business. At the moment he's PI on at least the following grants:
Black Hole Initiative: \$16 million so far from the Templeton Foundation
Collaboration on Celestial Holography: \$8 million from the Simons Foundation
DOE grant DE-SC0007870: \$3.5 million from DOE
The summary of this last one assures us that successful BSM theory has been done, and that the authors are on the road to computing the electron mass and understanding the origin of the universe:
"Vigorous efforts made over the last several decades have advanced our understanding of the fundamental laws of nature beyond the standard model of particle physics. Further advances would potentially include unification of the forces, the reconciliation of quantum mechanics and gravity, a derivation of the standard model couplings, a universal explanation of the area law for horizon entropy, and a theory for the origin of the universe."

Strominger has been in the fund-raising business for a long time. For an unusual example, see here
https://cerncourier.com/a/endowment-reward-for-string-theorists/

36. **Charles Cherqui**
January 10, 2024

Open question. Maybe I'm just slow but one thing I've never understood is if ads/cft relates 5-D Gravity and 4-D QFT, how can it have anything to do with String Theory. which is 10-D.

37. **Peter Woit**
January 10, 2024

Charles Cherqui,
The conjecture more precisely is about string theory on AdS5 X S^5

38. **James**
January 11, 2024

Peter,
I think it is time for you to write another book that would look at the string (mis-) adventure in its larger perspective. We are now seeing an entire generation gone by and the derailment of physics by a relatively small group of people. While some of them are yet alive we need to hear from them if any have the integrety to try and clear the air about what has happened.

39. **Bernard l'Amateur**
January 11, 2024

As far I know, string theory research is still done using pencil and paper, right?
I was wondering what Strominger would use \$16 millions for?
Is it for doing super complicated computations by hiring IBM supercomputers? Is it for paying postdocs \$200k+ a year?
Is it for purchasing expensive experimental equipment for testing their theories?
And how many years is such a grant spread over? Does this mean that research in string theory is still the most profitable business in HEP?

Thanks,
Bernard

40. **Peter Woit**
January 11, 2024

James,
I'm not planning on writing another book about the string theory business. The old one still holds up quite well, and I want to spend my time on other more promising pursuits.

I've done and continue to do my part to deal with the string theory hype problem with posts here on the blog. A question for all in the theoretical physics community is why you are doing nothing to deal with this problem. It's not going away.

41. **Peter Woit**
January 11, 2024

Bernard l'Amateur,
Strominger is just one of many PIs involved in the three grants I mentioned, so the money is divided amongst many people. I assume all of them fund the usual sorts of things:
1. Postdocs
2. Grad students (tuition + stipends)
3. Conferences + travel
4. "summer salary" for the PIs
5. Overhead for the universities involved

For instance, for the Simons collaboration grant, see postdoc ads here
https://simonscelestialholographycollaboration.org/positions/

This is funding a very significant number of postdocs and likely grad students, but as far as I know zero funding for permanent positions (except indirectly through the overhead). This exacerbates the problems of the field of
1. An overemphasis on hiring in a small number of specific areas that are "hot topics".
2. An overproduction of Ph.Ds and postdocs, who face long odds against getting a permanent position.

42. **David Roberts**

January 11, 2024

From that 2003 CERN Courier news item:

> Andrew Strominger of Harvard, who facilitated [Jeffrey Epstein's] gift

That did not age well!

43. **Peter Woit**
January 11, 2024

David Roberts,
To be fair to Strominger, 2003 was before Epstein's illegal behavior was publicly known. I haven't seen Strominger's name come up in the list of prominent theorists who were hanging out with Epstein at a conference he paid for in 2006, shortly before he was arrested.

44. **George**
January 12, 2024

Peter,

your comparison with Einstein hypothetically being followed by whole theoretical community in his pursuit of combining electromagnetism and gravity is spot on. One question remains, though. To be fair to Einstein, we need to assess achievements of prominent string theories BEFORE they started their string theory revolution. We all know remarkable legacy of Einstein and what he did prior to starting his unsuccessful quest. Are there such achievements to be found in works of Witten (in physics, not in mathematics) and other string theorists?

45. **Peter Woit**
January 12, 2024

George,
David Gross's discovery of asymptotic freedom and development of QCD was a huge advance and his Nobel Prize is well-deserved. During the early 1980s, before string theory, Witten did remarkable work that was very much physics, see for example his papers on "current algebra" (effective theory of mesons and baryons).

46. **Indiethphy**
January 12, 2024

Einstein program, in one of its incarnations, was actually followed, and to great success, by Weyl, Schrödinger, London, maybe/almost Klein, Yang-Mills, Schwinger, Gell-Mann, Glashow, Weinberg and others, in the form of the gauge principle, which is the backbone of the Standard Model. The story is of course well known, and it nicely rendered for instance in A. Zee's book "QFT as simply as possible ".

47. **Peter Woit**
January 12, 2024

Indiethphy,
I don't think gauge symmetry is really the "Einstein program". It's not something he followed up on, and that's one of the main problems with the research program he followed in the second half of his life. Among the people you mention who did pursue gauge theory, Weyl at the beginning is the only one who took a geometric perspective, the others didn't (Weinberg very explicitly claimed that thinking about geometry was a mistake). If Einstein had pursued gauge theory instead of what he did do, that would have changed the history a lot.

Besides ignoring gauge symmetry, the other problem with Einstein's program is that as far as I know, he paid no attention to the geometry of spinors.

If he had pursued spinor geometry and gauge symmetry in his attempts to unify physics, the results would have been radically more positive (even if he had continued to be skeptical about quantization) One could argue that the string theorists have been making exactly this mistake again.

48. **rfb**
January 12, 2024

Einstein did do some work on spinors. In the early 1930s, he and the mathematician Walther Mayer published several papers on semivectors, a type of generalization of spinors. They gave up on the idea after some discouraging words from Pauli along with a demonstration by Bargmann that the same results could be achieved by superposition of ordinary spinors. See Chapter 5 of "Einstein's Unification" by Jeroen van Dongen.

49. **D R Lunsford**
January 13, 2024

Actually Einstein did try to deal with spinors – they were sometimes referred to as semi-vectors. Distant parallelism was another go at local frames. Weyl's original gauge-invariant geometry was a direct attempt to extend GR and it lives on at the base of field theory. So IMO there is no comparison of Einstein's work and string theory delusion. Einstein was looking for a geometrical theory with a proper Hamiltonian that could then be quantized in the Dirac way. That was the right approach IMO and can still work. BTW Fock dealt with spinors in GR in a completely correct way. Fock had a better understanding of GR than almost everyone who wasn't Pauli, Weyl, Dirac, or Einstein.

-drl

50. **Shantanu**
January 13, 2024

Peter, something IT. A recent colloquium on string. theory at University of Alabama.
Would be interested in your take on this
https://www.youtube.com/watch?v=AdErRdW4jVM

51. **Peter Woit**
January 13, 2024

Shantanu,
Thanks. Took a quick look. It's an interesting example of what's going on with string theorists. 90% of it was advertising for string theory, all decades old. The last 10% was the news that the speaker is moving on from string theory to something else: "generalized symmetries".

The problem with this very common situation is that, while string theory research is on the outs, hep-th research is in the same situation as it has been for many decades: almost everyone works on the same short list of "hot" topics. Today there's a very short list of these, to see it is even easier than it used to be: just look at the list of "Simons collaborations", the winners of their competition to choose the latest few hot topics. Basically now these are:

1. "It from Qubit": study toy models supposed giving gravity from quantum information considerations. This may have peaked and be on the way down, after it jumped the shark with the wormhole publicity stunt a bit more than a year ago.
2. Amplitudeology a la Arkani-Hamed, or also the bootstrap: the revival of the failed S-matrix program of the late sixties.
3. Celestial holography: Strominger is a talented fund-raiser.
4. Generalized symmetries.

Getting a job if you're doing one of these is quite difficult. If you're not doing one of these it's impossible. Maybe one of them will give a big breakthrough in our understanding of fundamental physics, but no evidence for that so far. If the way forward is something other than 1-4, the field is in its usual position of making any young person working on the right way forward unemployable.

52. **Mark Hillery**
January 14, 2024

One wonders if space for different approaches could open up at places off the beaten track. This was the case with quantum information and foundations in the early days. It emerged from places like Wheeler's group at UT Austin and Charlie Bennett and company at IBM, and not from the IAS or Harvard. An example of a very successful career coming from this era is that of Bill Wootters. Bill came from Wheeler's group, where he was one of the authors on the no-cloning paper, and spent the rest of his career at Williams College, where he made fundamental contributions to entanglement theory as well as being one of the originators of quantum teleportation. Could something like this happen in high-energy theory, or is it out of the question?

53. **Eitan Bachmat**
January 14, 2024

Dear Peter
I t seems to me that your view of hot topics and the choices of the Simons foundation is too localized. I think the more correct view is that what is promoted is the advancement of symmetries, dualities and generalizations as beatiful organizing and cross disciplinary concepts which are extremely effective in math and science. More generally, it seems that cross disciplinarity is strongly promoted. There is a symmetry based collaboration on the engineering of fusion, essentially an elaboration of Noether's theorem, there is another about extreme wave phenomenon. Symmetries and symmetry breaking which is mostly classical, combining engineers, mathematicians and physicists, there is the homological mirror symmetry collaboration which is physics inspired pure math which has great results regardless of wether it has anything to do with actual physics and you can apply this analysis to most collaborations, There are a few pure math and a pure computer science collaboration, but these are the exceptions.
Given this interpretation, its easy to understand the choices related to hep. They are by the more mathematically sophisticated and symmetry and duality producing members of the community and the more mathatically sophisticated topics.

54. **Peter Woit**
January 14, 2024

Mark Hillery,
Wooters and his career is an interesting case. From his CV online, after his Ph.D. at UT Austin he stayed on two more years as a "postdoc/instructor", then went on to Williams, where he stayed the rest of his career. What he didn't do was get one or more regular research postdocs and then a tenure-track job at a research institution. With the kind of work he was doing, likely he would have had no chance at such jobs, even with Wheeler's backing.

I remember looking into liberal arts college jobs like the Williams one, since these places might not care so much about whether your research program is in the hot hep-th area. But there aren't so many such jobs, hard to get one without a leg up like backing from Wheeler.

It's even more the case now than in 1980 that you absolutely have to be working on one of a small number of hot topics to get a postdoc or a tenure-track job at a research university. Take a look at what successful candidates here
https://sites.google.com/site/postdocrumor/
are working on. From looking at quite a few examples, they're all exactly the same few topics. Maybe an accurate way to describe these topics would be not by technical names but
1. What Strominger is working on.
2. What Vafa is working on.
3. What Seiberg is working on.
4. What Maldacena is working on.
5. What Arkani-Hamed is working on.
I don't see anyone getting a job with a background working on ideas off the beaten track. What I find remarkable about the HEP theory community, faculty and funding agencies alike, is that they refuse to acknowledge that this system is broken and take steps to create a viable career path in which people could succeed working on less popular topics.

55. **Peter Woit**
January 14, 2024

Eitan Bachmat,
My comment was not about the Simons collaborations in general, but very specifically about the ones in hep-th, the complete list of which is as far as I can tell
1. Celestial Holography
2. Confinement and QCD Strings
3. Global Categorical Symmetries (this one is not just hep-th, but cross-disciplinary with a large math and condensed matter part)
4. It from Qubit
5. The non-perturbative bootstrap

Looking at the list, I wouldn't describe it as embodying mathematical sophisticated mathematics (other than the cross-disciplinary 3.). Again, besides 3, symmetry not a big theme for 2 and 4. For 1. and 5, the interest in symmetries is only in certain very specific ones (conformal invariance/ specific asymptotic symmetries).

My general impression of how the Simons Foundation chooses topics is that they focus on already active areas of research at elite institutions, and like to fund people who are already widely acknowledged as the best in their field. They also are interested in funding work that significantly involves computation, and, yes, cross-disciplinary work.

What I know well is the pure math and hep-th areas. There, the pure math things they are funding are very successful and active research directions, maybe their money helps accelerate progress. On the hep-th side I don't think their impact is positive, accelerating trends in an unhealthy subject is not a good idea.

56. **David Roberts**
January 14, 2024

@Peter

I certainly didn't mean to imply anything untoward with my comment on the quote about Strominger, I just meant that's it's a really unfortunate sentence, given what we know now.

57. **Oskar Skalski**
January 19, 2024

I'm just a person with interest in physics without any background (other than what I had at my IT studies) and I was once enamoured with string theory thanks to Brian Greene's documentaries on National Geographic. Then when I've delved more into the topic I saw all the shortcomings of this idea and how sometimes it is treated as almost a religion (vide Michio Kaku). But I try to follow every new interesting discussion on youtube (and WSF has some very interesting ones) so I've started it and then was a bit baffled with it. It seems like this guys are living in some parallel reality. What was more ridiculous all guests are proponents of ST and they didn't invite any skeptics for the discussion to maintain some balance. Riger Penrose would be a good choice for example. I didn't finish the whole discussion because it seemed that it wasn't worth my time. They just regurgitate old points without taking some self-reflection about the topic.
At least they didn't invite Michio Kaku. It seems like he became a meme this days.

58. **Peter Woit**
January 19, 2024

Oskar Skalski,
The string theory community in general recognizes that Michio Kaku regularly puts out indefensible hype about their subject, but does nothing about this. The depressing thing about the panel discussed here is that it makes clear that the reason for this is that the leaders of the subject don't have a problem at all with indefensible hype, they'll participate in it given the opportunity. Among the endless Kaku nonsense, hard to think of a case where he went on the way Strominger does about the current state of string theory deserving an A+++.

59. **Shlomi**
January 23, 2024

It's interesting that Strominger went from assigning ST "3 As, 2 Bs, 3 Ds and 2 Fs" in 2010 to A+ now! See https://www.math.columbia.edu/~woit/wordpress/?p=3206
What could be the reason for the seeming improvement?

60. **Peter Woit**
January 23, 2024

Shlomi,
Grade inflation at Harvard in recent years seems to be even worse than I had thought…

61. **Peter Woit**
January 24, 2024

Shlomi,
More seriously, that previous set of grades was given at a Harvard colloquium in front of his colleagues. If he had tried the "A+++" business there, everyone would have just laughed at him and if they thought he was serious, he would have completely lost credibility with his colleagues.

On the other hand, at this kind of informercial, he's assuming none of his colleagues are watching, or if they are, they're in on the idea that it's all right to lie to the public, in the service of retaining their support for your research.

62. **anon**
February 10, 2024

Sounds to me like a eulogy given at a funeral, praising the dead while carefully avoiding any negative speech (as Gross tried). Strominger gave a C back in 2010 when string theory was still kind of alive.

Spring Course

January 11, 2024
Categories: Uncategorized

Starting next week I'll be teaching a graduate topics course, with the general plan to develop much of the quantum field theory of the Standard Model in a form accessible to mathematicians, emphasizing the connections to representation theory. There's a course web-page here, notes will start appearing here once the course gets underway. While the course will be aimed at mathematicians, I'm hoping that some physicists might find it interesting and worth trying to follow.

The last time I did something like this was back in fall 2003. At that time the course was aimed at getting math students to the point of understanding the TQFTs for Chern-Simons theory and Donaldson theory and was very much based on the path integral. This time I'll be mostly sticking to flat space-time and using more representation theory. Also, a lot more about spinor geometry, as well about about how Euclidean and Minkowski space-time versions of QFT are related.

Comments

1. **Michael Weiss**
   January 12, 2024

   Peter, might it be possible to post Youtube videos of your upcoming lectures? I bet these would be widely viewed (nationally and internationally) in the coming decade, just as are such educational materials from MIT, Stanford and Cambridge UK.

   These physics-related Youtubes are typically free, and so I do not know how these institutions handle the economics of such outreach. Ditto for a course in introductory group theory by Dick Gross from ca. 20 years ago.

   PS Remember the videotaped EM Purcell lectures at the Cabot Library in the 1970s? What a loss to the next generation that these were not transferred to a web-based format, like Coleman's lectures on QFT from the same decade.

2. **Peter Woit**
   January 12, 2024

   Michael Weiss,
   This class will be aimed an at advanced level, small audience, taught in a small room with no video equipment. So no video, which I'd prefer anyway.

   I'm hoping to put a lot of effort into the notes and that the result will be something many people can get something out of. Ideally, after each lecture the notes posted will correspond closely to what I talked about. If people want to follow along with those and send me questions and comments that would be great.

3. **Timothy Keller**
   January 12, 2024

   I will be looking forward to your notes, especially the discussion of representations
   Just an algebraist with an interest in applications in physics.

4. **Art**
   January 19, 2024

   I think the link in the intro of your new notes is missing an intermediate site: https://www.math.columbia.edu/~woit/LieGroups-2023/qmnumbertheory.pdf. Looking forward to trying to follow along.

5. **Peter Woit**
   January 19, 2024

   Art,
   Thanks! Will fix.

Know Time Podcast

January 14, 2024
Categories: Uncategorized

For another podcast/interview with me that was recently recorded, see Maths, Twistors & String Theory. Know Time is a series of podcasts that is a project of Shalaj Lawania, and I was impressed by the effort he put into trying to make sense of a complicated and inaccessible subject. For an excellent pairing with what I have to say, see his earlier interview with Matthew Kleban, who has a more positive take on string theory, the multiverse, etc.

No Comments

Quick Links

January 20, 2024
Categories: Uncategorized

A few quick links:

- There's a one-day conference next Friday at the IHES, recognizing Dustin Clausen's appointment to a new Jean-Pierre Bourguignon Chair. Should be several interesting talks, see here.
- There's an ongoing conference at the KITP on the topic of What is String Theory? So far, none of the online talks address that issue. Evidently there was a discussion of the topic last Wednesday, but not recorded. Were any readers here in attendance and willing to report on that event? Next chance to find out what string theory is will be a Monday Blackboard Lunch talk by Gopakumar.
- In April there will be an IUT conference hosted by Zen University in Tokyo, see here. All the speakers but one are from RIMS. For news from the senior people devoted to IUT, Ivan Fesenko has moved to Westlake University in Hangzhou, and Shinichi Mochizuki is has been blogging here.
- There's a new Shanghai Institute for Mathematics and Interdisciplinary Science, headed by Shing-Tung Yau.
- For those following what happens with the small number of permanent positions in particle theory, news from 4 gravitons.

Update: One more, which I'm quite interested in. Scholze will be giving a series of three Emmy Noether lectures at the IAS in March, topic Real local Langlands as geometric Langlands on the twistor-P^1.

Comments

1. **Shantanu**
   January 23, 2024

   Peter : Gopakumar's talk is online.

2. **Peter Woit**
   January 23, 2024

   Shantanu,
   I watched the talk. Basically his "What String Theory Is" answer is an explanation of the hope that large N QCD is a string theory (zero about string theory providing quantum gravity or unification). He takes this story up to about 25 years ago and AdS/CFT, doesn't even mention the basic problem why this doesn't give you QCD (no way to get asymptotic freedom). Asked what his fondest hope for 20 years from now, it was to find the string dual to QCD.

   So, as for "What Is String Theory?": string theory unification dead, only hope is to keep going on the AdS/CFT/QCD front and somehow make progress on an idea that thousands of people have been stuck on for 25 years.

3. **HJM**
   January 25, 2024

   Regarding the ABC conjecture, I believe that this paper that came out today by Kirti Joshi might be of interest: 2401.13508.

4. **interested_bytander**
   January 25, 2024

   Related to IUT, although explicitly saying some steps are missing in Mochizuki's papers, is the latest preprint by Joshi claiming to have now obtained a proof of several variants of Mochizuki's Corollary 3.12 https://arxiv.org/abs/2401.13508

5. **Kurt Schmidt**
   January 27, 2024

   Kirti Joshi should do what Peter Scholze and Terence Tao did and get the Lean community to formally verify his papers on Mochizuki's corollary 3.12 in the Lean proof assistant so that there are no doubts as to whether his proof is right or not.

6. **Peter Woit**
   January 27, 2024

   Kurt Schmidt,
   There's no argument here that experts think works but needs more careful checking.
   What's needed in this case is just the standard procedure that the math community has developed over centuries to deal with a claimed proof: submit the paper to a first-rate journal for an impartial editor and referees to deal with in a manner acceptable to experts in the field.

   The public fiasco of the IUT papers was caused by the failure to do this.

   A common reaction I'm getting now when I ask experts about this is that too much time and attention has already been wasted on the topic, so not a good idea to give it more attention unless and until there are experts who have looked at it and think it works.
   So, unless there's news of that sort to report, enough of this for now.

7. **somereader**
   January 29, 2024

   It should be noted that in his latest paper Joshi claims to prove some lower bound on the size of certain sets Theta (so that something non trivial occurs in Cor 3.12), but not the Mochizuki type upper bounds that would lead to Diophantine applications (this was stated in the older paper https://arxiv.org/abs/2111.04890v1 "towards Diophantine estimates", and the new paper appears to be an expanded version of that paper at the global level, it doesn't provide a sharp upper bound, only in remark 7.8.3.2. that the upper bound is plausible in the global case).

8. **Peter Woit**
   January 29, 2024

somereader,
Thanks. I have been hearing comments about Joshi's proof that it is unclear whether his version of the Corollary may differ from Mochizuki's and not have the same implications, in particular not getting abc.

9. **Doug McDonald**
January 30, 2024

Peter: I know this is getting old. But, is it clear to anybody whether Joshi is claiming that this preprint is in fact supposed to truly prove abc itself (though the known steps following 3.12)? For a while I thought so, but interestingly I find that the string "abc" only appears twice in it, neither in a direct statement.

10. **Peter Woit**
January 30, 2024

Doug McDonald,

One thing I have heard from some experts is that it is unclear to them whether Joshi is claiming a proof of abc.

11. **Peter Woit**
January 31, 2024

To clarify the last comment, it's pretty clear that Joshi is not explicitly claiming a proof of abc in this last paper. It would be great if someone would get him to state clearly whether or not this is supposed to be a proof of abc, and if not, what is missing.

One possibility that I can think of is that he hasn't checked/doesn't trust Mochizuki's argument that 3.12 implies abc, and that he hasn't come up with his own alternative argument using his own methods.

12. **Kirti Joshi**
January 31, 2024

@Dr. Woit,

What you assert is indeed correct, I prefer to do my own due diligence independent of the claims of Mochizuki's IUTT IV. The abc-conjecture is an expected, but delicate consequence of Corollary 3.12 (and some other results), but that is a work in progress and my conclusions (with proofs) will appear in my paper: Construction of Arithmetic Teichmuller Spaces IV.

In answer to your earlier post, my recently released paper (Constructions III) does provide many versions of Corollary 3.12 including Mochizuki's version and I provide a `Rosetta Stone' which facilitates a detailed comparison of the two theories. My formulation and proof of the geometric case of Corollary 3.12 is given in Constructions II(1/2) . For precise assertions, please refer to my papers.

13. **Kurt Schmidt**
February 5, 2024

somereader, Peter Woit

Kirti Joshi said that he will be proving the upper bounds in a forthcoming paper:

https://mathoverflow.net/questions/435110/consequences-of-kirti-joshis-new-preprint-about-p-adic-teichm%c3%bcller-theory-on-th/463562#463562

Particle Physics Is Not In Crisis

January 20, 2024
Categories: This Week's Hype

For a low-rent version of the self-congratulatory program discussed here, Bad Boy of Science Sam Gregson has a new video up entitled Particle Physics Is Not In Crisis – but we can make improvements. Cliff Burgess plays the Strominger role, explaining that the idea that there's any problem with what's going on in particle theory is "a nothing-burger" and "a complete non-issue". Asked to rank any such problem on a scale of 0-10, he gives the Strominger-esque ".0001". Martin Bauer goes for "1".

The take on the question is much the same as Sean Carroll's four-hour plus explanation that there is no problem, but shorter. It's similar to Carroll in that no one who thinks there is a problem was invited to participate, or even gets mentioned by name. There's a repeated reference to mysterious "Twitter influencers", which I find very confusing because just about the only particle theorists I see spending time on Twitter going on about the state of the field are Bauer and Burgess. They can't mean me since I've so far resisted the temptation to enter Twitter discussions. The idea of trying to have a serious discussion of complex scientific issues in the Twitter format never made any sense to me, and (StringKing aside) I find it hard to think of any tweets by anyone that shed any light on serious issues in this area.

The more serious part of the program was the discussion among the two HEP experimentalists of the state of their field, which got a 5-6 on the crisis level scale. I wrote about the problem there five years ago, and very little has changed, other than that we're five years closer to the date when there will no longer be an energy frontier machine running anywhere in the world. The underlying problem wasn't really explained. CERN is working on it, but there is as of now no specific plan with specific budget numbers for what to build next. Maybe I misunderstood, but it seemed that Bauer and others were talking about how the field just needed to convince funding agencies to support budget numbers of order \$100 billion, which is a pipe dream.

Update: Latest podcast from Sean Carroll has nothing to do with the crisis in particle physics, but he starts off anyway with this:

> You may have heard there is a crisis in physics. No, there's not. I mean, there's little tiny crises, but that's the very standard procedure if you're doing science at the cutting edge, is all sorts of puzzles that we don't know the answer to.

"Little tiny crises" is I guess his version of the Cliff Burgess ".0001" and Andy Strominger "A+++".

Comments

1. **Gavin**
   January 20, 2024

   Just to emphasize, I am not and was not a string theorist (though I was an astrophysics postdoc).

   Are you really, really sure you want to claim StringKing "shed[s]… light on serious issues in this area"?

   I think his Twitter/X is obscene, toxic, and not serious. He represents a thread in physics academia that is at least as toxic to people in the field as the "groupthink" that can come from senior people. He's obnoxious, highly judgmental of other peoples' work without thinking he needs to provide an actual argument, and convinced of his own brilliance. This brand of junior physicist is quite common and (I speak from experience) responsible for driving many brilliant people away from the field. I strongly believe that in platforming this person, you are supporting a form of toxicity in academia at least as bad as the one you are fighting against (string theory groupthink, which I agree is also a problem).

2. **Peter Woit**
   January 20, 2024

   Gavin,
   That reference was tongue in cheek. The material on the "stringking" account is so obviously juvenile and idiotic that I still think it likely it's a parody trying to make string theorists look bad. That you think he's a common type among young string theorists doesn't look good for them.

3. **Sabine**
   January 20, 2024

   I have an idea about the mysterious twitter influencers lol

   Those guys crack me up. Like is anyone surprised that particle physicists don't want to admit particle physics has a problem, part of the problem being that they don't want to admit they have a problem?

   They're in for a rude awakening when funding bodies all over the world realize that particle physics eats up shitloads of money for very little in return and that they can use that money for better things.

4. **Bill**
   January 21, 2024

   They need grant mula and they obviously don't want to do much to get it. So they just repeat over and over again the same calculations. Most of the academe is like this.

   > I have an idea about the mysterious twitter influencers lol

   Sabine, do you think you are an influencer? lol

5. **Mike**
   January 21, 2024

   Surely they are chiefly referring to Eric Weinstein? He's not a Physicist or mathematician, but definitely a twitter influencer who spent a lot of time critiquing HEP on twitter.

6. **Alessandro Strumia**
   January 21, 2024

   Twitter discussions can be fast: Sam Gregson blocked me on Twitter for just answering "yes" to his question "is there a crisis?". The problem is that institutional

communications are similarly closed, up to the point that many serious physicists avoid getting involved in the unrealistic forced institutional optimism. Collider institutions seem to fear that, by openly discussing the problem, the system might collapse and the huge funds got by nuclear physics after 1945 might dry up. But in this way resources that still come are passively used to maintain the status quo. The hope was that LHC would have solved the problems by discovering big new physics, but who still hopes that? Who believes that LHC will really run until 2040? That 6000+ people are still needed to analyze LHC data?
The field should recognise the problem and try addressing it by shifting current big resources and best experts to trying to develop an innovative muon collider. If this is not done, the risk is that in ≈10 years from now the resources will evaporate anyway and the field will quietly fade away. Already now, bright students understand there is a problem and often avoid joining the field, so the problem grows.

7. **Krzysztof**
January 21, 2024

There is some recent progress on the future collider front:
"Construction is anticipated to begin around 2027-2028, pending government approval, with an estimated duration of 8 years. The commencement of experiments could potentially initiate in the mid-2030s."
https://inspirehep.net/literature/2740505

8. **Peter Woit**
January 21, 2024

Mike,
Hard to know whose arguments they are critiquing, which seems to be an intentional tactic. There were some graphics in the background, one of which included a blog post of mine (but it was a blog post about something Sabine Hossenfelder had written….).
https://www.math.columbia.edu/~woit/wordpress/?p=11551

9. **Peter Woit**
January 21, 2024

Krzysztof,
That's the Chinese CEPC proposal, which has not been approved for funding and as far as I know doesn't even have a site picked out. Similarly with the ILC proposal in Japan, it's very unclear whether these things will ever get funded. The big problem is the cost, coupled with the fact that they won't get to higher energies than the LHC.

10. **Thomas**
January 22, 2024

To be fair, Gligorov explains why it is hard to see how entering experimental particle physics as a graduate student is a good idea. The best you can hope for is to be a small, poorly paid, cog in a large machine with a very uncertain future.

11. **Peter Woit**
January 22, 2024

Thomas,
I should make clear that my problem with this is with the theorists, not the experimentalists. The experimentalists are coming off great successes (discovering the Higgs, ruling out SUSY) and facing a crisis not their fault but due to basic physics, which makes getting to higher CM energies very difficult and very expensive. They may be trying to put the best possible face on the problem, but they acknowledge they have a problem.

The theorists on the other hand are in a situation that is entirely their own fault, for decades pursuing failed ideas, while hyping these as great successes. Claiming that the situation now is A+++, .ooo1/10 of a crisis, or only a normal "very tiny" crisis is just absurd, and exactly the underlying source of the problem. If you refuse to acknowledge you have a problem, you're not going to do anything to deal with it.

12. **clayton**
January 22, 2024

four hours is a lot of hours to say "nothing to see here"

13. **Peter Woit**
January 22, 2024

clayton,
As Shakespeare put it:
"The lady doth protest too much, methinks"
Applies not just to four hours, but to "A+++" and ".ooo1"

14. **Mike**
January 23, 2024

A large chunk of the four hours was a really good overview of whats known and how, and what the problems are. The rest was an argument for why there is no crisis in HEP (which I found unconvincing). So worth listening to if you are a laymen …

15. **Krzysztof**
January 23, 2024

Peter,
1. it is TDR, not just CDR -> CEPC design is more advanced than that of FCC
2. in my opinion, politically/financially, the probability of CEPC approval >> P(FCC or ILC)
so, it is a big news

16. **John Baez**
January 23, 2024

There's definitely no problem with particle physics: that's why more and more particle physicists are assuring us there's no problem. 🙂

17. **Peter Shor**
January 24, 2024

Alessandro Strumia says

> Collider institutions seem to fear that, by openly discussing the problem, the system might collapse and the huge funds got by nuclear physics after 1945 might dry up.

It seems to me that if anybody is expecting to get funds for building a larger collider after the LHC runs end, assuming the LHC hasn't produced any hint of new physics at higher energies by then, they are completely delusional. (But then, a lot of high-energy physicists seem to be delusional.)

18. **Adam Treat**
January 24, 2024

The ones who are saying no problem are all tenured and very secure in their old positions and have no reason to acknowledge any problem. They are leaving the next generation of theorists in a terrible state, but what do they care? They've "made it" and admitting there is a problem admits their own failure. So we go…

19. **Diogenes**
January 24, 2024

There may or may not be anything sociologically "wrong" with particle physics but it is clearly destined to be less relevant and less important over time as the unknown portions become unknowable through experiment and have even less contact with human experience. That's what is really going on.

20. **Peter Woit**
January 25, 2024

Diogenes,

During the glory days of the subject 50s/60s/70s it had little contact with human experience, that's not the problem. The subject (unavoidably) moving into a mode where it no longer had experiment to keep it honest, and not replacing that with something else has always been to me the core of the problem. In my book I tried to express this with a line from Bob Dylan

"But to live outside the law, you must be honest"

What I find upsetting about recent claims that all is well is that they're taking lack of honesty about the situation to a new level.

21. **WTW**
January 25, 2024

Peter,
I'm reminded of something that I first saw online, written at the top of the first page of a geology notebook:

The X-Files Principle: If you truly believe in a hypothesis, all evidence will eventually prove it.

I.e., the human mind's ability for self-deception — to not just believe one's own BS, but to be able to justify it to oneself and to others through (mis)interpretation of "evidence" — is inexhaustible. And while it is admittedly a circular argument, the evidence for this seems to be everywhere around us ;-).

But in the specific case of theoretical HEP:
When there is little objective "evidence" on which to base one's "beliefs" in hypothetical constructs (aka "models"), and the mechanisms that do seem to bear fruit are themselves layered in poorly understood, almost semi-mystical "effective" theory, then a major determinant of what one believes is based on the characteristics of those who also hold the same beliefs.

Since theoretical physicists, as a group, tend to make it a point to identify "the smartest person in the room", that becomes a dominant factor, and one that exacerbates the above Principle. As everyone in a Physics department or research group knows, physicists are among the cleverest people on Earth. Unfortunately, "cleverness" is often mistaken for "insight" and even "wisdom" in one's (and others') search for adding to our knowledge. And ongoing confirmation bias among those groups of extremely cleverly intelligent people just exacerbates the above problem. Rather than the common (mis)conception (and self-justification) that our intelligence protects us from such bias, it can tend to reinforce and rationalize it. It becomes the equivalent of "doing God's work" to be instrumental in aiding and moving such a group's work forward, discovering ever more self-generated "evidence" of its worth as that work progresses, even when that effort is in the furtherance of a miasma.

So that leads to several questions in relation to the situation in theoretical physics. E.g.,
a) How much of this self-justification is due to self deception, with these people actually believing what they are saying, or
b) How much is actual lying, in order to maintain status, gain funding, etc. etc. etc.

I suspect it is a mixture of both, with a) being used to justify b). And I suspect that we all need to be reminded to heed the above geologist's warning, even including those who believe that there are severe, perhaps terminal problems with the ways that theoretical HEP has been worked on and developed.

22. **Peter Woit**
January 26, 2024

WTW,
This question has always bothered me. To put it simply, does Michio Kaku believe the nonsense he writes or is he lying? From looking at him and many other similar cases in other different areas, my guess is that he just sees truth as being irrelevant to what he his doing. He has an agenda, a job to do, and what's relevant is furthering the agenda, doing the job. You don't ask a criminal defense attorney "what's the truth of the matter about the criminal act your client may have committed?", that's not something he or she thinks about.

It is depressing though to see events like these recent ones where the physicists involved have clearly decided to join Kaku, deciding that defending their own interests or the perceived interests of their tribe is what their job is.

23. **George**
January 29, 2024

As Feynman said: "The first principle is that you must not fool yourself and you are the easiest person to fool." Unfortunately, theoretical physicists are among the worst offenders, not only those that do string theory, but others that do any kind of theory with little regard to experimental data (for example, most of statistical physicists).

Another issue is the sunk cost fallacy: string theorists have invested so much time in this theory, and bragged about solving the deepest problems in physics for decades, that backing down these claims must feel very frightening. That is why they all stick together, pat each other on the shoulders (grant and peer-review wise) and generally avoid any balanced discussion.

24. **Theorist1234**
January 30, 2024

So let us say that we are String theory researchers with permanent jobs and many years in the field.

What should we do?

Try to come out with ideas within Swampland, Black Holes, AdS/CFT or Machine learning directions?

Invent new directions?

Quit String theory and pursue other dirctions in Particle Physics or Cosmology?

Quit Particle Physics and go into other directions of Physics?

Quit Physics and move to Economics or Finance?

Somebody commented above "string theorists have invested so much time in this theory, and bragged about solving the deepest problems in physics for decades, that backing down these claims must feel very frightening"

One can back down the claim, but what difference does it make?

25. **Peter Woit**
January 30, 2024

Theorist1234,
If you have a permanent job in theoretical physics, you're being paid to do the best possible research/teaching in that field. The one thing you shouldn't be doing is continuing research activities in a failed research area. If there are other research directions in physics you find interesting and worthwhile, you should do those. If there aren't, you probably should quit the job and do something else with your life.

26. **Theorist1234**
January 31, 2024

In my opinion, Particle Physics overall is momentarily as failed area as String theory.
With no clear experiments in the near future, it will be futile years. Cosmology data might save the day or might not.

Meanwhile, we can try to cover some bits here and some bits there, hoping for the best.

Or go into Finance, Machine Learning or Quantum Computing.

27. **Bernard l'Amateur**
January 31, 2024

Peter,
I think it is unfair to ask these people to do something else. These are human beings after all. They have families and mortgages to pay, truth seeking comes second to these. They are working at highly competitive institutions. Asking them to do something else is like asking a mechanic to close their garage and go do a carpenter's job where competition is already very high too. Doing anything else means for them closing down their business. They just can't work on anything else, for they would have done it by now if they could. They cannot even quit their prestigious institutions to work in other low-profit institutions to be able to sincerely pursue their dream of unraveling the secretes of nature. The blame should entirely be put on the funding agencies that use public money and the millionaires who cannot use their brains and stop believing the BS these people try to sell them to keep their business open. This is simply the reality of the human society through history. People without brains have the money, people with brains have the talent to dupe the former to get their money. Real truth seekers don't ask for millions of funding. Just recall how the great physicists of history turned down offers from prestigious institutions just because they know that they wouldn't be able to properly pursue their dream at those institutions. But times have unfortunately changed, and people have to adapt to survive.

28. **Peter Woit**
January 31, 2024

Bernard l'Amateur,
The problem isn't funding agencies/rich people. They listen to what the most prominent and respected experts in the field tell them, don't independently make their own decisions about what is most promising to fund.

I think this is exactly why such experts are now going to the public and making ridiculous claims about how everything is A+++ and there is 0.0001% crisis. That things are not going well has become very obvious, not just to the public but also to their colleagues in neighboring fields. If this negative perception goes further, funding will be seriously threatened.

While there are some theorists for whom doing something else would be a problem, this is rarely the case with leaders in this field. The senior faculty at the IAS, Princeton, Harvard, etc. etc. have tenure and are well paid. Their children are not going to starve if they lose grant funding because they admit things have not worked out and they need to do something else.

29. **SRP**
February 2, 2024

I still find it odd that the experimental particle physicists who want to probe the energy frontier haven't united in their various committees around serious research and development, at realistic budgets, of advanced accelerator concepts that would obviate the need for giant accelerators at unrealistic budgets.

30. **Peter Woit**
February 2, 2024

SRP,
It's not that people in hep-ex wouldn't love for there to be better accelerator technology that would solve their problem and haven't been willing to support research in that area. The problem is basic physical limits:
1. For proton rings, people have worked very hard at getting higher magnetic fields, with only marginal improvements, not the order of magnitude you would need to make a difference.
2. For electron-positron rings, the synchrotron radiation losses go as E^4, no matter what you do.

3. For linear accelerators, getting big increases in energy gradient in a stable high luminosity beam seems to be very difficult.
4. Best prospect these days seems to be a muon ring, lots of people very interested, but short muon lifetime makes this very difficult.

31. **SRP**
    February 29, 2024

    I was thinking about advanced concepts that would slash the construction and operating costs, e.g. wakefields.

This Week’s Hype

February 1, 2024
Categories: This Week's Hype

For the last thirty years or so, one tactic of those who refuse to admit the failure of string theory has been to go to the press with bogus claims of “we finally have found a way to get testable predictions from string theory!”. I’ve written about dozens and dozens of these over the years (see here). In recent years the number of these has tapered off considerably, as it likely has become harder and harder to find anyone who will take this seriously, given the track record of such claims.

Today though, Quanta magazine has a new example, with an article that informs us

> An idea derived from string theory suggests that dark matter is hiding in a (relatively) large extra dimension. The theory makes testable predictions that physicists are investigating now.

This is about a proposal for a micron-scale large extra dimension, with no significant connection to string theory. I took a look at the “predictions” (see here) long enough to assure myself it’s more of the same, better to not spend more of one’s time on it. One positive thing to say about the article is that the writer did go ask string theorist experts about this, and while these experts tried to be polite, they clearly weren’t enthusiastic:

> While physicists find the dark dimension proposal intriguing, some are skeptical that it will work out. “Searching for extra dimensions through more precise experiments is a very interesting thing to do,” said Juan Maldacena, a physicist at the Institute for Advanced Study, “though I think that the probability of finding them is low.”
>
> Joseph Conlon, a physicist at Oxford, shares that skepticism: “There are many ideas that would be important if true, but are probably not. This is one of them. The conjectures it is based on are somewhat ambitious, and I think the current evidence for them is rather weak.”

Better though would have been to ask Sabine Hossenfelder what she thinks about this kind of thing (or not write about them at all)…

Update: Vafa has a new paper explaining the “prediction” of the extra dimension from Swampland conjectures. According to him

> The most direct way to test the dark dimension scenario is to check Newton’s gravitational inverse square law (ISL) at micron scale. Due to O(1) number ambiguities one can only predict this to appear at length scales 1 – 10 microns. Experiments checking this length scale would need to improve the current range bounds by a factor of 10.

What if such experiments can be done but don’t see anything, even down to 1/10,000th of a micron? No problem at all, that would be a new discovery that you need to change one of the many Swampland conjectures:

> We can only wait for the experimental verdict. Either way, we will learn exciting new physics!

Update: Sabine Hossenfelder explainer here.

Comments

1. **Low Math, Meekly Interacting**
   February 1, 2024

   I rather thought the whole idea of large extra dimensions, especially if there’s just one, had already been so severely constrained by collider and astrophysical observations that this sort of thing had fallen out of vogue. That someone found a way to evade such constraints in another model doesn’t seem terribly surprising, but that anyone considered it newsworthy certainly does. Quanta never needed clickbait, occasional flagrant promulgation of such notwithstanding. Just…why?

2. **Peter Woit**
   February 1, 2024

   LMMI,

   The Simons Foundation each year makes one or two “Targeted Grants in MPS”
   https://www.simonsfoundation.org/grant/targeted-grants-in-mps/
   which are
   “intended to support high-risk theoretical mathematics, physics and computer science projects of exceptional promise and scientific importance on a case-by-case basis.”
   One of the two such grants for 2023 was to Vafa for exactly this topic
   https://www.simonsfoundation.org/funded-project/on-the-cosmological-and-particle-physics-implications-of-the-string-swampland/
   Quanta insists that it is editorially independent of the Simons Foundation, but it is making its editorial judgements through a very similar lens to that used by the foundation to make grants, so not too surprising they end up identifying the same “projects of exceptional promise” to write about and to give money to.

3. **Low Math, Meekly Interacting**
   February 1, 2024

   Ah.

   I so loved Quanta when it first appeared I keep going back like an abuse addict, but enough’s enough, I think.

4. **Peter Woit**
   February 1, 2024

   LMMI,
   I do have some sympathy for the problematic situation the Quanta editors are in with respect to this kind of theoretical physics. Their mission is to write a sizable number of articles about important new advances in this field, but there really aren’t any, so what do they do? There are lots of people out there making dramatic-sounding claims about extra dimensions/dark matter, etc, all of which seem to be highly implausible. You’ve got to write something, so why not pick out the ones who are at Harvard and write about what they are doing? On the Foundation side, looking for what hep theory to fund, they’ve got basically the same problem, and deal with it in a similar way.

5. **David Roberts**

February 1, 2024

LMMI

The mathematics articles on Quanta are generally of very good quality, even if they have a very strong house style that they all seem to stick to, and which I now find less charming than I originally did. I'm not in the target market, though, and usually hunt for the link to the actual research paper and go read that, too.

6. **Eric Weinstein**
February 1, 2024

That is an interesting take Peter. I am not sure I see it as you do however. You write: "Their mission is to write a sizable number of articles about important new advances in this field" but this is not fully accurate to my way of thinking. I would say they are charged with writing a sizable number of articles about new developments that IN NO WAY repudiate, question or examine what is actually holding the field back.

What if that specific charge IS the problem? Why are we expected to protect the people who have been taking a gratuitious and unprecedented collection of 'premature' victory laps for the last 40 years as of 2024? That isn't a normal part of science.

As I see it the failed community has set themselves up as the player-referees of progress in the game of fundamental physics. Everything that repudiates their belief structure and bizarre way of life is excluded. Hence the one team that are allowed to be both players and referees always wins.

Imagine that a Vatican science magazine were to be charged with writing about all the exciting developments in biology that in no way called the account of man's creation by G-d detailed in Genesis into question? You would not get any report on actual biology.

It's not that there is nothing interesting going on. It is that there is nothing interesting compatible with the failed community going on. Hence the illusion. I may think your Twistor theory is wrong, but it is still interesting to me and I don't want to laugh at it or make it go away. I just want to find out if you are right and hopefully outcompete you if you aren't. Yet you are not compatible with this player-referee community. Ergo…no glossy articles. Sic transit, gloria mundi. Alas.

7. **Alessandro Strumia**
February 2, 2024

Technical question to string theorists: do I correctly understand that the dark dimension conjecture does not apply if the tiny cosmological constant arises as an unnatural cancellation (motivated by anthropic selection)?

8. **Yasin Şale**
February 2, 2024

Whenever I read or hear something about extra-dimensions, I recall Penrose's bland and cool reaction I have recently met in one of his interviews on YT: "But unfortunately our universe is three dimensional" 🙂

9. **Peter Shor**
February 2, 2024

Quanta Magazine does occasionally print articles about non-orthodox theories that compete with string theory; for example, see their article from last year on Jonathan Oppenheim's theory of non-quantized gravity.

They clearly should be writing more of these, and fewer string theory articles, but it's hard to fight the Zeitgeist.

10. **Sabine**
February 2, 2024

As I have said way too many times, the biggest problem with theoretical physicists is that they don't learn from their mistakes. (And neither, it seems, do science writers…)

11. **Peter Woit**
February 2, 2024

Eric Weinstein,
The Simons magazine/foundation combo is interesting to look at because they cover a wide range of science/math. For most areas while I'm sure there's valid criticism, on the whole they seem to be doing arguably a better job of choosing what to fund and what to cover than other organizations. In fundamental theory though, we're not the only ones to see a big problem, so why aren't they doing something?

The organization is fairly transparent: you can see that in this area there's only one person on the Quanta advisory board (David Gross). On the Foundation scientific advisory board there's really no hep-th (Shankar doing condensed matter, two others mainly in cosmology/astrophysics), but the head of math-physics is Gabadadze, one of whose specialties is extra-dimensional/modified gravity. That the organization is funding and covering this research promoting string theory based extra dimensional gravity models is unsurprising.

The problem though is that in this area they're just doing exactly the same thing they do in other areas. How do you get them to start treating it differently and more skeptically? It's in their DNA to listen to and support elite US institutions, and as long as the people there are saying all is well, we're doing A+++, they're not going to go against this. Unfortunately it seems to me that more and more people like Vafa and Strominger have decided it is in their interest to not acknowledge any problem.

Vafa I think is also a very good example of an important aspect of the problem. He has done wonderful work in areas like topological string theory, involving great mathematics and physics. But the criticism of string theory as "it's mathematics, not physics, string theorists need to stick to physics and come up with testable predictions" has moved theoretical physics away from any interest in bringing new mathematical ideas into physics. The description in the Simons Foundation abstract is a good example, it has him working on deviations from the inverse-square law, which looks much closer to the mathematics of the 17th century than anything new.

12. **maxstroke**
February 2, 2024

How does one get onto the advisory board? Of course, folks need to devote their time and energy as they see fit, but perhaps having Sabine or Peter Woit or other not-quite-so-dogmatic folks on the advisory committee would help.

I, for one, welcome our new Twistor-exploring overlords.

13. **Peter Woit**

February 2, 2024

Sabine,
I think a bigger problem is that they don't admit they've made a mistake. Hard to learn from your mistakes if you think you don't make any, that you're A+++.

14. **Peter Woit**
February 2, 2024

maxstroke,
That's certainly not going to happen.
In any case I don't think the advisory board is that important. I doubt that David Gross is getting consulted all that much. The Quanta writers and editors surely are doing what journalists do, calling up prominent experts in the field and getting their opinions. The problem is that most of the time in other fields they're getting sensible advice, but when they start calling up influential people in hep-th, they're getting told that wormholes are being created in Google labs and that progress is being made on getting testable predictions out of string theory, not that this is nonsense.

15. **Peter Woit**
February 2, 2024

As I try to figure out why anyone at Quanta thought it was a good idea to put out a new story with a "string theory finally makes testable predictions" sub-headline, one thought that occurs to me is that they know it's a sure way to get me to write a blog entry that will drive traffic to their site. I'm not coming up with any other plausible reason they would do this…

16. **Jerome**
February 2, 2024

I'll never visit Quanta again, as long as Wolchover is writing for them. The "real and traversable wormholes" fiasco of 2022 is perhaps one of the most egregious and unforgivable instances of blatant, intentional, malicious fraud in science journalism of my entire life, and I'm done with them until the day they pen a formal apology for their participation in the whole debacle. My jaw is still on the floor from the falsehoods they spouted and the degree to which they doubled down on their misinformation.

Their reputation is permanently tarnished.

17. **SRP**
February 2, 2024

New term: Woitbait.

18. **Eric Weinstein**
February 2, 2024

Sabine: This is not "Theorists not learning from their mistakes" as we all know. This is the ethics of a particular sub-crowd who installed themselves as the arbiters of all theoretical physics and who refuses to recuse themselves due to conflicts of interest. Imagine if Georgi and Glashow were convinced that protons were decaying left and right due to SU(5) but we weren't looking hard enough. Or if SUSY theorists were claiming that selectrons and gluinos were everywhere at the LHC but only virtuous people could see them. Or if Norman Mailer's publisher claimed that all of Philp Roth's books didn't exist. Or if the The Rolling Stones claimed that all good music that people liked was theirs and took the royalties. Or if a judge finding himself innocent of bribery. That is what we are talking about: the "String Mass Delusion" at the level of Qanon.

That is the level of nonsense with which we are now dealing. Like it or not, String Theory has gone from Idea to Program to Failure to Scam to Madness. And it is not about the equations or the spaces or the operators. I will say it again: you all are too focused on the problem of strings being with the science. The pseudoscience of String theory is its anti-scientific sociology. The equations and Calabi-Yau manifolds are not the problem. It's the lying. It's the bullying. It's the sabotage. It's the money. It's the skulduggery. It's the political economy. It's the failure to recuse yourself or find any critics with whom you will appear and test your ideas.

I don't care whether these folks are ethical in their private lives. That is nobody's business. But in the seminar room, they are not playing by the rules of science. And we need to stop tip toeing around this. As Edward Witten says "There are no other routes. There are only words." I mean, that is breathtaking in terms of self-indictment. No explanation needed for anyone paying attention.

I still can't even believe that is a real quote from a geometer and mind I have *mostly* revered since 1983; to my mind that is all you need to know after 40 years of bullying those who dare openly discuss the most profound and clear cut public failure in science any of us have ever seen. It doesn't get easier than this. But we are afraid to say it becaused it involves people we all know. And often like…at least when they are not lying to themselves, each other, us and the public. Pity that.

19. **Eric Weinstein**
February 2, 2024

Peter: I mostly agree with your comment above to me. I generally like Quanta as well and have a high opinion of many things that the Simons foundation does. Having talked last with Jim about 2 years ago in California, I can say that even he wasn't entirely sure how the Chern-Simons theory was supposed to be related to real physics. So the reporters rely on the experts. I get that.

But the reporters then have to start reporting and IVESTIGATING claims that there are serious questions about Quantum Gravity / String Theory / M-theory having become a funding/earning/bullying cult just the way they investigated Claudine Gay or Francis Collins and gain of function. Or Richard Nixon. Or Trump.

Call it the "StringAnon Mass Delusion". It's basically a variation on the idea of a citation cartel in academic publishing. Or the "Replication crisis" in soft sciences. Or asking why macro economists failed to see that 'The Great Moderation" was an investment banking scam threatening the world financial system. Or the Sokal Hoax. Or the Bogdanov affair. Or the Boskin Commissioners' attack on Social Security. Or the Gain of Function disaster.

All of those are now discussable. Why is StringAnon/M-Anon any different as a mass delusion? I'm not happy about it. I would prefer that they take the Dan Friedan route and admit the error. But given how they bully everyone else, it is as if Susskind, Strominger, Kaku, etc. have diplomatic immunity from common sense, scrutiny, journalism or science as a discipline which their bullying targets do not.

Quanta: if you are reading this, consider this an invitation. Reach out to anyone you trust.

20. **Peter Woit**
February 2, 2024

Jerome,
My understanding is that Wolchover has been on leave from Quanta for the last year or so to write a book, so you can't blame her for this latest.

On the wormhole fiasco, if you were a journalist and
1. the Spiropulu, Lykken, Jafferis claims of existence of wormholes in the lab were vouched for by Nature
2. Preskill, Susskind, Gross all told you this was real
3. the IAS faculty seemed to be on board
wouldn't it be journalistic malpractice not to tell the world about the wormholes? Put differently I think you're blaming the victim of a scientific fraud instead of the perpetrators.

21. **Sabine**
February 3, 2024

@Peter Woit Yes, indeed

22. **Sabine**
February 3, 2024

@Eric Weinstein

You seem to put the blame on certain individuals. I think it's a systematic problem. By this I mean a problem of how the scientific system operates. If science was "self-correcting" as many scientists naively believe, this should not happen — at least not to this extent. Why does it happen nevertheless? Because there's no "invisible hand" that corrects science any more than there is an "invisible hand" correcting trade. A system can only optimize what it's been configured to optimize.

So long as researchers get rewarded for producing papers that are deemed good if many of their colleagues like them, we will continue seeing these bubbles. It's a positive feedback: the more people work on a topic, the more they tell each other it's good research, the more they feel they're on the right track etc. People go where money goes and in academia, money goes where people go. Not hard to see how that's a positive feedback, is it?

The tragedy is that this is hardly a new insight, and yet nothing has been done about it. It would be simple enough if journals would just stop publishing such stuff because it isn't real science. Of course I don't think that's actually going to happen because (a) the people on the editorial board are the same people who work on this stuff and (b) the publishers make money from publishing it.

One would think that funding agencies would at some point realize it's a waste of money, but that doesn't happen either, because they outsource their reviews to the same people who work on the stuff. This system is basically incestual and I find it somewhat surprising that it doesn't work even worse than it does.

23. **Chris Rijk**
February 3, 2024

Speaking as an outsider who is interested in science, it feels to me that the above posts are describing the same underlying problem, just from different perspectives (eg top-down vs bottom-up).

What would I suggest doing about it? After thinking about it for a while I came up with this idea: Try to get 10 or more people who can reasonably be described as experts in the field to collectively write a book, with one chapter per person, where they describe their personal experiences and frustrations with this problem and make it clear that they believe that scientific progress is being held back. In effect a public petition to call attention to a problem that is too big for any one person to solve.

24. **Peter Woit**
February 3, 2024

Chris Rijk,
There already exists much more than what you are suggesting, probably ten whole books, not just chapters, of the sort you describe. Off the top of my head, besides my own, there's Smolin's, Hossenfelder's and Penrose's (Fashion, Faith and Fantasy). The reaction of the string theory community to these has been to refuse to engage with the arguments of the books and denounce the authors as ignorant of what they are writing about. The books have had an effect on the public, journalists and on non-string theorist scientists, with the general perception changing over the last 25 years from string theory as successful and promising, to string theory as a failure. 20 years ago when I was describing string theory as a failure, the reaction I'd get was that I was saying something very unusual and provocative, now this more often is dismissed as boring and well-known, just beating a long-dead horse.

Changing the behavior of journals doesn't seem a promising route at all. What would have an effect would be cutting off funding to and hiring in these fields. I think it's exactly because people see an increasing danger of this happening that you're seeing efforts like the World Science Festival panel and this Quanta article to fight back against widespread negative perceptions.

25. **S**
February 3, 2024

Sabine,

You write,

"This system is basically incestual and I find it somewhat surprising that it doesn't work even worse than it does."

It seems to me that that last part is key. It *does* work better than we should expect, and I think it's that phenomenon that causes people to become enamored of science as "self-correcting," etc. (and then take it farther than the facts warrant). In a very similar way, there *is* a kind of invisible-hand phenomenon with markets, which accomplish an incredible amount; without which fact, it would be hard to explain why some people become so impressed by them that they ascribe unrealistic powers of insight and virtue to them.

It seems to me that it is important, but incredibly hard, to prize and preserve that which does work while preserving what does not. For example, one can imagine "fixing" the journal or grant system by having untrained bureaucrats make decisions without consulting scientists. It's hard for me to imagine that this wouldn't be far worse, just as the twentieth century taught us that there are cures to market problems that are orders of magnitude worse than the disease.

I'm afraid I don't have any very clever solutions, but my point would be, it seems necessary to find ways to improve the system piecemeal around the edges (a la market regulation), keeping in mind its real strengths as well as the spectacular pathologies that a field like hep-th has made manifest in the last four decades.

26. **Diogenes**
February 3, 2024

There are well known conditions for the operation of competitive markets in regularly transacted goods. Here, we have much more of a luxury maket, where both the producers and consumers are a small elite. In this case, the ideas of competitive markets, available information, price discovery and so on, just collapse. One thinks of the art market – is it fair? How would one define that? As hep-th moves away from experimental contact it is very much like an art market, with rich buyers and sellers.

I will however disagree strongly with this kind of authoritarian impulse of SH:
"A system can only optimize what it's been configured to optimize." This is a very bad and popular kind of idea in academia that fits no historical evidence whatsoever. It is in fact what leads to the current kind of sub-optimal equilibrium. More physicists should take economics classes! Or think about spin magnets more! Or just reduce the number of degrees of freedom in the SM by one. How hard could that be?

27. **Sabine**
February 4, 2024

@S

The reason it partly works is that writing papers that are well-cited is in some cases indeed an indicator for relevant scientific work. The problem is that the cases when it isn't can run away and create these empty research bubbles.

I guess one could say that the hand is "invisible" in the sense that in both cases (markets/academia) it's a decentralized system. I hesitate to call it that because it's not like the rules of interaction (or call them regulations) are invisible. We know what the rules are. The issue is that a lot of actors (in either case) tend to blame "the system" as if that was a living thing rather than something that they themselves create.

I have written and talked about this for 20 years now and nothing has changed and honestly at this point I've basically given up hope the scientific community will do anything about it. They're just not interested. The only thing that could eventually make a difference is taxpayers realizing that their money is being wasted, demanding that the issue is cleared up. Alas, for this to happen the problem isn't big enough, basically, and in all fairness I guess the world has bigger problems.

I believe the major reason these research bubbles don't happen more often is that the scientists involved don't actually want to work on irrelevant nonsense because there isn't enough money to make in academia. They need to be genuinely confused. This is different to economic bubbles that people have a reason to contribute to even if they know they're bubbles because there's money to make (eg the 2008 economic crisis was an extreme example — actually many people knew it was a bubble, they just couldn't not contribute to it).

In the case at hand (Quanta article) it's rather obvious what the confusion is — I have written and talked about this several times before. There is a very large fraction of physicists (and science writers) who erroneously believe that a theory which is falsifiable is scientific. It's a basic misunderstanding of what Popper said: a theory which is not falsifiable is not scientific (or, equivalently, if it's scientific then it's falsifiable). This simple misunderstanding has funded basically thousands of researchers for decades. It's embarrassing.

28. **Anonyrat**
February 4, 2024

What can force a bunch of scientists that what they are doing is no longer science? Only retirement, death, and an unyielding skepticism of the next generation.

29. **Peter Woit**
February 4, 2024

Yes the way hep-th works may be more like an elite luxury goods market than a standard competitive market, but like such markets, those directly involved understand well how they work. In the hep-th case, central are jobs and funding. For jobs, there aren't a lot of them, you can easily follow the postdoc and tenure-track rumor mills to understand exactly who is getting these jobs and exactly what you need to work on and what kind of connections you need to get one one of these few jobs. For grant funding, in the US it's basically DOE + some NSF and Simons. Again, you can easily see who these small number of grants are going to and what you need to be doing to get one. Not hard to find out who is on the grant panels and making these decisions, or what criteria they are using to make them.

So, in principle changing this system would just require a relatively small number of people to acknowledge there's a problem and come up with a plan to do something about it. But, those who could do this are instead arguing everything is A+++.

As for the idea that the system will self-correct by retirements and skeptical younger people, note that Andy Strominger's father is a distinguished Harvard researcher in another field who only recently retired in his mid-nineties, still getting grants. My impression of younger people in hep-th is that for decades now this field has been weeding out anyone skeptical, more and more attracting the unusually credulous.

30. **entropic**
February 4, 2024

Condensed matter physicist here, who has been eagerly reading this excellent blog and the comments over the years; this is my first comment.

There is one characteristic of hep that has been bothering me since my student years and it has played a major role in my decision to pursue a different field of Physics, namely the existence of a person-cult atmosphere in it. There have always been in hep, even before the times of strings, semi-gods revered by their disciples, who recited their sayings as though they were speaking the words of irrefutable truth. I always found it bizarre to witness both my fellow students and mature researchers refer to whatever those (undoubtedly brilliant) colleagues said in religious awe [*], and following uncritically the research paths laid down by them. This attitude has reached monstrous proportions with strings. There are no doubt leading personalities in all fields (of Physics) but the situation in hep has always seemed extreme to me. This aspect of it, combined with the disturbing arrogance of many hep practitioners to look at other physicists as lesser beings, kept me away from the field.

I feel genuinely very sorry for the current state of affairs of hep and I am not experiencing any Schadenfreude about it. It makes me sad as a fellow physicist, also because I know that this hype-beast has already extended its tentacles elsewhere and it is hurting all parts of Physics, some more and some less. I believe, however, that in trying to analyze all the reasons for these negative developments in hep, one should also consider the cult-like sociology of the field as an additional factor: hep has been depending on the holy words of a few superstars for way too long. Maybe this is a lesson to be learned for other fields (and for hep in its effort to recover from its present state.)

[*] A particularly alarming sign has been whenever someone who had never met the leading hep-person Robert Smith [fictitious name] in his life, would start a sentence saying: "As Bob recently said …"

31. **jack morava**
February 4, 2024

@ Sabine :
… we will continue seeing these BUBBLES…

Speaking of condensed matter, I think this is an accurate diagnosis, and that it may be useful to think in these terms, with funding agencies as inefficient steampunk engines powered by political heat. Bubbles are quite interesting [cf K Uhlenbeck] and deserve to be better understood.

Arithmetic, Geometry and QFT news

February 5, 2024
Categories: Langlands

This week at Harvard's CMSA there's a program on Arithmetic Quantum Field Theory that is starting up and will continue through March. There's a series of introductory talks going on this week, by Minhyong Kim, Brian Williams, and David Ben-Zvi. I believe video and/or notes of the talks will be made available.

At the IHES and the Max Planck Institute, the Clausen-Scholze joint course on analytic stacks has just ended. For an article (in German) about them and the topic of the course, see here. What they're working on provides some new very foundational ideas about spaces and geometry, in both the arithmetic and conventional real or complex geometry contexts. Many of the course lectures are pretty technical, but I recommend watching the last lecture, where Scholze explains what they hope can be done with these new foundations.

Of the applications, the one that interests me most is the one that was a motivation for Scholze to develop these ideas, the question of how to extend his work with Fargues on local Langlands as geometric Langlands to the case of real Lie groups. He'll be giving a series of talks about this at the IAS next month.

Something to look forward to in the future is seeing the new Clausen-Scholze ideas about geometry and arithmetic showing up in the sort of relations between QFT, arithmetic and geometry being discussed at the CMSA.

Comments

1. **biostat person**
   February 5, 2024

   I believe the videos should be available on https://www.youtube.com/@harvardcmsa7486/videos, as they have posted various past conference videos on this site. Interested in it, but not having enough time to attend in person. Hopefully, they will post recorded videos on the same site.

2. **anon**
   February 6, 2024

   I've seen samizdat versions of notes from Scholze's Munster talk. Has he now proved the conjectures he made?

   I'd always idly wondered about the category of globalisations of a given Harish-Chandra module – we know it has canonical `maximum' and `minimum' elements, but nothing really about the structure of all such. Do these now form a well defined moduli space , and if so, what does its topology (or Hodge theory!) tell us about the representation, and how can we read these invariants from the HC-module, or its BB description on the flag variety?

   Its amusing how this counts as an `easy' application of this Clausen-Scholze technology. We await the subtle applications…

   Presumably it is clear how this CS analytic stuff describes the maximal and minimal locally analytic vectors in a Matsuki dual picture to usual BB, as probably done by Kashiwara directly. But how does it control all the `intermediate' extensions??? Very intriguing! Can one hope for _interesting_ HC modules, with very special moduli spaces of globalisations, parameterised by extremal points in some Langlands dual picture?

   Anyhow, I should watch Scholze talk#24 (and talks#1-talks#23) before commenting further.

   Did Clausen/Scholze explain (as an obvious example) representations of a Heisenberg group, and how the Stone-von Neumann theorem fits cleanly into their geometry and rep theory and D-modules, not just `sort of analogously'?

   Also I hope to learn from these talks: what more does the Clausen-Scholze stuff applied to the usual Hodge package produce in terms of constraints, when compared to the o-minimal analsysis of Klingler, Tsimerman, et al? Presumably quite a bit more, as one can now handle growth conditions….?

3. **Peter Woit**
   February 6, 2024

   anon,
   If you have access to notes of the Munster talk, you know a lot more about this than I do. From Scholze's comments in the last talk of the lecture series and from second hand reports, all that's clear is that Harish-Chandra modules are not the right thing here.

   My understanding is that this is about real local Langlands which is just for reductive groups. If it gives a proof of Stone-von Neumann that would be fun.

4. **Peter Scholze**
   February 6, 2024

   @anon

   Thanks for your comment/question! Things resolve themselves differently, and in some sense in a more boring way.

   Background: We define a category that is "a category of G-representations" for a real Lie group G, but thinking of representations in terms of sheaves on the classifying stack $[*/G]$. Here $*$ is really the analytic spectrum of the gaseous real numbers, and G is incarnated as an analytic space via regarding it as a real-analytic manifold (and using the corresponding functor to analytic spaces). Then we simply take the ("quasicoherent") derived category $D([*/G])$ of $[*/G]$.

   Now, somewhat surprisingly, every(?) Harish-Chandra module has a unique(?) corresponding object in $D([*/G])$. (Question marks because I did not yet carefully check in general.) The obvious question is, how can we single out a unique G-representation associated to a Harish-Chandra module? Part of the answer is that a little bit of magic seems to happen; another part of the answer is that there are actually two ways to turn an object of $D([*/G])$ into a representation of G: One can use $*$- or $!$-pullback along $*\to [*/G]$. One of them ($*$-pullback) gives the minimal globalization, the other ($!$-pullback) gives the maximal globalization.

   Here's a related story. There's this ring of infinite-order differential operators $\hat{D}$ on a complex manifold. Usually it's subtle to get a good theory of $\hat{D}$-modules working (for reasons of functional analysis). In our setup it's straightforward. One can then wonder whether this resulting category still has a Riemann-Hilbert correspondence to a category of sheaves. It turns out that $\hat{D}$-modules are just equivalent to sheaves! (No constructibility conditions or anything — all sheaves.) This is related to some classical stuff about hyperfunctions. [Disclaimer: I'm far behind on learning the classical theory…]

This story of $\hat{D}$-modules actually plays very well with the above story of G-representations, and they are easy to combine in the Beilinson-Bernstein localization business, leading indeed to the Matsuki dual picture. Many of the ideas here are really due to Rodriguez Camargo, who has developed this in the p-adic context. (This leads to the slightly strange situation that many things are now written over the p-adics but not over the reals… not because the reals are harder for these things, but mainly because so far it's mostly p-adic people using this theory.)

5. **anon**
February 6, 2024

Dear Peter S,

Thank you for responding to my question/comment.

I'm looking forward to reading the definition of your category of G(R)-modules — it sounds wonderful. (I'm *very* curious how it encodes the basic finiteness theorems and theorems about density of analytic vectors that drive the usual story of this, so that such modules have underlying HC modules, what is the notion of equivalence of modules, and the way that bad Banach space examples get excluded).

Also: Do you have a range of extension functors interpreting between * and ! pushforward, analogous to Beilinson's extension functors? (that have to do with properties – growth ? – of the function f^s?)

But I'm reeling from what you write about infinite order differential operators — that solves a problem that plagued the Kyoto school for many decades (with their various microlocal differential operator rings E, variants all defined with different growth conditions). What an extraordinary RH theorem you have – just the right analytic notion to capture *all* sheaves. That is extraordinary. Where can I read it!?!!! (And are there notes from your course, or should one just watch it all?) I'm happily imagining all of the questions one can now answer with these tools… wow.

(Trivia: I didnt quite understand from the above if the classification of your G(R)-modules is `automatic' from the definition – is it straightforward from the definition that they are global sections of sheaves on your gaseous classifying space, and those with trivial central character give good sheaves on your gaseous quotient flag variety/G(R)? What is your analogue of the BB-theorem that the flag variety is D-affine, or its mixed version, the Grauert Riemanschneider vanishing theorem. Or are you assuming a BB classification of HC-modules? Given a HC-module with generalised central character, don't we already know how to get the globalisations from the Matsuki dual sheaf to the BB-localisation, I think more or less functorially? If this is how it works, it feels a bit like cheating, or at least criminally underutilising your rather major breakthroughs.)

Anyhow, thank you. Sorry for burbling – I'm still trying to process your response, even without the supporting definitions.

6. **anon**
February 6, 2024

Apologies, I see I read too fast – * and ! are for pullbacks to a point, not pushforward of an open subvariety, hence are different notions of `underlying vector (Banach?) space' for your equivariant sheaf? I retract my question about analogues of Beilinson's functors, which is nonsense in this context. Indeed 'things resolve themselves differently'.

Please ignore any further nonsense in my previous question — I won't understand what is actually going on until I work through the definitions and compute what happens for SL_2

Peter W — thank you for allowing these questions on your blog. And for your links, which are very thought provoking

7. **Peter Woit**
February 6, 2024

anon,
"I won't understand what is actually going on until I work through the definitions and compute what happens for SL_2"
You're not the only one…

8. **Peter Scholze**
February 7, 2024

@anon

Thanks for your excitement ;-). Just some brief answers to a few points:

Our representations usually are on some (dual) nuclear Frechet space. Banach (or even Hilbert) spaces do not play a prominent role in our work. More broadly, norms/metrics in the classical sense are less relevant for us. (They are however key to many results — like the Hodge decomposition — but we hope that we can get such results using a very different incarnation of metric information, related to the "extended Berkovich line".)

About the Riemann-Hilbert theorem: We hoped that we would find time to discuss it in the course, but in the end there was no time. But it's almost a tautology for us! Namely, we realize $\hat{D}$-modules again as quasicoherent sheaves on some stack, the "analytic de Rham stack" X_{dR}. This is the quotient of the complex manifold X by the "overconvergent neighborhood of the diagonal". In the p-adic context, see the recent work of Rodriguez Camargo (on arXiv). On the other hand, usual (in arithmetic geometry-speak, "Betti") sheaves can be realized as quasicoherent sheaves on some other stack, X_{Betti}. This is a priori built in a slightly crazy way, covering X by totally disconnected sets; it was briefly discussed in the course, in my lecture giving the definition of analytic stacks.

Now our Riemann-Hilbert statement follows from a more primitive geometric statement, namely that the two stacks are isomorphic, $X_{\mathrm{dR}}\cong X_{\mathrm{Betti}}$! And once one understands what's going on, this is really a triviality. Namely, it's clear that there is a surjective map $X\to X_{\mathrm{Betti}}$ and that the induced equivalence relation on X is precisely the overconvergent neighborhood of the diagonal.

(Much of the hard work in the course was to get such a flexible notion of analytic stacks where in particular these things turn out to be isomorphic even though they are presented in very different ways. The key is the $!$-topology whose discussion was probably the most technical part of the course.)

9. **Peter Scholze**
February 7, 2024

PS: In the Liquid Tensor Experiment, I said that the theorem on the existence of the liquid analytic ring structure on the reals may be my most important theorem. Everybody seemed very puzzled about this judgment. But it is the key to get machinery like the above.

(Now we also have the gaseous analytic ring structure on the reals, which is easier to construct and also works for most applications, in particular the stuff above. But gaseous vector spaces are a much more radical departure from the usual setting of complete locally convex vector spaces, so for some purposes it is still very useful to have the liquid theory as well. I discussed one instance in the last lecture. In case someone wonders whether the techniques of our course help in the liquid case: So far no — we now have rather nice arguments for the existence of the solid and the gaseous analytic ring structure, but the existence of the liquid one is still a very difficult theorem.)

[My apologies to all physicists reading this for our abuse of physical terminology. Explanation: There are “condensed” vector spaces (=a variant of topological vector spaces), and within those various kinds of more “complete” ones. The weakest notion is “gaseous”, next comes “liquid” (at various “temperatures” = degree of convexity), and in the non-archimedean case things can even get “solid” (at “temperature 0”).]

10. **S**
February 7, 2024

I am quite bedazed by all of this, but if nomenclature requests are being entertained, I hope that in applications to number theory, we will soon have room-temperature superconductors?

Why physicists are rethinking the route to a theory of everything

February 7, 2024
Categories: Euclidean Twistor Unification

New Scientist this week has a cover story I can strongly endorse, entitled Why physicists are rethinking the route to a theory of everything. It's by journalist Michael Brooks, partly based on a long conversation we had a month or so ago. Unfortunately it's behind a paywall, but I'll provide a summary and some extracts here.

For a more technical description of the ideas I'm pursuing that are discussed in the article, see this preprint, which was intended to be a very short and concise explanation of what I think is the most important new idea here. This semester I'm mainly working on teaching and writing up notes for an advanced course for math graduate students about the Standard Model. I'll be adding to these as the semester goes on, but have just added preliminary versions of two chapters (9 and 10) about the geometry of vectors, spinors and twistors in four dimensions. These chapters give a careful explanation of the standard story, according to which spacetime vectors are a tensor product of left-handed and right-handed spinors. They don't include a discussion of the alternative I'm pursuing: spacetime vectors are a tensor product of right-handed spinors and complex conjugated right-handed spinors. I'll write more about that later, after the course is over end of April (and I've recovered with a vacation early May…).

It's well-known to theorists that the Standard Model theory is largely determined by its choice of symmetries (spacetime and internal). A goal of these notes is to emphasize that aspect of the theory, rather than the usual point of view that this is all about writing down fields and the terms of a Lagrangian. This symmetry-based point of view should make it easier to see what happens when you make the sort of change in how the symmetries work that I'm proposing. What I'm not doing is looking for a new Lagrangian. All evidence is that we have the right Lagrangian (the Standard Model Lagrangian), but there is more to understand about its structure and how its symmetries work. In particular, the different choice of relation between vectors and spinors that I am proposing is not different if you just look at Minkowski spacetime, but is quite different if you look at Euclidean spacetime.

The New Scientist article has an overall theme of new ideas about unification grounded in geometry:

> … a spate of new would-be final theories aren't grounded in physics at all, but in a wild landscape of abstract geometry…
>
> That might strike you as outlandish, but it makes sense to Peter Woit, a mathematician at Columbia University in New York. "Our best theories are already very deeply geometrical," he says.

There's some discussion of string theory and its problems, with David Berman's characterization "It can be a theory of everything, but probably it's a theory of too much." The article goes on to describe the amplituhedron program, with quotes from Jaroslav Trnka. After noting that it only describes some specific theories, there's

> Trnka thinks the amplituhedron approach might enable us to go even further. "One can speculate that whatever the correct theory of everything is, it would be naturally described in the amplituhedron language," he says.

Turning to a discussion of twistors, there's then a section describing my ideas fairly well:

> Woit is using spinors and twistors to create what he hopes are the foundations of a theory of everything. He describes space and time using vectors, which are mathematical instructions for how to move between two points in space and time – that are the product of two spinors. "The conventional thing to do has been to say that space-time vectors are products of a right-handed and a left-handed spinor," says Woit. But he claims he has now worked out how to create space-time from two copies of the right-handed spinors.
>
> The beauty of it, says Woit, is that this "right-handed space-time" leaves the left-handed spinors free to create particle physics. In quantum field theory, spinors are used to describe fermions, the particles of ordinary matter. So Woit's insights into spinor geometry might lead to laws describing the holy trinity of space, time and matter.
>
> The idea has got Woit excited. He has spent most of his career looking at other ideas, thinking they will go somewhere, and being disappointed. "But the more I looked at twistor theory, the more it didn't fall apart," he says. "Not only that, I keep discovering new ways in which it actually works."
>
> It isn't that Woit believes he necessarily has the answers. But, he says, it is good to know that, despite the long search for a theory of everything, there are still new possibilities opening up. And a better, if not perfect, theory has to be out there, he reckons, one that at least deals with sticking points like dark matter. "If you look at what we have, and its problems, you know you can do better," he says.

Other work that is described is Renate Loll and causal dynamical triangulations, as well as what Jesper Grimstrup and Johannes Aastrup call "quantum holonomy diffeomorphisms." For more details of the Aastrup/Grimstrup ideas, see this preprint as well as Grimstrup's website. I can also recommend his memoir, Shell Beach.

The article ends with:

> That said, when Woit – who has long been known as an arch cynic – is excited about the search for a theory of everything again, maybe all bets are off. Playing with twistors has changed him, he says. "I've spent most of my life saying that I don't have a convincing idea and I don't know anyone who does. But now I'm sending people emails saying: 'Oh, I have this great idea'."
>
> Woit says it with a grin, acknowledging the hubris of thinking that maybe, after so many millennia, we might finally have cracked the universe open. "Of course, it may be that there's something wrong with me," he says. "Maybe I've just gotten old and just lost my way."

Comments

1. **Eric Weinstein**
   February 7, 2024

   Reading this weirdly filled me with dread, which I found odd. Why isn't this a welcome relief?

   Perhaps it goes like this: I don't think this is at all true. The story we tell about String Theory, Particle Theory, Loop Quantum Gravity, etc is just not true.

   To recapitulate the standard story it would go something like this. "In the early 1980s physics was focused on the ultimate prize: a theory of everything called Quantum Gravity. In 1984, physicists were astonished and became understandably excited from a stunning breakthrough that they had findally found the theory of everything in a new version of String Theory. Enthusiasm ran high for the first decade that we were close to a theory of everything in string theory when, unexpectedly, there was a second Superstring revolution, perhaps even more profound than the first! String theory became m-theory, strings became branes and everything looked to be going even better than expected. The only other real competitor was Loop Quantum Gravity which did not make as much progress in a race that seemed to come down to two horses where one held a seemingly insurmountable lead over the other. Shortly thereafter the picture of AdS/CFT changed everything yet again proving that Strings were indeed connected to everything we knew from the gauge theory of real physics. Meanwhile, experimental Physics then triumphed repeatedly, with the discovery of neutrino mass, a non-zero cosmological constant, the discovery of the Higgs and even of gravity waves. But along the way this marvelous new picture came with a cost: the world was even more complicated than we had ever imagined. Patinence and perserverance would be needed to stay the course over the coming decades. But now, finally after 40 years, some are starting to question whether there aren't other avneues to be explored as well. Etc."

I tried to write the above as original prose in such a fashion that I can assure you there was almost no truth in what I wrote about theory in this time. Nevertheless it is written to be breathlessly repeeated by science journalists and program officers administering grants.

We weren't focused on quantum gravity in 1983. We were focused on different unification schemes and beyond the standard model programs. String mania wasn't understandable enthusiasm broadly experienced by the community at all. Further, the String field didn't simply progress, but instead actually invalided all of its own hyperbolic claims year after year (e.g. "There are only 5 theories it could be so it will be figured out quickly which on is right!" or "Branes are irrelevant: it's just string vibrations that explain it all!" or "Gauge theory simply can't do what strings can do as it is an entirely different paradigm!"). Loop quantum gravity wasn't the only other competitor. None of this is true! In short, the whole String theory story is more or less a lie akin to that discussed as the "Enforced Forgetting" of the history and truth seen in Central and Easter European communist nations that was made famous by Kundera

But most of all, I don't believe for a minute that "Physicists and mathematicians are now finally considering whether there are new directions!" I think we are afrraid to tell the real story of a small group of physicists who went about savagely attacking all competitors through insult, aspersion, and gaining control of the narrative of the field as everyone else looked on and said "What the Eff is going on with MK, LS, EW, MD, JH, CV, AS, DG, LM, savaging everyone who would not kneel or lie prostrate before the Golden Calf of String Theory/M-Theory? This is pyschotic. There are many people with other ideas who are being destoryed. Why wont these people openly debate other experts who disagree with them vociferously? Why wont they face the critques of fellow String Theorist who left the cult as well? How in a supposedly scientific field did they become their own policeman, judge, judge, jury and assasins?"

This is the story of a simple mass delusion like Qanon that lasted 40 years and which showed that science journalism, peer review, funding agencies and credentials do not actually work AT ALL. If they did we wouldn't be here. I mean, this is so insane I don't think it will ever make sense. It shows that we actually don't have a theory of science. If we did, this would be impossible. It could never ever have happened.

But to tell the real story about a competitor murdering band of around 10 leading individuals who somehow got near total control of the particle phyusics narrative and who used 4 decades to trash every competitor and critic, while aided by obliging science journalists who breathlessly repeated stories they could not understand, question or even check….that would call everything into question. EVERYTHING. And so we can't tell that story. Because, in particular, it implicates the storytellers. The Kakus. The institute. The Overbyes, Harvard. The Wittens,. Princeton. The Susskinds. The New York Times. The Stromingers. It's like "Murder on The Orient Express." Too many are guilty.

And yet, that is exactly what happened. We just watched a *tiny* number of zealots destroy the scientific integrity of a field essential to human progress and murder their competitors for 40 years by gaining control of the institutions rather than by succeeding in explaining the data, predicting new phenomena, correcting the past models and extending our knowledge.

I'm up for a rethink. But for the future of science, it is essential that their be a reckoning. Thia was a form of delusion and actual evil. Not hyperbolic evil. But pure a pure backstabbing, anti-collegial, excercise in pesudo-scientific superstition and cultish fantaticism. StringAnon if you will. I'm sorry to say this. But while it may have produced a lot of mathematics, as physics it more cult than we can bring ourselves to admit.

Let's do the work. Let's get clean and sober before we simply try to explain why we pissed away 40 years and millions and millions of dollars on a murderous cult that, if we are honest, simply does not share any of the norms of actual science.

2. **Eric Weinstein**
February 7, 2024

I apologize for numerous typos above (we are only given < 5 mins to see what we wrote properly displayed, and I couldn't finish in time given the length of the comment). I'm of course happy to correct them and repost for the sake of readability if that is possible Peter. Thanks.

3. **Pascal**
February 8, 2024

If Peter's work is to lead to a theory of everything as the New Scientist suggests, what is the plan to incorporate gravity?

4. **Hazelnut**
February 8, 2024

Peter, I've read this blog for a long time and I've always appreciated that you were a critic without your own agenda. You've always been extremely careful and one of your main demands over the years has been for more rigour. So I expected if you were to declare that you had your own "theory of everything" it would be something special.

As it stands I have seen a few vague blog posts, a slide deck and a preprint. Your fellow profilee Renate Loll has over a decade of work on CDT and dozens of publications. Her frequent collaborator Ambjørn started on this triangulation stuff in the mid 80s. Aastrup/Grimstrup have also been at this for over a decade and have published a number of detailed papers.

Now, of course a rigorous TOE is going to be hard, but if you think you've got something, Peter what are you talking about "This semester I'm mainly working on teaching and writing up notes"! You've posted about some sort of mathy graduate course you've been teaching for years. Forget the notes and work out the implications of your big idea. Publish them. Relate them to something in the real world: what do you have to say about Majorana particles (seems related?), axions, dark matter, dark energy etc.? You've been flirting with this for years, sit down and do physics. If you'd rather hint at your 'twistor unification ideas' in puff pieces and on podcasts, then I supposed I misjudged you.

5. **Peter Woit**
February 8, 2024

Pascal,
I mention this in the last preprint. There I even write down a Lagrangian (well known one for GR in chiral spinor variables). What I haven't done is reexamined the renormalizability problem in this context, there's still too much I don't understand to see if this will give something new about that.

Hazelnut,
There are two relevant preprints. My understanding of what is going on here keeps slowly improving. Things clarified a lot last fall when I realized there is a simple way of understanding what is different about what I am doing than previous work. I've tried to explain the idea in the preprint, in talks and in blog posts. There's a lot of work to do to better understand it and its implications and I'm doing this. You're comparing me unfavorably to groups of people who have been working on an idea for decades, but this is just me, I've been working on this particular idea for a few months by myself.

I'm finding basically no one understands the spinor geometry I'm using well enough to understand the new idea here. To try and change this and to sort out all the technicalities of what is going on for myself so I can make progress, one of the main things I've been doing has been to write up the notes I just made available in chapter 9 and 10. Yes, maybe this does have implications for Majorana fermions, I've got a lot of work ahead to understand this.

You misunderstand the course I'm teaching. Past courses were mainly aimed at undergraduates, trying to explain QM and representation theory in as simple terms as possible. This is for advanced graduate students, quite different. In a couple week I'll be lecturing about the details of 4d spinor geometry, using the notes I've just written. This is something I've never done before and will be helpful to me as well as hopefully enlightening to the students.
Later in the course I'll be lecturing on the details of spinor fields in the standard model, including issues of neutrino masses. This will be an opportunity for me to look at that problem through the lens of the new ideas.

I'm continually working on this. As long as no one else gets interested and it's just me, it's going to take a while to make progress. I'm not going to apologize for trying to explain to others why I think this is important and they should be interested too.

6. **Hazelnut**
   February 8, 2024

   Peter I am surprised you don't see the irony of what you wrote here. I am sure people working on E8, large extra dimensions, strings, MOND, IUT… all think their work is important and want to explain it to others.

   The point is that 'others' you should care about are other physicists who have a chance of understanding your work and engaging with it constructively. You have for years now criticized string hype. Do you think a blog, a podcast or a popular article about new "Theories of Everything" is the way you're going to interest other physicists in what you say are important and subtle ideas? How are you going to engage with your scientific peers? Why not, like your co-profilees, work out the consequences of your ideas in journals and conferences? Have you had any peer review of your preprints? When should I expect to read them in PRL?

   If your ideas are really as good as you think, you have spent a career building up to this, I don't see why you would not follow your own advice and build your theory carefully, rigorously and with due consideration of skeptical colleagues?

7. **Peter Woit**
   February 8, 2024

   Hazelnut,
   I am doing exactly what you suggest, other than your implication that I shouldn't talk about what I'm doing publicly. The preprint
   https://arxiv.org/abs/2311.00608
   was intended to explain as clearly as possible what I regard as a significant and important new idea. It was written to be readable by as many physicists as possible, and to be short enough to fit into PRL's format, and was circulated last fall to various experts in advance, who gave me helpful feedback. It was submitted to PRL and recently rejected, on grounds I can't really argue with, that this new proposal raises all sorts of questions that I don't have answers to.

   In retrospect I think it was a mistake to try to write this up in this way and submit to PRL. The short format there makes it impossible to provide needed technical background. One thing that has become clear to me is that the usual story of spinor geometry, especially how spinors in Minkowski spacetime and Euclidean spacetime are related, is not widely understood. Because of this I spent a lot of time carefully writing down details of this, in the chapters 9 and 10 I'm pointing people to. Once I've finished the project of this class in a couple months, I'll go back to writing a longer document, starting with those chapters, and working out more of the implications of the new proposal about spinors I'm making. At some point I'll go back to trying again with referees somewhere, but I'm hoping to get interest and feedback from others outside that venue.

   As for conferences, well no one has invited me to speak about this at one. I've looked carefully for conferences where a talk by me would be obviously appropriate, haven't seen any, would have tried contacting conference organizers if I had. I was invited to give one seminar talk about this recently, did so, and it's online. The response to that talk from those in attendance was encouraging.

   So, it seems to me that I'm doing exactly what you want, other than not fast enough making progress to the point I could claim dramatic success at a theory of beyond SM physics. You also seem to think I shouldn't be talking about some exciting new ideas until I have more of a finished product, but I'm not seeing the argument for that. Obviously I don't expect professionals in the field to understand what I'm doing and evaluate it based on stories in New Scientist, but I have carefully written down explicitly the technical details of what I am talking about and made those publicly available.

8. **Hazelnut**
   February 8, 2024

   So you are engaging in the normal process of science, where you have had an idea, written it down, failed to get it published, and are still working on it. Your ideas are not fully baked, and there's nothing wrong with that, 1000s of others are doing the same.

   The difference is that those others don't get profiled in the New Scientist or invited on Lex Freidman's podcast. This is happening for you based on your micro celebrity due to your book. Using that micro celebrity to promote your, admittedly, uncooked ideas seems very much on par with the various string hype people you've been criticizing for years.

   What would you say if some string person (take your pick) went on a popular podcast and had a glowing New Scientist profile written about them and their exciting new ideas about twistor string ads/cft etc.etc. You may say that these ideas have been out for 50 years and gone nowhere. Well your idea is hardly even an idea yet, by your own admission.

   Basically I think your attitude is hypocritical and you ought to be more humble. I think if you were applying the standards you've called for yourself, you should have declined to be interviewed for this piece and not spoken about your very preliminary work on a huge platform.

9. **Peter Woit**
   February 8, 2024

   Hazelnut,
   We're just going to have to disagree about whether I should speak publicly about what I see as an important new idea, even if its implications are not fully understood. Yes, the idea is not "fully baked", but there is a great deal about it that I do understand and have worked out. I have by now had a very long career of working on my own ideas that initially to me seemed promising, but as I worked on them it became clear that they had serious problems. The last few years have been the first time that the longer I work on an idea, the more I see not new problems with it but a solid new picture that hangs together. As indicated in the piece, I'm well aware that maybe this is just because I'm getting gaga, but I don't think so.

   I don't think the New Scientist writer decided to write about this because I wrote a popular book over twenty years ago. As he makes explicit in the article, he was well aware that using my "microcelebrity" to make dubious claims about my own work is not something I'm known to ever have done over the years.

The comparison to my critique of string theory makes so little sense I'm not going to bother trying to answer it.

As for journal publication, I believe I could publish something about this in a journal, don't though think that would prove very much. At the moment it seems more important to me to work on developing a better version of these ideas than trying to find the right journal/set of referees that would want to publish the current version. That an idea is published in some journal doesn't make it a good or interesting one.

10. **Hazelnut**
February 8, 2024

The issue isn't you speaking publicly, you surely understand the difference between a piece like this and, say, a conference talk. You surely know that if you hadn't written the book back then you would not be invited on these podcasts or profiled like this based on 2 preprints with 5 total citations.

You may not see the string comparison as apt, but if your part in this New Scientist piece was replaced with one of the scientists from the last Quanta article you talked about, you'd have something to say. While a journal publication might not mean much these days, it at least demonstrates your work has been reviewed by a couple of competent people and you just said the feedback from PRL was apt. Your reputation is exactly why I am surprised that this is how you are approaching things. Do science not PR.

11. **Peter Woit**
February 8, 2024

Hazelnut,
The sum total of time I've spent on the New Scientist piece that has been stolen from doing science is about 3 hours so far: one talking to Michael Brooks, one writing a blog entry, one arguing with you.

To make the obvious point, I've never anywhere criticized anyone with new and different ideas who is trying to get attention for them. What I have criticized is well-entrenched bad ideas that come with the fullest backing of journal publications, conference talks, etc. that you think I should get before talking to the public.

At this point I don't think people are paying attention to what I have to say because of a book I wrote 22 years ago that now sells about 5 copies/year and that few under the age of 40 have actually read. Journalists pay attention more because of the blog, which many have found to be a reliable source of information. Mathematicians and physicists who look at the unrefereed blog can decide for themselves whether I know what I'm talking about. Some think so, others think I'm a dangerous incompetent.

Most importantly, the technical details of what I'm talking about are out there, in a paper and a seminar talk, now with detailed background available if you want. I'm getting the impression you have no idea what this is all about, aren't really interested in the science.

12. **Marty**
February 8, 2024

Hi Peter,

I think there is some real wisdom in what Hazelnut is saying. His tone strikes me as that of a concerned friend who is trying to get you to consider a change of course.

I defended you at various times during the "string wars" era, especially on this blog, against what I saw as unjustified attacks on your motivations and character. I have viewed you as someone who values intellectual honesty and humility. Yet I also find myself agreeing with a lot of what Hazelnut says, especially about your using "popular science" channels to promote work that, according to you, is the product of only a few months of effort and is very much in a preliminary state.

I don't understand why you think promoting your ideas through popular channels is a good way to interest the physics community. I can imagine how most physicists who hear of the New Scientist piece could react with something like, "Huh? Where's the beef? Why is he going on about it?" What I don't imagine is that their next reaction is anything like, "If it's in New Scientist it must be very promising! I want to get involved!"

While some people seem to think any publicity is good publicity, I doubt you're one of them. But I think in this case premature publicity may work against you. It can lead serious-minded physicists to question your motivations, given the lack of an object record of significant, serious effort on your part to develop the ideas into a form they can evaluate. It would be reasonable for them to think, "Why take it seriously, since it's probably just another hype and PR effort like others we have seen over the years?

13. **Peter Woit**
February 8, 2024

Marty,
I'm really not understanding what you and Hazelnut are objecting to. Yes, of course, a story in New Scientist is not what is going to convince most theorists that they should take the idea I'm talking about seriously. For one thing, New Scientist has a long history of publishing breathless articles about bogus claims of string theorists.

What happened here is Michael Brooks sent me an email telling me he was thinking of writing about unified theories and wanted to talk to me. He's a serious science journalist with a Ph.D. in physics. What do you think I should have done? Told him, no, I don't talk to journalists like you? My policy has always been to talk to serious journalists who want to discuss topics on which I think I have something well-informed to say. I many times have turned down requests to talk about other kinds of topics.

So, we set a time to talk. A few years ago, my standard spiel about "do you think there will be a unified theory" would have been "yes I do, lots of reasons to think it's possible, and I have some vague ideas of my own, although nothing solid I want to talk about". But that's not what I now think: I am deadly serious that I think I have a very specific and very important idea. So, what should I have done? Should I have refused to tell him what I really think about a scientific issue? Not telling people what I really think is not how I go through life.

Should I have said "I have this idea, there's a preprint and I've given a talk about it, but you should not pay it any attention until it is properly vetted by the powers that be and published in a journal"? By the way, obviously there are journals now that will publish anything, so can you explain to me exactly what the list is of acceptable journals that would vet my ideas in a way that would allow me to talk to Brooks about them?

My comments about this particular idea being recent are giving a misleading impression. The funny thing here is that the very half-baked version of these ideas was published, very long ago, in a very reputable journal, see
Nuclear Physics B303 (1988) 329-342
The half-baked version of the ideas I'm talking about is 36 years old and properly published and vetted. But I've always been aware these were half-baked and I never promoted them as anything other than that.

What changed is that a few years ago I learned more about twistors and about Euclidean QFT, and this convinced me that these ideas that I had kind of given up on actually were quite promising. I wrote a somewhat more baked preprint at that point. Then, late last summer, I realized that there is a very precise change you can make in the standard spinor geometry story that realizes what I had been looking for. I wrote that up in the recent preprint. I've been thinking a lot about this since last summer and the more I learn, the more I think it's an important and precise idea (although working out its implications is a major project that will keep me occupied for a long time).

So, what am I doing wrong here (other than wasting time answering comments that I should be spending working on this…)?

14. **Hazelnut**
February 8, 2024

    Look, sorry if I'm coming across as attacking you, I'm trying to be constructive but internet comment sections are not designed for it. I think we are just going round in circles here, and I just re-iterate what the commenter above says. Which scenario do you think is more likely: the popular science aficionados who have come across you recently are going to start working on Euclidean Twistor unification? Or you write up your work, publish it, speak about it at physics conferences and, if it is as important as you think, some physicists start to work on it?

    I am as cynical as anyone about academia, publication etc. but I am even more cynical about the idea of publication by podcast. It is simply not the case that outsider ideas will be rejected out of hand by journals, case in point Renate Loll and *strup from the New Scientist piece. Do you think that some ideas on your personal blog and a couple of preprints are going to inspire young physicists to work on this? It can be a pain, but do the work and the physics community (outside of elite American universities) isn't as closed off as you think.

    As for my own understanding and motivations I left hep-lat/hep-th about 10 years ago for greener pastures, but would love to see it flourishing again.

15. **Peter Woit**
February 8, 2024

    Hazelnut,
    It seems you have no interest in the actual science here, but I'd encourage you to actually read the preprint instead of complaining about podcasts.

    And, no, I'm not complaining that my "outsider ideas" are being unfairly rejected by journals. I am every day "doing the work", very much enjoying trying to better understand the implications of the idea you don't want to know about.

16. **Marty**
February 8, 2024

    Peter,

    I appreciate your sincere, thoughtful reply. You have explained your position well, and I can understand why you might feel exasperated that anyone would find fault with having an honest, public conversation with an intelligent interviewer, just because he happens to work for a popular magazine about science.

    You ask what you might have done differently that would satisfy "critics" like Hazelnut and me. Obviously it isn't for me to say what you should or shouldn't do, and I can't speak for "Hazelnut," but I'll offer an opinion.

    For background, I too have been working on questions in fundamental physics that I think are very promising, work that is a continuation and evolution of my thesis work. My approach is somewhat different from yours — I'm not a Platonist; I think mathematical structures in nature offer idealized (but useful!) descriptions of underlying physical processes, instead of prior idealized mathematical *structures* (like preexisting manifolds) imposing constraints on fundamental processes — but that is irrelevant to this comment.

    What I think *is* relevant is that having worked alone, for a number of years now, with progress painfully slow too much of the time, I can empathize with your desire to get others excited, and maybe even get competent people interested enough to also work on your research program. In no way do I criticize that desire, nor your sincere efforts in that direction.

    But… (there's always a "but", isn't there?) I *personally* would be very cautious in promoting my ideas to a general audience without having some very concrete results, developed in enough detail that experts can agree there is something interesting there. So when the subject of my research comes up with people I don't know, I generally speak cautiously about it: if they are knowledgeable and interested, I'm comfortable going into more detail, and explain why I'm personally excited about it, but regardless of the "audience" I always emphasize that my program is still preliminary, requiring lots more work, and I'm not yet comfortable promoting it. (In fact, due to the many facets of the program, the interconnectedness between its facets, its ambitiousness and my personal sense of caution, I've not even attempted to publish on it just yet.)

    Anyway, that's my own approach. It isn't suitable for everyone, and it may not be for you either. It's very much out of sync with the "modern" belief that it's important to generate a lot of buzz to get the grant money flowing, but since I'm not relying on grants I can live with that. It's important to me that I don't have to worry I'm misleading people.

    And just to be clear, I think your discussing and even promoting your research efforts on your blog are entirely appropriate. The choice of whether to read or how to think about what you write is entirely up to your interested readers.

17. **Peter Woit**
February 8, 2024

    Marty,
    Thanks for your comments. Like you, I'm not dependent on grants, getting buzz to get a grant is not anything I'm interested in.

    What annoys me a bit about this discussion is that way back when I lived through years of people telling me they wouldn't read my book or take seriously my arguments about string theory, since what I was saying was out of sync with the conventional wisdom and the opinions of the experts of the field. Now I'm hearing that people don't want to read a five page paper or take seriously my claims about a solid new idea until it's been vetted by the same sort of conventional wisdom and same experts. Seems to me though that by now I have a track record people should take into account.

18. **Marty**
February 8, 2024

    Peter,

    Well, I hope you aren't interpreting my words as meaning people should not take your ideas seriously until they've been vetted by the sort of people who criticized you in the past for not publishing in the field you criticize. That would be very much at odds with what I was trying to say.

    In fact, back in the bad old "string wars" days I always thought arguments by your critics were bogus when they claimed you weren't qualified to assess or criticize string theory, simply because you weren't a "string theorist" or "active researcher," even though it was obvious from the specificity of your comments that you had spent significant time and effort to learn about what you were criticizing.

    My "criticism," if you want to call it that, is solely about the choice of medium for discussing your ideas. Since popular science media also have a vested interest in creating a level of buzz, I imagine it can sometimes work against the best interests of an interviewee…

Anyway, I ‘m glad you have found a direction to pursue seriously. Now you just need a grant so you can hire a postdoc/researcher or two to help you make faster progress. 🙂

19. **Hazelnut**
February 9, 2024

Okay I'll be blunt – only 'publishing' your ideas in preprints and blogs makes you look like a crank, using your position (as a Columbia professor, notable author, high profile blogger) to promote these unproven ideas in popular media makes you look like a hypocrite. Accusing people who are mildly critiquing you of having "no interest in science" is the kind of thing people used to say to you.

For the record I don't think you are a crank, I have read your preprint, understand the gist if not the fine details and my reaction to it was 'huh'. When you have actually worked out some physical prediction or interesting consequences of your idea that might be interesting. It is your job to convince the wider scientific community not vice versa.

20. **John**
February 9, 2024

Our local public library has the New Scientist magazine on its magazine rack.

https://coqlibrary.chilifresh.com/r1/polaris/search/title.aspx?ctx=1.1033.0.0.1&pos=1&cn=126950

The current issue on display is the Jan 20, 2024 issue so it will probably be a couple of weeks before the issue with this article appears in the library. So others interested in reading this article might want to check their local library to see if they have the magazine in their catalogue.

21. **Dom**
February 9, 2024

Having read this Blog for many years it seems to me that one of the default put-downs aimed at Peter from the string establishment is "Well why can't he come up with an alternative idea?'
It seems odd to then attack him for saying "well actually I am working on one" which is all I can see that he has done.
If the attack then becomes "it's so unfair that he uses his blog to publicise it" then his obvious response should be:
"Why don't you start your own blog then?"

22. **Felix**
February 9, 2024

The pearl-clutching about Peter talking to a popular science magazine is completely over the top. If a journalist asks to talk to you about a subject you're known to be interested in, and you say 'I'm working on something I think is extremely promising, nothing concrete yet, and who knows, it might be nothing', what is morally objectionable about that? Comparing this to decades of string theory hype or the 'black holes in the lab' scandal is bizarre. Can anyone name a single string theorist who has said anything close to 'Maybe I've just gotten old and just lost my way' about their research interests?

23. **RichardT**
February 9, 2024

I think there is nothing wrong in discussing in-progress new ideas in any forum. If Peter gets to a point where he wants to publish something more, then of course he can.

Criticising a decades old, well-entrenched set of ideas such as string theory is very different from attacking new and emerging ideas. I don't see Peter doing the latter, which is why I don't see any hypocrisy here.

24. **jack morava**
February 9, 2024

What Felix said.

25. **Peter Shor**
February 9, 2024

Peter, you say:

> Now I'm hearing that people don't want to read a five page paper or take seriously my claims about a solid new idea until it's been vetted by the same sort of conventional wisdom and same experts. Seems to me though that by now I have a track record people should take into account.

It's not just your track record that people should be taking into account; they should also be taking the track record of these "experts" into account.

26. **Peter Woit**
February 9, 2024

A serious question about journals, hope to hear from those who have thought more about this and know much more:

Why publish a theoretical physics paper in a journal? Note this is not about mathematics papers, where there is a very good argument that this is the right way to get proofs checked, crucial to having as reliable a literature as possible.

I can think of several arguments against journal publication:

1. Everyone reads new physics papers on the arXiv, not in a journal. The only exception to this I can think of was the wormhole publicity stunt paper, published in Nature, not available on the arXiv (still to this day I think).

2. Publication of a theoretical physics paper in a journal gives minimal information about its worth or reliability. See the example in 1, or look up the published works of the Bogdanov brothers.

3. By publishing a paper in a journal, you are handing over rights to the content (yes, I know, exactly which rights is a complicated story) to some organization, often a profit-making one that has its own interests which are not yours and not those of the community.

As for the arguments pro:

1. Journal publication is a standard credential used by funding agencies to decide about grants, universities to decide about hiring, promotion, pay raises, etc. In my particular case none of these things are relevant to my situation.

2. Feedback from referees can be useful in many ways.

3. Publication in a small number of high profile journals is good for PR. See the Nature story above. PRL encourages people to have their institutions put out press releases when a PRL paper comes out.

When I wrote the "Spacetime is right-handed" paper last fall, the main goal was to write something as short and as readable as possible that got the main technical idea across and explained why it was important and opened up a new way of thinking about unification. I did though have in the back of my mind the idea of submitting it to PRL, for reasons 2 and 3. Given some of the accusations in the comments above that I'm doing PR instead of submitting papers to journals, the funny thing is that the only case I can think of in recent years when I've done something motivated by PR was exactly this case of submitting that paper to that journal. It seemed likely that this PR move wouldn't work, that the paper would not be accepted, but I honestly think it contains original and important ideas that fit precisely what PRL says it is looking for and trying to publish.

There was the secondary motivation of hoping for useful feedback. There were two referee reports, one with nothing useful, the other with some detailed comments about the exposition which were helpful. Neither referee found anything wrong with the paper or argued that the idea was already in the literature. The reports both pretty much argued that the implications of the idea in the paper remain to be developed, so instead I should write a longer paper and further develop the ideas. I can't argue with that, plan to do so.

While that's what I'm working on, when done I'm not seeing a decisive argument in favor of submitting the preprint to a journal. It will be much too long for a PRL announcement, so 2. is inapplicable as well as 1. Yes, there might be helpful feedback from referees, but I've found that there are experts in this field I know who have been willing to look at a paper and provide such feedback before I submit the paper to the arXiv as a preprint.

Another part of the argument against submitting to a journal is that I unfortunately know very little about them. As mentioned above, I don't look at them when they come out and see what they publish. I also don't know their copyright and open access policies, or the exact nature of the organization behind them (how predatory on a scale of 0-100?). I'd have to do a bunch of research to gather information. Or, maybe a helpful commenter can point to the best possible option.

27. **Hazelnut**
February 9, 2024

I am sure all the other people who produce 1 preprint a year and refuse to even attempt to publish their work after a single rejection will soon have write ups in the New Scientist.

Peter has made his career out of clutching pearls whenever some string theory booster makes a public statement. You know all the string guys are also genuinely excited about their own ideas, just as Peter is? And while attacking these entrenched ideas what did he suggest instead? High among the advice was a call for rigour and for physics to take some lessons from math. I wasn't aware that mathematicians refuse to publish their work in reputable journals or go to conferences.

If this was someone you were not a fan of, what would you think of this attitude?Peter's reputation is as a critic not a researcher, so he cannot really lean on that here.

And yes, you can and should say no to journalists sometimes. I have done it myself.

28. **Chris**
February 9, 2024

Peter, I think what you are doing is great. The workings of science and the iterative and generative process of coming up with ideas is something that should be shown to people (all people, not just a select few self-anointed ones). People like Hazelnut and Marty seem to be fine with the old ivory tower approach to things. It's such an antiquated mindset.

Keep doing what you are doing!

29. **B. Malpani**
February 9, 2024

Hazelnut wrote:
You know all the string guys are also genuinely excited about their own ideas, just as Peter is?

No, I don't know that at all. And at this point in time, I can't believe it at all. I think that they are perfectly aware of their colossal failure: how could anyone think otherwise?

There's a reason why David Green came up with the idea that we won't be able to judge string theory for another 80 years – he wants to be sure that none of the string theorists are forced into that confrontation with their failure, or that their current critics can feel that they've been vindicated.

Speaking strictly for myself, petty attempts to impugn Woit for his completely unobjectionable interview in New Scientist just serve to reinforce my extremely low opinion of the probity of string theorists, and underscore their desperation to avoid being confronted with their decades of failure.

The intellectual and monetary capital, and careers, that have been squandered on string theory will remain squandered. Attacking Woit is not going to transform string theory from a scientific dead end into a success, sorry.

30. **JJ**
February 9, 2024

Peter has always allowed that string theory ideas are worth pursuing. His main point for decades has been that those ideas shouldn't be allowed to crowd out all other foundational ideas, in the field or the surrounding public discussion. He has always addressed himself to both audiences. Now he lets it be known to specialists and the interested public that he's got what he thinks is a promising idea. He doesn't say it's the only game in town, he's not looking to replace one variety of hype with another, he's not moving goalposts, he's just putting a new idea in precisely the places he has always said there should be lots of them. What's the problem again?

31. **Peter Woit**
February 9, 2024

All,
Please, enough about the tedious subject of string theory. As far as I know, Hazelnut/Marty aren't string theorists.

32. **Scott Aaronson**

February 9, 2024

FWIW, I've faced a similar problem: because of my role as a blogger and public commentator on quantum computing (and now also AI safety), I talk to journalists a lot, and often if they hear that I'm working on something new, they want to interview me about it, even if it's still half-baked, and even if it doesn't particularly deserve more attention than five dozen other things that my colleagues are working on. So then, should I refuse to talk?

I decided on the following compromise policy:

Don't specifically SEEK OUT "unfair advantages," which would give my research undue attention for non-research-related reasons.

BUT: explaining whatever you're currently excited about, with anyone who seems genuinely interested in it, is like 70% of the joy of science. So don't abstain from that joy. If people ask what I'm working on, I should just tell them, along with my honest assessment of how solid or important or worth writing about it is.

33. **Mitchell Porter**
February 9, 2024

Peter: How close are you, to writing a lagrangian for standard model fields coupled to gravity, in your right-handed Euclidean space?

34. **Peter Woit**
February 9, 2024

Mitchell Porter,
The problem isn't so much writing down the Lagrangian, which will just be usual one for the Standard Model + GR. The real question is whether the use of chiral variables opens up a way of constraining the usual SM, explaining something new. The main problems here are that to get something new you probably need a new idea about the geometry of the Higgs, and I'm trying various things there. On the GR side, you have the usual problem that you are doing Euclidean quantum gravity, how do you understand analytic continuation?

There's lots of possibilities to think about, but my time for this is limited for the next couple months as I try to focus on the course and the notes for that (some of which involve working out background material useful for trying out some new ideas involving the chiral geometry).

35. **S**
February 9, 2024

From the article:

"It isn't that Woit believes he necessarily has the answers. But, he says, it is good to know that, despite the long search for a theory of everything, there are still new possibilities opening up."

I see this as perfectly consistent with a dedication to rigor and a desire to avoid hyping things that may not be true. How many of the string theory articles that Peter complained about over the years had any such disclaimer? From what I can remember, extremely few, if any. Rigor is one thing; we also need new ideas to be rigorous about. When they come, there's nothing wrong with talking about them, even in public. I'll join in the accusations of hypocrisy if we're imminently treated to glossy hardcover books or magazine articles where Peter breathlessly explains to a lay audience what his ideas imply about the meaning of their lives, or we're treated to weekly posts promising experimental evidence in six months over a years-long timeframe.

Till then, all I see is a physicist who has a well-deserved platform being asked what he's excited about, and answering the question. Good for him for being excited about physics after everything he's seen.

36. **NoGo**
February 10, 2024

Hazelnut calls Peter Woit hypocritical. As fat as I know, this word means doing something yourself that you criticize others for.

As a reader of his book and this blog, I don't need a physics degree to tell that the gist of what Peter criticizes String theorists for is misrepresenting the state of their theories. Refusing to admit failure, claiming connection with experiment, insisting on "no other game in town", pretending they have a coherent well-defined theory etc. etc. all boil down to misrepresentation.

I don't see any evidence that Peter misrepresents his work in the interview with New Scientist. He is quite clear about this being an idea he likes which may yet turn wrong and which is not yet fully developed and is not endorsed by physics community.

I don't recall Peter ever criticizing String theorists just for talking to the "secular press" or the fact they promote their work, his beef is with _what_ they tell the press and _how_ they promote their work.

I find the accusation of hypocrisy completely bogus.

37. **Filip Hrdlicka**
February 10, 2024

It is a pity that the comment section focuses on just Peter's part of the article and not on other mentioned ideas like Aastrup/Grimstrup ideas.

38. **Robert A. Wilson**
February 10, 2024

Peter,
Can I ask you a mathematical/physical question about your model? I am sure your readers would prefer this to yet more amateur sociology.
The algebraic construction of twistors relies on a Clifford algebra for Spin(4,2), which can be either Cl(4,2) or Cl(2,4). I have heard your two talks and I have read your two preprints, and I still cannot figure out which one you use. So that is my question.
It is not a frivolous or irrelevant question, because the corresponding question for spinors is whether you use Cl(3,1) or Cl(1,3). The former gives you real (Majorana) spinors and the latter gives you pseudoreal/complex (Dirac) spinors. The Standard Model is completely clear that the latter describes physical matter, and the former does not. It is therefore an important question as to whether your twistors are real or pseudoreal, and I am looking forward to your answer.

39. **Doug McDonald**
February 10, 2024

A question for Peter that I don't fully understand (not just the answer, the question):

is the “right-handed” in your theory purely a mathematical definition, or is it anchored to the real world through the weak interaction and how your theory fits into the rest of the standard model?

40. **Peter Woit**
February 10, 2024

About those upset that I’m talking to the press about spinors instead of following the rules and talking only to physicists in carefully vetted venues (invited conference talks and refereed publications in the right journals). I realized that this is very familiar, it’s exactly the reaction I got nearly 20 years ago when I started getting quoted in the press about the problems of string theory. What I was saying then I think turned out to be quite accurate. We’ll see what the future holds for the ideas I’m talking about now, but I’m optimistic about their prospects.

41. **Peter Woit**
February 10, 2024

Robert A. Wilson,

Short answer is I don’t know. One difference with Clifford algebras is that the twistor story is very chirally asymmetric. One is making a choice of convention: points are right-handed spinors. Changing convention, what’s space-time and what’s internal geometry interchange.

That the different choice of convention (metric signature or def. of Clifford algebra) changes the nature of spinors and twistors is something mysterious. If what is going on gets properly understood, perhaps the two different things one sees here will both appear, playing different roles.

42. **Peter Woit**
February 10, 2024

Doug McDonald,
It’s a mathematical fact that spinors in 4d come in two kinds, a convention to call them “right” and “left”. What I’m proposing is very much related to the real world, basically that you can describe what appears in Minkowski spacetime as spacetime geometry just using one kind (“right” by convention), that the other kind are essentially the spinors of weak isospin.

43. **Robert A. Wilson**
February 10, 2024

Peter,
Chiral asymmetry is to do with what happens when you restrict to the even part of the Clifford algebra. One can of course change conventions as to which half of the splitting is left-handed and which is right-handed, and which half you use for spacetime and which half you use for internal symmetry. But that isn’t what I’m talking about. What is going on in the Dirac/Majorana spinor question is perfectly well understood, apart from some residual questions about neutrinos. I do not think it is in the least mysterious. Dirac spinors describe observed physics, and Majorana spinors do not. The same seems to be true for twistors, as far as I can see – pseudoreal twistors describe observed physics (three generations of massive fermions), and real twistors do not. If you disagree, then I would like to know why you disagree, because you probably have a deeper insight into the physics here than I do.

44. **Peter Woit**
February 10, 2024

Robert A. Wilson,
Again, I really don’t understand the possible physical significance of this issue. The place in physics it might be significant is in the story of right-handed neutrinos and neutrino mass terms. What I’m trying to understand is the relation between different real forms. In terms of the Clifford algebra you’re getting two possible different real forms to associate to Minkowski space-time. Maybe they both play a different role, but that is pure speculation.

45. **Eleni Petrakou**
February 11, 2024

For what continuing the “amateur sociology” discussion is worth,
Previous comments showed in detail why Peter’s behaviour has nothing to do with either hypocrisy or unfair advantages. But as a science journalist I’d feel like clarifying a couple of points to any interested earnest critics, assuming those exist in here.
For one, informing people of ongoing, potentially interesting, work on open questions is among the top objectives of a physics journalist. Everyone that knows about science knows that almost all of this work will lead to nothing, and this has nothing to do with reporting on it. What has to do with integrity, however, is presenting ongoing work as something else, usually a lot more successful.
Also, on a more practical side, journalists working on a topic will search for people involved in it. If word of mouth or a google search or prior knowledge provides names then, yes, we’ll go for them. If needs be we’ll look beyond them, but talking to the already known experts is in no way immoral.
Having to spell this out pains me, but reporting unresolved opinions is totally fine; and it has nothing to do with monopolizing grants because of having very resolved and failed opinions.

46. **Robert A. Wilson**
February 11, 2024

Yes, indeed, my question is about real forms, specifically whether you are working in SL(8,R) or SL(4,H) to describe the chirality of twistors. I understand that you don’t yet have an answer to that, but that you are looking at real forms of SU(4), where the corresponding question is to distinguish between SL(4,R)=Spin(3,3) acting on a Majorana spinor, and SL(2,H)=Spin(5,1) acting on a Dirac spinor. But then I can’t reconcile this question with properties of twistors acted on by Spin(4,2)=SU(2,2), or with your embedding of U(1) x SU(3) which requires real form SU(4) or SU(3,1).
So you will surely eventually have to address the question of SL(8,R) or SL(4,H), and when you do, it may be useful to note that SL(4,H) contains SU(2) x U(3), but SL(8,R) does not.

47. **Alex**
February 11, 2024

I wasn’t able to read the article because of the paywall, but it seems there’s no mention of Connes’ Spectral SM?

It really baffles me how little attention it gets. People would be very surprised if they knew how well developed and understood it is by now. Whenever anyone asks me about the unification problem, I always mention this approach as the best we have at this moment in time. In my opinion, of course…

48. **Bruys**
February 13, 2024

I don't understand the criticism of Peter for allowing someone to write about his ideas in New Scientist. What a boring world it would be if no-one discussed their ideas!

Peter has been critical of people making unreasonable claims for String Theory, not for discussing String Theory ideas – there is no hypocrisy being practised here.

49. **zzz**
February 14, 2024

Hnut:
"I have read your preprint, understand the gist if not the fine details and my reaction to it was 'huh'. When you have actually worked out some physical prediction or interesting consequences of your idea that might be interesting. "

you are all over the place here, is this an interested "huh"? or not ?
because (again) you are immediately contradicting your criticism,

50. **Peter Woit**
February 14, 2024

zzz,
I think "huh?" is a pretty universal reaction from physicists to that paper. The argument for a different relation between vector and spinor geometry is hard to comprehend if you haven't thought a lot about the usual version of this. I tried to write something short and as comprehensible as possible, but don't think that succeeded very well. Next there will be a much longer version, we'll see if that helps.

In the paper I try and point out how this different relation opens up possibilities for a new way to do unification. To me, these seem very promising and something that no one has looked at before, but time will tell. It's perfectly sensible for people to decide they want to ignore this until it's further developed, but I'm still seeing no argument for why I shouldn't keep trying to explain these ideas to anyone who will listen.

Two Items

February 14, 2024
Categories: This Week's Hype

If I'm going to point to something about string theory and say the same things as always about it, seems best to first start with the opposite, an item about something really worth reading.

- This spring I've been teaching a graduate course aimed at getting to an explanation of the Standard Model aimed at mathematicians. The first few weeks have been about quantization and quantum mechanics, today I'm starting on quantum field theory, starting with developing the framework of non-relativistic QFT. While trying to figure out how best to pass from QM to QFT, I've kept coming across various aspects of this that I've always found confusing, never seen a good explanation of. Today I ran across a wonderful article by Thanu Padmanabhan, who I knew about just because of his very good introductory book on QFT, Quantum Field Theory: The why, what and how. The article is called "Obtaining the Non-relativistic Quantum Mechanics from Quantum Field Theory: Issues, Folklores and Facts" and subtitled "What happens to the anti-particles when you take the non-relativistic limit of QFT?" It contains a lot of very clear discussion of issues that come up when you try and think about the QM/QFT relationship, a sort of thing I haven't seen anywhere else.

  Looking for more of Padmanabhan's writings, I was sad to find out that he passed away in 2021 at a relatively young age, which is a great loss. For more about him, there's a collection of essays by those who knew him available here.

- For something I can't recommend paying more attention to, New Scientist has an article labeled How to Test String Theory, which is mostly an interview with Joseph Conlon. Conlon's goal is to make the case for string theory, in its original form as a unified theory with compactified extra dimensions. On the issue of testability there's nothing new, just the usual unfalsifiable story that among all of the extra stuff (moduli fields, axions, extra dimension, extended structures) that appears in string theory and that string theorists have to go to great trouble to make non-observable, it's in principle conceivable that somebody might observe one of these things someday. But, that's not really what people mean when they ask for a test. For details of the sort of thing he's talking about in the New Scientist article, see here.

  I strongly disagree with Conlon about some of what he's saying, but the situation is very much like it has always been with many string theorists since way back to nearly forty years ago. We don't disagree about the facts, it's just that I've always looked at these facts and interpreted them as showing string theory unification ideas to be unpromising, whereas string theorists like Conlon somehow find reasons for optimism, or at least for believing there's no better thing to do with their time. Last time I was in Oxford, Conlon invited me to lunch at his College and I enjoyed our conversation. I think we agreed on many topics, even about what is going on in string theory, but it looks like we're always going to have diametrically opposed views on this particular question. For more from him, as far as popular books by string theorists go, his Why String Theory? book is about the best there is, see more about this here.

Update: A couple more.

- Curt Jaimungal has a conversation with Lee Smolin.
- Andy Strominger is giving talks on Celestial Holography at the KITP "What is String Theory?" program, first one is here. Lots of questions from the audience. One thing he makes clear is that this is not leading to a theory of quantum gravity. Stringking is back, his comments on this:

  > KITP program on what is string theory such a joke this week. Strominger shilling his celestial vaporware.

Comments

1. **Anonyrat**
   February 15, 2024

   I was in high school, just learning calculus, and T. Padmanabhan was doing his Master's at University College in Thiruvanantapuram, and one summer he organized with a co-student or two, an after hours class for interested students, where he taught the Calculus of Variations, Lagrangian and Hamiltonian mechanics and made it all intelligible to me – an amazing teacher!

   I wish that I had kept contact; but that one summer is a memory I cherish.

2. **tulpoeid**
   February 15, 2024

   I realized it's Padmanabhan who's written the mesmerizing textbook "Sleeping Beauties in Theoretical Physics"… such a loss…

3. **Diogenes**
   February 16, 2024

   A lot of this seems to be an embedding of "personal Bayesianism" into the scientific project. So people start with different personal "creedences" of some idea (perhaps highly correlated to their involvement in it) and update more or less frequently with new evidence according to their personal estimate of the value of the evidence, conditional probabilities, etc. So it's not really surprising to see different estimates of say [10%, 90%] creedences on theories based on some empirical "my truth" basis like this. Especially in some cases where there is no real evidence to update on, or priors to estimate.

   What seems a bit different here is the idea of "expected future personal Bayesianism", meaning personal forecasts of future progress getting rolled into current credeences with very high probability. If the progress was that automatic it would already have happened. So very high "expected future personal Bayesianism" creedences based on very little evidence or substance seem to be the cause of the sociological argument here.

   Sean Carroll's long podcast on the state of Physics kind of outlines the case for this, but while the first half of the podcast is very good, when he veers into the defence of "expected future personal Bayesianism" and insider hiring practices in academia it is less impressive, although I think very honest. Believing in the serial correlation of past individual scientist's successes with future successs over very long time horizons is counter to quite a bit of evidence.

   The question is what is the proposed alternative to insider "expected future personal Bayesiansm?" I suspect it would look something like the rich guy funding models of Perimeter and Simons, run like a venture capital fund, with the expected return of each investment near zero, but high overall (societal) returns to a diversified enough portfolio of ideas and individuals. But more funding to entrenched incumbents supported by "look these guys are really smart, and they almost did something incredibly cool 20 years ago" probably has a very average rate of return, especially if the pitch-deck (in the form of popular science books) is a bit cooked.

   An even bigger problem is whether these theories of Planck-scale physics make sense to attempt, given they can't be directly proven or evaluated. But although I see no evidence of this, my "expected future personal Bayesianism" gives me hope that they can.

4. **Michael Arthur Stringer**
February 17, 2024

Joseph Conlon is a very nice guy with a passion for both the mathematics and physics(?) found in String Theory. However, in the New Scientist interview his role as a defendant for the continuing relevance of String Theory (rather than a degenerate research program) falls pretty flat – he comes over more as a 'Witness for the Prosecution'.

5. **Joseph Conlon**
February 17, 2024

Thanks, Peter. I also enjoyed our lunch in Oxford. No personal quarrel; but I am as mystified by your views as you seem to be about mine.

If one (a) accepts that there is an awful lot we don't know about our current universe and (b) think that research on these questions is worthwhile (and my impression is we agree on both points) then I cannot really understand the hostility to string theory as a subject, especially when compared to the alternatives, although I could understand some allergic reaction to some of the bad PR done in the name of string theory.

Likewise, I can't really grok why you seem hostile to ideas if they have the label `string theory' attached, but indifferent if the same ideas appear without that label. For example, the physics part of the interview concerns ideas for physics in the epoch between inflation and BBN (e.g. moduli domination), which you seem quite scathing about above.

We really know essentially nothing, observationally, about this epoch. Nil, zip, dada. And it could cover up to a factor of 10^30 in time. So surely all theorists in the particle theory / cosmology zone with at least some interest in pheno should care about ideas for what could be in this epoch and how we could test them? This is up to half the lifetime of the universe on a log scale! Of course we should think about what happened them and how we might observe them.

I don't recall any posts by you criticising cosmologists who write papers about this era as unfalsifiable, etc — but many of the same/similar ideas (early matter domination/trackers/quintessence/kination etc) become 'not even wrong' when in a string context: and I cannot really understand why.

I can 'get' hostility to string theory as a subject from those who think that we should all work on climate physics, AI or quantum computing. I can get allergic reactions to bad PR. But I can't really understand intellectual hostility from anyone with a broad-minded interest in particle theory.

6. **Peter Woit**
February 18, 2024

Joe,
In this particular case the hostility is to claims that effects of moduli fields are a "test of string theory", which is misleading (a more accurate description of the implications of such fields would be as serious evidence against string theory).

In general I don't spend my time criticizing cosmologists or anyone else for working on whatever they find interesting, however unpromising or misguided it looks to me. The focus on string theory is because the way it has been pursued and hyped to the public and other scientists over the last 40 years has done a lot of damage to the kind of research I care about. This first drew almost all resources in the field to one speculative idea, which was bad enough. Now after decades of this idea not working out, it has discredited all research on unified theories.

I do try and spend my time on more positive pursuits, but the hype campaign won't stop and actually just gets more disturbing (see the wormhole fiasco, or the recent Brian Greene panel). About the only other person criticizing this is Sabine Hossenfelder, and my point of view is very different than hers on the problem (no, it's not "Lost in Math").

7. **Joseph Conlon**
February 18, 2024

Those aren't my words though. In what I say, I try and be super-careful about any expression like 'test of string theory'. As you know, if you talk to journalists (which generally scientists should do if asked) you don't control the blurb and you don't control the title: and likely something will be picked that you would never choose yourself.

So you either have to live with headlines you would rather were different, or not say anything at all: and I choose the former.

8. **Art**
February 24, 2024

It looks like some chapters have been recalled in your latest qftmath notes version. I was wondering if Padmanabhan's article had influenced your treatment.

9. **Peter Woit**
February 24, 2024

Art,
I've just this afternoon uploaded the latest version of the notes, see
https://www.math.columbia.edu/~woit/QFT/qftmath.pdf

The material I've been lecturing on the last week or two and have now finished (chapter 7) has been about non-relativistic qft, which is generally considered too simple to make it into most QFT books, which start with the relativistic theory. But it seems to me best to first understand the non-relativistic theory, where anti-particles decouple, making the whole story much more straightforward.

The earlier version I had posted had a partial chapter 7, including something I'd started writing on propagators. When I started adding more and gave a lecture, I realized that I was doing things in a confused and overly complicated way. Normally I figure this out while preparing a lecture, this time I didn't realize it until I was actually in the middle of the lecture (and realized that an argument had gone in a circle and come back to essentially the same starting point…). Apologies to the students for putting them through that, but others I hope will benefit because the experience made it possible to write up much clearer notes.

So, no, Padmanbhan not responsible, but preparing and delivering a lecture was, as always in teaching, very educational for me.

10. **Art**
February 24, 2024

Got it, thanks. I'm looking forward to the reappearance of Chapters 9 and 10 in a future version.

11. **Peter Woit**

February 25, 2024

Art,
Forgot to include those in this latest version. Will be getting to that part of the course later in the week and be rewriting them, new version should appear in a week or two.

12. **Michael Weiss**
February 27, 2024

Peter, you have previously commented on exciting-sounding analogies between bench-top experiments and black holes, including the information-loss paradox. Yet Nature Reviews Physics is again highlighting these analogies as sources of profound insights; see https://www.nature.com/articles/s42254-023-00630-y.epdf?sharing_token=zj4iqWa8yEwZ-i4Fl6fAitRgN0jAjWel9jnR3ZoTv0Nz0uC7Et1O9sYRTMTUg9QS9YGeXjxcQPTc-Ho86x7uS-7F0WJfvtsbRyG4MuSBzadIgdFFCqIuydBu_jceodTM88QhNAtDbDJSby0DN9GCTpPIuYpnXNwxYECfp0qBwBg%3D. Is this work significant because new insights are obtained into otherwise intractable systems, or is it because the analogies simply sound cool? Sorry for what may be naive questions.

13. **Peter Woit**
February 27, 2024

Michael Weiss,
This sort of thing has never made any sense to me. There's something obviously absurd on its face about claiming "I'm going to study motion of a fluid in a lab, and learn about quantized space-time degrees of freedom." I don't see how it's any different than me saying "I'm going to test my quantum gravity theory in which some variable satisfies a quadratic equation with solution a parabola by going outside and throwing a ball whose trajectory should follow the same parabola."

This Nature paper is somewhat less nonsensical than the Nature wormhole fiasco paper, and at least these authors didn't hold a press conference and convince Quanta to make a video about it.

14. **Rama**
March 3, 2024

https://www.youtube.com/watch?v=Pcd911qSTBk
Cosmological constants – Part 1 (Thanu Padmanabhan)
Oxford -Cambridge collaboration
https://www.youtube.com/watch?v=sPkspt1XrKQ
Cosmological constants – Part 2 (Thanu Padmanabhan)

Three Items

February 27, 2024
Categories: Uncategorized

Kind of like the last posting, but this time you get two worthwhile items to make up for one that's not.

- Dan Garisto has a very good article here examining the present state of high energy experimental particle physics and phenomenology. He also summarizes current thoughts about the future. The CERN FCC-ee proposal is still in feasibility study mode, with the big problem its high cost. Numbers like \$15 – \$20 billion have shown up in press reports, and Garisto has "tens of billions". The feasibility study is supposed to be finished late next year, and presumably a big part of it is people at CERN crunching numbers trying to figure out some plausible way this could work financially.
- There's a very good article at Quanta about efforts to put together version 3 of the very influential "Kirby List" of open problems in topology. For version 1, see here, version 2 here, and as far as I can tell, version 3 still not finished (but discussed here).
- Brian Keating has a video asking "What would It take for String Theory to move beyond the realm of pure math, Into verifiable territory"? The answer obviously is a conventional substantive scientific prediction of anything. By describing the problem with string theory as being that it is now in "the realm of pure math", I think Keating is missing the point. The problem with "string theory" is not that it's pure math, but that it's not a theory. "String theory" is now a 50 year old set of failed hopes and dreams that a theory might exist, see for instance Theorists Without a Theory. Every so often I try to figure out what people still pursuing this are up to, most recently today taking a quick look at KITP talks by Liam McAllister here and here.

  The problem shows up clearly at 10:23 of the first talk where he's talking about his goal, which gives up on studying all or even representative string theory solutions and tries just to find any "valid solutions". But what is a "valid solution"?

  > And by valid solutions, we should say what equations are we trying to solve? And, it's not the case that we can currently think about finding cosmological solutions of the exact theory in any non-perturbative sense.

  McAllister artfully avoids saying what everyone at the talk knows (after all, the talk is part of a program entitled "What is String Theory?"): you can't look for solutions to the theory because there is no theory. More specifically, no "exact" theory, just a long list of possible theories that one hopes might be in some sense approximations to a real theory. He then goes on to specify the extremely complex approximate theory he wants to work in, chosen by the "look under the lamppost" method as something you could actually imagine calculating. Whenever I look at things of this kind I'm completely mystified why anyone thinks it make sense to embark on insanely complicated calculations like these with essentially zero credible scientific motivation. Somehow though, he has a whole group of people doing this. By the end of the second talk he's giving his vision of the future, which features an old photo of a room of hundreds of men in suits and ties calculating with pencil, paper and slide rules.

  I was mystified twenty years ago why anyone thought this was a good idea, and the whole thing has just gotten stranger and stranger…

Comments

1. **Sabine**
   February 28, 2024

   The FCC cost estimate numbers I have seen or heard is something like USD 12 billion for the first stage, USD 20 billion for the first & second, and the "tens of billions" that includes the operation cost which is (conservatively) another billion per year. Even if these numbers are somewhat off, I think it's realistic that in total it'll be $40 billion at least.

2. **Sabine**
   February 28, 2024

   I find it interesting how graphic designers can't seem to resist the temptation to arrange the standard model particles in some sort of symmetrical configuration even if that doesn't make any sense. The infographic in the MIT Tech Review article is another example of that. If you Google it, you'll find many more examples. All with symmetries that have nothing to do with the physics.

3. **Sabine**
   February 28, 2024

   Everyone who thinks that SUSY "explains" the Higgs mass misunderstands what it means to "explain" something in scientific terms. The Higgs mass is a free parameter with and without SUSY. It's about time this naturalness nonsense stops.

4. **suomynona**
   February 28, 2024

   @Sabine:

   If you want a visualization of the Standard Model particles that makes an attempt to group things according to their actual physical properties, see this one:
   https://www.quantamagazine.org/a-new-map-of-the-standard-model-of-particle-physics-20201022/

5. **Mitchell Porter**
   February 28, 2024

   Peter, I assume you're aware of the recent use of machine learning to estimate physical yukawa couplings in various string compactifications, using simpler ones for which analytical results exist, as a way to benchmark the AI estimates? e.g. https://arxiv.org/abs/2402.01615

6. **Peter Woit**
   February 28, 2024

   Mitchell Porter,
   Doesn't matter how you do these calculations, they're still meaningless.
   Since people are throwing their money at AI, I assume this is both a great way to attract funding, as well as generate papers with minimal effort.

7. **Sabine**
   February 28, 2024

@suomynona sorry I don't know what you mean, the ones I see on that page make as little sense as all others I have seen

8. **David Yager**
February 28, 2024

Hi Peter,
Any plans for the eclipse next month? Going to this one won't involve an exotic trip around the world – although some parts of Ohio may seem like a third world country when traveling from Columbia.

9. **Peter Woit**
February 28, 2024

David Yager,
Will definitely go to somewhere on the eclipse track. I'm canceling my class for that day, and a couple days before will head up to Boston first then Burlington area day of eclipse if weather there looks good. Otherwise will head west looking for clearer skies. My 93 year old mother is very enthusiastic about coming along.

10. **maxstroke**
February 28, 2024

@Sabine: Are there any good infographics out there, to your mind? Or is the point that there is no "natural" way of grouping the standard model particles in a visual representation? If that's the case, then it seems any arbitrary viz would be as good (or bad) as any other… Blindfolded people touching the elephant and all that…

11. **Peter Woit**
February 28, 2024

If people are going to discuss this, I'd like to advertise the graphic for SM fermions from my book, see
https://www.math.columbia.edu/~woit/fermions.pdf

12. **Zachary Yezek**
February 28, 2024

Is anybody even still trying to turn the conjectured M-Theory (or its successors) into an actual concrete theory with specific principles & equations?

To use General Relativity as an analogy, if string theorists don't have the Einstein tensor or Field Equations yet do they at least know that the equation they're looking for will relate matter & energy distributions to warped space-time geometries? Do they at least know they need a tensor equation, or that it must satisfy the strong equivalence principle?

I ask because it isn't clear that even the "string" theorists are still working that problem. About the closest thing to it that I'm aware of is that "Swampland" project trying to find non-trivial consistency conditions that all the 'string vacua' must honor. Sort of meta-conditions on any such equation and its solution space. But even that effort appears to have produced little of substance, beyond the vaguest possibility that a positive cosmological constant is incompatible with string theory.

13. **Sabine**
February 29, 2024

@maxstroke I've never seen one that made sense. I thought about this a lot for my first book and came up with something that made sense (at least it's what I thought) but it was so complicated I realized it wouldn't do any good. I have my own infographic (it's the one I keep using in my videos) but it's omitting a lot of information about the gauge groups. Though at least I do have 8 gluons.

Just to give you an idea it was kind of similar to this:

https://static.scientificamerican.com/blogs/cache/file/65C0B45B-606B-44AF-83F80189256EB5D6_source.jpg?w=590&h=800&29A6937B-1463-45E9-823B3D940BC2F532

Kinda messy, but I think it's actually a pretty good summary of the SM Lagrangian. (Leaving aside that it has only 1 gluon…)

My point is just that humans seem to have a tendency to want things to be nicely symmetric and sorted and fit into easily understandable categories, and the standard model just isn't to their liking. I understand of course why graphic designers want pretty graphics, but I'm not at all sure that the message these graphics convey is helpful.

14. **Peter Woit**
February 29, 2024

Zachary Yezek,
As far as I can tell, no one has ever had a viable idea about M-theory of this kind. In particular, if you look at Witten's papers from the 90s, this is something he was very much looking for. But at some point he seems to have given up.

One goal of the KITP "What is String Theory?" program seemed to be to attract interest to this problem, but looking at the talks, I see no one even trying to address it.

15. **tulpoeid**
February 29, 2024

Twenty years ago I attended a theory summer school where a very personable professor would make remarks such as (while arriving at the solution of a long example on the whiteboard) "and now we'll do what string theorists have not been able to do for twenty years, we'll write down the lagrangian".
I wonder if there has been any progress with that.

16. **Peter Woit**
February 29, 2024

tulpoeid,
No.

17. **anon**
February 29, 2024

The thing I like most about the Association for Mathematical Research, the new politically-neutral alternative to the American Mathematical Society, is that it has the most

comprehensive list of problem lists I have ever seen:
https://amathr.org/problems/

18. **David Roberts**
February 29, 2024

@Zachary

I don't follow the research field actively, but given the proximity of the work of my one-time coauthor Urs Schreiber to my own interests in geometry, I watch what he is doing with Hisham Sati. They are slowly chipping away at a very principled approach to finding core principles of M-theory using contemporary mathematical tools and showing how scattered observations derived in special cases are consequences of them. It's not throwing up ones hands and blaming the swampland, but trying to get to a point of rigorously proving things posited in the 90s (like, for example, Witten's idea that brane charges are quantised in K-theory) from a central underlying principle.

I'm not sold on string/M-theory as The One True Theory, but from the point of view of mathematical physics it's much better than all the alternatives I see Peter pointing to. The fact it requires heavy formalism from mathematics developed in the past 20 years and even the past 5-10 years means that I suspect a lot of people in the physics world prefer to stick to claiming an mathematical model of a quantum system on a chip using linear algebra is really telling us about real-world cosmological wormholes, since it's a lot less effort to understand and explain, especially to the popular press.

19. **Peter Woit**
March 1, 2024

David Roberts,
The problem with Sati/Schreiber is that the answer to "what is M-theory" that they are pursuing has no connection to actual physics. The string theorists gathered at the KITP to discuss "what is string theory?" don't think of what Sati/Schreiber are doing as relevant to the question they are asking. For most physicists, the math apparatus Sati/Schreiber are using is a barrier to them getting interested, but there are physicists (Witten most notably) who can follow this and they show no signs of interest, exactly because there's no connection to physics.

This kind of thing has always been to me the main problem with string theory, more than the lack of connection to experiment. When string theory research finds a direction that is mathematically fruitful (take mirror symmetry as an example), it is a direction orthogonal to any connection to physics. When string theory research finds a direction that tries to connect to physics (any "string phenomenology"), it is mathematically hideous, orthogonal to interesting mathematics. This has always seemed to me conclusive evidence that whatever "string theory" is, it's a wrong idea about unification, that a right idea will not be orthogonal to good mathematics.

20. **Maximillian**
March 6, 2024

I can't get over the picture of hundreds of people in suits and ties in a room calculating with slide rules. I laughed so hard I cried.

21. **David Roberts**
March 7, 2024

@Peter

apologies for the slow reply, but what I posted was in response to the question by Zachary

> Is anybody even still trying to turn the conjectured M-Theory (or its successors) into an actual concrete theory with specific principles & equations?

Regardless if one thinks that M-Theory is even a theory with connection to physics or not, one can attempt to build a more principled theory that is based on some specific principles, as opposed to a bunch of ad-hoc conjectures and suppositions. It is a stretch to my imagination deny that Schreiber and Sati are trying to "turn the conjectured M-Theory into an actual concrete theory with specific principles". This is their literal stated goal. Whether these principles are physical or not is a separate question. They are certainly building up a mathematical theory and proving it gives rise to effects conjectured in the literature.

I repeat also: I'm not sold on it being a physical theory either (with the caveat that I'm not a physicist), but I certainly admire the intention to sort out the mathematics that lies behind a bunch of things in the M-theory literature.

(All this comes of course with the usual disclosures that I know Urs reasonably well, and I admire the mathematics he works on.)

A New Approach to Modularity

February 28, 2024
Categories: Langlands

I was assuming that Peter Scholze's [Emmy Noether lectures] at the IAS would be the big news about advances in the Langlands program this coming week, but an anonymous correspondent just sent me [this link]. Tomorrow Andrew Wiles will be giving a talk in Oxford on "A New Approach to Modularity", with abstract:

> In the 1960's Langlands proposed a generalisation of Class Field Theory. I will review this and describe a new approach using the trace formua as well as some analytic arguments reminiscent of those used in the classical case. In more concrete terms the problem is to prove general modularity theorems, and I will explain the progress I have made on this problem.

I'm curious to hear from anyone who knows what this is about or can report tomorrow after the talk. Wiles does have a certain track record of unveiling unexpected huge progress in a talk like this…

Update: Well, the talk should be over now. Sure someone who was there can let us know what happened?

Update: There's a [tweet with some photos].

Update: The [first of Scholze's talks] is available at the IAS youtube channel.

Comments

1. **GG**
   February 29, 2024

   I was at the seminar but I am reluctant to comment as I am not a number theorist. To my understanding, Wiles showed that proving the 2006 Michel-Venketash mixing conjecture would imply modularity. I can provide the photographs of the whiteboard if interested.

2. **Peter Woit**
   February 29, 2024

   GG,
   Thanks! That's consistent with third-hand reports I've heard from another source, that this is about new cases of modularity, but conditional on cases of the mixing conjecture. Better I not try and provide a garbled fourth-hand version, I hope someone else who was there can provide more details, ideally a set of notes. Not clear to me if my hosting photographs of a talk would be a good idea if the speaker/sponsoring institution don't approve.

3. **David Roberts**
   February 29, 2024

   FWIW, the comment on the first photo in the Twitter post saying "proved by" is I think referring to the Annals paper [*Joint equidistribution of CM points*] by Ilya Khayutin. This paper proves a particular special case of the Mixing Conjecture over the rationals.

4. **Z Y**
   March 4, 2024

   Isn't the mixing conjecture already proved if we assume GRH? That would make for a very interesting turn of events

Spring Course Notes

March 3, 2024
Categories: Uncategorized

This spring I've been teaching a course aimed at math graduate students, starting with quantum mechanics and trying to get to an explanation of the Standard Model by the end of the semester. Course notes for the first half of the course are available here, but still quite preliminary, in particular I need to do a lot of work on section 9.4 and add material to chapter 10. Hoping to get to this tomorrow.

There won't be much progress on the notes for the next couple weeks. This coming week I'm hoping to spend some time trying to understand Peter Scholze's IAS lectures, will go down to the IAS on Tuesday. On Thursday I'm heading out on a spring break vacation to the Arizona-Utah desert.

Perhaps a good way to think about these notes is that they're both aimed more at mathematicians than physicists (although I hope accessible to many physicists) and also designed more to supplement than to replace the discussions in the standard physics texts. So, a lot of the standard material is not there, since it's well-covered elsewhere, but there are a lot of topics covered that usually aren't.

One unusual aspect of the notes is that I spend a lot of time trying to explain non-relativistic quantum field theory, since that seems to me to be a better starting point than immediately diving into the relativistic case. I'd be curious to know if anyone can point me to a good discussion of the path integral formalism for non-relativistic quantum field theory, which is something I haven't found. This is one reason it's taking a while to finish writing up my own version.

Also original here I think is a careful discussion of the real forms of spinors and twistors. This in some sense is background for the new ideas about "spacetime is right-handed" which I've been working on. Nothing in the notes now about the new ideas, but I hope the explanation of the conventional story in these notes is useful.

Comments

1. **Jack**
   March 4, 2024

   Hey Peter,

   From the Physics side of things, there are some recommended texts that discuss the path-integral for non-relativistic QFT. In my experience, they are usually associated with advanced statistical mechanics and condensed matter. Not sure if this is what you are looking for, but a couple of standard choices are:

   – QFT in Condensed Matter Physics, Naoto Nagaosa
   – Quantum Many-Particle Systems, John W. Negele and Henri Orland

   All the best

2. **Adam**
   March 4, 2024

   The path integral for non-relativistic quantum fields (the so-called coherent state path integral) is everywhere in condensed matter theory. Two few textbooks, just off the top of my mind:
   Altland and Simons, "Condensed Matter Field Theory", Chapter 4
   Dupuis, "Field Theory of Condensed Matter and Ultracold Gases", Chapter 1 (that chapter is accessible for free here
   https://www.worldscientific.com/doi/epdf/10.1142/9781800613911_0001)

   This kind of path integrals can be generalized to many Lie algebras (su(2), su(1,1), etc), see e.g. Perelomov "Generalized coherent states and their applications".

3. **David Brown**
   March 4, 2024

   Rosenfelder, Roland. "Path integrals in quantum physics." arXiv preprint arXiv:1209.1315 (2012).
   https://arxiv.org/abs/1209.1315
   Rosenfelder, R., 1994. Particles and shadows: a generalized path-integral approach to non-relativistic field theories. Journal of Physics A: Mathematical and General, 27 (10), p.3523.
   https://iopscience.iop.org/article/10.1088/0305-4470/27/10/027/meta

4. **Peter Woit**
   March 4, 2024

   Thanks for the references!

   Closest to what I've been looking for are section 2.2 of Nagaosa and section 1.4 of Dupuis. These give an imaginary time path integral of the right sort, but do it for finite imaginary time, to do a finite temperature calculation. Not seeing a discussion of zero temperature and the relation via analytic continuation to the real time Wightman function.

   Yes, one can think of these as "coherent state path integrals", as well as "phase space path integrals". I wrote something very long ago about the problems with these, see https://www.math.columbia.edu/~woit/tqtandrepthy.pdf
   At the time I got interested in this when thinking about the Chern-Simons path integral. If you look at something very simple with the same structure (path integral over paths on a sphere as phase space) you see that there are very serious problems making any sense of such things. A fundamental aspect of quantization is that you can't just go directly from a symplectic manifold to a quantum system, preserving the symplectic invariance (the infinite-dimensional group of symplectomorphisms). There's something fundamentally conceptually wrong with the phase space/coherent state path integral, because it appears to do this. For the thing to make sense you need somewhere to smuggle in a polarization or somehow break the symplectic invariance.

   When your integral is just a Gaussian, then you can formally do the exact integral and get something sensible, at least in imaginary time. There's something interesting going on here which I don't fully understand. The formal "derivations" of a coherent state path integral though aren't really helpful, since they have obvious problems (you're "integrating" over "paths" with no continuity property, with formulas involving their derivatives).

5. **Adam**

March 4, 2024

Peter,

It is possible to do some calculations (in imaginary time) even if the action is not Gaussian. In the case of a quartic term, one can use a Hubbard-Stratonoich (HS) transformation to render the integral gaussian, integrate out the field, and then HS field. Because, as you remark, neither the original field nor the HS field are continuous, one needs to be extra careful. However, using insight from Ito calculus (the HS field is effectively a weight noise), one can recover the exact result, see Sec. II of https://arxiv.org/abs/1906.02571.

I've become interested in this problem when trying to understand how to (properly) perform non-linear change of variables in such integrals. Contrary to standard Feynman path integrals, here it is much more subtle because the field is not continuous (in the Feynman case, it is continuous but not differentiable, which allows for controlling the derivatives of new variables, however with the introduction of new "quantum potential" terms). No such luck here however. As far as I know, it is still an open problem.

6. **Peter Woit**
March 4, 2024

Adam,
Thanks for the comment and reference. These seem to be consistent with my understanding, that for configuration space path integrals you have continuity and if careful can do transformations on the the fields with results consistent with usual quantization. But for phase space or coherent state path integrals, no continuity, and (beyond linear transformations), the "integral" doesn't at all have sensible properties under change of variables.

7. **Adam**
March 4, 2024

Peter,
Are you saying that such non-linear changes of variables would be completely impossible? Would you have a reference that would substantiate that?
If it is in the reference you gave in a comment above, could you explain the argument for someone not really knowledgeable in CS TFT?

8. **Peter Woit**
March 4, 2024

Adam,
I haven't thought much about this in a very long time, and even then, I was focused on a different problem: how do you make sense of path integrals over paths in G/T (G compact, T maximal torus) and recover the usual quantization story there, where it's the representation theory of G?

There is a no-go theorem, the Groenewold-van Hove theorem. Because of that theorem, something must go wrong if you do non-linear symplectic transformations on the variables, in the sense that such transformations can't correspond to unitary transformations on the states. A different aspect of this is that, in addition to a phase space you need a polarization on that phase space to get a quantization, and the quantization depends subtly on the polarization. What I'm wondering about phase space path integrals is where the polarization choice comes in.

Again, a problem for me here is that I haven't thought seriously about this in a long time.

9. **Hansi**
March 4, 2024

For the path integral, i would suggest
https://link.springer.com/book/10.1007/978-3-540-76956-9

Mathematical Theory of Feynman Path Integrals
An Introduction
Authors: Sergio A. Albeverio , Raphael J. Høegh-Krohn , Sonia Mazzucchi
Part of the book series: Lecture Notes in Mathematics (LNM, volume 523)

That is what a math student would go for. It starts with non-relativistic path integrals and then goes over to quantum field theory, but all in theorem proof style. Some of the authors work at Bonn university.

10. **Micha**
March 5, 2024

Hi Peter,

there are some path integral books by Kleinert. I'm not so sure how close it is to what you are looking for. Here some version from his homepage.
https://hagenkleinert.de/documents/pi/HagenKleinert_PathIntegrals.pdf

11. **Arnold Neumaier**
March 6, 2024

Peter,
you wrote, "There's something fundamentally conceptually wrong with the phase space/coherent state path integral, because it appears to do this. For the thing to make sense you need somewhere to smuggle in a polarization or somehow break the symplectic invariance."

But you dind't notice that this is already smuggled in in the coherent state integral, since (as especially John Klauder emphasized repreatedly) a coherent state provides not only a symplectic structure but also a compatible metric structure, which is just a way to define the required polartization.

12. **Peter Woit**
March 6, 2024

Arnold Neumaier,
When you naively write down the coherent state path integral, looking at the measure and action, only difference with the phase space integral is the boundary conditions. Like in the phase space path integral, choice of polarization appears just in the boundary conditions.

It's been a long time since I looked at Klauder's attempts to define coherent state path integrals. I guess you can try and use the Kahler structure polarization to do this. But after spending a lot of time looking at this kind of thing years ago, I never found anything that seemed convincing.

13. **Anon**
March 6, 2024

I'm just a student, but I like the following books on path integrals:
1)G.Roepstorff- Path Integral Approach to Quantum Physics (Grad level mathematical physics book);
2)L.S. Schulman- Techniques and Applications of Path Integration (Undergrad-Grad level theoretical physics book)

14. **Peter Woit**
March 7, 2024

Anon,
Nothing in the Roepstorff book about phase space/coherent state path integrals, but it has an excellent detailed treatment of Euclidean quantum fields. The Schulman book near the end has a chapter on the phase space path integral, carefully explaining the problems with it.

15. **PS**
March 8, 2024

Hi Peter,

you might find a series of papers by Hashimoto et al useful, for example "Borel-Weil theory and Feynman path integrals on flag manifolds" (https://projecteuclid.org/journals/hiroshima-mathematical-journal/volume-23/issue-2/Borel-Weil-theory-and-Feynman-path-integrals-on-flag-manifolds/10.32917/hmj/1206128252.full) or "The Borel-Weil theorem and the Feynman path integral" (https://mathweb.tifr.res.in/sites/default/files/publications/studies/SM_13-Geometry-and-Analysis.pdf#page=325). Building on earlier work of Alekseev, Fadeev, and Shatashvili, they describe how to perform path integral quantization of coadjoint orbits the form G/T and, for the linear Hamiltonians corresponding to Lie group generators, how to regularize the path integral to obtain the usual representation theory of G. (I will admit I haven't fully gone through these papers, so I'm not sure how correct they are.)

16. **Peter Woit**
March 10, 2024

PS,
I've looked a lot at this kind of thing, but I don't think they're really "path integrals" in any useful sense. They depend crucially on exactly how you've chosen to discretize the "path" (which is an infinite set of points in a space, with no continuity properties). If you can't change variables, is this really an integral in any coordinate-invariant sense?

17. **PS**
March 11, 2024

I see. So your issue is with the phase space path integral in general. I guess from geometric quantization, it's not a surprise that one can only give a simple definition of the phase space path integral for classical observables whose flow preserves the polarization, and perhaps has to resort to a BKS-type construction for more general observables. The fact that the coordinate space path integral is invariant under general coordinate transformations would correspond to the fact that the cotangent lift of such transformations preserve the vertical polarization. But I have to admit, I've never given it much thought (I don't use path integrals much).

I've found one paper that discusses this: "Symplectic structures, BKS kernels and path integrals of BRST bosonic strings" (https://link.springer.com/article/10.1007/BF01550943).

18. **Arnold Neumaier**
March 12, 2024

Peter,

"It's been a long time since I looked at Klauder's attempts to define coherent state path integrals"

See:

J. R. Klauder and I. Daubechies,Quantum mechanical path integrals with Wiener measures for all polynomial Hamiltonians, Phys. Rev. Lett. 52 (1984), 1161.

I. Daubechies and J. R. Klauder, Quantum mechanical path integrals with Wiener measure for all polynomial Hamiltonians. II. J. Math. Phys. 26 (1985), 2239.

20 Years of Not Even Wrong

March 18, 2024
Categories: Uncategorized

The first entry on this blog was 20 years ago yesterday, first substantive one was 20 years ago tomorrow (first one that drew attacks on me as an incompetent was two days later). Back when I started this up, blogging was all the rage, and lots of other blogs about fundamental physics were starting around the same time. Almost all of these have gone dormant, with Sabine Hossenfelder's Backreaction one notable exception. She and some others (like Sean Carroll) have largely moved to video, which seems to be the thing to do to communicate with as many people as possible. There are people who do "micro-blogging" on Twitter, with the descendant of Lubos Motl's blog StringKing42069 on Twitter. I remain mystified why anyone thinks it's a good idea to discuss complex issues of theoretical physics in the Twitter format, flooded with all sorts of random stupidity.

Looking back on what I was writing 20 years ago it seems to me to have held up well, and there is very little that I would change. The LHC experiments have told us that the Standard Model Higgs is there, and that supersymmetry is not, but these were always seen as the most likely results.

My point of view on things has changed since then, especially in recent years. When I started the blog I was 20 years past my Ph.D., in the middle of some sort of an odd career. Today I'm 66, 40 years past the Ph.D., much closer to the end of a career and a life than to a beginning. In 2004 I was looking at nearly twenty years of domination of fundamental theory by a speculative idea that to me had never looked promising and by then was clearly a failure. 20 years later this story has become highly disturbing. The refusal to admit failure and move on has to a large degree killed off the field as a serious science.

The technical difficulties involved in reaching higher energy scales at this point makes it all too likely that I'm not going to see any significant new data about what the world looks like above the TeV scale during my lifetime. Without experiment to keep it honest, fundamental theory has seriously gone off the rails in a way which looks to me irreparable. With the Standard Model so extremely successful and no hints from experiment about how to improve it, it's now been about 50 years that this has been a subject in which it is very difficult to make progress. I've always been an admitted elitist: in the face of a really hard problem, only a very talented person trained as well as possible and surrounded by the right intellectual environment is likely to be able to get somewhere.

My background has been at the elite institutions that are supposed to be providing this kind of training and working environment. Harvard and Princeton gave me this sort of training in 1975-1984 and I think did a good job of it at the time, but from what I can tell things are now quite different. 40 years of training generations of students in a failed research program has taken its toll on the subject. I remember well what it was like to be an ambitious student at these places, determined to get as quickly as possible to the frontiers of knowledge, which in those times meant learning gauge field theory. These days it unfortunately means putting a lot of effort into reading Polchinski, and becoming expert in the technology of failed ideas.

One recent incident that destroyed my remaining hopes for the institutions I had always still had some faith in was the program discussed here, which made me physically ill. It made it completely clear that the leaders of this subject will never admit what has happened, no matter how bad it gets. Also having a lot of impact on me was the Wormhole Publicity Stunt, which showed that the problem is not just refusing to face up to the past, but willingness to sign onto an awful view of the future, as long as it brings in funding and can be sold as vindication of the past. Watching the director of the IAS explain that this was comparable to the 1919 experimental evidence for GR surely made more than a few of those in attendance at least queasy. This particular stunt may have jumped the shark, but what's likely coming next looks no better (replace quantum computing with AI).

The strange thing is that while the wider world and the subject I care most about have been descending into an ever more depressing environment of tribalistic behavior and intellectual collapse, on a personal level things are going very well. In particular I'm ever more optimistic about some new ideas and enjoying trying to make progress with them, seeing several promising directions. Whatever years I have available to think about these things are looking like they should be intellectually rewarding ones. Locally, I'm looking forward to what the next twenty years will bring (if I make it through them…), while on a larger scale I'm dreading seeing what will happen.

Update: For a place with extensive comments about this blog posting, see Hacker News.

Comments

1. **Oisin McGuinness**
   March 18, 2024

   Hi Peter,

   Enormous Congratulations on the 20th Anniversary!
   I've enjoyed the ride tremendously and looking for what's new in NEW is always a highlight of the day.
   Maybe I should stop harassing you to write about Lean… 🙂

   Best wishes for 20 more, or as long as you want!

2. **John Horgan**
   March 18, 2024

   Peter, I'm glad you've hung in there, you're invaluable for calling out hype and bullshit, as with the wormhole-in-a-quantum-computer story. And I'm glad you've found meaning in your own work, even if the world seems to be falling apart. Good luck with it.

3. **John SIrois**
   March 18, 2024

   Thank you Peter. I've been reading for all 20 years and I appreciate that you've kept the format, content and tone consistent over all this time. I learn alot here.

4. **Douglas Natelson**
   March 18, 2024

   Congratulations on 20 years! It's been enjoyable to read your writing over the years, and it's been an inspiration to continue my own blog, even though we're in some post-blogging era now. (Writing on medium or substack may represent some resurgence of long-form blogging, but both platforms have issues.)

5. **Ryan Snyder**
   March 18, 2024

   Thank you for your work, Peter. I only learned about you a couple years ago after wondering what the physics world was up to after having left it behind when I earned my Ph.D. I'm glad there are people like you out there. You've taught me a lot. Please keep writing.

6. **S**
March 19, 2024

Happy twentieth anniversary, Peter! You've created one of the most important blogs in physics, and even in mathematics (surprisingly enough). Your staying resolutely on-point through vast waves of criticism has surely contributed to the important things this blog has achieved — which it certainly has. Thank you for your work, and may it prosper through many more years!

7. **B. Malpani**
March 19, 2024

Congrats on the milestone! Although it is indeed regrettable that such a blog is still necessary.

"[Sabine Hossenfelder] and some others (like Sean Carroll) have largely moved to video, which seems to be the thing to do to communicate with as many people as possible."

Well, that's an example of the world getting worse, imo: a long time ago, the web used to be full of things to read. It's become a place of things to watch but for many things videos can never replace the written word.

8. **Aswin Mannepalli**
March 19, 2024

Dear Peter,

I don't meant to be melodramatic but your blog was one of the constants in my life. Ever since I did a physics summer program for high school students at Princeton, we little "rebels" would read your blog. When Nova was hawking d-branes, you kept us grounded. When people were fighting pointless wars over black holes, we turned to you for truth.

Thank you so much.

One last request – can you write a book about your human journey? Perhaps less technical than your last, but I would find it a beautiful testament to the life you lived – and the hope you give to others.

9. **Evgenii Rudnyi**
March 19, 2024

Thank you for your blog and your book. I have read it after Green's The Elegant Universe. It was very good for me at that time to read your book.

Good luck!

10. **Tom WEIDIG**
March 19, 2024

Congratulations from Luxembourg!

But StringKing is not Lubos, or is he?

I actually miss Lubos' blog… it was never dull and always interesting in some way.

11. **Anonymous**
March 19, 2024

Hello and congratulations on these 20 years!

I am not a physicist, I can assure you that the conversations around string theory outside the circle of fundamental physics have changed greatly in the last 20 years.

Nowadays in the world of science, everyone knows, more or less, that string theory has never given anything concrete (and is not at all close to doing so) and, although it is intellectually an interesting construction, it is not a physical theory and I hardly know any scientist who takes it seriously. This is even more visible with the younger generations.

12. **Sabine**
March 19, 2024

Happy anniversary! Your blog is one of the few I am still reading. Also Scott Aaronson is still around.

Google hasn't updated the blogger interface for a decade or so. They'll either have to entirely revamp it or phase it out at some point in the not-so-far future. This is why I have moved my written content to substack, in case anyone is interested https://sciencewtg.substack.com/

(Though I have someone else making the Substack posts on my behalf, the content comes from me.)

13. **Handle**
March 19, 2024

Congratulations and thanks! I've learned a tremendous amount from you over the years, and I'm grateful for it. Keep it up!

14. **Jian**
March 19, 2024

Congratulations on 20 years! I have learned a lot from your blogs.

15. **Low Math, Meely Interacting**
March 19, 2024

Nice reminder of how old I am, Dr. Woit.

It's shocking to realize how long I've been reading this blog. Almost as remarkable is how consistently well written and interesting it has been throughout its 20 years of existence.

Thank you, keep up the excellent work, and congratulations!

LMMI

16. **Jens**
March 19, 2024

Congratulations and thank you for all these years of thoughtful commentary!!! Your blog has been one of the best sources on the state of high energy physics, and I've enjoyed it very much. Your willingness to engage constructively with your critics is impressive and a great example of how intellectual discourse should be conducted.

17. **Scott Locklin**
March 19, 2024

Congrats on your anniversary, and thanks for saving all those kids from the noodle theory meat grinder. It was a child who pointed out the Emperor's clothes; that sort of childlike stating of the obvious (the true spirit of science) is a rare quality among adults these days. Being the first man to stand up and duke it out with all those "wicked smaht' yoyos shows great character.

18. **Chris Oakley**
March 19, 2024

Often controversial, but always well written and always worth reading. Congratulations on 20(+) years of service to the scientific community!

19. **QM**
March 19, 2024

Thank you for all of your efforts to hold a mirror up to science. It's only good if it passes close scrutiny.

20. **Matt Grayson**
March 19, 2024

Congratulations! It's been wonderful to see you stick to your rational perspective while so many imposing minds seem to have lost theirs. It's even better to see your research program showing promise and yielding interesting results.

May you have another successful twenty years!

21. **J**
March 19, 2024

Thank you for keeping this blog going for 20 years. I have always enjoyed reading it. The coverage of both theoretical physics and mathematics is great.

22. **AcademicLurker**
March 19, 2024

I discovered your blog some time in 2005 and have been a regular reader ever since. Congratulations on 20 years!

23. **Mark Hillery**
March 19, 2024

Hi Peter,

Congratulations on your 20th anniversary. I don't remember when I started reading your blog, but I found it an interesting window, with a strong point of view, into what was going on in high energy theory. This is a field I had some contact with in graduate school, but lost touch with after that. After reading the blog for a few years I actually ran into you at a New Year's Eve party in Brooklyn. Thanks for the public interest talk you gave at Hunter College, which led my English department colleague, Rebecca Connor, to describe the LHC as "the great white whale of physics," an amusing description that would never have occurred to me. A caution about elitism, though; not all things that are interesting come out of Harvard and Princeton.

24. **Steve Huntsman**
March 19, 2024

You convinced me to leave my two volumes of Green, Schwarz, and Witten (in my defense, acquired in 1996) on the shelf at my last job when I left in 2020. It was a pleasure to meet you about a decade before that and to read this blog since at least 2005. Thanks!

25. **theoreticalminimum**
March 19, 2024

Thank you so much for all that you've offered through your blog. You have done a tremendous service to the community of people out there paying attention (in ways technical and not so) to what you've shared, and I for one am very grateful. Thank you again.

26. **Nilay Kumar**
March 19, 2024

Hey Peter, huge congrats on the 20th! I've always appreciated your principled stance. Here's to many more!

Incidentally, I was going through some files on my old hard drive this weekend and ended up reading notes about linking numbers via Chern-Simons theory that I wrote for your course. Great stuff — hope that course is still going strong!

27. **Steve Esser**
March 19, 2024

Congrats on a great blog from a 20 year reader.

28. **Peter Woit**

March 19, 2024

Mark Hillery,

Thanks!

No intention to imply that Harvard/Princeton (or a small list of similar institutions) are the only place that interesting ideas come from. Such institutions though do have outside influence since many people look to them for leadership, they attract outsize amounts of funding and numbers of talented ambitious young people. At the moment they have outsize responsibility for the sad state of the field.

Thinking of cases where new ideas came from outside such institutions, one local example that came to mind is Lenny Susskind's co-discovery of string theory. His undergrad degree was City College and he did the string theory work while at Yeshiva.
But, maybe that's not the most convincing example…

29. **FA**
March 19, 2024

Congratulations! I have been following your blog since my undergrad days (also almost 20 years ago now).

30. **Ali**
March 19, 2024

Hi Peter,
Congrats on twenty years of chasing your own great white whale! I've been avidly following since the beginning and fondly remember our few conversations when I was an undergrad at Columbia in the 90's. I should have picked your ear more back then in retrospect. I distinctly remember you being much more positive about careers in math than physics. That was of course the right advice…too bad I didn't take it. To me it was clear that something was amiss with string theory back then and was not something I thought worth pursuing (especially as a grad student at Princeton at the tail end of the 90's). Still shocking how so few have spoken out about the abysmal situation over the years. What a shame for the great legacy of theoretical physics, now so badly tarnished. Wishing you another twenty years of blogging and even deeper inquiry.

31. **Wyrd Smythe**
March 19, 2024

Congratulations on 20 years of worthwhile posts! I've been following you for, I think, about 15 of them, and have enjoyed them immensely (bought and very much enjoyed your book, too). I'm an interested amateur fascinated by fundamental physics, and much of it has been over my head, but the learning experience has been wonderful (small steps over time will still get one someplace). Here's to 20 more!

I've noticed a general indulgence in fantasy (science fiction, at best, outright fiction at worst) in our society for many years now. Tragically, it seems even to have infected science: string theory, SUSY, and any multi-dimensional theory are just some examples. Physics seems lost in fantastical musings and wonderings. I'm so weary of "science" articles that center around "maybe", "might", and "could". That's not science to me, that's speculation.

And I regret the decline-to-near-vanishing of long-form blogging. Videos have their place, but the written word, I believe, is vastly superior. Much easier to take the time to chew on a single paragraph or to go back and check a part. And many video editors seem compelled to include lots of barely (or not even) relevant stock footage to illustrate and make the video "more interesting" (read: more distracting and annoying).

But I rant when I should instead just raise a glass to your accomplishments! Congrats again!

(BTW: It was fascinating to go back and read your first posts. The comments were *especially* fascinating!)

32. **Barak Shoshany**
March 19, 2024

Congratulations on the 20th anniversary, Peter! I've been reading your blog for many years, since around 2012, when I was an undergrad. I think it wouldn't be an exaggeration to say that you played a significant role (along with Lee Smolin, Sabine Hossenfelder, and others who put an effort into demystifying string theory) in shaping the direction of my physics career, as well as the way I explain physics to the general public.

33. **Eric Weinstein**
March 19, 2024

Congratulations on the Blog Anniversary Peter.

One thing you did not mention in your post is three developments that are due to you.

First and foremost, the appearance of your book "Quantum Theory, Groups and Representations" was an incredible event. That book joins books like "Geometric Quantization" and "Einstein Manfifolds" and "Spin Geometry" as one of the great works of modern scientific exposition of a topic that had previously been scattered in the literature and all but impossible for most to extract. That book also should have silenced any 'colleagues' who, in desperation, made attacks about your level of knowledge or understanding (no need to name names).

The next two things are the appearance of two of your own ideas about the origin of our fundamental theories. In one you appear to start with SU(4) as a primative input, quotient out by SU(3) x U(1) to get a world with electro-strong field content and then make arguments about Wick rotating between Euclidean and Lorentzian 4-submanifolds hoping to pick up an extra Weak Isospin SU(2) from the Spin(4) structure. While I don't personally think that is right, it does immediately provide a concrete world in which to build natural models and explore their consequences.

The next idea about Space-Time being chiral (which I admit I did not realize was a separate program until we talked) was to think about the oddity of how the Weak force is not only chiral but 'maximally chiral' in that it is not only a question of a complex representation not being equivalent to its conjugate representation, but SU(2) weak isospin being either 'on' or 'off' depending on the spinor being either Left or Right and the fields in question being either 'Matter' or 'Anti-Matter'. And here again you were exploring this spinorial idea based on the sui generis nature of our home dimension n=4 being the only one in which Spin(n) is semi-simple. I believe in a version of this, but that is not relevant here. The point is that you are trying to turn the weak assymmetry into a naturally geometric third 'on-off switch' after Handedness and Matter/Anti-Matter.

I bring these up in their own right, but since this is 'Not Even Wrong' I want to close with how these positive attempts to do new physics interact with the implosion of particle theory.

When you and Smolin wrote your books, I noticed that you were attacked for two different and incompatible reasons. Smolin was misportrayed as writing a self-promotional book to take a cheap shot at the successful community in an attempt to boost his pet theory. You on the other hand were attacked for the opposite reason! “Woit doesn’t have any alternate proposals, so why should he be complaining that there is something wrong?” was the grumbling I heard.

We now know the answer. It doesn’t matter whether you have a theory or do not have a theory of your own. String Theory/M-Theory is, to its faithful DEFINITIONALLY the ‘Only Game In Town’. This is part of a monotheistic religion which insists that there are no other ideas and no critical colleagues. There are ‘only words’ and ‘charlatans, populists and grifters’.

While I don’t agree with your models (yet at least), they are clever and not stupid, and I’m pleased to see you evolve into a colleauge who has proven himself now as a master expositor, and a theorist with your own research program, providing 1-2 other ‘games in town’.

Let me just close by saying that for years, the first place I check when I come back from a trip abroad is this clunky old ‘blog’. Just in case, physics came back to life. Thanks for doing this.

Eric

34. **MKT**
March 19, 2024

Love this comment by Oglop in the Hacker News discussion Peter linked to:

“This blog changed my life. I was on a path of going into research in physics, and I remember somehow finding this blog as an undergrad. It was like I was a soviet reading western news. After another year of courses where I began to notice exactly what the author mentioned, string theorists controlling purse strings and research directions, I gave up and left.”

It was like I was a soviet reading western news…wow.

35. **David Muldowney**
March 19, 2024

Congratulations Peter, a fantastic achievement.
Have been following your blog for 5 years or so with great interest.
Alway love to see a new entry – bizarrely it was one of the things that kept me going during those awful Covid lockdowns!
Had an instinctive allergy to String theory in my postgrad days back in the mid-90s.
To find someone (along with Smolin) who gave such eloquent voice to these doubts has been a great discovery

36. **Scott Aaronson**
March 19, 2024

Happy anniversary, Peter!!

FWIW, your blog was one of the central inspirations for me to start mine, and in particular, to use it to criticize wild quantum computing claims. Thank you.

Whether I share your perspective, have quibbles, or simply don’t know enough to say, I always find you refreshingly honest and well worth reading. Here’s to many more years, and hopefully new discoveries in physics that surprise everyone and render many old debates irrelevant.

37. **Severin Pappadeux**
March 19, 2024

Thank you!

38. **flippiefanus**
March 19, 2024

Congratulations Peter. Thank you for maintaining a blog in this crazy field where sanity prevails. I’ve never been a fan of string theory and it is great to read the perspectives of a somebody closer to the fire than I am.

I remind myself that Newton’s theory of light prevailed for a hundred years before it was replaced by a better description. String theory has not been around for a hundred years yet. I do believe it will eventually be replaced. However, Newton’s theory was replaced by something better. So the way to attack string theory is by doing what you are doing: develop a better theory.

39. **Lei**
March 20, 2024

Congratulation ! and Thanks a lot.

I read your article, subscribed your blog when I was an undergraduate. Now, I become a faculty in university.
Really hope your texts never disappear in its way as it was.

40. **Bruce Bartlett**
March 20, 2024

Congratulations Peter on twenty years of this blog! The blog has been wonderful, and Not Even Wrong (the book) was also a wonderful intellectual contribution. Besides the main feature of your book (holding a critical mirror), the math chapters on topological field theories were extremely well written and I learnt a lot from them.

41. **Mike**
March 20, 2024

I’ve been reading your blog for about 14 years. I really appreciate you sticking your neck out time and again. As someone outside of academia who does not personally know any physicists or mathematicians, this blog is one of the few places that I can learn about current developments (or the lack thereof). Heres to another 20+years of blogging! And lets hope the next 20 years will bring a totally unexpected left field (pun?) discovery that will revitalise fuadamental physics.

42. **Robin Whitty**
March 20, 2024

What are people's highlights from this outstanding 20-year endeavour? One of mine: https://www.math.columbia.edu/~woit/wordpress/?p=11709
Not even string theory: April 2020, Not Even Wrong hosts the heavyweights of arithmeatic geometry sparring over the ABC conjecture proof controversy.

43. **Peter Woit**
March 20, 2024

Robin Whitty,

Yes, on the mathematics side, that was a high point. Thanks go to Peter Scholze for being willing to engage publicly in a detailed technical debate using the comment section here. That was taking place at the height of the Covid lockdown, so it was a bit challenging to moderate that discussion without my usual advantage of having experts down the hall to consult with.

On the physics side, I'd like to point out that the first announcement of the Higgs discovery was not July 4, 2012, but on this blog June 17, 2012, see https://www.math.columbia.edu/~woit/wordpress/?p=4772
In that case, there are also several experts to thank, but they will remain anonymous.

Both cases show that a main reason for some of the best content that appears here is not my writing, but that a large number of expert physicists and mathematicians appreciate what I've been doing over the years and are willing to contribute their own knowledge and expertise to provide material of high-quality.

44. **Christoph**
March 20, 2024

Peter,

thank you for your example. You live in the way that every human should live: honestly and true to yourself.

All the best for your future.
Christoph

45. **Cédric Bardot**
March 20, 2024

Bravo Peter pour votre endurance à maintenir avec rigueur et constance cette chronique personnelle sur la recherche mathématique contemporaine et en particulier sur la physique des interactions fondamentales !
Merci également pour la convivialité et la qualité des échanges de vue dans votre espace des commentaires, fruit des efforts consacrés à le modérer.
Longue vie encore à ce blog et du bonheur pour son auteur.

> Il serait seulement à souhaiter que les géomètres n'eussent pas quelquefois abusé de la facilité qu'ils avaient d'appliquer le calcul à certaines hypothèses. C'est souvent le désir de pouvoir faire usage du calcul, qui les détermine dans le choix des principes ; au lieu qu'ils devraient examiner d'abord les principes en eux-mêmes, sans songer d'avance à les plier de force au calcul. La géométrie, qui ne doit qu'obéir à la physique quand elle se réunit avec elle, lui commande quelquefois. S'il arrive que la question qu'on veut examiner soit trop composée, pour que tous les éléments puissent entrer dans la comparaison analytique qu'on en veut faire, on sépare les plus incommodes, on leur en substitue d'autres, moins gênants, mais aussi moins réels, et l'on est surpris de n'arriver après un travail pénible qu'à un résultat contredit par la nature ; comme si après l'avoir déguisée, tronquée ou altérée, une combinaison purement mécanique pouvait nous la rendre.

Jean Le Rond d'Alembert

46. **Earl DuCaine**
March 20, 2024

Congratulations on 20 years! In its way your post was as important as Hilbert's problems, which outlined a set of mathematical problems he felt were of fundamental importance. Although a few turned out to not be real problems, and a few had solutions that turned out not to be very interesting, the best of them lead to revolutionary changes (arguably the last revolutionary charges so far) in the fundamentals of mathematics and its philosophy.

To stake out the unequivocal claim that string theory will lead to no necessary changes to the standard model and will continue to be nothing but a pernicious intellectual influence on its practitioners a bold, breathtaking pronouncement at the time. It was seminal and points to the grave situation we're in today — that it's surprisingly easy for academic institutions to be captured by an ideology which is 'not even wrong' and it's extraordinarily difficult for them to recover from not-even-wrongness.

47. **Paolo Bertozzini**
March 20, 2024

Dear Peter, my congratulations for the 20 years of "Not Even Wrong" 🙂 This blog, that I have been following from its inception, has been a constant source of critical thoughts on the current developments (and degeneration) in theoretical high energy physics, with inspiring posts on mathematical physics and much more. I would also like to stress not only the "critical" side of the content of the blog, but also the "conceptual" importance of the recent proposals for alternative roads (twistor unification, Euclidean QFT, spinorial geometry) that I find deeply interesting (also in view of some of my current lines of work). I am looking forward to even more exciting developments! Congratulations again 🙂 Paolo

48. **Stephane**
March 21, 2024

Congratulations Peter for the 20 years of your Blog.
What I like best is your effort to be pedagogical and informative.
I learned a lot from your blog.

PS: Michel Talagrand awarded the 2024 Abel Prize – https://abelprize.no/article/2024/michel-talagrand-awarded-2024-abel-prize

https://www.math.columbia.edu/~woit/wordpress/?p=12819

49. **Paul Titze**
March 21, 2024

Hi Peter,

Congratulations in keeping up NEW for so long, I always look forward to reading your posts, keep up the good work.

Cheers, Paul Titze.

50. **Shantanu**
March 21, 2024

Peter, congrats on 20 years of blog.
Once again, you have forgot to mention about neutrino mass which is evidence for Physics beyond standard model.

51. **Joseph Conlon**
March 21, 2024

Congratulations Peter on the anniversary. I have always thought you wrong on substance, but wrong for the best of reasons, i.e. caring about the subject.

52. **Michael Gogins**
March 21, 2024

Thank you for this blog which I have been reading for almost the whole 20 years.

I am not a scientist and I do not have a mathematical education, but I am deeply interested in the physics and associated philosophical implications. Also, the sociological side is also very interesting, perhaps more so, and I think I understand it better.

I'm not qualified to evaluate your own theoretical ideas, but I greatly respect you for insisting on contributing to the discussion, in an even-tempered way, in spite of often being labelled an "outsider" or "not an expert." That says more about your critics than it does about you.

I wish there were more blogs of this kind.

To me, the huge issue here is, who is going to preserve us from highly placed liars?

53. **Art**
March 21, 2024

Let me add my congratulations to the pile. While I treasure your blog and course notes publications, I can't help wondering if they're the best use of your time, now that you have a significant new idea to develop. What balance do you think is optimum?

54. **Alessandro Strumia**
March 22, 2024

Congratulations. Yours is the best physics blog, after that Lubos closed his own. Because you said some truth that nobody wanted to hear about strings, and Lubos did the same about wider academia problems.

55. **Cormac McGuinness**
March 22, 2024

Congratulations Peter on 20 years!

Still very much an avid and constant reader of NEW, having first read your American Scientist article in 2002 (https://doi.org/10.1511/2002.10.110). It has been a thorough and continuing education into the status within the world of HEP of experiment and theory and the ways in which the development of the latter has, in part stalled, due to the concentration on specific speculative theories. This has been of extreme interest to someone far from this field of physics. However shocked or disheartened by the program discussed and linked to above, I think you have reached and continued to inform a generation of physicists about asking the right questions about the approaches that are being followed.

On a lighter, or a darker note if you will!, there is always the total eclipse to look forwards to. I presume you will be visiting some point on the path of totality !? Will it be a shorter road trip for you than 2017 ? (certainly shorter than 2006).

I am sure we will all look forward to hearing about it and any other elements of physics or mathematics that you choose to highlight!

Best wishes for the future and regards from Ireland
Cormac (Mc)

56. **Peter Woit**
March 22, 2024

Art,
Writing posts for the blog doesn't actually take a lot of time. Where I have historically wasted a lot of time is by trying to follow exactly what is going on in string theory and things that have emerged from it. It's clear to me now that any minute spent trying to understand things like what "swampland program" research is, is a minute completely wasted, so I plan to do less of that.

57. **Peter Woit**
March 22, 2024

Cormac McGuinness,

Thanks! Definitely planning to try and see the eclipse soon, bringing along my 93 year old mother. Exact location to be determined only a day or two before the eclipse, based on what the cloud cover projections look like.

58. **Uair01**
March 22, 2024

Thank you very much! I'm just an engineer, but I've followed your blog since your book came out. I can't assess the mathematics, but I think you're right, based on the sociology. I'm afraid that this is the case here: "The market can remain irrational longer than you can remain solvent." But then translated to physics …

59. **John Baez**

March 22, 2024

Sorry to be late to the party. Congratulations on 20 years of your blog, Peter! I'm glad you've been persistently exposing the shortcomings of string theory – a much needed corrective to the hype. I think it really has had an effect. The triumphalism never ends, alas, but it's taken less and less seriously by outsiders.

The really sad part is how little progress we've made on fundamental physics since the 1980s. For anyone out there who wants to see evidence, check out my timeline here , along with the definition of "fundamental physics" that I'm using. Once I realized that this stagnation wasn't going to end anytime soon, I quit fundamental physics (or more precisely, relegated it to the status of a hobby). It really hurt: the dream dies hard, especially after one has spent decades on it. But it hurts more to keep banging ones head on the same old wall! I am not optimistic about any of the new theories of fundamental physics that I see. Thus I'm really glad your blog has expanded to cover more lively and promising endeavors, like the Langlands program.

60. **PeterP**
March 22, 2024

Congratulations, Peter, for this lifetime achievement and keep standing tall under the tremendous pressure from we all know who. Ironically, your name and blog are now forever attached to the string theory. However, we all believe you are on the right side of history here.

On a side note (or not so), I think folks still do not fully understand or grasp your idea and where it could lead that space-time and matter have opposite chiralities. If I may, you might keep elaborating and adding detailed calculations that would follow from it.

61. **Peter Woit**
March 22, 2024

PeterP,
Thanks! I'm working on exactly this now, hope to have something detailed written down by the end of the summer. In the meantime, my latest posting at least points exactly to where I think the missed possibility is in the standard discussion of the Standard Model.

62. **Santo D'Agostino**
March 22, 2024

Congratulations on twenty years of very valuable and appreciated service to the mathematics and physics communities! I have been reading your blog since the early days, and have learned a lot from it.

Continued best wishes!

63. **Andrew Thomas**
March 24, 2024

Congratulations on 20 years of blogging!

I remember fundamental physics seemed so exciting 20 years ago, with all the great blogs and – yes – even the string wars arguments. I don't understand quite why you're still so angry about string theory now as it seems to have faded away. The Strings conference seems to be on its last legs. In fact, everything seems so quiet now – I don't understand it. Where are all the new physics books? We used to get something big every few weeks, now nothing. It was more fun in the old days!

I hope Lubos is OK. I know he was your sworn enemy (well, most people's sworn enemy) but it seems rather sad that he's not around anymore.

64. **Peter Woit**
March 24, 2024

Andrew Thomas,
Unfortunately I don't think fundamental physics was truthfully exciting 20 years ago. It was already in a very bad way, with a huge amount of outrageous hype to the public about "exciting" ideas like the multiverse. Now the hype level has died down and the public is more aware of the problems with string theory, but the situation of actual fundamental theory research is significantly worse than 20 years ago.

What am I angry about? I'm looking back at a 40+ year career that started when the field was very healthy, and watched it be essentially destroyed and killed off. That the people who did this are holding public events assigning themselves grades of "A+++" is just disgraceful, and specifically aimed at making impossible any acknowledgement of what happened and any drawing of lessons for the future.

I also hope Lubos is OK, but I'm pretty sure stopping doing the kind of blogging he was doing was good for him.

65. **Anonymous**
March 24, 2024

"I also hope Lubos is OK,"

I was also concerned about whether Lubos is OK, and checked a little while ago to see if he was doing anything visible on the Internet. He seems to have a quora account that currently seems to be active:

https://www.quora.com/profile/Lubo%C5%A1-Motl

66. **Jim**
March 24, 2024

I was reading Not Even Wrong the book when I was a graduate physics (HEP-TH) student; the university librarian had gotten it surprisingly. 6 years later I left the field with 2 MSc (in my country and the US). I left the field halfway through my PhD after realising what the job prospects were about, what was demanded from me (commitment & huge risk Vs the monetary component) and started doing quantitative analysis and later machine learning.

It's been 9 years now that I do data science, a very rewarding career which also allowed me in my free time to research and publish (in A&A) an article regarding a method that uses ML on GAIA data.

I miss the days when I was innocent (in a sense) and naive (in a good way I'd like to believe) and wanted to pursue String Theory after QFT (my undergraduate thesis was an intro to QFT, later I did 4 grad courses on the subject). I was looking at the standard treatments of ST, charming as they were, but there was still an itch. Coming from you, Lee Smolin and Roger Penrose. I want to thank you since I was affected so much by your writings.

Plot twist now:
That being said, in an ideal world if financials were not an issue back then, I would still pursue these ideas, including LQG and twistors. As much time as needed. Just being at the forefront of knowledge without worrying about job prospects would have been the holy grail for me personally. I wouldn't mind if there was only remote connection to physical reality by some of TOE candidates, that's the idea frankly; understanding the concepts is rewarding on its own.

67. **DrDave**
March 25, 2024

Many have followed your blog, and you have made a difference. I am hopeful for the future as geniuses pop up every now and then.

68. **Paddy**
March 25, 2024

Congrats. But boy don't we grow old too fast. At least some consolation knowing that our justifiable anger at certain things were there even when we were young.

69. **Marty**
March 26, 2024

Hi Peter,

I am one of your longer-term regular readers of your blog. I continue to enjoy your articles whenever they appear, although they seem to appear at a somewhat reduced average rate compared to the early years.

I found out about your blog in March 2004, about one week after you started it, thanks to this posting in Physics Forums by the user "selfAdjoint". I had read prior posts of yours in Physics Forums, and because (unlike most string theory skeptics at the time) your posts seemed to be written by someone who had made real effort to understand string theory in the technical sense, I promptly headed over to your new blog!

Without a doubt, your blog played a key role in cementing my prior skepticism about string theory. I had applied to several doctoral programs for entry the coming Fall, and (like so many who are interested in theoretical physics) wanted to pursue high energy theory. Your writings were very helpful in making clear, ahead of time, how important it was for future academic job prospects to work in a trendy area. Your writings also made it clear that the general approach of string theory was at least partly orthogonal to the approach I wanted to pursue (an approach that was unpopular then, and is still unpopular now), which was to work on models for *constructing* quantum fields in an emergent sense from a single underlying, fundamental non-quantum (but also non-classical) field. So when it came time to look for a PhD advisor, it was neither a surprise nor disappointment that there was zero interest by any of the theorists in supervising non-mainstream work that I wanted to do. Staying with particle theory would have required working on either particle phenomenology (which at UCSC meant supersymmetry) or string theory. I ended up focusing on cosmology.

Interestingly but unfortunately, the only other physics grad student I knew at UCSC who also read your blog decided to go ahead with string theory anyway (I urged him to reconsider, but he believed going with the trend was best…). He worked with a well known string theorist, but unfortunately it didn't go as hoped — the approach he pursued at the urging of his advisor didn't work out, and he ended up not getting a postdoc. I think his advisor felt bad about how things worked out for him.

Anyway, this is just one bit of anecdotal evidence that you have provided a real service to others through this blog. Hopefully you continue to find blogging a worthwhile use of your time. I miss the time when you posted more frequently — sometimes posts now are weeks or even more than a month apart — but understandably it takes your time and energy, especially when you don't find much of personal interest to discuss.

I think your moderation style has worked well on the whole, for instance keeping pointless discussions from becoming even more pointless (especially during the string wars). There have been times I wished you would let a discussion run longer, but at least you made clear when you didn't feel you had the expertise or interest to moderate a wandering discussion.

70. **Z Y**
April 1, 2024

Congratulations on the 20 years. I'm curious if you still consider
"Why does the vacuum state break electroweak gauge symmetry?"
As the The Holy Grail of Physics nowadays

71. **Peter Woit**
April 2, 2024

ZY,
Yes, that's still the "Holy Grail". Since I wrote the posting 20 years ago, there has been the major development of the discovery of the Higgs. At that time it was possible that the LHC would tell us about something else than the Higgs, but that did not happen. So, we have nothing new from experiment to guide us on this question.

72. **JE**
April 3, 2024

Hi Peter,

Congratulations for your bravery, resolution and patience during these 20 years!

In the early days of this blog ("The Thin Line of Theory" thread, back in Jan. 2005) –at the height of the Quantoken-Lubos/ST-LQG wars– I commented that string theory might turn out to be nothing more and nothing less than a highly sophisticated human invention enabling many theoretical physicists to keep busy while another physicist came out with a sound theory of quantum gravity in which they could work.

I added that it was a well-devised survival tool that would end up being a marginal subject at math departments only when a simpler theory could unify GR and QFT, and that the LHC would make no difference (because it is NEW) even though no superpartners would be found across its energy range.

The latter happened to be true. The former has not yet happened.

Back then, much like Marty and other commenters, I was discouraged from pursing my own ideas by potential PhD advisors instead of going mainstream with them. As I was more interested in pursuing these ideas than in engaging with them in string theory or noncommutative QFT to get a PhD (partly thanks to this blog), I left academia and kept working on my stuff in my spare time.

As to what has not yet happened, experimental confirmation of any future new insights in particle physics will probably involve matching precision measurements in HEP and cosmological observations, because they will likely have cosmological effects that should fit with these observations. No need to reach higher energy scales in particle colliders, imho.

73. **Giovanni Ronchi**
April 9, 2024

Dear Prof. Woit,

I am a math high school teacher in Milan who got his Master's degree three years ago with a thesis on the homotopy theory of differential graded categories, and an avid reader of your blog

My initial interests to study math were motivated by physics. I later discovered the wonders of algebra and wanted to deepen my understanding of category theory, algebraic topology and algebraic geometry

Part of the motivation for the study of those areas has been the use of categories and homotopy theory in the works of Schreiber, Fiorenza et al. (which I couldn't and can't understand, tbh)

From your blog I understood that:
a) Speculative ideas in physics papers can be vary far from verifiable and/or in accord with our current data
b) String theory is not as promising as Greene and Kaku present it
c) The math I focused on is not as important for physics as I thought reading the n-Lab (my mistake, not nLab's)
d) Fundamental theoretical physics is not making fast progress eventhough hype is ever-present in scientific headlines
e) The link between representation theory and fundamental physics is greater than I expected (I have just started reading your book)

For these and many more ideas you shared I thank you and wish you other 20 years of happy blog- and non blog- life

Giovanni

Abel Prize to Michel Talagrand

March 21, 2024
Categories: Uncategorized

I was very pleased to hear yesterday that this year's Abel Prize has been awarded to Michel Talagrand. For more about Talagrand and his mathematics, see the Abel site, Quanta, NYT, Nature and elsewhere. Also, see lots of reactions on Twitter like this one.

Almost exactly ten years ago I got an email from someone whose name I didn't recognize, expressing interest in the notes I had made available online which would turn into the book on quantum mechanics. He was reading the notes and had some comments which he included, saying he thought they were trivial but maybe I would want to take a look. Some of them were of the type "I don't quite understand the argument on page X". Figuring that I'd help out an earnest reader with a weak background by explaining the argument a bit better, I took a look at the argument on page X. After a while I realized that what I had written was nonsense, a very different argument was needed. "I don't quite understand" was his way of politely telling me "you have this completely wrong."

I soon ran into Yannis Karatzas and asked him if he knew anything about this "Michel Talagrand". He told me "of course! He's amazing, almost got a Fields Medal". Over the next year or two I benefited tremendously from Michel continuing to read carefully through my notes and send me detailed comments. He was very much responsible for improving a lot the quality and accuracy of what I was writing. He had begun his own project of trying to understand quantum field theory by writing a book about it. The result is available as What Is a Quantum Field Theory?, which is a wonderful resource for anyone interested in a precise and accurate account of much of the basics of the subject. If you've seen Gerald Folland's excellent Quantum Field Theory: A Tourist Guide for Mathematicians, you can think of Talagrand's book as a much expanded version, giving the full story that Folland only sketched.

During many of my trips to Paris since that time I've gotten together with Michel and his wife Wansoo, and have also seen them here in New York. It has been a great pleasure to get to know Michel in person, he is a wonderful human being as well as a truly great mathematician.

Comments

1. **Peter Shor**
   March 21, 2024

   I've known Michel Talagrand for many years, since I solved a problem (for my graduate thesis) that he solved independently. He is a truly remarkable mathematician, and is a great choice for the prize.

2. **Ray**
   March 25, 2024

   There is also an interesting interview of Talagrand discussing his research on spin glasses here: https://caphes.ens.fr/history-of-replica-symmetry-breaking-in-physics/ The entire collection is actually quite interesting.

3. **Jonathan Baxter**
   March 28, 2024

   "What Is a Quantum Field Theory?" is an excellent book. I wish he had addressed path integrals though. Very curious as to any insights he may have had.

4. **Daniel**
   March 28, 2024

   Cute detail from Le Monde article:

   Il y a aussi le hasard romantique qui lui a fait rencontrer son épouse dans une petite ville de l'Ohio, aux Etats-Unis. Un concours de circonstances: à la suite d'un séjour professionnel au Canada, il rend visite à un collègue dans le pays voisin, dont la porte de bureau s'ouvre sur une étudiante coréenne, qui deviendra sa femme. Et dont le nom, Wansoo Rhee, sera associé à un futur prix de mathématiques (en probabilité, analyse fonctionnelle, informatique théorique ou combinatoire), décerné par une fondation qu'il a créée et qui est dotée des montants des prix qu'il a reçus. Il sera attribué tous les deux ans, à partir de 2032 – «soit lorsque j'aurais 80 ans, ou un an après mon décès si je meurs plus tôt», précise-t-il.

   https://www.lemonde.fr/sciences/article/2024/03/22/michel-talagrand-un-improbable-mathematicien-j-ai-voulu-prendre-des-risques_6223545_1650684.html

5. **Giovanni Ronchi**
   April 1, 2024

   A translation of Daniel's comment via Google Translate:

   "There is also the romantic chance that made him meet his wife in a small town in Ohio, in the United States. A combination of circumstances: following a professional stay in Canada, he visits a colleague in the neighboring country, whose office door opens to reveal a Korean student, who will become his wife. And whose name, Wansoo Rhee, will be associated with a future mathematics prize (in probability, functional analysis, theoretical or combinatorial computer science), awarded by a foundation that he created and which is endowed with the amounts of the prizes that he received. It will be awarded every two years, starting in 2032 – "either when I am 80 years old, or one year after my death if I die earlier," he specifies."

6. **Peter Shor**
   April 1, 2024

   @Giovanni Ronchi: Google Translate messed up slightly — it should read *theoretical computer science or combinatorics,* not *theoretical or combinatorial computer science*. Both interpretations of the French are possible, but I don't believe that combinatorial computer science is a recognized field of mathematics, either in English or in French.

7. **Auke Jongbloed**
   April 2, 2024

   @Peter Shor: DeepL is doing much better, it also correctly translates "deviendra sa femme" into "was to become his wife" instead of "will become his wife". Here's the translation of DeepL: "There's also the romantic chance that led him to meet his wife in a small town in Ohio, USA. A combination of circumstances: following a professional stay in Canada, he visited a colleague in the neighboring country, whose office door opened on a Korean student, who was to become his wife. And whose name, Wansoo Rhee, will be associated with a future mathematics prize (in probability, functional analysis, theoretical computer science or combinatorics), awarded by a foundation he has set up and which is endowed with the amounts of the prizes he has received. It will be awarded every two years, starting in 2032 – "when I'm 80, or a year after I die if I die earlier", he explains."

8. **Anonyrat**
   April 27, 2024

   I recently received my copy of ""What Is a Quantum Field Theory?" and after a quick scanning of its pages, I realized that my training as a physicist blinded me to the sloppy physicist's mathematics of Quantum Field Theory, and how much of a problem that it poses to mathematicians.

   Can/will QFT ever be put on a sound mathematical basis? Is it a worthwhile effort in that it will advance mathematics and physics?

9. **Peter Woit**
   April 27, 2024

   Anonyrat,

   The thing is that there are at least two different categories of sloppy use of mathematics by physicists in QFT, with very different significance.

   1. Writing things that are fundamentally correct, but in a way that makes it a struggle for someone to figure out exactly what you are saying and why it is true. The writer may or may not themselves understand exactly what the correct statement is and why it is true. Talagrand does a great job in his book of taking the usual problems of this kind in a QFT book and fixing them.

   2. Writing about a topic as if it is understood when it isn't. There are various places in QFT where understanding what is going on is an open problem. Mathematicians rarely if ever will make the mistake of writing about something like this without being aware of and addressing the problem. Physicists on the other hand, will happily write about something ignoring significant problems, assuming they are "just a technicality". Maybe the problem is just a technicality, maybe not, there are examples where we really don't know (e.g. non-perturbative definition of chiral gauge theory).

David Tong: Lectures on the Standard Model

March 22, 2024
Categories: Euclidean Twistor Unification, Uncategorized

David Tong has produced a series of very high quality lectures on theoretical physics over the years, available at his website here. Recently a new set of lectures has appeared, on the topic of the Standard Model. Skimming through these, they look quite good, with explanations that are significantly more clear than found elsewhere.

Besides recommending these for their clarity, I can't help pointing out that there is one place early on where the discussion is confusing, at exactly the same point as in most textbooks, and exactly at the point that I've been arguing that something interesting is going on. On page 7 of the notes we're told

> We can, however, find two mutually commuting $\mathfrak{su}(2)$ algebras sitting inside $\mathfrak{so}(1, 3)$.

but this is true only if you complexify these real Lie algebras. What's really true is
$$\mathfrak{so}(1, 3)\otimes \mathbf C = (\mathfrak{su}(2)\otimes \mathbf C) + (\mathfrak{su}(2)\otimes \mathbf C)$$
Note that
$$\mathfrak{su}(2)\otimes \mathbf C=\mathfrak{sl}(2,\mathbf C)$$

Tong is aware of this, writing on page 8:

> The Lie algebra $\mathfrak{so}(1, 3)$ does not contain two, mutually commuting copies of the real Lie algebra $\mathfrak{su}(2)$, but only after a suitable complexification. This means that certain complex linear combinations of the Lie algebra $su(2)\times su(2)$ are isomorphic to $so(1, 3)$. To highlight this, the relationship between the two is sometimes written as
> $$\mathfrak{so}(1, 3) \equiv \mathfrak{su}(2) \times \mathfrak{su}(2)^*$$

This is a rather confusing formula. What it is trying to say is that the real Lie algebra $\mathfrak{so}(3,1)$ is the conjugation invariant subspace of its complexification
$$(\mathfrak{su}(2)\otimes \mathbf C) + (\mathfrak{su}(2)\otimes \mathbf C)$$
where the conjugation interchanges the two factors. Tong goes on to use this to identify conjugating an $\mathfrak{so}(3,1)$ representation with interchanging its properties as representations of the two $\mathfrak{su}(2)\otimes \mathbf C=\mathfrak{sl}(2,\mathbf C)$ factors.

For a very detailed explanation of the general story here, involving not just the Lorentz real form of the complexification of $\mathfrak{so}(3,1)$, but also the other (Euclidean and split signature) real forms, see chapter 10 of the notes here. My "spacetime is right-handed" proposal is that instead of identifying the physical Lorentz Lie algebra in the above manner as the "anti-diagonal" sub-algebra of the complexification, one should identify it instead with one of the two $\mathfrak{sl}(2,\mathbf C)$ factors (calling it the "right-handed" one). Conjugation on representations is then just the usual conjugation of representations of the right-handed $\mathfrak{sl}(2,\mathbf C)$ factor.

Comments

1. **S**
   March 22, 2024

   Forgive my asking what I know you've almost surely addressed before on the blog, but am I right in remembering that this is also related to the highly confusing fact that the Spin groups in signature (1,3) and (3,1) are not isomorphic?

2. **Peter Woit**
   March 22, 2024

   S.

   (First, should note that Spin(1,3)=Spin(3,1), the problem is not there, but with the Clifford algebras, or pinors. Pin(1,3) is not equal to Pin(3,1) and one of the Clifford algebras is 4 by 4 real matrices, the other is 2 by 2 quaternionic matrices).

   No obvious relation to that (at least not that I understand).

   But in the picture I'm proposing there's a fundamental right-left asymmetry, unlike the usual Clifford algebra/space-time story. So, the issue you are mentioning, which shows up for pinors, not spinors, may show up somehow.

   Update: Moderating out further discussion of this issue as off-topic.

3. **Marvin**
   March 23, 2024

   Hi Peter,

   Thanks for having brough forward this link. I read some pages here and there, and I agree that it really look as a very clear and good tutorial on the Standard model.

4. **Chris Oakley**
   March 25, 2024

   I learned the Poincaré group from the ancestor of Tong's lecture notes at Cambridge DAMTP in 1980 and they were similarly careless about combining generators of rotations and boosts with complex coefficients, this being an undefined operation in the context of the group theory used up to that point. Interestingly, our scratchy lecture notes on Lie groups were all we had as there did not seem to be any text books worth the mention on the subject at the time. Maybe Peter's book fills this void. When I have time, when I retire, if I am ever able to, I will check it out properly!

5. **Robert A. Wilson**
   March 25, 2024

   I've never found anyone who can explain this careless mixing of complex combinations of rotations and boosts convincingly. It happens all the time, and is usually mathematical nonsense. I assume they must be trying to describe some real physics, but I can't see the connection between the physics and the mathematics. Tong, for example, follows convention in putting a scalar i into the definition of the Lie bracket, which unfortunately makes it impossible to talk about symplectic Lie algebras in any reasonable way, and makes it almost impossible to distinguish different real forms of the same complex Lie algebra. If you think, as I do, that the distinction between so(3,1) and so(4) is of utmost importance for quantum mechanics, then this is a rather serious issue.

6. **Peter Woit**
   March 25, 2024

   Robert A. Wilson,

   I think there is a straightforward mathematical version of what Tong and everyone else is trying to say in their textbooks, but to state it you need to distinguish between a real Lie algebra and its complexification, and distinguish between different real forms. I don't know of any textbook that does this (would be curious to hear if anyone else does know of such a thing). Some say as little as possible to avoid writing down nonsense, others do write down something that makes no sense. Behind this I think are various combinations of the author not understanding what is really going on, and the author understanding what is going, but deciding that keeping track of the necessary distinctions is too much trouble for this kind of textbook.

7. **Robert A. Wilson**
   March 25, 2024

   On page 26 of Tong's notes, he basically says that the physicists' version with Hermitian matrices generating unitary Lie algebras is equivalent to the mathematicians' version with anti-Hermitian matrices, but this isn't true for real (orthogonal) Lie algebras or quaternionic (symplectic) Lie algebras, only for complex (unitary) Lie algebras. In the real case there are more Hermitian than anti-Hermitian matrices, and in the quaternionic case it is the other way round. The quaternionic case does arise even in classical physics, for example in the symplectic gauge group of phase space, and it is quite tricky to handle this example using complex Hermitian matrices, rather than the natural anti-Hermitian (split) quaternionic matrices. This can lead to error, for example to using the 21 independent symmetric 6×6 real matrices instead of the 21 independent anti-Hermitian 3×3 quaternionic matrices, and thereby generating the wrong Lie algebra.

8. **maxstroke**
   March 25, 2024

   Not sure if this is directly on point, but Richard Behiel just a few days ago put out an amazing youtube titled "the mystery of spinors". For a layperson, it is helping me get the most tenuous foothold on some of the notation and concepts that folks here know in their bones. https://www.youtube.com/watch?v=b7OIbMCIfs4&t=1374s

9. **Arnold Neumaier**
   March 26, 2024

   Peter,

   "you need to distinguish between a real Lie algebra and its complexification, and distinguish between different real forms. I don't know of any textbook that does this (would be curious to hear if anyone else does know of such a thing). "

   Try Section 2.7 and Section 11.6 of v2 of the Lecture Notes
   'Classical and Quantum Mechanics via Lie algebras'
   by A. Neumaier and D. Westra,
   https://arxiv.org/abs/0810.1019

10. **Peter Woit**
    March 26, 2024

    Arnold Neumaier,
    What I had more specifically in mind was the case of the Lorentz group and its finite dimensional representations, where I don't know of a physics textbook that distinguishes between the real Lie algebra and its complexification, as well as between the other real forms of that complexification.

11. **Fadde'ev-Popov ghosts**
    March 29, 2024

    Woit, perhaps you do not consider these to be physics textbooks; however, they are quite accessible for hep-th:

    Miller, Symmetry Groups and Their Applications
    Lounesto, Clifford Algebras and Spinors

12. **Peter Woit**
    March 29, 2024

    Fadde'ev-Popov ghosts,
    I don't have a copy of the Miller book, but from what I remember it was a general group theory book aimed at physical applications, unlikely it had the specific facts about spinors and real forms that I'm referrring to.

    The Lounesto book is not the kind of book explaining relativistic quantum mechanics or quantum field theory that I had in mind. It does have a great deal of detail about the Clifford algebra in Minkowski signature, but what I'm referring to is not that, but the complexified and real form stories of the Lorentz and rotation groups in 4d, which I don't see there (although I didn't look closely, it possibly is there in some form).

13. **Arnold Neumaier**
    March 29, 2024

    There may be something in one of the following books, but I don't own them and cannot check:

    A. Barut and R. Raczka, Theory of group representations and applications. World Scientific 1986.

    B.C. Hall, Lie Groups, Lie Algebras, and Representations: An Elementary Introduction, Springer 2003,

    A.W. Knapp, Representation theory of semisimple groups. An overview based on examples, Princeton University Press 2001.

    B.G. Wybourne, Classical groups for physicists, Wiley 1974.

A Report From Mochizuki

March 25, 2024
Categories: abc Conjecture

I don't really have time to write seriously about this, and there's a very good argument that this is a topic anyone with any sense should be ignoring, but I just can't resist linking to the latest in the abc saga, the REPORT ON THE RECENT SERIES OF PREPRINTS BY K. JOSHI posted yesterday by Mochizuki.

To summarize the situation before yesterday, virtually all experts in this subject have long ago given up on the idea that Mochizuki's IUT theory has any hope of proving the abc conjecture. Back in 2018, after a trip to Kyoto to discuss in depth with Mochizuki, Scholze and Stix wrote up a document explaining why the IUT proof strategy was flawed. Scholze later defended this argument in detail and as far as I know has not changed his mind. Taking a look at these two documents and at Mochizuki's continually updated attempt to refute them, anyone who wants to try and decide for themselves can make up their own minds. All experts I've talked to agree that Scholze/Stix are making a credible argument, Mochizuki's seriously lacks credibility.

The one hope for an IUT-based proof of abc has been the ongoing work of Kirti Joshi, who recently posted the last in a series of preprints purporting to give a proof of abc, starting off with "This paper completes (in Theorem 7.1.1) the remarkable proof of the abc-conjecture announced by Shinichi Mochizuki…". My understanding is that Scholze and other experts are so far unconvinced by the new Joshi proof, although I don't know of anyone who has gone through it carefully in detail. Given this situation, an IUT optimist might hope that the Joshi proof might work and vindicate IUT.

Mochizuki's new report destroys any such hope, simultaneously taking a blow-torch to his own credibility. He starts off with

> .. it is *conspicuously obvious* to any reader of these preprints who is equipped with a solid, rigorous understanding of the actual mathematical content of inter-universal Teichmüller theory that the author of this series of preprints is **profoundly ignorant** *of the actual mathematical content of inter-universal Teichmüller theory*, and, in particular, that this series of preprints **does not contain**, at least from the point of view of the mathematics surrounding inter-universal Teichmüller theory, **any meaningful mathematical content whatsoever**.

and it gets worse from there.

Update: A commenter points to a response from Joshi here.

Update: Scholze has a comment on MathOverflow indicating precisely where Joshi's attempted proof runs into trouble.

Update: Mochizuki and those around him award themselves \$100,000 (this is the IUT Innovator Prize described here).

Comments

1. **Jeff Berkowitz**
   March 25, 2024

   Do I understand this correctly? Joshi worked hard in an attempt to patch up the perceived deficiencies in Mochizuki's proof, and Mochizuki shot him down? I guess because … in Mochizuki's eyes there are no "perceived deficiencies"?

2. **Peter Woit**
   March 25, 2024

   Yes, that's about it. He's basically, in a very unprofessional manner, attacking as an incompetent the one person outside his circle who believes his IUT methods are worth pursuing.

3. **Low Math, Meely Interacting**
   March 25, 2024

   A while ago I attempted to comprehend the plain English content in an earlier such communication. I forgot how much an onslaught of italics and bolding could make my eyes throb.

4. **NoGo**
   March 25, 2024

   Among other weirdnesses in this report one thing sticks out to me: in this report as in his previous ones Mochizuki steadfastly refuses to call Scholze and Stix by name, always using euphemisms like "RCS" or "redundant copies school". I cannot but notice a strange similarity to Vladimir Putin's steadfast refusal to ever mention by name his political opponent Alexey Navalny, until the latter's death.

   And speaking of unprofessional manner, I think Mochizuki hits the new lows. He basically tells that to him the papers of Joshi look like they were produced by ChatGPT, although he graciously gives the poor guy the benefit of the doubt!

   Well, calling Scholze (not by name) an ignoramus that makes bad undergrad-like mistakes sounds positively like a compliment by comparison…

   I hope the experts will tell us if there is any meaningful mathematical content in this report, but given his tone, and the hard-to-read style that Mochizuki adopts, I am not holding my breath.

5. **Eric Lehman**
   March 25, 2024

   Joshi has a Theorem 9.11.1 in one of his papers. Mochizuki comments:

   "[where we note that it is not clear whether or not the number "9.11…" assigned by the author to these key results in [CnstIII] was purely coincidental or a consequence of some sort of sense of rhetoric or humor that lies beyond my understanding]."

   I don't know whether Mochizuki is connecting Joshi's theorem numbering to the September 11 terrorist attack, the 911 emergency number in the US, or something else. In any case, I fear that more than anger may be at work in Mochizuki's mind.

6. **Sabine**
March 26, 2024

Next time I really want to insult someone I'll call them "ignorant of inter-universal Teichmüller theory" and that will certainly do the job.

7. **S**
March 26, 2024

This is painful to look at. It does not seem to me that Prof. Mochizuki is well.

8. **Mike**
March 26, 2024

Perhaps Mochizuki was also irked by Joshi's papers because he wants all the glory for an abc proof.

Besides a consensus that the proof doesn't exist, do experts consider that there is any interesting or useful mathematics in the IUT papers? If so, one sign of hope is that I think there's a real possibility that this could be settled via a formalisation effort within, say, 10 years. If the proof doesn't compile as Lean code, Mochizuki will have to argue that computer CPU's are incompetent at mindless elementary computational steps.

9. **Low Math, Meekly Interacting**
March 26, 2024

My admittedly cheap shot about formatting was kind of hinting at that suspicion: This does not read like the product of an entirely well mind. At minimum, a dearth of independent editorial feedback to moderate the tone (and the font) has allowed a tendency toward self-sabotage to run amok.

10. **Giant Toad**
March 26, 2024

@Mike: The expert consensus is that there is essentially nothing of interest in any of the IUTT papers. This is well-reflected in the discussion hosted on this blog back in April 2020.

It is also clear that there is zero chance of settling all of this by a computer formalization of Mochizuki's work, since at the heart of things (i.e. in the proof of Corollary 3.12) there is nothing actually there to feed into the computer. Something which is not already well-understood by humans cannot be formalized!!

11. **categoricalequivalent**
March 26, 2024

It seems that there are fewer mathematicians thinking this includes useful ideas. I think they, who insist that the theorem was proved and want that it is accepted by mathematicians, should deal with computerization by themselves. Although it was unrealistic a little ago, it is prospering more and more these days. Well, if it doesn't work well, then they can't avoid conceding the fact that the proof is incomplete, so they will hesitate to do it.

12. **Arpex**
March 26, 2024

The growing weirdness in Mochizuki's rebuttals, his peculiar sentence construction, his obsession with identifying non-existent hostile patterns in the criticized texts, and even his polarization between the Western and Eastern mathematical communities are probably not indicatives of arrogance and single mindedness, but rather of some form of cognitive decline, not much different from what happened to several prominent mathematicians (needless to say which one comes to mind first). In this case, this might not be the appropriate forum to discuss the situation or criticize a once prolific mathematician, who apparently is not able anymore to defend himself and his work in an articulate and objective manner.

13. **Jj**
March 26, 2024

Right or wrong, Joshi's work be judged on its merits and not be intermingled with the circus that surrounds Mochizuki..

14. **momerathe**
March 26, 2024

As a very gentle admonition: I feel it is bad form to speculate about someone's mental health, and such comments may be hurtful to those struggling with mental health issues and to their loved ones. Let's try to be kind.

15. **Peter Woit**
March 26, 2024

Arpex/momerathe,
I agree that such speculations are not a good idea, but recognition that there is a problem is important. Those surrounding Mochizuki in Kyoto should be doing something about this, not enabling his behavior but staging an intervention. Even if they believe he's completely right about the mathematics, why isn't anyone telling him that writing documents like this is destructive to his own interests?

16. **Anupras**
March 26, 2024

Although IUT has potential, Mochizuki is extremely arrogant (with his refusal to attend proposed conferences on IUT and with regards to clarification of the theory to non-experts, and that could be one of the reasons why most mathematicians have given up on it.

17. **Millstone beside bath**
March 26, 2024

Prior to the Comments against Joshi and his paper, from what Prof. Mochizuki wrote in his blog in Japanese this January, I have had serious concerns, not about the IUTT itself: https://plaza.rakuten.co.jp/shinichi0329/diary/202401020000/

I would never recommend using a translation tool with the expectation that meaningful content would be written. Only the translation of the title of the post here: Deification, Ethnocentrism, and Suppression of Rationality of the West based on Christianity and Judaism

The problem is already out of mathematics. I believe we should not help it get any worse.

18. **fwiw**
March 27, 2024

Peter,

> why isn't anyone telling him that writing documents like this is destructive to his own interests?

Well,

> 'In light of the blatant obviousness underlying (Reac1), many consultees were not interested in investing the time and effort necessary to discuss Joshi's series of preprints in detail and indeed strongly encouraged me to simply ignore them as well'

19. **martibal**
March 27, 2024

This is a kind of crash-test on how the maths community is healthier than the hep-th one. Although Mochizuki is a well recognised mathematician (as far as I understood), when he speaks no sense, then some prominent mathematicians say so. At the end of the day, only Mochizuki's pride is wounded.
When recognised physicists speak non sense in high energy theoretical physics, the all field is ruined…

20. **finitelocallyfreegroup**
March 27, 2024

I'm convinced that Scholze and Stix's objection to Mochizuki's Corollary 3.12 is valid, as Mochizuki hasn't been able to defend himself by spelling out all the details in all those years after the objection was made. Assuming the objection is valid, then nothing would save IUT, including any attempt to make IUT even fancier by incorporating perfectoid fields, because nothing prevents the real numbers to have nontrival automorphismes as ordered abelian groups.
But in any case Mochizuki's response to Joshi's hard work is unacceptable. In doing mathematics we pursuit the ultimate consensus that is unaffected by any personal status, political power or even the law of physics. Nobody is entitled to shut another mathematician up by personal attack or political hints.

21. **Peter Woit**
March 27, 2024

Millstone beside bath,
That's another document that would argue for an intervention from those close to Mochizuki.

fwiw,
The right advice to Mochizuki would have been not to ignore Joshi's work, but to respond professionally to the mathematical content.

22. **A. Wolf**
March 27, 2024

Peter, I feel your "right advice" to Mochizuki presupposes he has the ability to respond professionally to mathematical critique, which seems an unrealistic expectation at this point. The people who work with him surely realize he cannot respond in a professional manner, so telling him not to respond at all is probably the best advice they can offer.

I don't see an intervention forthcoming. If they're willing to publish his papers in Kyoto then they are very unlikely to back down on support, not only due to this action demonstrating an unwillingness to challenge him when they should, but also due to the escalation of commitment. Admitting he needs an intervention would be tantamount to an admission that the IUTT publications are evidence of a serious systemic error. This entire phenomenon is about saving face and the longer it persists the more entrenched the actors become.

23. **Peter Woit**
March 27, 2024

A. Wolf,
I fear that you are right. Concerning Martibal's comment above, I think the difference between this and the string theory case is that here the problem has remained localized in Kyoto, while in physics it spread through the entire community.

24. **Darren Untoward**
March 27, 2024

A response by Kirti Joshi to Shinichi Mochizuki's recent comments on his papers are here: https://mathoverflow.net/questions/435110/consequences-of-kirti-joshis-new-preprint-about-p-adic-teichm%c3%bcller-theory-on-th/467851#467851

25. **prefertobeanonymous**
March 27, 2024

Stix and Scholze ultimately did read Mochizuki's mathematical content and pointed out the first thing they thought wasn't correct (3.12).

It would be very nice if someone could do the same for Joshi's work. Mochizuki's document appears to be mostly disparaging and (I need to review it again) but he doesn't seem to say something like "I follow Joshi's work until paper ___ page ___ proposition ___" which Scholze and Stix HAD done for Mochizuki.

26. **Giant Toad**
March 27, 2024

Amused that Mochizuki insists on writing "Fontaine-Fargues curve", while the universally accepted name is Fargues-Fontaine curve. It's a small detail, but coming from someone who insists so forcefully on accuracy and carefulness etc., it is rather ironic.

27. **Enraged number theorist**
March 27, 2024

By the way did anybody manage to make any sense out of the Oberwolfach report
of Mochizuki?

It reminds me of the hallucination of ChatGPT (ops).

More seriously, I might be missing something, but that report aims to give a completely elementary description of his “main idea”.

Perhaps I am limited, but by reading it, there seems to be no fucking way he has a proof of abc. It seems more or less accurate that he has gone senile from that report.

If not, how is it possible that such an institution as Oberwolfach has accepted to lower their standard to the point of accepting this trash in their reports? Isn’t it enough that RIMS has already decided to trash its name for good? Shall we have this guy sabotage the prestige of other important mathematical institutions too?

I would love if some person involved with that publication could step in here and justify this choice, by making some explanation for this. To me this document makes no fucking sense!

28. **B. Malpani**
March 28, 2024

Some might think that this is a trivial detail, but keep in mind that Mochizuki is 54 years old; i.e. not a senile old man exhibiting what has been labeled “cognitive decline”.

Recall that he maintains a “Safety Confirmation Information for Shinichi Mochizuki” webpage; Internet Archive has crawled it as early as 01 April 2012. (Sorry but the date is what it is.) He seems to update this page often but not daily. (Weekly, perhaps?) I hope that the impulse that lead to such a webpage doesn’t further develop.

Current developments are, simultaneously, surprising, and somehow, not surprising.

Very sad.

29. **Peter Woit**
March 28, 2024

Enraged Number Theorist,
I assume you’re referring to
this

This was just one contribution to this workshop
https://ahgt.math.cnrs.fr/activities/workshops/MFO-RIMS23/
organized as part of this

https://ahgt.math.cnrs.fr/

There’s supposedly an eight-member committee supervising that, and Benjamin Collas seems to be the main person responsible for promoting IUT this way. He and that committee might want to discuss what they should do about the situation Mochizuki has created.

30. **The Cohomologist**
March 28, 2024

Hey Peter,

Noticed this odd comment on page 7 of Mochizuki’s report:

>>[where we note that it is not clear whether or not the number “9.11…” assigned by Joshi to these key results in [CnstIII] was purely coincidental or a consequence of some sort of sense of rhetoric or humor that lies beyond my understanding].

Any idea what this means? Is this a 9/11 reference?

31. **Enraged numer theorist**
March 28, 2024

Dear Peter,

Yes, exactly that report, indeed. Thanks for pointing out the name of the responsible person.

I hope Oberwolfach, he and the other organizers are aware that the number theory community is watching this, and nobody is amused, to put it mildly.

Given the public attention that this guy is drawing with his inflammatory bullying with the mathematical community at large, and given how little sense that report makes (and how much at stakes there is with these claims!), it is clear to many in the community that Oberwolfach and the organizers should do something to distance themselves from this person.

Accepting that nonsense as a report is a terrible testament of low standards of academic quality. And for an institutions like Oberwolfach, where reputation is pretty much everything, that’s a very dangerous move.

I trust somebody will do something about this!

32. **David Roberts**
March 28, 2024

@Enraged number theorist

that example of his in section 3.2 (the Riemann sphere being the gluing together of two affine complex charts), feels to me like either linguistic sophistry or a total unwillingness to learn what people mean when they talk about colimits like this.

I went through the process of calculating this colimit in an elementary way from the definition, in a pdf linked this blog post. And showed that doing the kind of apparently wicked identification Mochizuki condemns really doesn’t matter. It’s the kind of conceptual mistake you might expect an undergrad to make when learning about category theory.

Or for that matter, physicists 100+ years ago worrying about the “hole paradox” because they are sufficiently mathematically sophisticated to think about diffeomorphisms of manifolds and charts and whatnot correctly.

One can even do this gluing together of P^1 in a skeleton of the category of complex manifolds, or complex varieties or whatever, where you don't have the freedom to find a new, distinct copy of the affine line.

The affine line itself can be literally unique (not even up to isomorphism, but the only representative of its isomorphism class), but there are two different embeddings into the projective line, and hence we can still distinguish the coordinates on the two charts.

I really don't understand why this particular example is so important to Mochizuki, when it really portrays an unfamiliarity with how people speak in modern geometry.

33. **Enraged number theorist**
March 28, 2024

Dear David,

Thanks for the thoughtful reply.

What now brings particular gravity to the situation is that such futile discussions on his side are implied to be the "main idea" behind the proof of such a deep statement as ABC. As a report of an institute like Oberwolfach and to top it up now, from somebody that twice has shown extremely bullying behaviour with other professionals, drawing a lot of attentions from the larger public, which must be thinking something particularly clownish is going on with the number theory community: hard to refute that!

What is particularly sad about this, is that it might give to the outsiders the misleading idea that whether somebody has proved ABC or not could really rest on such futile discussions as the one of his report. That's really a sad unfortunate turn of events, since Diophantine Geometry is that one subject where the biggest gems (which are logically weaker results than ABC) have all came with tour-de-forces of absolute brilliance, from masters like Siegel, Bombieri, Faltings and Vojta (to name a few), with no empty trivialities, but one difficult idea after the next.

It is of the utmost importance that as a community we distance ourselves from this crank. Personally anybody can use their time as they wish (and I hope that as a community we will be able to address Joshi's investment and its intellectual value on its own ground). But as a community it is of utmost importance that this clown/bully does not anymore have a space in our most revered institutions.

I am pretty sure the German tax payers wouldn't be amused to know on what kind of farse Oberwolfach is wasting their time on. This sounds like a joke or an exaggeration. It is not:

As a community the bare minimum we can do to keep our credibility from a society that invests in our intellectual pursuits, is to keep such clowns at bay.

34. **GeoMan**
March 28, 2024

Like everybody, I've found Mochizuki's work and behavior on this baffling. But I've also found the Scholze/Stix reply uncompelling–it is, at best, a sketch of what's wrong with the IUT program. In a way this is not surprising; if the target of your objection is indeterminate, your objection will be too. So when Peter says that "all experts [he's] talked to agree that Scholze/Stix are making a credible argument", that is exactly correct: it's a credible argument, not a compelling one, and certainly not a disproof of IUT ABC.

In contrast, Joshi and Dupuy have attacked this problem the right way: comprehensively and with full rigor. Whether there are better things they could be doing with their time is another story . . .

35. **marksym**
March 29, 2024

On MO there's a question about a purely mathematical claim of Mochizuki stated in the note Peter links to, but apparently nobody knows either what it means or how to prove/refute it https://mathoverflow.net/questions/467696/global-character-of-abc-szpiro-inequalities

36. **Clarification needed**
March 29, 2024

What does it mean to "sum up local inequalities at each prime number of a number field"?

I surely don't know how to prove a statement when I don't even know what exactly is the statement!

Can somebody please clarify the precise meaning of this claim? I believe the MO's question does not do any job in making this precise.

This might be actually helpful. If Joshi is indeed "summing up local inequalities" (whatever that means) and if one can show that this can never give a proof of ABC through those triples as Mochizuki suggests, then at least we can get a cheap verification that Joshi's "proof" is (also) flawed/inexistent (like Mochizuki's one) and avoid waste time in empty discussions for 12 more years.

37. **Peter**
March 29, 2024

Peter Scholze is participating in the discussion on MO.

38. **Héhéhé**
March 30, 2024

@GeoMan

I think Scholze has better things to do than spend his time responding to a person who insults him (and others) and who is incapable of producing an understandable mathematical text, let alone a credible counterargument to his objections.

It is not up to Scholze to disproof IUT ABC. He has already done his part. It is up to Mochizuki to produce an irrefutable and clear proof. But, given the last 12 years, that probably won't happen.

If Joshi's demonstration proves incorrect, it's time to move on and leave Mochizuki alone in his delusions.

39. **TS**

March 31, 2024

It is interesting to see that the disproof of Joshi's proof that Mochizuki added in his revision is actually readable and has a fairly standard layout, at least relative to the surrounding text. I think there are several ways in which this is humorous, but let me just point out that the part where he seems the most like a normal, functioning mathematician is exactly the part which he says an AI could write.

40. **Darren Untoward**
April 1, 2024

Kirti Joshi has briefly responded to Peter Scholze's concerns about his proof in this MathOverflow response: https://mathoverflow.net/a/468180/472518 . Joshi mainly states that he is working on a full response to the points that Scholze raised, and thanks Scholze for his comments.

There are several linked MathOverflow responses on this topic now, they might be interesting reading.

41. **Curious observer**
April 2, 2024

A fuller response to various questions has been posted by Joshi on MO and his site https://mathoverflow.net/a/468180/472518

https://www.math.arizona.edu/~kirti/qa.pdf

42. **NoGo**
April 4, 2024

Apparently Mochizuki &Co awarded themselves $100,000 every year for 10 years: "The IUT Innovator Prize will be awarded annually for 10 years"

The same page states that "The IUT Challenger Prize of $1,000,000 will be awarded to the first mathematician to write a paper on the IUT theory that shows an inherent flaw in the theory". Why do I have a feeling that Scholze and Stix won't get it even if they publish their findings in a journal?..

43. **AO**
April 4, 2024

Leaving aside the non-mathematical part of the discussion, is it fair to say that Mochizuki's mathematical objection to Joshi's paper is now seen by other experts as fundamentally correct?

44. **martibal**
April 5, 2024

Where do they got this money from ? Did they manage to convince some rich guy to spend money on that (like the breakthrough prize) or did they mage to convince the japanese governement that this question was worth $1.000.000 ? In both case it is a bot worrying for the health of the math community.

45. **Adam Treat**
April 5, 2024

@Curious Observer

It seems Joshi has not yet responded to Scholze's objection restating Mochizuki's objection, but promises to do so in the future. From what I can tell the pdf you linked does not address this objection at all. Yet.

@AO

I have the same question, but from reading the comments on MO it certainly does seem like Scholze agrees with the specific mathematical objection that Mochizuki makes of Joshi.

46. **Soyoko**
April 30, 2024

New responses from Joshi to Mochizuki and Scholze.

https://www.math.arizona.edu/%7Ekirti/local-global-issue.pdf

https://www.math.arizona.edu/%7Ekirti/response-to-Mochizuki.pdf

47. **Unfollow**
May 2, 2024

Will Sawin has already pointed out a flaw in Joshi's response, here https://mathoverflow.net/questions/467696/global-character-of-abc-szpiro-inequalities#:~:text=I%20believe%20the%20claim,dealing%20with%22%20is%20wrong.

48. **Darren Untoward**
May 6, 2024

@Unfollow Joshi has very slightly updated his response on May 2: see https://www.math.arizona.edu/%7Ekirti/local-global-issue.pdf ; pages 3 and 6 inserted a line or two that arguably can be taken to be a response to the concerns about locality raised by Scholze and Sawin.

How I fell out of love with academia

April 5, 2024
Categories: Uncategorized

Sabine Hossenfelder today posted a new video on youtube which everyone in theoretical physics should watch and think seriously about. She tells honestly in detail the story of her career and experiences in academia, explaining very clearly exactly what the problems are with the conventional system for funding research and for training postdocs.

After a string of postdocs requiring moving and living far from her husband, she decided she needed to move back to Germany and applied for a grant to fund her research (I believe for this project). This is how she describes the situation:

> At this point I'd figured out what you need to put into a grant proposal to get the money. And that's what I did. I applied for grants on research projects because it was a way to make money, not because I thought it would leave an impact in the history of science. It's not that what I did was somehow wrong. It was, and still is, totally state of the art. I did what I said I'd do in the proposal, I did the calculation, I wrote the paper, I wrote my reports, and the reports were approved. Normal academic procedure.
>
> But I knew it was bullshit just as most of the work in that area is currently bullshit and just as most of academic research that your taxes pay for is almost certainly bullshit. The real problem I had, I think, is that I was bad at lying to myself. Of course, I'd try to tell myself and anyone who was willing to listen that at least unofficially on the side I would do the research that I thought was worth my time but that I couldn't get money for because it was too far off the mainstream. But that research never got done because I had to do the other stuff that I actually got paid for.

As that grant ended, she decided to try instead applying for grants to work on research that she found to be more promising and not bullshit, but those grant proposals were not successful. Since then, she has left the academic research system and concentrated on trying to make a career oriented around high-quality Youtube videos about scientific research.

It seems to me that Hossenfelder correctly analyzes the source of her difficulties: "The real problem I had, I think, is that I was bad at lying to myself." Those more successful in the academic system sometimes criticize her as someone just not as talented as themselves at recognizing and doing good research work. But I see quite the opposite in her story. Many of those successfully pursuing a research career in this area differ from her in either not being smart enough to recognize bullshit, or not being honest enough to do anything about it when they do recognize bullshit.

Comments

1. **Branislav Raskovic**
   April 5, 2024

   Re: "most of academic research that your taxes pay for is almost certainly bullshit. "

   Although some research may be BS, "most" is a gross exaggeration. The pursuit of most forms of intellectual investigation needs to be fostered and encouraged …. Even if no proximate utility. Her personal issues with her career should not taint the environment others are working in by making such gross exaggerations.

   I enjoy reading all your posts, and in general agree with most of your commentary, but this post of yours does much dis-service to many.

2. **Peter Woit**
   April 5, 2024

   Branislav Raskovic,
   I personally wouldn't make global statements about all academic research. But, in the field of fundamental theoretical physics, she is quite right that most academic research is now bullshit. This is not about "Her personal issues", or about intellectual investigation being attacked, or about criticism of research as lacking proximate utility. It's about the continuing disaster of overwhelming bullshit that has afflicted a field, for reasons that she explains.

3. **Garald**
   April 5, 2024

   Oh come on. You can write a grant proposal that you know will likely be accepted (all the while believing, rightly or wrongly, that it's interesting and perhaps doable) and then you can start working on things that are related, more interesting and more doable (by you). Then you just need to convince the grant committee that they are closely related, and that's subjective enough that you don't have to lie.

4. **Peter Woit**
   April 5, 2024

   All,

   I'm now deleting most of the comments that are coming in. Everyone wants to argue about how science/academia/etc works in general, but I don't think that kind of very general discussion is worthwhile (and I don't want to moderate it).

   Hossenfelder had some very important specific things to say, based on her specific experiences, which she has described with a very rare openness and honesty. This is an important challenge to others in the field, and I'd like to hear from those who have a serious response to that.

5. **David Brown**
   April 5, 2024

   I think it is worth emphasizing Hossenfelder's remarks about her exposure to the writing of textbooks. "The next problem was: the head of the institute made a lot of money selling textbooks. He wrote very little of these textbooks himself. Rather, he gave assignments for parts of the books to students and postdocs — which is why, in case you've ever wondered, these textbooks are so discontinuous and partly repetitive."

6. **Peter Woit**
   April 5, 2024

   David Brown,

Besides what she had to say about postdocs and grants, the stories about the textbooks (Greiner-Reinhardt) and funding for women were interesting, but perhaps specifically problems of that particular institution and that particular time.

7. **Eric Weinstein**
   April 5, 2024

   As we all know exactly what she is talking about, it will be curious to see who pretends otherwise this time.

8. **The Cohomologist**
   April 6, 2024

   I believe you've discussed this on your blog before, but perhaps it bears repeating—how much of the experience Sabine describes carries over to academic mathematics? Treatment of postdocs and grad students, etc.?

9. **Alessandro Strumia**
   April 6, 2024

   Theorists disappear because of two main problems: a) not being hired; b) being hired. In some countries some theorists get hired permanently (often after over-working on mainstream arguments for a few year), next they can do whatever they want. And many stop doing research. Sabine got problem a) probably because she openly criticised the field. Professional theorists always pretend being excited and never criticise colleagues, fearing getting them as referees. Then boring wrong stuff spreads. A possible fix would be allowing anonymous comments below each arXiv paper.

10. **martibal**
    April 6, 2024

    What Peter think about anonymous comments ? Who would moderate this (it is not difficult to imagine many situations of conflict of interest, personal revenge etc).

11. **Peter Woit**
    April 6, 2024

    Eric Weinstein,
    Though a theorist, here Hossenfelder carried out an important experiment, showing the same person can get a grant to write the 25,000th paper about ads/cft, but not be able to get a grant to write papers about other less popular topics.

    The Cohomologist,

    The situation in math is different and significantly better. For one thing, at least in the US, math "postdocs" are funded mostly not by grant money but by teaching and in general grants are smaller and much less significant.

    martibal,
    There would be problems with anonymous comments. In any case, the problem she is discussing is really the job situation, not the literature, which is a different issue. Also, given that the arXiv censors (see the trackback saga) people for posting non-anonymous critical comments on papers by influential theorists, they're unlikely to start allowing anonymous critical comments.

12. **Max Lein**
    April 6, 2024

    @Peter Woit
    I think you are painting with a broad brush here, particle physics and the theory of gravity (as in alternatives to/extensions of/modifications of GR) are not representative of theoretical physics at large. The theory of topological insulators was an important development in condensed matter physics and the theory of classical waves. Some of the techniques that were used in the systematic classification of hermitian/selfadjoint and especially non-hermitian systems came from particle physics. Improvements in manufacture (e. g. 3d printing) made many of these claims accessible to experimental verification.

    @The Cohomologist
    I'm a mathematical physicist by trade who works with both sides of the aisle, although in most jobs I was considered a mathematician. In my experience (which covers Canada, Japan and Europe), some of the pressures and problems are quite similar. Some obscure (and I mean this with love) fields only have a handful of people in it, and personal tiffs or perceived slights can certainly impact one's academic career.

13. **an old guy**
    April 6, 2024

    In 1990 I made the most difficult decision in my life to that point – exiting my graduate physics program @ a top-3 school (whatever top 3 means). It was to be the last time in my life that I felt like an utter failure. There were two years where the idealistic future I had envisioned had no ready replacement; everything felt like a submission to the hedonistic treadmill, the polar opposite of what motivated me to physics.

    What was Physics' offense?

    It was a factory of marginal thought, a vortex of group-think, and a bizarre form of Ponzi-scheme that felt sure to crash. I couldn't look away from this impression. I tried. I passed my qual, convincing myself that I just had to find the right group. I networked and probed. I volunteered my time. And nothing resonated, or when a group working a semi interesting topic would be critically probed, the reflexive defensiveness was palpable and void of humility. Eventually, the behavior and group-think took on an ugliness I could not ignore.

    I had to jump.

    I didn't have anywhere to land.

    As I've watched Dr. Hossenfelder over the years, in addition to feeling a sense of admiration and respect, a lighthearted reprieve due to her humor, a familiar sense of simpatico in probing the world and what structures lay beneath… I also see a haunting specter of what I could have become. It's chilling. I would have struggled to fight a hollowing bitterness that accompanies compromise.

    And it's not just Dr. H.

    As I've followed Dr. Weinstein, I've periodically attempted to square his career path with my brother-in-law's who did their graduate work at Harvard at the same time (some 30+ years ago). To be blunt: the exercise saddens me. I don't mean this glibly or arrogantly (though, that is likely how it will come across), but mine is the sadness of being right. Of forecasting correctly. Put another way, had I “stuck it out” within academia, the probability of residing within Thoreau’s disquieting aphorism–The mass of men lead lives of quiet desperation–was terrifying likely.

In short (apologies for the sililoquy), the dynamic Peter calls out, as do Dr. H & W, had a genesis decades ago. Decades ago! That little to nothing has changed is unconscionable; a poster-child of institutional rot. It is shameful condition for a profession so grand.

Then again, fast forward to now, there are those I admire that remain engaged or adjacent to the academic realm. I view them as representing the best of what I could have become, had I decided differently three+ decades ago.

Someone like Peter, who courageously and continuously calls out the perils of HEP group-think, while contributing to his field.

Or, someone like Dr. W who stubbornly develops his views outside of the sanctified realm, while living life rather well.

Or, most compellingly, someone like the indomitable soul of Dr. H. Bless her.

Adieu –

14. **Sabine**
April 7, 2024

Hi Peter, thanks for the link. Yes, that was the first of those projects, lots of buzzwords about AdS/CFT and quantum simulations and strange metals and so on. Worked like a charm!

Just one correction. I haven’t left academia, I am affiliated with the University of Munich (the Center for Mathematical Philosophy), it’s just that I’m not employed by them (partly because I don’t live in Munich and have no intention of moving there). I haven’t entirely abandoned the idea to apply for another grant again in the future — mostly because I’d like to have a team to work with — but at the moment I’m rather sick of it.

15. **gentzen**
April 7, 2024

> martibal,
> There would be problems with anonymous comments. In any case, the problem she is discussing is really the job situation, not the literature, which is a different issue. Also, given that the arXiv censors (see the trackback saga) people for posting non-anonymous critical comments on papers by influential theorists, they’re unlikely to start allowing anonymous critical comments.

Your best chance to comment on arXiv papers and get actual feedback is probably
https://scirate.com/
There are more platforms (i.e. I wrote in December 2017: “In addition to SciRate, also other options came up. The two most recent were openreview.net and fermatslibrary.com”), but I doubt that any of those alternatives is as “open to everyone”, accepted by scientific communities, and simple to use as SciRate. However, SciRate is non-anonymous, plain real world name. No idea whether or how that is enforced.

That Peter Woit is denied arXiv trackbacks is simply ridiculous. But those trackbacks failed to achieve their purpose anyway. And that me or you won’t get trackbacks makes perfect sense to me, because otherwise arXiv would risk spam attacks. Platforms like SciRate can handle spam, because they are less anonymous.

16. **Stacy McGaugh**
April 7, 2024

In addition to all the valid scientific issues mentioned above, it seems worth also considering the enormous constraints imposed on academic careers by personal constraints – the two body problem, wishing to have but not frequently uproot children, and even just having a geographic preference to live in a particular region (like where you’re from). The demands of an ambitious career path rather aggressively select against these normal human desires, skewing the ranks of the academy.

17. **BM**
April 7, 2024

Earlier this evening I decided to re-read an article in Inference Review by Emanuel Derman about option-pricing theory that I had stored in my bookmarks. Unfortunately it appears that the entire website has been taken offline; I thought you might like to know given your previous posts about this quite peculiar online journal.

This is obviously off topic but I’m afraid that comments on the older, related posts have been disabled; please feel free to delete as you see fit, Peter.

18. **Max Lein**
April 8, 2024

@Sabine
Munich is a great place for people working at the intersection of physics and philosophy. Apart from Harald Lesch, Detlef Dürr used to be very active.

@Stacy McGaugh
Yes, the lack of permanent positions is a huge problem, and scientists getting tenure later and later in their career (if at all). I got a tenured position at the tender age of 43. My lack of stability really had an impact on my personal life, e. g. we had children much later than we hoped for.

19. **martin**
April 8, 2024

I don’t understand why she couldn’t get a job at an university, where one can teach and do research. Having a salary makes you independent of grants, and for theoretical physics you can still do research even if you don’t always get a grant.

20. **ateixeira**
April 8, 2024

So the coments about the book writting are on Greiner’s books? That’s a pitty because I really liked all of his (actually not his books)…

21. **Peter Woit**
April 8, 2024

BM,

Inference lost its funding a while back, stopped commissioning articles. I hope the problem with the website is only temporary.

martin,
Permanent faculty positions in this kind of theory are very hard to get (likely impossible if you're someone not getting grants). And it you were lucky enough to get one, almost surely you would have to move to somewhere far from where your spouse, family, etc. are.

22. **Peter Woit**
April 8, 2024

By the way, skies very clear here in Maine. Later today will be in Jackman to see the eclipse.

23. **Amateur Physicist**
April 8, 2024

I had pretty similar experiences in my academic career. My solution was simply to leave academia and take up a teaching job, where I teach maths and physics for engineers. My salary is about twice I got in the University, the work is nice, and I have lots of time for my own research. I can study whatever I want, and so I am truly independent. Actually, most of my papers have been published since I left academia. So, leaving the academia does not necessarily mean an end of a research career.

24. **Andrew**
April 8, 2024

The evolutionary biologist Jerry Coyne has some thoughts on this here: https://whyevolutionistrue.com/2024/04/08/sabine-hossenfelder-hangs-it-up-and-some-personal-thoughts/

25. **Z Y**
April 8, 2024

Peter,

After the sad state of affairs that this comments thread reflects upon, it brings a smile to my face that an eclipse still excites physicists, enough to travel to a spot within the totality path.

26. **DrDave**
April 8, 2024

We should all support Sabine for telling her story. Also, not all of us can live on our YouTube earnings. She has put a ton of quality work into her channel.

27. **JE**
April 9, 2024

@Garald,

Re: "You can write a grant proposal that you know will likely be accepted (all the while believing, rightly or wrongly, that it's interesting and perhaps doable) and then you can start working on things that are related, more interesting and more doable (by you). Then you just need to convince the grant committee that they are closely related, and that's subjective enough that you don't have to lie."

Well, convincing some grant committee that your groundbreaking research project on e.g. new compactification methods for Calabi-Yau manifolds deserves a grant (based on academic achievements, published research, and so on) is very likely much easier than getting a grant for some 'promising' non-mainstream project of yours still these days. Convincing the committee that the former has anything to do with your more interesting and doable non-superstringy project may be tougher.

Several generations of theoretical physicists, like S.H., have suffered this 'bias' for the last 40-50-60 years. The problem does not seem to lie in that they have not been –or are not– creative enough to cheat a little bit on grant committees. The problem seems more related to the question of why string theory has (for so long) been and (still) is considered a mainstream research subject in HEP by grant committees, mass media, journals/referees and leading figures in the field.

28. **Daniel Shawcross Wilkerson**
April 9, 2024

Frankly, Sabine is exactly correct. I am a computer engineer and computer scientist and the situation in computing is the same. *Many* computing people leave academia for exactly this reason. I have heard a friend say something like all people are doing in computing is taking an idea from 20 years ago and publishing it again under a different name. That might not be 100% true, but it is certainly much of what gets funded.

Another way to put what Sabine is saying is that research funding is limited at the imagination of the people in the funding agencies. And that's being nice about it and not considering the possibility of a lot of cliquishness where people just fund their friends.

Further, Sabine is omitting the fact that people get funding based on publications and the "high status" people in academia sometimes use outright plagiarism to get said publications. Even when proved, the "high status" are above the law and therefore allowed to do so by the committees that are supposed to punish it. This is not a theoretical assertion: I and others have witnessed this first-hand at the highest levels (most famous people and universities).

For those who are not experienced with academia, if you listen, you can hear the ring of truth in Sabine's whole story, whether you want to admit it or not. We need people who are not researchers (that is, most people) to recognize that the funding system set up by the legislatures creates corruption. Only then can the situation be fixed. I have seen people instead reflexively take emotional umbrage at criticisms of the system, clinging to a notion of the purity of the ivory tower. Please stop: your willful blindness is just making the situation worse. For those of us who are researchers, you are making our lives intolerable. We are the ones discovering and inventing things for you and we could really use your help. Please listen.

29. **Jason Tyler**
April 9, 2024

Was just able to watch Sabine's video. Not too surprised about her revelations. Thank you for sharing and it is nice to see the fantastic support she is receiving here and on her channel. Academia needs someone like her desperately, to clear the fog and smooth things out. Follow your dreams and blow us all away.

30. **jj**
April 11, 2024

I'm long-term faculty at a mid-tier US college. My students are mostly good, occasionally very good. I enjoy wide discretion in my choice of courses and research. I have great colleagues. I work hard. I've developed some ideas that I believe in and that I'm proud to publish. With luck, some will be real contributions. I've received a few grants and fellowships, although my winning percentage is modest. I've served on national peer review committees. I support innovative work as I'm able. There are many people like me in academia. Getting a job like mine requires not only hard work but luck. That said, in my experience, people with talent and training comparable to mine who don't get a job like mine end up doing other things that they enjoy instead, often make more money than me, and mostly don't express serious regrets. For this reason, some of the comments here surprise me. Broad charges of "corruption", "outright plagiarism", "above the law", "willful blindness", etc. are not borne out by my experience. "We all know exactly what she is talking about" (Eric Weinstein) strikes me as sophistical. What we know is what SH said. What she's "talking about", especially when held against some of EW's more eye-raising public allegations, is another matter. What Stacy McGaugh observes — that the "demands of an ambitious career path rather aggressively select against these normal human desires" — could be said equally of the law, finance, entertainment, sport, or any other sector where conditions are right for the development of a "star system". In short, I see the problems with HEP that PW has been shedding light on for decades, and I feel his pain, but I'm not sure I see good reason for wholesale deprecation of "the system".

31. **RetiredGuy**
    April 11, 2024

    The situation with National Labs is not much prettier, although there is a much more job security than an untenured academic position, and much less than tenured one. But you still can only work on projects that can be "sold" to the sponsors even if it is "bullshit"; reasoning otherwise is severely discouraged.

32. **Peter Woit**
    April 11, 2024

    jj,
    There have been large numbers of comments coming in about problems of the academic research system in general and in lots of different areas. I've deleted almost all of them because I don't think this kind of general denunciation is helpful. I also though don't think the "everything is fine for me" counter-position is helpful. People really should try to respond specifically to the facts that Hossenfelder was presenting in an unusually honest manner.

    About the "star system" thing. The problem is that academic research is not like "law, finance, entertainment, sport", because in those fields people become stars by bringing in a lot of money, selling a lot of tickets, scoring a lot of goals, a very different situation. In particle theory in particular, for decades now people have been becoming "stars" and collecting \$3 million prizes on the basis of failed ideas.

    The remarkable thing in Hossenfelder's story is her experience with grants, showing that the same person who can get a research grant by spouting the conventional hype about the same topic everyone else is working on cannot get funding to work on ideas they find much more promising. This situation has been going on for decades and is one of the main reasons why this particular field is a disaster area right now.

    I've been thinking about my own early career experiences, back when I was a postdoc at Stony Brook 1984-1988. During those years, string theory completely dominated the subject of trying to use more mathematics to make progress in fundamental physics. I guess I was aware that the correct career move for someone with my interests was to find a hot string theory topic using some of the mathematics I had been learning and start working on that. But I didn't think string theory was at all promising as a fundamental theory, was arrogant enough to believe that my own ideas about what was promising (spinor geometry…) were better. I just couldn't see myself heading down a road of competing for grants/jobs etc. by working on things I didn't really believe in. Seemed a better idea to do what one thought best until the money ran out and you were forced to do something else. This ended up working out fine, although it involved a large amount of luck. I know lots and lots of good physicists of my generation who left the usual path for similar reasons, ended up but working not on different ideas about fundamental theory, but on completely different topics.

33. **jj**
    April 12, 2024

    Peter, things might be getting a little mixed up. My basic point isn't that everything is fine for me, it's that many people in academia, in my experience the vast majority, are neither crooks nor victims. One might get a different idea from some of the comments here. To be sure, writing successful grant applications isn't the same thing as doing worthwhile work, and I've seen people get much better at the former than they are at the latter. But again the number of such people isn't huge, and those who are uninterested in applying for grants (like me, usually) can often get by without them. Those who dislike the whole game for whatever reason or don't land a TT job mostly still have decent alternatives, so we're not talking about an all-or-nothing spin of the wheel. I'd be sad if your important complaints about HEP got mixed up with more questionable resentments. It's reassuring to hear that you're filtering the more egregious stuff.

34. **kodlu**
    April 14, 2024

    FWIW, Inference is back up: https://inference-review.com/contributor/emanuel-derman

35. **David Berlinski**
    April 19, 2024

    Inference is now back 0nline: inference-review.com.

What is String Theory?

April 11, 2024
Categories: Uncategorized

This semester the KITP has been running a program asking What is String Theory?, which is winding up next week, and was promising to "arrive at a deeper answer to the question in the title." It seems though that this effort has gone nowhere, with this report from the scene:

> Went to a string theory conference with many of the top researchers in the field centered around tackling the question "what is string theory" and the consensus after the conference was that nobody knows lmao

For an answer to the question from someone with a lot more experience, I recently noticed that Lubos Motl is very active on Quora, giving thousands of sensible answers to a range of questions, especially having to do with Central Europe. He explains the relation of string theory and M-theory (disagreeing with Wikipedia), and defines string theory as

> the name of the consistent theory of quantum gravity which covers all the vacua found in the context of critical string theory and M-theory.

I had trouble getting my head around the concept of an undefined theory known to be consistent when I first heard about it nearly 30 years ago, but it seems to still be a thing.

Comments

1. **Dom**
   April 11, 2024

   Unrelated but I thought I would mention that Peter Higgs died earlier this week, 8th April

2. **Peter Woit**
   April 11, 2024

   Dom,
   I didn't write a posting about this simply because I know nothing about him personally and nothing about his life's work beyond having looked briefly at his two famous papers. Better that people get information about him and his life and discuss that at a better venue.

   When I looked at the papers it was in the context of trying to carefully sort out the 1962-64 history, see
   https://www.math.columbia.edu/~woit/wordpress/?p=3282

3. **S**
   April 12, 2024

   Lubos's posts on quora are quite fascinating. Thank you for sharing that.

4. **David Brown**
   April 12, 2024

   In the link https://www.math.columbia.edu/~woit/wordpress/?p=3282 in the Update at the bottom of the entry for "The Anderson-Higgs Mechanism", the 1st "here" link goes to the American Journal of Physics website, but does not yield an expository article. Is something wrong with my browser, or is it the 1st "here" link?

5. **Interested Amateur**
   April 12, 2024

   One thing I've been very impressed with over the past twenty years is how Peter still doesn't bear a grudge against those within the String community that were outrageously abusive towards him on their blogs. I used to have a blind admiration towards the intellectual talents of one String theorist in particular; but this was quickly eroded over the years by his bizarre views on identity politics in general and his belief in 2014 that the results of BICEP would be proven correct over those of the 'jealous' Planck collaboration.

6. **Anon**
   April 12, 2024

   Do you find the recent DESI results hinting at decaying dark energy instead of a cosmological constant to be any vindication of the string theory swampland problem, if the results hold up? Before I remember you posting that if de Sitter is in the swampland then the straightforward interpretation is string theory is wrong since the universe is asymptotically de Sitter. The new results suggest there could be gaps in this argument.

7. **Peter Woit**
   April 13, 2024

   David Brown,
   I've updated that link so that it works again (although the article is now behind a paywall).

8. **Peter Woit**
   April 13, 2024

   Interested Amateur,
   The string theory story makes clear that the smartest people in the world can be wrong about something and tenaciously hold fast to a wrong idea against all evidence. I've never held a grudge against Lubos (although on some days I have been rather angry that the senior people around him that he looked up to weren't willing to explain to him that those who thought string theory was not the language in which God wrote the world had serious arguments and might be correct).

9. **Peter Woit**
   April 13, 2024

   Anon,
   The fact that no one knows what string theory is makes clear the nature of the "swampland program". When people make a "swampland conjecture", they are conjecturing

behavior of an unknown theory. If experiment conflicts with this, you can't use that to say the theory is wrong, since you don't know what the theory is. All you can do is say that the experiment has told you something about the unknown theory. Vafa is quite explicit about this, that in his program new experimental evidence can never tell you that your fundamental theory is wrong, since you don't know what it is. This isn't a normal kind of science in which you have a notion of testing a theory that could be wrong. Here the "theory" is unknown, so can't be wrong (so, one could say, "not even wrong"….).

10. **David Brown**
April 13, 2024

@Peter Woit, a pdf for "A Question of Mass" by Jeremy Bernstein, 2010
can be found at a website at the Fluminense Federal University (Universidade Federal Fluminense) in Brazil:
http://profs.if.uff.br/tro/lib/exe/fetch.php?media=ajp000025.pdf

11. **Peter Woit**
April 13, 2024

All,
Please, enough discussion of the Lubos Motl story, which is not the topic of this posting.

12. **NoGo**
April 14, 2024

Peter,

>I had trouble getting my head around the concept of an undefined theory known to be consistent

But maybe the consistency is a part of definition, not a property of the theory! As in (to paraphrase Nathan Seiberg) "if we find a consistent theory of quantum gravity, we will call it String Theory".

13. **Shantanu**
April 15, 2024

Peter, there is also a recent course on string theory at Perimeter institute (!)

14. **Peter Woit**
April 15, 2024

Shantanu,
The Perimeter Institute long ago changed from its initial emphasis on research outside the dominant string theory-based ideology. The lectures on string theory this year by Gaiotto are likely much the same as ones he has been giving there since 2014. At this point the director of Perimeter is a string theorist, and they hosted the 2023 Strings conference. I guess one can argue that they are following their initial contrarian model, going all in on string theory during the years that most of the rest of the physics community has given up on it.

Having a graduate course on string theory is very common. I see two problems with these courses, and quickly looking at Gaiotto's first lecture, his course likely will be an example:

1. The background and motivation given to the students is the same failed ideology that has killed the subject over the last 40 years. The students are not told any of the serious problems with this, why it doesn't work.

2. The technical material is most of the time a detailed immersion in Polchinski's string theory book. It may be the best string theory textbook, but it contains nothing about the problems with string theory, and it is a very challenging task for anyone to absorb the technical material it presents. The early graduate school years are crucial in training theorists, and if they spend one or more of these years immersed in the details of the quantization of the string, those are years in which they are not learning much more important things.

It could be worse though, Perimeter could be one of those institutions that teaches string theory at the undergraduate level….

15. **martibal**
April 15, 2024

But for sure there is also a course on loop gravity, noncommutative geometry, dynamical triangulation and all alternatives to string theory, right ?

16. **Alessandro Strumia**
April 16, 2024

Regge : QCD = string : quantum gravity

17. **Davide**
April 16, 2024

Peter,
I do my best to keep my course factual and hype-free. If you can pinpoint any inaccurate or deceptive statements, please let me know.
The main technical tools the students acquire during the course are the quantization of free fields in 2d, the definition of composite operators in free field theories and associated anomalies, some basic aspects of BRST quantization, the definition of disorder operators and boundary conditions in 2d free theories and rudiments of 2d CFT. Depending on the time availability, I may also sketch the 't Hooft expansion and the relation between bosonic and fermionic 2d QFTs (aka GSO projection).
Many of these tools are of general applicability in Theoretical Physics. I hope they will serve the students well independently of their final specialization.

18. **Peter Woit**
April 16, 2024

Davide,
Thanks for your comment.
From what I understand you're planning mostly a course on 2d qft, specifically CFT. That's fine and some of the techniques are of more general use. My problem is really with the whole conception and framework of such a course.

If you were teaching condensed matter physicists and saying that the point of the whole thing was to study certain classes of condensed matter phenomena in 2d, that would be fine. But students are coming into this believing what Polchinski's book and a 40 year old publicity campaign tell them, that the point of all this technology is a specific theory (the 10d critical superstring, defined by considering a CFT and doing very difficult integrations over moduli spaces of super Riemann surfaces). No one is telling them that the idea of doing this and getting QG + the SM by compactifying on a Calabi-Yau just doesn't work.

Looking at your first lecture, you're doing the standard thing of telling the students to think of a QFT as an integral over paths with complicated rules for how paths split and recombine. String theory is then motivated as going to an integral over surfaces.

But this isn't what the standard model QFT is. It's an integral over fields, with the fields having very specific and subtle geometrical significance (connections, spinors) and the action also having very specific and subtle geometrical significance (curvature, Dirac operator). The message the students get in most string theory courses is that the more advanced, deeper way of thinking about fundamental physics is to throw out all those structures and think about Riemann surfaces and 2d conformal symmetry.

In your first lecture you're not talking about CFT or the QG/unification motivation, but about the "paths to surfaces" motivation and other motivations, leading up to drawing the usual M-theory conjectural diagram. Back in 1995 this was an interesting conjecture, but nearly 30 years later I think anyone trying to sell it to students needs to at the same time clearly explain that looking for such a new kind of fundamental theory has gone nowhere and essentially everyone has given up on it. I guess related to what you were trying to explain is the idea of relating string theory to a large N expansion of QCD, which would be a good motivation.

Maybe my fundamental problem these days is that calling a course "string theory" at a time when experts no longer mean anything in particular by the words "string theory" comes with the need to make clear to the students exactly what you mean by "string theory" and exactly what the motivation is for studying this specific thing. If you don't do that, they're going to think what you mean is the definition and motivation given in the textbook you assigned, which at this point have all sorts of serious problems no one is telling them about.

19. **Simone Speziale**
April 17, 2024

@ martibal, I'm currently visiting, I gave my small contribution to that balance you are mentioning 😉 It was however just an evening lecture to the master students on semiclassical spin networks, Regge geometries and spinors.

There used to be a course on loop quantum gravity (I taught it once myself but that was almost 20 year ago and for UW students, PSI didn't even exist back then!), I dont know if it continues.

There is a LQG group here (of remarkably broad interests), a few students and postdocs working on LQG and on asymptotic safety, Renate Loll is a regular visitor for the CDT side, so fortunately `other games in town' are still represented, and it is possible for the master students to get some exposure to them and broaden their perspectives.

20. **martibal**
April 19, 2024

Noncommutative geometry seems to have disappeared after having been well (but shortly) represented by M. Marcolli. A bit scary to see how a place which was highly advertised as its beginning as a paradise for original thinkers has been (at least partly) swallowed by string and AdS/ CFT (according to the master program visible on the website). It was not enough to be dominant in Princeton, ENS and other famous places, one had to be sure that no place dealing with high energy theoretical physics was spared from string,

21. **Matthew Foster**
April 19, 2024

2D CFT remains a very useful framework for nonperturbative calculations in condensed matter physics. The calculation by Cardy and Calabrese of entanglement entropy using twist operators is a masterful example, whereby one only uses the Schwarzian derivative to get the transformation of the stress tensor from a cut plane (representing the division into subsystems A and B) to the whole plane, and combine that with the conformal Ward identity for the 3-point function of the stress tensor and the two twist ops. It's very hard to get the result even for free fields by elementary (constrained path integral) means–I've tried and failed!

Abelian and nonabelian bosonization remain useful for edge modes of topological liquids, spin chains, and 2D fractal surface-state wave functions of bulk topological superconductors. Also anyon statistics are ultimately traced back to monodromy properties of conformal blocks, although I get the impression that most workers in topological quantum computation bypass this.

On the other hand, though, the rate of new significant results is quite slow and there remains a steep learning curve to get through chapters 5-9 in the big yellow book, plus the prerequisite Lie algebra representation theory for WZNW models/nonabelian bosonization. Hence I don't teach this stuff to grad students in a course, and I'm generally loathe to recommend it for self-study either–it's just very hard to find good applications, for me at least. It's a pity though because the material is some of the most "beautiful" in statistical physics.

22. **Paolo Bertozzini**
April 21, 2024

@ Martibal

"Non-commutative Geometry" has not only been disappearing from the list of research areas represented in Perimeter … it has been almost completely disappearing from the panorama of research in fundamental physics and quantum gravity in particular. Practical first example: at the recent International Society for Quantum Gravity conference in Nijmegen Quantum Gravity 2023, where almost all areas of research in quantum gravity were significantly represented, there was not a single talk specifically dedicated to non-commutative geometry and the topic was barely mentioned in a few cases by researchers in other fields. Second example: in the recently published Handbook of Quantum Gravity there is not even a section properly dedicated to this subject. There might be many (and complex) reasons for such a "retreat" that are too long to be elaborated here.

23. **Andreas Karch**
April 24, 2024

As someone ALSO currently teaching a graduate level course on string theory out of Polchinski's book, I have to take exception. The original goal of string theory (getting quantum gravity in our world plus the standard model) has significant obstacles to overcome that no one currently knows how to tackle, this is fair enough. But still, at lesat in 10d superstrings give us a theory of quantum gravity in which graviton scattering order by order in perturbation theory is finite. That's amazing. Nothing else like it is known to mankind. Future theorists should learn it as a proof of concept ast least. The fact that the tools and tricks one needs to understand on the way are useful beyond that is an added bonus. As graduate chair here at UT I am unfortunately responsible for cutting our string sequence down from 2 semesters to 1. I find it hard to justify why string theory should get more time than cosmology, particle physics or advanced GR (all of which we teach as 1 semester courses). But that one semester I'll defned tooth and nail.

24. **Peter Woit**
April 24, 2024

Andreas Karch,
We'll just have to disagree about quantizing the 10d superstring and understanding the extremely complex story of how one is supposed to get amplitudes that have nothing to with the 4d world out of hopefully finite integrations over supermoduli spaces of super-Riemann surfaces.

On the list of things physicists have come up with that are amazing and worth teaching to students, I still think this topic is very, very far from the top.

Science Outreach News

April 16, 2024
Categories: Uncategorized, Wormhole Publicity Stunts

A few items on the science outreach front:

- The Oscars of Science were held Saturday night in Hollywood, with a long list of A-listers in attendance, led by Kim Kardashian. More here, here and here.

  You'll be able to watch the whole thing on Youtube starting April 21.

- The World Science Festival will have some live programs here in New York May 30 – June 2. One of the programs will feature the physicists responsible for the Wormhole Publicity Stunt explaining how

  > we may be able to create wormholes—tunnels through spacetime—in the laboratory.

- Stringking42069 is back on Twitter with his outreach efforts for the string theory community.

Comments

1. **Mike**
   April 16, 2024

   To state the obvious anyone who thinks Kim Kardashian adds to the glamour of science doesn't understand the psychology of why science is exciting to scientists. It's vomit inducing. Rather than celebrating science the superficial appeal to celebrity taints it.

2. **Z Y**
   April 16, 2024

   @Mike I could replace the KK name by any of the royals that give prizes and the meaning of your sentence wouldnt change muc, but i dont see many people complaining of that 🙂

3. **Interested Amateur**
   April 16, 2024

   I find the science awards ceremony to be patronizing and cringe: the good looking Chads and Stacies give out prizes to the not-so-good looking nerdy introverts. Growing up as a kid interested in science and maths, my hero was Spock and not captain Kirk.

4. **tulpoeid**
   April 16, 2024

   @ Z Y
   Let's keep some standards. I also don't like monarchy, but you can't equate any prize-giving royal, and actually most of the celebrities that attended this show, to Kim Kardashian. Not everyone is as bad as everyone else. Even swamps have a bottom. Let's keep the level of our proverbial bar in mind.

5. **ateixeira**
   April 17, 2024

   ZY you are totally right. Also the presenters and A-list celebrities present are not there for the scientists. They are there for the masses. The rationale apparently is that the masses will see the A-list celebrities getting along with the scientists and will be more attracted and conscientious of science.

6. **David Roberts**
   April 17, 2024

   If the King of Sweden is the one physically shaking the Nobel laureates' hands, I view the process as really one run by the various academic societies. The Breakthrough Prizes seem more to me to be like a explicit stunt by Yuri Milner to try to attach Hollywood glam to scientists and mathematicians. No one talks about the King of Sweden as somehow making the Nobel Prize more socially desirable and "cool", or pretends that's the case.

7. **Steve Huntsman**
   April 17, 2024

   God, how awful. Where's Perelman when you need him?

8. **Z Y**
   April 17, 2024

   @David Roberts About your last sentence, I dont think the Breakthrought Prize is considered by the public at large more socially desirable or cool that the other prizes just because all those celebs are at the ceremony. However I do have seen references in the general public that a royal giving a prize adds to the importance of that prize and you can really see all the panache that is devoted to the royals presence at those ceremonies, even in some of the winners speeches you hear paying respects to them. Having said that, I do have a high regard for the Nobel's committee and winners (nothing is perfect) but Milner's prize is not that bad, it does bring awareness to some of the recent discoveries of science at large that the Nobel cannot afford to recognize

9. **Peter Shor**
   April 17, 2024

   The celebrities do not determine who wins the Breakthrough Prizes — a committee of scientists including many of the previous prizewinners do. The unfortunate thing for the Physics prize is that many of the first prizewinners were string theorists, but I don't think you can blame this on the non-scientists involved.

10. **Diogenes**
    April 17, 2024

I am pleased to note that I don't think I recognize any of these names winning the Breakthrough Prize. That is some progress in directing the funds where they may start to be useful.

11. **Douglas Natelson**
April 17, 2024

The idea of the World Science Festival is great, and while I question the overall emphasis, if the topics rotate over the years through different disciplines, that's fine. The claim about actually producing wormholes in the lab (by using a quantum computer) is still anger-inducing, though.

12. **Gavin**
April 17, 2024

Glad to see you're pointing people to StringKing again so they can find content like this:

https://twitter.com/stringking42069/status/1780127988224282904

More seriously, I want to object again to you giving this troll any visibility on your platform.

13. **smitchmor**
April 17, 2024

Unfortunately, the full World Science festival format seems to be one of the casualties of the pandemic.

In years past the festival was centered around NYC's Washington Square Park. There was a science themed street fair (ideally coinciding with a beautiful spring day) with most events held at nearby NYU facilities.

A wide range of academic disciplines were represented; always had it's share of WOO! physics, but didn't dominate.

Speaking of celebrity, one annually recurring event was called the Flame Challenge, hosted by Alan Alda. The challenge involved encouraging clear cogent communication on seemingly simple questions such as "What is flame?… is color?… is sound?" to grade school students. It was a clinic in encouraging genuine interest in physical science, sans hype. Likely that health issues will prevent a repeat, but his genuine enthusiasm will be missed.

I can honestly say that I looked forward to the festival each spring and hope they it can be built back to something resembling it's previous incarnation.

14. **Rainy Sunday Afternoon Theoretical Physicist Drop Out**
April 18, 2024

This is a total off-topic, but reading that the Perimeter Institute is now directed by a string theorist makes me weep.

Is this the institute that Les Smolin "created" in Toronto, to work on alternative theories in fundamental physics/unification, excluding string theory and other mainstream approaches?

The sense of defeat is… (I don't have the right word).

P.S. Now, I have an image: it makes me think of the ending of the 1978 version of The Invasion of the Body Snatchers.

15. **Amitabh Lath**
April 20, 2024

I am a little uncomfortable with the Kim Kardashian bashing. I probably share your general distaste for the lifestyle but what are we saying to young people? That you cannot be someone that follows the Kardashians and hope to be accepted in the physics and math world? Now look around your departments and note the homogeneity and wonder where it comes from.

KK herself does seem to have an innate understanding of the highly complex system that is American Celebrity culture. She also navigated a marriage (with kids) to an apparently neurodivergent man with increasingly erratic behaviors. I do not believe she is dumb. I know we would all have preferred say, Michelle Obama, but if KK is bringing her millions of followers, let's not stick our noses in the air and shoo them away.

16. **Peter Woit**
April 20, 2024

Amitabh Lath,
I agree that criticizing Kim Kardashian personally isn't a great idea, and it's completely besides the point. The little that I know about her is confirmed by Wikipedia, that she's "famous for being famous".

From the beginning, Milner explained clearly that he felt a big problem with science was that it was not enough part of the celebrity culture and he wanted to do something about that. For him (and many other people), the problem with science is that scientists do not get the attention and respect that people like Kim Kardashian get. If this is what you believe, funding a Hollywood "Oscars of Science" and having Kim Kardashian show up to applaud scientists accepting multi-million dollar prizes makes perfectly good sense.

For myself, the problems with science I see are very different, and in the particular case of fundamental particle theory, more celebrity culture will make the problems worse, not better.

17. **Mike**
April 20, 2024

For what it's worth I wasn't bashing Kim Kardashian in my initial comment, just lamenting what I regard as the misguided idea that science should be made more "cool" by association with celebrities. It would be nice if people thought that figuring out the fundamental laws of physics, proving langlands conjectures, understanding how biological cells work and creating more efficient solar cells is awesome without the need for celebrity endorsement. But of course that wish is hopelessly naive and idealistic.

18. **Peter Woit**
April 20, 2024

Mike,

I don’t know about “cool”, but I don’t understand at all the belief that scientists are not respected, even less the idea that the way to deal with this is endorsement from reality tv stars. According to this
https://www.forbes.com/sites/tracybrower/2022/05/30/new-data-the-most-respected-jobs-and-5-ways-to-gain-respect-no-matter-what-your-work/
the profession of scientist is at the top of the list. What’s at the bottom? Reality TV star.

According to this data, associating scientists with Kim Kardashian is the single most effective thing you could do to make them less respected by the public.

19. **Mike**
April 21, 2024

Peter, I’m overreaching myself with regards to actual knowledge here but my conjecture is that a decent percentage of people acknowledge that being a scientist is a noble calling (even if the practical reality is often somewhat corrupted by the usual social/economic phenomena) while being a “celebrity” a la Kim Kardashian is far less honerable. So its not so much an issue of respect for the scientific professions (although the dumb commentary around climate change, Covid etc do seem to have increased anti-science sentiment). However, in practice most people are far more interested in the personal lives of celebrities, both those with meaningful accomplishments and those without, than they are about the actual content of science textbooks, research papers etc. I’d guess that this is approximately the reasoning behind the inclusion of celebrities into the promotion of the prizes: if people respect the scientific profession and are fascinated by the lives of these celebrities, if we bring them together maybe they’ll get more into the science? I’m not at all convinced.

20. **Amitabh Lath**
April 21, 2024

Mike and Peter, about a million kids who would not have paid any attention to science happened to catch a glimpse, however brief, because of Kim Kardashian. I do not care about her, but I do want to attract the 0.5% of those kids whose interest might be piqued by physics and math. They will help us expand and diversify (and man do we need it). One good way to get these kids to go rushing towards engineering or computer science rather than physics is to indicate that their love of pop/celebrity culture is somehow loathsome; you must be lovers of classical music and opera and jazz to succeed in our rarefied world.

21. **Amitabh Lath**
April 21, 2024

Look, let me give you a concrete example. We are just finishing up our application round for our summer physics program for high school students (Quarknet). They discover cosmic rays, measure muon lifetimes, analyze data from neutrino and collider experiments, have informal chats with graduate students, lab visits, it’s a blast. We try desperately to get a broad range of students in terms of gender, background, socioeconomic status, etc. Now some of these students, mostly female, POC, might be Kardashian fans. If we instructors come in with the sort of disdain you exhibit we will lose these students. They will clam up and not engage.

22. **Mike**
April 21, 2024

Amitabh Lath,
I want more diversity in science, I hope the breakthrough prize gambit works, and if it does I’ll be the first to celebrate.

I take your point though, it’s probably not productive for me to complain about the methodology on the internet, kids are impressionable and if its effective that’s the most important thing. However, as touchingly naive as it may be, I’d just rather live in a world where the science speaks for itself.
Best of luck with the summer program, sounds epic.

23. **David Roberts**
April 21, 2024

@Peter Shor

I’m under no illusion that the celebrities pick the winners. But with eg the Nobel or the Abel, it’s not the previous winners that pick the prize, but the independent and long-running academic bodies, which lends the weight to the prize.

I’d much rather see some really, really good science communicators (of which there are plenty on YouTube) be given decent space at the Breakthrough Prizes along with the Hollywood glitterati, say even equal billing. I tried watching the show live, once, when I happened to be in the US at the time. It was awkward. Actors can act, but they do not usually act as ‘talking science’ that well. Morgan Freeman reading off an autocue about mathematics and physics is much less enjoyable than someone who can speak extemporaneously and enthusiastically on the same topic, at least to me (of course, Freeman’s voice is wonderful to listen to, in any case).

But all of this is my own bias, and, as others have intimated, people like Tom Crawford or Ayliean or Tai-Danae Bradley that connect with the public in a way I sure never can, are good for the field; they show that maths isn’t just about stuffy old white men with beards (I resemble some of those adjectives, at least, and probably on the way to the others at some point).

24. **S**
April 22, 2024

Mike,

You say that people are far more interested in the personal lives of celebrities than the writings of scientists, notwithstanding the difference in respect accorded each. That may be true, but it seems to me that this kind of thing is *at most* likely to get the public interested in the personal lives of scientists as well — still not their writings. That would seem no kind of improvement

25. **Peter Shor**
April 24, 2024

@David Roberts says:

> But with eg the Nobel or the Abel, it’s not the previous winners that pick the prize, but the independent and long-running academic bodies, which lends the weight to the prize.

The Dirac Medal, which I assume is chosen the standard way, is still giving prizes to string theorists, while the Breakthrough Awards (at least for this year) seem to have stopped this practice.

26. **David Roberts**
April 25, 2024

@Peter Shor there's probably something to be said about sufficient and/or necessary conditions, here. Let us see how the Breakthrough prizes turn out after a few decades. 🙂

27. **That correlation has a high p-value**
April 30, 2024

I don't follow the argumet that KK, or similar actors, may increase, on net, the diversity of the math community. As a members of a minority group, fleeing away a shallow mass culture was a great apeal towards an intellectual life. To be sure, I am not arguing the merits of KK, just that her presence probably have zero net effect on diversity. My reading is that Milner just likes to hang out with scientists and mass culture actors (god knows why). The younger me would see KK and the string theory guys and secide to go work on avant garde movies instead, with its cooler and more diverse crowd that look down on KK.

This Week’s Hype

April 23, 2024
Categories: This Week's Hype

Until about a year and a half ago, the way to get funding in physics was to somehow associate yourself to the hot trend of quantum computing and quantum information theory. Large parts of the string theory and quantum gravity communities did what they could to take advantage of this. On November 30, 2022, this all of a sudden changed as two things happened on the same day:

- Quanta magazine, Nature and various other places were taken in by a publicity stunt, putting out that day videos and articles about how “Physicists Create a Wormhole Using a Quantum Computer”. The IAS director compared the event to “Eddington’s 1919 eclipse observations providing the evidence for general relativity.” Within a few days though, people looking at the actual calculation realized that these claims were absurd. The subject had jumped the shark and started becoming a joke among serious theorists. That quantum computers more generally were not living up to their hype didn’t help.
- OpenAI released ChatGPT, very quickly overwhelming everyone with evidence of how advanced machine learning-based AI had become.

If you’re a theorist interested in getting funding, obviously the thing to do was to pivot quickly from quantum computing to machine learning and AI, and get to work on the people at Quanta to provide suitable PR. Today Quanta features an article explaining how “Using machine learning, string theorists are finally showing how microscopic configurations of extra dimensions translate into sets of elementary particles.”

Looking at these new neural network calculations, what’s remarkable is that they’re essentially a return to a failed project of nearly 40 years ago. In 1985 the exciting new idea was that maybe compactifying a 10d superstring on a Calabi-Yau would give the Standard Model. It quickly became clear that this wasn’t going to work. A minor problem was that there were quite a few classes of Calabi-Yaus, but the really big problem was that the Calabi-Yaus in each class were parametrized by a large dimensional moduli space. One needed some method of “moduli stabilization” that would pick out specific moduli parameters. Without that, the moduli parameters became massless fields, introducing a huge host of unobserved new long-range interactions. The state of the art 20 years later is that endless arguments rage over whether Rube Goldberg-like constructions such as KKLT can consistently stabilize moduli (if they do, you get the “landscape” and can’t calculate anything anyway, since these constructions give exponentially large numbers of possibilities).

If you pay attention to these arguments, you soon realize that the underlying problem is that no one knows what the non-perturbative theory governing moduli stabilization might be. This is the “What Is String Theory?” problem that a consensus of theorists agrees is neither solved nor on its way to solution.

The new neural network twist on the old story is to be able to possibly compute some details of explicit Calabi-Yau metrics, allowing you to compute some numbers that it was clear back in the late 1980s weren’t really relevant to anything since they were meaningless unless you had solved the moduli stabilization program. Quanta advertises this new paper and this one (which “opens the door to precision string phenomenology”) as well a different sort of calculation which used genetic algorithms to show that “the size of the string landscape is no longer a major impediment in the way of constructing realistic string models of Particle Physics.”

I’ll end with a quote from the article, in which Nima Arkani-Hamed calls this work “garbage” in the nicest possible way:

> “String theory is spectacular. Many string theorists are wonderful. But the track record for qualitatively correct statements about the universe is really garbage,” said Nima Arkani-Hamed, a theoretical physicist at the Institute for Advanced Study in Princeton, New Jersey.

A question for Quanta: why are you covering “garbage”?

Update: String theorist Marcos Mariño on twitter:

> In my view, using today’s AI to calculate the details of string compactifications is such a waste of time that I fear that a future Terminator will come to our present to take revenge for the merciless, useless exploitation of its grandparents.

Update: More string theory AI hype here.

Comments

1. **Low Math, Meely Interacting**
   April 23, 2024

   But isn’t it amazing it took this long? It’s the sort of problem the AI hype machine was practically created for.

2. **Peter Woit**
   April 23, 2024

   LMMI,
   Actually people have been getting grants to use AI to supposedly study the string theory landscape for quite a few years now. Before the AI hype boom, this was part of the Big Data hype boom.

   What’s new is that Quanta finally got around to promoting this stuff. Unclear why that took so long, and why they waited until they had Arkani-Hamed on record describing it as garbage.

3. **clueless_postdoc**
   April 23, 2024

   This is purely a layman perspective. I read your post, then I read the quanta article. Never mind if what they’re doing is science, let alone good science, but if at some point people could write down a particular Calabi-Yau 3 fold, and say well if you compactify using this particular Calabi-Yau, you get the standard model. That would be pretty interesting, no? From reading your blog and various popsci books, it seems previously this failed because people simply did not have the computing power, but now it seems they do?

   Remark: “never mind if this is science….it would be pretty interesting?” – perhaps the reason I’m paid by a (poor) math department instead of a physics one.

4. **Peter Woit**
   April 23, 2024

   clueless postdoc,

The problem is that Calabi-Yaus come with a large number (say order 100) parameters. These are going to give you 100 new long-range gravitational strength forces. This looks nothing like physics, it's what Arkani-Hamed is referring to when he says "the track record for qualitatively correct statements about the universe is really garbage". This has nothing to do with any computational difficulty. The reason people gave up on this in 80s was not that the calculations were difficult, but that they were meaningless.

If you decide you're going to assume that you have some mechanism that dynamically fixes the moduli parameters to specific values, you can try and calculate standard model parameters for the effective 4d theory with those fixed parameters. But the result would depend on 100 parameters, and not be based on any underlying theory, again Arkani-Hamed's "garbage".

5. **Dan Winslow**
April 24, 2024

Well…sure, classical computing AIs for this are probably worthless. That's pretty obvious! However, one should think of the current effort as kind of a tune-up. When they start using AIs from QUANTUM computing, then maximum buzzword density will have been achieved and the whole argument will collapse into a single point of infinitely dense grant money.

6. **clueless_postdoc**
April 24, 2024

Hi Peter,

Thanks for the reply!
Again I am very much a layman. I agree the first option is probably not physics. But take the second option, suppose you manually take 100 or so parameters, fix them, and if my reading of the situation is correct, you get some extension of standard model that includes gravity, in a reasonably consistent way? Never mind if this destroys any way of getting it out of a simple fundamental theory, my impression is that we don't currently have any other way to do this (no other competing theory can go this far)?

My uninformed opinion, from a rather humble vantage point, is that having a Rube-Goldberg machine that does at least something is better than having nothing at all. (Never mind how much hype is or would be generated from this)

7. **Peter Woit**
April 24, 2024

clueless postdoc,
The problem is that if you fix the moduli, by hand or by the KKLT Rube Goldberg mechanism, you don't have a consistent theory. You do have an essentially infinite number of things you can calculate, so you can try and keep calculating, trying to make more and more complicated choices to get results that are not in obvious disagreement with observation, but this can't lead to anything meaningful.

I recommend that everyone interested in this follow the Twitter discussion with Marcus Mariño at
https://twitter.com/marino_beiras/status/1783042977922699457
Mariño is a string theorist expert on the subject of Calabi-Yaus, so very well equipped to know whether the calculations being advertised in Quanta are meaningful or garbage.

8. **john**
April 24, 2024

This Dirac Medal and Prize Award Ceremony might be of interest.

https://www.youtube.com/watch?v=tvyIkqqAQw8

They mention string theory in some of the work that was awarded prizes.

9. **zzz**
April 24, 2024

i like that Quanta magazine's graphic for AI-string theory excitement uses an image of an overhead projector. a classroom device that was used for ~20 years starting about 30 years ago.

10. **Peter Woit**
April 24, 2024

zzz,
From what I remember, overhead projectors are significantly older than that, but I guess the technology died right around the same time (late 80s) as the idea in the graphic that one could go from a Calabi-Yau to physics.

11. **Sabine**
April 24, 2024

Imagine how much worse this is going to get once we have widely available neural nets that can reliably be trusted to do maths. Theoretical physicists have created a research standard where maths being correct (more or less) is basically the only criterion for having a "scientific" theory and a publishable paper. Soon they'll be able to generate "theories" automatically and try to publish them.

Hey, GPT! Please come up with 5 new dark matter candidates that evade all existing bounds on direct and indirect detection but will show up in a muon collider at 500 GeV. Format for PRL.

Come to think of it, this might finally make them realize that this isn't how science works.

12. **Xirtam Esrevni**
April 24, 2024

"If you're a theorist interested in getting funding, obviously the thing to do was to pivot quickly from quantum computing to machine learning and AI, and get to work on the people at Quanta to provide suitable PR"

It wasn't just theorist, many software quantum computing companies did just this! Perfect example is Zapata Computing (https://zapata.ai) which pivoted to AI and then went public (ZPTA) this month; thankfully there stock has crumbled since the IPO.

The issue is you have both government and venture capital are so willing to through money at quantum computing and AI regardless of the use case or application.

13. **Mark Raugas**
April 24, 2024

Peter — we last saw each other when I came around to Columbia to talk about AI and math research my group was doing. It turns out I was at a recent Aspen workshop on this topic, with some of the people named in the article, so I thought I would share some thoughts.

I think in general, solving PDE using neural networks instead of traditional linear algebra solvers is interesting in a wide variety of areas in applied mathematics. The trained network becomes a numerical approximation to a solution to the PDE. This is typically called "Physics Informed Machine Learning" and has been around for some time.

I thought it was fun that the strings researchers were using these solvers to try to solve for Ricci flat metrics, purely from a mathematical standpoint, but I don't think it will solve the landscape problem, help define M theory, etc. Even if you could solve for all the classical complete intersection examples (they mention in the article that CY with large h_1,1 or h_2,1 they can't handle), you are very correct to point out the broader moduli space stabilization issues. But say one did have a library of these metrics, numerically evaluated, available as an oracle function. I wonder if that moduli problems somehow might become more tractable. It could be that a diffusion model could then find paths from one network to another (each representing a CY metric). A big problem would be in that case that a network has many more parameters than the input (like an image) and might be very expensive to train. Sampling from such a trained network, even if you could construct it, would yield candidate metrics on moduli space that would somehow have to be checked. I also wonder if you would not learn anything interesting. Say you train only on complete intersection CY examples, would you ever find a non-geometric example? I wonder if going down those trains of thought could be a distraction, ultimately.

Regarding AI, a person at the workshop asked "Why don't you just use a numerical PDE solver?" and the quick response was "That's too hard."

The speaker could dive into numerical PDE solvers if they had time, but it does point to a question regarding the democratization of technology. If the Python packages for contemporary machine learning are relatively easy to use, like Mathematica, and the older Fortran/C++ linear algebra solvers are hard to use, sociologically you will see people adapting themselves towards tools that are easier to use in their studies. That is an indirect product of hype — general hype around AI causes there to be economic investment in related infrastructure, and then the barrier to using related tools falls to the point where it is accessible outside the machine learning community. So people get herded into using those types of tools or approaches. This is not new.

14. **ab**
April 24, 2024

> I think in general, solving PDE using neural networks instead of traditional linear algebra solvers is interesting in a wide variety of areas in applied mathematics. The trained network becomes a numerical approximation to a solution to the PDE. This is typically called "Physics Informed Machine Learning" and has been around for some time.

Are there any rigorous results on the accuracy of these solutions? How do you know they're not just hallucinating?

15. **Magnatolia**
April 24, 2024

Has it been so common for physicists to pivot to AI in the last few years? I don't know any except Max Tegmark and Michael Douglas

16. **Alex**
April 24, 2024

New Witten interview here: https://youtu.be/PZ_qnBwSejE?si=K3HuWjpT2Bx33my8

Apparently, he wants to go back to the good old days of string theory in the 80s… which seems in line with this current hype about CY manifolds…

17. **Michael Weiss**
April 25, 2024

Returning to the past use of overhead projectors in teaching physics, the most creative example I experienced was due to the late Nobelist E.M. Purcell: he gave an entire lecture on an envelope, writing on the envelope, which was projected on the big screen. The subject of the class was "back of the envelope" calculations. I remember many Harvard freshmen laughing when Purcell set e =1 and pi = 1; Ed looked up in earnest dismay, stating that until one is prepared to due such approximate calculations, one is not ready to do physics. Perhaps the string theorists erred in not taking this maxim to heart?

18. **Eitan Bachmat**
April 27, 2024

ab,
There are sometimes, though not often, rigorous results, I would look at the work of Weinan from Princeton, a numerical analyst who now works on AI for solving differential equations, he has a talk at the recent ICM. In the direction of optimization and rigorous results, there are 2 NSF-Simons collaborations called MoDL (math of deep learning), I qould look mostly at the Berkeley centered one, led by Peter Bartlett. Usually results are on relatively simple models, but they are increasingly providing interesting insights. I would also consider the worka of Sanjeev Arora.

19. **Syksy Räsänen**
April 27, 2024

Magnatolia:

In cosmology alone, the latest conference on machine learning had over 400 participants: https://indico.iap.fr/event/1/

This talk by Guilhem Lavaux gives an overview of the kind of things machine learning is used for in cosmology: https://cloud.aquila-consortium.org/s/ELxwdMx43zXnNSz

20. **Magnatolia**
April 28, 2024

Syksy Räsänen, those kinds of applications make sense to me, but I was wondering more about physicists who have made a complete 180 like Tegmark, whose public profile is now based entirely on AI in the Silicon Valley style and whose papers are all like https://arxiv.org/abs/2209.11178. (Some of his AI papers are purportedly based in physics ideas, but as far as I can tell it's only advertising)

21. **Eitan Bachmat**
April 29, 2024

Magnatolia
Neural networks were studied by physicists as brain models and for optimization from the beginning, mainly in the statistical physics community and this continues tobl this day, some names are Hopfield, Amit, Sompolinski, Tishby, Zecchina, Montanari and there are many more. These researchers though are more on the academic side, trying to model the behavior of networks using a variety of techniques, out of equilibrium, bifurcation, spin glasses…
Those on the more commercial side usually would be found on wall street or silicon valley. Then, as pointed out, AI for exploring physics is becoming huge, with most applications, not in HEP, though analyzing CERN data would make a lot of sense.

Various and Sundry

April 28, 2024
Categories: Euclidean Twistor Unification, Langlands

The semester here is coming to a close. I'm way behind writing up notes for the lectures I've been giving, which are ending with covering the details of the Standard Model. This summer I'll try to finish the notes and will be working on writing out explicitly the details of how the Standard Model works in the "right-handed" picture of the spinor geometry of spacetime that I outlined here.

At this point I need a vacation, heading soon to France for a couple weeks, then will return here and get back to work. There may be little to no blogging here for a while.

On the Langland's front, Laurent Fargues is turning his Eilenberg lectures here last fall into a book, available here. In Bonn, Peter Scholze is running a seminar on Real local Langlands as geometric Langlands on the twistor-P1

Update: One more item. Videos of talks from a conference on arithmetic geometry in honor of Helene Esnault at the IHES last week are now available. Dustin Clausen's talk covers one of my favorite topics (the Cartan model for equivariant cohomology), making use of the new formalism for handling he has developed with Scholze for handling C-infinity manifolds in a more algebraic way.

Update: Now back from vacation. While I was away, Quanta made up for its nonsense like this with a very nice article about "Weil's Rosetta Stone" and what it has to do with geometric Langlands. In the comments people have pointed to the proof of geometric Langlands that has finally been finished, and New Scientist has an article (or see Edward Frenkel on Twitter here).

Comments

1. **Bernard l'Amateur**
   April 29, 2024

   Peter,

   I'm curious to know if there is a particular reason for which you're always coming to France for vacation during the summer. Are other European countries, like Germany, Switzerland, Spain, Italy, or even Britain or Australia not interesting to you? Is it always Paris your destination here in France? Is there a math or physics conference you wish to attend here in France? Also, don't you teach in the summer at your university? Do you have 3 months of vacation since the Fall semester there starts in September?

   Best,
   Bernard

2. **Peter Woit**
   April 29, 2024

   Bernard l'Amateur,

   I do travel a fair amount, for conferences or other reasons, have been to all the countries you mention in recent years, except Australia (have never been there, would very much like to go sometime). Paris is a favorite place to spend time in, for many reasons, including the fact that I partly grew up there. I usually don't go there during the summer, since that's when it's hot, full of tourists and all the actual Parisians have left town. This year in particular there's a very good reason not to be in Paris in the middle of the summer. This trip is not for a conference.

   I don't teach during the summer, but usually spend almost all of it in New York, which I enjoy, despite some periods when the heat/humidity is a bit much. It's not vacation, I work on some combination of my own research or writing projects and dealing with tasks associated with the department computer system that I'm responsible for. Also, lots of long bike rides.

3. **Marvin**
   April 30, 2024

   Hi Peter,

   Your are saying: "…including the fact that I partly grew up there". So are you fluent in french?

4. **Mark**
   April 30, 2024

   An update on the abc conjecture:

   Kirti Joshi has posted two responses to Peter Scholze and Shinichi Mochizuki about their criticisms of Joshi's proof of the abc conjecture:
   https://mathoverflow.net/a/470293

   The documents can be found directly here:

   https://www.math.arizona.edu/%7Ekirti/response-to-Mochizuki.pdf

   https://www.math.arizona.edu/%7Ekirti/local-global-issue.pdf

   The first document is directly addressed to Mochizuki himself, while the second document seems to be addressed to the general mathematical population.

   Section 0.3 of the second document looks crucial in my eyes, since it highlights what Joshi thinks the flaw is exactly in the proof of Mochizuki's original Corollary 3.12, and also in Joshi's proof of Theorem 9.11.1, which is his version of Corollary 3.12. According to Joshi, Mochizuki's proof of his Corollary 3.12 in IUT3 is incomplete because it relies upon Mochizuki's Corollary 2.2 in IUT4 being proven first, but Mochizuki never proved Corollary 2.2 in IUT4 before proving Corollary 3.12 in IUT3 in his set of papers.

   And as Joshi admits in the same section, he organized his Constructions papers in the same way that Mochizuki did, so that the two sets of abc papers can be compared. As a result, Joshi's Theorem 9.11.1 in Constructions 3 relies on Theorem 5.7.1 in Constructions 4 to be proven first, but he put Theorem 5.7.1 after Theorem 9.11.1, so there is a gap in Joshi's proof of Theorem 9.11.1.

What I'm not currently sure of is whether Joshi's proof of Theorem 5.7.1 in Constructions 4 also relies on Theorem 9.11.1 in Constructions 3 (and similarly if Mochizuki's Corollary 2.2 in IUT4 also relies on Corollary 3.12 in IUT3). If so, then there is a circular dependency which invalidates Joshi's proof and Mochizuki's original proof of the abc conjecture. If not, then Joshi should have valued logical correctness and logical completeness above following Mochizuki's evidently poor organization of Mochizuki's own proof outline, and put his proof of [Joshi's] Theorem 5.7.1 in Constructions 4 before his Theorem 9.11.1 in Constructions 3, so as to bridge the gap that is currently in his proof of Theorem 9.11.1 in Constructions 3.

5. **Alessandro Strumia**
April 30, 2024

Good moment for a vacation; let's hope that humidity will not damage Columbia in the next weeks.

6. **Peter Woit**
April 30, 2024

Marvin,
My standard joke is that I speak French fluently but like a 12 year old (age when I came back to US).

7. **Peter Woit**
April 30, 2024

Alessandro Strumia,
Yes, good timing for a vacation.
Last thing I want to do on my vacation (or ever) is host a discussion of this here. Last few months what I was seeing was often very different than what was on the internet and now I have no first hand information.
I've found the student run site bwog.com has quite complete and accurate reporting.

8. **Z Y**
May 9, 2024

The first of a series of five papers claiming to prove geometric Langlands is on the arxiv.

9. **Samuel**
May 9, 2024

Any reaction to the new proof of the geometric Langlands conjecture?

https://people.mpim-bonn.mpg.de/gaitsgde/GLC/

10. **kitchin**
May 9, 2024

Sam Raskin gave two talks available on Youtube outlining the proof, which is many hundreds of pages. The papers include clarifications of folklore, and the precise setting for previous results, so it should be a great contribution both in content and in documentation. He says the Betti analog is more beautiful, if I recall, and that there is room for more understanding in the general case. The abstracts of the four draft papers are fun to read, with the middle paper still under re-organization (five papers total).

* Harvard CMSA, on April 9
* IAS, on May 6, announcing the actual proof

They are similar talks.

11. **Jim Holt**
May 11, 2024

Peter, now that James Simons has died, would you consider writing up a brief blog entry on Chern-Simons theory and its potential role in quantizing gravity? This is, after all, JS's greatest achievement–much greater than out-investing Buffet and Soros. Yet it's almost impossible to explain. All the obits of Simons–like the ones today (5/11/24) on the front pages of the NY Times and the Wall St. Journal–allude to Chern-Simons in passing without further comment. I myself have a nugatory understanding of the theory, despite working through much of Baez & Muniain's book on gauge fields, knots, and gravity. Please enlighten us!

12. **Peter Woit**
May 11, 2024

Jim Holt,
On vacation without a keyboard so unable to do justice to Simons or Chern-Simons. Greetings from Montpellier!

13. **Jim Holt**
May 22, 2024

I enthusiastically second Edward Frenkel's suggestion (which might seem self-serving!) that a great place to learn how the Langlands program works is Frenkel's own book "Love and Math." His exposition of it there–especially geometric Langlands–is lively and quite beautiful.

14. **Cédric Bardot**
June 1, 2024

Incidentally here is a short inquiry to provide one reason why Paris looks like a paradise for mathematic(ian)s
This is a list of cities with the largest number of winners of the Fields medal:

Paris, France – 12 winners
Princeton, United States – 7 winners
Moscow, Russia – 6 winners
Cambridge, United Kingdom – 5 winners
Zurich, Switzerland – 4 winners
Berkeley, United States – 3 winners

Göttingen, Germany – 3 winners
Harvard, United States – 3 laureates
Kyoto, Japan – 3 winners
Stanford, United States – 2 winners
Warning:
I asked Chat Mistral AI to make the list. E “counted the number of Fields Medal winners who were affiliated with an institution in each city at the time of their award. It is possible that errors or omissions were slipped”.
Data source: https://www.mathunion.org/imu-awards/fields-medal/laureates.

Wormholes, Part Deux

May 27, 2024
Categories: Wormhole Publicity Stunts

I had thought that the universally negative reaction to the fall 2022 wormhole publicity stunt meant that we'd never hear more about this, with even the editors of Quanta magazine having understood that they'd been had. While away on vacation though, I learned from Dulwich Quantum Computing that all the authors of the original stunt are back, now claiming not just wormhole teleportation, but Long-range wormhole teleportation.

I'd also thought that no one at this point could possibly think it was a good idea to help these authors go to the public with their claims about creating wormholes in a lab. It seems though that this coming weekend if you're here in NYC you can buy tickets to listen to some of them explain in person

> the mind-bending speculation that we may be able to create wormholes—tunnels through spacetime—in the laboratory.

Comments

1. **Klaus**
   May 27, 2024

   Peter, is one or several of the authors Michio Kaku writing under a pseudonym?

2. **Peter Woit**
   May 27, 2024

   Klaus,
   I suppose Kaku might have decided that his credibility would be compromised if he claimed to have actually created a wormhole, so decided to write under a pseudonym, but that's "mind-bending speculation".

3. **B.Malpani**
   June 3, 2024

   Peter: just wondering: has there really been only one comment and your reply; or have there been additional comments that you've, uh, moderated?

   Just curious if this kind of hoax has, perhaps, been normalized.

4. **Peter Woit**
   June 4, 2024

   B. Malpani,
   Lots of moderated comments, basically people making worse jokes than the first one, or insulting the authors as morons (or me as a moron for mentioning their work).

   No substantive comments moderated out. Besides the authors likely no one out there is reading their work or willing to argue for it. Most physicists think this is nonsense, and not worth talking about (probably they're right…).

   I would be quite interested to hear from anyone who went to last weekend's event and could report on what happened there.

Strings 2024

June 4, 2024
Categories: Strings 2XXX

There's been very little blogging here the past month or so. For part of the time I was on vacation, but another reason is that there just hasn't been very much to write about. Today I thought I'd start looking at the talks from this week's Strings 2024 conference.

The weird thing about this version of Strings 20XX is that it's a complete reversal of the trend of recent years to have few if any talks about strings at the Strings conference. I started off looking at the first talk, which was about something never talked about at these conferences in recent years: how to compactify string theory and get real world physics. It starts off with some amusing self-awareness, noting that this subject was several years old (and not going anywhere…) before the speaker was even born. It rapidly though becomes unfunny and depressing, with slides and slides full of endless complicated constructions, with no mention of the fact that these don't look anything like the real world, recalling Nima Arkani Hamed's recent quote:

> "String theory is spectacular. Many string theorists are wonderful. But the track record for qualitatively correct statements about the universe is really garbage"

The next day started off with Maldacena on the BFSS conjecture. This was a perfectly nice talk about an idea from 25-30 years ago about what M-theory might be that never worked out as hoped.

Coming up tomorrow is Jared Kaplan explaining:

> why it's plausible that AI systems will be better than humans at theoretical physics research by the end of the decade.

I'm generally of the opinion that AI won't be able to do really creative work in a subject like this, but have to agree that likely it will soon be able to do the kind of thing the Strings 2024 speakers are talking about better than they can.

The conference will end on Friday with Strominger and Ooguri on *The Future of String Theory*. As at all string theory conferences, they surely will explain how string theorists deserve an A+++, great progress is being made, the future is bright, etc. They have put together a list of 100 open questions. Number 83 asks what will happen now that the founders of string theory are retiring and dying off, suggesting that AI is the answer:

> train an LLM with the very best papers written by the founding members, so that it can continue to set the trend of the community.

That's all I can stand of this kind of thing for now without getting hopelessly depressed about the future. I'll try in coming weeks to write more about very different topics, and stop wasting time on the sad state of affairs of a field that long ago entered intellectual collapse.

Update: The slides for the AI talk are here. The speaker is Jared Kaplan, a Johns Hopkins theorist who is a co-founder of Anthropic and on leave working as its Chief Science Officer. His talk has a lot of generalities about AI and its very fast progress, little specifically about AI doing theoretical physics.

Comments

1. **David Roberts**
   June 4, 2024

   > train an LLM with the very best papers written by the founding members, so that it can continue to set the trend of the community.

   Did anyone who consented to this being made public know anything about LLMs??

2. **Fred J**
   June 5, 2024

   As with many AI afficionados, these guys seem to have forgotten or, (more cynically) ignored the fact that even here, the "Garbage In, Garbage Out" principle still applies.

3. **Alessandro Strumia**
   June 5, 2024

   The first talk, by Montero, addresses the key issue but its conclusions look scientifically unserious: «There's constantly new stuff going on. It's extremely exciting… We can chart the String Landscape and find where we are in it» should be replaced with «If the string landscape has ≈ 10^500 vacua we have no idea how to find if we are in it, we got stuck, it's extremely frustrating».

4. **hrdlifil**
   June 5, 2024

   It is not true that there has been nothing to blog about. I would be extremely interested in a post about the recent proof of geometric Langlands.

5. **Felix**
   June 5, 2024

   Almost as depressing as the LLM bit is the idea that the 'founding members' should have anything to do with guiding the future direction of the field. I can't think of any other subject where you could say something like that with a straight face.

6. **Peter Woit**
   June 5, 2024

   hrdlifil,
   The problem with writing about the geometric Langlands proof is that I don't know much about it, and lack the time/energy it would take to learn enough to write something useful. Here is where I need guest bloggers…

7. **Peter Woit**
   June 5, 2024

Fred J.,
What's remarkable about the string theory community is that they seem quite happy with the subject's turn to producing what Arkani-Hamed identifies as garbage, to the point of finding an automated system for producing greater amounts of garbage an attractive vision for the future.

Felix,
The idea seems to be to find someway to get around Planck's insight into scientific progress ("one funeral at a time").

8. **jim**
June 5, 2024

training LLMs and then ask them to theorize about strictily quantitative aspects is not going to lead anywhere. LLMs are pretty bad in quantitative reasoning, at least not as good as in text-related queries.

For the matter at hand, string theory and more purely theoretical subjects with deep math background, it's completely hopeless to do so. And I am sure that some people have already attempted that, feeding pdfs massively about a subject and ask questions about it. At least I did.

Also check this Nature paper:
https://www.nature.com/articles/s42254-023-00581-4
(Science in the age of large language models)

9. **PeterO**
June 5, 2024

"train an LLM with the very best papers written by the founding members, so that it can continue to set the trend of the community"

I echo the sentiment of David Roberts.

I do not understand physics enough to judge or criticise string theory. I do know about LLMs. I am in shock at the stupidity of that statement. My secondary reaction is: oh, how I'm feeling now is maybe how it has felt for you all for the past 20+ years.

10. **Cobi**
June 5, 2024

Note that Yuji Tachikawa, who is behind the question and, presumably, also the corresponding hint " Train an LLM …" has a penchant for humorous paper titles and/or acknowledgements (see e.g. 1508.06679 or the acknowledgements in 1312.2684 and 2203.16689). I am pretty sure that the suggestion to train a language model on some papers was really just a joke.

11. **David Roberts**
June 5, 2024

@Cobi

that's what I hope. It undermines what I guess is the point of the document, and given that other people are talking, in total sincerity, about AI exploring the Landscape as a way to move forward, a worrying thought that people might take it seriously.

The work I respect the most on the mathematics underlying the hypothetical M-theory (divorced from any judgement about its relation to our universe), has been such that it could not have been extrapolated from "the very best papers written by the founding members, so that it can continue to set the trend of the community."

12. **Alessandro Strumia**
June 6, 2024

Cobi, surely the Tachikawa question is just a joke, but organisers wrote "100 questions… all of them very interesting" instead of selecting really interesting questions. That's the problem.

13. **Douglas Natelson**
June 6, 2024

I don't understand how anyone can claim that LLMs as they are currently implemented can really lead to serious progress in something like theoretical physics when they manifestly are incapable of even elementary quantitative understanding. Show me a single LLM that correctly answers the question "Which weighs more, one pound of feathers or two pounds of hammers?". The latest enterprise-level ChatGPT (along with every other one I've tried) says that they weigh the same, because that question is *almost* the same wording as a famous "trick" question. When LLMs can't reliably produce the accurate statement that two is greater than one, forgive me for not having much faith that they will produce deep insights into quantum gravity.

14. **Cobi**
June 6, 2024

I didn't mean to suggest that Yuji's question was a joke, just the corresponding hint. Whether the questions should have been further curated by the organisers is i guess a matter of taste. If the purpose was to have an open brainstorming session, then not making a preselection is kind of the point.

15. **Meng**
June 6, 2024

@Douglas GPT4 answered it correctly: "Two pounds of hammers weigh more than one pound of feathers. Two pounds is always heavier than one pound, regardless of what the items are."

16. **Peter Woit**
June 6, 2024

Douglas Natelson,

I don't think the question is whether LLMs will produce deep insights into quantum gravity, but whether they can do what current string theorists are doing as well or better. "Insights into quantum gravity" is not what we're getting from current string theory research, take a look at the talks at Strings 2024 to see what we are getting.

String theorists are already claiming to use LLMs to do original string theory research, in particular setting them to work on calculating things involving huge numbers of constructions of supposed string vacua and approximations to Calabi-Yau metrics. Kaplan's talk would have been more interesting if he looked at this sort of thing and where it is going. Such calculations are part of Arkani-Hamed's "garbage", and LLMs are already happily doing them. That they will soon (if not already) be able to do them better and faster than human string theorists is quite plausible. LLMs should also be able to use the corpus of tens of thousands of worthless string theory papers to get as good as humans at generating and publishing papers, at writing grant proposals and getting them funded.

In addition, there's a huge amount of money out there looking for "AI" projects to fund, and a huge appetite from science journalists like those at Quanta for stories about the wonders of how AI will lead to great research progress.

I wish Kaplan had been talking about these topics rather than the more generic topics he chose.

17. **Peter Woit**
June 6, 2024

About the Tachikawa question, see this exchange on twitter

https://x.com/AlessandroStru4/status/1798631213168840956

I think Tachikawa is (seriously) motivated by a very real problem for string theory that Strumia points to. The generation of the "First Superstring Revolution" was trained in real physics and did their formative research in QFT, but those trained from 1984 on often skipped lightly over this and their formative years were spent mastering technical issues in string theory. I've become increasingly pessimistic that theorists who start their career with that kind of training will ever do anything other than keep working down the blind alley they were sent down at the beginning of their career.

Worse than this is that the past 20 years or more of string theorists should have been well aware that this was a dead or dying field that hadn't worked out, but decided to devote their lives to it anyway. The whole subject has gone from attracting smart intellectually ambitious young people with the talent to work on promising new ideas to attracting instead intellectually ambitious young people unable to see the difference between a promising new idea and a failed one.

18. **Douglas Natelson**
June 6, 2024

@Meng, google's Gemini got it right, after I posted. Mea culpa. GPT via enterprise-level Bing Copilot got it wrong as of this morning. Perplexity got it wrong last night and gets it correct this morning. Interesting.

@Peter, I have no doubt that certain rote-like calculations (keep grinding through order by order diagrams, where there are patterns in what is being done) could be automated well. LLMs are already quite good at spitting out python code for common tasks based on what they've scraped from github. Still, fundamental lack of math literacy has to be a showstopper for many points of interest to physicists. Anyway, enough out of me.

19. **Erich Weiss**
June 6, 2024

The explosion of new data and observation in cosmology, and theory papers resulting from it seems very promising and interesting. The work on the cosmological constant, candidates for dark matter, gravity from entanglement, etc seems quite interesting.
Much more interesting than the dead horse named String Theory.

20. **Brett Smolenski**
June 6, 2024

The notion of training current LLM architectures on theoretical physics papers and expecting anything serious to emerge is laughable; however, the notion of using an approach similar to what was done with AlphaFold, but to search for 'compactifications of string theory to get real world physics', doesn't seem unreasonable. IF such a search could be made exhaustive and came up empty, wouldn't this falsify string theory? At least it seems it would be much better to have a machine spinning its wheels doing this than physicists.

21. **Peter Woit**
June 6, 2024

Brett Smolenski,

You don't need AI to investigate the question of whether string theory makes any predictions about the real world. Just ask any of the experts working in the field. This experiment has now often been done, with the result that the honest answer is simply no. The people working on this know there are no predictions, they just believe that there's still some hope of a breakthrough which would lead to predictions.

You will sometimes find people claiming that there are predictions. In all cases though either the people doing this don't know what they're talking about, or are using a non-standard meaning for "prediction". Vafa for example these days often claims that string theory makes "predictions", but he also says that if the "prediction" turns out to be wrong it doesn't mean that there's anything wrong with string theory.

22. **Anon**
June 7, 2024

Hi Peter,

Just want to say thank you. I was an advanced theoretical physics undergrad a few years ago hoping to do string theory as research (spurred on by Brian Greene's highly irresponsible book). Your blog (and your book) made me realise the sad state of affairs in the string theory community (as exemplified in Strings 2024) and I switched to math. Now, every time that I see the status of string theory now, I want to thank you.

23. **bitboy**
June 7, 2024

Researcher in over-hyped, speculative physics pivots to researcher in over-hyped subset of AI and now claims that the latter will magically help with the former! You can't make this stuff up.

24. **Bob Y**
June 7, 2024

"LLMs should also be able to use the corpus of tens of thousands of worthless string theory papers." This statement includes a serious threat to advancement of scientific knowledge unless existing policies of citation of copyrighted materials are enforced.

LLMs do not credit original sources for learning materials that are protected by copyrights. The citation of previous work provides three benefits: 1. Potential verification or indictment of the validity of source material; 2. Credit for prior work to scholars who depend upon such credit for general reputation enhancement including promotions and grants. 3. Credit to professional societies and publishers for providing outlets for the original research work.

25. **Eric Weinstein**
June 7, 2024

Not to worry. I asked ChatGPT to speak in what it takes to be the voices of Susskind, Witten, Kaku, Motl and others regarding String Theorists' past pronouncements on the advantages of String Theory over all other approaches.

Here are the results:

———————-

ChatGPT:

"Your approach to quantum gravity is so outdated, it belongs in a history book, not a physics journal."

"Comparing your theory to String Theory is like comparing a horse-drawn carriage to a spaceship—quaint, but hopelessly obsolete."

"If your theory had any less predictive power, it would be a random number generator."

"You know, your work might have merit—in an alternate universe where String Theory doesn't dominate."

"Your calculations are charmingly quaint, like a child's attempt to explain the cosmos with crayons."

"String Theory has elegance, beauty, and predictive power. Your theory has… well, it exists, I suppose."

"I haven't seen this many dead ends since the last maze I solved. Have you considered taking up puzzles instead of physics?"

"Your attempts at a unified theory are like a high school science project compared to the grandeur of M-Theory."

"In the realm of theoretical physics, String Theory is a symphony. Your work, I'm afraid, is just background noise."

"M-Theory is the only game in town, and it looks like you're still trying to figure out the rules of checkers."

———-

Well, while I may prefer the originals, by November, this should be much closer. And, it's not that far off now of what we have to show for 41 years of String Theoretic leadership in fundamental physics.

26. **tulpoeid**
June 8, 2024

@ Bob Y,

We humans also don't cite the textbooks and papers we've read to become proficient in a field. There is nothing wrong with this.

27. **Peter Woit**
June 8, 2024

See Scott Aaronson's blog for a link to another argument that we're just a very few years away from AI going "superintelligent". Thinking again about Brett Smolenski's comment, maybe the way things will work out is that superintelligent AI will not crank out more garbage research, but quickly reach the conclusion that string theory is a failed research program not worth pursuing. If that happens, what will be the reaction of the (human) physics research community?

Before that happens, AI should be able to resolve some simpler questions about part of the string theory research program, such as
1. Is the string theory landscape just pseudoscience?
2. Does the "Swampland Program" make any sense at all?

From the last bit of Strings 2024, it seems the situation is that there will be a Strings 2025 in January in Abu Dhabi, but no one yet has stepped up to organize a Strings 2026. Maybe it won't be necessary?

28. **Stipe Galić**
June 9, 2024

Reading this Woits piece, I had a flashback to the famous Bogdanov affair, where a PhD thesis on quantum gravity of questionable quality was approved and defended. An advisor who approved the thesis topic said: 'All these were ideas that could possibly make sense. It showed some originality and some familiarity with the jargon. That's all I ask.' LLMs are excellent at 'familiarity with the jargon'. Feeding AI hype into the string machine? Sounds like a lot of funding.

29. **John Baez**
June 10, 2024

Sadly that person was Roman Jackiw. I imagine he was back-pedalling furiously.

30. **bob**
June 17, 2024

@Smolensky: "an approach similar to what was done with AlphaFold, but to search for 'compactifications of string theory to get real world physics', doesn't seem unreasonable."

But you can test (in vitro/cristallography/ect) the results of AlphaFold.
And the fortune of AlphaFold (now AlphaFold3) is that is very very close to wetlab results.

31. **zzz**
June 17, 2024

alphafold and the protein structure problem is different , there are many thousands of solved proteins to learn from. in string theory there is nothing like that.

The Mystery of Spin

June 13, 2024
Categories: Uncategorized

The following makes no claims to originality or any physical significance on its own. For a better explanation of some of the math and the physical significance of the use of quaternions here, see this lecture by John Baez.

I've been spending a lot of time thinking about spinors and vectors in four dimensions, where I do think there is some important physical significance to the kind of issue discussed here. See chapter 10 here for something about four dimensions. A project for the rest of the semester is to write a lot more about this four-dimensional story.

Until recently I was very fond of the following argument: in three dimensions the relation between spinors and vectors is very simple, with spinors the more fundamental objects. If one uses the double cover $SU(2)=Spin(3)$ of the rotation group $SO(3)$, the spinor (S) and vector (V) representations satisfy
$$ S\otimes S = \mathbf 1 \oplus V$$
which is just the fact well-known to physicists that if you take the tensor product of two spinor representations, you get a scalar and a vector. The spinors are more fundamental, since you can construct V using S, but not the other way around.

I still think spinor geometry is more fundamental than geometry based on vectors. But it's become increasingly clear to me that there is something quite subtle going on here. The spinor representation is on $S=\mathbf C^2$, but one wants the vector representation to be on $V_{\mathbf R}=\mathbf R^3$, not on its complexification $V=\mathbf C^3$, which is what one gets by taking the tensor product of spinors.

To get a $V_{\mathbf R}$ from V, one needs an extra piece of structure: a real conjugation on V. This is a map
$$\sigma:V\rightarrow V$$
which

- commutes with the $SU(2)$ action
- is antilinear
$$ \sigma(\lambda v)=\overline{\lambda}\sigma(v)$$
- satisfies $\sigma^2=\mathbf 1$

$V_{\mathbf R}$ is then the conjugation-invariant subset of V.

If we were interested not in usual 3d Euclidean geometry and $Spin(3)$, but in the geometry of $\mathbf R^3$ with an inner product of $(2,1)$ signature, then the rotation group would be the time-orientation preserving subgroup $SO^+(2,1)\subset SO(2,1)$, with double cover $SL(2,\mathbf R)$. In this case the usual complex conjugations on $\mathbf C^2$ and $\mathbf C^3$ provide real conjugation maps that pick out real spinor ($S_{\mathbf R}=\mathbf R^2\subset S$) and vector $(V_{\mathbf R}=\mathbf R^3\subset V=S\otimes S)$ representations.

For the case of Euclidean geometry and $Spin(3)$, there is no possible real conjugation map σ on S, and while there is a real conjugation map on V, it is not complex conjugation. To better understand what is going on, one can introduce the quaternions $\mathbf H$, and understand the spin representation in terms of them. The spin group $Spin(3)=SU(2)$ is the group $Sp(1)$ of unit-length quaternions and the spin representation on $S=\mathbf H$ is just the action on $s\in S$ of a unit quaternion q by left multiplication
$$s\rightarrow qs$$
(we could instead define things using right multiplication).

There is an action of $\mathbf H$ on S commuting with the spin representation, the right action on S by elements $x\in \mathbf H$ according to
$$s\rightarrow s\overline{q}$$
(this is a right action since $\overline {q_1q_2}=\overline q_2\ \overline q_1$).

This quaternionic version of the spin representation is a complex representation of the spin group, since the right action by the quaternion $\mathbf i$ provides a complex structure on $S=\mathbf H$. While there are no real conjugation maps σ on the spin representation S, there is instead a quaternionic conjugation map, meaning an anti-linear map τ commuting with the spin representation and satisfying $\tau^2=-\mathbf 1$. An example is given by right multiplication by $\mathbf j$
$$\tau (q)=q\mathbf j$$
Note that in the above we could have replaced $\mathbf i$ by any unit-length purely imaginary quaternion and $\mathbf j$ by any other unit-length purely imaginary quaternion anticommuting with the first.

In general, a representation of a group G on a complex vector space V is called

- A real representation if there is a real conjugation σ. In this case the group acts on the σ-invariant subspace $V_\mathbf R\subset V$ and V is the complexification of $V_\mathbf R$.
- A quaternionic representation if there is a quaternionic conjugation τ. In this case τ makes V a quaternionic vector space, in a way that commutes with the group action.

Returning to our original situation of the relation $S\otimes S= 1 \oplus V$ between complex representations, S is a quaternionic representation, with a quaternionic conjugation τ. Applying τ to both terms of the tensor product the minus signs cancel and one gets a real conjugation σ on V.

What's a bit mysterious is not the above, but the fact that when we do quantum mechanics, we have to work with complex numbers, not quaternions. We then have to find a consistent way to replace quaternions by complex two by two matrices when they are rotations and and complex column vectors when they are spinors (so $S=\mathbf C^2$ rather than $\mathbf H$).

In my book on QM and representation theory I use a standard sort of choice that identifies $\mathbf i,\mathbf j,\mathbf k$ with corresponding Pauli matrices (up to a factor of i):
$$1\leftrightarrow \mathbf 1=\begin{pmatrix}1&0\\0&1\end{pmatrix},\ \ \mathbf i\leftrightarrow -i\sigma_1=\begin{pmatrix}0&-i\\ -i&0\end{pmatrix},\ \ \mathbf j\leftrightarrow -i\sigma_2=\begin{pmatrix}0&-1\\ 1&0\end{pmatrix}$$
$$\mathbf k\leftrightarrow -i\sigma_3=\begin{pmatrix}-i&0\\ 0&i\end{pmatrix}$$
or equivalently identifies
$$q=q_0 +q_1\mathbf i +q_2\mathbf j + q_3\mathbf k \leftrightarrow \begin{pmatrix}q_0-iq_3&-q_2-iq_1\\q_2-iq_1 &q_0 +iq_3\end{pmatrix}$$

Note that this particular choice incorporates the physicist's traditional convention distinguishing the 3-direction as the one for which the spin matrix is diagonalized.

The subtle problem here is the same one discussed above. Just as the vector representation is complex with a non-obvious real conjugation, here complex matrices give not $\mathbf H$ but its complexification
$$M(2,\mathbf C)=\mathbf H\otimes_{\mathbf R}\mathbf C$$

Note added: complexified quaternions are often called "biquaternions"
The real conjugation is not complex conjugation, but the non-obvious map
$$\sigma (\begin{pmatrix}\alpha&\beta \\ \gamma & \delta\end{pmatrix})= \begin{pmatrix}\overline\delta &-\overline\gamma \\ -\overline\beta & \overline\alpha \end{pmatrix}$$

Among mathematicians (see for example Keith Conrad's Quaternion Algebras), a standard way to consistently identify $\mathbf H$ with a subset of complex matrices as well as with $\mathbf C^2$, (giving the spinor representation) is the following:

- Identify $\mathbf C\subset \mathbf H$ as
$$z=x+iy\in \mathbf C \leftrightarrow x+\mathbf i y \in \mathbf H$$
- Identify $\mathbf H$ as a complex vector space with $\mathbf C^2$ by
$$q=z +\mathbf j w \leftrightarrow \begin{pmatrix}z\\ w\end{pmatrix}$$
Note that one needs to be careful about the order of multiplication when writing quaternions this way (where multiplication by a complex number is on the right), since
$$z+w\mathbf j= z+\mathbf j\overline w$$
- Identify $\mathbf H$ as a subset of $M(2,\mathbf C)$ by
$$q=z +\mathbf jw \leftrightarrow \begin{pmatrix}z&-\overline{w}\\ w& \overline z\end{pmatrix}$$

  This is determined by requiring that multiplication of quaternions in the spinor story correspond correctly to multiplication of an element of $\mathbf C^2$ by a matrix.

  With this identification
$$\mathbf i\leftrightarrow \begin{pmatrix}i&0\\ 0&-i\end{pmatrix},\ \ \mathbf j\leftrightarrow \begin{pmatrix}0&-1\\ 1&0\end{pmatrix},\ \ \mathbf k\leftrightarrow \begin{pmatrix} 0&-i\\ -i&0\end{pmatrix}$$

  This is a bit different than the Pauli matrix version above, but shares the same real conjugation map identifying $\mathbf H$ as a subset of $M(2,\mathbf C)$.

Update: There's a very new video here, where Keith Conrad discusses quaternions, especially the case of quaternion algebras over $\mathbf Q$ and their relation to quadratic reciprocity.

Comments

1. **john**
   June 13, 2024

   I have been following this series called "Spinors for Beginners" and they are up to video 20 in the series which covers the Lorentz Group. It's good background information to help me understand the spinor topics in your blog posts.

   https://www.youtube.com/watch?v=tztQrSRF_Ds&list=PLJHszsWbB6hoOo_wMb0b6T44KM_ABZtBs&index=21

2. **peter cameron**
   June 13, 2024

   Peter – You open your remarks with
   "…the fact well-known to physicists that if you take the tensor product of two spinor representations, you get a scalar and a vector. The spinors are more fundamental, since you can construct V using S but not the other way around."

   In geometric representation of Clifford algebra, wavefunction interactions can be modeled by the Clifford product, sum of dimension-reducing dot and dimension-increasing wedge products. So product of two vectors is scalar plus bivector, where bivector is what i think you are calling a spinor. This seems to be the inverse of what you say.

   In geometric representation of Clifford algebra, product of two bivector spinors is scalar plus quadvector. No one-dimensional vector there.

   Please can you explain to me how our perspectives seem inverted in this?

3. **Garrett Lisi**
   June 14, 2024

   The dance between spinors and biquaternions makes an appearance in a recent short paper I wrote, "C, P, T, and Triality." I would send you the hep-th arXiv link but the paper has been "on hold" for five weeks now. This is especially annoying since I don't think there's anything technically wrong with the paper. I strongly suspect one or more arXiv moderators just doesn't like me or my work so I'm getting special treatment.

4. **David Roberts**
   June 14, 2024

   @Garrett

   why not also host it elsewhere until then so people can see it?

5. **Garrett Lisi**
   June 14, 2024

   The arXiv is where it belongs, and the most reasonable place people would see it and possibly cite it. If they reject it, which I see no good reason for, maybe I'll retract all my papers in disgust.

6. **Peter Woit**
   June 14, 2024

   Garrett,

   Sorry to hear that. I hope anyone reading this who is in the good graces of the arXiv moderators will contact them to ask them to fix this.

   I'll also add to the posting what I should have originally had there, that "biquaternions" is a standard name for the complexification of the quaternions.

7. **Peter Woit**
June 14, 2024

Peter Cameron,
A bivector is not a spinor.
What I'm writing about in this posting is specific to 3d, but there's a very general story explaining what you bring up, simplest in even dimensions.

In dimension d=2n, the Clifford algebra (which includes vectors, your bivectors, etc), when complexified (allow complex coefficients) is isomorphic to the matrix algebra $M(2^n,\mathbf C)$. Spinors are column vectors $\mathbf C^n$ that these matrices act on. More invariantly, the Clifford algebra is the algebra of linear transformations of the spinor space S.

What you can build out of vectors is not spinors, but linear transformations of spinors. Since the linear maps on S are isomorphic to the tensor product $S\otimes S^*$, the general story is that (in even dimensions) the spinors are not something you can build out of vectors, but a "square root" of what you can build out of vectors (the complexified Clifford algebra).

8. **David Roberts**
June 14, 2024

@Garrett

I'm not saying don't keep pushing to get it on the arXiv, I'm saying pop it on your own website for now (or somewhere, anywhere, public), so that people can judge for themselves the content of the paper, now that you've just advertised it on a widely-read blog…. By doing nothing you're giving the arXiv moderation process the power to keep the paper out of sight.

9. **Garrett Lisi**
June 14, 2024

@David
Very well, I've made it available here:
https://deferentialgeometry.org/works/CPTaT.pdf

10. **flippiefanus**
June 15, 2024

"I still think spinor geometry is more fundamental than geometry based on vectors."
There is a subtlety with this notion that has frustrated me for a long time. It is the fact that the formulation of geometry in terms of vectors allows shift invariance (because there is nothing physically special about the choice of the origin), while the formulation in terms of spinors does not seem to make shift invariance so apparent. The choice of origin is somehow encoded into the associated spinors that cannot be so easily accessed. Perhaps you know of a way around this problem.

11. **Peter Woit**
June 15, 2024

flippiefanus,

Yes, vectors are essentially the same as shifts, they generate translations (momentum is a vector). If you define vectors in terms of spinors, nothing changes about translations. But if you express your vectors in terms of spinors (e.g. as matrices), you have some new things. In particular you have the Dirac operator, which is a vector, but acts on spinors.

In the usual discussion, the Dirac operator is written $D=\gamma^\mu \frac{\partial}{\partial x_\mu}$ which looks like it is a scalar. It really is a vector though…

12. **Robert Wilson**
June 16, 2024

To a mathematician, a spinor is not an element of C^2, but an element of H. Then you have absolutely no problem getting a real vector out of the square of the spinor, because you take the quaternionic tensor product of two 1-dimensional quaternion representations, which is automatically a 4-dimensional real representation.

13. **Robert A. Wilson**
June 16, 2024

The main problem, I think, with regarding a spinor as living in C^2 rather than H, is that it picks out a particular direction of spin as being somehow special. The question then is, which direction is it? This is not an easy question to answer. But it is an important question to ask. So I don't understand why physicists try to insist that it isn't a question.

14. **Peter Woit**
June 16, 2024

Robert A. Wilson,
If you're doing QM with spin, you need to have a complex state space. In particular, if you want to couple to EM, you need a distinguished choice of how the complex numbers act. I do suspect there's a mystery there.

15. **Robert A. Wilson**
June 16, 2024

Yes, I agree there is a bit of a mystery there – why does the mathematical symmetry of the quaternions need to be broken to C^2 to describe observed physics? There is no doubt that it does need to be broken, but why? And how?

16. **GS**
June 18, 2024

I think there are typos in your definition of "antilinear". I think it should be
\sigma (\lambda v) = \bar{\lambda} \sigma (v)

17. **Peter Woit**
June 19, 2024

GS,
Thanks! fixed.

18. **Peter Woit**
June 19, 2024

    testing

19. **akhmeteli**
June 30, 2024

    Peter Woit,

    "A bivector is not a spinor."

    For what it's worth, according to E. Cartan, The Theory of Spinors (Dover, 1981), Section 154, in the space of special relativity, "the semi-spinor is, then, the polarised bivector". See also E.T. Whittaker, On the relations of the tensor-calculus to the spinor-calculus. Proc. Roy. Soc. A 158, 38–46 (1937).

20. **Peter Woit**
June 30, 2024

    akhmeteli,
    The Whittaker reference makes clear that this is about the Klein correspondence, which is at the basis of the idea of twistors.
    What's going on is you can identify the Grassmannian of complex two planes (the half-spinor space at a point) in complex four dimensions (twistor space) with the space of two-forms satisfying a particular equation. So, yes, you can relate spinors to certain two form ("bivectors"). But, spinors are not bivectors, this is about special complex bivectors, and is very special to four dimensions.

Latest Breakthrough From String Theory

June 19, 2024
Categories: This Week's Hype

PRL has just published this paper (preprint here), with associated press release here. The press release explains that the authors have discovered how to use string theory to provide "an easier way to extract pi from calculations involved in deciphering processes like the quantum scattering of high-energy particles."

The press release has led to stories here, here and here, as well as commentary from Sabine Hossenfelder.

As for applications of this, the press release refers to Positron Emission Tomography, while one of the stories linked above gives the more modest explanation of what this is good for:

> The series found by IISc researchers combines specific parameters in such a way that scientists can rapidly arrive at the value of pi, which can then be incorporated in calculations, like those involved in deciphering scattering of high-energy particles, the release said.

Update: This just gets more and more idiotic as the press stories multiply. India Today now has Indian physicists untangle new pi series that could change maths forever. It would be helpful if the people who issued this press release had some sense of shame and had it withdrawn.

Comments

1. **PHL**
   June 19, 2024

   "The series found by IISc researchers combines specific parameters in such a way that scientists can rapidly arrive at the value of pi, which can then be incorporated in calculations".

   Is this serious? A new series for pi is interesting, especially if it converges significantly faster than any other series already known for pi. But, the comment above is ridiculous.

2. **Peter Woit**
   June 19, 2024

   PHL,
   There's no claim this converges faster than any other known series. I suspect these series are not something really new, they are not being published in a math journal where relevant experts would have looked at such a claim.

3. **HJM**
   June 19, 2024

   I think it does not converge faster than the Chudnovsky algorithm.
   To get 10 decimals of pi:
   – Chudnovsky series: 1 term
   – Series from the string theory paper: roughly 10 terms

4. **David Roberts**
   June 19, 2024

   That's painful.

5. **SM**
   June 20, 2024

   The precise claim made in the preprint (p. 9) is that the new series converges faster than the Madhava series for pi. Nowhere in the paper has it been claimed that it converges faster than all known algorithms.

   https://en.wikipedia.org/wiki/Leibniz_formula_for_%CF%80

6. **NoGo**
   June 20, 2024

   From one of the stories:

   "He added that there have been many derivations of formulae over time. The formula the team has found is close to what was found by Indian mathematician Sangamagrama Madhava, which was written in the 15th century, in a poetic language."

   So what exactly that team discovered? (Apart from a 15-century poem). What they broke through?

7. **clueless_postdoc**
   June 20, 2024

   It seems the calculation of \pi is just something they fed to the press story to attract the public's attention to this work, and many statements written by the press seem quite silly. Would anyone from the field mind commenting on the significance of the paper (and why it's accepted by PRL – presumably not because they managed to calculate \pi)

8. **Peter Woit**
   June 20, 2024

   clueless_postdoc,

   An excellent question, I was wondering the same. Seems mystifying why this was accepted by PRL, ignoring the pi nonsense.

Another question about PRL. They encourage people to issue press releases when a paper is published, but do they ever do anything when authors issue press releases misrepresenting what PRL published?

9. **Will Orrick**
June 21, 2024

The press coverage is cringeworthy, but ridiculing the work without looking at isn't terribly informative. The authors seem to have series for zeta(2) and pi that contain a free complex parameter. In other words, within the radius of convergence the values of the series are independent of the parameter. When the parameter is specialized to a particular value, each series reduces to a classical form: in the case of zeta(2) taking the parameter to zero gives the series of the Basel problem; for pi taking the parameter to infinity gives the Madhava series (a specialization of the series expansion of the arctan function). The classical series are extremely slow to converge. For other values of the parameter convergence can be vastly faster, but still, apparently, very slow relative to the state of the art. Whatever the significance of these series may be, it's probably not going to be in numerical analysis.

I find it very hard to tell with these kinds of things whether the result is new, but it very well could be. Every person listed in the paper's acknowledgments section appears to be a physicist, so it's not clear whether the authors reached out to any mathematicians prior to publishing.

10. **Peter Woit**
June 21, 2024

Will Orrick,
I did read the paper, and it's ridiculous. The fact that they have written down a series for zeta(2) depending on a complex parameter is not in itself anything interesting. There's zero evidence this series is any kind of advance in analytic number theory (much less one that "could change maths forever"), since there's zero evidence that they bothered to consult with anyone who knows anything about analytic number theory.

11. **Concerned Indian mathematician**
June 21, 2024

India at the moment is hyper-nationalistic, so such exaggerated news coverages aren't uncommon (in fact they are extremely common). So this coverage by Indian media is mostly a stance towards exaggerating Indian contributions to science (in the sense that the scientists are Hindus working at an Indian university rather than a foreign one.)
This is not to say that there aren't other legitimate contributions by Indians to the Sciences, but India Today is well know to be biased twards the hypernationalism mentioned above.

12. **Martin**
June 22, 2024

They say that they can deduce the identity for zeta(2), but they don't show how they did it. They also say that for lambda=0 the expression becomes the usual series for zeta(2), but it seems undefined for lambda=0.

13. **AnotherMartin**
June 23, 2024

I don't see what the problem with this is. It doesn't appear to me that they are claiming that this finding will revolutionize anything. If you think that it belongs to a hallway chat rather than an online publication, then I agree. But perhaps the same goes for this mockery of their work?

14. **kodlu**
June 26, 2024

Good followup on mathoverflow

https://mathoverflow.net/questions/473931/possible-new-series-for-pi

and mathstackexchange

https://math.stackexchange.com/questions/4937730/is-the-new-series-for-a-big-or-even-medium-deal

15. **Timothy Chow**
June 26, 2024

I was the one who posted on MathOverflow, asking if the series is new. It's a little soon to say for sure, but Jesús Guillera is one of the people I would think would recognize the series if it were known, and he didn't recognize it. I agree with Will Orrick's sentiment. There's no reason to react either positively or negatively to the hype; one should just ignore that, and evaluate the mathematical result on its own merits. The formula is not particularly suited for computation, but it could be interesting for other reasons that remain to be discovered.

By the way, "analytic number theory" is probably not quite the right keyword for this type of thing. "Transcendental number theory" is closer, but the experts who are most likely to recognize such a formula are those with an interest in experimental mathematics, high-precision computation, and infinite series. Or possibly special functions.

16. **Protomon**
June 27, 2024

Concerned Indian mathematician, is there reason to think there's a particular 'Hindutva' angle behind this and that it's not just the usual failing of the scientific press? I know Hindutva has successfully pushed many ahistorical narratives on math and science history into schools and popular thought, but in general I thought contemporary research is very under-emphasized by the Hindutva movement/current Indian government.

17. **Peter Woit**
June 28, 2024

All,
I've deleted most of the incoming comments that want to argue about the current situation in India, probably should have deleted anything referring to this at all, will do so in the future.

18. **Anonymous**
July 3, 2024

Some discussion here as well –

https://youtu.be/rGd7Db52w1Q?si=rs7kP1WuHSMt40FU

19. **Timothy Chow**
July 19, 2024

The YouTube channel Numberphile has just posted an interview with Sinha and Saha.

https://www.youtube.com/watch?v=2lvTjEZ-bbw

In light of some of the discussion here, it's worth pointing out that starting at 8:35, they mention that they consulted various mathematicians prior to publication, including Terence Tao.

20. **Peter Woit**
July 19, 2024

Timothy Chow,

What they actually say is that they consulted one mathematician, a friend of theirs (he supposedly consulted others, including Terry Tao), and he hadn't seen the formula. Also evidently they put a question on math stack exchange (after the paper had been submitted for publication) which no one responded to, see https://math.stackexchange.com/questions/4876995/parametric-representation-of-the-euler-beta-function-zeta-functions-and-pi

They also admit that a lot of the press coverage was nonsense, but take no responsibility at all for the misleading press release that was issued in their name and the nonsense that was in it (they say the press release was the idea of their dean, not theirs).

21. **Timothy Chow**
July 22, 2024

Peter, what exactly in the press release itself (as opposed to subsequent stories that were written based on the press release) do you consider to be nonsense? I see you mention PET, but PET is mentioned only as an example of an application of some theoretical ideas by Dirac that he did not expect to have applications. There is no claim that their own work has practical applications, to PET or anything else; quite the opposite. I don't see anything in the press release (at least regarding pi) that I consider to be "nonsense," and I feel confident that Sinha and Saha stand behind it (in contrast to your claim that they "take no responsibility at all" for it). Also you made a comment that you suspect that the series is not new, but the evidence so far (from MathOverflow) seems to be that it is new. Finally, you said that there's "zero evidence" that they consulted with anyone, which may have been true at the time you made the comment, but we now know that they did consult with at least one person who then consulted with others.

22. **Peter Woit**
July 22, 2024

Timothy Chow,
About what is nonsense, I addressed that already in the posting:
1. "an easier way to extract pi from calculations involved in deciphering processes like the quantum scattering of high-energy particles."
Besides being confusing, the "easier way to extract pi" claim is simply nonsense intended to mislead the reader.
2. Bringing positron emission tomography into it is complete nonsense, again, intended to mislead.

Another example:
3. "The series that Sinha and Saha have stumbled upon combines specific parameters in such a way that scientists can rapidly arrive at the value of pi, which can then be incorporated in calculations, like those involved in deciphering scattering of high-energy particles."
This is complete nonsense: the signficance of a a new series for pi is that you can calculate pi more quickly which helps your scattering amplitude calculations????
Whoever wrote it had no understanding of what they were writing. It reappears in lots of the press stories.

As for whether the series is "new", I can take any known series for pi, do a random transformation of variables and get a "new" series for pi. The question is whether it's "new" in an interesting way. I'm guessing also that the question of whether the series is interesting is more relevant for the series they write down for the zeta function, where the reason for interest in such a series is clearer (very unclear why a series for pi is interesting, other than the technical reason of faster convergence, which isn't a reason here).

The "zero evidence" they consulted anyone statement was accurate. Now a more accurate statement would be that they only consulted one person (Apoorva Khare), who had no expertise on such series expansions. Note that they thanked a lot of people in the acknowledgements, not including Khare. I'm guessing they only asked him about this after the paper was submitted (like the question on stack exchange).

I don't know what evidence you have that these two "stand behind" the press release. In the video here's their explanation of how it happened
"When the paper got published we sent it to our divisional chairman and director, both of whom are mathematicians. Both of them had good things to say, especially our divisional chairman. He said that we should actually make a press release focusing on pi so that's how this story centered around pi originated. I don't think it would have actually occurred to either of us to actually try to sell to the lay person that "look, we found a new formula for pi"."
He's very explicitly saying "this kind of publicity seeking is not something I would do, I did it because my divisional chairman told me to". There's also a lot of discussion about how much of the press coverage was misinformation. The obvious question of why that happened isn't addressed in the interview. The obvious answer is that the press coverage was misinformation because the press release was misinformation.

23. **Timothy Chow**
July 22, 2024

Peter,

It seems obvious to me that Sinha and Saha stand behind their press release, because in the interview they had ample opportunity to disavow or repudiate it, but they didn't. (It seems you agree with me that they didn't repudiate the press release, since their non-repudiation is one of your complaints.) True, the press release wasn't their idea in the first place, and they had mixed feelings about the reaction, but they never retracted anything they said, or said they wish they hadn't done it. But I won't continue to debate the point; if we really care, we could contact Sinha and Saha directly and ask them whether they stand behind their press release.

The things you call "nonsense" seem to me to be par for the course when it comes to science journalism. Perhaps I'm just more tolerant than most scientists on this score. In the long run, I find that the hyper-critical attitude toward science journalism that is so common in the scientific community amounts to shooting ourselves in the foot. For example, I disagree that "bringing positron emission tomography into it is complete nonsense." It's no more nonsense than saying, in response to whether a new theorem in number theory has practical applications, that there are currently no known applications, but that G. H. Hardy once boasted that number theory has no applications and yet much number theory has found applications in cryptography. Bringing up G. H. Hardy and cryptography is cliched, but it's not nonsense.

As for the series for pi itself, I take the attitude that much of mathematics amounts to the search for hidden structure. Any new identity involving a fundamental constant is potentially the tip of some iceberg, a small hint that some large undiscovered structure lies hidden beneath the surface. Chasing down such hints doesn't always work out, of course, but it's a pretty good heuristic. As an analogy, my Ph.D. thesis advisor Richard Stanley once wrote a survey paper on log-concave and unimodal sequences in algebra, combinatorics, and geometry. I used to be puzzled why people would invest so much effort into studying, and trying to prove, unimodality. What's so interesting about a unimodal sequence? It was only later that I understood that unimodality is usually interesting not so much for its own sake, but because it is so often a sign of interesting structure that lies hidden beneath the surface. Stanley doesn't make this "iceberg" claim explicitly in his paper, but if you read his survey with this thought in mind, then the examples he discusses provide ample evidence for icebergs.

Returning to identities for pi, there are boatloads of identities for pi that have been discovered empirically, and I think one would have to be a diehard skeptic not to believe that there must be some underlying theory that explains why these identities exist. In many cases, the identities are either unproved or the only known proof is a "mechanical" proof using the WZ method. These identities may not be so interesting for their own sake, but they are begging for an explanation (note that "explanation" and "proof" are not synonymous). A big reason why Ramanujan has had such an enduring legacy is that so many of his discoveries (including formulas involving pi) have pointed the way to previously unsuspected mathematical structures.

The Sinha-Saha identity was surprising to several of the experts who saw it on MathOverflow. I therefore take it as a working assumption that there is an iceberg, until someone shows otherwise.

Physical Intuition vs. “Math”

June 29, 2024
Categories: Uncategorized

A common theme in discussions online of the problems of fundamental theoretical physics is that the subject has gotten “lost in math”, losing touch with “physical intuition”. In such discussions, when people refer to “math” it’s hard to figure out what they mean by this. In the case of Sabine Hossenfelder’s “Lost in Math” you can read her book and get some idea of what specifically she is referring to, but usually the references to “math” don’t come with any way of finding out what the person using the term means by it. Here I’ll mostly leave “math” in quotation marks, since the interesting issue of what this means is not being addressed.

“Physical intuition” is also a term whose meaning is not so clear. Sometimes I see it used in an obviously naive way, referring to our understanding of the physical world that comes from our everyday interaction with it and the feeling this gives us for how classical mechanics, electromagnetism, thermodynamics work. Some people are quite devoted to the idea that this is the way to understand fundamental physics, sometimes taking this as far as skepticism about subjects like quantum mechanics.

Usually though, the term is not being used in this naive sense, but as meaning something more like “the sort of understanding of physical phenomena someone has who has spent a great deal of time working out many examples of how to apply physics theory, so can use this to see patterns and guess how some new example will work out”. This is contrasted to the person lacking such intuition, who will have to fall back on “math”, in this situation meaning writing down the general textbook equations and mathematically manipulating them to produce an answer appropriate for the given example, without any intuitive understanding of the result of the calculation. This is what we expect to see in students who are just learning a new subject, haven’t yet worked out enough examples to have the right intuition.

If the question though is not how to apply well understood fundamental theory to a new example, but how to come up with a better fundamental theory, I’d like to make the provocative claim that “physical intuition” is not going to be that helpful. New breakthroughs in fundamental theory have the characteristic of being unexpectedly different than earlier theory. The best way to come up with such breakthroughs is from new experimental results that conflict with the standard theory and point to a better one. But, what if you don’t have such results? It seems to me that in that case your best hope is “math”.

Here’s a list of the great breakthroughs of fundamental physics in the 20th century, with some comments on the role of “physical intuition” and “math”.

- **Special relativity**: According to physical intuition, if I’m emitting a light ray and speed up in its direction, so will the the speed of the light ray. The crucial input was from experiment (Michelson-Morley), which showed that light always travels at the same speed. Finding a sensible theory of mechanics with this property was largely “math”.
- **General relativity**: There’s a long argument about the role of “math” here, but I think the only way to develop “physical intuition” about curved spacetime is to start by learning Riemannian geometry (which Einstein did).
- **Quantum mechanics**: Here again, a crucial role was played by experimental results, those on atomic spectra. A large part of the development of the subject was applying “math” to the mysterious spectra for which there was zero “physical intuition”. Later on, a better understanding of the theory and better calculational methods involved bringing in a large amount of new “math” to physics, especially the theory of unitary representations of groups.
- **Yang-Mills theory**: This was pretty much pure “math”: replacing a U(1) gauge theory by an SU(2) gauge theory.
- G**ell-Mann’s eight-fold way**: Pure “math”.
- **The Anderson-Higgs mechanism**: The funny thing here is that Anderson did get this out of “physical intuition”, based on what he knew from superconductivity. Particle theorists ignored him (especially when it came time for a Nobel Prize), and their papers about this were often mainly “math”, more specifically argumentation about how the mathematics of gauge symmetry could give a loophole to a theorem (the Goldstone theorem).
- **The unified electroweak theory**: Looks to me more like “math” than “physical intuition”.
- **QCD and asymptotic freedom**: David Gross famously had the “physical intuition” that the effective coupling grows in the ultraviolet for all QFTs, based on experience with a wide range of examples. He set a mathematical problem for his student (Frank Wilczek), and when the “mathematics” was finally sorted out, they realized the usual physical intuition for QFTs had to be replaced by something completely different.

Making a list instead of the great disasters of 20th century theoretical physics, there’s

- **Supersymmetry**: OK, this one is “math”. I suspect though that the problem here is that the “math” is not quite right, but missing some other needed new ideas.
- **String theory**: As we’re told in countless books and TV programs, this starts with a new “physical intuition”: instead of taking point particles as primitive objects, take the vibrational modes of a vibrating string. Developing the implications of this certainly involves a lot of “math”, but the new fundamental idea is a physical one (and it’s wrong, but that’s a different story…).

Comments

1. **lun**
   June 29, 2024

   Let’s expand some of these a little bit though:
   Special relativity–>Logic shows that relativity (the Galilean kind) is in conflict with electromagnetism and Galilei’s transformations, intuition shows the third must go and be substituted by something that respects the first two, math was used to derive that.

   General Relativity–>Logic shows gravity must be updated to take special relativity into account and the most naive implementation will break the equivalence principle, intuition says the equivalence principle is fundamental and one must find a way to preserve it, logic and some thought experiments (Ehrenfests’s paradox) say that because of time dilation this inevitably involves Riemannian geometry, maths follows

   Quantum mechanics—>Experiment gives funny results, intuition shows that to explain them one needs some seemingly counter-intuitive ansatze (Bohr’s momentum quantization, the photoelectric effect), which are then progressively systematized using more and more sophisticated maths.

   Yang Mills/Gell-mann/Electroweak theory—>Intuition shows that perhaps it is worth to extent the techniques we used to classify spin to systems where there seem to be approximate degeneracies analogous to spin (isospin/strangeness), group theory follows.
   Intuition also guided the analogy between strong and weak isospin

   Asymptotic freedom—->Before Gross there was the parton model, with a mix of intuition and phenomenological maths by Feynman,Bjorken etc showing that “partons” weakly coupled at high Q^2 exhibit the sort of scaling seen experimentally.

   Higgs—>The necessity of renormalizability follows the deeply intuitive work of Wilson, through there is quite a lot of sophisticated maths to connect this to Gauge symmetry.
   The result that a theory broken by the Higgs mechanism is still renormalizeable is intuitive (the Higgs is in the IR, gauge symmetry needs to operate in the UV) but needs a lot of sophisticated maths to prove.

   In each case intuition has a crucial role in selecting what math to pick. The set of consistent mathematical systems is almost certainly infinitely larger than the set of consistent mathematical systems relevant to physics. So it is an interplay between the two.

2. **Anonyrat**
June 29, 2024

The path integral, Feynman diagrams, the Parton model are more physical intuition than math.

3. **Anonyrat**
June 29, 2024

Rohrlich, F. The unreasonable effectiveness of physical intuition: Success while ignoring objections. Found Phys 26, 1617–1626 (1996).
https://doi.org/10.1007/BF02282125

Abstract

The process of theory development in physics is a very complex one. The best scientists sometimes proceed on the basis of their physical intuition, ignoring serious conceptual or mathematical objections well known to them at the time.The results soon justify their actions: but the removal of these objections is often not possible for a very long time. Four examples are presented: Newton, Schrödinger, Dirac, Dyson. Some thoughts on this “unreasonableness≓ are offered.

4. **anon**
June 29, 2024

Why are you acting like these are mutually exclusive? It’s clear to me that new ideas always come from a combination of qualitative and quantitative reasoning, in support of each other.

Einstein had the qualitative ideas that led to General Relativity in mind long before he learned Riemannian geometry. He also said that “every true theorist is a kind of tamed metaphysicist.” The taming here is the translation of qualitative insight (“physical intuition”) into quantitative form (“math”).

5. **Sabine**
June 30, 2024

“Steven Weinberg, who was awarded a Nobel Prize for unifying the electromagnetic and weak interaction, likes to make an analogy with horse breeding: “[The horse breeder] looks at a horse and says “That’s a beautiful horse.” While he or she may be expressing a purely aesthetic emotion, I think there’s more to it than that. The horse breeder has seen lots of horses, and from experience with horses knows that that’s the kind of horse that wins races.”

But like experience with horses doesn’t help when building a racing car, experience with last century’s theories might not be of much help conceiving better ones. ”

That’s a quote from my book “Lost in Math” to say I agree with you.

6. **flippiefanus**
June 30, 2024

This is a topic close to my heart. It can be called “physics vs formalism”. In my view, there has never been a successful *unguided* venture into theory space (like string theory) . None of the successful examples that you discussed were unguided. Their advent took place within the context of observed problems revealed by experiments, often amidst several competing theories at the time. Even with general relativity: Einstein spent a long time pondering the problem, considering everyday examples (like a man falling from a window) to build up his “physical intuition,” until he came up with the equivalence principle. Only then did he venture into math, guided by this principle.

7. **Jim Baggott**
June 30, 2024

“Math” v “physical intuition” is surely about choosing a strategy when confronted with a challenging problem in theoretical physics. The choice obviously depends on the background and experiences of the chooser, and history shows that there is no ‘best’ choice. The real issue is the potential (or lack thereof) for a proper interplay between theory and experiment. All the examples of successful theoretical developments, no matter what strategy was adopted, involve experiment, because experiment in physics is the arbiter of ‘success’. In the cases of supersymmetry and string theory, these are unsuccessful either because these approaches make no contact with experiment, or because the ‘predictions’ they have made have simply not be upheld by experiment. The more substantial issue then arises from theorists justifying their continued commitment to an unsuccessful theory by seeking to change the definition of ‘success’.

8. **Nikita**
June 30, 2024

Doesn’t the canonical history of general relativity involve a rather “naive” form of physical intuition? “I am falling in an elevator”, “I am moving on a rotating disk”, etc.

9. **Paddy**
June 30, 2024

Pardon me for being facetious but many years ago I decided that “intuition” is just a word one uses for a body of knowledge learned so deeply and so many years ago that one doesn’t recall not knowing it. E.g., much of at least non-rel. QM seems superficially to be intuitive to me–which is patently ridiculous.

10. **Erin**
June 30, 2024

This is a fascinating topic but I come down on the other side. Quantum mechanics, SR and GR were all driven by physical observation and intuition. “Math” was the available tool to be used. If math was dominant, Hilbert would have figured out GR.

11. **Peter Woit**
June 30, 2024

Nikita,
Yes, it somehow has become “canonical history” that “math” wasn’t important, that Einstein figured out general relativity by using his physical intuition to think through what happens when you fall in an elevator. Enough of this kind of dubious argument about GR. If someone wants to point to a serious discussion of the topic, that fine. Otherwise, enough of that.

12. **Scott Aaronson**

June 30, 2024

After years of talking with physicists, I concluded with some amusement that when they said “physical intuition,” they just meant “guesses of which math to use for physics problems, whenever the guesses turn out to be correct.” 😀

13. **Peter+Shor**
June 30, 2024

To talk about a somewhat different field, statistical mechanics, Giorgio Parisi won the Nobel Prize in part for inventing the replica method. This is a case where it seems to me that the math is completely broken, but the physical intuition works anyway. But there’s not “no math”. There’s lots of equations, but none of them can be justified rigorously.

It seems to me that papers with few equations, but lots of hand-waving about physical intuition, are likely to be completely wrong.

14. **Eric+Weinstein**
June 30, 2024

This is a great idea for a post Peter.

I worry that the term ‘math’ is being used for two very different things: Geometric vs Analytic thinking.

Likely the term ‘Physical intuition” is also being bound to two very different ideas: Constructing Lagrangians vs Unpacking Lagrangians.

And, oddly I think these two subdivisions are mirroring each other. A mathematician trained in Geometry is often much closer to a Field Theorist trying to develop new Lagrangians than (s)he is to most hard core analysts. There are very things that separate mathematical from physical intuition in such cases but there are a few. In particular the concept of what is geometrically natural is slightly different to what is physicalliy natural in a Lagrangian. The “Mexican Hat Potential” quartic potential is obviously geometrically awkward as is the Yukawa Coupling as are the CKM/PMNS matrices. They come from experiment, but they don’t correspond to natural fiber bundle theory that mathematicians would stumble upon. Yet they are natural enough to a field theorist who might not see why the Yang-Mills term is incredibly more geometrically natural because the field theorist may believe that renormalizability is the proxy for naturality.

Conversely, the mathematical analyst might be more focused on keeping control of infinities in mathematical work while the perturbation/regularization/renormalization concerns mirror those in analysis need to keep infinities from spoiling everthing. Yet this is often about unpacking Lagrangians rather than constructing them.

This leads to two related but distinct concepts of elegance and beauty. I have never understood the argument that String Theory is beautiful…but at least I’d like to think I know WHY I don’t understand it. It is because it is largely based on a mathematics problem that doesn’t strike me as the way forward: “How do we treat the metric so that it doesn’t blow up or misbehave when added to the field content to be quantized?” That is really an analysis question to my mind which drags in a lot of geometry kicking and screaming. So you end up with beautiful geometric objects like Calabi-Yau manifolds but for very weird and often ugly reasons.

Ultimately, I think that what we are struggling with here is four quadrants: geometry, model building and phenomenology, QFT, and hardcore analysis on manifolds. We are looking for beautiful geometry to become a compelling natural Lagrangian. That is two quadrants. Then it will need to be unpacked by QFT types to discern what degrees of freedom would be observed at our effective energy scales. And that in turn would need to be made into rigorous analysis to be fully satisfactory. What I don’t understand is why those of us who base oursevels in one of these quadrants or the other too often try to discount those in the other three. It just seems totally self-defeating. One man’s opinion.

15. **Robert James Parkinson**
June 30, 2024

Perhaps a slight aside, but as far as I can tell “the math” means anything one does not have a really good intuition about.

And it’s not just physicists who talk this way either. Ask any (well-educated) person on the street what an average is and they’ll explain it in plain English with clarity and precision. Ask them about standard deviation and you’ll get a slightly muddier answer.

Ask them about the third moment of a random variable, and they’ll tell you that for something like that you gotta do the math.

So “math” seems to be the psychological state where one starts manipulating equations with pen and paper and knowledge in the hands begins to take over from knowledge in the mind.

Just my theory.

16. **Peter Woit**
June 30, 2024

Robert James Parkinson,
I put “math” in quotation marks because I think different people mean different things by it. That it is often getting used by non-mathematicians to mean “stuff I don’t really understand” sounds right.

I like a lot Scott Aaronson’s observation that “physical intuition” often doesn’t mean a non-mathematical understanding of a problem but does mean knowing what the right mathematical understanding of the problem is. To me the big problem of finding a way forward in fundamental theory is exactly that of finding the right mathematics. Mathematics is a set of languages and tools for using these languages, we need to identify which of these is needed to make progress. Eric Weinstein’s comment is along similar lines, with a specific proposal, mine would be different.

17. **Gavin**
June 30, 2024

Wow this is a wildly out of touch point of view of how physics is done.

To a physicist, “the math” means doing a formal calculation from first principles. There’s often an implication that the calculation is being done with a high degree of generality.

Working physicists often think it’s not a good idea to only use “the math” when doing real world problems. There’s a famous quote by Wheeler: “Never make a calculation before you know the answer.” There are a variety of *heuristic techniques* that physicists use to get a feel for the answer, including:
* Dimensional analysis
* Knowledge of relevant experimental results

* Experience with related calculations
* Knowing that the calculation must reproduce some known result as a special case
* Toy models

In fact, I don't think this heuristic mode that physicists use is particularly different from what mathematicians use; I think it is quite analogous to what Terry Tao refers to as the "post rigorous" stage of mathematics: https://terrytao.wordpress.com/career-advice/theres-more-to-mathematics-than-rigour-and-proofs/

Trying to find "a general solution" to some equation without a *physical question* motivating the endeavor is a classic path to getting lost in equations and not deriving insight from them.

In terms of finding new theories, I think all of your examples can be reframed in terms of a physical motivation. For example, Einstein's paper on special relativity starts by talking about how it is strange the current produced in a coil of wire moving relative to a magnet has two apparently different explanations in two different reference frames. This motivated him to take equations discovered earlier by Poincaire and others (whether or not he was actually aware that they were previously discovered) and *interpret* them physically in terms of various thought experiments, making them manifestly relevant for physics. Of course math still has a role, especially when it expands our conceptual understanding — like when Minkowski reinterpreted the Poincaire transformations in terms of spacetime geometry. But even then, the benefit of Minkowski's work was not blindly applying a formalism, but developing new ideas that allowed us to reinterpret existing results in a new framework where different intuition can be applied.

In reality, research is messy, and I think all tools and points of view are needed. But I think the way your article is framed that all discoveries in the 20th century are based on "math" and string theory is based on "physical intuition" is unabashedly political and not a serious attempt to understand the philosophy of science.

18. **Peter Woit**
June 30, 2024

Gavin Storm,
I think you're missing the point of my posting. It was specifically an intentionally provocative argument against the idea that, in a situation where current theory doesn't apply and you don't have any experimental guidance, the way forward is "physical intuition", not "math". I often hear this in discussions of the current lack of progress in fundamental physics ("the problem is that the string theorists are doing math, not using physical intuition"), and I think it's completely wrong-headed.

You're arguing about something completely different, about whether one should start trying to solve a problem with heuristic methods or with a rigorous formal calculation. Of course both mathematicians and physicists generally start with the first, later move on to the second.

My argument is about what heuristic method you should use. Should you look to the mathematical tools you have successfully used in the past (your built up "physical intuition") or should you look for new tools? The best example of the ones I gave in the posting might be the Gell-Mann one. He and everyone else stared a long time at the patterns of strongly interacting states they were seeing, not recognizing any pattern they were used to or anything that fit the mathematical tools they had in hand. He finally got around to talking to a mathematician who told him about the representations of SU(3), giving him a new mathematical tool that explained the patterns.

In the context of string theory, some people argue that it failed because, seduced by Witten, string theorists went out and adopted all sorts of new mathematical tools not previously useful in physics. I think it failed for a different reason: it was a completely wrong heuristic idea based on picking a wrong physical system (the vibrating string) as starting point.

19. **Bob Y**
July 1, 2024

I started my career as a theoretical high energy physicist and am now an experimental biological physicist. I have published in major refereed journals in both fields. I find Jim Baggot's comment above to be especially relevant and cogent. A simple question is always "What are the experimental data?" that either do or do not validate any "conclusion(s)" coming from math or physical intuition. Experiment is always the final arbiter in science although math, physical intuition as well as experiment can interact along the way to a final determination. Peter your last entry gives some very cogent examples relevant to both Baggot's comment and what I try to express.

20. **Peter Woit**
July 1, 2024

Bob Y,
The question I'm interested in is that of research in areas where there is no experimental evidence to interact with. Should theoretical quantum gravity research, for instance, be described as "not physics" because there is no experiment to interact with?

21. **Kirk**
July 2, 2024

The thing that really bugs me is people acting as if mathematics has nothing to do with intuition, therefore if there is any intuition at all, it must be physical intuition.

I've thought about this often, mostly in bafflement before I realized other people were defining 'math' very differently from me. To me, mathematics is just being incredibly careful and precise with your logical relationships (as even 'true/false' can be mapped to 0/1, partially true to some other number or a tensor, etc), so that even if you use different definitions from other people they'll still be able to follow your steps *because you defined your terms*; like how 'field' can mean two different technical things, the mathematical kind and the physics kind.

Intuitions about geometry are inherently intuitions also about math, they're not two separate things. The problem is the space of possible geometries in math is much wider than in physics, so usually, some guidance from the real world is useful.

However, this will break down in domains where we cannot even in principle do an experiment. From there, we will need mathematics: specifically, metamathematics of how different axiom systems relate to each other and why one might get 'picked out' as the more 'inertial' or 'equality preserving' of the two. But what I say here by 'mathematics' and what someone else hears when I say it may be two different things, and lead to an ugly and ultimately pointless argument: if you think mathematics is only calculations, you are not going to be thinking about all the work (axioms) that has to go in before you ever get to calculations so much.

22. **Peter+Shor**
July 3, 2024

If theoretical quantum gravity research is "not physics", it's hard to see what else it could be. It certainly isn't rigorous enough to be called "mathematics". I suppose a case could be made for it being "nonsense", but I think this is much too pessimistic (although it's true for some current theoretical quantum gravity research).

23. **Robert Cochrane**
July 4, 2024

Thank you for promoting and sympathetically moderating such an interesting and wide ranging discussion, as you previously did on Sabine Hossenfelder's piece about academic life.

First, intuition. I see this as a subconscious, unwritten and often unverbalised collection of prior knowledge which can be called on when action is needed and there is no specific data and no time to collect data. The more specialised the individual and the more typical for him or her the problem, the more reliable it becomes.

It is not reliable in new situations, merely a guide to a possible starting point. But in research situations it may save a lot of time by suggesting a first line of attack (e.g. an appropriate mathematical framework) based directly or subconsciously on previous experience. The analysis and calculations still need to be done and the approach may be unsuccessful. In this sense, it applies to math as well as physics – there are many famous conjectures which have been disproven.

On your examples, first, special relativity. Einstein's original 1905 paper on special relativity was entitled (in translation) "On the electrodynamics of moving bodies". Einstein's father ran an electrical business and the young Albert was fascinated by Maxwell's Equations, later saying that his predecessor was not Newton but Maxwell. His intention was to resolve a problem of asymmetry in the application of the equations. Special Relativity is just Part I of the paper.

He also refers but only in passing to the failure of experiment to show "motion of the earth relative to the light medium". He later said he knew of the M&M experiments but had not read the paper. As to math, there is nothing in his paper beyond the reach of a high school math graduate and the basic calculations are simple.

Next, general relativity. This is far more complex story and impossible to summarise briefly and accurately. The first steps taken by Einstein and others were the extension of special relativity to include special cases of accelerating frames and objects which do not require general relativity. Einstein wished to extend it to arbitrary accelerating frames and gravity and proposed using the principle of general covariance, which extends invariance of physical laws to arbitrary differentiable coordinate transformations and is in many ways was a natural extension of SR.

He did not have the mathematical knowledge to do this and sought help from an earlier classmate and friend, Grossmann, who pointed him towards tensors and the Ricci tensor in particular and assisted him with the math. But Grossmann believed, in error, in 1913 that the resulting mathematical framework for general relativity did not lead asymptotically to Newtonian gravity and they went round in circles for two years before returning to the 1913 tensor approach in 1915.

The basic mathematical framework needed was already in existence, but Einstein needed Grossmann to point it out, and even then, there were major difficulties in its application and finding solutions in practical circumstances, with initially erroneous predictions for key tests.

Leaving aside quantum mechanics and particle physics, your final question is whether research should be described as "not physics" if there is no experimental evidence. My view is quite conventional – research should be either explanatory of existing phenomena (i.e. based on data, even if only qualitative) or should lead to testable predictions. Its mathematical basis should as far as possible be well founded; this is usually the case, the problem is finding the right structure and adapting it to the application. Both special and general relativity did just that.

24. **Per Östborn**
July 14, 2024

Peter,

Referring to your exchange with "Nikita", I don't see why the involvement of a naïve form of physical intuition in the history of general relativity would imply that math wasn't important.

In this context, the essential thing to me is to identify the spark that inspires the pursuit of a theory. It may very well be naïve physical intuition even though the formulation of the theory itself requires a lot of math, possibly even novel mathematical ideas.

I don't know if the following counts as pointing to a serious discussion of the topic, as you requested, but I'll give it a try. Sparks of inspiration are personal, so we have to listen to the physicists themselves.

Einstein described in a lecture in Kyoto in 1922 how "the luckiest thought" of his life came to him in 1907 (Physics Today 35(8) pp. 45-47, 1982):

"The breakthrough came suddenly one day. I was sitting on a chair in my patent office in Bern. Suddenly a thought struck me: If a man falls freely, he would not feel his weight. I was taken aback. This simple thought experiment made a deep impression on me. This led to the theory of gravity."

Similarly, special relativity had been brewing in Einstein's mind since his teenage years (Autobiographical notes, Open Court, 1949):

"After ten years of reflection such a principle resulted from a paradox upon which I had already hit at the age of sixteen: if I pursue a beam of light with the velocity c (velocity of light in a vacuum), I should observe such a beam of light as a spatially oscillatory electromagnetic field at rest. However, there seems to be no such thing, whether on the basis of experience or according to Maxwell's equations. From the very beginning it appeared to me intuitively clear that, judged from the standpoint of such an observer, everything would have to happen according to the same laws as for an observer who, relative to the earth, was at rest. For how, otherwise, should the first observer know, i.e., be able to determine, that he is in a state of fast uniform motion? One sees that in this paradox the germ of the special relativity theory is already contained."

When it comes to the development of new fundamental theory, I would say that such sparks or general ideas are necessary. It is not enough to search for a mathematical framework that can accommodate new experimental results, it seems to me.

Michelson and Morely did their experiments in the 1880s, and mathematical efforts to account for the lack of aether drag began immediately, via Fitzgerald contractions and the like. However, Einstein's relativity postulate was necessary to jump from ad hoc theories to a new mechanics based on general principles that allowed the derivation of a lot of other, seemingly unrelated results.

I would say that the history of quantum mechanics is similar. Decades of experimental results that deviated from the predictions of classical physics were accounted for in an array of phenomenological mathematical models. However, it was not until the work of Heisenberg and Schrödinger that a single, coherent theory emerged that could be used to derive a lot of new predictions. And Heisenberg and Schrödinger both seem to have been inspired by simple, general ideas.

The abstract of Heisenberg's groundbreaking paper from 1925 reads in its entirety (Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen, Zeitschrift für Physik 33(1) pp. 879–893, 1925):

"The present paper seeks to establish a basis for theoretical quantum mechanics founded exclusively upon relationships between quantities which in principle are observable."

He goes on to write:

"It is well known that the formal rules which are used in quantum theory for calculating observable quantities such as the energy of the hydrogen atom may be seriously criticized on the grounds that they contain, as basic element, relationships between quantities that are apparently unobservable in principle, e.g., position and period of revolution of the electron."

Schrödinger had a different angle of attack. He was inspired by de Broglie's matter waves and seems to have fallen in love with the idea that continuously interacting waves is the substrate for the entire physical world. In the last paper before those that define wave mechanics, Schrödinger wrote (On Einstein's gas theory, Physikalische Zeitschrift 27 pp. 95–101, 1925):

"This means nothing other than to be serious about the De Broglie–Einstein wave theory of moving particles according to which these are nothing but a kind of 'foam crest' on a fundamental radiation wave."

The relativity postulate is a metaphysical principle that goes deeper than the apparent constancy of the speed of light. Heisenberg's strict epistemic ansatz in his 1925 paper predicates and goes beyond the choice to express mechanics by means of non-commuting matrices. Schrödinger's (presumed) idea that 'everything is waves' is more far reaching than the Schrödinger equation.

I think new such general ideas are needed to spark further development of fundamental physics. They don't have to be entirely correct to be fruitful. Whether Heisenberg's epistemic approach is appropriate is a matter of philosophical taste. Schrödinger's vision of waves was clearly insufficient. Newton's idea that space and time are absolute is wrong, but it was nevertheless immensely fruitful, of course, since it allowed the expression of the relations between all objects by means of their individual coordinates, the evolution of which could be expressed with differential equations.

Could the same be said about string theory? Could there be something to its mathematical structure even though the basic physical intuition about the "vibrational modes of a vibrating string" is misguided? If so, could the theory have been developed without this idea?

Sorry for the length of this comment, and for repeating lots of standard material. I guess my point is to defend "physical intuition". However, I would like to rephrase it as "metaphysical intuition", meaning intuition about which general principles describing the physical world are fruitful guides in the formulation of fundamental physics.

"Metamathematical intuition" is surely also needed, understood as intuition about which mathematical structures and requirements are fruitful for the same purpose. Dirac's equation seems to have been the result of such intuition, for example.

25. **Sebastian**
July 17, 2024

All theories in physics, including those mentioned in the post, are based on intuition, almost by definition. It is not possible to develop a correct and entirely new physical theory purely through rigorous formal derivation without incorporating one's perception of the phenomenon in question. Importantly, this perception pertains to one's understanding of the phenomenon rather than the phenomenon itself. When developing a new hypothesis, one's understanding cannot be guided by the not-yet-existing theory and, therefore, must be intuitive.

I (respectfully) believe that the examples provided by the author are misinterpreted and treated in an ahistorical manner. The most striking example is that of special relativity. The Lorentz transformation and related mathematical formalism were indeed derived years before Einstein's article through symmetry-based considerations of Maxwell's theory. However, it was Einstein's understanding of the important (non-mathematical) principle of relativity, particularly his intuition of time and distance as measured with clocks and rulers, that constituted the actual breakthrough. This breakthrough (a) changed our conceptual understanding of reality and (b) inspired further development of vast mathematical formalism beyond the Lorentz transformation. Notably, Einstein's discovery did not rely on the Michelson-Morley experiment, as the validity of the principle of relativity was not dependent on the existence of aether.

I believe the greatest breakthrough in our understanding of nature came from a physicist who had limited knowledge of and mistrusted mathematics: Sir Michael Faraday. Faraday introduced the concept of a "field" purely through intuition, departing from the idea of action-at-a-distance along straight lines (Newton's description of physics with ordinary differential equations) and instead proposing a "medium" and curved "force lines." This led to Maxwell representing physics with partial differential equations. Theorists often overlook that field theory, the foundation of our understanding of fundamental forces, was introduced by Faraday through his meticulously conducted and brilliant experiments on electromagnetism. Faraday's discovery process contradicts the author's statement: "The best way to come up with such breakthroughs is from new experimental results that conflict with the standard theory and point to a better one." Faraday did not have a "standard" theory to compare to, nor did he attempt to understand existing mathematical theories. Instead, he built a consistent conceptual framework of electric and magnetic force by formulating hypotheses about reality and testing them experimentally. He was able to extrapolate his non-mathematical ideas to make claims about the electromagnetic nature of light without experimental methods to prove it or any mathematical formalism. Learning about Faraday's story could teach many theorists a valuable lesson in humility and help prevent unsound philosophical claims about the connections between mathematics and reality.

There is no reason to believe that a story similar to Faraday's can not happen again in the XXI, XXII, or XXIII century, despite "big science" and "mathematics" being widely considered the only way of ensuring scientific progress.

26. **Peter Woit**
July 18, 2024

Sebastian,
This is exactly the point of view that I am arguing against. Given the conceptual and mathematical sophistication of our theories of modern physics, there is zero chance that they are going to be improved by a a new Faraday as you suggest.

A Few Items

July 2, 2024
Categories: abc Conjecture, Langlands

A few items, all involving Peter Scholze in one way or another:

- A seminar in Bonn on Scholze's geometrization of real local Langlands is finishing up next week. This is working out details of ideas that Scholze presented at the IAS Emmy Noether lectures back in March. Until recently video of those lectures was all that was available (see here, here and here), but since April there's also this overview of the Bonn Seminar, and now Scholze has made available a draft version of a paper on the subject.
- In three weeks there will be a conference in Bonn in honor of Faltings' 70th birthday. Scholze's planned talk is entitled "Are the real numbers perfectoid?", with abstract

  > Rodriguez Camargo's analytic de Rham stacks play a key role in the geometrization of "locally analytic" local Langlands both over the real and p-adic numbers. In both settings, one also uses a notion of perfectoid algebras, with the critical property being that "perfectoidization is adjoint to passing to analytic de Rham stacks". This suggests a "global" definition of perfectoid rings. We will explain this definition, and present some partial results on the relation to the established p-adic notion. Two natural open questions are whether tilting works in this setting; and what perfectoid algebras over the real numbers look like.

- On the abc conjecture front, Kirti Joshi has a new document explaining his view of The status of the Scholze-Stix Report and an analysis of the Mochizuki-Scholze-Stix Controversy. To some extent what's at issue is what was discussed by Scholze and others on my blog back in April 2020 (see here). Joshi is trying to make an argument that there is a way around the problem being discussed there, but I don't think he has so far managed to convince others of his argument (Mochizuki refuses to even discuss with him). He ends with the following:

  > Meanwhile, Scholze and I are having a respectful and professional conversation (on going) as I work to clarify his questions; while I continue to wait for Mochizuki's response to my emails.

  He also clarifies that he has not yet finished a water-tight proof of abc along Mochizuki's lines:

  > My position on whether or not Mochizuki has proved the abc-conjecture is still open (as my preprint still remains under consideration). In other words, I'm currently neutral on the matter of the abc-conjecture. However, I continue to work on to tie up all the loose ends.

Update: In the comments someone points to this conference at MIT next week, which will start off with a talk by Faltings on *Mordell past and present*. That conference will be followed by this one the following week.

Update: Erica Klareich at Quanta has a very nice article about the recent proof of Geometric Langlands. About the implications of this work, there's a nice quote from Peter Scholze:

> "I'm definitely one of the people who are now trying to translate all this geometric Langlands stuff," Scholze said. With the rising sea having spilled over into thousands of pages of text, that is no easy matter. "I'm currently a few papers behind," Scholze said, "trying to read what they did in around 2010."

He's not the only one struggling to understand what was known before this proof, and daunted at the prospect of trying to read the 800 pages of five papers (see here) that make up the full proof.

Comments

1. **Felipe Zaldivar**
   July 2, 2024

   On the Mordell conjecture, closer to home: https://mordell.org/

2. **Anon**
   July 2, 2024

   Can someone clarify, for a math literate audience, what Scholze's real local Langlands is doing?

3. **Villani's Spider**
   July 2, 2024

   You missed the final sentence of Scholze's abstract: " Two natural open questions are whether tilting works in this setting; and what perfectoid algebras over the real numbers look like."

4. **Boris**
   July 3, 2024

   Peter see this…and maybe you can write something…

   https://mathstodon.xyz/@tao/112557248794707738

5. **Peter Woit**
   July 3, 2024

   Boris,
   I fear that topic is one that I know very little about, and certainly couldn't improve on Tao's explanation. He points to talks at the Sarnak conference, for which videos should now be available.

6. **Peter Woit**
   July 3, 2024

   Anon,

   I'll try to say something, but hoping one or more of my readers is willing to do a better job…

Real local langlands classifies representations of real Lie groups. For the simplest example, think of SL(2,R) and its discrete and principal series representations. This is done in terms of "Langlands parameters", for which Scholze provides a new geometric interpretation, in terms of geometric Langlands, on the twistor P1.

This is the same classification of representations as before, but a new geometric point of view. Most dramatically, this is done with new methods replacing the usual geometry and analysis, methods that have developed out of work on the p-adic case. This provides some very new and different ways of looking at the real case, and a unified perspective on what is happening at the p-adic and real places. A speculative possibility is that this will lead to new global results (i.e. for Q, not Q_p and R), with Scholze's upcoming talk suggesting "a "global" definition of perfectoid rings."

7. **Will Sawin**
July 3, 2024

Indeed, one can see the videos at https://www.youtube.com/watch?v=dIe5hqTuB4k and https://www.youtube.com/watch?v=diASDVdMaN0

The first talk (by James Maynard) goes over the statement of the result, prior work, applications, and a brief summary of the proof. The second talk (by Larry Guth) explains the ideas behind the proof. I would encourage everyone who hears that description and thinks they will understand the first talk but not the second to try watching it – it's a masterpiece of finding the key ideas of a complicated proof and explaining them in simple terms.

8. **Anon**
July 16, 2024

There's something weird in the poster of "conference in Bonn in honor of Faltings' 70th birthday". I see Peter Sarnak name as one of the speakers, but he's not listed as the speaker in the abstract of the conference

9. **Peter Woit**
July 16, 2024

Anon,
A likely explanation is that he was planning to attend the conference, so on the poster, but at some point changed plans and won't be there, so, not on the program.

10. **Oisin McGuinness**
July 18, 2024

The conference for Faltings' 70th birthday has updated their streaming policy, see main page at https://www.mpim-bonn.mpg.de/faltings70

"Due to the great interest in this conference, we will stream most of the talks live via Zoom. The program indicates which talks will be streamed. The Zoom link will follow shortly before the conference. The talks will not be uploaded on the website later."

Excellent!

By the way, the Mordell Conference https://mordell.org site has been updated with slides of the talks, including that of Faltings (1st talk), and a talk on possible formalization by Michael Stoll (last talk), both fascinating in different ways.

Also see the photo of the participants, Faltings is near the top right….

11. **David+Roberts**
July 18, 2024

@Oisin McGuinness

actually, not posting the videos afterwards is less good than just live-streaming. Far fewer people will get the watch the talks as a result.

12. **Oisin McGuinness**
July 19, 2024

@David+Roberts

"Far fewer people will get the watch the talks as a result."

Agreed, but when I asked the organizers initially, some weeks back, they were not going to have anything, so what they are doing is an improvement.

It is a big shame (similarly for the recent Mordell Conjecture Conference) that recording is not standard anymore for many of these conferences. Some talks are clearly of possible historic import. Some foundational support is needed to support more recording! By contrast the IAS does a great job.

Are you listening Simons Foundation???

13. **Sandeep**
July 21, 2024

I might have missed, but was there a post on the proof of Geometric Langlands? I just read the Quanta article.

14. **Peter Woit**
July 22, 2024

Sandeep,

I haven't written about the Gaitsgory/Raskin et al proof, haven't followed the technicalities. Thanks for mentioning the Quanta article, I just read it and it's quite good. I'll add a link to that.

The Harvard Swampland Initiative

July 12, 2024
Categories: Swampland

The past few years I've been noticing more and more claims like this one, supposedly finding a way to "connect string theory to experiment". When you look into such claims you don't find anything at all like a conventional experiment/theory connection of the usual scientific method, giving testable predictions and a way to move science forward. As for what you do find, for many years I tried writing about this in detail (see here and here), but that's clearly a waste of time.

One thing that mystifies me about such claims is that I find it very hard to believe that most theorists take them seriously, always assumed that the great majority was with Nima Arkani-Hamed, who recently characterized the track record of this kind of thing as "really garbage". But if most theorists think this is garbage, why am I seeing more and more of it? A hint of an answer comes from the paper with the supposed connection, which describes the Harvard Swampland Initiative as the place the work initiated.

I hadn't been aware there was a Harvard Swampland Initiative, but it is a Research Center at Harvard, running an "immersive program" in which "participants collectively navigate the Swampland". More importantly, it has no less than ten associated postdoc positions. On the scale of different ways of having influence on a field, being able to hand out ten postdoc positions at Harvard is right up there. This goes a long way towards explaining to me what I've been seeing in recent years. It also makes me quite depressed: when I started my career in that department in the late seventies, the idea that fifty years later this is what it would come to is something beyond any one's worst nightmare at the time.

Update: In case anyone has any doubt about what the main goal of the Swampland program is, see here:

> Recently, physicists have started a new program called Swampland to overcome the criticism of string theory that arose in the 2000s.

"Recently" seems to refer to 2005, when this started.

Comments

1. **DKepler**
   July 13, 2024

   One aspect that is also becoming increasingly obvious is the intent of getting such works through in a prestigious journal such as PRL (arxiv format of the present one suggests the same for this work as well). Quote some numbers, throw in some data analysis, make some vague references to experiments, put in big names in Acknowledgements, and PRL is usually happy to accept. If you see timelines of some recently accepted PRL papers, you will see a few stringy ones that had merely 2-3 weeks between submission and publication!

2. **Sabine**
   July 13, 2024

   In a few years from now, one of them postdocs will fall off the bandwagon and draw attention to themselves as a critic of string theory. Someone will set up a blog. Someone will write a book.

   "The word is about, there's something evolving,
   Whatever may come, the world keeps revolving,
   They say the next big thing is here,
   That the revolution's near,
   But to me it seems quite clear
   That's it's all just a little bit of history repeating."

3. **fwiw**
   July 13, 2024

   > Quote some numbers, throw in some data analysis, make some vague references to experiments, put in big names in Acknowledgements, and PRL is usually happy to accept.

   That's just nonsense. PRL remains extremely selective and demanding. About half of all submissions to PRL are desk rejected in a few working days. Go ahead and try.

4. **Peter Woit**
   July 13, 2024

   fwiw,

   About PRL's extremely selective and demanding standards for string theory papers, I'll just point to
   https://www.math.columbia.edu/~woit/wordpress/?p=14001

   Enough about PRL, the real question here is why anyone thinks this is science or why any journal would publish it.

5. **fwiw**
   July 14, 2024

   hang on, is the harvard swampland initiative definitely a research center as opposed to an initiative to create seminar series, visitors, discussions etc on swampland? aren't the participants just harvard researchers who are joining in those activities, cf. ten post-doc positions handed out by the swampland research center, all hired by funding from the harvard swampland 'research center' for swampland

6. **Peter Woit**
   July 14, 2024

   fwiw,

   https://www.physics.harvard.edu/research-centers

7. **Some guy idk**
July 14, 2024

I'm a bit confused. The Swampland is broader than the string theory landscape, and I'd think much of the work being done there would have nothing to do with strings. Isn't that exactly what you've been advocating for? Quantum gravity and other steps towards unification outside of string theory?

8. **Peter Woit**
July 14, 2024

Some guy idk,
The swampland is Vafa's program, and in different places he emphasizes different aspects of it. He is certainly not setting it up as an alternative to string theory. For what he has in mind, look at his original 2005 swampland paper
https://arxiv.org/abs/hep-th/0509212
and for something more recent, lecture notes from two Harvard courses
https://arxiv.org/abs/2212.06187

In both places he makes very clear what the motivation for the program is: to fight the argument that "string theory predicts nothing" because you can get any low energy physics you want with an appropriate choice of "string vacuum". The origins back in 2005 were in the context of arguments over Susskind's "landscape", see
https://arxiv.org/abs/hep-th/0302219
Susskind essentially argued that you could get just about anything out of string theory, then anthropic arguments would determine where we were in the landscape. Vafa realized this was disastrous for string theory, and that at least one blogger was doing a lot of damage to string theory by pointing this out.

So, his motivation from the beginning has been to try to fight the "string theory makes no predictions" argument, sometimes by arguing "string theory gives a consistent quantum theory of gravity coupled to matter and maybe we can find generic consistency conditions for such things, so these would be string theory predictions".

I think if you listen to what he says about the swampland and string theory, you'll see that the whole point is to mount a defense of string theory and of the study of "string theory vacua". String theory is taken as an assumption that cannot be challenged. When asked if he would abandon string theory if the "predictions" he finds are disconfirmed, he says that no, he wouldn't. Such disconfirmation would just mean that one needed to modify one's understanding of "string vacua".

If you don't believe me that a main motivation here is responding to all too effective criticism that string theory can't predict anything, see here
https://www.math.columbia.edu/~woit/wordpress/?p=10486

9. **Some guy idk**
July 14, 2024

Thanks for responding!

What about "swampland conjectures" like the completeness of spectrum hypothesis (satisfied by string theory but not necessarily motivated by it) and weak gravity conjecture (has little or nothing to do with strings)? These things, as I understand, try to establish a network of conditions for a future complete theory of quantum gravity, and I would think that's worthy research that anyone could get behind.

10. **Mitchell Porter**
July 15, 2024

In "The Swampland: Introduction and Review" (arXiv:1903.06239), Eran Palti writes:

"One approach towards providing evidence for a Swampland conjecture is by utilising known vacua of string theory as a type of experimental data…

"Another approach that is commonly adopted to study Swampland criteria is utilising quantum gravity arguments directly in the low-energy effective theory. There are low-energy aspects of quantum gravity that are expected to be universal, in particular relating to black holes and to the holographic nature of gravity. Relative to [the first approach], this approach has the advantage of broader generality. On the other hand, the arguments often lack the explicit details and concreteness of string theory based evidence."

So there is a part of the Swampland program which is logically independent of string theory. It would be useful for this discussion, to know how extensive it is.

11. **Peter Woit**
July 15, 2024

Some guy idk/Mitchell Porter,
I just don't see any point to these string-inspired conjectures about which low energy physics is compatible with string-inspired general ideas about gravity. If your conjecture says that the SM is compatible with gravity, you've learned nothing. Whatever the correct explanation of gravity is, it has to be compatible with the SM at low energies because that is the way the world is. If your conjecture says that the SM is not compatible with gravity you've also learned nothing (beyond that your conjecture is wrong).

The main motivation for all this activity is to justify continuing doing meaningless calculations about "string vacua", partly in order to be able to make bogus claims about "connecting string theory with experiment", partly in order to not have to admit you've wasted 40 years on a wrong idea.

12. **David Brown**
July 18, 2024

The article by Rocci & Van Riet is available on arxiv.org .
Rocci, Alessio, and Thomas Van Riet. "The quantum theory of gravitation, effective field theories, and strings: yesterday and today." The European Physical Journal H 49, no. 1 (2024): 7.
https://arxiv.org/abs/2403.14008

13. **David Brown**
July 19, 2024

What is the "Not Even Wrong" perspective on the following?
"Swamplandish Unification of the Dark Sector" by Cumrun Vafa, 2024
https://arxiv.org/abs/2402.00981

14. **Peter Woit**
July 19, 2024

David,
The most amazing collection of misuses of the term “prediction” in any abstract of a paper, ever.

Same old, same old: hideously ugly unmotivated introduction of huge numbers of new degrees of freedom, followed by elaborate effort to explain why they’ve never been seen by pushing them to regions inaccessible to experiment. Then, claim that this leads to a “prediction”.

Various and Sundry

July 31, 2024
Categories: Uncategorized

A few items of different kinds:

- The Harvard Math department website has a wonderful profile of Dick Gross.
- The second International Congress of Basic Science ended a few days ago in Beijing. A huge number of interesting talks, video and slides available here. My Columbia colleague Richard Hamilton was one of the winners of a Basic Science Lifetime Award, unclear how much wealthier this makes him and his fellow awardees.
- Alphaxiv is a new website allowing for research discussion of arXiv papers.
- The Gates Foundation evidently is no longer going to pay publishing costs for open access journals. The problem is that this kind of funding incentivizes some sorts of bad publishing practices. They refer to

> unsavory publishing practices by poor actors (paper mills, questionable quality review, unchecked pricing)

- The Perimeter Institute last week had a summer school on Celestial Holography. This panel discussion explains some of what the people involved are trying to do.
- Curt Jaimungal has been planning a series of programs on *Rethinking the Foundations of Physics*, with the topic "What is Unification?". For the first program, featuring Neil Turok, see here. I'll record something on Friday, part of the plan evidently is a lot of questions. If you have one, you can try leaving it as a comment and I'll try and get some of them to Curt.

Comments

1. **David Brown**
   August 1, 2024

   "Beautiful Math – Dick Gross", National Museum of Mathematics, May 18, 2016
   https://www.youtube.com/watch?v=SJSVPmOFYqg

2. **Low+Math,+Meely+Interacting**
   August 2, 2024

   Enjoyed the Turok discussion, and following Q&A. Thanks for the pointer!

   Celestial holography and the related amplitudes and "modern S matrix" efforts appear to lean heavily on twistors, and attempt to find analogous connections between quantum gravity and a conformal field theory in flat spacetimes as is seen in AdS/CFT.

   Turok, et al.'s theory is conformally invariant, makes use of Twistor Theory to formulate that, and equates (if I understood correctly) breaking of this conformal invariance with electroweak symmetry breaking.

   Your own twistorial approach also seems quite different, with instead the breaking of spin group symmetry associated with the Higgs mechanism.

   Unless I've badly misunderstood, Twistor Theory is not a model of quantum gravity, but a different way of describing space-time that might be more fundamental and better suited to theories of quantum gravity. In that regard, these three and other approaches utilizing twistors might have little otherwise to do with one another.

   Then again, there might be deep connections between them. What do you think?

3. **Peter Woit**
   August 2, 2024

   LMMI,
   Twistors encode beautifully 4d spinors and 4d conformal symmetry and give a remarkable different way of thinking about space-time. A space-time point is by definition the spinor space at the point. This is going to appear naturally if you try and do anything with 4d conformal symmetry. So, not surprising the celestial holography people and Turok find twistor variables useful.

   Since the beginning, the hope of people working on twistors has been that these are the right variables for quantum gravity. This works in a sense for a chiral version of gravity, not for the usual gravity which is thought of as parity invariant.

   I'm pursuing a somewhat different version of this story, emphasizing the idea of looking not just at Minkowski spacetime twistors and Euclidean spacetime ones. Whether this has anything to do with these other programs, I don't know, it's not something I'm seeing indications of at the moment.

4. **Mark Hillery**
   August 3, 2024

   As a former journal associate editor, I found the announcement by the Gates Foundation that they would no longer pay APC's (article processing charges) very interesting. I hope this is a trend. I think it is time to admit that gold open access has not worked. Its proponents, who were initially idealistic scientists but soon came to include government bureaucrats, claimed wonderful things for it. Among these were the claim that anyone could read the scientific papers for free thereby improving access (true) and that this would somehow decrease the amount of money going to large publishers (false). The money to pay the APC's was supposed to come from the money institutions would save on subscriptions (at a cash-starved institution like CUNY this hope was laughable) and research grants. In regard to grants, not everyone has them, and the money that goes to pay APC's is money that is not used to pay things like graduate student salaries. What happened is that you got bureaucrats mandating that papers be published in gold open access journals (see the EU's Plan S), publishers adapting very nicely (the APC to publish in Nature is $12,290) and increasing the amount of money they made, and predatory journals sprouting up like mushrooms. It is time to go back to the drawing board, and refusing to fund APC's is a step in the right direction.

5. **SRP**
   August 4, 2024

   Way back in 1978 I took calculus from Dick Gross. Even as a young assistant professor he was an excellent and engaging instructor, very good at adding historical motivations to his lucid explanations and letting his sense of humor come through.

6. **David+Roberts**
   August 4, 2024

   The people who took the lead on gold OA (that is, paying a journal to make the article open access in the journal itself) where biologists who were **already paying $3000 in page charges per paper** for publishing closed access papers (colour figures etc. This was early 2000s, mind). This was merely a 'cost of doing business', which was small change in light of the huge expense of actually running an experimental lab. A quick search found a Reddit discussion about lab costs, and one respondent gave an estimate of $300k/year, outside salaries, it seems)
   They figured they could instead set up journals charge that amount but make them open access, so literally a net zero change in cost to themselves. Also, by not publishing physical journals they could cut costs, but also still pay for journal infrastructure. This multi-thousand dollar figure was latched onto by the established publishers, when it really should be less for them by economies of scale. And it completely pulled one over the rest of academia where people just don't have that cashflow, and of course you got the predatory publisher relishing this new norm.

7. **Z Y**
   August 7, 2024

   The ICBS does a good job in putting together an impressive number of mathematicians across all disciplines and regions of the world, akin to an ICM but every year. I wonder if there is something of this magnitude or bigger in the physics side of things. They have considerably expanded the number of laureates. Still I refuse to like the choosing of the words: Basic Science in the name, if there is not experimental biology at all. Are there experimental physicists showing their results in there? if not, maybe International Congress of Theoretical Science would be more apt.

Zombie Wormhole Publicity Stunt

August 2, 2024
Categories: Fake Physics, Wormhole Publicity Stunts

I had thought that the Wormhole Publicity Stunt could now be safely ignored, with almost everyone in the physics community agreeing that this was an embarrassing disaster that was dead and buried. Even the people at Quanta had realized that they had been misled into helping promote something that wasn't at all what it claimed to be. The Quanta promotional video that was a big part of the publicity stunt is still on Youtube, but they've added this text:

> UPDATE: In February 2023, an independent team of physicists presented evidence that the research described in this video did not create any wormholes, holographic or otherwise.

Today though, the World Science Festival put out a Zombie revival of the publicity stunt, under the bizarre title Did Einstein Crack the Biggest Problem in Physics…and Not Know It? It starts off with Brian Greene explaining that this may be the "Holy Grail" connecting string theory/quantum gravity to experiment, with such experiments not needing an expensive collider or space telescope, just a Google quantum computer.

Brought in for the discussion are the three architects of the publicity stunt, Daniel Jafferis, Joe Lykken, and Maria Spiropulu. At a couple points there is a mention that something might be "controversial", but there's zero explanation of what the "controversy" might be. After starting out with some background about wormholes and entanglement, the rest of the program is basically outrageous and misleading hype, without a hint of why anyone might be skeptical about it.

I've now wasted too much of my life trying to debunk bogus claims of this kind, with the wormhole nonsense just the latest and most egregious example. One learns over the years that it's impossible to stop this kind of thing, there's no way to kill off the Test of String Theory/Fake Physics enterprise. Even if you think something has been completely debunked, its proponents will always find some way to emerge from the grave and keep going. If anyone is aware of a source for the right kind of silver bullet to stop this, let me know.

Comments

1. **Jean-Pierre Laflamme**
   August 2, 2024

   On the web, the means to destroy zombie invasions may not be appropriate for the discussion that needs to be had here :).
   Jokes aside, perhaps we should target the sources of funding for this research which is going round in circles, possibly hampering the funding of other potentially more promising research.

2. **Sabine**
   August 3, 2024

   Fwiw, I think the hype has much decreased in the past decade, and I think that's to no small part b/c of your blog, so thanks for all the fish etc & don't give up

   As someone once told me, academia is like a big tanker ship, inertia is huge. Even if you stop the engines it'll keep going for a few miles and I think that's what we're seeing here.

3. **Eleni**
   August 3, 2024

   Oh! The attempted obituary here turned out very timely:
   https://3quarksdaily.com/3quarksdaily/2024/07/the-wormhole-fiasco.html

4. **John Baez**
   August 3, 2024

   Maybe start spreading the word that these people are just… weird.

5. **Rainy Sunday Afternoon Theoretical Physicist Drop Out**
   August 3, 2024

   I just watched the video with Brian Greene…

   And honestly, it is a mix of half-baked "common-man" examples of quantum mechanics systems using spin, which would embarrass a part-time contract lecturer were he to listen to his students talking this way in the coffee place near college after a few beers.

   Also with overtones of a Monty Python sketch.

   And these are the guys that got where I could never make it?

   I don't know whether to feel depressed… or proud…

   Regards

6. **Alessandro+Strumia**
   August 3, 2024

   It's your fault. Your criticism prompted string theorists to try doing something testable.

7. **Peter Woit**
   August 3, 2024

   Jean-Pierre Laflamme,
   It looks like the DOE grant that funded this is still active. I'm curious to know if these people will try to get another one to fund doing this, would hope that they would run into trouble with that.

Two problems though are:
1. To some extent this was funded by Google, and good publicity but bad science is probably fine with them, so they may keep funding it.
2. IAS faculty in this case seem to have convinced the IAS director that this was earth-shattering work on a par with the 1919 verification of GR predictions. I don't know that they're going to stop doing this kind of thing and they have a lot of influence.

8. **Patrick Moylan**
   August 3, 2024

   The title is another example of countless and unjustified Einstein hagiography (Did Einstein Crack the Biggest Problem in Physics…and Not Know It?).

9. **David+Roberts**
   August 4, 2024

   People just walk into Betteridge's law, it's astounding. Or else the person who was tasked with coming up with a headline knew what they were doing, and more or less trolled the people doing the stunt.

10. **Jean-Pierre Laflamme**
    August 4, 2024

    I do not claim to be a great expert in the field, but I think that the drifts we see come from the difficulty, or even the impossibility, of testing the different theories put forward. Previously, we could relatively easily disprove or confirm a theory, but today, laboratories or observatories are worth billions of dollars (CERN, FCC, JWST). Although they are very useful for the advancement of knowledge, they arrive, as we see currently, that they bring more questions than answers. Speculative physics can therefore take the lead if it brings theories that make us dream like wormholes, superluminal speeds or even teleportation. The solution would perhaps involve a multidisciplinary committee of wise persons who would come to moderate all this before it gets out of hand.

11. **Peter Woit**
    August 4, 2024

    Jean-Pierre Laflamme,
    Yes, that's the problem, but it has long ago gotten out of hand. And it has been the kind of "wise persons" one would appoint to the kind of committee you suggest who have often been the ones who have gotten us here. Quanta magazine and the IAS director consulted with such before joining the publicity stunt.

12. **Jean-Pierre Laflamme**
    August 5, 2024

    Peter,
    This is an interesting discussion. It is certain that for such a committee of wise to be effective, it would be necessary, as much as possible, for people who have a conflict of interest in the research subject to withdraw from it. But human nature being what it is, this is perhaps easier said than done.
    Note : I use the terms "Comittee of wise" as a traduction of "Comité de sages" in french, but I am not sure if the traduction is entirely good for english speaking persons.

13. **Peter Woit**
    August 5, 2024

    Jean-Pierre Laflamme,
    The translation is fine. The problem with this kind of proposal is that string theory is an extremely complicated and poorly understood subject, with string theorists themselves often not understanding parts of the subject. String theorists argue that anyone who is not a string theorist is incompetent to evaluate the state of the subject.

14. **Jean-Pierre Laflamme**
    August 5, 2024

    Peter,
    Keep up your blog like this. Your contributions surely contribute to keeping this unbridled research, I would say, in string theory, under relative control. In any case, in any field, and without falling into fake news, the right to constructive criticism remains essential in the search for truth.

Susskind: String Theory is Not the Theory of the Real World

August 7, 2024
Categories: Uncategorized

Lawrence Krauss has just put up a long interview with Lenny Susskind. I was listening to it while doing something else, found myself shocked when the discussion got to the current state of string theory to find that I mostly agreed with what Susskind has to say. Here's some of it (around 1:23):

> I can tell you with absolute certainty String theory is not the theory of the real world, I can tell you that 100%…. My strong feelings are exactly that String theory is definitely not the theory of the real world.

Here he's referring to "String theory" (with a capital S) as the superstring theory which has a known definition, at least perturbatively. He goes on to explain that he thinks it possible that some very different generalized or "string-inspired" theory might have something to do with the real world but that:

> we don't know and we don't know if String theory will help us find those things…. We are still uncertain about whether whatever it is: "generalized", "boundaries pushed", "string-inspired theories". We don't know and I think that's the bottom line now.

As for the idea that we just need to understand how to break the superstring supersymmetry to get the real world:

> People will say oh all you have to do is spontaneously break supersymmetry, blah blah well it's been 25 30 40 years by now that nobody's figured out how to do that.

On the "failure" description, he thinks String theory has not been a failure in the sense of showing gravitation and QM can coexist, but, as for particle theory:

> Whether it's been a failure in producing a theory of Elementary particles I would guess remains to be seen but String theory (with a capital S) is not the right theory.

On the wormhole publicity stunt (1:38):

> Not a very good experiment… the experimental implementation of it left more than a little bit to be desired. That got much too much hype, yeah.

Krauss brings up the the string theory hype problem, with

> we owe it to the public I think to be careful in what we say. I understand we get excited and it's fine for physicists to get excited with each other but we have to be careful of what we say we can do because if we don't it'll come down and bite us in the butt.

Susskind's response is:

> I completely agree with that, but on the other hand there is a tension between that and the importance of keeping excited and letting the public know why we're excited.

I'd have liked to hear Susskind's thoughts on what should be done when a bunch of theorists write popular books hyping ideas that don't work. Whose job is it to explain to the public that they were misled by overenthusiastic scientists? If he's not going to do that, would he at least be publicly supportive of those who do this job?

Another question I'd have liked to ask him would be: given that he agrees that the idea of using string theory to understand particle physics hasn't worked and we don't know that "string-inspired" is the right direction to go, what next? If "string-inspired" is not the way to go, should we just give up on going beyond the SM? If not, how would he encourage young people to work on non-"string-inspired" ideas about unification that might take us beyond the SM?

Comments

1. **John Horgan**
   August 7, 2024

   Susskind pushed really hard for landscape theory, which is a consequence of string theory, in his pop book The Cosmic Landscape. And as I recall he said on The Edge website that if you reject landscape theory you're probably a creationist. So now he's telling us that he takes it all back?

2. **Z Y**
   August 7, 2024

   This is awesome, he is the first high profile string theorist that I know that is publicly recognizing the things are they really are, it may start something… or not..

3. **Peter Woit**
   August 7, 2024

   John,
   I was thinking of writing more in this blog post, about the last part of the conversation with Krauss, where he talks about the landscape. There he's much more careful in his argument, no nonsense about rejecting the landscape anthropic argument as being akin to creationism, or rejecting the theory of evolution.

   He's much more careful there than in his popular book. His argument now is that if you had a unified theory and then knew "The equations" governing the world, you could look at the solutions to these equations. He acknowledges that maybe if we find "The equations", they will have only one, or a few solutions. Then any multiverse created by bubbling baby universe would have the same laws everywhere, no role for anthropics possible.

   But, he argues, if you have "The equations" and they have a vast number of solutions, then the multiverse picture with our universe a randomly chosen anthropically allowed solution is a consistent possibility. He tries to argue that no one has any other viable theory for the size of the CC, so this deserves some sort of status as best known explanation.

   The problem of course is that we don't know "The equations". My problem with what he was doing in the past was always that he and others were trying to argue that this was somehow evidence for string theory, or at least explained why string theory can't predict anything. What struck me about his current argument is that he no longer is doing this. Instead of arguing that the equations of the String theory-based KKLT construction were "The equations", now he's saying that String theory doesn't work as a unified theory, that we don't at all know what "The equations" are.

   So, now his anthropic landscape argument is just an abstract argument about an unknown theory with conjectured properties, no longer an argument based on string theory.

To pin him down, Krauss should have asked him specifically what he now thinks about KKLT as an explanation of the string vacuum and a unified theory. I'm guessing he might now take the correct position: you can't tell whether KKLT vacua make sense since you don't know "The equations" that they are supposed to solve.

4. **Peter Woit**
August 7, 2024

Z Y,
I think what he's saying here is what most prominent string theorists have been thinking for a long time. In private, I've found they often agree that known string theory is useless to do particle theory, say only reason to work on it is as a model of quantum gravity.

The question then is why the hype about string theory and unification is still getting heavily promoted by many people, with no push-back from those who know it is nonsense. It's one thing to justify hype from scientists overly enthusiastic about a new idea they are working on, but I don't think there's any way to justify hype from scientists trying to evade acknowledgement that their ideas from 20-30-40 years ago haven't worked out.

5. **Sabine**
August 7, 2024

amazing, thanks for digging up

6. **hrdlifil**
August 8, 2024

Of course String theory is not a theory of the real world, it is not a theory of anything. The real STRING THEORY – M-Theory cannot be built upon shaky handwaving grounds.

We first need to understand what is a full non-rational 2-dimensional σ-model conformal field theory, then what is full non-perturbative YM and finally then we can have the true rigorous non-perturbative M-Theory.

Yes this might be a 10 000 year problem, but every problem needs to start somewhere and there is no other game in town other than String theory which serves as a good starting point.

7. **Jim+Eadon**
August 8, 2024

"Krauss brings up the the string theory hype problem"
Dear Prof Woit, do you think that the laughable Quantum Wormhole hype is inadvertently crippling the String Theory bandwagon, by exposing the hype machine?
It would be ironic if the downfall of hype-driven String Theory is catalysed by hype itself, in this case, by non-ST (wormhole) hype.

8. **Peter Woit**
August 8, 2024

Jim Eadon,
The wormhole hype was so bad that even the string theorists recognized it and some got annoyed. Susskind though here seems to me to be taking the position that
1. This was "much too much hype".
2. But it is understandable that people do this kind of thing, it's not something worth doing anything to stop.

For some context, Susskind himself has a lot of responsibility for what happened. He was one of very few people the hypesters chose to let in on the planned hype campaign before it started. As part of this, he and a collaborator wrote the article in Nature accompanying the hype announcement. When I told the people at Quanta that they had been had, I was told that I was wrong, that they had consulted four experts while writing their article and producing their video. One of the four was Susskind.

9. **Adam+Treat**
August 8, 2024

Seems Susskind still hasn't grappled with Sabine's point that it is not possible to specify a probability distribution over possible universes even if we had 'the equations.' Good for him for divorcing the anthropic nonsense from "String theory" landscape, but still would be better if he grappled with the deeper point.

10. **Peter Woit**
August 9, 2024

Adam Treat,

I can imagine versions of "The equations" which would give dynamics of a big bang and baby universe creation which would imply a probability distribution. I can also image versions that wouldn't. The basic fact is that we know nothing at all about this, are ignorant not just of "The equations" but of what variables they are in.

One can entertain oneself by arguing about the properties of completely unknown equations, but this isn't science in any sense. You might as well argue about the Gods and what they are up to when they create universes.

11. **Peter**
August 9, 2024

I studied physics. It was a long time ago, and I never really practiced it, but the debate about string theory has reached the point that I don't even understand anymore what the expression "the equations" is supposed to mean in this context.
Does anyone have an idea what these equations should look like, other than being able to describe 10^500 (or whatever) universes, of which our universe might be one?

12. **Peter Woit**
August 9, 2024

Peter,
What Susskind is saying is:

1. For String theory, with a capital S, meaning the theory we know the equations for, there is no possible connection to the real world.

2. For some unknown better theory that does describe the real world, “string-inspired” or not, there will be “The Equations” which define it. We have no idea what these are. He wants to go on and make an argument for the anthropic explanation of the CC, based on assuming certain properties of the these unknown equations.

13. **Mitchell Porter**
August 13, 2024

There are many papers on supersymmetry breaking in string theory, as well as on non-supersymmetric string theories, so I don’t know what Susskind is talking about. Does anyone know what he’s talking about?

14. **Peter Woit**
August 13, 2024

Mitchell Porter,
Anyone who has actually been looking at those thousands of papers on supersymmetry breaking knows what Susskind is talking about: they don’t solve the problem. As Susskind explains, people have been trying for 40 years without success.

You can claim supersymmetry as a good idea because it has new symmetry generators so makes new predictions (equal mass superpartners with specific properties) if unbroken. The predictions are wrong, so you have to break SUSY (explicitly or dynamically). The problem is that any known way of doing this complicates the theory while either making new wrong predictions, or just allowing you to move the new predictions somewhere unobservable, predicting nothing at all observable. This has been the problem with SUSY, in QFT or string theory, since the beginning.

15. **Mitchell Porter**
August 14, 2024

Peter,
Perhaps I can explain myself by analogy with KKLT. The 2000s debate over KKLT was on matters like, is this really how reality works, is this still science, and so on. But the 2020s debate over KKLT is on matters like, is this how *string theory* works, or do nonperturbative effects actually collapse the whole construction.

What I can’t tell, is if Susskind is calling stringy models of SUSY breaking into doubt *as rigorous string theory* (analogous to KKLT 2020s debate), or if he’s just saying that they are all empirically unsatisfactory.

16. **Peter Woit**
August 14, 2024

Mitchell Porter,
The debate over whether non-perturbative effects stabilize moduli goes way back, KKLT itself was just part of that ongoing debate. In the 2000s leading figures in the field were debating this, the debate over whether this turned the subject into pseudo-science was a follow-on. Yes there are still people debating whether KKLT works now, but no one of any significance is paying attention any more.

It became clear early on in this debate that when you don’t have a non-perturbative theory, arguing over the technicalities of non-perturbative effects doesn’t really make sense. Susskind now I think is just following the mainstream opinion that, as currently understood, String theory with a capital S can’t answer this kind of question.

Whether he’d put some specific string theory model of SUSY breaking in the capital S category or not I don’t know, you’d have to ask him. But I think it’s clear he now realizes this is irrelevant: the ones that you could argue for don’t say anything about the real world anyway. From what he says, for a connection to the real world he’s not looking for some minor improvement on what is known, but for some different theory (his interest in ER=EPR indicates something very different than capital S string theory).

17. **Dr. Udai S Singh**
August 17, 2024

I just want to thank all of you people, and especially the progenitor of this blog, Dr. Peter Woit. It took me forever to finally find a sensible, scientific, nuanced rebuttal to all the String Theory crap I have been deluged with for decades. Now I can at least hold reasonable and logical conversations with amateur science enthusiasts who are being misled by this huge crowd of people like Kaku.

18. **Michael Naray**
August 18, 2024

This is no surprise, surely? If you watch Susskind’s lectures that have since been turned into the ‘Theoretical Minimum’ series of books, he unequivocally states to the students that ‘string theory is not a theory of the real world. It doesn’t work, it doesn’t match observations. It just doesn’t.’ And that was recorded over 10 years ago!

19. **Mitchell Porter**
August 21, 2024

Michael Naray – I have found where he says that: at 1:17:45 here

https://www.youtube.com/watch?v=NZ-ElsvYKyo

(at theoreticalminimum.com, this lecture is listed under “Supplemental Courses” as “String theory wrap-up”).

20. **B. Malpani**
September 11, 2024

…[i]the importance of keeping excited and letting the public know why we’re excited.[/i]

He can be as excited as he wants, but what is important is that he produce theories that make testable predictions. That, in reality, is what he’s paid to do, but he and his cohorts seem to have kinda given up on that. “Letting the public know why he’s excited” is *not* part of his job description; it seems to have become a license to lie for the sake of pressuring bureaucrats to continue to fund his failures.

Quick Links

August 9, 2024
Categories: Uncategorized

Starting to write a longer, more technical posting, but for now, a few quick links:

- The film *Particle Fever* (for more about this, see here) may get made into a musical. With a little luck they'll skip the nonsense about the multiverse that blemished the film.
- Tommaso Dorigo performs an experiment that the airline industry probably doesn't want publicized.
- My big problem with discussions of climate change has always been that I'm not able to evaluate the science myself, so when told to "Trust the Science", I get queasy, all too aware that in some parts of science I can evaluate, "Trust the Science" is a really bad idea. Luckily, there is someone with a track record I can trust, Sabine Hossenfelder, who has a new video about trusting scientists and climate change. She has carefully looked into this, and explains her conclusions: here you can trust the science, the problem is very real.

  If you want to argue about climate change though, you're going to have to find some place else.

Comments

1. **Phil HALPER**
   August 13, 2024

   Sabine does great video but please watch this example where she makes basic physics errors and is guilty of the overhyping of bad results she accuses others of: https://www.youtube.com/watch?v=foq4nVAwEao

2. **Peter Woit**
   August 13, 2024

   Phil Halper,
   I don't think Sabine is infallible. She has a clear inclination and agenda to try and take challenges to conventional wisdom seriously, looking into them carefully rather than just dismissing them. This can get her into trouble, but it's exactly what gives her a lot of credibility in this case, where she's siding with conventional wisdom.

3. **Davide Castelvecchi**
   August 14, 2024

   I feel like we keep going in circles. There's a long tradition of physicists who say they don't trust the science until they look into it for themselves: a decade ago, it was Richard Muller … https://www.nytimes.com/2012/07/30/opinion/the-conversion-of-a-climate-change-skeptic.html

4. **John Baez**
   August 14, 2024

   If you haven't watched all of Sabine's video, you might mistakenly guess she thinks climate scientists are
   *over*estimating the severity of the climate crisis.

5. **John Baez**
   August 14, 2024

   In case I wasn't clear enough: she thinks climate scientists tend to *understate* the severity of the climate crisis, at least in their public pronouncements, and she gives a list of reasons for why they would.

6. **martibal**
   August 14, 2024

   I really wonder why the reliability of climate change science needs to be discussed continuously ? Of course it is good to discuss scientific results, and string theory sadly shows that "trust the scientist" is not a sufficient argument, but I doubt many theoretical physicist have studied in great details the data analysis that lead to the discovery of – say – the Higgs boson. I guess many physicist just blindly trust the colleagues from CERN. So why is climate change science always put in doubt ?

7. **Peter Woit**
   August 14, 2024

   All,
   Please, no more comments about climate science. The only point of my posting was
   1. I personally have neither the time nor competence to evaluate the reliability of different climate models projecting the future climate.
   2. "Just trust the mainstream science establishment" doesn't work for me based on long experience in a different scientific field.
   3. I'm grateful that in Sabine Hossenfelder we have a science communicator who doesn't just tell us "trust scientists" and recommend the linked video.

8. **Tommaso Dorigo**
   August 18, 2024

   Hello Peter,
   thanks for the link! About radiation levels high up – I think that indeed for some of us a not insignificant part of our radiation dose comes from international flights. E.g. Gordon Watts just made the 2M miles mark with United, so presumably he's been up for 3M counting other companies. This is about 5000 hours at high altitude, where a x20 radiation level makes it about as much of a dose as one gets in 25 years on the ground. Gordon flies a lot, but there is people who flies much more (apart from airline pilots and hostesses, of course)… And now with the "all you can fly" programs (e.g. wizzair) this can only worsen.
   But I was indeed more concerned by observing the leaks from airport scanners!
   Cheers,
   T.

9. **David+Roberts**
   August 19, 2024

Bear McCreary is a phenomenal composer. If nothing else, the score to that musical will be well worth a listen.

Two More

August 13, 2024
Categories: Langlands, Uncategorized

Two more items:

- I can't recommend strongly enough that you watch the new Curt Jaimungal podcast with Edward Frenkel. The nominal topic is the recent proof of the geometric Langlands conjecture, but this is introductory material, with geometric Langlands and the proof to be covered in a part 2 of the conversation.

  Before getting into the story of the Langlands program at a very introductory level, Frenkel covers a wide range of topics about unification in math and physics and the difference between these two subjects. While there's a lot about mathematics, Frenkel also gives the most lucid explanation I've ever heard of exactly what string theory is, what its relation to mathematics is, and what its problems are as a theory of the real world. He has been intimately involved for a long time in research in this field, playing a major role in the geometric Langlands program and working together with both Langlands and Witten.

- Nordita this month is hosting a program on quantum gravity, aimed at covering a diversity of approaches. Videos of the talks are appearing here. The program includes an unusually large number of panel discussions about the state of the subject. One of these is a discussion of the Status of the string paradigm which has the unusual feature that two string theory skeptics (Damiano Anselmi and Neil Turok, who have worked on string theory) have been allowed to participate in the six member panel.

  The response to the failures over the last forty years seems to be that current researchers should not be held accountable for ways in which the string theory paradigm of the past has not worked out. Things are fine now that they have moved on to the Swampland program, have realized that progress on string theory will have a 500 year time-scale, and know that string theory is better than the Standard Model since it has a finite or countable number of ground states.

Comments

1. **Bertie**
   August 14, 2024

   What a marvellous podcast with Edward Frenkel! I think the host was getting frustrated that Frenken stayed at an elementary level in his explanation, but what a blessing this was for non-mathematicians like me. His introductory digression into string theory was also amazing (with hilarious reference made to Archimedes – yes jump out of the bath and run around naked, but not for 40 years).

2. **Some guy idk**
   August 15, 2024

   Would you say that the connection between geometric Langlands and S-duality is a curiosity or that it hints at something larger and of greater use?

3. **Peter Woit**
   August 15, 2024

   Some guy idk,
   Definitely this is a reflection of something deep about math and physics that we don't fully understand. For what is understood, Frenkel is a great source, I also recommend looking at David Ben-Zvi's talks and lecture courses on the subject.

4. **Karl Young**
   August 15, 2024

   Though I find Jaimungal a bit overbearing at times (e.g. the occasional melodramatic background music) I also find great value in what he does, particularly for retired generalist geezers like me who no longer keep up with the cutting edge of research in any area. And I have to agree that the interview with Frenkel was a total revelation. Frenkel's depth of knowledge and philosophically cogent views re. Mathematics, Physics, and Mathematical Physics, combined with his humor and common sense are a truly rare gift. I found it amazing that in that relatively short presentation I finally attained some insight into why proving Taniyama-Shimura-Weil in turn proved Fermat's Theorem as well as things like what's so important about N=4 supersymetry (maximally consistent extension of the Poincaré group). I wait with bated breath for the geometric Langlands conclusion !

5. **C**
   August 16, 2024

   They proved geometric Langlands. They soaked the metaphorical nut to the maximum. Now we must apply their technologies and hammer and chisel away at arithmetic Langlands until nothing is left.

6. **David in Tokyo**
   August 16, 2024

   Karl Young writes:
   "Though I find Jaimungal a bit overbearing at times"

   Maybe. But Frenkel would get stuck on a point and not move on, and Jaimungal would pull him back on track. Sure, I found myself disagreeing occassionally, but overall, he did a great job.

   "particularly for retired generalist geezers like me"

   Exactly. The magic of Frenkel is that he shows us non-specialists what we need to be looking into to get one step deeper into the material.

   This is the opposite of a common complaint I have with Quanta math articles: it'll say "a bit is a thing that can take on one of two values" and then jump into bleeding-edge-level math without showing you how to get there.

7. **Will Sawin**
   August 17, 2024

   @C

It's important to note that they only proved unramified geometric Langlands. The unramified geometric Langlands conjecture is often called the geometric Langlands conjecture because the ramified version is harder to state, and most work has been in the unramified case, but I would still guess that proving ramified geometric Langlands is easier than proving the function field Langlands conjecture, which is easier than proving the reciprocity conjecture for representations algebraic at infinity in the number field case, which is easier than proving the full Langlands conjectures over number fields.

8. **Ellie Kesselman**
   August 18, 2024

   I'm probably a first-time commenter here, but a long-time reader. Unlike the rest of the comments prior to mine, I wanted to thank you for sharing details of the Nordita program. I began with Status of the String as you recommended. I found the panel contributions of the two former string theorists (Damiano Anselmi and Neil Turok) to be the most informative and accessible. Anselmi presented first; too bad he was halved and even quartered in the video, all the way to the left of the screen. Turok didn't mince words, nor was he sarcastic! Both did good Q&As.

   Slightly off-topic: In the Wikipedia entry for Not Even Wrong, a few outgroup editors–myself included–have been trying to include mention of this blog. Not Even a single sentence has been allowed. Amusingly, the same editors who are the most anti-theist (their recent Wikipedia initiative was to categorize all Hindu deities, the Christian trinity, and G-d and the prophets of Judaism as "in-world characters") are also the most ardent supporters of unprovable physics.

9. **Low+Math,+Meely+Interacting**
   August 19, 2024

   Frenkel must be a lot of fun to interview. I've really enjoyed hearing his thoughts on these and other subjects that are now available online.

   It's tough to not get a bit discouraged, despite his jovial attitude. Insuperably large numbers of Calabi-Yau manifolds and the moduli stabilization problem are literally decades-old issues. It's nice to have it summarized so succinctly, but he could have provided exactly the same assessment 20 years ago, as others did, and have been doing. Yet today we have the Swampland Conjectures, one of which flatly denies our universe is what precision cosmology (which today is a fair term) tells us.

   The basic fact is that String Theory (with a capital S, I suppose) has been in violent disagreement with observation from the beginning, which is an astonishingly long time for anything remotely resembling science to be allowed to persist. That really is the bottom line, and philosophical arguments about beauty and the validity of anthropic reasoning, etc., etc. ad nauseam, should really have been minor asides to the basic, glaring fact of the matter: These ideas are just incredibly empirically wrong. They always have been, and there's precisely zero justifiable reason to think by now they won't always be.

   Occasionally this lands again on my brain like a physical blow, and I'm just bewildered again. You'd think by now I'd be inured to the disillusionment, especially given the state of the world at large, but afraid I'm not there yet.

10. **Peter Woit**
    August 19, 2024

    LMMI,
    What's discouraging are the many influential people in this subject who, forty years later, still promote the same hype. The interviews with Susskind and Frenkel are encouraging in that more and more people who understand this subject are willing to speak up and acknowledge that things haven't worked out.

11. **Low+Math,+Meely+Interacting**
    August 19, 2024

    Almost on cue, Quanta comes out with an article suggesting the putative DESI anomaly is support for the Swampland program. Ambulance chasing with a heap of grossly exaggerated relevance. My disillusionment continues unabated.

12. **Paolo Bertozzini**
    August 20, 2024

    Dear Peter, thanks for the interesting and useful links (as always).

    Let me start with a consideration on the quantum gravity program in Nordita: "(non-commutative) geometry" was essentially absent from the panorama of the many approaches to quantum gravity covered there. This is not surprising at all: (as I already mentioned here in comments to previous posts) it is already several years that approaches based on "geometry" somehow disappeared from the current trends in QG where, across all the several lines of research, there is a uniform substantial agreement on the fact that "geometry" is only *emergent* and the fundamental degrees of freedom are quantum *pre-geometric*. The main trouble for researchers that try to dedicate time and energy on the study of *geometry of QFT* in view of applications to QG (including spinorial/twistorial geometry in 4-d), is not so much now the hype due to string theory, holography or quantum information, but this kind of wide consensus that deprives general relativity (and geometry) from a role in a fundamental theory. This, in my opinion, is deeply related to the historical evolution of QFT "post-renormalization", with its ideological reliance on "effective field theory", reductionism and "emergence".

    Among the speakers/moderators invited in the several panel discussions in Nordita, I found particularly interesting N.Turok (with His intriguing alternative "mirror-cosmology") and (in view of my personal lines of research in algebraic QFT in QG) the point of view of S.Giddings on the encoding of geometry via the net of observables algebras on Hilbert space … something that in algebraic quantum field theory goes back to R.Haag and U.Bannier (and was further developed, in view of reconstruction of Minkowski space-time, by D.Buchholz and S.Summers).

    The podcast from Frenkel is for sure very pleasant: I do appreciate His great ability to simplify a very complicated topic in a way that still allows to grasp some fundamental ideas. Still, I suspect that "oversimplification" in the discussion about the relation between mathematics and physics is deeply misleading: it might serve as a naive explanation/justification for the huge past and present interest of some mathematicians in mathematical topics related to, or inspired by, string theory, but nothing more than that. The interplay between the notions of "mathematical truth" and "physical reality" is way more complex than the mere statement that physical reality is a singleton in a landscape of mathematical truths (that btw seems to suggest that mathematicians are living in a "delusion of imaginary landscapes"). Furthermore mathematics is not completely immune to the social pathologies created by hype and powerful influences that have been so evident in certain areas of theoretical physics: the role of "standard proofs" or "computer simulations" in mathematics is very similar to that of "reproducible experimental evidence" in physics and hence similar issues are going to be found when reliance on such pillars is shaking or just changing (see "theoretical-mathematics" as in Jaffe-Quinn or the current discussions on "automated theorems checking and AI" as exposed in the recent Scientific American interview to T.Tao or in M.Harris' blog).

    Best Regards 🙂

13. **cedric bardot**
    August 20, 2024

    Reading the program on Quantum Gravity : the title of the second week *Workshop on **Formal Aspects and Consistency of Quantum Gravity Approaches*** reminds me of the following quote of Pierre Cartier (who died a few days ago*)

> To conclude, I would like to offer some remarks about the word “formal”. For the mathematician, it usually means “according to the standard of formal rigor, of formal logic”. For the physicists, it is more or less synonymous with “heuristic” as opposed to “rigorous”. It is very often a source of misunderstanding between these two groups of scientists.

source: https://www.emis.de/journals/SLC/wpapers/s44cartier1.pdf
His death is a loss for the mathematical community I think from my narrow perspective as he was a bridge between mathematicians and mathematical physicists if not theoretical physicists and mathemagicians.
It might be worth noting *his dream of a cosmic Galois group* is still alive and a kicking example of hopes and dangers or subtleties in conjectures illustrated for instance by Francis Brown work on multizetas or Elliott Gesteau last preprint on the factorization paradox.

*https://www.lemonde.fr/disparitions/article/2024/08/19/pierre-cartier-mathematicien-francais-est-mort_6286756_3382.html

The Elegant Universe: 25th Anniversary Edition

August 20, 2024
Categories: Book Reviews

Brian Greene's The Elegant Universe is being reissued today, in a 25th anniversary edition. It's the same text as the original, with the addition of a 5 page preface and a 36 page epilogue.

The initial excitement among some theorists in late 1984 and 1985 that string theory would provide a successful unified theory had died down by the early 1990s, as it had become clear that this was not working out. This didn't stop the theory from continuing to be sold to the public in hype-heavy books such as Michio Kaku's 1995 Hyperspace. Interest in string theory among theorists was revived in the mid-nineties by the advent of branes/dualities/M-theory. The publication of the *The Elegant Universe* in 1999 brought to the public the same hyped story about unification, together with the news of the new "M-theory". The book was wildly successful, selling something like 2 million copies worldwide. A 3-hour PBS special based on the book reached an even larger audience.

From the beginning in 1984 I was dubious about string theory unification, and by the late 1990s could not understand why this was dominating physics departments and popular science outlets, with no acknowledgement of the serious problems and failures of the theory. From talking privately to physicists, it became clear that the field of particle theory had for quite a while become disturbingly tribal. There was a string theory tribe, seeing itself as embattled and fighting less intelligent other tribes for scarce resources. Those within the tribe wouldn't say anything publicly critical of the theory, since that would not only hurt their own interests, but possibly get them kicked out of the tribe. Those outside the tribe also were very leery of saying anything, partly because they felt they lacked the expertise to do so, partly because they feared retribution from powerful figures in the string theory tribe.

At some point I decided that someone should do something about this, and if no one else was going to say anything, maybe I needed to be the one to do so. My unusual position in a math department pretty well insulated me from the pressures that kept others quiet. I've told the story of the article I wrote starting at the end of 2000 here. It was put on the arXiv in early 2001 and ultimately published in American Scientist. Looking at it again after all these years, I think the argument made there stands up extremely well. While there was no direct reference to the Greene and Kaku books, there was:

> String theorists often attempt to make an aesthetic argument, a claim that the theory is strikingly "elegant" or "beautiful". Since there is no well-defined theory, it's hard to know what to make of these claims, and one is reminded of another quote from Pauli. Annoyed by Heisenberg's claims that modulo some details he had a wonderful unified theory (he didn't), Pauli sent his friends a postcard containing a blank rectangle and the text "This is to show the world I can paint like Titian. Only technical details are missing." Since no one knows what "M-theory" is, its beauty is that of Pauli's painting. Even if a consistent M-theory can be found, it may very well be a theory of great complexity and ugliness.

The subject of string theory and the state of fundamental physics was complicated and interesting enough that I thought it deserved a book length treatment, which I started writing in 2002 (the story of that is here). The book I wrote was not a direct response to T*he Elegant Universe*, but was an alternative take on the history and current state of the subject, trying to provide a different and more fact-based point of view.

During this time, Kaku came out with his own M-theory book, Parallel Worlds, published in 2004. Also in 2004, Greene published a follow-up to T*he Elegant Universe*, entitled *The Fabric of the Cosmos*, which was the basis several years later of a four-hour Nova special.
Over the last twenty-years there's been no let up, with Kaku's latest The God Equation, yet another hype-filled rehash of the usual string theory material. Greene regularly uses his World Science Foundation to do more string theory promotion, most recently putting out "The State of String Theory", where we learn the subject deserves an A+++.

In recent years I've often heard from string theorists who feel that their research is getting a bad name because of the nature of the Greene/Kaku material. They see this hype as something that happened long ago, back before they got into the subject, so ask why they should be held accountable for it. When asked why they won't do anything about the ongoing hype problem, it becomes clear that string theory tribalism is still a potent force.

Turning to the new material in this new edition of the book, much of it is the usual over-the-top hype, although often in a rather defensive mode:

> the past twenty-five years have been such an astonishingly productive period that exploring progress fully could easily fill an entire book on its own… The fact is, the past twenty-five years have been jam-packed with discoveries in which string theorists have scaled towering problems and dug deeply into long-standing mysteries… the decades of rich development in string theory carried out by some of the most creative, skeptical and discerning minds on the planet is the most readily apparent measure of the field's vitality. Scientists vote with their most precious commodity — their time. By that measure, and correspondingly, the measure of vibrant new ideas that have opened stunning vistas of discovery, string theory continues to be a source of inspiration, insight, and rapid progress.

While some scientists have left the field

> others, indeed so many others that string theory has been berated by for attracting too many of the highest-caliber scientists, have found that the pace of new theoretical discoveries and novel physical insights is so rapid and thrilling that they are propelled onward with vigor and excitement.

The epilogue mostly deals with three topics. The first is the failure to find SUSY at the LHC, which Greene explains is perfectly compatible with string theory, and that, even before the LHC turned on:

> there were theorists at that time who emphasized that string theory seems to favor superheavy superpartners, far too massive for the Large Hadron Collider or even any remotely realistic next-generations colliders to produce.

He acknowledges that at the present time string theory predicts nothing at all about anything, that even if we had a Planck scale collider:

> We would still need to understand the theory with greater depth to make detailed comparisons between calculations and data, but in that imagined setting experiment would guide theorizing much as it has across a significant stretch of the history of physics.

The second topic is the string theory landscape and the anthropic multiverse "prediction" of the CC, with about ten pages devoted to explaining that

> the dark energy has its measured value because if its value had been significantly different, we would not be here to measure it.

The final topic, taking up twelve pages, is AdS/CFT. The conclusion is that:

> *We now have powerful evidence that — shockingly — string theory and quantum field theory are actually different languages for expressing one and the same physics*. In consequence, the experimental luster of quantum field theory casts a newfound experimental glow on string theory.

No more "what is M-theory?", instead we're told that the question "What is the fundamental principle underlying string theory?" gets answered by:

> the new lesson seems to be that quantum mechanics already has gravity imprinted into its deep structure. The power of string theory is that its vibrating filiaments allows us to more easily see this connection.

The last section is "A Final Assessment." No A+++, but:

> In the arena of unification, both in terms of showing that gravity and quantum mechanics can be united as well as demonstrating that such a union can embrace non-gravitational forces and matter particles too, I give string theory an A. String theory surmounts the difficult mathematical hurdles that afflicted earlier work on unification and so, at least on paper, establishes that we have a framework in which the dream of unification can be realized.
> In the arena of experimental or observational confirmation, I give string theory an incomplete.

One thing I was looking for in the new material was Greene's response to the detailed criticisms of string theory that have been made by me and others such as Lee Smolin and Sabine Hossenfelder over the last 25 years. It's there, and here it is, in full:

> There is a small but vocal group of string theory detractors who, with a straight face, say things like "A long time ago you string theorists promised to have the fundamental laws of quantum gravity all wrapped up, so why aren't you done?" or "You string theorists are now going in directions you never expected," to which I respond, in reverse order "Well, yes, the excitement of searching into the unknown is to discover new directions" and "You must be kidding."

As one of the "small but vocal group" I'll just point out that this is an absurd and highly offensive straw-man argument. The arguments in quotation marks are not ones being made by string theory detractors, and the fact that he makes up this nonsense and refuses to engage with the real arguments speaks volumes.

Note: after tomorrow I'll be on a short vacation for a while in San Francisco and dealing with blog comments might take longer than usual.

Comments

1. **martibal**
   August 20, 2024

   You said Green's sold two millions. Just to have an idea of the impact of the critics versus hype, how much did you sell with your book ?

2. **Peter Woit**
   August 20, 2024

   martibal,
   I didn't pay much attention to that number, and almost all the sales were long ago (the book now sells only a small number of copies/year). I think the right order of magnitude was significantly more than 10,000, significantly less than 100,000.

3. **thphys**
   August 20, 2024

   I was one in a generation of physicists motivated to study string theory in graduate school in large part due to The Elegant Universe (I was in high school when it was first published). My entering graduate class at Stanford almost 20 years ago started with about a dozen students who initially wanted to study string theory, but nearly all moved on to other realms of theoretical physics after taking a course or two on string theory as first years. I think among many students then it became very obvious very quickly that the purported beauty of string theory was at most skin deep.

   It's a rather disappointing irony that with this re-release of The Elegant Universe, Greene seems to have leaned in to the content of the second half of the book, which was by far weaker than the first half (even with no skin in the string theory game). At the time anyway, from my naive student perspective, there were very few titles on the market that made the oddities and confusions of quantum mechanics and relativity so accessible and engaging as the first half of The Elegant Universe. With major results and Nobel prizes in both of these fields since its initial publication, it seems rather a shame that he has just doubled down on string theory. Though I guess that is at least consistent with the World String Festival that he hosts.

4. **Felix**
   August 20, 2024

   I read The Elegant Universe just before starting my undergraduate degree and was totally convinced by it that string theory was going to solve all of physics. It took a few months of studying in a physics department full of actual particle physicists to realise none of them took it seriously, and most of them thought it was embarrassing. I tried to watch the 'The State of String Theory' panel discussion but gave up very quickly once it turned into an uncritical string theory love-in. Michio Kaku has a lot to answer for, but I think Brian Greene is up there for guilt in terms of misinformation.

5. **Mark**
   August 21, 2024

   I did an undergraduate degree in theoretical physics thanks to books by Kaku and Greene. Thankfully an experimental particle physicist convinced me I would be better off switching to experimental work for my PhD.

6. **Peter Woit**
   August 21, 2024

   It's interesting to see that the comments back up an argument I've often heard from physicists when I criticize books like this. They tell me "sure, the material in that book about string theory is nonsense and misleading the public, but it's getting young people excited about physics and interested in becoming physicists. Once they do become physicists they'll realize this is nonsense and go on to do some real physics."

7. **Jan-Magnus Økland**
   August 21, 2024

   I believe that, in that book, Greene told the story of my once PhD advisor, the late Stein Arild Strømme and Geir Ellingsrud finding the wrong number of twisted cubics on the general quintic threefold, only to be corrected by a new string theory way of coming up not only with the corrected number but a recursive formula for degree d rational curves on there. Stein Arild quickly found the coding error and gave string theorists a win. That complex quintic threefolds are real six dimensional spaces with flat metric that could curl up in every space time point making ten dimensions and rational curves being important in that theory and giving candidates for a model of our world suddenly was all the rage. If I recall correctly there were too many candidates. However the count of 317206375 still stands. Not even wrong, being a fitting phrase for it.

8. **Paul**
   August 21, 2024

   thphys, that sounds like the incoming class of 2007. I was there and can confirm. I remember during graduate school orientation when a string theorist told the incoming particle theorists, "We don't have room for all of you, so you'll have to switch to something else." That's right after they admitted us knowing our intentions, and we accepted.

It's an interesting phenomenon of unintentional bait and switch. Physics departments love the extra attention, publicity, and enrollment figures, but not everyone likes the field that is doing the recruitment.

9. **Doug+McDonald**
August 21, 2024

In 1967 I was a Chemistry grad student (doing scattering experiments and their theory) and took an advanced QM course in Chemistry and sat in on Glashow's second-level physics course. One big thing at the time was (the tail end of) S-matrix theory and Regge Trajectories.

Today I was looking in Wikipedia and found, in an article on Quantum Chromodynamics the following:
"The relation between the short-distance particle limit and the confining long-distance limit is one of the topics recently explored using string theory, the modern form of S-matrix theory."

To me that seems to be very damning.

10. **Peter Woit**
August 21, 2024

Doug McDonald,
It's more the amplitudes program that is the modern form of S-matrix theory. String theory is something much worse….

That sentence is complete nonsense, and the quoted papers don't correspond to the claim. The idea that QCD in some other variables is some sort of string theory has been around for a very long time, I spent a lot of time thinking about that in grad school. The problem is exactly the opposite of what the sentence says: no one has any idea how to get the asymptotic freedom of QCD out of string theory.

To help Wikipedia out I deleted that sentence.

11. **Doug+McDonald**
August 21, 2024

Thanks for correcting Wikipedia.

12. **Eric+Weinstein**
August 21, 2024

I am not aware of anyone holding so much as a single multi-day conference on physics forcing a re-examination of whether this is "Academic Capture" by string theorists where the leaders of the failed string generation were forced into head to head contact with String Theory's critics.

By my estimation, it would cost the same ammount in 2024 dollars for a string of 3 conferences like Shelter Island, Oldstone and Poccono cost in the 1940 when Fundamental Physics ended its 19 year stagnation. Surely I am not the only one to have noticed this. Yet everyone treats this as if it were madness. Odd no?

Is there ever a point when this is too long, failed and insane to be a mistake, fad or accident? Would that be 50 years? 100 years? 200 years?

I don't think in 2024, this is gauranteed to be a bug. I think it must be considered at this point that we may be looking at a feature. String theory, unlike real physics, is safe and has that as an actual major advantage over say the Spinorial/Twistor approaches. Further it only acknowledges one rival that it has chosen itself (LQG) and only aggrees to contests it, itself judges by String standards.

Real physics remains the most dangerous subject known to man, as we know from WWII, Castle Bravo and the Atomic Energy Acts of 1946 and 1954. Imagine what the next theory might bring. It could be arbitrarily powerful and much easier to deploy.

String theory doesn't confer military advantage. It's harmless. You can do it with your friends. You can do it with your rivals. It's totally harmless and safe. What it does is simple to state: it kills funding, interest, morale and opportunity in vital rival theories of physics while stagnating the field, far better than any known alternative. It's a near perfect sterility program.

But, of course, that is madness to suggest that $500,000 dollars (or less!) could be used to figure out whether this is the only game in town as claimed by people who refuse to constructively and collegially engage their colleagues. After all, our String Theorists are far too busy succeeding and winning to consider wasting their precious time on alternatives…IYKYK. 😉

13. **Carl Feinberg**
August 22, 2024

I attended the "State of String Theory". Although Andy Strominger gave it an A+ (as opposed to an A+++, David Gross gave it a B+ and Ed Witten declined to give it a letter grade. Cumrun Vafa was a no show on the panel due to Covid.

14. **Diogenes**
August 24, 2024

Those of us who know, know. But if you were at Harvard in 1985 you know very smart people were pretty dubious about string theory but said "Hey let Ed go with it, he's unbelievably smart." I was there when Schwartz came to Jefferson Hall and spoke about the cancellations. It was interesting. There was still a lot of scepticism even then. It is sad to see what has become of Brian Greene. Not much else has worked out either.

15. **Bernhard**
August 24, 2024

The only string theorist I can recall who actually engaged in responding at least a few of the arguments made by you and Smolin was the late Polshinski, at least in the beginning. Since then, it has always been straw-man attacks.

16. **NS**
August 26, 2024

@Carl Feinberg. Strominger gives an A++ earlier in the discussion (at 43:00). Without any irony, he adds that otherwise he wouldn't be working in it.

17. **Redeemed string theorist**
August 30, 2024

This book is notorious for being solely responsible in making a bunch of millennial string theorists who now have no choice but to do string theory if they want an academic job.

The Terrifying Power of Mathematics

August 29, 2024
Categories: Uncategorized

Ted Jacobson has put on the arXiv a transcription of a 1947 Feynman letter about his efforts to better understand the Dirac equation, in order to find a path integral formulation of it. The letter also contains some fascinating comments by Feynman about mathematics and its relation to "understanding". In particular I like this one:

> the terrifying power of math. to make us say things which we don't understand but are true.

In some other places in the text he elaborates:

> The power of mathematics is terrifying – and too many physicists finding they have the correct equations without understanding them have been so terrified they give up trying to understand them. I want to go back & try to understand them. What do I mean by understanding? Nothing deep or accurate — just to be able to see some of the qualitative consequences of the equations by some method other than solving them in detail.

The Dirac equation is something wondrous and mystifying. If one tries, like Feynman, to find a simple understanding of it in conventional geometric terms, one is doomed to failure. It is expressing something about not the conventional geometry of vectors, but the deeper and much more poorly understood geometry of spinors.

In terms of Feynman's goal of finding a path integral formulation, the best answer to this problem I know of is the supersymmetric path integral. For one place to read about this, see David Tong's notes, in particular section 3.3.1. In this paper, Atiyah gives a closely related interpretation of the Dirac operator in terms of an integral over the loop space of a manifold, using a formal argument in terms of differential forms on the loop space. I don't think either of these though are what Feynman was looking for.

In any case, what one really cares about is not a single-particle theory, but the quantum field theory of fields satisfying the Dirac equation. Here there's a standard apparatus of how to calculate given in every quantum field theory textbook. These standard calculations involving Dirac gamma-matrices fit well with Feynman's "physicists finding they have the correct equations without understanding them have been so terrified they give up trying to understand them".

Comments

1. **John Horgan**
   August 31, 2024

   What Feynman calls "terrifying" is what Wigner calls "unreasonable." Why does math work so well, and what does it actually tell us about the world? Good questions, with no definitive answers.

2. **aldo6**
   August 31, 2024

   Interesting, thanks for the link.

   Unrelatedly: there's a piece in the Giardian about Grothendieck. https://www.theguardian.com/science/article/2024/aug/31/alexander-grothendieck-huawei-ai-artificial-intelligence

3. **aldo7**
   September 1, 2024

   Apparently there was a 5 part podcast about Grothendieck on Radio France last month https://www.radiofrance.fr/franceculture/podcasts/serie-alexandre-grothendieck-legende-rebelle-des-mathematiques

   And there's a conference on Topos next week https://ctta.igrothendieck.org/

4. **tulpoeid**
   September 3, 2024

   I'd be really interested in a longer explanation of what the last sentence of this post means to say.

5. **Peter Woit**
   September 3, 2024

   tulpoeid,
   I'm working on understanding details of how Wick rotation works for spinors, and one striking thing is how hard it is to find a clear, coordinate-invariant geometric description of how spinor fields work, even in Minkowski space. The geometry of spinors and the Clifford algebra is subtle, but the standard treatments in a QFT book don't even try to use this, instead just write down formulas and show that they work.

6. **Anon X**
   September 5, 2024

   Peter,
   I'm not sure this is what you're looking for, but the lovely book by Gregori L.Naber " The Geometry of Minkowski Spacetime-2nd edition" has a sizeable amount of material regarding spinors on Minkowski…

7. **Peter Woit**
   September 5, 2024

   Anon X,
   That's about spinors in Minkowski space-time, there's lots of places where this is done. Also lots of places where spinors in Euclidean space-time are discussed, which is a significantly different story. What's not well-known and much more subtle is the relation between these two (e.g. how does Wick rotation work for spinors?).

Podcast on Unification

September 2, 2024
Categories: Euclidean Twistor Unification

I recently did another podcast with Curt Jaimungal, on the topic of unification, which is now available here. As part of this I prepared some slides, which are available here.

The main goal of the slides is to explain the failure of the general paradigm of unification that we have now lived with for 50 years, which involves adding a large number of extra degrees of freedom to the Standard Model. All examples of this paradigm fail due to two factors:

- The lack of any experimental evidence for these new degrees of freedom.
- Whatever you get from new symmetries carried by the extra degrees of freedom is lost by the fact that you have to introduce new ad hoc structure to explain why you don't see them.

There's also a bit about the new ideas I've been working on, but that's a separate topic. Over the summer I've been making some progress on this, still in the middle of trying to understand exactly what is going on and write it up in a readable way. I'll try and write one or more blog entries giving some more details of this in the near future.

Comments

1. **Bryan**
   September 3, 2024

   I just watched the interview, and from my layman pov I thought it was great. Definitely the clearest explanation I've seen of your new ideas! My question is: in the presentation you talk about some of the questions unified theories have tried and failed to answer, so how does this approach of explaining wick rotation with imaginary time in Euclidean geometry help to answer these outstanding questions? Is it mainly about the ability to do certain lattice calculations that you alluded to? Or is it that formalizing the wick rotation like this might lead to other unknown future insights and predictions?

2. **Peter Woit**
   September 3, 2024

   Bryan,
   I'm afraid I didn't really explain this here, to explain the ideas carefully is another whole talk. Working out details still in progress, but the basic claim is that when you Wick rotate spinors from Euclidean spacetime to Minkowski, one of the two SU(2)s that makes up rotations in 4d Euclidean space-time becomes the internal SU(2) symmetry of the standard model (NOT the usually expected Minkowski space-time transformations). This gives an explanation of the SU(2) in the SM, and a new avenue for unifying things.

3. **Bryan**
   September 3, 2024

   Peter, thanks for the explanation. During the lecture I didn't know what you meant by "internal symmetries" but now it makes sense. Thanks!

4. **Gavin**
   September 4, 2024

   This is a genuine question, not meant as a challenge.

   If you identify one of the SU(2)'s going from Euclidean to Lorentzian signature with an internal symmetry, where does fermions of the other handedness come from? In other words, your Wick rotation will generate (say) right-handed fermions. If I understand correctly (and maybe I don't), instead of generating fermions that transform in a left-handed spinor representation, the Wick rotation will generate fermions that transform in the SU(2) weak gauge group. But we observe states which transform with a left-handed spinor representation (for example, the electron has a mass so it has both left and right handed spinor components). Where do those left-handed states come from in this picture?

5. **Peter Woit**
   September 4, 2024

   Gavin,
   Still working on writing up the details of this that would completely answer your question, but here are some things to keep in mind (note, here "right" and "left" are being used to label chirality, not discussing helicity).

   1. The way chirality (left vs. right) works is quite different in Euclidean and Minkowski signature. In Euclidean spacetime, there are independent SU(2) groups for each chirality. In Minkowski spacetime there is just one SL(2,C) group.

   2. The way chirality works in Minkowski spacetime is that the standard spinor representation of the SL(2,C) group is one chirality, the complex conjugate representation the other chirality. If a particle is right chirality, its antiparticle is left chirality. It's well-known that when you write the SM in terms of fundamental Weyl fields, you can take the fundamental fields to be all one chirality (say, right, to go with my "spacetime is right-handed" slogan). The left chirality particles are anti-particles of these fundamental fields.

   There's a lot more to say about this, again, exactly how the reconstruction of the Minkowski spacetime spinor fields from the Euclidean spacetime spinor fields works, and what happens to chirality as you do this, is something I'm still sorting out.

6. **Nigel B**
   September 4, 2024

   Presumably, Peter, your approach circumvents the theorems of Lochlainn O'Raifeartaigh and Coleman-Mandula relating to the impossibility of combining internal symmetries with relativistic symmetries.

7. **Peter Woit**
   September 4, 2024

   Nigel B.,

Those theorems say that in Minkowski spacetime you can only extend spacetime symmetries by symmetries that commute with spacetime symmetries ("internal"). If you extend by symmetries that don't commute with spacetime symmetries, you will get a trivial theory.

What I'm doing doesn't violate this, since the Euclidean left SU(2), after Wick rotation, commutes with Minkowski space-time symmetries.

This should be clear from the point of view of my last paper, where, from the complex point of view, encompassing both Euclidean and Minkowski, space-time symmetry is purely right-handed, commuting with the left-handed factor.

8. **Gavin**
   September 4, 2024

   Thanks! Looking forward to seeing the details!

9. **Mitchell Porter**
   September 4, 2024

   Regarding evidence in favor of conventional unification, one could mention the near-unification of the gauge couplings when extrapolated to high energies, and the fact that the generations form SU(5) or SO(10) multiplets. (I sometimes wonder to what extent anomaly cancellation alone, favors ensembles of fields that look like GUT multiplets.)

10. **Peter Woit**
    September 5, 2024

    Mitchell Porter,
    That a generation fits nicely into the spin representation of SO(10) may very well be some kind of hint about unification. But the SO(10) symmetry is very badly broken, and this embodies exactly the problem I'm pointing to: whatever positive you get from the new symmetry is overwhelmed by the huge negative of having to introduce all sorts of ad hoc new structures and parameters, just to break the new symmetry down to the SM symmetry, carefully hiding all observable effects of the new symmetry.

    On coupling constant unification, same problem. There you have the additional problem of needing a SUSY theory, so you get all the extra negatives of having to do SUSY breaking.

    Georgi and Glashow were right to give up on this a long time ago, even though it was their baby. Since that time, things have just gotten worse and worse for this idea every year, as limits on SUSY masses and the proton lifetime get pushed higher and higher.

11. **arno**
    September 6, 2024

    Dear Peter,

    I am a "chiral" theoretical physicist (studying quark matter) and I do not understand physically your statement: "The left chirality particles are anti-particles of these fundamental fields".

    For massive Weyl fermions, the mass mixes chirality and it is not a good quantum number. Very simply put, a left handed fermion for an observer is right handed for another inertial observer if he has the correct speed and this is possible because the fermion does not go at the speed of light. Hence the chirality is not a good quantum number because it is not observer independant. I do not see how your statement about the left chirality of particles being anti-particles is compatible with this physical explanation.

    But I am not a mathematician so may be the misunderstanding is purely a matter of physical interpretation of mathematical statement.

    Best,

    Arno

12. **Peter Woit**
    September 6, 2024

    Arno,
    I think there are various problems of terminology here. For me Weyl fermions are massless, so I'm not talking about mass terms, or SM Yukawas. For massive particles helicity is not chirality, have to distinguish the two.

    I don't want to get into this anyway, because what I'm talking about is just exactly the same story that is in every textbook if you just look at it in Minkowski space-time. For what I'm interested in, you have to think about Euclidean space-time spinors. Part of the story is that the way chirality works is quite different in the two cases. In Euclidean space time right and left chirality Weyl spinors are completely independent, while in Minkowski spacetime they are conjugates of each other.

13. **Bertie**
    September 6, 2024

    One question for Peter, have you used any mathematical construction so far that wasn't already published before 1973? In the optimistic scenario that you are on to something remarkable, how sad it would be if it could have all been done 5 decades earlier, just imagine the progress that might have been possible in the intervening period, compared to what we got.

14. **Peter Woit**
    September 7, 2024

    Bertie,

    Most of what I'm looking at involves results in math and physics not necessarily pre-1973, but from the 70s and early 80s, before the field reoriented around string theory in 1984-5. In particular, the main work on twistors in Euclidean signature and the relation of this self-dual Yang-Mills began in 1977-78.

    One thing I've been spending a lot of time looking at is the standard reference about wick rotation of spinor fields, an Osterwalder-Schrader paper from 1973.

15. **issa**
    September 7, 2024

peter:

i greatly enjoyed both the recap and the new ideas. i'm not well versed in these areas but i'm trying! 🙂

to ask a vaguely curt-like question and to tie the second half of your talk back to the first a little bit, would it be fair to summarize your approach (as manifested, not necessarily as conceived) as something akin to: "why are we off inventing new things (structures, dofs) which don't describe the universe when we haven't actually nailed down what we think we are sure on? and look, when i really try to nail down some of these existing technicalities everyone is hand waving (in this case spinors under wick rotation), things start to happen that seem like exactly what we were missing?"

16. **Peter Woit**
September 7, 2024

issa,
Yes, that's a pretty good summary.

17. **Mathieu**
September 7, 2024

Dear Peter,
On the unification topic, is there something like a yearly progress review that would summarize the successes and failures of the most studied currents, and present the promising new ideas ? (Sorry for the somehow naïve question)

18. **Peter Woit**
September 8, 2024

Mathieu,
Unfortunately a yearly progress review of the kind you are asking for would just say that their have been no successes, and no one is interested in discussing the failures. As for promising new ideas, lots of people with often dubious claims of them, but none that have gotten any wide acceptance.

19. **lun**
September 9, 2024

So any idea how SU(3) (QCD) and U(1) (the "rest" of the electroweak theory) emerge? Why is SU(2) "special"?

20. **Peter Woit**
September 10, 2024

lun,
Some related ideas about SU(3) and U(1) are in the "Twistor Unification" paper I wrote and talks about that. It is is the SU(2) and its chiral nature that has an explanation just from trying to understand Wick rotation for spinors, without getting into the twistor story.

21. **David Roberts**
September 10, 2024

I can't say anything about physics, but su(2) is pretty special from a mathematical point of view, since it is small, but mighty, and turns up everywhere. ^_^

(PS Peter— I don't know what happened with commenters' names all getting a + instead of spaces, but I finally remembered to edit to take it out)

Podcast About String Theory, Other Items

September 8, 2024
Categories: Uncategorized

A few weeks ago I recorded a podcast with Robinson Erhardt, which has now appeared as String Theory and the Crisis in Physics. We mainly talk about the current situation of string theory in physics and the history of how things have gotten to this point, topics familiar to readers of this blog.

Some other items:

- A Frequently Asked Question from students is for a good place to learn about the geometry used in gauge theory, i.e. the theory of connections and curvature for principal and vector bundles. Applied to the case of the frame bundle, this also gives a way of understanding the geometry of general relativity. One reference I'm aware of is *Gauge Fields, Knots and Gravity*, by John Baez and Javier P. Muniain, but I'd love to hear other suggestions. These could be more mathematical, but in a form physicists have a fighting chance at reading, or more from the physics point of view.
- For new material from mathematicians lecturing about quantum field theory, see Pavel Etingof's course notes, and Graeme Segal's four lectures at the ICMS (available in this youtube playlist).
- If you want to understand the mindset of the young string theory true believer these days, stringking42069 is back.
- There's something called "Plectics Laboratories" which has been hosting mainly historical talks from leading physicists and mathematicians. For past talks, see their youtube channel. For a series upcoming September 23-27, see here.
- The IAS is hosting an ongoing Workshop on Quantum Information and Physics. One topic is prospects for future wormhole publicity stunts based on quantum computer calculations, see here. At the end of the talk, Maldacena raises the publicity stunt question (he calls it a "philosophical question") of whether you can get away with claiming that you have created a black hole when you do a quantum computer simulation of one of the models he discussed.
- Ananyo Bhattacharya at Nautilus has an article on the role of physics in creating new math. While there is a lot there to point to, recent years have not seen the same kind of breakthroughs Witten and Atiyah were involved in during the 1980s and 90s. I'm hoping for some progress the other way, that new ideas from mathematics will somehow help fundamental theoretical physics out of its doldrums.

Comments

1. **Ab**
   September 8, 2024

   Nakahara!

   https://www.amazon.com/Geometry-Topology-Physics-Graduate-Student/dp/0852740956

2. **Jack**
   September 9, 2024

   Peter,

   In addition to Baez and Muniain, I have enjoyed "Differential Geometry, Gauge Theories and Gravity" by Göckeler and Schücker, as well as the classic "Geometrical Methods of Mathematical Physics" by Schutz.

   The Eguchi, Gilkey and Hanson review is also very readable.

   Kind Regards

3. **Kartik Prabhu**
   September 9, 2024

   I did a quick review in https://arxiv.org/abs/1511.00388v3

   Some more physics-oriented ones:
   Y. Choquet-Bruhat, C. Dewitt-Morette, and M. Dillard-Bleick, Analysis, Manifolds and
   Physics, Part I: Basics.

   Y. Choquet-Bruhat and C. Dewitt-Morette, Analysis, Manifolds and Physics. Part II: 92
   Applications.

   Some more mathematical ones:
   S. Kobayashi and K. Nomizu, Foundations of Differential Geometry. Volume I. Interscience Tracts in Pure and Applied Mathematics.

   S. Kobayashi and K. Nomizu, Foundations of Differential Geometry. Volume II. Interscience Tracts in Pure and Applied Mathematics.

4. **La Rosa**
   September 9, 2024

   Concerning the first point, I would suggest Hamilton's Mathematical Gauge Theory.

5. **Arnold+Neumaier**
   September 9, 2024

   Geometry of Physics by Frankel has somewhat higher mathematical standards.

6. **Phil Harmsworth**
   September 9, 2024

   The Eguchi, Gilkey and Hanson review mentioned above refers to T. Eguchi, P.B. Gilkey and A. J. Hanson, "Gravitation, Gauge Theories and Differential Geometry", Physics Reports 66 (6) (1980) pp. 213-393.

Chris Isham's book, Modern Differential Geometry for Physicists, World Scientific 1991 is very approachable.

Robert Hermann's book, Gauge Fields and Cartan-Ehresmann Connections, Math Sci Press 1975 is a good summary once you've mastered the material in the first two items.

7. **Andrzej+Daszkiewicz**
   September 9, 2024

   Two books by Gregory Naber, "Topology, Geometry and Gauge fields: Foundations" and "Topology, Geometry and Gauge fields: Interactions" keep a quite elementary approach, but go quite far and will be a good foundation for a more advanced reading.

8. **Garrett+Lisi**
   September 9, 2024

   I'm seeing lots of great suggestions above for covering the geometry of gauge theory. Over the years I've compiled some of my notes covering this and other topics into a personal wiki. People who enjoy a casual but mathematical wiki format may find it helpful. Here are three notes in it, of increasing sophistication:
   https://deferentialgeometry.org/#connection%20%5B%5BClifford%20vector%20bundle%5D%5D%20%5B%5BEhresmann%20principal%20bundle%20connection%5D%

9. **Cynicism**
   September 9, 2024

   Humble request: Could you stop giving air to Twitter?

   When you share a link to a tweet and you don't have an account, you can only see that tweet. Maybe. More often than not you just see a screen that says "Something went wrong. Retry?" It won't work if you retry.

   You might well say, "Well then go get an account." I used to have one but I deleted it when the terms of service got much more invasive. I have felt better about that decision since the list of owners came out.

   Moreover, Twitter is already inaccessible from Brazil. Surely there will be other countries. There is a great tradition of physics in Brazil: Bohm, Lattes, et al. Do you really want to cut them out from what you want to say about physics?

10. **Peter Woit**
    September 9, 2024

    Cynicism,
    Twitter is pretty much just a sewer of idiocy. Almost always when I link to something there it's to point to some kind of exceptional idiocy from string theorists. If you're a big fan of this kind of thing, maybe it would justify getting a twitter account, maybe not.

    Another reason for me to look at Twitter is that there are a few accounts that sometimes provide links to something interesting I didn't know about. In those cases what I put on the blog is not the Twitter source but the link.

11. **zzz**
    September 9, 2024

    i second that twitter request. i got an error or login request or some bs.

12. **Peter Woit**
    September 9, 2024

    zzz,
    Sorry, but if you want to experience the stringking phenomenon, you have to take the plunge into the Twitter sewer. I'm pointing to it rather than ignoring it because it gives some weird sort of insight into what is going on among some string theorists as they face the collapse of their subject.

    The whole thing reminds me a bit of a Hunter Thompson saying: "when the going gets weird, the weird turn pro."

13. **John Baez**
    September 9, 2024

    There is no such thing as Twitter.

14. **Peter Woit**
    September 9, 2024

    John Baez,
    Just because billionaires change names of things doesn't mean I have to use their new name. I'm boycotting using not only "X", but "Alphabet", "Meta" and "SLMath".

15. **Simone Speziale**
    September 9, 2024

    Dear Peter,

    indeed it would be nice if you would consider strengthening your boycott of using the name X to a complete boycott of linking to Twitter/X altogether… even if a sewage, I would think that linking to it still gives it relevance and encourages people to use it, which ultimately legitimizes Musk's take on it. And too bad if this makes us miss interesting sociological phenomena like stringking.

    Back to the question about references: on top of the many excellent ones already given, and for the specifics of the frame bundle approach to GR: the first few chapters of Chandra's mathematical theory of black holes are very nice and pedagogical.

16. **Johan**
    September 10, 2024

    Quite mathematical, but always keeping physics in sight, is

Shlomo Sternberg, Curvature in Mathematics and Physics, Dover, 2012.

17. **Karl Young**
September 10, 2024

Re. the podcast (and the one with Jaimungal) I have to say that it was nice getting a somewhat nuanced view of your take on the current state of physics (much of which rings true for me). As on old retired geezer who happily surfed the nonlinear dynamics fad and was able to parlay that into a happy, bottom feeding career in applied mathematical physics I have no skin in the game, but nonetheless found how pervasive the internal political influence of the string theorists, a little surprising. When I was in grad school in the late eighties, taking GR and QFT classes things seemed a little more wide open (though part of that may have been the result of a pretty strong crossover between cosmologists and high energy theorists in my department). Anyway that's all a rather long winded way of asking if there's a lot in Not Even Wrong that wasn't covered in these podcasts. I'm a pretty slow reader so at this age have to pick wisely, and though I'm sure I'd enjoy Not Even Wrong, if there's not much more than you expressed in the podcasts, it sounds like maybe I should go get up to speed on spinors instead (and any recommendations there are welcome).

18. **Peter Woit**
September 10, 2024

Karl Young,

As far as the string theory controversy goes, the two recent podcasts give a pretty good idea of my side of that, so if that's all you care about, you can live without reading the book. There is however, much material on other topics there, in particular a lot about the relation of math and physics, nothing to do with string theory.

Not much in the book about spinors, I'd agree that learning about those is a more fruitful pursuit than thinking about string theory. Hard to find good readable references on the topic. I've written some blog entries, should find time to write more and should create a category here so people could find them.

19. **lll**
September 11, 2024

Below I will put my two cents (literally!) on good references for learning the basics of (geometric) Gauge Theory:

i) before embarking on the project resulting in his monumental "Foundations of differential geometry" with Kobayashi, Nomizu wrote a booklet entitled "Lie groups and differential geometry", which I still find the best (mathematically oriented) introduction to principal bundles, connections, holonomy, etc;

ii) to those who have mastered Nomizu's little gem, I recommend reading Bleecker's "Gauge theory and variational principles", a slightly thicker and (essentially) self-contained account of the fundamentals of gauge theory, with emphasis on its variational aspects and including elegant discussions on spinors and on the unification of gauge fields and gravitation usually associated to Kaluza and Klein. Despite being too mathematically oriented on the whole, this jewel is filled here and there with insightful comments on the emergency of gauge invariance a more physically inclined reader may find fruitful.

20. **Scott Caveny**
September 11, 2024

Physics students wanting to understand more about the geometry of gauge theories might find the following texts useful:

1. 2004 A Course in Modern Mathematical Physics: Groups, Hilbert Space and Differential Geometry by Peter Szekeres

2. 2013 Mathematical Physics: A Modern Introduction to Its Foundations by Sadri Hassani

3. 2017 Differential Geometry and Mathematical Physics: Part I. Manifolds, Lie Groups and Hamiltonian Systems by Gerd Rudolph and Matthias Schmidt

4. 2017 Differential Geometry and Mathematical Physics: Part 2. Fibre Bundles, Topology and Gauge Fields by Gerd Rudolph and Matthias Schmidt

Physics students prepared to read mathematics literature (at the level of Choquet-Bruhat & Dewitt-Morett) may find useful content in

1. 1991 Modern Geometry: Part I: The Geometry of Surfaces, Transformation Groups, and Fields by B. A. Dubrovin, A. T. Foment, and S. P. Novikov

2. 1985 Modern Geometry: Part II: The Geometry and Topology of Manifolds by B. A. Dubrovin, A. T. Foment, and S. P. Novikov

3. 2017 Riemannian Geometry and Geometric Analysis Seventh Edition Jurgen Jost

21. **Aaron Cofer**
September 12, 2024

Thanks for posting the Etingof notes. It looks like a very interesting take on the subject (algebraic combinatorics view?), but that could be a reflection of my ignorance.

Do you know of any similar books/notes that take a more elementary approach along these lines?

22. **Anon X**
September 14, 2024

I think "Differential Geometry: Connections, Curvature, and Characteristic Classes" by Loring W. Tu is quite accessible. It's the sequel to his much liked "An Introduction to Manifolds".

23. **Hansi**
September 14, 2024

That one here,

Helga Baum, Eichfeldtheorie (Gauge field theory)
is in german but mathematically sound and rather simple:

https://www.amazon.de/Eichfeldtheorie-Differentialgeometrie-Faserb%C3%BCndeln-Springer-Lehrbuch-Masterclass/dp/3642385389

some of it is here on this pdf

https://www.mathematik.hu-berlin.de/~baum/Skript/KompaktkursHFB-05.pdf

For those who want a simple book on gauge theory that is easy to read, from a typical german math department which is not as high profile like Bonn….

This Week’s Hype

September 16, 2024
Categories: This Week's Hype

Today’s Washington Post has an opinion piece from Brian Greene, running under the demonstrably false title Decades later, string theory continues its march toward Einstein’s dream.

In the piece, the argument of string theory critics is given as:

> Critics argue that the situation is untenable, noting, “If you can’t test a theory, it’s not scientific.” Adherents counter, “String theory is a work in progress; it’s simply too early to pass judgment.” The critics retort, “Forty years is too early?” To which the adherents respond, “We’re developing what could be the most profound physical theory of all time — you can’t seriously cross your arms, tap your foot and suggest that time’s up.”

The problem with the results of forty years of research into string theory is not that progress has been too slow but that it has been dramatically negative. To see this, one can just compare the text of chapter 9 of Greene’s 1999 *The Elegant Universe*, which has an extensive discussion of prospects for testing string theory by finding superpartners, fractionally charged particles, or cosmic strings. Twenty-five years later, the results of experimental searches are in: no cosmic strings, no fractionally charged particles, and most definitively no evidence of superpartners of any kind from the LHC.

The other sorts of predictions advertised in that chapter are based on the idea that string theorists would better understand the theory and be able to make testable predictions about neutrino masses, proton decay, axions or new long range forces, the nature of dark matter, and the value of the cosmological constant. Instead of progress towards any of these, things have gone in the opposite direction: all evidence from better understanding of string theory is that it either naturally predicts things in violent disagreement with experiment (wrong dimension of space time, huge number of new long-range forces, …) or predicts nothing at all. 25 years later, Greene now goes with the latter:

> The challenge for string theory is that it has yet to produce *any* definitive, testable predictions.

The article goes on to make a different case for string theory:

> … string theory continues to captivate seasoned researchers and aspiring students alike because of the remarkable progress that has been made in developing its mathematical framework. This progress has yielded provocative insights into long-standing mysteries and introduced radically new ways of describing physical reality.
>
> For instance, string theory has provided unmatched insights into the surface of black holes, unraveling puzzles that have consumed some of the greatest minds, including Stephen Hawking. It has offered a novel, though controversial, explanation for the observed speedup of the universe’s expansion, proposing that our universe might be just one of many within a larger reality than conventional science ever imagined.

The problem here is that these supposed advances aren’t from advances in string theory. If you follow the link above that justifies “string theory has provided unmatched insights into the surface of black holes”, you’ll find the text:

> Most physicists have long assumed it would; that was the upshot of string theory, their leading candidate for a unified theory of nature. But the new calculations, though inspired by string theory, stand on their own, with nary a string in sight. Information gets out through the workings of gravity itself — just ordinary gravity with a single layer of quantum effects.

The string theory “explanation” for the value of the CC is just the “anthropic” explanation, which besides not really being a scientific explanation, has nothing to do with string theory.

The piece ends with something highly speculative and ill-defined (ER=EPR) that has nothing to do with string theory:

> Roughly, it’s as if particles are tiny black holes, and the entanglement between two of them is nothing but a connecting wormhole.
>
> If this realization holds up, we will need to shift our thinking about the unification of physics. We have long sought to bring general relativity and quantum mechanics together through a shotgun wedding, fusing the mathematics of the large and the small to yield a formalism that embraces both. But the duality between Einstein’s two 1935 papers would suggest that quantum mechanics and general relativity are already deeply connected — no need for them to marry — so our challenge will be to fully grasp their intrinsic relationship.
>
> Which would mean that Einstein, without realizing it, may have had the key to unification nearly a century ago.

Where string theory research is after 40 years is not on a continuing march forward towards “Einstein’s dream”, but in a state of intellectual collapse with no prospects of any connection to the real world, just more hype about vague hopes for something different, something for which there is no actual theory.

Update: I recommend checking out Sabine Hossenfelder’s latest youtube piece, which is mostly devoted to the Brian Greene wormhole publicity event stunt discussed earlier here. Near the end of the video, she tells a story that explains a lot about why this kind of thing keeps going on (see here). She had been writing a regular column for Quanta Magazine, but they stopped publishing her after she wrote a column in which she argued that physicists should not be misleading the public by claiming that the “black holes” supposedly on the other side of a duality from a given quantum system were actual physical black holes.

What she was warning about in 2019 is an essential part of the wormhole publicity stunt, and of other similar continuing efforts that have been going on for years. One impetus behind this nonsense has always been clear: there’s attention to be gotten, and money to be made. Another factor though is the one Hossenfelder explains here. Press outlets devoted to science want to publish positive news about scientific advances, want nothing to do with authors who explain that this positive news is nothing but hype.

Comments

1. **Paddy**
   September 16, 2024

   Read this WaPo “column” about 5am local time today. Been waiting for your response. Thank you. Was upset that they allowed such without a reasoned rebuttal.

2. **Bob Zannelli**
   September 16, 2024

   EPR=ER seems to be the latest rage but as Bob Wald has aptly said all these ideas to save quantum unitary in black hole evolution have a price tag of modifying quantum theory to the point of nonrecognition. (not his exact words) EPR=ER seem to me to be one of ugliest and ad hoc solutions to a problem it's not really clear there needs to be a solution for. Much more sensible in my opinion are the viewpoints of Wald, Unruh and Thorne and the late James Hartle who argue(d) that the black hole information problem isn't a real problem at all. What is the real problem are all the ad hoc theories trying to solve this problem.

3. **martibal**
September 16, 2024

What is the duality in Einstein two papers of 1935 that he is talking about ?

4. **Peter Woit**
September 16, 2024

martibal,
Einstein Rosen paper on wormholes versus EPR paradox paper.

5. **Diogenes**
September 16, 2024

The problem with string theory is not just that it doesn't predict anything per se (which it doesn't) but that in addition it doesn't make any existing theories more parsimonious or useful. For all kinds of technical reasons the unification part has failed. You are left with the quantum gravity part, which probably has worked better than any other approach, but still not that well at all. If moving away from point particles will always cause this much trouble it would be interesting to reflect upon why that is ..

6. **Peter**
September 16, 2024

I find the "we just need more time" argument particularly hard to understand.

There were roughly 50 years between the discovery of QM by Heisenberg and Schrödinger, and 't Hooft and Veltman. In those 50 years QM led to many interesting things: the Dirac equation, anti-particles, the neutrino, QED, the development of gauge theories, the description of the Higgs mechanism, the standard model … not to mention the Mössbauer effect, Raman spectroscopy, lasers, developments in solid state physics and so on. The standard model is a success, but it is far from the only success of QM.

It's now 40 years since the first superstring revolution. Physics' finest worked on it, uninterrupted by world wars. And as far as I understand, absolutely nothing comparable happened in string theory. In those circumstances, you have to have seriously strong guts to claim that you just need more time and everything will be fine.

7. **David**
September 16, 2024

I am a retired biophysics professor. This behavior just astounds me. I grew up in an environment where you went overboard to be careful that what you said was either true or you had to carefully qualify it. If I said something in a lecture or paper that turned out to not be quite right, I was mortified. If, for example, I had made the easily checked misstatement about the black hole link that you discuss, I don't think I could ever live it down. What is happening? Has there been a dramatic change in the academic scientific ethos, or is there something about this field that attracts this kind of behavior.

8. **Scott Aaronson**
September 16, 2024

I *think* I've been following this for long enough that I could simulate the next couple steps of the argument. Greene could say, for example, that the anthropic "explanation" for the CC isn't completely unrelated to string theory, since string theory gives you a context within which the dimensionless constants of the SM could vary in eternal inflation. He could also say that ER=EPR and the recent progress on the black hole information problem came from the string community, and plausibly wouldn't have emerged without that path (even if, at the end, the strings can be removed like scaffolds). You, in turn, could reply that it's nonsense to talk about a "string theory mechanism" for varying constants without a nonperturbative definition of the theory, and also, string theorists said 30 years ago that their goal involved unification of the forces and predictions confirmed by experiment, so why do they get to change the goal midgame to "quantum gravity insights from which the strings can be removed like scaffolding"?

9. **Peter+Shor**
September 16, 2024

I'm not convinced that the recent "progress" on the black hole information paradox holds water. Does anybody other than the authors really understand it?

If you know the results that you want to come up with, and you do enough non-rigorous manipulations of vaguely related quantities, you can almost always come up with the desired results. How sure are we that this isn't where these results came from?

10. **Philippe David**
September 16, 2024

Do you think the Washington Post would accept a countering opinion piece?

11. **Peter Woit**
September 16, 2024

Bob Zanelli,
My problem with ER=EPR is not that it's ugly and ad hoc, but that I don't see any actual calculation of anything that corresponds to what the Washington Post is advertising as the discovery that "particles are tiny black holes, and the entanglement between two of them is nothing but a connecting wormhole."
As far as I can tell, the problem is not that the theorists doing this have a theory that is ad hoc, but that they have no theory that is supposed to correspond to the real world.

12. **Peter Woit**
September 16, 2024

Peter,
I agree, but want to again emphasize that there has been progress in our understanding, but it's been in the opposite direction of that indicated by the Washington Post headline, which much more accurately should have been

"Decades later, string theory continues its march away from Einstein's dream."

13. **Peter Woit**
September 16, 2024

David,

I'm also astounded by what is going on here. 25 years ago I understood what Greene and others were doing as understandable enthusiasm for some specific ideas about physics (even though I had my doubts about them). Considering everything that has happened since then, I just don't understand at all why anyone involved in this would think it's a good idea to now go to the public claiming all is well and doubling down with even much more outrageous hype than that of 25 years ago.

14. **Peter Woit**
September 16, 2024

Scott Aaronson,

Yes, you qualify as a pro at this. That's exactly right.

15. **Peter Woit**
September 16, 2024

Philippe David,
I have no idea if the Washington Post would be interested in publishing such a piece. I do know that I feel I've done more than my part trying to deal with this kind of hype, so I'm not interested in being the one to work on arranging for and writing something. Among the many others who know enough about the subject to know what is seriously wrong here, I'd hope there are some who care enough about this to try and do something about it.

16. **Jean-Pierre Laflamme**
September 16, 2024

It seems increasingly clear that the very foundation of the theory, that fundamental particles are actually made of strings, is wrong or biased from the start. And this would be the reason why this theory is full of oddities or leads nowhere despite all the efforts made.
When you have taken a wrong turn from the start, you must have the humility to retrace your steps and start again in a more promising direction.

17. **wormholes**
September 16, 2024

When you plug simple numbers in ER=EPR, you see that it cannot work https://arxiv.org/pdf/2002.08178 . A wormhole connecting two entangled electrons would have a mass of 10^27 eV (in order to explain the entanglement entropy). Already grossly excluded with simple experiments.

18. **Alex**
September 16, 2024

Scott says,

"…why do they get to change the goal midgame to "quantum gravity insights from which the strings can be removed like scaffolding"?"

"Peter's" above hypothetical critique would already indeed be a valid one. But now let me add mine: why are we even granting that these are actual "quantum gravity insights" in the first place? When the hypotheses are so wildly speculative and poorly justified, the actual concrete models so far from anything resembling reality, the derivations so full of hand-waving, then, I think, we are more than allowed to be skeptic of even that basic point.

There may be something of interest from the quantum computing and quantum complexity point of view (I don't know, you tell me), but from the quantum gravity one (the hard problems are well known, are decades old), I see very little substance, as per usual.

We need to start to put the bar much more higher to what we call "quantum gravity" these days, otherwise almost anything is now passing for it. Of course, since there's no accepted theory or even accepted direction, that's difficult. My two cents are: let's stick to the basic and well known issues that appear when you try to apply the current QFT formalism to GR and build from that. What I see now is that people invent their grandiose theories or approaches, with very little justification for their hypotheses and then try to apply those the the mentioned issues; they then proceed to sell this as insights into quantum gravity. Problem with this is that there as many ways of doing that as grandiose theories or approaches the human mind can conceive (let's not throw LLMs into that mix, please!) Even the black hole information issue (one of the main motivations behind many of these proposals) needs a lot more understanding at the QFT in (classical) curved spacetimes regime before clearing things enough to see if we can even get "QG insights" about it. Bob Wald's (and also Unruh's) objections to its status as a supposed paradox that is unresolvable in standard QFT in curved spacetimes deserve more attention, in my opinion. I'm not saying he's right, but his takes on this are much more technically naunced and well informed than Susskind's ones, which have been much more influential in some communities in theoretical physics (particularly, the commnunity that gave rise to the subject being discussesed in this blog entry)…

19. **Shantanu**
September 16, 2024

Peter or someone else, how is fractionally charged particles evidence for string theory?

20. **yaron kedem**
September 17, 2024

A side note: It feels like this "40 years" time span has been with us for quite a while. The first revolution was 40 years ago but people have been talking about string theories for at least 10 years by then.

21. **anon**
September 17, 2024

I find the objections to this article slightly unreasonable. The argument of Greene's article is 1) string theory has made no experimental predictions 2) it has made deep and serious mathematical advances 3) one such, still conjectural and not understood even mathematically, are the concepts of duality 4) here is an example of such, which if physically applicable will have physical implications.

As a pure mathematician I can get behind all of those statements, and their consequence — string theory is not nonsense. It has failed, so far, at explaining anything about the physical world. But the many serious physical intuitions that have informed it — intuitions that are (were?) unavailable to pure mathematicians — have produced surprising and deep progress, with consequences in algebraic geometry and representation theory.

It is likely that any description of the physical world that involves mathematics will use these consequences – algebraic geometry (equations) and representation theory (symmetry) are very basic. That is not to say that string theory will be the foundation of any physical understanding, a statement about which I am at best agnostic.

You may find argument (4) overhyped by Greene, but I hope you do not differ in the rest of this assessment.

22. **Peter Woit**
September 17, 2024

anon,
You're making a different argument than the one in article, which is that string theory is making progress towards "Einstein's Dream" (which was not to do math). My post here is responding to the argument actually made.

I am ignoring the claims in the article about mathematics (which are overhyped) since the relation of string theory and mathematics is a complicated and separate subject.

As far as I can tell, there is no significant new mathematics involved in "ER=EPR", whatever that is.

Looking at the situation locally at Columbia, there was a period during the 1990s when there was significant joint activity between math and physics here, much of it organized by Brian. That stopped a long time ago, as his interests moved away from mathematics to purely physics issues.

23. **David Brown**
September 17, 2024

Einstein, Albert, and Nathan Rosen. "The particle problem in the general theory of relativity." Physical Review 48, no. 1 (1935): 73.
https://www.jp-petit.org/papers/cosmo/1935-Einstein-Rosen.pdf
Einstein, Albert, Boris Podolsky, and Nathan Rosen. "Can quantum-mechanical description of physical reality be considered complete?." Physical review 47, no. 10 (1935): 777.
https://utf.mff.cuni.cz/virtualni_vyuka/NPOZ008/FJDP_2023/EPR-PhysRev47(1935)777_preklad.pdf

24. **Peter Woit**
September 17, 2024

Shantanu,
The idea that string theory models naturally give fractionally charged states goes back to this Wen-Witten paper from the very early days (May 1985) of the string theory unification via Calabi-Yau story, see
https://doi.org/10.1016/0370-2693(92)91557-P

It was prominently featured in The Elegant Universe, but I've never seen anyone else in recent decades promoting this as a "prediction" or "test" of string theory.

25. **anon**
September 17, 2024

The WP has a "Today's Opinions" summary of its op-eds, written by Drew Goins, which described Greene's editorial as follows:

Frontiers in physics
Math and physics professor Brian Greene's op-ed isn't rocket science. At least you can test rocket science.

No, Greene is writing about string theory, the most fantastical and unprovable frontiers of science. As he explains for the uninitiated masses, string theory entirely holds up on its internal mathematics, but to actually test whether the outcomes it predicts are true, experimenters would need to re-create the conditions at the heart of black holes or at the moment of the Big Bang.

Does string theory being untestable mean that string theory is unusable? Absolutely not, Greene writes. Of course, if some flaw were to appear within the theory, or some experiment actually came along to debunk it, Greene would abandon it posthaste: "I'm an advocate for truth, not string theory."

But, until that moment, string theory is still unlocking fabulous maybe-truths about the universe. Let Greene's words wash over you: wormholes and black holes, "holographic duality," quantum-entangled particles that behave the same no matter the distance between them. Even if you don't fully understand them, they might still inspire awe … and buy-in.

26. **Peter Woit**
September 17, 2024

anon,
That this piece is not science but advertising trying to get "buy-in" is right. A good question for the Washington Post is why they are publishing advertising masquerading as science.

27. **Adam+Treat**
September 18, 2024

Scott,

"He could also say that ER=EPR and the recent progress on the black hole information problem came from the string community, and plausibly wouldn't have emerged without that path (even if, at the end, the strings can be removed like scaffolds)."

Imagine in 40 years time, Gil Kalai is proven right and QC is still not feasible. This would be like some other member of the QC community rebutting Gil Kalai by saying, "Well, Scott Aaronson's work on fingerprinting AI outputs is instrumental in AI safety and that came from the QC community and plausibly wouldn't have emerged without that path (even if, at the end, quantum computing could be removed like scaffolds." In other words, I give you the non-sequitur.

PS: I'm old enough to remember when ER=EPR was introduced as fanciful big conjecture / brainstorming and even Susskind seemed sheepish in putting it forward given all the handwaving. Now it is treated like a big breakthrough??

28. **Jonathan**
September 18, 2024

@Bob Zanelli, @Alex:

You might be interested in the interview Wald gave in 2020 to the AIP history project where he talks about these issues a bit:
https://www.aip.org/history-programs/niels-bohr-library/oral-histories/47228

29. **zzz**
September 18, 2024

“He could also say that Newtons gravitation came from the epicycle community, and plausibly wouldn’t have emerged without that path (even if, at the end, epicycles can be removed like scaffolds).”

30. **Brathmore**
September 18, 2024

Always keep in mind that Editors of Newspapers and Magazines can alter an author’s submitted article. We don’t always know precisely how the sausage was made.

31. **Peter Woit**
September 18, 2024

Brathmore,
The points made in the article are substantially the same as the material in the new edition of The Elegant Universe that was just published, see
https://www.math.columbia.edu/~woit/wordpress/?p=14092

A logical guess as to why this piece is being published now in the Washington Post is that it’s a byproduct of the publisher’s effort to promote the new book.

32. **Bob**
September 18, 2024

An analogy to the wormhole story I have been thinking lately: a lot of integrable systems can be described in terms of vector bundles on twistor space. If I create something in the lab in a setup where it becomes well-approximated by one of these integrable systems, did I just create twistor space in the lab? That would be a good headline: “Physicists create twistor space, a six (real) dimensional space, in the lab.”

33. **Supermembrane**
September 18, 2024

One could plausibly say that knot theory came from Kelvin’s vortex model of the atom, which was an attempt at a theory of everything from the 19th-century perspective, unifying electromagnetism, atomic physics, and possibly gravity, in terms of the dynamics of the ether. Ultimately, it failed as a theory of physics, made obsolete by relativity and quantum mechanics. Only knot theory remained after. Maybe string theory will meet a similar fate, in the end.

34. **Robert James Parkinson**
September 18, 2024

I could be wrong, but I get the impression that Mr. Greene is trying to protect physics from public backlash. I don’t really know, but it seems like the Physicists are trying to pivot away from string theory without saying so. Break the news to the public that they’ve been BSing you slowly and gently.

Maybe that is for the best. But going forward scientists should be much more cautious in their “science communication” and let the politicians campaign for funding.

35. **Peter Woit**
September 18, 2024

Bob,
I don’t think you can create twistor space in the lab. On the other hand I do think we actually live in twistor space and experience it directly (space-time points are indirectly observed quantities, when you open an eye you’re experiencing directly a CP1 in twistor space…).

36. **Peter Woit**
September 18, 2024

Robert James Parkinson,

Protection against public backlash is a big goal here, but I don’t think it’s physics in general that he’s trying to protect.

37. **Sabine**
September 19, 2024

Hi Peter, thanks for the link. I can’t believe we’ve been going on about this for almost 20 years!

38. **Scott+Aaronson**
September 22, 2024

Adam Treat: People, including me, HAVE made the argument that quantum computing research can partly be justified by the new things it’s led to in classical theoretical computer science. Watermarking of AI outputs is a terrible example, since that has nothing to do with quantum computing except for the involvement of a couple of the same people (e.g. me), but, say, lower bounds for triangle counting and locally decodable codes, lattice-based cryptosystems, new characterizations of counting classes, advances in lower bounds by polynomials, speedups for classical optimization problems via Quantum Monte Carlo running on classical computers, and much more have come from QC. But I’ve also been very clear that none of this can substitute for the big enchilada, the actual building of a scalable QC and its use to get some benefit over a classical computer for some problem that people care about.

39. **Scott+Aaronson**
September 22, 2024

Alex: One could take the position that, e.g., the black-hole information paradox isn’t worth discussing at all without a detailed theory of quantum gravity in which to do calculations. But if one *does* treat it as worth discussing, then I think it’s inarguable that AdS/CFT and “It from Qubit” have now completely changed the terms of discussion. E.g. we now know that the same Euclidean path-integral hocus-pocus that led to the prediction of Hawking radiation in the first place, also leads to the prediction that the radiation encodes the infalling information (even though it doesn’t provide a microscopic description of how). More generally, there’s now a toy setting where (1) spacetime is emergent from a more fundamental description and (2) black holes are understood in the conventional framework of QM.

40. **David Roberts**

September 23, 2024

There’s a difference between praising something that acted as a catalyst to discover other things that ultimately don’t depend on the catalyst, and praising the catalyst as a success *on its own terms*, when the other discoveries ultimately aren’t contributing to the continuing unsolved problem the catalyst was supposed to solve. A constant argument people make in grant applications here in Australia is for the whole “well, it contributes indirectly to blah blah blah, unforseen consequences etc”. But try telling the ARC that your grant money was deserved because you didn’t achieve anything your research application said you would address, but someone else has later borrowed some techniques and solved some other problem. Research inspiring other research that solved other problems is a basic given. If your research is so insular it can *only* somehow address one single problem and never connect to anything else, then that is a really risky situation. So praising a program for basically having a bare minimum feature every productive scientific or mathematical theory has, feels like pretty poor change to me.

41. **Peter Woit**
September 23, 2024

David Roberts,
In addition, in many cases the supposed new progress is as much or worse hype than the original string theory hype, promoted by the same people. The most egregious example would be the wormhole publicity stunt, promoted as “quantum gravity in the lab”, inspired by ads/cft/string theory. With no consequences for the original string theory hype, there’s now sometimes a doubling down on even more outrageous hype.

42. **Alex**
September 23, 2024

Some kind of consequence is the low number of faculty positions in hep-th. This looks like the worst job market in a long time:
https://particle.physics.ucdavis.edu/rumor/doku.php?id=current

Of course this predominantly affects young people. But most departments don’t want to hire in this field anymore (and haven’t for a while).

43. **Peter Woit**
September 23, 2024

Alex,
Unfortunately I think that’s right. You can get the public to continue to buy books by doubling down on the hype, but there is then a blowback effect that you lose credibility in your wider academic field, with people being unwilling to hire in your area.

I’m quite serious when I say that I see this subject as now being in a state of intellectual collapse. One is seeing much fewer significant new ideas than at any time in the past. String theorists over the years have often said “you can see the field is healthy because it is attracting talented young people”. But if there are no jobs and nothing is happening, these talented people move on to something else.

44. **Thomas Corrigan**
September 23, 2024

I just found the WPO article (FB showed it to me a week late), and as soon as I saw who had written it, I knew what to expect. Your blog is where I went to find some sanity, and did. Along with, of course, Sabine’s video. Just want to say “thank you”, from a retired theoretical nuclear physicist (Iowa State, then JHU/APL).

45. **Martin G**
September 26, 2024

Have you tried asking Quanta for the reasons why they stopped publishing Sabine, or will you simply take her assumptions and word for it?

46. **Peter Woit**
September 26, 2024

Martin G,
I don’t think it’s hard to figure out what Quanta’s point of view is on this, and I don’t see any reason to doubt what Sabine says. If you look at what they do and what she does, they are quite different things. She’s also not the compromising sort who would change what she does to fit with what they want to do.

Their big new project about quantum gravity is a very good example of how different what they do is from what she does. I’ll try and write something about that soon.

47. **Karl Young**
September 26, 2024

Interesting that Quanta is taking sides in the string wars. I still consider what they do extremely valuable in terms of providing access to information about current research for those who aren’t trying to read all the latest journal articles. But if they get too selective it seems that they run the risk of providing a skewed picture of what the research community regards as important. And they also seem to be getting a little sloppy, e.g. a recent, otherwise well written piece on the development of group theory and it’s use in physics, contained the statement “At high energies, group theory can be used to show that electromagnetism and the forces that hold atomic nuclei together and cause radioactivity are all manifestations of a single underlying force.“ It’s a bit ironic that after your nice non-technical description of the experimental failure of GUTs re. the recent podcasts, that they’d be so sloppy. Maybe I’m nit picking but for a publication that aims to provide good information on current developments in science that statement felt problematic.

48. **Peter Woit**
September 26, 2024

Karl Young,
That Quanta quote was from someone writing about math, not very well informed about physics. You could probably find similar examples of people writing about physics, saying not very well informed things about math.

Like the Simons Foundation, they’re focused on certain elite institutions, so for instance tend to reflect the point of view of people in Princeton at the IAS, and want to make the scientific community look good. So, rather different than Sabine Hossenfelder.

I wouldn’t describe Quanta at this point as “taking sides in the string wars”. In their latest series of articles about quantum gravity, they more or less ignore string theory. This fits well with what’s going on at places like the IAS, where quantum gravity research has long ago moved away from string theory (even if it’s hard to get them to admit it…).

Is Spacetime Unraveling?

September 26, 2024
Categories: Uncategorized

Quanta magazine has just put out an impressive package of material under the title The Unraveling of Space-Time. Much of it is promoting the "Spacetime is doomed" point of view that influential theorists have been pushing for decades now. A few quick comments about the articles:

- String theory is barely even mentioned.
- There is one article giving voice to an opposing point of view, that spacetime may not be doomed, an interview with Latham Boyle.
- The big problem with the supposedly now conventional view that spacetime needs to be replaced by something more fundamental that is completely different is of course: "replaced with what?". A lot of attention is given to two general ideas. One is "holography", the other Arkani-Hamed's amplitudes program. But these are now very old ideas that show no signs of working as hoped.

  Thirty years ago Lenny Susskind was writing about The World as a Hologram. The idea wasn't new then and seems to be going nowhere now. It was 17 years ago that Arkani-Hamed started re-orienting his research around the hope that new ways to compute scattering amplitudes would show new foundations for fundamental physics that would replace spacetime. Years of research since then by hundreds of theorists pursuing this have led to lots of new techniques for computing amplitudes (twistors, the amplituhedron, the associahedron, now surfaceology), but none of this shows any signs of giving the hoped for new foundations that would replace spacetime.

Instead of saying any more about this, it seems a good idea to try and lay out a very different point of view which I think has a lot more evidence for it. This point of view starts by noting that our current best fundamental theory has been absurdly successful. There are questions it doesn't answer so we'd like to do better, but the idea that this is going to happen by throwing the whole thing out and looking for something completely different seems to me completely implausible.

One lesson of the development of our best fundamental theory is that the new ideas that went into it were much the same ideas that mathematicians had been discovering as they worked at things from an independent direction. Our currently fundamental classical notion of spacetime is based on Riemannian geometry, which mathematicians first discovered decades before physicists found out the significance for physics of this geometry. If the new idea is that the concept of a "space" needs to be replaced by something deeper, mathematicians have by now a long history of investigating more and more sophisticated ways of thinking about what a "space" is. That theorists are on the road to a better replacement for "space" would be more plausible if they were going down one of the directions mathematicians have found fruitful, but I don't see that happening at all.

To get more specific, the basic mathematical constructions that go into the Standard Model (connections, curvature, spinors, the Dirac operator, quantization) involve some of the deepest and most powerful concepts in modern mathematics. Progress should more likely come from a deeper understanding of these than from throwing them all out and starting with crude arguments about holograms, tensor networks, or some such.

To get very specific, we should be looking not at the geometry of arbitrary dimensions, but at the four dimensions that have worked so well, thinking of them in terms of the spinor geometry which is both more fundamental mathematically, and at the center of our successful theory of the world (all matter particles are described by spinors). One should take the success of the formalism of connections and curvature on principal bundles at describing fundamental forces as indicating that this is the right set of fundamental variables for describing the gravitational force. Taking spin into account, the right language for describing four-dimensional geometry is the principal bundle of spin-frames with its spin-connection and vierbein dynamical variables (one should probably think of vectors as the tensor product of more fundamental spinor variables).

What I'm suggesting here isn't a new point of view, it has motivated a lot of work in the past (e.g. Ashtekar variables). I'm hoping that some new ideas I'm looking into about the relation between the theory in Euclidean and Minkowski signature will help overcome previous roadblocks. Whether this will work as I hope is to be seen, but I think it's a much more plausible vision than that of any of the doomers.

Update: John Horgan has some commentary here, taking the point of view that discussions of "Beyond Space-time" are fine, as long as you realize what you're doing is "ironic science" not science.

Comments

1. **Dan Winslow**
   September 26, 2024

   As a side note, I have to say that was some pretty impressive presentation work on the part of whoever is doing the web ui for that article. Really very nice and kind of cutting edge as far as I am aware. You can see the evolution towards a more magazine-style "i'm turning these glossy pages" kind of experience.

2. **NoGo**
   September 26, 2024

   Dan Winslow: To the contrary, I find this style of presentation pretty much unreadable/un-navigable. Luckily, it seems the articles themselves are accessible from the normal landing page in the normal way.

3. **Georgi Ivanov**
   September 26, 2024

   One of the linked articles in the main one that I found more interesting was about the 'Long-forgotten math' of von Neumann's algebras. The mathematician who rigorously grounded the third type, namely Alain Connes, has been the primary architect of noncommutative geometry. In that setting, spinors are the elements of the Hilbert space on which the Dirac operator acts. I wonder if you have found anything useful from this spectral formalism.

4. **Peter Woit**
   September 26, 2024

   Georgi Ivanov,
   My interest generally has been in very specific examples, hoping to get a deeper understanding of the Standard Model. Using the general theory of operator algebras is kind of at the other end of things, useful to say things independent of any specific theory (which is why theorists without a theory doing quantum gravity find it useful).

   Non-commutative geometry a la Connes is a different story, pretty much off-topic.

5. **Paolo Bertozzini**
   September 27, 2024

   Dear Peter (thanks for this extremely interesting post, mentioning Boyle's interview) and Georgi Ivanov (thanks for touching on the “operator algebra point”),

The problem with the now standard view that “space-time is only emergent”, and hence not fundamental, is huge: (as I already mentioned in previous messages here) essentially all of the main current approaches to quantum gravity are in agreement on this!
It is a big pleasure to see some dissenting voices like Boyle and Peter 🙂

There is a significant difference between “emergent” and “derived”: space-time can very well have a role and existence at the fundamental quantum level, without invoking “macroscopic emergence”, and still be “derived” (for example in a spectral way) from operationally defined quantum degrees of freedom (for example quantum fields and their operator algebras).

If Peter allows me just one very short excursion into the “off-topic” direction (operator algebras and non-commutative geometry): the interplay between von Neumann algebras (of quantum fields with their Tomita modular theory) and non-commutative geometry (including Connes’ non-commutative standard model features) is still a work in progress and could be much deeper (and interesting for 4-dimensional geometry) … I can assure that there some researchers currently still working on it 😉

Best regards (and sorry for the little “off-topic”) 🙂
Paolo

6. **Jim+Eadon**
September 27, 2024

I also noticed virtually mention of String Theory; wormholes made inside computers malarky, and that.
Does this mean that Quanta has finally got the message on those topics? Or do you think it that the string lobbyists are generally giving up?

7. **Peter Woit**
September 27, 2024

Jim Eadon,
I think the people at Quanta ultimately realized there are various problems with the wormhole claims and that these are not supported by most theorists in the field.

Those theorists interested in continuing to make way overhyped claims about string theory or wormholes are now not getting a sympathetic hearing from either their colleagues or most science journalists. They can now only do this without pushback at certain venues like Brian Greene’s panel discussions.

8. **Peter Woit**
September 27, 2024

I see that right now you can ask the Quanta journalists questions yourself, at
https://www.reddit.com/r/IAmA/comments/1fqogti/were_physics_journalists_who_have_spent_years/

9. **Garrett+Lisi**
September 27, 2024

This was not encouraging:

chevalierbayard
Is String Theory a dead end?

Natalie_Wolchover
I’ve done a lot of reporting and writing about string theory over the past couple of years for my forthcoming book, and it does not at all strike me as a dead end. There’s a huge amount of vitriol about it among certain science communicators, but in my view, they mislead the public on this. Among working theoretical physicists, string theory is a tool for studying quantum gravity and a conceptual framework that’s mathematically consistent, and which is related to quantum field theory – the language of the Standard Model of particle physics. Exactly how or whether string theory is related to reality remains unclear. But it’s part of a web of mathematical languages that is trying to tell physicists something and they aren’t going to stop studying it.

Charlie’s new article, Physicists Reveal a Quantum Geometry That Exists Outside Space and Time, covers fascinating new research that has an interesting tie-in to string theory: https://www.quantamagazine.org/physicists-reveal-a-quantum-geometry-that-exists-outside-of-space-and-time-20240925/

Charlie_Wood42
+1 to what Natalie said. I’ll also add that many theorists consider string theory to be the best working example of what a theory of quantum gravity might look like. It does some impressive and hard to do things like nailing exactly the entropy of a black hole, down to the multiplicative constant. That doesn’t make it right, but it does make it interesting. It also makes it a top contender until someone comes up with a more complete, or more testable framework. Lots of people definitely want that and are working on alternatives, but I don’t think anything has obviously dethroned it yet.

10. **Peter Woit**
September 28, 2024

Garrett Lisi,
Yes, that’s the Quanta editors at their worst. No one is arguing that physicists should stop studying string theory, just that string theorists and journalists who listen to them should stop the forty years of often outrageous hype and instead provide an accurate account of the state of the theory. I’m also not a fan of denouncing unnamed “unnamed science communicators” for unspecified statements.

It’s also amazing how successful the hype for the Strominger-Vafa calculation from nearly 30 years ago has been (it got them a 3 million dollar prize). There’s zero chance that Charlie Wood understands that calculation (for example, that it’s not actually a black hole…), but he’s sure about it impressively “nailing exactly the entropy of a black hole, down to the multiplicative constant.” There’s a generic problem with the Quanta coverage of string theory/quantum gravity that that they’re way too credulous about claims made by certain prominent people in this field, with the wormhole fiasco the most egregious example.

11. **Armin**
September 28, 2024

“No one is arguing that physicists should stop studying string theory, just that string theorists and journalists who listen to them should stop the forty years of often outrageous hype and instead provide an accurate account of the state of the theory.”

I completely agree with the point about stopping the hype, but, at least for that fraction of string theorists who went into it because they saw it as the key to finding the "holy grail of physics", doesn't assessing it realistically imply precisely that they should stop studying string theory?

Also, in the absence of a credible alternative, it might be too difficult to just stop a research program, as a matter of human psychology. It seems to me that as long as people can fall back on justifying what they are doing via "other benefits", they would rather stay with the familiar, even if it does not work, than venture into the unfamiliar which might not work and doesn't even provide a fallback.

What would you tell a person in that situation, Peter?

12. **Peter Woit**
September 28, 2024

Armin,
I'm just not understanding why anyone would want to spend their life studying an idea that doesn't work. If your job doesn't require this why do it? If it does, why not find another one? If you think your string-theory related research program is working, no reason you shouldn't keep at it. But, it is important though that people not deceive themselves and not be deceived by others about what works and what doesn't. Here there are a bunch of prominent string theorists (and journalists at Quanta…) who have something to answer for.

13. **Peter**
September 28, 2024

I studied physics, but for some weird reason spent most of my time in "the media". In the late 1980s, early 1990s, I did some (modest) science journalism. Back then, even if you weren't a specialist, you could have a certain "feeling" for new results. And if you didn't, there often was link between the result and a feasible or already performed experiment to check if the results were going in the right direction.

A simple example is cold fusion. As a physicist (or a chemist) you'd expect that chemical reactions are typically in the eV range. So where the hell does the energy for cold fusion comes from? In labs across the world, experiments were done that didn't show fusion at all. So, if you wanted to report about cold fusion, you had a starting point.

The arrival of string theory changed that dramatically. The subject is so complicated that you had to take the experts' word for it. There were no experiments to keep them honest. The space for the "independent judgment" of a reporter – always a delicate thing in science journalism – was reduced to zero.

So what do you do as a science reporter? I've been in the media long enough to know the answer: you cover you ass. You create a situation in which you can always say "I was was just reporting what the experts were saying. You got a problem with that? Go and tell the experts."

It's also an ideal situation for a herd mentality. You don't want to be summoned to the chief editor's desk to be asked "Hey, what the f!ck? Everybody else is writing that this result of string theory is the greatest thing since sliced bread, and you are skeptical? Who the f!ck are you? It was in a press release by goddamn Harvard! Goddamn idiot, next time we can forget about an exclusive!"

Herd mentalities are strange things, though. It's perfectly possible that somewhere in 2030 the herd will be marching in the opposite direction. "50 years of string theory and you have nothing to show?" And everybody in the herd will be screaming that they already knew in 2024 that string theory was a dead end, physically speaking.

14. **Mark Hillery**
September 29, 2024

I really wish Quanta would not write so much about the extremely speculative and totally-divorced-from-experiment parts of physics. It misrepresents what most physicists do. I don't know how someone who doesn't work in physics regards this stuff, but as someone who does, I have, for the most part, stopped reading it. As far as their latest cluster of articles on spacetime is concerned, I read the one about Wheeler, because I met him a few times and heard one of his talks. His style was oracular and the talk was quite cryptic. The only other article I looked at was the one that talked about von Neumann algebras, because my thesis advisor worked in algebraic quantum field theory (I never did, but I picked up a little knowledge of the subject). No mention of algebraic quantum field theory in the article, just that these algebras are now interesting because some people in quantum gravity are using them. The other articles in the cluster and the video – well I have better things to do. Some of their physics articles that are not about quantum gravity are very good, and I would like to see more of those.

15. **Mathias**
October 3, 2024

Hello Peter,

Saying that it's too early to seek for emergent theories of space-time because (among other things) 4d space-time geometry as we know it has still things to teach us concerning unification issues beforehand (a statement which is arguably sound) is not incompatible with the fact that ultimately space-time will turn out to be an emergent structure from some deeper degrees of freedom of the fundamental laws of physics.

Best,

16. **Bertie**
October 4, 2024

I read Professor Woit's Nov 2023 paper, which was reprised in a recent podcast. I find this work so tantalizing and would like to ask if there is likely very long to wait until a new paper outlining any new developments might appear? Thanks.

17. **Peter Woit**
October 4, 2024

Bertie,
I was planning a longer and more detailed paper after that, but as writing it realized that the fundamental technical issue of how you wick rotate spinor field theories from Euclidean to Minkowski is something I really need to get carefully sorted out. This is taking a while, with slow progress. Even for charged scalar fields what happens is tricky, spinor fields even more so.

18. **Marty**
October 5, 2024

Apologies for this off-topic comment, but…

I just watched Sabine's latest video, where you are mentioned by name at the end. I followed her blog during its entire active existence and have viewed a number of her videos since, and generally agree with most of her views and diatribes. But I can't remember ever feeling revulsion over her commentary like I did with her new video.

Not only was the title very misleading (conflating a narrow area of physics with physics in general), she decided to accuse certain physicists of intellectual dishonesty by name, without obvious provocation.

Even you were not spared — she sort-of complimented you, but immediately followed with a dismissive comment, one that a listener who is unfamiliar with your lengthy history as a skeptic would likely interpret as a suggestion that your underlying motivation is a desire to "sell" your own theory. (Seriously, Sabine? What theory? Do you mean Peter's limited writings on an idea he is excited about and actively exploring? Where is he suggesting he has a theory?)

Overall, I find the video disgusting for her careless denigration of some individuals' character, and of progress in physics overall, even if the actual physics content has merit.

19. **Peter Woit**
October 5, 2024

Marty,
For those wondering what this is about, see
https://www.youtube.com/watch?v=cBIvSGLkwJY

I'm much more sympathetic than you to the rant, since I feel exactly like that some days. Like for Sabine, seeing this argument reappear brought back bad memories of twenty years ago, and depressive thoughts about how the field is dead, people are going to just go on repeating the same arguments about failed theories for the rest of my life and beyond.

The problem with such rants, from me or from her, has always been the question "yes, but why don't you do something more positive than just get outraged about this?" For many years I didn't have an answer to that. There were general ideas that I would write about that seemed to me worth pursuing, but nothing specific that I could point to as highly promising. A few years ago when I started realizing that a set of ideas I'd given up on could actually work and started writing about them, criticism then shifted from "you should be ignored because you don't have an alternative" to "you should be ignored because all you are doing is pushing your own speculative alternative".

Enough time-wasting on this for today, back to trying to figure out exactly what is going on when you Wick rotate spinors…

Richard S. Hamilton 1943-2024

September 29, 2024
Categories: Obituaries

I heard this morning that Richard Hamilton passed away ~~yesterday~~ early this morning. He was a renowned figure in geometric analysis, and a faculty member here at Columbia since 1998. In terms of mortality, the last year or two at the Columbia math department have been grim ones, as we've lost five senior faculty at relatively young ages: Igor Krichever at age 72, Henry Pinkham at age 74, Lars Nielsen at age 70, Walter Neumann just last week at age 78, and now Hamilton at age 81.

Richard wrote a short autobiographical piece about himself at the time he was awarded the Shaw Prize in 2011, available here. There's an interview conducted by his Columbia colleague John Morgan here. Just a couple months ago, Richard was award the Basic Science Lifetime award in Mathematics. You can watch his lecture given in Beijing at the time here.

Richard shared with my four other colleagues that have recently passed away a truly generous outlook on life and other people, very much the opposite of some negative stereotypes of academics as narrow and competitive, hostile to their colleagues and institution. I'll miss him, as I miss the others we have recently lost.

Update: Frank Calegari has some memories of Walter Neumann here.

Comments

1. **Ian Stopher**
   September 30, 2024

   Thank you for posting your recollections of Richard Hamilton. I only know of him through his groundbreaking work on Ricci flow, but now I must dig a bit deeper to understand his great contributions.

2. **Jason Starr**
   September 30, 2024

   I remember Richard Hamilton and S. – T. Yau working on Ricci flow all weekend long, every weekend in the Harvard math department when I was a beginning graduate student. It is a terrible loss. I regularly refer to Hamilton's thesis for a more prosaic result: irreducibility of Hurwitz schemes of covers of higher genus curves (Hurwitz proved irreducibility in genus zero).

3. **Jason Starr**
   October 1, 2024

   By the way, I learned of this part of Hamilton's thesis through Walter Neumann (another sad loss). This part of his thesis was never published, although the same result does also appear in an article of Gabai — Kazez and in Joan Birman's "Braids" volume.

4. **Matt Grayson**
   October 1, 2024

   Richard was generous with his time and talents. Any new idea made his eyes light up and he'd approach the blackboard with a smile, a spring in his step, and a "let's see if it works!" He will be missed.

5. **James Allen Isenberg**
   October 1, 2024

   I have known Richard Hamilton for over 40 years. Just a year or so ago, we held a conference at the Simons center in honor of the 40th anniversary of his epic work on Ricci flow. He was his usual very animated, very insightful, and very fun-loving self. I will miss him very much.

6. **David Ross**
   October 2, 2024

   It is in large part thanks to Hamilton's work on Ricci, that Perelman was able to vanquish the Poincare Conjecture. Perelman would have been the first to say as much, but since he's not in these comments we'll have to say it for him.
   RIP.

All Langlands, all the time

October 3, 2024
Categories: Langlands

Trying to keep track of everything happening in the Langlands program area of mathematics is somewhat of a losing battle, as new ideas and results keep appearing faster than anyone could be expected to follow. Here are various items:

- Dennis Gaitsgory was here at Columbia yesterday (at Yale the day before). I don't think either lecture was recorded. Attending his lecture here was quite helpful for me in getting an overview of the results recently proved by him and collaborators and announced as a general proof of the unramified geometric Langlands conjecture. For details, see the papers here, which add up in length to nearly 1000 pages.

  For a popular discussion, see this article at Quanta.

  To put things in a wider context, one might want to take a look at the "What is not done in this paper?" section of the last paper of the five giving the proof. It gives a list of what is still not understood:

  Geometric Langlands with Iwahori ramification.
  Quantum geometric Langlands.
  Local geometric Langlands with wild ramification.
  Global geometric Langlands with wild ramification.
  Restricted geometric Langlands for ℓ-adic sheaves (for curves in positive characteristic).
  Geometric Langlands for Fargues-Fontaine curves.

  Only the last of these touches on the original number field case of Langlands, which is a much larger subject than geometric Langlands.

- Highly recommended for a general audience are the Curt Jaimungal – Edward Frenkel videos about the Langlands story. The first is here, the second has just appeared here, and there's a third part in the works. One scary thing about all this is that Frenkel and collaborators are working on an elaboration of geometric Langlands in another direction ("analytic geometric Langlands"), which is yet again something different than what's in the thousand-page paper.
- Here at Columbia, Avi Zeff is working his way through the Scholze proposal for a version of real local Langlands as geometric Langlands on the twistor P^1, using newly developed techniques involving analytic stacks developed by Clausen and Scholze. This is an archimedean version of the Fargues-Scholze work on local Langlands at non-archimedean primes which uses ideas of geometric Langlands, but on the Fargues-Fontaine curve. Together these provide a geometric Langlands version of the local number field Langlands program, with no corresponding geometric global picture yet known.
- Keeping up with all of this looks daunting. To make things worse, Scholze just keeps coming up with new ideas that cover wider and wider ground. This semester in Bonn, he's running a seminar on Berkovich Motives, and Motivic Geometrization of Local Langlands, promising two new papers ("Berkovich motives" and "Geometrization of local Langlands, motivically"), in preparation.

  As a sideline, he's been working on the "Habiro ring" of a number field, finding there power series that came up in the study of complex Chern-Simons theory and the volume conjecture. According to Scholze:

  > My hope was always that this q-deformation of de Rham cohomology should form a bridge between the period rings of p-adic Hodge theory and the period rings of complex Hodge theory. The power series of Garoufalidis–Zagier do have miraculous properties both p-adically and over the complex numbers, seemingly related to the expected geometry in both cases (the Fargues–Fontaine curve, resp. the twistor-P^1), and one goal in this course is to understand better what's going on.

- Finally, if you want to keep up with the latest, Ahkil Mathew has a Youtube channel of videos of talks run out of Chicago.

Comments

1. **DH**
   October 3, 2024

   Actually, geometric Langlands for Fargues-Fontaine curves is being dealt with – see my lectures on Akhil's channel for a detailed discussion of the current status of things.

2. **Quasiparticular**
   October 4, 2024

   "Keeping up with all of this looks daunting."

   And gosh, you've been writing about the Langlands program for two decades! At this time is there any component of the Langlands shockwave that pulls focus on physics? (I assume Langlands/QFT is a topic still dear to you)

3. **Peter Woit**
   October 4, 2024

   Quasiparticular,

   First of all, several people wrote in, outraged about how mathematical physics has become. This post was purely about developments in mathematics research by mathematicians, not about physics. Physicists are not involved at all in this.

   I've always been intrigued by tantalizing connections of this field to physics, but these recent developments don't seem to go in that direction. From its beginnings, there have been relations between geometric Langlands on a Riemann surface and 2d conformal QFT. Witten found a great deal of material relating geometric Langlands to a specific 4d QFT. David Ben-Zvi has written a lot about how the topological QFT point of view illuminates the larger structure of this subject. That the twistor P1 shows up here as the description of an archimedean point and in physics as the twistor description of a space-time point continues to fascinate me (but, may be meaningless..)

   So, nothing really new that I'm aware of on the relation to physics front, but, as pure mathematics, a great deal is happening here.

4. **kitchin**
   October 6, 2024

(Math) people might be interested in a conference in Copenhagen last summer, consisting of lecture series by Clausen, Efimov and Nikolaus. "Continuous K-theory, dualizable and rigid categories." Videos as well as good notes: https://www.math.ku.dk/english/calendar/events/masterclass-continuous-k-theory/ . I've only dipped into to the Clausen talks so far, joint work with Scholze. More analytic structure built into complex manifolds.

The second Curt Jaimungal – Edward Frenkel video Peter mentions contains a nice summary of three different Langlands'es, starting with the only one Robert Langlands endorses for the use of his name (the number theory one)! It goes from about the 1:40 mark to the end of the video. I don't recall it laid out so concisely in his book Love & Math. Somewhere else in the video Frenkel recounts his conversations with Langlands, which is charming. (And informative, are far as leading to making abstract things more like functional analysis, as far as I understand it.)

Two hundred year old number theory problems are a good validation of math, in a sense applied math. Since group theory started there, it makes some sense the algebra would grow to almost incomprehensible levels. As long as it solves problems.

5. **Jim+Eadon**
   October 7, 2024

   Dear Peter, Is the latest research by Scholze et al involving the Twister P1 have any impact on your own research, and, even, vice versa?

6. **Peter Woit**
   October 9, 2024

   Jim Eadon,
   So far I fear the impact on my own research has been negative, since I've spent a fair amount of time trying to understand it (which has been rewarding in its own way), but don't see any particular way to connect the way the twistor P1 appears there and in my own research.

7. **toto**
   October 30, 2024

   Seems like the lectures on the Habiro ring are online : https://archive.mpim-bonn.mpg.de/id/eprint/5132/

Various Items

October 24, 2024
Categories: Uncategorized

A few items that may be of interest:

- Edward Frenkel has a new Youtube show/podcast, entitled AfterMath. I gather that part of the concept here is a follow-on to his book Love and Math, but in this different format. He's always thought-provoking and well-worth listening to on almost any topic, I'm looking forward to seeing what he does with this.
- Also in the Bay area, Michael Peskin recently gave a talk on How Should We Think about 10 TeV pCM Colliders?. Much of it is a very sobering look at the possible known ways to build a collider capable of colliding elementary particles at 10 TeV in the center of mass. The technical challenges are daunting and if this is going to get done it's going to take quite a while and be very expensive.

  Besides the technological and financial problems, he faces up to the main problem of justifying such a project:

  > Are the secrets of electroweak symmetry breaking and the Higgs field to be found at 10 TeV ? If we believe in this, we must still find arguments to convince our skeptical scientific colleagues. If we don't believe in it, we are believing that there is no point in making the next step in collider physics.
  >
  > We cannot imagine the future of particle physics without grappling with this question.

  More specifically, he sees the challenge as

  > to get the money to build such a collider, we would need
  >
  > **definitive proof of violation of the Standard Model from HL-LHC or Higgs factories**
  >
  > or
  >
  > **a clear and compelling model to be tested** (as the MSSM was for LHC).
  >
  > In my opinion, this puts a large burden on the theory community
  >
  > 1. To be sure that an e+e- Higgs factory actually is built
  > 2. To put forward simple and attractive models of EWSB with a "little hierarchy"

  Unfortunately I don't see any evidence of any attractive ideas about 2., and the sad history of the hype about SUSY and naturalness means that people are going to be looking a lot more skeptically at any claims by theorists to have such a thing.

- Speaking of Peskin, if you're looking for an alternative to Peskin and Schroeder, there's a recent new QFT book that I've seen which appears to be quite good: Introduction to Quantum Field Theory, by Anthony Williams. It doesn't go as far as Peskin and Schroeder and other textbooks that get seriously into Standard Model physics, but it has a lot more detailed and careful explanations of the basics of relativistic quantum field theory. As such it should be significantly more readable by advanced undergraduates and beginning graduate students.
- Finally, two questions I'm wondering about, curious if anyone reading this knows the answer:

  Whatever happened about the bet between Ken Lane and David Gross over SUSY? Did Gross pay up?

  What's going on with the 2025 Breakthrough Prizes? In past years, these things have been announced in September, Hollywood "Oscars of Science" ceremony in the spring. This year, nothing in September, and October is almost over, so wondering if the Breakthrough Prize people have a new concept for the prizes for the coming year.

- One more thing: I just noticed that the SMF has recently published a 1963 text of Grothendieck's, his notes for a fall 1963 seminar at Harvard on duality theorems in algebraic geometry. Hartshorne ran the seminar and later wrote up notes, which were published as LNM 20, Residues and Duality.

Update: I've confirmed with Ken Lane that it seems David Gross won't admit that SUSY has been a failure and he's given up on Gross ever paying off on the bet. For more about the reaction of Gross and others to losing SUSY bets, see here
https://www.math.columbia.edu/~woit/wordpress/?p=8708
For documentation of the 1994 Gross/Lane bet, see page 62 of
https://indico.cern.ch/event/527162/contributions/2159007/attachments/1298122/1936489/deroeck_SUGRA_2016_v4.pdf

Update: Thanks to commenters who have answered both of my questions. Besides finding out what happened with the Gross/Lane bet, this comment explains that the new plan for the Breakthrough Prize is to announce it at the same time it is awarded at the Hollywood "Oscars of Science" event in April (so, 2025 prizes announced and awarded April 2025).

Comments

1. **Andrzej Daszkiewicz**
   October 24, 2024

   Thanks for the tip about the Williams' book.
   BTW, do you plan to add chapters 12-16 to your notes on QFT? I'm about to start reading them.

2. **James Lai**
   October 25, 2024

   Off topic: Hi Prof. Woit, I am eagerly looking forward to your QFT notes as I am half way through your book

3. **David Brown**
   October 25, 2024

   Did Gross pay his SUSY bet to Lane?
   According to a Physics Forum entry, Lane did enjoy an expensive restaurant meal at Gross's expense (in 2016? or 2017?).
   https://www.physicsforums.com/threads/interesting-anecdotes-in-the-history-of-physics.1059261/page-4

4. **Peter**
October 25, 2024

Give us your take on this year's physics Nobel Prize! Is it physics? Is it worth of a Nobel Prize? Is it an indictment of physics research? Has the Nobel committee jumped on a bandwagon?

5. **Peter Woit**
October 25, 2024

Andrzej Daszkiewicz/James Lai

Sorry, but I haven't found the time to work on that project. I've been working out the details of how Wick rotation works for chiral spinors. As part of that, I understand much better how Euclidean QFT/path integrals are related to the Minkowski QFT/operator formalism. Whenever I get to writing more about QFT I want to incorporate this material.

6. **Peter Woit**
October 25, 2024

David Brown,
I'm not sure that physics forum entry reflects anything other than that by 2017 there was sufficient LHC luminosity that Gross had lost by the precise terms of the bet. Did Lane really get his dinner?
While Gross over many years heavily promoted (as a "prediction" of string theory) the claim that the LHC would see superpartners, I'm not aware of any place where he ever admitted that this didn't work and that he had lost the bet.

7. **Peter Woit**
October 25, 2024

Peter,
I thought of writing about the Nobel Prize, didn't do so because it seemed that everyone likely could guess what I thought. Also, the reaction I was seeing online was lots of outraged physicists, so better to let them be the ones to complain about this, instead of more complaints from me.

In case you can't guess what I think about this, it reflects the ongoing problem the physics Nobel committee has, that large parts of fundamental physics have basically been dead for quite a while, so they have to find new areas to give the prize to. On the AI aspects, the last thing the world needs now is more rewards and attention given to AI research. While Hopfield has claims to be a physicist and one could make a reasonable case that the work he did counts as physics, the choice to reward Geoffrey Hinton with a physics prize is really hard to understand.

8. **David Brown**
October 25, 2024

In reply to an email inquiry, Professor Lane kindly sent the following response:
"No, Gross never paid. I think he never will. He is what is called a "welsher" — an unfortunate term; it has nothing to do with people of Wales, who are maligned by this term."

9. **Peter Woit**
October 25, 2024

For documentation of the Gross/Lane bet, see page 62 of these slides:
https://indico.cern.ch/event/527162/contributions/2159007/attachments/1298122/1936489/deroeck_SUGRA_2016_v4.pdf

10. **James**
October 25, 2024

Years ago one of Ken Lane's former students relayed the tale of Ken giving a talk to a group people involved in giving out funding. At the end, one of the questions was what will he do if his theory (technicolor) doesn't work out, and his response was something like "I guess I'll fall on my sword and retire." We had a good laugh about that, but now after reading this blog for a few years it's clear that such behavior is not universal.

11. **Mathematician**
October 28, 2024

From the Breakthrough Prize: The foundation will announce the next prize laureates in April 2025 coinciding with the annual ceremony. Moving forward, the yearly schedule will be altered with the nomination portal open from April to July (specific dates to be announced), with again the prize laureates being announced in April.

12. **Andrew**
October 29, 2024

Talking of introductory books on QFT, I wondered if you'd looked "A Prelude to Quantum Field Theory" by Donoghue and Sorbo?. It seems like it should be a good once over lightly for tourists who just want a bit more detail than, for example, Zee or Carroll's recent pop physics books on the subject, but I've seen comments that it goes a bit too far in the direction of terseness.

13. **Pasquale Di Cesare**
October 31, 2024

Peter Woit
Instead, I feel you should and hope you write more in depth about the recent Nobel. I guessed your reaction but more insight would shed light on progress in physics and epistemology. As long as Hopfied is concerned, it looks to me his RNN is a spin glass. Parisi's shared Nobel in 2021 was, in particular, also for resolving SK model of spin glass. He interestingly wrote: "I think that the Nobel prize in physics should continue to spread into more regions of physics knowledge," … "Physics is becoming wider and wider, and it contains many areas of knowledge that did not exist in the past, or were not part of physics." But I agree on the AI hype wagon, unfortunately.

14. **Peter Woit**
October 31, 2024

Pasquale Di Cesare,
I'm afraid it does look like the parts of physics I care most about are dying off (or dead already…). Funding and attention is moving towards other topics, ones I know little about and have no interest in taking the time to learn more. So, for topics like neural networks, seems best for me to not write about them, whether people want to

call them “biology” or “physics” or whatever.

15. **Stephane Dubedat**
November 4, 2024

> Are the secrets of electroweak symmetry breaking and the Higgs field to be found at 10 TeV ? If we believe in this, we must still find arguments to convince our skeptical scientific colleagues. If we don’t believe in it, we are believing that there is no point in making the next step in collider physics.

I have just recently started reading your amazing blog so I still don’t know for sure your opinions on the matter of the future of particle physics experiments.
It seems to be a popular trope that particle physics makes no new discoveries and only confirms prior knowledge (the standard model) and therefore that theoretical particle physics (string theory, GUTs, sterile neutrinos, glueballs, etc) is “dead”.
This trope is false and stems from the fact far too few people are aware of the experimental anomalies.
Moreover it also is a result of not considering wether current colliders are “underpowered” in order to constrain the existence e.g. of a new particle.
I invite the reader to see figure 3 page 11
https://arxiv.org/abs/2309.03870

The paper shows that there are actually many major anomalies in particle physics, moreover most importantly it shows there are anomalies at every order of magnitude of energy, from 0.1GEV to 1TEV.
The paper is very far from exhaustive as new anomalies are discovered each year in hep ex, even more if you take into account the interellated cosmological anomalies (such as the galactic center gamma ray excess or the photon count excess).

As such, particle physics (and cosmology) are, which is extremely misunderstood, in their golden age.
The other trope is that to discover new physics we need higher energy colliders..
As seen, spanning 5 order of magnitude of energy, there are anomalies at every level. Therefore it seems likely new anomalies will keep being discovered beyond 1 TEV, especially considering the known anomalies in ultra high energy cosmic rays.
Moreover there are particles that are best observed at 10TEV. The higher energy also increase the statistical power, allowing to move known anomalies beyond 5 sigmas.
However, while a 10TEV collider would be useful, especially via resurrecting the already dug desertron, in terms of cost versus reward, there is the extreme low hanging fruit of making low energy and 1TEV energy colliders with higher accuracy, as yes, there are more new physics to be found in the 0.1Gev to 1tev range than in the 10tev range. Much more important that raw energy, is the accuracy of the collider, which is the product of its luminosity (as shown by the tremendous success of belle II) and by its paradigm (electron collider, LINAC, muon collider), which is what is needed to once and for all confirm the myriad of known anomalies as scientific discoveries.
And this crucially lack funding despite being moderate cost…
While some major specific particle factories are being delayed.. the salvation and revolution in particle physics is coming very soon, via the arrival of two new higher than LHC precision colliders: NICA https://arxiv.org/abs/2401.12893
and FAIR https://arxiv.org/abs/1808.09550
Those two projects, will change particle physics forever as being the first colliders to not be underpowered, not regarding energy, but regarding precision, the signal to noise ratio (of which belle II has shown the path). And are operational in 1-2 years not the decades..
Another advance parallel and as useful as colliders, are experiments that sets new limits of the electric dipole moment and lacks funding.
Among the known anomalies, the one that should be confirmed the soonest (1-2 years) should be the lowest energy anomaly: the X17 particle.
Now in terms of “novel” ideas in particle physics that are actually original, there is one considerably underknown, which is the extreme prospect of resurrecting the Sakata model, which works very surprisingly well.
https://www.semanticscholar.org/paper/Sakata-Model-Revisited%3A-Hadrons%2C-Nuclei-and-Stefanovich/2b49b07cd8a0b82bbbfff2e5bbeb134721b3a9df

Of note is that the best empirical fit no longer is the standard higgs but a higgs triplet https://arxiv.org/abs/2404.14492

The Crisis in String Theory is Worse Than You Think…

October 31, 2024
Categories: Uncategorized

Curt Jaimungal has a piece out, an interview with Lenny Susskind, with the title The Crisis in String Theory is Worse Than You Think…. Some of what Susskind has to say is the same as in his recent podcast with Lawrence Krauss (discussed here). These days, Susskind sometimes sounds like Peter Woit:

> We live in the wrong kind of world to be described by string theory. No physicist has ever won a big prize for string theory. I can tell you with absolute certainty that it is not the real world that we live in. So we need to start over.

(interesting that Susskind seems to think the "Breakthrough Prize" is not a "big prize", maybe because he's one of the few well-known string theorists who hasn't gotten one).

Susskind says he himself is working on trying to extend string theory to something different which will work in dS space, not just AdS, but he agrees with my claim that this is something the field has essentially given up on:

> I actually don't know anybody who is working, striving to try to expand the theory into either de Sitter space, which is not supersymmetric, or just more generally into an expanded version of the theory. Older people worked on it in the past. They worked on something called spontaneous breaking of supersymmetry. Don't worry about what it means. It just means the theory wouldn't be supersymmetric, and they failed. Now, that's not a criticism of them. I worked on it, and I failed. That's not a criticism of anybody, but it's a fact that there is no precise theory which is not supersymmetric.
>
> That is intolerable, in a sense. It can't stay that way. We have to describe our world. That's our purpose, and as I said, I don't know anybody who's actually working on that. If you were to send out a message to all the world's theoretical physicists, anybody working on a generalization of string theory, you'd probably find some yeses, probably mostly among older people, and somehow we have to change this.

I'd argue the field has given up on it because, after decades of work, it's clear this goes nowhere, and sooner or later Susskind will realize this.

At one point Susskind starts making an odd argument, hard to reconcile with the current state of the subject:

> Look, there are still people who believe in the flat Earth, for God's sakes. There's people who believe all kinds of weird stuff. Don't think about individuals. Think about the consensus of the largest fraction of physicists working on these things, and you'll probably be right. The overall consensus of the field tends to be right. Peculiar individuals, no matter how famous they are, no matter how brilliant they are, if they're off that consensus, and they've been off that consensus for a long time, they're probably wrong. That doesn't mean for sure that they're wrong. Don't look for the weirdos. Look for what the consensus of the majority of well-respected, highly accomplished physicists believe. And you'll probably be right. There's no guarantee of it. There are very few cases where the consensus has gone wrong for a long period of time, just where some offbeat idea of some particular individual suddenly changes everything. I'm not saying it doesn't happen, but rarely. Penrose, what can I say? He believes all kinds of things that I wouldn't subscribe to. But more than that, things that the consensus wouldn't subscribe to.

Besides the weirdo Roger Penrose, he's no fan of the ideas of another weirdo:

> What is Peter Woit? If you look on the Internet, if you look on the archive, he has a small number of papers which are bad. They're bad mathematics and bad physics. They're just bad. I probably shouldn't say that. I probably shouldn't, but I'm going to say it anyway. He has nothing to offer at all. I assure you that if he had something that was compelling and interesting and that solved some problem, the physics community would notice him. I looked at his papers. I was unimpressed,

I guess his reaction is fair, partly since my own criticism of his work on the landscape is much the same (I have on the other hand had nice things to say about his textbooks, some of which appears as a blurb on the French edition of one of them).
In any case, if you start with the assumption that anything too far off the consensus is going to be unpromising, you don't need to spend much time looking at my work to confidently evaluate it as having nothing to offer.

Jaimungal does get Susskind to realize that the "if it's not close to the consensus, it's probably bad" argument is a dubious one, especially at a time when the consensus research program has clearly failed:

> But you're perfectly right. We should certainly be on the lookout for ideas which are not the consensus. We should be watching for them and not immediately dismiss them because they're not exactly the same as the ideas that we've been pursuing. For sure, we should be doing that. So I would agree with you about that. And maybe we haven't been diligent enough with some of these ideas…
>
> Most of the people I know, and that might even include myself to some extent, are derisive about a lot of these ideas. And they're correct that there is a very strong skepticism about them, and maybe to some extent, unfounded. We all know that. There's nothing hidden about that. The answer is I've looked at them, and I don't find anything compelling about them. If you call that derision, yeah, I am a little bit derisive. However, I would say maybe there are elements in those theories which will come back, come back in some different form, which will connect better with the things which I think are right. And that's a possibility, which I suspect most of my friends don't entertain.

Who knows, some day Susskind may come around to the idea that one of the SU(2)s in the 4d Euclidean rotation group being an internal symmetry is not complete nonsense. Once I finish writing up a more detailed version of what I've been working on I'll send him a copy. Maybe I'll even finally figure out a way to use this to do something new with Kogut-Susskind/Kähler-Dirac versions of fermions, and he'll be pleased that in 1977 he was on the right track…

Update: Somewhat related to the posting is this new rant from Sabine Hossenfelder. It's motivated by this from "Professor Dave", who has 3.4 million Youtube followers and is upset that she is hurting the credibility of scientists by criticizing what has happened in fundamental physics over the past 50 years (a topic he seems to know nothing about). While I disagree with her about some things, I strongly identify with:

> Why the fuck is it my fault that cranks think I'm their best friend because I'm pointing out that there's no progress in the foundations of physics? It's a fact. We haven't made progress in theory development for 50 years.

To connect explicitly to the topic of this posting, a big reason for the lack of progress is the way Susskind and other leaders of the field see things. In their minds it's not possible that the consensus (i.e. groupthink) of GUTs/SUSY/strings of the past 50 years could be wrong. Anyone who argues otherwise is a "weirdo" who doesn't understand the arguments behind the consensus and can't possibly have any useful ideas about an alternative.

Comments

1. **Anonymous**

October 31, 2024

The link in your final sentence requires authorization to properly view, this seems to be better: https://journals.aps.org/prd/abstract/10.1103/PhysRevD.16.3031.

2. **Peter Woit**
October 31, 2024

Anonymous,
Thanks, changed to that one.

3. **Alvaro Ledesma**
October 31, 2024

After this declaration, why does nobody mention Edwar Witten and its role in keeping consensus in this dead-end route? Great work in mathematics, sure. Genius. Not so much on keeping string theory in life support instead of taking the much more difficult decision of unplugging it.

4. **Robert Smart**
October 31, 2024

Prof Alexander Hamilton from Colorado has wandered into this area. In the abstract of an upcoming (1-Nov) talk (https://gaupdate.wordpress.com/2024/10/29/osmu-2024-lect-a-hamilton-string-theo-unif-geom-alg-01-nov-2024-online/) he writes "chief arguments against bosonic string theory without any supersymmetry are, first, that it does not admit fermions, and second, that its ground state is tachyonic, therefore unstable. But D-branes, discovered by Polchinski in 1995, are surfaces upon which open bosonic strings can end, and those D-branes can carry fermions.".

5. **SM**
October 31, 2024

Just want to acknowledge what seems to me as a mature, reasonably humble, and constructive reply to a public criticism. That's not easy.

6. **Felix**
October 31, 2024

Being accused of doing bad physics by the person who wrote 'Dear Qubitzers, GR=QM' (1708.03040) has to be some sort of badge of honour

7. **DrDave**
October 31, 2024

It's so tragically sad that halfway (!) to a moment of realization that could genuinely help the field, Susskind devolves into ad hominem attacks. So many follow this route…. If he found something in Peter's or Penrose's work that he disagreed with, why not engage in ideas, through a letter, or in a journal? You're a scientist; do science.

8. **Sabine**
November 1, 2024

Trouble is, if you search Inspire for papers on the topic string theory the paper production isn't tapering off. It's just that we don't hear about it much anymore. Swampland is tapering off. Wormholes are still going strong. And quantum simulations are at all all time high.

9. **tbgb303**
November 1, 2024

Hm, read Not Even Wrong when it came out – only just discovered your blog! (Yup, I'm a bit slow, but we get there in the end)

Just to repeat the previous poster, it is admirable to see ad hominem attacks politely brushed aside after finally being accepted as correct about String Theory's limitations and cultural stagnation. It's only taken nearly 20 years – hats off!

10. **Jim+Eadon**
November 1, 2024

"Susskind says [he] is trying to extend string theory to something different which will work in dS space"
So, Suskind is trying to formulate a superstring DeSitter holographic duality? I would have thought many have tried to do that: do you know of such a research program? Are there serious blockers?

11. **David Brown**
November 1, 2024

Please give us an umlaut: Kähler <— Kahler.

12. **Yorg**
November 1, 2024

The part where he said that he still believes in GUT despite no observed proton decay was also interesting.

13. **Low+Math,+Meely+Interacting**
November 1, 2024

Consensus? Well, once there was one about phlogiston. The luminiferous aether had a period of reasonably broad acceptance. Observation contributed significantly to changing the consensus.

In the present, with little hope of observation as a correction for the frontiers of high energy physics, consensus seems a rather dubious lodestone. Seemingly one now just has to muddle about, with the hopes that many people doing the same on some different tack will eventually lead to someone having a breakthrough, hopefully with observable consequences. Very difficult. Extremely risky. Thousands of theorists working, most or all of them doomed to failure. I fail to see how modern academic consensus could possibly support such a diversified endeavor. Major structural change would be needed, and it seems entirely unlikely to materialize. What a prison consensus has turned out to be.

14. **Peter Woit**

November 1, 2024

Dr. Dave,
Actually I much prefer the straightforwardness from Susskind to the usual tactic of string theorists who disagree with me, which is to do everything possible not say my name and pretend I don't exists, acting like it would be beneath them to engage with anything I do.

This was different partly because Jaimungal went out of his way to ask repeatedly and specifically about me, partly because Susskind in general makes a point of not mincing words and saying exactly what he thinks.

The way he went on about the "consensus" was fascinating. It is exactly the way most people in this field think, although they don't say so out loud. Because this is the way almost everyone behaves, nothing is going to change here until the day the "consensus" changes. This interview is fascinating, showing Susskind trying to reconcile two contradictory things: "the consensus is always right" and "the consensus is stuck".

15. **Peter Woit**
November 1, 2024

Sabine,
Yes, that's really strange. I'm serious when I say this is a subject that has intellectually collapsed and died, but that doesn't mean people have stopped writing papers. It really has become kind of a zombie situation.

16. **Peter Woit**
November 1, 2024

Jim Eadon,
Susskind's comments about other people not trying do dS holography are pretty odd. Obvious places where the string program got stuck and doesn't do what you want (M-theory, holography with non-negative curvature, etc) have been well-known for decades. People don't work on these because there's no promising idea, all evidence is that these are speculative ideas which don't work.

17. **Pauline**
November 1, 2024

Sting Theory will never end until this blog ends, as this blog has talked more about string theory than any other blog/youtube channel/media outlet over the past three decades.

The sooner Peter retires, the sooner physics can move on.

18. **clueless_postdoc**
November 1, 2024

"Actually I much prefer the straightforwardness from Susskind to the usual tactic of string theorists who disagree with me"

Having seen conflicts in other fields outside of physics, I tend to agree with this. The one thing more rude than having heated discussion is ignoring the person you disagree with and hoping/persuading others do the same. I've seen people ignore disagreements with others under the name of diplomacy, but IMO this behaviour is doing much more damage to the field.

(though on a more realistic note maybe it's sometimes not practical to always argue, protracted arguments are expensive time and energy wise and you never know if the person you argue with will behave reasonably)

19. **Peter Woit**
November 1, 2024

Pauline,
Maybe that's the explanation for why people are still writing over a thousand meaningless papers about string theory each year: they're doing it just to piss off Sabine and me, if we stopped, they'd stop.

20. **Ryan**
November 1, 2024

Maybe I'm being overly generous to Susskind, but I read his comments as saying that theories outside of the consensus are usually (implying not always) wrong. And that's correct – most theories that go against consensus turn out to be incorrect.

However, contrary to Susskind's admitted derision of non-consensus theories, a willingness to buck consensus should be applauded even if the effort ultimately fails, especially when consensus is stuck. It's that 1 theory out of 100 that actually works and upends consensus that makes the 99 failures all worthwhile.

21. **Yoh Tanimoto**
November 1, 2024

This is the first time I leave a reply here but I have been a long-time reader of your blog and appreciate very much your critical comments. I am also curious about your recent papers as I work on the Osterwalder-Schrader axioms and await enthusiastically your further developments.

Apart from the interesting admission of Susskind about the failure of the string theory as a theory for the real physics, I would like to know what he means when he says "(the String theory) is a very very precise mathematical structure, it exists, it's well-defined". It is well-defined in which sense? Do I understand correctly that even the perturbation series for the S-matrix is not proven to be finite even on the flat 10-dimensional spacetime?

22. **Peter Woit**
November 1, 2024

Yoh Tanimoto,
When physicists like Susskind say things like what you quote, you need to take into account that they are spending most of their time doing things like "ER=EPR" which are completely ill-defined. The minute they have any actual equations, then they have a "precise mathematical structure". If they can compute a one-loop term, then it's "very, very precise" and the theory is well-defined.

Possible problems with the superstring perturbation expansion in flat 10-d spacetime are not really the point. The problem is that to leading order this looks nothing like the real world. "Compactification" schemes to make it look like the real world quickly get you deep into ill-defined territory. Or, more accurately, anything well-defined doesn't look like the real world, you need to head off into regions where there is no well-defined theory to avoid falsification of your idea. This is what happens when you start with a wrong idea, except that normally people give up (or don't wait 40 years to do so).

23. **Bernhard**
November 1, 2024

Susskind's unprofessional behavior and continuous ad-hominem attacks are probably one of (but certainly not only) the reasons nobody (but string theorists) take string theory seriously anymore. Obviously none of us can shout it at a conference because we fear funding repercussions, but we all say it privately during coffee breaks. It is fine to criticize and even trash a paper – it is not fine to use the argument "who is X" to make a point.

24. **Arnold Neumaier**
November 1, 2024

Ryan,

"It's that 1 theory out of 100 that actually works and upends consensus that makes the 99 failures all worthwhile."

No. The good theories replacing more limited theories were always due to better insight, not due to trying all possibilities. Only those few failures that increase the insight in a significant way are worthwhile. It does not help to search under 99 or 9999 lamp posts when the key was lost where there was less light.

25. **Yoh Tanimoto**
November 1, 2024

I see… so, for mathematicians, it is not well-defined even in the flat 10d spacetime.

Well, I understand your point that the passage to 4d is much more problematic. I think I would still appreciate if something (quantum) were really well-defined even in 10d, but I'm afraid I won't see it as no one is working on it.

26. **Peter Woit**
November 1, 2024

Yoh Tanimoto,
I think the "consensus" view on 10 d superstring theory in flat space time is:
1. How to define the perturbation expansion for all terms in higher genus is a very technical problem, conjecturally all such terms will be finite.
2. The perturbation expansion is just an asymptotic expansion, and will not converge for non-zero coupling.

My interpretation of this has always been that, as far as mathematicians are concerned, this is not, even conjecturally, a truly well-definied theory. Susskind is using a different definition.

27. **Erich+Weiss**
November 1, 2024

Is Susskind claiming that string theory is "the consensus"? Among whom?
People he talks to? Most physicists I talk to have decided long ago that string theory is just failed physics. What on earth does he mean?

28. **Peter Woit**
November 1, 2024

Erich Weiss,
Who he talks to is theoretical physics faculty at Stanford, Harvard, IAS/Princeton and a few other places. While their colleagues in other fields of physics are realizing this is a failed project, they're sticking to the "consensus" that string theory is "the only game in town". For them, those in other fields who don't agree are just basically ill-informed about the complexities of string theory and its great achievements, and not as bright as they are. Those in the same field who disagree are "weirdos".

29. **Douglas Bundy**
November 1, 2024

Many years ago, I pointed Peter to a talk by the late, great Sir Michael Atiya, given at David Gross' group in California. I remember that he expressed his bewilderment at the baroque nature of string theory, and the amazing fact that QM depends upon the ad hoc invention of man, the square root of -1. He said, if string theory turned out to be true, he would find it entirely bizarre that nature was constructed that way. Maybe, Susskind's recommendation to "start over" should begin there. Just saying.

30. **Peter Woit**
November 1, 2024

Douglas Bundy,
I'd forgotten about that. For those interested, see
https://online.kitp.ucsb.edu/online/plecture/atiyah/

31. **Mitchell Porter**
November 2, 2024

Since his conversation with Lawrence Krauss, I have been puzzling over Susskind's remarks regarding supersymmetry. I still don't really get why he's so negative about the prospects for supersymmetry breaking within string theory. But maybe the real reason why he wants a non-supersymmetric generalization of string theory, is that his top priority is to understand quantum gravity in De Sitter space. That would make sense to me.

32. **Alex**
November 2, 2024

Just pointing out that it should be obvious that there's a difference between the usual consensus that happens in heatlhy science (which is an essential part to drive progress), which is strongly backed by empirical findings, and its degenerate, cancerous growth relative, the group think. Of course, what Susskind sees as the former in his field, most of the rest of the community has finally reached, or is reaching, a consensus (oh, the irony! haha) that it's actually the second thing, a tumor that has to be removed before it kills the patient.

33. **Juraj**
November 2, 2024

What Susskind calls “consensus” is nothing more than a groupthink. The change will not come from those that comfortably live from the current situation.

34. **Eric+Weinstein**
November 3, 2024

After listening to this carefully, I am of the opinion that something extraordinary happened during the interview, and it is due to the host that things got really interesting.

As I understand it, Lenny is trying to say something radical within the established frameworks, but without losing those paradigms and questioning them. It’s like saying “We need to challenge all our assumptions and go back to square one…within String Theory.” The host then does something that I have never seen. He respectfully lets Susskind go on, but points out the contradiction that no one has been able to get at for 40 years: how can all of these theorists say with certainty that String Theory is the “Only Game in Town” when they clearly have no idea if there is anything going on outside of String Theory?

What is unique about Jaimungal as host is that he has far more information than a non-physicist like Dennis Overbye or KC Cole. But as a non-physicist, he can push back on the interviewee for the first time. When the guest says something wrong, Jaimungal is right there pointing out that the claims being made are actually wrong. This has never happened before. Either you get interviewers like S. Carroll and NDGT who are not String Theorists (but can seem like marketers for Strings), or you get people who have to take what is claimed on faith. I think what just happened is that we saw our first earthquake interview of this type. Respectful. In depth. But not Public Relations for String Theory.

The result was that Susskind actually changed his position DURING the interview. He went from saying “Follow the consensus and avoid the weirdos almost always.” at the beginning, to “You should ignore what I have to say about other researchers and other games in town. I don’t know them. I don’t know those people so I shouldn’t say. Maybe they are doing things. We don’t know. We tend to be very derisive in String Theory. This is not exactly a secret. It’s entirely possible the answer is with someone who is outside.” I have never seen this before.

35. **Dawood Kothawala**
November 3, 2024

Alex: Over the past year, I am hearing more and more string theorists pompously claiming that information paradox has been solved using wormholes-entanglement connection of Susskind. And I am talking here about well known string theory researchers!

36. **Low+Math,+Meely+Interacting**
November 3, 2024

Having watched the entire interview, I found it a generally laudable discussion all around. My two main quibbles are the paradoxical advice to young theorists and the terse dismissals of “bad” physics. For the latter, I should point out I greatly appreciated Susskind’s concise summary of the difficulties of the Many World’s interpretation of QM. I’ve seen all of them before, but I think Susskind has a talent for rendering a pithy verdict. So it’s disappointing, if he’s actually put in the time to read a paper (which seems to be the case) to hear it simply dismissed as “bad”. I would have genuinely been interested in his reasons for thinking so. Apparently he couldn’t be bothered.

As for advising new generations of physicists to strike out on their own, he seems to be saying they should be willing to commit career suicide en masse for the good of the discipline. That doesn’t seem to me to be very helpful. Rather, I would have liked for him implore folks with tenure to give talented young mavericks a living, and provide them the opportunity to at least fail well. It likely does really come down to the incentive structure, and high-minded exhortations to follow Truth will never be enough.

37. **Marvin**
November 3, 2024

Eric+Weinstein

From the interview: “We tend to be very derisive in String Theory. This is not exactly a secret. It’s entirely possible the answer is with someone who is outside.”

Again, he can’t close the box definitively,by not saying that a completely new theory will come from outside, but instead pretending that an answer to String problems will come from outside. It is Zombieland and the Outsider.

38. **SDR**
November 3, 2024

That is almost as devastating as things could possibly be for string theory as a physical theory of nature. And his ad hominem attack is disappointing in somebody of his stature: it makes him look small-minded and vindictive.

39. **Peter Woit**
November 3, 2024

While Susskind does acknowledge the possibility that outsiders/weirdos might do something important, it’s in the context that they might come up with an idea that will build upon the consensus research program and vindicate it. I don’t hear him anywhere acknowledging that the whole program might be a mistake. For him and the others at Stanford/IAS/Princeton/Harvard/etc. who have been doing this for forty years, it seems to be completely inconceivable that the whole thing could be wrong.

40. **Matthew Foster**
November 4, 2024

String theory has always been a deeply weird program. Obviously many technical developments “along the way” were amazing and useful (2D CFT e.g.), but the original 30 years of worldsheet theory building is one of the strangest developments I can think of in theoretical physics: trying to build a many-particle theory of everything from the vantage point of a single string, splitting and merging into a few others, operating in an ambient fixed background spacetime.

And I still don’t get why the branching polymer result due to Froehlich et al wasn’t a big alarm bell for the field back in 1984. Ivan Kostov claimed to me that the problem doesn’t resolve in the critical dimension either. If the bosonic string doesn’t exist because summing the configurations of a liquid membrane always gives you singular branching surfaces, how can one be sure the same affliction doesn’t obliterate superstrings?

41. **Scott Caveny**
November 5, 2024

The production of approximately 1000 papers per year for the last 30 years is definitely worrisome: Search on \`String Theory\` at INSPIRE shows the 1984 \`first revolution\` with persistent production around 1000 papers per year since about 1996 (the \`second revolution\`)

https://inspirehep.net/literature?sort=mostrecent&size=25&page=1&q=String%20Theory

In an attempt to understand more about this persistence I came across Barry Bradlyn's analysis from 2009 (not sure if it was ever discussed on this blog as it seems relevant to the conversation)

https://web.mit.edu/demoscience/StringTheory/

Unfortunately, Bradlyn's analysis is now 15 years old. It might be interesting to learn what an update of that analysis demonstrates.

But assuming that a career is about 40 – 45 years, the persistence of this intensity of production over 30 years suggests the possibility that the field of String Theory research escapes Thomas Kuhn's mechanism of paradigms shifting as workers literally die off . The generation `born` in 1984 should be retiring now. If ST is a Kuhnian paradigm, then production should be dying off as the first generation retires. But that is not what seems to be happening.

`Echo chamber` effects tend to produce persistence but more practically funding produces root cause persistence. The feedback loop between funding and echo chamber effects is then a possible mechanism for the escape of ST from the natural history of a scientific paradigm.

42. **Mitchell Porter**
November 10, 2024

Matthew Foster,

I tried to find the result that you're talking about. These two 1985 papers by Ambjorn et al are still being cited:

https://inspirehep.net/literature/212827

https://inspirehep.net/literature/221042

In the second paper they explore what they call a regularization of the Polyakov action, which may avoid being dominated by the branched polymers for dimensions between 2 and 30.

I can't say if this work ultimately converged with nonperturbative studies of string path integrals within string theory; most of its current citations are in topics like "2D Liouville quantum gravity".

43. **Matthew Foster**
November 10, 2024

Hi Mitchell

The status is also reviewed here,
https://arxiv.org/abs/1310.8552

I actually became aware of this stuff as a cm theorist, due to applications of Liouville CFT to various condensed matter problems (although no results were directly obtained from LCFT as far as I can tell, and instead extracted from mappings to simplified glass-transition models).

I haven't dug too deeply here but spent some time discussing with Ivan Kostov at a conference. He said that many attempts had been made to tame the branching polymer solution (by adding higher-order terms to the N-G action), but these "irrelevant" operators failed to tame the IR behavior. It's a quite interesting statistical mechanics result, that the attempt to sum over "liquid" membranes becomes dominated by singular polymer configurations.

I can imagine various ways around this for the usual string program–these aren't superstrings, Minkowski vs Euclidean, etc. Maybe AdS/CFT allows one to somehow claim that these problems go away in the non-perturbative limit, since the corresponding boundary SUSY CFT is weakly coupled and one can do calculations.

Then maybe it's just dS vs AdS all over again?

44. **Andy Colombo**
November 20, 2024

Susskind is eighty-four, has a solid reputation and position, so my perception is that he can afford to say the "truth" on record, eventually. However, he chose his words very carefully, so as not to admit that an actual change of framework is needed. «We live in the wrong kind of world to be described by string theory» is, in my view, a masterpiece of linguistic manipulation, where string theory does not get to be associated with the adjective "wrong": the world itself does. «We need to start over» sounds more like "di Lampedusa strategy" to me: keep working on the same things, while pretending that an actual self-correction has taken place. Kudos to Curt Jaimungal for pointing out that the "consensus" is biased, for showing that the leaders of the "consensus" do not actually know what the colleagues outside of said framework are working on, and for respectfully keeping the whole interview on track: that was no small feat.

Living in a Post-truth World

November 6, 2024
Categories: Uncategorized

I grew up in the 1960s and 70s, at a time when fundamental physics was making huge dramatic progress and Western democracies were changing in equally dramatic ways, mostly for the better. It truly did seem that the Age of Aquarius was upon us, and that human societies were on a consistent route to progress, however uneven. By the late 1970s and early 1980s things had started to change, but that humanity and my chosen field of science were sooner or later moving forward still seemed self-evident.

By the late 1990s the situation started getting more disturbing. The likes of Newt Gingrich started taking over the Republican party, with a highly successful propaganda arm called Fox News running 24 hours a day, pushing lies about the Democrats, especially the Clintons (remember Whitewater?). For some mysterious reason, even the New York Times joined in. In theoretical physics, proponents of a failed theory dominated the subject, putting out endless propaganda to the public such as Michio Kaku's Hyperspace.

Around this time I started spending a lot of time trying to understand how these things could be happening. If someone is saying obviously untrue things, logically there are only two possibilities: they're ignorant and believe what they're saying, or they're dishonest, know very well that they are lying. Watching this kind of thing for many years, I started to realize that a better way of thinking about what was going on is that for many people (mathematicians being somewhat of an exception) the issue of truth just isn't very relevant. Newt Gingrich and Michio Kaku likely weren't thinking at all about whether what they were saying was true, they were thinking about what would get votes, sell books, or otherwise further their goals in life. Gingrich was doing what he was doing to save the republic, Kaku to pursue the dreams of Einstein, but both had enthusiastically entered a post-truth environment.

Over the last decade or two, things have gotten much, much worse. Those with a lot of influence in fundamental theoretical physics have driven the field to intellectual collapse by continuing to heavily promote failed ideas. The scientific method is based on abandoning failed ideas and moving on to better ones. As an undergraduate at Harvard I watched Glashow, Coleman, Weinberg, Witten and others quickly abandoning that which didn't work and moving on to impressive new ideas, with more of the same at Princeton during my graduate years. These days Harvard Physics features a group of people devoted mainly to propping up the failed string theory program (Vafa with the "swampland", Strominger with "A+++" and Jafferis with the wormhole publicity stunt). The situation at the IAS/Princeton isn't a lot better.

On the American democracy front, the Trump phenomenon embodies post-truth in its purest form, with the full triumph now of a movement devoted to saying whatever will get them to power, with less than no interest in whether any of it is true. I've spent a lot of time trying to understand why voters in the US voted the way they did in this latest and recent elections. Taking a look at last night's exit polling, the answer is pretty simple. Rich and poor voted much the same way, but those with the least education voted for Trump by a 28% margin, those with the most education voted for Harris by a 21% margin. The polite term for the first group seems to be "low information voters", but what's going on is that education is exactly what gives you the tools to look for the truth and not get taken in by lies.

The situation has gotten dramatically worse in recent years, as people get their information from social media, with the rise of powerful algorithms designed to generate outrage and "engagement" (sometimes designed and funded by bad actors). These send even some of the smartest people around deep into rabbit holes of lies.

So, given all this, how does one live a fulfilling life in a post-truth world? I'm 67 years old, now see little chance I'll be around to see a return to the sort of world I once knew where what was true mattered. On the citizen in a democracy front, over the last few days I've adopted a new policy. When I'm reading anything, at the word "Trump" I stop and move on to something else. Other terms will get added to that algorithm as needed. What's going on is all too clear, there's nothing I can do about it, and I need to stop wasting time and energy thinking about it more. I've deleted the Twitter account I was using (@peterwoit, not the @notevenwrong blog post announcement account) and won't anymore waste time in that sewer. I'll miss stringking42069, but one has to make sacrifices.

On the theoretical physics front, I'll give up wasting time paying attention to what string theorists are up to, and try to concentrate on more worthwhile intellectual activities. The blog will continue though, since it's one of the main positive things I can do to make a small dent in the post-truth information environment. I've always benefited greatly from the many readers who write to me to tell me about things I may not have seen. Keep those cards and letters coming, especially since I'll be spending less time looking for something new on the physics side of many of the usual topics I've covered.

Due to massive increases in the volume and sophistication of trolling, blog comments are now all moderated. If you want to argue that it's all the Democrats fault (yes, I know that they have their own post-truth problem with identity politics), or that theoretical physics is doing just fine, please go away. If you have an insightful and constructive suggestion about how to live in the post-truth world, I'm willing to listen.

Update: Violated my own new policy by reading the following two analyses of where we are, which are better informed than my own:

https://newrepublic.com/post/188197/trump-media-information-landscape-fox
https://www.inquirer.com/opinion/commentary/trust-mainstream-media-2024-election-20241110.html

Now shutting off comments and attempting to stick to more productive activity.

Comments

1. **Eric Weinstein**
   November 6, 2024

   It is like being in open water with no land in sight.

   I think your book on symmetry and quantum theory is one of the best things to happen in this world you describe. I look forward to recommending it to, and buying it for, my son.

   I don't fully agree with your conclusions here, but I understand them, and I respect them. Something began vanishing when we were young men (I am 8 years younger than you). Not Even Wrong is almost always the first thing I check when I reach the internet. Don't abandon it. It is a sandbar to many. Courage, my friend.

2. **John Hessler**
   November 6, 2024

   Peter—all I can say is we love you— always a commentary that goes to the core.

3. **Peter Woit**
   November 6, 2024

   Hi Eric,
   Thanks, although one problem with teaching the course that book is based on again this semester is that I'm very aware of problems with it and how it could be improved,

more so each time I teach the course. One more positive project I should work on is a rewrite.

4. **Anonyrat**
   November 6, 2024

   In “Microbe Hunters”, Paul De Kruif has these words from Louis Pasteur:

   “…..Do not let yourselves be tainted by a deprecating and barren skepticism, do not let yourselves be discouraged by the sadness of certain hours which pass over nations. Live in the serene peace of laboratories and libraries. Say to yourselves first: What have I done for my instruction? and as you gradually advance, What have I done for my country? until the time comes when you may have the immense happiness of thinking that you have contributed in some way to the progress and good of humanity….”

   If that seems far-fetched, I quote you Vaclav Havel:

   “Hope is definitely not the same thing as optimism. It is not the conviction that something will turn out well, but certainty that something makes sense regardless of how it turns out.”

   I interpret this as doing the right thing is always meaningful no matter how futile it may seem.

   IMO, you’re doing the right things.

5. **CWJ**
   November 6, 2024

   Amen, brother.

   I would like to point out that there is plenty of theoretical physics outside of particle physics–condensed matter, nuclear physics (my own field), astrophysics, biophysics… I love this blog, but am disappointed when physics is so narrowed to a single subfield.

   And recently I was at a nuclear physics conference, the fall APS conference, which was full of excited and energized young people, talking about their research and their schooling and networking with each other. There was a lot of positive and productive energy there, and it gave me considerable hope for the future.

   (Though I do worry terribly about the prospects for science funding in the immediate future.)

6. **Marty**
   November 6, 2024

   I completely agree with you — preserving sanity increasingly requires significant filtering. But too much filtering can mean losing touch with the world as it is, so where does one draw the line?

   FWIW (probably not a whole lot, given the current state of things), these are a few of the things I’ve done.

   1. We already have enough societal division, and I don’t want to contribute to it. I’ve found most people who I suspect strongly disagree with me on important political matters (e.g., whether Trump is mentally or dispositionally suitable for the Presidency) can be quite pleasant, humorous and even reasonable outside of politics. So I try to stay away from hot-topic political discussions unless I know them well, and regardless, I try not to define people by their political views (no matter how much I disagree). It’s sometimes really tough to do.

   2. I try to keep in mind that reasonable people can disagree on important issues, even after a lot of though and with the same background information, when they start out with different premises. And since one’s premises are not entirely objective — they are influenced by environment while growing up and “coming of age,” what we have been exposed to (or sheltered from), etc. — I don’t think it is fair or even morally justified to judge people because of their views if their views are honestly held and not purely self-serving. Some people just think some things are more important than other things; I know I do.

   3. I also try to remember that a lot of people just aren’t well equipped to think critically about what they hear, especially when it would require disagreeing with their friends and family and risk being pushed “outside” their social group. How much of that is their fault; to what degree are they responsible for their gullibility and willingness to embrace lies when they support what they want to believe? The answer isn’t clear to me; a lot of it seems like human nature combined with inadequate training in critical thinking. Are these flaws of human nature that will make non-authoritarian governments ultimately fail, or at least the variety of them that allows equal participation by all? I sure hope not.

   4. I stay away from social media content and news sources that are strongly biased in either direction, and *especially* avoid commenting on them. If someone sends me a link to an analysis that doesn’t look well thought out, I won’t read it. And in my experience it’s almost always (but not literally always) pointless to try to change a person’s views when they already have things All Figured Out. But you already know that much, much better than me I’m sure.

   That’s probably an already-too-long enough list from me.

   A sad part of the whole exercise we have just witnessed is that all the economy-related things people are angry about — inflation, interest rates, debt, etc. — will almost certainly become somewhat worse under a Trump administration than a Harris one. That on top of indifference to climate change and power vacuum/instability in the world that authoritarians can freely exploit…

7. **Peter Woit**
   November 6, 2024

   Marty.
   My problem isn’t knowing how to deal with people who think Trump is not a liar. They’re few and far between where I live and work. Problem is, how to live comfortably in a society where truth doesn’t matter, one that is becoming more and more degraded with each passing year?

8. **Paolo Bertozzini**
   November 6, 2024

   Dear Peter,

   yes, something has been going very wrong at some point in the ’70s-’80s, although the roots of the problem probably predate its visibility (also in theoretical physics).

   I do understand and share the disillusionment … often I even wonder if there has ever been a meaning for those “values” of the ’60s that we see now so irremediably compromised, or if it was all just propaganda as well. Paying less attention to the “annoying material” is for sure of great help (and I am using this strategy too) … but even more effective is to increase the communication with those few (?) that still have some constructive attitude and understand the current dysfunctional situation.

Your blog is of extreme relevance as a point of reference and information exactly for this kind of researchers in mathematical physics … and personally I am always looking to see further activity here … especially about the important ideas on Euclidean unification that I follow with great interest … hence my strongest encouragement to continue the good work here.

9. **Diogenes**
November 6, 2024

The same pathologies in academia that enabled string theory to survive have also enabled a set of campus born political ideas to survive for about the same length of time that are now seriously outdated. What you saw last night was a rejection of many of those ideas, mixed up with a lot of other toxic noise. Given your insights on the sociology of string theory survival perhaps use those insights to analyze that political phenomena as well.

10. **Garrett Lisi**
November 6, 2024

Truth always wins in science — that's pretty much tautological — it just often takes a distressingly long time, with much wasted time and anguish before it arrives. This blog has been a bright beacon of sanity above a turbulent sea of misguided high-energy physics ideas and publicity. I deeply hope you will not only get to continue to document the fall of string theory, but get to describe the multitude of ideas that come next — maybe even one that turns out to be true.

11. **Sabine**
November 7, 2024

I think there are many people who share your feelings and I guess you won't be surprised that I have had similar thoughts. You probably don't hear much about what is going on in Europe but the major difference, it seems, is that the Europeans aren't as loud, not that they are any better.

I think we could be more deliberate about community building. I know there are some communities among readers of some blogs or other outlets/newsletters which are doing well in the sense of rational discourse, but it would be worth collecting them. And, yes, I mean that as an exclusive community because, as we have all noticed I guess, everything else gets overrun by trolls eventually.

I have talked about this many times before with other people and nothing ever came out of it, so I'm not holding my breath, but maybe an academic underground of sorts is what the world needs at this point.

12. **Enno**
November 7, 2024

Isn't this a conquest of postmodernism, Foucault (power replacing truth), Derrida (applying this to language), Butler (to sex), … to dispense with the rather Christian concept of a universal truth? Then formal (school and academic) education, forms your mind rather to the worldview of the ruling class. Here we are.

13. **Ahania**
November 7, 2024

Book recommendation re this topic: Doctor Faustus by Thomas Mann.

14. **Paul Stevenson**
November 7, 2024

A lot in your post resonates, and I ditched Twitter earlier this year too and miss parts of it. Glad to read, though, that the blog wil continue

15. **Robert Smart**
November 7, 2024

Humans love to form groups, and one of our favourites is "We are the group that believes X". Think about it: this really works best when X is something that no reasonable person could believe. If X were actually verifiably true then you'd get all sorts of riff raff claiming to be in your group.
See the first minute of https://youtu.be/0tQTqOar7yc?si=GnE88cY5FwxoTKmR where Ren Zhengfei, the founder of Huawei, explains how self criticism is America's secret strength. Most scientists and mathematicians are so polite that we never find out who they think is wrong, let alone who they think are frauds. The world needs more like you and Sabine Hossenfelder. To get more scientists involved in the struggle for truth we need some mechanism which maintains anonymity until critiques are blessed by a significant group. Which doesn't mean such groups will always be right, but at least the conflict will be out in the open.

16. **Joseph Conlon**
November 7, 2024

I'm just going to throw in some historical perspective.

Isaac Newton was born at the start of the English civil war, the king was executed when he was 6 years old, and the country was extraordinarily turbulent politically until his 20s.

The generation who discovered quantum mechanics grew up during the first World War, then had the German civil war (Heisenberg had to sneak through the lines at night to get food for his family).

High levels of political turbulence is not uncorrelated with throwing up scientists and science of the absolute highest quality.

17. **Peter Woit**
November 7, 2024

Diogenes/Enno,
My reference to post-truth problems on the left was largely to excesses at universities that those who work there are well aware of. But, those have nothing to do with the post-truth problems in physics or American democracy. The identity politics excesses were much worse in 2020, when Biden narrowly won. Foucault might have something interesting to say about the triumph of post-truth politics, but it's not his fault.

Sorry, no more about this. It's another waste of time.

18. **Alex**
November 7, 2024

I really enjoy seeing someone talk about the amount of disinformation out there, and why people are willing to believe whatever comes their way as long as it suits them. I am a young graduate student in theoretical physics at the moment and I want to comment that I would conjecture that many of us who even think about studying string theory is because we want a good post-doc position somewhere.

Admittedly, I took a string theory course, and I can tell you I only took it to prepare myself, if I must be put into a position, to do research in strings. I doubt anyone in that course thought what we were studying was a deep fundamental truth, and that we were all just trying to either (a) prepare for research in strings (sadly for a post-doc) or (b) pretend we were mathematicians. Of course, there were those also who wanted to simply learn what was string theory (in which typing that out sounds ironic to me), and then never wanted to look at it again.

19. **Attendee**
November 7, 2024

You are being a bit too quick in believing that education would somehow help people get to the truth. After all, our string theory colleagues are some of the most educated people in the planet and they have no problem following whatever comes out of Harvard and Princeton. Indeed, if Physics PhDs can look you in the eye and say that they really believe that wormholes form when you entangle spins simply because Maldacena and Susskind say so, why should you be so surprised that people get taken in by demagogues?

At the end of the day, people are tribal – it is far easier to simply follow rather than to question. Very few people do what you do – which is to publicly air criticism. This is because people then worry about having to spend time defending the criticism and then dealing with the potential blowback. They secretly hope that the nonsense they are criticizing will die on its own. But, String Theory, Trumpism and Left-wing Identity Politics all show that this is not the case – bullshit doesn't die without a fight. It grows. The only way to end it is to actively fight it and I am doing it in my own way.

I have personally learned a lot from your blog – largely in terms of keeping me current on various physics news and updates on mathematics. So, thank you!

20. **Matt+Grayson**
November 7, 2024

When Congress changed the rules on foreign ownership of news organizations – opening the door for Murdoch & Co, the writing was on the wall. As you said, post-truth gained a huge vector (the infection kind) in FOX. Citizens United ended the chance of meaningful opposition.

I've asked historians for examples of cultures who have come back from our current state – a major power in thrall to a demagogue – without massive bloodshed, and I've yet to hear of one. In 2016, I felt that the only thing that will matter is, when the tanks surround the White House, will the guns be pointing in or out. In 2020, the Joint Chiefs supported an orderly transition of power. Next time? Who knows…

21. **Patrick M OKeefe**
November 7, 2024

"Watching this kind of thing for many years, I started to realize that a better way of thinking about what was going on is that for many people (mathematicians being somewhat of an exception) the issue of truth just isn't very relevant. Newt Gingrich and Michio Kaku likely weren't thinking at all about whether what they were saying was true, they were think"ing about what would get votes, sell books, or otherwise further their goals in life."
"On Bullshit" (2005) by the Princeton philosopher Harry G. Frankfurt is a witty and insightful exploration of this theme.

22. **Ulysses**
November 7, 2024

I have one sad data point to add towards the claim that level of education does not matter in how one's brain can be reorganized to operate within a post-truth framework, which seems to be strongly correlated with exposure to the social media brain virus. Both my siblings are also highly educated (PhDs + postdocs in a hard science + scientific careers). Both have disappeared in deep rabbit holes of lies and are of one mind. The inability to have a basic reasoned discourse with them is shocking to me.

The question that I really would like to have an answer to is: What makes me and others apparently immune? Presumably a similar level of exposure to the virus did not lead to the same outcome in me. Is there something that makes one's brain naturally resistant? Can such an ability be cultivated? Or is it innate? I am not a psychologist, but I suspect there must be a correlation with traits that are found in addictive personalities and brain changes that result from drug addiction (or religious conviction, which I consider being basically the same thing). There is no other way that I can explain the seemingly unbreakable attachment that manifests.

23. **Andreas Eder**
November 7, 2024

I'm 62 and I also have the feeling that things gradually started to go wrong over the last 30 years. Maybe it is a sign of age, but I certainly lost the positive outlook on the future.

@Enno: truth certainly is not a christian concept, that would rather be universal belief. I think it owes much more to the ancient greeks.

24. **Steven Bratman**
November 7, 2024

I have followed you for years, and appreciated your scientific perspective greatly. But I would like to offer a humanistic perspective: Humans only sometimes value truth as a virtue in itself. It comes and goes. We are not entering a post-truth world because we were never in a world profoundly devoted to truth. It's a constant struggle. Like ice age glaciers, there are simultaneous advances and retreats in different places. Optimism is at root an attitude, not a response to particular events. It is especially important in adverse circumstances, which occur frequently in human life. Despair is an indulgence, a humanly understandable one, but not useful to dwell in more than briefly. Others have faced and are facing great challenges, and so can we.

25. **Andrew M**
November 7, 2024

I'm not willing to throw in the towel on truth yet.

I think this pervading feeling of "post-truth" is part of the reason so many people are attracted to the "abc conjecture" story. How can we arrive at truth, when ultimately, for practical reasons of time and ability, we have to rely on consensus at times? How do we recognize if we are stuck in a Kyoto bubble? It really says something when a field rooted in logic itself struggled with the straying from embedded norms for discussing work. It just really struck a chord.

"Appeal to authority" is not a correct way to search truth, but without using consensus as a proxy, it would be near impossible to usefully share knowledge and progress individually or as a group / field / society. It seems, as usual, the answer is not at either end of the spectrum, but in messy nuanced moderation.

Returning to the topic at hand, yes, we probably should not waste as much time engaging with things we cannot individually change. But if none of us ever engage, nothing will ever change. Moderation is sadly almost always the answer.

26. **Peter Shor**
November 7, 2024

Attendee:

You say:

> Indeed, if Physics PhDs can look you in the eye and say that they really believe that wormholes form when you entangle spins simply because Maldacena and Susskind say so, why should you be so surprised that people get taken in by demagogues?

Do you know whether they actually believe this, or whether they're just saying this so that their job prospects aren't torpedoed by Maldacena, Susskind, and their buddies? Either way, it's a very sad state of affairs.

27. **Michael Gogins**
November 7, 2024

Thank you for this post.

In history, people who had to live in, shall we say, a-truthful societies have resorted to various strategies, among them:

(1) Monasteries such as Lindisfarne or St. Gall's.

(2) Friendships of private conversations, where friends were sometimes entrusted with manuscripts that have to be preserved from the secret police, well known now from the Stalinist USSR.

(3) Where officially supported jobs are not to be had, working on one's own time.

(4) Identifying those in power who are secretly sympathetic to a search for truth.

(5) Living and working far from the centers of power.

(6) Propagating real ideas in the form of satires and amusements.

(7) Being officially employed in one capacity, but actually doing one's most important work in another capacity — does this describe you?

28. **Peter Woit**
November 7, 2024

Michael Gogins,
The problem isn't that telling the truth is suppressed or that truth-tellers such as myself experience terrible retribution. I'm doing just fine. My non-tenure track faculty position nicely encompasses multiple duties, some of which are closely related to my research work (teaching advanced courses has been very helpful). The people suffering under the post-truth regime in physics are the theorists forced to think about the swampland. Who knows what will happen with our new post-truth government run by cronies of a deranged New York City real estate developer, but I'm betting that those who will suffer will not be well-off New Yorkers with graduate degrees but the people he scammed into voting for him.

The problem I'm dealing with is a different one, having to change the story I've always told myself about the importance of trying to understand the latest theoretical physics research and understand what is going on in our society, in order to participate in the progress of humankind. Now that those things have turned to post-truth and humanity seems to be reversing gears to ever more stupidity and ever more tribalism, I need to find a different story.

29. **Attendee**
November 7, 2024

Peter Shor,

I think some of them know that it is all nonsense but simply play along to get academic positions. But my sense is that a significant number of them genuinely believe it's true – they mumble a bunch of "algebra" about gravitational path integrals and believe that the result has been mathematically established. The "algebra" is a joke since all they do is write down a bunch of ill defined expressions and pick terms that they want. Astonishingly, it does not occur to them that the sheer ridiculous nature of the result should make them question the "algebra".

What can I say – this episode certainly made me question academia in general. In physics, experiment has provided a wonderful way to robustly establish the truth. We have now empirically seen a branch of physics go down a rabbit hole since this branch is explicitly uninterested in any kind of experiment. This has been lead by the so called "elite" institutions. Given this, what am I supposed to make of academic claims from other disciplines where experiments are practically hard to do?

30. **Anonyrat**
November 7, 2024

Whether or not Trump won, the American public would still have a huge proportion of post-truth people to contend with, between 40% to 50%. This problem was not going away with a Democratic win. This problem is beyond the scope of an individual and IMO, beyond the scope of this blog. It is like the seawaters lapping at your doorstep due to the sea rise from climate change.

Nothing happened on November 5th that suddenly shifted the situation in science or academia. You've been doing the right things, so keep fighting the good fight. We still don't have Lysenkoism, and these institutions still can recover, and maybe even be a bastion in the problem that I have deemed to be out-of-scope.

31. **B'Rat**
November 7, 2024

Italy has the dubious honor of leading by example when it comes to terrible statesmen: Mussolini inspired Hitler, and Trump is, in many ways, a repackaged Silvio Berlusconi. As an Italian, I think I can offer my 2 cents on why not engaging with Trump might actually be the right decision—and possibly even a solution.

From his debut on the right in 1994 to the crisis of 2012, the entrepreneur-turned-politician Berlusconi dominated the Italian scene, whether in power or not (and most of the time, he was). He was more con man than political leader—crass, populist, and self-serving. His promises were often blatant nonsense, and, as the owner of a large portion of Italy's media, he turned it into a propaganda machine. He made short work of his opponents, except for one timid, center-left economics professor, Romano Prodi, who beat him… **TWICE**. Thoughtful, meticulous, and unshakably calm, Prodi projected quiet integrity and consistency. His lack of charisma was legendary: he was so reliably dull that even Berlusconi's exemplary skill at painting opponents as dangerous communists couldn't stick. While others on the left kept their megaphones blaring denouncing Berlusconi as a fascist autocrat (and thus often committed themselves to obvious or perceived exaggerations), Prodi didn't take the bait. Politely outlasting everyone's theatrics with a warm smile and a stack of carefully prepared notes, he patiently explained the finer points of his proposals and appealed to reason, earning a reputation as a competent and reliable alternative to his flashy rival—a reputation his anti-Berlusconi colleagues struggled to achieve. (For the record, both of Prodi's terms ended when he was backstabbed by the usual coalition infighting on the left, which never capitalized on his strengths and is mostly still repeating the same mistakes with the current crop of politicians on the right.)

The moral of the story? I think that even at a personal level we shouldn't tunnel-vision ourselves in opposing the worst of the "other side." Instead, we should apply ourselves to building and proposing another model.

32. **D.H.**
November 8, 2024

I think it's essential to take note of the "education by race" statistics along with the plain education ones. It's specifically white non-college graduates who went hard for Trump. Non-white voters went overwhelmingly Democratic with virtually no difference between education levels. White college graduates, while decisively Democratic, landed at essentially a midpoint between the aforementioned groups.

Another thing perhaps worth mentioning: a friend of mine doorknocked for Harris in Minnesota, speaking with (in his estimation) 150+ people, specifically union member households. Among other things, he found it notable that in his experience pretty much 0% of people–nobody, not even strong Trump supporters–actually believed any specific claims Trump makes. Basically, they know he speaks in meaningless word salad, but nonetheless believe that he/the MAGA movement are nebulously, somehow, going to return them to a world that felt more normal and comfortable for them. Obviously education still bears upon this to some degree, but perhaps not along the particular lines that we might instinctively expect.

With no disrespect intended, I'd tentatively suggest that a perspective from within academia may naturally tend to overemphasize the intellectual element of our political situation and its "post-truth" ideological manifestation (as well as encouraging a rather skewed view on "identity politics" through the demented and hypocritical version that has taken strong root in the soil of university administration). More specifically, I strongly believe–and I think this exit polling generally evidences–that the fundamental problems are to be found in this society's as-yet unresolved stew of white supremacist, sexist, and otherwise bigoted attitudes which can be exploited to compel ideological allegiance to an obviously-doddering freak whose words you don't even believe.

As to how to live in this catastrophically post-truth world, I personally believe that for those in relatively stable positions within academia or government bureaucracies in America (I work in one of the latter), the immediate imperative is to think seriously about our colleagues–particularly our junior colleagues and/or students, and particularly those who are most likely at risk from any rapid changes in federal policies come January (e.g., immigrants/temporary residents, anyone who's "out" as transgender, etc.)–and consider how we might create countervailing or insulating structures/expectations within our own institutions, and how we might offer meaningful material support to those who might, regrettably, come to need it. This may seem different from what you're actually asking about here, but I'd argue that it's of primary and overriding importance in the near term if we want any chance of defending and growing institutions/communities committed to free and rigorous inquiry.

33. **Giovanni Ronchi**
November 8, 2024

Dear Professor Woit,

Thank you for your work on this blog, your books and your interviews elsewhere, it's really appreciated.
I hope you'll continue with the same energy as before

34. **John Horgan**
November 8, 2024

Peter, thanks for this. Your thoughtfulness and decency make me feel better. I still believe truth will prevail, as will justice, haltingly, messily. I still cling to my crazy belief that war will end in my lifetime! But yeah, for now, I'm going to focus on doing what I love, with people for whom I care. I have no time for anger or despair. John

35. **Steven**
November 8, 2024

Please check out the polling that shows Republican voters were *far* more wrong on factual questions than Democratic voters. This is relevant.

36. **Peter Shor**
November 8, 2024

Attendee:

I think a similar phenomenon may have occurred in artificial intelligence. There have been a number of experiments showing that the models of neurons used in neural nets are much too simplistic to actually be how the brain works. And yet computer scientists studying neural nets completely ignored these experiments and proceeded with their research undeterred.

However, using these neural nets seems to have experimentally produced machines with remarkable capabilities, so maybe there are enough similarities between the brain and the neural net model for the machines to work anyway. Or maybe the brain and neural nets work in completely different ways, but neural nets still work to produce useful programs.

The difference between neural nets and string theory may be that the AI researchers could test whether their neural nets were doing interesting things, and eventually figured out how to train them to produce remarkable results. As far as I can tell, the string theory and the It-from-Qubit people don't have *anything* to ground their research in, and thus are straying farther and farther from reality.

37. **Peter Woit**
November 8, 2024

D.H.
I don't think this has to do with racism/sexism/etc. A lot of the same people now voting for Trump voted for Obama and elected him. A majority of white women have consistently voted for Trump. Even more of them voted for him this year (versus 2020) when he was running against a woman. If you look at the election results by county from North Carolina, rural heavily white counties voted overwhelmingly for the Black gubernatorial candidate.

The tribalism at work here is not so much racial or gender based. It is strongly rural versus urban. Yes, most US voters now are just like the string theorists who have entered a post-truth state where they are not interested in what is actually true or not and just go by tribe, following what they hear from the "leaders". They neither know nor care about any relevant facts. Listening to string theorists justify what they are doing, it's generally the same untrue talking points, most of what I've heard from Trump voters asked to explain their vote, it's all Republican party talking points distributed by Fox News and other right-wing outlets.

38. **Alessandro Strumia**
November 8, 2024

Saying that now is post-truth is not true. You can freely write. If you are depressed, you can go walk at the beach. This was not allowed in the old world you miss, when Eastern Europe was under Soviet occupation, and only mainstream media existed. Now is a better world, despite residual problems.

39. **Ryan**
November 8, 2024

Coming from the UK I have no skin in this game, at least not directly yet. It seems like the US is having its Brexit moment. When the UK population voted to leave the EU there was a very similar reaction that everyone who had voted for it was either stupid, uneducated, racist or had been duped by propaganda. While that is certainly the case for some, it certainly wasn't the case for all. Also, similar to what D.H. said, most people didn't really vote for what the campaigners on the leave side were saying but they believed that leaving was the right thing to do.

From the outside, it appears that no one on the democrat side has ever really tried to engage with those who had voted for Trump in the past, they just assumed that they wouldn't be so "stupid" this time. This left them with no real choice but to vote for Trump again. In the end; if people are offered more of the same or a change, they'll sometimes pick change even if it's not necessarily the change they want.

I applaud your decision Peter to focus on what is important to you rather than waste time on that which isn't. If nothing else, something good has come out of what is a bad situation for you.

40. **Attendee**
November 8, 2024

Peter Shor,

I attended an It-from-Qubit conference at Stanford once to see what the fuss was about. They gave out delicious biscuits. I consumed a few and likely gained some weight. That is the only bit that I can say has quantifiably come from this collaboration.

I would say that the senior string theorists are a dejected lot these days. I have been in the field for a while and I can say that their body language visibly lacks the confidence of the past. The brash confidence of we-are-the-smarties-and-the-rest-of-you-are-dumb has been replaced with a sort of sad and dejected look. Thus Susskind's recent meltdown about the state of string theory doesn't surprise me.

But, none of this stops them from publicly lying about the progress in the field and so they continue brainwashing young people into joining it. I was raised Catholic and I have seen that defeated look in an old priest who has figured out that perhaps there isn't an almighty after all but if he admits to it, he loses his job and has to find a new line of work. So they continue preaching – and that system has lasted nearly 2000 years.

41. **Peter Woit**
November 8, 2024

Hi Alessandro,

One of the sadder things to see on Twitter in recent years has been your obsessive defense of the indefensible, as long as it was "anti-woke". You, Bret Weinstein and some others I know clearly joined the post-truth brigade because of mistreatment at the hands of the woke. Hopefully, now you'll feel much better, and your obsessive campaign to deal with the excesses of the woke by turning control of the world over to Trump and Putin ends better than I expect.

42. **Georg**
November 8, 2024

Alessandro Strumia,
Your characterization of traditional media of all flavors and political tendencies as „mainstream", suggesting that they all follow a uniform and presumably „woke" narrative, is a particular embarrassing contribution to the post-truth stream. Equating traditional media of the western world with the Soviet regime is a gross denunciation, especially when your hero is a big fan of the latter's dictatorial successor.

43. **Low Math, Meekly Interacting**
November 8, 2024

My knowledge of history isn't what it should be. However, it does seem roughly accurate to characterize the brief stretch of time from the Enlightenment to now as highly anomalous. Only quite recently has value in "truth" as we humanistically define it been widespread, especially by those in positions of power. It's not implausible that there could inevitably be a regression to the historical mean.

Around last Halloween I heard a summary of a study in some podcast, speculating on why people would choose to watch horror movies. Perhaps they help us cope with the real life horrors we may encounter. Like any form of play, jump-scares serve not only to amuse, but also to prepare.

Dystopian speculative fiction is nothing new, but there seems to be more of it around. When I was young we escaped into Narnia and Middle Earth. My daughter and all her friends love the Hunger Games. They stopped wanting to dress up like Hermione Grainger years ago. Now they want to be Harley Quinn. I think they're getting prepared.

It's hard for us who have lived long enough to witness the decline of the Enlightened West to imagine how such a thing could happen. But it's nothing short of a miracle we were born in a time in which we could grow up to take truth for granted. It was nice while it lasted.

44. **Peter**
November 8, 2024

Alessandro Strumia,

"Now is a better world, despite residual problems."

Not in all respects, and you know it. I studied physics and got my master's in 1983. If you would have told me then what the state of HEP theory would be in 2024, I would have thought you were nuts.

I once interviewed Jean Bricmont (from Sokal & Bricmont fame) and he explained why he thought truth was important: "Because truth is the only thing the poor and oppressed have." In the 1980s you still could voice those sentiments without being laughed out of court as a naive idiot. So yes, it was better then.

Being rather old now, I have the strange feeling that I'm living in a world where truth simply doesn't matter any more. Perhaps civilization has reach that stage in which, in many domains, we don't have to question anymore if statements are true. We can make up things as we wish, and a mythical thing called the "marketplace of ideas" will tell us who is right. Or who gets the money.

Is that a better world? I don't think so.

45. **Rich Townsend**
November 8, 2024

Populists like Trump need an unhappy and alienated population and they then exploit them in a few basic ways. This is not new and I think the periods when debate has been based on truth have actually been in a minority through history, even recent history.

The first way is to tell people what they want to hear even if it's completely impossible. People will believe it, or at least, give it a chance.

The second is to blame outsiders and demonise opponents. Trump's pronouncements on immigrants have been similar to those used by various dictators and racist regimes in the recent past. People, particularly if they have a sense of lost status, and particularly if they lean towards conservatism, are very willing to blame out-groups for society's ills. Likewise, accusing democrats off all kinds of nefarious and untrue activities.

The third, which very much ties in with this, is to present simple solutions to complex problems.

The fourth is to legitimise dark-side thoughts and actions. This can be thrilling – wow, I can say or do these previously forbidden things, I can treat those people as less than human.

It's amazing and horrific to look at some of the worst regimes in the 20th century, on left and right, and see how far people are willing to go when they buy into this stuff. Those regimes have been expert in the use of propaganda and lies, just as Trump and MAGA are.

What's different now is that the propaganda is also being spread peer-to-peer rather than just top-down. But the messages are nothing new.

Naively, when the internet was taking off, I though it would be a way for people to spread the truth and undermine despotic regimes. Sadly it's turning out differently.

46. **John Pohl**
November 8, 2024

Please feel for us working in the field of medicine. The anti-vax movement seems to have done nothing but intensify in a post-truth society. It is absolutely tragic.

47. **Ashley**
November 9, 2024

Dear Professor Woit,

I would add a comment from a cognitive behavioural perspective, even though I've retired and left the profession, the profession is still in me.

As a therapist, I always accepted that what my clients told me was true. The question of therapy then became how to get the client to explore why their truth didn't seem to work?

I'd like to say that all my clients came through with a better understanding of what was true, but the truth is that at best it was 50% of them. Homo sapiens are subject to seeing the world as they are, not the world as it is. We can rationalize this as the Dunning-Kruger effect, with a side-order of Gell-Mann amnesia , or as Robert A Heinlein said, "Man is not a rational animal; he is a rationalizing animal."

So, what I know is that refuting falseness takes work, and then the outcome is subject to confounding variables that psychology and neuroscience has yet to control.

48. **Philip**
November 9, 2024

One of the things I really like about this blog is the engagement of other scientists in the comments. I get a lot of value from multiple other practitioners going back and forth on ideas.

I used to get a lot of this benefit from edge.org comments, and their organized public debates — e.g. the back and forth between Susskind and Smolin on the anthropic principle. Does anyone know why edge ceased being that place that Sabine calls "academic underground"? Are there good alternatives, beyond blogs such as this?

PS I resonate with Peter's thoughts on twitter. In fact, I don't know how people engage in debates on Twitter in the first place. I tried. I couldn't. It's like trying to drink water by putting your head under the Niagara falls.

49. **Peter Woit**
November 9, 2024

Philip,

A big part of the explanation of why US democracy is in a post-truth environment is clearly that most people are now getting their news not from sources optimized for truth, but from sources optimized either for the interest of electing right-wing candidates (Fox News, etc.), or for "engagement" (Twitter, TikTok, etc.). About this, I'm no expert and if you want to discuss the problem, you should find someone knowledgeable.

I'm much more knowledgeable about sources of information about theoretical physics. The trend in the last decade or so seems to have been for there to be fewer blogs on the subject, for reasons I don't completely understand. Some blogs run by string theorists (e.g. Jacques Distler, Clifford Johnson, Lubos Motl…) have gone dormant or rarely used simply because the field is dead so there's nothing to write about. Some theorist blogs have moved to youtube, which is where you need to be now to get a large audience. Why virtually no young theorists are trying to blog about what they find interesting is somewhat of a mystery to me. If people can suggest examples of such, that would be great to know about.

About edge.org. What happened to it is far off-topic and I don't want to start a discussion of it here, but for those unaware, see
https://www.buzzfeednews.com/article/peteraldhous/jeffrey-epstein-john-brockman-edge-foundation

50. **clueless_postdoc**
November 9, 2024

Thanks for writing this. It is indeed very interesting how phenomena found broadly in society (i.e. entering a post-truth world) also manifests itself in the sciences. People like to pretend science happens in a vacuum outside of societal forces, but that simply isn't true. How attitudes in society influence the behaviour in the sciences is no doubt very interesting, and deserves much closer inspection (probably worth half a dozen sociology PhDs).

As for why people start to generate hype, my guess would be that it works. Compared to a group that doesn't generate hype, the one that generates hype gets an added advantage, then eventually everyone starts doing it out of necessity. I would imagine a reasonable portion of the people heavily promoting their works are in private sincere and genuine individuals, but do it because they think everyone is doing it and they need to do it to survive. (this applies to young people, I'd imagine if you do this for 10-20 years you forget what it means to be sincere and genuine anymore).

A way to combat this, I imagine, is when large portions of the community reaches consensus that this kind of hype generation is harmful. Without this consensus, you have nothing to lose by generating hype: at worst some raised eyebrows, but if people buy in you get to win big (plus, if enough talent gets draw in the field may take on a life of its own living solely off of the talent of the young people). With such consensus there is an added social cost/penalty for raising hype, so I'd imagine you'd only do it if you think is worth it.

51. **Quasiparticular**
November 10, 2024

Hi Dr. Woit,

Longtime reader: found the blog in undergrad with your article on 'The Principle,' that pro-geocentrism movie. I was a budding scientific skeptic and your blog was a sort of anchor to reality. It helped prepare me for the road ahead. Poetic that a mathematician is responsible for this: I'll let G.H. Hardy's writing from 'A Mathematician's Apology' explain:

'When the world is mad, a mathematician may find in mathematics an incomparable anodyne. For mathematics is, of all the arts and sciences, the most austere and the most remote, and a mathematician should be of all men the one who can most easily take refuge where, as Bertrand Russell says, "one at least of our nobler impulses can best escape from the dreary exile of the actual world."'

52. **Alessandro+Strumia**
November 10, 2024

Hi Peter and Georg,

Yes, I am happy that, as expected, "the indefensible" was preferred by the majority. Because I think that the woke problem is way bigger than the disinformation problem. Actually, disinformation can spread because "sources optimized for truth" (I think Peter means media and academic institutions) failed and lost the trust of common-sense people, by promoting political nonsense. So toxic that best people from the political side you prefer had to move to the opposite side, leading to the current situation.

What you call post-truth is the rejection of a top-down authoritarian attempt of replacing the old liberalism with a new consensus. In the chaotic rejection process, some unscientific beliefs circulate. That's not optimal, but it's needed.

As next needed step, political power will try fixing the shame of academic institutions that went political and started cancelling science. Your future president announced that universities engaging in censorship will face cuts to their federal funding.

I privately warned about these issues years before I decided that being mistreated is better than keeping silently accepting the forced consensus. The official institutional position of HEP is doubly wrong: everything is perfect and adding quotas will make it better.

53. **Peter Woit**
November 10, 2024

Alessandro,
I think your point of view that having the US overrun by disinformation is worth it to deal with the excesses of the woke in science is a serious mistake. It's the same serious mistake Cumrun Vafa has made in deciding that having fundamental theoretical physics overrun by non-science like the Swampland program is worth it as a tactic in his battle with anti-string forces.

In both cases, signing up for post-truth is a short-term tactic with dangerous long-term consequences. In Vafa's case, the collateral damage to physics and science in general from what he is doing will overwhelm anything helpful to string theory.

In the case of the excesses at universities, from what I see in the US, that situation was already getting better, but for the last year we've already been in a new environment where power has moved from the woke uninterested in the truth to Elise Stefanik and the like, who are even less interested. The new world led by Trump/Putin/Xi will be less woke, but it will be a place profoundly hostile to truths incompatible with their interests.

While you're worrying about a "top-down authoritarian attempt of replacing the old liberalism" with a new woke ideology, I think you should also be concerned about top-down authoritarian attempts to replace the old liberalism with the old fascism. Instead of signing up for that with Trump, why not stick to defending both truth and the old liberalism?

54. **Marshall Eubanks**
November 10, 2024

Here is a suggestion:

Experience and history suggests that you should make sure your affairs are in order, while we are still in the ancien regime. For example, if you need to renew your passport, do it now.

55. **Peter Orland**
November 10, 2024

Peter, I understand if you don't want to engage in argument with the comment-
"As next needed step, political power will try fixing the shame of academic institutions that went political and started cancelling science. Your future president announced that universities engaging in censorship will face cuts to their federal funding." – but this calls out for a response.

This provocative statement is typical of authoritarians. It assumes things not in evidence. Whatever cancelling and censorship is done by the "woke", it is laughably tiny in comparison to that of dictators and oligarchs. In evidence, I offer the enormous damage done to science in 20th Century Italy, Germany, the Soviet Union and other countries by repressive regimes. Once these regimes were fully established, the quality of science in their countries – except for that of a few insulated bright minds – took a nosedive.

Our emerging overlords in the US could not care less about the quality of science – or anything else. They want loyalty, not facts.

56. **GS**
November 10, 2024

I'm not sure that I understand the argument that wokism is so toxic that widespread disinformation and fascism is an acceptable alternative. Even in the most extreme "woke" system, what is the state of the world? Scientific progress is hampered slightly by quota systems that prioritize social equality over merit? A few people lose their jobs or are denied tenure because they speak out against the politics of academia? A handful of trans athletes gain an unfair advantage in sports? How many people's lives would truly be made worse by the injustices of this extreme version of wokeness?

It seems like lunacy to claim that those injustices are so great that they justify deporting/interring millions, using the military to eliminate/silence political opponents, stripping women of their bodily autonomy, abandoning Ukraine and Gaza, gutting public healthcare, rolling back environmental protections, and deregulating crypto/banking markets and tech companies in a manner that will certainly increase wealth inequality.

Because I fear that I have gone off topic from the original post and because I don't want my comment to be condemned to the aether, I'll suggest that the way to live in a post-truth world is to spend more time talking in-person with your neighbors. Have a beer, watch some sports, play a game of chess. Share your point of view and listen to theirs. You won't always agree, and it's almost certain that they won't understand your complaints about the state of theoretical physics, but maybe you'll just care about each other and enjoy being in each other's company. In the end, that's all we can really hope for.

57. **Alessandro Strumia**
November 11, 2024

Peter: as you mix swamp and woke, let me tell a story and next its moral. This year I wanted to present a good paper at a small workshop about modular invariance. Despite that the situation got better, workshops still need a gender quota, who happened to be a US DEI coordinator, who did not allow me, and no physicist dared to defend scientific standards. So I went instead to a big conference where the organisers could write: «this is a purely academic event. It does not promote any political opinion». At this big excellent multidisciplinary conference in China, Vafa gave an interesting talk about swampland. He also said that the universe is about to end and dark matter must be a 5th dimension. Many good string theorist were in the audience, but nobody said that this "conjecture" has obvious exceptions.

The moral is that Woits and Trumps who dare to question the consensus are needed. But they don't need to be fully right, because more free speech allows to correct the correction. We don't need to cancel Woit because he spreads immoral stringphobic disinformation. It's enough to keep free speech. That's why the woke are an important problem.

Peter Orland writes that political power fixing academia is authoritarian. No, it's democracy: the woke annoyed so many people that enough approve politicians acting on how public money is used by academia. This is unusual, because the goal of academia used to be truth, so better not be managed by politicians with different goals. But now part of academia abandoned its mission.

GS: the main reason why the woke problem needs to be fixed is that false academic scholarship lead to bad mayor political decisions. Better avoiding entering in the issues. To hear them, follow Elon Musk. He understands the issues as well as he lands innovative rockets. Unlike progressives lost in resentment, he shows that real progress can be achieved. Monuments can be built instead of vandalised.

58. **Peter Woit**
November 11, 2024

All,
I'm shutting off comments here after this. For one thing, it's too often interfering with my plan of trying to concentrate on productive activity.

Alessandro,
What you write just makes it clear to me that you are looking at everything through the lens of the DEI/woke issue in science. While this explains why you became an obsessive supporter of the much worse awfulness we now live with, I don't think this situation is of general interest, since the fraction of the electorate that cares even slightly about this is vanishingly small.

Why Sabine Hossenfelder is Just Wrong

November 11, 2024
Categories: Quantum Mechanics

Sabine Hossenfelder's latest video argues

1. There's no reason for nature to be pretty (5:00)
2. Working on a theory of everything is a mistake because we don't understand quantum mechanics (8:00).

These are just wrong: nature is both pretty and described by deep mathematics. Furthermore, quantum mechanics can be readily understood in this way.

Actually, the title and first paragraph above are basically just clickbait. Inspired by the class I'm teaching, I wanted to write something to advertise a certain point of view about quantum mechanics, but I figured no one would read it. Picking a fight with her and her 1.5 million subscribers seems like a promising way to deal with that problem. After a while, I'll change the title to something more appropriate like "Representations of Lie algebras and Quantization".

To begin with, it's not often emphasized how classical mechanics (in its Hamiltonian form) is a story about an infinite dimensional Lie algebra. The functions on a phase space $\mathbf R^{2n}$ form a Lie algebra, with Lie bracket the Poisson bracket $\{\cdot,\cdot \}$, which is clearly antisymmetric and satisfies the Jacobi identity. Dirac realized that quantization is just going from the Lie algebra to a unitary representation of it, something that can be done uniquely (Stone-von Neumann) on the nose for the Lie subalgebra of polynomial functions of degree less than or equal to two, but only up to ordering ambiguities for higher degree.

This is both beautiful and easy to understand. As Sabine would say "Read my book" (see chapters 13, 14, and 17 here).

This is canonical quantization, but there's a beautiful general relation between Lie algebras, phase spaces and quantization. For any Lie algebra $\mathfrak g$, take as your phase space the dual of the Lie algebra $\mathfrak g^*$. Functions on this have a Poisson structure, which comes tautologically from defining it on linear functions as just the Lie bracket of the Lie algebra itself (a linear function on $\mathfrak g^*$ is an element of $\mathfrak g$). This is "classical", quantization is given by taking the universal enveloping algebra $U(\mathfrak g)$. So, this much more general story is also beautiful and easy to understand. Lie algebras are generalizations of classical phase spaces, with a corresponding non-commutative algebra as their quantization.

The problem with this is that these have a Poisson structure, but one wants something satisfying a non-degeneracy condition, a symplectic structure. Also, the universal enveloping algebra only becomes an algebra of operators on a complex vector space (the state space) when you choose a representation. The answer to both problems is the orbit method. You pick elements of $\mathfrak g^*$ and look at their orbits ("co-adjoint orbits") under the action of a group G with Lie algebra $\mathfrak g$. On these orbits you have a symplectic structure, so each orbit is a sensible generalized phase space. By the orbit philosophy, these orbits are supposed to each correspond to an irreducible representation under "quantization". Exactly how this works gets very interesting, and, OK, is not at all a simple story.

Comments

1. **Skewered Cigar**
   November 11, 2024

   Can you say exactly what is needed to quantize a given theory? I was told by a physicist that you can never quantize with the Lagrangian alone, which surprised me. I would have assumed that with the Lagrangian alone, one can compute the phase space and then proceed from there.
   In other words, what is the smallest list of ingredients from which the rest of the ingredients and then the quantized theory can be cooked up?

2. **Peter Woit**
   November 11, 2024

   Skewered Cigar,
   Given a Lagrangian, you can get classical equations of motion (Euler-Lagrange), but you can't for every Lagrangian construct a quantum theory. If you try to use canonical methods, you first have to construct a phase space, but in general the Legendre transform won't have the properties you need. In addition, even if you do get that to work, you'll then have a Hamiltonian function that you don't know how to quantize (operator-ordering ambiguities, no sensible state space, etc.)

   Telling students that all you have to do is pick any Lagrangian and use it to define a quantum theory by a path integral is extremely misleading. That doesn't actually work except in special cases.

3. **Sabine**
   November 12, 2024

   Well played, Peter, certainly got me to click.

   I am sure you know this, but just so that no one else gets confused.

   My argument is not that nature is not beautiful. My argument is that there is no reason for nature to conform to any preconceived notions of beauty that humans have. This is very different from saying that nature is not beautiful. Indeed the argument in my book is that we learn what beauty even means from nature. (And there are several historical examples for how this happened in physics.) Problems arise if we do it the other way round: try to find natural laws using a certain notion of (mathematical) beauty.

4. **Kartik Prabhu**
   November 12, 2024

   @Skewered Cigar @Peter Woit

   You can construct a phase space from the Lagrangian. You start with the Lagrangian and vary it (as usual), but don't throw away the boundary term (not as usual). This boundary term is a (pre)symplectic potential, and a second variation gives a conserved (pre)symplectic form on the space of solutions of the theory. You can try this with usual quantum mechanics viewed as a field in 1-dimensions

   The whole "pre" stuff is about whether this symplectic form is degenerate or not; in any gauge theory it will have degenerate directions. However, you can take functions on the phase space which are constant along the degenerate directions, and define a Poisson bracket on such functions. Quantization then be done is the usual way, along with the usual issues of operator ordering for nonlinear functions, e.g. any Hamiltonian of interest.

Of course, all of this is much easier said than done, but in principle the Lagrangian gives you, at least, all the classical structure needed for quantization.

5. **S**
November 12, 2024

"have have" is a repeated word in your post.

Since this is extremely pedantic, I'll also add a relevant question: is "the orbit method" the same thing as the Mackey machine?

6. **Peter Woit**
November 12, 2024

All,
Sorry, the reference to Sabine Hossenfelder's views on beauty was just to get you to click on the post and hope you would read the interesting content here. I'm not about to host a discussion of this topic here now.

7. **Peter Woit**
November 12, 2024

S,
Thanks, fixed.

I don't think there's much relation between the two. I've mainly encountered the "Mackey machine" as a way to understand representations of semi-direct products, don't know if there's an analog in terms of behavior of orbits.

The orbit method was originally developed for nilpotent and solvable Lie groups, where it works well, and there are relations to Mackey's work on induced representations. For the much more challenging issue of semi-simple groups, I don't know of much relation to Mackey.

8. **Peter Woit**
November 12, 2024

Kartik Prabhu,
What you're describing ends up requiring something like the Dirac method of quantization of constrained Hamiltonian dynamics, with all sorts of trouble in consistently imposing constraints.

In any case, what one is doing when one does this is trying to get to exactly the Hamiltonian context I'm writing about, finding a physical phase space identified with R^2n with the standard symplectic structure and a Hamiltonian function on it. At all stages of this, the fact that you can't consistently find operators for all functions on phase space is a significant problem.

9. **Peter Woit**
November 12, 2024

Kartik Prabhu,
Another point to emphasize is that when you go through the constrained dynamics process you often will end up with a physical phase space which is not R^{2n} and cannot be quantized by canonical methods. This happens for some of the simplest interesting QFTs. A good example is Chern-Simons theory in 3d on a Riemann surface. The Lagrangians is very simple but the degeneracy problem means that your physical state space is going to be the space of gauge equivalence classes of flat connections on the Riemann surface. Quantizing this is a completely different story than canonical quantization and requires very different methods.

Trying to instead make sense of the path integral leads to a host of other issues.

10. **Arturo López**
November 12, 2024

I wonder why, if QM is understood, 't Hooft is trying to substitute it with deterministic calculations; why Penrose insists in the unresolved problem of the quantum measurement or why even Steven Weinberg published in his last years some papers introducing non-standard randomness in the wave function. It seems to me that great physicists (I mentioned three Nobel laureates) confess in the end they are not confortable with the current understanding of QM but they dare to speak honestly only when they are old and so respected they are beyond any suspect or reprisals.

11. **Peter Woit**
November 12, 2024

Arturo Lopez,

I've written extensively about this on the blog in the past, see for instance
https://www.math.columbia.edu/~woit/wordpress/?p=10533

I agree that there is a measurement problem, but I see it as the problem of how the classical description of the world emerges out of the quantum description. The quantum formalism in terms of representation theory is simple, beautiful, deep and very very powerful. The idea that you're going to replace that with something much more naive and completely different doesn't seem plausible to me (the Nobel Prize winners who have tried this don't seem to me to have gotten anywhere at all with it, Weinberg I think admitted as much).

I don't want to host another discussion of the measurement problem, happy to discuss the ideas about the relation between QM and representation theory in the posting.

12. **anon**
November 12, 2024

Hi Peter,

Some other comments on quantization which I think fit nicely with the orbit method but are not often discussed.

There is a sense in which the "points" of the "symplectic category" are Lagrangian submanifolds. I learned this from an essay by Alan Weinstein. The idea is that one considers a category in which the objects are symplectic manifolds, and the morphisms between a pair of such manifolds, M and N say, are Lagrangian submanifolds of the product M x N. (This is a reasonable notion of morphism because the graph of a symplectomorphim from M to itself is the same thing as a Lagrangian submanifold in M x M.) In this category, a "point" in M should mean a morphism pt -> M, or equivalently a Lagrangian submanifold in pt x M ~ M.

Now imagine you are a classical physicist and you arrive at this idea that the “points” in a symplectic manifold are really the Lagrangian submanifolds. You might decide that it really isn’t right to consider functions f(p,q) on phase space, but rather that one should choose a foliation by Lagrangian submanifolds and only study things like f(p) or f(q) which are constant on Lagrangian submanifolds. You can almost imagine how following this philosophy could lead a classical physicist to “discover” geometric quantization.

13. **Arnold Neumaier**
November 13, 2024

Peter Woit said:

Kartik Prabhu,
What you’re describing ends up requiring something like the Dirac method of quantization of constrained Hamiltonian dynamics, with all sorts of trouble in consistently imposing constraints.

One can get from a general presymplectic form = closed 2-form (no matter what its origin, in particular, the Lagrangian 2-form works) a Poisson algebra on the associalted Hamiltonian vector fields – except that now the latter do not include all vector fields. See Section 13.2 of my recent book

A. Neumaier and D. Westra, Algebraic Quantum Physics, Vol. 1: Quantum mechanics via Lie algebras, de Gruyter, Berlin 2024.

(or Section 18.1 of my free online lecture notes https://arxiv.org/pdf/0810.1019v2 from 2011). Fixing all Casimirs gives a symplectic manifold, and for certain values of the Casimirs these can be extended by geometric quantization.

Dirac’s method is only needed when one wants to have a more explicit description of the reduced symplectic manifold – since one then has to construct a complete set of generating Casimirs.

14. **Josh**
November 13, 2024

Hi Anon,

Would you expound on this a bit? I believe that the orbit method uses Kahler-polarizations, which don’t fit into the foliaiton context you described. Also, while states are (sometimes) constant along the leaves of a Lagrangian foliation, observables aren’t, so I find the idea that a classical physicist would discover quantization this way to be a reach. I think quantization is much easier to motivate by making the observation (which is never mentioned) that the quantum mechanics of projective space agrees with the classical mechanics of projective space, ie. the quantum and classical equations of motion agree on quantum observables.

15. **anon**
November 13, 2024

Hi Josh,

I realized after reading your comment that I am bit confused about the point you are making so take what I write now with a grain of salt. Akshay Venkatesh has a nice set of lecture notes on geometric quantization and the orbit method. In the last section, he explains the quantization of coadjoint orbits. It can be described using Lagrangian submanifolds, but they are Lagrangians of the complexified tangent bundle of the orbit, not of the orbit itself, as the story I sketched in my earlier comment would suggest. So as you point out, my earlier comment was too simple. That comment was really only aimed at canonical phase spaces. Why in the orbit method do we need to use Lagrangians in the complexified tangent bundle and not the orbit itself? Here is where I get confused but I think the point is that there is a G-action in the orbit story and a foliation by Lagrangians of the orbit itself would not be G-equivariant. I think my earlier comment needs a disclaimer: “if there is a G-action on the symplectic manifolds, you need to do everything in a G-equivariant way and the constructions are more complicated.” Maybe someone who understands this better than me can chime in.

16. **Peter Woit**
November 14, 2024

Josh/anon,

The Venkatesh lecture notes anon refers to are here

https://math.berkeley.edu/~fengt/249C_2017.pdf

This doesn’t address the orbit method in general, for that a place to start is Kirillov’s book
https://bookstore.ams.org/gsm-64/

For the case of semi-simple groups, the story becomes quite intricate, precisely because the representation theory of these groups (in the non-compact case) is similarly intricate, in a way that matches, but not quite. For this, see Vogan’s
https://math.mit.edu/~dav/PCorb.pdf
and a posting here with a link to some of his slides
https://www.math.columbia.edu/~woit/wordpress/?p=631

The relation between the general idea about Lagrangian submanifolds that anon is pointing to and quantization is explained by Segal in lecture 4 of these notes

https://web.math.ucsb.edu/~drm/conferences/ITP99/segal/segal2_n.pdf

This isn’t about the orbit method, it’s about trying to quantize general symplectic manifolds (without the G symmetry of a co-adjoint orbit). It’s not well-known as it should be that there really isn’t a viable way of doing this in general. For the case of cotangent bundles, Segal explains the relation between Lagrangian submanifolds and quantizing by using pseudo-differential operators.

17. **Curious Fish**
November 19, 2024

Are there good references on the Poisson geometry approach to classical mechanics? I am aware of pure math references on this topic, but not of references written for physicists. In particular, are there situations, or families of problems, where the Poisson approach makes a mechanical systems easier to study?

18. **Peter Woit**
November 19, 2024

Curious Fish,

For some answers to your question, see here
https://mathoverflow.net/questions/28532/classical-mechanics-motivation-for-poisson-manifolds

19. **Arnold Neumaier**
November 20, 2024

    Curious Fish,,
    ”are there situations, or families of problems, where the Poisson approach makes a mechanical systems easier to study?”
    Hydromechanics can be put into a Hamiltonian framework on Lie-Poisson algebras, but not on symplectic manifolds. See, e.g.,
    P.J. Morrison, Hamiltonian description of the ideal fluid. Rev. Modern Phys. 70 (1998), 467.

20. **Peter Woit**
November 20, 2024

    Curious Fish,
    This review article just appeared.
    https://arxiv.org/abs/2411.12551

The Impossible Man

November 15, 2024
Categories: Book Reviews

There's a new book out this week, a biography of Roger Penrose by Patchen Barss, with the title The Impossible Man: Roger Penrose and the cost of genius. Penrose is one of the greatest figures in physics and mathematical physics of the second half of the twentieth century, arguably the dominant theorist in the field of general relativity. His work on twistors is the most important new idea about space-time geometry post-Einstein, and I believe it will be studied long after string theory has been finally consigned to the oblivion of failed ideas. His 2004 book The Road to Reality is an unparalleled comprehensive summary of the geometric point of view on fundamental physics, a huge work of genius written to try and convey the deepest ideas around to as many people as possible.

The new biography provides a lot of detail about Penrose's life and work, well beyond what I'd learned over the years from reading his writings and those of others who worked with him. It does a good job of explaining to a wide audience some areas of his work, and how the background he grew up in helped make some of his great achievements possible. From an early age, Penrose was fascinated by geometry, and he became our greatest master at visualizing four dimensional space-time, generating deep insights into the subject. While one can motivate twistor theory in several very different ways, it came to him through such visualization.

Another thing I learned from the book was more of the story of the singularity theorems for which he was awarded the Nobel Prize in 2020. While Hawking often gets more attention for this, it seems that there's a good case that the creative ideas there were more Penrose's, with Hawking much better at getting attention for his work. That, despite having read a great deal about this story over the years, I'd never heard that Penrose saw things this way until reading this book is much to his credit.

In later parts of the book, the author handles well the issue of some of Penrose's more problematic later projects. Experts on cosmology are highly skeptical of his conformal cyclic cosmology ideas, and pretty much everyone thinks his involvement with Stuart Hameroff around questions having to do with consciousness has been misguided.

Penrose played an important role in my life, by suggesting to his publisher that they publish *Not Even Wrong* (for the story of that, see here). While, I haven't been in contact with him for many years, and only have met him in person briefly twice, he seemed to me unassuming and more likely to be friendly and helpful to others than your average academic.

Unfortunately, the book pairs a largely very good discussion of Penrose's scientific career with a very extensive and rather unsympathetic discussion of his personal life. If you read reviews such as the one today in the Wall Street Journal, you'll be told that Penrose's personal story "fits the template" of the genius as "deeply weird", with the book showing that "the cost of genius" is personal sacrifices by those around him.

The huge amount of material included in the book about Penrose's parents, his two long marriages and his relationships with his four children seems to me to paint a picture completely typical of his generation. That an upper-class British man growing up in the 1930s and 1940s would have an emotionally withholding father is not very notable. That a male academic of this period would have a marriage that failed after 20 years is not unusual, nor is having a wife with very valid complaints about giving up her own career and interests to follow her husband around to different positions. None of this has anything to do with Penrose's genius or great accomplishments, beyond the common phenomenon of successful people being too busy and preoccupied to provide enough attention and care to those around them.

The central part of the book is derived from a collection of 1971-76 letters between Penrose and Judith Daniels, a younger woman who had been a childhood friend of his sister. Penrose was unhappy in his marriage, very much in love with Daniels, and saw her as his muse, someone who could appreciate his work. Unfortunately for him, she had a boyfriend and no interest in a sexual relationship or marriage with him. The book goes on for pages and pages quoting these letters and explaining the details of exactly what happened. It's no more interesting than one would expect. One could argue that Penrose did do something rather objectionable to her, trying to get her to read the manuscript of his two volume joint work with Wolfgang Rindler, *Spinors and Space-time*.

The four chapters devoted to this story unfortunately are also the ones covering the time of his great work on twistor theory, which gets somewhat buried amidst the not very dramatic unrequited love drama. This section of the book ends with a dubious attempt to connect the two together:

> He couldn't let go of twistor theory, and he couldn't let go of Judith. In a single letter, he both lamented the impossibility of recreating the magic they once shared and attempted to do exactly that. He wouldn't take no for an answer — from her or from the universe.

For the years 1971-76, this book provides all the detail you could ever want about why Roger Penrose wanted to sleep with Judith Daniels and why she wasn't interested. For the details of the story of one of the great breakthroughs in understanding the geometry of the physical world, we're going to have wait for another book.

Comments

1. **CWJ**
   November 15, 2024

   Thanks for this thoughtful and illuminating review. I had read one other (non-scientist) review and was intrigued. Admittedly, now I am much less interested.

   I heard Penrose speak a couple of times (like 30 or 35 years ago), though that was firmly in the Emperor's New Mind era, which struck me as being based upon very poor argumentation. Even today with students I use it as an example of how someone can be clearly quite brilliant and make enormous contributions, but still be wrong about other things.

2. **Christopher Oakley**
   November 15, 2024

   Peter, you are now making me feel guilty about having enjoyed James Gleick's book about Feynman's private life. Maybe if had known Feynman personally, as I do Penrose*, I would have just felt that his private life was none of his damned business and that he should just leave it alone!
   * = not very well, but well enough to know that he's a nice, polite man.

3. **Peter Woit**
   November 15, 2024

   Hi Chris,
   Thing is, I normally greatly enjoy scientific biographies which include details of the subject's private life, the more outrageous the better. If a biographer wants to tell us that Feynman and Hawking were regulars at strip clubs, that's fine with me. Actually I thought the recent Hawking biography that had a lot about his private life was quite good, see
   https://www.math.columbia.edu/~woit/wordpress/?p=12235

   What I don't like about this particular treatment of Penrose's private life is

1. It appears to me to be a moralistic hit job, making out Penrose to be an unusually flawed human being (rather than the usually flawed kind).
2. It's boring (there's a whole chapter about the big moment when he and Judith both went to Trieste, stayed in the same hotel. She told him she was not interested in a romantic involvement with him and they went sightseeing).

4. **Diogenes**
   November 15, 2024

   The thing that makes great geniuses interesting and their stories signifcantly different from lots of other people is almost never their personal romantic lives. Their upbringings on the other hand are frequently very interesting, and probably relevant.

5. **Christopher Oakley**
   November 15, 2024

   Hi Peter, I know Penrose a bit better than you, but not by much. The main connection is that I am friends with one of his protégés and have spoken to him a few times at this person's parties. Yes, Feynman and Hawking loved the idea that they were the Wild Ones and would have wanted you to know all about their visits to Titty Bars, etc., but Penrose is definitely not that kind of person, and will not relish anyone dredging through his past. Maybe the author thinks that him being a Nobel Laureate, and all, makes him fair game, but I don't think so, and nor do I think it has much bearing on his work in mathematics.
   As an Oxford man myself, I might add to your analysis the fact that Oxford has held Penrose in the highest regard as long as I can remember, and the fact that Twistor Theory has had limited (so far) impact on physics is not held against him. Bear in mind that Theology, Philosophy and Classics are the high status subjects around these parts.

6. **Peter Woit**
   November 15, 2024

   Diogenes,
   This is certainly true in Penrose's case. He grew up in an unusual family, providing him an unusually rich intellectual background while growing up. This is described well in the book.

7. **AUT**
   November 15, 2024

   I think the parts of the book on Penrose's relationship with Daniels and the dissolution of his relationship with his wife are actually very important. They show very clearly how entwined his professional and personal life were and how even someone like Penrose can have a great (for lack of a better word) insecurity over his work. In a better world this kind of material would be completely frivolous, but in reality, when people think about great scientists like Penrose, they come in with a whole set of deeply ingrained ideas about what such a person must be like.

   For example, Penrose going on the Joe Rogan podcast was reported by Barss as a key episode in the ending of his relationship with his second wife. Taken in and of itself, that's completely uninteresting, but I think it's important to understand that when Penrose goes on the Joe Rogan podcast, he's getting as much out of it as Rogan, and arguably more – a massive audience and an open platform for ideas which he thinks have been unfairly dismissed. This kind of dynamic is usually pretty much invisible to (if not outright rejected by) the general public, so I think it's important to have such an open display of it. If even someone like Penrose can have such a gross craving for a certain kind of recognition, I think this can strongly inform how we see much lesser 'pop' scientists who are still often given every benefit of the doubt.

   On the other hand, of course I would love to see greater focus on technical biography and the research development of many parts of his work! Hopefully someone else will write a truly 'scientific' biography of Penrose.

8. **Peter Woit**
   November 15, 2024

   AUT,
   After reading the book, I just don't see how the failure of Penrose's first marriage was much different than the typical way half of such marriages fail. Like many people, he seems to have decided soon after the marriage that it was a mistake, but stayed in it hoping things would change. It's the biographer who seems to be claiming that there was something distinctive about the nature of genius responsible for this, but I'm not seeing it.

   Later in life Penrose became a public figure very much in demand, even if not quite on the Hawking scale. From then on, he had the usual temptations of celebrity to deal with, and one can question his choices (maybe a bad one was agreeing to open his life to this biographer…). I'm not sure what this set of problems has to do with being a genius.

9. **AUT**
   November 15, 2024

   I think that if there's a thesis statement for that part of the book, it's in this paragraph:

   "Joan, Judith, Salley, and Vanessa, each in her own way, made Roger's career possible. They managed his everyday life, freeing him to journey into black holes and tinker with conformal geometry. Beyond offering the praise and encouragement he considered essential to his creative process, they were variously sounding boards and editors, career counsellors, confidantes, and muses, willing or not. Some sacrificed their own careers and put his needs ahead of their own. They offered intellectual, logistical, and emotional support. Joan, whom Lionel treated as a blight on the Penrose mystique, served a crucial role in developing Roger's belief in his own exceptional talents. He used their unhappy marriage as justification for fully embracing the work that established his career and won him the Nobel."

   Aside from whether or not this kind of content is valuable as biography (and I think it is), I think it's very different from saying that there's something about genius which is responsible for various life conditions. If anything it's something more like the converse, that for some people the ability to creatively prosper is built on having a certain kind of environment and a certain kind of psychological and/or material (dis)comfort. It also reminds me of Shing-Tung Yau's autobiography, where I found it very striking that his wife was for some years raising their young children alone on the opposite side of the country, during some of the most productive years of his career.

   I think it's very valuable to understand that such tremendously productive and creative workers – call it genius if you like – often don't exist in a vacuum. It might already be plainly obvious to many of us, but to the broader public I think it's definitely not.

10. **Peter Woit**
    November 16, 2024

    AUT,
    Yes, that's a clear thesis statement. The problem is with the accusation that Penrose achieved what he did by treating the women in his life (and his children) unusually badly. Everyone is reliant upon those close to them for support. Most marriages of Penrose's generation involved a wife willing to forgo having her own career, move because of her husband's job, stay home to raise children and support her husband's career.

It’s fine for a biographer to point out that Penrose, like most men of his generation, was able to succeed because of such an arrangement. What I don’t see is any evidence from the book that there was anything even slightly unusual about Penrose’s story in this regard.

11. **Hubble constant**
November 16, 2024

Interesting, and there is now a NYT review of the book.

Penrose was doubtless one of the great original thinkers at his prime (which spanned decades), and I too found his semi-popular book The Road to Reality impressive. He has had many great hits, some of which Peter mentions above. However his later/recent efforts in cosmology were embarrassing. He nevertheless continued to push his theory (conformal cyclic cosmology), and even “find” evidence of it in the cosmic microwave background (with a collaborator who evidently agreed to do so) despite being told by cosmologists that his theory, and certainly the “finding” in the data, is basically nonsense. I guess in that regard he’s no different from other greats who could not be stopped from doing and claiming such things in later years (e.g. Michael Atiyah claiming he solved the Riemann Hypothesis, which may have been discussed on this blog).

12. **Peter Woit**
November 16, 2024

Hubble constant,

The Atiyah situation was different. In the last few years of his life he was suffering from a mild form of dementia, unable to follow complex arguments or understand what was a valid proof. I didn’t write about it on the blog, it seemed obviously best if everyone would just politely ignore his claims about proofs. It was too bad that some people publicly spread his claims about the Riemann hypothesis.

Penrose is different. Plenty of people with their faculties intact have dubious theories about cosmology and make unsupported claims about them (string cosmology anyone?). He’s been fond of contrarian ideas for a long time. Despite some effort, I never was able to completely understand how CCC was supposed to work.

13. **Andrew Thomas**
November 17, 2024

I briefly met Roger last year. I know that not only is he a brilliant physicist, he’s also a great guy, absolute gold. He told me the Road to Reality was the most difficult book he ever wrote, and he almost didn’t finish it.

I’m actually going to defend some of his work on consciousness, which I think gets a raw deal. Before he met Stuart Hameroff, he wrote The Emperor’s New Mind which remains a brilliant book (got me interested in physics). He was one of the first leading physicists to take an interest in consciousness, which was a brave move at the time.

At the core of the book, he expressed something that we maybe all feel but no major scientist had really said before – that there really is something fundamentally different between how our brains feel – how it feels to be conscious – and the very basic operation of a computer which might superficially be appearing to be doing the same thing as our brain, but is almost certainly not conscious. There’s something there which needs explaining, and Roger was the first major scientist to express that.

14. **CWJ**
November 17, 2024

Andrew Thomas,

I’m afraid I had the exact opposite reaction to The Emperor’s New Mind. First of all, lots of scientists have investigated the phenomenon and problem of consciousness, for a long time. Even just casually, I took an elective course on States of Consciousness in the early 1980s. And already back then people were investigating neural networks as working very differently from standard computers. To claim that Penrose was ‘the first major scientist’ to express that brains work differently from computers is wrong. (I don’t know how you are defining ‘first major scientist’. Brain biologists have know this for a very long time, and even John von Neumann seems to have realized this as far back as 1958.) Penrose seemed to have not bothered in the least to look up any research on consciousness. This doesn’t take away from his brilliant work in mathematical physics. But in the science of consciousness, he was not groundbreaking in the least.

15. **Andrew Thomas**
November 18, 2024

Hi CWJ,
There has of course been plenty of interest in consciousness from philosophers and psychologists for centuries, going back to Descartes. But there was no interest from what we might call the hard scientists until the work of neuroscientist Christof Koch in the early 90s, and others such as Stanislas Dehaene who basically created the modern field. I know the history of this field pretty well. Penrose’s 1989 work predates them. I actually suspect he might have influenced their work and – if so – we should consider him the father of the modern field of consciousness studies.

I don’t know why you mention neural nets – yes, they don’t work like computers but that has nothing to do with consciousness. I was also working with neural nets in the early 90s – my PhD is in that area. There was never the slightest mention of consciousness. Yes, I know that he was obviously not the first to suggest that “brains work differently from computers”, but we’re not talking about structure, we’re talking about consciousness. As far as I am aware, Penrose was the first to consider if computers were conscious (Turing had earlier wondered if they were “intelligent” in the Turing test).

I believe Penrose was a genuine pioneer in this area, and we should give him credit.

16. **Mitchell Porter**
November 18, 2024

Andrew Thomas,

Penrose was hardly the first skeptic of computer consciousness, or of fashionable reductionist theories of mind. He wasn’t even the first physicist to suggest a quantum theory of mind – though his specific ideas are highly original – and I think his theory achieved maximum potency only once he teamed up with the anesthesiologist Stuart Hameroff, who gave it biological concreteness by associating it with microtubules. But no doubt he opened quite a few minds, and gave skeptics of computationalism a new champion.

17. **Peter Woit**
November 18, 2024

All,
Enough about Penrose and consciousness. I don’t want to moderate a discussion of that since I know nothing about it and also don’t want to learn more.

18. **Luca Signorelli**
November 18, 2024

I have a semi-direct connection to RP through a "common friend" (I'm being deliberately vague here), and I'm relatively sure this book's reason for being is both problematic and highly unhelpful in trying to understand Roger's character and personality. I'll try to be as polite as I can – let's say that as far as I know, Roger is not your token gutter press cannon fodder, and so it's not anyone whose life story can be transformed into an instant hit job, the kind of stuff people love to read about a scientist (in case you don't know, people is interested in scientists life only when they're alleged victims, alleged loonies or alleged sleaze – try writing about a scientist whose life is remotely normal without "spicing it up" and any publisher will reject your book proposal in a millisecond.

So, I think the author tried to spice up Roger's life as much as possible. This is relatively commonplace in the world of celebrity biographies. Still, my understanding (and I'm being very cautious here) is that, well, being Roger now quite old, the circumstances surrounding the author's access to Roger's life details bear further scrutiny. I'm not implying anything illegal or such; it's just that Roger may have mistaken the book's scope when he did agree to talk with the author.

So, to sum it up, take the whole "Roger Penrose personal life story" part of the book with a massive pinch of salt. Or at least, that's my understanding.

19. **Peter Woit**
November 19, 2024

Luca Signorelli,
From the author's own description, Penrose (who is 93) has severe vision problems, and lives alone, without anyone to watch out for his interests. It appears to me that in this situation the author took advantage of a vulnerable elderly person.

20. **Low+Math,+Meekly+Interacting**
November 19, 2024

The details could be 100% accurate, and it would still be a disturbing and unseemly choice to publish this book, at least while the subject is still alive. I also find the whole account rather dreary and mundane, and entirely unhelpful in understanding anything of value about what makes Penrose interesting.

I do blame the fascination with people like Feynman for incentivizing such "humanizations" and I have, to some degree, been guilty of contributing my fair share of cash to keeping that cult alive. If Feynman is remembered as a narcissistic, sexist pig along with being a great physicist, well, he contributed as much as anyone in his autobiographical accounts to that cultivated persona. What did Penrose do to encourage dredging up petty embarrassments a half-century old? He's no Feynman, and this book just makes me sad.

21. **Peter Woit**
November 19, 2024

LMMI,
I don't think the character assassination here becomes less problematic once the target passes away.

The last thing I want to do is to get involved in the "Richard Feynman, misogynist who should be canceled or not?" debate. I'm not a fan of moralistic attacks on people's characters, especially not the version where one invokes present day moral values to attack people for how they lived in eras with different moral values.

While there's nothing wrong with a Penrose biographer explaining why his first marriage was an unhappy one, the reasons this was unhappy seem to me generic ones afflicting late 1950s marriages.

it seems to me that he's trying to make Penrose look bad for having a garden-variety late-1950s unhappy marriage. He's turning having a standard problem common in one's time into a moral failure

22. **Phil H**
November 20, 2024

Re: the singularity theorems, I'm not so sure that the "creative ideas there were more Penrose's, with Hawking much better at getting attention for his work".

Remember that Hawking and Ellis published "The large scale structure of space-time" in 1973. This book was for many years the standard reference for the application of global techniques in general relativity, i.e. it provided a useful resource for researchers in the field rather than being a publicity stunt. (Penrose might be referring to the Hawking personality cult that resulted from the publication of "A Brief History of Time" in 1988, but as Jane Hawking explained in her book "Travelling to Infinity: My Life with Stephen", that latter book arose from financial necessity rather than any need for attention or publicity.)

On twistors: it appears that much of the 20th century research was circulated via the twistor newsletter, and continued to be so for many years. A fellow PhD student was very interested in this material, so while on a visit to England in the early 1980s, I asked Penrose for copies. He refused to provide anything at all. So not "more likely to be friendly and helpful to others than your average academic" IMHO.

23. **Robert Cochrane**
November 20, 2024

Peter Woit and Phil H

The Twistor Newsletter was originally partly handwritten when mathematical typesetting was very difficult and was mimeographed (manually rotary stencilled) for circulation. It included work by a range of mathematicians, was not intended to replace publication and could be speculative. The editions even up to the early 1980s ran to hundreds of paper pages. To me, with memories of circulating newsletters in those days, It is not surprising that even a man of Roger Penrose' well known generosity rejected a request for copies of previous editions made on behalf of a third party.

On a more positive note, the entire archive is now freely available online thanks to the Oxford Mathematical Institute and its staff and contributors at:

https://people.maths.ox.ac.uk/lmason/Tn/

24. **Alex V**
November 20, 2024

"While Hawking often gets more attention for this, it seems that there's a good case that the creative ideas there were more Penrose's, "

After reading both papers, I was definitely left with the impression that the ideas used to prove the singularity theorems are Penrose's, while Hawking seems to have only applied those techniques to the case of a cosmological singularity.

Also, does anyone know if there has been a rebuttal to Roy Kerr's claim that the singularity theorems do not prove the existence of actual singularities?

25. **Andrzej Daszkiewicz**
November 20, 2024

Alex:
there is a video at the Phil Halper YT channel https://www.youtube.com/watch?v=foq4nVAwEao where some people, including Senovilla, Ashtekar, Warner and Penrose himself point to a crucial error Kerr made in his paper. I am not an expert, but they all seem to make a good point. Warner even plans to make it a homework problem in his future courses on GR: find the error in Kerr's argument.

26. **Scott Caveny**
November 20, 2024

Re: the singularity theorems, I'm not so sure that the "creative ideas there were more Penrose's, with Hawking much better at getting attention for his work".

In their chapter on global techniques and singularity theorems, MTW's Gravitation described in 1973 Penrose's `pioneering theorem on singularities` of 1965 as `the birth of global techniques` (employed by the primary workers in the field Geroch, Ellis, Hawking, etc).

27. **John Baez**
November 21, 2024

Kerr's argument is wrong. See this answer on Physics Stack Exchange:

https://physics.stackexchange.com/a/791566/112768

28. **Peter Woit**
November 21, 2024

All,
Sorry, but I'm not going to allow more comments debating the Kerr claims. This is a technical issue that has nothing to do with the book. From the limited amount I've done of looking into this, if there's anything there, it's a not very interesting issue about exactly what you mean by "singularity".

29. **Matthias**
November 22, 2024

On another physicist, Cambridge University Press has just released the online version of Steven Weinberg: A Life in Physics. Available here:
https://www.cambridge.org/core/books/steven-weinberg-a-life-in-physics/BB818FF83303ED57F5B1894A7CF5C178#

30. **Phil H**
November 22, 2024

Recent talk at the Isaac Newton Institute by Penrose, "From the Origins of Twistor Theory to Bi-Twistors and Curved Space-Times" – https://youtu.be/wWRBX_UtIu8?si=YiySvD73D0MxAgQ1.

Brings the history up to date.

31. **Cormac O'Raifeartaigh**
November 24, 2024

I met RP once at a GR centenary conference in King's college, couldn't get over how gentle and unassuming he was. We had a lovely quiet lunch together where he asked a lot about Dias and my late father. I met him again a few days later at another historical conference in Berlin, whereupon he treated me like an old friend!

32. **Thomas**
November 24, 2024

By the way: Weinberg's memoir, just published by Cambridge press, is very interesting.

This Week’s Hype

November 22, 2024
Categories: This Week's Hype

If a post-truth field of science is going to keep going, it needs to convince funders and the public that progress is being made, so there’s a continual need for people uninterested in truth and willing to produce appropriate propaganda. This is the 142nd edition of This Week’s Hype, which has been documenting this phenomenon for the past twenty years.

Such a post-truth project requires cooperation from institutions responsible for communicating science to the public. One such is the Royal Institution which sponsored a program of pure propaganda for string theory, now available on Youtube. From the transcript:

> I’m not in propaganda mode here, and we shall avoid propaganda mode… As you see, I’m trying not to go into propaganda mode… Once again I’m in no propaganda mode, but we are fairly sure…

If a speaker four times in a talk assures you that what he’s saying is not propaganda, one thing you can be sure of is that it is propaganda.

Another part of maintaining a post-truth scientific field is that you need people willing to write propaganda “scientific” articles, institutions willing to publish such articles and venues to promote them. A good example of this is The Standard Model from String Theory: What Have We Learned? now published in The Annual Review of Nuclear and Particle Science.

The publisher of Annual Reviews has a publication called Knowable Magazine, tasked with promoting their articles, and they’ve hired Tom Siegfried to write about this one under the title String theory is not dead. By the way, if somebody is hiring journalists to write propaganda pieces entitled “Field X is not dead”, you can be sure that field X truly is dead. Siegfried has had a very long career in the string theory propaganda business, going back nearly 30 years. See for instance this posting, which has some background on Siegfried.

In his very hostile review of *Not Even Wrong* for the New York Time, Siegfried explains that I’m completely wrong about string theory’s lack of predictions:

> …string theory **does** make predictions — the existence of new supersymmetry particles, for instance, and extra dimensions of space beyond the familiar three of ordinary experience. These predictions are testable: evidence for both could be produced at the Large Hadron Collider, which is scheduled to begin operating next year near Geneva.

Like all of those in the post-truth business, having one’s “predictions” turn out to not work doesn’t have any impact at all on one’s willingness to keep the propaganda campaign going.

A good giveaway that something is propaganda is a title that indicates that you’re not going to get just information about something, but also a sales job. Today the Higgs Centre in Edinburgh has a talk scheduled with the title What is string theory and why you should care?. The idea that people at a theoretical physics center would not know what string theory is after the past forty years is pretty laughable, so clearly the point of this talk is not the first part of the title, but the “you should care” part.

Update: Video of the Higgs Centre talk by string theorist Sašo Grozdanov is now available here. As usual in such things, lots of discussion of the quantization of the single-quantized theory of a bosonic string, which connects not at all to physics. No discussion of why the much more complicated things you would need to do to try and make this look like physics simply don’t work. Grozdanov’s acknowledges criticism of string theory, but claims that it’s just “sociological”, coming from people who are too impatient. According to him (and he says he’s embodying the consensus of the field):

- “It’s the only way forward”
- “We have nothing else”
- “It’s the only thing that works”

He acknowledges there’s no connection to the real world, interprets this though as only indicating that “we’re missing something” (since alternatives are not conceivable).

Comments

1. **Leo**
   November 22, 2024

   Dear Peter,

   Specifically regarding the hype on IA applications to quantum gravity/unification/theoretical physics I think this is about to go on steroids and as evidence there is this recent interview with Nobel Laureate Demis Hassabis

   https://www.youtube.com/watch?v=pZybROKrj2Q

   in the section of peer into the mysteries of the universe.

   If I remember correctly, he hopes to be able to apply it in the next 10-20 years and mentions wormholes and string theory. Luckly at least he mentions he dislikes the Multiverse….

2. **Peter Woit**
   November 22, 2024

   Leo,
   String theory hype is bad enough, last thing I want to do is get involved trying to do anything about AI hype. Took a quick listen to the Hassabis comments about the how AI and the end of all resource limit will allow us to operate at the Planck scale. Presumably the next generation DeepMind will explain to him at some point that he’s a fool.

   All, please don’t take this as an invitation to start discussing AI, the world is depressing enough as it is.

3. **Velvet**
   November 24, 2024

   Looking at the mostly negative comments under the video, we can see that even the general public no longer believes in string theory in recent years.

Don’t worry Woit, string theory is dying, slowly but surely

4. **Peter Woit**
November 24, 2024

Velvet,
Yes, and this kind of thing hastens the process. A typical reaction to a “String theory is not dead” headline likely is something like “Oh, that’s interesting, I hadn’t known that the consensus is that string theory is dead.”

5. **Peter**
November 24, 2024

Peter, are you happy or unhappy with the Siegfried piece? I thought it was great – one of finest pieces of “damning with faint praise” I’ve seen in recent months.
You write that Annual Review wanted to “promote” an article with the Siegfried piece. Well, it works, but probably not as they intended. The only reason why I wanted to read their article was because of a very perverse curiosity. How do you stay a True Believer when – as Siegfried writes – basically nothing works out as planned?
It’s also telling that the title, “String theory is not dead”, is nowhere to be found in Siegfried’s piece. It feels like the work of some sub-editor who, after a desperate search, couldn’t find anything better to say about the subject.

6. **Peter Woit**
November 24, 2024

Peter,
A good point that the title may not have been chosen by Siegfried.

Siegfried always struck me as what the string theorists might describe as a “useful idiot”, someone who had no understanding of what he was writing about, but could be relied upon to try and say something positive. That has become increasingly more difficult, so even a Siegfried at this point can only come up with praise that is rather faint.

He’s trying to portray the situation as:
” Success remains elusive, but real progress has been made. Questions plaguing physicists about not only the smallest bits of matter but also the properties of the entire universe may yet yield to string theorists’ efforts.”
where an accurate statement would be that no progress has been made, rather things have continued to go in the wrong direction.

Near the end there’s a section title “Tests are possible” which is the same dishonest bullshit that was in his NYT review of my book. Since it’s a section title, not in the main text, again this may be written by an editor, not him.